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The complementary impacts of nurse home visiting and quality childcare for children experiencing adversity

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Abstract

Australian Governments are increasingly understanding the impacts of early adversity, evidenced by ongoing policy and investment in two of the most widely implemented early interventions: nurse home visiting (NHV) and early childhood education and care (ECEC). Neither intervention fully redresses the developmental inequities engendered by early adversity, yet their synergistic impacts ("dynamic complementarity") are unknown. In this research, we aimed to (1) inform evaluation of policy implementation by (2) experimentally testing the dynamic complementarity of NHV and ECEC. We capitalised on an opportunity afforded by the Australian "right@home" randomised trial, which involved 722 pregnant women experiencing adversity, randomised to receive NHV or usual care to child age 2 years. Detailed data describing family-accessed ECEC were collected from parents at 3–4 years, and "quality ECEC" was categorised according to meeting quality recommendations defined by Australian policy and provision. Children's developmental outcomes (language, executive functioning, behaviour and well-being) were parent-reported or assessed directly at 4 years. At 4 years, 33 per cent of families had received neither intervention; 40 per cent NHV only; 14 per cent quality ECEC only; and 13 per cent had received both. We used linear regression to estimate differences in mean outcomes between exposure groups, including interaction between NHV and

ECEC. Unadjusted analyses indicated modest effects of the combination of NHV and ECEC, which attenuated after adjustment for socioeconomic confounders. We present the design and preliminary findings as an approach that could be used to evaluate equitable implementation at scale and enable policymakers to determine the most effective evidence-based policy.

KEYWORDS
child development, disadvantaged groups, early childhood education and care, nurse home visiting, randomised controlled trial

1 | INTRODUCTION

Childhood inequities are unfair and preventable differences in health and development that are predominantly due to social determinants such as poverty, low parental education and racism (Moore et al., 2017; World Health Organisation, 2011). The negative impacts of these determinants (termed adversity throughout) on child development begin from pre-conception (Moore et al., 2017) and increase the risk of poor health as well as reduced education, income and societal participation across the lifespan (Duncan & Magnuson, 2013; Pavalko & Caputo, 2013). At a societal level, entrenched disparities increase health and social care costs and reduce economic productivity (Kershaw et al., 2010; The Lancet, 2016). Investment in the early years offers the best economic and social return, with gains greatest for families experiencing adversity (Heckman & Mosso, 2014). However, no single intervention can redress inequities. Instead, the synergistic effects of multiple, complementary interventions (referred to as "dynamic complementarity") are likely to be necessary (Heckman & Mosso, 2014).

Despite being a high-income country, Australia's equity gap is substantial (AEDC, 2021). By the time children start school, those living in the poorest Australian suburbs are twice as likely to be vulnerable on at least one of five developmental domains than those in the wealthiest suburbs (33.2 per cent vs. 14.9 per cent) and three times more likely to be vulnerable on two or more domains (19.1 per cent vs. 6.7 per cent) (AEDC, 2021). Australian Governments are increasingly understanding the problems of early adversity and inequitable policy (e.g., Department of Social Services The First 1000 Days, National Action Preventative Health Statement), and the importance of early childhood is gaining prominence through the National Action Plan for the Health of Children and Young People (DoHA, n.d) and proposed Early Years Strategy (Australian Government, n.d). There is ongoing state and federal investment in the two of the most widely implemented early programs that offer potential for equity gains, nurse home visiting (NHV) and early childhood education and care (ECEC). However, their implementation is rarely experimentally evaluated, and there is limited evidence regarding their effectiveness at scale (Castillo & Petrie, 2022; Paulsell, 2022). Without evaluation, we miss opportunities to understand whether the benefits seen in research trials are delivered at scale, to test the synergistic impacts of interventions (dynamic complementarity) and to iteratively improve service impacts.

Although the theory of dynamic complementarity is appealing, evaluation is challenging. An ideal factorial design incorporating more than one early intervention and outcome measurement within the same cohort has been described by economists as "asking for lightning to strike twice" (Almond & Mazumder, 2013). When considering NHV and ECEC, the well-established evidence bases mean there is a lack of equipoise to justify establishing a

multi-intervention trial for the purpose of testing dynamic complementarity. Thus, intervention research is limited, and the evidence base mostly comprises economic modelling of historical longitudinal datasets to delineate the ecological effects of policies such as cash transfer schemes or education funding reforms (Attanasio et al., 2015, 2017; Johnson & Jackson, 2017; Lubotsky & Kaestner, 2016; Malamud et al., 2016; Rossin-Slater & Wüst, 2016).

Data from the Longitudinal Study of Australian Children support the potential cumulative benefit of five sequential early services that include NHV and ECEC (with antenatal care, parenting programs and the early years of school) on children's reading ability by middle childhood (Molloy et al., 2019). However, the service interventions were measured as indicators (yes/no to exposure) and the authors did not examine synergistic benefits. To date, the only intervention-level research to consider the complementary effects of NHV and ECEC on child outcomes before school-entry is the 1985 American Infant Health and Development Program (Garcıa & Gallegos, 2017). In this multisite randomised controlled trial (RCT), families with low-birthweight infants were allocated a multi-level intervention comprising NHV (birth to child age 3 years) and quality centre-based ECEC (child age 1 to 3 years), versus a control group who could access usual ECEC. Secondary data analysis demonstrated complementarity between increasing quality ECEC (the intervention/program) and improved language outcomes at 3 years; however, the design could not identify the contributions of the program's two components.

