

Examining the impact of an accommodation and support intervention in reducing homelessness amongst care leavers in Australia: A hybrid type-1 implementation-effectiveness study using propensity score methods

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ABSTRACT

Background: Young people transitioning from out-of-home care (OOHC) face elevated risks of adverse outcomes across multiple domains. Existing interventions have shown small or null effects when rigorously evaluated, highlighting the need to identify effective approaches to supporting care leavers.

Objective: Evaluate the impact of the Premier's Youth Initiative (PYI), an accommodation and support intervention, on homelessness outcomes for care leavers and explore its implementation.

Participants and setting: 295 eligible care leavers who received PYI in New South Wales, Australia between 2017–2020 and a matched comparison group drawn from locations where PYI was unavailable.

Methods: We undertook a hybrid type-1 implementation-effectiveness study that used linked administrative data from child welfare and homelessness services. Propensity score methods were applied to estimate the average treatment effect on the treated on ten measures of homelessness between ages 18 and 19. Implementation was explored through focus groups with participants, service providers and the funder to explore intervention acceptability as well as barriers and facilitators.

Results: Overall PYI had no impact on any assessed homelessness measures—with treatment effects consistently near zero. Subgroup analyses showed that Aboriginal participants experienced worse outcomes than non-Aboriginal peers. Participants who experienced homelessness during OOHC appeared to benefit more from PYI, relative to those who did not, but estimates were inconsistent. Implementation analysis identified high acceptability among participants but revealed substantial barriers, particularly inadequate planning for leaving care.

Conclusions: Interventions like PYI may need to commence earlier, be provided at greater intensity, or be targeted more effectively to the most vulnerable care leavers.

1. Introduction

Children and adolescents who experience abuse or neglect by their parents or carers can be permanently removed from their families and placed in statutory OOHC arrangements to protect their safety and wellbeing. If not restored to their families, adopted, or transferred to guardianship arrangements, young people can remain in OOHC until formal support ceases, which typically lasts until they are between 18 and 21 years of age (Strahl et al., 2021). These young people have often experienced significant trauma and disruption both before and during their time in OOHC, including multiple placement changes, school

moves, and irregular family contact (Sanders et al., 2021). These compounding disruptions can exacerbate difficulties in forming stable relationships, maintaining educational continuity and developing key life skills (Nuñez et al., 2022). When young people reach the statutory leaving-care age (when state parental responsibility ends), they exit OOHC and become care leavers. Many care leavers lack the material resources, social networks, and independent living skills needed to thrive (Courtney & Dworsky, 2006). Consequently, care leavers tend to have lower educational attainment and higher rates of unemployment, homelessness, criminal behaviour, financial stress, and physical and mental health challenges relative to peers without care experience

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(Gypen et al., 2017; Petäjä et al., 2022). Moreover, the assistance they receive before or after leaving OOHHC frequently proves insufficient to prevent these adverse outcomes (Taylor et al., 2024).

Rigorous evaluations of interventions for care leavers have generally found they are of limited effectiveness across a wide range of outcomes. Interventions for care leavers typically fall into two broad categories: transition support programs and extended care policies. A recent systematic review identified only 14 studies that used experimental or observational methods of sufficient quality to support causal inference, with all but one conducted in the United States (Taylor et al., 2024). The review found limited evidence that transition support programs improve outcomes, with most showing small or null effects when synthesised in a meta-analysis. Independent Living Programs—the most common type of transition support program—showed no meaningful impact across housing, education, employment and life skills outcomes. While some individual studies reported promising results, these effects were modest (Courtney et al., 2019; Lim et al., 2017). Evidence from two observational studies on extended care policies showed more promising results. In Washington State, extended care led to substantial reductions in homelessness and improved educational outcomes (Miller et al., 2020). In Illinois, extended care led to small improvements in educational attainment and reduced criminal justice involvement, although these effects diminished over time (Courtney & Hook, 2017). However, the review concluded that the scope and strength of evidence was insufficient to strongly recommend any approach (Taylor et al., 2024). Since that review, additional studies have reported persistent reductions in homelessness among care leavers following the introduction of extended care policies in the United Kingdom (Picker et al., 2024) and in the United States (Spindle-Jackson et al., 2024). In addition, an evaluation of Lifelong Links, a transition support program from the United Kingdom, was found to reduce homelessness amongst care leavers (Sanders and Picker, 2023). Collectively, these studies add to the growing—though still limited—evidence base suggesting that extended care policies and targeted transition support programs may help reduce homelessness among care leavers.

Like their peers internationally, care-experienced young people in Australia face significant challenges after leaving care. Using linked data from state and national sources, the Australian Institute of Health and Welfare (AIHW) found that young people between 18 and 30 years of age who had experience in care were 9–10 times more likely to access Specialist Homelessness Services (SHS)—with a peak at 21 % at age 18—compared to peers in the general population (AIHW, 2021, 2022, 2023). They were also three times more likely to receive income support payments between the ages of 16 and 30. Nearly half (46 %) of care-experienced young people received both income support and SHS, compared to 5.7 % of their peers without care experience—an 8.1-fold difference. The use of both SHS and income support was 1.6 times higher among those with experience of residential care (66 %) compared to foster care (41 %). Young people with care experience who were also identified as Aboriginal (59 %) were 1.4 times more likely than their non-Aboriginal counterparts (41 %) to access both forms of support (AIHW, 2023). These national patterns are also reflected at the state-level, care leavers in New South Wales (NSW), surveyed 4–5 years after leaving care (aged 22–23), reported experiencing high rates of homelessness and housing instability (Cashmore & Paxman, 2007). This group also had poor educational and employment outcomes—with less than half completing secondary school and only a quarter in full-time work or study—and widespread mental health problems that affected nearly half of respondents (Cashmore & Paxman, 2007).

Recognising this need, the NSW Department of Communities and Justice (DCJ) introduced a transition support program in 2017 that was designed to reduce homelessness amongst care leavers after exiting OOHHC at age 18: the Premiers Youth Initiative (PYI). This intervention featured four core components: (1) leaving care planning; (2) prosocial network development; (3) education and employment mentoring; and (4) transitional support. These components were delivered through the

provision of personal advice, education and employment mentoring, housing, transitional support and brokerage for additional supports. Service providers had significant latitude in the way they implemented PYI, and all intervention components could be provided by a single agency or in consortium with others providing specialist services (e.g., housing providers). Sites also had the freedom to determine how roles and responsibilities were allocated among three key workers—Personal Advisors, Education and Employment Mentors, and Transition Support Workers. These workers collaborated to provide support services tailored to the needs of care leavers. Client facing staff were expected to have completed training in motivational interviewing, trauma and addictions and trauma-informed practice (Taylor et al., 2020).

1.1. Objectives

This study aims to estimate the impact of PYI on homelessness outcomes for young people exiting OOHHC at age 18 and examine aspects of its implementation. We used a hybrid Type-1 implementation-effectiveness study design (Curran et al., 2012) to assess the effectiveness of the intervention, while also collecting information about its implementation. The primary objective was to examine whether receiving PYI reduced the likelihood of using Specialist Homelessness Services (SHS) in the 12 months post-OOHHC exit (i.e., from age 18 to 19). We did this by comparing PYI participants with a statistically-matched comparison group receiving services as usual, using propensity score methods to calculate the average treatment effect on the treated (ATT). The secondary objective was to explore its implementation by assessing intervention acceptability among PYI recipients and identifying barriers and facilitators to implementation from the perspectives of service providers and the funder.

