Kinship, Cohesion, and Space-Time:

A Network Analysis of the Indigenous Population of the Murray Darling Basin

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ABSTRACT

This thesis presents a formal empirical anthropological analysis of the Indigenous population of the Central Murray Darling Basin (CMDB) region of South East Australia. The first part of the analysis comprises a review of past theoretical assumptions underlying social anthropological models of Indigenous population dynamics generally. The second part comprises a conventional kinship network analysis of a 3138-person population model, and introduces a newly developed spatio-temporal kinship network analysis of the same population model. Results are compared.

The mainstream discipline of social anthropology, especially as practiced in Australia, is not a formal science but rather more closely aligned with philosophy and the humanities. It is nevertheless concerned with a field of study - human sociality - that also concerns a number of scientific disciplines. Despite this overlapping concern there is no disciplinary integration or 'consilience' between social anthropology and these other scientific fields. Irrespective of this lack of consilience, social anthropology has, since the early 1970s, been treated as a forensic science by the Australian judicial system with regard to legislation affecting Indigenous people. The result has been a slowly developing recognition that the discipline of social anthropology lacks a formal paradigmatic basis, and as such, is of limited use in either a forensic application, or in any other field that requires evidence based research, such as health and demography.

Historically, social anthropological research into Indigenous Australian sociality has played an important role in the discipline's theoretical development. Since the inception of social anthropology in the 1870s, research conducted with Indigenous Australian people has formed a key feature of all major theoretical texts, and has been used to support key theoretical assumptions. Most important among these assumptions has been a loose collection of tacit mathematical concepts, which correlate with basic principles of group theory. These group theoretic principles can be traced through more than a century of key texts in social anthropological theory, especially with regard to theories of language and kinship. The explication of this pattern forms the focus of the first part of the thesis.

The second part of the thesis introduces a relatively new methodology within social anthropology that has set as its goal the formalisation of the discipline, including a reorientation towards empirical evidence and integration with other fields of science concerned with human sociality. A key technique in this methodology is social network analysis (SNA), and its specialised form, kinship network analysis (KNA). Using this latter technique, the thesis re-analyses a number of group theoretical assumptions in Australian and international anthropological texts, and compares these with fresh analyses of newly assembled Indigenous population data from the CMDB region. A further, novel technique of spatiotemporal kinship network analysis (stKNA), developed by the author, is then also introduced and applied to this new data. This analysis indicates that rather than forming groups, Indigenous Australian populations are characterised by extensive and cohesive kinship networks spanning great geographic distances and deep historical time.

DECLARATION

This thesis comprises my own original work towards the degree of Doctor of Philosophy. Due acknowledgement has been made in the text to all other material used. Exclusive of figures, tables, bibliographies and appendices, the thesis is 133,801 words in length as approved by the Research Higher Degrees Committee.



James Rose, 23 June 2017

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Finally and most emphatically I would like to express my gratitude to people of the Murray Darling Basin, including the women, men, and young people to whose awesome and enduring culture this thesis is dedicated. Hopefully the research presented here will contribute something in the way of recompense for the words and deeds of both my personal and professional ancestors.

NOTE ON THE USE OF DIAGRAMS, GRAPHS, MATHEMATICAL EQUATIONS, AND 3D MODELS

This thesis utilises visual diagrams, graphs, and mathematical formalisations to illustrate certain key principles. While each figure and equation is accompanied by a long-form explanation in the body of the surrounding text, I have endeavoured to provide concise, plain language summaries in the accompanying captions as well. While mathematical equations are provided as formal demonstrations, they are not essential to the accompanying arguments, and may be skipped by readers. Equations have been enclosed in boxes to indicate this optional status.

Most three-dimensional models presented in this thesis can be viewed and manipulated on the Internet using the links in the accompanying captions. These models can also be downloaded to personal computers for offline viewing and manipulation. Models suffixed '.skp' can be viewed using the program SketchUp (www.sketchup.com). Models suffixed '.kmz' can be viewed using the program Google Earth (www.google.com/earth). Both of these programs are free to download and use, and can be installed on a range of operating systems.

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Chapter 0

General Introduction

0.0 Introduction

This thesis presents new research on the role and effects of historical time and geographic space on population dynamics, modelled in terms of kinship network models. The thesis uses the technique of kinship network analysis (KNA) to demonstrate autochthonous theories of choice, which guide decision-making at the micro-level of spousal selection, fertility, migration, and mortality, both reflect and influence the ways in which a population fluctuates in space and time at the macro-level of hundreds and thousands of people. The cumulative, measurable pattern of this fluctuation is referred to here as a population's spatiotemporal signature. The ultimate function of the thesis is to present a new technique for incorporating measurements of space and time into the graphic modelling of kinship networks, and visualising those effects. This new technique is termed here *spatiotemporal kinship network analysis* (stKNA), and allows kinship networks to be presented as colour-coded three-dimensional structures superimposed over geographic maps, such that population movement, and changes in population structure over time can be perceived intuitively as well as formally. This technological innovation is designed to fill a number of epistemic gaps in administrative and expert fields dealing with human sociality, including law, demography, and social anthropology.

In the terms of a potentially unified social science, described in detail in Chapter 1 below, the global human population is like any population defined by common speciation in being fundamentally kinship-based, deriving from a single ancestral origin, where relationships linking successive generations both to each other and among themselves, are modelled as kinship relations. Traditional models of speciation characterise the outgrowth of new species as 'tree-like', with individual vertices diverging from a common branch, in turn giving rise to subsequent branches (Stringer 2015). This traditional conception is frequently carried over into models of intra-special population diversity, giving rise to misconstrued folk definitions of variation within populations as the result of perpetual differentiation or 'branching'. In the social sciences this folk model has given rise to the theories of 'race', and 'tribe'. Significant analytical innovation over the past 15-20 years is now revealing this model to be inaccurate, both in terms of evolutionary biology generally (Posada & Crandall 2001; Huson & Bryant 2006) and in terms of human populations more specifically (Stringer 2015). These studies indicate that branching is both preceded and followed by relinkage *between* branches, giving rise not to tree-like structures, but to reticulated networks comprised of multiple relinkages. Within the field of social anthropology, the advent and refining of computational techniques for building and analysing large-scale kinship models is also indicating that systematic relinkage between so-called 'descent linages' is a critical feature of population cohesion. These studies further indicate that such relinkage is a deliberate and managed feature of all human marriage and fertility cultures where social cohesion is a corollary of population health (Kawachi & Berkman 2000). Variation between marriage and fertility cultures tends to occur within a limited spectrum of preferences for particular forms of relinkage, known as *matrimonial circuits* (Hamberger, Houseman, & White 2011 - see below). The current thesis focuses on this form of relinkage in both the culture and structure of the

Indigenous population of the Central Murray Darling Basin Region of South East Australia. It uses this example to demonstrate the fundamental role of space-time in the survival and cohesion of human populations, and in giving rise to regional cultures of sociality.

Existing techniques for modelling and analysing relinkage in human populations derive from the field of social network analysis (SNA), which construes real-world interaction between individuals[1] in terms of paths between vertices (or 'nodes'). Both paths and vertices can be categorized or 'signed' according to a potentially infinite set of observed attributes, and very large, complex network models can be constructed and represented as visual graphs. These graphs can be queried for patterns of relationship that would be otherwise undetectable to conventional anthropological and demographic studies. Kinship network analysis (KNA) is a branch of social network analysis that focuses on a specific subset of vertex and path attributes, namely the gender of vertices ($male, female, undefined$), and the kinship function of paths ($parent \rightarrow child, spouse \leftrightarrow spouse$). Within the broader field of social network analysis, kinship network analysis is highly specialised, particularly in relation to the effects of temporal flow, popularly conceived of as generational sequencing, which mediates between existing network structure and subsequent network development. In kinship network analysis temporal flow is a representation of the effects of time on the way new $parent \rightarrow child$ and $spouse \leftrightarrow spouse$ relationships are formed. With regard to $parent \rightarrow child$ relations for example, this explains the successive relationships between grandparent, parent, and child, whereas with regard to $spouse \leftrightarrow spouse$ relations, it explains the contemporaneous relationship between a father and mother that presages the birth of a child.

Both SNA and KNA construe network structure in terms of topology. Topology is a collection of geometric principles that describe the consistency of structure at varying scales, where the shape and location of a network in real-world space and time do not have any effect on the structure of the network. In topological terms, the structure of a network is determined by the attributes of, and relations *between* vertices, rather than the real-world space-time location of the vertices or relations themselves. For conventional network analytic purposes, this is not a problem, and in fact makes network modelling amenable to variable visualisations, since network shape and location can be manipulated in order to highlight different processes without affecting underlying network data represented *by* the graph. In kinship networks for example, graphs are typically manipulated to highlight horizontal generational layers, and vertical descent linages, which helps communicate the effects of time flow on emergent network structure, as successive generations of descendants follow one another. However, by excluding the real-world spatial and temporal attributes of each descendant, and the relationships between them, any causal link between space-time and network

[1] The term 'real-world' is conventionally used in social network analysis to refer to phenomena in observable data, as distinct from phenomena construed in theoretical models. Since network structures are features of both observable and data and theoretical models, the caveat 'real-world' is used here in accordance with this convention.

structure is rendered undetectable. Such real-world space-time measurement and analysis is known as topo*graphy*, and does not feature in the established repertoire of techniques used in either SNA or KNA. New techniques presented in the latter part this thesis incorporate topographic techniques into KNA topology in order to render their effects both discernable and measurable.

In the history of social anthropological and demographic research into Indigenous Australian populations, individual Indigenous status has generally been held to derive from inherent, self-evident attributes rather than from systemic population level processes such as historical and geographic distribution. From 1788 until the late 1960s these attributes were treated as primarily morphological and secondarily cultural, while since the early 1970s the ratio has been inverted so that the allegedly inherent status of 'being Indigenous' has been increasingly held to be cultural, and secondarily morphological. In other words, prior to the late 1960s people were counted as Indigenous if their skin was dark, and non-Indigenous if there skin was light, the underlying assumption being that this classificatory exercise reflected an objective criterion. Since the early 1970s people have tended to be counted as Indigenous if they identify themselves and/or are identified by others as Indigenous, the underlying assumption being that this semogenic exercise reflects a cultural criterion.

As outlined below, all thematic shifts in demography and social anthropology over the past century have conformed to either one of these two classificatory parameters in the disciplines' attempts to control the definition of the Indigenous Australian population. At no point has either demography or social anthropology managed to refine a definition of Indigenous status other than as an inherent personal attribute. The effects of this configuration on government policy and popular folk models of the Indigenous/non-Indigenous distinction have been largely negative, since all accepted definitions have been construed as a property of individuals, rather than as a social condition. The logically deductivist conclusion is that Indigenous status is reducible to personal style, however so defined, i.e. as either personal appearance, personal conduct, or a combination of both. Thus individuals are said to 'have' or 'not have' Indigenous ancestry, and/or to either 'have' or 'not have' Indigenous culture. In folk models, 'traditional people' are popularly held to possess both sets of personal attributes, while 'historical people' are popularly held to possess little or none of either, despite being acknowledged as Indigenous. All major public disagreements in the administrative fields that affect Indigenous Australians, such as law, academia, and media, are oriented towards proving who possesses or does not possess either the relevant ancestry or culture. Political capital in these fields is traded on the likelihood of one or another definition of ancestry or culture prevailing[2]. The recent development of

[2] With regard to definition by descent, recent examples of such trading include attacks by reactionary non-Indigenous media on the *ancestral* status of well-known Indigenous literary and academic figures, see Eatock v Bolt [2011] FCA 1103. With regard to definition by culture, recent examples include attacks by progressive Indigenous and non-Indigenous commentators on the *cultural* status of well-known Indigenous academic and legal figures as 'unrepresentative' of Indigenous people. A common

the 'People vs Population' framework (Rowse 2008, 2013; Taylor 2009), described below, has not yet ameliorated this situation, and key government agencies such as the Australian Bureau of Statics and the Australian Institute of Health & Welfare are at a loss as to the causes of bourgeoning indigenous self-identification in census data that significantly exceeds known fertility rates for Indigenous women.

Taylor (2009) precisely delineates the challenge now faced by demographers in replacing mainstream population demography with an Indigenous demography based on categories meaningful to Indigenous Australians:

> *"[T]he notion of an Indigenous population is an artefact of the use of some 'Indigenous identifier' question, whereas an Indigenous people is a more complex political artefact - 'a **dense texture of kinship, association and normative regulation**'. To date, what is commonly referred to as 'Indigenous demography' in Australia has been overwhelmingly population-focused, not people-focused. ...*
>
> *"Demographers have found a natural and successful niche in responding to the needs of government for gap analysis using census, survey and administrative data. While this produces useful input to macropolicy development and monitoring, translation of this analysis into coherent policy on the ground is often thwarted by the lack of an Indigenous peoples' demography. This is because **the categories and contexts deployed are uncritically those of the mainstream and are often not reflective of Indigenous institutions** or what Blaser (2004) and Peterson (2005) call 'life projects' reflecting the desires of Indigenous people to achieve meanings of life that are autonomous, independent of the State and the market. ...*
>
> *"[W]hat [might] an Indigenous peoples' demography look like[?] What categories and contexts of Indigenous social institutions might undermine the application of standard categories? While such questions are stimulated by the broad emerging dialogue between demography and anthropology it is ironic that Australian demographers largely pioneered this dialogue (Taylor 2009:118 - emphasis added).*

This sentiment illustrates an acute awareness in Australian demography of "a need for a more socioculturally sophisticated approach to demography" (Taylor 2009:121), construed as a more subtle rendition of the 'texture' of culture, as Taylor put is, however there are currently no mechanisms for

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theme on criticisms of Noel Pearson, Warren Mundine, Bess Price, Rachel Perkins, and Marcia Langton, is that they are not 'representative' of Aboriginal culture (Crikey, New Matilda, The Conversation, e.g. ' Pearson's grip on Indigenous policy is not backed by evidence' The Convsersation 20/8/2014;).

achieving this in a quantifiable way that might actually yield useful data. Taylor phrases the problem as follows:

> *"While such approaches undoubtedly add **texture** and **meaning** to analyses of human mobility, **what is generally neglected is the development of measurement techniques that capture the complexities of movement over space and time and facilitate comparison**" (Taylor 2009:123 - emphasis added).*

The underlying problem that Taylor identifies is a demographic paradigm in which key metrics of mortality, fertility, and migration are treated as classificatory signs, the referents of which are disconnected individuals within a group-theoretic population model. This is a model that is construed as a kind of bounded 'receptacle' in which people are contained, defined by the *a priori* classifications of demographers and anthropologists. Whether people fall inside or outside of that group theoretic receptacle is determined by the classification of the individuals themselves, since there is no means in conventional demography for classifying the relationships between individuals. Yet, in appealing to anthropology for assistance in transcending this limitation, demography is appealing to a discipline that also lacks a formal, self-consistent language for describing these relations.

In response to this shortcoming, initiatives such as the ABS' Australian Indigenous Geographic Classification (AIGC) framework were developed in order to attach a more specific geographic context to mobility data for Indigenous population dynamics. However, because these were still limited to a purely statistical paradigm, they maintained the 'container' approach, simply adjusting the parameters of the container to *a priori* geographic boundaries. As Taylor observes:

> *"While this is an attempt to get up close as it were, to produce a 'demography in situ', in some ways it misses the point altogether since the primary organizing principles of Indigenous social formation are both spatial and socio-relational and these invariably do not coincide to produce discretely bounded social groupings that mesh with units of the AIGC. In effect, the Indigenous cultural map is vastly different from that imposed by the AIGC. Consequently, formal statistical geographies are unlikely to provide for a demography of Indigenous polities that have rights and interests in particular places" (Taylor 2009:124).*

The 'spatial and socio-relational' organising principles to which Taylor refers are in fact those of kinship. Although the more obvious institutions of traditional language, religion, and economic activity might seem to be the most relevant in terms of the 'culture differentiating' idioms of classical social anthropology, which highlight immediately observable action at the expense of deep structure, all are symbolic *super*structural instantiations of the much more fundamental and pervasive *foundational* structures of kinship. The data and modelling presented in this thesis aim both to illustrate this dynamic, and to show how it can be discerned, quantifiably measured and intuitively

visualised in a way that makes it "a useful input to macropolicy development and monitoring" (Taylor 2009:124).

In the configuration provided by stKNA, the demographic object of 'Indigenous status' is construed as the emergent attribute of a population that exhibits a characteristic *spatiotemporal signature*. As elaborated at length in Chapter 5, a spatiotemporal signature is the set of points in space-time that can be measured as members of a population move through it, and thus give their encompassing social network temporal depth and geographic breadth. For the kinship networks that constitute the Indigenous population of Australia, these points comprise a subset of all possible points that people might occupy, firstly because these points are the observable attributes of individuals whose membership of the network is defined by their kin relations to one another, and secondly because they belong to a network defined by a set of events that form the basis of all kinship networks, namely birth, marriage, and death (Rose 2008), or as demography construes it, fertility, migration, and mortality. Whether or not an individual member of such a network exhibits a micro-level instantiation of this signature in the course of their own life trajectory is irrelevant to the encompassing macro-level structure of the entire network. In other words, the overall signature of a network does not depend upon conformity of signatures on the part of every individual in the network. Rather it is the decisions of each network member about when, where and with whom they enact kinship relations that contributes to the growth and development of the total network, and with it, emergent phenomena that cannot be predicted at the micro-level of individual acts and interpersonal interaction. Everybody within the network is thus carried along through space and time together. *Yet* each person contributes to that carriage. This new form of modelling - *spatiotemporal Kinship Network Analysis* - is the original contribution of the current thesis.

This current introductory chapter is divided into three parts. Section 0.1 outlines the context in which the study was undertaken and the motivation that forms its basis. This section describes the current state of social anthropology in Australia in terms of its interface with the judicial system's implementation of the Commonwealth Native Title Act (1993). Successive federal court decisions under this act, in which anthropological evidence is tendered in support of Indigenous claims to traditional land and resources rights, have found the state of that evidence to be substandard and incommensurate with other forms of expert forensic evidence. This broadly consistent judicial view forms the primary motivation for the development of the stKNA technique, which is designed to meet forensic standards outlined in landmark court decisions. Section 0.2 sets the more specific goal of the thesis in developing the stKNA technique in order not only to meet forensic standards of Australia's judicial system, but also to fit within a formally re-defined discipline of social anthropology that renders is it consistent with adjacent disciplines in the social sciences. Such a 'consilience' (Wilson 2001) opens up the possibility of for the technique's application in other fields of study, in social science and beyond. In explaining how such an application would operate, Section 0.2 introduces the terms and definitions of social anthropology's current 'crisis' of representation. Section 0.3 lays out

the strategy for the execution of the plan outlined in Section 0.2, spelling out the structure of the thesis and briefly summarising each chapter.

0.1 Context & Motivation

In presenting and demonstrating the new research technique of spatiotemporal network analysis, this thesis contributes to an international movement aimed at formalising the discourse of social anthropology. The overarching goal of this movement is to transform social anthropology into a discipline that espouses an *integrated* and *useful* theory of the subject matter in which it claims expertise, namely human social culture. Social culture, in this context, comprises an integrated system of social conduct within a given population, together with the autochthonous theories of behaviour, held by members of that population[3]. Although implicit in social anthropological discourse for much of the discipline's history, this definition has in recent decades become largely obscured, to the point where many practitioners find it difficult to articulate (Leaf & Read 2012).

As elaborated over the course of Chapters 1, 2 and 3, the discourse of mainstream social anthropology - that is, its accepted theory and methodology - has endured an inconsistent developmental history over the past 150 years. Most remarkably, as described in those chapters, it has transitioned since the early 1970s from weakly formal proto-science, to strongly *in*formal literary philosophy. This has resulted in its divergence from mainstream scientific discourse and a consequent dwindling in its already limited influence and explanatory power among related disciplines. These changes have not gone unnoticed in other expert and administrative fields, for which human social organization is of major interest. Apart from appearing alluring to demography, as outlined above, social anthropology's specialization in Indigenous social culture is also deemed important by the Australian legal system. Since the late 1960s it has emerged to play an important role in legal negotiations between Indigenous, government, and industry stakeholders over natural resource access. Judicial decisions have this provided some of the most incisive measures of the discipline's changing configuration over the past 45 years.

In what was possibly the first non-academic test of the discipline's expert status in Australia, a social anthropologist was called to give evidence in a 1971 legal claim by Yolngu traditional owners in Australia's East Arnhem Land. The claim was made against a bauxite mining company that had been granted a 12-year mining lease over Yolngu land by the Australian government, but without consultation with Yolngu landowners. The plaintiffs asserted that the mining lease was legally invalid, and an infringement on their pre-existing tenure rights. They submitted the findings of the anthropologist as a key part of their claim. As part of its defence, the mining company challenged the admissibility of this evidence, which resulted in the Judge addressing the matter directly in his decision. The relevant quote is worth giving at length since it provides a comprehensive snapshot of

[3] This is what distinguishes anthropology from formal demography on th one hand, and on the othr hand from what some anthropologists refer to as 'ethnography' - a kind of informal demography targeted at ethnically distinct populations.

anthropology's disciplinary reputation at the end of the 1960s, indicating that it was considered very much a scientific discipline:

> *"The court is expected to rule on the qualifications of an expert witness, relying partly on what the expert himself explains, and partly on what is assumed, though seldom expressed, namely that there exists a general framework of discourse in which it is possible for the court, the expert and all men [sic] according to their degrees of education, to understand each other. ...*
>
> *"My ruling is based on accepting that there is a valid field of study and knowledge called [social] anthropology which deals with the social organization of primitive peoples (the definition will serve well enough for the purpose in hand). The process of investigation in the field of [social] anthropology manifestly includes communicating with human beings and* ***considering*** *what they say. The anthropologist should be able to give his opinion, based on his investigation by processes normal to his field of study, just as any other expert does. To rule out any conclusion based to any extent upon hearsay the statements of other persons - would be to make a distinction, for the purposes of the law of evidence, between a field of knowledge not involving the behaviour of human beings (say chemistry) and a field of knowledge directly concerned with the behaviour of human beings, such as anthropology. A chemist can give an account of the behaviour of inanimate substances in reaction, but an anthropologist must limit his* ***evidence*** *to that based upon what he has* ***seen*** *the aboriginals doing, and not upon what they have said to him.*
>
> *"I do not believe that the law of evidence requires me to put chemistry into one category and anthropology into another. The matter can be tested, it seems to me, by applying the analogy of medical evidence, with which courts are so familiar. This is a field in which the 'facts' include both what the expert observes and what he hears from other persons - his patients. A medical expert gives evidence in this form: 'I have studied the subject of coronary heart disease. This study includes observing the anatomical and physiological facts; talking to patients and considering what they say; studying the literature, etc. I express the opinion as an expert that proposition X is true of coronary heart disease.' There is no doubt a considerable difference in degree between the extent to which statements made by other persons form the basis of conclusions in clinical medicine and in anthropology; but in my opinion it is not a difference in kind. (The example I have given by way of analogy has, of course, nothing to do with the rule that a medical witness may repeat what a patient said to him for the purpose of establishing the foundation for his opinion of that particular patient's condition)" (Blackburn J 1971:161-162).*

Although the plaintiffs ultimately lost the case, the precedent set by the hearing lead to the establishment of multiple legal frameworks for recognising Indigenous Australians' inherent land and resource tenure. These include various state and territory land rights acts introduced during the late

1970s and 1980s, and most recently the Commonwealth Native Title Act (1993), which has become the predominant federal framework for agreement-making between Indigenous land owners, government, and business. Justice Backburn's 1971 judgement is relevant to the current thesis not only because it formally designates social anthropology as the field of expertise most relevant to the study of Indigenous social culture, but also because it explicitly describes the basis on which it was considered, in 1971, to comprise a field of expertise comparable with any other field deemed admissible by the court. Blackburn's exemplary comparison of social anthropology with the fields of chemistry and medicine is highly pertinent. In 1971 social anthropology was generally considered, both by anthropologists and experts in other fields, to be following a common albeit delayed developmental trajectory towards full integration as a scientific discipline like any other. This was not to be the case however.

From the mid-1970s onwards, social anthropological discourse began to exhibit signs of what has come to be called 'the post-modern turn' (Harvey 1990). As elaborated in Chapter 2 in particular, this was the last of approximately four broad consecutive shifts or 'turns' among 20th century liberal arts disciplines, each of which consisted of major changes in their theoretical themes. Each turn incorporated criticism and rejection of existing or 'old' theoretical models, followed by the introduction of ostensibly 'new' ones to completely replace them, without any attempt to integrate even successful elements of preceding models into the new frameworks. The deliberate avoidance of integration between these thematic frameworks came to be described as 'radical breaks', and has been held up by liberal arts disciplines as evidence of their perpetual re-enlightenment (Harvey 1990; Jameson 1984). Between the emergence of social science in the 1870s and the present, these shifts have moved broadly through 'Historicism' (society as constant improvement), 'Functionalism' (society as having a purpose), 'Structuralism' (society a being organised), and finally 'Postmodernism' (society as having meaning).

During the 22 years that passed between the Gove land rights case and the introduction of the Native Title Act, anthropologists' involvement in natural resource negotiations had been constrained by narrow procedural reporting requirements of various states' land rights acts, most notably that of the Northern Territory, where most claims had been made under that jurisdiction's 1976 Aboriginal Land Rights Act (ALRA). Under this commission-based regime, social anthropologists are required to compile so-called 'land claim books', which generally take the form of catalogues of mythology, sacred sites, kinship terminologies, and claim group membership, all of which are held to define in turn the religious basis of the tenure, geographic extent of the estate under claim, and kinship composition of the estate-owning group. Because of the statutory rather than litigious threshold for establishing land rights, an 'Aboriginal Land Commissioner' may make a positive finding under the ALRA when the relevant criteria are met by procedural assessment. This is very different to the Native Title Act, under which evidence is assessed in an elaborate two-stage process, first in in a procedural test by a statutory tribunal, then by respondent parties who oppose the claim. If plaintiffs

and respondents cannot come to a negotiated agreement in relation to this evidence, then the matter is decided before a judge in the Federal Court. Although more burdensome, this latter framework opens the door to a much more dynamic and flexible construal of the evidence by experts, compared with the narrower albeit more efficient land rights framework. From a retrospective hypothetical standpoint it could have been possible for social anthropological research to evolve a new, more precise and technical sophistication under these newer, more forensic circumstances. As outlined below however, this has not yet happened.

In the view of the author, two factors contributed to the ensuing poor reception that post-1970s anthropological evidence encountered before the courts under the native title regime. Firstly, the ALRA and similar 1970s- and 1980s-era legislation had not made significant theoretical or methodological demands of the discipline. This allowed many anthropologists to operate on a kind of 'auto-pilot' when researching and compiling land claim books. As important and labour-intensive as many of these reports are, their prescribed catalogue-like formula requires little or no theoretical or methodological innovation beyond what had already been established by the late 1960s. Secondly, the advent of the 'post-modern turn' had led many social anthropologists to consider 'the judiciary' in a relativistic manner, as a mere construct of hegemonic Western European social culture, and an extension of 'the state'. Both were construed as the abstract antitheses of a perpetually 'subaltern' Indigenous population, of whom anthropologists held themselves to be the natural defenders (see e.g. Kapferer 2012) Thus, when social anthropologists began contributing expert reports to native title hearings, the court encountered a number of problems with the evidence.

Firstly, Federal Court judges began to notice that many of the experts giving evidence appeared to be openly advocating on behalf of claimants, whereas the condition of their evidence's admissibility was supposed to be its objectivity. Secondly, many of the anthropologists appeared to be incapable of non-literary reporting. Whereas the technical expert reports to which the court was accustomed typically consist of clearly distinguished observations and analyses (as per Blackburn J's account of chemical and medical evidence), social anthropologists tend to write in an editorial style, complete with rhetorical argument, hyperbole, and anecdote. Most worryingly for the court, many anthropologists seemed unable or unwilling to distinguish between observations they had made on the one hand, and their own speculative assertions on the other, instead blending them together into elaborate unified statements. The net impact on the court's perception was that a significant number, if not most, anthropological reports lacked any consistent or systematic reasoning linking observed behaviour to theoretical models of society. In the court's words, there tended to be little distinction, or explicated link between, facts and opinions.

In 2005, 34 years after the Gove land rights case, and 12 years after the introduction of the Native Title Act, Selway J made a positive finding in relation to another claim by Yolngu people over another area in East Arnhem Land, known as the 'Blue Mud Bay' decision. The position of social anthropology

was by now firmly institutionalized within the native title sector, and a significant number of Australian anthropologists, the author included, had come to specialise in it. Justice Selway was not happy with the performance of the field however, and singled out the expert evidence for special mention:

> *"There are at least two specific problems which seem to recur with such evidence in litigation concerning aboriginal rights. The first is the perceived problem of partiality ... [where] the evidence has more the ring of a convinced advocate than a dispassionate professional ... [and which] may even affect its admissibility ... The other specific problem ... relates to the form in which written anthropological reports often seem to be prepared. These often seem to be in the form of a book setting out various facts and conclusions in an interesting and informative way. Often there is no obvious link between the methodology adopted by the expert (be it observation, discussions, reference to the published literature or the expression of an opinion) and the apparently factual matters stated in the report. Presumably reports in that form are appropriate and accepted within the field of anthropology".*

In fact, the two specific problems identified by Selway J are the direct result of social anthropology's discursive evolution since its 'postmodern turn' during the 1980s and 1990s. The issue of explicit advocacy is a corollary of anthropologists' ongoing self-perception as civil society representatives and 'defenders' of Indigenous Australians. This was not a new phenomenon of the late 20th century, having begun with the very first ethnographic reports by missionaries in the 1850s[4]. But when combined with the new journalistic style of 'thick description' (Geertz 1973 - discussed below), unconstrained by a formal language aspiring to scientific status, what had previously been a staid ethical obligation not dissimilar to an implicit Hippocratic oath, now became an explicitly ideological undertaking. This view had not been a problem for anthropologists engaged in the land rights regimes of the late 1970s and 1980s for two reasons. Firstly, the sense of camaraderie that was engendered by working for newly formed Indigenous-controlled land councils proved too heady an atmosphere for a new generation of young anthropologists trained during the conservative 1960s and early 1970s, and many treated their work as a revolutionary cause rather than a professional duty. Secondly, the relatively minimal probative demands of the commission-based land claim assessment process in the NT and elsewhere, meant that there was little requirement for the kind of elaborated explanations that might have highlighted bias in the claim books written by anthropologists. The net effect was that anthropologists' ideological orientations were rarely revealed in land claim reports, leaving them unchallenged. Operating under the combined influence of a discursive theme that

[4] See e.g., the ethnography of Reverend Lancelot Threlkend at Lake Macquarie in eastern NSW, and that of Reverend William Ridley in southern QLD and in northern NSW. Ridley's advocacy is described in Chapter 4.

encouraged ideological commitment, and a professional environment that accommodated it, anthropological advocacy blossomed to detriment of professional capacity.

The second issue identified by Selway J - a failure to distinguish between fact and opinion, combined with a failure to demonstrate any systematic line of reasoning linking one to the other - is similarly linked to the evolutionary conditions of social anthropology's past. Although weak, the formalism that emerged in social anthropological discourse between its inception in the 1870s and the 1970s, was nevertheless grounded in an assumption that the procedural logic for describing human social culture was commensurate with the procedural logics of other fields of inquiry, based on making systematic observations of measurable phenomena, from which theoretical models could be derived. The only dimension in which early social anthropology differed from already established scientific disciplines was the mode by which it constructed theoretical models. The fields of physics, chemistry, and biology exhibited clearly delineated methods for conducting measurements, from which experts derived increasingly specific terms and definitions and increasingly general principles, both of which could be tested. By contrast, social anthropology remained anchored to anecdotal second-hand elicitations and observations, which were assembled into ad hoc models that either validated or contradicted existing models but rarely sought to methodically resolve any variation between them. A pervasive lack of rigour in both methodology and analysis meant that neither data nor theoretical models could be reliably tested, allowing bad data and inaccurate models to permeate anthropology much more than they did other scientific fields.

In the author's view there were two reasons for this. The main reason was not any inherent unsuitability to measurement on the part of human social cultures, but rather an unanticipated complexity in those systems, coupled with a failure on the part of social anthropology to adopt technologies necessary to detect that complexity. From the 1870s to the 1970s social anthropology maintained a dogmatically simplistic attitude towards the bases for the phenomena that its practitioners observed in the field. The second reason that bad data and inaccurate models disproportionately affected social anthropology, was that whereas physicists, chemists, and biologists freely acknowledged their dependence on continually improving metric technologies, social anthropologists have tended to rely almost exclusively on long-form written records without any use of mathematics or other formal methods. The result was that social anthropologists have often been limited to making only the most immediately obvious observations, making only those analyses that they can intuit in the absence of any technological assistance. In 2005, when Selway J made the criticisms quoted above, many social anthropologists remained constrained to notebooks, cameras, and audio recorders, with the possible exception of personal GPS for the recording of geographic site coordinates. This is still the case today (Martin *et al* 2013:2058). The parabolic rise of cheap, powerful computing hardware and largely free, sophisticated software, has had minimal impact on

either field methods, or the analyses that are applied to any resultant data[5], leaving elaborate and complex phenomena to be guessed at but never demonstrated. Like the aforementioned advocacy, Australian social anthropologists' retrograde attitude towards technology also acquired a strong ideological justification during the 1970s and 1980s, on the basis that improved technologies were 'hegemonic' and incompatible with the journalistic prescriptions of the thick description methodology. Although this was a specialised critique, it was nested inside a broader mistrust of science. The net effect of this history was a convergence between a recently ingrained antipathy to formal modelling and analysis, and historical misunderstanding of the way scientific theories logically accommodate increasing complexity.

Just as Blackburn J's decision had included the first test of Australian social anthropology's expert status, Selway J's decision included possibly the first concise acknowledgement that what was acceptable *within* the field of social anthropology, was not acceptable *outside* it. The Blue Mud Bay decision was one of a slew of native title decisions made between 2002 and 2006, in which the court was similarly critical of social anthropologists' evidence, so much so that in several instances native title was found not to have been demonstrated, and in one case, lodged on behalf of Western Desert claimants, the Judge stated that he believed sufficient evidence probably existed but that it had not been properly compiled or presented by the anthropologist contracted by the caimants[6]. The responses of practicing forensic experts to these outside assessments of their discipline have been generally defensive. Monographs such as Burke (2011), edited volumes such as Bauman (2010), and myriad journal articles such as Sansom (2007), Moreton (2010), and Palmer (2011), among others[7] have taken this line. With the exception of a small number, such as Trigger (2011 *inter alia*), these responses tend to attribute responsibility for anthropology's poor performance to the judicial system rather than to their own theory and methodology. The logic of this reaction tends to hinge on relativising the judicial system such that, as outlined above, it is construed as 'just another cultural institution' over which anthropology retains a superior moral and intellectual expertise.

Although this attitude reveals an interesting window on a tension between sections of academia and the judicial system in Australia, it is not the object of the current thesis. The broader focus here is the self-determined formulation of social anthropological discourse. As Blackburn J makes clear, the judicial distinction between 'facts' and 'opinions' are isomorphic with the scientific distinction

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[5] In the wide-ranging edited volume "Dilemmas in Native Title Anthropology" (2010) the only mention of computing was in relation to the compilation of spreadsheets, despite contributions from 8 senior social anthropologists. Other important recent volumes such as Burke (2011) and Trigger *et al* (2012) make no mention of technology or computing at all.

[6] Jango 2006.

[7] Criticism by academic anthropologists outside of forensic proceedings has been especially vociferous in their defence (see Responses to Trigger 2011).

between 'observations' and 'hypotheses'. Contemporary Australian social anthropologists have generally been unable or unwilling to make the distinction. The most common defence on the part of social anthropologists is a philosophical opposition to the existence of 'facts', which many regard as an imposition of a dominant system of perception onto another, subordinate system of perception. As will be elaborated in the following chapter, this attitude is now moribund because, as the judicial system in Australia has revealed, it routinely fails to provide any substantive causal explanations for the observable phenomena in which it claims expertise, and in so doing further diminishes the potential benefits that disadvantaged people may derive from the pursuit of their rights under the law.

The position forming the basis for the current thesis is consistent with that of Blackburn J, in disavowing any argument that social anthropology can or should be exempt from conciliation with other expert scientific fields. If social anthropologists truly wish to be accepted as experts in the subject of human social culture, we need to reconcile ourselves to the constraints of scientific discipline on broadly accepted expert fields such as chemistry and medicine. To do so, social anthropological discourse needs to be based on observations and theories isomorphic with demonstrable facts and testable opinion. Observations and theories must be linked by a line of reasoning that is consistent with an inductive logic relevant to the field of inquiry, rather a deductive logic derived from philosophical reasoning. In the study of human social culture, this logic must incorporate systematic collation of both the theories that populations maintain about their own activity, as well as the measurable instantiations of that activity apparent to outside observers. This cross-referencing of 'hearsay' and observation is not unique to social anthropology, but is accepted and respected across the fields of expertise in which humans are the subject, including medicine as Blackburn J point out, but also demography as noted above. The conditions now exist in which social anthropology may finally reach consilience with the community of expert fields to which it belongs. This will entail a transition from ad hoc, inconsistent philosophising, to a fact-based, testable science based on the systematic collection and collation of data, and the development of self-consistent theoretical models

The author has practiced as a forensic social anthropologist in the field of native title for more than a decade, working primarily with Indigenous communities in the western part of the state of New South Wales, but also in northern Victoria, southern Queensland, northern South Australia, and throughout the Northern Territory. During this time he has been employed primarily by an Indigenous-controlled non-governmental organization based in Redfern, Sydney[8]. In this capacity the author has provided independent expert advice in legal proceedings on behalf of Indigenous parties seeking recognition of traditional land and resource tenure in Australia's Federal Court on numerous occasions. At the time

[8] Originally named 'New South Wales Native Title Services Ltd' (2001-2009), and later simply 'NTSCORP Ltd' (2009-present).

of the thesis' submission the author had worked on a total of ten such claims, distributed across New South Wales and totalling more than 30,000 square kilometres, equal to approximately 40% of the landmass of New South Wales, or 4% of the Australian continent. The associated research involved interviews with more than 1000 traditional owners from more than 40 towns across South East Australia, and resulted in the identification of more than 80,000, primarily Indigenous individuals who have lived during the 228 since the British colonisation of Australia. These were the conditions under which it became apparent that a new research technique was necessary to comprehensively and accurately describe the population dynamics that give rise to distinctive Indigenous populations in Australia, and the quantifiable and probative basis of their ownership Australian natural resources.

0.2 Goal & Application

This thesis aims to describe and demonstrate a new research technique for modelling the intersection between autochthonous theories of sociality, and large, complex social structures that can be empirically demonstrated. As outlined above, the contemporary language of social anthropology struggles with a longstanding misunderstanding of the scientific method and its explanatory power. As further explained above, this misunderstanding has been used to justify the rejection of mathematical and associated technological augmentation in anthropological modelling and analysis (see e.g. Geertz 1973:12). The overall consequence has been a diminution of anthropology's usefulness as an explanatory tool and therewith, a diminished capacity to contribute to the advancement of related administrative and expert fields, such as law and demography. While the *context* for the current thesis' phylogenesis was its intersection with the administrative field of native title law, its *goal* is to contribute a methodological technique for intersection with a range of other fields, primarily demography. In developing a new research technique directly relevant to social anthropology's core subject matter, the current thesis also aims to make its applications and findings useful to other fields where human social culture is also a major concern, such as education, healthcare, and economics, among others. However, as these fields all comprise specific branches of more general areas of inquiry, the nexus is sought at a more general level, namely at biology, psychology, and semiotics.

While the broad collection of techniques known as social network analysis have been fully integrated among various scientific fields since the 1990s (Borgatti *et al* 2009), *kinship* network analysis is yet to find applications outside of social anthropology. As proposed in the current thesis, this is the result of a general inability on the part of both the social sciences and natural sciences to reconcile an integrated model of human existence and behaviour. On the one hand biology construes an exclusively genetic and evolutionary basis for this existence, while on the other hand the social sciences construe it on an exclusively semiotic basis. As discussed below, this lack of consilience stems from the discrete languages of physical and social science. While biological frameworks are sufficiently specific to model heredity and neurological processes, they are not equipped with the terms necessary to model community level kinship processes, such as patterns of spousal selection and child-raising. This scenario tends to reinforce organism-level explanations for what are actually social phenomena, because the inductive logic of scientific methodology is limited to building models from observable patterns, where those observations are limited to the available terminology. On the other hand, while social science frameworks are sufficiently specific to model personal ideology, they lack the empiricism and formalism to model patterns of choice beyond small numbers of individuals. The consequent ambiguity accompanying *de*ductive explanations then fails to demonstrate the existence of real patterns, either in ideology or relevant choice-making behaviour. Thus social anthropology fails to furnish functional explanations to other fields that stand to benefit from its findings.

Kinship network analysis partly bridges this gap. Although social scientific in subject matter, kinship network analysis is a research methodology that is consistent with the natural sciences. This is because it is an *in*ductive rather than *de*ductive technique, designed to count and classify real-life individuals, and the real-life relationships between those individuals, according to the accounts given by those people themselves, or according to observable events, or ideally both at once. This is because most human beings tend to have either or both a mother and a father*, however so defined*. The three relationships that bind together a child, a woman, and a man are recognised by kin terms that are universally known and used. This is not equivalent to the Western folk model of kinship known as 'genealogy', which states that there is a universal system of kin terms equivalent to the biological relations between a child and the woman and man whose intersecting reproductive potentials catalyse their birth. This conflation of a biological model with a kinship model is in fact a Western cultural construct, arising from the traditional legal implications of pre-scientific European kinship blended with late 19th century scientific discovery. One of the most incisive illustrations of traditional social anthropology's deviation from natural scientific language, is its historic inability to make this distinction, and on having realised its failure during the 1970s, to dismiss the entire field of kinship research as a synthetic imposition on arbitrary categories (Schneider 1984)[9].

Because the language of social anthropology is 'pre-paradigmatic' (Kuhn 2012[1962] - discussed below), it has been unable to resolve the concept of biological universality with the concept of cultural diversity. Since the 1970s postmodern turn, described above, this appears to have resulted in the demotion by social anthropology of the concept of universality altogether, as a one more metanarrative of 20th century Western philosophy within a lineage of such disposable perspectives. As Leaf & Read (2012) point out however, identifying universality in the human *use* of kinship theories is not equivalent to identifying universality in the theories themselves. The instance of a child referring to a woman by the English kin term 'mother' implies a biological link between them but does not instantiate it. This is because kinship theories encode social significance, not biological fact. In a 21st century Western community it is likely that the term 'mother' is *indicative* of a biological link between woman and child, however it is also likely to be indicative of a union between that woman and a man whom the child calls 'father', even if the woman has no biological link to the child at all. A hypothetical observer, who happened to be a parsimonious bearer of Western culture, may insist that the correct term for the child to use is 'step-mother', but would nevertheless concede that the child is more likely to use the term 'mother' or 'mum'. The use of the kin term 'aunt' is even more illustrative of the point, since a person addressed by such a term in a 21st Western community is *equally* likely to be biologically linked to the child as not. This is because it is equally likely that an 'aunt' is called 'sister' by an adult whom the child calls 'mother' or 'father, just as it is likely that the same person is

[9] Schneider (1984) argues that the very notion of 'kinship' is a concoction of anthropological theory without any corollary in real life, and that it should be 'abandoned by the discipline' (White & Houseman 2001; Read 2001).

called 'wife' by a person who in turn is called 'brother' by a person who in turn is called 'mother or 'father' by the child. Whereas the former 'aunt' is more likely to be biologically related to the child, the latter 'aunt' is less likely so. This variable convergence between biological definitions and kinship definitions is a coincidence of the history of Western kinship and the history of science. It has nothing to with any 'inherent' biological status of European kin terms. Kinship network analysis is the first formally defined technique, based on a coherent, self-consistent grammar that can accommodate theoretical variability among terminologies while also building elaborate models of dependent decision-making among large numbers of people. This is because it can be used to build quantitative population models according to whatever kinship definitions are relevant. In light of the demonstrable efficacy of KNA, the argument that kinship terminologies cannot be reconciled with a single logic fails on the test of consistency. If a population model is built using the definitions of the people it describes, then it is consistent with a single system, and comprises a kinship analysis.

The technique of kinship network analysis illustrates how theoretical models, when implemented in a relatively consistent manner by members of a population, result in network structures exhibiting independently observable and measurable patterning. This patterning avails to predictive modelling future network growth or contraction, together with local-level phenomena deriving from that network structure, such as options for relationship-formation within the network. To render a familiar introductory example: A population whose kinship terminology renders the terms 'mother' and 'aunt' interchangeable can be predicted to incorporate a significant number of children raised by women to whom either their mother or father refers to as 'sister', or to whom their 'uncle' is married. By extension, children who are raised collectively to refer to each other as 'brother' and 'sister' are more likely to refer to the same group of adults as 'mother' and 'father', even if there are more than two such people, and even if some of the children alternately refer to some of these people as 'uncle' and 'aunt'. This has flow-on effects upon patterns of residency, employment, migration, spousal selection, child-raising, and so on, where individual choices are guided by the twin factors of kinship proximity (how 'closely' a given kin term enacts a given relation), and corollary behavioural prescriptions (such as supporting one's parents in old age). These are phenomena that demography currently attempts to model using statistical methods devoid of relational properties. Such methods quantify and visualise trends but leave analysts to speculate as to relational causes and predictions. Kinship network analysis bridges this gap by documenting relationships between each countable entity - both individual people and personal attributes - and providing methods for visually illustrating patterns in those relationships.

The new technique of spatiotemporal kinship network analysis, presented in the latter part of this thesis, extends the functionality of kinship network analysis by providing a means to incorporate measurement of both historical time and geographic space into the available measurement of kinship relations. This is very different to existing attribute-measurement in either KNA or SNA, because time

and space have their own metric values, independent of the people and relationships that form, collapse, and reform within them.

A kinship network model is limited to the generative definitions that link its vertices together. In a 21st Century Western kinship network, a child is linked to the adult it calls 'mother' by a specific relationship type, and to the adult it calls 'father' by another specific relationship type. These two adults are linked to each other by a third type of relationship. A single network of all bearers of Western kinship culture can be constructed exclusively from these three types of direct or 'generative' relationships. In such a network, measurable attributes include those classificatory characteristics that can be elicited from, or observed among network members. These include 'types' of person, and 'types' of indirect relationship built from chains of direct generative relations. Time and space however, cannot be modelled in this way. Because of the clear relevance of time to kinship network growth (e.g. the requirement that a child be born *after* its parents), time is already incorporated into conventional network modelling techniques. However the manner by which it is modelled is not reflective of the way in which network members experience it. Rather than modelling time as a constant flow, conventional kinship network analysis models it in terms of 'classes' of attribute known as 'generations'. This definition allocates network members to slabs of time according to the order in which they are born relative to other network members. Members of kinship networks are thus divided into classes according the *longest* ancestral lineage in the network. If the longest lineage is 3 (i.e. grandparent, parent, child), then all members of the network will be divided into three common generations. Space is treated in the same way. In this model space is not modelled as a geographic plane - as experienced by living people - but as an abstract set of classes into which network members are allocated. In this approach, members of kinship networks are divided into classes according to the maximum *number* of lineages in the network. If there are five separate lineages (irrespective of whether they all branch from one ancestor), then all members will be divided into five locations.

By complete contrast the *spatiotmporal* KNA technique presented in this thesis treats space-time as a normalised three-dimensional space within which each network member and each relationship is located proportionally to one another. This means that the oldest date recorded in association with a network member becomes the upper limit in which the network is graphed, while the most recent date becomes the lower limit. All intervening dates recorded in association with remaining network members are located proportionally within this graphic span. Similarly, the most distant geographic sites recorded in association with network members become the outer limit of the network graph, while all remaining proximate sites are located between these outer limits. Whereas points in time are arranged along a single ordinal axis ranging from oldest to latest date in the network, geographic space is measured with *co*ordinates ranging along *two* axes (nominally north-south and east-west), producing a plane. The combination of the single axis of time with the two axes of space creates a three-dimensional vector space in which all vertices and relations are located. This arrangement

reveals the real-world morphology of a network as it alternately expands, contracts, and variously changes shape over time. The value of stKNA to evidence-based expert fields lies in its scope for detecting causal links between kinship phenomena and other social variables, where both time flow and geography are important. This is useful wherever kinship is a plausible factor in an emergent social dynamic, and there is a benefit in tracing the origins and likely trajectories of that dynamic.

0.3 Strategy & Execution

This thesis is structured to orient the reader towards its goal in a stepwise fashion. It does this in 5 stages distributed across 5 chapters, laying out the context, terms, and definitions of both pre-crisis and crisis era anthropology, its specific characteristics in Australia, and finally those of a reformed discipline capable of accommodating formal empirical techniques such as social network analysis, kinship network analysis, and spatiotemporal kinship network analysis.

Chapter 1 introduces the discipline of social anthropology in these terms, outlining the parameters of the discipline in relation to adjacent fields, along with its development and its current unsustainability. Leading experts in the fields of biology, psychology, and semiotics all provide micro-level definitions of social culture that integrate into a single, generalised macro-level framework, but also note that social science broadly, and social anthropology in particular, do not. Integration is achieved by defining the parameters of each field, at which they give way to adjoining fields. The current thesis proposes, in line with the evidence furnished by these other disciplines, that social anthropology *should* provide a theoretical model for explaining how human beings' abstract models of their own social behaviour is used, both to understand and guide that behaviour. This proposition is as old as anthropology itself. What is new however, is 1) the methodology for demonstrating, and the theory for explaining, the occurrence of this process in the real world, and; 2) an explanation of how this theory and methodology interface with the scientific disciplines listed above. Chapter 1 concludes by discussing the current calls from Australian demographers for social anthropology to develop more effective means for contextualising statistically significant patterns of fertility, migration, and mortality, which clearly vary among subsets of the global population, but in particular within Australia with regard to deleterious inequalities between Indigenous and non-Indigenous communities. Chapter 1 thus works through three levels of contextual setting, from 1) anthropology's location within a vertically integrated chain of disciplines dealing with sociality, to; 2) the empirical formal framework for a reformed social anthropological language, and finally to; 3) its application to the demography of Indigenous/non-Indigenous population dynamics.

Chapter 2 traces the emergence and consolidation of key assumptions in social anthropological theory, which conform broadly to tacit mathematical axioms of group theory. The chapter commences with an explanation of the effects that these assumptions have on construals of sociality and causality in social anthropological models, most importantly the deductive proposition that sets of people are inherently 'closed off' from one another into classifiable groups. Attempts to render this closure congruent with the complexity of real human interaction, using rhetorical devices such as the term 'intersection', or by the thematic re-organization of ancillary temporal and spatial ordering regimes, such as in historicist and structuralist models, only serve to entrench underlying group theoretic assumptions, and to disguise their methodological inadequacies. Chapter 2 illustrates the serial reorganization but ultimate maintenance of these assumptions throughout anthropology's pre-crisis and crisis eras. The chapter concludes by tracing the apparent subsumption of the discipline's

more formal pre-crisis language within a proliferation of competing, so-called 'inter-disciplinary' metaphors, which have become particularly acute in the Australian branch of the discipline.

Chapter 3 focuses more specifically on the effect of these tacit group theoretic assumptions on Australian social anthropology, which has produced a peculiar model of Indigenous Australian sociality that emerged under the auspices of the terms 'law and custom'. This has given rise to a collection of surprisingly persistent tropes regarding sociality and causality in Indigenous Australian cultures, called here the 'KLRE model' in reference to its key deduction, that Australian sociality is prescribed by four mutually conforming institutions: kinship, language, religion, and economy. The chapter traces the origin of the KLRE model in the tacit group theory of anthropology's late 19th century founders, Lewis Henry Morgan and Edward Burnett Tylor. Chapter 3 pursues this developmental trajectory through Morgan's keen student and correspondent, the colonist A.W. Howitt, Howitt's intellectual heir and founder of academic social anthropology in Australia, Radcliffe-Brown, Radcliffe-Brown's successor A.P. Elkin, and Elkin's students Ronald Berndt, Mervyn Meggitt, and Lester Hiatt. These students, together with their senior colleague W.E.H. Stanner formed what came to be known as the 'Sydney School' at the University of Sydney. This loose academic community had a significant and lasting influence on the orientation of Australian anthropology across the continent, including the legal views that guided the formation of the 1971 Northern Territory Land Rights Act, and 1993 Commonwealth Native Title Act. Chapter 3 concludes with an account of the Sydney School's hostility towards luminary European anthropologist Claude Lévi-Strauss in the late 1960s, and the consequent divergence of Australian anthropology from its European theoretical tradition.

Chapter 4 presents the formal terms and definitions of social network analysis as an alternative to the tacit group theoretic framework of the KLRE model, together with an empirical application of kinship network analysis to the Indigenous population of the Central Murray Darling Basin South East Australia. This exercise serves two purposes. The first is to provide an introduction to the core technical principles underlying the stKNA technique introduced subsequently in Chapter 5. The second purpose is to illustrate that there are distinctive patterns of sociality in the Central Murray Darling Basin population that have remained undetected for more than 150 years of social anthropological research with the corresponding population, and which undermine core group theoretic assumptions about societal closure in the KLRE model of Indigenous Australian sociality. After first laying out the more empirical, formal, and congruent ways in which SNA and KNA construe sociality and causality, Chapter 4 focuses on the field data of early social anthropologists Williams Ridley, and R.H. Mathews. These English colonists conducted independent firsthand interviews with members of the Central Murray Darling Basin population between the 1850s and 1890s, resulting in relatively sophisticated data on theories of kinship autochthonous to that population. The quality and detail of this data remains unsurpassed to the present day, making it eminently suited to kinship network analysis. Finally, the Chapter 4 compares the results of this analysis with a further analysis of

real kinship network data for the same population in a test for isomorphism between the patterns evident in autochthonous kinship theory, and patterns evident in the structure of the population to whom the theory belongs.

Finally, Chapter 5 extends the recently established technique of kinship network analysis by augmenting it with a new suite of spatiotemporal metrics. This form of measurement and analysis allows the already well-defined terms and definitions of kinship network analysis to be expanded into the realm of historical time and geographic space, and forms the primary original contribution of the current thesis. Chapter 5 introduces stKNA as the convergence of a set of geographical principles known as 'time-space geography' (Hägerstrand 1970), and kinship network analysis. The new technique is demonstrated on the same Central Murray Darling Basin population data treated in Chapter 4, in order to emphasise the natural extension of SNA furnished by stKNA. Chapter 5 begins by outlining existing methods for modelling space and time within social network analysis generally, and KNA specifically, and illustrating why these are insufficient for explaining the influence of space-time on population dynamics, in relation to both kinship and non-kinship populations. The chapter concludes by introducing a set of formal methods for packaging and inserting spatiotemporal metrics into KNA graphs, such that kinship networks can be modelled as 3-dimensioanl structures in real geographic space and historical time.

Chapter 1

Sociality, Science, and Anthropology

1.0 Introduction

> *"In all forms of human thought and activity, one cannot ask questions of nature or of origin before having identified and analyzed the phenomena and having discovered to what extent the relations which unite them suffice to explain them. It is impossible to discuss an object, to reconstruct the history which gave it its being, without knowing first what it is; in other words, without having exhausted the inventory of its internal determinations" (Lévi-Strauss 1966a:113).*
>
> *"The empirical status of formal analysis is a fundamental conceptual problem for philosophy and a basic methodological problem for science. The problem is especially crucial in the effort to establish a fully empirical version of anthropology (Leaf 2007:2).*

As outlined in the introduction, this thesis presents stKNA as a technique for detecting and modelling the spatial and temporal distribution of kinship-based populations. The technique falls within the remit of an empirical and formal language that is largely foreign to contemporary Australian social anthropology. In order to explain the potential role of stKNA in building a consilience between the so-called natural sciences and social sciences, the latter of which social anthropology is a part, the basis and origin of this alienation needs to be elaborated. The current chapter introduces the context, terms, and definitions of an emerging expert language that forms the theoretical and methodological basis for the rest of the thesis, and which has been called 'anthropology on a new plane' (Leaf & Read 2012). This approach attempts to bring together the three general topics of investigation in which anthropology is supposed to bear expertise: Collective human thought, collective human activity, and collective relations by which the two systematically influence each other. Together, this system of feedbacks is what is known as 'human culture' (White & Johansen 2005:9).

Historically, anthropological texts have tended to vacillate between prioritizing either human thought *or* human action, without a consistently formal framework by which to define either, much less a framework for identifying, explaining, and measuring the influence of one upon the other. The 'third' topic - the mechanism by which thought and action each gives rise to the other - has been most poorly defined, frequently lost from view as new theoretical perspectives and idioms refocus researchers' attention, first away from thought to action, and the back to thought again.

Since its inception in the 1870s, the discipline of social anthropology, with its various idioms of 'historicism', 'structuralism', 'functionalism', 'phenomenology', 'action theory', and so on, has been repeatedly defined and redefined by 'borrowings' from more clearly formalised and functional disciplines among both the natural and social sciences. Between the 1870s and 1910s the source disciplines were history and biology. From the 1910s to 1970s the sources were physics, engineering, and economics. From the 1970s to the present it has been psychology, semiotics, and literature. As elaborated below, this pattern has been identified by a number of observers within other disciplines, including semiotics, biology, and physics, among others.

At each new discursive 'turn' in the discourse, as each successive set of terms and definitions are cast off by a new generation of anthropologists switching their attention to some newly specified language, the claim is often made that a new 'paradigm' has been discovered, that the previous paradigm was a flawed mistake, and that the new one will reveal a clearer and more incisive account of how and why communities of people think and act in the apparently distinctive ways that they do (Appadurai 1988; Gupta & Ferguson 1992; Sahlins 2008 - discussed in the next chapter). However, at no point in the paste 150 years of anthropology's development, have any of newly borrowed perspectives on human thought and action been integrated into a systemic model of culture. This is no historical accident, but is rather an inherent feature of social scientific discourse, which the pedagogic sociologist Basil Bernstein has called its 'serial code', as elaborated in Section 1.2 below (Bernstein 1996; 1999).

A notable anomaly in this historical pattern of oscillation was the moment at which anthropology succumbed to what some have called its 'crisis' - the distillation of a broader reorganization of the social sciences (Clifford & Marcus 1986; Harvey 1990; Marcus & Fischer 1986). From the 1870s to the 1970s, throughout the cyclical introduction and dismissal of various idioms, the parameters of anthropology had accreted a degree of consistency. This is what allowed Lévi-Strauss to say in 1966, as cited above, that anthropology was the study of three fundamentally linked phenomena, which demarcated its field of expertise as distinct from that of other social scientific disciplines, and from natural science more broadly. Almost immediately afterwards however, the discipline submitted to an historically induced crisis of confidence, which saw its previously formal parameters disassemble into increasingly ad hoc and metaphorical speculation. For the purposes of the current thesis, this period of anthropological development, which extends into the present day, is referred to as anthropology's protracted 'crisis era'. It is described as historically induced in the sense that it was brought about by major changes material conditions external to the discipline, particularly the emergence of new research technologies.

Although new to the social sciences, historically induced crises are accepted as a normal part of discursive evolution in the natural sciences (Kuhn 2012[1962]). For each discipline of natural science, such as physics, chemistry, and biology, such normalisation depends upon the pre-existence of a broadly self-consistent and generalised theoretical framework, through which a given disciplinary language functions to a) *describe*, and b) *explain the causes of*, the phenomena in which it claims expertise, and which allows that language to develop successively more precise descriptions and explanations over time (Kuhn 2012[1962]). Such a framework fits the technically accurate definition of a paradigm. While it seems reasonable to suggest that in the late 1960s anthropology was approaching the development of just such a framework, the discipline's leaders did not manage to complete the transition prior to the onset of the crisis era. This has led external observers in the natural sciences to refer to social science as 'pre-paradigmatic' (Leaf & Read 2012; Kuhn 2012[1962]; Wilson 2001; Wilson & Wilson 2007; Richerson & Boyd 2005*)*. While unproblematic for the many

contemporary anthropologists who openly eschew science as an allegedly hegemonic construct of late Western philosophy (e.g. Marcus & Fischer 1986; Scheper-Hughes 1995), other external observers view anthropology's lack of a paradigm as more than a simple problem of technical acuity, but rather as an ethical obstacle to the universal recognition of human equality. The biologist E.O. WIlson puts it this way:

> *"Social scientists, like medical scientists, have a vast store of factual information and an arsenal of sophisticated statistical techniques for its analysis. They are intellectually capable. ... Still, it is obvious to even casual inspection that the efforts of social scientists are snarled by disunity and a failure of vision. And the reasons for the confusion are becoming increasingly clear. Social scientists by and large spurn the idea of the hierarchical ordering of knowledge that unites and drives the natural sciences. Split into independent cadres, they stress precision in words within their specialty but seldom speak the same technical language from one specialty to the next. ...*
>
> *"The social sciences are hampered in this regard by the residue of strong historical precedent. Ignorance of the natural sciences by design ... [T]heorists [have been] inhibited from probing in that direction by another problem endemic to the social sciences: political ideology. ... Franz Boas [a leading anthropologist of the 1900s-1930s], aided by his famous students Ruth Benedict and Margaret Mead [during the 1930s-1960s], led a crusade against what they perceived (correctly) to be the eugenics and racism implicit in Social Darwinism. With caution swept aside by moral zeal, they turned opposition into the new ideology of cultural relativism, ... [b]elieving it a virtue to declare that all cultures are equal but in different ways ...*
>
> *"Where cultural relativism had been initiated to negate belief in hereditary behavioral differences among ethnic groups - undeniably an unproven and ideologically dangerous conception - it was then turned against the idea of a unified human nature grounded in heredity. A great conundrum of the human condition was created: If neither culture nor a hereditary human nature, what unites humanity? The question cannot be just left hanging, for if ethical standards are moulded by culture, and cultures are endlessly diverse and equivalent, what disqualifies theocracy, for example, or colonialism? Or child labor, torture, and slavery?" (Wilson 2001:199-201)*

From Wilson's perspective the segregation of natural and social science is unjustifiable on *both* ethical and intellectual grounds. As confirmed by Bernstein (1996; 1999), Wilson locates the origins for this segregation in the social context of social science's foundation and reproduction within academia, where new idiomatic languages vie for *supremacy within* the discipline, rather than explanatory *integration of* the discipline. However, unlike Bernstein, who portrays this inequality as an intractable feature of Western pedagogy (see Section 1.2 below), Wilson advocates for what he calls 'consilience' between the natural and social sciences:

> *"[T]he social sciences are intrinsically compatible with the natural sciences. The two great branches of learning will benefit to the extent that their modes of causal explanation are made consistent. The first step in the approach to consilience is to recognize that while the social sciences are truly science, when pursued descriptively and analytically, social theory is not yet true theory. The social sciences possess the same general traits as the natural sciences in the early, natural-history or mostly descriptive period of their historical development. From a rich data base they have ordered and classified social phenomena. They have discovered unsuspected patterns of communal behavior and successfully traced interactions of history and cultural evolution. But they have not yet crafted a web of causal explanation that successfully cuts down through the levels of organization from society to mind and brain. Failing to probe this far, they lack what can be called a true scientific theory. Consequently, even though they often speak of 'theory' and, moreover, address the same species and the same level of organization, they remain disunited (Wilson 2001:205).*

From the perspective of contributors to anthropological knowledge, and experienced readers and speakers of its idioms, Leaf & Read (2012*)* go further than Wilson, and argue that for social science generally, and for anthropology in particular, the absence of a paradigm requires the complete reorganization of how human sociality is defined by social anthropology. Referring to Kuhn's (2012[1962]) assessment of the social sciences as 'pre-paradigmatic', they observe:

> *"Of course, saying 'pre-paradigmatic' rather than simply 'non-paradigmatic' implies a judgment about the future. There are indeed anthropologists who believe that anthropology will one day engage in scientific practice as sound, powerful, and widely accepted as those in the physical sciences. But there are others who say this never can be, and indeed never should be. We are among those that say that we [the discipline of social anthropology] are a science - and not just at some point in an unknown future. It can be done now.*
>
> *...*
>
> *"Human beings have two outstanding characteristics compared to all other species: the apparently enormous elaboration of our thought through language and symbolism and the elaboration of our forms of social organization. ... [Thought and social organization] are, however, two sides of one problem: we cannot understand what social organization is without recognizing that it is an ongoing construction involving thought, and we cannot understand what thought is without recognizing that its foundation and primary functions lie in developing and implementing social organization" (Leaf & Read 2012:16-17).*

Leaf and Read go on to propose what they call a 'new science' with an argument that bears quoting at length, given the gravity of the claim:

> *"[For natural science] observation and theory only come together ... through instantiation of the theory with observations ... This framework for relating observation and theory does not change in the paradigm shift between [branches of individual scientific disciplines]. The paradigm shift keeps intact the foundational level stipulating how systems of ideas and systems of observation are to be related.*
>
> *"When we say what is described here is a new science, we mean two things. Concerning ethnology, where there seems to be general agreement that there is yet no established paradigm, we mean that we are providing one. Regarding the established sciences such as biology where there are established paradigms, we mean that none of them are adequate for the task at hand. Social organizations have distinctive properties and require distinctive explanations of a sort that cannot be reduced to the explanations drawn from or grounded in any other science" (Leaf & Read 2012:20-21).*

While Leaf & Read provide a general yet formally defined set of terms and definitions, which they refer to as an 'empirical-formal' framework for social anthropology (see especially Leaf 2007; Read 2008), their primary objects of study are autochthones theories of social organization, rather than real-world instance. In contrast, anthropologist and complexity theorist D.R. White (2011) provides a more specific, and functional techniques for detecting and measuring social organization as it is instantiated in objectively measurable social interaction. He describes the nexus between this independently observable instantiation and the autochthonous theories of social organization, described by Leaf and Read, in terms of a 'three world problem' comprised of 'physical/behavioural', 'subjective mental', and 'objective cultural' worlds. White further proposes that that this nexus is most congruently describable in terms in objectively measurable networks of interaction spanning variable temporal scales:

> *"The topics of ... Read (2008) and Leaf (2007, 2008) on formal empirical models, confront the question of how these three worlds are related. How is it possible for 'objective' culture to take on a distinct and durable existence? Anthropology currently wrestles with apparently incommensurate dualities in the interfaces between brain as a physical organ and the mind as a nonmaterial dynamical organized response pattern mediating the organism-environment, ego-alter, and other interfaces. ...*
>
> *"The sciences today are undergoing major transformations, rethinking and re-synthesis. They in turn are affected by transformations in physics, biology and ecological psychology in dealing with complex systems and, in particular, the dynamics of complex systems. ...*
>
> *An experimentally supported solution to the mind-body conundrum is that organism and environment constitute a single system. Events are bounded in perception by changes in action that have networks of connected parts within events and recurrence across events. ...*

> *Time-series of episodic events as experiences thus lend themselves perceptually to network coding and analysis. Such studies may be done at many different time scales. In our studies of kinship networks for example (see White and Johansen 2005 for an ethnographic example [discussed in the current thesis below]) there are intergenerational events such as marriage, childbirth, death, migration, and proximal interactions within the culturally recognized and individually perceived event boundaries and time-scales of event sequences. The network links among events and actors exhibit structural and dynamical patterns, including recurrences for which tools exist for studying complex dynamics" (White 2011:5-6).*

The current thesis follows the lead of both Leaf and Read (Leaf 2007; Read 2008; Leaf & Read 2012), and White and others (Hamberger, Houseman & White 2011;Houseman 1997; 2002; Houseman & White 1998, 2013; White & Denham 2008; White & Johansen 2005) in utilizing newly available computational technologies, and the newly available large-scale datasets that have been rendered by this technology, to implement the theories and methodologies advanced by these researchers, and to reach for the ethically-motivated disciplinary consilience advocated by Wilson.

In order to make this contribution, the current chapter provides a context of the theoretic and methodological conditions of anthropology outlined above. This addresses three broad features, including: 1) a clarification of anthropology's parameters, as they define which aspects of human sociality anthropology is in a position to address; 2) a clarification of the inherently distinct research modalities of the natural sciences and social sciences, which differentiate the particular causes of human sociality, the causes of non-human sociality, and other forms of natural phenomena addressed by science, and; 3) a clarification of the reasons that anthropology was not able to establish a paradigm and achieve consilience with adjacent disciplines prior to the onset of its crisis era in the 1970s . Section 1.1 reviews the parameters set by anthropology's associated disciplines of biology, social psychology, and social semiotics. Section 1.2 explains the distinct and unequal manners in which natural science and social science make discoveries, and divergent developmental trajectories that resulted in this bifurcation. Section 1.3 provides a more specific overview of the development of social anthropology, and its early attempts at consilience with the natural sciences.

1.1 Consilience and the Parameters of Social Science

This section establishes the most general theoretical context within which the rest of the chapter is developed, by positioning social anthropology among adjacent disciplines dealing with human sociality, including biology, psychology, and semiotics. This involves outlining a coherent definition of the parameters of those disciplines from the perspectives of their leading practitioners. It also requires an acknowledgement of how these definitions contrast with the less coherent definitions advanced by some social anthropologists, in describing the parameters of their own discipline. As Kuhn (2012[1962]) points out, social science texts characteristically begin by restating theoretical assumptions, since there is typically little consensus around what those assumptions should be. This lack of consensus is a feature of what Bernstein (1996; 1999) calls the 'horizontal knowledge structure' of the social sciences and humanities, which accommodate multiple, competing, specialized languages within each discipline, and which in turn vie with one another for privileged status among expert speakers. In Bernstein's terms, each language carries its own unique idioms and perspectives, from which it casts a distinct thematic aspect upon the common discipline of human sociality. Rather than pick one or another language with which to associate itself, the current thesis withdraws to a broader vantage point by restating not the idiomatic assumptions of any single specific language, but by restating more general and fundamental assumptions about the science of sociality as a whole. Following Halliday (2001*)*, this thesis holds that each discipline of social science consists of a) a disciplinary subject, and; b) a collection of specialized languages for describing sociality, and the causes of that sociality. Following previously introduced examples provided by anthropologists such as Leaf & Read (2012), White (2011), and White & Johansen (2005), the current section elaborates a definition with a coherence approaching that of anthropology's adjacent disciplines.

In the terms provided by these authors, human sociality may be thought of as spanning a spectrum made up of overlapping regions, each of which is the subject of a distinct discipline with formally definable parameters (see Figure 1). This section describes the parameters of each of these disciplines in turn, explaining how each discipline provides the underlying explanations on which the other can be built. The objective of this exercise is to highlight the explanatory power and mutual functional benefit of 'consilience', as articulated by Wilson (2001) above. Such a consilience not only makes possible the development of a technical method such as stKNA, but also reconciles its logical position within social anthropology as a specialised language for modelling human sociality.

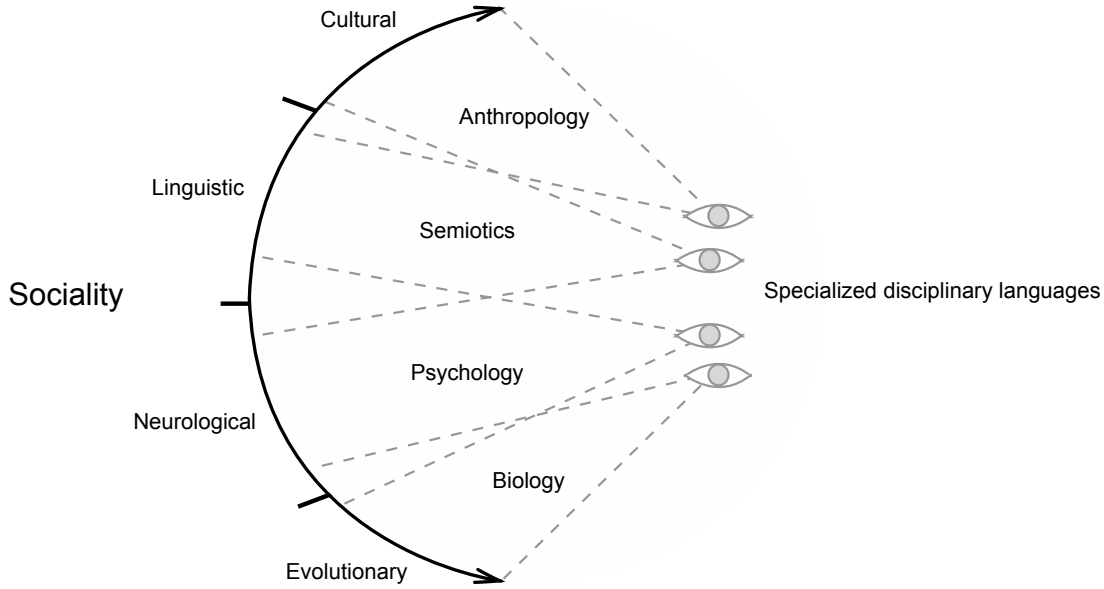


Figure 1: Overlapping regions of study among disciplines dealing with human sociality. These range from evolutionary advantage to variations in theoretical models among regional populations. These regions may be construed as cumulatively ordered along a spectrum. Each discipline is representative of subfields and closely related disciplines, e.g. sociology and human geography in relation to anthropology.

1.1.1 The Parameters of Social Anthropology

In contributing to the consilience of the new anthropology with the rest of scientific thought - that is, in establishing its parameters in relation to adjacent disciplines - it seems reasonable to locate the upper or more general limits of the discipline at biology - the physical limitations of humanity, the constraints of space-time on human physiology; and its lower or more specific limits at the discipline of semiotics - the construction of meaning, the plane on which humanity realises itself as a coherent system, occupying a region in space-time. Within these boundary definitions human beings exist at a nexus between their own sensations, and the specific environmental conditions (either natural or constructed) in which those sensations are able to function. Anthropological discourse typically makes these assumptions tacitly, which means that the definitions on which anthropologists depend are susceptible to inconsistency. The current section describes social anthropology's disciplinary parameters, drawing on the boundary definitions given by leaders in the respective adjacent fields of biology, psychology and semiotics.

At the level of biology, as distinct from other fields, it might reasonably be argued that human beings are like all organisms, comprised of an evolutionarily selected system of specialised cellular processes, drawing on nutritional inputs in order to sustain intergenerational reproduction. The dependence on nutritional inputs requires that the locations of individuals in space and time correspond with the locations of those inputs. At a more specific biological level, human beings' dependence on sexual exchange for reproduction requires that the locations of *multiple* individuals overlap with each other in space and time. Furthermore these two requirements are themselves interdependent, yielding the

phenomenon known as 'sociality' - the habitual and patterned cooperation of multiple individuals of a single species, readily observable when they take the form of communities, each of which comprises an integrated food-production and sexual-reproduction system. This area of inquiry is covered by the more specific sub discipline of sociobiology (Richerson & Boyd 2005; Wilson 2001; Wilson & Wilson 2007), discussed below at Section 1.1.2.

At the level of psychology, social interaction forms the ultimate context for, or 'constraint' on, the sub- and semiconscious interface between cognition, emotion, and behaviour (Greenfield & Keller 2004). In this field, sociality is defined by individual cognition of interaction as it gives rise to emotion, just prior to the conscious realisation of that emotion in meaningful terms. This 'pre-semiotic' dynamic is measurable in terms of changes in neurological state, marked by distinctive electrochemical signatures. These in turn trigger subconscious and semiconscious behavioural reactions below the level of semiotic realisation (Greenfield & Keller 2004), as elaborated below at Section 1.1.3.

At the level of semiotics human beings are, as far as we know, alone in our capacity for the creation of highly abstract meaning, or what semioticians call 'semogenesis'. Semogenesis is more than a system of Saussurian 'signifiers' and 'signifieds', in which arbitrary symbolic 'referents' are attached to things out there 'in reality'. More powerfully, semogenesis is a means for realizing reality itself, through our own elaborate and *self*-referential modes of communication. This is a mode that rests on the operation of complex lexicogrammatical mechanisms to generate multiple types of meaning simultaneously, including textual, ideational, and most importantly interpersonal, meaning - meaning about our relationships with each other, not with regard to objective things 'in reality', but with regard to the referents that we have already created. These semantic resources allow us to construe experience and enact social relations from a repertoire of options built in to all languages. This area of semiotics is covered by the more specific sub-discipline of sociolinguistics (Halliday 2003; 2005), discussed below at Section 1.1.4.

As presaged in the introduction to this thesis, the discipline of social anthropology has as its focus the study of human social culture - the patterns of social interaction typical of any given community of people, however that community is defined. Culture is not the mysterious entity, as some contemporary anthropologists portray it (see discussion of Ingold 2003 below), mainly because it is not an entity at all. Rather, it is an *attribute* of human existence - a *pattern* generated by the *interaction* of multiple people, as proposed by Leaf & Read (2012), White (2011), and White & Johansen (2006), (see Section 1.0 above). In the case of *social* culture, the interaction of multiple individuals generates characteristic patterns that are observable and measurable, and become more so the more they are observed. These patterns are also *accounted for* and *managed* in a meaningful way by the people to whom they belong (Leaf & Read 2012; White 2011; White & Johansen 2006). This is the field of inquiry to which the discipline of social anthropology is committed. In this context,

social anthropology occupies one region on a spectrum spanning technical knowledge about human beings (see Figure 1). On the one hand social anthropology has a more specific remit than sociobiology because its focus is on the social culture of one species of biota - Homo sapiens. On the other hand, it is more general than semiotics, because it focuses on the social *context* that gives human languages their meaning. Whereas biology extends only as far as the modelling and analysis of neurological structure, and semiotics extends only as far as the modelling and analysis of interpersonal meaning, social anthropology covers the organization of social interaction that arises from the *convergence* of both.

The discovery of social organization has been anthropology's greatest achievement, revealing that social culture can be measured in patterns of interaction at the level of whole communities. This discovery has also revealed that culture is encoded in theoretical models of positions and relations that people hold in their minds, and are cable of articulating as discrete, independently variable but often interlocking systems of ideas (White 2011; Hamberger Houseman & White 2011; Houseman 1997; Leaf 2013; Leaf & Read 2012). Social organization results from the instantiation of these ideas in community members' choices about how to interact with each other such that, where observable patterns of interaction can be measured they tend to further indicate the existence of shared systems of ideas. Ideas such as those about kinship, language, religion, and economy allow people to theorise about multiple specific roles that they play in relation to one another, and to develop theoretical models in which those roles are positioned within structures, such as families, language communities, churches, and businesses (White 2011; Hamberger Houseman & White 2011; Houseman 1997; Leaf 2013; Leaf & Read 2012).

Since the early 1970s however, the very idea of social organization has been criticised as theoretically erroneous (e.g. Ingold 2002 - see below), and demoted from its original status as the conceptual basis for the entire discipline of anthropology, to the status of a historical *theme* of early anthropological texts (Ingold 2002). What has consequently also been demoted, is the consensus that communities of human beings, at any scale, can be organized at all. Organization is now held by many social anthropologists to be a mental construct only, with no objective manifestation in reality. This demotion is elaborated in Chapter 2 with reference to Appadurai (1988), Clifford (2005), Geertz (1973; 1975), Gupta & Ferguson (1993), and Jameson (1984), among others. To briefly presage that more elaborate discussion, the demotion of the 'social organization framework' has lead in turn to a dissolution of anthropology's once clearly defined parameters, and a consequent partial transformation of social anthropology itself into a theme of 'cultural difference', rather than a discipline with a specific set of terms and definitions, as elaborated below at Section 1.3. One consequence of this dissolution has been that the adjectives 'ethno-', and 'cultural', have begun to appear as prefixes for disciplines comparing categories of phenomena *within* distinct ethnic communities, such as 'ethnomusicology', 'ethnobotany', 'cultural psychology', 'cultural studies', etc. As discussed in Section 1.3, many social anthropologists have in turn attempted to mimic the idioms

of other specialised languages in attempts to yield ostensibly new and 'better' ways of explaining causes of patterned social interaction. The idiomatic languages of semiotics and psychology have been popular borrowings, although generally without the accompanying formal modalities essential to the proper application of linguistics and psychology, such as an advanced understanding of grammar in the case of linguistics, or clinical studies in the case of psychology. As a result, where other idiolects are borrowed by anthropologists they tend to be stripped of the formal definitions and context that gives them explanatory power in their originating disciplines. Section 1.3 illustrates that social anthropologists themselves are divided on the value of this weakening of their discipline's parameters. Advocates tend to valorise it in ideological terms, claiming that by abandoning the parameters of their discipline they are freeing the individual subjects of their research from inherently degrading representations.

A typical example of this reasoning is given by Ingold's introduction to the *Companion Encyclopaedia of Anthropology* (2003), under the heading '*What is Culture*'. Ingold has been a leader in anthropological discourse during the 1990s and 2000s and is heavily cited in coursework reading material prescribed by Australian anthropology departments. On the definition of culture, he says:

> *"[C]ulture is not something that we can ever expect to encounter 'on the ground'. What we find are people whose lives take them on a journey through space and time in environments which seem to them to be full of significance, who use both words and material artefacts to get things done and to communicate with others, and who, in their talk, endlessly spin metaphors so as to weave labyrinthine and ever-expanding networks of symbolic equivalence. What we do* not *find are neatly bounded and mutually exclusive bodies of thought and custom, perfectly shared by all who subscribe to them, and in which their lives and works are fully encapsulated. The idea that humanity as a whole can be parcelled up into a multitude of discrete cultural capsules, each the potential object of disinterested anthropological scrutiny, has been laid to rest at the same time as we have come to recognize the fact of the interconnectedness of the world's peoples, not just in the era of modern transport and communications, but throughout history. The isolated culture has been revealed as a figment of the Western anthropological imagination. It might be more realistic, then, to say that people live culturally rather than that they live in cultures" (Ingold 2002:330).*

Such emotive assertions are intuitively appealing and difficult to counter on purely rhetorical grounds. However the formal definitions provided in support of the argument are characteristically weak. Firstly, 'culture' itself is not given a definition, but attributed with a series of 'false-positives': "*Culture **is not** something that we can ever expect to encounter 'on the ground*"; *What we **do not find** are neatly bounded and mutually exclusive bodies of thought and custom*"; " "*The idea that humanity as a whole can be parcelled up ... **has been laid to rest***". Secondly, the affirmative definitions that are provided are implied rather than explicit: " *What we find are **people** whose lives take them on a*

journey"; *" we have come to recognize the fact of the* ***interconnectedness*** *of* ***the world's peoples***". Neither of these latter statements actually clarify that the writer is talking about something called culture, but suggest that he is instead talking about people, possibly even in the absence of culture. The closing statement is the most informal of all: "*It might be more realistic, then, to say people live* ***culturally*** *rather than that they live in* ***cultures***". Although 'being realistic' is a compelling caveat, it does not diminish the obscurantist transformation of culture from an object that can be inhabited ('people living *in* culture') to an attribute ('people living cultural*ly*'). The most explicitly normative statement provided by Ingold regarding the discipline's orientation to culture reads as follows:

> *"[T]he concept of culture itself has obstinately resisted final definition ... It is clear that throughout the history of anthropology, scholars have adapted their notions of culture to suit the dominant concerns of the day, and they will no doubt continue to do so. Little is to be gained, therefore, from attempts to legislate on the proper meaning of the term" (Ingold 2002:330).*

How a concept can 'resist definition' if it is already acknowledged to *be* a concept is not explained, but given the central status of culture for the discipline of social anthropology, a comparable stance in biology, for example, might be to reject of 'the concept of the cell', in linguistics the rejection of 'the concept of grammar', or in psychology the rejection of 'the concept of emotion'. The most immediate question that arises from such powerful proscriptive statements would then be, *'If little is to be gained by experts by properly defining their subject matter, how much is to be lost by refusing to properly define it?*

Predictably, the biologists, psychologists and semioticians cited above have expressed concern at such repudiation of coherent definitions by social anthropologists. Through the rubrics of their respective languages, practitioners these disciplines are most struck by the contradiction between social anthropologists' explicit emphasis on the equality of all people, and their own perpetuation of inequality by denial of human unity under a single, clearly defined theoretical framework. The sociobiologist E.O. Wilson, in his 1998 treatise on discursive consilience, suggests that in rejecting common biological constraints on the human capacity for systematic organization, some anthropologists also inadvertently reject the overarching unity of the human species, relativising unequal health, education, and employment indicators as mere 'cultural variation' (Wilson 2001[1998]). Anthropological assertions that people live randomised individual 'journeys' pretend that we are not all constrained by the limits of healthily functioning minds and bodies, and therefore somehow able to live with equivalent fulfilment despite limited access to legal protections, education, healthcare, and employment (Wilson 2001). Meanwhile, semioticians such as Bernstein (1996; 1999) and Halliday (2005) point out that in rejecting a unifying language of integrated terms, social anthropology together with other social sciences, maintains an exclusive context-dependent literary code that prevents its integration with more accessible, context-*in*dependent scientific languages,

whose benefits to humankind are overwhelmingly demonstrable. The assertions of anthropologists such as Ingold, that human beings are condemned to engage in 'endless spin' in their engagement with each other, pretend that there is little or no coherent logic in the way meaning is generated among communities of speakers. This is a view reiterated by another highly regarded social anthropologist of the late 20th and early 21st centuries, Clifford Geertz, who famously argues that culture has the capacity to be without meaning at all, to be 'nonsense' (Geertz 1973; 1975)[10].

In the investigative space left for them by adjacent disciplines, social anthropologists have been largely free to define their own discipline as they see fit. Since anthropology's inception in the 1870s, this exploratory process has transitioned through a number of thematic phases, often corresponding with the temporary privileging of a particular language, including historical (evolution of culture), structural (organization of culture), functional (value of culture), and symbolic (meaning of culture). Just as natural scientific has done, so too has social science and anthropology encountered these themes in a largely serial manner, with each thematic focus spanning 2-3 decades, one after the other. As elaborated throughout the reminder of this chapter however, the major difference between the natural and social sciences in this regard, is that whereas the former have tended to refine and integrate discoveries made during each thematic phase of inquiry, building a single, increasingly integrated and self-consistent model, the latter have moved through a serial pattern of embrace and rejection, with little or no integration between discoveries at each step. This pattern is apparent even in the brief quote above where Ingold asserts that earlier discoveries of ('ideas about') discrete cultures and anthropological objectivity have both been 'laid to rest'. While Ingold provides a particularly exemplary quote for the purposes of introducing this tendency in anthropology, there are many other and more elaborate examples, as discussed below. According to Bernstein (1996; 1999) the social sciences are distinguished from the natural sciences by this pattern of serial adoption, promotion and then rejection of theoretical frameworks. By contrast, as the following section illustrates, the natural sciences engage in a reiterative process of observation, comparison, and theorisation. Where new observations are shown to diverge from earlier theories, they are amended, sometimes extensively, but rarely 'laid to rest'[11].

An increasing number of social anthropologists, led by authors such as Leaf and Read (Leaf 2007; Read 2008; Leaf & Read 2012), White (2011 *inter alia*), and others are now calling for a reversal of their discipline's drift into the realm of literature, re-formalizing its core definitions and demanding

[10] Asad (1982) critiques Geertz' thesis as disguising an implicit colonialism by asserting the capacity to discern 'sense' from 'nonsense' in autochthonous cultures (see Footnote 15 below).

[11] A powerful example of an extensive amendment to an earlier proposition is the Copernican Revolution, in which cosmological theory moved from the Ptolemaic or 'geocentric' model of the solar system, based on Sun and planets orbiting Earth, to the heliocentric model, based on Earth and planets orbit the Sun. This paradigm *shift* did not involve 'laying to rest' the entire model of orbital bodies, but rather a *reorganization* of the orbital relations *between* bodies.

that empiricism and formalism instead comprise the central tenets for any theorising in which their discipline engages. The following section presents views from prominent biologists, psychologists, and semioticians, on where the parameters of their disciplines' interface with social science, including social anthropology. For these authors, the parameters are defined by biological, psychological, and semiotic definitions of social organization, but also accompanied by distinct accounts of how the disciplines of social science and natural science have diverged under particular historical conditions, and most importantly, how consilience between them might be attained. These views are presented below.

1.1.2 The Parameters of Biology: Eusociality

Biology holds that all social activity has an adaptive basis in the evolutionary history of the relevant species, and is thus oriented towards the survival of that species under prevailing environmental conditions (Wilson 2001). Since social activity is a behavioural trait, its biological basis is located in the characteristic neurological structure of a species, more specifically in the organization of the brain and nervous system that results in behavioural, emotional, and cognitive function (Wilson 2001). Sociobiology holds that that these functions in turn generate a system of social interdependence, including sexual reproduction, food gathering cooperation, and communication (Wilson 2001). Social culture emerges from these two tiers of heritable attributes as a third, non-heritable tier of phenotypic attributes shared by *communities* of organisms (Wilson 2001).

At the heart of this system is the more specific impulse of altruism, which sociobiologists interpret as critical to any form of social cooperation (Wilson & Wilson 2007; Richerson & Boyd 2005). Altruism is defined in this context as any individual behaviour that generates benefit for another individual, and as having varying degrees of predictable reciprocity, ranging from no reciprocation at all, to equivalent exchange (Wilson 2001:155). In this theoretical model non-reciprocal altruism is a purely hereditary trait based on the evolutionary logic that if the altruistic individual behaves in a self-sacrificing manner, it must be for the benefit of closely related kin, since the conferral of benefit on the community contributes to the perpetuation of genes shared by the altruistic individual with other members of that community. This is the kind of behaviour that biologists hold to characterise cooperative sibling relationships in the insect and animal worlds. *Reciprocal* altruism by contrast, confers no such differential benefit on individuals, but instead implies that all individuals in the exchange benefit to some degree. Theoretical models of biological evolution that factor make distinction, account for what is called 'multilevel selection', where one 'level' of selective pressure is that driven by the adaptive success of individual organisms, while the other level of selective pressure is that driven by the success of the community as a whole.

From an evolutionary perspective this indicates that whereas non-reciprocal altruism confers broad benefit on a *genetically similar* community, but detriment on an individual, reciprocal altruism confers benefit primarily on the individual, and only secondarily on a community. The logic to this statement

is that altruists who expect some degree of return are factoring self-interest into their decision-making. Thus, while non-reciprocal altruism benefits the community as a whole, reciprocal-altruism benefits individuals *within* the community. Among communities of organisms exhibiting characteristic reciprocal altruism, the relative success of each community depends upon a sufficient proportion of individuals acting with a sufficient degree of reciprocal altruism. This gives rise to a distinct evolutionary pressure known as 'between-group' selection. This is the overwhelmingly predominant selective pressure among the Earth's biota, and tends to characterise the popular understanding of evolutionary theory. Although within-group selection and between-group selection generate contradictory evolutionary pressures (individual benefit vs community benefit), they always co-occur to some extent. This is because speciation is dependent on intergenerational reproduction, resulting in a highly flexible definition of 'group', ranging from entire species at the most general level though to mating pairs at the most specific level.

This model of reciprocal altruism suggests that, because the benefits of altruistic behaviour are distributed multilaterally, there is no imperative that participants in the exchange be closely related in genetic terms. This in turn indicates that reciprocal altruism is as likely to be a phenotypic trait as a genotypic trait, i.e. it is more likely to be a *learned cognitive and emotional behaviour*, than a genetically inherited one. In both cases, it is transmittable and thus subject to selective forces, since phenotypic variation is as important an evolutionary pressure as genetic heredity (Wilson 2001). However, the possibility of altruism being *learned* opens an evolutionary pathway to cooperation among unrelated individuals, based entirely on a culture of reciprocity.

Where between-group selection predominates within a species, biology identifies one of several forms of sociality: *presociality* (characterised by parental care for offspring), *subsociality* (characterised by parental care for offspring and occasional cohabitation of multiple adults), *parasociality* (characterised by parental care for offspring, consistent cohabitation of multiple adults, occasional cooperation in parenting), or *eusociality* (characterised by parental care for offspring, consistent cohabitation of multiple adults, consistent cooperation in parenting, overlapping generations , (i.e. a 'grandparental generation'), and division of labour among specific individuals, such as workers, guards, and queens in ant, bee, and wasp colonies). As the most elaborate and complex form of sociality recognised by biology, eusociality is said to characterise the form of social behaviour in which humans engage. As a taxa of behavioural types however, eusociality accounts only for phenotypic traits, not their evolutionary origin, evidenced by its attribution both to insects (where division of labour is genetically determined for the duration of an individual's life), and to primates (where division of labour varies according to age, gender and circumstance). Genetic heritability is thus not a criterion for eusociality or the social organization that characterises a specific set of relations with a eusocial community - eusociality is a *phenotypic* community trait. However, the capacity for sociality, and for complex sociality in particular, depends on a set of inherent capacities, most basically cooperation and its attendant traits of differential *affection* for community members,

aggression towards non-community members, and complex systems *communication* to facilitate coordination within communities. This is the point at which the languages of biology and psychology overlap.

The investigative fields of biology and social anthropology might be thought of as occupying the most widely separated regions on the spectrum of sociality identified in Figure 1 above. This is not because of their distinction as natural and social sciences, since biology is manifestly equipped to explain certain aspects of human sociality that social anthropology is not. Rather, these fields are widely separated by the lack of any overlapping parameters. Just as biology is unequipped to describe and explain the human dependence on meaning, social anthropology is unequipped to describe and explain the human dependence on biological processes.

1.1.3 The Parameters of Psychology: Emotion, Cognition, Behaviour

Psychology broadly recognises emotion, cognition, and behaviour as having inherent *neurological* parameters in brain and broader nervous system function, arising in particular from the relationship between heritable genes and chemical neurotransmitters, such as the well-known monoamines adrenaline, dopamine, and serotonin. However, the discipline of psychology also recognises sociality as the single primary *environmental* parameter for the function of the brain and nervous system. According to contemporary psychological models, individual cognition, emotion and behaviour are the result of a nexus between an *enabling* neurological functionality and a *constraining* environment (Greenfield & Keller 2004). For humans, this environment is overwhelmingly social, meaning that the context in which interfacing emotional, cognitive, and behavioural processes has evolved is itself constituted by other individuals undergoing the same kinds of processes. Psychology focuses specifically on this point, studying the links between depression, joy, anxiety, aggression, attraction, aversion, and other *emotional* states, the powerful instinctive *behavioural* responses that they trigger, and the way they are *cognised*. These processes are largely sub- and semiconscious, such as when the *emotion* of fear triggers a *behavioural* urge to flee, which is *cognised* sub- and semiconsciously as 'avoidance seeking'. The conscious realisation of these processes immediately gives way to semiotic processes, in particular linguistic construals of experience in terms of meaning.

Individuals do not experience their communities at a collective or macro-level, but at the micro-level of individual interactions. An individual may participate in community-level events, such as a religious ceremony or peak-hour city traffic, during which their community may manifest itself as a single entity, but in terms of engagement, each individual in that gathering is limited to watching or listening to the performance of specific dancers or singers, or striking up a conversation with specific fellow passengers. The basis of this limitation is cognitive ability, the convergence of multiple sensory faculties that give rise to perception, which in turn provides the object of interaction and cognition. The term 'cognition' itself has an elastic definition, referring to any kind of processing of 'inputs' and 'outputs', hence its relevance to multiple disciplines, from computer science and robotics, to

psychiatry. With regard to its definition for social psychology however, it refers exclusively to the subconscious and semiconscious perception of, and orientation towards, other people *just prior* to the point at which that perception gives way to conscious assessment. In these terms cognition equates with a 'construal of consciousness' (Halliday & Matthiessen 2006).

According to Halliday & Matthiessen (2006), consciousness is held to emerge through a rapidly repeating cycle of sensory stages, made up of an external *process* and internal *senser* simultaneously moving through time, and which together generate sub- and semi-conscious detection of a *projection* into an *outside world* (Halliday & Matthiessen 2006:131). It is this convergent state that is then construed, via ideas, as consciousness, and which can then be reported in terms such as "I see that ... ", "I hear that ... ", "I feel that ... ", "I think that ... ", etc. As a result, consciousness can be construed by a more or less congruent modality according to how strongly an individual maps participation and process onto process and senser roles[12]. The weaker the mapping, the more metaphorical the modality of construing, such that the senser might ultimately be construed as absent altogether, resulting in a process being construed as an object. A strongly congruent construal results in a cognitive report like "I see that the bus is here", where "I" and "the bus" are participants and "seeing" is the sensory process. By contrast, a strongly *metaphorical* construal results in a cognitive report like "the bus' presence has been detected", where the "the bus" is the only participant (the senser having been removed), and the process has been transformed into a nominal object called "presence". The spectrum or 'cline' between congruent and metaphorical construals gives rise to 'ideas' (Halliday & Matthiessen 2006). Ideas are eligible for reporting because they model the senser-process interface as an *interpersonal* exchange as well as an *ideational* one. This dualistic modelling is described as 'metaphorical' by Halliday & Matthiessen (Halliday & Matthiessen 2006), since the bridge between idea and interaction depends upon *sensing* being rendered equivalent to *acting*, transforming a receptive state into an active one, and transforming the environment into an actor, rather than an object. Construal of experience by metaphor occurs simultaneously with construal by congruence. A congruent mode of construal is one in which processes and participants are clearly demarcated and distinguished.

On the spectrum of sociality described by Figure 1 above, the region occupied by psychology bridges the gap between biology and semiotics by providing explanations for how organic neurology gives rise to meaning. However, since it is limited to micro-level processes of individual perception, psychology is still relatively distant from an explanation of culture. Although there exist specialised languages of both psychology and social anthropology that assert interdisciplinary expertise in each other's fields ('cultural psychology' on the one hand, and 'psychological -' or 'cognitive anthropology' on the other),

[12] Where 'strong' mapping construes participation and process as a sensor moving through time, but a 'weak' mapping does not.

both typically exclude the critical process of semogenesis, which mediates interpersonal cognition and community wide systems of meaning. This region of human sociality belongs to semiotics.

1.1.4 The Parameters of Semiotics: Semogenesis

Just as psychology models the bridge between neurological function and emotion, cognition, and behaviour, semiotics models the bridge between emotion, cognition, and behaviour, *and* its construal and enactment in terms of meaning. Semioticians describe this bridge as spanning 'lower order' and 'higher order' consciousness (Halliday 2005:189). From the social semiotic perspective of Lemke (1993), human social culture emerges from our unique experience of the world in terms of meanings, all of which derive from social interaction. Lemke elaborates this model as follows:

> *"Social semiotics offers the view that socially meaningful **doings** constitute cultures (social semiotic systems): that cultures are systems of interlinking, socially meaningful practices by which we make sense to and of others, not merely in explicit communication, but through all forms of socially meaningful action (speaking, drawing, dressing, cooking, building, fighting, etc.). Sign systems are abstractions from such practices (e.g. linguistic signs from speech), changing as social practices change*
>
> *"Sign systems are semiotic **resource** systems; they enable us to make meaningful actions (including utterances) by deploying these resources in recognizable, mostly habitual (and marginally creative) ways. The habitual ways in which we deploy them are identifiable as **semiotic formations**: the regular and repeatable, recognizably meaningful, culturally and historically specific patterns of co-deployment of semiotic resources in a community. A literary genre of a period is a semiotic formation; so also is an architectural style and type of building, a religious ritual, a typical holiday meal, the making of a particular type of costume. All these formations are defined in terms of the regular patterning of **actions**, of **social meaningful practices**, that members of a community are engaged in when producing them.*
>
> *"Semiotic formations provide an intermediate level of conceptual analysis between the microsocial (utterances, texts, particular acts and events) and the macrosocial (dialects, institutions, classes, ideologies), but more importantly**, they formulate the scale from microsocial to macrosocial** in terms of **actions** (social practices) and **patterns of relations of actions** (cultural formations) and not in terms of entities and aggregations of entities (individuals: corporate groups: societies). This is an essentially **cultural** view: social systems are systems of **doings**, not of **beings** as such. They are systems of interrelated cultural practices, not systems of socially interacting individuals as such. The ultimate theoretical constituents of a social system are not interacting dyads, not even individual members, but individual social and cultural practices. Social 'individuals' must be theoretically re-constructed; they are no longer 'givens' in a semiotic social theory" (Lemke 1993:2-3 - emphasis in original).*

Lemke explicitly sets the explanatory limits of semiotics at patterns of individual 'microsocial actions', beyond which lie 'macrosocial patterns of relations [between] actions'. Whereas the former are realised via semiotic resource systems, the latter are realised in culture - the 'regular patterning of actions - of socially meaningful practices'. The discipline of semiotics models what Lemke calls the 'scale' between these two strata. As a branch of semiotics, linguistics provides a more specific definition of meaning as realised in *grammar*. Halliday explains this as follows:

> *"Grammar evolved in the human species, and develops in the human brain, as a form of consciousness in which each instance - each act of meaning - simultaneously both construes (some portion of) experience, and enacts (some portion of) the social process. Typically these take place below the level of our (adult) awareness and attention; but wen we focus our attention we become aware first of all of the grammar's representative power - its potential for referring to perceptual phenomena. ..."*
>
> *"What the grammar does, in its ideational guise, is to transform human experience into meaning. The grammar construes a universe of things and relations, imposing categories on our perceptions of phenomena; in other words it sets up a theory of experience, modelling the immensely complex interaction between the human organism and its environment" (Halliday 2005:186).*

Halliday is here locating in human biological and neurological phylogeny the capacity for what he and Matthiessen elsewhere call 'higher order consciousness', generating three kinds of meaning by three distinct semantic functions: 1) *textual* meaning by *context*, 2) *interpersonal* meaning by *enactment*, and; 3) *ideational* meaning by *observation* (Halliday & Matthiessen 2006). In the linguistic model described by Halliday (2005) this phenomenon is referred to as 'metafunctional diversity', and comprises part of a protolanguage plausibly spoken by the evolutionary predecessors of *Homo sapiens*, and still apparent in the early stage of human infant language development. It is said to comprise a semantic regime wherein textual meaning is determined by the context in which speakers are interacting. Interpersonal meaning is enacted by distinctions between who says what to whom, e.g. 'Where is it?'; 'Here it is.'; 'Give it to me!'. Ideational meaning is realised in terms of the person, process, or thing to which the speakers refer, e.g. 'Mother'; 'Hold; 'Ball'. This 'primary semiotic' construes and enacts meaning in terms of what they refer to 'outside' of the grammar, in the environment.

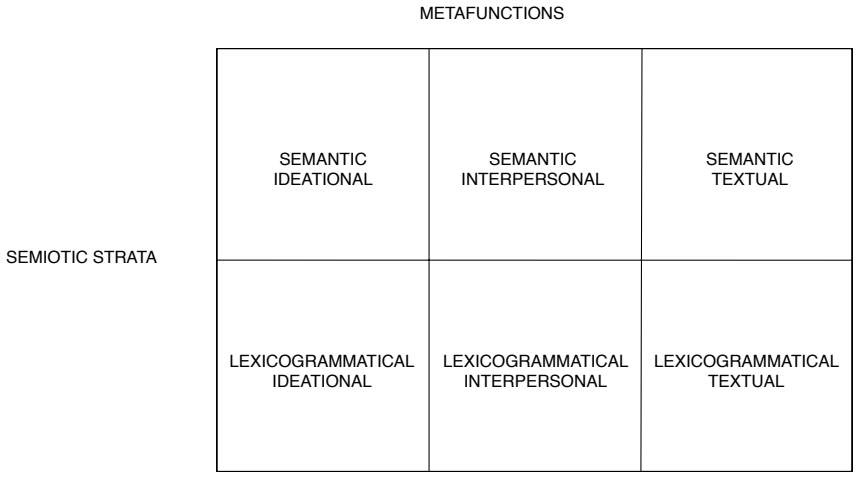


Figure 2: The stratification of metafunction that facilitates semogenetic remapping from reality to abstraction (adapted from Halliday 2005).

What distinguishes adult human language from protolanguage is the stratification of metafunction into two semiotic 'layers', made up of the aforementioned semantic stratum and together with a lexicogrammatical stratum (see Figure 2 above). In this arrangement metafunction is extended from a 'primary' semiotic inventory of linguistic 'signifiers' (lexical items for realising ideational, interpersonal, and textual meaning) and 'signified' (people, things, and processes in the environment), and used to construct a new stratum of signifiers. This 'secondary semiotic' is comprised of the same metafunctions, but instead of construing meaning about things, people, and processes in the environment, it refers to the lexical items in the primary semiotic. This is described as a metaphorical 'remapping' of meaning, from semantic to lexicogrammatical.

Remapping allows meanings construed in the primary semiotic, such as "a woman [actor] gives birth [process] to a child [goal]", to be transformed into "a matrifilial [qualifier] subnetwork [thing] of depth 2 [qualifier]"[13]. The extensibility of this remapping potential is unlimited, since any meaning in the primary semiotic can be re-construed into any form in the secondary semiotic multiple times and in different configurations. Remapping also facilitates powerful modes of classification because once entities, processes, attributes, and relations become susceptible to transformation they also become susceptible to classification in new ways. At a solely primary semiotic level, 'a woman gives birth to a child' might be classed along with other clauses built in the same model of 'a person transforms a thing', such as 'a man crashes a car'. However, after lexicogrammatical transformation into a structure like 'matrifilial subgraph of depth 2', the attribute 'matrfilial' can be compared with other attributes like 'patrifilial' and 'ambifilial'; the thing called 'subgraph' can be compared with other things like 'network', 'tree', and 'component'; the attribute 'depth 2' can be compared with other

[13] In the technical language of kinship network analysis - discussed in Chapter 4.

attributes like 'width 1' and 'height 2'[14]. None of this is possible without the interface between primary and secondary semiotic strata.

Classification of sociality by remapped relations between lexical items also enables the creation of subsets and supersets by the respective specification and generalization of relations. If the classificatory relations between things called 'matrifiliation', 'patrifiliation', and 'ambifiliation' are deemed equivalent, they can be related by a more general type of relation to a more *general class of thing* called 'filiation', of which they are all a more specific variety. They can also be related by a *more specific type of relation* to a *more specific class things*, such as 'lineality', of which they are a more general variety. This enables us to say that 'matrilineality is a type of matrifiliation'. It also enables us to say 'matrilineality is a type of lineality'. Both these statement imply a relation between lineality and filiation, and generate the emergence of complex but predictable lexicogrammatical structures, in this case those that constitute kinship network analysis (the ultimate subject of this thesis).

Beyond conferring great power on the universal human capacity to make meaning, semiotic stratification enables construction of theoretical models whose meanings are formalised by self-consistent classification and rules of mapping. An example in the case of a chemical model might be "an atom is made up of electrons, protons, and neutrons", where seven elements are partitioned into four objects (an atom, electrons, protons, neutrons) and three relations (atom-electrons, atom-protons, atom-neutrons), and are assembled into a single structure, conferring additional relations between electron-proton, electron-neutron, and proton-neutron by virtue of their isomorphic relations with the atom. A comparable example in the case of a kinship theory might be a statement such as "your mother's brother is your uncle", where seven elements are partitioned into four people (you, mother, mother's brother, uncle) and three relations (you-mother, mother-brother, you-uncle), and assembled into a single structure, conferring new isomorphic relations between mother's brother-uncle, mother-uncle, by virtue of their shared relation with you. None of the elements of either model need be present or even observable in order for either statement to be logically self-consistent and meaningful. Another kinship example, whose relevance will become clear in the following chapters, might be, 'a goanna woman marries an emu man, and together they have goanna children'.

Social semiotics is able to precisely describe how such models are constructed in a wide variety of linguistic settings, but lacks the explanatory resources to describe the instantiation of these models in the conscious decision-making that results in patterned social outcomes at the macro-level of a community. The closest social linguistics comes to this function is in explaining how different lexicogrammatical codes propagate among different language communities (see Bernstein). As Halliday says:

[14] Again, in the technical language of kinship network analysis - discussed in Chapter 4.

> *"The grammar of every natural language is - or rather includes - a theory of human experience; this engenders what its speakers perceive as 'reality'. So by examining the ideational component within the grammar, we can explore the* ***sources*** *of cultural formations and beliefs" (Halliday 1993:4 [emphasis added]).*

Thus, while semiotics, at the specific level of linguistics, can explain how experience is construed as the *source* of the models that people use to develop social organization, the *effects* of these models - the bridge between *construal* of the experience used in the construction of the models, and the *instantiation* of the models themselves - lies *beyond* semiotics. Language's facility for remapping, especially for semiotically transforming people and relations into things and attributes, together with its model-making power, provides the mechanics necessary to build operational theories of choice.

This section has mapped out the parameters of the disciplines 'adjacent' to social anthropology insofar as they share overlapping fields of inquiry on a 'spectrum' of human sociality, as shown at Figure 1 above. These fields are distinguished by the specialised idiomatic languages that have been developed to address them. The goal of this section has been to establish the most elementary basis for the theoretical modelling that follows in the rest of the chapter, and subsequently in the rest of the thesis. The more specific modelling presented in the rest of the chapter describes and explains the respective modalities and languages of anthropology's adjacent disciplines as either natural or social sciences. This exercise sets the more elaborate basis for the positioning of the new method of stKNA, introduced in the latter part of the thesis.

1.2 Towards a Re-Formalized Definition of Social Anthropology

> *"The justification for yet another language can be only whether, on the one hand, its use enables a more productive and more general perspective, and on the other, whether it leads to new research possibilities and new interpretations" (Bernstein 1999:158)*

This section introduces the most elementary characteristics of what Bernstein (1999) calls a 'new language', in relation to the propositions set forth above by Leaf & Read (2012), White (2011), Wilson (2001), and others. In order to address the ultimate goal of those propositions - an effective consilience between anthropology as a social science, and its adjacent disciplines as natural sciences - the current section describes the discrete ways in these two kinds of language function to construct theoretic models. As mooted in Section 1.0, the current configuration of anthropology is not sustainable if the goal is to render it useful as a means of explaining social culture in a systemic and functional way. The key anthropological theorists cited in that section, including White, Leaf, and Read, all point to an acute need for a re-formalised, empirically grounded, and technologically integrated language. This language needs to be capable of collecting and modelling data, building self-consistent analytical frameworks, and methodically adapting those frameworks in light of new data, in order to describe complex patterns of human sociality and to explain the causes the those patterns. What White, Leaf, and Read are advocating for is, in short, a new disciplinary language.

The current section outlines the structure of scientific revolutions, for both natural and social science, to illustrate why the definition of causality is so important for functional modelling. Whereas natural science has developed a language that accurately locates causality between force and matter, social science continues to use a language that treats human actors and interpersonal social processes as metaphorical objects, thereby obscuring a functional definition of causality, and retarding the field's ability to build accurate models that link micro-level phenomena of individual behaviour, with macro-level phenomena of social organization. Developing a concise overview of this distinction permits the stKNA method, and the broader subfield of kinship network analysis, to be more clearly located within the realm of a reformulated social anthropology. This concision affirms both KNA and stKNA as scientific modes whose content has thus far formed the domain of largely a largely non-scientific social anthropological language.

1.2.1 The Different Modalities of Natural Science and Social Science

The preceding three sections have been presented in order to show how a general field of sociality, construed as an ordered spectrum ranging from biological existence to the realization of meanings, is mapped by three separate disciplines, one (biology) belonging to natural science, and the other two (psychology and semiotics) belonging to social science. The presentation has been constructed in an ideal arrangement in order to show how a general principle (relationships between organisms) may be investigated by disciplines with ostensibly distinct theories and methodologies without challenging or contradicting one another. In this arrangement sociality does not take on mutually incoherent

definitions for each of the three disciplines, but rather exhibits distinct sets of interrelated *characteristics* in different regions of the spectrum.

As elaborated at Section 1.1.2, biology shows how sociality operates at an evolutionary scale in providing adaptive advantage under prevailing conditions. It also explains how the pathway by which sociality evolves may be either or both genetic and/or phenotypic. It further shows that sociality ranges along a spectrum of varying altruism, from reciprocal to non-reciprocal, and that the manner in which sociality benefits a given species varies accordingly.

As elaborated at Section 1.1.3, psychology explains how sociality operates at the scale of individual human beings, in the neurological function of sub- and semiconscious emotional, behavioural, and cognitive processes. These manifest in socially grounded instincts of perception, feeling, and reaction. Psychology further shows that these processes constitute the primary intra-organismal parameters of interpersonal interaction, which in turn constitutes the primary extra-organismal parameters of those processes.

As elaborated at Section 1.1.4, semiotics explains how sociality operates at the scale of interpersonal *consciousness* in terms of realizable meaning, where the emotional, cognitive, and behavioural processes of sub- and semi-consciousness are construed by lexicogrammar in terms of sensers and processes moving through time. These in turn give rise to different kinds of meaning operating at different levels of abstraction, ranging from nominal reference (e.g. 'That person is my mother') to metaphor (e.g. 'The Earth is the mother of humanity').

In this ideal model of how sociality may be investigated in a coherent and integrated manner, the discipline of social anthropology would fit at the end of the spectrum represented at Figure 1, where sociality transitions from the scale of interpersonal meaning to the scale of multi-personal metaphor. It would fit here because, as outlined above, social anthropology is the study of feedback between normative metaphors in use among communities of people on the one hand, and the effect of those metaphors when consciously implemented in real-life decision-making *by* those people on the other hand. These phenomena are simultaneously semiotic, psychological, and biological. *Yet*, the point at which they interface - at the nexus of an observable dialectic between metaphor and social organization - is beyond the explanatory reach of these other disciplines. This nexus is the disciplinary space left empty for social anthropology. Unfortunately however, at the current point in the historical development of anthropological knowledge, such parameters cannot be confidently advocated without a carefully constructed history and defensive argument.

The current thesis adopts the definition of social anthropology provided by White (2011 *inter alia*) and Leaf and Read (Leaf 2007; Read 2008; Leaf & Read 2012), which the latter call 'a new science'. In this rendition, social anthropology is the study of autochthonous models of social organization and the instantiation of those models in the organization of the societies to which they belong. As such,

the disciplinary parameters of social anthropology are not at all mysterious or difficult to grasp, as claimed by some (e.g. Ingold 2003). They are, however, counter-intuitive from the perspective of natural science, since they demarcate a field of inquiry that involves constructing what are, in effect, theoretical models of *pre-existing* theoretical models (Leaf & Read 2012). This is what differentiates social anthropology, and the social sciences generally, from the natural sciences. Natural science involves the construction of models of systems, for which any non-scientific pre-existing models are irrelevant. Physics, chemistry, biology, and even psychology, entail systematic observation of phenomena that will occur irrespective of humans' awareness of them (see Figure 3 below). This is not to deny that non-scientific models may be constructed in attempts to understand the same phenomena (e.g. astrological models of the cosmos). Rather, it is to acknowledge that pre-existing models are not a *factor* in the phenomena, that they cannot alter it (a robot cannot be landed on the surface of Mars using astrological models, for example). The effects of neurotransmitters in giving rise to emotions of love, fear, and anger, depend no more upon our awareness of them than do, say, our awareness of chromosomal recombination in relation to embryonic development and childbirth, or our awareness of gravity in relation to our feet staying on the ground. We may construct and refine *various* kinds of theoretical models for understanding these phenomena, but the phenomena themselves will occur irrespectively. Natural science makes observations and constructs models under these assumptions.

Social science, by contrast, involves the construction of models of systems where the systems themselves are contingent upon the pre-existence of *other* theoretical models. This is because these systems are comprised of the ideas that people hold about their own social organization, and the instantiated *effects* of those ideas *upon* that organization. This nexus is unparalleled in any other field of inquiry. In fact, this is so unique a convergence, that it yields what might be called social science's version of a 'singularity' - the recognition that scientific models are just phylogenetic variations among a panoply of other kinds of model, be they models of cosmogony, of kinship, of property relations, or of any other system of ideas found among human populations. Leaf (2007) describes this revelation as giving rise to a kind of 'struggle' on the part of anthropologists, to avoid blurring the distinction between their own theoretical models, and the autochthonies theoretical models that they study (Leaf 2007:7 - see APPENDIX 1 for extended quote).

Indeed, this is where the idea attached to the term 'culture' has become a problem for social science, since the meaning of the term is transformed from its normal natural scientific meaning - the property of multiple organisms being 'socially organised' (e.g. a 'bacterial culture') - to having two distinct and simultaneous meanings. Whereas non-human organisms exhibit only the attribute of an independently observable culture, humans both *exhibit* independently observable culture, and *construe* it as systems of ideas that they themselves can describe as a culture. This is the origin of the folk definition of culture as 'a thing' that one may 'possess', *viz*. 'my culture'. Thus, in social scientific terms, social culture is, a) independently observable social organization; *b)* the ideas that can be

elicited by members of that society about that organization, and; c) the causal links between organization and ideas (see Figure 3b).

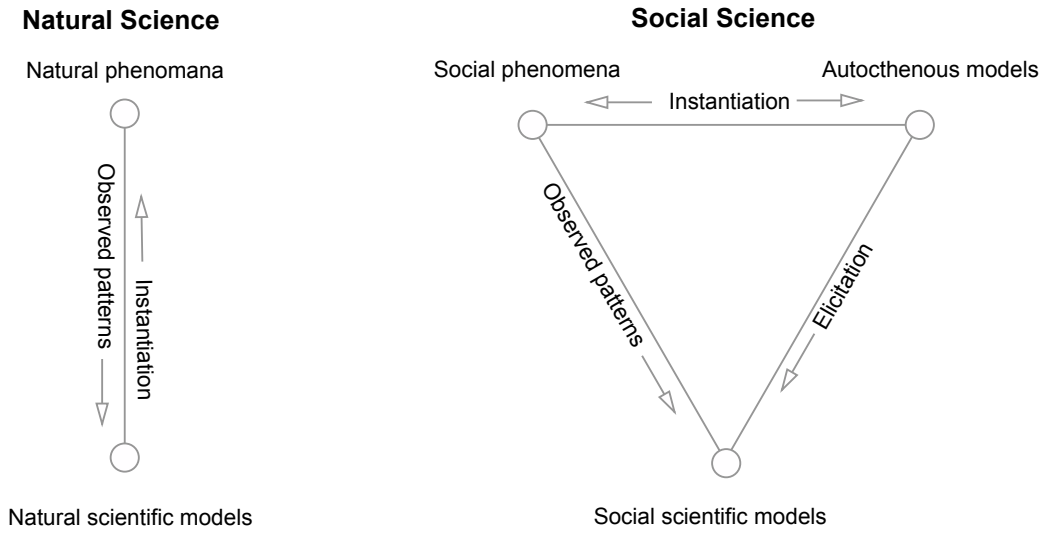


Figure 3: The construction of natural scientific models vs. social scientific models. Natural scientific models take into account only a) the patterns observable in the subject phenomena. Social scientific models take into account a) the patterns observable in the subject phenomena, together with b) the autochthonous models of the subject population, and, c) the causal links between autochthonous models and observable social phenomena.

All branches of social science share this framework. Psychology, which has arguably come the closest to a consilience with the natural sciences in integrating socially oriented models of human thought with natural scientific models of brain and nervous system function, nevertheless depends on elicitations given by patients in order to measure their perceptions and feelings against independently observable cognitive and behavioural function. These elicitations usually take the form of an explanation given in terms that are meaningful to both the patient and psychologist. An electroencephalographic scan may reveal patterned electrical activity in areas of the brain that correlate with these perceptions and feelings being elicited, rendering them independently observable, but they cannot be classified by a psychologist in terms such as 'love' or fear' until a patient explains their corresponding sensations *using those terms*. In other social scientific fields, such as economics and political science, theoretical models are similarly based on the explanations of pre-existing economic and political models provided by the participants in those economic and political systems. The pivotal economic model of a labour theory of value (LTV) for example, depends on the assumption that labour is a 'thing' that can be alienated, i.e. that it can be 'produced' by someone and exchanged for other kinds of 'thing' that have been produced by other people. Early economists did not develop the LTV model merely by observing the activity of people interacting. Rather they built it from elicitations from people who called themselves 'employees' and 'employers', and who already had their own models of how to organise a trade in labour. The reason that LTV made and continues to make such obvious sense to many economists, is that they themselves are participants in the economic system that they are modelling - each of them are employers and/or

employees at some point in their lives, and so are able to model causal links between the social interactions that they observe, and the systematic ideas that employers and employees hold, which motivate them to engage in those interactions in a systematic way. The total set of ideas, links, and interactions, is referred to by economists as 'an economic system'.

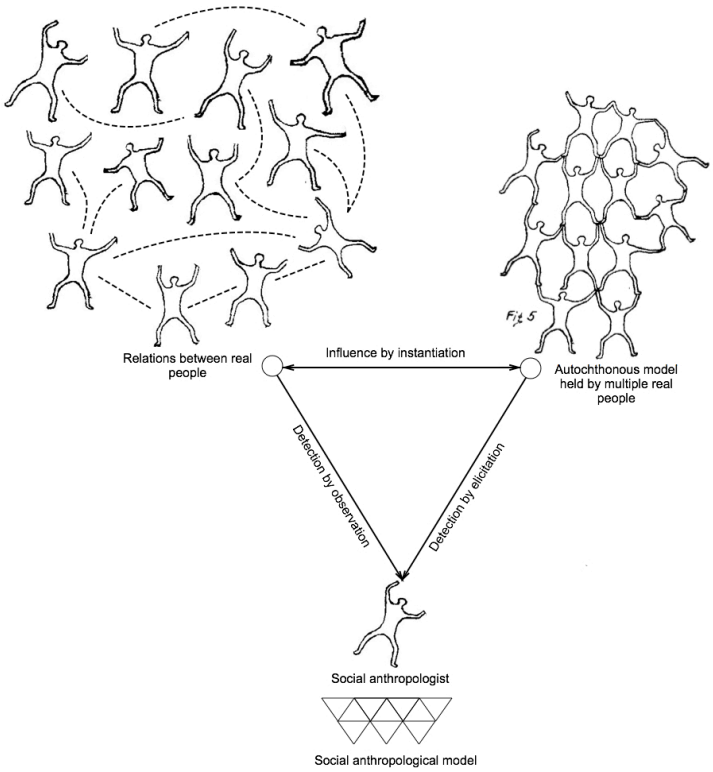


Figure 4: A model of how anthropology (ideally) makes models. The example autochthonous model at top-right was elicited from members of Gamilaraay speaking population of north-western NSW during a religious ceremony. It has been selected for its intuitive symbolism. The relationships between population members at top-left indicates observable ceremonial relations between population members. The geometric model at bottom represents a graph-theoretic model developed on the basis of structural isomorphism between autochthonous model and observable relations. The original anthropologist was R.H. Mathews (1895).

The most intuitively accessible example is possibly that of linguistics, which highlights the capacity of humans to learn complex and sophisticated lexicogrammatical functions using only context-dependent autochthonous models to guide them. The model that a language-speaker has of their own language is not the model that a linguist has of the same language, which is why school students have to be taught grammar, even though they have already learnt to speak the language fluently in childhood. The inability of early phonetics-oriented linguists to comprehend how language could be meaningful without a formal model of grammar, led them to a develop models of language that located meaning at the level of lexical items (i.e. sounds, words, and phrases), since these form the terms and definitions of non-linguistic folk models of language. However, by failing to observe the systemic relations between *access to* and *choices from* not only lexicon, but also grammar, and the social *contexts* in which language is used, these phonetics-oriented linguists deduced that language is simply a system of 'signifiers' arbitrarily attached to 'signifieds' in the real world. As such, they built a

model of language capable of generating taxonomies of sounds, words, and phrases, but incapable of explaining how these generate elaborate systems of metaphor for construing reality as an interconnected and self-consistent system of people, processes, and things (Halliday 2005 - see discussion at Section 1.1.4 above).

As elaborated in Chapter 2, anthropological knowledge also tends to construe people and relations in terms of a system of 'signifieds' and 'signifiers', which can be grouped together and given labels. Once a social 'group' is defined by a label, then other theoretically postulated (but not actually observed) members of that group can be *deduced* as existing according to their shared use of that postulated label. This further facilitates the deduction (and frequent assertion) that group labels are arbitrary. Once a 'signified' has been labelled with a 'signifier', there is a phonetics-oriented tendency to group that 'signified' together with whatever else has the same label, e.g. grouping 'tribe' with 'language'. While this technique may render sensible results at the micro-level of individual linguistic items, such as words and phrases, it tends to break down for more complex linguistic structures, and ceases to function altogether at the level of autochthonous models of social organization. The application of group theoretic principles to human social organization is a misapplication of natural scientific methodology, as represented in Figure 3, where the methodology has been applied to an existing autochthonous model as if it were a natural phenomena, to the exclusion of real world social behaviour and empirical data documenting that behaviour. In this misapplication, autochthonous models are treated as if they were a spontaneous occurrence, being 'invented' or 'adopted' by members of a population in an attempt to understand a social organization in which they were already engaged, and with which they would continue to be engaged whether their own model existed or not. This hypothesis, that social culture consists of labels arbitrarily applied to objectively observable groups, permits the further erroneous deduction that autochthonous models of sociality comprise nothing more than a distinctive 'perspective' on independently existing phenomena, and that anthropological models comprise another, alternate 'perspective'.

There are two very clear reasons that anthropological models and autochthonous models can never be two perspectives on the same phenomena. The first reason has been thoroughly explained by anthropologists such as Asad (1982), as the result of systemic material inequality between social anthropologists and the members of the populations with whom they work. Unlike sociologists, psychologists, economists, and linguists, social anthropologists are rarely fellow members of these populations, and thus subscribe to few if any of the autochthonous models, or bear genuinely equal social relationships with members of the those populations that are also rendered meaningful by those ideas. Social anthropologists are therefore tyoically precluded from making social decisions based on those autochthonous models, regardless of how sincerely they believe that they may be able to do so. This is quite separate to anthropologists' capacity for ethical conduct, empathy, or friendship, but rather goes to the distinct childhood socialisation of individuals, which typically remains beyond transcendence.

The second reason that anthropological models and autochthonous models can never constitute two perspectives on the same phenomena, is that autochthonous models must be the *object* of anthropological modelling, if that modelling is to have any power as an explanation of the feedback between ideas and social organization. If anthropologists locate their own model on the same plane as the autochthonous model, as an 'alternate perspective' on observed behaviour, then their model fails to explain how *ideas affect behaviour*, and how *behaviour affects ideas*; such a model is not only *deductive* (by deducing the structure of ideas in the absence of instantiated behaviour), but also a tacitly colonial one, since a model of this kind seeks to expropriate the original meaning given in the definitions of the autochthonous model, and replace it with the meaning of its own model - a model built in the absence of real-world evidence of isomorphic behaviour. This is the basis of Asad's criticism of Geertz, cited above, and other self-avowed 'post-colonial' anthropologists who declared themselves free of hegemonic bias[15]. The differences between an autochthonous model of social organization and a social scientific model of culture are stark. An autochthonous model is very clear to a real person in terms of the roles and relations that connect them to real people, and which roughly predict the ways in which those people will, or should, interact with each other.

