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Scaring or Scarring? Labour Market Effects of Criminal Victimisation

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Abstract

Little is known about the costs of crime to victims. We use unique and detailed register data on victimisations and monthly labour market outcomes from the Netherlands and estimate event-study designs to assess short- and long-term effects of criminal victimisation. Across offences, both males and females experience significant decreases in earnings (up to -12.9%) and increases in benefit receipt (up to +6%) after victimisation. The negative labour market responses are lasting (up to four years) and accompanied by shorter-lived responses in health expenditure. Additional analyses suggest that the victimisation is a life-changing event leading to escalation points in victims' lives.

JEL-codes: K4; J01; J12; I1
Keywords: Crime; victimisation; labour market outcomes; event-study design

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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, occurring both through the offender and the victim, are more 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? One important reason is the 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. In addition, 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 three 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 some light on potential mechanisms by considering additional health related outcomes, heterogeneities by gender and offence characteristics as well as other life-events after victimisation. 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 register data. Further, we are able to study potential spill-overs by linking individuals to their respective household members. Finally, when the offender is known to the police, we observe

[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).

whether the victim and the offender live in the same household which allows us to separate out domestic violence cases. This is, to our knowledge, the first study that uses victimisation register data to study these questions.

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 more 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 victims and 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 stronger effects 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.* (forthcoming) use data on crime incidents (assaults) geocoded at the building level to proxy for violent assaults during pregnancy of mothers living in that building. They report evidence of lower birth weights and a higher likelihood of pre-term births.[2]

We are only aware of two studies that estimate the effect of victimisation on labour market outcomes. Ornstein (2017) uses Swedish register data to study the effect of an assault on mortality, health and labour market outcomes. However, victimisation can only be 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]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 strong link between the two events for both violent and property offences. In the Dutch context, Salm and Vollaard (2019) 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. For a more detailed survey of the literature, see Bindler *et al.* (2020).

2,490 observations are for victims of violent and property crime, respectively.

Our main contribution is to credibly assess the impact of criminal victimisation on labour market and health outcomes using large panel data that allow us to differentiate between short- and long-term effects. We address the 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 gender and type of offence. One key advantage of our data is that the samples are large enough to allow for such a restriction without compromising statistical power. Second, unobservable characteristics may correlate both with the outcome and the probability of becoming a victim. To avoid resulting omitted variable bias, we exploit the long panel of labour market outcomes and estimate event-study designs with individual fixed effects controlling for any time-invariant individual traits. This approach is similar 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 and Kleven *et al.* (2019) who study the child penalty in earnings using event-study designs, respectively. Third, labour market outcomes may impact the chance of victimisation at the same time that victimisation affects labour market outcomes. We address this simultaneity concern by explicitly studying the timing of potential effects to identify sharp changes in labour market trajectories at the time of victimisation. Our labour market outcomes are available at the monthly level allowing us to zoom in close to the victimisation date. Finally, we look at correlated shocks (moving, divorce, child birth) to assess potential confounders.

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 both men and women. One year after a violent crime victimisation (assault, threat of violence, sex offences), earnings decrease by up to 7.5% for males and 10.4% for females, while days of benefit receipt increase by up to 5% for males and 6% for females. For domestic viol-

[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).

ence offences (females), we find larger point estimates with up to 17.9% decreases in earnings and 41.7% increases in days of benefit receipt. For property crimes (violent and non-violent burglary, robbery and pickpocketing), earnings decrease by up to 8.4% for males and up to 12.9% for females, while days of benefit receipt increase by up to 2.7% for males and 4.3% for females. For all offences but pickpocketing (for which we do not find any labour market impacts), we see sharp changes at the time of victimisation. Furthermore, for most offences the labour market outcomes do not return to pre-victimisation levels within the observation window (four years). The labour market responses, in particular for violent offences, are in many cases accompanied by short-term increases in health expenditure.

Next, we turn to the question of why the labour market effects are lasting. In addition to possible path dependency in labour market outcomes, we put forward an explanation which interprets the victimisation as a life-changing event - an escalation point - triggering other life-events. We study multiple (later) victimisations by looking at individuals with only one observed victimisation and by flexibly controlling for later victimisations. To address the possibility of a victim-offender overlap we restrict the sample to individuals without a criminal record in the years after the victimisation. In both cases the point estimates for earnings and days of benefits are attenuated, suggesting that both later victimisations and later criminal involvement contribute to the persistent effects seen for these outcomes. Further, we consider moving, divorce and child birth as three other life-events (correlated shocks) that may affect labour market outcomes. The fact that we do not see large changes before or exactly at the time of victimisation (with the notable exception of domestic violence) is reassuring in terms of potential omitted variables. Based on these results, we argue that the reported victimisation constitutes a sharp escalation point in a person's life, triggering other life-events as well as a trajectory of more victimisations and criminal involvement for individuals at the margin. We draw a parallel to the crime literature discussing offending (in particular obtaining a criminal record) as a life-changing event. Falsification tests, which randomly allocate victimisation months, suggest that our findings are not driven by a spurious relationship between the month of victimisation and the outcome. Finally, our results are robust to a number of specification and robustness tests, including alternative functional forms and sample restrictions.

The remainder of the paper proceeds as follows. Section 2 discusses the data sources and provides summary statistics, Section 3 explains our empirical strategy. Section 4 reports the results for violent and Section 5 for property offences. We discuss the short-term effects for males and females before turning to the long-term effects and a discussion of correlated shocks and possible determinants (multiple victimisations, victim-offender overlap, other life-events). Falsification and robustness results are presented in Section 6. We conclude in Section 7 by benchmarking our results and providing a back-of-the-envelope calculation of the resulting cost of crime estimates.

2 Data

Crime rates are not low in the Netherlands compared to U.S. offence rates (keeping in mind caveats of cross-country crime comparisons): In 2016, there were 2,451 property and 386 violent offences per 100,000 inhabitants in the U.S., compared to 3,391 and 529 in the Netherlands, respectively.[4] 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, the Netherlands provides an ideal setting to study our questions.

2.1 Register data on victimisation

The victimisation register data consist 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' (anonymised) social security numbers allowing us to link them to labour market and other register data. A valid (Dutch) social security number is recorded for about 90 percent of the individuals in the sample. The data contain the reporting date and the offence type, but not the geolocation of the crime.[5] We also observe the social

[4]Crime in the U.S. 2016 (last accessed October 29, 2019): https://ucr.fbi.gov/crime-in-the-u.s/2016/crime-in-the-u.s.-2016/topic-pages/offenses-known-to-law-enforcement.

[5]If individuals do not have a valid social security number, they can not be matched to any other data files. This may be either due to the victim not having a Dutch social security number (e.g. tourists) or the police/victim not registering the social security number. For 94 percent of the victimisations in our sample, the difference between the date of reporting the crime and the (start) date of the offence is less than a month.

security number of the suspected offender(s) whenever there is a known suspect. This allows us to link victims and offenders by households, a feature that we use to identify domestic violence offences.[6] The share of incidents with a known offender varies by type of offence and ranges from 72% for assault down to 9% for burglary.

For our analysis, we focus on the month of victimisation as the event time. As health outcomes are only available at the annual level, for these outcomes we focus on the year of victimisation instead. We study 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) and sex offences as well as robbery, burglary and pickpocketing. Following the classification used in the Netherlands (and contrary to classifications e.g. in the US), we will refer to robbery as a property crime.[7] If an individual is a victim of multiple offences during the same incident (e.g. robbery and assault), only the most severe offence is registered, defined as the one subject to the most severe punishment. As we explain later, we focus on the first observed victimisation and if an individual is victimised more than once, we use the calendar dates of each incident to determine which one was the first. When we split the sample by offence (see below), each individual is thus part of *one* subsample matching the offence category of the first recorded victimisation.

Within the categories of assault, threat of violence and sex offences, we identify domestic violence offences as those committed by either current or previous household members. This is our own definition; domestic violence is not recorded as a separate offence by the police. The differentiation between non-domestic and domestic violence offences is important, as illustrated in Table 1: The table shows descriptive statistics from the Dutch Victimisation Survey, a nationally representative survey of individuals aged 15 or older.[8] Respondents who report to

[6]In a previous version of this paper, we based our analysis on data that did not include any information on the offender. Instead, we used information on whether cohabiting household members were suspected of a crime around the time of the victimisation to proxy for domestic violence. See Bindler and Ketel (2019).

[7]*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. *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 remaining sexual offences. *Robbery* is the use of threat/violence to take and/or extort a good from another person. *Burglary* includes theft from a dwelling both *with and without* the use of violence. Excluded crimes include those with no clearly identifiable victim (e.g. offences against the public order) and with reporting concerns (e.g. bike theft).

[8]To keep the sample from the survey comparable to the sample from the register data, we only report results

have been victims of a specific crime in the last five years are asked for details of the *last* incident. Table 1 shows the share of 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, separately by offence and gender.[9] Especially for assault and threat there are clear differences between men and women: For assaults, 47% of females report that they knew the offender and 54% report a familiar location compared to only 24% and 30% for males, respectively. The pattern for threat is similar. Based on these survey responses, women are more likely to be victimised by someone known and/or at a known place. The share of women and men knowing the offender or reporting a familiar location is much more equal for the other offences (except sex offences). These numbers suggest that there are substantial differences in the type of victimisation experienced (on average) by women and men when it comes to assault and violent threat. To account for these potentially important differences, we split our analyses by gender and separate out domestic violence offences when looking at females.[10]

To avoid both the issue of measurement error that arises from selective reporting as well as more general identification problems concerning selection into victimisation we condition our analysis on the sample of crime victims. That means that instead of assuming that victimisation is a random event, we rather assume that the timing of victimisation is (conditionally) random. By conditioning the sample, we change the counterfactual from 'not being victimised' to 'being victimised at a later point in time'. We will discuss this assumption and the empirical setting in more detail in Section 3. We highlight already here that the assumption of (conditionally) ran-

for individuals aged 18-55. The survey is a repeated cross-section and data are available for 2005 to 2016.

[9]The offences in the survey correspond to the categories in the register data. "Known offender" includes the categories partner, ex-partner, family, neighbour, work and other known. "Familiar location" includes 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.

[10]Table 1 also shows that there are different reporting rates for the six offences. Between 54 and 60% of the individuals who self-report being a victim of an assault say that they have reported this to the police. For threat, this is only 29-38%. Burglary, pickpocketing and robbery have higher reporting rates (up to 72%), while reporting rates of sex offences are by far the lowest (15-25%). If we restrict the sample from the survey to those who answered that they reported the offence to the police, 44% (22%) of the female assault (male) victims report that they knew the offender and 51% (36%) reported a familiar location. For violent threat, the shares amount to 34% (28%) and 60% (49%), respectively. Further, reporting rates for assault and sex offences are higher for females who did not know the offender compared to those who knew the offender: 64% versus 56% for assault and 17% versus 10% for sex offences. For female respondents who report an assault by a known offender, the most common are "other known" (22%), ex-partner and neighbour (21%). For violent threat, the most common categories for known offenders are neighbours (40%), other known (26%) and work (17%) and for sex offences work (40%), other known (39%) and neighbour (14%), respectively.

Table 1: **Domestic Violence versus Non-Domestic Violence (Victimisation Survey)**

	Violent offences			Property offences		
Sample:	Assault	Threat	Sex	Burglary	Pickpocketing	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Females						
Known offender (1/0)	0.47	0.32	0.35	0.05	0.03	0.07
Familiar location (1/0)	0.54	0.50	0.38	n/a	0.27	0.17
Reported to police (1/0)	0.60	0.38	0.15	0.72	0.59	0.67
Observations	2,042	7,982	5,215	12,238	7,557	445
Panel B. Males						
Known offender (1/0)	0.24	0.20	0.25	0.04	0.02	0.04
Familiar location (1/0)	0.30	0.35	0.37	n/a	0.30	0.20
Reported to police (1/0)	0.54	0.29	0.25	0.72	0.55	0.56
Observations	3,551	11,592	861	11,019	4,381	385

NOTE- The table shows averages of the indicated variables for the six offences 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, for males in parentheses) are as follows: Known offender - 0.20, 0.12, 0.14, 0.96, 0.97, 0.56 (0.24, 0.12, 0.12, 0.96, 0.96, 0.54); familiar location - 0.19, 0.12, 0.14, na, 0.12, 0.25 (0.24, 0.12, 0.11, na, 0.10, 0.20); reported to police - 0.46, 0.55, 0.67, 0.53, 0.44, 0.51 (0.52, 0.55, 0.72, 0.56, 0.44, 0.49). 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 microdata from Statistics Netherlands.

dom timing of victimisation may be harder to defend in the case of domestic violence offences (which is why we separate these cases out) but proves likely to hold in other cases.

Importantly, we create subsamples by offence and gender.[11] 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 ensure that our offence subsamples are as homogenous as possible, we implement a number of sample restrictions. First, 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. Second, as we study labour market outcomes, we restrict the sample to individuals aged 18-55 at the time of observing the outcome for violent crimes and 26-55 for property crimes. The reason for the differential age restrictions is that victims of property crime are likely to be positively selected (one needs to own valuable

[11]For computational reasons, we draw a 50% random subsample from the sample of burglary victimisations. For domestic violence we only look at females (there are too few incidents of domestic violence with male victims in our data to allow for meaningful statistical analysis), and focus on assault and threat. Similarly, there are too few reported sex offences with male victims in our data.

property after all) and might still be in education during ages 18-25. To avoid observing them at a time of likely labor market entry (when earnings steeply increase), we use a higher age threshold for the property crime samples.[12] Third, 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. Overall, this leaves us with 805,648 individuals divided over the subsamples by offence type and gender. Panel A of Table 2 reports the resulting number of individuals for each offence. Burglary is the most common offence with 220,867 victims across the male and female samples, while those for robbery and sex offences are much smaller (23,223 and 36,978 individuals, respectively). These samples exclude the 41,082 female victims for which we coded the offence as a domestic violence offence.

Throughout the analysis, we investigate our sample restrictions in several ways. We 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 look at one-time victimisations only and as an alternative explicitly control for contemporaneous victimisations. We further restrict the sample to individuals who have not been a registered criminal suspect in the years *after* the victimisation and we discuss the age restrictions for the different samples as well as alternative sample definitions.

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 individuals' income spells including those for wages and earnings from self-employment, unemployment benefits, sickness and disability benefits as well as welfare benefits. For unemployment (UI) benefits, both eligibility period and level depend on individuals'

[12] 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 (Statistics Netherlands, 2013). For violent offences we include younger individuals because these (potentially more selected) individuals might enter the labor market at an earlier age as they might not go into higher education. Our age restrictions (in either case) result in unbalanced samples. As Borusyak and Jaravel (2017) discuss in more detail, the unbalancedness of the panel is not a problem for our individual fixed effects setting. For more details regarding the victim-age profiles, see a previous version of this paper (Bindler and Ketel, 2019).

labour market histories. In case of sickness/disability, an individual receives sickness benefits for up to two years and, if eligible, then transfers to disability insurance (DI). For simplicity, we pool both types and refer to them as disability insurance. The overall level and duration of DI benefits depend on the degree of disability and again 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) and levels depend on the household composition. There is no upper limit to the individual eligibility period for welfare benefits. For none of the aforementioned benefit types are there any significant (mandatory) waiting periods. We highlight here that the observed benefit spells start with the day of turning eligible, but actual benefit payments can be delayed (and paid retrospectively).

