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Intergenerational Welfare Persistence: Causal Evidence from Vietnam-Era Draft Lotteries

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Abstract

We contribute to understanding the causal mechanisms of intergenerational welfare persistence. We study veterans' pensions, leveraging Australia's Vietnam-era draft lotteries for identification and over three decades of administrative data with universal coverage. Deployed service increased veterans' expected welfare receipt by 7.5 years (100.5%) and reduced employment (3.2 years; 30.2%). Effects for veterans' spouses were qualitatively similar and approximately half as large. How did this affect their children? Despite high statistical power, we find almost no lasting effects on child outcomes, including welfare, employment, income, education, partnering and fertility. Direct estimates of causal intergenerational welfare elasticities are precise zeros, highlighting that welfare receipt does not always spill over to the next generation.

JEL classification

H53, H55, H56, I38, J22, J62

Keywords

intergenerational mobility, welfare dependency, veterans' benefits, disability pensions, conscription, labour supply

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Strong correlations in welfare receipt between parents and children have been found across countries for decades (Page, 2004; Black & Devereux, 2011; Cobb-Clark et al., 2022). This raises concerns about intergenerational welfare dependency, and legitimate questions about whether welfare programs help or harm recipients' children (Heckman & Mosso, 2014). To address these concerns, it is crucial to understand whether such associations are causal, or if they reflect other factors which affect both parents and children. It is also important to understand the mechanisms of transmission and hence the contexts in which transmission may be malleable to intervention.

Despite such concerns, there are few well-identified causal studies on this topic. The cleanest of these are Dahl et al. (2014) who use a random judge design, and Dahl and Gielen (2021) who use a birth-cohort regression discontinuity design. Both find strong causal intergenerational persistence of disability insurance in European countries. Another notable contribution is Shanan (2024) who studies increased welfare generosity for single parents in Israel using a difference-in-differences design and finds higher adult welfare use for girls and higher adult earnings for boys exposed to the reform.[1]

In each of these quasi-experimental settings, the compliers are marginal welfare recipients, who respond to specific changes in eligibility rules. These contexts vary greatly between the key studies, providing useful variation which has enhanced our understanding of transmission mechanisms. We discuss these mechanisms in detail in Section I, but there are some commonalities across studies. First, causal transmission seems to mostly occur within the same payment type.[2] Second, the formation of children's preferences over leisure and work may explain the results reported in most of these studies.

In this context, we study causal intergenerational welfare transmission in a very different setting—Australia's Vietnam-era veterans' pensions. In this setting, very few children are eligible to receive the same type of welfare payments as their parents did, since these are tied to deployed military service, which is relatively rare. This differentiates our setting from earlier work. Many of the key potential mechanisms of transmission are arguably not relevant in our setting, which allows us to focus on other mechanisms.

A series of papers has leveraged randomly assigned draft eligibility to study effects of Australian military service, similar to the corresponding US literature

[1] Another strand of literature has used state-time variation in welfare policy in the US. In particular, Hartley, Lamarche, and Ziliak (2022) studied reforms to the Aid to Families with Dependent Children/Temporary Assistance for Needy Families programs. Other examples are Antel (1992) and Levine and Zimmerman (1996). See also de Haan and Schreiner (2025) for an alternate approach that uses bounds. Another key strand of the non-experimental literature attempts to control for unobserved factors shared by parents and children, using panel data. The approach proposed by Gottschalk (1996) has recently been adopted by Cobb-Clark et al. (2022) and Riphahn and Feichtmayer (2024).

[2] Hartley et al. (2022) is an important, but qualified, counterexample. Their instrumental variable estimates suggest that intergenerational transmission of AFDC/TANF was indeed specific to AFDC/TANF receipt by daughters. However, after the major reforms in the 1990s which tightened eligibility for the program, the daughters substituted AFDC/TANF with other welfare programs. Overall, the reforms hence did not reduce transmission of welfare dependency to children.

beginning with Angrist (1990).[3] Importantly, the Australian context is different from the US. By far the largest impact of the Vietnam draft in Australia is a large increase in welfare receipt, coupled with disemployment (Siminski, 2013). Our analysis draws on linked administrative records containing a near-universe of the Australian population, which provides ample statistical power. Our main finding is no intergenerational transmission of welfare receipt in this setting. These estimates leverage draft eligibility as an instrument for parental welfare, for which the first stage effect is very large. This empirical strategy invokes a strong assumption—that military service does not affect child outcomes through other mechanisms. As we will argue in detail, potential violations are likely to bias the estimates in a direction that is favorable to our main conclusion.

We begin the analysis by revisiting the economic effects of military service for Australian men and their spouses. We draw on three decades of newly linked administrative records that facilitate a precise analysis of near-lifetime effects. We find that deployed service increased expected welfare receipt by 7.5 years on average (100.5% of the mean for draft ineligible men), and reduced employment by 3.2 years (30.2% of the mean).[4] Such effects are also large for their spouses (around half as large as the effects for men). For both the men and their spouses these effects are concentrated in disability-related payments, for which the eligibility criteria are very generous for deployed veterans (Siminski, 2013; Atalay et al., 2019). For veterans the additional welfare more than offsets the lost earnings, leading to higher incomes at all ages for which we have data.[5]

Next, we contribute to the relatively new literature on the intergenerational effects of military service, which has focused on the US thus far. In particular, Goodman and Isen (2020) and Deza and Mezza (2025) find various negative effects of US Vietnam-era draft eligibility for the next generation.[6] The authors of both papers identify a key limitation of the US setting – draft-avoidance behavior, which makes it difficult to attribute the estimated effects to military service.[7] In comparison, there was less scope for and limited evidence of such behavior in the Australian context.[8] There are also large differences in deployment rates across

[3]See especially: Siminski (2013); Atalay et al. (2019); Johnston et al. (2016); Ville and Siminski (2011); Siminski and Ville (2012).

[4]In contrast, the US literature shows little or no overall effect of service on employment (Angrist & Chen, 2011; Angrist et al., 2011, 2010; Goodman & Isen, 2020), notwithstanding the concerns raised by Angrist et al. (2010) and Autor et al. (2011).

[5]This reflects the generosity of veterans' pensions relative to regular civilian welfare payments. Disabled veterans commonly received both the Veterans' Disability Pension (Special Rate) and the Service Pension. The combined rate of these payments was about three times higher than the civilian disability pension and age pension, or about five times higher than the unemployment benefit.

[6]Goodman and Isen (2020) find decreased earnings for sons, with parental inputs a key mechanism, evidenced by negative effects on various human capital indicators. They also find increased military enlistment, suggesting occupational transmission between men and sons. Johnson et al. (2018) and Greenberg et al. (2024) document related findings for occupational transmission. See also Christofides et al. (2022) for an alternate analysis of tertiary education transmission. Deza and Mezza (2025) find increased rates of child risky behaviors, with paternal parenting styles a key mechanism.

[7]See Bitler and Schmidt (2012); Bailey and Chyn (2020); Card and Lemieux (2001); Kuziemko (2010); Wang and Flores-Lagunes (2022).

[8]There are institutional reasons for expecting less draft avoidance ((Siminski, 2013): Appendix).

the 15 affected Australian cohorts, allowing for separate identification of service effects and deployment effects, as we will show.

We are able to document children's 'exposure' to parental effects from ages zero to 50 years, including parents' welfare receipt and employment, family stability, parental income and poverty status. Veterans' children grew up in households that were much more likely to be receiving welfare; during their teenage years their fathers were around 35 percentage points more likely to receive welfare in a given year and their father's spouses around 13 percentage points more likely. Parent disemployment effects only emerge later, as children enter their 20s. Reflecting the generosity of veterans' payments, deployed service greatly reduced the probability of veterans' children living in poverty throughout their childhood (typically around 10 percentage points less likely to be in poverty at each age).

Next we show that veterans' children are more likely to receive welfare as young adults and spend over half a year more on welfare up to age 21. These effects are concentrated in different welfare types from those their parents received—namely payments for studying where veterans' children had access to special schemes. However, even with high statistical power, we find very few lasting effects on outcomes, including later life welfare receipt, employment, earnings, income, education, partnering and fertility. The sole exception to these null results is a small effect on sons' employment in protective services occupations, broadly consistent with the occupational transmission found in Goodman and Isen (2020).

As a final exercise we explore causal intergenerational and intragenerational welfare persistence under the strong assumption that the only mechanism for draft eligibility to influence children's welfare receipt is the earlier welfare receipt of their parents (or themselves). Our ordinary least squares implied elasticities for welfare are large (0.407) and precisely estimated (s.e. 0.003)—consistent with the non-causal literature on welfare persistence. However, when we instrument parental welfare receipt by draft eligibility this falls to a precise null effect of 0.014 (s.e. 0.042). For intragenerational persistence the results are less precise but we can still rule out the correlational effect sizes.

As mentioned above, the welfare persistence estimates assume that the only mechanism for draft eligibility to influence child welfare receipt is through parents' own welfare receipt. This is a big assumption, but we believe it is a conservative assumption, and a useful exercise for two key reasons. First, the effects on welfare and employment are by far the largest identified effects of Australia's Vietnam-era service, in what is now a substantial literature. Second, potential violations of this assumption are likely to bias the estimates in a direction that is favorable to our main conclusions. A key example is the effect of service on mental health. Service diminished the mental health of Australia's deployed veterans (Johnston

There is also no meaningful effect of draft eligibility on education ((Siminski & Ville, 2012): Section VII), or on crime (Siminski, Ville, & Paull, 2016), and men with children were not exempt from service.

et al., 2016). If anything, we could expect this to negatively affect their children's outcomes and increase welfare receipt. This would bias our estimates of the intergenerational transmission of welfare upwards. Similarly, if any of the negative parenting effects found for the US (e.g. Deza and Mezza (2025)) are also present in Australia, these would bias our key estimates upwards. However, as we will show, our estimates are already precisely estimated zeros. Other prominent US studies are also consistent with this assumption.[9]

These results show that intergenerational transmission of exogenous welfare receipt is not universal. In our context, veterans' pension receipt has not had an enduring effect on the welfare receipt or employment of the next generation. This is despite veterans' children being raised in families that were more likely to receive welfare, and having early direct exposure to the welfare system themselves.

The remainder of the paper is structured as follows. We provide a brief review of the mechanisms of intergenerational welfare transmission in Section I. In Section II we describe the setting, data, empirical strategy and present both the first stage and balance tests. The effects of Vietnam service on the first generation (conscripts and their spouses) are in Section III with the effects on the next generation in Section IV. In Section V we show the estimated intergenerational effects of welfare receipt. We conclude the paper with a discussion of the findings and implications for mechanisms of intergenerational welfare transmission.

I. Mechanisms of Intergenerational Transmission

The literature highlights several potential mechanisms for a causal transmission of welfare receipt. Following de Haan and Schreiner (2025), these can be summarized with reference to a simple formal statement:

$$P^* = P^*[W(h, L), \theta, B, \phi] \tag{1}$$

where a person's propensity to participate in welfare P^* is a function of their wage W which depends on human capital h and labor market conditions L, their taste for leisure, or (dis)taste for work θ, the welfare benefit B, and the utility cost of welfare participation ϕ, which may include stigma, time, money and effort.

Parental welfare receipt may affect any of these determinants of child welfare propensity P^*. Modeled parental behavior may affect child tastes θ as well as the stigma cost of welfare (a component of ϕ). Parents may influence children's beliefs about welfare benefits (B) and costs (ϕ) and may transfer knowledge of how to navigate government welfare administration (ϕ). Welfare may affect human capital h through a range of mechanisms. It may affect parental investments in children, through both time and money, which could affect h in either direction.

[9]In particular, Bruhn et al. (2024) find that combat deployment to Iraq and Afghanistan had "limited long-run adverse effects beyond death and injury directly resulting from combat". In contrast, expansions to veterans' compensation eligibility have been shown to have very large effects on labor supply (Autor et al., 2016).

Welfare may also compromise beneficial parental employment networks, perhaps replacing these with other networks less conducive to h. In a similar vein, welfare receipt early in an individual's own life may persist by influencing these same determinants: from taste and stigma, to perceptions and knowledge, through to human capital.

Which mechanisms are invoked in the settings studied by key earlier studies? Dahl et al. (2014) highlight child beliefs in the efficacy of applying for welfare (B), and possible reduced stigma (ϕ), as the likely mechanisms in their setting. Dahl and Gielen (2021) identify learning about formal employment (translating to h and/or θ), improved home environment (again affecting h and/or θ) and scarring as possible mechanisms. Shanan (2024) argues that gender-specific shaping of norms (θ and/or the stigma component of ϕ) is likely in their setting, with generous benefits also likely improving child human capital (h). Interpreting their own findings, Hartley et al. (2022) argue that TANF does not encourage investment in long-term economic self-sufficiency. This positions child human capital (h) as a key mechanism.

Importantly, the results of each of the above studies are consistent with the shaping of child preferences for leisure versus work θ being a potential key mechanism, which is a key focus of our paper. Knowledge and beliefs about the welfare system (B and ϕ) are far less relevant given the specific nature of veterans' pensions. And we observe key indicators of child human capital h, and conclude that it is unaffected by parental or own early life welfare receipt.

II. Data, Methods, and First-Stage Results

A. Setting and First-Stage Data

We begin by describing key features of the Australian Vietnam-era conscription lotteries (Langford, 1997). There were 16 twice-yearly draft lotteries held between 1965–1972, involving over 800,000 men in total. Men turning 20 in each half-year window were subject to the lottery held in March or September of that year.[10] Similar to the US, these lotteries were conducted by date-of-birth (DOB), with each DOB represented by a marble in a barrel. But the process was simpler than in the US. On each occasion, only a subset of marbles were randomly drawn. Men whose DOBs were drawn were eligible for conscription, while the others were not. Subject to a range of exemptions, deferments, medical testing and other conditions, a subset of the "draft-eligible" men were called up for service 2-3 months later.[11] They were required to serve 2 years full time in the Army as National Servicemen, which for some included deployment to Vietnam, usually

[10]For example, men who turned 20 in January-June of 1965 were subject to the first lottery, held in March 1965.

