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Going Through Hell – Increased Work Effort in the Aftermath of Terrorism in Norway*

Øystein Hernæs†

> If you're going through hell, keep going.
>
> Winston Churchill

Abstract

I analyze how sickness absence behavior in Norwegian municipalities was affected by exposure to the terrorist attack in Norway in 2011. The main finding is that in municipalities from which a resident was killed in the attack, sickness absence declined by 4% compared to municipalities without victims. The effect is precise, stable, and persistent. The results do not seem to be driven by changing labor market composition. The effect is found also in neighboring municipalities and is larger for people close to the age of the victims.

Keywords: absenteeism, psychology, sickness, trauma, terrorism, work effort

JEL Classification: A12, I12, J22

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1 Introduction

In the years after the 9/11 attacks in the US, there has been a growing interest in the economic effects of terrorism.[1] This literature has tended to focus on economy-wide aggregate measures, see e.g. Abadie and Gardeazabal (2008), and to study contexts of long-lasting conflict, such as the conflicts in the Basque country (Abadie and Gardeazabal, 2003) and Israel-Palestine (Becker and Rubinstein, 2011). Less is known about the economic effects at a less aggregate level in situations of less permanent conflict, which describes most industrialized countries. A terrorist attack in such circumstances typically leaves the material structure of society largely intact (IMF, 2001), and thereby provides a setting to test mechanisms operating through other channels than economic fundamentals.

On July 22, 2011, Norway was struck by a terrorist attack in which 77 people were killed. I analyze the effect of local exposure to the attack on sickness absence behavior. The attack was directed at the Norwegian government generally, and at the Labor party in particular. The perpetrator placed a bomb in the government quarters in downtown Oslo before he went on to the summer camp of the Norwegian Labor Party's youth organization, a 45-minute drive and a short boat trip from Oslo. The summer camp is a traditional yearly gathering of Labor youth party members from around the country and always takes place on the island of Utøya. Most of the victims (69) were participants at the summer camp. The fact that the victims came from different parts of the country allows identification of how people in a local community responded to the killing of one of the members of that community. Thus, although the country as a whole was deeply affected by the attack, communities were impacted to an unequal degree. I analyze how absence levels were affected in victims' and survivors' home municipalities, using other municipalities as a control group. Besides being an economic effect that is important in its own right, sickness absence has an alternative interpretation in the labor economics literature as a measure of work effort.[2] An advantage with sickness absenteeism in this

[1]See Frey et al. (2007), Schneider et al. (2015) and Enders (2016) for reviews.

[2]Studies of the effects of financial incentives (Johansson and Palme, 2005; Ziebarth and Karlsson, 2010; Duflo et al., 2012; Böckerman et al., 2018), monitoring (De Jong et al., 2011; D'Amuri, 2011;

context is that it is something an individual can influence on his or her own without depending on anyone else, in contrast to most other labor market outcomes, such as employment.

The paper relates to a large literature on the effects of post-traumatic stress from exposure to various types of dramatic events, such as combat in war, terrorism, death, violence and assault.[3] Most of this literature has been concerned with the negative effects on people experiencing a shock first-hand. Also for the Norwegian case, studies have documented elevated post-traumatic stress among people who personally experienced the attack (Hansen et al., 2013; Dyb et al., 2014). Thus, some forces would be expected to increase sickness absence after the attack. However, an emerging literature on "post-traumatic growth" – "the experience of positive change that occurs as a result of the struggle with highly challenging life crises (Tedeschi and Calhoun, 2004, p.1)" – suggests that there may also be forces working in the opposite direction. This literature has documented links between positive change and exposure to several forms of trauma.[4]

The notion of post-traumatic growth fits particularly well with the narrative about the effects of the attack that has dominated in Norwegian society. The prime minister at the time, Jens Stoltenberg, received international attention for a widely distributed speech held two days after the attack, where he instead of reacting with anger, emphasized "love" and called for "more democracy, more openness, and more humanity (Stoltenberg, 2011).[5] The message of letting the time of crisis be a source of positive change was quickly taken up by commentators and others and reiterated in several government speeches in the following weeks.[6] Further, the term "generation Utøya" has commonly come to mean the generation who was adolescent at the time of the attack, and who was supposedly shocked into a more serious life-outlook and a higher societal awareness by experiencing

De Paola et al., 2014), group interaction effects (Ichino and Maggi, 2000; Bradley et al., 2007; Hesselius et al., 2009), and employment protection (Ichino and Riphahn, 2005; Olsson, 2009) show that there is indeed a substantial degree of choice in absenteeism.

[3] See e.g. Neria et al. (2008) and Yehuda et al. (2015) for reviews.

[4] See Barskova and Oesterreich (2009); Hefferon et al. (2009) and Vloet et al. (2018) for reviews and Ford et al. (2003); Laufer and Solomon (2006); Blattman (2009) for studies on terrorism and other forms of violence.

[5] E.g. Friedman (2011); Orange (2012).

[6] Dagbladet (2011); VG (2011); 22. juli-senteret (2019)

so many of their peers being brutally executed.[7] Indeed, despite reporting increased levels of symptoms of stress and depression, a majority of survivors from Utøya also reported that they had experienced some positive personal change because of the attack (Dyb and Næslund, 2015). The development of a new, more responsible perspective on life can be a source of increased effort.

