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SURVIVING A MASS SHOOTING

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ABSTRACT

We use data on all middle and high school-aged children who survived a mass shooting incident on July 22, 2011 in Utøya, Norway, to understand how such events affect survivors, their families, and their peers. Using a difference-in-differences design to compare survivors to a matched control group, we find that in the short run children who survive have substantially lower GPA (nearly 0.5 SD), increased health visits, and more mental health diagnoses (nearly 400% increase). In the medium run, survivors have fewer years of schooling completed and lower labor force participation. Parents and siblings of survivors are also impacted, experiencing substantial increases in doctor visits and mental health diagnoses. However, there appear to be limited impacts on school-aged peers of survivors. While this event affected the entire country, we show that survivors and their families bear significant costs despite robust social safety nets and universal access to healthcare.

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1 INTRODUCTION

Exposure to childhood trauma is widely recognized as an important factor determining long run labor market outcomes. While children are subjected to various forms of trauma, ranging from health shocks to parental death and financial distress, the impacts of childhood exposure to violence and gun violence in particular has been on the forefront of media and policy attention.[1] This is in part due to well publicized mass shootings and school shootings in the US, but also increased attention towards gun violence in other countries (Sturup, Rostami, Mondani, Gerell, Sarnecki, and Edling, 2019). The broad public interest in children exposed to such events could indicate that the costs of victimization for this group are particularly large (Cook and Ludwig, 2002). However, empirical evidence on such costs is limited, primarily as a consequence of data availability and challenges in linking pre- and post event outcomes to individual victims.

In this paper we quantify the effects of surviving an episode of mass shooting on school aged children in Norway in the short and medium term. In addition, we document spillover effects of surviving such events on children's families and their school aged peers. The empirical challenge in such an exercise stems from the usual concern that children who are exposed to such events are different from those who are not, and could have fared differently even absent this exposure. In our setting, the context of mass shooting is one where it is unlikely that individual children were targeted for unobserved reasons, and we also leverage detailed administrative data and a matching strategy to identify "similar" children who were not directly exposed to the event in a difference-in-differences (DiD) design.

On July 22, 2011 around 600 individuals, mostly school aged children from all over Norway, were attending a youth camp on the island of Utøya when a politically motivated mass shooting took place. Using detailed administrative data on individual demographics and parental characteristics (including earnings), we match survivors to observably similar children who were not on the island and who also did not attend the same school as the affected children. These matched children form the control group for the affected survivors, which allows us to compare outcomes before and after the event across survivors and controls in a DiD design. With this design, we can account for time-invariant unobserved heterogeneity across survivors and matched controls as well as common changes over time. Under a standard parallel trends assumption, this allows us to estimate the effects of exposure to mass shooting on the survivors.

We consider a series of different outcomes for survivors, their families, and their peers to draw a comprehensive picture of the impacts of mass shooting exposure. For school outcomes (e.g., GPA), we compare survivors and controls who took the same-grade exams before and after the event, effectively relying on variation across different birth cohorts in the timing of event. For survivors' health outcomes (e.g., doctor visits) and their parental outcomes, we can follow all

[1] For an excellent recent review on the costs of victimization see Bindler, Ketel, and Hjalmarsson (2020).

survivors and controls both before and after the event, and rely on a DiD design with individual fixed effects. While for survivors' medium-term outcomes (e.g., employment and earnings), we do not observe pre-event outcomes for most individuals, and rely therefore on a simpler treatment-control comparison based on matching techniques.

We find that young survivors (aged 14-15) obtained lower test scores (by 0.5 SD) in middle school, and older survivors (aged 15-18) were less likely to finish high school (20 percentage points) after the event. All directly affected children (regardless of age) had more medical visits with a general practitioner (60% increase relative to the control mean) and increased psychiatric diagnoses (almost 400% increase relative to the control mean) in the immediate aftermath of the shooting. Parents of survivors were not meaningfully impacted in terms of labor market participation or earnings, although they do see significant increases in doctor visits and mental health diagnoses. The same is true for siblings of the direct survivors. We also find that siblings suffer in terms of test scores in middle school by scoring nearly 0.2 SD lower. We find no economically meaningful or statistically significant effects on test scores or health visits and mental health diagnoses among school peers. Approximately 7 years after the incident, in 2018, we observe that older survivors were 12% less likely to have completed college, had 5 percentage point lower labor market participation and 12% lower labor market earnings (conditional on being employed and this result is not statistically significant).

In writing this paper we contribute to a rich literature on the short and long run consequences of exposure to various types of violence during childhood across multiple disciplines.[2] Some of this work also focusses on the role of acts of terror (a term also used to describe the Utøya incident) on student test scores (Shany, 2016; Auger, Seymour, and Roberts Jr, 2004) and mental health (Otto, Henin, Hirshfeld-Becker, Pollack, Biederman, and Rosenbaum, 2007). More specifically, our paper adds to some of the recent work in economics focusing on gun violence and its effects on children. To provide a few key examples: Ang (2018) focusses on the effects on African American students who are exposed to police shootings in their neighborhood, Rossin-Slater, Schnell, Schwandt, Trejo, and Uniat (2020) focus on anti-depressant use by students after fatal school shootings, and Gershenson and Tekin (2018) examine the effects on school performance of students exposed to the "Beltway Sniper" in 2002 near Washington DC.

Two contemporaneous papers are closely related to this paper. First is the important work by Levine and McKnight (2020) who examine the consequences of being exposed to school shootings on test scores and well being of students in the US. Using several quasi-experimental techniques and first focussing on the Sandy Hook incident in Newtown, CT in 2012, they find that average test scores in schools are lower and chronic absenteeism higher after this event. They also find that these effects spillover to nearby schools. They next examine the Columbine shooting incident from 1999 in Columbine, CO and offer evidence that is consistent with the

[2]Some of the papers focusing specifically on trauma due to violence include Finkelhor, Turner, Shattuck, Hamby, and Kracke (2015), Moffitt (2013), Sharkey (2010) and Hurt, Malmud, Brodsky, and Giannetta (2001).

idea that affected students were more likely to commit suicide in subsequent years. Second is the excellent paper by Cabral, Kim, Rossin-Slater, Schnell, and Schwandt (2021) where they examine using a matched DiD strategy (where *schools* are matched as opposed to individuals as in our case) the impact of being exposed to school shootings in Texas between 1995-2016. They document adverse short term impacts on absence, high school graduation, and early labor market outcomes. We build on such findings by leveraging individual level data where we can follow students over time in terms of test scores and health visits, and also examine spillovers more precisely by examining family members and peers individually.

Finally, this paper provides additional evidence specifically on the impacts of the Utøya mass shooting. To highlight just a few: Alne and Serdarevic (2020) examine the impacts of losing a child during this event on parental labor market outcomes, Stene and Dyb (2015) and Dyb, Jensen, Glad, Nygaard, and Thoresen (2014) provide descriptive accounts of healthcare utilization by the survivors of the attacks, Gjerland, Pedersen, Ekeberg, and Skogstad (2015) examine health impacts on rescue workers, and Hernæs (2019) examines sickness absence responses in the short run at the municipality level. By examining the effects on test scores, high school completion, labor force partition, and earnings, and also by considering family and peer effects, we add meaningfully to this important area of research.

2 The July 22, 2011 incident in Utøya

On July 22, 2011, Anders Behring Breivik committed two acts of domestic terrorism in Norway. The first was a car bomb explosion in the capital Oslo, and the second, a few hours later at a summer camp on the island of Utøya. The camp was organized by AUF, a youth wing of the ruling (at the time) Norwegian Labor Party (AP). During the hour or so long period of indiscriminate killing, Breivik killed 69 people. A majority of those killed were between the ages of 16-18; the youngest victims were 14 years old, and the oldest was 51. Among the survivors, 66 were shot and wounded, while 585 survived without physical injuries (Helsedirektoratet, 2012).[3] Since this was a youth camp, a large fraction of the survivors were middle and high school aged children, who came from all over Norway.

