The Ripple Effect of Youth Adversity: Economic and Health Impacts on Families

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Abstract

The United Nations' Sustainable Development Goals highlight two crises facing children today: preventable deaths and exposure to violence. This thesis examines the multi-faceted impacts of these health shocks for youth victims, their families, and broader social networks. Across four distinct papers, the focus is centered on youth suicide and traffic fatalities, assault victimisation, and family violence. Leveraging New Zealand's comprehensive population-wide administrative data, the analysis delves into these relatively unexplored topics through linking affected children to their parents and peers, and closely following a range of high-frequency economic, social, and health outcomes in the months surrounding the event. Using two empirical methodologies, this research offers new evidence on the short- and long-term effects of these life-altering events for those affected by them.

Contents

List of Figures

List of Tables

Attestation of Authorship

I hereby declare that this submission is my own work and that, to the best of my knowledge and belief, it contains no material previously published or written by another person (except where explicitly defined in the acknowledgements), nor used artificial intelligence tools or generative artificial intelligence tools (unless it is clearly stated, and referenced, along with the purpose of use), nor material which to a substantial extent has been submitted for the award of any other degree or diploma of a university or other institution of higher learning.

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Additional information

This thesis contains four chapters which are written as stand-alone academic articles and have been directly included as such. For this reason, there is some repetition across these papers due to common literatures, same data source, and similar estimation strategies. This "manuscript-format" also naturally leads to some repetition between the manuscript chapters and the thesis introductory and concluding chapters.

I use APA (7th edition) referencing style throughout this thesis, with an overall reference list provided at the end of this document.

The first half of this thesis comprises two papers examining the impact of a child experiencing a fatal health shock on bereaved parents' outcomes. The second half shifts to non-fatal health shocks, exploring the effects of a child experiencing assault victimisation. Both halves of this thesis follow a similar format: one long paper and one short paper.

The academic papers contained in Chapters 2-5 were not written in the order they appear. The chronological writing order is as follows: Chapter 2 in 2022, Chapter 4 in 2023, and Chapters 3 and 5 in 2024.

Disclaimer: Statistics New Zealand

Chapter 1

General introduction

The United Nations 2030 Agenda for Sustainable Development sets out a series of goals (SDGs) aiming to address the ongoing crises of rising economic and gender inequalities, unemployment, and global health and mental health threats, among other things. This thesis examines public policy issues that align with at least two of these key SDGs.

SDG 3 aims to ensure healthy lives and well-being for all, including the specific goals of reducing global suicide mortality rates by one-third and halving the number of deaths from traffic accidents (United Nations, n.d.). Traffic and suicide fatalities are leading causes of external mortality, with rates particularly high for adolescents (UNICEF, 2023) and young people (OECD, 2023).

SDG 16 aims to promote peaceful and inclusive societies by significantly reducing all forms of violence and eradicating the abuse, violence, and torture of children (United Nations, n.d.).

Together, these SDGs highlight two crises facing children today: preventable deaths and exposure to violence. My thesis explores the economic and health impacts of experiencing these events for child victims and their surrounding network.

I use individual-level data from New Zealand, a country whose indicators of youth mortality and violence are alarmingly high. In 2015, New Zealand had 23 teenage suicides per 100,000, more than three times the OECD average of 7 per 100,000 (OECD, 2017). New Zealand also has consistently had some of the highest driver fatality rates in the OECD, particularly for young drivers (OECD, 2006). Regarding violence, self-reported victimisation rates from the International Crime Victim survey reveals that 21.5% of the New Zealand population was victimised over a 12-month period, relative to the OECD average of 15.5% (OECD, 2009). New Zealand also has some of the highest rates of family violence among OECD countries (Women's Refuge, 2024).

These poor statistics have led to many policy initiatives in the victimisation and youth mortality space. For example, the government made a long-term commitment to create a more victim-focused criminal justice system. This included initiatives such as the 2014 "victims of crime reform package", which aimed to improve the treatment of victims and place victims' rights at the centre of the justice system, and the 2023 "Better Outcomes for Victims" work programme to improve victim's experiences of the justice system. For suicide prevention, the "Every Life Matters" 2019 framework created a two-part plan to eradicate suicide in New Zealand. In following this framework, New Zealand introduced its first Minister of Mental Health in 2023 and assigned NZD $10 million for a Mental Health and Addiction Community Sector Innovation Fund to support initiatives aiming to improving mental health outcomes in New Zealand.

Although youth mortality and abuse remain critical public health issues in New Zealand and internationally, there is limited empirical evidence of the economic impacts of such events for those affected by them. To the extent that the costs of suicide and victimisation are estimated in New Zealand,[1] such calculations typically provide high-level estimates and explicitly exclude the quantification of spillover costs to, for example, family members of affected victims.

This thesis aims to address these gaps, using causal inference to uncover how parents respond to serious health shocks experienced by their children. Over four papers, I investigate two types of shocks: fatal and non-fatal. Fatal shocks include youth deaths from suicide and traffic accidents, while non-fatal shocks cover cases of (reported) youth assault, examining both family and non-family violence. The resulting estimates can be used as additional inputs to the overall estimation of the costs of victimisation and youth death in New Zealand, which can ultimately feed into targeted cost-benefit analyses for crime-reduction and death-prevention programmes.

The impact on parents may arise through two channels: directly, through emotional or psychological distress, and indirectly, through heightened caregiving responsibilities. Both of these pathways could lead to a reduction in parental labour supply. Since few households have private insurance for these events,[2] families are left to rely on public insurance to help them smooth their income across the shock. Comparing New Zealand's labour market characteristics to that for other OECD countries shows such support is

[1] For example, MartinJenkins (2020) estimated that the average lifetime cost for an individual abused in New Zealand State care is $857,000 NZD. The report acknowledges that this figure excludes any costs arising from increased ACC payments and unemployment insurance, as well as spillover costs for families of those abused (p.13). Regarding the economic cost of suicide, a report for the Ministry of Health in 2005 estimate each suicide to cost a total of $2,931,250 NZD (O'Dea & Tucker, 2005). However, the authors consider that while bereavement of family and friends is a considerable cost of suicide, "no substantial attempt was made to put any quantitative value on that impact" (p.25).

[2] Only 15% of workers in New Zealand have private income protection, which is half of the rate seen in Australia (Colquhoun, 2024). Even then, such protection is typically tied to the individual's own health stock and not to the health of other family members.

relatively low; for example, New Zealand ranks 26 out of 36 OECD countries for levels of employment regulatory protection[3]. Further, New Zealand's income compensation rates provided by unemployment benefits are relatively low. As at December 2024, the maximum job-seeker support benefit rate for an unemployed sole parent was $494.80 NZD per week, equivalent to only 53% of a full-time minimum wage worker's weekly earnings. These characteristics suggest that families affected by child loss or abuse in New Zealand may feel pressure to return to work sooner than desired to avoid substantial income loss.

I explore these dynamics through estimating the parental health and economic consequences of child adversity, providing insights about the extent to which public insurance schemes compensate any lost income arising from a child's health shock. These results can provide input to the normative discussion for whether and how New Zealand's social welfare net should be changed to better help families affected by these relatively low-frequent but high-impact shocks.

Population-wide linked administrative data

All four of the academic papers contained in this thesis use data from Statistics New Zealand's (hereafter 'Stats NZ') Integrated Data Infrastructure (IDI). The IDI is a research database comprised of administrative and survey data about people and households in New Zealand. The IDI is accessible via physical data labs housed in secure locations throughout the country. Only approved researchers have access to these data labs and must comply with a set of strict confidentiality rules when using the IDI data.

Data within the IDI cover a range of topics, such as education, income and work,

[3] See Kreiner and Svarer (2022) for more details and extensive references for these indicators.

population, health, and justice (Stats NZ, 2022). All individuals who have ever engaged with government agencies, Stats NZ surveys, and non-government organisations have records contained in the IDI. Stats NZ use probabilistic record matching on personal information (such as first name, last name, and birth date) to link individuals across registers and surveys. Stats NZ then assigns a unique identifier to each individual to ensure the data is de-identified. This unique identifier enables approved researchers to link individuals across different datasets and over time.

For all papers in this thesis, I use biological family ties to define a household or family unit. The starting point for this is the Department of Internal Affairs' birth register, which contains information on all registered births in New Zealand from 1840 onward. For each child born in New Zealand, I can link them to their biological mother, father, and siblings where relevant. One benefit of defining households as biological families is that biology is time invariant, so the household unit can be tracked over time. One drawback is that it requires the child to be New Zealand-born, so I cannot create household units for children born outside of New Zealand.

While there are several registers and surveys in the IDI that contain information about a family or household, they are only measured at a particular point in time, such as on Census night, or over a specific period, such as a benefit spell. The IDI does have a derived table that aims to create a full history of addresses across all sources. It follows a series of prioritisation and business rules to provide a 'best-guess' of one's residential address across time. However, it is commonly understood that this table is best for tracking the addresses of those in more permanent living situations. It can be unreliable for transient people, such as youth who may be more likely to report different addresses to different organisations (e.g., report their parents' address to the tax authority but report their student address to the health authority) or who face high administrative costs for updating their address information at all government agencies after every relocation. This is why biological family ties are the cleanest and most consistent way to define a youth's family and track them over time.

Empirical strategies

The objective of each paper in this thesis is to identify the causal effects of a youth's health shock on the ex-post outcomes for the youth's parents (and other networks, where relevant). The empirical challenge is to determine what would have happened to the parents had their child not experienced the health shock. We can think about this problem using the potential outcomes framework. Let D_i be an indicator for whether parent i's child experienced a health shock and Y_i be the observed outcome (e.g. employment) for parent i. Following Angrist and Pischke (2009), parent i's observed outcome is determined by two potential outcomes, as shown by Eq. 1.1.

$$Y_i = \begin{cases} Y_i^1 & \text{if } D_i = 1, \\ Y_i^0 & \text{if } D_i = 0 \end{cases} \tag{1.1}$$

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

The first potential outcome (Y_i^1) is the employment status of a parent had their child experienced a health shock and the second potential outcome (Y_i^0) is the employment status of a parent had their child not experienced the health shock, regardless of whether the health shock actually occurred. The difference between the two potential outcomes

(Y_i^1-Y_i^0) reveals the causal effect of the youth's health shock on the parent's employment.

However, only one potential outcome will be observed for parent i: if parent i's child experienced a health shock, then $D_i = 1$, so $Y_i = Y_i^1$, but if parent i's child did not experience a health shock, then $D_i = 0$, so $Y_i = Y_i^0$. This means the individual treatment effect cannot be calculated since it requires both potential outcomes to be observed for each parent i.

While the true treatment effect can never be calculated due to the unobserved potential outcome problem, it can be estimated. In light of this, there are three different parameters that researchers often estimate: the average treamtent effect (ATE), the average treatment effect for the treated (ATT), and the average treatment effect for the untreated (ATU) (Cunningham, 2021).

In this thesis, I recover the average causal effect of a health shock for parents whose child experienced the health shock, i.e. the average treatment effect on the treated, ATT (Eq. 1.2.). The first component of Eq. 1.2 is the expected employment rate for parents of youth who in fact experienced the health shock, the second component is the expected employment rate for parents of youth who experienced the health shock had the health shock not occurred. Naturally, only the former is observable.

$$ATT = E[Y_i^1 \mid D_i = 1] - E[Y_i^0 \mid D_i = 1] \tag{1.2}$$

One way to overcome this unobserved potential outcome issue is by introducing a first-difference estimator using two time periods. Here, I compare the average employment rate of affected parents before ($Post_i = 0$) and after ($Post_i = 1$) their child's health shock. If the observed pre-health shock employment rate of affected parents, $E[Y_i^1 \mid Post_i =$

$0, D_i = 1]$, is a considered an appropriate estimate of the post-health shock counterfactual employment rate for affected parents, $E[Y_i^0 \mid Post_i = 1, D_i = 1]$ then comparing the average employment rate before and after the child's health shock for affected parents will recover the ATT, as shown in Eq. 1.3.

$$\begin{aligned} ATT &= E[Y_i^1 \mid \text{Post}_i = 1, D_i = 1] - E[Y_i^1 \mid \text{Post}_i = 0, D_i = 1] \\ &\text{if } E[Y_i^1 \mid \text{Post}_i = 0, D_i = 1] = E[Y_i^0 \mid \text{Post}_i = 1, D_i = 1] \end{aligned} \tag{1.3}$$

However, the first-difference estimator of the ATT may be biased if factors other than the child's health shock, such as changing economic conditions, also affect parents' employment over time. Such time effects can be cancelled out by introducing a second difference to the estimator, i.e. employing a difference-in-differences strategy.

For a difference-in-differences approach, the change in the average employment rate for parents affected by a child's health shock is compared to the change in the average employment rate for a comparable control group over the same time period. In this set-up, the observed average employment rate of the comparison group in the post-health shock period ($E[Y_i^0 \mid Post_i = 1, D_i = 0]$) is used as an estimate of the unobserved average employment rate of affected parents in the post-child health shock period ($E[Y_i^0 \mid Post_i = 1, D_i = 1]$), and likewise for the pre-health shock period.

Thus, by arguing that $E[Y_i^0 \mid Post_i = 1, D_i = 0]$ is a credible estimate of $E[Y_i^0 \mid Post_i = 1, D_i = 1]$, and $E[Y_i^0 \mid Post_i = 0, D_i = 0]$ is a credible estimate of $E[Y_i^0 \mid Post_i = 0, D_i = 1]$, I can estimate the ATT by Eq. 1.4.

$$
\begin{aligned}
ATT = & \big[E[Y_i^1 \mid \text{Post}_i = 1, D_i = 1] - E[Y_i^1 \mid \text{Post}_i = 0, D_i = 1]\big] \\
& - \big[E[Y_i^0 \mid \text{Post}_i = 1, D_i = 0] - E[Y_i^0 \mid \text{Post}_i = 0, D_i = 0]\big]
\end{aligned} \tag{1.4}
$$

There are different ways to construct an appropriate comparison group. In all four papers of this thesis, I exploit variation in the timing of the child's health shock to identify a group of yet-to-be-affected parents and use those to estimate the counterfactual outcome for parents affected by their child's health shock in the present. This approach is known as a stacked difference-in-differences design. As explained across the papers, the specific context of the treatment event is used to determine the choice of timeframe for the future-treated comparison group.

In Chapter 3, I also employ a second empirical strategy to create a comparison group for affected parents. I use a matched difference-in-differences design, combining exact and propensity score matching to create a comparison group of never-affected parents who look, on average, the same as affected parents.

The remainder of this thesis proceeds as follows. Chapter 2 contains a short paper on the cost of youth suicide for bereaved parents: *Parental response to youth suicide: Evidence from New Zealand*. Chapter 3 extends the suicide study by comparing parental health and economic responses to youth suicide versus traffic fatalities: *Family impacts of child loss*. Chapter 4 turns to non-fatal health shocks, exploring the impacts of youth assault victimsiation: *The effect of violent assaults on youth victims and their parents*. Chapter 5 presents the final academic paper of this thesis, examining the economic impacts of victimisation within the context of family violence: *The household economic costs of parental assaults against children*. Finally, Chapter 6 provides a high-level discussion of the thesis and concluding remarks.

Chapter 2

Parental response to youth suicide: Evidence from New Zealand

Mikkel Mertz[1], Livvy Mitchell[2,3], Peer Ebbensen Skov[1,2]

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2.1 Prelude

This chapter was the first paper I wrote for my thesis. It is a short paper that estimates the impact of child suicide on parents' labour earnings and total income. It served as a proof-of-concept to show that the stacked difference-in-differences estimation strategy could work using high frequency data measuring effects from a relatively rare event, being youth suicide. This paper has been presented at the ROCKWOOL Foundation in Copenhagen in late 2022 and was published in Economic Letters in 2023 (volume 226, 2023). This chapter is presented exactly as it is published.

2.2 Abstract

The loss of a child is one of the most devastating shocks a parent can experience. We provide the first estimates of the direct effect of youth suicide on parental labour earnings. We use mortality data in the New Zealand Integrated Data Infrastructure to identify youth who died from self-harm, and birth records to identify affected parents and their wage and salary information. We graph the parental earnings profiles before and after the suicide event and construct counterfactual earnings profiles for the affected parents using a comparison group of parents hit by the same shock in the future. Our results show that labour earnings for affected households drop by approximately 6.5% compared to their counterfactual earnings following the child loss and that the earnings drop persists for at least two years following the suicide.

2.3 Introduction

Suicide remains a leading cause of death among people aged 15-29 worldwide, comprising around 8% of all deaths in this group (United Nations, 2021). While suicide and youth suicide rates have steadily declined across all OECD countries since the mid-1980s, no OECD country is on-track to meet the suicide reduction rate target set by the United Nation's 2030 Agenda for Sustainable Development (Ishimo et al., 2021). Hence, suicide remains a significant global health issue.

While the existing literature has documented how various factors influence the likelihood of suicide,[1] less is known about the impact of suicide for the surviving family members.

When parents lose a child, they experience an abundance of negative emotions such as extreme sadness, shock, and anger (Andriessen et al., 2019). These factors may have severe economic consequences in terms of, for example, reduced labour earnings, as shown in van den Berg et al. (2017)[2] who study parents in the aftermath of child-loss from non-intentional accidents.

In this paper we extend the economic consequences of grief literature by providing the first evidence on how parents' labour earnings respond to youth suicide. The empirical challenge is to establish the counterfactual observation: *what parents experiencing youth suicide would have earned if their child had not committed suicide.* To identify the causal impact, we compare affected households with households exposed to youth suicide in

[1] Lang (2013) shows mandated health benefits lower suicide rates. Kuroki (2014) finds higher male-to-female sex ratios are correlated with higher male suicide rates. Breuer (2015) shows unemployment increases lead to higher suicide rates. Blæhr and Søgaard (2021) find no effect of psychotherapy on suicide attempt.

[2] The robustness analyses in that paper include results for intentional accidents, but this combines suicide and homicide.

the future. This follows Fadlon and Nielsen (2019, 2021), but for suicide rather than health shocks. Our analysis uses population-wide administrative data from New Zealand (NZ), a country plagued by some of the highest youth suicide rates amongst developed countries (MSD, 2016; OECD, 2017; UNICEF, 2020). We show that youth suicide leads to an immediate decrease in household labour earnings of about 6.5% with no sign of recovery within 24 months post-suicide. Total income, including welfare benefits and public insurance payments, drops about 4.5% with no sign of recovery. Hence, public transfers help households recover some, but not all, of their lost labour earnings. Finally, we show that responses are strongest for high school graduates, while there is no significant response for university graduates (who are likely to have contractual bereavement periods) and a marginally significant response for those with no qualifications (who have limited labour earnings initially).

2.4 Data

We use population-wide linked administrative data from NZ's Integrated Data Infrastructure (IDI) for 1999-2019.

Identifying suicides. The NZ police are legally required to investigate the cause of sudden deaths. The police reports the details of their investigation to the coroner, who conducts a public inquiry into the circumstances of the death. A death is confirmed as a suicide if the coroner concludes the deceased intended to take their own life. The Ministry of Health's individual-level mortality registrations and diagnosis data are made available in the IDI.

Mortality Records. We identify all youth (10-25 years[3]) deaths from 2001-2018 where the underlying cause of death is intentional self-harm. Intentional self-harm is the first-equal most prevalent cause of death for this age group, contributing about 28% of deaths over this period.[4] We observe 2,250 youth suicides.

Birth Records. We link suicide victims to their parents via birth registers (83% can be linked to at least one parent[5]). We keep working-aged (30-60 years) parents and exclude observations where parents were deceased at the time of youth suicide. Our main sample includes 1,299 youth who can be linked to both parents, which we consider a household. Appendix Table A0.1 shows sample construction numbers.

Monthly Earnings. We link each parent to their monthly earnings records from the NZ tax authority's registers. These IDI records are available from 1999, allowing us to create an 18-month pre-event earnings profile for every parent in our sample.

For each household, we create an earnings profile spanning from 18 months before to 12 months after the suicide. Appendix Table A0.2 shows household-level descriptive statistics. Appendix Figure A0.1 shows the labour earnings distribution for the full population aged 30-60 and overlays selected percentiles from the earnings distribution of the suicide household sample. Measured in the calendar year two years prior to child loss, approximately 68% of parents have earnings below the population median while 19% of parents have no labour income.

[3]Ages 10-25 account for the average age parents think children will move out of home (RaboDirect, n.d.).

[4]Another 28% of deaths are attributed to transport accidents.

[5]17% of youth suicide victims are not born in NZ and therefore are not registered in the birth register.

2.5 Quasi-Experimental Research Design

Our objective is to identify the causal effect of youth suicide on parents' labour earnings. The main challenge is to estimate how the earnings profile for affected households would have looked in the absence of youth suicide. Using the approach by Fadlon and Nielsen (2019, 2021) but for suicide events, we construct counterfactual earnings profiles for households experiencing youth suicide at time t (treatment group) by using the earnings trajectories of parents who experience youth suicide Δ months later at time $t + \Delta$ (comparison group). In our primary specification we use $\Delta = 13$, allowing us to estimate the earnings impact for the first year after suicide.

Our suicide data span 216 months from 2001 to 2018. We match those treated in each month with their 'Δ-months later' comparison group. For $\Delta = 13$, this gives us 203 matched pairs. Within each matched pair, the time of youth suicide for the treatment group ($e = 0$), will serve as a placebo treatment time for the comparison group. By construction, for our primary specification, $e = 13$ indicates the actual 'treatment' time for the comparison group. Therefore, for the event timeline spanning -18 to 12 months, the comparison group is constructed using 31 pre-suicide months from households experiencing suicide in month 13. We proceed by estimating the following dynamic difference-in-differences model:

$$\begin{aligned} Y_{i,t} = \alpha + \beta \cdot treat_i + \sum_{e \neq -6, e=-18}^{12} \varphi_e \cdot M_e \\ + \sum_{e \neq -6, e=-18}^{12} \delta_e \cdot M_e \cdot treat_i + \gamma \cdot X_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{2.1}$$

Where $Y_{i,t}$ denotes labour earnings for household i at time t, $treat_i$ is an indicator equal to one if the household belongs to the treatment group, M_e are event time indicators (placebo event time indicators for the comparison group), and $X_{i,t}$ denotes calendar month and year fixed effects. The parameters of interest are the δ_e's that capture the difference in mean earnings between treatment and comparison households e months after (or before) youth suicide, relative to the difference in mean earnings between these two groups six months prior to suicide.

δ_e identifies the causal effect of youth suicide on household earnings under the assumption that in the absence of the youth suicide, the evolution of labour earnings among the affected households would, on average, be the same as the evolution of labour earnings among the not-yet-affected households. While this assumption is untestable, we provide evidence favouring it by showing the two groups follow the same earnings trend in the 18 months before suicide, i.e., estimates of δ_e are close to 0 for $e < 0$.

We estimate Equation 2.1 in levels rather than logs, because individuals with zero earnings constitute a large share of our sample. For ease of interpretation, we report estimates of δ_e in percent of the counterfactual earnings level of the treatment group, calculated as:

$$P_e = \frac{\delta_e}{E[\hat{Y}_{i,t} \mid e, \text{treat}_i = 1]} \tag{2.2}$$

Where $\hat{Y}_{i,t} = \hat{\alpha} + \hat{\beta} + \sum_{e \neq -6, e=-18}^{12} \hat{\varphi}_e \cdot M_e + \hat{\gamma} \cdot X_{i,t}$ is the predicted counterfactual earnings for household i in the treatment group at event time e.

Finally, to capture average treatment effects across all post-suicide months and for increased precision, we denote $e > 0$ months as 'after' and estimate a two-period difference-in-differences model:

$$Y_{i,t} = \alpha + \beta \cdot \text{treat}_i + \phi \cdot \text{after}_{i,t} + \delta_1 \cdot \text{after}_{i,t} \cdot \text{treat}_i + \gamma \cdot X_{i,t} + \epsilon_{i,t} \tag{2.3}$$

where δ_1 is the parameter of interest. We use Equation 2.2 with δ_1 in place of δ_e to calculate responses in percent of counterfactual earnings.

2.6 Results

Figure 2.1 shows estimates of the dynamic effect of youth suicide on household earnings (Panel A) and total income (Panel B). The x-axis denotes months since youth suicide. The household earnings response is immediate, significant, and long-lasting: after 2 months the suicide-affected households earn 7.5% below their counterfactual earnings. This response persists for the full 1-year observation window. In the 12 months following youth suicide, the average monthly earnings loss is NZD 276.93 or 6.49% of the counterfactual earnings level NZD 4,269.08.[6] Notably, the estimates of P_e in the 18-month window prior to youth suicide are all close to 0 and insignificant, i.e., the treatment and comparison groups follow the same earnings trajectory, supporting the common trends assumption. Appendix Figure A0.2 shows similar results for 6- and 24-month windows: the earnings drop is of similar magnitude (5-6% below counterfactual earnings). Notably, even in the 2-year post-suicide observation window, the earnings response shows no sign of recovery.

In Panel B we estimate the effect of youth suicide on households' total income to investigate if parents can recover their lost labour earnings via alternative income sources. Total income consists of labour earnings; withholding tax deducted payments; public in-

[6] Using the household sample but treating parents as individual observations allows us to study labour earnings responses along the extensive and intensive margins. On the extensive margin the employment drop is approximately 5%. On the intensive margin the labour earnings drop is approximately 3%.

surance Accident Compensation Corporation payments; various company-based earnings; and welfare benefits, parental leave, and student allowance payments. Results show total income drops, albeit slightly less than the drop in earnings: the average monthly earnings loss is 6.49% (Panel A) versus 4.70% for total income (Panel B). Moreover, while parents can offset some of their earnings loss with income from other sources, they still lose income due to their earnings response to youth suicide.

Repeating the analysis using the mother's share of total household income as the dependent variable shows there is no intra-household earnings shifting, i.e., parents respond in equal measure to youth suicide. Appendix Figure A0.3 shows the earnings drop for mothers and fathers analysed separately is 8.39% and 5.42%, respectively.

2.7 Heterogeneity

Next, we examine if the earnings response to youth suicide differs by households' socio-economic-status, proxied by highest educational attainment within the household. We estimate Equation 2.3 for three education groups separately.

Table 2.1 shows earnings responses are strongest among high school graduates, with smaller and marginally significant drops for households with no recorded educational qualification and insignificant drops for households with university graduates. A possible explanation behind this pattern is the strength of the labour market attachment prior to youth suicide. Individuals with no qualifications have limited labour market attachment, leaving little room for earnings to respond. University graduates have stronger labour market attachment and likely have contractual allowances for extended periods of bereavement and sick leave curbing their response.[7] High school graduates fall between these two groups, with employment rates similar to university graduates but with significantly lower earnings.[8]

2.8 Conclusion

This is the first paper to document household earnings responses to youth suicide. Results showed labour earnings for affected households dropped by 6.49% after suicide compared to their counterfactual earnings. The earnings drop persisted for at least two years post-suicide and is strongest for socio-economic groups with weak labour market attachment. These results may be used to improve the design of policies to help bereaved parents and to reduce the economic cost of bereavement.

[7] People with university qualifications are less likely to work in industries with high worker turnover rates than are people with high school or no qualifications (Stats NZ, n.d.-a, n.d.-b), and since workers in low-turnover jobs are more likely to meet the time-based eligibility criteria for employment leave, including bereavement leave (Employment New Zealand, n.d.), this suggests university graduates are more likely able to alleviate the earnings losses from youth suicide compared to other groups.

[8] Measured two years prior to suicide, individuals with no qualifications and high school graduates earn approximately 30% and 65% of university graduates, respectively.

2.9 Tables

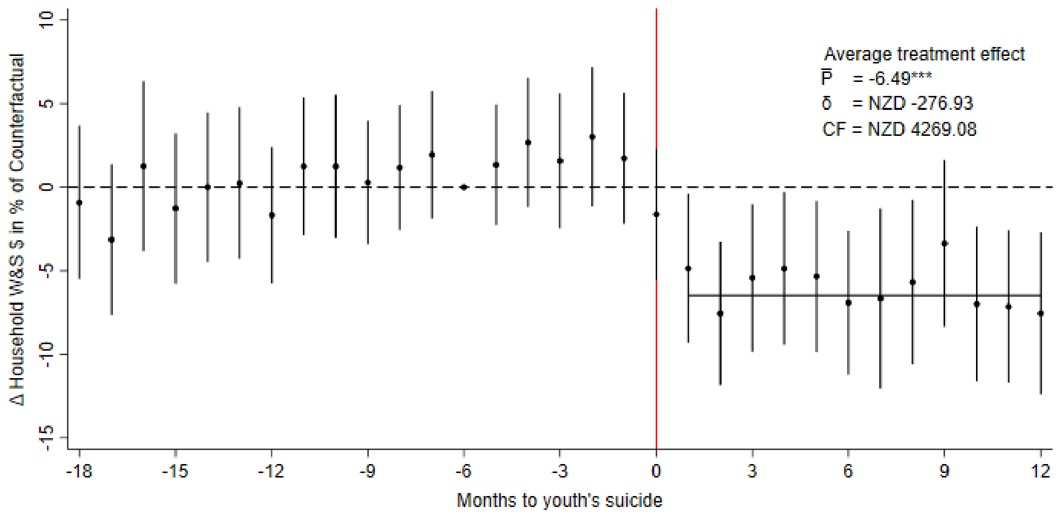
Table 2.1: Labour earnings response to youth suicide by households' socio-economic-status

Education	Labour earnings response (δ_1 NZD)	Labour earnings response in % of counterfactual (P_1)	Households
No qualification	-150.61*	-5.69*	516
High school	-368.00***	-7.62***	612
University	-305.72	-4.39	171

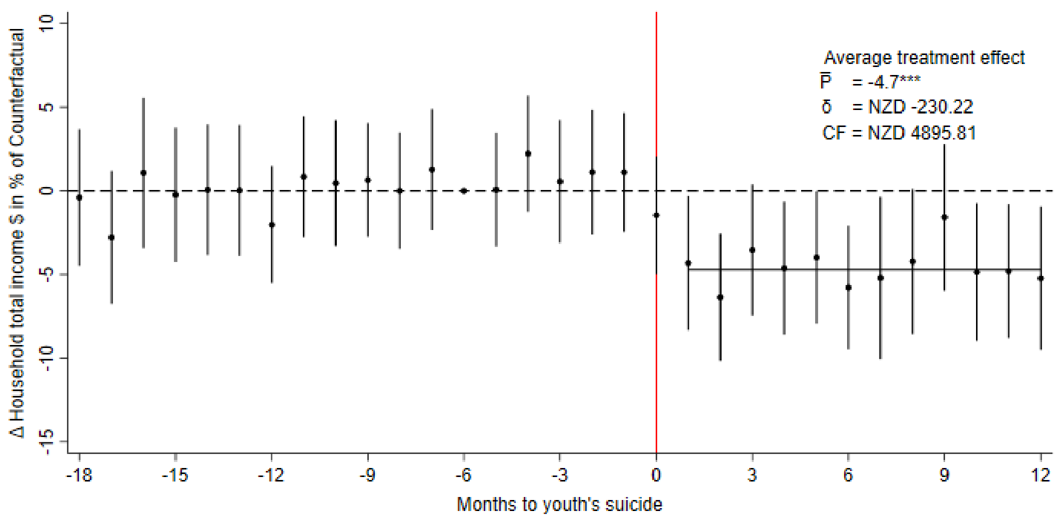
Notes: This table reports difference-in-differences estimates for households' labour earnings response to youth suicide separately for each education group using Equations 2.3 and 2.2. Column 2 reports δ_1 from Equation 2.3 and Column 3 reports results from Equation 2.3 converted into percent of counterfactual earnings using Equation 2.2. Counts of households in each educational attainment category are given in Column 4. We define household educational attainment by the highest completed degree of either parent. Stars denote significance as: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. Standard errors are clustered at the child level.

2.10 Figures

Figure 2.1: Household earnings response to youth suicide



(a) Labour earnings



(b) Total income

Notes: These figures show household labour earnings (Panel A) and total income (Panel B) responses to youth suicide using specification (1) and (2). Both measures are winsorised at the 99th percentile. The x-axis denotes months relative to youth suicide, normalised to period 0 for the treatment group (while the comparison group is treated at month 13). We normalise the comparison group's outcome to the earnings level of the treatment group six months prior to suicide. The vertical bars indicate the 95% confidence intervals. The black circles are the point estimates. The horizontal solid black line across months 1-12 is the average effect (δ_1) from Equation 2.3 measured in percent of the counterfactual earnings of the treatment group calculated according to Equation 2.2. The average effect in NZD and the counterfactual NZD earnings/total income are stated on the graph. Stars denote significance as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the child level. Both graphs include our full household sample n=1,299.

Chapter 3

Family impacts of child loss

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3.1 Prelude

This chapter is an extension of the published paper *Parental response to youth suicide: Evidence from New Zealand* from Chapter 2. Here, I compare the parental responses of youth suicide to that for youth traffic fatalities, the two most common causes of youth death in New Zealand. I use two estimation strategies in this paper and estimate the parental labour market costs of child loss over a longer time horizon than that in Chapter 2. Other differences in include adding mental health outcomes for parents, as well as the spillover effects to the youth's siblings and classmates. This paper was completed in 2024.

3.2 Abstract

Existing literature on the economic consequences of bereavement has primarily approached child loss as a singular event: but the cause of death matters. This study investigates how the economic and mental health impacts of child loss differ by cause-specific factors, focusing on suicide and traffic fatalities—two leading causes of youth mortality. Using rich administrative data from New Zealand and two empirical methodologies, we analyse short- and long-term effects on bereaved families. While traffic-bereaved parents recover earnings within four years, suicide-bereaved parents face persistent losses averaging 7% annually over a decade, with pronounced effects among low-income families. Mental health effects also extend to bereaved siblings and classmates, highlighting the widespread social toll of youth death, particularly from suicide.

3.3 Introduction

The death of a child is one of the most severe adversities a parent can endure, with far-reaching impacts on mental health, physical well-being, and economic stability. This profound loss disrupts daily functioning, undermines long-term health, and imposes significant economic strain.

Although the economic consequences of bereavement have attracted interest, the literature on the financial and social impacts of losing a child is still relatively sparse. van den Berg et al. (2017) examine the economic and health impacts of child loss due to non-intentional accidents, finding significant declines in parental labour income, employment stability, and marital cohesion. Mertz et al. (2023) further investigate the parental response specifically to youth suicide, underscoring the distinct social and psychological challenges of suicide, such as stigma, which they hypothesise may exacerbate economic impacts compared to other mortality causes. Despite these advances, current research has primarily approached child loss as a singular event without differentiating the economic consequences across causes of death.[1]

But the cause of death matters, as it can shape both societal and institutional responses to bereavement. Suicide, in particular, carries a historical and cultural stigma that intensifies the challenges faced by grieving parents. In many Christian communities, for example, suicides were once viewed as transgressions, with churches sometimes refusing burial in consecrated grounds or holding private services. This differentiation persists

[1]For example, in van den Berg et al. (2017), "non-intentional accidents" encompasses several different causes of death, including traffic accidents, drownings, suffocation, and accidents relating to electric current, steam, and falling objects. Notably it excludes suicide. While the authors discuss differing responses to non-intentional deaths versus anticipated deaths (e.g. cancer), the two death categories are not explicitly tested against each other.

in modern policy: in New Zealand, the Accident Compensation Corporation (ACC) offers funeral grants and survivor benefits to families bereaved by accidents, yet these entitlements are withheld if the death resulted from suicide. Such distinctions underscore how the cause of death can lead to varying levels of support, potentially amplifying the burdens on bereaved parents.

This study seeks to address this gap by testing whether the economic effects of losing a child differ based on cause-specific factors, focusing on suicide and traffic accidents—the two most common causes of youth mortality. Additionally, we explore heterogeneity in these impacts across parental gender, as well as mental health effects on wider family and social networks. By examining these layers of difference, this research aims to deepen our understanding of the economic toll of bereavement and inform policies that better support grieving families.

We build on existing research by leveraging rich, population-wide administrative data from New Zealand (NZ), allowing us to link birth, mortality, health, schooling, and tax records. The mortality records include detailed cause of death information, distinguishing between suicide and traffic accidents. The birth records enable direct linkage of deceased children to their parents and siblings, while school records allow us to trace classmates of the deceased. High-frequency data on employment, earnings, mental health service use, and prescriptions provide a foundation for examining the varied impacts of child loss on different networks.

To estimate the causal impact of child loss, our challenge is to identify a counterfactual outcome for bereaved parents had their child not died. For short-term, one-year effects, we follow the quasi-experimental approach used by Fadlon and Nielsen (2019, 2021) to mitigate selection bias, constructing a comparison group of parents who experience child loss in the near future. Specifically, we compare outcomes of parents whose child has just died with those whose child will die 13 months later. This design helps control for unobservable characteristics that may predispose families to particular outcomes, thus isolating the immediate impacts of child loss.

However, as the time separation between treated and future-bereaved groups increases, the assumption of parallel trends becomes less tenable. Thus, for long-term, 10-year effects, we turn to a matched control group approach, similar to van den Berg et al. (2017), identifying a cohort of comparable non-bereaved parents. We show that the results overlap with those obtained from the future-bereaved comparison group in the medium term, lending robustness to our findings.

Our analysis leverages high-frequency monthly payroll data along with pharmaceutical transaction data to document the immediate impacts of child loss, complemented by annual tax records that capture longer-term economic effects. The findings reveal notable differences in parental responses by cause of death. In the short term, parents experience an average monthly labour earnings loss of around 6%, accompanied by a marked increase in the use of mental health-related prescription drugs following their child's death, with similar immediate effects for both suicide and traffic fatalities. However, for the long run, while traffic-bereaved parents tend to recover within four years, suicide-bereaved parents face persistent earnings losses, averaging about 7% per year for the decade following the event. These losses are especially pronounced among lower-income parents, underscoring economic vulnerability within this group. Additionally, we find that mental health impacts extend to siblings and classmates, with effects more substantial among those bereaved by suicide.

The remainder of this paper is structured as follows. Section 3.4 provides an overview of relevant parental bereavement literature, focusing on both economic and health impacts. Section 3.5 offers background information on parental bereavement in the NZ context. Section 3.6 describes our data sources, including administrative linkages and high-frequency outcome variables. Section 3.7 explains our empirical strategy, with a focus on quasi-experimental and matching methods. Section 3.8 presents our main results on both short- and long-term impacts of child loss by death cause, including heterogeneity by parental income and gender. Finally, Section 3.9 concludes.

3.4 Literature

The existing literature on the impacts of bereavement is large and multi-disciplinary, with many papers studying the effects of spousal bereavement (see Ennis and Majid (2021)), sibling bereavement (see D'Alton et al. (2022)), and children bereaved by parental loss (see Guzzo and Gobbi (2021)).

We focus on a specific type of bereavement: namely parents experiencing the loss of a child. Papers within this domain have primarily focused on estimating the health effects of parental bereavement, with only a few studies investigating the economic consequences. We add to this literature in numerous ways. We first offer new estimates of parental mental health impacts of child suicide using large-scale administrative data. We also provide longer run estimates of the economic consequences of child loss by estimating parental labour market effects for up to 10 years after child loss. Finally, we test for differences within two specific external causes of death, namely suicide and traffic fatalities, as well as by parent gender.

Below we summarise the parental bereavement literature, categorised by whether the main focus is health or economic outcomes.

3.4.1 Parental health responses to child loss

Papers studying parental health effects of child loss unanimously find that the death of a child disrupts parents' mental and physical health, undermining daily functioning and well-being. However, these grief responses vary dramatically not only by the cause of death, but also by the gender of the parent.

By cause of death

Whether the effects of parental bereavement by child suicide are different from those for other causes of death remains unclear.[2] While some studies document clear differences in parental response by the child's death type, other studies find no evidence of differential effects of parental bereavement.

For instance, Seguin et al. (1995) and Feigelman et al. (2011) show that parents bereaved of child suicide experience more grief difficulties, feelings of depression and shame, and worse mental health outcomes, than parents whose child died by a traffic accident or another accidental death. de Groot et al. (2006) compare the need for professional help between parents of child suicide and parents whose child died by natural causes and found larger responses for the former. Although K. Dyregrov et al. (2003) show no differences

[2] Sveen and Walby (2008) reviewed 41 papers estimating the response to suicide versus other death types for bereavement in general (not for parents losing a child). Their review concluded there were no significant differences regarding the bereaved person's mental health, depression, anxiety, and suicidal behaviour, but some evidence that overall levels of grief may be higher for suicide bereavement.

in parents' subjective grief between a suicide or accidental death of their child, these responses are both significantly larger than that for parents whose child died by sudden infant death syndrome.

In contrast, Feigelman et al. (2009) document few substantive differences when measuring parents' grief difficulties for child loss to suicide, other traumatic external deaths, and natural causes. Rogers et al. (2008) find the same when comparing depressive symptoms, well-being, health problems, and marital disruption for parents of children who died by infant death, external causes, or due to illness.

Most of the literature exploring parental bereavement by different causes of child death are based on small samples from written surveys or interviews of parents who have experienced the loss of a child. In the abovementioned papers, the samples of parents bereaved by child suicide range from 30 (Seguin et al., 1995) to 462 (Feigelman et al., 2009).

Only a few papers use large scale administrative data to explore differences in parental bereavement by the cause of death of their child. One example is Bolton et al. (2013), who use administrative data for the entire province of Manitoba, Canada. The authors compare parents' health responses before and after a child suicide or motor vehicle accident, and compare that to a matched control group of non-bereaved parents. The administrative data allow for a larger sample size of 1,415 parents bereaved by child suicide. Their paper finds few differences in the relationship between bereavement and increased reports of depression, anxiety disorders, and marital breakups for parents bereaved by child suicide compared to traffic accidents.

Overall, it is well-established that child suicide leads to significant negative health responses by parents, but there is mixed evidence as to whether these responses are different from that for other causes of child loss. Understanding whether there are differences in health responses based on death type would offer valuable insights for policymakers, enabling them to tailor bereavement support services and related policies more effectively.

By parent gender

Papers exploring gender differences in parental bereavement typically study child loss in general, controlling for cause of death rather than comparing across death types. Evidence from written surveys and interviews of bereaved parents have long revealed differences in how mothers and fathers respond to losing their child. While mothers tend to experience greater and longer-lasting negative emotional responses to grief than fathers (A. Dyregrov and Matthiesen, 1987; Schwab, 1996; Sidmore, 2000; Alam et al., 2012 ; A. Dyregrov et al., 2019), fathers are more likely to cope with grief through task-focused activities, such as immersing themselves in work (Alam et al., 2012; Proulx et al., 2015; McNeil et al., 2021). Fathers are also more pleased than mothers about the level of communication and support in their relationship after losing a child (A. Dyregrov et al., 2019).

Evidence from administrative data reach the same general conclusion that parents' health is negatively affected by child loss and that such effects are greater for mothers than fathers. Like the survey- and interview-based studies mentioned above, these papers generally control for death type rather than looking at death causes separately.[3] A summary of these papers is provided below.

In the Danish setting, there is a series of papers that use population-wide register data

[3]The exception is Rostila et al. (2012) who find the differential risk of mortality for mothers and fathers experiencing child loss is similar between natural and unnatural causes of death.

to study the impact of child death on a range of health outcomes for parents. Results show that the loss of a child leads to an increased risk of cancer (Li, Johansen, et al., 2002), myocardial infarction (Li, Hansen, et al., 2002), multiple sclerosis (Li et al., 2004), psychiatric hospitalisations (Li et al., 2005), diabetes (Olsen et al., 2005), parental suicide (Qin & Mortensen, 2003), and all-cause mortality (Li et al., 2003), with stronger effects for mothers than fathers.

In other parts of Scandinavia, Rostila et al. (2012) and Christiansen et al. (2020) also document that child loss increases the risk of mortality for parents. Rostila et al. (2012) study the impacts of child loss on all-cause mortality amongst parents in Sweden and show that the heightened mortality risk differs between mothers and fathers depending on the child age and the follow up period. Christiansen et al. (2020) look at alcohol-related mortality for bereaved parents relative to non-bereaved parents in Norway, with larger effects shown for mothers over fathers.

Finally, van den Berg et al. (2017) also document that the loss of a child leads to an increased likelihood of divorce, hospitalisation, as well as mental health issues using Swedish administrative data. They show that the effects tend to be larger and more persistent for mothers than fathers.

Overall, the literature suggests parents respond differently to the loss of a child, with mothers exhibiting more pronounced health responses than fathers. Less is known about whether these gender differences also differ by the child's cause of death. While Rostila et al. (2012) found that parental gender differences in the heightened mortality risk after child loss is similar for natural and unnatural causes of death, no studies have explored these gender differences within different unnatural death types.

3.4.2 Parental economic responses to child loss

Less attention has been given to the economic consequences of parental bereavement from child loss. There are four papers that explore this, two based on interviews of bereaved parents and two using register data.

Dussel et al. (2011) surveyed 229 families who lost children to cancer in tertiary-care paediatric hospitals in America and Australia. The study found that parents experienced great financial hardship from the child's illness journey, with high rates of job separations, income losses, and increased poverty. Fox et al. (2014) surveyed 213 American parents who had lost a child and found significant productivity losses within the first six months following the death. The authors showed that the cost of unproductive hours exceeded the cost of reduced work hours.

van den Berg et al. (2017) are the first to study the parental economic consequences of child loss using population-wide administrative data. As prefaced above, the authors study parental bereavement in Sweden, comparing outcomes of parents who lost a child to non-intentional accidents with those of matched non-bereaved parents. Results show significant adverse effects on labour earnings and employment, with average labour earnings losses estimated to be 8.8% for fathers and 12.5% for mothers measured six years after the loss.

Mertz et al. (2023) add to van den Berg et al. (2017) by studying the parental labour market effects of child suicide, which is explicitly excluded from the Swedish study. Mertz et al. (2023) use administrative data from NZ to show parents' labour earnings and total income fall due to child suicide, with average household labour earnings falling by 6.5%.

This current paper is closely related to van den Berg et al. (2017) and Mertz et al.

(2023); however, we add to these studies, and the broader literature on parental bereavement, in several ways. First, we provide the first estimates of the mental health effects of parental bereavement to child suicide using large-scale administrative data. Second, we extend the time horizon for estimating the labour market costs of parental bereavement, from six (in van den Berg et al. (2017)) to 10 years, to better understand longer-term effects of bereavement. Third, we explicitly test for differences in parental responses by two specific causes of external deaths, as well as by parent gender. Finally, we estimate spillover effects to the wider network of siblings and classmates of the deceased child to paint a more complete picture of the devastation caused by youth death.

3.5 Background

3.5.1 Death processes

When an unnatural death occurs in NZ, the first respondents are often ambulance paramedics, followed by the NZ Police who are legally required to investigate the cause of death. If the death was the result of a road crash, there are a number of specialist road policing groups (for example, the Serious Crash Unit) that will also investigate the fatal crash.

As part of their investigation, the NZ Police will refer the death to the coroner, who will conduct a public inquiry into the circumstances around the death, which may include an autopsy by a pathologist. The NZ Police will then produce a final report of the cause of death. Once there is confirmation of the cause of death, the death is recorded in the Ministry of Health's mortality database. This database is made available to researchers in Statistics NZ's Integrated Data Infrastructure (IDI).

3.5.2 Bereavement support services in NZ

Whilst there are general bereavement support services available for bereaved parents in NZ, there are some differences in these services depending on the child's cause of death.

In terms of financial support services, there are two types of funeral grants available to bereaved families, the eligibility of which depends on how the youth died. The largest is provided by the Accident Compensation Corporation (ACC) scheme,[4] which is available for parents who lose a child to injury-related causes, meaning traffic fatalities likely fall under this definition.[5] The ACC Act 2001 explicitly disentitles claims relating to those who died from suicide or wilfully inflicted injuries.

For suicides and other deaths outside the scope of ACC, the Ministry of Social Development (MSD) offers a means-tested funeral grant. Parents who experience child loss can qualify for this funeral grant if their income is below a certain threshold.[6]

Beyond funeral grants, parents bereaved by child loss may be able to get financial assistant through the regular social welfare system, which is unrelated to the cause of death of their child. These benefits are mostly means-tested and depend on employment status, health and disability status, relationship changes, or caregiving responsibilities.[7]

[4] In general, the ACC scheme provides financial support to surviving family members. However, outside the ACC funeral grant, the 'standard' ACC scheme is unlikely to apply to parents bereaved by child loss. One of the main reasons for this is that most of the financial support provided by ACC is directed toward the deceased's surviving partner and children (i.e., those relying on the deceased's income). Thus, for the case of child loss, parents are usually excluded from the one-off survivor grants and loss of income payments. An exception might be if the parents were financially reliant on the child's income, in which case the parents would be considered a dependent of the deceased youth and could qualify for the income compensation scheme.

[5] As of September 2024, the ACC funeral grant is $7,793.13, which is approximately equivalent to 2.5 months of (post-tax) earnings for a full-time minimum wage earner. For murder or manslaughter victims (which may be the case for traffic fatalities), the family can get an additional $10,000 from the Ministry of Justice.

[6] The largest funeral grant is $2,559.20. For more information on the income eligibility thresholds, click here.

[7] For more information on the available MSD benefits, click here.