2. Methodology

Our rationale for using a hybrid Type-1 implementation-effectiveness study design is twofold. First, applying causal methods is warranted given the intervention's novelty and the limited evidence base for effective supports for care leavers (Taylor et al., 2024). Second, the intervention comprises multiple active components and was delivered to a vulnerable population within a complex service system where multiple implementation factors could plausibly influence outcomes (Fixsen et al., 2005). Documenting how the intervention was delivered alongside the effectiveness analysis enables interpretation of effect estimates in light of barriers, facilitators, and the service delivery context.

An evaluation of PYI was completed by the authors in 2020 and was published previously as a technical report (Taylor et al., 2020). The technical report relied on data with variable, and limited, follow-up periods after participants left OOHHC—necessitating the use of time-to-event modelling. The present study utilises an updated administrative data extract that allows for the assessment of homelessness outcomes for all participants within 12 months after leaving OOHHC at age 18.

All research was approved by the Monash University Human Research Ethics Committee (MUHREC: #18216). All participants who took part in primary data collection provided their informed consent. The updated quantitative analysis was conducted under a Research Agreement with DCJ, who retain ownership of unit-record data used in this study. Unit-record data was stored and analysed within Monash University's Secure Research Platform (SeRP). All quantitative analysis and visualisation was undertaken using R version 4.3.3 (R Core Team, 2024). Detailed supplementary material providing additional methodological detail and results is available via a dedicated website hosted on a GitHub repository that also contains our analytic code (Taylor, Roberts, Rose, et al., 2025).

We followed the STROBE (Strengthening the Reporting of Observational Studies in Epidemiology) guidelines (Elm et al., 2007) and the RECORD (REporting of studies Conducted using Observational Routinely-collected Data) extension (Benchimol et al., 2015). A

completed STROBE-RECORD checklist is provided in the supplementary material (Table S1) (Taylor, Roberts, Rose, et al., 2025). Additionally, we adhered to Thoemmes and Kim's (2011) reporting guidance for studies using propensity score methods (Table S2).

2.1. Intervention location

PYI was developed and funded by DCJ and was delivered by seven consortia of non-government agencies that provided either support services, housing services or both. During the evaluation period (2017–2020), PYI was available in ten administrative districts used by DCJ: Central Coast, Hunter, Illawarra Shoalhaven, Mid-North Coast, Nepean Blue Mountains, New England, Northern NSW, Southern NSW, South-Western Sydney, and Western NSW. As of 2025, the intervention is still provided in the same catchments under the name Youth Initiative (Homes NSW, 2025).

2.2. Participants and recruitment

2.2.1. Intervention inclusion and exclusion criteria

To be eligible for PYI, young people must be aged between 16 years, 9 months and 17 years, 6 months and meet one or more of the following four inclusion criteria: (a) they must either be in residential OOHC, (b) have a history of placement instability, (c) be in a permanent OOHC placement, or (d) have been in care for 12 months or longer (see Table S3). Young people who are incapable of living independently due to having high-support needs were not eligible to receive the intervention.

2.2.2. Intervention recruitment

Young people who met one or more of these criteria were identified through DCJ's administrative data systems, and PYI providers then approached them directly to invite them to participate in the intervention. No other referral pathways existed.

2.2.3. Evaluation recruitment

Care leavers who commenced PYI between 1 July 2017 and 30 March 2020 were included in the quantitative analysis. A convenience sample of PYI participants, PYI service providers and DCJ stakeholders were invited to participate in interviews or focus groups to understand the intervention's implementation. Detailed information about recruitment and consent procedures is available in the supplementary material (Sections S3.2–S3.4).

2.3. Data and measures

This study used linked unit-record administrative data from two DCJ-held data assets: (1) *ChildStory*, which captures child and family interactions with the child protection (1 January 1998 to 30 June 2021) and OOHC systems (1 January 1998 to 30 June 2021), and the (2) *Client Information Management System (CIMS)*, which records interactions with SHS (1 July 2015 to 30 June 2021). Individual records across data assets were linked using a statistical linkage key generated by DCJ. No records were excluded due to concerns about data quality. All PYI participants were identified in *ChildStory*.

As the primary goal of PYI was to reduce homelessness amongst care leavers after exiting OOHC at age 18, the key outcome interest in this study was homelessness—as measured by the use of SHS for housing-related reasons. SHS use was tracked between ages 18 and 19, with outcomes including SHS use on 18th and 19th birthdays, new (i.e., commencing after age 18) and ongoing (i.e., commenced before age 18) homelessness spells, unsheltered homelessness episodes, and need for short-term accommodation (all binary measures). Given the low frequency of such events, we also measured duration of SHS spell (continuous measure) and the number of distinct SHS spells (count measure).

Data on the use of SHS for housing-related reasons was sourced from CIMS. Given the nature of service provision—where individuals may be referred between SHS providers within a continuous support episode—we aggregated service interactions across providers into 'spells' to reflect uninterrupted periods of homelessness assistance. A 'housing-related' spell was defined as meeting any of the following criteria: (1) current or previous week residential dwelling was a tent, improvised dwelling, no dwelling (in the open), or motor vehicle, OR (2) sleeping rough or in non-conventional accommodation in the last week, OR (3) in short-term or emergency accommodation in the last week, OR (4) requiring short-term accommodation. Demographic characteristics and details of an individual's OOHC history were sourced from *ChildStory*.

2.4. Identification and estimation strategy

Our identification strategy capitalised on two key features of the intervention's implementation. Firstly, PYI was a pilot offered exclusively in distinct geographical catchments within NSW. This spatial restriction provides a natural comparison pool—care leavers in similar policy and administrative contexts without the ability to access to the intervention. Secondly, the study was initially commissioned to use a randomised controlled trial (RCT) with an individual's eligibility determined by four explicit criteria recorded in administrative data. The RCT did not proceed as planned; nonetheless, the four intervention eligibility criteria (denoted as X_i) were retained, providing a well-defined assignment mechanism. An individual was assigned to treatment if, and only if, they met one of the four eligibility criteria and resided in a location where PYI was offered. The transparent, administratively determined selection process provides a rare opportunity to credibly invoke a selection-on-observables assumption.

