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Lifting up the lives of extremely disadvantaged youth

The role of staying in school longer

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Research Summary

Why was the research done?

Research on the returns to education is plentiful but focuses mainly on the impacts of changes to compulsory schooling laws on outcomes of young people who stay in school longer because of the reform. Much less is known about specific effects of staying in school longer for extremely disadvantaged youth, independently of whether they remain in school until the compulsory age and whether staying in school is affected by varying compulsory schooling laws.

Using a sample of Australians who display high rates of early school-leaving, we compare the trajectories of respondents who left school at each incremental age between 14 and 17 with respondents who left at 18 years old or more, in terms of homelessness, incarceration, substance use and mental health issues.

What were the key findings?

Results indicate that leaving school before age 18 increases males' likelihood of experiencing homelessness, being incarcerated, using cannabis daily and illegal street drugs weekly several years after school-leaving.

In contrast, for females we find no evidence that early school leaving affects their likelihood of experiencing homelessness, being incarcerated, using cannabis daily and illegal street drugs weekly.

What does this mean for policy and practice?

The gender gaps in findings may reflect different reasons why males and females quit school (e.g. females may leave if they expect a baby) and different sets of support programs potentially offered to them as a result. Overall, they reinforce other results in the education literature that males' disadvantage in education is a critical policy issue. Identifying programs that keep disadvantaged young males in school can help them break cycles of multi-dimensional disadvantage.

Citation

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We acknowledge the Traditional Custodians of the lands on which we work and live across Australia.
We pay our respects to Elders past and present and recognise their continued connections
to land, sea and community.

1. Introduction

Growing up in economic and social disadvantage has long-lasting consequences on people's life trajectories, including for their children and future generations. Identifying pathways that enable young people to break cycles of multi-dimensional disadvantage and build a better life for themselves has long been an active area of research in social sciences. Schooling is often viewed as the main tool to lift the life of disadvantaged children by providing them an opportunity to build the necessary skills to lead a productive and successful life. Within the school system, finishing high school has become a focal point of education policies in many countries. Overall, the evidence suggests that dropping out of school is associated with lower employment, income, health, intergenerational mobility and higher crime rates (Rumberger, 1987, Rumberger 2020). But what do extra year(s) of school mean for the disadvantaged and the very disadvantaged students specifically? And can this help lift their lives out of multi-dimensional disadvantage? These are important questions, not just for fairness reasons, but also from a cost-benefit perspective (Machin, 2006).[1]

The extensive economics literature on the returns to schooling has paved the way to understanding better how extra years of schooling translate into outcomes. Importantly though, this literature addresses the issue of disadvantage somewhat indirectly. Indeed, some of the most popular methods in this literature identify the returns to schooling for the marginal student, the one who was close to dropping out, but didn't because of a coincidental difference in his circumstances - because he was born after a change in compulsory schooling laws, because he was born later in the year, because he was a few kilometres closer to a University or while his identical twin did not complete. This literature tends to find positive returns of extra schooling on labour market outcomes (Angrist and Krueger, 1991, Card 1993, Harmon and Walker 1995, Ashenfelter and Rouse 1998), especially for less advantaged groups (Meghir and Palme, 2005) and when the increase in schooling duration is accompanied with providing credentials for those with no qualifications (Grenet, 2013).

More recently, extensions of this literature have shown that extra years of schooling also translate to better outcomes in terms of physical health, wellbeing and satisfaction, marriage stability, incarceration and children's outcomes (Lleras-Muney, 2005; Oreopoulos and Salvanes, 2011; Van Kippersluis et al., 2011; Fletcher, 2015; deNew et al., 2021). These studies demonstrate the importance of education for a broad range of outcomes such as general measures of health and wellbeing (e.g. age at death, overall life satisfaction) as well as adverse outcomes.

Yet, this tells us remarkably little about the extremely disadvantaged students who sit further away from the margin and display a school-leaving age profile that greatly differs from the

[1] Cost benefit analysis estimates suggest that public and private costs of early school leaving reach $20,390 in Australia (Lamb and Huo, 2017) and $26,600 in Canada (Hankivsky, 2008) (in 2024 USD) for each early school leaver annually. These costings account for many (but not all) of the costs associated with early school leaving, including lost earnings and taxes, increased crime costs from public expenditures and costs for victims, additional public and private health expenditures, and extra welfare support programs and transfer payments. Aggregated to a cohort of early school leavers and extended to a lifetime, this represents tens to hundreds of billions of dollars lost for each cohort.

average population.[2] Indeed, it has been noted that the benefits of general expansions of education are not concentrated amongst the most disadvantaged students, thereby reducing the prospects for intergenerational mobility (Blanden et al. 2005). This leaves a gap in the literature around the returns from extra years of schooling for students from very disadvantaged backgrounds.

Other strands in the literature focus specifically on disadvantaged students. But they address the question of the returns to schooling indirectly by switching not just the duration of schooling but the whole school or neighbourhood environment. For example, lotteries have been used to randomly assign disadvantaged families to better neighbourhoods and identify the impact of the treatment on children's and parents' outcomes, including education. These have shown strong and robust positive effects of relocation on schooling and other outcomes (see Chyn and Katz 2021 for a review). In a similar vein, lotteries that assign disadvantaged students to better schools have found that treated students had better education and other long-term outcomes (Deming 2011; Dobbie and Fryer 2015). While these studies illuminate how the outcomes of very disadvantaged children change when they are assigned to a better environment, it doesn't shed light on mechanisms such as the direct impacts of extra years of schooling.

Our paper speaks to these two strands of literature by estimating the returns of staying additional year(s) at school for the most disadvantaged. First, we add to the evidence on the returns to schooling by zooming in on the population that is the most likely to leave school early, i.e. individuals who grew up in multi-dimensional disadvantage. This allows us to frame the study specifically for this population by analysing marginal increments in their education that are relevant to them, rather than increments that are relevant to the general population (finishing high-school). Second, we contribute to the literature which evaluates programs aimed at improving the lives of the very disadvantaged. More precisely, we focus on one of the main policy tool aimed at equalising opportunities (schooling) and investigate its impact on youth homelessness, incarceration, substance use and mental health conditions.

We use information collected in the Journeys Home survey, an Australian longitudinal survey of individuals who are homeless or at risk of becoming homeless (see Wooden et al., 2012 for details).[3] These data offer a unique chance to estimate the returns to staying in school longer for a sample of respondents who are amongst the highest centile of the population in terms of multi-dimensional disadvantage (Moschion and Van Ours, 2019) and experienced high levels of disadvantage in childhood.[4] We can therefore compare the outcomes of individuals who left school

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[2] Comparing average Australians from the Australian Census with a sample of extremely disadvantaged Australians from the Journeys Home survey shows that while seven out of ten Australians graduate from high school, only four out of ten do amongst the very disadvantaged (Scutella et al. 2013).

[3] It is important to note the outcome we analyse in the paper, youth homelessness, is different from the lifetime prevalence used to select the JH sample. In practice, the JH sample has experienced homelessness at various ages, some who have experienced it before leaving school (19 percent were age 0-14 the first time they were homeless); others at a similar time (32 percent were 15-17); others while still young but after leaving school (21 percent were 18-24) or later in life (28 percent were 25 or more) (Scutella et al. 2013). This provides variation while disconnecting the sample selection from the outcome analysed (see Moschion and Van Ours for more details and appendix 1).

[4] For instance, the JH sample experienced high levels of family disruptions with 26 percent having been placed in State care before the age of 18, 7 percent having biological parents die before they were 15, 29 percent (respectively 17 percent) living with a male (resp. female) caregiver that had a drug or alcohol problem, 10 percent (respectively 2

at different ages but otherwise display similar childhood trajectories. Another advantage of the Journeys Home survey is that it provides detailed respondents' histories with respect to outcomes that are relevant to multi-dimensional disadvantage: youth homelessness, incarceration, substance use, mental health conditions. This allows us to paint a detailed picture of how education can set people on different pathways.

There are good reasons to believe that education can be a lever to interrupt a cycle of multi-dimensional disadvantage. Indeed, staying longer in school can improve young people's cognitive and non-cognitive skills thereby increasing their chances of employment and income. Staying in school can also distract young people from activities with short-term rewards and long-term costs, such as engaging in substance use and criminal activities. There is clear evidence that early school-leaving is associated with our outcomes of interest, including some convincing attempts at estimating causal effects. This includes papers on: crime (Lochner and Moretti, 2004, Feinstein and Sabates, 2005, Machin et al., 2011, Lochner, 2020; Ward et al., 2021); substance use (Townsend et al., 2007; Van Ours and Williams, 2015 for reviews); homelessness (Cobb-Clark and Zhu, 2017); mental health (Grossman, 2015, Cutler and Lleras-Muney, 2006: Esch et al., 2014). But overall, the evidence is inconsistent ranging from null to large effects of early school-leaving. Importantly, this literature is not specifically on the most disadvantaged. With variations across papers in terms of the definition of early school-leaving, institutional settings, data used and identification strategies, it is hard to pinpoint the origin of differences in findings, thus leaving the question of the effect of early school-leaving for disadvantaged youth open for debate.

An important empirical challenge in identifying the potential benefits of extra schooling for disadvantaged youth is that correlations do not imply causation. Identifying causal effects is critical to policy design but is plagued by two main difficulties. Most of the outcomes of interest: (i) could also be the cause of early school-leaving; (ii) or could be simultaneously determined via the impact of external shocks, personality traits or other unobserved characteristics.[5]

To address possible threats to causal identification, we use a staggered difference-in-difference (DiD) framework to compare the outcome trajectory of respondents who left school at 18 years old or more (the control group) versus those who left "early" at 14, 15, 16 or 17 respectively (the treated group), before and after the event of interest (school-leaving).[6] Early school-leaving is defined as leaving school before age 18 because this corresponds to the age at which most young people finish high school (repetitions are extremely rare in Australia). Our

percent) living with a male (resp. female) caregiver that spent time in jail, 17 percent (respectively 38 percent) living with a male (resp. female) caregiver that was unemployed more than six months. Respondents also report extreme levels of violence during childhood with 57 percent reporting emotional abuse, 58 percent reporting physical violence and 25 percent reporting sexual violence (Scutella et al. 2013, McVicar et al, 2015).

[5] Multi-dimensional disadvantage is particularly hard to understand because there is an overlap of concomitant circumstances that increase the risks of staying in disadvantage, including low levels of education and employment and high exposure to violence, homelessness, substance use, crime. Past research using the JH survey has shown that correlations can be misleading and that they do not necessarily translate to causal effects (McVicar et al, 2015 and 2019; Moschion and Van Ours, 2021 and 2022).

[6] In our set-up everyone leaves school, and we compare the outcomes of treated and controls as in an event study, i.e. relative to the time of the event (school-leaving).

empirical strategy leverages recent developments in the methodological and empirical DiD literature (De Chaisemartin and D'Haultfœuille, 2020; Arhangelsky et al., 2021; Callaway and Sant'Anna, 2021; Cunningham, 2021; Goodman-Bacon, 2021; Wooldridge, 2021; Baker et al., 2022; Roth et al., 2023; de Chaisemartin and D'Haultfœuille, 2023). We account for heterogenous treatment effects across cohorts and time to correct for biases that arise from using units that were treated previously as control units when the timing of the treatment is staggered over time. We also use observed time-invariant covariates in an augmented inverse-probability weighting specification to improve the plausibility of the parallel trends assumptions across our many outcomes. The identifying assumption is that if respondents who left school early had not, their outcomes would have followed the same trajectory as respondents who did not leave school early, given that before leaving school the trajectories of the two groups were parallel.

Our findings show a clear gender pattern with early school-leaving being much more detrimental for males than for females. When males leave school between 14 and 17, they are at higher risk of experiencing youth homelessness, incarceration, and using cannabis daily and illegal street drugs weekly. The risk of incarceration and illegal street drugs use even increase longer term until at least age 25. Importantly, the older males leave school the milder those effects become, highlighting gains for every extra year of schooling. Effects for females are mostly insignificant except for females who leave very early (at 14 or 15) and short-term increases in the likelihood of homelessness and incarceration right after leaving school. Interestingly, we don't find any effect of early school-leaving on diagnosis of depression or anxiety disorder for either gender.

Compared to a standard difference-in-difference where a policy reform is exogenous to individual behaviour, our set-up differs because our event of interest, school-leaving, is ultimately an endogenous choice. By exploiting the relative timing of school-leaving and outcomes within a difference-in-difference framework (i.e. with a control group), we directly address concerns related to reverse causality and time-invariant unobserved heterogeneity.[7] Still, a bias could arise from shocks or behaviours simultaneously affecting both school-leaving and outcomes.