The overarching goals of this paper were to (1) inform evaluation of policy implementation by (2) experimentally testing the dynamic complementarity between NHV and quality ECEC in the Australian policy context. Drawing data from an existing Australian RCT of NHV ("right@home") (Goldfeld et al., 2017; Kemp et al., 2019), we investigated the complementary impact of NHV from pregnancy to child age 2 years and quality early childhood education and care from 3 to 4 years on children's developmental outcomes at 4 years (language, executive functioning, behaviour and wellbeing). Evaluation was based on family-accessed ECEC, defined as "quality" according to whether ECEC met quality recommendations within Australian policy and provision. The research design and findings are presented as an exemplar that could support governments to evaluate early childhood service delivery and enable evidence-based policy.

2 | METHODS

2.1 | Design

Post hoc/secondary analysis of data from right@home, a prospective RCT of NHV (ISRCTN89962120) (Goldfeld et al., 2017; Kemp et al., 2019). The right@home trial evaluated a NHV intervention that was custom-built for Australian families experiencing adversity, and embedded into the universal, child and family health (CFH) nursing service. It was impractical and unethical to re-randomise families to receive ECEC meeting quality recommendations versus poorer quality or insufficient exposure. Instead, families reported their ECEC access in annual, home-based interviews. Only post-NHV intervention ECEC data were considered for a range of reasons including:

(i) the focus of this paper is on evaluating the policy provision and, in Australia, the policy and funding focus for ECEC at the time of this study was on the preschool years (ages 3–5) and specifically on the benefits of implementing 3-year-old ECEC, with roll-out commenced or planned in the states of Victoria, New South Wales and South Australia;
(ii) reasons for using ECEC from 0 to 2 years are therefore more selective and diverse, for example, to enable employment of parents or carers, which is a marker of socioeconomic

advantage, or due to family vulnerability such as child protection interactions, which is a marker of socioeconomic disadvantage;

(iii) the ECEC data collected during the NHV program (pregnancy to 2 years) lacked the details necessary to map families' longitudinal use; and

(iv) our previous analysis showed little difference between group arms on ECEC use at 2 or 3 years (Price et al., 2022).

Data were analysed as a non-randomised factorial design comprising a randomised intervention (NHV program, pregnancy to 2 years) and families' naturalistic (self-accessed) use of quality ECEC (3 to 4 years). Figure 1 shows the analytic design.

2.2 | Setting and participants

Researchers recruited pregnant women attending antenatal clinics of 10 public maternity hospitals across the states of Victoria and Tasmania from 30 April 2013 to 29 August 2014 (details previously published (Goldfeld et al., 2017, 2018; Kemp et al., 2019; Price et al., 2017, 2018)). Eligible women: (i) had due dates before 1 October 2014; (ii) were less than 37 weeks gestation at the time of recruitment; (iii) had sufficient English to complete face-to-face interviews; (iv) lived within travel boundaries specified by participating areas; and (v) experienced adversity as defined by 2 or more of 10 risk factors at screening (young woman during pregnancy; not living with another adult; no support in pregnancy; poor health; a long-term illness, health problem or disability that limits daily activities; currently smokes; stress, anxiety or difficulty coping; low education; no person in the household currently earning an income; and never having had a job before) (Goldfeld et al., 2017, 2018; Kemp et al., 2019; Price et al., 2017, 2018).

Women were excluded from the right@home RCT if they: (i) were enrolled in an existing Tasmanian NHV program for 15-19-year-olds, (ii) did not comprehend the recruitment invitation (e.g., intellectual disability, or insufficient English), (iii) had no mechanism for contact or (iv) experienced a critical event (e.g., termination of pregnancy, stillbirth or child death). Figure S1 summarises the flow of participants through the study. Of 5586 women screened, 1427 (25.5 per cent) were eligible, and 722 of 1427 (50.6 per cent) enrolled in the right@home RCT by providing written informed consent at a home-based enrolment interview. At the trial's original endpoint of 2 years (when the NHV intervention ended), 558 of 722 (77.3 per

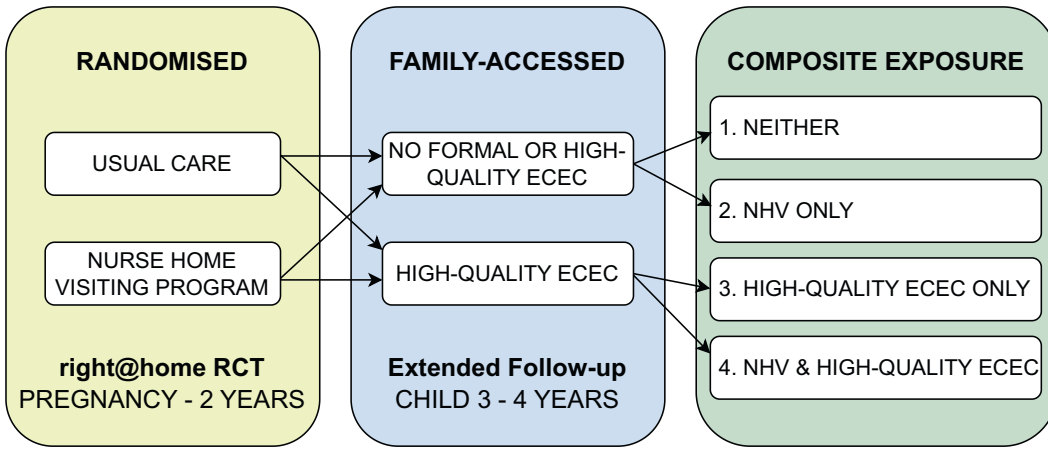

FIGURE 1 Analytic diagram. ECEC, early childhood education and care; NHV, nurse home visiting.

cent) re-enrolled by providing written consent for extended follow-up from child age 2 years to school entry.

