Working Paper in Economics No. 749

Scaring or scarring? Labour market effects of criminal victimisation

Anna Bindler, Nadine Ketel

Department of Economics, January 2019

ISSN 1403-2473 (Print)
ISSN 1403-2465 (Online)

UNIVERSITY OF GOTHENBURG
SCHOOL OF BUSINESS, ECONOMICS AND LAW

Scaring or scarring?
Labour market effects of criminal victimisation*

Anna Bindler   Nadine Ketel

This version: 3rd January 2019

Abstract

Little is known about the costs of crime to victims and their families. In this paper, we use unique and detailed register data on victimisations and labour market outcomes from the Netherlands to overcome data restrictions previously met in the literature and estimate event-study designs to assess the short- and long-term effects of criminal victimisation. Our results show significant decreases in earnings (6.6-9.3%) and increases in the days of benefit receipt (10.4-14.7%) which are lasting up to eight years after victimisation. We find shorter-lived responses in health expenditure. Additional analyses suggest that the victimisation can be interpreted as an escalation point, potentially triggering subsequent adverse life-events which contribute to its persistent impact. Heterogeneity analyses show that the effects are slightly larger for males regarding earnings and significantly larger for females regarding benefits. These differences appear to be largely (but not completely) driven by different offence characteristics. Lastly, we investigate spill-over effects on non-victimised partners and find evidence for a spill-over effect of violent threat on the partner's earnings.

JEL-codes: K14; J01; J12; I1
Keywords: Crime; victimisation; labour market outcomes; event-study design

*Acknowledgements: We are thankful for funding of this research by Vetenskapsrådet (VR) and to Statistics Netherlands for support regarding the data. We would like to thank Randi Hjalmarsson, Andreea Mitrut, Paul Muller, Mikael Lindahl, Margherita Fort, Peter Fredriksson and Magne Mogstad as well as seminar/conference participants at the University of Gothenburg, Tinbergen Institute Amsterdam, University of Bologna, Goethe-University Frankfurt, ESPE (Antwerpen 2018), University of Potsdam, the NBER Summer Institute (Crime 2018), EEA (Cologne 2018), EALE (Lyon 2018), NHH/FAIR Bergen, the CEP/LSE workshop on the Economics of Crime and Policing and the University of Cologne for helpful comments and discussions. All remaining errors are our own. Authors: Anna Bindler, Department of Economics, University of Gothenburg, P.O. Box 640, 40530 Gothenburg - Sweden; email: anna.bindler@economics.gu.se; Nadine Ketel, Department of Economics, University of Gothenburg, P.O. Box 640, 40530 Gothenburg - Sweden; email: nadine.ketel@economics.gu.se.

1 Introduction

Crime imposes many direct and indirect costs on a society. Direct costs include administrative costs for policing, courts and sanctions, and are relatively easy to measure. Indirect costs - which can occur both through the offender and the victim - are on the other hand notoriously difficult to measure. While there is a growing economics of crime literature dedicated to studying the potential costs and consequences of criminals interacting with the justice system – ranging from unemployment, earnings and recidivism to spill-over effects on their families – the same cannot be said for victim-related costs.[1] Yet, this knowledge gap, and the resulting underestimate of the social costs of crime, is potentially large: Sizeable population shares around the world are exposed to crime directly as victims, and many more indirectly through their family and neighbourhood relations to victims.

Why do we know so little about the causal effects of victimisation? The reasons are two-fold. First, there is a lack of high-quality micro-level victimisation data, with the existing literature relying on either small scale survey data, aggregate crime data or (more selective) hospitalisation data to measure or proxy criminal victimisation. Second, it is not trivial to disentangle correlation from causation, especially given the limited nature of the available data.

This paper begins to fill this large knowledge gap by studying four fundamental questions. First, what are the effects of criminal victimisation on individuals' labour market outcomes, including earnings (labour income) and benefit dependency? Second, are these effects temporary or do they persist over time? Third, why do these effects exist? We shed light on potential mechanisms by considering additional health related outcomes as well as heterogeneity by individual, household and offence characteristics. Finally, are there spill-over effects on non-victimised household members? We overcome the data limitations previously met in the literature by exploiting unique administrative data on victimisation from Dutch police records that can be linked to an 18-years long panel of labour market outcomes of Dutch register data.

[1]This is consistent with Becker's (1968) seminal model of crime emphasis on the determinants of criminal behaviour. For recent reviews of the empirical literature, see e.g. Chalfin and McCrary (2017), Draca and Machin (2015) or Nagin (2013). For reviews of the estimates of the cost of crime, see for example Chalfin (2015), Heaton (2010) or Soares (2015).

Further, we are able to study spill-overs by linking individuals in our data to their respective household members. This is, to our knowledge, the first study that uses victimisation register data to study these questions. Moreover, the panel nature of the data allows for a credible identification strategy: an event-study design with individual fixed effects.

Why would criminal victimisation affect labour market outcomes? Over and above direct effects, such as a deterioration in physical health (e.g. due to injuries), the literature discusses three main channels of changes in daily routines, behaviour and mental health outcomes: (i) increased levels of fear and anxiety, (ii) a reduced sense of freedom and changes in behaviour and (iii) the need for pre-emptive and deterrent strategies. These may affect an individual's choices regarding the type, time and location of work and hence impact observed labour market outcomes. In this paper, we estimate the net effect of victimisation on labour market outcomes.

To date, there is scarce empirical evidence identifying the causal impacts of victimisation. The small existing literature focuses on the behavioural responses to criminal victimisation, risk perceptions and changes in mental health and subjective well-being. Two recent studies estimate the effect of crime on mental health. Using four waves of Australian survey data and an individual fixed effects design, Cornaglia *et al.* (2014) find that violent (but not property) crime has a negative impact on mental health both for the victim of the crime as well as for non-victims (through exposure to crime). In contrast, Dustmann and Fasani (2016) find for the UK that local area property crimes cause mental distress, with effects being stronger for females. The literature also suggests negative effects of victimisation on subjective well-being and life satisfaction (e.g. Johnston *et al.* (2018) and Cohen (2008)). Currie *et al.* (2018) use data on crime incidents (assaults), geocoded at the building level, to proxy for violent assaults during pregnancy of mothers living in that building. Linking that crime data to birth records by maternal residential addresses, they report evidence of lower birth weights and a higher likelihood of pre-term births for children in utero (in the 3rd trimester) while the assault occurred.[2]

[2]Janke *et al.* (2016) find decreases in physical activity (walking) as a consequence of local area violent crime in England between 2005 and 2011. Hamermesh (1999) tests whether exposure to higher local area crime rates in the US induces a change in working times (night shifts), and finds such an effect for the homicide but not the overall crime rate. Braakmann (2012) studies the impact of victimisation and victimisation risk on changes in avoidance behaviour and time-allocation in Mexico. Dugan (1999) uses three years of data from the U.S. National Crime Survey to estimate the effect of criminal victimisation on households' decisions to move, and finds evidence for a

We are only aware of two studies that estimate the effect of victimisation on labour market outcomes. Ornstein (2017) uses a matching estimator and Swedish register data to study the effect of becoming an assault victim on mortality, health and labour market outcomes. However, victimisation is measured using hospital records, which is selective on both offence type and severity (i.e. assaults severe enough to result in a hospital visit). Velamuri and Stillman (2008) follow a similar approach as our paper and estimate the effect of criminal victimisation on labour market outcomes (and well-being) in an individual fixed effects model, but with only four waves of Australian survey data; of the resulting analysis sample (42,945 observations from 2002 to 2005) just 725 and 2,490 observations are for victims of violent and property crime, respectively.

Thus, our main contribution is to credibly assess the impact of criminal victimisation on a set of labour market and health outcomes using large panel data that allows us to differentiate between short- and long-term effects. We can study heterogeneity with respect to individual, household and offence characteristics and our data structure allows us to study intra-household spill-overs of criminal victimisation on non-victimised household members. We address our main empirical challenges - selection, omitted variables and simultaneity - in the following ways: First, the population of victims of crime may differ from the population of non-victims.[3] To avoid resulting selection biases, we restrict our sample to individuals who have been victimised at least once during our sample period (2005-2016) and conduct all analyses separately by type of offence. One key advantage of our data is that the sample is large enough to allow for such a restriction without compromising statistical power (with the exception of robbery and sex offences). Second, unobservable characteristics may correlate both with the outcome and the probability of becoming a crime victim. To avoid resulting omitted variable bias, we exploit the long panel of labour market outcomes and estimate an event-study design with individual fixed effects controlling for any time-invariant individual traits. This approach is similar

strong link between the two events for both violent and property offences. In the Dutch context, Salm and Vollaard (2016) document that risk perceptions with respect to neighbourhood crime are adjusted upwards with time of exposure, using longitudinal data of movers matched to data from the Dutch victimisation survey.

[3]See Hindelang *et al.* (1978), Cohen and Felson (1979), Miethe *et al.* (1987) and Miethe and Meier (1990) for discussions of theories of victimisation, including the lifestyle-exposure and routine activity hypotheses. For a study from the economics literature, see for example Levitt (1999).

to Grogger (1995) who studies labour market effects of being arrested using a distributed leads and lags model, or more recently Dobkin *et al.* (2018) who study the economic consequences of hospital admissions in an event-study design.[4] Third, labour market outcomes may impact the chance of victimisation at the same time that victimisation affects labour market outcomes. We address these concerns by explicitly studying the timing of potential effects to see whether there are sharp changes in labour market trajectories at the time of victimisation. In addition, we offer results from alternative estimation strategies (controlling for individual characteristics and lagged outcomes instead of individual fixed effects). Moreover, when we study household spill-overs, simultaneity is less of a concern assuming that the family member's behaviour does not correlate with the timing of victimisation.

Our main event-study results suggest that a criminal victimisation is linked to statistically and economically significant decreases in earnings and increases in benefit dependency: For assault, we find decreases in annual earnings of 9.3% and increases in annual days of benefit receipt of 11.4 days (14.7% at the mean) in the first calendar year after victimisation. For threat of violence, we find corresponding decreases in earnings of 6.6% and increases in benefit receipt of 10.4% at the mean. For these two offences, we see a sharp change at the time of victimisation. For robbery and sex offences (by far the two smallest subsamples), we cannot draw firm conclusions as the results are imprecisely estimated. For burglary and pickpocketing, we see decreases in earnings and increases in benefits, but there is a less clear escalation point. Moreover, the results for pickpocketing (in contrast to the other offences) disappear once we take later victimisations into account. For large parts of the paper, we will thus focus on assault and threat, but report all results in the (online) appendix which we will refer to when appropriate.

The reported effects of victimisation both on earnings and benefit receipt are persistent (up

[4] The more recent literature estimating the effect of arrests and incarceration on labour market outcomes takes advantage of random assignment to judges in order to overcome the endogeneity problem (see for example Aizer and Doyle, 2015; Kling, 2006; or Mueller-Smith, 2016). This is not possible in the case of victimisation. To introduce quasi-random variation, one needs to find a source of exogenous variation that alters the risk of victimisation. Braakmann (2012) instruments the individual victimisation by the share of individuals in the neighbourhood who have been victimised or consider victimisation as likely, thereby excluding the respective individual. To be a valid instrument, one must assume that the resulting measure of neighbourhood victimisation risk has no direct impact on the outcome variable. We do not believe this to be a valid assumption in our case, as local labour markets may be influenced by local crime rates and hence labour market outcomes would not be exogenous to the instrument.

to eight years) which might partly be driven by an extensive margin response (i.e. individuals leaving employment and/or entering benefits). We find some short-term increases in total health expenditure (up to 17.8% at the mean) and longer-lasting effects with respect to mental health expenditure (up to 43.1% at the mean and persistent over five years). Falsification tests, which randomly allocate victimisation years, suggest that our baseline findings are not driven by a spurious relationship between the year of victimisation and the outcome. Our results are robust to a number of specification and robustness tests, including different functional forms and/or sets of control variables and alternative sample restrictions (with respect to ages, multiple offences as well as victimisation years). For outcomes that we can observe at the monthly level, we show that the results follow the same pattern when we estimate event-study designs at the monthly instead of the yearly level (as at the baseline). Moreover, these results again highlight the sharp change in labour market outcomes at the time (month) of victimisation.

Are the results driven by other changes preceding and/or following the victimisation? That is, while our identification strategy controls for time-invariant individual characteristics, it does not rule out biases due to omitted variables that change over time. We first consider divorce and moving as two other life-events (i.e. correlated shocks) that may affect labour market outcomes. The share of individuals divorcing and/or moving increases in the year of and the year after victimisation, but not before. Second, we study multiple (earlier and later) victimisations by (i) restricting our sample to individuals with no reported victimisation in at least the previous four years (instead of two at the baseline), (ii) restricting the sample to individuals with only one observed victimisation and (iii) flexibly controlling for later victimisations. We find that multiple victimisations contribute to the long-term effects and argue that earlier (unobserved) victimisations may explain pre-trends that are in some cases seen at the baseline for the benefit outcome. Third, we address the possibility of a victim-offender overlap and restrict the sample to individuals without a criminal record in the years after the victimisation. The point estimates for earnings are attenuated suggesting that later criminal involvement contributes to the persistent effects seen for that outcome. Based on these results, we argue that the reported victimisation incident may be seen as a sharp escalation point, triggering other life events as well as a trajectory of victimisations and criminal involvement for those individuals at the margin.

Heterogeneity analyses suggest that men and women respond differently to victimisation: While we see slightly larger point estimates for men with respect to earnings, the differences are not or only marginally significant. In contrast, there are striking and significant differences for benefits. Benefit receipt increases by 14-20% relative to the mean for women, but by only 4% for men. For this outcome, we also find particularly strong effects for individuals cohabiting with a partner who is registered as a suspect of a crime following the victimisation: 22-31% relative to the mean compared to 1-7% for those cohabiting with a 'non-criminal' partner. This suggests that domestic violence plays an important role for female victims (in particular for assault and violent threat). We find support for this hypothesis using complementary information from the Dutch victimisation survey. For these offences, females are more likely than males to know the offender and the offence is more likely to take place at a familiar location.

Lastly, we study household spill-overs. Specifically, we estimate the impact of being the (cohabiting, non-victimised) partner of a crime victim. Such spill-overs are interesting for two reasons. First, if they exist, they are an important (and to date unmeasured) component of the social cost of criminal victimisation. Second, to the extent that the victimisation of a partner is an exogenous event, this abstracts from simultaneity concerns. We find empirical evidence for spill-over effects on earnings (but not benefits) for violent threat (7.3%) but not for assault.

The remainder of the paper proceeds as follows. Section 2 discusses the data sources and provides summary statistics. In Section 3, we explain our empirical strategy, present the baseline findings as well as results from falsification, specification and robustness tests, and then discuss correlated shocks and possible determinants of long-term effects. In Section 4, we present our heterogeneity analyses by individual, household and offence characteristics. In Section 5, we take the analysis one step further to study potential household spill-overs. Finally, Section 6 concludes by discussing our results and comparing them to the literature on earnings losses following adverse life events as well as existing cost of crime estimates.

2 Data

The Netherlands, as other countries, has seen decreasing trends in crime over recent years. Compared to 2015, the number of registered criminal offences decreased by 5.1% in 2016.[5] In 2016, there were 930,000 total registered criminal offences, i.e. a total crime rate of about 5,470.6 per 100,000 inhabitants. Compared to U.S. offence rates (keeping in mind caveats of cross-country crime comparisons), crime rates are not low in the Netherlands: In 2016, there were 2,450.7 property and 386.3 violent offences per 100,000 inhabitants in the U.S., compared to 3,391 and 529.4 in the Netherlands, respectively.[6] Both the number of registered crimes per 100,000 inhabitants and the development of crime over time are closely related to a weighted average of other countries in North/West Europe (Statistics Netherlands *et al.*, 2013). Given the comparability to other Western countries combined with the availability of high-quality register data, we believe that the Netherlands provides an ideal setting to study our questions.

For our analysis, we use detailed data on victimisation from Dutch register data. The key advantage of that data is that it can be linked to (other) Dutch register data, allowing us to construct a long panel of victimisation and labour market outcomes at the individual level. In the following, we describe the most important features of the different data sources.

2.1 Register data on victimisation

The victimisation register data consists of yearly files of all police registered victims of crime in the Netherlands, i.e. all victims of an offence reported to the police. These files are available from 2005 to 2016 and contain individuals' social security numbers allowing us to link them to labour market data.[7] A valid (Dutch) social security number is recorded for about 84 percent of the individuals in the sample.[8] The data report the date (as registered by the police) and the of-

[5]See Statistics Netherlands (last accessed on October 27, 2017): https://www.cbs.nl/en-gb/news/2017/41/further-drop-in-registered-crime-and-suspect-rates. A similar decrease is seen for the number of experienced crimes as reported by respondents in victimisation surveys (Statistics Netherlands *et al.*, 2013).

[6]See Crime in the U.S. 2016, Offences known to law enforcement (last accesses on October 27, 2017): https://ucr.fbi.gov/crime-in-the-u.s/2016/crime-in-the-u.s.-2016/topic-pages/offenses-known-to-law-enforcement.

[7]Since 2005 this information has been gathered centrally in a national database, for which the data is provided by the 25 local police forces.