The current section has described the distinct modalities of social and natural science disciplines in terms of their objects of study - the things about which they theorise. It has been shown that natural science constructs theoretical models of natural phenomena based exclusively on observations of those phenomena. By contrast, social science constructs theoretical models of social phenomena based on both observations of social interaction *and* observations of the theoretical models articulated by the people engaged in that interaction. At this level, social scientific modalities are thus more complex than natural science modalities. The next section presents an explanation of the distinct languages that social and natural science disciplines use to realize not only their own modalities, but also the expert knowledge that they develop as a result.

[15] Asad (1982) points out that Geertz' abandonment of structural models in favour of literary 'thick description' only served to camouflage his continuing failure to account for logical relations between autochthonous models and behaviour. Whereas his pre-crisis predecessors had failed this test of objectivity by claiming their models to be 'more accurate' explanations than the autochthonous models of the subject populations, Geertz and other crisis-era anthropologists failed the same test by claiming that autochthonous models *lacked* structure altogether.

1.2.2 Bernstein's Theoretical Model: Discourses, Knowledge Structures & Codes

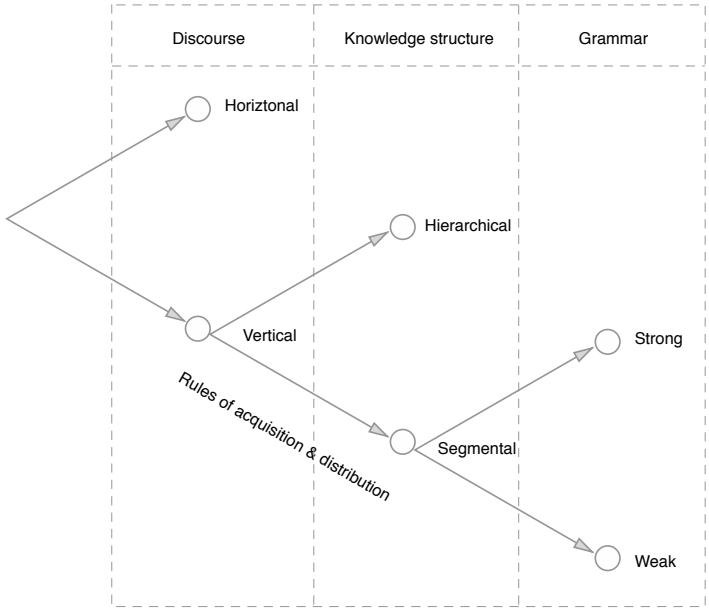


Figure 5: Bernstein's Theoretical Model of Discourses, Knowledge Structures and Distributive Codes (adapted from Bernstein 1999)

The distinction between social and natural science modalities, described above, has a corollary in the distinction between social and natural science *languages*. According to Bernstein (1996; 1999) this distinction is expressed at three levels for these two kinds of expert language. These levels include the contexts in which each kind of language used, the ways in which acquisition and distribution of each kind of language is regulated, and the ways in which each kind of language organises knowledge. Bernstein's extended model may be represented as a tree-like network structure, consisting of three sets of relations and three classes of related elements (see Figure 5 above). The bipartite and oppositional relations between each class (represented by forked lines) comprise 'rules for acquisition and distribution' by which the transmission of knowledge is regulated. Each set of bipartite relations that originates from these rules then yields, or terminates in, a bipartite and oppositional set of secondary elements, comprising a) horizontal and vertical discourses, b) hierarchical and segmental knowledge structures, and c) strong and weak grammars.

In the intermediate-level class of Bernstein's model, there is a division of expert languages into two related types of 'knowledge structure', based on distinctions between 'internal principles', and 'social bases'. Internal principles are those constituent structural properties that give each form of knowledge its specialised terms, definitions and logical relations, while social bases are those contextual factors that give each knowledge structure its pedagogic authority in a social setting. Both forms of knowledge structure are themselves elements within the upper-level class, *discourse type*, referred to as a 'vertical discourse'. Vertical discourse is in turn opposed to 'horizontal 'discourse'. The overarching duality of horizontal and vertical discourse is accompanied by an ideological evaluation typical among experts, which states that vertical discourse is used by hegemonic

communities to dominate users of horizontal discourse. This is an important element of so-called 'post-colonial' anthropology, which distinguishes its own knowledge (and academic knowledge generally) as 'official', in opposition to the knowledge of autochthonous populations, which it defines as 'local' or 'common sense'.[16] Bernstein distinguishes between vertical and horizontal discourses according to the ways in which they organise kinds of knowledge, with the former restricted to a localised scale and dependent on context, and the latter scale-free and independent of context:

> *"[With regard to horizontal discourse] we are all aware and use a form of knowledge, usually typified as everyday or 'common-sense' knowledge. Common because all, potentially or actually, have access to it, common because it applies to all, and common because it has a common history in the sense of arising out of common problems of living and dying. This form has a group of well-known features: it is likely to be oral, local, context dependent and specific, tacit, multi-layered, and contradictory across but not within contexts. However, from the point of view to be taken here, the crucial feature is that is it segmentally organised. By segmental, I am referring to the sites of realisation of this discourse. The realisation of this discourse varies with the way the culture segments and specialises activities and practices. The knowledge is segmentally differentiated.' ...*
>
> *"[By contrast,] a vertical discourse takes the form of a coherent, explicit, and systematically principled structure, hierarchically organised, as in the sciences, or it takes the form of a series of specialised languages with specialised modes of interrogation and specialised criteria for the production and circulation of texts, as in the social science and humanities" (Bernstein 1999:159).*

As noted, each of these two kinds of discourse tends to be associated with rules regulating access, evaluation, and transmission of knowledge. The functionality of horizontal discourse as a semiotic medium for transmitting knowledge is entirely dependent upon an individual's potential for *using* that discourse strategically. This potential is said to be delimited by each individual's personal 'repertoire' of discursive strategies, and the relevance of each strategy in that repertoire to any given situation. Personal repertoires are said to derive from maximal sets of such strategies, termed 'reservoirs', defined as the sum of those existing within the community of which an individual is a part. Repertoires may vary across a reservoir, but are described as overlapping around a 'common nucleus'. In order for knowledge to be effectively transmitted across a community, to the extent that repertoire and reservoir can be shown to exist and to be linked, there must also be a regulatory framework built around the social structure of that community. The more 'connected' a community, the deeper its reservoir, and the greater the possible variations among individual repertoires. Any

[16] In fact, the Geertz' 'thick description' explicitly promotes 'common sense knowledge' to define what it asserts to be the proper subject of anthropological research (Geertz 1975).

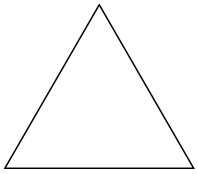
restriction on the 'flow' of knowledge from collective reservoir to personal repertoire will result in the partitioning of knowledge across the community. The *form* of this partition varies according to the corresponding rules of distribution.

For the 'common sense' knowledge of horizontal discourse, these rules are loose and unintegrated, such as in the examples provided by Bernstein, with the rules for learning how to tie one's shoelaces, or for learning how to use a toilet (Bernstein 1999). The partitioning of this kind of knowledge is 'segmental', meaning that each segment of knowledge is disconnected from each other, and useful only in isolated and specific contexts. This type of knowledge is usually practical and taught by demonstration. The rules for *learning* this knowledge - its distribution and acquisition rules - are also unintegrated and context-dependent, such that they are more likely to vary within each community, as well as from one community to another. The primary difference between teaching this kind of knowledge, relative to any other kind, is that it can be more easily regulated because of the weak relations between each segment of knowledge. Excluding one segment from a syllabus will not affect the teaching of any other segment, hence Bernstein's term 'segmental pedagogy'. This lack of integration between segments is due to the implicit significance of each segment. Any perceived integration between segments is really integration between contexts (such as the same person teaching multiple different segments, or different people teaching the same segment). This is *not* equivalent to actual integration between the segments themselves.

For the 'official' knowledge of vertical discourse, partitioning is not segmental but rather hierarchical, with distributive rules for connecting distinct kinds of *meanings* to each other, rather than for connecting distinct segments of knowledge to distinct contexts. The meanings that constitute knowledge in vertical discourse are thus *interdependent*, requiring a self-consistent, integrated set of distributive rules for teaching them. The self-consistent integration of knowledge in vertical discourse means that its significance is both explicit and independent of context. However, its inherent *self-consistency* also means that vertical discourse must be taught in a temporally sequenced manner in order for that consistency to be maintained. This requires what Bernstein calls 're-contextualization' in space and time, whereby the situation in which the knowledge is taught is strictly controlled. This is a core function of the academy, which, among other functions, serves to recontextualize distribution, so that knowledge is presented to certain members of certain communities according to certain evaluative principles.

In the intermediate-level class of Bernstein's model, the distributive rules of vertical discourse correlate with two kinds of knowledge *structure*, one hierarchical, and the other horizontal. Bernstein provides an incisive distinction worth quoting at length as follows:

> *"Briefly, a hierarchical knowledge structure' looks like the following:*



"This form of knowledge attempts to create general propositions and theories, which integrate knowledge at lower levels, and in this way shows underlying uniformities across an expanding range of apparently different phenomena. Hierarchical knowledge structures appear, by their users, to be motivated towards greater and greater integrating propositions, operating at more and most abstract levels. Thus, it could be said that hierarchical knowledge structures are produced by an 'integrating' code.

"In contrast, horizontal knowledge structures consist of a series of specialised languages with specialised modes of interrogation and criteria for the construction and circulation of texts. Thus, anyone of the specialised disciplines within the form of a horizontal knowledge structure found within the humanities and social sciences can be visually portrayed as:

$$L^1, L^2, L^3, L^4, L^5, L^6, L^7 \ldots L^n$$

"Thus, in the case of English literature, the languages would be the specialised languages of criticism; in Philosophy, the various languages of this mode of inquiry; and in Sociology, on which we shall focus, the languages refer, for example, to functionalism, post-structuralism, post-modernism, Marxism, etc. The latter are the broad linguistic categories and within them are the idiolects (theories) of particular favoured or originating speakers. Horizontal knowledge structures, unlike hierarchical knowledge structures, which are based on integrating codes, are based upon collection or serial codes; integration of language in one case and accumulation of languages in the other" (Bernstein 1999:162).

Bernstein is here classifying as 'hierarchical knowledge' what White, Leaf, Read, Halliday, Wilson, Richerson, and others distinguish as the natural sciences on the one hand, and as 'horizontal knowledge' what these researchers distinguish as social science and the humanities on the other. Although these other authors provide reasonably formal models for understanding the origins and practical consequences of this distinction, they do so using the specific terms of their own expert languages. Bernstein takes a perspectival 'step back' in expanding the scope of his model to incorporate the process by which those expert languages are actually acquired by future experts in the course of their academic training. As illustrated at Section 1.1 of the above, each expert discipline moves through successive phases of expansion or 'discovery', making new observations, and incorporating those observations into the particular models that comprise their respective fields of expertise. In Section 1.1 it was proposed that human sociality comprises an object of research at which four separate expert subjects or fields are directed, one from the natural sciences (biology), and 3 from the social sciences (psychology, semiotics, and anthropology). Of these, it was suggested

that all but social anthropology exhibit traits of what Leaf and Read have called 'formal-empiricism', that is, collection of data, observation of patterns in that data, and induction of causal relations generating the emergence of those patterns. Only social anthropology, in its contemporary crisis-era state, was highlighted as lacking this methodological framework, because of the conflation of its own theoretical models with the autochthonous models that form the subject of its study. Bernstein locates the origin of this conflation in the mechanism by which fields of knowledge develop, as follows:

> *"In the case of hierarchical knowledge structures, development is seen as the development of theory, which is more general, more integrating, than previous theory. ...*
>
> *"Opposition between theories in hierarchical knowledge structures [central part of Figure 5] is played out in attempts to refute positions where possible, or to incorporate them in more general propositions. At some point, sometimes later than sooner, because of special investments, a choice is possible provided the issue can be settled by empirical procedures. ... (Bernstein 1999:163).*

The lower-level class of elements in Bernstein's theoretical model is comprised of 'grammars' that form the organizing structure of horizontal knowledge structures themselves. Bernstein's explanation of the structuring effects of grammar is concise and bears quoting at length:

> *"It is … necessary to introduce yet another distinction between particular discourses with what might be called strong grammars and those with weak grammars [rightmost part of Figure 5]. The former are those discourses based on explicit, formally articulated concepts, relations and procedures, as in economics and linguistics, whereas in the latter discourses (with weak grammar) concepts, relations and procedures are much less formally articulated as in sociology and social anthropology" (Bernstein 1996:174).*
>
> *...*
>
> *"It might be useful here to make a distinction within horizontal knowledge structures, distinguishing those whose languages have an explicit conceptual syntax capable of 'relatively' precise empirical descriptions and/or of generating formal modelling of empirical relations, from those languages where these powers are much weaker. The former I will call strong grammars and the latter weak grammars. It is important to add here that 'strong' and 'weak' must be understood as relative within horizontal knowledge structures. From this point of view, economics, linguistics and parts of psychology would be examples of strong grammar. Mathematics would also be considered a horizontal knowledge structure as it consists of a set of discrete languages, for particular problems. Thus, mathematics and logic would be regarded as possessing the strongest grammars, although these languages, for the most part, do not have empirical referents nor are they designed to satisfy empirical criteria. Examples of weak*

> *grammars would be sociology, social anthropology, and cultural studies [as at the rightmost part Figure 5]" (Bernstein 1996:164).*

Thus, according to Bernstein's model, the discipline of anthropology is realized by a weak grammar in the lower-level class, organized by a horizontal knowledge structure in the intermediate-level class, which is part of a vertical discourse in the upper-level class. With regard to implications for the study of human sociality, as mapped out in Figure 1 above, anthropology (and the social sciences collectively) are thus distinguished from biology by a further characteristic of mutual exclusivity, apart from former's inability to incorporate meaning into its explanations. This bifurcation of elements in the model is the mode by which expert languages are distributed and acquired in the natural and social sciences respectively. Bernstein puts it this way:

> *"In the case of hierarchical knowledge structures [viz. natural sciences], the acquirer does not have the problem of knowing whether she/he is speaking physics or writing physics [or example], only the problem of correct usage. The strong grammar visibly announces what it is. For the acquirer, the passage from one theory to another does not signal a break in the language; it is simply an extension of its explanatory/descriptive powers" (Bernstein 1996:164).*

Biology is therefore 'out of the picture' as far as this part of the analysis is concerned, since it is comprised of a hierarchical knowledge structure, and is thus free of the 'problem of correct usage'. These mechanisms are facilitated by the 'grammar' of hierarchical discourse.

In the following section, the challenge of developing a correct usage and acquisition framework for anthropology is given further scope with regard to Kuhn's accounts of 'scientific revolutions'. Bernstein's model converges in important respects, both with Kuhn's (2012[1962]) description of natural science's historical internalisation of 'unproblematic' acquisition rules, and with Leaf & Read's (2012) call for such an internalisation to take place within anthropology itself. It is suggested that this convergence takes place around the common process of what is called 'discovery' in both vertical and horizontal knowledge structures. Anthropology is already capable of making discoveries. The remaining challenge is to develop a framework for ensuring that those discoveries have explanatory power.

1.2.3 How Discoveries are made in the Natural and Social Sciences

> *"The transition from a paradigm in crisis to a new one from which a new tradition of normal science can emerge is far from a cumulative process, one achieved by an articulation or extension of the old paradigm. Rather it is a* ***reconstruction of the field*** *from new fundamentals, a reconstruction that changes some of the field's most elementary theoretical generalizations as well as many of its paradigm methods and applications" (Kuhn 2012[1962]:84 - emphasis added).*

In 1962 Kuhn provided the first comprehensive account by a natural scientist, of the manner in which a vertical knowledge structure develops. Prior to Kuhn's outline of the 'structure of scientific revolutions', the prevailing view of natural science's explanatory power (at least among philosophers of natural science) was based on an ambiguous conception of something called 'the scientific method' (Louth 2011). This autochthonous folk model is construed as a perpetually reiterated process for 1) developing hypotheses; 2) accumulating data with more and more detail over the course of successively newer and more specific observations, and; 3) using those observations to prove or disprove hypotheses[17].

Kuhn's original contribution was to point out that natural scientific knowledge does not actually expand in this way. Firstly, the 'scientific method' has nothing to do with 'proving' or 'disproving' *a priori* hypotheses. In reality, what natural scientists do is make observations of natural phenomena, measure those observations, and then search those measurements for patterns. Mathematics provides a powerful, precise, and scalable language for making such measurements. These mathematically-described patterns then become the foundation for predictive models ('hypotheses'). Predictive models have two primary features. One feature is comprised of the conditions that determine what data are described by the model and, by implication, what data are *not* described. These conditions contribute to the definition of the discipline's 'parameters', as described in Section 1.1. The other feature of predictive models is their description of what happens *inside* of the data - the constituent elements and relations, however so defined, which *explain the detected* patterns. Some of the data are comprised of elements, while some of the data are comprised of relations *between* elements, and thus converge to comprise a *system* (Kuhn 2012[1962]).

The predictive properties of such a model lie in its definition of causality, i.e. the process by which a pattern *emerges* from observations. This is very different to constituting an *a priori* hypothesis in the terms construed by the folk model described above. In relation to the 'testing' phase of natural scientific research, the autochthonous notion of 'proving' and 'disproving' hypotheses are similarly unrelated to what actually occurs. What happens after a predictive model is built from measured observations is in fact a *search for isomorphism* between patterns in equivalently delimited data sets. This is why parameters are so important to the measurement process. Without them a theoretical model cannot be reapplied to search for patterns in new data, because it is impossible to know whether that data is of the same type as the data in which the pattern was originally detected. This reapplication involves a) collecting more data that fits the same parameters, and b) searching that data for the same kinds of patterns.

[17] This is consistent with enlightenment-era notions of a steady 'march of reason', that came to take on racist overtones during the historical overlap between the Industrial Revolution and Europe's colonial era, and whose echoes are evident in the thinking of foundational anthropologists Lewis Henry Morgan (1871) and Edward Burnett Tylor (1871)- see Chapter 2.

All of this comprises what Kuhn calls 'normal science', which, until the early 1960s, was the predominant autochthonous model among scientists of what science was (notwithstanding the folk models maintained by philosophers of science). Kuhn's clarifying contribution to the model was to point out that the development of scientific knowledge is actually comprised of two parallel processes: one 'normal', and one 'non-normal'. It is this non-normal process that Kuhn famously named 'revolutionary science'. In these terms, normal science is constituted by myriad acts of 'puzzle-solving' using already established theoretical models, or 'tools' as Kuhn also called them. Collections of such tools constitute what Kuhn called 'paradigms', the stable and accepted theoretical frameworks or tool*kits* that facilitate the normal scientific puzzle-solving prior to the occurrence of a scientific revolution. In Kuhn's account the existence of such a paradigm constitutes the prerequisite for a 'paradigm *shift*' - the collective movement by scientists away from the use of one set of theoretical models for predicting patterns, and towards the use of another set of such models. Once the paradigm shift or 'revolution' has occurred, and been consolidated, scientists in the relevant discipline may return to normal puzzle-solving under the conditions of the newly amended theoretical framework or 'toolkit'.

In Kuhn's depiction, the transition from normal science to revolutionary science is precipitated by what he calls a 'crisis' within the relevant discipline or disciplinary specialization. A crisis, in Kuhn's account, is characterised by an increasingly frequent detection of 'anomalies' in the puzzle-solving work of normal science. In this context, anomalies are those observations that cannot be explained by the terms and definitions of the existing paradigm. Anomalies are a permanent feature of normal science, however they usually occur at a consistently low level due to inherent features of the 'puzzle-solving' process, such as when an experiment is poorly constructed, data is corrupted, or findings misrepresented. Normal science is capable of dealing with such anomalies by a process of review and refinement of those factors. However, it is also this ongoing process of progressive refining that promulgates the likelihood of anomalies being detected.

> *"So long as the tools a paradigm supplies continue to prove capable of solving the problems it defines, science moves fastest and penetrates most deeply through confident employment of those tools. The reason is clear. As in manufacture so in science—retooling is an extravagance to be reserved for the occasion that demands it. The significance of crises is the indication they provide that an occasion for retooling has arrived" (Kuhn 2012[1962]:76).*

In Kuhn's account, periods of crisis also tend to involve an outbreak of insecurity within the discipline and a proliferation of suggestions about how to account for such anomalies within the limits of terms provided by the existing paradigm. Many of these suggestions are philosophical in origin, which is noteworthy because natural science usually 'holds philosophy at arms length', as Kuhn notes (2012[1962]:88). Indeed this is the one period in the puzzle-solving/revolutionary cycle in which hypothesis becomes an important part of the scientific process, as a formal means of articulating

speculative theoretical models in the absence of supporting observations. Although speculative, these hypotheses are not, however, 'invented' in the way that *a priori* propositions are invented. Rather, they are *reformulated* from parts of the existing paradigm (Kuhn 2012[1962]:84). It is during these periods of disciplinary instability that such amended models emerge with greater frequency, usually from researchers with minimal cultural capital invested in the discipline, either because they are young, or because their own cultural capital is invested outside of the discipline (2012[1962]:90; 144).

This is an extraordinarily incisive developmental model for a natural scientist to construct, given that it comprises a social scientific model incorporating both observed social behaviour (the behaviour of scientists), and a causal relation linking it to an autochthonous model of that same behaviour (how scientists think about scientific research). It is all the more extraordinary because, as outlined in Section 1.2.2 above, it corresponds very closely with Bernstein's account of pedagogic acquisition rules for hierarchical knowledge structures. Although Bernstein is interested primarily in the horizontal knowledge structure of social science and the humanities, not going in to the same level of detail as Kuhn with regard to natural science, he nevertheless points out that the development of hierarchical knowledge is not only "a function of the greater generality and integrating property of the knowledge" (Bernstein 1999:163), but also that:

> *"Opposition between theories in hierarchical knowledge structures is played out in attempts to refute positions where possible, or to incorporate them in more general propositions.* ***At some point, sometimes later than sooner, because of special investments, a choice is possible provided the issue can be settled by empirical procedures****" (Bernstein 1999:163 - emphasis added).*

Both Kuhn and Bernstein identify two features that distinguish natural science from social science. One is the relative absence or presence of paradigmatic frameworks that make normal 'puzzle-solving' research possible, via the progressively more sophisticated reapplication of models to new observations. The other is the *capacity* to build these paradigms, through terms and definitions sufficiently precise that they are able to identify parameters of the phenomena they are researching in the first instance, and in the second instance, the capacity to distinguish elements from relations, thereby identifying causality. Bernstein's analysis makes it clear that horizontal knowledge structures are divided between those with strong and weak grammar, such as in his examples, between linguistics and psychology on the one hand, and social anthropology and sociology on the other. But this analysis also makes clear that neither kind of humanities language is capable of the integration of which the natural sciences are capable, because of their hierarchical knowledge structure, and their distinctive rules for regulating the distribution and acquisition of knowledge (Bernstein 1999:163 - see APPENDIX 1 for extended quote).

This kind of competition is described by Kuhn as characteristic of a 'pre-paradigmatic' phase in the early development of natural scientific knowledge. It is distinct from what he calls the routine 'crisis' or 'revolutionary' phase in natural science because it does not occur against a background of normal scientific puzzle-solving. Rather it indicates a paucity of empirical specificity and formal theoretical modelling altogether, a paucity that in turn prevents social science from developing a coherent paradigm, and which traps it in a persistent pre-paradigmatic state (Kuhn 2012[1962]:178 - see APPENDIX 1 for extended quote).

It is clear that mainstream social anthropology does not meet the criteria for status as a science set by either Kuhn or Bernstein. Firstly, it openly dismisses the hierarchical integration of knowledge as a historicized 'meta-narrative' to which many of its practitioners are ideologically opposed (Harvey 1990:9). For Kuhn and Bernstein, hierarchical integration is the means by which natural science grows its explanatory power, so in these terms, as long as social anthropology rejects hierarchical integration , it also rejects the evolution of its own status as an expert discipline. The 'rejection-of-meta-narrative' stance also accords with Bernstein's account of anthropology's horizontal organization around discrete and competing languages, by treating various thematic phases and perspectives as fundamentally opposed and incapable of consilience. Secondly, many contemporary senior anthropologists still reject the past historical development of their discipline as a kind of ideological anomaly against which the discipline has now somehow inoculated itself (Eagleton in Harvey 1990:9. - see APPENDIX 1 for extended quote). Since the establishment of a paradigm depends upon disciplinary practitioners' "consensus about their past and present accomplishments" (Kuhn 2012[1962]:161), anthropologists' inability or unwillingness to reconciled the history of their discipline's development with its current form opposes them to such a consensus, and inhibits the emergence of a unifying paradigm (see Chapter 2 for elaboration). This scenario also matches Bernstein's characterisation of horizontal knowledge structures having no internal coherence. Thirdly, it is clear that anthropology is beset, as Kuhn puts it, by a "need [to] constantly to re-examine its first principles" (Kuhn 2012[1962]:163). This is evident in the great number of anthropological texts, both articles and monographs (including the current thesis) that commence with a discussion about 'what culture is' (see Chapters 2 and 3 for extended discussion, but see Ingold (2002) above for a more immediate example). This is also part of the perpetual redefinition of specialised language that Bernstein allocates to horizontal discourse. Fourthly, and most importantly, mainstream social anthropology clearly lacks a paradigm that allows normalised or institutional puzzle-solving to take place.

In an effort to reconcile its own position with the development of its discipline, the current thesis proposes that social anthropology *was* in the process of developing a paradigm during the first phase of its development during the 1870s-1960s, but that it lacked a sufficiently 'strong grammar' in Bernstein's terms, to steer that emergence towards a convergence of observations that would have allowed it to consolidate. The review of Bernstein (1996; 1999) provided above indicates that in the

horizontal knowledge structure of the social sciences, shifts in language are motivated by largely introspective or as he says 'narcissistic' imperatives (Bernstein 1999:163). Kuhn uses less emotive language, but identifies the same causal dynamic at work in stifling the emergence of vertically integrated, paradigmatic theory-making. Kuhn focuses on the social scientific habit of attempting to reject existing theories by 'falsification', i.e. deliberately seeking out 'counterinstances' (Kuhn 2012[1962]:77 - see APPENDIX 1 for extended quote). Kuhn's account begins to clarify the question of whether consilience was ever possible for the social science in general, and social anthropology in particular, and whether it may still be still possible. The current section has attempted to outline an answer by describing the preconditions for consilience as put forward by a number of authors.

The first part of this answer has been dealt with above, using the pedagogic models of Bernstein to explain how rules governing distribution and acquisition of social scientific knowledge must involve the destruction and replacement of an existing theory, rather than the subsumption of a limited language by a more expansive language. This is because "the set of languages which constitute any one horizontal knowledge structure are not translatable, ... make different and often opposing assumptions, with each language having its own criteria for legitimate texts, what counts as evidence and what counts as legitimate questions or a legitimate problematic" (Bernstein 1999:163). The second part of the answer has not yet been dealt with: "[T]hat the act of judgment that leads scientists to reject a previously accepted theory is always based upon ... the comparison of both old and new theories with nature" (Kuhn 2012[1962]:77).

Of course, as has been pointed out, social science studies human sociality rather than non-human nature, but the analysis holds true in both cases for the way social science currently 'evolves': Transitions from one social scientific theory to another over time do not involve cross-referenced assessments of either old theories *or* new theories with observable phenomenon. Rather, social science tends to 'validate' or 'invalidate' theories according to their inherent logics - the introspective logic of their own grammar. Because this grammar has historically been weak for mainstream social anthropology, the logic of each new theory generated by the discipline has been carried purely on it own rhetorical and ideological weight (Bernstein 1999:164 - see APPENDIX 1 for extended quote).

The competition for authoritative status as a speaker of some new language, rather than the cooperative refinement of puzzle-solving techniques, is thus what generates social science's relativistic attitude towards its own models, and its deductive postulations that the relation between 'signifiers' and 'signifieds' is arbitrary. 'Truth' for natural scientists has nothing to do with perspective, but everything to do with maximizing empirical observation, formalised representation, and congruent explanation. So how can social anthropological discourse transcend the limitations of the horizontal knowledge structure and weak grammar that limit it to a pre-paradigmatic state? In order to construct an answer to this question, and to position the current thesis within that answer, the following section highlights the flawed terms by which early social anthropologists attempted to cast

their discipline as a paradigmatic science, by emulating the idiomatic language of predominant natural sciences of the time.

1.3 The Failed Pre-Crisis Attempt at Vertical Integration

The previous section outlined the organization of anthropological discourse as one among a number of social scientific disciplines that exhibit a spectrum of relatively weak-to-strong grammars, but all of which are similarly constrained by a serial code that prevents their mutual integration or 'consilience'. The previous section also outlined how scientific discourse, by contrast, depends on the hierarchical integration of its various disciplines as the prerequisite for the emergence of unifying paradigms that facilitate puzzle-solving research within formally defined theoretical frameworks. The discussion of this distinction was directed at the abstract level of discursive organization.

In the current section, social anthropology is presented as a specific case of horizontal discourse with a distinct developmental history. The section highlights the point in this development when anthropology first incepted a specialised language of its own, termed 'structuralism', in an attempt to construct a paradigm of equivalent status to those of natural scientific disciplines. This language was given a name in order to distinguish it from previous, less explicitly formal frameworks. The inevitable failure of this exercise is shown to have been the result of a flawed model of science, drawn not from science itself, but from a philosophy of science known as 'positivism'. The broad ramification of this failure has been a sustained antithesis among some social anthropologists towards empiricism and formalism more generally, as discussed in detail in Chapter 2, based on a continuing conflation of philosophical positivism with actual science. Understanding the origins and dynamics of this historical event facilitates its distinction from the current project to develop a properly paradigmatic framework within which kinship network analysis and stKNA might be integrated.

1.3.1 Recognizing the Need for a Paradigm

> *"A discipline is defined according to its content: what it is that is under investigation. The study of life forms, for example, constitutes the discipline of biology; and such disciplines split up into taxonomies as their domains become more and more subdivided: first biology, then zoology, then invertebrate zoology, entomology and so on. A theme, on the other hand, is defined not by content but by aspect, perspective or point of view. Perhaps the earliest theme to emerge in European scholarship was mathematics. If I measure anything systematically I am doing mathematics; it does not matter what it is I am measuring. The 'content' of mathematics does not exist in the material world; it is created by the activity of mathematics itself and consists of ideal objects like numbers, square roots and triangles. In the nineteenth century, history became transformed into a theme, in the form of evolution: any phenomenon could be studied from the standpoint of how it evolved. The twentieth century then countered this with the theme of structuralism: any phenomenon studied from the complementary standpoint of how it is organized and held together" (Halliday 2001:176).*

The development of social anthropology is often accurately characterised as having started with a social Darwinist premise (see e.g. Wilson 2001), and as having been ideologically flawed from that point forward. The initial development of evolutionary theory in biology during the 1850s to 1870s corresponds with the emergence of social anthropology as a discipline, driven primarily by the British philosophers Lewis Henry Morgan and Edward Burnett Tylor. Initially these early investigators attempted to integrate their own nascent discourse using the terms provided by the dominant theme within biological science at the time, setting up the metaphor of a linear developmental vector for the 'evolution' of human social culture, marked by their own terms 'primitive', 'barbaric', and 'civilized'. However, the ideas used in the integration attempted by these authors, were weakly formulated and almost absent any empirical data. Although the early models of both biology and social anthropology relied on this linear metaphor to explain causes of perceived patterning - on the one hand variation among phylogeny of biota, and on other variation among the 'beliefs, laws, and customs' of human populations - they nascent disciplines differed completely in the modes by which they assembled supporting evidence.

Whereas the path-finding biologist Charles Darwin travelled the world making thousands of detailed observations of behaviour, morphology, and environmental context and then comparing these observations, Morgan and Tylor compared texts and extrapolated variations. Although they borrowed the theme of evolutionary biology, Morgan and Tylor (and their intellectual heirs), failed to recognize the incompatibility of their non-empirical, ad-hoc research modality with the empirically based and strongly formalised theories that they were attempting to mimic. Indeed, as will be discussed in Chapter 2 below, Morgan and Tylor in particular, along with numerous social scientists after them, reversed the relationship between theory and observation that had made evolutionary theory so powerful in explaining variation in biological speciation, but useless for explaining variation in human social culture. Whereas Darwin collected data, *induced* patterns in that data, and then developed a theoretical model for explaining those patterns, Morgan and Tylor appropriated the theory of evolution from an existing discipline, *deduced* a causal link with variation in human social culture, and then *selected* data to validate this deduction. The result was a model that had, within several decades, proven to be incompatible with increasingly diverse and detailed evidence from human populations in different parts of the world.

This incompatibility between data and theory is most evident in its implications for early anthropologists' grasp of evolutionary theory. Unlike the single selective vector, which Morgan and Tylor believed to drive social organization from a 'primitive' to 'civilized' state, biological evolution, as outlined in Section 1.1.2 above, is not described as linear at all, but comprised of multiple selective vectors, driving adaptive changes in both individuals and groups simultaneously, and in different directions, under different environmental conditions. Darwin's discovery that the resulting 'ultimate' selective vector resulted from a complex system of feedbacks in individual behaviour, group sociality, and environmental variation, was well beyond the theoretical capacity of either Tylor or Morgan, as

evidenced in their view that culturally distinct norms, such as kinship terminologies, were totally prescriptive in predicting the behaviour of associated populations. Even after the French sociologist Emile Durkheim made the ground-breaking theoretical distinction between 'mechanical' and 'organic' solidarity in 1893 (Durkheim 1893), as an analogy of the distinct effects of respectively strong and weak normativity of cultural convention, social anthropologists' commitment to a simplistic unilinear model of cultural evolution persisted until as late as the 1950s in some places, such as Australia (see Elkin 1954). Whereas biology had from the late 19th century integrated a sophisticated formal-empirical model of contextualised, multilevel adaptation into its explanation of variability in sociality within and between insect and animal species, social anthropology remained tethered to a linear model of human culture as comprised of more-or-less normative 'beliefs, laws, and customs', which resulted in predictable social structures of greater or lesser complexity.

> *"In studying the phenomena of knowledge and art, religion and mythology, law and custom, and the rest of the complex whole which we call Civilization, it is not enough to have in view the more advanced races, and to know their history so far as direct records have preserved it for us. The explanation of the state of things in which we live has often to be sought in the condition of rude and early tribes; and without a knowledge of this to guide us, we may miss the meaning even of familiar thoughts and practices. ...*
>
> *"The time for writing a systematic treatise on the subject does not seem yet to have come ; certainly nothing of the kind is attempted in the present series of essays, whose contents, somewhat miscellaneous as they are, scarcely come into contact with great part of the most important problems involved, such as the relation of the bodily characters of the various races, the question of their origin and descent, the development of morals, religion, law, and many others. The matters discussed have been chosen, not so much for their absolute importance, as because, while they are among the easiest and most inviting parts of the subject, ...*
>
> *"In the first place, when a general law can be inferred from a group of facts, the use of detailed history is very much superseded. When we see a magnet attract a piece of iron, having come by experience to the general law that magnets attract iron, we do not take the trouble to go into the history of the particular magnet in question. To some extent this direct reference to general laws may be made in the study of Civilization" (Tylor 1871:3).*

This early organization of social anthropological discourse, as one based on the 'borrowing' of themes from other explanatory discourses, while simultaneously failing to integrate its research modality, presaged the increasing divergence of what have come to be recognised as vertical and horizontal knowledge structures in expert languages. As Halliday (2001:176) points out, by the beginning of the 20th century, the theme of historicism had begun to be replaced by the theme of structuralism. Between Durkheim's seminal research in the 1890s, and the establishment of the first anthropology departments during the 1920s and 1930 (Radcliffe-Brown established Australia's first department at

the University of Sydney in 1925), structuralism was the predominant 'aspect' for explaining human sociality. In the terms provided by Bernstein, outlined in Section 1.2.1 above, it was also the first horizontal 'shift' to a specialized language for anthropology. This is because, while the inaugural anthropological theme of historicism had been entirely analogic in its borrowing from biology, the theme of structuralism was an independent production of the new social science, constructed not with analogies to non-human systems, but with more abstract and completely free-standing metaphors of 'parts', 'wholes', 'processes', and 'functions'. Although the new wave of second-generation anthropologists, such as the European-Americans Franz Boas and Bronislaw Malinowski, British Radcliff-Brown, and French Lévi-Strauss, professed a commitment to empirical data and formal modelling, this was undermined by the weak grammar of their newly constructed specialised language, and their own weak comprehension of natural scientific language. As the lead proponent of new theme of structuralism, Radcliffe-Brown provides some of the most incisive examples of this unfamiliarity:

> *"My view of natural science is that it is the systematic investigation of the structure of the universe as it is revealed to us through our senses. There are certain important separate branches of science, each of which deals with a certain class or kind of structures, the aim being to discover the characteristics of all structures of that kind. So atomic physics deals with the structure of atoms, chemistry with the structure of molecules, crystallography and colloidal chemistry with the structure of crystals and colloids, and anatomy and physiology with the structures of organisms. There is, therefore, I suggest, place for a branch of natural science which will have for its task the discovery of the general characteristics of those social structures of which the component units are human beings" (Radcliffe-Brown 1940:2).*

But why is the *structure* of the universe any more important to natural science than the *development* of the universe, or its dynamics, or indeed any other aspect of its existence? 'Structure', in this context, is a theoretical assumption - a positivist proposition deduced not from a properly scientific analysis of measured data, but from a misunderstanding of physics, chemistry and biology - defined not by their clearly demarcated objects of investigation, but by individual themes *within the parameters* of that investigation. This is particularly unfortunate, since in taking the form of an ode, this original misrepresentation of science defined the manner in which the speakers of the next anthropological language would attempt to overthrow it, in Bernstein's terms, by attacking science as a mere theme, just as Radcliffe-Brown had presented it, rather than as a hierarchically organised knowledge structure with discrete disciplines. Indeed this unintentional positivist misconstrual is tody routinely presented by social anthropologists as the reason that natural science should be considered incompatible with social science, a full 70 years after Radcliffe-Brown's enthusiastic confusion.

In the same 1940 text Radcliffe-Brown goes on:

> *"Science (as distinguished from history or biography) is not concerned with the particular, the unique, but only with the general, with kinds, with events which recur. The actual relations of Tom, Dick and Harry or the behaviour of Jack and Jill may go down in our field note-books and may provide illustrations for a general description. But what we need for scientific purposes is an account of the form of the structure. For example, if in an Australian tribe I observe in a number of instances the behaviour towards one another of persons who stand in the relation of mother's brother and sister's son, it is in order that I may be able to record as precisely as possible the general or normal form of this relationship, abstracted from the variations of particular instances, though taking account of those variations" (Radcliffe-Brown 1940:4).*

Again, this characterization of science is wrong. In fact, as detailed in Sections 1.1 and 1.2 above, science places exactly the inverse emphasis on generalisations and particulars, by *first* making carefully measured observations of particular, unique phenomena, with the attributes of all elements and process in those phenomena recorded as specifically and accurately as possible, and *then* by a search for recurring patterns in those phenomena. Only *after* a pattern has been detected, can it be generalised sufficiently, in the form of a theoretical model, that further attempts can be made to detect it in other particular and unique phenomena. Once the pattern, in the form of a theoretical model, has been *shown* to exist in multiple sets of unique data sets, by *collection* and *publication* of that data, consistently defined by the parameters of the discipline, may the theoretical model be presented in the generalised form to which Radcliffe-Brown refers. Contrary to being 'unconcerned with the particular', science is fundamentally concerned with the particular, since it is the particular in which patterns either do or do not exist. To "record as precisely as possible the *general or normal form*" is a self-contradiction, since a) it is only particular instances that anthropologists can ever record (since generalisations exist only in anthropologists' models), and; b) recording only that data deemed 'general or normal' by the anthropologist requires the deliberate *exclusion* of data that the anthropologist does not *consider* 'normal'. A natural scientist engaged in such conduct to 'abstract from the variations of particular instances', suggests that Radcliffe-Brown thought that scientific models somehow 'smoothed out' inconsistencies in particular data, presumably by ignoring them. As the summary of Kuhn's account showed above, 'particular variation' is not something that can or should be 'abstracted' out of observed data, but rather forms the very basis on which scientific models are built, by detecting variation, so that models can be amended and improved to account for that variation.

Thus structuralism, as anthropology's first independently developed, specialised language, in Bernstein's terms, was not only 'non-paradigmatic' in Kuhn's terms, and 'thematic' in Halliday's terms, comprised of weakly constructed grammar, unintegrated with its preceding language or with adjacent languages, it also *misconstrued* its own objective. As such, it was bound to fail as a mission to establish an empirically based, formally defined paradigm for anthropology.

1.3.2 Conflating 'Theme' with 'Paradigm'

For Leaf and Read (Leaf 2007; Read 2008; Leaf & Read 2012) this historical confusion, evident in the pronouncements of structuralism's chief advocates, has its origins in Aristotelian logic, which they suggest was abandoned by natural science at the beginning of the industrial revolution. Leaf and Read propose that social science has been saddled with versions of this confusion since the early 19th century, via a the influence of a series of British moral philosophers spanning John Locke, Thomas Hobbes, and finally Auguste Comte and John Stewart Mill. According to Leaf (2007), it was Mill who laid down the terms and definitions of logical positivism in the mid-18th century, that later social scientists inherited and interpreted as the official definition of natural science:

> *"Mill did not describe science as a system of experimental knowledge but as a system of 'propositions.' Propositions 'name' things and relations between things, and truth or falsity can be assigned to them. This top-down, deductivist, view of science is astoundingly wrong-headed when taken as any sort of close description of what scientists actually do. Logicians may sit about assigning truth or falsity to propositions in Mill's sense, but scientists certainly do not. A fortiori, they do not deal with relations between* ***propositions****. Scientists deal with observations and relations between* ***observations****, preferably experimentally clear observations. And observations, per se, are not true or false. They are what they are; the problem is to understand them. Such observations cannot be reduced to propositions or any other single form of expression, any more than a three dimensional object can be reduced to a two dimensional representation or a process over time reduced to a static picture" (Leaf 2007:1 - emphasis added).*

Leaf's 'reduction to single form or expression' correlates with Halliday's definition of a theme, as distinct to a discipline (Halliday 2001). The development of the specialized languages of 'historicism', and later 'structuralism' comprised just such reduction. According to Leaf and Read, Comte's and Mill's conception of logical positivism was directly adopted by anthropology's two key founders, . Morgan and Tylor, and from there, by Radcliffe-Brown, and later Lévi-Strauss. In 1966 Lévi-Strauss summarised his aspiration towards social anthropology's integration with the natural sciences this way:

> *"There has been much question lately as to whether anthropology belongs among the humanities or among the natural sciences. In my opinion, this is a false problem: anthropology is unique in not lending itself to such a distinction. It has the same subject matter as history, but for lack of time perspective cannot use the same methods. Its own methods tend rather towards those of sciences also synchronically oriented but not devoted to the study of man. As in every other scientific undertaking,* ***these methods aim at discovering invariant properties beneath the apparent particularity and diversity of the observed phenomena****" (Lévi-Strauss 1966b:126)*

And:

> *"According to the always marvellously clear views of the English master [Radcliffe-Brown], social anthropology is to be **an inductive science** which, like other sciences of this type, observes facts, formulates hypotheses, and submits these to the control of the experiment, **in order to discover general laws of nature and society**" (Lévi-Strauss 1966a:115).*

Lévi-Strauss here improves on Radcliffe-Brown's account, by correctly characterising the inductive logic of normal scientific puzzle-solving, but the problem remains that neither Radcliffe-Brown nor Lévi-Strauss 'observed facts' in the way that scientists 'observe facts'. Indeed, social anthropologists did, and still do, 'make notes' in their field journals, but they do not do so in a way that any scientist would consider scientific. These notes tend to be hand-written, often unintelligible, ad-hoc, without consistent parameters, definitions, or format. The result is that they cannot be formalised in any way that would render them amenable to independent measurement. That Radcliffe-Brown and Lévi-Strauss considered such field methods a sufficient basis for attempting scientific research lends further weight to the impression that they did not understand how science works, and further indicates, in retrospect, that their undertaking was doomed from the outset. More significantly, it reinforces the definition that Halliday (2001) gives of structuralism as a perspectival theme, quite distinct from a scientific paradigm that could facilitate puzzle-solving of the kinds described by Kuhn (2012[1962]). Radcliffe-Brown himself recognised this:

> *"I am aware, of course, that the term 'social structure' is used in a number of different senses, some of them very vague. This is unfortunately true of many other terms commonly used by anthropologists. The choice of terms and their definitions is a matter of scientific convenience, but one of the characteristics of a science as soon as it has passed the first formative period is the existence of technical terms which are used in the same precise meaning by all the students of that science. By this test, I regret to say, social anthropology reveals itself as not yet a formed science. **One has therefore to select for oneself, for certain terms, definitions which seem to be the most convenient for the purposes of scientific analysis** " (Radcliffe-Brown 1940:3 - emphasis added).*

This is precisely the kind of proposition-based logic that Leaf describes as characterising Mill's positivism, in which *a priori* propositions name things, so that they can assigned truth or falsity (Leaf *2007 - see quote above*). Contrary to Radcliffe-Brown's characterization, scientists do no *select* definitions, but rather *discover* them in the patterns revealed by the measurement of data. This is what makes such definitions amenable to independent testing by further observation, measurement, and duplication. Such definitions are therefore anything *but* convenient, as Kuhn explains in the way that anomalies accumulate in the puzzle-solving work of existing scientific paradigms, until they reach a critical crisis-inducing mass, resolvable only by the 'revolutionary' amendment of the paradigm.

No expert discipline, whether social anthropology, physics, chemistry, biology, or any other, can be defined any more by 'structure' than by 'history', 'function', 'process', or any other single thematic aspect. This is because, as Halliday (2001) points out, disciplines are defined by the *subject* matter that falls within their agreed parameters. If these parameters cannot be defined then the discipline has no subject, and associated themes or perspectives take on the illusion of comprising the subject itself. Thus 'social structure', although only one perspective on human sociality, appeared to Radcliffe-Brown, Lévi-Strauss, and many others, to comprise the *total subject* of their research, even though they were aware that it could not be properly defined. So too, 'functionalism' which was often suffixed onto social structure as 'structural-functionalism' between the 1930s and 1960s, 'post-structuralism', 'post-modernism', 'action theory', 'game theory', 'phenomenology', and any other number of specialised idioms developed, not to integrate with existing idioms and discoveries, but to replace them, as Bernstein explains (1999).

So what about the themes of empiricism and formalism, on which natural science has been shown to rely so heavily? The difference is that empiricism and formalism, like mathematics in the terms given by Halliday (1996), has no content. Empiricism and formalism do not prescribe an outcome like 'structure' or 'evolution', but instead impose metric limitations on the kinds of procedures that can be used to conduct research, that may in turn yield any kind of discovery, so long as it is *measurable* and *demonstrable*. Furthermore, by conducting research using empirical data and formal models, the definition of the discipline's parameters is made at once more specific with regard to the object of investigation, and more expansive with regard to its relative, integrated position among adjacent disciplines. Read (2008) describes the importance of formal measurement in observing patterns of both social activity and ideas, arguing that it is only from such formal measurement that coherent theoretical models of the relationship between the two can be developed:

> *"A good, formal representation makes it possible to determine (mathematically) the logical consequence of hypothesized structuring processes. Those processes become explanatory for patterning found in empirical observations when we find agreement between their consequences and patterning determined through empirical observations. What constitutes patterning for social systems is complicated, though, by the way behavior is framed through cultural idea systems. Behavior is neither driven exclusively by cultural idea systems nor are cultural idea systems simply a codification of already existing behaviors. Instead, there is a complex, time-dependent interplay between action and concepts involved in action that needs to be formally represented using both models that are the consequence of the posited structuring processes and models that represent the complex pattern that arises from behavior interfacing with cultural idea systems" (Read 2008:1).*

This section has outlined the conflation of perspectival themes with disciplinary paradigms, which occurred during anthropology's pre-crisis era during the late 19th and early 20th centuries. An

explicit desire on the part of leading anthropologists such as Lévi-Strauss and Radcliffe-Brown, to integrate the language of their own discipline into those of the natural sciences, was undermined by a misunderstanding of natural science's formal modality at a general level, and the formal structure of expert languages at a specific level. The next section adds to these two components of the conflation a third component - the absence of empiricism.

1.3.3 The Absence of Empiricism

As established above, empiricism is a mode of both defining an object of observation, and of measuring that object. For social anthropology to make the shift to a paradigmatic state, based on a formal empirical modality, the question that arises most immediately is, '*What* is to be measured?' This open-ended question can be specified by a more context-dependent reformulation: 'What *can* be measured?' As indicated at the beginning of the chapter, culture is often characterized by social anthropologists as impossible to measure because, they say, it is 'ethereal', existing only 'in people's minds'. However this perspective only makes sense if one accepts that culture is perpetually un-instantiated - that it is exists in a permanent state of potential that is never fully materially implemented. The position taken up in this chapter, elaborated in Chapters 2 and 3, and distilled in Chapters 4 and 5, is that all human activity is composed of instances of individual decision-making. These are the result of complex sequences of choices selected from ranges of options that are apparent to each person involved in the process of interacting with one another, and consequently bringing collective achievements into being. Whether those things are buildings, roads, clinics, schools, governments, businesses, families, or weekend picnics, they are all the product of conscious decision-making, and they are all measurable. The technological capacity for measuring the systematic organization of these choices determines both *what* can be measured, and the *extent* to which it can be measured.

If, as was elaborated in Sections 1.1 and 1.2, social anthropology is to be defined as the study of human social culture, then the *subject* of measurement is clearly social interaction, and the *object* of measurement are relationships between people. More specifically, a subject defined as social culture has no objective expression except in the interdependent interactions of multiple individuals who are real and specific. Whereas a subject *material* culture comprises objectively measurable *material* traces (as studied by the field of archaeology), a subject *social* culture comprises objectively measurable *relationships*. Under this rubric, all social subjects are comprised of measurable and objective relationships. If, to elaborate the distinction by hypothetical example, a social anthropological study focuses on a material trace, such as the building of a road, then the broader subject of measurement are those social interactions which gave rise to the construction of the road: *Which people* interacted with each others, via *what kinds of relationship*, to bring the road into existence? Just as Leaf shows that a proposition cannot be measured, the starting point for such an empirical exercise cannot be the *idea* of the road, but the *fact* of the road. A population may well

have ideas about road building but these cannot be measured without instantiated examples with which to compare them. The road itself forms the parameters of the study from which measurement begins, and works backwards in an exploratory manner. Starting with instantiation, the basic questions are: Who built the road? Which direct relationships motivated those people to build it? What kinds of relationship were they? What patterns do these relationships exhibit? Moving on to autochthonous models - to the systems of ideas maintained by the people involved - the questions are: What was the full range of perceptible choices available to the people in the formation of these relationships? How were these relationship-forming choices organised? Finally, moving on to causality, the question is: What measurable isomorphism exists between the patterns apparent in the autochthonous models of choice that were apparent to people involved in forming relationships, and patterns apparent in the instantiation of those choices? Put another way: Of the social choices that were available in the people's model of road building, which choices were actually made that led to the construction of the road? All of these questions, and the order in which they need be asked, are the same for any human-generated phenomenon. The causal properties of the system are discovered by working backwards from the end result.

1.4 Conclusion

This chapter has presented the parameters of a social anthropological paradigm delimited by the measurable object of study that comprises its subject: Social relations. Any questions that fall outside of these parameters are not social anthropological questions, but questions of semiotics, or psychology, ethnography, or of some other social scientific discipline. This approach is not a 'perspective' on social activity because it does not prescribe any particular theme, or any particular outcome. It does not make predictions about what may be discovered, whether structure, function, process, identity, or any other attribute. With regard the modality of natural science, one of the key points made by Kuhn (2012[1962]) is that new investigative technologies not only facilitate scientific discovery, but *induce* them, by first detecting hitherto unknown information, and then revealing more and more specific levels of detail in that information. These are levels of specificity that were, for reasons of scale, beyond the perception of investigators prior to the technological innovation. The more detail is revealed, the more complex the system becomes, incorporating more elements, more relations, and more variation, and the more coherent the theory of the subject. Even a phenomenon as intuitively intelligible as the construction of a road may incorporate thousands of relationships between hundreds of people, if the investigative process works sufficiently far back through interlinked sequences of decision-making to detect them. It is such complexity that has been misconstrued by social anthropology as beyond the realm of autochthonous comprehension - a complexity that cannot possibly be generated by the theories of the people engaged in the subject process. This deductivist logic, in the absence of empirical data, leads to the conclusion that such complexity should also be beyond the scope of social anthropology itself. Social anthropology assumes that if the autochthonous model does not account for measurable patterns in the data, then those patterns are exogenous to the culture under investigation. In the absence of formally patterned empirical data, theoretical models become detached from the relevant objects of study and evolve independently within the academy, as Bernstein (1996; 1999) observes. Since its inception in the 1870s, social anthropology has tended to work in this way: *Starting with a model* comprised of propositions, *assuming* the model to be maximally normative in prescribing a single option, and then *seeking* to validate or falsify the model by seeking out, selectively, examples of the model's manifestation in people's behaviour.

This chapter has presented the thesis' foundation in three parts. Firstly, Section 1.1 described the parameters of social anthropology as corresponding with a field on a spectrum of knowledge dealing with human sociality, with adjacent fields associated with the disciplines of biology, psychology, and semeiotics, along with various subfields and more closely related disciplines. This section further described a lack of vertical integration between social anthropology and these other disciplines, and introduced the principle of 'consilience' as a precondition for the viability of the stKNA method. Secondly, Section 1.2 described the specific components of the current non-integration between social anthropology and its adjacent disciplines in terms of inherent differences between the

modalities and languages of natural science and social science. This description specified the changes that need to occur within the discipline of social anthropology in order to accommodated formal empirical studies such as the one presented in the latter part of the current thesis. Thirdly and finally, Section 1.3 introduced a brief summary of the origins of the current non-integration between social anthropology and natural science more broadly. This summary traced the initial attempt at integration in the late 19th and early 20th centuries, which ultimately failed due to a lack of clarity among leading anthropologists of the time as to what such integration would require.

The next chapter proceeds from this brief summary of origins, to a more elaborate and detailed discussion of lingering consequences for the so-called 'post-structural' models of contemporary anthropological theory. Attention is given in particular to social anthropological research of Australian populations, which the discipline has persistently held up as exemplary illustrations of its founding principles. These are principles, which in the absence of empirical data and coherently formalised modelling, have taken the peculiar form of tacit group theoretic assumptions. The consequences of these assumptions have not only lingered, but have also suppressed the potential for both puzzle-solving and revolutionary breakthroughs in the discipline.

Chapter 2

Group Theory in Anthropological Models of Indigenous Australian Culture

2.0 Introduction

The previous chapter outlined the discursive basis on which the phenomenon of human sociality is construed by social science generally, and by social anthropology in particular. This construal was shown to be based on a serially organised, unintegrated set of discrete languages, located on a spectrum of strong-to-weak grammar. Of the most relevant discrete social science disciplines, including psychology and semiotics, social anthropology was shown to be constituted by the weakest grammar, similarly to humanities disciplines such as art and literature (Bernstein 1996; 1999; Halliday 2003; 2005). The previous chapter further outlined the history of social anthropology's divergence from the natural sciences, resulting from early practitioners' misconstrual of the empirical and formal basis of natural science (Wilson 2001; Kuhn 2012[1962]; Richerson & Boyd 2005). Despite the, non-empirical and weakly formal language of the discipline, it was shown that the subject matter of social anthropology is nevertheless distinguished from adjacent disciplines by its focus on the causal links between patterns of human social organization and autochthonous models *of* that social organization, giving rise to the nexus called 'social culture'. As discussed further in Chapter 1, human social culture was shown to be distinguished from social culture in other biota by its composition in a system of feedbacks between observable instances of social interaction, and systematic *ideas about* that interaction among society members, manifest as realizable choices apparent to each person. White (2011) characterises this system of feedback in terms of the so-called 'three-world problem', while Leaf and Read (2012) characterise it in terms of the 'generative' properties 'idea systems'. While the these insights from anthropological perspectives provide helpful heuristic frameworks, semioticians and linguists have provided a more specific model, by formally describing the way that languages bridges the functional transition from cognition, to the construction of metaphors for realizing that cognition in terms of ideas. It is this capacity for constructing metaphors, and assembling them into self-consistent abstract systems of ideas, that allows communities of people to develop models of their own sociality. The previous chapter concluded by illustrating how, using an accurate implementation of empirical-formal terms and definitions, the language of social anthropology might be re-organised around a hierarchical knowledge structure, capable of recognising both social organization, the idea systems that people maintain about that social organization, and the causal links between the two.

The current chapter describes the origin and development of the predominant theoretical framework that most specialised anthropological languages use for construing patterns of social behaviour, and the often tacit but highly deterministic conditions that it imposes on the consequent construal of causes driving that patterning. Following the discussion in Chapter 1, the definition of sociality and causality used here is covered by convergent explanations of both semiotics and empirical-formal anthropology as follows: From the micro-level of individual intentions, causality is sequentially organised. An individual person must first intend an effect, and then act in order to finally induce that effect. This axiom applies across a spectrum of 'control', ranging between the passive and agentive,

and thus holds irrespective of the extent to which an individual perceives choice in their intention (Halliday & Mattiessen 2006; Rose 2007; Thibault 1993). From the macro-level of an entire social system, causality is expressed by the patterned links between elements, comprising a nominally complete assembly of all possible or already actioned intentions (White 2011; White & Johansen 2005; Leaf 2008; Read 2007). For the purposes of exploratory analysis, the influence of micro-level choices may be synthesized in a macro-level system by testing, in an exploratory sense, the effects on the system of changing values for certain elements, such as by removing or adding certain elements, by increasing or reducing the quantities or strengths of certain elements, or by re-ordering them, and then measuring the response of other variables in the system. However, the total causal *potential* of the system lies in its integrity as a whole. This is how formal models of human social organization, by definition, contain information about the influence of both micro-level decision-making and macro-level conditions generated by the accumulation of those decisions. At the same time, the explanatory *scope* of such models is a function of the primary definitions that delimit *which* data is included in the model, and *how* the links between that data are defined.

The current chapter traces the emergence and persistence of axiomatic principle describing the configuration of human social relations as 'groups'. Because the term 'group' colloquially captures the macro-level idea of multiple people interacting as a collective, it features heavily in anthropological texts, but with little or no explicit definition. This chapter illustrates the persistent but tacit reference to a set of axiomatic principles that meet the formal mathematical definition of what is called a 'permutation group'. Section 2.1 outlines the effects of these axioms on models of sociality and causality, and illustrates how they are often rendered tacitly in informal anthropological texts. Section 2.2 recounts the establishment of these axioms in the foundational 19th century anthropological texts of Henry Lewis Morgan and Edward Burnett Tylor, and their persistence in contemporary anthropology. Section 2.3 outlines the emergence and consolidation in the pre-crisis era, and persistence in the crisis era, of a localised variation of group theoretic axioms in models of Indigenous Australian sociality. It is suggested here that these same underlying axioms formed the inspiration for, and continue to be sustained by, the evidentiary framework of the Northern Territory Land Rights Act (1976) and the National Native Title Act (1993).

2.1 Sociality and Causality in Group Models

In Chapter 1, Leaf and Read's (Leaf 2007; Read 2008; Leaf & Read 2012) model of empirical formalism was introduced and contextualized by Bernstein's (1996; 1999) distinction between strong and weak grammars in horizontal knowledge structures, and by Bernstein's further distinction between the horizontal knowledge structure of social science, and the vertical knowledge structure of natural science. In the current section I further specify the role and position of Leaf & Read's empiricism and formalism by distinguishing one from the other. Leaf (2007) approaches this distinction between formalism and empiricism with a description of '*un*empirical formalism', but does not elaborate on further implications for how this variation may be construed in anthropological models. To take this analysis further than Leaf we might treat empiricism and formalism as distinct attributes of anthropological models, each of which varies along a spectrum between 'minimal' and 'maximal' values. It this way, we can conceive of them as independent spectrums. While the most broadly applicable antonyms for 'empirical' and 'formal' language might be '*un*empirical' and '*in*formal' language, these spectrums have more specific manifestations in the specialised language of anthropology with regard to data-collection and theory-making, as illustrated below. In the case of data, much of social anthropology does not recognise it all, but refers instead to anecdotes - collections of disconnected, selective observations with no explicitly defined parameters. In the case of theoretical modelling it would be similarly imprecise to describe much of it as simply 'informal'. Much of the theoretical modelling that occurs in anthropological texts is not based on the construal of causal links between observations at all, but rather on comparisons of ad hoc collections of narratives. In these terms, if a spectrum of empiricism extends to a maximal value of 'empirical' at one end, then the other end extends to 'anecdotal recollection' (see Figure 6). Similarly, if a spectrum of formalism extends to a maximal value of 'formal' at one end, the other end extends to 'ad hoc narrative' (see Figure **7**).



Figure 6: A spectral construal of empiricism in theoretical models (adapted from Leaf 2007; Leaf & Read 2012)



Figure 7: A spectral construal of formalism in theoretical models (adapted from Leaf 2007; Leaf & Read 2012)

Accepting the analyses of Leaf & Read (2012), and of Bernstein (1996; 1999), that all modelling contains some degree of both empiricism and formalism, the two spectrums may be recombined as

axes defining a plane within which anthropological models are distributed according to four relative characteristics (see Figure 8):

a) Formal models backed by empirical data;
b) Formal models backed by anecdote;
c) Ad hoc models backed by empirical data;
d) Ad hoc models backed by anecdote.

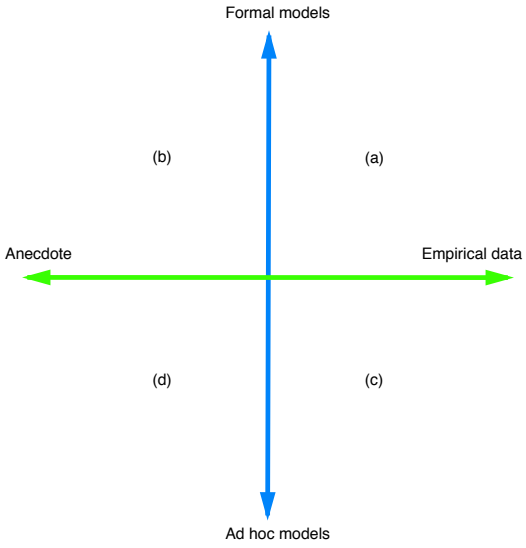


Figure 8: A superimposition of distinguished spectrums, elaborating on Leaf's (2007) identification of 'unempirical formalism'.

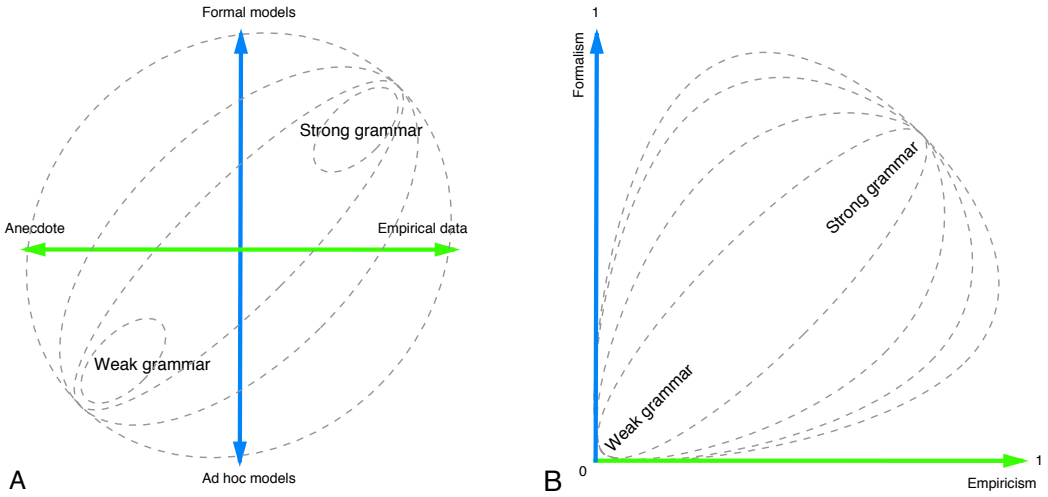


Figure 9: Two graphs construing Bernstein's strong & weak grammar as functions of empirical and formal metrics. Graph (A) extends the visualisation of two superimposed spectrums, while graph (B) introduces a normalized metric ranging from 0 to 1, where 1 represents (an impossible) maximum empiricism and formalism.

The relative strength or weakness of a model's grammar (the grammar of the language used to build the model) appears on this plane as a field (see Figure 9A). Weak grammar represents the maximal

convergent values of anecdotal recollection and ad hoc narrative, while strong grammar represents the maximal convergent values of formal modelling and empirical data. Figure 9B re-construes these field values as ranging along a metric scale from 0 to 1, where 0 represents a total absence of each value. Note that the field does not extent to maximal values for either formalism or empiricism. This communicates the impossibility of a model ever capturing *all* data on a phenomenon. Similarly, no model can ever be completely formal since, as White (1974; 2008) points out, the more axiomatic the 'rules' of a model, the less able it is to capture variability in the data. As the two values of the field approach their joint maximum, the field begins to fold back onto itself, creating a blunt outer edge. Just as 'the terrain can never be the map' (White & Jorion 1996), so too can the model never actually converge with reality.

The manner in which anthropological models construe sociality and causality lies quite apart from their relative empiricism and formalism. Following Halliday (2001; 2005) and Halliday & Matthiessen (2006), the *way* that sociality and causality are construed in a model is determined by the ways in which the model defines an *instance* of social interaction relative to all available options, where such instances comprise the object of measurement. The thesis advanced here is that a model may define sociality *explicitly* in terms of people, or *implicitly* in terms of personal attribute. This distinction between definitions of sociality has the effect of ordering it along a spectrum ranging between two polar 'types': one deriving from dynamic *interaction* between pairs of people, and one deriving from synoptic *comparison* between pairs of attribute. In the semiotic terms provided by Halliday and Matthiessen (2006:235) this spectrum ranges between 'congruent' and 'metaphorical' construal (see Figure 10).

A congruent construal has a clear order, such that < $Person\ A \xrightarrow{interacts\ with} Person\ B$>, as in for example 'Person A employs Person B'. By contrast, a metaphorical construal has no such order, such that <$Attibute\ A \neq Attribute\ B$>, as in for example 'Person A is an employ*er* but Person B is an employ*ee*'. In the former case, the cause is an unfolding process, and the effect is an exchange between agents, whereas in the latter case the cause is a static condition existing in perpetuity, and the effect is a classificatory object 'beyond' the realm of specific people, and their particular relationships with one another.

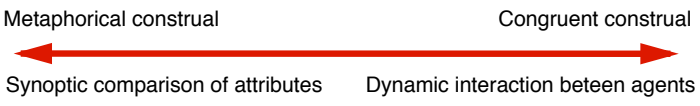


Figure 10: Congruent construal of sociality in theoretical models (adapted from Halliday & Matthiessen 2006).

The relative empiricism and formalism of these two types of construal - or in Bernstein's terms the relative 'strength' of the grammar in which they are construed - is not related to a model's relative congruence. Both formal and informal models, exhibiting respectively strong and weak grammars,

may equally be either congruent or metaphorical. This allows us to construe the spectrum between metaphor and congruence as an additional axis on the graph at Figure 9. When the three spectrums that are used to create this space are treated as metric orders, the relative positions of anthropological models can be visualised as vectors within the space. Figure 11 charts potential relative positions along each metric.

Whereas the relative strength of empirical and formal models occurs on a single plane created by distinct values (as graphed at Figure 9b), the integration of the third metric of congruence gives this measurement a further explanatory 'depth', generating a 3-dimenionsal metric space, illustrated at Figure 12. The interaction of congruence and empiricism generates a measurement of explicit sociality, while the intersection of congruence and formalism generates a measurement for explicit causality. A purely formal and empirical model, which is minimally congruent with cognized reality, may be described as exhibiting a maximally strong grammar, but it has minimal descriptive power as a model of human sociality and minimal explanatory power as a model of the causes of that sociality. The more congruent a model, the more explicit, and thus more powerful its descriptions and explanations. This is quite apart from what Leaf & Read (2012) describe as the 'coherence' or 'self-consistency' of a theoretical model.

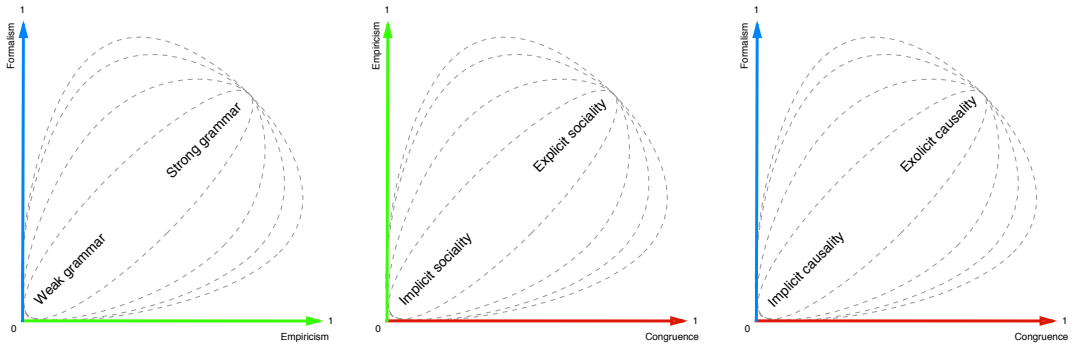


Figure 11: Three planes of intersecting order. The intersecting order of formalism and empiricism (left) yields degrees of weak-to-strong grammar (as per Leaf & Read 2012, and Bernstein 1996; 1999); the intersecting order of Empiricism and congruence (middle) yields explicit, powerfully descriptive models of sociality (as per Bernstein 1996; 1999, and Halliday & Matthiessen 2006); the intersecting order of formalism and congruence yields explicit (powerfully explanatory) models of causality (Bernstein 1999; Halliday & Matthiessen 2006).

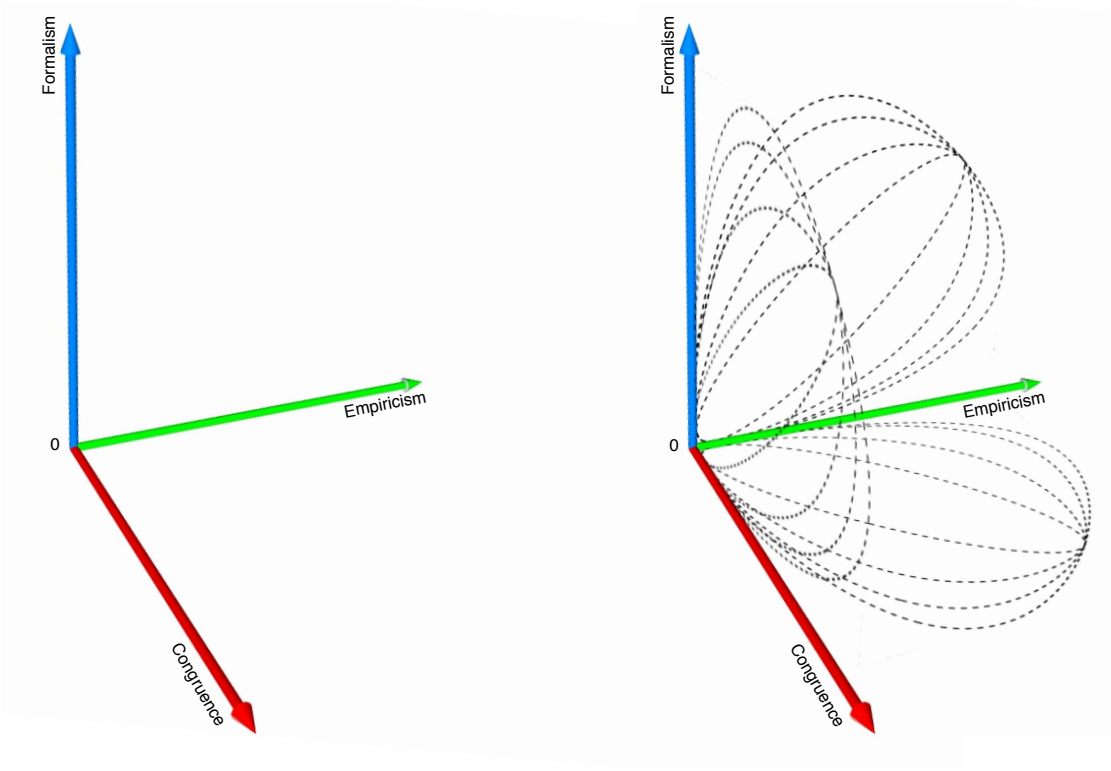


Figure 12: Intersecting measures empiricism, formalism, and congruence construed as a three-dimensional vector space. The space is shown empty (at left), and incorporating field patterns (at right) to illustrate relative possible positions on each intersecting plane.

To provide a hypothertical illustration: In the case that a given model (a) is highly empirical and formal, it may be based on large, clearly defined sets of data, represented using diagrams and standardised mathematical notation, but nevertheless use metaphorical construals of social interaction. Examples include models in which processes of interpersonal interaction are represented as statistical 'structures', such as in standard demography where populations are represented by proportional counts of individuals bearing different types of attribute, such as age range, income and education levels. Such a model may exhibit a high degree of self-consistency, but remain both weakly descriptive, and weakly explanatory. Alternatively, another model (b) might be both minimally empirical, and minimally formal, based on selective personalised anecdotes, and demonstrated with valorised ad hoc examples. It may nevertheless remain strongly congruent with processes of social interaction. Examples include narrative recounting of personal experiences and feelings, where the narrator asserts intimate familiarity with, or even *assumes*, the voice or perspective of other participants in the process. A third model (c) may be supported by a wealth of empirical data, be highly congruent due a close resemblance of that data to intuitive experience, but exhibit no formal self-consistency. Such a model is powerfully descriptive but does not explain anything. Such a model includes observation-based survey results, where the model consist of lists of information recognisable to participants in their original context, but where there are no causal links between units of information, such as between different responses to a common a survey question. Finally, a

fourth model (d) may be highly formal, highly congruent, but lack empirical data. In this case the model is both self-consistent and powerfully descriptive of intuitive reality, yet without demonstrable evidence of a factual basis. Such a model might take the form of a prescription for ceremonial 'rules', such as might be found in a religious ritual, where there are clear steps in a process unfolding in space and time, with specific roles and relations for participants, but where no actual evidence is presented for the ritual having occurred.

The current chapter suggests that relatively metaphorical construals can be (and typically *are*) embedded in both empirical formal and ad hoc and anecdotal models that social anthropologists use to construe sociality and causality. This tendency towards metaphor, based on anthropologists' comparison of their own classifications personal attribute, rather than on measurements of the relations between people, as described by the people themselves, has the dual effects of both limiting the explanatory power of social scientific analysis, and disguising the reasons for that limitation behind anthropologists' own autochthonous idea systems.

Historically, one of the greatest drawbacks of formalisations in social anthropology has been advocates' conviction that by imposing strongly axiomatic 'laws' or 'rules' on models of social organization, in the absence of parallel considerations of both the dimensions of empiricism and congruency, there would automatically follow a clarity and coherence of explanation. This is not necessarily the case however, and has generated some of anthropology's greatest theoretical errors followed by serious consequences, including the rejection of formalism and empiricism during the crisis era, as illustrated below. Diverging from Leaf & Read's (2012) position, White (White 1974; White & Denham 2008) has suggested that widespread axiomatic formalisation of anthropological models carries no inherent explanatory benefit unless it is accompanied by large datasets of real-world social interaction. In the absence of such data White argues, formalisation merely codifies the assumptions of anthropologists without testing those assumptions (White & Denham 2008:3 - see APPENDIX 1 for extended quote):

More specially, White & Denham warn against "situations where both ethnographic description and modeling introduce empirically inappropriate or uncalled-for assumptions" (2008:3). By 'ethnographic description', White & Denham are referring tacitly to the ad hoc and anecdotal 'thick description' promoted by Geertz (1972; 1975), Ingold (2002), Clifford (2005), and others (see below). Their point is that, although such descriptions may be highly *in*formal, to the point of overtly eschewing the idea of structure altogether, they are nevertheless likely to encode implicit assumptions about both sociality and causality. Any denial by social anthropologists that such assumptions exist in ethnographic description may in fact render them equally, if not more prone to generating paradoxical representations. White and Denham (2008) suggest that the solution is to acknowledge the explanatory power of empirically based formal modelling, but to allow for a degree of flexibility in the axioms on which the models are based in order to achieve congruence with lived

reality. The following section outlines how axiomatic principles of group theory, both implicit and explicit, encode sociality and causality along a spectrum of weak-to-strong formalism, but consistently minimal empiricism and weak congruence.

2.1.1 Sociality in Group Models

Models of sociality that refer to groups are based on the set theoretic principal that individual people can be counted, and that the extent to which they are counted represents the boundary of the theoretical model that can be constructed to represent them. As such, models derive their explanatory power from the classification of individual people within a larger total set into further subsets of people. Set theoretic principles, by themselves, impose no definitions on how a set of people is defined, or how subsets of that set should be defined (see Figure 13). This means that set theory provides very useful generalised axioms for delimiting data *about* people, but carries no explanatory power in and of itself, for detecting or measuring *patterns* in that data. Another way of putting this would be to say that set theoretic principles do not tell an social anthropologist which people to describe or why, much less *how* they should be classified. Any and all decisions in this regard, as far as set theory is concerned, are made at the discretion of the anthropologist.

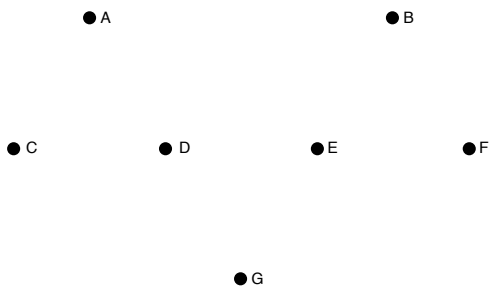


Figure 13: A set consisting of 7 elements with no order, no attributes, and no relations.

Set theory becomes 'specialised' into group theory when anthropologists begin classifying, deliberately or accidentally, explicitly or implicitly, subsets of people. Sets of people, modelled as such, have no patterning, because they contain no information about links between people or subsets of people. This holds true even where subsets of people can be named. Stating, for example, that a set of 100 people contains a subset of 50 people, does not meet the criterion of stating that 'structure' exists among those people. This is because the statement defines no *link* between set and subset, even though this may seem counterintuitive if one thinks of a subset as something that is 'inside' a set. In fact, as will be outlined below, this intuitive misconstrual of set theory is probably the main reason that group theory seems to have emerged *spontaneously*, as the predominant framework of social anthropology over the past 150 years. The act of classifying subsets of people necessitates the definition of classificatory links *between* people. This is equivalent to defining patterns of social relations.

Group theory diverges from set theory by the imposition of two principles upon a set: a) that elements in a set exhibit one or a combination of a limited number of attributes, and; b) those attributes define the way in which elements in the group interact, as distinct from the way in which elements *outside* of the group interact. In mathematical terms, elements are numbers, attributes are the values of those numbers, and the interactions between elements are the operations between values, such as addition, multiplication, and so on. We recognise that 1+2=3 for example, because of the value that each number carries and the predictable ways in which those values affect each other given the operation '+'. In the particular manner in which social anthropology construes group theory, elements are people, attributes are observed or alleged characteristics *of* people, while the interactions between elements are observed or alleged relationship *between* attributes (*not* the relationships between people themselves, as discussed below). This makes group models useful for making generalised, weakly congruent models of social interaction, by abstracting multiple particular people into sets of generalised attributes. As shown in Chapter 1, this is precisely what Morgan (1871) and Tylor (1871),, Radcliffe-Brown (1929; 1940), Lévi-Strauss (1963; 1966a; 1966b; 1968; 1969), and their followers understood to be the function of natural science, and what Radcliffe-Brown (1940) and Lévi-Strauss sought to achieve by developing the disciplinary theme 'structuralism'.

Of the variety of groups definable in purely mathematical terms, the type that most closely fits predominant anthropological models is possibly the *permutation group*. A permutation group is an algebraic model defined by a five mathematical axioms:

1) *Closure*
2) *Commutativity*
3) *Associativity*
4) The inclusion of an *identity element*
5) The inclusion of an *inverse element*

These axioms may seem abstract, but become intuitively comprehensible when rendered using examples. Modelling a set of people as a permutation group requires first mapping one-to-one correspondences or *bijections* onto people defined as members of that set, such that they are 'transformed' into group members (see Figure 14, Figure 15, Figure 16, and Figure 17). This transformation involves performing a function that 'marks' individuals as members of a group, such as by stating that they all speak a common language for example[18]. Bijection can be thought of as

[18] It is noteworthy that social anthropologists often refer to Indigenous Australians as members of one or more 'language groups', suggesting that individuals can be grouped by this attribute, but also implying that the language itself is grouped together with people. The correct usage of the term 'language *group*' actually refers to groups of languages, not groups of people (Halliday 1968). When linguists speak of groups of people 'marked' by the speaking of a common language, they refer to a 'language *communities*', not '*groups*'. This kind of divergence in definition is likely due to the higher standards of formal definition in linguistics, where implicit modeling is not tolerated to the extent that it is in social anthropology (see Chapter 3).

applying a 'cookie cutter' to a broader set of people. In formal terms, bijection is a type of common function *f* that can be a applied to a set *S* in order to generate a group *G*, such that $f(S) \rightarrow G$.

'*Closure*' describes a characteristic 'sealing off' of group members from other people, in such a way that they are bound together by a single type of relationship with one another. An intuitive example might be to distinguish people inside a particular room within a building from other people elsewhere in that same building. Generally, social anthropologists refer to this kind of interaction as an expression of 'solidarity', 'camaraderie', or 'endogamy', such that group members are represented as having some distinctive relationship among themselves, which sets them apart from non-group members. Closed interaction can be formalised as a function *f* operating upon each group member individually, in such a way that it links them to one or another of the other members in the group, such that $f(Group\ member\ x) \rightarrow Group\ member\ y$. In group theory there is only one such type of link for any given group. For example, where *f* is equivalent to 'solidarity', it might be stated that 'solidarity' applied to person *x* links them to person *y*, who is inside the group, but does not link them to, say, person *z*, who is outside the group.

'*Commutativity*' describes the irrelevance of *sequential order* in which members of a group interact, such that their membership of the group is not affected by the sequence of interaction, (e.g. 'a group comprises not only present members, but also past members and potential future members'). In other words, it does not matter *when* one given member of a group interacts with any other member, since the consequence is the same irrespectively. Commutativity in social groups might be formalised as: Group={Person A, Person B, Person C}={Person C, Person A, Person B} ={Person B, Person C, Person A}.

'*Associativity*' describes the characteristic irrelevance of *subgroup order*, when subgroups form within the group. In other words, the order of 'nesting' subsets, in which group members are also members of smaller subgroups, does not change the overall membership of the broader group (e.g. 'distinctions between clans or dialect groups does not affect individual membership of the broader nation or language group'). Associativity might be formalised as: Group={Person A, Person B, Person C} ={Person A, {Person B, Person C}} ={{Person A, Person B}, Person C} ={Person B,{Person A, Person C}}.

The inclusion of an '*identity element*' describes the characteristic presence within the group of at least one person or some other element, by which each of the other members in the group identifies themselves, such that they 'match' in some way. Social anthropologists, by coincidence, often refer to this function in terms of 'shared identity', 'selfhood', or 'personhood', e.g. 'Person A and Person B share an identify with Person C, thus forming members of group X'. A frequently used example of an identity element is an apical ancestor in classical kinship theory, where all members of a 'descent group' are defined as such by their 'common link' to that ancestor. All members of the group are, at this level, 'self', insofar as 'self' is a product of descent from that particular ancestor. The function of

an identity element on other members of the group might therefore be formalised as $f(Self) \rightarrow Self$. The inclusion of an identity element is one of the chief tests of group closure.

Finally, the inclusion of an '*inverse element*' describes the characteristic presence within the group of an 'inverted' form of the self, such that any member of the group can be exchanged or 'inverted' with it, without altering the overall composition of the group. In crisis era anthropological idiolects, such inversion is often referred to in terms of the link between 'self' and 'other', e.g. 'the opposite of self is other' and 'the opposite of other is self'. Common examples include male vs. female 'versions' of self, junior vs. senior selves, black vs. white selves, etc. The formalised function f of an inverse element might be represented as: $f(Self) \rightarrow Other$ and $f(Other) \rightarrow Self$.

Although the weak grammar (in Bernstein's terms) of many anthropological texts means that group theoretic axioms are rarely formalised to the extent that they are completely articulated, core elements are generally essential to the logical coherence of the text. Group models are typically represented by Venn diagrams, which clearly and intuitively illustrate how elements are closed under a common operation. Venn diagrams are typically drawn without any reference to particular elements within them, thus completing the transition from congruent to metaphorical construal of individual people and social interaction. Following on with the set of seven elements provided in the example at Figure 14, Figure 15, Figure 16, and Figure 17 illustrate this process.

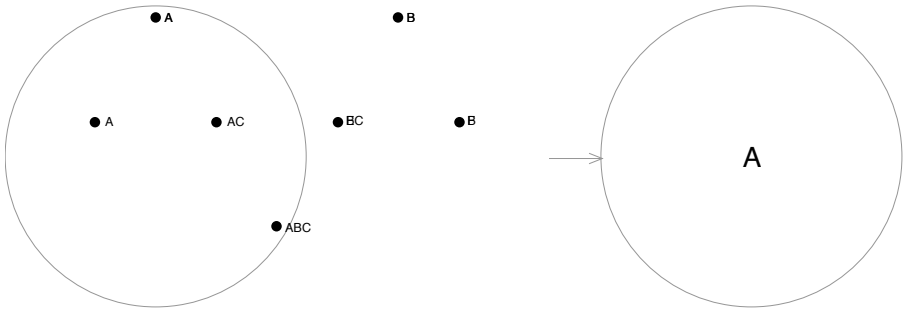


Figure 14: A subset of 4 people bijected into a group 'A'.

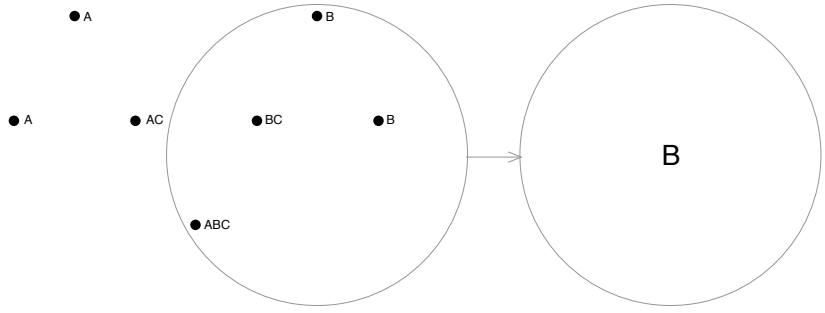


Figure 15: A subset of 4 people bijected into a group 'B'.

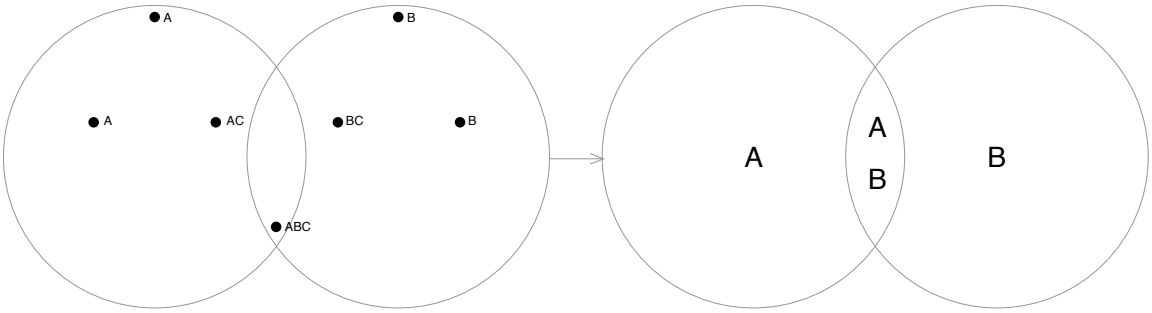


Figure 16: Two intersecting subsets of four people each, bijected into three groups 'A', 'B', and 'AB'.

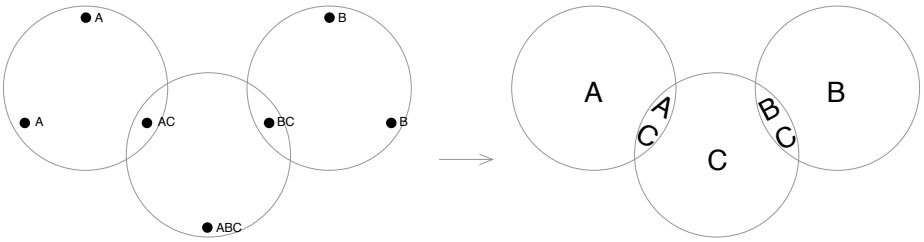


Figure 17: Three intersecting subsets of three people each, bijected into five groups, 'A', 'B', 'C', 'AC', and 'BC'.

Group models provide a highly formal and intuitive means for thinking about patterns of social activity. They also offer the capacity to incorporate empirical data, where that data comprises enumerated individuals exhibiting one or more predefined attributes. In the examples of group theoretical visualizations above, it is easy to imagine a range of social scientific data that might be represented by each group and their intersection, including gender, ethnicity, income distribution, age levels, and so on. In every instance however, the axiomatic definitions of 'group' remain in force, such that every relation between every pair of elements is subject to a single operational function, i.e. people can only be grouped together if they all meet the criteria of interacting with each other in the *same way*, by exhibiting a sociality that is consistently predictable and homogenous. This raises the question of how group theoretic models construe causality, and how *tacitly* group theoretic models in particular, severely limit the kinds of patterns that social anthropological models are able to realise.

2.1.2 Causality in Group Models

Group models are typically used as a method for *describing* social patterning, but they also impose a tacit form of *explanation* on that patterning. Chapter 1 introduced Halliday's model of semogenic re-mapping, by which metafunctions that realize meaning about processes, people, and things in the real world can be leveraged to realize further meaning about entities that exist only within language itself (Halliday 2001). As noted in the introduction to the current chapter, Halliday & Matthiessen (2006) elaborate further, showing how this remapping can generate relatively more or less congruent construals, where congruence is measurable according to the degree by which models abstract processes, people, and things into objects, and synoptic relations between objects. Causality is a special kind of such a relation:

> *"We have retained the term 'realization' to refer to the interstratal relationship between the semantics and the lexicogrammar: the lexicogmmmar 'realizes' the semantics, the semantics 'is realized by' the grammar. ... In any stratal system (i.e. any system where there are two strata such that one is the realization of the other) there is no temporal or causal ordering between the strata. It makes no sense to ask which comes first or which causes which. That would be like taking an expression such as x = 2 and asking which existed first, the x or the 2, or which caused the other to come into being (it is not like the chicken and the egg, which are temporally ordered even though in a cycle). There is a sense in which realization is the analogue, in semiotic systems, of cause-&-effect in physical systems; but it is a relationship among levels of meaning and not among sequences of events. ...*
>
> *"Every scientific theory is itself a stratal-semiotic system, in which the relation among different levels of abstraction is one of realization. This is to be expected, since all such theories are modelled on natural language in the first place; and, as we have seen, the semantics of natural language is itself a theory of daily experience" (Halliday & Matthiessen 2006:25).*

Group theoretic construals of causality may be treated as varying along two converging planes, as illustrated Figure 11 and at Figure 12 above, according to how they encode sociality in links between types of data. As noted above, group models are inherently constrained by their measurement of sociality in terms of links between individual *attributes*, rather than the links between individual *people*. The patterns of social organization that are consequently recognisable in group theoretic models are construed as patterns of attribute distributed across sets and subsets of people. This means that the causes of patterning must be similarly located in the *synoptic distribution of attributes*, rather than in the *dynamic and agentive relations between people*.

As further outlined in the previous section, group theoretic models construe sociality in the uniform closure of groups, where any given group constituted by one uniform link between each element in the group and the group's identity element. An identity element is itself *part* of the set from which the group is derived. This means that the identity element is either a person, or the group model is not limited to describing only people, even though it may be represented as doing so. Such a group model may, for example, describe both animals and people, both locations and people, or both artefacts and people at once, where the two are not differentiated, but where one forms the identity element for the other (as discussed in detail in the next section). In a group model, sociality originates *from within* the group, and *stays* within the group. Group composition does not and cannot change, no matter how members are rearranged or permuted. In group theory, sociality cannot flow *into* the group from the outside, nor can it flow *from* the group *to* the outside, through the formation of new relations. Neither the ordering, nor the subdivision of relations between group members can change the composition of the group, nor can they change the composition of other groups.

Tacit group theoretic models are often deeply embedded. Take the following example, drawing again on Ingold (2002):

> *"[A]s societies increase in scale, complexity and internal heterogeneity, the middle- and lower-level meanings of their built environments are likely to become increasingly prominent. Thus the modern city contains a plethora of social markers which indicate and* ***delimit the spaces available to members of different groups****, or to people of different identity or status, as well as instrumental markers which serve as guides to expected behaviour and as aids to wayfinding. People living in large and complex societies might not read their cosmologies in the landscape, as for example do Australian Aboriginal hunter-gatherers, but they do rely at every turn on artefactual cues in the built environment in order to navigate successfully in a highly differentiated social milieu" (Ingold 2002:337 - emphasis added).*

The axioms of Ingold's statement can be broken down as follows, where 'the modern city contains a plethora of social markers/artefactual cues/meanings', announces his opening definition of the dataset's parameters:

1) "... navigate successfully" = the operation under which the group is closed, where '*un*successful navigation' is deemed so unfeasible as to negate mention;
2) "... delimit[s] the spaces available to members of different groups" = associativity, where 'delimitation' refers to the formation of subgroups within the overall group of 'the modern city' ;
3) "... people of different identity or status" = commutativity, where 'status' refers to the order of relations between elements in the group;
4) "... people living in large and complex societies" = identity elements, or 'self', within the group;
5) "Australian Aboriginal hunter-gatherers" = inverse elements, or 'non-self', who may exist within the city, but only in an inverted state, in Ingold's terms.

Note that in the quote above, interaction is clearly between people, meanings, and spaces. This model may be characterised as minimally congruent, since people do not actually have social relationships with buildings, meanings, and spaces, bur rather with the people who express those meanings, and who inhabit those buildings and spaces. This lack of congruence constrains both the descriptive and explanatory power of the model, as the respective functions of the interaction between congruence and empiricism on the one hand, and between congruence and formalism on the other. Compounding this descriptive and explanatory weakness, the grammar of the language used to build the model is also weak, since the model is both ad hoc (with no explicit axioms), and anecdotal (no real examples of city living are actually provided). Note also however, that the model nevertheless implies that people possess *choices* in the way that they 'read', 'take cues', 'wayfind', and so on. This suggests a process of independent decision-making that might contradict the

otherwise uniform operation of 'successful navigation'. However, the obviousness of this contradiction, or 'paradox' as White & Denham (2008) put it, is reduced by the subtlety of the language used by Ingold - a language that submerges the group theoretic structure of the model under a layer of distracting metaphors. This contradiction is further camouflaged by the lack of empirical data, which, as laid out in the previous chapter, is normal for social anthropology. What we are left with is a set of tacit axioms that construe social interaction in terms of group membership without any acknowledgement that this has occurred. Ingold's model appeals to popular experiences of city-living as 'navigating a built environment', however it abstracts the real, measurable process of interactions between city inhabitants into metaphoric relations between people and buildings. The result is that causality, as an explanation of the patterns of social activity, is highly constrained.

2.1.3 Formal Models and Weak Grammar: Synthesizing Congruence by Imposing Order

As discussed in Chapter 1, the 'strength' of the grammar used to construct specialised languages, including the use of empirical data rather than anecdote, and formal models rather than ad hoc ones, varies from one social science discipline to another. Bernstein (1996; 1999) locates the strongest grammars among the social sciences in the disciplines of economics, psychology, and semiotics, and the weakest grammar in sociology and anthropology. In all the vertical discourses of academia, those with the weakest, most informal grammar, belong to the humanities and arts. Prior to the 1970s, leading social anthropologists insisted that their goal was to formalise their discipline to a point where it might achieve a paradigmatic parity with the natural sciences. Following the 1970s this aspiration was abandoned and derided. So what are the effects of such weak grammars on the formal models described above, when horizontally structured disciplines make explicit attempts to build them? And how has social anthropology's crisis era rejection of its earlier attempts at formalism affected contemporary attempts to model culture? How are contemporary social anthropologists making models without either formalism or empiricism?

The transition from pre-crisis era to crisis era is marked by at least three broadly distinct thematic changes, summarised in the previous chapter and amply reviewed elsewhere (see e.g. Harvey 1990). To recap, these involved, firstly, 'demoting' anthropology's disciplinary subject, the study of human social culture, from the status of an emerging paradigm - what Kuhn (2012[1962]) calls a 'pre-paradigmatic framework' - to the status of a theme or 'metanarrative' within the discipline, and then secondly, attempting to assimilate a number of specialised languages from adjacent social science and humanities disciplines, such as psychology, semiotics, and literature. Thirdly, and conversely, these and other disciplines attempted to adopt anthropology's newly liberated subject matter of 'ethnos' as the thematic perspective of 'cultural comparison'. In some instances of anthropology's 'borrowing', the theme was detached from anthropology altogether and rebadged as a freestanding subject of 'cultural studies'. Through all of this interdisciplinary re-ordering, anthropology retained the fundamental, weakly congruent group theoretic principals that had been used since the late 19th

century, to delimit an area of expertise in which its practitioners could retain authority: The distinction and comparison of human populations on the basis of variable patterns in social behaviour.

What changed in this transition was not the underlying pre-paradigmatic assumptions about groups of people, nor the unitary links by which they could be compared, but rather the *ordering* of those groups, the ordering of the elements and subgroups within them, and the functions under which those elements and subgroups could be closed into yet broader groups. As spelled out in the following section, the weak grammar and horizontal structure on which social anthropological (and other social scientific) knowledge is based, prevents speakers of any of anthropology's various specialised languages from distinguishing between the consistent causal imperatives of their tacit group-theoretic assumptions, and the irrelevance of re-ordering the components of those assumptions. It was, and continues to be the case, that the inherently weak congruence of group theoretic models, which render sociality in terms of metaphor rather than specific observations and formal construals, necessitates this ancillary ordering as a means to achieving otherwise absent congruence with human experience. Cyclically reinvented, *a priori* orderings of time, space, and normativity, provide the impression of new, more incisive explanations of social phenomena, but because the underlying group theoretic assumption remain intact, they ultimately fail to integrate the diverse and complex observations and experiences that real people, both anthropologists and interviewees, encounter.

Group models do not explain how experience unfolds in time as chains of interactions between people. Because they set up static structures into which people are grouped according to homogenous attributes, group theoretic models require the addition of ancillary order to give the impression of dynamic variability. Such ancillary ordering tends to draw most frequently upon allusions to space and time, and more recently in crisis era anthropological texts, upon allusions to symbolic authority (although rarely to *material* power). Common instances of spatial ordering include rankings between 'inside' and 'outside', 'centre' and 'periphery', 'core' and fringe', 'urban' and 'remote', etc. Common instances of temporal ordering include rankings between 'primitive' and 'modern', 'modern' and 'postmodern', 'colonial' and 'postcolonial', etc. Instances of ordering construed in terms of symbolic authority include rankings between 'hegemonic' and 'subaltern', 'dominant' and 'resistant', etc. Once people are abstracted into groups, these orders are applied as a means of synthesizing congruence within otherwise incongruent models, in order to imbue them with descriptive and explanatory power. Thus, where an anthropological model describes sociality in terms of two or more groups of people being distinct, based on some purportedly exclusive attribute, temporal, spatial, or authoritative order will be added to the model to *explain* the distinction. Some groups will be described as 'newer' than the others; some will be described as 'closer' than the others; and some will be described as 'more powerful' than others. Just as the underlying axioms of tacit

group theoretic modelling can be formalised, so too can the ancillary ordering used to construe causality in that modelling.

If a group '*A*' is thought of as a bijection of the set 'S', comprised of elements (A_a, A_c, A_d, A_g), as represented at Figure 14 above, then these elements can be thought of as ordered by '*O*', such that $(O_a \leq O_c \leq O_d \leq O_g)$, where $O_a$is less than or equal to O_c, which is less than or equal to O_d and so on. It then becomes possible to say that the elements in group '*A*' are ordered by '*O*', such that $A = \{(A_a, A_c, A_d, A_g) | O_a \leq O_c \leq O_d \leq O_g\}$, where '|' indicates 'ordered by' and '*O*' indicates whatever metric is being referenced. If the elements in group '*A*' are thought of as 'cultures', it becomes possible to say, for example, that they are ordered by time, such as $'Group\ A \equiv All\ human\ cultures = \{(Culture_a, Culture_c, Culture_d, Culture_g) | Primitive_a \leq Intermediate_c \leq Intermediate_d \leq Civilized_g\}$. This ordering leaves the homogenising closure of the group model intact (all cultures are still members of group '*A*'), while imbuing the model with an ancillary appearance of causal dynamism (viz. ' difference between cultures is caused by relative age'). As explained above, the order of elements in a group is irrelevant to the logical coherence of group theory as a model - it does not matter what order is imposed on the elements in order for the axioms of group logic to remain formally self-consistent. In the example above, elements of '$Group\ A$' remain grouped together as '$All\ human\ cultures$' irrespective of the temporal order attributed to them. $Culture_a$ might be ordered in time as '$Civilized_g$' just as readily as '$Primitive_a$', without altering the logical consistency of the group itself. This means that any order imposed on a group model is completely detached from the group model - it does not matter whether the order is supported by evidence or not, since its tacit function in this context operates to make an *in*congruent model *appear* congruent. It should be noted that that the tacit quality of the function does not make the ancillary order itself implicit. While the function tends to remain implicit, the ancillary order may be realised in variably implicit or explicit terms, as illustrated below.

In social anthropology's pre-crisis era, such ordering was explicit, formal, and often overtly ideological in supporting the interests of anthropologists' own communities. In discipline's subsequent crisis era by contrast, this ordering ordering has changed so that it is now generally implicit, ad hoc, and covertly ideological in its support for social anthropologists' own communities (although it is usually *overtly* ideological in its support for the communities in whose cultures anthropologists specialize). The result is that people who were once construed by anthropological models as members of 'primitive', 'remote', and 'weak' groups, are now construed by reformulated models as members of 'traditional', 'on-country', and 'resilient' groups. The ancillary ordering remains, albeit in reconfigured terms. The fundamental consistency between the pre-crisis and crisis era construals, is the theoretic modelling that perpetuates homogeneity among group members, limits the potential for identifying complex patterns of relations between individual people, and limits the potential for identifying complex causes. In the course of making group models *appear* more congruent, the ordering of

groups acts to mask the fundamental incongruity of group theory as an explanation of inherently complex human sociality.

The following section traces the emergence and persistence of group theoretic principles in social anthropology, first in the explicit and formal terms of the pre-crisis era, as developed by Tylor, Morgan, Radcliffe-Brown, Lévi-Straus, and others, and later in the implicit and ad hoc terms of the crisis era developed by Geertz, Sahlins, Clifford, and others. This section illustrates how, by rearranging the temporal and spatial ordering mechanisms that are used to render group theoretic models congruent, contemporary anthropology perpetuates an inherently incongruent theoretical framework for describing human sociality and for explaining its causes.

2.2 The Development of Group Theoretic Models in Anthropology

It is proposed here that weakly axiomatic permutation groups have formed the dominant anthropological model of sociality for most of the discipline's existence, both before and since the onset of its crisis. This began with the inception of anthropology's comparative premise in the historicist framework of Morgan and Tylor in the late 19th century, then carried on with the structuralism of Radcliffe-Brown and Lévi-Strauss during the pre-crisis era, before being reconfigured in the anti-structuralist themes of Geertz, Sahlins, Clifford, and their followers in the post-crisis era. At each juncture, anthropological research on Australian cultures featured heavily, either as a result of first hand research such as that by Radcliffe-Brown, or as a result of reformulation of local research by others such as Morgan and Lévi-Strauss. Until 1949, these models were invariably rendered without reference to formal mathematical definition. As discussed in detail in the following section, the prevalence of group theoretic thinking was finally spelled out by mathematician André Weil, in his appendix to Lévi-Strauss' famous kinship monograph '*Elementary Structures*' (Weil 1969[1949]). However, the full significance of this discovery was not widely realised, and even after 1949 Lévi-Strauss himself continued to work using only moderately formalised analogies of closed groups. Lévi-Strauss maintained that the models he developed were a direct reflection of his field data, emerging from inherent patterns in informants' own accounts, rather than from a particular explanatory *construal* of that data on his part (Jorion 2000). The 20-year delay in the translation of Weil's analysis from French to English in 1969 did not help matters, and meant that it did not become widely accessible to Anglophone anthropologists until the early 1970s.

The following section illustrates that the sequential shifts in thematic aspect from one specialised language to another (outlined in the previous chapter) has had no effect on the prevalence of group theoretical assumptions, such that they remain as central to post-modern social anthropological theories of sociality and causality, as they do to structural-functional social anthropology, historicist social anthropology, and all other interstitial variants. This suggests that group theoretic axioms, whether implicit or explicit, do not form a thematic 'aspect' in the terms outlined by Bernstein (1996; 1999), but rather a 'pre-paradigm' of the discipline in the terms outlined by Kuhn (Kuhn 2012[1962]:11). Such pre-paradigmatic theories reside outside the realm of the serial code of Bernstein's analysis (Bernstein 1996; 1999), in much deeper layers of metaphor that are autochthonous to anthropologists' own culture. These are less susceptible, if not completely impermeable, to intermittent changes in specialised language, often for ideological reasons, such as was the case of pre-Copernican geocentrism in pre-paradigmatic cosmological theory (see previous chapter). So if group theoretic principles remain intact, albeit implicit throughout the serial variation of anthropological language, what *does* change during the shifts from one language to another? In the following section it is proposed that what changes is not the arrangement of elements, subgroups, and groups in each new model, but the arrangement of *orders* imposed upon them. This

rearrangement of ordering is the only restructuring possible under a group theoretic paradigm, since all other axioms of group theory are fixed and closed, no matter how weakly formalised the model.

As outlined in the previous section, permutation groups locate causality in the uniformity between elements in a group, and the potentiation for those elements to be compared with each other, without altering the composition of the set from which the group is derived. The particular *ordering* of these elements, along with their division into particular subgroups, has no effect on the explanations that group theory gives for the patterning of social behaviour. This is because causality is located in the function '*f*' by which elements are compared. Where a group is composed of elements such as 'societies', 'nations', 'cultures', 'persons', 'peoples', and so on, the explanation of patterning comes from the construal of the uniform link *between* those elements. In group theoretic terms, the particular order in which those uniform links are arranged does not effect the outcome, viz. their membership of the group. Whether society 'A' is compared with society 'B', or 'society' 'B' is compared with society 'A', has no effect on the inclusion of further comparison of each society with yet another society 'C'. All remain comparable by dint of their *a priori* classification *as* 'societies', i.e. by their closure under the function *f* that defines them as such, viz. 'every society possesses a kinship terminology, a language, a religion, and an economy', etc. This means that at each turn social anthropology undergoes a horizontal transition from the predominance of one specialised language to the predominance of another, such as the transition from historicism to structuralism, or from structuralism to post-structuralism, the kinds of functions that are used to close elements into groups, together with the kinds of ordering that are imposed on those groups, can be creatively 're-imagined' without altering the fundamental group theoretic assumptions that contextualize the re-imagining. In this light, the ostensible 'abandonment of metanarrative' that was held to 'lay to rest' the error of structuralist theory (Ingold 2002), was really only the abandonment of a particular kind of temporal ordering (historicist), instantly replaced by a new one (a-historicist).

The paradox for group theoretic models of sociality is that while, on the one hand, they cannot encode causality due to the axiomatic irrelevance of order to their configuration (i.e. 'associativity' and 'commutativity'), nor, on the other hand, can any explanatory model of sociality ever be *un*ordered. This is because all human languages require that cause precede effect, as Hallidaty & Matthiessen (2006) make clear. Instead, what varies from one thematic language to the next in the history of social anthropological theory, is *how* this cause-effect order is construed on the congruence-metaphor spectrum described by Halliday & Matthiessen (2006). The arbitrary variability of this cause-effect ordering is essential in maintaining the value of the group theoretic framework to social anthropological theory. Without it, group theoretical models could not allow social anthropologists to re-construe social processes as static objects, or 'structures', or to contest different configurations of order. In these terms, so-called 'post-structuralist' models are no less structured than structuralist models, since both rely as heavily on abstraction from interpersonal social process to metaphorical object, as each other.

As noted previously, common orderings in anthropology are temporal and spatial. On a temporal scale, elements and subgroups are frequently ranked sequentially as 'ancient', 'primitive', 'resistant', and so on, while others are ranked as 'modern', 'civilized', 'vulnerable', etc. On a spatial scale meanwhile, the same elements and subgroups are often ranked as 'remote', 'distant', 'peripheral', etc., while others are ranked as 'local', 'near', 'central', and so on. In recent decades, such orderings have been generalised into more abstract, all-encompassing dichotomies such as 'self' and 'other', ranked on a spectrum between 'most self-like' and 'most other-like'. However, in no theoretical model where such ordering occurs, is order put forth as a causal factor for sociality. Rather, temporal and spatial order tends to be cited as an *effect* of patterned social organization, and deemed inherent to the group in question.

Where societies are ranked on a temporal scale of primitive-to-civilized for example, that ordered ranking is typically presented as a *consequence* of some other causal factor, like distinctive technology, religion, or kinship terminology. Where cultures are located on a geographic spectrum of 'local' vs. 'remote', that order is typically presented as the effect of distinct 'trade', or 'ecology', etc. In each case, the *causes* are located in relations between inherent 'types' of social patterning, such as 'life in a harsh environment', or 'worship of the desert landscape'. Meanwhile, the *effects* are located in the ordering of those types in time and space, such as 'ongoing connection', where the latter are attributed to the former. In contemporary Australian social anthropology it is quite normal for researchers to describe physical 'connection to country' (a type of geospatial ordering) as an effect of 'traditional laws and customs', for example (Bauman 2010).

In the following section, examples are provided in the form of widely cited studies from three major phases in the serial development of the discipline: Morgan and Tylor at the discipline's inception in the 1870s, Radcliffe-Brown at the beginning of its formalisation, Lévi-Strauss at its late pre-crisis peak in the 1960s, and Geertz, Gupta & Ferguson, and Sahlins, at its crisis era genesis from the 1970s onwards.

2.2.1 Pre-Crisis Group Models: Tylor, Morgan, and Lévi-Strauss

Under the dominant historicist theme of early social anthropology, the shift towards or away from uniformity in social organization was seen as the marker of a linear transition from 'primitive' to 'civilized' *forms* of social organization. This made it seem logical to propose that elements named 'societies' or 'cultures' could be grouped into various *states* of uniformity, including 'more' and 'less' uniform states. The alleged process of a trend towards uniformity within any such group was treated as the *cause* of patterns in social behaviour among individuals. Thus, degrees of similarity were held to indicate corresponding degrees of uniformity between elements in one or another group: The less similarity, the broader the group, and the more 'complex' the links between elements within the group. Conversely, the greater the similarity, the narrower the group, and the more 'elementary' the

links. This logic was applied consistently from a macro-level of entire societies, to a micro-level of individuals.

Consider the following example from Tylor (1871):

> *"Culture or Civilization, taken in its wide ethnographic sense, is that complex whole which includes **[a] knowledge, belief, art, morals, law, custom**, and any other capabilities and habits acquired by **[b] man as a member of [c] society**. The condition of culture among the [c] **various societies of [d] mankind**, in so far as it is capable of being investigated on general principles, is a subject apt for the study of laws of human thought and action. ... [T]he uniformity which so largely pervades civilization may be ascribed, in great measure, to **[e] the uniform action of uniform causes**" (Tylor 1871:1- alphabetical index added to aid following discussion).*

Referring to the alphabetical index added this quote, Tylor's 'discovery' was that all of human culture could be modelled as part of (d) a single superset called 'mankind', comprised of (c) a subset of distinct 'societies', each of which in turn consisted of a subset of (b) 'men', each of which in turn consisted of a subset of (a) 'knowledge, belief, art, morals, law, and custom' (see figure below). Clearly the model is tacit, since Tylor gives no indication that he was aware of its group theoretic characteristics. The model nevertheless fulfils the axioms of definition as a permutation group, as follows: Firstly, mankind itself is treated as a group closed under the operation 'uniform thought and action', meaning that no elements of the set exist outside of it, viz. no societies, men, laws and customs, etc., can be defined *without* 'uniform thought and action', such that they could exist outside the group 'mankind'. Secondly, the 'order' of each element in the set, at least at this stage of Tylor's analysis, does not affect the membership of the set, i.e. the sequence in which 'societies, men, laws and customs', are arranged in relation to one another, does not change the final outcome - belonging to 'mankind'. Thirdly and similarly, the arrangement of subsets within the set does not change the final outcome, i.e. the variable nesting of 'beliefs, laws, and customs' within the minds of men, the nesting of men within societies, and the nesting of societies within mankind, does not affect the overall membership status of elements within the group or subgroup. The fourth and fifth axioms, being respectively the inclusion of an identity element, and inverse element, are not so clear in this brief quote. However, there are indications elsewhere that Tylor conceived of 'uniform thought and action' as an operation that could be 'undone' by '*non*-uniform' thought and action, or what Durkheim later called 'anomie' (Durkheim 1893), as an inverse operation (Tylor 1871:189 - see APPENDIX 1 for extended quote).

Figure 18 illustrates Tylor's (1871) model graphically, such that each circle encloses a set of subset elements, and each bidirectional arrow '↔' indicates the associative operation of their relation to each other. Figure 19 illustrates the elaboration of the model to include all subset elements in a

single diagram, with operations removed for visual clarity. An overall set theoretic statement describing the structure of each diagram is given at Equation 1 and Equation 2 respectively.

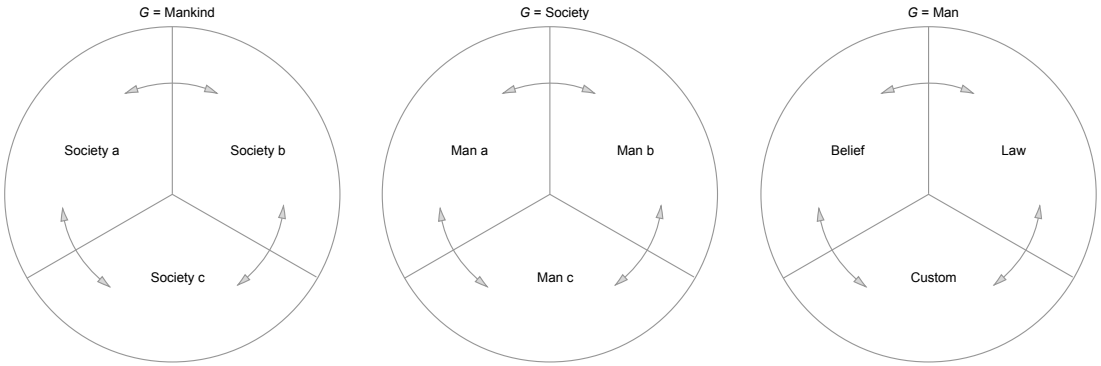


Figure 18: Tylor's (1871) group theoretic model of 'Mankind'. Elements within groups and subgroups *G* are closed under the operation '↔', 'uniform thought and action'. The group 'man' is closed under the uniformity beliefs, laws and customs; the group 'society' is closed under uniformity of men; the group society is closed under uniformity of societies. Uniformity between any two elements in a set imposes uniformity on other elements within the set, such that element in the group can be permuted with another without changing overall consistency.

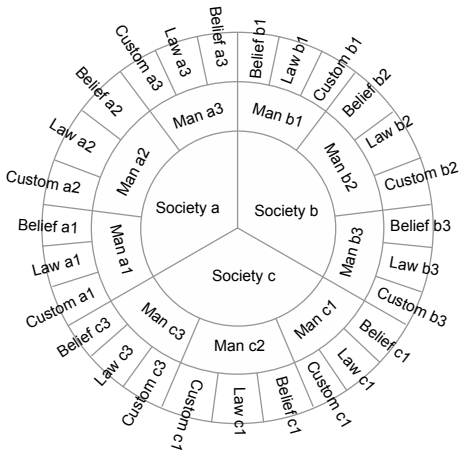


Figure 19: Elaboration formalisation of Tylor's 1871 model of human sociality in diagrammatic form. This illustrating the construal of 'varieties' of subsets and elements as (a,b,c, ... *n*) and (1,2,3, ... *n*), extended to include all subset elements, with operations removed for visual clarity.

$$Culture = \{Mankind \supset Society \supset Man \supset (Belief, Law, Custom)\}$$

Equation 1: Simplified formalisation of Tylor's (1871) model of human sociality. The set theoretic basis of Tylor's model can be represented in formal terms as follows, where 'Culture' is construed as a superset comprised of a subset called 'Mankind', in turn comprised of a subset 'Man', in turn comprised of a subset 'Belief, Law, Custom'.

$$Culture \equiv Mankind = \begin{cases} Society_a \begin{cases} Man_1^a\{Belief^a, Law^a, Custom^a \\ Man_2^a\{Belief^a, Law^a, Custom^a \\ Man_3^a\{Belief^a, Law^a, Custom^a \end{cases} \\ Society_b \begin{cases} Man_1^b\{Belief^b, Law^b, Custom^b \\ Man_2^b\{Belief^b, Law^b, Custom^b \\ Man_3^b\{Belief^b, Law^b, Custom^b \end{cases} \\ Society_c \begin{cases} Man_1^c\{Belief^c, Law^c, Custom^c \\ Man_2^c\{Belief^c, Law^c, Custom^c \\ Man_3^c\{Belief^c, Law^c, Custom^c \end{cases} \end{cases}$$

Equation 2: Expanded formalisation of the set theoretic basis underlying Tylor's (1871) model. More specifically, Tylor states that 'Culture' is *equivalent* to (≡) 'Mankind', which in turn consists of *varieties* (a, b, c, ... *n*) of subset elements, viz. 'man as a member of society'; 'various societies of mankind', etc.

Critically, Tylor asserts that the logic of this classificatory model was not his own creation, but the result of observations (Tylor 1871:11-12). Each classification was held by Tylor to be self-evident in the links *between* elements, i.e. their 'uniform actions of uniform causes'. This introduces *process* into an otherwise static model (although as shown below it is still insufficient to illustrate anything like congruent human sociality). Since this process is so clearly defined by Tylor, it can be formalised as a binomial operation between elements in the set, the effect of which is consistent throughout the set, as 'the uniformity that largely pervades civilisation' (Tylor 1871:1). This is important because in transforming a static model into a systemic one, the operation of 'making uniform' also changes the model from a *set* theoretic model into a *group* theoretic one. The 'operationalized' model is represented formally at Equation 3, where the function *f* ('making uniform') acts upon 'society x', to generate or 'cause' 'society y'.

$$Mankind \equiv f(Society_x) \rightarrow Society_y$$

$$Society \equiv f(Man_x) \rightarrow Man_y$$

$$Man \equiv f(BLC_x) \rightarrow BLC_y$$

Equation 3: Tylor's (1871) model of sociality is 'operationalized' into a systemic model of causality by construing 'uniform thought and action' as function *f* operating on elements and subsets to permute '→' one into another. For Tylor, the cause of patterning in social relations is a tendency towards homogeneity among elements and subsets.

In the socio-economic context of the mid-19th century, the new explanatory power of this model lay in the opportunity that it opened up for comparing supposedly distinct populations on the basis of

their common constituent elements. Causality in Tylor's (1871) model lies in the relative uniformity of the links between elements in each subgroup, where change leads either towards or away from uniformity among elements. Thus, the uniformity among beliefs, laws, customs, etc., marks the uniformity of an individual man as a self-consistent being, while the uniformity of multiple men marks them as members of the same society, and uniformity among societies (insofar as they are subject to the uniformity laws of thought and custom) mark them as joint members of mankind. 'Culture', in Tylor's model, is construed as a pattern of uniformity because uniformity is the basis of permutation groups.

As Tylor acknowledges however, this permutability, driven by a tendency towards uniformity, cannot explain any kind of *divergent* dynamism that might give rise to difference among elements and subsets. In other words, while 'uniform actions from uniform causes' (Tylor 1871:1) explains the trend *towards* uniformity among societies, men, and beliefs, laws and customs, and thereby their coalescence into groups, it does not explain the movement *away* from uniformity. The application of a fundamentally incongruent group theoretic model thus generates a demand for further, ancillary application for some kind of selective vector - a mechanism for explaining the emergence of diversity.

Tylor's model holds that elements in the group may be re-ordered in time and space by 'distribution in geography and history' (Tylor 1871:11-12 - see APPENDIX 1 for extended quote). It is important to distinguish this ordering from the operation of 'uniform action', since while the latter determines membership of the group and various subgroups, the former does not. More specifically, the ordering of beliefs, laws, and customs within the minds of individual men, and the ordering of men within societies, or of societies within mankind, has no effect on this overall uniformity. The distinction between this classificatory unity of the group on the one hand, and the *ordering* of the group's constituent elements in time and space on the other, is explicit and emphatic in Tylor's own text (Tylor 1871).

$$Mankind = \{Society_a, Society_b, Society_c | t_{primitive} < t_{barbaric} < t_{civilized}\}$$

Equation 4: An illustration of the ordering of mankind's constituent societies by time, using Tylor's and Morgan's classifications 'primitive', 'barbaric', and 'civilized' (Tylor 1871; Morgan 1871).

In Tylor's model, the ordering effects of time and space are mutually consistent. As one would expect, time is, ordered sequentially. But, more surprisingly, so is space. Tylor achieves this 'geographic order' by imposing a temporal order on multiple societies, and through them, a temporal order on the geographic positions at which those societies are located. Groups, subgroups, and elements are thus located on a scale ranging from 'more proximal' to 'more distant' relative to each

other, where the *relative present proximity* is held to indicate an intermediate geographic point of origin at a point *in past time*. In this ancillary application of order, time *imposes* order upon geographic space by first making the current geographic locations of groups and elements 'new', and then making the supposed intermediate geographic origin 'old'. Where groups and elements, such as societies and cultures, are subject to the imposition of temporal order by designation as 'primitive' or 'civilized', the geographic locations with which they are associated can the be similarly ordered.

Recalling the example provided in the introduction to this section, the formal statement at Equation 1 shows how the group theoretic part of Tylor's model is *first* described as a closed set, and *then* ordered by time. A subset consisting of 'Society a', 'Society b', and 'Society c' are *first* bijected into a group, and *then* ordered by a distinct sequence of time 'primitive', 'barbaric', and 'civilized'. The two parts of the statement are not dependent on each other for their own respective coherence.

The same combination of tacit group assumptions and ancillary ordering regimes are evident in the work of anthropology's cofounder Lewis Henry Morgan (1871)[19]. In the same year that Tylor published '*Primitive Culture*', Morgan publishing his own similarly influential comparative analysis '*Systems of Consanguinity and Affinity of the Human Family*', which focused on the classification of global kinship terminologies. This text lays out a framework almost identical to that of Tylor's, cataloguing regional human populations, along with an ancillary temporal order for 'explaining' their alleged evolutionary links to one another (Morgan 1871:14 - see APPENDIX 1 for extended quote).

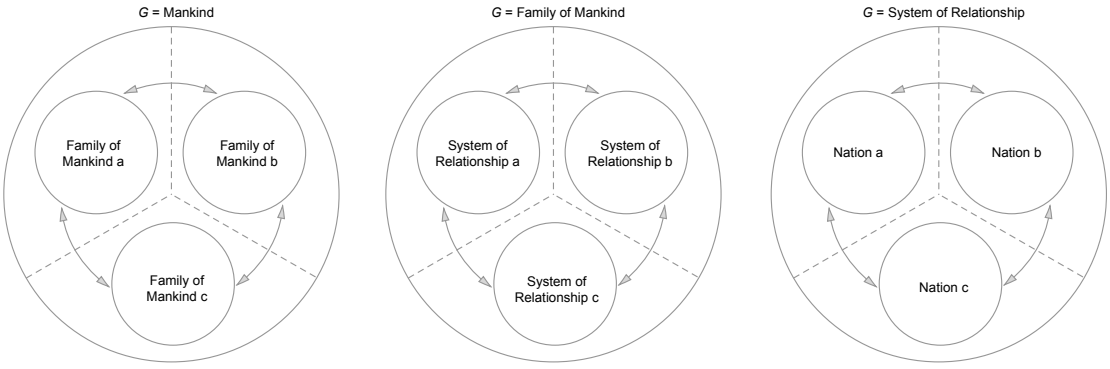


Figure 20: Morgan's (1871) model of Mankind, where elements of group *G* are closed under operation '→' or 'stability of forms'.

[19] James George Frazer is often listed together with Morgan and Tylor as the third cofounder of the discipline of social anthropology, particularly with regard to his 1890 comparative analysis of global mythology '*The Golden Bough*' (Frazer 1890). The current thesis focuses on Frazer's predecessors partly for sake of brevity, but more importantly because of Morgan's critical role in the development of kinship theory in Australia, which is the ultimate subject of the thesis.

$$Mankind \equiv f(FoM_x) \rightarrow FoM_y$$

$$Family\ of\ Mankind \equiv f(SoR_x) \rightarrow SoR_y$$

$$System\ of\ Relationship \equiv f(Nation_x) \rightarrow Nation_y$$

Equation 5: Morgan's (1871) set-theoretic model is 'operationalized' by the function *f* 'stability of form' operating on elements and subsets FoM ('Family of Mankind', SoR 'System of Relationships', and 'Nations), such that each is permuted '→' into another within the group.

Like Tylor, Morgan here begins with the superset 'Mankind', or as he also calls it 'the Human Family'. Whereas Tylor populates this superset with elements that he calls 'societies', Morgan populates the superset with elements called '*families of* mankind'. Each such family is in turn populated with multiple 'systems of relationship' or 'systems of consanguinity', which are in turn populated with 'nations'. Like Tylor's 'uniform thought and action', the binomial function in Morgan's model comprises 'stability of form', under which the group is closed - no family of mankind, system of consanguinity, or nation can permute in any way that results in a product *outside of* the elements contained in the human family, since this function operates on all elements in the system to make them comparable (compare Figure 18 left and Figure 20 left). Also like Tylor, Morgan orders group elements by both time (their relative 'perpetuation through indefinite time') and geographic space (their distribution among 'disconnected areas').

