Author/s:
Sadrforati, Mohammad Mahdi

Title:
Conceptual Change: Rationality, Progress and Communication

Date:
2019

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Mohammad Mahdi Sadrforati

ORCID identifier: 0000-0003-4145-4554

PhD thesis

December 2019

Philosophy Department, SHAPS, University of Melbourne

This thesis is being submitted in total fulfilment of the degree. The degree is not being completed under a jointly awarded degree.

Declaration

This thesis comprises only my original work towards the PhD. Due acknowledgement has been made in the text to all other material used. The thesis is fewer than the maximum word limit in length, exclusive of tables, maps, bibliographies and appendices.

Mohammad Mahdi Sadrforati

Abstract

Conceptual change in science first became a hot topic five decades ago, when questions were raised about rationality and progress through scientific change. Among the first philosophers who raised these concerns, particularly in terms of the incommensurability thesis, were Thomas Kuhn and Paul Feyerabend. The main target of the incommensurability thesis was to reject the continuity of scientific change, which would ultimately question the progress of science, and rationality of scientific choice among rival scientific theories. Although philosophers of science have widely discussed this issue since the 1970s, a common understanding of conceptual change has been absent. What most scholars discussed were about specific cases of conceptual change, such as the change from 'phlogiston' to 'oxygen' or from the Newtonian use of the term 'mass' to the way Einstein employed it. With the heated discussion focused on these and similar cases, the notion of conceptual change *per se* has largely been overlooked.

The first and most well-known approach to explaining conceptual change, embraced by scientific realists, was the referential approach. This approach explains rationality and progress of science in terms of the stability of reference. In order to explain referential continuity, theories of reference borrowed from the philosophy of language. If reference stays stable across scientific change, then we can explain many complicated cases studies where the progress and rationality of science were questioned. However, in the first years of the twenty-first century, a younger generation of philosophers, mainly philosophers of biology, argued against the referential approach in favour of a non-representational approach. In fact, fixing a unique reference for some key biological terms is notoriously difficult, if possible at all. Therefore, the reference of some biological terms should be determined depending on the context of use and on user's coordination intentions.

In this thesis, my main aim is to evaluate the strengths and weaknesses of the new non-representational approach to conceptual change. I argue that, while raising valid objections to the referential approach, the non-representational approach fails to explain communication and to fully vindicate rationality and progress in science. On demonstrating philosophical shortcomings of this new approach, I articulate my own framework for evaluating the adequacy of an account of conceptual change. Although proposing a new account of conceptual change is beyond the scope of this thesis, I briefly outline a possible way to meet the *desiderata* on an adequate account of conceptual change, opening a new space for further research to satisfy the proposed framework.

Acknowledgments:

First and foremost, I would like to thank both of my supervisors. Howard Sankey and Laura Schroeter have been incredibly helpful, supportive and patient throughout the process of writing this thesis, looking at many half-baked and poorly expressed ideas and helping winnow out those worth pursuing. I have profited from many discussions with them and from their suggestions for ways to develop ideas. This thesis would look nothing like it currently does without their help.

I would also like to thank my wife who was more than helpful in this long journey studying in Australia. I left family and friends to pursue my professional goals in this land. I should thank my parents, my mother in law and all my friends who helped me to make up my mind and supported me before and during my study. Also, I should thank my daughter, Sama, who was born in the middle of my study and created a fresh motivation for me to finish this thesis.

Mohammad Mahdi Sadrforati
University of Melbourne
November 2019

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Introduction

On the very first day of life, when a human baby first opens her eyes or touches her mother, she is building *concepts*, the basic representational units of thought. She acquires beliefs, even very basic ones, about the features of her environment. Her knowledge of the material world changes from birth to death by a process of adding or removing beliefs associated with specific concepts as she revises her understanding of stable topics. Philosophical questions arise about how to explain this process of knowledge acquisition through *time*. For example, on the second day, when the baby notices that her mother's milk is warm, her concept of *milk* (or perhaps *food*) becomes richer, as new information or expectations are added to her initial stock of assumptions. One philosophical question is whether this revised concept of *milk* is the same as the previous one or not. What is the relation between these two steps of learning? Is the baby epistemically warranted in revising her concept from the first day to the second on the basis of her experiences? What, exactly, does the change of baby's concept consist in? Does the second concept continue to refer to the same object as the first? Is it more accurate than the first? More importantly, are there two concepts in this example, or is there one concept that is evolving?

All of these questions are raised in the literature around 'conceptual change', an interdisciplinary topic widely discussed in philosophy, psychology, and education. What links all these subjects together is the idea of a *concept* as a basic representational unit of thought. Although there is no consensus among theorists about how to define 'concept', it is agreed that concepts enable us to recognise and make inferences about the objects they represent. For instance, my concept *milk* determines which objects I am disposed to count as belonging to the category of milk and think of it as having white colour or fatty taste. When it comes to studying scientific concepts, complicated issues arise concerning the concepts expressed by theoretical terms such as 'gene' or 'electron'. By theoretical terms, I mean, roughly speaking, terms that refer to non-observable entities that figure in scientific explanations. The way we understand these terms at a given time is largely determined by the theories in which they figure. During the process of scientific theory change, the way we understand these terms changes constantly. Sometimes, these changes are so radical that philosophers conclude that the concepts expressed by theoretical terms after a revolution should not (even cannot) be the same as the concepts they originally expressed. It is argued, for example, that the reference of 'atom' in contemporary physics is *incommensurable* to the one used 100 years ago.

Paul Feyerabend and Thomas Kuhn were among the first philosophers of science to formulate the 'incommensurability thesis'. Their starting point was the then-standard descriptivist theory of reference, according to which theoretical terms in science are implicitly defined by the scientific theories in which they are employed. On this approach, we can only evaluate a scientific claim, such as 'atoms are indivisible', as true or false relative to the referential standards determined by a given scientific theory. Since the history of science involves many cases where rival scientific theories use the same term with different referents, they essentially conclude that scientific theories employing those terms might be incommensurable due to the lack of a fixed comparison standard. For instance, consider the historical shift from Newtonian physics to Einstein's theory of relativity. While the former conceived 'mass' as referring to a stable feature of all matter, the latter used the same term in a relativistic way. According to the incommensurability thesis, the term 'mass' before and after this theoretical revolution expresses completely different concepts (or meanings) and refers to radically different things. Before the paradigm shift, claims like 'the mass of this bookshelf is constant independent of its relation to other objects' was true, whereas afterwards it was false. It follows that successive scientific theories in physics were talking about completely different things with the term 'mass', which is a central term in science. Therefore, there is no way to translate the claims about 'mass' made by the earlier theory into claims using that term in the later ones. Nor is there any way of rationally comparing the truth of the two theories, since truth-conditions are determined relative to different theories. Without stability in meanings and concepts over time, the two theories are referentially and rationally incommensurable.

Although the problem of incommensurability is not the only philosophical question concerning conceptual change, it has attracted the most attention among philosophers of science.[1] The problem of incommensurability and related problems about conceptual change can be seen as a threat to scientific realism. If each scientific theory implicitly defines its own theoretical terms and sets its own standards for determining the reference of those terms, then there is no way to say that scientific paradigm shifts can get us closer to the truth about the same unobservable features we were originally theorising about. Theoretical terms have no objective reference that remains stable over the course of scientific inquiry. A radical form of this objection is called 'philosophical relativism', a term coined by Paul Feyerabend. According to this doctrine, there is no higher norm or standard that can allow us to compare the truth of different theories: "any distribution of truth values over traditions is acceptable" (Feyerabend 1978, p. 83). In other words, we are not in a position to judge one theory as closer to the truth than another, since truth and falsity can only be assessed from within the perspective of a particular theory. However, this conflicts with the commonsense realist assumption that scientific inquiry can lead us closer to the truth about the very entities we were theorising about all along.

The incommensurability thesis also raises specific challenges to some scientific realists' further commitments, such as rationality of theory choice and scientific progress. In principle, a minimalist scientific realist could be committed only to the idea that theoretical terms in the mature science pick out features of the external world whose nature is determined independently of human thought. This scientific realist would combine a claim about ontology and a claim about reference. Strictly speaking, there is no logical incompatibility in combining this minimalist scientific realism with the claim that different scientific theories are rationally incommensurable. A minimalist scientific realist, therefore, will reject any direct link between scientific realism per se and an account of conceptual change.[2] That said, the more scientific realism is elaborated with compatible commitments about epistemology and communication in science, the more potential there is for conflict between the incommensurability thesis and this full-blooded scientific realism. A full-blooded scientific realist, I will suggest, should seek to explain not only how our theoretical terms manage to pick out objective features of the world, but also how scientists manage to communicate across different theoretical paradigms and how they are rationally guided by their current conceptual understanding toward a more accurate account of nature. A full-blooded scientific realism should explain why our successive theories are not rationally incommensurable.

One of the first theorists to take up this challenge was Israel Scheffler (1967), who proposed a referential solution to the incommensurability problem. He argued that reference remains stable through major shifts in theoretical paradigms.[3] If reference can remain stable through

[1] About three decades ago it reached to its peak when Burian acknowledged that "conceptual change in science has been a 'hot' topic" (1987, p. 3).
[2] Sankey 2008, p .2.
[3] "[I]t is the sameness of reference that is of interest" (Scheffler 1982, p. 57).

radical changes in theory, then philosophers of science can explain how radical paradigm shifts can take us closer to the truth about the very same features of the world that the earlier theories were examining. Accordingly, successive theories are not incommensurable after all: the earlier theory and the later theory share the same reference, and the truth of their claims can be directly compared. This referential approach was later backed by proponents of the causal theories of reference, which sought to explain how, exactly, the reference of scientific terms is fixed, as well as how it can remain stable despite changes in theory. Different causal theories of reference have been imported from philosophy of language to fulfil this role in the philosophy of science. Some of the most sophisticated theories of reference that some scientific realists have appealed to are hybrid theories, according to which both causal and descriptive factors play a role in fixing the reference of scientific terms so that they remain stable across scientific revolutions. For instance, Howard Sankey takes a referential approach to conceptual change backed by a sophisticated hybrid theory of reference. Since reference is determined by a mixture of causal and descriptive components on his account, he admits that the 'scope of referential variance is much reduced' in virtue of the causal factors (2000, p. 134). What theories taking a referential approach to conceptual change share, from the simple to the sophisticated, is the idea that the problems associated with conceptual change should be explained by referential continuity.

The referential approach to conceptual change was widely accepted until the beginning of the 21st century, when the notion of conceptual change in science once again became an active area of research. While earlier theorists working on conceptual change tended to focus on the physical or chemical sciences, a younger generation of philosophers focused on the biological sciences. From a historical point of view, the 20th century philosophers of science largely failed to recognise the ways in which biology was critically different from physics and chemistry. Of course, biological case studies were discussed by famous philosophers such as Philip Kitcher, David Hull, Paul Thagard, and Richard Burian in the 80s and Elliot Sober in the 90s, among others. But these discussions were not at the centre of discussions concerning conceptual change. In the early 2000s, younger philosophers of science began to focus on biological case studies in examining how theoretical terms ground reference, communication, and progress in science. Paul Griffiths, Sandra Mitchell, and Gunter P. Wagner — followed by Kenneth Waters, Miles McLoed, Marcel Weber, Alan Love, and Ingo Brigandt — are some of the philosophers of science working on these problems.

One important challenge these theorists raised for the referential approach to explaining scientific continuity through conceptual change is that some key theoretical terms in biology do not seem to have stable referents. Unlike most of the scientific terms in chemistry and physics, biological terms often refer to radically different features when used in different conversational contexts. Indeed, it has been argued that terms such as 'gene' and 'homology' have *floating reference* — meaning that the reference-fixing conditions of a term can vary depending on different contexts of use. It has also been argued that some contemporary biological terms can pick out different kinds as their reference, depending on the scientific context. For example, the term ‘homology’ is used in both phylogenetic and developmental biology, and it refers to different features in each subdiscipline.

These observations about the instability of reference for theoretical terms in biology led Ingo Brigandt, among others, to propose a non-representational approach to explaining the continuity of scientific inquiry through conceptual change, in contrast to the widely accepted referential approach.[1] If the reference of biological terms does not remain stable through different contexts of use and theoretical contexts, then we cannot explain the rational commensurability of biological theories by appealing to the stability of reference. A non-representational approach holds that an explanation of conceptual change should focus on users’ epistemic goals in using a term, as well as theorists’ interests in interpreting them, not just a term’s reference or the theoretical commitments it is associated with. This new approach seeks to explain rational theory change and successful communication across *all* scientific domains — including biology — whose theoretical terms may involve floating reference.

The non-representational approach to conceptual change could be considered an alternative response to the problem of incommensurability. But it also has the broader philosophical potential to explain the evolution of scientific language and even to explain scientific justification in a more flexible way. While the referential approach has been favoured by scientific realists who seek to show how we manage to make steady progress towards the truth about a stable reference, the non-representational approach seeks to explain the rational commensurability of different scientific theories in terms of stable epistemic goals and/or stable theoretical commitments. Although a non-representational approach does not necessarily reject the importance of reference, reference plays a relatively minor role (or no role whatsoever) in explaining the continuity of scientific communication and inquiry with a specific theoretical term. Brigandt’s non-representational conceptual analysis, in particular, accords the least importance to reference and the most importance to the stability of scientists’ epistemic goals in using a particular theoretical term. In general, what stays stable through rational conceptual change are the epistemic goals associated with a term, rather than its reference. So, philosophers of science seeking to understand the continuity of science should change their focus from *referential stability* to *epistemic stability*.

In this thesis, my main focus is on the viability of the non-representational approach to conceptual change over the referential approach. After the first chapter, in which I introduce the notion of ‘conceptual change’ and set my main terminological conventions for the thesis,

[1] It would be nice if I could contrast a referential with a non-referential approach, but in fact Brigandt does not explicitly reject reference and so it’s unfair to call his account ‘non-referential’. In Chapters 3 and 4, I will show that he criticizes the referential (and representational) approach for focusing only on reference. Instead a non-representational approach like Brigandt’s changes the focus of an account of conceptual change from stability of reference to stability in patterns of use. Brigandt’s approach is non-representational in that the most important component of concepts that determines their stability/change and also determines reference is an epistemic factor.

I explore the rationale for moving away from the referential approach. The second chapter explains the importance of biological case studies. I will show why these make trouble for the classical referential explanation of the rationality and progress of science through conceptual change. In fact, theoretical terms in biology are crucially different from those in physics and chemistry, and these differences have major consequences for the characterization of conceptual change. Indeed, explaining conceptual change in biology is not just the main rationale for rejecting a referential account of conceptual change, but non-representationalist accounts are designed specifically to help determine the reference of biological terms. In this way, the discussion in Chapter 2 helps to motivate the subsequent analysis of a particular non-representational account of conceptual change.

In Chapters 3 and 4, I focus specifically on Ingo Brigandt's non-representational account of conceptual change, because it is one of the most highly developed accounts of continuity through conceptual change; it also stands in clear opposition to the referential approach. These two chapters are dedicated to first explaining and then criticizing Brigandt's theory of concepts. In Chapter 3, I explain how epistemic goals play a central role in Brigandt's non-representational account of what remains stable through changes in scientists' understanding of theoretical terms and how this stability of epistemic goals helps to explain the rationality of theory change. In Chapter 4, I highlight how Brigandt's appeal to epistemic goals contrasts with scientific realists' appeal to stability of reference in explaining how we compare successive scientific theories. I propose four normative theses that full-blooded scientific realists need their account of conceptual change to satisfy. The first two are very basic realist theses: (1) an objective world independent to our minds exists, and (2) the central theoretical terms of our mature scientific theories have their reference in that external world. The second two theses are more specific to conceptual change: (3) our understanding of those theoretical terms is getting closer to the truth in the process of conceptual change, and (4) scientists are epistemically warranted in making such changes. I then argue that a non-representational approach in general — and Brigandt's account in particular — is not consistent with this package of full-blooded scientific realist commitments. Brigandt, I will show, is a minimal scientific realist: he does not reject the objectivity of the external world but denies the other theses.

I believe that Chapters 5 and 6 are my major contributions to the discussion of conceptual change in the philosophy of science. In Chapter 5, I argue that Brigandt's account of concepts cannot explain rational communication across different contexts. I go on to argue that the problematic element of Brigandt's account is its central idea: his appeal to continuity in the epistemic goals associated with theoretical terms in a given context. In short, my objection is that there is no objective fact of the matter what a scientist's epistemic goals are, independent of the contingent interests of an interpreter. The objection I raise also applies to several other non-representational accounts of conceptual change, according to which the conceptual continuity is only defined relative to the explanatory interests of an interpreter. In other words, a non-representational approach explains the rational continuity of conceptual change based upon an external theorists' contingent explanatory interests. I take this to be a major objection to the non-representational approach to conceptual change, which makes it not only incompatible with a scientific realist account of conceptual change but encounters major philosophical issues. As a consequence, I believe we should explore a return to the referential approach.

Chapter 6 is my final proposal about how to adjudicate the conflict between the referential and non-representational approaches to conceptual change. In general, I am sympathetic to a standard scientific realist position, but I do not believe that the existing realist accounts of conceptual change — with their explicit or implicit reliance on referential stability — can fully explain what is going on in the biological case studies. In this chapter, I draw on the lessons of the previous chapters to derive constraints on a full-blooded realist theory of conceptual change. These constraints aim to keep the commitments made by a full-blooded scientific realist while simultaneously accommodating the complexities brought to light by the case studies involving theoretical terms in biology. Satisfying these criteria, I argue, would allow a theory of concepts to vindicate (full-blooded) scientific realism and explain how we can rationally evaluate biological theories whose theoretical terms do not have stable reference.

The goal of this thesis is not the proposal of a fully elaborated account of conceptual change. My primary aim is to articulate the constraints on a fully adequate theory of conceptual change. One may worry that these constraints cannot be satisfied. To allay such worries, I conclude by briefly sketching one promising strategy for satisfying the constraints. Drawing on recent work in the philosophy of language, I sketch a *relational* account of concept individuation and explain how it has the potential to satisfy both the constraints on stable concepts and on objective reference. Although this proposal is merely a sketch, I will argue that it has many advantages over a non-representational account: being in accord with scientific realism and more importantly explaining successful communication among experts and non-experts. Further research will be needed to fully flesh it out and to determine whether some versions of the relational theory of concepts can ultimately vindicate a scientific realist account of conceptual change.

Chapter 1

What is This Thing Called Conceptual Change?

1- Introduction

The notion of conceptual change is used by scholars across a variety of disciplines, notably the philosophy of science, education and cognitive developmental studies.[1] The great range of variation in the approaches taken as regards this notion reflects its importance. The idea of conceptual change also has great importance in accounts of human learning, and in theorising the growth of knowledge that takes place across different stages of human life. Science in particular is a domain of knowledge for which a robust account of conceptual change promises a sound understanding of its growth and characteristic patterns of reasoning. Thus have philosophers and historians of science been attracted to this notion, albeit in many different respects. Debates concerning conceptual change in science were

[1] For a psychological study of conceptual change see (Carey 1985), and for an educational study see (Vosniadou ed. 2013).

mainly initiated around sixty years ago, in the second half of the twentieth century by the works of Thomas Kuhn, Paul Feyerabend and Norwood Hanson.

Kuhn is among the prominent philosophers of science who have worked on conceptual change and surveyed its consequences in science. The first edition of his famous *The Structure of Scientific Revolutions* (1962) attracted attention not only from philosophers of science but also from scholars in other relevant areas. Although he did not explicitly use the term 'conceptual change' in this book, he addresses a large number of ideas and claims pertaining to 'conceptual transposition', 'conceptual transformation', 'refinement of concepts', 'conceptual adjustment', 'reconceptualization' etc.[1] More importantly, Kuhn made a provocative claim about the comparison of rival *paradigms* during scientific revolutions.[2] Scientists working in a specific paradigm employ specific standards, problems, conceptual systems, and generally perceive the world according to the framing of that paradigm. In rejecting a standard measure in comparing rival paradigms, Kuhn was led to criticize the *cumulative* account of scientific advance. Further, he argued against the feasibility of a neutral methodological framework or a neutral language across scientific revolutions, by which scientists can come to a complete understanding of each other's concepts. His use of the notion of the *incommensurability* of scientific paradigms, as well as Feyerabend's (1962), focused attention on the broader notion of conceptual change.

Incommensurable scientific paradigms, Kuhn believed, may use the same words but do so with different intensions and extensions. So, scientists passing from one paradigm to the next experience conceptual change, according to which the meaning and reference of key concepts may be altered. Borrowing from psychology, Kuhn sometimes described this concept alteration as a 'gestalt switch'. The reason is that after major scientific changes, scientists come up with new problems and standards in framing their understanding of nature. This change can leave them using the same words but associated with modified concepts. For example, a term like 'planet' may be employed with different meanings and references in the Ptolemaic and Copernican paradigms.[3] The most severe cases of conceptual change take place when the references of key concepts change, breaking the referential continuity between those concepts. These severe cases occur usually when there is a change in the theories that employ those concepts. Kuhn, and in a stronger sense Feyerabend, made a strong connection between scientific terms and the theories that employ them, and this

[1] Kuhn also started using the term (conceptual change) in his other works. He used it in a symposium entitled "Criticism and the Growth of Knowledge" at the fourth international Colloquium in the philosophy of Science held in London in July 1965. See. Lakatos and Musgrave (1970, p. 249).

[2] There exists an ambiguity in Kuhn's usage of 'paradigm'. Shapere (1984, p. 39) discusses different readings of Kuhn's paradigm from a 'recurrent and quasi-standard illustrations' to 'strong network of commitment' to 'patterns' or 'exemplars'.

[3] In this particular case, while 'planet' in the Ptolemaic system includes a reference to the sun, Copernicans take the reference of this term as including the earth but excluding the sun (Kuhn 2000, p. 94).

connection produces some radical cases of conceptual change when theories change in a revolutionary manner.[1]

In the two decades immediately following those original publications, conceptual change became a ‘hot’ topic in philosophy of science.[2] Debates concerning conceptual change in that period were largely concerned with the particular problem of incommensurability and reference (continuity), due to the deep influence of Kuhn and Feyerabend. Although the problem of incommensurability had lost most of its thrust by the late 90s, the notion of conceptual change has remained a hot topic since then. The main reason for this was the advent of a new alternative approach to conceptual change. From the early twenty first century, some younger philosophers of science began discussing conceptual change from a new perspective, according to which conceptual change should not be reduced to the problem of incommensurability, and, in the same vein, should not be reduced to the problem of reference[3] (Love 2005; Brigandt 2006b). I introduce this new approach later in this chapter, and provide a full and detailed account in chapter 3, but for now we must discuss the very feasibility of the idea of conceptual change and the philosophical debates which have surrounded it.

There is little doubt that the theoretical structures and categories of science have changed over time.[4] The continuous process of empirical discovery through novel observations and experiments provides scientists with an ever-changing body of evidence.[5] New evidence demands revisions (minor or major) in the conceptual apparatus that scientists use in describing natural entities and phenomena. Conceptual change occurs precisely because of this process of change in the conceptual apparatus. However, the main philosophical interest lies in the way this change has to be understood, or the way that its consequence should be explained.[6] These general issues have an intrinsic connection with more basic issues regarding the nature of concepts and the account of concept identity. Different characterizations of concepts and their identity conditions lead philosophers of science to radically divergent accounts of conceptual change. This variation is huge, such that some philosophers reject the feasibility of any change in the meaning or reference of concepts, and view radical cases of conceptual change simply as conceptual replacement (Lennox 2013a).

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[1] Feyerabend (1981) writes “the interpretation of an observation language is determined by the theories which we use to explain what we observe, and it changes as soon as those theories change” (p. 211).

[2] Burian acknowledges that “conceptual change in science has been a ‘hot’ topic” (1987, p. 3).

[3] “The heterogeneity of conceptual change suggests that there is more to the conceptual practices of scientists than reference and not every conceptual change is the result of or dependent on changes in scientific theories” (Love 2015, p. 16).

[4] “Terms in scientific theories do not have static meanings but are defined and redefined within the context of their evolving usage” (Cushing 1990, p. 35).

[5] “Often conceptual innovation is a response to an empirical discovery” (Brown 2007, p. 78)

[6] “Scientific cultures develop and change… in short, the ways in which we interact with our physical environment, and the ways in which we think about it, have changed and will continue to change. But how is such change to be understood?” (McGuire 1992, p. 132).

In this chapter, I aim to address different understandings of conceptual change. This aim sometimes requires consideration of theories of concepts or concept identity. But I try to focus on the issues from a philosophy of science perspective, with less emphasis on philosophy of language. Later in this chapter I explain some of the most significant and problematic consequences of conceptual change, namely the threat that conceptual change may pose to the rationality and progress of science. Scientific realists, as prominent targets of these threats, have sought reasonable explanations of these problems so as to propose an acceptable resolution. I will argue that any such reasonable explanation must occupy a defensible position between naïve realism and radical relativism. The present chapter in general plays an introductory role for the rest of my thesis, in which I evaluate and assess a novel approach to conceptual change. Hence, I end this chapter by introducing different approaches to conceptual change and reviewing their costs and benefits.

2- Definitions and Understandings

2.1. Some General Points

A convenient way to convey a general understanding of conceptual change is by listing different kinds or categories of changes that may take place in the course of scientific change. In a broad sense, conceptual change is a heterogeneous notion,[1] encompassing a wide range of evolutions in the language of science. To identify different kinds of conceptual change is not only beneficial for giving a general understanding of this notion, but begins to suggest just how radical the changes in the overall structure of concepts can be. The range of conceptual change starts from simple addition or deletion of a belief, and extends all the way to a conceptual revolution in which new concepts grow out of previous ones with completely different meanings and reference. However, based on different criteria and for different purposes, different philosophers have provided slightly different characterizations of conceptual change. Let's begin with one of the most cited ones.

Thagard (1992, p. 35; 1999, p. 150) provides a list of different categories of conceptual change, mainly in order to propose a 'ranking of severity' among those categories. Running from the least to the most severe cases of conceptual change, he cites nine different forms of conceptual change: (1) adding a new instance to the reference of a concept, (2) adding a weak rule in identifying the reference of a concept (e.g. that whales are seen in Arctic ocean), (3) adding a strong rule that plays a role in problem solving and explanations in identifying the reference of a concept (e.g. that whales eat sardines)[2], (4) adding a new part-relation to our

[1] Alan Love writes: "conceptual change is heterogeneous; it includes …" (2015, p. 14).

[2] Thagard asserts that the difference between the weak and strong rules is a pragmatic one (1992, p. 35). It seems to me that a strong rule is more concerned with explanations nomically or essentially grounded in the essential properties of the referent, while weak rules deal with accidental generalizations that don't depend on these essential properties. For example, he gives another example of weak and strong rules. The fact that tuberculosis is more common in prisons is a weak rule to identify the reference of this disease, while the fact that this disease is bacterial (i.e. people with tuberculosis have Mycobacterium tuberculosis) is a strong rule (Thagard 2008, p. 375).

understanding of referents or decomposing a previously known concept into different parts. This kind of change is very important, for it has taken place in physics and chemistry several times. In particular, when unobservable entities such as molecules, atoms or sub-atomic particles were discovered, scientists were required to *decompose* their already formed concepts in order to add these new referents to the domain of their concepts. The first four kinds of conceptual change in this list are directly related to the identification of referents of concepts. However, it is not the case that all kinds of conceptual change concern the reference of concepts.

A more *severe* example of conceptual change (Thagard's fifth kind) takes place by adding a new kind-relation that relates two kinds of entities to the same concept or *differentiates* them. In the process of differentiation, new concepts emerge form already formed concepts. For example, two different concepts of *velocity* and *acceleration* emerged out of the less specific idea of *motion* in kinematics (1992, p. 35). The next kinds of conceptual change are (6) the introduction of a new concept into taxonomic structure, (7) collapsing part of a kind-hierarchy and abandoning the previous borders among concepts, (8) reorganizing taxonomic hierarchies by shifting objects from one category to another, and (9) changing the principles of the hierarchical tree or tree-switching. While the first four items were directly related to the identification of referents of concepts, the rest concern a change in the position or order of concepts in the general hierarchy of concepts. According to this listing, changing the hierarchical tree of concepts represents the most radical kind of change in the meaning of concepts. This form of ranking conceptual change based on their meaning change severity is traditional, coming from literature in education science or psychology (Carey 1991; Chi 1992; Dykstra 1992).[1]

Lennox (2013a), on the other hand, argues that it is not the case that all conceptual change entails change of meaning. Contrasting his perspective with the traditional accounts, Lennox cites different taxonomical structure of the instances of conceptual change (excluding meaning change): (1) introduction of new concepts, (2) development of new sub-categories of a concept, (3) development of more general concepts designating wider categories, (4) the rejection and/or replacement of concepts, (5) reclassification of a class of entities under another category (pp. 113-114). According to him, these cases of conceptual change should be classified as changes in the *conceptual structure of science*, not as a change in the meaning or reference of concepts.[2] Needless to say, since these cases of conceptual change do not involve change of meaning or concept identity, the problems that could be raised due to these

[1] Each of these scholars ranks the severity of different instances of conceptual change by their own terminology: For example, Chi (1992) uses 'normal conceptual change' vs 'radical conceptual change'. Carey (1992), which has stronger influence on Thagard, uses 'weak restructuring' and 'strong restructuring'.

[2] I will touch upon this point later, but for now it is important to note that according to Lennox concepts have a flexibility in terms of meaning and reference. So, a well-formed concept has the ability to include new meanings and new referents which were not intended at the first formation event. So, there is a minor difference between his conception of 'referent' and others: for him concepts pick out an individual that falls into the set of objects the concept applies to, rather than the whole set or property that the concept picks out.

issues are already resolved. Lennox's aim of listing these kinds of conceptual change is to emphasize the importance of changes that do not involve meaning or reference change.[1] There are comparable studies in education science, focusing on the importance of 'conceptual exchange' rather than meaning or reference change (Hewson 1981). The important idea here is the existence of different points of view in classifying instances of conceptual change.

Apart from the variation in purposes and the slightly different classifications provided by philosophers, there is a certain consensus about the instances of conceptual change. Philosophers of science usually view the change from 'phlogiston' to 'oxygen' or from Newtonian 'mass' to Einstein's 'mass' as cases of conceptual change. More provocative examples are 'gene', 'homology' and 'cirripeds' in biology, which have recently come into philosophers' consideration. Although most of these instances are agreed to be instances of conceptual change, still there is no clear definition or commonly agreed understanding of the general notion of conceptual change among philosophers of science.[2] One reason for these disagreements is the absence of a clear border among relevant terms in this area. Basically, the term 'scientific change' carries a broader sense *including* conceptual change, while the 'incommensurability thesis' can be viewed in a narrower respect, and 'theory change' is another term altogether. These terms are closely connected, such that some philosophers use them interchangeably.[3] For example, while Thagard explicitly states that "conceptual change in the history of science is theory change" (2008, p. 385), Alan Love makes a more precise claim that "understanding conceptual change is only a means to the end of understanding theory change" (2015, p. 16).

Another, more important, reason for the absence of an agreement on 'conceptual change' is the ambiguity that lies in this very term. Part of this ambiguity stems from different understandings of the term 'concept'; and part of the ambiguity also comes from the variation of understandings about the meaning of 'change' in 'conceptual change'. Certain basic assumptions about concepts and their properties, reference determination or different realizations of concept identity, are hidden or implicit in the works of philosophers, and they may be unaware of the significance of these assumptions. For example, it is argued that Kuhn and Feyerabend, when they initially discussed the incommensurability thesis, implicitly assumed a descriptive theory of reference for scientific concepts (Sankey 2009, p. 197; 1994, Ch.5). This significant but (probably) hidden assumption led them to argue for reference change during scientific revolutions, on the basis that the descriptive content of a concept changes. For the moment, I focus only on certain important assumptions regarding concept formation in order to convey an understanding of conceptual change, while acknowledging

[1] Another listing can be found in Love (2015).

[2] Three decades ago, Burian described the situation as follows: "there is no general agreement about what it is, how it works, how it should be evaluated, or how far-reaching its consequences are. … There is no settled consensus concerning the nature or the working of conceptual change. There is no generally accepted terminology" (Burian 1987, p. 3).

[3] Although I try to use them in the right places, due to their close connections this thesis will contain instances in which these notions can be seen to be interchangeable.

the importance of other background assumptions that may contribute to forming one's understanding of the notion.

2.2. Change of Concepts vs. Concept Replacement

In the literature there exist at least two different (but not incompatible) understandings of the notion of conceptual change. One views it as a highly abstract notion referring to a variety of different changes in the *conceptual structure of science*. These changes include, but are not limited to, a simple re-classification of concepts within different contexts. For example, the simple insight that Whales are mammals and not fish induced a change in the concept of 'whale', although nothing changed in terms of the reference or probably the overall meaning of this scientific term. A change in the conceptual structure of science does not necessarily entail a change in the meaning, identity or reference of a concept, because radical referential or meaning change can be identified as concept replacement rather than concept change. So, it seems that a change in the conceptual structure of science, rather than the identity or reference of concepts, can instantiate 'conceptual change' in a more precise way. This understanding can include some of the most radical kinds of conceptual change mentioned by Thagard. The second, more disputable, understanding of conceptual change takes it as a process in which the meaning and/or reference of concepts undergoes change—meaning is roughly construed as including a Fregean sense. This is a wider understanding, including but not limited to changes both in the reference and meaning of the concepts. Indeed, this is the traditional understanding of conceptual change, which was probably assumed by philosophers like Kuhn and Feyerabend, since both their accounts of incommensurability[1] depended on the same observation of meaning variance in the course of scientific changes.

It seems, then, that meaning change is not a common property of all cases of conceptual change that philosophers of science are interested in. More importantly, it is not the case that each conceptual change entails a reference change. Introduction of new concepts into a scientific taxonomy, due to the development of science in a particular field (e.g. scientific discoveries), or rejection of empty or ill-formed concepts, are instances of change in the conceptual structure of science, but do not necessarily involve meaning change. 'Phlogiston' is a well-known concept that was gradually abandoned in the eighteenth century, and a new concept of 'Oxygen' emerged out of that. However, it is implausible to suppose that the meaning of the former changed to the latter, or that the reference of 'phlogiston' gradually changed to the actual reference of 'Oxygen'.[2] The fact that Kuhn and Feyerabend used a variety of examples including changes in conceptual structure of science (e.g. 'Oxygen' and

[1] Kuhn and Feyerabend meant slightly different things when they used 'incommensurability'. In fact, while Feyerabend used this term to describe the absence of a logical connection between rival theories, Kuhn's view evolved into a modest account according to which a literal translation of rival theories is impossible.
[2] Kitcher (1993) argued for a provocative claim according to which the term 'dephlogisticated air' in the mouth of Priestley could in some cases refer to the actual Oxygen.

‘Temperature’) and changes in the meaning of concepts (e.g. ‘Planet’ and ‘Mass’) reflects their neglect of these differences.

A quick reflection on the term ‘conceptual change’ shows us that we are dealing with a process in which some aspects or all components of a concept (C1) change to become another concept (C2). It is clear that C2 is not C1 in certain respects, but there must be a connection between them in order to call this a case of *conceptual change* at all. Yet what gives C1 its specific identity that distinguishes it from C2 is controversial among philosophers. For example, Lennox (2013a) conceives of concepts as cognitive tools of understanding that have an important *property of open-endedness*. This means that the reference of a *properly formed* concept is not confined to the objects that are intended at the first encounter, but it includes reference to entities implicated in yet-to-be-discovered facts about the entities it subsumes. These to-be-discovered facts include new ‘*characteristics’* of already known objects, and more importantly ‘*potential infinity of entities’* that can be picked out by the concept in the future. Let’s consider these two aspects of the open-endedness of concepts in a scientific example. Clerk Maxwell in the 1860’s discovered that light is a form of electromagnetic wave, propagated in an electromagnetic field. From then on, light is included in the extension of electromagnetic wave, though it was not prior to this discovery. Lennox’s point implies that the concept of ‘electromagnetic wave’ in the 1860’s already included reference to light as a yet-to-be-discovered entity, so after this discovery no conceptual change took place. The second aspect of this claim (i.e. new characteristics of objects) is less provocative. For example, in the early 20th century, Einstein discovered that the energy of electromagnetic wave is not propagated through a *continuous* field. Rather, an electromagnetic field is a quantized field of individual particles having energy. For Lennox, this new discovery was also included in the concept of ‘electromagnetic field’, although it may not add a new referent to the concept.[1]

According to this view, concepts do not lose their identity through the growth of knowledge about the referents of that concept, but there are certain flexibilities in concepts enabling them to include new referents or imply new inferential roles.[2] Cases that involve radical reference change are not instances of conceptual change but of concept replacement. Thus, a majority of the well-known examples of conceptual change simply refer to a ‘*development*

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[1] This account of open-endedness seems to add an epistemic property to a semantic account. The fact that some new referents are revealed after specific discoveries, is an epistemic fact. But the idea that a properly formed concept contains reference to not-yet-discovered entities is a semantic claim. I will discuss such epistemic accounts of semantics in chapter 5.

[2] One can see a line of similarity between this form of open-endedness and Burian’s account of ‘open reference’ (2005, p. 136). The reason is that both of them are trying to give some sort of flexibility to the range of objects that a concept can refer to. Lennox admits this similarity and thinks that this flexibility is required in explaining the ‘input of new information’ in the course of science advancement (ibid, p. 117).

in the conceptual structure of science' (ibid, p. 114), because newly discovered characteristics are among the not-yet-discovered properties of the previous reference.[1]

For Lennox, conceptual change has a broader domain than that to which traditional philosophers of science had confined it. It is "a highly abstract notion that subsumes a variety of different sorts of change" (ibid. p. 113). These changes do not necessarily alter the meaning or identity of concepts; instead, a concept is retained as long as it picks out the referents that are included in the set of yet-to-be-discovered entities. Lennox defends the idea that in the absence of *radical* referential change, change in the identity of concepts is very unlikely: "[conceptual changes in some cases] do not *require* a change in the reference of the concept—these concepts might well subsume the same referents before and after the conceptual reclassification, and in that sense retain their identity" (p. 130). In addition, some radical referential changes should be treated as concept replacement, rather than a change in the identity or meaning of concepts. In other words, if the reference of a concept changes outside the range of its open-ended set of referents, we may either replace the concept, or we encounter serious problems in using that concept.

The general strategy of finding stability through scientific change is very popular among philosophers of science. Some appeal to this strategy in terms of the notion of reference fixing of terms/concepts (Putnam 1975; Devitt 1981) and others go with a more general stability of concepts even with reference change (Griffiths 1997; Love 2005; Brigandt 2006).[2] The general idea is to make concepts more flexible in retaining their identities after minor changes. Philosophers like Lennox put forward a very strict condition of change, while others have less strict conditions. The aim of all, however, is not to deny changes in science or development of the language of science; rather it is to resist those traditional models of conceptual change that insist on radical concept change. By traditional models, I mean those accounts of conceptual change that view concepts as units that undergo variation as long as their relationship with other concepts or the theories that employ them change. It is an account that to a great extent fits with Feyerabend (1962) and (maybe mistakenly) Kuhn (1962).[3]

The idea that concepts can retain their identities through scientific changes has costs and benefits. The benefits include providing a stable concept through a variety of advancements in science, preventing the problems that I explain in the next section. However, a potential problem with this strategy is to give a very strict account of concept stability in which concepts are extremely flexible so that nothing can change them at all. What we need of a modest theory of concepts, although I am unable to develop one here, is a general account of criteria

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[1] Lennox follows Ayn Rand in this regard. Rand writes: "a concept is an 'open-end' classification which includes the yet-to-be-discovered characteristics of a given group of existents. All man's knowledge rests on that fact" (1979, p. 66)

[2] Griffiths (1997, p. 192) asserts that "concepts can retain their identity across radical changes in theory" (quoted from Lennox 2013a, p. 120).

[3] See Feyerabend (1981, p. 211).

for sameness or difference of concepts over time, which will explain *how* they are both flexible and changeable through theory change and probably even reference change.

The advantage of Lennox's account, in particular, is that he allows concepts to *integrate and reintegrate* information. Concepts are open-ended, meaning that "the formation of [a] concept includes a commitment to integrating everything that will later be learned about its referents" (2013b, p. 205). This idea is aligned with our ordinary experience of learning, especially early childhood education: we frequently add a new belief or object to the properties of a concept, and the concept has the ability to incorporate that new piece of knowledge. However, the hard question is to find a point where we can say 'That's enough; this is not my previous concept'! In other words, as far as I am aware Lennox does not have a full account of what a 'properly formed concept' is and how far it can withstand change in attribution without change in identity. I agree with him in putting forward a flexible account of concepts, but I disagree with him in the 'commitment to integrating *everything*', for concepts cannot include everything whatsoever that is learnt about their referents. There must be a point where the concepts change their identity, without being replaced. He mentions (2013b) the case study of 'atom' in order to suggest that 'change of definition' and 'reference change' can take place while the concept is still stable. This brings about a radically flexible account of concept identity that seems to preclude the possibility of any conceptual change, allowing only concept replacement in very rare cases.

A modest version of this account should allow concepts to keep their identities despite some revisions in referential or linguistic relations. For concepts are flexible units of thought, if not fully open-ended, that permit minor changes in meaning and reference without leading to identity change. So, the concept should have an inclusive-exclusive ability by which the new information would be added or deleted from the concept. This allows us to find continuity in the language of science. Meanwhile, this account should also be controlled, so as not to allow every change to be considered in the same way. The history of science is full of episodes in which scientists decided to change their terms due to a non-tolerated change of concepts ('phlogiston' and 'aether' are examples of this). Similarly, there exist situations where scientists decide to keep a term, but there is a huge difference between the current understanding of a term and its original form in terms of reference and meaning ('atom' and 'gene' are examples of this). My account in chapter 6 is an attempt to make this trade-off, although this issue still stands in need of very broad and extensive research by the philosophical community.

3- What's Wrong with Conceptual Change?

Conceptual change in science was widely recognized by proponents and opponents of the incommensurability thesis. The incommensurability thesis amounts to a discontinuous reading of the advancement of science. Kuhn and Feyerabend have slightly different characterizations of this thesis (see Sankey 1997, p. 426-427), but the literature discusses them together. According to Kuhn (1962), for instance, there are genuine revolutions in the theoretical frameworks of sciences so that a mutual incommensurability between the proponents of two rival paradigms may occur. In particular, scientists before and after a scientific revolution employ different sets of coordinated standards, problems and conceptual systems, and they generally perceive the world according to the framing of different paradigms. According to this view, a change in the theoretical framework of a scientific paradigm will lead to a change in the meaning of key concepts and to conceptual change. So, during scientific revolutions where radical theoretical changes take place in the structure of scientific theories, concepts enter a state of incommensurability, whether by means of referential or meaning change. As an illustration of the former, Kuhn writes:

> … but the physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name. (Newtonian mass is conserved; Einsteinian is convertible with energy. Only at low relative velocities may the two be measured in the same way, and even then they must not be conceived to be the same.) (1970, p. 102)

One of Laudan's characters in his dialogue-based book (1990) gives an example of meaning change between concepts in incommensurable theories:

> The meaning of a term or concept is given, at least in part, by the network of assumptions with which it is associated. Thus, the meaning of 'point' or 'line' in Euclidean geometry is different from the meaning of those same terms in Riemannian geometry (p. 124).

Kuhn, like Feyerabend who was writing at the same time, used the term 'incommensurability' to indicate a range of disparities between rival scientific paradigms. However, his understanding of the notion of incommensurability, unlike Feyerabend, is broad and not fixed. While Feyerabend restricted his account to semantic incommensurability of scientific theories (1978, pp. 66-67), Kuhn originally used the term to include methodological, observational and semantic accounts. However, Kuhn put more emphasis on methodological and semantic accounts in his later writings. Some argue that Kuhn's notion of incommensurability itself underwent a major transformation (Sankey 1993), while Kuhn himself viewed his later works more as specifications of the same core of ideas (Kuhn 2000, 33ff.).[1] What is certain is that in his later works, Kuhn asserts that incommensurability does not mean non-comparability; rather it means the inability to translate the concepts, theories and standards of one paradigm into the other (2000, p. 237-238). A 'point-by-point comparison' is therefore explicitly rejected, for there is no common language within which two theories from two rival paradigms could be fully expressed (1976, p. 191; 2000, p. 189).

But what is wrong with the incommensurability thesis? Does it pose any threat to any orthodox accounts in the philosophy of science? Kuhn's characterization of scientific change raised some challenging questions. Philosophers of science, mainly positivists who had held a

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[1] Feyerabend and Kuhn took different paths regarding the incommensurability thesis. While Kuhn made some revisions and clarifications in response to his critics, Feyerabend maintained his strong view of the incommensurability of rival scientific theories in the domain of perceptions and the framework of thought and action (1975, p. 271).

continuous and incremental understanding of scientific growth, encountered a turn in the philosophy of science whereby the transition between different paradigms were conceived as non-continuous phases in the scientific enterprise. Although positivism and different kinds of scientific realism have different tenets, they generally agree on the steady (or linear) progress of science. Traditionally, progress in science was defined as a cumulative advancement of science toward the truth (Sarton 1957, p. 5, Brag 1936, p. 41). Even the most recent realist theories of scientific progress define progress as an 'increase of truths known about the same entities to which earlier theories referred' (Sankey 2017, p. 202). However, in the light of Kuhnian discontinuous understanding of science, such accounts of scientific progress fell under an anti-realist shadow.

The idea that transitions between scientific paradigms are non-continuous phases of the development of science raises two potential problems for the notion of progress. First, while the traditional conception requires a steady improvement of scientific knowledge, a Kuhnian suggestion implies that 'every gain in our knowledge is accompanied by attendant losses'.[1] The reason is that sometimes the paradigm shift could lead to fewer truths overall (e.g. losing some of the precision or accuracy of Ptolemaic predictions right after the advent of Copernican theory), while still getting us closer to the truth by providing a better theoretical model of the entities. Second, such a Kuhnian suggestion conflicts with convergent realism, the idea that science improves in an ever-increasing approximation to the truth *about the same referent*.[2] This means that during conceptual change the subject matter (or reference) of study changes, so that scientific progress cannot be considered as an increase of knowledge about the same subject matter.

Further, this non-continuous account is especially problematic for those scientific realists who seek to define scientific progress in terms of an ever-growing verisimilitude (or approximate truth) *about the same set of entities*. For example, Ptolemaic geocentric astronomy provides some truth-like beliefs about the stars and their movements, while a Copernican model entails *more* truth-like beliefs about these entities. For Kuhn, however, there is no neutral and theory-independent ground by which this progress can be claimed, except the idea that endorses the improvement of science as providing better instruments to solve scientific puzzles of a time. So, scientific progress occurs by means of increasing the efficiency of scientific instruments (e.g. theories and concepts) in solving more problems (Kuhn 1970, pp. 166-170). Although new paradigms make progress by increasing the number of puzzles that can be solved, they lose problem-solving power in other areas. Kuhn, and subsequently Laudan, held that the move from Ptolemaic geocentric theory to heliocentric theories was accompanied by some losses of problem solving.[3] Scientific realists, in contrast, typically

[1] (Laudan 1977, p. 3). It is important to note that 'Kuhn loss' by itself does not imply a direct problem for scientific progress as long as the gains are more than the losses. But according to Kuhn and Laudan there is no determined measure to compare losses and gains, thus the continuous progress of science is in question.

[2] See Laudan (1981): 'A Confutation of Convergent Realism'.

[3] This thesis is called 'Kuhn-loss', the title having been coined by Heinz R. Post in his (1971), "Correspondence, Invariance and Heuristics," *Studies in History and Philosophy of Science*, 2, pp. 213–255.

formulate progress in terms of truth, for they view it as the essential aim of science.[1] Thus, a discontinuous account of scientific change would endanger the accumulation of true beliefs.