[8]In the victimisation files, 15.8 percent of the individuals do not have a valid social security number and can therefore not be matched to any other data files. This may be either due to the victim not having a Dutch social

fence, but not the location of the crime.[9] For our analysis, we focus on the year of victimisation, as not all outcome variables are available at a lower than annual level. In a robustness test using the month of victimisation instead, we demonstrate that our results are robust to this choice for the main labour market outcomes which can be observed at the monthly level. We focus on the most common violent and property crimes that are not victimless (and match categories used in the economics of crime literature): assault, threat of violence (including stalking), robbery and sex offences as well as burglary and pickpocketing.[10]

We condition our analysis on the sample of crime victims to avoid both (i) the issue of measurement error that arises from selective reporting of victimisation and (ii) more general identification problems concerning selection into victimisation. Specifically, victimisation is hardly a random event. Instead of assuming that it is, we condition the sample on victimised individuals and rather assume that the *timing* of victimisation is (conditionally) random. Hence, we change the counterfactual from not being victimised to being victimised at a later point in time. We will discuss that assumption and the empirical setting in more detail in Section 3.1.

Importantly, we create subsamples for the above six offences and conduct our analysis separately for each. Splitting the sample has the advantage that it results in more homogenous samples as we compare victims of the same offences. For instance, we avoid pooling victims of pickpocketing with victims of sexual crime. To further assure that our offence subsamples are as homogenous as possible, we implement the following sample restrictions: First, we drop individuals who, within the same year, are registered to be a victim of multiple offences (within

security number (e.g. tourists) or the police/victim not registering the social security number. The distribution of registered victims with and without a social security number is quite similar across our offence categories.

[9]The current data does not contain information regarding the offender. However, we have information on whether the individual him- or herself as well as household members have been a suspect of crime. We use that information to proxy for domestic violence, see Section 4 for further details. If we get access to more detailed information about the offender in the future, we will test the robustness of our results to the use of this proxy.

[10]*Assault* is the deliberate infliction of pain or physical injury. *Violent threat* includes both threat and stalking, where threat is the systematic and/or deliberate violation of another person's privacy with the intent to create fear and/or enforce an action/toleration of an action; stalking is systematic and/or deliberate harassment affecting another person in their freedom and security. *Robbery* is the use of threat/violence to take and/or extort a good from another person. *Sex offences* include rape, sexual assault, blatant offences to modesty, acts of sexual nature violating socio-ethical standards (i.e. with minors and/or abuse of authority) and other remaining sexual offences. *Burglary* includes theft from a dwelling both *with and without* the use of violence (in contrast to definitions in other countries). Excluded crimes include those with no clearly identifiable victim (e.g. offences against the public order) and those with reporting concerns (e.g. bike theft).

our offence categories). This holds for 4.7 percent of the sample.[11] For outcomes observed at the annual level, we cannot empirically distinguish the effects of multiple victimisations within one year. Moreover, the effects of a sequence of two types of victimisation (within one year) is likely different from that of a single victimisation. Next, we look at the effect of the first observed victimisation for each individual. To address the possibility of a previous victimisation, we restrict the sample to individuals who have not been a registered victim of crime for *at least* the two previous years and, therefore, drop those victimised in 2005 and 2006 (no long enough pre-period available). We allow subsequent victimisations to contribute to the estimated effects, i.e. we estimate the combined effect of the first and any future victimisations. Third, as we study labour market outcomes, we restrict the sample to individuals aged 18-55 at the time of observing the outcome.[12] Fourth, we exclude individuals who are a registered criminal suspect in the years of or before the victimisation, to not confound the labour market effects of victimisation with the labour market effects of offending that are documented in the literature. Lastly, we exclude individuals who are not registered at a valid address in the Netherlands. Panel A of Table 1 reports the resulting number of individuals in the sample for each offence category; the total sample includes 1,007,519 individuals. Of these, about 22% are victims of assault, 13% of threat, 3% of robbery, 3.5% of sex offences, 42% of burglary and 17% of pickpocketing.

Section 3.4 investigates these restrictions in several robustness checks. First, we include victims of multiple offences within one year and assign them to each subsample corresponding to the respective offence. Second, we further exclude 2007 and 2008 victimisations to focus on individuals who have not been victimised in the *four* (instead of *two*) previous years. To deal with later victimisations, we (i) look at single victimisations only and (ii) explicitly control for contemporaneous victimisations. Third, we discuss the results for ages 26-55 which addresses the concern that youth below the age of 26 may not have fully entered the labour market yet. Fourth, we restrict the sample further to individuals who have not been a registered criminal

[11]If an individual is a victim of multiple offences during the same incident (e.g. robbery and assault), the police will only register the most severe offence. Therefore, if an individual is registered to be a victim of multiple offence categories in one year, these victimisations were separate events.

[12]This results in an unbalanced sample. As Borusyak and Jaravel (2017) discuss in more detail, the unbalancedness of the panel is not a problem for our individual fixed effects setting.

suspect in the years after the victimisation.

2.2 Register data on outcomes

The victimisation register can be linked to a number of Dutch administrative records that are available from 1999 to 2016. We extract data on labour market outcomes from registers that contain information about individuals' income including wages and earnings from self-employment, unemployment benefits, disability and sickness benefits as well as welfare benefits.[13] We use that information to construct our primary labour market outcomes: (i) earnings and (ii) days on (any type of) social benefits in a given year. Earnings include both wage earnings and income from self-employment (i.e. labour income).[14] We measure benefit dependency as the number of days during which an individual receives any social benefits, but ignore the actual amount as it may be a function of previous income. Further, we generate dummy variables for an extensive margin analysis: For earnings, this is an indicator equal to one for earnings above the 5th percentile (about 1660€ per year); for benefits, this is an indicator equal to one for any positive benefit income in a given year.

In addition to these primary labour market outcomes, we use further registers to create secondary outcomes (expenditure on physical and mental health, as reported in annual health insurance data and available from 2009) as well as measures of other life-events (moving, divorce, offending). We also extract demographic information for control variables as well as heterogeneity analyses (gender, year of birth, marital status, household composition, offending partner and municipality/neighbourhood codes).[15]

[13]For unemployment (UI) benefits, both eligibility period and level depend on individuals' labour market histories. Until 2016, an individual could receive UI benefits for a maximum of three years. The level and duration of disability (DI) benefits depend on the degree of the disability and again on the labour market history. Welfare benefits are provided to households with no or no sufficient means of living. They are means-tested (on both income and wealth); the level of benefits depends on the composition of the household. There is no upper limit to the individual eligibility period for welfare benefits.

[14]For our analysis, we use log earnings, replacing zero earnings with a small number. Further, to account for inflation, we correct wages by the annual CPI provided by CBS Statline, using 2015 as the base year.

[15]Our baseline specification includes municipality fixed effects. In 2016, there were 390 municipalities. In one of our robustness checks, we instead control for neighbourhood fixed effects (almost 3000 neighbourhoods). Offending behaviour is coded from annual individual-level data on suspects of crime (by offence). On average, 90 percent of registered suspects are convicted (Statistics Netherlands *et al.*, 2013). In the remainder of the paper, we will refer to the registered suspects using the terms criminal record and criminal suspects interchangeably.

Table 1: **Summary Statistics**

	Violent offences				Property offences		Non-victims
Sample:	Assault	Threat	Robbery	Sex	Burglary	Pickpocketing	Random
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Background characteristics (measured in the year of victimisation)							
Female	0.48	0.51	0.50	0.89	0.45	0.72	0.52
Age	34.4	38.5	32.6	31.7	42.1	36.9	41.0
Immigrant	0.20	0.18	0.27	0.11	0.15	0.18	0.10
Partner (0/1)	0.46	0.56	0.33	0.41	0.64	0.52	0.70
Children (0/1)	0.40	0.50	0.23	0.38	0.49	0.40	0.51
Observations	220,917	126,982	33,554	35,533	424,024	166,509	1,520,147
Panel B. Yearly labour market and other outcomes							
Earnings (>p5)	0.78	0.80	0.79	0.74	0.86	0.82	0.83
Earnings (in 2015 €)	19,360	24,545	17,951	14,847	34,748	22,041	27,914
Benefits (>0)	0.28	0.27	0.23	0.29	0.17	0.18	0.15
Days benefits	77.4	74.9	63.7	85.1	45.5	47.3	41.4
Total health costs (in €)	2,056	2,096	1,781	2,804	1,602	1,673	1,457
Mental health costs (in €)	601	487	538	1112	282	337	214
Observations (NxT)	2,610,249	1,737,264	348,011	353,755	5,932,520	1,986,934	19,060,884

NOTE- The table shows the sample means of the indicated variables for each of the six offence subsamples as indicated at the top of each column as well as for a 15% random sample of the population. Panel A reports the (cross-sectional) background characteristics in the year of victimisation for all individuals in the respective sample; Panel B reports the (longitudinal) yearly labour market and additional outcomes. SOURCE- Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

2.3 Descriptives

Table 1 presents summary statistics for each offence subsample (columns (1) to (6)). Panel A provides background characteristics, measured in the year of victimisation. There are notable differences in the demographic composition across offences: While the majority of assault and burglary victims are male (52% and 55%, respectively), females are overrepresented among the victims of pickpocketing (72%) and sex offences (89%). Victims of violent crimes tend to be younger (31.7-38.5 versus 36.9-42.1 years for property crimes), and are less likely to have a partner or children. These rather large compositional differences in terms of observable characteristics support our strategy to conduct our analysis separately by offence.

Panel B of Table 1 reports the average yearly labour market outcomes across offence subsamples. Note that these sample means are based on both pre- and post-victimisation years. Earnings are higher in the property than in the violent crime samples (22,041€-34,748€ compared to 14,847€-24,545€). For benefit dependency (days of benefit receipt) the opposite is true (41.4-47.3 versus 63.7-85.1 days). Total and mental health expenditure tend to be higher in the violent than in the property crime samples; the largest health expenditures are associated with sex offence victims (2,804€ for total and 1,112€ for mental health).[16]

As highlighted before, our analysis is conditioned on individuals who reported a criminal victimisation for the respective offence. How selected is that sample? For comparison, column (7) of Table 1 shows corresponding descriptives for a random sample of the population. We draw a 15% random sample from the population not registered as a victim and apply the same restrictions as to our analysis sample.[17] Clearly, there are differences between the victimised and non-victimised sample: While the gender and age composition is comparable, there are differences in terms of household composition and labour market outcomes as well as health expenditures. This is in particular the case when we compare the random sample to victims of violent crimes. As our individual fixed effects approach does not allow for (time-invariant) controls accounting for such compositional differences, this again highlights the importance of restricting our sample to victims of crime only.

Finally, Figure 1 shows victimisation-age profiles for each offence subsample (before restricting on ages). The two vertical lines mark ages 18 and 55, the youngest and oldest individuals in our sample. Again, there are clear differences across offences: First, as also seen in Table 1, the number of victimisations differs substantially across offences. Second, while for assault, robbery and pickpocketing the peak of the victimisation-age profile is reached in the late teens, it is earlier (mid teens) for sex offences and much later (mid forties) for burglary.[18] For violent threat, the victimisation-age profile appears quite flat. These compositional differences once again speak in favour of conducting our analysis separately by offence.

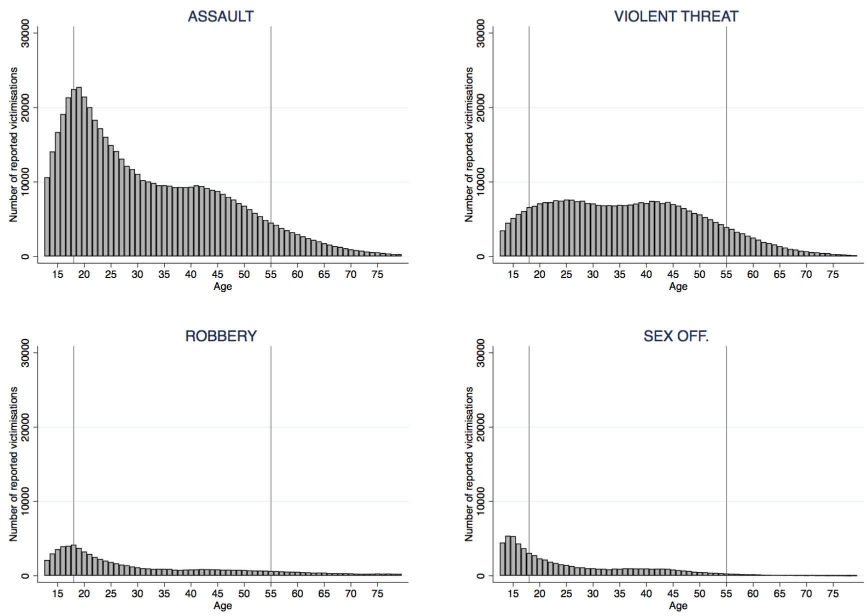
[16]Appendix Table A1 reports sample averages of the two main labour market outcomes (earnings and days of benefits) for the different subgroups considered in our heterogeneity analysis (see discussion in Section 4).

[17]Specifically, we restrict that sample to ages 18-55 at the time of observing the outcome and exclude individuals registered as a suspect of crime or without a valid address in the Netherlands.

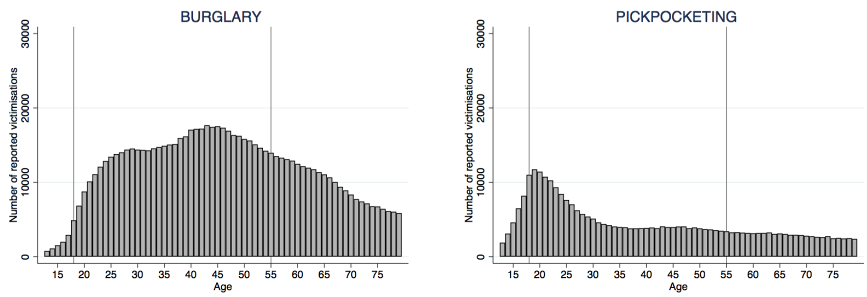
[18]Sex offences range from rape to sexual relationships with minors (s.a.). It is possible that the peak at younger ages is driven by certain sub-offences.

Figure 1: **Victimisation-Age Profiles**

Panel A. Violent offences



Panel B. Property offences



NOTE - The figure plots the age profiles of registered victimisations (age at the time of victimisation). Each figure refers to one of the six offence subsamples (Panel A for violent offences and Panel B for property offences). The two vertical lines mark ages 18 and 55. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

3 Labour market effects of victimisation

3.1 Empirical strategy

Ultimately, we are interested in the causal relationship between criminal victimisation and labour market outcomes. To identify such a causal effect, we have to overcome two identification problems. First, unobservable characteristics may correlate both with the outcome variable and the probability of becoming a victim of crime. For example, the (unobserved) ability to recognise and avoid risky situations may correlate with the ability of succeeding on the labour market. To address this potential omitted variable bias (over and above conditioning the sample on victimised individuals), our baseline estimation approach uses individual fixed effects in an event-study design. This strategy is similar to Grogger (1995) who studies the labour market effects of being arrested in a distributed leads and lags model, or more recently Dobkin *et al.* (2018) who study the economic consequences of hospital admissions in an event-study design. The basic idea in our setting is to compare labour market outcomes for the same individual before and after victimisation, thereby controlling for any (unobservable) individual traits which are constant over time.

Second, labour market outcomes may impact the chance of victimisation at the same time that victimisation affects labour market outcomes, which would result in simultaneity bias. For example, daily routines may change depending on an individual's employment situation, in return affecting the risk of victimisation. We address such simultaneity concerns by explicitly studying the timing of any effect of victimisation on labour market outcomes in the event-study design, and pay close attention to any pre-victimisation effects/trends. In addition, we conduct robustness tests with alternative estimation strategies, controlling for lagged labour market outcomes and including a range of individual controls instead of individual fixed effects. In Section 5, we further look at household spillovers, i.e. the labour market outcomes of a victim's spouse before and after victimisation. Here, simultaneity is less of a concern if one is willing to assume that the behaviour of one family member is unlikely to directly cause the victimisation of another.

Our baseline approach is based on an event-study design with individual fixed effects, conditional on having been a victim of crime (for a given offence) at any point during our sample period. Let Y_{ital} denote the respective outcome (as detailed in the previous section) for individual i in age group a and location l (municipality) at time t (year). Our event-study design approach addresses the simultaneity concern in the spirit of a Granger test for causality by allowing for leads of the treatment.[19] We further allow the effect to vary with time since victimisation to differentiate immediate from long-term effects. That is, we estimate the following equation in which the coefficient β_s varies with time to and since victimisation s:

[19]See Granger (1969) for the original article or e.g. Angrist and Pischke (2009) for an overview.

$$Y_{ital} = \mu + \beta_{-5} \cdot V_{it,-5} + \sum_{s=-3}^{4} \beta_s \cdot V_{it,s} + \alpha_i + \alpha_t + \alpha_a + \alpha_{t,a} + \alpha_l + u_{ital} \tag{1}$$

$V_{it,s}$ is a dummy equal to one if the calendar year minus the victimisation year is equal to s (e.g. $V_{it,0} = 1$ if both calendar and victimisation year are equal to 2010). To avoid using the (unbalanced) left-hand tail as a base period, the omitted period is defined as four years before victimisation (-4).[20] We include individual, year, age group and municipality fixed effects α_i, α_t, α_a and α_l, respectively. To control for age specific trends (e.g. due to cohorts entering the labour market at different times), we further include an age-group specific year fixed effect $\alpha_{t,a}$.[21] Finally, standard errors are clustered at the individual level to account for serial correlation in the error term, and we estimate the model separately by offence (see above).

A causal interpretation of the parameters in (1) relies on the assumption that the *timing of victimisation* is random, conditional on our sample restrictions (sample of victims) and control variables (including individual fixed effects). This has two implications. First, we have to assume that there are no time-variant unobservables correlating with both the time of victimisation and the labour market outcome. Second, we have to assume that there is no simultaneity, i.e. that the timing of victimisation is uncorrelated with the outcome. One may be concerned about the credibility of these assumptions. Estimating an event-study design as described above allows us to assess this to the extent that a violation would result in (unexplained) pre-trends.