As in Tylor's case, the explanatory power of Morgan's model came from the comparative opportunities that it opened up to the nascent discipline of social anthropology. Whereas Tylor's model made the broad object of 'culture' comparable, Morgan's model made the more specific object of *kinship terminologies* comparable across 'nations', and 'families of mankind' (see Figure 18 left and Figure 20). The key features of this model are elaborated in Chapter 3 below, which outline its continuing influence on social anthropological thinking about Indigenous Australian kinship today.

From the beginning of the 20th century through to the 1960s, the overt historicist ordering of Tylor's and Morgan's models was assailed by three generations of their disciplinary heirs, including luminaries Boas (1913; 1920), Rivers (1913; 1924), Malinowski (1913; 1922), Lowie (1914; 1917; 1948); Radcliffe-Brown (1931), Murdock (1932; 1949), Firth (1951), Fortes (1955; 1969), Leach (1966), and ultimately Lévi-Strauss (1969[1949]). The most compelling result of this almost 100 years of research was the acknowledgement that bourgeoning field data showed much more complex and diverse patterns of sociality than Morgan and Tylor's models could accommodate. From this revelation emerged a broad demand for a single theoretical framework that could be applied to all human cultures, while simultaneously accommodating the complexity that was being revealed by

individual instances of fieldwork. The result was an increasingly explicit and convergent effort to formalise the language of social anthropology.

This growing consensus culminated in the emergence of what Bernstein (1999) refers to as the 'idiomatic language' of structuralism. The appeal of this language was the clarity that it seemed to bring to previously ad hoc assumptions about patterning in social organization. Because of the high level of inconsistency that had built up over nearly a century of individual anthropologists inventing their own terms for overlapping observations of obviously related phenomena, the strategic objective of structuralism was to generalise the subject of individual observations into the simplest, most abstract possible terms. The authority that was recruited to promote the language was that of natural science, such that the new language was described as being 'more like' science, than previous languages.

Thus, 92 years after Morgan and Tylor published their foundational monographs in 1871, Lévi-Strauss' presented his definition of the new language in 1963 as follows:

> *"The question then becomes that of ascertaining what kind of model deserves the name 'structure.' This is not [only] an anthropological question, but one which belongs to the methodology of science in general. Keeping this in mind, we can say that a structure consists of a model meeting with several requirements.*
>
> *"First, the structure exhibits the characteristics of a system. It is made up of several elements, none of which can undergo a change without effecting changes in all the other elements.*
>
> *"Second, for any given model there should be a possibility of ordering a series of transformations resulting in a group of models of the same type.*
>
> *"Third, the above properties make it possible to predict how the model will react if one or more of its elements are submitted to certain modifications.*
>
> *"Finally, the model should be constituted so as to make immediately intelligible all the observed facts. These being the requirements for any model with structural value ..." (Lévi-Strauss 1963:279).*

The parallels between Lévi-Strauss' characterisation of structure on the one hand, and Tylor's and Morgan's characterisation of 'mankind' on the other, are remarkable. These are as follows:

a) "*... [T]he structure [i.e. structural model] ... is made up of several elements, none of which can undergo a change without affecting changes in all those other elements*" (Lévi-Strauss 1963:279). In other words, a structural model comprises a set of elements, closed under the operation 'uniformity', i.e. if change is uniform across the set, then the set is closed and permutable, meeting the key definitions of a permutation group. This is equivalent to

Morgan and Tylor's construal of 'mankind' as a set constituted by 'cultures' and 'societies', which are in turn constituent by subsets of element such as 'beliefs, laws, and customs', closed under the operation of 'uniform thought and action' (Tylor 1871) and 'stability of form' (Morgan 1871).

b) "*[F]or any given model there should be a possibility of ordering a series of transformations resulting in a group of models of the same type*" (Lévi-Strauss 1963:279). In other words, elements in the set can be ordered. This is equivalent to Morgan and Tylor's ordering of elements and subsets in their model by time $\{primitive < barbaric < civilized\}$ and space 'geographic distribution'.

c) "*[T]he above properties make it possible to predict how the model will react if one or more of its elements are submitted to certain modifications*" (Lévi-Strauss 1963:279). In other words, when order is imposed on elements in a given set, predictable patterns result - subsets of elements that are located at a 'more primitive' point in a temporal sequence, will also be located at a 'more distant' point in a geographic sequence, relative to subsets of elements that located at a 'more civilized' point in that same temporal sequence, i.e. 'cultures' and 'societies' that are more civilized will be relatively closer together geographically, while 'cultures' and 'societies' that are more primitive will be relatively closer together geographically.

d) "*[T]he model should be constituted so as to make immediately intelligible all the observed facts* ..." (Lévi-Strauss 1963:279). In other words, the model can be applied to any an all forms of human activity.

Lévi-Strauss applied this model to all social phenomena, which he considered as entirely symbolic insofar as "social anthropology is devoted ... to the study of institutions considered as systems of r*epresentations*" (Lévi-Strauss 1963:3 - emphasis added). These 'institutions', like Tylor's 'knowledge, belief, art, morals, law, custom' (Tylor 1871:1), comprised those systems of ideas that explicitly articulate roles and relations, such as kinship terminologies (Lévi-Strauss' primary focus during the 1930s-40s), and mythology (his primary focus from the 1950s onwards).

In this regard, Lévi-Strauss' modelling remained consistent with that of his predecessors, in dealing only with those 'observable facts' that were apparent in the field, insofar as they could be coherently articulated by the people with whom he worked. Lévi-Strauss did not believe that his own theoretical assumptions placed any constraints on how he construed his observations in abstract models. As one of his students observed much later:

> *"Lévi-Strauss wants his formalisations to literally emerge from the empirical facts with no prejudice whatever about what the formalisation would reveal. The last thing he would wish is to be influenced by any a priori imposition resulting from the shape of a mathematical object.*

> *This feature of his view of theory would be striking in seminars, where he would apply the principle to guests' presentations. At question time he would go to the board and would start somewhat like this, 'If I understood you well, we are dealing with a structure of this particular shape…'. Never would he utter phrases such as 'permutation group', even if his modelling would in the end amount to entirely redefine what is actually a permutation group" (Jorion 2000:11).*

It is likely that this kind of mathematical illiteracy, spelt out by Jorion in Lévi-Strauss' case, was also a factor in masking the axiomatic assumptions underlying the models of many other influential anthropologists. Not only did this illiteracy result in a widespread inability to *detect* the disjuncture between their own models and the autochthonous models of the cultures that they were studying, but it also facilitated the persistence of the group theoretic thinking that underpinned the original emergence of the discipline in the works of Morgan and Tylor. The accompanying convention of excluding empirical evidence, and of relying instead on ad hoc observations, seems to have further masked this already invisible disjuncture. Thus, at the height of structuralism's reign as the dominant language of anthropology, the primary units of study - kinship, language, religion, and economy - remained unchanged, and the primary links between them - uniformity - remained intact. Lévi-Strauss' "ordering [by] a series of transformations" (Jorion 2000:11) merely perpetuated the disguise with a reconfigured causal explanation. The strength of the desire for a formalised language, built from general principles, overshadowed the corresponding necessity for empirical data. Meanwhile, the addition of various ancillary orderings cobbled onto that faux scientific language, continued to hide its inadequacy as an explanatory framework.

2.2.2 Crisis era Group Models: After Lévi-Strauss

A major change between the pre-crisis anthropology of the 1870s-1970s, and the crisis era anthropology of the present lies in the construal of causality in terms of normative force. Pre-crisis anthropology construed normativity as absolute. Where individuals were held to be members of a group, then their sociality was deduced as being directly predictable from the 'rules' prescribed by that group. Any breach of such rules was treated as an anomaly, which resulted in abnormally high levels of intra-group tension or 'anomie' (Durkheim 1893). This view is present all the way through social anthropology's history, from late 19th century texts of Morgan and Tylor onwards. One of Lévi-Strauss' key achievements towards the end of the pre-crisis era was to propose that normativity was not absolute, but rather varied in strength along a spectrum ranging between maximal *pres*cription to minimal *pro*scription. He divided this spectrum of relative normativity into four regions that he termed 'elementary' (maximally *pres*criptive, with no social options), 'semi-elementary' (minimally *pres*criptive, with a limited range of options), 'semi-complex' (maximally *pro*scriptive, effectively limiting options to a small range), and 'complex' (minimally *pro*scriptive, resulting in a broad range of options) (Lévi-Strauss 1969[1949]; White 2011).

In presenting this model of kinship, Lévi-Strauss expanded the principles on which elements in a social group could be ordered. Whereas the models of Tylor, Morgan, and others imposed a temporal and spatial order on elements, ranging between 'primitive' and 'civilized', and between 'close' and 'distant', with causality located in the single operation of 'uniformity of thought and action', Lévi-Strauss specified the definition of that operation into four types. He thus imposed temporal and spatial order not only upon people, societies, and cultures in the supergroup 'Mankind', but also imposed an order comprised of degrees of complexity on the operation of normativity. Lévi-Strauss' new order was not separate from Morgan and Tylor's spatio-temporal ordering regime, but integrated *with* it by the correlation that Lévi-Strauss imposed between 'primitive' time and 'elementary' normativity on the one hand, and between 'civilized' time, and 'complex' normativity on the other (Lévi-Strauss 1966). This ordering stated that the degree of prescription in 'beliefs, laws, and customs' should diminish in correlation with a shift towards temporal modernity.

In the same way that Tylor (1871) had sought order in the variable definition of laws and customs of distinct societies, and Morgan (1871) had sought order in the variable definition of consanguinity and affinity among distinct systems of relations, Lévi-Strauss (1969[1949]) now sought order in the further variable definition of normativity itself. This was effectively an extension and formalisation of Durkheim's distinction between mechanical and organic solidarity, where mechanical solidarity comprised one subclass of economic relations between members of a single society, giving rise to one subgroup 'primitive', while organic solidarity gave rise to another, 'modern' (Durkheim 1893; Lévi-Strauss 1969[1949]:xxxiii). The difference is that Durkheim, as a sociologist rather than an social anthropologist, applied the distinction to models of normative labour relations within a single 'subset' of mankind - the industrialising population of late 19th century Europe - and held that the transition from 'repressive' to 'permissive' degrees of normativity was an inherent feature of industrialisation over time.

Lévi-Strauss maintained the Tylor's and Morgan's ordering principle of time and space, such that his own concept of 'elementary structures' corresponded with 'primitive' groups, and 'complex structures' corresponded with 'civilized' groups. In Lévi-Strauss' model socilaity still resides in the uniform relations that unify each group, as they do in Tylor's and Morgan's models. The difference is that in Lévi-Strauss' model the measurement of uniformity is extended such that uniformity *U* is ordered by normativity *N* (see Equation 6):

$$U(N) = \left(N_{elementary} < N_{semi-elementary} < N_{semi-complex} < N_{complex}\right)$$

Equation 6: Formal representation of Lévi-Strauss' ordering of normativity (Lévi-Strauss 1949; White 2011), where the unity of all human culture cultures '*U*' is ordered by ranks of normativity '*N*'.

Lévi-Strauss treats normative prescription as a corollary 'function' of structure (1969[1949]: xxxiii). In his view, causality resides *within* structure, such that degrees of complexity can be used to predict degrees of causality. Elementary structures, as the most normative, have the most strongly predictive causality, while at the other end of the sequence, complex structures have the least predictive causality. The result was that populations exhibiting apparently elementary structures, such as the kinship terminologies of Indigenous Australian populations, were deemed to exhibit *maximally* predictable social organization. Conversely, those exhibiting apparently complex structures, such as the kinship terminologies of the British population (which colonised Australia during the 19th century), were deemed to exhibit *minimally* predictable social organization. Despite the increase in complexity of this model, neither the concept of 'normative rules' nor the assumption that they originated in group membership, had changed. The degree of normative 'force' in a culture was held by Lévi-Strauss to derive from group membership, according to group theoretic assumptions that had been held by Morgan and Tylor before him.

Since the onset of anthropology's crisis, normativity has been, to varying degrees, rejected as a concept altogether. Instead, causality is now often assumed to originate in a metaphorical object termed 'moral order' or 'moral economy' (Gupta & Ferguson 1992; Harvey 1990), which is construed as a field under continuous contestation, in a process of imposition and resistance, by members of a group and subgroups who seek to control it. This is part of a broader redefinition of the theme of 'social structure', now defined as antithetical to the theme of individual 'agency' (Hays 1994). Whereas the former is portrayed as rigid and serving the vested interests of powerful or 'hegemonic' figures in a group, the latter is portrayed as 'flexible' and as serving the interests of members of subgroups (or subaltern members of the same group) in maintaining 'recognition space'. Whereas pre-crisis anthropological language makes heavy use of metaphors and analogies to linear sequences and hierarchies, with structure frequently represented as a 'snapshot' or 'diagrammatic' view on sequentially ordered elements in a group, crisis era anthropology makes equally heavy use of metaphors that conjure flat surfaces, such as 'planes', or 'fields' (see below for examples).

In striving to maintain the supposed originality of the new crisis era language, contemporary anthropological texts is often at pains to emphasise these distinctions between old and new metaphors. An enduring example is that of Gupta & Ferguson (1992) in their well-known article *'Beyond "Culture": Space, Identity and the Politics of Difference'*. Note that Gupta & Ferguson place the term 'culture' in inverted commas, to indicate their view of its dubious status (Gupta and Ferguson 1992:6-10 - see APPENDIX 1 for extended quote). Gupta & Ferguson's distinction between patterns of social interaction, and the causes of those patterns, is not as explicit as it is in the pre-crisis models cited above. But their terminology is nevertheless strikingly similar to that of their disciplinary ancestors: They still identify sets of elements delimited by group theoretic axioms of closure, identity, commutativity, and associativity; the elements in their model are still 'societies, nations, and cultures'; they still identify geographic distribution of those groupings ('space, place, and

culture'); they still identify hierarchy in the order of space ('centre' and 'periphery'/'colony' and 'metropole'); they still identify sequentially ordered degrees of normativity ('hierarchical power relations') distributed across space.

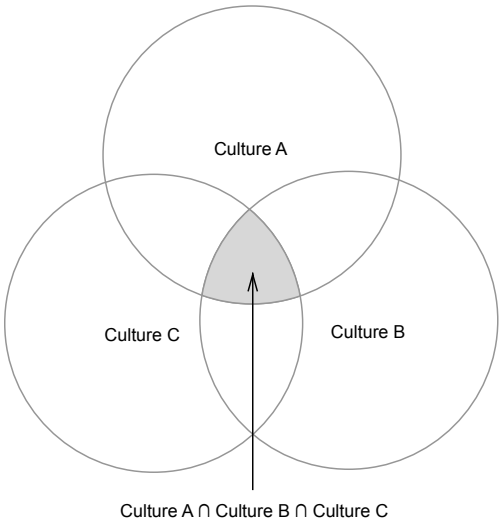


Figure 21: Gupta & Ferguson's (1992) model in diagrammatic form, illustrating the set theoretic intersection of varieties of 'culture'. Each instance of intersecting culture defines 'self'. All intersections are 'self' because all intersections share the same elements of the three sets. Note that 'intersection' of multiple group attributes is no less prescriptive than the disjoint unions inherent in the models of Morgan (1871) and Tylor (1871) over a century earlier.

$$Self = \{Culture_a \cap Culture_b \cap Culture_c\}$$

Equation 7: Formalisation of Gupta & Ferguson's (1992) model, in which the superset 'Self' is comprised of intersecting subsets 'Culture'.

All of this indicates that Gupta and Ferguson are alluding consistently to a group theoretic model of sociality, based on the same informally construed axioms that pervade their predecessors' models. The main change is in the operation linking elements together into groups and subgroups. Whereas the pre-crisis models impose an operation of one-to-one uniformity between elements, Gupta and Ferguson impose a one-to-many operation of 'intersection'. If Gupta and Ferguson's superset may be taken as 'self', composed of the intersection of multiple subset cultures, each of which intersects by the operation '$\cap$', then their model may be represented this way (see Figure 21 and Equation 7; Figure 22 and Equation 8):

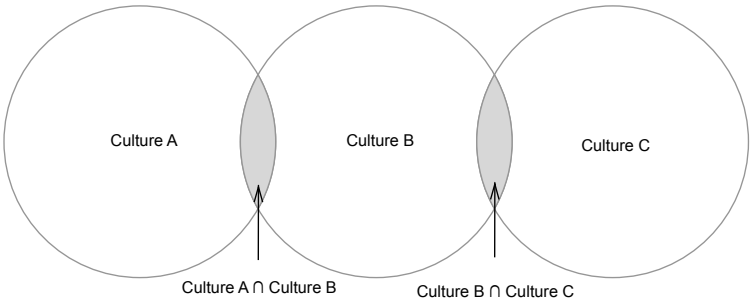


Figure 22: A diagrammatic representation of Gupta & Ferguson's 1992 model, illustrating instances of 'self' and 'other' as disjoint sets, where Culture A and Culture B intersect, Culture B and Culture C intersect, but Culture A and Culture *do not* intersect. The two intersections do not share the same elements of the three sets. Culture A and Culture B are disjoint sets.

$$'Other' = \{Culture\ A \cap Culture\ C\} = \emptyset$$

Equation 8: A formal representation of Gupta & Ferguson's (1992) model of 'otherness' as an empty disjoint union, where the union '∩' of Culture A and Culture C is empty '∅', i.e. they contain no elements in common.

We can see here that the set 'self' in these models is the equivalent to the set 'man' in Tylor's and Morgan's models, and forms the locus for causality. The thematic 'self'/'other' distinction common to a significant proportion of crisis era anthropology, is the operational equivalent of the 'uniform'/'not uniform' distinction that pre-crisis anthropology used to explain patterns of social organization. The difference in operation is that whereas *uniformity* detects patterns by the isomorphism of all constituent elements or subsets, as Gupta & Ferguson (1992) point out, intersection detects patterns by the intersection of various combinations of elements and subsets.

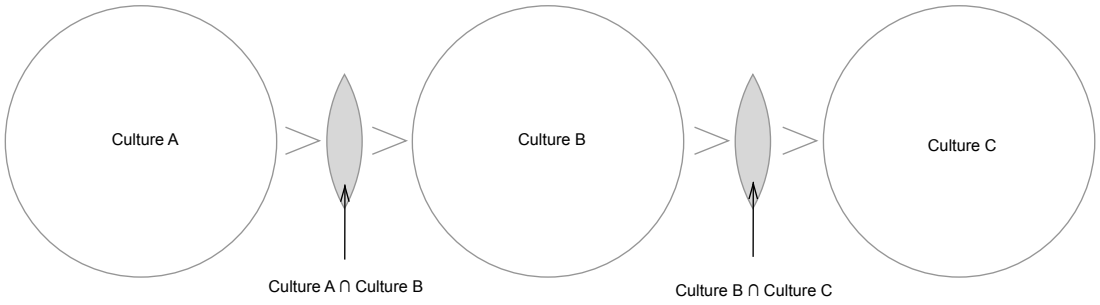


Figure 23: A diagrammatic representation of Gupta & Ferguson's (1992) model, illustrating the imposition of order '>' on the overall set. Their addition of 'blurring' does not affect the imposition of an ancillary order on the group theoretic basis of the model - it simply adds further steps to the sequence in the form of 'intersection'. The groups 'Self' (Culture A) and 'other' (Culture B) still remain separated.

$$S(C) = (\{Culture\ A, (Culture\ A \cap Culture\ B), Culture\ B, (Culture\ B \cap Culture\ C), Culture\ C\} | S_a < S_b < S_c < S_d < S_e)$$

Equation 9: Formal representation of Gupta & Ferguson's (1992) ancillary imposition of 'blurred' ordering on the group theoretic model of 'culture-play'. The distinction between culture sets and set intersections remains intact. Ordering is simply extended to apply to the intersections as well as the originating sets.

Similarly to the way in which Morgan (1871) and Tylor (1871) distinguish members of one society from members of another according to their non-isomorphic 'beliefs, laws, and customs', Gupta & Ferguson (1992) distinguish 'self' from 'other' according to whether their respective intersections are linked by a common culture. Put another way, if 'the self' is defined by the intersection of multiple cultures, 'the other' is defined by the intersection of multiple cultures with whom one's own cultures *do not* intersect. This is still a group theoretic model, where the term 'identity' denotes elements included in the group, and the sets 'self' and 'other' are still defined by the inclusion or exclusion of a subset of those elements. The intersection of two subgroups is still a group, albeit one with the additional characteristic of sharing its elements with two other groups. The supposed discovery - that so-called 'peripheral' people ("those who inhabit the border" - Gupta and Ferguson 1992:6-10) have identities constituted by the attributes of more than one group - does not in anyway diminish the group-theoretic basis on which sociality is modelled. Just as Ingold (2002) attributes built environments with the property of an identity element (defining the common attribute that binds people into a group (see Section 2.1.2), so too do Gupta & Ferguson describe "space achieving a distinct identity" (Gupta & Ferguson 1992:6-10 - see APPENDIX 1 for extended quote).

In similarly influential texts, Appadurai (1988), Clifford (2005), and Sahlins (2008) elaborate on variations of this re-ordered group theoretic construal of sociality. The same core axioms are evident among all of these models: Despite oblique reference to individuals and to personal agency, discrete groups still comprise the primary unit of culture, and the uniform relation within elements in each group is still the causal factor in generating a predictable patterns of sociality. In each model, this relation is still comprised of a single homogenising operation under which each set of elements is closed into a group, and the elements 'self' and 'other' fulfil the axioms of 'identity' and 'inverse identity' in every case.

In Appadurai's (1988) model, the unit is 'most groups', closed under the operation 'knowledge of other worlds', while 'self' ('native') and 'other' ('non-native'), as identity element and inverse element respectively, cancel each other out, *viz*. "natives, people confined to and by the places to which they belong, ... never existed" (Appadurai 1988:39 - see APPENDIX 1 for extended quote). In Appaduria's model, *all* people can be classed as native, which means that nobody is 'really' native - in other words, any person can be permuted with any other without disrupting the overall membership of the set.

In Clifford's (2005) model the unit is 'ethnic groups', closed under the operation of 'contact, overlap, and similarity'. 'Identity' is the element that 'marks' the elements of one group as against another, while the inverse element are those that are 'not marked', but are still part of the same set, or 'neighbors' as Clifford puts it. Permutation is evident in the assertion that "a group's distinction, its tradition or common culture, is always a secondary creation, not a primary cause or origin" (Clifford 2005:26 - see APPENDIX 1 for extended quote). In other words, the causal factor in driving patterns of sociality is not uniformity, but rather differentiation (variable arrangement of elements does not affect group membership).

For Sahlins (Sahlins 2008), the unit of culture is the 'micro-community', closed under the operation of 'feeding and caring'. The identity element is 'self' while the inverse element is 'other', operating on each other to yield 'the African individual', i.e., "He does not exist except to the extent he is 'outside' and 'different' from himself" (i.e. self-neutralized). Permutation by commutativity and associativity is realised by the assertion that, "[I]f ''I am another,' then the other is also my own purpose. As members of one another, kinsmen lead each other's lives and die each other's deaths" (Sahlins 2008:49 - see APPENDIX 1 for extended quote). In other words, the specific sequence of interactions or subset of relations between individual elements is irrelevant to the overall membership of the micro-community, since each can be substituted by the other.

Spatial, temporal, and normative orders, as described for Tylor (1871), Morgan (1871), and Lévi-Strauss (1969[1949]), are also all consistently applied as ancillary operations imposed on each group metaphor, in an attempt to construe individual experience. Since none of the usages of the term 'group' (or 'societies' in Sahlin's case) describes real people interacting with each other in anything resembling a socially congruent manner of interpersonal speech or action, order is imposed as an ancillary explanation of causality. For Appadurai (1988) this order takes the form of 'migration, trade, and conquest'. For Clifford (2005), order takes the form of 'syncretic combination', and 'trafficking'. In Sahlin's (2008) model, order is imposed in the form of 'integration of certain relationships'. This casts a veil of dynamic and agentive process over what is actually the depersonalised, homogenous and static structure of an unchanging group.

In each case, empiricism is absent and formalism is minimal, while congruence fluctuates between generalised and nominalised events such as 'contact', 'migration', and 'trafficking' on the one hand, and total abstraction such as 'sharing biographies' on the other. This gives readers maximal leeway in trying to 'imagine' what each of the three anthropologists is actually talking about, making it easier for the individual reader to insert their own experiences of congruent social processes into each text, and thereby finding each of the models congruent in terms that are familiar rather than incoherent. These ancillary orders of pre-contact, contact, and subsequent intersection are illustrated in the graph at Figure 23 and formalised in the equation at Equation 9.

Returning to the more elaborate example of ancillary order provided for Gupta & Ferguson (1992) above, it is telling of crisis era anthropology's literary turn that Jameson's (1984) 'hyperspace' metaphor, to which the two authors refer, was coined by a literary critic rather than a social scientist working with measurable data. Jameson's full definition comes in the context of an article titled '*Postmodernism, or the cultural logic of late capitalism*' (Jameson 1984). Jameson (2002), like Ingold in the quote at the beginning of this chapter, uses the architecture of built environments as an analogy for human culture, which, as implied by both Gupta & Ferguson (1992) as well as by Ingold (2002), is indicative of a new and superior specialised language. Jameson says:

> *"The newer architecture therefore - like many of the other cultural products I have evoked in the preceding remarks - stands as something like an imperative ... to expand our sensorium ... to some new, as yet unimaginable, perhaps ultimately impossible, dimensions" (Jameson 1984:83 - see APPENDIX 1 for full quote)*

And yet, when social anthropologists such as Gupta, Ferguson, Appadurai, Sahlins and others attempt to invoke the metaphor of hyperspace, they are not 'expanding their sensorium', but rather rearranging the constituent elements and functions of the same group theoretic model that was invented by their Victorian-era predecessors, and then re-imposing temporal, spatial, and normative order upon those elements to render them at once 'new' and 'more relevant'. Real, living people, the patterned social interactions between them, and the measurable causal factors in those patterns, are all still construed as elements within closed groups. The relations between them are still construed as unitary operations under which those groups are closed, whether defined as 'intersections' or not. And order is still imposed on those elements and groups in terms of normative degree - as more or less constrained. The new, uniquely idiomatic language of crisis era anthropology may *assume* a new thematic aspect (in the terms outlined by Bernstein 1999) but, once formalised, it reveals no greater concession to *real* individual experience, *real* negotiation among agents, or any other more nuanced, or sophisticated account of *actual* social complexity, than do the models of 1871.

This has direct bearing on the trajectory of anthropology in South East Australia, and its effects on the way that Indigenous/non-Indigenous population dynamics are construed, firstly by the distinction between Aboriginal and non-Aboriginal people, and secondly by the distinction between parts of the Aboriginal population. In the following section, the history of this trajectory is traced out, beginning with the inception of social anthropology in South East Australia in the late 19th century, its peak in the 1940s, and its subsequent abandonment as a desolate field, on the alleged basis that the Aboriginal population was no longer sufficiently distinct from the non-Aboriginal population to warrant anthropological attention.

2.3 Development of Group Models of Indigenous Australian Sociality: The KLRE Model

The pre-paradigmatic group theoretic assumptions that underlie much anthropological thinking, as established above, have taken a distinctive localised form in Australian anthropology. This manifests in what I refer to here as the 'KLRE model', in which culture is modelled as a superset comprised of subset 'laws & customs', kinship, language, religion, and economy. As with international developments in anthropological theory, group theoretic ideas in Australian anthropology have passed through the two major phases of pre-crisis and crisis era. Between the 1870s and 1970s these models were largely explicit, with group theoretic terms clearly albeit inconsistently defined, and with the aspiration to formal scientific representation overtly spelt out. Since the 1970s the models have become implicit, with much of the terminology rendered more obscure and metaphorical, and with the aspiration to formalism firmly quashed.

The concomitant development of Australian and international anthropological group theoretic thinking has been assured by the important place given to Australian field 'data' in successive anthropological languages throughout their various thematic shifts. As can be seen in the texts cited above, Indigenous Australians are consistently positioned as exemplary of a particular set of attributes, held to distinguish them among the world's people. The exact definition of these attributes varies from one specialised theme to the next, from kinship, to language, to religion, to economy, and so on. However the primal positioning of these 'culture elements' on three particular kinds of ordered scale has generally remained consistent. In the pre-crisis era, Indigenous Australian social cultures were held to manifest the 'most primitive', 'most remote', and 'most prescriptive' attributes of all human cultures, representing the extreme end of temporal, spatial, and normative scales, respectively. In the subsequent crisis era, where the temporal, spatial and normative scales have been reorganised, the extreme position of Indigenous Australians on each of these scales is rendered in more neutral, abstract terms. Thus in relation to temporal order, 'most primitive' is now 'most ancient', in relation to spatial order, 'most remote' is now 'most ecologically harmonious', and in in relation to normative order, 'most prescriptive' is now 'most traditional' (Bauman & Macdonald 2011; Austin-Broos 2011; Glowczewski 1999; Sutton 2010; Toussaint 1992).

Note that, although more abstract, the superlative terminology remains intact, illustrating the persistence of the ordering mechanisms that are used to impose a more explicit cause-and-effect grammar on otherwise implicit, unitary, and homogenizing group theoretic principles. The inherent constraints of the old unitary model are thus veiled with an ostensibly fresh explanatory veneer of a new suite of orders. Whereas 19th century anthropologists may have imposed this order on their group theoretic models to render them more intuitive in an era of high colonialism, many late 20th- and early 21st century anthropologists continue to impose an ordering on the same group theory. The unchanged logic of the group theoretic model, revealed by its formalisation, is obscured by the shallower logic of the newer metanarratives. More importantly, this continuing use of superlatives has maintained and reinforced Indigenous Australians as an exhausted subject, typically as 'the most

studied people on Earth' (Foley 1988; Beckett 2002), helping to entrench and protect an ossified 150 year-old anthropological model of Australian culture.

The current section traces the development of group-theoretic thinking in the construal of Indigenous Australian sociality, beginning in the late 19th century, and moving through various thematic re-orderings to the early 21st century. This developmental history is divided into the two phases corresponding with the early and late pre-crisis era, which in turn corresponds with the initial emergence of the KLRE model, followed by an intense period of numerous competing reorganizations.

2.3.1 Early Pre-Crisis Inception of Group Theory: Emergence of the KLRE Model

Australia's first popular anthropological monograph, *Kamilaroi & Kurnai* (Fison & Howitt 1880 - contemporary spelling: *Gamilaraay & Gaanay*), was published 9 years after Morgan and Tylor's foundational 1871 texts. The two British-born authors of *Kamilaroi & Kurnai* were the missionary Lorimer Fison and geologist Alfred William Howitt. Like Morgan and Tylor, neither Fison nor Howitt cited any any firsthand interviews conducted with the people whom their research claimed to represent. Instead they relied on answers to anonymised questionnaires that they distributed to educated colonial pastoralists squatting on the territories of Gamilaraay and Gaanay people, along with other anecdotal accounts. Fison and Howitt were taught the questionnaire technique by Morgan himself, as a supposedly scientific means of collecting data on kinship terminologies (Gardner 2009; McConvell & Gardner 2013). The goal of these questionnaires, as with Morgan's own research, was to collate tables of terms that could be compared with each other and with British terms, as a means of cataloguing relative positions in a standardised group theoretic schema.

Morgan had originally made contact with Fison in the late 1860s, when Fison, then working as a Methodist missionary stationed in Fiji, received an unsolicited request from Morgan seeking information on kinship terminologies in the region (McConvell & Gardner 2013). The data subsequently collected by Fison was ultimately included in Morgan's 1871 monograph, and he and Morgan went on to develop a continuing correspondence. After relocating to Australia in the early 1870s, Fison then introduced Morgan to Howitt, who had been employed as a 'Protector of Aborigines' in the South East Australian state of Victoria, and the latter two went on form a collaboration of their own (Fison & Howitt 1880). Morgan died shortly after the publication of *Kamilaroi & Kurnai*, but not before writing the book's preface.

On the basis of the support he received from Morgan, Howitt launched a career that lead him to be crowned the founder of Australian anthropology (Stanner 1972), writing a significant number of articles through the 1890s, culminating in another monograph in 1904, *The Aborigines of South East Australia*, before dying in 1908. Throughout this career, Howitt remained loyal to the methods taught to him by Morgan, conducting almost no interviews with Indigenous people himself, and relying

instead primarily on second-hand questionnaires solicited from British colonists. Although Fison did not pursue a career in the new discipline, both he and Howitt invested significant energy in establishing and maintaining correspondence with the foundational British social anthropologists of the time, including not only Morgan and Tylor, as well as sympathetic Australian novices Baldwin Spencer and F.J. Gillen. This effort was accompanied by an equally concerted defence against, and marginalisation of, other emerging Australian anthropologists. Most notably among these was Robert Hamilton Mathews and the coincidentally named John Mathews, who questioned the factual basis of Howitt's group theoretic models (as elaborated in Chapter 4).

Morgan announced the gifting of his own group-theoretic logic to Fison and Howitt in the introduction to Kamilaroi and Kurnai (Morgan in Fison & Howitt 1880:2-7 - see APPENDIX 1 for extended quote). Aside from the extreme racism evident in this treatise, notable even by late 19th century standards, the contours of Morgan's previously articulated 1871 model (Morgan 1871) can be clearly delineated. In the opening paragraph, Morgan first presents the temporal ordering of groups and elements as ranging between those of a 'lower condition' and 'less resistance' on the one hand, and the 'superior races' and 'more advanced tribes and nations' on the other (Morgan in Fison & Howitt 1880:2-7). Next, he introduces the core elements of his set theoretic taxonomy as 1) 'mankind', comprised of 2) 'tribes and nations', each of which in turn consists of; 3) 'man', each of which in turn bears; 4) the attributes of 'arts, institutions, manners, customs, and plan of life', etc. Although the specific terminology varies from that used in his 1871 earlier model, the same three subsets are still in evidence, now virtually indistinguishable from Tylor's own 1871 model (Tylor 1871), as a) 'Mankind', b) 'Nations', c) 'Man', and d) 'institutions, manners, customs' (see Section 2.2.1 above). After introducing these core elements, Morgan proceeds to introduce the 'uniform operation of the human mind' as the function under which elements in the each group are closed, and under which they are collectively closed as members of a single group 'mankind'. The ancillary temporal order of 'savagery<civilization' is then reinforced, and elaborated with a spatial order in terms of 'an organization transmitted to various descendants upon all the continents'. This model is represented visually at Figure 24 as follows, and formally at Equation 10.

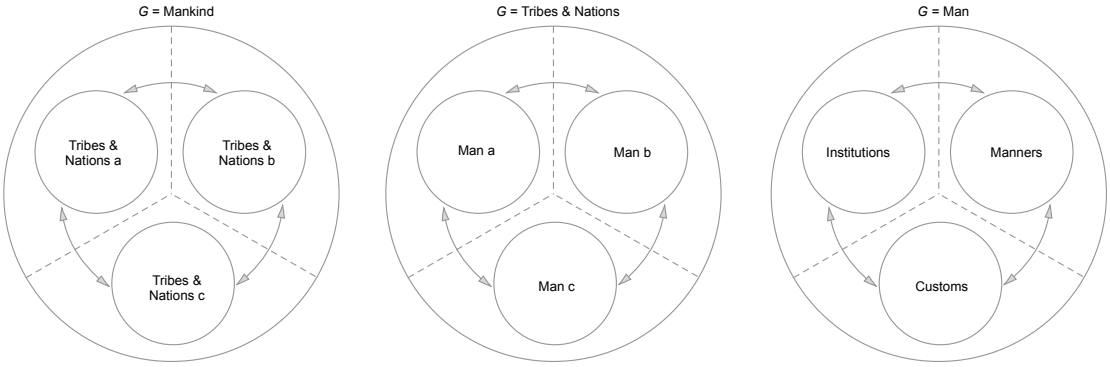
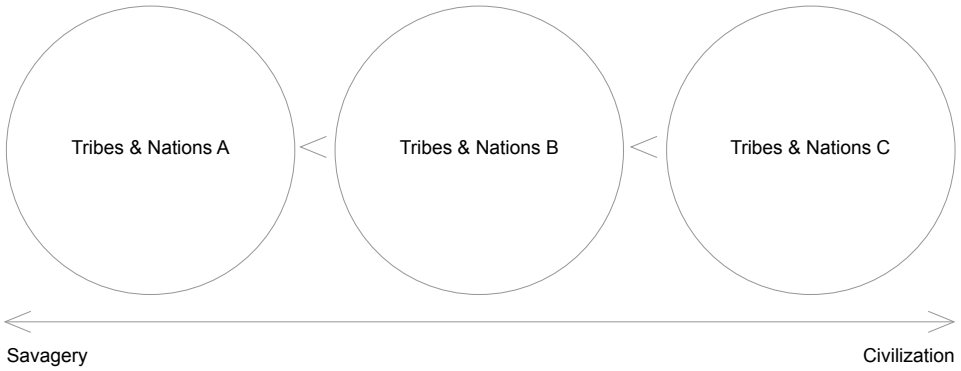


Figure 24: Diagram of the group theoretic model articulated by Morgan in his introduction to Fison & Howitt's (1880) '*Kamilaroi and Kurnai*', comprised of subsets all closed under the operation '↔', 'uniform operations of the human mind'.

The group 'mankind' is closed under the operation acting on a set of all 'tribes & nations'. The group 'tribes & nations' is closed under the operation acting on all men. The group 'man' is closed under the operation acting on all 'institutions, manners, and customs'.

$$Culture = \{Mankind \supset Tribes \ \& Nations \supset Man \supset \{ Institutions, Manners, Customs\}\}$$

Equation 10: Formalisation of the set theoretic basis for the model articulated by Morgan in his introduction to Fison & Howitt's (1880) '*Kamilaroi and Kurnai*'.



Equation 11: Diagram of the group theoretic model ordered by time '↔' where 'savagery'<'civilization', articulated by Morgan in his introduction to Fison & Howitt's (1880) '*Kamilaroi and Kurnai*'.

$$Time(Mankind)$$
$$= \left(\{Tribes \ \& \ Nations_a, Tribes \ \& \ Nations_a, Tribes \ \& \ Nations_a\} \middle| Time_{Savagery} \leq Time_{Savagery} < Time_{Civilization}\right)$$

Equation 12: Formalisation of Morgan's (1880) group theoretic model of 'Mankind', ordered by time, articulated by Morgan in his introduction to Fison & Howitt's (1880) '*Kamilaroi and Kurnai*'.

Fison and Howitt's *Kamilaroi & Kurnai* is divided into seven parts dealing with two elements in the third strata of the subgroup (viz. 'institutions, manners, customs'), namely religion ('legends') and kinship ('marriage in the group'), as well as two modes of ordering, including spatial ('the extent of the distribution of the organization'), and normativity ('severe penalties for violating the rule of marriage'). Neither language nor economy is given significant attention in this early instantiation of the KLRE model.

By the time of his 1904 monograph, Howitt had refined this model to the point where he was able to lay down the core elements and the homogenising function under which they were closed into a group theoretic product (Howitt 1904:41-42; 50-51 - see APPENDIX 1 for extended quote). Howitt here constructs a group theoretic model comprised of multiple 'nations', each of which is in turn

comprised of 'tribes'. Groups of nations are closed or 'bound together' under the common function of uniformity operating on each element within each group, fulfilling the conditions of 'a common relationship' (uniform kinship terminology), a 'common speech' (uniform language), and 'a definite tract of country' (uniform geographic distribution) (Howitt 1904:41-42; 50-51). The model is visualised in the diagram at Figure 25, while a formalisation is provided at Equation 13. Note that the entity termed 'definite tract of country' is presented with the properties of both an element within the set 'tribe', and a spatial metric that can be used to impose geographic order on the rest of the set. This paradox is elaborated below.

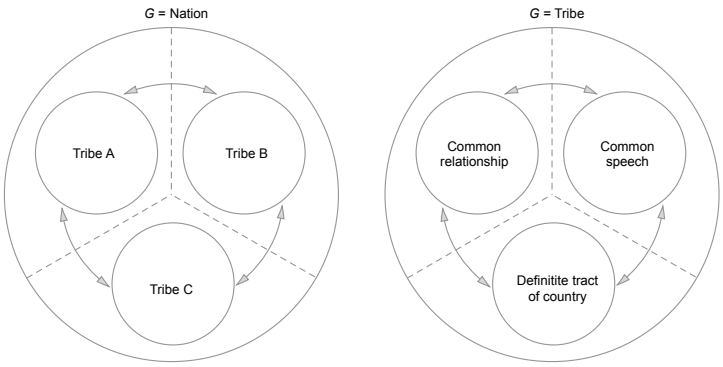


Figure 25: Diagram of the group model articulated by Howitt (1904) in 'Aboriginal Tribes of South East Australia', where elements in the group are closed or 'bound together' under the operation of '↔', 'commonality'.

$$G = (Abriginal\ Australia \supset Nations \supset Tribes \supset \{Kinship, Language, Ecology\})$$

Equation 13: Formalisation of the set theoretic basis for Howitt's (1904) group model of Australian sociality, where the group '*G*' comprises the superset 'Aboriginal Australia', in turn comprised of subset 'Nations', in turn comprised of subset 'Tribes', each of which is in turn comprised of subset kinship terminologies, languages, and ecology. Each subset is closed into a distinct group by the operation of 'uniformity', such that each tribe is bound together by ye uniformity of its constituent elements.

In the previously cited quote Howitt makes no mention of individual people. People are implicit as elements in the group 'tribe', as *bearers* of 'common relationships', *speakers* of 'common speech', and *occupiers* of 'definite tracts of country'. This is consistent with the abstraction of real people into metaphorical objects first employed in both Morgan's and Tylor's models. In the elaboration that immediately follows however, Howitt re-concretizes this abstraction (to a limited extent) by referring specifically to 'mothers', 'fathers' and 'sons' (Howitt 1904:43 - see APPENDIX 1 for extended quote).

Here Howitt's terminology becomes explicitly group theoretic, describing the bijection (or 'remapping') of the elements in the set 'tribe' onto what he refers to as two distinct 'lesser groups', which he also refers to as types of 'local division'. One of these group theoretic elements is termed

'social organization' (aka 'horde'), while the other is termed 'local organization' (aka 'clan'). 'Mothers', 'fathers' and 'sons' are explicitly mentioned as differentiated members of these two groups. This terminology lacks any isomorphism with autochthonous South East Australian terminology or models of kinship. It can nevertheless be visualised as shown at Figure 26, and formalised as shown at Equation 14clearly illustrating group theoretic axioms of closure and associativity.

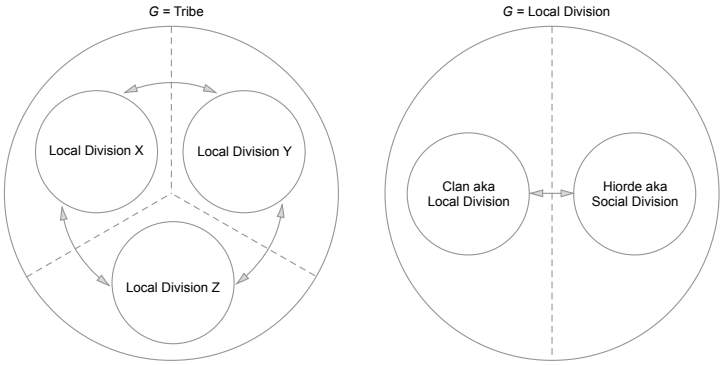


Figure 26: Diagram of Howitt's (1904) bijection of the tribe under the operation 'common relations' generates a further group theoretic object termed 'local organization', where '↔' is an abstracted process of 'group marriage' (i.e. marriage between groups, as distinct from the congruent process of marriage between actual people).

$$f(Tribe) \rightarrow (Local\ Divisions \ni \{Horde, Clan\})$$

Equation 14: Formalization of Howitt's (1904) bijection of implicit human elements in the set 'Tribe', onto the group 'Local divisions', closed under the operated '↔' 'group marriage', and composed of two permutable elements 'Horde' and 'Clan'.

After establishing the group theoretic basis of his model, Howitt proceeds to impose a spatial order onto each of its constituent subgroups, asserting that they have a geographic expression whereby each 'clan' and 'horde' forms 'a geographical division of a tribe'. As noted above in relation to other instances of ancillary re-orderings of group models, in an example of semantic remapping of the kind identified by Halliday & Matthiessen (2006), Howitt misidentifies the *ordering* of these elements as another *element* within the encompassing subgroup 'tribe'. This seems intuitively logical if geographic distribution is viewed as an *attribute* with the same status as other attributes, such as the subscription to a particular kinship terminology, or speaking of a particular language. In other words, where a tribe is defined by its members' uniform exhibition of a single attribute, such as subscribing to a single kinship terminology, or speaking a single language, it *seems* logical to include geographic distribution of the tribe as a similarly uniform attribute insofar as it enables Howitt to say that individuals can be grouped according to their occupation of a continuous geographic region. However, whereas language-use, and subscription to a particular kinship terminology impose no particular order on the links between individuals within a group, geographic distribution *does* impose

order. This is because geography has an inherent *metric* function, rather than the *unitary* function of a single attribute. Whereas a particular named language, such as 'Kamilaroi or 'Kurnai', imposes a unitary function on those who speak it by allowing them to be closed or 'bound together' under the operation of that function (e.g. 'person *x* speaks language *y*'), geography imposes no such closure. Geography, as a measure of space, is not a unitary attribute, but an extensible measure in the same way that time is an extensible measure. This is what allows us to say that something or someone is 'closer' or 'further way', or exists 'earlier' rather than 'later'.

Theoretical attributes that are used to close groups under a uniform function have no such gradation - they either apply or do not apply. One either speaks a language or does not speak it; one either uses a kinship terminology or does not use it - there is no degree. By misconstruing space as a unitary attribute in his model of sociality, Howitt highlighted the inconsistency of the group theoretic logic that he had inherited from Morgan. Through his correspondence with people familiar with Aboriginal society (albeit British colonists rather than Aboriginal people themselves), Howitt had *intuited* that Australian sociality incorporates an important and systematic geographic dimension, however because his commitment to Morgan's group theoretic model superseded his interest in making empirical observations, Howitt was unable to amend that model in order to incorporate it in a congruent way.

The effect for Howitt in his theory-making efforts was that he discovered an anomaly manifest in the contradiction between modelling sociality as abstract groups, and modelling the distribution of those groups in geographic space as a congruent ordering. However, because of his lack of formal training, combined with his exclusion of empirical data, he was unable to identify this anomaly as the product of a mismatch between theory and observation, resulting in exactly the kind of inconsistency described by Leaf & Read (2012), and the 'paradox' described by White & Denham (2008). As Kuhn (2012[1962]) describes with regard to natural scientific anomalies, Howitt likely recognised this mismatch (despite misunderstanding its causes), and attempted to compensate by *synthesizing* congruence out of an ill-suited model - tacking on other ideas (i.e. treating geographic space as both a group theoretic element, *and* an ancillary ordering mechanism), rather than amending the model itself (see discussion of Kuhn in Chapter 1).

By 1904 Howitt had formally added the final elements of economy and religion to those of kinship and language in the group theoretic template, establishing the foundations of KLRE model that would come to dominate Australian anthropology for the remainder of the century. Howitt linked variations in economic organization and ecology to his better rehearsed ideas on kinship with direct regard to George James Frazer (1890), who by now had joined the pantheon of recognized disciplinary founders, together with Morgan and Tylor (Howitt 1904:153-155 - see APPENDIX 1 for extended quote). Thus was completed the set of key elements from a which a group theoretic model of Aboriginal culture could be bijected: Kinship, religion, language, and economy were now established

as the abstract subgroups of each group theoretic 'tribe' object, closed under the operation 'uniformity' (see Figure 27 and Equation 15), and veiled with an ancillary order of time and space in terms of the spread of those tribes across different ecological zones. The result was that while some became trapped in a permanent state of temporal 'primitivism' by this model, some lucky others were liberated into a state of 'advancement'.

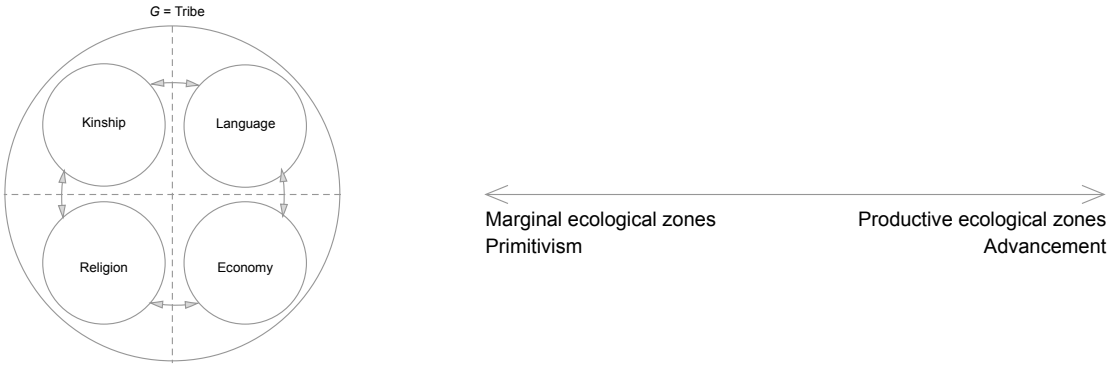


Figure 27: Diagram of Howitt's 1904 formalization of the group theoretic KLRE model, comprised of the elements Kinship, Language, Religion, and Economy, closed under the common operation '↔', 'uniformity' (left), accompanied by his ancillary ordering mechanism for explaining variability among tribes (right).

$$f(Nation) \rightarrow (Tribe_x \ni \{Kinship_x, Language_x, Religion_x, Economy_x\})$$

Equation 15: Formalization of Howitt's (1904) model of a 'Tribe' as a bijection on the set 'Nation', compo sed of the elements Kinship, Language, Religion, and Economy, closed under the common operation '↔', 'uniformity'.

It is important to note that, with the exception of kinship terminologies, nowhere in any of his journal articles or monographs does Howitt provide any visualisations or formalisations of his or anybody else's theoretical modelling. It is equally important to note that nowhere does Howitt publish any actual instances of the patterns of social interaction that he describes, neither examples of language being spoken, nor of kinship terms being used, nor of religious myths or rituals being recited by real and specific individuals, nor of actual geographic movement of real and specific individuals. The net effect is that readers of his texts are left with no demonstrable evidence as to the basis of any of his assertions. Although he refers frequently to his non-Indigenous intermediaries, whom he names, and to 'the natives' with whom these intermediaries in turn spoke or elicited responses to questionnaires, there is no mention of any specific Indigenous people doing or saying any of the things that Howitt claims that they did. In short, in the terms provided by Houseman & White (2013), and Leaf & Read (2012), there is no observable data against which to seek out isomorphism with Howitt's modelling.

This dearth of evidence cannot be attributed to the conventions of late 19th century publication, since other articles of the era, including those of William Ridley, R.H. Mathews, among others, as

discussed in Chapter 5, *do* include such data. Nor can the absence of published data be overcome by examining Howitt's personal notes and correspondence, where one may expect to find it, since apart from lists of kin terms these documents do not contain any specific observations either. The facts are that, firstly, Howitt rarely if ever spoke to the Indigenous people about whom he wrote. Secondly, very little of the correspondence that he received from non-Indigenous intermediaries contains specific information about real instances of language use, kinship terms being applied to real people, religious ceremonies being observed, or of people's actual geographic movements. In the few instances where there *are* references to the specific statements by particular Indigenous people, those people are never named, making it impossible to identify them in other sources and thereby cross-reference Howitt's accounts of their statements. Thirdly and finally, Howitt, as instructed by Morgan, dispatched questionnaires without any facility for including people's personal details or circumstances, such as where they lived, who their parents, spouses or children were, or even their own names (McConvell & Gardner 2013), thus rendering the elicited data almost useless as a measure of real-world sociality.

This approach was not arbitrary or accidental, but entirely in keeping with the foundational pre-paradigmatic definition of anthropology advocated by Morgan (1871) and Tylor (1871), that human culture consists of generalised unitary principles that exist 'beyond' the particular instances of individual people, as clearly articulated by those authors themselves, and by Radcliffe-Brown, Lévi-Strauss, and others after them (see Section 2.2 above). This notion is precisely that which was inherited by Howitt's own intellectual heirs in Australia, and carried over into their research on Indigenous sociality during the remains of the 20th century.

2.3.2 Late Pre-Crisis Consolidation of Group Theory: Reorganization of the KLRE Model

Verdon & Jorion (1981) provide a detailed history of the development of these group theoretic principles among Howitt's Australia heirs, including Radcliffe-Brown himself, Radcliffe-Brown's successor at the Univeristy of Sydney, Adolphus Peter Elkin, followed by Elkin's various students, including Ronald Murray Berndt, Lester Hiatt, Mervyn Meggitt, W.E.H. Stanner, and others, spanning the century of Australian anthropology's pre-crisis era until the 1970s. Verdon & Jorion's (1981) account clearly illustrates the theoretical contortions, contradictions, and failures that accompanied this history, as each successive heir attempted to reconcile their increasingly more specific and personalised field data with the limitations of the KLRE model, apparently without realizing the formal basis of the principles that they were using. The result is a 75-year geology of labelling, un-labelling, and re-labelling groups, subgroups and elements, accompanied by a constant rearranging of the temporal, spatial, and normative ordering of those elements in an effort to render metaphors congruent with observed social behaviour.

After Elkin replaced Radcliffe-Brown as the Chair of the University of Sydney's anthropology department in 1932, Radcliffe-Brown became the preferred target of criticism among Elkin's students,

or the 'Sydney School' as they came to be known (Verdon & Jorion 1981). Radcliffe-Brown thus came to eclipse Morgan and Howitt as the perceived original source of the group theoretic framework, to the extent that he was even attributed as the creator of the terms 'horde' and 'clan' (terms which Radcliffe-Brown adopted and attempted to redefine, but certainly did not invent, as illustrated in the previous section, and discussed in detail in Chapter 3).

Verdon & Jorion (1981) characterise the target that Radcliffe-Brown presented to the Sydney School in terms of his adoption of the asserted link between group identity and geographic distribution, as follows:

> *"In his 1931 essay, Radcliffe-Brown indicated what he understood by 'social organization' [in Australia], that is, the manner in which groups are formed: 'Individuals are united together into groups on the basis of sex and age, of community of language and custom (tribe), of possession and occupation of a territory (horde), and on the basis of kinship and marriage (family, clan, section, moiety)'. He thus defined groups by the elements which bind their members together, elements such as sex, language, territory, the various 'social glues' which generate different types of grouping. 'Patrilineal descent', for the clan, and 'territory', for the horde, are both elements which generate solidarity or internal cohesion and thereby act to form groups and to differentiate them from other groups. This representation, admittedly, made it difficult to find any raison d'être for the existence of the clan, except that of increasing the horde's (presumably loose) unity through additional means. Superimposed onto the horde, the clan only fostered the local group's solidarity through descent and symbolic representation. In our opinion, this understanding of social organization in terms of solidarity and differentiation blurred the articulation between clan and horde, and also led Radcliffe-Brown to a relatively rigid position on the problem of boundaries as necessary to generate differentiation" (Verdon & Jorion 1981:93).*

The continuity of Howitt's group theoretic framework in Radcliffe-Brown's thinking is clear, where 'sex, language, and territory' function as 'social glues', effectively closing sets of people into groups under a common exhibition of those attributes. Thus 'clans' are closed under the operation of common male gender (patrifiliation), while 'hordes' are closed under the operation of common geographic distribution (territoriality). Verdon & Jorion's concern about Radcliffe-Brown's ' rigid position on the problem of boundaries', alludes to the incompatibility of an ordering mechanism (geographic distribution) with an inherently *un*ordered group theoretic framework, although they do not spell this out. The disagreement that the Sydney school had with Radcliffe-Brown hinged on the link between geographic distribution on the one hand, and various elements in the group theoretic framework on the other.

Among the various relabelled groupings that emerged between Radcliffe-Brown's departure from Australia in 1932, and the end of the pre-crisis era in the 1970s, were

- 'Social groups' (closed under uniform interaction, e.g. Piddington 1935; Berndt 1959; Meggitt 1964);
- 'Local groups' (closed uniform presence in a given geographic region, e.g. Elkin 1934; Berndt 1959; Hiatt 1962; Meggitt 1964; Stanner 1965);
- 'Ownership groups' (closed under uniform ownership of a given geographic region, e.g. Berndt 1964);
- 'Occupation groups' (closed under uniform occupation of a given geographic region, e.g. Hiatt 1962; Berndt 1964);
- 'Ritual groups' (closed under uniform participation in a given ritual, e.g. Elkin 1969; Hiatt 1962; Berndt 1974)
- 'Economic groups' (closed under uniform participation in a an economic process, e.g. Elkin 1949; Hiatt 1966);
- 'Spiritual groups' (closed under uniform belief in a particular cosmogony, e.g. Piddington 1971);
- 'Legal groups' (closed under uniform adherence to a particular jural regime, e.g. Meggitt 1966),
- 'Estate groups' (closed under uniform association with in a given geographic region, e.g. Stanner 1960, 1963, 1965);
- 'Patrifillial groups' (closed under common patrifilial descent, e.g. Elkin 1950; Hiatt 1962, 1965; Shapiro 1969; Piddington 1970);
- 'Matrifillial groups' (closed common matrifilial descent, e.g. Elkin 1933);
- 'Residential groups' (Shapiro 1973);

... and so on.

In every instance, groups are closed under the relevant operation so that nobody comes in, and nobody goes out of the group in question. Where terms like 'recruitment' or 'adoption' are used in conjunction with definition of the group, they do not refer to any kind of permeability that might be considered to breach the axiomatic closure of a group, but rather to an identity element within each group, such as 'geographic area' for local, ownership, occupation, residential, and estate groups; 'ritual' for ritual groups, 'myth' for spiritual groups, 'law' for legal groups, 'father' for patrifilial groups, 'mother' for matrifilial groups, and so on. These entities form identity elements by leaving the members of a group 'unchanged' by their interaction with it - by 'marking' group members according to their common association with it. 'Recruitment' and 'adoption' here refer to 'the conditions of membership', not to an actively variable process whereby a real person may be a member of a group at some point in time, but not at another, and not to a process whereby an individual may exercise any real agency as a person cognizant of their own future potential.

The theoretic axioms of associativity and commutativity of elements are inherent in all of these representations too, but are masked by ancillary orderings, imposed in various attempts to render each abstract model congruent with observed behaviour, such as by using metaphoric gradients of 'core' and 'periphery', 'strict' and 'permissive', 'certain' and 'uncertain', so as to give the impression that the group metaphor fit the complexity of actual human behaviour.

It was during this peak in the late pre-crises era debate, that the problem with trying to treat geographic space an element *within* a closed group became most pronounced. Verdon & Jorion credit Shapiro with first distinguishing between "a 'core area', which consists of the totemic sites, and the surrounding land, 'whose identity is increasingly uncertain with distance from the core'" (Shaprio 1979 in Verdon & Jorion 1981:93). Verdon & Jorion suggest that this new realisation, albeit still informal in terms of its failure to distinguish an ordering function from a group theoretic function, was amplified by the Sydney School's redefinition of the latter as a demonstrable *activity*, rather than an abstract *property* (Verdon & Jorion 1981:94). In other words, Elkin, Meggitt, Berndt and Hiatt redefined the function under which a group could be closed as 'those who perform a particular activity', rather than 'those who exhibit a particular attribute'. The group theoretic model remained intact, but its axioms were weakened, as White puts it, in order to encompass real social activity, and thus 'make the model more plausible' (White & Denham 2008:3).

Verdon & Jorion themselves do not identify the group theoretic model as the source of the problem, stating instead that:

> *"[A] more accurate description of Aboriginal social organization would ultimately depend upon a systematic broadening of the concept of group which has already enabled Australian ethnographers to overcome limitations in Radcliffe-Brown's interpretation. By using a conceptual framework which seeks to transcend the limitations of this functional notion of group, we hope to clarify the analysis of Australian Aboriginal social organization a bit further" (1981:94).*
>
> *"[W]e have relied on a more systematically 'operational' definition of group (a notion based on what groups do, what they perform)" (Verdon & Jorion 1981:94).*
>
> *"We ... define groups in the simplest manner possible, by postulating that a group presupposes one, and only one, type of activity, and at least one (but often many) criteria of membership. We therefore set aside any idea of 'corporateness' or 'ownership' in the definition of group. If corporateness denotes collective action and solidarity, we simply take it for granted and consider it unproblematic" (Verdon & Jorion 1981:94).*

Jorion elsewhere links this explicit construal of formal group theory to the innovations of Lévi-Strauss and Weil in 1949 (Jorion 2000), so it is clear that in 'repeating basic assumptions of this model'

(Verdon & Jorion 1981:94) he and Verdon are conscious of the axiomatic constraints of group theory as a model of sociality.

Verdon & Jorion's (1981) solution to the impasse is not to question the validity of the group theoretic basis of the model, but instead to revisit normativity as a mechanism for ordering elements within the groups in question. In this instance, variation in normativity, or prescriptive strength, is construed as 'degrees of ownership', where 'rights to a territory' are sequentially ordered along a scale from maximal to minimal according to who '*is* allowed' and who 'is *not* allowed' to exploit a particular resource. For Verdon & Jorion (1981) this provides a new basis on which to define groups of people, by establishing which individuals have which rights to which resources, and grouping them accordingly.

Verdon & Jorion (1981) claim to pre-emptively avoid any dispute about the nomenclature of the group by referring to it as a 'corporation' with respect to the resource in question. Simultaneously, since all resources are supposedly natural, all such groups have a geographic attribute corresponding to the resource in question (Verdon & Jorion 1981:95 - APPENDIX 1 for extended quote). Verdon & Jorion (1981) thus go so far as to distinguish groups 'defined with respect to ownership', from groups 'defined with respect to activity', and to name the latter as the true function under which groups should be closed. Meanwhile, the novelty of their ordering approach is to make the identity element more specific, by naming it as 'a particular type of resource'. The original problem of how to force the ordering principle of geographic space into a more convincing definition as a unitary group theoretic element is thus (apparently) resolved by specifying 'resource' as the identity element. Since resources, unlike people, are immobile, they have a single geographic attribute. By defining the identity element in a group as a type of tangible *resource* rather than as less tangible type of personal *attribute*, like totem or filiation, Verdon & Jorion make the group model seem more coherently explanatory or, in their words, 'plausible' (Verdon & Jorion 1981:94), irrespective of whether that group is called a 'tribe', 'clan', 'horde', or 'corporation'.

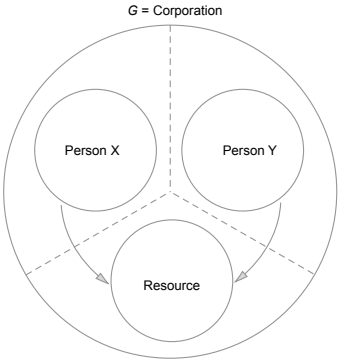


Figure 28: Diagram of Verdon & Jorion's (1981) model of 'corporate rights', construed as a set of people and a given resource closed under the uniform operation '↔', 'resource access'. Membership of the corporation is thus 'defined' by equivalent rights to access a given resource.

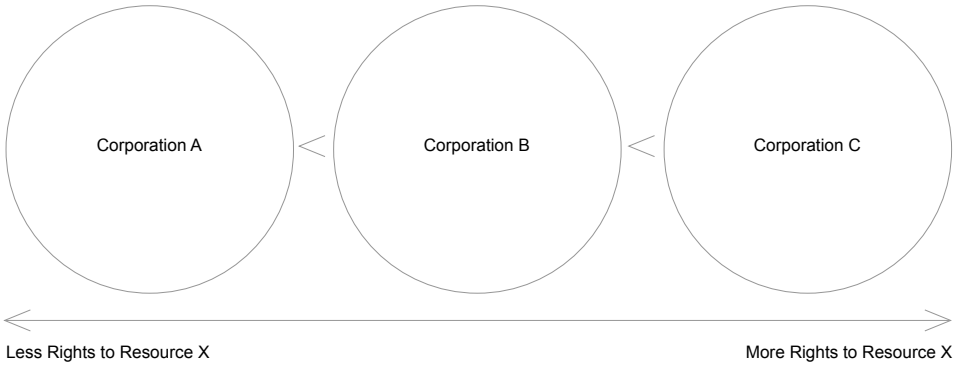


Figure 29: Diagram of ancillary ordering of corporations according to 'degrees of access right'.

As with Howitt's original encounter of the contradiction in trying to construe a spatial metric as a group element (Howitt 1880), Verdon & Jorion (1981) also encounter a contradiction in trying to distinguish between a group and a 'category' - the term they use to name a given point on a spectrum of resource access rights, where some people have 'more rights' and others have 'less rights'. Their attempt to transcend this contradiction drives them to compare the hunter-gatherer resource regimes in Australia with nomadic-pastoral resource regimes in East Africa. This allows them to introduce a metaphoric object that they term 'a crowd' (Verdon & Jorion 1981:96), which is in their definition a group, but one that is not closed under the common function of a single kind of resource right to a particular geographic area (Verdon & Jorion 1981:96 - see APPENDIX 1 for extended quote).

In pursuing this effort to define a group as both simultaneously closed and not closed, and both simultaneously ordered by a spectrum of intensity (what they call a 'category'), while simultaneously encompassing those points as elements in a group, Verdon & Jorion (1981) employ an elaborate metaphor of 'concentric circles' (see Figure 30). This is clearly another kind of ordering, just as degrees of geographic distance, temporal depth, and normative prescription, are also kinds of ordering.

In the course of giving their new ordering an 'operational' explanatory power, congruent with observable social behaviour, Verdon & Jorion use it to measure degrees of religious knowledge or what they call 'spiritual danger' (Verdon & Jorion 1981:99). Similarly to the way earlier anthropologists couple multiple types of ordering, such as the way in which Lévi-Strauss couples 'primitive' age with 'elementary normativity', Verdon & Jorion couple 'degrees of knowledge' with 'degrees of resource rights' (Verdon & Jorion 1981:100 - see APPENDIX 1 for extended quote).

Verdon & Jorion (1981) go on to extrapolate that the unitary or corollary coupling between these two kinds of ordering, 'gradient of ownership' and 'gradient of knowledge', extends to define groups of people closed under common kin terms, distributed between the inner and outer rings of the concentric circles shown at Figure 25. This clarifies the definition of the previously described 'crowd' as a superset, subsets of which are bijected into closed groups of varying 'exclusivity'. Verdon &

Jorion (1981) construe each group as a successively more exclusive 'concentric circle', ordered by consistent degrees of 'esoteric knowledge', 'spiritual danger', and 'rights of ownership'.

As with previous instances of ancillary ordering applied to group theoretic objects, from Morgan (1871) and Tylor (1871) onwards, sociality is construed in the uniform membership of closed groups, while causality is construed in the sequential ordering of those groups. The visual analogy of concentric circles ordered by generalised 'core' and 'periphery' in Verdon & Jorion's (1981) model, does not in any way undermine the self-consistency of underlying group theoretic closure. It still precludes the emergence of any kind of complexity that might arise via a deviation from either closure or order. More specifically, there is no possibility of variation in group membership or inconsistency in ordering, such as might occur when, for example, an individual holding of more esoteric knowledge also happens to bear lesser rights, or when an individual loses or gains group membership over time, rather bearing the same rights consistently throughout their life.



Figure 30: Diagram of Verdon & Jorion's (1981) model of a 'crowd', consisting of a set 'S', from which multiple groups are bijected under the operation of uniform rights of access to a given resource. Verdon & Jorion (1981) construe each group as a successively more exclusive 'concentric circle', ordered by consistent degrees of 'esoteric knowledge', 'spiritual danger', and 'rights of ownership'. Note that their visual analogy of concentric circles ordered by 'core' and 'periphery' is consistent with the group theoretic principle of closure.

Throughout the late pre-crisis period of the 1960s and 1970s, Australian social anthropology experienced a bourgeoning use of visual metaphors and analogies of the kind deployed by Verdon & Jorion (1981), which quickly overtook more explicit debates about how to define groups in axiomatic terms. This shift accompanied the transition from pre-crisis to crisis era, and with it, a shift from the specialised language of 'structure' to a newly specialised language of 'meaning'. In the idiom of this new language, pioneered by Gupta & Ferguson (1992) among others (see above), the object was to develop a model, not of social organization, but of 'mind' and 'ways of being'. To this end, Australian social anthropologists began to focus increasingly on the question which visual metaphor or analogy was 'most like' the analogies and metaphors that Indigenous Australians themselves were supposed to 'think with'. The previously unambiguous attempts to resolve inconsistencies in the KLRE model were now subsumed under new, more tacit language.

2.3.3 Crisis Era Subsumption of Group Theory: The KLRE Model Becomes Tacit

Verdon & Jorion (1981) represents one of the last pre-crisis attempts to reconfigure group-theoretic models as a congruent explanation of Indigenous Australian sociality. The end of the pre-crisis era in Australian social anthropology was finally marked by the publication of Schneider's 1984 '*Critique of the Study of Kinship*' (Schneider 1984), along with its warm reception by Australian practitioners (Jeremy Beckett pers. comm. 2009). Myers' (1986) *'Pintupi Country, Pintupi Self*' was the first major Australian work of the new era, demonstrating a significant effort in avoiding group theoretic principles by personalising the experiences of Pintupi people as culture bearers, rather than as anonymous vehicles of group theoretic attributes (Myers 1986). This text was accompanied by others such as Morphy (1988), Williams (1986), and Yengoyan (1984; 1986), which manifest a blossoming of more intimate, nuanced, and ostensibly congruent accounts of people 'living *in* culture' (Ingold 2002), rather than simply following what had previously been glossed as the prescriptive 'rules' of generalised mechanisms existing beyond the realm of human agency. As mentioned above, these studies made frequent use of new themes of 'flexibility' and 'agency', with thematically consistent group theoretic elements, especially the functions under which groups were closed, such 'selfhood', 'personhood', 'negotiation', 'contestation', and so on.

Fortunately in some regards, although unfortunately in others, the emergence of this new approach was at variance with the formal requirements of the new 1976 Aboriginal Land Rights Act (ALRA), of Australia's Northern Territory administration. As detailed in the introduction to this thesis, the ALRA specifically required the formal definition of social groups in which land tenure could be legally vested, together with the formal definition of land to which those rights applied. Although comprising a commission-based process with a relatively low probative onus, the ALRA nevertheless enshrined these group theoretic concepts in defining the evidence it required for successful land claims.

Since the ALRA was, to a large extent, developed with the input of social anthropologists during the late 1960s (Hinkson & Beckett 2008), anthropologists working under the Act faced few theoretical challenges in using existing group theoretic models as a basis for explaining group membership. Rather, the problem that emerged from social anthropologists' engagement with the formal demands of the ALRA was in providing *formal* explanations of how groups of Indigenous people instantiated discrete, unitary links of the kind described by Morgan, Howitt, Radcliffe-Brown and the Sydney School, with discretely bounded areas of land. The newly de-formalised, 'anti-cultural' language of social anthropology, with its promotion of personalised experiential 'journeys', and subordination of prescriptive rules, revealed much more complex patterns of social interaction, especially in geographic terms, than the late pre-crisis modelling had acknowledged.

Although some of this 'newly discovered' social complexity can be attributed to the more permissive and agentive language of the crisis era, the bulk of the revelation is more likely due to the demand for

data made by the Land Rights Act. Under this legislation, Australian social anthropologists were required, for the first time in the history o their discipline, to write reports that focused not on generalized abstractions, but on real people with real names and relationships, real locations in geographic space (primarily in terms of their links to sacred sites), and the real passage of time over which people interacted with each other, performed ceremonies, and travelled. Despite the variation between the formal demands of the ALRA and the anti-formalist orientation of the new anthropological language, the low probative demands of the Act, together with the pre-crisis training of many of the anthropologists involved, meant that Australian social anthropology as a whole was able to carry on using the group theoretic models of its pre-crisis configuration.

By the early 1990s, when Australia's federal Native Title Act (NTA) was introduced, the theoretical friction that had built up during the 1980s between the crisis era language of social anthropology and the formal requirements of the law, had reached a terminal level. As the introduction to this thesis spells out, the NTA, as a litigious court-based process involving solicitors, barristers, judges, witnesses and experts, placed a much greater probative burden on anthropological evidence than the ALRA had done previously. Such is this burden, that under the NTA, social anthropologists are now required to explicitly demonstrate an objective disinterest in the outcome of an Indigenous community's claim, such that any detection of 'advocacy' by a judge can result in dismissal of their evidence altogether (Baumann 2010). Furthermore, the NTA's litigious framework allows respondent stakeholders to call their own anthropological experts to give evidence against that of Indigenous claimants. This has resulted in social anthropologists facing off against each other in court, and attempting to undermine each other's research.

Under the NTA, scrutiny of theoretical consistency is now at an all-time high, both in the eyes of the judiciary, and in the eyes of the Australian anthropological community itself. As Verdon & Jorion (1981) point out, Australian anthropologists have never been strangers to theoretical disputes, with each new generation from Howitt (1880; 1904) onward, adhering closely to the pattern identified by Bernstein (1996; 1999), of attempting to introduce variations on, if not to completely overthrow, existing languages with new sets of terms and idioms. However, the Native Title Act has imposed a new kind of pressure on this serial code, especially as senior and respected anthropologists have begun to have their evidence rejected by the Federal Court for insufficiently demonstrating coherent Indigenous social organization (Sansom 2007). This treatment by the judicial system represents an unprecedented challenge to the intellectual and political authority of Australian social anthropology.

One of the earliest signs of this new, more intense dynamic emerged from a dispute between two veteran specialists on the Yolngu speaking population of North East Arnhem Land, Nancy Williams and Ian Keen. By the mid 1990s, both Williams and Keen had worked with the Arnhem Land population for several decades, developing intimate personal relationships with Yolngu people,

assisting with land rights claims, and publishing a number of innovative studies on kinship, language, religion, and economy (Keen 1982, 1984, 1990 *inter alia*; Williams 1982, 1986, 1994 *inter alia*).

In 1995 Keen launched a major critique of what he called 'group metaphors' used in the modelling of Indigenous Australian sociality, employing a detailed survey of the diverse and inconsistent range of metaphors that had been used in previous publications on Yolngu social organization, including the work of Williams among others. Keen advocated for the replacement of these literary devces by more sophisticated metaphors of 'strings'. He introduces his argument as follows:

> *"I argue that, like the enacted cultural constructs on which they draw, anthropological terms such as 'clan' depend on relatively simple central metaphors. In contrast, the constitution of Yolngu 'groups,' as analyzed here, involves complex imagery that cannot be reduced to a few key metaphors. Nevertheless, the kinds of tropes that are embedded in the framing of groups and group relations - as well as the metaphors that Yolngu people themselves employ to describe relations among persons and groups - are quite different from those on which the anthropological constructs depend. Anthropology's descriptive language should reflect those tropes more closely. Concepts such as lineage, clan, descent group, and corporate group depend on images of segmentary structure, external boundaries, and taxonomic hierarchy. These constructs go hand in hand with concepts of land and country, which also entail spatial metaphors of enclosure and boundaries and which imply hierarchies of small bounded places contained in larger ones of a different type. None of these tropes fit Yolngu modes of 'group' identity and relations, which involve images drawn from the human body and plants, and beliefs about ancestral journeys and traces. Far from being constituted by enclosure within boundaries, Yolngu 'group' identities, like those of place, extend outward from foci. Connections among such identities are not those of enclosing sets but open and extendible 'strings' of connectedness" (Keen 1995:502)*

Throughout the article Keen makes frequent references to metaphors that he argues are more faithful to those maintained by Yolngu themselves, including 'strings', 'webs', and 'networks'. Although Williams was only one among a number of anthropologists cited by Keen in the course of his criticism, including Berndt, Hiatt, Morphy, Myers, and Shapiro, among others, Williams responded in 1999 with a very specific and coherent defence of group theoretic metaphors:

> *"I argue to the contrary that Yolngu people [do] use constructs that may be mutatis mutandis more appropriately likened to the English-labelled concepts 'descent group', 'lineage', and 'clan', and that 'clan' does less violence to Yolngu concepts than 'strings of indeterminate length and the like" (1999:126).*

Importantly, Williams enlists an extended quote from the museum anthropologist Bouquet (1997), to reinforce her argument:

> *"[G]enealogy is methodologically prior to category, and has been ever since Rivers transformed it into an explicit method of anthropological inquiry. The genealogical method is an instrument or tool for collecting information with the visualization of genealogy in diagrammatic form one of the crucial steps in that transformation. It is indeed one of the steps in* ***moving from the concrete level of named individuals to the abstract level of social relation****s. That transformation can be seen in the difference between diagrams in which named individuals are present and those in which triangles, circles and other symbols have already replaced them—for a variety of purposes (composition of households, inheritance of property, names, etc.) and not just terminological analyses" (Bouquet 1997 in William 1999:127 - emphasis added).*

The most important part of this quote for Williams' rebuttal of Keen's advocacy of network analogies, is the claim that 'named individuals' are 'more concrete' than the social relations between those individuals, which are said to be 'more abstract'. This represents a new form of ordering, not previously seen in Australian anthropologists' attempts to construe group theoretic models as congruent with observable human sociality. As discussed at length above, all previous attempts had focused on orderings in space, time, and normativity, with the effect of attributing causality to structures that did not otherwise exhibit any causality. So what explanatory power is derived from ordering people and relations on a scale of 'more' to 'less' abstract, as Williams does? The answer to this question lies in the terms on which Keen and William argue.

The argument between Keen and Williams is not about the pre-paradigmatic status of group theory as a framework for explaining sociality, but about how to more effectively *represent* group theory. By the mid 1990s, the primary focus of anthropology was no longer on how to order the elements of group theoretic models themselves, but on how to order their *representation* in anthropological texts. It is significant that Bouquet is an anthropologist of museum collections, a branch of the discipline that focuses on constructing compelling representations of artefacts that have been decontextualized from their original social setting. Bouquet's imposition of an order upon people and relations, that not only separates them from each other, but also construes one as more abstract than the other, can only be persuasive if people and relations are treated synoptically, as different classes of metaphorical object, distinguished by different attributes, instead of dynamic and agentive process unfolding through time. This is a perfect distillation of the incongruence that group theory imposes on explanations of human sociality, by separating people from social relations. Williams' endorsement of this view is given in a further quote:

> *"[In] my conceptualisation of group, and the useful distinction between group and category ... I use the terms 'set', 'social category', and 'social group', [such that] they have the following meanings: set refers to a collection of entities that are classed together for some purpose because of one or more attributes they share; a social category is a category of people 'grouped conceptually because of some socially relevant features they share'; and a social*

> *group is people grouped through regular interaction and who are corporate in one or more respects. With respect to a Yolngu title-holding group's relationship to land, that is, the constitution of the clan, it is corporate (in the sense defined above) with respect to the joint interests of its members in an area of land and/or waters, and their interaction is predicated on their joint interests" (Williams 1999:136).*

At first sight, Keen's paper might appear to construe a genuine departure from the group theoretic principles that had at that time dominated Australian anthropological thinking for 115 years since the publication of Fison & Howitt's '*Kamilaroi and Kurnai*' (Fison & Howitt 1880), and which Williams so clearly captures. The most important indication that this is not the case, lies in the explicit approach that Keen takes to assigning validity to anthropological models on the basis of whether the *type* of metaphor used by one kind of model is 'more accurate' than the type used by another model. Although, like Williams, Keen emphasises a commendable number of direct citations of Yolngu people explaining kinship, linguistic, religious and economic terms, he refers to no observed instantiations of these terms in Yolngu people's social interaction. Phrased more explicitly, Keen includes a rich, albeit ad hoc, collection of elicited statements from Yolngu people about the meaning of these terms, but does not compare these statement with any measurable observations of social interaction, which might show *how* they function as means of understanding and coordinating such interaction. Rather, Keen's argument is focused on the relative merits of three different sets of metaphor: one belonging to Yolngu, and two belonging to anthropologists (himself and Williams).

Williams, in her response to Keen, takes the same approach, beginning her article with an extended quote from Yolngu writer Merrkiyawuy Ganambarr (Ganambarr 1994). This quote bears repeating here because of the striking absence of any kind of group theoretic concept whatsoever:

> *"I want to explain to you what yothu yindi really means. You have probably heard about the rock band Yothu Yindi. Yothu Yindi* ***is really a relationship term****. The relationship holds for people, land and all that we see about us, for things such as animals, plants, wind, water and many more.*
>
> *"Yothu means 'child' or 'baby' and yindi means 'big' but in the expression yothu yindi, yothu refers particularly to the mother****. This expression always involves two things in a relationship to each other****. So if I am someone's or something's yothu, they are my yindi. ...*
>
> *"We can also use yothu yindi to talk about* ***relationships between different things****, about people, tribes, ceremonies, land and so on. For example, I am the yothu ('child') of the tribe Gumatj. Gumatj is the yindi ('mother'). Why? Because my mother is a Gumatj woman. So all Gumatj people are my yindipuJu (-pulu is a suffix used to describe people, clans, etc., related through the mother's side). Also I am the next yindi for my yothu ('child'), but I am also yindi for anyone who has a Datiwuy mother.* ***So we see how the relationship extends from the***

> ***immediate family to cover whole tribes***. *Because of this, men are also considered yindi ('mother') and men can be yothu yindi to each other. All the members of the rock band Yothu yindi* ***are in this relationship to each other****, and that's why they chose the name" (Ganambarr 1994 in Williams 1999:126 - emphasis added).*

Whereas neither Williams nor Keen present any systematic observations with which to seek isomorphism in their models, Ganambarr provides enough data, albeit anecdotal, to disassemble both models. This is all the more remarkable given that Ganambarr's observations are presented in a quote intended to *support* one of the models that it undermines (i.e. Williams'). Not only does Ganambarr explain that Williams' erstwhile 'group terms' are actually 'relationship terms', but that even the corresponding vertical moiety terms *Dhuwa* and *Yirritja*, which by 1999 had reached a more or less settled definition among anthropologists as groups closed under a function of patrifiliation and exogamy, are here in Ganambarr's own words described not as closed groups, but as *emergent structures* arising from the systematic, and repeated instantiation of *yothu* and *yindi* relations *between* real people who are also cognisant of these structures (as reflected in the *Gumatj* and *Datiwuy* clan names). This completely contradicts William's enlisted quote of Bouquet (1997), that people and relationships can be separated from each other and ranked according to degrees of abstraction. From the perspective of Ganambarr's explanation it is difficult to distinguish Keen's (1995) critique of group metaphors from William's (1999) defence of them, since *neither* actually recognises emergence as the factor that breaches both their models, and that distinguishes between group theory and network theory.

By the late 1990s, the contradiction between group theoretic principles of closure, identity, associativity, and commutativity on the one hand, and the clear spatial, temporal, and normative ordering of real people in relation to each other, and in relation to space and time, reached a peak. Illustrating the deeply pervasive continuity of group theoretic models in Australian anthropology, the 2011 edited volume, '*Unsettling Anthropology*' (Bauman & McDonald 2011), with chapters contributed by more than a dozen Australian anthropologists, the word 'group' is used more 600 times in overwhelmingly uncritical terms, in applications such as 'named groups', 'claim groups', 'group boundaries', 'land-holding groups', 'social groups', 'territorial groups', 'tribal groups', 'language groups', 'group formation', and so on. Although the editors make specific mention of the problem of 'closure' that group models impose on definitions of sociality (Bauman & McDonald 2011:23-24), their proposed solution follows Gupta & Ferguson (1992) in advocating for 'flexibility in group description', rather than searching for an alternative to the group theoretic model altogether (se discussion of Gupta & Ferguson 1992 above).

In a now fairly routine anthropological complaint about incompatibility between the Native Title Act and anthropological theory, Bauman & McDonald (2011) cite Rumsey (1996) in lamenting the lost opportunity provided by the native title act, for introducing better models of sociality:

> *"Unlike the ALR(NT)A [NT Aboriginal Land Rights Act], the NTA [Native Title Act] had the potential to allow claimants to 'make a virtue out of ... multiplicity rather than treating it as something to be swept under the carpet in order to present an appearance of neatly bounded corporate groups'" (Bauman & McDonald 2011:23-24).*

However there is no indication in the editors' introduction, nor anywhere else in the volume, that such 'multiplicity' might refer to anything other than yet another re-ordering of the same group theoretic principles, using yet another kind of ancillary ordering mechanism to give it explanatory force and the appearance of congruence. Bauman & McDonald conclude their discussion on the continuing vitality of group theoretic terms with this statement:

> *"So, although there was, and may yet still be, some potential for flexibility in group description under the NTA, the prudent legal response ... has for the most part favoured a model of neatly bounded and inflexible corporate groups. This model is at least not necessarily inconsistent with claimant perceptions of group composition" (Bauman & McDonald 2011:23-24).*

This is a surprising assertion, given that, by 2011, when '*Unsettling Anthropology*' was published, there had been a number of highly publicised failures on the part of senior social anthropologists to demonstrate precisely this consistency. Possibly most notorious among these was the 2006 dismissal of the 'Jango' native title claim (Jango v Northern Territory of Australia [2006] FCA 318). This claim was made by a very experienced legal team on behalf of a Central Australian community whose kinship terminologies, language, and religion, are still very much consistent with the form that they took when first documented in the 1930s. Despite this consistency, the senior social anthropologist called to provide evidence in support of the claim, was unable to prove that his own group theoretic description of the claimants' culture matched either the description given by members of the community themselves, or that given by earlier anthropologists (Jango v Northern Territory of Australia [2006] FCA 318; Sansom 2007). The arguments of Keen (1995), Williams (1999), and Bauman & McDonald (2011), cited above, reveal that the core tenets of the KLRE model survive in Australian anthropology to the present day, albeit tacitly. The model is sustained both by the discipline's continuing dependency on metaphor and analogy rather than formalism, empiricism, and congruence, as well as by an accretion of precedent-setting judgements made in the federal court, where only the oldest and most frequently repeated anthropological assertions regarding 'law and custom' survive review.

2.4 Conclusion

This chapter has presented a summary of the development of group theoretic modelling in international and Australian social anthropology, concluding with its distinctive expression in Australian social anthropology in particular, as the 'KLRE model' of Indigenous sociality. The goal of this summary has been to illustrate how a widespread lack of familiarity with formal mathematical concepts within the discipline has masked the constraining influence of the model on the range of theoretical principles available to anthropological modelling. Combined with a low regard for empirical data, the main impact of implicit group theoretic models has been to sublimate diverse relationships between real people into an incongruent and homogenising concept of closed groups. Because of a general lack of awareness of the analytical constraints of this type of model, and because of the localised influence of the Northern Territory Aboriginal Land Rights Act, and the national Native Title Act, group theoretic thinking has persisted in Australia up to the present day. This is despite an intuitive discomfort among many of those who continue to use it in their research, including Keen (1995), Williams (1999) and Bauman & McDonald (2011), among others.

Chapter 1 illustrated anthropology's early attempts at developing theoretical models with explanatory power equivalent to those of the natural sciences, by mimicking the latter's formalism. That chapter also illustrated the failure of these attempts due to a lack of comprehension among early anthropologists regarding the relationship between formalism and empiricism, and weak definitions of both. The current chapter has illustrated how these disciplinary traits - a lack of formal training, combined with an ambivalence towards quantifiable data - have allowed antiquated concepts of sociality and causality to survive within Australian anthropology into the 21st century. This is despite the pressure of judicial demands to reform the discipline, and in the face of eloquent counter-examples from Indigenous people that show anthropological models to be inaccurate (e.g. Ganambar 1994).

The next chapter provides a more specific account of how the tacit group theoretic axioms of the KLRE model have been successively reformulated with regard to the roles of language and kinship. In this account, leading anthropologists working in Australia between the 1880s and 1970s, including Howitt, Radcliffe-Brown, Elkin, Berndt, Hiatt, Meggitt, and Stanner, laid down the fundamental assumptions that continue to be drawn upon by anthropologists studying the Indigenous Australian population today.

Chapter 3

The Evolution of the KLRE Model in Australian Anthropology

3.0 Introduction

The previous chapter outlined the origin and development of group theoretic modelling in social anthropology, and its effect on the discipline's capacity to develop congruent models of human sociality and causality. It was shown that through a combination of discursive limitations, ongoing misunderstanding of formal methods, and disinclination to privilege empirical data, social anthropology has sustained a broadly constant, tacit use of group theoretic principles for explaining both ad hoc observations of social organization, and the idea systems articulated by culture bearers. The previous chapter concluded by focusing on the particularly acute influence of this developmental history on Australian anthropology, where group theoretic concepts remain especially entrenched in the modelling of Indigenous sociality.

The primary outcome of this influence is a tendency for anthropological models to construe Indigenous Australian sociality as sets of discrete elements - what Leaf & Read (2012) refer to as 'idea systems' - linked together into one-to-one groups. It was proposed in the previous chapter that the key elements of this model comprised kinship, language, religion, and ecologically determined economy, giving rise to what might be called the 'KLRE model' of sociality, now firmly encoded in the legal idea of 'law & custom' under Australia's Native Title Act. In contrast to the independent variability that Leaf & Read (2012) propose for the connections between idea systems in any given culture, the KLRE model encodes a tacit group theoretic closure that requires cultures to be split off from one another into discrete 'societies'. Chapter 1 illustrated the development of this group theoretic model from Morgan's own 1871 inception of 'beliefs, laws, and customs' as uniform attributes of discrete societies, such that variation could be used as a means of distinguishing groups of attributes from each other, groups of societies from each other, and groups of people observing those laws and customs from each other (Morgan 1871). These models make predictions about individual behaviour based on the abstraction of interpersonal social processes into abstract collective attributes. Within the emerging local discipline in Australia, the KLRE model was consolidated between the 1920s and 1940s under the stewardship of Radcliffe-Brown and Elkin at the University of Sydney, together the group of students who came to be known as the 'Sydney School' (see Section 2.3.2). It then went through a period of frenetic reorganization during the 1950s and 1960s, before being subsumed within the much more tacit language of social anthropology's crisis era. Despite this subsumption, the KLRE model lives on to the current day, particularly in the so-called 'applied anthropology' of Australia's litigious native title sector, where it underpins core assumptions regarding the vitality of distinctive Indigenous cultures. Foremost among these is the assumption that discrete societies are identifiable by language, and instantiated in explicitly prescriptive 'rule-based' kinship terminologies.

The current chapter presents specific illustrations of group theoretic models of sociality and causality as they have evolved within Australian anthropology's focus on language and kinship. The term 'evolution' in this context, describes the successive adaptations over time, by Australian social

anthropologists, of a model that has exhibited a serial failure to accommodate new data as it has emerged over the past 150 years. Section 3.1 describes Australian anthropology's construal of language as an identity element in group theoretic models of Indigenous sociality, where it is used to close off sets of people into discrete 'language groups', appended with respectively discrete spatial, temporal, and normative orders. This portrayal diverges markedly from contemporary *set* theoretic models of language used by comparative linguistics, which posit no such closure, finding instead open and dynamic sets of 'language *communities*', whose members speak ranges of interrelated language varieties. Section 3.2 presents a developmental history of Australian anthropological group theory, based on the discipline's preoccupation with kinship terminologies as corollaries of 'rules' for marriage and descent. This history commences with the discipline's theoretical divergence from, and exclusion of, comparative linguistics' set theoretic modelling. This history indicates that Australian anthropology's misunderstanding of the formal basis of comparative linguistics, together with its isolation from pre-crisis developments in European anthropological theory (specifically Lévi-Strauss' (1969[1949]) work on kinship), led the local discipline to its current crisis state. The current chapter presages in-depth modelling and analysis presented in Chapter 4, illustrating the advantages of moving beyond a group theoretic model.

3.1 Ancillary Order & Causaliity: The Divergence of Linguistics & Anthropology

As discussed at length in the previous chapter, the process of bijecting certain elements of a set into a group requires the application of a single function to all elements in the set, such that only *some* of those elements are 'remapped' or 'transformed'. The elements of this new subset can only be defined as a group if they meet certain axiomatic criteria, namely closure, associativity, commutativity, the inclusion of an identity element, and the inclusion of an inverse element. All of these criteria are premised on the consistent function of a single operation between elements in the group, such that any iteration of that operation will only ever result in a permutation of the existing set of elements. In other words, no relational exchange or process between elements in a group will ever generate an element *outside* of a group. In yet other words, where groups are concerned 'no one comes in and no one goes out'. Chapter 2 illustrated that major anthropological texts spanning Morgan (1871) and Tylor (1871), to Geertz (1973), Appadurai (1988), and Ingold (2012), hinge on the tacit construal of such group closure in order to achieve theoretical self-consistency. There is however a contradiction in this dependency, in that it restricts the capacity of these models to render sociality and causality congruent with real-world observations of social activity. In order to achieve this congruence, group models tend to be accompanied by ancillary applications of some kind of order, typically in the form of imposed spatial (i.e. geographic), temporal (i.e. historical), and/or normative (i.e. prescriptive) sequences. Such ordering is defined as 'ancillary' because it is not a prerequisite of group theoretic models. Rather, it is an 'add-on' that is used to make group theoretic models appear 'more like' the social reality that people experience.

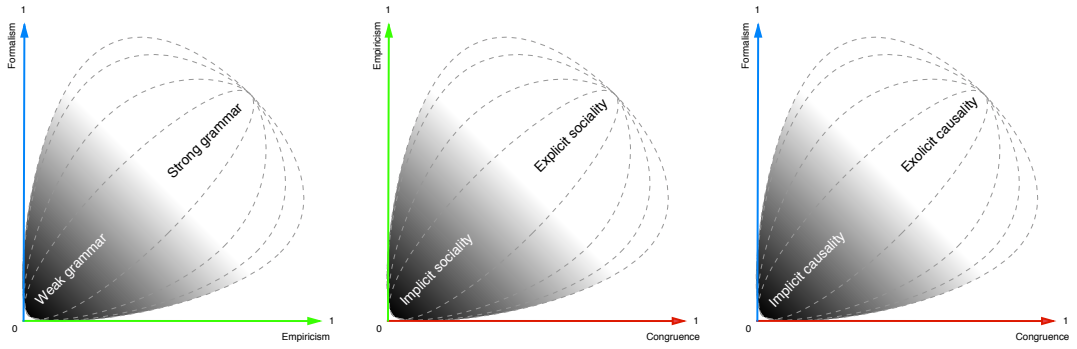


Figure 31: Group models exhibit a minimal convergent formalism, empiricism, and congruence, indicated here by shading.

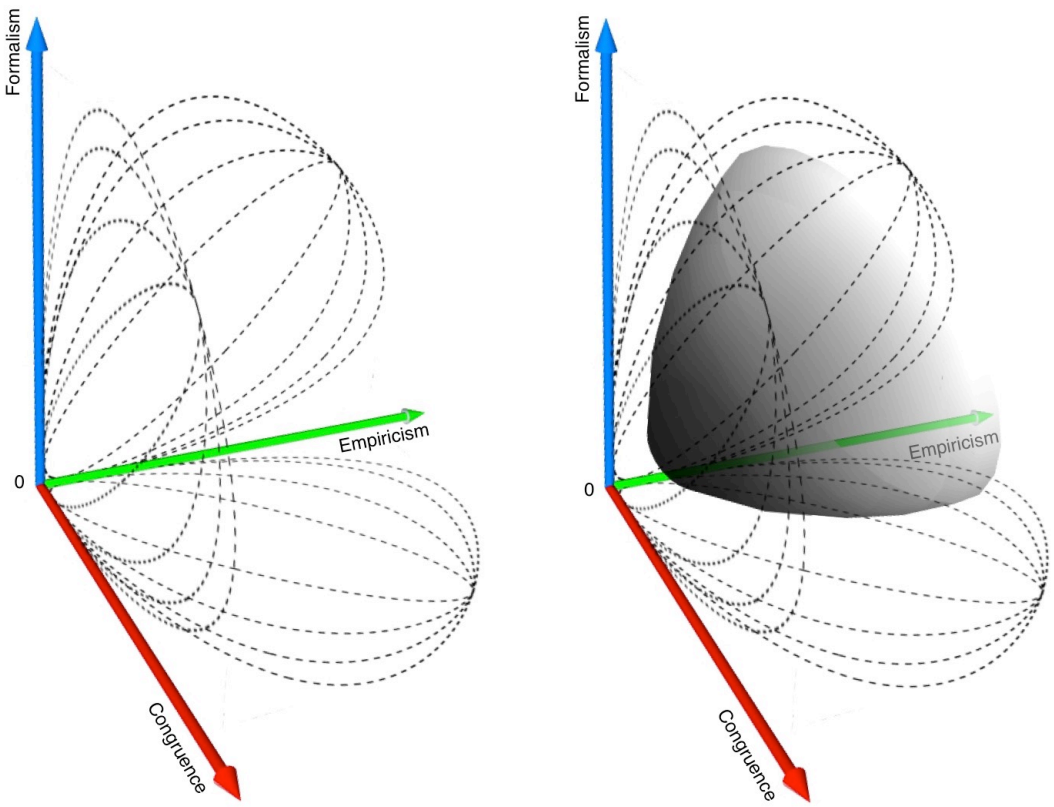


Figure 32: Group models occupy the near-zero corner of the theoretical vector space described in Chapter 2, representing minimal convergent formalism, empiricism, and congruence. The image on the left represents an empty vector space. The image on the right shows a shaded manifold generated by minimal convergent measurements.

3.1.1 Identity, Scale, and Ancillary Order in the KLRE Model

Group theoretic models of Australian sociality are sufficiently entrenched in anthropological literature that their historical origins often remain unexamined. In particular, the theory that Australian sociality is based on 'language groups' is often taken for granted, without any formal acknowledgment that comparative linguistic models make no such assumptions. In fact, the adoption of the term 'language group' by anthropology is itself erroneous, since the technical definition of this linguistic term refers to sets of language varieties, rather than to groups of people speaking one or more such varieties (Halliday 1968 see footnote 18). The lack of consilience between anthropological models of language's role in Indigenous sociality, and comparative linguistics' models of language variation, provides an important window onto the weaknesses of group theoretic models of sociality. More importantly still, this lack of consilience has generated some of the most revealing inconsistencies in attempts to explain the spatial, temporal, and normative order claimed to explain patterns in Australian sociality. The current section provides a brief review of language's promotion as an identity element within Australian anthropology's KLRE model, and its constraining impact on the development of the discipline.

As shown in Section 2.3 of the previous chapter, early iterations of the KLRE model construe sociality at two scales, comprising 1) superordinate 'tribes' and 'nations', which enclose 2) elementary 'laws and customs'. The KLRE model appends these latter elements with ancillary orders of space, time, and normativity as a means of imbuing the overall model with the impression of explanatory causality. Section 2.3 highlighted Howitt's (1880) early adoption of this model from Morgan (1871) and Tylor (1871), and the adaptations that he made to it an effort to render it more congruent with the ad hoc observations of amateur observers. The current section presents an account of the further adaptation to this model by Australian anthropologists. In this process, the scale and complexity of the model are extended such that erstwhile elements of kinship, language, religion, and economy are re-construed as subgroups, which themselves contain elementary instances of theoretical instantiation of institutional 'activities' (see Figure 33), with ancillary orders re-appended to the latter. At the most general, or 'macro-level' of this refined model is the object of social anthropological study - typically 'culture', and historically in Australia, 'tribe'. This object is construed as closed under a single operation of uniform relations between constituent elements, dependent upon total normativity, or what Lévi-Strass (1969[1949]) called 'elementary structure'. At the intermediate scale of the model, these elements are further bijected into discrete subgroups that each comprise an individual institution, or 'law or custom' (Morgan 1871) by which the object 'culture' is held to be realised: kinship, language, religion, and economy. Finally, at the most specific or micro-level of the model, each of these subgroups devolves into constituent elements. In the case of kinship these are specific sociocenctric kin terms, such as 'moieties', 'sections', and 'subsections'; in the case of language they are the specific language varieties that can be differentiated from one another, such as dialects and language groups; in the case of religion the elements are specific myths, ceremonies, and sacred sites; finally, in the case of economy, these element comprise specific resources together with particular modes for exploiting them. Citing Verdon & Jorion (1981), Section 2.3.2 in the previous chapter illustrated numerous examples of this 'scalable' function of group theory in the late pre-crisis texts of Australian anthropology, up to the 1970s and early 1980s. The current section focuses on the relation between this scaling function of the model, and the manner in which ancillary order is appended to the model as a means for construing causality.

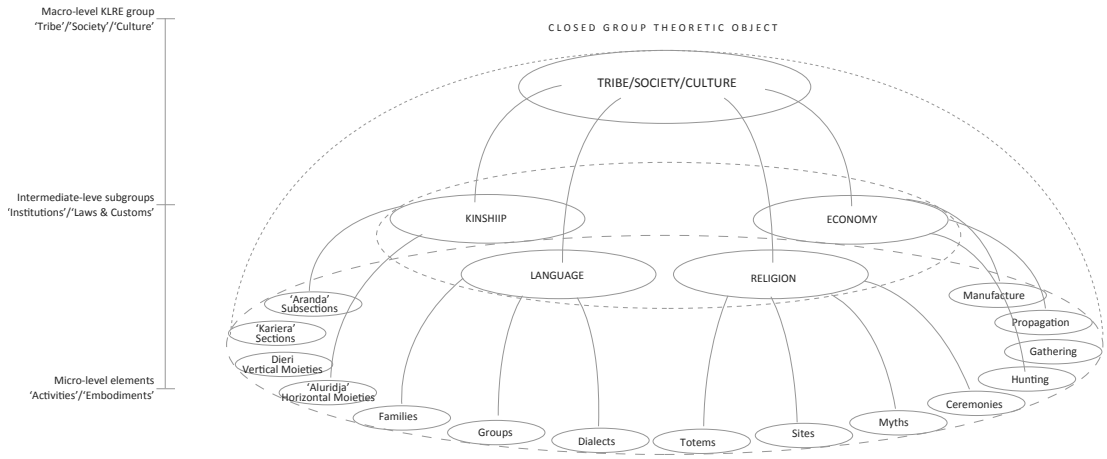


Figure 33: Saleability in the KLRE model, illustrating the permutability of three closed 'tiers': 1) the supergroup historically referred to as 'tribe', comprised of 2) the KLRE subgroup or 'institutions', in turn enclosing 3) elemental 'activities' or 'embodiments' of each institution, such as kinship terminologies, language varieties, sacred sites and ceremonies, and hunting and gathering.

Although this scalability appears congruent in the way that Australian social anthropologists have historically applied it in broad, abstract terms, its group theoretic axioms become increasingly incongruent with observable data at the micro-level of real social activity. This is because, as group theoretic axioms are imposed on sets of micro-level 'culture elements' in the real world, such as real interactions between kin, real instances of spoken language, real religious rituals, and real instances of hunting and gathering, the model's lack of causality becomes increasingly obvious. Chapter 2 explained how causality is encoded in the language of natural science in the form of explanatory cause-and-effect sequences. Chapter 2 also explained that, because group theoretic models specifically preclude an ordering function, they require ancillary order to be appended to them as a means of imbuing them with explanatory power. Such ancillary ordering is reasonably compelling at the macro-level of the KLRE group, and at the intermediate level of individual 'laws and customs', where elements and subgroups are highly abstract, such that an 'institution' can be said to comprise evidence of a distinct culture operating at a distinct time, in a distinct geographic region. However, such ancillary ordering regimes become increasingly implausible at the micro-level of specific social activities that bear their own, self-evident orders, which are clearly apparent to observers. Temporal, spatial, and normative orders appended to group theoretic objects lack the extensibility and flexibility to deal with the complexity of real-world human interaction. Spatial order imposed on language for example (expressed as the geographic distribution of language), breaks down at the level of individual speakers, in situations where multiple languages are spoken by individual people, when a person speaks a language that their parents or community does not speak, or when a person speaks a language in a location not contiguous with other speakers of the same language. This tension between the closed, non-causal group structure of the model led to a major crisis during the 1960s, manifest in a debate between two of Elkin's students, W.E.H. Stanner and, and the younger Lester

Hiatt, who had studied under the African culture specialist John Barnes at the new anthropology department at the Australian National University.

To deal with the inconsistency between ancillary orders appended to group theoretic objects, and the complexity of self-evident order at the micro-level of individual interactions, later iterations of the KLRE model treat individual instantiations of 'law and custom' as identity elements for the entire group theoretic object called 'culture'. In this way, a given 'culture' (or a given 'tribe') is defined as a macro-level group enclosing intermediate-level subgroup institutions, each of which contains micro-level elements, *one of which* acts to unify the entire group through an identity function (Figure 34). Thus a specific class of social event, such as 'hunting kangaroos', is treated as the identity element for the whole KLRE group, where the operation of hunting on any other class of event, such as using a particular kin terminology (as an element of the subgroup kinship), using a particular language variety (as an element of the subgroup language), or performing ceremony (as an element of the subgroup religion), leaves that element unaffected. In other words, when a social anthropologist links the hunting of kangaroos to the performing of a ceremony, and the use of a certain language, the comparison does not generate links to any other element *outside* of the KLRE group to which the three activities are said to 'belong' as elements inside that group, as activities of type *X*, in which People *X* engage. This is the logical process underway in examples of late pre-crisis group theoretic models presented in Section 2.3.2 of the previous chapter, yielding 'kinship groups', 'language groups', 'ritual groups', 'economic groups', etc., such that they are all held to converge upon each other, provided a sufficiently identity element can be put forward by the anthropologist. In each case, it is not the intermediate-level subgroup, either of kinship, language, religion, or economy that acts as the identity element for the culture, but rather the micro-level element of a particular *expression* of one of those intermediate entities - a so-called 'activity'. Using a micro-level entity as an identity element allows anthropologists to construe congruence between clearly ordered field data (which consists of constantly unfolding interactions between individual people - people *doing* things), and the inherently *un*ordered KLRE group model, where little if any congruence can be empirically demonstrated.

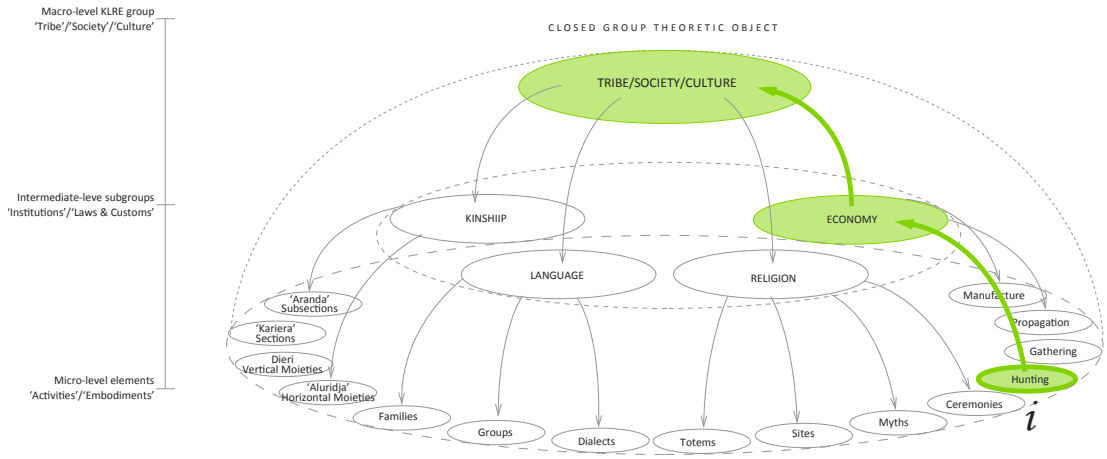


Figure 34: The use of a micro-level element as an identity for the rest of the KLRE group, exemplified here by 'hunting'. Identifying all other intermediate-level subgroup institutions and their respective constituent elements according to an intuitively ordered activity, such as a particular hunting trip, construes congruence in the otherwise incongruent group theoretic model.

A necessary prerequisite for using a given micro-level element as the identity for the macro-level group theoretic object, is the evisceration of that element's inherent and observable order. This requires a deliberate exclusion of empirical data that might emphasise order, such as specific records of when and where a particular event occurred, who was involved, which individuals interacted with which others, who spoke to whom, what was said, and so on. These processes exhibit an inherent order because they unfold in the course of people's interactions with one another as they move through space and time. By excluding empirical measurements from field data, social anthropological language grants latitude to construe temporal, spatial, and normative order in any manner an author chooses to use, without regard for the complexity of observable reality. The way that social anthropological language avoids the complexity of real-world field data is by providing a grammar for extrapolating, or 'scaling up', from a small number of anecdotal accounts of specific activities to broad generalisations. In this technique, a small number of real individuals are construed as manifestations of a group theoretic object whereby they themselves each become a group theoretic entity, 'embodying', 'performing', or 'inscribing' all the elements and subgroup institutions of the culture group, closed under the unity of their own consciousness. The evisceration of real world order occurs when the activities of these few individuals are conflated with all plausible instantiations of underlying idea systems across the population to which the anthropologist refers, and the specific activities of other population members are not actually recorded. Rather than emerging with greater clarity concomitantly with a greater number of articulations by a greater number of people observed or interviewed, generalised idea systems are held to 'reside' *within* each individual in a kind of platonic distillation. Similarly, the activities of one or two people are held to manifest the systematic actions of all people. This in turn reinforces the late Western assumption that an individual somehow encapsulates a distillation of the entire culture 'group' to which they belong.

Equations 1, 2, 3, and 4 below, illustrate the ancillary ordering typically applied to micro-level elements within of each of the intermediate-level KLRE subgroups. In each case, order is presented as deriving from a property of the elements themselves. The ordering of kinship terminologies is held to derive from the number of terms that each terminology contains. The ordering of languages is held to derive from the geographic distribution of the regions in which they are spoken. The ordering of religions is held to derive from the geographic distribution of the sites to which religious mythology and associated totemic actors refer. The ordering of economies is held to derive from the geographic and seasonal distribution of the resources on which economic activities are predicated. In none of these ordering regimes do actual relationships between actual people comprise a factor.

$$Kinship = \{Term_a, T_b, T_c, \ldots T_x | Nt_1 < Nt_2 < Nt_3 < \ldots Nt_n\}$$

Equation 16: Standard anthropological model of 'Kinship', construed as a subgroup comprised of variable terminologies T, ordered according to the number of terms N each contains.

$$Language = \{LV_a, LV_b, LV_c, \ldots LV_x | Reg_1, Reg_2, Reg_3, \ldots Reg_n\}$$

Equation 17: Standard anthropological model 'Language', construed as a subgroup comprised of discrete language variations LV, ordered by spatial position, i.e. geographic region, R.

$$Religion = \{Myth_a, Crmny_b, Totem_c | Site_1, Site_2, Site_3, \ldots Site_n\}$$

Equation 18: Standard anthropological model of 'Religion', construed as a group consisting of elements 'Mythology', 'Ceremony', and 'Totems', ordered by sacred sites whose geographic distribution expresses the sequence of myths as totemic figures travel through time and over the landscape.

$$Economy = \{Hunt, Gath, Prop, Manu | Res_1, Res_2, Res_3, \ldots Res_n\}$$

Equation 19: Standard anthropological model of 'Economy', construed as a group consisting of elements 'Hunting', 'Gathering', 'Propagation, and 'Manufacture', ordered by the spatial (geographic) and temporal (seasonal) distribution of specific Resources to which they are applied.

The common feature of each subgroup institution is the lack of any role either for real individuals and the interactions between them, or for measurements of real instantiations of the activities in question. In this model, kinship is construed in the lexicon and grammar of the terminology, not in in the use of kinship terms by real people; language is construed in the lexicon and grammar of the broader language, not in its realization of meaning for real people; religion is construed as a set of embodied metaphors rather than the other way around - the metaphorical remapping of real relations between real people; and finally, economy is construed as a set of productive forces applied to a set of productive means, rather than specific instances of personalised labour exchanges between real people. Any measurement that occurs under the auspices of these group theoretic

suppositions is a measurement of an *a priori* data class, rather than of observable real world processes. The group theoretic logic of the KLRE model simply cannot accommodate the possibility of diversity in autochthonous idea systems, much less the complexity of real social activity that extends beyond the confines of a single idea system.

The next section presents a comparison of Australian anthology's *group* theoretic model of language with Australian linguistics' *set* theoretic model of language. The purpose of this exercise is to highlight the discrepancy between Australian social anthropology's construal of what it considers a critical part of Indigenous Australian culture (i.e. language), and the construal of the same phenomenon by the specialized models of comparative linguistics. This discrepancy reveals not only a lack of integration between contiguous disciplines dealing with the same phenomenon of language (as discussed in Chapter 1), but also the resistance of social anthropology's KLRE model to amendment in the face of an alternative linguistic model that is more formal, empirical, and congruent with observable data.

3.1.2 Emergent Order in Comparative Linguistics' Set Theoretic Models

From the 1870s to 1910s, the study of Australian languages was treated largely as a branch of generalised ethnography, exemplified by Curr's (1886) edited compilation *The Australian Race*, comprised of lists of words collected by colonising squatters, missionaries, and government officials, from an unknown number of unnamed Indigenous speakers across the continent. Early anthropologists such as Ridley (1856; 1875 *inter alia*), Howitt (1880 *inter alia*), and R.H. Mathews (1895 *inter alia*), incorporated varying degrees of linguistic study in their work, usually with reference to Curr, and usually comprised of similar kinds of word lists. This changed during the 1910s when the formal-empirical basis of linguistic research rapidly consolidated, and the explanatory power of its modelling moved ahead of the ad hoc observations and speculative reasoning of early social anthropology. By the 1920s, comparative models of Australian languages had become sufficiently robust to be able to demonstrate that most language varieties belonged to a single linguistic family, later termed 'Pama-Nyungan'. The recognised pioneer of this discovery was the anthropologist and linguist Wilhelm Schmidt. During the 1910s Schmidt published the first major attempts at formal-empirical comparative study of Australian languages (Schmidt 1912; 1917; 1918 *inter alia* (see Bowern 2010; Koch 2004), in which he systematically classified degrees of morphological variation in lexicon across a range of languages. Although this effort was unprecedented, it was also weakly executed (Koch 2004:25). It was followed however by Capel (1956), O'Grady *et al* (1966), Wurm (1971), and finally Dixon (2002; 2012; *inter alia* - see Koch 2004; McConvell & Bowern 2012), who consolidated and refined the language that has come to be known as comparative linguistics. In the terms laid out by Bernstein (1996; 1999), Halliday (2001; 2005), and Kuhn (2012[1962]) (see Chapter 2), comparative linguistics thus began to take on a degree of idiomatic specialisation that rendered it a discipline in its own right. Anthropology no longer possessed the technical capacity to accommodate the more

formalised, and empirically oriented language used by linguists, and wordlists ceased appearing in anthropological texts from the 1910s onwards. This did not stop social anthropologists referring to the role of language as a factor in Australian sociality however.

Strangely, there is no indication that Australian social anthropologists were cognizant of this split between linguistics and their own discipline at the time that it happened, or that the substance of the divergence has been comprehended subsequently. Radcliffe-Brown himself only partially acknowledged it explicitly in 1940, 30 years after he commenced fieldwork in Australia, and 10 years after he had departed permanently for the USA. Even then, he indicates that he still considered linguistics to be a 'branch of anthropology' (Radcliffe-Brown 1940). This suggests that during the 18 years over which he conducted anthropological research in Australia, he had little if any working understanding of comparative linguistics, despite treating language as the critical factor in his model of Australian sociality. Radcliffe-Brown's arrival in Australia from South Africa in 1910, two years after the death of Howitt, marked the beginning of anthropology's entry into the realm of official academia. After a brief excursion in Western Australia, Radcliffe-Brown's first fieldwork included a series of interviews with residents of the Murray Darling Basin in New South Wales, which formed part of the basis of his first published article on Australian cultures in 1918, 'Notes on the Social Organization of the Australian Tribes'. This commenced with a bullet-point list of terminological definitions relevant to the still-nascent discipline, the first of which was a definition of the term 'tribe':

> *"By a tribe I mean **a collection of persons who speak what the natives themselves regard as one language, the name of the language and the name of the 'tribe' being generally one and the same**. This word 'tribe' has been used very loosely by writers on Australian ethnology. For example, Howitt sometimes applies the word to what I shall call a tribe; at other times he applies it to subdivisions of a tribe; and in yet other instances he uses it to denote a group of tribes having similar customs and using the same word for 'man' [see citation of Howitt in previous chapter]" (Radcliffe-Brown 1918:222).*

This first declaration of definitions by Radcliffe-Brown contains two important pieces of information. Firstly, his assertion that the name of a language and the name of a tribe are 'generally one and the same thing', shows that he is defining Aboriginal sociality as determined entirely by what linguists call a 'language community' (Halliday 1968), and what Radcliffe-Brown himself later called a 'speech community' (Radcliffe-Brown 1940:7 - see below). This is the first appearance of Radcliffe-Brown's main influence on Australian social anthropology, as outlined throughout the current section. Secondly, however, Radcliffe-Brown acknowledges in this 1918 quote the lack of clear definitions in his predecessor's rendition of the KLRE model. Radcliffe-Brown points out that his forebears referred inconsistently to various 'subdivisions of a tribe', i.e. communities defined by attributes *other than* language, which Radcliffe-Brown differentiated as 'customs' and later as 'social structures' (see

below). This suggests that Radcliffe-Brown was conscious that he was raising the status of language above that of other institutions within the KLRE model, to the level of an identity element. This intentionality becomes progressively explicit over the following 12 years.

Five years after his opening gambit, Radcliffe-Brown states:

> *"[T]he tribe, which was, like all Australian tribes,* ***a language group*** *and little or nothing more" (Radcliffe-Brown 1923:424).*

Then, 6 years later:

> *"The primary mark of a tribe is that* ***it consists of persons speaking one language****, or dialects of one language.* ***Its unity is primarily linguistic****. The name of the tribe and the name of its language are normally the same.* ***So that the easiest way to ascertain to what tribe an individual belongs is usually to ask him what language he speaks****.* ***In addition to this unity that comes from a common language there is also a unity of custom throughout the tribe"*** *(Radcliffe-Brown 1930:36).*

There are *three* important pieces of information in this latter statement. Firstly, a reinforcement of Radcliffe-Brown's 1918 declaration that Australian sociality is determined by language, including a specification that minor variations in language ('dialects') do not equate to variations in 'customs' or 'social structure'. The second piece of information relates to the dubious elicitation methods acknowledged by Radcliffe-Brown, that in defining a 'tribe' an Australian anthropologist need only ask an Indigenous Australian "what language he speaks". This statement provides a glimpse into how positivism operates within the discipline in Australia, whereby an *a priori* theoretical construct like 'tribe' is first postulated, and then selectively confirmed by asking interviewees not about the construct itself, but about something else entirely, in this case *language*. The important third piece of information in the statement is Radcliffe-Brown's specification of the role of language as an identity element within the KLRE model, as driving "a unity of custom": "*In addition to this unity that comes from a common language there is also a unity of custom throughout the tribe".* In other words, the tacit effect of language on other customs is null. This definition is entirely consistent with the group theoretic principle of 'uniformity of belief, law, and custom' provided by Tylor 59 years earlier (Tylor 1871 - see discussion in previous chapter).

In his 1940 article 'On Social Structure', Radcliffe-Brown states:

> *"A language is a connected set of speech usages observed within a defined speech-community. The existence of speech-communities and their sizes are features of social structure. There is, therefore, a certain very general relation between social structure and language. But if we consider the special characteristics of a particular language - its phonology, its morphology, and even to a great extent its vocabulary - there is no direct connection of either one-sided or*

> *mutual determination between these and the special characteristics of the social structure of the community within which the language is spoken.* ***We can easily conceive that two societies might have very similar forms of social structure and very different kinds of language, or vice versa. The coincidence of a particular form of social structure and a particular language in a given community is always the result of historical accident****. There may, of course, be certain indirect, remote interactions between social structure and language, but these would seem to be of minor importance.* ***Thus the general comparative study of languages can be profitably carried out as a relatively independent branch of science [i.e. linguistics]****, in which the language is considered in abstraction from the social structure of the community in which it is spoken.*
>
> *"But, on the other hand, there are certain features of linguistic history which are specifically connected with social structure. ...*
>
> *"Thus the spread of language, the unification of a number of separate communities into a single speech-community, and the reverse process of subdivision into different speech-communities, are phenomena of social structure.* ***So also are those instances in which, in societies having a class structure [i.e. sociocentric kinship terminology], there are differences of speech usage in different classes [i.e. those communities exhibiting distinct kinship terminologies].***
>
> *"I have considered language first, because linguistics is, I think, the branch of social anthropology which can be most profitably studied without reference to social structure" (Radcliffe-Brown 1940:7).*

There is an important contradiction between this 1940 view of linguistics as a branch of anthropology, but nevertheless 'disconnected' from the study of sociality by its 'internal structure' of vocabulary and grammar, and Radcliffe-Brown's earlier (1918; 1923; 1930) views of language variation as a *corollary* of variation in other 'social structures'. Certainly, he does not acknowledge any such contradiction with the position that he maintained during his 20 years in Australia, by which he consistently defined the Australian unit of culture as 'a language group and nothing more' (Radcliffe-Brown 1923:424). Nor does Radcliffe-Brown acknowledge the convergence of Schmidt's (1912; 1917; 1918 *inter alia*) comparative linguistic research with the commencement of his own work in Australia, Schmidt having by then shown that language variation *could* be systematically classified, but making no assertion that these classifications correlate with any other kinds of community, such as kinship, religious, or economic communities. Given the emphasis that Radcliffe-Brown placed on language, the absence of citations of linguistic research in his articles is mysterious. As shown below, he was an early forerunner in this regard, with none of his anthropological heirs showing evidence of rectifying this ignorance of linguistics, despite relying on the role of language as a unifying factor in their own theories.

Note the further apparent contradiction between the earlier 1930 view of 'tribe' as being equivalent to 'speech community', and the 1940 view that, 'We can easily conceive that two societies might have very similar forms of social structure and very different kinds of language' (Radcliffe-Brown 1940:7). This suggests that Radcliffe-Brown was beginning to modify his previous view, and weaken the axiomatic basis of language's status as an identity under which 'tribes', 'nations', or 'societies' could be closed into 'language groups'. However, by the time Radcliffe-Brown had abandoned the language group equivalence model, it was too late. Elkin had taken over as anthropology professor at the University of Sydney, and was busy promulgating the out-dated model. The anthropological unit of study, the 'tribe', mapped as a group theoretic object, was now closed under the identity of this element of language, so that the 'culture elements' of kinship, religion, and economy were all held to be unified by their convergent association with distinct languages, yielding 'language group' as a de facto (and later more fashionable) term for the same theoretical object previously referred to as 'tribe'. By the 1940s this was no longer a theoretical position limited to heirs of Elkin known as the 'Sydney School' (including Berndt, Stanner, Meggitt, and others as discussed below), but had dispersed throughout Australian social anthropology. The first major survey project of this 'grouping-by-language' approach, was completed by the Harvard University anthropologist Norman Tindale in 1940, resulting in a map titled *Tribes of Australia* (Tindale 1940). This was not the first regional mapping exercise undertaken by an anthropologist, with predecessors including Elkin (1930s), Radcliffe-Brown (1920s), and Mathews (1890s), among others. However, Tindale's map was the first to define contiguous regions of land as corresponding with coherent KLRE groups, rather than simply with language groups, as linguists had previously done (see Bowern 2010; Koch 2004; McConvell & Bowern 2012).

Apart from the previously discussed misidentification of a 'language *community*' as a 'language *group*' (Halliday 1968), this new orthography implied that linguistic morphology similarly constrained definitions of kinship terminologies, religious terminologies and symbolism, and economic organization. The logic of the KLRE model effectively extended the assumption that if a set of people are defined by use of a single language variety, such as a dialect, then that language determines the unity of all other elements of those people's culture (see Figure 35).

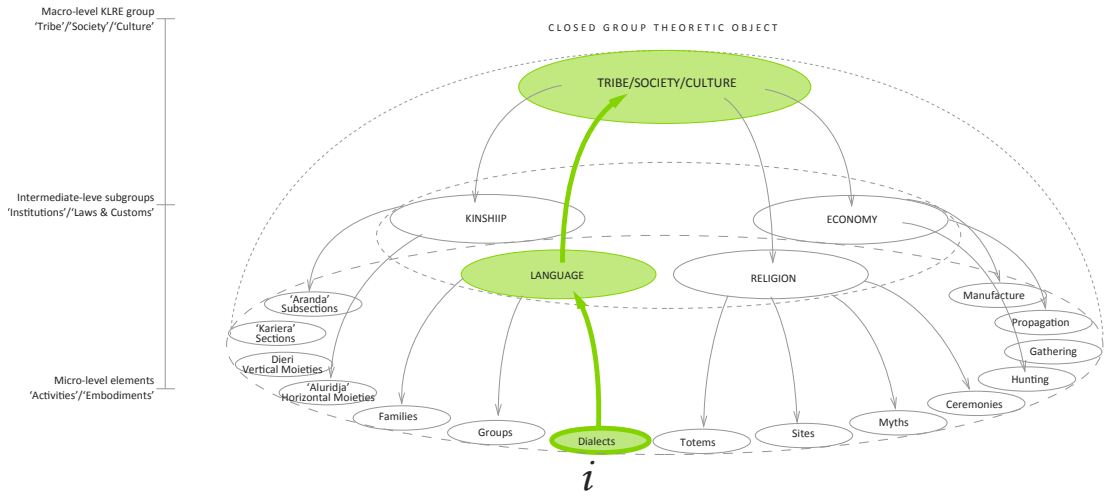


Figure 35: The extension of language's role as an identity element *i* within anthropology's post-comparative linguistics group theoretic models. Individual 'beliefs, laws, and customs' are held to constitute groups in their own right. The emergence of language as an identity element in the model means that each group of such laws and customs always includes language, interaction with which leaves them unchanged.

The divergence of linguistics from general ethnography as a discrete field in its own right, together with the treatment of language as an identity element by anthropologists within their own KLRE model, led to the emergence of a tentative symbiosis between social anthropology and linguistics. However, like any symbiosis, the contribution of each party to the relationship has not been equal, being broadly useful to both disciplines in a heuristic sense, but stopping short of a consilient and functional integration. Historically, linguists might have found the term 'tribe' a useful simulacrum for alluding to social processes beyond their expertise, but the set theoretic logic of independent and complex language variation was fundamentally misconstrued in the course of its borrowing by anthropology. As outlined below, the structure of the relationship was determined, not by the newly discovered *set* theoretic logic of the comparative linguistic model, but by the pre-existing *group* theoretic logic of anthropology's KLRE model. In the course of its adoption by social anthropology, the terminology of comparative linguistics, particularly the term 'language group', was stripped of its formal linguistic definition in order to fit the rigidly closed structure of the KLRE model.