It should be noted here how much this discontinuous account of scientific change is connected to a discontinuous account of conceptual development. It is argued that there is a direct relation between theory change in history of science and *conceptual ontogenesis*. Alison Gopnik writes “the concepts of the new and old theory and of the evidential description are incommensurab[le]” (1988, p. 199). She compares the development of concepts for children to theory change in science and concludes that “concepts are embedded in complex theories and there is no simple way of comparing them” (ibid, p. 205). The important point here is that the analogy that she makes between a discontinuous reading of the history of science and conceptual development cannot be rejected by rejecting Kuhn’s view of theory change[2]. A discontinuous account of theory change is likely to lead to discontinuity of conceptual development and vice versa, mainly because it is implicitly embraced that concepts are partly or fully defined by the theories that employed them. The idea that theories constitute the meaning of concepts they contain is usually attributed to the Inferential Role Semantics (IRS) in philosophy of language, but I will discuss this theory in chapter 3. For now let’s return to the problems of a discontinuous view of science.

The rationality of theory choice in science, in a similar vein, could be in danger if the discontinuity of scientific paradigms is taken seriously. It is typically assumed that a selection among a number of rival theories is governed by a method (even an implicit one) that to a large extent involves observational data. In the case of a discontinuous understanding of conceptual change, it is argued that the two rival theories utilize different methods, which could be incommensurable (Methodological Incommensurability). So, during scientific revolutions, where the standards, methods and questions radically change, theory choice (and similarly choice among their key concepts) for the proponents of a paradigm can be non-rational. For there is no commonly accepted method shared between the rival paradigms. This process would ultimately lead to epistemological relativism, given the absence of a higher norm for rational theory selection. There exist signs of this relativistic view in Kuhn’s works:

> The premises and values shared by the two parties to a debate over paradigms are not sufficiently extensive for that. As in political revolutions, so in paradigm choice—there is *no standard higher than the assent of the relevant community*. (1970, p. 94, my italics)

However, in the postscript of the same work (1970, §6)[3], Kuhn tries to reject the relativistic charges against his account of incommensurability. He emphasizes that incommensurability neither means nor implies a form of epistemological incompatibility; instead it suffices to

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[1] Some realists make claims about increase of knowledge (Bird 2007), rather than truth (Sarton 1957) or approximate truth (Niiniluoto 1999).

[2] Gopnik (1996) describes this analogy as ‘the scientist as child’ suggesting that the process of scientific change for scientists is very similar to what happens for children during the process of learning general concepts.

[3] This book is originally published in 1962, but Kuhn added some revisions and a full post-script to its second edition in 1970.

adopt a conceptual incompatibility stemming from the theoretical difference in the structure of rival paradigms. In a later work, Kuhn elaborates his account of incommensurability by referring to the notion of *ineffability of propositions* among rival paradigms. This means that scientists understand and are able to express certain statements *only after* the introduction of a new theory. As an example, a Newtonian physicist using his own lexicon is simply unable to express certain Aristotelian propositions (2000, p. 244).[1] However, scientists belonging to the newer paradigm can understand the past paradigms in the way that historians try to understand old statements by setting aside their current conceptions. This kind of incommensurability is sometimes called *taxonomic incommensurability.* According to taxonomic incommensurability, which is the latest version of his incommensurability thesis, the transition between incommensurable theories leads to the alteration of lexicons associated with each class of categories in science. A radical scientific change causes change in scientific taxonomies and makes impossible the translation between old and new taxonomies.

Kuhn does provide some shared norms for rational selection among rival theories or concepts. He puts forward some higher and inter-paradigm values, probably non-objective, to govern a rational scientific enterprise. He asserts:

> … the rationality of the standard list of criteria for evaluating scientific belief is obvious. Accuracy, precision, scope, simplicity, fruitfulness, consistency, and so on, simply are the criteria which puzzle solvers must weigh in deciding whether or not a given puzzle about the match between phenomena and belief has been solved. (2000, p. 251)

Although problems of conceptual change are not restricted to scientific progress and rational theory choice, these problems can be summarized as stemming from a discontinuous account of scientific change. The incommensurability thesis in its varying manifestations (Sankey 1993) captures what is most problematic in this strict discontinuous view. Conceptual change or scientific change could be dramatically less harmful if the discontinuity between any two steps of change could be resolved. Otherwise, it is required to explain how science has been developed without any connection between different stages of its development. On the other hand, a radically continuous account of science (e.g. steady accumulation of truth in the course of scientific development) leads to accounts such as the early positivists. These accounts, among other problems, are unable to explain radical departures from previous (wrong) theories. Thus, if both a radically continuous and a radically discontinuous account fail to explain the process of scientific change, any adequate explanation of conceptual change should seek a trade-off between these two extremes. Such an explanation should retain our intuition of science as a gradually progressive enterprise that enables scientists to be innovative in changing their concepts to newer ones. While I will discuss this issue in chapters 4 and 5, it should be remembered that throughout the thesis when I say 'explaining

[1] He writes: "Using our conceptual lexicon, these Aristotelian propositions [relation between force and motion or impossibility of void] cannot be expressed—they are simply ineffable—and we are barred by the no-overlap principle from access to the concepts required to express them". (2000, p. 244)

conceptual change' I mean explaining the issues associated with conceptual change, namely scientific progress and rational continuity.

4- Approaches to Conceptual Change

Different readings of the problems associated with conceptual change engender different approaches, and hence lead to different responses. The traditional approach views Kuhn's challenge mainly as a *semantic* issue, arguing that problems like incommensurability are based on a problematic theory of reference. Israel Scheffler was among the first philosophers to base their work on such a semantic reading of Kuhn's challenge (1967). He used the Fregean dichotomy between sense and reference to emphasize the importance of sense variation in most cases of scientific change. According to this view, most of the meaning variations in science are caused by variation in the senses that are attached to scientific terms; not by variation of reference. Following this approach, it is argued that the problems caused by Kuhn's challenge are based on a problematic theory of reference, and it is this problematic theory which leads to the consequence of loss of referent through cases of scientific change. It is argued, for example, that a descriptive theory of reference-fixing ultimately leads to the incommensurability thesis; so using a causal or causal descriptive theory instead can resolve the threats posed by a Kuhnian account of conceptual change (Sankey 1994).

A wide range of philosophers have followed this general approach (Scheffler 1967; Putnam 1973; Fine 1975; Devitt 1979; Newton-Smith 1981; Hacking 1983; Burian 1985; Papineau 1987; Kitcher 1993; Sankey 1994; Psillos 1999). All are committed to a referential strategy in responding to the challenge, claiming that "the remedy is to begin with the notion of reference" (Kitcher 1978, p. 522). This is the standard response to the challenges of conceptual change that have occupied most of the literature for much of the past forty years. Scientific realists, for example, usually take this general approach, because a realist account of scientific progress seems to be under attack from a discontinuous understanding of scientific change, and also because of their commitment to the reference of theoretical terms during scientific development. Although commitment to a given notion of reference or any specific theory of reference is not a requirement of being a scientific realist, realists usually approach the challenge semantically, and indeed it is interpreted as a semantic problem for scientific realism. Different sorts of theories of reference have been proposed in order to retain the continuity between rival scientific theories in the course of change. A causal or causal-descriptive theory of reference has been the favorite choice of scientific realists, since such a theory can connect scientific concepts in subsequent paradigms via a causally fixed reference. Such a referential response is also compatible with one of the main characteristics of scientific realism, that scientific terms including theoretical and observational terms have their referents in the mind-independent external world.

On the other hand, there exists a novel non-referential approach to this issue. This non-referential (non-semantic) approach begins by criticizing the referential response. An *epistemic* approach, for example, challenges the semantic approach for focusing only on reference. The main tenet of this approach is that some *other* important elements contribute to the *semantic* analysis of certain cases of scientific change. For example, some cases of conceptual change in biology (see Chapter 2) involve a change in the epistemic interests guiding inquiry, while the reference stays stable. So, it can be argued that some epistemic conditions have implications for semantic analysis of concepts. Ingo Brigandt (2006b) goes further by arguing for the provocative idea that the Kuhnian challenge is originally an epistemic, rather than a semantic problem. According to him, although Kuhn employed a semantic idea in characterizing his challenge, his argument starts with an epistemic point and comes to rest on an epistemic conclusion: "Kuhn's argument starts with an epistemic point (difference in belief) and using a semantic detour (differences in meaning) he draws the epistemic conclusion" (ibid, p. 22). This provocative claim cannot be applied to Feyerabend's work, because he is explicit in the semantic structure of his account of incommensurability. But Brigandt's point is not a claim in the history of philosophy. Rather, his main claim is that a focus on reference is by no means sufficient to explain conceptual change. He correctly addresses a large number of philosophical works that deal with the problem of incommensurability from a purely referential perspective, including theories that involve Fregean sense in determining reference.[1] Brigandt's proposal is thus that *semantic incommensurability*[2] cannot be fully resolved only in terms of reference within a semantic approach. He asserts that "an account of reference does not address at all the idea that meaning differences entail epistemic troubles" (ibid. p. 23).

Reinforcing his objection, Brigandt describes certain biological case studies in the history of science which even the most sophisticated theories of reference (i.e. hybrid theories) are unable to explain. Previously, philosophers of biology had tended to address the complexity of biological concepts and theories through comparison with cases drawn from physics or even chemistry (Ellis 2001; Dupré 2013; Weber 2014). Yet, Marcel Weber provides a detailed account of the history of genetics to illuminate the differences between physical and biological sciences in terms of scientific change. According to him, 'there may be causal as well as descriptive elements involved when experimental biologists attached the term "gene" to some class of unknown factors… however, this causal descriptive apparatus was never sufficient to pick out… a molecular constitution or something of this sort' (2014, p. 444). For the case of 'gene', he introduces the notion of 'floating reference' in order to show how the reference of this biological term has changed deeply and frequently. Weber's conclusion is that some biological kinds and their associated concepts are "relational, functional, variable, generic, and sortal" and these properties are 'toxic' for a referential response to incommensurability (ibid). Weber holds that these deep referential variations in biology are

[1] The most famous one is Kitcher's normative insight that "the remedy is to begin with the notion of reference" (1978, p. 522)

[2] I lay emphasis on 'semantic incommensurability' because a philosopher from the semantic approach may reply to Brigandt's proposal by arguing that he is dealing with the methodological incommensurability problem, instead of the semantic problem. I think both of these approaches deal with 'semantic incommensurability', while Brigandt construes it originally and mainly as a problem with epistemic consequences.

not problematic for scientific realism, but they may conflict with the accounts that try to defend scientific progress or rationality of conceptual shifts by fixing reference. According to him, in biology, unlike other scientific disciplines, 'truth comes much easier'; so, defending scientific realism does not require reference fixing.

Brigandt himself adduces other biological case studies, namely the concepts of *Homology* and *Evolutionary Novelty*, to suggest that some factors are more significant than reference (e.g. contextual and epistemological factors) in the explanation of conceptual change (2012).[1] For example, the term 'homology' fulfils different functions in three different disciplines: evolutionary, evolutionary-developmental (evo-devo), and molecular biology. Each of them is associated with a specific concept. Although the reference of these concepts is to a large extent shared, these concepts are critically different, because they are used in pursuit of different epistemic goals. For example, while the epistemic goal of using the evolutionary concept of 'homology' includes explanation of the transformation of traits from one generation to another, the molecular notion of 'homology' is used where we seek to find out the molecular structure of homologous organisms. According to Brigandt, in agreement with Love (2005),[2] concepts incorporate an epistemic component that determines which epistemic goals and values a concept is used for. This epistemic component is the key to distinguishing certain biological concepts whose reference and meaning are less important in their individuation. Also, this component is what retains concept identity, when reference and meaning change. Thus, an adequate account of conceptual change should highlight this crucial component, instead of focusing on the reference alone. Having considered the deficiencies that a semantic approach is faced with, Brigandt rules out all purely referential responses to incommensurability as 'insufficient'.

While an epistemic approach argues for the *inadequacy* of a semantic approach, a semantic approach would discredit the *authenticity* of an epistemic approach. To be more precise, there is a distinction between *semantic* incommensurability and *methodological* incommensurability. While the former addresses the issue of semantic variance of certain scientific terms employed by rival theories, the latter points to the lack of common standards for theory appraisal (Sankey 2009, p. 197; Sankey and Hoyningen-Huene 2001, p. ix). It might be thought that the difficulties raised by the epistemic approach should be placed under the methodological incommensurability thesis. So, for example, failure of reference fixing in some biological case studies is not due to a semantic issue, but an epistemic one. Although this distinction resolves a couple of issues, I believe that it is not a full solution of the conflicts between these two approaches. The reason is that some epistemic factors could have

[1] I previously set this assumption that by 'explaining conceptual change' I mean explaining the rationality and progress through conceptual change. In other words, I simplified the question of explaining conceptual change to explaining the main issues associated with it. So, an account that explains what such changes consist in can be regarded as an explanation once it provides solutions for the rationality and progress of change.

[2] Alan Love (2005) introduces new terminology for this purpose, with some differences and consequences. While Brigandt uses 'Epistemic goals and values pursued with using a concept', Love prefers the 'problem agenda' that each concept sets in practice.

bearings on the semantics. Reference fixing (the semantic response) itself sometimes involve determination of epistemic factors (In the next chapter I will give some examples where the epistemic factors play role in reference fixing). Also, some proponents of the epistemic approach to conceptual change, such as Brigandt, explicitly direct their points and objections to the semantic understanding of conceptual change. So, it seems that we should treat this novel approach as an important rival to the standard semantic approach.

As a final remark, it is important to note that the epistemic approach is not the only alternative to the semantic approach. Another approach considers the Kuhnian challenge as a set of problems based on specific metaphysical assumptions. Paul Hoyningen-Huene (1993) has a (neo-) Kantian reading of Kuhn's challenge, separating the world-in-itself from the phenomenal world (noumenon vs. phenomenon). While the world-in-itself is unchangeable, in some cases of scientific change (which may involve world change) scientists change their understanding of the phenomenal world. Since a careful consideration of this approach would require a dedicated work in itself, I leave this alternative and other possible ones for another occasion. For the purposes of this thesis, I focus on the epistemic approach to conceptual change.

The thesis begins with a chapter dedicated to the importance of biological case studies. I will argue that biological concepts provide complicated cases of conceptual change in which the epistemic and evolutionary factors play important roles for giving an adequate explanation of conceptual change. It is also important to consider these case studies because the main proponents of the epistemic approach, namely Ingo Brigandt and Alan Love, use biological case studies. In chapter 3, I will discuss a particular example of an epistemic understanding of conceptual change. This chapter explains Brigandt's theory of concepts, which entails that the content of concepts includes reference, inferential roles and epistemic goals pursued using a concept. He puts the emphasis on the epistemic goals in order to address what he calls a non-representational account of conceptual change. Chapter 4 provides a detailed philosophical evaluation of Brigandt's framework of conceptual change in the light of a standard package of realist views. I will show that Brigandt's account is minimally realist in endorsing that theoretical terms can have a reference, but it rejects some important commitments of scientific realism that form part of the standard realist package of views. I agree with a number of Brigandt's claims, especially the ones that suggest the importance of epistemology in explaining conceptual change and in fixing the reference of scientific concepts. This represents a major advance on standard accounts of conceptual change and reference determination, for few works in philosophy of science and philosophy of language have considered these epistemic issues. However, I disagree with Brigandt where he assigns an independent role to the epistemic goals as a content of concepts, as well as where he makes the strong claim that reference determination is 'relatively unbound and open'. Therefore, chapter 5 sets out a framework for an epistemic theory of reference determination. My aim in that chapter is to preserve a referential approach in concept evaluation, and to give an important role, but not directly reference-fixing, to the epistemic factors. This strategy has a crucial influence on my understanding of scientific realism and scientific progress, but I believe that my understanding still will remain a realist one. Although this final chapter tries to cover a lot of ground, the issues therein could easily have been extended to more chapters or even to an entire doctoral thesis. Limitations of space will require an abbreviated treatment of these otherwise fascinating issues.

5- Conclusion

In this chapter, I set out different understandings of the notion of conceptual change. So, this chapter works as an introduction for all other chapters. Here, I started with Kuhn's and Feyerabend's characterizations of the incommensurability problem. According to this problem, two rival paradigms could be completely disconnected, so that there is no higher norm to compare rival theories or to argue for a cumulative growth of knowledge in terms of truth. I showed that a discontinuous understanding of science would raise significant problems for an orthodox understanding of science, namely that scientific choice among concepts and theories is rational and the advance of science is progressive. These discussions raised a wider issue of conceptual change in science in which the concepts associated with key scientific terms change altogether. I showed that there is no consensus among philosophers of science on a common definition of conceptual change, although some examples of this notion are widely discussed in the literature. Some view conceptual change as a matter of concept replacement, entailing that change in the meaning or reference of concepts does not inevitably change their identities. Others argue that concepts change once their meaning and/or reference change. I argued that a modest account of conceptual change should allow concepts to keep their identities despite some revisions in referential or linguistic relations. This allows us to find continuity in the language of science, which is the solution (*remedy)* of problems such as incommensurability. Meanwhile, this account should also be controlled, so as not to make static concepts that cannot be changed at all. This is a framework for what I will argue for in the last chapter of this thesis.

Finally, I draw a distinction between a traditional and a novel approach to conceptual change. The referential approach is identified by its referential perspective, which entails seeking for a good theory of reference to explain conceptual change. On the other hand, the novel (non-representational) approach seeks to expand the factors that contribute to explaining how conceptual change is compatible with rational continuity and scientific progress. According to them, epistemic interests are important elements of conceptual understanding that do play a role in reference determination, and consequently help in explaining conceptual change. So, the non-representational approach proposes to take these important epistemic factors into account. This focus on how epistemic interests shape inquiry and conceptual change is a ground for the issues I explore in chapters 3 to 5.

Chapter 2

The Importance of Biological Cases of Conceptual Change

1- Introduction

For a long time, philosophy of science was heavily oriented toward physics. This was partly due to the fact that physics had long been seen as an ideal model of a science which all other sciences should emulate; and where it was not openly assumed that the other sciences were reducible to physics, it was at least implicit that what applies to physics is applicable to all other sciences. Physicist Ernest Rutherford once said, "All of science is either physics or stamp collecting".[1] In the last four or five decades, however, life sciences have attracted increasing interest from philosophers of science. This change of interest was unattainable until biologists and philosophers of biology recognized the importance of life sciences in comparison with the physical and chemical sciences. One of the first serious complaints of the bias towards physics in philosophy of science was voiced by Ernst Mayr in his *Footnote on the Philosophy of Biology* (1969). He noticed that philosophy of science had become rather similar to the philosophy of

[1] This is a famous quotation which is found in the literature. There exists, however, some doubts that Rutherford did say exactly these words, since there is no authoritative direct quotation. An investigation of this matter is provided at the website Quote Investigator: http://quoteinvestigator.com/2015/05/08/stamp/

physics: “many physicist-philosophers naively [assume] that what applies to physics will apply to any branch of science” (p. 197). The key issue is not simply that biology, as Mayr observed, does not follow the other branches of science; biology in fact raises difficult problems of its own, which challenge widespread philosophical notions in physics and chemistry. This chapter will set out some of the *complications*[1] that biology brings to philosophy, specifically concerning the particular issue of conceptual change in science.

It is now three decades since Burian noticed that “conceptual change in science has been a ‘hot’ topic” (Burian 1987, p. 3). Debates concerning conceptual change in those decades were largely concerned with the particular problem of incommensurability and reference (continuity), due to the deep influence of Kuhn and Feyerabend. According to the problem of incommensurability, a single term may refer to two different things within the conceptual frameworks of two different theories. Assuming that each scientific term is associated with a contentful mental representation, sometimes called concepts, we may say that concepts are faced with the same problematic situation: possibly referring to more than a single referent in different frameworks, or maybe even referring to nothing at all. Taking the problem of incommensurability seriously, in case of a referential gap between two concept users within different conceptual frameworks, the common subject matter of scientific investigation changes, so it seems there is no common ground for rational communication and progressive theory choice[2]. Due to the referential approach to the notion of conceptual change, most of the responses to such problem aimed to find a stable reference for scientific terms and concepts, in the hope that reference would survive through conceptual change (for example, a variety of causal theories of reference were used to solve this problem[3]).

Although the problem of incommensurability lost most of its force by the late 90’s, the notion of conceptual change has remained a hot topic since then. For there exist important concerns such as conceptual progress in science and rational conceptual change, which could remain even if the incommensurability problem is avoided. So, a standard referential response to the incommensurability problem remains a viable approach to explaining referential stability through conceptual change. The ‘referential approach’ to the notion of conceptual change, thus, deals with it as a referential problem, and hence tries to explain it by focusing on reference. Undoubtedly, the goal of referential accounts of conceptual change has not been

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[1] I use the word ‘complication’ instead of alternatives such as ‘complex’, ‘interesting’ etc. because I do not want to address the various aspects of biological concepts, but only to refer to the difficulties that biology may import into the study of conceptual change *due to providing additional aspects that play a role in explaining conceptual change*. I will explain that biological case studies have certain features which cause complications for the traditional philosophy of science which takes the referential approach and deals with physics as its exemplar.

[2] In response to the critics against this strong reading of incommensurability problem, Kuhn himself revised it to a modest or local account of incommensurability. This account confines the incommensurability consequences to the translation failure between two rival paradigms (Kuhn 1983, p. 670-671).

[3] Also, some of descriptive or causal-descriptive theories of reference can be considered as approaching the problem of conceptual change from such a referential approach, since their main concern is to identify a referential continuity through conceptual change, even in terms of the descriptions of reference that rationally connects the stages of a conceptual change.

to justify or motivate why certain conceptual changes have taken place, rather, they try to argue for conceptual *continuity in terms of reference*[1]. From now on, I recognize the 'traditional' accounts of conceptual change as holding such an approach.

In addition, most of the original case studies of conceptual change were concerned with the physical sciences. The case of the Newtonian framework of physical terms ('mass', 'force', etc.), in contrast with that for Einstein's, was among the most prominent.[2] However, in recent decades the debate has shifted in both focus and approach. After the rise of biology as a significant issue for philosophers and historians of science, the question of conceptual change has reappeared from a new perspective, discrediting the explanatory potential of the previous referential approach. Mainly using biological case studies, this novel approach has to some extent made 'conceptual change' a hot topic again in the philosophy and history of science. In fact, biology furnishes philosophy of science with complicated case studies in which a concept can radically change in terms of reference *and* all other semantically relevant features, and yet there is no irrationality or lack of progress in the process of change. So, a novel approach to conceptual change holds that conceptual change should not be reduced to the problem of incommensurability, and, in the same vein, it should not be reduced to the problem of reference (i.e., to the referential approach) (Brigandt 2012; Love 2015). One of the main sources of evidence for this novel approach comes from biological case studies, such as 'gene', 'homology' and certain complicated biological classifications.

I do not describe the novel approach to conceptual change in this chapter; this will be a topic for later chapters. Instead, my proposal is that the novel approach is basically supported by these complicated examples from biology. In dealing with *living* objects, biology has some special features that make its cases of conceptual change challenging. So, one of my tasks is to frame a rationale for focusing on biological case studies in this dissertation. I am not suggesting that biology is more important than physics; neither will I argue for a version of reductionism in which biology can be reduced to physics or chemistry. My claim in this chapter is far more restricted: *when it comes to studying conceptual change, biological case studies involve certain features, as opposed to other scientific fields, that make complications for the traditional referential accounts of conceptual change.* These complications promote an alternative, not-purely referential, account of conceptual change. I will argue that dealing with living, evolving, changing and historical objects on the one hand, and having an interest-relative and local theoretical basis on the other, makes biology a suitable field for studying conceptual change. It is important to notice that I am not going to disqualify other case studies in general. Rather, my final aim is to provide a rationale to pay more attention to the life sciences in this discussion. In particular, it becomes important to take such complicated

[1] Andersen (2001), for example, sets the goal of her study to permit 'a continuous process of referential change' (p. S50).

[2] Occasionally, chemical and biological case studies can be seen in the literature from those years. For example, 'Phlogiston', which was replaced by 'Oxygen', is one of the most cited examples (Kuhn 1970). Kuhn later discussed the term 'mammal' as a case study (1990). However, the mainstream was still devoted to the concepts of the physical sciences: electron, force, atom, gravity, magnetic fields, heat, star, etc.

biological case studies into account where the alternative approaches have already been proposed (in fact since the early 21th century) and have already challenged the traditional referential approach (I discuss one of the novel approaches in Chapter 4).

In the following, I begin with a complicated case of conceptual change, namely the concept of *homology*, which has motivated many works in the literature (Section 2). After showing why the traditional approach cannot explain this biological example, I state my reasons for the view that the complications behind the biological case studies are in principle based on the historicity of its objects, though it is not confined only to biology (Section 3). Finally, I defend my claim against some misunderstandings regarding the simplification of physics and chemistry and I open up a channel for other fields of science, especially chemistry, to be seen as providing similar complicated case studies for the particular question of conceptual change (Section 4).

2- A Complicated Case Study

In this section I sketch some of the complicated aspects of the concept of *homology* when studied from the point of view of conceptual change. My main aim is to show that this biological case study provides the rationale for going beyond a simple referential approach to conceptual change, the one which reduces the question to referential incommensurability. In particular, my claim in this section is that studying the history of 'homology' involves further important factors, such as the evolution of living organs and the importance of scientists' decisions on some significant issues, which other physical or chemical case studies somehow lack.

The philosophical relevance of the concept of homology comes from the fact that it is a central concept in biology that has undergone ongoing semantic changes. Despite long neglect of this central concept by philosophers, it is now more than two decades since discussions regarding the philosophical implications of this concept first appeared in the literature (Griffiths 1994, 2006; Brigandt 2002, 2006b; Brigandt & Griffiths 2007[1]). Homology is one of the most important notions in current comparative and evolutionary biology,[2] crucial for individuating biological characteristics based on their natural relevance to the corresponding characteristics in other organisms. More importantly, some argue that 'homology' is a natural kind term that suggests its importance in biology and philosophy.[3] So, before discussing the complications of this case study for conceptual change, I begin with its complications as a natural kind term.

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[1] The *Journal of Biology and Philosophy* dedicated the 22th volume of the journal (2007) to philosophical discussions of 'homology'.

[2] Some believe that the concept is central for *all* of biology (Wake 1994, p. 268).

[3] The causal theory of reference in philosophy of science was originally proposed for natural kind terms (Putnam 1975) and many important scientific concepts are regarded as natural kinds by philosophers of science.

2.1. Natural Kind-ness

Roughly speaking, homology is the state of having a similarity between a pair of structures, traits or parts, in different biological species.[1] A fuller definition would center on the idea that homologous organisms have the same bodily parts (morphological structures). Homologous structures are usually explained by having the same ancestor, which ultimately depends on an evolutionary account of biology. A well-known example is the structure of the bat's wing, whale's flipper and the human arm, which are all the forelimbs of mammals. These units from different species are quite similar in structure, having a common topology, and form a group of homologous structures. The example shows that homologous structures do not necessarily possess the same function (e.g. one function of a bat's wing is to fly, but not a human's arm). Rather, having similar structures is a tool to individuate natural units within a living system and to categorize organisms on the basis of this similarity.

There are a number of arguments that this concept is in fact a natural kind concept. Brigandt (2006b) recalls that a natural kind is traditionally viewed as 'cutting nature at its joints', and argues the concept of homology does precisely this task 'in a complete literal sense', for homology individuates biological characters by breaking organisms down into their natural units (p. 130). Also, Brigandt follows Richard Boyd (1999) in holding that natural kinds are *projectable*.[2] According to this view, there is a basis for projecting one natural kind member from another one, based on a shared natural property. Having distinguished natural kinds as projectable categories, Brigandt maintains that homology provides such projectability. The reason is that the homologous structures are inherited from the common ancestor, and are therefore quite likely to project a similar property into the corresponding structure, based on the similarity that the associated structures have. Brigandt concludes that the "projectability of many properties is a hallmark of natural kinds and this is my reason for claiming that homologues form a natural kind" (p. 169). Given the idea that membership of a natural kind group requires having some underlying mechanism or structural property in common with other members (sometimes called an 'essence'), Brigandt argues that *common ancestry* is that underlying property, which ultimately explains the projectability of properties in a group of homologues.

However, positing natural kinds in biology is not as straightforward as it seems. Traditionally, debates over this question pertained to the concept of the species, that is sometimes called the 'species problem' (e.g. Ghiselin 1974; Hull 1978). The problematic issue in establishing 'biological kinds' is that live organisms are unceasingly evolving and there exist different methods of of categorization, in such a way that selecting a criterion to demarcate kinds is not compromised. The case of homology suffers from a further problem: as the concept's

[1] Contemporary biology contrasts 'analogy' or 'homoplasy' with 'homology'; the former refers to the existence of similarities within two structures without a common ancestor, and sometimes to parallel evolution.

[2] "…The theory of natural kinds is about how schemes of classification contribute to the formulation and identification of projectable hypotheses…" (Boyd 1999, p. 147)

definition and the associated practice suggest, homology is a *relational property* (i.e. being homologous to certain given structures). Homology is a 'sameness' relation between different structures (usually two structures) that demarcates the respect in which those structures are members of a kind. This implies that homology is not an intrinsic feature of organisms considered independently. But it is constituted at least in part by a relation with other organisms. Homology differs from a chemical notion such as the atomic number, which independently picks out the samples of a single atom-kind. Instead, one structure belongs to a particular natural kind only in conjunction with other structures.

MacLeod (2013) argues that "in the life sciences kind concepts exhibit a diversity of grouping practices that are flattened out by conceptualizing them as natural kinds" (p. 109). He thinks that theories of natural kinds should take epistemic considerations more deeply into account, as opposed to essentialist considerations. The reason is that usually the underlying essentialist (causal) relations are 'inaccessible, nonlinear, or generally complex', so that grouping biological entities is better done based on successful practice. When it comes to the concept of homology, MacLeod distinguishes phylogenetic and developmental accounts of homology. While the former identifies homologous pairs with synapomorphy (i.e. shared features of groups that contain all the descendants of a closest ancestor), the latter concerns the role of development in evolution. The fact that these two versions of the concept of homology have different aims, strategies, and sometimes references, makes it plain that "the use of natural kind theory in this case risks flattening out an essential methodological disagreement between these homology concepts" (p. 116). So, although both concepts involve the notion of the similarity of two structures due to having common ancestry, methodological disagreements lead them to identify homologous structures differently and to group the instances of a *natural kind* differently in practice. For the sake of brevity, I pass over the added complications concerning a similar trait being the result of the same ancestry *or* of inheritance from developmental features.[1]

Even Brigandt (2006b), who defends a natural kind account of homology,[2] admits that his notion of natural kindhood is 'vague' and denies that there is a strict demarcation line between natural and non-natural kinds. Actually, the fact that homology is relational, and to some extent a matter of degree[3], leads to a questionable status as a natural kind compared with features picked out by paradigm natural kind concepts. Brian Ellis, an essentialist in the natural kind debate, accepts that there is a degree of messiness in the life sciences:

> Because of the messiness of biological kinds, and in order to develop a theory of natural kinds adequate for the purposes of ontology, I have broken with the tradition of using biological

[1] For more against the idea of 'homology' as a natural kind term, see West Eber-hard (2003). For similar complications for the case of 'gene' see Weber (2014).

[2] Brigandt follows Boyd's (1991; 1999) account of natural kinds as homeostatic property clusters.

[3] Remane (1956) proposes a probabilistic approach to homology, according to which whether 'homology' is applicable to its instances is determined by the number of criteria that are met and the extent to which they are satisfied (Brigandt 2006b, p. 193).

> examples, and taken the various kinds of fundamental particles, fields, atoms and molecules as paradigms. (Ellis 2001, p. 170)

One reason for this messiness is that biological entities are characterized by evolutionary change, making it difficult to find a stable essence over time. The historicity of biological kinds entails the instability of their putative essences. Many arguments against construing biological terms as natural kind terms are based on this observation, *namely that there is no internal shared essence among different members of a kind*. Rosenberg and McShea, for example, assert that biological taxa "do not have any essential properties. There is no set of necessary and sufficient conditions for being a robin" (2008, p. 42). Instead, it seems to be a *relational* or *external* feature, which is shared among all members of a kind.

Here, I am not arguing against the mainstream idea that 'homology' refers to a natural kind. At least from a practical point of view, it seems that 'homology' has a good enough fit with the usual tasks that we require from a natural kind term in other scientific fields (e.g. justifying projectability). A concept of homology also provides a structural and evolutionary understanding of biological taxonomy, which ultimately makes scientific reasoning easier. Rather, my intention in this sub-section has been to show the complications that are attached to categorizing this biological concept due to the interference of some relational and external factors. Also, I have suggested that it might be the case for other biological concepts as well. Generally speaking, many biological kinds are different from straightforward kinds in having relational and external properties. For example, if 'biological species' is defined as "groups of interbreeding natural populations that are reproductively (genetically) isolated from other such groups" (Mayr 2004, p. 177), then the relevant biological kind would involve a relational property: the ability to breed with *other members* of that group. Also, this is an external property, because it does not refer to a fully intrinsic essence of a member by itself.[1] These properties make it indeterminate whether some borderline individuals are members of that kind, because it is indeterminate whether they can meet the conditions (e.g. two individuals inhabit far away islands, but they *can* breed with each other). I will identify some reasons behind this complication in section 3.

2.2. Conceptual Change

One interesting feature of the concept of homology, especially for philosophers of science, is the fact that it was introduced a long time ago (in the 18th century), well before the advent of Darwin's evolutionary theory. But nowadays the term 'homology' is associated with a variety of understandings and concepts. When it comes to studying conceptual change, it helps to focus on a concept that presents a range of understandings through different periods – here, from pre-Darwinian to Darwinian and then modern understandings of 'homology' in biology.

[1] Another external factor in specifying a biological natural kinds is scientists' *point of view* in the classification of organisms. This will be discussed in the next section.

In reviewing these periods, I try to focus on the importance of non-representational[1] elements that ultimately distinguish different concepts corresponding to a term. Here, I briefly explain conceptual change and semantic variation[2] in the case of 'homology' in order to show that the traditional referential approach[3] that is pursued in explaining conceptual change (e.g. Scheffler 1967; Putnam 1973; Papineau 1987; Sankey 1994; Psillos 1999, Andersen 2001, and etc.) is not quite successful dealing with this biological example.

In science, homology (homologous structures) was recognized when biologists began to distinguish between structural and functional *similarities* among species (two structures are 'analogous' if they are *functionally* similar). For pre-Darwinian biologists, homology was a similarity among body plans caused by a developmental law or based on a blueprint in the mind of God.[4] It is a received idea that Darwin's evolutionary theory paved the way for *homology* to be seen as caused by the same ancestors. This means that Darwin's theory separates pre- and post-Darwinians in the meaning of 'homology'. So, theory change, in this case, brings about a radical meaning change. However, despite the received idea, Brigandt (2012) argues that the early *post*-Darwinian comparative biologists still held the traditional developmental idea. For example, Darwin himself maintains a developmental definition of homology in his *Origin of Species* (1859)[5]. This fact suggests that there might be factors other than the theory that are involved in shaping conceptual change (i.e. the evolutionary theory could not completely and instantly change the concept). Further, it is argued that one single concept of homology was used in practice both before and after the advent of evolutionary theory, although certain changes in conceptual understandings took place. The main reason for positing such conceptual continuity between the pre- and post-Darwinian concepts is the fact that the 'actual concept usage' remained largely untouched. In defining actual concept usage, I should first briefly explain the 'homology criterion'.

'Homology' is not one of those *theoretical* scientific terms whose meaning could be fixed by a theoretical definition[6] or by the reference itself. Instead, successful practice in the use of

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[1] I prefer 'non-representational' instead of 'non-referential' in this context to imply that even those theories of reference that include something like sense or descriptions of reference cannot successfully explain this case study.

[2] These are two different notions, since I acknowledge that conceptual change can take place without meaning change, there can be change of meaning without there necessarily being conceptual change (see Chapter 1).

[3] A referential approach to conceptual change concentrates on the notion of reference to explain the rationality of theory change and possibility of conceptual progress. Following this approach, a good theory of reference can resolve apparent philosophical problems, such as that of scientific progress, by finding a stable reference through conceptual change.

[4] Sir Richard Owen, an English biologist, had a theistic account of homology, using the notion of Archetype (original pattern of construction) (1848).

[5] He maintains that homology is a natural pattern or a correspondence of body plans: "We have seen that the members of the same class, independently of their habits of life, resemble each other in the general plan of their organisation. This resemblance is often expressed by the term 'unity of type'; or by saying that the several parts and organs in the different species of the class are homologous". (Darwin 1859, pp. 191, 434)

[6] Adolf Remane in his important book 'Die Grundlagen des naturlichen Systems, der vergleichenden Anatomie und der Phylogenetik' (1956) argues that although there are different homology criteria (positional, embryological and phylogenetic), biologists should use a concept of homology that is established based on the

this term requires a great deal of knowledge of how to apply it to homologous structures (criteria of homology) and how the practice is organized around well-established instances of homology (identified by morphological or taxonomic practice) (Brigandt 2006). Actually, one of the most important features of the concept of homology is in identifying a pair of structures in different organisms as homologues. But such a role cannot be played by appealing to reference alone. For in cases where the reference is the same, it is the homology criteria that determine whether two samples should be considered homologous or not (e.g. bat's wing and the human arm might be homologous or not in different respects). Thus, the notion of 'homology criteria' is a crucial element in determining the samples that fall under the term. For example, the way biologists recognize homologous structures, via specific homology criteria such as appealing to positional or embryological analogies is crucial to bringing about different understandings and sometimes assign different references to the concept.[1] The point here is that homology criteria (e.g. positional or embryological analogies) are not factors determined by causal or referential relations. Instead, in each context scientists implicitly or explicitly decide which criterion determines the relevant conceptual understanding.

It was only around the sixth decade of the 20th century that scientists could come up with a new 'genuine' criterion of homology based on evolution, which is the assessment of homologies based on phylogenetic trees. According to this criterion, biologists could determine whether the sameness of a particular trait or structure is the result of common ancestor (homology) or the result of self-evolution (homoplasy). However, early post-Darwinian biologists did not employ a new criterion, though they changed some of their theoretical assumptions about homology. In fact, both pre- and post- Darwinian biologists used the concept for quite similar instances, and used it in similar *inferences and explanations* and for similar *epistemic goals*: morphological descriptions and comparisons of groups of organisms and classification of higher taxa species. It is true that for post-Darwinian biologists the advent of phylogenetic trees enjoyed the support of evolutionary theory; *however*, this change "proceeded simply by reinterpreting previously known" relations in a common-ancestry orientation; "No genuinely new evidence was needed to arrive at phylogenetic trees" (Brigandt 2006b, p. 390). In fact, it was not the general theory of evolution but the advent of a new criterion of homology that introduced a prominent example of conceptual change by demarcating the new evolutionary concept of homology from the classical concept.

Yet, this is not the end of the story. Supported by evolutionary theory and based on an evolutionary criterion, biologists could explain similar structures among species more easily than with pre-evolutionary accounts in which phylogenetic traits were not addressed. The classification of species and description of organisms could be done with much more accuracy in the light of drawings of the *tree of life*. But the emergence of new epistemic goals and

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use of all criteria. This means that the concept of homology has a degree of confirmation proportional to the number of criteria and the extent that they are satisfied by the samples.

[1] A similar problem can be found in the concept of 'evolutionary novelty', in which proposing a definition is irrelevant and what is important is how to practice this concept in context (Brigandt 2012).

values (i.e. new approaches in biology) formed another case of conceptual change. Beyond the conceptual change from a classical account of homology to the phylogenetic account, nowadays 'homology' is used to pick out different types of structural commonalities and to express different meanings in at least three different fields of biology: systematics/evolutionary, evolutionary-developmental (evo-devo), and molecular biology. The difference between these three accounts of 'homology' is mainly based on the divergent epistemic goals pursued with the concepts. While the phylogenetic account (in systematics/evolutionary biology) seeks an evolutionary understanding of similarities in order to classify and compare species, the developmental account (in evolutionary developmental biology) criticizes them for not providing a developmental explanation for the re-appearance of particular structures in different generations. Developmental homology is based on causal structures originating in species' development and aiming to provide an explanation of the *inheritance of structures*. On the other hand, the molecular account (in molecular biology) uses the concept of homology as a means to explain the causal/mechanistic ways that genes bring about their molecular *products* (Brigandt 2012; McLeod 2013).

This variation in meaning and reference—which is primarily a result of variation in epistemic goals—is another example of conceptual change compared to the classical concept. This means, for example, that the phylogenetic concept grew out of the classical concept.[1] In short, the pre- and early post-Darwinian biologists used the same classical concept with some theoretical differences, and then an obvious conceptual change took place by changing from the classical to the phylogenetic account. Now, the phylogenetic concept and at least two other concepts are actually associated with 'homology' in contemporary biology and it depends on an individual scientist's context and decision to employ either one of them. Despite the fact that the reference of 'homology' in these different concepts *to a large extent* overlaps,[2] the variation in epistemic goals which is determined in *actual concept usage* differentiates their associated concepts. *Actual concept usage* determines which concept is at stake (e.g. molecular or developmental), based on a particular epistemic goal, theoretical background and set of referents. This entails that using a concept of homology, for example the molecular one, in a wrong context, for example the developmental discussion, hampers successful communication.

This is what seems inconsistent with at least some traditional accounts of reference and meaning, which take the meaning of theoretical scientific terms to be exhausted by an explicit theoretical definition or by their role in an overall theory (Carnap 1966; Lewis 1970; Papineau 1996). Although the theory that employs a term or the definitions associated with that term may determine the proper usage of a certain concept within scientific inferences and explanations, I showed that some biological concepts are determined only by using contextual

[1] Following Love (2015, p. 14), Thagard (1992, p. 35) and other standard definitions, I take the birth of a new concept out of another as a clear example of conceptual change.

[2] One important example that reference of different homology concepts do not overlap is 'serial homology', in that the developmental homology, unlike a phylogenetic account, takes it as a case of homology.

factors relevant to the theoretical interests of scientists (their epistemic goals). Also, an explanation of conceptual change in biology cannot be given by the usual causal or descriptive theories of reference, since some cases of conceptual change are constituted primarily by the variation of epistemic goals. In the same vein, even the current causal-descriptive theories of reference, cannot adequately explain conceptual changes that take place in biological concepts like homology, *simply because the epistemic purposes that a concept is used for do not boil down to causal relations or descriptions of the referent*. I believe that the epistemic goals and the kinds of inferences and explanations supported by a concept play a crucial role in analysing the evolution of a concept, suggesting that these elements cannot be fully exhausted by some statements about the referent. These non-representational elements—the theoretical interests and goals that guide scientists to use the proper concept on a particular occasion—determine the *actual concept usage* in the first place.[1]

Thus, it seems that the explanation of conceptual change in this typical case of biology invokes elements over and above the reference and reference fixing invoked by the traditional accounts of conceptual change. Also, homology is not the only concept that involves such complications: there are other similar biological examples in the literature (e.g. Weber 2014; Waters 2014; Stearns 2015; Wagner 2015; Brigandt 2010a). Marcel Weber argues that such non-referential complications took place in the history of 'gene', where reference is 'floating'. Thus, he concludes that the project of 'reference fixism fails' in general (2014, p. 444). In another place, he writes: "It seems to me that the situation [in biology] is thus entirely different from the well-known cases of conceptual change in the physical sciences" (2005, p. 225). While philosophers of science have traditionally tried to resolve the problems surrounding the usual cases of conceptual change (e.g. mass and phlogiston) via theories of reference (causal, descriptive or causal-descriptive theories of reference determination), these biological cases involve elements whose resolution would appeal to factors that extend *beyond reference alone* (such as scientists' epistemic goals and values), and which are not addressed in traditional theories of reference. This insufficiency of the traditional approach opens up a window for novel, non-purely-referential accounts of conceptual change.

[1] The difference between my account of 'actual concept usage' and Brigandt's is that for him it refers only to the kind of inferences and explanations that are supported by a concept, while it seems to me that epistemic considerations including the epistemic goals pursued using the concept are contained in the actual concept usage (i.e., Brigandt excludes epistemic goals from actual concept usage in order to give them an independent status). Here is not the place to argue for this difference, yet, in short, it seems to me that the actual understanding and usage of a concept is based on all the beliefs and assumptions associated to the concept. More importantly, Brigandt introduced epistemic goals to explain a philosophical problem (i.e. rationality of conceptual change), but the actual concept usage may contain other epistemic factors. A competent linguistic user requires all these factors (i.e. reference, inferential roles, epistemic goals and other relevant epistemic factors) to recognize the right concept in a particular discussion. This requirement can be implicit or explicit including or excluding one or several components, but no one is special or more basic.

3- Why Biology?

So far, I have mentioned some examples of the complications stemming from biological cases of conceptual change, yet a key question clearly remains unaddressed: Why does biology incorporate such complicated cases? Before addressing this question, I should emphasize that the complication of biology and complication of biological cases of conceptual change are two different claims. My concern in this chapter is with the latter claim, explaining why the change of some biological concepts is complex in ways that make trouble for the traditional referential accounts of conceptual change. My suggestion is that where the number and intensity of changing elements that play roles in the explanation of conceptual change increase (e.g. reference, meaning, epistemic goals and values), the explanation of such cases becomes unavailable to the approaches that adopt simpler accounts of conceptual change; and this is simply because they do not take those additional elements into account. It is important to add that these complications are not limited to biology, but rather that in biology they are more common and more familiar. In the previous section I showed that analyzing the concepts that are associated with 'homology' requires extra non-referential elements of change (e.g. epistemic goals and values); *these elements* differentiate the concepts expressed by the term, which is not explainable on a purely referential approach. In this section, my aim is to find the reasons why biology is so important in studying conceptual change.

3.1. Evolution

Biological entities have all the properties of physical objects (because they *are* physical objects) in addition to further features exclusive to living creatures.[1] Some put this point as follows: "all known material processes and explanatory principles apply to organisms, while only a limited number of them apply to nonliving systems" (Simpson 1964, p. 106). Others, adopting another point of view, argue that unlike artifacts and machines, living objects are not designed to perform an intentional purpose, but have a function that has been historically acquired (Kampourakis 2014, p. 91). This is not the place to discuss the differences between living and non-living objects. Whatever it is, my concern lies more with the fact that some objects have an additional property of being *alive* (animate). Regardless of whether we think of the property of being alive as something distinct from physical elements (vitalism) or something explainable within the physical domain,[2] it is clear that attributing the property of being alive to an object singles out a distinctive aspect of that physical object. This means that even if we think of life as nothing more than "structural organization of certain molecules",[3]

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[1] Dupré (2013) describes biological systems as "extremely complex" (p. 23).

[2] Sometimes it is argued that there might be no distinction between living and non-living creatures (i.e. all is a matter of degree). However, it seems that there is a difference between two samples at two ends of the spectrum. For example, there is a vivid dissimilarity between a stone and a human. Regardless of philosophical debates on this topic, whatever this difference is enough for my argument.

[3] In his article 'Life in Limbo', Rensberger writes: "Life is nothing more, nothing less, than the structural organization of certain molecules" (*Science* 80, November 1980, pp. 36-43) More academically, Sober (1993) says "It is a sound working hypothesis… that living things are nothing but structured chunks of matter" (p. 24).

there exist some specific *processes* that non-living objects lack. What makes this additional feature important in our discussion is that it appears historically oriented.