A key advantage of the event-study approach is that even if there are visible pre-trends, we can assess sharp changes in labour market outcomes around the time of victimisation. To directly address remaining concerns, we offer four approaches: First, we conduct two types of falsification tests to assess whether the patterns estimated at the baseline are driven by e.g. remaining time trends. Second, we use alternative estimation strategies including flexible individual controls and lagged labour market outcomes instead of individual fixed effects. Third, we address the question of correlated shocks and explicitly discuss other life-events (other than labour market outcomes) that precede and follow the victimisation. This not only assesses

[20] Note that we choose -4 instead of -1 as the base period in order to transparently document potential pre-trends.

[21] We use the following age groups: 18 to 20, 21 to 25, 26 to 30, 31 to 35, 36 to 40, 41 to 45, 46 to 50, and 51 to 55 years old.

correlated shocks at the time of victimisation but also helps to understand potential drivers of long-term effects. Finally, we study household spill-overs (a yet unstudied component of the social cost of crime) for which simultaneity concerns play a lesser role.

3.2 Baseline results

Labour market outcomes

Our baseline results for each offence category are shown in Figure 2 for log earnings and in Figure 3 for days of benefit receipt. Each figure displays the estimated coefficients and 95% confidence intervals for the estimated $\hat{\beta}_s$ corresponding to equation (1).[22] The two vertical lines mark the beginning and end of the year in which the victimisation is reported. This particular coefficient averages between those individuals victimised at the beginning of the year (i.e. for whom we would expect to already see an effect) and those victimised towards the end of the year (i.e. for whom an effect might not yet be visible). In other words, the year of victimisation is partially treated whereas the year following the victimisation is the first full year of treatment.[23]

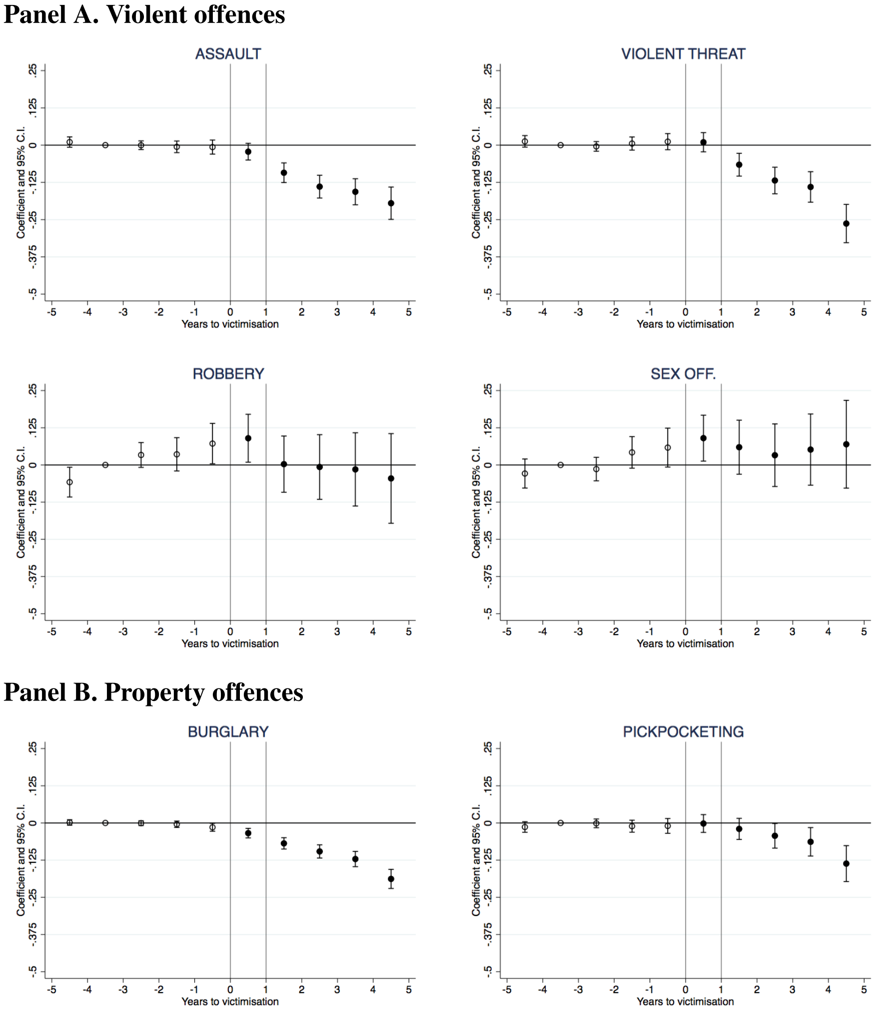
Panel A of Figure 2 shows the results for log earnings for all four violent offence samples. Starting with assault and violent threat, criminal victimisation significantly decreases earnings. In the first full year following the victimisation (+1) the estimated effects are large: -9.3% for assault and -6.6% for violent threat. Notably, for these two offences, there are no significant pre-trends and the earnings effects persist over the next five years - an observation which we come back to shortly. For robbery and sex offences, the two smallest crime categories, it is hard to draw firm conclusions from our results. First, the point estimates are suggestive of pre-trends. Despite this upwards trend, there appears to be a drop in earnings at victimisation (a deviation from the pre-trend). But, the point estimates are imprecisely estimated throughout.

Panel B of the same figure shows the earnings results for our two property offences, burglary and pickpocketing. Note that the confidence intervals in particular for burglary are generally

[22] The estimated coefficients and standard errors for assault and violent threat are reported in columns (1) and (2) of Table 2. Similar tables for the remaining offences are available in the Online Appendix.

[23] In Section 3.3, we discuss the robustness of our results to an event-study design at the monthly instead of the annual level for the main labour market outcomes. This allows us to look at the timing of victimisation within the victimisation year; however, the month of victimisation itself is again partially treated.

Figure 2: **Baseline Results - Log Earnings**



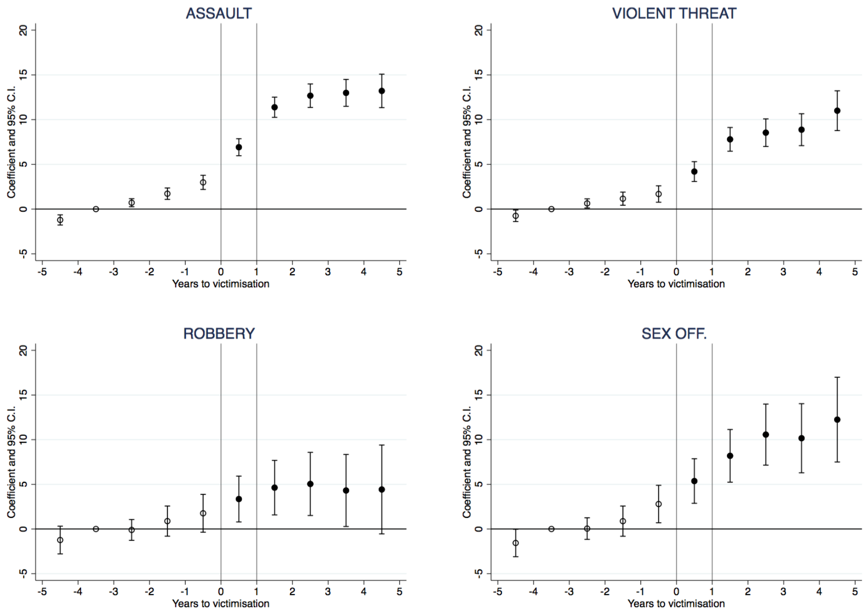
NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with log earnings as the dependent variable. Each figure refers to one of the six offence subsamples (Panel A for violent offences and Panel B for property offences). The two vertical lines mark the start and end of the victimisation year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

smaller than for the violent offences, but sample sizes are also by magnitudes larger (see Table 1). For burglary, we see a decrease in earnings which is comparable in magnitude to the violent threat point estimates (6.9%).[24] The results for assault, violent threat and also burglary are robust to restricting the sample to individuals who only report one victimisation during the sample period (see Section 3.3). For pickpocketing, we see marginally significant decreases in earnings which disappear once we apply that restriction. This suggests that they are not driven

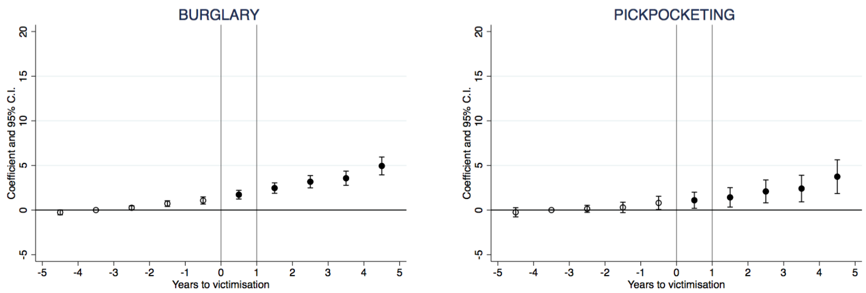
[24]Recall that (in the Dutch context) burglary includes both burglary with and without the use of violence. That is, this category includes fairly severe offences (more comparable to crimes classified as robbery in other countries) and one can plausibly expect to see an effect on labour market outcomes to the same extent as one would expect that for violent offences.

Figure 3: **Baseline Results - Days of Benefits**

Panel A. Violent offences



Panel B. Property offences



NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with days of benefit receipt as the dependent variable. Each figure refers to one of the six offence subsamples (Panel A for violent offences and Panel B for property offences). The two vertical lines mark the start and end of the victimisation year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

by pickpocketing per se, but likely by more severe later victimisations.[25]

The results for our second main outcome, the annual number of days of benefit receipt (i.e. benefit dependency), are shown in Figure 3. They are consistent with our findings for earnings: Following criminal victimisation, there are significant and sizeable increases in the number of days of benefit receipt for assault and violent threat. For the year following the victimisation

[25]Throughout, the confidence intervals increase with time since victimisation. This can be explained by the unbalancedness of our sample: The last year of victimisation in our sample is 2016, same as the last year of observed labour market outcomes. That means that point estimates for the lags further away from the victimisation year are identified of a smaller sample than the point estimates close to the victimisation year. We find that our findings are robust to restricting the sample, for instance, to exclude victims from 2015 and 2016 (see Online Appendix). Further, note that including year as well as individual fixed effects actually deals with concerns about selective unbalancedness, as described in Borusyak and Jaravel (2017).

(+1), we find increases in the days of benefit receipt by 11.4 days for assault and 7.8 days for violent threat (corresponding to 14.7% and 10.4% at the mean, respectively, and relative to the omitted period four years before victimisation). The impact of victimisation on benefit receipt again persists over time for these two offences. When we split up the results by type of benefits (UI, DI/sickness, welfare), we find increases in the number of days of benefit receipt for all types but with clearly the largest point estimates for welfare (results available upon request).

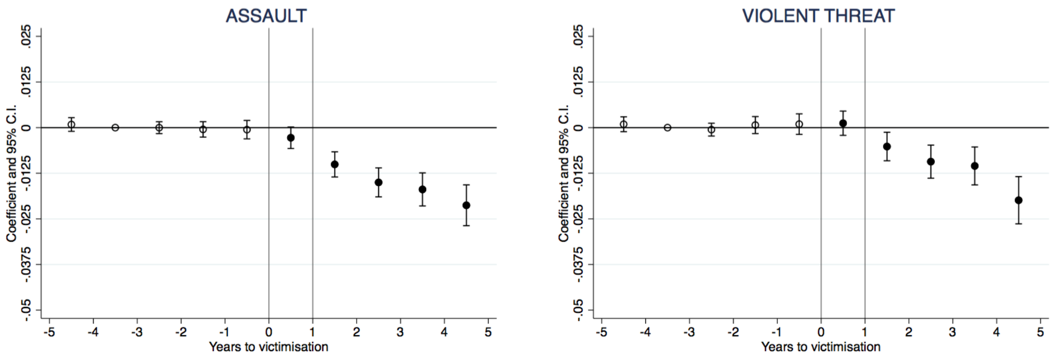
While there were no significant pre-trends for earnings, these are now visible for both assault and violent threat. We will discuss two potential explanations: First, other life-events (happening around the time of victimisation) may contribute to the changes in labour market outcomes. We will study potential candidates (divorce, moving). Second, there may be unobserved victimisation(s) preceding the first observed victimisation, i.e. there could be an earlier treatment effect that shows up as a pre-trend. In that case, the pre-trends would not reflect an identification problem per se, but rather be the consequence of earlier victimisations. We will investigate these points in more detail in the following sections. However, it is important to highlight that for both assault and threat we observe a large increase in the point estimate in the years of and following the victimisation that is not plausibly explained by a continuation of the pre-trend alone, as illustrated in Appendix Figure B1. By fitting a linear trend through the four pre-victimisation point estimates (using simple OLS), we can visualise the continuation of the pre-trend in the absence of victimisation. For both assault and violent threat, there is a clear deviation from this trend.[26] That is, the victimisation can be interpreted as a sharp 'escalation point', even if other events contribute to the changes in labour market outcomes. We will return to that argument in due course. For robbery and sex offences as well as for the property offences, such an 'escalation point' is less visible in the results.

The remainder of the paper focuses on the results for two offences: assault and violent threat. Besides space constraints, we do this for a number of reasons. First, sample sizes for robbery and sex offences are an order of magnitude smaller than for the other offences (see Table 1), and the resulting estimates are imprecise. Moreover, the composition of victims of

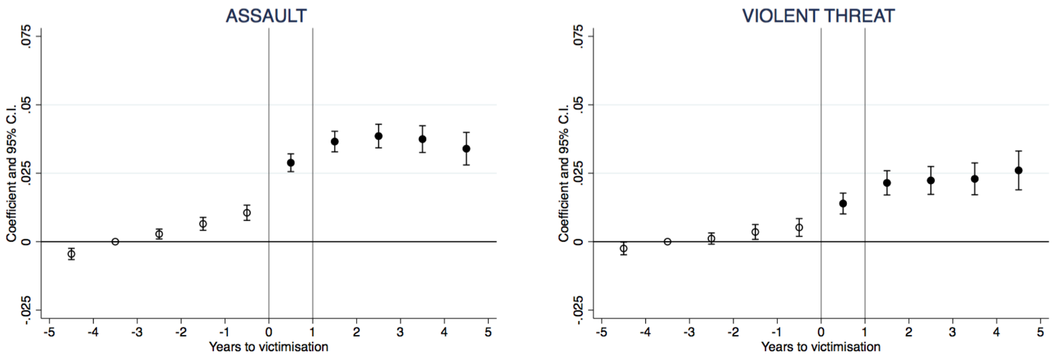
[26]The difference between the predicted trend and the estimated coefficients could be interpreted as a lower bound of our point estimate for the effect of victimisation. See Dobkin *et al.* (2018) for a similar graphical approach.

Figure 4: **Baseline Results for Extensive Margin**

Panel A. Earnings above 5th percentile



Panel B. Any benefit income



NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with a dummy for earnings above the 5th percentile as the dependent variable in Panel A and a dummy for any positive benefit income in Panel B. The figures to the left show results for assault, those to the right for violent threat. The two vertical lines mark the start and end of the victimisation year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

sex offences may differ along the age-victimisation profile given the range of sub-categories. Second, the estimates for the two property offences are precisely estimated, but there is a less clear escalation point visible in the results (at least for the benefit outcome). However, we stress that the results for all offences are available in the Online Appendix or on request. Throughout the paper, we reference these results when appropriate,

For both assault and violent threat, the effect of victimisation on earnings and benefits is persistent, if not increasing over time. First, it is plausible that individuals for instance change their job and/or career or labour supply decisions, leading to lower earnings.[27] Second, and related to the latter, the estimated effect is a combination of an intensive and extensive margin effect:

[27]To further investigate this channel we also explored data on hours of work and job changes. Such data is only available from 2006 onwards. Furthermore, there is no information on hours worked for self-employed and workers with flexible contracts. The results unfortunately contain too much noise for a clear-cut conclusion.

In addition to reductions in earnings, there may be changes in employment status increasing the number of individuals with no earnings. Indeed, this is supported by the fact that we find increases in benefit receipt. We thus also estimate the same specifications as at the baseline, but measuring the outcome at the extensive margin (i.e. earnings above the 5th percentile or any positive benefit income). The results are shown in Figure 4: For both offences, a similar pattern is seen as at the baseline. Not surprisingly though, magnitudes differ: In the year after victimisation (+1), the extensive margin effect for assault (violent threat) amounts to -1.3% (-0.6%) at the mean for earnings and +13.1% (+7.8%) for benefits.[28] These results imply that at least part of the effects on earnings and benefits is driven by an extensive margin response, i.e. a change in employment status, that may contribute to the persistent effects. We will discuss further potential explanations for these patterns in Section 3.4, including other life-events, multiple victimisations and a possible victim-offender overlap.