Radcliffe-Brown's (1940) revelation, that it might be possible for the elements of the KLRE model to vary independently of one another, came too late to arrest the rise of language as an identity element that closed off into discrete groups what might otherwise have evolved into more flexible set theoretic entities. This raises an important question: How was it possible that Australian comparative linguistics could advance along an independent theoretical trajectory, developing its own solution to the problem of complex language variation, while Australian social anthropology persisted with such a rigid and clearly incongruent group theoretic model, despite treating language as such an important part of that model? As discussed in Chapter 2, the late pre-crisis era, beginning in the 1970s, saw the jettisoning of the formal language that might have helped early Australian anthropologists to identify the underlying problems of tacit *a priori* assumptions in their discipline's group theoretic logic.

Concerns that were raised by some anthropologists about the impact of group theoretic models on land rights and native title claims (e.g. Merlan 1981; Rumsey 1989), not only came too late, but were also, thanks to the new de-formalised language of the discipline, unable to detect or articulate the underlying basis of that impact, in terms of tacit mathematical assumptions. Despite these concerns being explicitly directed at the disproportionate emphasis placed on language in anthropological texts, there is no indication that Australian anthropology had assimilated the set theoretic basis of comparative linguistic modelling. In order to understand the effects of comparative linguistics on anthropological construals of sociality, it is necessary to briefly summarise the formal set theoretic principles at work in the former field.

3.1.3 Set Theoretic Logic in Comparative Linguistics: Immanent Causality

The comparative linguistic model that slowly emerged following Schmidt's work in the 1910s (Schmidt 1912; 1917; 1918) is based on the classification of degrees of variation among lexical items, together with two causal principles explaining that variation. A lexical item in this case may be any discrete unit of meaningful text (whether spoken or written), ranging from an irreducible sound (phoneme), through to a clause comprising a sample of a vocabulary (lexis) together with a self-consistent relational structure (syntax). Lexical variation is generally considered to be driven by one or a combination of two causal processes: innovation and/or diffusion, where innovation describes spontaneous change over time within an existing language variety, and diffusion describes the transmission of a lexeme from one existing language variety into another. Bowern (2010) and Koch (2004) trace the development of this model in Australian linguistics from Schmidt (1912; 1917; 1918) through Capel (1956), O'Grady *et al* (1966), Wurm (1971), to Dixon (2002; 2012). In this account the basis of the model has remained largely stable over an 80-year period, with minor amendments driven mainly by debate over the relative influence of innovation versus diffusion, which are sometimes construed as conflicting rather than complementary (see e.g. Dench 2001; McConvell & Bowern 2012).

Comparative linguistics utilizes general set-theoretic principles to realise the effects of generative forces as *language change*, whereby degrees of change are classified as 'levels'. Today comparative linguists are reluctant to name these classes, or to apply universal parameters to their definition, although in the past, there was less such aversion. In the best-known cases, the 'lexicostatistical' techniques formalised by O'Grady *et al* (1966), and refined by Wurm (1971) for Australian languages, are generally treated as generating hierarchies of subset linguistic items, with the broadest items treated as top-level supersets, and narrowest items treated as lowest level subsets (see Table 1). In more recent decades, the certainty of the percentage-based classifications behind lexicostatistics has come into question (see e.g. Dixon 2002 *inter alia*), however the set theoretic principles of the approach remain intact. Most fundamentally, the model accommodates a relational flexibility, so that variation generated by newly discovered research data leads to either or both the redefinitions

of sets and/or the redefinition of relations between sets. If, for example, one of two language varieties previously classed as distinct dialects of a common language, is found to contain more distinct lexical items than previously thought, it may be re-classified as a whole separate language, linked to the erstwhile 'sister dialect' by a more distant common ancestral language. Conversely, if two language varieties that were once thought distinct, are found to contain more lexical items in *common* than previously recognised, they may either be reclassified as dialects of a single language, or the language group to they both belong may be redefined in order to accommodate both of them. The model is not as rigid as that typical of anthropological models of sociality, because it is not closed.

Table 1:Taxonomic relationships between linguistic classifications, after Wurm (1971)

Shared vocabulary	**Relationship**
<15%	Different families
16<25%	Different groups of the same family
26<50%	Different subgroups of the same group
51<70%	Different languages of the same subgroup
71<99%	Different dialects of the same language

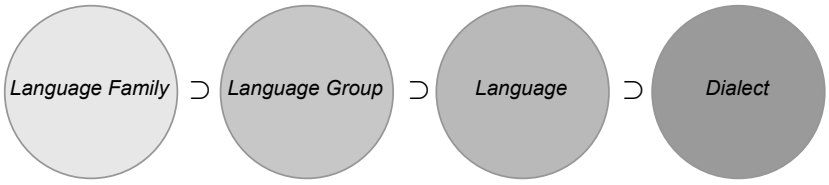


Figure 36: The set theoretic basis of comparative linguistics, formalized by O'Grady *et al* (1966), and refined by Wurm (1971), ranked according to degrees of variation among lexical items, typically words. Dialect is a subset variation of language, which is a subset variation of a language group, which is a subset variation of a language family (language subgroup has been removed for the sake of brevity). Even those critical of lexicostatistical assumptions (e.g. Dixon 2002) maintain supports for its underlying set-theoretic principles.

As explanatory principles, linguistic innovation and diffusion depend on two distinct classes of order: innovation depends on an exclusively temporal (historical) order (see Figure 38), while diffusion depends on a combination of temporal *and* spatial (geographic) order (see Figure 39). Innovative order is solely temporal, while diffusive order is temporal *and* spatial. For innovation to have causal, explanatory power, it requires sets of common lexical items to exist in two otherwise distinct language varieties at different points in time. By contrast, in order for diffusion to have causal, explanatory power, it requires common lexical items to exist in two otherwise distinct language varieties, at either the same *or* different times, *as well as* in different geographic locations. Language

varieties may thus be classified into sets, which exhibit a partial order as a function of the variation between those sets. The difference between this partial ordering of sets of language varieties, and the ordering applied to the group theoretic models of language used in Australian anthropology, is that the former is inherent in the original classification, whereas in the latter it is ancillary. In other words, the classification of language varieties as sets of lexical items, defined by a given degree of similarity, generates an order related to the immanent meaning potential of those lexical items - two lexical items mean the same thing, but exhibit different morphology. By comparison, the classification of 'culture elements' into groups, such as the grouping of kinship terminologies according to the number of terms in each terminology, generates no order. The *ancillary* order yielded by a raw count of kin terms is *by-product* of the use of the terminology by real people interacting with each other, but is not a feature of the terminologies' meaning potential. Likewise the grouping of religions according to common elements of myths, ceremonies, and totemic figures, does not generate order. The *ancillary* order yielded by a spatial distribution of sacred sites associated with those elements is a *by-product* of their functions as metaphors for relationships between real people who bear the real geographic order. Equally similarly, the classification of economic systems into groups according to means of exploitation, generates no order. The *ancillary* order yielded by the geographic distribution of certain hunting and gathering methods is a *by-product* of the cooperation of real people in exploiting those resources. These issues are discussed in detail in the next section with regard to kinship.

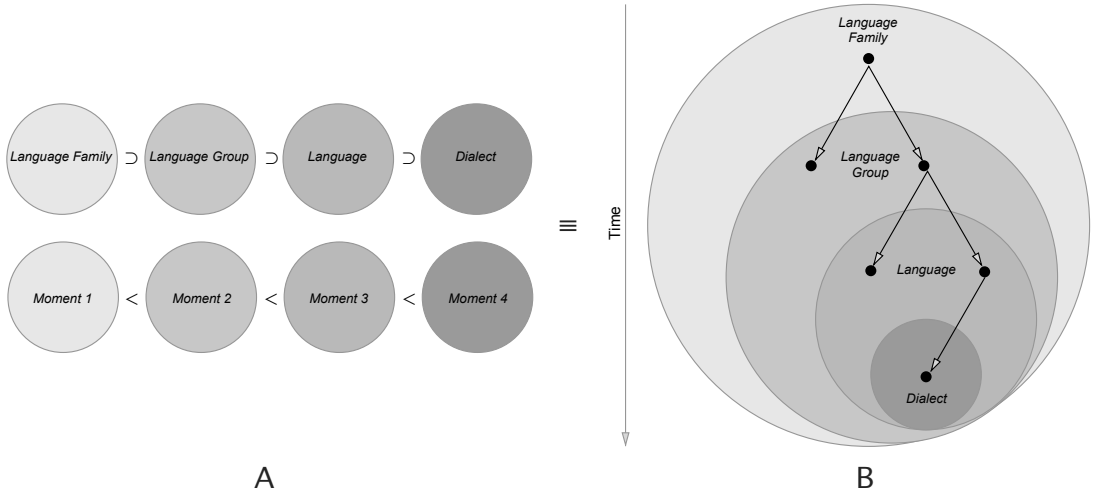


Figure 37: The ancillary ordering by time of a comparative linguistic set theoretic model (A). Each pair of vertices represents two deviations within the same subset (B). Within comparative linguistics, a graph of the relationships between such subsets is referred to as a 'phylogenetic tree'. A phylogenetic tree explains language variation as the result of change over time, but does not stipulate a cause of that change.

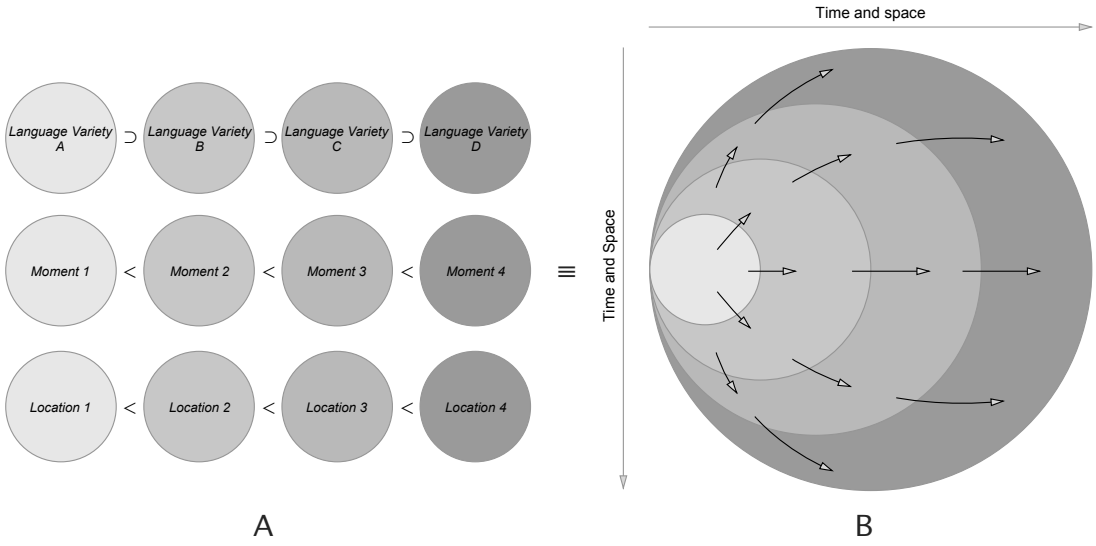


Figure 38: Visualisation of an areal diffusion model of language change, comprising the underlying set-theoretic framework of comparative linguistics (A, top row) further ordered by an ancillary measure of time (A, middle row), as well as space (A, bottom row). Visualised as a geographic plane (B), this conjoint model explains the distribution of related languages as a function 'loan items' (typically loan *words*) moving from a source language to a destination language.

Like anthropological models of sociality, linguistic models of lexical difference render individual people implicitly, so that the object of measurement are not specific interactions between specific people (i.e. not actual texts consisting of real instances of speech between real people), but abstracted *attributes of sets of people*, defined by generalised patterns (i.e. the morphology of lexical items held to constitute a language imputed from individual speakers). The difference between comparative linguistics and anthropology in this regard, is that the linguistic model nevertheless *measures* texts and their meanings (comprised of lexical items), whereas anthropological models *represent* (but do not measure) idea systems, such as kinship terminologies, religions, and economies. Both the texts measured by comparative linguistics, and the idea systems represented by anthropology, are decontextualized from their real-world instantiation, with minimal data samples treated as maximally indicative of whole languages and cultures respectively. However, anthropological models are further abstracted by the lack of measured observations. In other words, whereas comparative linguistics measures *ideas* about language (but not the *instantiation* of language in real instances of speech), the standard model used by social anthropology measures neither idea systems nor their instantiation, instead providing only isolated and ad hoc observations of people doing and saying things.

The purpose of the current section has been to show, citing Radcliffe-Brown as a founder of the language-as-identity reformulation of the KLRE model, how the closure of group theory colonises the terms, definitions, and ultimately the model of an overlapping discipline, in this case 'language groups' as defined by comparative linguistics. The following section presents an in-depth example of how sociality is construed in the KLRE model at the micro-level of kinship, and how the

misunderstood definition of 'language group' that developed in Australian anthropology over the early 20th century has come to impact on construals of consanguinity and affinity as two dimensions of Australian kinship. The purpose of this coming exercise is to show how the subsumption of group theoretic axioms within this additional subset of KLRE model (kinship terminologies in addition to language varieties), colonises the discipline's entire construal of sociality. This is not an exclusively critical exercise. Rather, it lays the foundation (in Kuhn's (2012[1962]) terms) for introducing a more empirical, formal, and congruent model in in Chapters 4 and 5, which better explains the observable link between autochthonous ideas and instantiated social activity in relation to both language *and* kinship.

3.2 The Standard Model of Australian Kinship

The previous section illustrated the lack of congruence between the *ancillary* ordering attributed to language within Australian anthropology's *group theoretic* KLRE model of sociality, and the *inherent* order observable in its geographic and temporal distribution within Australian comparative linguistics' *set theoretic* model of language change. The previous section also pointed out that neither anthropology's group theoretic model, nor comparative linguistics' set theoretic model account for the complexity of real-world instantiation. The objective of the previous section was to highlight the discrete implications of different kinds of tacit mathematical formalisation, and the consequent lack of congruence between related social scientific fields when the observable interactions of real people are not taken into account.

The previous section showed that Australian social anthropology attempts to circumvent this lack of congruence by treating 'culture elements' such as language, as subgroups within a three-tiered group theoretic structure. This theoretical structure is comprised of a macro-level supergroup 'culture', intermediate-level subgroups or 'institutions' (kinship, language, religion, and economy), and finally micro-level elements comprised of the 'activities' said to constitute these institutions. The current section traces the historical application of this same strategy to Australian anthropology's treatment of kinship terminologies.

Although language has formed the identity element for the KLRE model since the early 20th century, Australian social anthropology remained fixated on kinship throughout its pre-crisis era, from the 1880s through to the 1970s. Internationally, this fixation was triggered by Morgan's (1871) historicist emphasis on Australian kinship as the 'most primitive' relic of human sociality, and then sustained by the influence of Morgan's broader kinship theories on the discipline as a whole. Within Australia, this international attention stimulated an interlocutory fieldwork industry, with successive generations of Australian anthropologists seeking to build their reputations through the 'discovery' of previously undocumented terminologies, by classifying them according to Morgan's 1871 schema. This historical process soon generated insurmountable inconsistencies however, in which increasingly comprehensive descriptions of kinship terminologies from widely distributed communities revealed a mismatch between the inherently *un*ordered group theoretic models used by anthropologists to represent terminologies, and the clearly ordered geographic distributions of the real people and social relations to which the terminologies referred.

The complexity of this history requires that it be broken into three parts. Section 3.2.1 shows how Morgan's 6-part kinship classification schema influenced the work of Howitt, and how Howitt selectively cited ad hoc observations to help promote it, incepting the notorious 2-part clan/horde ancillary order that would plague Australian anthropology for the next century. Following Howitt's death in 1908, Radcliffe-Brown adopted the former's 2-part model, and promoted the status of language to an identity element, establishing the basis for what would later come to be known as

'language groups'. Following Radcliffe-Brown's departure for the USA in 1930, Elkin adopted the former's model, making only cosmetic changes during the 1940s, which remain the staple of Australian kinship studies to the present day. Section 3.2.2 shows how the so-called Sydney School, comprised of Elkin's student's Berndt, Hiatt, and Meggitt, launched a posthumous attack on Radcliffe-Brown's model, and sought to reformulate it again. Stanner later joined the Sydney School from the Australian National University but, opposing Berndt's, Hiatt's, and Meggitt's reformulations while also making his own changes, contributed to cementing the final form of the standard model, as it survives to the current day. Section 3.2.3 proposes that the reformulations undertaken by the Sydney school's members were made possible by their ignorance of Lévi-Strauss' theoretical innovations in the field of kinship, published in French in 1949, but not in English until 1969 (Levi-Strauss 1969[1949])). By the time that this research had become accessible to Australian anthropology, the crisis era of the discipline was imminent, and the Northern Territory Land Rights Act was already in the process of being formulated (see Section 0.1 above).

The convergence of these two factors, combined with an open hostility on the part of Hiatt and Meggitt towards Lévi-Strauss' theoretical work, meant that kinship research in Australia has remained in a state of effective suspended animation since the late 1960s. Together with the ongoing lack of consilience between anthropology and comparative linguistics, these factors have had the effect of sustaining the antiquated KLRE model as a tacitly group theoretic framework for explaining sociality among Indigenous Australians.

3.2.1 Morgan, Howitt, Radcliffe-Brown & Elkin: Consanguinity & Affinity as Ancillary Orders

Following the divergence of linguistics from anthropology in the 1910s, the single, possibly most attractive feature of kinship for Australian anthropology, was its explicit composition of a lexicon comprised of discrete terms, and a grammar comprised of specific 'rules' of usage. This feature of Australian kinship terminologies endeared them to the kind of 'structural' modelling that leading anthropologists such as Radcliffe-Brown, and later Lévi-Strauss, perceived as the distinguishing feature of the natural sciences (see Chapter 2). This apparent feature of Australian kinship terminologies also rendered Australian anthropologists important interlocutors for the international discipline, as fieldworkers with direct access to Indigenous Australian people, and as the conduits of data for successive theoretical models. This confluence of factors led to a particular emphasis on kinship within the KLRE model that had far-reaching consequences, both for broader anthropological assumptions about Indigenous sociality, and for the specific configurations of anthropological models of Australian kinship. Chief among these consequences was the development of a taxonomy of kinship terminology 'types', which were ranked according to various forms of theoretic ancillary orders, including time, space and normativity.

The most important particular feature of Australian kinship terminologies is their universal incorporation of what are now known as 'sociocentric' terms (see e.g. White & Houseman 2001),

often referred to in English as 'skins' (e.g. McConvell & Bowern 2012), and which are allocated to all people upon their formal incorporation into traditionally-oriented Indigenous communities. Up until the mid-20th century almost every Indigenous person in Australia bore at least one such term, held by Australian anthropologists to predict a typology of relationships between that person and other community members. During the pre-crisis era of social anthropology's development, sociocentric terminologies represented an ideal object of study because of the apparent facility by which they could be represented as systems of interacting parts, in a manner intuitively comprehensible to Europeans with basic mathematical education.

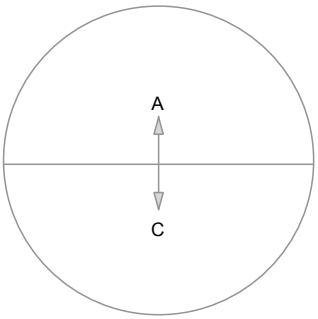
Anthropologists believed that they had found in Australian cultures a kind of elementary programmatic form of social organization, akin to the predictable operation of machinery. Furthermore, the variation of kinship terminologies among communities distributed geographically across the continent appeared to neatly approximate a corresponding geographic variation in language, which comparative linguists had been able to successfully model using phylogenetic techniques (see Section 3.1 above). In the minds of early anthropologists, sociocentric terminologies were thus also amenable to the kinds of ancillary spatial and temporal ordering that had proven so productive at more general levels of theoretical model-making, as shown in Chapter 2.

What is referred to here as the 'standard model' of Australian sociocentric kinship terminologies developed between late 19th and early 20th centuries, leading to its broad acceptance by Australian anthropologists by the 1940s. Its development can be traced over four generational phases, correlating with the approximate age ranges of the researchers involved in its development. It was incepted by Australia's first generation of early, non-academic social anthropologists, led by Howitt, then adopted and refined by its second, professional academic generation, led by Radcliffe-Brown, and finally refined by its third, wholly institutionalised generation, led by Elkin. By the time that the fourth generation of Australian anthropologists were graduating in the 1950s and 1960s, including Berndt, Stanner, Hiatt, and Meggitt, among others (see below), the model was thoroughly entrenched. Although the standard model was ultimately formalised by André Weil in his appendix to Lévi-Strauss' 1949 *Elementary Structures* (Weil 1969[1949]), the 20-year delay in the translation of this text from French to English, in 1969, appears to have inhibited the acknowledgement and adoption of proper formalisations in Australia.

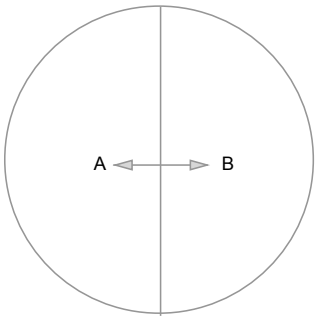
The subfield of 'kinship algebra' that flowed from Weil's work, and reached its peak in Europe and the US during the early 1980s, made no headway into Australian kinship analysis. In the absence of a formal English-language model on which to base further research, a significant body of literature was generated in Australian social anthropology journals between the 1950s and early 1970s, by theoretical debates around how best to phrase the axioms of the pre-1949 model (see discussion of Verdon & Jorion 1981 in Chapter 2). Despite these largely semantic disagreements, the core axioms of the standard model of kinship continued to be republished intact through to the early 1980s. At

this point the overall popularity of kinship research among Australian anthropologists collapsed, largely due to the anti-positivist arguments of Schneider (Schneider 1984 - see footnote 9).

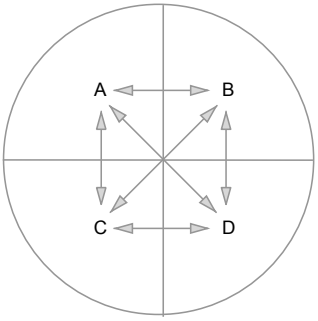
Although analyses of kinship have remained broadly neglected by Australian anthropology since this time, the standard model continues to form the foundation for practitioners' understanding of sociocentric terminologies, and is a staple of the applied 'toolkit' for Australian fieldworkers (see e.g. Sutton 1998). Thus, while significant advances have been made in expanding and refining formal empirical models outside of Australia, as indicated in Section 3.2.2 below, the pre-1949 axioms of the standard model remain in force in Australia to the present day. The current section traces the evolution of this model through key contributing figures of the four generations of Australian anthropology. Particular focus is given to the standard model's construal as a subgroup institution within the broader KLRE model of Australian culture (as illustrated previously in Section 3.1 above), and consequent construals of Australian sociality as a uniformly normative phenomenon, closed under a tacitly axiomatic, group theoretic definitions, as introduced in Chapter 2.



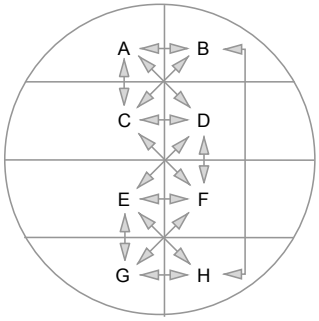
Horizontal- or *'generational' moiety* terms, named 'Aluridja type' by Elkin (1939 *inter alia*). 'A' and 'B' group people in alternate generations.



Vertical- or 'matrimonial' moiety terms, termed 'Dieri type' by Howitt (1880 *inter alia*). 'A' and 'B' group people who intermarry.



Section terms, named 'Kamilaroi type' by Howitt (1880 *inter alia*) and later 'Kariera type' by Radcliffe-Brown (1930 *inter alia*). 'A' and 'B', and 'C' and 'D' group people who intermarry. 'A' and 'C', and 'B' and 'D' group alternating generations.



Subsection terms, termed 'Aranda type' by Radcliffe-Brown (1930 *inter alia*). The permutation of relations is the same as those in section terms, but are extended so that additional groups are generated.

Figure 39: The group-theoretic standard model of Australian sociocentric kinship terminologies. Vertical arrows represent consanguineous relations between terms, horizontal arrows affinal relations. Closure is signified by encompassing circles. Terms are held to define elements within each group, which permute from the matrimonial combination of another two

terms, repeating in an endlessly predictable modular cycle for each terminology. The model is closed under absolute and uniform normativity (Weil 1969[1949]).

As an explicit group theoretic model, the standard model of sociocentric terminologies bijects sets of kin terms into groups (see Chapter 2), with each of the four variations in Figure 39 above comprising such a group. These are closed under two kinds of operation: *consanguinity* (parent-child relations) and *affinity* (spouse-spouse relations). In this configuration, each term forms part of a monoid (Boyd 1969; White 1974), meaning that it is permutable with exactly two other terms: One via a consanguineous operation (vertical and diagonal arrows in Figure 39), and the other via an affinal operation (horizontal arrows in Figure 39). If a person is attributed with a given term 'A' in any of the four variations of the standard model, then they stand in an affinal position relative to a person bearing term 'B', and a consanguineous position in relation to a person bearing the term 'C'. The permutability of terms in each of the four variations of the standard model arises from their *associativity* (the ability for operations to be performed in any order), and *commutativity* (the ability of terms to operate on each other in any 'direction', as indicated by double-ended arrows). The *closure* of the group model means that it is often presented as encoding an 'infinite cycle' of varying modular length (two, four, or eight steps to complete a single cycle). This feature of the model is sometimes used as a rhetorical device in anthropological texts to emphasise the 'timelessness' of Indigenous Australian religious cosmogony (although this portrayal carries the complication of undermining personal agency, implying that Indigenous Australians are people 'without time').

A further, related complication arising from the group theoretic basis of the standard model, is that the concept of 'self' is rendered neutral or 'passive', since any a hypothetical person using the terminology might be associated with any one of the terms[20]. As decontextualized grammars, considered without reference to real-world application, sociocentric terminologies are differentiated from self-centric terminologies by the absence of an explicit 'self' position[21]. However, because the standard model is axiomatically group theoretic, 'self' is realised by the function of an (implicit) identity element, such that the implied term 'self' can operate on any of the explicit terms without altering their operation (such as in the statement, "if woman 'A' marries man 'B' her child will be 'C'"). This group theoretic construal has the further implication of rendering self as a neutral or 'non-active' element, compounding the interpretation of 'weak agency' in representations of Australian culture.

[20] Hence the '*socio*centric' character of the terminology, which effectively presents a 'birds-eye view' of roles and relations that hypothetical people might occupy, as opposed to a '*self-centric*' terminology, which provides a lexicon and syntax for addressing others.

[21] However in *practice* the position of self is realised implicitly in a concrete social setting, where speakers all bear positions (see Stanner 1979 for Durmagam's illustration of the principle, described in Section 3.2.2 below).

The anthropological 'discovery' of Australian kinship terminologies in the late 19th century was amplified by Morgan's (1871) monograph *Systems of Consanguinity and Affinity*, introduced in Chapter 2. Morgan's then-groundbreaking thesis was that all human languages bear a special vocabulary for referring to pairs of people related either by parent-child relations (consanguinity) or spouse-spouse relations (affinity). Although this ostensibly simple hypothesis encodes a complex set of assumptions deriving from European kinship culture, it nevertheless allowed Morgan to develop a powerful set of classifications for the vocabularies of which he was aware. These included 6 sets of terms and relations, or 'systems', named after the populations in which they were in use, including 'Omaha', 'Crow', 'Iroquois', 'Eskimo', 'Sudanese', and 'Hawaiian'[22].

Each system was distinguished by the allocation of what Morgan perceived to be equivalent terms and relations from the perspective of a single person or self, corresponding with 'sibling' in the same generation, and 'parent' in the alternate generation. This generational partition was further divided by a gender partition, which when applied to a parent term, generated 'mother' and 'father', while when applied to a 'sibling' term, generated 'brother' and 'sister'. The further conjunction of generational and gender partitions, yielded 'cousin', 'parallel cousin', and 'cross cousin' in the same generation as self, and 'aunt' and 'uncle', in the same generation as parents (see Figure 40). These definitions were then used to designate relative terms in other so-called systems.

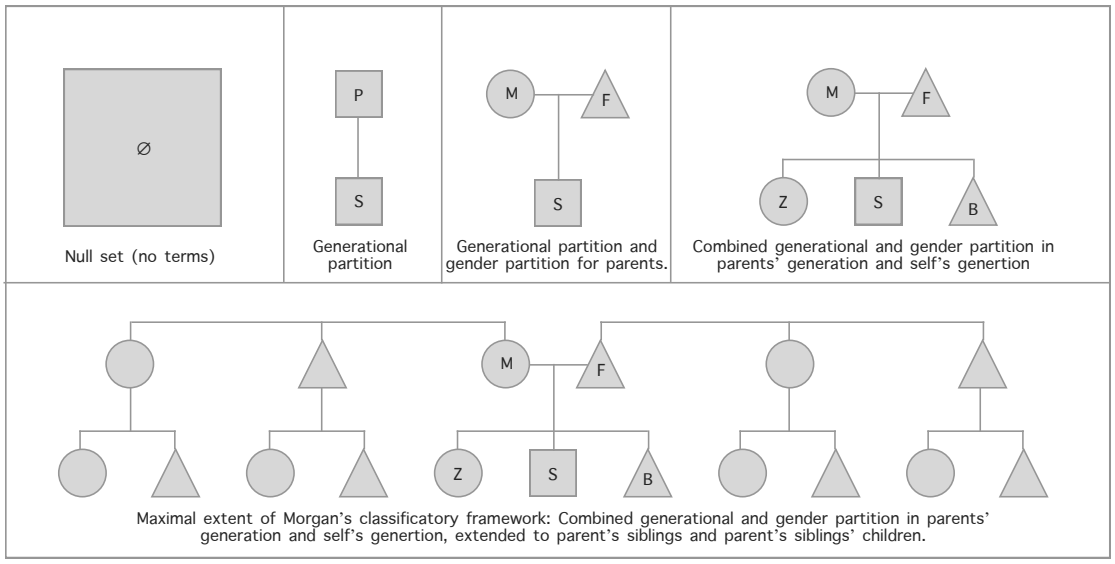


Figure 40: Illustrating the extent of Morgan's (1871) kinship typology, generated from the step-wise population of a null set, successively partitioned by generations and gender, to the theoretical limits of the world's terminologies.

[22] Originally named by Morgan (1871) using a blend of terms from antiquity and Victorian colonial geography, including 'Aryan', 'Semitic', 'Uralian', 'Malayan', 'Turanian', and 'Ganowanian', but clarified and renamed by Kroeber (1909) and Rivers (1914). See also Lowie (1917; 1928); Murdock (1949); White (1958).

The new power of Morgan's model lay in its logical self-consistency. It could be used to detect kinship 'systems' distinguished from each other on the basis of their relative inclusion or exclusion of specific terms. For example, the 'Eskimo system', which includes British kinship terms (Figure 41), was distinguished by its inclusion of only two terms for parents' siblings ('aunt' and 'uncle'), and only one term for all parent's siblings' children ('cousin'). All other five of Morgan's six classes included either a greater or lesser number of terms in each case. The 'Hawaiian' system for example, applied a minimal number of terms to the equivalent relationship positions - the terms 'mother' and father' to all parent's siblings, and 'brother' and 'sister' to all parents' sibling's children. Meanwhile the 'Sudanese' system applied a maximal number of terms to equivalent relationship positions - with every possible generational/gender distinction (as determined by the model) realised by a unique term.

Morgan titled systems with fewer terms 'classificatory' on the basis they 'merge into a single class' two or more distinctly related individuals, those whom other terminologies distinguish. These latter terminologies Morgan titled 'descriptive', on the basis that they provide distinctly descriptive terms for individuals whom other terminologies merge together into a single class. Whereas Morgan's 'Eskimo' system distinguishes 'mother' and 'aunt', for example, and is therefore 'more descriptive', the 'Hawaiian' system merges both individuals under the term 'mother', and is therefore 'more classificatory'.

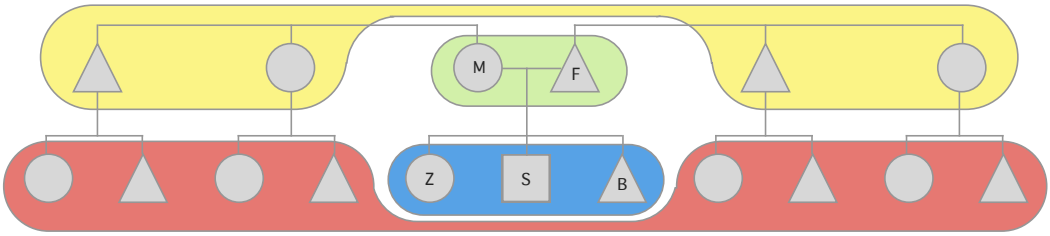


Figure 41: Morgan's (1871) 'Eskimo' typology, with 'merged' positions colour-coded (siblings' and 'parents' blue; 'cousins' and 'uncles/aunts' red and yellow). Note that, according to Morgan's model the 'Eskimo' typology is 'more classificatory', and therefore 'more primitive' than the 'Iroquois' terminology. Although 'Eskimo' is the typology to which British terminologies technically belong, Morgan attempted to argue that British terminologies were 'Sudanese' via their association with more descriptive Roman-era Latin terminologies (Morgan 1871).

Among Morgan's six typologies was one titled 'Iroquois' (see Figure 42), distinguished on the one hand by the 'merging' under one term of parents and same-gender parents' siblings (mother's sisters and father's brothers), and on the other hand the distinction of parents from alternate-gender parents' siblings (mother's brothers and father's sisters). Under such a terminology the children of the former are known as 'parallel cousins', and are merged together with self and parents' children, while children of the latter are classed apart as 'cross cousins'.

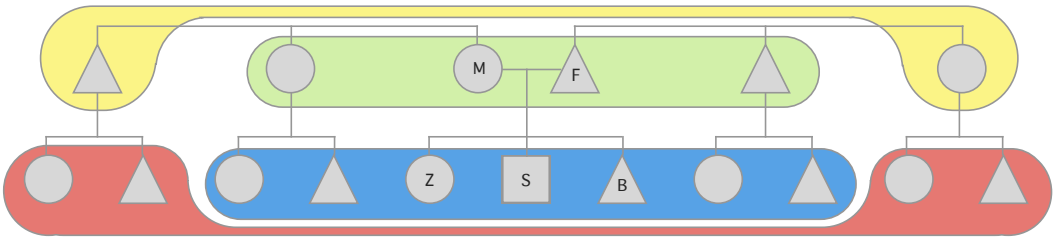


Figure 42: Morgan's (1871) 'Iroqois' typology, with 'merged' positions colour coded ('parallel' kin, aka 'siblings' and 'parents', blue and green; 'cross' kin, aka 'cousins' and 'uncles/aunts', red and yellow). This is the typology later claimed to characterise Australian terminologies (Fison & Howitt 1880 *inter alia*).

Note that this typology is restricted to the classification of terms for consanguineous kin. A seventh, more recently recognised model, termed 'Dravidian' or 'Dravidianate' after the South Asian language community among whom it was first identified by Lounsbury (1964), and comprehensively described by Trautman (1981, 1998, 2001), extends the terminological cross/parallel distinction from consanguineous kin to affinal kin as well, such that each cross-cousin position is structurally equivalent to a 'spouse' position in the terminology, e.g. 'cross-cousin' = 'spouse', while parents' alternate gender siblings are structurally equivalent to 'spouse's parent' in the terminology (see Figure 43).

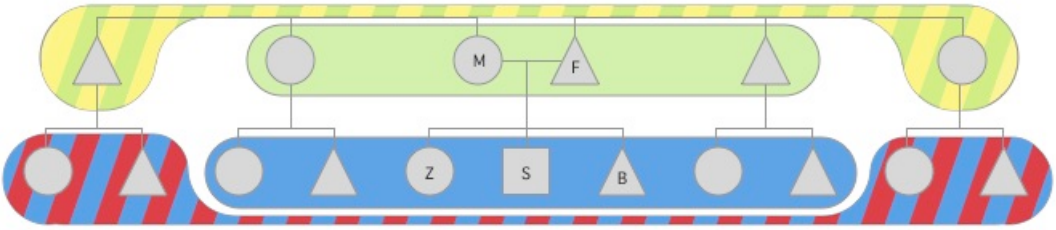


Figure 43: Morgan's (1871) 'Iroquois' typology, incorporating a 'cross-cousin marriage preference' that was later clarified by Lounsbury (1964) as representing a new typology that he named 'Dravidian'. The later distinction was determined by the optionality of the classification of cross-kin, who may be referred to as *either* consanguines *or* affines, as indicated here by dual shading.

This terminological isomorphism between cross-cousin and spouse is held to encode a so-called 'cross-cousin marriage preference', with normative force on people's marriage choices in the real world. Furthermore, in Lounsbury's (1964) account there is no distinction in Dravidianate terminologies between consanguineous and affinal cross kin, such that one set of terms is reportedly borne by both, irrespective of whether terminology users consider consanguineous links to exist between ego and alter. The result is that 'wife's father' is addressed as 'mother's brother' irrespective of whether these positions are genealogically corroborated.

This classificatory logic led to the early conflation of Iroquois and Dravidianate terminologies by social anthropologists, hence the temporal lag between Morgan's initial discovery in 1871 and Lounsbury's

distinction in 1964. This lag was perpetuated by Lowie (1928) and Murdock (1947) (see White & Houseman 2001), and ushered in a long-lived characterisation of Australian terminologies as encoding a 'cross-cousin' marriage preference, extending from Morgan, through Howitt, Radcliffe-Brown, Elkin, and including Lévi-Strauss and beyond, as discussed below.

In the period preceding Lounsbury's discovery, the standard model of Australian sociocentric terminologies grew out of the 'group marriage' hypothesis of Morgan (1871) and his loyal follower Howitt (1880; 1904. - see discussion in Chapter 2), and reached its zenith with the elaborations of Radcliffe-Brown (1930 *inter alia*). The apparently self-evident, albeit tacit group theoretic features of closure, permutability, associativity, commutativity, inverse elements, and identity elements, which emerged as the model developed, all point to a highly self-referential framework. Rather than seeking isomorphism with autochthonous kinship grammars, demonstrably meaningful to real world culture bearers in specific and complex circumstances, Morgan's group theoretic model ingrained the foregone conclusion that Australian kinship was of an inherently 'primitive' type, based on the self-referential logic of tacit group theory.

Note that there is no inherent order in Morgan's model. As described in detail in Section 2.2.1, the construal of variation among kinship terminologies in terms of discrete 'systems' serves a bijective function, such that elements of a set (all known terminologies) are remapped into a group of six terminology 'types', which are closed under the uniform operation of consanguinity[23]. Although there is a partial order implied in the application of generational and gender partitions to sets of terms in Morgan's model, it does not confer a congruent explanation on either the links between terms within a single system, or on links between systems. In other words, it does not yield an inherent order among the terminology types that might explain the variation in terminologies over either historical time or geographic space.

In Halliday & Matthiessen's (2006) analysis (see Chapter 2), Morgan's terminological typology comprises a classification without order, and thus without explanatory power. In order to imbue his model with such power, Morgan hypothesised that the distinction between classificatory and descriptive terms could be ranked along a spectrum, with 'most classificatory' at one end, and 'most descriptive' at the other. To this spectrum he then appended an *a priori* temporal order, speculating that classificatory terminologies such as his 'Iroquois type' reflected an older kinship culture, while descriptive terminologies such as his 'Sudanese type' reflected a newer culture (Morgan 1871:vi; Trautman 2001). In other words, Morgan *confected* an order from the number of terms in each terminology - asserting that an increase in number of terms corresponded to an increase in

[23] Where affinity is implied by exclusion. In Weil's later formalization relations are treated as elements, and individuals are treated as functions operating on those elements, such that one kind of marriage *generates* another kind, subsuming the dimension of consanguinity (Weil 1949).

civilization[24]. This ancillary order allowed Morgan to postulate a causal 'flow' of culture change across space and time, with 'primitive' classificatory kinship cultures yielding to later 'barbaric', and then 'civilized' descriptive cultures, as the latter spread from one geographic region to another (Morgan 1871:vi). Morgan referred to a hypothetical original form of human kinship culture as 'group marriage', whereby parents, siblings, and spouses were undifferentiated (Morgan 1871:vi).

Although not in possession of Australian data when *Consanguinity and Affinit*y was published, Morgan later asserted in his introduction to Fison & Howitt's influential 1880 text *Kamilaroi & Kurnai* (Fison & Howitt 1880 - see discussion in Chapter 2 above), that Australian kinship terminologies were of a classificatory 'Iroquois' type (which Morgan had originally called 'Turanian'). This text served two important functions in the development of the standard model for Australian anthropology, both marking Howitt as the preeminent representative of Australia's first generation of social anthropologists, while at the same time establishing Morgan's six-part classificatory model as an authoritative theory for the emerging discipline.

The 'Iroqois type' classification, attributed to Australian terminologies by Morgan, was based on advice sent to him by Howitt. This advice indicated that a single 'merged' term was used for both parents and same-gender parents' siblings (i.e. one term for mother and mother's sister, and one term father and father's brother respectively), while alternate gender parents' siblings (mother's brother and father's sister) bore distinct terms ('uncle' and 'aunt'). Even more compelling for Morgan and Howitt's theory, the children of same-gender parents' siblings were apparently denoted by the same term as parents' children ('brother' and 'sister'), while the children of alternate-gender parents' siblings bore distinct terms (which they equated to the British term 'cousin') (Fison & Howitt 1880:60). This seemed to strongly support Morgan's model and made a valuable ally of Howitt in particular, as the latter went on to promote Morgan's theories over the course of his career (e.g. Howitt 1883; 1885; 1891; 1904).

Further to supporting the taxonomic structure of Morgan's terminological model, Australia's great geographic distance from other parts of the world appeared to neatly reinforce the ancillary ordering mechanisms that Morgan also asserted, with classificatory systems corresponding to older, more remote, and thus 'more primitive' cultures. Even more useful for Morgan were Australian sociocentric terminologies, which appeared maximally classificatory due to their application of only a limited number of terms to all people, irrespective of the position of self. Howitt, backed by Morgan,

[24] An early warning that this ordering mechanism was arbitrary is visible in the caveat applied by Morgan to the British kin terminology. The relatively small number of terms in the British terminology indicates that it is 'more classificatory' than 'descriptive' according to Morgan's model, and therefore more primitive than, say, the Sudanese terminology. However Morgan argued that since classically educated Britons were competent in the use of the highly descriptive Roman terminology, they should be properly classified civilized (Lowie 1928).

then embarked on a life-long campaign to promote a model of 'group marriage', which he claimed was evidenced by a 'half-sister marriage' prescribed by the sociocentric terminologies of South East Australian people[25] (Fison & Howitt 1880:23 - see APPENDIX 1 for extended quote).

This effort on Howitt's part, initiated the theoretical evolution of the 4-part standard model of Australian sociocentric terminologies, presented at Figure 39 above. In this regard Howitt helped to extend Morgan's own model by developing a further subgroup of terminology types according to relative numbers of sociocentric terms, and, following Morgan's lead, arranging them in a temporal order (see Figure 44). In 1880, Howitt distinguished three supposedly distinct terminology types, including 'Dieri type', 'Kamilroi/Turra type', and 'Kurnai type', corresponding with the group theoretic structures at Figure 39 (top-right) for Dieiri, and Figure 39 (bottom-left) for Kamilaroi and Turra[26] with an alleged absence of terminological structure altogether for Kurnai - Fison & Howitt 1880:342 - see APPENDIX 1 for extended quote).

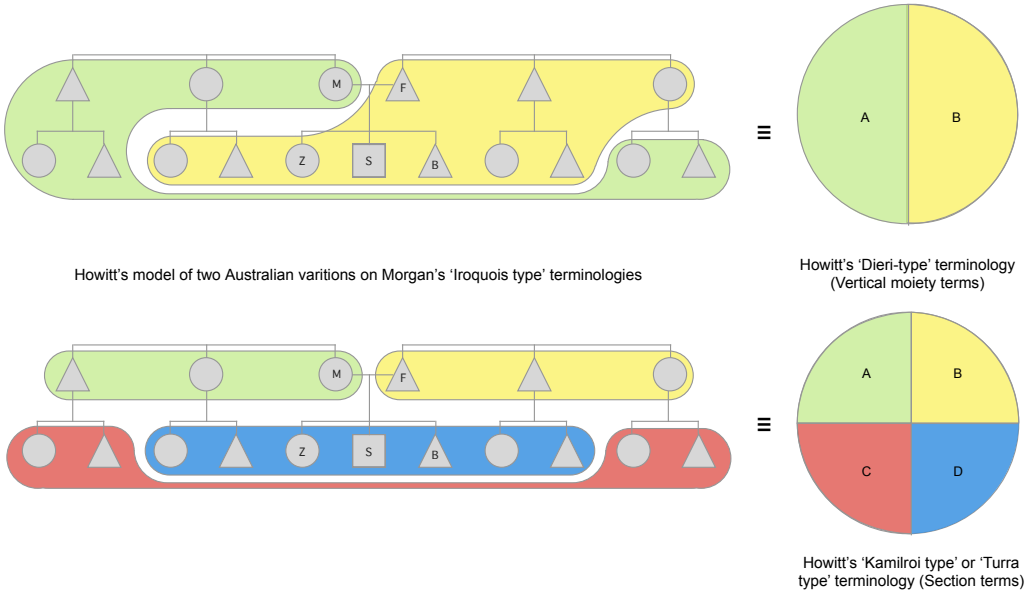


Figure 44: A comparison of two of Howitt's (1880) supposed variations on 'Iroquois type' kinship characterizing Australian terminologies, showing imputed isomorphism with sociocentric terminologies on the right. These have subsequently been reclassified as variations of a 'Dravidian' typology (White & Houseman 2001). The third terminology typed distinguished by Howitt's (1880), 'Kurnai type' was construed as a null set due an alleged absence of terminological structure altogether.

Howitt maintained this view for the rest of his life. As he neared death in 1908 however, he began to shift his emphasis from temporal to spatial order, possibly in response to an increasing number of ethnographic observations from across Australia, which although ad hoc, indicated a mismatch

[25] Howitt's contemporaries Spencer & Gillen, working in Central Australia, also allied themselves to this view, which helped to anneal the model against criticism by more experienced and informed researchers (Spencer & Gillen 1898).

[26] Where the variation in the gender definition of a vertical moiety is irrelevant.

between the geographic distribution of language, and that of kinship terminologies (Howitt 1904:137 - see APPENDIX 1 for extended quote). This shift is significant in indicating a waning in the certainty that had accompanied Howitt's (and Morgan's) earlier *a priori* assumptions. The circumspection came too late however, as the majority of Howitt's earlier writing formed the basis on which the subsequent generation of Australian anthropologists relied for their own work.

Following Howitt's death, Radcliffe-Brown inaugurated Australia's second generation of anthropologists by taking up the former's classification of Australian terminologies. Echoing Kroeber's (1909) and Rivers' (1914) re-labelling of kinship 'types' in Morgan's model (see footnote 22, this section), Radcliffe-Brown refined and renamed Howitt's terminology types over the course of several articles published between 1918 and 1929 (Radcliffe-Brown 1918; 1929 *inter alia*). This exercise ultimately generated the 3-part classification 'Dieri-', 'Kariera-', and 'Aranda-type' systems (see Figure 39). Here, Radliffe-Brown's 'Karieria-type' replaces Howitt's 'Kamilroi-/Turra-type', while his 'Aranda-type system' represents a new addition (Radcliffe-Brown 1930)[27]. After 1923 Radcliffe-Brown became more explicit in distinguishing his position on both the temporal order appended to variation in kinship terminologies by Morgan and Howitt, and the role of kinship as a subgroup 'institution' of the KLRE culture model (Radcliffe-Brown 1931:427 - see APPENDIX 1 for extended quote).

Rather than emphasise the ancillary temporal order of historical development for which Morgan and Howitt had advocated, Radcliffe-Brown was interested in emphasising a *geographic* order. For Radcliffe-Brown, geographic order appeared to emerge from variation in terminologies borne by apparently distinct communities distributed across the continent. This position resonates with his model of 'tribe', described in the previous section, as a completely unified entity bound together by interdependent variations across all four of their constituent subgroup institutions, kinship, language, religion, and economy, and identifiable by language variation (see Figure 45).

Radcliffe-Brown's explanatory order derived from this theoretical uniformity of institutions within each 'tribe', such that 'asking a native what language he speaks' acts as a cookie-cutter, in the terms defined in Chapter 2, to reveal his equivalently discrete and corresponding kinship terminology, religion, economy, and ergo, the 'tribe' to which he belongs (see Figure 45). As the previously cited quote shows (Radcliffe-Brown 1931:427 - see APPENDIX 1 for extended quote), Radcliffe-Brown had specifically criticised Kroeber (1909) for suggesting that the structures of kinship, language, religion, and economy could vary independently of one another within a given cultural community. The reason that Kroeber's model of *in*dependent variation between kinship terminologies and other institutions appeared threatening to Radcliffe-Brown in 1931, was that Radcliffe-Brown had spent the

[27] Radcliffe-Brown initially classifies these as two broader sets, 'Type I' ('Kariera type', previously referred to as 'Kamilaroi type'), and 'Type II' ('Dieri type' and 'Aranda type') terminologies, within which he alluded to 'less developed' and 'more developed' variants (Radcliffe-Brown 1918; 1923)

past 13 years arguing precisely that these institutions did *not* vary independently. Thus, even though he and Kroeber were united by their criticism of Morgan's (and Howitt's) *a priori* temporal order, Kroeber converged with Howitt in Howitt's later view that kinship terminologies could vary independently of those other subgroup institutions by which a 'tribe' might be determined. Morgan's and Howitt's group theoretic model of kinship thus survived in Radcliffe-Brown's own reconfigured model, but with sufficient modification that Radcliffe-Brown was able to claim a distinctly original contribution.

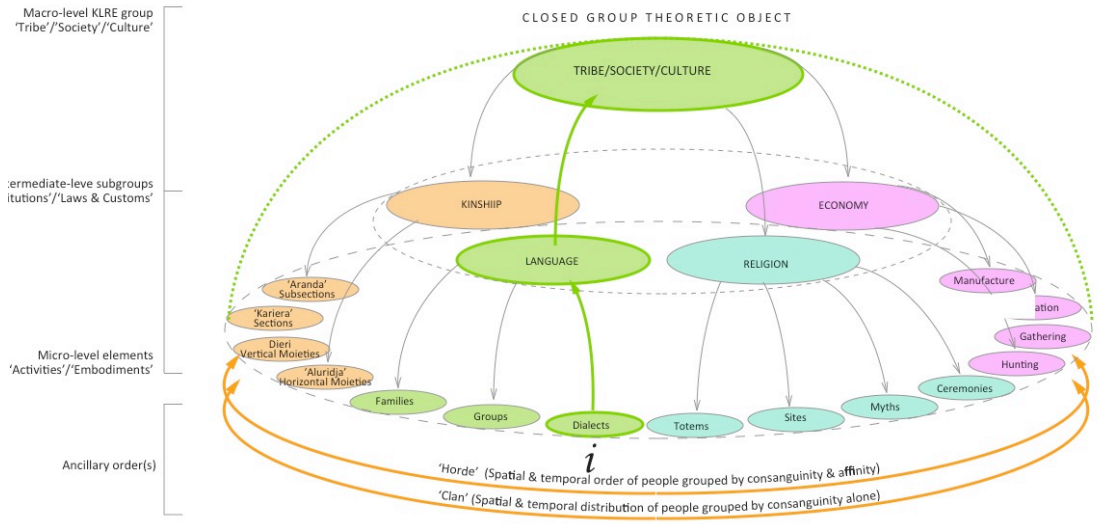


Figure 45: Radcliffe-Brown's 1930s reformulation of the KLRE model (Radcliffe-Brown 1930; 1931). The superordinate group 'tribe' is identified by the culture element 'dialect', while ancillary spatial and temporal order is appended to culture elements of the subgroup 'kinship', and used to explain the distribution of the entire group.

In Radcliffe-Brown's modified construal, visualised at Figure 45, kinship is used as an ordering mechanism to attribute causality to the entire group theoretic model of society, where the terms in the sociocentric terminology are held to predict the distribution of people in space and time. This order is not appended directly onto the superordinate group 'tribe', or the subgroup 'kinship', but rather onto the 'culture elements' of this latter institution - the individual terms within the terminology. Radcliffe-Brown's view of the role of Australian kinship terminologies was that they prescribed two kinds of group theoretic entity that could be observed in the spatial and temporal distribution of real people: One group delimited by uniform relations of consanguinity alone, and the other group delimited by relations of consanguinity *together with* relations of affinity. The terms that Radcliffe-Brown used to refer to these two theoretical groups varied over the duration of his career, but towards the end of his life he began to refer to them more consistently as 'clan' and 'horde'[28]

[28] Howitt (1904) was the first to adopt a dualistic use of these terms, although he uses them with a completely different meaning: 'horde' for denoting sets of people bijected into groups by common matrifilial kin terms, and 'clan' for denoting sets of people bijected into groups by common patrfilial kin terms (see Chapter 2 for discussion).

(Radcliffe-Brown 1956:635 - see APPENDIX 1 for extended quote). These concepts subsequently came to take on major significance for Australian anthropologists' view of Indigenous sociality, including continuing support for the 19th century KLRE model. The theoretical link that Radcliffe-Brown constructed between clans and hordes as observable groups of *people* on the one hand, and moieties, sections, and subsections, as groups of *ideas* represented by kin terms on the other hand, had been spelled out 27 years earlier (Radcliffe-Brown 1929:411 - see APPENDIX 1 for quote). Correlating these statements with his other descriptions of the standard model cited above, visualised at Figure 39, Radcliffe-Brown's latter statement construes an isomorphism between groups of terms and groups of people (see Figure 46).

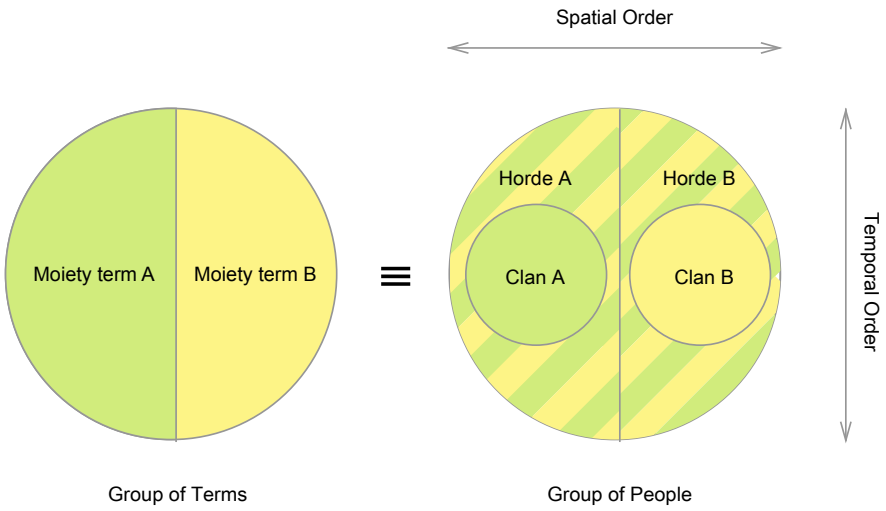


Figure 46: Radcliffe-Brown's (1929; 1956) predictive model of isomorphism between idea systems (sociocentric terminologies) and observable patterns of behaviour (geographic concentrations of people). Both are group theoretic objects in the model, with observable behaviour partitioned into a supergroup (horde) and subgroup (clan), collocated in space and time. Note the discrete uniformity, and closure of the sociality construed.

In Radcliffe-Brown's (1956) adaptation of the KLRE model, autochthonous kinship terminologies are corollaries of anthropological group theoretic entities 'clan' and 'horde'. For Radcliffe-Brown, the link between idea systems and observable social behaviour is one based on isomorphism between group theoretic structures, which depended on kinship terminologies themselves being group theoretic. *The link is theoretically self-consistent but not empirically demonstrated.* In order for clans and hordes to be logically self-consistent theoretical structures - closed groups delimited by uniform relations of patrilineal descent - it was also necessary for moiety, section, and subsection terminologies to be construed as closed groups delimited by uniform relations of descent. This *a priori* necessity meant that it was imperative for the group theoretic basis of sociocentric terminologies to remain intact in anthropological representations.

As with his adoption of Radcliffe-Brown's KLRE model, Elkin, Australia's second anthropology professor and inaugural member of the country's third generation of anthropologists, also adopted

wholesale the former's typological model of kinship terminologies, along with the ostensible link between them. In relation to the terminological typology, Elkin added a further classification that he termed 'Aluridja type' (see Figure 39 - top-left)[29], bringing the number of recognised sociocentric 'systems' found in Australia to a total of four (Elkin 1939; 1940; 1955). Elkin also consolidated an alternate terminology for these terms, including 'subsection' (Radcliffe-Brown's 'Aranda type'), 'sections' (Radcliffe-Brown's 'Kariera type'), and moieties (Radcliffe-Brown's 'Dieri type'), which later came to form the standard lexicon, as shown at Figure 39 (Elkin 1939; 1940; 1955).

Just as Radcliffe-Brown had differentiated himself from Howitt on the issue of spatial and temporal order, so too Elkin sought to distance himself from his predecessor by innovating with ancillary orders. In this regard, it was Elkin who introduced to Australian social anthropology the theme of post-contact culture *change*. This theme distinct from the trope of culture *collapse* which had formed an important focus for predecessors such as Howitt (1880; 1904), and which remained a major element of Radcliffe-Brown's responses to some of Elkin's criticisms (Radcliffe-Brown 1956). The change that Elkin sought to promote in respect of kinship terminologies was one of cultural *diffusion* between Indigenous communities, triggered by vaguely defined 'disruption' of British colonisation[30]. This new form of ancillary spatial and temporal order, appended to the group theoretic standard model of Australian kinship, became the basis on which Australia's fourth generation of social anthropologists developed their own views of Indigenous kinship.

3.2.2 Berndt, Hiatt, Meggitt, and Stanner: Space & Time as Ancillary Orders of Kinship

Although Elkin remained professor of Australia's first anthropology department from 1932 until his death in 1979, his pre-eminence within the national discipline subsided after the 1940s. His dwindling role as an academic innovator belied his deeper influence on the discipline however, as his graduating students emerged to become the fourth generation of professional Australian social anthropologists, and to form the vanguard of Australia kinship research during the 1950s and 1960s. In particular, a group who came to be known as the 'Sydney School', including Stanner, Berndt, Meggitt, and then Hiatt (see discussion in Chapter 2), emerged to provide the final theoretical contributions to Australian kinship theory, concluding a century of theoretical reformulations, and leaving Australian kinship theory where it stands today.

In the late 1950s Berndt, who was at that time a recent graduate, began to question the uniformity-based closure of the KLRE model[31]. Having conducted his first fieldwork in inland South East Australia

[29] What Elkin called 'Aluridja type' he also sometimes referred to alternately as 'generational levels' (White 1981)

[30] This new form of temporal and spatial order was distinct from the 'intelligent parasitism', also invented and promoted by Elkin, which described an inevitable assimilation of Indigenous people into British colonial society (Elkin 1952).

[31] This was a question already raised by Elkin (Radcliffe-Brown 1954), but not resolved.

under Elkin's close supervision in the 1940s (Berndt & Berndt 1943), Berndt went on to conduct more extensive and independent research with people of the Western Desert in Central Australia (Berndt 1959 *inter alia*). As a result of his observations there, Berndt found Elkin's previously-discussed support for Radcliffe-Brown's reformulated KLRE model untenable. Despite these concerns, Berndt took the 4-part sociocentric kinship typology developed under this regime as established, and in no need of further investigation. This typology demonstrated to Berndt an 'ideal' program of intermarriage between lineages of consanguineous descendants identified by each terminology type. Berndt also accepted without question the identity function of language (described in Section 3.1 above) in bijecting subgroup institutions of kinship, language, religion, and economy into a single group theoretic object called, which he chose to call 'bloc' (see Figure 47).

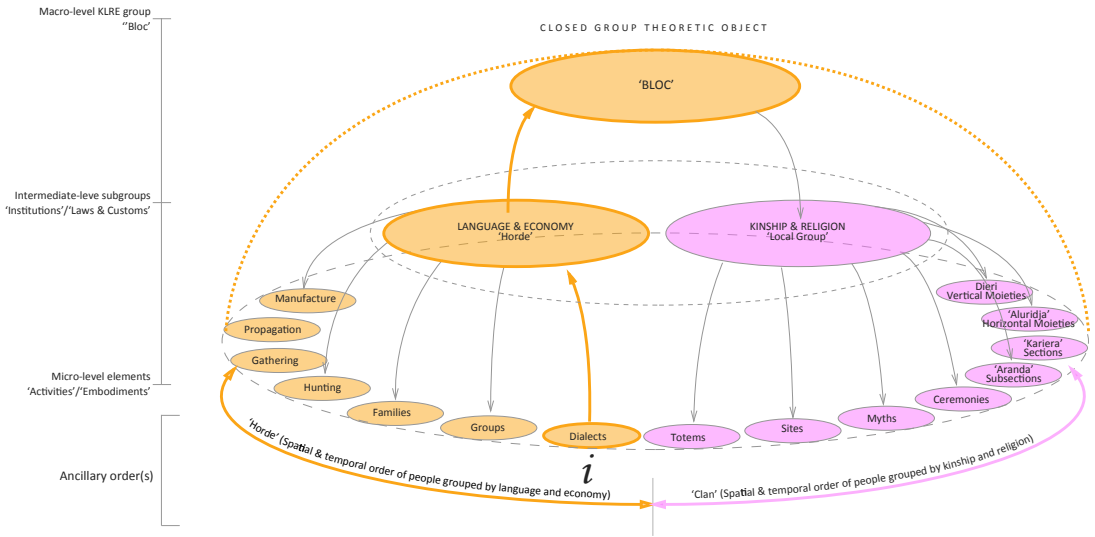


Figure 47: Berndt's (1959) adaptation of the KLRE model, conjoining subgroups 'language and economy' into a single entity called 'horde', and kinship and religion into a single entity called 'local group'. The identity function of language does not change in closing the entire group off. Nor does the role of ancillary spatial and temporal order in imbuing the object with causal 'dynamism'. The appending of two discrete orders allows Berndt to explain a lack of congruence between micro-level observations and the closed KLRE group.

The difficulty for Berndt was that his observations suggested to him a lack of congruence between the spatial order predicted by the kinship terminology, in which on the one hand abstract groups defined by consanguineous lineality were supposed to occupy discrete, contiguous tracts of lands, cumulatively aggregating into a single language-identified territory, and on the other hand the spatial order of real people, whom he did not see as instantiating this model. Unfortunately this lack of congruence between theoretical models and observed reality was only *intuited* by Berndt, rather than measured, and he struggled to clearly articulate it. Lacking self-consistent definitions and empirical data with which to realise his intuition, he instead distinguished between existing group theoretic entities 'local group' and 'horde', defined by abstract 'activity types' rather than the observed actions and attributes of real people. For Berndt, these types of activity were 'kinship' (defined by discrete instances of consanguinity), 'language' (defined by the use of discrete dialects), 'religion' (defined by

discrete totems, instances of ritual, mythology, and responsibilities to totemic sites), and 'economy' (defined by food gathering over discrete tracts of land) (Berndt 1959). The key problem in Berndt's eyes was that the spatial ordering of each of these activities did not appear to be isomorphic with one another, as Radcliffe-Brown had predicted. Radcliffe-Brown had clearly stated that Australian sociocentric kinship terminologies encoded a preference for affinal exchanges between consanguineous lineages, as illustrated at Figure 46), and that these lineages physically occupied contiguous sets of geographic regions delimited by both religious and economic sites to which rights were hereditarily transmitted (Radcliffe-Brown 1918; 1923; 1929; 1956 *inter alia*). Such consanguineous lineages were thus spatially fixed in Radcliffe-Brown's model, with only women moving between them at the time of marriage.

In rejecting Radcliffe-Brown's model, Berndt (1959) walked a fine line in maintaining his loyalty to Elkin, since Elkin had adopted the model wholesale. Thus, while critical of Radcliffe-Brown's rigidly closed, and uniformly ordered model, Berndt appeared to compromise by tacitly endorsing the language-identified version of the KLRE model that Elkin continued to support, along with the *a priori* 'horde'/'local group' distinction, while at the same time attempting to subtly partition the four institutional subgroups of kinship, language, religion, and economy in such a way that they appeared more congruent with his own observations.

To this end, he advocated two changes to the KLRE model developed by his predecessors. Firstly, he redefined the group theoretic object 'horde' as *encapsulating* both users of a discrete dialect (as an element grouped within the institution 'language') and members of a discrete food-gathering workforce (as an element grouped within the institution 'economy'). Simultaneously, he redefined the theoretical object 'local group' as *delimited by* both discrete consanguinity (as an element grouped within the institution 'kinship'), and by discrete totems (as elements grouped within the institution 'religion'). Secondly, he partitioned the uniform spatial order predicted by Radcliffe-Brown (and Elkin) into two discrete fields, so that 'hordes' and 'local groups' each occupied discrete geographic regions that had no systematic relation to each other. In this reconfigured spatial order the geographic tracts occupied by hordes were conterminous and cumulative, whereas the geographic tracts occupied by local groups were random and disjointed, although both were subsumed within a single closed, spatial distribution affiliated with a single language (see Figure 47; Berndt 1959:83-84 - see APPENDIX 1 for extended quote):

Berndt thus managed to edge away from Elkin's endorsement of Radcliffe-Brown's uniformly ordered model of the tribe, while simultaneously criticising Radcliffe-Brown by suggesting that Elkin's exculpation "might be inferred" (Berndt 1959:83-84). The more important issue here however, is the way that Berndt construes kinship, which he mysteriously restricts to consanguineous descent, and separates from 'genealogical articulation' as a potentially 'implied' rather than 'real' expression of sociality (Berndt 1959:83-84). Because Berndt subscribes to a definition of 'local groups' as the

inevitable products of the standard model, with section and moiety names inherited consanguineously, his failure to detect the existence of local groups by ad hoc observation compels him to find some other way to account for the validity of the 'horde'/'local group' model. This he achieves, by separating kinship terminologies from the genealogical relations that they encode, and in doing so, eviscerating the autochthonous model of its meaning for Western Desert people, along with the inherent order of the very observable thing that he is attempting to classify: Spatially cohesive communities of people. Rather than looking for isomorphism between the autochthonous logic of a kinship terminology and the observable behaviour of real people, Berndt deliberately severs the link between the two. So total is this evisceration that even the adjective 'local' is stripped of its usual meaning as part of the term 'local group', referring to a thing that is situated in a single *location*. Instead, in Berndt's usage, it now refers to people who are distributed across multiple distinct locations, and who exhibit no systematic cohesion, except the vague and speculative auspices of 'real or implied kinship' (Berndt 1959:83-84).

Berndt here seeks instead to replace the usual, observable order of real people with what he calls the 'significance' of kinship terminologies (Berndt 1959:83-84). Most strikingly, he states frankly the deliberate stripping away of empirical data from the autochthonous idea systems he intends to re-classify, as an irrelevant "*mass of empirical material*" (Berndt 1959:83-84). Unfortunately the hidden 'significance' that he sought to uncover, is attributed entirely by him alone, rather than by the people who actually use the kinship terminologies in question. These terminologies, in Berndt's construal, are not autochthonous idea systems for realizing meaningful and observable relations between real people, but rather an abstracted 'ideal' concept, detached from 'real' expressions of sociality. For Berndt, this explains why Radcliffe-Brown's patrifilial 'local groups' exist only in an abstract theoretical dimension, and exhibit an arbitrary and disjointed spatial order. In Berndt's view local groups are mental precepts of Indigenous people that manifest only in the abstract terms of the kinship terminology, and not in the real and observable instantiations of people's lives.

Berndt includes numerous references to relatively specific Western Desert locations in his 1959 text, and to specific, recorded numbers of people, their imputed languages, and geographic movements. However, all underlying orderings in space and time, including geographic distributions, and changes in those distributions over time, refer to abstract group theoretic objects closed under one term or another, rather than to either real and specific individuals, or to real and specific relationships between them (Berndt 1959:91; 93 - see APPENDIX 1 for extended quote). Berndt instead provides his own definition for 'the actual significance of the tribe' (Berndt 1959:83). Through the newly refined definition of *a priori* theoretical object called 'local group', its distinction from an *a priori* theoretical object called a 'horde', and the argument that, because both the latters' respective spatial orders are (allegedly) dimorphic, there is no such thing as the dependent theoretical object called 'tribe'. Instead, Berndt replaces this group theoretic object with another group theoretic object called a 'bloc', which is still identified by language ('dialect variation'), still encompasses permutable

institutions of kinship, language, religion, and economy, and is still appended with a spatial and temporal order, derived from micro-level elements, such as kinship terminologies, sacred sites, and economic activity. The difference is that these are now partitioned, with one spatial and temporal order appended to the <language-economy> subgroup 'hordes', and another applied to the <kinship-religion> subgroup 'local groups', as shown in Figure 47 (Berndt 1959:105 - see APPENDIX 1 for extended quote).

Berndt's new invention - a clarified definition of the group theoretic object called by him, 'local group', also referred to as a 'totemic descent group', and later as 'clan' (Berndt 1959) - is effectively a conjunction of two subgroups within the KLRE model: kinship and religion, where totems and their affiliated sacred sites, myths, and ceremonies, are acknowledged as being transmitted via kin term-based lineages. His corollary invention - a clarified definition of the object called 'horde' - later referred to by him as 'band', which comprises parts of multiple intermarried 'local groups' (Berndt 1959) - is effectively a conjunction of the remaining two subgroups: Language and economy, where economically productive communities are acknowledged as speaking convergent language varieties (see Figure 47).

Berndt openly and deliberately treats sociocentric kinship terminologies, which realise the transmission of associated totems, responsibilities to sacred sites, myth, and ceremonies, as *detached from* the real, observable relationships that give rise to observable sociality. His theoretical innovation is to divide the subgroups of the KLRE model into two parts: One concrete and observable with one kind of temporal and spatial order (the 'horde'), and the other abstract and unobservable with a discrete spatial and temporal order (the 'local group'). This allows him to explain why his observations do not match the group theoretic model of 'tribe' (or 'bloc'), while simultaneously maintaining the standard model of sociocentric kinship terminologies which so neatly approximates Indigenous Australian's own explanations of their sociality.

In 1962, three years after Berndt's influential article was published, Hiatt, then a postgraduate student at the nascent anthropology department at ANU[32], launched an even more explicit and excoriating criticism of Radcliffe-Brown's model, asserting that there was *no* uniformity between kinship, economic, and religious subgroups within the KLRE model, as the latter had argued. Citing newly documented field data, including Berndt's, Hiatt pointed out that there was a lack of congruence between a) the geographic distribution of sites associated with any given totem; b) the geographic distribution of people who bore that totem and thus formed a descent group, and; c) the geographic distribution of people who cooperated economically, replacing Berndt's term 'local group' with his own 'totemic descent group' (Hiatt 1962:286 - see APPENDIX 1 for extended quote). This

[32] The department was established in 1951 under Nigerian Nupe specialist S.F. Nadel as foundation chair. Nadel died in 1956, after which Hiatt switched to Sydney where he eventually became professor.

challenge marked the beginning of the great debate documented in detail by Verdon & Jorion (1981), as described in Chapter 2. The central argument in this dispute was no longer about the classification of kinship terminologies at all, but entirely about the classification of spatial order as an explanatory mechanism attached to closed models of abstract groups.

Two years after Hiatt's opening foray, anther of Elkin's students, Meggitt, reinforced Hiatt's position in separating the spatial order of what he and the Sydney School[33] perceived to be *economically* defined groups, from the spatial order of what they perceived to be *kinship* defined groups, and consequently, *religiously* defined groups (Meggitt 1964:170 - see APPENDIX 1 for extended quote). Meggitt's inversion of the definition of a 'horde' (used here it to refer to what Berndt called a 'local group', and what Hiatt called a 'totemic descent group') reveals the severity of anthropology's ongoing inability to develop a self-consistent technical language[34]. Meggitt's overall position however, clearly supports that articulated by Hiatt in 1962, and Berndt in 1959 (Berndt 1959; Hiatt 1962), attacking Radcliffe-Brown's model of the KLRE group, as exhibiting a uniform spatial and temporal order. Instead of the geographic and consanguineous lineage-based cohesion advanced by Radcliffe-Brown, Meggitt advanced a model of *ecologically* driven idiosyncratic population movements. These movements he argued, were rendered intelligible by kinship terminologies, albeit in metaphorical terms. Apparently encouraged by Meggitt, Hiatt clarified and reinforced his original 1962 criticism of Radcliffe-Brown in a further article in 1968 (Hiatt 1968:99-100 - see APPENDIX 1 for extended quote).

Berndt's, Hiatt's, and Meggitt's attacks on Radcliffe-Brown did not go unopposed. In 1965, one of Elkin's earliest and most senior students, Stanner[35], responded directly to Hiatt's 1962 criticism, asserting that the 'activiy types' of the four KLRE subgroup institutions *were* consistently ordered in space and time (Stanner 1965). Here, Stanner argued that the lack of consistency observed by Hiatt and others was not the result of incongruent classification in Radcliffe-Brown's model, but of over-simplified spatial and temporal order. According to Stanner, all members of a given language-identified superordinate group (whether termed 'tribe', 'bloc', or 'society') exhibited two simultaneous forms of order: One religious, and one economic. Religious order was manifest on some occasions, while economic order was manifest on others. Thus, while on hand temporally distinct, they were on the other hand spatially coherent, with every individual within a given superordinate 'society' (in Stanner's terms) maintaining a consistent attachment to a restricted

[33] Which had now grown to include Hiatt together with Elkin, Berndt, and Meggitt (Verdon & Jorion 1981).

[34] Radcliffe-Brown had himself frequently and inconsistently inverted the definition these terms (Stanner 1965).

[35] Stanner studied for an MA under Elkin in 1934, but had been earlier influenced by Radcliffe-Brown while the latter was still professor of the department. Stanner was 11 years older than Berndt, 19 years older than Meggitt, and 26 years older than Hiatt. Unlike these junior anthropologists he had also seen active wartime service, in which he helped establish the all-Indigenous military patrol unit in Arnhem Land that developed into what is now called NORFORCE.

geographic space delimited by the religious sites associated with their own totem, each of which was encompassed by a more extensive geographic space delimited by the economic sites on which they hunted and gathered. These spaces were contiguous with each other according to Stanner, and aggregated into a single superordinate geographic region that he called a 'domain', coextensive with the superordinate group or 'society', identified by language (Stanner 1965 - see Figure 48).

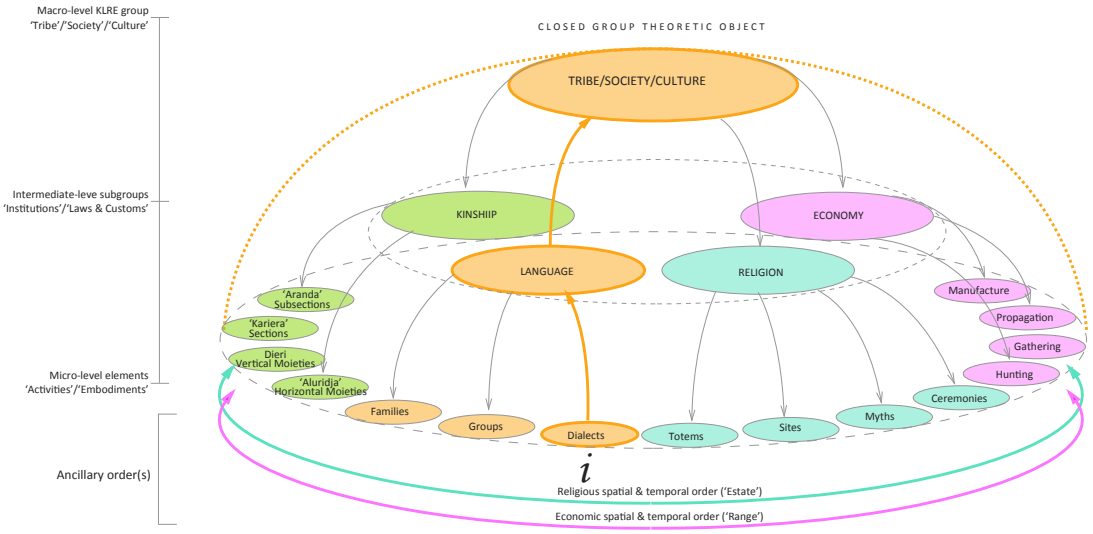


Figure 48: Stanner's (1965) adaptation of Radcliffe-Brown's construal of the KLRE model. It is identical except for the addition two simultaneous ancillary orders - one religious, the other economic. Language still functions as the identity for the group, as in all previous variations, including those of Radcliffe-Brown (and Elkin), Hiatt, and Meggitt.

Stanner (1965) thus reaffirmed his support for Radcliffe-Brown insofar as spatial order was concerned, being ultimately delimited by the material conditions of a single given ecological zone. However, Stanner now introduced his own, possibly most important contribution to the KLRE model, by distinguishing between two classes of spatial order within such an ecological zone, which he referred to as 'estate' and 'range'. An 'estate' in this reformulation, was a spatial order defining the geographic distribution of religious sites and activities, while a 'range' was a spatial order defining the geographic distribution of secular sites and activities (Stanner 1965:2 - see APPENDIX 1 for extended quote).

The importance of this reformulation was that it applied not to any aspect of the group theoretic *object* 'society', but rather to the ancillary *order* appended that object. Whereas Radcliffe-Brown's 1930s reformulation had been to clarify distinct subgroup institutions as kinship, language, religion, and economy *within* the group theoretic object 'tribe', and to promote language as the institution that formed the identity for that object, Berndt's 1959 reformulation had been to merge two of these subgroups together and specify micro-level activities as their ordered manifestations, yielding 'clan' and 'band' as subgroups of the macro-level object that he termed 'bloc' (as described above). Hiatt, following Berndt, clarified that this implied an interdigitation between the two different subgroups of people, but did not affect the closure of the group theoretic object or its identification by language

(which he continued to call 'tribe'). Now, in 1965, Stanner refuted both Berndt's and Hiatt's arguments, that Radcliffe-Brown's original closure of the tribe object had been incorrect, and re-asserted the distinction between subgroup institutions of kinship, language, religion, and economy. However, Stanner also argued that Radcliffe-Brown had been wrong to append a single geographic order to these institutions. Instead, Stanner proposed that 'estate' and 'range' were different kinds of order that could be appended to the entire society (Stanner 1965:16 - see APPENDIX 1 for extended quote).

Stanner never responded to Hiatt's previously described second attack on Radcliffe-Brown in 1968. Ten years later Stanner (1979) instead provided a recount of an interview with friend and longtime interviewee Durmagam, a senior man from the Port Keats area of the northwestern Northern Territory. In this seminal paper Stanner emphasized the clarity and self-consistency of autochthonous theories of exchange and alliance between sets of people bijected into totemic descent groups, leaving the impression that he intended it to form a delayed implicit backhand to Hiatt's and Meggitt's 1960s arguments, that no such thing ever existed in reality.

Although Hiatt, Berndt, and Stanner appeared to have all moved on from the kinship debate by the 1970s, Meggitt made one last contribution in 1972. In this effort he reprises Berndt's and Hiatt's fusion of kinship with religious subgroups, and residential with economic subgroups, but in greater detail, and with greater creative license. Whereas the 1960s debate was premised on an acceptance of sociocentric kinship terminologies as thoroughly defined in line with the 1940s-era standard model (see Figure 39), allowing the focus to be directed instead towards the so-called 'actual significance' of the links *between* kinship and other subgroup institutions (Berndt 1959), Meggitt now returned specifically to the structure of sociocentric kinship terminologies themselves, using Western desert Walpiri terminologies as an example (Meggitt 1972).

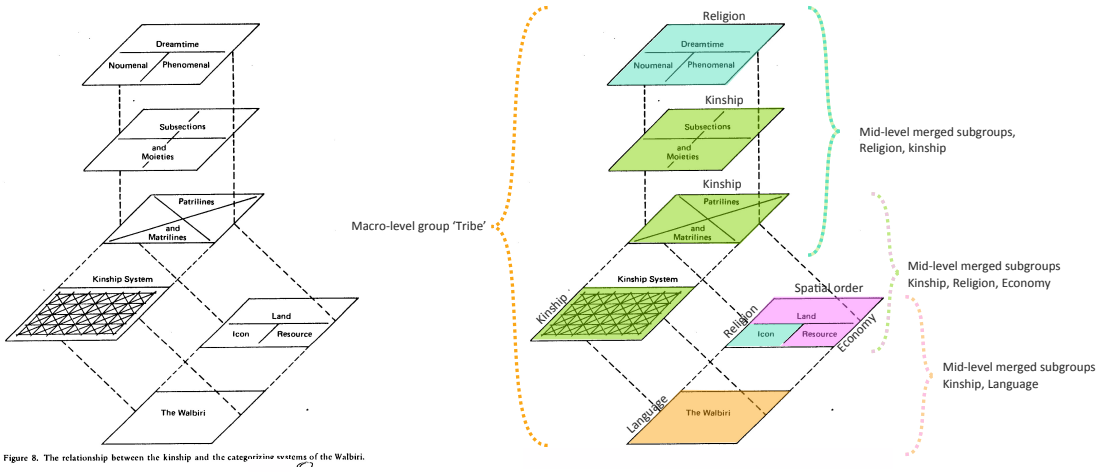


Figure 49: Meggitt's (1972) reprisal of Berndt's (1959) and Hiatt's (1962) attempted resolution of the KLRE group model with increasingly incongruent field data. Note the preeminent role attributed to sociocentric kinship terminologies 'subsections' and 'moieties'.

In this later exercise, illustrated at Figure 19, Meggitt proposes that subgroups of people within the supergroup 'tribe' cannot in fact be divided at all, neither according to the four independent institutions of kinship, language, religion, and economy, nor according to the merged institutions of kinship-religion and language-economy proposed by Berndt, Hiatt, and himself a decade earlier. Instead, Meggitt presents all four of these elements as comprising a single merged entity. Helpfully, Meggitt visualises this theoretical merging of KLRE subgroups as a vertically ordered 'stack', beginning with the already merged kinship-religion subgroup on the top, and language-economy subgroup on the bottom, and then blending them into what he calls a single "thoroughgoing monism" (Meggitt 1972:82 - see APPENDIX 1 for extended quote)

This new reformulation renders the earlier, segregated spatial orderings of Berndt's and Hiatt's models irrelevant, since there are now no longer any distinct subgroups to which distinct ranges of spatial order can be appended, neither 'hordes', 'clans', 'local groups', 'bands', or any other such entity. In this regard, Meggitt is tacitly returning to Radcliffe-Brown's earlier underlying unitary order, or ecological 'domain' as Stanner called it (Stanner 1965). Meggitt glosses this renewed emphasis on uniformity as "consistency" (Meggitt 1972:80-82 - see APPENDIX 1 for extended quote):

What Meggitt is ultimately arguing for here, is the total merger of all KLRE subgroups under the aegis of kinship, such that within the overall group theoretic object 'tribe' there is only one subgroup called 'kinship', which manifests in a range of activities that may themselves be classified as 'religious kinship', 'economic kinship ', 'linguistic kinship ', and so on. The single remaining spatial order is simply 'land' over which the entire group called 'tribe' is distributed without differentiation (see Figure 50). In an apparent effort to maintain the legacy of Berndt's and Hiatt's earlier distinction between religious and economic groups however (and in a reference to Kant and Durkheim unprecedented in Australian anthropology at the time), Meggitt distinguishes between 'noumenal' and 'phenomenal' activities (Meggitt 1972:71). Consistent with his own earlier representations, and also with Berndt's and Hiatt's previous representations before him, this new distinction does not demarcate subgroups of *people* within the supergroup 'tribe', but instead demarcates subgroups of *activities*. In other words, sets of activities (rather than people) are bijected into groups, construing any given person as capable of engaging in both 'noumenal' and 'phenomenal' activity (see Figure 50). Interestingly, this approximates the 'secular' and 'religious' distinctions made by Stanner (1965). The difference is that whereas Stanner had previously applied these distinctions to spatial order, Meggitt (1972) continued, like Berndt (1959) and Hiatt (1962;1968), to apply them to the distinct activities of people.

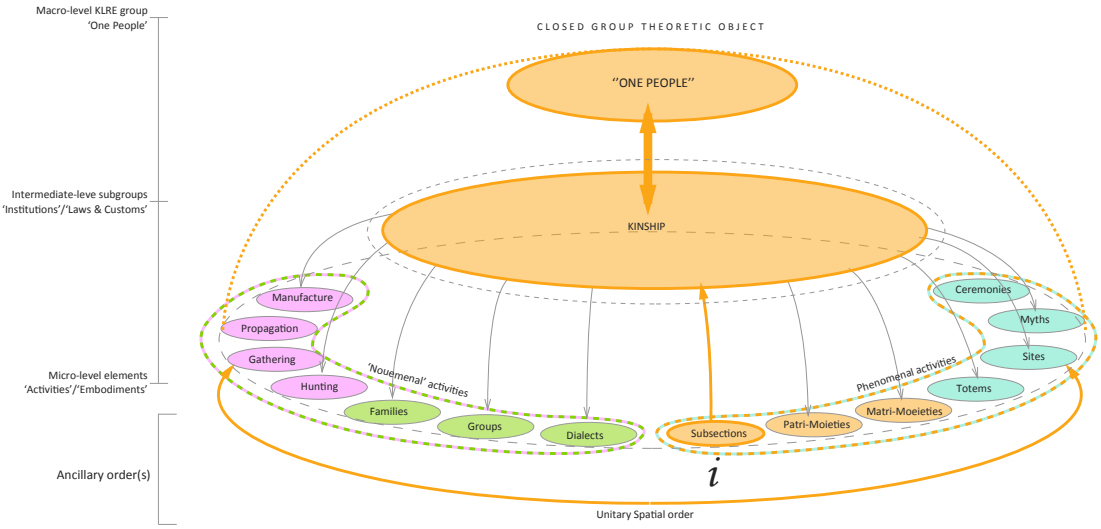


Figure 50: Meggitt's (1972) reconfiguration of the KLRE model. The macro-level group object 'tribe' (alternately referred to *s* 'one people') is constituted by a single intermediate-level subgroup 'kinship', which encompasses all individuals, and replaces the two subgroups, kinship-religion and language-economy, of Berndt's (1959) and Hiatt's (1962;1968) previous model. Meggitt instead partitions micro-level, elemental activities into two subgroups, 'noumenal' and 'phenomenal'. The identity element *i* is implied to be constituted by sociocentric kinship terminologies rather than language variation. Causality is appended to the model in the form of a single spatial order over which both noumenal and phenomenal activities are distributed.

The overall disagreement between Hiatt, Meggitt, and Berndt on the one hand, and Stanner on the other, was not about the fundamental viability of the KLRE model, as a closed group theoretic object comprised of subgroups of permutable intermediate-level institutions, and manifest by the micro-level elemental activities of theoretical group members (whose personal movements, economic, kinship, and linguistic activities were never actually documented in their publications). Rather, the debate was about the best strategy for rendering congruence between a fundamentally closed, un-ordered anthropological model of abstract group theoretic objects on the one hand, and on the other hand, Indigenous models of sociality and highly complex intuited patterns in field observations, both of which clearly failed to support the anthropological model. This lack of congruence between autochthonous models, observed behavior and anthropological models, became the focus of late pre-crisis anthropology's great champion, Lévi-Strass.

3.2.3 Lévi-Strauss: Normativity as an Ancillary Order

As discussed in Chapter 2, Lévi-Strauss had already, in his seminal 1949 work *Elementary Structures*, identified the key problem for anthropology as a discrepancy between autochthonous models, anthropological models, and observable sociality (Lévi-Strauss 1969[1949]). He sought to address this problem using a bundle of new theoretical constructs tied together under a rubric he called 'exchange and alliance'. *Elementary Structures* extolled a radical generalization of the metaphors underpinning social anthropological theory, to the extent that any and all human idea systems should be ultimately reducible to the encoding of reciprocity (exchange-based alliances) between ostensibly distinct sets of

individuals. The generality of this principle implied that it extended to whole of the KLRE model used to explain Australian sociality. Lévi-Strauss' primary innovation in respect of sociocentric kinship terminologies, was to group them under a principle he called 'dual organization', such that that all variations among such terminologies could be characterised as permutations of a closed set that tended towards a reciprocal equilibrium among self-identified 'groups'.

As shown in the previous chapter, Lévi-Strauss ordered these variations along a normative series, ranging between 'generalized exchange' and 'restricted exchange', giving rise to 'elementary' and complex' structures, with intermediate forms that he termed 'semi-complex'. This allowed him to offer an explanation for the struggle experienced by Australian anthropologists in resolving their models of sociocentric kinship terminologies with their instantiations in real-world population dynamics. Lévi-Strauss proposed that the reason lay in their failure to grasp that Indigenous Australians' *own ideas* about reciprocity formed the basis of *actual* reciprocity among them, irrespective of whether perceived rules corresponded with anthropologists' isolated, ad hoc observations of people's behaviour.

Lévi-Strauss argued that Australian terminologies did not necessarily prescribe a 'rule' for cross-cousin marriage, as Howitt had originally claimed in 1880 (Howitt 1880 - see Section 3.2.1), but rather encoded a more general principle for building matrimonial alliances among sets of nominal (if not actual) collateral relatives. In this regard, Lévi-Strauss drew heavily on texts by Radcliffe-Brown and Elkin to argue that variations in numbers of sociocentric terms among geographically distinct Indigenous Australian populations, did not express different kinds of marriage rules, in terms of greater or lesser normativity (as described for the 4-part standard model presented in Section 3.2.1 above), but rather greater or lesser theoretical elaborations on a single common principle of dual organization. In the terms laid out in the previous chapter, Lévi-Strauss considered all Australian terminologies as elementary insofar as they were maximally prescriptive, and thus expressive of a form of restricted exchange. In Lévi-Strauss' view, there was no variation within this classificatory framework, with a corresponding implication for consistency in real world Indigenous Australian sociality. In 1949, this was a completely novel concept for most Australian anthropologists for two reasons: Firstly, it attributed to Indigenous Australians an agency that almost no social anthropologists had been willing to acknowledge[36]. Secondly, it posited a single unified model for the entire Australian population, based on variable symbolic *expressions* of this agency using variations in the kinship terminology. Lévi-Strauss' most confronting proposition was that all Australian kinship terminologies were marginally innovative variations on a single principle of reciprocal exchange and alliance across the continent's entire population.

[36] Although see Chapter 4 for discussion of William Ridley's and R.H. Mathew's late 19th century pre-emption of the idea.

Although no Australian anthropologists appear to have read *Elementary Structures* until its first English translation in 1969 (see Schefller 1970), Lévi-Strauss had articulated some of its key elements in intervening English-language articles, and at an infamous conference on hunter-gatherer cultures in 1966 titled '*Man the Hunter'* (Lee & Devore 1968). Members of the Sydney School, Hiatt and Meggitt, also papers presented at this conference, where they attacked Lévi-Strauss' thesis. Hiatt in particular made a scathing criticism of Lévi-Strauss' model of kinship terminologies as autochthonous theories of social reciprocity, claiming that no such thing existed in Australia (Hiatt 1968:65; 174 - see APPENDIX 1 for extended quote).

In support of his attack on the idea that Indigenous Australians possessed sufficient agency to develop and innovate variations around a single common principle of reciprocity, Hiatt described an elaborate and somewhat condescending hypothetical scenario in which "an aboriginal law-maker named Plato proclaimed [a kinship] constitution" (Hiatt 1968:65). Augmenting this fiction with references to his own ad hoc observations of marriage patterns among members of the Gidjingali language community of Arnhem Land, and to his 1965 attack on Radcliffe-Brown (Hiatt 1965 - see 3.2.2 above), Hiatt concluded that reciprocity was merely an incidental implication of the kinship terminology, which did not in fact correspond with his own genealogical data. Hiatt claimed that this data (which he did not present), showed that *competition* over women between elder and junior men was the predominant theme of Australian sociality, rather than an equilibrium-oriented matrimonial exchanges (Hiatt 1968:174).

Realising that Hiatt's intransigence rested on his inability to recognise the capacity of Indigenous Australians to articulate self-consistent theories of their own sociality, Lévi-Strauss emphasised in his reply the relationship between autochthonous models, anthropological models, and observable instantiation. In doing so, he criticised the argument that theoretic closure of Australian kinship terminologies could be construed as evidence of those theories' irrelevance to 'what people actually do', as made by Hiatt and Meggitt[37] (Lévi-Strauss 1968:211 - see APPENDIX 1 for extended quote). In reality, Lévi-Strauss had clearly and exhaustively articulated what he meant by 'preferred degrees' of normativity in 1949 (Lévi-Strauss 1969[1949] - see Chapter 2), proposing that kinship terminologies encode much more than narrow, 'morally proper' rules for Australian people, but rather presented a reservoir of optionally appropriate courses of social action, from which each individual could draw according to personal repertoire and prevailing context. Finally, to remove any ambiguity about his own position, Hiatt rejected the entire possibility of an extended, preference-derived kinship network among Indigenous Australians (Hiatt 1968:211-212 - see APPENDIX 1 for extended quote).

Following the English publication of Lévi-Strauss' by then 20 year-old *Elementary Structures* in 1969, the year after the contents of the *Man the Hunter* conference were published (Lee & Devore 1968),

[37] And Stanner before them - see Section 3.2.3.

the argument between Berndt, Hiatt, and Meggit on the one hand, and Stanner on the other (see Section 3.2.2), evaporated over the course of the 1970s. Hiatt lost interest and switched focus to "the immaterial realms of mythology and symbolism" (Hamilton 1997:5), while Stanner committed himself in the opposite direction, towards the highly material realm of policy reform in Indigenous Affairs (Dexter 2008). Berndt too, like Hiatt, drifted into a focus on art and religion. None of these major contributors to the study of Indigenous Australian population dynamics give any indication of having been aware of Lévi-Strauss' seminal analysis before this time[38]. An additional likely cause for their resistance to these new ideas is noted by Keen (1988), who observes that the rise of legal definitions used in the Northern Territory Aboriginal Land Rights Act (1972) may have had the effect of muting anthropological debate, since these terms explicitly require corporate membership of land claim parties to be both completely normative, and defined by descent. Both of these notions predate Australian anthropologists' exposure to Lévi-Strauss' innovations, and the fervour with which anthropologists such as Hiatt and Meggitt resisted them may go some way to explaining how the Northern Territory Land Rights Act, and its legislative heir, the federal Native Title Act (1993), came to depend so heavily on what were already out-dated theoretical assumptions on the part of Australian anthropologists.

In any case, by the 1980s the standard model of Australian kinship terminologies had become fixed in the form described in Section 3.2.1 above (see Figure 39). What Radcliffe-Brown and Elkin had called 'Aranda-type systems' were now referred to as 'subsection systems'; what they had called 'Kariera-type systems' (and what Howitt and others before them has called 'Kamilaroi-type systems'), were now called 'section systems'; what they had previously called 'Dieri-type systems' were now called 'moiety-' and increasingly 'matrimonial moiety systems', and finally; what Elkin had called 'Aluridja systems', were now called 'generational moiety systems'. All were, and still are, deemed to be uniformly prescriptive. Today, Australian anthropological texts frequently apply a geographic order to this classificatory framework, such that sets of terms are described as being distributed over 'regions' (Sutton 1998), while the increasing contribution from linguists, noted by Keen (1988), has led to temporal order becoming once again fashionable, whereby terminologies with fewer terms are held to comprise 'less evolved' versions of terminologies with more terms, a position identical to that of Morgan' (1871). In all cases, the distinction between terminological 'systems' are still held to reflect distinctions between the way in which Indigenous Australians socialize in discrete regions of the continent. This assumption has the effect of reinforcing a social anthropological model of Indigenous Australians as the descendants of a historical population that has diffused across the continent, akin

[38] Berndt makes his first reference to Lévi-Strauss in 1967 (Berndt 1967), Hiatt in 1967 (Hiatt 1967), Meggitt in 1966 (Meggitt 1966; Fox 1967), and Stanner in a little earlier in 1963 (Stanner 1963). Except for Hiatt, whose reference to Lévi-Strauss is made in the course of defending Meggitt from an unflattering book review by Fox, these mentions are all perfunctory, limited to one or two sentences.

to British colonization, rather than as a single ancient community of stable, regionalised populations that have endured ice ages, sea-level rises, and ecological fluctuations of an almost incomprehensible scale.

Lévi-Strauss' reformulation of kinship theory was disruptive to the entire debate in Australia because it negated the prescriptive or 'normative' basis of what Australian anthropologists had been referring to as the 'rules' of sociality. Lévi-Strauss presented a coherent model suggesting that what Leaf and Read (Read 2008; Leaf & Read 2012) more recently call 'idea systems', and what Halliday (1993; 2001) and Bernstein (1996; 1999) call 'cultural codes' (see Chapter 1), are not uniformly normative frameworks akin to operating manuals, but instead meaningful realisations of real-world networks of interdependency between individuals and the communities in which those individuals perceive themselves as belonging. Lévi-Strauss' key point in *Elementary Structures* was that kinship terminologies are distillations of these ideas, where they form the fundamental realisation of how such networks should be sustained and reproduced over time and across space, through the physical reproduction of real and specific individuals and the richly meaningful relationships linking them together. As described in Chapter 2, Lévi-Strauss formulated these terminologies not as uniformly prescriptive, as they had been previously construed in the language of Morgan (1871) and others, but as ranging dynamically between two poles of obligation - *pre*scription on the one hand, and *pro*scription in the other (Lévi-Strauss 1969[1949]:119 - see APPENDIX 1 for extended quote).

Lévi-Strauss' consistent point is that *pre*scription realised by a sociocentric terminology on the one hand, and *pro*scription realised by an egocentric terminology on the other hand, together form an inherent ordering mechanism that generates a causal flow of relationship formation in any given kinship culture. The so-called 'rules' for relationship formation and the sociocentric terms that encode them in Australian cultures, are neither one and the same (as implied by a hypothetical total normativity argued by Morgan and Howitt), nor separate (as implied by hypothetical mismatch between facts and models, argued by Hiatt and Meggitt). Rather, in Lévi-Strauss' terms, they are '*co-extensive*' (Lévi-Strauss 1969[1949]:199 - see APPENDIX 1 for extended quote):

The reason that leading Australian anthropologists of the 1960s found this model so destabilising, was that it revealed their classificatory framework of four geographically and historically distinct kin term 'systems', to more likely comprise a single, unified system, realised in a variety of meaningful ways from one regional community to another. Lévi-Strauss' model of exchange and alliance strongly indicated that they were in fact all variations on a single common theme of reciprocal exchanges of spouses among *self-defined* communities of descendants, irrespective of wether anthropologists themselves could make sense of those communities. In this model, the number of terms in any given terminology was irrelevant to the unification, through alliances, of the continent's entire Indigenous population.

Despite his theoretical breakthrough, Lévi-Strauss was still hampered by social anthropology's pre-paradigmatic language. Like his predecessors, he provided in *Elementary Structures* a model of sociality whose high level of abstraction rendered it notionally applicable to a wide range of observable instantiations. Also like his predecessors, Lévi-Strauss provided an ancillary ordering mechanism for imbuing this model with explanatory power in the form of an 'if-X-then-Y' cause-and-effect sequence. The key difference between this ancillary order and the ancillary order of previous models was that it did not claim to reflect any historical 'evolution' of human sociality, nor corollary 'diffusion' of successively newer forms of sociality across geographic space and historical time (Lévi-Strauss 1969[1949]:136 - see APPENDIX 1 for extended quote).

In White's terms (White 2007; White & Denham 2008), Lévi-Strauss thus 'weakened the axioms' of the standard model of kinship, to the point that it could begin to offer some isomorphism with both the patterns clearly apparent in the kinship terminologies explained to anthropologists by Indigenous Australians themselves, *and* in the patterns of Indigenous Australian's observable relationships with one other. Lévi-Strauss' model of sociality was one based not on the uniform convergence of discrete institutions with discrete societies, yielding playbooks of 'rules' to which members of social 'groups' diligently conformed in the demonstration of their own distinction. Instead, his model provided a universal framework for interpreting the diverse range of patterns evident in the thinking of the world's people, and in the instantiation of their own ideas in their own myriad instances of social interaction, giving rise to complex webs of interaction unified by a common general structure. With specific regard to Australians, Lévi-Strauss' ordering mechanism allowed him to consistently, coherently, and formally describe (with the help of Wéil), the mechanism by which he believed these patterns to be encoded (Lévi-Strauss 1969[1949]:xxxiv - see APPENDIX 1 for extended quote).

What Lévi-Strauss' model did *not* do was provide an explicit explanation for the extension of networks beyond closed circuits of exchange and alliance, or for their extension across real historical time and real geographic space. Lévi-Strauss' model, although revolutionary, was still fundamentally group theoretic. In fact, in the light of Weil's mathematical formalisations, included as an appendix to *Elementary Structures*, the 'exchange and alliance' model was presented as *explicitly* group theoretic, rather than merely tacitly so, as in the models of Australian anthropologists and their 19th century progenitors, Howitt and Morgan. Because Lévi-Strauss was preoccupied with the *meaning* of kinship terminologies, and the abstract metaphors of opposition and reciprocity distilled within them, he deliberately marginalised the relevance of what he called 'mechanical' and 'statistical' data in his analyses (Lévi-Strauss 1969[1949]).

Lévi-Strauss' key point is that kinship terminologies exhibit their own inherent order, expressed in the tendency towards one or another kind of network structure - either directly predictable and absolutely normative 'mechanical', or complex and ultimately unpredictable 'statistical' - which emerges as the result of culture bearers' instantiation of terminologies in decision-making (Lévi-

Strauss 1969[1949]:xxxiv). While revelatory, this point also indicates the greatest weakness Lévi-Strauss' theorising: his own tendency towards reductionism. This tendency found its most explicit expression in his ideas about 'structures of the mind', which he imagined as conforming to the same kind of 'dual organization' that he detected in idea systems "as basic and immediate data of mental and social reality" (Lévi-Strauss 1969[1949]:136). That he was able to reach such a deductive conclusion, in spite of his own conscious weakening of anthropology's rule-based axioms, suggests that Lévi-Strauss was handicapped by the same lack of a properly scientific paradigm that hampered both his predecessors and contemporaries. This is reflected especially in his use of the term 'data', as somehow predicated on "mental and social reality" rather than on an anthropologist's capacity or inclination to make numerous and consistent observations, and to measure those observations in a manner that would allow for the construction of a testable model.

In some ways, this reductionist tendency is excusable, given the material conditions of the time. Houseman and White (1998) note that Lévi-Strauss was working in an era when the technological capacity of social scientists generally, and social anthropologists in particular, was still highly constrained. Apart from the kind of formal mathematical language supplied by Weil (1969[1949]), there was no obvious way, in the absence of accessible computing, to test the postulated distinction between mechanical and statistical structures in marriage alliances, through the collection and analysis of large volumes of relational information. Indeed, the distinction between mechanical and statistical models, and the marginalisation of both in favour of an emphasis on "mental reality" only makes sense at a relatively small scale, where there is no possibility of tracing emergent patterns *from* micro-level *to* macro-level scales.

This draws us back to the account given by Kuhn (2012[1962]), in Chapter 1, of the role of technological innovation in facilitating increasingly sophisticated measurements of ever larger and more complex bodies of data, thereby giving rise to the revolutions from which paradigmatic science emerges. Although Lévi-Strauss weakened the tacit axioms of anthropology's group-theoretic language, in line with White's proposals (White 1974; 2001 - see above), and as observed by Jorion (2000), he possessed neither the technological capacity nor a formal understanding of scientific inquiry that might have allowed him to pursue his intuition towards his ultimate goal: The establishment of a paradigm for social anthropology. This same context, together with the peculiar historical conditions of Australian social anthropology as described in detail above, also conspired to mute Lévi-Strauss' influence on the Australian anthropology before it was overtaken by the onset of the crisis era, discussed in the previous chapter.

3.3 Conclusion

This chapter presented key stages in the development of Australian anthropological modelling of Indigenous Australian sociality. It was shown that for the past 150 years this has conformed broadly to what might be called the KLRE model of sociality, whose inception was documented in Chapter 2. This is a weakly formalised, tacitly group theoretic model, which deduces that Indigenous Australian population dynamics and individual behaviour are predicated on four subgroups of ideas and activities, alternately referred to 'institutions' or 'laws and customs', including kinship, language, religion, and economy. The model deduces further that each of these subgroups is manifest in sets of individual instantiations of the corresponding institution, generally referred to as 'activities', 'inscriptions', 'embodiments', 'performances', and so on. As a means of construing causal explanation in what would otherwise be a static, *a priori* classificatory schema, the KLRE model incorporates optional ancillary temporal and spatial orders. These tend to take the form of one or another kind of historical and geographic ordering of activities, with emphasis on one in particular, most commonly low-level language varieties (dialects), but optionally one or another specific manifestation of economic, religious, or kinship behaviour. This latter entity tends to form the axiomatic identity element for the enclosing group into which all activities, elements, beliefs, laws, and customs are closed - typically a tribe, society, or nation. Historically, this deductive group theoretic model has led to anthropologists distinguishing 'groups of people' on one or another such basis, such as speakers of distinct language varieties, users of a particular productive technology, or participants in a particular kind of ritual. The current chapter has focused on the lack of consilience between this method of classifying sets of people, and the method for classifying both sets of language varieties (from the perspective of comparative linguistics), and sets of kinship terminologies (from the perspective of exchange and alliance theory).

This chapter has shown that since the late 19th century each major stage in the development of the KLRE model has been marked by a reorganization of the spatial and temporal ordering of elements and activities within it. This serial reorganization has generated a widespread impression within the discipline of social anthropology, of continuous improvement and refinement of the model, and a sense of incremental advances in anthropological construals of Indigenous Australian sociality generally. In particular, it has been shown in the current chapter that language and kinship have come to comprise two 'institutions' or 'laws and customs' upon which the group theoretic model depends for logical coherence, and which allows it to imply causality without empirical substantiation. The purpose of the current chapter has been to highlight the lack of mutually proportional formalism, empiricism, and congruence in this approach, which remains unresolved to the present day. Section 3.1 drew attention to the discrepancy between the open and flexible nature of the set theoretic logic used by comparative linguistics in the modelling of Indigenous Australian language, compared with the rigid and closed group theoretic logic used by social anthropology for the same purpose, along with the resultant lack of consilience between the two disciplines. Section 3.2 highlighted the

stepwise transitions among key historical authors in the field, from Morgan, to Howitt, Radcliffe-Brown, Elkin, Berndt, Hiatt, Meggitt, to Stanner, in their serial reorganization of ancillary ordering mechanisms appended to this ad hoc, anecdotally supported, and incongruent model. Section 3.2 concluded with an account of the resistance on the part of some leading Australian anthropologists to Lévi-Strauss' critical innovations in the field of kinship during the late pre-crisis era, and the consequent ossification of the KLRE model.

The next chapter presents an overview of one major international development in the field since this time, tracing a path directly from the insights of Lévi-Strauss, to a rapidly maturing methodology that holds out the possibility for resolving some of these challenges - kinship network analysis.

Chapter 4

Network Models of Australian Sociality:

Cohesion in the Central Murray Darling Basin Population

4.0 Introduction

The previous chapter described the evolution of group theoretic modelling in Australian social anthropology, with specific regard to the construal of language and kinship as subgroup institutions within discrete Indigenous 'tribes'. The chapter concluded with an account of the stasis that descended upon Australian anthropology at the end of the pre-crisis era in the late 1960s, and the cementing of the group theoretic KLRE model as the standard account of Indigenous Australian sociality. Kinship and language were shown to play important roles in this model, with language acting as a macro-level identity element for group theoretic 'tribes', 'societies' or 'blocs', and kinship terminologies acting as micro-level 'rules' for individual interactions within these abstract entities. In this way, the KLRE model has come to be used in Australian anthropology as a means for predicting macro-level social structures in terms of language, and micro-level social interaction in terms of linguistically determined kinship terminologies.

The current chapter highlights an alternate, parallel history in the development of Australian anthropological theory, based not on group theoretic metaphors of depersonalised attributes, but on *network* theoretic models of real, observed social interaction between real, known individuals. Here, kinship and language are not construed as abstract institutions closed into theoretical groups according to their ideal properties, but as the expressions of real and specific people, maintaining real and specific relationships with one another, as they move through time and space. Kinship terminologies and the particular historical and geographic contexts in which they are used, take on a fundamental role in this alternate anthropology of Australia, not as validations of *a priori* theoretical entities suspended in a timeless void, but as evidence of extensive networks of individuals realising, cultivating, and managing their social cohesion in a variety of meaningful ways across time and space.

The current chapter traces the development of network theoretic principles in the work of early Australian anthropologists William Ridley and R.H. Mathews, both of whom conducted extensive fieldwork with members of the Central Murray Darling Basin (CMDB) population in South Eastern Australia between the 1850s and 1910s, and links this to contemporary network theoretic research with the same population in the 2010s. As with group theoretic models, network models are generally rendered tacitly in the texts of these early anthropologists, reflecting their inability to sufficiently formalise their observations and analyses. However the emergence of this alternative framework had major repercussions for the way in which sociality and causality were construed in the relevant texts, particularly with regard to the axiom of closure, and the recognition of complex diversity in people's ideas and interactions.

Unlike group theoretic models, network models are inherently open, and are therefore amenable to exploratory measurements of complexity and extensibility, both in terms of time and space, and in terms of normativity. Furthermore, because there is no inherent limit on the scope of network models, the elements and relations within them can be measured by multiple criteria as new data

becomes available. A single language for example, might emerge as being associated with multiple kinship terminologies, while a single kinship terminology might emerge as being associated with multiple languages, as new links are discovered. This capacity for emergence is made possible by social network theory, because the associative relations between terminologies and languages are not derived from the classification of their abstract properties (such as morphology, lexicon, and syntax), but from the observable interactions between the people who use them to realise meaningful relationships. This is the core distinction between the research conducted by Ridley and Mathews on the one hand (see below), and Howitt and his heirs on the other (see previous chapter). While the latter focus their energies on classifying and reclassifying abstract group theoretic objects and elements, such as 'clans', 'hordes', 'moieties', 'sections' and 'subsections', Ridley and Mathews present data on the actual people they interviewed (including their names, and the names of their community members), along those people's own explanations of language and kinship, and the material situations in which they used them. The result is a more complex, yet more self-consistent account of Australian kinship, with negative implications for the viability of the KLRE model. Although Ridley and Mathews were limited by the technological and ideological constraints of their time, their relative intellectual independence allowed them to explore data for patterns free of group theoretic preconceptions.

Unencumbered by the theoretical alliance that bound Howitt to Morgan and Tylor's models, Ridley and Mathews were able to observe that the people of the Central Murray Darling Basin could *not* be closed off into discrete groups identified by language, demonstrated exclusively by discrete sets of terms and rules for prescribing their marriage and descent. Instead, these communities seemed to be intrinsically interconnected, sharing a range of idea systems that appeared to vary independently of one another, from one part of the population to another. Possibly also as a result of their intellectual independence, Ridley and Mathews were able to record kinship terminologies (along with mythology, music, ritual, and language) in much greater detail than any of their contemporaries, and more so than any of the small number of anthropologists that worked with the CMDB population in the century that followed. Although almost completely neglected in later Australian kinship research[39], this detail reveals links between CMDB terminologies and people, far more complex than those admitted by Howitt, Radcliffe-Brown, Elkin, or Berndt in the 50 years of research following Ridley's & Mathews' deaths at the turn of the 19th century. This independence allowed Ridley and Mathews to begin a tentative, albeit tacit and weakly formalised, relational survey of CMDB cultures that has produced datasets readily testable using contemporary network theoretic methods.

The relationships in a network model are not between idea systems themselves, but between the people who utilise those idea systems in the course of making decision in real-world social contexts.

[39] The only text to cite Ridley's & Mathew's detailed work on CMDB kinship terminologies in any detail is Testart (1980).

This is reflected by a much heavier emphasis on personal discretion in network theoretic texts than in group theoretic texts. Because of these characteristics, network models present a means for building congruent as well as coherent explanations of how multiple people interact using a variety of distinct idea systems simultaneously, making decisions at a personal level, according to choices apparent to them, rather than 'adhering', 'subscribing', or 'obeying' rules. Network models can be used to describe and explain both idea systems and social behaviour simultaneously, as interdependent factors in a single system. They thus represent a much more powerful mechanism for construing complex sociality and causality, without compromising formal representation. Kinship network models apply these methodological advantages to the modelling of populations as complex self-generating systems.

The current chapter is organised into three parts: Section 4.1 introduces the core aspects of network theory relevant to modelling sociality and causality, together with the specific technical grammar of kinship network analysis. Section 4.2 reviews the network theoretic models of CMDB sociality developed by Mathews and Ridley in the late 19th century. Section 4.3 presents contemporary large-scale network models of the CMDB population and compares patterns in those models with the patterns inferred by Ridley's and Mathew's elicitations 150 years earlier. The current chapter makes intensive use of two software packages: *Pajek* (mrvar.fdv.uni-lj.si/pajek), developed by Andrej Mrvar, and *Puck*[40]

[40] Puck, written in Java, was developed beginning in 2007 by Klaus Hamberger within the framework of the "Kinship and Computing in Anthropology and History – Traitement Informatique des Phe´nome`nes de Parente´ (TIPP)" project directed by Michael Houseman and Cyril Grange (French National Research Agency grant ANR-NT05-4_43503). Its version 2.0, based on a new object model and integrating simulation modules, was developed by Klaus Hamberger, Christian Momon, Edoardo Savoia and Telmo Menezes within the framework of the "Kinship Simulations – Simulations de la Parente´ (SimPa)" project directed by Franc_ois He´ran (French National Research Agency grant ANR-09-SYSC-013-02). The software itself, as well as a user manual, can be downloaded for free from www.kintip.net. The source code is available at http://sourceforge.net/projects/tip-puck.

4.1 Network Models of Sociality as a Function of Kinship

Compared with group theoretic models, network models represent an alternative, more congruent construal of human sociality, capable of representing the complexity of both observable social interaction, and the idea systems maintained by people engaged in that interaction. In order to recognize how this capability functions, it is necessary to review the similarities and differences between these two kinds of formalization. Like group theory, network theory has a formal, mathematical foundation. Both group theory and network theory are relational, meaning that they derive descriptive power by construing links between objects. The process of bijecting or 'remapping' certain elements within a set into a group, as described in Section 2.1.1, is relational. So too are the operations between elements within a group, such as the operation between group elements and an identity element (see also Chapter 3). However, whereas any given group exhibits only one unitary and homogenising operational link, any given network may exhibit multiple kinds of such links. This is more than a philosophical question of anthropological metaphors doing greater or lesser 'violence' to autochthonous metaphors (see Chapter 2). Rather, it goes to an overdue acknowledgement of the inherent capacity of formal, mathematics-based modelling to accommodate the rich and detailed complexity of human sociality.

Like group theory, network theory commences from a set theoretic basis by applying a relational classification to only *some* of the elements within an originating set. However, rather than applying a single bijective function on the originating set in order to achieve this, network theory distinguishes between two different *kinds* of element in the originating set, as vertices and ties respectively, with the further qualification that they be alternately linked, i.e. vertex-tie-vertex. In other words, whereas group theory renders a single, uniform relation between all elements in a subset, and defines them as members of a closed group as a result, network theory renders two different kinds of elements as linked together in unique combinations of *chains*. Any element in the originating set that meets these criteria is part of the network. One major effect of this configuration is that networks are inherently *open*, with the capacity to both grow and contract according to the extent of available data. Another major effect of this configuration is that network models may be used to *explore* for new data, since there is no predefined boundary imposed on what may be considered 'inside' or *part* of the network, and what is considered 'outside' or *not part* of it. The only requirement is that elements be demonstrably linked. Like following a path in the darkness, there is no axiom in network theory that states where or why a researcher must start or stop exploring the extent of a network, which means that each path, together with all its loops and branches, may be followed as far as the data extends. Yet another effect is that networks contain an *inherent* order (unlike the *ancillary* order of groups - see Section 2.1.3), because of the requisite alternating sequence of vertex-tie-vertex from which chains are composed. This means that there is no need to *impose* ancillary order after the fact, in an attempt to imbue the model with congruent causality that explains human action. Whatever spatial, temporal, and normative attributes are *exhibited* by individual pairs of vertices, are also

collectively expressed in the overall shape of the network, and in the shapes of regions within it. In other words, as elaborated below, the *causes* of the network's existence are embedded in its structure.

In the terms provided by Halliday & Mattiessen (2006), network theory provides a much more *congruent* model of human sociality than group theory. It does so firstly by realizing social behaviour as a process of *interaction*, rendered as relations between vertices (representing individuals linked by relationships), and secondly by realizing it as an *unfolding* process, in which networks grow and contract as relationships form and dissolve. Thirdly, network models incorporate order as an inherent property of that growth and contraction. Whereas group theoretic models must *impose* order on groups, subgroups, and elements, because of the uniformity of the single relation it imposes on these entities, order in network models is realised by the sequential formation of relations between vertices. This is because, as each relation forms, the network a) *moves* from one state to another, thus exhibiting a temporal order; b) changes *shape* (thus exhibiting a spatial order, and; c) reflects the influence of normative conservation in the reproduction of network structure. As observations become more extensive, patterns in the network become clearer. Normativity, rather than comprising an independent order, is an emergent property, expressed by the degree to which a network changes shape over space and time. If a network is shown to exhibit a particular shape over a given period, the degree of normativity operating upon that network will result in a corresponding degree of conservation over a subsequent period of equivalent duration. With network theoretic models, there is thus no need to impose a normative ranking according to the degree of options available in an idea system, such as kinship, language, religion, or economy. Rather, normativity can be measured by the degree of variability exhibited in a network's change over time.

The current section describes how the formal language of network analysis encodes sociality and causality that it is both dependent on empirical data, and congruent with autochthonous theories of sociality and causality.

4.1.1 Sociality and Causality in Network Models

Chapter 1 outlined two distinctions between natural and social science, one intrinsic and the other extrinsic. The first distinction is the inescapable role of autochthonous idea systems in human social culture, compared with its total absence from natural phenomena. The second distinction is the weak grammar and horizontal knowledge structure of social science, compared with the vertical knowledge structure and strong grammar of natural science. To briefly recap the first, intrinsic distinction: Whereas natural science detects patterns in observable natural phenomena and builds theoretical models explaining the causes of those patterns, social science detects patterns in both observable social phenomena (interactions between people) *and* the idea systems articulated by the people engaged in those phenomena. Following White (2011), and Leaf & Read (2012), social *culture* is the term for a system of feedbacks between observable social interaction, and autochthonous ideas

about that interaction. According to this framing, social anthropological models of culture are models of this feedback. To briefly recap the second, extrinsic distinction, introduced by Bernstein (1999) and given technical linguistic definition by Halliday and Mattiessen (2006), social science and the humanities more broadly are constrained by the grammar habitually used to realise meaning *about* social interaction. Terms are contested and used inconsistently, definitions are ambiguous, authors' identities tend to be equally if not more important than the texts themselves, and resultant models are neither self-consistent, nor integrated across or between disciplines. Consequently, the particular perspectival theme (in Bernstein's terms), which a given social anthropologist decides to use in describing sociality, automatically encodes a particular type of *a priori* causality (in Halliday and Mattiessen's terms) irrespective of whether or not it is explicitly stated, or even fully comprehended by the anthropologist. The result is often a tacit model of causality. Where this occurs, the causal patterns construed by the model are *deduced* and supported by selective, ad hoc reporting, rather than *induced* by systematic large-scale data surveys, as in natural science (Leaf 2007; Read 2008). The current section outlines the ways in which network theory provides a grammar (in Bernstein's terms 1996; 1999) that at once realises the *in*trinsic compound substance of social science data - as comprised of both *observations of* relationships and *ideas about* those relationships - and simultaneously resolves the *ex*trinsic distinction generated by the weak grammar and horizontal structure of social science language.

As shown in Chapter 2, group theory, whether explicit or tacit, formal or informal, radically constrains the capacity of social anthropologists to identify feedback between autochthonous ideas and observable behaviour. There are two reasons for this. The first is that group theory is maximally metaphorical and minimally congruent with social behaviour, linking people together not by interpersonal social relations, but by the attributes of identity elements, which are supposed by theoreticians to define group membership. The second reason for this constraint is that the links imposed by on people by group theory are homogenizing, one-to-one links, rather than diverse, one-to-many links, meaning that group membership 'closes off' discrete sets of people from one another. Thus, even a tacit, weakly articulated group theoretic statement, such as 'Person 1 is a member of Group X', has powerfully deterministic implications for how that person's sociality is understood, and what causes of that sociality can be realized in any subsequent explanation. Network theory, by contrast, imposes no such limitations. The only condition imposed by network theory is that the data be formally and consistently defined in terms of vertices, pairs of vertices, and types of pairs (as elaborated below). Social data modelled in this way is not only maximally congruent with processes of interaction between real and specific people, but also facilitates the detection and mapping of feedbacks between people's interactions, and their *own ideas about* those interactions. This is not to say that autochthonous idea systems are necessarily also network theoretic themselves, but that when modelled as networks of links between decisions made in the real world, chosen from among a range of apparent options (such as the range people from which one can or should choose to form

certain kinds of relationship), network models reveal patterns of causality that would otherwise be masked by assumptions about group consistency. As discussed at length in the previous chapter, attempts to render these group theoretic limitations congruent, by imposing ancillary spatial, temporal, and normative order upon group models, still results in a false, *a priori* uniformity, which undermines anthropologists' capacity to detect otherwise unknown patterns of sociality and causality. Chapter 2 showed that Lévi-Strauss' (1969[1949]) ancillary ordering of kinship complexity is a case in point, where degrees of prescriptive 'complexity' are located in decontextualized kinship terminologies, rather than in the instantiation of people's actual kinship behaviour. Chapter 2 also showed that the imposition of temporal order on group models by anthropology's 19th century founders, particularly Morgan (1871) and Tylor (1871), serves the same purpose.

Like group theory, network theory has its basis in the mathematical principles underpinning set theory, most importantly the idea that sets of elements can be partitioned into successively smaller or 'nested' subsets of elements, based on variations between one or more attributes. Beyond this common principle, group and network theory diverge under mutually distinct axioms, which strongly affect the ways in which they construe sociality and causality. Where only a limited *range* of axioms are applied in the course of modelling social interaction, only specific *types* of group and network have viable explanatory power. As discussed in the previous chapter, social *groups* tend to be construed specifically as permutation groups, primarily on the basis that sets of personal attributes are 'closed' (an axiom that states a person exhibiting a certain attribute can only ever be inside of a group where that attribute is an identity element, and can never be outside of that group), as well as the related axioms of commutativity and associativity (which state that particular sequences of interaction are irrelevant to group membership). Social *networks*, by contrast, tend to comprise specifically *directed, signed graphs* - 'directed', on the basis that relationship-formation tends to be choice-driven, and as such can be extended by one person *towards* another person (whether bilaterally or unilaterally); and 'signed' on the basis that both relationships and people are typically classified by people themselves, when they engage in relationship formation, such as when people who are 'friends' or 'enemies' are mutually classified by relationships of 'amity' or 'animosity' respectively. In other words, both relationships and the people linked together by those relationships, form a self-organising system of classification. These basic axioms yield complex implications for the kinds of patterning that network models can detect, and for the kinds of explanations that they can describe as causes for that patterning.

Social network models bear a number of other formal characteristics that distinguish them from group models, of which two are most important here. Firstly, social network models are based on a fusion of graph and set theoretic principles that capture two primary classes of data simultaneously, rather than only one, as is the case in group models: Whereas group theoretic models measure only individuals and their attributes, network models measure individuals and their attributes, as well as *relations between* individuals and the attributes of those relations. Secondly, as mentioned above,

network models are inherently extensible, whereas group models are inherently closed. Extensibility is manifest in the capacity of networks to exhibit topological properties, where deliberate, exploratory variation and inclusion or exclusion of different combinations of relationship type, reveals interdependent dimensions of network structure, which in turn cause the network to change shape as new relationships form and old ones dissolve. By contrast, group models are closed under the axiomatic definition inherent in the principle of group membership. A person cannot be *both* inside *and* outside a group, but only *either* inside it *or* outside (an axiom that cannot be weakened by the 'intersection' for multiple groups, as discussed previously in Chapter 2). In other words, group theory states that a person cannot be a 'little bit' in a group - they are either in it or they are not. Nor does group theory permit their membership to vary over time, since group closure is permanently fixed by definition. This means that, unlike networks, groups can never change their shape. This is partly what makes group theory so intuitively attractive to social anthropologists as a mechanism for predicting social phenomena: Once the conditions of group membership have been established, it is not possible to breach them without rendering the entire model incoherent.

Group and network theoretic models also have distinct capacities for visual representation. As illustrated in the Chapter 2, group models tend to be represented visually as enclosed shapes known as Venn diagrams, either with or without subsets and elements visualised as smaller shapes within them, where larger shapes represent homogenous attributes, and smaller shapes represent individuals. Venn diagrams visualise the homogenizing function of group relations by making the representation of individuals entirely optional. Because group theory incorporates no graph theoretic aspect, Venn diagrams represent an intuited abstraction of group theoretic principles, often giving rise to informal ideas about 'boundaries', and 'boundedness', in addition to the existing formal axiom of closure already encoded in group theory. Network models, by contrast, suffer no such limitation. In network theory, representation of individuals is not optional - individuals *must* be represented. Unlike groups, networks have the inherent property of forming graphs, made up of vertices linked by paths. Social networks are represented by a specific subset of set and graph theoretic axioms that treat vertices as individual people, and paths as relationships between those people (see Figure 51). In formal terms a social network can be characterised as $<Graph = \{Vertices, Pairs\ of\ Vertices, Types\ of\ Pair\}>$, where 'vertices' are people, 'pairs of vertices' represents a relationship between two people, and 'types of pair' represent types of relationship (White 2004; Hamberger *et al* 2011).

