Domestic Violence Reports and the Mental Health and Well-being of Victims and Their Children*

Manudeep Bhuller Gordon B. Dahl Katrine V. Løken Magne Mogstad

Abstract: We study the costs associated with domestic violence (DV) in Norway by comparing outcomes before and after a DV report, using those who will be victimized in the future as controls. A DV report is associated with increased mental health diagnoses for both victims and their children and reduced financial resources. Victims experience marital dissolution, more doctor visits, lower employment, reduced earnings and higher use of disability insurance. Their children are more likely to receive child protective services and commit a crime. Using a complementary RD design, we find declines in children's test scores and grade completion.

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*Manudeep Bhuller is a Professor at University of Oslo, manudeep.bhuller@econ.uio.no. Gordon B. Dahl is a Professor at UC San Diego, gdahl@ucsd.edu. Katrine V. Løken is a Professor at Norwegian School of Economics, katrine.loken@nhh.no. Magne Mogstad is a Professor at University of Chicago, magne.mogstad@gmail.com. The authors received generous financial support from the European Research Council (ERC Starting Grant 727579).

Data availability statement: The data employed in the analysis were drawn from a series of Norwegian administrative registers, including sensitive data on crime, victimization and mental health. Due to personal data protection regulations, these data cannot be made publicly available. The data analysis for this research was conducted at restricted access servers at Statistics Norway. The data protection impact assessment (DPIA) for this research was performed in collaboration with the Data Protection Officer at Statistics Norway and is stored under file no. 2021/4279. Researchers can gain access to the data by submitting a written application to the data owners. Applications must be certified by the Data Protection Officer at Statistics Norway and/or the Norwegian Data Protection Authority in order to ensure that data are processed in a manner that protects the personal integrity of individuals surveyed. Conditional on this approval, Statistics Norway will then determine which data one may obtain in accordance with the research plan. Inquiries about access to data from Statistics Norway should be addressed to: mikrodata@ssb.no. More information is available at:

https://www.ssb.no/en/omssb/tjenester-og-verktoy/data-til-forskning

1 Introduction

Domestic Violence (DV) is a prevalent phenomenon worldwide, with almost one third of women reporting some form of physical or sexual violence by a partner in their lifetime (WHO, 2021). While slightly lower in high income countries, lifetime rates of intimate partner violence are still around 25%. This raises important questions about the mental health and well-being effects on both victims and their children. Yet despite the prevalence and seriousness of domestic violence, little is known about its effect on victims, and even less is known about its impact on children.

Estimating the consequences of domestic violence has proven challenging for several reasons. The first is data availability. The ideal dataset would be a long and representative panel with victimization information linked to a variety of mental health and well-being outcomes for victims and their children. However, actual datasets on DV are generally limited to either self-reports from retrospective surveys or reports to police. Moreover, in most countries, even this type of data cannot be merged with the relevant outcomes. A second challenge is the threat to identification from correlated unobservables, since families reporting domestic violence may have had worse mental health and well-being even in the absence of domestic violence.

Our study makes progress on these challenges in the context of Norway, offering new insights into the costs of domestic violence on both victims and children. Our work draws on several strengths of Norwegian registry data. We construct a panel dataset containing complete administrative records of all police reports related to domestic violence over a 22 year period. We are able to link this data to a rich set of outcomes in both the short and medium run. Importantly, we can match victims to their children.

To address the identification challenges, for most of our outcomes, we take advantage of our individual-level panel data to compare the outcomes of victims (and their children) before and after the reporting of a DV event. These within-person comparisons eliminate any time-invariant heterogeneity. Nevertheless, a potential concern is that outcomes may have changed over time for reasons other than the DV event, such as calendar time. To address this concern, we take a second difference using families who will report a domestic violence event in the future as a control group for families who report a domestic violence event today. Once we condition on age, the results based on the within person comparisons and the difference-in-difference design are remarkably similar. This suggests that whether and how we control for time-varying factors (other than age) matters little for our results.

To study child school outcomes, we use a different design, as the outcomes are only observed once. In this case, we take advantage of the timing of the DV report, leveraging whether it occurs just before or just after students take a consequential nationwide exam which is used for admission to high school. We employ a regression discontinuity (RD) design to compare exam scores before versus after the reported DV event date, where the running variable is the test date minus the date of the DV reporting event. We use a similar design to study whether the child completes the first year of high school on time.

Interpreting our treatment (i.e., the reporting event) and the resulting treatment effect estimates is complicated for several reasons. First, treatment could reflect violence which was either more severe or different (e.g., someone saw it) and hence resulted in a police report.[1] Second, the report itself could trigger access to shelters, social support programs or child services. Our estimates will capture both of these effects. Third, not all DV is reported to the police, and unfortunately, we are unable to study the effects of unreported DV. Relatedly, reported DV may reflect a longer buildup of violence and conflict within the household. If this is the case, the treatment effects we estimate capture the incremental increase in violence at the time of a report plus the reporting effect. However, if this is the case, we would also expect to see differential pre-trends in outcomes, which we generally do not.

Our analysis yields three main sets of results. First, we find sizable and sharp changes in the home environment following a DV reporting event. A large fraction of victims sever their relationship with the offender, with marital dissolution increasing by 50% relative to the mean and entry into marriage falling by 79%. Accompanying this change in household structure, there is a drop in the financial resources available to victims and their children; per capita household consumption expenditure (using EU weights for adults and children) drops by 9%. Most of this is due to a drop in spousal income which is not made up by higher transfer payments (which include government programs and private alimony/child support payments).

Second, we find large changes in mental health. For victims, there is a 35% increase in mental health diagnoses in the year of the event, with a tapering off after 3 years. Drilling down into specific mental health conditions, victims see statistically significant increases in mood, depression, sleep and anxiety disorders in the short run. For children, there is an immediate 19% increase in mental health diagnoses, with a sustained average increase of 15% in the four years after the event. Broken down by type of condition, children experience statistically significant increases in mood, depression, sleep and anxiety disorders in the longer run.

Third, changes along other dimensions suggest a decline in well-being for both victims and their children. For victims, there is a short run increase in the number of doctor visits (11%), and longer-run decreases in employment (-4%) and earnings (-5%). There is also an accompanying rise in the use of disability insurance which closely mirrors the drop in employment. For children, we find additional impacts on the use of child protective services (63% increase) and youth crime (54% increase). Using a complementary RD design, we also find a sharp drop in exam scores (-8%) for children taking the test immediately before versus after a DV reporting event, and a similar decline in the probability of completing the first year of high school (-8%).

OLS estimates which do not leverage the panel nature of the data, but instead compare victims to non-victims, yield even larger impacts. Even after controlling for observable characteristics and lagged outcomes, OLS effect sizes are generally larger by a factor of two or more. This comparison highlights the importance of controlling for time-invariant unobserved heterogeneity and the difficulty of accounting for selection bias into victimization using cross-sectional data.

Our paper contributes to a literature across the social sciences on the consequences of DV for children and their families. How children exposed to DV fare later in life has been an important area of research, particularly in developmental psychology (see, e.g., early studies by McCloskey et al. (1995), Sternberg et al. (1993) and Hughes (1988)). However, most of this research is based on cross-sectional data and often uses small, non-representative survey samples. It also relies on self-reported, retrospective DV exposure measures, and hence faces serious measurement challenges. For extensive meta-analyses of the developmental psychology literature documenting associations between DV and later life child outcomes see Vu et al. (2016), Evans et al. (2008), Holt et al. (2008) and Kitzmann et al. (2003). For reviews of exposure measures used in this literature, see Latzman et al. (2017), Edleson et al. (2007) and Holden (2003).

Recent work in economics focuses on a specific type of domestic violence affecting children: arguably exogenous episodes of violence during pregnancy. To assess impacts on child outcomes, administrative birth records are linked to police crime records (Currie et al., 2022) and data on hospitalizations (Aizer, 2011).[2] In other studies, economists have documented how children's exposure to DV creates negative spillovers on their classroom peers' test scores and school outcomes (Carrell and Hoekstra, 2010, 2012) and future college outcomes and earnings (Carrell et al., 2018).[3]

More broadly, our paper contributes to a literature assessing the consequences of criminal victimization; see Bindler et al. (2020) for an overview. Most closely related to our study, Bindler and Ketel (2022) link police records to victim's labor market outcomes and provide short- and long-term estimates using event studies, differentiating between DV and non-DV victimization. Using hospitalization records, Ornstein (2017) compares victims of DV assaults to non-assault patients. Using survey measures of reported life satisfaction, Johnston et al. (2018) and Cohen (2008) provide estimates of compensating income equivalents for violent and home burglary victimization, respectively, while Peterson et al. (2018) provide estimates of the lifetime economic burden of DV violence. Further, Dustmann and Fasani (2016) and Cornaglia et al. (2014) explore how local area crime affects victims' and nonvictims' mental health.

One of the contributions of our study relative to the existing literature is the ability to look not only at a victim's outcomes, but also their children's mental health, crime and educational outcomes over an extended period.[4] The negative effects of a DV event are large and generally persistent, both for the victim and their children. This is despite well-funded social support programs for victims in Norway, suggesting that DV events have negative consequences which are difficult to fully ameliorate ex post. Hence, policies or changes in societal norms which could prevent DV in the first place could have a high social return.

The remainder of the paper proceeds as follows. We begin by describing our setting and data. We then lay out our research designs and discuss how our estimates should be interpreted. In Section 4 we present our estimates for victims, their children and the home environment.

2 Setting and Data

2.1 Domestic violence in Norway and around the world

We begin by describing our setting of DV in Norway, highlighting both similarities and differences compared to other countries. Domestic violence is a worldwide problem. The Organization for Economic Co-operation and Development (OECD) has collected data on the lifetime prevalence of physical and/or sexual violence against women by a partner in countries where official national statistics are available (OECD, 2022). While DV disproportionately affects women in low- and middle-income regions (Africa 37%, Eastern Mediterranean 37%, South-East Asia 38%), it is also strikingly prevalent in wealthier regions (Americas 30%, Europe 25%, Western Pacific 25%). Zooming in, for Norway the lifetime prevalence of intimate partner violence is 27%, with a 6% prevalence in the last 12 months. These rates are similar to other first world countries. For example, the lifetime and 12-month rates are 29% and 5% in the United Kingdom, 22% and 3% in Germany, 26% and 5% in France, 19% and 6% in Italy and 28% and 5% in Sweden. If anything, Norway's DV rates are on the high side given their level of income.