We provide additional evidence that time-varying unobserved heterogeneity is unlikely to drive our results, supporting a causal interpretation. First, despite significant correlations, our identification strategy eliminates all effects for females. Second, we run a falsification test on parental separation. Indeed, just like our outcomes, the timing of parental separation coincides with the age at which the child leaves school. In contrast to our outcomes, it is likely to affect education outcomes but less likely to result from school-leaving. Giving credence to our causal interpretation, significant correlations disappear in our difference-in-difference estimates and we find no "effects" of early school-leaving on parental separation. Finally, our results are robust to controlling for the uptake of other adverse behaviours. For example, the effect of early school-leaving on the use of illegal street drugs is robust to controlling for cannabis use, homelessness, incarceration and the diagnosis of mental health conditions. Any bias would therefore have to be gender-specific, unrelated to other adverse behaviours and parental separation.

[7] Estimating the impact of childbirth, another endogenous decision, Kleven et al. (2019) validate causal identification using an event study.

To address concerns around recall, we also show that our results hold for a subsample of younger respondents, whose responses are less likely to be affected by recall errors suggesting that our main conclusions are not driven by systematic errors affecting respondents' retrospective information (see for example Henry et al. (1994) and Moffitt et al. (2010)).

The set-up of this paper is as follows. In section 2, we present an overview of previous studies. We distinguish between studies on the returns to schooling and studies of the links between education and disadvantage. Section 3 describes the data we use in our analysis providing an overview of the main sample characteristics. In section 4, we discuss the set-up of our analysis and the main parameter estimates. Section 5 concludes.

2. Previous studies

Our paper is at the crossroad of the literature on the returns to schooling and the literature focusing on the evaluation of programs that aim at improving the life of the most disadvantaged.

2.1 The returns to schooling

Early studies mainly focused on the returns to schooling in terms of post-secondary education outcomes (level attained) and labour market outcomes (participation, income) (see Machin, 2006 for a review). While there is still some debate in this dense literature, it is fair to say that most of the evidence points to positive effects of education on labour market participation and income, despite some uncertainty around the magnitude of these effects, the mechanisms (skills upgrade vs signalling effects) and heterogeneity (Angrist and Krueger, 1991, Card 1993, Harmon and Walker 1995, Ashenfelter and Rouse 1998, Meghir and Palme, 2005, Grenet 2013; Lleras-Muney and Shertzer, 2015; Clark, 2023). Recently, Hofmarcher (2021) uses information about 37 compulsory schooling reforms in 23 European countries to study the effect of education on poverty finding large significant negative effects of additional years of schooling on the likelihood of living in poverty.

More recent studies extend the analysis of the returns to schooling beyond the labour market, demonstrating that taken together the returns to schooling are potentially substantial. In some studies, a relationship is established but not a causal connection. In other studies, there is a clear causal effect from education to pecuniary outcomes but not to non-pecuniary outcomes. Oreopoulos and Salvanes (2011) argue that one of the problems is that education is found to have a positive effect on income which in itself affects many life outcomes. Distinguishing between the direct effect of education on non-pecuniary outcomes and the indirect effect through higher income is non-trivial, although they emphasize that some outcomes are less likely to be affected through an income effect.

Nevertheless, there is some evidence that extra schooling translates into healthier and longer lives via better self-reported health, a healthier diet and weight, less cardiovascular problems, less reliance on disability pension and later deaths (Lleras-Muney, 2005; Oreopoulos and Salvanes, 2011; Van Kippersluis et al., 2011; Fletcher, 2015; Li and Powdthavee, 2015); happier lives with higher overall and family life satisfaction and more stable lives through lower rates of divorce, teen pregnancies and incarceration (Oreopoulos and Salvanes, 2011; deNew et al., 2021).

Of particular interest to our paper, the link between early school-leaving and our outcomes of interest has been established in a number of papers, some of which provide convincing evidence of plausible causal effects. Using the JH survey, Cobb-Clark and Zhu (2017) document a pattern from childhood homelessness and education to adult employment.[8] The literature on the effects of schooling on crime is more extensive and Lochner (2020) provides a recent literature review emphasising clear reductions in crime and incarceration for males who complete high school in many countries. He also documents some evidence for women. In Australia, a 2006 reform which reduced early school-leaving in Queensland has been shown to have decreased crime for males and females in the long term at the extensive margin (Beatton et al., 2018) but increased in-school violence from students with prior criminal records (Beatton et al., 2022). In terms of substance use, Townsend et al. (2007) provide a systematic review highlighting the connections between early school-leaving and different substances (tobacco, alcohol, cannabis, other illicit drugs). Beyond correlations, the evidence is quite inconclusive with some positive effects of early school-leaving on teen smoking and drinking (Jensen and Lleras-Muney, 2012) as well as cannabis use (Kogan et al, 2005) and some small/null effects (Van Ours and Williams, 2009, find insignificant effects on cannabis use for males but small effects for females). Testament to the extensive literature on the effect of education on health outcomes, three literature reviews provide complementary (albeit differing) conclusions. Based on an earlier set of papers, Cutler and Lleras-Muney (2006) present some causal evidence from primary and secondary schooling to health while Grossman (2015) concludes that more research is warranted to fully understand whether more schooling leads to better health. Zooming on mental health outcomes, Esch et al. (2014) concludes that early school-leaving can lead to the development of mental health issues (internalising disorders such as depression and anxiety).[9]

Much of the above literature contributed to distinguishing the causal impact of completing high school on outcomes from reverse causality and the effect of omitted confounding factors. Confounding factors are an important threat to causal interpretation given that education shares many of the same determinants as outcomes of interest (family background, school quality, personality traits). To overcome this challenge, economists often leverage a shock or natural variation to the level of schooling unrelated to other factors that determine outcomes. As a consequence, the effects identified in this literature mostly correspond to the local average treatment effect on the compliers, in other words the impact for the marginal student who was close to dropping out but did not because an invisible force pushed him over the line. This means

[8] Outside of economics, papers which study the predictors of homelessness often emphasize the role of education without taking a stab at causality (Brakenhoff et al., 2015; Nilsson et al., 2019; Giano et al., 2020; Batterham, 2021).
[9] These effects running from early school-leaving to outcomes do not preclude effects in the other direction. In fact, a number of the literature reviews mentioned above also document that early school-leaving can result from cannabis use (van Ours and Williams 2015); crime (Ward et al., 2020); mental health issues (Esch et al., 2014).

that while we made important progress on estimating causal effects, there is still a gap relating to the returns to schooling for non-marginal students and in particular the most disadvantaged. Data limitations to study a small marginalised group and the difficulties to find a suitable control group that displays similar disadvantage but pursued their education have hindered progress in this direction.

2.2 Education for the extremely disadvantaged

Complementary to the literature on the returns to education, another strand of economics literature specifically focuses on bettering the lives of very disadvantaged families. The objective of this literature is to identify programs that will support pathways out of poverty and intergenerational disadvantage. Given their broad objective, this literature focuses on evaluating the causal impact of the program under study rather than the sole effect of education. These programs are randomly assigned and generate changes in families' environments, which can involve a complete relocation of the family to more advantaged neighbourhood or just a move of school for the treated child. For example, the Moving to Opportunity program has generated a wealth of evidence about who and under what circumstances treated individuals benefit from relocation of the family.

Findings from various relocation experiments or quasi-experiments have shown positive long-term effects for the children in these households on education (secondary and post-secondary education) and labour market outcomes. These effects tend to be concentrated on children who moved before their teenage years and increase with the duration they are exposed to the new neighbourhood (Chetty, Hendren, and Katz, 2016; Chyn, 2018; Chetty and Hendren, 2018; Chyn and Katz, 2021). In relation to the research question of this paper, this literature also finds that growing up in a less disadvantaged neighbourhood reduces risks of adverse outcomes such as: arrests (Chyn 2018); incarcerations (Chetty et al. 2020); teenage pregnancies (Chetty and Hendren 2018). Other lottery-based programs that assigned disadvantaged students to better schools have found that treated students had better education outcomes, were less often arrested and incarcerated, and less likely to become pregnant as a teenager (Deming 2011; Dobbie and Fryer 2015). We are unaware of evidence as to whether relocations affect children's homelessness, substance use and mental health.

An issue with interpreting these findings is that these relocations involve much more than extra years of schooling. They also change the quality of the school they attend, the peers children are with at school and in the neighbourhood, their exposure to violence and poverty, their parents' health and wellbeing (Chyn and Katz, 2021). As a result, it is hard to tease out how much of the effects on long-term outcomes are due to extra years at school.

In our paper, we propose to shed light on this specific mechanism and whether extra years at school can improve the odds of children growing up in disadvantage (even without changing schools). In addressing this question, we use insights from this literature which points to the importance of the timing and specifically changes that occur earlier rather than later. Given that many disadvantaged children do not finish high school, we focus on estimating the impact of extra years of schooling in secondary school between 14 and 18 years old.

3. Data

3.1 Journeys Home

The sample for Journeys Home was drawn from Australian administrative data and covers a broad sample of the most disadvantaged population. The sampling base was the universe of income support recipients which is managed by a single entity in Australia – Centrelink. In May 2011 (when the sample was drawn), more than one in five Australian residents were receiving some kind of income support payment (such as childcare payments, rent assistance, disability benefits, unemployment benefits, and so on). From this population subset, the most disadvantaged income support recipients were identified using a two-step process – these people comprise the JH sample. First, about 70 percent of the JH sample was drawn from income support recipients who had been flagged by Centrelink as being homeless (N=581) or at-risk of homelessness (N=625). Second, the JH team used very detailed information about the histories of income support recipients and statistical modelling to estimate predicted probabilities of homelessness for all income support recipients that had not been flagged by Centrelink directly. The last 30 percent of the JH sample was drawn from the subsample of income support recipients whose predicted probabilities of homelessness were in the top 2 percent of all estimated probabilities (N=475, median predicted probability=12 percent) (Wooden et al. 2012). Six waves of data were collected every six months between September 2011 and May 2014.

JH respondents are disadvantaged along all standard socio-economic dimensions. Back of the envelope calculations presented in Moschion and Van Ours (2019) show that the JH sample corresponds to the lowest percentile of the Australian population in terms of multi-dimensional disadvantage. For example, compared to the Australian population JH respondents display lower levels of education (e.g. 39 vs 71 percent graduated from high school) and employment (20 vs 63 percent were employed at wave 1), higher levels of mental health conditions (e.g. 54 vs 12 percent had been diagnosed with depression), substance abuse problems (e.g. 39 vs 15 percent used illicit drugs in past 6, respectively 12 months) and incarceration (18 vs 2 percent) (See Scutella et al. 2013 for more details). JH respondents also display high levels of homelessness, but it is worth noting that only 25 percent of the 1,681 respondents were actually homeless at wave 1. This is one of the strengths of the JH survey as we can compare homeless respondents with similarly disadvantaged but housed respondents.

In this paper, we select the sample using the following restrictions. First, given our focus on the effect of the school-leaving age, we retain only respondents for whom the school-leaving age is available (N=1,628). Some of the younger respondents have not yet left school at wave 1, resulting in missing school-leaving age data for a significant proportion of those aged between 15 and 18 (between 9 and 17 percent are missing the school-leaving age), i.e. who will leave school older. This results in a sample of young respondents disproportionately representing respondents who drop out of school early. This issue subsides for respondents aged 19 at wave 1, with only 1 out of 80 19-year-olds not providing this information. Indeed, 99% of JH respondents have left school by age 19. Therefore, we also restrict the sample to respondents who were at least 19 years old at wave 1 (N=1,396).[10]

We define early school-leaving as leaving school before 18 since most young people in Australia finish high school at 18 years old (repetitions are extremely rare in Australia). Respondents who left school before 18 are therefore in the treatment group, and those who left from 18 onwards are in the never treated group. We restrict our sample to birth cohorts from which we have both treated and control individuals born in the same or the following year. Indeed, comparing treated respondents to control respondents born later and even much later would be misleading. We therefore drop respondents born between 1930 and 1947 for which no respondent left school at 18 or older. From 1948 onwards, there are only four non-consecutive birth cohorts with no treated respondents: 1949, 1952, 1959, and 1961 (N=1,377).