We used the potential outcomes framework (Rubin, 1974) to specify our causal question. Let $Y_i(1)$ and $Y_i(0)$ denote individual i 's potential outcomes under intervention ($D_i = 1$) and comparison ($D_i = 0$) conditions, respectively. The observed outcome is therefore:

$$Y_i = D_i Y_i(1) + (1 - D_i) Y_i(0)$$

Our estimand of interest is the average treatment effect on the treated (ATT):

$$ATT = E[Y_i(1) - Y_i(0) | D_i = 1]$$

Identification of the ATT requires satisfying the selection-on-observables assumption:

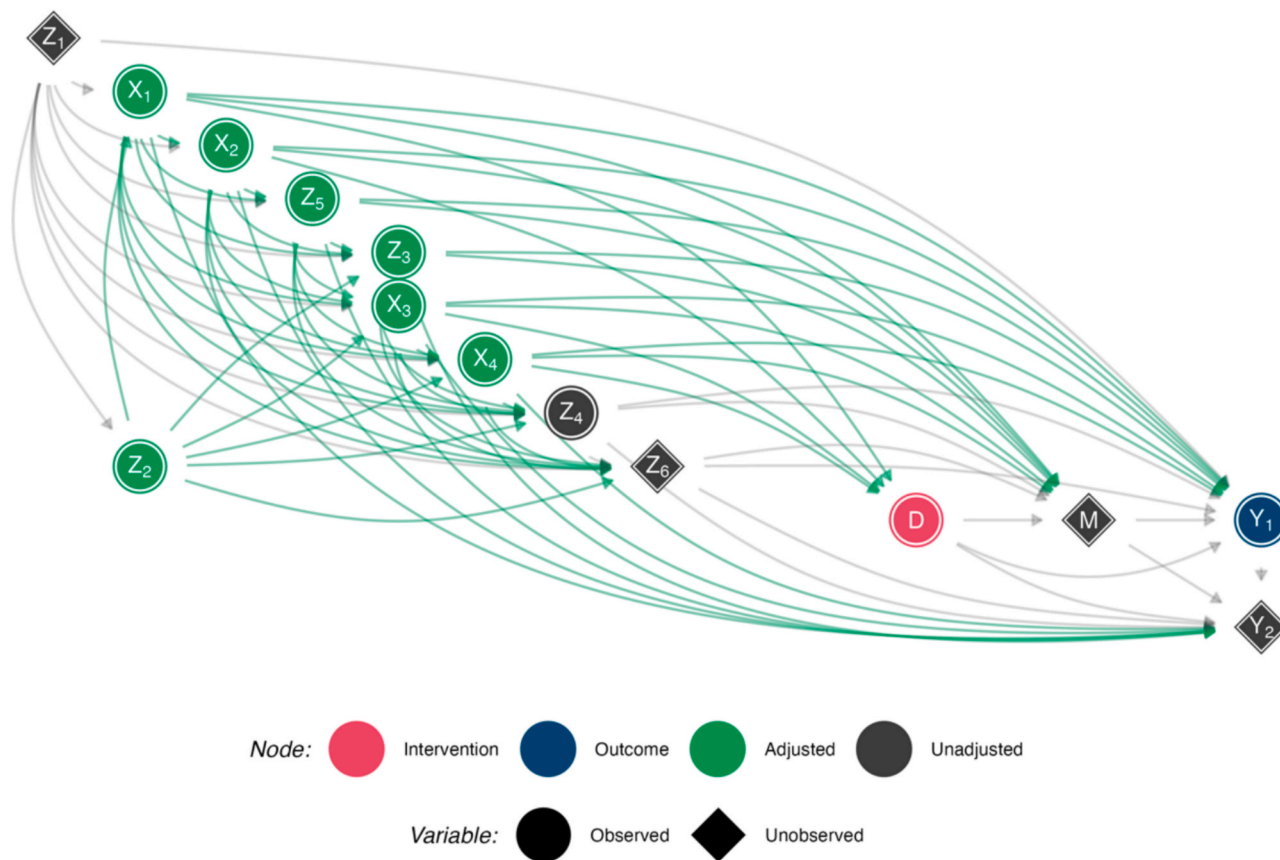
$$Y_i(1), Y_i(0) \perp D_i | X_i$$

which states that, conditional on X_i , potential outcomes are independent of treatment assignment. Given the explicitly defined selection criteria and the use of detailed administrative data, we argue that this assumption is plausible.

To test this assumption, we developed a directed acyclic graph (DAG) that models the data generating process for this study (Fig. 1). The DAG was refined during a three-round Delphi process with a panel of experts ($n = 20$) with content and technical expertise (Taylor, Roberts, & Shlonsky, 2025). The minimal adjustment set derived from this DAG using the backdoor criterion aligned exactly with the intervention's eligibility criteria X_i . This means that conditioning on the intervention criteria is theoretically sufficient to block all confounding relationships between the treatment and outcome of interest.

2.5. Propensity score matching specification

We implemented propensity score matching using a series of iterative specifications to achieve covariate balance. Propensity scores were estimated using a generalised additive model (GAM) with a probit link function using the *MatchIt* package (Ho et al., 2011). The model included both our minimal adjustment set X_i derived from our DAG and four



Key:

Y₁: Use of SHS after age 18, Y₂: Other (unobserved) outcomes, D: Intervention (PYI), M: Mediators
 X₁: 12 months or more in OOHC, X₂: History of placement instability, X₃: In residential care placement during eligibility period,
 X₄: In permanent care placement during eligibility period, Z₁: Factors that occurred before or during OOHC, Z₂: Parental responsibility of the Minister during eligibility period,
 Z₃: In kinship care placement during eligibility period, Z₄: Self-placed from placement after age 16, Z₅: Use of SHS between age 16 and 18, Z₆: Factors that occurred before 18

Fig. 1. Directed Acyclic Graph of the data generating process for this study.

additional pre-treatment covariates Z_i that improved balance without introducing confounding or collider bias (see Fig. S1).

We applied two matching approaches: (1) 1:1 nearest neighbour, where each treated unit was matched to a single comparison unit with the closest propensity score, and (2) full matching, which optimally partitions the full sample into matched sets containing one treated unit and a variable number of weighted comparison units. In both specifications, matching was done without replacement and comparison units outside the region of common support were trimmed. No treated units were excluded.

2.6. Treatment effect estimation

Treatment effects were estimated for binary, count, and continuous outcome measures using logistic, Poisson, and linear regression models respectively. Each model included the minimal adjustment set of covariates derived from our DAG X_i , as it has been shown to reduce dependence on the specification of the matching model, while potentially increasing precision and reducing bias from any residual imbalance (Ho et al., 2007). We do not include the additional covariates Z_i in our model since X_i is sufficient for identification.

Our general regression model specification took the form:

$$g(E[Y_i]) = \beta_0 + \beta_1 D_i + X_i \beta$$

where Y_i is the outcome for individual i , D_i is the treatment indicator, X_i is the vector of covariates representing our minimal adjustment set, and

$g(\cdot)$ is the appropriate link function (identity for continuous outcomes, log for count outcomes, and logit for binary outcomes).

The ATT was estimated using g -computation (Snowden et al., 2011) with the *marginaleffects* package (Arel-Bundock et al., 2024). Standard errors (SE) and confidence intervals (CI) were estimated using cluster-robust methods that account for dependence between matched sets (Abadie & Spiess, 2022). For non-linear models, where the delta method approximates standard errors, we used a cluster bootstrap with 4999 replications (Austin & Small, 2014). Following Westreich and Greenland (2013), we did not interpret individual regression coefficients, which lack meaningful causal interpretation.