[11]In the Australian context, the term 'ballot outcome' is commonly used for those whose DOB was drawn. Here, we use the term 'draft-eligibility', which is the equivalent term in the better-known US context.

for around 1 year. This was followed by 3 years of part-time service in the Army Reserves.

The 'first-stage' database for this setting captures the relationship between draft eligibility and service, as well as deployment to Vietnam, for each birth cohort. These first-stage relationships for each birth cohort are shown visually in Figure 1.[12] There is a clear effect of draft eligibility on military service for each of the first 15 cohorts. A particularly useful feature is that deployment varies considerably between cohorts. Following previous work, we will leverage this variation to show that almost all effects of service are limited to those who were deployed to Vietnam.

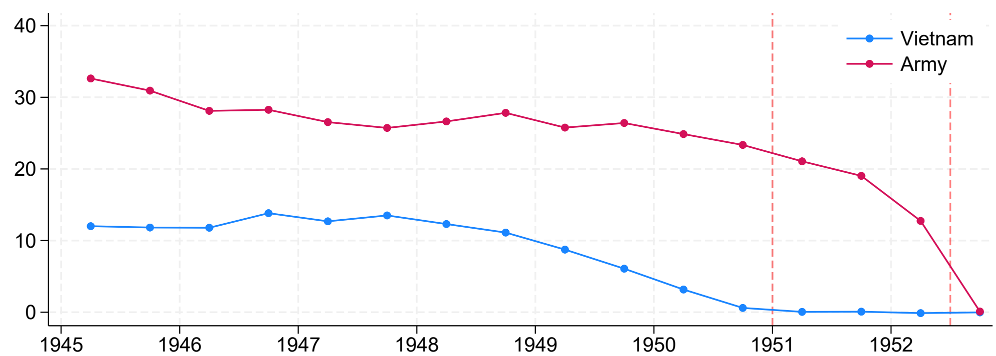


Figure 1. First stage effects of draft eligibility on % probability of army and Vietnam service by birth cohort

Note: Shows the additional percentage probability of army service and deployment to Vietnam for those whose DOBs were drawn in the Australian draft lottery (deemed 'balloted in', or 'draft eligible'), by six-month birth cohort. Dashed vertical lines indicate the points from which draft eligibility had very little effect on Vietnam deployment and army service respectively. These same first-stage regression estimates are shown in tabular form in Siminski (2013), Table 2. These data are available from the Siminski (2013) replication files: `https://drive.google.com/file/d/1jeJASPgekJvySRWbZ7TIlGY93SvJKH8g`.

We use the first-stage data in combination with a separate outcomes database, described in the next subsection, where we can also observe birth cohort and draft eligibility. The two data sources are not linked at the individual level, but they do not need to be linked to implement the Two-Sample 2SLS (Inoue & Solon, 2010) regression models that we use. However, the first-stage database is specific to the male population at the time of the draft (1965-1972), while the outcomes data were collected decades later. The two populations hence differ due to migration and mortality. We address migration to a large extent by excluding men who

[12]The majority of draft-eligible men did not serve in the military. This is due to various exemptions and deferments, discussed in detail by Langford (1997). By far the largest of these is rejection due to not meeting the medical, psychological and educational standards required by the army.

migrated after 1986 from the outcomes data. But we are unable to exclude men who arrived between 1972 and 1986. For transparency and simplicity, we do not attempt to adjust the first-stage database to offset this.[13]

B. Outcomes Data and Summary Statistics

Outcomes data are drawn from the family release of the Australian Taxation Office's Longitudinal Information Files ('ALife') (Abhayaratna, Carter, & Johnson, 2022; Fischer et al., 2025). ALife links individuals over time from their 1991 tax returns onward, with the family release providing links to spouses and children. Earlier versions of this dataset have been used in studies of intergenerational income mobility in Australia (Deutscher, 2020; Deutscher & Mazumder, 2020). For this paper, we were also provided with information on the birth cohort and draft eligibility of men subject to the draft.

Our extract consists of a primary population of men born between 1945 and 1952 inclusive (those subject to the draft), and a secondary population of their spouses and children. From the secondary population, we drop the small number of individuals who are the spouse or child of more than one of the primary men (due to re-partnering) or who have more than one relationship type with the primary men (such as a child who later becomes a spouse). This ensures a clearly defined treatment for individuals in the secondary population. From the primary population, we drop individuals with DOBs of 1 January, 1 July or 10 October since these are overrepresented in the administrative data, and make corresponding adjustments to the first-stage data (as in Siminski, 2013). We also drop the associated spouses and children. These exclusions are modest—of the initial population 2.2% are linked to multiple members of the primary sample, 0.2% have multiple relationship types and 1.6% have an excluded primary DOB.

We also drop men (and their spouses and children) who likely migrated well after the draft lottery. Ideally, we would drop all men who migrated to Australia after the year of their draft lottery, but place of birth and year of arrival are not available in the ALife data. However, we can drop more recent migrants. We do this by dropping men who did not appear in Australia's universal healthcare system (Medicare) records until 1986 or later. Medicare was introduced on 1 February 1984 and of those remaining in our sample 67.2% signed up in 1984 and 12.3% in 1985. In 1986 the inflow was 1.3% of the sample, with a modest, steady decline from that point on, consistent with the inflow of migrants. The net effect of all exclusion criteria is to drop a little under 18% of the population.

[13] As we will show in the balance tests in Table 2 our population of first generation men is only around 7% larger due to the inclusion of these migrants. While this will mechanically reduce the true size of the first-stage relationship, there is an offsetting bias we are not able to account for related to differential mortality. Conscripts (and veterans in general) have lower mortality rates than the general population, due to health-related military selection (Wilson, Horsley, & van der Hoek, 2005). By design, this is also the case for their draft-ineligible counterparts. For this reason, compliers account for an increasing share of the population as they age, thereby increasing the size of the first-stage relationship over time. These issues are discussed in more detail in Siminski, 2013.

The ALife data are from a variety of linked administrative data sources. Income tax returns from 1991-2021 capture information on earnings, taxable government payments and total income for those filing tax returns, while third-party reported data from 2002–2021 provide information on earnings and government payments for those who have such income, but do not file tax returns. For outcomes relating to government payments and total incomes we restrict attention to this period from 2002 on to avoid understating incomes. Given the comprehensive nature of the data, those not in the administrative data are assigned zero income, earnings and welfare, provided they did not die in the given year. We define a household-level poverty indicator set at 10% above the (civilian) pension rates for the relevant household (single or couple). Finally, we draw on higher-education loan program (HELP) data from 1989-2021 as a proxy for university attendance. All income variables are in 2021 Australian dollars. For more detail on the key variables and benchmarking against other sources see Appendix B.

Our main analysis draws on the first 12 birth cohorts of the primary sample and the associated secondary sample. Table 1 shows summary statistics for the main estimation samples of 678,966 men, their 651,831 spouses and 1.44 million children. Around one quarter of the men were 'draft-eligible'. The average year of birth is 1948 for men, 1952 for spouses and 1979 for children. Most men had families, with 89% having at least one identifiable spouse, and an average of 2.22 children. Panel B shows additional summary statistics at the person-year level. Employment rates are lower for the first generation, reflecting their age, with around 52% of the men and their spouses employed in a given year. Employment rates are 64% for children. Conversely, welfare receipt is more common in the first generation over our window of observation.

We observe individuals over a long period of time. Figure 2 shows histograms of the distribution of men, spouses and children by year of birth (Panel A) and the ages at which we observe them (Panel B). By construction the men have a defined window of birth cohorts whereas the birth cohorts of spouses and children have a much wider spread. We have good coverage of outcomes for ages from the mid 40s to mid 70s for the men and spouse samples. Most children can be observed from their teens through to their mid 40s. Some children, who were born in later years, are observed from birth onwards.

C. Empirical Strategy and Specification

Our empirical strategy throughout the paper leverages the randomness of Australia's Vietnam-era conscription lotteries. There are, however, many different ways to do this, so we show various versions of the results, accordingly, and justify our preferred approach below.

Figure 3 shows the causal pathways of primary interest; it is not a complete Directed Acyclic Graph with all causal pathways. Related US work (Goodman & Isen, 2020; Deza & Mezza, 2025) has focused on reduced-form effects—the effects of father's 'draft eligibility' (i.e. Z in Figure 3) on children's outcomes (C). The

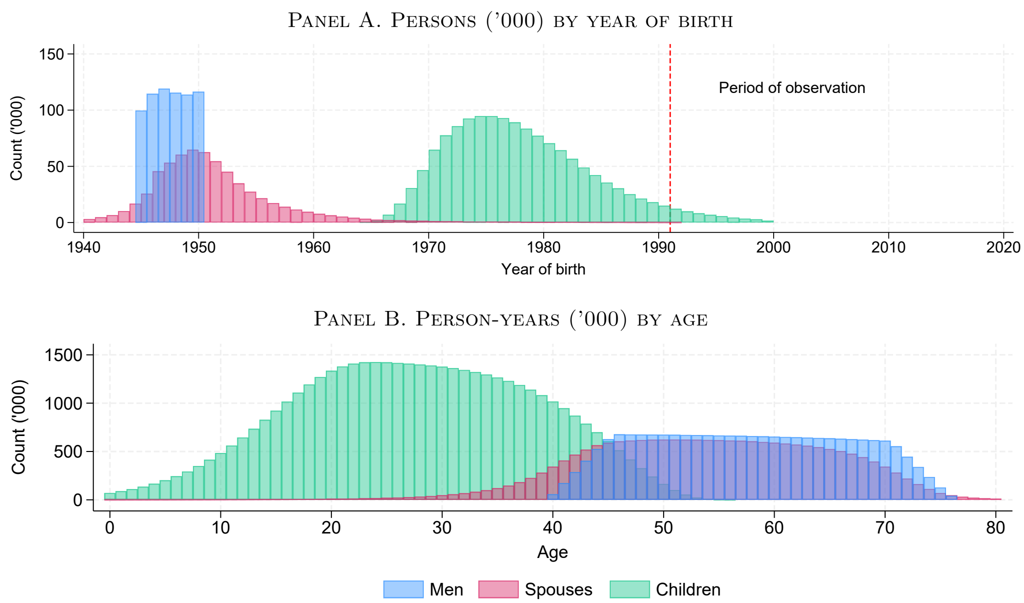
Table 1—Summary statistics for three populations of interest

	Parents				Children	
	Men		Spouses			
	Mean	SD	Mean	SD	Mean	SD
Panel A: Person-level variables						
Draft eligible	0.25	0.43	0.25	0.43	0.25	0.43
Year of birth	1948.1	1.8	1951.7	6.6	1978.5	6.3
Female	0.00	0.00	0.99	0.08	0.49	0.50
Has spouse	0.89	0.31	1.00	0.00	0.77	0.42
# children	2.22	1.47	2.31	1.37	1.60	1.34
Years employed	15.50	9.34	15.81	10.11	19.79	8.08
Years welfare	7.35	6.71	6.70	6.90	3.69	5.38
Income (AUD'000)	968	915	702	611	1,122	816
Earnings (AUD'000)	1,044	966	676	663	1,097	856
Welfare (AUD'000)	120	161	95	117	39	81
N	678,966		651,831		1,441,181	
Panel B: Person-year level variables						
Age	57.6	9.1	54.0	11.0	27.6	10.8
Employed	0.52	0.50	0.52	0.50	0.64	0.48
Receiving welfare	0.39	0.49	0.34	0.47	0.18	0.39
Protective services	0.02	0.15	0.00	0.06	0.03	0.16
At university	0.00	0.05	0.01	0.08	0.06	0.23
Income (AUD'000)	49.9	58.4	35.5	38.1	56.2	53.1
Earnings (AUD'000)	34.7	46.3	22.1	29.4	35.6	44.6
Welfare (AUD'000)	6.3	10.2	4.9	8.2	2.0	5.5
N	20,199,417		19,727,537		44,283,373	

Note: Displays summary statistics (mean, standard deviation) for the three analysis samples. Note the person-level income variables are totals over different time periods: 1991-2021 for earnings; 2002-2021 for income and welfare.

primary rationale for this is that draft eligibility may affect children not only by inducing military service (R) for some of their fathers, but potentially also due to draft avoidance behavior of other fathers. This is reasonable, as there is evidence of various avoidance behaviors, including 'dodging up' through education (Card & Lemieux, 2001), 'dodging down' by committing crimes (Kuziemko, 2010), and by having children (earlier) (Bitler & Schmidt, 2012; Bailey & Chyn, 2020), all of which could affect children's outcomes. For Figure 3, this would imply causal pathways from Z to C which do not all pass through R. But the Australian context is different. There are institutional reasons for expecting less draft avoidance (Siminski, 2013: Appendix). There is no meaningful effect of draft eligibility on education (Siminski & Ville, 2012: Section VII), or on crime (Siminski et al.,

FIGURE 2. DISTRIBUTIONS OF YEAR OF BIRTH AND AGE FOR THREE POPULATIONS OF INTEREST



Note: Shows the distribution of the main analysis samples by year of birth and age. The analysis samples focus on men in the first twelve birth cohorts (born in the 1945-1950 calendar years), their spouses and their children.

2016), and men with children were not exempt. It is also difficult to interpret the magnitude of reduced-form draft-eligibility effects.

For the reasons stated above, our preferred results are from instrumental variable models. The simplest approach would be to use Z as an instrument for R. However, as we show in Appendix A, almost all effects of service are confined to those who were deployed to Vietnam (V). Most of the results we will show in the paper hence treat Z as an instrument for V, using only the first 12 birth cohorts (since less than 1% of conscripts from the last four cohorts were deployed). We outline this procedure below in detail. Appendix A describes the test for direct effects of R, and shows corresponding results.