We define age-specific treatment groups before age 18 to study separately the effect of leaving school between 14 and 17 years old. Groups of respondents leaving school at each age until 13 are too small to analyse and are therefore removed from the analysis (N=1,298). Studying the effect of leaving school at 14, 15, 16 and 17 years old separately is important as for most of our respondents, school was compulsory until 15. This allows us to identify the effects of leaving before the compulsory age (at 14); at the compulsory age (at 15) and after (at 16 or 17). [11]

We follow the uptake of homelessness, incarceration, daily cannabis use, weekly illegal street drug use, depression, and anxiety disorder from age 12 to 25. We start at 12 years old to have at least two years pre-treatment for all treatment groups with the first treatment group leaving school at 14. This allows us to test the parallel trend assumption. We observe outcomes up to age 25, which allows us to track outcomes for 8-11 years after treated respondents have left school (depending on the age at which they left school between 14 and 17). The downside of extending the window of observation is that it increases the number of respondents for whom the observation of the outcomes is censored, i.e., missing because when last asked about the outcome they were younger than 25 and had not started yet. To compare results across outcomes, we further restrict the sample to respondents for whom all outcomes are available (N=954). This is the strictest restriction, and we provide evidence that our results are robust to releasing it.

[10] An alternative would have been to update the status in terms of school-leaving and all outcomes over the course of the survey. However this is not feasible as updates to school-leaving and incarceration status are not collected beyond wave 1, and information on the use of illegal substances and mental health conditions are not updated beyond wave 3. It is therefore easier to bring back the information as of wave 1.

[11] In Queensland, students used to start school earlier than in other States until 2008, so that many students from Queensland would finish high school at 17. We don't have information about the State in which respondents studied and our respondents are very mobile so their State of residence at wave 1 is not a very good proxy. In terms of our estimations, this will not affect the treatment effects at 14, 15 or 16 for which the respondents who left at 17 are in the not-yet treated group. For the treatment effect at 17, it implies that some leaving at 17 will have completed high school possibly underestimating treatment effects at this age (when comparing to those who left later at 18 or more). Results show that effects for those leaving at 16 are quite similar to those at 17 reducing possible concerns.

To follow the uptake of our outcomes of interest from age 12 to 25, we use the retrospective information collected in JH at different waves and reconstruct a balanced panel dataset. Specifically, we use the information collected about the age at which respondents first experienced: (i) homelessness and incarceration in wave 1; (ii) daily cannabis use, weekly illegal street drug use, depression, and anxiety disorder in wave 3. By relating the timing of school-leaving to the timing of the outcomes, we control for the sequence of events as well as individual fixed effects and age fixed effects to minimise concerns due to reverse causality and confounding factors.

The outcomes of interest are dummy variables that switch from zero to one at the age at which the respondent first experienced the relevant outcome. For homelessness, we use a broad definition of homelessness defined as having stayed in crisis accommodation, squatted in abandoned buildings, slept rough, lived in a caravan, cabin or mobile home, a hotel or motel or stayed temporarily with friends with no alternatives. This is a standard definition of homelessness in the international (e.g. O'Flaherty, 2019) and Australian literature (e.g. Van Ours and Moschion, 2019). This broad definition is of particular relevance here as all these housing options can follow early school-leaving and associated conflicts with caregivers. Note that only the overall age of onset for homelessness is available. We don't know when respondents experienced each of these housing conditions the first time. In the same way incarceration switches to one at the age at which the respondent first went to a juvenile detention centre, remand, or prison. Analysis of incarceration is warranted given results found by previous studies on the effect of education on crime. It is of particular interest to understand if leaving school early can precipitate incarceration of youth facing extreme disadvantage as they may have to resort to illegal behaviours to sustain themselves to a larger extent than less disadvantaged youth.

We also use the information on the age of onset for the use of illegal drugs and the diagnosis of mental health conditions retrospectively collected at wave 3. Respondents were asked about their use of cannabis on the one hand and other types of illegal street drugs (with a specification that this may include amphetamines, such as speed and ice, heroin, cocaine, ecstasy, and so on). In terms of mental health conditions, respondents were asked about whether they were ever diagnosed with anxiety and depression by a doctor or other health professional, such as a psychologist or psychiatrist and if so the age of the first diagnosis. As before these outcomes are dummies that switch to one at the age at which the respondent first experienced the outcome.

Our final balanced sample has 573 males and 381 females. Given the evidence that females and males react differently to leaving school at different ages (DeNew et al., 2021) and that the causes of homelessness, incarceration, substance use, mental health conditions vary by gender (McVicar et al., 2015; Johnson and Moschion, 2019; Moschion and Van Ours, 2022), we conduct the analysis separately by gender.

There are two aspects of the sample selection which may generate concern: selection into the JH survey and selection into our sample of analysis. The first potential issue relates to the fact that JH's sampling methodology mainly focused on housing disadvantage. This link between the sample selection and one of our outcome of interest may generate a bias in our estimates of the relationship between educational attainment and homelessness. This would occur if the sample was selected in a way that is also correlated with early school-leaving. That is, if the unobservables determining the selection into the JH sample are correlated with the unobservables determining our outcome (homelessness by age 25) in a way that relates to early school-leaving. Although this cannot be ruled out completely, such bias is unlikely to arise because while the selection into JH was based on lifetime homelessness, our paper focuses on youth homelessness. As a result, unobservables that differentiate the treatment and control group in our analysis specifically relate to childhood experiences and differ from what drives selection in the JH survey. In addition, we showed in Moschion and van Ours (2022) that youth homelessness was independent from what determined selection into the JH sample: the Centrelink flag, and a high predicted probability of homelessness. More precisely, "the rates at which people were flagged by Centrelink and the predicted probability of homelessness are similar for the respondents who experienced homelessness by age 30 compared with those who had not (flag rate: 71% vs 72%; predicted probability: 18% vs 15%)". A second potential source of bias relates to the selection of our sample of analysis and in particular to whether we sub-select specific types of JH respondents. Appendix A shows that being in our sample of analysis is not related to experiencing our adverse outcomes of interest before age 25.

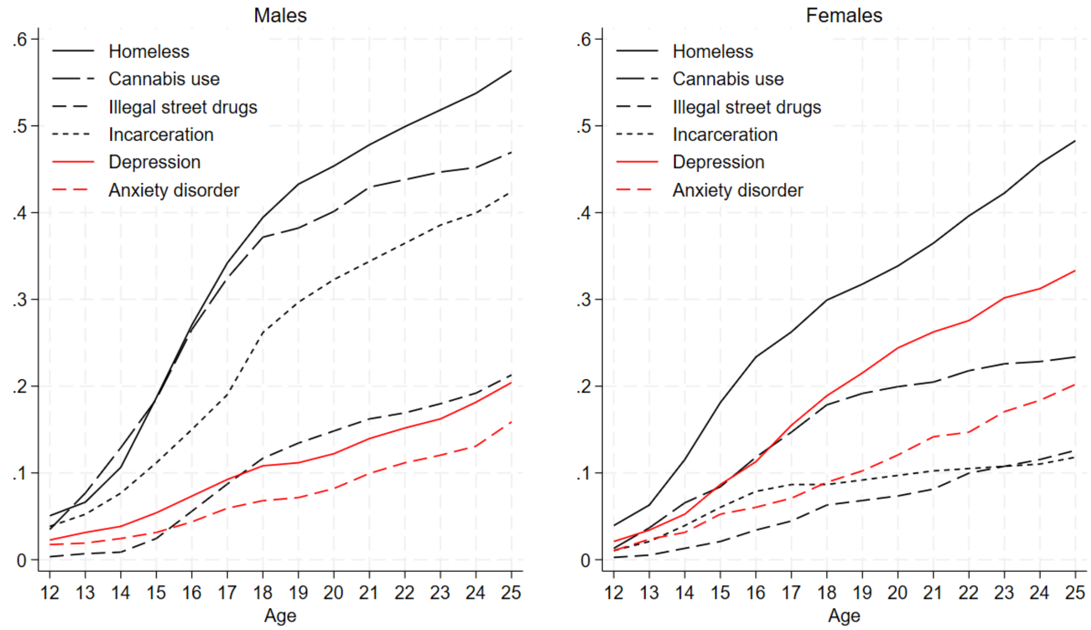
3.2 Sample characteristics

Our balanced sample of individuals from the JH survey is extremely disadvantaged and experiences adverse life outcomes far more often than the average Australians. Using information on the age of onset for our outcomes of interest (homelessness, incarceration, cannabis use, illegal street drug use, depression, and anxiety disorder), Figure 1 shows the lifetime prevalence of these outcomes between 12 and 25 years old. On the left-hand side, the figure for males shows that homelessness, cannabis use, and incarceration are the most common outcomes.

By age 25, 56% of males have experienced homelessness, 47% have used or are still using cannabis on a daily basis, and 42% have been incarcerated. The right-hand side figure shows a different pattern for females with mental health conditions being more common than for men. By age 25, one in three females in our sample has been diagnosed with depression, and one in five has been diagnosed with an anxiety disorder. Homelessness is also prevalent among females with almost half of the sample having experienced it by age 25. Daily cannabis use, weekly use of illegal street drugs, and incarceration are less common among females. Importantly, the prevalence rates are not only high but also increase progressively, with onsets both before and after the age at which respondents leave school. This makes JH the ideal sample to study whether early school-leaving precipitates (or not) the occurrence of these outcomes.

Figure 1 Lifetime prevalence of adverse life events by age

a. Males **b. Females**



Note:

Based on a sample of 573 males (8022 obs.) and 381 females (5334 obs.).

Figure 2 shows the lifetime prevalence of outcomes between ages 12 and 25 for different school-leaving ages (14, 15, 16, 17, and 18 or more). The top panel shows the figures for males, and the bottom panel shows the figures for females. For both genders, there is a clear pattern by which prevalence rates for homelessness, incarceration, and the use of illegal drugs are higher and increase faster for respondents who leave school before 18. Interestingly though the gaps in prevalence rates appear particularly high for men compared with women. For example, males who left school between 14 and 16 are more likely to face homelessness than those who left school from age 17. The difference between leaving school at age 17 or later on is small. By age 25, around seven out of ten males who left school at 14 have experienced homelessness, and around six out of ten males who left school at 15 or 16 have had that experience. This compares to less than half of the males who left at age 17 or later. Patterns are similar for women although the risk of homelessness appears lower for women who left school from age 16 compared with men.

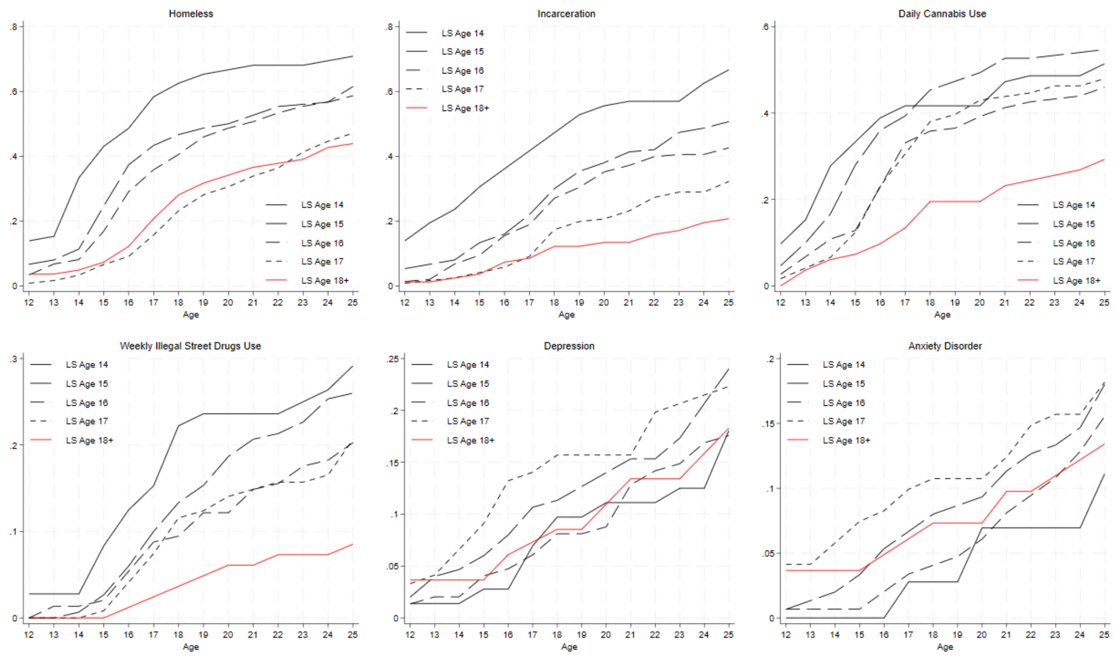
Incarceration displays a similar pattern with incarceration being more prevalent for males who left school between 14 and 17, while for females the increased prevalence of incarceration is especially clear for women who left school at ages 14 and 15.

Daily cannabis and weekly illegal street drug use similarly display high prevalence rates for males who left between 14 and 17, while for women the association appears stronger for females who left at 14 or 15 than for those who left from age 16. Interestingly, the use of daily cannabis appears much higher for females who left at age 15 than those who left at 14 or 16.