2.6.1. Treatment effect heterogeneity

The presence of treatment effect heterogeneity was explored by plotting individual treatment effect estimates against their propensity scores, following Rosenbaum and Rubin (1983). We examined these plots to see if the fitted lines for the treatment and comparison groups visibly differed across the range of the propensity score and thus reveal potential effect moderators.

2.6.2. Subgroup analysis

Conditional average treatment effects (CATTs) were estimated for two pre-specified subgroups: Sex and Aboriginal and Torres Strait Islander status. Subgroup analyses were also undertaken for variables where the heterogeneity diagnostics suggested the potential for meaningful effect modification. All CATTs were estimated with the full sample using subgroup-treatment interaction terms. Given the small samples

in each subgroup and the relative rarity of the outcome of interest, we used parsimonious specifications without additional covariates. For binary outcomes, we applied maximum penalised likelihood with powers of the Jeffreys prior as penalty to reduce bias using the *brglm2* package (Kosmidis & Firth, 2021).

2.6.3. Sensitivity analysis

We assessed sensitivity to unmeasured confounding using tipping point analysis (McGowan, 2022) to determine the strength of confounding required to nullify our findings.

2.7. Implementation analysis

2.7.1. Data collection

We examined implementation through focus groups and interviews with key stakeholders (October 2019–August 2020): PYI participants (8 focus groups, $n = 36$), PYI service and housing providers (8 focus groups and 6 interviews, $n = 42$ participants), and funder representatives (5 focus groups, $n = 15$). Data collection protocols were informed by the Consolidated Framework for Implementation Research (CFIR) (Damschroder et al., 2009). Participant characteristics and data collection protocols are provided in the supplementary material (Sections S3.2–S3.4).

2.7.2. Qualitative analysis framework

A modified version of the CFIR framework was used to examine implementation barriers and facilitators at four critical junctures in the client journey: intervention entry, pre-leaving care, transition period, and independent living. Analysis followed a four-step process: data familiarisation, application of both CFIR-derived and emergent codes, categorisation of themes into barriers and facilitators, and synthesis across stakeholder perspectives. We used data convergence to triangulate findings across stakeholder groups to determine the alignment of conclusions (Palinkas et al., 2011). A separate thematic analysis examined intervention acceptability through participant perspectives on relationships with workers, support effectiveness, service appropriateness, and social connections.

3. Results

3.1. Population characteristics

Characteristics of individuals who received PYI ($n = 295$) are summarised in Table 1. Participants were evenly distributed by sex (49.1 % male, 50.9 % female). Almost a third (32.9 %) identified as Aboriginal and Torres Strait Islander, despite comprising only 7.6 % of the NSW population aged 15–19 in 2021 (ABS, 2023, 2024), which is consistent with their ongoing over-representation in OOHc in Australia (Davis, 2019). Since eligibility for PYI relied on administrative data and commencement dates were unavailable, we report characteristics with reference to the period they were eligible for the intervention (i.e., between ages 16 years, 9 months to 17 years, 6 months) rather than at a fixed baseline. Among eligibility criteria for the intervention, which were not mutually exclusive, almost all had experienced placement instability or had been in OOHc for twelve months or more. The mean duration of time an individual had been in OOHc at the start of period at which they became eligible for PYI was ~ 9 years. During the period at which they were eligible, placements were distributed across residential care (20.7 %), foster care (43.1 %), and kinship care (31.2 %). Notably, 30.9 % of participants had experienced at least one spell in SHS for housing-related reasons between the age 16 and 18.

3.2. Matching results

We evaluated multiple matching algorithms (nearest neighbour, full, optimal pair, and genetic matching) and propensity score estimation

Table 1
Baseline characteristics of intervention group.

Characteristic	Value
Sample size (n)	295
<i>Demographics:</i>	
Male	145 (49.15) ^a
Female	150 (50.85) ^a
Aboriginal or Torres Strait Islander	97 (32.88) ^a
Non-Aboriginal or Torres Strait Islander	198 (67.12) ^a
<i>Eligibility criteria for intervention:</i>	
Residential care placement during eligible period	61 (20.68) ^a
Twelve months or more in OOHc	292 (98.98) ^a
History of OOHc placement instability	292 (98.98) ^a
Permanent placement during eligible period	218 (73.9) ^a
<i>Other variables of interest:</i>	
Foster care placement during eligible period	127 (43.05) ^a
Kinship care placement during eligible period	92 (31.19) ^a
Parental responsibility of the Minister during eligible period	287 (97.29) ^a
Months spent in care at point at which eligible for intervention	107.36 (54.6) ^b
Placement breakdown after age 16	65 (22.03) ^a
Placement ended due to disruptive behaviour after age 16	107 (36.27) ^a
Self-placed, missing or absent from placement after age 16	54 (18.31) ^a
Independent living placement after age 16	59 (20) ^a
Spell in homelessness services after age 16	91 (30.85) ^a

^a N (%)

^b Mean (SD)

methods (generalised linear models, gradient boosting, random forests, and GAMs with various link functions). While nearest neighbour matching with a probit-link GAM achieved good balance for most covariates, one variable exceeded the conventionally accepted standardised mean difference (SMD) threshold of 0.1. Applying full matching to the same model improved balance across all covariates but reduced the effective sample size (ESS). The final specification yielded an ESS of 196.8—representing the equivalent number of equally weighted comparison units—compared to 295 in the nearest neighbour specification (see Table 2). This highlights the bias-variance trade-off: nearest neighbour matching offers greater precision through a larger sample, whereas full matching minimises bias by improving covariate balance, albeit at the cost of increased variance. Given our primary goal was to reduce bias through optimal covariate balance, we selected full matching as our preferred specification. Balance diagnostics, including covariate balance (Fig. S1) and common support (Fig. S2) for both specifications are available in the supplementary material.

3.3. Effectiveness results

3.3.1. Overall results

Overall, receiving PYI does not appear to alter the likelihood of care leavers experiencing homelessness-related outcomes between their 18th and 19th birthdays. Point estimates of the ATT for each of the ten measures of homelessness service usage were consistently near zero, and confidence intervals encompassed no effect under both matching specifications (Table 3).

All outcomes represent negative events (e.g., entry into a SHS spell), so a negative ATT estimate would favour PYI. However, the observed estimates typically centre near zero, with confidence intervals comfortably spanning no effect. This suggests that, on average, PYI does not meaningfully shift the likelihood or magnitude of these outcomes relative to the comparison group that received usual services. Given that these events occur in only approximately five percent of the sample, their rarity, along with the relatively small sample size, may hinder detection of small or subtle effects. The results are highly consistent across both matching specifications, reinforcing the conclusion that

Table 2
Results from two selected matching specifications.

Specification	Intervention		Comparison				
	PYI sample	PYI matched	Sample	Matched	ESS	Unmatched	Discarded
Nearest Neighbour	295	295	422	295		127	0
Full matching	295	295	422	406	196.8	0	16

Table 3
ATT results for both matching specifications.