Biological organisms by no means work in a way similar to machines, which consist of more or less rigid connections among more or less stable parts. In other words, animated organisms are always subject to processes that rebuild and replace their worn parts with newer and sometimes more suitable ones. In fact, nothing in biology is stable, except certain continuing processes of self-regulation. Innate change, which is a by-product of the property of being self-sustaining, is an inseparable part of a biological entity. In the long term, *evolution is the historical process*[1] that a living organism reflects by its very existence. During evolution, species of organisms *change* in a way that enhances their ability to survive in their environmental conditions. This ontological property (i.e. being in a state of permanent change) differentiates biological entities from physical and chemical ones.[2]

This difference plays a critical role in studying conceptual change, because *where there is constant change in the reference of biological concepts itself, change of associated concepts is more likely*. One simple reason for this is that reference is part of the meaning of a concept. Let's explain this idea with an example. With respect to cases of conceptual change, the concept of *gold* is by no means comparable to the concept of *tiger*, in that the reference of the former seems fixed relative to time but reference of the latter involves constant evolutionary change. It is true that the rate of evolution in tigers is so slow and it does not make a big difference in our understanding of the concept of 'tiger'. However, it is important to note that evolution is cumulative, which means that the birth of an evolved organism does not automatically exterminate the previous form. Instead, it is possible that a particular (maybe artificial) breeding may bring about two different forms of an organism with crucially different properties. Now, it becomes very difficult to decide which one belongs to the kind and which one does not, since there is no strict border (or commonly accepted essence of kind) to demarcate the samples of one form from another one. The result is that our biological concepts have more tolerant borders, making it more likely for conceptual change to happen.[3]

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[1] The importance of the historicity of biology is reiterated by philosophers of biology. Rosenberg and McShea (2008) put the historicity of biology in a short but decisive sentence: "Biology is inescapably historical" (p. 9). More directly, Rosenberg writes "To begin with, biology is a historical science, since…" (2001, p. 755). Sober has a similar line of thought in his *Philosophy of Biology* (1993, pp. 14-18). The idea might go back to Mayr's original idea that "Evolutionary biology is a historical science" (2004, p. 32).

[2] One might claim that all objects, not only living ones, are in a state of historical change in the sense I mean for evolutionary biology. Cosmologists, for example, suggest that physical laws are also historically determined and are subject to change in long-term history. If we accept this thesis, which I myself accept, it should be added that at least the speed of change is far faster in biological cases than physical ones.

[3] Note that when Kripke (1980, p. 317) pointed out the case of 'tiger', he meant to rebut a descriptivist view and to suggest that what fixes reference is a condition like: X is of the same internal structure as tiger-exemplars, where empirical discovery might suggest constitution as being relevant internal structure. However, here I am addressing a further issue: that not only the descriptivist views, but a Kripkean causal theory cannot adequately identify the reference of these terms. The problem is that the very *internal structure* is in constant change.

3.2. Classification

My explanation for the complexity of biological case studies can be discussed from another point of view. In fact, scientists use a variety of procedures to classify biological phenomena and deal with natural regularities. Classification in biology is heavily based on scientists' knowledge and relies upon scientists' individual interests, since biological systems are complex structures that are grouped together with degrees of approximation. Let's clarify this. LaPorte (2004) reports that while Coelacanths (a kind of lobe-finned fish) are classified with higher vertebrates by *cladists* because they have closer ancestors, an *evolutionary taxonomist* classifies them with other kinds of fish due to prioritizing the level of shared structures over having the same ancestors (see Figure 1).[1] Each of these has its own benefits and limitations, which need to be weighed and decided upon by biologists with respect to their context and scientific aims. The view point of cladists, sometimes called the *historical viewpoint*, calculates a collection of shared characters through certain algorithms that maximize the importance of shared (inherited) characters, while minimizing the number of non-inherited developmental characters. Mayr (1976), however, suggests that "the number of evolutionary statements and predictions that can be made for many [cladistic groups] is often quite minimal" (p. 436). As the cladists take only the genealogical elements of phylogeny into account, the fact that fish have more in common with their evolutionary families rather than with the mammals is neglected, Mayr believes. On the other hand, cladists argue that this weakness of predictive potential is the cost of not taking evolutionary change into consideration (Ridley 1986; Rieppel 1992). However, it is not wrong or right to classify in this or that way, for a scientist's decision on a particular occasion may determine the proper classification.

[1] Similarly, MacLeod (2013) mentions that crocodiles are grouped by cladists with birds rather than lizards and snakes.

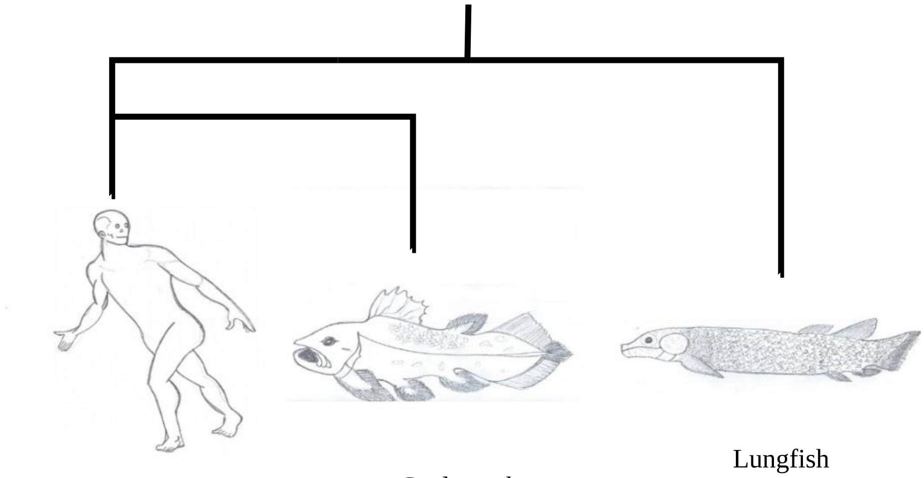

Figure 1. Coelacanths are grouped by Cladists with higher vertebrates, not other lobe-finned fishes, because of having closer ancestors. (The image is captured and redrawn from LaPorte (2004)). MacLoed (2013) draws a similar diagram to imply that crocodiles can be grouped with birds rather than lizards and snakes.

Consideration of evolutionary change in classification, unlike a pure cladist approach, recovers the predictive weakness, but this decision raises another problem usually called the 'problem of arbitrariness'. According to this problem, grouping biological clades based on evolutionary changes is arbitrary, since "decisions about how much evolutionary change is sufficient to break a clade are by no means straightforward" (LaPorte 2004, p. 80). In other words, due to the fact that there is a gradual and continuous change among organisms, there can be no objective decision on the way scientists should cut the joints. Hence, biologists sometimes recognize whole clades as taxa and sometimes parts of them are considered as a single taxon. Of course, it is unlikely that scientists would group organisms based on scientifically irrelevant criteria[1], but depending on different classification criteria and purposes organisms can be grouped together differently. Thus, not only is the choice among the procedures of classification (i.e. cladistics or evolutionary) based upon scientists' interests, the choice within the particular approach of evolutionary classification (i.e. where to break a clade) seems interest-based. Of course, each way has its benefits and costs, and these issues guide scientists to opt for the most suitable approach for *their* epistemic interests and purposes.

Given the complications that occurred in the classification of biological kinds because of different contexts or based on the variety of purposes in question, identification of the actual reference and meaning of some biological concepts goes beyond the explanatory powers of the traditional accounts — which only take referential issues into consideration. I am highlighting an additional dimension of change, which is present specifically in the life

[1] I do not mean to provide a criterion (or a set of criteria) for scientific relevance. Even a classification based on organisms' color or weight might be relevant in a specific context. Rather, it is context and scientific aims and goals that distinguish relevant from irrelevant scientific criteria.

sciences, and which was not addressed in the traditional approach. As an example, because of a similar complication over drawing taxonomic boundaries by the early twentieth century it was not definitely decided whether the giant Panda should be classified as a 'bear' or 'raccoon' or even if it should be classified in its own class (O'Brien 1987). It has some clear similarities with the paradigm bears, but so many differences (including the physical and behavioral characteristics) distinguish it from a typical bear. The concept *bear* that was already in use before the first discovery of giant panda in the 19th century could either include or exclude a further extension, depending on the decision made by cladists or evolutionary taxonomists.[1] Obviously, consideration of such interest-based elements leaves the concept with less firm borders and more vulnerable to conceptual change. Insofar as an account of conceptual change has not recognized such a possible element of change, it cannot offer any explanation for such cases.

It should be added that sometimes scientific progress opens up new theoretical approaches (e.g. scientific purposes, values, point of views, etc.) regarding the way scientists conceptualize the same observations and case studies, and that ultimately this adds to the complication of conceptual change. In the case of the Panda, a novel empirical discovery led to a dispute about how to extend existing procedures of biological taxonomists to new cases. The molecular studies in the late twentieth century strongly suggested that Panda is a bear[2]. But, surprisingly, the problem reappeared in another form. Molecular scientists drew a phylogenetic tree of all bears and by comparing the similarity of panda's genealogy tree with other bears, they figured out that the *giant panda* should be considered as a bear whereas the *red panda* is among raccoons. Yet, the genealogical drawings allow us also to have a clade of bears including giant panda and a clade of bears excluding them—just like having a clade including all bears and raccoons in the same clade. So, it remains undecided which clade is the suitable one, depending on the context of study. Sometimes even it is preferred to consider the panda in its own family, rather than comparing them with bears or raccoons. LaPorte (2004) asserts that "The giant pandas do form a natural group, and so do the bears with or without the pandas" (p. 186). Thus, it is true that sometimes scientific progress adds to the complication of biological classifications by opening up a new method or approach.

3.3. Some Final Remarks

So far, I have shown that evolution and the way biologists classify their objects provide some complicated cases of conceptual change that cannot be explained by the traditional accounts of conceptual change. Some final remarks here will help me close this discussion. It is usually admitted that the meaning of scientific concepts is, at least partly, determined by the theories that employ them. This should be the case for biological concepts too. However, biological theories or law-like statements are different from other sciences in several respects, namely

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[1] It has recently been decided, based on molecular studies of DNA, that giant pandas are bears (Lindburg, D. G. and Baragona, K. 2004).

[2] O'Brien, Stephen J. (1987) The Ancestry of the Giant Panda, *Scientific American 257* (5): p. 102-107.

that they are far more local than physical or some chemical theories. This means that the level of universality that can be seen in physical and chemical theories is usually more than the level in biology. For example, a set of universal theories in chemistry tells us the number of atoms, weight, and molecular structure of water, no matter *where* and *when* it is. However, biologists and philosophers of biology have become accustomed to deal without these straightforward (context invariant) theories and concepts. In genetics, it is argued that several theories are true only about a sub-class of genes (Weber 2014, p. 441). For example, the term 'gene' in classical genetics does not properly refer to *prokaryotes* genes (e.g. bacterial genes) because they do not properly behave according to Mendelian laws.[1] This does not mean that geneticists do not seek any universal theory applicable to all genes, but in biology in contrast to physics "no-one expects there to be a universal genetics" (ibid, p. 440). Also, some of the very important generalizations in biology are only contingently true. For example, theories of evolutionary biology usually hold of a limited population for only a few years (Brandon 1997, S455; Beatty 1995, p. 46)[2]. Thus, some biological theories and concepts have restricted domains as the result of the complexity of their entities and different perspectives toward those domains.

Evolution—at least as a demarcating factor that distinguishes living from non-living entities—has made a wide range of entities with highly complex structures and behaviours. To cope with the complexity of the way biologists discover biological truth via theories or concepts is complicated and highly interest relative. This is why biological case studies have led philosophers to rethink their questions from different perspectives. Both the epistemic goals and interests of scientists and the evolution of biological entities provide the ground for my claim that in biology sometimes explaining conceptual change goes beyond a purely referential account. This is mainly because finding the reference of some important biological terms requires us to consider all these contextual factors. Let's explain this issue with an example in genetics.

It seems a minimal demand on scientists that they have a shared definition of a central term in a particular sub-discipline. The term 'gene' in molecular genetics lacks such a straightforward definition. A well-known molecular account of genes holds that genes are 'segments of DNA that cause changes in linear sequences within different polypeptides'. Yet, this definition suffers from at least three problems: vagueness, admission of exceptions and ambiguity. It is vague since there is no clue to know where a gene begins and where it ends (e.g. are so-called coding regions or introns part of genes?). 'Gene' admits exceptions since we have many DNA segments that determine linear sequence of RNAs but do not causally contribute to determining the linear sequences in polypeptides (e.g. tRNA and rRNA pose

[1] Prokaryotes are usually unicellular units, so their genetic recombination is partial (or as Weber describes it 'irregular'), while Eukaryotic organisms, such as plants and animals, are basically multicellular and their genetic recombination is regular: sexual recombination in terms of meiosis and fusion of gametes.

[2] Beatty has a radical claim that biology lacks any law, while Brandon argues for a modest claim in which contingent evolutionary generalizations can be called 'laws'.

exceptions). Thirdly, it suffers from ambiguity, since a single DNA segment contains a number of segments that causally contribute in determining the linear sequence of different polypeptides caused by the differential splicing of the same RNA molecule (Waters 2014, p. 123-124). So, in different contexts and depending on different interests, different samples could be picked out by the term ‘gene’.

These complications in biological concepts suggest the importance of scientists’ interests and goals in determining the actual meaning and reference of a concept. Yet, it is worthwhile noting that the epistemic goals or variation of interests are formed *primarily* at the level of scientific communities, not the individual scientist. For example, two different accounts of classical and molecular genetics both are shaped by a large number of scientists over a long time. These different understandings of ‘gene’ have formed different sub-disciplines in genetics. It is only in the second place that scientists on different occasions determine the context of communication and employ a particular concept in the hope of successful scientific practice. In this stage, a mistake in using a concept in a wrong context may hamper communication.

Among the issues that make some biological case studies referentially complicated, it seems that evolution is highly significant. Evolution ultimately distinguishes the subject matter of biological sciences from physical sciences. Obviously, physical or chemical entities can be viewed from different perspectives and for different epistemic interests, so biology is not a unique field in this respect. However, evolution is unique to biological entities in ways that may ultimately make conceptual change in biology more complicated. Without this evolutionary process of change, things become more straightforward. Even some of the theoretical complications mentioned earlier, such as classification in biology, ultimately could be reduced to the ontologically complex structure of biological entities. So, it seems that ontological complexities play a crucial role in complicating conceptual analysis, reference fixing and finally explaining conceptual change in biology. Endorsing the supreme impact of such ontological implications, Weber finishes the seventh chapter of his book by writing:

> “I have explained the possibility of floating reference in terms of the nonessentialistic character of biological concepts, which allows biologists to lay down different natural classifications, depending on the investigative methods available as well as on theoretical interests. Thus, the ontology of living organisms has consequences for concept formation that have not been noted previously” (Weber 2005, p. 228).

As I have explained, evolution as an exclusive characteristic of biological entities and the relevance of epistemic issues in theorizing and conceptualizing of biology creates complications for the purely referential accounts of conceptual change. It is worthwhile clarifying my reasons with a caveat. The fact that I reviewed this complication from different perspectives (e.g. evolution and classification) does not entail that there is a clear distinction between these difference makers. In actual biological cases, there are complicated examples in which the importance of scientists’ interests and the complexity of entities due to evolution both play a role in complicating the field of study. There is no way to sharply distinguish the factors in play, because they are interconnected in the course of a specific case study. For example, one of the reasons that evolutionary taxonomy involved interest-relative issues is the very existence of gradual and continuous evolutionary change that takes place only in the biological entities. My primary aim, in this section, was to highlight the non-referential complications that biological cases of conceptual change may contain. In the next section, I discuss some complications of chemistry that might be influential in studying conceptual change, but which do not tell against my general claim in this chapter.

4- What about Chemistry?

In recent studies, some authors have argued that chemistry is as complicated as biology. Joyce Havstad (2017) argues against the idea that biological kinds are complicated to classify whereas chemistry is a more clear-cut and neat scientific area. She believes that "there is very little support for the widespread notion that biology is a messy, anti-microstructuralist junkyard whereas chemistry is a neat and tidy microstructuralist paradise". In order to show this, she raises some complicated cases of chemical compounds, such as water molecules, chemical mixtures, such as cement and concrete and complex macromolecules, such as proteins, to suggest that chemical kinds are not as tidy as they seem. She argues against the claim that the interests that govern chemical classification 'are more unified' than biology.[1] She shows that classification in both biology and chemistry is highly sensitive to epistemic factors, and then concludes that these two subjects are not 'so different after all'. In another study, discussing proteins in particular, she claims that sometimes hierarchical classification schemes or taxonomies in chemistry vary, not because of the variation of scientific projects or scientific purposes (like the differentiation that I discussed for homology); but because different classification systems track different sets of properties of the same entities (Havstad 2016).

Havstad's claims do not pose problems for my argument, and this is for two reasons. First, her main target in these papers is a naive microstructuralist view of chemistry, where "microstructuralism about a natural kind is the thesis that membership of that kind is conferred by microstructural properties" (Hendry 2006, p. 865). Havstad's case of water molecules discredits the microstructuralistic picture of 'liquid water is necessarily H_2O', for there are a number of assortments of the molecule (e.g. H_3O^+ and OH^-) that are macroscopically significant and so 'water' requires that other chemical requirements be met. To this end, her arguments entail a rejection of the idea that chemical kinds are microstructurally identified (i.e. the kinds are constituted by the structure of their molecules or atoms). Second, the main goal of Havstad's study is to correct the prevailing ignorance of

[1] It is referring to an argument by Robin Findlay Hendry in which he says "Chemistry, I argue, is different [from biology]: the interests that govern its classifications are more unified, and in most cases membership of its kinds is conferred by microstructural properties. (2006, p. 865)

the complexity of chemical classification, so the complication that she attributes to the classification of proteins can be applied to any complex chemical or biological structure. In the case of proteins, she writes "The different inferences granted by the different classification systems arise because the different classification systems track different sets of properties and capabilities of the proteins" (2016, p. 84). This entails that complex structures can be viewed and grouped together from different points of view and based on different properties. The insight is, also, applicable to complex biological entities that may have a variety of properties and one can view them from different perspectives. *The complication* that I based my argument on is of a higher level (i.e. the historicity of entities). I'm not claiming that biology is the only scientific discipline in which historicity plays a significant role (probably we can add geology, astrophysics, geography, and etc. to the list). But my point is to attract attention to biology as a discipline that involves historical entities, rather than philosophers' paradigm disciplines like physics and chemistry. Biological entities have a historical element of change (i.e. they evolve) that makes them highly complicated as regards classification and identification. So, the complications that biology makes for the traditional philosophy of science in this debate are of a higher level compared to chemistry (because the complications of chemical taxonomy are applicable to biology too).

Of course, a possible approach to chemistry and even to physics is to consider it from an evolutionary point of view. Where a class of subject matters *can* be fruitfully studied from an evolutionary perspective, I would accept that it displays no relevant difference from biology, at least as far as my argument is concerned. For example, proteins can be sorted in terms of superfamilies, families and subfamilies, based on the extent of their evolutionary relationship. This would lead to the same complication that I argued for biology so far. Yet if proteins can be studied from this historical point of view, I doubt that they are actually being studied from the perspective of chemistry. Bartol (2016) preserves a place for biochemical kinds with possible multiple approaches. According to him, proteins should be treated *historically* in biology (as evolving entities) and *microstructurally* in chemistry. I have nothing to say about the microstructuralistic stance of Bartol in chemistry, yet my point is that the borders between biology and chemistry, or even physics, can be unclear when it comes to evolution. My argument concerned the complications that biological cases of conceptual change produce for the traditional accounts of conceptual change due to the presence of extra elements of change; yet the case of 'protein' could be as complicated as 'homology' if proteins present evolutionary behavior.

Lastly, and most importantly, I would like to address possible misunderstandings of my claim in this chapter. I am not suggesting that biology is more important than physics; nor did I argue that biology can be reduced to physics or chemistry. Even the claim that biology is more complicated than physics or chemistry or that it provides more complicated conceptual change case studies is a stronger claim than I make here. My claim is far more restricted: namely, when it comes to studying conceptual change, there are cases in the life sciences that are complicated by means of incorporating extra epistemic dimensions beyond reference. The result is that these complicated case studies lead to alternative accounts of conceptual change, which take these dimensions into consideration. Also, since the extra dimensions are not addressed in the traditional approach, the novel non-referential approaches to conceptual change have already challenged them, and in this debate biological case studies play a critical role.

5- Conclusion

In this chapter, I argued that some biological case studies pose significant problems for the traditional approach to conceptual change. While the traditional approach focuses on the reference and meaning of concepts during the process of change, there exist clues to show that sometimes the epistemic goals of scientists play a role in explaining biological cases of conceptual change. The concept of homology is an example to illuminate the importance of scientists' interests and goals in determining the actual concept that is associated to a particular token of this 'homology'. If I am successful, the way is open for alternative approaches to conceptual change, distinct from the purely referential approach which has traditionally been pursued by philosophers of science. The interest-relative elements are beyond the reach of a purely referential approach, so a successful explanation of such case studies is unavailable for those traditional accounts. To fill this lacuna, alternative approaches have been introduced in the last decade.

Biology is the main source of complicated case studies for the novel approach to conceptual change. The reason is that it provides complicated case studies containing extra dimensions of change. In particular, evolution, and the associated epistemic difficulties in accounting for the biological classifications, laws and explanations are what make the biological cases challenging. Similar difficulties, but at a different level, may appear in special fields of physics or chemistry, such as in the study of macromolecules. However, I argued that my claim can be extended to these areas, if they approach their objects from an evolutionary point of view. After all, a practical reason that highlights the significance of my study is that in the last ten years the novel non-referential approach to conceptual change has emerged and already challenged the traditional accounts.

Chapter 3

Ingo Brigandt's Non-Representational Account of Conceptual Change

1- Introduction

In Chapter 1, I briefly discussed novel approaches to conceptual change in comparison with the traditional ones. In this chapter, I discuss one of the examples of the novel approach in further detail. Traditionally, the problem of conceptual change, which emerged mainly in the light of Kuhn's and Feyerabend's works in the early 1960s, focused on the reference of the concepts. In fact, both the philosophers who highlighted the problem and those who strove to bring it to a resolution implicitly or explicitly focused on the referential aspect of concepts. In other words, the concepts were mainly considered in terms of the referents they represent. The use of theories of reference as a way to solve problems such as incommensurability was in itself a reflection of this tendency. However, starting in the first years of the twenty-first century, a number of younger philosophers of science have followed a different path to propose a novel approach to the problem of conceptual change—one that focuses on non-representational and pragmatic aspects of concepts. Proposed less than fifteen years ago, this novel approach has since then presented itself as able to not only explain rational conceptual change, but also to account for the failure of the more traditional (referential) approach.

The path for the development of this novel approach was in fact paved by the inability of causal theories of reference to explain rational and progressive conceptual change.[1] Initially, causal-descriptive theories of reference were attempts to replace the inadequate response of causal theories. Meanwhile, one of the first moves beyond such traditional accounts of conceptual change was Kitcher's (1993) context-sensitive theory of concepts. His framework appeals to more than mere reference stability, identifying conceptual changes via the change of *reference potentials*. The notion of 'reference potential' involves a differentiation between *term tokens* and *term types*. A term type is associated with different *modes of reference* that can be determined by causal or descriptive ways of reference determination. Similar to the Fregean notion of 'sense', Kitcher's notion of 'modes of reference' presupposes that, although the referent is externally determined, reference determination is sensitive to "differences in cognitive content" (1993, p. 78). According to Kitcher, different tokens of a term, such as 'phlogiston', may have different ways of connecting to the world relative to different contexts of use: while one token use of the term may successfully refer, another one may fail to do so. The totality of the different ways in which a given meaning or sense can be used to pick out a reference relative to a context are called *reference potentials*.

Kitcher took the threat of incommensurability to be originally a referential problem, believing that scientists will be able to '*formulate'* (but maybe not '*resolve'*) their conceptual disagreements by appealing to his notion of reference potentials (Kitcher 1978, p. 528). However, his account started the move towards a non-representational explanation of conceptual change by appealing to some factors beyond mere reference. In fact, he takes the cognitive and context-sensitive structure of concept usage into account, as tools for solving the problem of conceptual change in terms of sameness or difference of reference. The novelty of Kitcher's response, though still referential, is the introduction of an uncertain element of reference determination that relies on the speaker's intention. Kitcher (1993) believed that the reference of the concept *phlogiston* for Priestly, for example, is not fixed independently of the context. Priestly could refer to oxygen when he mentioned a bottle of gas filled with what he called 'dephlogisticated air', or he could refer to *nothing* when he talked about 'phlogiston', which is an empty word (Kitcher 1993).

A completely non-representational approach to conceptual change was first proposed by Alan Love (2005, 2015) and Ingo Brigandt (2006b), followed by other philosophers such as Miles MacLeod (2012) and, to some extent, Hasok Chang (2011) and Kenneth Waters (2014)[2]. The basis of the non-representational approach can be traced back to philosophers of language with a strong preference for positing a non-representational aspect of concepts (Brandom

[1] Strong objections against a pure causal theory of reference were raised by Devitt and Sterelny (1999) and Kuhn (2000).

[2] Definitely, some serious studies in philosophy of biology were of importance for this novel approach. Among these works are David Hull and Michael Ruse's (eds.) (2007) *The Cambridge Companion to the Philosophy of Biology*, the works by Karola Stotz and Paul Griffiths, namely their paper *Genes: Philosophical Analyses Put to the Test* (2004) and their online project in http://www.representinggenes.org. Marcel Weber's (2005) *Philosophy of Experimental Biology* also played an important role in this discussion.

1994; Gupta 1999). Following the ideas of these philosophers, Love (2005) has been concerned with the notion of 'conceptual clusters', believing that such clusters should be characterised in terms of their usage:

> Our focus on use rather than representation […] was driven by the recognition that many features of scientific practice are missed when a symbolic, representationalist approach to scientific knowledge structures is adopted. (p. 417).

Love (2005) maintains that a crucial part of concepts is their ability to set an epistemic 'problem agenda'. Brigandt (2006b), on the other hand, is *more explicit* in using the notion of 'conceptual change' and non-representational semantics in his work: "The motivation was that using these additional, non-representational semantic features offers a richer philosophical account of successful scientific practice involving language use" (p. 381). The fact that Brigandt was one of the first philosophers who identified with this line of reasoning is the first reason why I chose him as the main guiding figure in this thesis. The second reason for choosing Brigandt is the fact that his account of conceptual change is based on biological case studies and, as I argued in Chapter 2, biological cases of conceptual changes are highly complicated. The third reason is that Brigandt has further pursued extending his first work (2006b, a doctoral dissertation) for nearly ten years (2006a; 2010a; 2011; 2012; 2013; 2015). This long-lasting project has attracted a large number of scholars to this new non-representational approach.[1] In what follows, I provide a synoptic view of Brigandt's epistemic approach to conceptual change.

According to Brigandt (2006b, p. 38-40), a good theory of conceptual change has to account for three main tasks. First, such a theory should explain conceptual change so that it portrays scientists as rational concept users. Sometimes, concepts do change internally (for example, due to a revision of their theoretical characterizations) without leading to different concepts. In other cases, concepts change in a way that a novel concept emerges out of an older one, or the original concept splits into several concepts. We need a theory to explain why conceptual change rationally does (not) take place. Whereas traditional accounts of conceptual change sometimes assume that meaning change is 'epistemically problematic' due to the lack of a stable reference, Brigandt tries to find a theory that explains the rationality of conceptual change while allowing change in both reference and meaning. This issue will be discussed in Section 3. The second task to be accomplished by a good theory of conceptual change is to explain what makes a conceptual change a case of conceptual progress—and more specifically, of scientific progress. The goal of this task is to evaluate the degree to which a conceptual change is progressive. Beyond these two conditions, there is a more basic *desideratum* for an adequate theory of conceptual change to explain the successful

[1] An example of the strong interest of philosophers of science in non-representational studies, in line with studying biological case studies, is a collaborative project between the University of Minnesota, the University of Calgary and the University of Geneva. This project is called "From Biological Practice to Scientific Metaphysics" and is sponsored by the John Templeton Foundation. Three prominent scholars on this project are Alan Love, Kenneth Waters and Marcel Weber.

development of concepts. Brigandt (2006b) maintains that concepts form "historical lineages like species" (p. 38), so a philosophical account of conceptual change needs to track the historical development of a concept. This task requires us to provide a philosophical tool (e.g., an account of concept individuation) to trace back concepts and to determine "whether a term corresponds to one or several concepts" (Brigandt 2006b, p. 38-39). Since the third task is a more fundamental metaphysical one than the first two and basically deals with a theory of the nature of concepts, rather than a theory of conceptual change, I will focus on the first two tasks and only briefly address the third one.[1]

Brigandt argues that, since the traditional theories of conceptual change focus on a purely semantic approach, particularly on reference, they are unable to successfully satisfy the above conditions. He considers Howard Sankey (1994) and Michael Devitt (1979) as two typical representatives of this approach, because of their purely semantic response to the incommensurability thesis.[2] Instead, in order to explain successful communication among scientists, an adequate account should explain "when and why scientists have an implicit *understanding* of the reference (and meaning) of their concepts, as well as the concepts used by the rival theories" (Brigandt 2006b, p. 22, original emphasis). Some traditional accounts agree with Kuhn that a literal translation between competing paradigms is often impossible, but a shared reference prevents their incomparability. However, traditional accounts miss the point that hampered communication or incomparability of sentences among scientists, posited by the incommensurability thesis, is not merely due to the non-translatability of their sentences. Along with the referential factors, these problems can also be caused by some non-representational factors, such as different epistemic goals in studying *the same entity*.

Since preserving successful communication among scientists plays a significant role in Brigandt's account of conceptual change, he puts a strong emphasis on semantically relevant components of conceptual understanding. Hence, Brigandt puts forward an account of scientific concepts (Section 2.2), appealing to three *different* elements: (i) reference; (ii) inferential role (IR); and (iii) the epistemic goal pursued by concept usage (EG). Both reference and inferential role are familiar to philosophers of language, whereas the last element, EG, is a novel component of conceptual understanding introduced by Brigandt. While reference preserves realism, and IR explains the successful communicative and epistemic practices in

[1] Although Brigandt puts forward his own account of concept possession and concept identity, he explicitly asserts that the conceptual issues he is interested in are pragmatic and so it is not important to study whether semantic variation corresponds to one or several concepts. He thinks the distinction between semantic and pragmatic aspects of concept's component is less important than the role these aspects play in explaining philosophical questions about individual cases (Brigandt 2011, p. 184; 2012, p. 93).

[2] As an example, Sankey asserts "Identity of reference of sentence components is *enough* for the truth of one to be able to preclude the truth of the other. And such conflict between the sentences of rival theories is enough for comparison with respect to particular points of disagreement" (1994, p. 40, my italics). Or he argues that "comparison of content requires *only* that expressions be related via reference, not that they have the same meaning" (ibid, p. 73, my italics). Using words like 'only' or 'enough' in these quotes suggest his purely semantic viewpoint. Dudley Shapere's works (1982; 1989), suggesting an account of rational conceptual change based on the continuity of scientific reasons, is an alternative approach.

science, the novel component (EG) is responsible for explaining the rationality and advancement involved in conceptual change (so it is very important in studying conceptual change). In this account, the benchmarks for a good theory of concepts (conceptual understanding) consist in explaining successful communication in practice and in accounting for the rationality of conceptual change. Brigandt's theory claims to meet these two benchmarks.

2- Brigandt's Account of Conceptual Change

2.1. Finding a Rationale

Traditional accounts of conceptual change emerged mainly in response to Kuhn's (1962) and Feyerabend's (1962) thesis of incommensurability. According to the incommensurability thesis, a term may refer to two substantially different things within the conceptual framework (paradigm) of two different theories. Therefore, rational communication among scientists and rational choice among rival theories may be hampered because these theories deal with different subject matters (i.e. reference). Most responses to the problem of conceptual change have focused on the (stability of) reference of terms used in different theories (i.e., the referential approach). This response was first propounded by Israel Scheffler's (1982) referential approach to conceptual change: "for the purpose of mathematics and science, it is the sameness of reference that is of interest" (p. 57). To some extent, both causal and causal-descriptive theories of reference claim that, although it might be possible for a term to be used with different meanings in different theories, the stability of reference across these theories enables raising competing claims about the same referent. A comparison among competing theories is said to be possible because they refer to the same referent (see Putnam 1973 for a causal theory of reference and Sankey 1994 for a causal-descriptive theory).

A non-representational approach to conceptual change, like that of Ingo Brigandt, aims at responding to the problem of incommensurability and, more specifically, at explaining the rationality of conceptual change by proposing a theory of conceptual understanding (i.e., a theory of the semantically relevant components of concepts). This strategy can be qualified as a *non-representational approach,* in that conceptual change should be studied focusing on other, (non-) *truth-conditional aspects* of concepts.[1] This strategy puts us in a position to solve the problems of conceptual change not only by explaining reference stability, but also by providing the theoretical resources to explain the rationality of cases where reference has changed. *This means that conceptual change may be seen as rational in virtue of an 'overlap in conceptual understanding',[2] even when this overlap does not include reference*. In contrast, the focus of the traditional accounts is on referential stability (including the stability a reference-fixing sense or simply of reference). However, since referential stability is only one

[1] Semantic properties such as [intension and] extension of concepts are truth conditional, for they yield truth-values in Tarski's theory of truth.
[2] As Brigandt believes that the semantic content of concepts is determined by three different elements (reference, IR and EG), an overlap in any of these components would guarantee the rational change.

of the elements involved in the issue on Brigandt's account, he concludes that it "does not solve Kuhn's original challenge" (2010a, p. 21).

In addition to the possibility of reference change, this novel approach allows for the possibility of meaning change. This is an advantage when compared with those philosophers of language who rule out the possibility of meaning change without 'changing the topic'[1] (Chalmers 1996; Chalmers and Jackson 2001). There should be a place for the empirical progress of science to modify the meaning of some central concepts and still view the change as rational. The novel approach admits that scientific progress would change the reference or meaning of some key scientific concepts without triggering 'epistemically problematic' communication among scientists. This implies that rationality of scientific change is not necessarily linked to meaning or reference stability. Brigandt (2006b) asserts:

> While traditional accounts within and beyond the philosophy of science have assumed that meaning change is epistemically problematic, the present discussion attempted to work towards an account of concepts according to which conceptual change in science is rational. (p. 382)

Brigandt (2006a) puts forward the idea that regarding scientists as rational (and referring to the same subject and pursuing the same question) does not inevitably require them to refer to the same entity or share the same meaning when using a specific term. Instead, as long as scientists use scientific terms "for the same epistemic purposes" (Brigandt 2006b, p. 389), they are rational in using terms with different referents. It is the stability of purposes and interests attached to using a concept that prevents users from talking past each other. Therefore, change of reference or any change in the inferential roles of a concept (including its definition) is rational (or "epistemically warranted") if the "epistemic goal is met to a larger extent than hitherto" (Brigandt 2010a, p. 24; 2006b, p. 5). Furthermore, the change is rational and comparison between the rival accounts is possible as long as scientists are justified in using a concept with novel meanings or reference in a scientific conversation and can produce inferences and explanations relevant to previous discussions. Indeed, it is argued that such concept change is a necessary part of scientific progress, as empirical investigation of theoretical concepts reveals cases where scientists have to repair their conceptual understandings by changing their meaning or even reference; yet, these changes are rational in virtue of having some *overlap*.

That is, the solution proposed by Brigandt is to introduce a framework for characterizing different semantically relevant aspects of concepts and then to explain the rationality of conceptual change by appealing to some overlap in the semantic properties of concepts. The

[1] Chalmers and Jackson (2001) discuss the concept of *consciousness.* On some accounts, no physical characteristics should be involved in analysing the concept. In that case, a revised understanding of this concept that involves physical properties would count as a 'change of topic' – i.e., it would be the introduction of a new, distinct concept (Chalmers and Jackson 2001, p. 349-350). Brigandt (2013) criticizes this approach based on its limitations in explaining conceptual change in some biological case studies. Brigandt's account of concept identity will be briefly discussed in Chapter 4.

rationale for this proposal is that conceptual change is a change "in the term's semantic properties/the mental representation's content" (Brigandt 2010a, p. 21), where concepts are contentful mental representations associated with terms. In short, Brigandt is distinguishing different semantically relevant properties of the mental representations associated with particular terms in a public language in order to put us in a position to determine whether particular changes in those semantic properties (i.e., conceptual change) are rational.

2.2. A Theory of Concepts

As mentioned earlier, Brigandt's (2006b) framework of concepts distinguishes three different semantic properties of a concept: (i) reference; (ii) inferential role (IR) and (iii) epistemic goal pursued with concept usage (EG). Before explaining the aims and motivations for having each component, let us briefly explain what these components are and where they come from. Reference is the most basic property of a concept, which assures us that the language of science is reflecting the reality by means of a representational attachment of the concept to something in the external world. Causal interactions with the world make such a relation possible. Generally, scientific terms are introduced to pick out a set of entities, kinds or natural phenomena (e.g., 'oxygen', 'gold' and 'earthquake').

Inferential role, the second feature of concepts, is the first point that distinguishes Brigandt's framework from pure causal theories. The assumption that inferential roles (IRs) of concepts are semantically relevant features of concepts distinguishes Brigandt's theory from a direct reference theory where the semantic content of concepts is nothing but their reference (e.g., atomistic semantics by Fodor 1990, 1998; Soames 2002; Salmon 1986). Non-causal approaches to concepts hold that having a concept is to possess certain sorts of cognitive abilities to recognize the referent or draw inferences about it, or to have standing beliefs[1] about it. Roughly speaking, for Brigandt (e.g., 2010b), IR stands for what is called 'meaning', 'intension' or what Kitcher calls 'reference potential'. More generally, IR resembles the Fregean notion of 'sense' in that it distinguishes co-referential concepts by *the way a referent is represented*. IR is supposed to be any aspect of substantive understanding of the reference—Including explicit beliefs, inferential dispositions, recognitional dispositions, action dispositions, beliefs about necessary and sufficient conditions for being X, beliefs about necessary properties of X, and so forth. Therefore, IR is not limited to a set of necessary and sufficient conditions that define a scientific term.[2] For Brigandt, IR incorporates all sorts of explanations, inferences, and the conditions of application that a concept supports.

A general gloss on IR is Conceptual Role Semantics (CRS). CRS maintains that "the meanings of expressions of a language (or other symbol system) or the contents of mental states are

[1] 'Standing beliefs', as captured by Clark and Chalmers (1998), are any beliefs that one may have but that are not *occurrent* at present. For example, the idea that dinosaurs once roamed the earth could have been the reader's standing belief before reading this footnote. After reading this footnote, this belief becomes occurrent.
[2] For some philosophers like Carnap (1956), it is very tempting to define theoretical terms of science by a set of necessary and sufficient conditions.

determined or explained by the role of the expressions or mental states in thinking" (Greenberg & Harman 2005, p. 295). Having a concept is to possess certain abilities or understandings in a way to allow us to use it. This approach is based on the notion of meaning-is-use in that understanding one's concepts consists in knowing how-to-use the associated symbols and being at ease with using them. Some philosophers, such as Ned Block (1998), called the project 'Conceptual Role Semantics' (CRS) or 'Inferential Role Semantics' (IRS)[1]. Brigandt (2012) explicitly admits that his "approach is consistent with the common idea that 'meaning is use'" (p. 79). For him, IR is constituted by the dispositions used in reasoning, so invoking particular aspects of IR will explain how the subject reasons in particular cases. Particularly in science, since the IR of a concept contains inferences, explanations, and certain experiments supported by the concept, being able to use a concept in specific situations is a necessary part of possessing that concept (i.e., being able to communicate with other scientists or conduct experiments using a concept is necessary to count as competent in determining the meaning of the concept). Brigandt's framework sides with the non-causal theories of semantic content, in opposition to purely causal theories, in taking IR to be aspects of the subject's understanding of the meaning (which Brigandt takes to be implicit or explicit 'beliefs about the term's referent'), and he holds that this understanding is "meaning-constitutive" and "determine[s] the reference" (Brigandt 2010a, p. 22).

Brigandt emphasizes that IR is a non-representational notion, as this aspect of concepts is invoked to fulfil an epistemic task—that of supporting successful inferences and explanations—rather than merely representing a referent and settling truth-conditions. Brigandt also argues that IR is not attached to concepts as used by a single scientist or in a certain time: rather, it is the pattern of conceptual understanding associated with a particular term in a scientific community over time. For example, the IR associated with the public language expression 'species' (i.e., its meaning), including its definition, did not change immediately after a small number of scientists understood (or accepted) Darwinian evolution theory. Instead, the new understanding of 'species' gradually grew as the community of scientists came to accept Darwin's theory. Said differently, due to the division of linguistic labour, the inferences and explanations involving the term 'species' gradually constituted the IR of the corresponding concepts of competent speakers. Eventually, it is the whole community's usage across time that determines whether an implicit or explicit revision in the dominant IR associated with the term is accepted or rejected.

There is another difference between Brigandt's account of IR and the classical accounts. While standard Inferential Role Semantics (IRS) holds that inferential roles are intra-linguistic relations (narrow content in the context of philosophy of mind), Brigandt (2006a) construes IR as a mind-world relation where empirical information participates in IR determination (p. 13). Based on this interpretation, he introduces the notion of 'material inference' where the

[1] Fregean CRS as a particular form of IR maintains a similar stance in that grasping the sense is a requirement of having a concept. Brigandt's framework is similar to CRS in that meaning is not fully determined by means of reference.

logical form of an inference is less important in its justification than the empirical information (content) associated with it[1]: "A material inference is licensed by the empirical content embodied in the concepts" (Brigandt 2010b, p. 31). Hence, his account is similar to what Harman (1987) calls a 'long-armed' inferential role semantics in that conceptual roles reach out into the world. Therefore, acquiring novel *empirical* beliefs in the course of scientific practice (i.e., a process that involves external relations to the features of the world) may help constitute an IR; similarly, these beliefs are more significant in revising a scientific concept's meaning. The empirical content of an induction is grounded in the empirical information embodied in the concepts contained in the premises and conclusion. Thus, scientific induction is basically contingent upon empirical considerations regarding the meaning of its concepts. Based on this idea, the IR of scientific concepts should sometimes be revised in the light of different empirical properties in order to prevent illicit inferences.

For example, Paul Griffiths (2002) provides an argument that the concept of innateness conflates the following three independent biological properties: (i) having a trait that has an adaptive history; (ii) having a universal trait within a species; and (iii) having a trait insensitive to environmental influences in development. All these empirical properties are scientifically valid and support the relevant set of inferences in a field of study; however, the diversity of accounts creates confusion that leads to illicit inferences. For instance, Griffiths (2002) mentions a dispute in the field of animal behaviour studies between Konrad Lorenz and other early ethologists, on the one hand, and developmental psycho-biologists (such as Daniel Lehrman), on the other hand. Griffiths argues that this dispute was caused by illicit inferences based on a misuse of the concept of innateness. Hence, by virtue of carrying crucial empirical knowledge (IR) that directly affects its content, concepts employed in an inference sometimes determine the validity of that inference.

The epistemic goals (EGs) pursued with concept's use, the third component in Brigandt's account, concern the novelty of his epistemology-oriented theory of concepts. It is clear that each scientific field pursues a set of epistemic goals that organizes the practices of that field. The aims and values of each field support explanations, discoveries and inferences that occur in the course of that scientific field's practice. What distinguishes EGs from IR is the fact that, while IR involves commitments *about the referent*, EGs involve commitments about *scientific practice*, or "a desire as to what a scientific community should achieve" (Brigandt 2012, p. 97). Furthermore, while IR includes inferences and explanations about the reference using a concept, EGs include *the reasons or the aims* of those inferences and explanations.[2] Said

[1] Brigandt gets the notion from John Norton (2003) and Wilfrid Sellars (1953) who take the justification of induction in virtue of its content. However, there are two differences between Norton's (2003) and Brigandt's accounts. First, while Norton (2003) takes induction as a material inference, Brigandt extends the notion to all scientific reasoning including scientific explanation and discovery. Second, while Norton (2003) takes content as the only determinant of the validity of an induction, Brigandt (2010b) maintains that, although content is much more important in determining the validity of an inference, the form itself plays some role.

[2] Sometimes Brigandt describes these aims as 'epistemic products' or 'kinds of inferences and explanations'. Therefore, concepts are intended to deliver particular epistemic products (2006b, p. 4, p. 50).

differently, only certain kinds of inferences, explanations and practical investigations are supported by a concept, and these kinds are dictated by EGs. The key distinction between IR and EG is object-level commitments about X itself and meta-level commitments about the importance of different uses of the concept/term ‘X’. In some cases, the purpose pursued by using a concept is more crucial than its reference or the theoretical frameworks it is inferentially connected to. A term can occur in two different scientific fields with two different EGs, but with the same set of inferential roles and the same reference, in which the EG determines the ‘central function’ of that term relative to the different fields. More specifically, the EG sets the standards for rational changes in reference or inferential roles. A referential change appears to be rational as long as it satisfies the EG to a higher degree compared to the previous account.

Next, although EG is more often assigned to a scientific field, it is common to assign EG to some central concepts in science. For instance, the concept of ‘gene’ in the classical period was designed largely to predict the patterns of inheritance. Similarly, using the concept of ‘natural selection’ is often required to explain evolutionary adaptation, and so on. Given that EG is more basic and usually more stable than other components, it can serve as a ground to explain conceptual progress and the rationality of conceptual change.

As an example, the term ‘homology’ in contemporary biology is used in at least two different ways with different epistemic aims and characterizations. The first view, sometimes called the *historical* or *phylogenetic* viewpoint, recognizes homologous structures with the shared features of monophyletic groups through cladistics analysis or tests (Rieppel 1992). The aim of the phylogenetic viewpoint is to classify the most closely related species sharing a common ancestor. The second view, also called the *biological* or *developmental* viewpoint, considers the role of development in evolution instead of phylogenetic traits. The developmental viewpoint recognizes its case studies by shared developmental constraints (Wagner 1999). This scientific practice reflects the EG pursued by using ‘homology’ – that is, finding causal-mechanistic processes governing biological homologues. Therefore, there are two concepts expressed by the term ‘homology’, each used in a particular field of scientific research, but with the same reference and, to some extent, similar IR. The difference between the two forms of ‘homology’ lies in their different EGs (see Chapter 4 for further discussion on concept identity).

A final point deserving of mention here with regard to EG is the fact that, since EG is determined by the relevant community of scientists (rather than by an individual scientist) and in a particular period of time, it is not quite explicit. Although scientists could explicitly confirm whether or not a proposed EG is within their goals, they would not explicitly talk about the ultimate function of their concepts. It is beyond scientists’ routine to detect the specific goal of a concept or discipline. Instead, philosophy of science or science studies is responsible for establishing which EG is motivating scientists. Accordingly, Brigandt writes:

> [P]hilosophers can articulate this concept's epistemic goal to make the operation of scientific practice intelligible and possibly contribute to science by making the relevant scientists more aware of and reflective about the function of their concepts. (Brigandt 2012, p. 83)
>
> I do not require that the epistemic goal is a belief explicitly held by these scientists. Instead, the epistemic goal is constituted implicitly by how a scientific community uses a term. (2011, p. 183)

An important feature that distinguishes this framework from alternative theories of concepts is what I call the '*independence claim*'. According to this claim, the three semantically relevant components of concepts (reference, IR and EG) are distinct, important and mutually independent 'dimensions' of concepts (Brigandt 2006a, p. 4, p. 50; 2006b, p. 4; 2010a, p. 19; 2012, p. 98; 2013, p. 86). However, this does not entail that there is no connection between the components of concepts—rather, they are linked to each other to constitute the total semantic content of a concept. As becomes clear from the discussion above, the most controversial and novel part in Brigandt's framework of concepts lies in the third '*genuine*' and '*independent*' component (EG) (Brigandt 2006a, b; 2010a; 2012; 2013).