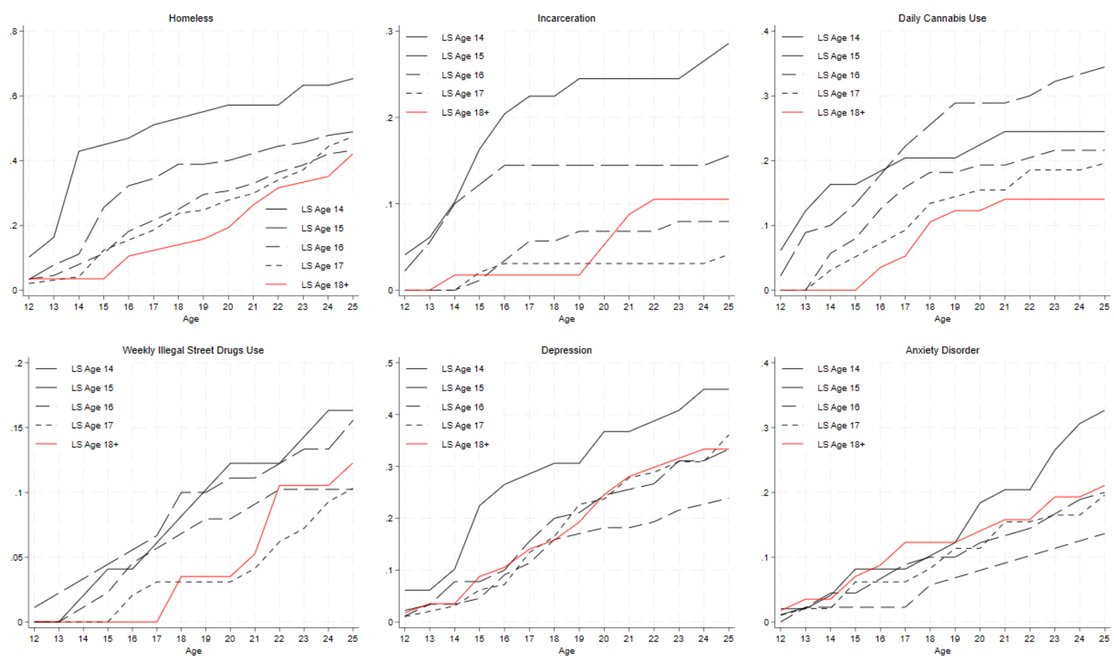
This pattern by the age of school-leaving is much less clear for mental health conditions, except for depression for females where those leaving school at 14 display much higher rates of depression than females leaving school from 18 years old.

Figure 2 Lifetime prevalence of adverse life events by age and age of school-leaving

a. Males



b. Females



Note: Based on a sample of 573 males (8022 obs.) and 381 females (5334 obs.).

Table 1 shows the prevalence rates by age 25 for the different adverse life events by the age of school-leaving. This confirms the correlations between the age of school-leaving and the prevalence rates of homelessness, incarceration, cannabis and street drugs use. The declines in the risk to experience adverse outcomes by school-leaving age are not gradual and vary across outcomes and gender. For males, there seems to be a turning point if they leave school from age 17 for homelessness and from age 18 for other outcomes. For females, the critical age is around 16. Overall, males who left school before 18 are 25pp more likely to have been incarcerated, 21pp more likely to have used cannabis daily, and 14pp more likely to have used street drugs weekly or experienced homelessness by age 25 than males who left school at 18 or later. Females who left school early are 11pp more likely to have used cannabis daily by age 25. For depression and anxiety disorder, no pattern appears for males, while there seems to be a u-shape relationship for females for whom the lowest rate of diagnosis of a mental health condition appears for those who left school at age 16.

Table 1. Lifetime prevalence of adverse life events by age 25 and age of school-leaving (%)

School-leaving	Early						Early-18+
Age of school-leaving	14	15	16	17	14-17	18+	
a. Males							
Homeless	71	59	61	47	58	44	14 ***
Incarceration	67	51	43	32	46	21	25 ***
Cannabis daily	51	54	46	47	50	29	21 ***
Street drugs	29	26	20	21	23	9	14 ***
Depression	18	24	18	22	21	18	3
Anxiety disorder	11	18	16	18	16	13	3
Observations	72	150	148	121	491	82	
b. Females							
Homeless	65	49	43	47	49	42	7
Incarceration	29	15	8	4	12	11	1
Cannabis daily	24	34	22	20	25	14	11 **
Street drugs	16	16	10	10	13	12	1
Depression	45	33	24	36	33	33	0
Anxiety disorder	33	20	14	20	20	21	-1
Observations	49	90	88	97	324	57	

Note: ***(**) Difference significant at a 1% (5%) level.

Despite these clear patterns, the descriptive evidence tells us very little about causality. It is a reasonable assumption that leaving school early may increase the risks of facing adverse life events. But it is also reasonable to conjecture that the effect goes the other way and that life events lead to respondents leaving school early. A number of confounding factors could also interfere with the relationships, such as difficult childhood circumstances (disrupted families, sexual and physical violence) or personality traits (risk aversion or time preferences) for example. Importantly, as shown in Figure 2 the occurrence of adverse life events happen both before and after respondents have left school: indeed their prevalence increases before and after the school-leaving age. This provides variation in the sequence of events across respondents that we can leverage in the econometric analysis. We turn to a difference-in-difference framework to shed some light on the potential effect of early school-leaving on adverse life events.

4. Empirical analysis

4.1 Set-up of the analysis

The nature of our research question can be framed within a Difference-in-Difference framework to investigate how leaving school early (compared to leaving school later) affects subsequent outcomes. The treatment group consists of early school-leavers and the control group consists of those who have not yet left school at this age. Respondents leaving school at 18 or more are the never-treated group. By observing respondents from 12 to 25, we observe them pre-treatment, i.e. before they leave school, and post-treatment, i.e. from the age at which they leave school onward. The difference in outcomes between the two groups pre-treatment represents the baseline difference that is unrelated to leaving school. The difference between the two groups post-treatment includes the baseline difference and the additional treatment effect of leaving school early. The treatment effect we are seeking to identify corresponds to the difference between the pre-treatment and the post-treatment difference. By controlling for the sequence of events pre- and post-treatment and adjusting for potential confounding factors that generate differences between the treatment and control group, DiD estimation identifies the causal effect of the treatment.

In a simple two group – two periods set-up identification of the treatment effect relies on two main assumptions. The first assumption is the parallel trend assumption: in the absence of the treatment unobserved differences between the treatment group and the control group would have remained constant. This provides us with the counterfactual outcome that we would have observed for early school-leavers if they had not left school early. The second assumption is that individuals do not anticipate the treatment and change the trajectory of their outcomes pre-emptively before actually leaving school. If the two main assumptions hold, a DiD-analysis can be implemented using two-way fixed effect (TWFE) regressions:

$$Y_{it} = \alpha_i + \beta_t + \gamma\, D_{it} + \varepsilon_{it} \qquad (1)$$

where Y_{it} is the outcome of interest for individual *i* at age *t* and D_{it} is a dummy variable which is equal to 1 for the early school-leavers once they left school and 0 otherwise. Furthermore, α_i represent individual-specific fixed effects, β_t are age-specific fixed effects and ε_{it} are idiosyncratic and time-varying unobservables. Under the parallel trends assumption and no anticipation, γ measures the impact of early school-leaving on outcomes.

However, this simple DiD set-up yields biased estimates of the treatment if there are more than two periods and units experience the treatment at different points in time or ages (staggered treatment) and if the treatment effects are heterogenous across groups. Goodman-Bacon (2021) shows that staggered TWFE estimates are weighted averages of all possible two-groups/two-periods DiD estimators. The intuition is that the estimated treatment parameter is a convex weighted average of DiD comparisons between pairs of groups in time periods in which one group changed its treatment status and the other group did not. As a result, these regressions may have a “bad comparison” problem as groups that are already treated are used as controls for groups that are treated later. The use as a control group of individuals who were treated in earlier periods may lead to biased parameter estimates as the outcomes of this control group may partly reflect treatment effects (see for further details Appendix B1). De Chaisemartin and D’Haultfœuille (2020) further provide an example showing that some of the weights that constitute the weighted average of separate treatment effects can be negative. Using simulations, Baker et al. (2022) provide evidence that it is the combination of the staggered treatment and time-varying treatment effects that generate bias in the TWFE estimates. In fact, they find that DiD estimates are unbiased with either: (i) a single treatment period even with heterogenous treatment effects over time; (ii) or a staggered treatment with homogeneous treatment effects over time.

Since school-leaving can occur at various ages, we are in the context of a staggered treatment. In addition, Figure 2 suggests that the effect of early school-leaving may be heterogenous with prevalence rates varying with the age of the individual. As a result, a simple TWFE approach may lead to biased estimates.

To address the issues that arise with a staggered treatment, several alternative estimators have been proposed in the literature. Roth et al. (2023), de Chaisemartin and D’Haultfœuille (2023) and Callaway (2023) give an overview of various DiD estimation procedures. We follow Callaway and Sant'Anna (2021) and produce two sets of estimators which allow to avoid “bad comparisons” by using only never-treated and not-yet treated units as controls. The simplest ATET we estimate is a weighted average of the ATET for various times and ages. This deals with biases arising because of the staggering of the treatment and potential heterogenous treatment effects. We refer to this as the regression adjusted specification (RA). Then we produce a more complex estimator which uses observed covariates to control for pre-treatment differences between control and treated units by weighting observations based on their likelihood of being treated.[12] The likelihood of treatment is based on a propensity score which is calculated using observed characteristics. We use characteristics which contribute to determining the treatment i.e., the school-leaving age: dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent’s caregivers were separated in the previous period. We refer to this specification as the augmented inverse-probability weighting specification (AIPW). In Appendix B.2 we present and discuss the results from alternative estimation procedures, including a synthetic DiD (Arhangelsky et al., 2021), an extended TWFE approach based on Wooldridge (2021) and stacked estimates (discussed in Cunningham (2021) and Baker et al. (2022)).

[12] Estimates following these procedures can be obtained using the XTHDIDREGRESS command in STATA18. The command produces the simplest ATET (regression adjustment, RA) and the more complex estimator using covariates (augmented inverse-probability weighting, AIPW). In the AIPW procedure, weights are constructed based on the inverse of the propensity scores, which represent the likelihood of receiving treatment given observed covariates.

In terms of data structure, the staggering (i.e. respondents leaving school at different ages) implies that the age and the timing of the treatment do not coincide. In addition, a specificity of our set-up is that everyone leaves school, the treated, the not-yet treated and the never-treated. To conduct the analysis, we transform the timing of the treatment from the school-leaving age to a time-to-event, i.e., number of years before and after leaving school: time-to-event = school-leaving age - age. This allows us to identify the effect of school-leaving relative to the time since leaving school, as in an event study. In our case *t=0* represents the time at which respondents leave school.

4.2 Results

4.2.1 Main average treatment effects on the treated

Table 2 shows our main parameter estimates of the effects of early school-leaving on adverse life events. Panel a shows the estimates for males, panel b for females. By way of exploratory analysis the first column shows OLS parameter estimates to establish simple correlations between school-leaving and adverse life outcomes. For males and females, early school-leaving significantly coincides with homelessness, incarceration and the use of cannabis daily. For males the effects are particularly large with a probability of experiencing those adverse life events between 17 and 22 percentage points larger for respondents who left school early after they have left school. For females, the magnitude of these associations varies much more ranging from 6 percentage points for incarceration to 17 percentage points for homelessness.