I find that the attack lead to a substantial reduction in sickness absence in municipalities affected more severely by the attack. With the preferred empirical specification, the sickness absence rate fell by 0.24 percentage points following the attack in municipalities that lost an inhabitant. In municipalities that had inhabitants present on Utøya, the main site of the attack, but lost none, the sickness absence rate fell by 0.13 percentage points. Since the average absence rate is 6.10 % in this time period, these effects correspond to drops in the absence rate of 3.9 % and 2.1 %, respectively. The results are robust to controlling for several types of municipality characteristics and time trends. Absence rates also declined in municipalities sharing a border with a municipality with a deceased inhabitant. The estimated effect is larger in smaller municipalities and for workers in their early 20's. For these workers, close in age to most of the victims, it is 9 % – more than twice that for the population at large. I do not find evidence that the results are due to changing labor market composition, suggesting that the estimated post-traumatic growth effect is real and supporting the popular narrative about positive change.

Three recent contributions that are methodologically and topically related to my study are Tsai and Venkataramani (2015), Kim and Kim (2017) and Clark et al. (2018), who all also use a residency-based measure of terrorism exposure and a differences-in-differences approach. These authors analyze effects on well-being from exposure to the 9/11 attack in New York, the 2015 Charlie Hebdo attack in France and the Boston marathon bombing, respectively. To the best of my knowledge, this paper is the first to show that a community's exposure to a traumatic shock can instigate positive change in

[7] For academic articles, see Wollebæk et al. (2012); Bergh and Ødegård (2013); Lödén (2017); for accounts of "generation Utøya" in the media, see e.g. Mæland (2016); Haug (2016), and Nordanger (2017).

labor market behavior.

2 Data

Data on victims come from two sources: The home municipalities of the 77 deceased victims are collected from Stormark (2011). In total 52 municipalities had at least one inhabitant killed. In 45 of the 52 municipalities with deceased victims, the age of youngest victim was 21 or below, in three municipalities, it was in the late 20's, in two municipalities, it was in the 40's and in the final municipality, it was 51. The main site of the attack was the island of Utøya. There 66 people were injured, while 585 survived without physical harm (Norwegian Directorate of Health, 2012). Data on the 127 municipalities from which there were inhabitants present on Utøya has been acquired from the 18 County Governors, who disbursed funds targeted at following up these individuals in their respective municipalities. These data only indicate that the municipality had inhabitants participating on the camp and include the municipalities with deceased victims, as participating municipalities would typically have more than one participant. There is also no available information about the number of survivors from individual municipalities. I categorize municipalities into three mutually exclusive "treatment" groups depending on how severely they were affected by the attack: A $T1$ group comprising municipalities from which an inhabitant was killed in the attack, a $T2$ group covering municipalities that lost none, but that had at least one inhabitant was present at the main site of the attack, and finally a control group consisting of municipalities with no participating members.

Quarterly data on physician-certified sickness absence are computed by Statistics Norway based on administrative registers covering the full Norwegian population. Sickness absences longer than 3 or 8 days require a certificate from a physician.[8] The maximum length of a sickness spell is 52 weeks, after which more permanent disability options are investigated if the individual has not recovered. The definition of the sickness absence rate is "man-days lost due to own sickness as a percentage of contractual man-days." The

[8] Sickness absences for 3 days or less are self-certified. For employees with employers that have signed up to the "inclusive work life" framework, self-certification for absences up to 8 days are allowed. Self-certified absences are not covered by the the administrative registers.

sickness absence rate is adjusted for contracted hours and is the best available measure of work-hours lost due to sickness. Another advantage with the absence rate is that it is published by age group, in addition to by municipality and quarter.

The rest of the municipality data is also based on population registers, and is extracted from Kommunedatabasen, a database on Norwegian regional data administered by the Norwegian Social Science Data Services (NSD). Five of the country's 428 municipalities, all from the control group, are dropped because of inconsistent time series caused by municipality mergers.

Table (1) shows descriptive statistics for several pre-attack municipality-level variables by future attack exposure, Columns (1)-(3), and tests of equal means between T1 and the control group, Columns (4)-(5), and T1 and T2, Columns (6)-(7). There are some systematic differences between the three groups. First, the T1 group has a higher level of sickness absence than the two others do. Second, there is considerable oversampling of large and densely populated municipalities into the treatment groups. This is not surprising, in that such municipalities likely had more participants, both because of the larger population itself and because more centralized municipalities offer easier travel routes. Third, income and education are considerably higher, as is common for more urban areas. There are also statistically significant differences in several other variables, though the differences here are smaller. This is the case for the share of the labor force in the public sector, the share of the labor force in the health or social sector, the female share of the labor force, the share of inhabitants receiving disability benefits, the Labor party vote share in 2009 and the Progress party vote share in 2007. Even though many of these variables change relatively little over time and will therefore largely be absorbed by municipality fixed effects, several will be included as control variables in the empirical analysis.

Although what is important for my purposes is that the trends of the groups balance, since all permanent differences will be absorbed by the municipality fixed effects, I include a discussion in section A.1 in the online appendix of where the difference in pre-level sickness absence levels might come from. The conclusion is that they seem to be related