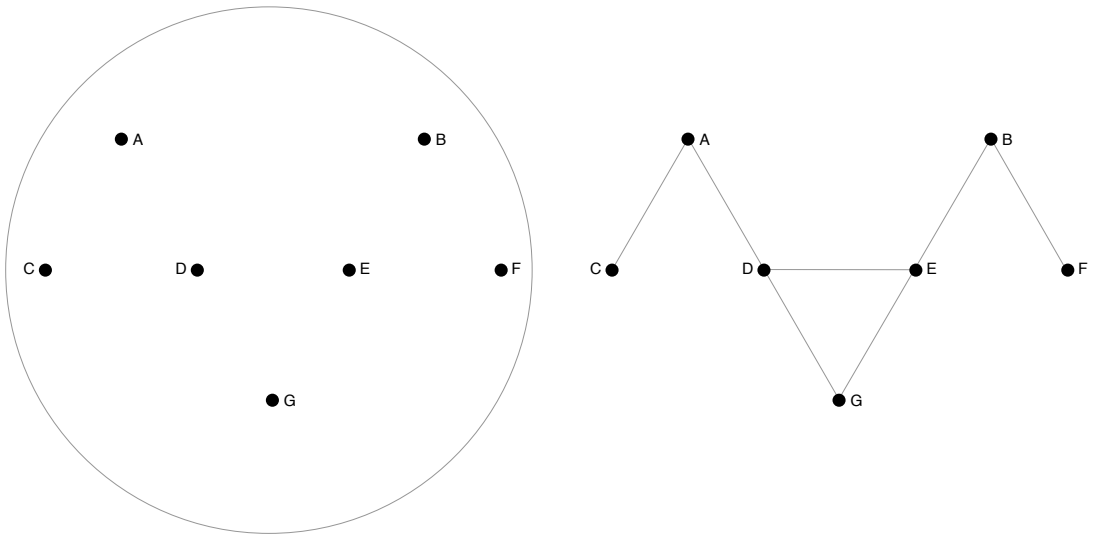


Figure 51: Social group (left) vs. social network (right).

Because the representation of individuals in network models is not optional, network models are less susceptible to construing sociality implicitly or metaphorically, as is typically the case in group models[41]. Instead, formal network models 'force' sociality into a more explicit and congruent construal in terms of real and specific relationships between real and specific people. As noted above, network models describe the attributes of both elements and relations as two different kinds of elements within a single set. This significantly increases the explanatory power of an already powerful technique, by enabling the classification of both people and relationships according to the attributes of both. In total, social networks model a) *whether or not* people are linked by relations; b) *the number* of relations by which people are linked; c) *the attributes of the people* linked together; d) *the attributes of the relation(s)* linking people together, and most importantly; e) *the shape of the overall network* in which people are linked.

Section 2.1.3 explained that sequential order is critical to congruent models of causality. Causal models do not require ordering if they are not concerned with congruence between the model and lived experience. However, they *do* require ordering if congruence is a priority, because of the way that causality is construed in the grammar of language (Halliday & Matthiessen 2006). This is why group theoretic models tend to be accompanied by one or more ancillary ordering mechanisms in anthropological theory - as a means of rendering otherwise *in*congruent models intelligible to readers. Group models, by themselves, have no order, in the same way that Halliday & Matthieessen's example "x=2" has no order (2006:25 - see Section 2.1.2). Group models therefore lack any mechanism for explaining how patterns of social interaction are caused. Social networks, by contrast, contain two distinct and equally important types of inherent order. One of these is a

[41] Unless the network model is itself tacit or metaphorical - as in the case of Keen (1999 - see previous chapter, see also Morphy 1988)

function of networks' overall (macro-level) *shape*, while the other is a feature of individual (micro-level) relational *directionality.* In (A) below, directionality has been left absent, showing only the network's overall shape, while at (B) the network has both a distinct macro-level shape and a distinct micro-level directionality.

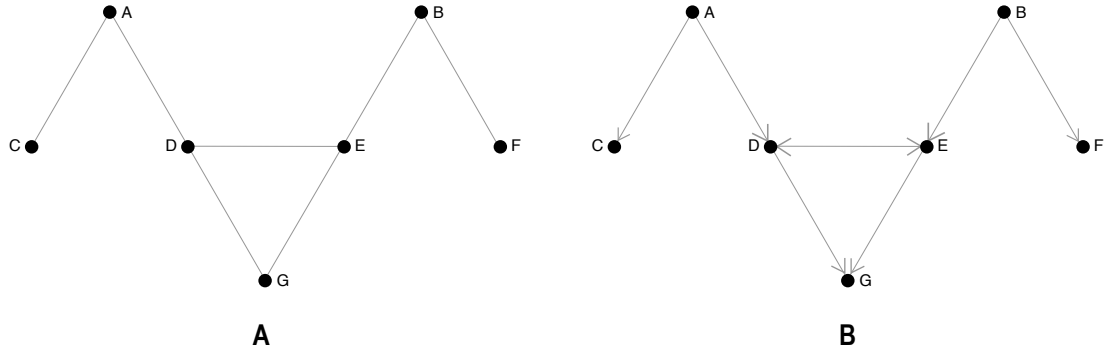


Figure 52: Two versions of the same unsigned network, one (A) undirected, and one (B) directed. Both networks encode causal flow in their overall shape, but only network B encodes it in the direction of individual relationships.

Both of these types of order generate interdependent forms of causal flow over time and space, where the earlier *formation* and/or continuing *existence* of a specific relationship or specific individual, in a particular region of the network, determines the later formation and/or continuing existence of other relationships and the connection of other specific individuals in other regions of the network. Networks thus have inherent temporal and spatial order, although this order is topo*logical*, rather than topo*graphic*. This means that network order can be measured in terms of the distances and configuration of relations between individuals within the network itself, but not in relation to factors 'outside' of the network, such as historical time, or geographic space (discussed in the next chapter).

Figure 53 illustrates the causal role of each of seven relationships in determining the overall shape of the undirected network graphed at (A). By removing each one of the relationships in turn, causal dependency within the network can be visualised. In three of the eight instances of such removal (first graph, top-left and last two graphs, bottom-right), the network remains connected, but is 'weakened' in distinct ways. In the remaining five instances, the network is *dis*connected into two parts, with variable effects on the composition and shape of each part. Figure 54 illustrates the causal role of each of seven *individuals* in the graph from (B) in determining overall network shape. By removing each one of these vertices in turn, causal dependency of the network can be similarly visualised, with the further effect that when an individual vertex is removed, all its incident relationships also cease to exist. The *order* of this dependency is encoded in the shape of the network at a given point its development. Any change in network shape thus reveals the respective influence of each relationship.

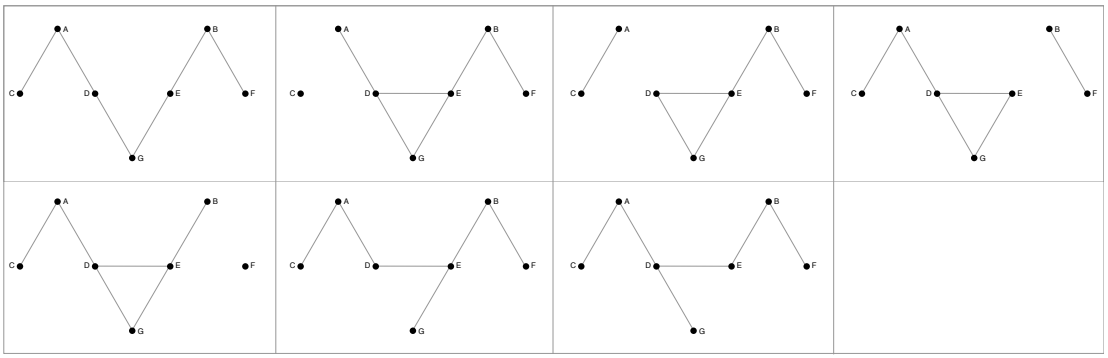


Figure 53: Illustration of the causal role of each relation in overall network shape, demonstrated by removing one of each of seven relations and observing the consequent effects on the network. Out of the seven instances of such removal, three (first top-left and last two bottom-right) show continuing connectivity. The remaining five instances show the breaking up of the network in different ways.

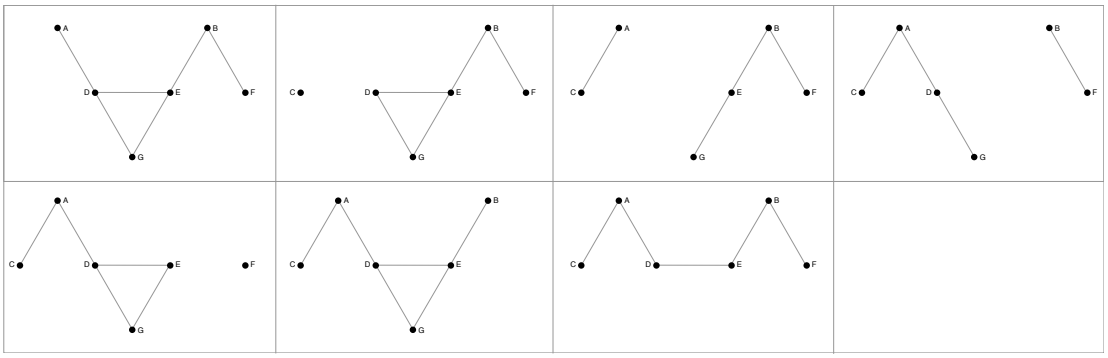


Figure 54: Illustration of the causal role of each vertex in overall network shape, demonstrated by removing one of each of seven vertices and observing the consequent effects on the network. Out of the seven instances of such removal, five (first top-left and last two bottom-right) show continuing connectivity, although the networks shrink to varying degrees. The remaining two instances show the breaking up of the network in different ways.

Where relationships in the network have a specific direction, the effects of removal are more specific. The networks at Figure 53 and Figure 54 represent 'moments' or 'instants' of change in a network over time. Although they reveal dependencies across the connected network at (A), and thereby reveal causal relations between specific people and specific relationships, they do not reflect any inherent order between each other. Because the network at (A) is undirected, the set of its seven developmental instances shown at Figure 53 and Figure 54 could be arranged in any sequence without altering the causal relations between them[42]. In the terms described in Chapter 2, the direction of causal *flow* is not determined at the macro-level of the whole network, even though causality itself is determined at the micro-level of individual vertices.

This is not the case for the directed network at (B). Because the relations in this network are directed, the flow of causality is inherently determined. We can see that these relations form a set of

[42] Cf. the arrangement of group theoretic elements.

four consistently directed chains of three or more vertices, namely <A→D→G>, <A→D→E→G>, <B→E→G>, and <B→E→D→G> (see Figure 55). Furthermore, we can see that these chains intersect and converge as they unfold. Firstly, relation <D→E> intersects with relation <E→D> (i.e. each of the two relationships extend from one vertex to the other, but in different directions), and secondly, all four chains converge on <G>. In addition, we can see *when* these intersections and convergences occur in the development of the network. Because all four chains are consistently directed, their constituent relations can be numbered as shown in Figure 55. Note the difference in chain lengths. With regards to causal flow, this difference also influences overall network growth. Assuming for example, that the network starts growing from vertex A and vertex B at the same moment, we see that relations <A→D> and <B→E> form first, followed by relations <D→E> and <E→D>, which form *at the same time* as relations <D→G> and <E→G>. Chains <A→D→G> and <B→E→G> converge on vertex G before chains <A→D→E→G> and <B→E→D→G>, because the former two chains are shorter.

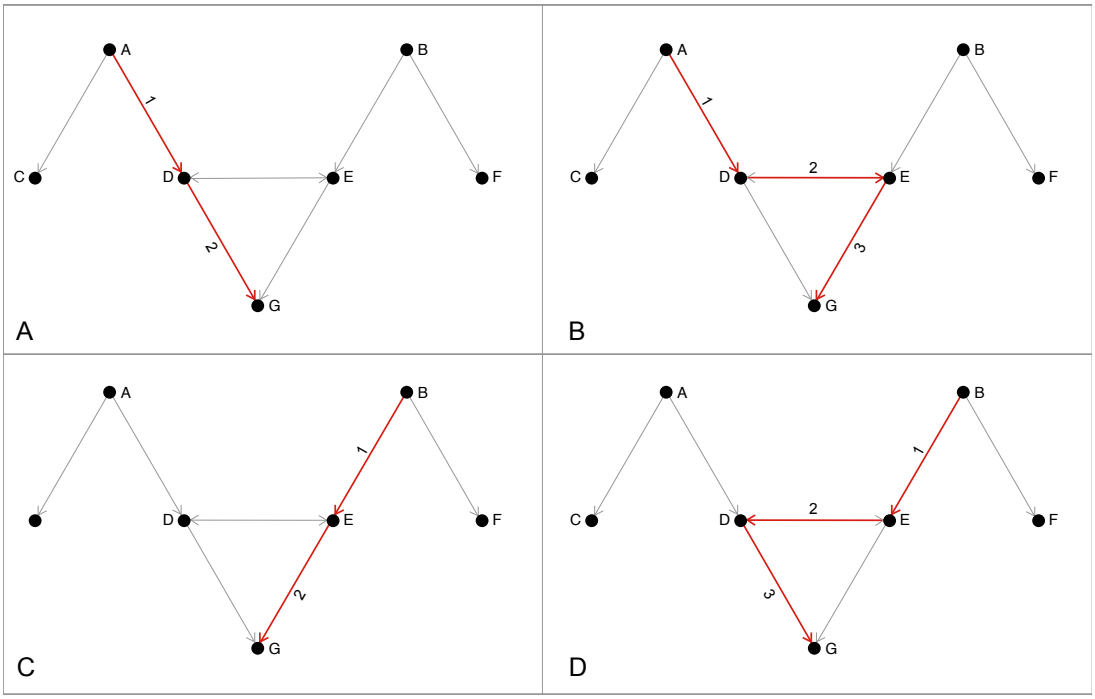


Figure 55: Four consistently directed chains or 'paths' tracing out interdependent causal flows in the same network.

As noted above, this graph (derived from the graph at Figure 51) is unsigned, meaning that none of its vertices or relationships is classified. A such, it exhibits only two of the three features defining a social network, listed above: 1) vertices, and; 2) pairs of vertices, but not; 3) 'types of pair'. The classification of 'types of pair' (i.e. types of relationship) brings us closer to understanding the network theoretic role of idea systems in the feedback *between* their own structure (i.e. the organization of ideas), and the structure of observable social processes (the structure of social

relations). An intuitive illustration is provided by a well-known[43], self-consistent model, described from a personal perspective as follows: 'Any friend of my friend is also my friend'; 'any enemy of my friend is also my enemy'; 'any friend of my enemy is my enemy, and; 'any enemy of my enemy is my friend' (see Figure 56). In this idea system for classifying friends and enemies, the pre-existing relationship between the speaker and a second person forms the prerequisite for the subsequent formation of the relationship between the speaker and a third person. These formations follow a sequential chain of causality as it flows through into the formation of an entire signed and directed network.

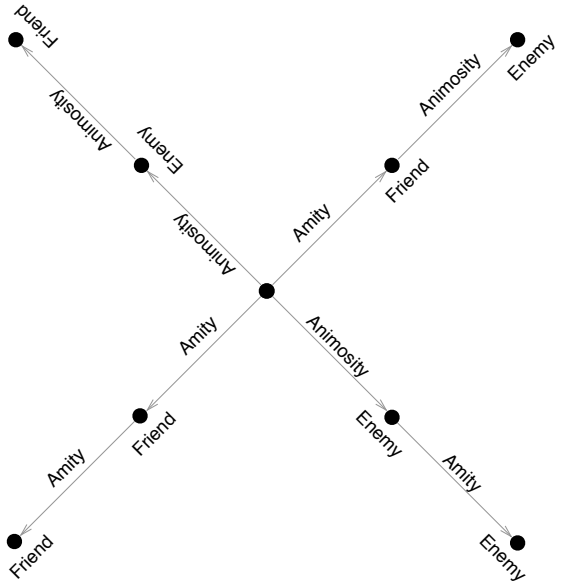


Figure 56: Graph of the causal flow of dependent classification across a signed network, starting from a personal perspective at the centre. The graph is 'signed' because vertices have been classified as either friends or enemies, and relations have been classified as either 'amity', or 'enmity'.

The graph at Figure 56 illustrates this causal principle in the simplest possible way, with none of the four chains relinking (i.e. not forming circuits by reconnecting with each other). In Figure 56 all relationships commence from the person at the centre ('self' or 'ego'), and spread outwards without influencing one another. However, if the same set of friend/enemy and amity/animosity classifications is applied selectively to the graph from (B), the effects of causal flow become more complicated, even though the network it represents contains fewer relationships. This is because the graph at (B) contains a *circuit* between vertices D, E, and G (see Figure 57). In this situation, the idea system that a person uses to classify other people and their relationships with those people, feed back into each other, and ultimately back to the person themselves, linking them all together.

[43] See e.g. White & Johansen (2005:9); Leaf & Read (2012:188-189).

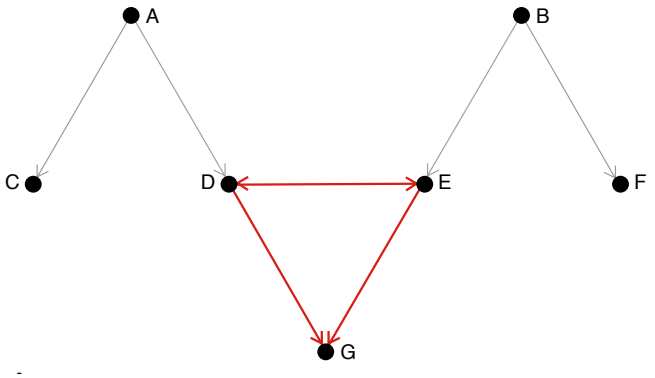


Figure 57: A circuit linking people <D,E,G>. Each person in the circuit in linked to each other by two different paths. IF the classification of relationships in one path is based on the classification of another, time becomes a factor in idea systems underpinning the classification.

As illustrated in Figure 53 and Figure 54, removing either one person or one relationship does not separate people linked together by a circuit, because each of them maintains a redundant (i.e. secondary) link that keeps them connected. In such cases, where causality can flow along more than one sequential path between a first and third person (i.e. a 'self' and an secondarily connected 'other'), there arises a potential to yield more than one effect on the network, depending on *which* sequence is chosen, and *when* it is instantiated. This is where idea systems become powerful factors in social processes, by providing a framework for choosing one particular sequential path to another person, over one or more other possible paths. Even if a person retains the ability to calculate a chain of no more that two sequential relations (as in the case at Figure 56), others in their circuit will make calculations that generate further sequences, and extend either further into the network, or connect back to the first person. This process gives rise to complex, self-generating social network dynamics. Furthermore, relative timing of relationship formation (as illustrated at Figure 55 above) becomes critical to its members' classification of themselves, other people, and relations within the network.

In order for the friend/enemy and amity/animosity idea system to 'make sense' (i.e. in order for it to be self-consistent), the circuit between people <D,E,G> can be constituted by only four distinct sequences of relationship. These are illustrated at Figure 58: Either all three relations are signed by amity, or two are signed by animosity and one is signed as amity. In the case of the latter, the sequence can be ordered three different ways: 1) <amity, amity, animosity>; 2) <animosity, amity, amity>, or; 3) <amity, animosity, amity>. There are two factors in the casual process surrounding this variability: 1) micro-level instantiation of individual relationships (i.e. whether any given relationship that forms at a particular moment is signed as either animosity or amity), and; 2) macro-level consequence of that instantiation for the rest of the network.

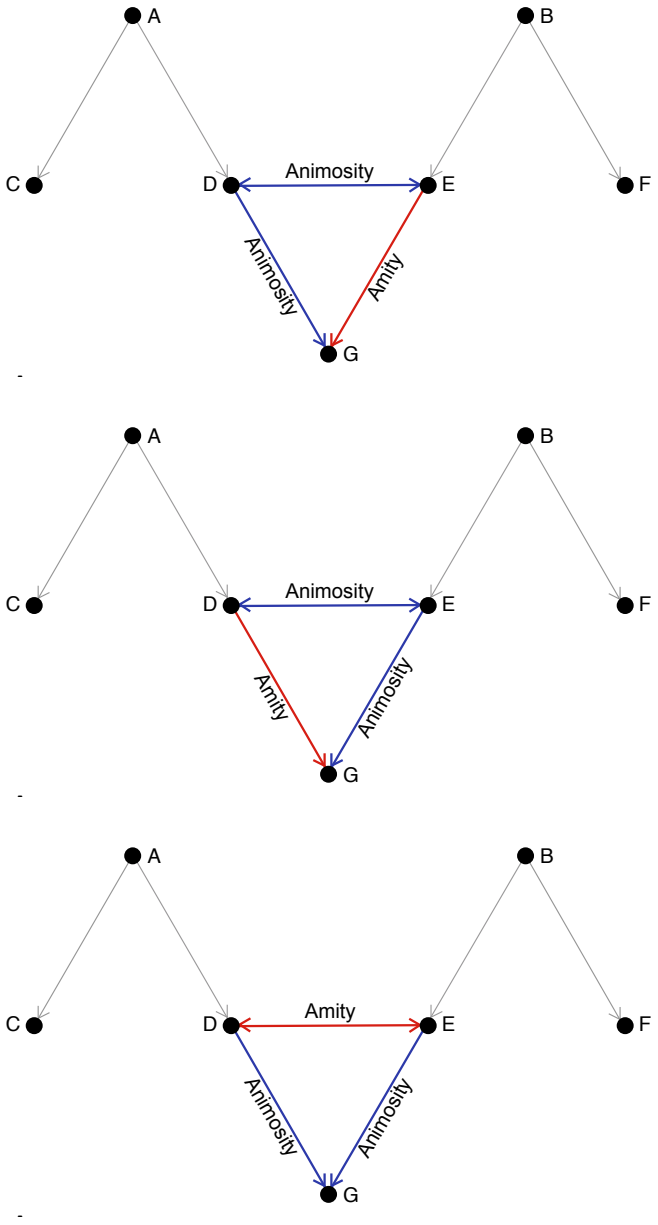


Figure 58: Three possible realisations of the 'amity-animosity' idea system.

At a micro-level, the configuration of the circuit is generated by the relative timing at which a relationship forms compared to others. It was shown earlier that consistently directed graphs contain an inherent topological order, such that in the case above, relationship <D→G> forms at the same time as relationship <D→E>, and relationship <E→G> forms at the same time as relationship <E→D>. Both are second stage relationships, preceded by relationships <A→D>, and <B→E> (see Figure 55). This means that, among all four stage-two relationships (<D→E>, <E→D>, <D→G, and <E→G>), the one that is signed (i.e. classified) first will determine how the remaining three will be signed. If, within the amity/animosity idea system, there is a further preference for amity over animosity, then whichever stage-two friendship forms first, will determine the relative sizes of both friendship subnetworks. At a macro-level, the configuration of signing within the circuit <D,E,G> determines the structure of the rest of the network. In the examples illustrated above, the four first stage-one

relationships (<A→C>, <A→D>, <B→E, and <B→F>) have been left unsigned. However, if we assume (for the sake of clarity) that they are all signed as *amity* then the network breaks up in three different ways, as shown at Figure 59, Figure 60, and Figure 61. The result is what might be recognised intuitively as two 'groups' of friends, A and B, connected to each other by links of animosity. However, from a network theoretic perspective, they are not 'groups', but rather 'subnetworks' embedded within a single, larger connected network.

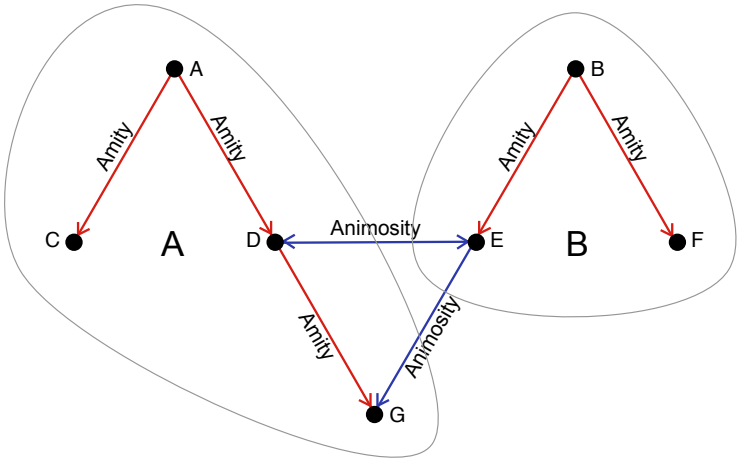


Figure 59: Frienship subnetwork 'A' is larger than 'B' because of its inclusion of person <G>.

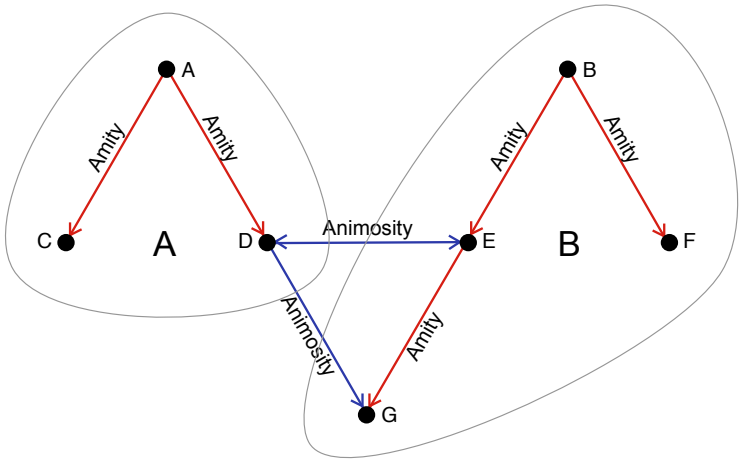


Figure 60: Frienship subnetwork 'B' is larger than 'A' because of its inclusion of person <G>.

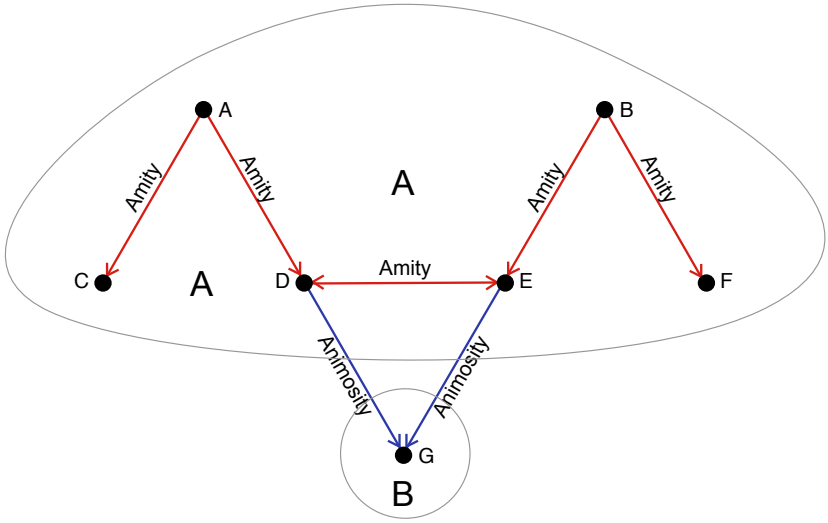


Figure 61: Person 'G' is excluded from the friendship network 'A'.

These simple graphs illustrate the flow of causality through a directed and signed network, where idea systems for classifying friends and enemies contribute to the emergence of complex feedback processes, linking individuals together by multiple, real and redundant relationships, as well as by multiple, overlapping ways of interpreting those relationships. Treating the observable effects of this feedback process as 'groups' obscures the dynamic between micro-level decision-making, and macro-level group structure. Treating observable social aggregations as 'groups' also stifles researchers' ability to comprehend and measure micro- and macro-level processes as interdependent factors within a single system. The feedback between micro-level links among directly connected pairs of individuals, and the macro-level structure of the entire network, is where the full explanatory power of social network analysis is focused. This feedback also converges with the previously defined object of social anthropological research: The feedback between instantiation of autochthonous idea systems, and observable social processes that both contextualize, and result from that instantiation. White and Johansen characterize this feedback as follows:

> *"When people interact, their behavior and their comprehension of that behavior shape some of the local or ego-based properties of their networks ([Figure 62]). Some people have more links than others (indegree and outdegree), for example. People may choose friends so as to avoid inconsistencies, like friends of friends who are enemies; and they are also more likely to choose friends who are already friends of friends. ... Each different kind of behavior sets up certain shapes and variabilities as to how networks look from the individual's perspective. These are micro properties of a network. Micro properties may have consequences. ... Among the consequences are emergent properties that we may identify as those that bring about 'entitivity' because they emerge along with discrete network units of organization that are consequential in having effects on other behaviors. These we call configurational effects. ... Further, these [structures] may include effects on the macro properties of a network. Macro*

properties of networks may or may not be directly linked to micro properties but in either case macro properties alter the context of everyone in a network, and may affect how people interact. This is the feedback loop shown by the outer circuit of arrows in Figure 1.2 [Figure 62].

Figure 1.2: Some Feedback Processes in Networks

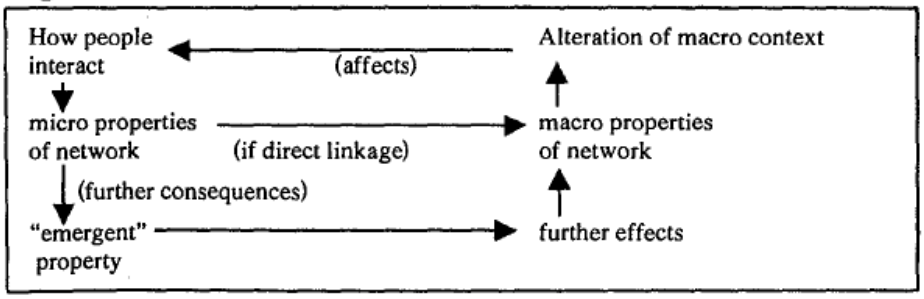


Figure 62: Construal of sociality and causality in social network models (White & Johnsen 2005:9)

A further powerful function of social network analytic techniques is their ability to reveal patterns of preferential relationship formation independently of data about the autochthonous idea systems that encode those preferences (White 2011; Hamberger *et al* 2011). The patterns of a network's growth in time and space reveal the structure of idea systems that provide a normative framework for network members' choices, independently of specific first-hand accounts of those idea systems by culture bearers. Because the shape of the network *emerges* from the accumulation of personal choices in a particular order (topological time) and in particular directions (topological space), the network can be treated as *encoding* a system of causality, by revealing patterns of choice in patterns of relationship formation (see discussion below). Where data about autochthonous idea systems *is* known, it can be compared with the patterns of independently observed choices, by synthesizing a separate 'idea system' network built from the choices encoded in the idea system alone, with the actual patterns of choice encoded in the real-world network.

Network theoretic models, applied consistently both to idea systems, *and* to observable social behaviour, have the capacity to describe complex patterns not only in both, but also in the causal links between them (see Section 2.1), in a way that group theoretic models do not. Applying network theoretic models in this way is not equivalent to deploying *metaphors* of networks, webs, or matrices. Debates such as those between Keen (1995) and Williams (1999), discussed in Chapter 2, centre on isomorphism between anthropological metaphors and autochthonous metaphors, yet refer only to anecdotal and ad hoc observations, and isolated quotes. Thus, while both Keen and Williams cite observations in support of their respective metaphors, and even though Keen deploys network *thematic* metaphors, it is impossible to test either of their observations, since neither anthropologist publishes accompanying data against which their metaphors might be tested. Although Williams does provide an extended quote from Ganambarr (1994) in support of her argument, this quote does not

indicate any prima facie correlation with the model advanced by Williams, even though it alludes to a universe of real people and social relations with which Williams is undoubtedly familiar. Although the arguments that the two anthropologists put forward in support of their respective models directly contradict each other, neither provides any measurable indication of feedbacks between idea systems and instantiations of those idea systems in the lives of real people. Rather, they provide highly abstracted, and weakly formalised descriptions of idea systems, where one idea system is that of Yolngu speaking people, and the other idea systems are those of Keen and Williams themselves.

This goes to the point raised in Section 2.1: Network thematic *metaphors* alone are not sufficient to implement a network analysis, but at the same time, neither is formalisation alone sufficient to render a model congruent with real-world sociality. For a model to be both minimally metaphorical and maximally congruent with real-world sociality, it must offer both the most accurate and comprehensive description of patterns in the data that it is modelling, *as well as* the most self-consistent explanation of the causes of those patterns. Weakly formalised metaphors that *connote* networks, webs, matrices, strings, and so forth, may be more congruent than metaphors of groups, as supported by Ganambarr's (1994) explanation of *Yothu Yindi* relations. However, network metaphors have no more explanatory power than group metaphors because like group metaphors, network metaphors do not empirically and formally describe *how* they influence or are influenced by, real and specific interactions between real and specific people.

The *Yothu Yindi* idea system, described with such rich intimacy by Ganambar (1994), is cited here deliberately. The real world relationships between Yolngu individuals and their mother's brothers are realised in powerfully meaningful terms by the *Yothu Yindi* model of kinship. However the standard social anthropological model of kinship characterises this relational model in terms of abstracted group theoretic entities, more frequently glossed using the moiety terms *Dhuwa* and *Yirritja*, closed under a similarly abstracted process of hereditary transmission. It is clear from Ganambar's account that this anthropological construal is not congruent with Yolngu cosmogony. Nor is the anthropological construal of *Dhuwa* and *Yirritja* terms as 'groups' empirically demonstrated by modelling of observed instances of interaction between people bearing *Yothu Yindi* relations with one another. All that the standard anthropological model of moiety terms offers is an amplified formalism, derived from the prescriptive logic of tacit group theory. The debate between Keen (1995) and Williams (1999) is over which perspectival *theme* is sufficiently rhetorically compelling to warrant appendage to this model.

Network theoretic models of sociality do not encode causality in the identity of groups, but rather in the relations between people. Such models explain social structure as an emergent property, emanating from the interactions between individuals, and expanding outwards to encompass a whole social universe as those individuals move through the topological space and time of a continuous and potentially infinite social field,. The congruence between this way of construing sociality, and the way

in which real people construe sociality, is evident in Ganambar's original explanation. This might be useful to revisit here:

> *"I want to explain to you what yothu yindi really means. ... Yothu Yindi is really a relationship term. The relationship holds for people, land and all that we see about us, for things such as animals, plants, wind, water and many more.*
>
> *"Yothu means 'child' or 'baby' and yindi means 'big' but in the expression yothu yindi, yothu refers particularly to the mother. This expression always involves two things in a relationship to each other. So if I am someone's or something's yothu, they are my yindi. ...*
>
> *"So we see how the relationship extends from the immediate family to cover whole tribes" (Ganambar 1994).*

The current section has presented a general summary of terms and definitions illustrating the paradigmatic potential of social network analysis, as against the limitations of what Kuhn (2012[1962]:178) called the 'pre-paradigmatic framework' of tacit group theory (see Chapter 1). The following section presents a more specific summary of a branch of social network analysis developed to deal directly with the kinds of idea systems that encompass *Yothu Yindi* relations - kinship network analysis.

4.1.2 Network Models of Kinship Terminologies and Kinship-Based Populations

Chapter 3 introduced sociocentric kinship terminologies in the context of their historical discovery and early, flawed attempts to model them using tacitly group theoretic axioms. In the current section both sociocentric and egocentric terminologies are redefined in the light of contemporary, network theoretic axioms provided by Hamberger *et al* (2011), and Leaf & Read (2012). For the reasons discussed in the previous section this new framework renders kinship terminologies as both more formally defined, empirically grounded, and congruent with observable reality on the one hand, and on the other hand as free of the prescriptive functionality implied in Lévi-Strauss' (1969[1949]) normative order. It follows that such a framework requires explanation in a language that has "an explicit conceptual syntax capable of 'relatively' precise empirical descriptions and/or of generating formal modelling of empirical relations", or what Bernstein calls a "strong grammar" (Bernstein 1999:164).

Before introducing the formal grammar of kinship network analysis, it is necessary to reinforce the definition of the term 'kinship' introduced earlier in the thesis, and to specify its relationship with the term 'genealogy'. The latter is a non-technical term in widespread use among English language speakers, who do not generally use the word 'kinship' in relation to their own social universe. The term 'genealogy' could be reasonably associated with the grammar of what Bernstein (1999) calls a 'horizontal discourse' in European culture, insofar as it refers to the tracing of successive generations

of a person's progenitors, and is not vertically integrated with any other areas of knowledge construed by the English language (see Section 1.1.2). The conventional use of the term in social anthropological language generally conforms to this 'common sense' meaning. By contrast, the term 'kinship' is highly contested in anthropological language, as illustrated at length in Chapters 2 and 3. As also established in those chapters, the term 'kinship' is used in the current thesis to refer exclusively to what Leaf & Read (2012 *inter alia*) call an 'idea system' - elaborated in the current chapter as a vertically integrated, self-consistent grammar comprised of well-defined lexicon and syntax, used to generate highly specific, meaningful relations between individuals, which are instantiated in real-world interactions. Even more specifically, in the definition provided by Hamberger *et al* (2011), this grammar generates observable networks that emerge from complex systemic feedbacks between micro-level individual decisions, and macro-level population-wide conditions in which those decisions are made (see Section 1.1).

The meaningful links between the terms 'kinship', 'genealogy', and 'biology' in the English language are what Radcliffe-Brown would call a 'historical accident' of European culture (Radcliffe-Brown 1940 - see Section 3.1.2). European culture bearers tend to think of their own kinship terminology as being consistent with scientific language, and as in contrast with the kinship terminologies of non-European, especially 'primitive'. In this way the widespread use of the term 'genealogy' among English speakers *assumes* a biological dimension, even though it usually has no scientific basis (except in rare situations where medical testing is involved). *Within* scientific language by contrast, 'genealogy' has a very limited meaning, used almost exclusively in relation to evolutionary biological processes, and certainly not in relation to human social processes. Hamberger *et al* (2011) explain this culturally specific definition as follows:

> *"Biology is the privileged model for kinship inasmuch as it affords a universally intelligible code for expressing the latter's fundamental relations: procreation as a model for filiation, sex as a model for marriage, and so forth.* ***However, being a model for a relationship does not mean being the essence of this relationship****. There are many social networks that are clearly kinship networks, both according to structural criteria and in the minds of those concerned, but where filiation, marriage, and gender cannot be defined exclusively in reference to biological givens. Kinship is a type of social structure, and if people all over the world have chosen to express its basic relations in a biological idiom, there are numerous cases where these relations are defined independently of, and sometimes even in opposition to, biological relationships. Defining kinship in network terms recognizes this fact (Hamberger et al 2011:534).*

In this thesis, the application of term 'kinship' is restricted to a) an autochthonous idea system, and; b) the objectively measurable relationships that are formed by the instantiation of that idea system. This means that we may talk about a) a 'kinship terminology' as a model of that idea system, and; b) a 'kinship network' as a model of the relationships formed through the *instantiation* of that idea

system. The latter is in fact a kinship-based *population model*, construed in the grammar of network analysis, which highlights the relevance of KNA to demography, as proposed in the introduction to the thesis. The term 'genealogy' is taken here to refer to a quasi-scientific Western folk model of biology and fort this reason is is not used.

Section 3.2.3 of the previous chapter illustrated the disruption caused to canonical[44] kinship theory by Lévi-Strauss' late pre-crisis era theoretical innovations (Lévi-Strauss 1968; 1969[1949]). At the heart of this perturbation was a weakening of the tacit group theoretic axioms of classical kinship theory, which posited a uniform 'rule-based' logic for the instantiation of marriages between descent groups, supposedly illustrated by sociocentric terminologies. Lévi-Strauss' (1968; 1969) introduction of an ancillary order for ranking degrees of variable spousal preference had the effect of loosening the rigid closure of this model (see Section 3.2.3). This loosening in turn facilitated his further proposal, that all affinal relationships comprise instantiations of reciprocal alliances between social collectives, the more expansive meaning of which is realised in the abstract terms of autochthonous idea systems, including kinship terminologies. In Lévi-Strauss' new construal, kinship terminologies did not impose 'rules' for prescribing spouses, but rather realised, in terms meaningful to culture bearers, more abstract autochthonous theories about the reproduction of social cohesion in the form of *networks* of people (Lévi-Strauss 1969[1949]:xxxiv - see discussion in Chapter 3).

Despite providing a more nuanced and sophisticated framework for interpreting patterns in kinship terminologies, the Lévi-Strauss' reformulated model still depended on a distinction between hypothetical 'mechanical' and 'statistical' patterns in kinship population data. Lévi-Strauss himself deemed social anthropology incapable of modelling anything but patterns at the former end of this spectrum, deducing that the exclusively 'statistical' patterns of generalised exchange lie beyond the scope of the discipline (Houseman & White 1998; White & Johansen 2005). At the root of this perception was an absence of what Bernstein (1999) terms a formal grammar for consistently modelling kinship structures of any kind, let alone network structures, and a lack of technical capacity within the discipline for developing such a grammar (see discussion in Chapter 1). While Lévi-Strauss was able to develop a sophisticated idiomatic language for describing patterns in terminologies, he was thus unable to develop a formal language of equivalent sophistication for describing patterns in kinship *population* data. White & Johansen (2005) describe this limitation as follows:

> *"Lévi-Strauss ... took the observation that [kinship] networks were too complex to deal with, as a justification for an approach to kinship based on norms and strategies as abstracted systems of rules. His models of how kinship networks might be constructed are based on highly reduced distillations of elementary sets of role relationships, and while these were not those of the*

[44] In Bernstein's sense (1999:64 - see APPENDIX 1 for extended quote).

> *nuclear family, they were considered to be elementary exchange relationships" (White & Johansen 2005:67).*

This observation, that Lévi-Strauss found kinship too complex to be treated as anything but an abstract theory, builds on an earlier diagnosis made by Houseman & White (1998). This suggests that Lévi-Strauss' view was an artefact of his own historical situation, where the structure of existing data, available computational power, and lack of consilience between anthropology and mathematics, limited the conceivable scope of anthropological research into kinship networks (Houseman & White 1998). A more productive view, in Houseman & White's (1998) opinion, is taken by treating kinship as the emergent product of a dynamic feedback between theoretical preferences on the one hand, and the pre-existing structure of real-world networks on the other:

> *"We ... hope to avoid some of the common analytical pitfalls that follow from ... a reliance upon static, ahistorical descriptions, the distinction between 'mechanical' and 'statistical' representations (Lévi-Strauss 1958:311), or what may well turn out to be a largely inappropriate emphasis on consanguineal unions [such as cross-cousin marriage, as discussed above]. Instead, our starting point is the marriage network itself, which we attempt to grasp as a structured whole. In doing so, we seek to provide a representation of the alliance system that is at once formal and statistical in nature, amenable to analysis both in terms of structure and in terms of historical change. Here, marriage practice is envisaged neither as informed by some ideal synchronic scheme (a 'mechanical' model), nor as a mere collection of individual behaviours (a 'statistical' model), but as a dynamic coordination of such behaviours: a network model. Moreover, we see the recurrent features of this coordination as being rather more loosely related to preferential and/or classificatory precepts as is usually (and often tacitly) supposed. Indeed, the very idea of a network model argues against the notion that action is to be analysed in terms of pre-existing normative or formal principles. Rather, it favors the idea of emergent characteristics arising from social interaction itself and governed by various feedback processes. Thus, in our view, a systematic account of real kinship connectivities constitutes a necessary first step in the development of more realistic, albeit more complex alliance models (Houseman & White 1998:2).*

Houseman & White's (1998) approach opens up a new perspective on kinship, which liberates it from misidentification as a group theoretic 'law and custom' (Morgan 1871 - see discussion in Chapter 2), as one among a closed set of other such institutions susceptible to a hypothetical 'non-adherence'. Instead, this new approach re-construes kinship as a fundamental and incontrovertible *property* of all human sociality. As a property of complex patterns of interaction, rather than a group of rules matched to a group of behaviours, complex networks of kinship relations lay the basis for exploratory testing for causal relations between patterns of ideas, and patterns of relationship-formation at large scales.

The formally defined scope of this undertaking also coalesces with the more radically framed project of Leaf and Read, in building a 'new Anthropology' based on empirical formal analysis, as outlined in Chapter 1 (Leaf 2007; 2009; Read 2008; 2013; Leaf & Read 2012), and summarised in the following statement:

> *"In summary, by empirical formal analysis we mean the kind of analysis that reveals the formal principles underlying idea systems. ... These are systems of ideas that are internally consistent and depend on identifiable rules of computation or definition**, yet they are also 'expansive' of what they seem to describe.** ... What empirical formal analysis yields, then, are the coherent idea systems constitutive of an organizational or conceptual universe in a community and the logical principles that unify it, that make it coherent and thereby make it objective for its adherents" (Leaf & Read 2012:104).*

To recap, this approach sets up three relationships between a) the theoretical model of the social anthropologist; b) the theoretical model of the subject population, and; c) observable patterns of behaviours generated by the instantiation of the population's own theoretical models in the form of demonstrable choices (see Figure 63). In empirical formal terms, the theoretical model of the anthropologist is *in*accurate if it cannot establish, on the basis of consistently organised and observed data, a correlation between the theoretical models prevalent among members a given population, and the patterns evident in the behavioural dynamics of that population. In other words, the anthropological model fails if it cannot demonstrate the *instantiation* of ideas in behaviour. This is no longer a classical social anthropological quest for anecdotal indications of 'people following cultural rules', but rather an exploratory modelling of the systemic feedback between reservoirs of meaningful options *apparent* to people, and the choices that they go on to make in the formation of relationships in the real world.

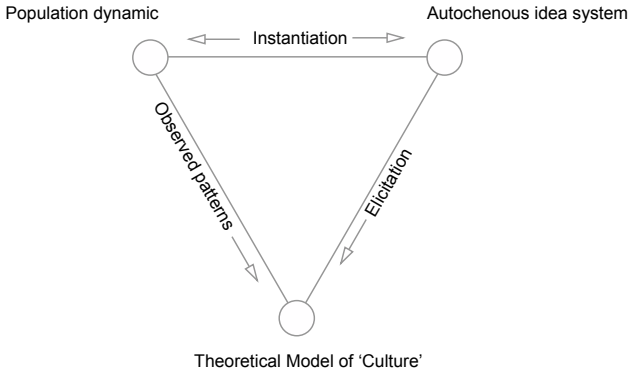


Figure 63: Empirical Formal Analysis.

From a network theoretic perspective, logical relations that link kin terms together allow terminology users to calculate all terms from a small subset of 'generative', 'core', or 'primary' terms (Leaf 2007, 2013; Leaf & Read 2010, 2012; White 1974; White & Jorion 1996). The generative terms of the British terminology for example, are 'parent' (encompassing 'mother' and 'father'), 'child' (encompassing

'daughter' and 'son'), 'sibling' (encompassing 'brother' and 'sister') and 'spouse' (encompassing 'wife' and 'husband'). Thus, the term 'grandparent' can be generated from 'parent's parent', the term 'grandchild' can be generated from 'child's child', and the terms 'aunt' and 'uncle' can be generated from parent's sibling, and so on. These latter terms are referred to as 'derivative' or 'secondary' (Leaf & Read 2012). The limits of the British terminology for example, are marked by the derivate terms 'great-grandchild', 'great-grandparent', and 'second cousin' (i.e. great-grandparent's great-grandchild). Beyond this kinship 'distance', British kin terms become infinitely ordinal or reduplicative, such as '*third* cousin' (ordinal), 'cousin *thrice*-removed' (ordinal), '*great*-great grandparent' (reduplicative), and '*great*-great grandchild' (reduplicative). The distinction between generative and derivative terms varies from culture to culture, and has important implications for the embedded logic of each terminology, as discussed below.

Following the definitions provided by Hamberger *et at* (2011) and (Hamberger 2011), the relations between terms in any kinship terminology can be measured numerically according the *length*, *height*, and *width* of kinship chains, where each link in the chain is a relation between two individuals. Length is a total count of the links in a chain, height is the maximum number of *consistently oriented* links (i.e. directed links that all 'point' in the same direction), and width is the count of consanguineous components (not separated by a marriage relation). Using these metrics to model the relational structures of egocentric and sociocentric terminologies allows the operation of both terminologies to be interpreted as a single, integrated grammar for construing sociality. From an *egocentric* perspective, kin terms mark successive changes in length, height, and width (*L,H,W*) relative to self, moving from a nominal centre outwards to the limits of the terminology. Generative terms for the British terminology graphed at Figure 64 and Figure 65 illustrate that both ego's parents and children stand at (1,1,1), one's siblings stand at (2,1,1), and one's spouse stands at (1,1,2). The limits of the British kinship terminology are marked by length 6, height 3, and width 3. For example, 'great-grandparent' and 'great-grandchild', both stand at 3,3,1), while 'sibling-in-law' (specifically sibling's spouse's sibling's spouse) stands at (6,1,3), and 'second cousin' stands at (6,3,1) relative to ego (see Figure 66 and Figure 67). Thus, egocentric terms realise kinship positions relative to ego by measuring direct links in indirect kinship chains in three different ways simultaneously.

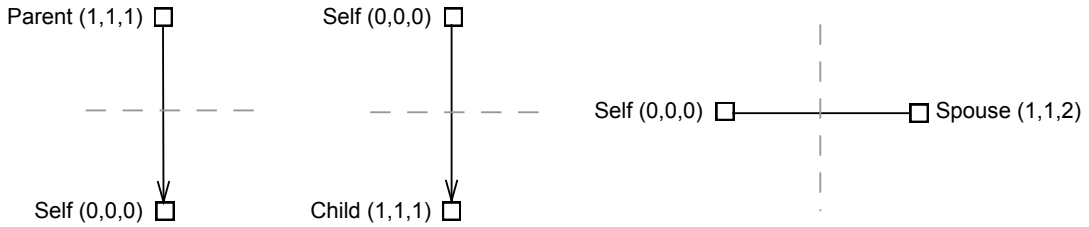


Figure 64: The function of egoocentric terms in representing length, height, and width (L,H,W), showing 'self' (0,0,0) in three positions, with changes in values marked by dashed lines. Note that measures for width and height have different starting values.

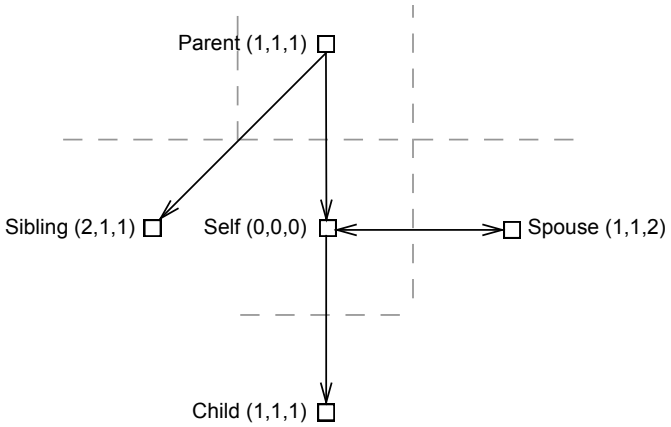


Figure 65: The function of length, height, and width in the generation of egocentric kin terms, showing convergent British examples.

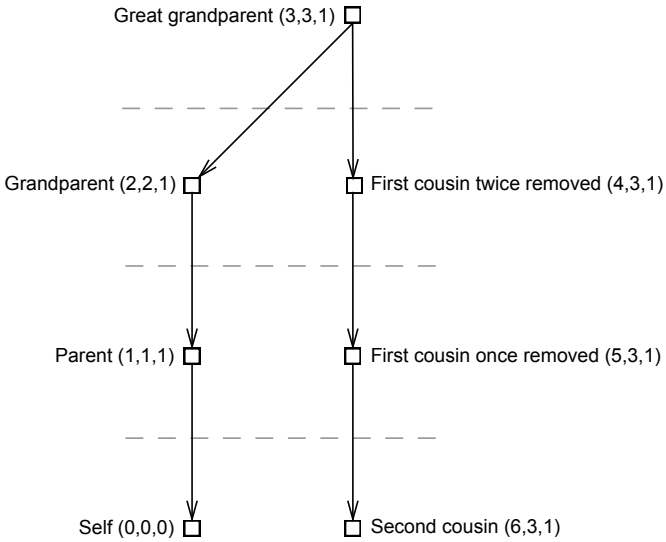


Figure 66: Visualised numeric notation, illustrating the maximal height of the British kinship terminology, at H=3 ('great grandparent') and length 6 ('second cousin').

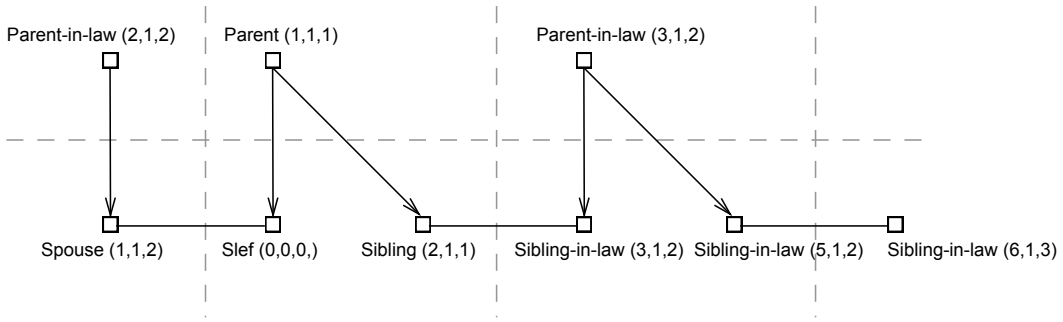


Figure 67: Visualised numeric notation for 'sibling-in-law' (sibling's spouse's sibling's spouse) illustrating the maximal length and width of the British kinship terminology, L=6, W=3.

Recollecting the *sociocentric* perspective introduced in Chapter 3, kin terms mark changes in length, height, and width between *any* two people according to *modular counts*[45] of intervening relations, irrespective of nominal perspective (such as the perspective taken in an egocentric terminology) or overall chain length. Thus, two people bearing the same vertical moiety term mark the width of their relationship as odd (*W* mod 2 = 1), whereas two people bearing opposite vertical moiety terms mark the length of intervening relations as even (*L* mod 2 = 0). Two people bearing the same horizontal moiety term mark the height of intervening relations as even (*H* mod 2 = 0) whereas two people bearing opposite horizontal moiety terms mark the height of intervening relations as odd (*H* mod 2 = 1). Two people bearing the same section term mark height *and* width of relations as even (*H* mod 2 = 0) and odd (*W* mod 2 = 1) respectively, whereas any pair of people for whom this is not the case will bear different section terms[46]. Figure 68 illustrates the principles of this modularity in minimal terms, while Figure 69 and Figure 70 visualise their application to the 'great-grandparent', 'second cousin', and 'sibling-in-law' relations described above.

[45] *N* modulo *n* refers to what remains of *N* after division by *n*. E.g. 2 modulo 2 = 0, whereas 3 modulo 2 = 1, and 1 modulo 2 =1. See Houseman & White (2001) and White & Denham (2008) for further details of modularity in Austro-Dravidian kinship terminologies.

[46] Any two people bearing the same *subsection* term also mark intervening length, height, and width as even. The difference is that subsection terms measure width by modulo 4 instead of modulo 2, such that where two people stand at (2,2,2), they will nevertheless bear different subsection terms, whereas in a section terminology they would bear a common term. Two people bearing the same subsections thus mark their intervening length, height, and width as (*L* mod 2 = 0, *H* mod 2 = 0, *W* mod 4 = 0).

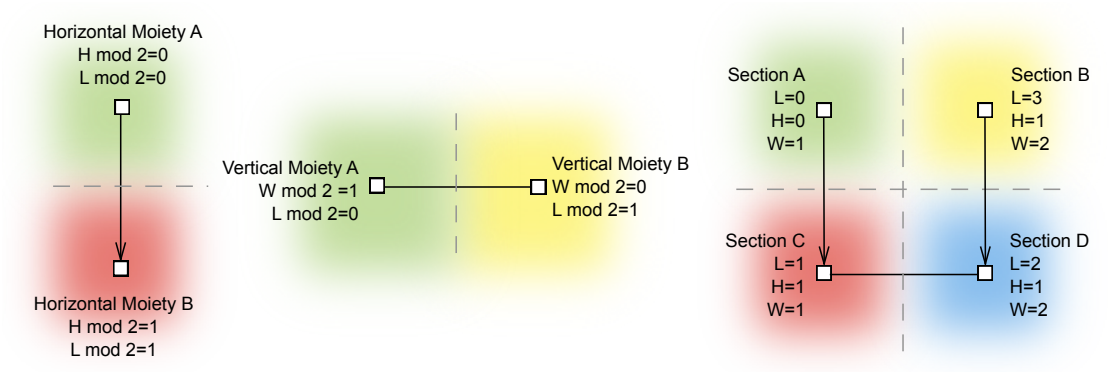


Figure 68: The function of egocentric terms in representing length, height, and width, modelled after the British egocentric kin terms at Figure 64 above.

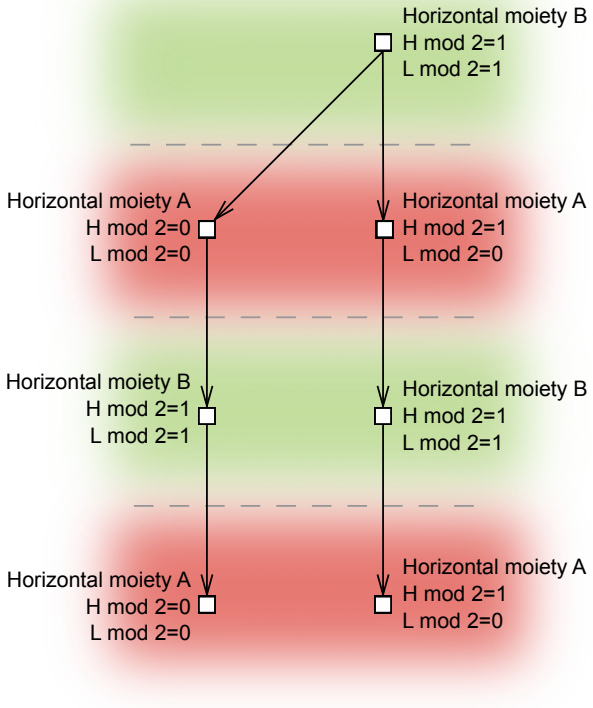


Figure 69: Visualised numeric notation applied to horizontal moiety terms, after the 'second cousin' kinship chain illustrated at Figure 66 above. Modular count starts at bottom-left.

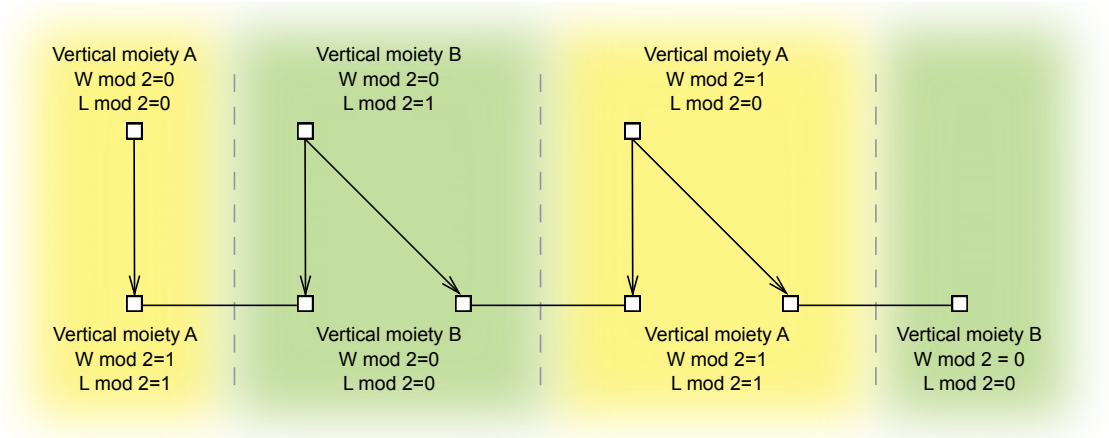


Figure 70: Visualised numeric notation applied to vertical moiety terms, after the 'sibling-in-law' kinship chain illustrated at Figure 67 above. Modular count starts at top-left.

It must be emphasised that the use of length, height, and width metrics to model the grammar of kinship terminologies, does not constitute a 'more accurate' modelling of observable relationships that autochthonous terminologies are themselves already modelling. Instead, the use of these metrics represents a form of modelling that is entirely *independent of* the autochthonous model, yet which strives for *congruence with* it insofar as it *explains* the way in which the terminology functions to realise meaningful relationships.

Length, height, and width are most accurately mapped by the now widely accepted positional notation developed by Barry (Barry 2004), where 'F' = woman, 'H' = man[47], '.' = marriage, '()' = ancestor, and relationship direction commences with ascent but changes to descent following '()', such that FF.H = 'woman's mother's husband' (2,1,1), F()F.H = woman's 'sister's husband' (2,1,1), and F()FH = 'woman's sister's son (2,1,1) (see Hamberger *et al* 2011 for English language overview). This notation negates any reference to specific kin terms, requiring neither an implied 'ego' position to describe a kinship chain, nor a set of British acronyms and its implied definitions of kinship, *viz.* 'M' (mother), 'F' (father), 'Z' (sister), 'S' (son), 'Sp'(spouse), etc. Positional notation also quickly reveals the structure of a kinship chain, because the number of characters represents the chain length, while the longest sequence of characters between pivots and parentheses represents the height, and the number of points represents the width. Thus HFF(F)HHF, in which '(F)' indicates a female apical vertex, has a length of 6, height of 3, and width of 1. These attributes are much less clear in the equivalent standard anthropological notation of the same chain 'MMBSD', or 'second cross cousins' in the British terminology.

[47] From the French *femme* (woman) and *homme* (man).

Apart from these advantages, positional notation also shows more clearly how a kinship chain encodes a sequence of choices regarding marriage, independent of the autochthonous terms attached to each choice. This is because, when a kinship chain is used to describe the links between two potential spouses, it shows not a list of intervening egocentric British kin terms such as 'MMBSD', but rather the independently observable changes in one or a combination of gender, generation, and filiation that accompany each kinship relation[48].

Sets of kin terms and accompanying relations are rendered meaningful as a systemic theory of sociality because they *encode* these changes in a manner that is meaningful to culture bearers, which allows culture bearers to make decisions that are meaningful in the real-world context of their own lives. Culture bearers think neither of 'MMBSD' nor ' HFF(F)HHF' when regarding a person in such a position, but instead refer to a particular kin term, which *encodes* that position. This coding generates a range of options for relationship-formation, which takes on the appearance, to an outside observer, of a preferential spectrum whose parameters might be intuited as 'ideal' and 'impossible'. In all kinship terminologies, the chain between a hypothetical self position and a reciprocal egocentric term for 'spouse' thus has the potential to be structurally equivalent to one or more other chains in the terminology that end in terms *other than* 'spouse', such as when H.F is equivalent to HFF(F)HHF. This is not because the individuals concerned necessarily *consider* 'spouse' to be equivalent to another term, but because in the kinship model of the relevant culture 'spouse' is the term *available* to mark the relative positions *instead of* another term.

This is a very different principle to those that have characterised group theoretic attempts to formalise kinship analysis. The process of selecting one term to describe a relationship instead of another possible term is fundamentally a process of culturally specific choice. Furthermore, it is a process of choice that results in specific material effects on the encompassing kinship network. The aggregation of these effects in turn generates observable patterns across a population. At a micro-level, two people who address each other as 'second cousins', for example, will not have the same effect on the network as two people who address each other as 'spouse', even though these two individuals may stand in equivalent kinship positions to each other when their relationship is formally modelled. Among the most obvious effects, is that one couple are unlikely to raise a child, while another is very likely to do so. When many similarly linked individuals make a choice one way or the other, a reticular pattern emerges at the macro-level of the encompassing kinship network (White & Houseman 2013). Such formalisation requires no imposition of *a priori* classifications, nor an imposition of ancillary ordering mechanisms as a means of generating causal explanation. Rather, it shows empirically and congruently how patterns of culturally specific choice become observable in

[48] This is distinct from Goodenough's (1965;1970) early attempt at formal kinship modelling, 'componential analysis', which depends entirely upon vernacular definitions in order to maintain formal consistency (*cf.* Houseman & White 1998).

the emergent structure of the network, and can be linked back to the generative function of the kinship terminology's grammar.

Structural equivalence in kinship terminologies is concomitant with a kinship network phenomenon known as 'relinkage' (Houseman 1997; White 1997; White 2004; Hamberger *et al* 2011; White & Houseman 2013), wherein any two people who are directly linked, are also *in*directly linked by a redundant secondary chain of relations such that they form a circuit (as described more generally for social networks in the previous section). Where such a circuit is consanguineous, it must by definition incorporate at least one other, shorter matrimonial circuit of minimum width 1 (Hamberger *at al* 2011)[49]. In other words, for a person to be part of a consanguineous circuit, there must also be at least one marriage in that circuit, the child of which can be excluded without breaking the circuits (see Figure **71**). It is thus possible to describe a consanguineous circuit as composed of two intersecting circuits: A matrimonial circuit, and a parental triad (two parents linked to each other and to a child), the significance of which is elaborated below. In standard anthropological kinship models, such marriages are described egocentrically from a male perspective, e.g. "Sp = MMBSD". By contrast, using positional notation the same chain is simply marked as a circuit: HFF(F)HHF, whereupon it can be represented as starting from any position in the chain or travelling in any direction, without altering its sequential structure. Starting from the second position, for example, the circuit can be represented as FF(F)HHF.H, from the third position as F()HHF.HF, from the fourth position as (F)HHF.HFF, from the fifth position as HHF.HFF(), from the sixth position as HF.HFF()H, and from the seventh/final position as F.HFF(F)HH. The circuit can also be completely reversed: FHHF(F)HFFH. This phenomenon is illustrated in Figure 72 using the example of a matrimonial circuit. When using positional notation to describe matrimonial circuits it is conventional to treat a marriage relation as the 'closing relation' for the circuit, and to commence the path with a vertex that is also a *pivot*, i.e. a vertex that is adjacent to that marriage relation.

[49] Where a circuit is incorporated into another circuit, it is referred to as an '*induced* circuit'. Induced circuits including a marriage relation are referred to as 'matrimonial rings' (Hamberger *at al* 2011).

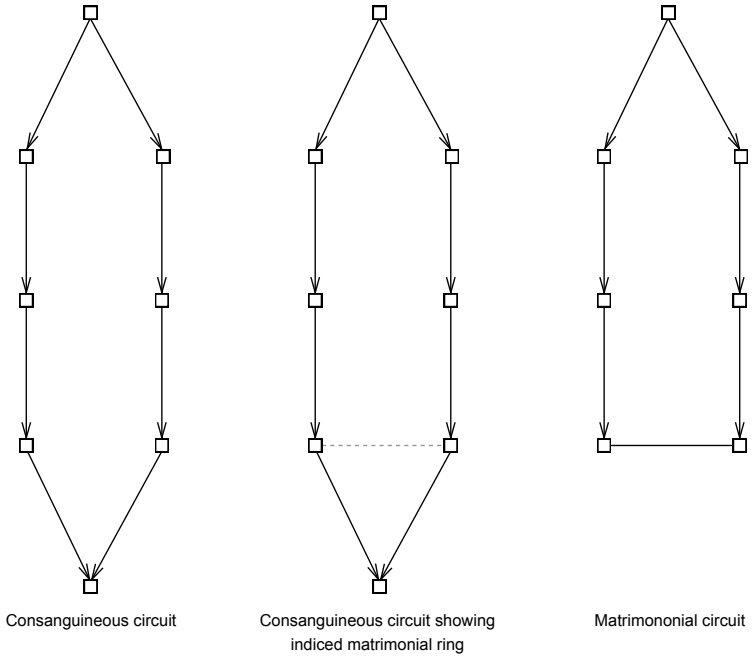


Figure 71: A consanguineous circuit (left) and matrimonial circuit (right), showing an intermediate graph (centre), where the matrimonial circuit appears as an induced subgraph, indicated by a dashed line (a matrimonial relation).

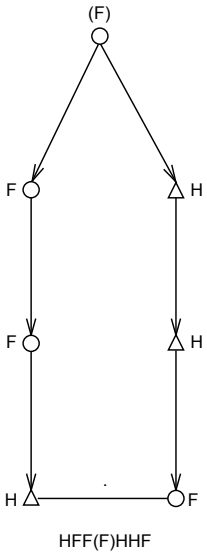


Figure 72: An example of positional notation mapped onto a matrimonial circuit.

Contemporary large-scale network studies such as those of Johansen & White (2005) further indicate that matrimonial relinkage is typically redundant when sufficient kinship population data is available. This means that where it can be shown that spouses who are linked by at least one matrimonial circuit within a large kinship network, those spouses are likely to be further linked by *multiple* matrimonial circuits in the same network or a larger, encompassing network. This is because matrimonial circuits accumulate over time to form very large and very complex structures of variable network density and cohesion. In this context, it seems reasonable to observe, in line with Lévi-

Strauss' 'exchange and alliance' proposition, that kinship terminologies function as autochthonous theoretical frameworks for comprehending and managing the emergent complexity of large connected kinship networks over time and space[50] (Lévi-Strauss 1969[1949]: xxxiv). Furthermore, White (2004), and White & Johansen (2005) have shown at such circuits accumulate into what are called '*k-components*' or '*marriage cores*', comprised of multiple intersections of such circuits, manifest in topological regions of variable density. White (2004) and Hamberger *et al* (2011) elaborate on this phenomenon by providing a formal language and accompanying procedures for detecting the 'cycle basis' of marriage cores, comprised of a set of irreducible circuits or 'rings'. The prevalence of one or another such set in a given network is held to provide insights to the prevailing kinship culture among network members, which give rise to more elaborate, reducible (or 'generalised') network structures. These principles are elaborated below.

Revisiting Morgan's (1871) typological schema in the light of this more formal network theoretic language, his 'Iroquois' terminology (see Section 3.2.1), recognised for its 'descriptive' distinction between 'parallel' and 'cross' consanguineous relatives, together with its 'classificatory' merger of siblings with 'cross-cousins', realises a suite of new meanings. Firstly 'parallel' siblings H()H = F()F and 'cross' siblings H()F = F()H are no longer ego-alter pairs partitioned into two subgroups. Instead they are realised as two sets of consanguineous chains or 'arches' (Hamberger *et al* 2011), distinguished by the unique sequences of changes between gender and generation. In relation to parents' siblings' children, for example, a 'parallel cousin' is the child of a father's brother (HH()HH = HH()HF = FH()HH = FH()HF) or mother's sister (FF()FF = FF()FH = HF()FF = HF()FH), whereas a 'cross cousin' is the child of a father's sister (HH()FH = HH()FF = FH()FH = FH()FF) or mother's brother (HF()HH = HF()HF = FF()HH = FF()HF). Of these relationship chains, a number of pairs are isomorphic, comprising reversed or 'mirror images' of each other. Among parallel kin, HH()HF is isomorphic to FH()HH, while FF()FH is isomorphic to HF()FF . Among cross kin, HH()FH is isomorphic to HF()HH; HH()FF is isomorphic to FF()HH ; FH()FH is isomorphic to HF()HF, and; FH()FF is isomorphic to FF()HF . Ultimately, what remains is a distillation of 10 chain 'types', with 6 corresponding to Morgan's 'parallel cousin' group, and 4 to his 'cross cousin' group (see Figure 73-Figure 76).

[50] This would indicate that egocentric and sociocentric terminologies function concomitantly, as a grammar for micro-level dynamics, and theory for macro-level social structure.

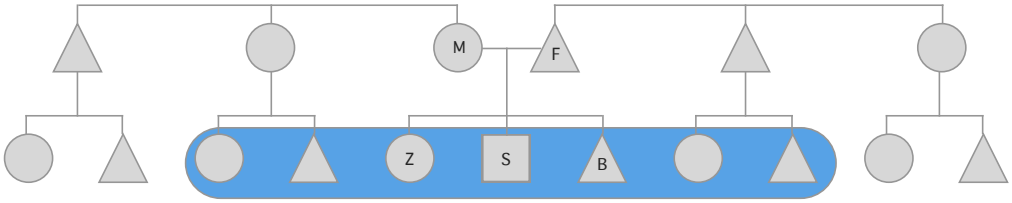


Figure 73: Morgan's (1871) group theoretic classification of parallel cousins, partially annotated with the standard model's Anglocentric nomenclature.

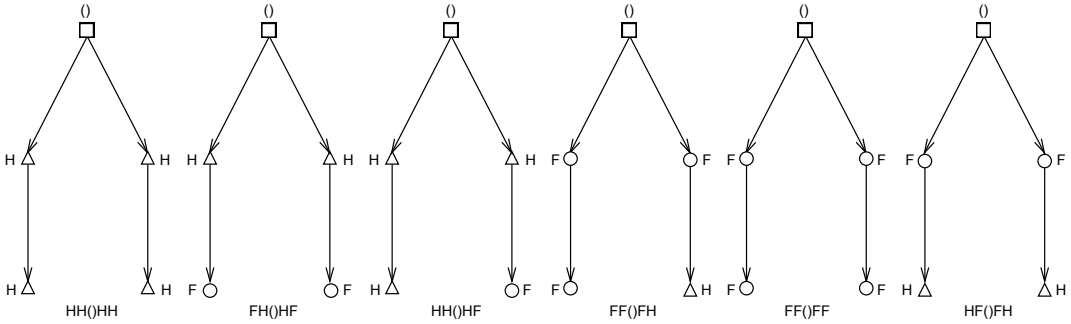


Figure 74: Six unique kinship chains characterising Morgan's (1871) 'parallel cousin' classification, annotated with positional notation.

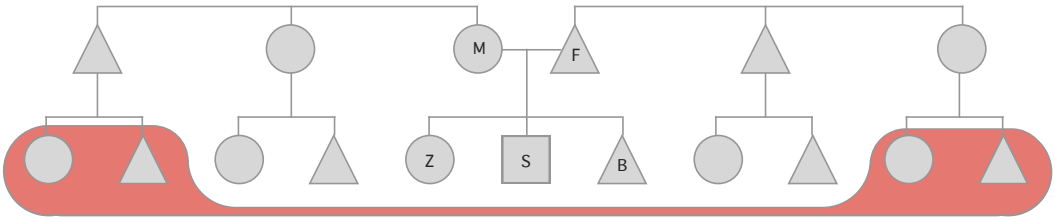


Figure 75: Morgan's (1871) group theoretic classification of cross cousins, partially annotated with the standard model's Anglocentric nomenclature

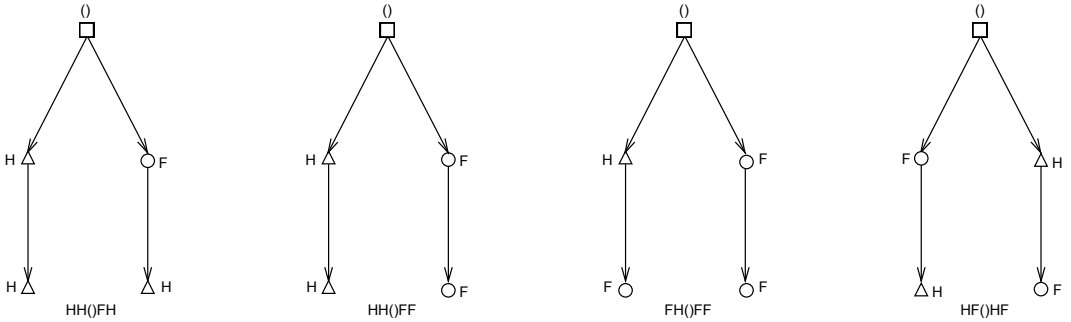


Figure 76: Four unique kinship chains characterising Morgan's (1871) 'cross cousin' classification, annotated with positional notation.

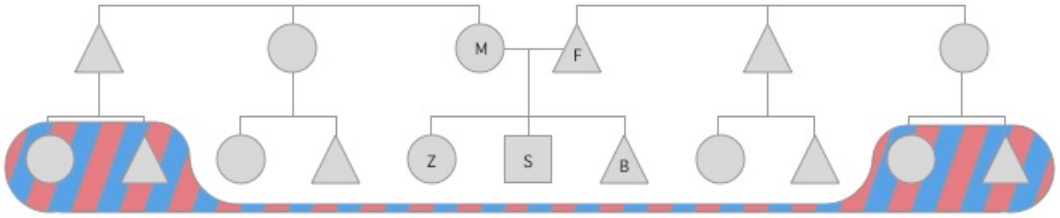


Figure 77: Lounsbury's (1964) group theoretic classification of cross cousins merged with spouses, partially annotated with the standard model's Anglocentric nomenclature

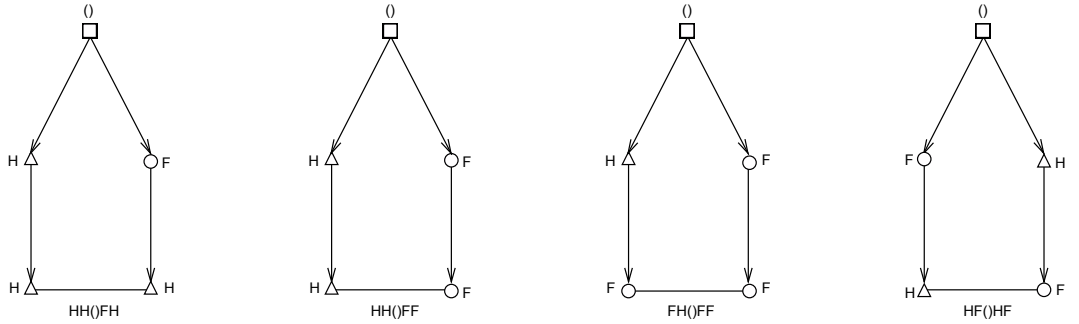


Figure 78: Four unique kinship circuits characterising Lounsbury's (1964) 'cross cousin'/Spouse' classification, annotated with positional notation

This network theoretic model accounts for the distinctions found by Morgan (1871) between 'classificatory' and 'descriptive' terminologies (see Chapter 3 for discussion), but without the attendant positivist deduction regarding the 'merging' of otherwise distinct positions under one or more common terms, or 'classification' of otherwise common positions under distinct terms. Morgan's ancillary order (the theoretical cline between 'most classificatory' and 'most descriptive' terminologies) functioned by counting unique terms relative to a limited *a priori* set of kinship positions, rather than by mapping the *full reservoir* of terms in a given terminology. Exploratory network theoretic modelling of *real* terminologies, as advocated by Houseman & White (2013), reveals a much more informative set of patterns however, by showing not a binary 'classification' or 'merging' of limited set of terms and positions, but instead a reservoir of potential pathways for realising distinctly meaningful relationships between real people in a terminology users' community. This approach indicates that kinship terminologies function as grammars, not by attaching classifications to things 'out there in the world' using categories 'in here in the mind', but by realising people and relations themselves *via* the terminology. In other words, the terminology itself *gives rise to* the people and relations in a speaker's social universe, rather than merely suffixing pre- or proscriptive 'rules' *onto* otherwise arbitrary labels. Furthermore, the grammar itself realises this meaning as a function of a systemic interdependence between its lexicon of terms and its syntax of so-called 'rules', such that the logical relations *between* terms are not merely attached to the latter, but emerge *from* the very real social settings in which the grammar is used. In Leaf & Read's (2012)

terms, the consistent logical relations that link terms together are *manifest* in the real-world relationships between people bearing those terms (Leaf & Read 2012; Read 2013).

Using the terms and definitions provided primarily by Hamberger *et al* (2011) and Houseman & White (2013), the current section has provided a brief introduction to the grammar of kinship network analysis, and its relationship with the tacitly group theoretic standard model of kinship devised by Morgan (1871). The section began by clarifying the distinction between the common sense term 'genealogy', as the weakly defined name of an autochthonous European kinship theory, and the technical term 'kinship' as a strongly defined field of empirical formal research. The section then proceeded to re-construe the group theoretic assumptions of the standard model in network theoretic terms by modelling both the egocentric and sociocentric terminologies that are used to define the Dravidianate (formerly 'Iroquois') categorization of Australian terminologies. In the course of this exercise it was illustrated that the so-called 'merging' function asserted by standard model Dravidianate terminologies, is actually an incongruent construal of generative relationships that give rise to reticular structures or 'circuits', as shown by White (2010a, 2010b) and Houseman & White (2013.).

4.1.3 Defining Cohesion in Kinship Network Terminologies

> *"As social cohesion is one of the most subtle concepts in social science, at least in terms of measurement, its logical rendering is dependent upon insights into the fundamentals of network structures. ... Cohesion can be used for measuring the extent to which networks may be constructed so as to be relatively invulnerable to structural ruptures through disconnection by removal of vertices [individuals], or to the disruption of flows by the closure of paths [relationships]" (White & Johansen 2005:327).*

The previous section provided a general introduction to kinship network analysis, and indicated how its terms and definitions might be used to re-construe the data underlying the group theoretic standard model. The current section introduces a higher order of terms and definitions that go to a specific feature of kinship networks: Cohesion. Applying measures of network cohesion to re-construed CMDB sociocentric terminology models shows that they encode specific types of relational choices, whose instantiation tends towards the generation and regeneration of cohesion across kinship networks over time.

As discussed above, kinship circuits represent a special kind of connectivity. The reticular structures identifiable in kinship terminologies exhibit a level of connectivity *above* the base criterion for a network's existence (where individuals are linked together by a minimum of one relationship each). This points to a potential on the part of kinship terminologies *to give rise to* such structure in real-world population networks, because of the way that they encode that reticulation, where individuals are *re*linked to each other through multiple relationships. Real-world kinship circuits comprise a kind

of structure that is very different from genealogical 'trees' - the most common form of network structure associated with 'common sense' Eurocentric kinship models. In genealogical trees - whether they extend backwards in time from a single descendant 'root' to multiple ancestor 'branches', or forwards in time from a single ancestral 'apex' to multiple descendants - the entire structure is comprised of individuals linked to one another by no more than a single cognatic path. However if any two people in a kinship network are linked by *more* than one path, then the measure of the network's connectivity, known by the variable K, increases. An intuitive way of defining a network's K connectivity (whether that of a kinship network or any other kind of social network) is the number of relationships that must be removed in order for it to decompose into two or more disconnected parts, or *components*. In these terms, a component is a *connected* network.

Any social network comprised of people linked to each other exclusively by a single path exhibits a connectivity level of $K1$. Such networks preclude circuits, and may take either one or a combination of only two forms: A single chain, in which each person is linked only to a maximum of only two others (see Figure 79A) or a 'star'-like structure with one vertex linking others together (as in the case of genealogical 'trees' - see Figure 79B). Networks exhibiting $K1$ connectivity are less cohesive and thus more vulnerable to decomposition (White 2004; Nooy *et al* 2014). By contrast, networks with a connectivity level of $K2$ or higher must by definition include circuits, whose reinforcing effect increases as the value of K increases (White 2004; Nooy *et al* 2014). Such networks are described as more cohesive because the removal of individual or vertices or ties is less likely to cause the network to decompose. Just as $K1$ networks take the graphic form of a linear path, star, or tree, the simplest form of a $K2$ network is a triadic circuit (see Figure 79C), which has the same structure as a parental triad in a kinship network, described in the previous section. Removing a single vertex or tie from such a structure transforms it into a chain such as that at Figure 79A, but does not break it up into separate components.

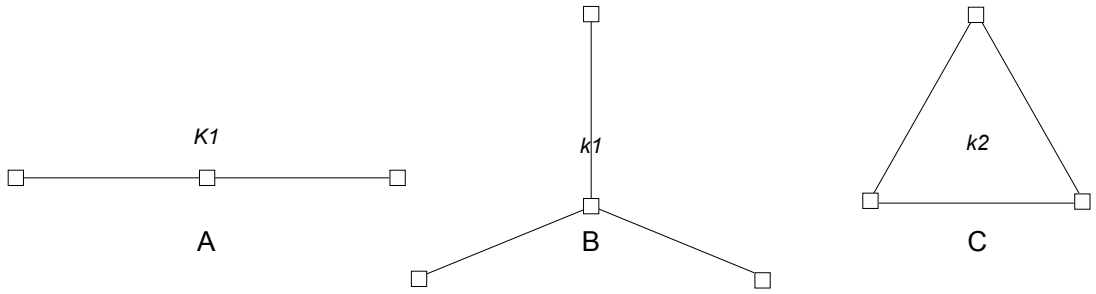


Figure 79: Examples of variable cohesion. Graphs A and B exhibit K<2 connectivity, where the removal of one tie disconnects the corresponding network into multiple components. Graph B exhibits K=2 connectivity, where removal of a tie or vertex disconnects the network.

If the connectivity level of a developing network increases beyond $K2$ over time, interesting properties begin to emerge in the network's structure. Of most relevance to the current discussion is the proliferation of multiple independent circuits, defined by their respectively unique compositions

from sequences of vertices and ties, where each circuit contains at least one tie not included in any of the others. Such circuits exhibit variable levels of intersection, where they share one or more ties in common. This principle is illustrated at Figure **71** above (see previous section) using the simple example of a consanguineous kinship circuit and its decomposition into a matrimonial kinship circuit and parental triad. Although such circuits are defined as independent in terms of their unique compositions, their intersection reflects emergent interdependence. In the case of a consanguineous circuit for example, a marriage edge between spouses precedes the emergence of a child and the incident consanguineous arcs that link it to its parents. Figure 80 below illustrates the simplest possible network of $K3$ connectivity (top-left), together with five independent circuits into which it can be decomposed. Each tie in the network facilitates a different circuit, which in turn contributes to the particular patterning of the network's overall cohesion. Another important feature of this example is that it illustrates how the intersection of almost any two circuits induces a third circuit from the existing set. The intersection of Circuit 1 and Circuit 3 induces Circuit 5 for example. Commutating the operation generates the same set - i.e., the intersection of Circuit 5 and Circuit 3 induces Circuit 1. These examples apply not only to social networks, but to any network. The significance of this phenomenon for kinship terminologies is elaborated below. Formal mathematical definitions for cohesion and related measures are provided at Equation 20 below.

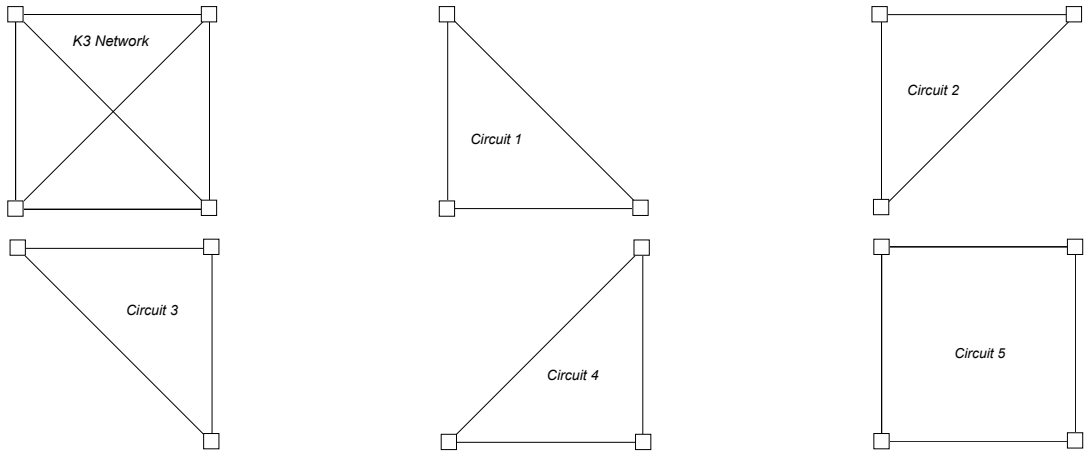


Figure 80: A K3 network and five independent circuits into which it can be decomposed.

The concepts of network cohesion and connectivity are related to measures of a network's average degree (the average number of ties incident to a network's vertices), and the distribution of that degree across the network as a whole (Nooy 2014; White & Johansen 2005). These measures have proven mathematical definitions. The average degree Av for a graph G is defined as $Av(G) = \frac{2t}{v}$, where the variable t represents the number of ties in the network, and variable v represents the number of vertices, such that for the graph at Figure 79A, $Av(G) = 1.3$, while for the graph at Figure 79B, $Av(G) = 1.6$, and for the graph at Figure 79B, $D(G) = 2$. Meanwhile, the distribution P of a

given degree d is defined as $P(d) = \frac{v}{v_d}$ (Nooy 2014; White & Johansen 2005). For networks such as those graphed at Figure 79C and Figure 80, where all vertices clearly have the same degree as each other, this calculation is straightforward. For Figure 79C, $P(2) = \frac{3}{3} = 1$, while for Figure 80, $P(3) = \frac{4}{4} = 1$. For these networks, connectivity is evenly distributed, hence the value '1'. However, for networks such as those graphed at Figure 79A and Figure 79B, where average degree is represented by fractions, cohesion is distributed unevenly. Given that we are dealing with average degrees of between 1 and 2 for both these networks, it makes sense to test for degree distribution at these two parameters. Testing for $P(1)$ in Figure 79A, yields 0.66 while testing for $P(2)$ yields 0.33. Testing for $P(1)$ in Figure 79B yields 0.25, while testing for $P(2)$ yields 0.75. This indicates that further to being minimally cohesive, with no circuits present, the connectivity exhibited by these networks is concentrated around a minority of their vertices. The minimal connectivity of these networks is a function of their centrality (a measure that can also be formalised mathematically - Nooy 2014; White & Johansen 2005). In both social networks and kinship networks, the variation among these measures reflects the overall cohesion of real communities, defined as people connected to each other by real, cognised relationships. White (2008) has developed a method for measuring such variation in terms of 'k-cores', which can be graphed in the form of an undulating manifold.

Equation 20: Long form explanation of formal mathematical measures for network cohesion and connectivity.

A re-construal of the standard model in network theoretic terms reveals particular types of reticulation in the grammar of the terminologies themselves. As demonstrated in Section 4.1.2 above, the consanguineous and affinal preferences classified in Lounsbury's (1964) 'Dravidian' addition to Morgan's (1871) original 6-part terminological typology are more congruently construed as reticular paths between spouses, rather than marriageable 'groups of egos' and 'groups of alters'. As will be shown below, Australian sociocentric terminologies encode precisely such structures, validating their distinct classification in Lounsbury's extended 7-part taxonomy (1964). In addition to this confirmation however, the general acknowledgement that circuits are encoded in sociocentric kinship terminologies induces a further revelation: The same grammar of spousal preference that generates matrimonial circuits also generates *other* types of intersecting circuit *independent* of those predicted by the standard model. In this case, the discovery of almost any two circuits infers the existence of one or more *other* circuits, which are not explicated in the terminology. As described in the previous section, Lounsbury's (1964) discovery that Dravidian terminologies encode an option for addressing commonly related relatives by either one of two different kin terms, also reveals that there is more than one way to realise the relationship because Dravidian terminologies provide a grammar that realises more than one kinship path between 'self' and 'other' positions.

The remainder of this section shows, following White and Denham (2008), that not only is this optionality also apparent in the Austro-Dravidian terminologies of Australian cultures generally, but that the matrimonial circuits explicitly defined in the terminologies also infer the likely existence of

many other types of circuit that are *not* explicitly defined by those terminologies. The cumulative effect of these multiple intersecting circuits is a tendency towards the generation of kinship network cohesion over deep time. This raises the possibility that the terminologies themselves conduce to the emergence of cohesion across very large kinship networks irrespective of morphological variation among the terminologies themselves, or in the apparent degree of their pre- or proscriptive normativity, as asserted by the standard model of Australian kinship.

At the ostensibly 'simplest' level of what the standard model calls a 'moiety' terminology (see Chapter 3), there are only two vertices, 'moiety A' and 'moiety B', linked either by marriage in the case of vertical moieties, or filiation in the case of horizontal moieties. In network terms, this yields a minimally cohesive $K1$ component (see Figure 81). This characterisation is made possible by the standard model because it treats both gender and filiation as underdetermined for vertical moiety terminologies, and both gender and marriage as underdetermined for horizontal moiety terminologies. Treating terms as constituent elements of 'groups' subsumes the distinction between male and female bearers of each term, and treats gender as irrelevant to the causality of the model. Likewise, filiation is treated as irrelevant for vertical moieties according to a deductive reasoning that terms do not distinguish between generations, in the same way that marriage is treated as irrelevant for horizontal moieties according to the deduction that terms do not distinguish between spouses (see Chapter 3). A direct re-construal of the standard model's group theoretic grammar in network theoretic terms yields the simple $K1$ network shown at Figure 81 because of these assumptions, where a lack of gender is indicated by square vertices and a single generation is indicated by G^0.

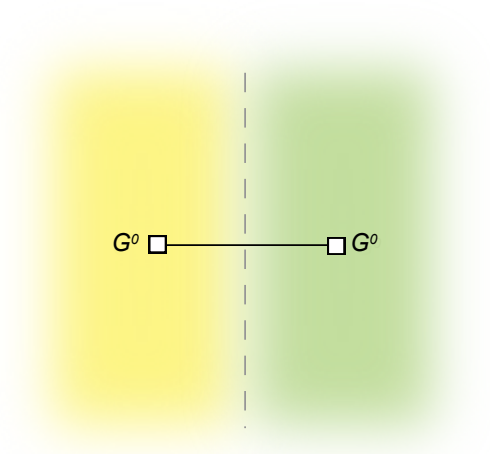


Figure 81: Re-construal of underdetermined group theoretic vertical moiety model in network theoretic terms, where filiation and gender are not defined. G^0 indicates a single generation while square vertices indicate undefined gender. The result is a simple network of $K1$ connectivity. Compare with Figure 68 above.

In contrast withe the standard model, and as Ganambar (1994) points out in relation to *Yothu Yindi* terms (see Chapter 3), the relationship encoded by a vertical moiety terminology is one of interdependence between real people whose gender is an *explicit* factor, i.e. as the relationship

between a child and their *mother* bearing *opposite* moieties (with child and child's father bearing the *same* moiety). Although individual gender is not realised explicitly by the terms themselves (*yothu* and *yindi* being gender neutral), gender is nevertheless implicitly determined by the intersection of marriage *between* both moieties, and patrfilial descent *within* each moiety. In other words, the *Yothu Yindi* terminology realises gender in a way that is dependent on the *use* of vertical moiety terms in the context of real people instantiating real relationships with one another, as with Ganambar's (1994) reference to 'mother'. The full semogenic potential (Halliday 2001) of the terminology is extrapolated from the terms themselves, but only from a model of the terms in use *among real people*, as Ganambar (1994) emphasises. By contrast, the standard model's construal of gender as irrelevant is thus incongruent with Yolngu people's meaningful experience of the terminology, exemplified by Ganambar's explanation. The first step in addressing this incongruence in the standard model is to take the implicit determination of gender into account, by representing *both* marriage *and* filiation in graph theoretic terms, as shown as Figure 82 below. This results in a simple circuit with $K2$ connectivity, such that every term is linked to every other by both one filial tie and one affinal tie. Filiation is indicated by directed vertical ties, with generations differentiated as G^0 and G^{+1}.

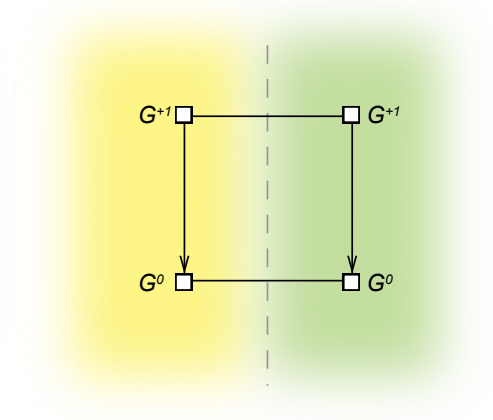


Figure 82: Re-construal of group theoretic vertical moiety model in network theoretic terms, taking filiation as well as affinity into account. The result is a simple circuit of connectivity K2. Filiation is indicated by directed vertical ties, with generations differentiated as G^0 and G^{+1}. Compare with Figure 68 above.

In the context of the kinship network theoretic model presented in Section 4.1.2 abov, this more congruent construal is still inconsistent however, for the primary reason that it describes marriage between individuals whose parents are already married. Although this is conceivable in deductive positivist terms (as reasoned by Morgan and Howitt (1880) in their theory of 'half sister marriage' - see Chapter 3), it is contradicted by the terminologies themselves, which allocate the same term to siblings, and alternate terms to spouses. This means that in order to construe the reult as a single simple circuit as at Figure 82, either the terminology is contradicted (the anthropological model is incongruent with the autochthonous model), or the relationships are contradicted (the anthropological model is incongruent with the observable reality). How are these contradictions

resolved? The answer is by taking into account the implicit realisation of gender. Note that the gender of vertices in Figure 81 and Figure 82 above have been left undetermined as per the standard model. If gender is construed explicitly however, and filiation is acknowledged, the result is the graph at Figure 83[51]. This shows two generations, G^0 and G^{+1}, with gender defined for vertices in G^{+1}, and includes directed ties for filiation, with vertical ties within each moiety, and diagonal ties transecting both moieties.

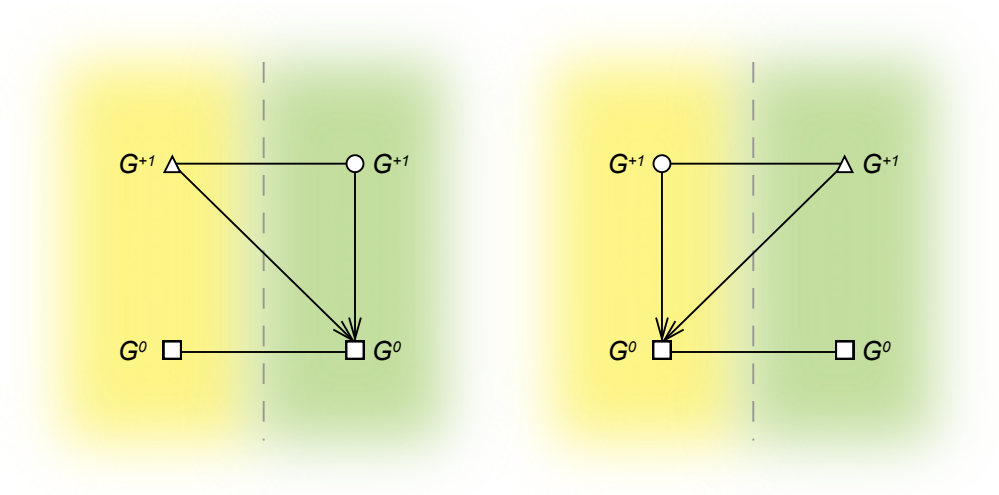


Figure 83: Implied matrifiliation, where the gender of vertices in G^{+1} is determined by the coincidence of both filiation and marriage, while the gender of vertices in G^0 remains underdetermined due to a partial incidence (one vertices has no incident filial ties). Compare with Figure 68 above.

Note that the genders of the child and child's spouse vertices at G^0 in Figure 83 have been left in an undetermined state, represented by square vertices rather than triangles or ellipses. If transmission of moiety terms is extrapolated to a further, lower generation (G^{-1}), then gender *must* be realised for the intermediate generation as well, in order to project further transmission. In addition to realising the genders of children and children's spouses, this exercise yields the further effect of inducing four kinds of pattern. If the graph remains in a planar state, embedded in a 2-dimensional surface, and the extrapolation is reiterated until all possible filial and affinal ties relative to male and female vertices have been realised, then the graph must grow laterally, as in Figure 84. These four graphs show all possible circuits according to the standard model's construal of a moiety terminology.

[51] Which assumes matrifilial transmission - (F)H and (F)F - as is the case for the sociocentric terms of the Central Murray Darling Basin population, as elaborated below (but *un*like *Yothu Yindi* associated terms of the Arnhem Land population, which ate are transmitted patrifilially)

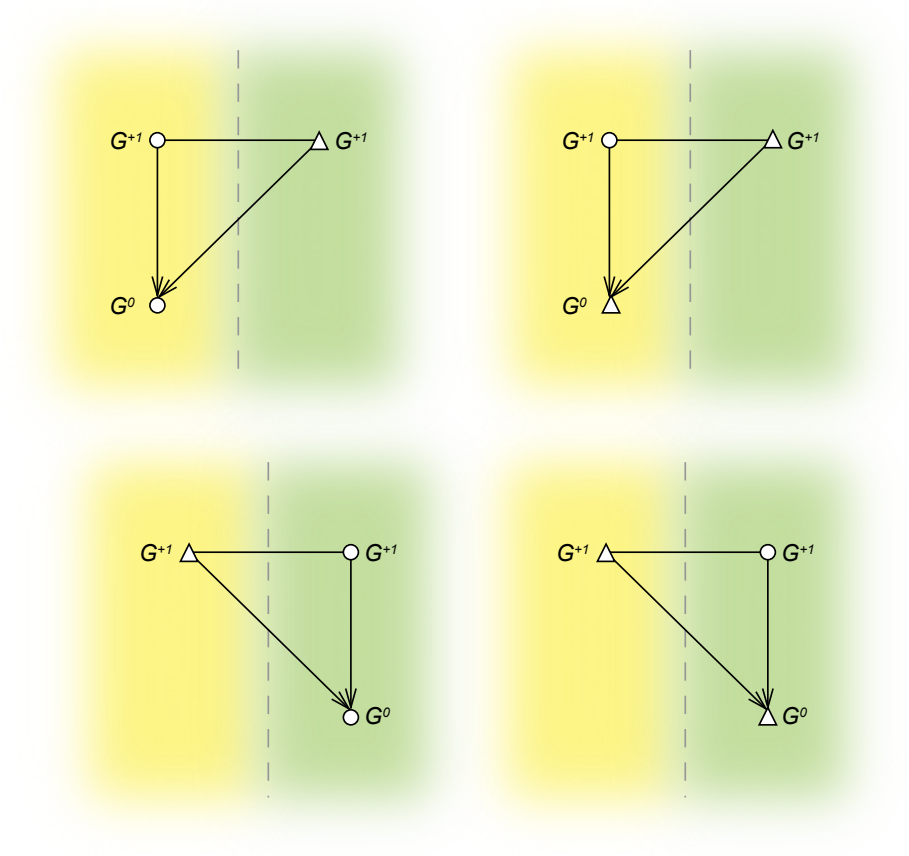


Figure 84: Extrapolation of implied patrifiliation (H)H and (H)F embedded in 2D space, for all possible filial and affinal ties, and male and female genders in a vertical moiety terminology.

Note that in such a 2D embedding the moiety terms must be visualised reiteratively, in order to describe all possible consanguineous and affinal relations. Like the graph at Figure 82, this is an incongruent construal of the way vertical moiety terms encode relationships, for the primary reason that moieties tend to be realised as a single continuum in autochthonous models, rather than in multiple separate reiterations. How to overcome this inconsistency? A 3-dimensional embedding of the graph allows it to be re-construed as a non-planar structure that *wraps back on itself*, as shown at Figure 85. This graph combines the four plausible expressions of the standard model's minimum circuits, into a single, self-consistent structure. This structure may be referred to as a 'moiety terminology network'.

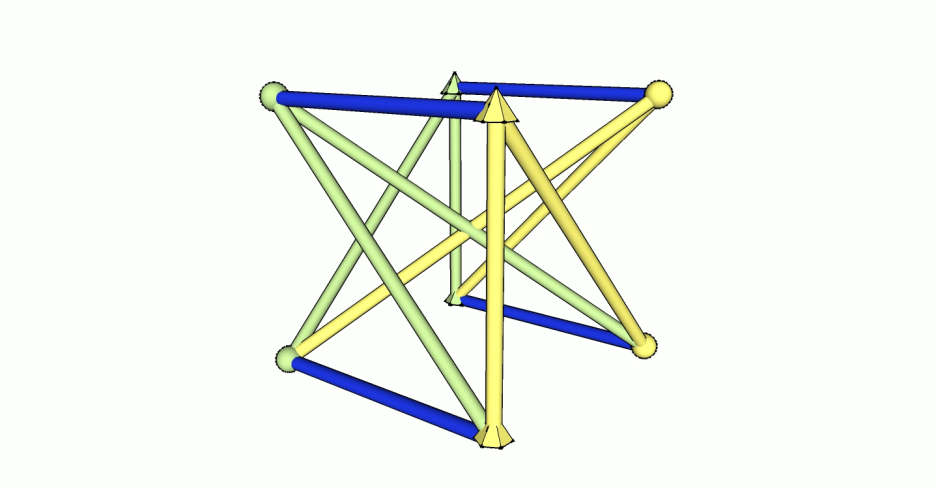


Figure 85: Extrapolation of patrifilially transmitted vertical moiety terms embedded in 3D space, for all possible filial and affinal ties, and male and female genders. Moiety colours have been remapped on (H)H and (H)F ties. H.F and F.H ties are blue. (F)H and (F)F ties yield a moiety change. See <goo.gl/ueU5Qc> for interactive 3D model.

Returning to the issue of cohesion, a cursory assessment of the 3D embedded graph at Figure 85 reveals that each vertex is embedded in the moiety network by 3 incident ties, comprising one affinal, one patrifilial, and one matrifilial relationship. The graph thus comprises a more cohesive, dense marriage 'core' (Houseman & White 1998) with a *K* count of 3. Most importantly for the current discussion, the network can be decomposed into a small number of distinct *types* of circuit, each of which is instantiated one or more times. Focusing on the affinal relationship at bottom left of the graph (for the sake of consistent visual perspective), Figure 86 illustrates 16 independent circuits, highlighted in red, where each circuit is comprised of a unique combination of relationships between 8 vertices. This indicates that for each of the 8 individual vertices in the network there are 16 independent ways of reaching each of the other 7 vertices. Although incorporating a syntax of only two terms, the grammar of a vertical moiety terminology construed in this way thus undermines the notion of simple 'direct exchange' between two opposing non-gendered 'halves'.

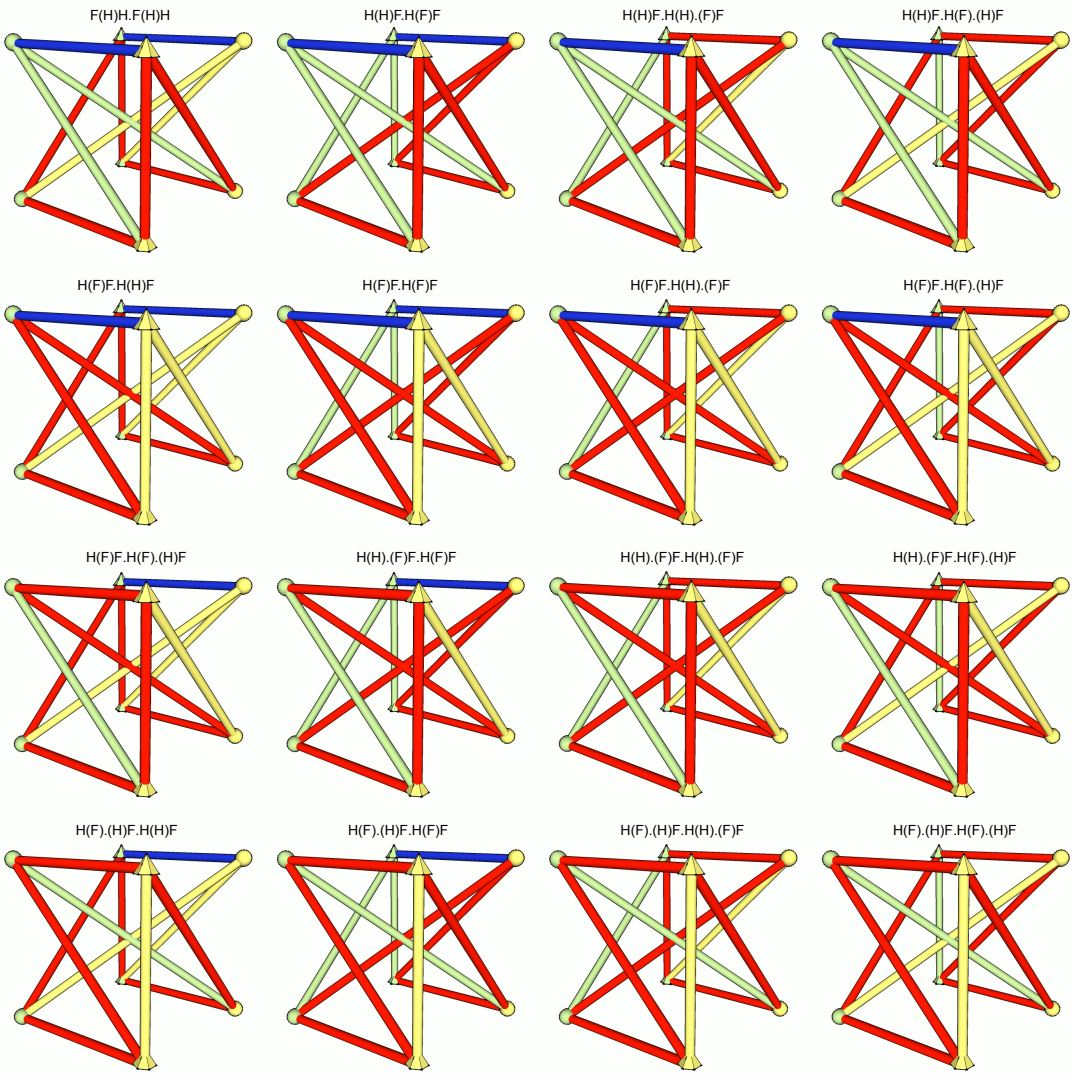


Figure 86: Sixteen circuits viewed from the perspective of a single highlighted marriage, realised by network modelling of a vertical moiety terminology.

Despite this profusion of possible relationships between each two individuals, all sixteen circuits can be reduced to a subset of 10 structurally equivalent types. To understand how this is so, we can start by observing that Figure 86 reveals all 16 circuits to be composed from a limited set of types of paths, where each type is defined by the sequence of gender and generational changes along the course of the circuit (Table 2). For this purpose, each individual tie is classified according to whether or not it links vertices of the same gender or different genders, and whether or not it is consanguineous or affinal, i.e. HH = FF; FH = HF; (F)F = F(F) = (H)H = H(H) = F(F), and; F.H = H.F. Thus, on the one hand, the circuits H()F.H()F and F()H.F()H share the same structure and represent a single circuit type. On the other hand, H(H)F.H(H)F and H(F)F.H(F)F do not share the same sequence of gender changes, and so represent distinct circuit types. The graphs illustrated at Figure 86 above can be organised into sets of such circuit type as shown at Table 2.

Table 2: Sixteen matrimonial circuits represented in the graph at Figure 86, classified according to 3 schematic structures, and 10 circuit types. Because of the minimal height of the network, the two lower generation marriages (those at G^0) form part of every circuit.

Schema: X(X)X.X(X)X	**Schema: X(X)X.X(X).(X)X**	**Schema: X(X).(X)X.X(X).(X)X**
H(H)F.H(H)F [1 instance]	H(H)F.H(H).(F)F [2 instances]	H(H).(F)F.H(H).(F)F [1 instance]
H(F)F.H(H)F [2 instances]	H(H)F.H(F).(H)F [2 instances]	H(F).(H)F.H(H).(F)F [2 instances]
H(F)F.H(F)F [1 instance]	H(F)F.H(H).(F)F [2 instances]	H(F).(H)F.H(F).(H)F [1 instance]
	H(F)F.H(F).(H)F [2 instances]	
3 circuit types/4 instances	4 circuit types/8 instances	3 circuit types/4 instances

As an ideal structure, the terminological network modelled at Figure 85 is symmetrical and self-reinforcing, with all vertices in each generation possessing the same range of relational options as each other. With more elaborate extrapolations, such as lateral extension by further marriages (yielding greater network width), and vertical extension by further descent (yielding greater network height), the vertical moiety terminology gives rise to increasingly long and complex types of cycle. The graph at Figure 85 above covers the minimal height of 1 (spanning two generations), used for demonstrative purposes, with the effect that all circuits are matrimonial, with no common ancestors for any couple, and thus no consanguineous circuits. However, extrapolating the model to height 2 (spanning three generations) yields marriages between specific kinds of consanguineous relative - *cross-cousins* - as famously identified by the standard model (see Figure 87 below). Height 2 is the minimum at which consanguineous circuits can be induced in the moiety terminology network.

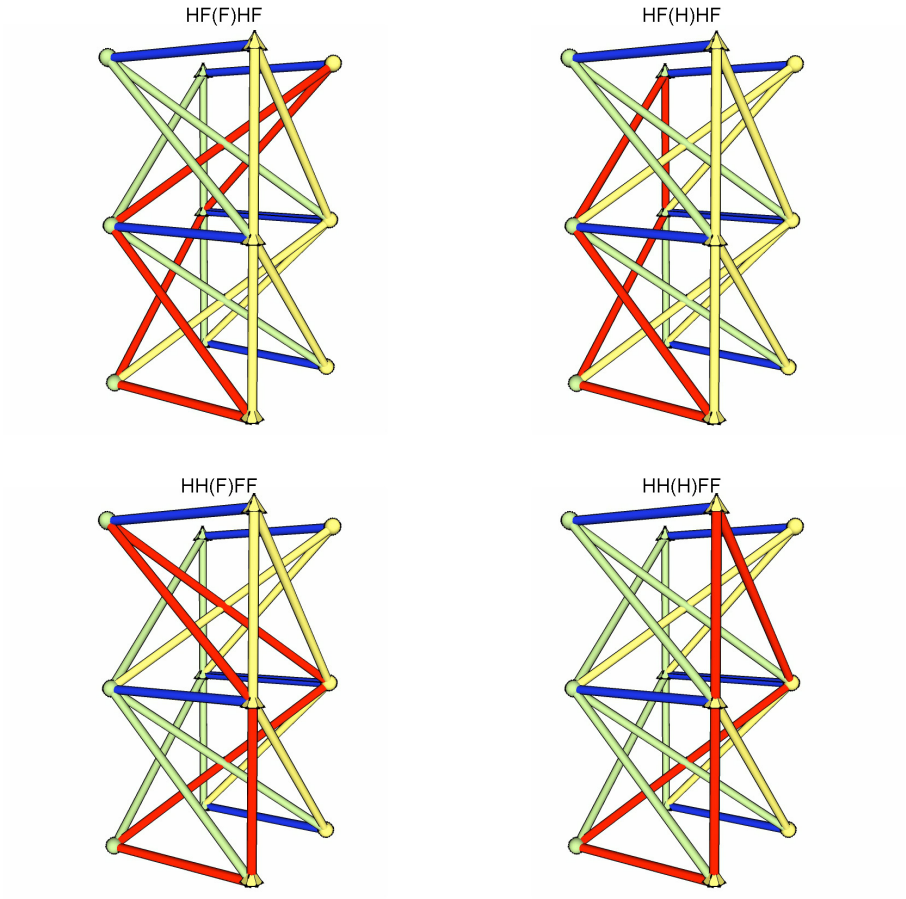


Figure 87: Shortest possible consanguineous circuits realised by a vertical moiety terminology extrapolated to height 2, showing those linking spouses at bottom left.

Table 3: Eight instances of four consanguineous circuit types present in the height 2 network model of a vertical moiety terminology graphed at Figure 89.

XX(X)XX
HH(H)FF [2 instances]
HH(F)FF [2 instances]
HF(H)HF [2 instances]
HF(F)HF [2 instances]

Although the standard model of vertical moieties construes this feature as encoding a 'cross-cousin marriage preference' where spouses share a particular grandparent in common (see Chapter 2), a kinship network construal of the standard model's account of a moiety terminology, illustrated in Figure 87, allows us to see that consanguineous relations form only a small minority (8 instantiations of 4 circuit types) of all optional circuits at the same generational height. Quite apart from two kinds of cross-cousin, and in addition to the 16 matrimonial circuits evident at height 1, even more matrimonially connected individuals are construed as 'marriageable' at height 2, rendering the so-

called 'preferential' option of cross-cousin marriage somewhat marginal in terms of overall choices. Figure 8 below illustrates a further 16 matrimonial circuits linking the same spousal couple highlighted in Figure 86 and Figure 87 above, all realised using the same supposedly 'simple' vertical moiety terminology.

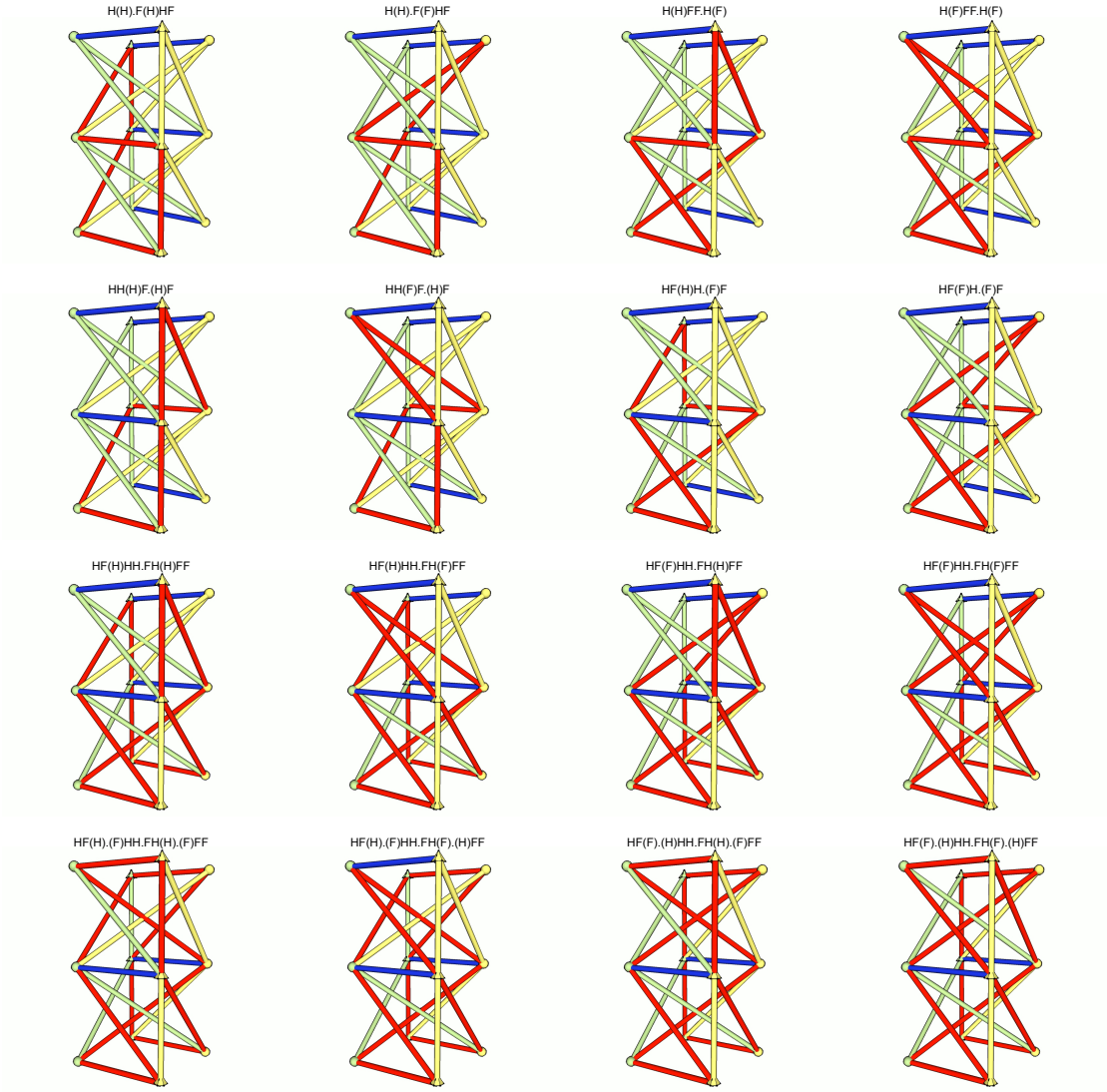


Figure 88: Sixteen matrimonial alternatives the four consanguineous cross-cousin circuits in Figure 87 linking the same two spouses using the same 'simple' vertical moiety terminology. This is in addition to the sixteen circuits illustrated in Figure 86 and Figure 87.

Table 4: Thirty-two instances of matrimonial circuits linking each pair of terms in a vertical moiety terminology at height 1. This yields 16 additional circuits linking the focus couple at bottom left, in addition to the 16 matrimonial circuits graphed at Figure 86 and classified at Table 2, and in addition to the four consanguineous circuits graphed at Figure 87 and classified at Table 3.

XX(X)X.(X)X	XX(X)XX.XX(X)XX	XX(X).(X)XX.XX(X).(X)XX

H(H).F(H)HF [2 instances]	HF(H)HH.FH(H)FF [2 instances]	HF(H).(F)HH.FH(H).(F)FF [2 instances]
H(H).F(F)HF [2 instances]	HF(H)HH.FH(F)FF [2 instances]	HF(H).(F)HH.FH(F).(H)FF [2 instances]
H(H)FF.H(F) [2 instances]	HF(F)HH.FH(H)FF[2 instances]	HF(F).(H)HH.FH(H).(F)FF [2 instances]
H(F)FF.H(F) [2 instances]	HF(F)HH.FH(F)FF [2 instances]	HF(F).(H)HH.FH(F).(H)FF [2 instances]
HH(H)F.(H)F [2 instances]		
HH(F)F.(H)F [2 instances]		
HF(H)H.(F)F [2 instances]		
HF(F)H.(F)F [2 instances]		

Extrapolating a network theoretic model of a vertical moiety terminology to height 2 (i.e. three generations) is the minimum necessary step to realise the standard model's 'cross-cousin' consanguineous marriage 'preference'. At this same height however, the terminology links the same two (theoretically) consanguineous relatives to each other by no less than 32 potential *non*-consanguineous paths. In light of this high number of non-consanguineous marriage options, the positivist deduction underlying the standard model, that the structure of a vertical moiety terminology somehow encodes 'preference' for cross-cousin consanguineous marriages, begins to appear tenuous. The weakness in this deductive reasoning is amplified as the network model of the terminology is extrapolated further, where the length, width and height of the circuits become ever greater. At these increasing scales, the number of independent matrimonial circuits that link together potential spouses, increases much faster than the number of consanguineous circuits that link the same spouses. The likelihood of marriage between two consanguineously related individuals bearing opposite moiety terms thus dwindles as the likelihood of marriage between two *matrimonially* related individuals bearing the same terms rapidly increases.

What are the consequent implications for the standard model, in which the coincidence of multiple positions in a terminology (such as 'spouse' and 'cross-cousin') is deduced as having configurational effects on the social organization of a population? So thoroughly does a network theoretical model weaken the axioms underpinning the standard model of kinship, that Hamberger *et al* (2014) provide a warning on attempting to import them into any subsequent assessment of the structures revealed by network theoretic analyses:

> *"In most cases, simply counting the occurrence of this or that connection (for example, a cousin relation) between spouses presumed to be favored or avoided by the matrimonial system is insufficient if not misleading. Far from being unambiguous indicators of social institutions, such configurations may result from a wide range of different factors: matrimonial behavior, demographic or spatial constraints, observer bias, indirect network effects, or simple chance" (Hambereger et al 2014:564).*

Perhaps one of the clearest indications of obsolescence revealed in the standard model by a network theoretic re-construal, is the further and consequent indication that there is no difference in complexity, or cohesive effects on the network, between the relationships encoded in any of the four terminology 'types' of classified by the standard model for Australian cultures. A vertical moiety terminology has been used above for demonstrative purposes because of its alleged 'simplicity', presented by the standard model as generating a form of 'direct exchange' between two 'filial groups'. As previously explained, the standard model holds that this 'minimally complex' terminology of only two terms occupies one end of a serially ranked spectrum, the opposite end of which is occupied by the 'maximally complex' subsection terminology, due to its incorporation of eight terms. A network construal of these terminologies (visualised at Figure 89 below) indicates that they realise the same kind of circuits with minimal variation[52]. Mathematical proof has been given by White (2011).

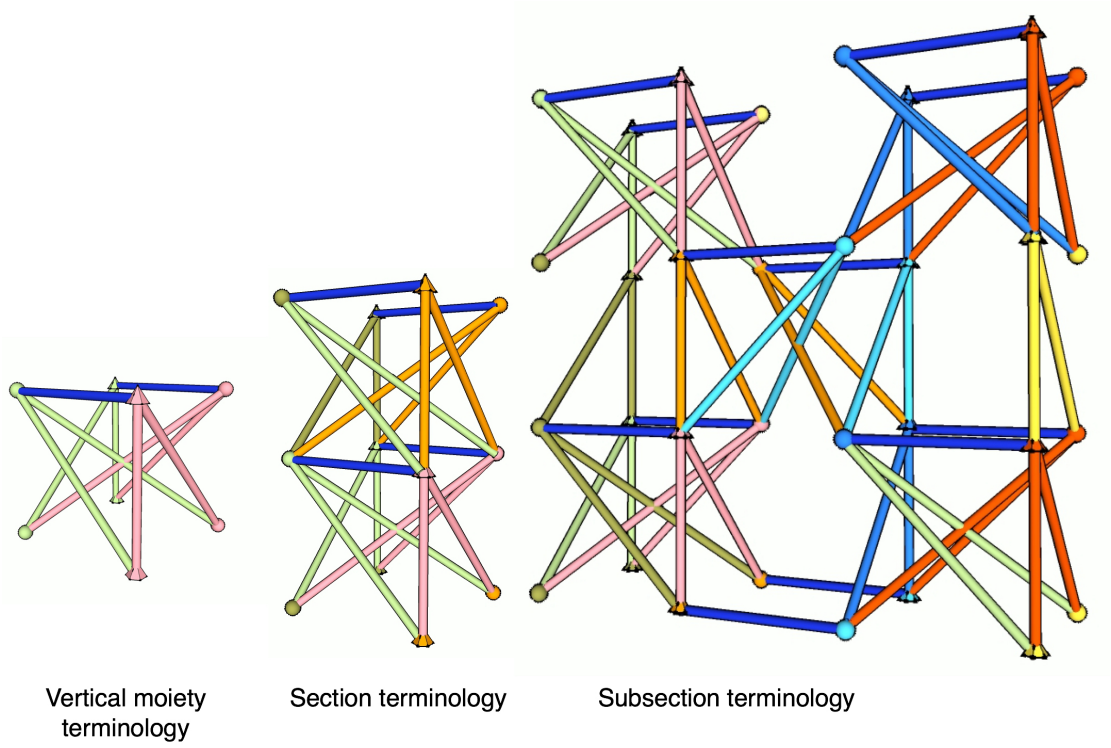


Figure 89: Section and subsection terminologies (middle and right) do not significantly alter the types of circuit structures that can be realised by reiterations of a moiety terminally (left), for linking potential spouses. A network construal indicates that variation between terminologies is limited solely to the number of terms, and is not reflected in the complexity of the functional grammar of each terminology.