Table 2 Main parameter estimates of the effect of early school-leaving

	OLS	TWFE		RA		AIPW	
	(1)	(2)		(3)		(4)	
a. Males	ATET (SE)	ATET (SE)	PT	ATET (SE)	PT	ATET (SE)	PT
Homeless	0.171(0.039)***	0.083 (0.027)***	^^	0.090 (0.041)**	^	0.068 (0.042)	
Incarceration	0.189(0.031)***	0.060 (0.022)***	^^^	0.143 (0.030)***		0.128 (0.033)***	
Cannabis daily	0.201(0.036)***	0.120 (0.024)***		0.116 (0.035)***		0.099 (0.036)***	
Street drugs weekly	0.086(0.020)***	0.031 (0.017)*	^^	0.084 (0.024)***		0.086 (0.023)***	
Depression	0.006(0.026)	0.002 (0.015)		0.001 (0.024)	^	-0.000 (0.024)	
Anxiety disorder	-0.004(0.023)	0.010 (0.013)		0.017 (0.021)		0.015 (0.021)	
b. Females							
Homeless	0.166(0.042)***	0.083 (0.028)***	^^	0.068 (0.041)*	^^^	0.051 (0.039)	^^^
Incarceration	0.058(0.026)**	-0.004 (0.014)	^^^	0.008 (0.023)	^	0.018 (0.020)	^
Cannabis daily	0.096(0.034)***	0.029 (0.023)	^^	0.015 (0.037)	^^^	-0.030 (0.045)	^^^
Street drugs weekly	0.026(0.022)	-0.001 (0.015)		0.004 (0.026)		-0.012 (0.029)	
Depression	0.019(0.041)	0.003 (0.027)		0.008 (0.040)		-0.032 (0.039)	
Anxiety disorder	-0.017(0.035)	-0.021 (0.020)		-0.005 (0.031)	^	-0.001 (0.029)	

Notes: Based on a sample of 573 males (8022 obs.) and 381 females (5334 obs.). ATET = Estimate of the average treatment effect on the treated with the not-yet treated used as control units; SE = standard error clustered by individual. PT: parallel-trends test with H0: the treatment effects in all the pre-treatment periods are zero. The OLS estimates include no explanatory variables except for age fixed effects. The weights for the AIPW estimates are built using: dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent's caregivers were separated in the previous period. *** (**,*) and ^^^(^^,^) indicate significance at a 1% (5%, 10%) level.

The use of illegal street drugs weekly is associated with early school-leaving but only for males and the magnitude is smaller than for other life events (9 percentage points). Depression and anxiety are not related to early school-leaving for either gender. Controlling for time-invariant characteristics which may co-determine the school-leaving age and the onset of adverse life events hardly affects the raw correlations (Appendix Table B.1). The magnitude of some parameter estimates is marginally reduced but the general pattern is very much the same.

Column 2 onwards present DiD estimates which control for time and individual fixed effects. We first present estimates using a traditional TWFE approach in column 2 to get a sense of the size of the bias in the OLS arising from time-invariant unobserved differences between the treatment and control group. Point estimates are much reduced in size indicating that early school-leaving probably share some of the same drivers as our outcomes of interest and that in some cases the outcomes may be the cause rather than the consequence of early school-leaving. Nevertheless, for males, early transitions out of school significantly increase the likelihood of transitions into homelessness, incarceration and the use of both cannabis daily and illegal street drugs weekly. In contrast, most effects disappear for women for whom early transitions out of school only increase the likelihood of transitions into homelessness.

As discussed in the previous section, TWFE may be biased in the context of a staggered treatment and heterogenous treatment effects. In appendix B2 we discuss the traditional TWFE in more detail applying the Bacon-decomposition showing that the bias in the traditional TWFE-estimates comes from `bad comparisons', i.e. using early treated units as controls. This will affect TWFE estimates if the 'bad comparisons' represent a large proportion of the control group and if the estimates for that group differ significantly from estimates using other control units. In our case, the TWFE appear to be biased for males' incarceration and use of illegal street drugs (Table B.2). In these cases, the early treated units tend to display higher prevalence of adverse outcomes thereby biasing the effect of leaving school early towards zero.

To adjust for these biases, we present results from a regression adjusted specification in column 3 which eliminates 'bad comparisons'. Indeed, these effects represent an average of individual effects at different ages and for different timings of the treatment in comparison to the not-yet treated. As a result, they identify the effect of leaving early at a specific age compared to leaving later, i.e. leaving at 14 vs leaving from 15 onwards, leaving at 15 vs leaving from 16 onwards… leaving at 17 vs leaving from 18 onwards. Results in column 3 suggests that on average leaving school early increases the risk of homelessness by 9pp for males and 7pp for females, and incarceration, cannabis daily and street drugs weekly for males by respectively 14, 12 and 8pp. These estimates are mostly similar to the TWFE estimates with a few noteworthy differences. For males, the effects of leaving school early on incarceration and the use of illegal street drugs weekly are substantially larger. This is consistent with findings from the Bacon-decomposition.

An issue with estimates in column 3 is that six out the twelve estimates appear to have non-parallel trends pre-treatment, i.e. the difference between the treated and control units in some pre-treatment period diverge from the average difference between treated and control units across all pre-treatment periods.[13] Specifically, the parallel trend assumption is violated for males for homelessness and depression and for females for homelessness, incarceration, cannabis daily and anxiety disorder. As a result, it is unclear how the significant positive effects of early school-leaving on homelessness for both genders should be interpreted.

To get a clearer view of the outcomes that are affected by early school-leaving, we use an AIPW model to test whether the parallel trends assumption holds after conditioning on observed covariates (dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent's caregivers were separated in the previous period). After conditioning, all estimates for males have parallel trends pre-treatment. For females three out of the six outcomes studied display parallel trends pre-treatment, and two out of the three that don't relate to non-significant effects that are far from any commonly used significance level (incarceration and cannabis daily). The only exception is homelessness for females which displays non-parallel trends and is insignificant, but still of a meaningful size. It is hard to definitely conclude whether early school-leaving increases homelessness for females. Overall, the parallel trend assumption holds for most outcomes, so we focus on the AIPW specification in what follows.

In terms of magnitude, the differences in ATET between the last two columns are small. On average leaving school early increases males' risk of homelessness by 7pp (borderline significant), incarceration by 13pp, cannabis daily by 10pp and street drugs weekly by 9pp. It is interesting to note that even in a population that is at-risk of becoming homeless staying longer in school reduces the probability of becoming homeless at a young age. There is a clear gender gap with effects for females being mostly insignificant, small in magnitude and inconsistent in signs (despite some uncertainty for the effect on homelessness). Effects of early school-leaving on depression and anxiety disorder are not significantly different from zero for either gender.

To further test the reliability of our AIPW estimates, we leverage other types of corrections that have recently been developed. In Appendix B.3 we present and discuss the results from three alternative estimation procedures. First, we present results using an extended TWFE approach based on Wooldridge (2021) that accounts for heterogenous treatment effects by saturating the model with interactions between the treatment and cohort-time dummy variables. Second, we present results from a synthetic DiD (Arhangelsky et al., 2021) which allows heterogenous treatment effects and uses reweighting and matching to make the pre-treatment trends of treatment and control groups similar (instead of just parallel). Finally, we present stacked estimates (Cengiz et al, 2019) which takes a different approach by creating a sub-dataset for each specific event (i.e. each school-leaving age in our context) retaining only the treated and control units that are relevant to that specific event. This then allows to run a standard TWFE specification on the full dataset which contains the stacked sub-datasets.

All of these alternatives aim to tackle the same issue of a staggered and heterogenous treatment from a different perspective. It is therefore interesting to compare the results they

[13] We test whether treatment effects in all pre-treatment periods are equal to zero. If so, the parallel trends assumption holds in pre-treatment periods.

produce. Results across all three are extremely similar to each other and to our AIPW specification. Early school-leaving increases males' likelihood of experiencing homelessness, incarceration, cannabis daily and illegal street drugs but has no effect on the diagnosis of mental health conditions. For females the only outcome which appears to be affected by early school-leaving is homelessness. Estimates for all other outcomes are far from conventional significance levels. If anything, our AIPW produces more conservative estimates which tend to be smaller in magnitude. Importantly estimates on homelessness appear significant for all three alternative models and both gender.

4.2.2 ATET by duration since school-leaving

Our empirical strategy to account for heterogenous effects over time allows us to break our overall average treatment effects in yearly effects from the year in which respondents leave school and up to 11 years after leaving school.[14] Figure 3 shows these yearly treatment effects, in panel a for males and panel b for females. Consistently with ATETs reported in table 2, early school-leaving increases males' likelihood of experiencing homelessness, incarceration, and using cannabis daily and illegal street drugs weekly. The effects on homelessness and cannabis daily kick-in straight when leaving school and reach a plateau within the first two years of around 7pp for homelessness and 10pp for daily cannabis use. In contrast, the effects on incarceration and illegal street drugs increase over time to reach 17pp and 10pp respectively seven years after leaving school. For depression and anxiety disorder the treatment effect of early school-leaving is close to zero over the whole period. For females, the ATET is close to zero over the whole period for most outcomes except for homelessness and incarceration which increase in the first few years after leaving school and later on disappear. Looking at the estimates pre-treatment suggests that the non-parallel trend in the average effect for homelessness appears from the estimate two years pre-treatment which is at odds with other pre-treatment periods, rather than a clear pre-treatment trend.

4.2.3 ATET by age of school-leaving

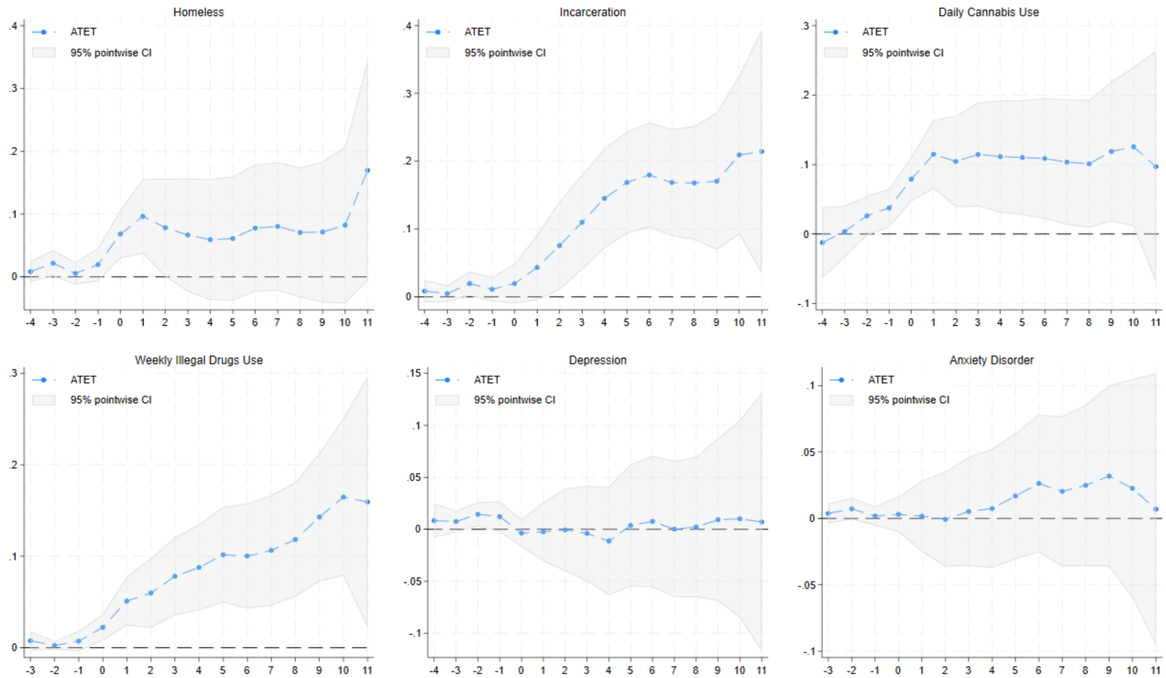
Another source of heterogeneity arises directly from the staggered treatment, i.e. the fact that respondents leave school at different ages. As suggested by Figure 2, leaving at age 14, 15, …, 17 may affect adverse life outcomes differently. Table 3 shows AIPW estimates of the ATET by age of early school-leaving, in panel a for males and panel b for females.

Across the board, the effects of early school-leaving appear particularly large for young people who left school at 14 or 15. For example, the largest effects for males suggest that leaving school at 14 increases the risk of homelessness by 19pp and leaving school at 15 increases the risks of: incarceration by 15pp, of cannabis daily by 14pp, and of illegal street drugs weekly by 11pp.