There was a massive response from the government following this attack. An independent commission examined the police responses and preparedness for these types of threats (see NOU (2012:14)). In terms of treatment of the victims and their families, since 2003, Norway has had a national scheme for compensating victims of criminal injuries. Survivors as well as the bereaved family members of those murdered at Utøya received compensation through this scheme, and the amounts paid out were typically higher than the compensation given to other

[3]Given the small sample size of children who faced direct physical injury as a result of this incident, we are unable to fully distinguish victims with physical and non-physical injuries for ethical and statistical reasons.

victims of violent crime (Nilsen, Langballe, and Dyb, 2016). There was also an increased allocation of resources to municipalities with survivors to take care of those that needed more follow-up services and additional expenses for health specialists (Helsedirektoratet, 2012). These payments and resources are in addition to what is generally provided under a generous welfare state with universal health care and insurance schemes (such as sickness benefits and disability benefits) to help those in need.

3 DATA

We link several administrative data sources from Norway for this paper. These include the victim database from the Norwegian Police Directorate and population level panel data drawn from school, health, and tax registers.

3.1 SCHOOL LEVEL DATA

School registers contain data on all middle and high school students. For the analysis concerning younger survivors we use 10th grade data from middle school and this data is available from 2005-2016. We have two measures of performance in middle school: grade point average (GPA) and national exam scores. Given compulsory schooling laws, all individuals are required to be enrolled in 10th grade in Norway, and as a consequence, we observe GPA for nearly everyone in our victims database. This GPA is the average of the grades in all subjects. Grades are normalized for each academic year-cohort separately. To identify peers, we consider pupils who attended the same middle school in grade 10. Nearly all schools in Norway are public and school switching is uncommon. Exam scores are from national exams, where everyone takes the same exam and is externally graded. The scores are standardized for each academic year-cohort separately.

For high school students we use enrollment and completion data, which we have from 2002 to 2016. We do not use data on high school GPA as attending high school is a choice in Norway, and part of the effect on survivors could be that they drop out of high school. We therefore choose to focus on completion of grade 1 and grade 3 in high school on time and on not getting a high school diploma. Academic track programs in high school are 3 years, while vocational track programs can be longer. For peers, we can only study completion of grade 3. The reason is that we need to define peers in high school before July 22, 2011, as the victims might drop out or change high school after the event, making the peers endogenous. Peers are therefore defined in grade 1 in high school, and only for those starting high school before July 22, 2011.

For higher education data, we use enrollment education files, available from 2002 to 2016, and information on the highest level of education completed, available up to 2016. We create an

indicator equal to one if the individual is registered as enrolled in any college or university level education in a given academic year and we also create an indicator equal to one if the individual has completed any college or university level education.

3.2 HEALTH OUTCOMES

To analyze health outcomes, we use the Control and Payment of Reimbursement to Health Service Providers (KUHR) database, available from 2006-2018, with information on visits to general practitioner (GP), emergency units (ER), and referrals to specialists. All Norwegians belong to the patient list of a certified GP. The register includes information on each patient's unique personal identifier, date and time of contact, and diagnosis according to the ICPC-2-diagnosis codes. For GP visits, we sum all GP consultations in each calendar year for survivors, and in each academic year (August-June) for peers. For psychological diagnosis we create an indicator equal to one if the individual has any psychological diagnoses in the ICPC-2 (P01-P99) and ICD-10 diagnosis codes, during a year. For peers, the indicator is equal to one if there is a psychological diagnosis during the academic year.

3.3 LABOR MARKET DATA

To analyze labor market outcomes for parents, we use data from tax registers and matched employer-employee records, available from 1987-2018. Earnings include labor earnings, sickness benefit and parental leave benefit, but not e.g. permanent disability insurance, pension income or entrepreneurial income. We create labor force participation variables with an indicator equal to one if earnings are larger than 0 (we have also tested an indicator equal to one if earnings are larger than two substantial gainful activity amounts, equivalent to almost 22 000 USD in 2019). For earnings, we use the log of earnings and drop observations with zero earnings. The earnings measures are CPI adjusted with 2015 as the base year. We also include a dummy equal to one if parents are married january 1st in a given year.

3.4 VICTIM DATA

Our victim data come from an administrative police register, which comprises all crimes reported to the Norwegian police, including the Utøya incident. This database contains information on the month and place of the crime, which makes it possible for us to identify survivors of Utøya (see Figure B1 in the Online Appendix). Using this, we find registered victims in July 2011 in the municipality of Hole (where Utøya is located). A person is defined as a survivor if the registered crime is attempted murder (*drapsforsøk*) in the database, and not murder (*drap*). A few of the registered victims were not registered residents in Norway, and hence they

are excluded from our analytical sample. Figures B2 and B3 in the Online Appendix show that most of the survivors were school aged children and came from municipalities all over Norway. Table B1 and B2 show additional descriptives on survivors.

3.5 MATCHING

For each survivor, we find a matched control group, with individuals who were not directly affected, using the protocols described as follows based on Coarsened Exact Matching (CEM). To be a match, the control individual must have the exact same value on all the following variables: birth year, gender, mother's and father's education measured in 3 levels in 2010 (completed middle school or less, completed at least some high school, and completed at least some university education), mother's and father's age in 2010 in tertiles (for each birth cohort), mother and father's earnings in 2010 in tertiles (for each birth cohort), and the centrality of municipality of residence, where we use Statistics Norway's centrality classes (6 levels) to distinguish areas with high or low population density. We also include categories for missing data for parental education, age, and earnings.

In addition, to reduce the risk of finding control individuals that know or go to the same school as a survivor or deceased, we remove individuals that live in the same municipality or city district as a survivor or deceased that is born in the same year as the individual. To identify residence of the survivor and the matched control, we use city districts for the four largest cities in Norway, Oslo, Bergen, Trondheim, and Stavanger and otherwise use the municipality of residence on January 1, 2011.

For medium-term outcomes, we redo the matching procedure in the same way as described above, but also include quintiles (for each cohort) of exam scores from the 10th grade. We only include matched individuals taking the 10th grade exam before July 22, 2011 (cohorts born in 1986-1995). Cohorts born before 1986 are excluded, because middle school data is only available from 2002.

4 ESTIMATION

We use a difference-in-differences (DiD) approach with the following main specification:

$$Y_{it} = \beta Surv_i + \gamma Post_t + \eta Surv_i \times Post_t + \lambda X_{it} + \varepsilon_{it}$$

where Y_{it} is the outcome for individual i in period t. $Surv_i$ is an indicator equal to one if the individual is a survivor, or a family member or a peer of the survivor, and zero if the individual is in the control group. For school outcomes, $Post_t$ is an indicator equal to one if individual i is

from a birth cohort that completed the grade pertaining to Y_{it} in the 2011/12 academic year or later, and zero otherwise. For health outcomes of survivors, as well as parental outcomes, $Post_t$ is simply an indicator for whether the outcome is measured in year 2012 or later. And X_{it} is a set of controls, including gender, parental education and age, and the centrality of municipality.

For school outcomes, we only observe each individual once in each regression. The DiD estimate is then the difference between survivors and control individuals that "should" be finishing the grade after July 22, 2011, minus the equivalent for survivors and control individuals that "should" have finished the grade before July 22, 2011. The main identifying assumption is that given the matching, the counterfactual cohort profile for the survivors (or their families or peers) in the absence of Utøya would have been identical to the one for the matched control group. This approach allows us to account for secular changes across birth cohort that are unrelated to the event. We follow this approach also for younger siblings and peers of survivors. And for peers, we use the same approach for health outcomes.