A high rate of DV exists in Norway despite its reputation as a relatively gender-equal society. Figure 1 plots the lifetime prevalence of DV for over 100 countries included in the OECD data against the ratio of female-to-male labor force participation. Norway has more gender equality compared to most countries by this measure, yet DV is still high. More generally, a higher rate of women in the labor force in a country does not appear to reduce the incidence of DV. While this could reflect a greater willingness to report, it nonetheless highlights that DV is a widespread problem, with no country being immune.

All developed countries have a variety of programs to address the problem of domestic violence, although the intensity of programs varies across countries (see the UN Global Database on Violence Against Women (UN, 2021)). Norway is a country with a particularly extensive set of public services available to DV victims (Norwegian Police Directorate, 2022). There are publicly provided shelters, victim compensation schemes and efforts to make victim addresses confidential. There are also generously funded child protective services, including subsidized or free family counseling and in-home social support services, as well as foster care and adoptive programs for children in abusive situations.

In Norway there are crisis centers which specifically target DV, catering to the urgent needs of temporary shelter, food and basic utilities. Besides overnight stays, these crisis centers are open for day visits and provide information and counseling services. By law, all local municipalities have been required to provide these services since 2011. At present, there are 50 such centers spread across Norway which provided 60,000 overnight stays and 2,200 day visitors in 2020. Migrants and other women who lack a social network and support outside of their family tend to be overrepresented. Depending on the nature of a victim's health situation and urgency, there is also free emergency health care and treatment at local public health stations.

The government also provides free support to DV victims through support centers for victims of crime and the Office for Compensation of Victim of Violent Crimes (OCVC). The support centers assist victims of DV and other types of violent crime with information, legal guidance and help accessing services (e.g., help filing police reports and filling out applications to the OCVC). According to the Norwegian National Crime Survey 2020 (Løvgren et al., 2022) one in five women experiencing severe violence contacted either a support center or a public health station. The OCVC processes and grants funds for victim compensation, subject to a police report being filed.

Our findings should also be interpreted in the context of a generous welfare system more generally. There is publicly provided health insurance, although waiting times for mental health services could be an obstacle. There are also sickness benefits, unemployment insurance, disability insurance and a variety of extra support programs for families with children (e.g., paid parental leave and subsidized childcare).

2.2 Data

Our project draws on several strengths of Norwegian registry data. We create a panel dataset containing complete administrative records of all police reports related to domestic violence over a 22 year period (1997-2018). We merge this data with administrative register data maintained by Statistics Norway. This rich longitudinal database covers every resident from 1967 to 2016. For each year, it contains individual demographic information (including sex, age and number of children), socioeconomic data (including years of education, earnings and employment), as well as geographical identifiers.

This panel dataset is possible due to the ability to link datasets based on unique identifiers for each individual. The victimization dataset comes from the Norwegian Police Directorate and contains information for all reported victimizations to the police over the period 1997-2018. We observe the date of the report, date of the crime, type of victimization and the location. We link this information with administrative data that contains complete records of all criminal charges and suspected offenders. Finally, we merge these criminal justice datasets with the administrative registers provided by Statistics Norway.

To measure domestic violence events, we use data that records victims and suspects of all criminal acts recorded by police. One nice feature of this data is that suspects are included even when no charges are filed. However, as in most crime datasets, there is no formal category which is labeled as "domestic violence." We therefore define DV as a suspected crime in which (i) at least one suspect is or has been in a relationship with the victim and (ii) the crime type is one we consider to be related to DV. Our definition of a relationship includes past and present partners as well as parents of current or future children. This definition excludes crimes against women not perpetrated by an intimate partner. For crime types, we require the crime to be coded as some form of violence, harassment, threat, sexual abuse, nonsexual abuse or specifically family-related crime. Our measure of domestic violence is a dummy variable equal to one if an individual is victimized in at least one such crime in a given calendar year.[5] We follow victims and children over time, and define family outcomes based on who is living with the victim in each period.

More importantly for the present study, we received permission to link these data to health and well-being outcomes for both the victim and their children. We have these data for the period

2006-2019, and use diagnosis codes for physician visits to separate out mental health versus physical health related visits and different types of mental health diagnoses.

We refine the data along several dimensions. All event study outcome variables are defined at the yearly level. When there are multiple domestic violent events in the same year, we treat the first event as the relevant observation.[6] For the same DV event with more than one victim, we keep all victims, clustering our standard errors at the family level when needed. For the same DV event with more than one offender, we keep all offenders.[7] This rarely occurs for domestic violence, but is more common for other crime types.

To facilitate the use of our DiD design, we restrict our sample to families without a DV report for the prior four years. This means that our results are not necessarily externally valid for families with repeat offenses in shorter time frames.[8] This effectively limits the sample to DV events occurring between 2001-2015 for most of our outcomes, and between 2007-2015 for our mental health outcomes. We further restrict our main sample to offenders and victims who are age 30-50 at the time of the event. We do this primarily because most families have children at home during these ages.

2.3 *Descriptive statistics*

Our main estimation sample contains 17,163 domestic violence events occurring between 2001 and 2015. As is well known, DV disproportionately affects women, with females comprising 83% of victims in our sample.[9] Our outcome variables for victims are mental health diagnoses, number of doctor visits, employment, earnings and disability insurance participation. For the children of victims, our outcomes are mental health diagnoses, the use of child protective services, placement in foster care, criminal charges, exam scores on a national test and completion of the first year of high school. We also look at measures related to the home environment, including victim entry and exit into marriage, moving, per capita household consumption expenditure and spousal earnings. We provide definitions and summary statistics for each of these outcomes when we discuss our estimates in Section 4.

For now, we document the typical time patterns for a few of our key victim and child outcome variables. In Figure 2, the solid line in panel (a) plots whether the victim has received a diagnosis for a mental health condition in a given year during the nine years before and after the reporting of a domestic violence event. The dashed line plots the same outcome for a matched set of control individuals, where we use one-to-one propensity score matching based on age, gender and immigrant status. For this descriptive exercise, the matched controls are non-victims who do not experience a DV event in any year.

The first fact which emerges is that victims have a substantially higher probability of receiving a mental health diagnosis long before the reporting of a DV event. Nine years before the DV event, 35% of victims have a mental health condition compared to only 20% of non-victims. This is despite matching important demographic controls as age, gender and immigration status. The second fact is that there are trend differences prior to the event. While matched non-victims experience a gradual increase in mental health diagnoses over time as they age, victims are on a much steeper trajectory. Third, in the year of the DV report, victims see an upward spike in mental health diagnoses. This could reflect either (i) increased trauma due to DV abuse or (ii) increased recognition of a mental health condition after a police report. Either way, the divergent trends prior to treatment suggest the two groups are not comparable. Panel (b) plots time patterns for victim employment. As in panel (a), the mean levels of employment are different. While the two groups have rising and similar pre-trends for much of the pre-period, two years before and continuing after the DV event, victims' employment rates flatten. This stands in contrast to matched non-victim employment, which continues to rise as women age and their children get older on average.

Panels (c) and (d) report similar time patterns for two of our child outcomes. The first graph plots the fraction of children receiving a mental health diagnosis. While the levels differ, the pre-trends are fairly similar for children of victims compared to children of matched non-victims. There is also a noticeable jump in diagnoses for treated children at the time of the event. The second graph plots the use of child protective services. There is a sharp rise in child protective services at the time of the event, which could be due to increased trauma to the child or because a police report triggers an intervention by social workers. In contrast to panel (c), the pre-trends indicate the matched controls are not a good comparison: child protective service use is relatively flat for the matched controls, but has a strong upward trend for children whose parents will be victims.

In addition to describing our data, the graphs presented in Figure 2 highlight the hazards of using difference-in-difference estimators based on a matched set of controls which never experience a DV event. While pre-trends line up somewhat in panels (b) and (c), they diverge sharply for panels (a) and (d). A similar lack of parallel pre-trends (and divergent levels) is found for several of our other victim and child outcomes, as shown in Appendix Figure A1. This motivates the differences-in-differences (DiD) design described in the next section, where we compare victims and their children who experience a DV reporting event today to those who will experience a DV event in the future. As we show, using this type of comparison group eliminates pre-trend differences.

3 Research Designs

3.1 *Difference-in-differences*

Our goal is to describe the pattern of outcomes for victims and their children before and after a DV report. For most of our outcomes, we use a DiD design which exploits variation in the timing of a DV report, with individuals who will experience a DV event in the future serving as controls.

The logic behind our design is to take advantage of individual-level panel data to compare the outcomes of individuals (and their children) before and after the reporting of a DV event. These within-person comparisons eliminate all observed and unobserved time-invariant heterogeneity. The pre-event outcome of an individual is used as the estimate of her own counterfactual post-event outcome in the absence of an event. A remaining concern is that the outcome may have changed over time for reasons other than the event itself, such as age or calendar time effects. To address this concern, we take a second difference by adding the additional control group of individuals with a similar set of characteristics who will experience a DV event in the future. Later in the paper, we will compare these DiD estimates to first difference estimates which use many cohorts to flexibly control for time-varying covariates.

We use a potential outcome framework to describe our research design. Let $D_{i,t}$ be a dummy variable for whether a family member i has experienced a DV reporting event by period t. We can write the observed outcome $Y_{i,t}$ in terms of the potential outcomes as $Y_{i,t} = D_{i,t}Y_{i,t}(1) + (1 - D_{i,t})Y_{i,t}(0)$, where $Y_{i,t}(1)$ and $Y_{i,t}(0)$ denote the potential outcomes with and without a DV report by period t. To understand the treatment effect we want to estimate, start with one cohort of victims who report a DV event in year c. For this specific cohort, the treatment effect in any post-DV reporting year $c + m$ is defined as the expected difference in potential outcomes:

$$\theta^{c,m} = E[Y_{i,c+m}(1) - Y_{i,c+m}(0) | i \text{ reports DV in } c] \tag{1}$$

The challenge in estimating this parameter is that there is a missing data problem: we do not observe the average outcome for DV reporters from cohort c in year $c + m$ if they had counterfactually not reported, $E[Y_{i,c+m}(0) | i \text{ reports DV in } c]$.