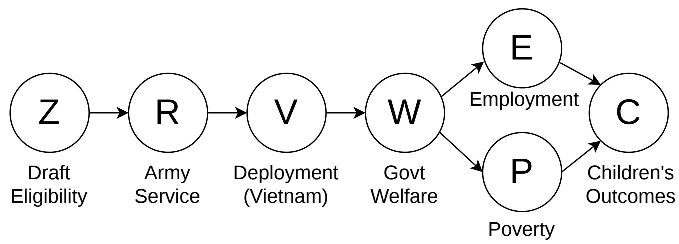
Our estimator is Two-Sample 2SLS regression (Inoue & Solon, 2010), similar in spirit to Angrist (1990) and much subsequent draft-lottery work. As mentioned above, our preferred specification positions deployed service as the treatment variable. It is an over-identified model, where the effect of draft eligibility is allowed to vary between birth-cohorts, resulting in a vector of 12 binary instruments. The first stage equation is hence:

$$V_{ij} = \pi_j Z_{ij} + \gamma_j + \varepsilon_{ij} \tag{2}$$

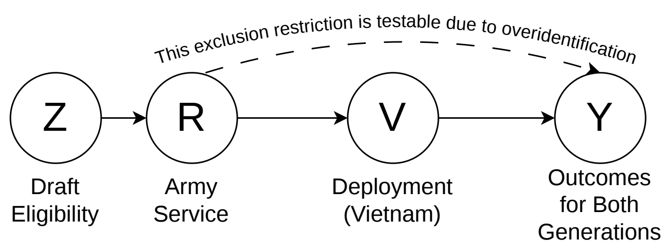
where V_{ij} is deployed service (in Vietnam) for person i in (6-month birth) cohort

Figure 3. Causal Graphs

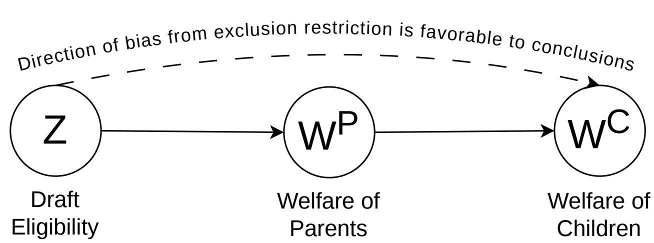
Panel A. Main causal pathways of interest



Panel B. Draft eligibility as instrument for deployed service



Panel C. Draft eligibility as instrument for parental welfare



j, Z_{ij} is the binary draft-lottery outcome for this person, π_j is the cohort-specific first-stage effect, γ_j represents cohort fixed effects and ε_{ij} is the residual. The corresponding baseline structural equation is:

$$Y_{ij} = \beta V_{ij} + \alpha_j + u_{ij} \tag{3}$$

where Y_{ij} represents some outcome variable (such as number of years of employment), and β represents the coefficient of interest. The second stage regression, which is estimated using a separate data set, is hence:

$$Y_{ij} = \beta \hat{V}_{ij} + \alpha_j + \xi_{ij} \tag{4}$$

where $\hat{V}_{ij}$ is the predicted value from the first-stage regression.[14]

We also use a dynamic specification, which more fully exploits the 31 years of outcomes data. This specification is fully interacted with age (a) at the time the outcome is observed, and is hence equivalent to running equation (4) separately for Y measured at each individual year of age:

$$Y_{aij} = \beta_a \hat{V}_{ij} + \alpha_{aj} + \xi_{aij} \tag{5}$$

As mentioned, the i subscript above indexes persons. For some of the analysis, this indexes the men who were subject to the lotteries. But we also show corresponding results for their spouses and for their children. For those analyses, i indexes persons from the relevant populations of spouses, or children. For those analyses, we use the same first-stage fitted values as we do for the men. Ideally, we would re-estimate the first-stage regression accordingly, weighted by whether they had a spouse, or by the number of their children. In practice, we do not have that information in the first-stage database. However, there is no obvious reason to suspect that first-stage effects differ for men with different numbers of children, or by whether they subsequently partnered, and so any resulting bias is likely to be small. Throughout the analysis, we calculate robust standard errors, clustered on (first-generation) date of birth.[15]

D. Tests of Sample Selection and Attrition

Table 2 shows results from a series of tests for sample selection and attrition. All of these are reduced-form effects of draft eligibility, for the first 12 birth cohorts of the primary sample. Except for the first row, all estimates use individual-level data through regressions of the form: $Y_{ij} = \pi Z_{ij} + \gamma_j + \varepsilon_{ij}$.

Panel A shows estimated effects of draft eligibility on primary sample selection. We first consider whether draft eligibility (Z) affects the likelihood of appearing

[14] The first-stage fitted values are available from the Siminski (2013) replication files: `https://drive.google.com/file/d/1jeJASPgekJvySRWbZ7TIlGY93SvJKH8g`.

[15] Because service probabilities are observed in population-level data rather than estimated from a sample, there is no need to adjust the second-stage standard errors for first-stage estimation uncertainty.

Table 2—Balance tests

	Coef.	s.e.	Mean for draft-ineligible men	N
Panel A: Coverage and Attrition				
Ratio of observations	0.001	(0.003)	1.07	24
Alive in 2021	-0.000	(0.001)	0.876	678,966
Panel B: Spouses				
Has spouse	0.002**	(0.001)	0.891	678,966
# spouses	0.001	(0.002)	1.058	678,966
Spouse year of birth	-0.020	(0.016)	1952	604,727
Spouse female	0.000	(0.000)	0.996	605,145
Panel C: Children				
Has children	0.002*	(0.001)	0.839	678,966
# children	-0.001	(0.004)	2.226	678,966
Child year of birth	-0.038**	(0.017)	1979	569,635
Child female	-0.000	(0.001)	0.487	569,634

Note: Each row shows the estimated effect of draft eligibility from separate regressions with a particular dependent variable. For the first row only (labeled 'Ratio of observations'), the data are collapsed to cohort-by-draft-eligibility level, for a total of 24 observations. The dependent variable is the number of men in the ALife data divided by the corresponding number of men in the first-stage data, and the regression is weighted by the number of men in each cell. For all other rows, the regressions are run on the primary sample of men. Differences in sample sizes reflect missing values. The sample in each row is restricted to the first 12 birth cohorts. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

in the ALife data. For this, we collapse the data to cohort-by-draft-eligibility level, for a total of 24 observations. Here, the dependent variable (Y_{jz}) is the number of men in the ALife data divided by the corresponding number in the first-stage data. This regression is $Y_{jz} = \pi Z + \gamma_j + \varepsilon_{jz}$, and is weighted by the number of men in each cell in the first-stage data. The mean suggests the number of men in ALife is 7% higher than in the first-stage data, on average, consistent with modest migration into the population. But this does not differ by draft eligibility, since the estimate of π is small (0.1 percentage points) and not statistically significant. There is certainly no evidence of 'missing' draft-eligible men, as the point estimate is positive. Turning to the second row, ALife data suggest that 12.4% of men in these cohorts had died by 2021. But there is no significant effect of draft eligibility on mortality. This is consistent with earlier work with mortality records over 1994-2007 (Siminski & Ville, 2011) and 1994-2011 (Johnston et al., 2016).

The potential effects of draft eligibility on partnering and fertility are also important, given we will also estimate effects of service for spouses and children. These are shown in Panels B and C. There is a significant positive effect on the probability of having a spouse at some point between 1991 and 2021, but the effect is small, at 0.2 percentage points. The results also suggest no effect on the number of spouses observed during the 31-year data window. Potential effects on the characteristics of spouses are also important. The ALife data are limited in their ability to explore this. However, we find no effect on spouse birth year or sex. Earlier work suggests no effect of draft eligibility on spouse education for this same population (Atalay et al., 2019: footnote 15).

Fertility decisions could feasibly be affected by draft eligibility through a number of mechanisms. But the data reveal only small effects on veterans' fertility (Panel C). There is a small, marginally significant effect (0.2 percentage points) of draft eligibility on the probability of having at least one child, but no significant effect on the number of children born to each man. There is no effect on child sex, which is unsurprising. But the estimated effect on child year of birth is statistically significant at the 5% level. The point estimate implies that children of draft-eligible men were born around 2 weeks earlier on average. This seems trivial, but we nevertheless control for child's year of birth (as a quadratic), when we estimate effects on children's outcomes in person-level models.[16] Doing so is also helpful for precision. The children's year of birth distribution (see Figure 2) is very wide, generating much residual variation for many outcomes, if not controlled for.

III. Results for First Generation

The most dramatic first-generation effects of Australians' Vietnam-era service are welfare receipt and disemployment. Siminski (2013) argues that work disincentives in the veterans' compensation system drive these effects, making the Australian context fundamentally different from the corresponding US experience. We reaffirm this result in new ways, drawing on much richer data than has been used before for this setting.

A. Effects at Each Age

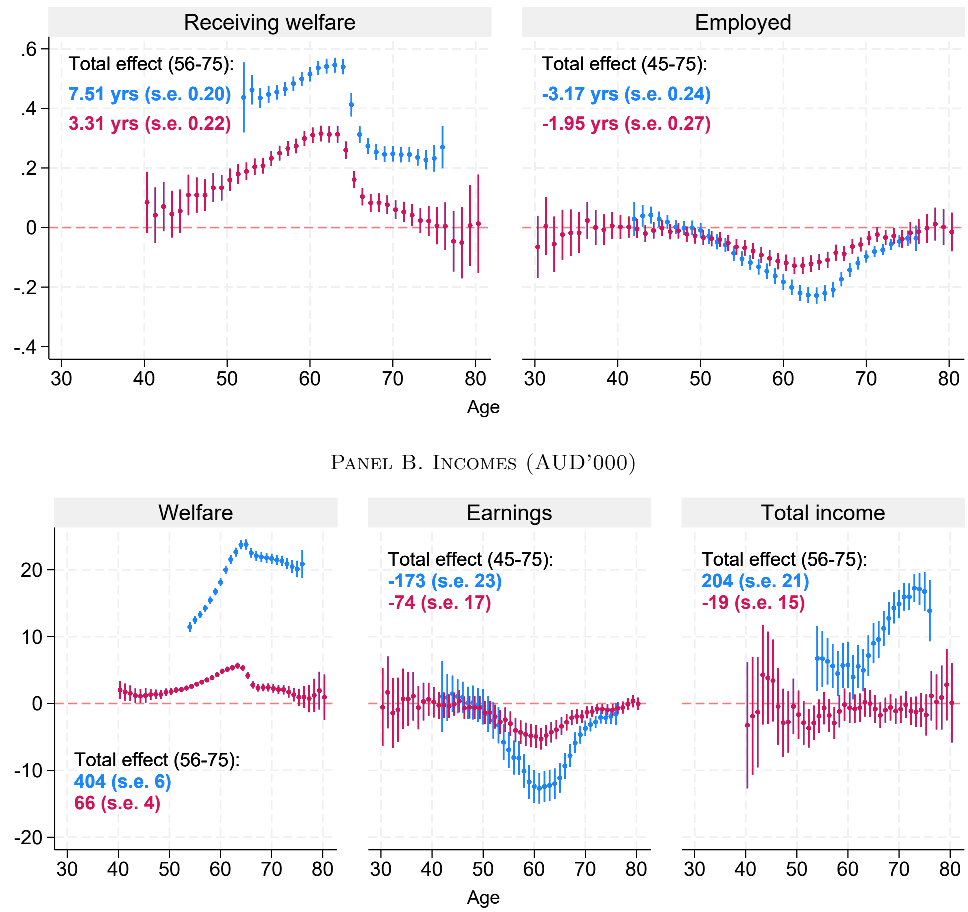
Figure 4 shows estimated effects of deployed service at each year of age. Results are shown for conscripts (blue) and their spouses (red), for the binary outcomes of welfare receipt and employment (Panel A), and for the dollar value of welfare, earnings and income received in a given year (Panel B).

Panel A shows pronounced effects on the probability of receiving welfare. For conscripts these are over 40 percentage points from when we start observing outcomes in their early 50s. The effects rise to 55 percentage points by age 64, before falling to a little over 20 percentage points from their late 60s onwards. Effects for their spouses start near zero but are already 10 percentage points by their

[16]For computational ease we do not control for child year of birth when estimating our dynamic specification (5). This specification is already fully interacted with age, so these controls would instead serve to control for year of observation effects. Including these controls does not meaningfully change our results.

Figure 4. Effect of Vietnam service on outcomes of conscripts and their spouses by age

Panel A. Probability of outcomes



Note: Shows the age-specific coefficients and 95% confidence intervals on Vietnam service from equation (5) for a variety of outcomes for conscripts (blue) and their spouses (red). Regressions for conscripts (spouses) are estimated for all ages from 40 (30) to 76 (80) for employment and earnings, and from 50 (40) for other outcomes due to data limitations discussed in Section II. We also show the "total effect" over particular age ranges, by summing the age-specific estimates (we sum over 31/20 years for comparability with appendix results where we sum over our 31/20 years of observation). For readability we do not show the coefficients for conscript outcomes at the youngest ages (40-41 or 50-51); these coefficients are identified only from the youngest cohort, which had very low probability of Vietnam service, and are hence imprecisely estimated.

mid 40s rising to peak at around 30 percentage points by age 64. Discontinuities at age 65 reflect the civilian age-pension eligibility age, which was 65 for all the men in our sample and 65-67 for most of their spouses.[17]

[17]The female eligibility age for the civilian age pension was gradually increased from 60 (for those born before July 1935) to 67 (for those born from 1957 onwards). For (deployed) veterans' partners, the

Increased welfare receipt is almost entirely driven by disability-related payments, a fact we will return to when examining the second generation. Appendix Figure A2 illustrates this, showing that the welfare incomes of conscripts and their spouses mirror their income from disability-related payments. Disability payments for veterans had several notable features that would be expected to drive increased uptake relative to the civilian population (see Siminski & Ville, 2012; Atalay et al., 2019). First, claims had to be accepted unless it could be proven beyond reasonable doubt that the injury or disease was not caused by military service. Second, the payment rates were more generous than civilian disability pensions. And third, spouses of men receiving these pensions were also automatically eligible for a pension regardless of their age.