2.3 | Nurse home visiting (NHV) intervention

After the home-based enrolment interview, participants were randomised to control ($n = 359$) or NHV intervention ($n = 363$) arms following a computer-generated schedule stratified by site and parity (first-time parent vs. other). Table S1 describes the NHV intervention and usual care in detail. In brief, intervention group participants were offered 25 nurse home visits (mean 23.2 home visits received), of 60–90 min, commencing antenatally and delivered mostly by the same nurse trained in the right@home NHV model of care (Goldfeld et al., 2017; Kemp et al., 2019). Visits focussed on promoting parent care of the child, responsive parenting and the home learning environment. The comparator was the usual child and family health service, which included six (Tasmania) or nine (Victoria) consultations (20–40 min each), mainly delivered in community clinics, with some limited program flexibility depending on family need. Annual, home-based, follow-up interviews were conducted by researchers blinded to allocation, and families were requested not to disclose their randomisation status. Researchers reported any breach (unblinding) to the research manager; only four and three instances were reported at the 3- and 4-year follow-ups, respectively. The participants, research managers and nurse teams delivering the intervention were aware of allocation. The nurses providing usual care were unaware that their clients were participating in the trial unless clients informed them (not measured). Table S1 also provides a brief summary of the NHV impacts and supporting publications.

2.4 | Quality early childhood education and care (ECEC)

The National Quality Standard (NQS) is Australia's ECEC framework that benchmarks the quality of formal (government approved and subsidised) ECEC at the centre level. The decisions underpinning our definitions of formal ECEC, quality, dose, and duration, were based on Australia's policy and provision of ECEC at the time of data collection. Table S1 describes the ECEC measures in detail. Briefly, exposure to quality ECEC from 3 to 4 years was defined a priori as a child attending a formal ECEC centre: (i) with a NQS rating of at least "meeting" Australia's ECEC quality standards, (ii) for at least 15 hours per week and (iii) for a duration of at least 12 months (with this duration completed at least one month before the 4-year home-based interview). Families did not meet the "quality ECEC" definition if children were not attending formal ECEC; attending formal ECEC that did not meet the NQS benchmark; or attending formal ECEC for less than 15 hours per week or less than 12 months in duration. As described in the Results section, of 351 families who accessed formal care, only 118 (34 per cent) were classified as quality according to this definition.

As Table S1 describes, Australia's ECEC policy was underpinned by evidence for the minimum required dose for effectiveness and elements required for quality; however, the evidence for the developmental benefit to Australian families—especially those experiencing adversity—was limited. A review of the NQS domains by the ReStacking The Odds project demonstrated that high-quality ECEC should be defined as care that "exceeds" the NQS on three of the benchmark's seven domains (educational program and practice, staffing arrangements and relationships with children, see Table S1) (Molloy et al., 2021). At the time of the study, less than a quarter of Australian services delivered this level of quality and, although high-quality care is disproportionately beneficial for children experiencing adversity, high-quality services were more often available in areas of greater socio-economic advantage (Molloy et al., 2021).

In this study, of the 118 children who received quality ECEC according to the policy-aligned definition in Table S1, $n = 53$ (45 per cent) were accessing care that exceeded the NQS, and this sample size was small to meaningfully analyse.

2.5 | Developmental outcomes

Table 1 describes the child outcome measures collected at 4 years. We included all of the child developmental outcomes collected by the original RCT that could be considered for interpreting complementarity (language, executive functioning, behaviour and well-being, all assessed with validated measures). To support families with lower literacy and aid retention, data were collected via home-based, parent-reported interviews, which also comprised parent-reported and direct assessments of children.

2.6 | Covariates

Covariates were selected a priori and detailed in Table S1; all were measured at the home-based enrolment interview in pregnancy except where specified. Maternal measures included age at child's birth, education status, employment, antenatal risk, self-efficacy and mental health. Child measures included parity, gender, age at assessment and enrolment site. Family measures included Socio-Economic Indexes for Areas (SEIFA) postcode-level Index of Relative Socio-economic Disadvantage (IRSD), main language spoken at home and availability of quality ECEC in postcode.

TABLE 1 Child outcome measures at 4 years.