If the parent is employed at the time of their child's death, they are legally entitled to at least three days paid bereavement leave.[8]

If the deceased had private life insurance, the surviving family members may be eligible for a payout. However, in most cases, insurance companies only provide life insurance for those aged 18 and older,[9] so this is irrelevant for cases of child death under age 18.[10]

Regarding non-financial services, there are many organisations that provide free emotional and practical support for bereaved families in NZ, for example Victim Support, Lifeline, Grief Centre, and Depression Helpline. Some organisations specialise in specific types of bereavement, reflecting the understanding that grief responses can differ by cause of death. For example, the Mental Health Foundation and Skylight provide support programmes for those bereaved by suicide, and Brake Aotearoa New Zealand offers support for those bereaved by a road crash.

3.6 Data

We use linked register data from Stats NZ's IDI.[11] We use mortality registers to identify deceased youth and their cause of death. The biological parents and siblings of the deceased youth are identified through the NZ birth register, while school enrolment records allow us to identify the deceased's classmates. We obtain labour market information from the Inland Revenue Department's (IRD) monthly tax register. Measures of mental health

[8] For more details, click here

[9] For example, see Southern Cross Health Insurance.

[10] Life insurance is also sometimes attached to one's KiwiSaver pension scheme (e.g. Booster), which provides free accidental death cover if the person dies between ages 18 and 65 and meets certain KiwiSaver contribution requirements.

[11] The IDI is a population-wide register that links individuals across government and non-government datasets using probabilistic matching on first and last names, birth dates, and addresses. For more information, click here.

are constructed using administrative records from the Ministry of Health and PHARMAC, which capture transactional-level mental health service usage and prescription data, respectively. In the sections that follow, we outline the sample construction process, define the outcome variables, and present descriptive statistics.

3.6.1 Sample construction

The Ministry of Health's mortality register contains information on all recorded deaths in NZ. We identify all youth aged 0-30 who died between 2001-2019 (N=13,512).[12] Using ICD-10 codes, we identify the youth's cause of death, of which the two most common are suicide (N=3,363 or 24.9%) and traffic accidents (N=3,318 or 24.6%).[13,14]

We link the deceased youth to their biological parents using the NZ birth register and restrict the sample to parents who were of working age (ages 30-60) at the time of their child's death. For the occasional cases where a mother or father has experienced the loss of more than one child, we focus on the first death event. We append the mother-child and father-child dyads to give 4,587 parent-child dyads for the suicide sample and 4,311 parent-child dyads for the traffic sample.[15]

A similar process is followed to identify the biological siblings of the deceased youth. For the sibling analysis, we restrict the deceased NZ-born sample to be those who died

[12]Excluding utero and neonatal deaths.

[13]Suicides are identified from the "Intentional self-harm" ICD-10 codes X60-X84. Traffic deaths are identified from the "External accidental deaths: Transport accidents" ICD-10 codes V00-V99.

[14]The third most common specific cause of death is drowning and submersion into/while in natural water, comprising approximately 2.3% of the deceased youth sample. If considering a higher-level group of mortality causes, the next most common is deaths from different types of neoplasms, of which collectively make up about 10% of the sample.

[15]Approximately 80% of deceased youth are linked to the NZ birth register, meaning about 20% were born overseas. We are unable to identify the parents of youth born outside NZ due to the absence of a NZ birth certificate. After applying age restrictions to parents of NZ-born children who died by suicide or traffic accidents, the linkage rate is around 65-70% depending on the death type. The rate is higher for mothers than fathers, as some birth certificates do not list the father.

at ages 10-30 so the younger siblings are at least young children (and not infants) at the time of death. We consider an individual to be a sibling of the deceased if they share the same biological mother on their NZ birth certificate. Like for parents, we focus on the sibling's first experience of a sibling death, giving a total of 6,066 siblings bereaved by suicide and 4,914 siblings bereaved by traffic fatalities.

To identify classmates of deceased youth, a few steps are followed. First, we restrict the sample to schooling aged children (ages 5-15) at the time of death.[16] Second, we link the deceased to the Ministry of Education's school enrolment register to observe their most recent enrolment and keep only those whose "reason for leaving" was coded as "deceased". This allows us to identify the school the youth was attending when they died.[17] Third, we use the enrolment register to identify children enrolled at the same school as the deceased and define classmates as children born in the same year and who attended the same school as the deceased at the time of death.[18] For cases where the school cohort experiences more than one death event, we only use the first death. This results in 24,072 classmates of suicide victims (207 schools) and 18,984 classmates of traffic victims (219 schools).

[16]We define schooling age as 5-15 to allow one a one-year post-death period before compulsory schooling ends for classmates.

[17]An implication of this linking process is that we are not able to identify classmates of youth who died in between school enrolments (e.g, if they were switching schools or moving from an intermediate to high school).

[18]Note that this is a proxy measure of classmates. Some schools may combine two birth cohorts in a single classroom, but this varies by school and year-level.

3.6.2 Outcome variables

Mental health outcomes

We draw on two health registers to create measures of mental health. First, we use the Ministry of Health's Programme for the Integration of Mental Health Data (PRIMHD) register, which contains individual-level usage of publicly funded mental health support services. We focus on the most common type of support services, being those provided by community mental health teams.[19] These services are often conducted in groups, focusing on behaviour therapy, mindfulness, stress management, and emotional support exercises. We create an indicator equal to one if the individual (parent, sibling, or classmate) attended at least one mental health service in a month, and equal to zero otherwise.

Second, we use the Ministry of Health's and PHARMAC's pharmaceutical register to observe prescriptions for different mental-health related issues. The pharmaceutical data provide information about the prescribed drug and the date the prescription was dispensed at a local pharmacy. We create a combined indicator equal to one if the individual filled out a prescription for anxiolytics (for anxiety), anti-depressants (for depression), or anti-psychotics (for psychosis) in a month, and equal to zero otherwise.

Labour market outcomes

We use the IRD's tax register to obtain information on parents' monthly labour earnings and define an employment indicator equal to one if they received positive labour earnings in a month, and equal to zero otherwise. The tax register also contains information on

[19]As opposed to alcohol- or drug-focussed teams, or teams based within hospitals or other medical wards.

monthly benefit earnings, which combines earnings from all main taxable benefits.[20] Since the ACC funeral grant is tax-free, this is not included in the IRD's measure of benefit earnings. Finally, we create a measure of total income by summing earning across all taxable sources.[21]

3.6.3 Descriptives

Figure 3.1 plots the age-death profile of deceased youth by cause of death for the years 2001 to 2019. Figure 3.1 shows that while traffic fatalities happen throughout the youth age distribution, peaking at age 18, suicide deaths begin in the early teenage years, peaking at age 21. About 75% of the deceased youth sample are male for both suicides and traffic deaths.

Table 3.1 compares characteristics of bereaved parents to the general population of parents in NZ. Column 1 provides the 2018 characteristics for the sample of parents whose child died by a traffic accident (one year later in 2019) and column 2 provides the same for an age-matched sample of parents from the general population.[22] A similar comparison is made in columns 3 and 4, comparing suicide-bereaved parents to a matched sample of parents from the general population. The asterisk in column 2 [4] illustrates significant differences between traffic[suicide]-bereaved parents and their age-matched general population of parents. We focus only on parents experiencing child loss in 2019 so that information sourced from Census 2018 is measured pre-child loss.

[20] This includes the emergency benefit, job seeker support, sole parent support, supported living payments, young parent payment, and youth payment. These are all means-tested.

[21] All taxable sources include: wages and salaries, benefit earnings, claimants income, withholding payments, paid parental leave, pension payments, and student allowance.

[22] The age-matched general population of parents is created on one-to-one exact matching on parent age, such that the sample size and average age is the same as the bereaved sample of parents.

In general, bereaved parents (columns 1 and 3) are more likely to be of low socio-economic status, more likely to be Māori, less likely to have completed higher education, more likely to live in deprived areas, and more likely to have lower labour earnings and higher benefit receipt than the general population of parents. Thus, bereaved parents appear to be a highly selected sample.

In contrast, differences between traffic-bereaved parents (column 1) and suicide-bereaved parents (column 3), are small and not statistically significant. The only exception relates to age. The two-year age gap between traffic-bereaved parents (48 years) and suicide-bereaved parents (50 years) is statistically significant at the 1% level, reflecting the different age-distributions of children who die by suicide (21.5 years) versus traffic accidents (20.5 years).

3.7 Empirical Strategy

The main objective of this paper is to identify the causal effect of child loss on parents' labour market participation and to test whether such responses differ by the child's cause of death and by parent gender. The main empirical challenge is to estimate bereaved parents' outcomes had their child not died. Because of the strong selection into child loss (see Table 3.1), simple comparisons between parents who do and do not experience child loss are likely to yield biased estimates. Instead, to estimate immediate effects, we exploit the timing of the child's death to create a comparison group of parents who experience the same type of child death in the future. To identify longer-term effects, we use propensity score matching to create a comparison group consisting of parents who never experience the loss of a child, but who are similar to parents that do in terms of their observable characteristics.

We explain the construction of these comparison groups and the difference-in-differences design below. For simplicity, this discussion is in the context of parents' labour earnings responses to child loss; however, the same strategies are used for other outcome variables and for the responses by siblings and classmates.

3.7.1 Difference-in-differences: Yet-to-be-treated comparison group

To identify the immediate effects of child loss on parental outcomes, we exploit the potential randomness in the timing of child loss.[23] Specifically, for parents who experience the loss of a child in month t, we construct a comparison group consisting of parents who experience the loss of a child 13 months later ($\Delta = 13$). We centre the event timeline around month 0, which is the month of child loss for the treated parents and is a placebo treatment month for the comparison group, who subsequently lose a child in month 13. The treatment group's event timeline runs 12 months before and after the child loss, which is 25 pre-death months for the comparison group. We then stack treated and yet-to-be-treated pairs over each month between 2001 and 2019.

For ease of explanation, we begin with a fully-interacted two-way fixed effects regression, as shown by Eq. 3.1.

$$Y_{i,t} = a_i + b_i \cdot \text{Treat}_i + c_i \cdot \text{Post}_{i,t} + d_i \cdot \text{D}_{i,t} + \varphi \cdot X_{i,t} + u_{i,t} \tag{3.1}$$

Where:

[23] This follows Fadlon and Nielsen (2019, 2021) who study the effect of (adult) fatal and non-fatal health shocks on the health and economic behaviours of spouses and other family members.

$$\mathrm{D}_{i,t} = \mathrm{Treat}_i \cdot \mathrm{Post}_{i,t}$$

$$\theta_i = \theta_0 + \theta_1 \cdot \mathrm{Z}_i \quad \text{for} \quad \theta \in \{a, b, c, d\}$$

Substituting $\mathrm{D}_{i,t}$ and θ_i gives:

$$\begin{aligned} Y_{i,t} = a_0 + a_1 Z_i + b_0 \cdot \mathrm{Treat}_i + b_1 Z_i \cdot \mathrm{Treat}_i \\ + c_0 \cdot \mathrm{Post}_{i,t} + c_1 Z_i \cdot \mathrm{Post}_{i,t} + d_0(\mathrm{Treat}_i \cdot \mathrm{Post}_{i,t}) \\ + d_1 Z_i(\mathrm{Treat}_i \cdot \mathrm{Post}_{i,t}) + \varphi \cdot X_{i,t} + u_{i,t} \end{aligned} \tag{3.1a}$$

In Eq. 3.1, $Y_{i,t}$ is labour earnings for parent i in time t, a_i is a constant, $Treat_i$ is an indicator equal to one if parent i is in the treatment group and zero otherwise, $Post_{i,t}$ is an indicator equal to one if time t is within event months 0-12 and zero otherwise, and $D_{i,t}$ is the interaction between these two indicators. $X_{i,t}$ is a set of birth year and calendar time fixed-effects, and standard errors are clustered at the deceased-level.

Z_i is a cause-of-death indicator, which is equal to one if parent i's child died by suicide and is equal to zero if parent i's child died by a traffic accident.[24]

In Eq. 3.1a, the d_0 identifies the causal effect of losing a child in a traffic accident on parent's labour earnings and the d_1 identifies the additional effect on parent's labour earnings for parents whose child died by suicide, over and above the effect of child loss from a traffic accident. When $Y_{i,t}$ is a labour market variable, we also report our causal estimates of d_0 and d_1 as a percentage of the counterfactual outcome of the treatment group, as shown in Eq. 3.2.

[24] For precision, we do not interact the fixed effects with Z_i, but the point estimates are unchanged if we do.

$$P_e = \frac{d_e}{E[\hat{Y}_{i,t} \mid e, \text{Treat}_i = 1]} \tag{3.2}$$

Where the predicted counterfactual outcome for parent i in event time e is given by: $\hat{Y}_{i,t} = \hat{a} + \hat{b} + \hat{c} + \hat{\varphi} \cdot X_{i,t}$.

These effects have causal interpretation under the assumption that had the child death not occurred, the labour earnings trajectory of the treated group would have continued in the same way as that for the comparison group, and that there are no other shocks to the labour earnings of parents in the treatment group occurring simultaneously.

We provide evidence in favour of the parallel trend assumption by showing that the earnings of the treatment and comparison groups follow similar trends in the months leading up to the treatment group's child loss. To do so, we estimate a dynamic difference-in-differences design, where we substitute the $Post_{i,t}$ indicator in Eq. 3.1 with a set of 25 event month indicators (M_e) (i.e. $M_e = 1[e = t]$). The event month indicators range from 12 months before to 12 months after the death. As before, month 0 indicates the death month and we use month -3 as a reference category. This dynamic design is expressed in Eq. 3.3. Support for the parallel trends assumption is shown if the pre-child death estimates ($e < 0$) are close to zero.

$$Y_{i,t} = a_i + b_i \cdot \text{Treat}_i + c_i \cdot \text{Tline}_e + d_i \cdot D_{i,e} + \phi \cdot X_{i,t} + u_{i,t} \tag{3.3}$$

Where:

$$\mathrm{D}_{i,e} = \mathrm{Treat}_i \cdot \mathrm{Tline}_e$$

$$\theta_i = \theta_0 + \theta_1 \cdot Z_i \quad \text{for} \quad \theta \in \{a, b, c, d\}$$

$$\mathrm{Tline}_e = \sum_{e=-12, e\neq -3}^{12} (\gamma_e \cdot M_e)$$

Eq. 3.1 and 3.3 are used to estimate the short-term health and economic effects of parental bereavement by child suicide compared to child loss from traffic accidents. The same approach is used to estimate spillover effects to the mental health of siblings and classmates, in which case the comparison groups for siblings and classmates, respectively, are those affected 13 months later.

To investigate if there are differential responses by parent gender, we estimate separate regressions by the child's cause of death, and replace Z_i in Eq. 3.1 and 3.3 with an indicator equal to one if the parent is a mother and zero if the parent is a father.

3.7.2 Difference-in-differences: Matched comparison group

We estimate the long-term effects of child loss using a matched difference-in-differences design.[25] To do so, we transition from monthly to annual data, a key reason being that a 10-year post-period is more intuitive than a 120-month post-period. Appendices B1 and B2 discuss the implications of this shift.[26]

In the matching approach, the treatment group is still bereaved parents, but the

[25] Similar designs have been used in van den Berg et al. (2017) who match bereaved parents who lost a child to non-intentional accidents or cancer to a sample of non-bereaved parents to evaluate the economic effect of child loss; Aneja and Xu (2021) who estimate the labour market consequences of employment segregation in the U.S. by matching black civil servants to white counterparts; and Adams-Prassl et al. (2022) who evaluate the effects of between-colleague assaults in Finland by matching victims and perpetrators to workers who didn't experience violence.

[26] Appendix B3 shows that, using annual data, our medium-term results are consistent across both the yet-to-be-treated comparison group setup and the matched comparison group setup.

comparison group is constructed from a pool of never-bereaved parents.[27] We construct separate comparison groups for parents bereaved by suicide and for parents bereaved by traffic accidents.

We construct the matched comparison groups in two steps. First, we do exact matching on parent gender and age (measured in 5-year bins from 30-60). Second, we use propensity score matching and identify the 10 nearest neighbours based on the following characteristics: calendar year (death year for treated parents, placebo death year for never-treated parents[28]), child age, and the outcome variable measured three years pre-death.[29]

To estimate the dynamic effects of parental bereavement over a 10-year post-period, we replace the event month indicators (M_e) in Eq. 3.3 with event year indicators (EY_y). These indicators range from 4 years before to 10 years post-death, with the death occurring in year 0 and year -1 set to as the base year. This specification is shown in Eq. 3.4, where standard errors are clustered at the deceased-youth level.[30]

$$Y_{i,t} = a_i + b_i \cdot \text{Treat}_i + c_i \cdot \text{Tline}_y + d_i \cdot D_{i,y} + \varphi \cdot X_{i,t} + u_{i,t} \tag{3.4}$$

Where:

[27]"Never" bereaved means these parents have not experienced child loss within our sample timeframe.

[28]For example, for parents observed in 2007, this will be event-year 0 for parents whose child died in 2007, and will also be event-year 0 for non-bereaved parents.

[29]We include the outcome variable squared, cubed, and raised to the fourth power. When the outcome variable is labour earnings, we also include indicators if the parent is unemployed in each of the three pre-death years.

[30]Note that any effects on the standard errors from the matching procedure are not incorporated in this calculation of standard errors. However, van den Berg et al. (2017) and Adams-Prassl et al. (2022) also take a standard approach without an adjustment for the pre-estimation matching procedure.

$$\mathrm{D}_{i,y} = \mathrm{Treat}_i \cdot \mathrm{Tline}_y$$

$$\theta_i = \theta_0 + \theta_1 \cdot Z_i \quad \text{for} \quad \theta \in \{a, b, c, d\}$$

$$\mathrm{Tline}_y = \sum_{y=-4, y \neq -1}^{10} (\gamma_y \cdot EY_y)$$

We also estimate a dynamic specification where the 10-year post-death indicators are grouped into years 0-4 and years 5-10, and use the equivalent of Eq. 3.2 to translate these annual estimates into a percentage of counterfactual outcomes.

3.8 Results

We first investigate the effect of child loss on parents' mental health and labour market outcomes, and whether these responses differ by the child's cause of death or by the parent's gender.[31] We then explore if there are spillover effects on the mental health of the deceased's siblings or classmates.

3.8.1 The effect of child loss for parents

Parents' mental health

We begin by examining how child loss affects parents' mental health, providing a deeper understanding of the emotional and psychological toll that accompanies such a loss. Table 3.2 presents our estimates of the effect of child loss on parents' mental health using the two-way fixed effects specification (Eq. 3.1 and 3.1a). For each mental health outcome,

[31] We also test for parents' physical health responses using a hospitalisation register and prescriptions for pain-relief medication and find no response. This suggests the parental responses presented in this section are due to the child death itself and not from any injuries the parents may have sustained at the same time. Additionally, we also find no effect of child loss on marital status.

the first row contains the estimates for traffic deaths (d_0) and the second row for suicide deaths ($d_0 + d_1$). We report the difference between the two estimates (d_1) in Column 4.

Table 3.2 reveals that bereaved parents increase their use of mental health services and prescriptions for mental health related issues when their child dies. For mental health services, suicide-bereaved parents increase their usage by 0.87 percentage points (or 218% of baseline) on average per month, over the first year. This excess uptake is significantly larger than that for traffic-bereaved parents. For prescription drugs, the effect is also larger in magnitude for suicide-bereaved parents, although the difference is not significant.

Next, Table 3.3 explores whether the mental health response differs between mothers and fathers. For each death type and each mental health indicator, we report the effects for fathers and mothers, with the difference shown in column 4. We find that while both parents experience a deterioration in their mental health from child loss, the effects are significantly larger for mothers, especially when the child's death was due to suicide.

These results are obtained by estimating Eq. 3.1 conditional on the child's death type, replacing Z_i with an indicator equal to one if the parent is a mother and equal to zero if the parent is a father. Thus, each row is estimated separately for traffic or suicide deaths, and the responses for mothers and fathers are tested against each other. For example, looking at prescription use resulting from child suicide, we see the likelihood that mothers fill out prescriptions for mental health-related issues increases by 4.46 percentage points, on average per month, while the uptake for fathers is 1.85 percentage points. This difference 2.60 percentage points is highly significant.

Parents' labour market participation

Short-term effects

We next estimate the short-run labour market effects of losing a child using yet-to-be treated parents as the comparison group. We estimate Eq. 3.3 where Z_i is the cause-of-death indicator and the outcome variable is either labour or benefit earnings. Figure 3.2 plots the estimated effect of child loss on earnings and benefit income for parents bereaved by suicide (Panel 3.2a) and traffic deaths (Panel 3.2b). The combined baseline earnings in month -3 is equivalent to a weighted average of the death-specific baseline earnings. Table 3.4 presents the results from the two-way fixed effect model in Eq. 3.1. It is structured similarly to Table 3.2, with the addition of expressing the effects as a percentage of counterfactual (CF) income using Eq. 3.2.

Figure 3.2 shows an immediate decrease in labour earnings for parents experiencing child loss. While the drop in parents' labour earnings is persistent throughout the 12-month post-death period for suicide, the drop is initially larger and then stabilises in the latter months for traffic fatalities.[32] Despite these differing trends, the average monthly labour earnings losses within the first year post-death is the same for both suicide-bereaved and traffic-bereaved parents ($120 NZD or 6.3%, Table 3.4).

The lost labour earnings are not offset by comparable increases in benefit income. While there is a small but statistically significant rise in parents' benefit income (about $13 (or 6%) per month, for both traffic- and suicide-bereaved parents) these gains are small relative to losses in labour earnings. It is therefore unsurprising that we see a

[32]A similar pattern is seen on the extensive margin when we plot the effects on parents' employment rate (see Appendix Figure B0.1).

significant loss of total income for parents bereaved by child loss (Table 3.4).

Table 3.5, structured similarly to Table 3.3, reveals gender differences in parents' short-term responses to child loss. For traffic-bereavement, mothers' labour earnings losses ($160 or 8.3%) are statistically significant larger than that for fathers' ($64 or 3.5%). Conversely, fathers' short-term labour earnings responses to child suicide ($166 or 8.4%) are larger than that for mothers ($95 or 5.0%), although this difference is statistically insignificant.

Long-term effects

We now estimate the long-run effects of child loss on annual earnings for bereaved parents using a matched comparison group of never-treated parents. Figure 3.3 illustrates the impact of child suicide on parents' labour earnings, and Figure 3.4 presents the same for traffic fatalities.

Figures 3.3a and 3.4a plot annual labour earnings for bereaved parents over their event timeline ranging from four years before to 10 years after the child's suicide or traffic fatality, respectively. We also plot the labour earnings trajectories for the general population of non-bereaved parents and a matched comparison group of non-bereaved parents.[33] In line with the descriptive statistics presented in Table 3.1, these figures show that child loss from suicide or traffic fatalities is more prevalent among parents with low earnings.

Comparing the earnings trajectory of the treatment group to that of the matched comparison group illustrates the long-term treatment effects. For suicide bereavement, the raw plot (Figure 3.3a) shows a notable decrease in parents' labour earnings when their child dies that persists for the entire 10-year post-death period. In contrast, for traffic

[33]The matching procedure is done separately by death type, as outlined in Section 3.7.2.

bereavement (Figure 3.4a), we see a small decrease in parents' labour earnings within the first few years after child loss, but this seems to recover from year 4 onward.

Figures 3.3b and 3.4b plot the point estimates and 95% confidence intervals from Eq. 3.2 for suicide and traffic deaths, respectively. In both cases, the point estimates are generally centred around zero and are insignificant in the years leading up to the child's death, providing empirical support for the parallel trends assumption. While the estimated earnings losses for parents losing a child to suicide are large and statistically significant in all 10 years after child loss (Figure 3.3b), the earnings losses for parents losing a child to traffic accidents are smaller in magnitude and only significant in the first year after child loss. It appears that earnings recover for years 4-7, and then start to decline again, with the year 9 and 10 losses being similar in magnitude to the year 1 losses, but insignificant (Figure 3.4b).

Table 3.6 presents estimates of the effect of child loss on parents' labour earnings splitting the post-period into two 5-year bins (years 0-4 and years 5-10), estimating Eq. 3.4 for all parents, and for mothers and fathers separately. The first two rows shows the average annual labour earnings loss for parents bereaved by child suicide is $1,457 per year (or 6.6% of counterfactual earnings) over the first five years and $1,900 year (or 7.7%) for the remaining five years. These effects are statistically significant at the 1% level. In contrast, the average annual labour earnings losses for traffic-parents sits around $350 (or about 1.6%) in each 5-year period, both of which are insignificant.

The subsequent rows of Table 3.6 reveal a striking gender difference in parents' long-term responses to child loss.[34] Mothers experience significant earnings losses regardless

[34]Here we estimate Eq. 3.4 separately for mothers and fathers and where Z_i is the cause-of-death indicator.

of the cause of death, with magnitudes consistently larger for suicide but not significantly different from traffic fatalities. In contrast, the cause of death plays a much larger role for fathers. Fathers experience large and significant labour earnings losses following their child's suicide, but show positive (though insignificant)[35] earnings responses to child traffic fatalities.

We posit two possible explanations for the positive point estimate for traffic-bereaved fathers. First, existing literature on gender differences in parental bereavement suggests fathers are more likely to deal with grief through immersing themselves in work and distracting themselves with task-based activities (Alam et al., 2012, Proulx et al., 2015, McNeil et al., 2021). Thus, it is possible that fathers increase their time spent at work as a coping mechanism for the loss of their child.

The second explanation could be that mothers and fathers are able to coordinate their labour market responses when their child dies by a traffic accident. Perhaps the household decides that the mother will take some time off work, consistent with the literature showing mothers have greater negative emotional responses to grief than fathers (e.g. Sidmore (2000)), and that the father will increase time at work to compensate for the mothers' decreased earnings.

However, these explanations are only consistent with fathers' responses to child loss from a traffic accident, since fathers experience significant labour earnings losses when bereaved by child suicide.[36] These differences are not reconciled by fathers' mental health response to child suicide versus traffic fatalities: the point estimates in Table 3.3 show

[35] These effects are somewhat close to the 10% significance level, with the t-statistic for the increase in fathers' average labour earnings for years 0-4 is 1.47 and for years 5-10 is 1.40.

[36] Father's annual average labour earnings losses across years 0-4 after child suicide is significant at the 10% level (t=-1.76), while the effect across years 5-10 borders significance, with a t-statistic of -1.63.

larger responses for fathers of child suicide than traffic for the uptake in prescription drugs, while the opposite for fathers' uptake in mental health services.

Perhaps there may be an unobservable difference in fathers' grief responses to child suicide compared to traffic deaths, which makes it harder to continue working in the case of the former. This finding adds to the parental bereavement literature, suggesting that gender differences in bereavement from child loss also varies by the cause of death of the child, and not just for natural versus unnatural causes of death.

Heterogeneity in long-term effects

Figure 3.5 explores heterogeneity in parents' five-year labour earnings responses to child loss across the earnings distribution. We plot the effects as a percentage of counterfactual outcomes for employed parents across five earnings quintiles.[37]

Figure 3.5 shows that earnings losses from youth suicide are evident across the whole earnings distribution,[38] with proportionately larger effects for lower earners (18% of counterfactual earnings) than for higher earners (5% of counterfactual earnings). The distribution of losses seen for youth traffic fatalities are mostly concentrated amongst lower earners. A similar pattern is seen for the extensive margin effects in Appendix Figure B0.3.

[37]Earnings quintiles are calculated separately by death type. The patterns in Figure 3.5 remain the same if instead we estimate the earnings quintiles for suicide and traffic parents together, as shown in Appendix Figure B0.2. Appendix Table B0.1 presents the average pre-death earnings and employment rate within each quintile across death type, for both the quintiles calculated separately by cause-of-death and those calculated for suicide and traffic parents together.

[38]To benchmark average pre-death earnings of bereaved parents, we obtain annual labour earnings information for all employed adults aged 30–60. Pooled across 2000 to 2012, the median earnings for this group is $36,000. Comparing these to the average earnings of bereaved parents in each quintile (presented in Appendix Table B0.1) reveals notable differences. Bereaved parents in quintiles 1, 2, and 3 have average earnings below the population median, while those in quintile 4 are approximately at the median. This pattern underscores that child loss disproportionately affects lower earners. In fact, average earnings in quintile 1 fall below the 5th percentile of the total employed population.

Distribution of parental labour earnings responses to child loss

To conclude our analysis of the parental economic cost of child loss, we examine the distribution of individual treatment effects to better understand the average treatment effects for bereaved parents. We explore this heterogeneity by focusing on the effects measured at year 4, which is chosen as a mid-point between the short- and long-run effects discussed earlier. Future steps could investigate whether any heterogeneity in the individual treatment effects also varies over event time.

For each treated parent, we observe their labour earnings one year prior to child loss (year -1) and five years after (year 4). The matching procedure outlined in Section 3.7 identifies 10 comparison parents for each treated parent. We then calculate a weighted average of these 10 neighbours' earnings at year -1 and at year 4. Using these four observations, we estimate the individual difference-in-differences treatment effect and plot the kernel density function of these effects for bereaved mothers (Figure 3.6) and fathers (Figure 3.7).[39] We overlay select percentile points, as well as the average treatment effect.

Figures 3.6 and 3.7 illustrate that many parents experience relatively small treatment effects, with each distribution having approximately 80% of the effects falling within one standard deviation of $0 and 95% within two standard deviations.

Although all four distributions are centred around $0, notable variations exists. For suicide-bereaved mothers (Figure 3.6a), the distribution shifts leftward, with the 10th and 25th percentiles farther from zero than the 90th and 75th, respectively. This right-skew drives the negative average treatment effect, shown by the dashed red vertical line. A

[39] This relates to the point plotted at year 4 on the x-axis in Figures 3.3b and 3.4b, for suicide and traffic-bereavement respectively. We explore the distribution of individual treatment effects within the year 4 average treatment effect for each death type, and for mothers and fathers separately.

similar right-skew appears for traffic-bereaved mothers (Figure 3.6b) and suicide-bereaved fathers (Figure 3.7a). In contrast, traffic-bereaved fathers (Figure 3.7b) exhibit a slightly left-skewed distribution, resulting in a small, positive average treatment effect.

These findings highlight the heterogeneity in bereaved parents' earnings responses: most parents experience small labour earnings losses 5-years after child loss, while a smaller group suffer much more, particularly for suicide-bereaved mothers.

3.8.2 Spillover effects to siblings and classmates

Finally, we examine if there are mental health effects of youth death for the wider networks of siblings and classmates, and whether these effects differ by cause of death. Tables 3.7, 3.8, and 3.9 presents the results from estimating Eq. 3.1 for all siblings, siblings aged within 5 years of the deceased, and siblings aged within 3 years of the deceased, respectively. Results show that when a youth dies, this has adverse effects on the mental health of their siblings, particularly those bereaved by suicide. Youth suicide causes an excess uptake in the use of mental health services and prescriptions for mental health related issues for their siblings, and this impact tends to be greater for siblings closer in age to the deceased. These effects are larger than that for siblings bereaved from traffic accidents, although the difference is only significant for the use of mental health services.

Table 3.10 presents the estimates of Eq. 3.1 for the mental health effects for the classmates of the deceased.[40] Mental health responses are only evident for classmates bereaved by suicide and not traffic deaths. Considering the lower baseline rates, the

[40]We estimate Eq. 3.1 clustering standard errors at the deceased-level. Given the structure of our data, this is effectively equivalent to clustering at the school-cohort level, as we examine the first experience of youth loss within each school-cohort.

relative increases in these mental health indicators for suicide-bereaved classmates are similar in size to that for closer-aged suicide-bereaved siblings.

3.9 Conclusion

In this paper we estimated the health and economic impacts of child loss for bereaved parents in New Zealand. We focus on the two most common causes of youth mortality – suicide and traffic accidents – and explore how parental bereavement responses differ across death type and by parent gender.

We begin by documenting significant detrimental effects on parents' mental health immediately following child loss. Mothers have significantly larger responses than fathers, particularly when bereaved by child suicide.

Using two empirical strategies, we then show significant labour market responses to child loss. Short-term effects estimated from a comparison group of yet-to-be-treated parents reveal significant labour earnings losses that are not compensated by increases in benefit earnings. These earnings losses are evident for both child suicide and traffic fatalities, although the response to child traffic death is mostly driven by mothers.

Using a matched comparison group of non-bereaved parents, we estimate the longer-term economic costs of child loss. Results reveal three notable patterns. First, the effect of child loss on parents' labour earnings differs by the child's cause of death. For parents bereaved by child suicide, losses continue for up to 10 years, with effects ranging 7-8% of counterfactual earnings. In contrast, the labour earnings losses for parents bereaved by a child traffic accident are smaller (1.6%) and less persistent.

These estimates are lower than that in van den Berg et al. (2017), who estimate the six-year impact of child loss from non-intentional accidents to be 12.5% for mothers and 8.8% for fathers. One reason for these differences could be the variation between the New Zealand and Swedish welfare systems, particularly in bereavement and unemployment policies. For example, Sweden has an unemployment insurance program that has an 80% earnings replacement rate (Kolsrud & Spinnewijn, 2024), while the maximum replacement rate for a job-seekers benefit in New Zealand is just over 50% of a full-time minimum wage salary. For bereavement, there is no fixed number of days of bereavement leave in Sweden, although most employers offer paid leave for up to 10 days (Vacation Tracker, 2024). In contrast, New Zealand employers are only obliged to pay three days of bereavement leave (Employment New Zealand, 2024). Thus, the weaker support in New Zealand may compel bereaved parents to return to work earlier than they would like in order to avoid significant income losses.

Second, heterogeneity across the earnings distribution reveals the economic effects of child loss are particularly pronounced among lower-income parents, highlighting the vulnerability of these families.

Third, there are stark differences in bereavement responses across parent gender. Mothers experience significant earnings losses for both suicide- and traffic-bereavement, while fathers only have significant earnings losses when their child dies by suicide. These patterns add new insights to the parental bereavement literature, showing that the economic cost of grief differs by parent gender and within different types of unnatural deaths.

Finally, we document significant mental health effects to the deceased's wider network. Spillover effects to siblings are evident for both suicide and traffic deaths, with some evidence that the response is larger and more significant for suicide-bereavement.

Classmates' mental health responses to youth death are entirely driven by suicide bereavement.

3.10 Tables

Table 3.1: Summary statistics: Bereaved parents versus general population of parents

Characteristic	**(1) Traffic -bereaved parents**	**(2) Age-matched population of parents**	**(3) Suicide -bereaved parents**	**(4) Age-matched population of parents**
Parent age (years)	48.13 (0.54)	48.13 (0.54)	50.31 (0.35)	50.31 (0.35)
Age of youth (years)	20.60 (0.50)	14.92 (0.56)***	21.84 (0.25)	16.32 (0.46)***
Ethnicity: European (%)	48.21 (3.88)	62.50 (3.75)**	49.47 (2.97)	65.26 (2.82)***
Ethnicity: Māori (%)	41.07 (3.82)	16.07 (2.86)***	36.84 (2.88)	18.95 (2.33)***
Ethnicity: Pacific (%)	5.36 (1.66)	7.14 (2.00)	5.26 (1.29)	6.32 (1.45)
No qualifications (%)	17.86 (2.90)	10.71 (2.41)	16.84 (2.23)	13.68 (2.02)
High school qualification (%)	41.07 (3.83)	55.36 (3.84)***	46.32 (2.97)	53.68 (2.96)***
Bachelor's and above qualifications (%)	10.71 (2.41)	26.79 (3.42)***	9.47 (1.74)	25.26 (2.57)***
Missing education information (%)	30.36 (3.57)	5.36 (1.84)***	27.37 (2.63)	7.37 (1.59)***
NZ Deprivation Index (Index 1-10)	6.71 (0.24)	5.14 (0.29)***	6.18 (0.19)	5.07 (0.18)***
NZ Deprivation Index 2018 missing (%)	23.21 (3.22)	3.37 (1.56)***	17.89 (2.28)	5.26 (1.29)***
Labour earnings in 2018 (NZD)	25930 (3054)	430434 (3211)****	25196 (2602)	52587 (4213)***
Benefit earnings in 2018 (NZD)	3862 (537)	1417 (336)	3850 (399)	912 (209)***
Total counts	168	168	285	285

Notes: The table shows 2018 descriptive statistics for four samples of parents: (1) those bereaved by child loss from traffic accidents in 2019, (2) the general population of parents matched by age to traffic-bereaved parents, (3) those bereaved by child suicide in 2019, and (4) the general population of parents matched by age to suicide-bereaved parents. Other ethnicity categories (e.g, Asian, and Middle-Eastern, Latin-American, and African (MELAA)) are excluded due to low sample counts in the bereaved parent samples. The NZ Deprivation Index is an area-based measure of deprivation, with 1 representing the least deprived areas and 10 representing the most deprived. Education information and the NZ Dep Index is sourced from Census 2018. Asterisk in column 2 indicates significant differences between column 2 and column 1. Asterisk in column 4 indicates significant differences between column 4 and column 3. All counts have been randomly rounded to base 3. Asterisk denote statistical significance as follows: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.2: The effects of child loss on parents' mental health: By death type

Outcome	Death type	(1) Counts	(2) Baseline	(3) Effect	(4) Difference
Mental health services	Traffic	1479	0.4	0.54 (0.13) ***	
	Suicide	2295	0.4	0.87 (0.13) ***	0.33 (0.18) *
Prescriptions	Traffic	3204	6.34	2.67 (0.34) ***	
	Suicide	3795	6.34	3.25 (0.33) ***	0.58 (0.47)

Notes: This table provides the average treatment effects for the impact of child loss on parents' mental health outcomes. We estimate Eq. 3.1 where Z_i is an indicator for suicide relative to traffic accidents. The baseline (the average over 12 pre-death months) is a weighted average of the baselines for the suicide and traffic samples separately. Standard errors are shown in parentheses. Counts differ for each mental health outcome due to different data availability for the mental health and pharmaceutical registers. All counts have been randomly rounded to base 3. Asterisk denote statistical significance as follows: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.3: The effects of child loss on parents' mental health: By parent gender

Death type	Outcome	Parent	(1) Counts	(2) Baseline	(3) Effect	(4) Difference
Traffic	Mental health services	Father	663	0.35	0.54 (0.18) ***	
		Mother	813	0.35	0.53 (0.17) ***	-0.01 (0.25)
	Prescriptions	Father	1467	5.76	1.61 (0.42) ***	
		Mother	1734	5.76	3.47 (0.50) ***	1.86 (0.63) ***
Suicide	Mental health services	Father	1026	0.43	0.24 (0.18)	
		Mother	1269	0.43	1.39 (0.19) ***	1.15 (0.26) ***
	Prescriptions	Father	1701	6.82	1.85 (0.41) ***	
		Mother	2097	6.82	4.46 (0.48) ***	2.60 (0.62) ***

Notes: This table provides the average treatment effects for the impact of child loss on parents' mental health outcomes, within death type and across parent gender. We estimate Eq. 3.1 where Z_i is an indicator for mothers relative to fathers, conditional on cause of death. The baseline (the average over 12 pre-death months) is the weighted average of the baseline for mothers and fathers separately. Standard errors are shown in parentheses. Counts differ for each mental health outcome due to different data availability for the mental health and pharmaceutical registers. All counts have been randomly rounded to base 3, so the sum of mother and father counts may not add up to the overall parent count. Asterisk denote statistical significance as follows: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.4: The effects of child loss on parents' labour market outcomes: By death type

Outcome	Death type	(1) Counts	(2) Baseline	(3) Effect %CF	(4) Effect	(5) Difference
Employment	Traffic	4260	47.37	-5.61	-2.75 (0.53) ***	
	Suicide	4584	47.37	-3.65	-1.75 (0.47) ***	1.00 (0.70)
Labour earnings	Traffic	4260	1820.26	-6.28	-120.51 (22.86) ***	
	Suicide	4584	1820.26	-6.27	-120.26 (27.39) ***	0.24 (35.13)
Benefit income	Traffic	4260	222.25	6.14	13.11 (4.96) ***	
	Suicide	4584	222.25	5.67	12.16 (4.82) **	-0.95 (6.89)
Total income	Traffic	4260	2143.72	-4.38	-97.45 (24.05) ***	
	Suicide	4584	2143.72	-5.57	-125.44 (28.34) ***	-27.99 (36.84)

Notes: This table provides the average treatment effects for the impact of child loss on bereaved parents' labour market outcomes. The baseline (average over 12 pre-death months) is a weighted average of the baselines for the suicide and traffic samples separately. The effects in column 4 are estimated by Eq. 3.1 where Z_i is an indicator for suicide relative to traffic accidents. Column 3 reports these effects as a percentage of counterfactual earnings using Eq. 3.2. Standard errors are shown in parentheses. All counts have been randomly rounded to base 3. Asterisk denote statistical significance as follows: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.5: The effects of child loss on parents' labour market outcomes: By parent gender

Death type	Outcome	Parent	(1) Counts	(2) Baseline	(3) Effect %CF	(4) Effect	(5) Difference
Traffic	Employment	Father	1962	48.73	-2.76	-1.35 (0.74) *	
		Mother	2298	48.73	-7.26	-3.72 (0.72) ***	-2.37 (1.00) **
	Labour earnings	Father	1962	1793.69	-3.5	-64.45 (38.21) *	
		Mother	2298	1793.69	-8.25	-159.86 (25.09) ***	-95.41 (43.99) **
	Benefit income	Father	1962	202.61	1.65	3.35 (6.45)	
		Mother	2298	202.61	10.83	20.19 (7.27) ***	16.84 (9.58) *
	Total income	Father	1962	2092.14	-2.82	-60.62 (41.02)	
		Mother	2298	2092.14	-5.51	-121.73 (26.09) ***	-61.11 (47.51)
Suicide	Employment	Father	2064	46.1	-2.63	-1.22 (0.66) *	
		Mother	2520	46.1	-5.23	-2.49 (0.65) ***	-1.27 (0.91)
	Labour earnings	Father	2064	1844.95	-8.41	-166.62 (54.05) ***	
		Mother	2520	1844.95	-4.96	-94.67 (22.10) ***	71.95 (56.99)
	Benefit income	Father	2064	240.51	3.48	8.24 (5.56)	
		Mother	2520	240.51	7.17	16.40 (7.33) **	8.16 (8.95)
	Total income	Father	2064	2191.65	-7.17	-167.09 (54.49) ***	
		Mother	2520	2191.65	-4.5	-102.01 (25.24) ***	65.08 (58.74)

Notes: This table provides the average treatment effects for the impact of child loss on bereaved mothers' and fathers' labour market outcomes. The baseline (average over 12 pre-death months) is the weighted average of the baseline for mothers and fathers separately. The effects in column 4 are estimated by Eq. 3.1 where Z_i is an indicator for mothers relative to fathers, conditional on cause of death. Column 3 reports these effects as a percentage of counterfactual earnings using Eq. 3.2. Standard errors are shown in parentheses. All counts have been randomly rounded to base 3, so the sum of mother and father counts may not add up to the overall parent count. Asterisk denote statistical significance as follows: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.6: Gender differences in long-term labour earnings responses to child loss by death type

Parent group	Death type	(1) Bereaved parents	(2) Baseline	Years 0-4 (3) Effect NZD	(4) Effect % CF	(5) Difference	Years 5-10 (6) Effect NZD	(8) Effect % CF	(8) Difference
Parents	Traffic	2409	19715	-371 (321)	-1.76		-343 (569)	-1.49	
	Suicide	2457	19715	-1457 (371) ***	-6.58	-1086 (487) **	-1899(596) ***	-7.7	-1556 (810) *
Mothers	Traffic	1293	14765	-1397 (320) ***	-8.33		-1821 (539) ***	-9.37	
	Suicide	1344	14765	-1650 (349) ***	-9.7	-253 (469)	-2040 (594) ***	-10.39	-220 (779)
Fathers	Traffic	1116	25572	813 (554)	3.11		1366 (974)	4.97	
	Suicide	1110	25572	-1211 (688) *	-4.3	-2024 (876) **	-1723 (1057)	-5.63	-3088 (1416) **

Notes: This table presents estimates from Eq. 3.2 where the outcome variable is parents' labour earnings, and the post-death annual event indicators are pooled for years 0-4 and years 5-10. We run Eq. 3.2 first for all parents, and then separately for mothers and fathers. We report the point estimates in NZD, with standard errors in parentheses. The point estimates are relative to the baseline annual earnings in year -1, which is a weighted average of the separate baseline earnings for suicide and traffic bereaved parents. The counts of bereaved parents in the first column have been randomly rounded to base 3, so the sum of mother and father counts may not add up to the overall parent count. Asterisk denote statistical significance as follows: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 3.7: Mental health impacts on the deceased's siblings (all ages)

Outcome	Death type	(1) Deceased	(2) Siblings	(3) Baseline	(4) Effect	(5) Difference
Mental health services	Traffic	762	1992	0.81	0.10 (0.17)	
	Suicide	1239	3411	0.81	0.52 (0.16) ***	0.43 (0.23) *
Prescriptions	Traffic	1566	3924	2.46	0.82 (0.21) ***	
	Suicide	1992	5301	2.46	0.88 (0.21) ***	0.07 (0.30)

Notes: This table provides the average treatment effects for the impact of youth death on bereaved siblings' mental health outcomes, looking at all siblings of the deceased youth. We estimate effects across death type following Eq. 3.1 where Z_i is an indicator for suicide relative to traffic accidents. The baseline (the average over 12 pre-death months) is a weighted average of the baselines for the suicide and traffic samples separately. Standard errors are shown in parentheses. Counts differ for each mental health outcome due to different data timeframes in the mental health and pharmaceutical registers. All counts have been randomly rounded to base 3. Asterisk denote statistical significance as follows: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 3.8: Mental health impacts on the deceased's siblings (aged +/- 5 years)

Outcome	Death type	(1) Deceased	(2) Siblings	(3) Baseline	(4) Effect	(5) Difference
Mental health services	Traffic	657	1194	0.9	0.01 (0.22)	
	Suicide	1074	1953	0.9	0.70 (0.21) ***	0.69 (0.30) **
Prescriptions	Traffic	1368	2364	2.71	1.00 (0.26) ***	
	Suicide	1752	3117	2.71	1.06 (0.31) ***	0.06 (0.40)

Notes: This table provides the average treatment effects for the impact of youth death on bereaved siblings' mental health outcomes, looking at siblings aged within 5 years of the deceased youth. We estimate effects across death type following Eq. 3.1 where Z_i is an indicator for suicide relative to traffic accidents. The baseline (the average over 12 pre-death months) is a weighted average of the baselines for the suicide and traffic samples separately. Standard errors are shown in parentheses. Counts differ for each mental health outcome due to different data timeframes in the mental health and pharmaceutical registers. All counts have been randomly rounded to base 3. Asterisk denote statistical significance as follows: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 3.9: Mental health impacts on the deceased's siblings (aged +/- 3 years)

Outcome	Death type	(1) Deceased	(2) Siblings	(3) Baseline	(4) Effect	(5) Difference
Mental health services	Traffic	540	792	0.96	0.01 (0.31)	
	Suicide	906	1281	0.96	0.96 (0.27) ***	0.95 (0.41)**
Prescriptions	Traffic	1128	1575	2.92	0.71 (0.32) **	
	Suicide	1464	2055	2.92	1.07 (0.38) ***	0.36 (0.50)

Notes: This table provides the average treatment effects for the impact of youth death on bereaved siblings' mental health outcomes, looking at siblings aged within 3 years of the deceased youth. We estimate effects across death type following Eq. 3.1 where Z_i is an indicator for suicide relative to traffic accidents. The baseline (the average over 12 pre-death months) is a weighted average of the baselines for the suicide and traffic samples separately. Standard errors are shown in parentheses. Counts differ for each mental health outcome due to different data timeframes in the mental health and pharmaceutical registers. All counts have been randomly rounded to base 3. Asterisk denote statistical significance as follows: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

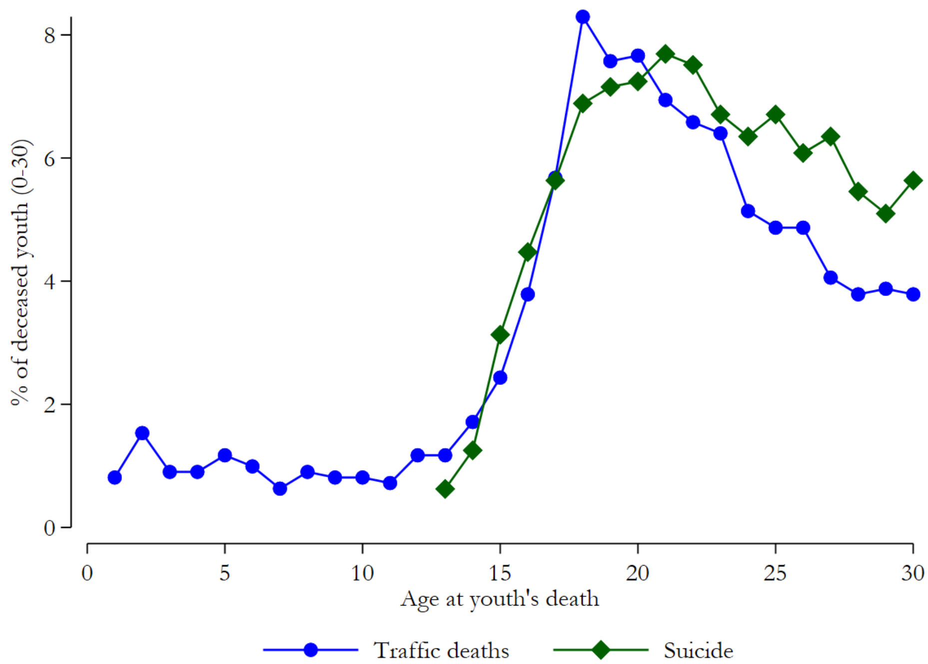
Table 3.10: Mental health impacts on the deceased's classmates

Outcome	Death type	(1) Deceased	(2) Classmates	(3) Baseline	(4) Effect	(5) Difference
Mental health services	Traffic	120	9936	0.3	0.04 (0.05)	
	Suicide	147	16752	0.3	0.25 (0.06) ***	0.22 (0.08) ***
Prescriptions	Traffic	207	18984	0.45	0.07 (0.07)	
	Suicide	201	24072	0.45	0.19 (0.08) **	0.12 (0.10)

Notes: This table provides the average treatment effects for the impact of youth death on bereaved classmates' mental health outcomes. We estimate effects across death type following Eq. 3.1 where Z_i is an indicator for suicide relative to traffic accidents. The baseline (the average over 12 pre-death months) is a weighted average of the baselines for the suicide and traffic samples separately. Standard errors are shown in parentheses. Counts differ for each mental health outcome due to different data timeframes in the mental health and pharmaceutical registers. All counts have been randomly rounded to base 3. Asterisk denote statistical significance as follows: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

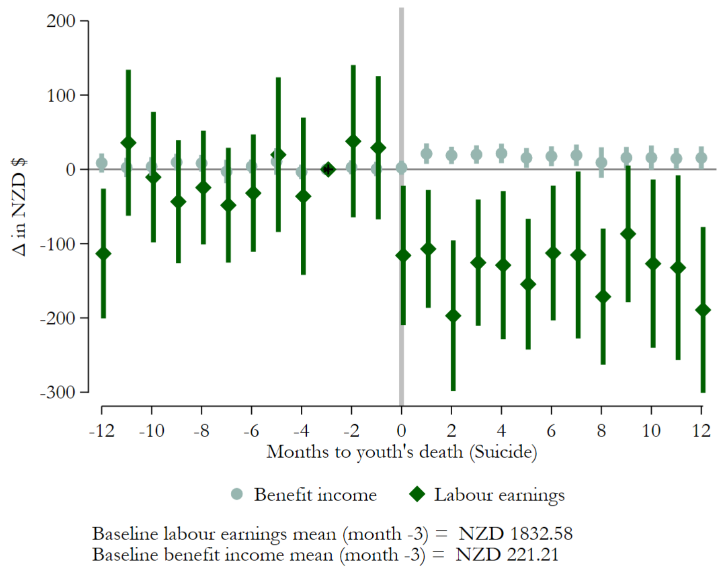
3.11 Figures

Figure 3.1: Age-death curve: Youth suicide and traffic fatalities (2001-2019)

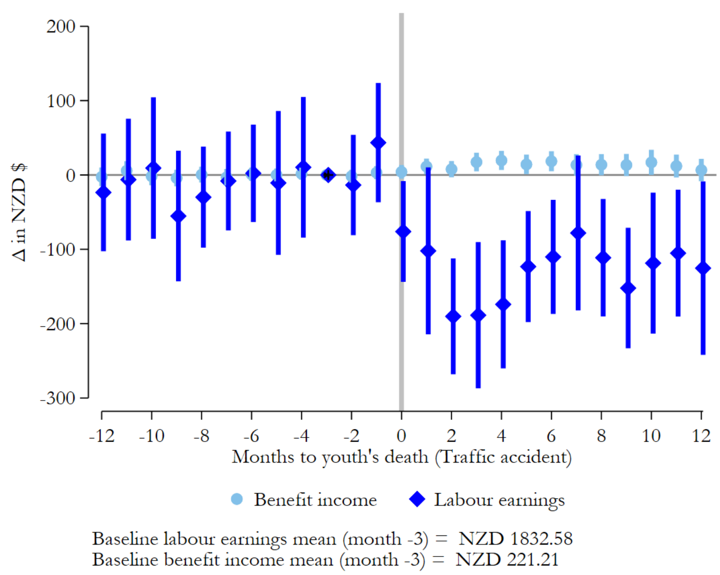


Notes: This figure plots the age-death profile for the sample of youth (ages 0-30) who died from suicide (N=3,363) or traffic accidents (N=3,318) over 2001-2019.