Outcome	Estimated Potential Outcomes		Treatment Effect		
	Intervention group	Comparison group	ATT	SE	95 % CI
<i>Full matching:</i>					
In SHS spell on 18th birthday ^a	0.047	0.067	-0.02	(0.022)	[-0.068, 0.019]
In SHS spell on 19th birthday ^a	0.064	0.051	0.013	(0.022)	[-0.032, 0.055]
New SHS spell between 18th & 19th birthday ^a	0.142	0.142	0.000	(0.03)	[-0.06, 0.055]
New or ongoing SHS spell between 18th & 19th birthday ^a	0.169	0.180	-0.011	(0.034)	[-0.076, 0.056]
New unsheltered homelessness episode between 18th & 19th birthday ^a	0.129	0.134	-0.006	(0.031)	[-0.074, 0.049]
New or ongoing unsheltered homelessness episode between 18th and 19th birthday ^a	0.153	0.173	-0.020	(0.035)	[-0.09, 0.046]
In new SHS spell that requires short term accommodation between 18th & 19th birthday ^a	0.142	0.142	0.000	(0.03)	[-0.06, 0.055]
In new or ongoing SHS spell that requires short term accommodation between 18th & 19th birthday ^a	0.169	0.180	-0.011	(0.034)	[-0.076, 0.056]
Number of distinct SHS spells between 18th & 19th birthday ^b	0.227	0.351	-0.124	(0.079)	[-0.279, 0.031]
Days in SHS spell between 18th & 19th birthday ^b	15.427	18.725	-3.298	(4.931)	[-12.963, 6.368]
<i>Nearest Neighbour matching:</i>					
In SHS spell on 18th birthday ^a	0.047	0.062	-0.014	(0.017)	[-0.049, 0.019]
In SHS spell on 19th birthday ^a	0.064	0.048	0.016	(0.018)	[-0.02, 0.053]
New SHS spell between 18th & 19th birthday ^a	0.142	0.128	0.014	(0.027)	[-0.038, 0.068]
New or ongoing SHS spell between 18th & 19th birthday ^a	0.169	0.169	0.000	(0.029)	[-0.057, 0.057]
New unsheltered homelessness episode between 18th & 19th birthday ^a	0.129	0.121	0.008	(0.026)	[-0.043, 0.061]
New or ongoing unsheltered homelessness episode between 18th and 19th birthday ^a	0.153	0.161	-0.008	(0.028)	[-0.063, 0.047]
In new SHS spell that requires short term accommodation between 18th & 19th birthday ^a	0.142	0.128	0.014	(0.027)	[-0.038, 0.068]
In new or ongoing SHS spell that requires short term accommodation between 18th & 19th birthday ^a	0.169	0.169	0.000	(0.029)	[-0.057, 0.057]
Number of distinct SHS spells between 18th & 19th birthday ^b	0.227	0.307	-0.080	(0.059)	[-0.196, 0.036]
Days in SHS spell between 18th & 19th birthday ^b	15.427	19.667	-4.240	(4.458)	[-12.979, 4.498]

^a For binary outcomes, potential outcomes represent the estimated predicted probability of experiencing the outcome in each group (ranging from 0 to 1). The ATT represents the difference in these probabilities between groups (risk difference).

^b For continuous and count outcomes, potential outcomes represent the estimated mean value in each group. The ATT represents the mean difference between groups.

there is no large or systematic difference attributable to PYI.

To facilitate interpretation and synthesis, we have reported results in multiple formats—for both full (Table S8) and nearest neighbour (Table S9) specifications in the supplementary material. For binary outcomes, we report risk differences, relative risks, and odds ratios. For count outcomes, we present both the raw mean difference and its standardised form (Cohen’s *d*). For continuous outcomes, we report mean differences in the natural units as well as Cohen’s *d*. We have opted to provide this range of effect measures as (a) it allows readers to interpret results in their preferred metric, and (b) it may support including this study in future *meta*-analyses work. In Fig. 2, we present a complete summary of our results, including both matching specifications and all or our subgroup analyses, with results visualised using Cohen’s *d* to place them on a common scale.

3.4. Treatment effect heterogeneity

We tested for treatment-effect heterogeneity in the ATT model and in six additional conditional ATT analyses stratified by binary variables included in the matching model (see Figs. S3–S12). Across all ten outcomes, treatment-effect heterogeneity was observed for individuals who had used SHS between ages 16 and 18 (hereafter referred to as ‘housing vulnerability’). This pattern was observed in both matching

specifications, although it was more pronounced under full matching. These findings suggest that housing vulnerability is a potential moderator, warranting further examination through subgroup analysis.

3.4.1. Subgroup analyses

Subgroup analysis was undertaken to estimate the conditional ATT (CATT) on being male, Aboriginal or Torres Strait Islander or previously experiencing housing vulnerability.

No difference was detected for the existence of moderation by sex (see Tables S10 and S11) in both specifications.

We found that Aboriginal status moderates the treatment effect in four of the ten examined outcomes in our full matching specification. For the outcome in SHS on 18th birthday, Aboriginal participants experienced an increased risk (CATT: 0.068, 95 % CI: [0.024, 0.126]), while non-Aboriginal participants experienced a decrease (CATT: -0.057, 95 % CI: [-0.126, -0.012]). The difference in risk differences (-0.125, 95 % CI: [-0.201, -0.049], *p* = 0.001) provides some evidence of a negative moderation effect. Similar patterns were observed for new or ongoing SHS spell between 18th and 19th birthday (-0.164, 95 % CI: [-0.308, -0.020], *p* = 0.026), new or ongoing SHS spell requiring short-term accommodation (-0.164, 95 % CI: [-0.308, -0.020], *p* = 0.026), and days in SHS spell between 18th and 19th birthday (-29.651, [-49.273, -10.029], *p* = 0.003). In each of these