One approach to deal with this issue is to use a differences estimator, comparing average outcomes m years after a reporting event in year c to average outcomes n years before the reporting event:

$$E[Y_{i,c+m} - Y_{i,c-n} | i \text{ reports DV in } c] \tag{2}$$

The problem with this approach is that other things could be changing over time which directly affect outcomes, such as changes in economic conditions. However, confounding time effects can be differenced out if one can find a control group which would have experienced the same change in outcomes between $c+m$ and $c-n$, but who did not report by year $c+m$. The DiD estimator is:

$$E\left[Y_{i,c+m}-Y_{i,c-n}\middle|i \text{ reports DV in } c\right]-\ E[Y_{i,c+m}-Y_{i,c-n}|i \text{ has not reported DV by } c+m] \qquad (3)$$

The first term in this expression differences out time invariant individual characteristics by comparing the same victim before and after a DV report. Differencing off the second term accounts for common time effects.

We use families that will experience a reporting event in the future as controls, using only years before they report (see Fadlon and Nielsen (2021) for an example of this idea). The logic for using this control group is based on the insight that families which never experience a DV report are markedly different, both in levels and trends, as shown in Section 2.3. As we will show, this control group of later reporters displays parallel pre-trends compared to the treatment group for almost all victim and child outcomes.

We implement our DiD estimator in a regression framework, following Callaway and Sant'Anna (2021). For each cohort *c* and each event time *m*, we create a subsample of treated individuals who report in year *c* and a control group who do not report by either period *c* or period $c+m$, whichever is greater. For each of these subsamples, we estimate:

$$Y_{i,t}=\alpha^{c,m}+\beta^{c,m}\mathbf{1}\{i \text{ reports in } c\}+\delta^{c,m}\mathbf{1}\{t=c+m\}+\theta^{c,m}\mathbf{1}\{i \text{ reports in } c\}\mathbf{1}\{t=c+m\}+u_{i,t}^{c,m} \quad (4)$$

Where $\alpha^{c,m}$ is the control group mean in the baseline year, $\beta^{c,m}$ is a fixed effect for treated individuals in cohort *c*, $\delta^{c,m}$ is a fixed effect for event time *m*, and $\theta^{c,m}$ is the parameter identified in equation 1. We estimate the model separately for each cohort *c* and then take the average of the estimates at each event time *m*, weighted by cohort size. This approach avoids the problems identified by Callaway and Sant'Anna (2021), Sun and Abraham (2021) and Chaisemartin and D'Haultfoeuille (2020) and ensures that our estimates are positively weighted averages of treatment effects. This approach also allows the effect of a DV reporting event to have a heterogeneous impact based on when it occurs. We estimate the model in a single, fully interacted step to allow for easy calculation of standard errors using the approach described in Novgorodsky and Setzler (2019).

To improve the comparison of the treatment and control groups, we use 1-to-1 propensity score matching on age, gender and immigrant status (in period $t-2$) to account for different distributions over time between the treatment and control groups. We match on a limited number of variables for two reasons. First, the cell sizes otherwise get small, and second, it turns out that matching on these variables does a good job at eliminating any pre-trends. The age adjustment is particularly important when using variables which have a steep age gradient, such as earnings. We focus our analysis on DV cases which are closer to being "first reports"; specifically, we condition our sample to not have any DV reports in the prior four years. We further restrict the sample to have no more than a 7 year gap between any two comparison periods.

3.2 *Regression discontinuity*

To study child school outcomes, we use a different design, as each outcome is only observed once. In this case, we take advantage of the timing of the DV report, leveraging whether it occurs just before or just after a consequential nationwide test taken at the end of compulsory schooling (the year the child turns 16). The nationally administered exams combined with a student's GPA are used to determine admission to high schools. We similarly leverage whether the timing of a DV report occurs just before or after the expected date of graduation from compulsory school; for brevity, in what follows we describe the RD design for the exam outcome; the logic is similar when the outcome is on-time completion of the first year of high school.

We employ a regression discontinuity (RD) design to compare outcomes before versus after the DV reporting event, where the running variable for child i, x_i, is the test date minus the date of the DV reporting event. Our RD specification for exam performance, $Y_{i,t}$, is:

$$Y_{i,t} = \gamma_t + \mathbf{1}[x_i \geq 0]g_l(x_i + \pi) + \mathbf{1}[0 < x_i]g_r(x_i) + u_{i,t}$$

where t is the date of the test, γ_t are time fixed effects, g_l and g_r are separate polynomial functions to the left and the right of the cutoff (which occurs at $x_i = 0$), and $u_{i,t}$ is an error term. The idea is that students who experience a DV reporting event prior to the exam date might be adversely impacted in their preparation and focus for the exam, while students who take the exam before the event should not be similarly affected by an abrupt DV shock.

3.3 *Interpretation*

An important caveat in any study of DV is that it is often a hidden crime, and hence not well recorded in datasets. When interpreting our estimates, it is important to remember that we are studying whether DV is being reported to the police. Police-reported DV likely represents the tip of the iceberg, reflecting more serious cases of abuse. We limit our sample to the first incident of DV reported to police, where there has not been a report in the prior four years. But this does not necessarily mean there are no acts of unreported violence during these four pre-event years; a police report could occur after repeated incidents of unreported domestic violence.

Since our treatment variable is a reporting event, this raises several concerns about identification and interpretation. The first is that we cannot study effects of unreported DV. Second, treatment reflects multiple things. It captures the combination of (i) any increase in the severity or type of violence (e.g., someone witnessed it) which led to a police report and (ii) any increase in access to shelters, social support programs and child services which are triggered by police report. Our estimates capture both of these effects, and we cannot separate them out. Third, if reported DV reflects a longer buildup of violence and conflict in the household, treatment captures the incremental increase in violence at the time of the report plus the reporting effect. However, if this is the case, we would also expect to see differential pre-trends in outcomes, which we generally do not.

A separate concern which is not specific to reporting per se, but rather to many event studies more generally, is reverse or simultaneous causality. To take an example, it could be that victim job loss causes DV rather than DV causing job loss. As we will show, the timing of when outcomes are affected alleviates some of these concerns, as we find little evidence of effects in advance of the reporting event. This suggests things are not differentially changing leading up to treatment. But it is still possible that two events (e.g., job loss and DV) are determined simultaneously or that the anticipation of an event causes an offender to lash out violently against their partner. For example, the threat of losing one's job could cause stress in the family and lead to DV. In this case, the reporting event would capture the joint effect of a DV report and the stress of an impending job loss, and our design cannot address this potential confound. This concern is present in any event study examining the effects of people's choices (as opposed to externally manipulated treatments), and we cannot rule it out.

The interpretation of our RD estimates as causal for school outcomes requires a different assumption, namely, that the timing of a DV report is not perfectly manipulable relative to the exam date (or expected graduation date from compulsory schooling). This would be violated, for example, if offenders choose not to commit DV or victims choose not report a DV event right before their child takes the exam. While it is in theory possible that offenders time their DV, many researchers believe DV is an unplanned loss of control and often regretted ex post (e.g., Card and Dahl (2011)). And while a victim could choose not to report, this seems unlikely to be a first-order consideration. We can test this RD assumption by seeing whether victim and child pre-characteristics are continuous at the cutoff as well as by using the McCrary test for discontinuities in the density around the cutoff. Panels (a)-(e) of Appendix Figure A2 shows that victim age at event, victim years of education, victim citizenship status, victim gender and child gender are all continuous at the cutoff. In panel (f), we also see that the density is not significantly different to the left or the right of the cutoff.

4 Results

In this section, we present our main findings. We first explore how a DV reporting event affects the home environment, including divorce, marriage, moving and household financial resources. We then explore the downstream effects of a DV event on a victim's mental health, doctor visits, employment, earnings and participation in disability insurance. Turning to child well-being, we examine mental health, child protective services, foster care, criminal activity, academic performance and completion of the first year of high school. We follow this up with a more detailed exploration of changes in mental health for both the victim and child.

4.1 Changes in the home environment

We begin by characterizing how a DV report affects household structure and the resources available to victims and their children. This helps to more fully describe the shock that is hitting a family, and provide insight into why more than just the trauma of DV abuse could contribute to worse mental health and well-being for both victims and their children. We define the victim's family to include the victim and anyone else they live with in the current period.

Figure 3, panels (a) and (b) plot the DiD estimates described in Section 3.1 for a victim's exit from and entry into marriage. For this analysis, we condition on marital status in the year 2000, which is before any of the DV events occur in the data. The x-axes are measured in event time, so that period 0 is the year of the DV report, and t-2 is used as the baseline. The horizontal bars denote 95% confidence intervals. Point estimates summarizing these graphs, along with outcome means, can be found in Table 1. As a reminder, we restrict the sample to families which have not experienced a DV event for at least four years.

Panel (a) shows how exit from marriage is affected by treatment. There is little evidence of differential pre-trends or anticipation effects. In the year of the DV report, marriage rates fall by 7 percentage points. One year after the event, marriage rates fall by 35 percentage points and remain low. This drop represents roughly a 50% reduction relative to the pre-DV event mean in period t-2. Panel (b) documents a similar pattern for entry into marriage. For this graph, we limit the sample to those who were not married in the year 2000. There is a small drop in marriage rates the year of the event, a 20 percentage point drop after one year, and then a persistent drop of approximately 26 percentage points after two years. Relative to the baseline non-married rate, this is a sizable 79% reduction. Consistent with the decline in marriage, panel (c) shows a drop in the number of children born to the victim after a DV report, with some evidence for a small pre-trend.

These results indicate that a large fraction of victims sever their relationships with offenders after a DV event. To explore this further, we look at the residential locations of victims before and after the event. Our data allow us to see whether a person remains in the same municipality or moves to a new one. There are 428 municipalities situated within 19 counties in Norway. In panel (d) there is no evidence for differential moving prior to treatment, but a significant and declining probability of remaining in the same municipality after treatment. The decline over time could reflect the fact that it takes victims some time to find a new place to live. By year 6, there is a 3.6 percentage point effect, which represents a 6% drop relative to the mean. This result is consistent with victims attempting to distance themselves from their offenders by moving away and starting fresh. It is important to note that our measure does not capture all moves, as victims could also move to a different location within the same municipality.