Figure 3.2: The effect of child loss on parents' labour market outcomes



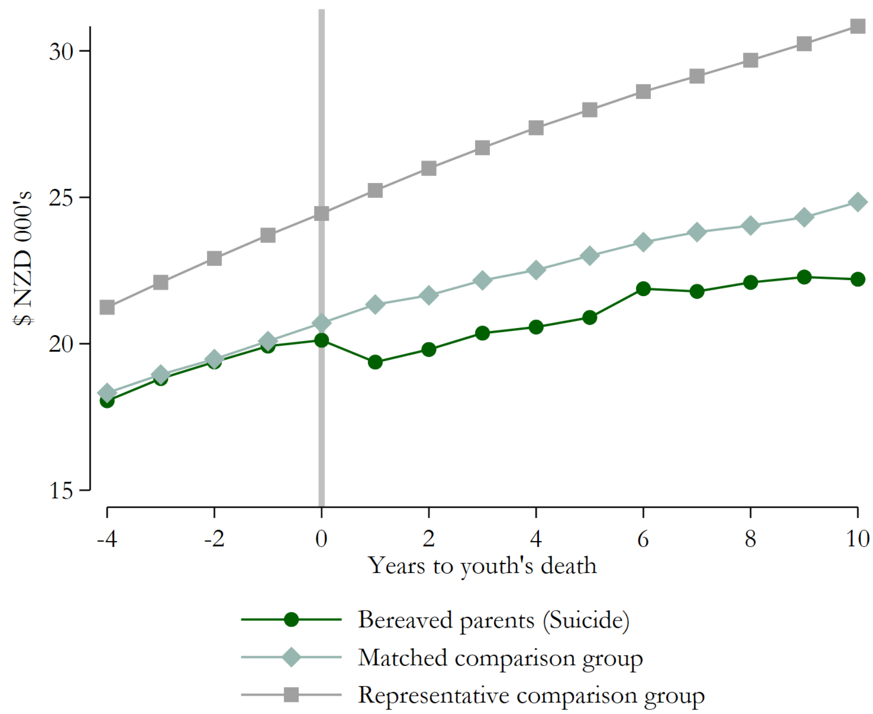
(a) Child loss by suicide



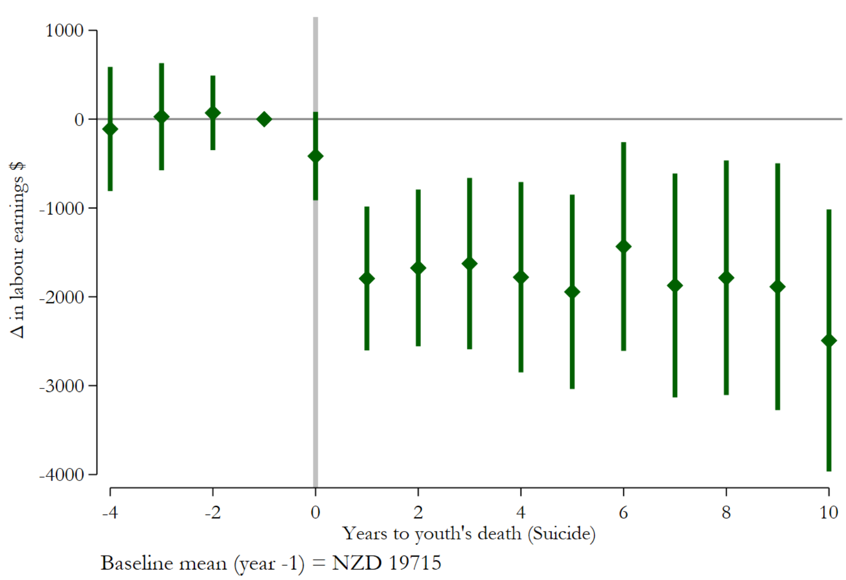
(b) Child loss by traffic accident

Notes: This figure shows the short-term dynamic effects of child loss on parents' labour earnings and benefit income. We estimate Eq. 3.3 where Z_i is the cause-of-death indicator, clustering standard errors at the child-level. We plot the monthly average treatment effects for suicide-bereaved parents (Panel A) and for traffic-bereaved parents (Panel B) from this interacted specification. We normalise the comparison group's outcome to the outcome level of the treatment group three months prior to the child's death. We plot the point estimates together with the 95% confidence intervals.

Figure 3.3: Long-term labour earnings trajectories for suicide-bereaved parents



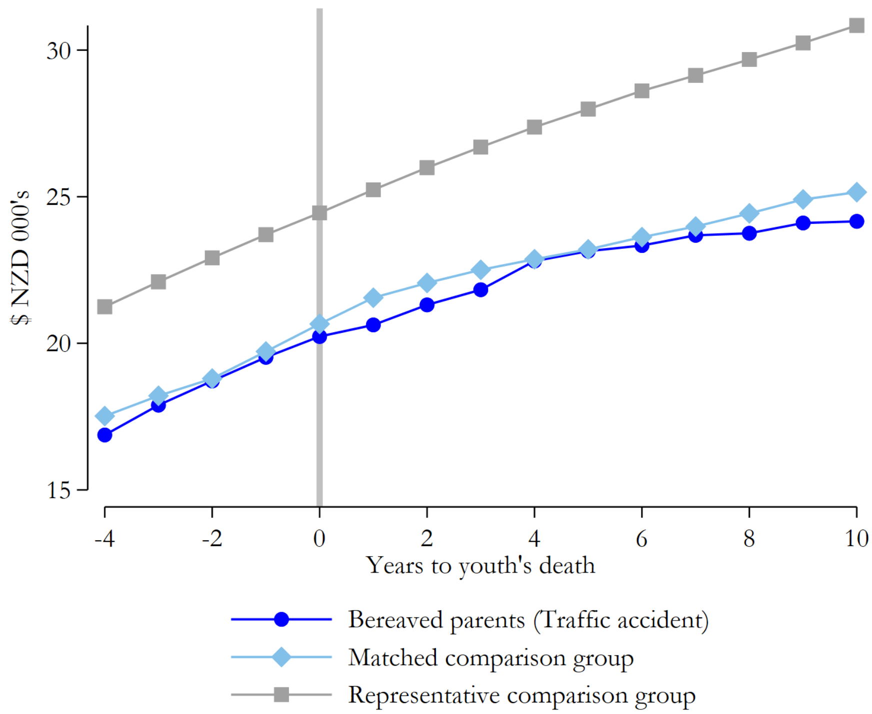
(a) Raw plots



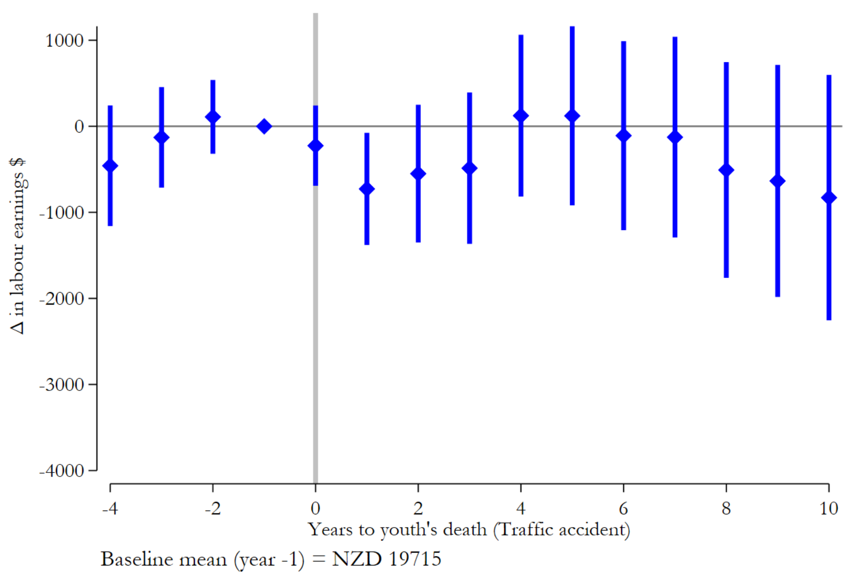
(b) Regression estimates

Notes: This figure illustrates the long-term effects of child suicide on parents' labour earnings. Panel A plots the treatment group's raw labour earnings over the event timeline, as well as the labour earnings trajectories for a representative comparison group and a matched comparison group. Panel B plots the point estimates and 95% confidence intervals from the difference-in-differences model in Eq. 3.4, comparing the treatment group to the matched comparison group, benchmarking differences at year -1.

Figure 3.4: Long-term labour earnings trajectories for traffic-bereaved parents



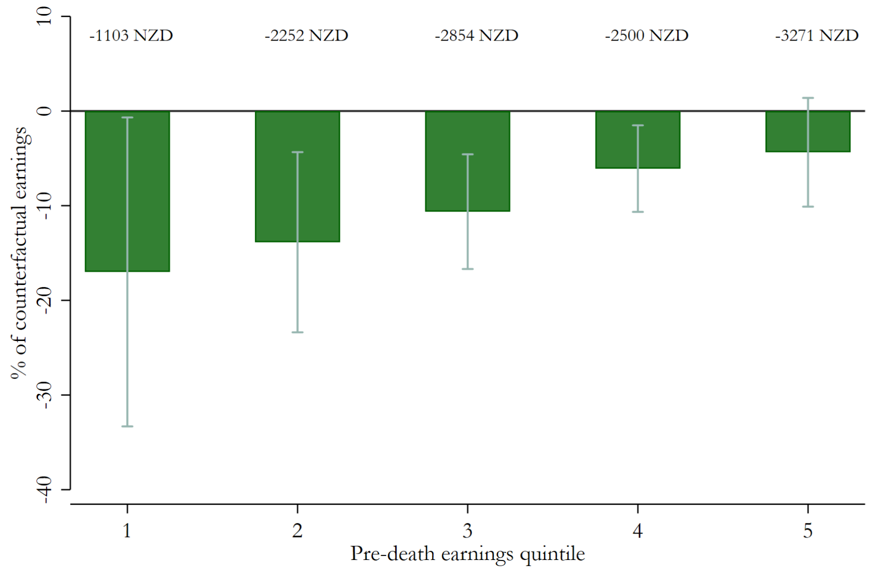
(a) Raw plots



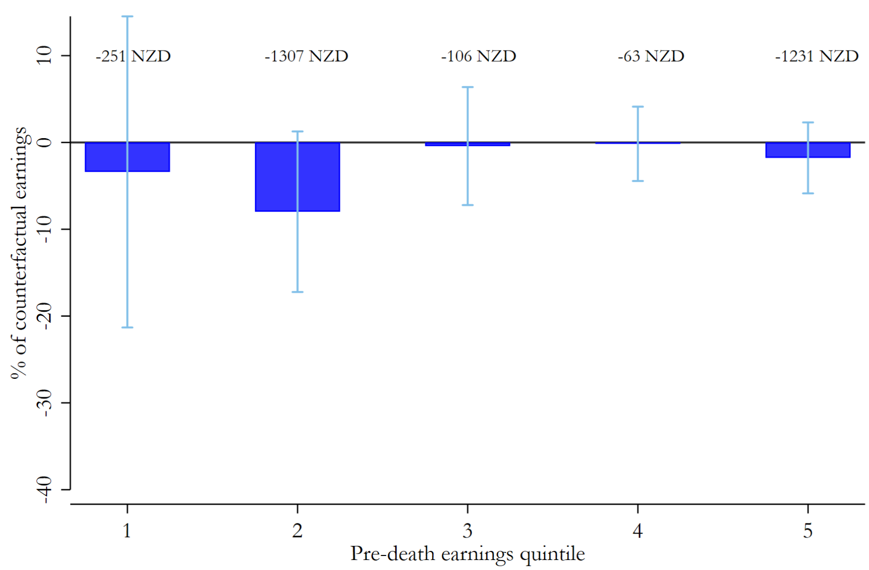
(b) Regression estimates

Notes: This figure illustrates the long-term effects of child traffic fatalities on parents' labour earnings. Panel A plots the treatment group's raw labour earnings over the event timeline, as well as the labour earnings trajectories for a representative comparison group and a matched comparison group. Panel B plots the point estimates and 95% confidence intervals from the difference-in-differences model in Eq. 3.4, comparing the treatment group to the matched comparison group, benchmarking the differences at year -1.

Figure 3.5: Labour earnings responses to child loss: Distribution of effects



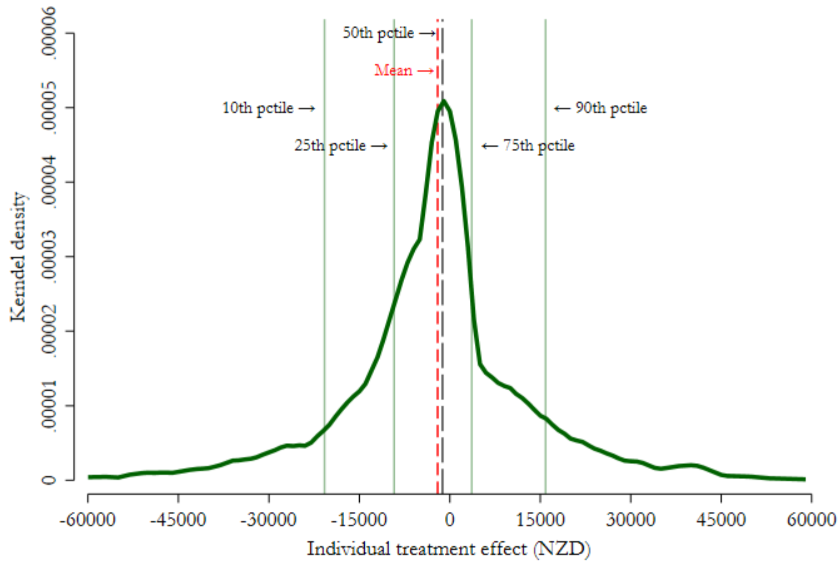
(a) Child loss by suicide



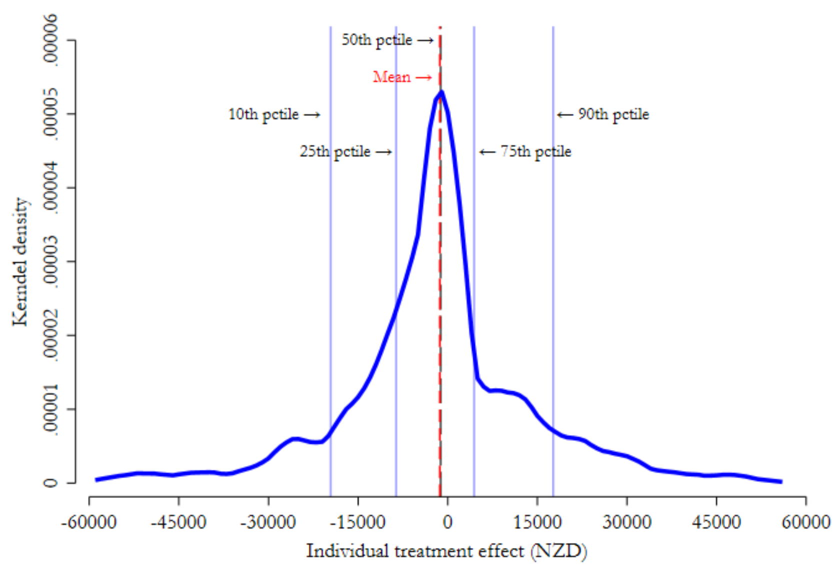
(b) Child loss by traffic accident

Notes: These figures explore heterogeneity in parental responses to child loss across parents' earnings distribution. To split parents into earnings quintiles, we average pre-death earnings in the four years before child loss and divide those with positive earnings into five quintiles where Q1 is the lowest earners and Q5 is the highest. This is done separately for suicide- and traffic-bereaved parents. We estimate Eq. 3.4 separately for each quintile, using labour earnings as the outcome variables and then translate each annual effect into a percentage of the counterfactual outcome. We plot the average annual earnings responses measured within years 0-4 after the death. Bars represent 95% confidence intervals.

Figure 3.6: Distribution of bereaved mothers' labour earnings losses



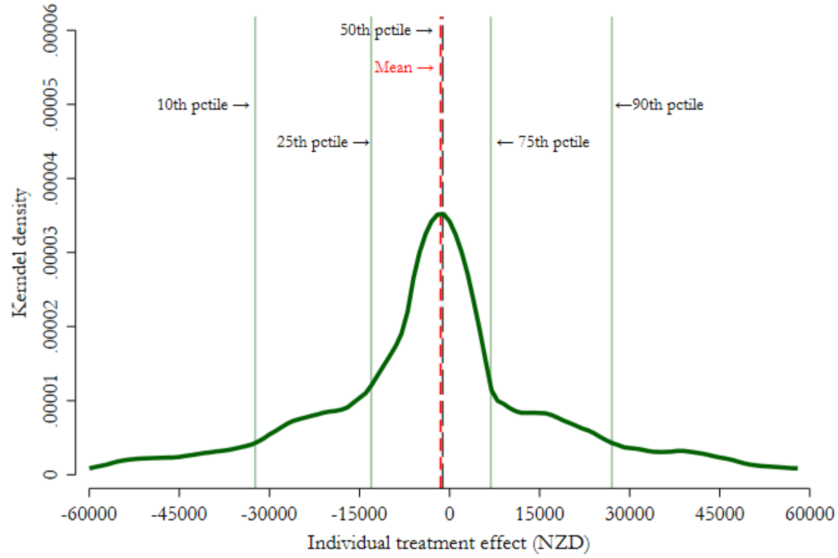
(a) Child loss by suicide



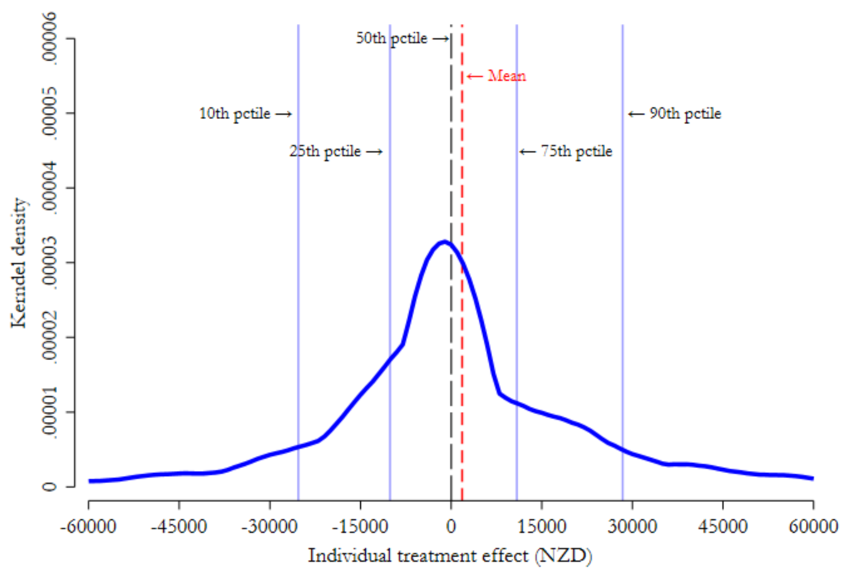
(b) Child loss by traffic accident

Notes: These figures plot the kernel density function of individual treatment effects for the impact of child loss on mothers' labour earnings. To calculate the individual-level difference-in-differences estimate, each treated mother is matched to 10 non-bereaved mothers, and labour earnings are compared five years after child loss (year 4) to that one year prior to child loss (year -1). The kernel density plots have been trimmed at each end due to confidentiality reasons. We overlay select percentiles and the average treatment effect for each distribution.

Figure 3.7: Distribution of bereaved fathers' labour earnings losses



(a) Child loss by suicide



(b) Child loss by traffic accident

Notes: These figures plot the kernel density function of individual treatment effects for the impact of child loss on fathers' labour earnings. To calculate the individual-level difference-in-differences estimate, each treated father is matched to 10 non-bereaved fathers, and labour earnings are compared five years after child loss (year 4) to that one year prior to child loss (year -1). The kernel density plots have been trimmed at each end due to confidentiality reasons. We overlay select percentiles and the average treatment effect for each distribution.

Chapter 4

The effect of violent assaults on youth victims and their parents

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4.1 Prelude

The second half of this thesis turns to non-fatal shocks experienced by youth, namely victimisation. In this chapter, I estimate the effect of non-family violence assaults on youth victims and their parents. I explore the health, schooling, and labour market responses to assault victimisation for the youth victim, and the mental health and labour market responses for the parents of the youth victim. I have presented a full seminar of this paper at the NZ Ministry of Justice (2023), Motu Economic and Public Policy Research (2023), the ROCKWOOL Foundation in Copenhagen (2023), and the Aarhus University Labour and Public Policy Seminar Series (2023). Feedback from these seminars have led to the inclusion of an abundance of robustness and heterogeneity tables in this paper. I also presented this paper the New Zealand Association of Economists Conference (2024), where it won the Seamus Hogan Research Prize for the best public policy paper written by a student; the Jan Whitwell prize for the best presented PhD paper; and the Stats NZ prize for the paper deemed to have the best use of official statistics.

4.2 Abstract

We study the impacts of assault victimisation for youth victims and their parents using New Zealand's unique administrative data. We construct counterfactual outcomes using youth and parents who are affected by assault victimisation in the future. Assault victimisation has negative effects on youth's physical and mental health, and their schooling and labour market outcomes. This adversity faced by youth victims has spillover effects to parents' well-being. Parents respond to youth victimisation by decreasing their labour

market attachment and increasing their usage of mental health support services and anti-anxiety medication. These results emphasise that the costs of victimisation goes well beyond the first-order effects on victims.

4.3 Introduction

Victimisation, particularly physical assault, is one of the leading factors affecting individuals' sense of personal safety, security, well-being, and quality of life (OECD, 2009). Existing studies on the consequence of falling victim to violence systematically find negative effects on victims' labour market outcomes, health stock, and human capital accumulation (Bindler & Ketel, 2022; Cornaglia et al., 2014; Johnston et al., 2018). Despite crime and victimisation being a phenomenon disproportionally experienced by youth (Davies et al., 2017), little is known about the consequences of victimisation for youth victims. Given that a traumatic experience in one's formative years can cause long-term negative health, well-being, and economic outcomes (see, for example, Webbink et al. (2012), Conti et al. (2021), and Henkhaus (2022)), it is important to understand how victimisation impacts youth and if the effects are persistent.

Further, it is well-established that the costs of crime extend beyond the impact on victims. Existing studies, for example, have shown evidence of negative spillover effects of crime to neighbourhoods, and society more generally, through heightened fear and perceived risk of victimisation (see, for example, Dustmann and Fasani (2016)). We add to these studies by focusing on the spillover effects of crime to one specific group, namely parents of youth victims. Parents are the primary group supporting and invested in youth. Since family members' health plays an important role in determining one's labour supply

(Fadlon & Nielsen, 2019), it is reasonable to expect that parents' labour market outcomes would be impacted by their child's assault, either directly through a psychological impact or indirectly through providing additional care or support to the child.

Our paper contributes to existing literature by answering two questions. First, what is the causal effect of youth assault victimisation on victims' physical and mental health, their labour market outcomes, and their in-school behaviour? Second, what is the spillover effect of youth assault victimisation to their parents' mental health and labour market outcomes?

We investigate these questions using unique administrative data from New Zealand, a country with one of the highest victimisation rates among OECD countries (OECD, 2009). We identify youth victims (ages 0-30) of non-family violence assaults (hereafter simply referred to as 'assaults') using police reports from 2014 to 2022 and link victims to their parents through birth registers. The richness of the data allows us to link victims and their parents to a range of other registers to study physical health, mental health, labour market, and schooling outcomes immediately before and after the assault. To address the sorting into victimisation, we follow Bhuller et al. (2022) and Dustmann and Mertz (n.d.) by using full-population administrative data on reported victimisations and exploit variation in the timing of the assault to identify causal effects. Specifically, we use a dynamic difference-in-differences specification that compares the outcome trajectories of youth victims assaulted in the present to that of youth who experience assault victimisations in the future. The key identifying assumption is that these groups would have followed the same outcome trajectory in the absence of victimisation.

We first estimate the effects of youth assault victimisation on victims' outcomes. Assault victimisation leads to an immediate increase of 5.21 percentage points (or 222%) in the probability of going to hospital and a 4.43 percentage point (or 106%) increase in the probability of filling out pain-relief prescriptions in the month of victimisation. Victimisation also leads to poorer mental health, causing increased use of mental health services (up to 100% of the baseline mean, depending on the mental health service), and prescriptions for mental health-related illnesses (up to 38% of the baseline mean, depending on the medication) in the month of victimisation. These results complement existing studies on the impact of victimisation adult victims' physical and mental health (see e.g. Cornaglia et al. (2014), Bhuller et al. (2022), and Dustmann and Mertz (n.d.)).

Furthermore, we document substantial earnings and income losses from victimisation for working-aged youth (ages 18-30). For these victims, monthly earnings drop by $71.52 NZD (or 5%) on average in the 12 months after assault victimisation. These effects are larger for victims whose offender is at their same workplace and continue up to four years with no sign of recovery. This persistence is similar to the long-run effects for adult victims in Bindler and Ketel (2022), Bhuller et al. (2022), and Dustmann and Mertz (n.d.).

In addition to publicly-funded unemployment insurance programmes, New Zealand offers an Accident Compensation Corporation (ACC) scheme, which aims to compensate up to 80% of lost labour earnings for those unable to work due to injury. This means victims who were employed at the time of their assault and who suffered physical injuries that disabled them from working are eligible for ACC compensation. However, we show that only a fraction of youth victims were actually eligible for ACC, as only 5 percent were hospitalised with physical injuries due to being assaulted and the pre-victimisation employment rate was only 48%. Thus, ACC plays a small role in the overall compensation for victims of crime. Even when combining ACC earnings with other publicly-funded benefits, we show the average monthly compensatory earnings for youth victims of assault is only $17.28 NZD (or 4.2%), or about one-quarter of the monthly labour earnings losses.

For schooling-aged (ages 5-18) victims, we show that victimisation increases their probability of starting a schooling intervention for learning difficulties, behavioural issues, or truancy issues by 1.17 percentage points (41%) in the month of the assault, and that these estimates are larger for victims who attend the same school as their offender.

Probing heterogeneity in youth's responses to assault victimisation, we show that youth are more likely to be physically hurt and suffer larger earnings losses when their offender is a friend or stranger, while the mental health response of victims is more severe when the offender is a romantic relation.

We next estimate the spillover effects of youth victimisation to parents' mental health and labour market outcomes. We show that youth victimisation causes an immediate uptake of mental health services and anti-anxiety medication for parents, and that parents suffer average monthly earnings losses of $14.20 NZD (or 0.63%) in the 12 months following the victimisation of their child. Notwithstanding a small take-up of compensatory benefits, parents' earnings losses are carried over to total income losses. Finally, we show that earnings losses are greater for parents of youth who were more severely hurt (either physically or mentally), suggesting that one potential mechanism for this parental response is increased caregiving demands from their victimised child.

This documentation of the parental effects of youth victimisation adds to the literature studying the societal cost of victimisation, highlighting that parents are a specific and important network to consider when estimating the indirect impacts of crime. In fact, back-of-the-envelope calculations reveal that parents make up one-third of the total annual labour earnings losses resulting from youth victimisation. Thus, the societal cost of youth victimisation will be heavily underestimated if one does not take into account the spillover effects to parents.

The remainder of this paper is set out as follows: Section 4.4 summarises the small empirical literature on the consequences of victimisation; Section 4.5 provides a comparison on New Zealand's victimisation rates internationally and over time, followed by a discussion on the victimisation policy setting in New Zealand; Section 4.7 describes the New Zealand register data and defines key variables; Section 4.8 sets out the difference-in-differences estimation strategy and the interpretation of our treatment effect; Section 4.9 contains the results for the effects of youth victimisation on youth victims and their parents; and Section 4.10 concludes.

4.4 Literature

There is no single way to estimate the total cost of crime across all dimensions. The types of costs included in an estimate of the cost of crime depends on the available data and the empirical methodology (Soares, 2015). Accordingly, the existing literature studying the consequences of victimisation for victims, and spillover effects to society, can be divided into two strands. The first strand consists of studies estimating the cost of victimisation using survey data (mostly from Australia) and selected register datasets, either at the area-level (such as crime rates or house prices) or the individual-level (such as school roles, court judgments, or sex offender registers). The second strand is newer, smaller, and consists of studies using population-wide register data, predominately from

Scandinavian countries, to estimate the consequences of falling victim to crime. Due to the size of the datasets used in these studies, they can study more specific offence types, and a wider range of outcomes. These two strands of literature are summarised below.

4.4.1 Cost of victimisation: Survey and non-population-wide register data

The cost of victimisation can be evaluated directly for victims, as well as spillover effects to society. One approach for quantifying direct victimisation costs is to assign a monetary value to the health and well-being losses suffered by victims. For example, M. A. Cohen (1988) uses jury-awarded compensation for equivalent personal injury accidents to monetarise victimisation costs from the pain, suffering, and fear endured by victims. Other approaches involve panel survey data with information on an individual's victimisation status and subjective well-being. Velamuri and Stillman (2008), Cornaglia et al. (2014), and Johnston et al. (2018), each use the HILDA (Household, Income and Labour Dynamics in Australia) survey and unanimously find significant negative effects of victimisation on victims' well-being, mental health, and life satisfaction, particularly for violent crime compared to property crime. Velamuri and Stillman (2008) also document significant labour earnings losses due to violent victimisation for male victims.

In addition to the costs borne by victims, members of society are also negatively impacted by crime through having an increased fear and perceived risk of being victimised. Existing literature has quantified these spillover effects of crime for residents and communities, with different empirical methodologies (depending on the data available) estimating different types of societal costs.

One way to quantify the societal effects of crime is to estimate the community's willingness to pay to avoid crime as evidenced by stated or revealed preferences. Papers studying stated preferences rely on survey data and contingent valuation methods. For example, M. A. Cohen et al. (2004) surveyed USA residents asking how much they are willing to pay to fund crime-prevention programs in their neighbourhood and find the typical household would pay between \$100 and \$150 USD per year to reduce crime rates by 10%. Cornaglia et al. (2014) use the HILDA survey to show that each person in a community of 100,000 people is willing to pay \$0.77 AUD to reduce the crime rate by one victim.

Papers exploring revealed preferences of crime avoidance use housing market data and hedonic pricing models to show that people in fact pay more to live in areas that have lower perceived risks of crime. These papers focus on the spillovers from crimes that directly relate to geographical units, such as property crimes and locations of sex offenders. For example, Gibbons (2004) link area-level crime data to property price data to show that a one-tenth standard deviation decrease in the neighbourhood density of incidents of criminal damage increases property values by 1%. Linden and Rockoff (2008) combine housing market data with a sex offender registry and find house prices are 4% lower when within 0.1 miles of a sex offender.

Together, these papers inform about how society feels about the cost of crime in general, taking into account all costs believed to be important from their subjective perspective. Other papers take a deeper dive into the societal impacts of crime by estimating a specific type of cost, such as mental health costs. This can be achieved by linking panel survey data containing mental health and well-being information with local-area crime statistics. For example, Cornaglia et al. (2014) link HILDA survey respondents to Australian local-area crime data and find that increases in the rate of violent crime is associated with negative mental well-being effects for non-victims, particularly through changing behaviour around engaging in social activities. Dustmann and Fasani (2016) link the British Household Panel Survey with local-authority-level crime data in England and Wales to show that a one standard-deviation increase in the local crime rate causes an increase in the self-reported mental distress of residents in the order of 8%-15%. [1]

4.4.2 Cost of victimisation: Population-wide register data

The literature estimating costs of victimisation has recently been expanded due to the growing availability of population-wide register data. There are at least three advantages of using such data in this context. First, register data generally covers the full population, so samples sizes are much larger. This enables researchers to explore heterogeneous impacts of victimisation which otherwise may not have been possible due to low statistical power. Second, register data are high-frequency, so researchers can observe outcomes more closely around a victimisation event. Third, multiple registers can be linked together and tracked over time, so researchers can explore a wider range of tangible victimisation costs and estimate such costs over short- and long-run horizons.

However, it is worth observing that while victimisation status is formally identified in

[1] These papers evaluate the society spillover effects of victimisation for the types of crime that are most prevalent in society, namely violent and property crime. Another common form of victimisation is bullying, of which a small literature shows being bullied in adolescence causes long-lasting and large negative impacts on physical and mental health (Eriksen et al. (2012), Sarzosa and Urzua (2021)), and later schooling outcomes (e.g. Eriksen et al. (2014)). There exists other papers that estimate the spillover effects of crime to society for more extreme crime events, such as school shootings (e.g. Cabral et al. (2020)) or mass shootings (e.g., Bharadwaj et al. (2021)). These crimes involve the most serious forms of violence and are more likely to be one-off events, so the resulting spillover effects to society are likely different to that from more common types of crime that present a more imminent threat to residents.

register data, it relies on victims reporting their assault to the police or seeking medical attention. Comparing reported victimisation rates in police register data versus survey data reveals that reporting rates are higher in the latter and characteristics between the two differ (European Union Agency for Fundamental Rights, 2021; U.S. Department of Justice, 2023).[2] Consequently, studies that use police or hospitalisation registers to evaluate the effects of victimisation may not speak to the average effect of victimisation for all self-identified victims, which might otherwise be better captured from survey data.

Similarly, while police register data provide detailed information on the exact victimisation event and the offences suffered, it provides limited information on the different types of costs caused by victimisation. Tangible costs, such as lost labour earnings, may be estimated by linking police registers to employment records, but this does not capture the intangible costs of crime, such as the cost of feeling insecure or unsafe. Thus, results relying on register data will likely underestimate the complete costs of victimisation, which would otherwise be better captured by contingent valuation surveys.

The first of these papers using population-wide register data is Ornstein (2017), who estimates the effects of interpersonal violence on health and labour market outcomes for victims hospitalised in Sweden. Propensity score methods are used to match victims aged 20-54 with comparable non-victims. Results show violent crime results in large and persistent increases in mortality and suicide, as well as losses in labour earnings and total incomes for victims.

Bindler and Ketel (2022) extend Ornstein (2017) by using Dutch police records to identify a larger group of victims, not just those who end up in hospital. Bindler and

[2] Comparing the rates of victimisation reported and not reported to the police by type of violent crime in 2022 suggests victims of robbery are more likely to report to the police compared to victims of rape, sexual assault, or assault. See Table 6 of U.S. Department of Justice (2023).

Ketel (2022) adopt an event study design to track labour market outcomes of victims aged 18-55 before and after the victimisation event. Results suggest that assault victimisations lead to labour earnings losses of about 8% measured 12 months after victimisation, with victims also exhibiting short-term increases in total and mental health expenditure.

Using police-reported victimisations in Denmark, Dustmann and Mertz (n.d.) expand on Bindler and Ketel (2022) by estimating the effects of victimisation on a broader range of individual health outcomes, not just health-related expenditures. They also introduce a gender component to the labour market costs of victimisation and link victims back to their childhood family context to study the relationship between childhood socio-economic status and victimisation risk. Using a difference-in-differences estimation strategy, where yet-to-be-victimised individuals are used as a comparison group for individuals victimised in the present, results show significant labour earnings losses for victims aged 18-50, lasting up to five years after victimisation. Women experience larger crime-induced earnings losses than men, and the authors attribute part of this effect to the gender gap in the types of victimisations experienced by men and women, as well as women having worse physical and mental health consequences from violent victimisations than men.

Finally, Bhuller et al. (2022) use police reports in Norway to estimate the costs of victimisation specifically for cases of domestic violence. Like Dustmann and Mertz (n.d.), the authors also employ a difference-in-difference estimation strategy using those who are victimised in the future as controls. For adult victims ages 30-50, domestic violence police reports cause mental health diagnoses to increase by 35% and employment and earnings to decrease by 4% and 5%, respectively.

Regarding the spillover effects of victimisation to non-victims, only Bhuller et al. (2022)

does this in detail.[3] In addition to estimating the costs of domestic violence victimisation for the victim (the parent), they also estimate the impacts for the victim's children.[4] Results from the dynamic difference-in-differences strategy show domestic violence leads to a 19% increase in mental health diagnoses for the victim's children within one year of the event, driven mostly by mood, sleep, and anxiety disorders. Using a regression discontinuity design around the timing of the parent's domestic violence police report relative to the timing of national exams, the authors also document a significant decline in children's test scores for those who took the test just after the domestic violence event compared to those who took the test just before. Similar results are seen for the likelihood of completing the first year of high school.

There are two notable gaps in the literature evaluating costs of victimisation. The first gap is that there is a lack of evidence on the effects of victimisation for youth victims ages 17 and under. Studying the effects of youth victimisation is important for at least two reasons. First, victimisation is more likely to happen to younger people than older people (see Appendix Figure C0.1). Second, experiencing a traumatic victimisation during one's formative years may have long-lasting negative impacts on youth's physical and mental health, development and well-being, and consequently human capital accumulation (Webbink et al., 2012; Gerson and Rappaport, 2013; Conti et al., 2021; Henkhaus, 2022). Thus, understanding the extent to which victimisation affects youth can better inform

[3]Bindler and Ketel (2022) state in a footnote that they also tested for spillovers on non-victim household members but they find no effects for the victim's cohabiting partner.

[4]Such estimates relate to the vast literature on the consequences of family violence for children and their families. For recent meta-analyses on the effects of children's exposure to domestic violence, see Devaney (2015) and Vu et al. (2016). For reasons explained in Section 4.7, family violence victimisation is beyond the scope of this paper. Bhuller et al. (2022) also relates to the literature exploring the spillover effects of parental health shocks to children (see, for example, Kristiansen (2021), and Glaser and Pruckner (2023)).

recovery and rehabilitation processes to help alleviate any long-term negative effects of victimisation.

The second literature gap is that, outside the context of parental domestic violence victimisation, there is little evidence on the household spillover effects of victimisation. Since there is scarce evidence on the impact of victimisation for youth victims, it is unsurprising there is no evidence on the spillover effects of youth victimisation to parents. Youth are still heavily involved with, and reliant on, their parents, so understanding how parents are affected by youth victimisation is an important contribution to the literature estimating the different costs of victimisation to society.

4.5 Background

New Zealand has a high victimisation rate by international standards, with a self-reported victimisation rate of 21.5% over a 12-month period (relative to an average of 15.5% in OECD countries (OECD, 2009)). Further, evidence from the New Zealand Crime and Victims Survey[5] shows that New Zealand's personal victimisation prevalence rates have continued to grow over recent years. Appendix Section C1 provides more details about these comparisons over time and across OECD countries.

When an individual is victimised in New Zealand, the victim (or a witness) can call the New Zealand Police to attend the incident. Upon arriving at the scene, the police will open a crime report and document a range of information relating to the incident, such as the date and time for which the offence becomes known to the police, details about the offence(s), whether the victim knew the offender, and details about the victim or the

[5]Publicly available results tables from the New Zealand Crime and Victims Survey can be found here.

person who reported the crime (New Zealand Police, 2016). This information is collected in the New Zealand Police Recorded Crimes Victims (NZRCV) register. After obtaining initial information about the crime, police will then decide if the incident warrants an investigation and, if so, will proceed to investigate the matter.[6] We have access to confidentialised police reports in the NZRCV, of which we focus on investigated incidents of crime.[7]

4.6 Conceptual Framework

This section outlines the conceptual framework that guides our empirical analysis of how assault victimisation affects youth victims and their parents. While the broader research questions have been introduced earlier, the aim here is to clarify the pathways through which such events may lead to measurable changes in health, education, and labour market outcomes.

Victimisation in adolescence can disrupt core aspects of life at a critical stage of development. A serious assault may lead to physical injury, but even in the absence of lasting physical harm, it can trigger emotional distress, social withdrawal, and institutional disengagement. For example, a student assaulted by peers may avoid school, not because of incapacity, but because of fear, shame, or stigma. A parent caring for a traumatised child may reduce their labour supply, either temporarily or in the longer term.

The framework that follows organises potential responses into a small number of conceptual channels and links each to specific outcomes observed in administrative data. It is

[6] Appendix Section C2 provides additional discussion on the rights and services available for victims in New Zealand, including how victims can be involved with police proceedings, the support services available for victims, and the principles and the rights of victims set out in the Victims Code.

[7] 98.8% of all crime occurrences are investigated.

not intended as a full structural model, but as a way to clarify the causal logic motivating the empirical design.

4.6.1 Conceptual channels and youth responses

Adolescent assault can trigger multiple, overlapping responses. While each victim has a unique experience, the analysis distinguishes three main pathways: health shocks, psychosocial disruption, and behavioural withdrawal. These channels may interact and produce short- or medium-term effects on health, schooling, and early labour market attachment.

The health shock channel captures physical injury or the onset or worsening of psychiatric conditions such as anxiety, depression, or post-traumatic stress. If this channel is active, we expect to see a sharp increase in hospital contacts immediately following the assault. Psychiatric diagnoses may appear with some delay, reflecting the time required for symptoms to develop and for formal assessment or treatment to occur.

The psychosocial channel reflects emotional and social responses such as shame, stigma, or loss of confidence, which can undermine academic engagement (Heckman et al. (2006); Waddell (2006)), even in the absence of a formal diagnosis. These effects may show up as gradual increases in mental health prescriptions or consultations, and/or as changes in school performance or behaviour.

The behavioural withdrawal channel describes cases where the student begins to avoid school or other settings associated with the assault.[8] This disengagement may lead to

[8]This behavioural response is consistent with Routine Activity Theory (L. E. Cohen & Felsen, 1979), which suggests that individuals adapt their daily routines to reduce exposure to potential offenders. This may involve avoiding specific locations, times of day, or activities perceived as risky.

increased absenteeism, disciplinary episodes, or reduced academic progress, particularly when the offender is a peer or school-based contact. Among older youth, similar avoidance could affect early attachment to the labour market, especially if the assault occurred in a workplace or training environment.

4.6.2 Conceptual channels and parent outcomes

The consequences of youth assault may extend beyond the victim and affect the wider household, particularly parents. These effects arise through both direct psychological responses and indirect caregiving demands, each with distinct implications for mental health and labour market outcomes.

First, parents may be affected directly by the trauma of the event. Even without being physically present or involved, they may experience anxiety, guilt, or emotional distress in response to the assault (e.g. Holt et al. (2014); Holt et al. (2017)). These reactions may manifest in increased use of mental health services and, in some cases, reduced labour supply due to mental strain or the need to take time off work.

Second, parents may respond indirectly by providing care or support to the affected child. This can take the form of time (e.g. taking leave to accompany the child to medical or school appointments) or money (e.g. covering unexpected costs or replacing lost income). These indirect responses are theoretically consistent with models of intra-household insurance, in which family members adjust their behaviour to buffer shocks affecting others (Fadlon and Nielsen (2019); Fadlon and Nielsen (2021)). The net effect on labour supply is ambiguous: caregiving through time may lead to temporary withdrawal from the workforce, while financial support may require increased earnings. Empirically, Andersen et al. (2020) show that parents provide informal insurance to adult children following negative income shocks, highlighting the role of family as a flexible support mechanism across different forms of need.

4.7 Data

Our analysis is based on individual-level data from various administrative registers within Stats NZ's Integrated Data Infrastructure (IDI). First-time youth victims are identified from police data on victimisation reports and are linked to their parents using New Zealand's birth register.[9] The linkage feature of the IDI allows us to attach information from various administrative registers to the same individual via a unique individual identifier.

4.7.1 Youth victim and parent samples

We identify first-time youth assault victims from the NZRCV register. Our initial sample comprises 150,600 youth victims (ages 0-30) who reported a personal (i.e., not property) victimisation between July 2014 and April 2022 and where the report was investigated by police.[10] The most common victimisation offence for this group was assault, experienced by 119,463 youth victims (79.3%).

We focus on non-family violence (non-FV) assaults, as defined by bloodline, because

[9] Appendix Section C3 provides more detail about the data construction process for these samples.

[10] This number reflects police-reported personal victimisations and thus differs from self-reported conventional victimisations referenced earlier from OECD (2009). In New Zealand, self-reported victimisation rates are much higher than police-reported victimisation rates because more than 75% of personal offenses are not reported to the police (Ministry of Justice, 2020). The New Zealand under-reporting rate is higher than that in Denmark (47%, Statistics Denmark (n.d.)) but lower than in Finland (90%, Adams-Prassl et al. (2022)).

we are interested in parents' responses to youth victimisation and do not want this to be conflated by any family-offender dynamics. This exclusion is possible due to unique information included the NZRCV register about a victim's relationship to the offender at the time of the incident. We drop 15,513 victimisations (13.0%) where the youth identified their offender as a parent, grandparent, sibling, child, a caregiver relative, or another family member.[11] Our final sample comprises 103,950 first-time youth victims who experienced a non-FV assault victimisation (hereafter referred to as 'assault victimisation').