Table 1: Descriptive statistics 2010 (pre-attack) by future attack exposure status

	mean			T1 vs. Control		T1 vs. T2	
	T1	T2	Control	Δ	t	Δ	t
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
sickness absence rate, %	6.30	6.04	5.97	0.332	1.79	0.262	1.56
population, 1000	41.77	13.43	3.94	37.830	6.68	28.341	3.41
population density	0.73	0.62	0.41	0.319	8.37	0.116	2.93
income (USD 1000)	51.22	50.15	46.99	4.233	6.37	1.075	1.28
unemployment males, %	2.69	2.49	2.60	0.094	0.59	0.200	1.85
public sector, share of labor force	0.21	0.20	0.25	-0.040	-3.89	0.010	1.18
health and social, share of labor force	0.21	0.20	0.21	0.005	0.92	0.015	2.91
female workers, share of labor force	0.47	0.47	0.46	0.015	6.51	0.005	2.38
education, average years of schooling	11.96	11.79	11.55	0.412	7.90	0.170	2.78
disability recipients, share of inhabitants	0.07	0.07	0.07	-0.007	-2.20	0.000	0.10
welfare recipients, share of inhabitants	0.03	0.02	0.03	0.001	0.43	0.001	0.78
Labor party vote share 2007	0.30	0.31	0.30	0.009	0.43	- 0.001	-0.06
Labor party vote share 2009	0.37	0.36	0.34	0.029	2.11	0.014	0.96
Progress party vote share 2007	0.18	0.16	0.10	0.076	5.39	0.015	0.99
Progress party vote share 2009	0.24	0.24	0.22	0.021	2.25	-0.000	-0.03
N	52	122	249				

Note: Sickness absence rate measures "man-days lost due to own sickness as a percentage of contractual man-days;" population density is the share of inhabitants in a municipality living in an "urban settlement" (a hub of buildings inhabited by at least 200 persons, with a minimum distance of 50 m between buildings); income is average gross income of all persons aged 17 or more, converted to USD with an exchange rate of 1 USD=6.5 NOK; unemployment is percentage of men with no earnings, averaged through the year; public sector counts share of workers employed in municipal or state administration; health and social denotes share of workers employed in health and social services, both public and private.

to more people participating from urban municipalities, which tend to have somewhat higher absence rates than more rural municipalities in the same area.

3 Empirical strategy

3.1 Identification

The identification comes from the geographical distribution of participants and victims. I employ a difference-in-differences design with two treatments that vary between municipalities: Whether one or more individuals from the municipality were killed in the attack ($T1$); and whether one or more people were present on Utøya, but of whom none were killed ($T2$). Thus, the treatments are mutually exclusive. Since $T1$ can be considered similar to, but more intense than $T2$, I expect the effects to have the same direction, but the effect of $T1$ to be stronger. The identifying assumption is that conditional on area and time fixed effects, participation at the camp and the identity of the victims are uncorrelated with determinants of sickness absence. This assumption will be challenged in several ways later.

Another critical assumption is that people living in a municipality from which someone was killed or survived received a higher degree of exposure to the attack than people living in other municipalities. Several mechanisms would have contributed to this. First, the most immediate channel would have been direct and indirect personal connections, as people from the same municipality are much more likely to know each other.

Second, local identity is strong in Norway. This can be evidenced by high turnout in municipal elections (60-65 %) and the fact that most within country migration takes place within municipality boundaries (Statistics Norway, 2013). In a 2004 survey, only around half of respondents felt an affiliation ("tilhørighet") to people in their neighboring municipalities (Frisvoll and Almås, 2004). This indicates a higher degree of interest in things that have to do with one's municipality.

Third, differential media coverage probably played a role: There is a substantial share of people who report to read district and local newspapers (Statistics Norway, 2014), of which the country has almost 200 (Høst, 2012). Local newspapers typically cover local events and information about local individuals, increasing the chance that the whole municipality would receive knowledge of local youth's participation at Utøya.

Fourth, news in district and national newspapers in Norway routinely include information about the municipality of residence of people they are covering, which highlights that this information is seen as capturing a salient feature of a person's identity. This was also true in reporting on the victims of the 22 July attack. In fact, basic information about the deceased victims, which includes their home municipality at the time, continues to be maintained on line by two major Norwegian news outlets (NRK, 2015; VG, 2015). The fact that news about individuals is routinely accompanied by information about their municipality of residence also indicates the perception that this information captures a salient feature of a person's identity. Such reporting was the case with the July 22 attack as well, and in fact lists of victims and basic information about them, such as name, age, and home municipality, continue to be maintained on line by two of the major news outlets to this day (NRK, 2015; VG, 2015).

Finally, there were several public memorial events organized for the general population around the country in the aftermath of the attack. In the election survey after the 2011 election, respondents were asked if they had attended any of these events. 15 percent of respondents reported that they had, and people from municipalities with deceased victims at a 50% higher rate than people from control municipalities. When controlling for population and pre-attack sickness levels, the cross-sectional association between living in a *T1* municipality and having attended a memorial event was still substantial and statistically significant.[9] In some municipalities, the organization of public memorial events around July 22 has become a yearly occurrence that is advertised at the central government website(Regjeringen, 2019).

3.2 Time trends

It is essential for the difference-in-differences design that time trends are parallel, I therefore show in Figure (1) the time trends in the pre- and post-period. From Figure (1) alone it is possible to see the basic findings. The groups clearly follow very similar trends before the attack. The dashed, vertical line indicates the quarter in which the attack took place, after which the level of the T1 group visibly drops relative to the others, while the T2 group exhibits a more delayed response.