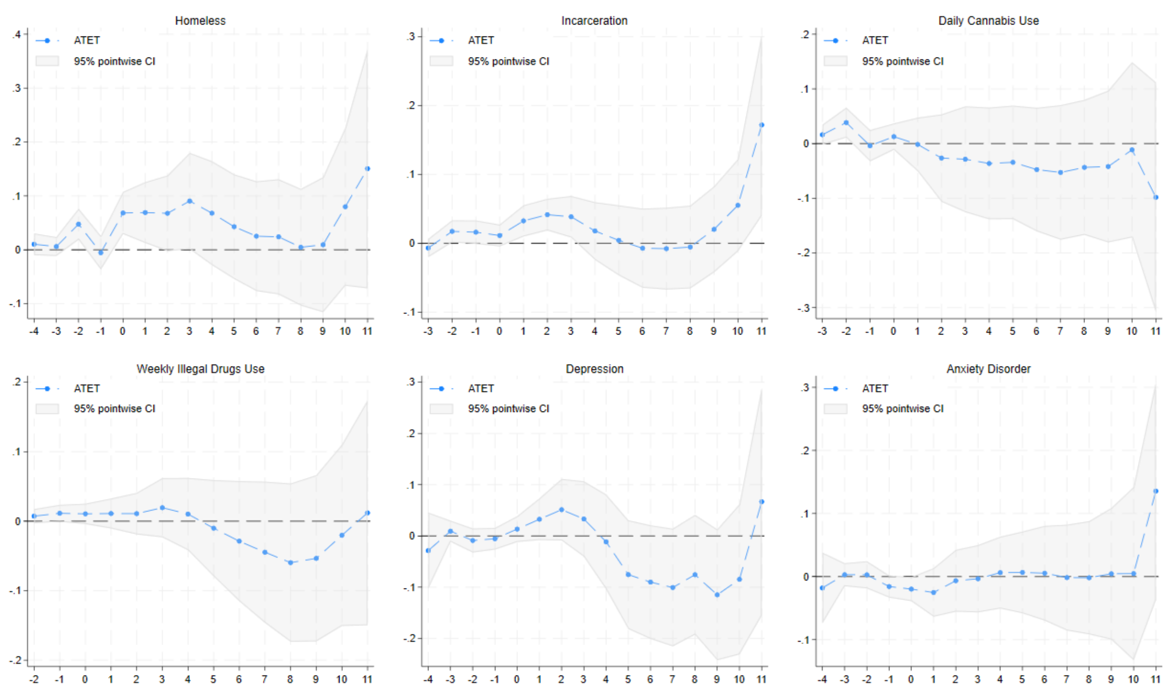
[14] Note that from t=7, the ATET progressively excludes respondents who left school later as they are over 25. In year 7, the ATET corresponds to the effect for respondents who left between 14 and 18 (as those who left at 19 are 26 7 years after leaving school); in year 8 to those who left between 14 and 17; …and in year 11 to those who left at 14. Similarly in years -4 and -3, the ATET corresponds to the effect for respondents who left respectively from 16 onwards and 15 onwards. In other words, only estimates shown between t=-2 and t=6 are based on a fully balanced panel. Estimates based only on the fully balanced panel produce similar results (see appendix B.5.5.)

Figure 3 AIPW estimates by duration since school-leaving

Males



Females



Note: Based on a sample of 573 males (8022 obs.) and 381 females (5334 obs.). Graphs created using the AIPW specification to display: ATET = Estimate of the average treatment effect on the treated with the not-yet treated used as control units; CI = 95% confidence interval. The weights for the AIPW estimates are built using: dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent's caregivers were separated in the previous period.

For females, leaving school at 14 increases the risks of: homelessness by 25pp and incarceration by 13pp. Even when they leave school really early, the risk of illegal drug use is not increased for females. When young people leave at 16 or 17 years old, the effect of leaving school early tend to be smaller in magnitude but some effects remain significant. Indeed, leaving school at 16 still increases the risk of incarceration, cannabis daily and illegal street drugs for males, but for those who leave at 17 only the risks of incarceration and illegal street drug use remain significant (resp. 11 and 6pp). Leaving school from the age of 16 does not appear to affect any outcomes for females, or homelessness for males.

Table 3 AIPW estimates effects of early school-leaving by age of school-leaving

a. Males	Age 14	Age 15	Age 16	Age 17
Homeless	0.186 (0.070)***	0.070 (0.054)	0.030 (0.055)	0.022 (0.053)
Incarceration	0.132 (0.066)**	0.150 (0.044)***	0.115 (0.045)**	0.110 (0.036)***
Cannabis daily	0.070 (0.069)	0.141 (0.047)***	0.100 (0.047)**	0.057 (0.046)
Street drugs weekly	0.095 (0.050)*	0.108 (0.030)***	0.062 (0.029)**	0.058 (0.030)*
Depression	0.010 (0.034)	0.022 (0.030)	-0.018 (0.034)	-0.007 (0.024)
Anxiety disorder	-0.001 (0.025)	0.034 (0.026)	0.010 (0.030)	0.005 (0.025)
b. Females				
Homeless	0.253 (0.077)***	0.103 (0.062)*	-0.070 (0.062)	-0.004 (0.046)
Incarceration	0.131 (0.053)**	0.018 (0.023)	0.011 (0.030)	-0.044 (0.022)
Cannabis daily	-0.043 (0.068)	0.028 (0.057)	-0.064 (0.068)	-0.044 (0.055)
Street drugs weekly	0.041 (0.041)	-0.003 (0.036)	-0.020 (0.039)	-0.047 (0.033)
Depression	0.088 (0.070)	-0.116 (0.052)**	-0.059 (0.055)	0.010 (0.052)
Anxiety disorder	0.078 (0.046)*	-0.023 (0.040)	-0.028 (0.039)	-0.001 (0.037)

Notes: Based on a sample of 573 males (8022 obs.) and 381 females (5334 obs.). See Table 1 for the number of observation by age of school-leaving. ATET = Estimate of the average treatment effect on the treated with the not-yet treated used as control units; SE = standard error clustered by individual. The weights for the AIPW estimates are built using: dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent's caregivers were separated in the previous period. *** (**,*) indicates significance at a 1% (5%, 10%) level.

4.3 Sensitivity analysis

4.3.1 A falsification test using parental separation

Appendix B.4 shows parameter estimates of the effect of early school-leaving on another adverse life outcomes: parental separation. Although only suggestive, this reinforces the plausible causal interpretation of our findings. Indeed, Table B.4.1 shows that parental separation is correlated with early school-leaving: transitions out of school tend to coincide with parental separation. The magnitudes are large (11 percentage points for males and 14 percentage points for females), significant at 1% and of comparable magnitude to the other adverse life events in Table 2. Figure B.4.1 shows that the rates of parental separation are much higher and progress faster for children who leave school early compared to those who leave from 18 onwards. While it appears possible that parental separation leads to children leaving school early or that a shock leads to both parental separation and children's early school-leaving, it is harder to imagine that children's early school-leaving would by itself lead to parental separation. Accounting for the sequence of events and time-invariant unobserved heterogeneity in our staggered DiD framework is sufficient to completely eliminate these correlations. We find no significant effect of early school-leaving on parental separation for either gender and the magnitudes of these estimates is more than seven times smaller.

4.3.2 Using different samples for each outcome

So far, our analysis is based on a balanced sample of 573 males and 381 females for whom we have complete information about when they experienced all adverse life events. The main advantage of the restricted sample is that all estimation results are based on the same groups of males and females. The disadvantage is that we might lose many observations for some life events simply because information about other life events is missing. To investigate how sensitive our main findings are to this aspect of sample selection we redid the main analysis for every life event on a separate sample. For some life events this increases the number of observations substantially: information on the onset of homelessness is available for 737 males and 535 females (Table B.5.1). The smallest samples are for illegal street drugs use for which there is information for 620 males and 420 females. Table B.5.1 shows the AIPW estimates of early school-leaving on the adverse life events based on the different samples for each outcome. The general pattern of the point estimates is very similar to results in Table 2, column (4). The only differences are new significant effects on homelessness for males (which is borderline significant in Table 2) and incarceration for females. Overall, our main findings are not driven by the use of a balanced panel across outcomes.

4.3.3 Varying the sample with respect to the age of respondents

In selecting our sample of analysis, we have applied a number of restrictions related to the age of respondents. In what follows, we test the robustness of our results to these restrictions. First, our analysis is based on retrospective information raising a concern around the accuracy of respondents' answers. JH asked respondents about their school-leaving age and the age they were the first time they experienced each of the adverse life events. Older respondents may find it more difficult to remember exactly the timing of events. To investigate the extent to which recollection errors may bias our main findings we rerun our main analysis for a sample of younger respondents who are 45 at most at wave 1. The parameter estimates are presented in Table B.5.2. Although the precision of the estimates is lower than before, the point estimates are very similar. The only differences are the significant effects of early school-leaving on homelessness for males and incarceration for females. (at 10%-level). This could be due to older female respondents not remembering the age of first incarceration accurately or younger cohorts of female respondents being more likely to be incarcerated after leaving school early.

Second, we release the restriction to respondents born from 1948 onwards. Results presented in Table B.5.3 show that 12 males and 5 females are added to the sample, with no noticeable change in the AIPW estimates. Finally, we restrict the sample to respondents who were at least 25 years old in wave 1, losing 11 males and 4 females. This removes any possible effect stemming from the censoring of outcomes for respondents who are between 19 and 24 at wave 1 and have not yet experienced all the outcomes. In the baseline these are removed from the sample as they have some outcomes missing while this is not the case for the 15 respondents who have experienced all the outcomes by wave 1. Results reported in Table B.5.4 are very similar to the main results, apart from the effect on homelessness being significant at 10% for males.

As discussed in the results section our main estimates are based on a panel which is not fully balanced in the very early and late years of our observation window. Indeed, only a smaller sample can be observed three and four years prior to school-leaving and seven years or more after school-leaving. If particularly small or large effects arise in those years as a result of sample selection, they may affect the overall ATET. Table B.5.5 re-estimates our preferred model, restricting the observation window between -4 and 6 years after school-leaving.[15] The parameter estimates are close to those presented in Table 2. For females all parameter estimates are insignificantly different from zero. For males the effects on incarceration, cannabis use and illegal street drugs use are highly significant while the effects on the other outcomes are insignificantly different from zero.

4.3.4 Varying the treatment and control groups

Other decisions affect the composition of the treated and control groups. First, in our AIPW estimates we chose to include all units that are not yet treated in the control group. This means that the control group will change as the age of school-leaving increases. Indeed, when estimating the ATET of leaving school at 14, those who leave school at 15 are in the control group, but they are not when estimating the ATET of leaving school at 15. Changing the control group across our mini-experiments may affect the estimates especially as we have found that ATET varied with the age of school-leaving. Table B.5.6 reports results when the control group only includes the never treated units (who leave school at 18 or more). This doesn't alter our main results.

We have excluded from our sample the 31 males and 31 females who left school before 14 as: (i) these groups are too small to study the effect of their specific age of school-leaving, (ii) they could have had very particular trajectories that would not match the control group; (iii) some have a school-leaving age that appear very young. Re-including them does not alter our estimates (Table B.5.7), but they do make the pre-trends unparallel for males for three outcomes, suggesting that this group may indeed have had a different start in life to other respondents.

Finally, we check whether the results change if we define the group of respondents that are never treated as those who leave school at 19 or more (instead of 18 or more). This moves part of the control group in the treatment group and increases most estimates, making the effects on homelessness and cannabis daily significant for women (Table B.5.8). By increasing the gap between treated and control units, it also yields many un-parallel trends pre-treatment.

[15] To have a fully balanced panel, we removed from our sample 8 males and 4 females who left school at age 20 or later.

4.3.5 Multiple hypothesis testing

In our analysis, we investigate the effect of early school-leaving on the prevalence of six adverse life events, separately for males and females. This may induce a multiple testing problem by which significant coefficients emerge by chance even if there are no real effects. Indeed, simultaneously testing S true null hypotheses at level α implies that on average αS are falsely rejected. To investigate whether we have a multiple testing problem, we use the Bonferroni method. If $p_1, p_2, \ldots, p_S$ are the unadjusted *p*-values, the Bonferroni-adjusted *p*-values are defined as $p^b_i = min(1; Sp_i)$. The results are reported in Table B.5.9 (Bonferroni, 1935; Clarke et al., 2020). As shown, the significance of the effects on incarceration, cannabis use and illegal street drugs use is reduced but still present. [16]

4.3.6 Exploring possible mechanisms

Multi-dimensional disadvantage implies extensive overlap between the marks of disadvantage, in this case the age of school-leaving and our adverse life events. It is possible that the effect of early school-leaving on a specific outcome is in fact driven by the impact of early school-leaving on another outcome. To explore this possibility we re-run our main estimates controlling for other outcomes in the previous period. Results indicate that most estimates remain unchanged (Table B.6.1): early school-leaving has a direct impact on incarceration, cannabis daily and weekly street drugs for males. For instance, the impact on street drugs does not operate via the impact on cannabis. In contrast, the effect of early school-leaving on homelessness is much reduced for both genders, indicating that this effect operates indirectly. Detailed analysis in which we include lagged outcomes individually in the homelessness equation indicates that the effect of early school-leaving on homelessness operates via incarceration and cannabis daily (the effect of early school-leaving on homelessness is respectively reduced to 0.043(0.041) and 0.056(0.042) when introducing these lagged outcomes). This matches results from our earlier papers on substance use in which we find that for males cannabis daily triggers first homelessness episodes, but illegal street drugs do not (McVicar et al, 2019). Consistently, the current findings suggest that early school-leaving leads to cannabis and later homelessness while the effect on illegal street drugs appears independent from the effect on homelessness. Also aligning with previous evidence (Moschion and Van Ours, 2022), while the onset of depression increases the likelihood of first homelessness episodes for males and females, in our context where early school-leaving does not affect diagnosis of depression, depression does not appear as the pathway from early school-leaving to homelessness.