It might be easy for philosophers of language to accept that some epistemic or practical factors play a role in determining the content of a concept. For example, Fodor (1998) would agree that concept (content) acquisition *causally* depends on these factors, but it is not *metaphysically* content constitutive. Brigandt's account is sophisticated in this respect. On the one hand, his *independence claim* preserves an independent place for epistemic goals besides reference and inferential roles. Furthermore, Brigandt repeatedly calls his theory a 'theory of concepts' or describes EG as a 'component of content'. On the other hand, Brigandt (2006b, p. 26; 2012, p. 79) admits that his semantic account should be taken not as a metaphysical theory of concepts, but as a 'heuristic tool' to solve a specific philosophical question or as a 'methodological guideline' to analyse conceptual change.[1] Thus, he thinks that EG is an important semantic aspect of explaining conceptual change, but he is less than fully committal about whether it plays a role in a metaphysical theory of the nature of concepts.

To summarize, Brigandt's framework of concepts distinguishes three features of conceptual content. While reference is the most basic semantic aspect of concepts (since it settles truth conditions), inferential roles that a concept plays in scientific inference, explanation and justification are an important (second) aspect of concepts which helps explain successful communication and concept use. The third semantic aspect of concepts—namely, EGs that consider epistemological commitments—is Brigandt's novel contribution. He claims that EG can be used to distinguish similar concepts with identical reference and IR involved in two

[1] More interestingly, Brigandt (2006b) asserts that "[the claim is not that my semantic account must *always* be adopted as a theory of concepts; rather the subsequent case studies attempt to display the advantages of adopting it for the present purposes" (p. 60, emphasis added). This quote suggests his belief in a *conditional* theory of concepts, which fits with his overall pragmatic approach.

closely related fields of scientific research. EGs are also key to explaining the rationality of conceptual change in certain problematic cases in the history of science.

3- The Application of the Three-Fold Theory of Concepts

As discussed in Section 2, Brigandt proposed a three-fold theory of the semantic properties of concepts to explain rational conceptual change. In this section, I discuss the purpose of each component and the reason(s) why Brigandt thinks that his theory, unlike traditional referential accounts, can resolve the problems of conceptual change. As I discussed earlier, for Brigandt, a good theory of conceptual change should be capable of addressing the following three different issues: (i) explaining rationality of conceptual change; (ii) preparing a platform for the evaluation of the degree of conceptual/scientific *progress*; and (iii) explaining a successful scientific *practice* by identifying different concepts contained in a scientific field.

Before addressing these issues, it is important to mention that Brigandt has a pragmatic perspective on all conceptual components. For him, the ultimate goal of any theory in philosophy of science is to explain the way scientists successfully interact with the world and communicate among themselves. Therefore, in his theory, reference is sought to provide successful "interaction with the world", inferential roles are considered to make successful "verbal behaviour" and, more importantly, epistemic goals are added to philosophically account for the "rationality of semantic change and meaning variations" (Brigandt 2011, p. 177).[1] Brigandt's objection against the traditional accounts of conceptual change is that they cannot explain successful communication among scientists without "epistemically problematic" issues arising between them (2006b, p. 382). Therefore, in his account, his aim is to explain how concepts successfully support scientific practice, and how such an account should be defended on the basis of its 'philosophical fruitfulness'. Brigandt (2011) writes:

> I will defend my account of what a 'concept' is in terms of its fruitfulness for explaining phenomena of interest to philosophers. My [account] aligns with a pragmatist perspective, since science is to a large extent a pragmatic enterprise (p. 172).

In what follows, I will briefly describe how successful scientific practice would be explained by Brigandt's account (see also Section 3.2 for a discussion of his account of concept individuation). According to Brigandt (2010a), a purely causal theory of concepts (e.g., semantic atomism) cannot provide an acceptable framework for explaining scientific practice,

[1] In Brigandt's terminology, which I follow here, there are three distinct but relevant notions*: conceptual change*, *semantic variation* and *meaning change*. Conceptual change is the most general notion that can occur with or without meaning change. Thus, we have cases where conceptual change takes place without meaning change (e.g., introduction of a new concept) and cases of meaning change that does not involve conceptual change (e.g., the concept of 'homology' before and after the evolutionary theory). Also, semantic variation refers to cases where one original concept splits into more concepts in different but relevant fields (e.g., the concepts of 'homology' in developmental and phylogenetic accounts). Semantic variation is a case of conceptual change, but it does not necessarily involve meaning change.

since the "linguistic behaviour of agents" using a concept contributes to determining the actual reference and to supporting scientific practices of inference, explanation and justification (p. 22). The reason for this claim is that, for Brigandt (2006b), "a difference in the [linguistic] behaviour of two persons is to be explained by ascribing different contents to them" (p. 61). Definitely, IR is the relevant feature of a concept that supports scientific theorizing and practice. Sometimes, it is the IR (broadly construed to include material inferences) that differentiates two similar concepts within scientific disciplines. Some examples discussed below will drive this point home.

As briefly discussed in Chapter 2, classical geneticists implicitly assumed a one-one relation between genes and phenotypic traits. This controversial assumption led to an empirical misconception conflating genotypes and phenotypes. Based on this historical fact, Weber (2005) argues that in some cases it is impossible to assign one referent to the classical geneticists' concept. Weber (2005) suggests that "geneticists were not tracking a single natural kind of entities that they called 'genes'" (p. 224). Also, regarding terms such as 'unit-character' (early versions of what today we call 'gene'), Weber (2005) suggests that this term is non-referential, since it is impossible to assign a real referent to this term. However, how can we explain the partially successful practice of biologists with this unstable (empty) reference? Classical geneticists' successful conceptual and scientific practice invokes something more than reference or a purely external causal relationship with the referent. Brigandt believes that the way this concept figures in the *use* (i.e., inferences and explanations) during that period supports successful practice.

The emphasis on *use* in IR suggests that adding some theoretical characterizations to the definition of a term cannot completely fill the gap of a purely referential account. This is so because, in some cases, competency in using the concept plays a significant role. The case of 'homology' illustrates this point. As discussed previously, the concept of 'homology' is not one of those *theoretical* scientific concepts that might substantially be determined by definition, nor by reference itself; instead, the successful practice with this concept requires a great deal of knowledge about how to apply it to homologous structures (criteria of homology) and how the practice is organized around well-established instances of homology (identified by morphological or taxonomic practice). Actually, one of the most important features of using the concept of 'homology' is to identify a pair of structures in different organisms and call them homologues. However, such a task cannot be explained by appealing to reference alone (since the reference might be the same; and it is the homology criteria that determine whether these two samples are considered homologous or not). Hence, determining the content of the concept itself requires the set of *homology criteria*. For example, the way biologists recognize homologue structures (via specific homology criteria, such as appealing to positional or embryological analogies) do bring about different understandings of the concept itself.[1]

Post-Darwinian scientists continued to use a concept of 'homology' in ways that were quite similar to those previously used by pre-Darwinians. In the 19th century, the evolutionary theory could not bring about a new concept of homology, simply because this theory could not propose a novel homology criterion.[2] Although scientists around the 1960s were able to finally introduce a genuine criterion of homology based on evolution, early post-Darwinian biologists did not establish homologies based on phylogenetic trees. To be more precise, both pre- and post-Darwinian concepts underwrote similar inductive inferences from the traits of one species to the corresponding trait in other species, although the post-Darwinians changed their definition to include having the same ancestors. As a result, the concept as used in both periods supported morphological descriptions and comparison of a group of organisms and classification of higher taxa species. It is true that, for post-Darwinian biologists, the advent of phylogenetic trees enjoyed the support of evolutionary theory; however, "this [change] proceeded simply by reinterpreting previously known' relations in a common-ancestry orientation. […] No genuinely new evidence was needed to arrive at phylogenetic trees" (Brigandt 2006b, p. 390). In other words, due to the stability of IR, which plays a role in the constitution of the concept's content, the concept of 'homology' did not change after the evolution theory.

Thus, the continuity of concept usage could explain successful scientific practice across a major scientific shift. Although adding IR as a relevant aspect of the content of concepts would explain some cases of conceptual continuity, we still need a general account of rational and progressive conceptual change. As discussed in Chapter 2, there are biological case studies where both IR and reference are changing simultaneously (e.g., the history of concepts such as 'atom' or 'oxygen'). However, by the same token, it can hardly be said that these concepts are incommensurable. Despite noticing some differences in their reference and IR, the scientists in our examples were able to successfully communicate with each other. Therefore, to account for this 'success', we need an explanation to elucidate the continuity of conceptual progress and the rationality of change by means of another element to fix the standards of those changes in IR and reference.

4- Epistemic Goals, Rationality of Change and Conceptual Progress

As discussed above, the aims of Brigandt's framework include both covering the rationality of conceptual change and setting a platform to evaluate scientific progress by adding a novel

[1] A similar problem can be found in the concept of 'evolutionary novelty' in which proposing a definition is quite irrelevant and what is important is how to use this concept in the context (Brigandt 2012).

[2] Of note, the stability of IR is not the only reason that makes pre- and post-Darwinian concept of 'homology' the same. For Brigandt, it is rather the stability of EG that, in accordance with the stability of IR, did the job.

component (namely, EG) to the content of concepts. The epistemic goals and values (EGs) pursued by using a concept, which are assigned by a wider community of concept users within a scientific field, are the ultimate conditions that set the standards for rational change. Brigandt (2006b, p. 402) explicitly argues that, in his framework, EGs are the most important component, while reference is the least important one (see Figure 1). Moreover, EGs are the most non-representational component of concepts that is not directly *about* the referent; instead it reflects the epistemic abilities a concept provides for the whole community of scientists within a specific field. Since the three components jointly determine the content of concepts, scientists act rationally when they change the IR and reference of their concepts, provided that there is an ultimate stable component within the concepts.

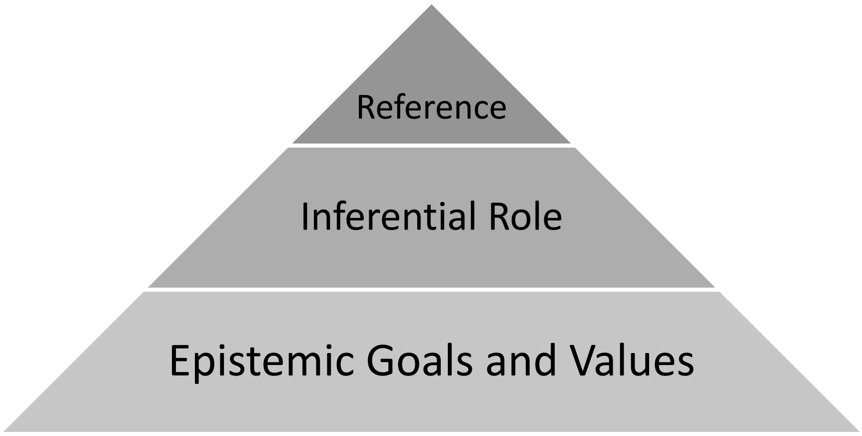

Figure 1: The hierarchy of the components of concepts (based on importance)

According to this three-fold conceptual framework, conceptual change is any change occurring in one of the three components of a concept. Each of these components can change independently or along with others, which would cause a semantic change or semantic variation. Therefore, there must be a stable component in each change to support the revision, namely in terms of setting the standards for rational change of other elements. According to this framework, it was rational for later post-Darwinian biologists to use the revised concept of 'homology' despite its reference and meaning change, since it permitted them to improve the level of satisfaction of their fixed EG—namely, morphological comparison and taxonomic classification. As suggested by the pyramid in Figure 1, the lower layer (EG) is the most stable feature of concepts; thus, it can be held that, during several cases of conceptual change, it was the stability of EG that preserved the rationality of conceptual change. With this in mind, it is very unlikely that problems such as incommensurability could emerge, as this would be the case only in the event of a simultaneous change of all three components, without the satisfaction of at least one of the following two conditions: (i) gradual change; (ii) awareness of the change by relevant scientists (Brigandt 2012, p. 89).

Many of the historical cases of conceptual change in science involved changes in reference or meaning (generally inferential role); however, as long as EG remained stable, the rationality of change was secured. It is possible to have a change in all three parts of a concept in a *gradual* fashion so that the rationality of change remains secure. For example, the molecular concept of 'gene' grew out of its classical (Mendelian) concept. These two concepts (the classical and the molecular) differed in all three of the semantic properties, yet the change occurred *gradually*, so each change occurred while other components remained stable. In this case, when the functional and molecular structure of genes was discovered in the mid-1940s and 1950s, one conceptual aspect was shared by both classical and molecular accounts: the idea that genes are units of physiological/phenotypic *function* (Brigandt 2006b, p. 28). Thus, the change in the inferential role and reference of 'gene' was rational despite a gradual change in the epistemic goals, as, in a stepwise fashion, the change in the inferential role was rational with respect to the immediately preceding epistemic goal.

More specifically, if a concept is used in different time slices t_0, t_1, t_2, … t_n, where t_0 and t_n significantly differ in all aspects of content, there is a stable component in each move that secures the rationality of that change (Brigandt 2010a, p. 30). Let us assume that this move constitutes scientific progress: in the move from t_0 to t_1, there might be a change of reference and IR; yet, due to a stable EG, it is counted as a progressive and rational move. However, in progression from t_1 to t_2, we may have a partial change of EG and other changes. The gradual and partial modification of EG may occur in other steps to t_n, rendering EG_n completely different from EG_1. Although EG might change from t_{n-1} to t_n, the stable components of reference and IR might preserve rationality and scientific progress in the use of the concept. This abstract representation of the process depicts a possible way whereby two versions of a concept may differ in all three components, but in a progressive and rational way.

This latter point is also suggested by Shapere (2001) and Nersessian (2008) in a broader way.[1] The idea is that when a semantic change is seen in a one-to-one comparison, concepts may appear as incommensurable "in some extreme sense" (Shapere 2001, p. 199). However, "the real issue there is not one of side-by-side comparison, but of *tracing the reasons* for adaption of successive alteration" (Ibid., emphasis added). Brigandt's framework is more specific, in that it places more emphasis on *tracing EG,* rather than on the broader notion of 'reason'. Yet, Brigandt and Shapere would agree on the idea that scientific changes should be investigated in a 'piecemeal' or 'gradual' fashion, rather than compared at the endpoints. Similarly, Nersessian (2008) asserts that the incommensurability issue is a product of a misunderstanding of science where scientific change is studied at "the endpoints of a long process, and [does] not take into account the fine structure in between" (p. X).

However, Brigandt's third component, EG, performs another function—namely, that of meeting the second *desideratum* of a good theory of conceptual change. When it comes to conceptual progress, it is commonly argued that the following two basic conditions should be satisfied: (i) the sufficient condition is the reduction of the previous concept to the next one (Kemeny & Oppenheim 1956; Nagel 1949); and (ii) the necessary condition is the presence of

[1] Shapere (2001) uses U_1 to U_K to show the gradual (or, in his terminology, 'piecemeal') investigation of scientific change.

a stable reference. As I discussed in the case of the term 'gene', there is no reduction from the classical account to the molecular one, as, among other reasons, the classical account is still in use. In addition, I showed that it is quite difficult to find a specific reference for the concept of 'gene' in the course of its conceptual change. Likewise, Burian et al. (1996) and Weber (2005) explicitly argue that the reference of 'gene' has dramatically changed. To account for such cases, Weber (2005) introduced the notion of 'floating reference'. Yet, it is clear that 'gene' has been subject to progress throughout the last century.

In line with Brigandt's theory of conceptual change, it is quite possible to explain conceptual progress even in the face of both reference change and IR change. In the light of new empirical knowledge afforded by new discoveries and experiments, the IR that was responsible for supporting scientific practice is modified. For example, the definition of the concept of 'atom' (a theoretical term) has continuously changed from a century ago. Meanwhile, these modifications tend to satisfy EG in as complete and detailed a manner as possible. There is a constant progress of science in terms of meeting the most ultimate component of concepts. This progress starts from the moment that a concept's IR is not able to meet EG, which is the case of misconception (e.g, classical genealogy before distinguishing phenotypic and genotypic), to the time when IR is devised to meet EG to some extent (e.g., classical genealogy after developing a genotype-phenotype distinction), and to the time when meeting EG is at its maximum degree so far (e.g., contemporary classic genealogy).

Within Brigandt's framework of concepts, one way to evaluate scientific progress is by comparing the level of satisfaction of the EG. According to this account, progress is "increase in success" in terms of meeting epistemic goals and values (Brigandt 2006b, p. 402). Also, progress is made if the EG itself becomes extended or more precise. For example, in the concept of 'gene', the goal to predict the pattern of inheritance is not given up in contemporary genetics—instead, molecular research is added to the relevant EG. Therefore, contemporary genetics incorporates at least two versions of a concept that grew out of a simple EG. Furthermore, progress can also be made when the EG itself is modified, while a stable reference or IR preserves the rationality of change. This kind of progress is more revolutionary and less regular, as EG should be able to support a novel IR that has appeared in the light of new empirical knowledge. In what follows, I discuss how this framework can be applied to some scientific case studies.

5- Two Rational and Progressive Case Studies

In this section, I will attempt to show the significance of Brigandt's theory of concepts in explaining two biological cases of conceptual change and semantic variation.[1] Rather than discussing the scientific details or the historical evidence of the concepts, I will focus on the

[1] See footnote 2 (p. 41) for the explanation of the difference between these notions.

philosophical application of the rational and progressive conceptual change. The case studies in question are the concepts of 'gene' and 'homology'.

The philosophical relevance of the concept of 'gene' derives from the fact that it is a central concept in biology referring to a set of entities without a fixed set of properties or a commonly accepted definition[1]. It is also widely suggested that 'gene' is a natural kind term, although this claim has sparked several heated debates in the philosophy of science[2]. However, the philosophical relevance of this term to the present thesis is that it has undergone ongoing conceptual change and semantic variation. Emphasising this point, Falk in the title of his paper (2000) describes 'gene' as a "concept in tension". The term 'gene' is interesting for philosophers not only because it has experienced so many reference changes, but also because several distinct concepts have been associated with it.

Brigandt hypothesized the following three different stages traversed by the concept(s) of 'gene': (i) classical (up to 1920); (ii) transitional (1930-1940); and (iii) molecular (1960-1970). Although the transitional period has not been explicitly recognized as a genuine period in the relevant literature (e.g., Kitcher 1982; Griffiths & Stotz 2007), it has played a crucial role in the account of conceptual change. Given that the molecular account of 'gene' evolved from the classical account, it becomes necessary to assume a transitional period when a stable ground connected the two stages. This stable ground was formed out of the three components of the concept (reference, IR and EG). One stable component of conceptual content in each period secured the continuity of conceptual change and grounded its rationality. That EG played the role of the stable component is far likelier than the other components, since, as discussed previously, it is the most fundamental feature of the concept.

When it comes to the classical stage, the epistemic goal (EG_1) was 'the prediction of the Mendelian patterns of inheritance'. While there was no clear distinction among phenotypic and genotypic traits, it was difficult to propose a clear explanation of inheritance based on genotypic properties. In *Elemente der Exakten Erblichkeitslehre* (1909), Danish botanist Wilhelm Johannsen (1857-1927) set apart phenotypic traits from genotypic ones. This distinction made it plain that a particular resemblance of an ancestor and its descendant depends on corresponding identities in the genotypic constitution. However, a clear understanding of the nature of genes, except for the idea that genes are responsible for the

[1] Indeed, giving a fixed intrinsic property of being gene is very difficult. Finta and Zaphiropoulos (2001) argue that genes are not well-defined and clear regions of DNA, but "statistical peaks within a genome-wide pattern of expression of the genetic information" (p. 160, as quoted in Brigandt 2006b, p. 364). Similarly, Gilbert and Sarkar (2000) claim that the function of genes is essentially context-sensitive (Both quoted from Brigandt 2006b, p. 364)

[2] It is better to say that 'gene' is a natural kind term that can designate at least some natural kinds. In Chapter 2, I mentioned that, depending on the context and meaning of a specific use, the concept can pick out different extensions. For example, Weber (2005) suggests that "geneticists were not tracking a single natural kind of entities that they called 'genes'. Instead, different historic versions of the concept referred to different natural kinds, which were not coextensive" (p. 224). A similar line of argument can be found in Brigandt (2006b, p. 362) and Griffith and Stotz (2007).

similarity of observable traits, was still lacking.[1] Yet, once the distinction was made, the inferential role (IR_1) was formed including the distinction, making room for the satisfaction of the concept's EG_1. Consequently, the IR_1 of 'gene' included, but was not restricted to, a picture of genes *as units of mutation, recombination and function*. These characterizations were meant to support relevant inferences, explanations and conduct of experiments related to the prediction of patterns of inheritance.

In the transitional period, although the concept of 'gene' was construed by the classical account, new discoveries revealed some new properties of the material structure of genes—namely, DNA sequences. The empirical knowledge about genes entered into the shared understanding of 'gene', thereby changing its inferential role to IR_2 and altering its reference. However, a prediction of inheritance pattern was still to be found. Indeed, IR_2 attracted geneticists' attention to *one aspect* of EG_1 that takes genes as units of function[2] (Brigandt 2010a, p. 30). Later, the novel empirical information led biologists to gradually change their EG to make it fit with a functional understanding of genes. Accordingly, the molecular geneticists' EG of the concept of gene was changed to explaining 'the way gene produces its products by studying processes that take place within single cells' (EG_2; see Section 4 for a discussion of this gradual change of EG using the time-slice abstraction).

Now, while classical geneticists did not possess any commonly agreed knowledge about the function and internal structure of 'gene', except some sketchy information about the chromosomes (Waters 1994), molecular concept users were more concerned about the material and functional structure of genes to explain its products. In the molecular account, gene is a heterogeneous kind of DNA sequence that in one-one/one-many/many-one/many-many ways produce a certain set of proteins (IR_2). Therefore, the move from the classical concept to the molecular concept altered the three components of the concept, so as to gradually arrive at two distinct concepts (see Table 1 for an illustration).

Classical	Transitional(s)	Molecular
IR_1	IR_2	IR_2
EG_1	EG_1 Gradually →	EG_2

Table 1: The concept gene has undergone change in all three components of the concept

[1] Johansen writes, "[n]o hypothesis about the nature of this 'something' should thereby be constructed or supported" (1909, p. 124, as quoted in Brigandt 2006b, p. 295).

[2] It was a component of the classical concept EG; otherwise, it could not bring about phenotypic patterns of inheritance.

Table 1 illustrates that classical and transitional periods share one component (EG_1). Likewise, the common component between the transitional and molecular periods is IR_2, so that, in each transaction, there is at least one stable component. The fact that IR_2 in the transitional period contains minor differences as compared to the IR_2 in the molecular period does not pose a serious threat to the rationality of change because, in a micro-sliced understanding of change, each change takes place while there is still stability in at least one of the other components. In sum, although the molecular concept of 'gene' differs in all aspects of conceptual content from the classical concept, it gradually grew out of the classical concept. To conclude, scientists are rational to change their concepts based on the assumption that a common ground secures rationality. The same holds for conceptual progress, as in each step of the transition the EG is either better satisfied or becomes broader in scope.

Of note, in this particular case, the reference of gene, as its first component, has undertaken constant change throughout all these periods. Weber (2005) describes the concept as having a 'floating reference', suggesting that the reference of 'gene' has changed constantly in each of its periods so that geneticists use this term to refer to distinct kinds of entities (though with overlaps). Weber suggests that "geneticists were not tracking a single natural kind of entities that they called 'genes'" (p. 224). To this end, he argues that the extension of the molecular 'gene' concept includes bacterial operons, which the classical concept does not include. There are also some genes at Drosophila's ASC locus that the molecular concept does not cover. Brigandt's response to this constant referential variance is to determine the reference by adding a new feature to the content (EG) to demarcate concepts from each other.

In addition to the semantic change, Brigandt's framework can explain semantic variation in some historical cases, such as with the two distinct concepts we associate with 'gene'. In fact, current biologists use 'gene' in different senses based on different aspects and aims. As explained in Chapter 3, the classical account of 'gene' based on EG_1 (but with some modifications of IR) is used even in contemporary studies. Moreover, there is no agreement whether or not DNA sequences are parts of a gene; rather, genes *can* be taken as part of DNA sequences. Furthermore, some decades ago, geneticists assumed a one-one relation between genes and their protein products. However, molecular geneticists of those years believed that DNA sequences were the functional structure of a gene. Yet, in the light of new developments, it appears that genes and their products are in a many-many relationship, based upon the way that an individual would construe the 'gene'. Therefore, a blind use of 'gene' in the current scientific community can be construed in a variety of ways (aims and contexts of use) and endowed with different IR and EG. According to Brigandt, there are two basic factors that determine the reference and meaning of different 'gene' usages (semantic variations). First, which functional or structural part of gene is an individual addressing? Second, what explanatory or investigative goals are pursued by using this concept? The specific research question adopted by the biologists in a given context "influences which of the possible structural or functional features of genes are relevant for this term use" (Brigandt 2013, p. 69).

A similar story could be conveyed for the concept of 'homology', which Brigandt exploited in even greater detail than that of 'gene' in his early works. The rationale for Brigandt's selecting this concept lies in its significance in biology *and* in the constant changes this concept has experienced throughout. As "the central concept for *all* of biology" (Wake 1994, p. 265), 'homology' appears to figure in arguments as a natural kind term.[1] A broad understanding of homology is a sameness or similarity of structures between two species. Due to the evolutionary theory of species, it is common to think of homologous structures as having the same ancestor; however, pre-Darwinian biologists thought that there is a developmental law or a blueprint in the mind of God that explains the similarity among homologous structures (comparative biology). Although Darwinism changed the standard definition of 'homology' to require possession of the same ancestor (IR change), the EGs of pre- and post- Darwinian homology were the same. The epistemic goals pursued by using this concept were (i) to provide a systematic morphological description of species; and (ii) to classify them into relevant groups with more similarities. Even Darwinian evolutionary theory did not change this EG for a while (Brigandt 2012, p. 86). Rather, any change in IR occurred so that to improve the resolution of morphological descriptions and taxonomic classification and maximally meet EG. In short, a phylogenetic definition of 'homology' improved the resolution of meeting the classification of species by comparing them based on their possession of the same ancestor; and a more fine-tuned morphology based on phylogenetic principles could describe species in more precise and unified anatomical descriptions.

Meanwhile, the definition of 'homology' is peripheral in its IR, since the primary role of IR is to account for successful scientific practice. In this case, a more significant factor in IR constitution, which explains the way the scientific community uses the concept, is what usually called 'homology criteria'. While traditional biologists' investigation was restricted to the phylogenetic structures in the relevant position of adult's structures (positional), nineteenth century biologists added embryological criteria to recognize homologue structures (embryological). These changes in the homology conception's inferential role did not cause an irrational semantic change, simply because the EG was stable during the change. The case of semantic change of 'homology' as compared to 'gene' is more straightforward; yet, it becomes more complicated when explaining semantic variation.

Despite the fact that the advent of evolutionary theory did not change the EG, the concept of 'homology' experienced an EG change in the variations of meanings (*semantic variation)*. In contemporary biology, this concept is understood in at least two different ways:[2] phylogenetic (historical) and developmental (biological). While the former is based on genetic heritage and aims to classify and compare species (EG_1), the latter is based on causal structures originated in species' development and aims to provide a causal-mechanistic

[1] See MacLeod (2013) for a detailed discussion of the concept of 'homology' and its status regarding natural kinds.

[2] Brigandt (2006b) addresses three different types of this concept. He adds 'molecular concept of homology' as the phenomenon of molecular biology of the 1980s.

explanation of heritage structure (EG_2). These different goals led to the emergence of different IRs and generated two different concepts associated with the same term. As I argued, these two are distinct concepts because their IR, EG and reference are all distinct. In addition, although there is a large overlap between the reference of phylogenetic and developmental accounts, their references are not fully identical[1].

The possibility of irrational change can be similarly explained using Brigandt's framework. As long as there is a smooth movement from classical to contemporary concept of 'homology', the epistemic change is warranted, because there is at least a shared component for each micro-change. In addition, the current semantic variations do not bring about incommensurable concepts, since contemporary biologists agree on some overlaps by means of the reference, IR or even some part of EG.[2] However, if there is a difference in EG that scientists are not aware of, there is a risk of problematic communication (Brigandt 2012, p. 89). Yet, this risk of hampered communication might be resolved by a clear discussion of their basic goals, although such a successful communication is not guaranteed. To this end, if we assume an imaginary scenario where a scientist from many years ago, say Mendel, comes to our time and argues with contemporary biologists; in that hypothetic scenario, he would not be able to rationally discuss a common subject matter with present day scientists using the same concept. And this is not very odd—the same holds true for some current biologists who are unaware of different EG and IRs.

6- Contrasting Brigandt's Work with Others

For scientific realism, two major problems arising from conceptual change are incommensurability and conceptual progress. As discussed earlier, the traditional strategy for defending scientific realism is based on the stability of reference. However, the classical referential approach faces difficulties in cases when conceptual change involves a change in reference. Overall, two standard realist responses to a change in reference have emerged. The first one is to deny reference change in non-radical cases of conceptual change. On this approach, some particular cases of conceptual change are investigated to show that there is no radical change in reference; instead, it is argued that most such cases involve minor additions or deletions of objects in the extension of the concept (Thagard 1992, 1999). The second response is to distinguish two successive concepts during a radical conceptual change by assigning a reference to the newer one and describing the older version as an empty term. This is the standard realist response to the case of *phlogiston* (Fine 1975; Enç 1976; Nola 1980; Kroon 1985). Both strategies outlined above are motivated by a central (implicit or explicit) assumption of referential approach to concepts. The causal-descriptive theory of reference, which comes in slightly different versions, is the most up-to- date tool for philosophers of

[1] In contrast to phylogenetic account, developmental homology takes *serial homologues* (i.e., repetitive relation in the segments of *the same* organism) as an extension of homology.
[2] There is an agreement on the *main* instances of homologue structures (reference) or the homology criteria (part of IR).

science to preserve the stability of reference while explaining rationality and progress in science. However, I have shown that, at least in biology, there are central concepts that have undergone both radical reference change and meaning change and still seem to be cases of rational and progressive change.

While Brigandt disagrees with a purely referential approach to conceptual change, he argues that a theory of reference determination and concept individuation should consider epistemic goals and values (see Chapter 4 and 5 for further discussion). This non-representational factor (a matter of scientific goals and values) seems to be overlooked by other theories of concepts and theories of reference. Therefore, when scientific realists respond to the problems of conceptual change by proposing a theory of reference determination (provided that reference stability alone can provide the answer), they do not consider this important factor. As a result, current theories of reference cannot distinguish among different concepts of 'homology' or 'gene' in contemporary science, where they have almost identical referents. It seems very hard to explain how 'homology' in a particular case does refer to the same referents (same pair of organisms) but picks out different concepts in developmental and phylogenetic biology. Furthermore, a differentiation of EG would sometimes bring differences in both meaning and reference, since EG supports the actual scientific practice. The answer lies in EG, for it sets the standards to which a scientist makes her inferences and explanations. Therefore, an adequate theory of reference determination would require considering EG as a crucial factor that brings about a differentiation among concepts.

The strength of Brigandt's theory of concepts comes from the insight regarding the involvement of epistemic goals (EG) in concept analysis. The biological case studies show that epistemic considerations, including the epistemic goals pursued with particular concepts, play a vital role in explaining the rationality of conceptual change. However, this insight should be seen within the context of its guiding assumptions. Brigandt's framework for studying conceptual change is pragmatic and non-truth conditional, as it contains an element that does not seek to represent the natural world (namely EG) but is instead based on values/aims of scientific practice. Therefore, Brigandt's approach is more attractive to the philosophers who view science as strongly affected by social values, such as Barker and Kitcher (2013) and Douglas (2009). This is an important point, since understanding the pragmatic and non-truth-seeking nature of Brigandt's framework help us to understand it in comparison with other more traditional theories of conceptual change. As Brigandt explicitly asserts (2006b):

> [T]raditional semantic accounts have construed concepts in terms of reference and/or assumed that a concept consists in certain beliefs about the referent. By introducing the notion of epistemic goal and conceiving of it as an independent component of a concept's content, my account maintained that in addition to the beliefs scientists have, another important feature of a concept is what scientists are trying to achieve by having those beliefs. (p. 386)

Importantly, Brigandt's account of conceptual change is against the well-known thesis of incommensurability characterized by Kuhn and Feyerabend. An analysis of Kuhn's (1962) main argument that gives rise to the incommensurability problem would illuminate Brigandt's particular stance against it. Kuhn's argument can be separated into two steps. First, given a general understanding of scientific paradigms, it is argued that scientists may associate two completely different meanings with the same word within two different paradigms. The second step is to argue that the aforementioned semantic difference generates fundamental epistemic problems, including incommensurability or rejection of the rationality of scientific choice. A common realistic or naturalistic response to Kuhn has been to rebut the first step (Kitcher 1978; Devitt 1979; Sankey 1994). According to this approach, referential overlaps prevent a radical meaning difference between the concepts of rival paradigms. Of course, the strategy of finding reference stability (or referential overlap) is not the only realist way to argue against the problem of incommensurability. For instance, Kitcher (1993) allows that a concept may have some empirical misconceptions in its initial stages, but that conceptual change may eventually clear up these misconceptions so that the concept comes to refer to a single natural kind.

In contrast to this realist approach, Brigandt suggests that Kuhn's argument starts with an epistemic insight (i.e., that different uses of concepts can be associated with different patterns of belief, or IRs). According to Brigandt (2006b, p. 22), Kuhn makes a semantic detour (invoking meaning change) to support his epistemic verdict (that there is no rational way to choose among rival theories that involve distinct paradigms). Brigandt's analysis leads him to focus on the second step of Kuhn's reasoning: that radical meaning change would generate a problem for rational theory selection or the incommensurability problem. Based on his biological case studies, Brigandt acknowledges the possibility of simultaneous reference and meaning change. However, he rejects the Kuhnian conclusions. In Chapter 4, I will contrast Brigandt's response to Kuhn with a realist response. I will argue that Brigandt's framework does not support scientific realism. Furthermore, in Chapter 5, I will argue that Brigandt's approach generates a problematic form of radical holism. However, we should not neglect the important lesson that Brigandt has taught us: in studying conceptual change, it is important to consider epistemic factors.

7- Conclusion

In this chapter, I explained Ingo Brigandt's account of conceptual change, a prominent example of non-representational approach, and compared it to the referential approach. Brigandt's account distinguishes three semantically relevant components of concepts: (i) reference; (ii) inferential role; and (iii) epistemic goals. Reference is the least important part on Brigandt's account, while epistemic goals are the most important, as they provide a convincing ground for the explanation of rational conceptual change. Epistemic goals are the most stable elements in conceptual change and play a central role in explaining the progress of science through changes in reference and inferential roles. Furthermore, epistemic goals and values are non-representational components of concepts – that is, they express scientists' explanatory aims and interests in developing a concept, rather than represent the reference of that concept. In this chapter, I argued that complicated case studies involving reference change and change in IR, which seem fraught with difficulty when explained using the traditional approach, can be easily explained using Brigandt's theory.

Chapter 4

Evaluating a Non-Representational Account of Conceptual Change

1- Introduction

In Chapter 3, I outlined a non-representational approach to conceptual change. I explained that, in contrast to the traditional approach that considers conceptual change as a purely semantic and referential issue, this novel approach assumes a new stance on which epistemic factors as well as reference are taken into account. As one of the central figures of the non-representational approach, I selected Ingo Brigandt and investigated his works in further detail. One reason for this selection was that Brigandt has been particularly explicit in seeking to resolve problems of conceptual change. Furthermore, he takes biology as the main focus of his work, and hence, in view of my argument that biology produces complicated conceptual change case studies (see Chapter 2), his arguments on conceptual change deserve a thorough consideration. After a comprehensive analysis of Brigandt's works in Chapter 3, I now turn to a critical evaluation of his stance as compared to a standard realist account of conceptual change. My aim in this chapter is to examine a non-representational approach to conceptual change from a scientific realist point of view.

The classical scientific realist approach to conceptual change was initially proposed in the late 60s by philosophers who sought to defend scientific realism against the potential objections of Kuhn and Feyerabend. Israel Scheffler was among the first philosophers who argued for a referential and semantic approach to conceptual change. Using the Fregean dichotomy of sense and reference, Scheffler (1967) showed that the meaning variation that occurs in some scientific revolutions does not need to entail a reference change, rather it is the variation of sense that occurs in most cases of scientific change. According to Scheffler, problems such as incommensurability have no purchase, since referential stability connects two different paradigms. Following him (1967), a large number of realists have sought to develop a referential explanation of conceptual change (e.g., appealing to a theory of reference), each focusing on different aspects of scientific realism (Putnam 1973; Martin 1971; Fine 1975; Devitt 1979; Leplin 1979; Levin 1979; Newton-Smith 1981; Kitcher 1978, 1993; Hacking 1983; Burian 1985; Miller 1987; Papineau 1987; Sankey 1994, 1997; Psillos 1999, 2012; Andersen 2001).

Given that the ultimate aim of the referential approach is to support a version of scientific realism, or at least to rebut certain scientific anti-realist objections, each effort in this regard is implicitly or explicitly based on what I call 'a classical realist package for conceptual change' (see Section 2 for further discussion). This package provides a basis on which to explain conceptual change, while maintaining a full-blooded account of scientific realism, in the face of potential problems such as the incommensurability thesis.

In this chapter, I will explain that a full-blooded realist account of conceptual change, in my view, should have four components in accord with a standard form of scientific realism. I take Psillos' characterization of scientific realism (1999), consisting of metaphysical, epistemological and semantic theses, as the standard form of scientific realism. Then, I call this account of conceptual change the Classical Realist Package. In Section 2 of this chapter, I explain the package and, based on this explanation, evaluate Brigandt's account of conceptual change in Section 3. Rather than defending the classical realist package *per se*, my aim will be to fully scrutinize its characteristics and to argue that, although Brigandt's account is consistent with the metaphysical thesis, it does not comply with other important aspects of the package. Therefore, although Brigandt tries to rebut the objections initially raised by Kuhn and Feyerabend regarding the incommensurability thesis and scientific progress, his works do not in the end support such conclusions in favour of scientific realism. I will argue that Brigandt's account is in fact incompatible with a typical version of scientific anti-realism; however, the fact that a theory is incompatible with scientific anti-realism does not inevitably lead to a defence of the realist package. A non-representational account of conceptual change, such as the one put forward by Brigandt, does not focus on the key notions of 'truth' and 'reference', so it does not fully correspond to what has usually been claimed by scientific realists in the case of conceptual change. Finally, I will explain one of Brigandt's main objections to the strategy that scientific realists usually take, which distinguishes his view from the referential approach. Explaining this objection prepares the ground for my next chapter, which can be seen as a return to the referential approach.

2- The Classical Realist Package for Conceptual Change

2.1. Basics

Scientific realism encompasses a wide range of accounts each focusing on one aspect of realism. A standard definition of scientific realism characterizes it according to the following three theses: (i) a *metaphysical* thesis, according to which the external world has a mind-independent and objective structure; (ii) a *semantic* thesis, according to which scientific theories should be interpreted at their face value—meaning, in this case, that they are *truth-conditioned* descriptions of both observational and non-observational domains, and that the theoretical and non-theoretical terms and concepts employed by those theories *have objective referential purport*; and (iii) an *epistemic* thesis, according to which current successful scientific theories are well-confirmed and (approximately) true descriptions of the world (Psillos 1999, p. xvii). Based on this standard definition, a full-blooded scientific realism can be distinguished from other weaker versions of scientific realism or different variations of scientific anti-realism. Hereafter, I will refer to this definition as 'the standard' account of scientific realism in general.

In line with the aforementioned definition, I articulate the main theses of a *classical realist package for conceptual change* (hereafter referred to as 'the classical package'[1]). Although the classical package is not literally drawn from the standard account of scientific realism, it is an application of the realist framework to the specific question of conceptual change. I articulate the characteristics of the classical package in terms of four independent theses (T1, T2, T3 and T4), each reflecting one level of realists' commitment. The first thesis (T1) reflects a general metaphysical commitment to the mind-independent domain; the second thesis (T2) makes a normative claim regarding the referential function of central theoretical terms in science. The third thesis (T3) discusses a realist understanding of scientific progress in terms of getting closer to the truth about the mind-independent domain.[2] Finally, the last thesis (T4) concerns the rationality of conceptual change in terms of subjective justification and a solid connection with the truth about the mind-independent domain of reality.

It is worthwhile to distinguish a realist account of conceptual change from the general doctrine of scientific realism. Although the former is based on the latter, there might be different caveats and variations involved with each. The main reason for this being so is that, while conceptual change *as an explanandum* is more concerned with language and the process of human thought making rational connection with the world and other language users, the general notion of scientific realism deals with broader *explananda* (e.g., the reality

[1] I will call specific theories of conceptual change that respect the classical realist package 'realist theories'. Given that holding scientific realism is a matter of degree (and can range from a radical realist via a modest to a weak realist), the degree to which a theory of conceptual change is realist might vary.

[2] In Chapter 6, I will briefly mention that this thesis can be satisfied in two ways: an account that takes scientific progress as getting closer to the truth *about the same entity* referred to by the previous theory, and an account that allows progress to happen *about a domain of entities*.

of theoretical entities, the referential nature of theoretical terms, and epistemic success of scientific enquiry). In short, the classical package is a specific application of the standard definition of scientific realism. Each component of the classical package brings an account of conceptual change one step closer to what we usually know as a full-blooded account of scientific realism. Therefore, a full-blooded realist account of conceptual change will incorporate all of the abovementioned four components, while maintaining only the first component is the weakest realist stance that can be demarcated from an anti-realist account.

2.2. The Classical Realist Package

The classical realist package contains four normative theses that jointly demarcate what a full-blooded realist account of conceptual change should be like. These four theses are supposed to perform as job descriptions for such a realist theory, not the theory itself. In other words, while a full-blooded realist wants to provide a substantive theory that fills these roles, there are different possible ways to go. It should also be noted here that four of the theses are not about conceptual change *per se*. Rather, the first thesis is a general metaphysical suggestion necessary for all realist accounts. This can be stated as follows:

> (T1): (**Objectivity**) There is a domain of mind-independent objects, properties, relations and facts, which helps to explain the success of scientific theories that employ a given set of theoretical terms.

This preliminary thesis sets scientific realism apart from traditional idealism or phenomenalism or some kinds of radical social constructivism. It suggests that, if the unobservable entities posited by scientific theories exist, they are in the mind-independent realm that is independent of our knowledge. What T1 does is to acknowledge the existence of this domain and its independence from human thought.[1] Scientific realists usually argue for its basic significance in one way or another.[2] However, endorsing this thesis is not enough for one to be considered a realist in studying conceptual change. I will argue that a minimal realist in this regard must endorse at least the next thesis (T2).

Furthermore, a realist account of conceptual change must accept that the central terms of successful scientific theories,[3] including theoretical terms, refer to an entity in the external

[1] The importance of a connection between human knowledge and this mind-independent reality will be discussed further in T4.

[2] Psillos 1999, p. xviii; Sankey 2008, p. 16; Hacking 1983, p. 27.

[3] Philosophers have different interpretations of what it means for a theory to be 'successful'. For Laudan (1984), a theory is successful "so long as it has worked reasonably well, that is, so long as it has functioned in a variety of explanatory contexts, has led to several confirmed predictions, and has been of broad explanatory scope" (p. 110). Following Psillos (1999), successful scientific theories are truth-like theories with novel accurate predictions and applications. It is also argued that the central entity terms in such theories successfully refer to an entity in the external world. The kind-constitutive properties of the entities are the causal origins of the information associated with the term (Psillos 1999, p. 281). Kitcher (1993), however, holds a broader definition. According to him, the term 'phlogiston', which is an empty term for Psillos (1999), is partially successful in referring in virtue of particular uses of the term where the user was causally (ostensively) connected to a sample of oxygen.

world (e.g., atom, electron, gene, homology, etc.). It is the most basic and primary thesis of a realist account of conceptual change, since the rejection of such central theoretical terms undermines the realistic structure of an investigation in the first place. We can formulate this thesis as follows:

> (T2) (**Reference**) Central theoretical terms employed by successful scientific theories refer to particular entities in the external, mind-independent world.

Thus, by making a connection between the language of science and reality, T2 requires a commitment to the reality of mind-independent entities mentioned in T1. This dependence ensures that T2 is consistent with the standard definition of scientific realism. For scientific terms to refer to the features of the mind-independent world, it is necessary that the world has such mind-independent structure (i.e., a metaphysical commitment of scientific realism). However, T2 is not a purely metaphysical thesis, in that it concerns the relation between representations and the world and makes some semantic commitments concerning the referential success of key scientific terms. Indeed, an explanation of conceptual change without commitment to the referential success of key scientific terms cannot be considered a realist theory. According to T2, the truth (or approximate truth) of scientific theories requires a successful reference to mind-independent, objective features of reality. According to scientific realism, if a theory is predictively accurate, but fails to refer to the intended objects of reference, it is a false theory.

Furthermore, T2 is not a thesis specifically about conceptual change *per se*, as it is concerned with the semantic function of specific concepts and terms in scientific theories, rather than changes involving those concepts. Nonetheless, T2 secures a '*minimal realist'* stance for an account of conceptual change. In this regard, a minimal realist takes theoretical scientific concepts to be referentially successful but does not make a realist claim about the aim of conceptual change or its rationality. Said differently, a minimal realist is realist about the representational function of concepts, but not necessarily about conceptual change. This position is still realist, since accepting T2 distinguishes a realist account from those of social constructivists or idealists who believe that the content of scientific theories can be reduced to the epistemic and cognitive abilities of individuals or social communities. A minimal realist may argue that conceptual change does not get us closer to the truth but cannot argue that central theoretical terms of previous theories are not referential merely because those theories are shown to be false from the current perspective.[1]

In addition, while T2 holds that key scientific concepts refer to external features of the world, this thesis does not specify whether or not the reference exhausts the semantic content of those concepts. A *strong reading* of T2 holds that the meaning of scientific terms (and the

[1] Laudan's (1981) famous argument, usually called the pessimistic induction, is the clearest way of arguing for this reference failure. Also, Putnam (1978, p. 22-25) discusses this argument more clearly focused on theoretical concepts, rather than theories.

concepts they express) is exhausted by their referents (e.g. Fodor 1994, 1998, 2004[1]). However, a *modest reading* of T2 holds that there are further factors involved in specifying the meaning of those terms over and above specifying their referents (e.g. Greenberg & Harman 2005). Importantly, this modest reading is not an anti-realist account, since it does not deny reference. Rather, it holds that certain epistemic and cognitive abilities help to constitute the content of scientific terms and concepts. However, what is important for my discussion is that, insofar as the strong and the modest versions of T2 posit a worldly referent for theoretical scientific terms, they both are realists.

What makes both the strong and the modest readings of T2 realist is that they require the referential success of key theoretical terms in mature scientific theories. The referential success relates T2 to a fundamental argument for scientific realism, usually called the 'No Miracles Argument' (originally proposed by Hilary Putnam, 1975). According to this argument, scientific realism is "the only philosophy that doesn't make the success of science a miracle" (Putnam 1975, p. 75). The reason is that the best way for explaining the success of science in terms of empirical predictions and explanations is to take those theories to be true. Otherwise, if a theory does not engage with the world and yet successfully predicts and explains natural phenomena, this success will be a miracle. The No Miracle Argument uses both T1 and T2 to explain the referential success of key scientific theories. The key strategy here is the transition from *success* of scientific theories to the referential *engagement* with the world. This general idea can also be applied to the referential success of central concepts employed by scientific theories. If the entities put forward by science to explain the structure of the world (e.g. atom, gene, oxygen, etc.) do not exist, then it would be implausible for a theory which refers to them to successfully explain natural phenomena. Putnam (1978) seems to agree with this application of his argument: "if these objects [posited by theories] don't really exist at all, then it is a miracle that a theory which speaks of gravitational action at a distance successfully predicts phenomena" (p. 19-21).

Now let us turn to the third thesis of the classical package.

> (T3) (**Scientific Progress**) Scientific progress consists in updating one's theories and conceptual understanding in such a way as to get closer to the truth about the domain of mind-independent structures.