For health outcomes of survivors, as well as parental outcomes, we can observe individuals over time. For these outcomes, we can thus include individual fixed effects, and exploit within-person changes in outcomes before and after the event across survivors and controls. The DiD estimate is then the difference in outcomes of all survivors (or parents of survivors) and all control individuals from 2011 and onwards, minus the equivalent difference in the years before.

For middle school outcomes, as well as high school outcomes and health outcomes for peers, we cluster standard errors at the school-cohort level. For health outcomes of survivors we use robust standard errors. For family outcomes we cluster standard errors on the mother's personal identifier.

For medium-term outcomes, we cannot use the difference-in-differences approach, as we do not have a pre-period. As mentioned in Section 3.5, we match on middle school 10th grade exam scores when we consider medium-term outcomes. This is because we observed level differences between survivors and control individuals in exam scores (and GPA), which may correlate with or drive differences in future outcomes. We hope to reduce this by also matching on exam scores. To this end, a simple treatment-control comparison is performed:

$$Y_i = \beta Surv_i + \lambda X_i + \varepsilon_i$$

In all estimations (except for peers), we weight the observations of the control individuals using the following formula:

$$W = \frac{m_C}{m_T} \times \frac{m_T^s}{m_C^s}$$

where W is the weight for each individual, m_C and m_T are the number of control individuals

and survivors in the estimating sample, and m_C^s and m_T^s are the number of control individuals and survivors in the same stratum, i.e. with the same coarsened characteristics in the matching procedure.

5 RESULTS

Figure 1 graphically presents the DiD estimates of the effects for survivors. Each dot is the difference between survivor mean outcome and the matched control mean outcome. The line gives the 95% confidence interval. Panels (a) and (b) plot the effects on middle school exam scores and GPA relative to 2011. We see no significant differences in the years prior to 2011, while there is a drop in 2012 and 2013. Table 1 gives us the size and significance levels and shows a drop of around 0.3 of a standard deviation on exam scores (column 1), and 0.5 SD decrease in GPA (column 2), where only the latter is significant at the 1% level. Returning to Figure 1, panels (c) and (d) show similar patterns for completion of 1st and 3rd grade in high school. Table 1 confirms that these are significant effects with a drop of 18 and 20 percentage points (columns 3 and 4) for the probability of finishing the first and third year in high school, respectively. Finally in Figure 1, panels (e) and (f), we see large increases in the utilization of health services. As seen in Table 1 the effects are a 0.64 increase in the number of GP visits (from a base of about 1) and a large portion of this is related to the incidence of psychological diagnoses, which increases by 36 percentage points from a base of around 10%.

For survivors, we then estimate the effects on earnings by looking at the medium-term effects in panel B of Table 1. In 2018, survivors have 12% lower earnings in the labor market compared to the matched control group, although this effect is not significant. We also see that both education (6 pp lower probability of completing higher education by 2018) and labor force participation (5 pp lower in 2018) are affected.

Turning to spillovers to family members, Table 2 summarizes the main estimates for parents. In Table 2 we estimate effects on both mothers' and fathers' labor force participation, log-earnings, the incidence of any sickness absence from work, an marital status. For mothers and fathers only the effect on sickness absence is significant and economically meaningful. Column 3 therefore suggests that mothers increase sickness leave take up by around 28%, while fathers increase by around 5%. These effects are mirrored when we examine doctor visits and mental health diagnoses for parents. While there is a substantial increase in doctor visits and mental health diagnoses for both parents, mothers seem much more impacted on this margin than fathers. Figures A4 and A5 show these results graphically. Table 3 turns to examining siblings of the direct survivors. This table shows that siblings also see lower test scores and increases in mental health diagnoses. While the effect on siblings is substantial (0.22 SD effect on test scores and GPA), nearly 25% increase in mental health diagnoses, the magnitude of the effects

is smaller than what we see for the direct survivors. Figure B6 in the Online Appendix shows these results graphically.

The next group we study are school mates of the survivors. In Table 4 we look at exams and GPA for middle school peers, completion of 3rd year for high school peers and GP visits and psychological diagnosis for all peers. While there is a significant negative effect on the test scores of female peers and a small increase in mental health diagnoses for female peers, since these are the only statistically significant effects across a range of outcomes we examine in this table, we do not wish to place undue focus on these effects. Our reading of Table 4 suggests muted effects on the peers of direct survivors.

6 Conclusion

This paper shows that children who survive gun violence face tremendous costs in terms of school performance and mental health in the short run. In the medium run, survivors have lower years of education and worse labor market outcomes. Parents and siblings of direct survivors are also impacted in terms of health and school test scores.

While this setting allows us to examine effects of a mass shooting event on survivors and those around them, we want to be upfront that this was a unique event (like most mass shootings) and there are challenges in drawing broad conclusions from this study that might apply to the case of school shootings or other forms of gun violence in the United States and elsewhere. The event we study was one of the deadliest gun attacks on Norwegian soil and the government response to victims, survivors, and their families was tremendous. The attack was also not on school premises as most mass shootings that involve children typically are, which means schools were not directly affected and there was no resorting of students and resources. Norway is also a setting where gun violence and mass shootings of this kind is extremely rare. While some of these factors (social support and government assistance) imply that the results we find are *net* of such compensatory actions, other factors such as the rarity of this event in this context might imply that in places where gun violence is more common, student, peer, and family reactions might be more muted. Finally, our design relies on comparing those who are directly affected to a “control" group who might also be impacted by this event. Ultimately the empirical design only allows us to pick up *relative* effects and does not capture broad, overall impacts on the population. These considerations should be taken into account when thinking about how these results help make sense of similar events in different contexts.

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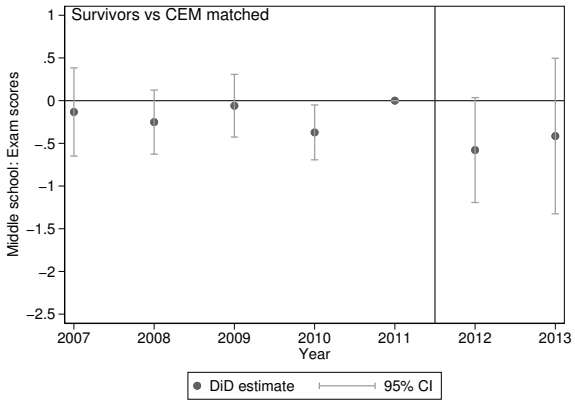
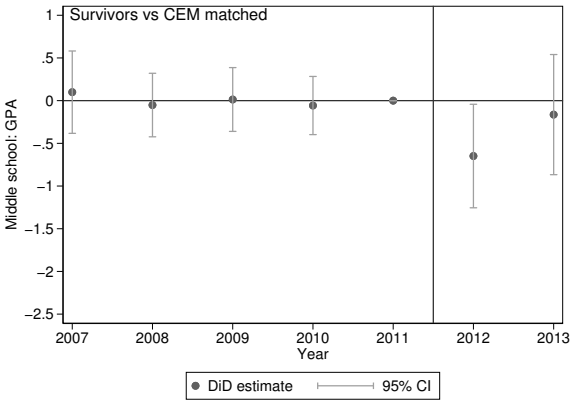
Figures

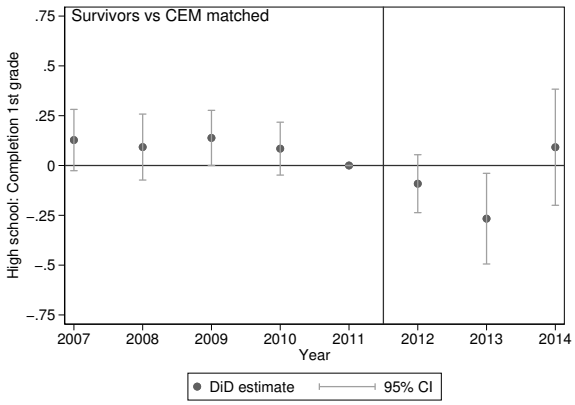
FIGURE 1: Yearly DiD Estimates: Comparisons of Survivors and CEM Matched Control