We use these data to construct our primary labour market outcomes: earnings and days of any type of social benefit receipt in a given month. Earnings include both wage earnings and income from self-employment.[13] We measure benefit dependency as the number of days during which individuals receive any social benefits, but ignore actual amounts which may be a function of previous income. Further, we generate an indicator that is equal to one for *any* positive earnings in a month for an "extensive margin" analysis.

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 available from 2009) as well as measures of other life-events (moving, family outcomes, offending). Finally, we extract demographic information (gender, year of birth, marital status, household composition, offending behaviour and municipality/neighbourhood codes).[14]

2.3 Descriptives

Table 2 presents summary statistics by gender for both violent (columns 1 to 5) and property offences (columns 6 to 11). There are notable differences in the demographic composition across offences (although some of them are due to the different age restrictions of the violent

[13] 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.

[14] Our main specification includes municipality fixed effects. In 2016, there were 390 municipalities. Offending behaviour (for the victim) is coded from annual individual-level data on suspects of crime. On average, 90 percent of registered suspects are convicted (Statistics Netherlands *et al.*, 2013). In this paper, we will refer to the registered suspects using the terms criminal record and criminal suspects interchangeably.

Table 2: **Summary Statistics - Violent and Property Crimes**

	Violent crimes					Property crimes						Comparison	
Offence:	Assault		Threat		Sex	Burglary		Pickpocketing		Robbery		Non-victims	
Gender:	Male	Female	Male	Female	Female	Male	Female	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Panel A. Background characteristics (measured in the month of victimisation)													
Age	30	31	38	35	26	46	44	39	41	35	39	42	42
Immigrant	0.16	0.22	0.18	0.20	0.12	0.13	0.15	0.15	0.19	0.27	0.28	0.09	0.10
Partner (0/1)	0.28	0.25	0.49	0.35	0.23	0.64	0.52	0.46	0.51	0.26	0.35	0.60	0.66
Children (0/1)	0.19	0.33	0.37	0.43	0.24	0.42	0.47	0.35	0.42	0.19	0.33	0.45	0.54
Total nr. victimisations	1.28	1.41	1.37	1.39	1.29	1.15	1.16	1.14	1.13	1.22	1.22	n/a	n/a
One victimisation	0.81	0.74	0.77	0.75	0.80	0.88	0.88	0.88	0.89	0.83	0.84	n/a	n/a
Observations	126,859	83,349	66,879	59,730	36,978	122,925	97,942	37,991	103,854	10,619	12,604	225,378	241,337
Panel B. Monthly labour market and annual health outcomes													
Earnings (0/1)	0.79	0.63	0.82	0.68	0.65	0.89	0.75	0.86	0.73	0.74	0.69	0.87	0.74
Earnings (in 2015 €)	2,063	1,104	2,711	1,407	1,076	4,092	1,967	3,535	1,804	2,395	1,750	3,217	1,540
Benefits (>0)	0.15	0.26	0.15	0.24	0.23	0.11	0.17	0.13	0.17	0.24	0.23	0.10	0.12
Days benefits	4.49	7.99	4.63	7.23	7.11	3.23	5.06	3.93	5.31	7.31	7.15	3.06	3.61
Total health costs (in €)	1,490	2,582	1,597	2,499	2,854	1,299	2,036	1,545	2,067	2,028	2,284	1,156	1,735
Mental health costs (in €)	455	740	394	570	1,122	226	326	340	352	711	495	190	224
Observations (NxT, Mio.)	15.9	11.3	10.2	9.3	4.3	17.5	14.0	4.6	12.4	1.1	1.5	32.8	35.8

NOTE- The table shows the sample means of the indicated variables for each offence/gender subsample as indicated at the top of each column. Panel A reports the (cross-sectional) background characteristics in the month of victimisation for all individuals in the respective sample; Panel B reports the (longitudinal) monthly labour market and additional outcomes (the health outcomes are at the annual level). SOURCE- Results based on calculations by the authors using microdata from Statistics Netherlands.

and property crime samples): Victims of violent threat, burglary and pickpocketing tend to be older and are more likely to have a partner or children than victims of assault, sex offences or robbery. Victims of property offences are more likely to be a victim only once during our observation window: 83%-89% compared to 74%-81% for violent crimes. Panel B of Table 2 reports the average monthly labour market outcomes across the subsamples; the sample means are based on both pre- and post-victimisation years. Again, there is heterogeneity in earnings and benefit dependency between offence subsamples, but within-offence differences between males and females are even larger. As is generally the case in the Netherlands, females are less likely to work, have lower earnings and are more likely to depend on benefits.[15] For comparison, columns (12) and (13) of Table 2 show corresponding descriptives for a 5% random sample drawn from the population not registered as a victim and to which we apply the same restrictions as to our analysis sample. The non-victims are different especially from the victims of violent crime in terms of household composition, labour market outcomes as well as health expenditures. As our individual fixed effects approach does not allow for (time-invariant) controls to account for such (quite large) compositional differences, the above highlights the importance of restricting our sample to victims of crime and supports our strategy to conduct the analysis separately by offence and gender.

3 Empirical Strategy

Ultimately, we are interested in the causal relationship between criminal victimisation and labour market outcomes for which we have to overcome two identification problems. First, unobservable characteristics may correlate both with the outcome 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 the potential omitted variable bias over and above conditioning the sample on victimised individuals (which takes care of unobservables *common* to all victims compared to non-victims), we adopt

[15] Appendix Table A1 provides descriptive statistics for (female) domestic violence victims for comparison. On average, they are older, more likely to have a partner and/or children and more likely to be a non-western immigrant than victims of non-domestic violence offences. They are also more likely to have multiple victimisations within our observation period. Panel B illustrates negative selection in terms of labour market outcomes: Even though on average older than non-domestic violence victims, their earnings are lower, they are less likely to work and more likely to have a positive benefit income.

a quasi-experimental approach and use an event-study design with individual fixed effects. This strategy is similar to Grogger (1995) who analyses the labour market effects of being arrested in a distributed leads and lags model, or more recently Dobkin *et al.* (2018) and Kleven *et al.* (2019) who use event-study designs with suitable sets of fixed effects to study the economic consequences of hospital admissions and the earnings gender gap, respectively. 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 and time-invariant individual traits.

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 this simultaneity concern in the spirit of a Granger test for causality by explicitly studying the timing of any effect of victimisation on labour market outcomes in our event-study design and pay close attention to any pre-victimisation effects/trends. Importantly, we can leverage our rich data to zoom into the monthly level and identify sharp changes in labour market outcomes at the time of victimisation and to closely study other life events around the time of victimisation.

Let Y_{itmal} denote the respective outcome for individual *i* in age group *a* and location *l* (municipality) in year *t* and month *m*. We estimate the following equation in which the coefficient β_s varies with time to and since victimisation *s*:

$$Y_{itmal} = \beta_0 + \sum_{s=-49, s\neq -1}^{49} \beta_{1,s} \cdot V_{itm,s} + \alpha_i + \alpha_t + \alpha_m + \alpha_{t,m} + \alpha_a + \alpha_{t,a} + \alpha_l + u_{itmal} \quad (1)$$

$V_{itm,s}$ is a dummy equal to one if the difference between the calendar month minus the victimisation month equals *s* (months). For instance, $V_{itm,0}$ is equal to 1 in the month of victimisation and equal to 0 otherwise. The omitted period is defined as the month preceding the victimisation (*s*=-1).[16] We include individual, year, month, year by month, age group and mu-

[16]This avoids using the (unbalanced) left-hand tail as a base period. The dummies $\beta_{-49,s}$ and $\beta_{49,s}$ capture four years or more before and four years or more after victimisation.

nicipality fixed effects α_i, α_t, α_m, $\alpha_{t,m}$, α_a and α_l, respectively. To non-parametrically control for age specific trends (e.g. due to cohorts entering the labour market at different times), we include an age-group specific year fixed effect $\alpha_{t,a}$.[17] Standard errors are clustered at the individual level, and we estimate the model separately by offence and gender.

A causal interpretation of the parameters in (1) relies on the assumption that the *timing of victimisation* is random, conditional on our sample restrictions (victims only) and control variables (including individual fixed effects).[18] Again, this means that there should be no time-variant unobservables correlated with both the time of victimisation and the labour market outcome and no simultaneity, i.e. the victimisation timing should be uncorrelated with the outcome. Estimating event-study designs as described above allows us to assess the plausibility of these assumptions to the extent that a violation would result in pre-trends.

A key advantage of the event-study design is that even if there are visible pre-trends, we can still assess sharp changes in labour market outcomes around the time of victimisation. To directly address remaining concerns, we offer three approaches: First, we explicitly discuss other life-events (other than labour market outcomes) that precede and follow the victimisation. This not only assesses correlated shocks at the time of victimisation but also helps to understand potential drivers of long-term effects. Second, we test whether our results are sensitive to specific sample definitions. Finally, we conduct two different falsification tests to assess whether our main results are driven by spurious relationships, e.g. remaining time trends (see Section 6).

4 Results: Violent Crime

We start our discussion with violent crimes which are typically considered more severe offences than property crimes (discussed in Section 5). We separately present results for male victims and female victims, for whom we make the explicit distinction between non-domestic and domestic violence as highlighted in Section 2. We first focus on the short-term effects of violent crime victimisation, and then proceed to discuss the longer-term effects as well as

[17]The age groups are 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.

[18]Including individual fixed effects matters a lot when we sequentially build our specification. Simple OLS yields significant point estimates for all leads and lags that differ substantially from those in the fixed effects estimation. 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. Controlling for (available) time-invariant individual controls instead of individual fixed effects does not improve significantly upon a simple OLS specification.

correlated shocks and life events leading up to and/or following the victimisation.

4.1 Males

Our results regarding labour market effects for male victims of violent crimes are shown in Figure 1. Each figure plots the estimated coefficients $\hat{\beta}_s$ and corresponding 95% confidence intervals for equation (1).[19] The two vertical lines mark the beginning and end of the month in which the victimisation is reported. This particular coefficient averages between victimisations at the beginning and those towards the end of the month. The month following the victimisation is the first full month of treatment. For easier reference, the black-shaded markers correspond to the twelve months before and after the victimisation, our "short-term" period for which the sample is more balanced and less susceptible to e.g. outliers and sample differences. We return to the longer-termed effects in Section 4.3.

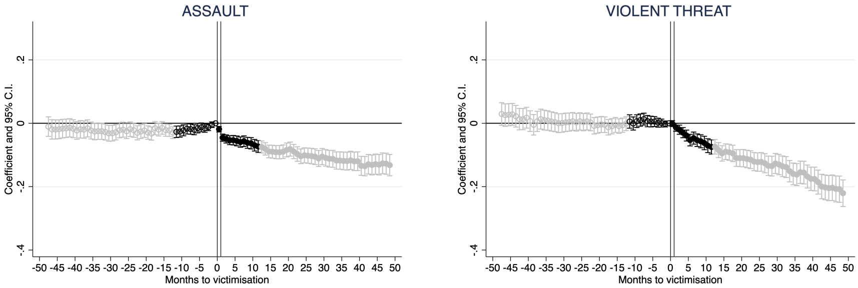
Panel A shows the results for log earnings for assault and violent threat. For both offences, the effects are very precisely estimated and reveal a sharp decrease in earnings at the time of victimisation. For assault, earnings decrease by 4.5% in the month following the victimisation and by 7.5% twelve months after and for violent threat, by 1.3% and 7.4%, respectively. While we see a discrete jump in the point estimates after victimisation for assault, for violent threat we instead see a trend break with a less immediate decrease in earnings. Notably, there are no pre-trends and the earnings effects persist over the next four years for both offences - an observation which we will come back to. We find an almost mirror image of these results when we look at the days of benefit receipt in Panel B: For assault, there is an immediate increase by 3.5% relative to the mean which is lasting over the subsequent months and still amounts to 2.9% twelve months after victimisation. For violent threat, the increase is less immediate and accelerates from initially 1.1% up to 5% twelve months after victimisation.

Panels C and D of Figure 1 show the results for total and mental health expenditure, respectively. Especially for assault, there is a clear spike in total health expenditure in the year of victimisation, corresponding to a 27.7% increase relative to the mean. While the effect levels out subsequently, the point estimate in the year after victimisation still reflects a 10.8% in-

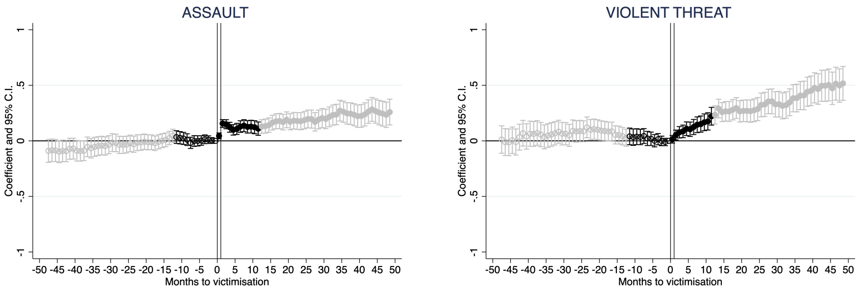
[19]We also estimate the coefficients for the tails (four or more years before/after victimisation) as specified in equation (1), but omit them from the graphs.

Figure 1: **Males - Violent Crime**

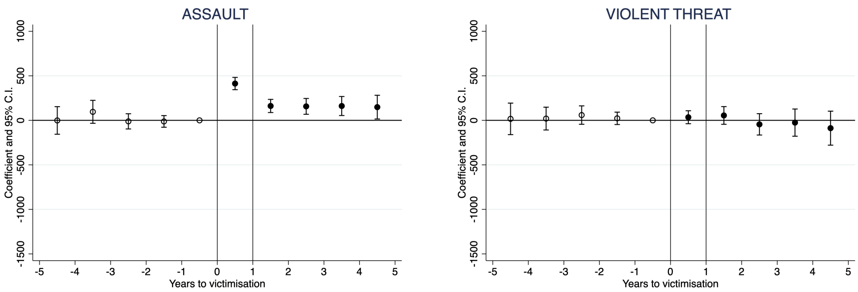
Panel A. Log earnings



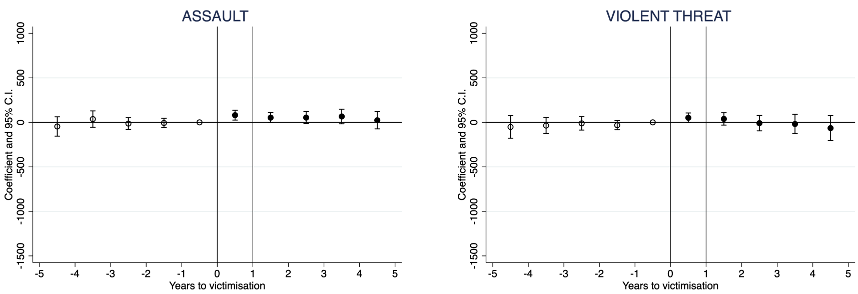
Panel B. Days of benefits



Panel C. Total health expenditure



Panel D. Mental health expenditure



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

crease relative to the year before the victimisation. These results are in line with the notion that assaults cause physical harm and can inflict the need for short- and long-term treatment. Consistent with that, we do not see any significant changes in overall health expenditure for violent threat (although there is a marginally significant short-term increase in mental health expenditure by 13% relative to the mean).