The third thesis concerns one aspect of the epistemic success of conceptual change according to the classical package. It has already been assumed that the central concepts of successful theories are to a considerable extent successful in referring to the external entities and

[1] According to Fodor's (2004) atomistic semantics, the other mental or cognitive characteristics involved in possession of a concept are *causally,* but not *metaphysically* related to the concept. Fodor (2004) describes his account as a "Cartesian" view of concepts "according to which having [concept] C is being able to think about Cs as such" (p. 29). In fact, it is the informational relation between symbols of thought and external properties that constitutes the content of concepts, while the other epistemic abilities explain why this information is originated and maintained in the human mind (see Margolis 1998; Laurence & Margolis 2014).

statements produced by the theories that are approximately true descriptions of the world. However, T3 goes beyond this assumption by focusing on a dynamic truth-seeking aim of change in science. In particular, successful instances of conceptual change in science—i.e., those that occur in mature and successful scientific theory changes[1]—are said to track the truth. Accordingly, significant instances of conceptual change in science occur in those theories that produce more true beliefs as compared to earlier versions of those theories. Of note, the new concepts are referentially better than earlier versions, meaning that those concepts can more accurately pick out the intended entities. In other words, the aim of conceptual change (the direction of change) should be to get science closer to the truth about the domain of mind-independent entities, which is the definition of progress for scientific realists.[2]

For most classical scientific realists, progress is defined in terms of getting closer to the truth or approximate truth.[3] Compared to earlier theories, current successful scientific theories are well-confirmed and provide an increased amount of truth about an underlying reality. In addition, scientific terms or concepts employed by current theories more accurately and/or more precisely refer to objective entities than their predecessors. A prominent case of progress can be seen in the transition from the phlogiston theory to Lavoisier's oxygen theory. Current science holds that the latter theory more truly describes a set of natural phenomena, since it replaced an empty concept of 'Phlogiston' with a referring concept of 'Oxygen'. With this in mind, conceptual change should be directed toward the scientific goal set by scientific realists (i.e., truth), and, compared to the concepts we employed previously, our current successful concepts should more accurately represent the world.

A combination of T1, T2 and T3 allows for different realistic accounts of conceptual change, while demarcating stronger versions of the classical package from other weaker realist packages, including the minimal realist. While a dominant account of progress for scientific realists involves getting closer to the truth *about the same entities* postulated by earlier theories (e.g. Sankey 2017, p. 211), a combination of T1, T2 and T3 enables a definition of progress that view it as getting closer to the *truth about the same domain of entities*, which does not necessarily involve getting closer to the *truth about the current reference*. This is so because T3 does not specify whether the later truth is about the same entities referred to in the previous theory. In Chapter 6, I will argue that there are good reasons to believe that scientific progress can occur even without increasing true beliefs *about the same entities*. Other than these variations, the idea that conceptual progress in science is only *contingently* related to increasing truth (i.e., sometimes it is, and sometimes it is not) is not congruent with the classical package. Also, the idea that conceptual change takes place in order to better

[1] See footnote 3, p.83.

[2] This is an important thesis, such that some scientific realists consider 'aim realism' (i.e., that the aim of science is to discover more truth, and science is progressive in this respect) as a main doctrine of scientific realism (Sankey 2008, p. 13).

[3] This belief is sometimes defined in terms of verisimilitude (Niiniluoto 1999).

satisfy scientists' interests[1] is not consistent with the classical package. In what follows, I discuss some consistent and inconsistent readings of T3.

What account of progress should be used in order to say that an account of conceptual change is in accord with the classical package? There are two competing approaches to scientific progress: pragmatic and epistemic. In the pragmatic approach, which is usually attributed to Kuhn's (1962) and Laudan's (1977) accounts, progress is viewed as a *successful* improvement towards solving scientific problems and puzzles. It is important here to note that 'solving' in the pragmatic approach does not *literally* mean providing a true answer to one specific question, which is what we usually understand as 'problem solving'. Rather, astronomers who spent hundreds of hours viewing and recording the movement of stars and planets were contributing to the progress of science, though they were not answering a specific question. For Kuhn (1962) and Laudan (1977), in particular, whether or not a given theory solves a particular problem should be *decided* by the community of scientists, meaning that there is no external matter of fact to assure this relation (Laudan 1977, p. 127). Furthermore, whether or not a solution is good should be decided in a way that is neutral as to whether the solution counts as true, since we "do not have any way of knowing for sure that science is true" (Ibid). Indeed, assuming the pragmatic approach, it is not correct to say that a later version of a theory is getting closer to the truth than an earlier one, as the referential match between theories and their 'real counterpart' in nature seems 'illusive in principle', Kuhn believes (1970, p. 206). This is a pragmatic approach to progress since it posits purely pragmatic standards in determining the advancement of science. Based on these assumptions (and by virtue of not involving the notions of truth or reference in formulating progress), the pragmatic approach sharply contrasts with what a scientific realist requires of an account of progress.[2] In other words, what makes the pragmatic approach inconsistent with a realist project is the central focus of the former on *success* (in problem solving) rather than on gaining *knowledge*.[3]

In contrast to the pragmatic approach, the epistemic approach holds that scientific progress involves an increase of verisimilitude (truth-likeness, approximate truth) in theoretical claims using scientific concepts. The core assumption of the epistemic approach is that a new theory should incorporate more true statements (and produce more true beliefs) than previous versions of the theory. However, this condition can embrace different readings. For instance, Bird (2007, 2008) makes a distinction between two realist approaches to scientific progress: the one that takes *truth* to be the central notion in defining progress, and the other that takes *knowledge* to play this role. Proponents of both views endorse realist accounts of progress,

[1] The addition that "scientists' interests *may* be shaped by the truth" does not make it consistent with the package.
[2] For example, according to Sankey (2008), "[t]he realist must adopt a perspective that is situated outside the human perspective" (p. 4).
[3] It is worth mentioning here that the pragmatic approach does not necessarily deny the truth-seeking aim of science; rather, instead of the notion of truth, it focuses on scientific puzzles and scientists' goals.

compatible with T3 of the classical package.[1] However, Bird defends an account of progress that involves knowledge, as, for him, growth of truth does not satisfy our intuitions about the nature of progress. According to him, an accidental increase of verisimilitude (i.e., true beliefs produced by an irrational method or by a lucky guess) does not account for progress, as, upon realising that the method was unreliable or faulty, scientists reject earlier beliefs. Therefore, an account of progress focused only on truth would be in terms of progress and regress, which goes against our intuition of progress involving a steady growth (Bird 2007, p. 65-66). Although I agree that an accidental capture of truth should not be seen as progress, it appears to me that an irrational method that *consistently* produces true beliefs must be at least implicitly or partially based on a truth-conducive method. Therefore, when scientists reject their previous beliefs, they are not rejecting that those beliefs were (somehow) progressive as compared to older accounts.

This being said, I agree with Bird that a fully adequate account of progress, and of scientific realism more generally, should incorporate both notions of truth and justification (i.e., it should be concerned with knowledge, as true beliefs without justification could be generated by a lucky guess). Yet, whereas Bird (2007) combines an account of scientific progress with an account of rationality,[2] I discuss the rationality of conceptual change separately, as constituting the next thesis (T4) of the classical package. It appears to me that separating these two issues is closer to what scientific realists usually do.[3] The rationality claim can pick up on what I defined as progress in T3. It argues that scientific practice is rational only if scientists are justified in taking theoretical updates to fulfil the objective *aim of science* and if the subjective epistemic justification is non-accidentally connected with the truth. Therefore, although the rationality of conceptual change will be constructed on the aim of science, I believe that T3 and T4 are two independent claims. I formulate T4 as follows:

> (T4) **(Rationality)** Scientists are rational in changing their conceptual practices only if (i) the change is subjectively justifiable as constituting scientific progress and (ii) this subjective justification has a non-accidental connection with the truth about the world.

The crux of a rational conceptual change is to determine in virtue of what scientists are epistemically justified in accepting, abandoning or fine-tuning their conceptual understandings. The problem of irrationality appears when, during conceptual change, the subject matter changes, or an individual cannot find a ground to decide between competing options. In the first case, it is not clear how an individual scientist is justified to use the same word to refer to completely distinct objects in different situations. A discontinuity of concept usage may also hamper communication, as scientists are no longer talking about the same

[1] Bird (2007) admits that a cumulative truth view of scientific advancement with appropriate epistemic conditions is good enough to explain several scientific changes (p. 74).
[2] "Progress and rationality cannot diverge that easily" (Bird 2007, p. 66).
[3] My view is in line with Bird's (2007) saying that "[r]ealists have typically sought an account of progress in terms of increasing verisimilitude (truth-likeness, approximate truth) rather than increasing knowledge" (p. 64).

subject. Following my argument in Chapter 1, fulfilling this task requires some sort of continuity between the old and new versions of a concept in order to fill the gap. However, after having this continuity, a rational change requires an *obligatory* reason that gives scientists a basis for a comparison between competing concepts. The reason for adding this second element is that the mere continuity may offer several options to replace the old concept, while only one of them is rational to take. My focus here is to describe the conditions that provide an adequate explanation of these issues (continuity and obligation) so as to distinguish a full-blooded realist package from anti-realist accounts.

One initial condition for a rational conceptual change is *subjective justifiability* by means of having sufficient reasons to believe that the new understanding is coherent with the older version.[1] The virtue of coherence assures a concept user that the topic has not changed, and that continuity is preserved. However, the replaced concept should also be an improved version in terms of satisfying scientific aim(s)— a rational concept user would not change a concept with a worse or equal one. In T3, I discussed that scientific realists usually set the aim of science as getting closer to the truth about the domain of mind-independent reality. Accordingly, scientific realists believe that scientists are subjectively rational in fine-tuning their concepts as long as there is evidence that a change will improve their understanding of nature. Indeed, in order for scientists to make a rational theory choice, the new concept should have some theoretical virtues — virtues of a kind that help scientists to produce a true explanation of nature — that persuade scientists that the new concept fits better with the aim of science.[2] . Therefore, the basic inferential rules, including induction, give rise to a rational scientific decision as long as they "tend to generate true beliefs" (Psillos 2012, p. 108; Goldman 1979, p. 11). This certainly does not mean that we have direct access to the truth to assess competing scientific theories (i.e., it is not necessarily true that *truth* is the result of good reasoning). However, this condition merely asserts that there should be sufficient reasons for individual scientists to warrant a change of conceptual understanding. Indeed, for the full-blooded scientific realists, the notion of rationality has close connections with the aim of science—which relates T4 to T3.

However, subjective justifiability is not enough for a realist account of conceptual change. Along with subjective justifiability, we also need a secure connection between this condition and the objective world. An anti-realist would be happy to propose an account of rational change in terms of internal coherence of beliefs or communal acceptance without having a higher standard that connects this rational decision to the external reality.[3] Similarly, anti-

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[1] While coherence may have different characterizations, here I follow van Fraassen (2007) in construing it in a broad way; including logical, pragmatic, probabilistic coherence, etc.

[2] For example, McMullin (1987) claims that the theoretical virtues of science are those that scientists use to come up with 'good theory' and those that "thought to be symptoms of truth" (p. 66). Also, Boyd (1980) suggests that theoretical virtues of science are those that are satisfied by true theories (p. 622). Psillos (1997) accepts these realistic views, asserting that the explanatory power of scientific theories, those that decide which theory should be opted in a rational theory selection, is pertained to the confirmation and truth (p. 3).

[3] Kuhn (1970) believes that "there is no standard higher than the assent of the relevant community" (p. 94).

realists are not bothered by an accidental access to the truth about the structure of the world, as they frame the question of rationality and aim of science in terms of values other than *the truth* of the objective world (i.e., neutral to the notion of truth).[1] Indeed, many scientific anti-realists agree that science and scientific reasoning are 'rational', but rationality is not explained by getting closer to the truth about mind-independent objects and properties (see. Kuhn 1970, *Postscript*, van Fraassen 1980; Laudan 1984).

According to standard scientific realism,[2] true beliefs are systematically generated using scientists' use of ampliative-abductive methods. These methods tend to produce true beliefs and theories about both theoretical and observational entities. Sound scientific reasoning or a rational decision, according to this account, hinges upon a method that assesses the rival theories based on their success in producing more true beliefs. Consequently, evaluation of the success of those methods depends on empirical findings and investigations. In this respect, Psillos (1997) writes:

> Sound reasoning is intimately bound up with truth and the capacity to generate and maintain true conclusions from true premises. Truth emerges as the basic cognitive virtue of sound reasoning. Achieving true beliefs is the aim in light of which methodological and reasoning strategies should be evaluated. (p. 709)

The theories produced in such a truth-seeking process admit the referential success of their key concepts, both theoretical and non-theoretical, although thus success is not guaranteed. For as discussed above in relation to T2, the central concepts employed by approximately true theories should refer to real entities.[3] This implies that scientists are justified in changing their concepts in the same way as they are justified in theory choice. Therefore, scientists are rational in generating new theories and forming new concepts, including theoretical ones, only if their beliefs are subjectively warranted, and if these warrant beliefs are non-accidentally connected to the truth via a truth-conducive method. In this context, a full-blooded realist package diverges from an account of rationality proposed by a structural realist. For a structural realist, the approximate truth of scientific theories does not entail referential success of key theoretical concepts that these theories use.

If we compare such a full-blooded realist package, consisting of both conditions, with weaker accounts, we will see that minimal realists do not focus on a commitment to an account of the rationality of theory change and conceptual update as such (i.e. commitment to T3 and T4). However, this does not mean that minimal realists reject rationality or progress of science

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[1] Laudan (1978) defends a problem-solving account of rationality in which "within the problem-solving model, however, we make no assignments of truth or falsity" (p. 43). Likewise, he summarizes that "determinations of truth and falsity are *irrelevant* to the acceptability or the pursuitability of theories and research traditions" (Ibid, p. 120).

[2] I set Psillos' (e.g., 1999) account of scientific realism as the standard account.

[3] Psillos (1997) follows McMullin in that "calling a theory 'approximately true', (...) would be a way of saying that entities of the general kind postulated by the theory exist" (1997, p. 713). Also, I argued that Putnam's (1975, 1978) 'No Miracle' argument can be applied to referential success of key scientific terms.

altogether. Rather, the notion of rationality and progress will be defined in terms of (pragmatic) factors other than the truth (e.g., success in puzzle solving). In this regard, the main difference between a minimal realist and an anti-realist is the commitment to T1 and T2, which suggests the importance of referential success of theoretical terms. In other words, any realist account of conceptual change must acknowledge, at least, the existence of theoretical entities referred to by successful scientific theories. In section 3, I will explain why Brigandt's account is realist *and* minimalist; however, theorists like Laudan and van Fraassen who would reject T2 should be regarded as anti-realists, because they reject or withhold the referential success of theoretical entity terms. Indeed, the last two theses of the realist package, which concern the dynamics of conceptual change, distinguish a full-blooded package from a minimal realist one: a full-blooded realist package is committed to the truth as the aim of science.

An anti-realist account of conceptual change lacks several important factors that makes it inconsistent with the classical package. These factors include anti-realists' tendency to reject the non-accidental connection between (1) successful science and theoretical truth and (2) between successful theories and reference. There are different worries among scientific anti-realists regarding the referential success of key theoretical terms. For example, the pessimistic induction, originally proposed by Laudan (1981), is based on some historical examples of referential failure of previously successful theories.[1] This argument, in different forms and with different examples, was proposed by different philosophers even before Laudan's (1981) famous paper. For example, Putnam (1978) discusses the possibility of a meta-induction which may become 'overwhelmingly compelling' so that "no term used in science of more than fifty (or whatever) years ago referred, so it will turn out that no term used now (except maybe observation terms, if there are such) refers" (p. 25). These worries seem to be incompatible with my T2.

Furthermore, according to an anti-realist account of rational conceptual change, scientists are justified in changing their concepts only if there is an overall consistency between their beliefs before and after the change. For example, van Fraassen (2000, 2002) defends such a purely *coherentist* account of scientific rationality in that the beliefs and values of a rational scientist should be coherent before and after the change. Definitely, van Fraassen (2002, p. 151) does not deny the rationality of change in science; however, his account of "rational change" appeals to subjective elements (e.g., emotions, behaviours, etc.) in bridging "the logical gap between the prior and the posterior epistemic states" (Ibid, p. 108). This sort of explanation of rationality, which involves the invocations of subjective emotions instead of appealing to the states of affairs in the world, is very lax because every theory change becomes rational as long as it does not lead to logically incoherent corpus of beliefs. There are many *permitted*

[1] A number of authors criticize the list of referential failures proposed by Laudan (1981). For instance, Mizrahi (2013, p. 3219) argues that Laudan's (1981) list is not a random sample of the target population of successful theories.

ways for scientists to be rational.[1] Therefore, "rationality is precisely what is rationally permitted" and thus, "nothing is needed above and beyond coherence" (van Fraassen, 2000, p. 277).

This account of rational change does not suffice for the classical package, although coherence is necessary. Constraining scientific choices to a wide range of not-fully-irrational choices (i.e., *permitting* so many choices to be rationally chosen), van Fraassen's (2000, 2002) account of rational scientific change says nothing about whether and, if so, when a specific belief or theory should be accepted. It also does not specify the cases that involve incremental steps, "with no clear picture at any stage of a choice between two well-defined alternatives" (McMullin 2007, p. 173). The classical package requires an account of rationality in which objective evidence supports *a particular* choice among alternative options, as it makes more true statements than false statements provided by previous options. Said differently, scientists should be justified in selecting a certain theory based on the objective evidence they have gathered, over and above the mere coherence of their beliefs. This implies that the classical package is not only committed to the *permission* account of rationality, but also requires scientists to have sufficiently strong evidence to *obligate* them to change their concepts.[2]

Thus, a full-blooded package not only endorses a coherent structure of beliefs and theories (i.e., without formal inconsistency), but also strongly supports the new content by the facts about the entities in the world. In other words, the negative aspect of rational conceptual change pertains to the formal structure of change, while its positive aspect is governed by objective truth that governs successful conceptual change, such that the change is *truth-seeking* (and *evidence-based*). In this regard, a rational conceptual change selects the concepts with more referential success and those that provide a more predictive and explanatory power to the theory they are employed by, rather than the concepts with more communal acceptance. Therefore, the classical package not only rejects a purely coherentist account of rationality in favour of an obligation(ist) account in explaining conceptual change, but also adopts the truth as the ultimate value of science.

2.3. Strategy

By making commitment to the four theses outlined in Section 2.2, the classical package not only supports the standard account of scientific realism, but also seeks *referential continuity* in order to secure the rationality and progress of change. In particular, the standard response to the problem of incommensurability—a most important challenge of conceptual change—

[1] Van Fraassen (2002) refers to this this kind of rationality as "bridled irrationality" (p. 92, p. 97).

[2] Psillos (2007) defends such an account as follows: "[…] rationality involves *permission.* […] Yet, rationality also involves *obligation.* [...] Purely formal criteria (deductive and probabilistic coherence) are not sufficient for rationality" (p. 164)

is *referential continuity*.[1] The referential approach to conceptual change holds that, in order to explain rationality and progress through conceptual change, we should focus on the notion of reference (e.g., seek for referential stability). This is the main reason why, seeking to secure the stability of reference across change of concept, philosophers of science have tried to defend a theory of reference determination. For instance, Kuhn's (1962) and Feyerabend's (e.g. 1962) works on the problem of incommensurability have been argued to be based on a *descriptive* theory of reference (e.g. Sankey 1991; 2009), while the proponents of scientific realism have usually tried to defend a version of the causal or causal-descriptive theory of reference (e.g., Putnam 1973; Psillos 1999). The referential approach also includes Kitcher's (1978, 1982, 1993) account, in which the reference of different tokens of a concept is determined based on contextual factors. It is true that Kitcher steps beyond a traditional referential approach by appealing to contextual factors; however, due to the reliance on the notion of reference as the main component, Kitcher's approach views conceptual change from a *semantically representational point of view*.

On the other hand, a *non-representational approach* is not primarily interested in the notion of reference in explaining conceptual change. The key idea here is that, along with representational aspects, concepts contain certain important non-representational aspects that contribute to the semantic analysis of conceptual change in the course of the history of science. Among those non-representational aspects are, for example, epistemic goals pursued using a concept, which may have a more important role in analysing conceptual change. Moreover, this approach retains the idea that studying conceptual change based on reference alone—be it descriptive or causal—is insufficient. In Chapter 3, I argued for the insufficiency of a referential approach in dealing with complicated biological case studies. Moreover, although a referential approach ignores this possibility, scientific concepts are, to an extent, shaped through the contributions they make in scientific justifications, explanations and discoveries (Brigandt 2006b, p. 416). This novel approach is supported by (developmental) psychological studies of concepts, in that the concepts should be taken as figuring in inferences and explanations, instead of merely securing reference (Keil and Wilson 2000; Murphy 2002).

3- Brigandt and the Classical Package

3.1. Brigandt as a Minimal Realist

As I explained in Chapter 3, Brigandt's semantic project attributes three independent properties to scientific concepts: Reference, Inferential Roles (IR) and Epistemic goals. The first component that connects concepts to external reality is reference. By positing this semantic component, Brigandt implies a commitment to the second thesis of the classical package. It is also implicitly acknowledged that reality has a mind-independent structure

[1] Sankey (1994, p. 4) describes this response as 'the standard response' and explains the referential approach in Chapter 2. He suggests that this approach stems from Scheffler (1967) and Putnam (1975).

discoverable by employing theoretical terms.[1] 'Gene' and 'homology', two central terms in biology, are theoretical and, according to Brigandt, refer to their referents, even though their reference might vary over time. In fact, while Brigandt has provocative ideas about reference determination and individuation, none of them implies that theoretical terms are unable to pick out real referents. For instance, Brigandt's (2005) account of reference determination reserves some role for the interpreters' interest: depending on the philosophical question at stake, interpreters can assign different referents to one's concept (p. 22). This claim needs to be evaluated based upon Brigandt's account of concept pluralism, which I will discuss later. But for now, Brigandt does not deny the fact that key scientific concepts refer to features of the mind-independent world. For him, concepts pick out referents in different respects, and all of them can possibly be successful.[2] Therefore, Brigandt is a scientific realist insofar as theoretical concepts refer to real entities in a mind-independent world. In fact, the main motivation to put forward the first component of concepts—i.e., reference—is to ensure that concepts enable successful verbal behaviour and interaction with the world (Brigandt 2011, p. 177).

Brigandt's commitment to reference reflects that his non-representational approach to conceptual change does not necessarily deny minimal scientific realism. Indeed, there is a need to distinguish between *non-representational* and *anti-representational* approaches to conceptual change. While the latter would give no role to reference in sematic analysis, the former approach to conceptual change maintains that, along with reference that plays a role, there are also other important factors that contribute to semantic analysis. A non-representational account of conceptual change can remain minimally realist as long as it retains the first two theses of the classical package. For Brigandt, not only does reference provide the realistic aspect of his position, but also the inferential roles of concepts (the second component of concepts) are connected to empirical evidence. (For an example of this feature go to the Chapter 3, section 2.2, where I discussed the concept of innateness). Indeed, Brigandt construes 'inferential roles' as not confined to intra-linguistic relations (narrow content). For Brigandt (2010b), contrary to usual interpretations of Inferential Role Semantics (IRS), the empirical content embodied in the concepts licenses the validity of a particular inference. Due to their empirical content, scientific concepts may appear in an inappropriate inference and make that inference invalid (p. 36). For example, as argued by Paul Griffiths (2002, p. 76), cognitive and behavioural scientists use the concept of 'innateness' with different meanings, which sometimes leads them to fallacious inferences.

In their entirety, Brigandt's writings suggest that he would agree with a version of the referential success thesis (T2). The only difference between Brigandt's reading of T2 and that

[1] Brigandt is less explicit in this regard, because his discussions are more about semantics than metaphysics.
[2] As Love and Brigandt (2017) assert, "there is more than one correct way to account for individuality means that there is more than one way to be an individual. The successful pairing of different characterizations of individuality with the diverse investigative and explanatory aims of biologists is an indicator of the *structure of reality qua* biological individuals" (p. 342, emphasis added).

of classical realists emerges from Brigandt's emphasis on the interest-relative process of reference determination. While the orthodox account assigns a determinate reference to a concept, Brigandt denies that there is a determinate fact of the matter as to what a given deployment of a concept refers to. Instead of suggesting that the reference essentially depends on the interests of an interpreter, this suggests that a concept contains a large and relatively open set of reference relations and inferential roles that enable us to explain the rationality of scientists by using specific parts of their conceptual understandings. For example, when early biologists used the concept of gene, they were not aware that their epistemic goals and inferential roles would pick out different objects in the near future. This is the inclusive-exclusive feature of concepts that I discussed in Chapter 1. However, this unorthodox idea is not inconsistent with T2, for it is not denying that successful scientific terms refer to *particular* entities in the external world.

For Brigandt, it is only the epistemic goals and values pursued with using a concept (EG), which is his third component of concepts that play the non-representational role. The main motivation for adding EG to IR and reference (as components of concept) is to explain the different ways in which conceptual practices can vary between individuals and over time. Therefore, Brigandt uses this element to explain rationality and progress without committing himself to a referential approach to conceptual change. Indeed, such a project should not be seen as providing a metaphysical theory of concepts (or concept identity) by proposing an anti-representational analysis of conceptual change. On different occasions, Brigandt describes his account as a 'heuristic tool' for resolving a specific philosophical question or as a 'methodological guideline' in analysing conceptual change (2006b, p. 26; 2012, p. 79). For Brigandt, suitability of a concept for scientific aims, which is set by the community of scientists, depends on empirical facts about the features of the world. This task is accomplished mainly by the reference component of concepts and partly by the total inferential roles that a language user associates with concepts. Therefore, I conclude that Brigandt is (at least a minimal) scientific realist and, accordingly, insofar as he acknowledges the relevance of reference in shaping successful practice with scientific concepts, his proposal is in accord with the first two theses of the classical package (T1 and T2).

3.2. Brigandt and Conceptual Progress

With regards to T3 and T4 on progress and rationality, respectively, Brigandt departs from the classical package. Brigandt puts forward an account of concepts according to which certain semantically relevant aspects of conceptual understanding are invoked to explain both the rationality and progress of conceptual change. He believes that the putative features of concepts are reference, inferential role (i.e., a set of contextually relevant aspects of understanding the reference), and the epistemic goals pursued when using a concept. Considering that reference has the lowest importance among these components, and the non-representational aspect (epistemic goals) has the highest importance, Brigandt tries to explain conceptual progress by focusing on the epistemic element.

In general, conceptual progress involves change in one of the components of conceptual understanding. However, since epistemic goals have the most stable status among other elements, the other two (Reference and IR) are more likely to change. The goal of these changes is to meet epistemic goals to a higher degree. Brigandt argues that, while the reference of 'gene' has changed from time to time, the concept of gene as an independent unit of thought has not always changed with the change of the reference. Rather, the reference of 'gene' has been modified in order to better meet the pre-established aim and values of scientists. Similarly, change of inferential roles is rational as long as it allows the concept to better meet the epistemic aims and values. Brigandt (2002) argues that "[c]hange in the concept's inferential role (definition) is rationally warranted if the new inferential role meets the concept's epistemic goal to a higher degree than the previous inferential role" (p. 87). Accordingly, the notion of progress is defined as the change of conceptual components in order to meet scientists' epistemic goals.

Of note, however, this framework works properly only when epistemic goals remain fixed through a change in IR or reference. Whenever epistemic goals undergo change, which is a necessity in explaining the dynamic of scientific change, explanation of progress gets complicated. In this case, progress is established through the extension of previous epistemic goals so as to improve the description of the relevant reference or to provide more accurate inferential roles. History of genetics contains episodes when the epistemic goals pursued using the concept of 'gene' have changed dramatically, while the reference and inferential role of the concept were not stable too. In this complicated case, Brigandt believes that, despite a gradual change in epistemic goals, the change in all three components was progressive, because all the changes were progressive in a stepwise fashion with respect to the immediately preceding epistemic goal, and any change in EG was grounded in immediately preceding stable reference or IR.[1] Therefore, although the epistemic component itself changes, it still remains as the standard scale of progress.

Accordingly, the most significant factors in the evaluating of conceptual progress and its extent are epistemic goals and values. In order to be regarded as progressive, the reference or the inferential roles of a typical concept should evolve to meet the epistemic goals in an improved way. Brigandt (2006b) views conceptual progress as increase in success' vis-à-vis meeting scientists' practical aims and values (p. 402). This brings to mind similarities with the 'pragmatic' account of progress that I defined previously. While the classical package suggests

[1] If a concept is used in different time slices t_0, t_1, t_2, … t_n, where t_0 and t_n significantly differ in all aspects of content, there is a stable component in each move that secures the rationality of that change (Brigandt 2010, p. 30). Let us assume that this move constitutes scientific progress: in the move from t_0 to t_1, there might be a change of reference and IR; however, due to a stable EG, it is counted as a progressive and rational move. However, from t_1 to t_2, we may have a partial change of EG and other changes. The gradual and partial modification of EG may occur in other steps to t_n, rendering EG_n completely different to EG_1. Although from t_{n-1} to t_n the EG might change, the stable components of reference and IR might preserve rationality and scientific progress in the use of the concept. This abstract representation of the process depicts a possible way whereby two versions of a concept may differ in all three components, but in a progressive and rational way.

that progress should be towards achieving more true beliefs (or verisimilitude), Brigandt holds an idea that progress is achieved by a better satisfaction of scientists' interests specified in terms of the questions that philosophers attribute to them.[1] For example, since the epistemic goal of molecular geneticists is to find the causal/mechanistic way that proteins bring about their products, any conceptual change that provides a better means to meet this interest counts as progressive. This is true regardless of the fact that scientists' goals and values might not aim at increasing truths.[2] Undoubtedly, Brigandt does not follow the strategy of referential stability that the classical realists *usually* take, for he believes that reference is not the most stable component of concepts. Brigandt (2011) concludes that his approach "aligns with a pragmatist perspective, since science is to a large extent a pragmatic enterprise and since scientists freely invoke scientific values and interests and defend their accounts in terms of their fruitfulness at meeting scientific aims" (p. 172).

Brigandt's account of progress and a full-blooded classical package would share the idea that scientific progress is constituted by achieving an epistemic goal. However, for the realists, the epistemic goal of scientists is identical to the objective aim of science—e.g. scientists might be interested in the impact of climate change on biological populations, to explain how the planets were formed, or to understand the way viruses are passed on, but for the scientific realists all of these are ways of getting closer to the truth of the mind-independent world. For Brigandt, however, epistemic goals are determined by the questions or interests that explain scientists' use of a concept. These questions and interests gain significance in a normative (or social) context, but not necessarily in a truth-seeking context. Importantly, aiming at the truth about the world does not necessarily require referential stability, which I defined as the common strategy of realists. Although most realist accounts of progress require an improvement in explaining *the same entity*, some realist accounts allow for stepping beyond this entity. For example, Kitcher (1993) defines conceptual progress as "when we adjust the boundaries of our categories to conform to kinds and when we are able to provide more adequate specifications of our referents" (p. 95) and adds that conceptual shifts in science are understood progressive if "involving improvements in the *reference potentials* of key terms" (p. 96, emphasis added). Having considered Kitcher's (1993) understanding of reference potentials,[3] we can see that having the same entities is not required for a full-blooded package. In fact, there is a possibility of an account of getting closer to the truth about the domain that does not constitute getting closer to the truth about the current entity

[1] Brigandt does not differentiate between the scientists' own goals and the goals that we, philosophers, attribute to them. He believes that epistemic goals and values might be implicitly followed by scientists, and it is philosophers' task to assign scientific goals to scientists so that to resolve different philosophical questions (2011, p. 183).

[2] According to Brigandt (2011), there is no difference between scientific and philosophical concepts in terms of the condition of progress—that of better meeting the goals: "My methodologically naturalistic strategy is to recommend the same approach for *philosophical* concepts" (p. 191).

[3] "[A] compendium of the ways in which the references of tokens of that term can be fixed for members of the community" (Kitcher 1993, p. 97).

that one's term refers to. This approach to conceptual progress is interesting, as it is referential, but not in a classical fashion (see Chapter 6 for further discussion).

3.3. Brigandt and Rationality

In Chapter 1, I argued that the key to explaining rationality and progress of conceptual change is continuity. The problem of incommensurability, among other issues, amounts to a discontinuous reading of the advancement of science. Therefore, many philosophers, including both realists and anti-realists, have sought to make a connection between the conceptual components of the successive concepts. The most common strategy in this respect has been using reference as the shared component. As I showed earlier, reference stability does not necessarily need to involve stability of referents, but the stability of reference potentials or descriptions that determine reference would suffice. Yet stability of reference is an ideal in explaining rational conceptual change, as it can explain continuity of conceptual change both in the subjective and objective respects. Reference is objectively shared among different individuals, and it must be determined under a common understanding of the relevant community.

However, Ingo Brigandt presents evidence in favour of the idea that, in some important cases of conceptual change, reference is not stable either causally or by descriptions determining reference. Accordingly, his account of rational conceptual change requires a more stable component—namely, epistemic goals. In Chapter 3, I discussed that, for Brigandt, rationality of change in our conceptual understandings is acquired by having a relatively stable set of epistemic goals. A fixed set of epistemic goals usually assures us of the identity of a single concept before and after a modification; in this case, the modification should be attributed to other peripheral aspects of conceptual competence. For example, Brigandt (2006b) argues that the concept of homology remained stable before and after the advent of the theory of evolution, as "there is substantial continuity throughout the 19th century, whereas particular definitions of homology… are largely peripheral to the content of the homology concept" (p. 9). Later, he adds that "considerations about reference turn out to be peripheral" (ibid, p. 23). This does not mean that the continuity of reference or inferential roles cannot explain rationality of conceptual change; however, Brigandt (2006b) believes that some important cases of conceptual change in biology should be explained using stability of epistemic goals and scientific interests.

Epistemic goals are so important in Brigandt's account of concepts because their variation or stability plays an important role in his understanding of conceptual change. According to his works, while scientists use different concepts of 'gene' (e.g., molecular, classical, etc.) based on different epistemic goals, the molecular concept of gene has remained stable for around four decades. This was so because, *despite substantial changes of reference and inferential roles*, the molecular concept of gene was used "to pursue the same epistemic goal" (Brigandt 2006b, p. 372). Therefore, all changes that have occurred for this single concept since the 1970s were connected together via this stable epistemic goal. This implies a very provocative account of concept identity where concepts are individuated on a very flexible ground.[1] In order to evaluate Brigandt's view of rationality, it is important to understand his account of concept identity, mainly because he tries to explain the continuity of conceptual modifications by the stability of epistemic goals.

Brigandt proposes an account of concept identity in which a concept can be individuated in multiple ways (constituting thus a *plural account* of concept identity). There is no single identity condition for concepts. The pluralism of concept identity enables individuation of the concept expressed by a word either based on epistemic goals (in response to a philosophical question), or on contextually relevant inferential roles (in response to another question) or, in some easy cases, based on the stability of reference.[2] In each case, depending on one's philosophical interests, concept individuation is based on different individuation criteria. Brigandt (2006b) argues: "I assume that any claim about concept identity has to lay out the particular individuation criteria used for this study and to explain what philosophical purposes are met by them" (p. 102). What this entails is that even when we consider one semantic component of a single concept, we still have different legitimate choices of individuation (Ibid.). Accordingly, different aspects of the overall conception might be relevant in different cases of concept individuation. Consequently, there is no unique or objective factor explaining concept identity.

This flexibility stems from the *pragmatic* nature of Brigandt's project where context is privileged over inferential and referential issues.[3] Brigandt (2006b) also notes that, along with epistemic factors, some more general considerations such as 'institutional', 'economic' and 'social factors' may also play a role in individuating scientific concepts (p. 109). Accordingly, for Brigandt (2006b), the justification of a change in the content of our concepts is based on other contextually relevant aspects of understanding that remain stable. Concepts are individuated by whatever aspects of meaning satisfy our various philosophical interests: "a particular way of individuating is justified as long as this particular account of content yields a philosophically successful study of the change of the term under consideration" (Ibid.).

[1] I addressed another flexible account of concept identity in Chapter 1 (James Lennox's account). The main advantage of these accounts is to secure the continuity of concepts while reference and meaning are changing. However, they are very odd in the philosophy of language.

[2] For example, Brigandt believes that a sound explanation of scientific progress in genetics required a simplified picture of the Mendelian concept of gene in order to show that it had no clear account of the material structure of genes (then, the molecular concept made progress by adding this new information to the concept). In contrast, when it comes to the explanation of rational change, Brigandt individuates the concept of gene in a different way. In this case, the Mendelian concept should be understood as containing some minor material understandings of genes (then, by tracing a material understanding of the structure of genes back to the classical accounts, he is ready to explain rationality of change).

[3] This opens up the possibility of two scientists having the same concept even if they have disagreements on both reference and inferential roles, merely due to having the same scientific aims and values. Indeed, "epistemic goal was the most salient semantic property for concept individuation" (Brigandt 2006a, p. 389). In another passage, he substitutes the phrase 'the most salient' with 'the most significant'.

Brigandt uses the stability of epistemic goals, which is determined depending on the interpreters' interests, to argue for concept identity and then rational conceptual change. This does not mean that scientists are not pursuing an epistemic goal, or that interpreters are allowed to attribute some arbitrary goals in place of the scientists' epistemic goals. Rather, the pragmatic sense of Brigandt's theory grants interpreters some flexibility to attribute different epistemic goals based on their philosophical interests. In this respect, Brigandt explicitly defends a very loose account of concept identity where what is more important than individuating a concept are the benefits that this task has for philosophers. Thus, while the continuity needed to explain the rationality of conceptual change is not necessarily justified by the sameness of a fixed conceptual component, it is enough to show "why a context-sensitive use of a term can be beneficial to scientific practice" (Brigandt 2012, p. 94). Similarly, the question of concept identity loses its importance, in that what enables successful communication is sufficient similarity in the totality of inferential roles or epistemic goals. Therefore, Brigandt (2012) goes beyond a mere "pluralistic account of concept individuation", and indeed argues against the philosophical importance of this issue:

> Since on my account a term has three semantic properties (reference, inferential role, epistemic goal) and can change in each of them, I do not think that there is a unique account of concept individuation. I consider it to be philosophically more important to account for change in any of a term's semantic properties than to debate whether this amounts to a separate concept being used. (p. 88)

Brigandt (2006b) explicitly substitutes the notion of *concept identity* with the notion of *concept similarity*, as there are always some similarities and differences between two concepts (p. 104). Similarity of two concepts is based on their putative conceptual components (i.e., reference, IR and EG). Hence, rather than proposing a theory of concept *identity*, Brigandt's account is more about the *similarity of concepts* with respect to the three postulated features of concepts. The aforementioned substitution resonates with what Ned Block (1987) and Gilbert Harman (1993) have defended as semantic similarity for the possession of the same concepts. According to Block (1987), the 'crude dichotomy of same/different meaning' should be abandoned in favour of a 'multidimensional gradient of similarity of meaning' (p. 624). Of note, however, Brigandt's framework does not work in the same way. The problem emerges for Brigandt when, to his account of concept similarity, he adds an interest-relative element. In fact, while Brigandt's account of concept similarity contains no objective or unique similarity metrics, considering a particular usage and context would suffice for the interpreters to take those concepts sufficiently similar. For Brigandt (2006b), this means that it is impossible to define an overall similarity account considering all inferential roles (p. 104, p. 108). Rather, the ultimate criterion that determines whether two concepts are similar is successful communication.

Returning to our main question of rational conceptual change, Brigandt tries to explain the rationality of his biological case studies by continuity of epistemic goals. However, since there is no 'single overall similarity metrics' over this component, the decision on whether or not a particular case of conceptual change is rational depends on both the conversational context of the speakers and on external interpreter's theoretical interests. The case is even worse, as deciding on whether or not a particular example even counts as a case of conceptual change is not fixed. For Brigandt, concepts are individuated relative to the contingent philosophical interests of an interpreter—i.e., which explanatory features of conceptual practices happen to interest us in explaining the communicative situation. Therefore, the project of explaining the rationality of conceptual change becomes one of finding why it is beneficial (i.e., in the interest of the speaker) to revise certain features of concepts.

Assuming this interest-relative account of conceptual change, the goal of explaining rationality becomes *too easy* to accomplish. As long as an interpreter can identify *any* reason for changing a concept, no matter how pragmatic or contingent, the change can be counted as a rational scientific change. In fact, Brigandt's lax conception of rationality makes any conceptual change rational as long as there are shared aims or values in using a concept. There are at least two reasons why his claim is excessively lax and easy-going. First, the sameness of epistemic goals is determined in a communicative context, where successful communication is taken to be a sign for sharing a 'sufficient enough' set of inferential roles and epistemic goals (Brigandt 2006b, p. 389).[1] Second, since epistemic goals themselves are not fixed and objectively determined entities, they are at least partly relative to interpreters' philosophical interests. Indeed, there is no fixed and commonly accepted epistemic goal even for a particular scientific field. In historical studies, several epistemic goals could be attributed to eighteenth century biologists. Although these epistemic goals are supposed to be implicitly pursued by scientists, it is the interpreter's role to attribute goals to them based on different philosophical purposes. This uncertainty makes it easy for a philosopher to find, or create, a shared goal when s/he is required to show the rationality of a particular change.

In sum, my proposal is that Brigandt's pragmatic account of rational continuity, which isolates aspects of concepts so to answer some particular questions in philosophy, is not consistent with what we require of a full-blooded realist account. While this might be a beneficial pragmatic account in terms of explaining rational conceptual change, an adequate realist account should explain why scientists are subjectively and objectively justified to change their concepts. For Brigandt, the rationality of change can be secured by sharing a common epistemic goal or value among old and new concept users. He argues: "[a] stable epistemic goal causally determines and rationally justifies historical change in inferential role and reference" (Brigandt 2012, p. 98). In fact, the main aim in adding a third conceptual property to the components of conceptual competence is to account for the rationality of change. As I described T4, the difference between a realist and an anti-realist package is not the fact that one argues for the rational change, and the other against it. Philosophers such as Kuhn, Laudan and Van Fraassen have not rejected the rationality of scientific change altogether.

[1] Brigandt (2006b) adds that "concept possession consists in having a minimal set of inferential dispositions and epistemic abilities so as to be able to meaningfully communicate with other scientists and to conduct scientific research" (p. 389, emphasis added).

Rather, what they have denied is the marriage of notions such as rationality and truth.[1] These philosophers do indeed hold a version of rationality (and even progress) for science in terms of subjective justifiability (T4), but not the one proposed by the classical package—the one that focuses, in addition to a non-accidental connection, on the truth about the structure of world. In what follows, I compare two anti-realist accounts of change with the one defended by Brigandt.

Van Fraassen recognizes a logical gap between prior and posterior epistemic states during scientific revolutions, which requires a rational explanation. Van Fraassen (2002) asserts that this logical gap is between epistemic states "when they differ on what counts as intelligible" (p. 108). In order to secure the *rationality* of change, van Fraassen bridges them with a Sartrean account of emotions (that focuses on the role that emotions can play in cognition and theory change, rather than views emotions as mere psychological reactions). According to van Fraassen (2002), "the change that makes change possible goes so deep that it foregoes any prior rationale. It is a change through (some analogue of) emotion, playing the role Sartre described, in which old values and views are let go" (p. 151). When scientists encounter unintelligible competing concepts or theories, they feel dissatisfied or frustrated. However, as soon as these feelings depart, scientists are ready to frame or form new concepts as rationally acceptable rivals. By the intervention of emotions, we perceive the world differently and are prepared to change our concepts. In addition, holding an emotional element that bridges old and new scientific accounts is not inconsistent with the empiricism of van Fraassen's philosophy, for he acknowledges that emotions also have a cognitive role. The bridging emotions are "not a postulated hidden variable but something encountered in experience" (Ibid., p. 108).

The crucial issue for our discussion at this point is that the transforming role that van Fraassen gives to emotions is based on the fact that emotions are properties of individuals, rather than objective facts of matter. These emotions *might* be directed toward something outside consciousness (i.e., they might be functional/teleological), but still they are non-reflective and "inhabit the mind" (Kindi 2011, p. 332). Assuming that van Fraassen's (2002) usage of emotions is internally consistent, although some deny it,[2] his account of change does not explain rational conceptual change in the way that I described a full-blooded realist account. Indeed, while this account may satisfy the justifiability condition of T4, since appealing to emotions provides no objective warrant for rational change, it is not in accord with T4.[3] It is a very lax conception of rationality, as compared with what I defined for the classical package,

[1] Laudan 1977, p. 127; Kuhn 1970, p. 206. Van Fraassen (1989, p. 177) argues that truth about unobservable entities is undecidable. Also, he adds that there should be a balance between 'desire to truth' and 'avoiding error'. This balance is not made on an *objective* basis (van Fraassen 2002, p. 87), but it is "contextually qualified by our interests and values" (Ibid. p. 90).

[2] Cf. Kindi (2011).

[3] As long as van Fraassen (2002) favours a teleological explanation of *rationality* of scientific change in terms of interpreting scientists' behaviour in a charity-based way, his account is close to Davidson's (1973) account of rationality.

as it views any change as rational as long as it does not lead to an incoherent set of beliefs (Psillos 2007, p. 156).

Brigandt's account is similar to van Fraassen's in that, while both accounts might be successful in meeting the justifiability condition of T4, their theories do not touch the non-accidental access to truth about the world. Furthermore, in their accounts of rationality, both Brigandt and van Fraassen are neutral about truth. They do not deny that statements about unobservable entities *may be* true, they both hold that no one has epistemic access to claim whether these statements are in fact true or false. Furthermore, in van Fraassen's (2002) account, the notion of 'emotions' plays a role similar to that of Brigandt's 'epistemic goals and values', in that both notions impose non-representational constraints on theory change, and both are used to explain rational continuity across scientific change. Brigandt (2011) explicitly asserts that "unlike truth-conditional features of meaning, the epistemic goal does not consist in a belief about states of the world—it is not even a desire as to how aspects of the world studied by science should be like" (p. 183). According to Brigandt (2011), there are no higher norms, beyond social and pragmatic factors, that can vindicate the rationality of an epistemic goal change (assuming that these goals are not fully or directly linked to the truth). Therefore, the notion of truth plays no role in Brigandt's (2011) account of progress and rationality. For instance, as he argues, "I do not view *truth* as a regulative ideal in science. […] truth is not an aim in itself" (Ibid., p. 188).

An even more relevant precursor for Brigandt's project is Thomas Kuhn. Retaining the rationality of scientific changes both within and across scientific revolutions, Kuhn's (1962) account is not framed in terms of truth about the structure of reality, which is a must for a full-blooded realist package. Rather, Kuhn (1977) tries to respond to the charge of irrationality of scientific change and relativism by invoking some *shared* and *permanent* criteria[1] (e.g., simplicity, fruitfulness, etc.[2]) that govern the puzzle-solving spirit of science (p. 335). Following Kuhn's (1962, 1977, 2000) approach, there are some inter-paradigm 'values' governing a rational scientific enterprise. We can see that even Kuhn takes science as a clear case of *rational* enquiry. However, such an account of rationality misses the point of the real challenge, as there is no doubt that science as a whole is in the process of becoming a more efficient instrument for solving scientists' puzzles by means of providing more powerful and simpler tools. The controversial point of this challenge is the further idea that the new scientific theory should count as a privileged exemplar of rational inquiry into nature (Friedman 2002, p. 184).

[1] Immanuel Kant was among the first philosophers to develop such common criteria to preserve rationality. According to him there exist universal and fixed cognitive abilities, such as 'forms', 'pure concepts' or 'categories', that govern the rationality of human thoughts.

[2] Kuhn (2000) writes: "[T]he rationality of the standard list of criteria for evaluating scientific belief is obvious. Accuracy, precision, scope, simplicity, fruitfulness, consistency, and so on, simply are the criteria which puzzle solvers must weigh in deciding whether or not a given puzzle about the match between phenomena and belief has been solved" (p. 251).