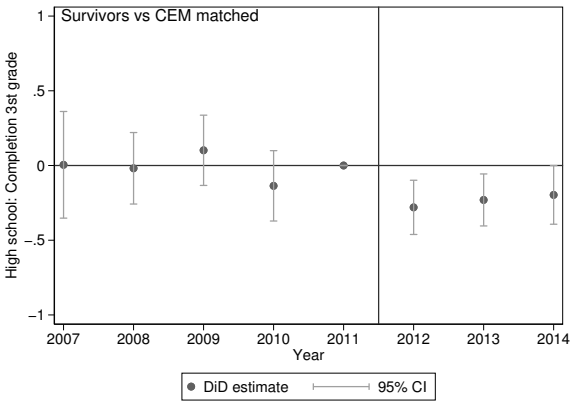
(A) Middle School: Exam Scores



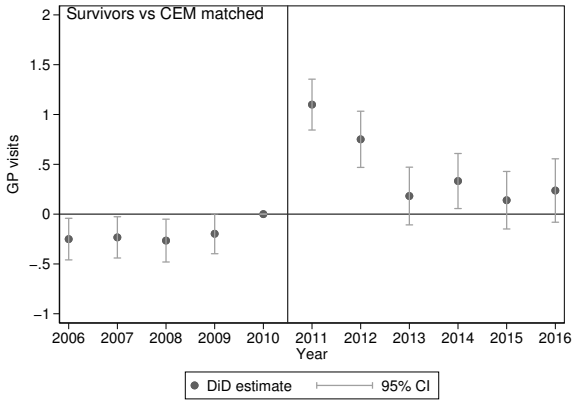
(B) Middle School: GPA



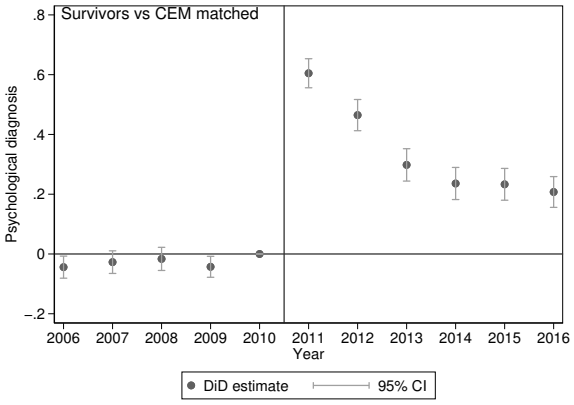
(C) High School: Completed 1st Grade



(D) High School: Completed 3rd Grade



(E) Number of GP Visits



(F) Psychological Diagnosis

Notes: Survivors have been matched to control individuals using coarsened exact matching (CEM) on birth year, gender, parental earnings, education and age, and the centrality of the residential municipality. The sample in panels (a) and (b) includes cohorts 1991-1997. Panel (c) includes cohorts 1990-1997 and panel (d) 1990-1996. Panel (e) and (f) includes cohorts 1983-1997. The DiD estimate is the difference in weighted mean outcome between the survivors and control individuals, minus the equivalent in 2010. In panel (a)-(d) we compare outcomes for individuals finishing a certain level of education in a given year, to those finishing in academic year 2010/11. In panel (e) and (f) we observe outcomes for all individuals in the sample in each year. In panel (a) and (b), outcomes are standardized within each academic year, and standard errors are clustered on the school-cohort level.

Tables

Table 1: Impacts on Short- and Medium-term Outcomes of Survivors

	Middle School		High School		Health	
	(1) Exam Scores	(2) GPA	(3) Completion 1st Grade	(4) Completion 3rd Grade	(5) Number of GP Visits	(6) Psychological Diagnosis
Panel A: Short-term						
DiD	-0.326	-0.527***	-0.189***	-0.204***	0.644***	0.367***
	(0.233)	(0.196)	(0.0592)	(0.0557)	(0.0849)	(0.0165)
Pre-event mean, survivors	0.385	0.393	0.806	0.681	1.057	0.0954
N	6401	6605	7567	7697	9032	9032

	Higher Education		Labor Market	
	(7) Enrolled	(8) Completed	(9) LFP	(10) Log-Earnings
Panel B: Medium-term				
Treated-Control Difference	0.0475	-0.0641**	-0.0527***	-0.122
	(0.0334)	(0.0321)	(0.0204)	(0.0936)
Dependent mean, control	0.477	0.432	0.936	12.22
N	2021	2021	1958	1643

Notes: In column (1)-(6), survivors born in 1983-1997 have been matched to control individuals using coarsened exact matching (CEM), on birth year, gender, parental earnings, education and age, and centrality of municipality. In column (7)-(10), the same matching but adding 10th grade exam scores has been done for individuals born in 1986-1995. The sample in column (1) and (2) displays outcomes for the end of 10th grade for academic years 2006/07-2012/13 (cohorts born in 1991-1997). Column (3) and (4) includes outcomes academic years 2006/07-2013/14 and 2007/08-2014/15 respectively (cohorts 1990-1997 and 1990-1996). Column (5) and (6) includes all matched individuals for each year 2006-2016. Column (7) is measured in academic year 2014/15, column (8) in 2016, and column (9) and (10) in 2018. The DiD estimates (column (1)-(6)) is the difference in weighted mean outcomes between the survivors and the control individuals after the academic year 2010/11 (calendar year 2010 for health outcomes), minus the equivalent before. Outcomes in column (1) and (2) are standardized for each year. The estimates in column (7)-(10) is the difference in weighted mean between survivors and the control individuals. Standard errors are clustered at the school-cohort level for columns (1) and (2), otherwise we use robust standard errors.

Standard errors in parenthesis.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

Table 2: Impacts on Parents of Survivors

	Labor Market			Marital Status	Health	
	(1) LFP	(2) Log-Earnings	(3) Any Sickness	(4) Married	(5) Number of GP Visits	(6) Psychological Diagnosis
Panel A: Mothers						
DiD	0.00242	0.00310	0.0671***	-0.00367	0.474***	0.142***
	(0.0134)	(0.0242)	(0.0179)	(0.0146)	(0.161)	(0.0169)
Pre-event mean, treated	0.890	12.85	0.241	0.611	2.830	0.212
N	8505	6962	8505	8504	8505	8505
Panel B: Fathers						
DiD	0.00455	-0.00361	0.00912	-0.0138	0.171	0.0743***
	(0.0108)	(0.0314)	(0.0141)	(0.0163)	(0.133)	(0.0151)
Pre-event mean, treated	0.897	13.13	0.181	0.644	2.161	0.141
N	8247	6634	8247	8247	8247	8247

Notes: Survivors born in 1983-1997 have been matched to control individuals using coarsened exact matching (CEM), on birth year, gender, parental earnings, education and age, and centrality of municipality. Their family members are found by using mother and father identification numbers. All estimations are for years 2006-2016. The DiD estimate is the difference in weighted mean outcome between the parents survivors and the control individuals after 2010, minus the equivalent before. LFP and any sickness benefit is defined as having any positive labor income or sickness benefit in a given year. Earnings are CPI adjusted. Being married is measured 1 January the following year. Standard errors are clustered on mother ID.