The labour market responses are immediate for assault but more gradual for violent threat. Why are there such differences? In contrast to assault, in many cases a "one-off" incident, threat can be a longer-lasting offence that may be ongoing over a sustained period of time. This is reflected by a much longer average time between the 'start date' and 'end date' of a given offence (as reported in our data). The timing of the *immediate* decrease in earnings and increase in benefit receipt for assault victims is remarkable though. Such a prompt (average) response in earnings could be due to job losses. In the Netherlands, workers can lose their jobs immediately if they have temporary or zero-hour employments. On average, about 20% of the workforce in the country hold fixed-term contracts. This proportion is considerably higher in our (violent crime) samples: 43% of male assault victims hold fixed term contracts and 18% have temporary work arrangements such as via agencies. One month after victimisation, 8.2% of the male assault victims who had a fixed-term employment at the time of victimisation are without a job, compared to 3% of those who had permanent contracts. Another month later, these percentages amount to 11.6% and 4.5%, respectively. These numbers offer a plausible explanation for the immediate response in earnings. As information on contract types is not available for all outcomes years, we complement these descriptive observations by showing the extensive margin results in Panel A of Appendix Figure B1 (i.e. the likelihood to have any type of employment). In line with the intensive margin results (log earnings), this likelihood decreases immediately after victimisation for assault victims. In terms of magnitude, the extensive margin can explain some, but not all of the (earnings) effects. The implication is that the estimated effect in Figure 1 combines changes at both margins.[20]

The timing of the changes in benefits is similar, with again an immediate response for assault victims. To reiterate, we code our variables from benefit spells. It is possible that the

[20]The extensive margin results can also be seen as a specification test with respect to how we treat zero earnings.

start date of the spell is reported retrospectively such that we can observe immediate changes in the likelihood of entering benefit regimes. Panel A of Appendix Figure B2 shows separate analyses by type of benefit (unemployment insurance, disability insurance, welfare benefits). There is an immediate and significant increase in the uptake of disability insurance (specifically for assault), a more gradual increase in welfare benefits and not much of a response in the uptake of unemployment benefits. Keeping in mind that disability insurance includes sickness payments, these results suggest that assault victims experience a decrease in earnings as they transition to sickness benefits, maybe due to severe injuries. This, in fact, is consistent with the results for health expenditure as described above.

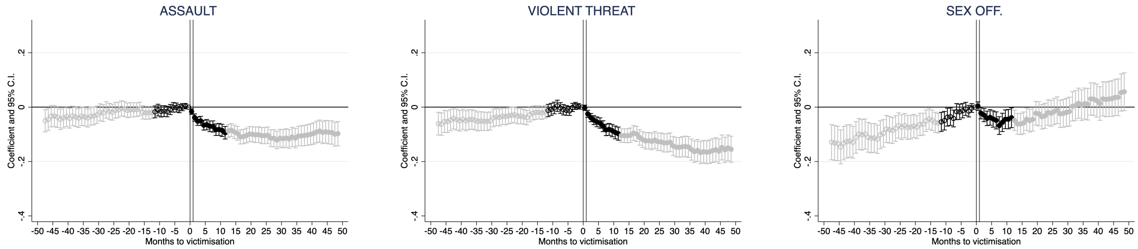
4.2 Females

Non-domestic violence. The results for females are shown in Figure 2. These figures *exclude* all offences that have been classified as domestic violence and to which we will come back later. Starting with earnings in Panel A, there is an immediate decrease by 3.8% one month and 8.8% twelve months after an assault. After that, point estimates level out but never return back to zero. For violent threat, we see a similar pattern but slightly different magnitudes (2.5% and 10.4%, respectively). For sex offences, there is a trend break with a decrease in earnings in the immediate months after victimisation - however, the pattern suggests that the sample might not contain enough observations in the later months to sufficiently control for the upwards trends in earnings as individuals become older. Maybe even more striking are the findings for benefit receipt (Panel B). Starting with violent threat and sex offences, we observe a clear trend break at the time of victimisation. For violent threat, benefit dependency increases by 2% (relative to the mean) one month after and by 5.3% one year after victimisation. For sex offences, the point estimates suggest an immediate increase by 1% and 4.7% one year later.

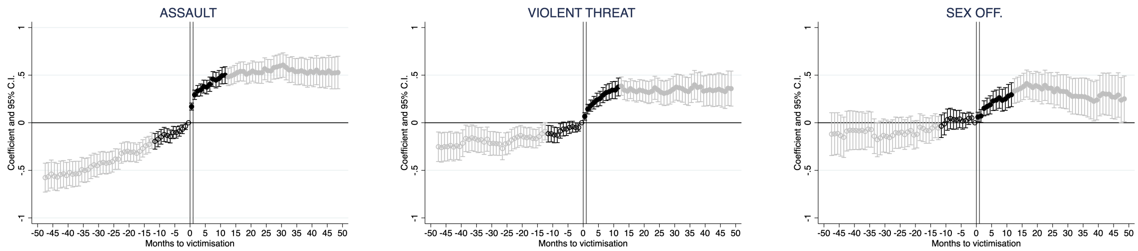
While there were no such concerns for either earnings or benefits so far (especially for men), only for assault there now are significant pre-trends in benefits. These may be due to unobserved victimisations preceding the first observed victimisation, i.e. an earlier treatment effect showing up as a pre-trend in this particular sample. The pre-trends would then not reflect a simultaneity problem, but rather be the consequence of earlier victimisations. This could in particular be the case if some domestic violence offences are not classified as such by our

Figure 2: **Females - Violent Crime**

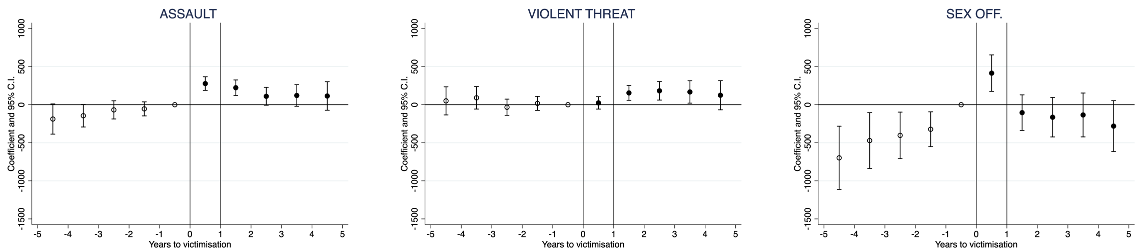
Panel A. Log earnings



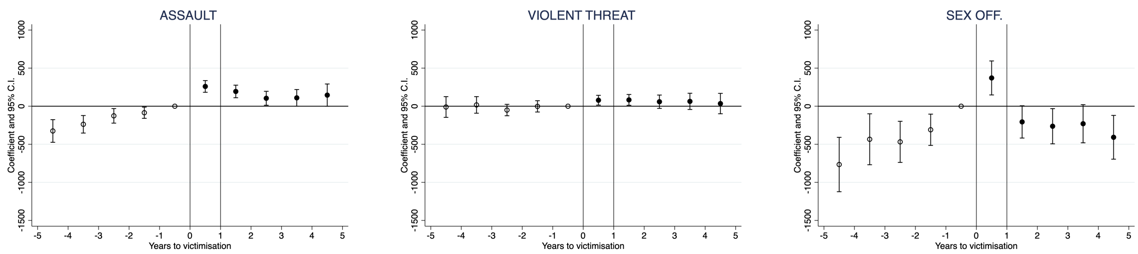
Panel B. Days of benefits



Panel C. Total health expenditure



Panel D. Mental health expenditure



NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with log earnings (Panel A), days of benefits (Panel B), total health expenditure (Panel C) and mental health expenditure (Panel D) as the dependent variable. The figures show results from left to right for assault, violent threat and sex offences. The solid vertical lines mark the start and end of the victimisation month (year). SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

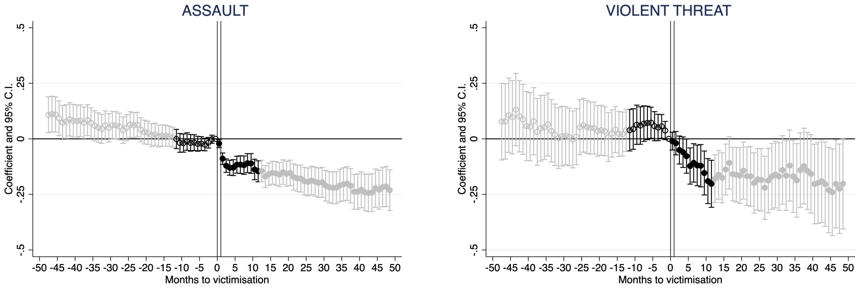
definition, e.g. because the offender was not known (not reported) to the police or not registered at the same address as the victim. We investigate this point in more detail soon and provide results supporting this argument. An alternative explanation is that other life-events (happening before the time of victimisation) contribute to the changes in labour market outcomes. We study potential candidates in our discussion of correlated shocks (Section 4.3), but find that (in contrast to earlier victimisations) other life-events prior to victimisation are unlikely to drive this pre-trend. Regardless, it is important to highlight that for assault we observe a sharp and discrete change with a large increase in the point estimate in the months of and following the victimisation which - at least in the short-term - is not plausibly explained by a continuation of the pre-trend alone. That is, the victimisation can be interpreted as a sharp *escalation point*: Benefit dependency increases by 3.7% (relative to the mean) one month and by 6% twelve months after relative to the month before victimisation.

Panels C and D of Figure 2 show the results for total and mental health expenditure, respectively. Similarly to males, overall (annual) health expenditure increases immediately for assault by 277€ or 10.7% relative to the mean in the year of victimisation. There is a comparable, but short-term increase for sex offences of 414€ (14.5%) in the victimisation year. However, in the latter case there are trends leading up to the victimisation and confidence intervals are wider as the sample size is considerably smaller. In comparison to mental health expenditure in Panel D, we find that the patterns are strikingly similar and that level increases in mental health expenditure explain large parts of the overall health expenditure. Relative magnitudes are larger though as the average expenditure for mental health is low compared to overall cost.

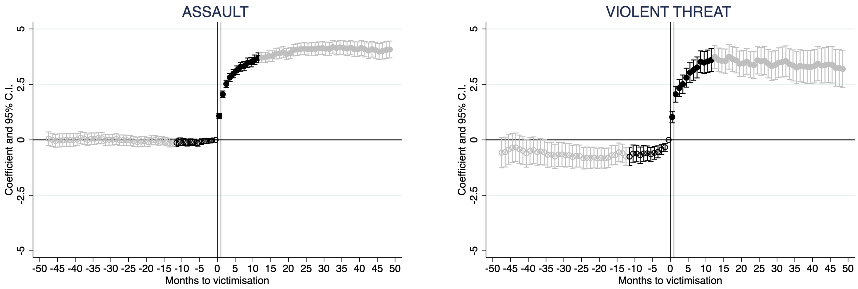
As for males, the labour market responses are immediate and likely for similar reasons. The extensive margin analysis (Panel B in Appendix Figure B1) suggests again that there is some transition into non-employment. With regards to benefit types (Panel A, Appendix Figure B3), the increase in benefit dependency stems from a combination of disability insurance and welfare benefits. The increase in welfare benefits is larger than for males (but shows the same pattern of pre-trends as seen for benefits overall and to which we return soon). Comparing magnitudes in the month after victimisation (i.e. the size of the jump at the escalation point), we find that welfare dependency increases by 2.2% for male victims but 5.5% for female victims of assault.

Figure 3: **Domestic Violence - Females**

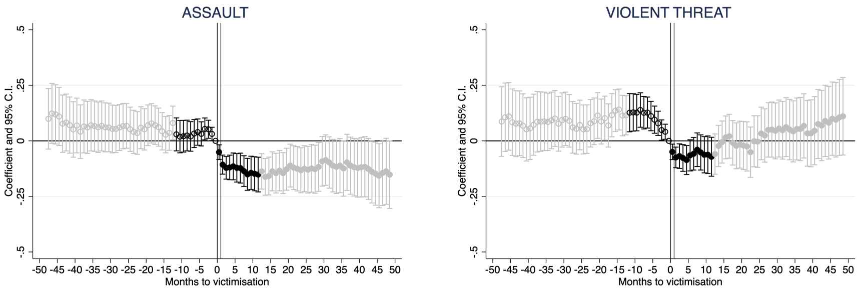
Panel A. Log earnings, offender is partner



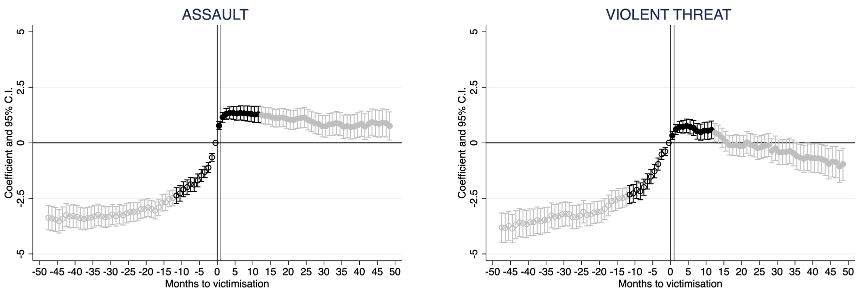
Panel B. Days of benefits, offender is partner



Panel C. Log earnings, offender is ex-partner



Panel D. Days of benefits, offender is ex-partner



NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with log earnings (Panels A and C) and days of benefits (Panels B and D) as the dependent variable. The figures show results from left to right for assault and violent threat. The solid vertical lines mark the start and end of the victimisation month. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

Domestic violence. Next, we shortly shift the discussion to domestic violence offences. Our main goal is to offer a possible explanation for the pre-trend seen in the specific case of benefit dependency of assault victims. We argue that this pre-trend can plausibly be explained by remaining domestic violence cases that cannot be identified as such. A secondary interest lies in sketching the labour market responses to domestic violence, adding to the existing literature and ongoing policy debate regarding domestic violence.[21]

Appendix Table A1 provides summary statistics split by type of offender (partner or ex-partner) and shows that victims of domestic violence are more negatively selected than victims of other violent crimes. The majority of domestic violence cases are assaults: 21.4% of the observed assaults are classified as domestic violence, but only 10.1% of violent threats and 1.6% of sex offences (resulting in too few observations for meaningful analysis).