We also find sustained disemployment effects for conscripts and their spouses from their early 50s to their mid 70s, most of which are highly significant. They are largest in their early 60s—up to 23 percentage points for conscripts and 13 percentage points for their spouses, again before civilian age pension eligibility begins for the draft ineligible group. Disemployment effects are also unsurprising given the benefit design, as the highest payment rates required that individuals work fewer than eight hours per week. There is also a suggestion of positive employment effects at younger ages for conscripts (early 40s), but these are relatively small, and not highly significant.

Finally, Panel B shows corresponding effects on welfare, earnings, and total income. The signs of the welfare and earnings effects are as expected and generally follow the same patterns as for employment and welfare receipt. One notable difference is the much steeper slope in the welfare payment effect in Panel B, compared to the binary welfare receipt effect in Panel A. This is because most veterans who eventually received the highest (special rate) of disability compensation first received lower rates. On average, the increased welfare payments more than offset reduced earnings for conscripts and merely offset them for spouses. Average effects on total income are positive at each age, and largest at older ages for conscripts, but around zero for their spouses.

The total effects of service can be summarized in various ways. One way is to sum the estimated coefficients across ages. These are shown in each panel of Figure 4. For conscripts, they suggest that deployed service increased welfare receipt by 7.51 years (100.5% of the mean for the draft ineligible) across ages 56-75, and decreased employment by 3.17 years (30.2% of the mean) across ages 45-75. For spouses the effects are 3.31 more years of welfare receipt (a 44.8% increase) and 1.95 fewer years of employment (an 18% decrease). In Appendix Tables A2 and A3, we show further results for conscripts and spouses using alternate

eligibility age for an equivalent age pension is lower, but has also increased, from 55 (for those born before July 1935), to 60 (for those born from 1949 onwards). The spouse population in our data spans almost all of these cohorts. Veterans' spouses were hence eligible for a retirement pension between 5 and 7 years earlier as a result of their husband's service. In our sample, the majority of 'treated' spouses became eligible at age 60, between 5 and 6 years earlier than their untreated counterparts. Other pensions for veterans' spouses were also available at younger ages (see Atalay et al. (2019)).

specifications where we instead sum outcomes over the window of observation before running the regressions. These yield similar conclusions to those discussed here.

IV. Intergenerational Effects During Childhood and Adulthood

A. Children's Exposure to Parental Impacts

Military service greatly increased welfare receipt by veterans and their spouses, which in turn reduced their employment but raised veterans' incomes. How this affected their children may depend on their children's age at exposure and its duration, a point also emphasized in the new literature on neighborhood effects, beginning with Chetty, Hendren, and Katz (2016). To better understand childhood exposure we switch our estimating equation to:

$$Y_{aij}^{Parent} = \beta_a \hat{V}_{ij} + \alpha_{aj} + \xi_{aij}. \tag{6}$$

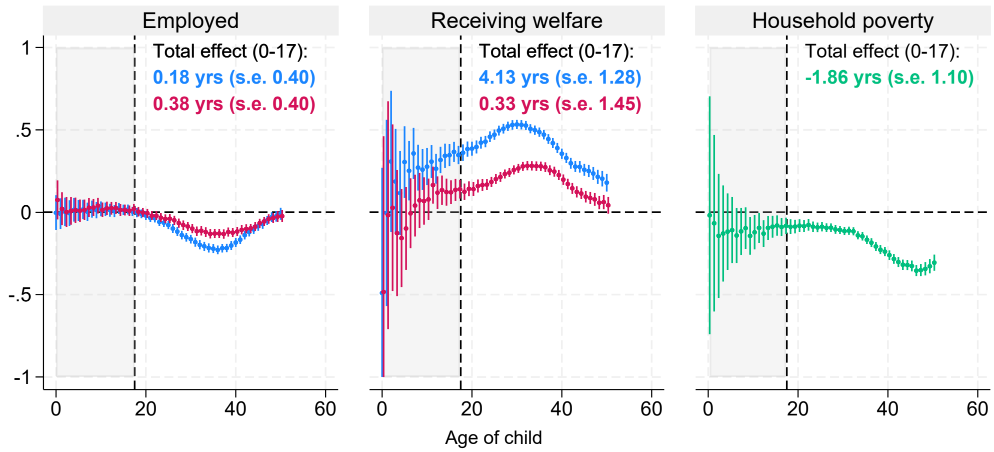
This is similar to equation (5). Here, however, the dependent variable Y_{aij}^{Parent} is a parental outcome for child i, observed at child age a. The key parameter is β_a—the effect of deployed service on parent outcomes at each child age. Outcomes include each parent's employment and welfare receipt, and household poverty. Figure 5 summarizes these dynamic exposures from ages 0-50.[18]

The largest and clearest effects are on the probability of welfare receipt. Vietnam service increased the exposure of veterans' children to welfare during their childhood: their fathers spent 4.13 (s.e. 1.28) more years on welfare during this time. The estimates are more precise in later years; during the teenage years both fathers and spouses are more likely to be welfare recipients, with fathers 35 percentage points more likely to be receiving welfare and spouses 13 percentage points more likely. Parental employment is also significantly affected, but only for adult children. The treatment during childhood is thus a purer 'exposure to welfare' treatment rather than also including any direct effects of having parents not in work.

Finally, reflecting the generosity of veterans' payments, deployed service led to veterans' children spending 1.86 (s.e. 1.10) fewer years in poverty while growing up. This beneficial consequence of exposure to welfare may serve to improve later life child outcomes. As discussed in Aizer, Hoynes, and Lleras-Muney (2022), there has been an increasing move to a more holistic approach in the empirical literature—aided by improving data—to consider the long-run intended benefits of welfare rather than focusing only on the negative incentive effects. That said, the largest effects on parental household poverty occur much later in the lives of their children, at a point when parents will have largely left the workforce and

[18]Estimates at younger ages are identified from children born to older fathers. This also makes estimates at these ages less precise, particularly for welfare and household poverty, for which our data begin in 2002.

Figure 5. Effect of Vietnam service on parent outcomes—conscripts, their spouses and combined households—by child age



Note: Shows the child-age-specific coefficients and 95% confidence intervals on Vietnam service from equation (6) for a variety of parent-level outcomes. We also show the "total exposure effect" over childhood, by summing the age-specific estimates. To aid comparison, the scale is the same across the subplots. Shading indicates childhood, ending in the vertical line at age 18.

the relative levels of veterans' and civilian welfare payments drive the results.

The reduced-form balance tests in Table 2 already suggest there was little effect on the wider family environment in terms of marital stability or family size. In Appendix Figure A3 we show this in a dynamic setting. From age 0 to 50 the effect of Vietnam service on the probability of either parent reporting a separation is a precise null. There may of course be unobserved differences in the family environment, such as the effects of worse mental health of fathers, as found in Johnston et al. (2016).

B. Intergenerational Effects on Employment, Welfare and Income

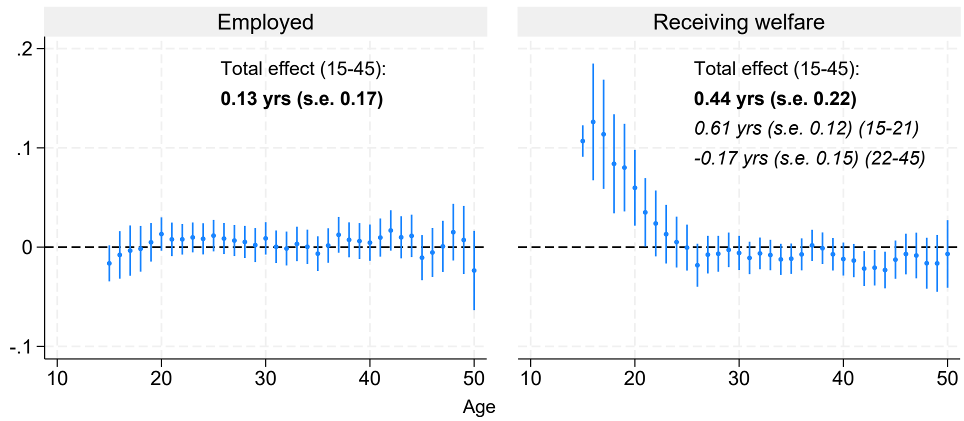
We now turn to child outcomes and explore the intergenerational effects of father's deployed service and subsequent exposure to welfare receipt. Estimated dynamic effects on child outcomes are shown in Figure 6. Following the presentation of earlier results, we focus first on binary outcomes (Panel A) before turning to incomes (Panel B).

There is only one clear effect on children's outcomes—they were more likely to receive welfare themselves from ages 15 to 21 (Panels A and B). Veterans' children spent 0.61 (s.e. 0.12) more years on welfare and received AUD4,340 (s.e. 830) more income from welfare over this age range as a result of their father's service. Total income also appears slightly higher in the late teens and early 20s.

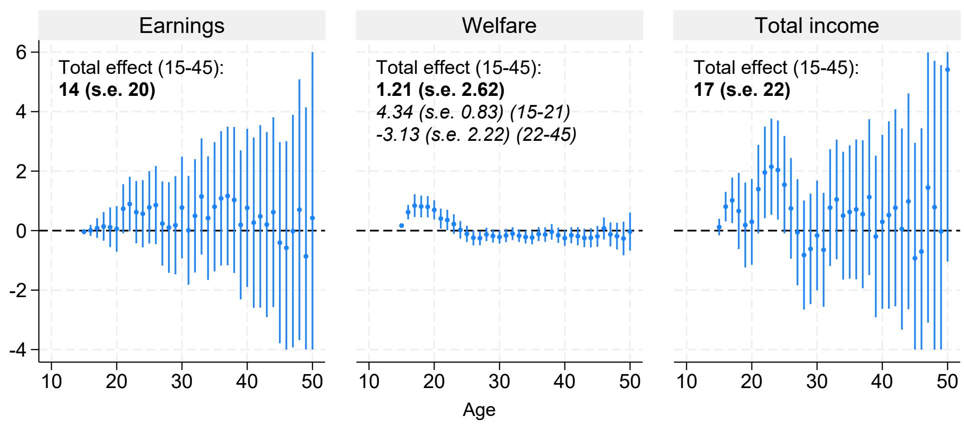
The welfare effects for children are dramatically smaller and in different welfare payment types than for parents. In Table 3 we show the total welfare effects in

FIGURE 6. EFFECT OF VIETNAM SERVICE ON CHILDREN BY AGE

PANEL A. PROBABILITY OF OUTCOMES



PANEL B. INCOMES (AUD'000)



Note: Shows the age-specific coefficients and 95% confidence intervals on Vietnam service from equation (5) for a variety of outcomes. We also show the "total effect" over particular age ranges by summing the age-specific estimates.

terms of welfare received from 2002-2021 (fixing the time period rather than the age span). Conscripts and their spouses received an additional AUD396,000 and AUD64,000 in welfare respectively over this time period, increases of 352% and 69% of the respective means. These were almost entirely driven by disability-related payments, with small offsetting decreases in all other payment types.[19] For children, there is no meaningful or statistically significant increase in disability

[19]We suspect the increase in 'miscellaneous income support' for conscripts' spouses reflects payments for veterans' widows, which appear in this category. This would also be consistent with the fact that the effect for these payments steadily increases with age.

payments. Indeed, we can rule out at a 95% confidence level increases of more than AUD595 (less than 6% of the mean). The same is true for unemployment benefits which might have been expected to increase had parental welfare receipt and non-employment influenced child preferences for leisure. Instead the sole positive effect is driven by payments relating to studying.

The positive effects at young ages for study-related welfare payments are likely explained mechanically by institutional details of welfare eligibility. The children of veterans who had died or were disabled were eligible to receive support while studying through the Veterans' Children Education Scheme. From age 16 this payment could be paid directly to the child; from the point of tertiary study this was a requirement. Therefore the effects shown indicate active engagement in the welfare system by children rather than merely payments being made in respect of them.

Despite this early engagement with and exposure to the welfare system, from their mid 20s through to age 50 our effects are precise zeros (Figure 6). This is reaffirmed in Appendix Table A4, which shows further results for the children over the full time period of observation. None of these estimates are statistically significant. Given the high statistical power we wield and the numerous hypothesized mechanisms for intergenerational transmission, these 'precise zeros' are a striking set of results. To illustrate their precision, consider the 95% confidence intervals for key variables: the total effects in Figure 6 rule out disemployment effects from age 15-45 any larger than 0.16 years, 0.7% of the mean. They also rule out positive effects on welfare receipt from ages 22-45 any larger than 0.09 years, or 1.5% of the mean. Similarly, the estimated effects for each income category are precise zeros.