Section terminologies make explicit what is implicit in vertical and horizontal moiety terminologies: Whereas a vertical moiety terminology construes affinity explicitly but filiation implicitly, and whereas a horizontal moiety terminology construes filiation explicitly but affinity implicitly, a section

[52] A subsection terminology pushes the height of consanguineous circuits out from G+2 to G+3.

terminology construes both variables explicitly. In network theoretic terms, whereas vertical moiety terminologies specify changes in width, and horizontal moieties specify changes in height, section terminologies realise *both* simultaneously (as shown in Section 4.1.2 above). However, the effect of this convergence of two kinds of explication is null, making no difference to the kinds of circuits that the three terminologies realise in the real world. The way in which individuals are embedded in an observable real-world kinship network, relinked to one another via consanguineous and matrimonial circuits by their sociocentric terminology, remains the same for all three kinds of terminology. Subsection terminologies vary only marginally in the types of consanguineous and matrimonial circuits that they realise, including all except XX(X)XX consanguineous variations, and XX(X)X.(X)X matrimonial variations.

What does this mean for the potential for sociocentric terminologies to realise cohesion in a meaningful way? Cohesion is a kind of macro-level property of social networks, which emerges when the micro-level processes of personal decision-making converge to generate circuits in these networks over time. The structures of these circuits determine the patterning and intensity of cohesion that emerges cross the network. In the network models of vertical moiety terminologies provided above, these structures are manifest in sets of circuit types that are interdependent. Variations in the frequency of instantiation of one kind of matrimonial circuit over another, realised as marriages taking place within a network over time, thus affect the intensity and structure of cohesion that emerges across the network more broadly. As circuits accumulate and intersect, more elaborate and complex forms of cohesion emerge. Sociocentric kinship terminologies encode very specific sets of interdependent circuit types that transcend variations in the number of terms in each given terminology. In short, the number of terms in each of the three kinds of terminology is irrelevant to the kinds of circuits that they encode, and thus to the kind of cohesion that emerges from their instantiation.

White (2011) goes further with specific reference to endogamy as a measure of cohesion, explaining that distinct variations in culture emerge within more cohesive regions of a network, and then feedback into that cohesion:

> *"Cohesive groups have patterned interactions that are self-reinforcing and self-stabilizing in certain spatiotemporal frames. Study of these interactions can also account for individual choices, the emergence of cohesive units as socially and cognitively recognizable entities and the consequences of these units and their changes through time for coordinated group behaviors. This kind of information may differ significantly from interview or observational accounts of individuals acting independently. The network concept of structurally cohesive subnetworks of varying intensity ... provides ways to study to what extent social groups, affected by their patterns of cohesion, come to be self-reinforcing and consequential in their effects" (White 2011:2)*

The current section has dealt with network theoretic construals of the sociocentric terminologies presented by Australian anthropology's standard model, in particular horizontal and vertical moieties, and section terms. The following section moves away from the standard model, and towards the much more detailed and specific sociocentric terminologies of the Central Murray Darling Basin population. We of the 21st century do not have the privilege of hearing the bearers of this latter culture explain their terminologies in their own terms[53]. However we *do* have the privilege of drawing on the extensive records of two of Australia's most empathic and thoughtful early anthropologists, both of whom were taught the languages of the people they interviewed, to better comprehend their explanations.

The following section uses the empirical formal terms of kinship network analysis to reintroduce hitherto neglected data on South East Australian sociocentric terminologies and their operation as multiplex theories of population reproduction, consisting of multiple distinct idea systems operating in tandem. By combining archival ethnographic data from R.H. Mathews (1894-1918), Radcliffe-Brown (1915-1924), and Tindale (1938), and extrapolating kin term data to a large encompassing network of kin documented in publically available birth, death, and marriage records, it has become possible to test the theoretical logic of the Central Murray Darling Basin terminology against real-world kinship decision-making in the South East Australian population over a period of more than a century.

[53] Sociocentric terminologies fell from widespread use among CMDB population members in the 1940s and 1950s (Elkin 1945; Reay 1945; Beckett 1958)

4.2 Network Models of CMDB Kinship Terminologies: William Ridley & R.H. Mathews

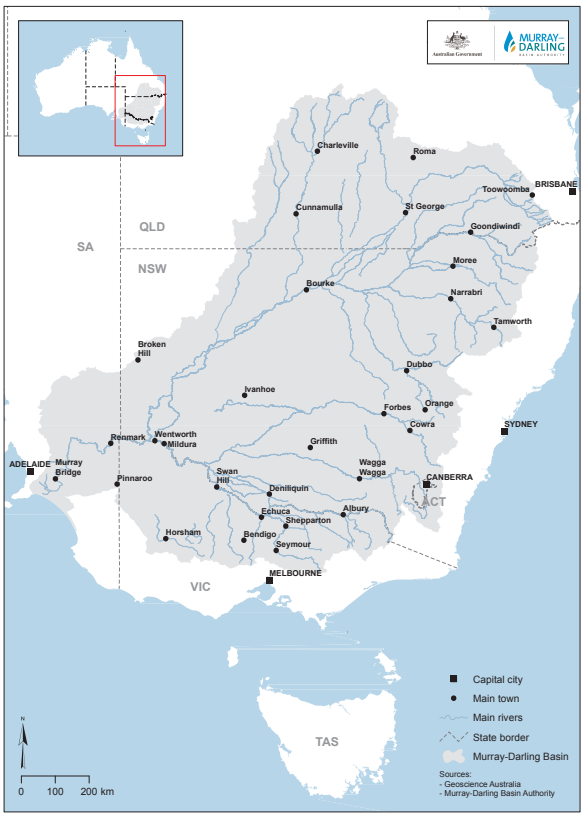


Figure 90: Map of the Murray Darling Basin region of South East Australia (©Murray Darling Basin Authority)

The network theoretic re-construal of standard model sociocentric terminologies introduced above, encodes a range of types of matrimonial and consanguineous circuits. These are distributed across distinct 'levels' of the terminology, defined by the standard model as horizontal and vertical 'moieties', 'sections', and 'subsections'. A network theoretic analysis of the supposedly 'most simple' level of these terminologies, described in Section 4.1 above, reveals that these circuits aggregate and intersect as the terminology is used to describe increasingly distant relations. At height two, where the terminology generates a consanguineous 'cross cousin' link between theoretical spouses, there are more than 47 other, non-consanguineous circuits, also realised by the terminology, that link the same two people. There are two implications for the standard model that flow from this modelling: 1) The ancillary order of elementary<complex terminologies is shown to be false, since all 4 terminology 'types' of the standard model encode the same types of circuits, and; 2) the predictable intersection of these circuits generates increasing levels of cohesion across hypothetical networks as the terminology is extended to more and more distant theoretical kin. The newly available capacity to measure this phenomenon using KNA techniques has far-reaching implications for social anthropology as a discipline. Formal empirical evidence that network cohesion is an emergent property of autochthonous theories of choice, not only recasts the parameters of kinship studies, but also points to much wider implications for theories of human sociality at large. Focusing on kinship

terminologies that are clearly defined in the anthropological literature, as the current thesis has done so far, provides a concrete means for shedding light on the structure of these implications.

The current section changes tack by modelling more specific, and less abstract kinship terminologies. Rather than the highly generalised and abstract structures of the standard model, these latter terminologies comprise the most concise and reliable accounts of the Central Murray Darling Basin kinship culture ever recorded. These accounts are contained in five published texts by two early social anthropologists specialising in Australian cultures: William Ridley (1856; 1875), and Robert Hamilton Mathews (1895; 1897; 1905). These accounts are used for three reasons: 1) They represent the most coherent application of tacit network theoretic principles in early Australian anthropological theory; 2) they are the most consistently and reliably substantiated accounts of kinship terminologies from anywhere in Australia during this period, and; 3) the instantiation of these terminologies can be mapped onto real kinship networks for the CMDB population, developed independently from a large corpus of publically available archival records (see Section 4.3). This combination of features render CMDB kinship terminologies an ideal subject, both for demonstrating the paradigmatic potential of social network analysis generally, and for exemplifying the possibility of modelling the cohesion of the entire Indigenous Australian population.

The CMDB population incorporates two different language groups, more than ten distinct languages, three distinct kinship terminologies distributed across the communities speaking those language, two distinct religions, and economies adapted to multiple ecological zones, including both dense and open forest, mountains, savannah, semi-arid desert, and permanent wetlands. This diversity belies a systemic cohesion among the population that is almost completely unrecognised in existing anthropological literature. The CMDB population formed the first object of social anthropological research in Australia, and many if not all of the major themes that came to dominate the discipline during the pre-crisis era, including kinship, language, religion, and economy, were all first applied to studies of the inhabitants of the Central Murray Darling Basin. This historical accident is also the reason that the CMDB population was subject to the first tacit instances of both group theoretic and network theoretic modelling by early anthropologists.

The Central Murray Darling Basin was the first inland region occupied by British colonists after they expanded beyond the mountains west of their beachhead at Sydney Cove in the early 19th century. The territories of CMDB population members were thus among the first to be forcibly expropriated by colonists, and the Central Murray Darling population among the first to attempt systematic articulation with the British colonial economy. For these reasons, the population was also one of the first to be subjected to intensive and ongoing colonial violence, from the 1820s through to the 1920s. The first systematic anthropological studies in Australia were made in direct response to this violence, when Wesleyan missionary William Ridley conducted two major fact-finding missions in the region between southern Queensland and central Northern New South Wales during the 1850s, in order to

publicise the pandemic human rights abuses that were occurring there. The publicity that Ridley's research received over the course of several publications (1854; 1856; 1866; 1875 *inter alia*), in turn attracted the attention of Fison and Howitt, who drew heavily (although not consistently) on Ridley's original documentation of kinship, language and religion in promoting their own theories about Indigenous sociality. Because the Central Murray Darling Basin also incorporates some of the most fertile grazing country in South East Australia, Fison and Howitt were able to draw further on correspondence with a number of educated colonists squatting in the region as a means of validating their theories. As elaborated below, Ridley can be reasonably credited both with being the true progenitor of anthropology in Australia, and with being the first researcher to apply network theoretic principles in his research. To this end, he had studied and learned to speak Gamilaraay, and recorded detailed data on kinship, language, and religion from people in the region, together with their own names and the names of their kin. This is in marked contrast with the decontextualized and depersonalised data collected from British intermediaries by Fison and Howitt. Possibly because Ridley's data did not support the group theoretic model Howitt inherited from Morgan, and partly because Ridley was more concerned with prosecuting the CMDB population's protection from frontier violence, his research unfortunately came to be eclipsed by that of Howitt.

Apart from Ridley, another early social anthropologist, R.H. Mathews, also developed the rough framework of a tacit network theoretic model based on research he conducted with members of the CMDB population over several decades. Although Mathews commenced anthropological field research in the late 1880s, after Tylor and Morgan had established themselves and their theories as the dominant force in Australian anthropology, and after Howitt had assumed the mantle of Australia's first social anthropologist, Mathews nevertheless maintained a significant research advantage comparable with that of Ridley, in being a fluent speaker of several CMDB languages, notably Wiradjuri and Gamilaraay. Unlike Fison and Howitt who were both British-born, Mathews was raised on a rural property in the CMDB region, before commencing a successful career as property surveyor in the same region. He not only grew up with Indigenous members of CMDB population employed his family's property, but also later employed members of the community on his survey team, with whom he spent months away in the bush (Thomas 2011). Mathews' expert anthropological work emerged through professional and personal friendships with Central Murray Darling Basin people. Mathews was also openly critical of Howitt, particularly his assertion that Central Murray Darling people could be further grouped into two distinct sets, one with a constrained geographic distribution, termed 'clan', and the other with a scattered distribution, termed 'horde'. Finally, Mathews was scathing of Howitt's thesis of group marriage, and accused him of gross oversimplification of Indigenous kinship terminologies in an effort to render it congruent with Morgan's ill-suited group theoretic model (1871). Both Mathews and Ridley reported on what Leaf and Read (Leaf 2007; Leaf & Read 2012) would later refer to as distinct 'idea systems', including

kinship terminologies, language, and religion, however the ways in which they did so did not conform to the homogenizing group theoretic principals laid down by Morgan (1871) and Tylor (1871).

The current section traces the gradual revelation in the work of Ridley and Mathews, of a complex 'multi-layered' terminological system in use among members of the CMDB population. It commences with the first known documentation, produced by Ridley in 1856, follows a two-stream developmental trajectory of gradual incorporation of more terms, coupled with repeated attempts to resolve inconsistencies, and ends with Mathews' death in 1918, leaving behind the most comprehensively documented instances of CMDB terminologies known to exist.

4.2.1 Ridley's 1856 and 1875 Models: Emerging Complexity

Over the course of a number of articles and books published between the 1850s and his death in 1878, William Ridley presented two versions of an evolving model of the Central Murray Darling Basin kinship terminology (Ridley 1854; 1856; 1866; 1875 *inter alia)*. Among these texts, the most comprehensive and concise of these are Ridley (1856) and Ridley (1875). The first of these two versions of Ridley's model corresponds most closely, with that adopted by Howitt in 1880, two years after Ridley's death. This was the model subsequently promoted by Howitt as the basis for his 'group marriage' and 'half-sister marriage' hypotheses, in line with Morgan's influential 6-part taxonomy (Morgan 1871 - see discussion in Chapter 3). This earlier version of Ridley's model is also the more simple and generic, later coming to form the middle tier in the four part taxa of the standard model, known as a 'section terminology'. By the 1870s, after further interviews with Central Murray Darling Basin people, Ridley realised that his preciously documented terminology was incomplete, and sought to elaborate it with 'totem terms', together with a slew of 'exceptions' to the prescriptive rules thought to give the terminology normative causal power (Ridley 1875). It was at this point that Ridley's modelling moved decisively away from the kind of 'elementary' structures that came to characterise Australian kinship throughout the 20th century, and into the realm of what Lévi-Strass would later call 'complexity' (Lévi-Strauss 1968; 1969[1949]).

Ridley's first account of Gamilaraay section terms were recorded from unnamed speakers at Warialda in 1854, published with the distinct male/female forms "*Mŭrrī*/*Mātā*", "*Kŭbbĭ*/*Kāpŭtā*", "*Ippai*/*Ippātā*", and "*Kŭmbō*/*Būtā*" (Ridley 1856). *Mŭrrī*/*Mātā* and *Kŭbbĭ*/*Kāpŭtā* terms formed one vertical ('matrimonial')) moiety while *Ippai*/*Ippātā* and *Kŭmbō*/*Būtā* formed the other. *Mŭrrī*/*Mātā* and *Ippai*/*Ippātā* formed one horizontal ('generational') moiety while *Kŭbbĭ*/*Kāpŭtā* and *Kŭmbō*/*Būtā* formed the other. Ridley documented the corresponding transmission mechanism as a classic modulo-2 circuit of alternating matrifilial inheritance. All marriages were described as endogamous in the horizontal moiety and exogamous in the vertical moiety. In the terms laid down by the standard model, this account anticipates the definition of a 'section' terminology by 24 years, as represented by Howitt (Fison & Howitt 1880). It was all the more ground breaking for demonstrating inherent mathematical principals in non-Western idea systems a full 93 years before Weil's formalizations in

Lévi-Strauss' *Elementary Structures* (Lévi-Strauss 1969[1949]), apparently validating its group theoretic basis (see Figure 92).

In one family all the sons are called "ippai," the daughters "ippātā." In a second family, all the sons are called "mŭrrī," the daughters "mātā." In a third family, all the sons are called "kŭbbĭ," the daughters "kāpŏtā." In a fourth family, all the sons are called "kŭmbō," the daughters "būtā."

I. An "ippai" may marry either an "ippātā" (of another family) or a "kāpŏtā."
II. A "mŭrrī" or "baia" may marry *only* a "būtā."
III. A "kŭbbĭ" may marry *only* an "ippātā."
IV. A "kŭmbō" may marry *only* a "mātā."

RULES OF DESCENT.

I. The children of "ippai" by "ippātā" are all "kŭmbō" and "būtā."
II. The children of "ippai" by "kāpōta" are all "mūrrĭ," or "baia," and "mātā."
III. The children of "mūrrī" or "baia" are all "ippai" and "ippātā."
IV. The children of "kūbbī" are all "kŭmbō" and "būtā."
V. The children of "kŭbbĭ" are all "kŭmbō" and "kāpŏtā."

Figure 91: First published account of Gamilaraay sociocentric kin terms, from Ridley (1856:288)

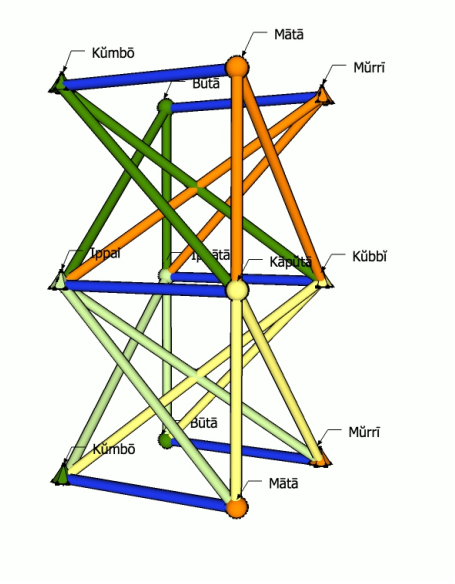


Figure 92: Contemporary graphs of Ridley's (1856) original model of Gamilaraay section term transmission. See <goo.gl/BS0i9O> for interactive 3D model.

A network theoretic construal of these relationships generates the graph shown at Figure 92. Notwithstanding the matrifilial rather than patrifilial transmission of terms, and one minor 'anomaly' in Kuhn's terms (Kuhn 2012[1962]:76), the kinship circuits realised by Ridley's model are identical to

those graphed at Figure 87 and Figure 88 in the previous section, including 4 types of consanguineous circuit and 16 types of matrimonial circuit.

The 'anomaly' in Ridley's 1856 model is contained in its first 'rule', which states that a man bearing the term *Ippai* may marry *either* a woman baring the term *Kāpŭtā or* a woman baring the term *Ippātā*, and that a woman bearing the term *Ippātā* mary marry either a man bearing the term *Kŭbbĭ* or the term *Ippai.* In effect, men and woman bearing the section terms *Ippai* and *Ippātā* had an option to marry spouses bearing their own sections. Ridley added a caveat to this 'exception', that such a marriage was permissible if the prospective spouses were not members of the 'same family', although he did not define the term 'family'. Taking the narrowest possible definition, this would mean that siblings could marry each other[55]. Graphing such a variation on a network construal of the standard model generates the image at Figure 93.

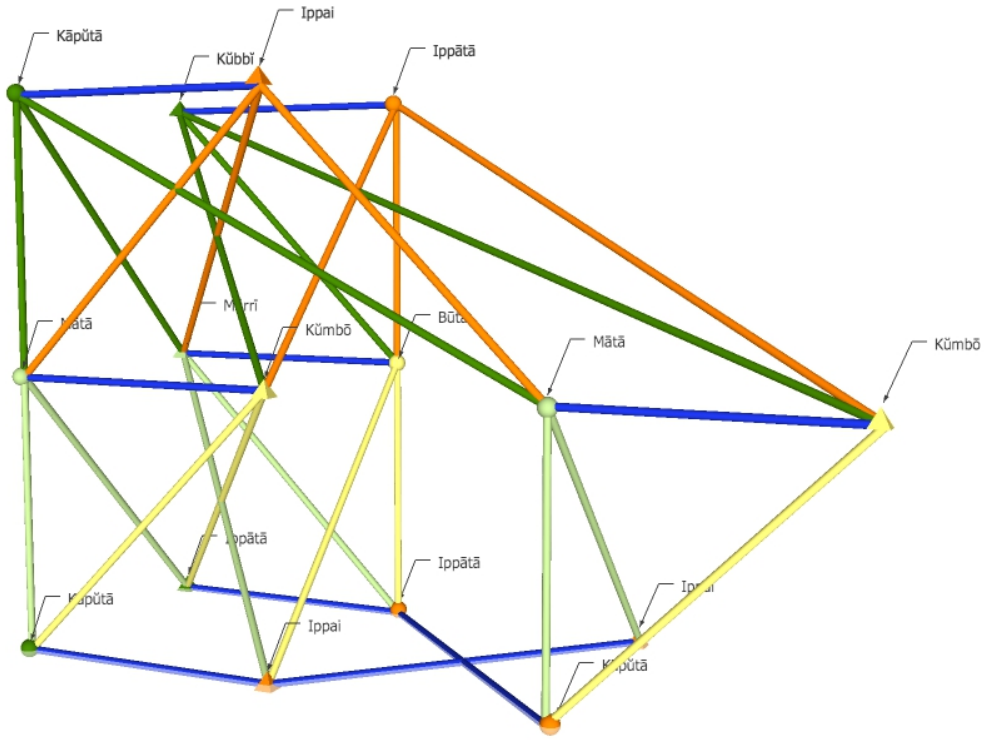


Figure 93: Ridley's (1856) model of MBD section terms, incorporating an anomalous 'exception' in which Ippai and *Ippata* terms were permitted a choice of marriage with *either Kaputa* and *Kubbi* respect, *or Ippata* and Ippai respectively. See <goo.gl/78WWME> for interactive 3D model.

The most immediate effect of this anomalous option is that the otherwise symmetrical structure of the standard model is now altered by two factors: 1) An uneven degree distribution (the number of

[55] See Moisseeff (2011) for a similar discussion of the flawed assumptions of Spencer & Gillen (1898) regarding 'marriage prohibition' for Central Australian kinship communities. In these cases the so-called 'prohibition' applied to only to co-resident bearers of a given pair of kin terms, rather than to all bearers of those terms.

ties incident to each vertex are no longer equal for all vertices), and; 2) a consequent effect that the pattern of the cohesion is now different, as the regions in which the more connected vertices are located have become more cohesive. What does *not* change is the structure of circuits giving rise to that cohesion.

The number of consanguineous circuit types conforming to structure XX(X)XX is the same, however the number of instances of each circuit type is doubled from 2 to 4. All of these retain a 'cross' pattern of gender switches. This includes the newly modelled 'endogamous' *Ippai*/*Ippata* marriages ('endogamous' only insofar as the group theoretic standard model treats marriage between individuals bearing the same section term as 'marriage within the group'). For matrimonial circuits, all instances are also doubled, from 4 to 8. The only variation is in the number of circuit types conforming to structure XX(X)X.(X)X. In Ridley's 'anomalous' model, there are half as many such circuit types (4 instead of 8), yet for each type there is double the number of instances (4 instead of 2). These are itemized in Table 4.

Table 5: Circuits types and instances encoded in Ridley's (1856) 'anomalous' model of the CMDB kinship terminology. The inclusion of a single 'optional marriage' for individuals bearing the 'Ippai' and 'Ippata' terms, doubles the number of potential circuits that bind the network together, thereby increasing its overall cohesion.

XX(X)XX	**XX(X)X.(X)X**	**XX(X)XX.XX(X)XX**	**XX(X).(X)XX.XX(X).(X)XX**
HH(H)FF [4]	H(H).F(H)HF [4]	HF(H)HH.FH(H)FF [8]	HF(H).(F)HH.FH(H).(F)FF [8]
HH(F)FF [4]	H(H).F(F)HF [4]	HF(H)HH.FH(F)FF [8]	HF(H).(F)HH.FH(F).(H)FF [8]
HF(H)HF [4]	H(H)FF.H(F) [4]	HF(F)HH.FH(H)FF [8]	HF(F).(H)HH.FH(H).(F)FF [8]
HF(F)HF [4]	H(F)FF.H(F) [4]	HF(F)HH.FH(F)FF [8]	HF(F).(H)HH.FH(F).(H)FF [8]

At height 1, illustrated in Figure 93, degree distribution is altered in two ways compared to the standard model illustrated in Figure 92. Firstly people bearing *Ippai* and *Ippata* terms at G^0 now have a minimum degree of 4 rather than 3, as they retain two parents but gain an additional marriage option. Secondly, the grandparents of the people, bearing *Ippai* and *Ippata* terms at G^2, now have a minimum degree of 4 rather than 3, as their children at G^1 diverge in the manner by which they are embedded in the rest of the network via their own children (via the marriages of their grandchildren). Cohesion thus becomes unevenly distributed as *Ippai* and *Ippata* terms take on a more central role within the terminological network (Ridley intuited this apparently 'more important' role as a kind of 'privilege' bestowed on bearers of *Ippai* and *Ippata* terms). Thus, although the theoretical network realised by the terminology becomes more cohesive as a single structure, this cohesion is concentrated around one region of the network. Although faithfully recording and publishing what he was told by culture bearers, Ridley chose to characterise the *Ippai*/*Ippata* phenomenon as an 'exception' to the terminology, and he disregarded it in much the same way as Radcliffe-Brown

advocates 'smoothing out' details in favour of generalisations in his 1940 (Radcliffe-Brown 1940:4) mischaracterisation of the scientific process (see Section 1.3.1).

A further nine years passed after Ridley's initial discovery until, in 1875, he learned of further such 'exceptions' to the erstwhile 'exogamy rule' of section terms, not only for bearers of *Ippai* and *Ippātā* section terms, but for each of the six other section terms as well (Ridley 1875). During this period Ridley was also told by Gamilaraay speakers of "another division [of the terminology], founded on the names of animals", which he referred to as 'totems' (1875:161). In the first published account of not only Gamilaraay, but any Central Murray Darling totem terms, Ridley listed six, together with corresponding section terms. This yielded a radically more complex terminology of 24 composite terms, together with optional matrimonial relations (see Figure 94).

Besides this division into four classes, there is another division, founded on the names of animals, as bundar (kangaroo), dinoŭn (emu), dūlī (iguana), nurai (black snake), mūtĕ (opossum), murriīra (padymelon), bilba (bandicoot).

In the four classes there are on the Namoi ten divisions. They are—I (1), Murri and Matha Duli, (2) M. and M. murriīra; II (3), Kumbo and Būtha Dinoun, (4) K. and B. Nurai; III (5), Ippai and Ippatha Dinoun, (6) I. and I. Nurai, (7) I. and I. Bilba; IV (8) Kubbi and Kubbotha Mute, (9) K. and K. Murriīra, (10) K. and K. Duli. (In some parts there are additional subdivisions.) Ten rules of marriage are established in relation to these divisions :—

I. Murri Duli may marry Matha Murriira, and any Butha.
II. Murri Murriira may marry Matha Duli, and any Butha.
III. Kumbo Dinoun may marry Butha Nurai, and any Matha.
IV. Kumbo Nurai may marry Butha Dinoun, and any Matha.
V. Ippai Dinoun may marry Ippatha Nurai, Kubbotha Duli, and Kubbotha Murriira.
VI. Ippai Nurai may marry Ippatha Dinoun and Kubbotha Mute.
VII. Ippai Bilba may marry Ippatha Nurai and Kubbotha Murriira.
VIII. Kubbi Mute may marry Kubbotha Duli and Ippatha Dinoun.
IX. Kubbi Murriira may marry Kubbotha Mute and Ippatha Nurai.
X. Kubbi Duli may marry Kubbotha Murriira and Ippatha Bilba.

The rules of descent are these :—

I. The second name, or the totem, of the sons and daughters is always the same as their mother's.
II. The children of a Matha are Kubbi and Kubbotha.
III. The children of a Butha are Ippai and Ippatha.
IV. The children of an Ippatha are Kumbo and Butha.
V. The children of a Kubbotha are Murri and Matha.

Thus the mother's names, not the father's, determine the names of the child in every case.

Figure 94: Ridley's (1875) account of Gamilaraay sociocentric kin terminology & transmission, comprising the first publication of CMDB totem terms in association with corresponding section terms, and so-called 'exceptions' to the extra-moiety marriage rule.

Construing these terms and relations in a network theoretic model consistent with those above, results the graph at Figure 95. Note that this is even less symmetrical than that incorporating Ridley's 'anomaly' at Figure 93.

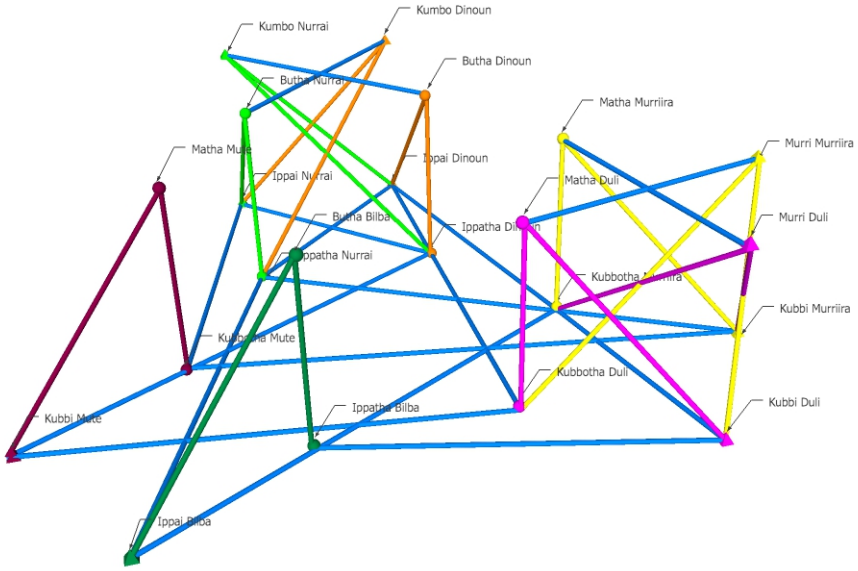


Figure 95: Ridley's (1875) model of the CMDB kinship terminology, incorporating section terms as well as 'further division' that he termed moieties. The number of terms for the CMDB terminology was now increased from 8 to 36. Vertices are colour-coded according to totem. See <goo.gl/NqYSBj> for interactive 3D model.

Although Ridley now understood that Gamilaraay people took their formal names[56] from a composite of section name + totem name (e.g. *Matha* + *Duli* (trans. '*Matha* Goanna')), he still did not comprehend the role that totem terms played in 'opening up' the marriage options available to each terminology bearer. He indicates this misunderstanding by modelling totem terms as inherently stratified, relegating their semantic potential not only to mere 'further divisions' of section terms, but as also divided among themselves according to the range of options each provided, either 'more or less privileged' (Ridley 1875 - either 'complex' or 'semi-complex' options in Lévi-Strauss' terms (Lévi-Strauss 1969[1949]) - see Chapter 2). Complex options are those that confer access to both 'exceptional' options and 'conventional' options, but also *restrict* some 'conventional' options. As an example of semi-complex marriage options in Ridley's model, *Matha Duli* women are able to choose any *Kumbo* man for a husband ('conventional'), **and/or** a *Murri Murriia* (transl. *Murri* Pademelon) man ('exceptional'). As an example of complex marriage options (c) in the model, *Ippatha Dinoun* (transl. *Ippatha* Emu) women are able to choose a *Kubbi Mute* (transl. *Kubbi* Possum) husband ('conventional'), **and/or** an *Ippai Nurrai* (transl. *Ippai* Black Snake) husband ('unconventional'), **but not** a *Kubbi Duli* or *Kubbi Murria* husband (both of which are 'conventional' in Ridley's model).

[56] Ridley listed the names of some of his Gamilaraay interviewees, and other Gamilaaay individuals mentioned by them. These people were '*Murri Bundar*' (Grey Kangaroo totem of thee Marrii section), '*Ippai Mute*', (Possum totem of the Yibaay section), '*Murri Duli*' (Goanna totem of the Marrii section), and '*Murri Ganur*' (Red Kangaroo of the Marrii section). Ridley also noted that Gamilaraay people bore unique and often quixotic nicknames (1875).

What does this new model reveal about the kinds of circuits that the terminology encodes, and about the cohesion to which the circuits could potentially give rise if realised in a kinship network? In order to answer these questions, three conditions of Ridley's latest model should be noted. The first is that the model contains two kinds of relationship definition: 1) A set of generic relationships matching those presented in his earlier models, limited to section terms alone, and without totem terms incorporated, and; 2) more specific relationships that explicitly state which section+totem composite terms can form matrimonial dyads. The second condition of Ridley's model that should be noted is that it is limited to two generations, and thus devoid of consanguineous circuits. The third noteworthy condition of the model is that it is self-evidently incomplete, lacking male instances of *Matha Mute* and *Matha Bilba* composite terms, which can be anticipated as *Murri Mute* and *Murri Bilba*. These three conditions affect the way in which the model can be construed in network theoretic terms. Addressing the three conditions in order: Firstly, a much clearer construal can be achieved by excluding the generic section-only terms and relations modelled in Ridley's earlier texts. The effect of including these relations is to add only relational 'noise' to the new data, and doing so provides no new insights. By focusing exclusively on the newly documented composite terms and relations, it becomes possible to see whether they might cause any effect on reticulation and cohesion in a hypothetical kinship network. Secondly, Ridley's model is limited to two generations, meaning that it encodes only shallow matrimonial circuits ('direct exchange' in Lévi-Strauss' (1969[1949]) terms). It is still worth modelling in this limited state because, as will be shown below, it dramatically increases the matrimonial width of these shallow circuits, along with degree distributions, and most significantly, cohesion, relative to those realised by the generic section-only terminology described in Section 4.1.3 above.

Table 6 itemises the matrimonial circuits composing Ridley's 1875 model as presented, with composite terms only, at height 1, width 2, with *Murri Mute* and *Murri Bilba* terms missing. This census reveals exactly the same variety of circuit types, and exactly the same number of instances of these circuits, as those composing the earlier generic model built by Ridley (1856), and as found in the standard model for all Australian kinship terminologies (see Section 4.1.3). This is important insofar as it reinforces Ridley's intuited conclusion, that totem terms represent mere 'further divisions' of the section terminology, and do not alter the structures to which it gives rise. The one hint that this might not be the case, is that all of the these circuits are limited to one region of the terminological network occupied by bearers of *Nurrai* and *Dinoun* totems, whose intermarriage matches that outlined by Ridlet's 1856 model.

Table 6: Standard model matrimonial circuits realised by Ridley's (1875) model of 24 composite terms. Cf. Table 2. Note that that there is exactly the same variety of circuit types and instances. What changes is the width (see Table 7 below).

Schema: X(X)X.X(X)X	**Schema: X(X)X.X(X).(X)X**	**Schema: X(X).(X)X.X(X).(X)X**

H(H)F.H(H)F [1 instance]	H(H)F.H(H).(F)F [2 instances]	H(H).(F)F.H(H).(F)F [1 instance]
H(F)F.H(H)F [2 instances]	H(H)F.H(F).(H)F [2 instances]	H(F).(H)F.H(H).(F)F [2 instances]
H(F)F.H(F)F [1 instance]	H(F)F.H(H).(F)F [2 instances]	H(F).(H)F.H(F).(H)F [1 instance]
	H(F)F.H(F).(H)F [2 instances]	

Ridley's intuited conclusions fall apart when the circuit census on the same model is extended beyond width 2. The fact that Ridley's model documents 6 totem terms tells us that there will be 6 consanguineous components in a network construal of the model. The graph at Figure 95 shows further, at first glance, that these consanguineous components are all linked together to form a single matrimonial component, i.e. a single connected network. What is not clear however, is the *extent* to which these consanguineous components are linked, i.e. how they cohere. Table 7 itemises the variation in circuit types, together with their occurrence, at widths 3, 4, 5 and 6. These have been limited to the simplest schema of marriage between consanguineous arches <X(X)X> for the sake of brevity, but further variations including marriage between matrimonial subcomponents <X(X).(X)X>, and between both can be assumed.

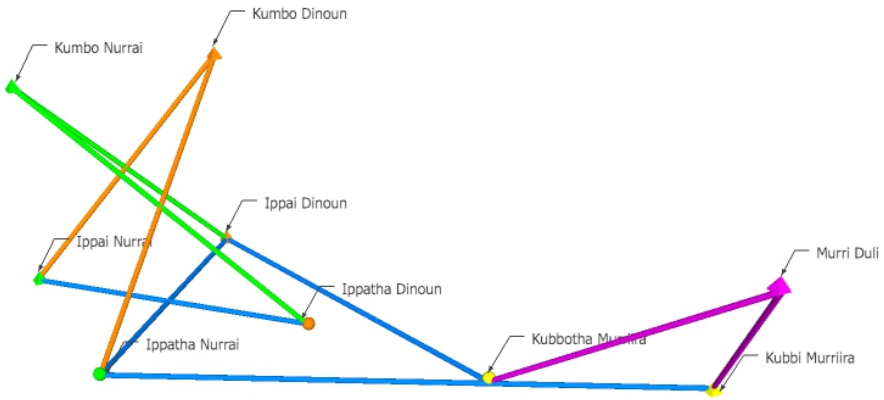


Figure 96: One of 20 circuits of width 3 realised by Ridley's (1875) composite term terminology model (Ippai Nurrai - Ippatha Nurai = Kubbi Murriira - Kubbotha Murriira = Ippai Dinoun - Ippatha Dinoun). See <goo.gl/ZV86rV> for interactive 3D model.

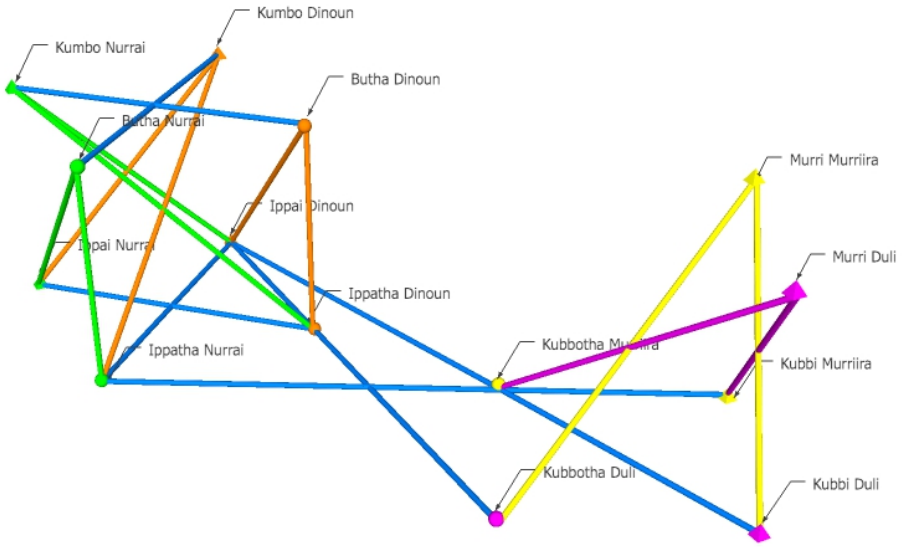


Figure 97: One of 56 circuits of width 4 realised by Ridley's (1875) composite term terminology model (Kubbi Murriira - Kubbotha Murriira = Kubbi Duli - Kubbotha Duli = Ippai Dinoun - Ippatha Dinoun = Ippai Nurrai - Ippatha Nurai). See <goo.gl/r1quv7> for interactive 3D model.

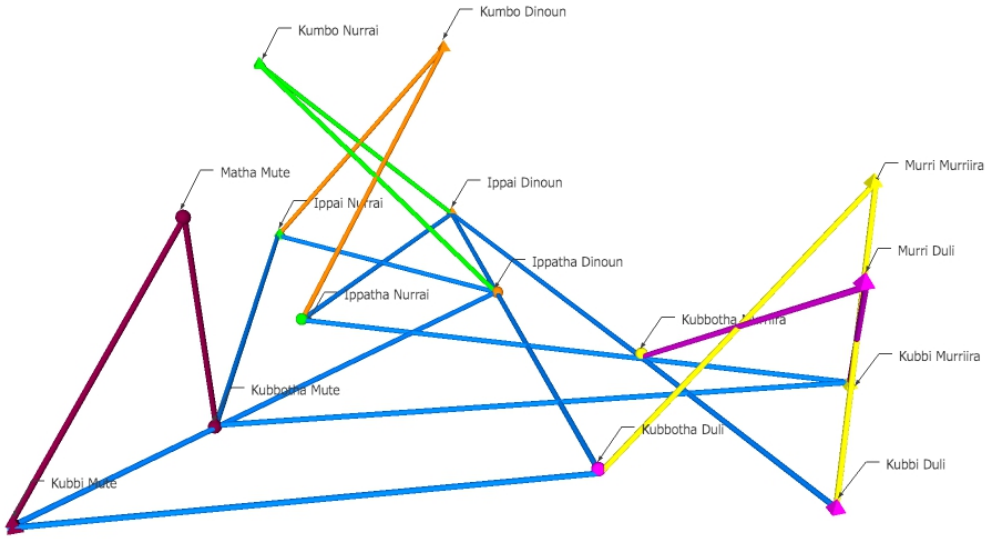


Figure 98: One of 56 circuits of width 5 realised by Ridley's (1875) composite term terminology model (Kubbi Mute - Kubbotha Mute = Ippai Nurrai - Ippatha Nurai = Kubbi Murriira - Kubbotha Murriira = Kubbi Duli - Kubbotha Duli = Ippai Dinoun - Ippatha Dinoun). See <goo.gl/8tq86q> for interactive 3D model

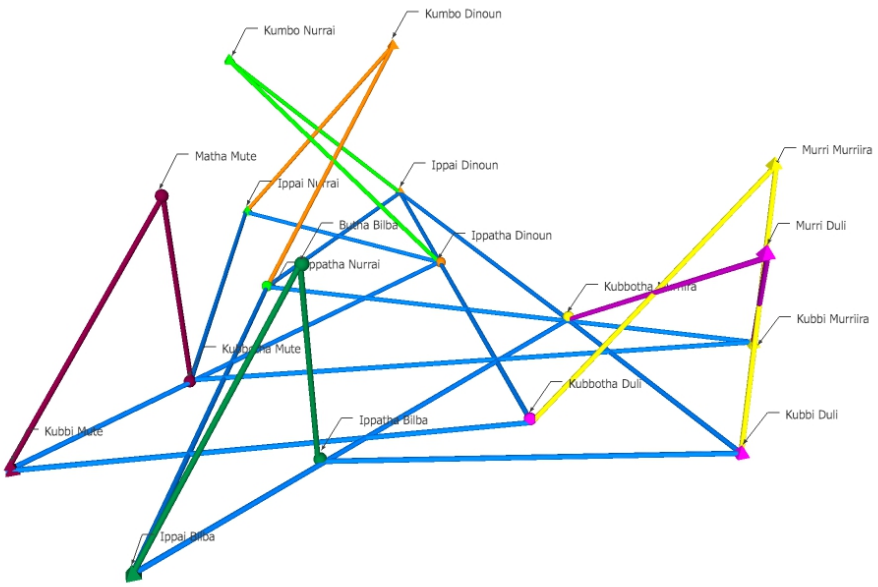


Figure 99: One of 32 circuits of width 6 realised by Ridley's (1875) composite term terminology model (Ippai Bilba - Ippatha Bilba = Kubbi Duli - Kubbotha Duli = Ippai Dinoun - Ippatha Dinoun = Kubbi Mute - Kubbotha Mute = Ippai Nurrai - Ippatha Nurai = Kubbi Murriira - Kubbotha Murriira). See <goo.gl/iwoFy8> for interactive 3D model.

There are several processes going on here, which need specification. Firstly, as circuits grow wider in the terminological network (i.e. incorporating more consanguineous components), they incorporate more terms. In Ridley's 1856 model, which is identical to the standard model's so-called 'section terminology', the width of circuits is limited to 2, and all circuits are of a consanguineous 'cross-cousin' type. However because the average degree distribution for each term in Ridley's 1875 model is significantly higher than in his 1856 model (3.36 instead of 3), because the terminology contains many more terms (22 instead of 16), and because the width of the terminology is so much greater (6 instead of 2), the variety of possible circuit types increases faster as the width of circuits increases, than it would had the vertices all borne the lower degree distribution of the standard model. 'Cross-cousin' marriages thus shrink to a small minority of all possible circuits, even at the same shallow height of 2 generations (height 1) of Ridley's model. This is partly indicated by the number of circuits that are realised at width 2 (see Table 6 above). At width 2, the variety of possible circuits in Ridley's 1875 model is identical to that possible in his 1856 model, and these are all limited to those 8 composite terms that bear the same possible varieties of intermarriage as those encoded in the 1856 model. Beyond width 2 however, as circuits extend beyond two sets of totems to include a third, the variety of possible circuits and the number of possible instantiations of those circuit types increases dramatically. Such increased widths cannot be realised by the standard model without multiplying the same set of 16 terms. At width 3, there are 20 possible instances, at widths 4 and 5, 56 possible instances, while even at width 6, where every consanguineous component is marked by a common totem, there are still 32 possible instances. The overall effect is that the increasing width of matrimonial circuits, extending to more and more totemic components, gives rise to higher and higher levels of cohesion across the network, together with *a more even distribution of cohesion*. In

short, Ridley's new, more specific, and more extensive 1875 model completely undermines the emphasis placed on consanguineous 'cross-cousin' marriage in his 1856 model, along with the standard model to which it gave rise. Ridley's neglected 1875 model shows that even at shallow heights of two generations, there are 164 possible ways that 6 totemic components can form redundant matrimonial circuits in the Central Murray Darling Basin population.

This raises a clear question about the importance of filiation in kinship cohesion. As noted above, there are no consanguineous circuits of any kind in Ridley's 1875 model. In order to induce consanguineous circuits, the model must be extrapolated vertically over an additional 2 generations, to height 3 (see Figure 100 below). This increases the size of the model from 22 to 72 terms, doubles the number of matrimonial ties, and increases the overall complexity of the model. Even at this scale however, only 6 consanguineous circuits can be induced (see Figure 101). These 6 circuits all conform to a shallow XX(X)XX 'cross-cousin' pattern identical to those present in both Ridley's 1856 model, and the standard model.

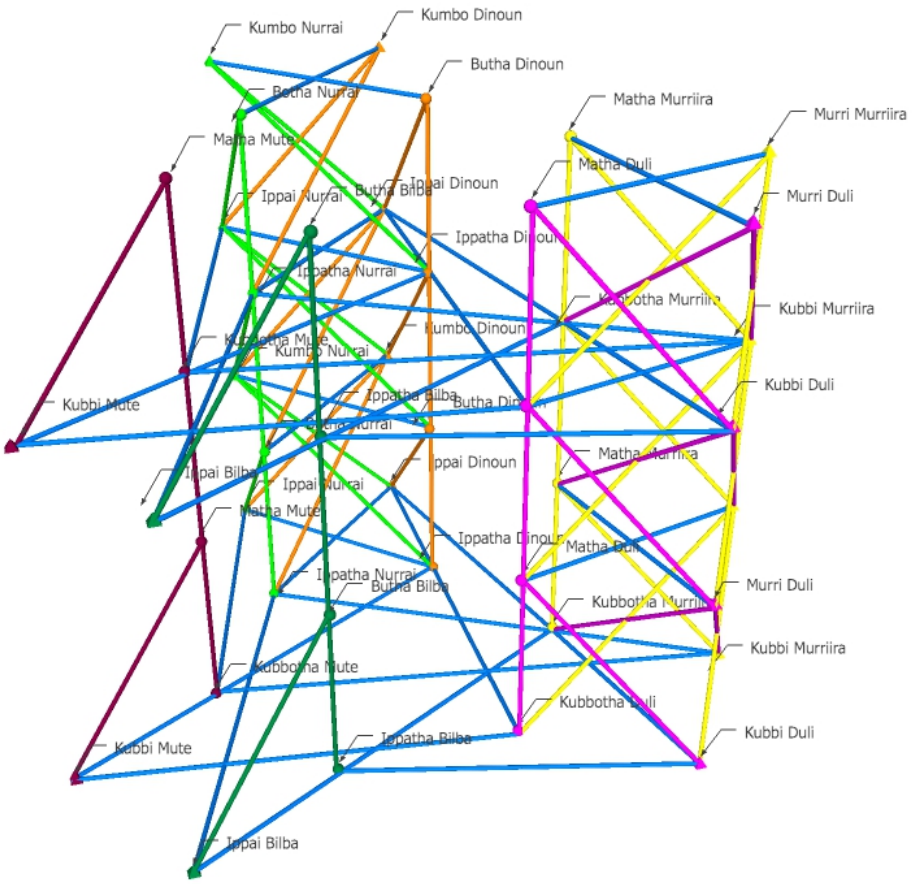


Figure 100: Network theoretic construal of Ridley's (1875) model, extrapolated to height 3 to induce consanguineous circuits. See <goo.gl/gZHSSv> for interactive 3D model.

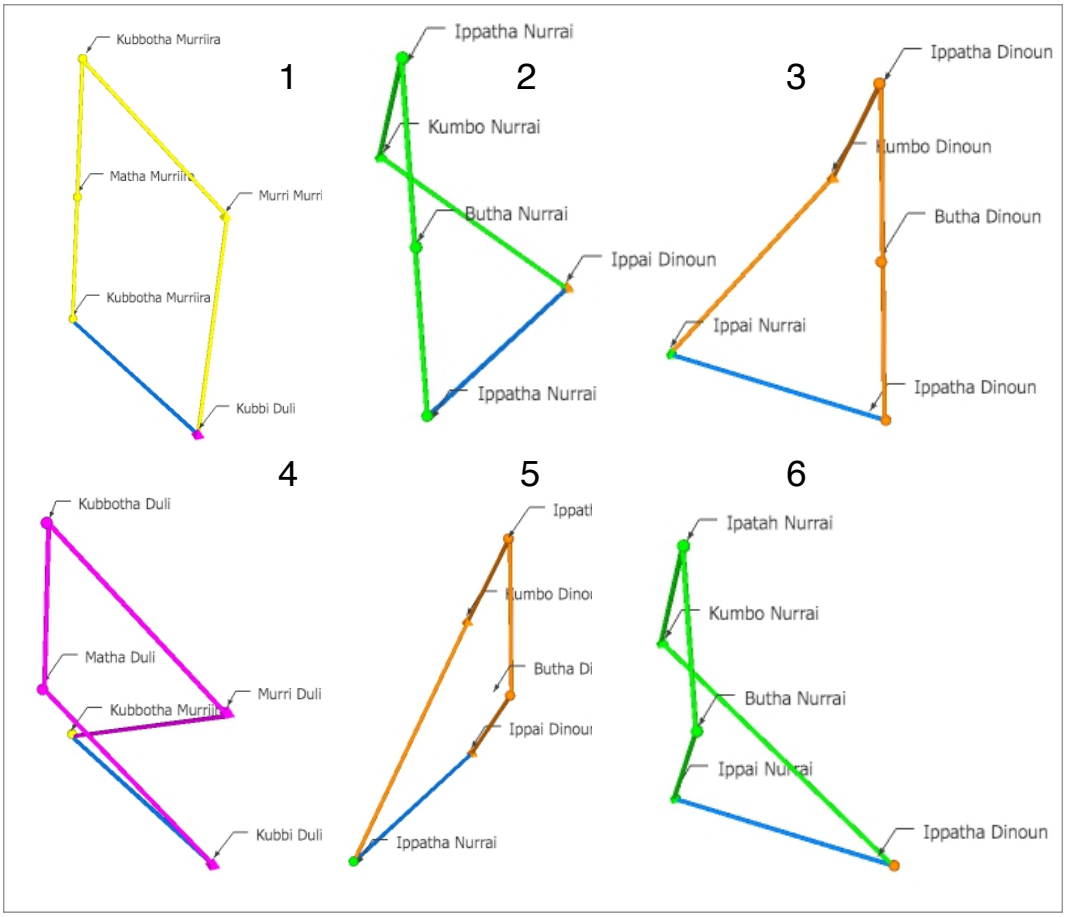


Figure 101: All six shortest possible consanguineous circuits induced in Ridley's (1875) composite model. They only occur in an extrapolation of the documented terminology, extended to height 3, even though the circuits themselves are all height 2.

This duplication of results would, at first glance, appear to support the importance of the XX(X)XX 'cross-cousin' structure as 'an ideal marriage type', albeit unlikely to occur in the real world, given its statistical insignificance in the both Ridley's 1856 and 1875 models. The more specific effect of composite terms in Ridley's 1875 model however, reveals a complication in that, for each pair of spousal terms, at least one parent term for each spouse is the 'sibling term' of the other. In other words, for all 6 consanguineous marriages induced from Ridley's 1875 model, all are marrying a spouse whose father has the same totem as their own mother, and therefore themselves. Figure 101 shows this clearly in the consistent colouring of the filial relations linking spouses together. This contradicts the condition clearly stated by Ridley (1856) that spouses could not be members of the 'same family'. The possibility of inducing consanguineous circuits that do not exhibit this contradiction, by further vertical extrapolation of the terminology, is a moot point. If the shortest possible circuits exhibit this contradiction on the basis of the 'anomalous' conditions presented by Ridley, then there is an inherent flaw in the model that should not be 'smoothed out' by theoretical speculation (Radcliffe-Brown 1940 - see Section 1.3.1). The ultimate effects are twofold: 1) The theoretical consanguineous circuits that can be induced by extrapolating Ridley's height 1 model comprise less than 10% of all possible matrimonial circuits in the model, and; 2) this small number of

consanguineous circuits breaches Ridley's clearly stated condition that individuals do not marry within the 'same family' (because it generates a 'father-in-law' bearing the same totem as 'mother').

The further contribution of Ridley's 1875 model to understanding CMDB kinship in network theoretic terms, is the range of circuit *types* that it encodes without actually naming them. In the height 4 extrapolation of the model, shown above at Figure 100, there are three circuit types in particular, that are worth considering, both because of the significant number of couples that they incorporate, and because of their role in real-world kinship cohesion (discussed in Section 4.3 below). Two of these are width 2, while one is width 3.

Table 7: Occurrence of three circuit types in Ridley's (1875) sociocentric CMDB terminology model that do not occur in the standard model.

Schema: X(X)X.XX(X)XX	**Schema: X(X)X.X(X)X. X(X)X**	**Schema: X(X)X.X(X)X. XX(X)XX**
23% of all couples	53% of all couples	29% of all couples

The current section has presented William Ridley as a hitherto unrecognised pioneer of Australian anthropology, not only for being the first to model sociocentric terminologies by more than a quarter of a century, but more importantly for having presented a model more complex and congruent with that of Central Murray Darling Basin people than any other to be presented for at least another 40 years. Fison & Howitt (1880) appropriated Ridley's preliminary 1856 model for their 1880 monograph 'Kamilaroi & Kurnai', but ignored his 1875 model and neglected to make any mention of totems, let alone role of totems in forming a much more elaborate and complex composite terminology. Ridley's research was thus marginalised from the emerging mainstream of social anthropology, and his reputation consequently dwindled from the annals of Australian anthropological history, as it was swept up in the group theoretic Social Darwinism of Lewis Henry Morgan and his Australian heirs. Only one other early anthropologist was able to replicate Ridley's findings in interviews with Central Murray Darling Basin people: R.H. Mathews. The following section illustrates not only Mathews' corroboration of Ridley's research, but also his extension and elaboration upon it.

4.2.2 Mathews' 1895, 1897, and 1905 Models: Flowering Complexity

Beginning in the 1890s, Mathews extended Ridley's detailed observations of the CMDB terminology to include a raft of new terms, including not just section and totem terms, but initially vertical moiety terms (Mathews 1895; 1897) and later, two types of term not previously recorded anywhere in Australia, which Mathews named 'Bloods' and 'Shades' (Mathews 1905). Mathew's models maintained the formalism establish by Ridley but reached new levels of specificity by including many more *instances* of term types and compositions. This advance on Mathews' part facilitates the construction of more diverse and complex network models.

In 1895 Mathews published the second-ever account of composite CMDB sociocentric terminologies (Mathews 1895). This time he included the same section terms originally reported by Ridley in 1856, as well as totem terms as Ridley had done in 1875 (although three times as many as Ridley - 18 rather than 6). Finally, reaching a new level of complexity in his account, Mathews also reported the names of two vertical moieties that he was told about by Gamilaraay and Ngiyampaa speakers: '*Dilbi'* and *'Kupathin'*. Unlike Ridley, who presented the composite terminology in the form of a list of matrimonial prescriptions for each composite term, Mathews presented his data in the form of a table, ranked from 'most inclusive' terms or 'classes', to 'most specific' terms or "totem names" (see Table 8 below).

Table 8: The second ever published account of CMDB totem terms (Mathews' 1895), 20 years after Ridley's (1875) article, modelled as tertiary subset elements of matrifilial section terms or 'sub-classes', the latter in turn comprising secondary subset elements of vertical moiety terms or 'classes'.

Classes	Sub-Classes	Totem Names
Dilbi	*Murri /Matha Kubbi/ Kubbitha*	*Kangaroo, opossum, bandicoot, padamelon, iguana, black duck, eagle-hawk, scrub-turkey, yellow-fish, bony-fish, bream, etc.*
Kupathin	*Ippai/Ippatha Kumbo/Butha*	*Emu, carpet-snake, black snake, red kangaroo, wallaroo, frog, codfish, and others*

Mathews' 1895 article is therefore important for contemporary understandings of the CMDB terminology for two reasons: Firstly it extends the range of items within the terminology from 2 strata consisting of 8 sections and 6 totems respectively, to 3 strata consisting of 2 vertical moieties, 8 sections and 18 totems respectively. Secondly, it contains the first published report, albeit inconsistently phrased, that patrifiliation plays a major role in the construal of marriage options, as illustrated below (Mathews 1895:23).

In addition to this newly elaborated and more complex account of the terminology, Mathews also introduced a prohibition on individuals marrying spouses whose fathers bear the same totems as themselves. This effectively negates all of the height 2 'cross-cousin' consanguineous circuits detected in Ridley's 1875 model. Mathews states:

> *"The laws of marriage and descent are also governed by the totemic regulations which still further restrict a man in his choice of a wife; the rule being that marriage between persons of the same totem is not permitted.*
>
> *"There are also social laws which prevent a man marrying a woman who is held to be too near in blood to admit of a lawful union with her. For example, a man would not be permitted to marry the daughter of his mother's own brother, although she belongs to the group of women who, according to the class [i.e. 'section'] law, are his potential wives. He must marry a*

> *daughter of one of his mother's tribal brothers [i.e. bearing the same section name as his MB but not the same totem]. Another obstacle to marriage arises out of local proximity by birth. In some tribes, a man cannot marry a woman of the same sub-tribe [Mathews provides no definition for this term, but 'local proximity' strongly suggests geographic proximity], although she may be of the proper sub-class [section term] for him to marry" (Mathews 1895:23).*

For example, a *Kupathin* woman would have the option of marrying a man bearing one of only 10 of the 11 totems in the *Dilbi* moiety, because one of those totems would belong to that woman's father. Of these 10 options, she might choose any one, however any children she may have by that marriage would have access to the same set of options as the *Kupathin* woman's own mother, rather than the options that she had access to. Unfortunately, although it is fairly clear that a horizontal moiety terminology realise this semi-complex combination of pre- and proscriptions by 'merging' an individual with their grandmother under the same section term, Mathews provides no such account in 1895. We can, nevertheless, build a network model based on the definitions Mathews provides. These two features of the model give rise to a second, emergent distinction: Mathews' 1895 model significantly increases the variety and number of circuits that can be realised in real-world kinship networks (see below).

Despite maintaining a model that implied a hierarchy of successively more specific 'divisions', Mathews appears to have struck upon an early relational form of modelling the associations between various strata of terms. Rather than treating these more specific strata as 'subsets' of more general terms, which had no ideational function beyond the regulation of 'group marriage' (Cf. Fison & Howitt 1880), Mathews appears to have seen each strata of terms as conferring distinct *types* of attribute to their bearers. Furthermore, within each type of attribute, each single attribute conferred distinct sets of options with regard to marriage. In this sense, the 'exceptions' to the so-called 'moiety exogamy rule' conferred by certain totem terms were not unusual exceptions to a prevailing rule, but rather formed the basis for a single complex of interrelated choices available to each individual bearer. This perspective appears to have dawned upon Mathews slowly over 10 years between 1895 and 1905, and its development can be traced out over three key articles in 1895, 1897 and 1905 (Mathews 1895; 1897; 1905).

The difference between Mathew's 1895 model and Ridley's 1875 model is that Mathews suggests that the fundamental role of totem terms superseded the role of section terms as the primary factor in individual choices of marriage partner:

> *"The Rev. W. Ridley ... says that among the Namoi river [Gamilaraay speaking] tribe, Murri Iguana. could marry either a Butha - the woman whom the class laws permitted him to marry - or a Matha of a totem different to his own. The same privilege was extended, mutatis mtutandis, to the men of the other sub-classes".*

...

"That the totems influence marriage and descent is well known, but the manner in which it is done has not hitherto been shown, because this branch of the subject has not received sufficient attention.

"The work which is most important and necessary to be done at the present time, is to accurately trace out and formulate the details of the laws which rule the intermarriage of the totems and the distribution of these totems under the four classes" (Mathews 1895:24-27 - emphasis added).

Mathews thus commenced to focus on the transmission of, and marriage options conferred by, totem terms, treating moiety and section terms as associated with totem terms, but as fundamentally separate types of term, with regard to the social relations that they encoded in a meaningful way. Table 9 is a direct copy of Mathew's 1895 list of the 18 totem terms associated with each vertical moiety. Although in this article Mathews did not describe the 'influence' that certain totems exerted on marriage and descent, he did show for the first time that marriage was not simply between 'sections' of people, but between people of specific totems.

Table 9: Mathews' (1895) list of Gamilaraay totem terms, comprising 18 terms divided unequally between *Dilbi* and *Kupithin* vertical moieties.

Dilbi Moiety	**Kupathin Moiety**
Kangaroo [poss. Macropus giganteus]	Emu [Dromaius novaehollandiae]
Opossum [Trichosurus vulpecula]	Carpet-Snake [Morelia spilota]
Bandicoot [Perameles nasuta]	Black Snake [poss. Pseudechis guttatus]
Padamelon [poss. Thylogale stigmatica]	Red Kangaroo [Macropus rufus]
Iguana [Goanna - Varanus varius]	Wallaroo [Macropus robustus]
Black Duck [Anas superciliosa]	Frog [poss Limnodynastes fletcheri]
Eagle-Hawk [poss. Aquila audax]	Codfish [Maccullochella peelii]
Scrub-Turkey [Alectura lathami]	
Yellow-Fish [Macquaria ambigua]	
Bony-Fish [aka Bony Bream - Nematalosa erebi]	
Bream [Bidyanus bidyanus]	

Mathews' report of a prohibition on height-2 consanguineous circuits complicates a network theoretic construal of his data in the mannerr used to model Ridley's data. The lack of such a proscription in Ridley's data allows all optional marriages to be modelled simultaneously, as they are in Figure 95. As shown in Figure 101, simultaneous modelling of all optional marriages allows for the induction of consanguineous circuits when the model is extrapolated to height 3, revealing height-2 circuits where spouses' parents bear the same totem. Excluding such circuits, as required by Mathews' 1895 proscription, makes it impossible to induce consanguineous circuits at such a shallow height. Phrased another way, Mathew's 1895 model does not realise consanguineous circuits, either cross or parallel, at height-2,. The only way to re-construe Mathews' semi-complex model in network

theoretic terms is to extrapolate the height of the model on a totem-by-totem basis, until all possible marriages have been mapped. When applied to all terms simultaneously, this reiterative procedure generates phenomenal complexity because of the condition that each totem term may be linked to multiple paternal totem terms. For a *Grey Kangaroo* term for example, the paternal term of which might be any one of 7 other terms, there are 6 potential spousal terms, each of which has 10 potential paternal terms of their own (i.e. any one of the 11 in their own *Kupathin* moiety, excluding their own - see Figure 102). Mathews indicates that he was aware of this emergent complexity as a feature of the terminology, having attempted to roughly sketch the micro-level processes in network theoretic terms in one of his notebooks (Mathews 1903 - see Figure 102).

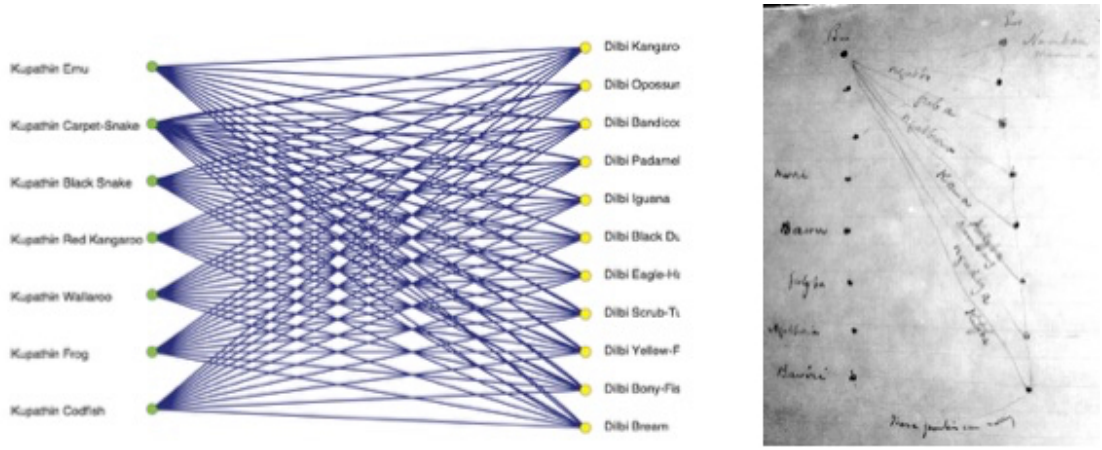


Figure 102: A contemporary graph of Mathews' (1895) model of Gamilaraay inter-totemic, extra-moiety marriages, following Ridley's 1875 model, shown at left. No such graph was ever published by Mathews, although a sketch is contained in his unpublished field notes, shown at right (Mathews 1903). This is the earliest known relational model of an Australian sociocentric kinship terminology, sketched by Mathews during interviews with Dharawal speakers on the South East Coast in 1903. Each vertex represents a totem while each diagonal line represents a marriage. The totems are aligned along two vertical axes representing ideally exogamous moieties (Mathews 1903)[58]

The impracticality of building a complete network model of Mathews' terminology - which would need to be at least 22 generations in height in order to accommodate all possible spousal combinations - means that its utility to the current discussion is limited. Figure 103 provides a graphic indication, limited to height 3, of what such a model might look like[59]. The striking complexity of the

[58] Despite this laboriously unintuitive published model of relational properties, there are indications that Mathews had a much more subtle and sophisticated understanding of the structural complexity that could be generated by these 'exceptional marriages'. On the last page of a 1903 field journal containing notes from interviews with Dharawal speakers, Mathews has sketched out a relational diagram of allowable marriages between subsets of totems belonging to each of the Dharawal moieties.

[59] This is a visual indication only.

structure, compared with that of Ridley's 1875 model at Figure 100, points to the very high number of circuits that bind it into a single cohesive network.

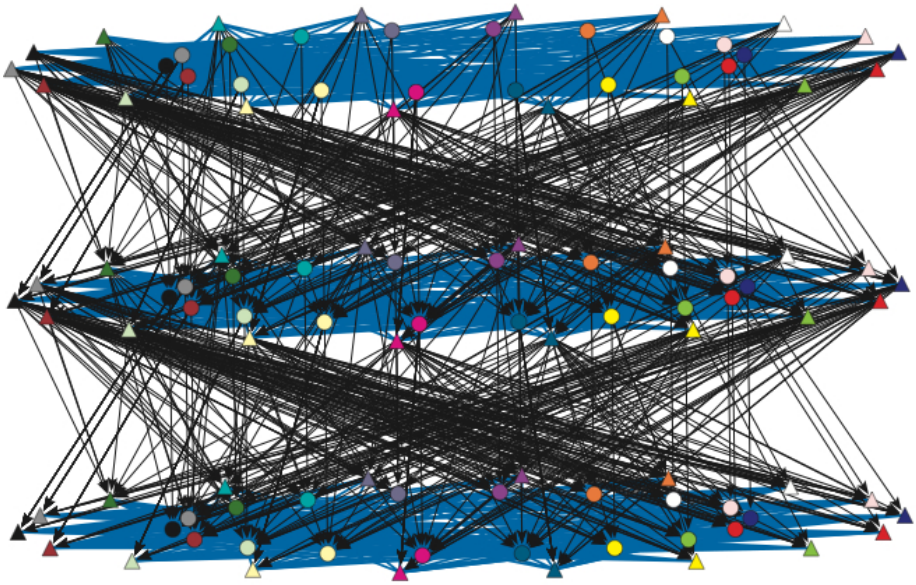


Figure 103: Contemporary graph of Mathews' 1895 model, extrapolated to a generational height of 2 in order to detect consanguineous circuits, all of which are close cross-cousin exchanges. Vertices are colour-coded according to totem. Blue edges indicate marriage, black arcs descent.

Despite the insurmountable complexity of Mathews' 1895 model, we can nevertheless illustrate the operation of its generative processes at the more limited scale of subgraphs *within* the whole terminology. Figure 104 illustrates matrimonial relinkage between two consanguineous totemic components, *Kangaroo* and *Black Snake*, under the semi-complex conditions given by Mathews in 1895. Figure 105 highlights the decomposition of this subgraph into its two shortest possible consanguineous circuits of height 3. Note that unlike the shortest possible consanguineous circuits in Ridley's 1875 model, Mathew's 1895 model implies an interdependence between totemic components where both consanguineous circuits include bearers of both totems, and closure of the circuits is impossible among bearers of the a single totem. The significance of this terminological phenomenon for the real world kinship cohesion of the Central Murray Darling Basin population is profound, as elaborated below.

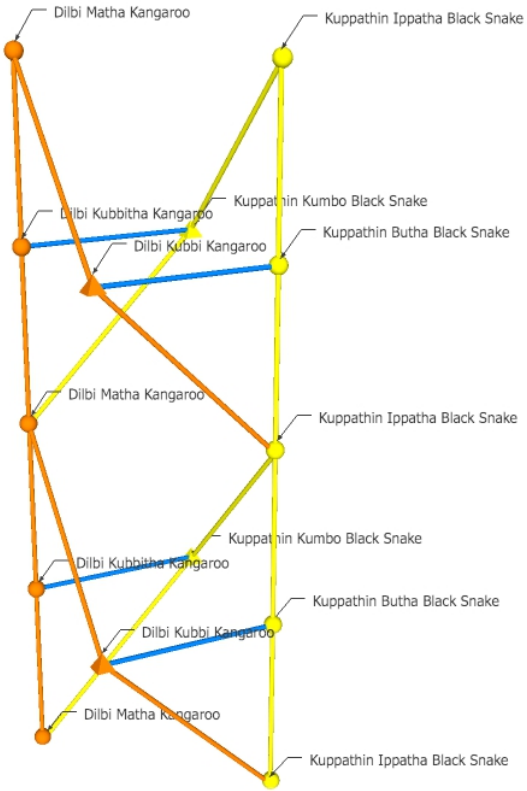


Figure 104: Subgraph of matrimonial reticulation between two sets of totem terms (*Kangaroo* and *Black Snake*), including their 3-part moiety+section+totem variations, discovered by Mathews (1895). A prohibition on selecting a spouse whose father bears the same totem as self (also discovered by Mathews) generates a helical structure of matrimonial exchange between the two sets of terms. See <goo.gl/BM2955> for interactive 3D model.

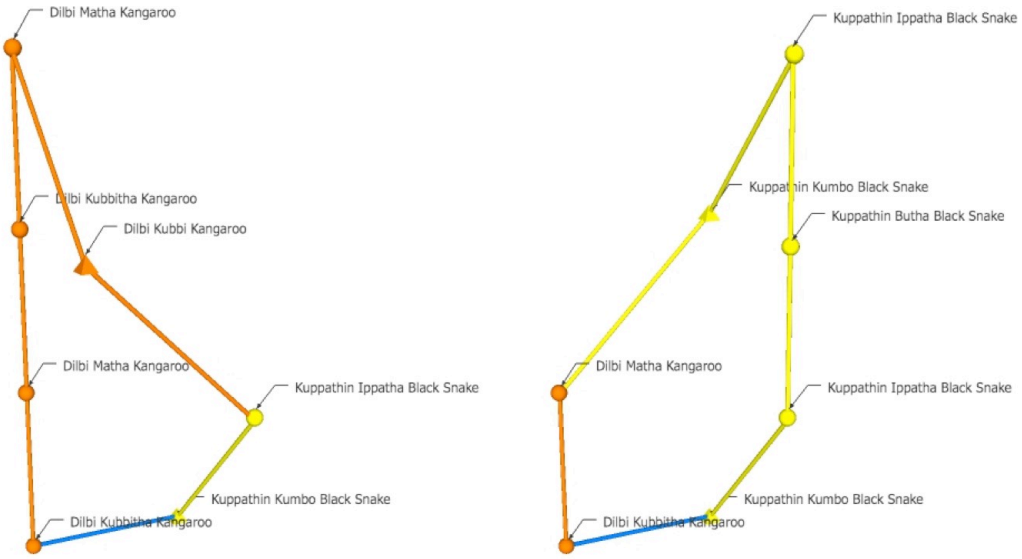


Figure 105: Two shortest possible consnguienous circuits induced from the subgraph of Mathews (1895) model at Figure 104. Note that borth circuits require bearers of both the *Kangaroo* and *Black Snake* totems in order to achieve closure.

Note that in Figure 104 only mothers are visible for each of the spousal couples in the lowest generation. This is an effect of Mathews' 1895 proscription on parents-in-law bearing the same totem as self. In order to reveal the totemic terms of parents-in-law, it is necessary to extrapolate the width of the subgraph at Figure 104 from 2 to 4, and the height from 3 to 4. *Frog* and *Bandicoot* totems are used as examples, although bearers of the *Kangaroo* totem in the *Dilbi* moiety might marry bearers of any of 7 other totem bearers in the *Kupathin* moiety except for their own father, *Black Snake*. Meanwhile, bearers of the Black Snake totem in the *Kupathin* moiety might marry bearers of any of 11 other totem bearers in the *Dilbi* moiety except for their own father, *Kangaroo*. An according extrapolation of the subgraph generates the expanded network illustrated in Figure 106, where the fathers of the two spousal couples are revealed as *Frog* and *Bandicoot*, each embedded in their own consanguineous component of height 3. The subgraph thus expands from 12 to 30 individuals, and from 2 to four totemic components. The original consanguineous *Kangaroo-Black Snake* subgraph also become more strongly cohesive, as the expansion of the network provides more scope for reticulation within the original core. Phrased more specifically, the *Kangaroo* and *Black Snake* mothers of the married couples in Figure 104, are now linked both to each other, and to other members of their own totemic components through their respective marriages to *Frog* and *Bandicoot* men. This phenomenon is illustrated in Figure 107.

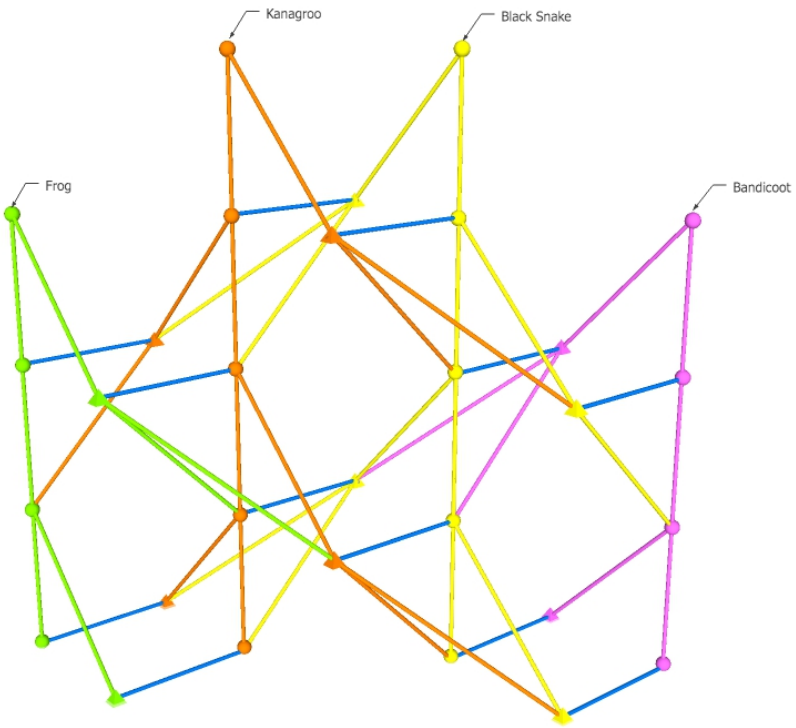


Figure 106: An expanded subgraph of that shown in Figure 103, extended from width 2, as shown in Figure 105, to width 4, such that it includes four totemic partitions. Labelling has been simplified in order not to clutter the graph. See <goo.gl/U5l5Ll> for interactive 3D model.

The original *Kangaroo-Black Snake* subgraph at Figure 104 is composed of many dozens of matrimonial circuits, besides the two consanguineous circuits illustrated at Figure 105. For the expanded *Kangaroo-Black Snake-Frog-Bandicoot* subgraph at Figure 106, there are many *hundreds* of matrimonial circuits, as well as many dozens of consanguineous circuits. There can be no question therefore, that Mathews' new and more specific model of the CMDB terminology reveals a striking tendency towards the generation of cohesion in real world kinship networks, where culture bearers construe their relationships, and realise their social universe, through the semi-complex grammar of their terminological model. Most important in this regard, is the necessary process of 'recruitment' or 'co-option' at each alternate generation of the terminology. This phenomenon is revealed in the expanded subgraph at Figure 106 by the grammar's 'drawing-in' of additional totemic components in order to achieve reticular closure. Of the many hundreds of matrimonial circuits that link various regions of this subgraph together, a special subset links members of all four totemic components. Of these, a further subset of 32 circuits represent the shortest possible means by which all four totemic components can be linked together. Figure 107 illustrates one of these 32 circuits, extending to length 14, height 4, and width 2. At these dimensions it is cognizable to members of the middle generation (grandparents of the spousal couples) in terms that would be familiar to European terminology users such as Morgan (1871) and Lounsbury (1964). For this middle generation, all members of the circuit could be addressed as 'parents', 'grandparents', 'children', 'grandchildren', 'siblings', 'siblings-in-law', and/or 'cousins', rendering the entire circuit navigable via primary and secondary real-world relationships. Acknowledging that there are 19 totemic terms in Mathews' 1895 model, it becomes plausible that the Central Murray Darling Basin people who explained this terminology to him, had first-hand knowledge of such circuits instantiated in the reality of kinship-based populations. This goes to the key point raised at end of Section 4.1 above: The complexity of the grammar of a given terminology does not directly define the limits of the social structures that it is capable of realising. Rather, it is the multitude of ways in which that grammar may be *applied* in a real-world kinship setting that opens the door to an emergent complexity. Deducing simple structures from such basic criteria as the number of terms, or the emphasis ostensibly placed on one or another kind of pro- or prescription, gives little to no indication of how the terminology functions to generate real kinship networks over time and space.

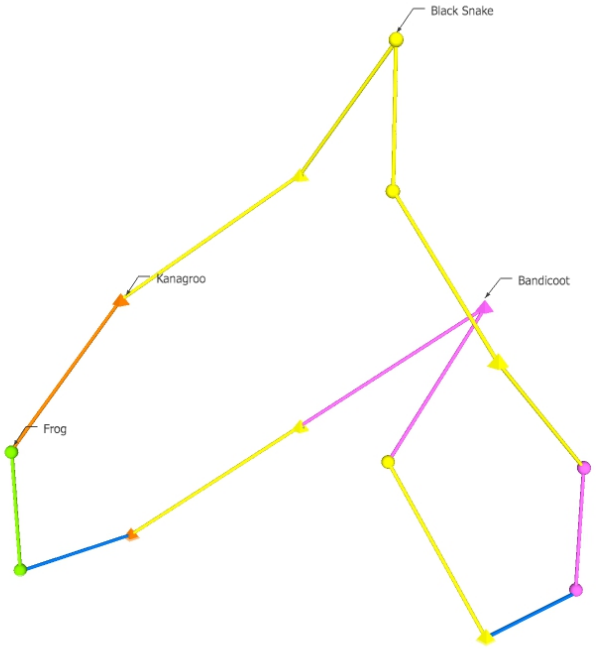


Figure 107: One of 32 shortest possible matrimonial circuits that bind the four totemic components together into a single component in Mathews (1895) model. The circuit does not exceed height 4, width 2, meaning it is cognizable to members of the middle generation as incorporating grandparents, grandchildren, aunts, uncles, and cousins (in European terms). See <https://goo.gl/wFNUGq> for interactive 3D model.

By 1897 Mathews had learned of significantly more totems in the CMDB terminology, publishing a list of 66 totem terms together with their associated moiety terms (Mathews 1897 - see Table 10). He had also identified an even larger number of so-called 'exceptions' to the theoretical section exogamy rule, first proposed by Ridley (1856) and doggedly maintained by Howitt from the 1880s onwards (Fison &Howitt 1880 *inter alia*). Following Ridley's (1875) example, Mathews modelled these 'family marriages' as optional marriage choices available to people bearing one of a subset of totems associated with each moiety term (Mathews 1897). These subsets included 7 totem terms associated with the *Kupathin* moiety and 12 totem terms associated with the *Dilbi* moiety. These are graphed at Figure 107, which illustrates all of the allowable intra-moiety marriages, with allegedly 'ideal' extra-moiety (moiety exogamy) marriages removed for clarity.

Table 10: Mathews' (1897) list of Gamilaraay totem terms, comprising 66 terms divided equally between *Dilbi* and *Kupithin* vertical moieties.

Dilbi Moiety	**Kupathin Moiety**
Ants	Belar [tree variety - Casuarina cristata]
Bandicoot [Perameles nasuta]	Black Snake [poss. Pseudechis guttatus]
Black-duck [Anas superciliosa]	Box-tree [Eucalyptus microcarpa or moluccana]
Bony Fish [aka Bony Bream - Nematalosa erebi]	Bream [Bidyanus bidyanus]
Bowerbird [Chlamydera maculata]	Brigalow [tree variety - Acacia harpophylla]
Brown Kangaroo [poss. Macropus giganteus]	Bubber Snake [unidentified]

Bumble [tree Variety - Capparis loranthifolia]	Carpet Snake [Morelia spilota]
Carbeen [tree variety - Corymbia tessellaris]	Centipede [unidentified]
Crow [Corvus coronoides]	Codfish [Maccullochella peelii]
Eaglehawk [poss. Aquila audax]	Common Fly
Fish Hawk [poss. Todiramphus macleayii]	Coolabah [Eucalyptus coolabah]
Ground Iguana [Goanna - Varanus varius]	Curlew [Burhinus grallarius]
Jack-ass [Kookaburra - Dacelo novaeguineae]	Death-adder [Acanthophis antarcticus]
Jewfish [Tandanus tandanus]	Emu [Dromaius novaehollandiae]
Locust [Grasshopper - unidentified]	Galah-parrot [Cacatua roseicapilla]
Moon	Goonhur [unidentified]
Opossum [Trichosurus vulpecula]	Gum-tree [Eucalyptus ssp.]
Padamelon [poss. Thylogale stigmatica]	Jew-lizard [Pogona barbata]
Peewee [Grallina cyanoleuca]	Kurria [Mythical water creature]
Pelican [Pelecanus conspicillatus]	Magpie [Gymnorhina tibicen]
Pine-tree [Callitris ssp.]	Native Bee [unidentified]
Pleiades [Constellation]	Native-cat [Quoll - Dasyurus maculatus]
Porcupine [Echidna]	Native-companion [Brolga - Grus rubicunda]
Salt-bush [Atriplex muelleri]	Night Owl [poss. Podargus strigoides]
Scrub Turkey [Alectura lathami]	Plain-turkey [Ardeotis australis]
Sparrow-hawk [poss. Accipiter cirrocephalus]	Quail [Turnix ssp.]
Sun	Rainbow
Tibi-bird [unidentified]	Red Kangaroo [Macropus rufus]
Water	Reddish Butterfly [unidentified]
White Butterfly [unidentified]	Ring-tailed Opossum [Pseudocheirus peregrinus]
White Cockatoo [Cacatua galerita]	Swan [Cygnus atratus]
Wood-duck [Chenonetta jubata]	Wallaroo [Macropus robustus]
Yellow-bellied Fish [Macquaria ambigua]	Water Mole [Platypus Ornithorhynchus anatinus]

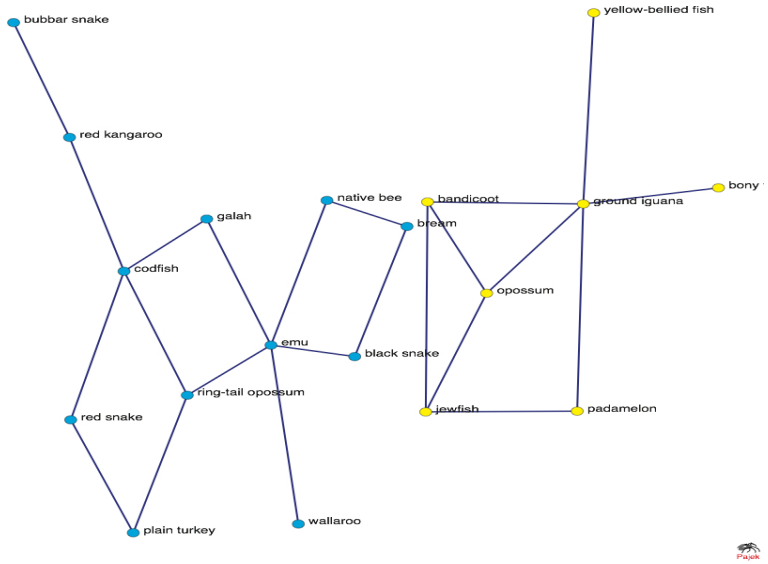


Figure 108**: Contemporary graph of allowable Gamilaraay intra-moiety marriages listed by Mathews (1897:162-163).** ***Dhee*** **marked by blue vertices are associated with the** ***Kupathin*** **moiety, yellow vertices with the** ***Dilbi*** **moiety.** ***Kupathin Emu*** **and** ***Dilbi Ground iguana*** **have the greatest number of options** ***Dilbi Bubbar Snake***, ***Dilbi Wallaroo***, ***Kupathin Yellow-Belly fish*** **and** ***Kupathin Boney Fish*** **have the fewest options. N ote that Mathews does not distinguish between sections.**

By 1905 Mathews' model of the CMDB terminology had reached maximal complexity. Since 1897, over the course of later interviews with Ngiyampaa speakers at Bourke, Brewarrina, Quambone, and

elsewhere, Mathews also learned of term types that he translated as 'Blood' and 'Shade'. Although it was Ngiyampaa speakers who taught Mathews the role of these elements within the terminology, he was also advised that the terms were known to speakers of other languages in region, in including Gamilaraay and Yuwalaraay among others. In 1905, Mathews reported a strata of three terms referring to different sitting positions in the shade of trees, including 'inner-', 'middle-', and 'outer-shade', named in both Gamilaraay and Ngiyampaa, '*nhurrai*', '*wau-gue*', and '*winggu*' respectively. As with moiety and totem terms, Mathews reported that transmission of these terms took the mode of direct matrifiliation.

> *"Besides the phratries [i.e. moieties], sections and totemic groups above illustrated, the whole community is further divided into what may, for convenience of reference, be called 'castes.' These castes regulate the camping or resting places of the people under the shades of large trees in the vicinity of water or elsewhere. The shadow thrown by the butt and lower portion of a tree is called 'nhurrai '; that cast by the middle portion of the tree is wau-'gue'; whilst the shade of the top of the tree, or outer margin of the shadow, is 'winngu'".*
>
> *"[T]he men, women and children, whose prescribed sitting places are in the butt and the middle shades of the tree are called 'guai'mundhan,' or sluggish blood, whilst those who sit in the top or outside shade are designated 'guai'gulir,' or active blood. This further bisection of the community into Guaimundhun and Guaigulir, which may be referred to as 'blood' divisions, has happened so long ago that the natives have no explanation regarding it. The Guaigulir people - those who occupy the 'winggu' or outer margin of the shade are supposed to keep a strict watch for any game which may appear in sight, the approach of friends or enemies, or anything which may require vigilance in a native camp.*
>
> *"**Each phratry, and consequently every section and every totemic group, contains men, women and children belonging to the Guaimundhun and Guaigulir bloods, with their respective shades.** We have just seen that the Guaimundhun people are subdivided into nhurrai and wau-gue, the butt and the middle shades, whilst the Guaigulir folk are intact in the winggu or distal shade" (Mathews 1905:5-7).*

Blood.	Father	Mother	Ofispring
Guaimundhun	Nhurrai	Winggu	Winggu
	Nhurrai	Wauguē	Wauguē
	Wauguē	Winggu	Winggu
	Wanguē	Nhurrai	Nhurrai
Guaigulir	Winggu	Wanguē	Wanguē
	Winggu	Nhurrai	Nhurrai

Figure 109: 'Blood' and 'Shade' term relations recorded by Mathews (1905:8).

Assisted by Mathews' reinforcement of his existing caveat, that spouses "are what we call second cousins, being a brother's daughter's child and a sister's daughter's child" (1905:9), this terminology can be graphed as shown in Figure 110, where each of the three compound terms *Guaigulir Winggu*, *Guaimundhun Waugue*, and *Guaimundhun Nhurrai* is colour coded. As with the network re-construals above, this graph is extrapolated from the height 1 'elementary' model provided by Mathews at Figure 109, in order to realise shortest possible consanguineous circuits.

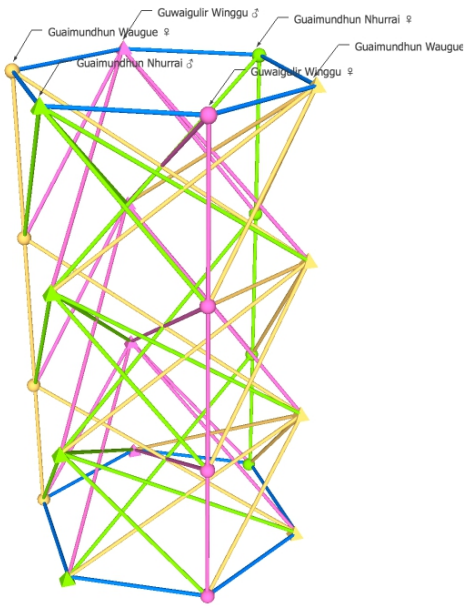


Figure 110: Network model of Mathews' (1905) documentation of compound 'Blood' and 'Shade' terms for Ngiyampaa and Gamilaraay speakers in the CMDB population. See <http://goo.gl/aSqnYp> for interactive 3D model.

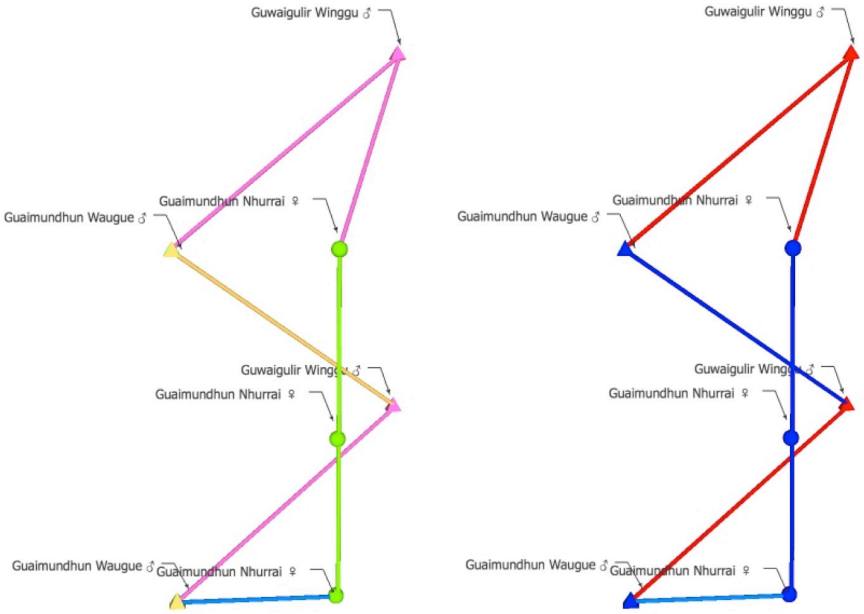


Figure 111: Two iterations of a subgraph of Mathews' (1905) 'Blood' and 'Shade' terminology model, graphed at Figure 110. The circuit on the left is colour coded for shade terms, while the circuit on the right is colour coded for blood terms.

As outlined above, Mathews' 1897 model establishes that the vertical moiety terms *Kupathin* and *Dilbi* realise only a statistical tendency towards exogamy among bearers of totem terms associated with each of them, rather than an outright prescription. The entrenched definition of vertical moieties in the standard model, as construing a so-called 'exogamy rule', is thus unsupported by the CMDB terminology. How does this compare to the compound Blood-Shade terminology documented by Mathews in 1905? Figure 111 illustrates the shortest possible consanguineous circuits that can be realised using Mathews' description of this latter terminology. As with his earlier accounts of totem and vertical moiety terms, these close at height 3. Also as with his models of other terminologies, there is a profusion of matrimonial circuits at the same height, weakening any theoretical axiom that the terminologies predict so-called 'cross-cousin' marriage (including that posited by Mathews himself, as in the quote above). Figure 111 indicates two forms of significance operating in tandem, revealed by the signing of vertices and ties, and emphasised by the topological structure of the circuit. The iteration on the left shows that the circuit draws in all three shade terms in the course of linking spouses together. This points to an effect of 'spacing' or 'pushing out' the path of the circuit such that it traverses each of the shade terms at least once. Meanwhile, the iteration on the right side of Figure 111 shows that the blood terms *Guaigulir* and *Guaimundhun* mark gender switches at each generation, in the same manner as that described by Houseman & White (1998) for the phenomena of 'sidedness'. As pointed out there, such switching between sides is not a corollary of any kind of exogamy rule, but rather an emergent property of networks where parents' gender is consistently realised in terms of spousal laterality. That this way of realising sociality is *not* the same as a prescription for exogamy between groups can be seen in the circuits above, where the marriage

between consanguineous relatives is between individuals bearing the *same* blood term. The effect of the compound blood-shade terminology then, would appear to be maximization of gender switching in a manner not achieved by the grammars of either the totem terminology, or moiety terminology.

Mathews' three major discoveries, in 1895, 1897, and 1905, extended and elaborated the core relational structure of the Central Murray Darling Basin terminology originally detected by Ridley during the 1850s and 1870s. These discoveries showed a complexity and elaboration that was suppressed by Howitt's imposition of Morgan's (1871) group theoretic model on a narrowly selective subset of South East Australian terminologies in 1880 (Howitt 1880 *inter alia*). Howitt's success in marginalising Mathews' research (Thomas 2011) meant that these discoveries were ignored or treated as 'erroneous' by successive waves of social anthropologists who worked with members of the CMDB population, as discussed in the previous chapter. The current section has applied the formal, empirical, and congruent technique of kinship network analysis to Ridley's and Mathew's original accounts of the CMDB terminology in order to evaluate this characterisation. The result has been to reveal major flaws in the deductive logic of the standard model, and the predictions it makes about supposedly 'ideal marriage types' in Indigenous Australian kinship cultures more broadly.

4.2.3 Patterns of Cohesion in the Terminology

It is proposed here that the lexicon and syntax of the classical CMDB terminology, particularly those parts relating to totem terms, provide an indirect and underdetermined means for encoding the embeddedness of real individuals in real kinship networks. This implies that the roles of totems in relation to one another do not impose programmatic 'rules for marriage and descent' as assumed in the standard model, but rather provide a *reservoir of relational meaning* from which real individuals may draw in construing their positions within a meaningful social universe. Most importantly for the current thesis is the hypothesis that this reservoir of meaning orbits a core or 'macro-level' paradigm of social cohesion, of which reciprocity, as defined by Lévi-Strauss (1969[1949]), is a peripheral or 'micro-level' dynamic. In this reformulation, exchange and alliance are secondary functional properties of a system the deeper adaptive success of which is manifest in measurable cohesion. This cohesion is yielded at a macro-level for populations whose members tend towards reciprocal forms of interaction. These are processes that can be formally demonstrated in network theoretic modelling of the terminologies themselves.

All of the terms modelled above are embedded in reticular networks of exchange, with each set of terms occupying a distinct point on the spectrum between elementary and complex structures, in Lévi-Strauss' terms (1969[1949]). Far from comprising distinct sets, discretely associated with distinct subgroup institutions or 'kinship systems', which have been used by standard model theorists to distinguish superordinate groups of 'cultures' or 'societies', the CMDB terminologies incorporate *multiple levels of complexity simultaneously.* These extend from the so-called 'elementary' structures of vertical moieties, encoding direct exchange, all the way through to maximally complex structures

of 66 totems, yielding highly generalised forms of exchange that Lévi-Strauss was able only to imagine (Lévi-Strauss 1969[1949]:xxiv).

This scale can be measured by the frequency of circuit shapes together with their relative roles in generating cohesion at a macro-level of a network, where spousal choices are exercised reiteratively over multiple generations. Since 1856 when Ridley first published a detailed account of the Murray Basin Basin terminology as discussed above (Ridley 1856), data has been available to show that consanguineous cross-cousin circuits comprise a marginal option for spousal selection for members of the CMDB population. As further discussed above, network theoretic modelling shows that these shallow and narrow consanguineous circuits are also marginal to the cohesion of terminological networks, whereas deeper and wider matrimonial circuits are not only more numerous, but also play a more important role in generating cohesion in those networks. Since the 1890s, when Mathews published even more detailed accounts of the CMDB terminology (Mathews 1895; 1897; 1905), data has been available to show that the standard model is fundamentally flawed, and that it bears little-to-no relationship with the manner in which Central Murray Darling Basin people realised sociality through their sociocentric terminology (see above). Despite this, the standard model has persisted, aided by a dearth of kinship-based population data for the population against which to compare the reticular patterning encoded in the terminology.

According to the group theoretic standard model under which Lévi-Strauss (1969[1949])) laboured, an intuited theme of exchange was applied to groups defined by heredity, classically either matri- or patrifilial, such that the respective movement of either men or women between groups caused them to biject into larger 'societal' supergroups, or 'cultures'. The so-called 'descent groups' that are held to constitute these super-groups are deductively reasoned to exist because of the hereditary basis on which transmission of terms is reckoned in all sociocentric terminologies. However, if, taking Lévi-Strauss' (1969[1949]) primary theoretical innovation - that terminologies reflect principles for realising *meaning* about sociality rather than 'rules' for predicting action - then the structures generated by the instantiation of the terminology in the real world take precedence over any theoretical distillation of its so-called 'rules'.

The highly detailed data published by Ridley (1856; 1875) and Mathews (1895; 1897; 1905) reveal reticular structures far more complex and variable than any previously detected by analysis of the standard model. Most importantly, these structures tend towards a single convergent effect - the generation of variable cohesion. In light of these revelations, totems, and the dreamings to which they belong, are much more congruently construed as metaphors for real and tangible social aggregations - measurably concentrated regions within a demonstrably continuous kinship network - rather than discrete, atomised subgroups within a larger theoretical supergroup, whether referred to as a 'tribe', 'nation', or 'society'. This is most clearly evident in the interdependence of terms in the terminological network, where more prolific, wider, and deeper circuits are comprised of maximal

numbers of terms. Under these circumstances, a sociocentric terminology realises observable features of a network topology, with names attached to regions of greater cohesion, and distinctions between names implying regions of lesser cohesion. In these terms, the theoretical entities identified intuitively by Ridley, Mathews, and subsequent waves of anthropologists as discrete 'tribes', 'societies', and 'nations', might be more properly identified as more cohesive regions of a single continuous kinship network of First Australians, spanning the entire continent. This hypothesis is elaborated below.

The reticular structures evident in the terminological models above generate an emergent cohesion among terms. In the following section, this emanant patterning is tested against a 400-person kinship network sample, where the individuals all bear terms. The test itself comprises a search for patterning in the circuit that render the network sample cohesive.

4.3 Kinship and Cohesion in the South East Australian Population

The current section presents kinship network data and modelling for the population from which Ridley and Mathews collected the kinship terminologies described above. The data relates to that part of the population for which kinship data has been collected and, based on the known principles for terminological transmission, has had kinship terminologies attributed to it in the form of extrapolated metadata. Functionally, a set of individuals identified in the archival record as bearing known sociocentric terms, has been identified from other archival sources, their ancestors and descendants traced, and kinship terms attributed to those ancestors and descendants on an extrapolatory basis. The resulting network model is then tested for key patterns identified in the terminological models reviewed in the previous section. In particular, the tests target patterns of reticulation giving rise to emergent network cohesion.

This exercise, presented below, is comprised of four parts: Firstly, a description and contextualisation of the broader kinship-based population data set, including its provenance, consistency, anonymity, and scope. Secondly a presentation of the individuals identified in the ethnographic record, their provenance and anonymity, the range of terminological metadata (sociocentric kin terms) that can be attributed to them, and the technical procedure by which their ancestors and descendants have been identified. Thirdly, the presentation of the kinship network derived from this process, and a survey of the kinship patterns exhibited by this network, particularly those reticular structures that correspond with Ridley's and Mathew's models as described above, and their relationship with the real-world network's overall cohesion. Fourthly, an analysis of the dataset for patterns of cohesion, and a comparison of these patterns with those detected in the kinship data presented in the previous section.

4.3.1 The Central Murray Darling Basin Kinship Data

The CMDB population model is built from a dataset spanning the Indigenous population of South East Australia between the late 18th century and the present. This dataset is stored in a relational database management system (RDBMS) designed, constructed and maintained by the author over 12 years' employment with the native title representative body NTSCORP Ltd[60]. The kinship dataset stored in this system is comprised almost entirely of publically available birth, death, and marriage (BDM) certificates held by the NSW BDM registry. These documents, together with a smaller proportion of other archival documents, including newspaper articles, scholarly texts, and assorted government records are transcribed into the system and coordinated using a process of aggregated relational linking that allows individuals to be identified in multiple separate records and linked

[60] Over more than a decade's employment as anthropologist at NTSCORP, the author designed and implemented the collection, storage, and analysis protocols for this data, collected more than 50% of the data itself, and constructed a comparable or larger proportion of the model.

together using indexes of 'event types' (Rose 2008). The data itself is stored in a 'native' form, consisting of individual transcripts of more than 15,000 BDM certificates, linked to digitized copies of original source documents.

The primary product of this dataset comprises a kinship-based population model for South East Australia, termed 'Unified', under a continuous process of elaboration and refinement since 2006. This model has been constructed collaboratively by the author and colleagues within the Research Unit at NTSCORP. The Unified model currently incorporates approximately 80,000 individuals of predominantly Indigenous ancestry, spanning the period between the late 18th century and the present. This model is constructed from the previously mentioned dataset using commercially available genealogical software 'Family Historian'[61], stored in a conventional GEDCOM file format. This file format has been adapted to accommodate culturally specific metadata, the most important of which for the purposes of the current thesis, are sociocentric terminologies, in addition to universal metadata name, and dates of birth, death, and marriage.

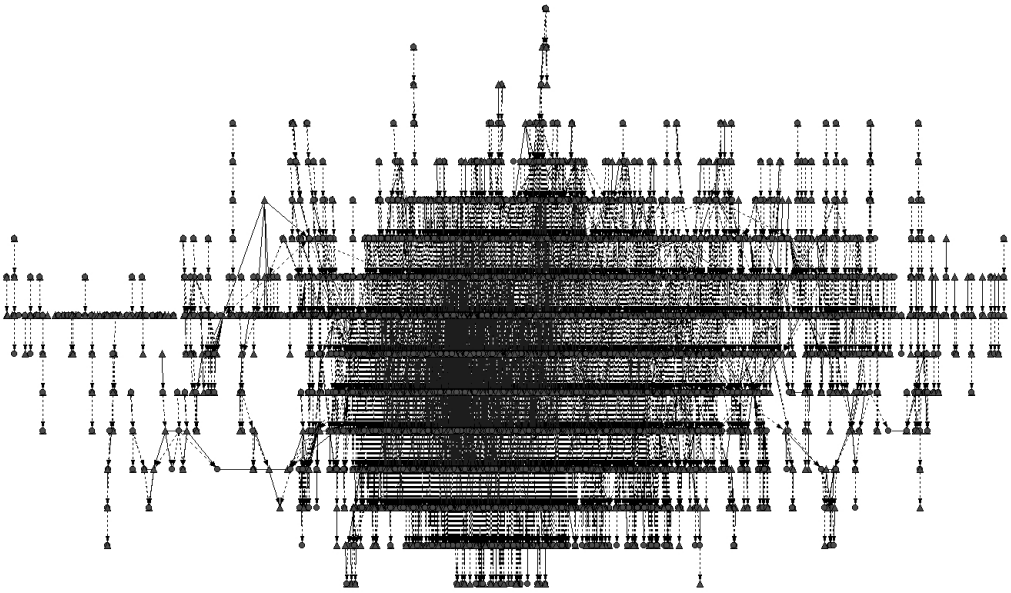


Figure 112: Unified kinship network, totalling 76,048 individuals.

The secondary product of the dataset is a network model, stored in the form of an Ore graph[62], and routinely updated as the Unified GEDCOM file passes through significant changes, typically the addition of 100 or more individuals. The Ore network model is stored in a separate FileMaker RDBMS, developed independently by the author for the purposes of modelling and analysing patterns in geographic and other network metadata. This database system allows the network model to

[61] Family Historian (www.family-historian.co.uk).

[62] See Section 4.1.2 above.

remain dynamically linked to the original GEDCOM file in a manner that allows network clusters and partitions to be created and adapted for the Ore model, using metadata stored in the original GEDCOM file. In this way the RDBMS system acts as an 'intermediary' modelling and analysis tool between convention genealogical software Family Historian on the one hand, and more technical social network analytic Pajek software on the other hand (see footnote 40). The most important function of this RDBMS is the tailored anonymization of individual records in the GEDCOM file, in such a way that names are replaced by the unique numeric identifies allocated in the original GEDCOM file. These numbers, known as RINs (Record Identification Numbers), constitute the consistent match field that link both the GEDCOM and Ore models together, as well as to metadata in the customised RDBMS, and to original sources records in the NTSCORP RDBMS.

Data used for the current analysis was extracted on 29 May 2013 and 2 February 2016, comprising individual sociocentric kin term metadata extracted from the relational database in the first instance, and a copy of the entire Unified network model in the second instance. At this time the Unified network model incorporated 76,048 individuals[63] (represented by the Ore graph at Figure 113). Of these people, 151 individuals were identified as bearing sociocentric kin terms relevant to the current project (listed at APPENDIX 2, and highlighted in yellow at Figure 113). Tracing matrifilial chains incorporating this cluster of 151 individuals generates an expanded subnetwork of 3280 individuals (highlighted in yellow at Figure 114). Using methods described in Section 4.1.3, this expanded subnetwork was extracted from the encompassing 76,048-person network (see Figure 115), revealing 26 separate K1 matrimonial components. The largest of these K1 components, incorporating 3138 people was then extracted from the 3280-person subnetwork (see Figure 116), resulting in a loss of 40 of the 151 individual listed at APPENDIX 2, and leaving 111 (highlighted in yellow at Figure 116). This 3138-person K1 matrimonial component was then further reduced to a matrimonial core of 295 individuals (see Figure 117). Of the 111 individuals bearing attributes in the 3138-person component, a further 69 were ultimately excluded in the reduction process. Although only 42 of the 151 individuals originally identified in APPENDIX 2 remain in the 295-person matrimonial core shown at Figure 117, the preceding process of extrapolating their kin terms to their respective encompassing matrifilial components results in 28[64] sets of kin terms remaining. A further effect of this extrapolatory process is that multiple types of term (e.g. totem terms as distinct from section terms), which have been variably recorded for multiple individuals with single components, can be composited into a single set of terms. For example, where only a totem term has been recorded for one individual, and only a section term has been recorded for another individual in the same matrifilial component, those two terms can be composited into a single set of terms and attributed to

[63] The Unified kinship model grows at an average of ~5,000 individuals per year.

[64] Not to be confused with the 26 K1 matriflial components found in the 3280-person extracted from the full Unified kinship network.

all individuals in that component based on principles of matrifilial transmission documented by Ridley and Mathews (see Section 4.2). Thus, the majority of the 28 sets of kin terms recorded for 28 corresponding matrfilial components in the 295-person marriage core are composites of multiple terms recorded for different individuals by different ethnographers at different points in history. The cluster includes individuals who have been identified as bearing any of the sociocentric terms listed in Table 11 (see APPENDIX 2).

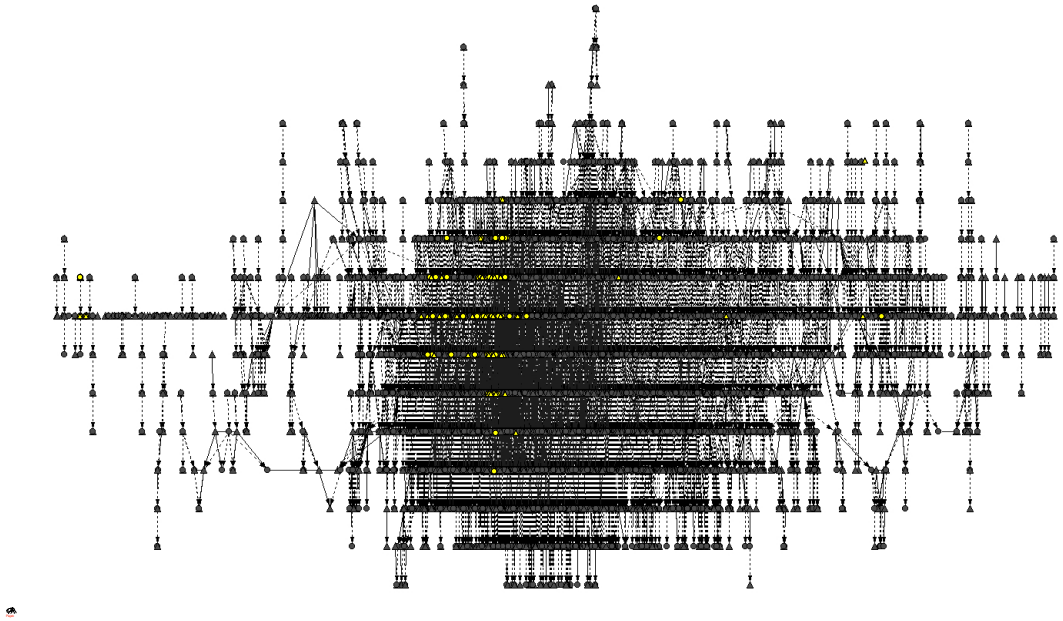


Figure 113: Unified kinship network showing 151 individuals (yellow) for whom sociocentric data has been discovered. Triangles represents man, ellipses women. Horizontal bands indicate calculated generational partitions. Vertical bands indicate calculated concentrations of more closely related individuals.

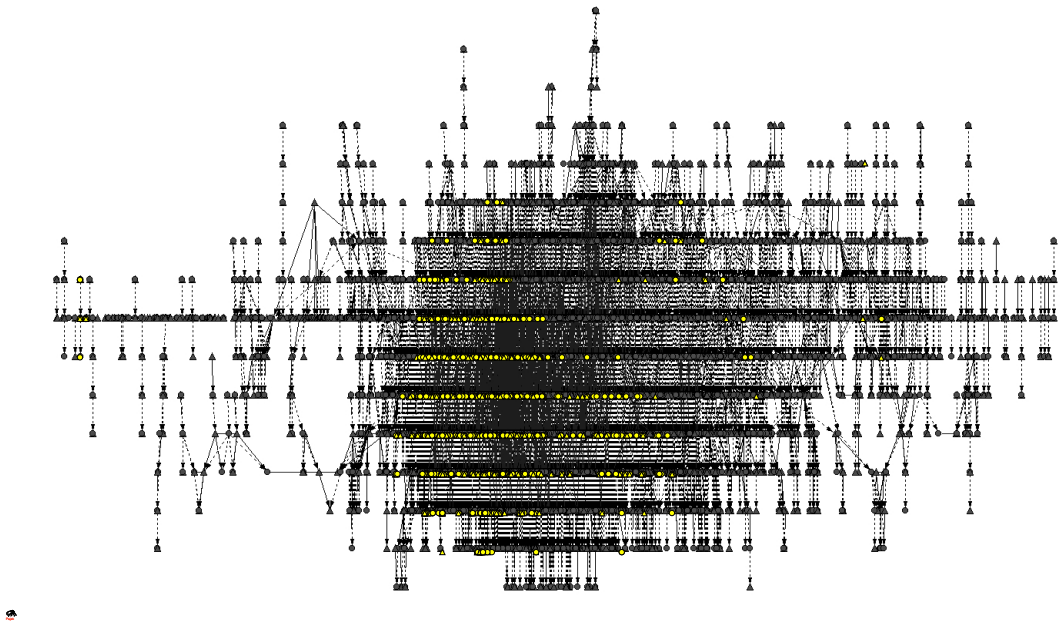


Figure 114: Unified kinship network superimposed with extrapolated 3280-person subnetwork (yellow) comprised of all matrifilial components encompassing the 151 individuals for whom sociocentric metadata has been discovered.

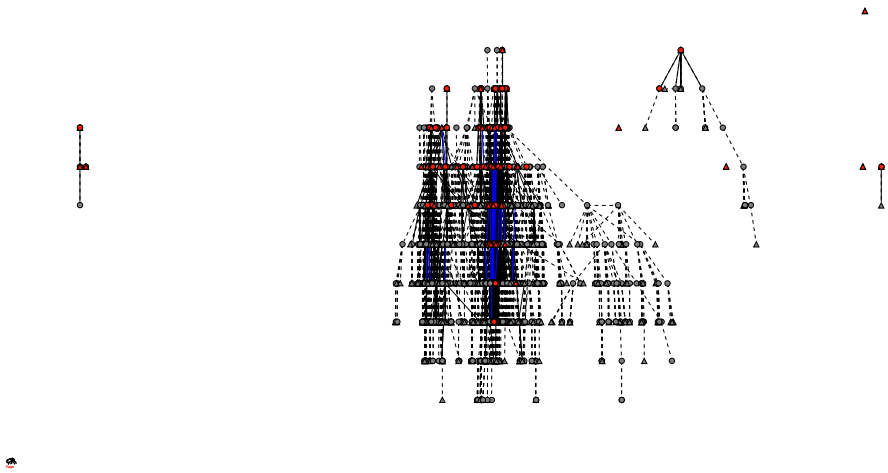


Figure 115: 3280-person kinship network consisting of all matrifilial components encompassing 151 individuals for whom sociocentric metadata has been discovered. Note that the network is not connected.

The primary effect of reducing the corpus to only matrifilial relations is that all its constituent bicomponents are comprised entirely of K2 matrimonial cores that are also matrifilial. Besides helping to make the network more manageable in size, this also helps to highlight the links between network structure and sociocentric terms, which as indicted in Section 4.2 above, are inherited matrifilially across the CMDB population. Broader cognatic relations (including patrifilial as well as matrifilial relations) are obviously important to the entire network, as is relinkage between

components for which sociocentric terms are unknown, however the point of the current exercise is to explore for structures that emerge from the instantiations of a sociocentric terminology framework, which are also intuitively cognizable at the micro-level of individual decision-making. The fundamental question asked here is about causal links between documented autochthonous theories of sociality, observed instances of individual decision-making at the micro-level of relationship-formation, and emergent macro-level properties of a formally modelled network.

The 3280-person subnetwork graphed at Figure 115 above, which includes all of the 151 individuals identified in APPENDIX 2 above, encompasses 101 matrifilial components (or 'lineages'), extrapolated by tracing matrifilial chains to their maximal extents. These matrifilial components range in size between one person (with no known matrfilial ancestors or descendants) and 378 people. This is significant, because the sum of the individuals who form part of these components represents the number of Indigenous people in South East Australia for whom some combination sociocentric kin terms has ever been identified in a single large-scale study. More relevant to the current investigation into kinship cohesion however, is the observation that the 3280-person subnetwork graphed at Figure 115 is disconnected. This means that it consists of several matrimonial components, which are internally linked by both relations of marriage and of descent, but which are not linked to each other by either type of relationship. This lack of connectivity is partially visible in the graph at Figure 115, where a number of smaller components can be seen floating off to each side of the largest overlapping components in the centre of the graph. In total, the 101 *matrifilial* components noted above are composed of 28 *matrimonial* components, where relations of marriage bind combinations of matrfilial lineages together, into what Lévi-Strauss (1969[1949]) would call 'alliance networks'. Of 28 such 'alliance-based' entities in the 3280-person subnetwork, one stands out as particularly large, comprising 3138 people, or 95.67% of the entire subnetwork. This component is graphed at Figure 116 below. As noted above, extracting this largest matrimonial component from the broader set of 101 components, results in the loss of 40 of the 151 individuals identified in APPENDIX 2. The remaining 111 individuals are highlighted in yellow in Figure 116 in order to illustrate their relationships with the corresponding matriflial components of which they form a part.

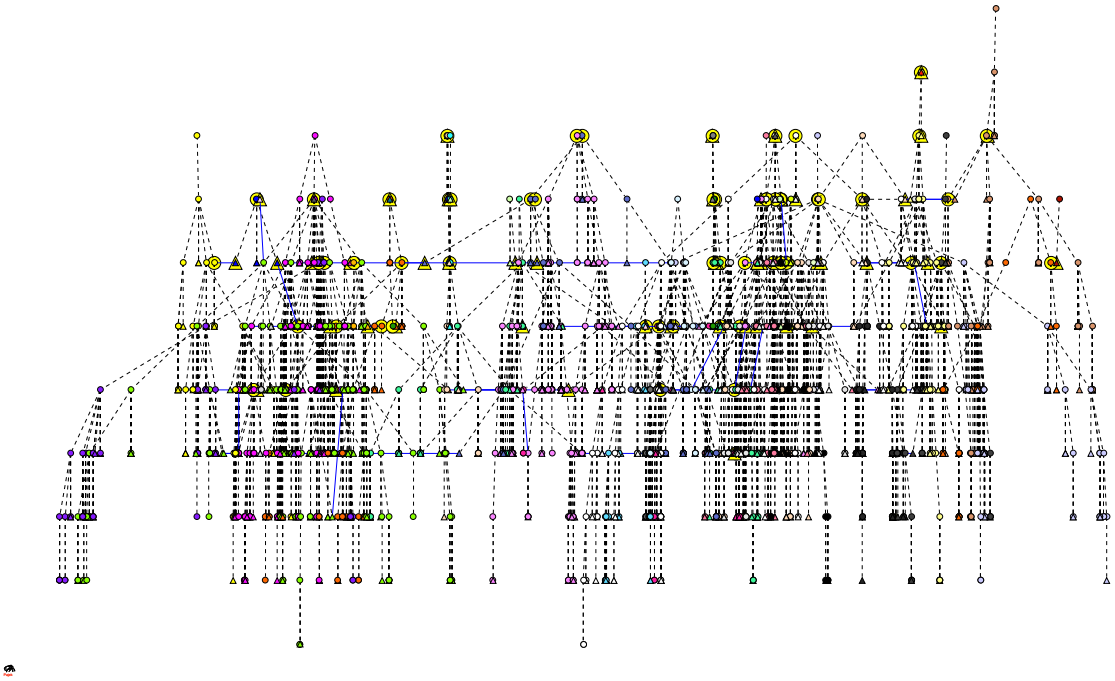


Figure 116: 3138-person matrimonial component for which sociocentric terms are known for all people, based on matrfilial extrapolation of terms known for 111 of the 151 people described in APPENDIX 2. This has been extracted from the 76,048-person Unified kinship network above and redrawn for clarity. The 111 Identified individuals are highlighted in yellow. The matriflial components or ('lineages') extrapolated from FF/(F)F cognatic relations linking to these individuals are distinguished by distinctly coloured sets of vertices.

The 3138-person subgraph at Figure 116 illustrates the intermarriage of distinctly coloured matrifilial components (or 'lineages') over as much as eight generations. This graph also illustrates the principle of extrapolation by which components are identified, where individuals highlighted in yellow correspond with those listed in APPENDIX 2, and are 'embedded' as identifying elements within each matrifilial component. Although this 3138-person subgraph comprises a single matrimonial component it is nevertheless *minimally cohesive*. As described in Section 4.1.3 above, this means that its internal connectivity depends on a minimum of one relationship between each pair of vertices (individuals), which if broken would cause it to decompose into multiple smaller components. The 3138-person subgraph at Figure 116 has a K-connectivity of 1.

From the perspective of kinship cohesion, the 3138-person subgraph represents what might be characterised as an intermediate entity, between an *un*connected network comprised of multiple components on the one hand (such as that graphed at Figure 115), and a *maximally* connected component on the other, where the minimum number of ties between individuals is raised as far as possible while still describing a significant number of people. The 3138-person subgraph does not meet the criteria for reticulation described Section 4.1.3, which requires a minimal connectivity threshold of K2, generated by relinking marriages between spouses who share either or both pre-existing cognatic or matrimonial ties. However, the 3138-person subgraph *does* incorporate such K2 substructure within it, comprising a smaller subset of 295 individuals, or 9.4% of its total population.

This *matrimonial core* (see Hamberger *et al* 2011) exhibits highly informative properties. It is graphed at Figure 117 below.

The smaller size of this graph permits more elaborate marking-up, including coloured envelopes distinguishing each matrifilial component (or 'lineage'), together with yellow highlights corresponding to the individuals identified in APPENDIX 2, together with written labels corresponding to the composite sociocentric terms associated with those individuals. As noted above, one effect of reducing the 3138-person component at Figure 116 to the smaller matrimonial core at Figure 117, is the loss of some of the 111 individuals identified in APPENDIX 2. It total, a further 69 were ultimately excluded in the reduction. However, as also noted above, the preceding extrapolatory process leaves behind the attributes of those excluded, in the form of the attributes of their remaining matrifilial relatives who are part of the core. Before launching into a circuit analysis of this this latter 295-person subnetwork, it should be noted that its structural importance for the larger network is already presaged by the fact that it contains all 28 of the same matrfilial components present in the encompassing 3138-person subgraph described above. Taking into account the further point, mentioned above, that this larger entity describes kinship relations between 95.67% of all people in the ethnographic record for whom sociocentric kin terms are known, this fact is highly significant. It indicates that although the 295-person matrimonial core comprises only 9.4% of the people known to bear sociocentric kin terms, it nevertheless represents a *complete sample* of those kin terms, as borne by 95.67% of people documented in the ethnographic record.

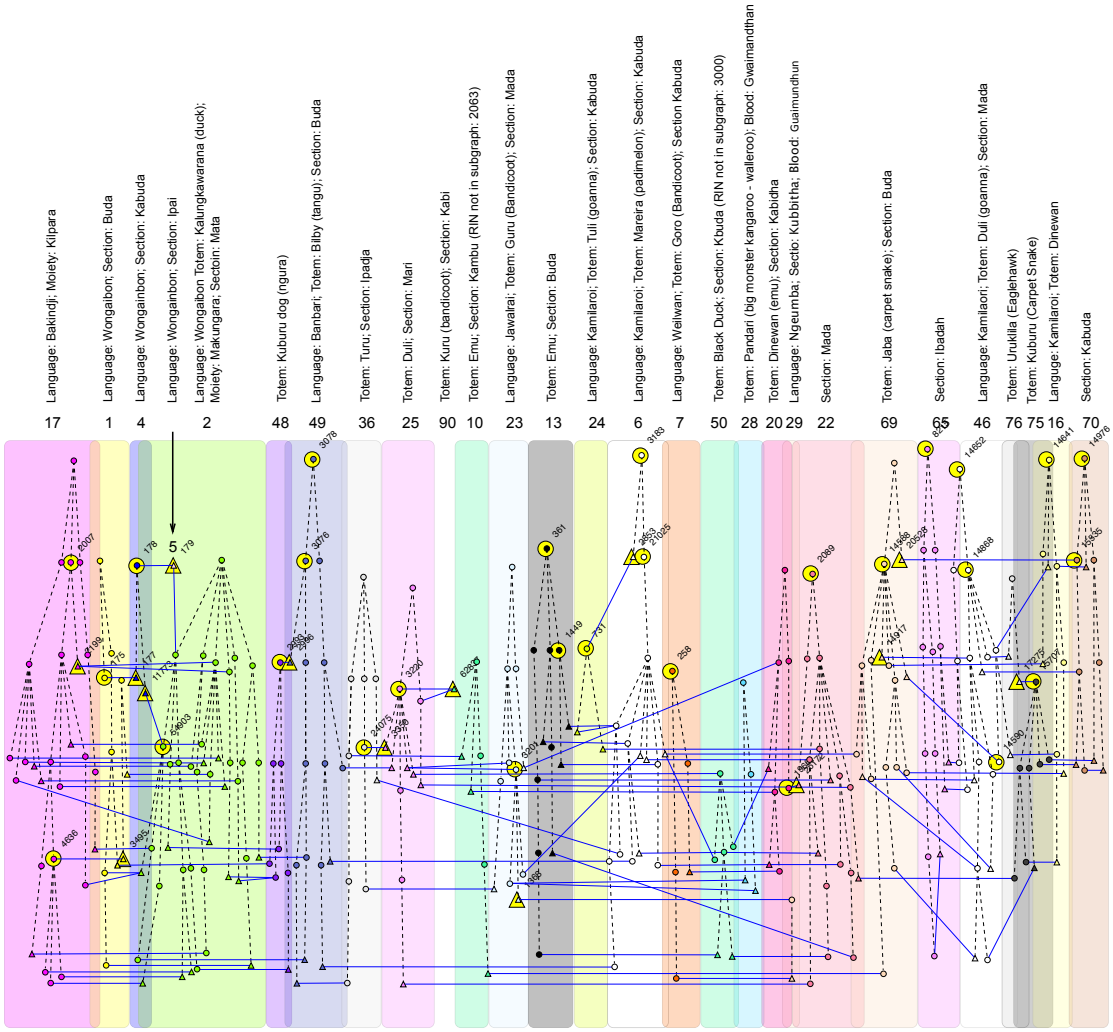


Figure 117: 295-person matrimonial bicomponent, comprised of relinking marriage network where one or a combination of sociocentric attributes (language, totem, section, moiety, blood, and/or shade) is known for every individual. Individual vertices are coloured by matrifilial component (i.e. 'lineage'). This dataset can be accessed at <www.kinsources.net>.

Before embarking on an analysis of cohesion present in the matrimonial core, some important preliminary observations should be made. It has been made clear that this entity is a reticular, cohesive structure, defined in terms of the minimum number of relationships binding its vertices together. Less clear perhaps, is the relinkage between its matrifilial components or 'lineages', represented by the coloured vertical bands in Figure 117. These entities are fundamentally important to Australian anthropology's standard model of kinship, and to the former's broader KLRE model of Australian sociality. As discussed at length in Chapter 3, the standard model construes terminologies as bijected into closed groups, comprising four broad types distinguished by the numbers of terms in the terminology. The KLRE model further bijects these terminology types into macro-level entities referred as 'tribes', 'societies', and so on, which are most commonly identified by language. The sociocentric attributes of the matrimonial components highlighted in Figure 117 clearly show relinking marriage between not only multiple kinship terminology types, but also between

components associated with distinct languages. This point is taken up again in the conclusion, but is flagged here to set a context for more specific observations that are made below.