Given the changes in relationship status between the victim and offender, there could be an accompanying drop in financial resources available to victims and their children. As a summary measure of the household's resources, we look at per capita household consumption expenditure in Figure 4. The consumption expenditure measure is defined as total household disposable income minus net savings. Total household disposable income equals total annual gross household income minus paid taxes and including net received transfers (including transfers from government programs as well as alimony and child support payments). Net savings are equal to the change in net wealth from the beginning to the end of a year minus capital gains. Importantly, our per capita consumption expenditure measure is adjusted for household size using the EU-scale weights for adults and children as in Eika et al. (2020).

There is no evidence of differences prior to treatment and little change in per-person consumption expenditure in the year of treatment or the first year after. Starting two years after the DV incident, there is a large and persistent drop, despite the fact that there are fewer children in the victim's household. In the post period, per-person household consumption expenditure falls by 9% (29,048 NOK or $3,631 using an exchange rate of 8 NOK per USD). As graphed in panel (b) and reported in Table 1, much of this drop can be accounted for by a decline in the spousal earnings of a victim, which falls by approximately 122,425 NOK or $15,303. A majority of this drop in spousal income, in turn, is due to victims no longer being married to the offender.[10] When interpreting these amounts, it is important to remember that per capita consumption expenditure is adjusted for household size taking into account the spouse leaving.

4.2 *Mental health effects*

We now turn to the mental health impact of a DV incident on both victims and their children. We define the outcome variable as having at least one mental health-related visit during the year. As in Bhuller et al. (2021), we use the ICD-10 and ICPC2 classification codes to identify mental health diagnoses.[11] Later in the paper, we break down mental health visits by type of disorder. When interpreting our findings, an important caveat is that we cannot distinguish between a deterioration in mental health versus a rise in the use of mental health services. In other words, while increased visits could be a signal of worse mental health after a DV incident, they could also be driven by police and social services pushing victims to get the support and help they need.

Figure 5, panel (a) provides year-by-year estimates using the DiD estimator. Panel (a) plots event time coefficients for the victim and finds evidence of a small rise in mental health visits in the year prior to the DV reporting event, which could be due to increasing problems in the couple's relationship prior to the DV incident. In the year of the DV incident, there is a much larger and more pronounced rise in mental health visits of 15 percentage points. Relative to the mean in period *t-2*, this amounts to a 35% increase. The effect tapers off over time, so that 3 years after the event, there is no longer a difference relative to the control group. As Table 2 documents, the longer-run average impact from period *t+1* to *t+4* is a 7% increase.

Figure 5, panel (b) plots similar mental health impacts for children. There is no anticipation effect for children. In the year of the event, however, there is an uptick in children receiving a mental health diagnosis. The immediate impact is a 3.3 percentage point rise in mental health visits, which amounts to a 19% effect relative to mean. As shown in Table 2, in the longer run (*t+1* to *t+4*), there is a statistically significant average increase in mental health diagnoses of 2.6 percentage points, or a 15% increase relative to the mean.[12]

4.3 *Heterogeneous effects on mental health*

To provide more insight into the changes in mental health associated with a DV report, Table 3 provides a further breakdown into types of mental health diagnoses. The six categories are mood disorders, addiction related diagnoses, depression disorders, social problem diagnoses, sleep disorders and anxiety disorders. These 6 categories are defined based on the ICD-10 (International Classification of Diseases) and ICPC-2 (International Classification of Primary Care) codes.[13] We define outcomes which are dummy variables for whether an individual had a visit to a doctor for a specific type of disorder during a year. We point out that the outcomes are not mutually exclusive, as an individual can have several different types of diagnoses in the same year.

Panel (a) of Table 3 reports estimates for victims and starts with the immediate impacts in the year of the event. There are statistically significant increases in mental health diagnoses for 5 of the 6 categories (there is no impact on addiction disorders). The largest impact is found for social problems, which makes sense, as this disorder includes poverty and relationship problems (see footnote 13). There is a 152% rise in social problems relative to the mean in the year of the event. Turning to the other mental health categories, there is an immediate 61% rise in mood disorders and a 68% rise in anxiety disorders. Likewise, there are 36% and 35% increases in depression and sleep disorders, respectively. Given that the dependent means are relatively high for these outcomes, the associated percentage point effects are nontrivial. In terms of longer run impacts, there is statistically significant evidence of lasting mental health effects for mood disorders (16% increase) and anxiety disorders (22% increase). The remaining estimates are all positive, but not statistically different from zero.

Turning to children, the first thing to note is that mental health diagnoses are less common, with means in the pre-period (*t-2*) which are roughly one-fourth to one-fifth as large compared to adults. With this in mind, in the year of the DV event, there are statistically significant increases of 27% for mood disorders relative to the mean, 43% for sleep disorders and 22% for anxiety disorders. The remaining disorders are too imprecise to be informative. In terms of longer run impacts (the average between *t+1* to *t+4*), there is statistically significant evidence for a rise in mood disorders (50% increase), depression (89%), sleep disorders (64%) and anxiety disorders (49%).

One interesting contrast between victim and child mental health is that victims see uniformly larger immediate effects compared to the longer term, while for children the opposite generally holds. One interpretation is that victim mental health improves over time as they separate from an abusive partner, whereas children struggle to recover from the dramatic changes in the home environment documented in Section 4.1.

In Table 4, we conduct a different type of heterogeneity exercise. We estimate how impacts on mental health (not broken down by type) vary for different splits of the data. Column 1 repeats our baseline DiD estimate for comparison. In columns 2 and 3, we split the sample based on whether the mother was working in the year 2000, which is before any of the DV events have taken place. The immediate effect in the year of the event is larger if, prior to a DV event, the victim was employed versus not employed (19 versus 12 percentage points). Similarly, the immediate effect is larger for victims who did not have a prior mental health visit (19 versus 9 percentage points) or who were in a long-term relationship lasting 4 or more years (13 versus 2 percentage points). These differences are both economically and statistically significant. In the longer run, only the difference by prior mental health visits remains statistically different. These patterns provide useful insights into the types of victims who are most affected by DV. The differences by prior mental health visits are particularly interesting, as they suggest that the DV reporting event was more likely to represent the onset of a new mental health condition (or at least the treatment of a new condition). In contrast, for children, none of these splits of the data, for either the immediate or longer run, result in estimates which are statistically different from each other.

4.4 *Other effects on victims and their children*

We now turn to other effects on the well-being of both victims and their children. We find a variety of negative outcomes associated with a DV incident. One possibility is that these negative outcomes are due to worse mental health. But it is also possible that these negative effects are directly driven by the trauma of the DV event, and hence could contribute to worse mental health. Regardless of the connections between the various outcomes, they capture the changes associated with a DV reporting incident and are important in their own right.

Figure 6 plots the DiD estimates by event time. Panel (a) graphs the estimated effect of a DV event on the number of doctor visits in a year, defined as the number of visits an individual makes to their primary care physician. There is an immediate increase in doctor visits in the year of the event and an elevated number of visits in $t+1$, but no lasting impact. Panels (b) and (c) plot employment and earnings estimates, respectively. There is no evidence of pre-trends for either of these outcomes. Employment falls significantly starting in $t+1$, whereas there is already a drop in earnings in the year of the event. Employment and earnings remain depressed, with some evidence that they partially recover by the end of our sample window. Finally, there is no pre-trend in the use of disability insurance (DI), but evidence for a permanent rise in its use which begins already in the year of the event.

Table 5 reports point estimates corresponding to this figure for the immediate and longer run. In the short run, DiD estimates an 11% increase in doctor visits, a 3% drop in victim earnings and a 7% rise in disability insurance participation. In the longer run, there is a 4% drop in employment, with an accompanying drop in average annual earnings of roughly 12,000 NOK (or $1,5000). This drop in victim earnings, while nontrivial, is considerably smaller than the loss in spousal earnings reported in Table 1. Finally, there is a longer term 11% rise in DI participation. Interestingly, the percentage point rise in DI use is roughly equal to the drop in employment. A consistent explanation for the drop in victim's employment and earnings is that DV causes physical or emotional harm which makes it more difficult to work. It is also possible that increased contact with social services encourages a worker to apply for DI, particularly if a woman with disabilities is more dependent on an abusive partner.

Table 6 reports mental health and other outcomes for victims broken down by gender. While the overwhelming majority of victims are women, 17% of victims are men. Given that most of the baseline sample is women, it is not surprising that the estimates for women are generally quite close to the baseline estimates, with many economically and statistically significant effects. For men, the estimates are noisier given the smaller sample size. But there is enough precision to document negative effects on mental health and employment in the short run, and employment and earnings in the longer run. Summarizing the gender differences, the general pattern is that there are larger impacts on health outcomes (mental health, doctor visits and disability insurance) for women and stronger impacts on labor market outcomes (employment and earnings) for men. For example, women are more than twice as likely as men to be diagnosed with a mental health issue in the short run, a difference which is statistically significant (p-value<.01). And male earnings fall by 29,021 NOK in the long run compared to only 8,999 NOK for women, a difference which is statistically significant at the 10% level (p-value=.07).

Next we turn to other effects on children using our DiD design for child protective services, foster care placements and youth criminal activity. In Norway, child protective services are designed to provide parental guidance and additional resources to improve the household environment, with in-person visits. If necessary, the social worker can also recommend the child be removed from the household and placed in foster care. As Figure 7, panel (a) shows, there is an immediate 6 percentage point rise in child protective services, which as Table 7 documents, amounts to a 63% increase relative to the mean. Child protective services continues to be involved for three years after a DV event. This increase could be due to a worsening family environment after a DV event, but it could also be triggered by social workers being sent to the house after a police report. Panel (b) provides some evidence that foster care placements increase 4 or 5 years after an event, but this outcome is relatively rare and the estimates are imprecise. For youth crime, we find a statistically significant effect in the year of the incident (2 percentage point increase, or 54% increase relative to the mean), but not in other years.

When interpreting the negative effects accruing to both victims and their children, a natural question is whether a DV report is a one-time event or leads to a string of future reports. To answer this, we estimate DiD estimates by event time, where the outcome variable is a DV report. As a reminder, we condition our sample to not have any DV reports in the prior four years, so the estimate is 0 by construction from t=-4 to t=-1. Likewise, in the year of the event (t=0), the estimate is 1. There is some evidence for persistence in the years which follow, with a 10% increase in t=1, 6% in t=2, 4% in t=3, and 3% in both t=4 and t=5 (see Appendix Figure A3). These increases could reflect either an increased proclivity to report or an increase in abuse.