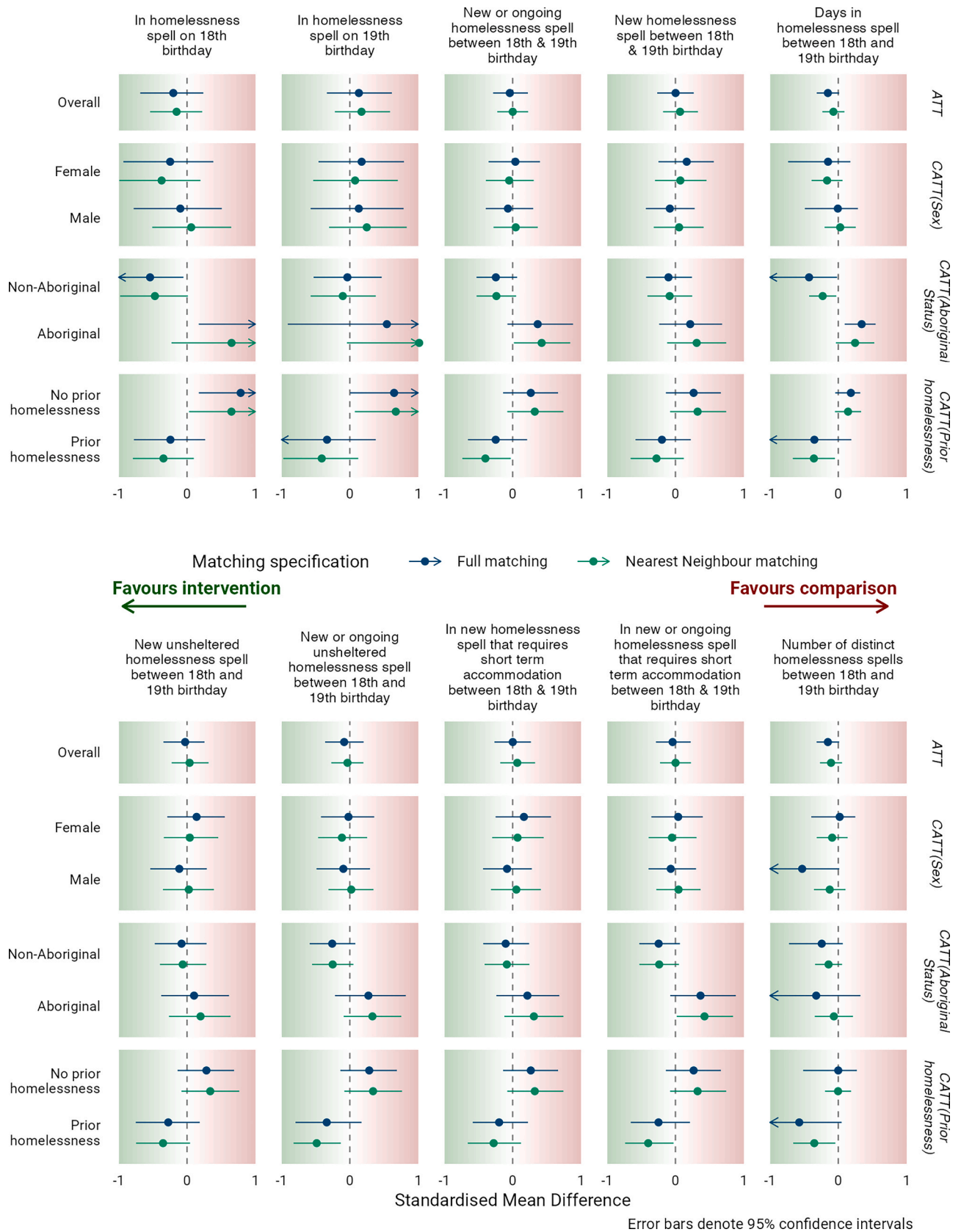


Fig. 2. Treatment effect results for overall (ATT) and subgroup analysis (CATT) presented as SMD for both matching specifications.

cases, Aboriginal participants experienced less favourable outcomes compared to their non-Aboriginal counterparts.

To put these results into context, for results reported as a risk difference (RD), we calculated the Number Needed to Treat (NNT) or Number Needed to Harm (NNH) by taking the inverse of the point estimate of the CATT ($1/RD$) (Laupacis et al., 1988). For Aboriginal participants, an NNH of ~ 15 ($1/0.068 = 14.71$) indicates that for every 15 youth who received PYI, one additional youth will be in SHS on their 18th birthday. Conversely, for non-Aboriginal participants, an NNT of ~ 18 ($1/-0.057 = -17.54$) means that 18 youth must receive the intervention to prevent one additional youth from being in SHS on their 18th birthday. Results from the nearest neighbour specification corroborated these results and identified similar moderation effects in subgroup models for two additional outcome measures: (a) in homelessness spell on 19th birthday and (b) new or ongoing unsheltered homelessness spell between 18th and 19th birthday (see Tables S12 and S13).

Moderation findings for housing vulnerability varied by matching specification. Results from the nearest neighbour estimates suggested that prior homelessness moderates the treatment effect in seven out of ten outcomes (Table S14). Young people *without* prior homelessness experienced a higher risk (CATT: 0.045, 95 % CI: [0.012, 0.088]; NNH: ~ 22) of being in an SHS spell on their 19th birthday. Whereas, those with prior homelessness were all at a lower risk of experiencing a new or ongoing SHS spell between their 18th and 19th birthdays (CATT: -0.173 , 95 % CI: [-0.313 , -0.018]; NNT: ~ 6), days in SHS spell between their 18th and 19th birthdays (CATT: -34.535 , 95 % CI: [-62.702 , -6.368]), number of distinct SHS spells between their 18th and 19th birthdays (CATT: -0.399 , 95 % CI: [-0.754 , -0.044]), a new or ongoing SHS spell that requires short term accommodation (CATT: -0.173 , 95 % CI: [-0.313 , -0.018]; NNT: ~ 6), or new or ongoing unsheltered homelessness spell (CATT: -0.203 , 95 % CI: [-0.340 , -0.059]; NNT: ~ 5).

The full matching estimates pointed in the same direction (see Fig. 2); however, they were subject to wider confidence intervals that encompassed the null (Table S15). In other words, the nearest neighbour results suggest potential heterogeneity in treatment effects by housing vulnerability and the full matching results are consistent with this possibility, however they are not detected at conventional levels of statistical significance.

3.4.2. Sensitivity analyses

Since we did not detect any precise ATT estimates, we have included results of our tipping point analysis in section S4.3 of the supplementary material.

3.5. Implementation results

3.5.1. Intervention acceptability

Intervention acceptability—defined as the perception among stakeholders that an intervention is agreeable, palatable, or satisfactory (Proctor et al., 2011)—was examined through focus groups with participants. Thematic analysis revealed that participants' perceptions of acceptability were largely shaped by four interconnected domains: relationships with workers, effectiveness of support, appropriateness of services, and peer relationships. Firstly, participants often highlighted the importance of strong, trusting connections with frontline staff. Many felt that workers' empathy, consistency, and willingness to listen were central to the intervention's overall value, particularly during critical junctures like entry into the intervention and preparation for leaving care. As one participant shared "They listen more than they talk... and they ask us what we want... we have a choice about what we want to do...". Secondly, views on the intervention's effectiveness centered on whether the support provided aligned with participants' self-identified needs. They often contrasted the way in which PYI providers supported them with negative experiences from OOHC. As one participant

noted: "My [OOHC] case worker changed so many times, these guys [PYI] really make things happen". While some described the assistance as timely and well-tailored—particularly around housing navigation and life skills—others expressed a desire for broader or more intensive resources in areas such as mental health and longer-term stability. Thirdly, participants commented on the appropriateness of services, noting that a flexible, person-centred approach helped them feel respected and acknowledged. At the same time, some participants wanted clearer communication about eligibility, referrals, and ongoing support options, underscoring the need for transparent and consistent processes. Lastly, participants' interactions with one another were seen as an influential factor. Positive peer relationships often reinforced feelings of shared experience and mutual support. As one participant observed "We have a group chat where we can chat and swap tips... it's really helpful".

3.5.2. Barriers and facilitators

We conducted a thematic analysis of implementation barriers and facilitators, organising findings across four phases of the client journey: (a) engaging with PYI; (b) in PYI and before they left OOHC; (c) as they transitioned from OOHC; and (d) living independently in the community.