[16] The reference to Bonferroni (1935) is from Clarke et al. (2020). Apparently, the Italian mathematician Carlo Emilio Bonferroni introduced this line of thinking without proposing it as a correction for multiple hypothesis testing. The Bonferroni method is quite conservative. Less conservative methods to adjust p-values have been proposed by Holm (1979) and Anderson (2008) but since our main conclusions remain using the Bonferroni method, they would also using less conservative methods.

5. Conclusions

This paper investigates the impacts of leaving school early (before age 18) for extremely disadvantaged young people. Using the Australian JH survey, we focus on outcomes that are particularly prevalent in this population: homelessness, incarceration, cannabis and illegal street drug use and the diagnosis of depression and anxiety. The aim is to gauge the role that staying in school longer can play in lifting up the lives of these young people. We add evidence to the international literature on the returns to education by focusing on a specific treatment (increasing the duration of schooling) for a subset of the population that may display specific treatment effects.

From retrospective information collected on the age at which respondents left school and the age of onset for each outcome, we build a panel dataset and adopt a difference-in-difference identification strategy to identify the causal effect of early school-leaving (the treatment). Respondents leaving school between 14 and 17 are treated from the time they leave school while those leaving from 18 onwards are never treated. Following the recent literature on staggered treatments and heterogenous treatment effects (across time and cohorts), we use a variety of estimation procedures to correct for biases that arise when using the units previously treated as control units and allow for heterogenous effects. Another consideration is the reliance of DiD on the parallel trend assumption which doesn't hold for some of our outcomes. To improve parallel trends pre-treatment we use reweighting techniques based on observable characteristics such that the control and treated units are more comparable. Overall, our results do not depend much on the estimation procedure used.

Across the board, there is a clear gender gap in the effect of early school-leaving by which leaving school early substantially increases the risk of adverse life events for males while effects for females are concentrated in the short-term for females leaving very early. More precisely, leaving school early significantly increases males' risk of homelessness and use of cannabis daily as soon as they leave school and despite becoming insignificant, these effects do not change much as they grow older. The probability of incarceration and using illegal street drugs weekly is also markedly greater for males leaving school early but these effects also increase over time. Importantly, the effects of leaving school early are noticeable for males who leave between 14 and 17 even if the effects tend to decrease as the age of school-leaving increases.

For females, the effects of leaving school early are mostly small, insignificant and inconsistent in sign. The only exceptions are increases in the risk of homelessness and incarceration in the first few years after leaving school and when females leave school at a very young age (14 or 15 years old). Effects of early school-leaving on depression and anxiety disorder are insignificant for both genders.

These gender gaps may reflect different reasons why males and females quit school (e.g. females may leave if they expect a baby) and different sets of support programs potentially offered to them as a result. Overall though they reinforce other results in the education literature that males' disadvantage in education is a critical policy issue. Our results suggest that if extremely disadvantaged males stayed in school longer, they would be less likely to become homeless, be incarcerated and use illegal drugs.

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Appendix A – Sample selection

Given the sample restrictions described in the text, we use 954 respondents in our analysis out of the original wave 1 sample of 1,682 respondents. This analysis sample could be biased if the restrictions used correlate with our outcomes of interest (in ways that we cannot control for in our fixed effect models). To investigate whether we have a biased sample, we perform linear regressions with being present in our sample as the dependent variable and adverse outcomes as explanatory variables, looking at both whether the respondent has experienced the outcome by age 25 (which corresponds to the last age in our window of analysis) and the average age of onset. The relevant parameter estimates are shown in Table A1.

Table A.1: Regression estimates of the likelihood to be in our sample on the experience of adverse outcomes

	By 25 years old	Age of onset
Homelessness	-0.019 (0.017)	0.000 (0.001)
N	994	949
Incarceration	-0.042 (0.022)*	0.000 (0.002)
N	1,017	434
Cannabis daily	0.010 (0.019)	0.007 (0.002) ***
N	892	420
Illegal street drugs weekly	-0.003 (0.024)	0.004 (0.003)
N	897	227
Depression	-0.023 (0.021)	0.000 (0.001)
N	961	534
Anxiety disorder	-0.031 (0.025)	0.001 (0.002)
N	950	422

Notes: The outcome is a dummy equal to 1 for the 954 respondents in our sample and 0 for other wave 1 respondents. The number of observations is restricted to the 1,031 respondents who were at least 25 years old at wave 1 because of right censored observations (respondents who are less than 25 and have not yet experienced an adverse outcome). The number of observations differs for each outcome based on the availability of the experience of the adverse outcome and its age of onset. Robust standard errors in parentheses; *** (*) indicates significance at a 1% (10%) level.

From this table, we conclude that respondents in our sample have experienced adverse outcomes at approximately the same average age as respondents who are excluded from our sample, except for a small positive correlation on the use of cannabis daily (for those who used cannabis daily, starting one year older increases the likelihood of being in our sample by 0.7pp). They are also as likely to have experienced adverse outcomes by 25 years old. Significant correlations are sparse and reduced in a fixed effect framework which controls for time-invariant unobserved heterogeneity.

Appendix B Additional Estimates

B1. OLS estimates

Table B.1 OLS estimates of the effect of early school-leaving on adverse life events (with controls)

Males	**Homeless**			**Incarceration**			**Cannabis daily**		
Early school-leaving	0.135	(0.036)	***	0.170	(0.029)	***	0.176	(0.035)	***
Indigenous	0.044	(0.041)		0.125	(0.039)	***	-0.066	(0.043)	
State care	0.136	(0.038)	***	0.115	(0.037)	***	0.069	(0.039)	*
Family support	-0.011	(0.002)	***	-0.006	(0.002)	***	-0.003	(0.002)	*
Parental separation (-1)	0.088	(0.033)	***	0.020	(0.030)		0.136	(0.034)	***
	Street drugs weekly			**Depression**			**Anxiety Disorder**		
Early school-leaving	0.079	(0.020)	***	-0.006	(0.025)		-0.018	(0.022)	
Indigenous	-0.067	(0.022)	***	-0.020	(0.029)		-0.005	(0.023)	
State care	0.016	(0.024)		0.047	(0.029)		0.024	(0.024)	
Family support	-0.002	(0.001)		-0.003	(0.001)	**	-0.004	(0.001)	***
Parental separation (-1)	0.045	(0.023)	*	0.052	(0.024)	**	0.053	(0.021)	**

Females	**Homeless**			**Incarceration**			**Cannabis daily**		
Early school-leaving	0.104	(0.041)	**	0.050	(0.024)	**	0.074	(0.035)	**
Indigenous	0.092	(0.044)	**	0.056	(0.036)		0.007	(0.040)	
State care	0.148	(0.049)	***	0.106	(0.038)	***	0.055	(0.041)	
Family support	-0.010	(0.002)	***	-0.001	(0.001)		-0.002	(0.002)	
Parental separation (-1)	0.046	(0.036)		-0.031	(0.024)		0.075	(0.033)	**
	Street drugs weekly			**Depression**			**Anxiety Disorder**		
Early school-leaving	0.017	(0.022)		-0.019	(0.039)		-0.043	(0.035)	
Indigenous	-0.055	(0.018)	***	-0.058	(0.036)		-0.028	(0.027)	
State care	0.040	(0.026)		0.107	(0.041)	***	0.058	(0.034)	*
Family support	-0.001	(0.001)		-0.004	(0.002)	**	-0.002	(0.001)	
Parental separation (-1)	0.023	(0.020)		0.058	(0.031)	*	0.080	(0.025)	***

Note: Based on a sample 573 males (8022 obs.) and 381 females (5334 obs.). Also included in the analysis: quarter of birth, age fixed effects. In parentheses standard error clustered by individual. *** (**,*) indicates significance at a 1% (5%, 10%) level.

B2. Bacon-decomposition

As indicated in the main text, Goodman-Bacon (2021) shows that TWFE estimates are weighted averages of all possible two-group/two-period DiD estimators. Some of these comparisons across groups may be "bad" when already treated units act as controls because they were treated in an earlier period (rather than the current period). If the treatment changes the trend in the outcome in subsequent periods, then using an already treated unit as control leads to biased estimates given that the control is in fact affected by the treatment.

Table B2 shows the results of the Bacon-decomposition of the ATET estimated using a traditional TWFE. The overall ATET is split-up in three components. The first component is based on comparing units that are treated at some point vs. units that are never treated as controls. The second component is based on comparing units treated early (treated) vs. units that are treated later (control). The third component is based on comparing units treated late (treated) vs. units that are treated earlier (control). The bias arises from the third component which has a weight of 41% whereas the first two components have a joint weight of 59%. The bias appears particularly large when the estimates of the third component differ substantially from the first two components, which is the case for example for males' incarceration and use of street drugs weekly. In this case, while the TWFE sits between the three components, the regression adjusted estimates (Table 2, column 3) corrects the bias and is distinct from the TWFE, i.e. 0.143 and 0.084 respectively for incarceration and the use of illegal street drugs weekly. In cases where the estimates for the three components are close enough, estimates using standard TWFE or regression adjusted estimates yields very similar results.

Table B.2 Parameter estimates TWFE and Bacon-decomposition

	Males	Bacon-decomposition			Females	Bacon-decomposition		
	TWFE Estimate	Treated-never	Early-late	Late-early	TWFE estimate	Treated-Never	Early-late	Late-early
Homeless	0.083***	0.095	0.146	0.051	0.083***	0.085	0.134	0.066
Incarceration	0.060***	0.145	0.052	-0.035	-0.004	0.009	0.047	-0.034
Cannabis daily	0.120***	0.157	0.071	0.091	0.029	0.042	0.026	0.015
Street drugs	0.031*	0.088	0.015	-0.030	-0.001	0.010	0.016	-0.019
Depression	0.002	0.021	-0.028	-0.012	0.003	-0.005	0.030	0.004
Anxiety disorder	0.010	0.023	-0.005	-0.001	-0.021	-0.018	-0.004	-0.031
Weights	1.000	0.474	0.117	0.409	1.000	0.481	0.116	0.404

Notes: Based on a sample of 573 males (8022 obs.) and 381 females (5334 obs.). The TWFE estimates are also presented in Table 2. *** (*) indicates significance at a 1% (10%) level.

B3. Alternative DiD estimators

Table B3 shows parameter estimates for three alternative estimation procedures. The first column shows the parameter estimates using an extended TWFE approach based on Wooldridge (2021). In this approach ATET are estimated using a linear model with dummies for treatment cohort and time period, and a set of cohort-by-time treatment indicators. The second column of Table B3 shows the Stackedev-estimates (discussed in Cunningham (2021) and Baker et al. (2022)). The third column provides estimates based on the synthetic DiD (SDiD) (Arhangelsky et al., 2021). In SDiD, pre-trends are reweighted to match trends for control units to trends for treated units before the treatment starts. The time trends are made identical by reweighting and DID analysis is applied to the reweighted panel. Arkhangelsky et al. (2021) indicate that with a staggered treatment the SDID estimator is applied repeatedly – once for every starting date and then the weighted average of the estimators is calculated.

Table B.3 Alternative DiD estimators of the effect of early school-leaving

	Ext TWFE	Stackedev	SDID
a. Males	ATET (SE)	ATET (SE)	ATET (SE)
Homeless	0.107 (0.042)**	0.096 (0.028)***	0.102 (0.038)***
Incarceration	0.154 (0.031)***	0.145 (0.022)***	0.150 (0.030)***
Cannabis daily	0.140 (0.036)***	0.156 (0.025)***	0.130 (0.034)***
Street drugs weekly	0.089 (0.024)***	0.088 (0.018)***	0.089 (0.025)***
Depression	0.010 (0.025)	0.021 (0.017)	0.014 (0.025)
Anxiety disorder	0.018 (0.021)	0.021 (0.014)	0.020 (0.023)
Individuals	573	573	573
Observations	8022	11,466	8022
b. Females			
Homeless	0.092 (0.043)**	0.092 (0.030)***	0.103 (0.047)**
Incarceration	0.020 (0.024)	0.013 (0.017)	0.022 (0.022)
Cannabis daily	0.037 (0.038)	0.042 (0.026)	0.004 (0.004)
Street drugs weekly	0.012 (0.026)	0.012 (0.017)	0.016 (0.026)
Depression	0.005 (0.043)	-0.002 (0.029)	0.013 (0.040)
Anxiety disorder	-0.010 (0.034)	-0.016 (0.022)	-0.001 (0.027)
Individuals	381	382	381
Observations	5334	7728	5334

Notes: Based on a sample of 573 males (8022 obs.) and 381 females (5334 obs.). ATET = Estimate of the average treatment effect on the treated with the not-yet treated used as control units; SE = standard error clustered by individual. *** (**,*) indicates significance at a 1% (5%, 10%) level.