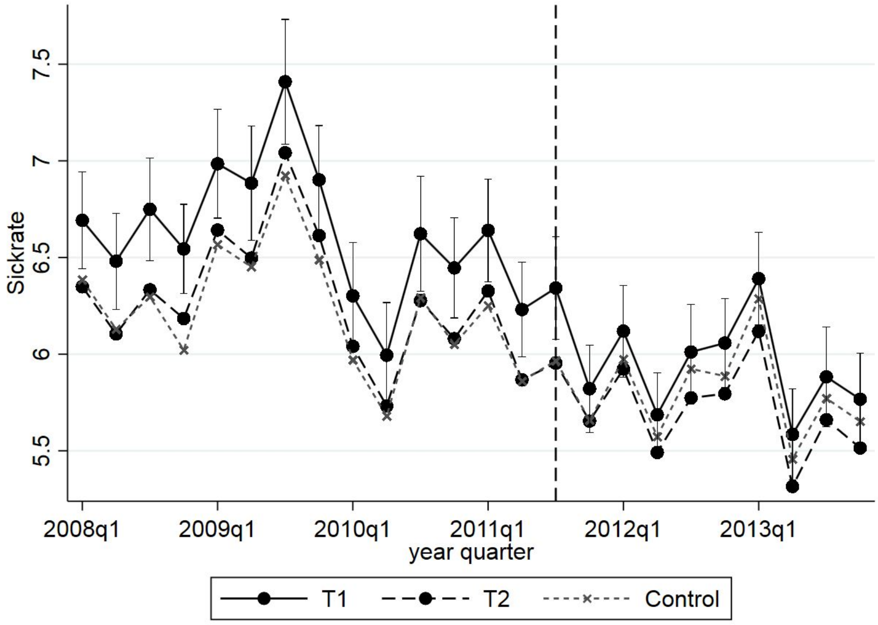
3.3 Empirical model

I estimate the following linear baseline empirical model:

$$y_{mt} = \beta_1 \; T1_{mt} + \beta_2 \; T2_{mt} + \delta_m + \gamma_t + \epsilon_{mt} \tag{1}$$

[9]See Table (A6) in the online appendix .

Figure 1: Sickness absence 2008 - 2013 by quarter and attack exposure



Note: The dashed, vertical line indicates the quarter in which the terrorist attack took place. The capped, vertical lines show 95% CI for the T1 group. The large drop in 2009–2010 was associated with a severe outbreak of swine flu in 2009 and public attention directed at the high sickness absence level around the same time because of a proposed reform of a part of the welfare system (*IA-avtalen*) (Nossen, 2011; Norwegian Institute of Public Health, 2015). These factors affected the whole country, and are absorbed by time fixed effects in my regressions.

y_{mt} denotes sickness absence rate in percent; $T1$ and $T2$ are indicator variables which are 0 for all municipalities and all time periods up to and including the third quarter of 2011, then switch to 1 in the remaining periods for those municipalities that received the respective attack exposures. Whether or not to include the quarter in which the attack took place makes essentially no difference for the results. $T1$ indicates that at least one inhabitant of the municipality was killed in the attack, $T2$ that at least one inhabitant of the municipality was present on Utøya, but no inhabitants were killed. Thus $T1$ and $T2$ are mutually exclusive.

All regressions will include municipality fixed effects δ_m and time (year x quarter) fixed effects γ_t, and cluster standard errors at municipality. This means that common shocks like seasonal variations will be differenced out, and that I do not estimate national-level effects, but rather the additional response in local communities. As robustness checks I include several time-varying municipality covariates, interactions between time trends and the 2010-levels of the municipality covariates, and municipality-specific linear time trends. Results from a log-linear specification are included in section A.4 in the online appendix .

I estimate the sum of positive and negative effects at the municipality-level, and always at the margin between a treated municipalities and the rest of the country, which of course also was affected to some degree. To the extent that the same forces operate on the whole population, the fact that I look only at changes relative to the rest of the country means that I can be seen as estimating a lower bound.

4 Results

4.1 Baseline results and robustness checks

Table (2) displays the main results. The first column shows the results from a specification with no controls other than time and municipality fixed effects. The sickness absence rate dropped by 0.27 percentage points after the attack in municipalities from which an inhabitant was killed, and 0.15 percentage points in the municipalities with participating, but surviving inhabitants.

Adding a list of municipality-level covariates relevant to the labor market in the second column does not have a large impact on the results, as these covariates do not vary very much and municipality fixed effects are already included. More stringent robustness tests are given by the third and fourth columns, which add municipality-specific linear time trends (3) or linear time trends interacted with the pre attack-levels of relevant covariates (4). In particular, specification (4) includes separate time trends based on (pre-attack) population and population density, as we saw from the descriptive statistics that the treatment and control groups differed substantially on these measures. The vote share of the Labor party in the 2009 election is also included in the same way to capture whether there were particular developments taking place in municipalities characterized by high (or low) support for the Labor party, which itself of course was hit severely by the attack. Specification (4) allows municipalities to follow different time trends depending on the levels of these characteristics before the attack.

It is reassuring that the results are robust across specifications. Nevertheless, in the rest of the article, specification (4), which flexibly allows for differential development in time between municipalities, is adopted as the preferred specification. Sickness absence here declined by 0.24 and 0.13 percentage points in the two treatment groups, or around 4% and 2% of the average of 6.10 %, respectively. To put these numbers into perspective, note that contracted hours in the T1 group before the attack amounted to 3.8 million work days per year, 0.24 percentage points of which is 9,120 work days. Thus, per year, the primary effect in average implies almost 10,000 fewer work days lost due to sickness absence. Since the effects on those closest to the victims are almost certainly absence-increasing and there are spillovers between municipalities because of personal relationships, the true absence-decreasing effects are likely even larger. The absolute size of the estimated coefficients are very similar when considering women and men separately, ref. Table A7 in the online appendix .

As a check on the assumption that physical closeness to victims is what constitutes the differential exposure to the attack, I define another margin of intensity of exposure: municipalities that share a physical border with a $T1$ municipality. Column (5) shows that the point estimate for this third exposure indicator as expected is comparable to, though a little smaller than, that of $T2$. The estimated effect of $T1$ is now larger, as the additional neighbor indicator picks up some of the municipalities from the control group that were also exposed to the attack. Section A.3 in the online appendix shows that the estimated effect is also larger in smaller municipalities.