Several barriers were identified during engagement with PYI. Firstly, as a pilot, PYI and its providers were unfamiliar to many OOHC case-workers, leading to inconsistent engagement and delays in connecting eligible youth to services. As one PYI provider shared "If they [case worker] didn't think their client needed PYI, or would benefit from it, they wouldn't prioritise engaging with us... it was really frustrating". Secondly, while young people with significant disabilities that prevented them from living independently were ineligible for PYI, this information was not captured in administrative data, requiring time-consuming manual screening by PYI providers. Finally, the closed referral pathway and the initial evaluation design (the use of an RCT) initially created some suspicion at multiple levels of the OOHC system in sites where PYI was provided. However, the structured eligibility criteria and use of administrative data to identify eligible young people was also identified as facilitating the identification of at-risk youth—particularly those who self-placed—who might have been overlooked through standard referral channels. As one PYI provider noted: "Without it some of these kids would have fallen through the cracks... they didn't have a caseworker, they needed us to go hunting for them."

While young people were still in OOHC, the major barrier faced by PYI providers was that many clients lacked basic aftercare preparation like leaving care plans, identification documents, and access to financial services — despite statutory requirements for this to occur. As one representative from DCJ noted "DCJ has made it really difficult to get sign off on leaving care plans... PYI has helped us show this". This required PYI staff to divert time and resources toward securing these basic entitlements rather than focusing on goal-setting and attainment. Providers felt that engaging with care leavers at an earlier age provided them with more time to build relationships and advocate for aftercare support.

As they transitioned from OOHC, several structural barriers impacted service delivery. Limited employment opportunities and appropriate housing stock in regional areas made it difficult to support care leavers to find either employment or housing. PYI's emphasis on private rental market placements created implementation challenges, as providers felt that this option wasn't suitable for all participants. As one PYI housing provider explained, "It's tough because kids don't normally move out of home at 18 and here we have these kids, that are coming straight out of care and into a private rental without knowing what's normal functioning in a house." While accommodation was available through PYI, providers reported underutilisation of allocated housing slots despite perceived scarcity, suggesting a misalignment between intervention design and local implementation contexts. Implementation

facilitators emerged through providers' adaptive responses. Success in housing placement was achieved through developing relationships with real estate agents and property owners. Providers enhanced service integration by connecting participants with complementary supports through vocational educational institutions that structured the delivery of practical skill-building activities, particularly around budgeting and tenancy management.

After young people left care, the reliance on private rental markets continued to be a barrier, where limited affordable housing stock constrained providers' placement options. While shared housing was utilised to address this barrier, implementation success varied based on whether participants had choice in these arrangements. Service delivery was further complicated by property management issues, particularly around visitor-related damages, which strained provider relationships with real estate agents. The absence of clear operational definitions for "success" created implementation uncertainty, affecting both service boundaries and exit planning. Implementation facilitators centred on sustained worker engagement and ongoing flexible support. Providers identified that young people required approximately 12 months post-care to develop adequate support networks. Finally, providers observed that they required 12–18 months to codify and deliver services they perceived to be effective, highlighting the substantial time investment needed to achieve stable intervention delivery.

4. Discussion

This study found that, on average, receiving PYI did not meaningfully alter the likelihood or magnitude of homelessness-related outcomes for care leavers between their 18th and 19th birthdays. Across ten measures of homelessness service usage, point estimates were consistently near zero, and confidence intervals encompassed no effect (Fig. 2, Table 3). The consistency of the results across both full and nearest neighbour matching specifications strengthens confidence in the absence of any large or systematic impact. Taken together, these results suggest that PYI did not alter short-term housing trajectories for most participants in the critical first year after leaving care. These results are consistent with the findings from the systematic review by Taylor et al. (2024) which found that transition support programs like PYI have small or null effects on most outcomes—including housing and homelessness. At the same time, our findings do not categorically exclude more nuanced or longer-run effects for participants as they enter early adulthood.

The overall lack of an impact of PYI may have been affected by significant system-level barriers, across all stages of program engagement, even though the program was well received by both care leavers and stakeholders. Deficiencies in leaving care planning crowded out the delivery of core components of the intervention. Poor aftercare planning and limited housing options—particularly in the private rental market—were recurrent challenges for providers. These findings are consistent with reviews of leaving care planning by the NSW Office of the Children's Guardian (2022) and Victorian Commission for Children and Young People (2020). Furthermore, PYI may have been more effective if delivered with different timing for some young people, as 31% of participants had already experienced at least one spell of homelessness service use before turning 18. Indeed, assisting care leavers in their transition to independence is a challenging area of practice, and results from this study are consistent with similar transition support programs that also tend to show very small, if any, positive effects (Taylor et al., 2024).

While average effects were close to zero, subgroup analysis identified evidence of treatment-effect heterogeneity among two population subgroups: Aboriginal and Torres Strait Islander participants and those with prior experience of homelessness whilst they were in OOH. Specifically, PYI appears less effective for Aboriginal participants than for their non-Aboriginal peers. Although this pattern was evident under both matching specifications and across multiple measures of the outcome,

our confidence in this finding is limited by the uncertainty in the estimates. Even with this uncertainty, we believe it warrants highlighting because this is the first causal study of a care-leaver intervention in Australia; it is therefore the first to examine if effects for such a program differ between Aboriginal and non-Aboriginal young people. The disparities observed here align with findings from AIHW (2021, 2023) that found that Aboriginal care-experienced young people are more likely to utilise homelessness services and income support relative to their non-Aboriginal peers. They also reflect broader inequalities in the OOH system: Aboriginal children and young people are more likely to be placed in OOH and while they are in care face elevated risks of poorer development outcomes, placement instability and involvement with youth justice while they are in care (AIHW, 2025; Asif et al., 2024; Davis, 2019; McFarlane, 2018; SNAICC, 2024). Our findings suggest that these inequities extend to the effectiveness of transition interventions for care leavers.

We also observed evidence of treatment effect heterogeneity for those who had experienced homelessness between ages 16 and 18. Under nearest neighbour matching, these young people appeared to benefit more from PYI compared with their peers without such histories. However, estimates from the full matching specification were less precise, and confidence intervals crossed the null. Given the discrepancy between these approaches, this result must be considered as exploratory. Even so, this finding is consistent with the leaving care literature emphasising that care leavers are heterogeneous in both their needs and experiences before and after their exit from care (Stein, 2006). At the same time, it challenges the "care cliff" narrative that positions emancipation and its immediate aftermath as the apex of vulnerability (Rome & Raskin, 2019). Instead, our results suggest that some of the most vulnerable young people may already be at heightened risk prior to aging out, underscoring the importance of early identification and intervention. It is possible that focusing the PYI intervention on care leavers who were at the highest risk (e.g., those who previously self-placed or used SHS) may have allowed PYI to be provided at a higher intensity, thereby concentrating support where the marginal benefit of the intervention was likely to be the greatest.