Table 3—Effects of Vietnam service on welfare received (AUD'000) by payment type

	Total welfare payments	Welfare payments relating to...						
		Age	Disability	Unemploy-ment	Care	Study	Misc. income support	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Conscripts								
Vietnam	396.259***	-14.721***	412.452***	-4.026***	-1.859***	-0.009	0.155	4.268***
	(6.222)	(2.378)	(5.884)	(0.488)	(0.717)	(0.040)	(0.233)	(0.253)
Mean	112.472	66.200	35.244	5.718	3.502	0.068	0.810	0.930
N	593,713	593,713	593,713	593,713	593,713	593,713	593,713	593,713
Panel B: Spouses of conscripts								
Vietnam	63.907***	-7.025***	59.949***	-3.145***	-3.047***	0.085	15.131***	1.959***
	(3.534)	(2.204)	(2.084)	(0.511)	(1.008)	(0.074)	(1.303)	(0.142)
Mean	93.237	46.881	20.363	5.997	7.729	0.195	11.493	0.578
N	611,400	611,400	611,400	611,400	611,400	611,400	611,400	611,400
Panel C: Children of conscripts								
Vietnam	-1.619	0.051	-1.160	-0.995	0.032	1.011***	-0.583	0.024
	(1.779)	(0.060)	(1.067)	(0.674)	(0.339)	(0.204)	(0.730)	(0.089)
Mean	39.341	0.061	10.240	10.972	2.077	4.324	11.270	0.397
N	1,425,832	1,425,832	1,425,832	1,425,832	1,425,832	1,425,832	1,425,832	1,425,832

Note: Displays coefficients and standard errors from regressions of the stated individual outcomes on the probability of Vietnam service for the birth cohort and ballot outcome of the individual (Panel A) or their lottery-eligible spouse (Panel B) or father (Panel C). All specifications include cohort fixed effects with robust standard errors clustered at the level of the lottery-eligible individual, spouse or father's date of birth. Panel C also includes a quadratic in age. Mean outcomes for those with balloted-out individuals, spouses or fathers are also shown. Welfare payments are summed over 2002-2021. The population is restricted to those who did not die in or before 2021. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C. Human Capital, Family Dynamics and Occupation

Related US literature has documented detrimental effects for veterans' children on various outcomes broadly related to human-capital investment. Our data are limited in their ability to examine this in detail. However, the evidence we do have suggests no such pattern for Australia. The results from the section above are already suggestive of this, showing no effects on welfare beyond age 21, employment or earnings. Table 4 shows results for other relevant outcomes (columns (1)-(3)). It suggests no effect on ever having attended university, ever having had a spouse, or the number of children by 2021. This is true for all children (Panel A), and also when we split by gender (Panels B and C).

Table 4—Effects of Vietnam service on conscript's children

	Human capital and family dynamics			Occupation (years)	
	University	Has spouse	# children	Military	Other protective services
	(1)	(2)	(3)	(4)	(5)
Panel A: All children					
Vietnam	0.005	0.002	-0.022	0.026	0.065
	(0.011)	(0.007)	(0.026)	(0.021)	(0.042)
Mean	0.404	0.776	1.594	0.098	0.283
N	1,425,832	1,425,832	1,354,280	1,362,174	1,362,174
Panel B: Female children					
Vietnam	0.006	-0.002	-0.024	0.019	-0.019
	(0.015)	(0.010)	(0.035)	(0.017)	(0.040)
Mean	0.477	0.805	1.671	0.031	0.137
N	696,419	696,419	664,355	666,148	666,148
Panel C: Male children					
Vietnam	0.005	0.008	-0.017	0.031	0.146**
	(0.014)	(0.011)	(0.035)	(0.038)	(0.070)
Mean	0.335	0.748	1.519	0.163	0.422
N	729,413	729,413	689,925	696,026	696,026

Note: Displays coefficients and standard errors from regressions of the stated individual outcomes on the probability of Vietnam service for the birth cohort and ballot outcome of the individual's lottery-eligible father. The specification includes cohort fixed effects and a quadratic in year of birth, with robust standard errors clustered at the level of the lottery-eligible father's date of birth. Mean outcomes for those whose father was balloted-out are also shown. Outcomes are any university attendance over 1991-2021, any spouse over this period, the number of children and years spent in either a military occupation or protective services excluding the military. The population is restricted to those who did not die in or before 2021. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In the US, children of draft-eligible men are more likely to serve in the military themselves (Johnson et al., 2018; Goodman & Isen, 2020). The Australian occupational classifications allow us to examine not only military service but also spillovers into other protective services such as policing, firefighting and security. This is particularly relevant in the Australian context where the armed services are around half the size of those in the United States as a proportion of the labor force.[20] Columns (4)-(5) of Table 4 show the effects of Vietnam service on the years spent in these occupation groupings.

For Australian children we see small positive but statistically insignificant effects on military service coupled with a clearer effect for protective services. Male children experience larger effects, and the only statistically significant ones. Male children of conscripts have 0.031 (s.e. 0.038) more years of military service—a 20% increase relative to the control-group mean. While imprecise and statistically insignificant, this effect size is quite similar to that in Goodman and Isen (2020), who find a father's military service increases his son's military service by about 25%. A clearer effect is seen in other protective services, where sons spend 0.146 (s.e. 0.070) more years—an increase of 35%. This is nonetheless a modest effect when considering aggregate labor market outcomes given we observe occupations over a 23-year window.

These results are consistent with causal occupation transmission arising from preference and skill formation, but paint a nuanced picture of the extent and limits of such transmission. Effects are clearest and largest for sons, consistent with stronger transmission from parents to same-sex children. However, the effects appear not to be confined to the narrow occupation grouping of the treatment, with spillovers to the broader protective services occupation group. This is consistent with other recent findings pointing to the potential for such spillovers over and above direct transmission: Andrews, Hill, Price, and Wilson (2025) show having a close childhood neighbor in an occupation increases the likelihood of being in the same occupation but also in occupations with similar education requirements.[21]

V. Intergenerational Effects of Welfare Receipt

Despite major effects on parental employment, welfare receipt, and poverty, we have found no meaningful effects on children's aggregate outcomes beyond modest occupational transmission. In this section, we reframe these results as a test of the (causal) intergenerational transmission of welfare receipt. As per Figure 3 Panel C, the structural equation we have in mind here is:

$$W_{ij}^{Child} = \beta W_{ij}^{Parents} + \alpha_j + u_{ij} \tag{7}$$

[20] World Bank indicators put armed forces personnel as a percentage of the total labor force at 0.4% for Australia and 0.8% for the United States in 2020, and 0.8% and 1.7% respectively in 1990 (World Bank, 2026).

[21] Given the focus of our paper we have not shown children's exposure to parental occupation. This is partly obscured by the parent disemployment effects, which mean conscripts spent 2.873 fewer years with an occupation over our window. They are nonetheless more likely to report a military occupation, with 0.026 (s.e. 0.010) more years in military service, twice the mean for the control group.

where W_{ij}^{Child} is years of welfare receipt for a child i with father in birth cohort j, and $W_{ij}^{Parents}$ is a measure of the same outcome for their parents (summed across them).[22] If we treat this as a purely statistical relationship and estimate directly via OLS we can estimate a standard non-causal intergenerational association in welfare. However in our setting we will go further and instrument for $W_{ij}^{Parents}$ using draft eligibility:

$$W_{ij}^{Parents} = \pi_j Z_{ij} + \gamma_j + \varepsilon_{ij} \tag{8}$$

This is the same approach used in earlier sections, but with $W_{ij}^{Parents}$ as the endogenous variable instead of V. To aid interpretation, we will express the results as implied elasticities ($\hat{\eta}$), defined as:

$$\hat{\eta} = \hat{\beta}\frac{\bar{W}^{\text{Parents}}}{\bar{W}^{\text{Child}}}, \tag{9}$$

with $\hat{\beta}$ as per equation (7) and $\bar{W}$ the means for the estimation sample. We will explore both years in receipt of welfare and also years reliant on welfare (defined as years receiving welfare while also not employed).

The IV analysis invokes a strong exclusion restriction; it assumes that parental welfare receipt is the only mechanism by which draft eligibility affects children's welfare receipt. This is not a fully realistic assumption, but it is a conservative assumption. In particular, we know that deployed service had detrimental effects on mental health (Johnston et al., 2016), which has the potential to affect children detrimentally. We believe this is a useful exercise for two key reasons. First, the effects on welfare (and downstream effects on employment and poverty) are by far the largest identified effects of Australian Vietnam-era service for conscripted men and their spouses. Second, the direction of bias from violations of this assumption is likely favorable to our main conclusions. If these other mechanisms (e.g. father's mental health) affect children detrimentally, then our estimates are biased upward, and should be treated as an upper bound. However, as we will show, they are already precise zeros, thereby strengthening the conclusion of no intergenerational transmission of welfare dependency.

This exercise reveals strong intergenerational associations in welfare without any corresponding causal transmission. Table 5 shows the results, with welfare receipt in the first two columns and welfare reliance in the next two columns. (Appendix Table A5 shows the corresponding coefficient estimates). We also show the F-statistic on the excluded instruments.

For Panel A, the treatment variable is parents' years of welfare receipt or reliance across 2002-2021. Similarly, the outcome variable is child's years of welfare receipt or reliance over the same period. Column (1) reveals very strong intergenerational association. These OLS results show an estimated elasticity of 0.407.

[22] We restrict to those with both a father and another identifiable parent.

This implies that parents spending 1% more years on welfare is associated with a 0.4% increase in child years on welfare. But the IV results (Column 2) suggest this is not causal, at least not in our context. The IV estimate in Column (2) is close to zero. This is despite the eligibility features discussed above which should mechanically increase intergenerational welfare persistence. This estimate is not statistically significant, and is precisely estimated. The results for welfare reliance are similar to the welfare receipt results.

Table 5—Implied elasticities in outcomes between and within generations

	Outcome			
	Welfare receipt		Welfare reliance	
	OLS	IV	OLS	IV
Panel A: Intergenerational – Own years of outcome				
Parent years	0.407***	0.014	0.508***	0.006
	(0.003)	(0.042)	(0.005)	(0.070)
N	1,143,861	1,143,861	1,143,861	1,143,861
F-statistic on IVs		68.959		53.123
Panel B: Intergenerational – Own years of outcome after age 21				
Parent years	0.375***	-0.017	0.472***	-0.018
	(0.003)	(0.046)	(0.004)	(0.074)
N	1,143,861	1,143,861	1,143,861	1,143,861
F-statistic on IVs		68.959		53.123
Panel C: Intergenerational – Own outcome in year T				
Parent years to T-1	0.276***	0.016	0.337***	-0.001
	(0.002)	(0.023)	(0.003)	(0.045)
N	25,590,577	25,590,577	25,590,577	25,590,577
F-statistic on IVs		90.525		54.478
Panel D: Intragenerational – Own years of outcome after age 21				
Own years to 21	0.521***	0.194	0.682***	0.415*
	(0.003)	(0.177)	(0.005)	(0.219)
N	160,543	160,543	160,543	160,543
F-statistic on IVs		2.024		1.540
Panel E: Intragenerational – Own years of outcome after age 21 (just identified)				
Own years to 21	0.521***	0.125	0.682***	0.093
	(0.003)	(0.224)	(0.005)	(0.350)
N	160,543	160,543	160,543	160,543
F-statistic on IV		16.097		8.982

Note: Displays scaled coefficients and standard errors from regressions of the stated individual outcomes for the child generation on analogous parent outcomes (Panels A and B), earlier parent outcomes (Panel C) or earlier child outcomes (Panels D and E). Specifically: Panel A regresses child years in welfare receipt or welfare reliant over 2002-2021 on parent years in the same state over the same period; Panel B is the same but only summing child outcomes over the years in which they are strictly over age 21; Panel C switches attention to person-years and regresses child outcome in year T on parent rate of receipt (years in the same state up to that year over years observed); Panels D and E are the same as Panel B but the key independent variable is now child rate of receipt in the same state up to and including age 21 and the key dependent variable is the same rate beyond age 21. In Panels D and E we restrict to children observed for all ages from 15-21 inclusive. All regressions include a quadratic in year of birth and father-cohort fixed effects, with robust standard errors clustered at the level of the father's date of birth. In the instrumental variables specification we instrument for the key independent variable with the father's cohort interacted with the ballot outcome (Panels A-D) or simply father's ballot outcome (Panel E). The population in Panels A and B is restricted to those who did not die, or have their parents die, in or before 2021. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

To avoid the mechanical welfare eligibility transmission channel, we repeat the analysis with child welfare receipt restricted to ages 22 and over (Panel B). Here, the OLS estimates are similar to Panel A. The IV estimates are lower, but are again close to zero and are not statistically significant. Are these 'precise zeros'? The upper bound of a one-sided 95% confidence interval in Column (2) is 0.059. That is, we can rule out a 10% increase in parental welfare receipt increasing child welfare receipt by any more than 0.59%. This contrasts with strong transmission in other settings. In Dahl et al. (2014), having a parent receive disability insurance after having initially been denied it leads to their children increasing their receipt by 6 percentage points over the following 5 years, relative to a mean of 3%. The welfare reliance results are again similar, but with slightly larger standard errors.

A potential issue for the approach used so far is that child outcomes are measured over the same period as parental outcomes (2002-2021). This is not a problem if one is willing to assume that future parental outcomes are anticipated (by the parent and/or the child) and can hence already affect child behavior. An alternative perspective is to assume that only realized parental behaviors affect child behavior. Panel C shows results that make full use of the available data under this assumption. For this version, equation (7) is replaced with a model estimated with child-year data:

$$W_{ijt}^{Child} = \beta \tilde{W}_{ij,<t}^{Parents} + \alpha_{jt} + \xi_{ijt} \tag{10}$$

where W_{ijt}^{Child} is a binary measure of child welfare receipt in year t, and $\tilde{W}_{ij,<t}^{Parents}$ is years of parental welfare receipt in all previous years normalized by the number of prior years in which the parents were observed (effectively the rate of receipt). The results are qualitatively the same as the earlier panels. The OLS columns show large associations. The IV columns show precise zeros.