Figure 3 reports the results from our regressions. We start with the case in which the offender is the current partner at the time of victimisation (Panels A and B). The results are striking: Earnings drop immediately after an assault and decrease more gradually for violent threat. For benefits, pre-trends are largely flat followed by extremely sharp increases after victimisation for both offences. The benefit results are driven by household-level means tested welfare benefits (as seen before), and it is possible that the flat pre-trends are partly mechanical: At the *escalation point*, when these women report offences to the police, the household plausibly dissolutes and the female victim (immediately) becomes eligible for welfare benefits. However, an entirely mechanical explanation would not be consistent with the sharp decreases that are seen for earnings. An alternative explanation for the absence of pre-trends is that these reported victimisations are first-time victimisations and that there are no earlier, unobserved incidents that would lead to pre-trends. This argument is supported by the findings for domestic violence cases in which the offender is an ex-partner (Panels C and D of Figure 3). While the results for earnings look comparable, we now see a clear pre-trend in benefits for both assault and violent

[21] For recent studies on the negative impacts of domestic violence on the victim see e.g. Currie *et al.* (forthcoming), Ornstein (2017) and Peterson *et al.* (2018). As described before, we identify domestic violence as incidents in which the (known) offender is or was registered at the same address as the victim. We are confident that we do not misclassify non-domestic violence cases as such, but might not be able to capture the universe of domestic violence incidents. In particular, our "non-domestic violence" sample may still contain domestic violence cases which are not reported as such (offender unknown to the police) or intimate partner violence where the partner is (or was) not registered at the same address as the victim.

threat, but - as was the case in Figure 2 - there is a distinct change at the time of victimisation. Given that the offender here is an ex-partner, it is plausible that there are earlier but unreported instances of domestic violence leading up to what we term the escalation point. If such earlier instances of domestic violence lead to effects such as seen in the case in which the partner is the offender (Panels A and B), they can reasonably explain the pre-trends in the benefit outcome. In that sense, our results also document underreporting of domestic violence cases, a fact that is consistent with what was seen earlier in the statistics from the Dutch Victimisation Survey.[22] To the extent that our sample of *non-domestic violence* offences contains incidents that actually are domestic violence in nature but cannot be identified as such, our findings here can explain the observed pre-trends in benefits for females in the one case of assault in Figure 2. We will return to the notion of confounding life-events (e.g. household dissolution) as an alternative explanation in our discussion of correlated shocks in the next section.

While the main purpose of our domestic violence discussion is to support our argument that the pre-trends in the main assault regressions for the benefit outcome (for females) can plausibly be explained by non-identified domestic violence cases, the magnitudes of the estimates are interesting per se. When the current partner is the reported offender (Panel A and B), earnings drop by 8.9% in the month immediately after an assault (14.4% one year later) and gradually decrease for violent threat up to 17.9% one year after victimisation. At the same time, relative to the mean benefits sharply increase by 23.1% one month and 41.7% twelve months later following an assault (22.9% and 41.6% for violent threat, respectively). When the ex-partner is the offender (Panel C and D), there is a sharp change at the time of victimisation despite pre-trends: Benefit receipt in the month after compared to the month before victimisation, i.e. precisely around the escalation point or trend break, increases by 11.3% for assault and 6.1% for violent threat. At this point, we are reluctant to say more about anything that happens before and drives the pre-trends - this is of interest in and of itself and we leave it for future research.

[22] In contrast to the benefit outcome, overall there are no significant pre-trends in *earnings* leading up to the month of victimisation (neither for domestic violence nor earlier for non-domestic violence). Possibly, the women who are affected in their earnings are not the same as those who are affected in benefit recipiency *before victimisation*. Indeed, to the extent that the pre-trends are driven by unobserved prior instances of domestic violence, this argument is consistent with the observations made earlier that female victims of domestic violence are negatively selected - not only compared to the general population but also compared to the population of female victims of non-domestic violence offences.

4.3 Long-term effects and correlated shocks

Long-term effects. So far, we have focused our discussion on the effects within the first year after victimisation. This has some advantages: We can focus on the sharp changes around the time of victimisation when interpreting the magnitudes and limit the risk of confounding events compared to a longer-term perspective. Further, the panel is most balanced around the time of victimisation and thus less susceptible to e.g. outliers and sample differences.[23] However, our rich data allow for a longer-termed perspective which has hardly been studied before but is important from a policy perspective.[24]

The grey-shaded markers in Figures 1, 2 and 3 represent the long-term estimates up to four years after victimisation. For men and women, our results document lasting changes in earnings and benefit dependency which are still visible four years after victimisation. For both assault and violent threat, the point estimates for the benefit outcome remain quite stable over this period and increase slightly for earnings. Such scarring effects 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. Indeed, other literatures find similarly persistent labour market effects of adverse events: Studies on the effects of job displacement report decreases in earnings lasting up to 12 years after displacement in 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

[23] As a reminder, our sample restrictions are based on the age at which the outcome is observed: Individuals enter the sample as they turn 26 and leave the sample as they turn 55. By construction, we observe everyone at the time of victimisation. The last victimisation year in the sample is 2016, the same as the last year of observed labour market outcomes. Our findings are robust to excluding 2015 and 2016 victims, such that we observe at least 24 months of outcomes for all victims in the sample. Further, including year as well as individual fixed effects actually deals with concerns about selective unbalancedness, as described in Borusyak and Jaravel (2017). Finally, the unbalancedness can explain why the confidence intervals increase with time since victimisation.

[24] Another important component of the social cost of criminal victimisation is the potential impact on non-victimised household members. Not taking this into account could lead to an underestimation of the extent to which individuals and families are affected. We test for changes in labour market and health outcomes for the (cohabiting) partner of a victim of a violent crime in an analogous design to equation (1). The results (pooling men and women) are shown in Appendix Figure B4: We do not find an immediate impact on the partner's earnings (Panel A) and some - albeit comparably small - response in benefits (Panel B). There is, however, no similarly sharp escalation point at the time of victimisation as seen for the victims themselves. Finally, we do not find any spillovers on health expenditure, with the exception of a short-termed increase in mental health expenditure for violent threat of 18.7% at the mean which is marginally significant (at the 10% level). Taken together, our results suggest that there are some, but limited (both in terms of occurrence and in terms of magnitude) spillover effects on the cohabiting partner. This is consistent with Cornaglia *et al.* (2014) who report a negative impact of victimisation on other family members which, however, is weak in terms of statistical significance.

displacement in Norway (Huttunen *et al.*, 2011). Oreopoulos *et al.* (2012) report that graduating from college during a recession results in initial earnings losses of 9% which only disappear ten years after graduation. Importantly, they find that workers at the lower end of the (predicted) earnings distribution suffer from larger and more permanent losses. As documented in Table 2, victims of violent crime are on average at the lower part of the earnings distribution and may thus be at higher risk of lasting losses in earnings as suggested by our results.

An alternative explanation is that the victimisation is a life-changing event (an escalation point) triggering other life-events that contribute to the effects on labour market outcomes seen in the long-run. If the victimisation *exactly* coincides with another major life event, it could be that even in the short-run the sharp changes in earnings and benefits which we observe at the time of victimisation are not driven by the victimisation but by other disruptions in life. To shed more light on these questions, we study correlated shocks and life events leading up to and/or following the victimisation. We start by looking at multiple victimisations, next address the possibility that a victim-offender overlap contributes to the long-term effects and finally study moves, divorces and the birth of a child as potentially relevant life-events.

Multiple victimisations. So far, we have focussed on the first victimisation. If there are later victimisations that also affect labour market outcomes, these might contribute to the estimated long-term (and even short-term) effects. This would imply attenuated coefficients for a sample with just one victimisation between 2007 and 2016 (74-88% of victims in the subsamples, see Table 2). Results for this sample are reported in Appendix Table A2. We report the estimated coefficients six months before, one month after and twelve months after victimisation and include the baseline estimates as a benchmark. Restricting the sample to those with just one victimisation attenuates the estimates for both outcomes (more for females than for males) but leaves the overall pattern intact. Taking assaults in Panel A as an example and looking at twelve months after treatment, we find a 7.6% (6.7%) decrease in earnings for males (females) compared to 7.5% (8.8%) at the baseline, and a 0.11 (0.37) days increase in benefit receipt compared to 0.13 (0.48) at the baseline. Our previous results combined the effect of the first and subsequent victimisations. The results here suggest that multiple victimisations contribute to the persistent labour market responses - but given the large share with only one victimisation they are unlikely to capture the full story.[25]

Criminal record. Can the lasting effects of criminal victimisation be explained by a victim-offender overlap? We restrict our sample to not having a criminal record *prior* to victimisation, but individuals may subsequently engage in crime which in turn may impact their labour market outcomes (as documented in the literature). We address this by restricting the sample to those who do not have a criminal record at any point during the sample period, including the time *after* victimisation.[26] If subsequent criminal activity indeed drives our results, specifically in the longer-term, point estimates should be smaller for a sample of non-offenders. Appendix Table A2 shows that compared to the baseline, the point estimates for one and twelve months after victimisation are indeed attenuated, in particular for earnings and now more for males than for females. Given that males constitute the larger share of offenders, the latter may not be too surprising. Looking at twelve months after an assault, earnings decrease by 5.3% (7.7%) for males (females) compared to 7.5% (8.8%) at the baseline, and benefits increase by 0.11 (0.45) days compared to 0.13 (0.45) at the baseline. The smaller estimated impacts of victimisation for non-offenders suggest indeed that later criminal involvement (and resulting labor market effects) may contribute to the long-term effects in particular for males' earnings.

Correlated shocks. Finally, we study moves, divorce and child birth as potential correlated shocks. As there is limited within-individual variation in these outcomes, we re-estimate equation (1) omitting individual but still including all other fixed effects (except for location fixed effects when studying moves).[27] Figure 4 shows the results from these regressions for moves (Panel A), divorce (Panel B) and birth of a child (Panel C). We combine the results for

[25] As an alternative, we re-estimate our baseline model but include an additional control for any contemporaneous victimisation. As for single victimisations, we find slightly attenuated coefficients and more so for females than for males (see Appendix Table A2). We also estimate the direct effect of the second victimisation conditional on having more than one victimisation but otherwise following equation (1),and see clear changes in earnings and benefits at the time of the second victimisation (results available on request). However, we are hesitant to give these estimates a causal interpretation, as we know that these individuals have had a previous victimisation that affected their labour market outcomes (and as is reflected by pre-trends in these specifications).

[26] 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.

[27] In practice, including individual fixed effects makes little difference and the estimation results are similar. We prefer the specification without individual fixed effects in these models with low-probability binary outcomes. In a previous version of this paper, we showed unadjusted means instead of the regressions omitting individual fixed effects. Our conclusions are largely the same.

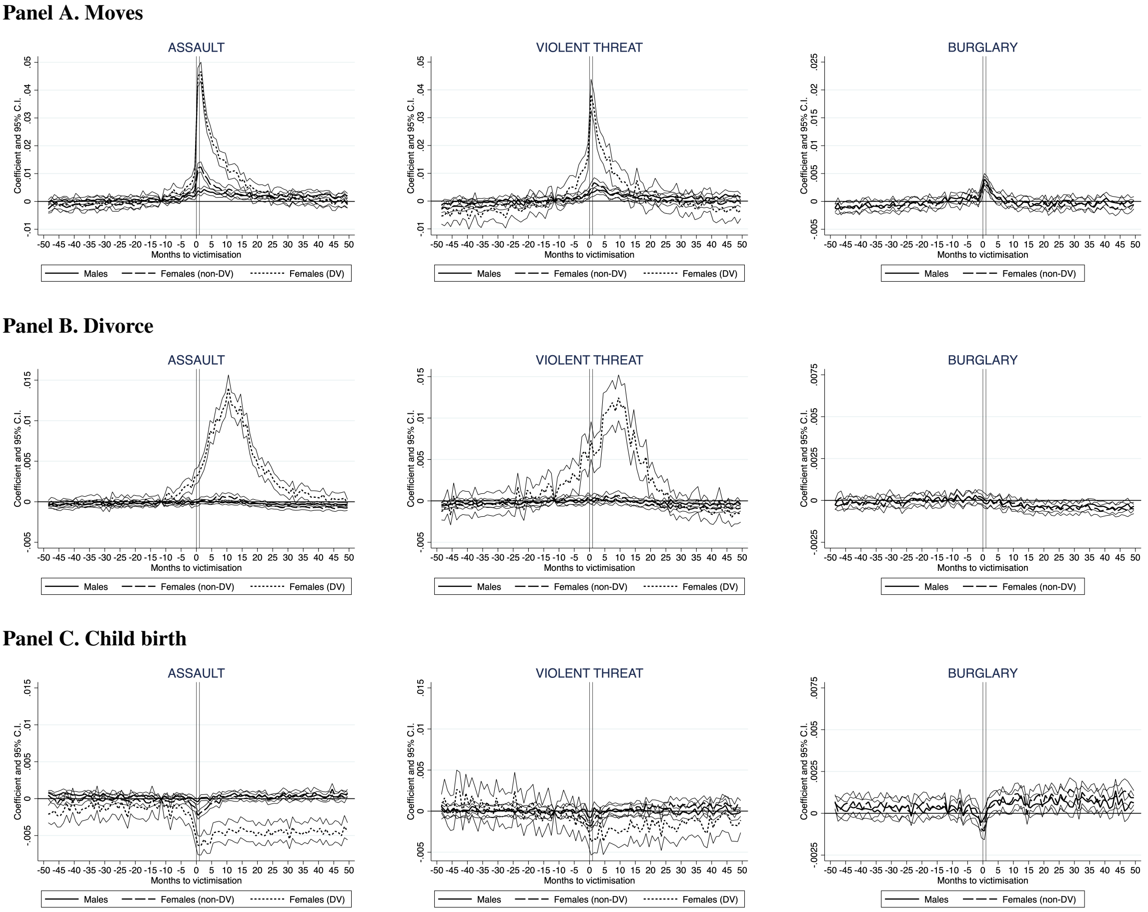
males, females and for female domestic violence into one graph per offence and outcome. We show the results for the two main violent crime offences assault and violent threat (as well as for burglary, discussed in Section 5); results for all remaining offences are available on request. In contrast to our baseline, the omitted month in the event-study is one year (instead of one month) before victimisation to avoid benchmarking the estimates to the pregnancy period in the child birth outcome regressions and to be consistent across outcomes.