We identify parents of youth assault victims using the New Zealand birth register, which includes unique identifiers for each individual's biological mother and father (where possible). We focus on each parent's first experience of youth victimisation and condition on the parent being of working-age (30-60 years) at the time of the victimisation. We can identify at least one parent for 62,424 youth (60.1%),[12] with fathers being identified for 52,182 youth (50.0%) and mothers for 60,957 youth (58.6%).[13] We append the mother-youth and father-youth pairs to create our parent sample comprising 113,139 parent-youth pairs (relating to 62,424 unique youth).

4.7.2 Offenders of youth victims

We obtain information about the offenders of youth assault victims through two sources. First, the New Zealand Recorded Crimes Victim (NZRCV) register contains information

[11]For completeness, we also include step-parents and step-children as part of the family violence definition. Although step-family members are not blood-related, they often taken on the family caregiving roles as if they were biological family. Of all victimisations between 2014 and 2022, less than 1% are cases where the victim identified the offender as either a step-parent or a step-child.

[12]This low linkage rate is primarily due to the exclusion of children born overseas, since these children are not in the New Zealand birth register and therefore cannot be linked to parents. A secondary reason for the lower linkage rate is that the parents of NZ-born children must be aged 30-60 at the time of the child's assault.

[13]We can identify both parents for 51,660 youth (49.7%).

about the relationship of the offender to the victim (ROV) from the perspective of the victim at the time of the police report. Of the 103,950 first-time youth assault victims, 14.34% were assaulted by a romantic partner, 11.69% by a friend, 10.46% by a stranger, and 63.50% by an offender that the victim could not identify.

Second, for victims whose offenders were subsequently proceeded against by police, we link victims to their offenders through a common occurrence ID (i.e., a fight ID) in both the NZRCV register and the New Zealand Police Recorded Crimes Offender (NZRCO) register. We identify an offender(s) for 41.4% of our youth victim sample. Some victims have multiple offenders, and some offenders have assaulted multiple victims. This results in 51,405 unique victim-offender pairs (comprised of 43,008 unique youth victims and 41,277 unique offenders).[14] We then use the unique individual identifiers to link victims and their offenders to other registers, such as school and employment records, to learn more about their relationship. Among schooling-aged victim-offender pairs (3,411), 48.28% were classmates; among working-aged victim-offender pairs (13,368), 14.72% were colleagues.[15]

We use these classifications of victim-offender relationships to probe heterogeneity in youth's responses to assault victimisation by the closeness to their offender.

4.7.3 Outcomes

Using the unique individual identifiers, we obtain information from various registers about youth victims and their parents for the period surrounding victimisation. For youth,

[14] Appendix Section C4 provides more information on the demographic make-up of these victim-offender pairs. It also describes how many offenders were subsequently charged and convicted for their assaults against youth, and the types of punishments they received in a criminal court.

[15] The classification of classmates and colleagues is measured in the six months leading up to, and including, the victimisation event. Classmates are defined by a school ID (not a classroom ID).

we add information about hospitalisations, pharmaceutical prescriptions, usage of mental health services, labour market outcomes, and schooling interventions. For parents, we add information about pharmaceutical prescriptions, usage of mental health services, labour market outcomes, and marital status. These outcomes are defined below.[16]

Physical and mental health outcomes

Hospitalisation records are obtained from the Ministry of Health's Public Hospital Discharges Event dataset. We create a hospitalisation indicator that equals one if the youth spent any time in hospital during the month and equals zero otherwise. We use the International Statistical Classification of Diseases and Related Health Problems (ICD) codes to create an indicator for physical injuries,[17] and we use the five-point patient clinical complexity code to identify the severity of the injury.[18]

We obtain information on prescriptions from pharmaceutical data provided by the Ministry of Health and PHARMAC. The register provides detailed information about the prescribed drug and the date for which it was dispensed from the pharmacy to the healthcare user. To evaluate youth's physical health effects of victimisation, we define a pain-relief indicator that equals one if the youth victim filled out a prescription(s) for analgesics in a given month. To evaluate the mental health effects of victimisation for youth and their parents, we create separate indicators that equal one if they filled out a

[16] Appendix Section C5 provides the youth and parent sample counts for different outcome variables which differ by age group and data timeframe.

[17] For each hospitalisation spell, there is the International Statistical Classification of Diseases and Related Health Problems (ICD) code that provides a clinical description of the patient's condition. Physical injuries are defined as codes S00-S99 and T00-T14. These codes relate to injuries to the head, neck, thorax, abdomen, lower back, lumbar, spine, pelvis, shoulder, upper arm, elbow, forearm, wrist, hand, hip, thigh, knew, lower leg, ankle foot, or unspecified truck/limb/body region.

[18] We differentiate between youth being marginally hurt (no clinical complexity, rank 0) and more severely hurt (minor to catastrophic clinical complexity, ranks 1-4).

prescription(s) for anxiolytics (to relieve anxiety), anti-depressants (to relieve depression), or anti-psychotics (to relieve psychosis) in a given month.

Individual-level usage of mental health services is obtained from the Ministry of Health's Programme for the Integration of Mental Health Data (PRIMHD) register. This register contains information on activities and services accessed by healthcare users to recover from mental health and addiction-related issues. For a given mental health team, we create indicators for youth victims (or parents) accessing mental health and addiction services at least once during the month. While there are many different types of mental health service teams, we focus on the two most common (the community team; and the alcohol and drug team) and the most serious (the inpatient team).[19] See Appendix Section C6 for a full explanation about the idiosyncrasies of each team. We group together all other mental health teams as a fourth category.[20]

Together, we use these outcome variables to test the health shock mechanism for youth's responses to assault victimisation. Unfortunately, we do not have high-frequency data on one's feelings of shame, stigma, or self-confidence, so cannot directly test the psychosocial channel.

[19]The community (CMT) mental health team provides a range of community-based mental health activities, such as cognitive behaviour therapy, learning mindfulness, stress management, and emotional self-regulation techniques. The alcohol and drug (AOD) team provides mental health services both in the community and in hospital settings. Their services are similar to that provided by CMT teams, but also include activities relating to substance abuse, detoxification, and counselling for addictive behaviour. The inpatient (IPT) mental health team provides the most serious type of mental health care, aimed at those whose mental health condition is urgent or presents a danger to themselves or others. The IPT team provides 24-hour care and treatment services in hospital-like settings and provides services such as crisis respite care and psychiatric rehabilitation hospital beds

[20]Note that these mental health services are opt-in programmes and are available to everyone irrespective of whether a crime was reported. This means any uptake in mental health services by the victim (or parent) reflects voluntary participation, rather than any requirement or eligibility criteria following a police report.

Labour market outcomes

We define five labour market variables using the Inland Revenue Department's (IRD) Employer Monthly Schedule (EMS) data. First, labour earnings equals the sum of all wages and salaries earned in given month. Second, an employment indicator equal to one if the individual had positive labour earnings in a given month, and zero otherwise.[21] Third, a benefit and Accident Compensation Corporation (ACC) variable that equals the sum of benefit income and ACC earnings in a given month.[22] Fourth, a benefit and ACC receipt indicator that equals one if the individual received positive benefit or ACC earnings in a given month, and zero otherwise. Finally, we sum earnings across all taxable income sources[23] to define a total income variable.[24]

An observed labour market response to youth victimisation can be thought of as direct evidence of the behavioural withdrawal channel, or could equally be a consequence of the health shock or psychosocial channels, or a combination of all three.

[21] To mitigate reporting issues in the tax register, we set wages and salaries equal to zero (and hence the employment indicator equal to zero) if the reported labour earnings is less than $100 NZD per month for parents, and less than $50 NZD per month for youth. The results are unaffected by this choice of threshold.

[22] Benefits included in the EMS are the "main" benefits available to individuals, including the emergency benefit, job seeker support, sole parent support, supported living payments, young parent payment, and youth payment. These are all means-tested. ACC compensation is available for employed individuals who, because of an injury resulting from an accident, are unable to work. The individual must be out-of-work for at least one week and have a medical certificate to prove so. The ACC compensation rate is 80% of prior labour earnings. See Appendix Section C2 for further details about ACC. See also: https://www.acc.co.nz/

[23] Labour earnings, benefit and ACC earnings, withholding payments, paid parental leave, pension payments, and student allowance.

[24] The labour earnings, benefit and ACC earnings, and total income variables are winsorised at the 99th percentile by age, sex, and calendar month.

Schooling outcomes

To examine educational impacts, we would ideally use test scores or school attendance data. In the absence of these, we obtain information about youth's schooling behaviour from a student interventions dataset compiled by the Ministry of Education. This register contains records of all instances where a primary or secondary school student has required additional support outside the ordinary education curriculum because they exhibited behavioural issues, attendance issues, learning difficulties, or other concerns. We create an indicator equal to one if a youth started a school intervention in a given month, and zero otherwise.[25]

This indicator can be thought of as a proxy for school disruption, which in turn may reduce overall educational attainment. Much like the impacts on youth's labour market attachment, any observed effect of victimisation on these schooling outcomes could be the downstream effect of the youth's health, psychosocial, or behavioural responses, or any combination of these.

4.7.4 Descriptive statistics

Table 4.1 compares the demographic and socio-economic composition of youth assault victims with the general population of youth. Column 1 presents the 2018 characteristics for a sample of youth victims (assaulted one year later in 2019), column 2 provides the same characteristics of a sample of youth from the 2018 general population matched to the same age-distribution as the victim sample, and column 3 provides the same but for

[25] In addition, we explore youth's uptake of the two most common types of interventions, namely those that resulted in stand-downs and suspensions and interventions for truancy-related issues. See Appendix Section C7 for an explanation of these interventions.

the whole population of youth in 2018.

Comparing columns 1 and 3 in Table 4.1 shows youth victims are significantly more likely to be Māori (39.67% versus 24.25%), have no qualifications (8.75% versus 5.00%), and have lower labour earnings ($11,715.96 versus $12,857.08) and rely more heavily on benefits ($2,539.29 versus $652.39) than the general population of youth. These differences persist even when adjusting the general population of youth to have the same age distribution as the youth victim sample (column 2).

These differences are also prevalent when comparing the characteristics of parents of youth victims to a matched sample of parents and the general population of parents of youth (see Appendix Table C0.1).

4.8 Empirical Strategy

4.8.1 Difference-in-differences model

Our objective is to identify the causal effect of assault victimisation on the outcomes of youth victims and their parents. The main empirical challenge is to estimate what would have happened to youth victims' outcomes had they not been victimised. The ideal experiment for estimating the causal effect of youth victimisation would be to randomly assign assault victimisations among youth and track outcomes over time. This type of experiment is clearly infeasible, so we follow Fadlon and Nielsen (2019, 2021), Bhuller et al. (2022), and Dustmann and Mertz (n.d.), and use a quasi-experimental research design that exploits variation in the timing of victimisation. Specifically, we construct counterfactual outcomes for youth victimised in the present using the outcomes of youth victimised in the future.[26]

We observe victimisations across 94 months from July 2014 to April 2022. We match individuals victimised in month v (the treatment group), with a v + Δ comparison group of youth experiencing an assault victimisation Δ-months later. We set $\Delta = 13$, resulting in 81 matched treatment-comparison groups.

For each treatment and comparison group, we create a monthly panel dataset spanning from 12 months before to 12 months after the treatment group's victimisation month. We assign month zero as the treatment group's victimisation month, which is a placebo victimisation month for the comparison group, who proceed to be victimised in month 13. This allows us to estimate the effect of victimisation for up to 12 months after the treatment group is victimised. The same setup is used to compare parents of youth victimised in month v to parents of youth victimised in month v + Δ.

We stack the monthly panel datasets across all treatment and comparison groups and estimate the following dynamic difference-in-differences regression model.

$$\begin{aligned} Y_{i,t} = \alpha + \beta \cdot treat_i + \sum_{e \neq -3, e=-12}^{12} \varphi_e \cdot M_e \\ + \sum_{e \neq -3, e=-12}^{12} \delta_e \cdot M_e \cdot treat_i + \gamma \cdot X_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{4.1}$$

In Eq.4.1, $Y_{i,t}$ is the outcome for youth i in relative month t, α is a constant, $treat_i$

[26] An alternative estimate of the treatment group's counterfactual labour earnings would be a linear extrapolation of the treatment group's pre-victimisation outcome (months -12 to -1) over the post-victimisation period (months 0-12). This comparison would eliminate time-invariant heterogeneity in the outcome but would not account for time-varying effects, such as calendar time. This extrapolation is visualised in Appendix Figure C0.2, using youth's labour earnings as the outcome. The linear extrapolation of the treatment group's pre-victimisation labour earnings lies almost perfectly on top of the yet-to-be-victimised comparison group's earnings. This suggests that our estimates of the effect of assault victimisation on youth's earnings are robust to whether and how we control for time-varying factors affecting youth's outcomes.

is a dummy equal to one if the youth belongs to the treatment group; M_e are event time indicators (i.e., M_e=1[e=t], where 1[ ] is an indicator function) ranging -12 to +12 months around the treatment group's victimisation timeline, with month -3 set as the baseline category and month 0 being the victimisation month (v), and $X_{i,t}$ includes birth year, calendar year, and calendar month fixed effects. Standard errors are clustered at the youth-level.[27]

The coefficients of interest are the δ_e's. The δ_e's identify the causal effect of victimisation on youth victims' outcome (Y), e months after victimisation, assuming that the outcome trajectory of victimised youth would be, on average, the same as the outcome trajectory of the yet-to-be-victimised youth in the absence of victimisation (i.e., the common trends assumption). While the common trends assumption is untestable, we show evidence in favour of the assumption by showing that the outcomes of the treatment and comparison groups follow the same trend prior to the victimisation of the treatment group. Specifically, we show that estimates of δ_e are close to zero in the months leading up to the victimisation of the treatment group ($e < 0$). When Y is a labour market outcome, we also report estimates of δ_e as a percentage of the counterfactual outcome of the treatment group, calculated by Eq.4.2

$$P_e = \frac{\delta_e}{E[\hat{Y}_{i,t} \mid e, \text{treat}_i = 1]} \tag{4.2}$$

Where $\hat{Y}_{i,t} = \hat{\alpha} + \hat{\beta} + \sum_{e\neq-3,e=-12}^{12} \hat{\varphi}_e \cdot M_e + \hat{\gamma} \cdot X_{i,t}$ is the predicted counterfactual outcome for youth i in the treatment group at event time e.

Further, to estimate average effects across all post-victimisation months, we replace

[27]For the parent-level analysis, both the youth's birth year and the parent's birth year are included as fixed effects.

the event time indicators in Eq.4.1 with a post-victimisation dummy equal to 1 for months during and after victimisation ($e \geq 0$) and zero for months prior to victimisation ($e < 0$). This two-period difference-in-differences model is specified in Eq.4.3. The coefficient of interest from Eq.4.3 is θ, which identifies the average effect of victimisation for youth victims under the same assumptions as for Eq.4.1.

$$Y_{i,t} = \alpha + \beta \cdot \text{treat}_i + \phi \cdot \text{post}_{i,t} + \theta \cdot \text{treat}_i \cdot \text{post}_{i,t} + \gamma \cdot X_{i,t} + \epsilon_{i,t} \tag{4.3}$$

To explore heterogeneity in the response to youth victimisation, we augment Eq.4.1 by allowing the estimated effect of victimisation (δ_e) to vary with a characteristic, Z, where δ_e in Eq.4.1 is substituted for $(\lambda_0 + \lambda_1 Z_i)$ in Eq.4.4.

$$\begin{aligned} Y_{i,t} = \alpha + \beta \cdot treat_i + \sum_{e \neq -3, e=-12}^{12} \varphi_e \cdot M_e + \\ \sum_{e \neq -3, e=-12}^{12} (\lambda_0 + \lambda_1 Z_i) \cdot M_e \cdot treat_i + \gamma \cdot X_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{4.4}$$

In Eq.4.4, λ_0 captures the impact of victimisation for youth with characteristic $Z_i = 0$, and the sum of $\lambda_0 + \lambda_1$ captures the effect of victimisation for those with characteristic $Z_i = 1$.[28] Thus, λ_1 alone identifies the additional effect of victimisation for youth with characteristic $Z_i = 1$ compared to $Z_i = 0$.

The two-period equivalent of Eq.4.4 is given by Eq.4.5.

[28] We define Z as a binary variable in Eq.4.4 and Eq.4.5 for ease of explanation. Note that Z can also be a categorical variable, such as the victim-offender relationship variable (ROV) which has four categories (no offender, romantic relation, friend, stranger). In this case, we also estimate λ_2 [λ_3], which identifies the additional effect of youth with characteristic $Z_i = 2$ [$Z_i = 3$] compared to $Z_i = 0$.

$$Y_{i,t} = \alpha + \beta \cdot \text{treat}_i + \phi \cdot \text{post}_{i,t} + (\lambda_0 + \lambda_1 Z_i) \cdot \text{treat}_i \cdot \text{post}_{i,t} + \gamma \cdot X_{i,t} + \epsilon_{i,t} \tag{4.5}$$

In Eq.4.5, the parameter of interest is λ_1, which captures the average monthly effect of victimisation for youth victims with $Z_i = 1$, over and above the effect for those with $Z_i = 0$.

4.8.2 Interpreting the treatment effect

Interpreting the treatment effect of victimisation for first-time victims requires consideration about what the treatment effect is and what it means to be a first-time reported victim. These two points are discussed below.

It is possible that our treatment effect reflects multiple treatments: the victimisation event itself and the reporting of the victimisation. For example, a victimisation event may cause negative mental health outcomes for the victims and reporting the victimisation may trigger eligibility for mental health support. While we cannot fully distinguish between these two treatments, there are no policy documents in New Zealand that explicitly state eligibility for social support programmes or financial aid for victims depend on the victim reporting their victimisation. Spokesperson for Manaaki Tāngata Victim Support, Dr Petrine Hargrave said "anyone who experienced a crime in New Zealand was entitled to free support, regardless of whether they reported the crime" (Manaaki Tāngata Victim Support, 2022). This is echoed in New Zealand's Victims Code where the principles surrounding support for victims in their psychological, physical, and financial recovery must be applied regardless of the victim's reporting status. Further, Te Whatu Ora's patient triage process for accessing health services is based on clinical need[29] rather than an official victimisation status. Thus, given the New Zealand context, we believe our treatment effect is mostly capturing the effect of the victimisation event itself, although we cannot rule out that it might partly also reflect the reporting of the victimisation.

Next, it is important to consider what it means to be a *first-time reported* victim in the New Zealand police register. Our sample likely comprises two types of victims: first-time youth assault victims and first-time *reported* youth assaulted victims. It may be the case that all first-time reported youth victims are also first-time youth victims if all youth contacted the police when they first experienced being assaulted. However, this may not always be the case.

One reason these two groups may differ is because the NZRCV register begins in July 2014, so if a youth reported a victimisation(s) pre-July 2014 it would be unobserved in the NZRCV data. Another reason that first-time reported victims may not be first-time victims is that the decision to report a victimisation depends on several key factors, such as the severity or location of the crime. It is possible that a first-time reported youth victim may have experienced a victimisation(s) in the past, but the current event was the first time it was severe enough to warrant police involvement. Alternatively, a youth victim may have been victimised in the past, but the current event was the first time it was seen by a witness who subsequently reported it to the police.

We cannot perfectly distinguish between first-time youth victims and first-time reported youth victims. However, we employ two approaches to test the possible extent of

[29]For more information on the five triage categories Te Whatu Ora uses to assess patients' needs, see here.

this issue. First, to address the concern about the NZRCV start date, we repeat our main analyses on the sub-sample of youth victims with no recorded victimisations in the first 12 months of the NZRCV data (i.e., those victimised between July 2015 and April 2022, rather than the full period of July 2014 to April 2022). Presented in Appendix Tables C0.2 and C0.3, results are substantially unchanged.

Second, as discussed in Section 4.7, we focus on non-family violence assaults. One of the reasons for doing this is because the reporting of FV victimisations can be more likely a result of ongoing abuse that eventually crossed the reporting threshold than a first-time occurrence (Bhuller et al., 2022), thus the reports of non-FV assaults are more likely to be first-time victimisation occurrences. We further explore this idea of offender proximity by distinguishing between non-FV assault victimisation where the victim's offender was known (e.g. a friend) versus not-known (e.g. a stranger).

4.9 Results

In this section, we present the findings for the effect of assault victimisations on youth victims (section 4.9.1) and their parents (section 4.9.2). We begin with the physical health effects of youth assault victimisation, followed by the mental health effects. For working-aged youth, we show the effects of youth victimisation on labour market outcomes, while for schooling-aged youth, we explore the effects of youth victimisation on schooling behaviour. For parents, we study their responses to youth assault victimisation across mental health outcomes and labour market outcomes.

We report some robustness checks in Appendix Tables C0.2, C0.3, C0.4, and C0.5 where we estimate the effects of assault victimisation on youth's and parent's outcomes using three alternative samples. First, we exclude youth who were victimised in the first 12 months of the police register. As mentioned above, this is to partly address the potential reporting issue of identifying "first time" victims as opposed to "first time reported" victims. Second, we exclude youth who were also offenders in the month leading up to, and including the day of, the victimisation event. This is to show that the estimated effects are in fact a response to victimisation, and not confounded by any effects of simultaneously being an offender. Third, for youth, we focus on the sub-sample of youth that have linked parents in the parent-level analysis. For parents, we exclude those who themselves were a victim of crime in the 12 months leading up to, and including, the youth's victimisation. The estimates across all sub-samples are generally very similar to the estimates reported from the main sample of youth and parents.

Appendix Section C8 presents sensitivity analyses addressing multiple hypotheses testing using the Bonferroni adjustment. Our main narrative regarding youth victims and their parents remains consistent under this correction, irrespective of how to count the number of independent hypotheses tested.

4.9.1 The effects of assaults on youth victims

Physical health responses to youth victimisation

Figure 4.1 shows estimates of δ_e (see Eq.4.1) from 12 months before to 12 months after victimisation of the treatment group in month 0, where the dependent variable is an indicator for victims being hospitalised (Panel 4.1a) or for victims filling out prescriptions for pain relief medication (Panel 4.1b).

Estimates of δ_e are close to 0 in the months leading up to victimisation in both panels of the figure, lending support to the common trends assumption. In the month of victimisation (month 0), the probability of going to hospital increases by 5.21 percentage points (or 222% of the baseline mean of 2.35% in month -3).[30] About 72% of this increase (3.76 percentage points) is due to non-complex physical injuries while the remaining 28% of this increase (1.45 percentage points) is due to moderate-to-catastrophic physical injuries.

Correspondingly, victimisation leads to an excess uptake in pain relief prescriptions of 4.43 percentage points (from a baseline mean of 4.18%) for youth victims in the month of victimisation.

Tables 4.2 and 4.3 show heterogeneity in the estimated effect of victimisation on youth's physical health across assault severity, age, victim-offender relationship, and gender. Row 1 shows the month 0 effect from Eq.4.1 and the month -3 baseline (also depicted in Figure 4.1). The subsequent rows contain the results from interacted difference-in-differences regressions (Eq.4.4) where we interact the treatment effect with a heterogeneity characteristic listed in the first column. We provide the month -3 baseline and the month 0 treatment effect for each group, and the difference between the treatment effects for a given group relative to a reference group.

All groups experience a physical health shock when assaulted, but there are important differences in the health responses across groups. For instance, for youth victims of serious assault, victimisation increases the probability of going to hospital by 12.35 percentage points in the month of victimisation, while this increase is just 2.76 percentage points for victims of other assaults. The resulting difference of 9.59 percentage points is statistically

[30] When disaggregating the month 0 hospitalisation effect by physical versus non-physical injuries, we find that almost 100% of this month 0 spike is due to physical injury hospitalisations.

significant at the 1% level. Furthermore, older youth and male victims are significantly more likely to be hospitalised and receive pain relief prescriptions than their counterparts. In addition, Tables 4.2 and 4.3 show that youth assaulted by a stranger or friend experience worse physical health responses than youth assaulted by a romantic relation or those with no identified offender.[31]

Mental health responses to youth victimisation

We next, explore the impact of victimisation on two measures of youth's mental health: the use of mental health services and the receipt of health-related medications. Tables 4.4-4.7 show the month 0 estimates for the four mental health teams respectively (community, alcohol and drug, inpatient, and all other teams). Tables 4.8-4.10 show similar estimates for three mental health-related prescriptions (anti-anxiety, anti-depressants, and anti-psychotics). Both tables are structured as Tables 4.2 and 4.3.

Assault victimisation leads to an immediate increase in the likelihood of using all types of mental health services (row 1 in each Table 4.4-4.7). The biggest uptake of mental health services is for community (CMT) team services (Table 4.4), increasing 1.14pp from a baseline of 1.37%. These CMT services include cognitive behaviour therapy, and learning mindfulness, stress management, and emotional self-regulation techniques, with the length of each session lasting at least two hours.

Assault victimisation also increases the probability that victims receive mental health-related medications in the month of victimisation (row 1 in each Table 4.8-4.10). For example, in Table 4.8, the probability of filling out a prescription for anti-anxiety medi-

[31] The physical health responses of youth assaulted by friends and youth assaulted by strangers are not statistically significantly different from each other.

cation increases by 0.20 percentage points in month 0 (37.74% of the month -3 baseline mean of 0.53%).

The estimates across Tables 4.4-4.10 also reveal substantial heterogeneity in youth's mental health responses to assault victimisation. Teen (ages 13-19) and young adult (ages 20-30) victims are significantly more likely to use mental health service and mental health-related medications than child (ages 0-12) victims.[32] Furthermore, while the mental health of all victims is impacted by victimisation, the immediate uptake in community mental health services and anti-anxiety medication is greater for victims of more severe assaults.

Finally, estimates reported in Tables 4.4-4.10 show that youth assaulted by a romantic relation are more likely to require mental health services and prescriptions due to the assault relative to youth assaulted by other types of offenders. This contrasts the physical health response to victimisation (Tables 4.2 and 4.3), which is largest for youth assaulted by friends or strangers.

Labour market effects of youth victimisation

We next investigate the labour market consequences of victimisation for working-aged youth victims (ages 18-30). Figure 4.2 presents the estimates of the impact of victimisation on labour earnings and benefits and ACC earnings (Panel 4.2a), and total income (Panel 4.2b), estimated separately by Eq.4.1. Panel 4.2a shows that in the first month after victimisation, victims' labour earnings decrease by $68 NZD, and 12 months after victimisation youth victim's monthly labour earnings losses from victimisations have in-

[32]The mental health responses of teen victims and young adult victims are not statistically significantly different from each other.

creased to \$133 NZD (relative to the mean earnings of \$1,511 NZD in base month -3).[33] Panel 4.2a also shows that victimisation leads to an immediate uptake of benefit and ACC earnings in the first month after victimisation (an increase of \$40 NZD), but this response vanishes over time. Consequently, youth experience a clear net-loss in total income due to being assaulted (Panel 4.2b), which is consistent with the existing literature studying adverse effects of health shocks on earnings (see e.g. Halla and Zweimuller (2013), and Fadlon and Nielsen (2021)).[34]

Tables 4.11-4.14 report heterogeneity in the labour market effects of victimisation for different groups of working-aged youth. The first row of each table provides the average monthly effect estimated using the two-period difference-in-differences model specified in Eq.4.3. In the subsequent rows, we provide the average monthly effects for different groups of youth victims estimated by Eq.4.5.

Table 4.12 shows that all groups of working-aged youth victims experience labour earnings losses due to assault victimisation. Males have significantly larger earnings losses than females (\$105 NZD versus \$40 NZD per month), and victims assaulted by strangers have significantly larger earning losses than their counterparts (\$141 NZD versus about \$60 NZD per month).

Interestingly, while labour earnings losses from victimisation are similar by severity, youth assaulted more severely have significantly higher benefit and ACC earnings post-

[33] To provide confidence that the estimates of δ_e's from Eq.4.1 are driven by a treatment group response to victimisation, Appendix Figure C0.3 plots the raw wages and salaries for the treatment and comparison groups over the treatment group's victimisation timeline. Results confirm the negative treatment effects estimated by Eq.4.1 are caused by a drop in labour earnings for the treatment group rather than an increase in labour earnings for the comparison group.

[34] Appendix Figure C0.4 plots the estimates of δ_e's from Eq.4.1 over a 24-month and a 48-month post-victimisation horizon and demonstrates that the gap between the treatment and comparison group's labour earnings continues to grow for up to four years post-victimisation.

victimisation (Table 4.13). This latter difference reflects New Zealand's ACC scheme providing financial compensation to those who suffer a physical injury. As a result of this differential compensation, the net loss in total income is significantly lower for youth experiencing more severe assaults (Table 4.14).

In the last three rows of Tables 4.11-4.14 we restrict the sample to those who were employed (for at least one month) in the six months prior to victimisation and whose offender could be identified and was also employed in the six months prior to the victimisation event. This sample allows us to explore heterogeneity in youth's labour market response to victimisation by whether the victim and offender worked together. By construction, the baseline employment rate and monthly labour earnings are much higher for this sample than the overall working-aged youth sample in row 1. This exercise reveals a striking heterogeneity. While the average earnings losses for youth victims who worked with their offender is \$472 NZD, it is \$333 NZD for victims whose perpetrator worked elsewhere. This pattern is consistent across all labour market outcomes.

Youth victimisation and schooling interventions

For schooling-aged (ages 5-18) victims, we investigate whether assault victimisation causes youth to exhibit behavioural issues, attendance issues, learning difficulties or other concerns at school that result in a schooling intervention. Row 1 of Table 4.15 presents the month 0 treatment effect estimates from Eq.4.1. Upon being victimised, the likelihood of starting a schooling intervention increase by 1.17 percentage points (relative to a month -3 baseline mean of 2.88%).[35]

[35] Panel C0.5a of Appendix Figure C0.5 presents the corresponding graph plotting δ_e from Eq.4.1 for the effect of assault victimisation on the uptake of schooling interventions. Panels C0.5b and C0.5c provide the equivalent for different types of schooling interventions. These latter results suggest that the

The subsequent rows of Table 4.15 show heterogeneity the month 0 uptake in schooling interventions as estimated by Eq.4.4. We see an immediate take-up of schooling interventions for both primary and secondary-school aged youth, for male and female victims, and irrespective of assault severity. The last three rows of Table 4.15 restrict the sample to those attending a school in the six months prior to victimisation and whose offender could be identified and was also at a school during that time. This sample allows us to explore heterogeneity in youth's schooling intervention rate by whether the youth victim and offender went to the same school. The month 0 uptake in schooling interventions due to assault victimisation is larger for youth whose offender is at the same school (1.04% points) compared to youth whose offender went to a different school (0.33% points), however these point estimates and the difference between them are not statistically significant, likely because the schooling sample is rather small.

4.9.2 The effects of assaults on parents of youth victims

Having established youth's responses to assault victimisation, we now explore the flow-on effects of youth victimisation for the victim's parents' mental health and labour market outcomes.[36]

Parents' mental health responses to youth victimisation

Like youth's mental health, we explore the impact of youth victimisation on parents' use of mental health services and receipt of mental health-related medications. Tables

effect of assault victimisation on stand-downs or suspensions occurs in the month of the victimisation, while the effect on truancy interventions tends to happen in the first few months after the victimisation.

[36]When testing whether youth victimisation impacts parents' marital stability, we find no effect on the probability that the parent gets divorced, or the probability the parent gets married, when their youth is assaulted. Results are available upon request.

4.16-4.19 present the month 0 estimates of the effects for the four mental health teams respectively (community, alcohol and drug, inpatient, and all other teams) and Tables 4.20-4.22 present the month 0 estimates for the three mental health-related prescriptions (anti-anxiety, anti-depressants, and anti-psychotics). These tables are structured in the same way as the equivalent youth tables.

The tables reveal that youth assault victimisation has an immediate negative effect on parents' mental health. Youth victimisation causes a 0.17 percentage point increase in the likelihood that parents use community mental health services in the month of the victimisation event (35% increase from a month -3 baseline of 0.48%) and a 0.14 percentage point increase in the likelihood that parents fill out prescriptions for anti-anxiety medication (0.10% increase from a baseline of 1.31%). These estimates are significantly larger for mothers than fathers, and for parents of children (ages 0-12) than parents of teens (ages 13-19) and parents of young adults (ages 20-30). There is limited heterogeneity in parents' mental health responses to youth victimisation across other domains. Further, we do not see any effects of youth victimisation on parents' use of other mental health services or receipt of other types of mental health-related medications.

Parents' labour market responses to youth victimisation

Figure 4.3 plots the estimates of δ_e from Eq.4.1 where the dependent variable is parents' labour earnings (blue) or benefit/ACC earnings (red) in Panel 4.3a, and where the dependent variable is total income in Panel 4.3b. In the aftermath of youth victimisation, parents' labour earnings slowly decrease over the subsequent 12-month period, and by month 12, parents' monthly labour earnings loss from youth victimisation is \$20 NZD.[37] There is no significant uptake in monthly benefit/ACC earnings for parents when their child is victimised, which is perhaps unsurprising since the ACC scheme is mostly targeted at the injured healthcare user. As a result, we see a net-loss in the total income of parents when their child is assaulted.

While some of this labour market response might be due to parents' mental health suffering when their child being assaulted, we argue that most of the parental response is due to parents reducing working hours to increase time spent caring for their child. We show evidence in line with this explanation in Tables 4.23-4.26, which show heterogeneity in parents' labour market response to youth victimisation.

Table 4.24 shows that parent's monthly labour earnings losses are significantly larger for those whose child were seriously assaulted and injured (\$36 NZD), than the losses borne by parents whose child experienced less severe types of assault (\$7 NZD).

Furthermore, we know from previous results that youth assaulted by a romantic relation are significantly more likely to use mental health services and mental health-related medication due to the assault, than youth assaulted by other offenders. Tables 4.24 and 4.26 show that parents' labour earnings and total income responses, respectively, are also greater when their child is assaulted by a romantic relation compared to other relations, suggesting that parents may take more time away from work when their child experience greater mental health problems from victimisation.

Finally, Tables 4.23-4.26 reveal that there are no significant differences in parents' responses by the gender of their child, that both mothers and fathers experience significant

[37] Appendix Figure C0.6 plots the dynamic difference-in-differences estimates for parents' labour earnings over a 24-month and 48-month post-victimisation period. Results suggest parents' labour earnings do not recover to counterfactual levels even up to four years after their youth was assaulted.

employment and labour earnings losses when their child is assaulted, and that mothers have significantly higher benefit and ACC earnings than fathers.

4.10 Conclusion

This paper contributes to the literature exploring the consequences of victimisation by specifically focusing on the experiences of youth victims. Using detailed register data on the population of reported assault victims in New Zealand, we estimate the impact of non-family violence assault victimisation on youth victim's physical and mental health, their labour market outcomes, and their schooling behaviour.

We show that assault victimisation leads to significant adverse effects for youth victims. We document immediate increases in hospitalisations and the use of pain-relief medication, as well as the use of mental health services and mental health-related medications. In addition to physical and mental health responses, we show adverse effects of youth victimisation both in the labour market and at school. For working-aged youth, we document substantial labour earnings and total income losses from victimisation, highlighting that compensatory earnings from public benefits and the ACC injury-compensation scheme play a small role for youth victims of crime. For schooling-aged youth, we show assault victimisation causes an immediate increase in the likelihood of starting a schooling intervention for learning difficulties, behavioural issues, or truancy issues. Taken together, it is clear that adolescent victimisation can disrupt multiple dimensions of life beyond physical health, often leading to emotional distress and institutional disengagement.

We document strong heterogeneity in the response to youth victimisation by assault severity. Youth who experience a more serious and injurious assault have more severe physical and mental health responses and larger labour market responses relative to youth who experience less serious assaults. Further, New Zealand's unique administrative data then allowed us to probe heterogeneity in youth's response to victimisation by their relationship to their offender. Youth assaulted by a friend or stranger are more likely to go to hospital and be prescribed pain relief, while youth assaulted by a romantic relation are more likely to require mental health services and related prescriptions due to the assault. Labour market and schooling responses are also larger when the youth victim is in closer proximity to their offender, namely being colleagues or classmates, respectively.

Finally, we estimate the spillover effects of youth victimisation on parents. Using New Zealand's birth register, we link youth victims to their parents and estimate the impacts of youth assault victimisation on parents' mental health and labour market outcomes. Youth victimisation has an immediate impact on parent's mental health, as evidenced by a significant immediate uptake in the use of mental health services and anti-anxiety medication. Thus, the trauma of a child's assault has clear emotional consequences for parents.

We then show parents' labour earnings decrease when their child is assaulted, with heterogeneity analyses revealing that labour earnings losses are largest for parents whose children suffer most from victimisation. These findings are consistent with a parental caregiving response, though we cannot rule out that the emotional toll of their child's trauma also contributes to this effect.

Further, back-of-the-envelope calculations reveal that parents make up a significant proportion (one-third) of the total annual labour earnings losses resulting from youth victimisation, thus failing to consider the indirect effects on parents will result in a considerable underestimation of the societal costs of youth victimisation.

4.11 Tables

Table 4.1: Summary statistics: Youth assault victims versus general population of youth

Characteristic	Youth assault victims	Matched general population of youth	General population of youth
Age (in years)	18.93	18.93	15.75 ***
Female (%)	48.96	48.80	48.54
Ethnicity: European (%)	33.55	46.13 ***	45.51 ***
Ethnicity: Māori (%)	39.67	22.63 ***	24.25 ***
Ethnicity: Pacific (%)	9.71	9.76	9.18 **
Ethnicity: Asian (%)	8.93	18.01 ***	17.50 ***
Ethnicity: Other (%)	2.36	2.81 ***	3.04 ***
No qualifications as at Census 2018 (%)	8.75	6.34 ***	5.00 ***
High school qualification as at Census 2018 (%)	35.57	39.63 ***	28.25 ***
Bachelor's and above as at Census 2018 (%)	4.29	10.44 ***	8.50 ***
Missing Census 2018 education information (%)	51.39	43.59 ***	58.24 ***
NZ Deprivation Index (ranging 0-10)	6.95	5.90 ***	5.87 ***
NZ Deprivation Index 2018 missing (%)	19.35	9.69 ***	9.26 ***
Labour earnings in 2018 (NZD)	11,715.96	16,565.88 ***	12,857.08 ***
Benefit earnings in 2018 (NZD)	2,536.29	956.18 ***	652.39 ***
Total counts	12,726	12,726	1,978,086

Notes: The table shows 2018 descriptive statistics for three samples: youth victims (victimised in 2019), a matched sample of the general population of youth (2018), and the full general population of youth (2018). The matched sample of youth has the same age distribution as the youth victims. The NZ Deprivation Index is an area-based measure of deprivation, with 1 representing the least deprived areas and 10 representing the most deprived. Asterisk in column 2 indicate significant differences between column 2 and column 1, while the asterisk in column 3 indicate significant differences between column 3 and 1. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 4.2: Heterogeneity: Youth's hospitalisation rate

		Count	Baseline	Month 0 treatment effect	Difference in Z's
All	All	93,243	2.35	5.21 (0.12) ***	
Assault severity	Other assaults	69,336	2.17	2.76 (0.12) ***	
	Serious assault, injury	23,907	2.89	12.35 (0.29) ***	9.59 ***
Age group	Ages 0-12	13,998	1.56	3.71 (0.24) ***	
	Ages 13-19	25,689	1.89	4.38 (0.20) ***	0.67 **
	Ages 20-30	53,556	2.78	6.01 (0.17) ***	2.30 ***
ROV	No offender identified	57,348	2.21	5.34 (0.15) ***	
	Offender = romantic	14,457	4.24	2.74 (0.35) ***	-2.60 ***
	Offender = friend	11,388	1.90	6.20 (0.33) ***	0.86 **
	Offender = stranger	10,047	0.93	7.00 (0.32) ***	1.66 ***
Female	Male	46,800	1.35	7.84 (0.16) ***	
	Female	46,431	3.36	2.57 (0.17) ***	-5.28 ***

Notes: This table provides the month 0 treatment effects for the impact of assault victimisation on youth's hospitalisation rate. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The last column in each panel provides the difference in treatment effects between the benchmark category and selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. Note that the sum of the male and female sub-samples does not equal the overall sample because there are some youth victims with missing gender information. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 4.3: Heterogeneity: Youth's pain relief prescriptions

		Count	Baseline	Month 0 treatment effect	Difference in Z's
All	All	99,630	4.18	4.43 (0.14) ***	
Assault severity	Other assaults	73,794	4.02	2.69 (0.15) ***	
	Serious assault, injury	25,836	4.67	9.50 (0.30) ***	6.81 ***
Age group	Ages 0-12	15,315	6.16	1.78 (0.37) ***	
	Ages 13-19	27,594	3.06	4.43 (0.23) ***	2.65 ***
	Ages 20-30	56,721	4.20	5.09 (0.18) ***	3.31 ***
ROV	No offender identified	62,487	4.31	3.85 (0.17) ***	
	Offender = romantic	14,745	4.59	3.11 (0.35) ***	-0.73 *
	Offender = friend	11,874	4.10	6.19 (0.40) ***	2.35 ***
	Offender = stranger	10,527	2.98	7.66 (0.41) ***	3.81 ***
Female	Male	50,208	3.48	6.07 (0.19) ***	
	Female	49,407	4.89	2.78 (0.19) ***	-3.30 ***

Notes: This table provides the month 0 treatment effects for the impact of assault victimisation on the likelihood the youth fills out a prescription for pain relief medication. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The last column in each panel provides the difference in treatment effects between the benchmark category and selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. Note that the sum of the male and female sub-samples does not equal the overall sample because there are some youth victims with missing gender information. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.4: Heterogeneity: Youth's mental health services (Community teams)

		Count	Baseline	Month 0 treatment effect	Difference in Z's
All	All	93,243	1.37	1.14 (0.08) ***	
Assault severity	Other assaults	69,336	1.44	1.02 (0.09) ***	
	Serious assault, injury	23,907	1.19	1.51 (0.15) ***	0.49 ***
Age group	Ages 0-12	13,998	0.78	1.06 (0.16) ***	
	Ages 13-19	25,689	1.82	1.55 (0.16) ***	0.49 **
	Ages 20-30	53,556	1.32	0.96 (0.10) ***	-0.10
ROV	No offender identified	57,348	1.45	1.33 (0.10) ***	
	Offender = romantic	14,457	1.30	1.51 (0.19) ***	0.17
	Offender = friend	11,388	1.71	0.66 (0.22) ***	-0.67 ***
	Offender = stranger	10,047	0.66	0.11 (0.15)	-1.22 ***
Female	Male	46,800	1.30	0.89 (0.10) ***	
	Female	46,431	1.45	1.40 (0.11) ***	0.51 ***

Notes: This table provides the month 0 treatment effects for the impact of assault victimisation on youth's participation in community mental health services. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The last column in each panel provides the difference in treatment effects between the benchmark category and selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. Note that the sum of the male and female sub-samples does not equal the overall sample because there are some youth victims with missing gender information. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.5: Heterogeneity: Youth's mental health services (Alcohol and drug teams)

		Count	Baseline	Month 0 treatment effect	Difference in Z's
All	All	93,243	0.54	0.23 (0.05) ***	
Assault severity	Other assaults	69,336	0.50	0.22 (0.05) ***	
	Serious assault, injury	23,907	0.66	0.25 (0.10) **	0.03
Age group	Ages 0-12	13,998	0.04	-0.01 (0.03)	
	Ages 13-19	25,689	0.56	0.21 (0.09) **	0.23 **
	Ages 20-30	53,556	0.66	0.30 (0.07) ***	0.31 ***
ROV	No offender identified	57,348	0.52	0.19 (0.06) ***	
	Offender = romantic	14,457	0.68	0.42 (0.14) ***	0.23
	Offender = friend	11,388	0.67	0.41 (0.15) ***	0.22
	Offender = stranger	10,047	0.30	0.00 (0.10)	-0.19
Female	Male	46,800	0.61	0.21 (0.07) ***	
	Female	46,431	0.48	0.26 (0.06) ***	0.05

Notes: This table provides the month 0 treatment effects for the impact of assault victimisation on youth's participation in alcohol and drug related mental health services. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline].The last column in each panel provides the difference in treatment effects between the benchmark category and selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. Note that the sum of the male and female sub-samples does not equal the overall sample because there are some youth victims with missing gender information. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.6: Heterogeneity: Youth's mental health services (Inpatient teams)

		Count	Baseline	Month 0 treatment effect	Difference in Z's
All	All	93,243	0.09	0.09 (0.02) ***	
Assault severity	Other assaults	69,336	0.08	0.09 (0.03) ***	
	Serious assault, injury	23,907	0.10	0.07 (0.04) *	-0.02
Age group	Ages 0-12	13,998	0.01	-0.01 (0.01)	
	Ages 13-19	25,689	0.08	0.07 (0.04) *	0.07 *
	Ages 20-30	53,556	0.11	0.12 (0.03) ***	0.13 ***
ROV	No offender identified	57,348	0.09	0.11 (0.03) ***	
	Offender = romantic	14,457	0.07	0.04 (0.05)	-0.08
	Offender = friend	11,388	0.14	0.03 (0.06)	-0.09
	Offender = stranger	10,047	0.02	0.07 (0.04) *	-0.04
Female	Male	46,800	0.07	0.13 (0.03) ***	
	Female	46,431	0.10	0.04 (0.03)	-0.09 **

Notes: This table provides the month 0 treatment effects for the impact of assault victimisation on youth's participation in inpatient mental health services. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The last column in each panel provides the difference in treatment effects between the benchmark category and selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. Note that the sum of the male and female sub-samples does not equal the overall sample because there are some youth victims with missing gender information. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.7: Heterogeneity: Youth's mental health services (Other teams)

		Count	Baseline	Month 0 treatment effect	Difference in Z's
All	All	93,243	0.26	0.13 (0.03) ***	
Assault severity	Other assaults	69,336	0.27	0.11 (0.04) ***	
	Serious assault, injury	23,907	0.25	0.20 (0.07) ***	0.08
Age group	Ages 0-12	13,998	0.09	0.11 (0.06) **	
	Ages 13-19	25,689	0.51	0.27 (0.08) ***	0.15
	Ages 20-30	53,556	0.19	0.07 (0.04) *	-0.04
ROV	No offender identified	57,348	0.28	0.16 (0.04) ***	
	Offender = romantic	14,457	0.18	0.20 (0.09) **	0.04
	Offender = friend	11,388	0.40	0.02 (0.11)	-0.14
	Offender = stranger	10,047	0.09	0.05 (0.06)	-0.10
Female	Male	46,800	0.29	0.13 (0.05) ***	
	Female	46,431	0.23	0.14 (0.05) ***	0.00

Notes: This table provides the month 0 treatment effects for the impact of assault victimisation on youth's participation in all other mental health services excluding community, alcohol and drug, and inpatient services. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The last column in each panel provides the difference in treatment effects between the benchmark category and selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. Note that the sum of the male and female sub-samples does not equal the overall sample because there are some youth victims with missing gender information. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 4.8: Heterogeneity: Youth's anti-anxiety prescriptions

		Count	Baseline	Month 0 treatment effect	Difference in Z's
All	All	99,630	0.53	0.20 (0.04) ***	
Assault severity	Other assaults	73,794	0.51	0.15 (0.04) ***	
	Serious assault, injury	25,836	0.60	0.35 (0.08) ***	0.20 **
Age group	Ages 0-12	15,315	0.00	0.00 (0.01)	
	Ages 13-19	27,594	0.15	0.15 (0.05) ***	0.14 ***
	Ages 20-30	56,721	0.86	0.27 (0.06) ***	0.27 ***
ROV	No offender identified	62,487	0.47	0.17 (0.05) ***	
	Offender = romantic	14,745	0.83	0.56 (0.12) ***	0.39 ***
	Offender = friend	11,874	0.54	0.10 (0.10)	-0.07
	Offender = stranger	10,527	0.45	-0.01 (0.10)	-0.18 *
Female	Male	50,208	0.35	0.14 (0.04) ***	
	Female	49,407	0.72	0.26 (0.06) ***	0.12 *

Notes: This table provides the month 0 treatment effects for the impact of assault victimisation on the likelihood the youth fills out a prescription for anti-anxiety medication. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline].The last column in each panel provides the difference in treatment effects between the benchmark category and selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. Note that the sum of the male and female sub-samples does not equal the overall sample because there are some youth victims with missing gender information. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 4.9: Heterogeneity: Youth's anti-depressants prescriptions

		Count	Baseline	Month 0 treatment effect	Difference in Z's
All	All	99,630	3.03	0.34 (0.08) ***	
Assault severity	Other assaults	73,794	3.01	0.31 (0.09) ***	
	Serious assault, injury	25,836	3.11	0.41 (0.16) ***	0.10
Age group	Ages 0-12	15,315	0.44	-0.04 (0.06)	
	Ages 13-19	27,594	2.42	0.23 (0.13) *	0.27 *
	Ages 20-30	56,721	4.00	0.48 (0.12) ***	0.53 ***
ROV	No offender identified	62,487	2.77	0.28 (0.09) ***	
	Offender = romantic	14,745	3.74	0.91 (0.23) ***	0.64 **
	Offender = friend	11,874	4.07	0.05 (0.24)	-0.23
	Offender = stranger	10,527	2.32	0.17 (0.21)	-0.11
Female	Male	50,208	2.17	0.31 (0.09) ***	
	Female	49,407	3.91	0.36 (0.13) ***	0.06