Both Kuhn's and van Fraassen's accounts of rationality lacks the positive component in rational change. They might be successful in identifying the fully irrational changes, which should be very rare, but they are unable to propose an obligatory account of rational change: an account that explains why scientists are justified in selecting a certain choice based on the objective evidence available. Although Kuhn, van Fraassen and Brigandt all deny a full-blooded realist account of rational conceptual change, it would be a category mistake to suppose they all share the same understanding of 'rationality'. I briefly set out the difference between these three accounts.

While Kuhn acknowledges the variation of conceptual frameworks in each paradigm, in order to find a value universally held constant throughout the development of science, he appeals to common values accepted by all paradigms (i.e., simplicity, fruitfulness, etc.). These *inter*-paradigm values are supposed to bridge the gap between the problematic disagreements of revolutionary breaks.[1] Therefore, whenever no shared (neutral) framework between rival scientific paradigms (theories or concepts) is in place, *rational* choice is grounded on these general criteria that "function as values" (Kuhn 1970, p. 199). Kuhn is aware that these values are not of "logical or mathematical" nature but can be applied differently in favour of different choices. Basically, in the cases that there is no shared framework of criteria, these values are used by scientific parties to "persuade" or "convert" the other parties. Hence, as argued by Friedman (2001, p. 54), Kuhn's notion of 'rational' conceptual change refers to scientists' capacity to engage in argumentative deliberation or reasoning aimed at bringing about an agreement or consensus between paradigms with different concepts.

For Brigandt, the 'rationality' of conceptual change is attained if scientists share the same goal in particular uses of a concept in a given context. Brigandt's notion of rationality refers to the scientists' capacity to engage in effective means-ends deliberation or reasoning aimed at maximizing their chances of success in pursuing an already determined goal or value. A good sign of this rationality is successful communication. According to this account, we can talk about rational choice when two scientific parties share the same goal and are engaged in a successful communication, with both parties trying to provide a better explanation for the same puzzle. As I explained in Chapter 3, for Brigandt, sufficient similarity in epistemic goals and values is a necessary condition for successful scientific practice and communication. However, these shared goals do not necessarily include identification of an accurate representation of the external world. In fact, the only constraint on the shared epistemic goals that secures rationality is that these goals provide the basis for effective communication.

[1] Friedman's (2002, p. 50-56) objection to Kuhn is that any judgment to assess the satisfaction about whether these values are satisfied will itself depend on some particular conceptual frameworks, which is invoked by a specific paradigm. However, those conceptual frameworks cannot be shared between paradigms. Therefore, there is no neutral standpoint from which to judge the success of different frameworks, even when they share the same overarching theoretical values of simplicity, fruitfulness, and so forth. Thus, the problem of conceptual relativism cannot be responded to this way (also see Friedman 2001, p. 55; 2008, p. 252).

Unsurprisingly, both Kuhn's and van Fraassen's accounts of rationality diverge from the realist package that I defined above. But it is a surprise that Brigandt's account does not cohere with that package. While Brigandt has never argued for scientific realism or a theory of reference, his project grows as a response to the conceptual change problems raised by Kuhn and Feyerabend. Brigandt tries to give an account of rational conceptual change that is beyond the referential approach and is devoid of the problems such as incommensurability or relativism. His central tenet, in this discussion, is to propose a conceptual framework where rationality and progress of science are explainable, while Kuhn's explanation is not satisfactory.

In sum, in this section, I have argued that Brigandt's account is not closer to the classical package than the accounts of his anti-realist rivals. Brigandt's account of rationality lacks the obligatory condition, and his account of progress is not defined in terms of getting closer to the truth. Furthermore, epistemic goals—a central element of Brigandt's framework—are completely interest relative. While epistemic goals are generated by scientists and may have some empirical relations, they are non-representational elements reflecting scientists' interests. It is apart from my own critique that these epistemic goals are determined relative to interpreters. Therefore, Brigandt's account is at most a minimal realist.

4- Objecting to the Theories of Reference

As I previously described in Chapter 1 following Scheffler (1967), one of the first responses to the problematic issues of theory change was to focus on reference. The proponents of this approach hold that a comparison of two theories in order to explain the rationality of change is based on appeal to reference. The main tenet of the approach is that "theories are comparable if they share reference" (Sankey 1994, p. 36). Locating referential stability as the gist of the classical package raises the issue of how to determine the reference of different scientific terms and concepts through *a good theory of reference determination*.

Brigandt is primarily concerned with arguing against a referential approach to conceptual change. Consequently, in explaining the problematic issues concerning conceptual change, even for natural kind terms, he is strongly against the current theories of reference determination. For Brigandt, none of the causal, descriptive or causal-descriptive theories of reference can adequately explain the progress and rationality of change—in particular, complicated biological case studies, notably 'gene' and 'homology'. In what follows, I describe his objections and comment on them.

Having considered biological case studies, Brigandt correctly concludes that, at least in some cases, the standard methods of reference determination are *insufficient* to fix reference (let us call this 'the weak claim'). Therefore, in addition to the elements posited by standard theories of reference, other elements are also needed. In this respect, Brigandt (2005) notes: "My proposal is that even in the case of natural kind concepts we need a broader account of reference fixing" (p. 12). Such critical elements to add to the causal and descriptive ones are epistemic goals and values. While not referential in essence, these elements contribute to reference determination. For example, both phylogenetic and developmental biologists use the term 'homologous' to refer to almost the same (relation between) entities[1] (e.g., human's arm and bat's wing). However, the fact that their epistemic goals differ—one of them considering homologues for classificatory and comparative purposes, the other seeking an explanation of how structures originate in evolutionary developments—leads them to associate *different concepts with this term* (see Chapter 2). Thus, as there is a difference-making component that plays a role in different usages of 'homology', the developmental and phylogenetic concepts cannot be distinguished by the traditional theories of reference.

However, Brigandt does not restrict his proposal to a mere request for another (broader) theory of reference determination. Instead, he adds a stronger claim concerning the insufficiency of the *general notion* of "the theories of reference" or the referential approach to conceptual change (let us call this 'the strong claim'). In his view, we should give up the useless referential approach and take a non-representational approach to conceptual change by focusing on elements other than reference. Disparaging reference in explaining conceptual change as 'the least important' component is a clear consequence of Brigandt's distrust in theories of reference. The reason is that, for him, unlike the standard theories of reference that take causal and descriptive conditions as the only relevant conditions, the set of elements that contribute to reference determination are 'relatively open' and, therefore, not amenable to being gathered into a theory of reference determination: "[T]he set of conditions that determine reference of both a term type and *any of its tokens* is relatively open and unbounded" (2006b, p. 88, emphasis in the original). These conditions may include contextual elements, such as the speakers' IR and EG, scientific discussions where the term was used, and facts about objective external systems that this scientific discourse is roughly tracking. Definitely, this does not prevent a particular use of a concept from having a fixed reference relative to the context of use and interests of the interpreter; instead, the openness of conditions appeals to an epistemic understanding of concepts where some reference-fixing conditions are prioritized by the interpreters due to their specific questions.

To summarize Brigandt's objection: in explaining conceptual change, a typical theory of reference determination has two interconnected deficiencies. First, it is not the case that causal and descriptive elements are the only factors that contribute to reference determination; rather, other contextual and pragmatic conditions should also be taken into account. There is evidence that biological case studies have raised issues in which the assignment of reference to a concept requires more constraints than the standard descriptive or causal factors. This weak claim of Brigandt leads to an appeal for a 'broader theory of

[1] Serial homology where the developmental account includes it in the reference is an exception; where the phylogenetic account excludes this class of referents. Serial homology is a notion attributed to similar structures *within an individual organism*. Examples are hairs in mammals, leaves in plants or vertebrae in vertebrates where the same structure multiply occurs. Owen defines it as "representative or repetitive relation in the segments of the same organism" (in Webster's Revised Unabridged Dictionary, G. & C. Merriam, 1913 at http://www.finedictionary.com/Serial%20homology.html [accessed 10th December 2019]).

reference'. However, according to the strong claim, since those additional conditions are relatively open—i.e., they appeal to the interpreters' explanatory interests, thus allowing interpreters to privilege different elements of the speaker's context of use as relevant to fixing reference—a reference-focused approach to conceptual change should be abandoned. Brigandt argues that current theories of reference determination cannot successfully explain the rationality and progress of conceptual change in the history of science. For now, I will go along with the weak claim that the current theories of reference determination are insufficient; yet, in Chapter 5, I will propose the outline of a modified theory.

Regarding the current standard theories of reference, it appears that none of them can fully address the elements that Brigandt ascribes to certain biological concepts. One standard causal theory of reference for natural kinds goes back to Putnam (1975). According to Putnam (1975), we select a sample of a kind by ostension or stereotypical description, and then our term refers to whatever has the (internal) structure of the kind that this sample belongs to. Some philosophers also use similar argumentation for unobservable kinds: that the reference is the kind causally responsible for observations (Newton-Smith 1981; Sterelny 1983). However, the problem with these sorts of theories is that they cannot address non-causal factors (e.g., fully descriptive ones or contextual conditions) that contribute to reference fixing. For example, the serial homology concept (i.e., similar structures within the segments of *the same* organism) in two different accounts of homology—namely, phylogenetic and developmental accounts—refers to different sets of referents. This referential difference occurs due to the difference in certain non-representational factors (see Chapter 3). The proponents of causal theories may respond to this case by appealing to a revised idea that phylogenetic 'homology' picks out samples excluding serial homology, while the developmental term picks out samples that belong to serial homologues. Stanford and Kitcher (2000) put forward an idea that three elements of foils, samples and stereotypical properties are involved in reference determination. Foils are added in order to exclude or include a set of referents from concepts' extension (leading to the differentiation of phylogenetic and developmental concepts in terms of serial homology). However, this proposal does not work, as it does not always fit biological practice. Adding serial homology as foils was not important for early concept users. The insistence on excluding serial homologues from the referents of phylogenetic 'homology' is a recent issue. The difference in extension between phylogenetic and developmental concepts is a *consequence* of an earlier dispute on epistemic goals that eventually led to a differentiation between two different concepts in the early 20th century. Therefore, how could Stanford and Kitcher's (2000) account explain the earlier difference between these two concepts? Brigandt's project requires a theory that would adequately address these epistemic issues.[1]

[1] Some sophisticated causal theories of reference may accept that pragmatic or contextual factors impact or influence our intuitions about reference determination; however, Brigandt seeks an independent position for those non-representational issues as reference determinants.

Descriptive theories of reference are likely to be insufficient in addressing such issues. The standard interpretation of descriptive theories is that the descriptive content that language users associate with a concept determines the referent. However, this account overlooks the non-representational factors that play a role in reference determination. For standard descriptive theories, the descriptions are *only about the referents* (e.g., 'gold' refers to a yellow metal), while what Brigandt (2005) needs from an adequate theory should also incorporate descriptions concerning the epistemic aims and values of scientists, or even some pragmatic descriptions about things other than the referent (p. 11). In the case of homology, the descriptions that distinguish phylogenetic and developmental accounts are not merely about the referents, but also reflect non-representational interests in referents.

The aforementioned limitation characterises even the most up-to-date versions of descriptivism (e.g., causal-descriptive theories of reference). For example, Kitcher's (1982, 1993) account of reference invokes both descriptive and causal factors in that the intention of a speaker determines which factors are salient in picking out the appropriate referent (cf. Devitt and Sterelny 1999). For instance, Priestly's 'dephlogisticated air' could refer to oxygen on one occasion, and could be an empty term in another context. Yet, as long as these sophisticated theories continue differentiating concepts based on their referents, these theories still follow the referential approach in the assessment of conceptual change in science. Accordingly, they cannot distinguish two concepts with the same extension but different epistemic goals. Kitcher's sophisticated account does not include the *philosophical interests* of interpreters that contribute to attributing a referent to one's token of a term in addition to her/his personal intentions.[1] Brigandt (2005) notes:

> I suggested that the philosophical interests that underlie reference ascription and the study of conceptual change may bear on which features are reference determining in a particular case. Thus, reference need not always be fully and unambiguously fixed by the speaker and her physical and social environment. (p. 22)

Therefore, a crucial difference between Kitcher's contextualism in reference determination and what Brigandt requires of such a theory is that while the former involves the context of the speaker's utterance in fixing the reference, the latter involves two layers of contextualism: context of utterance and the context of philosophical analysis. Thus, in Kitcher's account, if one fixes the context of utterance in addition to the descriptive and causal conditions, a unique reference can be singled out. However, in Brigandt's account, all the descriptive, causal and context-dependent conditions do not suffice to pick out a single referent, for 'the explanatory interests of the person ascribing reference' may contribute to reference determination (Brigandt 2005, p. 22). This is the rationale of Brigandt's strong claim that not

[1] There is another problematic issue for the standard causal-descriptive theories of reference. Brigandt (2006a) addresses this issue by remarking that, although both causal and descriptive elements are invoked for legitimate reasons, there is no clear-cut and reasonable way to avoid the problems inherited from each of these conditions alone. Specifically, Brigandt (2006a) writes: "a theory of reference that invokes both causal and descriptive factors could in principle inherit the problems of the purely causal theory in addition to the problems of the purely descriptive theory of reference" (p. 19).

only epistemic aims and values of scientists should be considered in explaining conceptual change, but also that trying to explain conceptual change with a non-relativised, objective account of reference determination is futile.

In Chapter 6, I will propose a framework for an overall theory of reference determination as a means to overcome the insufficiencies that Brigandt's weak claim correctly identifies. However, my aim in this chapter was to show that Brigandt's non-representational approach essentially discredits the referential strategy usually assumed by the classical package to explain conceptual change. As discussed previously, the referential approach to conceptual change (i.e., the idea that referential stability provides the continuity required for conceptual progress and rationality) is not a necessary component of the classical package; however, this approach is crucial for most realist philosophers who are concerned with conceptual change. It is traditional among philosophers of science to view problems such as incommensurability from the semantic and referential points of view. For example, Kitcher (1978) explicitly asserts that "[t]he remedy is to begin with the notion of reference" (p. 522). In contrast to this long-established tradition, Brigandt (2006b) views the problem of "meaning incommensurability as an epistemic challenge", rather than as a mere referential challenge. According to Brigandt (2006b), although the problem of incommensurability involves semantic considerations, epistemic implications of that problem are more important, and so "considerations about reference turn out to be peripheral" (p. 22). In short, he thinks that, if we want to explain the rational continuity involved in scientific progress, we need to focus primarily on the epistemic goals and inferential roles associated with theoretical terms, rather than on external facts about the reference.

The fact that scientific realists seek to argue for referential stability (or referential overlap) does not stem from their neglect of reference change[1]—rather, the underlying reason is that scientific realists have sought to explain scientific enterprise as a truth-seeking practice. The most straight-forward way to connect scientific language and the truth about mind-independent system is through reference.[2] Accordingly, an astute scientific realist should seek to both (1) defend a referential framework where the theoretical concepts of a scientific theory represent specific elements of an objective external system of entities and properties, and (2) explain the rationality and advancement in science, even in those cases where theoretical terms undergo a radical reference change in the history of science. While, for scientific realists, these two conditions are connected, Brigandt's theory is aligned only with the second task, as his concerns include explaining the rationality of scientific change. However, Brigandt criticizes the referential approach. The more he proceeds in arguing for the second task, the weaker his commitments become to the first. For Brigandt (2006b), scientific "language does not (always) mirror the world yet is fully engaged with the world"

[1] As Sankey (2000) notes, "[g]iven that reference may vary in post-baptismal use, I allow that reference may change in the transition between theories" (p. 134).

[2] Some scientific realists have argued for versions of scientific realism without appealing to reference (e.g., Cruse and Papineau 2002).

(p. 68). To this end, the first aim of language is to underwrite successful practice (e.g., conducting experiments, giving explanations and discoveries) and "representational notions such as referents and truth-values are to be assigned in a subsequent step only" (Ibid.). This is the main point of a non-representational approach to conceptual change—the one that prioritizes contextual and pragmatic conditions over the referential ones. In Chapter 6, I will seek a theory of reference determination that would satisfy both a referential approach and a realist account of conceptual change in the light of our biological case studies.

5- Conclusion

In this chapter, I aimed to show that a non-representational approach to conceptual change is not fully aligned with the traditional scientific realist package for conceptual change. The detailed description of the classical package provided in this chapter was not meant to defend this account—rather, my aim was to show that a non-representational approach cannot vindicate the sorts of theses endorsed by scientific realists. Among the most important conditions that a non-representational approach cannot vindicate are commitments to the notion of reference in an account of conceptual change and of truth in a realist account of progress (i.e., science that aims to get closer to the truth about mind-independent reality).

Brigandt's account of conceptual change differs from what a full-blooded scientific realist seeks to vindicate with the classical package. Specifically, Brigandt's account of rational conceptual change in science is *non-representational, non-truth conditional, pragmatic and non-referential*. First, it is *non-representational* because epistemic goals, the main factor in explaining conceptual change, do not represent external reality. Instead, epistemic goals reflect an interpreter's perspective on the scientists' interests, and these goals may be shaped by social or other pragmatic factors about the scientist's context. Second, Brigandt's account is *non-truth conditional* because epistemic goals cannot be assessed by truth conditions: "More importantly, unlike truth-conditional features of meaning, the epistemic goal does not consist in a belief *about states of the world*—it is not even a desire as to how aspects of the world studied by science should be like" (Brigandt 2011, p. 183). Third, Brigandt's account is *pragmatic* in virtue of his assuming conceptual content to be implicit in linguistic practice. Brigandt (2006b) notes "I assume that a term obtains its semantic properties […] in virtue of its overall use" (p. 5). The epistemic goals pursued by scientific concepts are not consciously entertained by scientists but are reflected in their practice. The kind of reasons Brigandt provides for his account (e.g., using the notion of fruitfulness instead of metaphysical notions) and the problem-solving behaviour that he attributes to science are other reasons for being pragmatic: "science is to a large extent a pragmatic enterprise and […] scientists freely invoke scientific values and interests and defend their accounts in terms of their fruitfulness at meeting scientific aims" (Ibid, p. 172). Finally, Brigandt's account of conceptual change is also *non-referential*, since his explanation of rationality and progress through conceptual change does not turn on referential stability. In fact, reference is the 'least important' factor in conceptual analysis, while the epistemic goal is the most important one: "Overall, reference was the least important semantic property for these philosophical tasks [explaining successful practice of concepts and semantic change]. While many traditional accounts of conceptual change have focused on reference […]" (Brigandt 2006b, p. 402).

I conclude that Brigandt's explanation of conceptual change, which is a promising recent version of the non-representational approach, is not a full-blooded realist one, even though it seeks to address the problems that anti-realists raised for realism, such as incommensurability and scientific progress. Rejecting scientific anti-realism does not suffice for a defence of full-blooded scientific realism. Brigandt is a minimal scientific realist insofar as he believes in the reality of mind-independent world and referential success of the key scientific terms. As a minimal realist, he agrees that certain features of concepts, such as reference and inferential role, reflect the objective structure of the world; he also agrees that whether or not a concept is suitable for a scientific goal depends in part on the empirical facts about that structure. However, by denying that getting closer to the truth about mind-independent reality is the aim of science, and by rejecting the idea that the only standard of success for scientific concepts, models and representations is to capture the structure of objective reality, Brigandt departs from the classical package of scientific realism for conceptual change Therefore, we can conclude that, since Brigandt's basic notions of scientific progress and rationality are defined in a non-realist sense, his account of conceptual change is *non-realist*. Brigandt defines progress as 'increase in success', construed in this case as a pragmatic success in solving puzzles. Furthermore, the kind of rationality Brigandt defends is purely pragmatic rationality (as opposed to truth-seeking epistemic rationality). Conceptual change is rational, he claims, if *any* semantically relevant component is shared among concept users before and after the change. All that is required is sufficient continuity for successful communication (relative to some interpretive standard). In sum, Brigandt's pragmatic standards for rational continuity and progress in science are very different from the full-blooded realist's account of rationality as an effort to get closer to the truth about mind-independent reality and progress as success in this endeavour.

Chapter 5

Against Interest-Relativity

1- Introduction

In Chapter 4, I evaluated Brigandt's account of conceptual change and showed that it is not in accord with a realist package for explaining conceptual change. The chapter concluded with a discussion of two serious objections raised by Brigandt against almost all theories of reference determination. To recapitulate, Brigandt's weak claim is that the current theories of reference determination cannot explain context variability in fixing the reference of scientific terms. I accept the weak claim, along with Brigandt's crucial point that sometimes the non-representational factors, such as epistemic goals, play an important role in fixing the meaning and reference of scientific concepts. However, Brigandt went further by making a stronger claim—namely, that the problem of theories of reference cannot be fixed on *any* representational approach. If these two objections are sound, then Brigandt has a good reason to abandon the referential approach to conceptual change in favour of a non-representational approach.

In this chapter, I critique Brigandt's strong claim by outlining the reasons why his account cannot successfully explain conceptual change. I start by identifying how Brigandt's account of conceptual stability essentially depends on the contingent explanatory interests of an outside interpreter. I then argue that this approach cannot provide an adequate explanation of concept stability. On Brigandt's approach, there is no objective fact of the matter about whether meaning and reference remain stable over time or between speakers. Given that interpreters' interests fix semantically relevant properties of concepts the rational continuity of scientific change depends on the context of interpretation. In particular, Brigandt's account of conceptual change cannot provide any genuine normative account of rationality where there are better or worse options to choose from. Consequently, explaining rationality through conceptual change becomes a matter of *ex post facto* rationalization (which runs the risk of becoming trivial). I argue that this approach takes the notion of scientific rationality in a very minimal form.

At the same time, however, I accept the importance of some epistemic goals in reference determination. Therefore, I need a positive framework that not only recognizes the importance of epistemic goals and contextual factors in reference fixing, but also explains successful scientific communication by providing a rational guidance through scientific change. In the concluding section of this chapter, I introduce a set of *desiderata* for a positive framework of reference fixing. These *desiderata* reflect my concerns in providing an objective stability of meaning, explaining successful communication using a concept in different contexts, and giving a rational guidance (norm) in selecting objectively better alternatives. Furthermore, my positive framework (see Chapter 6) will explain how science in general is guided to a more accurate isomorphism between the total contextually precisified set of concepts and the total causal system.

2- The Weak and Strong Claims

The main target of Brigandt's weak and strong claims against the current theories of reference, reviewed in Chapter 4, is the sufficiency of current theories of reference, including the causal, descriptive and hybrid ones. The classical realist approach to conceptual change begins with the notion of reference.[1] In this approach, it is typically claimed that conceptual change is rational and progressive only if the reference of a concept remains the same throughout theory change. This is why philosophers who have responded to the problem of incommensurability usually started with a theory of reference. Basically, proposing *a good theory of reference* paves the way towards an account of conceptual change that is consistent with the classical scientific realism. In this context, it makes perfect sense why Brigandt, in promoting his non-representational account of conceptual change, targets theories of reference. In other words, the insufficiency of the referential approach would be a good reason to step beyond reference and seek a non-representational response.

[1] This is a rephrasing of Kitcher's (1978) famous quotation in explaining conceptual change and the problems it brings about: "[T]he remedy is to begin with the notion of reference" (p. 522).

Let us summarize *the weak claim.* It is commonly assumed that rational and progressive conceptual change requires some sort of continuity.[1] Almost all accounts of rational conceptual change aimed to find a shared conceptual property, such as reference,[2] that would persist through scientific changes. Accordingly, each version of a concept was expected to be connected to its immediate predecessor via some conceptual property. Acknowledging this requirement of continuity, the proponents of the non-representational approach argued however that reference cannot be that shared component. In this thesis, I explained how Brigandt's biological case studies show that the current theories of reference are insufficient in explaining conceptual change. The main reason for this failure is that some important epistemic and contextual factors, which are not addressed in those theories, contribute to reference fixing. In addition, the references of some biological terms can radically and rapidly change depending on the context of use, making it very difficult to satisfy the requirement of referential continuity. Recent studies in the history of science have demonstrated that some concepts have 'floating reference', meaning that, with the development of scientific knowledge, different natural kinds can be denoted by the same term (Weber 2005, p. 227). This places the referential approach to conceptual change in a weak position, as current theories of reference fail to pick out the reference of some scientific concepts.

While I agree with this weak claim and endorse the necessity of finding a broader understanding of reference determination, Brigandt does not stop here. He further argues that *the set of conditions that determine the reference of a scientific term is relatively open and unbounded.* These conditions include contextual and pragmatic factors, such as the context of use and the purpose of an interpreter in assigning reference to a single *token* use of a term. Therefore, there are no limited or fixed conditions relevant to reference determination only for a term type, but also for a token of a term. Brigandt (2006b) asserts that "any theory that assumes that a concept is defined by a clearly delimited set of reference determining conditions cannot account for freely floating reference in the course of history" (p. 87). This claim is different from what Kitcher argued for in his work. While for Kitcher a particular use of a term type (each token of a term) has its reference in virtue of the existence of fixed and clearly determined conditions on reference fixing, for Brigandt fixing the reference of even one single token of a concept involves many conditions. These conditions are not restricted to causal and descriptive ones, as, in some contexts "virtually any belief may be reference determining" (Ibid., p. 88). Therefore, only the entire context of use can determine which conditions are relevant in fixing the reference.

If this claim is right, the problem in explaining complex biological case studies is not superficial—rather, it stems from a basic shortcoming of the referential approach to

[1] In Chapter 1, I argued that continuity is a necessary condition for the rationality and progress of scientific change. However, it is not the sufficient condition, because the mere continuity (e.g., shared semantic content) does not justify change of concepts.

[2] Shapere (2001) assumes reasoning to be the shared component that assures rationality of conceptual change. Therefore, scientists are rational in changing their concepts if their reasoning with those concepts overlaps during theory change.

conceptual change. Since the factors relevant to reference fixing for each token of a term are unbounded, the reference can easily and radically change between different contexts and relative to interpreters' interests. Therefore, it is almost impossible to give one referential explanation for all cases of reference stability through theory change. While a simple causal connection can provide the continuity required for explaining rational change in some contexts, in other contexts—particularly those where the reference is radically changed—only some overlaps in epistemic goals may explain rationality of change. For example, the reference of 'homology' is not completely fixed by causal and/or descriptive factors; instead, a wide range of relevant beliefs and statements would contribute to reference determination depending on different contexts. *Thus, reference is 'an emergent semantic property' which should be assigned to a token of a scientific term depending on a specific interpretation in a context of use*. The upshot is that, since the reference of some scientific terms cannot be fixed using a straightforward referential mechanism, the referential approach to conceptual change fails to accomplish its task. This failure is due to the fact that systematic theories of reference rely on certain specific reference determination conditions, while Brigandt's point is that those conditions are 'relatively open and unbounded'.

This strong claim against theories of reference determination, as I call it, emphasizes the importance of many beliefs, including those associated with a particular context of use and interpreters' interests, in the determination of reference. Brigandt emphasizes that, in each context, some reference fixing factors become more or less salient. For instance, he notes that "in some contexts a factor is not reference determining, while in other cases it may be" (Brigandt 2006b, p. 370), or that "the degree to which a particular factor is salient is context-sensitive" (Ibid., p. 63). In Section 4 of this chapter, I will explain that, for Brigandt, reference fixing requires a specific context of use, as well as the interpreter's selection among the relevant epistemic goals and relevant users. Therefore, *one specific use* of 'homology' could be used for different epistemic goals and to achieve different communicative purposes. In one respect, 'homology' is used to classify and compare species, while in another respect, it is used to causally explain inheritance structures (see Chapter 2). Furthermore, and more importantly, *one specific use* of 'homology' may have several epistemic goals that, in different *interpretive contexts,* could be assigned to the term and taken to determine its reference.

In Brigandt's strong claim against theories of reference, we can speak about two sources of context dependence. The first such source is the *context of use*, which reflects the conditions under which members of a language community use a token of a term to refer to a certain object. These conditions include epistemic goals, background beliefs, history of use, and conversational context shared by users.[1] These contextual conditions constrain the reference of a concept. For example, when the term 'gene' is used in molecular genetics, users are aware that their epistemic goals are to 'causally explain the molecular products of proteins'.

[1] Of note, while the external facts about the physical system they are interacting with are important in shaping the context of use, they should not be considered as a main contextual factor.

Such use of the molecular term 'gene'—for example, in a conversation in a molecular lab—is supported by the well-known papers and previous discoveries in this research tradition. Also, users of this molecular term can be more specific about the segments of DNA with or without intron, which may ultimately exclude bacteria from the reference. In this example, the importance of context can play an even more significant role: molecular term 'gene' can refer to different segments of DNA under different cellular designs with specific machinery.[1] Therefore, depending on the stage of gene expression, different segments of DNA can be called 'gene'. This contextual difference is so important that some philosophers believe that the concept of 'gene' refers to a *relational kind*, meaning that merely finding an intrinsic property of entities is not sufficient to pick out a unique reference (Weber 2014, p. 431). Instead, in different contexts, different segments of DNA could be called 'gene'.

The second source of context dependence is *the context of interpretation*, which refers to the *interest-relative* ascription of semantic properties. On Brigandt's account, semantic properties are attributed to one specific use of a term from an interpreter's point of view, in order to explain a philosophical question. So, based on different philosophical questions of an interpreter, a single token can be assigned different epistemic goals and, *as a consequence, different* inferential roles and references. Brigandt (2006b) explicitly differentiates the epistemic goals of a gene concept when the philosophical interest is 'studying scientific progress' and when it is to explain 'rational theoretical change' (p. 114). In the latter case, as Brigandt (2004) argues, philosophers should employ a "more fine-grained scheme of concept individuation" (p. 9).

Let us clarify this second kind of context dependence and the significant role that interpreters' interests play in reference fixing. In fact, a context of use involves too many epistemic goals, too many conceptual roles and a too large set of background beliefs that could all potentially contribute to reference fixing. Moreover, in the objective world, there are many referential candidates with which the concept user may be causally interacting. To single out a determinate reference in one interpretive context, we need to prioritize among these competing factors. According to Brigandt, what drives our selection among these factors and their prioritization based on the type of semantic continuity are the interpreter's interests. Sometimes, in order to assign the proper set of semantic properties to a term use, the interpreter should select who to include as part of the conversational context. However, there are many ways of deciding who to include as part of the conversation to be interpreted.

As an example, let us consider the concept expressed by the term 'unit-character', one of the earlier understandings of the process of heredity that assumes a one-one relation between traits and units of heredity. Morgan and colleagues discovered that there is no one-one relation between units of heredity and relevant phenotypical traits. Thus, the crucial question here is: Does the term 'unit-character' fail or succeed in referring?

[1] See Weber (2014, p. 431) and Boyd (1993, p. 483).

A classical theory of reference determination, even the one that considers the context of use, would simply answer 'yes' or 'no'. For example, following Kitcher's contextual theory of reference, Marcel Weber (2005, p. 207) argues that 'unit-character' fails to refer. Specifically, Weber (2005) argues that, in order to understand this concept, we should rely on its descriptive mode of reference. Therefore, since Morgan and his colleagues taught us that there is a many-many relation between traits and units of heredity, the term 'unit-character' had to be abandoned (ibid). However, Brigandt (2006b) criticizes this approach for the lack of flexibility in attributing semantic properties, arguing that, since the term was partially successful in practice between 1900 to 1910, it is "impossible to assign a unique referent" to this term (p. 295). Brigandt thinks that the failure to explain this successful research practice "demonstrates the limited usefulness of a semantic interpretation that boils down to assigning referents to terms" (p. 295). Thus, his solution is to avoid assigning a *fixed set of semantic properties* to a token use of the term: neither making the term co-referential with 'gene', nor arguing for its referential failure. Instead, he argues for a "semantically fruitful assignment of reference", where in different interpretive contexts different referents can be assigned to the term.

The idea that referential assignments are only true relative to a context of interpretation is a highly controversial thesis. In effect, it means that there are *no objective facts of the matter* about the reference of a token use of a scientific term. In the end, which reference is assigned can vary according to the interests and goals of an external interpreter. Therefore, the very same token use of a term could refer to different properties relative to different interpretative contexts. Accordingly, one might hesitate to attribute this thesis to Brigandt, especially since his focus is more on the importance of epistemic goals in the absence of a neat set of reference determination conditions. However, I believe that this relativity to the context of interpretation is essential to Brigandt's viewpoint, as an unbounded set of potential reference fixing conditions will never single out a determinate reference. In essence, there are too many competing epistemic goals and inferential roles that could possibly play a role in reference determination. Brigandt is aware that the context of use alone contains unlimited elements, including a large network of beliefs and aims of each individual user. This could threaten successful communication– because, if the total network of each use is part of the meaning, then meaning would never be shared. Thus, semantic holism is the first threat. To explain communication, Brigandt refers to interpreters' interests that, on his view, ultimately select among this large and messy set of conditions. In acknowledgment of such interest relativity, Brigandt (2005) writes:

> The same concept or term (token) may [be] studied with different interests in view, different studies may assign different reference fixing conditions, which may but need not lead to different assignments of referents. (p. 22)

In other words, in order to assign specific semantic properties and then find the continuity necessary for explaining conceptual change, Brigandt needs to constrain the unlimited set of conditions. Otherwise, Brigandt's insistence on the importance of unbounded conditions on reference fixing yields a kind of semantic holism about the features that determine reference. Therefore, the interpreters should select specific epistemic goals that suit their questions and then fix the inferential roles and reference afterwards. It is only in this way that users of a scientific term can be construed as sharing the same meaning and reference—and successfully communicating in a given context. In Section 3, I will discuss Brigandt's strategy for explaining shared meaning in further detail. While he uses a form of semantic supervenience to show that his account is holistic on the meta-semantic level, it does not posit holistic meanings. I will demonstrate that this strategy also turns on the interests of interpreters.

3- The Supervenience Strategy

As discussed above, an important task of Brigandt's account of concepts is explaining successful communication. One important condition of successful communication is sharing meaning(s). If two individuals do not share the same meanings [concepts], then they may use the same term, but talk past each other. Brigandt is aware that one threat against his strong claim is such loss of the shared meaning. Indeed, the *network of all semantically relevant properties* of an individuals' use of a term on each context is unique and is very likely to differ from those networks of other users. For Brigandt, concepts are flexible units of thought that are expressed by a term and whose semantic properties are assigned relative to both the context of use and the context of interpretation. In this process, a large set of conditions may be involved in semantic assignments. Depending on the context, 'virtually any belief may be reference determining' and 'different studies may assign different reference fixing conditions (Brigandt 2005, p. 16). Therefore, to explain communication, Brigandt needs to explain shared meanings. However, to explain shared meanings, he must avoid meanings that include the entire holistic network of all conditions and factors that may contribute to the understanding of a token use of a term.

Brigandt's strategy is to define (shared) meanings or (shared) concepts as a proper subset of total facts about individuals' use of a term in a particular conversational context. Let us assume that the components of a concept are reference, inferential roles and epistemic goals. Thus, among all objects that could possibly be the reference of a term, and among the wide range of inferential roles and epistemic goals that a term use may support, only a proper subset of them is shared between two specific language users. This proper subset can explain successful communication by providing a precise overlap between the concept users. Meanings can be shared, as, for any two users of a scientific term, we can identify some proper subset of their total overlapped networks. On this account, *meaning is a set of shared components between relevant users of a term*. For Brigandt, this account of meaning is a doctrine about concept possession, which implies that two individuals can share one concept with different networks of inferential roles, epistemic goals, as well as physical and historical contexts.

Brigandt calls this strategy *meaning supervenience,* because the shared meaning (i.e. the content of a shared concept) *supervenes* on the network of all semantically relevant properties of token uses of the term,[1] instead of being *identical* to each token's holistic network. In other words, one single meaning can emerge from two different networks of semantic properties in a context. As long as there are *sufficient similarities or overlap* in two concept users' networks of semantic properties, these users can share the same meaning and successfully communicate. The point of this strategy is that a non-holistic and shared meaning (and reference) will emerge from an unlimited and unbounded set of conditions and factors relevant to individuals' understanding and use of a term.[2] Since a concept's meaning is not identified with the whole network (but supervenes on that), the supervenience base for assigning meanings (concepts) is holistic; yet, the resulting meanings (concepts) remain non-holistic. Brigandt (2004) argues:

> The strategy is to say that two persons share the same concept as long as their total conceptual roles associated with a term are sufficiently similar. The function that assigns a meaning to every conceptual role is not a one-one mapping, instead different total conceptual roles count as the same concept. On this account, meaning supervenes on total conceptual role; and we have a holism about meaning determination in that the supervenience basis that determines meaning is holistic. (p. 3).

The supervenience strategy is basically used to argue that a proper subset of semantic properties [shared meaning] is not holistic, even though it is derived from the holistic structure of the total inferential roles that different individuals associate with the term. Generally speaking, Brigandt denies that meanings are holistic. That is, he denies that meanings—the semantic properties shared in conversation and inquiry–include the total *network of semantic properties* (i.e., the totality of the inferential roles, epistemic goals and referential candidates in a given context of use). Instead, "what is holistic is the total inferential role, which forms the very basis for concept ascription" (Brigandt 2006b, p. 63). Therefore, the strategy is to push holism from meaning to meaning determination. This

[1] Brigandt uses total inferential role to imply the network of all semantically relevant properties of an individual. Total inferential role has usually been identified as narrow content that is literally inside the head, but Brigandt (2006b) takes it as constituted by a mind-world relation (p. 102). The point is that total inferential role itself is not the content of a concept– since it is supported by an individual's use of a term, and it will not remain stable over time. Rather, inferential role, which is the communal and minimally shared part of total inferential role, is the content. This terminology seems confusing.

[2] The doctrine of 'meaning-supervenes-on-inferential-roles' comes from two classical papers by Warfield (1993) and Pagin (1997) who defend a kind of semantic holism with regards to meaning determination. According to Warfield (1993) and Pagin (1997), meaning is not identical to the totality of inferential properties but supervenes on total inferential roles. The general idea is that "while X's being identical with Z implies that X supervenes on Z, X's supervening on Z does not imply that X is identical with Z" (Warfield 1993, p. 302). Therefore, it is possible to have different total inferential roles and, at the same time, have the same concept (meaning). This implies that, although different language users have different beliefs and conceptions, a sufficiently similar pattern of understanding can emerge out of the totality of their beliefs to allow for successful communication. The strategy promises a solution to such problems with meaning holism as lacking a shared concept and instability of meaning among community members. Brigandt (2006b) uses a similar strategy by distinguishing 'total inferential role' (i.e., the set of inferences endorsed by an individual) and 'inferential role' (i.e., the set of inferences shared among concept users). It is the latter notion that constitutes meaning, and there is no holism at this level.

holism in the supervenience base also affects reference determination—though the reference itself is not holistic.

By identifying a specific subset of semantic properties shared by all the relevant parties of a conversation, this strategy helps to explain successful communication in each context. For example, in a context where two unknown biologists are using a concept of 'gene' to investigate the pattern of inheritance, they will share a great deal of theoretical knowledge and goals, as well as physical apparatus for testing their hypotheses. The factors that can potentially affect these two scientists' respective understanding and use of the term are unbounded and holistic; these factors could result in their referring to different kinds of thing(s). However, only a proper subset of these conditions is relevant to explaining their *successful communication* about genes in that particular context. If we want to explain their successful coordination, we need to isolate some factors from their respective networks that are shared. Therefore, meaning determination should include the large bodies of beliefs, goals and facts about the two scientists' shared apparatus that help them coordinate within this context. Our aim, then, is to pick out a proper subset of overlapping semantically relevant properties shared by both scientists in this context as the shared meaning. This shared meaning can then be used to determine the reference of both scientists' use of the term relative to this context – and this interpretive goal.

Of note, according to Brigandt, the context of interpretation plays an essential role in determining the shared meanings. In order to choose which features to include in shared meanings, Brigandt invokes the context of interpretation to help choose the relevant semantic properties among a holistic network of all relevant beliefs and goals. In fact, different meanings can emerge out of the same network of semantic properties. It is an interpreter's decision to identify which meaning should come out of the supervenience strategy based on specific interests. Otherwise, the supervenience strategy itself *does not* determine which meaning should be attributed to a term use in a specific context. Suppose a philosopher who wants to explain the rationality and partial success of scientists who used the term 'unit-character' *before* Morgan and his colleagues' discovery of many-many relation between traits and units of heredity. Insofar as they were successful in using the term, they were tracking a real feature of their environment, playing a specific causal-explanatory role in the reproduction of phenotypical traits. Then, insofar as they were successful, these theorists were tracking facts about genes. On this interpretive project, the aim is roughly to show that the term 'unit-character' could refer to genes, even though, at that time, users of the term had some peripheral wrong theoretical beliefs about there being a one-one relation between genes and traits. The philosopher can select a certain subset of the semantic network of the user of the term 'unit-character' before the discovery and show that this subset has a wide overlap with subsets of users of the term 'gene' before and after that discovery.

In another context of interpretation, however, the interpreter might choose different aspects of the semantic network and a different reference for the term 'unit-character'. For instance, a philosopher may want to explain how the scientific community made progress by choosing to abandon the term 'unit-character' after Morgan's discovery. In that case, the philosopher would be interested in highlighting the differences between current uses of 'gene' and past uses of 'unit-character'. This interpreter would then choose a different subset of semantic properties for the very same token use of the term 'unit-character' in the past. What is important for this explanatory project is the past scientist's theoretical commitment to a one-one relation between unit-characters and traits, which does not align with many-many relation between genes and traits. When the meaning associated with the past use of 'unit-character' is individuated in this way, it makes sense to say that the term failed to refer. Then, the conclusion is that, on Brigandt's account, the meaning and reference of a token use of a term can vary depending on the explanatory interests of an external interpreter.

In sum, the context of use by itself does not suffice to determine a unique meaning for a token use of a term. Choosing who to include in the conversational context and which proper subset of their total conceptual networks to select as the meaning expressed is the prerogative of the interpreter. In Section 4, I will develop this point further.

4- Interest-Relativity

I have already shown that Brigandt uses the supervenience strategy to find the meaning shared between different users of a term. Meaning is a proper subset of the total network of semantic properties assigned to a token use of a term by an interpreter. Therefore, depending on who the interpreter chooses to regard as part of the same conversational context and depending on which concerns the interpreter is interested in, the very same use of a term can be assigned different meanings. According to the strong claim, an unlimited number of conditions is relevant in reference determination, and these conditions should be narrowed down by philosophers.

The interests of interpreters select and prioritize among all relevant conditions and factors that play a role in reference determination. While each context of use limits the factors that contribute to reference determination, these factors are not sufficient to single out a unique reference. It is a philosophical task to prioritize which semantic properties are crucial in a specific context of use based on a philosophical interest. In one way, the type of semantic continuity an interpreter is looking for may fix how fine-grained an epistemic goal should be. In addition, the choice of who to include as part of the conversational context may play a role in semantic assignment. I already cited Brigandt's (2005) claim that a term token could be studied with different interests where each interest assigns different references to that token use (p. 22). The point here is that, even in one specific context of use, there is no fixed semantic content for a certain term. Depending on different interpretive contexts, different meaning and reference can be assigned to a concept.

In Chapter 3, I showed that the core of Brigandt's non-representational account of concepts is his emphasis on epistemic goals as a semantic component of concepts. Epistemic goals, a substantive component of concepts, not only play a crucial role in reference determination, but also set the standards for evaluating the rationality and progress of conceptual change. However, in this chapter, my focus is more on the *interest-relative* nature of epistemic goals. In fact, there is no clear criterion/rule for attributing *one specific* epistemic goal to a particular scientific community or a communicational context. There is no rule to fix the relevant epistemic goals in a context; therefore, *there is no clear distinction between the epistemic goals that contribute to meaning constitution and those that do not*. In theory, then, a wide range of scientific goals can be involved in determining the reference of a single use of a term. In case studies, the open questions would be "Which scientific aims and values determine the meaning of a particular use?" or "Who is responsible for determining the epistemic goal of a scientific field?" (or "What determines who's responsible?"). While the original epistemic goal of genetics was to predict the pattern of inheritance, it later came to also include the goal of explaining the way genes bring about their molecular products. In general, the epistemic goals associated with a term can always get narrower or broader. For example, while the epistemic aim of 'explaining the process of reproduction in nature' could be a very broad goal of genetics, within this broader project, there can be narrower epistemic goals of 'explaining the synthesis of proteins by 'DNA isolation' or 'mRNA isolation''.

Interpreters assign meanings/concepts (reference, inferential roles and epistemic goals) to particular uses of scientific terms based on how fruitful such ascriptions can be in meeting interpreters' own explanatory interests. If an interpreter is interested in explaining rational communication, s/he can choose a proper subset of the total network shared to vindicate this goal. However, when the interpreter's interest shifts to explaining the progress made by the scientific community in understanding specific causal mechanisms in the world, different subset of semantic properties can be selected as the meaning of that term. Accordingly, the interpreter can ascribe any reference, inferential roles or epistemic goals to *one particular token* as long as it is '*fruitful'* for the interpreter's own philosophical goals, such as explaining rationality or progress of conceptual change (Brigandt 2011, p. 172). This idea is in accord with Brigandt's (2011) excessively flexible account of concept individuation (reviewed in Section 3.3 in Chapter 4 of the present thesis), in which the conditions of concept individuation are fixed depending on the interpreter's philosophical interests. The idea that epistemic goals and inferential roles are selected based on contingent interests of an interpreter leads to a completely flexible position where the theory of reference and theory of concepts lose their importance. As Brigandt (2011) argues,

> One philosophical question is what theory of reference […] can be provided. […] From my perspective, the more relevant issue is that the philosopher is able to ascribe a particular referent, an inferential role, and an epistemic goal to a concrete concept in a scientific context, and to defend this ascription in terms of its fruitfulness in understanding the use and change of concepts. (p. 185)

In fact, Brigandt's non-representational account of concepts gradually gets closer to a completely interest-relative theory of meaning and reference determination. In this account, it is not philosophically important what the reference of a concept is, and ultimately what the concept itself is. The referential approach to conceptual change is barren—not because current theories of reference are in a weak position to identify the correct reference of some biological kind terms, but because the reference of the same term token may vary across different contexts of interpretation. In other words, as long as an interpreter can explain 'why a context-sensitive use of a term can be beneficial to scientific practice', *any* referential assignment is justified. Although concepts are units of thought, the content and individuation of these units are decided upon by philosophers. This entails that conceptual stability is not an objective matter, but rather is a matter of putting forward a fruitful explanation of scientific practice.

5- What's Wrong with Being Interest-Relative

At least one of the major aims of introducing epistemic goals was to explain the rationality of conceptual change by finding some continuity in the semantic properties of concepts. In Brigandt's account of concepts, it is significant that epistemic goals are pursued by individual concept users, constituted in the community of users, but are attributed by interpreters' interests. Consider a geneticist who has many goals in using 'gene': (G1) to predict the next generation's traits, (G2) to manipulate a specific gene in order to make a new trait in the next generation, and (G3) to causally explain the production of genes out of amino acids. According to Brigandt's framework, (G2) is not an epistemic goal shared among and accepted by the community of users of 'gene'. Furthermore, while (G1) and (G3) could be the geneticist's epistemic goals, it depends on the interpreter's interest to assign one of them to a particular use of this term in order to nail down a specific reference. If (G1) is taken as the dominant goal, then 'gene' expresses a classical concept. However, if (G2) is taken as the dominant goal, then it may express a molecular concept. Therefore, before the interpreter's decision, the reference is underdetermined.