The fact that sickness absence is directly related to being in the labor market makes it paramount to investigate whether a shift in labor force composition might explain the results. A first step in this analysis consists of estimating the effect on the share of employees who are absent. A drawback with the absence rate is that it does not differentiate the extensive and the intensive margins. In contrast, the share of employees who are absent provides a measure of the extensive margin. Small coefficients on the share of employees who are absent would indicate that the effect on the absence rate is caused by relatively few individuals with a large degree of absence either recovering or dropping out of the labor market, whereas coefficients closer to the ones estimated for the overall absence rate would indicate that the effect is more widespread. As can be seen in Column (6), the estimated coefficients are almost the same as in Column (4), showing that the effect on the absence rate is not driven by a small group of people.

However, of selective drop-out from the labor market is still a possibility – in particular, one potential mechanism is some people sorting into disability after the attack. Using quarterly data available from 2010 onwards, I do find a slight increase in the disability rate, see Column (7), however far from large enough to explain the drop in absentees. Another possibility is that high-absence individuals dropped out of the labor market, hence I investigate the employment rate of the working age population, see Column (8). These results are less conclusive, however, the point estimate on $T1$ goes in the opposite direction of what would be expected if an effect on the composition of workers was driving the results. A figure graphing the employment rate over time for the three groups is provided as Figure (A3) in the online appendix .

Table 2: Main results and robustness checks

	Sickness absence					Absentees	Disability	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T1	-0.266***	-0.266***	-0.216**	-0.236***	-0.275***	-0.196***	0.027	0.157
	(0.062)	(0.063)	(0.086)	(0.065)	(0.072)	(0.067)	(0.044)	(0.140)
T2	-0.151***	-0.145**	-0.101	-0.131**	-0.119**	-0.144**	0.008	-0.095
	(0.058)	(0.058)	(0.080)	(0.059)	(0.060)	(0.064)	(0.034)	(0.130)
T1 neighbor					-0.085			
					(0.063)			
socioeconomic controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
population*time trend	No	No	No	Yes	Yes	Yes	Yes	Yes
pop.density*time trend	No	No	No	Yes	Yes	Yes	Yes	Yes
Labor vote*time trend	No	No	No	Yes	Yes	Yes	Yes	Yes
municip. time trends	No	No	Yes	No	No	No	No	No
year*quarter f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
municip. f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-sqr	0.242	0.244	0.380	0.248	0.249	0.272	0.116	0.582
N	10152	10152	10152	10152	10152	10152	5499	2538
# municipalities	423	423	423	423	423	423	423	423
depvar mean	6.10	6.10	6.10	6.10	6.10	5.86	7.07	69.44

Note: Outcomes measured in percent. Point estimates are to be interpreted as effects in percentage points. Socioeconomic controls include population density, population, average years of schooling, share of workforce employed in health and social sector, and female share of workforce. In specification with covariate*time trend, population and population density are measured in 2010, Labor party vote in 2009 election. Time period 2008q1-2013q4. Standard errors clustered at municipality. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

The attack thus seems to have lead to a form of positive change, also known as post-traumatic growth. Such an interpretation has also received support from the woman who was the secretary general of the Labor Party's youth organization at the time of the attack and who was herself one of the survivors from Utøya. In a newspaper interview conducted in relation to an early presentation of the results in this article, she stated that the findings made sense to her, as she had found it very important to be at work after the attack and had noticed how people had wanted to contribute, both in the short and long term (Haug, 2014).

4.2 Dynamics

To investigate the dynamics of the effects and to challenge the assumption that the treatment and control groups followed similar time trends, I include a series of indicators for the timing of the attack. Time t is the quarter in which the attack took place, i.e. the third quarter of 2011, and serves as the baseline. Thus $T1_{t-1}$ denotes a variable that takes the value 1 in the quarter before for those municipalities that received exposure $T1$ and 0 at all other times all other municipalities. I include such indicators eight quarters before the true attack (t-1 to t-8) and for the nine remaining quarters in the data window after the attack (t+1 to $t+9$), the end of the data window. The logic is the same for $T2$. The results are reported in Table (A8) in the online appendix . The estimated coefficients from the baseline specification (4), containing municipality and time fixed effects, socioeconomic controls and pre-attack covariates interacted with linear time trends, are plotted graphically in Figures (2) and (3).

It is reassuring that the coefficients on the shocks before the true attack $T1$ are quite precisely centered on zero. The event study estimates of $T2$ are more ambiguous. For the year immediately preceding the attack, there is no particular pattern, whereas in the year before that, a declining trend might be occurring. This raises the concern that the municipalities without participants might not be a good comparison group for the municipalities with only surviving inhabitants. The choice of comparison group is further investigated in section A.2 in the online appendix . From $t+1$ onwards, the drop of about

0.2 percentage points for $T1$ sets in immediately. The estimated coefficients for $T2$ shows a different, gradually declining pattern.