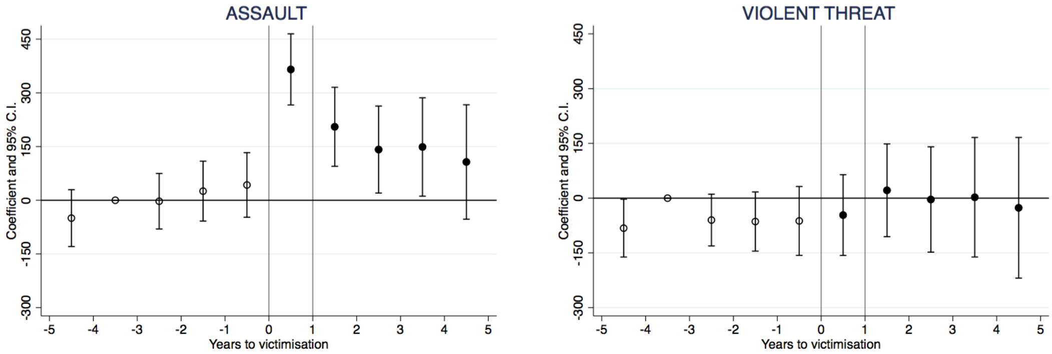
Health outcomes

Labour market outcomes may be affected through a deterioration in physical and/or mental health. Panel A of Figure 5 shows the results for total health expenditure. Especially for assault, there is a clear spike in the year of victimisation, corresponding to a 17.8% increase relative to the mean. The increase in total health expenditure is strong in the short-run and levels out subsequently. For violent threat we do not see an increase, which is in line with the fact that this offence does not involve actual physical violence towards the victim. Note that there are no significant pre-trends for total health expenditure. Panel B of Figure 5 illustrates the estimates for health expenditure explicitly marked as mental health expenditure. Again, there is a sharp increase in the year of victimisation for assault (43.1%) but also evidence for an increase for violent threat (21.1%). The increase in mental health expenditure, a proxy for deterioration in mental health, is in line with the findings reported in Cornaglia *et al.* (2014) based on Australian survey data as well as in Dustmann and Fasani (2016) based on aggregate-level data from the

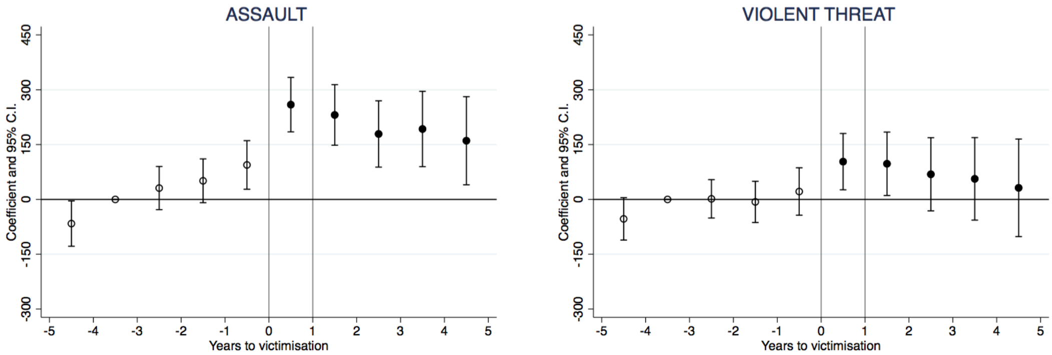
[28] In our baseline specification for log earnings, we replaced negative and zero earnings with a small value before taking logs. The extensive margin exercise is also a robustness test of that approach.

Figure 5: **Health Expenditure**

Panel A. Total health expenditure



Panel B. Mental health expenditure



NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with total health expenditure as the dependent variable in Panel A and mental health expenditure in Panel B, both for assault (left) and violent threat (right). The two vertical lines mark the start and end of the victimisation year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

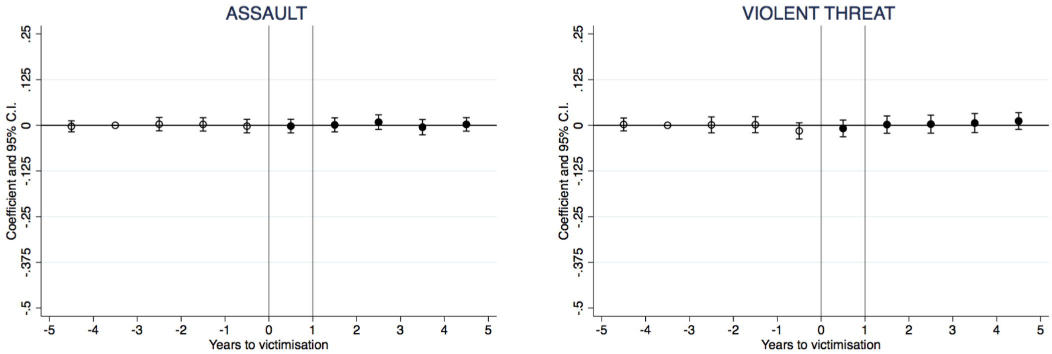
UK, showing deterioration in mental health as a consequence of crime exposure.[29]

It is also worth pointing out that, despite the small sample and quite large standard errors, we find a large and significant increase in health (and in particular mental health) expenditure for sex offences. While there is a visible pre-trend (maybe due to earlier, unreported victimisations), a sharp change can be seen at the time of victimisation. For the other offences (robbery, burglary and pickpocketing), no such changes are found (see Figure B1 in the Online Appendix).

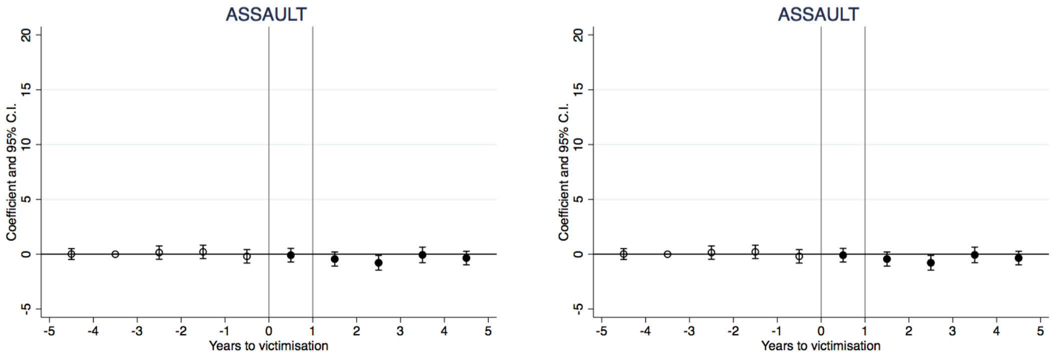
[29]We have explored the possibility of using the severity of the health shock as a proxy for the (otherwise unobserved) severity of the offence to study heterogeneous labour market responses. This comes with two problems though. First, we would condition our analysis on an outcome. Second, the health expenditure data is only available from 2009 onwards (as opposed to 1999 for the labour market outcomes) which limits us in terms of statistical power and further sample splits.

Figure 6: **Falsification Test (I) - Placebo Victimisation Year**

Panel A. Log earnings



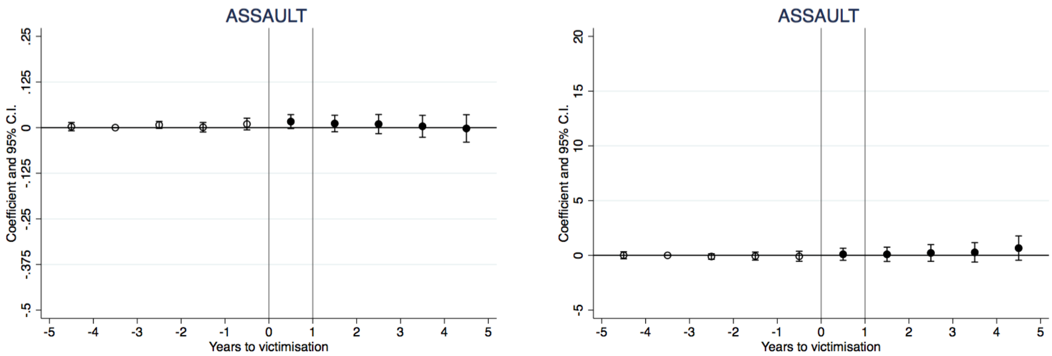
Panel B. Days of benefits



NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) when assigning a placebo victimisation year. The dependent variable is log earnings (Panel A) and days of benefit receipt (Panel B) for assault (left) and threat (right). The two vertical lines mark the start and end of the victimisation year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

Figure 7: **Falsification Test (II) - 15% Random Sample**

Log earnings **Days of benefits**



NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) when using a 15% random sample from the non-victimised population (based on the assault sample size/composition) and assigning a placebo victimisation year. The dependent variable is log earnings (left) and days of benefit receipt (right). The two vertical lines mark the start and end of the victimisation year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

3.3 Falsification, specification and robustness tests

Falsification tests

To rule out that our baseline specification picks up a spurious relationship between the year of victimisation and the outcome of interest (maybe due to remaining time trends or economic shocks) we conduct two types of falsification tests. First, we randomly draw a year from the list of potential victimisation years, assign that year as a placebo instead of the actual victimisation year and re-estimate equation (1). Figure 6 illustrates the results for log earnings (Panel A) and days of benefits (Panel B), using the same scale as the baseline results. Assigning placebo instead of actual victimisation years, we generally do not find significant effects on either of the two outcomes. Moreover, the point estimates are small and close to zero.[30]

As a second falsification test, we draw a 15% random sample of the non-victimised population, apply equivalent sample restrictions and assign a placebo year of victimisation (keeping the offence composition of the sample constant). This second falsification test improves on the first by abstracting from dynamic selection issues (only non-victims are included here), but has the disadvantage that the sample composition and baseline outcomes are different from those in our main analysis sample, as discussed earlier (Table 1). The results are shown shown in Figure 7. Again, they are supportive of our baseline results not being driven by any spurious relationship: The coefficients are generally not significantly different from and close to zero.[31]

Specification tests

Appendix Tables A2 and A3 show the estimated coefficients and standard errors when we sequentially build the baseline specification for both log earnings and days of benefits, respectively. Column (1) starts with the simple OLS without any controls or fixed effects. Column (2) adds year, age and year by age group fixed effects (to capture age-group specific time trends) and column (3) includes municipality fixed effects. Column (4) adds individual fixed effects and represents the baseline specification - equation (1) - as shown in Figures 2 and 3. Including

[30]The same is true for the other offence categories; see Figure B2 in the Online Appendix.

[31]The figures are based on the assault sample size/composition; equivalent figures based on the other offence sample sizes yield the same conclusions. Results are available upon request.

individual fixed effects matters a lot, as seen when comparing columns (1) - (3) to column (4) for both offences (assault in Panel A and violent threat in Panel B). Simple OLS yields highly significant point estimates for all leads and lags that differ substantially from those in the fixed effects estimation. Including individual fixed effects results in the leads going to zero while the lags are markedly smaller but remain significant. This implies that even conditional on being a victim of a specific offence, there are unobserved differences between individuals that need to be taken into account.

What type of unobserved differences do the individual fixed effects pick up? To answer that question, columns (5) to (7) provide the results of alternative specifications: Column (5) starts by controlling for time-invariant individual controls (gender, non-western immigrant) instead of individual fixed effects. In fact, just adding these controls does not appear to improve significantly upon a simple OLS specification. Moving to column (6), we add victimisation year by age group fixed effects (to flexibly allow for different effects by age group). The results appear to be more similar to the baseline in column (4), which suggests that the victimisation year explains part of the unobserved differences that are picked up by the individual fixed effects. Lastly, column (7) includes the first and second lag of the outcome variable as a control variable. While this leads to quantitatively smaller point estimates, there is a sharp change in the year of victimisation. This specification has the advantage that it allows us to control for predetermined labour market histories, yet it comes at the cost of 'controlling for outcomes' in the years following the victimisation. This could contribute to the fact that on the one hand we see mitigated pre-trends for the benefit outcome (compared to the individual fixed effects specification), but on the other hand attenuated point estimates especially in the longer-run (although still significantly different from zero three years after victimisation).[32]

[32]For the other offence categories (see Online Appendix Tables A7 to A10), we see a similar pattern in columns (1) to (4). Moreover, for most cases where significant pre-trends were observed in the baseline with individual fixed effects (mainly for benefits), the leads again go towards zero in the lagged outcome specification. For earnings, a significant post-victimisation decrease in earnings is still seen for robbery, burglary and pickpocketing but not for sex offences.

Robustness tests

Our baseline sample is restricted to ages 18 to 55. A recent report by Statistics Netherlands suggests that about 80% of 18-year olds are still in education, but that this share drops to about 30% by the age of 26 (and remains stable thereafter).[33] That is, the majority of individuals will have entered the labour market by age 26 but not necessarily age 18. As our sample conditions on victimisation, the share of individuals who have entered the labour market before age 26 (i.e. who did not complete higher education) may be larger than in the general population. In addition and as shown before, the peak age of victimisation is below age 26 for all offences but burglary. We therefore include ages from 18 onwards in our baseline sample, but show the results for a more age-restricted sample in Appendix Figure B2.[34] The results are robust in terms of magnitude and precision for both outcomes, log earnings and benefit receipt; the pre-trends for the benefit outcome are arguably less pronounced in the older sample.[35]

We further conduct the following robustness tests: To test functional form assumptions, we use level instead of log earnings. We change the set of controls by i) adding controls for moving over and above location fixed effects and ii) including (finer level) neighbourhood instead of municipality fixed effects. We test the robustness of our results towards sample restrictions and specifications by i) including victims of multiple offences *within* a year, ii) leaving two victimisation years out at a time to test whether specific victimisation years drive the results, iii) excluding outcomes for the year 2016 (in other words, including individuals in the sample who are victimised only after the sample period), iv) excluding individuals for whom we do not observe any pre-victimisation labour market outcomes and v) adding more leads (eight years) to the specification. Our results are generally robust to all these changes (see Online Appendix Tables A1 to A4 for assault/threat; results for the remaining offences are available on request).

Lastly, we estimate the event-study design at the monthly level for the two main labour

[33]See: https://www.cbs.nl/NR/rdonlyres/E327EC88-89C2-443C-B4AF-83F0E926EABB/0/20131002v4art.pdf (in Dutch, last accessed on 26 June 2018).

[34]Studying the impact of criminal victimisation on labour market entry and/or educational outcomes is an interesting question in itself, which we leave to future research as it exceeds the scope of this paper.

[35]The results for the remaining offences are similarly robust to the alternative age restriction. However, the pre-trends are unaffected in these specifications. Results are available upon request.

market outcomes (earnings and benefit receipt) as shown in Online Appendix Figures B3 and B4.[36] Two conclusions can be drawn. First, the patterns are consistent with those found at the annual level both for earnings and benefits and across offences. Second, for both assault and violent threat, the sharp change in labour market outcomes - the 'escalation point' - which we observed at the (annual-level) baseline is seen immediately in the month of victimisation.

3.4 Correlated shocks and long-term effects

Two observations stand out from what we have discussed so far: First, our results suggest that there are long-lasting changes in earnings and benefit dependency following a victimisation. These are visible up to four years after the victimisation year and could in part be driven by the extensive margin effect discussed earlier. For instance, individuals may leave the labour market and not return for years, or remain long-term dependent on benefits once entering a specific benefit scheme.[37] An alternative explanation is that the victimisation per se may be an escalation point triggering other life-events that contribute to the effects on labour market outcomes seen in the long-run. Second, we see pre-trends for the benefit outcome in the main specification. Are these driven by other factors (life-events) leading up to the first observed victimisation that negatively affect labour market outcomes and thereby lead to visible pre-trends?

To shed more light on these questions, we study correlated shocks and life-events potentially contributing to long-term effects. In other words, are there other events leading up to and/or following the victimisation which contribute to the pre-trends and estimated long-term effects? We start by looking at divorce and moving as potentially relevant life-events.[38] Next, we study multiple (both earlier and later) victimisations. Lastly, we address the possibility that a victim-offender overlap may contribute to the long-term effects.[39]

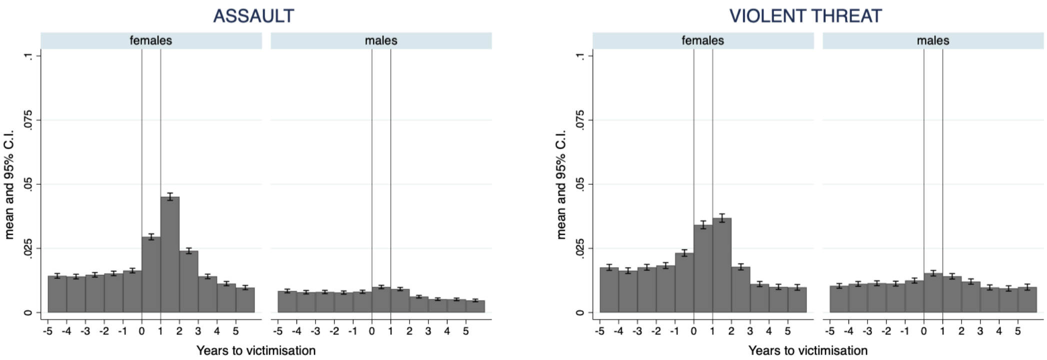
[36] These are the two main outcomes for which we have monthly information. For assault (violent threat), we know the month of victimisation for 97% (93%) of the individuals in the baseline sample. For computational reasons, we have to restrict this robustness check to three years before and after victimisation, and for burglary, we further draw a 25% random sample from the baseline sample.

[37] Unlike in other countries, in the Netherlands there is no limit on the number of years an individual can claim welfare benefits which makes such state-dependence a plausible explanation.