4.5 Comparison to OLS and first differences

OLS using cross-sectional data. How do these event study estimates using future victims as controls compare to OLS which uses non-victims as controls? In Table 8 we report OLS estimates based on a sample which includes treated observations and matched observations which do *not* experience a DV event, using 1-to-1 propensity score matching based on age, gender and immigrant status. We focus on four key outcomes – two victim outcomes and two child outcomes – in the short and long run. Going across columns (1)-(3), we progressively add in more control variables. For comparison purposes, in column 4 we repeat our main event study estimates from prior tables.

Consider the outcome of victim mental health in the year of the event (*t*=0). OLS estimates a large 38.9 percentage point increase in mental health diagnoses. As control variables are added to the regression in column (2) for education, year, age, gender, immigrant status, married and number of children, the estimated coefficient drops only slightly. Adding in the average lagged outcome for the 6 periods before the event in column (3) reduces the estimate to a 29.7 percentage point effect. Looking at the other outcomes, both additional controls and lagged outcomes have an impact on the OLS estimates. For example, the estimated effect on victim employment in the long run falls from -13.3 percentage points (column 1, OLS) to -9.8 (column 2, OLS with additional controls) to -5.8 (column 3, OLS with lagged outcome). The question, of course, is whether controlling for additional (unobserved) variables would reduce the coefficients even further.

More importantly, the OLS estimates in column (3) are uniformly larger in absolute value compared to our DiD estimates. The DiD estimates shown in column (4) are generally smaller by at least 50% and often more. Continuing with the examples above, the DiD estimate for victim mental health in the immediate term is half the size of the most saturated OLS model (15.1 versus 29.7 percentage points) and similarly for victim employment in the long run (-2.5 versus -5.8 percentage points). In other words, even after conditioning on control variables and lagged outcomes, OLS continues to overstate effect sizes relative to our DiD estimates. One interpretation is that it is difficult to get rid of selection bias using cross-sectional data and non-victim households as controls.

An alternative interpretation recognizes that families with later reports are likely to be experiencing DV as well, but have not yet called the police (i.e., the shock is about reporting or a particularly serious event). If one is interested in estimating the effect relative to a benchmark of a family without violence, then the comparison to non-victims could well be better, with the DiD estimates severely understating the total harm from domestic violence.

First differences. The key advantage of using panel data is that we can perform within-person comparisons, thus eliminating any time-invariant confounders. Using the additional control group of those who will experience a DV event in the future goes a step further by accounting for common time-varying confounders, such as age or calendar time. Instead of taking this second difference, an alternative is to use many cohorts in the first difference estimation and flexibly control for covariates which could have a differential impact over time. Table 9 presents first difference estimates for the same four outcomes as in Table 8. The first column reports raw first difference estimates without any controls. Adding in controls for age, year, gender and immigrant status generally reduces the estimates. To visualize the impact of these controls, Appendix Figure A4 plots both raw and residualized outcomes by event time. The residualized outcomes display pre-trends which are fairly flat, consistent with the assumption of the first difference estimator that there should be no changes over time in the absence of treatment. In contrast, the non-residualized outcomes still have some evidence of pre-trends.

The final column in Table 9 repeats our baseline DiD estimates for comparison. Most of the first difference estimates are remarkably close to the DiD estimates. This stands in stark contrast to the large differences observed when using cross-sectional data and OLS estimation. We draw two conclusions. First, most of the selection bias is removed by using panel data to difference out individual fixed effects. Second, while it is important to account for time-varying factors to eliminate any remaining bias, both our main DiD approach and the first difference approach seem to work reasonably well.

4.6 RD estimates for child educational outcomes

As a final exercise, we examine two educational outcomes for children. For this analysis, we use an RD design, as the structure of the data differs – we observe child outcomes at a point in time rather than repeatedly over time (see Section 3.2). The first panel in Figure 8 plots performance on national exams taken at the end of compulsory schooling. This is a consequential set of tests, as a weighted average of the exams plus a student's GPA is used to determine high school admission and placement. We standardize the national exam score to be mean zero and standard deviation one in the entire population of test takers. The running variable is the date of the test minus the date of the domestic violence event. The graph plots means in 6 month bins before and after the cutoff date of 0, with a window of +/-6 years.

Children to the left of the cutoff take the exam before the DV incident, while those to the right take the exam after the DV incident. The graph also shows linear trends based on the underlying data and using triangular weights along with 90% confidence intervals. The graph in panel (a) reveals an upward trend in exam scores as a function of the running variable. This could be due to the fact that families which experience their first police-reported DV event when a child is young have a worse family environment compared to those who experience their first DV event when a child is older. More importantly, the main takeaway from the RD graph is that there is a sharp drop at the cutoff in exam scores.

In the second panel of Figure 8, we look at a different educational outcome. Here we use on-time completion of the first year of high school as our outcome variable. The running variable is now the day of expected completion of the first year of high school minus the day of the DV reporting event. We note that high school is not compulsory in Norway, and only 59% of our sample complete their first year on time. There is a noticeable drop in on-time completion after a reporting event.

Table 10 reports RD estimates corresponding to the figures. Children taking the national exams after a DV incident experience 8 percent of a standard deviation decline, a drop which is statistically significant. For on-time completion of the first year of high school, we find a 5 percentage point drop at the cutoff, which translates to an 8% decrease. These estimates are robust to using a smaller window of +/- 3 years instead of +/- 6 years.[14] These results provide evidence that a DV reporting event is disruptive in the short run, in ways which could have long-lasting negative effects. However, we are quick to note that it is also possible that things will improve in ways which cannot be observed with our data. It would be interesting to look at effects as children grow up, which will be possible several years from now when longer-term data become available.

5 Discussion

Our paper adds to a growing literature on the causes and consequences of DV. We find that a DV report is a disruptive event for families with immediate consequences, as well as lingering effects which last for several years. A DV report is associated with a large change in the home environment, with lower rates of marriage and a decline in financial resources for victims and their children. There are sizable increases in mental health visits for both victims and their children, with specific rises in diagnoses for mood, depression, sleep and anxiety disorders. Accompanying these increases in mental health diagnoses, there is a decline in a victim's employment and earnings and a rise in the use of disability insurance. For children, there is an increase in child protective services and youth criminal activity, and drops in academic test scores and timely completion of the first year of high school.

These estimates capture the changes associated with a DV report, and do not isolate spikes in abuse separately from any interventions which might be triggered by a DV report. A DV report could have a positive impact if it gets an abusive partner out of the household or if it facilitates access to doctors, for example. But our reading of the combined pattern of estimates is that victims seem to generally suffer after a DV report (fewer financial resources, higher unemployment and increased DI use). The pattern for child outcomes similarly indicates a harmful impact (increased crime, lower test scores and lower high school completion). This suggests that the increase in mental health diagnoses is not merely due to increased access to doctors after a report, but instead that mental health worsens after a DV report. Consistent with this interpretation, we find that victims without a prior mental health diagnosis experience increases in mental health diagnoses which are twice as large. Future research could net out reporting effects by looking at homicides or nondiscretionary hospital visits after a DV incident, and estimate the corresponding mental health effects on victims and children. This would require different data and family linkages than those available in our setting.

Endnotes

1. As an alternative to police reports, some scholars have used hospitalization records (e.g., Aizer (2010, 2011)) while others have used crime victimization and health surveys for non-fatal violence (Miller and Segal, 2019). In terms of reporting behavior, Miller and Segal (2019) find that integration of female officers in local police departments increases DV reports, while Rice and Castello (2018) find that access to healthcare affects DV reporting among illegal migrants.
2. Relatedly, Persson and Rossin-Slater (2018) provide evidence on the effects of increased stress during pregnancy (due to the death of a relative) on birth outcomes and the exposed child's physical and mental health.
3. An expanding literature in economics also provides evidence on the determinants of DV. For instance, economists have investigated how the incidence of DV depends on household bargaining power and gender wage gaps (Aizer, 2010), marital endowments (Menon, 2020), unemployment (Anderberg et al., 2016), unconditional cash income (Heath et al., 2020; Hidrobo et al., 2016; Roy et al., 2019), women's health (Papageorge et al., 2021), emotional cues (Card and Dahl, 2011), healthcare access (Rice and Castello, 2018), legal drinking age (Chalfin et al., forthcoming), age-specific access to alcohol, drugs, smoking, or bars/clubs (Bindler et al., 2021), male combat service (Cesur and Sabia, 2016), traditional family structure (Tur-Prats, 2019), police arrest practices (Chin and Cunningham, 2019; Angrist, 2006), presence of female police officers (Miller and Segal, 2019), and lockdowns during the Covid-19 pandemic (Miller et al., 2022).
4. By providing evidence on the collateral effects on child protection services involvement and foster care, we also contribute to a literature studying the impacts of these social services (see, e.g., Bald et al. (2022, a), Bald et al. (2022, b), Gross and Baron (2022), Drange et al. (2022), Rittenhouse (2022), Doyle and Aizer (2018), Aizer and Doyle (2014), Doyle (2008, 2007)).
5. In four percent of the cases there is also a separate charge at the same date for the suspect linked to the child. This sample is too small to separately study DV cases involving both victims and children from those only involving victims. In all the cases we study, children are affected negatively either by being a victim or by being in an abusive household.
6. This matters for our classification of the type of DV abuse in heterogeneity analyses and for pinning down the timing for our RD analyses. Roughly 20% of victimization incidents

are dropped due to repeated DV within the same year.

7. In 1.1% of cases, the victim is also an offender.
8. The four year restriction eliminates 30% of victim-offender observations.
9. Moreover, most victimizations are male on female, with 98% of female victims having a male offender. For male victims, 88% of offenders are female.
10. Earnings of the offender, not conditioning on marital status, fall by 29,615 NOK (s.e.=4,515).
11. The health data only starts in 2007 which limits the possibility to go beyond four years before and after the event.
12. In contrast, in unreported results, we find little evidence of an increase in physical health diagnoses, either for victims or their children.
13. While most of the categories are self-explanatory, the social problem diagnosis code merits further explanation. This category is the ICPC-2 code of “Z Social Problems,” and includes subcodes such as Z01 Poverty/financial problem; Z03 Housing/neighbourhood problem; Z05 Work problem; Z08 Social welfare problem Z09; Z12 Relationship problem with partner; Z16 Relationship problem with child; Z20 Relationship problem parent/family; Z22 Illness problem parent/family; Z27 Fear of a social problem.
14. The estimates using +/- 3 years are -.064 (s.e.=.035) for exams and -.070 (s.e.=.026) for on-time completion.