Receptive and expressive language	Clinical evaluation of language fundamentals (CELF) preschool second edition Australian standardised edition (Wiig et al., 2006). Direct assessment of child language skills across three subscales: Sentence structure, word structure and expressive vocabulary, and a combined core language score. Subtest scores reported as age-specific normative scaled scores ($m = 10$, $SD = 3$) and core language score reported as standard score ($m = 100$, $SD = 15$); higher scores indicate greater proficiency
Mental health and behaviour	25-item Strengths and Difficulties Questionnaire (SDQ, 4- to 10-year-old version) (Goodman, 1997; Goodman & Scott, 1999) assessing total difficulties and two domain scores: internalising difficulties (combined score of emotional and peer problems) and externalising difficulties (combined score of behaviour and attention/hyperactivity). Parent-reported; higher scores indicate more difficulties
Quality of life	21-item Pediatric Quality of Life Inventory (PedsQL) (Varni et al., 2007) assessing general well-being using two subscales: Physical functioning socio-emotional functioning. Parent-reported; higher scores indicate better functioning
Attention and executive function	Two subtests of the National Institutes of Health (NIH) Toolbox Early Childhood Cognition Battery (Ages 3–6 years), (Slotkin et al., 2012) assessing attention and executive function: Flanker Inhibitory Control & Attention Test and Dimensional Change Card Sort. Both administered to the child as direct assessments on the NIH Toolbox iPad application. Subtest scores examined as standardised age-corrected scores ($m = 100$, $SD = 15$); higher scores indicate greater proficiency

2.7 | Statistical analysis

Participant characteristics were described using mean and standard deviation (SD) (continuous variables) or number and percentage (categorical variables), overall and by each of the four exposure groups: receiving neither NHV nor quality ECEC, NHV only, quality ECEC only and both NHV and ECEC. Differences in mean outcomes between participants receiving neither NHV nor ECEC (reference group), NHV only, quality ECEC only or both NHV and ECEC were estimated using linear regression models that included an interaction between NHV and ECEC. As per the statistical analysis plan for the original RCT (Goldfeld et al., 2019), we fitted a "partially adjusted" model (Model 1) that was adjusted for the two stratification factors used during randomisation (parity and study site). Because of the non-randomised factorial exposure, and in recognition of the complex interrelated factors contributing to adversity (Harrison et al., 2010), we fitted two additional adjusted models. In the second model ("Fully adjusted" Model 2), we adjusted for parity and study site as well as baseline covariates known to confound the association between service participation and child development (maternal age at child's birth, education, antenatal risk; self-efficacy; mental health; Socio-Economic Indexes for Areas; and main language spoken at home; and child gender, age at assessment; and probability of quality ECEC providers in postcode, see Table S1).

As the Australian federal government subsidises costs of formal ECEC according to income and hours of work, these characteristics may confound any relationship between ECEC access and child outcomes. Thus, the third model ("Fully Adjusted" Model 3) included all variables in Model 2 and additionally accounted for maternal employment at 2 and 4 years (Table 1). Postnatal measures of employment were included because they would be more indicative than baseline employment. All models were fitted using cluster-robust standard errors to account for clustering by the nurse providing NHV or site of usual care provision. Estimates are reported with 95% confidence intervals. Data were analysed using Stata/IC v16.1 (Stata, College Station, TX, USA).

2.8 | Sample size

The original sample size target of 714 used for the right@home RCT was calculated to have 80 per cent power to detect a minimum effect size of 0.3 SD difference between NHV and usual care for a continuous primary outcome (parent responsivity as measured with the Home Observation of the Environment Inventory) at the .05 significance level; average intra-class correlation .02 within 18 nurse clusters; and 40 per cent attrition by 2 years (details previously published) (Goldfeld et al., 2017; Kemp et al., 2019). Post hoc power calculations are not recommended and were not performed (Zhang et al., 2019). As the analyses reported in this study were considered exploratory, we interpret results with caution, focussing on overall patterns while considering uncertainty in estimation.

2.9 | Missing data

Following best practice, a combination of multiple imputation (MI) and inverse probability weighting (IPW) was used to account for missing data and differential attrition arising from loss to follow-up (Seaman et al., 2012). Multivariate normal imputation was used to address missing data within the participating sample ($N = 465$ participants who provided any 4-year data) using 50 imputations, initial burn-in of 100 iterations and a further 100 iterations between imputations (Schafer, 1997; Von Hippel, 2009). The imputation model included the ECEC and outcome variables, covariates (Table S1) and auxiliary variables (from the same and earlier

time points, Table S2) that were highly correlated with incomplete variables. Imputation was performed separately by NHV allocation status (Seaman et al., 2012). Following MI, adaptive rounding was applied to binary analysis variables. Inverse probability weights were then applied to account for families who provided no 4-year data, to represent the cohort originally randomised ($N = 722$) and account for potential bias resulting from differential attrition.

2.10 | Ethics approval

The right@home trial and secondary data analysis were approved by these Australian Ethics Committees: Royal Children's Hospital (HREC 32296); Peninsula Health (HREC/13/PH/14); Ballarat Health Services (HREC/13/BHSSJOG/9); Southern Health (HREC 13084X); and Northern Health (HREC P03/13) in Victoria, and University of Tasmania (HREC H0013113).

3 | RESULTS

3.1 | Participant characteristics

At 4 years, 465 families provided data (Figure S1). This represents 64 per cent of the $N = 722$ originally randomised in the right@home trial. Baseline characteristics were similar between those who provided data at 4 years and the cohort originally randomised (Table 2). The main reason for missing data was loss to follow-up over time (Figure S1). At 4 years, 351 of 465 (75 per cent) of children were enrolled in formal ECEC with a median dose of 20 hours, range 0–50 (noting that $n = 6$ used less than 2.5 hours per week including $n = 2$ who recorded zero hours). Of the 351 enrolled in formal ECEC, 256 (73 per cent) attended care that met or exceeded the national quality benchmark and 271/351 (77 per cent) attended a centre for 15 hours or more per week. As shown in Table 2, 33 per cent, 40 per cent, 14 per cent and 13 per cent of children were categorised as receiving neither exposure (reference group), NHV only, quality ECEC only or both, respectively. Due to the nature of defining quality ECEC using a combination of three variables, there was overlap in distributions of dose (hours used) between the groups. For the $n = 118$ categorised as receiving quality ECEC according to this study's policy definition, the median dose of formal ECEC was 30 h, range 15–50 h. Of the $n = 319$ children (with available data) who did not meet the quality ECEC definition, the median dose was 10 h, range 0–50 h; this included $n = 220/319$ who used formal ECEC (mean 20 h, range 0–50).