Notes: This table provides the month 0 treatment effects for the impact of assault victimisation on the likelihood the youth fills out a prescription for anti-depression medication. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The last column in each panel provides the difference in treatment effects between the benchmark category and selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. Note that the sum of the male and female sub-samples does not equal the overall sample because there are some youth victims with missing gender information. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 4.10: Heterogeneity: Youth's anti-psychotics prescriptions

		Count	Baseline	Month 0 treatment effect	Difference in Z's
All	All	99,630	1.58	0.24 (0.05) ***	
Assault severity	Other assaults	73,794	1.60	0.22 (0.06) ***	
	Serious assault, injury	25,836	1.50	0.29 (0.10) ***	0.07
Age group	Ages 0-12	15,315	0.47	0.01 (0.05)	
	Ages 13-19	27,594	1.19	0.15 (0.09) *	0.14
	Ages 20-30	56,721	2.05	0.35 (0.07) ***	0.33 ***
ROV	No offender identified	62,487	1.59	0.28 (0.06) ***	
	Offender = romantic	14,745	1.49	0.39 (0.14) ***	0.11
	Offender = friend	11,874	2.16	0.16 (0.16)	-0.12
	Offender = stranger	10,527	0.92	-0.12 (0.11)	-0.40 ***
Female	Male	50,208	1.60	0.16 (0.07) **	
	Female	49,407	1.56	0.32 (0.07) ***	0.17 *

Notes: This table provides the month 0 treatment effects for the impact of assault victimisation on the likelihood the youth fills out a prescription for anti-psychotics medication. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The last column in each panel provides the difference in treatment effects between the benchmark category and selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. Note that the sum of the male and female sub-samples does not equal the overall sample because there are some youth victims with missing gender information. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.11: Heterogeneity: Youth's employment rate

		Count	Baseline	Average treatment effect	Difference in Z's	%CF
All	All	68,907	47.75	-2.55 (0.18) ***		-5.00
Assault severity	Other assaults	50,031	50.75	-2.67 (0.21) ***		-5.23
	Serious assault, injury	18,876	44.09	-2.27 (0.34) ***	0.40	-4.49
Age group	Ages 0-12	21,384	46.15	-2.53 (0.32) ***		-4.98
	Ages 13-19	22,362	50.91	-2.80 (0.30) ***	-0.27	-5.47
	Ages 20-30	25,161	49.52	-2.18 (0.28) ***	0.35	-4.31
ROV	No offender identified	38,130	47.59	-2.10 (0.24) ***		-4.17
	Offender = romantic	14,274	35.41	-2.37 (0.39) ***	-0.27	-4.67
	Offender = friend	7,413	51.06	-2.68 (0.55) ***	-0.58	-5.26
	Offender = stranger	9,090	74.13	-4.74 (0.43) ***	-2.64 ***	-8.92
Female	Male	33,321	58.11	-3.67 (0.25) ***		-7.05
	Female	35,586	40.36	-1.51 (0.25) ***	2.16 ***	-3.03
Workplace sample	Workplace sample	13,368	76.99	-15.61 (0.43) ***		-17.20
	Different work	11,400	81.83	-15.05 (0.46) ***		-16.68
	Same work	1,968	75.79	-18.82 (1.12) ***	-3.78 ***	-20.03

Notes: This table provides the average treatment effects for the impact of assault victimisation on youth's employment rate. The first row provides the average treatment effect for the overall youth victim sample as estimated by δ_e in Eq.4.3, where the baseline mean is the predicted average outcome over the pre-victimisation months for the treatment group. The workplace sample row provides the baseline and average treatment effect for the sample of youth victims who have linked offenders with workplace information, as estimated by δ_e in Eq.4.3. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.5. Eq.4.5 fully interacts the two-period difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The third column in each panel provides the difference in treatment effects between the heterogeneity group and the selected benchmark. We use Eq.4.2 to express the average treatment effects as a percentage of the youth victim's counterfactual earnings (%CF) in the last column of each panel. Standard errors are shown in parentheses. All counts have been randomly rounded to base 3, so may not add up to the overall total. Note that the sum of the male and female subsamples does not equal the overall sample because there are some youth victims with missing gender information $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 4.12: Heterogeneity: Youth's labour earnings

		Count	Baseline	Average treatment effect	Difference in Z's	%CF
All	All	68,907	1,435	-71.52 (6.03) ***		-4.43
Assault severity	Other assaults	50,031	1,577	-71.62 (6.98) ***		-4.44
	Serious assault, injury	18,876	1,337	-74.47 (10.96) ***	-2.85	-4.60
Age group	Ages 0-12	21,384	1,023	-57.02 (8.84) ***		-3.56
	Ages 13-19	22,362	1,607	-78.32 (10.22) ***	-21.30	-4.83
	Ages 20-30	25,161	1,841	-73.53 (10.60) ***	-16.51	-4.55
ROV	No offender identified	38,130	1,436	-61.30 (8.20) ***		-3.82
	Offender = romantic	14,274	886	-62.89 (10.71) ***	-1.59	-3.92
	Offender = friend	7,413	1,529	-67.17 (18.12) ***	-5.86	-4.17
	Offender = stranger	9,090	2,798	-140.82 (16.59) ***	-79.52 ***	-8.36
Female	Male	33,321	1,984	-104.83 (9.33) ***	-6.36	
	Female	35,586	1,069	-40.21 (7.21) ***	64.63 ***	-2.54
Workplace sample	Workplace sample	13,368	2,295	-354.21 (14.99) ***		-12.86
	Different work	11,400	2,496	-333.21 (16.15) ***		-12.20
	Same work	1,968	2,145	-472.07 (37.34) ***	-138.86 ***	-16.44

Notes: This table provides the average treatment effects for the impact of assault victimisation on youth's labour earnings. The first row provides the average treatment effect for the overall youth victim sample as estimated by δ_e in Eq.4.3, where the baseline mean is the predicted average outcome over the pre-victimisation months for the treatment group. The workplace sample row provides the baseline and average treatment effect for the sample of youth victims who have linked offenders with workplace information, as estimated by δ_e in Eq.4.3. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.5. Eq.4.5 fully interacts the two-period difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The third column in each panel provides the difference in treatment effects between the heterogeneity group and the selected benchmark. We use Eq.4.2 to express the average treatment effects as a percentage of the youth victim's counterfactual earnings (%CF) in the last column of each panel. Standard errors are shown in parentheses. All counts have been randomly rounded to base 3, so may not add up to the overall total. Note that the sum of the male and female subsamples does not equal the overall sample because there are some youth victims with missing gender information $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 4.13: Heterogeneity: Youth's benefit and ACC earnings

		Count	Baseline	Average treatment effect	Difference in Z's	%CF
All	All	68,907	405	17.28 (2.15) ***		3.95
Assault severity	Other assaults	50,031	397	9.06 (2.38) ***	2.03	
	Serious assault, injury	18,876	523	40.29 (4.41) ***	31.23 ***	9.72
Age group	Ages 0-12	21,384	296	9.10 (3.30) ***	2.04	
	Ages 13-19	22,362	457	15.55 (3.58) ***	6.45	3.54
	Ages 20-30	25,161	524	28.26 (3.73) ***	19.16 ***	6.63
ROV	No offender identified	38,130	424	9.03 (2.83) ***		2.03
	Offender = romantic	14,274	699	48.92 (5.42) ***	39.89 ***	12.05
	Offender = friend	7,413	351	10.77 (5.77) *	1.74	2.43
	Offender = stranger	9,090	105	7.44 (3.66) **	-1.59	1.66
Female	Male	33,321	232	10.06 (2.55) ***		2.26
	Female	35,586	618	23.77 (3.27) ***	13.71 ***	5.51
Work-place sample	Workplace sample	13,368	206	66.72 (4.60) ***		36.47
	Different work	11,400	203	63.69 (4.94) ***		34.24
	Same work	1,968	230	83.94 (11.95) ***	20.26	50.64

Notes: This table provides the average treatment effects for the impact of assault victimisation on youth's benefit and ACC earnings. Benefits include emergency benefit, job seekers benefit, sole parent support, supported living payment, young parent payment, youth payment. ACC is earnings from the accident-compensation scheme. The first row provides the average treatment effect for the overall youth victim sample as estimated by δ_e in Eq.4.3, where the baseline mean is the predicted average outcome over the pre-victimisation months for the treatment group. The workplace sample row provides the baseline and average treatment effect for the sample of youth victims who have linked offenders with workplace information, as estimated by δ_e in Eq.4.3. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.5. Eq.4.5 fully interacts the two-period difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The third column in each panel provides the difference in treatment effects between the heterogeneity group and the selected benchmark. We use Eq.4.2 to express the average treatment effects as a percentage of the youth victim's counterfactual earnings (%CF) in the last column of each panel. Standard errors are shown in parentheses. All counts have been randomly rounded to base 3, so may not add up to the overall total. Note that the sum of the male and female subsamples does not equal the overall sample because there are some youth victims with missing gender information $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.14: Heterogeneity: Youth's total income

		Count	Baseline	Average treatment effect	Difference in Z's	%CF
All	All	68,907	1,913	-55.04 (5.54) ***		-2.58
Assault severity	Other assaults	50,031	2,051	-62.86 (6.47) ***		-2.94
	Serious assault, injury	18,876	1,940	-36.24 (9.98) ***	26.62 **	-1.72
Age group	Ages 0-12	21,384	1,390	-49.13 (8.29) ***		-2.31
	Ages 13-19	22,362	2,138	-61.43 (9.39) ***	-12.31	-2.88
	Ages 20-30	25,161	2,451	-47.27 (9.73) ***	1.86	-2.23
ROV	No offender identified	38,130	1,943	-52.26 (7.55) ***		-2.46
	Offender = romantic	14,274	1,655	-22.15 (9.85) **	30.11 **	-1.06
	Offender = friend	7,413	1,956	-56.50 (16.75) ***	-4.24	-2.65
	Offender = stranger	9,090	2,975	-126.09 (15.67) ***	-73.84 ***	-5.73
Female	Male	33,321	2,311	-90.45 (8.68) ***		-4.18
	Female	35,586	1,749	-21.98 (6.60) ***	68.47 ***	-1.05
Workplace sample	Workplace sample	13,368	2,559	-270.48 (13.34) ***		-9.05
	Different work	11,400	2,759	-256.96 (14.35) ***		-8.64
	Same work	1,968	2,430	-345.21 (33.56) ***	-88.25 **	-11.27

Notes: This table provides the average treatment effects for the impact of assault victimisation on youth's total income. Total income includes labour earnings, benefit and ACC earnings, withholding payments, paid parental leave, pension payments, and student allowance. Standard errors are shown in parentheses. The first row provides the average treatment effect for the overall youth victim sample as estimated by δ_e in Eq.4.3, where the baseline mean is the predicted average outcome over the pre-victimisation months for the treatment group. The workplace sample row provides the baseline and average treatment effect for the sample of youth victims who have linked offenders with workplace information, as estimated by δ_e in Eq.4.3. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.5. Eq.4.5 fully interacts the two-period difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The third column in each panel provides the difference in treatment effects between the heterogeneity group and the selected benchmark. We use Eq.4.2 to express the average treatment effects as a percentage of the youth victim's counterfactual earnings (%CF) in the last column of each panel. Standard errors are shown in parentheses. All counts have been randomly rounded to base 3, so may not add up to the overall total. Note that the sum of the male and female subsamples does not equal the overall sample because there are some youth victims with missing gender information $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.15: Heterogeneity: Youth's schooling interventions

		Count	Baseline	Month 0 treatment effect	Difference in Z's
All	All	29,481	2.88	1.17 (0.18) ***	
Assault severity	Other assaults	23,058	2.92	1.07 (0.20) ***	
	Serious assault, injury	6,423	2.71	1.53 (0.38) ***	0.46
Age group	Ages 5-12	10,062	2.55	1.12 (0.30) ***	
	Ages 13-18	19,419	3.05	1.21 (0.22) ***	0.09
ROV	No offender identified	21,690	2.88	1.42 (0.21) ***	
	Offender = romantic	1,182	1.79	0.96 (0.67)	-0.46
	Offender = friend	4,653	3.49	0.33 (0.47)	-1.08 **
	Offender = stranger	1,956	2.02	0.48 (0.54)	-0.93
Female	Male	15,993	3.40	1.21 (0.26) ***	
	Female	13,488	2.26	1.13 (0.24) ***	-0.08
School sample	School sample	3,411	4.29	0.67 (0.59)	
	Different school	1,764	4.57	0.33 (0.87)	
	Same school	1,647	3.99	1.04 (0.81)	0.72

Notes: This table provides the month 0 treatment effects for the impact of assault victimisation on youth's schooling intervention rate. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The schooling sample row provides the month -3 baseline and month 0 treatment effect for the sample of youth victims who have linked offenders with schooling information, as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The last column in each panel provides the difference in treatment effects between the benchmark category and the selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. Note that the sum of the male and female sub-samples does not equal the overall sample because there are some youth victims with missing gender information. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table 4.16: Heterogeneity: Parents' mental health services (Community teams)

		Count	Baseline	Month 0 treatment effect	Difference in Z's
All	All	103,149	0.48	0.17 (0.04) ***	
Assault severity	Other assaults	77,253	0.46	0.19 (0.05) ***	
	Serious assault, injury	25,896	0.52	0.12 (0.09)	-0.07
Age group	Ages 0-12	12,897	0.90	0.68 (0.17) ***	
	Ages 13-19	33,300	0.53	0.13 (0.08) *	-0.55 ***
	Ages 20-30	56,952	0.35	0.09 (0.05) *	-0.59 ***
ROV	No offender identified	60,921	0.57	0.24 (0.06) ***	
	Offender = romantic	17,430	0.38	0.04 (0.09)	-0.20 *
	Offender = friend	13,701	0.46	0.14 (0.11)	-0.09
	Offender = stranger	11,097	0.17	0.09 (0.09)	-0.15
Female	Male	50,232	0.49	0.16 (0.06) ***	
	Female	52,917	0.47	0.18 (0.06) ***	0.02
Parent	Father	47,436	0.37	0.11 (0.06) **	
	Mother	55,719	0.57	0.22 (0.06) ***	0.11

Notes: This table provides the month 0 treatment effects for the impact of youth assault victimisation on parents' participation in community mental health services. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The last column in each panel provides the difference in treatment effects between the benchmark category and the selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.17: Heterogeneity: Parents' mental health services (Alcohol and drug teams)

		Count	Baseline	Month 0 treatment effect	Difference in Z's
All	All	103,149	0.33	0.02 (0.03)	
Assault severity	Other assaults	77,253	0.32	0.02 (0.04)	
	Serious assault, injury	25,896	0.36	0.04 (0.07)	0.02
Age group	Ages 0-12	12,897	0.77	0.26 (0.15) *	
	Ages 13-19	33,300	0.38	-0.03 (0.06)	-0.29 *
	Ages 20-30	56,952	0.21	-0.00 (0.04)	-0.26 *
ROV	No offender identified	60,921	0.38	0.03 (0.05)	
	Offender = romantic	17,430	0.27	-0.02 (0.07)	-0.06
	Offender = friend	13,701	0.37	0.01 (0.10)	-0.02
	Offender = stranger	11,097	0.12	0.07 (0.06)	0.04
Female	Male	50,232	0.34	-0.02 (0.05)	
	Female	52,917	0.32	0.06 (0.05)	0.09
Parent	Father	47,436	0.40	0.00 (0.05)	
	Mother	55,719	0.28	0.04 (0.04)	0.03

Notes: This table provides the month 0 treatment effects for the impact of youth assault victimisation on parents' participation in alcohol and drug related mental health services. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The last column in each panel provides the difference in treatment effects between the benchmark category and the selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 4.18: Heterogeneity: Parents' mental health services (Inpatient teams)

		Count	Baseline	Month 0 treatment effect	Difference in Z's
	All	103,149	0.05	0.01 (0.01)	
Assault severity	Other assaults	77,253	0.05	0.01 (0.02)	
	Serious assault, injury	25,896	0.06	0.01 (0.03)	0.00
Age group	Ages 0-12	12,897	0.08	0.08 (0.06)	
	Ages 13-19	33,300	0.05	0.00 (0.03)	-0.08
	Ages 20-30	56,952	0.04	-0.00 (0.02)	-0.09
ROV	No offender identified	60,921	0.05	0.01 (0.02)	
	Offender = romantic	17,430	0.07	-0.03 (0.03)	-0.05
	Offender = friend	13,701	0.05	0.03 (0.04)	0.02
	Offender = stranger	11,097	0.01	0.04 (0.03)	0.03
Female	Male	50,232	0.05	0.00 (0.02)	
	Female	52,917	0.05	0.01 (0.02)	0.01
Parent	Father	47,436	0.04	0.01 (0.02)	
	Mother	55,719	0.06	0.01 (0.02)	-0.00

Notes: This table provides the month 0 treatment effects for the impact of youth assault victimisation on parents' participation in inpatient mental health services. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The last column in each panel provides the difference in treatment effects between the benchmark category and the selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total.*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table 4.19: Heterogeneity: Parents' mental health services (Other teams)

		Count	Baseline	Month 0 treatment effect	Difference in Z's
	All	103,149	0.07	0.00 (0.02)	
Assault severity	Other assaults	77,253	0.05	0.02 (0.02)	
	Serious assault, injury	25,896	0.12	-0.05 (0.04)	-0.07
Age group	Ages 0-12	12,897	0.10	0.14 (0.07) **	
	Ages 13-19	33,300	0.09	-0.03 (0.03)	-0.16 **
	Ages 20-30	56,952	0.05	-0.02 (0.02)	-0.15 **
ROV	No offender identified	60,921	0.07	0.04 (0.02)	
	Offender = romantic	17,430	0.09	-0.05 (0.04)	-0.09 **
	Offender = friend	13,701	0.08	-0.07 (0.05)	-0.10 **
	Offender = stranger	11,097	0.03	-0.03 (0.03)	-0.06 *
Female	Male	50,232	0.06	0.00 (0.02)	
	Female	52,917	0.08	-0.00 (0.02)	-0.01
Parent	Father	47,436	0.08	-0.02 (0.02)	
	Mother	55,719	0.06	0.02 (0.02)	0.04

Notes: This table provides the month 0 treatment effects for the impact of youth assault victimisation on parents' participation in all other mental health services excluding community, alcohol and drug, and inpatient services. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The last column in each panel provides the difference in treatment effects between the benchmark category and the selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.20: Heterogeneity: Parents' anti-anxiety prescriptions

		Count	Baseline	Month 0 treatment effect	Difference in Z's
	All	109,116	1.31	0.14 (0.04) ***	
Assault severity	Other assaults	81,477	1.33	0.16 (0.05) ***	
	Serious assault, injury	27,639	1.24	0.05 (0.08)	-0.12
Age group	Ages 0-12	14,031	1.50	0.36 (0.13) ***	
	Ages 13-19	35,460	1.30	0.18 (0.08) **	-0.19
	Ages 20-30	59,625	1.26	0.06 (0.05)	-0.30 **
ROV	No offender identified	65,640	1.36	0.19 (0.06) ***	
	Offender = romantic	17,703	1.16	0.02 (0.09)	-0.17
	Offender = friend	14,226	1.37	0.06 (0.11)	-0.13
	Offender = stranger	11,550	1.14	0.11 (0.13)	-0.08
Female	Male	53,436	1.31	0.18 (0.06) ***	
	Female	55,680	1.30	0.10 (0.06) *	-0.08
Parent	Father	50,268	0.87	0.06 (0.05)	
	Mother	58,848	1.67	0.20 (0.07) ***	0.15 *

Notes: This table provides the month 0 treatment effects for the impact of youth assault victimisation on the likelihood the parent fills out a prescription for anti-anxiety medication. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The last column in each panel provides the difference in treatment effects between the benchmark category and the selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.21: Heterogeneity: Parents' anti-depressants prescriptions

		Count	Baseline	Month 0 treatment effect	Difference in Z's
	All	109,116	6.20	0.00 (0.09)	
Assault severity	Other assaults	81,477	6.34	0.02 (0.10)	
	Serious assault, injury	27,639	5.79	-0.05 (0.17)	-0.07
Age group	Ages 0-12	14,031	6.98	-0.15 (0.27)	
	Ages 13-19	35,460	6.34	-0.07 (0.16)	0.07
	Ages 20-30	59,625	5.93	0.09 (0.12)	0.24
ROV	No offender identified	65,640	6.27	0.08 (0.12)	
	Offender = romantic	17,703	5.48	0.17 (0.20)	0.09
	Offender = friend	14,226	6.96	-0.48 (0.26) *	-0.56 **
	Offender = stranger	11,550	5.96	-0.12 (0.26)	-0.20
Female	Male	53,436	6.38	-0.07 (0.13)	
	Female	55,680	6.02	0.07 (0.12)	0.14
Parent	Father	50,268	4.05	0.15 (0.11)	
	Mother	58,848	8.03	-0.12 (0.14)	-0.27

Notes: This table provides the month 0 treatment effects for the impact of youth assault victimisation on the likelihood the parent fills out a prescription for anti-depression medication. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The last column in each panel provides the difference in treatment effects between the benchmark category and the selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.22: Heterogeneity: Parents' anti-psychotics prescriptions

		Count	Baseline	Month 0 treatment effect	Difference in Z's
	All	109,116	2.16	0.07 (0.04)	
Assault severity	Other assaults	81,477	2.15	0.07 (0.05)	
	Serious assault, injury	27,639	2.19	0.06 (0.08)	-0.01
Age group	Ages 0-12	14,031	2.72	0.22 (0.15)	
	Ages 13-19	35,460	2.14	-0.00 (0.08)	-0.22
	Ages 20-30	59,625	2.04	0.07 (0.05)	-0.15
ROV	No offender identified	65,640	2.30	0.01 (0.06)	
	Offender = romantic	17,703	2.17	0.14 (0.09)	0.13
	Offender = friend	14,226	2.07	0.26 (0.12) **	0.25 *
	Offender = stranger	11,550	1.51	0.03 (0.11)	0.02
Female	Male	53,436	2.07	0.02 (0.06)	
	Female	55,680	2.25	0.11 (0.06) *	0.09
Parent	Father	50,268	1.72	0.10 (0.06) *	
	Mother	58,848	2.54	0.04 (0.06)	-0.05

Notes: This table provides the month 0 treatment effects for the impact of youth assault victimisation on the likelihood the parent fills out a prescription for anti-psychotics medication. The first row provides the month -3 baseline and month 0 treatment effect for the overall sample as estimated by δ_e in Eq.4.1. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.4. Eq.4.4 fully interacts the difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The last column in each panel provides the difference in treatment effects between the benchmark category and the selected heterogeneity group. All counts have been randomly rounded to base 3, so may not add up to the overall total. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table 4.23: Heterogeneity: Parents' employment rate

		Count	Baseline	Average treatment effect	Difference in Z's	%CF
All	All	113,136	48.58	-0.32 (0.10) ***		-0.66
Assault severity	Other assaults	84,333	49.23	-0.19 (0.12)		-0.40
	Serious assault, injury	28,803	46.77	-0.71 (0.20) ***	-0.52 **	-1.44
Age group	Ages 0-12	14,721	45.71	-0.44 (0.31)		-0.89
	Ages 13-19	37,008	50.56	-0.27 (0.18)	0.17	-0.55
	Ages 20-30	61,407	48.11	-0.24 (0.13) *	0.20	-0.50
ROV	No offender identified	68,847	47.77	-0.39 (0.13) ***		-0.79
	Offender = romantic	17,853	45.28	-0.32 (0.24)	0.07	-0.65
	Offender = friend	14,526	49.66	-0.23 (0.27)	0.16	-0.47
	Offender = stranger	11,913	57.12	-0.10 (0.27)	0.28	-0.21
Female	Male	55,614	50.04	-0.27 (0.14) *		-0.56
	Female	57,522	47.21	-0.38 (0.14) ***	-0.10	-0.77
Parent	Father	52,179	48.93	-0.25 (0.14) *		-0.50
	Mother	60,957	48.32	-0.39 (0.14) ***	-0.15	-0.80

Notes: This table provides the average treatment effects for the impact of youth assault victimisation on parents' employment rate. The first row provides the average treatment effect for the overall youth victim sample as estimated by δ_e in Eq.4.3, where the baseline mean is the predicted average outcome over the pre-victimisation months for the treatment group. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.5. Eq.4.5 fully interacts the two-period difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The third column in each panel provides the difference in treatment effects between the heterogeneity group and the selected benchmark. We use Eq.4.2 to express the average treatment effects as a percentage of the youth victim's counterfactual earnings (%CF) in the last column of each panel. Standard errors are shown in parentheses. All counts have been randomly rounded to base 3, so may not add up to the overall total. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.24: Heterogeneity: Parents' labour earnings

		Count	Baseline	Average treatment effect	Difference in Z's	%CF
All	All	113,136	2256.13	-14.20 (5.24) ***		-0.61
Assault severity	Other assaults	84,333	2322.88	-7.18 (5.91)		-0.31
	Serious assault, injury	28,803	2183.90	-35.51 (9.20) ***	-28.33 ***	-1.52
Age group	Ages 0-12	14,721	2040.55	-21.30 (13.82)		-0.92
	Ages 13-19	37,008	2404.85	-8.77 (8.63)	12.53	-0.38
	Ages 20-30	61,407	2275.99	-11.08 (6.44) *	10.22	-0.48
ROV	No offender identified	68,847	2251.05	-17.82 (6.62) ***		-0.77
	Offender = romantic	17,853	1931.10	-29.88 (10.32) ***	-12.06	-1.28
	Offender = friend	14,526	2232.38	4.65 (12.44)	22.47	0.20
	Offender = stranger	11,913	3102.17	2.49 (15.58)	20.31	0.11
Female	Male	55,614	2424.81	-10.48 (7.19)		-0.45
	Female	57,522	2155.09	-18.24 (6.71) ***	-7.76	-0.79
Parent	Father	52,179	2806.63	-15.73 (8.27) *		-0.68
	Mother	60,957	1843.31	-12.95 (5.56) **	2.78	-0.56

Notes: This table provides the average treatment effects for the impact of youth assault victimisation on parents' labour earnings. The first row provides the average treatment effect for the overall youth victim sample as estimated by δ_e in Eq.4.3, where the baseline mean is the predicted average outcome over the pre-victimisation months for the treatment group. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.5. Eq.4.5 fully interacts the two-period difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The third column in each panel provides the difference in treatment effects between the heterogeneity group and the selected benchmark. We use Eq.4.2 to express the average treatment effects as a percentage of the youth victim's counterfactual earnings (%CF) in the last column of each panel. Standard errors are shown in parentheses. All counts have been randomly rounded to base 3, so may not add up to the overall total. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.25: Heterogeneity: Parents' benefit and ACC earnings

		Count	Baseline	Average treatment effect	Difference in Z's	%CF
All	All	113,136	341.44	3.46 (1.48) **		0.99
Assault severity	Other assaults	84,333	341.66	2.11 (1.67)		0.60
	Serious assault, injury	28,803	371.50	7.57 (2.85) ***	5.46 *	2.19
Age group	Ages 0-12	14,721	528.14	12.10 (4.66) ***		3.54
	Ages 13-19	37,008	380.82	6.67 (2.57) ***	-5.43	1.92
	Ages 20-30	61,407	287.55	-0.11 (1.76)	-12.21 **	-0.03
ROV	No offender identified	68,847	386.48	6.06 (1.95) ***		1.74
	Offender = romantic	17,853	333.26	6.65 (3.20) **	0.59	1.91
	Offender = friend	14,526	331.07	-1.62 (3.86)	-7.68 *	-0.46
	Offender = stranger	11,913	181.27	-7.68 (3.40) **	-13.74 ***	-2.12
Female	Male	55,614	330.10	3.58 (2.03) *		1.02
	Female	57,522	367.72	3.45 (2.01) *	-0.12	0.99
Parent	Father	52,179	274.39	-0.73 (2.21)		-0.21
	Mother	60,957	413.31	7.05 (1.83) ***	7.78 ***	2.03

Notes: This table provides the average treatment effects for the impact of youth assault victimisation on parents' benefit and ACC earnings. Benefits include emergency benefit, job seekers benefit, sole parent support, supported living payment, young parent payment, youth payment. ACC is earnings from the accident-compensation scheme. The first row provides the average treatment effect for the overall youth victim sample as estimated by δ_e in Eq.4.3, where the baseline mean is the predicted average outcome over the pre-victimisation months for the treatment group. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.5. Eq.4.5 fully interacts the two-period difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The third column in each panel provides the difference in treatment effects between the heterogeneity group and the selected benchmark. We use Eq.4.2 to express the average treatment effects as a percentage of the youth victim's counterfactual earnings (%CF) in the last column of each panel. Standard errors are shown in parentheses. All counts have been randomly rounded to base 3, so may not add up to the overall total. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

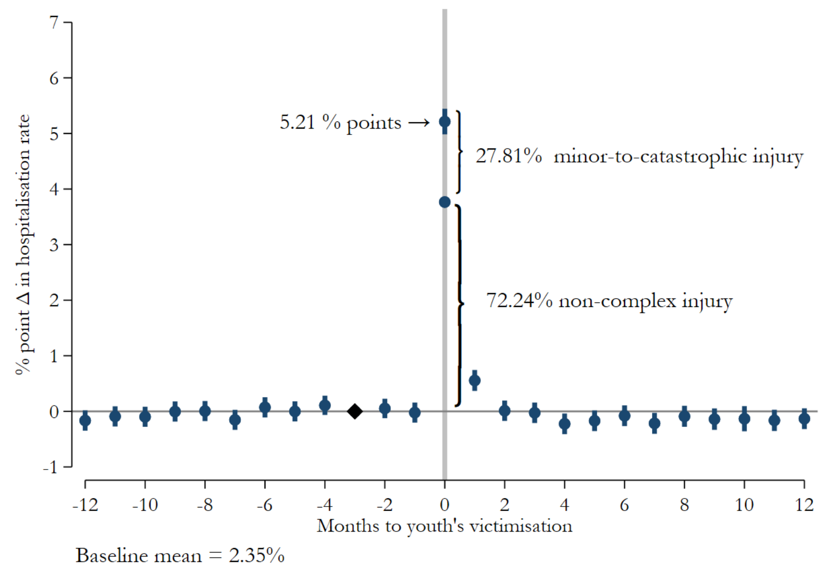
Table 4.26: Heterogeneity: Parents' total income

		Count	Baseline	Average treatment effect	Difference in Z's	%CF
All	All	113,136	2682.79	-9.05 (5.03) *		-0.33
Assault severity	Other assaults	84,333	2756.19	-3.71 (5.67)		-0.14
	Serious assault, injury	28,803	2635.07	-25.38 (8.96) ***	-21.66 **	-0.92
Age group	Ages 0-12	14,721	2674.38	-1.55 (13.00)		-0.06
	Ages 13-19	37,008	2883.59	2.87 (8.34)	4.42	0.10
	Ages 20-30	61,407	2642.46	-13.04 (6.25) **	-11.49	-0.47
ROV	No offender identified	68,847	2731.50	-7.77 (6.35)		-0.28
	Offender = romantic	17,853	2326.24	-23.70 (9.69) **	-15.93	-0.86
	Offender = friend	14,526	2648.10	1.56 (12.07)	9.33	0.06
	Offender = stranger	11,913	3386.25	-9.77 (15.59)	-2.00	-0.36
Female	Male	55,614	2846.45	-4.05 (6.95)		-0.15
	Female	57,522	2608.58	-14.20 (6.44) **	-10.15	-0.52
Parent	Father	52,179	3192.90	-12.57 (8.07)		-0.46
	Mother	60,957	2325.30	-6.13 (5.26)	6.44	-0.22

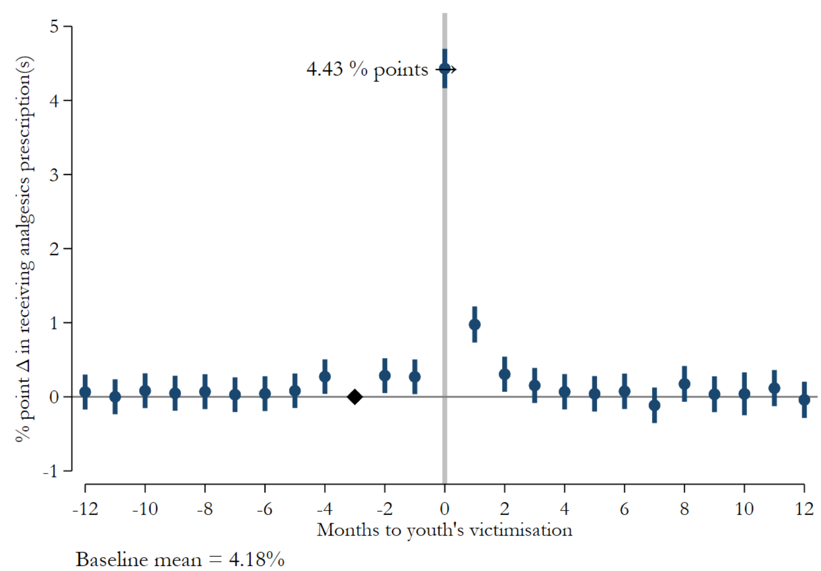
Notes: This table provides the average treatment effects for the impact of youth assault victimisation on parents' total income. Total income includes labour earnings, benefit and ACC earnings, withholding payments, paid parental leave, pension payments, and student allowance. Standard errors are shown in parentheses. The first row provides the average treatment effect for the overall youth victim sample as estimated by δ_e in Eq.4.3, where the baseline mean is the predicted average outcome over the pre-victimisation months for the treatment group. The remaining rows probe effect heterogeneity across different groups as estimated by Eq.4.5. Eq.4.5 fully interacts the two-period difference-in-differences model with the heterogeneity variable, such that a weighted average of the treatment effects[baseline] within each heterogeneity category gives the overall treatment effect[baseline]. The third column in each panel provides the difference in treatment effects between the heterogeneity group and the selected benchmark. We use Eq.4.2 to express the average treatment effects as a percentage of the youth victim's counterfactual earnings (%CF) in the last column of each panel. Standard errors are shown in parentheses. All counts have been randomly rounded to base 3, so may not add up to the overall total. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

4.12 Figures

Figure 4.1: The effects of assault victimisation on youth victims' physical health



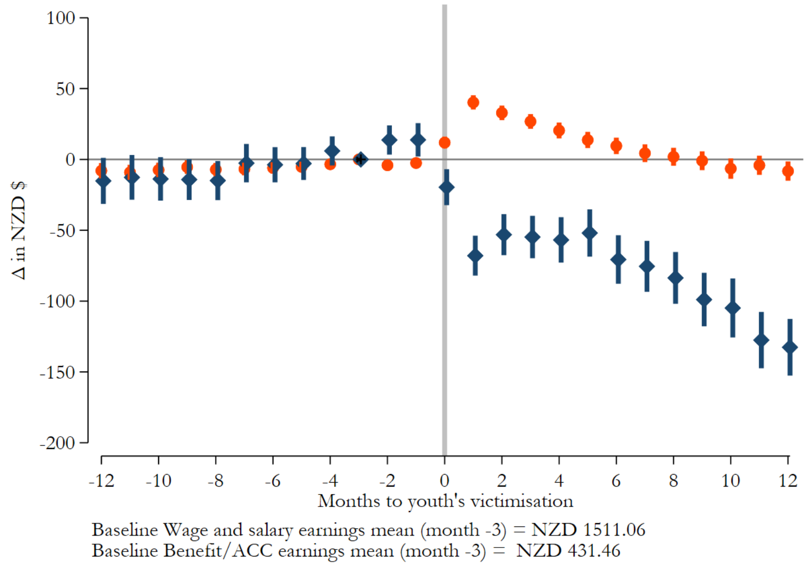
(a) Probability of hospitalisation



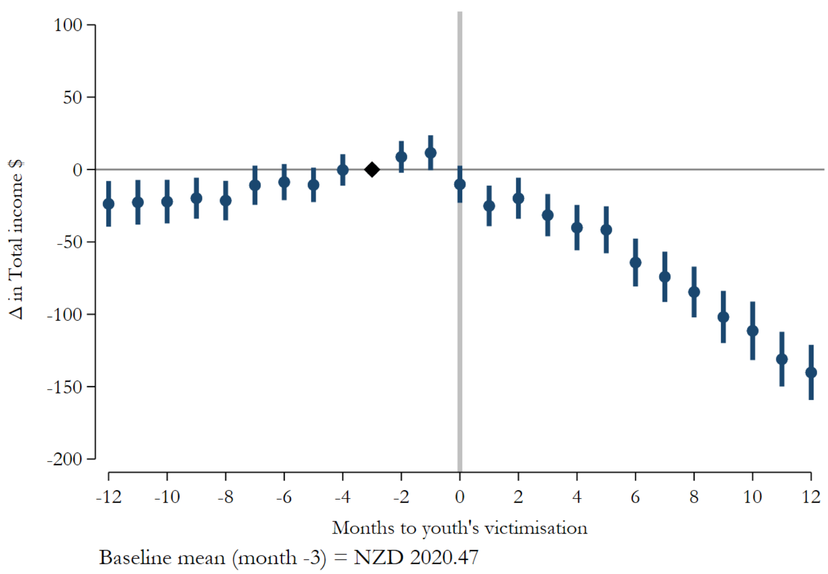
(b) Probability of receiving pain-relief prescriptions

Notes: This figures estimates the effect of assault victimisation on youth's physical health. Panel 4.1a shows the effect on hospitalisation and Panel 4.1b shows the effect on pain-relief medication prescriptions. Each series show the percentage-point impact of youth victimisation on the outcome at each event time (δ_e defined in Eq.4.1). We normalise the comparison group's outcome to the outcome level of the treatment group three months prior to victimisation. The circles are the point estimates, and the vertical bars depict 95% confidence intervals. Standard errors clustered at the youth-level.

Figure 4.2: Youth victim's labour market response to assault victimisation



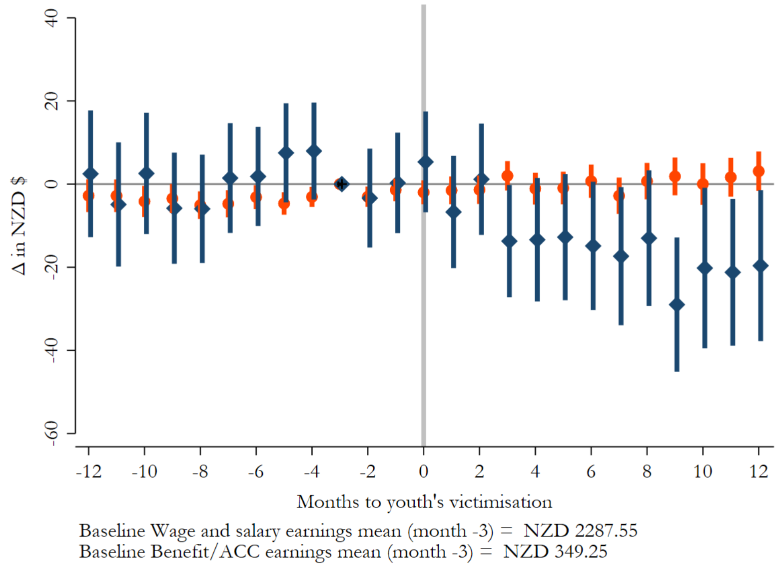
(a) Labour earnings and benefit/ACC earnings



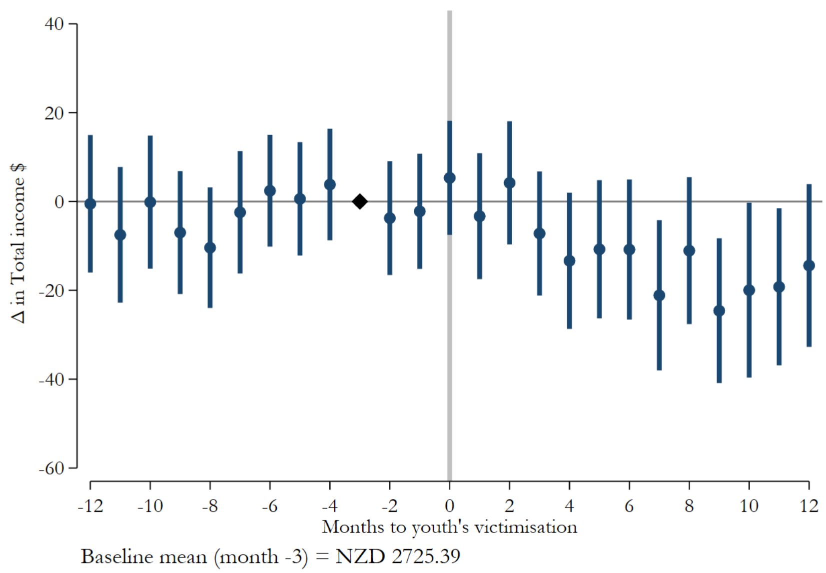
(b) Total income

Notes: This figure shows the main estimates of the effect of assault victimisation on youth's labour market outcomes. Panel 4.2a shows the effect on labour earnings (blue diamonds) and benefit and ACC earnings (red circles) and Panel 4.2b shows the effect on total income (the sum of labour earnings, benefit and ACC earnings, withholding payments, paid parental leave, pension payments, and student allowance). Each series show the point estimates (NZD) for the impact of youth victimisation on the outcome at each event time, i.e. δ_e's defined in Eq.4.1. We normalise the comparison group's outcome to the outcome level of the treatment group three months prior to victimisation. The vertical bars depict 95% confidence intervals with standard errors clustered at the youth-level.

Figure 4.3: Parents' labour market response to youth assault victimisation



(a) Labour earnings and benefit/ACC earnings



(b) Total income

Notes: This figure shows the main estimates of the effect of youth assault victimisation on parents' labour market outcomes. Panel 4.3a shows the effect on labour earnings (blue diamonds) and benefit and ACC earnings (red circles) and Panel 4.3b shows the effect on total income (the sum of labour earnings, benefit and ACC earnings, withholding payments, paid parental leave, pension payments, and student allowance). Each series show the point estimates (NZD) for the impact of youth victimisation on the parent-level outcome at each event time, i.e. δ_e's defined in Eq.4.1. We normalise the comparison group's outcome to the outcome level of the treatment group three months prior to victimisation. The circles are the point estimates, and the vertical bars depict 95% confidence intervals with standard errors clustered at the youth-level.

Chapter 5

The household economic cost of parental assaults against children

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5.1 Prelude

In the previous chapter, *The effect of violent assaults on youth victims and their parents*, I explicitly excluded cases of family violence, where the youth was assaulted by a family member. This was due to concerns about pre-trends in outcomes, given the family violence literature suggests there is a different reporting threshold for family violence than violence from outside the household. Initially, I intended to revisit this topic as a short paper to complete the thesis, similar in scope to the Economics Letters article on the economic impacts of youth suicide in Chapter 2.

As we began analysing the immediate economic impacts of family violence victimisation, it became clear that this project required more than a letters-type article. The high-frequency nature of our monthly earnings variable, together with the ability to separate offending and non-offending parents, revealed earnings trends that differed from existing literature. Specifically, unlike prior studies using annual data, we uncovered evidence of an Ashenfelter-type dip in offending parents' earnings in the months leading up to the child's assault. These findings prompted us to dedicate the final months of my PhD to refining the empirical design and interpreting the treatment effects. In my view, the high-frequency nature of payroll data enabled me to detect the pre-earnings dip hypothesised in earlier literature but previously undetected due to the limitations of annual data. This opens avenues for further exploration, including establishing potential causal links between income shocks and family violence.

5.2 Abstract

Child-directed assaults by parents inflict lasting economic consequences on both offending and non-offending parents, reshaping labour and benefit income in unexpected ways. Leveraging high-frequency linked administrative data from New Zealand and a difference-in-differences approach, we provide new insights into the family violence literature, focusing on parental labour market dynamics in the months surrounding the assault. We document a significant decline in labour earnings for offending parents commencing several months before the assault, suggesting financial strain could play a role in the onset of family violence. Following the assault, non-offending parents experience the largest labour earnings loss—about 60% of the total—but this is partially offset by increased benefit income, primarily through sole-parent support, leaving them with 40% of the total income loss. These findings provide new evidence on the economic toll of family violence, revealing distinct patterns of income adjustment and highlighting the role of public insurance in mitigating the financial burden.

5.3 Introduction

The most likely perpetrator of violent crimes against children is a parent (Child Matters, 2019). Following an assault on a child by one parent, both parents may experience significant economic repercussions. The offending parent may face formal penalties or social consequences that impact employment, while the non-offending parent may experience labour disruptions as they support their child, address emotional impacts, or seek alternative housing.

Previous research on family violence primarily addresses partner violence, with documented health impacts on victimised parents and spillover effects on children and foetuses (for example, Stubbs and Szoeke (2021), Bhuller et al. (2024), Currie et al. (2022)). However, parental responses to assaults on children remain under-explored, for at least two reasons: child victims are often too young to fill out crime surveys, and meaningful analysis requires linking the child to both parents and identifying their relationship to the offender.

Our study overcomes these challenges by using unique register data from New Zealand (NZ), a country with one of the highest rates of child abuse amongst OECD countries (Child Matters, 2019; UNICEF, 2023). A key feature of NZ's police victimisation register is that it specifies the relationship between victim and offender, enabling us to identify children assaulted by a parent and to distinguish between offending and non-offending parents through linked birth records.

Our research question asks: *What are the short-term parental labour market responses to assaults against their children?* Focusing on police-reported physical assaults by parents against NZ-born children ages 0-12, we track parents' labour, benefit, and total income through tax records before and after the assault.

To estimate the labour market outcomes for parents had their child not been assaulted, we apply a difference-in-differences model, following Fadlon and Nielsen (2019, 2021), Mertz et al. (2023), and Bhuller et al. (2024). By comparing families experiencing victimisation today with families experiencing victimisation 25 months later, we isolate the causal effects of reported child assaults on parents' labour market outcomes, under the identifying assumption that the outcomes of these two groups would have followed the same trajectory in the absence of the assault.

Our analysis reveals interesting patterns of parental labour market behaviour in the months surrounding their child's assault. For offending parents, we observe a notable drop in labour earnings beginning months before to the assault, suggesting that financial difficulties may contribute to the risk of family violence. Non-offending parents, on the other hand, experience large labour earnings losses after their child has been assaulted, bearing about 60% of the total earnings loss. This reduction in labour supply may stem directly from the non-offending parents' emotional dress as they come to terms with the trauma their child has experienced, or indirectly through the need to take time off work to provide care and support for the child. Importantly, both mechanisms may operate in tandem.[1]

Due to differing benefit adjustments, offending parents bear 60% of the total income loss from child assaults, corresponding to an average reduction of 4-5% of counterfactual income. This loss is largely driven by offending parents who were subsequently imprisoned, making them ineligible for benefit payments. In contrast, benefit income for non-offending parents increases after the child assault, primarily through sole-parent support. This increase in benefits may overstate the non-offending parent's financial position, as it likely reflects their new role as the primary provider, thereby reducing their equivalised income to support the household.

[1]Exploring parents' mental health responses to parent-to-child assaults is currently beyond the scope of this paper. However, we know from Holt et al. (2014) that even without direct involvement, parents experience high levels of anger, anxiety, guilt, and distress when their child experiences trauma, especially in the context of family violence.

5.4 Data

We use Statistics NZ's population wide linked administrative data from 2014 to 2023.

Reporting a victimisation. When the NZ police become aware of an alleged crime, they attend the incident and open a victim's report. This report is populated with details about the offence and the victim. A unique feature of the NZ police reporting procedure is that the police officer documents the relationship between the victim and offender when the crime occurred. They will work with the victim and any witnesses to determine whether the offender was, for example, a parent, grandparent, sibling, friend, stranger, or whether no offender was identified. These victims' reports are available to researchers from 2014 onward.

NZ police victim register. We identify all child (ages 0-12) assault victims whose offence was reported to the police and where the alleged offender was determined to be a parent (N=5,598). We restrict the sample to NZ-born children (N= 2,895) so parent-links can be identified.[2]

Birth records. We link victims to their biological parents using the NZ birth register and keep parents who were of working age (ages 30-60) when their child was assaulted. If more than one of the parent's children have been assaulted, we focus on the first reported incident. We append the mother-child and father-child pairs to create our parent sample (N=4,803).[3]

NZ police offender register. Corresponding to the NZ police victims register is an offender register that contains all incidents where police proceeded against an alleged

[2] We likewise use this victimisation register to identify whether a parent was also (reported) victimised at the same time as their child.

[3] The parent sample is less than double the child sample because only 85% of the child victims have two parents listed on their birth certificate that were both working-age at the time of the assault.

offender. The victim and offender registers can be linked through a common occurrence ID (akin to an assault ID). We link our parent sample to both the police offender and the police victims registers, enabling us to distinguish non-offending (N=2,499) from offending parents (N=2,304).[4]

Tax register. We link parents to the NZ tax register to obtain information on taxable income, including labour earnings and benefit income. Benefit income is the IRD's composite measure of all taxable 'main' benefits, including the emergency benefit, job seeker support, sole parent support, supported living payments, young parent payment, and youth payment. Importantly, even when household conditions determine benefit eligibility and payout, benefit income is recorded at the individual level.