Standard errors in parenthesis.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

TABLE 3: Impacts on School and Health Outcomes of Siblings of Survivors

	Middle School		Health - Younger Siblings		Health - All Siblings	
	(1) Exam Scores	(2) GPA	(3) Number of GP Visits	(4) Psychological Diagnosis	(5) Number of GP Visits	(6) Psychological Diagnosis
DiD	-0.227*	-0.220*	0.0522	0.0281	0.0377	0.0311***
	(0.132)	(0.128)	(0.106)	(0.0182)	(0.0818)	(0.0119)
Individual FE	No	No	Yes	Yes	Yes	Yes
Pre-event mean, treated	0.396	0.318	0.984	0.0783	1.524	0.120
N	2992	3196	4693	4693	12054	12054

Notes: Survivors born in 1983-1997 have been matched to one control individual using coarsened exact matching (CEM), on birth year, gender, parental earnings, education and age, and centrality of municipality. Their siblings are found by using mother and father identification numbers. The sample columns (1)-(4) includes only younger siblings and columns (5)-(6) include all siblings. Column (1) and (2) includes academic years 2006/07-2015/16, and columns (4)-(6) includes years 2006-2016. The DiD estimate is the difference in weighted mean outcome between the siblings of survivors and the control individuals after 2010 (academic year 2010/11), minus the equivalent before. Exam scores and GPA are standardized. Standard errors are clustered on mother ID.

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Impacts on School Peers of Survivors

	Middle School		High School	Health	
	(1) Exam Scores	(2) GPA	(3) Completion 3rd Grade	(4) Number of GP Visits	(5) Psychological Diagnosis
Panel A: All Peers					
DiD	-0.0694	-0.0251	0.00679	-0.0600	0.00802
	(0.0519)	(0.0574)	(0.0278)	(0.0468)	(0.00580)
Pre-event mean, peers of survivors	0.00316	-0.0181	0.540	1.379	0.101
N	51700	52290	84717	115492	115492
Panel B: Female Peers					
DiD	-0.161***	-0.0224	-0.0184	-0.0683	0.0140*
	(0.0565)	(0.0656)	(0.0269)	(0.0731)	(0.00798)
Pre-event mean, peers of survivors	0.164	0.212	0.649	1.721	0.114
N	24940	25562	41306	56269	56269
Panel C: Male Peers					
DiD	0.0256	-0.0225	0.0301	-0.0540	0.00218
	(0.0620)	(0.0718)	(0.0320)	(0.0378)	(0.00654)
Pre-event mean, peers of survivors	0.164	-0.234	0.432	1.051	0.0885
N	25948	26728	43411	59223	59223

Notes: Survivors born in 1983-1997 have been matched to one control individual using coarsened exact matching (CEM), on birth year, gender, parental earnings, education and age, and centrality of municipality. Peers are individuals in the same school and cohort as the survivor or matched control individual. For column (1) and (2) peers are defined in 10th grade, and we include cohorts 1991-1997. For column (3), peers are defined in 1st grade of high school, and we include cohorts 1988-1994. In column (4) and (5) we combine the samples of column (1) and (2), and only include the academic year they were supposed to finish 10th grade in middle school or 3rd grade in high school. We include academic years 2006/07-2012/13 in all regressions. The DD estimate is the difference in mean outcomes between the peers of survivors and the peers of control individuals after the academic year 2010/11 (calendar year 2010 for health outcomes), minus the equivalent before. Outcomes in column (1) and (2) are standardized for each year. Standard errors are clustered at the school-cohort level.

Standard errors in parenthesis.

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

ONLINE APPENDIX A: DETAILS ON DATA AND SAMPLE SELECTIONS

SURVIVORS

- *Middle school*
  - Academic years: 2006/07-2012/13. Note that 2012/13 is the last year with a sufficient number of survivors to be included in the sample.
  - Cohorts: 1991-1997.
  - Only survivors with a matched control individual are included.
  - Remove pairs of survivors/control if either the survivor or the control individual are not found in the middle school grade registry the year they turned 16.
  - Only observe each individual once in each regression.
- *High school*
  - Academic years: Grade 1: 2006/07-2013/14. Grade 3: 2008/09-2014/15. Note that 2014/15 is the last year with with a sufficient number of survivors to be included in the sample. For grade 3, 2014/15 is also our last available year.
  - Cohorts: Grade 1: 1990-1997. Grade 3: 1990-1996.
  - Only survivors with a matched control individual are included.
  - Completion coded as zero if individual is not found in the high school registry data set in the relevant year.
  - Only observe individual once in each regression.
- *Health*
  - Calendar years: 2006-2016.
  - Cohorts: 1983-1997.
  - Only survivors with a matched control individual are included.
  - Individuals are observed every year. If they are not found in the KUHR-dataset in the relevant year, variables are set to zero.
- *Long-term outcomes*
  - Years: Enrolment in higher education in 2014/15. Completion of any higher education by 2016. Labor force participation (LFP) and income in 2018.
  - Cohorts: 1986-1995.

  - If the survivor or the matched control individual has emigrated or died before 2015 (Enrolment) or 2016 (Completion, LFP and income), then the survivor-control pair is removed from the sample.
  - Note that different cohorts are measured at different ages and we use the last available year of data for each outcome.

PARENTS

- Parents are found with mother's and father's ID in the population registry.
- If someone is a parent of more than one survivor (or control individual), we duplicate/triplicate this individual so that we observe them as many times as they have children for each year. Since each survivor has a control individual, there will be an equal number of parent observation in total.
- All parents of children born in 1988-1997.
- Only parents of matched survivors are included in the sample.
- Restrict to individuals living by December 31, 2014.
- Residents in Norway for the whole period (has used immigration and emigration dates).
- *LFP, ln(Income) and Any sickness benefit*
  - Years: 2006-2014
  - Estimations for mothers and fathers are run separately.
- *Consumption*
  - Years: 2006-2014
  - Note that both parents are included separately in the estimation. If they cohabit, the household is observed multiple times.

YOUNGER SIBLINGS

- Individuals are defined as siblings if they have the same mother.
- Some victims had a sibling that was also a victim, and the oldest sibling has then been chosen as the focal victim.
- We remove control individuals for victims with older siblings that were also victims.
- We remove control individuals for victims without any (non-victim) siblings.

- We remove victims with control individuals without siblings.
- Siblings are defined as younger if they are younger than the oldest victimized sibling. If they are born in the same year as the oldest victimized sibling, they are excluded from all analyses.
- *Middle school exam score*
  - Academic years: 2006/07-2015/16
  - Cohorts: 1991-2000

PEERS

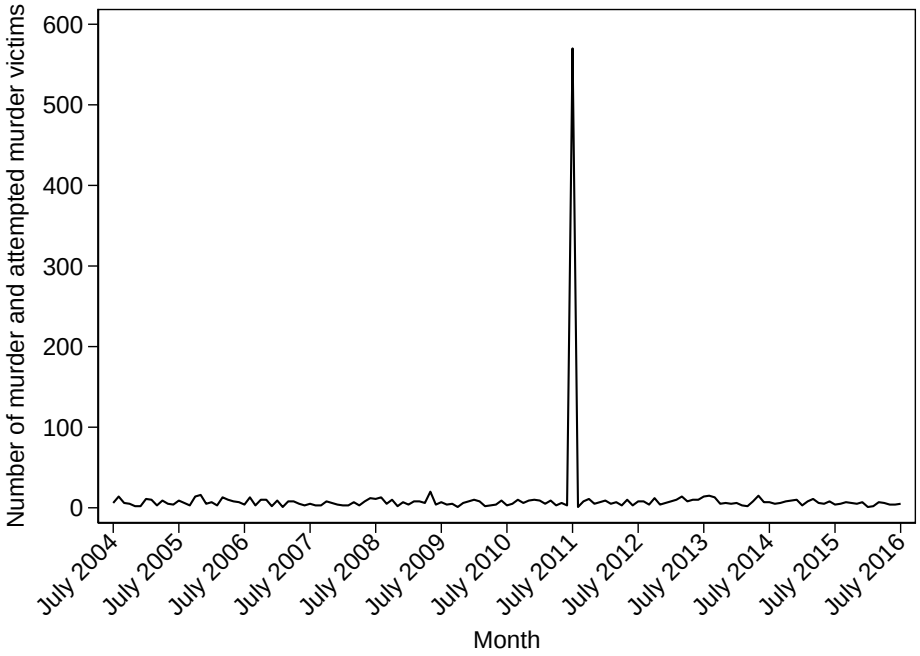
- In the following, we call the survivors and their matched controls as the focal individuals.
- *Middle school*
  - Peers are individuals in the same school and cohort as the focal individual.
  - We only include schools of survivors that have a match.
  - Only observe each individual once.
  - Academic years: 2006/07-2012/13. Note that 2012/13 is the last year with a sufficient number of survivors to be included in the sample.
  - Cohorts: 1991-1997.
- *High school*
  - We define peers as being in the same school and cohort as the focal individual in grade 1 of high school.
  - We only include those starting high school before July 22, 2011 (time of event), otherwise high school choice/enrolment could be endogenous.
  - We only include schools of survivors that has a match.
  - Academic years: 2006/07-2012/13. We cannot observe longer, as they have to have started high school before July 22, 2011.
  - Cohorts: 1988-1994.
- *Health*
  - We pool peers from middle school and high school sample.
  - We observe them in the same academic year as they are in the middle school or high school sample.