Table 2 shows substantive differences in demographic characteristics between exposure groups. Compared with women who received both NHV and ECEC, women who received neither (reference group) experienced higher levels of adversity during pregnancy in terms of lower education status; more unemployment and income from social welfare; higher antenatal risk; poorer mental health; higher neighbourhood disadvantage; and were more likely to speak a language other than English. Women who used quality ECEC, compared with those who did not, had higher levels of education, more employment, were less likely to receive a benefit/pension as their main source of income, and were also more likely to be participating with a first-born child. Women who did and did not receive NHV had similar characteristics, consistent with the original randomisation.

3.2 | Exposure to NHV and ECEC and child outcomes

To aid comparison, Figure 2 presents the estimated mean differences in outcomes from the reference group (those who received neither NHV nor ECEC) obtained using each of the three

TABLE 2 Baseline characteristics of trial cohort and participants retained at 4 years by exposure group.

Characteristic	RCT cohort ($n=722$)	4-year data ($n=465$)	1. Neither/Reference ($n=146$)	2. NHV[a] only ($n=173$)	3. ECEC[b] only ($n=60$)	4. NHV & ECEC ($n=58$)
Maternal						
Age (years), mean (SD)	27.6 (6.2)	28.1 (6.1)	28.5 (6.3)	27.7 (5.9)	29.3 (6.5)	28.0 (5.6)
Education status, *n* (%)						
Did not complete high school	162 (25.0)	101 (23.7)	37 (27.2)	36 (23.1)	13 (22.8)	8 (14.8)
Completed high school	51 (7.9)	35 (8.2)	7 (5.1)	20 (12.8)	2 (3.5)	5 (9.3)
Completed vocational training	366 (56.5)	242 (56.8)	81 (59.6)	81 (51.9)	33 (57.9)	34 (63.0)
Completed a university degree	69 (10.6)	48 (11.3)	11 (8.1)	19 (12.2)	9 (15.8)	7 (13.0)
Marital status, *n* (%)						
Single/not living with partner	195 (27.0)	119 (25.6)	36 (24.7)	51 (29.5)	10 (16.7)	13 (22.4)
Married	218 (30.2)	152 (32.7)	48 (32.9)	57 (32.9)	23 (38.3)	21 (36.2)
Living with partner, not married	295 (40.9)	186 (40.0)	60 (41.1)	63 (36.4)	26 (43.3)	22 (37.9)
Separated but not divorced	9 (1.2)	5 (1.1)	1 (0.7)	1 (0.6)	1 (1.7)	1 (1.7)
Divorced	5 (0.7)	3 (0.6)	1 (0.7)	1 (0.6)	0 (0.0)	1 (1.7)
Unemployed, *n* (%)	478 (66.2)	289 (62.2)	100 (68.5)	118 (68.2)	25 (41.7)	24 (41.4)
Family income from benefit/ pension, *n* (%)	309 (42.8)	187 (40.2)	65 (44.5)	82 (47.4)	13 (21.7)	11 (19.0)
High antenatal risk[c], *n* (%)	447 (61.9)	270 (58.1)	92 (63.0)	95 (54.9)	38 (63.3)	23 (39.7)
Poor self-efficacy[d], *n* (%)	491 (68.2)	321 (69.2)	105 (71.9)	120 (69.8)	40 (66.7)	39 (67.2)
Poor mental health[e], *n* (%)	337 (46.7)	206 (44.3)	64 (43.8)	75 (43.4)	23 (38.3)	30 (51.7)
Child						
First born, *n* (%)	267 (37.0)	172 (37.0)	44 (30.1)	61 (35.3)	27 (45.0)	30 (51.7)
Female, *n* (%)	345 (49.6)	244 (52.5)	69 (47.3)	98 (56.6)	29 (48.3)	37 (63.8)
Family						
SEIFA Index of Social Disadvantage Quintile, *n* (%)						

(Continues)

TABLE 2 (Continued)

Characteristic	RCT cohort ($n = 722$)	4-year data ($n = 465$)	1. Neither/Reference ($n = 146$)	2. NHV[a] only ($n = 173$)	3. ECEC[b] only ($n = 60$)	4. NHV & ECEC ($n = 58$)
1 (most disadvantaged)	296 (41.0)	191 (41.1)	62 (42.5)	82 (47.4)	17 (28.3)	16 (27.6)
2	57 (7.9)	35 (7.5)	12 (8.2)	12 (6.9)	7 (11.7)	2 (3.4)
3	264 (36.6)	177 (38.1)	52 (35.6)	60 (34.7)	25 (41.7)	30 (51.7)
4	60 (8.3)	36 (7.7)	10 (6.8)	11 (6.4)	8 (13.3)	5 (8.6)
5 (least disadvantaged)	22 (3.0)	14 (3.0)	4 (2.7)	5 (2.9)	2 (3.3)	3 (5.2)
Language other than English, *n* (%)	61 (8.6)	34 (7.4)	10 (7.0)	16 (9.4)	6 (10.0)	1 (1.8)

Note: Of the 465 families who provided 4-year data, 28 were missing ECEC data, 39 were missing data on maternal education status, 1 was missing data on self-efficacy, 12 were missing data for Socio-Economic Indexes for Areas (SEIFA) and 6 were missing data on main language.