Figure 118 below highlights the interdependency between those entities that both the standard model of kinship and the KLRE model of sociality construe as 'separate' groups. In this graph, matrifilial components are reduced to individual vertices, leaving intact the matrimonial relations binding them together. The weight of these lines varies in proportion to the number of marriages between components, providing a visualisation analogous to an alliance network. The most frequent number of marriages, indicating what might be characterised as the most 'intense' alliance, is between components 17 and 2, associated with the *Barkindji* and *Wongaibon* languages respectively, and with two correspondingly distinct sets of sociocentric terms - vertical moieties only in the former case, and a composite of vertical moieties, and sections in the latter.

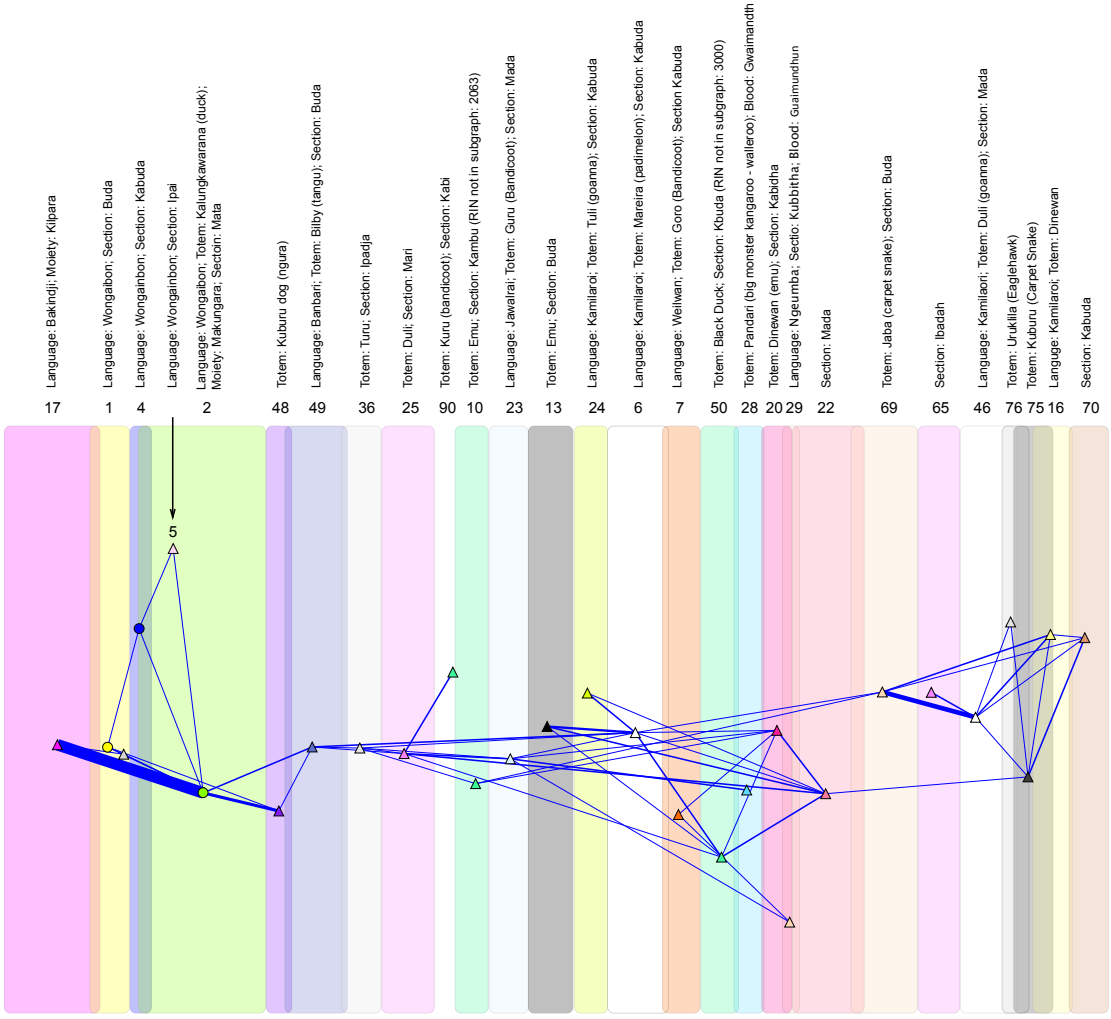


Figure 118: An alliance network of the matrimonial core, with all matrimonial components reduced to individual vertices.

4.3.2 Patterns of Cohesion in the Kinship Network

Section 4.1.3 introduced and explained the feedback relation between cognisable, micro-level matrimonial circuits, evident in the standard model of sociocentric terminologies, and macro-level cohesion in broader kinship networks where such terminologies are selectively instantiated. This theoretical principle was elaborated in Section 4.2.3 using specific examples of the CMDB terminology recorded by proto-anthropologists Ridley (1875) and Mathews (1895). There it was shown that the same 'generic' circuit types found in the network theoretic construals of the standard model are also found in Ridley's and Mathew's much more detailed models. It was further shown however, that Ridley's and Mathew's models, when rendered in network theoretic terms, also describe more complex circuit structures than the standard model, where the majority of circuits are non-consanguineous, *contra* the latter. The current section presents the results of a circuit census conducted in the 295-person matrimonial core described above and graphed at Figure 117.

In keeping with the previously established objective of seeking isomorphism between anthropological models, autochthonous models, and observed social dynamics, a subset of circuit types has been delimited by the convergence of at least two out of three of the following conditions: >1 circuit in the matrimonial core; >1 circuit in Ridley's 1875 model, and/or; >1 circuit in Mathew's 1895 model. By using these parameters to filter through the many circuit types that occur in the three network models listed, a subset of 5 circuit types can be induced, as illustrated below. Cumulatively, the circuits induced by this exploratory process incorporate 40% of the individuals in the network (what might be characterised as a 'significant minority'), as well as 53% of all matrimonial components (what might be characterised as a 'significant majority'). These results of this census are graphed at Figure 119 to Figure 124 below.

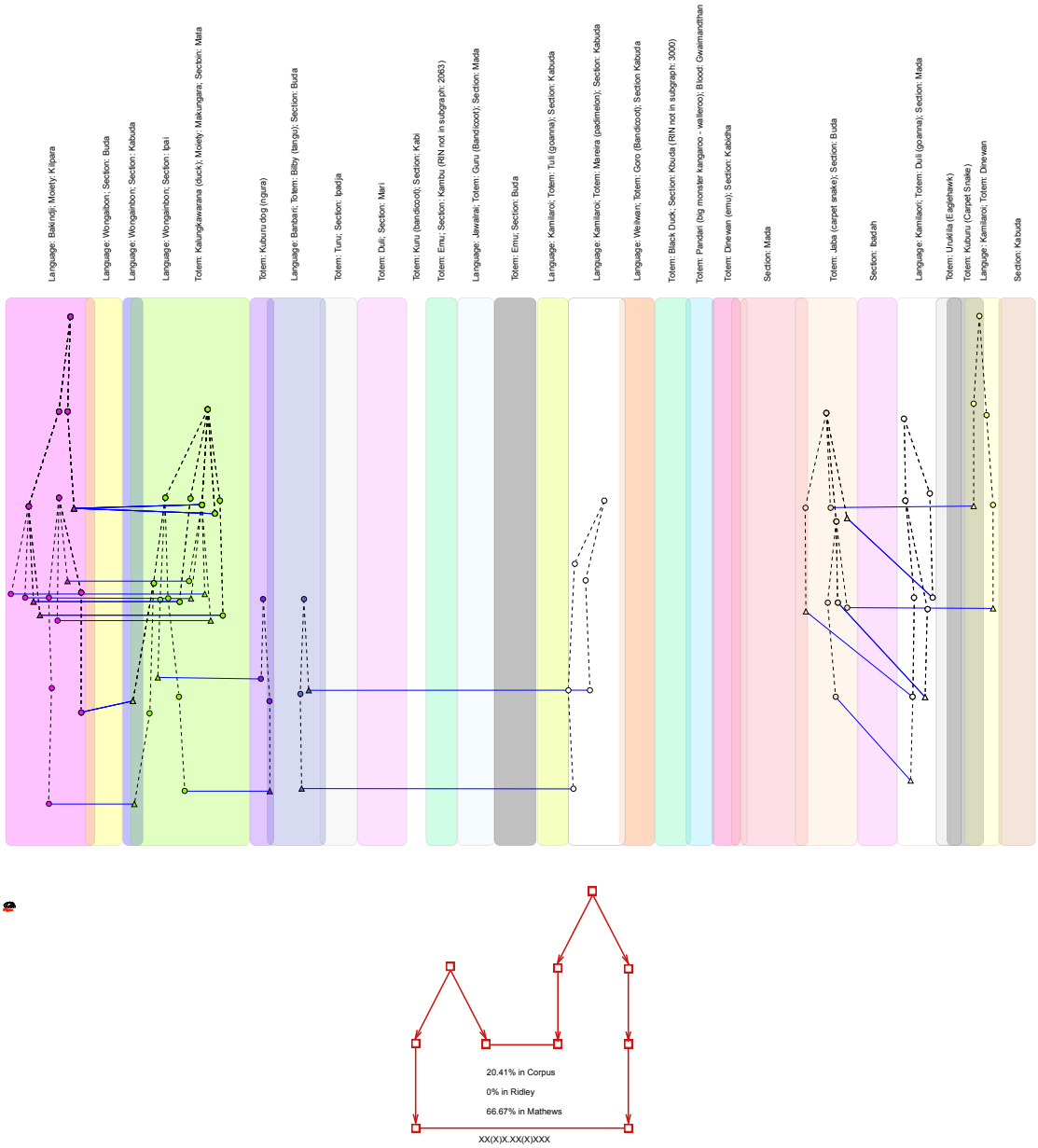


Figure 119: Circuits of structure XX(X)X.XX(X)XXX, incorporating 20.41% of all couples in the matrimonial core and 66.7% of all couples in Mathews' (1895) model, but none in Ridley's (1875) model.

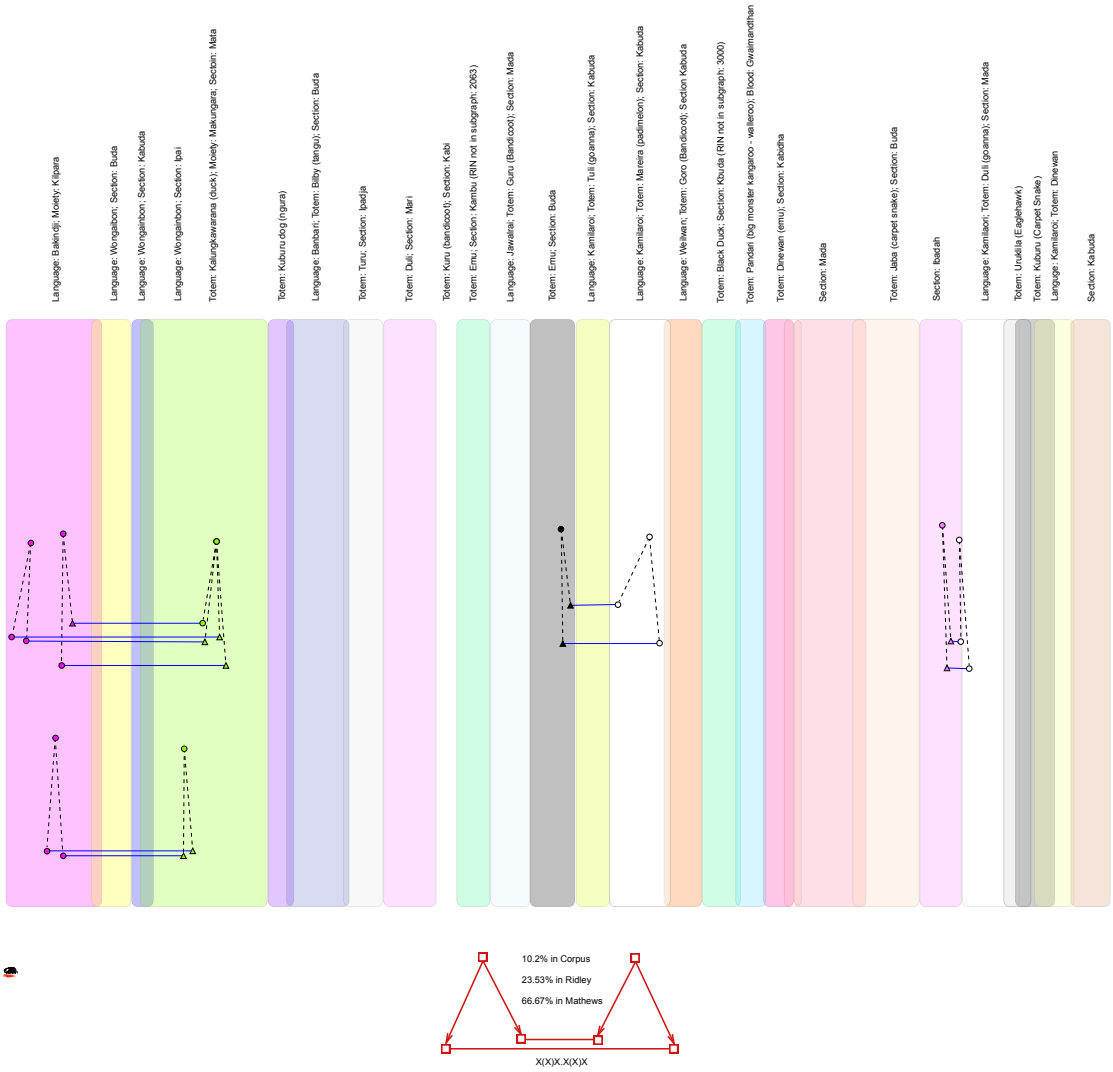


Figure 120: Circuits of structure X(X)X.X(X)X, incorporating 10.2% of all couples in the matrimonial core, 66.7% in Mathews' (1895) model, and 23.53% in Ridley's (1875) model.

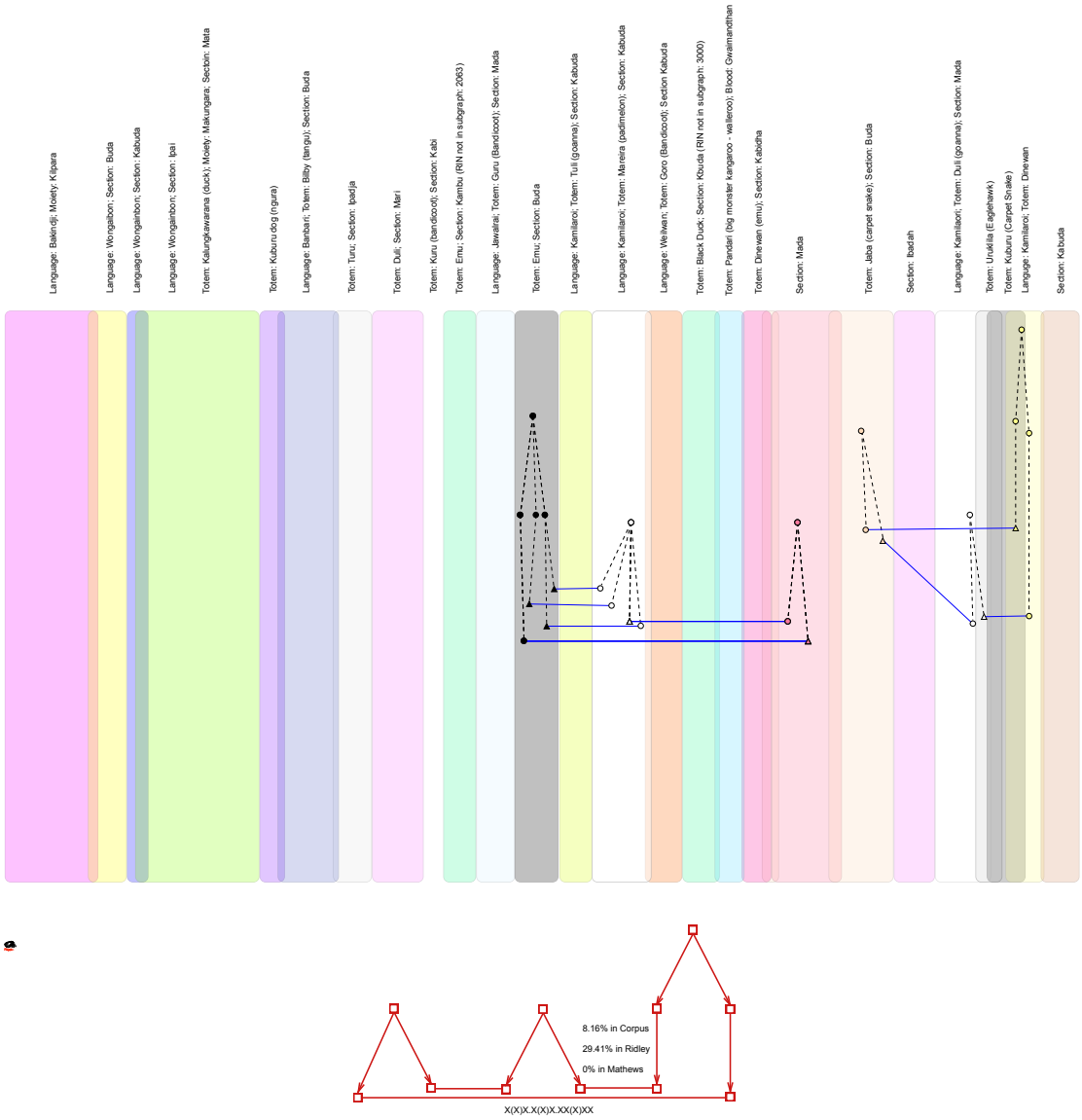


Figure 121: Circuits of Structure X(X)X.X(X)X.XX(X)XX incorporating 8.16% of all couples in the matrimonial core and 29.41% of all couples in Ridley's (1875) mode, but none in Mathews' (1895) model.

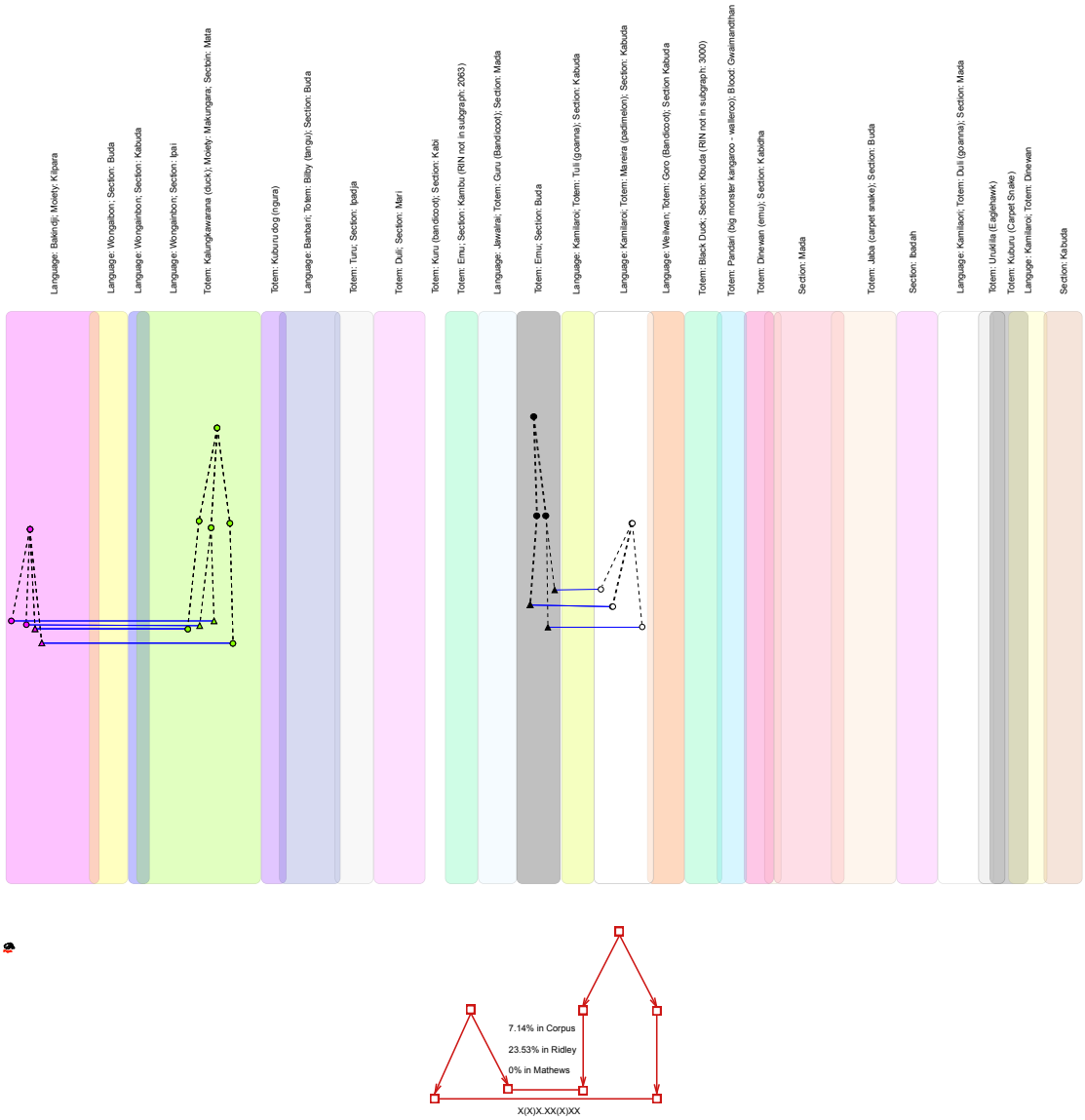


Figure 122: Circuits of Structure X(X)X.XX(X)XX incorporating 7.14% of all couples in the matrimonial core and 23.35% of all couples in Ridley's (1875) mode, but none in Mathews' (1895) model.

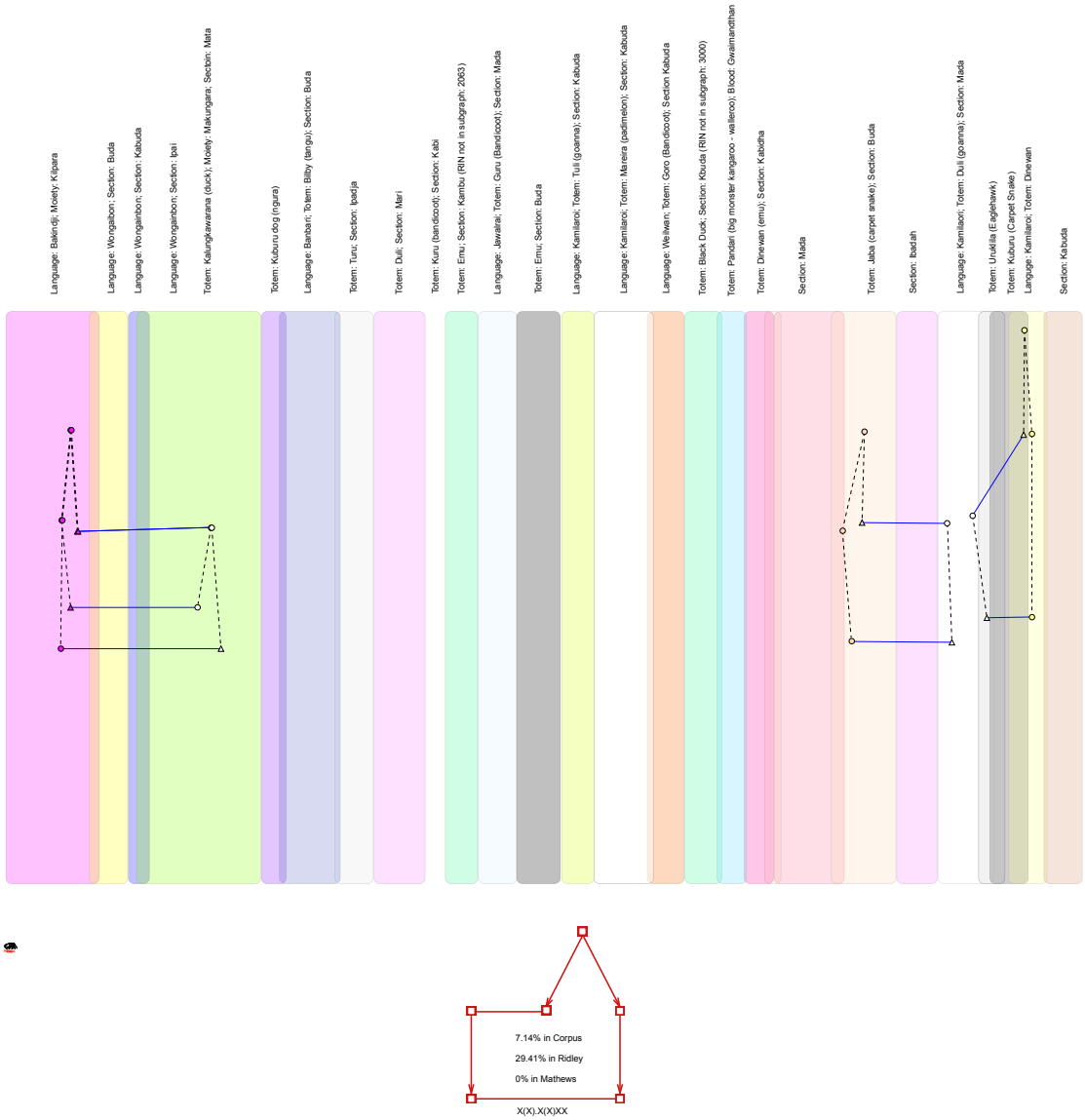


Figure 123: Circuits of structure X(X).X(X)XX incorporating 7.14% of all couples in the matrimonial core and 29.41% of all couples in Ridley's (1875) mode, but none in Mathews' (1895) model.

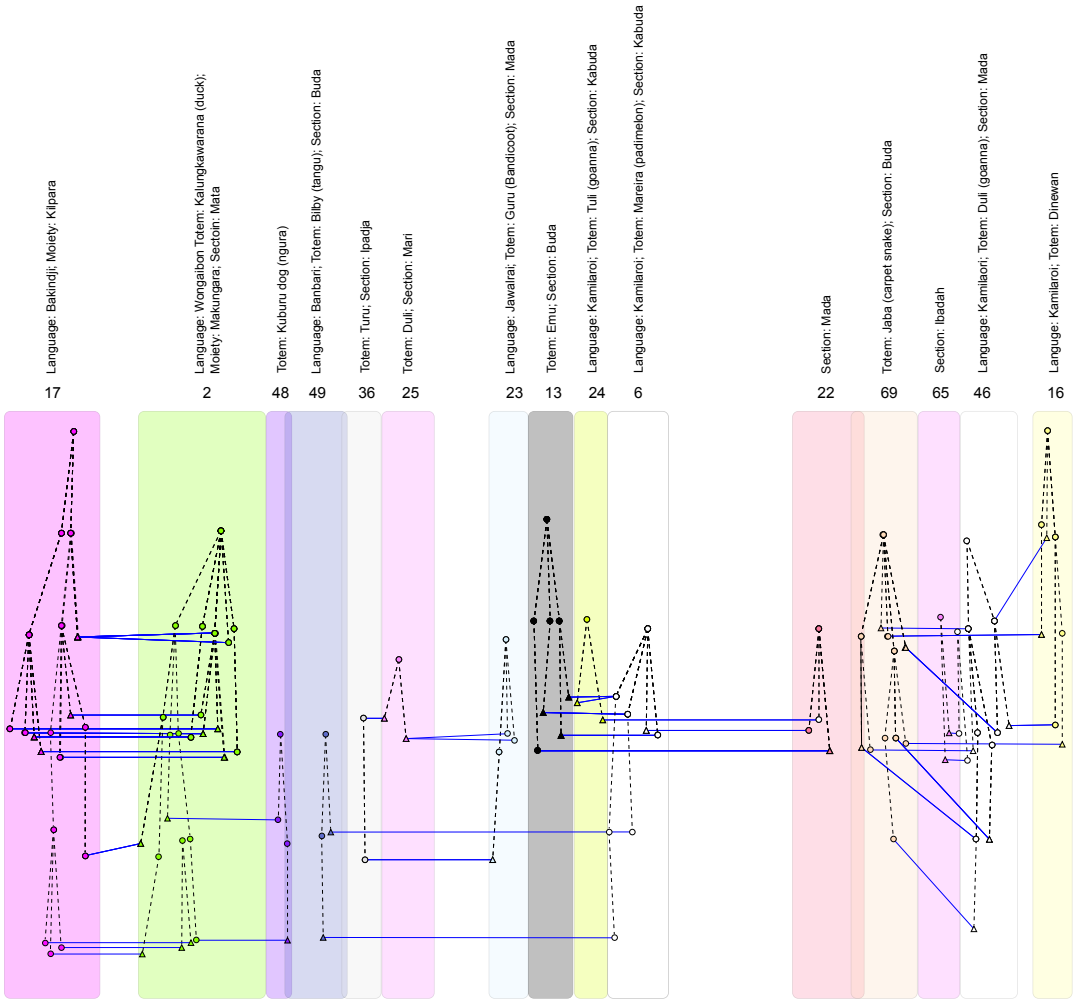


Figure 124: Superimposition of all instances of all five circuit types co-occurring in the matrimonial core, and in Ridley (1875) and Mathews (1895).

It is important to note that despite the large proportion of the 295-person network constituted by these circuits, they do not generate sufficient cohesion by themselves to unify the entire matrimonial core. This can be seen in the graph of superimposition of all circuit instantiations at Figure 124, showing 4 disconnected parts. The 60% of individuals and 47% of matrimonial components that constitute the remainder of the core are incorporated into types of circuit that do *not* co-occur in Ridley's and Mathew's models, but which nevertheless link these co-occurring circuits together. Limitations of space prevent a census of these non-co-occurring circuits here, although they are interesting, including for example, a significant number of so-called 'skewed' marriages where pre-existing relationships between spouses place them in ostensibly distinct generations (as discussed by Denham 2013 *inter alia*). The majority of these non-co-occurring circuits are also relatively short and plausibly cognisable to their constituent members.

It must be emphasises that the goal of the census presented in the current section has not been to test whether or not circuits documented in anthropological models occur in the real world kinship networks of the people whose idea systems those models describe (although this point is validated

incidentally). Nor has the goal been to test whether or to what extent real-world marriages may be designated as either 'correct' or 'incorrect' in relation to any so-called kinship 'rules' represented by anthropological models. Rather, the goal has been to test whether and to what extent the *patterns of cohesion* evident in terminological models give rise to broader and more complex forms of cohesion across a real world network where they are known to be instantiated, and to measure the properties of this emergent, higher order cohesion. To this end, the group theoretic standard model of Australian kinship, with its four-part typology of horizontal and vertical 'moieties', 'sections', and 'subsections', has been shown to be insufficiently detailed, especially in light of the modelling conducted by Ridley (1856; 1875) and Mathews (1895; 1897; 1905). It is observed that circuits which co-occur in both Ridley's (1875) and Mathew's (1895) models, and in the matrimonial core of real world kinship networks in the CMDB population, incorporate nearly half of all individuals in that core, while circuits occurring in the core alone account for the other half. By comparison, *none* of the circuits predicted by the standard model occur in the matrimonial core. The fact that the co-occurring circuits comprise such a large minority, while the remaining non-co-occurring circuits that link the latter together are comparably short and potentially cognisable, suggests an interdependence between the two sets of circuits that is not explicitly construed by the terminology itself. In light of these observation it seems reasonable to interpret the relationship as a generative one, where those circuits explicitly construed in the relevant models *give rise to* other circuits, which in turn function to bind the entire core into a single reticulated entity.

4.4 Conclusion

In the current chapter, kinship network analysis has been presented as a methodological pathway towards consilience between social anthropology and its adjacent natural science fields. Kinship network analysis is a special branch of social network analysis that bears acute relevance to Australian social anthropology in particular, because of the thematic emphasis that this branch of the discipline has historically placed on kinship in construing the sociality of Indigenous populations, and its broader theorising about the causes of that sociality. Kinship network analysis also brings a broader corrective force to bear upon the intrinsic and extrinsic distinctions that separate social anthropology and social science from the natural sciences, by providing a language that is capable both of accommodating autochthonous theories of sociality, and of construing those theories in terms that are at once congruent, formal and empirical. The convergence of these characteristic functions in the language of kinship network analysis allows individual decision-making processes, manifest in observable relationships between real people, to be placed at the forefront of KNA models of sociality. This is a goal towards which social anthropology has struggled for decades. The current chapter has sought to meet the criteria for a consilient social anthropology, described in Chapter 1 by the triad between, a) autochthonous models; b) anthropological models; and c) observed sociality. For the purposes of the current chapter Ridley's (1875) and Mathew's (1895) terminological models constitute the relationship $a \rightarrow b$; the 295-person matrimonial core extracted from the Unified network constitutes relationship $a \leftrightarrow c$; the census conducted on circuits co-occurring in both Ridley's and Mathew's models, and in the Unified network constitutes relationship $c \rightarrow b$.

Using kinship network analysis re-applied to optimally detailed terminological models recorded by Ridley and Mathews, the current chapter has shown firstly that autochthonous theories of kinship are much more than either 'rules' for social interaction, or arbitrary semantic perspectives on forms of interaction that would exist irrespectively. Rather, it has been shown that these autochthonous theories comprise systems of ideas for realising observable and meaningful social *cohesion* across populations that the KLRE model otherwise regards as disconnected and discrete. This undermines the standard model's tacitly group theoretic regime for classifying kinship terminologies according to the morphology and number of terms in their lexicons, pointing instead to a single underlying syntactic logic for managing social relations across ostensibly diverse communities. Through the inclusion of language as part of the list of composite sociocentric terms presented at APPENDIX 2, within a reticulating matrimonial network core, the current chapter has also found that this second great theme of Australian anthropology is *not* a delimiting factor in Indigenous sociality. On the contrary, it has been shown that the most striking examples of matrimonial alliances in the real world kinship core of the Central Murray Darling Basin population are actually between speakers of distinct languages (Ngiyampaa and Paakantyi), weakening the KLRE model's prediction of linguistically based normative closure on macro-level 'societies', 'tribes', and 'blocs'.

In the course of elaborating the mode by which network theory construes space and time, the current chapter has also shown that the cohesion of kinship based populations is an emergent property from *successive* decisions about relationship formation, where decision-makers factor in the outcomes of previous decisions among their predecessors. This is an inherent temporal factor in the logic of matrimonial circuits, which close on *new* marriages between people who are already linked though *past* relationships. Time is also a key factor in the ongoing maintenance of cohesion across the broader encompassing network, which is underpinned by such circuits. In established social network theoretic terms, cohesion is itself rendered as a spatial property of graphs, where people who are more intimately related are visualised within more tightly constrained regions of a plane (as reflected in the various Ore graphs illustrated above). The following chapter focuses on these established topological approaches to network theoretic construals of time and space, and the potential for their further improvement.

Chapter 5

Spatializing Kinship networks

5.0 Introduction

This chapter introduces geographic techniques for modelling social networks generally, and kinship networks specifically. Building on existing theoretical advances in human geography, a new technique is introduced and described that models social and kinship networks geographically and temporally. This technique, termed here 'spatiotemporal kinship network analysis' (stKNA), allows reliable measurement of social networks distributions in space-time, such that geography and time-flow may be incorporated into analyses of relational kinships structures and their reproduction within geographic space. Of particular relevance to the current thesis, the stKNA technique allows for the congruent modelling of space and time in terms of the *spatiotemporal signatures* of cohesive kinship network structures, and the matrimonial circuits from which they emerge.

The current chapter is divided into three sections. Section 5.1 reviews existing social network theoretic techniques for modelling geographic distance and time flow. Here it will be shown that these techniques are geared towards the modelling of abstract vertex attributes such as 'closeness' and 'distance', rather than the recognition of real-world space-time as an inherent macro-level property of social networks. Section 5.2 reviews existing attempts to develop space-time models of human activity and human interaction. Within the established literature these originate entirely from within the field of human geography, beginning with the conceptual principals of time-geography proposed by Thorsten Hägerstrand in the 1970s (Hägerstrand 1968; 1970), and culminating with the detailed GIS and ICT formalizations of Miller (1991; 1999; 2005), Yu, and Shaw in the past decade (Shaw & Yu 2009; Yu & Shaw 2011; Yu 2006). These geographic formalizations are compared with independent, parallel efforts by the author over the past 8 years to develop a network theoretic model of social 'events' and relations for the purposes of illustrating systematic occupation of land (Rose 2008a; 2008b; 2009; 2010). Finally, Section 5.3 presents a method for integrating the geometric techniques of time-geography into the set- and graph-theoretic framework of SNA, such that social and kinship networks may be spatialized using real-world space-time vectors. The method is then demonstrated on the 295-person matrimonial core introduced in Section 4.3.

Compared to previous chapters, the current chapter makes relatively heavy use of mathematical modelling. This is included solely for the purpose of formal proofing, in line with the thesis' overall aspiration to invoking the strong grammar (Bernstein 1996; 1999) of empirical formalism, as advanced by Leaf & Read (2012 *inter alia*). This mathematical modelling is not integral to the broader principles outlined in the text of the chapter however, and can be skipped over without detracting from the substance of the propositions themselves.

5.1 Existing Spatiotemporal Concepts in Social Network Analysis

The measurement of real-world spatiotemporal attributes is not a core function of social network analysis. There appear to be two main reasons for this, the first being that network analytic techniques are based directly on set and graph theory rather than geometry, and so do not automatically lend themselves to the measurement of real-world space. Social network analysis is by definition concerned with topology rather that topography, measuring the number and types of relations between given sets of vertices rather than their location in objectively measurable physical space. Space is construed by SNA as an analytical device applied to the representation non-spatial network properties, such as clustering, centrality, density, etc. The second likley reason that measurable spatiotemporal attributes do not form a core function of SNA, is that most network studies appear to be generally unconcerned with the real-world spatial attributes of the networks under consideration. Where geographic space is considered in such studies, it tends to be treated as an interesting visual phenomenon, rather than a property resulting from and contributing to network structure. By contrast, time *is* treated as an observable property of social networks, rather than an analytical device alone. This is due to the inherent temporal attributes of network topology, namely network growth, whereby ties are formed in an emanant manner precisely because of preceding network ties. The self-organizing complexity of networks thus demands that time be modelled as a real-world phenomena. As a result of the different treatment of space compared with time, conventional SNA models do not currently construe space-time as an inherently integrated and measurable property of social networks. Typically, the two properties are integrated in a weakly congruent 2- and 3-dimensional graphic representations by stratifying time into vertical layers and then applying topological specializations such as 'spring-embedding' to each successive layer. The following section reviews contemporary SNA models of space and time. Section 5.1.1 presents the definitions of space and time used in kinship, citation and diffusion analysis, given by Nooy *et al* (2011). Section 5.1.2 presents Scott's (2000) explanation of the only widely accepted geometric technique currently used in SNA, multidimensional scaling.

5.1.1 Space and Time as a vertex Attribute: Diffusion Analysis, Genealogies & Citations

The set- and graph theoretic foundations of social network analysis cause space to be treated in a very particular way, such that it is reduced to an abstract measurement of 'social distance'. Social distance is a graph theoretic property that emerges from the relative values of chains of indirect ties and individual tie strength within a single network. In such a graph, vertices that are not connected will be the furthest part, while vertices that are 'most connected' will be closest together. As a relational attribute, social distance is represented in graphs by the length of a tie between two vertices. The spatial positions of individual vertices thus also emerge as relative tie length is calculated reiteratively. This network property is not that of real geometric distance between individuals or groups, or their real spatial positions on the surface of the earth, but of the abstract relational distance between them. Nooy *et al* (2011) explain this convention as follows:

> *"The most important principle [of network graphs] states that the distance between vertices should express the strength or number of their ties as closely as possible. In a map, the distance between cities matches their geographical distance. In psychological charts spatial proximity of objects usually expresses perceived similarity. Because social network analysis focuses on relations, a drawing should position vertices according to their ties" (Nooy et al 2011:17).*

The corresponding example that is provided by Nooy *et al* (2011:51) is a network of international metals commodity trading, which is graphed according to the position of national administrative boundaries on a map of the Earth (see Figure 125), and the value of trading between each pair of nations. In this rendition of space, real-world geography is secondary to the abstract positions of national economies in a world trade system that emerge from their trading relations. These abstract positions are stratified into three partitions classified by 'core', 'semi-peripheral', 'peripheral' status, as an indication of the value of trade ties (Figure 126). Because the geographic location of a given nation is not treated as an influence on the value of trade ties it forms with other nations, it is classed by itself as a separate set of vectors representing x and y values on a plane. These values are not considered important, since the goal of analysis is to detect a) how much each nation trades, and; b) with which other nations it trades. The goal of analysis is not to detect correlations between these measurements, and where the corresponding nations are located in the real world, even though there are clear visual patterns in geographical distribution of ties (Nooy *et al* 2011:52).

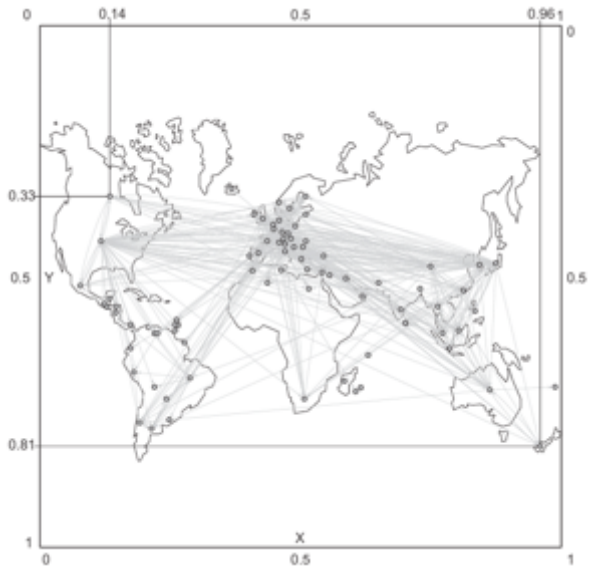


Figure 125: A conventional SNA representation of geographic space (Nooy *et al* 2011:52).



Figure 126: Conventional SNA representation of space according to tie strength (Nooy *et al* 2011:55).

This construal of distance follows automatically from the intersection of set and graph theory in which vertices and ties are plotted according to measurable classes of attributes, whether those be formed by discrete partitions, or continuous vectors. The recognition of real geographic space is thus limited to its representation by discrete classes of distance, such as 'near' or 'far', or by continuous vectors, such '0, 2, 3, ... 100km'. In the analysis of the world trade system presented by Nooy *et al* (2011:51), the real geographic distance between nations is not considered, for example to detect whether there is a correlation between 'core' status and geographic proximity as measured by number kilometres. This is because conventional network analysis is topological by definition, rather than topographic, meaning that the measurable value of ties is not necessarily a physical property of ties themselves, determined by the real-world context in which the exchange occurs, but rather a property of the type of exchange that occurs.

The measurement of time in conventional social and kinship network analysis follows this paradigm, as explained in Nooy *et al*'s account of network diffusion (2011:186-206). This technique treats time as a series of vectors marking temporal intervals, such as hours, days, years, etc., each of which can be attributed to a given vertex according in terms of a structural index of tie-formation - the moment when a tie is formed with that vertex. This allows analysts to visualize the development of a network, and to detect and measure the role of each vertex in facilitating consecutive stages in that development, using techniques such as main path analysis (see previous chapter). One example provided by Nooy *et al* (2011), is the diffusional of a new mathematics method among state schools in the USA between 1958-63 (2011:186-188 see Figure 127). Like the world trade network depicted in Figure 125, ties between vertices represent exchanges, in this case the transmission of the new method from one school to another via friendships among school superintendents. By graphing the

flow of time as a series of vertical bands representing consecutive years, Nooy *et al* (2011) are able to clearly visualize the uptake of the method across the network of 38 schools, over 6 years.

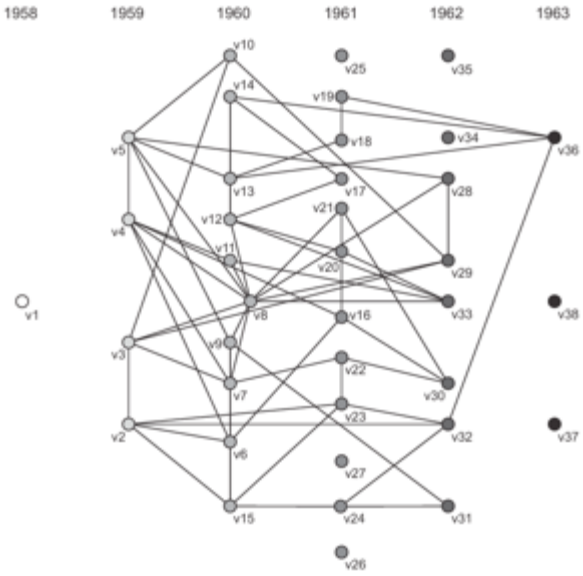


Figure 127: A conventional SNA model of temporal flow in the measurement of diffusion (Nooy *et al* 2011:188).

Vectors are also used to index 'generations' in conventional graphs of kinship and citation networks, where they are used to partition vertices according to their relative ascent/descent relations (see Figure 128 and Figure 129). Rather than indexing temporal moments in the real world, generational partitions stratify stages in a network's development. These stages are implicitly temporal, since lower generations cannot precede higher ones, but the time is not 'real' because it does not correspond with real temporal vectors (days, months, years, etc.) in the same way that diffusion modelling does.

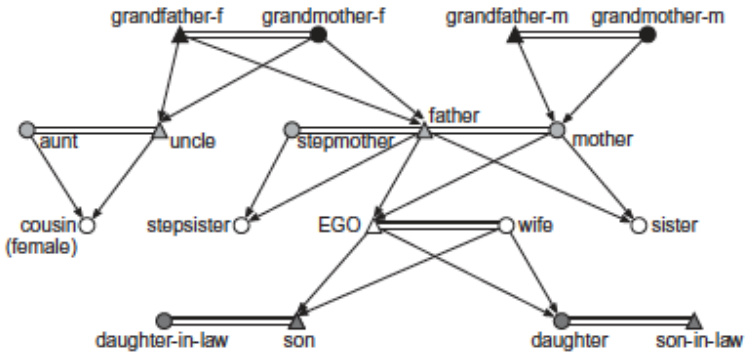


Figure 128: A conventional SNA model (Ore graph) of temporal flow in the measurement of kinship generations (Nooy *et al* 2011:262).

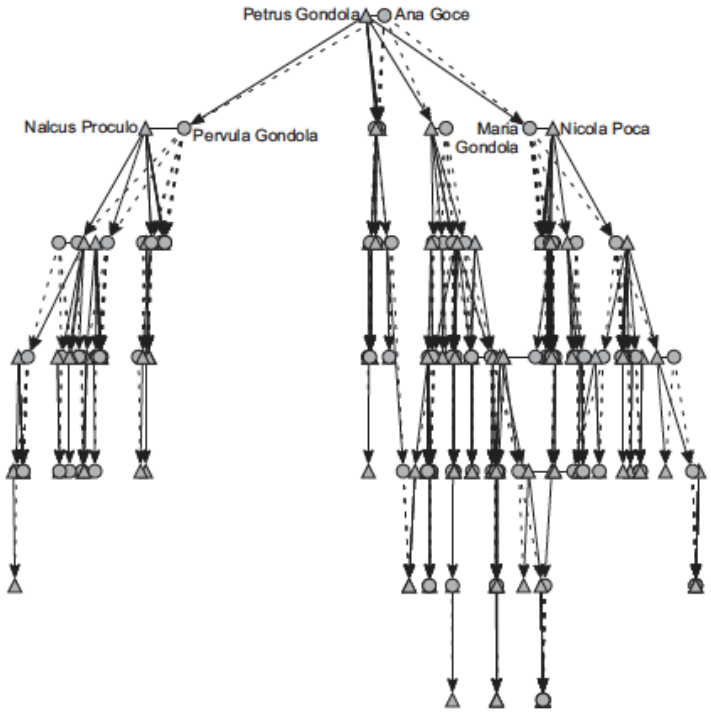


Figure 129: A conventional kinship network model of temporal flow in the measurement of kinship generations (from Nooy *et al* 2011:264), Compare with Ore graphs presented in the previous chapter.

Nooy *et al* (2011) explain the representation of time in kinship modelling as follows:

> *Time is responsible for a special kind of asymmetry in social relations, because it orders events and generations in an irreversible way. Social identity and position is partially founded on common ancestors, whether in a biological sense (birth) or in an intellectual manner: citations by scientists or references to predecessors by artists. This is social cohesion by common descent, which is slightly different from cohesion by direct ties. .... Social communities and intellectual traditions can be defined by a common set of ancestors, by structural relinking (families that intermarry repeatedly), or by long-lasting co-citation of papers. Pedigree is also important for the retrospective attribution of prestige to ancestors. For example, in citation analysis the number of descendants (citations) is used to assign importance and influence to precursors. Genealogy [i.e. kinship] is the basic frame of reference here, so we discuss the analysis of genealogies first (Nooy et al 2011:259).*
>
> *...*
>
> *In a representation of a genealogy as a network, [descent relations are represented as] arcs between parents and children. In a sociogram of kinship ties that is known as the Ore graph [Figure 128], men are represented by triangles, women by ellipses, marriages by (double) lines, and parent– child ties by arcs. Note that the arcs point from parent to child following the flow of time (Nooy et al 2011:262).*

By creating a series of temporal vectors that index sets of vertices according to time flow, diffusion and generational modelling allow analysts to measure the change of network structure in response to preceding changes, such as by the publication of a new text, which is subsequently cited by other authors, or by the birth of a child who goes on to have their own children, grandchildren and great-grandchildren. However, whereas the 'moments' in time are conventionally used in diffusion modelling, they are not conventionally used in generational modelling. Network analysis of kinship phenomena such as generational 'skewing' (see previous chapter) begins with a calculation of generational membership, based on existing real and/or classificatory ties between spouses. In network studies of skewing, time flow is treated a secondary attribute of the network, and is typically limited to the calculation of age-difference between spouses as a factor influencing the deliberate logic of skewing (see e.g. Denham 2012; White & Denham 2008; White & Jorion 1996).

5.1.2 Geographic Distance as a Tie Attribute: Multi-Dimensional Metric Scaling

Possibly the most thorough general discussion of space in the context of conventional social network analysis is contained in Scott's (2000) explanation of multidimensional metric scaling (MDS). MDS is a system of measurement that uses tie attribute vectors to determine tie length, such that all distances between vertices in a connected graph will be proportional to the value of those vectors. Although this is a measurement technique that borrows from geometric theory, it is not based on physical geometry. Rather it uses geometric techniques to generate 'analogies' to physical geometry, as Scott explains:

> *The basic idea behind multidimensional scaling (MDS) is that of using the concepts of space and distance to map relational data. Any model of space and distance in which there are known and determinate relations among its properties is termed a 'metric'. A metric framework has rather interesting characteristics so far as the mapping of relational data is concerned. If a configuration of points and lines can be made into a metric map, then it is possible to measure 'distances' and 'directions' in ways that differ from those of graph theory. In graph theory, the distance between two points is measured by the number of lines in the path that connects them. Distance, then, is measured as 'path distance'. The metric concept of distance is much closer to the everyday understanding of physical distance. In a 'Euclidean' metric, for example, the 'distance' from A to B, which is exactly the same as the distance from B to A, is measured by the most direct route that can be taken between them.' It is a distance that follows a route 'as the crow flies', and that may be across 'open space' and need not - indeed, it normally will not - follow a graph theoretical path. ... MDS, at its simplest, is an attempt to convert graph measures such as path distance into metric measures analogous to physical distance (Scott 2000:148).*

Scott here acknowledges that the graph theoretic techniques underpinning social network analysis do not sufficiently incorporate geometry to allow for the direct measurement of space. Although he

does not mention the existing spatial properties of graphs, which emerge from the measurement of tie strength (see Nooy *et al* 2011*,* above), Scott does note that the absence of geometric techniques limits the analytical power of SNA. The solution offered MDS, Scott explains, derives from its ability to generate spatial 'analogies':

> *Using the output from an MDS program, a configuration of points can be plotted within a space defined by the number of dimensions discovered. It is then that the process of interpretation can begin. There are two issues for interpretation: the meaning of the dimensions and the significance of the spatial arrangement of the points. In an atlas map, for example, the dimensions can, in general, be treated unambiguously as the East/West and North/South dimensions of ordinary geographical space. With models of social networks, however, the initial task of the analyst will be to arrive at a sociological interpretation of the dimensions. It may be decided, for example, that one dimension reflects the economic resources of individuals, while another reflects their political affiliations (Scott 2000:162).*

Scott here distinguishes 'ordinary geographic space' as separate from 'sociological interpretation'. As with the prioritization by Nooy *et al* (2011) of graph and set attributes over the observable geographic properties of attributes and relations, Scott treats geometry as a useful visualization technique rather than an inherent feature of the vertices and ties being visualized. Like the 'non-metric' application of x and y vectors to vertices for the cosmetic representation of trade ties, or the application of partitions to successive generations in genealogies and citation networks, MDS represents an application of a time-space 'analogy' rather than a real measurement of real time flow or geographic distance.

Conventional SNA techniques thus provide insufficient functions for modelling and analysing social relations in real-world space-time. While diffusion analysis and dependent measures (such as main path analysis) offer powerful strategies for detecting the influence of time on networks, there is no equivalent technique for measuring the distribution of networks in real geographic space on networks, or the influence of real geography on network formation. The following section documents counter-directional efforts among human geographers to incorporate models of social relations into well-developed geometric models of time-space. In an almost complete absence of network theoretic techniques, they develop a set of formal geometric techniques that are perfectly suited to the specialization of networks.

5.2 Modelling Human Interactions in Geography

In the late 1960s Swedish geographer Thorsten Hägerstrand discovered that it was possible to formally model the spatiotemporal signatures of individual people and communities, by observing the multiple points that individuals occupied in geographic space at each in a series of moments in time, both in isolation and when interacting (Hägerstrand 1968; 1970). Hägerstrand used standard Euclidean vectors to generate these models in three dimensions, with x and y vectors denoting points on a geographic field and z vectors denoting points in time. This modelling yielded two types of geometric structure: space-time paths, and space-time prisms (see Figure 131). With regard to the former, each path described an individual's trajectory through space-time over a given period. With regard to the latter, the volume and distribution of the structure described the multiple areas that could have been occupied by an individual at each in a series of points in time, based on an extrapolation of the distances they did actually travel over the same given period. Each point along a space-time path was designated an 'activity point', while each path segment connecting a pair of adjacent activity points was designated a 'path segment'. Hägerstrand called this modelling technique 'time geography', and it was pursued by a number of European and US geographers exploring potential real-world applications, including most noticeably, contributors to the 1978 volume *Timing Space & Spacing Time* (Carlstein *et al* 1978) and the 1981 volume Space and Time in Geography (Hägerstrand & Pred 1981), particularly Parkes & Wallis (1978), Lenntorp (1978) and Pred (1981a; 1981b). As with Levi-Strauss' (1969[1949]) earlier discoveries regarding the complexity of kinship structure (see Chapter 2), Hägerstrand's innovation in the comprehension of human movement pre-dated the emergence of the computing power necessary to refine and comprehensively graph his models. Over the last 20 years however, with the proliferation of location-based services (LBS), such as mobile phone networks, and location-aware technologies (LAT) such as GPS, time-geography has resurged within the discipline of human geography, as both a theoretical framework for detecting patterns of human activity and interactions in space-time, and as a data model for observing that activity.

While most of the contemporary applications of time geography continue with the predominant theme of Hägerstrand's original research (e.g. the maximization of transport efficiency under the constraints of existing route networks and time limitations), three researchers have pursued a secondary theme - the formal relationship between space-time geography and social interaction. Miller (1991; 1998; 2005), Shaw, and Yu (Shaw & Yu 2009; Yu & Shaw 2011; Yu 2006) have made significant progress on this front, with Miller providing formal descriptions for the models discovered by Hägerstarnd (1968; 1970) and refined by Lenntorp (1978), Pred (1981a; 1981b) and others, and Shaw and Yu providing the conceptual basis for the integration of formal time-geographic models and social network theory. In the current section, the general concept and formal modelling of time-geography will be introduced and described, and then shown to provide crucial elements for

incorporating measurements of space-time topography into the existing network theoretical techniques for modelling the topology of relational social structure.

5.2.1 Hägerstrand's 'Time-Geography'

In his first English language paper on time-geographic modelling and analysis, Hägerstrand describes the technique as a geographic recognition of three 'families of constraint' on human activity: 1) 'capability'; 2) 'coupling', and; 3) 'authority'. These operate in a hierarchical manner on the movement of individuals through space-time. The movement itself is comprised of 'activity bundles', which are ordered in temporal series to form directed 'space-time paths'. All of these elements - families of constraint, activity bundles, and space-time paths - exist in a relational state or 'socio-economic web' (Hägerstrand 1970:10). This is a system whose net effect, according to Hägerstrand, is the prevention of entropy in both personal and social human culture. He introduces the basic principle of individual spatiotemporal 'counter-randomness' a follows:

> *"In time-space the individual describes a path, starting at the point of birth and ending at the time of death. ... The concept of a life path (or parts of it such as the day path, week path, etc) can easily be shown geographically if we agree to collapse three-dimensional space into a two-dimensional plain [sic] or even a one-dimensional island, and use perpendicular direction to represent time. In a Garden of Eden environment … the path could be a true time-space random walk. In a more earthly environment it cannot be so … Assuming that continued survival is the first choice of those who have already set out on their life path, then some sort of counter-randomness programming has to occur" (Hägerstrand 1970:10-11).*
>
> *"It would be impossible to offer a comprehensive taxonomy of constraints seen as time-space phenomena. But three large aggregations of constrains present themselves. The first of these could be tentatively described as 'capability constraints', the second as 'coupling constraints,' and the third as 'authority constraints' …*
>
> *"Capability constraints' are those which limit the activities of the individual because of his biological construction and/or the tools he can command. Some have a predominant time orientation [such as sleeping an eating] … Other constraints are predominantly distance oriented, and as a consequence enable the time-space surrounding of an individual to be divided into a series of concentric tubes or rings of accessibility, the radii of which depend on his ability to move or communicate and on the conditions under which he is tied to a rest place … It follows him as a shadow when he moves. Two such tubes can never be brought to coincide completely but they have to be close to coincide for procreation, nursing, and some kinds of play and fighting … Food has somehow to be brought inside the tube at regular intervals" (Hägerstrand 1970:12).*

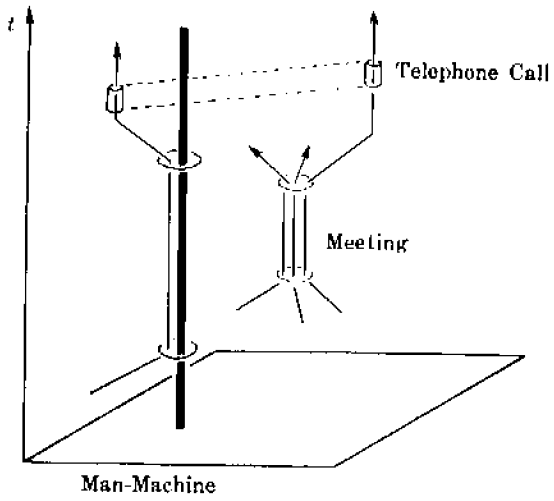


Figure 130: Hägerstrand's (1970) model of individual space-time paths and activity bundles.

Figure 130 shows a reproduction of Hägerstrand's intuitive, non-formal graph of the two key metaphors in his explanation, with a horizontal plane indicating a geographic field and a perpendicular arrow *t* indicating a one dimensional linear progression of time. The directed 'paths' follow an inexorable upward trajectory ordered by *t*, but exhibit variable positions on the horizontal geographic plane. These paths are 'bundled' together at two points in the diagram, by a set of four 'rings' that indicate activities: In the foreground of the horizontal plane a man and machine are bundled together by two rings, while in the background, three individuals are bundled together in a meeting. In the upper area of the graph, the man and an unspecified individual interact remotely via a telephone call.

Although the idea is not elaborated either here or in any of his subsequent texts, it is highly pertinent that Hägerstrand acknowledges the necessity for individual 'life-paths' to 'coincide' for procreation and child-care. The clear convergence of this geographically oriented 'coupling constraint' with the well-established kinship network concept of marriage, described most recently by Hamberger *et al* (2011), is elaborated below. In order to grasp the full methodological functionality of this convergence, it is necessary to move beyond 'life-paths', and to describe Hägerstrand's views on the aggregate manifestation of individual paths as spatiotemporal prisms:

> *"If we look closer at the time-space volume within [an individual's] reach, it turns out to be not a cylinder but a prism. It not only has a geographic boundary; it has time-space walls on all sides. Depending on where the stops are located and how long they last, the walls of the prism might change from day to day. However it is impossible for the individual to appear outside the walls. …*
>
> *"Wherever the location and duration of stops inside the daily prism, the path of the individual will always form an unbroken line. He cannot pass a certain point in time-space more than*

> *once but he always has to be at some point. Over a lifetime he steers his path though a string of daily prisms, growing in radius during the earlier years of his life and shrinking at an advanced age. Life becomes an astronomically large series of small events, most of which are routine and some of which represent very critical gates.*
>
> *"The path inside a daily prism is to a pronounced degree ruled by 'coupling constraints'. These define where, when and for how long, the individual has to join other individuals, tools and materials, in order to produce, consume, and transact. … We may refer to a grouping of several paths as a 'bundle'" (Hägerstrand 1970:14).*

Thus for Hägerstrand social interactions also form a type of constraint or 'counter-randomness programming', as quoted above. At a macro-level, the basic requirements to a) produce sustenance for life-paths, and; b) reproduce new paths, impose structure on the trajectory of each individual path, as well as on the aggregate trajectories of multiple paths. 'Prisms' are the graphic manifestation of this structure in Euclidean space-time, and may represent the structure of constraint either on individuals or communities. It is the interaction between this aggregation of paths, 'capability constraints', and 'coupling constraints', that gives rise to the final element in the hierarchy of Hägerstrand's 'socio-economic web':

> *"The third family of constraints, which I would like to discuss, relates to the time-space aspects of authority. The world is filled with a device which we may call the 'control arena' or 'domain'. These words are essentially spatial. However I would suggest that the concept of a domain be redefined to refer to a time-space entity within which things and events are under the control of a given individual or a given group. … The purpose of domains seems to be to protect resources, natural as well as artificial. … In time-space, domains appear as cylinders the insides of which are either not accessible at all or are accessible only upon invitation or after some kind of payment, ceremony, or fight …"*
>
> *"Thus, there exists a hierarchy of domains and certain kinds are beyond escape. Those who have access to power in a superior domain frequently use this to restrict the set of possible actions which are permitted inside subordinate domains" (Hägerstrand 1970:16).*

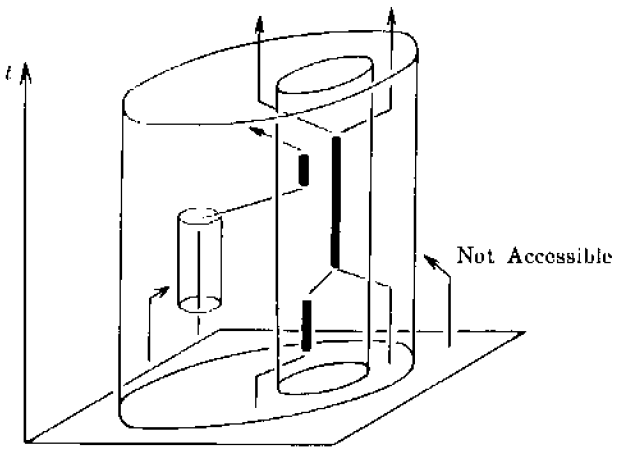


Figure 131: Hägerstrand's (1970) hierarchy of space-time domains, with spatiotemporal prisms used to graph three types of constraint.

This third form of constraint, the 'domain', is possibly the most abstract in Hägersatrand's model, since it represents an extrapolation rather than an interpolation, from observed 'activity bundles', of a structure which is itself not observable. Figure 131 illustrates all three forms of constraint operating in conjunction. The shaded prisms designate capability and coupling constraints, according to whether paths diverge to multiple geographic locations or converge on a single location. The clear prisms designates a 'control arenas' encompassing individual, convergent, and aggregate individual paths, to the putative exclusion of others. The narrowest clear prism in the foreground encompasses an individual path to the exclusion of another individual path. The next widest prism encompasses a convergent segment of two paths, as well as a divergent segment of a third path. The widest prism encompasses all segments of three paths but excludes two other paths.

As with the intuitive metaphors of 'life-path', 'activity bundle', 'capability constraints' and 'coupling constraints', the analogy with social network analytic models is obvious, insofar as a system of paths and vertices represent the topology of social interaction. Similarly, the metaphor of a geometric 'prism' representing constraints on the creation, maintenance, convergence and divergence of sets of these paths, can be described as analogous to the effects of preferential attachment on network structure, for example. As described by White & Johansen (2005) and White *et al* (2004), simple mechanisms for directing the formation of certain ties over others, will constrain, or rather *give rise to*, particular network structures. As with Hägerstrand's brief mention of 'procreation' and 'nursing' as critical types of coupling constraint, the aggregate effects of path interaction as patterns of network structure are elaborated below. One final aspect of Hägerstrand's model must be detailed first however: his recognition, inherent in the concept of time-geographic constraint, that both individual and aggregate patterns in spatiotemporal paths and prisms, are a product of choice as well as constraint.

> *"Viewed in a space-time perspective, then, we have two diverse systems in interaction. One is the predominantly time-directed warp of individual life-paths, which make up the population of an area and the concomitant capability constraints. The other is the more space-oriented set of imposed constraints of domains and bundles to which the individual may or may not have access according to his needs and wants. The population forms a kind of traffic flow in a road net with generally rusty gates. Loose ends of life-paths either have to discover for themselves how to open new roads and domains, locally or after migration into more permissive regions, or they disappear. … Social scientists know very little about interactions of constraints, as seen from the point of the life-path of the individual" (Hägerstrand 1970:19).*

> *"Given the list of needs and their statistical biography, it would be the task of the analyst to try and find out how much of various kinds of livability items would be simultaneously attainable under various assumptions of technical, economic and spatial organization. And since, once born, everybody has to be somewhere, everybody should be included in the picture, the child as well as the entrepreneur. … A time-space web model in the sense of a flow of life-paths, controlled by given capabilities and moving through a system of outside constraints which together yield certain probability distributions of situations for individuals should be applicable to all aspects of biology, form plants, to animals to men. However, … it is only men who can to a large extent choose between different constraints … The choice of constraints has always been a very piecemeal affair … History and cultural anthropology show that it is possible to live under a tremendous variety of constraint systems, even if all have specific drawbacks, as seen from the viewpoint of individual survival and welfare" (Hägerstrand 1970:20).*

As explained below, Hägerstrand's theoretical innovation is hampered by a lack of formalization, exposing it to the kind of corruption caused by weak grammar and horizontal knowledge structures typical of social science, as described by Bernstein (1996; 1999) in Chapter 1. Certainly, this lack of coherent vertical integration in the language of human geography or 'regional science' discourse of the late 1970s and early 1980s, is evident in the 'elaboration' of time-geography by various contributors to Carlstein & Parkes (1978) and to Pred (1981), where the core elements of Hägerstrand's model - activity bundles, paths, and constraints - are re-interpreted with weak formalization or no formalization at all. Possibly for this reason, there is no clear evidence in any of the time-geography literature from 1970 to the present (apart form one text by Parkes & Wallis 1978[65]), that indicates recognition of the direct fit between Hägerstrand's model of social interaction,

[65] Parkes & Wallis (1978) describe the only formal network-analytic model of time-geographic activity discovered in the review of related literature for this thesis. In their study of 22 people, they use set and graph theory to model individual 'day-paths' over a 24 hour period, although critically, without any spatial dimension. Rather than using conventional social network analytic techniques to graph observed sets of relations between observed sets of people, however, Parkes & Wallis graph relations between people and 'events types'. There are two weaknesses in this study that make its data and theoretic models unsuitable to application in the current thesis. The first is that the 'event types' themselves were not observed during the

and a network analytic model of social interaction. This is particularly unfortunate since Hägerstrand ultimately noted the need for formal mathematical description of the time-geographic concept in order to apply it beyond simple simulations:

> *"It has been said before that the choice of constraints has always been a piecemeal affair. Even in theoretical studies, social scientists have tended to take them for granted according to available experience. With a suitable technique for grouping constraints in time-space terms, one could perhaps hope to be able to boil down their seemingly endless variety to a tractable number. Simulation comes to mind as a way of analysis until more general mathematical tools become available" (Hägerstrand 1970:21).*

5.2.2 Miller's Formalization of Time-Geography

Citing as a motivation the lack of a formal description in the work of Hägerstrand and other early pioneers of time-geography, Miller (2005) presents a number of graph theoretic statements that might form the basis for further development. In particular, he establishes definitions for the core elements of time geography as follows:

a) Control Points c ('activity bundles' in Hägerstrand's terms);
b) Path Segments S, forming links between sets of control points C;
c) Paths P, formed by chains of path segments;
d) Potential Path Area PPA, formed by the aggregation of the greatest possible 2-dimensional areas reachable from each control point in a given path, according to the greatest length of existing adjacent path segments;
e) Potential Path Space PPS ('space-time prism' in Hägerstrand's terms), formed by the aggregation of the greatest possible distances reachable from each control point in space-time, according to the greatest length of existing adjacent path segments;
f) Potential Path Tree PPT, formed by the aggregation of all potential paths through a given potential path area.

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study, but rather pre-defined, and then used to classify individual observed events, which themselves do not form unique vertices. These 'event types' have neither an inherent temporal or spatial value. This of course, constitutes the positivistic imposition of an external proposition and therefore the corruption of independently existing patterns in the data, as described by Leaf (2007) and Read (2008). The second weakness in Parkes & Wallis' application of network theory to a time-geographic study is that they ignore Hägerstrand's central theoretical premise, that the time-geography represents the interaction between individuals in space *and* time (1970). By observing exclusively the temporal paths of isolated individuals in, over a scale as short as 24 hours, without any geographic dimension, Parkes & Wallis miss the opportunity to set a precedent for the convergence for time-geography as originally described by Hägerstrand (1970), and social network analysis.

Miller's (2005) formalizations provide two important set- and graph-theoretic statements for describing a structure that has topographic attributes, rather than topological attributes, as network statements do. Firstly, he established that sets of vertices and ties have real spatial and temporal attributes that can be observed and measured using vectors (see Equation 21 and Equation 22). Secondly, he shows that these ties have relational properties that bind sets of vertices together (see Equation 23 and Equation 24). These two statements are then unified in order to generate a single formal geometric statement with relational properties (see Equation 25). The following definitions provided by Miller (2005) comprise a combination of algebraic equations and worded explanations in the context of a single extended quote. Re-worded explanations have been added to my own captions to provide further clarity:

> *"A space–time path consists of two major components: (i) a sequence of control points and (ii) a corresponding sequence of path segments connecting these points. Control points are measured locations in space and time. The minimum information available at each control point is its location and the time when the location was recorded:*

$$c_i \equiv c(t_i) = x_i$$

Equation 21: Formal definition of a control point c, ordered in time *t* and space *x*. c_i is congruent to the convergence between t_i and x_i (Miller 2005).

> *"Where x_i is a location in space and t_i is a moment or instant in time. The set of control points that determine the path are a finite list of space–time observations strictly ordered by time:*

$$C = \{(c_S \dots, c_i, c_j \dots, c_E) | t_S < \cdots < t_i < t_j < \cdots < t_E\}$$

Equation 22: Formal definition of a set of control points C, each of which is defined as above (Miller 2005).

> *"Where t_S, t_E are the start time and end time (respectively) for the path, that is, the first and last observed locations.*
>
> *"Control points can correspond to three types of locations in the real world: (i) known activity locations; (ii) locations where the path experiences a change in direction (e.g., a turn at a street intersection) or velocity; and (iii) arbitrary locations where a spatial reference and time stamp are recorded" (Miller 2005:26).*

…

"Path segments are the unobserved locations in space that connect temporally adjacent control points. Given the information available at the control points, the simplest representation of the unknown observations is a straight line segment between observed points (Moreira, Ribeiro, and Saglio 1999; Pfoser and Jensen 1999). We can define the unobserved segment as an interpolation between adjacent control points using time as a parameter (Miller 2005:26):

$$S_{ij}(t) = (1 - \alpha)x_i + \alpha x_j$$

Equation 23: Formal definition of a path segment S, ordered in time t and space x, where α is the difference between two point in t and x (Miller 2005).

Where:

$$\alpha = \frac{t - t_i}{t_j - t_i}$$

Equation 24: Formal definition of the passage of time α, as the differene between two points in time, t_i and t_j (Miller 2005).

The entire space-time path $P(t)$ consisting of the set of control points C and connecting one segment S_{ij} is thus described:

$$P(t) = \begin{cases} C_i, & t \in (t_S, \dots, t_i, t_j, \dots, t_S) \\ S_{ij}, & t_S < t < t_E \end{cases}$$

Equation 25: Formal definition of a space-time path P comprised of sets control points C and sets of path segments S (Miller 2005).

This set of statements comprising Equation 21-Equation 25, allows the graphing of all the control points and path segments as shown in Figure 132, where the 2-dimenional geographic plane x has been collapsed into a 1-dimensional axis (Miller 2005:27). If the geographic plane is represented in 2-dimensions, the graph takes on a more intuitive appearance closer to that of Hägerstrand's originals images, see Figure 133.

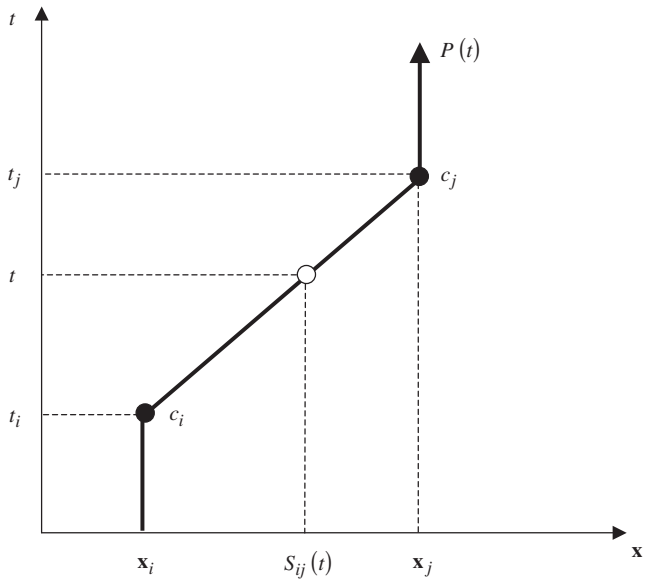


Figure 132: 2-dimensional graph of a formalised space-time path *P(t)*, comprised of time *t*, space *x*, control points *C*, and path segments *S* (Miller 2005:27).

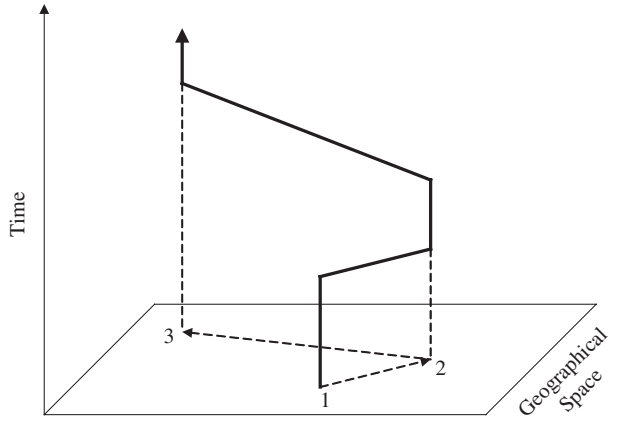


Figure 133: A 3-dimensional version of the data represented in Figure 133 (Miller 2005:20).

Miller's (2005) formalization of Hägerstrand's (1970) theoretical time-geographic principles offers clear applications for the modelling of spatialized social and kinship networks. This is because sets of control points and path segments as defined by Miller (2005), are structurally equivalent to sets of vertices and ties as defined by Hamberger *et al* (2011), while space-time paths as defined by Miller (2005) are structurally equivalent to types of pair as defined by Hamberger *et al* (2011). The difference, is that whereas the social network definition of a graph *G* is comprised of vertices V, pairs of vertices *P*, and types of pair *T*, the time-geography definition of a path *P*, is comprised of control points c, sets of control points *C*, and path segments *S*. Path segments are assumed to be of a single type, since they describe the movement of a single individual. While the inter-operability of these

two formal models is elaborated in Section 5.3, it is apparent that Miller does not make the relational properties of time geography explicit. In the next part of Section 5.2,, recent work by two other human geographers, Yu and Shaw (Shaw & Yu 2009; Yu & Shaw 2011; Yu 2006), will be shown to elevate and specify the relational properties inherent in time-geography to a formal standard compatible with SNA.

5.2.3 Yu & Shaw's Extension Of Time-Geography to Spatiotemporal Relationships

Citing the progress made by Miller (2005) in formalizing the key elements of time-geography inherent in Hägerstrand's theoretical propositions of activity points and path-segments, Yu (2006), Shaw & Yu (2009), and Yu & Shaw (2011) proceed to emphasize the further need for formalizing the relational properties that these structures exhibit. Building on Hägerstrand's original theory of "activity bundling", whereby individual space-time paths intersect (Hägerstrand 1970; Miller 2005), Yu (2006) sets out a clear framework for the application of time-geography to the modelling of 'human interaction'. This framework consists of two analytical functions: 1) the classification of social interaction according to spatiotemporal attributes of that interaction, and; 2) using these classifications to interpolate otherwise unobserved interactions. Both functions operate by defining social interaction in terms of semantic exchange. Yu (2006) explains the relational properties of time-geography as follows:

> *"Human interactions can be considered as special cases of human activities. While an activity can be conducted by any number of individuals an interaction involves multiple individuals, each of whom has to provide certain action to participate in the interaction. … Semantic contents of interactions present an effective approach to the organization of interactions. Each interaction has its specific semantic content (e.g. a meeting). The semantic content is shared by all individual activities, each of which represents the involvement of a participant in the interaction, even though individual activities may have different spatiotemporal characteristics. Using sematic content, several individual activities sharing the same semantic content can be grouped together to represent an interaction among the individuals. In addition, the spatiotemporal pattern of an interaction can be determined by examining the spatiotemporal relationships of individual activities under the same semantic content" (Yu 2006:10)*

Yu (2006) thus adapts the time-geographic object classes identified by Miller (2005) as follows:

a) Individual activities, defined by discrete spatiotemporal 'stations' $< x_i, y_i, t_i >$ (equivalent to Miller's 'control points')
b) Individual space-time paths and constituent path segments, defined by sets of spato-temporal stations ($\{< x_0, y_{0,}, t_0 >, < x_1, y_{1,}, t_1 >, \ldots , < x_n, y_{n,}, t_n >\}$, (equivalent to Millers paths and path-segments)
c) Semantic content (the semantic attribute of an activity, such as participating in a meeting or sending an email)
d) Shared activities, defined by shared semantic content (e.g. participating in a meeting, or being the sender or recipient of an email)

These elements are classified by Yu (2006), as well as later by Shaw & Yu (2009), and Yu & Shaw (2011), according to four classes of spatiotemporal conditions that are necessary for shared activities to occur:

a) Co-existence (synchronous physical presence)

b) Co-location in space (asynchronous physical presence)

c) Co-location in time (synchronous tele-presence)

d) No co-location requirement in either space or time (asynchronous tele-presence)

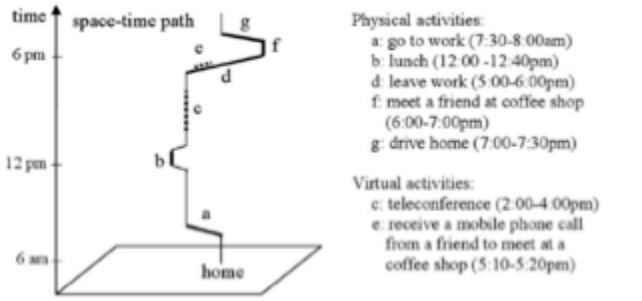


Figure 134: Example of an individual space-time path and constituent segments (Shaw & Yu 2009)

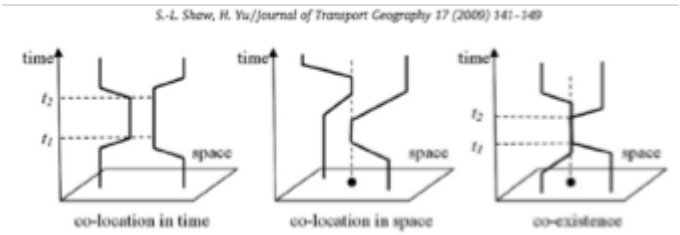


Figure 135: Three classes of social interaction mappable using time-geography (Shaw & Yu 2009)

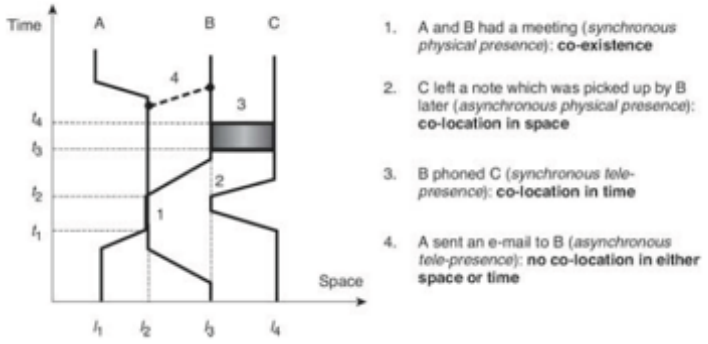


Figure 136: An elaborated example of social interaction constituted by the convergence of three individual space-space time (Yu & Shaw 2011)

These four classes of relational activity are rendered in 3 dimensions in Figure 134 and Figure 135, and in 2 dimensions in Figure 136. Figure 134 illustrates an individual space-time path consisting of observed activities, and adjacent path segments. Figure 135 illustrates three classes of interaction between multiple paths: 1) co-location in time, where both of two individual paths consist of activities observed at the same moment in time, but separated in space; 2) co-location in space where both of two individual paths consist of activities observed at the same place, but separated in time, and; 3)

co-location in space and time, where both of two individual paths consist of activities observed at the same moment on time as well as the same place. Figure 136 illustrates the interaction of three individual paths by different classes of interaction.

Shaw & Yu's (2011) definition of social relationships in this context, delimits those individual space-time paths necessary to unify a set of individual and joint 'activities' into a given, discrete 'project', according to the consistent semantic content of those activities.

> *"Besides representing each individual activity, a GIS design should be able to model the complex relationships among individuals and their activities to effectively support activity analysis. Two general types of relationships among individual activities can be identified. The first type includes relationships of activities among multiple individuals, which are usually recognized as interactions among people. For example, an interaction can be two groups of people having a conference call or a person sending an email to another person. The other type of relationship focuses on the sequential organization of activities of one or multiple individuals because the sequential order of activities presents a key to the understanding of the process of activities. ... Time geography uses the concept of project to define a series of activities conducted by a person or a group of people to achieve a specific goal. … A project can last for hours, days, or years and different locations may be involved in various activities associated with the project. When a project involves multiple individuals, interactions among the participating individuals can be carried out with different spatiotemporal modes [viz. SP, AP, ST, AT] that result in complex spatiotemporal patterns" (Shaw & Yu 2011:261-262).*

Under the heading of '*temporal linear referencing and dynamic segmentation method*' Yu (2006) provides a set of formal statements for describing the representation of these entities and their interaction:

> *"A spatiotemporal point feature, which occupies a single position in the 3D framework, is represented with a triplet* $< x, y, t >$, *where* x *and* y *are for a location in the 2D plane and* t *is used for time. A spatiotemporal line feature is represented as a sequence of triplets (*$\{< x_0, y_{0,}, t_0 >, < x_1, y_{1,}, t_1 >, \dots , < x_n, y_{n,}, t_n >\}$, *where* $t_0, < t_1 < t_n$*). [Figure 137] shows how these spatiotemporal features are represented in the 3D GIS framework" (Yu 2006:9).*

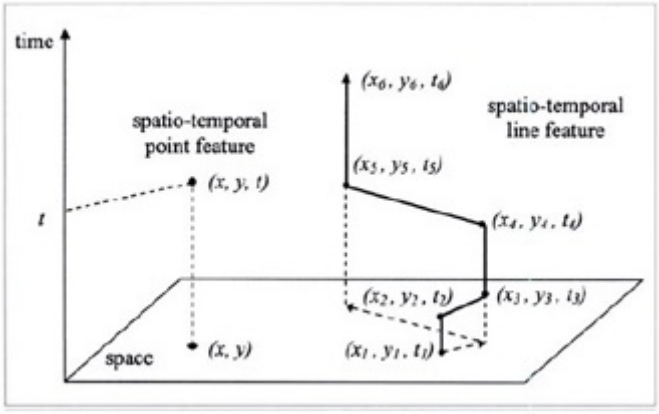


Figure 137: Yu's (2006) visualisation of space-time paths.

"A spatiotemporal line feature in the 3D GIS design can be directly used to represent an individual's space-time path. ... For instance, a straight-line segment on a persons space-time path is represented as $\{< x_1, y_1, t_1>, < x_2, y_2, t_2 >\}$ *and we want to find the person's location at time* t_0 *(where* $< t_1 < t_0 < t_2$*). The person's location in the 2D plane can be calculated using the following formulae (Yu 2006:9):*

$$x_0 = x_1 + (x_2 - x_1)\frac{t_0 - t_1}{t_2 - t_1}$$

$$y_0 = y_1 + (y_2 - y_1)\frac{t_0 - t_1}{t_2 - t_1}$$

Equation 26: Yu's (2006) formalization of space-time paths.

The difference between these formalizations and those provided by Miller (2005), is that whereas Miller treats space-time path segments as "unobserved locations in space that connect temporally adjacent [observed] control points" (2005:26), Yu (2006) treats space-time paths as "a linear referencing system to measure locations and *store attributes*" (2006:9 - emphasis added). Yu (2006) goes on: "Space-time paths are considered as *containers for human activities*. Both physical and virtual activities take place within their specific time-space contexts. Therefore, each activity, as an episode in one's life, occupies a portion on the space-time path of an individual" (2006:9). In emphasizing a formal means for *predicting* the location of an individual at an unobserved point in time t_0, Yu highlights the significance of the unobserved interstitial segments as well as observed points in an individual's path[66]. Unobserved path-segments are not meaningless, but rather *indicative*

[66] The asymmetric analytical significance attributed to unobserved space-time paths is inverse to the specificity of the formalizations provided by the two geographers because, whereas Miller (2005) gives a distinct definition of a space-time paths

of individual attributes. For example, multiple paths that intersect along unobserved segments indicate the shared attribute of co-existence. Even though in such circumstances neither individual is actually observed at the same point, their joint presence can be formally modelled. This is because the definition of interpolated intersecting space-time segments (where an interpolated triplet $< x, y, z >$ is *found* to be equal for multiple paths), is structurally equivalent to the definition of observed co-existence of multiple points (where the observed triplet $< x, y, z >$ is *seen* to be equal). 'Co-existence' in Yu & Shaw's (2011) terms, and 'coupling' in Hägerstrand's (1970) terms, emerges as a relational attribute from the interpolation of unobserved path segments, because the same spatiotemporal point is shown to have been occupied by more than one individual. Space-time is thus shown to be a class of relational attribute belonging to multiple people irrespective of whether it is observed. Because co-location in space-time is a criterion for semantic exchange, interpolation of co-location indicates that semantic exchange is *more likely*, even if it is not observed. Shaw and Yu (2009) put this as follows:

> *"The above example [illustrating a straightforward time-geographic model of observed interactions, such as at Figure 135 and Figure 136] shows how we could explore the interrelationships among activities, individuals, and locations if we have access to the detailed information of the entire spatiotemporal process. When such information is absent, analytical functions are needed to examine the spatiotemporal relationships. We have developed functions to analyze various spatiotemporal relationships among space–time paths, including the "co-existence", "colocation in time", and "co-location in space" relationships (Yu, 2006). ... These functions provide a foundation to analyze additional spatiotemporal relations. For example, the functions for analyzing the co-existence and the co-location in space relationships can be used to* ***derive*** *activity bundles and stations in physical space. [Figure 139] shows the space–time paths of a population migration history dataset of 2800+ individuals covering a 90-year period" (Shaw & Yu 2009:147 - emphasis added).*

Figure 139 clearly illustrates the analytical power of properly formalized relational modelling that time geography has to offer. The scale of the graph presented in Figure 139 depends not only on the self-consistent classification of observed time-geographic activities, but also on the secondary

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and comprised of three object classes (*viz.* control points c, sets of control points C, and path segments S) Yu (2006) gives a definition of space-time path segments that is built from variable sets of a single object class, space-time control points, without any additional object classes. The effect of this inconsistent asymmetry is that Miller (2005) provides a more comprehensive set of formalizations but a weaker explanation of their descriptive power, whereas Yu (2006) provides a more simple set of formalizations but a more elaborate explanation of their descriptive power. In the following Section 5.3, Miller's (2005) formalizations and Yu's (2006) descriptive framework are used in tandem.

classification of 'derived' activities that are implied by interpolated path-segments linking sets of activities together into complete individual paths, and sets of individual paths together into complex spatiotemporal networks. Without deploying an explicitly network analytic framework, Yu and Shaw nevertheless arrive at a network-oriented conclusion, *viz.* that consistent classes of attribute observed among interacting individuals, when modelled using formalized definitions of time-geographic elements, reveal complex patterns of individual behaviour at a micro-level, and complex patterns of interaction at a macro-level.

Incorporating Yu's (2006) formalizations, Yu & Shaw (2011), proceed to elaborate a technique for modelling the micro-macro linkage between individual activities and large-scale interactions in space and time. They achieve this by applying a data model of nested sets of multiple space-time paths within larger path structures termed 'projects'. A given project is defined by the distinct 'events' necessary for it to be completed, while each 'event' consists of the individual and social interactions contributing to its execution. The entire three-tiered system of related elements is explained as follows:

> *"[Figure 138] shows a space–time GIS model that presents an attempt to manage various relationships of activities. Each individual has one space–time path, which stores the person's observed movements in space and time. An individual can have multiple activities. Each activity can be associated with its corresponding space–time path through the aforementioned temporal linear referencing and dynamic segmentation method. An event object is included to help us track the interactions among individuals. Each event is composed of one or more activities. When an event involves multiple activities, it represents an interaction of multiple individuals. A project object is composed of multiple events that are organized in a particular sequence to achieve a goal set by an individual or a group of individuals. An event is allowed to be associated with more than one project because an event may function as a part of multiple projects. Interactions among individuals, events, and projects are modeled as relationships" (Yu & Shaw 2011:262).*

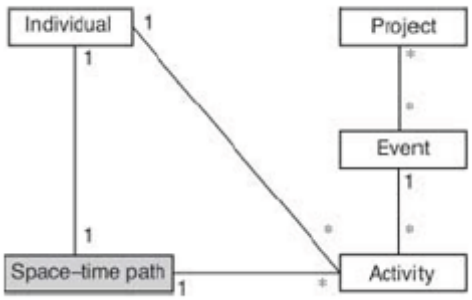


Figure 138: Shaw & Yu's data model for a relational implementation of time-geography (Shaw & Yu 2011).

This theoretical model not only consolidates the inherent relational basis of time-geography, but also gives more rigorous formal definition to the set-theoretic terms of that basis. It is this rigorous formalization that allows Shaw & Yu (2009), and Yu & Shaw (2011) to scale up simple models, such as those in Figure 134 and Figure 137, into highly complex and elaborate models representing the movements of thousands of people across deep time (90 years) and large areas of space (continental USA), as in Figure 139.

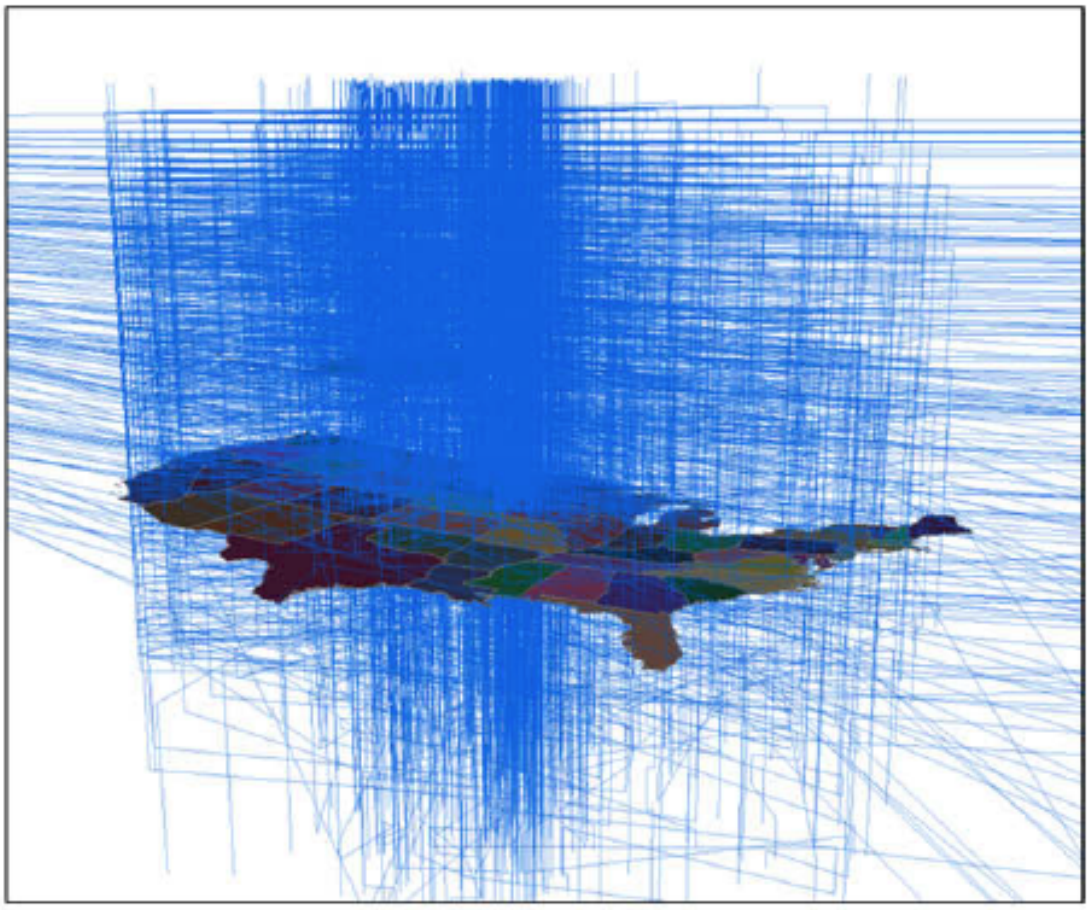


Figure 139: Visualization of >2800 individual space–time paths spanning 90 years and overlayed on a cadastre of the USA (Shaw & Yu 2009).

In their formalization of the relational properties of time-geography, Yu and Shaw set the basic parameter for social interaction as semantic exchange. From this threshold they identify four classes of space-time social 'event' which may 'contain' semantic exchange: a) co-location in space *and* time (co-existence); b) co-location in space; c) co-location in time, and; d) colocation in neither space *nor* time. These events form the 'building-blocks' from which groups of people construct space-time 'projects'. Projects, in Yu and Shaw's terms, are entirely semantic exercises. In terms of general social network analytic theory, this approach to modelling interaction corresponds with studies of network processes such as citation diffusion, wherein the semantic content of an original text and its citation in other texts is used to define a graph of the relationships between the original text and those

subsequently cited. Since Yu and Shaw start from a time-geometric basis rather than a network analytic one, it is important to give regard to the terms originally set by Hägerstrand (1970) when he defined the theoretical model. By restricting the relational properties of time-geography to semantic exchange, Yu and Shaw do not take up Hägerstrand's observations regarding the basic requirements for the existence of individual 'life-paths'. The fundamental parameter for individual existence is only one of the four classes of spatiotemporal event they identify, *viz.* the coupling constraint of co-location in space *and* time, necessary for physical reproduction, both of individuals, *and* of their means of production. As discussed in Section 5.2.1, co-location in space-time is the activity to which Hägerstrand (1970) gave the greatest emphasis, with all other identifiable activity constraints comprising attempts to lift the restrictions imposed by co-location (such as by making telephone calls instead of physically meeting with a person). For Hägerstrand, it was not exclusively the semantic content of social interaction that defined the classes of constraint operating on space-time activities, but rather both the semantic *and* material content of social interactions. This is because he considered it essential to recognize the *active participation* and *choice* of individuals and groups in setting activity constraints, both on others and on themselves (Hägerstrand 1970:20).

The following section summarizes the partly formalized propositions of Rose (2008a; 2008b; 2009; 2010), including the analytical techniques of 'event indexing' and 'structural event analysis'. In this approach to the modelling of spatiotemporal events, the goal is to identify patterns of interaction among classes of spatiotemporal event, not according to spatiotemporal attribute such as co-location, but rather according to the symbolic and material goals of event participants, such as marriage and child-rearing, hunting, structured ritual activity, etc. Although originally developed in ignorance of Hägerstrand's (1970) theoretical framework, 'structural event analysis' complies completely with the definitions he outlined in 1970 by retaining a focus on the interdependence of both semantic and physical constraint.

5.2.4 Rose's Spatiotemporal Model of Structural Event Analysis, and Proposed SNA Integration

Yu & Shaw's 'activity analysis' framework (Shaw & Yu 2009; Yu & Shaw 2011; Yu 2006), with its constituent space-time events, paths and projects, closely resembles the relational data model termed 'structural event analysis' (SEA), proposed and implemented by Rose (2008a; 2008b; 2009; 2010). In the latter model spatiotemporal 'events' are defined or 'indexed' according to the intersection of one or more individuals, one location and one instance in time (see Figure 140). 'Event types' are classified according to the explicitly stated material *or* semantic objectives of participants, such that large, complex sets of individual and collective events can be mapped in three-dimensional space-time at a general level, in order to provide useful subsets informative data. Examples of space-time events defined by these terms, include 'reproductive', 'economic' and 'symbolic' classes, such as and 'marriage' or 'birth', 'hunting', and 'ritual initiation', respectively. By setting such limitations on the definition of 'events', researchers may analyse the intersection of multiple event classes using

standard network analytic techniques. For example, a set of economic events such as all observed instances of hunting in a particular region over a particular period, can be compared with a corresponding set of reproductive events, such as marriages of individuals associated with the same region. Events that overlap in either or both space and/or time, indicate an underlying relational structure for the observed activity (see Figure 141). For example, if an individual is observed hunting in an area occupied by their spouse's family, then a structural event analysis will reveal a link between hunting and marriage events, such that both events correspond with the same location, although not the same moment in time. If a ritual event is observed occurring in the same place as the hunt, and that ritual includes the same individual again, then a further link will be revealed with that event. Existing network analysis software such as Pajek (see footnote 40) is suitable for this kind of modelling, provided that the event class definitions are consistent. Furthermore, because this approach defines kinship relationships as a class of relational event (specifically, 'reproductive events') the structural event analysis framework can accommodate comparison between economic and symbolic events, and existing kinship models.

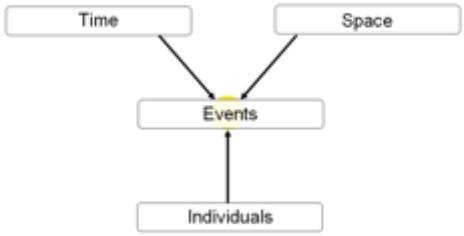


Figure 140: Formal definition of a space-time 'event' (Rose 2008)

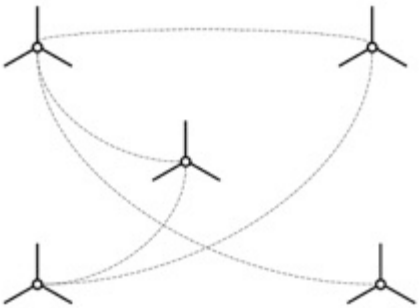


Figure 141: Analogic graph of relations between intersecting space-time events (Rose 2010).

The overlap between kinship events and other classes of relational events is described by Rose (2009), as follows:

"Event indexing is based on the interaction of time, space and agency where an event is described as the product of their intersection, i.e. the intersection of an individual, a place and a time ([Figure 142]). This is consistent with the tripartite model of land tenure where agency is realised in an individual, space is realised in areas of land and time is released in instances of history.

"This method can be used to test for different classes of event. For example, a resource event may be described as one whereby one man goes hunting on a particular area of land and kills a kangaroo at a particular time. As an indexed event, this manifests as the intersection of that man as an individual agent, the spot where he killed the kangaroo, and the time at which he killed it – one individual, one place and one time. The resultant event is the killing of a kangaroo. This is therefore a resource event. There may be more than one individual involved. There may be several men and women, which would mean that the method would detect several different events, one for each agent.

"These events can be described as related by the fact that they occurred at the same time and at the same place and were all resource events. This in turn unites the individuals involved in these distinct ways. It creates a network of relationships of particular kinds – temporal relations, spatial relations and relations of resource-use (see figure 3). The several different individuals are all in the same place at the same time and have all played a role in the killing of the kangaroo" (Rose 2009:3)

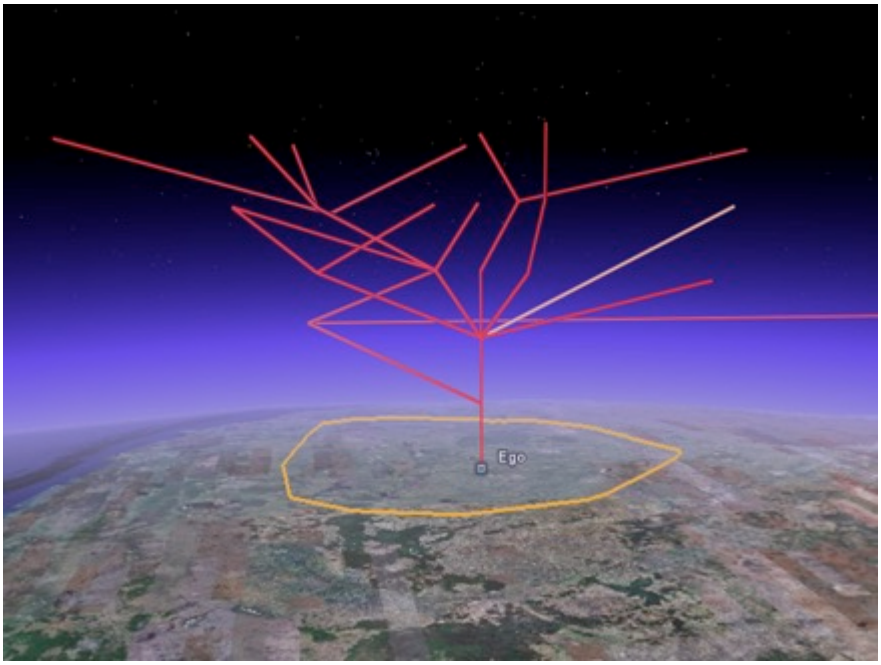


Figure 142: Rose's data model for a relational implementation of 'event indexing' (Rose 2008; 2009).

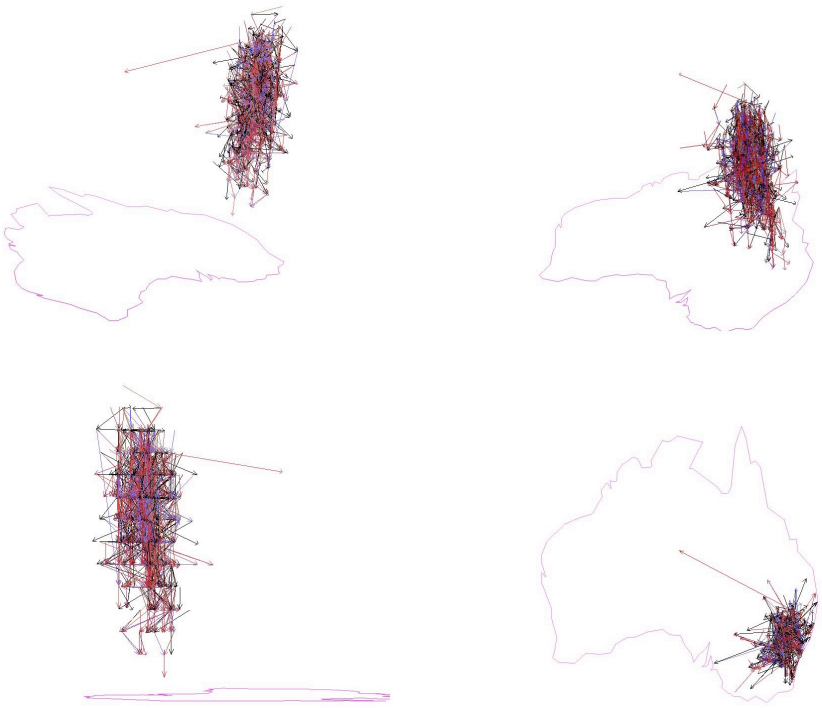


Figure 143: Large-scale implementation of 'structural event indexing' applied to >3000 birth events (Rose 2010).

At the time of writing, the structural event analysis framework has been actively implemented for over 10 years in the NTSCORP Research Unit Data Management System (see Section 4.3), which tracks over 150,000 primarily Indigenous-descended residents of South Eastern Australia between the late 18th century and the present, and identifies more than one million spatiotemporal events (see Rose 2008a; 2008b; 2009; 2010). This implementation has been used primarily with regard to the visualization of reproductive events, particularly births. Figure 142 shows an event index of all observed birth events corresponding with the set of individuals delimited by ancestry. Figure 142 shows the same classes of corresponding spatiotemporal events, scaled up to >3000 events. Until 2013 the primary goal of this implementation was to generate compelling evidence of Indigenous land-use and occupation, within the litigious context of Australia's Commonwealth Native Title Act[67], however initial legal responses have been muted, with concerns expressed that it is "too technical"[68].

[67] See Chapter 0 for contextual overview. SEA was developed in this context as a means for building reliable large-scale data sets and models for illustrating consistent, long-term occupation and resource-use among Indigenous Australians, within the geographic areas under claim.

[68] SEA was used to develop evidence for native title claims 'Gomeroi People' (NSD2308/2011) and 'Ngiyampaa People' (NSD2012/415). Junior internal legal staff advised that the relational patterns detected were 'too technical' and too 'data-

Irrespective of this specious legal view, the analytical power of approach is compelling and has garnered widespread interest among Australian research bodies, particular in regard to the spatialization functions that it offers to existing datasets with relational properties. This model is elaborated further in 5.3 below.

5.2.5 Time Geography and Social Networks

The theoretical model of time-geography is relational because the lines that Hägerstrand (1970) uses to describe space-time paths are, in formal terms, relations between vertices. In network analytic terms, each time-geographic graph of an individual's movement is being treated as the graph of a network whose vertices designate changes in migratory trajectory and whose ties designate the relations between those changes. Using geometric statements, Miller (1991; 1999; 2005) has developed formalizations for the modelling of time geography sufficiently rigorous to allow its application in location-aware technologies and location-based services. While his primary goal is predictive insofar as the aim is to infer likely trajectories between observed control points, he does not elaborate the constraining effect of these trajectories on the capacity for interaction between space-time paths, i.e. *social* interaction. This is because, as Miller (1999) in particular points out, the role of network modelling in this context is to solve efficiency problems in existing transport networks. Yu (2006) Shaw & Yu (2009), and Yu & Shaw (2011) partly redress this omission, by re-emphasizing the social basis of time-geography, the restrictions of space-time on social interaction, and provide formal statements for describing control points and paths which can be applied to a potential network analysis of spatiotemporal data. However, they too omit an explanation for how such an analysis might be implemented using the set- and graph-based statements of network analytic theory.

The primary concern among of these authors is with the efficient use of space-time as a means to an end, typically individual and collective travel. Consequently, Hägerstrand, Miller, Shaw, and Yu are all focused on the measurement of velocity and shortest possible paths through Euclidean space, which geometric algebra is well equipped to handle. Possibly for this reason, none of these geographers have branched into to the use of formal network analytic techniques to understand human movement through space-time. In any case, despite the early recognition by Parkes & Wallis (1978 - see footnote 65), and later confirmation by Yu and Shaw (Yu 2006; Shaw & Yu 2009; Yu & Shaw 2011), that social network analysis has a clear relevance to time-geography, there appears to have been no attempt to formally integrate the network theoretic techniques of SNA into time-geography.

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oriented', that respondent parties' experts would not be able comprehend those patterns or their significance, and that this would be detrimental to the prospects of the claim in the court.

Conversely, none of the measurements of space that are currently used in social network analysis provide sufficient means for attributing values to relations between people. Scott's (2000) description of metric multidimensional scaling (MMS) acknowledges that while line values may be derived from the metric distance between pairs of vertices (such as, in his example, the pre-recorded distance between two towns on a road map), there is no recognized technique for standardizing line measurements in three dimensions (Scott 2000:151). Similarly Nooy *et al* (2011) treat spatial distance in terms of dyadic attribute, where the function of measuring distance is the allocation of partitions ranking levels of physical proximity (Nooy *et al* 2011:44-56). In the accounts of both Scott (2000) and Nooy *et a*l (2011), then, physical distance is a category of relational attribute that is useful primarily for comparing *the effects of* ***abstract*** *dyad proximity on the emergent* ***topology*** *of a network*. This is in contrast to the *effects* ***of real-world*** *dyad proximity on the emergent* ***topography*** *of a network*. In conventional SNA there is no established means for making set-based of observation of real-world distance, such that the effect of that distance on the future structure of the network can be measured. This is quite different to the recognized usefulness of measuring distance in time-geography, where the value of such measurement lies in comparing *the effects of changing path direction on the topography of multiple interacting paths*.

In using a relational paradigm in the construction of spatiotemporal data models both Rose (2008a; 3008b; 2009; 2010), Yu and Shaw (Yu 2006; Shaw & Yu 2009; Yu & Shaw 2011) describe what is in fact a *two-mode network* (Nooy *et al* 2011:103; White & Harary 2001:313; Scott 2000:41), whereby a observed set of formally defined 'events' form bridging vertices that link together individuals in a network of indirect relationships, based on joint participation in those events. Although Rose (2008a; 2008b; 2009; 2010*)* draws an informal theoretical link between the demonstrated power of social network analysis applied to kinship graphs, and the potential for network analysis of relational spatiotemporal event data, no formalization has been provided to date.

The following section combines the formal definitions provided by Miller (2005) with the conceptual relational framework proposed by Yu, Shaw and Rose (Yu 2006; Shaw & Yu 2009; Yu & Shaw 2011; Rose 2008a; 2008b; 2009; 2010), to extend the network analytic statements provided by Hamberger *et al* (2011), such that it may be used to spatialize social network data. This exercise is brought applied to the kinship network data presented in the previous Chapter, specifically in relation to the terminologically defined circuits that were shown to lie at the heart of the matrimonial core presented in Section 4.3.

5.3 Integrating Time-geographic and Social Network Analytic Models

The previous two sections presented summaries of network spatialization techniques from the fields of social network analysis and time-geography. Section 5.1 showed that conventional SNA approaches to the measurement and analysis of space and time are limited to abstractions of tie strength and diffusion. While diffusion analysis provides an accurate metric for the real-world influence of time on social and kinship networks, there is no corresponding metric for the influence of real-world geography. Section 5.2 showed that within the field of human geography, recent efforts have been directed towards the formalization of time-geographic concepts, such that they may be used to measure the movements and interactions of multiple people within space-time, and consequently to detect the constraints imposed on those dynamics by space-time. While time-geography offers powerful geometric statements for modelling movement and interaction in space-time, its lack of a network theoretic framework prevents analysis of network phenomena that it detects.

The current section presents a set of general statements for simultaneously modelling kinship network and time-geographic structures in a single analytical framework, termed here spatiotemporal kinship analysis (stKNA). The primary focus is on the interaction between kinship structures and their spatiotemporal context, however the general nature of the statements is intended to allow for their application to any social network formation. Section 5.3.1 shows how the formal description of kinship networks described by Hamberger *et al* (2011) may accommodate the time-geographic metrics provided by Miller (2005), such that kinship networks may be modelled in space-time. Section 5.3.2 presents concrete examples of stKNA modelling applied the conventional KNA models developed in Chapter 4. The focus here is on those matrimonial circuits that co-occur both in the 295-person matrimonial core, and on the seminal terminological models of Ridley (1856; 1875) and Mathews (1895; 1897; 1905), presented in the same chapter. Patterning apparent in these following stKNA models is discussed with regard to principles of network cohesion, as distinct from the concept of spatiotemporal 'constraint' developed by Hägerstarand (1970). Section 5.3.3 discusses implications of this patterning for the standard model of Australian kinship, and the broader KLRE model of Australian sociality of which it forms a part, as discussed in Chapter 3. In particular, the tacit group theoretic assumptions that are held to predict conventional models of Indigenous society are shown to be superseded by the network theoretic processes detectable and measurable using the stKNA technique.

5.3.1 Space-time Paths as a geographic and historical metric for Kinship Networks

As seen in the previous chapter, the formal definition for a network graph, $G = (V, P, T)$ describes the constitution of a graph *G* by vertices *V*, pairs of vertices *P*, and types of pairs *T* (White 2004). In this model, a pair of vertices *P* designates the *tie* between two vertices, thus *P* may exhibit attributes, which can be classed by *T*. This is also how the attributes of relations are designated in a kinship

graph consisting of a limited set of vertices, edges and arcs. Where $G = (V, E, A\sim)$, V is a set of vertices, E is a set of edges linking spouses, and A is a set of arcs linking parents to children by five types of pair: H.F, FF, FF, HF and HH (Hamberger *et al* 2011). In a spatiotemporal graph there is no such pre-existing definition. In Miller's (2005) terms, all that is required is a set of control points ordered by time, and path segments linking those control points together. Thus, in the strict terms of Miller's formalization, an individual can potentially occupy any position on the surface of the Earth at any time during their life, without regard to the kinship relations that give rise to their existence in the first instance. What will be demonstrated here, is the efficacy of using Miller's (2005) model for a space-time path comprised of control points and segments, where each control point C forms a spatiotemporal value for V, and each segment $S_{ij}(t)$ forms a spatiotemporal value for either P, in the case of a general network graph, and for either E or A in the case of a kinship network graph[69].

An important distinction between the network theoretic definition given by Hambergeret *et al* (2011), and the time-geographic definition given by Miller (2005), is that Hamberger *et al* (2011) treat all elements in the set as *observed* (which is a fundamental requirement for kinship research where the constancy of relations forms the basis of the kinship structure), whereas Miller (2005) treats path control points *C* as observed, but path segments *S* as *un*observed (acceptable in geographic research where spatial observations track classes of vertices by linking together observations made of them). Hence Miller's (2005) inclusion of linear interpolation in describing the trajectory of each segment of a path (see Equation 23 and Equation 24). In the standard topological setting of kinship network analysis such interpolation is not necessary, since what matters is 1) whether or not a relationship *exists* between a pair of vertices, and; 2) what *type* of relationship it is (where the attribute of the relation is generated by the pair of vertices that it links together). In time-geography on the other hand, while the existence of the relationship is critical, there is an implicit assumption that it can be of only one type, namely the physical trajectory of an individual *through* space and time. There is thus no need to specify a path type, except to say that it represents the movements of a given individual as against any other individual. This is one of the formal reasons that kinship network analysis and time-geography may be described as ideally consilient – both provide specific functions to the other which greatly enhance their descriptive and analytical power.

While linear interpolation may be treated as superfluous to measurement of network topology, it remains necessary for the fuller application of time-geographic modelling to kinship networks, because while network graph definitions describe the *existence* of particular arcs and edges, they

[69] While Yu's (2006) conceptual model is more comprehensive than that of Miller (2005), particularly in its relational elaboration, Yu's set-based statements are not as comprehensive as Miller's. In particular, Yu does not provide a specific statement for describing sets of path segments, and types of set. While Miller's statements form the basis for the adaptation of time-geography to SNA in the current thesis, Yu & Shaw's (2011 *inter alia*) conceptual framework forms the framework for the analysis of network emergence, discussed in Chapter 4.

cannot describe *where they are* in space-time (as they can for vertices with the use of vectors), and they cannot describe the observable relations between these spatiotemporal properties. Miller's (2005) description of spatiotemporal control points and path segments thus also describes the ordered set of spatiotemporal *attributes* for an ordered set of network *elements* that have already been described by Hamberger *et al* (2011).

This means that for $G = (V, E, A\sim)$, where V_{a1} is a man[70] born at C_a, and $V_{b2}$is a woman born at C_b, and V_{c1} is their male child born at C_c. The set of vertices, edges and arcs that comprise the graph are ordered in space and time by control points C_a, C_b, C_c and by segments S_{ab}, S_{ac}, S_{bc}. The corresponding graph is defined as follows:

$$G = \{(V_{a1}, V_{b2}, V_{c1}), (E_{ab}), (A_{ac}, A_{bc})\}$$

Equation 27: Marriage triangle described in standard kinship network topographic terms.

If, following the consistent direction time t, we assume that the male child V_c is born after the woman $V_b$who is born after the man V_a, then using Miller's (2005) ordered set of temporal intervals (see Equation 21 and Equation 22) we may say that the corresponding control points $(\mathrm{c_a}, \mathrm{c_b}, \mathrm{c_c})$ are ordered in time by $(\mathrm{t_S} < t_a < t_b < t_c < t_E)$ as shown at Equation 28:

$$C = \{(c_a, c_b, c_c) |\, t_S < t_a < t_b < t_c < t_E\}$$

Equation 28: Ordering of control points *c* by moments in time *t*.

In words, C equals the set $(\mathrm{c_a}, \mathrm{c_b}, \mathrm{c_c})$ such that it is ordered by start time t_s which is less than the moment at which the man is born t_a, which is less than the moment at which the woman is born t_b, which is less than the moment at which the child is born t_c, which is less than end time t_E.

If we assume further, that the woman, man and boy described by G, are all born in different locations in space-time x, then we may designate these locations as (x_a, x_b, x_c). For example, the control point C_a, which describes the birth of the man V_a, is equivalent to $C(t_a) = x_a$, where t_a is the moment in time when he was born and x_a is the geographic location at which he was born.

[70] Following Hamberger *et al* (2011), V indicates vertex; subscript a indicates male gender, subscript b indicates female gender; subscript n indicates distinct individual.

This allows the description of corresponding linear space-time segments $S_{\alpha\beta}$ between each of the control points (c_a, c_b, c_c) as follows:

For S_{ab}: Where:

$$S_{ab}(t) = (1-\alpha)x_a + a_{xa} \qquad \alpha = \frac{t - t_a}{t_b - t_a}$$

Equation 29: Spatiotemporal attributes of H.F marriage edge ab, desrcibed as a path-segment S_{ab}.

For S_{ac}: Where:

$$S_{ac}(t) = (1-\alpha)x_a + a_{xa} \qquad \alpha = \frac{t - t_a}{t_c - t_a}$$

Equation 30: Spatiotemporal attributes of HH descent arc ac, desrcibed as a path-segpemnt S_{ac}.

For S_{bc}: Where:

$$S_{bc}(t) = (1-\alpha)x_b + a_{xb} \qquad \alpha = \frac{t - t_b}{t_c - t_b}$$

Equation 31: Spatiotemporal attributes of FH descent arc bc, desrcibed as a path-segpemnt S_{bc}.

In order to generate a single integrated graph comprised of these 3 space-time path segments, it is necessary to invoke Miller's definition of a "space-time lifeline" (2005:33). Although he includes in this composite object both paths and prisms, only paths will be used here. Excluding prisms, Miller defines a space-time lifeline as follows:

$$P^q(t) = \begin{cases} C_i^q, & t \in t_s^q, \dots, t_i^q, t_j^q, \dots, t_E^q \\ S_{ij}^q(t), & t_i^q < t < t_j^q \end{cases}$$

Equation 32: A spatiotemporal lifeline q (Miller 2005).

In words, the superscript q indicates membership of a single lifeline for a given path P, moment in time t, control point C and segment S. Thus we may say that the spatiotemporal path segments described by Equation 29, Equation 30, and Equation 31, are all segments of path P^q. They are not strictly members of a single 'life', in Miller's (2005) sense, because they describe the kinship relations between 3 distinct individuals. However, as a *parental triad* (Hamberger *et al* 2011:540), we may

consider this weakly acyclic network a single elementary structure within a larger kinship network, and thus denotable by P^q in the same way that a life-path is denotable by P^q.

Therefore, for the marriage triangle described in standard kinship network topographic terms at Equation 27 above, the corresponding space-time path, in Miller's (2005) terms, is defined as follows:

$$G^q \equiv P^q(t) = \begin{cases} C_a^q, & t \in \left(t_S^q < t_c^q < t_b^q < t_a^q < t_E^q\right) \\ S_{ab}^q(t) & t_b^q < t < t_a^q \wedge t_a^q - t_b^q = t_{ab}^q \\ S_{ac}^q(t), & t_c^q < t < t_a^q \wedge t_a^q - t_c^q = t_{ac}^q \\ S_{bc}^q(t), & t_c^q < t < t_b^q \wedge t_b^q - t_c^q = t_{bc}^q \end{cases}$$

Equation 33: A spatiotemporal kinship network graph G^q defined in space-time by P^q.

In words, graph G^q which consists of three vertices (V_{a1}, V_{b1}, V_{c2}), one edge E_{ab} and two arcs (A_{ac}, A_{bc}), is defined in space-time by path P^q which consists of control points and path segments. The three vertices are ordered in space-time by three control points (C_a, C_b, C_c). The one edge and two arcs are ordered in space-time by three path segments (S_{ab}, S_{ac}, S_{bc}). Figure 144 shows the graphed results.

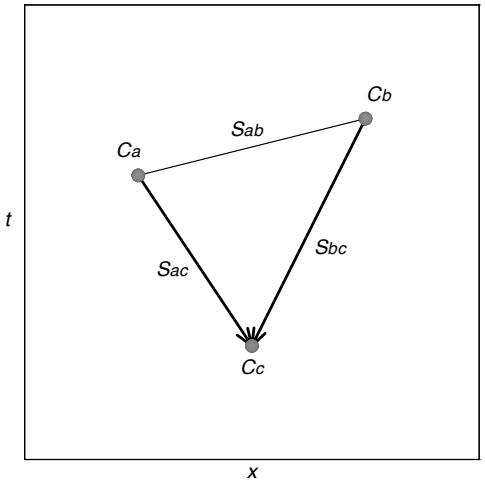


Figure 144: Canonic parental triad ordered in time t and space x.

Of course, as with Miller's (2005:27) example (see Figure 132), geographic space has been collapsed into the single dimension x in Figure 144. If we graph x as a plane, by matching it with a second dimension y, we get the graph at Figure 145. This is achieved by extending the definition of each control point to $C_a \equiv C(t_a) = x_a, y_a$.

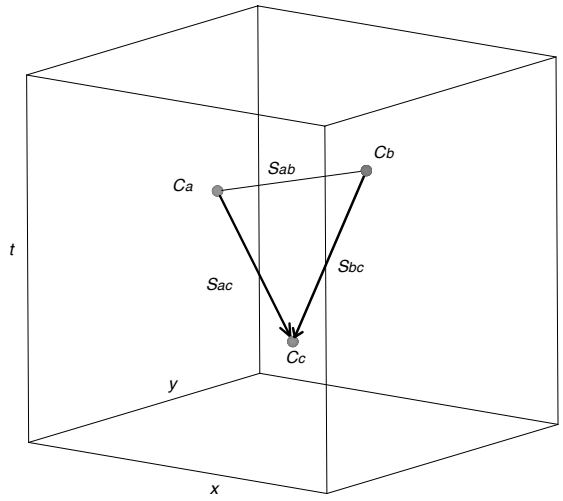


Figure 145: Canonical parental triad ordered in time t and space xy.

Finally, it must be emphasized that since a control point C is one of many potential *attributes* of any given vertex V, when that vertex represents a person, C must be treated as an geometric *projection* of V such that for any given vertex, $C_\alpha \equiv \pi_C(V_\alpha)$. This means that in using a formal time-geographic metric, we are explicitly recognizing the metric as one class of tuple among many others.

5.3.2 The Spatiotemporal Signature of the Central Murray Darling Basin Population

Chapter 4 presented the formal terms and definitions furnished by social network analysis for describing cohesion. Social cohesion under these conditions is an emergent property of networks whose vertices (representing individual people) form reticular relationships with one another, such that members of the network are bound together by more than one chain of relationships. Chapter 4 also presented the more specific terms of and definitions furnished by kinship network analysis, which describe the role of particular types of circuit in generating cohesion across kinship-based populations. Chapter 4 showed that there is a causal link between the types of kinship circuit encoded in the Central Murray Darling Basin (CMDB) kinship terminology, and cohesion evident in the matrimonial core of the CMDB population itself, which indicates that the former underpin kinship cohesion in that population. Applying the *st*KNA techniques to this matrimonial core shows that it exhibits a distinct geospatial signature (see Figure 146 and Figure 147).

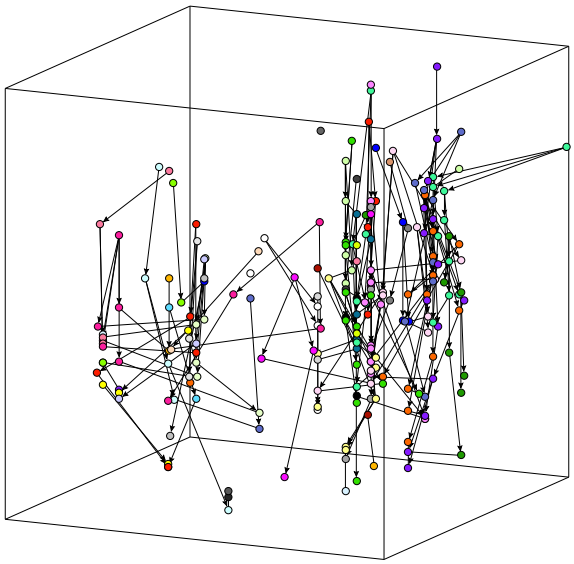


Figure 146: CMDB matrimonial core, with vertices located in a normalized vector space according to birth date and birth place (oldest birth date at top). Vertices are colour-coded by matrifiliation as per 2D 4 rendition in Chapter 4.