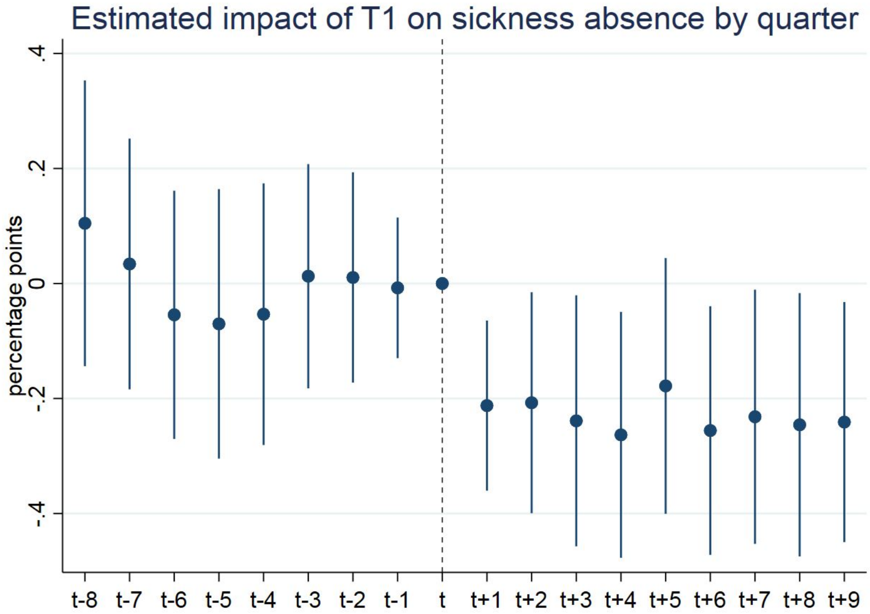


Figure 2: Impact of T1 by quarter. Estimated coefficients from specification (4) in Table (A8). Time t is the quarter in which the attack took place, i.e. the third quarter of 2011. Each bar displays the estimated effect of a one-period treatment taking place in the time period indicated on the horizontal axis. Vertical bars indicate 95 % confidence intervals.

4.3 Duration of absence

Given that much of the third quarter of 2011 took place after the attack, one may wonder why there seems to be no effect in that quarter. The main explanation is that long absence spells (more than one month) are behind 2/3 of the overall absence rate, thus any intervention not leading to early return from spells that have already started is bound to operate with a significant lag. In addition, it is possible that there were initial absence-increasing effects which counteracted the absence-decreasing effects. Both these explanations may be true, and although I cannot disentangle them with the present data, there are some observations that suggest the importance of the impact through long-term absenteeism: First is the continued, gradual decline, which would follow readily from a reduction in new long-term spells. Second, when analyzing data on the duration of completed spells, the share of short spells (4-16 or 8-16 days depending on employer)

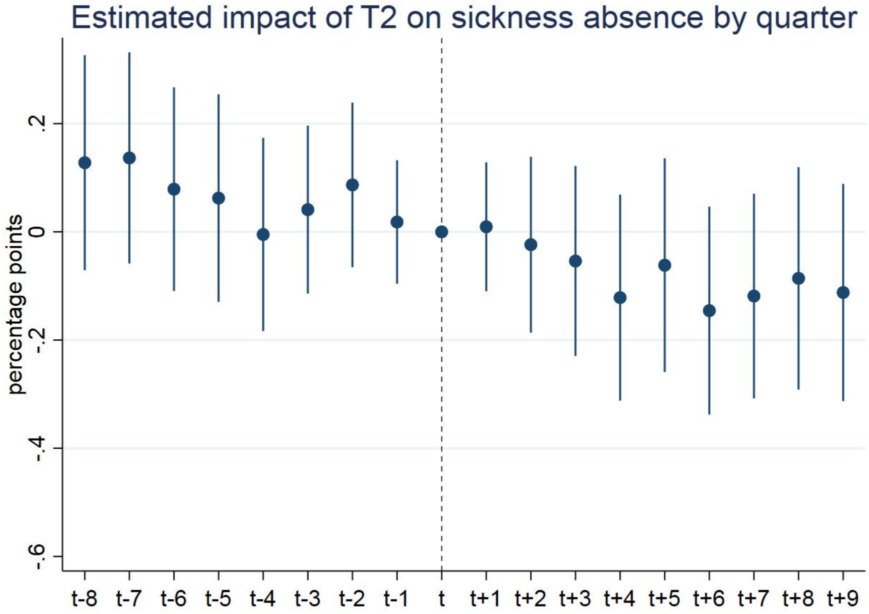


Figure 3: Impact of T2 by quarter. Estimated coefficients from specification (4) in Table (A8). Time t is the quarter in which the attack took place, i.e. the third quarter of 2011. Each bar displays the estimated effect of a one-period treatment taking place in the time period indicated on the horizontal axis. Vertical bars indicate 95 % confidence intervals.

increased, while the share of either medium (16 days-12 weeks) and long (>12 weeks) spells declined, ref. Table 3.

The results from the analysis of the dynamics of the effects and the duration of absence support the basic identification strategy for $T1$ municipalities and show that the effects are highly persistent. The persistence indicates that people do not increase their presence at work out of a demand for information or of curiosity, forces that would likely dissipate with time.

4.4 Placebo test from randomized treatments

As a final robustness test, I randomize treatment status (T1, T2, Control) across municipalities (keeping the timing constant) and estimate placebo effects with the baseline specification.[10] This procedure is repeated 1000 times. Figure (4) shows the distribution of the resulting estimates of T1 and T2. It is reassuring that the majority of the estimated placebo effects fall around 0 and that negative estimates of the same or larger magnitude

[10] I am grateful to an anonymous reviewer for suggesting this structure for the placebo test.