For male victims of assault and threat, the likelihood of moving increases at the time of victimisation relative to one year earlier but at a relatively small scale (0.3 percentage points for both offences in the month after victimisation). For females, the increase in the likelihood of moving is higher (1.2 percentage points for assault, 0.07 for threat) which is consistent with the stronger labour market responses seen earlier. However, the magnitudes are at best moderate when compared to female victims of domestic violence: Here, the likelihood of moving increases slightly before victimisation and peaks sharply at 4.7 percentage points for assault and 3.8 percentage points for threat. Panels B and C show the results for the family outcomes divorce and child birth. While there is no noticeable change in the probability of divorce for either male or female victims of (non-domestic) assault and threat, the pattern for domestic violence stands out again. Prior to victimisation, the likelihood of divorce starts to increase, accelerates after victimisation and peaks at 1.4 and 1.3 percentage points ten and nine months after victimisation for assault and violent threat, respectively.[28] These sizeable increases are relative to divorce rates of 0.14% and 0.2% twelve months before victimisation in the respective samples. Lastly, for child birth there is a dip around the time of victimisation which is smallest for males, somewhat larger for females and again noticeably larger for female victims of domestic violence. Naturally, it is plausible that expecting and new parents change behaviour during pregnancy and almost by construction have a lower likelihood of falling victim to street crimes (e.g. as they stay at home more). Our regressions cannot identify the direction of causality and cannot be interpreted causally at this point.

These results suggest two conclusions: First, for males it is unlikely that our findings regarding the effects of victimisation are driven by correlated shocks, as there are no large enough

[28] In the Netherlands, 90% of requests for divorce are approved within one year and upon agreement within two months. Source: https://www.rechtspraak.nl/Uw-Situatie/Echtscheiding/Paginas/doorlooptijd.aspx.

Figure 4: **Correlated Shocks**



NOTE - The figure shows the results from estimating equation (1) without individual effects for the following outcomes moves (Panel A), divorce (Panel B) and child birth (Panel C). The figures show results from left to right for assault, violent threat and burglary. The solid vertical lines mark the start and end of the victimisation month. The figures combine the results for males, females without domestic violence and for domestic violence (where applicable). SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

changes (if any) that would convincingly explain our results. Second, for female victims of domestic violence it is apparent that there are adverse outcomes preceding but also following the victimisation. These could plausibly explain the pre-trends seen earlier in these cases (see Figure 3) and at the same time contribute to the large negative labour market effects. Moreover, we sometimes observe similar, but quantitatively less strong patterns for female victims of offences (in particular assault) that are not classified as domestic violence in our data. Again, this is consistent with the idea of misclassification, i.e. our inability of identifying domestic violence cases in the data if either the partner is not (was never) registered at the same address as the victim or the offender is not reported. Consequently, such *de facto* domestic violence (or intimate partner violence) cases could drive the patterns seen in the correlated shocks and the pre-trends seen in the one case of female assault victims for benefit receipt (see Figure 2).

Victimisation as an escalation point. What is the main take-away of our above analyses? Controlling (in different ways) for later victimisations or a later criminal record attenuates the estimated effects which suggests that the victimisation might set an individual at the margin on a trajectory of such events in the future. Next, our correlated shocks analysis shows that other life-events do not plausibly confound our estimated effects at the time of victimisation for non-domestic violence offences. Finally, the likelihood of family dissolution increases after victimisation, in particular for female victims of domestic violence. However, the results also suggest that these events are not the sole explanation of the pre-trends seen for the benefit outcome for domestic violence victims; these are more likely to be driven by earlier, unobserved victimisations. Importantly, and as we emphasised before, despite such pre-trends there is a visible and sharp change in labour market trajectories at the time of the (observed) victimisation. Taken together, our results indicate that the observed victimisation leads to an escalation point in the victim's life.

There is a noteworthy parallel to the crime literature regarding offending as a life-changing event. The notion of state dependence in criminal behaviour is built on the idea that current activities and events can transform a person's life in such a way that later criminal activity becomes more likely (Nagin and Paternoster, 2000): "[...] the state dependence process asserts that an observed correlation between past and future criminal behaviour reflects the fact that the act of committing a crime transforms the offender's life circumstances in such a profound way that it alters the probability that subsequent criminal acts will occur. The state dependence process, then, is a process of contagion in which an offender's current activities makes their life circumstances worse, accelerating the probability of future crime."[29]

5 Results: Property Crime

The previous section focussed on violent crime. In this section we shift attention to property crime (burglary, robbery, pickpocketing) which is typically considered to be less severe than violent crime, at least in terms of punishment severity. At this point, we note that there is no

[29]The idea of hysteresis has found empirical support in the literature. Bell *et al.* (2018) report a lasting effect of entering labor market during recessions on criminal involvement over the life-course; Mueller-Smith and Schnepel (forthcoming) document adverse labour market effects of convictions compared to diversion in the criminal justice system. For further discussion of the life-course view of crime, see Sampson and Laub (2005).

uniform classification of property crime across countries. While the FBI classifies robbery as one of the violent index crimes in the US, it is considered a property crime in the Netherlands. Further, in the Dutch context the offence category burglary contains both burglary with and without the use of violence. The implication is that in our setting, property crime includes fairly severe offences.

5.1 Males

Figure 5 shows the results for earnings and benefits. For burglary, there is a precisely estimated decline in earnings of 1% one month and 4.3% twelve months after victimisation. The pattern for benefits is mostly a mirror image: Pre-trends are flat and point estimates increase after victimisation, but remain small and not significantly different from zero within the first few months after victimisation. The labour market patterns are accompanied by a small short-term increase in the likelihood of moving in the month of, but not prior to victimisation, suggesting that moving (maybe a natural response to a burglary which by definition takes place in the victim's home) is one contributing factor as it might be linked to changes in work patterns (see Figure 4).[30] Though the estimates for robbery are less precise as the sample is much smaller, there is a sharp drop in earnings exactly after victimisation. In the first month, this amounts to about 8.4%, i.e. the immediate effect is larger than for burglary. After that, the point estimates remain negative (around -6%) but are no longer significantly different from zero. There is again a discrete change for benefits (a 2.7% increase in the month after victimisation), but confidence intervals are wide and it is hard to draw statistically firm conclusions. When split by benefit type, as shown in Panel B of Appendix Figure B2, we find a more precisely estimated increase in days of sickness/disability benefits for robbery. Finally, for pickpocketing there is no sharp change at the time of victimisation for none of the outcomes and the coefficients are not significantly different from zero over the subsequent months (as one would expect given the lesser offence severity). However, we also do not see entirely flat patterns but a slight trend in the point estimates: We stress here that the sample of male victims of pickpocketing is

[30]Our results are robust to adding controls for moves. Similar to the case of violent crime, we find that some but not all of the earnings effects can be explained by changes at the extensive margin (Panel C, Appendix Figure B1). As for assault and threat, the estimates for earnings are attenuated when we restrict the sample to individuals with only one victimisation and those with no criminal record throughout the sample period (see Panel C, Appendix Table A2).

comparatively small (about the size of the sample of female sex offence victims, see Table 2). Likely, there is a selection process into reporting to the police and we are thus hesitant to give the pickpocketing results any strong interpretation but rather show them for completeness.

Even though most property crimes do not directly affect an individual's physical health (with the exception of violent burglaries/robberies), victims of property crimes could suffer from increased mental health problems. Panels A and B of Appendix Figure B5 show the results for total and mental health expenditure. For burglary, health expenditure is unaffected by the victimisation. For robbery, there is an increase in total health expenditure in the year of victimisation of 356 Euro (18%), largely driven by a surge in mental health expenditure of 323 Euro (45%) and consistent with the sharp increase in sickness/disability insurance benefits mentioned above. The increase in mental health expenditure is in line with the findings reported in Dustmann and Fasani (2016) who report a deterioration in mental health as a consequence of property crime exposure using aggregate-level data from the UK. Cornaglia *et al.* (2014) find that mental health is affected only by violent crimes (and not by property crimes). Note again that the different classification of robbery (as property or violent crime) across countries imposes some caveats to such direct comparisons.

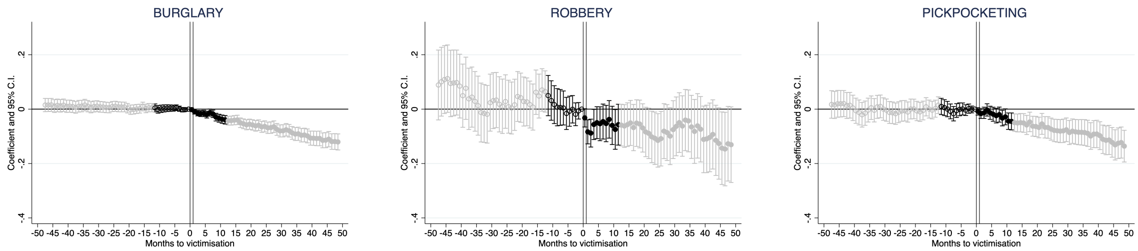
5.2 Females

Figure 6 shows the corresponding results for female victims of the three property crimes. The patterns are qualitatively quite similar: For burglary, earnings decrease by 0.8% one month after victimisation and by 2.6% twelve months after victimisation. In contrast to males, the increase in benefit receipt is significantly different from zero, but relatively small in magnitude (0.05 days one month and 0.11 twelve months after victimisation). While the confidence intervals are again wider for robbery, the point estimates suggest a clearer decrease in earnings and increase in benefits following the victimisation. As for male victims, there is no visible immediate decrease in earnings or increase in benefits for victims of pickpocketing. The pickpocketing sample for females is almost the same size as the burglary sample and once we have such a large (and potentially less selected) sample, the results for pickpocketing are, also in the long run, precisely estimated null effects.[31]

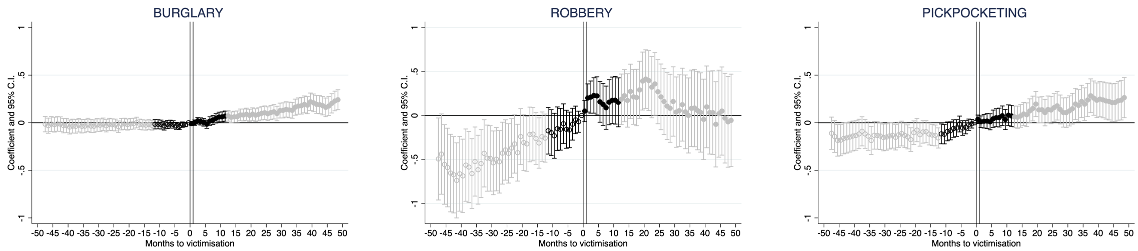
[31] The extensive margin results and benefit type splits can be found in Appendix Figures B1 and B3, respectively.

Figure 5: **Males - Property Crime**

Panel A. Log earnings



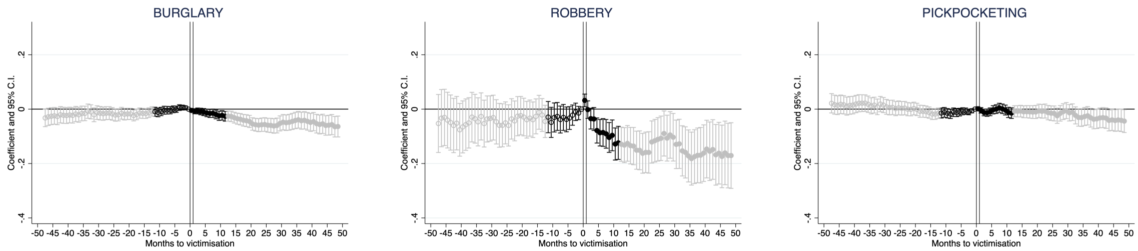
Panel B. Days of benefits



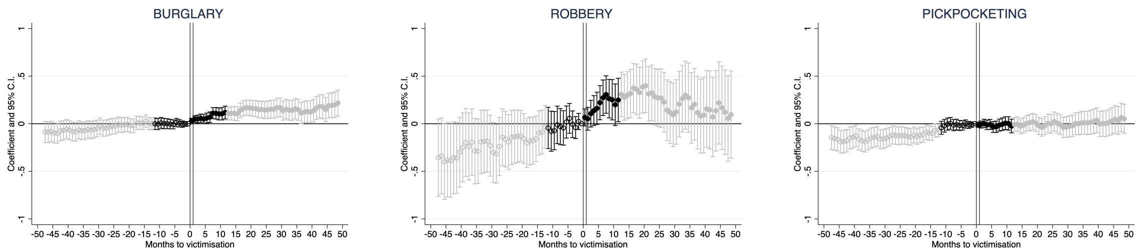
NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with log earnings (Panel A) and days of benefits (Panel B) as the dependent variable. The figures show results from left to right for burglary, robbery and pickpocketing. The solid vertical lines mark the start and end of the victimisation month (year). Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

Figure 6: **Females - Property Crime**

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 benefits (Panel B) as the dependent variable. The figures show results from left to right for burglary, robbery and pickpocketing. The solid vertical lines mark the start and end of the victimisation month (year). Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

Finally, for burglary there is a significant increase in the total health expenditure by 4% relative to the mean in the year after victimisation, accompanied by a 15% (18%) increase in mental health expenditure in the year of (after) the victimisation (Panels C and D, Appendix Figure B5). Again, our sample of burglaries includes those with violence and it is possible that these drive both the health effects and in turn the labour market responses. In terms of health expenditure, more visible is though an immediate decrease in the year of victimisation for pickpocketing, which might plausibly be due to behavioural changes and avoidance strategies.

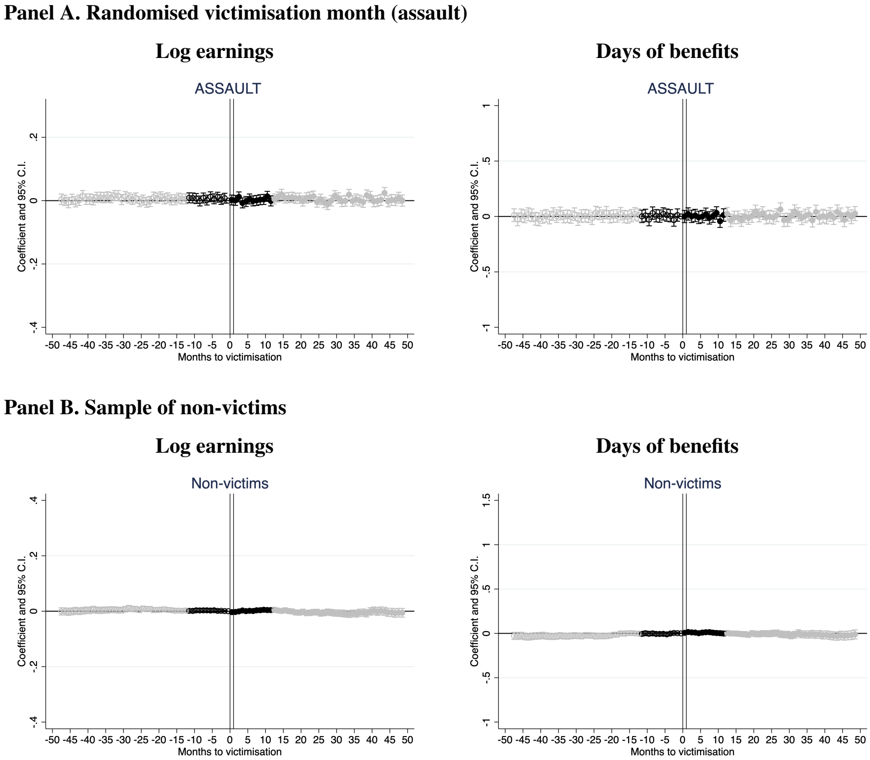
6 Falsification and Robustness Tests

Falsification tests. To rule out that our baseline specification picks up a spurious relationship between the month of victimisation and the outcome of interest (due to remaining time/cohort trends or economic shocks) we conduct two types of falsification exercises. First, we randomly draw a month from the list of potential victimisation months, assign that month as a placebo instead of the actual victimisation month and re-estimate equation (1), for simplicity pooling males and females. Second, we draw a 5% random sample of the non-victimised population, apply equivalent sample restrictions and assign a placebo month of victimisation. These two exercises allow us to test for remaining trends (or economic shocks) driving the main results which may not be covered by the (extensive) set of fixed effects in equation (1): If that was indeed the case and the estimations picked up a spurious relationship, we would expect to see some of this also in the placebo tests. Panels A and B of Figure 7 (same scale as for the main results) show the results for the first and second falsification test, respectively: The coefficients are generally not significantly different from and close to zero.[32]

Robustness tests. We conduct a number of robustness tests to assess whether our results are affected by alternative specifications or by changing the sample. We begin by including a linear trend. For identification that means that we have to omit one additional pre-victimisation month (Borusyak and Jaravel, 2017). Next, we estimate equation (1) for level instead of log earnings and we include age specific fixed effects instead of age group fixed effects. We alternate our sample definition in a number of ways: We start by dropping the last year of victimisations

[32]The same holds for the other offence categories; results are available on request.