The final two panels shows results from an alternate perspective. These are intragenerational elasticities. As discussed earlier, veterans' children were more likely to receive welfare in early adulthood, for mechanical reasons related to eligibility. The estimates in Panels D and E quantify the relationship between early welfare receipt and later welfare receipt. For this version, equation (7) is replaced with:

$$\tilde{W}_{ij}^{Child\ aged\ 22+} = \beta \tilde{W}_{ij}^{Child\ aged\ 15-21} + \alpha_j + \xi_{ij} \tag{11}$$

where we again normalize the years in a given state by the number of years observed. We restrict attention to those children whom we observed from ages 15-21. The results again show strong OLS associations, combined with statistically insignificant IV estimates. This time, however, the IV estimates are not precise and instruments themselves are weak, as reflected in the F-statistic. Our preferred estimates are those relating to welfare receipt and where we instrument using the ballot outcome alone (Panel E). These results, while still imprecise, do rule out causal elasticities that are larger than the OLS elasticities, with over 90%

confidence. And the point estimates are close to zero, which is consistent with there being no causal intragenerational effect of early welfare receipt on later welfare receipt.

VI. Conclusion

We have shown that deployed service had very large effects on economic outcomes for Australia's Vietnam veterans and their spouses. It increased their welfare receipt by a combined 11 years on average and decreased their combined employment by 5 years, while lifting total incomes. As a consequence, their children were much more likely to be raised in welfare-receiving households, and less likely to be exposed to child poverty. Despite this, we have found almost no effects on the outcomes of these children. Under arguably conservative assumptions, this implies no causal intergenerational welfare persistence for this population. This contrasts with much of the literature on this topic.

Here we consider potential explanations for this discrepancy, and the lessons we can take from this setting for broader contexts.

A. Do Veterans' Pensions Inform Child Welfare Decisions?

In the literature on welfare dependency, intergenerational effects are often found to be payment-type specific. For example, an exogenous increase in parental disability insurance may increase child disability insurance receipt, but not child receipt of other payment types (as in Dahl et al., 2014). In our context, the potential for such payment-specific transmission is essentially absent. The generous veterans' pensions available to deployed veterans are not available to children, unless they became deployed veterans themselves, which is comparatively rare for this generation. Our results are therefore consistent with causal transmission operating only within the same payment type.

Similarly, the interaction between Vietnam veterans and the relevant agency (the Department of Veterans' Affairs, DVA) was quite different from what civilians faced. Their engagement with the DVA was a hostile, decades-long fight for recognition and compensation. Arguably, this fight was eventually successful, at least in terms of the generosity of compensation. But it is unclear how this experience influenced their children's desire and ability to engage with welfare agencies. It likely came with scarring, but also learning about collective action, and how to engage with government agencies. Their children likely witnessed strong animosity towards government. They also saw that applying for government support can be difficult, but can be quite lucrative.

On the other hand, we have shown that veterans' children were drawn into receiving welfare while studying before the age of 21, perhaps reflecting the Veterans' Children Education Scheme. While this was in a sense 'mechanical', it likely gave the children direct experience of and familiarity with welfare beyond the veterans' pensions.

Overall, this unusual setting arguably avoids the welfare stigma mechanism (a component of ϕ). But it does not completely avoid the effort cost mechanisms (another component of ϕ), especially through early direct exposure to welfare.

B. Does Parental Modeling Affect Child Preferences for Work/Leisure?

As shown in Section IV, children's exposure to parental disemployment was very large and sustained. Our results therefore do not support the hypothesis that parental nonemployment affects children's 'taste for leisure' (θ). However, this exposure did not commence until children reached adulthood. One may expect this to have a weaker impact than exposure at younger ages, but related evidence on this is mixed.[23] Nevertheless, it is possible that this mechanism may only affect behavior as the children themselves approach retirement age.

C. Human Capital

A priori there are many ways that child human capital (h) could be a mechanism in our setting. However, we have found no evidence that human capital was actually affected. The estimated effects on university attendance are precise zeros, as are the effects on employment and earnings (suggesting no effect on the wage). While there may be offsetting factors at play, the human capital of veterans' children was neither harmed nor enhanced, on average.

Veterans' payments can be classified in two categories: untied welfare support and welfare reliance (only available for those not working). There is evidence of positive effects of untied social assistance on children's outcomes (for example Aizer, Eli, Ferrie, & Lleras-Muney, 2016). In our context, it is possible that positive effects of welfare support offset negative effects of welfare reliance. Deployed service greatly increased both types of welfare, but we are unable to cleanly disentangle their impacts on the next generation.

Relatedly, we have shown that parental deployed service substantially reduced child poverty and increased average parental income (probably due to the generous veterans' payments). These improved the home environment in which the children grew up, again potentially offsetting any negative effects of parental disemployment.

D. Intensity of Service in Vietnam

Our results also contrast with the US literature on intergenerational effects of military service, where negative effects are found for children. The different

[23]The main estimation sample in Dahl et al. (2014) also consists of children aged 18+ when exposure commenced (at the time of the appeal decision). Despite this, they found strong intergenerational effects. In subsequent analysis (Section V.B) they find that the results do not vary much by age of child, or by whether children lived at home at the time of the appeal decision. Age of exposure in Dahl and Gielen (2021) was also not young (average 15.6 at the reform date). However, they found larger effects for children who were younger at exposure.

veterans' compensation systems are a likely explanation for this, with the Australian system being much more generous (Siminski, 2013). Another potential explanation is the nature of military service itself. Casualty rates for Australians deployed to Vietnam were roughly half those of their US counterparts (Johnston et al., 2016). This suggests that direct detrimental effects of service may also have been smaller for those who returned. This lends further support to our focus on the intergenerational effect of welfare receipt, rather than the effect of service itself.

E. Summary

The Australian Vietnam-era draft lotteries provide a well-identified setting in which welfare receipt does not transmit from one generation to the next. The results reaffirm that causal intergenerational welfare dependence is context-specific and far from inevitable. In this setting, several mechanisms are arguably absent. Parental welfare receipt is unlikely to directly reduce the costs of welfare receipt for children—veterans' payments are provided by a different agency and have different rules. Similarly, parental receipt of veterans' payments is unlikely to reduce the stigma cost for children of receiving civilian welfare. We have also shown that children's human capital was unaffected. Perhaps the most important potential remaining mechanism is the taste for leisure. Veterans and their spouses worked much less. This has been a potential mechanism of intergenerational transmission in much of the existing literature—that welfare-receiving parents provide an example that may influence their children to prefer leisure over work. But our results do not show this. This is despite the mechanical intergenerational transmission for the children of welfare recipients during their youth. Taken as a whole, this suggests policymakers need not worry about the *generalized* transmission of welfare from one generation to the next or from early adulthood into later adulthood, but should instead focus on more specific transmission mechanisms that are being revealed in this nascent literature.

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Appendix A: Additional Results

As discussed in Section II, there are many different specifications that can be used to leverage the randomly assigned draft eligibility. In much of the analysis (Section V is an important exception), we have focused on 2SLS models, with deployed service as the endogenous variable. Our preferred specification is an overidentified model (as per equation (4)), estimated using the 12 cohorts where there is a first-stage effect on deployed service. Here we present results from many alternative specifications. By doing so, we show the sensitivity of the key results, and scrutinize the preferred specification.

To illustrate the range of plausibly valid approaches, Table A1 shows results from eight specifications. All are estimated using person-level data, with years of employment as the outcome variable. The results are presented in three panels, one for each population group.

The first column shows reduced-form results, with a single binary instrumental variable (draft eligibility). The next two columns show just-identified 2SLS results, which both use the same binary instrument. Column (2) treats army service as the (sole) endogenous variable, while Column (3) treats deployed service as the endogenous variable. Columns (4) and (5) show results from specifications equivalent to those in Columns (2) and (3), but these are over-identified, as per equation (2). Overidentification improves efficiency considerably, by leveraging the large cohort differences in the first-stage, as depicted in Figure 1. This explains the smaller standard errors in Columns (4) and (5), as compared to Columns (2) and (3).

Column (6) also draws on the over-identified specification. Here there are two endogenous variables—army service and deployment, following Siminski (2013). The second-stage equation is:

$$Y_{ij} = \beta_R \hat{R}_{ij} + \beta_V \hat{V}_{ij} + \alpha_j + \xi_{ij}. \tag{A1}$$

The coefficients β_R and β_V are separately identified because the relative magnitudes of their first-stage effects differ considerably between cohorts. This can again be seen in Figure 1. The estimates in Column (6) suggest that army service had no effect on employment (for each population), unless that service included deployment. Consequently, the effects of deployment in Column (6) are similar to those in Columns (5), (7) and (8), where the army service variable is excluded. We estimate results for other outcome variables with this important specification (and its dynamic counterpart) in the tables that follow.

Column (7) shows results from the specification preferred for much of the analysis. It is the same as Column (5), except we focus only on the first 12 cohorts. This is justified because there is no first-stage effect on deployment for any of the later cohorts. Their exclusion leads to some gains in precision. In practice, it is clear that the results in Columns (5) and (7) are very similar to each other. The other reason to prefer the Column (7) specification is that descriptive statistics (such as those in Table 1) are for a more relevant population with the last three cohorts excluded. Finally, Column (8) shows the impact of clustering (on the primary sample's date of birth) on standard errors. It shows robust standard errors that are not clustered, and is otherwise the same as Column (7). A comparison of Columns (7) and (8) shows that clustering makes little difference for the primary sample, or for their spouses (increasing the standard error by 2% and 5% respectively). It makes a larger difference for their children, where the standard error increases by 19%. We interpret this to reflect within-family correlations, whereby the sample of children often includes multiple children of the same father.

The exclusion of army service from the preferred specification is scrutinized further in Figure A1. It shows results for key outcomes for men and spouses. It shows estimated effects of deployment using the preferred dynamic specification (equation (5)). It also shows the estimated effects of deployment and army service, when both are included in the model, as per the dynamic equivalent of equation (A1). Each panel of this figure shows estimated effects of army service that are close to zero at every age. It also shows that the estimated effects of deployment are similar in the two specifications. There is one exception—the estimates for receiving welfare for men. At older ages, the estimated effects of army service are non-zero (albeit small). This means that the effects of deployed service on welfare receipt are slightly overestimated at older ages for men. We explore this further with static specifications and more outcome variables in the tables that follow.

The total effects of service on veterans' outcomes are shown in Table A2 below. These are estimated using person-level data. The outcome variables are defined as the total of the annual outcomes across the full 31-year data window (1991-2021) for each man (or 2002-2021 for the welfare variables and total income). Panel A uses the preferred static specification (equation (4)). Panel B uses the full specification (equation (A1)) to test the sensitivity of results to the exclusion of army service from the model.

Panel A shows that deployed service is estimated to have decreased employment by 3.0 years (18.4% relative to the mean). It also increased years of welfare receipt by 8.0 years (109% relative to the mean). Columns (3) and (4) decompose this into years of welfare support (defined as welfare received in years where the person was working), and welfare reliance (welfare received in years where the person was not working). Welfare reliance accounts for 80% of the welfare effect. Total earnings were accordingly reduced by an average of \$164,325, or 14.9% of mean earnings. This reduction in earnings was more than offset by increased pension income (\$396,259), even without observing pensions before 2002. The estimated effect on total income is \$178,508 (17.5% relative to the mean), and this is highly statistically significant.

The employment variable we use in most of the analysis is coarsely defined, out of necessity. It is constructed as a sum of binary indicators, each set to one if earnings exceed zero in each year. This ignores possible effects on the intensive margin of employment (part-time work and/or part-year work). To gauge potential bias, we can draw further on the earnings data, by treating total earnings as an alternative proxy measure of work quantity (ignoring any effects on the wage, on the assumption this is negligible). While not shown in the table, the estimated effect on log total earnings (-0.176 (s.e. 0.043)) is slightly smaller and less precise, but similar to, the corresponding effect on log total employment years (-0.211 (s.e. 0.024)). These results provide reassurance for the validity of the main disemployment estimates.

Panel B shows that army service does not have a significant effect for most outcomes. The exceptions are the welfare variables, for which army service is statistically significant but small. The estimated effects of deployment are broadly similar in the two panels, albeit slightly smaller for some key outcomes in Panel B.

Total effects of deployed service for spouses are shown in Table A3, which follows the structure of Table A2 above. Panel A shows that deployed service decreased spouse employment by 1.8 years over 1991–2021, or 11.0% relative to the mean. It also increased welfare receipt by 3.7 years over 2002–2021. Total earnings were reduced by \$66,993 (9.5% relative to the mean), offset by higher government payments (\$63,907). The effect on total income is not statistically significant.

Panel B shows the corresponding estimates for the full specification. Army service is not statistically significant in any column. The effect of deployed service is similar across panels for most outcome variables.

The total effects (1991-2021; 2002-2021 for welfare and total income) of military service on veterans' children's economic outcomes are shown in Table A4. Panel A shows results for all children. None of these estimates are statistically significant. Examining these results by sex (Panels B and C) yields few additional insights. Most of the estimates remain as precise zeros for each sex. Panel D shows that army service is not statistically significant if included in the model, for any outcome.