[a]Nurse home visiting defined as allocation to the right@home intervention group.

[b]Quality early childhood education and care (ECEC) defined as attending formal ECEC of at least "meeting" the national quality standard, for at least 15h per week, and attending at least 12months of any formal ECEC by the 4-year interview.

[c]High antenatal risk is 3 or more risk factors on the brief risk factor screening survey (vs. 2 risk factors, a right@home RCT eligibility criterion).

[d]Poor self-efficacy is any lack of self-efficacy versus no lack of self-efficacy on 3 items drawn from the UK Millennium Cohort.

[e]Poor maternal mental health is scoring in top 15 per cent (according to UK normative data) on any subscale of the Depression, Anxiety, Stress Scale short form versus not (<85th percentile).

models (Tables S3–S5). In the partially adjusted model (Figure 2, Table S3), the sample estimates reflected a general pattern of higher performance by children who received both NHV and quality ECEC versus neither (i.e., Group 4 vs. the reference, Group 1), as indicated by positive mean differences (MDs) on CELF core language (MD = 6.01, 95% CI (0.1, 12.0), $p = .048$) and word structure (MD = 1.35, 95% CI (0.3, 2.5), $p = .02$), and NIH dimensional change card sort (MD = 6.45, 95% CI (1.5, 11.4), $p = .01$). Children receiving NHV only (Group 2) or ECEC only (Group 3) had lower SDQ internalising behaviour scores (MD = −0.55, 95% CI (−1.0, −0.1), $p = .01$) and better PedsQL socio-emotional scores (MD = 2.59, 95% CI (−0.1, 5.3), $p = .06$) than those receiving neither (reference, Group 1). Overall, confidence intervals for MDs were wide and included zero as well as a range of clinically important values for most outcomes.

The estimated mean differences attenuated when the range of potential baseline confounders were included (Fully Adjusted—Model 2, Figure 2, Table S4), and further again when the postnatal measures of employment were included (Fully Adjusted—Model 3, Figure 2, Table S5). In Model 3, compared with the reference (Group 1), children who received NHV only (Group 2) had lower internalising scores (MD = −0.51, 95% CI (−0.9, −0.1), $p = .01$), and there was an indication that children who received both exposures performed better on the NIH dimensional change card sort (executive functioning, MD = 4.59, 95% CI (−0.8, 10.0) $p = .09$). Otherwise, there was little evidence of differences in 4-year-old child outcomes according to exposure group, or evidence for interaction between NHV and quality ECEC.

4 | DISCUSSION

Using secondary analysis of an established trial of NHV with a cohort of Australian families prioritised for their experience of early adversity, this study aimed to (1) inform evaluation of

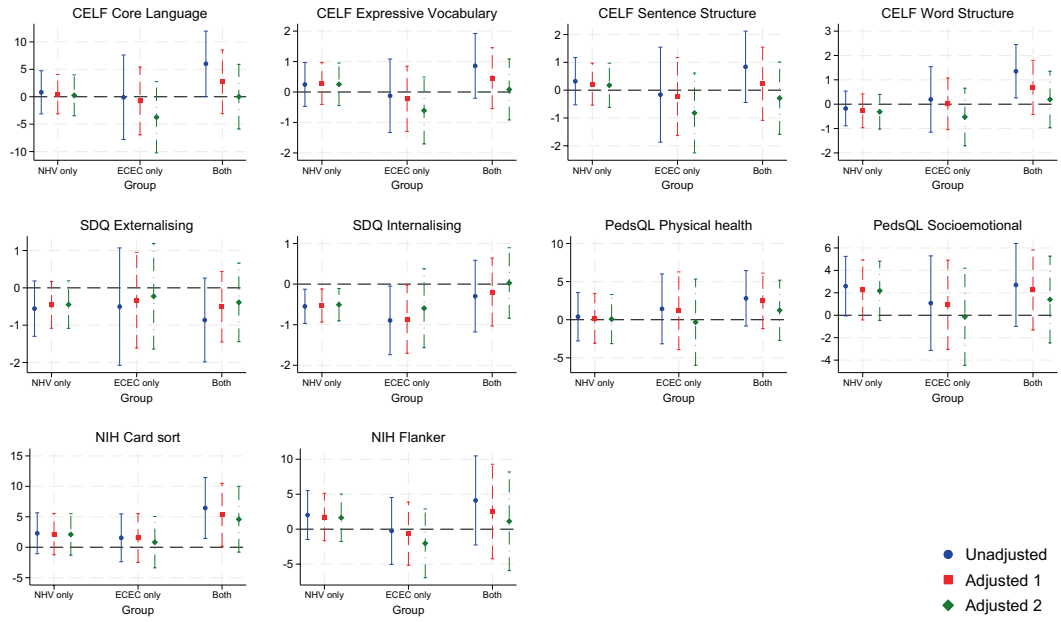

FIGURE 2 Estimated mean differences in outcomes between the exposure groups and the reference group. The reference group received neither exposure. The black line indicates no mean difference between exposure and reference groups. CELF, Clinical Evaluation of Language Fundamentals Preschool Second Edition (CELF-P2); ECEC, early childhood education and care; NHV, nurse home visiting; PedsQL, Pediatric Quality of Life Inventory; SDQ: Strengths and Difficulties Questionnaire; NIH Toolbox Early Childhood Cognition Battery.