Figure 7: **Falsification Tests**



NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the placebo regressions with log earnings on the left and days of benefits on the right as the dependent variable. The figures show results where the victimisation month is randomised for the original assault sample (panel A) and for a sample of non-victims (panel B). The solid vertical lines mark the start and end of the victimisation month. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

in the data (2016) as we do not observe the post-outcomes for months and years after the victimisation. Further, we drop individuals for whom we do not observe pre-outcomes (as they only enter the sample at the time of victimisation). These two tests allow us to assess whether the unbalancedness of the sample affects our estimates. Next, we drop two victimisation years at a time to show that our results are not driven by specific 'cohorts' of victims (e.g. those who become a victim of crime during the Great Recession). By specifically excluding victimisations in 2007 and 2008, we take one of our sample restrictions one step further: 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 restrict our sample to individuals who have not reported any victimisation within the previous *four* instead of *two* years, thereby relaxing this assumption. Further, by dropping two victimisation years at a time, we can assess to what extent there are heterogeneous treatment effects. Abraham and

Sun (2018) show that in dynamic event-studies the coefficients corresponding to a given lead or lag are a non-convex average of heterogeneous cohort-specific average treatment effects on the treated. If cohort-specific treatment effects are indeed heterogeneous, the coefficient corresponding to a given lead or lag could pick up spurious terms consisting of treatment effects from other periods. In our setting, cohort-specific treatment effects could for example arise if the effect of a victimisation during a recession is very different from that in a boom. By omitting two victimisation years at a time we can assess whether our results are sensitive to that.

The results of our robustness analyses are shown in Appendix Tables A3 to A8. As earlier, to ease comparison across the specifications we only report the coefficients for six months before, one month after and twelve months after victimisation, and include the baseline estimates as a benchmark. We show the results for earnings and benefits separately for males (columns 1 to 6) and females (columns 7-12). Adding a linear trend, we find that our results are mostly robust, despite this demanding specification - we impose a linear trend on top a number of fixed effects as detailed in equation (1). For level earnings, we find qualitatively similar results as for log earnings, as shown in the tables. The results are further robust to including specific age fixed effects instead of age group fixed effects (results available on request). In a previous version using annual data (Bindler and Ketel, 2019), we additionally demonstrated the robustness of our results to using neighbourhood instead of municipality fixed effects, which picks up on potential lower-regional level unobserved heterogeneity.[33] Finally, our results are robust to altering our sample choices. Neither dropping outcomes for 2016 (i.e. creating a control group that is never treated within the sample window) nor restricting the sample to individuals who are observed at least once before their victimisation nor leaving out two victimisation years at a time changes our conclusions. In other words, we do not find evidence of heterogeneous treatment effects by year of victimisation. Lastly, we apply the same age restriction to the violent crime samples as to the property crime samples (i.e. restrict to ages 26-55). With the exception of earnings for sex offences, our results are robust.[34]

[33] As we now use monthly instead of annual data, this exercise becomes computationally too demanding to replicate. We do not expect our conclusions regarding the robustness to neighbourhood fixed effects to change.

[34] It is worth highlighting here that the age-victimisation profile for sex offences peaks during teenage years. Excluding individuals below the age of 26 leaves us with a quite low number of observations.

7 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 for both males and females. Our main results show that criminal victimisation leads to statistically and economically significant losses in earnings (up to 10.4% for violent crime, 12.9% for property crime including robbery and 17.9% for domestic violence within the first year after victimisation) and increases in benefit dependency (up to 6% for violent crime, 4.3% for property crime and 41.7% for domestic violence). The losses in earnings and increases in benefit dependency persist over time (up to four years and longer), indicating that a victimisation is an event with long-lasting consequences. Further analyses suggest that later victimisations and criminal involvement as well as life-events (moves and family outcomes) may contribute to the persistent effects. Taken together, our results indicate that the observed victimisation is a life-changing event leading to an escalation point in the victim's life.

How do our results compare to findings in the literature? For assault (which is most comparable with existing estimates), we find a decrease in earnings by 7.5% for males (8.8% for females), and an increase in the number of days of benefit receipt by 2.9% (6%) 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. We conclude that our estimates lie within a reasonable range of the most comparable estimates.

Based on our estimates, the losses in terms of labour market outcomes alone are sizeable. Using a simple back-of-the-envelope calculation, we find that the average accumulated loss within the first year following an assault amounts to 73% of average monthly earnings for males, 161% for females and 137% for female domestic violence victims. In terms of benefits, the respective increases correspond to 32% of the average monthly number of days on benefits for males, 63% for females and 311% for female victims of domestic violence. In total, our results accumulate to about 366 million Euros losses in earnings and 1.45 million days of benefits within only the first year after an assault (based on 241,080 assault victims in our sample from 2007-2016). Given the sample restrictions needed for our analysis, the aggregate numbers have to be taken with a pinch of salt; yet, they provide an indication of the cost of victimisation when it comes to labour market outcomes.[35] In comparison, in 2012 the total Dutch expenditure on public and private safety (including prevention, policing, criminal justice and support of offenders and victims) was 13.1 billion Euros. Of this, 50.1 million Euros were 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 social cost of crime. Second, they speak to the non-trivial question of suitable compensation for victims: Are there labour market costs 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 (see e.g. Johnston *et al.*, 2018). We consider the results in our study as early causal evidence that victimisation has sizeable negative and lasting labor market consequences. 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. Moreover, the Netherlands has an in comparison generous welfare system - both in terms of health insurance and social welfare. While our study can not speak to this directly, one may only speculate whether the negative impacts of victimisation in other countries with less generous support systems, more inequality and/or different access to health care are larger than what we document here.

[35] Similar calculations can be provided for the other offences.

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A. Appendix Tables

Table A1: **Summary Statistics - Domestic Violence**

Offence:		Assault			Threat			Sex	
	DV (partner)	DV (ex-partner)	non-DV	DV (partner)	DV (ex-partner)	non-DV	DV (partner)	DV (ex-partner)	non-DV
Gender:	Female	Female	Female	Female	Female	Female	Female	Female	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Background characteristics (measured in the month of victimisation)									
Age	37	35	31	39	37	35	37	36	26
Immigrant	0.36	0.32	0.22	0.32	0.25	0.20	0.20	0.10	0.12
Partner (0/1)	1.00	0.05	0.25	0.99	0.08	0.35	0.99	0.17	0.23
Children (0/1)	0.77	0.78	0.33	0.79	0.76	0.43	0.80	0.81	0.24
Total nr. victimisations	1.49	1.52	1.41	1.64	1.63	1.39	1.51	1.45	1.29
One victimisation	0.70	0.69	0.74	0.64	0.65	0.75	0.67	0.73	0.80
Observations	16,866	5,802	83,349	3,248	4,031	59,730	289	324	36,978
Panel B. Monthly labour market and annual health outcomes									
Earnings (>0)	0.56	0.58	0.63	0.59	0.61	0.68	0.60	0.60	0.65
Earnings (in 2015 €)	996	1,022	1,104	1,085	1,136	1,407	1,012	1,060	1,076
Benefits (>0)	0.29	0.33	0.26	0.30	0.32	0.24	0.26	0.35	0.23
Days benefits	8.88	10.19	7.99	8.99	9.89	7.23	8.04	10.66	7.11
Total health costs (in €)	2,649	2,776	2,582	2,482	2,640	2,499	3,137	3,377	2,854
Mental health costs (in €)	641	712	740	509	585	570	892	1,160	1,122
Observations (NxT, Mio.)	2.9	1.1	11.3	0.59	0.77	9.3	0.05	0.06	4.3

NOTE- The table shows the sample means of the indicated variables for each offence/gender subsample as indicated at the top of each column. Panel A reports the (cross-sectional) background characteristics in the month of victimisation for all individuals in the respective sample; Panel B reports the (longitudinal) monthly labour market and additional outcomes (the health outcomes are at the annual level). See the data section for a description of how we classify domestic violence (DV) offences. SOURCE- Results based on calculations by the authors using microdata from Statistics Netherlands.

Table A2: **Multiple Victimisations and Criminal Record**

Outcome:		Log earnings			Days of benefits			Log earnings			Days of benefits	
Gender:		Males			Males			Females			Females	
t:	-6	+1	+12	-6	+1	+12	-6	+1	+12	-6	+1	+12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Assault												
Baseline	-0.015**	-0.045***	-0.075***	-0.000	0.156***	0.129***	-0.001	-0.038***	-0.088***	-0.103***	0.294***	0.481***
	(0.008)	(0.006)	(0.010)	(0.024)	(0.018)	(0.031)	(0.009)	(0.007)	(0.012)	(0.033)	(0.025)	(0.046)
Single victimisations	-0.014	-0.051***	-0.076***	-0.011	0.154***	0.109***	-0.017*	-0.032***	-0.067***	-0.042	0.256***	0.366***
	(0.008)	(0.006)	(0.011)	(0.025)	(0.020)	(0.034)	(0.010)	(0.008)	(0.014)	(0.037)	(0.028)	(0.052)
Ctrl for mult. victimisations	-0.015**	-0.046***	-0.075***	-0.000	0.155***	0.128***	-0.001	-0.035***	-0.086***	-0.103***	0.278***	0.474***
	(0.008)	(0.006)	(0.010)	(0.024)	(0.018)	(0.031)	(0.009)	(0.007)	(0.012)	(0.033)	(0.025)	(0.046)
No criminal record	-0.025***	-0.033***	-0.053***	0.047*	0.118***	0.107***	-0.009	-0.030***	-0.077***	-0.097***	0.267***	0.445***
	(0.008)	(0.006)	(0.011)	(0.024)	(0.018)	(0.033)	(0.009)	(0.007)	(0.013)	(0.034)	(0.026)	(0.048)
Panel B. Violent threat												
Baseline	0.005	-0.013**	-0.074***	0.019	0.053**	0.233***	-0.012	-0.025***	-0.104***	-0.066*	0.143***	0.384***
	(0.009)	(0.006)	(0.012)	(0.031)	(0.023)	(0.042)	(0.009)	(0.007)	(0.013)	(0.035)	(0.026)	(0.049)
Single victimisations	0.007	-0.015**	-0.068***	-0.000	0.070***	0.247***	-0.020*	-0.026***	-0.087***	-0.024	0.124***	0.305***
	(0.010)	(0.007)	(0.014)	(0.035)	(0.026)	(0.048)	(0.010)	(0.008)	(0.014)	(0.039)	(0.029)	(0.055)
Ctrl for mult. victimisations	0.005	-0.014**	-0.075***	0.019	0.058***	0.236***	-0.012	-0.022***	-0.103***	-0.066*	0.124***	0.378***
	(0.009)	(0.006)	(0.012)	(0.031)	(0.023)	(0.042)	(0.009)	(0.007)	(0.013)	(0.035)	(0.026)	(0.049)
No criminal record	-0.009	-0.005	-0.055***	0.048	0.047**	0.175***	-0.026***	-0.020***	-0.098***	-0.032	0.132***	0.384***
	(0.009)	(0.006)	(0.012)	(0.031)	(0.023)	(0.043)	(0.009)	(0.007)	(0.013)	(0.036)	(0.026)	(0.050)
Panel C. Burglary												
Baseline	0.004	-0.009**	-0.042***	-0.018	0.001	0.073**	-0.001	-0.008*	-0.026***	-0.007	0.049***	0.108***
	(0.006)	(0.004)	(0.008)	(0.020)	(0.015)	(0.028)	(0.006)	(0.005)	(0.010)	(0.024)	(0.017)	(0.035)
Single victimisations	0.004	-0.010**	-0.038***	-0.013	0.003	0.067**	-0.002	-0.006	-0.025**	-0.002	0.045**	0.108***
	(0.006)	(0.005)	(0.009)	(0.021)	(0.015)	(0.030)	(0.007)	(0.005)	(0.010)	(0.025)	(0.018)	(0.037)
Ctrl for mult. victimisations	0.004	-0.009**	-0.042***	-0.018	-0.000	0.072**	-0.001	-0.008	-0.026***	-0.007	0.047***	0.107***
	(0.006)	(0.004)	(0.008)	(0.020)	(0.015)	(0.028)	(0.006)	(0.005)	(0.010)	(0.024)	(0.017)	(0.035)
No criminal record	0.002	-0.007	-0.034***	-0.021	0.002	0.045	-0.001	-0.005	-0.018*	-0.003	0.041**	0.093***
	(0.006)	(0.004)	(0.008)	(0.020)	(0.014)	(0.028)	(0.006)	(0.005)	(0.010)	(0.024)	(0.017)	(0.035)

NOTE - The table shows the estimated coefficients and standard errors for the regressions corresponding to equation (1) using different specifications as indicated in the left column, for assault (Panel A), threat (Panel B) and burglary (Panel C). Standard errors are clustered by individual. *** p<0.01, ** p<0.05, * p<0.1. SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