- Only observe each individual once.
- Academic years: 2006/07-2012/13 (August-June).

ONLINE APPENDIX B: ADDITIONAL RESULTS

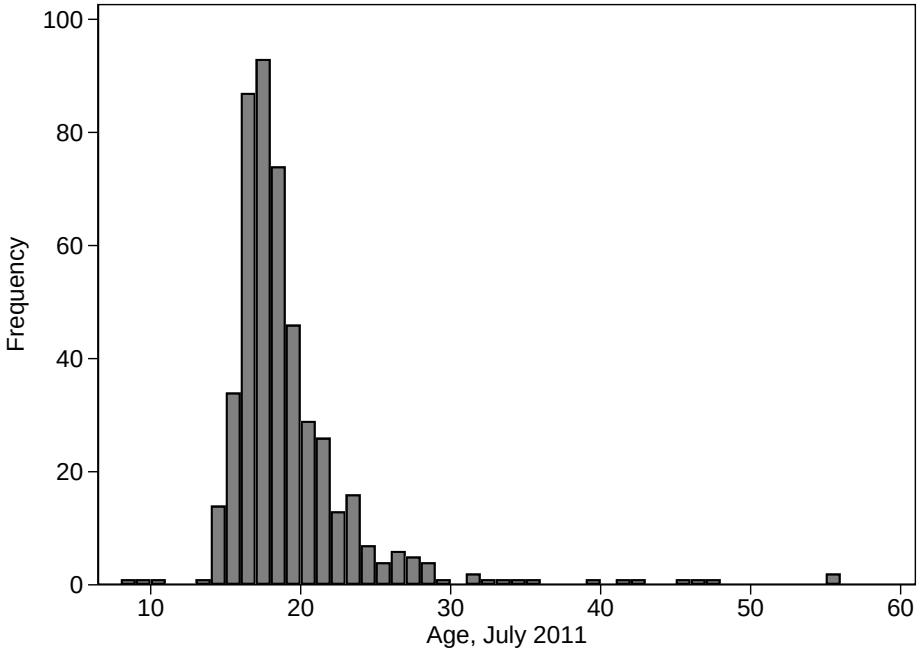
ADDITIONAL FIGURES

FIGURE B1: Number of Murder or Attempted Murder Victims – July 2004 to July 2016



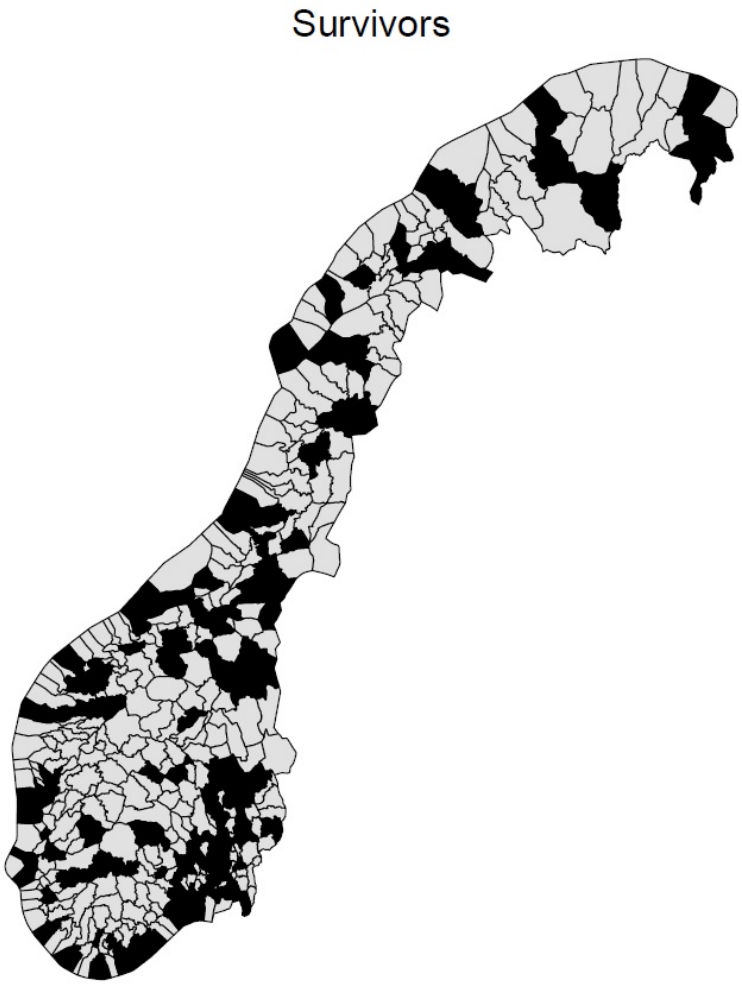
Notes: In the figure we show the number of individuals with a registered victimization in a given month, coded as murder or attempted murder.

FIGURE B2: Age Distribution of Survivors of the July 2011 - Utøya Incident



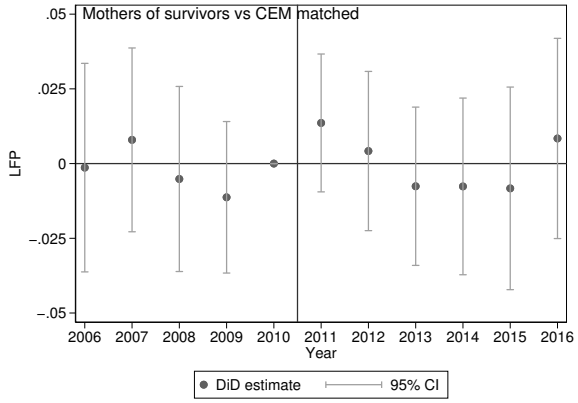
Notes: Only residents of Norway are included in the figure.

FIGURE B3: Map: Residence Municipality of Survivors of the July 2011 - Utøya Incident

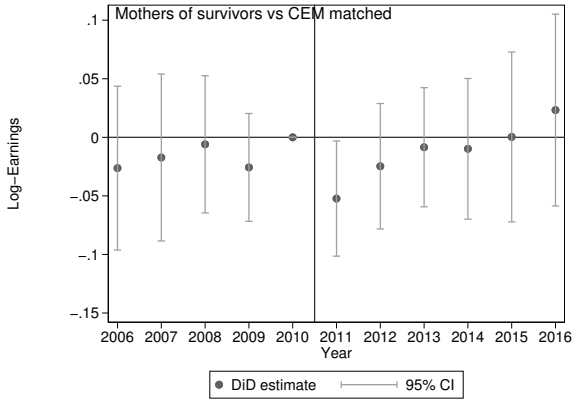


Notes: Survivor's municipality of residence is defined by the mother's residential municipality in 2010 (with some exceptions).