Table A1—Effects of ballot and service on conscripts, spouses and children: robustness

	Reduced form	Just-identified		Over-identified				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Conscripts								
Draft eligible	-0.250***							
	(0.026)							
Army		-0.985***		-1.021***		-0.208		
		(0.103)		(0.100)		(0.227)		
Vietnam			-3.202***		-2.985***	-2.483***	-2.986***	-2.986***
			(0.334)		(0.262)	(0.604)	(0.262)	(0.258)
N	751,089	751,089	751,089	751,089	751,089	751,089	593,713	593,713
Panel B: Spouses								
Draft eligible	-0.118***							
	(0.028)							
Army		-0.466***		-0.527***		0.295		
		(0.111)		(0.109)		(0.234)		
Vietnam			-1.514***		-1.794***	-2.505***	-1.795***	-1.795***
			(0.359)		(0.293)	(0.634)	(0.293)	(0.278)
N	772,779	772,779	772,779	772,779	772,779	772,779	611,400	611,400
Panel C: Children								
Draft eligible	0.015							
	(0.015)							
Army		0.060		0.063		0.081		
		(0.058)		(0.058)		(0.122)		
Vietnam			0.196		0.140	-0.055	0.134	0.134
			(0.188)		(0.158)	(0.332)	(0.158)	(0.133)
N	1,793,945	1,793,945	1,793,945	1,793,945	1,793,945	1,793,945	1,425,832	1,425,832
Cohorts	1-15	1-15	1-15	1-15	1-15	1-15	1-12	1-12
Clustered SEs	X	X	X	X	X	X	X	

Note: Displays coefficients and standard errors from regressions of the number of years employed between 1991-2021 on: the ballot outcome (1); probability of army service (2), (4); probability of Vietnam service (3), (5), (7), (8); or both the probability of army and Vietnam service (6). Ballot outcome and probabilities are for the lottery-eligible individual. For (2) and (3) they are specific to the ballot outcome only and the results are just-identified; for (4)-(8) they are specific to the ballot outcome and cohort and the results are over-identified. All specifications include cohort fixed effects. Results for children also include a quadratic in year of birth. Robust standard errors are clustered at the level of the lottery-eligible individual's date of birth in all specifications except (8). The populations are restricted to those who did not die in or before 2021. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2—Effects of Vietnam service on conscripts

	(1) Years employed	(2) Years welfare	(3) Years welfare supported	(4) Years welfare reliant	(5) Earnings (AUD'000)	(6) Welfare (AUD'000)	(7) Income (AUD'000)
Panel A: Baseline specification							
Vietnam	-2.986***	7.968***	1.505***	6.463***	-164.325***	396.259***	178.508***
	(0.262)	(0.210)	(0.083)	(0.193)	(26.272)	(6.222)	(24.983)
Mean	16.255	7.328	1.325	6.003	1,103.151	112.472	1,018.813
N	593,713	593,713	593,713	593,713	593,713	593,713	593,713
Panel B: Full specification							
Vietnam	-2.483***	7.047***	1.126***	5.921***	-81.131	389.768***	275.055***
	(0.604)	(0.421)	(0.163)	(0.388)	(64.802)	(10.271)	(59.263)
Army	-0.208	0.382**	0.158***	0.225	-34.513	2.697	-40.100*
	(0.227)	(0.149)	(0.058)	(0.137)	(24.611)	(3.154)	(22.408)
Mean	16.710	6.883	1.290	5.593	1,147.230	105.109	1,055.222
N	751,089	751,089	751,089	751,089	751,089	751,089	751,089

Note: Displays coefficients and standard errors from regressions of the stated individual outcomes on the probability of Vietnam (Panel A) or Vietnam and Army Service (Panel B) for the individual's birth cohort and ballot outcome. Both specifications include cohort fixed effects, with robust standard errors clustered at the level of the individual's date of birth. Mean outcomes for those who were balloted-out are also shown. Employment and earnings outcomes are summed over 1991-2021; welfare and income are summed over 2002-2021. The population is restricted to those who did not die in or before 2021. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3—Effects of Vietnam service on spouses of conscripts

	(1) Years employed	(2) Years welfare	(3) Years welfare supported	(4) Years welfare reliant	(5) Earnings (AUD'000)	(6) Welfare (AUD'000)	(7) Income (AUD'000)
Panel A: Baseline specification							
Vietnam	-1.795***	3.746***	0.785***	2.961***	-66.993***	63.907***	-23.663
	(0.293)	(0.207)	(0.083)	(0.191)	(19.427)	(3.534)	(17.367)
Mean	16.342	6.593	1.297	5.296	701.924	93.237	724.947
N	611,400	611,400	611,400	611,400	611,400	611,400	611,400
Panel B: Full specification							
Vietnam	-2.505***	3.461***	0.661***	2.800***	-119.693***	63.397***	-29.182
	(0.634)	(0.415)	(0.167)	(0.372)	(43.507)	(6.846)	(38.896)
Army	0.295	0.118	0.051	0.067	21.911	0.204	2.311
	(0.234)	(0.146)	(0.060)	(0.131)	(16.025)	(2.387)	(14.217)
Mean	16.776	6.189	1.284	4.905	727.353	87.150	744.514
N	772,779	772,779	772,779	772,779	772,779	772,779	772,779

Note: Displays coefficients and standard errors from regressions of the stated individual outcomes on the probability of Vietnam (Panel A) or Vietnam and Army Service (Panel B) for the birth cohort and ballot outcome of the individual's lottery-eligible spouse. Both specifications include cohort fixed effects, with robust standard errors clustered at the level of the lottery-eligible spouse's date of birth. Mean outcomes for those whose spouse was balloted-out are also shown. Employment and earnings outcomes are summed over 1991-2021; welfare and income are summed over 2002-2021. The population is restricted to those who did not die in or before 2021. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE A4—EFFECTS OF VIETNAM SERVICE ON CHILDREN OF CONSCRIPTS

	(1) Years employed	(2) Years welfare	(3) Years welfare supported	(4) Years welfare reliant	(5) Earnings (AUD'000)	(6) Welfare (AUD'000)	(7) Income (AUD'000)
Panel A: Baseline specification; all children							
Vietnam	0.134	-0.042	0.034	-0.076	10.248	-1.619	9.172
	(0.158)	(0.122)	(0.065)	(0.083)	(17.996)	(1.779)	(17.381)
Mean	19.817	3.695	2.194	1.501	1,096.618	39.341	1,121.953
N	1,425,832	1,425,832	1,425,832	1,425,832	1,425,832	1,425,832	1,425,832
Panel B: Baseline specification; female children							
Vietnam	0.087	-0.057	0.067	-0.124	12.548	-2.074	22.014
	(0.218)	(0.167)	(0.092)	(0.118)	(20.252)	(2.500)	(19.590)
Mean	19.584	4.174	2.455	1.720	891.733	46.546	911.909
N	696,419	696,419	696,419	696,419	696,419	696,419	696,419
Panel C: Baseline specification; male children							
Vietnam	0.185	-0.020	0.007	-0.027	8.220	-1.038	-3.653
	(0.215)	(0.152)	(0.081)	(0.100)	(26.948)	(2.191)	(25.443)
Mean	20.040	3.236	1.944	1.292	1,292.383	32.456	1,322.648
N	729,413	729,413	729,413	729,413	729,413	729,413	729,413
Panel D: Full specification; all children							
Vietnam	-0.055	-0.183	-0.023	-0.160	-14.439	-3.747	-13.615
	(0.332)	(0.268)	(0.144)	(0.189)	(34.633)	(3.951)	(33.176)
Army	0.081	0.055	0.022	0.033	10.456	0.849	9.634
	(0.122)	(0.101)	(0.054)	(0.071)	(12.210)	(1.491)	(11.663)
Mean	19.441	3.762	2.254	1.507	1,063.393	39.694	1,096.908
N	1,793,945	1,793,945	1,793,945	1,793,945	1,793,945	1,793,945	1,793,945

Note: Displays coefficients and standard errors from regressions of the stated individual outcomes on the probability of Vietnam (Panels A-C) or Vietnam and Army Service (Panel D) for the birth cohort and ballot outcome of the individual's lottery-eligible father. Both specifications include cohort fixed effects and a quadratic in year of birth, with robust standard errors clustered at the level of the lottery-eligible father's date of birth. Mean outcomes for those whose father was balloted-out are also shown. Employment and earnings outcomes are summed over 1991-2021; welfare and income are summed over 2002-2021. The population is restricted to those who did not die in or before 2021, with Panel B restricted to female children and Panel C restricted to male children. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

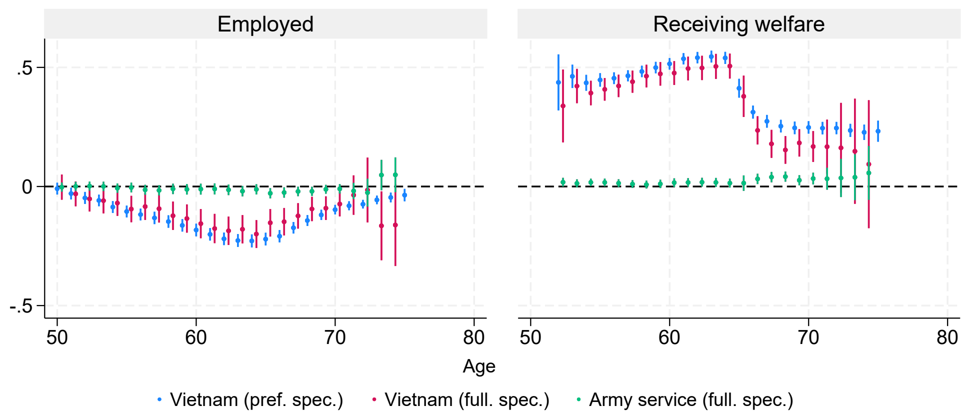
Table A5—Correlational and causal persistence in outcomes between and within generations

	Outcome			
	Welfare receipt		Welfare reliance	
	OLS	IV	OLS	IV
Panel A: Intergenerational – Own years of outcome				
Parent years	0.102***	0.004	0.062***	0.001
	(0.001)	(0.011)	(0.001)	(0.009)
N	1,143,861	1,143,861	1,143,861	1,143,861
F-statistic on IVs		68.959		53.123
Panel B: Intergenerational – Own years of outcome after age 21				
Parent years	0.079***	-0.004	0.050***	-0.002
	(0.001)	(0.010)	(0.000)	(0.008)
N	1,143,861	1,143,861	1,143,861	1,143,861
F-statistic on IVs		68.959		53.123
Panel C: Intergenerational – Own outcome in year T				
Parent years to T-1	0.202***	0.011	0.135***	-0.000
	(0.001)	(0.017)	(0.001)	(0.018)
N	25,590,577	25,590,577	25,590,577	25,590,577
F-statistic on IVs		90.525		54.478
Panel D: Intragenerational – Own years of outcome after age 21				
Own years to 21	0.507***	0.189	0.664***	0.404*
	(0.003)	(0.172)	(0.005)	(0.214)
N	160,543	160,543	160,543	160,543
F-statistic on IVs		2.024		1.540
Panel E: Intragenerational – Own years of outcome after age 21 (just identified)				
Own years to 21	0.507***	0.121	0.664***	0.091
	(0.003)	(0.218)	(0.005)	(0.341)
N	160,543	160,543	160,543	160,543
F-statistic on IVs		16.097		8.982

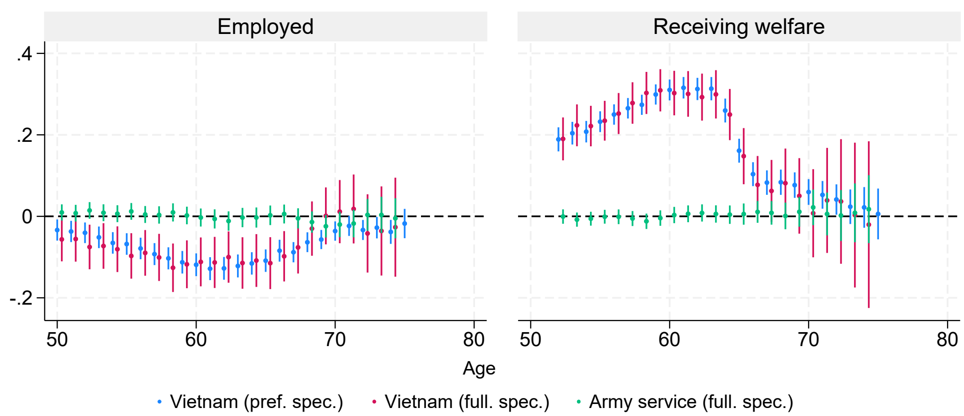
Note: Displays coefficients and standard errors from regressions of the stated individual outcomes for the child generation on analogous parent outcomes (Panels A and B), earlier parent outcomes (Panel C) or earlier child outcomes (Panels D and E). Specifically: Panel A regresses child years in welfare receipt or welfare reliant over 2002-2021 on parent years in the same state over the same period; Panel B is the same but only summing child outcomes over the years in which they are strictly over age 21; Panel C switches attention to person-years and regresses child outcome in year T on parent rate of receipt (years in the same state up to that year over years observed); Panels D and E are the same as Panel B but the key independent variable is now child rate of receipt in the same state up to and including age 21 and the key dependent variable is the same rate beyond age 21. In Panels D and E we restrict to children observed for all ages from 15-21 inclusive. All regressions include a quadratic in year of birth and father-cohort fixed effects, with robust standard errors clustered at the level of the father's date of birth. In the instrumental variables specification we instrument for the key independent variable with the father's cohort interacted with the ballot outcome (Panels A-D) or simply father's ballot outcome (Panel E). The population in Panels A and B is restricted to those who did not die, or have their parents die, in or before 2021. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A1. Effect of Vietnam and Army service on probability of employment and welfare receipt by age