Table A3: **Robustness Checks - Assault**

Outcome:		Log earnings			Days of benefits			Log earnings			Days of benefits	
Gender:		Males			Males			Females			Females	
t:	-6	+1	+12	-6	+1	+12	-6	+1	+12	-6	+1	+12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Baseline	-0.015**	-0.045***	-0.075***	-0.000	0.156***	0.129***	-0.001	-0.038***	-0.088***	-0.103***	0.294***	0.481***
	(0.008)	(0.006)	(0.010)	(0.024)	(0.018)	(0.031)	(0.009)	(0.007)	(0.012)	(0.033)	(0.025)	(0.046)
Linear trend	-0.011	-0.041***	-0.072***	0.039	0.193***	0.171***	0.020**	-0.017	-0.068***	0.150***	0.543***	0.735***
	(0.008)	(0.008)	(0.012)	(0.026)	(0.027)	(0.038)	(0.010)	(0.011)	(0.015)	(0.038)	(0.040)	(0.058)
Level earnings	-2.562	-9.435***	-19.240***	-	-	-	-0.838	-6.608***	-17.373***	-	-	-
	(2.381)	(1.681)	(3.523)				(2.075)	(1.470)	(3.119)			
Drop 2016 (outcomes)	-0.011	-0.044***	-0.078***	-0.002	0.164***	0.117***	-0.000	-0.038***	-0.094***	-0.101***	0.289***	0.474***
	(0.008)	(0.006)	(0.010)	(0.024)	(0.019)	(0.033)	(0.009)	(0.007)	(0.013)	(0.034)	(0.026)	(0.048)
Drop victims w/o pre-obs.	-0.018**	-0.045***	-0.087***	-0.031	0.160***	0.141***	0.002	-0.029***	-0.089***	-0.126***	0.292***	0.483***
	(0.008)	(0.006)	(0.010)	(0.024)	(0.019)	(0.034)	(0.009)	(0.007)	(0.012)	(0.034)	(0.026)	(0.049)
Drop 2007/08 victims	-0.020**	-0.051***	-0.073***	0.025	0.170***	0.141***	0.007	-0.041***	-0.109***	-0.145***	0.293***	0.572***
	(0.009)	(0.007)	(0.012)	(0.028)	(0.021)	(0.039)	(0.011)	(0.008)	(0.015)	(0.039)	(0.029)	(0.057)
Drop 2009/10 victims	-0.017**	-0.044***	-0.083***	0.001	0.137***	0.152***	0.003	-0.034***	-0.093***	-0.072*	0.302***	0.497***
	(0.009)	(0.007)	(0.011)	(0.027)	(0.020)	(0.036)	(0.010)	(0.008)	(0.014)	(0.037)	(0.028)	(0.053)
Drop 2011/12 victims	-0.017*	-0.045***	-0.075***	0.013	0.147***	0.130***	-0.006	-0.037***	-0.085***	-0.092**	0.298***	0.525***
	(0.009)	(0.007)	(0.011)	(0.027)	(0.020)	(0.036)	(0.010)	(0.008)	(0.014)	(0.037)	(0.029)	(0.052)
Drop 2013/14 victims	-0.009	-0.044***	-0.070***	-0.031	0.170***	0.120***	-0.001	-0.045***	-0.086***	-0.132***	0.306***	0.410***
	(0.008)	(0.006)	(0.011)	(0.025)	(0.020)	(0.035)	(0.010)	(0.008)	(0.014)	(0.036)	(0.028)	(0.052)
Drop 2015/16 victims	-0.016*	-0.044***	-0.072***	-0.001	0.156***	0.100***	-0.004	-0.035***	-0.070***	-0.084**	0.273***	0.430***
	(0.008)	(0.006)	(0.010)	(0.026)	(0.020)	(0.034)	(0.010)	(0.007)	(0.013)	(0.036)	(0.027)	(0.050)
Ages 26-55	-0.011	-0.037***	-0.075***	0.043	0.146***	0.210***	-0.001	-0.026***	-0.075***	-0.112***	0.323***	0.576***
	(0.009)	(0.006)	(0.012)	(0.033)	(0.025)	(0.046)	(0.010)	(0.007)	(0.014)	(0.042)	(0.032)	(0.060)

NOTE - The table shows the estimated coefficients and standard errors for the regressions corresponding to equation (1) using different specifications as indicated in the left column by gender and for the outcomes log earnings and days of benefits. Standard errors are clustered by individual. *** p<0.01, ** p<0.05, * p<0.1. SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

Table A4: **Robustness Checks - Violent Threat**

Outcome:	Log earnings			Days of benefits			Log earnings			Days of benefits		
Gender:	Males			Males			Females			Females		
t:	-6	+1	+12	-6	+1	+12	-6	+1	+12	-6	+1	+12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Baseline	0.005	-0.013**	-0.074***	0.019	0.053**	0.233***	-0.012	-0.025***	-0.104***	-0.066*	0.143***	0.384***
	(0.009)	(0.006)	(0.012)	(0.031)	(0.023)	(0.042)	(0.009)	(0.007)	(0.013)	(0.035)	(0.026)	(0.049)
Linear trend	-0.009	-0.027***	-0.089***	0.013	0.046	0.230***	0.016	0.003	-0.078***	0.054	0.261***	0.507***
	(0.010)	(0.010)	(0.014)	(0.034)	(0.036)	(0.050)	(0.011)	(0.012)	(0.016)	(0.041)	(0.043)	(0.062)
Level earnings	6.326*	-7.365***	-36.117***	-	-	-	-6.809***	-6.328***	-36.838***	-	-	-
	(3.567)	(2.516)	(5.413)				(2.459)	(1.666)	(3.746)			
Drop 2016 (outcomes)	0.008	-0.013*	-0.077***	0.018	0.049**	0.241***	-0.007	-0.026***	-0.103***	-0.076**	0.129***	0.346***
	(0.009)	(0.007)	(0.012)	(0.032)	(0.024)	(0.044)	(0.010)	(0.007)	(0.013)	(0.037)	(0.027)	(0.052)
Drop victims w/o pre-obs.	0.004	-0.012**	-0.078***	0.009	0.053**	0.213***	-0.016*	-0.023***	-0.108***	-0.074**	0.141***	0.390***
	(0.009)	(0.006)	(0.012)	(0.031)	(0.023)	(0.042)	(0.009)	(0.007)	(0.013)	(0.035)	(0.026)	(0.051)
Drop 2007/08 victims	0.003	-0.013*	-0.080***	-0.008	0.053**	0.212***	-0.010	-0.029***	-0.103***	-0.050	0.159***	0.448***
	(0.010)	(0.007)	(0.013)	(0.035)	(0.025)	(0.049)	(0.010)	(0.007)	(0.015)	(0.039)	(0.029)	(0.058)
Drop 2009/10 victims	0.013	-0.017**	-0.070***	0.006	0.063**	0.195***	-0.017*	-0.018**	-0.099***	-0.042	0.150***	0.395***
	(0.010)	(0.007)	(0.013)	(0.035)	(0.026)	(0.048)	(0.010)	(0.008)	(0.014)	(0.040)	(0.029)	(0.057)
Drop 2011/12 victims	0.006	-0.001	-0.068***	0.027	0.049*	0.257***	-0.016	-0.023***	-0.115***	-0.103**	0.145***	0.381***
	(0.010)	(0.007)	(0.014)	(0.035)	(0.026)	(0.048)	(0.010)	(0.008)	(0.015)	(0.040)	(0.030)	(0.056)
Drop 2013/14 victims	0.001	-0.017**	-0.076***	0.043	0.060**	0.251***	-0.012	-0.026***	-0.105***	-0.067*	0.141***	0.361***
	(0.010)	(0.007)	(0.013)	(0.034)	(0.025)	(0.047)	(0.010)	(0.007)	(0.014)	(0.039)	(0.029)	(0.055)
Drop 2015/16 victims	0.005	-0.015**	-0.073***	0.028	0.039	0.241***	-0.007	-0.027***	-0.093***	-0.066*	0.122***	0.330***
	(0.009)	(0.007)	(0.013)	(0.034)	(0.025)	(0.045)	(0.010)	(0.007)	(0.014)	(0.039)	(0.029)	(0.054)
Ages 26-55	0.008	-0.004	-0.052***	0.024	0.034	0.219***	-0.009	-0.014**	-0.073***	-0.072*	0.144***	0.361***
	(0.009)	(0.006)	(0.013)	(0.034)	(0.025)	(0.047)	(0.009)	(0.007)	(0.014)	(0.039)	(0.029)	(0.057)

NOTE - The table shows the estimated coefficients and standard errors for the regressions corresponding to equation (1) using different specifications as indicated in the left column by gender and for the outcomes log earnings and days of benefits. Standard errors are clustered by individual. *** p<0.01, ** p<0.05, * p<0.1. SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

Table A5: **Robustness Checks - Sex Offences**

Outcome:		Log earnings			Days of benefits	
Gender:		Females			Females	
t:	-6	+1	+12	-6	+1	+12
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline	-0.017	-0.022**	-0.049**	0.039	0.071**	0.333***
	(0.014)	(0.011)	(0.019)	(0.047)	(0.034)	(0.066)
Linear trend	0.036**	0.030*	0.002	0.092*	0.124**	0.392***
	(0.016)	(0.017)	(0.024)	(0.054)	(0.057)	(0.082)
Level earnings	-4.937	-2.649	-8.514*	-	-	-
	(3.225)	(2.269)	(4.980)			
Drop 2016 (outcomes)	-0.019	-0.027**	-0.064***	0.031	0.088**	0.342***
	(0.015)	(0.011)	(0.020)	(0.049)	(0.036)	(0.069)
Drop victims w/o pre-obs.	-0.015	-0.019*	-0.055***	-0.022	0.075**	0.350***
	(0.014)	(0.011)	(0.020)	(0.048)	(0.036)	(0.070)
Drop 2007/08 victims	-0.014	-0.016	-0.040*	0.066	0.067*	0.341***
	(0.017)	(0.012)	(0.023)	(0.055)	(0.040)	(0.082)
Drop 2009/10 victims	-0.017	-0.024**	-0.046**	-0.018	0.070*	0.332***
	(0.016)	(0.012)	(0.022)	(0.053)	(0.039)	(0.076)
Drop 2011/12 victims	-0.018	-0.028**	-0.060***	0.058	0.063*	0.312***
	(0.016)	(0.012)	(0.021)	(0.051)	(0.038)	(0.073)
Drop 2013/14 victims	-0.014	-0.013	-0.037*	0.039	0.069*	0.324***
	(0.016)	(0.012)	(0.021)	(0.051)	(0.038)	(0.074)
Drop 2015/16 victims	-0.022	-0.029**	-0.056***	0.052	0.083**	0.367***
	(0.016)	(0.012)	(0.020)	(0.051)	(0.038)	(0.071)
Ages 26-55	-0.032**	-0.003	-0.021	0.026	0.080*	0.327***
	(0.015)	(0.011)	(0.023)	(0.061)	(0.045)	(0.088)

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

Table A6: **Robustness Checks - Burglary**

Outcome:		Log earnings			Days of benefits			Log earnings			Days of benefits	
Gender:		Males			Males			Females			Females	
t:	-6	+1	+12	-6	+1	+12	-6	+1	+12	-6	+1	+12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Baseline	0.004	-0.009**	-0.042***	-0.018	0.001	0.073**	-0.001	-0.008*	-0.026***	-0.007	0.049***	0.108***
	(0.006)	(0.004)	(0.008)	(0.020)	(0.015)	(0.028)	(0.006)	(0.005)	(0.010)	(0.024)	(0.017)	(0.035)
Linear trend	-0.004	-0.017**	-0.050***	-0.010	0.009	0.084**	0.015*	0.007	-0.011	0.037	0.093***	0.153***
	(0.007)	(0.007)	(0.010)	(0.023)	(0.024)	(0.034)	(0.008)	(0.009)	(0.013)	(0.029)	(0.031)	(0.045)
Level earnings	8.046**	-4.873*	-27.979***	-	-	-	0.343	-2.504	-9.674**	-	-	-
	(3.722)	(2.849)	(5.779)				(2.408)	(1.775)	(3.762)			
Drop 2016 (outcomes)	0.003	-0.011**	-0.043***	-0.014	-0.005	0.069**	-0.004	-0.011**	-0.031***	-0.006	0.049***	0.097***
	(0.006)	(0.005)	(0.009)	(0.021)	(0.015)	(0.030)	(0.007)	(0.005)	(0.010)	(0.025)	(0.018)	(0.036)
Drop victims w/o pre-obs.	0.004	-0.012***	-0.041***	-0.015	0.003	0.069**	-0.002	-0.010**	-0.026***	-0.011	0.043**	0.120***
	(0.006)	(0.004)	(0.008)	(0.020)	(0.015)	(0.029)	(0.006)	(0.005)	(0.010)	(0.024)	(0.017)	(0.035)
Drop 2007/08 victims	0.008	-0.007	-0.035***	-0.027	0.006	0.080**	0.001	-0.010**	-0.022**	-0.010	0.051***	0.107***
	(0.007)	(0.005)	(0.009)	(0.023)	(0.017)	(0.034)	(0.007)	(0.005)	(0.011)	(0.027)	(0.020)	(0.041)
Drop 2009/10 victims	0.001	-0.012**	-0.050***	0.006	-0.001	0.063*	-0.001	-0.009*	-0.031***	-0.032	0.043**	0.138***
	(0.006)	(0.005)	(0.009)	(0.023)	(0.017)	(0.032)	(0.007)	(0.005)	(0.011)	(0.027)	(0.020)	(0.040)
Drop 2011/12 victims	0.005	-0.009*	-0.045***	-0.033	-0.000	0.054*	-0.003	-0.008	-0.028**	0.027	0.047**	0.115***
	(0.007)	(0.005)	(0.010)	(0.023)	(0.017)	(0.033)	(0.007)	(0.005)	(0.011)	(0.028)	(0.020)	(0.040)
Drop 2013/14 victims	0.003	-0.005	-0.039***	-0.023	0.004	0.088***	0.001	-0.007	-0.017	-0.006	0.059***	0.080**
	(0.006)	(0.005)	(0.009)	(0.022)	(0.016)	(0.032)	(0.007)	(0.005)	(0.011)	(0.026)	(0.019)	(0.039)
Drop 2015/16 victims	0.002	-0.014***	-0.045***	-0.013	-0.005	0.083***	-0.001	-0.005	-0.031***	-0.012	0.047**	0.099***
	(0.006)	(0.005)	(0.009)	(0.021)	(0.016)	(0.030)	(0.007)	(0.005)	(0.010)	(0.026)	(0.019)	(0.038)

NOTE - The table shows the estimated coefficients and standard errors for the regressions corresponding to equation (1) using different specifications as indicated in the left column by gender and for the outcomes log earnings and days of benefits. Standard errors are clustered by individual. *** p<0.01, ** p<0.05, * p<0.1. SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