The implementation of PYI was characterised by significant system-level barriers despite strong acceptability among key stakeholders and the identification of able facilitators of success. Thematic analysis of focus groups with participants revealed that acceptability of the intervention was driven chiefly by strong worker-participant relationships, the perceived effectiveness of support, appropriate service alignment with participants' needs, and the potential for peer relationships to bolster mutual support. It is promising that care leavers found PYI highly acceptable, suggesting they welcome the provision of supportive interventions of this kind. Participants also often contrasted the support they received through PYI with the limited leaving care planning provided by their OOH caseworkers, and anecdotes indicated that self-placed young people were more likely to engage with PYI. However, because the intervention's reach was not captured in administrative data, we could not formally test this hypothesis. Facilitators included the use of eligibility criteria and administrative data to identify eligible at-risk youth, developing relationships with stakeholders (e.g., real estate agents), and providing ongoing support in a flexible manner. Of these, the use of administrative data in identifying participants was especially important, as it enabled PYI to reach young people who had self-placed and who would otherwise not have been referred through conventional pathways.

Taken together, these findings highlight the challenges inherent in delivering a novel multi-component intervention to a vulnerable population with complex needs. It underscores the importance of considering implementation context alongside effectiveness.

4.1. Limitations

Several limitations must be acknowledged, relating to the scope and

use of administrative data, the selection of participants into the intervention and the potential for unmeasured confounding. Firstly, the scope of the outcome data was limited to SHS use as a proxy for homelessness. Thus, we were unable to consider other important outcomes—such as employment, income support, arrests, convictions, incarceration, unplanned pregnancies, and emergency department presentations. Furthermore, we were only able to observe outcomes between the ages of 18 (when participants left OOHC) and 19, which is a relatively short window during which the trajectories of those receiving PYI may not have fully diverged from their counterfactual. This limited observation period, combined with the low frequency of the outcome, limited our ability to detect modest effects, even if they were present. Additionally, the use of administrative data to measure homelessness only captures individuals who engaged with SHS, thus failing to account for those who experience hidden homelessness (e.g., couch surfing), potentially underestimating the true incidence of housing instability (Metraux & Tseng, 2017).

Secondly, our understanding of the selection of participants into PYI—and thus the intervention's reach—is also limited. Although we could identify who was eligible and who commenced the program, we do not know who was offered but declined, who completed, or the extent of support received. This constrains our ability to explore intervention uptake and its potential impact on the external validity of our findings.

Thirdly, as with any study relying on selection-on-observables, the potential for unmeasured confounding must be acknowledged. Whilst we are confident in our ability to account for the intervention's selection criteria, we lack sufficient information to identify individuals in the comparison group who were not capable of living independently. Young people with health conditions or disabilities that prevent them from living independently would have been ineligible for PYI. These individuals were likely supported by a different system that includes accommodation and would thus, comparatively, have a lower likelihood of homelessness. Our inability to identify and exclude these individuals from our comparison pool potentially biases our estimates toward usual services.

4.2. Implications for practice and policy

With nearly one-third of PYI participants accessing SHS before leaving care, interventions such as PYI—that are specifically designed to prevent homelessness among care leavers—must begin earlier and be more precisely targeted to those at the highest risk. Since PYI's inception, NSW has expanded support for care leavers. Until age 21, they can now receive either (a) a financial allowance to remain with their carer, or (b) a payment to help cover accommodation and living expenses for those living independently (DCJ, 2025). These supports are typically characterised as extended care' in the literature (Mendes et al., 2025) and we adopt this term here for consistency, even though DCJ does not use this term to describe these supports. In addition, care leavers in NSW can also access the Specialist Aftercare Program that provides intensive casework, mentoring, and tailored support for care leavers with moderate to high needs (DCJ, 2025).

Extended care provisions are a scalable initiative and are appealing as a broad policy option (Mendes et al., 2025). Some evidence suggests extended care can reduce homelessness among care leavers, but the evidence in its favour is not conclusive, especially with respect to heterogenous treatment effects and the sustainment of outcomes over time (Taylor et al., 2024). Meanwhile, interventions like PYI, pose scaling challenges and may not be suited for broad adoption as a first-line strategy.

Poor aftercare planning has been identified as a practice issue in NSW (Office of the Children's Guardian, 2022; Taylor et al., 2020). Quality improvement methods such as audit and feedback, which can pinpoint and rectify deficiencies in service delivery, may be a viable option to improve practice (Grimshaw et al., 2019). Care leavers are not a monolithic group. While extended care could help some achieve a

more stable transition to independent living, it likely will not benefit others, particularly those already disengaged from their placements. The use of predictive modelling with administrative data could help identify high-risk care leavers earlier, enabling more targeted, pre-emptive responses (Cuccaro-Alamin et al., 2017).

4.3. Implications for research

Selection-on-observables approaches, such as matching, are widely used in program evaluations, but the strong assumptions they require—namely that all relevant confounders have been observed—often receive insufficient scrutiny. To address this, we employed a DAG to make our assumptions explicit. Use of a DAG does not guarantee freedom from unmeasured confounding. However, it clarifies the causal pathways that we hypothesise are present and enables others to assess potential biases in our analysis, and we recommend their use. Likewise, we also recommend application of the potential outcomes framework as it supplies a transparent and formal means of framing a causal question. Finally, the use of g-computation can help avoid the pitfalls of misinterpreting regression coefficients, commonly known as the Table 2 fallacy (Westreich & Greenland, 2013). Huntington-Klein (2022) and Cunningham (2021) provide detailed, accessible and practical resources for practitioners interested in applying such methods.

Looking ahead, there remains a pressing need to improve our understanding of how care leavers and care-experienced young people fare across critical domains—such as education, employment, mental health, and housing stability—at different ages so that we can develop policies and/or interventions that provide appropriate support at the right time. Linked administrative data offers excellent opportunities to undertake this important epidemiological work. While administrative data may lack the granular detail of surveys (or other qualitative approaches), the ability to use it to follow an entire cohort makes it uniquely suited for tracking care leavers, who can otherwise be hard to reach.

5. Conclusion

PYI did not appear to significantly reduce the risk of homelessness for most participants. The intervention was markedly less effective for Aboriginal young people, although it did appear to potentially benefit those with prior experience of homelessness. We cannot definitively determine whether PYI's limited impact stems from issues with the intervention's design, timing of delivery, or quality of implementation.

Evaluating interventions like PYI is essential for advancing our understanding of 'what works,' even (and perhaps especially) when results show limited or no effect. In the absence of experimental studies, careful application of observational methods, such as those employed in this paper, can play a crucial role in building the evidence base. By illuminating both the strengths and weaknesses of current approaches, such work helps practitioners to refine future services and ensures that support for care leavers is guided by the best available evidence.

6. Authors' contributions

Author roles were classified using the Contributor Role Taxonomy (CRediT; <https://credit.niso.org/>) as follows: David Taylor: formal analysis, writing, visualisation, conceptualisation, methodology, project administration; Jessica Roberts: data curation, conceptualisation, methodology, editing; Vanessa Rose: conceptualisation, methodology, funding acquisition, project administration, editing; Alex Gyani: conceptualisation, editing, methodology; Aron Shlonsky: conceptualisation, editing, methodology, supervision, editing.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence

the work reported in this paper.

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Data availability

The administrative data used in this study cannot be shared publicly as they remain the property of DCJ. Access to these data requires formal permission from the data custodians. The analytic code used to produce the findings reported in this paper is available in our public GitHub repository <https://github.com/davetaylor/pyi-impact-study>.

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