Health registers. We create two physical health indicators to provide insights on how the parent-assault affected the child's health stock. Using a public hospital discharges register, we create a hospitalisation indicator equal to one if the child visited a hospital in a given month. Using a pharmaceutical register, we define a pain-relief indicator equal to one if the child had a prescription filled out for analgesics in a given month. Appendix Table D0.1 shows that child victims face increased rates of hospitalisation and pain-relief prescriptions after parental assaults, underscoring the physical impacts of these incidents.

Appendix Table D0.2 compares the demographic and socioeconomic composition of parents whose child was assaulted with the general population of parents, revealing that

[4]Our sample of child victims are identified from the police-victimisation reports for cases where the police officer lists the relationship between the child victim and their offender to be a "parent". To identify the offending parent, we use the police-offender register. Thus, if the child was reported to have been assaulted by a parent, but the police did not proceed against the parent, then the parent(s) would be classified as non-offending. About 7% of the child victim sample do not have an offending parent linked, while 14% have two offending parents. We test the sensitivity of our results by repeating analyses on the sample of children who have two linked parents (N=3,783) of opposing offender status (N=3,216), giving 1,608 offending and 1,608 non-offending parents. Results remain materially unchanged.

parent-to-child assaults are more prevalent amongst parents of lower socioeconomic backgrounds.

5.5 Empirical Strategy

Following the approach of Fadlon and Nielsen (2019, 2021), Mertz et al. (2023), and Bhuller et al. (2024), we estimate the counterfactual outcome for affected parents using the earnings evolution of parents whose child was assaulted at a later time as the comparison group. Consistent with their approach, we define event time as the number of months since victimisation such that month 0 is when the (reported) parent-to-child assault occurred.[5]

However, the family violence literature suggests that victimisation often escalates prior to reporting, implying that initial reports may not reflect the first occurrence of abuse. Bhuller et al. (2024) addresses this issue but, using annual data, found little or no evidence of differential pre-trends in the year leading up to victimisation reports. Similarly, in the context of reported workplace violence, Adams-Prassl et al. (2022) note that their annual data do not reveal a drop in income in the period prior to the violent incident.

In contrast, the higher-frequency nature of our monthly earnings data allows for closer inspection of the crucial period leading up to first reporting. As shown in Figure 5.1, we observe an Ashenfelter-type dip[6] in the treatment group's earnings, particularly during months -4 to -1. Further inspection of the raw event time averages by parents' offending

[5]The NZ police victimisation data differentiate between the reported date and the earliest date for which that offence occurred. However, we do not know when the overall abuse started. In 90% of cases, the first-occurrence date occurs within the same month as the reported date. We use the first-occurrence date in our estimations; however, for simplicity, we refer to month 0 as the reported assault start date, acknowledging that this may not represent the actual onset of abuse.

[6]Following Adams-Prassl et al. (2022), we use the terminology of an 'Ashenfelter-type dip' when referring to the drop in earnings in the months leading up to the assault.

status indicates that the Ashenfelter-type earnings dip is driven primarily by the offending parents (Appendix Figure D0.1).

This pre-assault earnings dip provides compelling graphical evidence of potential reverse causality for offending parents. For instance, an offending parent losing their job in month -4 may experience financial stress that precipitates an assault against their child, rather than the assault causing the parent to lose their job.[7]

The presence of this pre-event dip complicates the selection of suitable comparison groups for estimating 1-year effects. Parents treated 13 months in the future, for example, will only have eight 'unaffected' months for comparison before the pre-assault earnings decline begins to influence their outcomes. Furthermore, this decline highlights the importance of the pre-event baseline month, as its selection will significantly affect the magnitude of the estimated treatment effect.

Figure 5.1 also plots the labour earnings profile of a comparison group treated 25 months in the future, where the level-difference between the treatment and comparison groups are benchmarked at month -7. The vertical dashed line at month 0 separates the pre- and post-assault periods. We estimate the causal effects across months 1-12 (grey area) to give the one-year labour market response to parent-to-child assaults. Visually it appears that the comparison group's labour earnings trajectory provides suitable counterfactual profile for the treatment group, excluding the months affected by the Ashenfelter-type dip.[8]

[7]Appendix Figure D0.2 explores this idea by plotting job exit rates for offending and non-offending parents. For each parent, we define a job-exit indicator equal to one if the parent was employed in the previous month but not employed in the current month, and equal to zero if the parent was employed in the previous month and remained employed in the current month. There is no clear evidence that the drop in labour earnings observed for offending parents over event months -4 to -1 is linked to job exits, suggesting that the pre-event earnings dip is likely an intensive margin response.

[8]The period following the second dashed vertical line is excluded from our analysis since this includes

We focus our attention on the first 12 months immediately following the reported victimisation. Our estimating equation is the following dynamic difference-in-differences model (Eq.5.1).

$$\begin{aligned} Y_{i,t} = \alpha + \beta \cdot treat_i + \sum_{e \neq -7, e=-12}^{12} \varphi_e \cdot M_e \\ + \sum_{e \neq -7, e=-12}^{12} \delta_e \cdot M_e \cdot treat_i + \gamma \cdot X_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{5.1}$$

where $Y_{i,t}$ is labour earnings for parent i in relative month t; α is constant; $treat_i$ is a treatment indicator; M_e are event time indicators ranging from 12 months before to 12 months after the (reported) assault occurred, with the baseline measured at month -7; and $X_{i,t}$ includes birth year and calendar time fixed effects. Standard errors are clustered at the child-victim level.

The δ_e's identify the causal effect of the child's (reported) assault on parents' labour earnings under the identifying assumption that, had their child not been a victim, the earnings growth of the treated parents, on average, would have continued on the same trajectory as that of the comparison group. We provide some empirical support for this assumption by showing our estimates of δ_e are around zero for $e < -4$, excluding the period for which the Ashenfelter-type dip occurs.[9]

To obtain the annualised effect size, we sum the monthly effects from $\sum_{e=1}^{12} \delta_e$, capturing the cumulative impact of the assault on parents' earnings over the first year following the incident.

months where the comparison group becomes compromised by their own pre-event earnings dip, making them an inappropriate comparison group for estimating effects in the second year.

[9]This assumption is better supported when estimating effects for non-offending parents, since they do not exhibit the pre-event earnings dip like the offending parents do.

To translate the NZD effects into relative terms, we report estimates of δ_e in percent of the counterfactual earnings level of the treatment group, calculated as:

$$P_e = \frac{\sum_{e=1}^{12} \delta_e}{\sum_{e=1}^{12} E[\hat{Y}_{i,t} \mid e, \text{treat}_i = 1]} \tag{5.2}$$

Where $\hat{Y}_{i,t} = \hat{\alpha} + \hat{\beta} + \sum_{e \neq -7, e=-12}^{12} \hat{\varphi}_e \cdot M_e + \hat{\gamma} \cdot X_{i,t}$ is the predicted counterfactual earnings for parent i in the treatment group at event time e.

For offending parents, the estimate likely reflects a composite of pre-existing financial strain and the additional consequences of the assault itself. In contrast, for non-offending parents, the estimated effects predominantly capture the immediate and ongoing economic disruptions resulting from their child's victimisation.

5.6 Results

Our analysis reveals that following an assault on their child, offending parents experience the larger income loss. However, this asymmetric burden arises from an unexpected pattern where labour earnings losses are skewed towards non-offending parents, and it is benefit adjustments that ultimately cause the overall income gap.

Figure 5.2 presents the δ_e estimates from Eq.5.1 with 95% confidence intervals, illustrating parents' labour earnings responses and benefit changes from 12 months before to 12 months after the assault (month 0). Two notable patterns emerge: first, an Ashenfelter-type dip in labour earnings appears in the months leading up to the reported assault, consistent with the raw event-time averages in Figure 5.1.[10] Second, following

[10]Recall that Appendix Figure D0.1 showed this pre-event dip is driven entirely by the offending parents.

the assault, labour earnings stabilise at a reduced level, approximately NZD 100 lower per month relative to their counterfactual earnings, with no signs of recovery over the 12-month period. This sustained reduction results in a cumulative annual loss of NZD 1,265.

Figure 5.3 breaks down the income effects by parents' offending status. We interact Eq.5.1 with an indicator for offending versus non-offending parents and plot the annualised estimates of average labour earnings, benefit income, and total income effects with 95% confidence intervals. The first row presents the full sample of parents, showing an annualised income loss of NZD 1,267, driven entirely by reduced labour earnings.

The second row focuses on offending parents, showing a total income loss of NZD 1,926, composed of reductions in both labour earnings (NZD 1,175) and benefit income (NZD 560). As foreshadowed, these effects likely reflect a combination of pre-existing financial pressure and the child assault itself.

This contrasts with non-offending parents in the third row of Figure 5.3, who experience a smaller total income loss (NZD 660), as their labour earnings losses (NZD 1,310) are partially offset by benefit gains (NZD 730), primarily from sole parent support and unemployment benefits.[11] Since there is no evidence of a pre-event earnings dip for non-offending parents, we are confident in interpreting these estimates as capturing the economic consequences of their child's assault.

Together, these results indicate that offending parents bear approximately 75% of the total income burden and 47% of the labour earnings loss following an assault. However, these shares are somewhat sensitive to the choice of estimation baseline, due in part to

[11]The sole parent support benefit is designed to help single parents find part-time work or prepare for future employment and generally has lower job-seeking requirements than the standard job-seeker unemployment benefit.

the Ashenfelter dip in labour earnings observed for offending parents. Across baseline specifications, Appendix Figure D0.3 shows that offending parents consistently bear, on average, two-thirds of the total income loss from child assaults. The share of lost labour earnings for offending parents remains relatively stable around 40%, except for the final months leading up to the assault where it approches zero.

We extend our analysis by categorising parents based on the offending parent's imprisonment status and the non-offending parent's victimisation status, with detailed results in Figure 5.4.

Offending parents who receive imprisonment sentences (about 5%, see Appendix D1) experience substantial reductions in both labour earnings and benefit income.[12] The loss in benefit income, like the loss in labour earnings due to incapacitation, is unsurprising, as NZ public policy stipulates that imprisonment also leaves these parents ineligible for benefit payments (Social Security Act 2018, s 217). Together, these reductions lead to total income losses amounting to roughly 50% of their counterfactual income, or about half of what they otherwise would have had.

Non-incarcerated offending parents experience more modest, though still substantial, declines in both labour earnings and benefit income, resulting in a total income reduction of about 5% of their counterfactual income. For these parents, the reduction in benefit income likely reflects a change in their caregiving or household status rather than ineligibility. If the assault led to separation, the offending parent's benefits may no longer reflect a co-habiting state, thus reducing their support levels.[13]

[12]The risk of imprisonment is 150% higher for offending parents whose child required hospitalisation in the month of the assault.

[13]For example, the weekly benefit rate is lower for sole parents compared to co-residing couples with children (see here for more details).

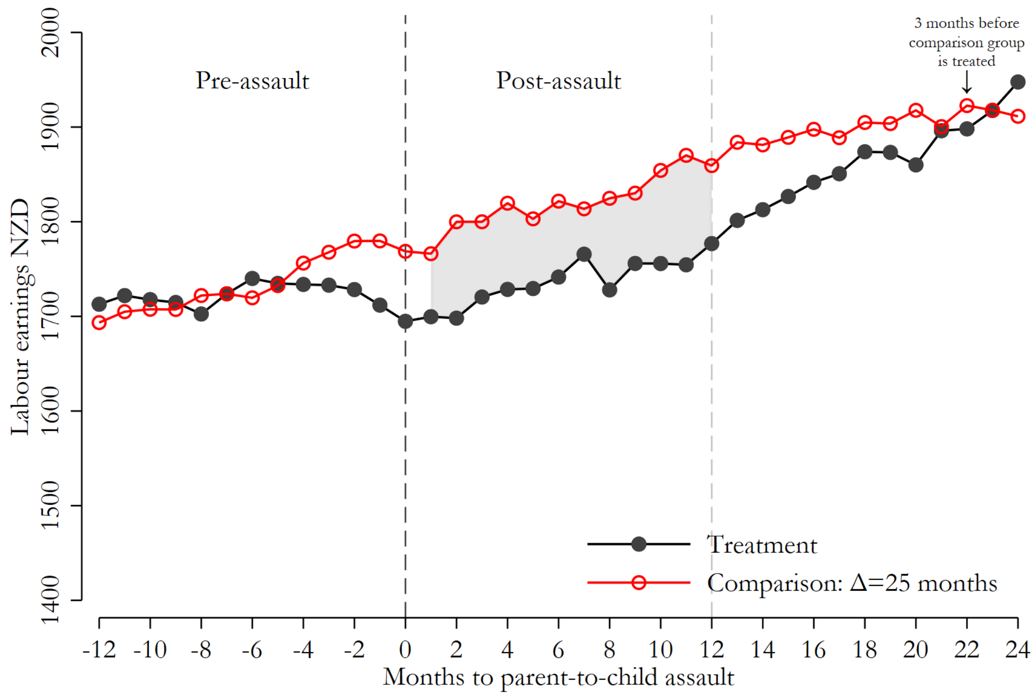
Among non-offending parents, benefit increases primarily reflect a shift toward sole caregiving responsibilities, as shown by a marked rise in sole parent support payments. For non-offending parents who were not themselves directly victimised (about 90%), the persistence of labour and income losses suggests that their income adjustments are primarily a response to their child's victimisation and resulting shifts in household structure, rather than any direct impact on themselves.

5.7 Conclusion

Our findings highlight the substantial economic fallout of family violence for parents following an assault on their child, with offending parents absorbing about two-thirds of the total income loss due to reductions across both labour earnings and benefits. The presence of an Ashenfelter-type dip in offending parents' labour earnings in the months preceding the assault indicates that these effects reflect a combination of pre-existing financial strain and the economic consequences of the child assault itself. Non-offending parents, meanwhile, experience a larger reduction in labour earnings, partially offset by increased benefit income, primarily through sole-parent support — suggesting a shift to sole caregiving with a lower equivalised income. Both parents experience a notable shift in income composition, with benefits constituting a greater share, driven by adjustments reflecting changes in caregiving roles and household structure. These results underscore the immediate economic scars of family violence and highlight the role — and challenges — of the public insurance system in mitigating this burden.

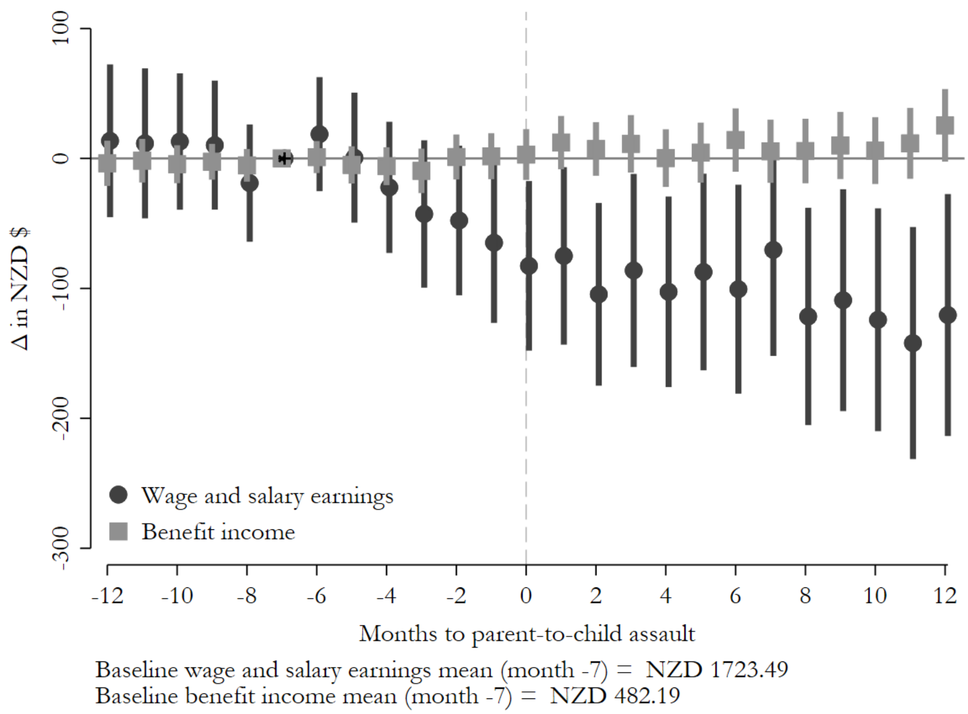
5.8 Figures

Figure 5.1: Labour earnings profiles of treatment and comparison groups



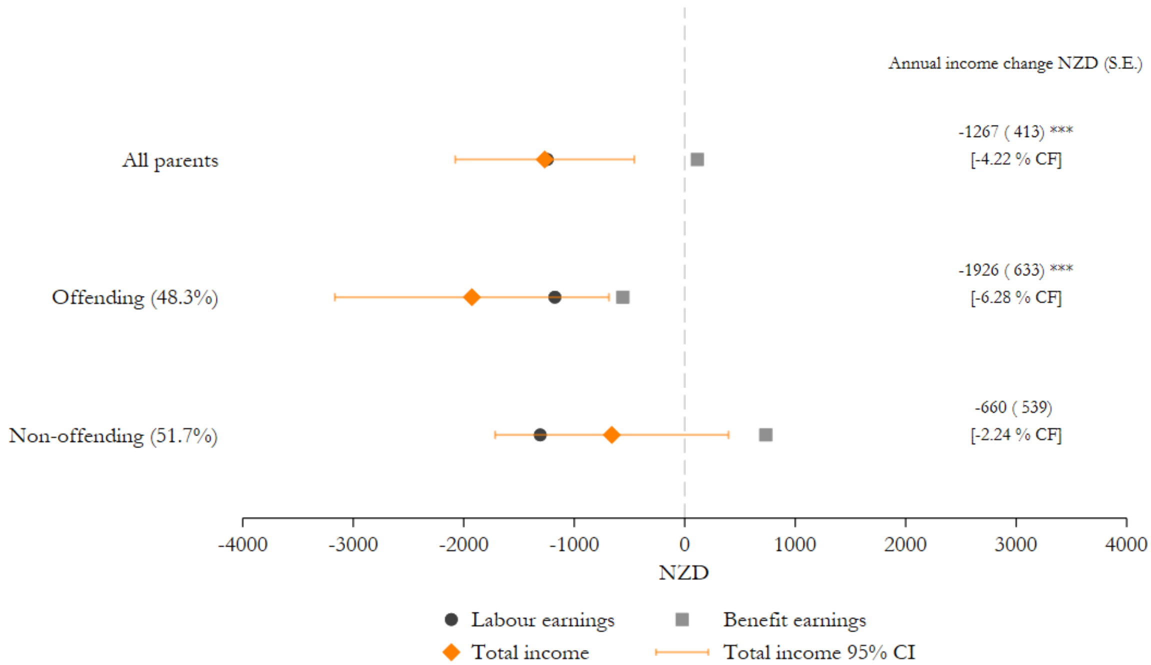
Notes: This figure plots the labour earnings trajectory of the treatment group and the comparison group over the treatment group's event timeline.The comparison group is defined as those who experience the same reported victimisation shock 25 months in the future. The dark dashed vertical line on month 0 is when the reported parent-to-child assault occurred, separating the pre- and post-assault periods. The shaded grey area over months 1-12 is the period for which we estimate causal effects, avoiding the later event months when the comparison group become compromised by their own Ashenfelter-type dip.

Figure 5.2: Parents' labour market response to their child being assaulted by a parent



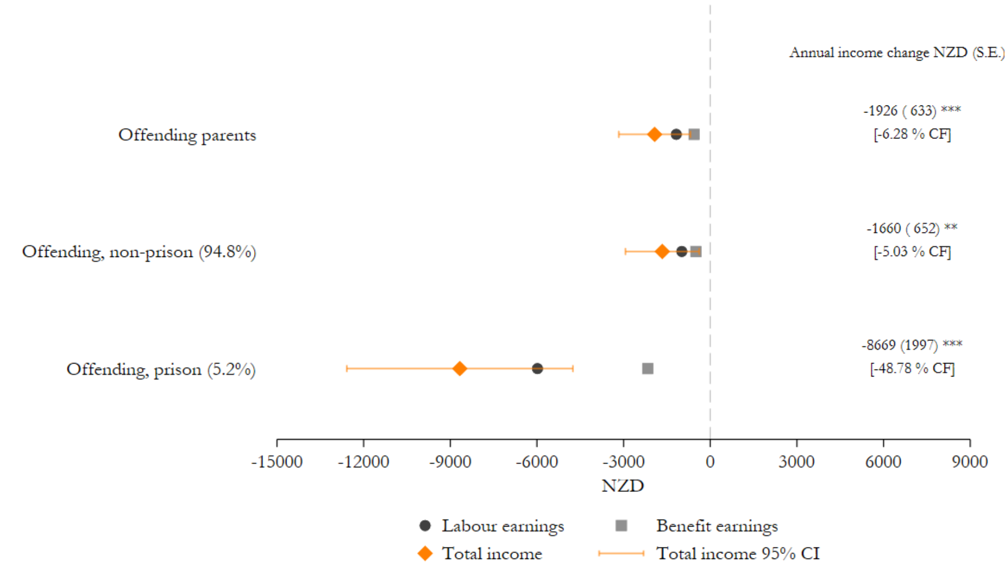
Notes: This figure shows the estimates of the effect of parent-to-child assaults on all parents' labour and benefit income. We plot the impact of child assaults on the parent-level outcome at each event time, i.e. δ_e's defined in Eq.5.1. We normalise the comparison group's outcome to the outcome level of the treatment group seven months prior to victimisation. The markers are the point estimates, and the vertical bars depict 95% confidence intervals with standard errors clustered at the child-level.

Figure 5.3: Dynamics of parents' labour market responses to their child being assaulted by a parent

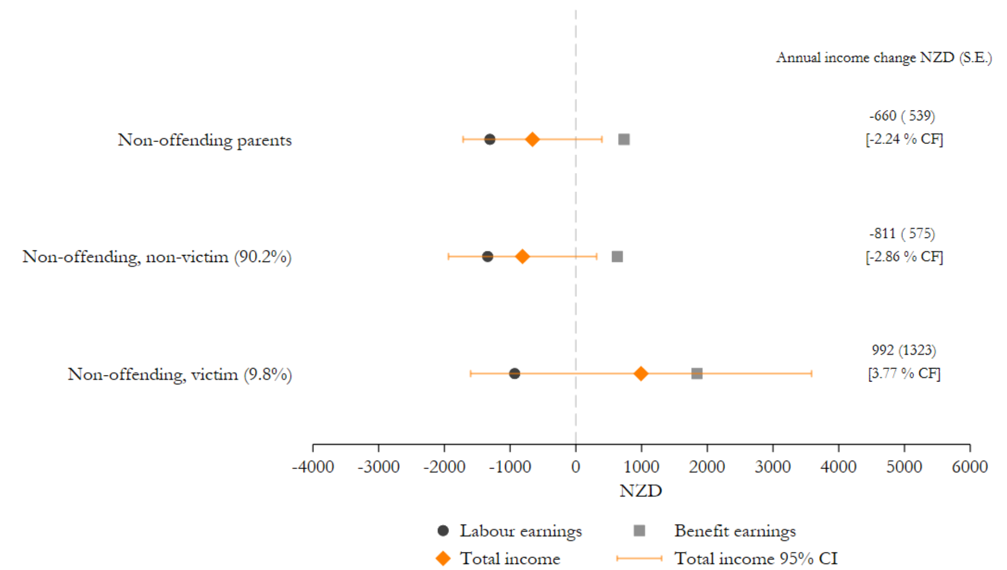


Notes: This figure shows the annual effect of parent-to-child assaults on parents' labour market behaviour for offending and non-offending parents. The circle markers are labour earnings effects, square markets are benefit income effects, and the solid diamond markers are total income effects. 95% confidence intervals are plotted around the total income estimates. These annual estimates are the cumulative total over months 1-12. The "all parents" effects are a weighted average of the "offending parent" and "non-offending" parent effects. The total income point estimates are stated on the right-hand side, with standard errors in soft parentheses, and significance shown by asterisks as follows: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. In hard parentheses is the annual income change reported as a percentage of counterfactual income using Eq.5.2.

Figure 5.4: Dynamics of parents' labour market responses to their child being assaulted by a parent: Extended



(a) Offending parent



(b) Non-offending parent

Notes: This figure shows the annual effect of parent-to-child assaults on parents' labour market behaviour for offending parents (Panel A) and non-offending parents (Panel B). The circle markers are labour earnings effects, square markets are benefit income effects, and the solid diamond markers are total income effects. 95% confidence intervals are plotted around the total income estimates. These annual estimates are the cumulative total over months 1-12. The overall effect for each group is a weighted average of the sub-group effects. The total income point estimates are stated on the right-hand side, with standard errors in soft parentheses, and significance shown by asterisks as follows: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. In hard parentheses is the annual income change reported as a percentage of counterfactual income using Eq.5.2.

Chapter 6

Discussion and concluding remarks

This thesis explored the profound and multifaceted impacts of adversity and loss, focusing on assault victimisation, family violence, and youth mortality. Across four papers, I used New Zealand's comprehensive population-wide linked administrative data to examine economic, social, and mental health outcomes for youth victims, their parents, and broader social networks, providing new evidence on the short- and long-term effects of these life-altering events.

The first two papers investigated the economic and social consequences of youth mortality. The first paper provided the foundation for empirically evaluating the effects of a relatively rare but high-impact event, being youth suicide. Using a quasi-experimental design, I showed that parental labour earnings dropped by approximately 6.5% in the first two years following their child's suicide.

The second paper extended this analysis by using two empirical methodologies to estimate both short-term and long-term responses to child loss, revealing stark differences across death type. Suicide-bereaved parents experienced an average annual earnings loss of 7% for up to a decade, particularly among low-income families, while traffic-bereaved parents tended to recover within four years. This paper also highlighted mental health impacts on siblings and classmates, underscoring the broad social toll of youth deaths, particularly those from suicide.

The second half of this thesis shifted the focus to the impacts of youth assault victimisation, uncovering how these events affected both the youth victim and their parents. The third paper focused on non-family violence assaults, showing significant negative effects on youth victims' physical and mental health, schooling behaviour, and labour market outcomes. The paper provided the first evidence of spillover effects to parents of youth victims, including decreased labour market attachment and increased use of mental health services and anti-anxiety medications. These results emphasised that the costs of youth victimisation goes well beyond the first-order effects on victims.

The final paper extended the victimisation literature by investigating the economic consequences of child-directed family violence. Unique data from New Zealand's police victimisation and offending registers allowed us to differentiate between offending and non-offending parents, revealing an asymmetry in total income losses after the child's assault. Offending parents bore the greater total burden due to reductions in both earnings and benefit eligibility. While non-offending parents faced larger labour earnings losses, these were partially offset by increased benefit income. Insights from the benefits data suggested that these benefit adjustments likely responded to changes in household composition, where non-offending parents shifted into sole caregiving roles.

Together, these four papers provided a comprehensive analysis of the intergenerational and societal impacts of adversity faced by New Zealand youth. They highlighted the economic vulnerabilities and mental health challenges faced by youth victims, their parents, and broader networks. These insights can help inform policies aimed at mitigating the long-term consequences of trauma and loss while providing more targeted support for affected families.

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Appendix A

Appendix material for Chapter 2

Table A0.1: Sample construction

Sample restriction	Observations
Youth deaths 2001-2018	7,878
(1) Total youth suicides	2,250
(2) Mothers identified via birth register and not deceased	1,683
(3) Mothers aged 30-60 at time of child death	1,656
(4) Fathers identified via birth register and not deceased	1,452
(5) Fathers aged 30-60 at time of child death	1,347
(6) Total households = youth suicides with both parents linked within age range	1,299

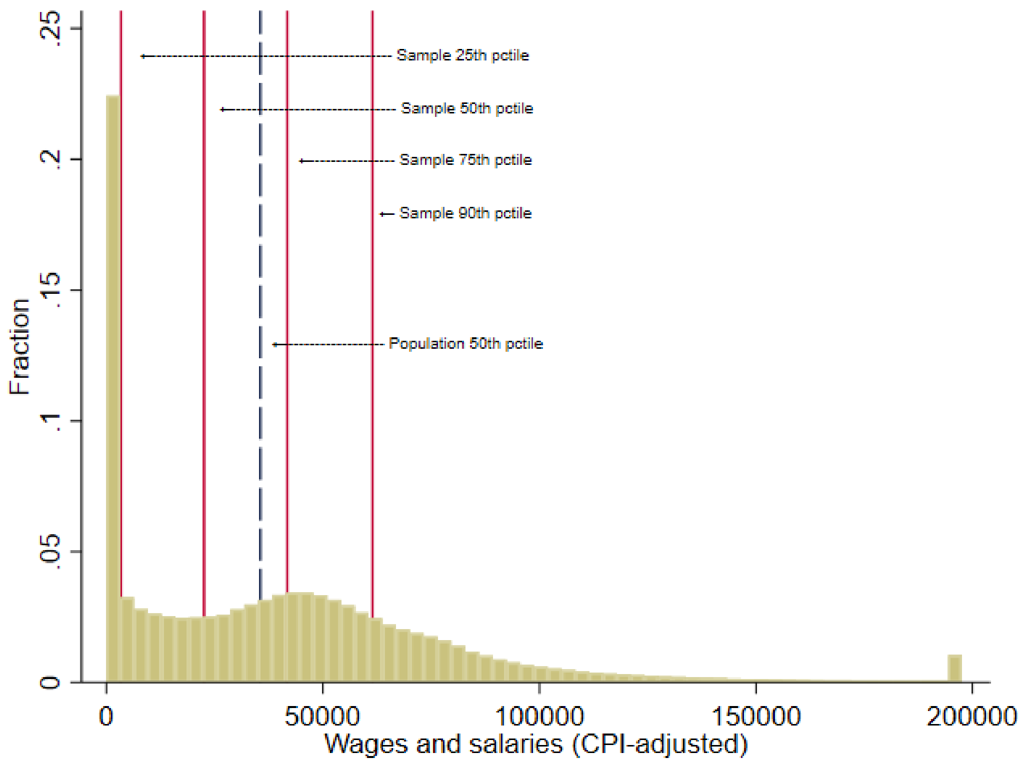
Notes: This table shows the sample construction numbers. We use mortality datasets to identify youth deaths and youth suicides. We use birth registers to link youth suicide victims with their parents. This requires the youth to be NZ-born, with at least one parent identified on their birth certificate. This data limitation is the reason our mother, father, and household samples are notably smaller than the total number of youth suicides. We require each parent to be aged 30-60 at the time of youth suicide. We define a household as a youth suicide victim linked to both parents who are of working age at the time of child death.

Table A0.2: Household characteristics

Household characteristics	Both-parent households
Counts	1,299
% daughter suicide	29.10
% child European	48.50
% child Māori	42.96
% child Pacific	6.70
% child Asian	1.39
Average age of child at death	19.71
% Both parents have no qualifications	39.72
% At least one parent has high school qualifications	47.11
% At least one parent has university qualifications	13.16
Average monthly household W&S 2-years prior to youth suicide	3,868
Average monthly household total earnings 2-years prior to youth suicide	4,540

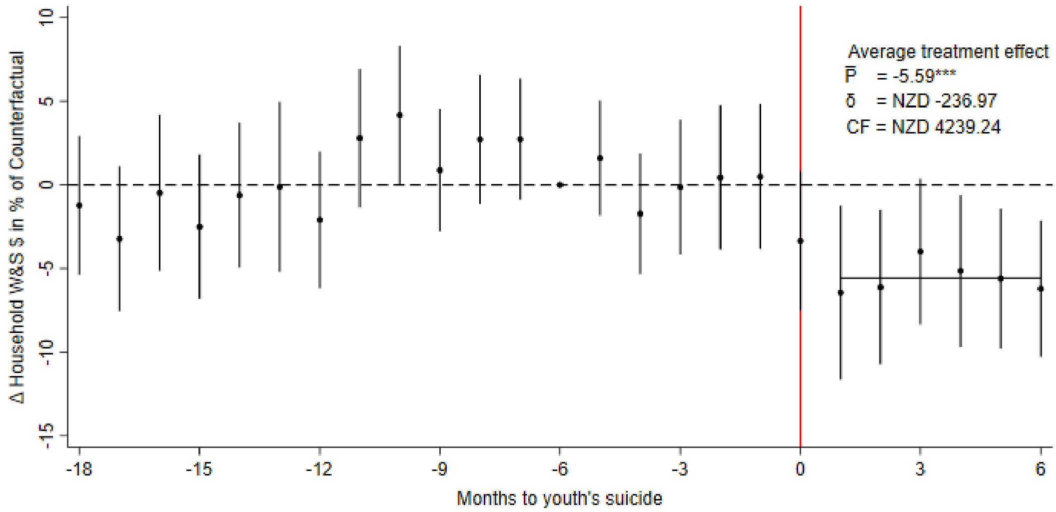
Notes: This table describes our main sample of households. We define household educational attainment by the highest completed degree of either parent, imputed from Census 2013. Average wages and salaries and total earnings variables are measured in NZD, winsorised at the 99th percentile.

Figure A0.1: Full population (age 30-60) labour earnings distribution versus selected percentiles from the earnings distribution of households experiencing suicide

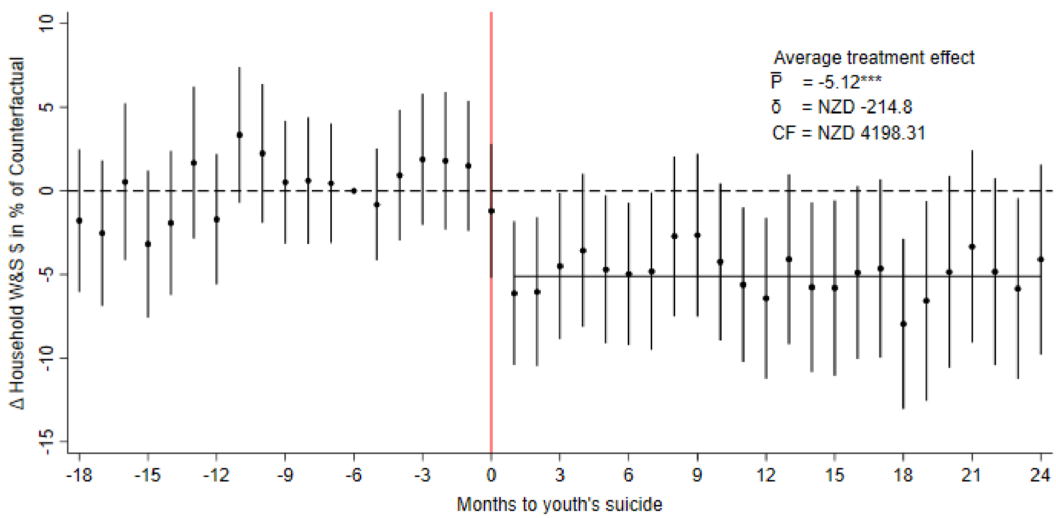


Notes: This figure presents the annual earnings distribution of the full population of 30-to-60-year-olds. We overlay selected percentiles from the annual earnings distribution of households experiencing suicide to benchmark our sample against the population. For the total population, we obtain the (CPI-adjusted) annual earnings distribution for all 30-to-60-year-olds pooled across 1999-2016. For the suicide sample, we calculate each households' per-parent (CPI-adjusted) annual earnings in the calendar year two years prior to youth suicide (ranging 1999-2016 as suicide data runs 2001-2018). After appending all years, we calculate the 25th, 50th, 75th, and 90th earnings percentiles from the suicide household sample and overlay these markers on the total population earnings distribution.

Figure A0.2: Household labour earnings response to child suicide across different estimation windows



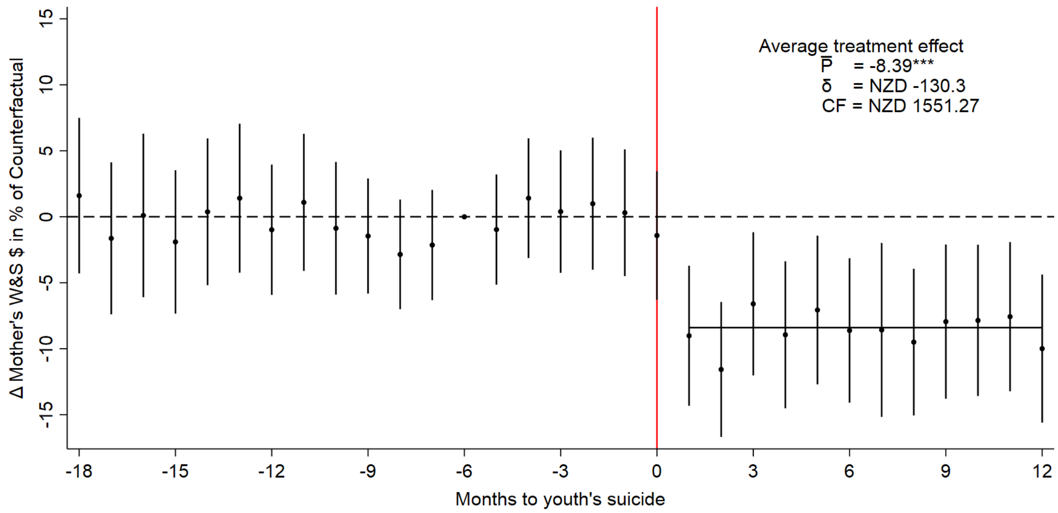
(a) 6-month window ($\Delta = 7$)



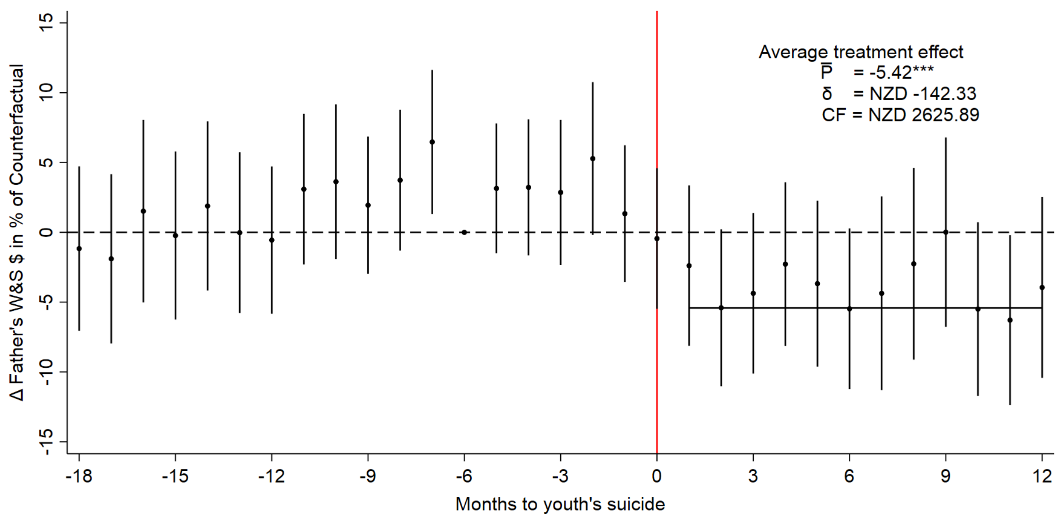
(b) 2-year window ($\Delta = 25$)

Notes: These figures show household labour earnings responses to child suicide using Equations (1) and (2), varying the 'Δ-months later' comparison group time definition across 7 and 25 months. Labour earnings are winsorised at the 99th percentile. The x-axis denotes months relative to youth suicide, normalised to period 0 for the treatment group (while the comparison group is treated at month Δ). We normalise the comparison group's outcome to the earnings level of the treatment group six months prior to suicide. The vertical bars indicate the 95% confidence intervals. The black circles are the point estimates. The horizontal solid black line across months 1-12 is the average treatment effect estimated by Equation 2.3 measured in percent of the counterfactual earnings of the treatment group calculated according to Equation 2.2. The average effect in NZD and the counterfactual NZD earnings are stated on the graph. Stars denote significance as: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. Standard errors are clustered at the child level. All graphs include our full household sample n=1,299.

Figure A0.3: Mothers and fathers' labour earnings response to child suicide



(a) Mothers



(b) Fathers

Notes: These figures show mothers' (Panel A, n=1,656) and fathers' (Panel B, n=1,347) labour earnings responses to child suicide using Equations (1) and (2). Labour earnings are winsorised at the 99th percentile. The x-axis denotes months relative to youth suicide, normalised to period 0 for the treatment group (while the comparison group is treated at month 13). We normalise the comparison group's outcome to the earnings level of the treatment group six months prior to suicide. The vertical bars indicate the 95% confidence intervals. The black circles are the point estimates. The horizontal solid black line across months 1-12 is the average treatment effect estimated by Equation 2.3 measured in percent of the counterfactual earnings of the treatment group calculated according to Equation 2.2. The average effect in NZD and the counterfactual NZD earnings are stated on the graph. Stars denote significance as: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Standard errors are clustered at the parent level.

Appendix B

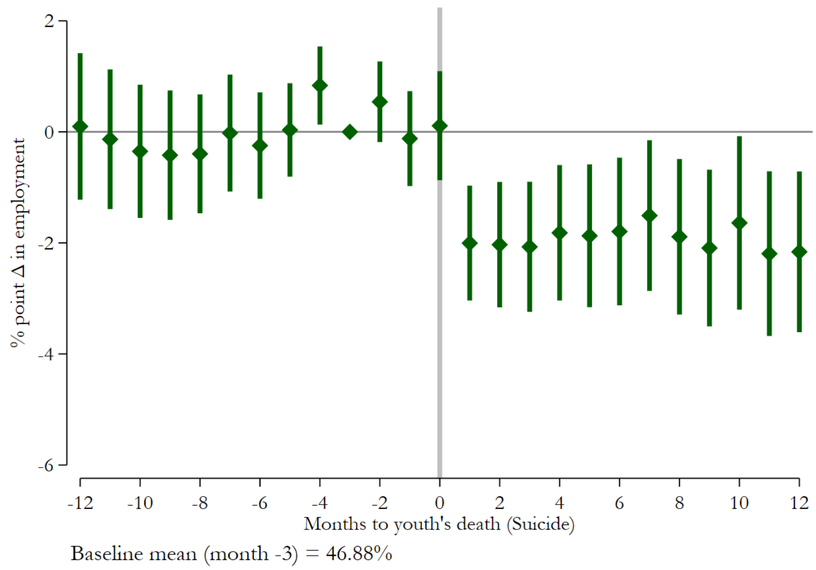
Appendix material for Chapter 3

Table B0.1: Differences in pre-death average outcomes across earnings quintiles by death type

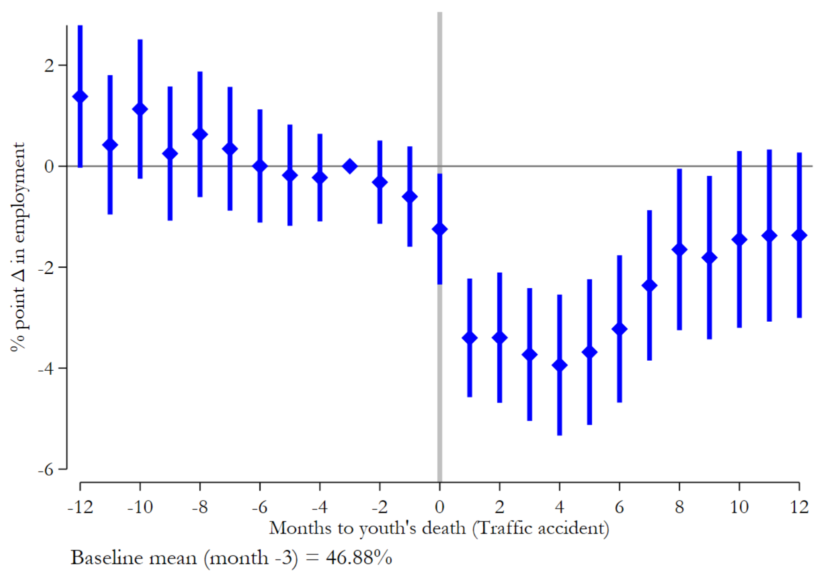
Quintile type	**Quintiles**	**Average earnings**		**Average employment**	
		(1)	(2)	(3)	(4)
		Traffic	Suicide	Traffic	Suicide
Separately	1	1644 (74)	1764 (84)	48.61 (1.51)	47.17 (1.41)
	2	9510 (168)	10844 (208)***	78.43 (1.30)	83.82 (1.21)***
	3	22377 (223)	24113 (237)***	93.69 (0.77)	95.55 (0.72)*
	4	36819 (257)	39149 (260)***	98.99 (0.40)	99.19 (0.36)
	5	66598 (1357)	75528 (2330)***	99.84 (0.24)	99.84 (0.23)
Combined	1	1725 (77)	1673 (81)	49.47 (1.51)	46.45 (1.43)
	2	10054 (177)	10215 (201)	79.17 (1.24)	82.40 (1.27)*
	3	23272 (222)	22888 (226)*	93.64 (0.73)	95.38 (0.73)
	4	38074 (260)	37528 (250)	99.20 (0.37)	97.98 (0.39)
	5	67521 (1413)	73607 (2218)**	99.17 (0.26)	99.55 (0.21)

Notes: This table compares the labour market outcomes across death type and within pre-death earnings quintiles. Panel A presents the earnings quintiles when estimated separately for suicide and traffic parents, while Panel B relates to quintiles calculated across both groups of bereaved parents combined. The first two columns present the pre-death average labour earnings (NZD) for traffic-bereaved and suicide-bereaved parents, while the second two columns present the same for average employment rates. Standard errors are in parentheses. Asterisk on the suicide-columns denote whether the difference between the suicide-mean and traffic-mean is statistically significant, as follows: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Figure B0.1: The effect of child loss on parents' employment



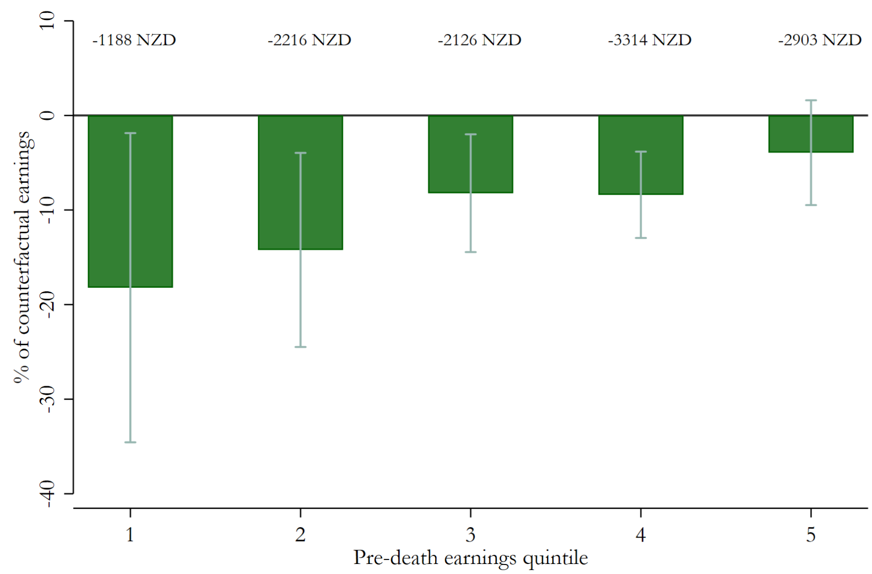
(a) Child loss by suicide



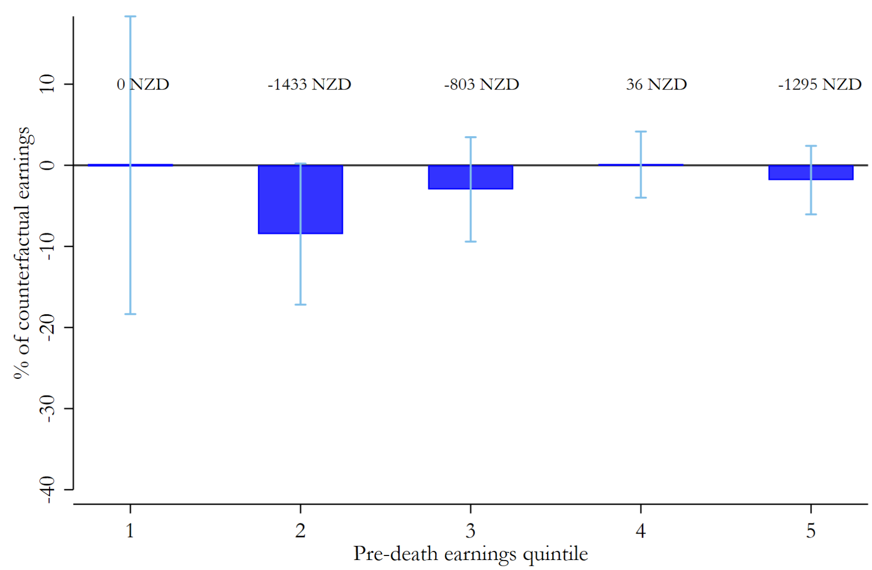
(b) Child loss by traffic accident

Notes: This figure shows the short-term dynamic effects of child loss on parents' employment rate. We estimate Eq. 3.3 where Z_i is the cause-of-death indicator, clustering standard errors at the child-level. We plot the monthly average treatment effects for suicide-bereaved parents (Panel A) and for traffic-bereaved parents (Panel B) from this interacted specification. We normalise the comparison group's outcome to the outcome level of the treatment group three months prior to the child's death. We plot the point estimates together with the 95% confidence intervals.