The parameter estimates in the three alternative estimation procedures are very similar to those obtained in our main analysis presented in Table 2.

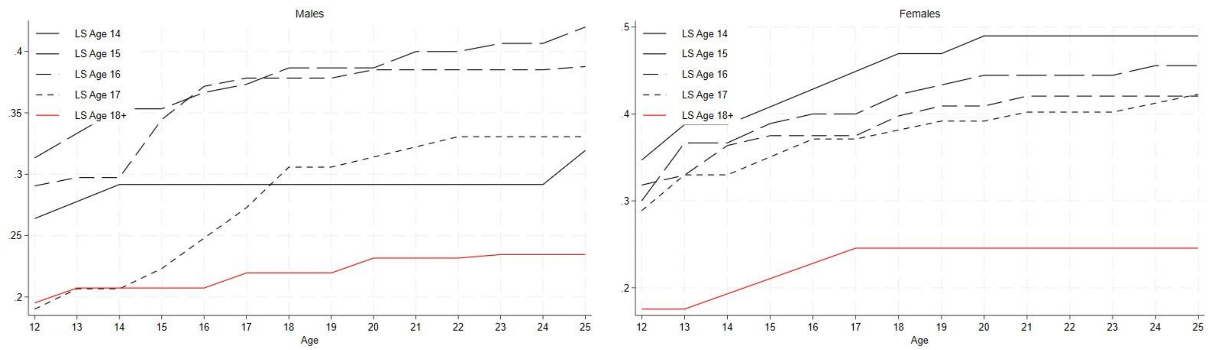
B4. Falsification test: parental separation

Table B.4.1 OLS and AIPW estimates of the effect of early school-leaving on parental separation

	OLS	(SE)	AIPW	(SE)
Males	0.110	(0.040)***	0.017	(0.016)
Females	0.143	(0.049)***	0.014	(0.020)

Note: Based on a sample of 571 males (7994 obs.) and 381 females (5334 obs.). ATET = Estimate of the average treatment effect on the treated with the not-yet treated used as control units; SE = standard error clustered by individual. The OLS estimate includes no explanatory variables except for age fixed effects. The weights for the AIPW estimates are built using: dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent's caregivers were separated in the previous period. *** (**,*) indicates significance at a 1% (5%, 10%) level.

Figure B.4.1 Lifetime prevalence of parental separation by age and age of school-leaving



B5. Alternative samples

Table B.5.1 AIPW estimates – different samples for each outcome

	Males				Females			
	Estimate	(SE)	PT	Obs.	Estimate	(SE)	PT	Obs.
Homeless	0.079	(0.038)**		737	0.054	(0.040)	^^	535
Incarceration	0.129	(0.034)***		628	0.047	(0.020)**	^	394
Cannabis daily	0.074	(0.036)**	^^	684	-0.004	(0.044)	^^	454
Street drugs weekly	0.101	(0.025)***		620	-0.008	(0.033)		420
Depression	0.009	(0.026)	^^	648	0.021	(0.038)		472
Anxiety disorder	0.015	(0.023)		639	0.038	(0.029)	^^	450

Note: ATET = Estimate of the average treatment effect on the treated with the not-yet treated used as control units; SE = standard error clustered by individual. PT: parallel-trends test with H0: the treatment effects in all the pre-treatment periods are zero. Obs. = number of observations The weights for the AIPW estimates are built using: dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent's caregivers were separated in the previous period. *** (**,*) indicates significance at a 1% (5%, 10%) level.

Table B.5.2 AIPW estimates - respondents are less than 46 at wave 1

	Males			Females		
	Estimate	(SE)	PT	Estimate	(SE)	PT
Homeless	0.086	(0.048)*		0.065	(0.043)	^^^
Incarceration	0.149	(0.038)***		0.040	(0.023)*	^
Cannabis daily	0.074	(0.043)*		-0.036	(0.054)	^^
Street drugs weekly	0.095	(0.028)***		-0.011	(0.032)	
Depression	0.013	(0.025)	^	-0.028	(0.044)	
Anxiety disorder	0.025	(0.024)		-0.005	(0.034)	

Note: Based on a sample of 411 males (5754 obs.) and 286 females (4004 obs.). ATET = Estimate of the average treatment effect on the treated with the not-yet treated used as control units; SE = standard error clustered by individual. PT: parallel-trends test with H0: the treatment effects in all the pre-treatment periods are zero. The weights for the AIPW estimates are built using: dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent's caregivers were separated in the previous period. *** (**,*) indicates significance at a 1% (5%, 10%) level.

Table B.5.3 AIPW estimates – no restriction on respondents' year of birth

	Males			Females		
	Estimate	(SE)	PT	Estimate	(SE)	PT
Homeless	0.061	(0.041)		0.049	(0.039)	^^^
Incarceration	0.122	(0.032)***		0.017	(0.019)	^
Cannabis daily	0.094	(0.035)***		-0.026	(0.044)	^^^
Street drugs weekly	0.078	(0.024)***		-0.012	(0.029)	
Depression	-0.002	(0.024)		-0.031	(0.039)	
Anxiety disorder	0.013	(0.021)		0.000	(0.029)	
Individuals	585 (+12)			386 (+5)		

Note: ATET = Estimate of the average treatment effect on the treated with the not-yet treated used as control units; SE = standard error clustered by individual. PT: parallel-trends test with H0: the treatment effects in all the pre-treatment periods are zero. The weights for the AIPW estimates are built using: dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent's caregivers were separated in the previous period. *** (**,*) indicates significance at a 1% (5%, 10%) level.

Table B.5.4 AIPW estimates – restriction on respondents' age at wave 1 (25+)

	Males			Females		
	Estimate	(SE)	PT	Estimate	(SE)	PT
Homeless	0.081	(0.042)*		0.048	(0.039)	^^^
Incarceration	0.138	(0.030)***		0.010	(0.019)	^
Cannabis daily	0.098	(0.036)***		-0.026	(0.045)	^^^
Street drugs weekly	0.089	(0.022)***		-0.016	(0.039)	
Depression	-0.003	(0.024)		-0.034	(0.039)	
Anxiety disorder	0.013	(0.021)		-0.003	(0.029)	
Individuals	562 (-11)			377 (-4)		

Note: ATET = Estimate of the average treatment effect on the treated with the not-yet treated used as control units; SE = standard error clustered by individual. PT: parallel-trends test with H0: the treatment effects in all the pre-treatment periods are zero. The weights for the AIPW estimates are built using: dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent's caregivers were separated in the previous period. *** (**,*) indicates significance at a 1% (5%, 10%) level.

Table B.5.5 AIPW estimates – balanced panel (t=-4 to 6)

	Males			Females		
	Estimate	(SE)	PT	Estimate	(SE)	PT
Homeless	0.034	(0.03)		0.033	(0.037)	^^^
Incarceration	0.096	(0.027)***		0.013	(0.013)	^
Cannabis daily	0.088	(0.032)***		-0.025	(0.042)	^^^
Street drugs weekly	0.052	(0.021)**		0.004	(0.018	
Depression	-0.010	(0.021)	^	-0.014	(0.033)	
Anxiety disorder	0.007	(0.019)		-0.003	(0.022)	
Individuals	565			377		

Note: To create a balanced panel individuals who left school from age 20 onward are removed from the sample. ATET = Estimate of the average treatment effect on the treated with the not-yet treated used as control units; SE = standard error clustered by individual. PT: parallel-trends test with H0: the treatment effects in all the pre-treatment periods are zero. The weights for the AIPW estimates are built using: dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent's caregivers were separated in the previous period. *** (**,*) indicates significance at a 1% (5%, 10%) level.

Table B.5.6 AIPW estimates – only never treated units are in the control group

	Males			Females		
	Estimate	(SE)	PT	Estimate	(SE)	PT
Homeless	0.068	(0.042)		0.056	(0.040)	^^^
Incarceration	0.127	(0.033)***		0.020	(0.020)	^
Cannabis daily	0.104	(0.036)***		-0.028	(0.046)	^^^
Street drugs weekly	0.082	(0.024)***		-0.011	(0.029)	
Depression	0.002	(0.024)	^^	-0.032	(0.039)	
Anxiety disorder	0.015	(0.020)		-0.001	(0.029)	
Individuals	573			381		

Note: ATET = Estimate of the average treatment effect on the treated with the not-yet treated used as control units; SE = standard error clustered by individual. PT: parallel-trends test with H0: the treatment effects in all the pre-treatment periods are zero. The weights for the AIPW estimates are built using: dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent's caregivers were separated in the previous period. *** (**,*) indicates significance at a 1% (5%, 10%) level.

Table B.5.7 AIPW estimates – no restriction on age of school-leaving (14+)

	Males			Females		
	Estimate	(SE)	PT	Estimate	(SE)	PT
Homeless	0.066	(0.040)	^^^	0.076	(0.038)**	^^
Incarceration	0.133	(0.032)***	^	0.033	(0.021)	^
Cannabis daily	0.105	(0.035)***	^^	-0.017	(0.043)	^^^
Street drugs weekly	0.088	(0.024)***		-0.005	(0.027)	
Depression	0.008	(0.024)		-0.028	(0.037)	
Anxiety disorder	0.015	(0.020)		0.000	(0.028)	
Individuals	604 (+31)			412 (+31)		

Note: ATET = Estimate of the average treatment effect on the treated with the not-yet treated used as control units; SE = standard error clustered by individual. PT: parallel-trends test with H0: the treatment effects in all the pre-treatment periods are zero. The weights for the AIPW estimates are built using: dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent's caregivers were separated in the previous period. *** (**,*) indicates significance at a 1% (5%, 10%) level.

Table B.5.8 AIPW estimates – the never treated left school at 19+

	Males			Females		
	Estimate	(SE)	PT	Estimate	(SE)	PT
Homeless	0.167	(0.053)***	^^^	0.155	(0.030)***	^^^
Incarceration	0.154	(0.066)**	^^	0.032	(0.025)	
Cannabis daily	0.140	(0.044)***	^^^	-0.126	(0.070)*	^^^
Street drugs weekly	0.123	(0.013)***		0.011	(0.036)	
Depression	-0.004	(0.034)		-0.155	(0.141)	
Anxiety disorder	0.018	(0.025)		-0.028	(0.054)	
Individuals	573			381		

Note: ATET = Estimate of the average treatment effect on the treated with the not-yet treated used as control units; SE = standard error clustered by individual. PT: parallel-trends test with H0: the treatment effects in all the pre-treatment periods are zero. The weights for the AIPW estimates are built using: dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent's caregivers were separated in the previous period. *** (**,*) indicates significance at a 1% (5%, 10%) level.

Table B.5.9 Multiple hypothesis testing

	Males		Females	
	AIPW	Bonferroni	AIPW	Bonferroni
Homeless	0.105	0.620	0.159	0.954
Incarceration	0.000	0.000	0.317	1.000
Cannabis daily	0.004	0.024	0.544	1.000
Street drugs weekly	0.001	0.006	0.710	1.000
Depression	0.923	1.000	0.415	1.000
Anxiety disorder	0.461	1.000	0.978	1.000

Note: p-values are reported. AIPW = Estimate of the average treatment effect for the AIPW model in Table 2. The weights for the AIPW estimates are built using: dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent's caregivers were separated in the previous period.

B6. Mechanisms

Table B.6.1 AIPW estimates – controlling for the lag of other adverse life events

	Males			Females		
	Estimate	(SE)	PT	Estimate	(SE)	PT
Homeless	0.033	(0.043)		0.038	(0.039)	^^^
Incarceration	0.131	(0.032)***		0.019	(0.020)	^
Cannabis daily	0.117	(0.033)***	^	-0.025	(0.044)	^^^
Street drugs weekly	0.087	(0.022)***		-0.018	(0.027)	
Depression	0.008	(0.022)		-0.033	(0.038)	
Anxiety disorder	0.020	(0.019)		0.000	(0.029)	
Individuals	573			381		

Note: ATET = Estimate of the average treatment effect on the treated with the not-yet treated used as control units; SE = standard error clustered by individual. PT: parallel-trends test with H0: the treatment effects in all the pre-treatment periods are zero. The weights for the AIPW estimates are built using: dummies for the quarter of birth; Indigenous status; whether the respondent was in State care as a child; an index for family support and whether the respondent's caregivers were separated in the previous period. *** (**,*) indicates significance at a 1% (5%, 10%) level.