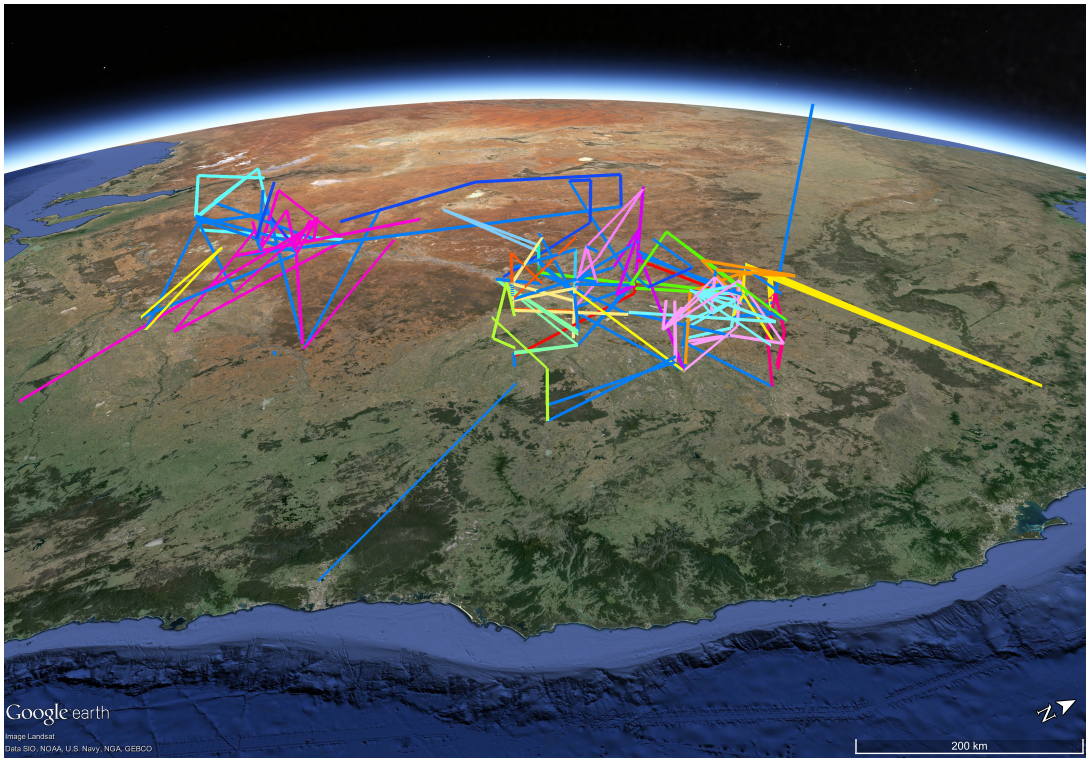


Figure 147: CMDB matrimonial core, with vertices located according to actual birth places and normalised birth date distribution (oldest birth date at highest altitude). Vertex colour shown in Figure 146 is projected onto tie colour for clarity. See <goo.gl/3qXwOQ> for 3D model.

Figure 146 reveals that the matrimonial core is not only dense in a purely relational sense, as illustrated by measures of cohesion in Chapter 4, but also *spatially* dense within the normalised vector space derived from its most widely distributed vertices. Meanwhile, Figure 147 shows further

that the birth locations of members of the CMDB matrimonial core are concentrated in the geographic region after which the dataset is named, the Central Murray Darling Basin. The striking visual clarity of this distribution is not diminished by its 'predictability', i.e. the expectation that a population anecdotally associated with a geographic region in the ad hoc ethnographic record, can actually be *demonstrated* as such when empirically and formally modelled.

The spatiotemporal model illustrated at Figure 146 and Figure 147 does not represent the entire matrimonial core presented in Chapter 4, but rather a subgraph of that core, comprised of vertices and ties for which spatiotemporal projections have been observed. Of the 295 people comprising the original core, this model thus describes the locations of 220. Despite this reduction, the model's extant spatiotemporal patterning is highly informative, especially in the context of the sociocentric attributes identified in Chapter 4. Firstly, each of the matrifilial components associated with distinct sets of terms exhibits distinctive geographic patterning. Secondly, extant examples of the five circuit types found to exist in both Ridley's (1875) and Mathew's (1895) terminological models, and in the matrimonial core, also exhibit distinctive patterning.

Of the 27 matrifilial components identified in the matrimonial core, all lose some members in the course of being reduced to only those vertices with observed projections. Despite this loss, all 27 components are still represented. Although the geographic distribution of each of these components is interesting in its own right, it is impractical to present them all here. Instead, the four largest, comprising component 2 (29 vertices), component 17 (18 vertices), component 46 (15 vertices), and component 69 (14 vertices), are presented (see Figure 148 to Figure 153).

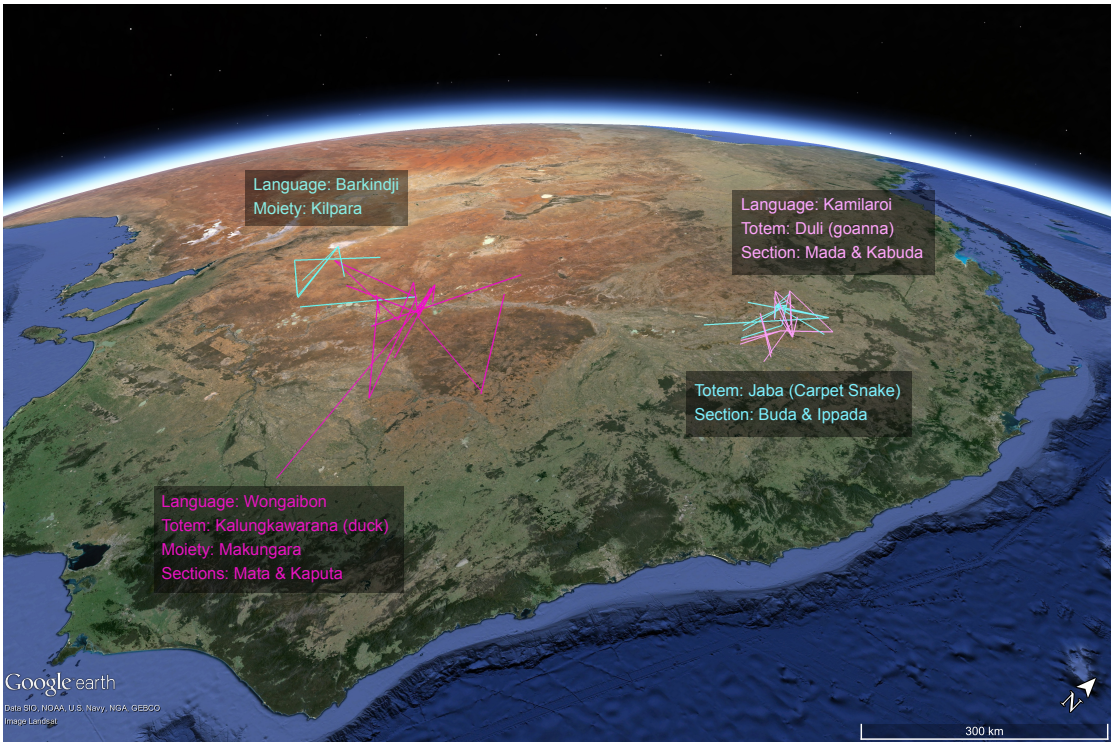


Figure 148: Four largest matrfilial components from the matrimonial core shown simultaneously. See <goo.gl/9AF8vu> for 3D model.

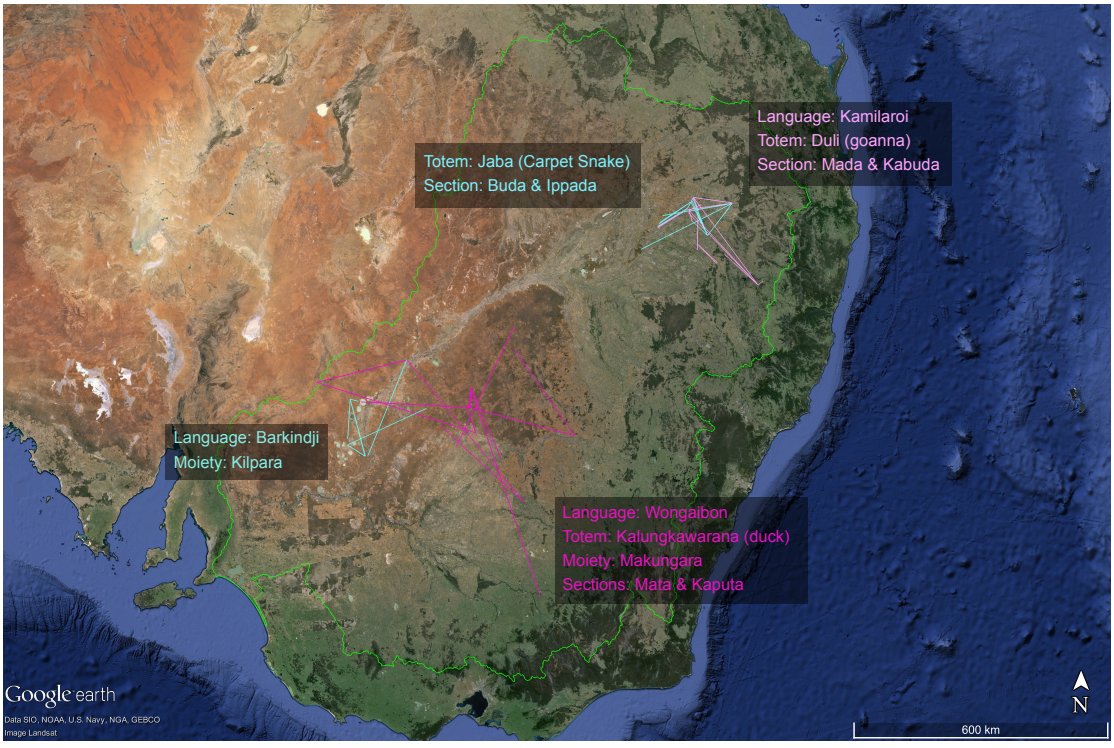


Figure 149: Four largest matrfilial components shown simultaneously, together with Murray Darling Basin extent (green boundary). See <goo.gl/UYjM4U> for 3D model.

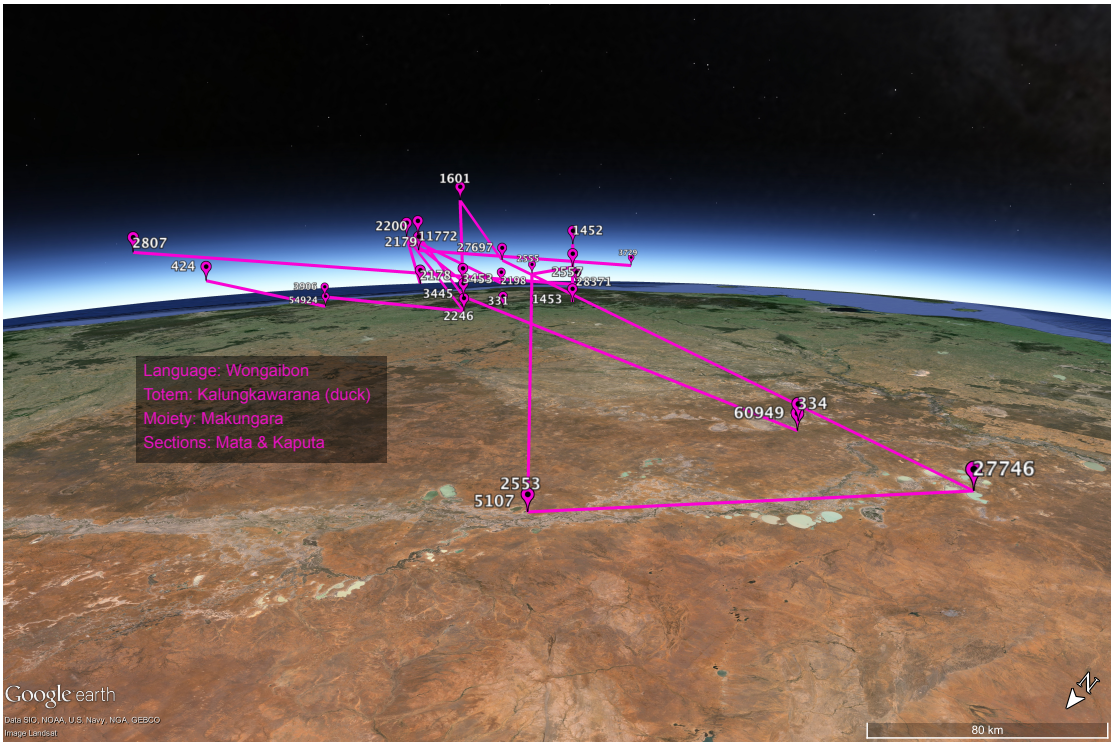


Figure 150: Close-up angled view of component 2. See <goo.gl/DkAfWm> for 3D model.

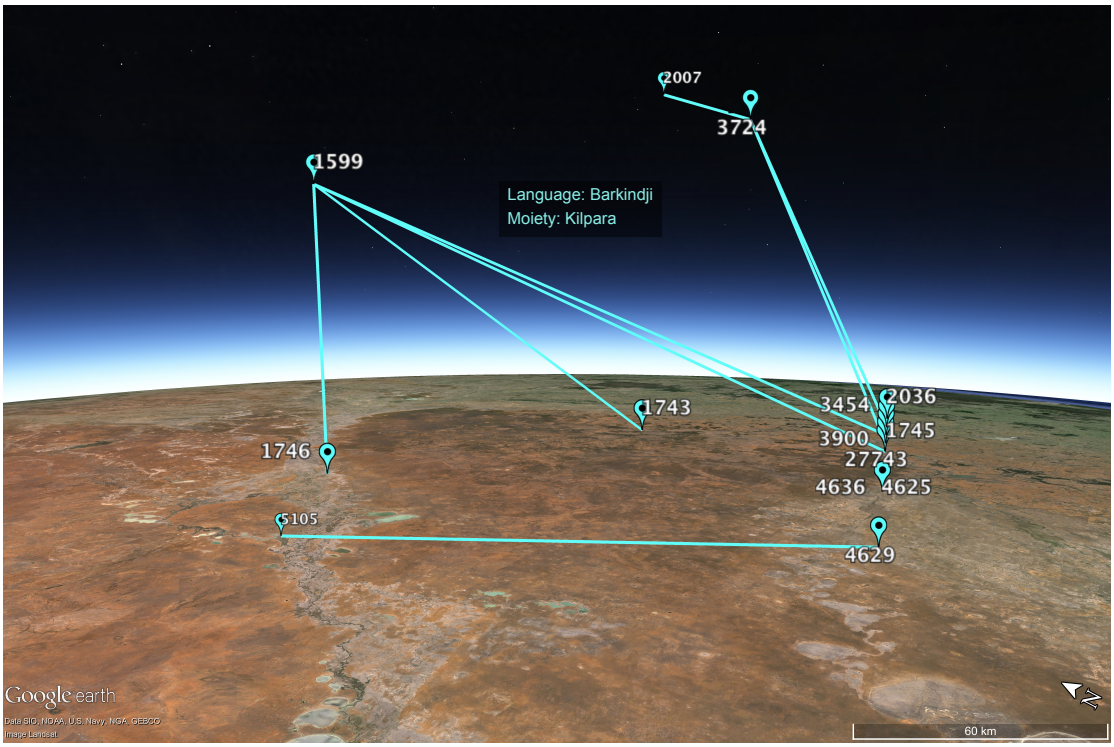


Figure 151: Close-up angled view of component 17. See <goo.gl/gExpI2> for 3D model.

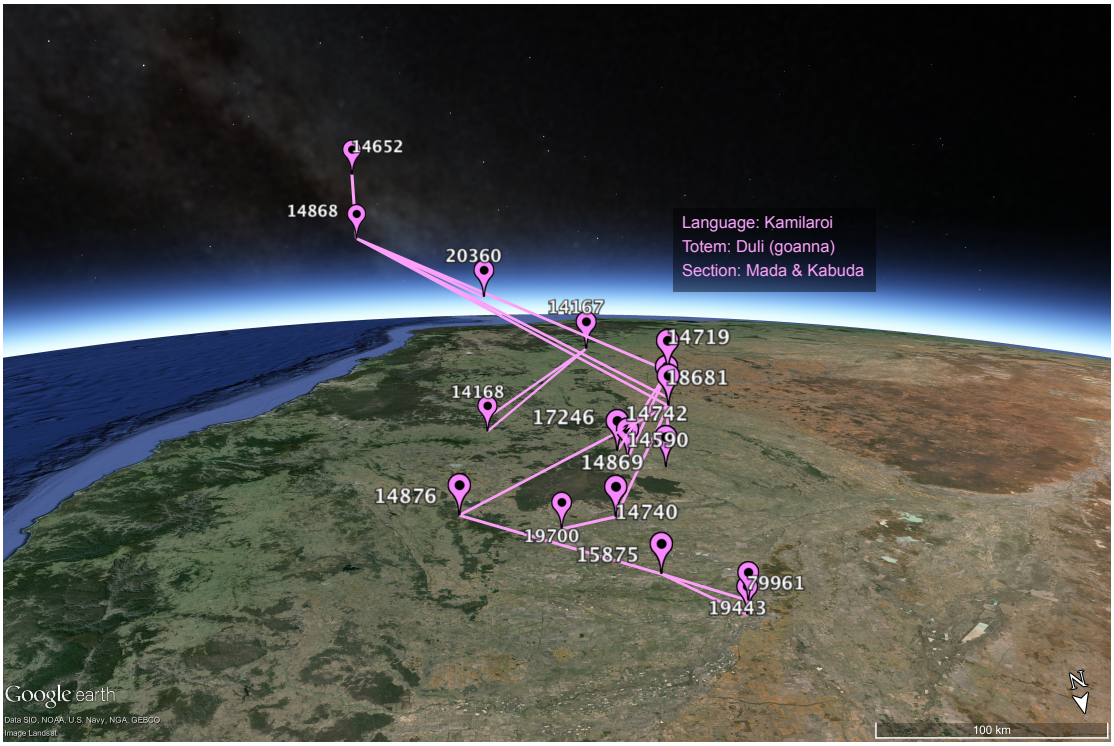


Figure 152: Close-up angled view of component 46. See <goo.gl/8bdHzB> for 3D model.

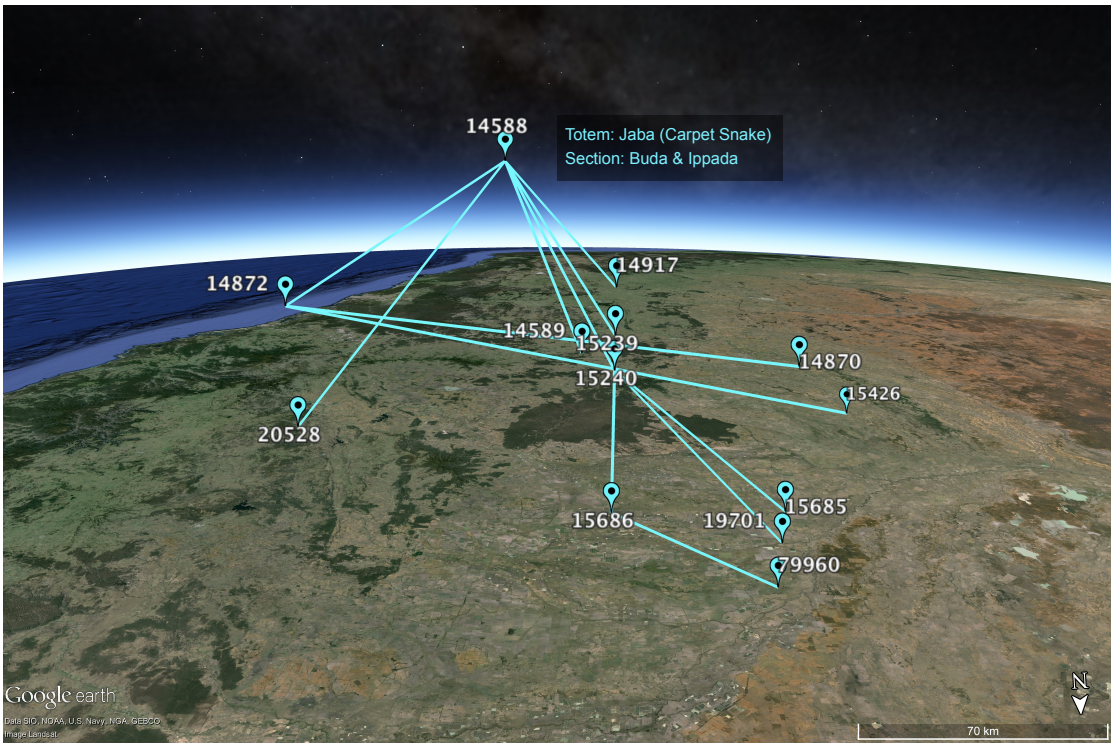


Figure 153: Close-up angled view of component 69. See <goo.gl/mt422H> for 3D model.

The most immediately obvious feature of these distributions is their limited extent: All are relatively discreet in terms of the geographic areas that they cover. The most extensive component, 2 - attributed with the *Wangaypuwaan* language, and *Kalangkuwarana (duck)* totem, also has the

greatest number of vertices, at 29. This components is nevertheless constrained to that part of the Murray Darling Basin known as the Cobar Peneplain, identifiable visually by the arid, orange-coloured 'triangle' encompassing it on the areal photographic base map. The second largest component, comprised of 17 vertices and bearing the language *Barkindj,* and *Kilpara (Crow)* moiety, is more constrained still, overlapping with the *Wangaypuwaan* component, but concentrated over a single contiguous stretch the Murray River itself. The next two largest components, *Jaba (Carpet Snake)*, and *Duli (Goanna)*, are almost completely co-extensive, covering a confined region along the northern edge of the Northwest Slopes & Plains region, demarcated by the single continuous waterway known variously as the Macintyre, Barwon, and Namoi River.

The geographic extents of the four components above suggest a correlation between matrifilial component membership and geographic 'constraint' in Hägerstrand's (1970) terms (see above). The deliberate exclusion of patrifilial components negates the a more specific conclusion, however the results are adequate for the purposes of demonstrating the functionality of the stKNA technique. In terms of a broader preliminary implication for time geographic constraint, it seems reasonable to suggest that component membership is at least a *factor* in constraining the geographic distribution of birth locations for component members in this particular dataset. This opens the question as to whether geographic constraint of this kind might be properly equated with 'geographic *cohesion*' for the network theoretic component members concerned. As discussed in Chapter 4, genealogical trees are K1 structures, without relinkage among vertices whose only criteria for inclusion is that they be linked to each other by single paths tracing to a single apical ancestor. The matriflial components described here comprise exactly this type of structure. The question of whether or not the geographic extent of such structures is a corollary of cohesion at a geographic level, is not ideally answered by treating them as an expression of cohesion, even if they each form part of a larger and more cohesive matrimonial core with a connectivity level of K2 (as shown in chapter 4). The spatialized graphs above indicate that there is indeed a geographic constraint operating on these matrifilial components, but do not in themselves point to a *causal link* between that constraint and cohesion of the kinship network.

The more congruent factor to consider is the property of reticulation itself, more specifically that reticulation exhibited by the five circuit types that were shown in Chapter 4 to co-occur in both the optimal terminological models of Ridley (1875) and Mathews (1895), and in the 295-person matrimonial core. Of the five circuit types meeting these criteria all are represented in the reduced 220-person stKNA model of the matrimonial core (see Figure 147), although not all *instances* of these circuit types survive the reduction to those with spatiotemporal projections. Their properties are nevertheless interesting. Figure 154 to Figure 158 below present close-up spatiotemporal views of each instance of these circuit types.

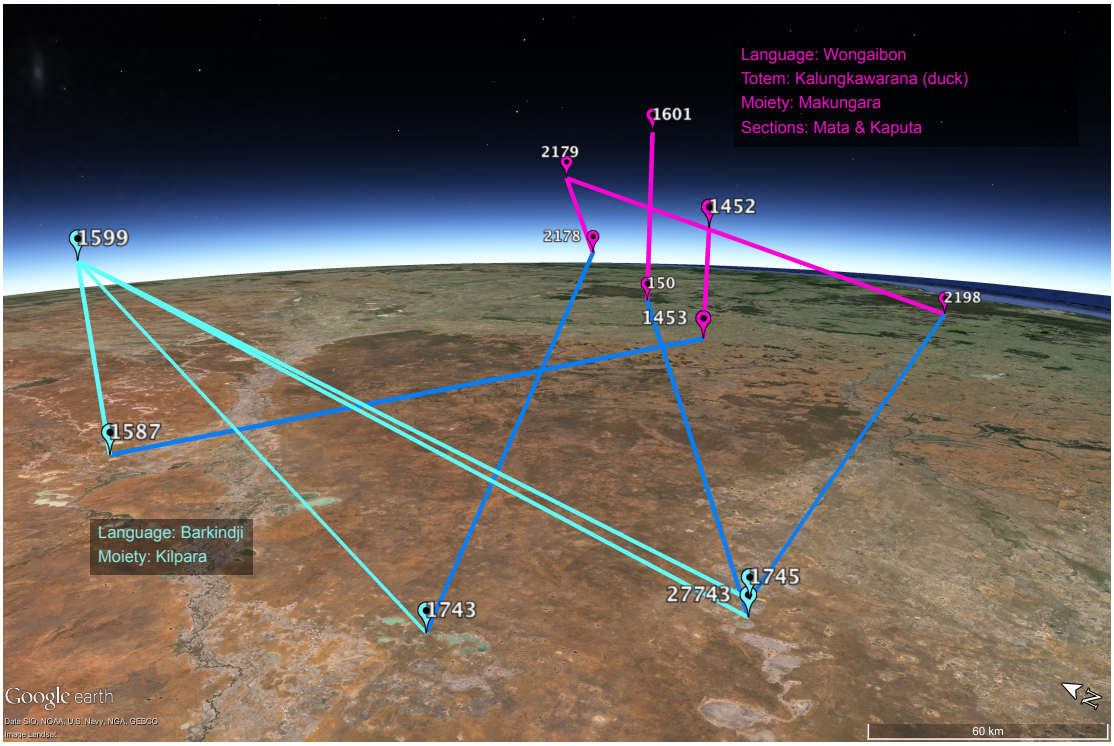


Figure 154: Five spatialized circuits of structure X(X)X.XX(X)XX, out of 7 detected in conventional KNA graph in Chapter 4. All incorporate marriages (blue ties) between members of components 2 (pink) and 17 (aqua). Note that the projected circuits are not complete, e.g. v1601 and v1452 do not relink. See <goo.gl/RRIlvj> for 3D model.

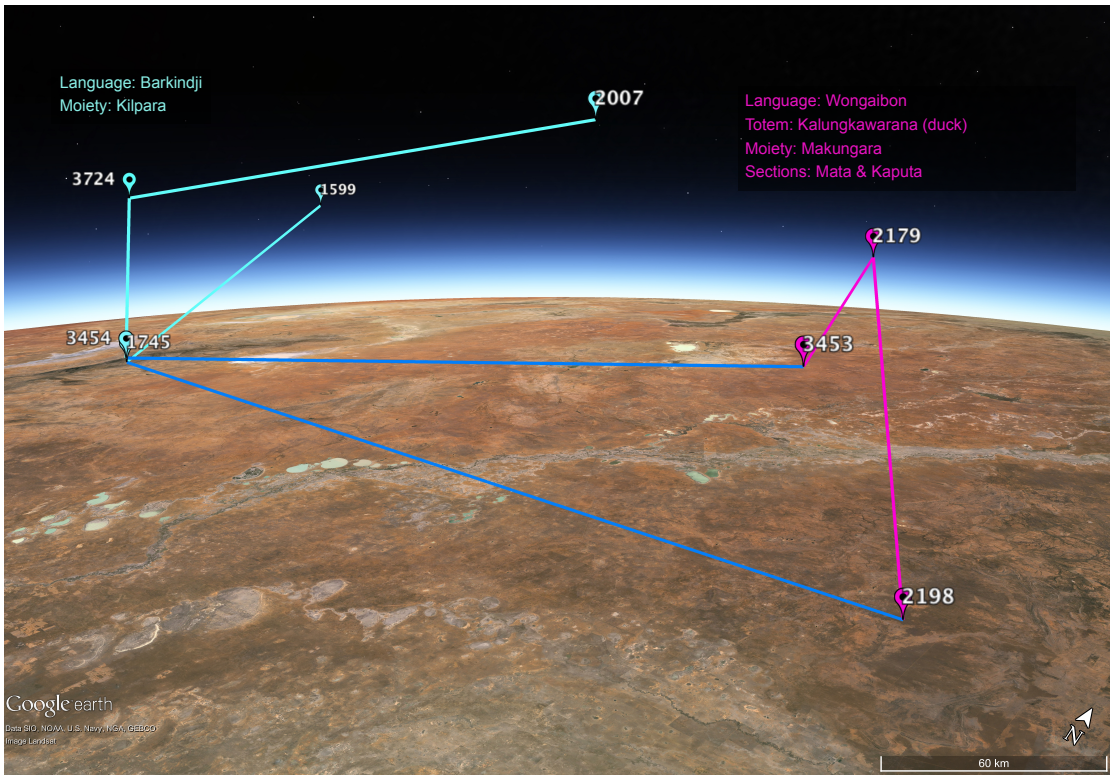


Figure 155: One spatialized circuit of structure X(X)X.XXX(X)XXX, out of 6 detected in conventional KNA graph in Chapter 4. All incorporate marriages (blue ties) between members of components 2 (pink) and 17 (aqua). Note that the projected circuit is not complete, e.g. v1599 and v2007 do not relink. See <goo.gl/vpTd3Z> for 3D model.

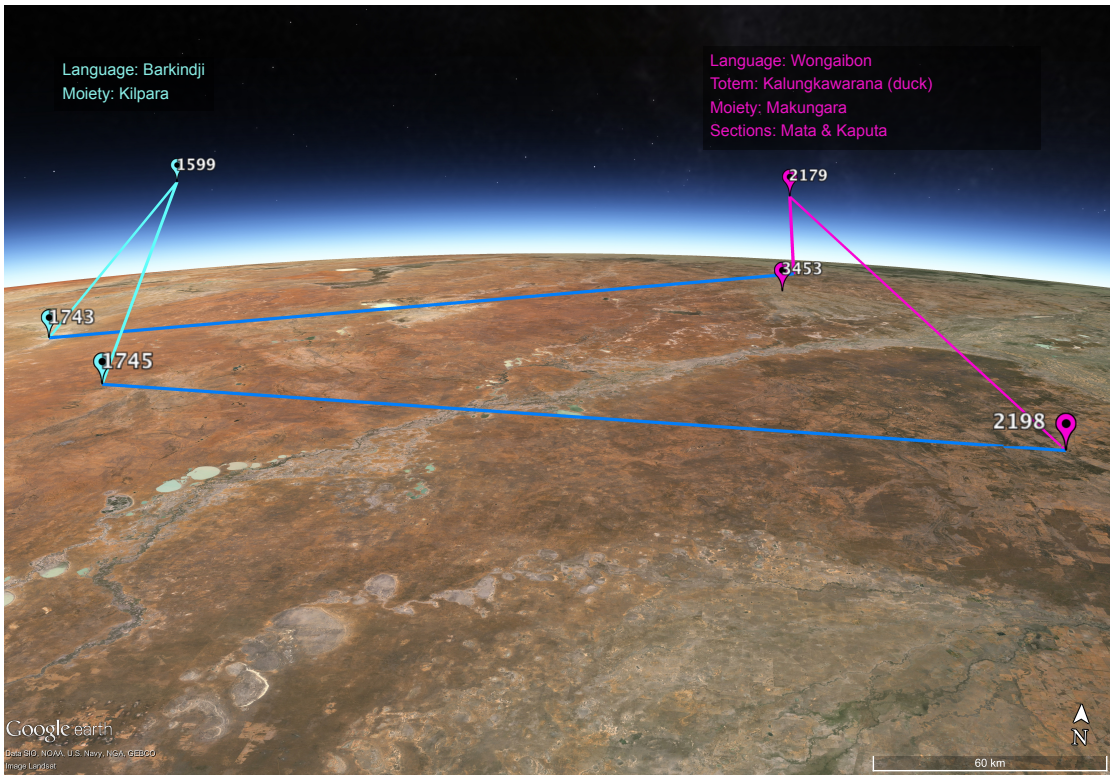


Figure 156: Two spatialized circuits of structure XX(X)X.XX(X)XXX, out of 15 detected in conventional KNA graph in Chapter 4 . All incorporate marriages (blue ties) between members of components 2 (pink) and 17 (aqua). Despite having the appearance of a single closed circuit, the two projected circuits are not complete. See <goo.gl/ehFCMu> for 3D model.

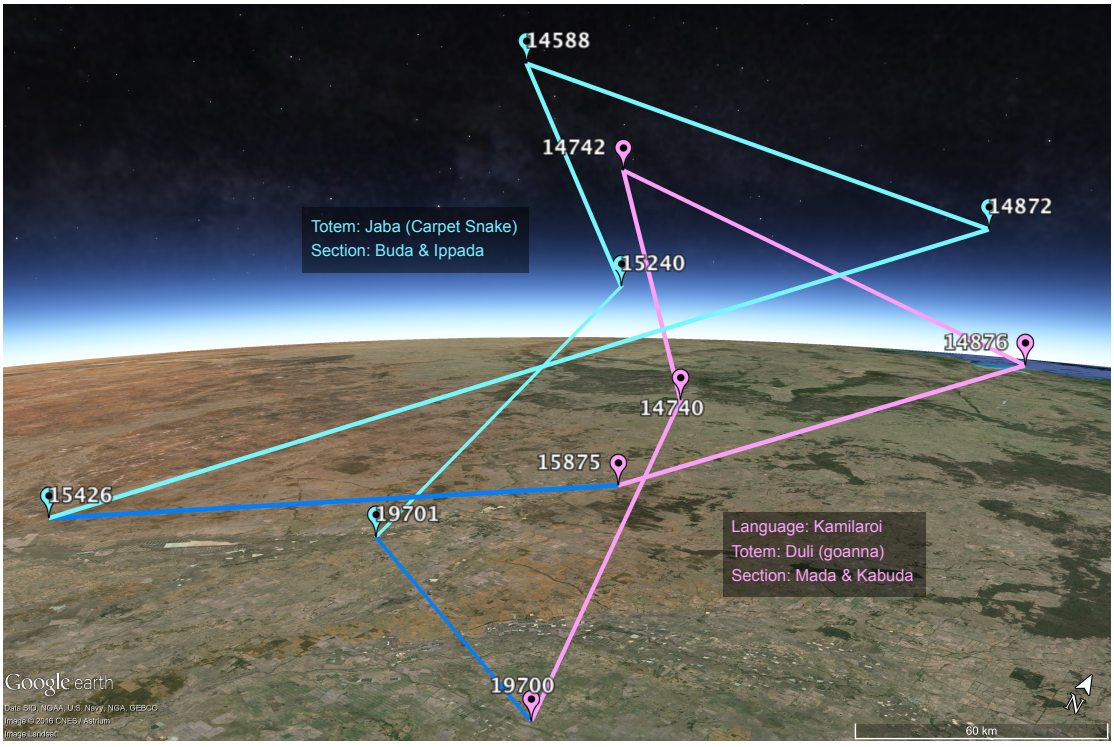


Figure 157: One spatialized circuit of structure XX(X)XX.XX(X)XX, out of one detected in conventional KNA graph in Chapter 4. It incorporates marriages (blue ties) between members of components 46 (pink) and 69 (aqua). This circuit is one of few for which spatiotemporal projection is complete. See <goo.gl/9Yp3nj> for 3D model.

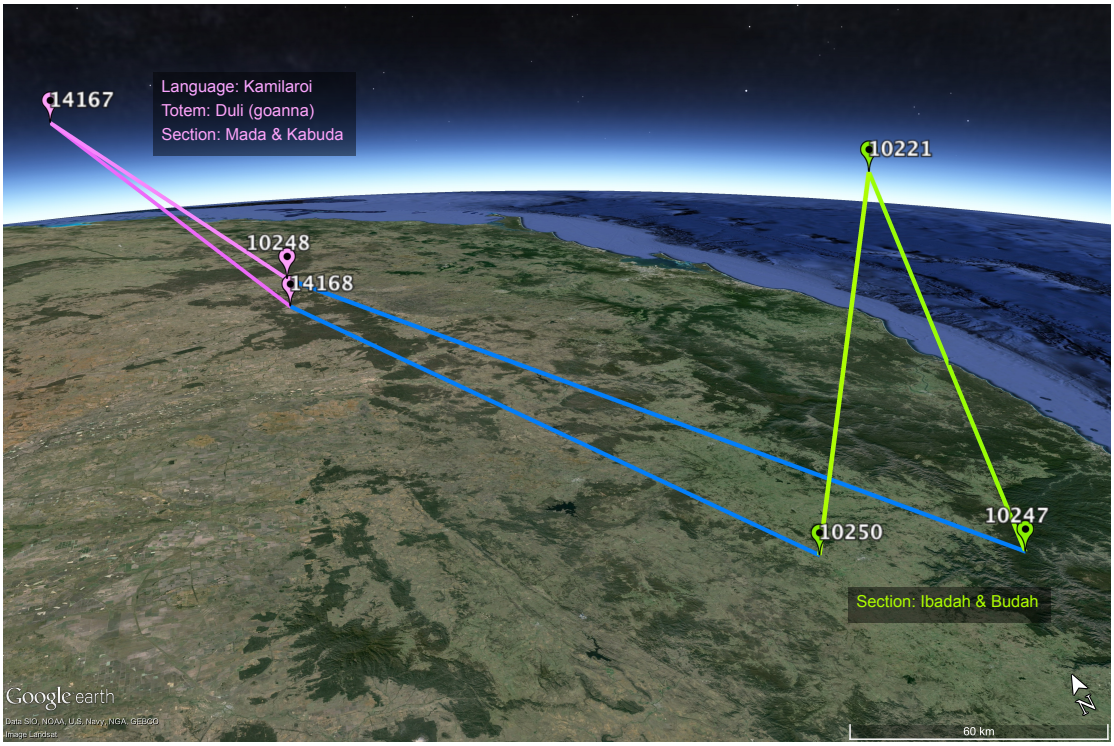


Figure 158: One spatialized circuit of structure X(X)X.X(X)X, out of four detected in conventional KNA graph in Chapter 4. It incorporates marriages (blue ties) between members of components 46 (pink) and green (65). This circuit is one of few for which spatiotemporal projection is complete. See <goo.gl/7WEIIa> for 3D model.

Perhaps not surprisingly, the most complete circuits (those for which most vertex projections are observable) incorporate the largest matrifilial components, i.e. the larger the component, the more statistically likely that birth locations and birthplaces will be discovered for at least some of its vertices. In particular, we see that of the circuits most comprehensibly describable in spatiotemporal terms, most incorporate marriages between components 2 and 17, and between components 46 and 69. The only exception is what Lévi-Strauss would call an 'elementary direct exchange' between components 46 and 65, shown in Figure 158.

The most striking feature of these spatiotemporal models is that they clearly visualise matrimonial exchanges between geographically constrained matrifilial components. The most obvious case is the aforementioned 'direct exchange' between components 46 and 65. However, even in the most visually complex example, shown at Figure 157, with two marriages between components 46 and 69, the marriages still visibly relink to distinct structures that are 'wrapped around' each other in terms of their spatiotemporal distribution. The causal factor in the formation of this shape, as far as the spatiotemporal model is concerned, is the historical movements of mothers, such that each child within each component is born in successively different locations over time, but which do not extend beyond a given geographic region. The shape of the structure to which they belong is an emergent property of the network that emanates from these changes, and would cognizable to its members. More significantly still, it is the attendant fact that the spatiotemporal shape belongs to a kinship *circuit*, rather than to a tree, which suggest a corollary between kinship cohesion and spatiotemporal

cohesion. Visualising matrimonial relinkage between matriflial components in terms of space and time, illustrates a causal process *built in* to the shape of the circuits themselves.

By viewing these micro/macro feedback processes as causal factors, the spatiotemporal distribution of all 27 components illustrated at Figure 147 above takes on clearer significance at the level of the whole 295-person network. This K2 core is comprised entirely of circuits. The special subset of five circuits types chosen for analysis here represents only a small proportion, yet their spatiotemporal projections - their *spatiotemporal signatures* - point to causal processes that may extend to the whole matrimonial core. As noted in Chapter 4, although further discussion is beyond the scope of this thesis, there are a significant number of other circuit types in this core that do *not* co-occur in Ridley's (1875) and Mathew's (1895) models, but whose spatiotemporal projections point to even more complex emergent properties.

5.3.3 Implications of Spatiotemporal Kinship Network Analysis for the KLRE Model of Sociality

Chapter 4 introduced kinship network analysis as a framework for resolving deep-seated problems with existing social anthropological models of Indigenous Australian kinship and broader sociality. The resolution was shown to flow from the way in which sociality and causality are encoded in the language of social network analysis, particularly the formal empirical basis of its terms and definitions, and its highly congruent construal of sociality as observable relationships between real individuals. What does the technique of spatiotemporal kinship network analysis contribute to this resolution?

As outlined above, space and time are observable forms of order that set ultimate parameters on human existence and interaction. Models of space and time that are broadly congruent with people's experience of interaction with one another, furnish powerful semogenic potential for realising and managing that interaction. Chapter 3 showed that over the course of 150 years of serial adaptation, Australian anthropology's KLRE model has consistently construed space and time as ancillary orders rather than as observable factors in social interaction. Focused on an ad hoc exercise of classifying groups of institutions, rather than on formally and empirically modelling real interactions between real people, the KLRE model adopts *a priori* temporal and spatial metrics as means for imbuing its classificatory framework with otherwise absent causal power. Between the 1870s and 1970s these metrics typically consisted of temporal scales ranging between 'primitive' and 'civilized' forms of sociality, and spatial scales ranging between maximum and minimum proximity to the geographic locus of civilization (usually defined by the highest concentrations of European people - whether Europe itself, or its colonial capitals in Australia). As shown in Chapter 4 by contrast, kinship network analysis (and social network analysis more broadly) provide a framework for construing sociality with inherent causal *flow*, where the temporal order of relationship formation at a micro-level has configurational effects on encompassing networks at a macro-level, and where the relationship between micro- and macro-level processes is one of contextualizing feedback. Although this construal renders the KLRE model obsolete in terms of the former's greater formalism, empiricism,

and congruence, the almost exclusively topological construal of space and time in the language of KNA excludes more congruent renditions of *geographic* space and *historical* time. The technique of stKNA provides a lexical and syntactic extension to the language of KNA that overcomes this, and further compounds the obsolescence of the KLRE model.

The KLRE model construes the Indigenous population of the Central Murray Darling Basin as bijected into a number of groups identified by language variation. This is the result of a tacit and ad hoc adoption of a comparative linguistic model, which is now largely stable and which identifies three language groups and upwards of 7 distinctive languages above the level of dialect (Dixon 2002; Wafer & Lissarrague 2008). Social anthropological literature spanning the inception of the discipline in Australian in the 1880s (Fison & Howitt 1880) to recent decades (Keen 2005; McDonald 2000), uniformly attributes these group theoretic entities with discrete kinship terminologies, religious beliefs, systems of economic production, as well as geographic distribution. The stKNA modelling presented in the current chapter indicates that despite these theoretical classifications, the real people comprising the population of the Central Murray Darling Basin, to whom these institutions belong, are fundamentally interdependent. This modelling shows that their physical existence in geographic space and historical time is the result of repeated and systematic geographic interspersion and intermarriage, manifest in observable and demonstrable matrimonial cohesion across the entire population. This finding indicates that, while it is clear that morphological variations in kinship terminology, language, religion, and economy exist within this population, they are not causal factors in the sociality of the population as a whole. Their regional variation across the network, evident both in the conventional KNA graphs presented in chapter 4 and in the stKNA graphs presented in the current chapter, is more likely indicative of a self-reinforcing process *deriving from* cohesive processes.

Section 5.3.2 indicates that the spatiotemporal signatures of co-occurring matrimonial circuits have characteristic patterns. Figure 148 illustrates that each of the 27 projected components is associated with a geographic area that is visibly 'constrained' in Hägerstrand's terms (1970). Meanwhile Figure 154 to Figure 155 show that each of the 10 projected circuits link pairs of these components together into larger network entities covering more extensive geographic regions. Several of these circuits, including those graphed at Figure 154, Figure 155, and Figure 157 fold back onto each other geographically over time, during the course of several successive generations. All relinked regions of the matrimonial core are geographically adjacent to one another.

It is important to re-emphasise the origin and definition of the parameters that were set in choosing the population data that has been analysed here. This is a matrimonial core from a deliberately selected subnetwork. That subnetwork was extracted from a much larger network - the Unified kinship model of South East Australia - according to the attributes of its members, *viz*. ethnographic records of one or a combination of sociocentric kin terms. That subnetwork comprised 3128 people.

The matrimonial core extracted from that subnetwork comprised 295 people. This core is what has been modelled in the current section. These parameters did not target any particular predefined 'class' of sociocentric attributes, such as a particular language, or kinship terminology type. Leaving the data field relatively open in this way maximised the potential for exploratory analysis of network structures and feedback processes with sociocentric terminologies, rather than limiting them to *a priori* assumptions. Such exploratory analysis is impossible under the rubric of either the KLRE mode or the standard model of kinship that it incorporates.

With regard to the group theoretic standard model of four kinship terminology 'types', denoting discrete sets of configurational 'rules' with predictable real-world effects, the conventional KNA analysis conducted in Chapter 4 indicates that it is no longer convincingly sustainable. The analysis in Chapter 4 yielded no evidence that the number or morphology of terms in a terminology has any configurational effects on the shape or extent of the matrimonial network emerging from the micro-level marriage choices among people who use the terminology. The results of the analysis suggest that at least for vertical moiety terms, section terms, totem terms, and blood terms, the shape of circuits is consistent across a multi-terminology kinship network.

5.4 Conclusion: From Spatialized Social Networks to Socio-spatial Networks

It is important to note at this point that what has been described in this chapter is the *partial integration* into existing social and kinship network theory of time-geographic theory, by using the latter to describe spatiotemporal attributes for vertices and ties whose topology has already been established by conventional social network theory. Tools for measuring topography have thus been integrated into an existing toolset for measuring topology. This allows kinship graphs to be spatialized according to inherent measures of space and time that are observed in the network data. The integration of topographic and topological toolsets is intended to facilitate the extension of data models and consequently theoretical models and analyses, from measures of networks to measures of spatialized networks. Importantly, the original topological network data remain unaltered in this approach, since the only change being made to the modelling and analysis process is the inclusion of additional attribute data.

In demonstrating the new technique of spatiotemporal kinship network analysis, the current chapter has drawn on a conventional KNA analysis to illustrate the further insights that can be gained from a model that is more congruent with the lived experience of time and space. To this end, it might be useful to consider stKNA not so much as an 'extra' toolset that can be appended to conventional techniques, as a further dimension of kinship network analysis that offers scope for identifying and measuring extant network properties that otherwise remain obscured. In this view of stKNA we might speak of socio-spatial networks rather than social networks that have been graphed in using a particular kind of spatialized metric.

The stKNA technique lends itself to the triadic methodological framework introduced in Chapter 1, comprised of a confluence between a) autochthonous models of sociality; b) anthropological models of; and c) observed instances of sociality. As articulated there, Ridley's (1856; 1875) and Mathew's (1895; 1897; 1905) terminology models constitute the relationship $a \rightarrow b$; the 295-person matrimonial core extracted from the Unified network constitutes relationship $a \leftrightarrow c$; the census conducted on circuits co-occurring in both Ridley's and Mathew's models, and in the Unified network constitutes relationship $c \rightarrow b$. The current chapter has presented a further augmentation of relationship $a \leftrightarrow c$, by elaborating the terms, definitions, and methodology of kinship network analysis in a manner that extends its formal, empirical, and congruent grammar to describe the roles of space and time in the generation of social cohesion.

Chapter 6

General Conclusion

The original contribution of this thesis is the new research technique termed spatiotemporal kinship network analysis (stKNA), together with two network-based datasets used to demonstrate it. The latter include a compilation of individual identifiers derived from archival ethnographic material associated with the Central Murray Darling Basin population between the late 19th and early 20th centuries, and a 295-person matrimonial core for which those identifiers can be associated with an observable kinship network of real people. The primary intended function of the stKNA technique is to render observable the spatiotemporal properties of kinship networks. This function has a range of applications, spanning social anthropology, law, and demography. The breadth of this span points to the technique's potential role in building not only a more consilient social anthropology, but also a framework for construing Indigenous/non-Indigenous Australian population dynamics in a clearer manner, that is both qualitatively rich, and quantitatively informative. To this end, the question of individual identification that so vexes legal and demographic researchers in Australia is negated by the construal of Indigenous status as a *social condition*, rather than as an *individual property*. In other words, stKNA facilitates the detection of patterns in population data that are indicative of Indigenous status.

This thesis has proposed that kinship network analysis and its related techniques represent a scientific revolution for social anthropology in the terms defined by Kuhn (Kuhn 2012[1962]). Chapter 1 drew on the general perspective of natural science philosophy (e.g. Kuhn 2012[1962]; Leaf & Read 2012; Richerson & Boyd 2007; Wilson 2001; Wilson & Wilson 2007), and the more specific insights of semiotics-based discourse analysis (e.g. Bernstein 1996; 1999; Halliday 2005; Halliday & Matthiessen 2006) to describe both the manner in which the specialized languages of social science and natural science are unnecessarily separated from one another, and the manner by which both could achieve consilience. Chapter 1 highlighted the unique parameters of social anthropology among social science languages as predisposing it to such integration, but pointed out that in its conventional form social anthropology lacks a grammar that is sufficiently formal, empirical, and congruent with observable social processes. Kuhn's (2012[1962]) developmental model of natural science languages predicts that technological advances pave the way for breakthroughs in what are otherwise incremental expansions in systematic observation and modelling. Kuhn proposes further that such breakthroughs are typically accompanied by further specialisation within existing disciplines, as their models become increasingly empirical and formal (2012[1962]). Leaf & Read's (2012) prescription for a 'New Anthropology' conforms to this prediction by describing the criteria for a paradigm that they term 'Empirical Formalism'. In line with Houseman & White's (1998) observations on the accessibility of increasingly powerful computing in social anthropology, Chapter 1 concludes by noting that complexity in human social processes is no longer an impediment to large-scale and detailed measurement, and that network analysis provides an ideal framework for coordinating such an exercise. It was proposed that in order to understand *how* such a framework can facilitate consilience

between social anthropology and natural science, the theoretical assumptions historically underpinning social anthropology's non-scientific models need to be made clear.

Chapter 2 described a pattern of tacitly group theoretic axioms permeating key social anthropological texts, from the inception of the discipline in the 1870s through to the present era. After presenting detailed models of this patterning in foundational texts by Tylor (1871) and Morgan (1871), and in influential texts from the 1990s and 2000s, Chapter 2 focused attention on the influence of these underlying assumptions on theories of Indigenous Australian sociality. It was shown that Morgan directly influenced the erstwhile founder of Australian anthropology, A.W. Howitt, whose major 1880 work (Fison & Howitt 1880) then triggered the emergence of what is called here the 'KLRE model' of Indigenous Australian sociality. This non-empirical, weakly formal, and tacitly group theoretic model predicts that the entire Indigenous Australian population is divided into discrete entities termed 'tribes' or 'societies', and that each bears equally discrete subgroups of distinct 'laws an customs' that prescribe the observably distinct behaviour of its individual group members. Chapter 3 traced the evolution of this model throughout 20th century Australian social anthropology, and highlighted the lack of empirical evidence or formal definitions furnished in support of it. Particular attention was paid to the serial adaptation of *a priori* ordering regimes, introduced at different points over the model's evolution in repeated attempts to render it congruent with increasingly detailed but contradictory field data. It was also noted that as comparative linguistics emerged as a distinct field in its own right, based on set theoretic rather than group theoretic principles, an effort by anthropologists such Radcliffe-Brown (1918; 1923; 1940 *inter alia*) was made to scaffold the KLRE model onto it, by using language as an identifying element for group theoretic 'societies'. The zenith of this effort was shown to converge with the arrival in the 1970s of legislative regimes for the recognition of Indigenous land rights, with the consequent effect that language became enshrined as a widespread folk model for Indigenous sociality, which then influenced subsequent judicial and policy thinking. The litigiously-oriented 1993 Commonwealth Native Title Act explicitly refers to Morgan's and Tylor's 19th century theories of discreet 'laws and customs' as identifiers on which rights-based agreements should be made between government, industry, and discrete groups of Indigenous Australians. In this adoption of unsubstantiated pre-paradigmatic anthropological theory, Indigenous sociality is construed as prescribed by normative principles. Where such 'laws and customs' cannot be shown to correspond to observed contemporary behaviour, claims to rights-based benefits are now typically denied to Indigenous people. This form of exclusion corresponds to that described by Taylor (2009) for demographic studies, where Indigenous people are excluded from critical surveys because of the inadequacy of individual identifies that treat Indigenous status a personal property, rather than as a condition of the population in which Indigenous people are embedded.

The conventional KNA modelling and analysis presented in chapter 4 illustrated how sets of individual identifiers, derived from *Indigenous* theories of sociality, rather than 19th century European

assumptions about group dynamics, can be used to delimit subsets of informative population data modelled as networks. Chapter 4 went on to show that independently observable circuit structures in these networks emerge from the systematic instantiation of Indigenous theories of sociality, which encode specific kinds of circuit over time. These independently observable structures then generate contextual precedents within the population for further instantiations of autochthonous theories, in what White & Johansen (2005) have described as a 'feedback process'. This process in turn generates structures that are not clearly identifiable in Indigenous theories of sociality, at least insofar as the most reliable anthropological models suggest (as per network theoretic construals of Ridley (1875) and Mathews (1895) presented in Chapter 4). More specifically, at least half of the circuit structures that emerge from the instantiation of kinship theories in a real-world kinship network, and which are critical to its cohesion, are *not* detectable in the models of the kinship theories themselves. These observations point to a way for detecting what have been characterised as 'invisible' regions of the Indigenous Australian population (Smith *et al* 2008), where a lack of explicit identifiers in pre-existing demographic and anthropological models mean that people who are *part of* an Indigenous population are not *identified as* Indigenous. Using large scale and qualitatively rich datasets facilitated by novel computing approaches, kinship network analysis is able to detect structures that can only plausibly result from the instantiation of distinctive autochthonous theories of sociality, even where no records (anthropological or otherwise) exist for such theories. In other words, by searching for tell-tale signs of a kinship culture in the *structure* of population data, kinship network analysis renders redundant the need for *a priori* individual identifies such as the legal precept of 'law and custom', or even the consistent appellation of a single particular language name.

The stKNA modelling and analysis presented in Chapter 5 extends the conventional topological scope of KNA by furnishing an integrated spatiotemporal metric congruent with observable geographic and historical orders. This facilitates the detection, modelling, and analysis of circuit structures based not only on their topo*logical* properties, but also on inherent topo*graphic* properties. Projections of the resultant *spatiotemporal signatures* of given kinship networks reveal their function in generating population cohesion across both real geographic space and real historical time. The most pertinent effect of this functionality, for setting conditions on the identification of Indigenous status, is in construing that status as a spatiotemporal property of populations whose cohesion has a signature that locates it either inside or outside a particular spatiotemporal region. Chapter 5 showed that the cohesive matrimonial core of the Indigenous CMDB population of South East Australia[71], exhibits a spatiotemporal signature that locates it within a highly cohesive region, *viz*. the Central Murray Darling Basin between the mid 19th century and the late 20th century (the period over which records are available). Although not documented here, it can be reasonably assumed that a matrimonial core

[71] As defined by matrimonial circuits co-occurring both within it, and within documented sociocentric kinship terminology models.

for the non-Indigenous population of this region, however its parameters might be set[72], does not share this spatiotemporal signature. More broadly, the stKNA technique has the potential to delineate a distinction between the entire Indigenous and non-Indigenous population of Australia on the basis of spatiotemporal patterning in their corresponding kinship networks, with reference to only a small sample of identifiers.

This thesis has contextualised its contribution against the historical position of social anthropology as an 'authoritative language' (Bernstein 1999) for construing Indigenous Australian sociality. The historical performance of this role has been characterised as generally inadequate in several respects. Most fundamental among these is the demonstrable assumption on the part of some Australian social anthropologists that a vertical integration of their discipline into the knowledge structure of natural science would be somehow antithetical to the interests of Indigenous Australians, or to the interests of any population forming the object of social anthropological study. A further, corollary assumption is that integration of social anthropology into the natural sciences would imply that human beings are a 'natural phenomenon', tempting the spectre of a racist Social Darwinism. But, as laid out in detail in Chapter 1, and as elaborated throughout this thesis, these assumptions are only sustainable in the absence of a coherent understanding of science, and in the absence of a view that all people should reasonably expect their own, culturally specific theories to be taken sufficiently seriously as to be treated in a scientific manner. Science is the study of observable systems using formal and empirical models and analyses that yield sufficient congruence *with* these systems as to allow deliberate engagement. Human beings, through their almost certainly unique capacity for semogensis, *already* engage with such systems on a daily basis when they employ their own theories of sociality to engage with each other, consequently accreting a contextual social fabric that sets the context for further engagement. We call this dynamic feedback between theory and practice 'culture'. The assumption that social anthropology could be somehow exempt from this process verges on religious, based on a belief that its own alterity exists without evidence as to how it is achieved. Furthermore, the assumption of such an exception denies to the populations at whom social anthropology is directed, the right to be treated with the dignity afforded other subjects of science, such as in medicine. Indeed, in arguments against anthropology's reformulation as a science, the example set by medicine is often mysteriously left out, along with that discipline's complete dependence on the languages of biology, chemistry, and physics. Medicine's clear consilience with these adjacent fields does not in anyway diminish the critical benefit that it provides to real, living human individuals, with unique experiences and personalities. For the first time in history, technological conditions are of such a configuration that social anthropology no longer has any justifiable reason to continue denying its object the benefits that consilience has to offer.

[72] E.g. All descents of the first European colonists to settle in the region in the mid -to-late 19th century, or alternatively those members of the European population who lived in the region when it was at its highest density.

In conclusion, it is perhaps worth reprising the quote given in Chapter 1 of this thesis, citing the seminal 1971 judgement of Blackburn J that set in motion the legislative recognition of Indigenous Australian land ownership:

> *"I do not believe that the law of evidence requires me to put chemistry into one category and anthropology into another. The matter can be tested, it seems to me, by applying the analogy of medical evidence, with which courts are so familiar. This is a field in which the 'facts' include both what the expert observes and what he hears from other persons" (Blackburn J 1971:162).*

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APPENDIX 1 Extended Quotes

1.2.1 The Different Modalities of Natural Science and Social Science

Eagleton in Harvey (1990:9):

> *Post-modernism signals the death of such 'metanarratives' whose secretly terroristic function was to ground and legitimate the illusion of a 'universal' human history. We are now in the process of wakening from the nightmare of modernity, with its manipulative reason and fetish of the totality, into the laid-back pluralism of the post-modern, that heterogeneous range of lifestyles and language games which has renounced the nostalgic urge to totalize and legitimate itself .... Science and philosophy must jettison their grandiose metaphysical claims and view themselves more modestly as just another set of narratives".*

Leaf (2007:2):

> *"In the physical sciences, experiments often reveal relationships that can be expressed mathematically or in other highly formalized ways, such as the conceptual models of the atom, evolutionary tree diagrams, or chemical notation. It is the same in the social sciences, but with an added twist. In the physical sciences, there is generally a quite straightforward distinction that can be observed between the formalization as an aspect of our analysis and the data that the formalization represents. For the social sciences, this may also be true in some cases but in others* ***the data may actually consist of formal systems****. In this case, the problem is not only to find them and represent them, it is also to do so without contaminating the representations with concepts that are alien to them. Locating and characterizing such systems offers new epistemologies for anthropology and hence takes us to the frontiers of the discipline, where the struggle to recognize new phenomena is inseparable from a struggle to find new ways to think about them. How we do it has been subject to various formulations, and many of these have been fundamentally misleading and distracting" (emphasis added).*

1.2.3 How Discoveries are made in the Natural and Social Sciences

Bernstein (1999:163):

> *"Development, in the case of a horizontal knowledge structure, cannot be a function of the greater generality and integrating property of the knowledge because, as has been shown, such developments simply are not possible in the case of a horizontal knowledge structure. So what counts as development [in a horizontal knowledge structure]? I suggest that what counts as development is the introduction of a new language. A new language offers the possibility of a fresh perspective, a new set of questions, a new set of connections, and an apparently new*

problematic, and most importantly, a new set of speakers. This new language is likely to be taken up by the younger speakers of the particular horizontal knowledge structure. This new language can then be used to challenge the hegemony and legitimacy of more senior speakers. The latter may be cut off from acquiring the new language because of trained incapacity arising out of previous language acquisition, and a reduced incentive, arising out of the loss of their own position. ...

"Indeed, built into the construction of the language [of horizontal knowledge structures] is the protection of its discreteness, its strategies of apparent uniqueness, its non-translatability, and its essential narcissism. Motivations under this discursive regime are oriented to speaking/acquiring/developing the hegemonic language or its challenge or marketing a new language".

Bernstein (1999:164):

"In the case of hierarchical knowledge structures, the acquirer does not have the problem of knowing whether she/he is speaking physics or writing physics, only the problem of correct usage. The strong grammar visibly announces what it is. For the acquirer, the passage from one theory to another does not signal a break in the language; it is simply an extension of its explanatory/descriptive powers. ***However, if the social sciences are considered****, the problems of acquisition arise particularly where the grammar is weak. The acquirer may well be anxious whether he/she is really speaking or writing sociology [for example]. In these conditions, it is likely that canonical names will be a useful resource. Later, the names will be associated with languages or, in some cases, the language will come before the exemplars. Thus, managing names and languages together with their criticisms becomes both the manner of transmission and acquisition. There is, however, a prior issue. Because a horizontal knowledge structure consists of an array of languages, anyone transmission necessarily entails some selection, and some privileging within the set recontextualised for the transmission of the horizontal knowledge structure. The social basis of the principle of this recontextualising indicates whose 'social' is speaking. The social basis of the principle of the recontextualising constructs the perspective of the horizontal knowledge structure. Whose perspective is it? How is it generated and legitimated? I say that this principle is social to indicate that choice here is not rational in the sense that it is based on the 'truth' of one of the specialised languages. For each language reveals some 'truth', although to a great extent, this partial 'truth' is incommensurate and language specific. The dominant perspective within any transmission may be a function of the power relations among the teachers, or of pressure from groups of acquirers ..." (emphasis added).*

Kuhn (2012[1962]:77):

> *"Let us then assume that crises are a necessary precondition for the emergence of novel theories and ask next how scientists respond to their existence. Part of the answer, as obvious as it is important, can be discovered by noting first* ***what scientists never do when confronted by even severe and prolonged anomalies. Though they may begin to lose faith and then to consider alternatives, they do not renounce the paradigm that has led them into crisis****. They do not, that is, treat anomalies as counterinstances, though in the vocabulary of philosophy of science that is what they are. ...*
>
> *"No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature. That remark does not mean that scientists do not reject scientific theories, or that experience and experiment are not essential to the process in which they do so. But it does mean—what will ultimately be a central point—that the act of judgment that leads scientists to reject a previously accepted theory is always based upon more than a comparison of that theory with the world. The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgment leading to that decision involves the comparison of both paradigms with nature and with each other.*
>
> *"There is, in addition, a second reason for doubting that scientists reject paradigms because they are confronted with anomalies or counterinstances. ... The reasons for doubt sketched above were purely factual; they were, that is, themselves counterinstances to a prevalent epistemological theory. As such, if my present point is correct, they can at best help to create a crisis or, more accurately, to reinforce one that is already very much in existence. By themselves they cannot and will not falsify that philosophical theory, for its defenders will do what we have already seen scientists doing when confronted by anomaly. They will devise numerous articulations and ad hoc modifications of their theory in order to eliminate any apparent conflict. Many of the relevant modifications and qualifications are, in fact, already in the literature. If, therefore, these epistemological counter-instances are to constitute more than a minor irritant, that will be because they help to permit the emergence of a new and different analysis of science within which they are no longer a source of trouble" (emphasis added).*

Kuhn (2012[1962]:178):

> *"If [my] description has at all caught the essential structure of a science's continuing evolution, it will simultaneously have posed a special problem: Why should the enterprise sketched above*

move steadily ahead in ways that, say, art, political theory, or philosophy does not? Why is progress a perquisite reserved almost exclusively for the activities we call [natural] science? ...

"Notice immediately that part of the question is entirely semantic. To a very great extent the term 'science' is reserved for fields that do progress in obvious ways. Nowhere does this show more clearly than in the recurrent debates about whether one or another of the contemporary social sciences is really a science. These debates have parallels in the pre-paradigm periods of fields that are today unhesitatingly labeled [natural] science. ... [Some] argue that psychology, for example, is a science because it possesses such and such characteristics. Others counter that those characteristics are either unnecessary or not sufficient to make a field a science. ... [I]f precedent from the natural sciences serves, they will cease to be a source of concern not when a definition is found, but when the groups that now doubt their own status achieve consensus about their past and present accomplishments. It may, for example, be significant that economists argue less about whether their field is a science than do practitioners of some other fields of social science. Is that because economists know what science is? Or is it rather economics about which they agree? ...

"Throughout the pre-paradigm period when there is a multiplicity of competing schools, evidence of progress, except within schools, is very hard to find. ...

"We have, for example, already noted that once the reception of a common paradigm has freed the scientific community from the need constantly to re-examine its first principles, the members of that community can concentrate exclusively upon the subtlest and most esoteric of the phenomena that concern it. ...

"To that end it may help to point out that the transition need not (I now think should not) be associated with the first acquisition of a paradigm. The members of all scientific communities, including the schools of the 'pre-paradigm' period, share the sorts of elements which I have collectively labelled 'a paradigm.' What changes with the transition to maturity is not the presence of a paradigm but rather its nature. Only after the change is normal puzzle-solving research possible. Many of the attributes of a developed science which I have above associated with the acquisition of a paradigm I would therefore now discuss as ***consequences*** *of the acquisition of the sort of paradigm that identifies challenging puzzles, supplies clues to their solution, and guarantees that the truly clever practitioner will succeed" (Kuhn 2012[1962]:178).*

2.1 Sociality and Causality in Group Models

(White & Denham 2008:3):

"We emphasize three points here. First, mathematical models can be extremely useful as a counterpoint and theoretical sounding board as against ethnographic description, narrative history, and cultural theory. [However there exists] a large class of situations where both ethnographic description and modeling introduce empirically inappropriate or uncalled-for assumptions that need reconsideration. The more extreme of such unwarranted assumptions may surface as apparent paradoxes. Second, it is often with a weakening of the axioms of models and of the assumptions of the ethnography that we encounter fit between models and reality. Third, it is crucial to develop testable propositions and to put alternatives to the test as to how models and reality should fit according to these alternatives. These should involve robust criteria for evaluating the fit of alternative models, the accuracy of ethnographic data and interpretations, and whether weakening empirically inappropriate or uncalled for assumptions is the needed reconsideration."

2.2.1 Pre-Crisis Group Models: Tylor, Morgan, and Lévi-Strauss

(Morgan 1871:vi;14):

"In the systems of relationship of the great families of mankind some of the oldest memorials of human thought and experience are deposited and preserved. ...

"... As systems of consanguinity each contains a plan, for the description and classification of kindred, ... Whether as organic forms they are capable of crossing the line of demarcation which separates one family [of mankind] from another, and of yielding evidence of ***the ethnic connection of such families, will depend upon the stability of these forms*** *, and their power of* ***self-perpetuation in the streams of the blood through indefinite periods*** *of time. ... The results are contained in the [following] Tables. ...*

"A large number of inferior nations are unrepresented in the Tables, and to that extent the exposition is incomplete. But it is believed that they are formed upon a scale sufficiently comprehensive for the determination of two principal questions: First, whether a system of relationship can be employed, independently, as a basis for the classification of nations into a family [of mankind] and, secondly, whether the systems of two or more families [of mankind], thus constituted, can deliver decisive testimony ***concerning the ethnic connection of such families when found in disconnected areas [ordering]****? Should their uses for these purposes be demonstrated in the affirmative, it will not be difficult to extend the investigation into the remaining nations. ...*

"After ***the several forms of consanguinity and affinity, which now prevail in the different families of mankind****, have been presented and discussed, the important question will present*

*itself, **how far these forms become changed** with the progressive changes of society. The uses of systems of relationship to establish the **genetic connection of nations will depend**, first, upon **the structure of the system**, and, secondly, **upon the stability of its radical forms**. In form and feature they must be found able, when once established, to perpetuate themselves through indefinite periods of time" (emphasis added).*

(Tylor 1871:11-12):

*"A first step in the study of civilization is to dissect it into details, and to classify these in their proper groups. Thus, in examining weapons, they are to be classed under spear, club, sling, bow and arrow, and so forth; among textile arts are to be ranged matting, netting, and several grades of making and weaving threads; myths are divided under such headings as myths of sunrise and sunset, eclipse-myths, earthquake-myths, local myths which account for the names of places by some fanciful tale, eponymic myths which account for the parentage of a tribe by turning its name into the name of an imaginary ancestor; under rites and ceremonies occur such practices as the various kinds of sacrifice to the ghosts of the dead and to other spiritual beings, the turning to the east in worship, the purification of ceremonial or moral uncleanness by means of water or fire. Such are a few miscellaneous examples from a list of hundreds, and the ethnographer's business is to classify such details with a view to making out their distribution in geography and history, and the relations which exist among them. What this task is like, may be almost perfectly illustrated by comparing these details of culture with the species of plants and animals as studied by the naturalist. To the ethnographer, the bow and arrow is a species, the habit of flattening children's skulls is a species, the practice of reckoning numbers by tens is a species. **The geographical distribution of these things, and their transmission from region to region**, have to be studied as the naturalist studies the geography of his botanical and zoological species. **Just as certain plants and animals are peculiar to certain districts, so it is with such instruments as the Australian boomerang, the Polynesian stick-and-groove for fire-making, the tiny bow and arrow used as a lancet or phleme by tribes about the Isthmus of Panama, and in like manner with many an art, myth, or custom, found isolated in a particular field".** Just as the catalogue of all the species of plants and animals of a district represents its Flora and Fauna, so the list of all the items of the general life of a people represent that whole which we call its culture. And just as distant regions so often produce vegetables and animals which are analogous, though by no means identical, so it is with the details of the civilization of their inhabitants. How good a working analogy there really is between the diffusion of plants and animals and the diffusion of civilization, comes well into view when we notice how far the same causes have produced both at once. In district after district, the same causes which have introduced the cultivated plants and domesticated*

> *animals of civilization, have brought in with them a corresponding art and knowledge. The course of events which carried horses and wheat to America carried with them the use of the gun and the iron hatchet, while in return the old world received not only maize, potatoes, and turkeys, but the habit of smoking and the sailor's hammock" (emphasis added).*

(Tylor 1871:189):

> *"European scholars can still in a measure understand the belief of Greeks or Aztecs or Maoris in their native myths, and at the same time can compare and interpret them without the scruples of men to whom such tales are history, and even sacred history. Moreover, were the whole human race at a uniform level of culture with ourselves [Europeans], it would be hard to bring our minds to conceive of tribes in the mental state to which the early growth of nature-myth belongs, even as it is now hard to picture to ourselves a condition of mankind lower than any that has been actually found. But the various grades of existing civilization preserve the landmarks of a long course of history, and there survive by millions savages and barbarians whose minds still produce, in rude archaic forms, man's early mythic representations of nature".*

Here it becomes quite clear that individual elements in the set 'mankind', specifically the respective 'mental state' of Greek, Aztec, Maori, and (non-Greek) European men, are construed by Tylor as operating upon ('understanding') each other as members of a common mankind, but that in the course of doing so, that 'mental state' of each element is inverted from a uniform to non-uniform condition. Using the formalisations listed at Section 2.1.1 above, it might be possible to state that the function f ('understanding'), operates on each member of the group in such a way that it inverts its value, such that $f(European\ culture) \rightarrow Greek, Aztec, or\ Moari\ culture$. In Tylor's terms, European scholars' ability to "understand the belief of Greeks or Aztecs or Maoris in their native myths", while at the same time remaining part of a common family of man, has the effect of inverting or 'making other' the beliefs of those people.

2.2.2 Crisis Era Group Models: After Lévi-Strauss

(Appadurai 1988:39):

> *"[W]ere there ever natives, in the sense in which I have argued the term must be understood? Most groups that anthropologists have studied have in some way been affected by the knowledge of other worlds, worlds about which they may have learned through migration, trade, conquest, or indigenous narratives. As we drop our own anthropological blinders, and as we sharpen our ethnohistorical tools, we are discovering that the pristine Punan of the interior of Borneo were probably a specialized adaptation of the larger Dayak communities, serving a*

specialized function in the world trade in Borneo forest products; that the San of Southern Africa have been involved in a complex symbiosis with other groups for a very long time; that groups in Melanesia have been trading goods across very long distances for a long time, trade that reflects complex regional relations of supply and demand; that African 'tribes' have been reconstituting and deconstructing essential structural principles at their 'internal frontiers' for a very long time.

"Even where contact with large-scale external forces has been, till recently, minimal, as with some Inuit populations, some populations in lowland South America, and many Australian aboriginal groups, these groups have constituted very complex 'internal' mosaics of trade, marriage, conquest, and linguistic exchange, which suggests that no one grouping among them was ever truly incarcerated in a specific place and confined by a specific mode of thought (see, for example, Myers 1986). Although assiduous anthropologists might always discover some borderline examples, my general case is that natives, people confined to and by the places to which they belong, groups unsullied by contact with a larger world, have probably never existed" (emphasis added).

(Clifford 2005:26):

"[E]thnic groups, are subcultures of a wider polity ... They have no natural or autochthonous origin and must be articulated in situations of contact, overlap, and similarity. Populations, ideas, and practices routinely cross their borders and combine syncretically. ... [T]he sense of a group's distinction, its tradition or common culture, is always a secondary creation, not a primary cause or origin. Groups select certain traits with which they mark an identity, while trafficking among the many customs and practices they share with neighbours.

"Barth observes that groups often show quite dramatic internal variety in their ecological adaptations while nonetheless sustaining a sense of common group identification through a selective marking of culture traits" (emphasis added).

(Gupta & Ferguson 1992:6-10):

"Representations of space in the social sciences are remarkably dependent on images of break, rupture, and disjunction. ***The distinctiveness of societies, nations, and cultures is based upon a seemingly unproblematic division of space****, on the fact that they occupy 'naturally' discontinuous spaces. ...*

"This assumed isomorphism of space, place, and culture results in some significant problems. First, there is the issue of those ***who inhabit the border****, that 'narrow strip along steep edges'*

of national boundaries. ... A second set of problems raised by the implicit mapping of cultures onto places is to account for ***cultural differences within a locality****. ... Third, there is the important question of postcoloniality.* ***To which places do the hybrid cultures of postcoloniality belong****? ... Last, and most important, challenging the ruptured landscape of independent nations and autonomous cultures raises the question of understanding* ***social change and cultural transformation as situated within interconnected spaces****. The presumption that spaces are autonomous has enabled the power of topography to conceal successfully the topography of power.*

" ... [I]nstead of assuming the autonomy of the primeval community, we need to examine how it was formed as a community out of the interconnected space that always already existed. ... [B]y always foregrounding ***the spatial distribution of hierarchical power relations****, we can better understand the process whereby a* ***space achieves a distinctive identity as a place****. Keeping in mind that notions of locality or community refer both to a demarcated physical space and to clusters of interaction, we can see that* ***the identity of a place emerges by the intersection of its specific involvement in a system*** *of hierarchically organized spaces with its cultural construction as a community or locality ...*

"It is for this reason that what Jameson (1984) has dubbed 'postmodern hyperspace' has so fundamentally challenged the convenient fiction that mapped cultures onto places and peoples. ... In the pulverized space of postmodernity, space has not become irrelevant: ***it has been reterritorialized*** *in a way that does not conform to the experience of space that characterized the era of high modernity. It is this that forces us to reconceptualize fundamentally the politics of community, solidarity, identity, and cultural difference ...*

"The apparent ***deterritorialization of identity*** *that accompanies such processes has made Clifford's question (1988:275) a key one for recent anthropological inquiry: 'What does it mean, at the end of the twentieth century, to speak . . . of a native land? What processes rather than essences are involved in present experiences of cultural identity?' ... In this culture-play of diaspora, familiar lines between 'here' and 'there,' center and periphery, colony and metropole become blurred" (emphasis added).*

(Jameson 1984:80-83):

"[T]his latest mutation in space - postmodern hyperspace - has finally succeeded in transcending the capacities of the individual human body to locate itself, to organize its immediate surroundings perceptually, and cognitively to map its position in a mappable external world. And I have already suggested that this alarming disjunction point between the

body and its built environment—which is to the initial bewilderment of the older modernism as the velocities of space craft are to those of the automobile—can itself stand as the symbol and analogue of that even sharper dilemma which is the incapacity of our minds, at least at present, to map the great global multinational and decentred communicational network in which we find ourselves caught as individual subjects. ...

"My implication is that we ourselves, the human subjects who happen into this new space, have not kept pace with that evolution; there has been a mutation in the object, unaccompanied as yet by any equivalent mutation in the subject; we do not yet possess the perceptual equipment to match this new hyperspace, as I will call it, in part because our perceptual habits were formed in that older kind of space I have called the space of high modernism. The newer architecture therefore—like many of the other cultural products I have evoked in the preceding remarks—stands as something like an imperative to grow new organs, to expand our sensorium and our body to some new, as yet unimaginable, perhaps ultimately impossible, dimensions."

(Sahlins 2008:49):

"Ethnographic reports speak of 'the transpersonal self' (Native Americans), of the self as 'a locus of shared social relations or shared biographies', of persons as 'the plural and composite site of the relationships that produced them' (New Guinea Highlands). Referring broadly to the African concept of 'the individual,' Roger Bastide writes: 'He does not exist except to the extent he is 'outside' and 'different' from himself.' Clearly the self in these societies is not synonymous with the bounded, unitary and autonomous individual as we know him in particular, as in our social theory if not our kinship practice. Rather, the individual person is the locus of multiple other selves with whom he or she is joined in mutual relations of being; even as, for the same reason, any person's self is more or less widely distributed among others. ...

"In sum and in general, in kin relationships, others become predicates of one's own existence and vice versa. I do not mean the interchange of standpoints that is a feature of all direct social relationships according to the phenomenologists. I do mean the integration of certain relationships, hence the participation of certain others in one's own being. And if ''I am another,' then the other is also my own purpose. As members of one another, kinsmen lead each other's lives and die each other's deaths. One works and acts in terms of relationships, with others in mind, thus on behalf of one's child, cross-cousin, husband, clansmen, mothers' brother, or other kinsperson. In this regard, Marilyn Strathern observes of New Guinea peoples, neither agency nor intentionality is a simple expression of individuality, inasmuch the being of the other is an internal condition of one's own activity. Not only work but consumption itself is

'no simple matter of self-replacement,' Strathern notes, 'but the recognition and monitoring of relationships.' Unlike the classic bourgeois individualism, the body is not the private possession of the individual. A body is the responsibility of the micro-community that feeds and cares for it" (emphasis added).

2.3.1 Early Pre-Crisis Inception of Group Theory: Emergence of the KLRE Model

(Howitt 1904:41-42; 50-51):

"I use the word 'tribe' as meaning a number of people who occupy a definite tract of country, who recognise a common relationship and have a common speech, or dialects of the same. The tribes-people recognise some common bond which distinguishes them from other tribes, usually a tribal name, which may be their word for 'man', that is, an aboriginal of Australia. ...

"But while individual tribes are thus distinguished from others, there are numerous cases in which the word for 'man' is common to the languages of a considerable number of more or less nearly related tribes, indicating a larger aggregate, for which, in default of a better term, I use the word 'nation' ... "

"In all the native tribes of Australia there are geographical divisions of the community determined by locality, and also divisions of the tribe on which the marriage regulations are based. The former are distinguished by certain local names, while the latter are denoted by class names, or totems, and more frequently by both class names and totems. ...

"The tribes to the east of the Grey and Barrier Ranges form two nations, which with another, ..., on the opposite side of the Darling, occupy practically the whole of its course from the Barwon River to its junction with the Murray.

"The two former nations comprise the following tribes ...

"There are three other tribes which I have not been able to assign to either of these nations. ...

"All the tribes forming the above-mentioned nations are bound together over an enormous district by the same two class system [i.e. kinship terminology comprised of two terms], having the same names for the classes, and being not only distinguished thereby from the analogous great organization of which the Dieri [tribe] is a representative, but also from other tribal groups to the south, now to be described."

(Howitt 1904:43):

"As I have said, the tribe is also divided into a number of lesser groups, say X, Y, Z, etc. (I take these for the sake of convenience, but there may be many more); these divisions of the tribe are local, and therefore differ essentially from the classes or totems, which are groups of the social organization. In order to prevent confusion between the lesser division of each of these different organizations of the tribe, the term 'clan' is used for the subdivision of a tribe which has descent in the male line, and 'horde' for that in which there is female descent. The clan and the horde are each therefore a geographical division of a tribe.

"Thus the local organizations, X, Y, Z, etc., are made up of individuals belonging to both A and B. The children of the horde belong to the horde, e.g. the children of X males are of X horde and so forth, but since A (male) must marry B (female) with descent in the female line, the son of X is XB. In other words, the son is of the father's horde and tribe, but of the mother's totem and class; of the local division to which the father belongs, but of the mother's social division. In extreme cases, where descent has come to be in the male line, the hordes or clans as well as the social divisions are found to be exogamous; the exogamic law has, seemingly, passed over from the social to the local division, which therefore in such cases regulates marriage. It is as if an English village had determined that its children should marry beyond its bounds, the sons bringing their wives to the village, while the daughters went to the villages whence their brothers took their wives."

(Howitt 1904:153-155):

"Dr. Frazer's views are, shortly, as follows.' The Intichiuma ceremonies [of the Arrernte language community of arid Central Australia] appear to indicate that each totem group was charged with the superintendence of some department of nature, from which it took its name. The control was by magical means to procure for the members of the community, on the one hand, a plentiful supply of all the commodities of which they stood in need; and, on the other hand, an immunity from all the perils and dangers to which man is exposed in his struggle with nature.

"I could more easily imagine that these early savages might, through dreams, have developed the idea of relationships with animals, or even with plants. ...

"The hypothesis suggested by Professor Haddon is that groups of people, at a very early period, by reason of their local environment, would have special varieties of food. ...

"Now compare such a series of tribes with regard to these magical food-producing ceremonies, and also as to the climatic conditions under which they live. We shall find that the Lake Eyre tribes are under a minimum rainfall, a very high temperature, and a prevailing aridity, with

fertile intervals, when there is abundance of animal and vegetable food supplies. At the further end of the series, whether in Queensland, New South Wales, Victoria, or South Australia, the tribes living, say, on the coast lands, are under climatic conditions very different from those of Central Australia, with a good rainfall, a more temperate climate, and a plentiful and constant food supply, both animal and vegetable. This comparison comes out clearly when the tables of rainfall, given in the introductory chapter, are inspected. This comparison will fall in line with former conclusions, namely, that the tribes of the Lake Eyre basin have remained in a far more primitive condition socially than those of the south-east of Australia. If so, it would point to conditions of better climate, and more abundant and regular food supply, as potent causes in the advancement of the social condition of the south-east tribes."

(Morgan in Fison & Howitt 1880:2-3; 7):

"The Australian tribes are melting away before the touch of civilization, even more rapidly than the American aborigines. In a lower ethnical condition than the latter, they have displayed less power of resistance. They now represent the condition of mankind in savagery better than it is elsewhere represented on the earth - a condition now rapidly passing away, through the destructive influence of superior races. Moreover, it is a condition of society which has not hitherto been thought worthy of special scientific investigation, although it is one of the stages of progress through which the more advanced tribes and nations of mankind have passed in their early history, and although some of the more important institutions of civilized states must be sought, in their rudimentary forms, in this very condition of savagery in which they originated. In a few years nothing will be known of the arts, institutions, manners, customs, and plan of life of savage man, except as they are preserved in memoirs like the present.

...

" A comparison of the several forms of the gentile organization among different races, leaves no doubt that it is the same organization wherever found, but in different stages of development. ... It is shown by the composition of the group, by the mutual obligations of its members, by the rules in respect to marriage, descent, and inheritance, and of the character and tenure of the office of chief, in which there is a substantial concurrence among them all, with narrow limits of variation. It shows that the principal races of mankind, white, red, yellow, and black, derived this organization from their ancestors in a far anterior condition of the respective societies which gave it birth, and that the organization was transmitted to their several descendants, who are now found upon all the continents. We are thus enabled to trace, by its uniformity, the operations of the human mind, in its upward progress from savagery to civilization ..."

2.3.2 Late Pre-Crisis Consolidation of Group Theory: Reorganization of the KLRE Model

(Verdon & Jorion 1981:95):

> *"A corporation uses specific criteria of membership which give equal rights of ownership to all those who qualify; there are no possible gradients of rights varying from individual to individual [i.e. the group is closed under uniform function]. A corporation thus presupposes 'boundaries' in membership, and because of this also delineates its resources. If land is the resource to be owned by a corporation, we should expect this land to be bounded. We call such boundaries 'jural', and 'estate' the bounded resources owned by a corporation."*

(Verdon & Jorion 1981:96):

> *"Criteria of membership defining groups, categories and corporations confer equal rights upon those who meet the criteria. Where ownership is neither individual nor corporate, however, rights actually vary in their intensity; stemming, as they do, from occupancy and use, the rights are to be measured (subjectively) according to the time and intensity of occupancy and use."*

(Verdon & Jorion 1981:99):

> *"These 'degrees of spiritual danger' are further matched by 'degrees of esotericism' in the religious knowledge owned by the totemic corporations, and we could represent this variation by a series of concentric circles. The inner core of that knowledge would consist of that part of the knowledge which is most closely associated with the totemic sites, and which empowers its 'knowers' (the native theologians, or scholars) to reproduce the species. This knowledge is the most esoteric, and is strictly reserved to those who are ancestral reincarnations, who have been initiated and who are the closest to the ancestor in their life-cycle (that is, the closest to death, when the ultimate fusion with the ancestor-totem takes place). Those who have access to the inner core also have access to the less esoteric knowledge. Individuals outside the totemic corporation are excluded from the inner core knowledge, but they can be taught the knowledge contained in outer rings. To be accurate, we must consequently modify our earlier statement, and mention that the totemic corporation is defined with respect to the ownership of the most esoteric knowledge only; other degrees of knowledge, while under the corporation's control, can be imparted to outsiders."*

(Verdon & Jorion 1981:100):

> *"This ontological distance which governs access to the various levels of religious knowledge is also used as a scale to measure a 'gradient of rights of ownership' which stem, to a certain*

degree, from occupancy and use of the land surrounding the totems. In Aboriginal Australia, land ownership is thus complicated by the religious nature of the environment. One can only occupy and exploit the resources of a given area if one knows where the 'mines' (the dangerous places) are, and can avoid spiritual accidents. The problem thus has two facets: on the one hand, ownership is associated with intensity of use and occupancy, and on the other, this use and occupancy depend upon a prior religious knowledge, the rights to which are measured by an individual's ontological distance from the ancestor."

3.2.1 Morgan, Howitt, Radcliffe-Brown, & Elkin: Consanguinity & Affinity as Ancillary Orders

Fison & Howitt (1880:32):

"The chief object of this memoir is to trace the formation of the exogamous intermarrying divisions which have been found among so many savage and barbaric tribes of the present day, and to show that what the Hon. Lewis H. Morgan calls the Punaluan family, with the Turanian system of kinship, logically results from them. The Australian classes are especially valuable for this purpose, because they give us what seem to be the earliest stages of development."

Fison & Howitt (1880:342):

"The tribes which are discussed in this work may be placed in a series, in accordance with the peculiarities of their several social organizations. This series would commence with tribes such as the Dieri and the Kamilaroi, which have modified group marriage, the exogamous class divisions, descent in the female line ... It would proceed through such tribes as the Turra, having the exogamous class divisions, but with descent through the father ... The series would terminate with such tribes as the Kurnai, in which the class divisions survived only in a modified form; in which individual marriage was established, and the characteristics of group marriage are only discernible, more or less indistinctly, in surviving customs."

Howitt (1904:137):

"I may note here that the boundaries of a class system [a given terminological group] are usually wider than those of a tribe, and that the boundaries of any one type of system have a still wider range, and include those aggregates of tribes which I have termed nations. All such aggregates are bound together by a community of class organization which indicates a community of descent."

"It is not possible in the space here available to undertake detailed sociological analysis of the Australian organization. But a brief discussion seems desirable in order to remove misconceptions that have arisen in theoretical discussions.

Radcliffe-Brown (1929:411):

"[T]he male members of a horde ... all, belong to one patrilineal moiety (consisting of two of the sections or four of the sub-sections)."

Radcliffe-Brown (1931:427):

"The first question that requires to be dealt with is that of the relation between social organization and the terminology of kinship. There are two views on this subject that I wish to controvert. One is the view of Lewis Morgan, adopted from him by Howitt ... , which is to the effect that the kinship terminology of Australian tribes is not correlated with the existing social organization but is correlated with and has its origin in a hypothetical ['primitive'] condition in which individual marriage did not exist, but groups of men were united in some sort of marriage bond with groups of women. The second view is one which is held by Professor Kroeber, that there is in general no very close correlation between the kinship terminology of a people and their social institutions."

Radcliffe-Brown (1956:635):

"The 'clan' ... is a small body of patrilineal kin. I introduced the term 'horde' in order to avoid certain ambiguities in the word 'clan'. A clan is ... a 'unilineal descent group'. A child belongs to the clan of its father. The clan is corporate in the sense that its adult male members can and do engage in collective action, and that as a clan they have collective ownership and control of ***a certain territory with its food resources and its 'totem centers' with their associated rites and myths****. The 'horde' is a collection of [patrifilial] families which* ***regularly co-operate in the food quest****, a [patrifilial] family consisting of a man with his wife or wives and their unmarried children.* ***The unity of the horde and its connection with a certain territory result from the fact that all the married men of a given horde are members of one particular clan****. A woman belongs to her father's clan but to her husband's horde" (emphasis added).*

3.2.2 Berndt, Hiatt, Meggitt, and Stanner: Space & Time as Ancillary Orders of Kinship

Berndt (1959:83-84):

"Elkin defines a tribe as consisting of people 'related by actual or implied genealogy, who occupy and own a definite area of territory,' use a common language or dialect, are known by a

distinct name or otherwise claim (or are acknowledged by others as having) some sort of separate identity, and share a common cultural background.

"Elkin mentions that such a definition does not fit all empirical cases, and that the important test is the linguistic one: but he admits that dialectal variation may occur within such a unit. The tribe, he says, is both a linguistic and a territorial group, but it is not important economically and politically ... The basic composition of a tribe entails subdivision into local groups.

"In comparing Elkin's conception of an Australian tribe with that of Radcliffe- Brown, apart from differences in emphasis it is clear that there is basic agreement. Further, they conform broadly with the general categorization noted above, except that in the Australian material the aspect of territorial integrity is viewed in terms of a collection of segments called 'local groups' and it is these which are the land-owning units. The probably crucial factor of interaction between members of a tribe is not explicitly brought out, although it might be inferred from (for example) Elkin's statement on tribal composition, as comprising people related through actual or implied kinship. Radcliffe-Brown, however, suggests that this should not be taken as a criterion, since kin ties extend over arbitrarily demarcated tribal boundaries. It is nevertheless this aspect of interaction, whether or not it is genealogically articulated, which I would consider of some importance in any definition of the widest social unit - in this case the tribe. The point, then, is to see what significance it actually has; and the procedure here will be to take up a number of features rather than to present a mass of empirical material."

Berndt (1959:91; 93):

"Our contention, ... in which we are in basic agreement with Elkin, is that these groups [aggregations of 'local groups'] are not 'tribes', that there are no strict boundaries, that movements were relatively frequent, and that what we are faced with is, rather, a cultural and social bloc" (ibid., 91).

Berndt, following Radcliffe-Brown's 1923 strategy, then provides a bullet-point list of definitions:

"Without further demonstration, we can say:

"(1) That [a given] area may be distinguished linguistically;

"(2) That a great deal of movement among local groups has taken place; one question here is the significance of local group 'country' and anchorage. ...

"(3) That the names used [for these areas], and usually called tribal names, are actually not such ... They are ways of classifying groups of people according to dialect variations. This is done without emphasizing local 'anchorage' and has no direct territorial significance" (ibid., 93).

Berndt (1959:105):

"My purpose has been, primarily, to delineate the widest functionally significant social unit - a unit which we can broadly define in terms of the interaction of its members, on a relatively regular and consistent basis. ... The significance of this wider unit rests primarily on the degree of interaction taking place among its members. Traditionally, those who ... occupy contiguous stretches of country would more probably be found coming together for seasonal meetings, and contacts between them would be stronger than with those further away. But this nucleus, by no means fixed since wandering was the norm, would consist of members of different local groups, different hordes and different dialect units. We cannot speak of it as a kin group, although relationships between members included in it would be articulated in kin terms. Further, representatives of more distant local groups and hordes might be present: the occasional coming together of those who are for the greater part of the year living apart, visits from people from areas relatively far away, are a notable feature of such gatherings. It is those who meet regularly and consistently, even if intermittently - and are closely involved in reciprocal duties and obligations - who make up the widest functionally significant group."

Hiatt (1962:286):

"It is now clear that the male members of several totemic descent groups did not live together on single pieces of land. They commonly lived in communities that contained male members of several totemic descent groups and regularly sought food over areas that included totemic sites other than their own."

Hiatt (1968:99-100):

"There is no quarrel with Radcliffe-Brown's statements about the tribe as a linguistic division or the patriclan [the totemic descent group] as a land-owning unit. The argument is about the horde - more precisely, about residential associations, exploitation of resources, and group movements. ...

"[O]bservers since 1930 have reported that unrestricted movement of foodseekers over broad regions that included the totemic sites of many [totem-defined] patricians. This does not mean that all comers had free access to every locality. A Walbiri community, for example, included

members of from six to twelve patricians who habitually ranged over a defined area that included the totemic estates of the constituent clans. But they entered the territories of other communities on invitation."

Meggitt (1964:170):

"[I]t seems to me that, generally, considerations of kinship and descent per se were minimized in defining local groupings, whereas the fact of locality was in itself largely irrelevant in defining kin-groups.

"At this juncture the main point to be made about local groups is that, although the 'tribe' was ideally the maximal local unit, in fact the whole of its members rarely, if ever, assembled as a functioning entity. Instead, a tribe usually comprised a number of scattered local groups, among which the patrilineal, patrilocal horde was a rarity despite Radcliffe Brown's assertions ... Indeed the number, size, composition and interrelations of local sub-groups within a tribe varied with the ecological region, with the seasons, with current disputes and so on."

Meggitt (1972:80-82):

"Activities such as hunting and gathering, ritual performance, and childbirth must take place in some locality or other. ***Given the strain toward consistency already remarked on in the Walbiri categorical view of their universe, it is not surprising that they bring together their male-female and noumenal-phenomenal distinctions in spatial terms****. Because of the actions of dreamtime heroes and other totemic beings, the land included in the tribal territory and its environs is not a neutral entity. On the one hand, it functions, especially for the men, as a timeless chart of the continuing noumenal causality of the dreamtime ... But that same land is the material support of the phenomenal flora and fauna on which the Walbiri, as contemporary human beings, depend for their subsistence. In that sense, they see the land as the resource base that they exploit, and shrewdly, too.* ***This dual recognition of their environment as [religious] icon and as [economic] material resource is expressed even in the ordering of their camp*** *...*

"At this point the reader may well ask what all this has to do with the Walbiri system of kinship. First, let me repeat an earlier assertion: if we confine ourselves solely to analysis of the dyadic relationships expressed in that system whether discretely or in sets, there is no way of accounting for the other manifestly significant features of Walbiri society that I have been discussing. What is more important, their kinship system, observed as a structured pattern of behavioral interactions, is indeed integrated with the wider scheme of categorization but brought into it, as it were, from below. That is to say, moieties, sub-sections, and lines of

*descent or filiation do not emerge contingently from the behavioral process as the result of a simple addition of particular, similar dyadic relationships exhibited on specific occasions. Rather, the broader categories that are in effect deduced from fundamental metaphysical postulates of the Walbiri in varying combinations are systematically superimposed on and engage the ongoing kinship matrix or process to denominate appropriate actors and activities. Here the mediating or articulating function of the sub-section system is of great significance**. It is by this means that the kinship system, including terminology, as an expression of the people's behavior and thought, becomes continuous with and part of the total code or schema** [i.e. the superordinate group theoretic object or 'tribe'], in somewhat the same way as the spatial aspects of that behavior are incorporated into the code [i.e. the spatial order appended to subgroups and elements within the group]."*

Meggitt (1972:82):

"[The kinship] code helps Walbiri to make cognitive sense of their surroundings, but in addition it is part of, and displays, a thoroughgoing monism that is inevitably axiologically normative. In this respect the code not only makes statements that imply moral obligation, whether on the part of men, animals, plants, or meteorological phenomena, but it also specifies the way in which the sanctity of such propositions can be verified. They all stem from (and in terms of Walbiri postulates can be shown to do so) assertions about the nature of the creative period, the dreamtime. For this reason the code is doubly satisfactory; it accounts for events, especially men's actions, without admitting the possibility of refutation and, by virtue of attributing necessity to these actions, commands them without appearing to be arbitrary."

Stanner (1965:2):

"The relations between a group and its territory are among other things ecological relations. The relations between members of the group in respect of the territory in itself, or as a locality of a larger entity, are among other things social relations. Territoriality and sociality lie in different systems of relations. The conventional approach to local organization has tended to compound and thus to blur the distinctions. The result has been a certain ambiguity and confusion. The study is inherently interdisciplinary. It may be said in Radcliffe-Brown terms to fit in between the studies of the first and second of three connected adaptative systems: the ecological, the institutional or social, and the cultural. (The 'ritual relations' between a group and its territory are of course one among several classes of social relations.) The approach made in this paper is based on that contention.

"The pattern of aboriginal territoriality should be looked at first from an ecological point of view. The evidence allows us to say that each territorial group was associated with both an estate and a range. The distinction is crucial. The estate was the traditionally recognized locus ('country', 'home', 'ground', 'dreaming place') of some kind of patrilineal descent-group forming the core or nucleus of the territorial group. It seems usually to have been a more or less continuous stretch. The range was the tract or orbit over which the group, including its nucleus and adherents, ordinarily hunted and foraged to maintain life. The range normally included the estate: people did not usually belong here and live there but, in some circumstances the two could be practically dissociated. Estate and range together may be said to have constituted a domain, which was an ecological life-space."

Stanner (1965:16; 19):

"I do not think there can be serious question of four things.

"(1) Some sort of exogamous patrilineal descent-group was ubiquitous.

"(2) It had intrinsic connection, not mere association, with a territory.

"(3) There was a marked tendency towards, though no iron rule requiring, matrilocality and virilocality.

"(4) The group thus formed was basic to both territorial and social organization, however concealed by other structural groups (e.g. phratries, moieties, sections, etc.) or by dynamic emphases.

"The descent-group was necessarily divided by out-marriage. The 'perpetual to and fro' (Radcliffe-Brown) further distributed it. So did personal inclinations, family reasons, quarrels, ritual occasions, and the dispersal-clustering sequence. Matrilineal and affinal factors could obscure its manifestations. There was probably no occasion or situation when all its members collected for any purpose. It was not as visible, or as discrete, a group - indeed, not a 'group' in the same sense as the collections amongst which its members might be distributed. The most discrete of such collections was the horde or band. The size, composition and duration of this, a veritable, visible but variable group, were determined by many forces."

And:

"The idea that a collection of descent-groups could as a normal rule of life wholly detach themselves from all territorial limits and with entire freedom indiscriminately share a countryside, so that the collective life-space was unstructured - this seems to be the import of

Hiatt's argument. It is quite unestablished as a rule for all Australia. Territoriality and the institutional and cultural arrangements that went with it, were more than mere reference-data consulted so as to keep large affairs straight."

3.2.3 Lévi-Strauss: Normativity as an Ancillary Order

Hiatt (1968:65; 174):

"Lévi-Strauss attaches fundamental importance to mechanisms of reciprocity. He believes that marriage regulations and kinship systems can best be understood by regarding them as 'a kind of language, a set of processes permitting the establishment, between individuals and groups, of a certain type of communication' ...

"I [have] argued that ... [Indigenous] people were not using their kinship system as a communication apparatus of the kind postulated, and that they gave little indication of recognizing this as a possibility or of holding it as an ideal. The discrepancy between the model and the facts is wide."

Hiatt (1968:211-212):

"Lévi-Strauss says that it has become fashionable to deny that rules stated in the language of preferred degrees and in the language of sections or subsections can be translated back and forth by the aborigines at will. If by 'preferred degrees' he means categories of kin with whom a marriage would be morally proper, then it is clear from my paper that I do not subscribe to the fashion. What I deny is that a marriage rule stated in terms of kin categories or subsections is equivalent to a rule defining a man's right to only certain women within these categories or subsections. ...

"The fact remains, as [Lévi-Strauss'] final remarks indicate, that he does believe that his analytical models have empirical counterparts ... . I have argued that there is little evidence of either a statistical or ideological kind to support such a view ... ; elsewhere [Hiatt 1965] I have tried to demonstrate that there is even less evidence for an extended network of exchange."

Lévi-Strauss (1968:211):

"I agree that it is important to find out what really happens in any society, notwithstanding its proclaimed rules. ***However, my past work on kinship has been mostly concerned with a different problem: to ascertain what was the meaning of the rules, whether they be applied or not. For rules have their own life if only in the mind of the people who are able to state them and to translate them back and forth at will in the language of preferred degrees or in***

***that of sections and subsections**. This should suffice to convince us that these two languages [rules and terminologies] are not as wholly disconnected as it has become fashionable to pretend. Finally, it takes more than mere naiveté to believe that since the theoretical model of [a kinship] system requires only four patrilines [as in 'Aranda type' subsection terminologies], the system itself could not ensure marriage exchange between 28 clans [i.e. patrilineal descent groups] or many more. This empirical complexity [is] commonplace **... For the network can be woven on a shifting basis where the four lines do not need to remain the same ones**" (emphasis added).*

Lévi-Strauss (1969[1949]:xxxiv):

"[I]n the case of both elementary and complex systems, the use of the term 'preferential' does not suggest a subjective inclination on the part of individuals to seek marriage with a certain type of relative. The 'preference' expresses an objective situation ... in which, in the absence of a clearly formulated prescription, the proportion of marriages between a certain type of real or classificatory relative (taking 'classificatory' in a vaguer sense than defined by Morgan) is higher than were it the result of chance, whether the members of the group are aware of this or not. This objective proportion reflects certain structural properties of the system. If we were able to isolate these properties, they would turn out to be isomorphic with those which are directly knowable in societies which exhibit the same 'preference', disguised, however, as a prescription, and in practice obtaining exactly the same result.

In the case of [direct exchange on one hand, and generalized exchange on the other], the result would be, on the one hand, alliance networks ideally tending to close themselves (although not necessarily doing so), and on the other hand, and more especially, relatively long networks, as compared with those to be observed or imagined in societies with ... a correlative shortening of the cycles.

"In other words, I do not dispute that there is an ideological distinction to be made between the prescriptive or preferential forms of a type of marriage. But the extreme terms always allow for a continuous series of intermediary applications. I postulate, therefore, that this series constitutes a group, and that the general theory of the system is only possible in terms of this group and not in terms of some particular application or other. The system should not be broken down, reduced by analysis to the various ways in which men may choose to represent it. Its nature arises objectively from the type of divergence engendered between the form taken by the alliance network of a society, and the form which would be observed in this same society if marriages were made at random. Fundamentally, the sole difference between prescriptive marriage and preferential marriage is at the level of the model. It corresponds to the difference

which I have since proposed between what I call a 'mechanical model' and a 'statistical model'; i.e., in one case, a model the elements of which are on the same scale as the things whose relationships it defines: classes, lineages, degrees; while in the other case, the model must be abstracted from significant factors underlying distributions which are apparently regulated in terms of probabilities'.

Lévi-Strauss (1969[1949]:119):

"The very nature of the principle of reciprocity allows it to act in two different and complementary ways, either by setting up classes [realised by sociocentric kin terms] which automatically delimit the group of possible spouses, or by the determination of a relationship or a group of relationships [realised by egocentric kin terms], so that in each instance it can be said whether a prospective spouse is to be desired or excluded. Both criteria are given simultaneously, but their relative importance varies. The class [i.e. sociocentric kin term] is of the greater importance in dual organizations or in societies with marriage classes [such as in Australia], whereas on the other hand the relationship [i.e egocentric terms] is used first of all, in a negative form, with the simple prohibition on incest [such as in Britain]."

Lévi-Strauss (1969[1949]:199):

*"[T]here is one special case in which both aspects of the principle of reciprocity co-exist, or rather have the same relative importance, and where they overlap exactly and cumulate their effects. This is marriage between cross-cousins. In this, more than any other, the class and the group of individuals determined by the relationship are co-extensive. This is why Morgan, Tylor and Frazer were led to observe that dual organization had the same kinship terminology as systems of cross-cousin marriage, and that the cross-cousins were distributed as if they belonged to different moieties. In fact, cross-cousin marriage is to be distinguished from the prohibition of incest in that the latter employs a system of negative relationships [*proscription*], and the marriage of cross-cousins a system of positive relationships [*prescription*]."*

Lévi-Strauss (1969[1949]:136):

On this issue, Lévi-Strauss was explicit, targeting Frazer (1890) as the most prominent proponent of Morgan and Tylor's original theories:

"Seeing the connexion between cross-cousin marriage and marriage by exchange should have led to the discovery of the universal, permanent, and basic structure of marriage. By contrast, Frazer saw cross-cousin marriage as an historical form of marriage, marriage by exchange as another such form, and he concentrated on placing these, together with other forms such as

*dual organization and the classificatory system, in a temporal sequence, and on setting up causal connexions between them. What we regard as the means of escaping from cultural history he attempted to interpret **as** cultural history. The very thing we see as a necessary condition of society, he sought to analyse into stages of social evolution. It seems moreover that Frazer has some inkling of the possibilities laid open by his theory, but he mentions them only to recoil immediately in horror. With regard to Australian marriage classes, which are always even in number, he writes:*

> *"'This suggests, what all the evidence tends to confirm, that these various groups have been produced by the deliberate and repeated bisection of a community, first into two, then into four, and finally into eight exogamous and intermarrying groups or classes; for no one, so far as I know, has yet ventured to maintain that society is subject to a physical law, in virtue of which communities, like crystals, tend automatically and unconsciously to integrate or disintegrate, along rigid mathematical lines, into exactly symmetrical units.'*

"We certainly would never think of comparing societies with crystals. But if, as we try to show here, it is true that the transition from nature to culture is determined by man's ability to think of biological relationship as systems of oppositions ... and if it is true, finally, that exchange is the immediate result of these pairs of oppositions and that the dichotomy of cousins reflects the exchange, then no doubt it could be said that: 'Human societies tend automatically and unconsciously to disintegrate, along rigid mathematical lines, into exactly symmetrical units.' But perhaps it must be acknowledged that duality, alternation, opposition and symmetry, whether presented in definite forms or in imprecise forms, are not so much matters to be explained, as basic and immediate data of mental and social reality which should be the starting-point of any attempt at explanation."

APPENDIX 2 Discovered Sociocentric Kin Terms for the CMDB Population

Table 11: Table of individuals identified in the Unified kinship network for South East Australia, for whom socioentric kin terms have also been discovered in the ethnographic record.

RIN	TOTEM	SECTION	MOIETY	BLOOD[73]	LANGUAGE	SOURCE
153	Nguri	Kambu		Gwaimandthan		Radcliffe-Brown, A.R. 1915-1916. Notebook E2. University of Sydney Archives (P129.1-7).pdf
175		Buda			Wongaibon	Tindale, N. 1939. Menindee Genealogies. MS. Tindale Archives, South Australian Museum
177		Muri			Wongaibon	Tindale, N. 1939. Menindee Genealogies. MS. Tindale Archives, South Australian Museum
178		Kabuda			Wongaibon	Tindale, N. 1939. Menindee Genealogies. MS. Tindale Archives, South Australian Museum
178	Jungkai (mallee-hen)	Puta	Ngilpungara			Berndt, R.M. & Berndt, C. 1943. A Short History of Acculturation at Menindee Government Station, Darling River. MS. University of Sydney Archives P130-3-29.pdf
179		Ipai			Wongaibon	Tindale, N. 1939. Menindee Genealogies. MS. Tindale Archives, South Australian Museum
179	Maruwai (red kangaroo)		Makungara		Wongaibon	Berndt, R.M. & Berndt, C. 1943. A Short History of Acculturation at Menindee Government Station, Darling River. MS. University of Sydney Archives P130-3-29.pdf
257	Goro (Bandicoot)	Mada			Weilwan	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
258	Goro (Bandicoot)	Kabuda			Weilwan	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
260		Mada			Weilwan	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
263	Nguri (emu)	Kambo	Ngarawan	Gwaimandthan		Radcliffe-Brown, A.R. 1915-1916. Notebook E2. University of Sydney Archives (P129.1-7).pdf
276		Ibada				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
344	Guru (Bandicoot)	Kumbo	Muggulu		Wongaibon	Mathews, R.H. ND. Notebook 4 'Darling River & Cobar Notes'. National Library of Australia (MS 8006. Series 3. Folder 6).pdf
360	Padimelon	Mari				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
361	Emu	Buda				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
566		Murri		Guaimundhun	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41) 67-87
567		Matha		Guaigulir	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41) 67-87
571		Hipai				Reay, M. 1945. A half-caste Aboriginal community in northwestern New South Wales..pdf
629	Kuru (bandicoot)	Mari			Wongaibon	Tindale, N.B. 1939. Menindee Genealogies. MS. Tindale Archives, South Australian Museum.pdf
731	Tuli (goana)	Kabuda			Kamilaroi	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
989			Marrain (animals with hair)		Murawarri	Radcliffe-Brown, A.R. 1915-1916. Notebook E2. University of Sydney Archives (P129.1-7).pdf
990	Marawei (kangaroo)		Makangara	Gwaigulir		Radcliffe-Brown, A.R. 1915-1916. Notebook E2. University of Sydney Archives (P129.1-7).pdf
1051	Guru (Bandicoot)	Kabuda				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
1298	Possum (kurakai)					Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
1335	Bilba	Madha			Wailwan	Mathews, R.H. ND. Notebook 7b Untitled. National Library of Australia (MS 8006. Series 3. Folder 7).pdf
1368	Guru (Bandicoot)	Kabi				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
1449	Emu	Ibada				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
1750		Matha		Guaimundhun	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41) 67-87
1751		Ippai		Guaimundhun	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41) 67-87

[73] Despite multiple attributions of Blood terms, only two instances of Shade terms have been discovered for individuals whose position in the Unified kinship network has also been discovered. These two terms have been listed as footnotes listed in the Blood column for the two relevant individuals, RIN 2230 and RIN 72851.

1797	Goro (Bandicoot)	Kabuda			Weilwan	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
1991	Turu (snake)				Weilwan	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
2007			Kilpara		Bakindji	Tindale, N. 1939. Menindee Genealogies. MS. Tindale Archives, South Australian Museum
2063	Emu	Kambu				Radcliffe-Brown, A.R. 1929. Notebook E9. University of Sydney Archives (P129.1-14).pdf
2089		Madha				Radcliffe-Brown, A.R. 1930. Field and research notes and genealogies. MS. University of Sydney Archives (P130-03-45).pdf
2138		Kubbitha		Guaimundhun	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41) 67-87
2157		Kubbi		Guaigulir	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41) 67-87
2199			Kilpara		Bakindji	Tindale, N. 1939. Menindee Genealogies. MS. Tindale Archives, South Australian Museum
2204			Makwara		Bakindji	Tindale, N. 1939. Menindee Genealogies. MS. Tindale Archives, South Australian Museum
2230	Kangaroo Rat	Kumbo		Bindiguin (thick blood)[74]	Murawurri	Mathews, R.H. ND. Notebook 4 'Darling River & Cobar Notes'. National Library of Australia (MS 8006. Series 3. Folder 6).pdf
2310		Kubbi		Guaigulir	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41) 67-87
2332	Nguri (emu)	Kambu		Gwaigulir		Radcliffe-Brown, A.R. 1915-1916. Notebook E2. University of Sydney Archives (P129.1-7)
2484		Matha		Guaimundhun	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41).pdf
2853	Mareira (padimelon)	Mari			Kamilaroi	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
2960	Goro (Bandicoot)	Kabuda			Weilwan	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
2993	Kuburu dog (ngura)					Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
2996	Bilby (tangu)				Banbari	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
2997	Black duck	Mada			Wongaibon	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
2998	Emu	Kambu			Morwari	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
2999	Emu	Ipai			Wailwan	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3000	Black duck	Kabuda				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3001	Black duck	Mari				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3011	Black duck	Kabuda				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3013		Kabi				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3014	Emu	Ibada			Morowari	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3017	Emu	Buda				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3018		Mari				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3019	Emu (ngurinj)	Buda				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3038	Pandari (big monster kangaroo - walleroo)			Gwaimandthan		Radcliffe-Brown, A.R. 1915-1916. Notebook E2. University of Sydney Archives (P129.1-7).pdf
3076	Bilby (tangu)	Ibada				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3078		Buda			Banbari	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3183	Mareira (padimelon)	Kabuda			Kamilaroi	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3190	Duli (goanna)	Mari			Kamilaroi	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3201	Guru (Bandicoot)	Mada			Jawalrai	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3208	Snake	Ipai			Kamilaroi	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3220	Duli	Kabidja				Radcliffe-Brown, A.R. 1930. Notebook E1. University of Sydney Archives (P129.1-6)

[74] *Munggula (butt of tree)*.

3221	Black snake (turu)	Kambu			Kamilaroi	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3224	Duli	Kabi				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3229	Possum (kurakai)					Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3270	Emu	Kambo			Weilwan	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3274	Turu (snake)	Buda			Weilwan	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3277	Padimelon	Ipai				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3278	Bilby	Kabi			Ngemba	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3279	Padimelon	Buda			Ngemba	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3302		Mari				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3306		Ipai		Gwaimandthan		Radcliffe-Brown, A.R. 1915-1916. Notebook E2. University of Sydney Archives (P129.1-7).pdf
3311	Wiru (padimelan)	Ipai	Kilpungara	Gwaimandthan		Radcliffe-Brown, A.R. 1915-1916. Notebook E2. University of Sydney Archives (P129.1-7)
3350	Duli	Mari				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3356		Kambu		Gwaimandthan		Radcliffe-Brown, A.R. 1915-1916. Notebook E2. University of Sydney Archives (P129.1-7).pdf
3461	Kara (eaglehawk)	Kabi		Bindi goair		Radcliffe-Brown, A.R. 1915-1916. Notebook E2. University of Sydney Archives (P129.1-7)
3462	Kuli (kangaroo)	Budtha		Gilya goair		Radcliffe-Brown, A.R. 1915-1916. Notebook E2. University of Sydney Archives (P129.1-7).pdf
3495	Pataru (squirrel)	Kapi	Makungara		Wiradjeri	Berndt, R.M. & Berndt, C. 1943. A Short History of Acculturation at Menindee Government Station, Darling River. MS. University of Sydney Archives P130-3-29
4496		Kumbo		Guaimundhun	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41).pdf
4636	Kalti (emu)	Ipata	Ngilpungara		Pakandji	Berndt, R.M. & Berndt, C. 1943. A Short History of Acculturation at Menindee Government Station, Darling River. MS. University of Sydney Archives P130-3-29
8215		Ibidah				SMH (23/2/1929)
76568	Mallee hen	Kumbo		Guaigullinba	Wongaibon	Mathews, R.H. ND. Notebook 4 'Darling River & Cobar Notes'. National Library of Australia (MS 8006. Series 3. Folder 6).pdf
10026	Gunundhurra (brown hawk)	Kubbi				Mathews, R.H. ND. Notebook 7a 'Criterion Hotel'. National Library of Australia (MS 8006. Series 3. Folder 7).pdf
11773	Jungkai (mallee-hen)	Ippai	Ngilpungara			Berndt, R.M. & Berndt, C. 1943. A Short History of Acculturation at Menindee Government Station, Darling River. MS. University of Sydney Archives P130-3-29
13413		Matha		Guaimundhun	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41) 67-87
13416	Yunggai (Mallee-hen)	Kumbo	Muggulu		Wongaibon	Mathews, R.H. ND. Notebook 4 'Darling River & Cobar Notes'. National Library of Australia (MS 8006. Series 3. Folder 6).pdf
14588	Jaba (carpet snake)	Buda				Tindale, N.B. 1938. Boggabilla Genealogies. MS. Tindale Archives, South Australian Museum.pdf
14590		Kabuda				Tindale, N.B. 1938. Boggabilla Genealogies. MS. Tindale Archives, South Australian Museum.pdf
14641	Dinewan				Kamilaroi	Tindale, N.B. 1938. Boggabilla Genealogies. MS. Tindale Archives, South Australian Museum.pdf
14652	Duli	Mada			Kamilaroi	Tindale, N.B. 1938. Boggabilla Genealogies. MS. Tindale Archives, South Australian Museum.pdf
14748						1892. Police Salary Register - Trackers. State Records Authority of NSW (11-16336 Reel 1971).pdf
14867	Dinewan (emu)	Ibai			Kamilaroi	Tindale, N.B. 1938. Boggabilla Genealogies. MS. Tindale Archives, South Australian Museum.pdf
14868	Duli	Kabuda			Kamilaroi	Tindale, N.B. 1938. Boggabilla Genealogies. MS. Tindale Archives, South Australian Museum.pdf
14917	Jaba (carpet snake)	Ipai				Tindale, N.B. 1938. Boggabilla Genealogies. MS. Tindale Archives, South Australian Museum.pdf
14975	Emu	Ippai				Mathews, R.H. Notebook. National Library of Australia (NLA MS 8006-3-3 - Book 2).pdf
14976		Kabuda				Tindale, N.B. 1938. Boggabilla Genealogies. MS. Tindale Archives, South Australian Museum.pdf
15428	Crow	Ippai			Dhanggatti	Mathews, R.H. ND. Notebook [Unnumbered] 'Initiation Ceremonies of the New England Tribes'. National Library of Australia (MS 8006. Series 3. Folder 12).pdf
15535		Mada				Tindale, N.B. 1938. Boggabilla Genealogies. MS. Tindale Archives, South Australian Museum.pdf
15707	Kuburu (Carpet Snake)					Tindale, N.B. 1938. Woorabinda Genealogies. MS. Tindale Archives, South Australian Museum.pdf
16767	Kuru (bandicoot)		Muanbuan	Gwaimundun		Radcliffe-Brown, A.R. 1915-1916. Notebook E2. University of Sydney Archives (P129.1-7).pdf
16928	Emu	Ippatha			Wongaibon	Mathews, R.H. ND. Notebook 3 'Tharawal & Kamilaroi languages'. National Library of Australia (MS 8006. Series 3. Folder 6).pdf

16954		Kabi			Weilwan	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
17275	Uruklila (Eaglehawk)					Tindale, N.B. 1938. Woorabinda Genealogies. MS. Tindale Archives, South Australian Museum.pdf
17282	Bilba	Kabi			Kamilaroi/Weilwan	Radcliffe-Brown, A.R. 1929. Notebook E9. University of Sydney Archives (P129.1-14).pdf
17353					Guinbrai	Mathews, R.H. ND. Notebook 3 'Tharawal & Kamilaroi languages'. National Library of Australia (MS 8006. Series 3. Folder 6).pdf
17547	Kogai (possum)	Mari		Gilya goair		Radcliffe-Brown, A.R. 1915-1916. Notebook E2. University of Sydney Archives (P129.1-7).pdf
18319	Dinewan (emu)	Ibai			Kamilaroi	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
18582		Ipatha		Guaigulir	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41) 67-87
19566	Dinewan	Buda				Tindale, N.B. 1938. Boggabilla Genealogies. MS. Tindale Archives, South Australian Museum.pdf
19661	Dinewan	Kabidha				Radcliffe-Brown, A.R. 1930. Field and research notes and genealogies. MS. University of Sydney Archives (P130-03-45).pdf
19803	Ground Iguana	Kubbi				Mathews, R.H. Notebook. National Library of Australia (NLA MS 8006-3-3 - Book 2).pdf
19804	Opossum	Kubbitha				Mathews, R.H. ND. Notebook 5 'The Bora held at Gundabloui'. National Library of Australia (MS 8006. Series 3. Folder 11).pdf
20490	Duli (sand goanna)	Mari			Kamilaroi	Tindale, N.B. 1938. Boggabilla Genealogies. MS. Tindale Archives, South Australian Museum.pdf
20516	Duli	Kabi				Tindale, N.B. 1938. Boggabilla Genealogies. MS. Tindale Archives, South Australian Museum.pdf
20528	Ground goanna (duli)	Kambo			Kamilaroi	Elkin, A.P. 1944. Notebook 5. University of Sydney Archives (P130.3-5)
20539	Kuburu (Carpet Snake)				Kambri	Tindale, N.B. 1938. Woorabinda Genealogies. MS. Tindale Archives, South Australian Museum.pdf
21024	Mareira (padimelon)	Mada			Kamilaroi	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
21025	Mareira (padimelon)	Mada			Kamilaroi	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3209	Goanna	Mari			Kamilaroi	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
3210	Snake	Buda			Kamilaroi	Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
21079		Ibada				Tindale, N.B. 1938. Brewarrina Genealogies. MS. Tindale Archives, South Australian Museum.pdf
733	Bandicoot	Mara			Wangaypuwan	Elkin, A.P. 1944. Notebook 5. University of Sydney Archives (P130.3-5)
24075	Turu	Ipadja				Radcliffe-Brown, A.R. 1930. Notebook E1. University of Sydney Archives (P129.1-6)
25172		Mari				Radcliffe-Brown, A.R. 1930. Field and research notes and genealogies. MS. University of Sydney Archives (P130-03-45).pdf
25173	Dugin (whip snake)				Yualarai	Radcliffe-Brown, A.R. 1930. Field and research notes and genealogies. MS. University of Sydney Archives (P130-03-45).pdf
25184		Mari				Radcliffe-Brown, A.R. 1930. Field and research notes and genealogies. MS. University of Sydney Archives (P130-03-45).pdf
26100	Dinoin Gnoura Gnora (Emu)	Ippai				Mathews, R.H. ND. Notebook 1 'Kamilaroi Bora'. National Library of Australia (MS 8006. Series 3. Folder 12.).pdf
26409		Mari		Gwaimandthan		Radcliffe-Brown, A.R. 1915-1916. Notebook E2. University of Sydney Archives (P129.1-7).pdf
30641	Eaglehawk					Mathews, R.H. ND. Notebook 4 'Aboriginal Initiation Ceremonies'. National Library of Australia (MS 8006. Series 3. Folder 12).pdf
54903	Kalungkawaran a (duck)	Mata	Makungara			Berndt, R.M. & Berndt, C. 1943. A Short History of Acculturation at Menindee Government Station, Darling River. MS. University of Sydney Archives P130-3-29
55013	Flying Squirrel	Kubbi				Mathews, R.H. ND. Notebook 1 'Initiation Ceremonies of the Gundagai Tribe'. National Library of Australia (MS 8006. Series 3. Folder 11).pdf
58909	Mundbawari (Short Nose Bandicoot)					Mathews, R.H. ND. Notebook 7a 'Criterion Hotel'. National Library of Australia (MS 8006. Series 3. Folder 7).pdf
58910	Kookaburra				Gundunggurra	Mathews, R.H. ND. Notebook 7a 'Criterion Hotel'. National Library of Australia (MS 8006. Series 3. Folder 7).pdf
59316	Mullian (Eaglehawk)					Mathews, R.H. ND. Notebook 7a 'Criterion Hotel'. National Library of Australia (MS 8006. Series 3. Folder 7).pdf
60332	Dinewan (emu)	Madha				Radcliffe-Brown, A.R. 1930. Field and research notes and genealogies. MS. University of Sydney Archives (P130-03-45).pdf
61202	Eaglehawk	Ippai				Mathews, R.H. ND. Notebook 4 'Aboriginal Initiation Ceremonies'. National Library of Australia (MS 8006. Series 3. Folder 12).pdf
62837	Kuru (bandicoot)	Kabi				Radcliffe-Brown, A.R. 1930. Notebook E1. University of Sydney Archives (P129.1-6).pdf

67489	Dinewan	Mari				Radcliffe-Brown, A.R. 1930. Field and research notes and genealogies. MS. University of Sydney Archives (P130-03-45).pdf
72302	Emu	Kumbo				Mathews, R.H. Notebook. National Library of Australia (NLA MS 8006-3-3 - Book 2).pdf
72456		Kubbi		Guaigulir	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41) 67-87
72457		Ipatha		Guaimundhun	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41) 67-87
72850	Guru (Bandicoot)	Kubbi	Muggula		Wongaibon	Mathews, R.H. ND. Notebook 4 'Darling River & Cobar Notes'. National Library of Australia (MS 8006. Series 3. Folder 6).pdf
72851				[75]		Mathews, R.H. ND. Notebook 4 'Darling River & Cobar Notes'. National Library of Australia (MS 8006. Series 3. Folder 6).pdf
72999		Murri		Guaimundhun	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41) 67-87
73710	Eaglehawk					Mathews, R.H. ND. Notebook 1 'Kamilaroi Bora'. National Library of Australia (MS 8006. Series 3. Folder 12.).pdf
73838	Codfish	Oombi			Eetha	Mathews, R.H. ND. Notebook 1 'Initiation Ceremonies of the Gundagai Tribe'. National Library of Australia (MS 8006. Series 3. Folder 11).pdf
74339		Kumbo		Guaigulir	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41) 67-87
74340		Kubbitha		Guaigulir	Ngeumba	Mathews, R.H. 1907. Notes on some Aboriginal Tribes. Journal of the Proceedings of the Royal Society of NSW (41) 67-87
74483	Padamelon	Murri				Mathews, R.H. ND. Notebook 5 'The Bora held at Gundabloui'. National Library of Australia (MS 8006. Series 3. Folder 11).pdf
74484	Ground Iguana	Matha				Mathews, R.H. ND. Notebook 5 'The Bora held at Gundabloui'. National Library of Australia (MS 8006. Series 3. Folder 11).pdf

[75] Shade: *Goaigallimba*