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Figures and Tables

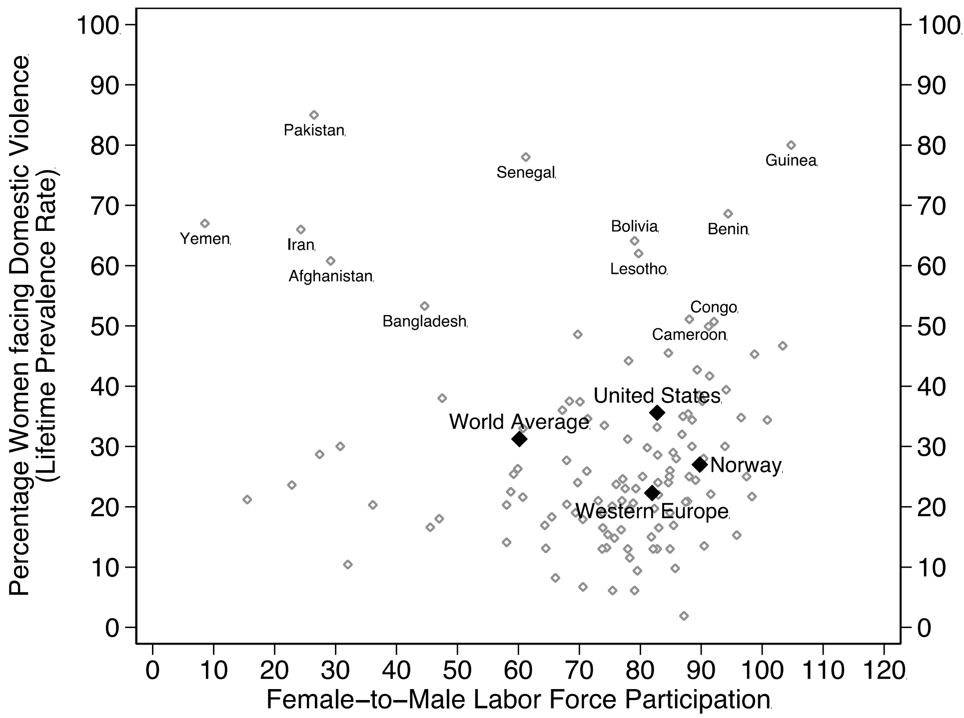
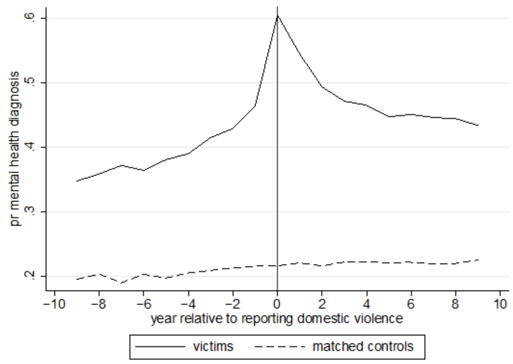
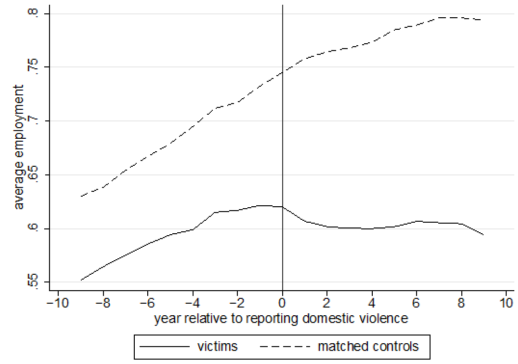


Figure 1. Lifetime prevalence of domestic violence around the world

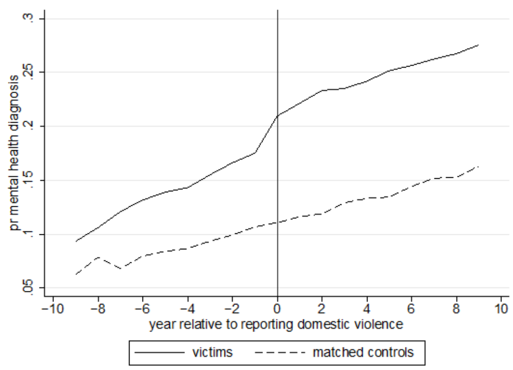
Notes: Data for DV come from the OECD's Violence Against Women Database. Data for labor force participation rates come from the World Bank/ILO. Both variables are for 2019. The World Average and Western Europe markers represent population-weighted averages. Western Europe includes Austria, Belgium, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Luxembourg, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland and the UK. The sample is restricted to 118 countries with a population of 2 million or above.



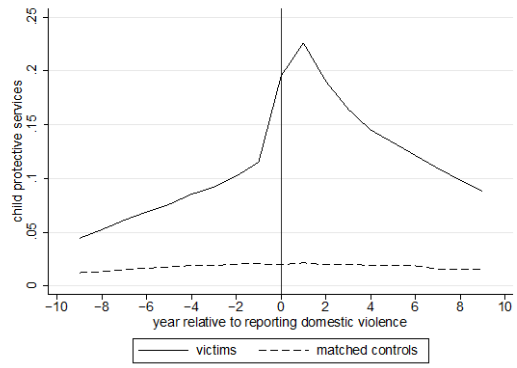
Panel A: Victim mental health diagnosis



Panel B: Victim employed



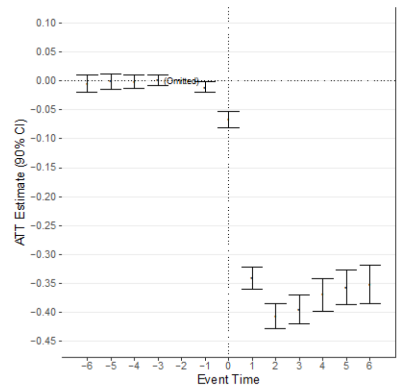
Panel C: Child mental health diagnosis



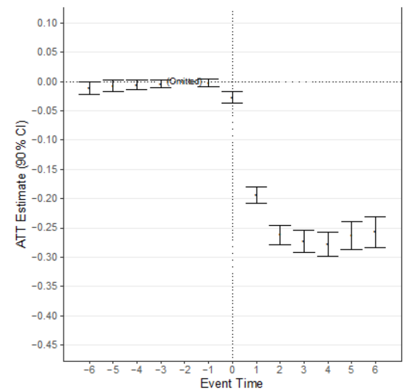
Panel D: Child protective services

Figure 2. Time patterns for victim and child outcomes compared to matched controls who are never victimized

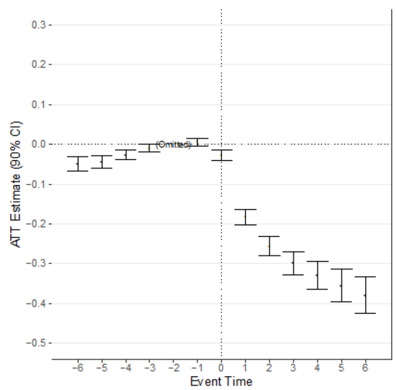
Notes: Solid lines are for victims and dashed lines are for a matched set of controls (on age, gender and immigrant status) which are never victims during the period. See text for definitions of the outcomes.



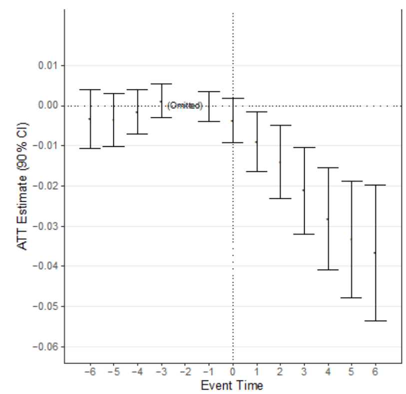
Panel A: Married | married in 2000



Panel B: Married | not married in 2000



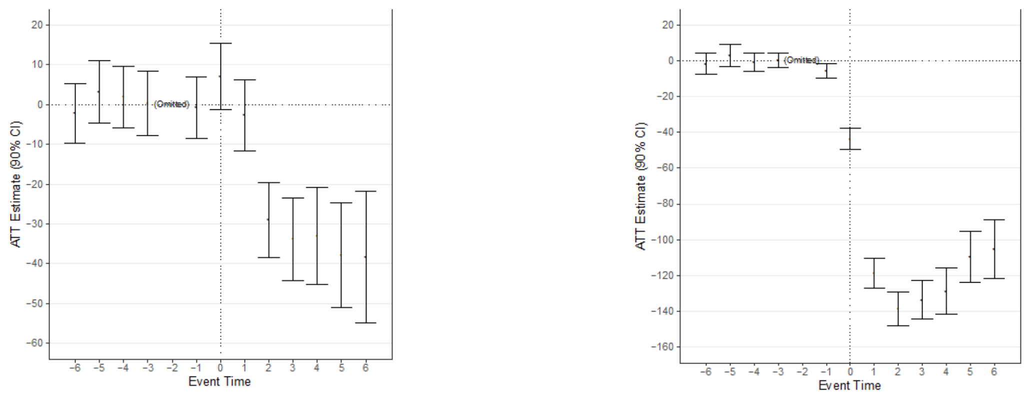
Panel C: Number of children



Panel D: Reside in same municipality as in 2000

Figure 3. Family structure

Notes: The figures show DiD estimates for being married, conditional on being either married (panel a) or not married (panel b) at baseline, for victims around the DV event. Panel c shows number of children. Panel d documents moves across municipalities. See corresponding estimates in Table 1.