Figure B0.2: Labour earnings responses to child loss: Distribution of effects with common quintiles



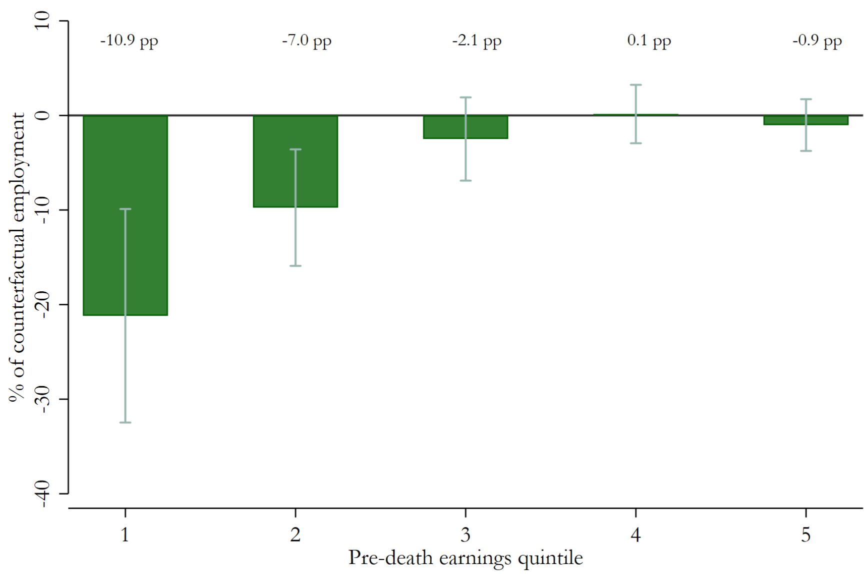
(a) Child loss by suicide



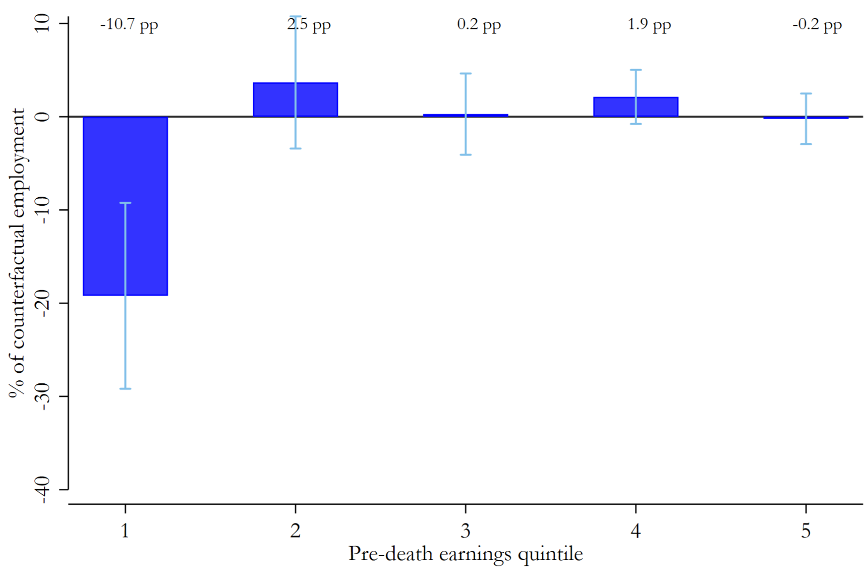
(b) Child loss by traffic accident

Notes: These figures explore heterogeneity in parental responses to child loss across parents' earnings distribution. To split parents into earnings quintiles, we average pre-death earnings in the four years before child loss and divide those with positive earnings into five quintiles where Q1 is the lowest earners and Q5 is the highest. This is done for suicide- and traffic-bereaved parents collectively. We estimate Eq. 3.4 separately for each quintile, using labour earnings as the outcome variables and then translate each annual effect into a percentage of the counterfactual outcome. We plot the average annual earnings responses measured within years 0-4 after the death. Bars represent 95% confidence intervals.

Figure B0.3: Employment responses to child loss: Distribution of effects



(a) Child loss by suicide



(b) Child loss by traffic accident

Notes: These figures explore heterogeneity in parental responses to child loss across parents' earnings distribution. To split parents into earnings quintiles, we average pre-death earnings in the four years before child loss and divide those with positive earnings into five quintiles where Q1 is the lowest earners and Q5 is the highest. We estimate Eq. 3.4 separately for each quintile, using employment as the outcome variables and then translate each annual effect into a percentage of the counterfactual outcome. We plot the average annual employment response measured within years 0-4 after the death. Bars represent 95% confidence intervals.

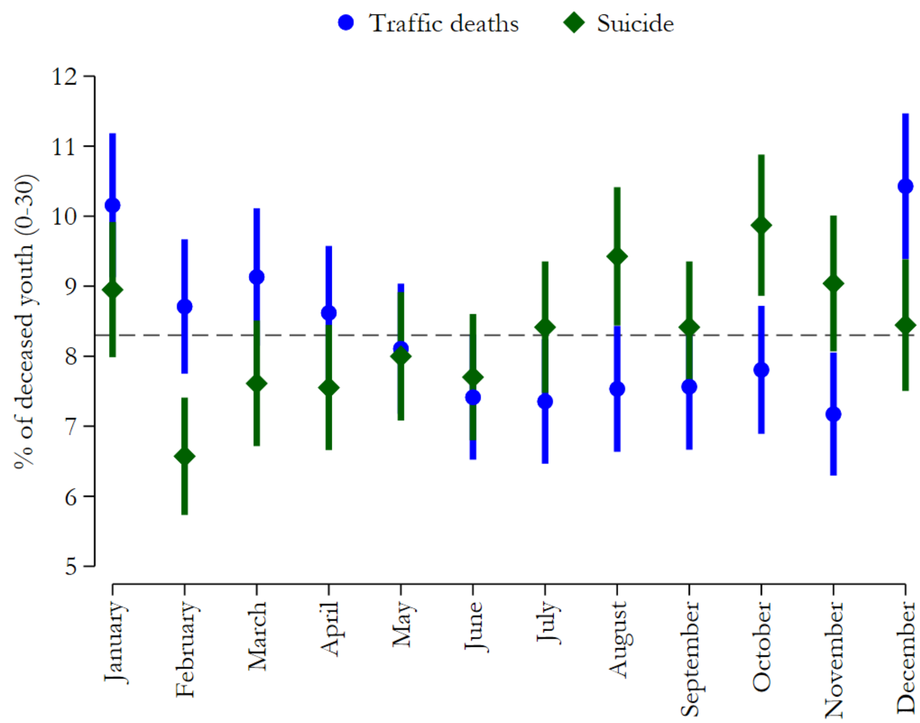
B1 Transition from monthly to annual estimates

One of the main contributions of this paper to the parental bereavement literature is estimating the long-run effects of child loss on parents' labour market outcomes. To do this, we transition from monthly to annual data and estimate effects 10 years after child loss. This shift has at least two important implications for the comparability between monthly and annual estimates.

First, using annual data loses precision in the timing of child death. The high-frequency monthly data enable us to zoom-in to the exact timing of the death. While there would be some variation in death days within a calendar month, these differences would only affect a few weeks of earnings within month 0. However, at the annual level, year 0 is defined as the calendar year of death, which allows for much more variation in how many pay cycles are affected within year 0. For January deaths, all labour earnings in year 0 would be measured after the loss of the child, whereas for deaths in December, most earnings in year 0 would be pre-child loss.

This imprecision wouldn't be a concern if suicide and traffic fatalities were evenly distributed throughout calendar months. Appendix Figure B1.1 plots the percentage of youth suicides and fatal traffic accidents by calendar month, pooled over 2001-2019. If suicides and traffic deaths were evenly distributed, we would expect 8.3% of the sample to die each month, as depicted by the horizontal dashed line. This is not the case: youth deaths are seasonal and the pattern of which depends on the cause of death. There are significantly more traffic deaths in the warmer months (December-March), broadly corresponding with NZ's summer holidays and school break. Youth suicides are significantly more likely to happen in the latter half of the calendar year, corresponding with the end of winter (August) and the high school and university exam period (October-November).

Figure B1.1: Percentage of youth suicide and traffic fatalities by calendar month (2001-2019)



Notes: This figure plots the calendar month of death for the sample of youth (ages 0-30) who died by suicide (N=3,363) or traffic accidents (N=3,318) over 2001-2019. The 95% confidence intervals are plotted around the monthly means for each death type. The dashed black line represents an even distribution of deaths throughout the calendar year.

Seasonal patterns in youth suicides and fatal traffic accidents make the monthly and annual estimates of parental responses to child loss difficult to compare directly. This is especially true if labour earnings losses are temporary, as the observed recovery will vary between monthly and annual settings depending on how much of year 0 is 'treated.'

Instead, we can derive an annual OLS estimator to understand how the monthly and annual estimates relate to each other. Appendix B2 does this in a simple OLS setting to show that the estimated average annual effect is the sum of the variance-covariance weighted average of the effects from deaths in each calendar month.

The second implication of switching from monthly to annual data is that our sample changes due to maintaining a balanced panel. With the currently available IDI data, we can measure deaths from 2001-2019 and labour market outcomes from 2000-2023. This means, for every parent, we have at least 12 months of pre-child death earnings and 48 months of post-child death earnings. In annual terms, this is only 1 year pre-death and 4 years post-death. To estimate longer-term annual effects, we require the treatment group to be observed over a longer time-horizon, reducing the number of death years from which our balanced sample is constructed. Specifically, we use deaths from 2004-2013 to allow 4 pre-death years and 10 post-death years for the annual specification.[1]

B2 Relating monthly to annual OLS estimates

To understand the difference between an annual versus monthly estimate of the effect of child loss on parents' labour earnings, we begin with a simple OLS model where $D_{i,t}$ is an indicator if parent i's child died in time t and $Y_{i,t}$ is parent i's annual labour earnings in time t.

$$Y_{i,t} = \alpha_i + \beta_i \cdot \mathrm{D}_{i,t} + \epsilon_{i,t} \tag{B.1}$$

Time t is measured in calendar years comprising 12 calendar months. $D_{i,t}$ is therefore the sum of indicators for deaths that occurred in calendar months 1-12.

[1]Whilst not presented in this paper, we test for comparability between the monthly versus annual data using the yet-to-be-treated comparison group approach. The purpose of this exercise was to see if the effects of losing a child to suicide on parents' monthly labour earnings is approximately equivalent to the effect on parents' annual labour earnings divided by 12 when enforcing a similar pre-period on the balanced samples (i.e., where the pre-period is made up of 48 pre-death months or 4 pre-death years). Results confirm that the effects are equivalent. This differs from the monthly results presented in the main paper, since we use 12 pre-death months when estimating effects over 12-months post-death.

$$D_{i,t} = d_{i,t}^1 + d_{i,t}^2 + \cdots + d_{i,t}^{12} \tag{B.2}$$

The OLS estimator for $\hat{\beta}$ is:

$$\hat{\beta} = \frac{\text{Cov}(Y_{i,t}, D_{i,t})}{\text{Var}(D_{i,t})} \tag{B.3}$$

Substituting the death month indicators into the annual equation (Eq B.2 into B.1) and then substituting for $Y_{i,t}$ gives:

$$\hat{\beta} = \frac{\text{Cov}(\alpha + b_1 d_{i,t}^1 + b_2 d_{i,t}^2 + \cdots + b_{12} d_{i,t}^{12} + \varepsilon_{i,t}, D_{i,t})}{\text{Var}(D_{i,t})} \tag{B.4}$$

Expanding and dropping the constant and error terms due to no covariance:

$$\hat{\beta} = b_1 \left(\frac{\text{Cov}(d_{i,t}^1, D_{i,t})}{\text{Var}(D_{i,t})} \right) + b_2 \left(\frac{\text{Cov}(d_{i,t}^2, D_{i,t})}{\text{Var}(D_{i,t})} \right) + \cdots + b_{12} \left(\frac{\text{Cov}(d_{i,t}^{12}, D_{i,t})}{\text{Var}(D_{i,t})} \right) \tag{B.5}$$

Thus, the annual estimate $\hat{\beta}$ is the sum of the variance-covariance weighted average of the separate effects of child loss on parent's earnings for deaths in each calendar month, where the weights (b_1, b_2, $\ldots$, b_{12}) represent the relative contribution of each month's effect to the total annual effect ($\hat{\beta}$). These weights will differ by the child's cause of death since suicide and traffic deaths have different seasonal patterns throughout the calendar year.

B3 Comparability of annual estimates by choice of comparison group

To estimate the short-run economic costs of child loss we use a future-treated comparison group, and to estimate the long-run economic costs of child loss we use a matched-comparison group. To test the robustness of our findings across these methods, we use annual data in both the yet-to-be-treated and matching setups and observe whether the effects are similar over the medium run.

We use the balanced annual panel from the matching setup, which runs four years before to 10 years after the child's death, relating to deaths occurring between 2004 and 2013. We focus on the medium-run effects measured four years after the death event. For the matching setup, these year-4 effects are visualised in Figures 3.3b and 3.4b. For the future-treated setup, we create a comparison group of parents who experience child loss in year 5.

Results in Appendix Table B3.1 show that the annual estimates are similar using each comparison group. Parents bereaved by child suicide experience significant annual labour earning losses of about $1,800 measured four years after the death event, while traffic-bereaved parents have a small, positive and insignificant labour earnings response.

Table B3.1: Medium-term annual labour earnings responses to child loss using different comparison groups

Comparison group	**(1) Traffic NZD [CI]**	**(2) Suicide NZD [CI]**
Matched non-bereaved	124 [-816, 1064]	-1779 [-2850, -708] ***
Future-bereaved	377 [-1433; 2189]	-1852 [-3527, -177] **

Notes: This table shows the annual average labour earnings response to losing a child to a traffic accident (column 1) or to suicide (column 2), measured in the fourth year after the death event. The matched comparison group is created following the same matching procedure in Section 3.7. The future-bereaved comparison group are parents who experience the same child death event 5 years in the future. The point estimates are provided in NZD, with the 95% confidence intervals in parentheses. Asterisk denotes significance as follows: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Appendix C

Appendix material for Chapter 4

Table C0.1: Summary statistics: Parents of youth assault victims versus general population of parents

Characteristic	Parents of youth assault victims	Matched general population of parents	General population of parents
Age (in years)	47.10	47.10	44.97 ***
Female (%)	19.14	13.49 ***	11.84 ***
Ethnicity: European (%)	47.22	62.86 ***	60.61 ***
Ethnicity: Māori (%)	35.81	18.05 ***	18.14 ***
Ethnicity: Pacific (%)	8.30	5.98 ***	6.54 ***
Ethnicity: Asian (%)	3.58	9.93 ***	11.41 ***
Ethnicity: Other (%)	2.35	3.19 ***	3.30 ***
No qualifications as at Census 2018 (%)	18.60	13.29 ***	12.72 ***
High school qualification as at Census 2018 (%)	46.85	53.35 ***	52.29 ***
Bachelor's and above as at Census 2018 (%)	9.93	25.61 ***	26.84 ***
Missing Census 2018 education information (%)	24.62	7.78 ***	8.16 ***
NZ Deprivation Index (ranging 0-10)	6.51	5.33 ***	5.38 ***
NZ Deprivation Index 2018 missing (%)	19.07	4.30 ***	4.70 ***
Labour earnings in 2018 (NZD)	28,881.27	44,911.43 ***	44,255.53 ***
Benefit earnings in 2018 (NZD)	3,373.99	1,494.27 ***	1,536.11 ***
Total counts	12,147	12,147	1,172,154

Notes: The table shows descriptive statistics for three samples: parents of youth victims, a matched sample of the general population of parents of youth, and the full general population of parents of youth. The matched sample of parents has the same age distribution as the parents of youth victims. The NZ Deprivation Index is an area-based measure of deprivation, with 1 representing the least deprived areas and 10 representing the most deprived. Asterisk in column 2 indicate significant differences between column 2 and column 1, while the asterisk in column 3 indicate significant differences between column 3 and 1. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table C0.2: Sample robustness: Youth's physical and mental health outcomes

Outcome	Sub-sample	Counts	Month -3 baseline	Month 0 treatment effect
Hospital-isation	Main sample	93,243	2.35	5.21 (0.12) ***
	Excluding first 12 months of NZRCV	77,145	2.27	5.21 (0.13) ***
	Excluding victims who are also offenders	88,113	2.31	5.10 (0.12) ***
	Excluding victims without linked parent(s)	62,562	2.55	5.55 (0.15) ***
Pain relief	Main sample	99,630	4.18	4.43 (0.14) ***
	Excluding first 12 months of NZRCV	83,538	4.23	4.46 (0.15) ***
	Excluding victims who are also offenders	94,365	4.22	4.40 (0.14) ***
	Excluding victims without linked parent(s)	66,297	4.50	4.83 (0.17) ***
CMT Team	Main sample	93,243	1.37	1.14 (0.08) ***
	Excluding first 12 months of NZRCV	77,145	1.35	1.17 (0.08) ***
	Excluding victims who are also offenders	88,113	1.30	0.99 (0.08) ***
	Excluding victims without linked parent(s)	62,562	1.59	1.26 (0.10) ***
AOD Team	Main sample	93,243	0.54	0.23 (0.05) ***
	Excluding first 12 months of NZRCV	77,145	0.51	0.26 (0.05) ***
	Excluding victims who are also offenders	88,113	0.48	0.18 (0.05) ***
	Excluding victims without linked parent(s)	62,562	0.60	0.30 (0.06) ***
IPT Team	Main sample	93,243	0.09	0.09 (0.02) ***
	Excluding first 12 months of NZRCV	77,145	0.08	0.09 (0.02) ***
	Excluding victims who are also offenders	88,113	0.08	0.07 (0.02) ***
	Excluding victims without linked parent(s)	62,562	0.09	0.12 (0.03) ***
All other teams	Main sample	93,243	0.26	0.13 (0.03) ***
	Excluding first 12 months of NZRCV	77,145	0.26	0.16 (0.04) ***
	Excluding victims who are also offenders	88,113	0.25	0.06 (0.03) *
	Excluding victims without linked parent(s)	62,562	0.28	0.15 (0.04) ***
Anti-anxiety	Main sample	99,630	0.53	0.20 (0.04) ***
	Excluding first 12 months of NZRCV	83,538	0.54	0.23 (0.04) ***
	Excluding victims who are also offenders	94,365	0.51	0.18 (0.04) ***
	Excluding victims without linked parent(s)	66,297	0.61	0.25 (0.05) ***
Anti-depressants	Main sample	99,630	3.03	0.34 (0.08) ***
	Excluding first 12 months of NZRCV	83,538	3.09	0.34 (0.08) ***
	Excluding victims who are also offenders	94,365	2.99	0.34 (0.08) ***
	Excluding victims without linked parent(s)	66,297	3.53	0.34 (0.10) ***
Anti-psychotics	Main sample	99,630	1.58	0.24 (0.05) ***
	Excluding first 12 months of NZRCV	83,538	1.59	0.24 (0.05) ***
	Excluding victims who are also offenders	94,365	1.52	0.19 (0.05) ***
	Excluding victims without linked parent(s)	66,297	1.81	0.27 (0.06) ***

Notes: This table contains the month 0 treatment effects for the effect of assault victimisation on youth victims separately by Eq.4.1 for different samples. For each outcome, row 1 repeats the treatment effects for the main sample, row 2 excludes youth victims who were victimised within the first 12 months of the police register, row 3 excludes youth victims who were also an offender in the month leading up to, and including the day of, the victimisation event, and row 4 is only the youth victims that have parents linked for the parent-level analysis. All counts have been randomly rounded to base 3, so may not add up to the overall total. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table C0.3: Sample robustness: Youth's labour market and schooling outcomes

Outcome	Sub-sample	Counts	Pre-death baseline	Average monthly treatment effect
Labour earnings	Main sample	68,907	1434.66	-71.52 (6.03) ***
	Excluding first 12 months of NZRCV	57,183	1504.70	-74.28 (6.89) ***
	Excluding victims who are also offenders	64,437	1465.33	-67.95 (6.26) ***
	Excluding victims without linked parent(s)	46,383	1412.76	-64.82 (7.23) ***
Benefit and ACC earnings	Main sample	68,907	405.12	17.28 (2.15) ***
	Excluding first 12 months of NZRCV	57,183	394.11	16.00 (2.39) ***
	Excluding victims who are also offenders	64,437	400.53	18.98 (2.21) ***
	Excluding victims without linked parent(s)	46,383	455.29	19.04 (2.74) ***
Total income	Main sample	68,907	1913.00	-55.04 (5.54) ***
	Excluding first 12 months of NZRCV	57,183	1974.22	-59.05 (6.33) ***
	Excluding victims who are also offenders	64,437	1939.58	-49.95 (5.74) ***
	Excluding victims without linked parent(s)	46,383	1946.79	-46.03 (6.57) ***
		Counts	Month -3 baseline	Month 0 treatment effect
Schooling intervention	Main sample	29,481	2.88	1.17 (0.18) ***
	Excluding first 12 months of NZRCV	24,735	2.76	1.25 (0.19) ***
	Excluding victims who are also offenders	28,389	2.75	1.13 (0.18) ***
	Excluding victims without linked parent(s)	20,415	3.15	1.35 (0.22) ***

Notes: This table contains the treatment effects for the effect of assault victimisation on youth victims separately for different samples. For labour market outcomes, this table provides the 12-month post-victimisation average monthly treatment effect for youth victims as estimated by Eq.4.3, where the baseline is measured across the pre-period from months -12 to -1. For schooling outcomes, this table provides the month 0 treatment effects estimated by Eq.4.1, with the baseline set to month -3. For each outcome, row 1 repeats the treatment effects for the main sample, row 2 excludes youth victims who were victimised within the first 12 months of the police register, row 3 excludes youth victims who were also an offender in the month leading up to, and including the day of, the victimisation event, and row 4 is only the youth victims that have parents linked for the parent-level analysis. All counts have been randomly rounded to base 3, so may not add up to the overall total. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table C0.4: Sample robustness: Parents' mental health outcomes

Outcome	Sub-sample	Counts	Month -3 baseline	Month 0 treatment effect
CMT Team	Main sample	206,304	0.48	0.17 (0.04) ***
	Excluding first 12 months of NZRCV	163,644	0.47	0.21 (0.05) ***
	Excluding parents of youth victims also offenders	194,298	0.48	0.15 (0.04) ***
	Excluding parents who were victimised in 12 months prior to youth's victimisation	194,544	0.41	0.15 (0.04) ***
AOD Team	Main sample	206,304	0.33	0.02 (0.03)
	Excluding first 12 months of NZRCV	163,644	0.33	0.02 (0.04)
	Excluding parents of youth victims also offenders	194,298	0.33	0.03 (0.04)
	Excluding parents who were victimised in 12 months prior to youth's victimisation	194,544	0.30	0.00 (0.03)
IPT Team	Main sample	206,304	0.05	0.01 (0.01)
	Excluding first 12 months of NZRCV	163,644	0.05	0.02 (0.02)
	Excluding parents of youth victims also offenders	194,298	0.05	0.00 (0.01)
	Excluding parents who were victimised in 12 months prior to youth's victimisation	194,544	0.05	0.00 (0.01)
All other teams	Main sample	206,304	0.07	0.00 (0.02)
	Excluding first 12 months of NZRCV	163,644	0.06	0.02 (0.02)
	Excluding parents of youth victims also offenders	194,298	0.07	0.01 (0.02)
	Excluding parents who were victimised in 12 months prior to youth's victimisation	194,544	0.06	-0.00 (0.02)
Anti-anxiety	Main sample	218,232	1.31	0.14 (0.04) ***
	Excluding first 12 months of NZRCV	175,569	1.33	0.16 (0.05) ***
	Excluding parents of youth victims also offenders	205,959	1.31	0.14 (0.04) ***
	Excluding parents who were victimised in 12 months prior to youth's victimisation	205,482	1.21	0.12 (0.04) ***
Anti-depressants	Main sample	218,232	6.20	0.00 (0.09)
	Excluding first 12 months of NZRCV	175,569	6.33	0.04 (0.10)
	Excluding parents of youth victims also offenders	205,959	6.23	0.01 (0.09)
	Excluding parents who were victimised in 12 months prior to youth's victimisation	205,482	5.99	-0.00 (0.09)
Anti-psychotics	Main sample	218,232	2.16	0.07 (0.04)
	Excluding first 12 months of NZRCV	175,569	2.18	0.04 (0.05)
	Excluding parents of youth victims also offenders	205,959	2.15	0.06 (0.04)
	Excluding parents who were victimised in 12 months prior to youth's victimisation	205,482	2.04	0.05 (0.04)

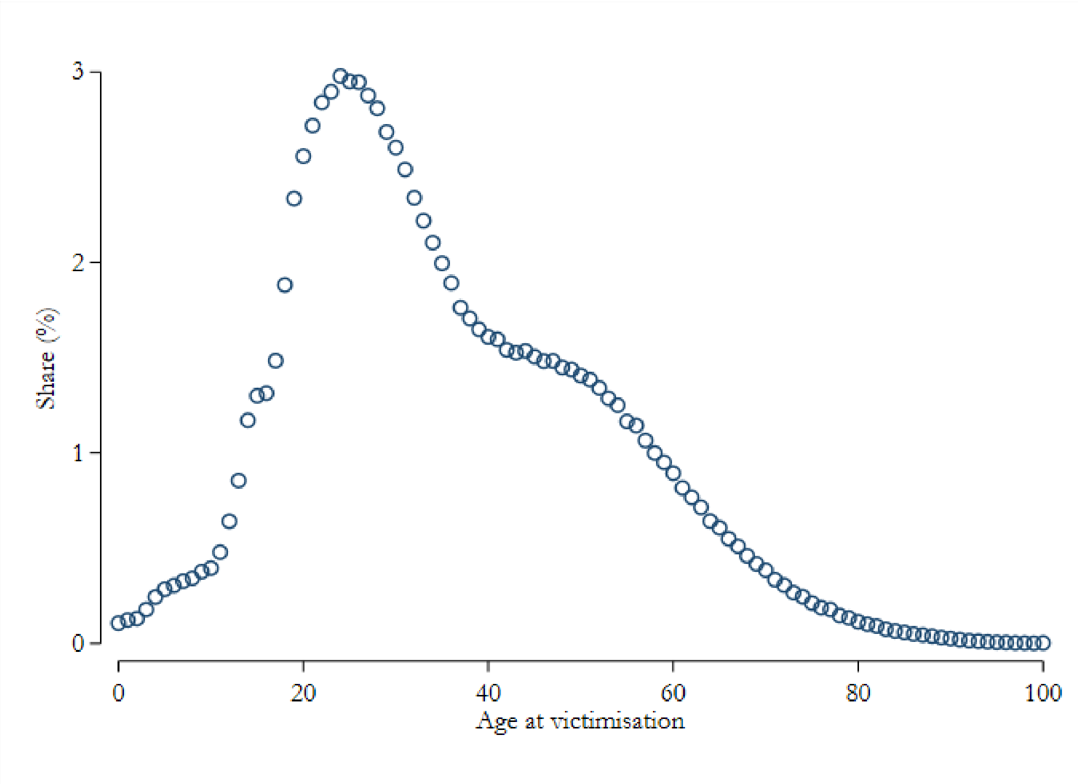
Notes: This table contains the treatment effects for the effect of youth assault victimisation on parents' outcomes separately by Eq.4.1 for different samples. For each outcome, row 1 repeats the treatment effects for the main sample of parents, row 2 excludes parents of youth victims who were victimised within the first 12 months of the police register, row 3 excludes parents of youth victims who were also an offender in the month leading up to, and including the day of, the victimisation event, and row 4 excludes parents who themselves were victimised in the 12 months leading up to the youth's victimsiation. All counts have been randomly rounded to base 3, so may not add up to the overall total. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table C0.5: Sample robustness: Parents' labour market outcomes

Outcome	Sub-sample	Counts	Pre-death baseline	Average monthly treatment effect
Labour earnings	Main sample	226,272	2256.13	-14.20 (5.24) ***
	Excluding first 12 months of NZRCV	183,609	2353.37	-17.94 (6.08) ***
	Excluding parents of youth victims also offenders	213,834	2288.65	-15.76 (5.43) ***
	Excluding parents who were victimised in 12 months prior to youth's victimisation	212,880	2287.66	-11.86 (5.43) **
Benefit and ACC earnings	Main sample	226,272	341.44	3.46 (1.48) **
	Excluding first 12 months of NZRCV	183,609	341.89	3.43 (1.70) **
	Excluding parents of youth victims also offenders	213,834	338.55	3.76 (1.52) **
	Excluding parents who were victimised in 12 months prior to youth's victimisation	212,880	317.50	2.75 (1.49) *
Total income	Main sample	226,272	2682.79	-9.05 (5.03) *
	Excluding first 12 months of NZRCV	183,609	2784.80	-13.74 (5.86) **
	Excluding parents of youth victims also offenders	213,834	2713.13	-10.37 (5.22) **
	Excluding parents who were victimised in 12 months prior to youth's victimisation	212,880	2690.03	-7.00 (5.22)

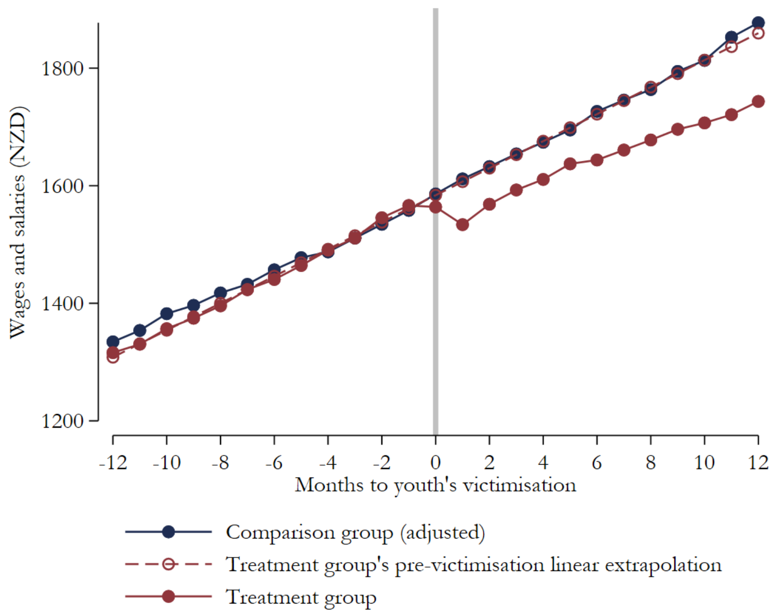
Notes: This table contains the treatment effects for the effect of youth assault victimisation on parents' outcomes separately for different samples. This table provides the 12-month post-victimisation average monthly treatment effect for parents of youth victims as estimated by Eq.4.3, where the baseline is measured across the pre-period from months -12 to -1. For each outcome, row 1 repeats the treatment effects for the main parent sample, row 2 excludes parents of youth victims who were victimised within the first 12 months of the police register, row 3 excludes parents of youth victims who were also an offender in the month leading up to, and including the day of, the victimisation event, and row 4 excludes parents who themselves were victimised in the 12 months leading up to the youth's victimsiation. All counts have been randomly rounded to base 3, so may not add up to the overall total. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Figure C0.1: Age distribution for all reported victims



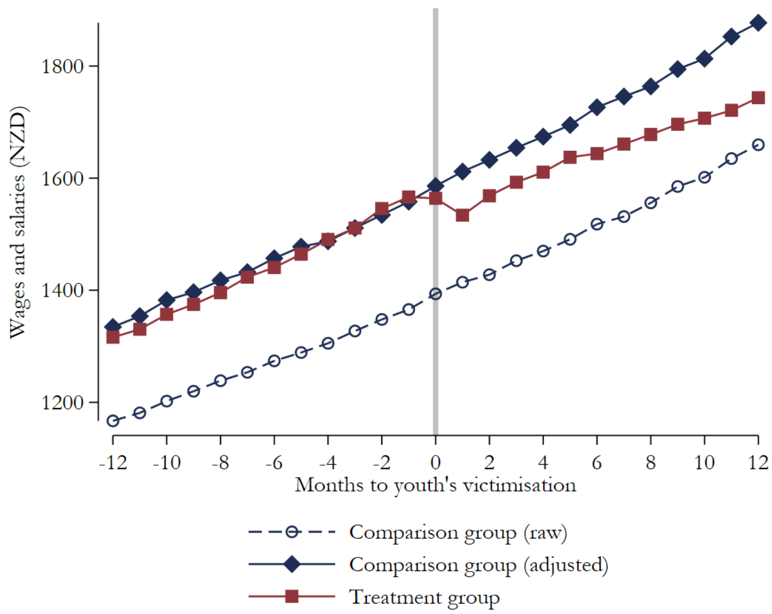
Notes: This figure shows the age distribution of all reported victims in New Zealand from 2014 to 2023. The y-axis shows the share of the sample, and the x-axis shows the age at the time of the victimisation. The age-victimisation curve peaks at age 24.

Figure C0.2: Event timeline of labour earnings for youth treatment and comparison groups, including linear extrapolations



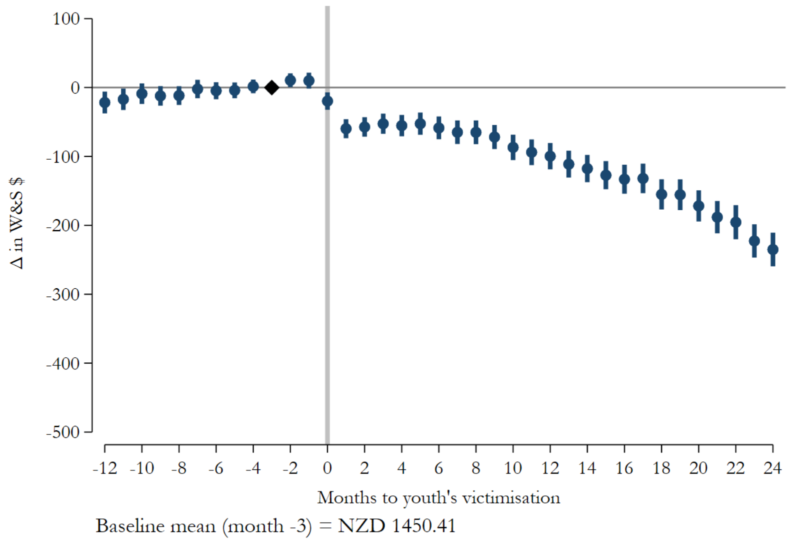
Notes: This figure plots the average labour earnings for working-aged youth (ages 18-30) over a victimisation timeline spanning from month -12 to 12, with victimisation occurring in month 0. There are 60,657 youth in the treatment group and 67,389 in the comparison group. The adjusted labour earnings for the comparison group include age and calendar time fixed effects and a level shift to equal the month -3 labour earnings of the treatment group. The treatment group's pre-victimisation linear extrapolation of the labour earnings is estimated over months -12 to -1.

Figure C0.3: Event timeline of labour earnings for youth treatment and comparison groups

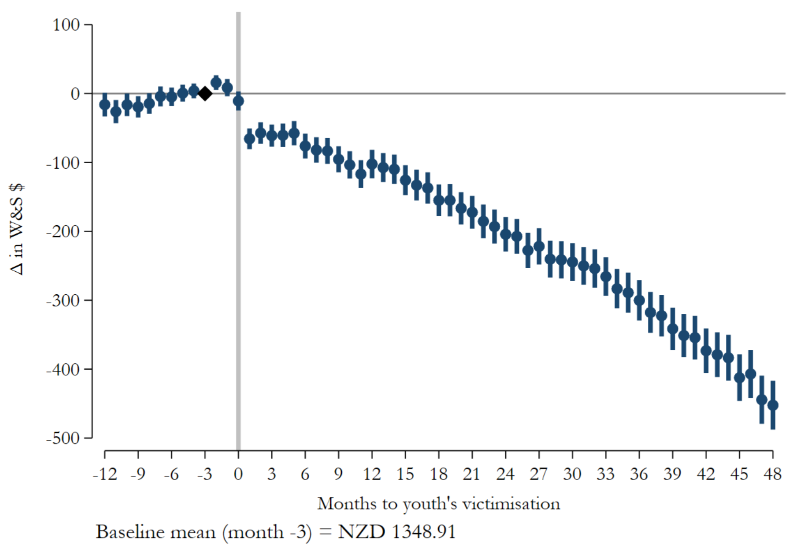


Notes: This figure plots the average labour earnings for working-aged youth (ages 18-30) over a victimisation timeline spanning from month -12 to 12, with victimisation occurring in month 0. There are 60,657 youth in the treatment group and 67,389 in the comparison group. The adjusted labour earnings for the comparison group include age and calendar time fixed effects and a level shift to equal the month -3 labour earnings of the treatment group.

Figure C0.4: The effect of assault victimisation on youth victims' labour earnings 2-years and 4-years post-victimisation



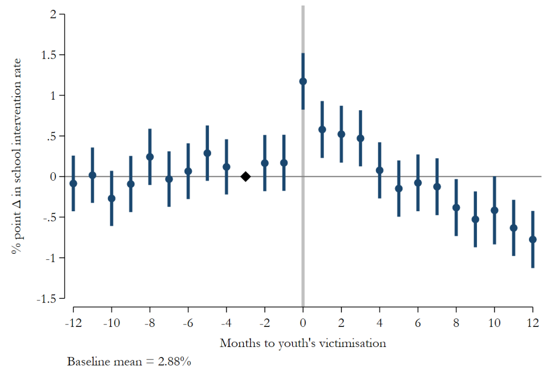
(a) 24 months post-victimisation



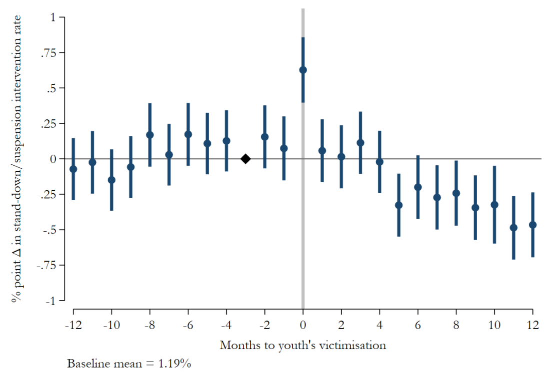
(b) 48 months post-victimisation

Notes: This figure shows the estimates of the effect of assault victimisation on youth's labour earnings. Panel C0.4a shows the effect over a 24-month horizon such that the comparison group is victimised in month 25, and Panel C0.4b shows the effect over a 48-month horizon such that the comparison group is victimised in month 49. Each series show the point estimates (NZD) for the impact of victimisation on youth's labour earnings at each event time, i.e. δ_e's defined in Eq.4.1 but over a longer event timeline. We normalise the comparison group's outcome to the outcome level of the treatment group three months prior to victimisation. The circles are the point estimates, and the vertical bars depict 95% confidence intervals with standard errors clustered at the youth-level.

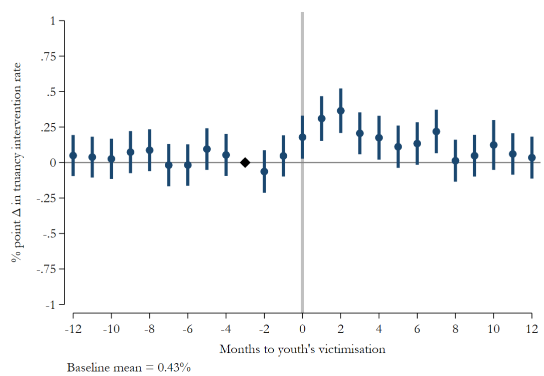
Figure C0.5: The effect of assault victimisation on youth's schooling interventions



(a) Any interventions



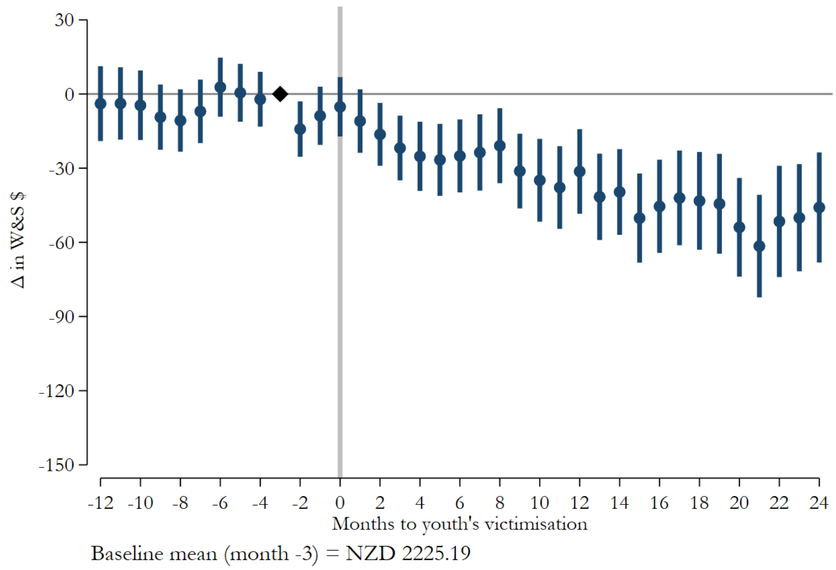
(b) Stand-down or suspension interventions



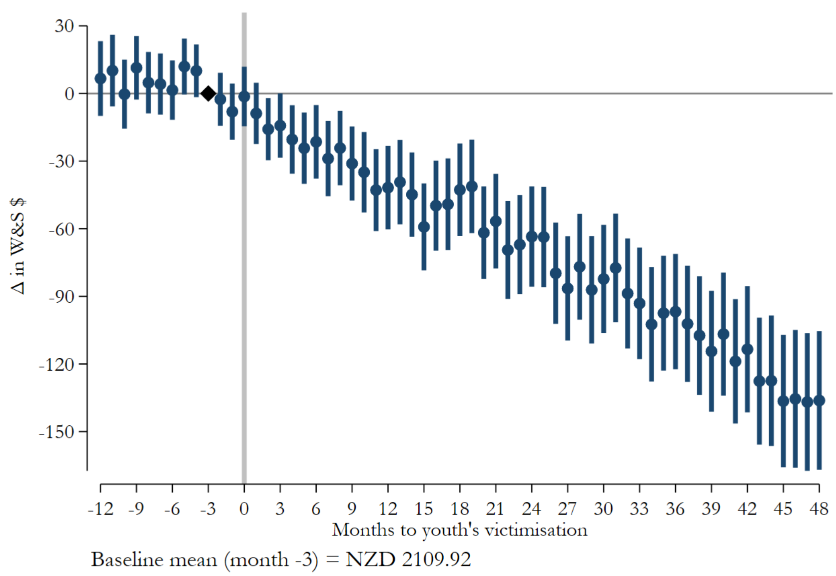
(c) Truancy interventions

Notes: This figure shows the estimates of the effect of youth assault victimisation on youth's schooling intervention rate, for any intervention (Panel C0.5a), for stand-down or suspension interventions (Panel C0.5b) and for truancy interventions (Panel C0.5c). Each series show the percentage-point impact of youth victimisation on the schooling outcome at each event time, i.e. δ_e defined in Eq.4.1. The circles are the point estimates, and the vertical bars depict 95% confidence intervals with standard errors clustered at the youth-level. The observed downward trend across months 7-12 in Panels C0.5a and C0.5b suggest an incapacitation-like effect whereby students who started the schooling interventions in the month of (and just after) the assault are already part of the intervention, so are less likely to be observed as "starting" the programme in the later months. This downward trend is not seen for the truancy-related interventions (Panel C0.5c), where the largest uptake occurs in the months following the victimisation.

Figure C0.6: The effect of youth assault victimisation on parents' labour earnings 2-years and 4-years post-victimisation



(a) 24 months post-victimisation



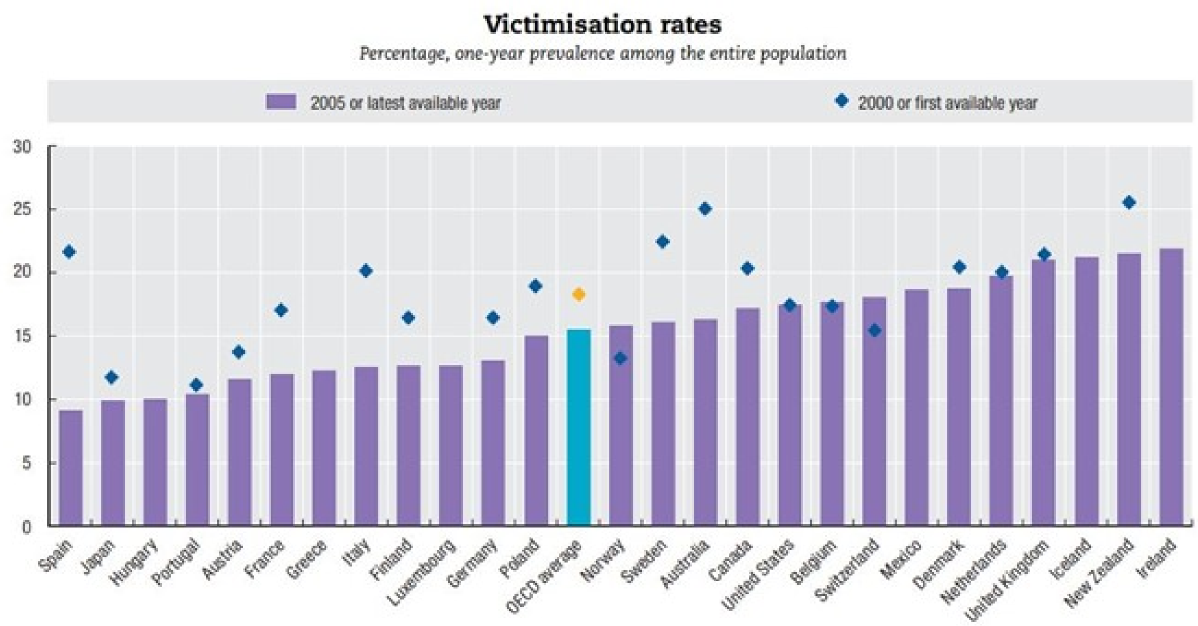
(b) 48 months post-victimisation

Notes: This figure shows the estimates of the effect of youth assault victimisation on parents' labour earnings. Panel C0.6a shows the effect over a 24-month horizon such that the comparison group is victimised in month 25, and Panel C0.6b shows the effect over a 48-month horizon such that the comparison group is victimised in month 49. Each series show the point estimates (NZD) for the impact of victimisation on parents'labour earnings at each event time, i.e. δ_e's defined in Eq.4.1 but over a longer event timeline. We normalise the comparison group's outcome to the outcome level of the treatment group three months prior to victimisation. The circles are the point estimates, and the vertical bars depict 95% confidence intervals with standard errors clustered at the youth-level.

C1 Victimisation rates in OECD countries

The OECD use the International Crime Victim (ICV) survey to compare standardised crime experiences across countries.[1] The population prevalence rate of conventional victimisations across OECD countries using self-reported ICV data from 2000 and 2005 is reprinted in Figure C1.1 below.

Figure C1.1: Percentage of OECD populations being victim of a conventional crime in the previous 12-months



Source: OECD (2009).

Figure C1.1 shows that 15.5% of the population of OECD countries self-reported being victim of a conventional crime within the previous 12 months. New Zealand has one of the highest victimisation rates, where 21.5% of the population self-reported being a victim of

[1] Comparing victimisation rates across countries and over time is a difficult task due to data availability and comparability issues between countries. Police-reported victimisations depend on the efficiency and reliability of a country's police force, such that increasing trends in police-reported victimisation could be due to a rise in victimisations or simply an increase in policing resources (OECD, 2009). Thus, the International Crime Victim Survey was created to produce estimates of victimisation that can be used for international comparison. For more information on the ICV survey, see here.

conventional crime in the last 12 months. Looking at the Northern European countries for which evidence on the impacts of reported victimisation is published, we see Norway and Sweden are about equal to the OCED average, and Denmark and the Netherlands have slightly higher victimisation prevalence rates (18.8% and 19.7%, respectively).

Of the 26 OECD countries included in the study, New Zealand also has one of the highest self-reported one-year prevalence rates of assault victimisations (4.9%), exceeded only by Iceland (5.9%) and the United Kingdom (5.4%), and equalled by Ireland. The OECD population average for a one-year assault prevalence rate is 2.9%.[2]

Not only does New Zealand have a high victimisation rate by international standards, but evidence from a national victimisation survey suggests that victimisation prevalence rates have grown over recent years. The New Zealand Crime and Victims Survey[3] shows the number of self-reported personal offences per 100 adults has increased from 59.91 in 2018 to 76.91 in 2022,[4] and the rate of interpersonal violence offences increased from 17.93 per 100 adults to 29.11 per 100 adults over the same period.[5] Taken together, these statistics on New Zealand's victimisation rates warrant it as an important context for studying the consequences of victimisation.

[2]See page 269 of the OECD Factbook 2009 (here) for a table on assault prevalence rates across OECD countries.

[3]Publicly available results tables from the New Zealand Crime and Victims Survey can be found here.

[4]"Personal offences" include theft and personal property damage, robbery and assault, fraud and deception, cybercrime, sexual assault, and harassment and threatening behaviour.