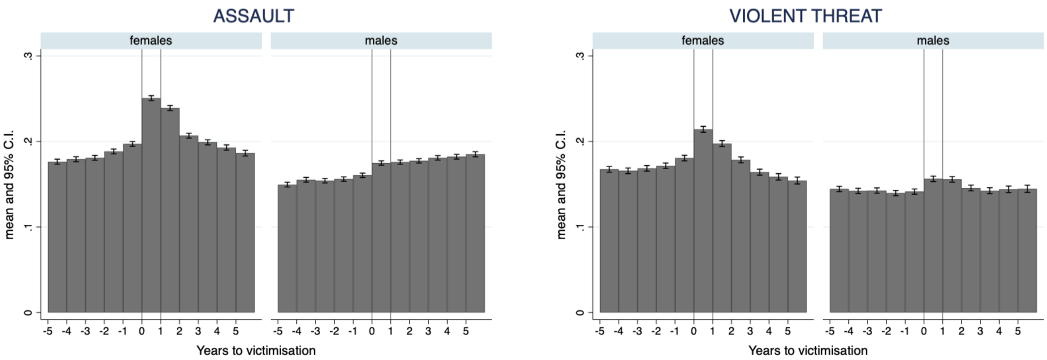
[38] High-school dropout could be another candidate, if it leads to an escalation point in life for youth. We do not think that this is plausible in our setting, however, given the robustness of our results to restricting our sample to

Figure 8: **Correlated Shocks - Divorce and Moving**

Panel A. Divorce



Panel B. Moving



NOTE - The figure shows the raw mean (and 95% confidence interval) by time to and from victimisation and by gender. Panel A shows the mean for a dummy variable indicating divorce in a given year and Panel B for a dummy variable indicating a change in address. The two vertical lines mark the year of victimisation. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

Divorce and moving

Family disruption (in particular, divorce) and moving decisions may - if preceding the victimisation - alter both labour market outcomes and the risk of victimisation, or - if following the victimisation - be a consequence of the latter. Figure 8 shows the (unadjusted) share of individuals for whom we observe a divorce or a move (change of address).[40] We compute that share separately for the five years before and after victimisation, respectively, as well as by gender.

Two observations stand out: (i) The share of individuals who divorce or move in a given year changes around the time of victimisation and (ii) to a larger extent for females than for

ages 26-55 instead of 18-55 (see Figure 2).

[39]Note, however, that individuals with a criminal record leading up to or in the year of victimisation were already excluded from the sample.

[40]In the Netherlands, 90% of requests for divorce are approved within one year. If both partners agree on the divorce, 90% of all cases are approved within two months. Source: https://www.rechtspraak.nl/Uw-Situatie/Echtscheiding/Paginas/doorlooptijd.aspx.

males. Specifically, the share of female assault victims who divorce in a given year increases from 1.6% in the year before to 4.5% in the year after victimisation (0.8% and 0.9% for males, respectively). Similarly, the share of female violent threat victims who divorce increases from 2.3% to 3.7% (1.2% and 1.4% for males). A similar pattern is seen for moving: The share of females who move in a given year increases from 19.7% in the year before to 23.9% in the year after victimisation in the assault sample (16.0% and 17.6% for males) and from 18.1% to 19.8% (14.1% and 15.6% for males) in the violent threat sample. In contrast, the share of divorces and moves is quite flat leading up to the year of victimisation.

Though descriptive, these figures nonetheless suggest two conclusions: First, the likelihood of individuals divorcing or moving appears higher in the years of and subsequent to the victimisation, and particularly so for females. Given the timing (shares being rather flat before victimisation), it is reasonable to assume that these divorces and moves follow rather than precede victimisation. However, it is plausible that such life-events may contribute to the earnings losses and increase in benefit dependency following the victimisation as seen in our baseline results. Second, the figures suggest important differences between females and males when it comes to victimisation. We will discuss that point in more detail in Section 4.[41]

Multiple victimisations

So far, we have focussed on the first observed victimisation but have not explicitly addressed the possibility of multiple victimisations. This has two implications: (i) The pre-trends discussed in Section 3.2 may in fact be driven by previous, unobserved victimisations and (ii) we may overestimate the impact of a criminal victimisation on labour market outcomes if we attribute all of the effect to one victimisation, ignoring later ones. To address the former, we take our sample restriction one step further and exclude the victimisation years of 2007 and 2008. The baseline restricts the sample to individuals who have not reported any criminal victimisation within (at least) the two years prior to their observed victimisation and assumes that the effect is not confounded by any victimisation earlier than that. Excluding 2007 and 2008 victimisations, we

[41]Online Appendix Figure B5 shows corresponding figures for the remaining offences. A similar change in the share of victims who divorce or move at the time of victimisation is not seen for these offences.

restrict our sample to individuals who have not reported any victimisation within the previous *four* instead of *two* years, thereby relaxing this assumption. To address (ii), we first restrict the sample to individuals who have not reported more than one victimisation between 2007 and 2016 (88% of individuals in our sample), and second re-estimate our individual fixed effects baseline model but include dummies to control for any contemporaneous victimisations.

Appendix Figure B3 reports the results when we exclude victimisation years 2007 and 2008. The overall pattern remains unchanged, with comparable magnitudes to the baseline (slightly smaller for violent threat, robbery and sex offences). But, at least for violent threat, there is no longer evidence of a pre-trend in benefits. While this could of course be driven by a slightly different sample, it is plausible that restricting the sample to individuals with four instead of two years of no observed victimisation matters: As seen in Section 3.2, there are labour market effects up to four years and more after victimisation. Indeed that implies that the significant pre-trends found earlier may at least partly be attributed to earlier (unobserved) victimisations.[42]

Table 2 presents the results for log earnings (columns (1) to (4)) and days of benefits (columns (5) to (8)) when we take later victimisations into account. For ease of comparison, columns (1) and (5) repeat the baseline estimates, respectively. Columns (2) and (6) correspond to restricting the sample to single victimisations only, while columns (3) and (7) are based on the full sample but control for contemporaneous victimisations. Restricting the sample to single victimisations generally attenuates the estimated coefficients for both outcomes but leaves the overall pattern intact. Taking assaults (Panel A) in column (2) as an example and looking at the first full year of treatment (+1), we find a 6.3% decrease in earnings compared to 9.3% at the baseline, and a 10 days increase in benefit receipt compared to 11.4 days at the baseline. Similarly, we find slightly attenuated coefficients when we control for additional victimisations, with an 8.3% decrease in earnings and a 11.0 days increase in benefit receipt. This is consistent with what one expects: The baseline combines the effect of the first and subsequent victimisations.

Together, these results suggest that multiple victimisations matter; yet, the qualitative pattern of our estimates remains the same independently of whether or not we explicitly take them

[42] We come to similar conclusions for the remaining offences. The results are available on request.

Table 2: **Multiple Victimisations and Criminal Record**

Outcome:	Log earnings				Days of benefits			
Specification:	Baseline	Only single victimisations	Controls for later victimisations	No criminal record	Baseline	Only single victimisations	Controls for later victimisations	No criminal record
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Assault								
-5	0.010(0.009)	0.011(0.010)	0.012(0.009)	0.005(0.009)	-1.214(0.291)***	-1.305(0.313)***	-1.283(0.291)***	-1.136(0.301)***
-3	-0.000(0.007)	0.005(0.008)	-0.001(0.007)	0.006(0.008)	0.713(0.227)***	0.676(0.242)***	0.730(0.227)***	0.589(0.234)**
-2	-0.006(0.010)	0.003(0.011)	-0.007(0.010)	0.006(0.010)	1.722(0.326)***	1.634(0.348)***	1.760(0.326)***	1.594(0.337)***
-1	-0.007(0.012)	0.002(0.013)	-0.008(0.012)	0.012(0.013)	2.987(0.403)***	2.781(0.431)***	3.050(0.403)***	2.636(0.419)***
0	-0.022(0.014)	-0.007(0.016)	-0.024(0.014)*	0.008(0.015)	6.920(0.484)***	6.217(0.519)***	7.003(0.484)***	6.256(0.504)***
+1	-0.093(0.017)***	-0.063(0.018)***	-0.083(0.017)***	-0.039(0.018)**	11.382(0.576)***	9.966(0.619)***	11.028(0.575)***	10.240(0.603)***
+2	-0.139(0.019)***	-0.091(0.021)***	-0.121(0.019)***	-0.063(0.020)***	12.675(0.670)***	10.402(0.722)***	12.010(0.669)***	10.670(0.703)***
+3	-0.156(0.022)***	-0.086(0.024)***	-0.131(0.022)***	-0.057(0.023)**	12.991(0.768)***	10.086(0.829)***	12.075(0.766)***	10.352(0.807)***
+4	-0.195(0.028)***	-0.067(0.030)**	-0.156(0.028)***	-0.046(0.029)	13.211(0.955)***	8.507(1.037)***	11.844(0.952)***	8.976(1.009)***
Panel B. Violent threat								
-5	0.013(0.010)	0.009(0.011)	0.013(0.010)	0.011(0.010)	-0.752(0.328)**	-0.883(0.357)**	-0.772(0.328)**	-0.705(0.335)**
-3	-0.004(0.008)	-0.009(0.009)	-0.005(0.008)	0.003(0.008)	0.636(0.260)**	0.809(0.284)***	0.643(0.260)**	0.644(0.266)**
-2	0.005(0.011)	-0.002(0.012)	0.005(0.011)	0.014(0.012)	1.163(0.374)***	1.322(0.409)***	1.179(0.374)***	0.967(0.383)**
-1	0.012(0.014)	0.011(0.015)	0.011(0.014)	0.030(0.014)**	1.686(0.468)***	1.598(0.511)***	1.715(0.468)***	1.396(0.481)***
0	0.010(0.016)	0.010(0.018)	0.008(0.016)	0.040(0.017)**	4.196(0.567)***	3.648(0.617)***	4.240(0.567)***	3.596(0.583)***
+1	-0.066(0.020)***	-0.055(0.021)**	-0.058(0.020)***	-0.021(0.020)	7.797(0.676)***	6.966(0.737)***	7.531(0.675)***	6.778(0.697)***
+2	-0.119(0.023)***	-0.095(0.025)***	-0.105(0.023)***	-0.049(0.023)**	8.541(0.788)***	7.347(0.861)***	8.070(0.787)***	6.876(0.815)***
+3	-0.140(0.026)***	-0.107(0.029)***	-0.122(0.026)***	-0.052(0.027)*	8.876(0.909)***	6.972(0.994)***	8.255(0.908)***	6.465(0.942)***
+4	-0.263(0.033)***	-0.190(0.036)***	-0.238(0.033)***	-0.114(0.034)***	10.998(1.135)***	7.840(1.245)***	10.140(1.134)***	7.093(1.178)***

NOTE - The table shows the estimated coefficients and standard errors for the regressions corresponding to equation (1) using different specifications as indicated at the top of each column. Standard errors are clustered by individual. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

into account: There is an immediate, significant and persistent impact of victimisation for both earnings and benefits. Yet, the fact that the point estimates are attenuated once we explicitly take later victimisations into account indicates that these contribute to the persistent effects. Indeed, it is possible that one victimisation increases the risk of later victimisations and sets an individual on a track to later victimisations.

Criminal record

Finally, can the lasting effects of criminal victimisation be explained by a victim-offender overlap? While we restrict our baseline sample to not having a criminal record prior to victimisation, individuals may subsequently engage in criminal activity. We address this more explicitly by restricting our sample to those individuals who do not have a criminal record at any point during the sample period (i.e. even after victimisation).[43] The results are reported in columns (4) and (8) of Table 2 for earnings and benefits, respectively. Compared to the baseline, the point estimates are attenuated for earnings, but the main conclusions still hold. For benefits, the results are quite robust compared to the baseline. Together, this does suggest that later criminal involvement (and resulting labor market effects) drive part of our long term effects for earnings, but to a lesser extent for benefits. These differences may be due to underlying gender heterogeneity, which we will discuss in more detail in Section 4.

Victimisation as an escalation point

What is the main take-away of our analysis of correlated shocks and long-term effects? First, our results are suggestive that earlier victimisations, but not other life-events, contribute to the pre-trends seen for the benefit outcome. Importantly, despite these pre-trends, there is a visible and sharp change in trajectory at the time of the (observed) victimisation. The results in this subsection indicate that this victimisation leads to an escalation point in the victim's life: At the time of victimisation, but not before, there is an increase in the probability of divorce and

[43] The likelihood of having a criminal record at any point after the victimisation varies considerably by offence: For assault, violent threat and robbery, this is the case for approximately 10 percent of the individuals in the sample, while for sex offences, burglary and pickpocketing only for around 5 percent.

moving (in particular for females). Furthermore, the results attenuate once we control for later victimisations or a later criminal record, suggesting that a victimisation might set an individual at the margin on a trajectory of such events in the future.

Finally, Appendix Figure B4 shows the estimates when we restrict the sample to individuals with no criminal record, at the same time controlling for contemporaneous victimisations and allowing for even longer-term effects. Although very demanding on the data, we find patterns consistent with the above: The earnings effects are attenuated, especially in the long run (for assault), while the effects on benefit receipt remain significant, sizeable and lasting.

4 Heterogeneity analysis

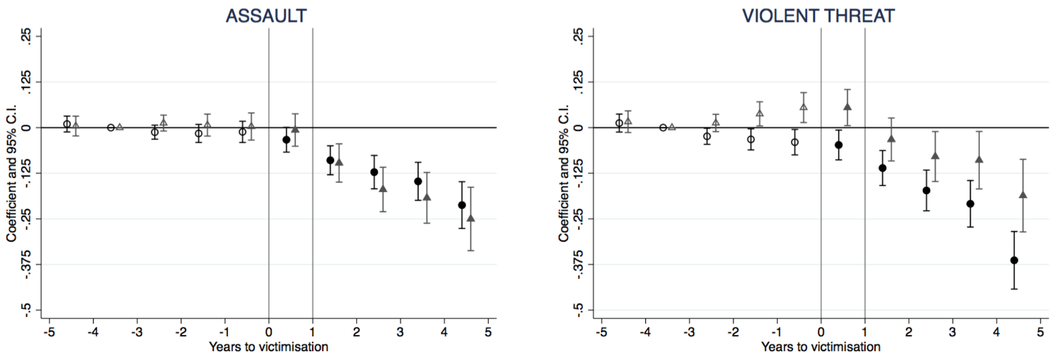
So far, we have established a significant and lasting impact of criminal victimisation on both earnings and benefit receipt. Is everyone affected in the same way or is there any heterogeneity? More specifically, are there differences in the response to victimisation and/or (unobserved) heterogeneity in offence characteristics? In the following, we present heterogeneity analyses with respect to the victim's gender. We next exploit household-level data and use information about the offending behaviour of the victim's partner to proxy for the offence being related to domestic violence. Finally, we support our findings by describing heterogeneity in offence characteristics exploiting supplementary information from the Dutch Victimisation Survey.

Gender heterogeneity

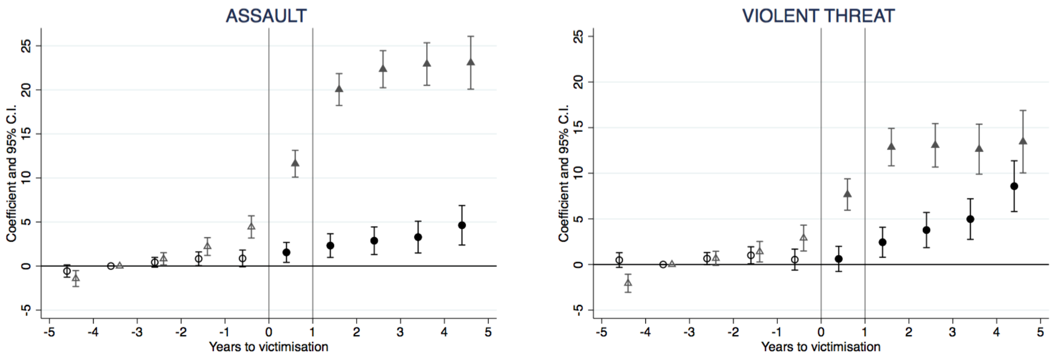
We start by separately estimating the effects for males and females. The results are presented in Panel A and B of Figure 9 for log earnings and days of benefit receipt, respectively. The figures show the estimated coefficients and 95% confidence intervals for all leads and lags for males (black circle) and females (grey triangle); the vertical lines again mark the year of victimisation. For earnings (Panel A), we do not see significant differences between males and females when it comes to assault. For violent threat, the point estimates diverge over time with larger magnitudes for men but - if at all - only marginally significant differences. For benefits (Panel B), there are

Figure 9: **Gender Heterogeneity**

Panel A. Log earnings



Panel B. Days of benefits



NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with log earnings (Panel A) and days of benefit receipt (Panel B) as the dependent variable for assault (left) and threat (right), separately by gender. The black circles represent the results for males, the grey triangles for females. The two vertical lines mark the start and end of the victimisation year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

much more striking differences both for assault and threat with point estimates for females being substantially larger than for males. In the first year after victimisation (+1), there is an increase in the number of days of benefit receipt for females of 20.0 days for assault (20% relative to the mean) and 12.9 days for violent threat (14%). In comparison, the point estimates are substantially smaller for males (4% relative to the mean, for both offences). These results imply that either the margin at which males and females respond to criminal victimisation is different or the type of offence differs by gender.[44]

[44]For burglary and pickpocketing, we see larger point estimates for men when it comes to earnings and overall no significant differences for benefits. For robbery and sex offences, standard errors are large and no firm conclusions can be drawn. We also look at gender heterogeneity for health expenditure. The results for assault suggest that mental health responses are larger for women than for men, while there are no differences in total health expenditure. We do not see significant differences for violent threat. The results are available on request.