policy implementation by (2) experimentally testing the dynamic complementarity of NHV and ECEC. In the right@home cohort of 722 families prioritised for their experience of adversity during pregnancy, 4-year-old children who received both quality NHV (pregnancy to 2 years) and quality ECEC (3 to 4 years) had a pattern of improved scores across a range of developmental measures when compared with children receiving one or neither intervention. While these differences appeared related to social determinants both in pregnancy (e.g., poor mental health) and postpartum (e.g., parental education and employment), benefits for executive functioning (NIH dimensional change card sort) remained after adjustment and warrant further investigation. While the NHV component was randomised, the ECEC component was not. Moreover, the analysis was limited by the group size once the NHV trial arms were further differentiated by use of quality ECEC. The limited evidence for dynamic complementarity is reflected. This study emphasises the variety of decisions that must be considered when evaluating equitable implementation at scale, such as type, quality, duration, dose and child age at delivery, which are discussed below. Rigorous evaluation of policy implementation will enable policy makers to determine the most effective evidence-based policy.

The limited, existing evidence base supports the theory of complementarity in larger cohorts. The American Infant Health Development Program demonstrated that increasing quality ECEC in addition to NHV was associated with improved language outcomes in 3-year-old children (Garcıa & Gallegos, 2017). The differences highlight a critical aspect of the analyses: the fidelity of the ECEC exposure. The American program offered a quality ECEC program with standardised staff training and curriculum; these features are similar to other quality ECEC programs such as the Perry Preschool and Abecedarian programs that are associated with short- and long-term language gains (Karoly et al., 2005). This contrasts with the heterogeneity of curriculum delivery and staffing arrangements in the "real-world" Australian ECEC services. As the Australian NQS benchmark comprises composite scores of several domains, ECEC providers could have attained lower ratings on service features more strongly associated with developmental gains (such as quality of pedagogical practice or educator-child relationships), but still met the standard. Indeed, recent analysis of increased enrolments in Australia's expanded four-year-old funded preschool program found no evidence of association with increased school readiness (Castillo & Petrie, 2022). However, the analysis did not consider elements such as quality rating, dose or duration.

In contrast with our findings, observational modelling has demonstrated the potential additive benefits of "stacking" early service interventions on Australian children's reading ability at 9 years of age (Molloy et al., 2019). This may be because middle childhood offers a clearer measurement opportunity for developmental differences than our analyses at 4 years of age. In addition, the impact of social determinants on service access may be more pronounced for the right@home than for the population-level cohort, which missed some families experiencing high levels of adversity. It may also be that our analyses included greater adjustment for socioeconomic drivers, or that the residual confounding evident in our Partially Adjusted (Model 1) and Fully Adjusted Model 2 were also present in the Australian longitudinal data analysis (Molloy et al., 2019), which could arise from factors such as service availability, and knowledge and trust of services and access to entitlements (Price et al., 2022).

Notably, the 15 h per week of ECEC chosen to align with Australian ECEC funding agreements is less than children experiencing adversity need to benefit measurably. Research suggests that cognitive and language benefits result from at least 30 hours of high-quality care per week (Bennett, 2008; Loeb et al., 2007; Sylva et al., 2004). Ideally, we would have examined the impacts of at least 30 h of ECEC that exceeded the NQS, and as per the definition of quality from ReStacking The Odds (Molloy et al., 2021). One year of quality ECEC may also have been insufficient to produce measurable developmental benefits (Fox & Geddes, 2016). As mentioned above, it may also be that 4 years is too early to measure benefits of NHV and ECEC over and above the pervasive impacts of early adversity. It is common for benefits of these

programs to emerge in school age children (Karoly et al., 2005). These unanswered questions regarding dose, duration and quality illuminate the level of detail that policymakers must consider if they are to evaluate the benefits of policy implementation and continuously improve service delivery.

Our cohort was established to assess a two-group (NHV/control) design, so statistical power was limited to detect differences between the four groups which had been further classified based on quality ECEC use. As ECEC exposure could not be randomly allocated, the lack of evidence for impacts of ECEC or complementarity on developmental outcomes, may be due to similarities in dose (hours used) between ECEC groups. This reflects the relatively arbitrary nature of the subsidised dose and caregivers' preferences for ECEC use. Additionally, our analyses could not account for the availability or proximity of places in the quality of ECEC centres and whether parents were actively trying to seek higher quality or different ECEC. As the current Australian Government funding drivers of ECEC are primarily work hours and income, and the right@home cohort was largely not in paid employment, accessing ECEC regardless of quality may have been limited. We assessed multiple outcomes, which can increase the likelihood of false-positive findings and, finally, ECEC use was measured via parent report so may be subject to recall bias.

Despite the limitations, the study had strengths. The highly detailed ECEC data included the names and addresses of centres, which enabled matching with NQS quality indicators for a large cohort of Australian families experiencing adversity, who are typically excluded from health services and their research evaluations (Bonevski et al., 2014). Randomisation of the original cohort effectively achieved exchangeability between trial arms with minimal differential loss-to-follow-up. The NHV intervention and follow-up were conducted with a high degree of fidelity (Kemp et al., 2019) and we used best practice missing data analyses to account for missing data and attrition over time in estimation (Seaman et al., 2012; Sterne et al., 2009).