[5]"Interpersonal violence offences" includes robbery and assault, sexual assault, harassment and threatening behaviour, and personal property damage where the offender is known to the victim.

C2 Being a victim in New Zealand

Here we provide commentary on the processes victims can follow once being victimised in New Zealand. This includes how victims can be involved with the police investigation process, the support services available to victims via Manaaki Tāngata Victim Support, and details about New Zealand's Victims Code.

Victim's involvement in the investigation

When the New Zealand police open an investigation for serious crimes, a police officer will be assigned to the victim to ensure that they are given enough support and information throughout the investigation process. Victims of serious crimes can also apply to be a part of the Victim Notification Register where they can receive updates about the proceedings against their offender. The idea behind the Victim Notification Register is to improve victims' peace of mind by knowing how their investigation is going and whether their offender has been imprisoned or not (New Zealand Police, n.d.).

Support services available for victims

Manaaki Tāngata Victim Support is a first-response organisation that provides free and confidential emotional and practical support for all victims, irrespective of whether the crime was reported. Support workers will assess the eligibility of victims for financial support, help victims develop an emotional support plan, explain the justice system processes and how victims can be involved, connect victims to other support agencies, and generally advocate for the rights of victims (Manaaki Tāngata, n.d.). Guidance is also available for family members supporting children and young people who have experienced a crime.

Victims can contact Manaaki Tāngata Victim Support directly or can be referred from other agencies, such as the New Zealand Police.

In addition to the free services provided by Maanaki Tāngata Victim Support, there are some financial schemes available to victims to fund activities concerning their recovery. For example, victims who suffer injuries can get Accident Compensation Corporation (ACC) support. ACC is a no-fault scheme that aims to help people recover from physical (and sometimes mental) injuries. The scheme helps to cover the costs of a person's recovery, including payments toward treatment, help at home and work, and compensation for lost income due to the injury (Accident Compensation Corporation, 2023). The New Zealand Government legislates what can and cannot be compensated through the ACC scheme.

In general, the ACC earnings-compensation scheme is available to individuals who suffer an accident-induced injury and who, at the time of the injury, are employed, self-employed, or shareholder-employed. The injured individual is required to show a medical certificate to prove they are unable to do some or all of their work duties. ACC compensation is available from one week after the injury if the person is still not able to return to work. ACC covers 80% of lost labour earnings due to injury, which is based on the individual's earnings in the four weeks prior to injury for short-term compensation periods, or the year prior to injury for longer-term periods. If an individual's employer offers part-time work or alternative duties for the individual while they are injured, such labour earnings can be earned on top of the ACC's 80% compensation,[6] allowing the injured individual to recover up to 100% of lost earnings.

[6] As at 10 April 2024, the minimum ACC payout is 80% of the adult minimum wage and the maximum ACC payout is $2,257.16 per week. The rate of compensation is set by the Accident Compensation Act 2001.

There are extra funding opportunities available for people who have experienced sexual offences. ACC offers additional funding for people who have experienced sexual abuse and assault, covering therapy costs for the victim and the family members of the victim.[7] Victims of sexual offences and other serious crimes may also be eligible for the Ministry of Justice's Victim Assistance Scheme (VAS), which provides financial support for costs related to the crime, the justice process, and recovery.[8] VAS includes general grants, funded counselling services, and grants for travel and accommodation, court attendance, and any crime scene costs.

The principles and rights of victims

In 2014, New Zealand underwent a "Victims of Crime" reform that shifted the focus of the justice sector more toward the rights and experiences of victims. From this emerged a Victims Code[9] that sets out eight key principles for the treatment of victims. These are: safety, respect, dignity and privacy, fair treatment, informed choice, quality services, communication, and feedback. These principles apply to all victims, irrespective of whether the victimisation was reported.

The Victims Code also sets out 11 rights of victims in the criminal and youth justice systems. These rights of victims only apply to those who report the crime to the New Zealand Police or whose proceedings are heard in court (Ministry of Justice, n.d.). The rights of victims are: to be given information about programmes, remedies, and services; to be given information about investigation and criminal proceedings; to make a victim

[7] https://www.findsupport.co.nz/
[8] https://www.victimsupport.org.nz/financial-assistance
[9] https://victimsinfo.govt.nz/assets/Victims-code/Victims-Code.pdf

impact statement; to express your views on name suppression; to speak official languages in court; to get back property held by the state; to be informed about bail and express your views; to receive information and notifications after sentencing; to have a representative receive notifications; to make a submission relating to parole or extended supervision orders; and family group conferences.

Overall, New Zealand has emotional and financial support services available to victims of crime and has made efforts to prioritise the rights of victims in the justice sector. The findings of this paper further inform about the types and magnitudes of losses experienced by victims and their families in New Zealand. Such insights could contribute to cost-benefit analyses surrounding the funding and targeting of such victim support programmes.

C3 Sample construction

The population of first-time youth victims is derived from the New Zealand Police Recorded Crime Victims (NZRCV) dataset in the IDI. The NZRCV register holds records of all victimisation incidents that came to the attention of the New Zealand Police from July 2014 onward. The NZRCV dataset contains one record for each instance of a person being victimised per 1-digit ANZSOC[10] division.[11] There are 1,826,220 offence-occurrence records in the NZRCV register, of which 1,804,740 are investigated.

[10]ANZSOC is the Australian and New Zealand Standard Offence Classification codes. See here for more details.

[11]For example, if a victim suffered both an injury-causing offence (ANZSOC 02) and was robbed (ANZSOC 06) during the same victimisation incident, they would have two entries in the victimisation register for that incident. However, if the victim suffered both a serious assault (ANZSOC 0211) and a common assault (ANZSOC 0213) during the same victimisation incident, only the principal offence (the offence with the highest ranked ANZSOC code, in this case, 0211 serious assault) would be recorded in the register since both offences relate to the same 1-digit ANZSOC division (02)

We apply the following restrictions to the universe of investigated victimisation occurrences to arrive at our sample of first-time youth assault victims. First, we observe victims at their earliest recorded report of victimisation, giving 1,219,770 unique first-time victims. Second, we restrict the sample to those with non-missing age information and non-spurious death records, leaving 829,962. Third, we focus on victimisations before April 2022 (to allow a one-year follow up period post-victimisation), and on youth aged 0-30 at the time of victimisation. This results in 335,790 youth victims. Fourth, we drop youth victims who left to reside overseas during the study period, result in 314,469 first-time youth victims.

The final two data restrictions concern the type of offence suffered by the youth victim. Table C3.1 shows the types of offences experienced by the 314,469 first-time youth victims in our sample. Panel A provides the counts of first-time victims for personal offences, while Panel B provides victim counts for theft offences. We focus on personal victimisations and restrict the sample to the most common type of personal victimisations, namely assaults. There are 119,463 unique youth victims who experience either common assault (49,887), serious assault not resulting in injury (36,807), or serious assault resulting in injury (32,769).

Finally, we restrict the sample to youth victims whose assault was not a case of family violence (FV). To identify FV cases in the NZRCV data, we use a categorical variable that indicates the relationship of the offender to the victim, as reported by the victim. We define family violence by bloodline and categorise a youth victimisation as FV if the youth identified their offender as being a parent, grandparent, sibling, child, a caregiver relative, or another family member. For completeness, we also include step-parents and step-children as part of the family violence definition because although step-family members are not blood-related, they often taken on the family caregiving roles as if they were biological family. Table C3.2 presents the counts of first-time youth assault victims for non-FV cases and FV cases separately. Victims of non-FV assaults make up about 89% of all first-time youth assault victims. Our final youth victim sample comprises 106,518 first-time youth victims who experienced a non-FV assault victimisation.

Table C3.1: First-time youth victims by offence

	ANZSOC	Offence	Count
Panel A. Personal offences	02	Common Assault	49,887
	02	Serious assault not resulting in injury	36,807
	02	Serious assault resulting in injury	32,769
	03	Sexual assault	20,958
	06	Aggravated robbery	7,161
	06	Blackmail and extortion	930
	05	Abduction and kidnapping	921
	06	Non-aggravated robbery	705
	01	Driving causing death	231
	01	Murder	129
	01	Manslaughter	69
	01	Attempted murder	33
Panel B. Theft offences	08	Theft of a motor vehicle parts or contents	55,773
	08	Illegal use of a motor vehicle	49,809
	08	Theft (not elsewhere classified)	49,185
	08	Illegal use of property	7,104
	08	Theft from a person	4,002
	08	Theft from retail premises	819
	08	Theft of a motor vehicle	612
Total first-time youth victims			314,469

Notes: This table shows the types of offences experienced by 314,469 first-time youth victims who reported being victimised between July 2014 and April 2022. The post-count NZ Police Recorded Crime Victims data counts a victim once per 1-digit ANZSOC offence per victimisation occurrence. This is why the sum of counts across all offence categories exceed the total victim count since victims can be counted twice if they suffered offences from different 1-digit ANZSOC categories. Offences exclude those that are victim-less (e.g. drug offences).

Table C3.2: First-time youth victims of assault by family violence status

	Assault offence	Count
Panel A. Non-family violence	Non-FV Common assault	46,374
	Non-FV Serious assault not resulting in injury	30,432
	Non-FV Serious assault resulting in injury	27,144
	Total Non-FV assault	103,950
Panel B. Family violence	FV Common assault	3,513
	FV Serious assault not resulting in injury	6,375
	FV Serious assault resulting in injury	5,625
	Total FV assault	15,516
Total first-time youth assault victims		119,463

Notes: Counts of first-time youth assault victims by family violence status as identified in the New Zealand Police Recorded Crime Victims data. Family violence is classified as victimisation cases where the victim identified their offender as being a parent/step-parent, a grandparent, sibling, child/step-child, a caregiver relative, or a family member not further defined.

C4 Victim-offender pairs: Descriptive statistics

The richness of the IDI allows us to link the victimisation police register to the offender police register, and then to other registers to find out about the demographic make-up of victim-offender pairs, as well as the punishments received by offenders for assaulting youth in New Zealand. Descriptive statistics resulting from these linking procedures are provided below.

Demographic make-up of victim-offender pairs

We identify 51,405 unique victim-offender pairs by linking the NZRCV to the NZRCO register (the police-victim and police-offender registers) via a unique occurrence ID. Male offenders comprise nearly 77% of the 51,405 offender-victim pairs. Specifically, 41.78% of the offender-victim pairs are male-male and 34.92% are male-female, while 13.87% are female-female, and 9.43% are female-male. The three most common victim-offender pairs in terms of ethnic composition are: Māori-Māori (27.78%), European-European (17.60%), and European-Māori (15.93%).[12] Pacific Peoples are offenders in 12.69% of pairs, while this is only 4.02% and 1.93% for Asian and MELAA people, respectively.

Figure C4.1 shows the age distribution of youth victims (blue circles) and their linked offenders (red circles). By construction, youth victims are between 0 and 30 years old at the time of victimisation. The figure shows that the age-curves for victims and offenders are similar, but not perfectly overlapping and specifically there are some older offenders assaulting youth.

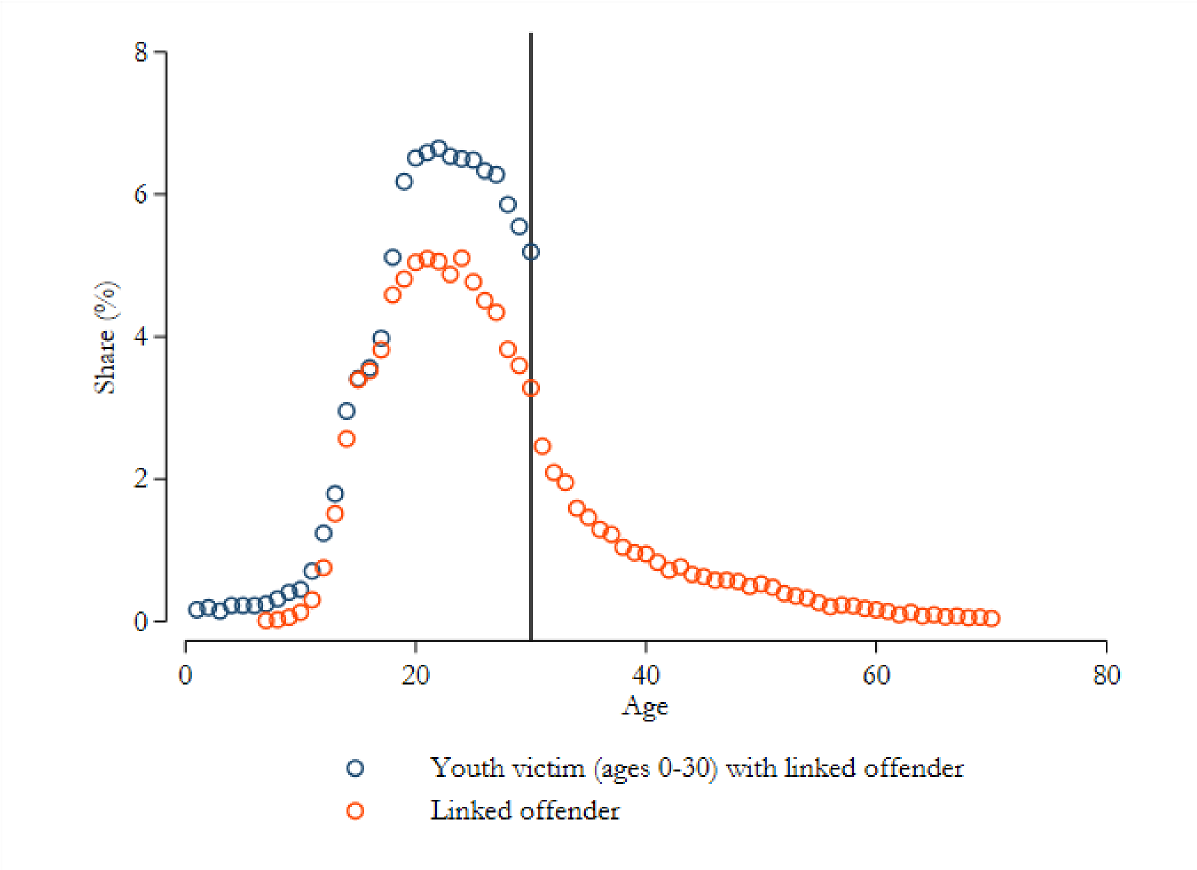
Punishments received by offenders of youth assault victims

The IDI database also includes information on those who interact with New Zealand's criminal justice system. We link the unique individual identifiers of the offenders of youth victims to the Ministry of Justice Court Charges register to observe what punishments the offenders received for assaulting youth. Of the 41,277 unique offenders proceeded against by the New Zealand Police, 27,048 (65.5%) were officially charged for the offence(s). Of those that were charged, 19,470 (72.0%) were convicted.

For each convicted offender, we observe the sentence(s) they received and the length of each sentence. Figure C4.2 shows the percentage of unique convicted offenders that received each of the eight main sentences available in New Zealand's jurisdiction. The average maximum sentence length (measured in days or hours, depending on the sentence) received by these offenders is labelled on top of each bar. The bars are not mutually

[12]Ethnicity categorisation follows Stats NZ's prioritisation rules, ranked as follows: Māori, Pacific Peoples, Asian, MELAA (Middle-Eastern, Latin American, African), and then New Zealand European.

Figure C4.1: Age distribution of youth victims and their linked offenders

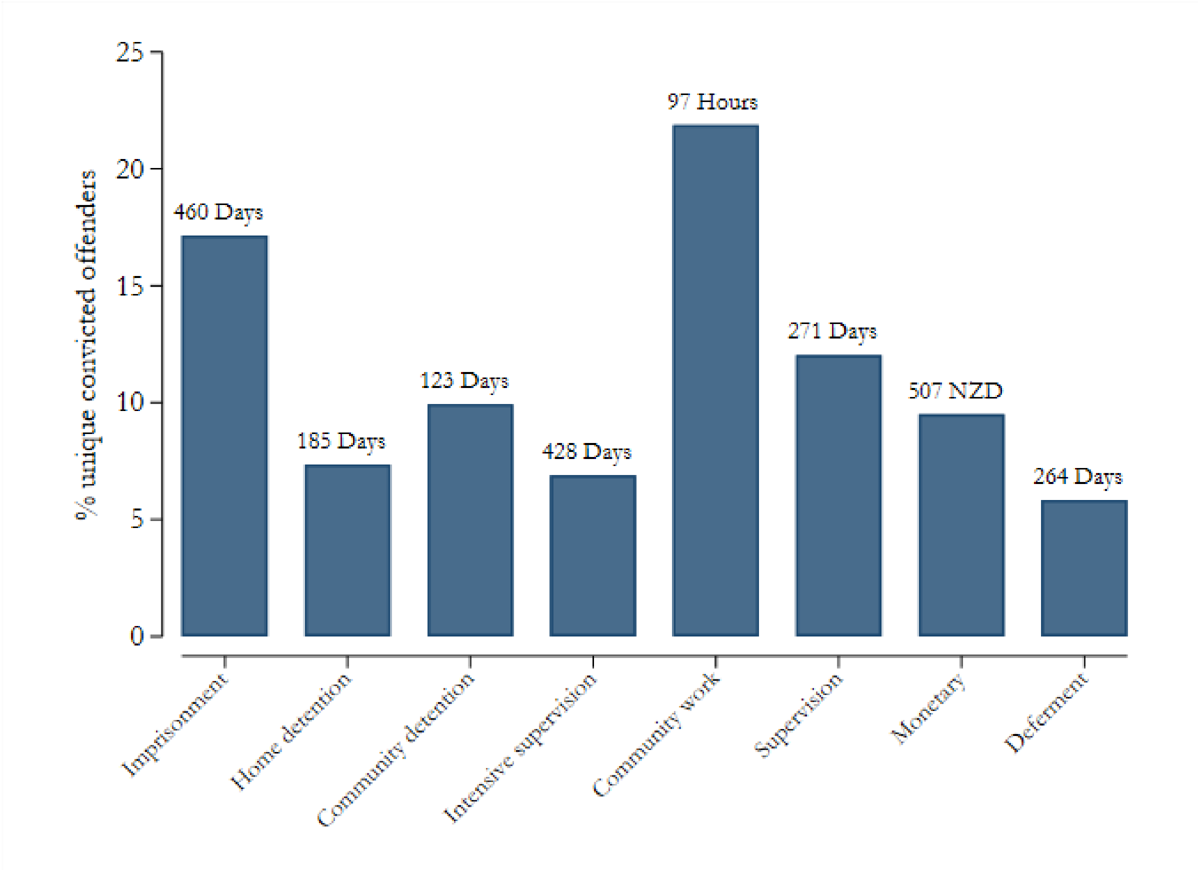


Notes: This figure compares the age distribution of youth assault victims (blue) and the age distribution of their offenders (red). Only youth who have linked offenders are included in the figure. The y-axis shows the share of the sample, and the x-axis shows the age at the time of the victimisation. By construction, youth victims ages range 0-30, while the linked offenders can be up to 70 years old. The age curves are not perfectly overlapping, suggesting it isn't always the case that it is youth assaulting youth.

exclusive since offenders can receive more than one sentence type.

Figure C4.2 shows that offenders of youth assault victims receive sentences across the full punishment spectrum. The most common type of sentence handed down to these convicted offenders is community work, affecting approximately 22% of convicted offenders with an average maximum length per community work sentence of 97 hours. The next most common sentence is imprisonment (17% of convicted offenders), of which the average maximum sentence length for these offenders is 460 days.

Figure C4.2: Sentences received by convicted offenders of youth assault victims



Notes: There are 19,470 convicted offenders of youth victims. These percentages do not sum to 100% since offenders can receive more than one type of sentence at their sentencing hearing. On top of each bar is the average maximum sentence length for each sentence type. This is calculated as a per-sentence average.

C5 Sample counts across outcomes

Overall, while we have 103,950 youth victims and 113,139 parent-youth pairs, the size of our estimation sample differs depending on the outcome analysed. For example, educational outcomes are only measured for school-aged victims, so the estimation sample for evaluating the effect of youth victimisation on schooling interventions is limited to youth victims aged 5-18. The schooling interventions data are also only updated to February 2022, so the victimisation sample must be restricted to victimisations between July

2014 (the start of the NZRCV register) and February 2021 to allow a one-year post-victimisation period. Table C5.1 summarises the data source and age restrictions associated with each youth outcome variable. Table C5.2 presents the same for the parent-level analyses.

Table C5.1: Youth victims: Sample counts by outcome of interest

Theme	Outcome	Data provider	Available to	Victimisation end date	Age group	Victim counts
Health	Hospitalisation	MOH	June 2022	June 2021	0-30	93,243
	Mental health services	MOH	June 2022	June 2021	0-30	93,243
	Prescriptions	PHA	Dec 2022	Dec 2021	0-30	99,630
Education	Schooling interventions	MOE	Feb 2022	Feb 2021	5-18	29,481
Labour market	Labour market	IRD	April 2023	April 2022	18-30	68,907

Notes: This table provides details on the data used for each of the youth outcomes included in our analysis. The table lists the data source, the outcome variable coverage period, the associated victimisation coverage period, and the unique counts of youth victims. MOH = Ministry of Health; PHA = PHARMAC; MOE = Ministry of Education; IRD = Inland Revenue Department.

Table C5.2: Parents of youth victims: Sample counts by outcome of interest

Theme	Outcome	Data provider	Available to	Victimisation end date	Youth age group	Parent counts
Health	Mental health services	MOH	June 2022	June 2021	0-30	103,152
	Prescriptions	PHA	Dec 2022	Dec 2021	0-30	109,119
Labour market	Labour market	IRD	April 2023	April 2022	0-30	113,139
Marriage	Divorce /marriage	DIA	Mar 2023	Mar 2022	0-30	112,137

Notes: This table provides details on the data used for each of the parent outcomes included in our analysis. The table lists the data source, the outcome variable coverage period, the associated victimisation coverage period, and the unique counts of parents of youth victims. MOH = Ministry of Health; PHA = PHARMAC; IRD = Inland Revenue Department; DIA = Department of Internal Affairs.

C6 Additional information: PRIMHD register

The Ministry of Health's Programme for the Integration of Mental Health Data (PRIMHD) register informs about individuals who are seen by publicly-funded secondary mental health and addiction service providers. The PRIMHD data contain dates of referrals received by the mental health and addition service provider and the start and end dates for which the healthcare user participated in the service.

There are many ways to be referred to a PRIMHD service and there are many types of organisations that provide PRIMHD services. A person can be referred to mental health and addition teams from a range of organisations, such as accident and emergency, court liaisons, Corrections or Justice or Police, hospitals, public health, the education sector, private practitioner, social welfare, etc. The types of organisations that provide the mental health and addition services include district health boards, government organisations, private hospitals, primary health organisations, non-governmental organisations, responsible authorities, charitable trusts, education and research institutions, pharmacies, general practices, and others.

Services provided by PRIMHD organisations are divided into teams. The team type determines which activities and support services are most appliable. The two most common PRIMHD teams are the community team (CMT) and the alcohol and drug (AOD) team. We look at these teams separately since they make up the largest proportion of mental health services used by healthcare users. We also separately look at the inpatient (IPT) team since this team provides some of the most serious treatments for their healthcare users, such as crisis respite care hospital beds. Activities provided by these three teams are summarised below:

Community Mental Health (CMT) Team

The CMT Team provides a range of community-based mental health activities. Such activities include group and day programme attendance services, which often use cognitive behaviour therapy, mindfulness, stress management, distress tolerance, and emotional self-regulation techniques to help healthcare users improve on their addictions, self-harm, suicidal thoughts, mood disorders, traumatic brain injuries, and eating disorders. These types of services are typically conducted in group sessions, which could be two hours, a full day, or an overnight stay in the community (e.g. a campsite). These activities can also be undertaken in cultural-specific settings, such as a marae, and can have family/whānau present and involved.

Other types of community-based activities include crisis attendances (e.g. via helplines or community mental health service providers), liaising with external mental health, court, or other support services, and providing employment and educational support to assist healthcare users in gaining employment.

Alcohol and Drug (AOD) Team

The AOD team is both hospital-based and community-based, meaning their activities are mostly a combination of the IPT team and the CMT team. One notable difference between these teams and the AOD team is that the latter also provides substance abuse attendance and detoxification beds to help healthcare users with withdrawal management in a home setting. They also provide treatment and counselling services for people involved in opioid substitution treatment (OST) services.

Inpatient Mental Health (IPT) Team

The main activities provided by the IPT team include providing bed nights for intensive care, acute, and sub-acute inpatient services, as well as crisis respite care and substance abuse and psychiatric rehabilitation beds. These are 24-hour care and treatment services in hospital-like settings that can be for short or long periods of time. Healthcare users would typically use inpatient mental health team services if their mental health condition is urgent or presents a danger to themselves or other people, if their mental health symptoms are severe, or if their requirements are unable to be met in less supported settings.

Other teams include co-existing problems teams, forensics teams, eating disorder teams, early intervention teams, forensic teams, intellectual disability and dual diagnosis teams, kaupapa Māori teams, needs assessments and service coordination teams, residential/accommodation teams, and speciality teams, intellectual disability teams, maternal mental health teams, integrated primary access and choice teams, and specialist psychotherapy teams. These teams are combined to create an “All other teams “ category for our analysis.

For more information about the activities each team provide, see the report “Guide to PRIMHD Activity Collection and Use” (Ministry of Health, 2021).

C7 Additional information: Schooling interventions register

The student interventions register contains schooling behaviour records extracted by multiple education sources and compiled by the Ministry of Education. While students can be subject to many different types of interventions,[13] the most common interventions are stand-downs and suspensions and interventions relating to truancy. These are defined below.

Stand-down and suspension interventions

A student may be subject to a stand-down, suspension, exclusion, or expulsion if their behaviour at school "constitutes gross misconduct, continual disobedience, or behaviour risking serious harm" (Ministry of Education, 2009). When a student misbehaves at school, an investigation will take place by staff member to look at what happened, how serious the misconduct was, and consider what needs to be done about it. The principal makes the final decision on whether to stand-down or suspend the student by considering whether the behaviour "set a harmful or dangerous example to other students", the student's individual circumstances, and what punishment is appropriate under such circumstances (Ministry of Education, 2009).

[13] Possible school interventions include: Early Leaving Exemption, Homeschooling, Mapihi Pounamu, Boarding Bursary, Reading Recovery, Off Site Centre, Special Education from ENROL, ORS Students, Over 19 at Secondary, High Health Students, Special School Students, Over 14 at Primary, SE Other students, Resource Teachers: Literacy, Truancy (unjustified absence), Student Health Check, Gateway, Secondary Tertiary Programme, Interim Response Fund, Teen Parent Unit, Special Education, Boarding Allowance, Aspire Scholarship, Puawaitanga Scholarship, Year 9 Plus, Resource Teacher: Learning and Behaviour, ESOL, Alternative Education, Suspension, Standard down, and Non Enrolment Truancy Service.

Truancy interventions

Truancy interventions relate to non-enrolment and unjustified absences. The Ministry of Education has an internal enrolment system, ENROL, that contains records all student enrolments. Authorised staff update students as either enrolled, changed schools, or left the school system. A student is flagged for non-enrolment truancy if they have been withdrawn from one school but failed to enrol in another school within 20 consecutive school days since their "last date of attendance". A student is considered truant if they have been unjustifiably absent for five or more full days in a school term (Ministry of Education, 2024).

When these truancy situations occur, the school can initiate an intervention involving the student and their family with the goal of improving attendance in the future. The school can also choose to submit a referral to the Ministry of Education's Attendance Service Application (ASA). ASA is made available to approved school staff, typically school administrators and principals, throughout New Zealand to record unjustified absence referrals and non-enrolment notifications. Staff are encouraged to lodge an unjustified absence referral in ASA if the student has been unjustifiably absent (without satisfactory explanation) and if the school's own efforts have been unsuccessful in returning the student to school.[14]

C8 Adjustments for multiple hypotheses testing

This section examines whether the primary storyline of the paper is sensitive to an adjustment for multiple hypotheses testing. We apply the Bonferroni correction (see, for

[14]For more information about ASA, see here.

example, Sedgwick (2012)), which adjusts the critical significance threshold (z-value) by dividing by the number of independent hypotheses tested (M).

Given the structure of this paper, defining the number of independent hypotheses (M) is not straightforward. Our analysis covers two overarching domains (youth; parents), with several sub-domains (physical health; mental health; schooling; labour market). Moreover, outcomes within sub-domains are often correlated; for example, the three mental health prescription outcomes may collectively represent a single underlying mental health hypothesis.

To mitigate this ambiguity, we present the results across a range of values of M, from 1 to 50, to see at what point, if any, the results start to unfold. For each value of M, we start with the conventional critical z-value of 1.95 (corresponding to a 5% significance level) and divide by M to adjust the significance threshold. For each tested outcome, we plot its original t-statistic and assess whether its significance status changes as the critical value increases with M.

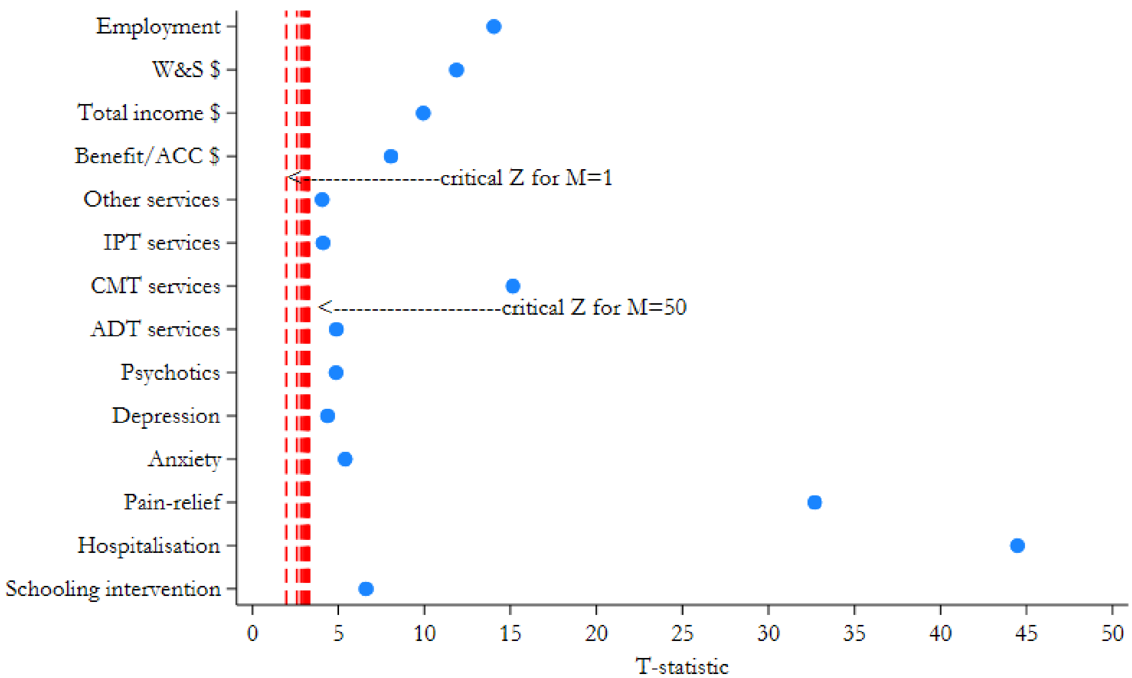
Figures C8.1 and C8.2 present the results for youth and parents, respectively. On the y-axis we list the tested outcomes, while on the x-axis we plot their original t-statistic as circle markers. Markers are blue if originally significant at the 5% level and black otherwise. The vertical dashed red lines represent the critical z-values, starting at 1.96 (for M=1) and increasing with the Bonferroni adjustment for higher M values.

Figure C8.1 demonstrates that all youth outcomes remain significant even when adjusting for up to 50 independent hypotheses. Figure C8.2 indicates that for parents, adjusting for M = 5 causes the effect on benefit/ACC earnings to lose significance, and adjusting for M = 10 reduces the labour earnings effect to marginal significance (around

10%). Otherwise, the effect on parents' employment, community mental health service use, and anti-anxiety prescriptions remain significant when adjusting for up to 25 independent hypotheses.

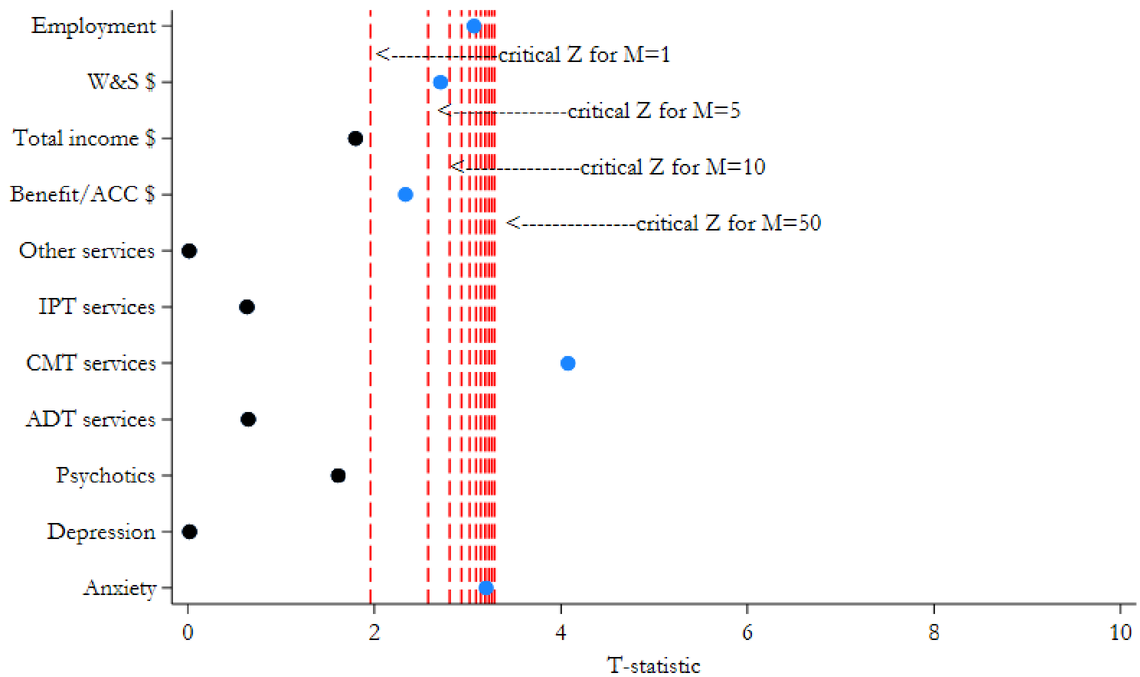
Overall, these figures suggest that applying the Bonferroni correction does not materially alter the main results of the paper. Accordingly, regardless of how the number of independent hypotheses is defined, concerns about multiple hypotheses testing are unlikely to compromise our conclusions.

Figure C8.1: Bonferroni adjustment: T-statistics and adjusted Z-values for youth outcomes



Notes: Each point represents the original t-statistic of an outcome included in the main youth results. Blue circles indicate outcomes that are statistically significant at the 5% level under the conventional threshold ($Z \geq 1.96$); black circles indicate non-significance. Vertical red dashed lines mark the increasing Bonferroni-adjusted critical values, calculated as the z-value corresponding to a 5% significance level divided by M, where M ranges from 1 to 50. Outcomes are ordered by sub-domain and plotted on the y-axis.

Figure C8.2: Bonferroni adjustment: T-statistics and adjusted Z-values for parent outcomes



Notes: Each point represents the original t-statistic of an outcome included in the main parent results. Blue circles indicate outcomes that are statistically significant at the 5% level under the conventional threshold ($Z \geq 1.96$); black circles indicate non-significance. Vertical red dashed lines mark the increasing Bonferroni-adjusted critical values, calculated as the z-value corresponding to a 5% significance level divided by M, where M ranges from 1 to 50. Outcomes are ordered by sub-domain and plotted on the y-axis.

Appendix D

Appendix material for Chapter 5

Table D0.1: Summary statistics: Children's physical health indicators before and after being assaulted by a parent

	(1) Hospitalisation	(2) Pain-relief
Baseline	1.35 (0.22)	7.33 (0.50)
Month of assault	5.07 (0.42)	8.75 (0.54)

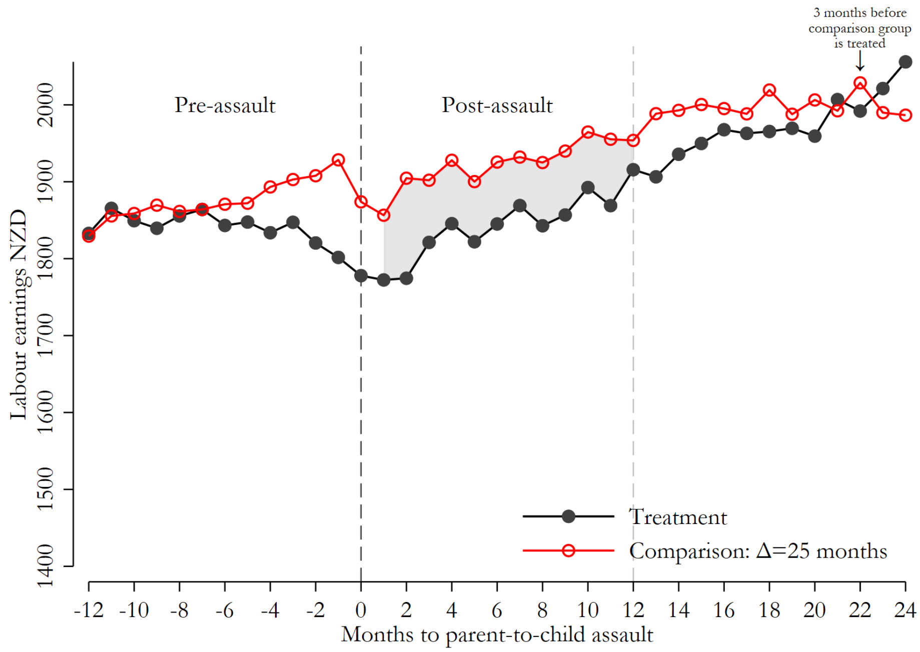
Notes: This table shows the percentage of reported child victims assaulted by a parent who went to hospital (column 1) or filled out a prescription(s) for pain-relief medication (column 2). In total, there are 2,895 child victims. We provide the percentages measure at baseline (seven months prior to assault) and at the time of the assault. Standard errors are shown in parentheses. The numbers show an uptake in both physical health indicators when the assault occurred.

Table D0.2: Summary statistics: Parents whose child was assaulted by a parent versus general population of parents

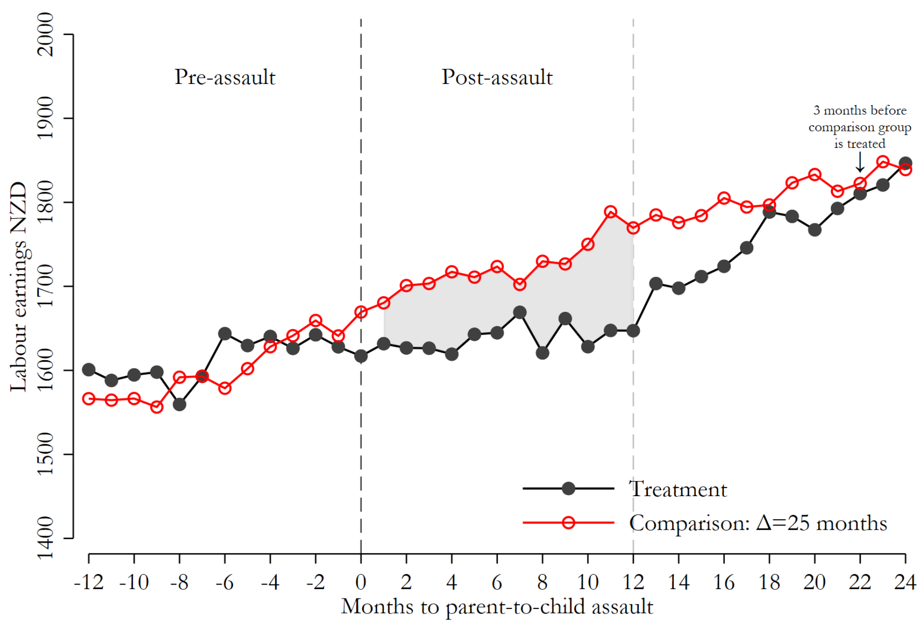
Characteristic	(1) Parents of children assaulted by a parent	(2) General population of parents
Parent age	38.55 (0.25)	39.76 (0.01) ***
Age of child	8.63 (0.13)	5.41 (0.00) ***
Ethnicity: European	32.08 (1.74)	55.96 (0.06) ***
Ethnicity: Māori	35.42 (1.79)	17.46 (0.05) ***
Ethnicity: Pacific	18.75 (1.46)	7.28 (0.03) ***
Ethnicity: Asian	12.08 (1.20)	15.82 (0.04) ***
Ethnicity: Other	2.08 (0.53)	3.49 (0.02) **
No qualifications, Census 2018	17.92 (1.43)	9.80 (0.04) ***
High school qualification, Census 2018	48.75 (1.86)	49.76 (0.06)
Bachelor's and above qualifications, Census 2018	10.00 (1.12)	32.42 (0.06) ***
Missing Census 2018 education information	23.33 (1.58)	8.02 (0.03) ***
NZ Deprivation Index	7.36 (0.11)	5.40 (0.00) ***
NZ Deprivation Index 2018 missing	16.67 (1.39)	4.70 (0.03) ***
Labour earnings in 2018	23203.00 (1200.19)	44321.41 (65.45) ***
Benefit earnings in 2018	5298.40 (282.34)	1552.61 (5.82) ***
Total counts	720	660816

Notes: Column 1 shows 2018 descriptive statistics for parents whose child was assaulted by a parent (where the reported assault occurred in 2019) and column 2 shows the same for the general population of parents of 0–12-year-olds (measured in 2018). The NZ Deprivation Index is an area-based measure of deprivation, with 1 representing the least deprived areas and 10 representing the most deprived. Asterisk in column 2 indicate significant differences between column 2 and column 1. Despite a 1.2-year age difference between the two parent groups, the same significant differences across characteristics remain even when using a matched sample with the same age distribution as parents of assaulted children. Standard errors are shown in parentheses. Significance is shown at conventional levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure D0.1: Labour earnings profiles of treatment and comparison groups: By parents' offending status



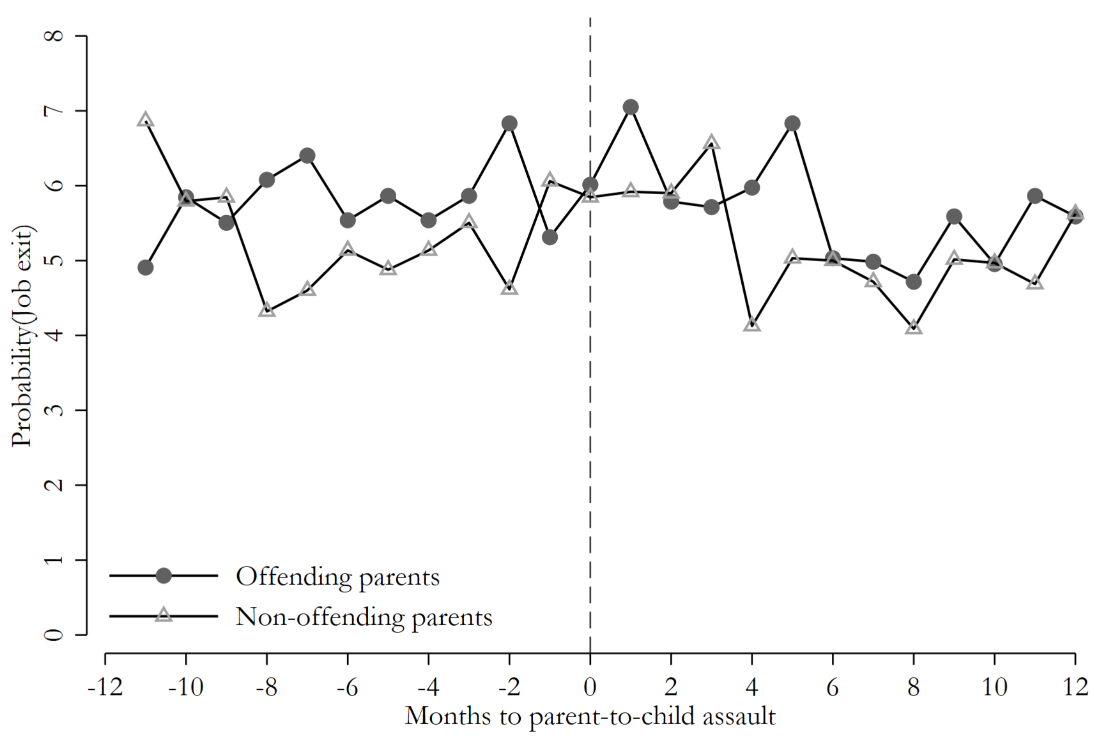
(a) Offending parent



(b) Non-offending parent

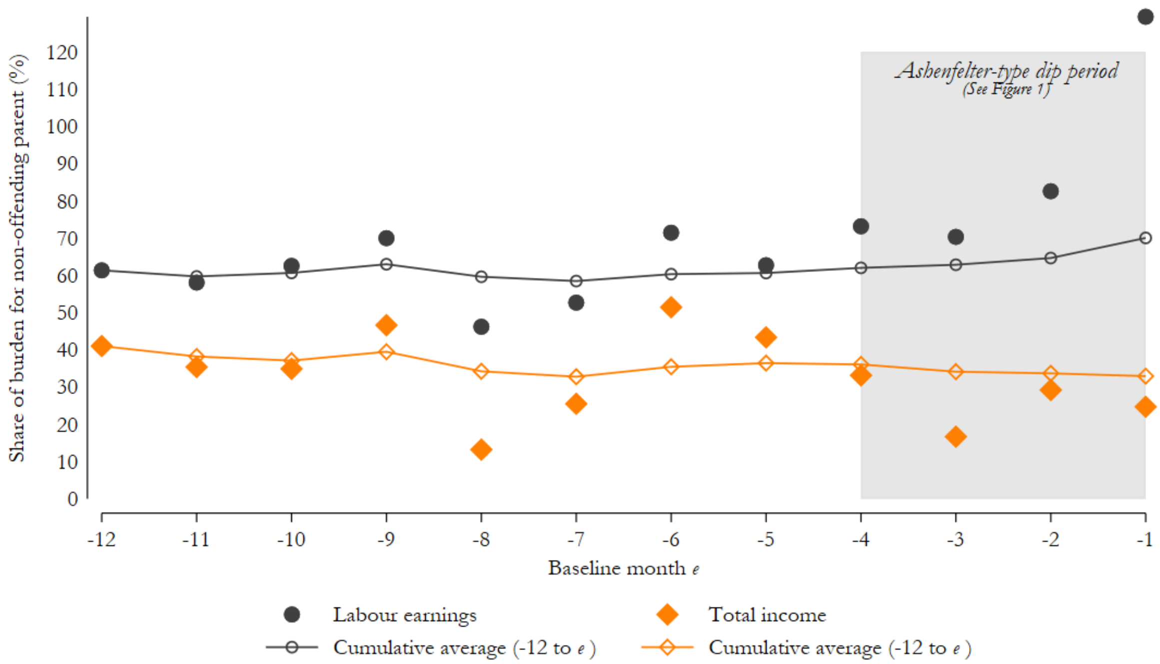
Notes: This figure plots the labour earnings trajectory of the treatment group and the comparison group over the treatment group's event timeline, where the reported parent-to-child assault occurred in month 0. Panel A presents this for offending parents and Panel B for non-offending parents. The comparison group is defined as those who experience the same reported victimisation shock 25 months in the future.

Figure D0.2: Job exit rates by parents' offending status



Notes: This figure plots the likelihood of job exits for treated parents over the treatment group's event timeline, separated for offending and non-offending parents. The job exit indicator equals one if the parent was employed in the previous month but was not employed in the current month, and equals zero if the parent remained employed across both months. Thus, there is no average job-exit rate for month -12 since we must observe the employment indicator in the month prior.

Figure D0.3: Share of economic burden between non-offending and offending parents by choice of baseline



Notes: This figure shows the share of annual labour earnings (circles) and total income (diamonds) responses to parent-to-child assaults between non-offending and offending parents, varying the choice of baseline from month -12 to -1 in Eq.5.1. The solid markers are the shares resulting from each baseline specification, while the hollow markers represent the average share cumulating across baseline months -12 to -1. Shaded light grey area highlights the baseline months that Figure 5.1 identified as being affected by an Ashenfelter-type dip.

D1 Criminal consequences for parents who assaulted their child

We link the offending parent from the police offender records into the Ministry of Justice court register to observe what punishments they received after assaulting their child. Of the 2,304 offending parents proceeded against by police, 774 (34%) were changed with an assault offence(s) and 513 were subsequently convicted (a conviction rate of 66%).

For each convicted parent, we observe the sentence(s) they received at court. 22% of convicted parents received imprisonment sentences (4.9% of all offending parents), with an average maximum sentence length of 449 days. Another 22% of convicted parents received supervision sentences, averaging 274 days. Community work (16%), community detention (13%), intensive supervision (11%), and home detention (8%) sentences were also handed out.