This sort of underdetermination makes it easy for a philosopher to find a shared goal when s/he wants to explain the rationality of a particular change. In fact, an interest-relative account makes it very easy to explain rational change—it may even be a trivial task—because every radical change can be explained as rational in accordance with some philosophers' interests. As long as an interpreter can identify *any* reason for changing the total conceptual role associated with a term, no matter how pragmatic or contingent, the change can be counted as a rational scientific change. For philosophers, one way of doing so is to assign a very broad epistemic goal to a token of concept in order to enable finding continuity between this token and its earlier (or later) versions. For example, one of the epistemic goals of using 'gene' is to explain the nature of biological reproduction. This is a very broad goal, and many concepts pursue this goal. Therefore, it becomes very easy to connect the uses of 'gene' in classical and molecular genetics by arguing that they both pursue the same broad epistemic goals. On this interpretation, the reference of 'gene' becomes a very broad set of objects, including all objects that fall under this term despite misconceptions or defective theories. This enables an easy explanation of the rational continuity of using 'gene' during the last century. As, even if all the semantic properties of a concept radically change, there still is a very broad epistemic goal shared by all users of the same term, which removes the threat of irrational conceptual change.[1]

More importantly, when semantically relevant components of concepts are determined relative to interpretive interests, there are no objective norms of scientific rationality that can guide concept users in choosing how to change their understanding of a term over the course of inquiry. If the goal of semantic interpretation is to explain why users of a term are minimally rational in revising their understanding of its reference over time, then all the theorist needs to do is to identify some feature of users' prior understanding that would help to explain why they made those changes. Since most people usually act for reasons, the result will be that almost everyone will count as *minimally rational* in this sense. According to Donald Davidson (1982), even seemingly *irrational* actions, like drinking a saucer of mud[2], have "a rational element at the core" in the minimal sense of doing something based on one's prior beliefs and desires (p. 173). This explains why the action seemed like a good idea at the time. However, I think that this minimal rationality is not sufficiently strong to explain what is distinctive of scientific rationality through theory change.

In Chapter 4, I explained that Brigandt's conception of rationality is not in accord with full-blooded realism, as his account does not yield a non-accidental connection with the truth about the structure of world. On his account, one can count as scientifically rational, even if one's belief revisions are not aimed at getting a better understanding of the system one is manipulating and studying. At this point, I would like to add that his account of rationality does not appear to explain a genuinely *normative sense of rationality*, the one on which there are objectively better or worse options one could choose. Instead, Brigandt's sense of rationality appears to be geared to rationalizing whatever choice one happens to have made. Therefore, Brigandt's account does not seem apt to provide any rational guidance or constraints on scientists' theoretical choices. Yet, this approach seems to be wrong to me. An account of meaning and conceptual continuity should place constraints on which choices are scientifically better than others. Based on the evidence available at the time, and considering that the aim of science is to give a better isomorphism between representational system and a specific causal system in the world, there should be an objective fact of the matter of which choices are more scientifically rational than others.[3] Such a normative account of rationality

[1] Similarly, it is always possible to assign specific epistemic goals to scientists' concepts so that the later concepts satisfy the epistemic goals of those concepts' earlier versions to a greater extent and scientific *progress* is always achieved.

[2] This example (drinking saucer of mud) comes from: Anscombe, G. E. M. (2000). *Intention*. Cambridge, MA: Harvard University Press (p. 70).

[3] While there may be some indeterminacy, there should also be some determinate cases of better and worse choices.

is very different from merely giving an *ex post facto* rationalization of what actually took place among scientists. I would assume that, in principle, scientists can make bad theoretical decisions.

There are at least two reasons why a typical interest-relative account is excessively lax and easy-going with conceptual change. First, as already discussed, epistemic goals as a substantive component of concepts are not objectively determined facts about a particular use of a term; rather, they are at least partly relative to interpreters' philosophical interests. Therefore, interpreters can in principle manipulate these factors in accordance with their interests. Second, the sameness of epistemic goals is determined in the context, where successful communication is taken to be a sign of sharing a 'sufficient enough' set of inferential roles and epistemic goals (Brigandt 2006b, p. 389).[1] Thus, interpreters can in principle manipulate how much overlap is sufficient for sharing a concept. Definitely, a non-representational account is highly successful in explaining the complex communicative contexts between individuals who do not appear to share a stable reference. This can easily explain that at least their epistemic goals are identical (or sufficiently overlapping). In this connection, Brigandt (2006b, p. 63) gives an example of a Drosophila geneticist who can easily communicate with a layman about *fruit fly*, as long as they succeed in catching it.

A major drawback with contextual (both context-of-use and context-of-interpretation) accounts of reference determination is that they fail to explain the shared meaning *across contexts*. Let us illustrate this failure in an example. According to Brigandt's contextual account, as long as language users' epistemic goals are interpreted as identical, reference can be assigned in a way to make their communication successful. Take two geneticists from two labs, one working on molecular genetics (with EG_1), and the other one on classical genetics (with EG_2). Given that they are collaborating, are aware of each other's goals, and know which EG is important in a particular discussion, they share the same reference and use the same concept on each given use of the term 'gene'. In fact, in one particular discussion, the epistemic goals shared between them ultimately fix the reference and meaning of their central concepts. Similarly, one of those geneticists can successfully communicate about genes with an ordinary person, as long as they share a minimum set of beliefs about each other's goals and inferential roles in practice. Therefore, we can assume that one geneticist uses the same term in three different contexts of use (within his research field, across scientific fields, and with ordinary people) with different epistemic goals and, ultimately, with different reference.

On the other hand, we know that it is very likely that different epistemic goals bring about different concepts, as epistemic goals are the dominant semantic property. Brigandt (2006b) repeatedly refers to this idea: "The point of my semantic approach is precisely to link concepts

[1] Brigandt (2006b) adds that "concept possession consists in having a minimal set of inferential dispositions and epistemic abilities so as to be able to meaningfully communicate with other scientists and to conduct scientific research" (p. 389, emphasis added).

to the epistemic products and the theoretical goals of a scientific field" (p. 273); or "variation in a concept's epistemic goal may be an important determinant of semantic change" (p. 116). The outcome is that the geneticist who uses the term 'gene' in different contexts may use 'different concepts', as they 'support different types of inferences and used for different goals' (Brigandt 2006b, p. 9). If I want to be more precise about Brigandt's account, I should say that different uses of the same term in different contexts differ *regarding the various semantic properties*, since having the 'same' or 'different concepts' is not philosophically important in Brigandt's (2010a) pragmatic approach (p. 25). While it may seem helpful to differentiate concepts expressed by the same term in different contexts, the problem is to explain how these different concepts (i.e., different uses of the term) are related to each other. In the cases where the epistemic goals are incompatible, such as the use of 'gene' in classical *vs.* molecular genetics, the scientist is using the word to express two concepts in two different contexts. This issue leads to the radical situation where each scientist (or one in different contexts) is counted as expressing different concepts with the same term.[1]

This issue becomes worse when it comes to the context of interpretation where a single use of a term could count as expressing different concepts: (i) when interpreted relative to different ways of individuating the conversational context, or (ii) when interpreted relative to different theoretical interests. Let us say that our geneticist uses 'gene' in a public lecture. Regardless of what his/her main intention is, interpreters' choices affect how the conversational context is individuated, and which elements are privileged in reference determination. For instance, the interpreter may be interested in what is communicated to everyone in the audience, in which case the geneticist's use of 'gene' expresses a vague concept which perhaps fails to secure any reference; alternatively, s/he may want to focus on the research the geneticist was reacting to, in which case his/her use of 'gene' expresses a very precise concept with a fine-grained reference. For any given interpretative choice, we can normally find some commonality across the users of a term, and any semantic assignment could be true as long as it can explain the successful communication between the selected members of a conversation. However, there is nothing that is objectively stable and shared by all users of the term, which constitutes their conceptual competence independently of an interpreter's perspective. This raises the question of how concept users manage to communicate and coordinate across a wide range of conversations, and what in our patterns of understanding allows for this. What is lacking in this account is a shared conventional meaning.

In response to this objection, Brigandt would probably insist that his framework of concepts is not devised as a metaphysical theory of concepts. Rather, this interest-relative account is a

[1] Of course, we do have ambiguous expressions – like 'bat' for the animal and 'bat' for a cricket bat. In these cases, there is a clear distinction between the meanings or concepts expressed by different uses of the same term. However, the case of 'gene' is not like that. There seems to be an important aspect of understanding that is shared between contexts of use, which guides the scientist's different way of understanding the term in different contexts.

methodological framework to answer specific philosophical questions about specific cases of conceptual change (Brigandt 2006b, p. 6, 51, 418; 2012, p. 78-79; 2013, p. 86). This means that epistemic goals, as the most important semantic property of a use of a term, should not be taken as a stable semantic content or meaning of the term (type). However, in some other works, Brigandt (e.g., 2011) explicitly highlights that the set of epistemic goals is a semantic property of a term "just like reference and inferential role (intensions)" (p. 184). He also proposes a theory of concepts based on the objections he raises against Fodor's atomistic theory of concepts and conceptual role semantics (CRS), which are two metaphysical theories of concepts. Therefore, Brigandt's work lacks a clear distinction between semantic and epistemic properties.[1] This unclear distinction stems from his pragmatic approach to concepts, where the interpreter's interests are privileged over the actual semantic content of concepts. Brigandt (2011) writes:

> […] one could contend that epistemic goal is not a semantic property (and thus not a component of a concept), but an epistemic property (merely tied to concepts), maybe on the grounds that accounting for conceptual change is an epistemological rather than genuinely semantic task. (p. 184)

Providing no clear answer to this anticipated contention, other than a genuinely pragmatic one, Brigandt argues that, as long as the particular philosophical questions can be answered, the distinction between the semantic and pragmatic components is not so important. Accordingly, conceptual contents are determined based on philosophical interests. For example, reference should be assigned in such a way as to explain the rational interaction of individuals with the world, while reference, along with inferential roles, makes it possible to explain successful communication of language users. The justification for introducing epistemic goals as an independent component of concepts is to account for the rationality of semantic change. Therefore, on this approach, it is possible that interpreters assign different semantic properties to a single use of a term in response to different philosophical questions. It seems that the result of this pragmatic approach is an anti-realist account of concepts, since concepts are seen as "tools that philosophers develop for a certain philosophical purpose" (Brigandt 2011 p. 185). In this account, concepts are not some fixed units of thought, nor are they some fixed meanings for terms. Instead, concepts are only *ad hoc* philosophical tools attributed to particular uses of language in order to explain users' behaviour based on interpreters' interests.

[1] In this thesis, I am not proposing a clear distinction between the semantic and epistemic properties of concepts. In general, however, semantics is concerned with the stable meanings of term types, whereas epistemology should address changing understanding of term tokens.

6- Towards a Framework for a Theory of Reference Determination

In this chapter, I acknowledged that some non-representational factors contribute to fixing reference. However, I argued that the leap from this claim to the stronger claim (that reference is determined only relative to unbounded sets of conditions and to interpretive interests) is problematic. In particular, this approach to concepts fails to explain how there could be genuine epistemic norms governing scientific reasoning. Furthermore, it also fails to account for stable meanings and concepts across contexts of use. I believe these are important objections to non-representational accounts of conceptual change. However, I have not yet explained how a representational account could do a better job in explaining these phenomena. As we saw, reference determination of biological concepts involves some complications that render impossible a referential explanation of conceptual change with current theories of reference. In addition, epistemic factors highlighted in Chapter 2 still require explanation. Therefore, the current challenge is to reconcile the importance of epistemic goals in reference fixing with a referential approach to conceptual change. That is, we want to accept Brigandt's weak claim entailing that current theories of reference need to be revised, while rejecting his strong claim in proposing a non-representational account.

The above require a positive framework for a theory of reference determination. Such a framework should both consider the context of use and avoid appealing to the context of interpretation. The major difficulty here is to reconcile context variability with the stability of meaning across different conversational contexts and throughout scientific progress over time. Said differently, if the meaning and reference vary across contexts and through different periods of time, how can we explain the connection between different versions of a concept? I think this is the most important question in the topic of conceptual change: we need to find a path between change and stability. While scientific concepts should have a flexibility to allow for referential variance across different usages of a term, this flexibility should simultaneously not lead to an interest- relative understanding of concepts where even radical referential changes in understanding and use may not change the concept. It is not my aim to provide the final word on this basic question; however, in Chapter 6, I will attempt to propose a general framework for a theory of reference and a theory of concepts that incorporate these properties.

Seeking to explain the rationality and progress of change through a good theory of reference, my framework will by default side with a referential approach to conceptual change. Also, the classical realist package detailed in Chapter 4 can be preserved by means of keeping a referential approach to conceptual change. The realist package ensures that rational conceptual change aims to get science closer to a more accurate understanding of the structure of the world. By relating the contextually formed concepts to a set of objectively existent referents, this positive framework should explain how science in general is guided towards a more accurate isomorphism between the total contextually 'precisified' set of concepts and the total objective causal system.

Chapter 6

A Proper Account of Conceptual Change

1- Introduction

I began this thesis with the annoying announcement that the classical referential approach to conceptual change, with the flavour of scientific realism, fails to account for the complication of some biological case studies. Ingo Brigandt, among other philosophers of biology, taught us that the epistemic goals of using some biological concepts play a crucial role in reference determination and, ultimately, in explaining conceptual continuity. However, through a detailed discussion of non-representational accounts of concepts, I raised several concerns about using those accounts in explaining conceptual change. One of those worries was the idea that such accounts are not compatible with a full-blooded understanding of scientific realism (see Chapter 4 for a detailed review). I also showed that the importance of contextual factors in reference determination cannot be overestimated. In Chapter 5, I argued that a limitation of Brigandt's account of conceptual change is that semantic contents are assigned to concepts in an interest-relative way. However, the question remained how to collect all these constraints into one overarching account.

Based on the objections against a non-representational account of concepts discussed in Chapter 5, I begin this chapter listing the most important *desiderata* for an account of conceptual change to be compatible with a full-blooded understanding of scientific realism. My aim here is to vindicate the referential approach to conceptual change, on the one hand, but also to leave room for the possibility that concepts change their referents if necessary. Therefore, this list provides some constraints on an account of conceptual continuity and reference determination. In principle, a good theory of concepts should be capable of explaining how conceptual competence can ground smooth communication. However, in science, explaining successful communication should also provide a normative guidance towards a more accurate understanding of the physical system one interacts with. Such guidance will give scientists better/worse strategies to keep/change their concepts based on an objective knowledge of a particular domain of reality. My framework should also provide referential flexibility for concepts—but, at the same time, explain referential continuity in terms of getting closer to the truth about the reference of a particular use of a term. The upshot is to view the relation between the language of science and the objective world as an epistemic relation, in which users of a term in each specific context aim to refer to one aspect of the world.

After formulating the required *desiderata* for an ideal theory of conceptual change that fits with a full-blooded scientific realism (Section 2), I will explain how these conditions can be satisfied within a theory of concepts and reference. A defence of the referential approach to conceptual change would not be complete unless all *desiderata* are met simultaneously. Therefore, I will propose a framework of an account of concepts that satisfy all these conditions together (Section 4). I will also provide an example of an account of concepts with the potential to satisfy my *desiderata* (Section 4.3). Of note, however, my framework requires further work to fully explain how such an account can satisfy the conditions. I believe that the theories that satisfy my *desiderata* would remain faithful to the referential approach to conceptual change and a general understanding of full-blooded scientific realism. In Section 2, I distinguish the constraints on both a theory of concepts and a theory of reference.

2- Six+One Constraints on a Theory of Conceptual Change

A. Communication (The Static Condition)

The first—and probably the most important—criterion of a good theory of concepts is to explain communication. As discussed previously, purely referential theories have difficulties explaining successful communication in the case of some biological terms whose reference can vary across different contexts. For instance, if a biological term like ‘gene’ shifts its reference in different contexts, purely referential theories of conceptual change will fail to account for conceptual continuity across contexts. Then, how do competent users of the term understand the various reference shifts across different contexts of use? In addition, I also argued that non-representational theories of concepts have limitations in terms of explaining shared meaning across different communicative contexts. For instance, I argued that geneticists working on different scientific questions (i.e., with different epistemic goals or different theoretical background) refer to different things using the same term. Given the possibility of simultaneous variations in all three semantic dimensions of concepts (i.e., reference, epistemic goals and theoretical background), it remains unclear how different groups can still successfully communicate. More importantly, communication between ordinary people and experts, who do not share the same epistemic goals, may be hampered due to not sharing the same concepts. Therefore, what is crucial for a good theory of concepts is to provide a common ground shared between all competent users of a concept; this common ground cannot be the precise reference or the underlying scientific theory in which the concept figures in different contexts. I call this the *static* condition, as it appeals for a basic commonality of concepts shared between ordinary users and experts who associate a term with different epistemic goals and inferential roles—particularly when uses of the term may pick out different referents in different contexts.

B. Epistemic Guidance (The Dynamic Condition)

Given that conceptual change is all about the process of revisions and replacements, for a proper account of conceptual change, it is not enough to stay with the static condition. Scientists are not free to choose or stipulate *any* concepts over those that are currently used by a scientific community. In previous chapters, I explained that a good theory of concepts should provide resources to explain the rationality of conceptual change. In other words, there should be a normative epistemic guidance that will help rational speakers to get closer to the truth about the reference. The stability of epistemic goals, or any one of the semantic elements, is not rationally sufficient to change a concept to another, while there is and there should be a guide to determine the better/worst options based on the available options in a given context. For example, the decision to replace the concept of 'phlogiston' with a concept of 'oxygen' is a rational change based on available evidence. Available evidence shows the referential failure of the former concept and the overall benefits of the latter (e.g., referential success, consistency with other theories, simplicity, etc.). Furthermore, the concepts of 'phlogiston' and 'oxygen' posit radically contrary causal roles for their reference, which makes it very attractive to consider it a case of concept replacement. Therefore, the rational choice has to replace the concept of 'phlogiston' with that of 'oxygen'. I call this the *dynamic* condition. If an account of conceptual change has to align with a full-blooded scientific realism (see Chapter 4), then this normative guidance should provide the basis for the thinkers to get closer to a better understanding of the overall causal system one interacts with. Accordingly, conceptual change should afford rational guidance: it is rational to change a concept or revise it only if one is justified that the new concept provides a more accurate understanding of a real feature of the world.

C. Progress (The Direction Condition)

In addition to guidance for changing concepts, we should specify the direction of rational enquiry. Therefore, the third important condition of a theory of concepts is to explain how concepts help guide us towards a better isomorphism between the representational system and a specific causal system in the world. I call this the *direction* condition. Our understanding of scientific concepts can be modified so as to more accurately characterise particular aspects of the causal system. Traditionally, conceptual progress was satisfied if and only if the concept keeps the reference stable and gives a better understanding of the same referent. However, I will argue that scientific progress does not necessarily entail referential stability; rather, the context variability in the reference of some biological terms does not necessarily break the conceptual continuity. Said differently, I depart from a traditional understanding of scientific realism according to which progress is made only if the later ways of understanding a term provide more truth about the same referent. Sometimes, referring to different aspects of an element in a causal system (i.e., referring to more specific features of that element of the system, or features that answer to more specific interests or epistemic goals) will result in an increase of knowledge about a domain of mind-independent reality. It is possible for a concept to achieve a more accurate understanding of the total system while its reference is allowed to float from one conversational context to another. (While the reference may change, conceptual continuity is explained by meeting the first *desideratum*, as I will explain it in Section 4.)

D. Perspective-Independent Reference Fixing

In Chapter 5, I argued that a major problem with Brigandt's non-representational account of concepts is that reference assignment is relative to the contingent interests of external interpreters. However, my framework for an account of conceptual change entails that concepts should have a perspective-independent reference. This is far beyond a metaphysical claim that the referent itself is mind-independent (metaphysical realism)—rather, it is a semantic claim about a theory of reference fixing. The objective reference fixing does not fail to take the contextual factors into account but distinguishes the interpretive context from the context of use. More precisely, I acknowledge the possibility of having different plausible approaches to an entity or phenomena in different contexts. A single aspect of a causal system can be individuated (referred to) in different ways for different theoretical or practical purposes. However, the objectivity of reference fixing means that, after fixing all contextual factors (including the epistemic goals of discussion and its relevant audience), there is no place for interpreters' contingent interests.

E. Context Variability

In previous chapters, I argued for the importance of contextual factors in reference determination. In particular, the same term can be used to pursue different epistemic goals in different contexts, which ultimately results in this term referring to a different set of entities. For example, depending on the context, the term 'gene' can plausibly pick out different referents and imply different meanings. In this respect, I agreed with Brigandt about the importance of epistemic goals in reference determination; however, I departed from Brigandt's theory and other non-representational accounts of concepts in giving a semantic status to the epistemic goals. The context variability of reference fixing is not limited to the epistemic goals; rather, the more general notion of 'context of use' makes variation in the reference fixing of a term. A good example of this is Philip Kitcher's (1993) insight that the word 'dephlogisticated air' could refer to oxygen in some contexts. Therefore, reference fixing requires to set the contextual factor, including users' epistemic goals, and come up with an appropriate way how to run this factor through the process of reference fixing (in Section 4 of this chapter, I give a *pre-task role* to the epistemic and contextual factors).

F. Approximate Truth

According to scientific realists, a successful scientific theory must be (approximately) true about the entities it posits, including theoretical entities. Thus, a good theory of reference should make successful scientific theories (at least approximately) true by assigning elements of reality to the terms employed by those theories. With regard theoretical terms, a theory of reference should assign referents that at least roughly conform to the causal roles posited by the scientific theories where they figure. Consequently, theories are *approximately true* as long as they establish a rough isomorphism between the representational system and the causal structure of the world; in addition, theories are *more accurate* as long as this isomorphism becomes more precise. Thus, a significant desideratum of a theory of reference is to fix reference so as to vindicate the approximate truth of the causal roles associated with a given theoretical term in a successful scientific theory. Moreover, as a scientific theory is revised towards increasing its predictive accuracy and other theoretical virtues, the assignment of reference should vindicate this progress by construing it as a better approximation to the truth about the original referents of the theoretical terms involved.

G. Corrigibility

Thus far, I have outlined constraints that apply to theories of concepts and theories of reference separately. However, the final constraint applies to both (or connects them together). Let us begin with Quine's lesson that none of the assumptions about natural kinds is immune to rational revision in the light of empirical inquiry.[1] Scientific realists usually endorse that our theories of the nature of entities referred to by theoretical terms are corrigible—since we do not have an *a priori* access to the nature of the objective natural kinds in our environment. I call this the *corrigibility* desideratum. However, this implies that an acceptable account of reference determination cannot posit rationally incorrigible reference-fixing criteria (a privileged set of IRs and EGs), which serves as a difficult constraint on the assignment of reference. Specific claims about the reference can in principle be modified in the light of empirical inquiry.[2] This desideratum also means that an acceptable account of

[1] Quine (1953, p. 43).

[2] A scientific realist can consistently endorse a *cluster theory of reference fixing*, where the best satisfier of a cluster of descriptions fixes reference. This cluster may stay incorrigible, although incorporating some false descriptions. I acknowledge that such an account is consistent with the corrigibility constraint because it concedes no a priori access to the nature of reference and allows to change the cluster itself when major changes take place.

concepts individuation cannot rely on rationally non-revisable aspects of understanding (IRs and EGs) to test whether the same concept has been preserved between contexts, across time or between interlocutors.[1] In particular, there is no *infallible* inferential role (IR or intension) that a speaker must accept in order to count as employing a particular concept. A similar lesson follows from Kripke's and Putnam's observation that ordinary speakers can be competent with the meaning of a term despite ignorance or error about how to identify the reference.[2]

The commitment to rational revisability of conceptual understanding and reference determination conditions is an important commitment of a full-blooded scientific realist account of conceptual change. This is so because this commitment captures both the mind-independence of the referents of theoretical terms and the contingent nature of our epistemic access to those referents. However, it sets an important challenge for the theorists who hope to vindicate it. The challenge is to find a way that, unlike Quine, would preserve the realist view of both reference and concepts and, in addition to that, say how reference is fixed and how concepts are individuated while there are no infallible aspects of thinkers' understanding. In Section 4, I will try to draw a framework to respond to this challenge. The desiderata outlined in this section are shown in Figure 1.

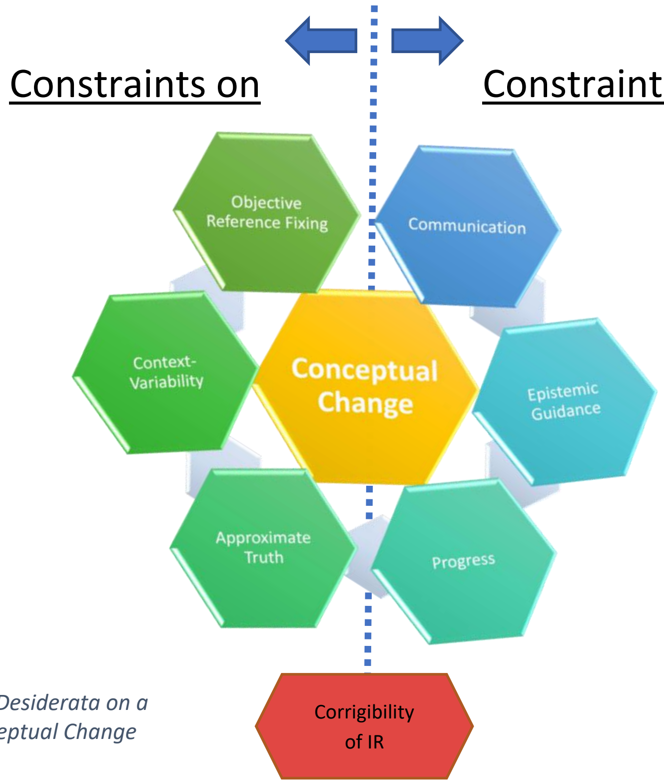

Figure 2: Six plus One Desiderata on a Good Theory of Conceptual Change

[1] Of note, the lack of solid rock reference-fixing criteria does not mean that any inferential role or epistemic goal can be revised, and users still keep the same concept. As in natural evolution where a new species cannot breed with the supposed similar kinds, there is a moment where we cannot rationally revise the causal role associated with a term without changing the concept. It is difficult, if not impossible, to pin down this moment or to determine its necessary and sufficient conditions.

[2] Kripke (1980), Putnam (1973b).

3- How Good Are Other Views?

Now that I have outlined all my main desiderata of an adequate account of conceptual change, I can evaluate theories of concepts and reference in satisfying these conditions. The importance of this evaluation is that it gives an overall view of why traditional accounts of conceptual change could not overcome all complicated issues of biological case studies. In addition, this evaluation can be beneficial in drawing the ultimate framework of an adequate account that would be capable of explaining the stability of meaning in communication *and* the possibility of radical evolution of understanding over time and between contexts. Of note, my aim here is not to survey all previous attempts at resolving these tensions—rather, I seek to get a broad structural characterisation of the efforts made so far. I characterize these efforts into three broad categories: (i) causal theories of reference; (ii) novel theories of concepts; and (iii) pluralist views about our understanding of the structure of the world.[1]

One of the most well-known responses to explaining the rationality and progress of conceptual change turned on causal theories of reference. A typical causal theory of reference seeks to vindicate the stability of reference through a revision of the descriptions associated with a scientific concept. In my terminology, a causal theory can satisfy desiderata A, C and F, all of which require some sort of stability of concepts through theory change. Scientific realists take the referential stability afforded by causal theories of reference to give them an advantage over anti-realists and relativists, who claim that each scientific paradigm may shift the reference of a term to pick out an entirely different referent. However, a typical causal theory cannot vindicate the idea that a term used with the same meaning can pick out different referents in different contexts. According to desideratum B (i.e., epistemic guidance), it can be epistemically rational to take a scientific term to pick out slightly different referents in different scientific contexts. However, causal theories were proposed precisely for their ability to keep the reference of a term stable despite theoretical and contextual variations. As we have seen in the case of theoretical terms in biology, scientific concepts are not always referentially stable in this way. Currently, some kinds of causal-descriptive theories are used so that to reconcile both the stability of reference and context variability.[2] While I have no issue with this approach, in Chapter 2, I provided reasons for the insufficiency of hybrid theories in explaining biological case studies.

A second approach to the problem is to treat reference as only one semantic component in a theory of concepts. In Lennox's (2013a) account of concepts (see Chapter 1), a well-formed concept is defined as a referentially open-ended unit of thought; therefore, when a *new* referent falls into the extension of a concept or some revisions are made in the concepts' inferential role, the concept has the flexibility to retain its identity. I argued that this is a

[1] I have not outlined a comprehensive or logically bounded taxonomy. My characterization is only an investigation of major efforts made to resolve the tension.

[2] There are different versions of a hybrid theory of reference, each with different potentials (see, e.g., Psillos 1999; Evans 1973; Bird 2000; Psillos 2012).

significant move forward in explaining the complicated biological cases (and it can successfully satisfy my crucial desideratum E); yet, it is still an incomplete project. Lennox (2013a, 2013b) does not give an account of concept possession conditions, and it is very likely that his account will fail to support any determinate conditions for concept identity. What my desideratum D requires of an adequate account is to give objective conditions for a change of the semantic value of a concept. Said differently, Lennox does not provide an objective condition to determine whether or not a concept is changed in a particular context; without providing this crucial condition, one can reduce Lennox's account to a theory that is stricter than a causal theory or more flexible than a descriptivist one. In the absence of an objective condition, one's account of conceptual change is vulnerable to the threat of interest relativity.

The third approach to conceptual change, of which Brigandt is a prominent example, also focuses on concepts rather than on reference; however, unlike Lennox (2013b), Brigandt places more emphasis on epistemic components and reduces the importance of reference. In Chapter 3, I discussed in detail how Brigandt's theory of concepts suffers from interest relativity. The advantage of Brigandt's theory of concepts' lies in its explaining biological case studies. Indeed, while classical theories of reference have difficulties in explaining scientific progress in biological cases that involve floating reference, Brigandt's theory can keep the stability of concepts in radical referential shifts (thus satisfying desideratum C). The introduction of epistemic goals as a novel semantic component of concepts helps to understand how the same term with the same meaning can pick out different referents in different situations. This is the attractive aspect of some non-representational theories of concepts that allows it to satisfy my desideratum E.

Furthermore, a notable difference between Brigandt's semantic theory and Lennox's account of conceptual change (i.e., the second and third approaches) is the importance of epistemic goals in meaning constitution. While Lennox limits his project to giving a normative structure of concepts, Brigandt adds an assumption that epistemic goals play a crucial role in his semantic theory. I argued that, among other issues, the most important problem of this sort of a non-representational account of concepts is its perspective dependence (desideratum D) (see Chapter 4 for further detail). Moreover, if one assumes that epistemic or practical factors at least partially constitute the *content* of concepts, the isomorphism between the representational system and reality is broken. This opens the gate for stronger non-representational theories to hold that the world itself does not have an overall structure; concepts are shaped in response to scientists' manipulative interests (Waters 2017). By means of rejecting the existence of theoretical entities, this account is not anti-realist; however, it is definitely not realist, as it assigns an active role to epistemic agents to describe the world based on their interest-created structure. Waters (2017, p. 92) argues that the world is such complex and messy that the concepts and theories are not directly reflective of the world. Therefore, according to Waters, conceptual practice in the case of molecular 'gene' does not represent a natural class and, more importantly, this concept (and its theory) cannot give a single correct description of DNA.[1]

As long as I set my goal on a scientific realist and a flexible account of conceptual change in terms of reference change, these three approaches fail to satisfy my desiderata. Accordingly, in Section 4, I will develop a new framework where these desiderata can be successfully met. I accept that users' interests and contexts of use make it possible to articulate different concepts (and theories) about one referent. Sometimes, articulation of concepts makes it possible to refer to different objects from different perspectives. However, my additional realist constraint is that there is only one truth about one specific aspect of an element (i.e., the truth of one theory about X is translatable into truth about X in another theory). This does not mean that all aspects of an entity X can be fully accounted by a single (ultra) theory; rather, it means that all theories of aspects-of-X can be seen (reduced to) as theories of X. Indeed, our intuition about rational scientific inquiry implies a referentially continuous and conceptually flexible explanation of conceptual change. In the remainder of this chapter, my aim is not to defend or propose one specific theory, but to argue for a referential framework which makes room for further research on an adequate account of conceptual change.

4- A New Framework

While it is very easy to put forward ideal conditions on an account of conceptual change, such a proposal would be useless unless there are resources to satisfy all conditions simultaneously. Accordingly, in this section, I sketch a framework that has the potential to do so. My framework can be divided into two parts. First, I focus on an account of concepts and how linguistic coordination on the same topic is possible even when language users do not share any particular set of inferential roles or epistemic goals. In this first part, my aim is to sketch a framework for individuating concepts that can satisfy my first three desiderata. On establishing such an account of concept competence, I then turn to the question of reference determination: How can sameness of concept secure co-reference within a given conversational context? Thus, in the second part, my aim is to explain how the proposed conceptual framework can satisfy my second three desiderata on an account of reference determination. The two-fold structure outlined above is designed to address both the conceptual and referential desiderata I outlined in Section 2. In addition, the proposed framework also accommodates the corrigibility desideratum to connect the conceptual and referential parts.

[1] Waters (2017) concludes with a rejection of having the best, principled, unified and comprehensive way to describe the world, which I think is not in accord with my realist stance (particularly, desideratum F). Waters (2017) argues that, since scientists construct causal models to explain *their way of understanding* the world, they articulate "a multiplicity of theoretical concepts and causal principles" (p. 130). See also Waters (2014); Kellert, Longino and Waters (2006).

4.1. A Relational Account of Concepts

As I explained in Section 2, a central challenge in finding an adequate account of concepts is to explain the possibility of linguistic communication, rational epistemic guidance and epistemic progress. More specifically, an adequate account of concepts must explain how different concept users, with different conceptions of the (alleged) reference, different background beliefs and different epistemic goals, manage to non-accidentally coordinate on the same subject matter (Desiderata A: Communication). In addition, concepts are supposed to provide normative guidance to help navigate rational empirical enquiry into the nature of one subject matter—that is, epistemic guidance must be based on thinkers' prior understanding and available evidence (Desiderata B: Rational Guidance). Finally, according to full-blooded scientific realism, this rational enquiry guided by one's prior understanding can non-accidentally lead thinkers closer to the truth about the reference of their concept (Desiderata C: Progress). The classical Fregean way to meet this challenge has been to claim that concept users must *share a specific pattern of understanding* (IR and EG) that is sufficiently detailed to fix the reference of the concept in the context. However, as discussed in Chapters 2 and 3, such universally shared patterns of understanding are missing in many (if not most) cases of scientific communication.

Moreover, the classical accounts, which presuppose a specific shared pattern of understanding, have difficulties explaining the corrigibility condition (Constraint G). Many scientific realists—particularly those who see progress as a key feature of scientific realism—agree with Quine (1951, p. 43) that *no specific element* of conceptual understanding is immune to rational revision. These realists would resist the Fregean idea that all users of a concept implicitly master a pattern of understanding that grounds analytic truths, as the history of science has taught us that specific reference fixing assumptions, scientific theories and epistemic methods are all rationally revisable in the light of novel empirical enquiry and theoretical advances. Therefore, rational epistemic processes can lead to open-ended revisions in the patterns of understanding, which would lead to variations between individual speakers.

One promising way to overcome this challenge is to explain competence with the same concept by having the *right causal connection between individual users*, rather than sharing some theoretical patterns of understanding (e.g., sharing designated IRs or EGs). This strategy can help explain how users, without sharing the same pattern of understanding that suffices to fix the reference, can still successfully communicate about the same subject matter and keep track of a single reference. A helpful analogy for this sort of causal condition is anaphora—i.e., where speakers can non-accidentally coordinate on the same reference without sharing any particular criterion (IR or EG) for identifying that reference. For instance, three individuals (A, B and C) might use anaphoric pronouns to non-accidentally coordinate in speaking about Kevin Rudd, even though they each have a very different understanding of who he is and how to identify him.

A. The man I just met was an Australian politician, wasn't he?
B. He was the PM who banned guns in Australia.
C. Oh I know him—he was my next-door neighbour.

The fact that these interlocutors are all causally interacting in conversation and intending to coordinate their use of 'he' with each other explains how they manage to non-accidentally co-refer (and know that they do so) despite not sharing any specific criterion for identifying the reference. Their coordinating intentions can allow them to co-refer even despite different opinions about who exactly they all are referring to.

Definitely, the relational strategy for explaining semantic coordination is not entirely novel. Kripke (1980) and Devitt (1981) appealed to causal-historical chains of co-referential intentions to explain how individual speakers manage to semantically coordinate their use of proper names despite a wide-ranging variation in individuals' understanding. More recently, a number of philosophers of language have appealed to the similar causal relations among individual uses of an expression to explain non-accidental semantic coordination for a variety of different types of expression (Burge 1982; Millikan 1984; Kaplan 1990; Fine 2007; Schroeter 2016; Richard 2019). A common assumption of these relational theories of semantic coordination common is that causal-historical relations between individuals' uses of a term play a role in demarcating the units for semantic interpretation (cf. Schroeter 2012).

Let us clarify the relational approach to concepts with a more relevant example about a theoretical scientific term. A cancer patient (P), her doctor (D) and a genetic researcher (R) might be able to use the term 'gene' on a given occasion. Therefore, in a given conversation in the clinic, it is completely plausible that all of them coordinate on the very same reference, partly in virtue of the fact that they intend to communicate about the same topic. A typical conversation would be as follows:

P: I am worried that I have a *gene* that predisposes me to breast cancer.
D: Don't worry. I'll conduct some tests to see if you have one of the risky *genes*.
R: I have been studying the *genes* implicated in breast cancer for ten years—and I'm afraid some of your tests are out of date.

In this conversation, it seems that the three interlocutors do not necessarily share the same substantive understanding of *genes* related to breast cancer. Plausibly, each of them associates 'gene' with rather different ways of understanding its reference. The patient's understanding of genes is likely to be quite incomplete and may contain a number of erroneous assumptions. In contrast, the doctor and the researcher would understand the reference of 'gene' more accurately. On another level, the doctor and the researcher would also vary in their specific understanding of the cancer genes (and probably, their reference fixing criteria are different), as the doctor's understanding may not include the detail of genes' molecular structure and behaviour. However, the fact that all three intend to coordinate on the same subject matter (i.e., specific defective genes raise the risk of cancer) within a specific context (i.e., diagnosing a genetic issue) secures their successful communication. In Section

4.3, I will explain to what extent these interlocutors are sharing the same reference; here, I will restrict my claim to explaining semantic coordination. On a relational approach to concept individuation, successful linguistic communication depends on interlocutors standing in the right causal historical relation with each other. The default presumption among the users (and theorists) will be that appropriately linked uses of a term must be given for the same semantic interpretation.

Of note, each effective episode of communication requires some overlap in understanding. However, the point of a relational account is that there is no *strictly necessary* pattern of understanding that all users of a term must share for them to express the same concept. In the anaphora example (Kevin Rudd) discussed above, all three interlocutors (A, B and C) were causally interacting in a conversation and intending to coordinate their use of 'he' with each other, although they did not share any specific criterion for identifying the reference. In that specific conversation, speakers could add or delete some beliefs to their own reference fixing criteria, but still keep track of the same subject matter, in virtue of the causal interactions with other speakers in the conversational context. For example, suppose an extra speaker (D) enters the conversation and adds:

> D: You're wrong B, that guy is Kevin Rudd, but John Howard was the PM who banned guns in Australia.

In accepting D's claim, earlier interlocutors (A, B and C) must reject B's earlier characterisation of the referent as the person who banned guns. However, it is plausible that all four interlocutors continue to knowingly co-refer to the same person (Kevin Rudd), while revising their criteria for identifying that person (i.e., that he is not the person who banned guns). In this conversation, each individual has his own idiosyncratic bundle of information associated with the linked anaphoric expressions. Each individual's understanding of the identifying characteristics of the referent may be partly ignorant or mistaken. Yet, the anaphoric relations linking the interlocutors to each other will normally suffice to ensure that all linked uses of the term are assigned to the same referent—whatever that referent turns out to be. (As we'll see below, the assignment of a referent may depend not only on aspects of interlocutors' understanding, but also on external facts about their environment). Therefore, anaphoric relations between speakers' use of an expression provide a strong default presumption in favour of co-reference. However, this presumption can be defeated when there is a whole-sale mismatch between interlocutors' patterns of understanding. For example, if yet another extra speaker (E) comes to the discussion and adds:

> E: Wait, no, he's an American actor—he plays Ant-Man!

Here, E's understanding of 'he' is just too different from that of speakers A—D to plausibly co-refer with them. E's understanding of the referent of 'he' is arguably too peripheral from the conversation, too different from that of other speakers, as well as too deeply anchored in his interaction with a specific actor, to succeed in co-referring with other interlocutors. It is plausible that E is referring to Paul Rudd, the actor, not Kevin Rudd, the former Australian PM that A met in the first place. In such cases of a radical mismatch between interlocutors' referential commitments, the default presumption in favour of semantic coordination can be defeated.

A relational model of conceptual competence works in a similar way. By using the same term, 'gene', individuals signal their intention to coordinate with others on the same reference in the same conversational context. For instance, in the sample conversation between the patient, the doctor and the researcher, each interlocutor *implicitly* intends to coordinate her use of the term with those of other speakers to pick out the same reference. Unlike in the case of anaphora, however, the conversational context is not limited to the three participants in the conversation—these three individuals are relying on a larger conversational context that includes also a medical and genetic research program on investigating the hereditary factors that influence breast cancer. Therefore, in this case, the conversational context extends to include a much longer series of interlocutors with whom each of the three interlocutors, in addition to coordinating with each other, intends to coordinate. For example, the patient may have heard about genetic factors in breast cancer from a magazine article, whose author was reporting on the basis of interviews with genetic researchers in a specific research program, while the doctor may have acquired her knowledge of genes from medical journal articles and textbooks linked to another group of genetic researchers, and the researcher herself may have many different links to other researchers in the area. In this way, the participants of the conversation are causally linked—not just to each other, but also to continuous causal-historical *representational tradition* into the past and out into the social community where each speaker implicitly takes his use of the term 'gene' to co-refer with those of his interlocutors.[1]

On a relational model of concepts, successful communication depends on speakers' uses of a term like 'gene' being appropriately (causally) linked to the same continuous representational tradition—*rather than by those uses being associated with the same pattern of understanding.* Simply participating in the same representational tradition normally suffices to secure successful communication—provided that a speaker is not strongly committed to clashing patterns of understanding (IR and EG) that cannot be reconciled with those of others in the representational tradition. In our example, using the term 'gene' in their conversation, all three interlocutors (Patient, Doctor, and Researcher) understand themselves to be coordinating on the same subject matter. To accomplish this coordination, there is no need for each of them to share any specific pattern of understanding (i.e., the same specific set of IR and EG).

[1] I borrow the notion of a 'representational tradition' from Schroeter and Schroeter (2014). Different relational theorists propose different accounts of how exactly these traditions are individuated.

4.2. Meeting the Constraints on Concepts

While the Fregean model of concepts requires that the same concept (and thus communication) shares exactly the same reference-fixing criteria, on a relational model, sharing the same concept is relatively easy—it involves coordinating intentions to link speakers to the same representational tradition and the absence of strongly clashing patterns of understanding. In turn, this easy access to conceptual competence helps to explain how experts and novices, despite their very different patterns of understanding, can communicate information about a subject matter. (I will discuss the issue of how reference is determined on a relational model in Section 4.4. For now, it is enough to note that speakers in a conversational context all *take* their uses of 'gene' to co-refer, so they take themselves to communicate about the same thing). Therefore, a relational account provides a promising explanation of how conceptual competence grounds communication between novices and experts about the same subject matter, despite variation in their patterns of understanding.

A relational account also offers a promising model to explain how users of the term 'gene' can coordinate on different subject matters in different conversational contexts. Referring to our previous example (i.e., the conversation between the patient, doctor and researcher), the interlocutors' use of the term 'gene' is linked via a continuous representational tradition back to specific scientific uses of the term about the mechanisms that predispose hereditary factors that increase the risk of a certain kind of cancer. While this specific tradition of using the term need not be known by the interlocutors, it helps to tie their talk to specific scientific fields and research agenda that can narrow down the referential candidates for their use of the term.

Let us now consider a different conversational context—the one where a mother (M) and an uncle (U) are causally tied to a different representational tradition of using the term 'gene'.

> M: My son has blue eyes—he must have gotten those genes from his father's side, they surely didn't come from our side of the family.
>
> U: Didn't mom's uncle have blue eyes? The gene for blue eyes could've skipped a generation.

Although the two interlocutors may not realize it, their uses of 'gene' in this conversation are tied to a slightly different representational tradition that leads back to theoretical work in Mendelian genetics and inductive experimentation in selective breeding for specific traits. This link to a broader representational tradition can help explain how M and U manage to coordinate on a slightly different subject matter than the one in the earlier conversation between P, D and R (linked to conversations within molecular genetics and about the genes that increase the possibility of cancer). Therefore, the causal-historical traditions involved in the two conversations are slightly different, and this is something that an expert like R could in principle understand. Furthermore, an expert like R would be in a position to recognise that the use of 'gene' in the two conversations picks out slightly different elements of the biological hereditary system. For naïve users, however, it is enough to simply participate in these different conversations and to intend to coordinate for the communication to be successful. Simply entering into the relevant representational traditions normally suffices to knowingly communicate about the same topic, even if one does not know any precise criteria for defining it, and even despite participants' in the conversation having different criteria.

Therefore, a relational model of concepts appears to offer a promising way for a full-blooded scientific realist to meet the Communication Constraint (Desideratum A) on concept identity. Unlike purely referential accounts of concepts, a relational account can explain the possibility of successful communication despite variation in the reference of 'gene' between different conversational contexts. Moreover, unlike non-representational accounts of concepts, such as Brigandt's account, a relational account can explain successful communication between speakers despite variation in both IR and EG associated with their use of the term 'gene'. As I promised earlier, satisfying this constraint helps to explain how non-experts manage to successfully communicate with others, though not having the same pattern of understanding as other experts in the field. This is what Brigandt's account of concept individuation might have troubles to account for. Consequently, a relational model is well placed to meet this static communication constraint on concept identity.

Now, let us consider Epistemic Guidance (Desideratum B). A full-blooded realist should seek to explain how individual speakers can use their current understanding of 'gene' to rationally guide them towards a more accurate understanding of the reference. Since purely referential accounts of concepts place no explicit constraints on competent speakers' understanding (IR and EG), it is not clear how these accounts can explain rational guidance. In addition, we have already seen that non-representational accounts of concepts fail to distinguish between *epistemically* rational guidance towards the truth about the reference of a particular term use and *practically* rational guidance in changing the reference of the term.[1] However, how could a relational model of concepts fulfil this role?

One might worry that the mere intention to co-refer with others in a representational tradition will not provide sufficient normative standards for speakers to rationally choose between different characterizations of the reference. In fact, relational models have more resources to explain rational change than it may seem at the first glance. A competent speaker's understanding of 'gene' will not be confined to the mere presumption of semantic coordination with others. On the contrary, each speaker will have his own substantive understanding of the reference (his own total pattern of IRs and EGs). Also, competent speakers' assumption of semantic coordination with others in a conversational context entails that the substantive understanding of others in their representational tradition is about the

[1] In contrast, the Fregean model of concepts appears to be apt to explain rational guidance towards the truth about the reference, since conceptual competence on that model consists in implicit knowledge of criteria that settle the reference (via a specific set of IR and EG). The problem with the Fregean account is that it fails to satisfy the Communication and Corrigibility Constraints, since it requires all conceptually competent speakers to share specific and accurate reference-fixing criteria.

same thing. As long as speakers assume their conversational partners to be their epistemic peers (or superiors), they will assume that they have more or less accurate information about the reference of their shared use of the term in a context. In this way, a speaker's presumption of semantic coordination with others actually expands the quantity and quality of substantive understanding (total IR and EG) available for rational guidance in revising one's beliefs about how to define the reference of 'gene' in a context.