Offence heterogeneity

A limitation of the victimisation register data is that it contains little detail on the circumstances of the offence. However, differences *within* our offence categories may play an important role: Assault, for example, could be domestic violence or a pub fight. If our results were entirely driven by domestic violence, this would call for a different type of policy response than the latter case. To get a better understanding of the underlying offence composition, we restrict the sample to individuals who cohabit with a (non-victimised) partner in the year of victimisation.[45] Appendix Figure B5 shows the results: For both, assault and threat, the point estimates are large for individuals living with a partner (note that there are no significant pre-trends for benefits in this specification). Notably, we can draw similar conclusions based on the results for sex offences (results are available on request). Taken together, we see larger effects for women and for those cohabiting with a partner (at least for the benefit outcome) which points towards domestic violence as an important channel.[46]

To address this, we exploit the household information in the register data to separately estimate the effect of a criminal victimisation for victims who do or do not live with a (non-victimised) partner who becomes a registered suspect of crime in the year(s) of or following the victimisation.[47] In other words, we split the sample by whether the partner has a criminal record or not. This again helps to shed light on underlying channels, if one is willing to assume that those who live with a non-criminal partner are less likely to be a victim of domestic violence than those who live with a partner registered as a suspect of a (violent) crime in the year of or following the victimisation.[48] The results are shown in Figure 10, where the black

[45] We restrict the sample to individuals who live with non-victimised partners to abstract from potential household spillovers (see Section 5). We can follow a similar approach for all offences, but for this part of the analysis focus on those that could plausibly entail domestic violence (i.e. assault, violent threat and sex offences).

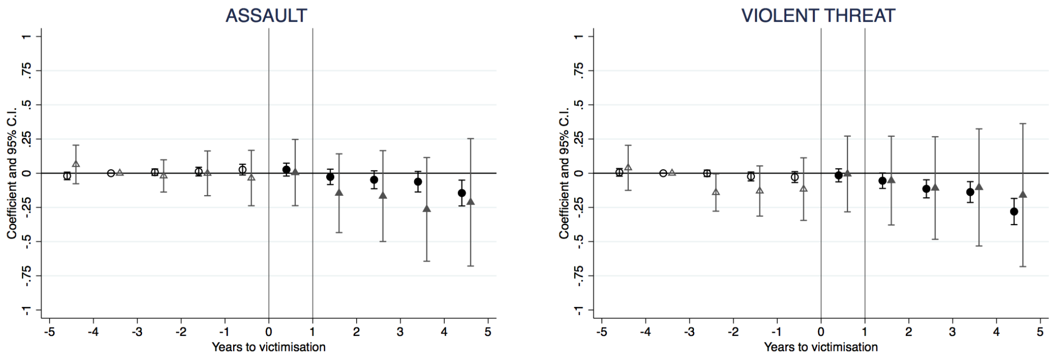
[46] Alternatively, living in a household with a partner may provide an insurance mechanism that allows for a response at a different margin (e.g. the extensive margin, to the extent that there is choice). While this may be a valid explanation for employment and/or earnings, it seems less plausible for benefits.

[47] As we mentioned before, on average 90 percent of registered suspects are convicted (Statistics Netherlands *et al.*, 2013). Note that while we do not further split the samples by gender, almost 80% of the victims with a criminal partner (following that definition) are female. Appendix Table A1 provides the respective summary statistics for the outcome variables.

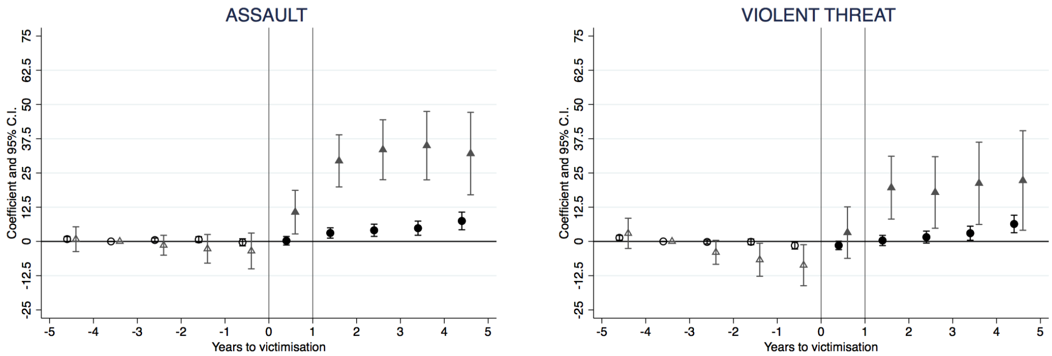
[48] We use the fact that the household member (partner) obtains a criminal record as a proxy for domestic violence, but cannot exclude alternative explanations. If household income decreases, because the partner is incarcerated, benefit dependency may increase. The patterns found for divorce and moving suggest that this is unlikely to be the

Figure 10: **Cohabiting Partner with Criminal Record**

Panel A. Log earnings



Panel B. Days of benefits



NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with log earnings (Panel A) and days of benefit receipt (Panel B) as the dependent variable for assault (left) and threat (right), separately by whether the individual cohabits with a partner with or without a criminal record. The black circles represent the results for individuals with a partner *without* a criminal record, the grey triangles for individuals with a partner *with* a criminal record. The two vertical lines mark the start and end of the victimisation year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

circles represent point estimates for victims with a non-criminal partner and the grey triangles for those with a criminal partner. Starting with log earnings in Panel A, we do not see significant differences for assault (although we lose precision to some extent) and for threat, we only find precise estimates for those whose partner is not registered for a criminal offence. Note that the magnitudes for the latter are still sizeable (-5.5%) in the first fully treated year (+1).

More striking differences are seen when we look at days of benefit receipt in Panel B: For both offences, point estimates are attenuated for the sample of victims with non-criminal partners compared to the estimates in Appendix Figure B5. But, the effects for victims with registered criminal partners are visibly larger: For assault, there is an increase in days of benefit

sole driver. Alternatively, the results could be the consequence of living with a partner with a violent inclination (i.e. exposure to a criminogenic environment), including (but not restricted to) exposure to domestic violence.

receipt by 31% relative to the mean in the year following victimisation for those with a criminal partner compared to 7% for those with a non-criminal partner. For violent threat, there are increases of 22% and 1%, respectively. Keeping in mind that the baseline effect for the benefit outcome was broadly driven by females, these results are indeed consistent with an explanation that largely attributes the negative labour market impact of violent crime victimisations to domestic violence.[49] In fact, this is also in line with the patterns seen in Figure 8: The share of females in the assault and violent threat samples who divorce and/or move in a given year increases in the year(s) of and following the victimisation relative to the years before.

Dutch Victimisation Survey

While the above strongly points towards underlying differences in the types of offences experienced by male and female victims, we are limited in what we can say about the circumstances of the offence based on the register data. Instead, we complement our analysis by descriptive statistics from the Dutch Victimisation Survey. The Dutch Victimisation Survey is a nationally representative survey of individuals aged 15 or older. It is designed as a repeated cross-section and data are available for the years 2005 to 2016. Respondents are asked whether they were victims of a specific crime category in the last five years, corresponding to the categories in the register data. If respondents answer positively to having been victimised of that crime in the last five years, questions follow about the details of the *last* incident, including the calendar date and circumstances. Depending on the type of crime (and the survey wave), these questions include whether respondents knew the offender, how they were 'related' to the offender, the location of the crime, and whether they reported the offence to the police.

We use the survey data to descriptively complement our heterogeneity analyses.[50] Table 3

[49]Indeed, we see a similar (if not stronger) pattern when we further break down the sample to victims of these violent crimes who in the year of victimisation are cohabiting with a partner who becomes a registered suspect of a *violent* crime in that or one of the following years. For sex offences, sample sizes for those living with a registered criminal partner become too small and the results are imprecisely estimated.

[50]To keep the sample from the survey comparable to the sample from the register data, we only report results for individuals aged 18-55. Appendix Table A4 reports descriptive statistics for the victims that respond to the victimisation survey. As can be seen in Panel A, there are differences between the samples (self-reported victimisation instead of registered victimisation), but the general pattern with respect to the age and gender distribution across offences is similar.

Table 3: **Offence Characteristics (Victimisation Survey)**

	Violent offences				Property offences	
Sample:	Assault	Threat	Robbery	Sex	Burglary	Pickpocketing
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Females						
Known offender (1/0)	0.59	0.53	0.08	0.40	0.07	0.04
Familiar location (1/0)	0.54	0.50	0.17	0.38	n.a.	0.27
Reported to police (1/0)	0.60	0.38	0.67	0.15	0.72	0.59
Observations	2,042	7,982	445	5,215	12,238	7,557
Panel B. Males						
Known offender (1/0)	0.35	0.35	0.07	0.32	0.09	0.02
Familiar location (1/0)	0.30	0.35	0.20	0.37	n.a.	0.30
Reported to police (1/0)	0.54	0.29	0.56	0.25	0.72	0.55
Observations	3,551	11,592	385	861	11,019	4,381

NOTE- The table shows averages of the indicated variables for each of the six offence subsamples as indicated at the top of each column. The share of missing responses by offence (in the order of the table, from left to right) are as follows: Known offender - 0.22, 0.12, 0.55, 0.14, 0.96, 0.97; familiar location - 0.22, 0.12, 0.23, 0.13, na, 0.12; reported to police - 0.50, 0.55, 0.50, 0.68, 0.54, 0.44. Note that these are for the large part driven by not all questions being asked in each survey wave. SOURCE- Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

shows the share of survey respondents (victims) who report that they knew the offender, that the crime took place at a familiar location and that they reported the offence to the police.[51] We separately show these measures for each offence category in columns (1) to (6) and by gender (Panels A and B). As before, we focus our discussion on assault and threat in columns (1) and (2): For assaults, 59% of the women report that they knew the offender, compared to only 35% for men. With respect to the location of the crime, 54% of women report a familiar location compared to 30% for men. A similar pattern emerges for threat in column (2): 53% (35%) of women (men) report knowing the offender and 50% (35%) report a familiar location. That is, based on these survey responses, women are more likely to be victimised by someone known and/or at a known place. Importantly, the share of women and men knowing the offender or reporting a familiar location is much more equal for the other four offences. These numbers suggest that there are substantial differences in the *exact* type of victimisation experienced (on average) by women and men when it comes to assault and violent threat and imply that at least

[51] We have categorised offenders and locations as follows: "Known offender" includes the categories partner, ex-partner, family, neighbour, work and other known. "Familiar location" includes the categories home, other dwelling, in a car, at work, at school, sport field/canteen, work/school, elsewhere. "Unfamiliar location" includes the categories bar/restaurant/club, on the street, on train/tram/bus stop, in public transport, in a shop, park/parking/beach. The indicator "reported to police" is based on self-reports.

some of the effect found for females is driven by domestic violence.[52]

5 Household spill-overs

So far, we have discussed the effect of criminal victimisation on the victim's own labour market outcomes. This section takes our analysis one step further and studies how these events impact non-victimised household members. Our motivation for studying such spill-overs is twofold: First, they may be an important (and yet unstudied) component of the social cost of criminal victimisation. Not taking them into account could lead to an underestimation of the extent to which individuals and families are affected. Second, studying household spill-overs overcomes a potential limitation of the individual fixed effects approach in terms of simultaneity, if one is willing to assume that one family member's behaviour is unlikely to directly cause the victimisation of another. For the scope of this paper, we limit the analysis to cohabiting partners of the victim.

Empirical strategy

To estimate spillover effects on the victim's partner, we extend the individual fixed effects approach as follows:

$$Y_{jhtal} = \gamma_0 + \gamma_{-5} \cdot V_{iht,-5} + \sum_{s=-3}^{4} \gamma_s \cdot V_{iht,s} + \alpha_j + \alpha_t + \alpha_a + \alpha_{t,a} + \alpha_l + w_{jhtal} \tag{2}$$

where *j* denotes the partner whose labour market outcomes we observe, *i* the victimised partner, and *h* the household. Otherwise, the specification follows equation (1). We exclude partners who have either been victimised themselves or have a criminal record (both leading

[52]Table 3 also shows that there are different reporting rates for the six offences. Around 57% of the individuals that self-report to be a victim of an assault say that have reported this to the police. For threat this is only 33%. Robbery, burglary and pickpocketing have higher reporting rates (62%, 72% and 58%, respectively), while reporting rates of sex offences are by far the lowest (16%). We can further restrict the sample from the survey to those who answered that they reported the offence to the police to mirror the sample in our main analysis. For assault, 62% (38%) of the female (male) respondents report that they knew the offender and 51% (36%) reported a familiar location in the assault sample. For violent threat, the shares amount to 66% (54%) and 60% (49%), respectively.

up to and after) the victimisation. The latter restriction is necessary to not confound a potential spillover effect of the partner's victimisation with labour market consequences of offending. All other sample restrictions mirror the restrictions of our baseline sample.[53]

Spillover effects on partners

Figure 11 shows the results when we estimate spillover effects on cohabiting partners, again both for assault and threat. Panel A and B show the results for the two main outcomes, earnings and days of benefit receipt. For assault, we see no evidence of spillover effects on the partner of a victim: The point estimates are close to and not significantly different from zero. For violent threat, however, we find that earnings decrease by 7.3% following the victimisation of the partner. The point estimate is actually larger than the respective point estimate for the victim (a 5.4% decrease in earnings in that sample). There is also a small but insignificant increase in benefits for the partners of victims of violent threat.[54]

We do not take a strong stance on the mechanisms leading to such spillovers and leave that to future research. There are plausible candidates though that could explain such patterns, such as the need to stay at home to care for a sick or injured partner, joint moving decisions or disrupted families as a consequence of the victimisation and caused stress.[55] The bottom line of our analysis is that, for some offences, partners of victims are negatively affected by the victimisation when it comes to labour market outcomes. This has two implications: First, in order to understand the social cost of crime, one has to take into account these spillover effects (and learn more about the mechanisms). Second, finding such negative impact on partners suggests that living with a partner does not generally provide an insurance mechanism or safety net for a victim of crime. If that was the case, one would have expected to see no change or an

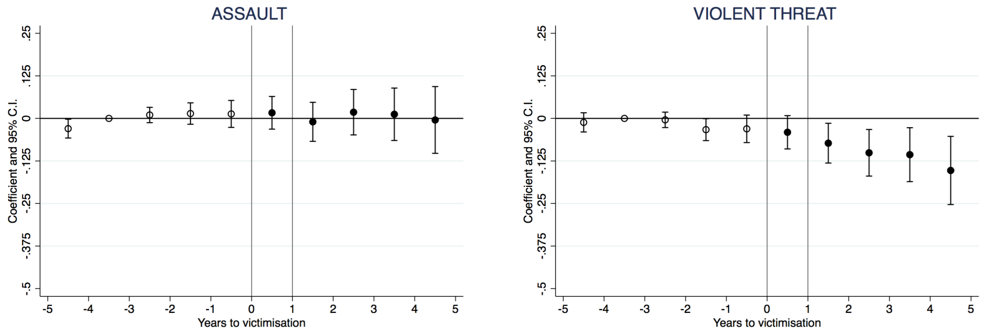
[53] Table A5 in the Appendix provides descriptive statistics for the cohabiting partners of victims.

[54] We cannot conduct the same analysis for all offences, in particular property offences. For instance, burglary is an offence at the household rather than at the individual level and we cannot empirically distinguish a spill-over from a victimisation effect itself. The results (available on request) for robbery and sex offences are imprecisely estimated.

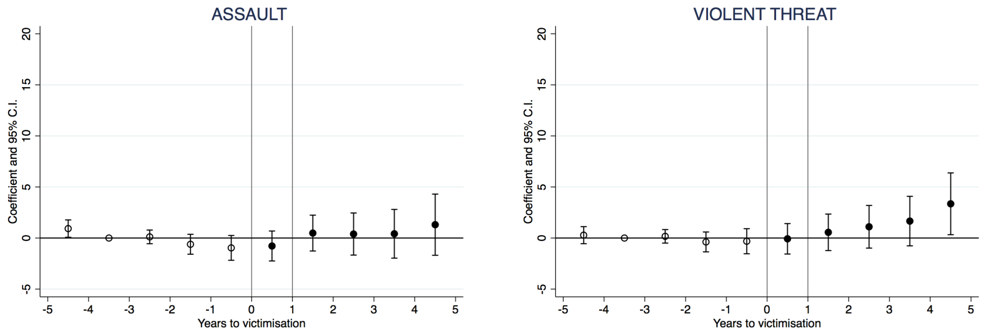
[55] Looking at spillover effects on health expenditure for the partners, we find point estimates close to and not significantly different from zero. When we restrict the samples to households in which the *victim* does not obtain a criminal record at any point after the year of victimisation, the point estimates regarding the spillover effects of violent threat are attenuated (but not zero). We do, however, lose precision with further sample restrictions, which limits our ability to draw robust conclusions.

Figure 11: **Spill-over Effects on Non-Criminal Partners**

Panel A. Log earnings



Panel B. Days of benefits



NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (2), where the household member is the (non-criminal) cohabiting partner of the victim. Panel A shows the results for log earning, Panel B for days of benefits, for assault (left) and threat (right). The two vertical lines mark the start and end of the victimisation year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

increase (decrease) in earnings (benefits) - which is clearly not the case.

6 Discussion and conclusion

What are the effects of criminal victimisation on individuals' labour market outcomes? Using detailed longitudinal register data from the Netherlands, we estimate event-study designs to evaluate the impact of criminal victimisation on earnings and benefit dependency. Our main results for assault and violent threat show that criminal victimisation leads to statistically and economically significant losses in earnings (6.6-9.3%) and increases in benefit dependency (10.4-14.7%) that persist over time (up to five years and longer). We put forward an explanation of victimisation as an escalation point triggering additional life events that may contribute to these lasting effects. For property crimes, such escalation point is less visible in the results and we cannot make firm conclusions for robbery and sex offences due to imprecise estimation results. The magnitude and precision of our estimates differ by gender, suggesting (i) differential responses to victimisation and/or (ii) underlying heterogeneity in offence characteristics and severity. The results from our heterogeneity analyses, complemented by descriptive evidence from the Dutch Victimisation Survey, suggest that for assault and violent threat, domestic violence largely (but not completely) drives the results for females with respect to benefit dependency but not earnings. Lastly, we find some evidence that partners of victims are negatively affected in terms of labour market (but not health) outcomes, albeit not for all offences.