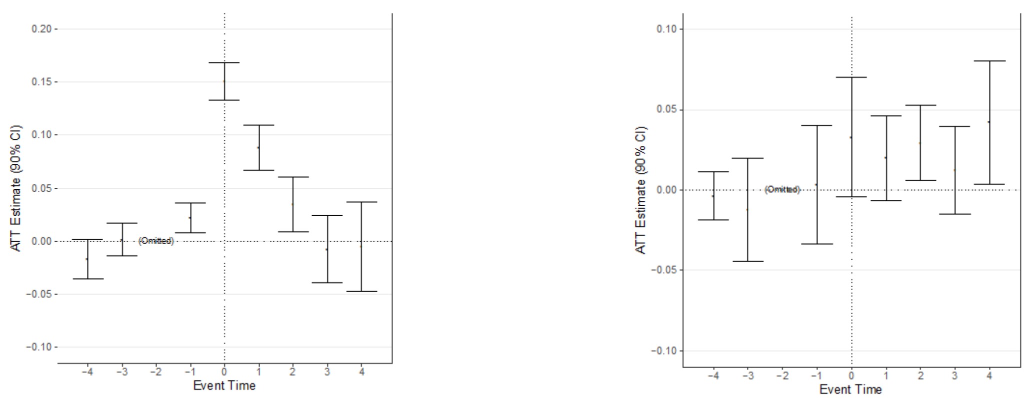


Panel A: Per capita consumption expenditure

Panel B: Spousal earnings

Figure 4. Financial Resources

Notes: The figures show DiD estimates for per-capita consumption expenditure for the victim's household and is defined as total household income net of taxes and transfers, subtracting out net savings, and adjusting for household size using EU-scale weights for adults and children (panel a) and spouse disposable income (panel b). See corresponding estimates in Table 1.

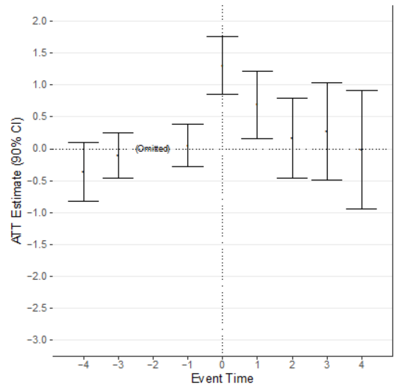


Panel A: Victim mental health diagnosis

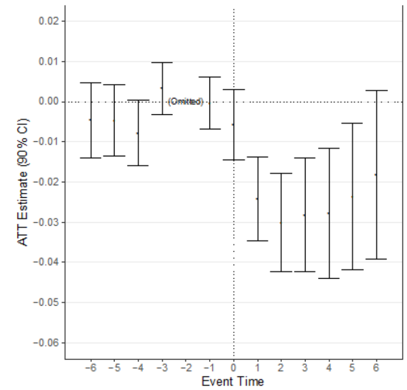
Panel B: Child mental health diagnosis

Figure 5. Mental health: Victims and their children

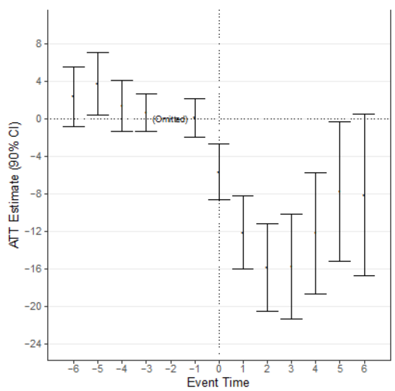
Notes: The figures show DiD estimates for mental health diagnosis defined as having at least one mental health-related visit in the year. See corresponding estimates in Table 2.



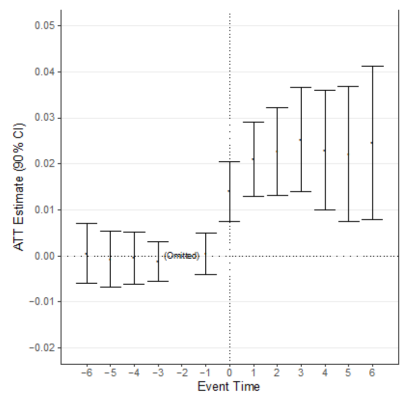
Panel A: Doctor visits



Panel B: Employment



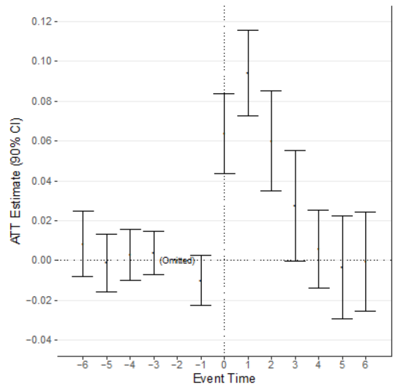
Panel C: Earnings in 1,000 Norwegian Kroner



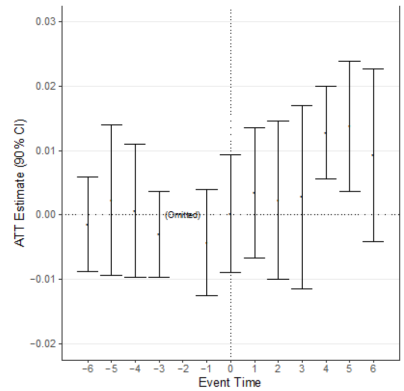
Panel D: Disability insurance participation

Figure 6. Other outcomes: Victims

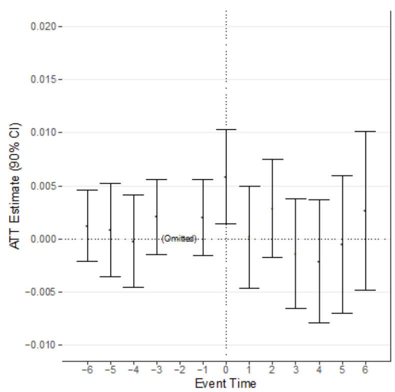
Notes: Doctor visits is the number of visits to a primary care physician in a year data only starts in 2007 so the long run average impact is for t+1 to t+4. Employment refers to earning more than the minimum amount required to qualify for a variety of government-provided employment benefits. Earnings is measured in Norwegian kroner (NOK); the exchange rate is roughly 8 NOK to 1 USD. See corresponding estimates in Table 5.



Panel A: Child protective services



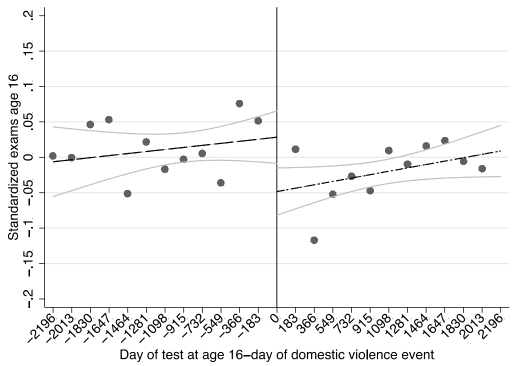
Panel B: Foster care



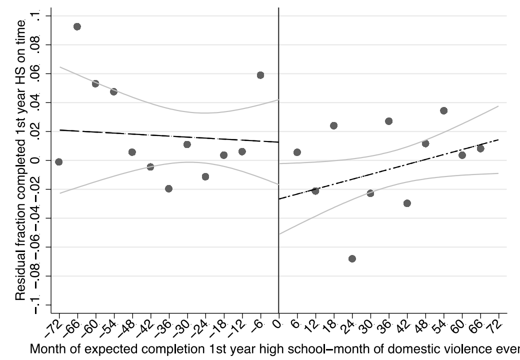
Panel C: Charged with a crime

Figure 7. Child event study outcomes

Notes: Child protective services and foster care are dummy variables which equal 1 if child protective services or foster care occurred at any point in the year. Crime is whether a child is charged with a crime and above 16 years old. See corresponding estimates in Table 7.



Panel A: National exam score



Panel B: On-time completion of 1st year of high school

Figure 8. Child educational outcomes, RD estimates

Notes: Plots of outcomes with dots representing averages for 6 month bins. The estimated lines are based on the underlying, daily data residualized by controlling for pre-determined characteristics, victim age, education, immigrant status, gender and child gender. Gray lines denote pointwise 90% confidence intervals. See corresponding estimates in Table 10.

Table 1. Home environment

Dependent Variable:	*DiD estimate* (1)	Dep. mean [% effect] (2)
Long run average impact (t+1 to t+6)		
Married | married in 2000 (surviving marriages)	-.390**	.77
	(.013)	[-51%]
Married | not married in 2000 (new marriages)	-.260**	.33
	(.011)	[-79%]
Number of children	-.030	2.27
	(.017)	[-13%]
Live in same municipality as in 2020	-.024**	.64
	(.006)	[-4%]
Per capita consumption expenditure	-29,048**	318,195
	(5,994)	[-9%]
Spousal earnings	-122,425**	285,562
	(8,471)	[-43%]

Notes: For marriage outcomes in total, N=17,267 (thirty-five percent of victims were married in the year 2000); for municipality outcome, N=16,971; for consumption, N=16,725 and spousal earnings, N=16,728. Dependent mean refers to period t-2, and % effect is the DiD estimate divided by the dependent mean. Standard errors in parentheses, clustered by victim.
**significant at the 5% level, *significant at the 10% level

Table 2. Mental health: Victims and their children

Dependent Variable:	DiD estimate (4)	Dep. mean [% effect] (5)
Immediate impact (t=0)		
Victim mental health	.151**	.43
	(.009)	[35%]
Child mental health	.033*	.17
	(.019)	[19%]
Long run average impact (t+1 to t+4)		
Victim mental health	.028*	.43
	(.015)	[7%]
Child mental health	.026*	.17
	(.015)	[15%]

Notes: N=8,406 for victims, N= 15,557 for children. For mental health visits, data only starts in 2007 so the long run average impact is for t+1 to t+4. Mental health diagnosis defined as having at least one mental health-related visit in the year. Dependent mean refers to period t-2, and % effect is the DiD estimate divided by the dependent mean. Standard errors in parentheses, clustered by victim.
**significant at the 5% level, *significant at the 10% level