Panel A. Conscripts

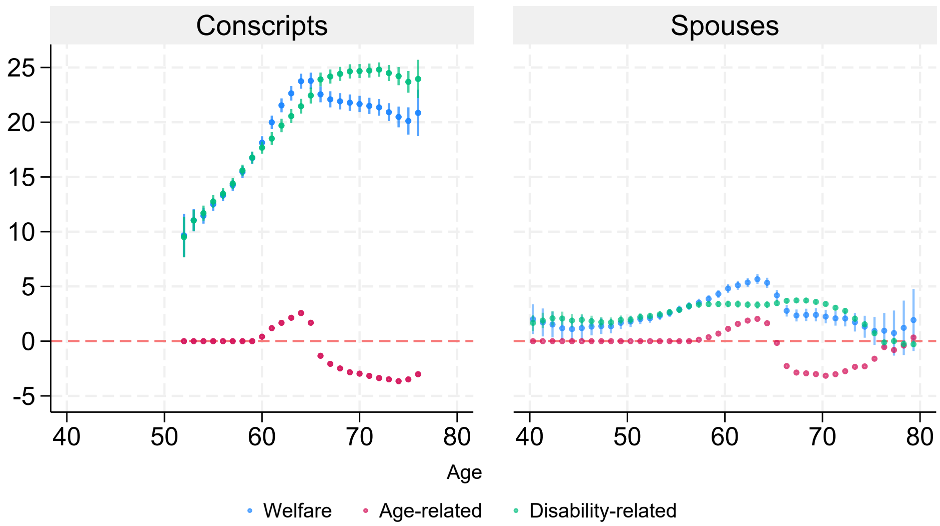


Panel B. Spouses



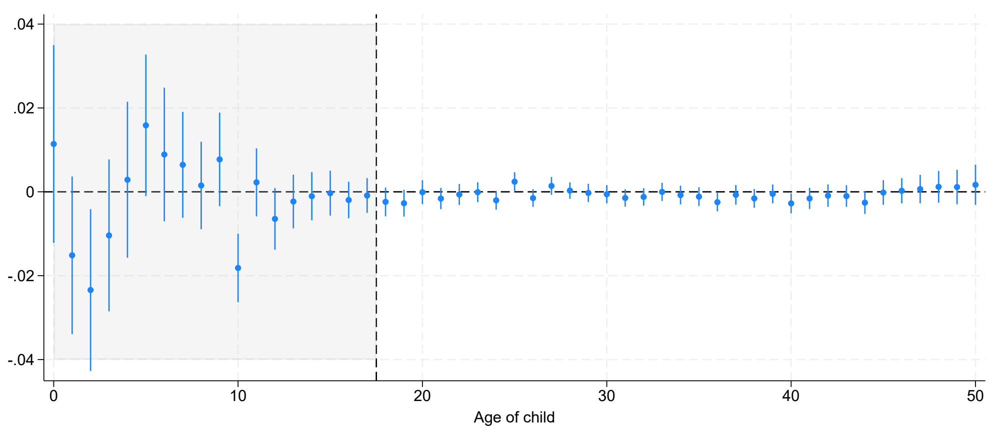
Note: Shows the age-specific coefficients and 95% confidence intervals on Vietnam and Army service from equation (5) and the dynamic version of (A1). For conscripts, employment can be observed from around age 40; welfare receipt is observed only later due to data limitations discussed in Section II. To aid comparisons we show all results over roughly the same age ranges. The scale differs between panels.

Figure A2. Effect of Vietnam service on welfare income (AUD'000) of conscripts and their spouses by age



Note: Shows the age-specific coefficients and 95% confidence intervals on Vietnam service from equation (5) for welfare income received by conscripts and their spouses, by payment type. Regressions for conscripts (spouses) are estimated for all ages from 50 (40) to 76 (80) due to data limitations discussed in Section II. For readability we do not show the coefficients for conscript outcomes at the youngest ages (40-41 or 50-51); these coefficients are identified only from the youngest cohort, which had very low probability of Vietnam service, and are hence imprecisely estimated.

Figure A3. Effect of Vietnam service on probability of parental separation by child age



Note: Shows the child-age-specific coefficients and 95% confidence intervals on Vietnam service from equation (6) for the outcome that either parent reports a separation at the given child age. Shading indicates childhood, ending in the vertical line at age 18.

Our analysis draws on the Australian Taxation Office (ATO) Longitudinal Information Files ('ALife') (Abhayaratna et al., 2022). This brings together data from ATO client records, higher education loans from 1989, tax returns from 1991, superannuation funds from 1997, and employer-reported earnings and welfare agency reported payments from 2002. It is updated annually. Importantly, the dataset also contains family links that draw on administrative data from Australia's universal public health system ('Medicare') (see Fischer et al., 2025).

The ALife dataset has been curated for research purposes, with a concerted effort to document the dataset and understand its strengths and limitations.[24] ALife has been widely used in studies ranging from intergenerational mobility, to retirement and tax policies.[25]

In Australia, the individual is the primary unit of taxation, with household income tests predominantly restricted to transfer payments. As a result, individual-level outcomes are recorded in the administrative data. The Australian financial year runs from 1 July through to 30 June. Unless otherwise stated, we refer to financial years throughout this paper, referring to them by the calendar year in which they end.

B1. Employment and Earnings

An individual is employed in a given year if they have nonzero wage and salary income for that year. This could be self-reported or reported by their employer (from 2002). Prior to 2002 we only have self-reported incomes from tax returns to draw on. However, we expect high levels of compliance. The tax-free threshold in this period was well below the full-time minimum wage, meaning individuals were required to lodge tax returns. Further, even though the ALife data do not contain employer-reported wage and salary income, these reports were still being made to the ATO and were available for compliance activities. Finally, the informal sector is relatively small in the Australian economy (Elgin, Kose, Ohnsorge, & Yu, 2021). However, this definition will understate employment for the self-employed, who may receive income in other forms.

B2. Government Payments

An individual is a welfare recipient if they have any government payments or pensions for that year. For these outcomes we restrict our analysis to 2002 on, so we can draw on the agency-reported data. This is because a wider set of payments

[24] The main papers along these lines are: Abhayaratna et al. (2022) (general overview); Polidano et al. (2020) (superannuation data); Fischer et al. (2025) (family links) and Hathorne and Breunig (2022) (occupation data).

[25] For example: Deutscher and Mazumder (2020); Deutscher (2020) (intergenerational mobility); Carter and Breunig (2019); Chan, Morris, Polidano, and Vu (2022); Carter and Breunig (2024); Breunig, Deutscher, and Hamilton (2024) (retirement and tax policy).

are reported to the ATO than those required to be reported by individuals on their tax return. This includes non-taxable payments such as disability pension and invalidity service pension where they are paid to individuals below age pension age, which are likely to be an important source of welfare for our first-generation populations.[26] We use the disaggregation of welfare payments into age-related, disability-related and other categories provided by the ATO. We make one change to this. Service pensions are allocated by the ATO to the age-related category as the reporting does not indicate whether these payments are being made due to age or disability. However, we can observe when an individual starts receiving payments. As such, we recode age-related payments as disability-related where an individual begins receiving them below the age-related Service Pension eligibility age. Due to the more generous payment rates, there is little incentive to switch from disability to age pensions.

B3. Occupation

Individuals also report their occupation on their tax return. However, occupational classifications and quality have changed over time. The ATO uses two approaches to provide consistent occupation classifications over the ALife period. The first uses the text description of the occupation taken directly from the tax return (paper or electronic) (available from 1999) and uses an algorithm provided by the Australian Bureau of Statistics to map that to an Australian and New Zealand Classification of Occupations (ANZSCO) code. The second approach uses the occupation code derived by ATO processing at the time, and draws on concordances between earlier occupational classification schemes and ANZSCO. ALife occupation data are only provided to researchers at the 2-digit level, but our code has been run by ATO staff on more finely disaggregated occupation data (at the 6-digit level) so that we can identify military service.

We focus on three broad occupation classifications – protective services, military service and protective services excluding the military. In Table B1 we show the occupation codes and titles included in each of these groupings. The table also highlights how military occupations are split across the standard 2-digit codes.

An individual is considered to be in protective services or military employment if either the text- or code-based approach puts them in the relevant grouping. This maximises our ability to detect employment in these groups. Hathorne and Breunig (2022) provide a much more detailed examination of the ALife occupation data. They find that while the distribution of occupations is similar to that observed in representative survey data, the ALife data understates occupational mobility. For this reason we focus on occupational outcomes observed over the full period (years in a given occupational grouping) rather than dynamics.

[26] See: `https://www.ato.gov.au/individuals-and-families/income-deductions-offsets-and-records/income-you-must-declare/amounts-you-do-not-include-as-income` [Accessed: 3 January 2025].

Table B1—Codes and titles for protective service and **military** occupations

11 - Chief Executives, General Managers and Legislators	
111212	**Defence Force Senior Officer**
13 - Specialist Managers	
139111	**Commissioned Defence Force Officer**
139112	Commissioned Fire Officer
139113	Commissioned Police Officer
139211	**Senior Non-commissioned Defence Force Member**
44 - Protective Service Workers	
4411	**Defence Force Members - Other Ranks**
4412	Fire and Emergency Workers
4413	Police
4421	Prison Officers
4422	Security Officers and Guards

Note: This table provides the codes used to identify protective service and military occupations. These occupations are split across three separate 2-digit codes as shown. The first two require 6-digit codes to identify military occupations within them, while the last only requires 4-digit codes. Note that there is no equivalent to 'Defence Force Senior Officer' that can clearly be linked to other protective service occupations.

B4. Other Variables

For the child generation, we can proxy for university attendance from the higher education loans data. The vast majority of Australian citizens attending university do so with the support of a government-provided income-contingent loan. We consider someone as having ever attended university if they appear in this loan data. We consider them as attending university if their debt increases by more than the indexation amount (the consumer price index).

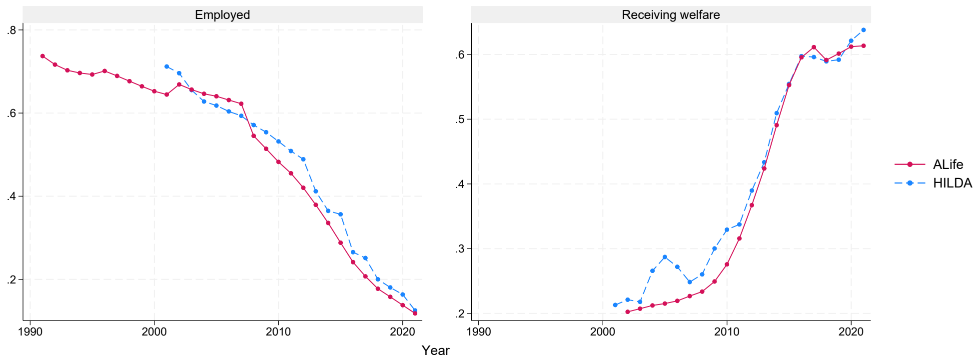
We use the ALife family file for variables relating to whether someone has ever had a spouse (and the number of distinct spouses). We can also use this file for the presence and number of children for the men and their spouses. However, the family links do not currently extend beyond children born in the year 2000. As such, for the child generation we use the number of dependent children reported on the tax return for fertility information.

B5. Benchmarking

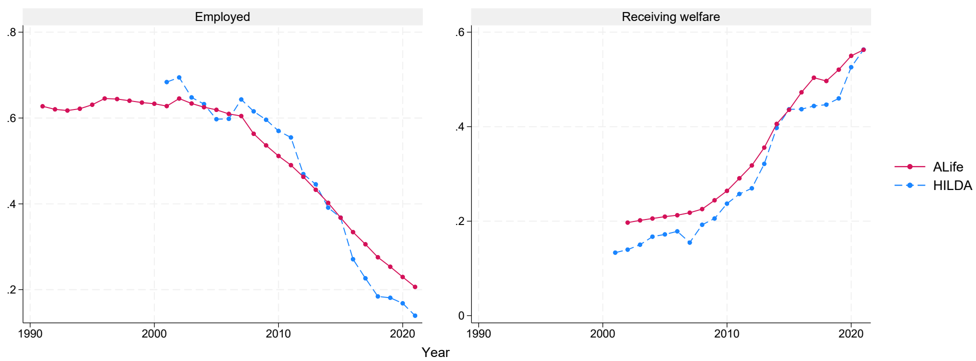
We now compare the key outcomes for our populations of interest in the ALife dataset to those observed in the Household Income and Labour Dynamics in Australia (HILDA) survey. In Figure B1 we show, for each population, the evolution over time of the proportion employed and the proportion receiving welfare. The broad levels and trends are very similar. Perhaps the most notable deviation is the higher proportion employed in the HILDA data – this may reflect our administrative-data measure understating self-employment. While more expansive definitions of employment are possible (see Carter and Breunig (2019)), these draw on a wider variety of tax return items and are not possible to code consistently over the full 1991-2021 window of analysis.

Figure B1. Proportion of population in employment and welfare receipt by year

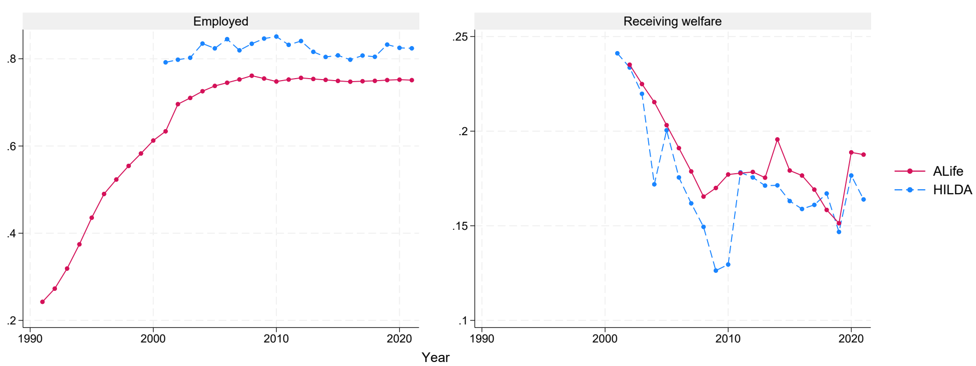
Panel A. Men



Panel B. Spouses



Panel C. Children



Note: For each population of interest, shows the proportion of individuals employed and in receipt of government payments by year in either the ALife data (unbroken lines) or the HILDA data (dashed lines). In the HILDA data we isolate the populations of interest as: males born in Australia between 1945 and 1950 inclusive (the first twelve cohorts) ('Men'); female partners of males born in Australia between 1945 and 1950 inclusive ('Spouses'); and individuals with fathers born in Australia between 1945 and 1950 inclusive ('Children'). We define employment and welfare receipt as in the administrative data – receipt of nonzero wage and salary income and welfare income respectively. The HILDA proportions are weighted by responding person weights.