Furthermore, relatively naïve users of the term 'gene' (like the patient (P) in the conversation about cancer) would be in a position to recognise that the doctor (D) and the researcher (R) are both better informed about the representational tradition she is participating in. In turn, those two experts (D and R) will have more articulated patterns of understanding that allow them to identify further epistemic resources to guide their inquiry into the precise nature of the reference of 'gene' in this context, if they choose to do so. However, the conversation between two naïve speakers in the second conversation—mother (M) and uncle (U)—does not allow for direct reliance on the expertise of the other. Yet, their shared interest in explaining trait inheritance in this conversational context would put them in a position to look for further information that would rationally lead them towards Mendelian genetics in characterizing the precise topic of their conversation, should they choose to pursue the matter. Therefore, a relational model does seem to afford the resources for explaining rational guidance, despite its minimalism about the substantive understanding required for sharing a concept.

Next, with regard to the Epistemic Progress constraint (Desideratum C), an account of epistemic progress *towards a true* characterization of the nature of genes requires an account of reference determination of particular uses of the term 'gene'. In what follows, I will outline an account of how a relational model of concepts could explain how rational epistemic guidance might produce a *greater isomorphism* between speakers' understanding of a term like 'gene' and the element of the causal system of biological inheritance (see also Section 4.4 for a detailed discussion of reference determination). If a speaker's total system of understanding associated with 'gene' becomes (i) finer-grained or (ii) more closely isomorphic to the objective causal mechanisms underlying biological reproduction, then that change in understanding should count as *epistemic progress*, regardless of what one holds about the semantic notion of truth.

Arguably, a relational model is in a good position to explain this sort of epistemic progress in a speaker's understanding of 'gene'. I would distinguish conceptual progress of the following two kinds of language users: (i) non experts and (ii) experts. As I have already argued, having relationally connected to a representational tradition, relatively naïve users of 'gene' are provided with epistemic resources to guide their understanding to a finer-grained and more accurate account of genes. For example, user P has access to the experts in the field of medical genetics (D and R) to improve her knowledge of genes. The longer D or R talks to P, the more precise and accurate information she gets. Now, let us consider the expert researcher (R) in that conversation. She is in a position to recognize a meaningful connection between all uses of 'gene' across different conversations, and this rough understanding is likely to be shared by most competent speakers (e.g., it is used to refer to an aspect of reality responsible for some biological reproduction processes). Moreover, R is in a position to recognise the representational traditions associated to different conversations using the term 'gene'. Also, she should be able to identify the more specific substantive assumptions (IRs), research goals (EGs) and experimental paradigms implicated across alternative contexts. Accordingly, when she tries to communicate with different coordinating intentions—for example, when participating in a discussion with her new colleague—R can come to develop a finer-grained understanding of aspects of the same causal system. Here, I assume that learning new aspects of a causal system may lead to a finer grained understanding of the overall system. From a conceptual point of view, when R learns a new use of 'gene', which refers to a different set of referents for different epistemic questions, she acquires a new concept.

Therefore, unlike a pure referential model of conceptual change, the relational model can explain epistemic progress without positing a single stable reference for the term 'gene' across all conversational contexts. Furthermore, unlike in Brigandt's non-representational account of concepts, there is an explanation of genuine epistemic progress that is rationally guided by the individual's prior conceptual competence.

Finally, let us consider the Corrigibility Constraint. A full-blooded realist, as I previously suggested, should be inclined to agree with Quine that any *specific aspect of substantive understanding* (specific patterns of IR and EG) is in principle open to rational revision in the light of empirical inquiry. If this is correct, then no non-revisable IR or EG should be rationally regarded as the competence condition of a concept. Said differently, there should be no fail-safe insight into the nature of the empirical world. By not requiring any specific substantive aspect of understanding to count as conceptually competent, a relational model of concept individuation is well-placed to vindicate this realist commitment. Therefore, interlocutors in our sample conversations can have quite different patterns of substantive understanding (IR and EG), without jeopardising their conversational coordination in a given context. In each case, we may be able to identify some specific assumption or goal that all current interlocutors share; however, it does not follow that this assumption or goal is strictly required for conceptual competence in the context. Similarly to the anaphora case, we can always imagine other speakers successfully joining the conversation without sharing that specific aspect of understanding. Admittedly, there must be some general compatibility among different speakers' understanding (i.e., they cannot disagree in every respect), but no specific aspect would be strictly required. This lack of strict requirements on speakers' substantive understanding (IR and EG) suffices to meet the Corrigibility Constraint on conceptual competence. This constraint plays an important role in a scientific realist's stance, as it allows for an open-ended epistemic inquiry and a revision of beliefs in the light of novel empirical inquiries.

This concludes my outline of how a relational model of concepts could satisfy the four constraints on a full-blooded realist account of concepts outlined in Section 2. In Section 4.3, I will consider a specific example of a relational model due to Mark Richard (2019).

4.3. Mark Richard's Relational Account of Concepts

In this section, I review an example of a relational theory that could successfully meet my first three desiderata on an account of concepts, in addition to the corrigibility desideratum. Here, I will focus on Mark Richard's (2019) recent account of concepts in his *Meaning as Species*. Here, my aim is not to cover all details of Richard's (2019) fully elaborated position. I am also not going to defend the view he is proposing in the book. Rather, I confine myself to introducing a relational framework and showing how Richard's (2019) analogy between a relationally-individuated notion of a biological species and a relationally-individuated notion of meanings (or concepts) can help to understand the promise of the approach for a scientific realist account of conceptual change.

Richard's main concern is to give an account of linguistic competence that explains successful and reliable uses of terms in communication, even in cases when there is no perfect overlap in users' associated pattern of understanding. Richard makes an analogy between this issue and the variation of genomes in the members of a biological species, who successfully mate with each other without a perfect overlap in their alleles: "[a]s there is variation among the genomes of individual speakers, so will there be variation across the flock about what is commonly presupposed by users [of a term]" (Richard 2019, p. 4). Both in biological species and lexical items, there is variation within a certain range of similarity. In the case of species, this pattern is causally explained by the causal-historical relations linking an interbreeding population and biological inheritance relations among them. The notion of shared meaning, Richard suggests, is like a biological lineage where there is no set of necessary and sufficient conditions for belonging to the same species; however, the range of variation in the alleles of individual members is constrained in ways that allow for mating among members of a group.

Richard (2019) suggests that the patterns of similarity and variation we see in meanings (or concepts) can be explained by similar causal-historical mechanisms. In *Meaning as Species*, sameness of meaning is explained to consist in a rough overlap in the patterns of shared understanding at a given time in a linguistic community or what Richard (2019) calls "interpretive common ground", or ICG (p. 65). ICG is what individual speakers expect random other users of a term in their linguistic community to presuppose as common knowledge. Richard's aim is to show that the meaning of lexical items is their ICG, knowing that ICG "is pretty clearly a species-like phenomenon" (p. 4). Thus, ICG *does not* provide a neat set of necessary and sufficient conditions for being a competent user of a term. Rather, the range of variation is constrained in ways that allow for successful communication ('mating') among members. Therefore, having the same-meaning relation depends on causal-historical relations among members of a single representational tradition with a word, which fosters a rough family resemblance in their associated patterns of understanding. This family resemblance allows for semantic coordination, meaning that interlocutors take themselves to understand each other (in particular contexts). What enables communication with one shared concept is a rough overlap in the overall patterns of understanding of interlocutors in particular contexts, which tends to reinforce the overlapping patterns of ICG among interlocutors who engage in regular conversation.[1]

In sum, Richard proposes a structural analogy between biological species as causal-historical lineages of individuals with roughly overlapping patterns of alleles, on the one hand, and public meanings as lineages of individuals' overall understanding of a word with roughly overlapping assumptions about ICG, on the other hand. According to Richard (2019), communication does not require any perfect match in ICG between speaker and hearer – only a sufficient overlap, and the precise overlap can vary from one conversation to the next.

Let us now consider how Richard's picture might apply to the meaning of 'gene'. Roughly, ICG for 'gene' (i.e., what we expect ordinary speakers of English to presuppose) is that genes are basic units in the causal processes by which biological organisms pass traits down to their offspring. In addition, users of this term tend to presuppose a naïve realism about genes: that is, we take it to be common ground among competent users of the term 'gene' that there are some real causal mechanisms that play causal roles about which we can learn more. Therefore, this basic ICG helps to explain how ordinary speakers coordinate on the same topic. Their shared understanding provides some rational guidance for further inquiry—helping to orient naïve speakers towards certain elements within objective systems of biological reproduction. It can also be supplemented with more specific shared ICGs in specialized contexts. For instance, when an ordinary speaker is in a conversation with an expert, and both speakers are aware of this epistemic imbalance, it may be part of their shared ICG in the context that the expert's opinions are more accurate in identifying specific relevant features of whatever is picked out by their use of 'gene'. On the other hand, in a conversation between two molecular geneticists, their shared ICG may be supplemented with presuppositions about detailed scientific theories, facts about a specific history of experimentation and the experimental apparatus employed, particular details of the reproductive systems of certain biological species, and so forth. Therefore, in this conversation between two molecular geneticists, we can expect them to share a much richer set of presuppositions in their ICG. On Richard's (2019) view, these overlapping ICGs can help explain how individuals coordinate their own understanding of a term with that of others in a context. If the ICGs associated with 'gene' are not sufficiently similar, speakers' efforts to

[1] Richard (2019) develops this account of shared meaning via a version of use theory. He argues that having the right relation with ICG (or being part of the same representational tradition) requires one to know how one is supposed to use a concept (p. 110). However, his understanding of 'knowing how-to-use' implies that one user of a term expects that her audience presupposes what she herself presupposed in her own use of the word.

communicate would be hampered.[1] Importantly, having similar ICGs does not mean having some necessary elements of shared understanding—rather, it means that a successful communication requires competence with the concepts.

The interlocutors' ICGs can also afford epistemic guidance in their efforts to refine their understanding. Successful interlocutors will share some rough family resemblance in their ICGs and other patterns of understanding associated with a given term. As discussed above, the most basic ICG for 'gene' can be supplemented in different ways within particular conversational contexts. These more detailed ICGs can help interlocutors to focus on particular aspects of biological reproductive systems relevant to the representational tradition they are connecting to. Using my previous example of a conversation between the patient, doctor and researcher, all three interlocutors are relying on the researcher's ICG to guide them towards the broader research tradition.

The fact that the understandings of all interlocutors are more or less shaped by the broader representational tradition helps to understand how their ICGs could be improved at the end of their conversation. For example, if the doctor and researcher give the patient some new information about her infected genes, the ICG between them could be improved to that extent. Furthermore, the fact that all three speakers interact with specific elements of biological system through theory and experimental practice helps to explain how they all could be pointed in the direction of a greater isomorphism between their system of beliefs involving 'gene' and elements in the biological systems manipulated in the given scientific practice.

Therefore, Richard's (2019) account of the historical mechanisms that stabilize ICGs illustrates how the four constraints I formulated for an account of competence with the same concept might possibly be satisfied by a relational model of concepts. Richard's (2019) picture allows for semantic coordination despite variation in individuals' understanding, and the (approximately) overlapping ICGs appears to be suitable to provide both rational epistemic guidance and progress towards a more accurate understanding of the reference. Definitely, there is much work to be done to fully vindicate these realist constraints. Other relational theorists have proposed slightly different accounts of the mechanisms involved (see Millikan 2017). However, for now, my goal is only to demonstrate that the relational approach to concepts is a promising route to pursue.

4.4. A Relational Account of Reference Determination

4-4-1- Basics

Thus far, I have outlined a relational account of concepts to provide a better structure for understanding conceptual change. In this section, I will complete the framework with an

[1] For instance, if it were part of the novice's presupposition that 'genes' were a type of trousers worn on casual occasions, this would significantly hamper their efforts to communicate with a molecular geneticist about the nature of genes.

account of reference determination. The aim of this account is to meet the referential constraints discussed in Section 2. Before that, however, let us recall some terminology.

I suggested that a *concept* expressed by a term should be individuated by a representational tradition, which is a lineage of links among different individuals' uses of the term in different contexts and their associated *patterns of understanding.* On this relational approach, concepts are individuated by speakers' implicit *intentions to semantically coordinate* with others on a single reference in particular conversational contexts. These *coordinating intentions* demarcate a restricted lineage of causal-historical relations among users of a term, or what I call a *representational tradition*. By looking at the other uses to which a term is linked to in the right tradition, a particular use of the term in a given conversational context is then assigned a *reference*. Representational traditions are associated with overall patterns of understanding (about) and environmental interactions (with) an object that specifies the characteristics of concepts. Therefore, speakers' coordinating intentions implicitly connect a particular use of a term to a long lineage of uses, all of which express the same concept.

Most current theories of reference determination for scientific terms fail to reflect these complex coordination intentions associated with particular uses of a term in a given context. Instead, theories of reference usually seek to fix the reference of all competent uses of *a scientific term*, regardless of the concept it expresses. In previous chapters, I argued that the reference of some scientific terms is constantly changing across time or is sensitive to the context, which would trigger different reference fixing conditions for each particular use.[1] A context-neutral approach to scientific terms ultimately leads to an under-determination of reference. As a result, this led many philosophers of science to conclude that, at least for some scientific terms, all theories of reference fixing must fail (Kitcher 1992; Brigandt 2010b; Weber 2014). In particular, biological terms such as 'gene' seem to have 'floating reference': depending on the scientific contexts where they are used, such terms pick out different referents.

The relational account I propose in this chapter is a way to rescue the referential approach from this threat of floating reference. My basic strategy to this end is to restrict my aim to determining the reference of *one particular concept* (a specific lineage within a representational tradition), instead of a *public meaning* (the lexical tradition as a whole) or one *individual's specific pattern of understanding*. Since a competent but inexpert user of a term could be unaware of all specific details of reference-fixing conditions, the reference of his term token may not be fully determined using his knowledge about the object. Also, a public meaning shared by all competent users of a term could refer to a broad range of objects falling under the term's extension. In order to precisely determine the reference of a

[1] Kitcher (1984) acknowledges that different biological enquiries require different concepts of species, some of them classifying different individuals into a species: "our objective interests may be diverse, that we may be objectively correct in pursuing biological inquiries which demand different forms of explanation, so that the patterning of nature generated in different areas of biology may cross-classify the constituents of nature" (p. 330). See also Dupré (1993) for a similar line of thought.

particular term use, we need to understand which concept is expressed in a context. This concept is a very specific and articulated unit of thought, characterized by the representational tradition and implicitly intended by language users.

4-4-2- A Pre-Task

The reason why I started outlining my framework with a relational account of conceptual competence is that this allows us to individuate the concepts expressed by a term on a given occasion based on interlocutors' coordinating intentions in a conversational context. The key point of a relational account of reference determination is to demarcate the expressed concept *before* fixing the reference. Therefore, in order to determine the reference of a term like 'gene', we need to link a particular use of the term to a representational tradition in order to demarcate the expressed concept and then start fixing the reference. A relational account of reference determination implies that not all uses of 'gene' will express the same concept, and not all uses of the term will have the same reference. The reference of 'gene' can vary across different contexts, simply because the concepts expressed by that term are different.

As discussed previously, a conversational context includes facts about interlocutors' coordinating intentions, which link them to a wider representational tradition. It is this representational tradition that ultimately determines the concept expressed by a particular use of a term. Using coordinating intentions to determine the relevant representational tradition is what I call *a pre-task for theories of reference*. The pre-task of reference fixing is not part of a theory of reference, as it does not tell us how the reference is determined by the facts about a representational tradition. Furthermore, reference is not determined only by the coordinating intentions. Rather, the pre-task individuates the object of interpretation—the relevant representational tradition—before any theory of reference can be applied. In the case of terms that are capable of expressing several concepts in different contexts, like 'gene', the pre-task is to narrow down the overall lexical tradition with that term to a more specific representational tradition that reflects the specific coordinating intentions of interlocutors in a particular conversational context.

What my outline of a relational account above suggests is that what generally determines that particular uses of a term will be assigned the same reference is the speakers' (implicit) intentions to coordinate with other speakers, marking out a specific representational tradition within a broader lexical tradition with the term (which allows for communication across contexts). One worry here is that '*speakers' intention to coordinate with others'* could be linked to a flawed, or at least a holistic understanding of reference held by other users. This issue is more critical when a term use in the scientific context needs to be linked to the proper representational tradition, not the one held by non-experts or a holistic understanding of all scientists. Having no distinction between different groups of experts who use the same term in different contexts to pick out different referents brings about the same issue that we had with the molecular and classical geneticists. A way to untie my notion of representational traditions from this kind of holism is then required. Admittedly, providing a comprehensive theory of how representational traditions (and concepts) are specifically individuated would require articulating a full theory of concepts, but this goes beyond the scope of the present thesis. But in the remainder of this chapter, I will discuss how reference is assigned to one specific representational tradition and it would answer some worries about holistic individuation.

4-4-3- Reference of Representational Traditions

The central insight of a relational account of reference determination is to take the representational traditions as the unit to which reference is assigned. This means that determining the reference of a concept is not restricted to an individual's pattern of understanding, including his IR, EG or coordinating intentions. Rather, reference determination should consider *the total understanding associated with the whole tradition and its history of interaction with the environment*. One specific lineage of representational tradition ideally contains all external factors considered by experts, as well as specific interests underlining one aspect of the external world. This characterizes an articulated concept with a precise set of reference-fixing conditions.

In response to the concern I raised regarding holism and individuating a specific lineage of representational tradition, I have two points to raise. The first point concerns experts' uses of a term, where they need to be connected to the most accurate representational tradition available for that term in the context. Here, one way (among other possibilities) is to use Putnam's notion of 'division of linguistic labour' (1973). According to this strategy, individual speakers defer to 'a special subclass of speakers', treating those experts' understanding of the reference as more accurate than their own (p. 705). In this case, we need a very articulated and specific tradition, rather than a typical pattern of understanding. But this is not the only possible strategy, what it does is to individuate a specific representational tradition in terms of a chain of deferential relations among particular users. So, when a researcher uses the term 'gene' we need to determine which representational tradition most accurately captures his/her intention to use the term. This account does not suffer from the interest relativity problem I raised in chapter 5, because the decision on what is the most relevant representational tradition is not dependent on the external interpreters. In contrast, they have the difficult task of finding the best way to objectively interpret a language user by connecting one particular use to the most appropriate representational tradition.

The second point concerns non-expert users. I admit that a specific lineage of representational tradition could be wrong about one or another property of the object it refers to. Due to the fallible nature of scientific knowledge, a representational tradition may turn out to fail to refer. However, what is important about a representational tradition is that they specify the best available understanding of a subject matter considering all contextual and empirical factors. There is no dissimilarity of interests or dispositions associated with a tradition, since it is an overlapped knowledge of experts understanding that all competent users rely on when using a term. Then, a good theory of reference determination is the one that best serves all the aspects of a representational tradition.

Therefore, when an ordinary user of 'gene', like the patient (P) in our cancer conversation example, uses the term in a particular conversation, her usage is linked to the relevant representational tradition, which in this case the link goes through with the researcher's (R) most up to date information about the genes responsible for breast cancer. This tradition, according to the assumption, has no ambiguity in terms of epistemic variables—whether molecular or classical genetics is intended, which segment of DNA in which cellular circumstance with which products is intended, and so forth. While these specifications would even go beyond R's knowledge and might (not) refer to the genetic cause of breast cancer, the semantic value of patient's term depends on the entire tradition. In this way, it is guaranteed that the communication is successful and there is no floating reference in principle associated with the articulated concept.

One concern here is that, on a relational account, making reference depends on the whole representational tradition, far removed from P's own understanding and commitments to the subject matter. Therefore, it can be concluded that the assigned reference is completely irrelevant to what P has in mind. Yet, this is not completely true. The semantic value of a concept should be connected to one's pattern of understanding as much as it reflects the truth conditions of one's beliefs. This connection is guaranteed by one's causal-historical link to the representational tradition via coordinating intentions. When a rational user implicitly intends to coordinate with others about a subject matter, he is rationally committed to the reference depending in part on facts about that representational tradition and about its relation to the world about which he may be ignorant or mistaken. Without such coordinating intentions, individuals' understanding of a term would ultimately fully depend on their own current criteria for identifying the reference, which would be insensitive to others' opinions and evidence. However, this is not how humans do or should regulate their beliefs—particularly, in the case of theoretical terms. In short, there is a good reason to think that the connection between individual speakers and the concepts they express is grounded in an implicit intention to coordinate with a representational tradition through interaction with conversational peers.

The next step for a theory of reference is to propose an account of how the semantic value of a particular concept is determined based on all the information collected from a representational tradition. In this respect, a scientific realist can appeal to many different theories of reference determination (e.g., Lewis 1984; Millikan 2017; Kroon 2011)[1]. I will not

[1] Lewis' (1984) account of 'Reference Magnetism' can possibly capture the idea, since interpreting language users in a way to maximize their truth utterances is a way to take coordinating intentions into consideration. Similarly, Millikan's (2017) framework of unicept/unitracker is relevant in this topic, for she defines unitrackers as the evolving mechanisms of recognizing reference of more stable units that she calls unicepts. In this respect, I can also mention a more philosophy of science-minded philosopher of language, Fredrick Kroon (2011), who articulates an account of reference determination that takes the importance of epistemic conditions into

seek to choose among them. What is important for my purpose is simply that there should be some ways of assigning reference to representational traditions that can vindicate full-blooded scientific realism. In particular, it should meet the identified constraints on an adequate theory of reference for scientific realism. I believe that many different specific theories of reference, when combined with a relational theory of concepts, can meet these desiderata. Let us turn now to consider these constraints.

4.5. Meeting the Constraints on Reference Determination

Let us turn now to our constraints on an account of reference determination: Objective reference-fixing, Context variability of reference, Approximate Truth of successful scientific theories and Corrigibility of one's current understanding. A relational account of reference determination, I believe, should be attractive for scientific realists, mainly because it can meet the referential *desiderata* I presented previously. However, how can a relational account of reference determination meet these constraints?

An example will drive the point home. Suppose that R in the example of a conversation about cancer is a junior scientist in her first week at work. She tells D that "I will examine P's genes in my lab and get back to you about the results". While R might be ignorant or mistaken about how exactly to individuate the strings of DNA she has to examine, given that she has a solid connection with the relevant scientific representational tradition, we can rationally interpret her as using 'gene' to refer to the way of partitioning of DNA that is relevant to the scientific tradition she is engaged with. By assigning reference based on the scientific representational tradition that R intends to coordinate with, a relational approach to concepts can draw on all of the specialised knowledge, experimentation and other practices that R herself is ignorant of in assigning reference to her use of the word. Therefore, unlike classical representationalist accounts, a relational approach to reference determination does not *wholly* depend on the individual speaker's own patterns of understanding and use. Furthermore, unlike a non-representationalist approach, a relational approach does not rely on an external interpreter's contingent explanatory interests in deciding which representational traditions are relevant to interpretation, and which aspects of the speaker's understanding the referential assignment should vindicate. Instead, a relational approach seeks to treat speakers' coordinating intentions as individuating the representational traditions that serve as the basic *units for semantic interpretation;* it then takes the facts about the understanding and use within that tradition to be objective inputs into a substantive theory of reference determination.[1]

Now, let us consider how this picture helps meet our constraints. Constraint D requires that reference assignment is perspective-independent. In this respect, a relational approach to

account. This list can be largely extended, but what all such theories share is the idea that reference determination requires some extra information about users' intention in the context and one's causal relation with her audience.

[1] As specified in Section 4.1, for present purposes, we can remain neutral about which theory of interpretation is correct, as long as it treats representational traditions as the unit for interpretation.

reference determination has much in common with the classical individualist theories of reference determination, including both causal and/or descriptive theories. Similarly to the classical theories, a relational theory would insist that the facts about the correct reference assignment do not depend on external interpreters' contingent interests. The key difference between a classical theory of reference and the relational account is simply in the units for semantic interpretation. A classical theory takes the unit for interpretation to be a token use of a term, so that semantic interpretation is based on an individual's own understanding and use of a term in his own immediate environment. In contrast, a relational theory takes the basic units for interpretation to be a socially and temporally extended representational tradition, so that interpretation is based on the patterns of understanding and use associated with that tradition and its wider physical and historical environment. Despite these differences, both types of theories (classical and relational) can be thought of as functions that take facts about understanding, use and environment as input and yield a semantic content as output. According to this functional understanding, both types of theories can allow that reference determination is entirely perspective-independent. I have already suggested that relational theorists are free to choose between different theories of the nature of this objective reference determination function.

Next, let us proceed to the Context Variability Constraint (Constraint E). In Chapters 5, I have argued that the context of use can make a difference to the reference picked out by a scientific term like 'gene'. My suggestion for determining the reference of such context-sensitive terms was to individuate the (relational) concepts expressed by them relative to each conversational context before fixing a reference for that concept. I argued that each term use would be linked, via coordinating intentions, to specific lineages within the broader 'lexical' representational tradition of using the term. Ultimately, each such lineage is taken to express one specific concept, which will be interpreted as referring to a particular referent. This means that, depending on a speaker's coordinating intentions, different uses of the term 'gene' might be connected to different traditions that express different concepts and refer to different objects. Therefore, the relational approach to conceptual competence provides a natural way of meeting the Context Variability Constraint (E).

Now let us turn to the Approximate Truth Constraint (Constraint F), which suggests that a full-blooded realist theory of reference should construe successful scientific theories and research programs as getting closer to the truth about objective features of the world. In order to meet this constraint, we need to reconcile two seemingly contradictory commitments. The first is a strong realist commitment about the objective nature of the systems studied by science: Since there is only one way that the world actually is, there is ultimately only one true scientific way to describe it. For example, there is one best way to describe atoms, and scientists should aim to get closer to that ideal theoretical characterization of atoms. The background assumption here is that the aim of scientific theorising is to identify the single best characterization of the objective causal relationships that define the nature of the studied causal systems. The second commitment, seemingly conflicting with the first one, is that scientific terms can express different concepts and, ultimately, refer to different theoretical kinds in different conversational contexts. For example, 'gene' can refer to different aspects of the causal system of biological reproduction in different scientific subfields. Consequently, each research program constructs its own causal models to explain one specific perspective on the causal system it studies. Accordingly, different research programs in genetics approach the units of biological reproduction with different theoretical tools—each of which is scientifically valid. Therefore, it appears that there is no single true way to describe the world—rather, there are multiple ways.[1]

Prima facie, the relational approach to reference seems to come down on the side of the second commitment: with specific terms like 'gene' or 'atom', there are multiple representational traditions that are involved in representing the relevant causal systems in the world. Therefore, a concern here might be that the relational approach could not vindicate the full-blooded realist's ambitions to discover and describe a single objective way the world is. However, once the relational approach is correctly understood, there is ultimately no conflict between the above two commitments.

According to this approach, the difference between the classical and molecular concepts of gene is determined by different lineages of a *single* broad *lexical representational tradition* with the term 'gene'. The lexical tradition includes everyone who counts as minimally competent with the lexical meaning of the term 'gene', which unites all the different more specialised scientific lineages with scientifically naïve uses of the term. When we focus on the lexical level, there is a rough common-sense understanding shared by almost everyone: that 'gene' is used to keep track of basic units in the causal systems governing biological reproduction. However, within this broad lexical tradition with 'gene', we can distinguish several important sub-traditions or lineages associated with more specialised sets of theories, epistemic goals and scientific practices. In particular, we can distinguish between different Mendelian and molecular genetic representational traditions. The key to assigning reference is to determine how to demarcate the relevant representational tradition relevant to the conversation at hand, by relying on participants' implicit coordinating intentions. This task will involve some delicate decisions about how individuals are related via coordinating intentions. However, once this task is complete, the theory of reference can assign one specific reference to the term 'gene' based on the understanding, use and environmental interactions involved in the relevant representational tradition. Once we fixed the reference of 'gene', there will be only one true description of the facts about the designated objects. The different sub-traditions with the term 'gene' may refer to distinct ways of individuating the entities in the objective causal system of biological reproduction, there is no contradiction between the idea that the word may represent different features of the world on different occasions of use and the idea that there is one single true description of the world.

[1] For a similar objection, see Waters (2014) who argues in favour of a pluralist view against a fundamental view of concepts.

Therefore, a relational account of reference is fully compatible with the idea that scientific theories within each specific sub-tradition can be approximately true, and the scientific practices associated with each of those sub-traditions can get closer to the truth about the specific aspect of the causal system it represents. Admittedly, a full vindication of the Approximate Truth Constraint will depend on the precise theory of reference determination. In this thesis, I have not offered such theory. However, my claim is that, for vindicating the approximate truth and progress of scientific theories, a relational approach to reference determination will be as well placed as any non-relational approach. Moreover, the account of different sub-traditions associated with a single broader lexical tradition may help to explain how the variety of different scientific traditions with the term 'gene' are all approximately true, and how their combination helps to more accurately describe the general causal system of biological reproduction than it could be done by sticking to a single referential assignment for the term 'gene'.

Finally, I should consider the Corrigibility Constraint on a full-blooded realist theory of reference (Constraint G). A relational approach can easily explain why individual speakers' current patterns of understanding might be subject to ignorance or error, and could be corrected by empirical inquiry. If reference is assigned based on the patterns of understanding and use associated with the representational tradition as a whole, an individual user's pattern of understanding may fail to reflect the dominant patterns within that representational tradition. Moreover, on a full-blooded realist theory of reference, even the dominant patterns of understanding can be corrigible in the light of empirical facts about the causal systems the tradition has interacted with. Therefore, a relational theory of the basic units for semantic interpretation will make individual understanding doubly corrigible: first, via learning about their representational tradition and, second, via learning about the causal systems it has interacted with. I have already demonstrated how speakers like P and D can improve their reference fixing criteria via communication with a specialised researcher like R. Thus, ordinary speakers' opinions are corrigible in the light of what they may learn from experts. Moreover, via communication with colleagues and interaction with the empirical facts in the laboratory, R herself may have epistemic reasons to revise her beliefs about the referent of 'gene'. New empirical information and empirical theorising can give R and her colleagues sound epistemic reasons to accept a revised way of defining 'gene' for her theoretical purposes as better capturing what 'gene' meant all along within their shared research tradition.[1]

4.6. Reference Fixing for Molecular 'Gene'

In order to see how this relational approach to concepts and reference works in practice, let us consider the controversial case study that has been the centre of our discussion in this

[1] Whether such revised definitions will count as a change of reference or a discovery of the correct reference of their prior use of the term will depend on the details of the case and on the precise theory of reference determination we accept. But the point remains that the relational approach to concepts sketched here allows for genuine epistemic corrigibility, for both individuals and groups.

thesis: fixing the reference of 'gene'. In this section, I will focus on articulating a particular conception of the term 'gene' in different conversational contexts, which ultimately leads to assigning different concepts to different uses of the term. To accomplish this task, I will use Kenneth Waters' (2014) schematic account of molecular genes.

Let us begin with a commonly used understanding of 'gene' as used by molecular geneticists. In molecular genetics, 'gene' is plausibly described as follows:

> G1: '*segments of DNA that causally determine differences among (linear sequences) RNA and are consequently responsible for the differences among polypeptides'*[1].

From the above definition, we can assume that scientists working within the field of molecular genetics have been exposed to a textbook understanding along these lines, and that most of them would be able to produce a more or less similar account if pressed. Then, this rough understanding can be assumed as an interpretive common ground (ICG) in professional conversations among molecular geneticists using the term 'gene'. However, this basic understanding of the molecular notion of 'gene' comes with critical deficiencies in determining the precise reference of the term in a given conversational context. The problem of this basic understanding of molecular genes is that it is "vague, exceptional and ambiguous" (Waters 2014, p. 123-124). First, it is vague because, although genes are the only units that cause those differences among polypeptides, it is not clear where the exact borders of genes as real entities are. For example, are introns, which are segments of DNA and non-coding sections of the RNA transcript, which are spliced out before the translation of RNA into proteins occurs, parts of genes? Second, our basic understanding of the reference of 'gene' is exceptional, because some DNA segments do causally determine RNA, but they are not involved in coding the linear sequence of polypeptides. For example, genes corresponding to tRNA or rRNA are not 'transcribed'[2] into polypeptides. Third, this account of 'gene' is ambiguous in terms of reference fixing, since a DNA is open to the production of more than one polypeptide. In fact, several splicings of mRNA can transfer the multiplicity of overlapping segments to ribosomes for the production of different proteins (Waters 2014, p. 24)[3]

To better reveal the depth of this problem, I should add that the underdetermination of the reference of the molecular 'gene' is not an empirical inadequacy that scientific progress could possibly resolve in the future. Different epistemic goals and points of view can play a role in

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[1] Following Waters (2014, p. 123).

[2] Transcription is the first step in gene expressions in that a particular segment of DNA or the information associated with this segment is copied into an mRNA. The next step is called translation in which the mRNA is translated into a polypeptide chain using rRNA and tRNA (the letters before RNA stands for the kind or function of that specific RNA: m=messenger, r=ribosomal, t=transfer).

[3] Providing different reasons to express the complication in the reference fixing of 'gene', Marcel Weber (2014) states the following five characteristics of genes that make them different from other straight-forward scientific terms: 1) being a relational kind; 2) being a functional kind; 3) being a variable kind; 4) being a generic kind; and 5) being a sortal kind (p. 431). Weber (2014) and Waters (2014) share the idea that fixing the reference of 'gene' is by no means straight-forward.

the reference assignment for uses of 'gene' in different scientific communicative contexts. For example, qualifying introns as part of genes is neither objectively right nor wrong, if we focus on the general lexical tradition with that term. This is where an appeal to specific research traditions can help. It is only when we restrict our attention to more specific representational traditions within certain research fields and explanatory paradigms that a verdict about including introns as part of genes can be determined. Particular scientific practices within the broader representational tradition with the use of 'gene' in molecular genetics are crucial to determining the precise reference of the use of 'gene' by a molecular geneticist on a given occasion.

Now, if molecular geneticists' general understanding of 'gene' (i.e. the ICG among molecular geneticists) does not suffice for precise reference fixing, what is the point of their ICG? My claim is that this basic ICG provides a shared minimal understanding among the competent users of the term in their scientific community. This rough shared understanding, which can be thought of as common ground in a local representational tradition, may help to narrow down the potential domain of referents that can be assigned to uses of the term by molecular geneticists in scientific contexts. While the local representational tradition provides an *incomplete descriptive characterization of a particular role within a causal system*, this is not specific enough to fix a unique reference. This rough shared understanding has been developed through empirical theorising, and it is useful in guiding geneticists in their research (though it is in principle corrigible in the light of further inquiry). Within a specific context, this rough understanding of the reference of 'gene' can be further *articulated*—either explicitly through stipulation or implicitly through experimental or theoretical commitments or epistemic goals—to a sufficient level that uses of the word in that context pick out one specific referent (i.e., one specific way of individuating segments of DNA). This process of reference determination depends on the overall disciplinary matrix associated with the specific representational tradition involved in the speaker's current use of the term 'gene', including specific epistemic goals, background theories, inferential roles and mechanical apparatus that figures in the manipulation and control of a physical system.[1]

So, how do we determine the reference of 'gene' in a conversation between two genetic specialists who work in different research traditions? Let us suppose that these geneticists can successfully communicate with each other using a more articulated ICG than the one minimally shared by all competent speakers. This does not mean that they are connected to the exact same scientific sub-tradition sufficiently articulated to fix a reference—rather, it means that they belong to the narrower group of molecular geneticists who share a more

[1] Being an 'articulated' concept is a relative status. An articulated concept for a representational tradition can be the public meaning for another sub-community of language users. For example, while the fact that genes are segments of DNA would be among broadly shared information of scientists, it would not be such for ordinary users. Also, conceptual changes can make articulated concepts more articulated. For example, an articulated concept of gene five decades ago is not identical to what current geneticists perceive as precisely referring to one object.

articulated understanding of the term ‘gene’.[1] This stipulation is a sign that the interlocutors’ respective uses of ‘gene’ are connected to a more specific lineage of the lexical tradition associated with the discipline of molecular genetics. Then, in order to identify which way of individuating segments of DNA counts as a ‘gene’ in this context, we need to know the crucial aspects of the representational tradition associated with the conversation. Each sub-tradition within molecular genetics will involve a large number of specific epistemic and empirical commitments, only some of which will be crucial to fixing the reference. Experts in each scientific research program implicitly distinguish among the conditions that play and do not play a role in reference fixing. Here, I rely on Waters’ (2014) highly articulated account of molecular genes with variables for crucial reference-fixing factors.

According to Waters, molecular geneticists’ understanding of the word ‘gene’, considered independently of particular research contexts, is not specific enough (or sufficiently ‘articulated’, in my terminology) to pick out a determinate referent; however, filling in some variables helps to narrow down the domain of possible referents to a unique set of objects. Waters’ (2014) schematic account of gene is as follows:

> G2: ‘a gene *g* for linear sequence *l* in product *p* synthesized in cellular context *c* is a potentially replicating nucleotide sequence, *n*, usually contained in DNA that determines the linear sequence *l* in product *p* at some stage of DNA expression’[2].

Let us elaborate on this account in an example. A shared set of genes between humans and mice, called ‘PAX6’, is responsible for the development of eyes and sensory organs. PAX6 is itself part of a large family of genes called ‘PAX’ (paired box genes). In addition to different positions (loci) on the chromosomes in different species, this gene has a slightly different structure. For example, the mouse PAX6 has three confirmed *promoters*, while the human version has four *promoters*. Narrowing down our focus to one promoter (say, promoter alpha) in one species (say, humankind), there are 16 *exons* constituting any RNA transcript. While a combination of some of these exons is responsible for producing protein isoform A (NP_001121084), another combination in the same range produces protein isoform B (NP_001245391), and so on. While each of these sub-kinds in different production circumstances could be called ‘gene’, the general way of understanding ‘gene’ in molecular genetics does not suffice to specify which exact sequence is referred to when someone says ‘gene’.

A verdict can only be justified by considering the relation between two experts having a conversation in the relevant field of genetics: e.g., is one talking about the others’ experimental manipulations? Are they comparing human and mice DNA? Are they focusing

[1] They might be aware of their differences as well. For example, geneticist A may know that her conversational partner B does not agree with her latest scientific article about the properties of a specific segment of DNA.
[2] Waters (2014, p. 125).

on isoform A or B? and so forth.[1] Answering these questions is crucial to individuating which sequences of DNA constitute genes in this conversational context. In addition, external facts about the physical systems in question will also be relevant to determining the precise reference of their use of the term. Therefore, in order to fill in the variables in Waters' (2014) analysis to determine the articulated concept expressed by 'gene' here,[2] semantic interpretation should take into account a nuanced understanding of the conversational context.

4.7. Relational Account Is Not Simply Another Descriptivism

One might wonder why a relational account is not merely a version of a descriptivist theory of reference determination. In fact, Brigandt (2006b) warned of this temptation:

> It is surely possible to use a very broad notion of 'description', including any type of implicit knowledge connected to a concept and its use. On such an account, the features to which I appeal are in fact reference-determining 'descriptions'. (p. 256)

In the case of a relational account of reference, what shall prevent us from construing G2 as just a set of reference-fixing descriptions shared by all molecular geneticists, which they rely on to identify the reference of their term in a given context? And if this is right, what role does a relational approach to concepts play in my account? In what follows, I will clarify my relational account and distinguish it from a descriptivist one.

G1 was intended to analyse the ICG, shared by competent geneticists, which helps to narrow down the domain of possible referents for their use of 'gene'. However, as Waters (2014) notes, G1 is too vague to single out any determinate reference for the term. I suggested that Waters' second analysis, G2, provides a blueprint for identifying the *articulated* concepts expressed by molecular geneticists' use of 'gene' in specific contexts of use. However, we should not think of G2 as a simple reference-fixing definition that provides a recipe for identifying the reference of 'gene' in *any* context of use.

Two important characteristics distinguish my proposed relational account from a descriptivist one. First, the variables in G2 cannot be automatically filled by facts about an individual speaker's physical or social environment. Said differently, the facts about the potential referential candidates (sequences of DNA) and a comprehensive understanding about the genetic theories do not suffice to fulfil G2. Rather, G2 (or any such account) holds that an interpretation of 'gene' in a conversational context depends on speakers' intentions to coordinate with a specific representational sub-tradition and on relations between that sub-

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[1] This does not mean that users are explicitly aware of these questions; rather, it is possible that their practice provides evidence for theorists to assign a relevant reference to their usage.

[2] This schematic account is corrigible in the light of further inquiries. For example, it is possible to propose another more complex account where the number of these variables is larger or smaller. Admittedly, we do not have access to a fixed (correct) definition of the reference, so we can meet the corrigibility constraint outlined in Section 2.

tradition and specific features of the environment. The parameters mentioned in G2—Including the linear sequence (l), genetic product (p), cellular context (c) and nucleotide sequence (n)—are the crucial variables that theorists (or scientists) need to identify as relevant to the particular sub-tradition that each coordinating intention is ultimately connected to. Therefore, G2 does not provide an automatic recipe for identifying the reference in any expert conversation. Instead, it is a rough guide for semantic theorists that need to be filled in when articulating a semantic interpretation.

Second, individual scientists do not need to be reflectively aware of the interpretive constraints specified by G2. Rather, expert scientists tend to be sensitive to these factors in identifying the precise reference of 'gene'—or at least they should be. In addition, I have already argued that some ordinary users (e.g., speaker P) can refer to specific sequences of DNA, by being in an appropriate relation with the experts (e.g., speaker R). Thus, G2 is not an *a priori* analysis of the molecular concept of gene, but a rough rule of thumb about empirical factors in the representational tradition relevant to semantic interpretation in a context. As long as scientists in their research tradition can coordinate and construct theories that better approximate the truth, individual scientists do not need any recipe for identifying the reference of their terms. Therefore, while G2 is something that semantic theorists and philosophers of science have a strong interest in, it does not need to be grasped by all competent users.[1]

While G1 functions as the ICG among competent geneticists to narrow down the domain of referents, G2 is a template, or rule of thumb, that theorists can use to articulate the concepts expressed by 'gene' in different contexts. Two important characteristics of G2 distinguish a relational account from a descriptivist one. First, G2 is not a descriptive definition that can be automatically filled in with contextual information that can be identified independently of the linguistic practices of the community of users. Instead, it is a rough guideline for semantic theorists indicating which empirical facts about the specific representational tradition linked to a given utterance of 'gene' in molecular genetics are relevant to determine the correct semantic interpretation. Second, individual scientists do not need to be reflectively aware of these interpretive constraints. Rather, scientists tend to, or should be sensitive to these factors in singling out the precise reference of 'gene'. Therefore, G2 is not an *a priori* analysis of 'the molecular concept of gene', but a rough rule of thumb about empirical factors in the representational tradition that are relevant to semantic interpretation in a context.

One may insist that this relational account will ultimately return to a kind of descriptivism. This objection acknowledges that in concrete empirical situations the various parameters will need to be nailed down in order to determine what can be called a 'gene' in different production circumstances. But when it comes to establishing the initial reference of a term we need to consider the descriptive reference determining conditions that are at least

[1] I am not arguing that Waters (2014) intended a relational account with his introduction of G2; rather, I used his formula of G2 to satisfy the constraint I put on an account of conceptual change.

implicitly grasped by the scientists.[1] It is undeniable that a relational account may require some descriptive elements to fix reference, but what distinguishes a relational account from a descriptivist one is that descriptions are not enough to determine the precise referent of a term token. We already know that there is not enough in the head of individual speakers to fix any determinate reference. We need to appeal to an account of reference that links non-expert speakers to the understanding and use of other members of the *relevant* community. A relational account can successfully determine the reference of an *articulated* concept by connecting a particular term use to the relevant representational tradition and hence by deciding which causal/descriptive elements should be considered. While a relational account cannot avoid causal/descriptive elements in reference fixing, it claims that the current theories of reference are not adequate in doing the task.

4.8. Reference Fixing for Ordinary Users

Having explained the central points of a relational account of reference, it is now easy to explain how reference of 'gene' is fixed in the case of ordinary non-scientific speakers. An ordinary speaker, like patient P, will be entirely ignorant of the technical concepts involved in analyses like G1 and G2. However, her use of the term 'gene' may be hooked up, via her coordinating intentions to the representational traditions of experts like her doctor D and researcher R. Therefore, P's utterance of 'gene' can be linked to the same representational tradition intended by D and R. Accordingly, the mechanisms involved in the relational approach to concepts can help secure the intuitive verdict that speakers P, D and R can all co-refer in their conversation about the genes that make breast cancer more likely. Accordingly, P's naïve use of 'gene' can find its reference by G1 or G2, since she has a secure way of connection to a specific sub-tradition.

Next, in the case of mother (M) and uncle (U) who talked about M's son's blue eyes, it is unlikely that their use is connected to any specific research tradition in molecular genetics. However, as long as they are using the term in a consistent way to refer to some kind of biological reproduction unit responsible for patterns of inheritance, we can claim that their use is connected to a broad Mendelian conception of gene. This specific use of 'gene' would not pick out a very precise segment of DNA with a specific protein production, but it is sufficiently precise to pick out a range of DNA segments responsible for the inheritance of eye colour across human reproduction. The link that secures this broad reference and assures us that M is successfully referring to something in the external world is her intention to coordinate with all general knowledge she has acquired through TV, schools, social interactions, and so forth on this subject matter[2]. These causal-historical relations might

[1] Thanks to the second examiner of the thesis for raising this objection.

[2] A relational account uses one's intention to coordinate with others as an external factor in the context of use. But one may argue that M and U could be confused about genes: maybe their term refers to nothing in the world (such as my example in the next paragraph), or it's only partially referring to an object. I will not discuss these options because I think it is enough for a relational account to show that what's important in reference determination (or concept individuation) is related to external factors in the context of use. I acknowledge that

ultimately reach to a scientific knowledge about genes, but it is not specific enough to individuate an articulated concept. Although M's use of the term is not connected to a specific sub-tradition and so referentially underdetermined, it is sufficient for M and U to successfully communicate about the same broad aspect of the causal system. In this case, we can say that M and U *partially refer* to the reference of each viable articulated concept (cf. Field 1974).

Now, let us suppose another case with a term user fully disconnected with the scientific traditions or someone who deceitfully tries to use the term in a misleading way. In this case, someone like Donald Trump would say: "My family's success is due to our good genes". In this statement, the use of term 'gene' is broadly connected to the most general understanding of the ICG associated with the term; yet, since there is no secure way to connect this use to a scientific tradition, theorists should arguably assign no reference to this particular use of 'genes'. While the speaker's statement has meaning and everybody can understand it (or may be offended by it), there is no causal-historical relation that connects his coordinating intention to a specific representational tradition that is sufficiently articulated to secure a determinate reference. This makes it plausible to conclude that, in this case, the reference of the term 'gene' is empty.

5- Conclusion

In this chapter, based on the considerations raised and discussed in previous chapters of this thesis, I formulated the constraints on an adequate account of conceptual change. My aim was to make this account consistent with a full-blooded scientific realism and also reflective of the issues raised by a non-representational approach to conceptual change. Having proposed the constraints, I then outlined the relational approach to individuating the units of semantic interpretation. I then argued that this relational approach has the potential to vindicate the full-blooded realists' commitments about reference, while fitting with the constraints on concept individuation needed to explain successful communication. While not proposing a full theory of reference determination or a theory of concepts, as these tasks are beyond the scope of the present thesis, I suggested that, in terms of vindicating full-blooded realist commitments about reference, the proposed relational approach to concepts is no worse off than an individualistic approach. Moreover, I argued that this relational approach to reference determination and concept individuation allows for a significant improvement over classical accounts of reference determination vis-à-vis explaining the epistemic role of concepts. A thorough explanation of the epistemic roles played by concepts and a detailed development of a relational model are tasks for further research. My primary aim in this thesis has been to demonstrate what a scientific realist account of conceptual change should seek to explain. In this final chapter of the thesis, I have sought to show that a full-blooded scientific realist can potentially account for the epistemic and communicative roles that concepts seem to play in scientific inquiry. In sum, I hope to have shown that, even for those theorists who

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there are costs associated with the various ways of proceeding, but this is the cost that theorists should pay for their theories of concepts or reference.

are not attracted to scientific realism, there are important common-sense constraints on a theory of conceptual change that need to be explained, or explained away, by any adequate theory of conceptual change.

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