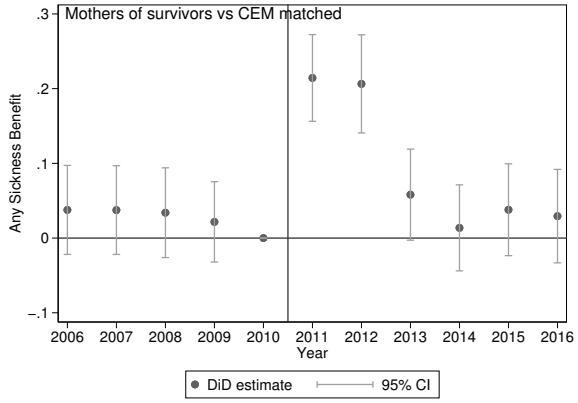
FIGURE B4: Yearly DiD Estimates: Mothers of Survivors vs CEM Matched Control



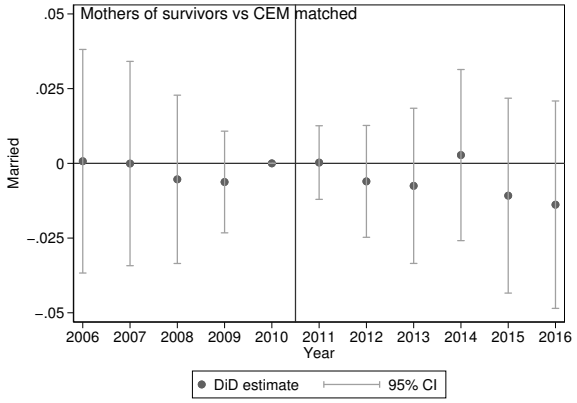
(A) LFP



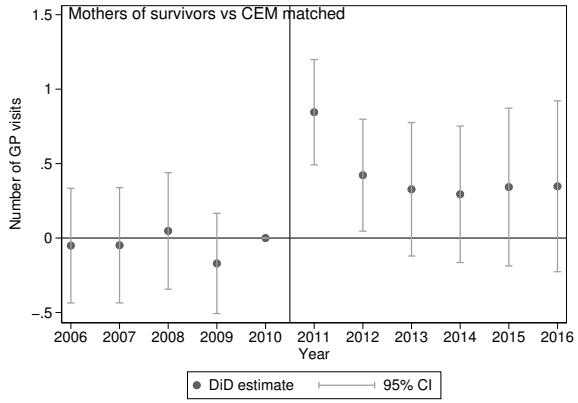
(B) Log-Earnings



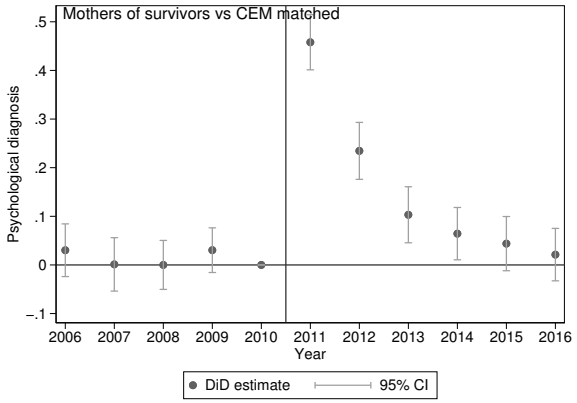
(C) Any sickness benefit



(D) Married



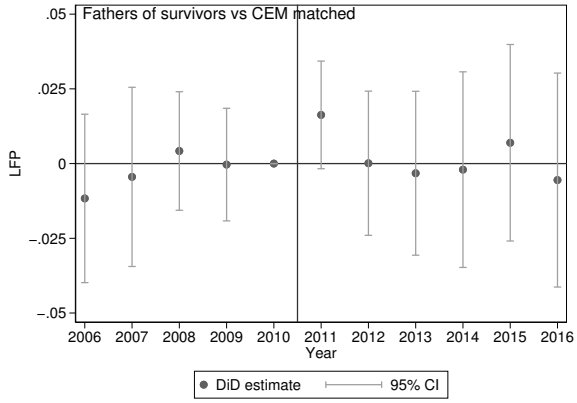
(E) Number of GP visits



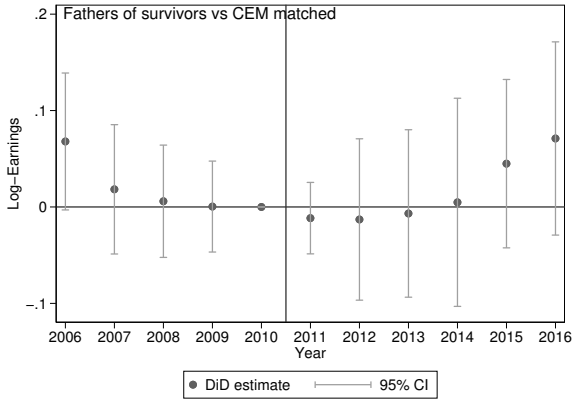
(F) Psychological diagnosis

Notes: Survivors have been matched to control individuals using coarsened exact matching on birth year, gender, parental earnings, education and age, and the centrality of the residential municipality. The DiD estimate is the difference in weighted mean outcome between the mothers of survivors and the control individuals, minus the equivalent in 2010. LFP and any sickness benefit is defined as having any positive labor income or sickness benefit in a given year. Earnings are CPI adjusted. Being married is measured 1 January the following year. Standard errors are clustered on mother ID.

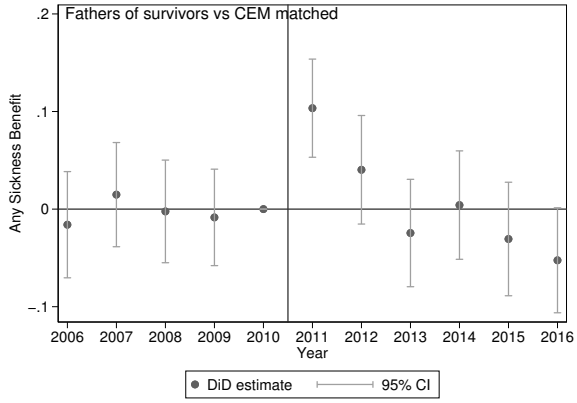
FIGURE B5: Yearly DiD Estimates: Fathers of Survivors vs CEM Matched Control