How do our results compare to findings in the literature? Focusing on assault (as it is most comparable with existing estimates), we find a decrease in earnings (labour income) by 9.3% and an increase in the number of days of benefit receipt by 14.7% in the year after victimisation. As stressed before, evidence on the causal impacts of criminal victimisation is scarce. To date, the closest study to ours is that by Ornstein (2017). She finds that earnings for female assault victims (identified by hospitalisation records) decrease on average by 25% and for male assault victims by 14%, paralleled by a larger increase in sick leave uptake by women (31 days annually) compared to men (15 days annually). Our estimates of losses in labour income are smaller, but also based on a less selected sample of assault victims. Looking at another traumatic life event, van den Berg *et al.* (2017) report a 12.5% (8.8%) average annual loss in earnings for mothers (fathers) after the death of a child. The bottom line of these comparisons is that our estimates lie within the range of the most comparable estimates.[56]

Based on our estimates and using a simple back-of-the-envelope calculation, the average losses in labour income accumulated over the first four years following an assault (violent threat) amount to 41% (32%) of an average annual labour income in the respective sample. This

[56]How do our findings compare to those reported in studies of labour market consequences for offenders? Grogger (1995) reports a 4% immediate and short-term decrease in earnings following arrests for the U.S., while Western *et al.* (2001) in their review of the literature record earnings penalties from imprisonment ranging from 10% to 30% with no statistical effects on employment rates. Yet, the population of criminals is highly selected and not necessarily a good comparison group to our sample of (non-criminal) victims. Note that other literatures find similarly persistent labour market effects of adverse events. For instance, the labour economics literature on the effects of job displacement reports decreases in earnings lasting up to 12 years after displacement in the case of Sweden (Eliason and Storrie, 2006) and amounting to a loss in earnings of about 3% for displaced relative to non-displaced workers seven years after displacement in the case of Norway (Huttunen *et al.*, 2011).

average loss only in earnings compares to about 10% of the average cost estimate of a serious assault in the U.S. in 2007 as reported in Heaton (2010). Given the sample restrictions needed for our analysis, it is hard to provide a clear-cut number for the aggregate labour market cost of criminal victimisation. Again using a simple back-of-the-envelope calculation, our results would yield an accumulated loss just in earnings of about 350.7 million Euros per year for assault (196.4 million for threat). In comparison, in 2012 the total Dutch expenditure on public and private safety (including preventention, policing, criminal justice and support of offenders and victims) amounted to 13.1 billion Euros. Of this, only 50.1 million Euros was specifically aimed at supporting victims. In the same year, victims (of all offences) received 34.5 million Euros in compensation from offenders (Statistics Netherlands *et al.*, 2013).

Our findings of persistent labour market cost of criminal victimisation have important policy implications. First, they speak to the ongoing debate concerning the cost of crime: What is the social cost of crime? Second, they speak to the non-trivial question of suitable compensation for victims: Are there labour market cost and should they be taken into account? While this ultimately depends on the policy aim, agents of the criminal justice system (e.g. judges or juries) are often challenged to award an appropriate compensation amount to the victim and having guidelines for these amounts is valuable, as discussed in Johnston *et al.* (2018). Naturally, given the scarcity of empirical evidence on the topic, more research will be needed to robustly inform the policy debate on questions regarding criminal victimisation and labour market outcomes. Our results suggest that there are labour market cost of criminal victimisation that should be taken into account, if the policy aim is to fully compensate the victim for any losses.

References

AIZER, A. and DOYLE, J. J. (2015). Juvenile incarceration, human capital, and future crime: Evidence from randomly assigned judges. *The Quarterly Journal of Economics*, **130** (2), 759–803.

ANGRIST, J. D. and PISCHKE, J.-S. (2009). *Mostly harmless econometrics*. Princenton University Press.

BECKER, G. (1968). Crime and punishment: An economic approach. *Journal of Political Economy*, **76** (2), 169–217.

BORUSYAK, K. and JARAVEL, X. (2017). Revisiting event study designs, with an application to the estimation of the marginal propensity to consume, mimeo.

BRAAKMANN, N. (2012). How do individuals deal with victimization and victimization risk? Longitudinal evidence from Mexico. *Journal of Economic Behavior & Organizaton*, **84**, 335–344.

CHALFIN, A. (2015). The economic cost of crime. In W. Jennings (ed.), *The Encyclopedia of Crime and Punishment*, Malden, MA: Wiley-Blackwell, pp. 1–12.

— and MCCRARY, J. (2017). Criminal deterrence: A review of the literature. *Journal of Economic Literature*, **55** (1), 5–48.

COHEN, L. E. and FELSON, M. (1979). Social change and crime rate trends: A routine activity approach. *American Sociological Review*, **44**, 588–608.

COHEN, M. A. (2008). The effect of crime on life satisfaction. *The Journal of Legal Studies*, **37** (S2), 325–353.

CORNAGLIA, F., FELDMAN, N. E. and LEIGH, A. (2014). Crime and mental well-being. *Journal of Human Resources*, **49** (1), 110–140.

CURRIE, J., MUELLER-SMITH, M. and ROSSIN-SLATER, M. (2018). *Violence while in utero: The impacts of assaults during pregnancy on birth outcomes*. NBER Working Paper 24802.

DOBKIN, C., FINKELSTEIN, A., KLUENDER, R. and NOTOWIDIGDO, M. J. (2018). The economic consequences of hospital admissions. *American Economic Review*, **108** (2), 308–352.

DRACA, M. and MACHIN, S. (2015). Crime and economic incentives. *Annual Review of Economics*, **7**, 389–408.

DUGAN, L. (1999). The effect of criminal victimisation on a household's moving decision. *Criminology*, **37** (4), 903–930.

DUSTMANN, C. and FASANI, F. (2016). The effect of local area crime on mental health. *The Economic Journal*, **126**, 978–1017.

ELIASON, M. and STORRIE, D. (2006). Lasting or latent scars? Swedish evidence on the long-term effects of job displacement. *Journal of Labor Economics*, **24** (4), 831–856.

GRANGER, C. W. (1969). Investigating causal relations by econometric models and cross-spectral methods. *Econometrica*, **37**, 424–438.

GROGGER, J. (1995). The effect of arrests on the employment and earnings of young men. *The Quarterly Journal of Economics*, **110** (1), 51–71.

HAMERMESH, D. S. (1999). Crime and the timing of work. *Journal of Urban Economics*, **45** (2), 311–330.

HEATON, P. (2010). *Hidden in plain sight: What cost-of-crime research can tell us about investing in police*. Occasional Paper, RAND.

HINDELANG, M. S., GOTTFREDSON, M. and GAROFALO, J. (1978). *Victims of personal crime*. Cambridge, MA: Ballinger.

HUTTUNEN, K., MOEN, J. and SALVANES, K. G. (2011). How destructive is creative destruction? Effects of job loss on job mobility, withdrawal and income. *Journal of the European Economic Association*, **9** (5), 840–870.

JANKE, K., PROPPER, C. and SHIELDS, M. A. (2016). Assaults, murders and walkers: The impact of violent crime on physical activity. *Journal of Health Economics*, **47**, 34–49.

JOHNSTON, D. W., SHIELDS, M. A. and SUZIEDELYTE, A. (2018). Victimisation, well-being and compensation: Using panel data to estimate the cost of violent crime. *The Economic Journal*, **128** (611), 1545–1569.

KLING, J. (2006). Incarceration length, employment and earnings. *American Economic Review*, **96** (3), 863–876.

LEVITT, S. D. (1999). The changing relationship between income and crime victimization. *Economic Policy Review*, **5** (3), 87–98.

MIETHE, T. D. and MEIER, R. F. (1990). Opportunity, choice, and criminal victimization: A test of a theoretical model. *Journal of Research in Crime and Delinquency*, **27** (3), 243–266.

—, STAFFORD, M. C. and LONG, J. S. (1987). Social differentiation in criminal victimization: A test of routine activities/lifestyle theories. *American Sociological Review*, **52** (2), 184–194.

MUELLER-SMITH, M. (2016). The criminal and labor market impacts of incarceration, mimeo.

NAGIN, D. S. (2013). Deterrence in the 21st century: A review of the evidence. In M. Tonry (ed.), *Crime and justice: An annual review of research*, Chicago: University of Chicago Press.

ORNSTEIN, P. (2017). *The price of violence: Consequences of violent crime in Sweden*. Working Paper 2017:22, IFAU.

SALM, M. and VOLLAARD, B. (2016). The only way is up: Time of exposure and perception of local crime risk, mimeo.

SOARES, R. R. (2015). Welfare cost of crime and common violence. *Journal of Economic Studies*, **42** (1), 117–137.

STATISTICS NETHERLANDS, WODC and RAAD VOOR DE RECHTSPRAAK (2013). *Criminaliteit en rechtshandhaving 2013*. Report, Justitie in Statistiek.

VAN DEN BERG, G., LUNDBORG, P. and VIKSTROM, J. (2017). The economics of grief. *The Economic Journal*, **127** (604), 1794–1832.

VELAMURI, M. and STILLMAN, S. (2008). The impact of crime victimisation on individual well-being: Evidence from australia. In P. S. Morrison (ed.), *Proceedings of the Joint LEW13/ALMRW Conference*, Victoria University of Wellington, pp. 583–95.

WESTERN, B., KLING, J. R. and WEIMAN, D. F. (2001). The labor market consequences of incarceration. *Crime & Delinquency*, **47** (3), 410–427.

A Appendix Tables

Table A1: **Summary Statistics by Subgroup**

	Violent offences				Property offences	
Sample:	Assault	Threat	Robbery	Sex	Burglary	Pickpocketing
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Ages 26-55						
Earnings (in 2015 €)	24,067	28,165	24,789	20,146	38,394	27,704
Days benefits	94.1	82.4	87.6	100.5	48.8	59.7
Panel B. Females						
Earnings (in 2015 €)	12,989	16,459	17,095	13,014	21,920	18,032
Days benefits	101.2	92.4	66.8	86.1	56.1	50.9
Panel C. Males						
Earnings (in 2015 €)	25,259	32,877	18,811	29,558	45,396	32,342
Days benefits	55.3	56.9	60.6	77.3	36.7	38.0
Panel D. Cohabiting with (non-victimised) partner						
Earnings (in 2015 €)	30,829	34,210	28,212	22,466	42,630	28,661
Days benefits	50.0	44.0	48.6	46.4	28.7	34.9
Panel E. Cohabiting with (non-victimised) criminal partner						
Earnings (in 2015 €)	16,588	18,772	18,734	13,767	27,783	20,862
Days benefits	95.5	88.6	70.1	103.0	58.5	60.7

NOTE- The table shows averages of the indicated variables for each of the six offence subsamples as indicated at the top of each column. Each panel reports the averages of the two main (longitudinal) yearly labour market outcomes for the subgroup indicated at the top of the panel. SOURCE- Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

Table A2: **Specification Tests - Log Earnings**

Specification:	OLS	+ year x age f.e.	+ municipality f.e.	+ individual f.e.	OLS + ind. ctrls.	+ vict. year x age	+ lagged outc.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Assault							
-5	0.093(0.009)***	0.176(0.013)***	0.184(0.013)***	0.010(0.009)	0.181(0.012)***	0.029(0.009)***	0.007(0.009)
-3	-0.027(0.007)***	-0.038(0.008)***	-0.040(0.008)***	-0.000(0.007)	-0.046(0.008)***	-0.006(0.008)	0.009(0.009)
-2	-0.046(0.009)***	-0.080(0.010)***	-0.084(0.010)***	-0.006(0.010)	-0.097(0.010)***	-0.015(0.010)	-0.001(0.010)
-1	-0.056(0.010)***	-0.119(0.013)***	-0.125(0.013)***	-0.007(0.012)	-0.152(0.012)***	-0.026(0.013)**	0.005(0.010)
0	-0.097(0.010)***	-0.174(0.015)***	-0.184(0.015)***	-0.022(0.014)	-0.221(0.014)***	-0.053(0.015)***	-0.009(0.010)
+1	-0.211(0.011)***	-0.276(0.017)***	-0.289(0.017)***	-0.093(0.017)***	-0.338(0.016)***	-0.128(0.018)***	-0.074(0.010)***
+2	-0.286(0.012)***	-0.316(0.019)***	-0.334(0.019)***	-0.139(0.019)***	-0.400(0.019)***	-0.142(0.020)***	-0.066(0.011)***
+3	-0.314(0.012)***	-0.323(0.022)***	-0.347(0.022)***	-0.156(0.022)***	-0.430(0.021)***	-0.132(0.023)***	-0.043(0.012)***
+4	-0.303(0.013)***	-0.345(0.027)***	-0.389(0.027)***	-0.195(0.028)***	-0.523(0.026)***	-0.095(0.029)***	-0.031(0.013)**
Panel B. Violent threat							
-5	0.043(0.010)***	0.255(0.015)***	0.251(0.014)***	0.013(0.010)	0.214(0.014)***	0.033(0.011)***	0.008(0.009)
-3	-0.014(0.008)*	-0.070(0.009)***	-0.068(0.009)***	-0.004(0.008)	-0.057(0.009)***	-0.009(0.009)	0.010(0.010)
-2	-0.022(0.010)**	-0.135(0.012)***	-0.130(0.012)***	0.005(0.011)	-0.109(0.012)***	-0.009(0.012)	0.020(0.011)*
-1	-0.040(0.011)***	-0.210(0.015)***	-0.202(0.015)***	0.012(0.014)	-0.171(0.015)***	-0.017(0.015)	0.025(0.011)**
0	-0.086(0.012)***	-0.294(0.018)***	-0.285(0.018)***	0.010(0.016)	-0.246(0.017)***	-0.037(0.018)**	0.024(0.011)**
+1	-0.219(0.013)***	-0.438(0.021)***	-0.428(0.021)***	-0.066(0.020)***	-0.387(0.020)***	-0.128(0.021)***	-0.046(0.012)***
+2	-0.328(0.014)***	-0.536(0.025)***	-0.528(0.024)***	-0.119(0.023)***	-0.482(0.024)***	-0.169(0.025)***	-0.045(0.013)***
+3	-0.398(0.015)***	-0.596(0.028)***	-0.589(0.028)***	-0.140(0.026)***	-0.541(0.027)***	-0.175(0.028)***	-0.025(0.014)*
+4	-0.613(0.017)***	-0.845(0.036)***	-0.843(0.036)***	-0.263(0.033)***	-0.791(0.034)***	-0.246(0.036)***	-0.039(0.014)***
Year, age, year x age FE	-	✓	✓	✓	✓	✓	✓
Municipality FE	-	-	✓	✓	✓	✓	✓
Ind. FE	-	-	-	✓	-	-	-
Ind. controls	-	-	-	-	✓	✓	✓
Vict. year x age FE	-	-	-	-	-	✓	✓
Lagged (2x) outcomes	-	-	-	-	-	-	✓

NOTE - The table shows the estimated coefficients and standard errors for the regressions corresponding to equation (1) with log earnings as the dependent variable using different specifications as indicated. Standard errors are clustered by individual. *** p<0.01, ** p<0.05, * p<0.1. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

Table A3: **Specification Tests - Days of Benefits**

Specification:	OLS	+ year x age f.e.	+ municipality f.e.	+ individual f.e.	OLS + ind. ctrls.	+ vict. year x age	+ lagged outc.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Assault							
-5	0.670(0.306)**	-7.110(0.429)***	-7.298(0.425)***	-1.214(0.291)***	-7.181(0.419)***	-1.115(0.309)***	-0.595(0.264)**
-3	-0.415(0.215)*	1.877(0.232)***	1.920(0.232)***	0.713(0.227)***	2.035(0.232)***	0.423(0.234)*	0.019(0.293)
-2	-0.635(0.283)**	4.064(0.334)***	4.160(0.333)***	1.722(0.326)***	4.430(0.332)***	1.137(0.339)***	0.359(0.311)
-1	-0.552(0.314)*	6.570(0.413)***	6.720(0.412)***	2.987(0.403)***	7.261(0.409)***	2.244(0.423)***	0.725(0.304)**
0	2.911(0.340)***	11.915(0.492)***	12.155(0.490)***	6.920(0.484)***	12.921(0.486)***	6.183(0.509)***	3.554(0.318)***
+1	7.906(0.373)***	17.967(0.576)***	18.259(0.574)***	11.382(0.576)***	19.283(0.568)***	10.919(0.605)***	5.076(0.337)***
+2	10.929(0.400)***	20.428(0.657)***	20.833(0.654)***	12.675(0.670)***	22.206(0.646)***	12.022(0.703)***	2.139(0.350)***
+3	13.066(0.425)***	21.824(0.740)***	22.388(0.736)***	12.991(0.768)***	24.124(0.725)***	12.229(0.802)***	1.295(0.374)***
+4	19.906(0.464)***	24.958(0.933)***	26.065(0.925)***	13.211(0.955)***	28.946(0.906)***	11.814(0.997)***	0.951(0.409)**
Panel B. Violent threat							
-5	-3.390(0.336)***	-9.710(0.488)***	-9.456(0.484)***	-0.752(0.328)**	-8.604(0.477)***	-1.544(0.349)***	-0.178(0.292)
-3	1.323(0.247)***	3.106(0.270)***	2.984(0.270)***	0.636(0.260)**	2.748(0.269)***	0.882(0.270)***	0.212(0.328)
-2	2.725(0.326)***	6.278(0.397)***	6.038(0.396)***	1.163(0.374)***	5.562(0.394)***	1.705(0.395)***	0.096(0.345)
-1	4.660(0.368)***	9.815(0.503)***	9.429(0.502)***	1.686(0.468)***	8.733(0.498)***	2.807(0.501)***	0.043(0.345)
0	8.759(0.402)***	15.204(0.614)***	14.729(0.611)***	4.196(0.567)***	13.844(0.605)***	5.828(0.610)***	1.805(0.364)***
+1	14.244(0.447)***	21.304(0.734)***	20.783(0.731)***	7.797(0.676)***	19.813(0.723)***	9.869(0.735)***	3.399(0.385)***
+2	17.678(0.488)***	24.378(0.853)***	23.865(0.848)***	8.541(0.788)***	22.820(0.838)***	10.732(0.863)***	0.924(0.410)**
+3	20.571(0.532)***	26.935(0.976)***	26.431(0.970)***	8.876(0.909)***	25.346(0.957)***	11.228(0.998)***	0.784(0.444)*
+4	29.814(0.615)***	35.409(1.266)***	34.981(1.255)***	10.998(1.135)***	33.829(1.233)***	13.139(1.273)***	0.770(0.472)
Year, age, year x age FE	-	✓	✓	✓	✓	✓	✓
Municipality FE	-	-	✓	✓	✓	✓	✓
Ind. FE	-	-	-	✓	-	-	-
Ind. controls	-	-	-	-	✓	✓	✓
Vict. year x age FE	-	-	-	-	-	✓	✓
Lagged (2x) outcomes	-	-	-	-	-	-	✓