Table 3: Effect on the duration of absenteeism

	Share of spells			Cases, per 1000 inhabitants		
	Short	Medium	Long	Short	Medium	Long
T1	0.009**	-0.004	-0.005**	1.095	-0.514*	-0.686***
	(0.004)	(0.002)	(0.002)	(0.798)	(0.297)	(0.239)
T2	0.006*	-0.000	-0.006**	0.437	-0.189	-0.669***
	(0.003)	(0.002)	(0.002)	(0.607)	(0.255)	(0.241)
depvar mean	0.55	0.28	0.17	53.92	26.62	15.60
R-sqr	0.239	0.088	0.259	0.447	0.165	0.170
N	8460	8460	8460	8460	8460	8460

Note: Duration of completed spells. Short: 4-16 or 8-16 days depending on employer; Medium: 16 days-12 weeks; Long: >12 weeks. Empirical specification includes municipality fixed effects, year*quarter fixed effects, socioeconomic controls (population density, population, average years of schooling, share of workforce employed in health and social sector, female share of workforce) and linear time trends interacted with population and population density measured in 2010 and Labor party vote in 2009 election. Standard errors clustered on municipality. * p<0.10, ** p<0.05, *** p<0.01

as in the main analysis, -0.236 and -0.131 percentage points, are found in only 0.4 and 2.3 percent of cases, respectively.

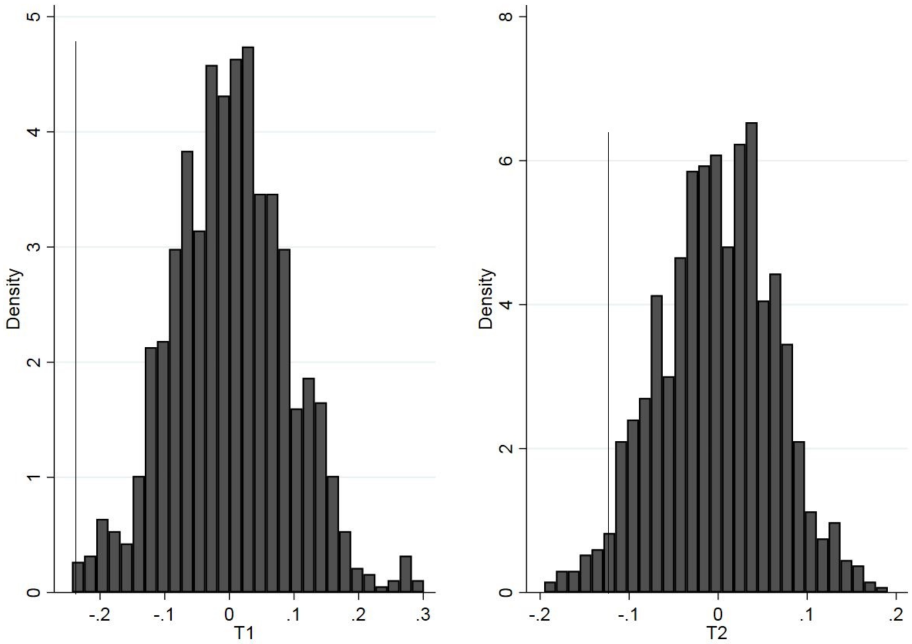
4.5 The impact on the young

A notorious feature of the attack was the young age of the victims, leading to a popular notion that the attack particularly impacted young people. The purported impact has often been taken to be one of growth, typically in terms of increased political awareness and societal engagement, and has been based on both quantitative research (Wollebæk et al., 2012; Bergh and Ødegård, 2013) and interviews with youth politicians (Mæland, 2016; Nordanger, 2017). Evidence from the Palestinian-Israeli conflict has also shown that relatively young people are particularly impressionable by terrorism (Jaeger et al., 2012). To examine the narrative of age-differentiated post-traumatic growth, I disaggregate the analysis by age. If the popular notion of positive change that differs by age is correct, we should expect the effects to be larger for the youngest workers.

Table (4) shows the results when the working population is divided into nine age groups.[11] The estimated effects are largest for people in their early 20's, whose absence

[11]The same age groups are compared over time, thus the exact individuals who are part of an age group will vary. Individuals in the lowest age bracket (20-24) make up around 10% of total employment,

Figure 4: Distribution of placebo estimates, randomized treatments



Note: Treatment status (T1, T2, Control) allocated randomly to municipalities 1000 times. Free-standing, vertical lines indicate estimates from the preferred specification, Column (4), Table 2. Empirical specification includes municipality fixed effects, year*quarter fixed effects, socioeconomic controls (population density, population, average years of schooling, share of workforce employed in health and social sector, female share of workforce) and linear time trends interacted with population and population density measured in 2010 and Labor party vote in 2009 election. Time period 2008q1-2013q4.

was reduced by almost 0.4 percentage points. For prime working age groups, the estimated reduction from *T1* was between 0.1 and 0.3 percentage points, while for the oldest workers, the estimate was also almost 0.4 percentage points. When taking into account the very different underlying absence levels of the age groups, the young stand out even more strikingly. The two bottom rows display the percent change for each of the groups. Among workers closest to the victims' age, i.e. those aged 20-24, absence rates dropped by 9 %, while the estimates for most other groups range between -1% and -5%. There is invariably more noise in these estimates, as each observation results from aggregating over a smaller number of individuals. Nevertheless, the results are substantial, consistent and quite precise.[12] Thus, the reduction in sickness absence was much larger among

those aged 25-54, considered to be the prime working age by OECD and other statistics agencies, make up around 65%, while people in their late 50s and 60s are around 20% of the total.

[12]For some age groups, some municipality-quarters are missing for certain small municipalities as numbers are not published if they are the result of too few individuals. In Table (4) these observations are treated as missing at random. The results are almost identical when all such municipalities are

young people than among older ones, supporting the popular notion of post-traumatic growth that differed by age.