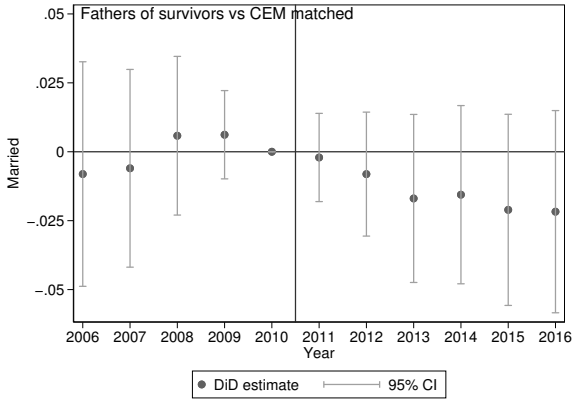
(A) LFP



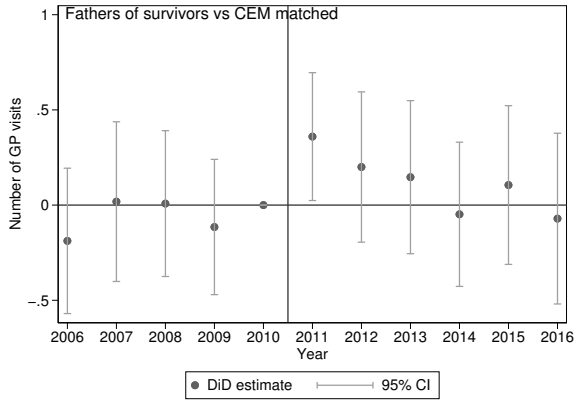
(B) Log-Earnings



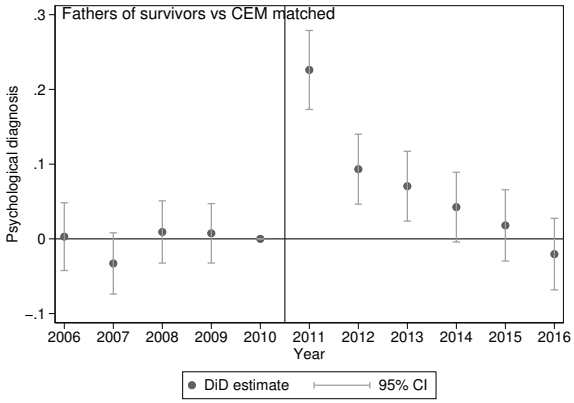
(C) Any sickness benefit



(D) Married



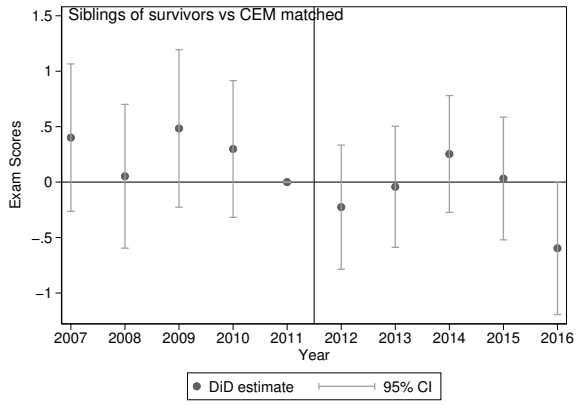
(E) Number of GP visits



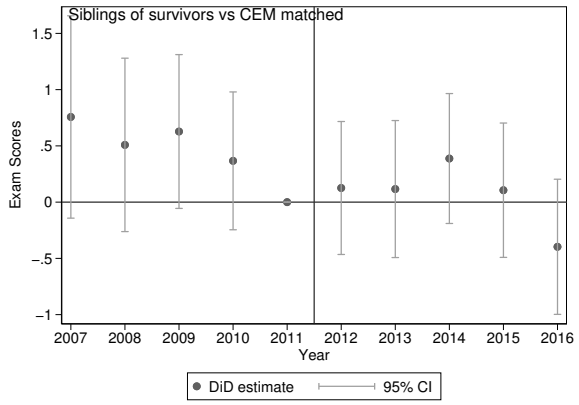
(F) Psychological diagnosis

Notes: Survivors have been matched to control individuals using coarsened exact matching on birth year, gender, parental earnings, education and age, and the centrality of the residential municipality. The DiD estimate is the difference in weighted mean outcome between the mothers of survivors and the control individuals, minus the equivalent in 2010. LFP and any sickness benefit is defined as having any positive labor income or sickness benefit in a given year. Earnings are CPI adjusted. Being married is measured 1 January the following year. Standard errors are clustered on mother ID.

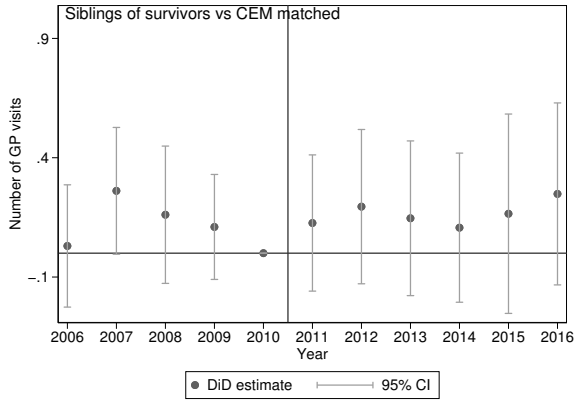
FIGURE B6: Yearly DiD Estimates: Siblings of Survivors vs CEM Matched Control



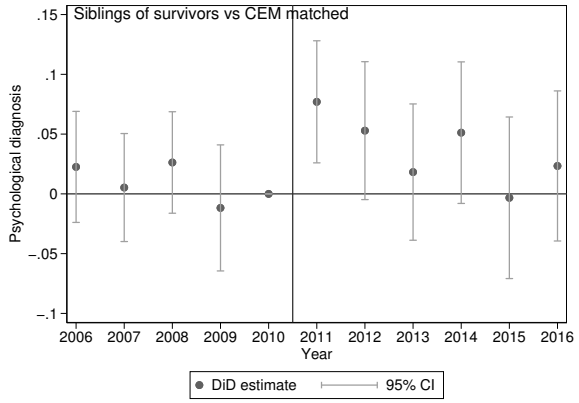
(A) Younger: Exam Scores



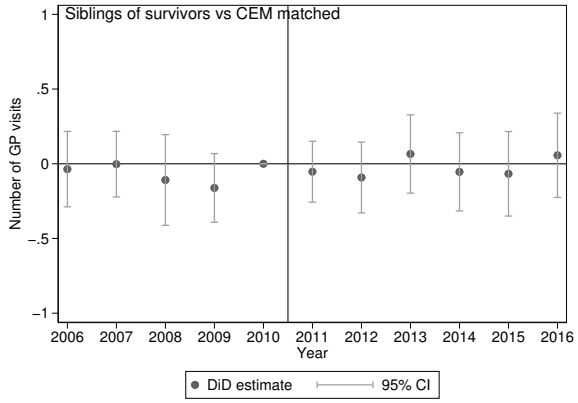
(B) Younger: GPA



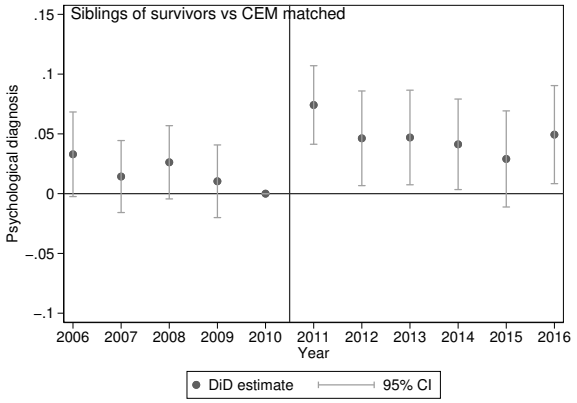
(C) Younger: N. GP visits



(D) Younger: Psych. Diagnosis



(E) All: N. of GP visits



(F) All: Psych. diagnosis

Notes: Survivors have been matched to control individuals using coarsened exact matching on birth year, gender, parental earnings, education and age, and the centrality of the residential municipality. The DiD estimate is the difference in weighted mean outcome between the siblings of survivors and the control individuals, minus the equivalent in 2010. The sample panel (a)-(d) includes only younger siblings and columns (e) and (f) include all siblings.

ADDITIONAL TABLES

TABLE B1: Descriptive Statistics – Characteristics of Survivors

	Mean
Female	0.46
Mother's age at birth	28.56
Mother's earnings in 1000 NOK, 2010	351.74
Mother with higher education	0.48
Father's age at birth	31.53
Father's earnings in 1000 NOK, 2010	473.87
Father's age at birth	31.53
Father with higher education	0.37
Parents live in urban area	0.36
N	438

Notes: The sample consists of survivors born in 1983-1997. Survivors without matched control individuals from the Coarsened Exact Matching (CEM) procedure are left out of the sample.

TABLE B2: Descriptive Statistics – Outcomes of Survivors

	Pre-event	Post-event
Middle School		
Exam scores	0.38	-0.17
GPA	0.39	-0.35
High School		
Completion 1st grade	0.81	0.64
Completion 3rd grade	0.68	0.46
Health		
Number of GP visits per year	1.06	2.31
Psychological diagnosis in a year	0.10	0.52

Notes: The full sample consists of survivors born in 1983-1997. Survivors without matched control individuals from the Coarsened Exact Matching (CEM) procedure are left out of the sample. The sample size for each row varies depending on the estimation.