Although there is global policy interest in tackling childhood inequities, it is extremely challenging to develop and translate interventions that alter the health and developmental trajectories of children experiencing adversity. In Australia, there are minimal indications of closing the equity gap (AEDC, 2021), presenting a significant yet preventable public health issue requiring multi-sectoral action. Empirical evidence of dynamic complementarity could inform how services are prioritised and sequenced for maximum benefit and relevance to children and families. As Australian health and education departments roll out various early childhood programs, there are opportunities to evaluate dynamic complementarity as a mechanism to reinforce and boost early childhood investments. Child and family health services are delivered in all jurisdictions with the intention of universality and almost all deliver versions of NHV (Price et al., 2021). Some NHV programs are, or are adapted from, established models while others are developed in-house or take the form of embedded outreach services. Australia has also seen years of ongoing ECEC policy reform, which most recently includes the roll-out of, or plans to roll-out, 3-year-old kindergarten in a number of states, and the federal Plan for Cheaper Child Care (Australian Government Department of Education, 2022).

In the state of Victoria, the Educational and Developmental Gains in Early Childhood (EDGE) study is conducting a five-year pre-/postevaluation of the state-wide roll-out of 3-year-old kindergarten, which is the first of its kind in Australia. Concurrently, Generation Victoria, known as GenV, has recruited a state-wide cohort of births to create a research platform that will enable rapid evaluation of programs and policy implementation to overcome the need for building studies from the ground up. In addition, in 2023, the Department for Education in the state of South Australia conducted a Royal Commission to gather the evidence on best-practice universal 3-year-old ECEC for addressing developmental vulnerability (South Australia Department for Education, 2023). Many of the recommendations focus on embedding evaluation into implementation to answer the specific questions of what works for who and when. This includes large-scale evaluation of universal roll-out, as well as smaller

studies of integrated service delivery and place-based approaches for children and communities experiencing higher levels of developmentally vulnerability and those who are often excluded from existing service design (South Australia Department for Education, 2023). Should an early childhood monitoring system be established (also a recommendation), it could enable the measurement and evaluation of program benefits across sectors.

These settings offer unparalleled opportunities for evaluating the complementary impact of services such as high-quality ECEC and NHV. However, robust evaluation designs are reliant on outcome measures across the whole of population (available for both intervention and control sites) across the time of the evaluation (which may be several years). Outcome measurement may be addressed through the proposed national preschool developmental assessment, if the measure is embedded with enough forethought, and if permissions for data sharing (e.g., of ECEC access, enrolment and attendance data) are enabled between sectors. In South Australia, for example, it would be possible to use trial designs such as the stepped-wedge to randomise the roll-out of NHV and ECEC across localities to evaluate the complementary impact of the two programs (Venkataramani et al., 2020). The stepped-wedge is a good design for effective interventions that rely on quality implementation to be impactful and lend themselves to policy trials where roll-outs are inevitable (Hemming et al., 2019). The other benefit of this design is that there is the potential of improving implementation over time (especially if process issues related to quality, etc. are measured); an important aspect often not available in the standard trial design.

Using the preschool developmental assessment as the baseline measure, conducted with each annual cohort of children, the South Australian Government could conduct the first, real-world and randomised evaluation of the complementary benefits of NHV and ECEC, complemented by routine implementation data on dose, duration and quality. It would be important that the developmental assessment occur immediately preceding roll-out of the intervention in the allocated arms (e.g., the start of each calendar year) so that the annual assessments measure any changes in children's development produced by program roll out over time. Notably, in the current study, some children were receiving long periods of care (up to 50 h) within settings that did not meet quality standards. Availability of quality care is a critical consideration in any roll-out, as is ensuring that poor quality care does not have a negative impact on children. Quality should be closely monitored in any attempt to achieve reach and capacity within provision of care, and requires careful planning around workforce preparation and provision.

Without sustained investment, benefits of effective programs dissipate over time (Heckman & Mosso, 2014). By failing to evaluate the effects and complementarity of early childhood policies, we may miss opportunities to boost children's chances across the lifespan. Rigorous evidence relies on thoughtful evaluation with careful planning and recording, and where individual randomisation is not possible, evaluation design should be aligned with policy roll-out (Venkataramani et al., 2020). Our study design and findings reiterate the applicability and feasibility of future research to evaluate dynamic complementarity via policy implementation at scale, opening the potential to understand synergistic benefits of policies for the families who are both the focus of policy and stand to benefit most.

AUTHOR CONTRIBUTIONS

Huu Nghia Joey Nguyen: Conceptualization; Methodology; Validation; Investigation; Data curation; Writing—original draft; Writing—review & editing; Formal analysis. **Fiona Mensah**: Conceptualization; Methodology; Resources; Writing—review & editing; Supervision; Investigation; Funding acquisition. **Sharon Goldfeld**: Conceptualization; Investigation; Funding acquisition; Methodology; Writing—review & editing; Project administration; Supervision; Resources. **Rheanna Mainzer**: Methodology; Validation; Visualization; Writing—review & editing; Formal analysis; Data curation. **Anna Price**: Conceptualization; Investigation; Funding acquisition; Writing—original draft; Methodology; Visualization; Writing—review & editing; Project administration; Data curation; Supervision; Resources.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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