Table 3. Mental health by type of disorder: Victims and their children

Dependent Variable:	DiD Estimate (1)	s.e. (2)	Dep. mean (3)	% effect (4)
(a) Victim				
Immediate impact (t=0)				
Mood disorders	.140**	.009	.230	61%
Addiction disorders	.000	.004	.058	0%
Depression disorders	.068**	.008	.188	36%
Social problems	.096**	.006	.063	152%
Sleep disorders	.019**	.005	.055	35%
Anxiety disorders	.134**	.008	.197	68%
Long run average impact (t+1 to t+4)				
Mood disorders	.037**	.014	.230	16%
Addiction disorders	.005	.007	.058	9%
Depression disorders	.012	.013	.188	6%
Social problems	.011	.009	.063	17%
Sleep disorders	.002	.008	.055	4%
Anxiety disorders	.044**	.014	.197	22%
(b) Children				
Immediate impact (t=0)				
Mood disorders	.013**	.004	.048	27%
Addiction disorders	-.010	.011	.014	-71%
Depression disorders	.019	.012	.026	73%
Social problems	-.007	.018	.023	-30%
Sleep disorders	.006**	.003	.014	43%
Anxiety disorders	.009**	.004	.035	22%
Long run average impact (t+1 to t+4)				
Mood disorders	.024**	.010	.048	50%
Addiction disorders	.005	.011	.014	36%
Depression disorders	.023*	.013	.026	89%
Social problems	-.028	.023	.023	-121%
Sleep disorders	.009**	.004	.014	64%
Anxiety disorders	.017*	.010	.035	49%

Notes: See notes to Table 2. For mental health visits, data only starts in 2007 so the long run average impact is for t+1 to t+4. Mental health diagnosis defined as having at least one mental health-related visit in the year for a specific disorder. Dependent mean refers to period t-2, and % effect is the DiD estimate divided by the dependent mean. Standard errors in parentheses.
**significant at the 5% level, *significant at the 10% level

Table 4. Heterogeneous mental health effects: Victims and their children

		Victim employed in 2000		Mental health visit in 2007		Long relationship (>4 yrs. in 2000)	
	Baseline	Yes	No	Yes	No	Yes	No
Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Immediate impact (t=0)							
Victim mental health	.152**	.176**	.118**	.086**	.191**	.130**	.017
	(.009)	(.013)	(.013)	(.015)	(.012)	(.013)	(.013)
Child mental health	.033*	.035	.034**	.039**	.030	.037	.026**
	(.019)	(.027)	(.011)	(.018)	(.026)	(.033)	(.011)
Long run average impact (t+1 to t+4)							
Victim mental health	.026**	.030	.026	-.014	.056**	.028	.034
	(.011)	(.021)	(.023)	(.027)	(.020)	(.018)	(.022)
Child mental health	.026*	.019	.044**	.030	.030	.021	.024
	(.015)	(.021)	(.018)	(.034)	(.018)	(.020)	(.019)

Notes: See notes to Table 2. Splits based on values prior to a DV event, which is the year 2000 for columns 2, 3, 6, and 7, and the year 2006 for columns 4 and 5 (since for mental health visits, data only starts in 2007). Dependent mean refers to period t-2, and % effect is the DiD estimate divided by the dependent mean. Standard errors in parentheses.

**significant at the 5% level, *significant at the 10% level

Table 5. Other outcomes: Victim

	DiD estimate	Dep. mean [% effect]
Dependent Variable:	(4)	(5)
Immediate impact (t=0)		
Doctor visits	1.31**	12.3
	(.23)	[11%]
Employment	-.006	.61
	(.004)	[-1%]
Earnings in NOK	-5,638**	225,149
	(1,524)	[-3%]
Disability insurance	.014**	.21
	(.003)	[7%]
Long run average impact (t+1 to t+6)		
Doctor visits	.362	12.3
	(.363)	[3%]
Employment	-.025**	.61
	(.008)	[-4%]
Earnings in NOK	-11,920**	225,149
	(3,119)	[-5%]
Disability insurance	.023**	.21
	(.006)	[11%]

Notes: N=10,223 (doctor visits); N=17,267 (employment, earnings, disability insurance). For doctor visits, data only starts in 2007 so the long run average impact is for t+1 to t+4. Dependent mean refers to period t-2, and % effect is the DiD estimate divided by the dependent mean. Standard errors in parentheses, clustered by victim. **significant at the 5% level, *significant at the 10% level

Table 6. Heterogeneous victim effects: Gender

Dependent Variable:	Baseline (1)	Gender: Female (2)	Gender: Male (3)
Immediate impact (t=0)			
Victim mental health	.152**	.167**	.075**
	(.009)	(.010)	(.025)
Doctor visits	1.31**	1.49**	.71
	(.23)	(.27)	(.50)
Employment	-.006	-.004	-.018*
	(.004)	(.005)	(.010)
Earnings in NOK	-5,638**	-5,794**	-4,091
	(1,524)	(1,489)	(5,273)
Disability insurance	.014**	.017**	.000
	(.003)	(.004)	(.008)
Long run average impact (t+1 to t+6)			
Victim mental health	.026**	.042**	-.045
	(.011)	(.017)	(.039)
Doctor visits	.362	.243	-.570
	(.363)	(.648)	(1.05)
Employment	-.025**	-.023**	-.045**
	(.008)	(.008)	(.018)
Earnings in NOK	-11,920**	-8,999**	-29,021**
	(3,119)	(3,170)	(10,505)
Disability insurance	.023**	.026**	.018
	(.006)	(.007)	(.014)

Notes: Number of observations by gender: Mental Health: 6,932 females, 1,474 males. Doctor Visits: 8,358 females. 1,865 males, Employment, disability insurance: 14,228 females, 3,039 males, Earnings: 14,098 females, 3,026 males. Baseline estimates come from Tables 2 and 5. Standard errors in parentheses, clustered by victim.

**significant at the 5% level, *significant at the 10% level

Table 7. Other outcomes: Children

	DiD estimate	Dep. mean [% effect]
Dependent Variable:	(4)	(5)
Immediate impact (t=0)		
Child protective services	.064**	.102
	(.010)	[63%]
Foster care	.002	.019
	(.005)	[11%]
Charged with a crime	.020**	.037
	(.008)	[54%]
Long run average impact (t+1 to t+6)		
Child protective services	.031**	.102
	(.012)	[31%]
Foster care	.008	.019
	(.006)	[42%]
Charged with a crime	.006	.037
	(.007)	[16%]

Notes: N=41,828 (child protective services, foster care), N=27,827 (charged and aged 16 or above). Dependent mean refers to period t-2, and % effect is the DiD estimate divided by the dependent mean. Standard errors in parentheses, clustered by victim.
**significant at the 5% level, *significant at the 10% level

Table 8. Comparison to OLS

Dependent Variable:	OLS (1)	OLS w/ additional controls (2)	(2) + lagged outcome (3)	Baseline DiD (4)	Dep. mean (5)
Immediate impact (t=0)					
Victim mental health	.389**	.382**	.297**	.151**	.43
	(.007)	(.007)	(.007)	(.009)	
Employment	-.125**	-.096**	-.028**	-.006	.61
	(.005)	(.005)	(.004)	(.004)	
Child mental health	.099**	.091**	.060**	.033*	.17
	(.004)	(.004)	(.004)	(.019)	
Child protective services	.176**	.145**	.113**	.064**	.10
	(.002)	(.003)	(.002)	(.010)	
Long run average impact (t+1 to t+6)					
Victim mental health	.171**	.158**	.121**	.028*	.43
	(.004)	(.004)	(.004)	(.015)	
Employment	-.133**	-.098**	-.058**	-.025**	.61
	(.005)	(.004)	(.004)	(.008)	
Child mental health	.069**	.058**	.046**	.026*	.17
	(.003)	(.003)	(.003)	(.015)	
Child protective services	.119**	.099**	.087**	.031**	.10
	(.001)	(.002)	(.002)	(.012)	

Notes: N=20,151 (mental health visits); N=20,446 (doctor visits); N=32,902 (employment, disability insurance); N=28,304 (earnings). DiD only for victims (half of the sample). OLS is based on a sample of non-victims matched to controls; for details, see the text. Column (4) repeats estimates from Tables 2 and 5. For doctor visits and mental health visits, data only starts in 2007 so the long run average impact is for t+1 to t+4. Additional controls include year, years of education, age, gender, immigrant status, married and number of children. Lagged outcome is the average of 6 periods before treatment. Dependent mean refers to period t-2. Standard errors in parentheses, clustered by victim.

**significant at the 5% level, *significant at the 10% level

Table 9. Comparison to first differences

Dependent Variable:	First difference (relative to -2) (1)	First difference w/ controls (2)	Baseline DiD (3)	Dep. mean (4)
Immediate impact (t=0)				
Victim mental health	.176**	.162**	.151**	.43
	(.007)	(.008)	(.009)	
Victim employed	.003	-.004	-.006	.61
	(.005)	(.006)	(.004)	
Child mental health	.044**	.025**	.033*	.17
	(.004)	(.004)	(.019)	
Child protective services	.074**	.033**	.064**	.10
	(.004)	(.004)	(.010)	
Long run average impact (t+1 to t+6)				
Victim mental health	.051**	.034**	.028*	.43
	(.006)	(.006)	(.015)	
Victim employed	-.014**	-.025**	-.025**	.61
	(.004)	(.005)	(.008)	
Child mental health	.074**	.033**	.026*	.17
	(.004)	(.004)	(.015)	
Child protective services	.084**	.078**	.031**	.10
	(.003)	(.002)	(.012)	

Notes: N in columns (1) and (2) for immediate and long run, respectively, are 19,429 and 71,379 (mental health visits), 34,534 and 97,732 (employment), 36,948 and 147,321 (child mental health visits), 83,656 and 244,066 (child protective services). Column 2 adds in controls for age, year, gender and immigrant status. Baseline DiD estimates taken from Tables 2,5 and 6. Standard errors in parentheses, clustered by victim.
**significant at the 5% level, *significant at the 10% level

Table 10. RD estimates: Schooling

	RD estimates	Dep. mean [percent effect]
National exam score	-.077**	-.42
	(.030)	[-18%]
On-time completion of 1st year of HS	-.047**	.59
	(.023)	[-8%]

Notes: N=19,583 children for national exam score; N=13,564 children for on-completion of 1st year of HS. RD estimates using triangular weights and a window of +/-6 years around the event. National exam score is normalized to be mean 0 and standard deviation 1 for the entire sample of test takers. Control variables include years of education, age, gender, immigrant status, married and number of children for the victim and child gender. Standard errors in parentheses, clustered by victim.
**significant at the 5% level, *significant at the 10% level