Table 4: Effect on sickness absence – by age group.

	age20-24	age25-29	age30-34	age35-39	age40-44	age45-49	age50-54	age55-59	age60-66
T1	-0.387	-0.289	-0.121	-0.299	-0.152	-0.203	-0.131	-0.383	-0.362
	(0.138)	(0.147)	(0.162)	(0.135)	(0.119)	(0.136)	(0.133)	(0.160)	(0.192)
	***	*		**				**	*
T2	-0.371	-0.372	0.102	-0.063	-0.074	-0.283	-0.116	-0.062	-0.119
	(0.126)	(0.133)	(0.128)	(0.134)	(0.122)	(0.121)	(0.129)	(0.158)	(0.195)
	***	***				**			
R-sqr	0.078	0.066	0.059	0.044	0.051	0.040	0.048	0.047	0.068
N	10085	10093	10104	10133	10138	10143	10146	10140	10142
# municip.	423	423	423	423	423	423	423	423	423
depvar mean	4.28	5.65	6.13	6.01	5.94	6.01	6.42	7.01	8.37
depvar sd	2.01	2.33	2.34	2.16	1.97	2.02	2.21	2.40	3.02
% change									
T1	-9,0	-5,1	-2,0	-5,0	-2,6	-3,4	-2,0	-5,5	-4,3
T2	-8,7	-6,6	1,7	-1,0	-1,2	-4,7	-1,8	-0,9	-1,4

Note: Sickness absence is measured in percent. Estimation on subsamples of age groups. Empirical specification includes municipality fixed effects, year*quarter fixed effects, socioeconomic controls (population density, population, average years of schooling, share of workforce employed in health and social sector, female share of workforce) and linear time trends interacted with population and population density measured in 2010 and Labor party vote in 2009 election. Time period for sickness absence 2008q1-2013q4. Standard errors clustered on municipality. * p<0.10, ** p<0.05, *** p<0.01

Despite not finding evidence of a mechanism in terms of labor market participation for the general population above, it is important to investigate these channels also for the young. This is done in Table (5). Column (1) shows the baseline estimate for the age group 20-24 from above. Column (2) replicates the finding from Table (2) of an increased effect when taking into account whether a municipality was a *T1* neighbor. This is as expected, as the additional neighbor indicator picks up some of the municipalities from the control group that were also exposed to the attack. Since trends in educational attainment might matter especially for the younger group, Column (3) adds an interaction between pre-attack education level and a time trend. It is reassuring that the results are robust to allowing for differential trends in educational attainment in this way. Unfortunately, the measure of disability employed for the general population in Table (2) is not available by age. Instead, I include a measure of enrollment in education, which in any case may be a more relevant outcome for young people. Thus, increased educational enrollment

excluded entirely.

does not seem to explain the results. Finally, Column (5) investigate employment. The point estimate on $T1$ is here marginally significant, however, it again goes in the opposite direction of what would be expected if an effect on the composition of workers was driving the results. Figure (A4) in the online appendix shows employment rates by exposure group graphically.

Table 5: Estimated effects for 20-24 year-olds

	Sickness absence			Enrollment	Employment
	(1)	(2)	(3)	(4)	(5)
T1	-0.387***	-0.451***	-0.306**	-1.595	0.761*
	(0.138)	(0.149)	(0.138)	(0.022)	(2.243)
T2	-0.371***	-0.352***	-0.322***	-0.677	0.217
	(0.126)	(0.128)	(0.122)	(0.019)	(1.861)
T1 neighbor		-0.137			
		(0.122)			
education levels*time trend			Yes		
R-sqr	0.078	0.078	0.081	0.009	0.344
N	10085	10085	10085	2520	2538
# municipalities	423	423	423	420	423
depvar mean	4.28	4.28	4.28	18.94	71.89

Note: Outcomes measured in percent. Empirical specification includes municipality fixed effects, year*quarter fixed effects, socioeconomic controls (population density, population, average years of schooling, share of workforce employed in health and social sector, female share of workforce) and linear time trends interacted with population and population density measured in 2010 and Labor party vote in 2009 election. Time period for sickness absence is 2008q1-2013q4, for other outcomes 2008-2013. Standard errors clustered at municipality. * p<0.10, ** p<0.05, *** p<0.01

Some studies from the fields of political science and psychology have found associations between traumatic shocks and subsequent increases in various measures of social capital. To investigate whether this might be a relevant mechanism for the results on work effort, I analyze the effect on turnout in elections, membership in organizations, and beliefs of whether people care about others or can be trusted, which have been used as measures of social capital (Knack and Keefer, 1997; Guiso et al., 2004). Data on these variables come from the election surveys undertaken after the elections in 2003, 2007, and 2011, which permit analyses at the individual level. Since the largest effect in the labor market analysis was found for young people, the analysis is focused on respondents below 30. The results are given in Table A9 in the online appendix . Most of the estimates are negative, which is opposite of what one would expect if increased social capital was driving the results.

5 Conclusion

I analyze workers' response to the July 22, 2011, terrorist attack in Norway. I find that the sickness absence rate in municipalities from which an inhabitant was killed in the attack declined substantially after the attack took place. The decline was nearly one quarter of a percentage point and constituted a percent decline of 4%. In municipalities from which no one was killed in the attack, but that had someone who or experienced it first-hand, there was also a substantial decline, of around 2%. As a decline in absenteeism is typically a beneficial development, the results speak to the literature on post-traumatic growth, which is "the experience of positive change that occurs as a result of the struggle with highly challenging life crises (Tedeschi and Calhoun, 2004, p.1)."

I do not find evidence of an explanation in terms of labor market composition, which suggests that the estimated growth effects are real. A decline also in neighboring municipalities, a larger effect in smaller municipalities and a differential response between age groups stand out, suggesting knowledge of and identification with victims as important factors. In particular, the decline in the absence rate for workers in their early 20's is twice that for workers in other age groups. This supports the popular notion that young people were particularly impacted by the attack because of belonging to the same generation as the victims.

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