NOTE - The table shows the estimated coefficients and standard errors for the regressions corresponding to equation (1) with days of benefit receipt as the dependent variable using different specifications as indicated. Standard errors are clustered by individual. *** p<0.01, ** p<0.05, * p<0.1. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

Table A4: **Sample Composition - Dutch Victimisation Survey**

	Violent offences				Property offences	
Sample:	Assault	Threat	Robbery	Sex	Burglary	Pickpocketing
	(1)	(2)	(3)	(4)	(5)	(6)
Background characteristics (in the year of victimisation)						
Female	0.37	0.41	0.54	0.86	0.53	0.63
Age	33.9	37.1	35.3	33.6	38.4	36.6
Immigrant	0.10	0.09	0.24	0.12	0.16	0.14
Partner (0/1)	0.48	0.60	0.47	0.47	0.67	0.57
Children (0/1)	0.34	0.43	0.36	0.33	0.51	0.43
Observations	5,593	19,574	830	6,076	23,257	11,938

NOTE- The table shows averages of the indicated variables for each of the six offence subsamples as indicated at the top of each column. SOURCE- Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

Table A5: **Summary Statistics - Non Criminal Cohabiting Partners**

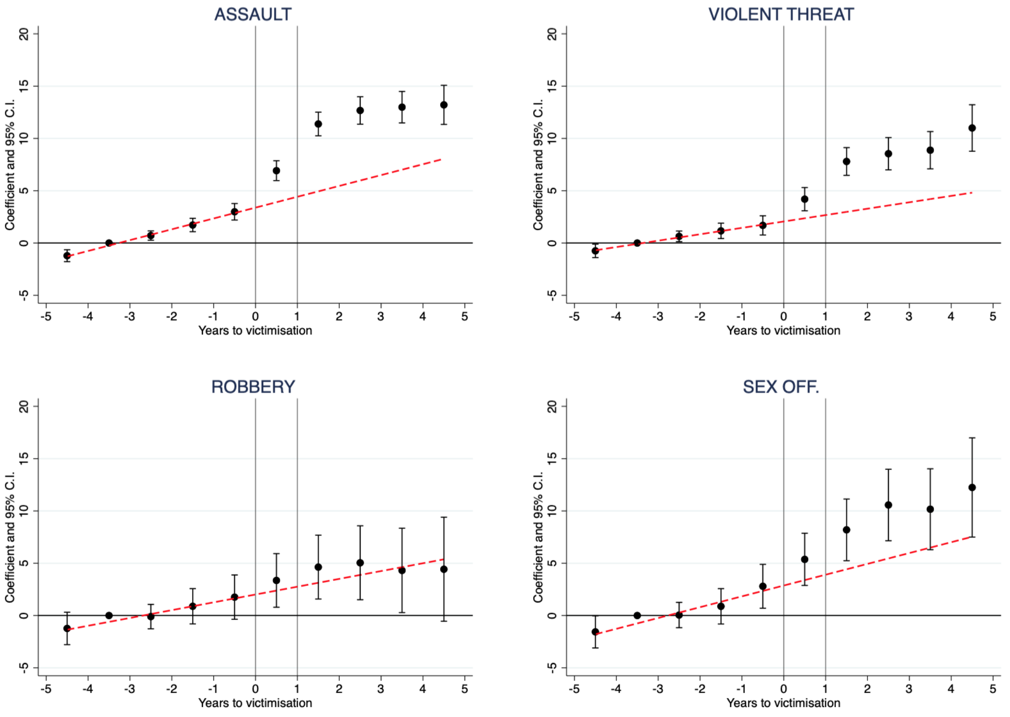
	Violent offences				Property offences	
Sample:	Assault	Threat	Robbery	Sex	Burglary	Pickpocketing
	(1)	(2)	(3)	(4)	(5)	(6)
Earnings (in 2015 €)	24,794	25,920	31,909	37,843	31,957	39,470
Days on benefits	43.3	39.7	42.1	29.3	28.6	30.8
Obs. (NxT)	614,686	592,266	69,629	95,496	2,476,498	713,233

NOTE- The table shows averages of two main (longitudinal) yearly labour market outcomes for each of the six offence subsamples as indicated at the top of each column. SOURCE- Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

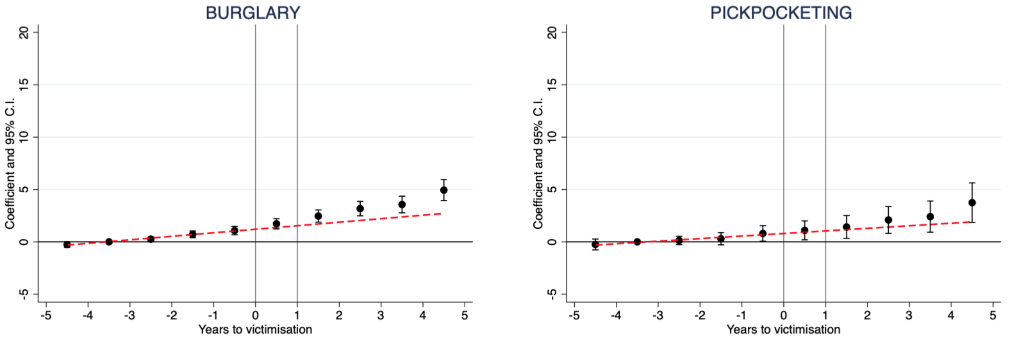
B Appendix Figures

Figure B1: **Estimated Continuation of Pre-Trend (Days of Benefits)**

Panel A. Violent offences



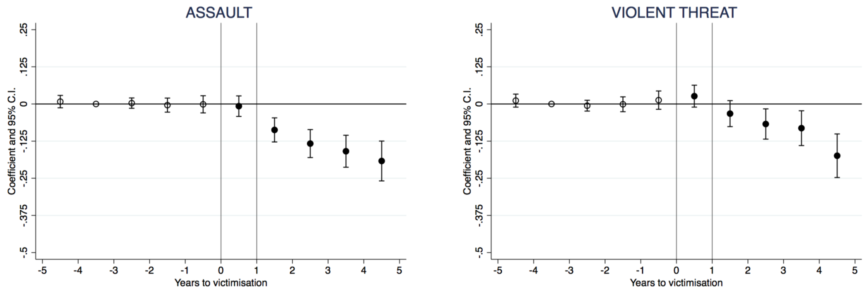
Panel B. Property offences



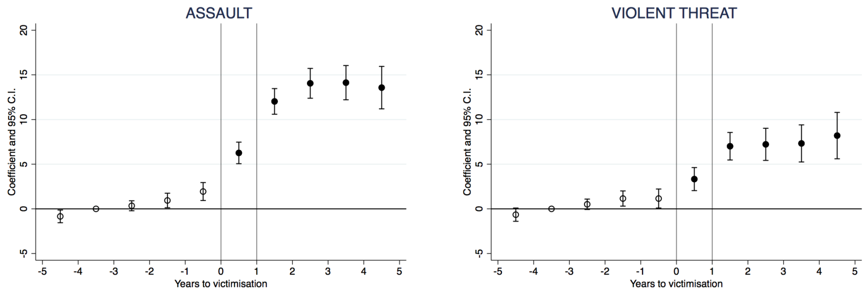
NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with days of benefit receipt as the dependent variable for violent offences (Panel A) and property offences (Panel B). The dashed red lines mark the estimated continuation of the pre-trend, estimated by fitting a linear trend through the four pre-victimisation point estimates. The two vertical lines mark the start and end of the victimisation year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

Figure B2: **Baseline Results for Ages 26 to 55**

Panel A. Log earnings



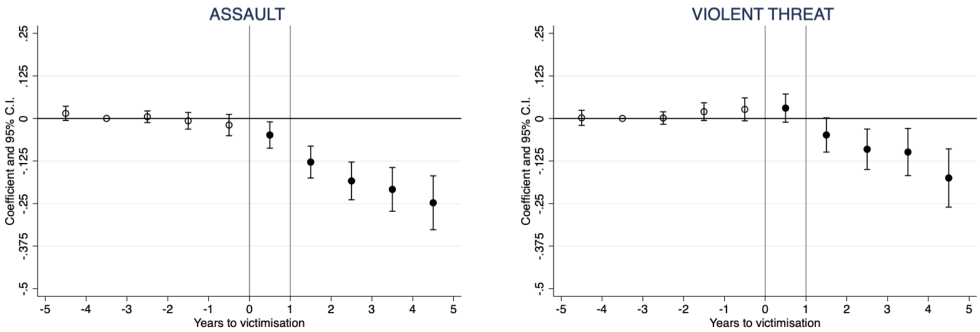
Panel B. Days of benefits



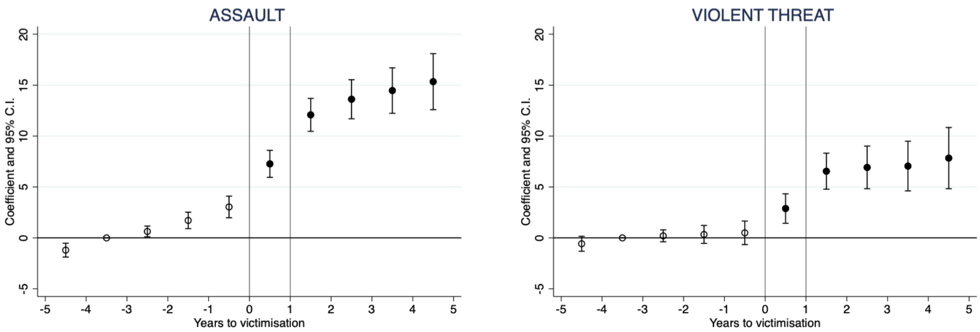
NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with log(earnings) as the dependent variable in Panel A and days of benefit receipt in Panel B. The figures to the left show results for assault, those to the right for violent threat. The sample is restricted to individuals aged 26 to 55. The two vertical lines mark the start and end of the victimisation year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

Figure B3: **No (Registered) Victimisation in Previous Four Years**

Panel A. Log earnings



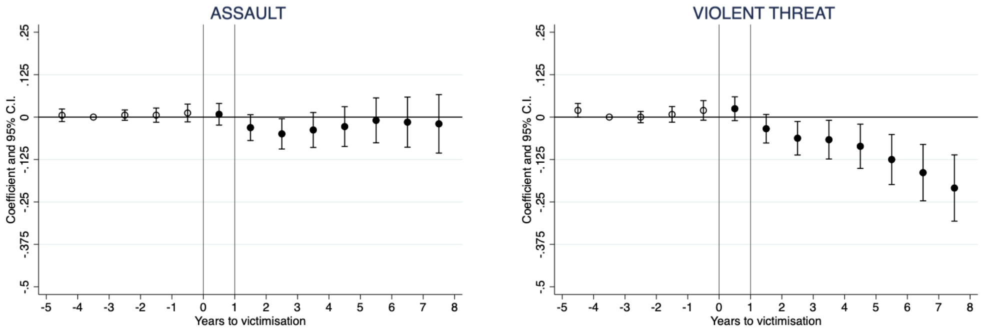
Panel B. Days of benefits



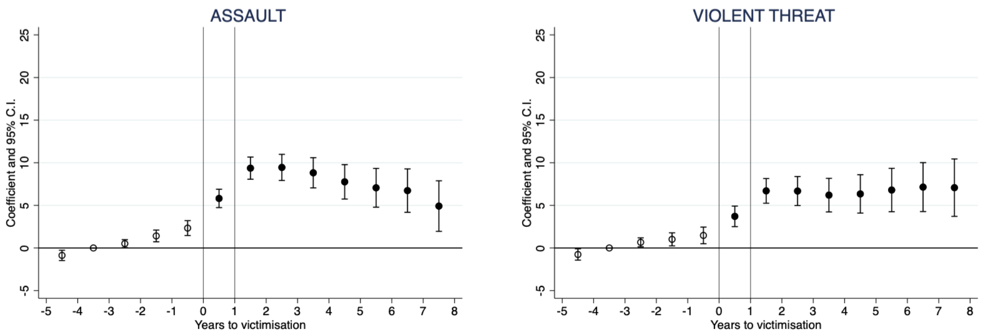
NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with log(earnings) as the dependent variable in Panel A and days of benefit receipt in Panel B. The figures to the left show results for assault, those to the right for violent threat. The sample is restricted to victimisations starting in 2009. The two vertical lines mark the start and end of the victimisation year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

Figure B4: **Long-Term Effects**

Panel A. Log earnings



Panel B. Days of benefits

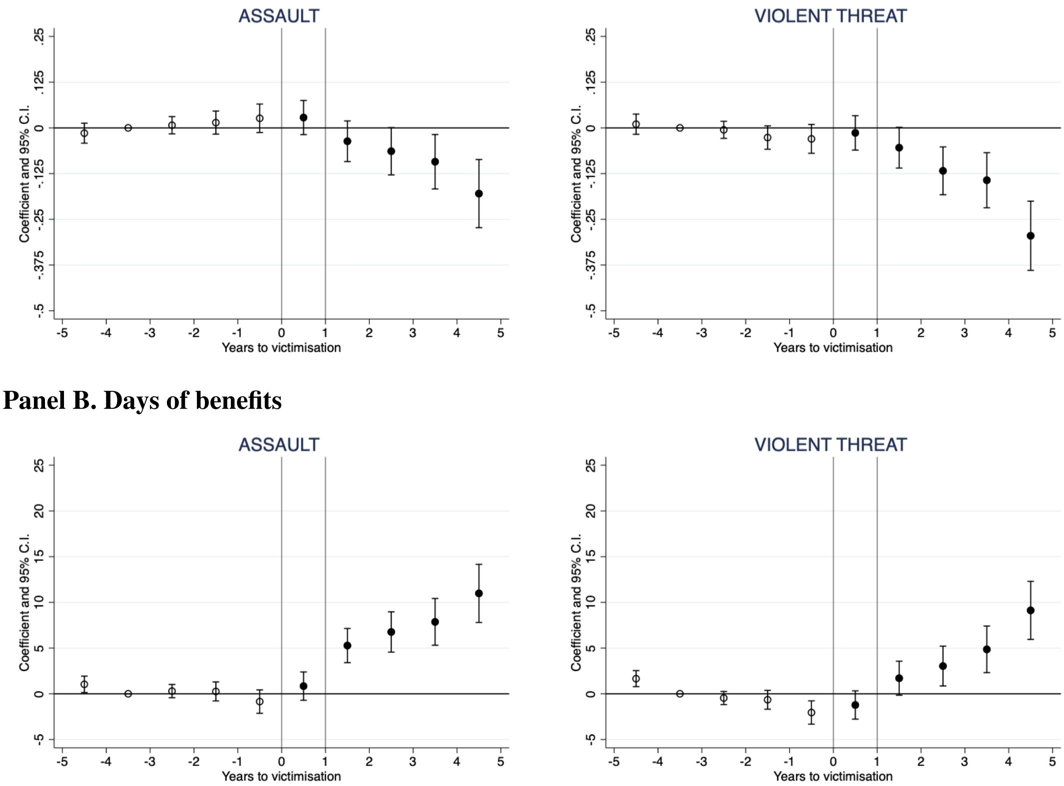


NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with log(earnings) as the dependent variable in Panel A and days of benefit receipt in Panel B. The figures to the left show results for assault, those to the right for violent threat. The sample is restricted to not having a criminal record, controlling for contemporaneous victimisations and allowing for longer-term effects. The two vertical lines mark the start and end of the victimisation year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.

Figure B5: **Victims with Cohabiting Partner**

Panel A. Log earnings

Panel B. Days of benefits



NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with log earnings (Panel A) and days of benefit receipt (Panel B) as the dependent variable for assault (left) and threat (right), restricted to individuals who live with a (non-victimised) partner. The two vertical lines mark the start and end of the victimisation year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using non-public microdata from Statistics Netherlands.