Table A7: **Robustness Checks - Pickpocketing**

Outcome:		Log earnings			Days of benefits			Log earnings			Days of benefits	
Gender:		Males			Males			Females			Females	
t:	-6	+1	+12	-6	+1	+12	-6	+1	+12	-6	+1	+12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Baseline	-0.001	-0.016*	-0.050***	-0.013	0.010	0.062	-0.011	-0.004	-0.008	-0.018	-0.008	-0.009
	(0.012)	(0.009)	(0.016)	(0.042)	(0.031)	(0.058)	(0.007)	(0.005)	(0.010)	(0.027)	(0.019)	(0.038)
Linear trend	-0.009	-0.024*	-0.058***	0.039	0.062	0.119*	-0.021**	-0.014	-0.018	0.050	0.060*	0.060
	(0.014)	(0.014)	(0.020)	(0.047)	(0.048)	(0.068)	(0.009)	(0.010)	(0.014)	(0.032)	(0.034)	(0.049)
Level earnings	12.338*	-11.769**	-25.412***	-	-	-	-2.821	-1.902	-4.319	-	-	-
	(6.649)	(4.833)	(9.627)				(2.578)	(1.821)	(3.835)			
Drop 2016 (outcomes)	0.004	-0.018*	-0.044**	-0.011	0.011	0.059	-0.012	-0.005	-0.013	-0.013	-0.010	-0.017
	(0.013)	(0.010)	(0.017)	(0.045)	(0.033)	(0.061)	(0.007)	(0.005)	(0.011)	(0.028)	(0.020)	(0.039)
Drop victims w/o pre-obs.	0.002	-0.015*	-0.059***	-0.010	0.005	0.058	-0.010	-0.007	-0.012	-0.020	-0.004	-0.003
	(0.012)	(0.009)	(0.017)	(0.043)	(0.032)	(0.061)	(0.007)	(0.005)	(0.010)	(0.027)	(0.019)	(0.039)
Drop 2007/08 victims	0.001	-0.014	-0.070***	0.011	0.009	0.127*	-0.011	-0.004	-0.012	-0.036	-0.011	0.006
	(0.014)	(0.010)	(0.019)	(0.049)	(0.035)	(0.069)	(0.008)	(0.006)	(0.012)	(0.031)	(0.021)	(0.045)
Drop 2009/10 victims	-0.010	-0.015	-0.032*	-0.008	0.014	0.032	-0.011	-0.005	0.001	-0.015	-0.001	0.020
	(0.013)	(0.010)	(0.018)	(0.047)	(0.035)	(0.065)	(0.008)	(0.006)	(0.012)	(0.030)	(0.021)	(0.043)
Drop 2011/12 victims	-0.001	-0.022**	-0.053***	-0.080*	-0.009	0.012	-0.015*	-0.007	-0.013	-0.017	0.001	-0.010
	(0.014)	(0.010)	(0.019)	(0.048)	(0.035)	(0.066)	(0.008)	(0.006)	(0.012)	(0.031)	(0.022)	(0.043)
Drop 2013/14 victims	0.001	-0.021**	-0.063***	0.001	0.029	0.108	-0.005	-0.002	-0.004	-0.024	-0.019	-0.022
	(0.014)	(0.011)	(0.019)	(0.048)	(0.036)	(0.067)	(0.008)	(0.006)	(0.012)	(0.031)	(0.021)	(0.043)
Drop 2015/16 victims	0.007	-0.012	-0.035*	-0.001	0.012	0.043	-0.011	-0.004	-0.010	-0.005	-0.008	-0.030
	(0.013)	(0.010)	(0.018)	(0.047)	(0.034)	(0.063)	(0.008)	(0.006)	(0.011)	(0.029)	(0.020)	(0.040)

NOTE - The table shows the estimated coefficients and standard errors for the regressions corresponding to equation (1) using different specifications as indicated in the left column by gender and for the outcomes log earnings and days of benefits. Standard errors are clustered by individual. *** p<0.01, ** p<0.05, * p<0.1. SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

Table A8: **Robustness Checks - Robbery**

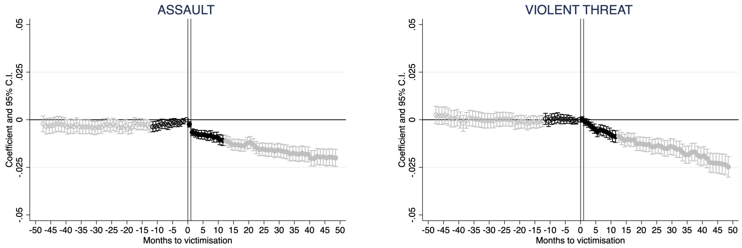
Outcome:		Log earnings			Days of benefits			Log earnings			Days of benefits	
Gender:		Males			Males			Females			Females	
t:	-6	+1	+12	-6	+1	+12	-6	+1	+12	-6	+1	+12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Baseline	-0.016	-0.083***	-0.059	-0.157	0.199**	0.184	-0.038	-0.002	-0.129***	0.018	0.052	0.308***
	(0.030)	(0.023)	(0.039)	(0.110)	(0.082)	(0.147)	(0.023)	(0.016)	(0.032)	(0.086)	(0.061)	(0.120)
Linear trend	-0.055	-0.122***	-0.099**	0.059	0.413***	0.408**	-0.014	0.021	-0.107**	0.180*	0.215**	0.472***
	(0.034)	(0.035)	(0.048)	(0.121)	(0.126)	(0.178)	(0.028)	(0.029)	(0.041)	(0.102)	(0.108)	(0.154)
Level earnings	-7.726	-31.053***	-6.575	-	-	-	-12.051	-0.704	-23.449**	-	-	-
	(11.976)	(8.949)	(16.948)				(8.078)	(5.936)	(11.840)			
Drop 2016 (outcomes)	-0.011	-0.089***	-0.058	-0.153	0.212**	0.179	-0.038	-0.007	-0.136***	0.012	0.071	0.298**
	(0.031)	(0.023)	(0.041)	(0.113)	(0.084)	(0.150)	(0.024)	(0.017)	(0.033)	(0.088)	(0.063)	(0.123)
Drop victims w/o pre-obs.	-0.019	-0.075***	-0.068*	-0.152	0.208**	0.102	-0.034	0.005	-0.137***	0.003	0.064	0.365***
	(0.030)	(0.023)	(0.041)	(0.114)	(0.084)	(0.156)	(0.023)	(0.016)	(0.033)	(0.087)	(0.063)	(0.126)
Drop 2007/08 victims	-0.019	-0.112***	-0.094*	-0.147	0.251**	0.206	-0.015	0.003	-0.151***	-0.021	0.048	0.334**
	(0.037)	(0.028)	(0.050)	(0.139)	(0.102)	(0.193)	(0.028)	(0.020)	(0.041)	(0.107)	(0.073)	(0.156)
Drop 2009/10 victims	0.002	-0.081***	-0.087*	-0.170	0.220**	0.155	-0.060**	-0.005	-0.131***	0.120	0.056	0.412***
	(0.034)	(0.026)	(0.045)	(0.127)	(0.092)	(0.168)	(0.027)	(0.019)	(0.036)	(0.096)	(0.069)	(0.136)
Drop 2011/12 victims	-0.028	-0.078***	-0.074*	-0.190	0.088	0.298*	-0.042	-0.001	-0.116***	0.014	0.061	0.289**
	(0.034)	(0.025)	(0.044)	(0.120)	(0.089)	(0.164)	(0.027)	(0.018)	(0.036)	(0.097)	(0.070)	(0.134)
Drop 2013/14 victims	-0.022	-0.072***	-0.006	-0.142	0.218**	0.032	-0.034	-0.000	-0.124***	0.036	0.029	0.216
	(0.033)	(0.025)	(0.044)	(0.120)	(0.091)	(0.165)	(0.025)	(0.018)	(0.036)	(0.095)	(0.067)	(0.132)
Drop 2015/16 victims	-0.003	-0.082***	-0.059	-0.177	0.232***	0.250*	-0.033	-0.006	-0.130***	-0.046	0.065	0.284**
	(0.032)	(0.024)	(0.041)	(0.118)	(0.086)	(0.150)	(0.025)	(0.017)	(0.034)	(0.091)	(0.065)	(0.125)

NOTE - The table shows the estimated coefficients and standard errors for the regressions corresponding to equation (1) using different specifications as indicated in the left column by gender and for the outcomes log earnings and days of benefits. Standard errors are clustered by individual. *** p<0.01, ** p<0.05, * p<0.1. SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

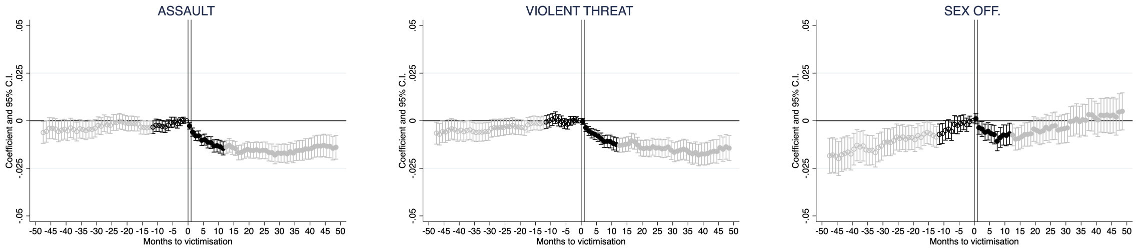
B. Appendix Figures

Figure B1: **Extensive Margin Results - Earnings**

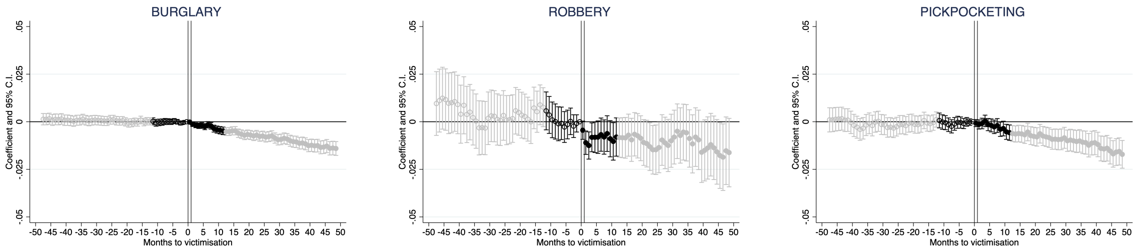
Panel A. Violent Crime - Males



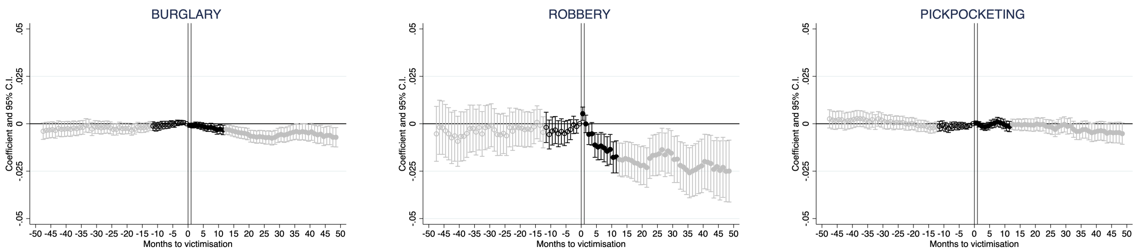
Panel B. Violent Crime - Females



Panel C. Property Crime - Males



Panel D. Property Crime - Females

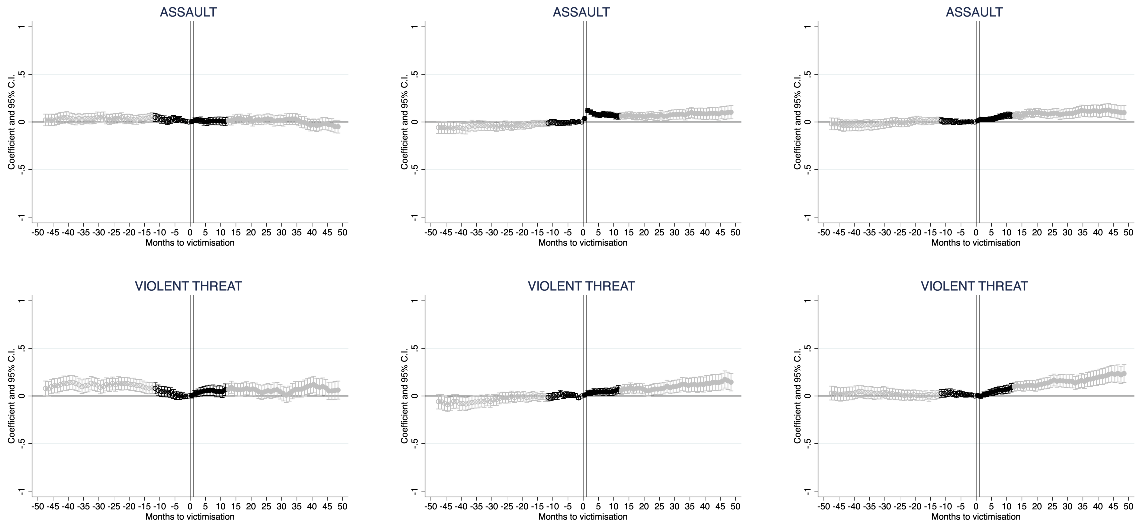


NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with any earnings as the dependent variable. The figures show results for violent crime for males in Panel A and females in Panel B, and for property crime for males in Panel C and females in Panel D. The two solid vertical lines mark the start and end of the victimisation month. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

Figure B2: **Split by Benefit Type - Males**

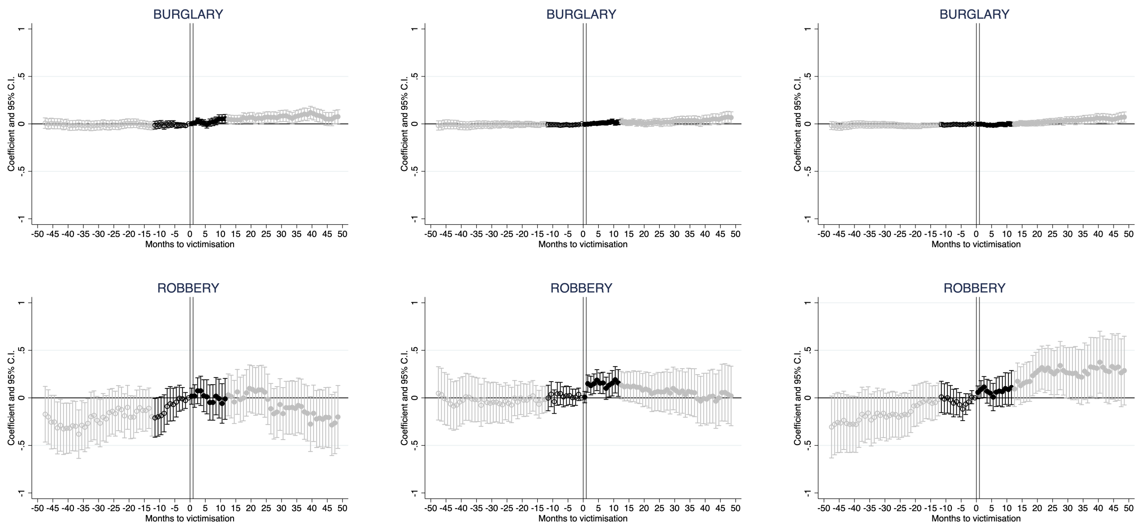
Panel A. Days of Benefits - Violent Crimes

Unemployment Insurance | **Disability Insurance** | **Welfare**



Panel B. Days of Benefits - Property Crimes

Unemployment Insurance | **Disability Insurance** | **Welfare**

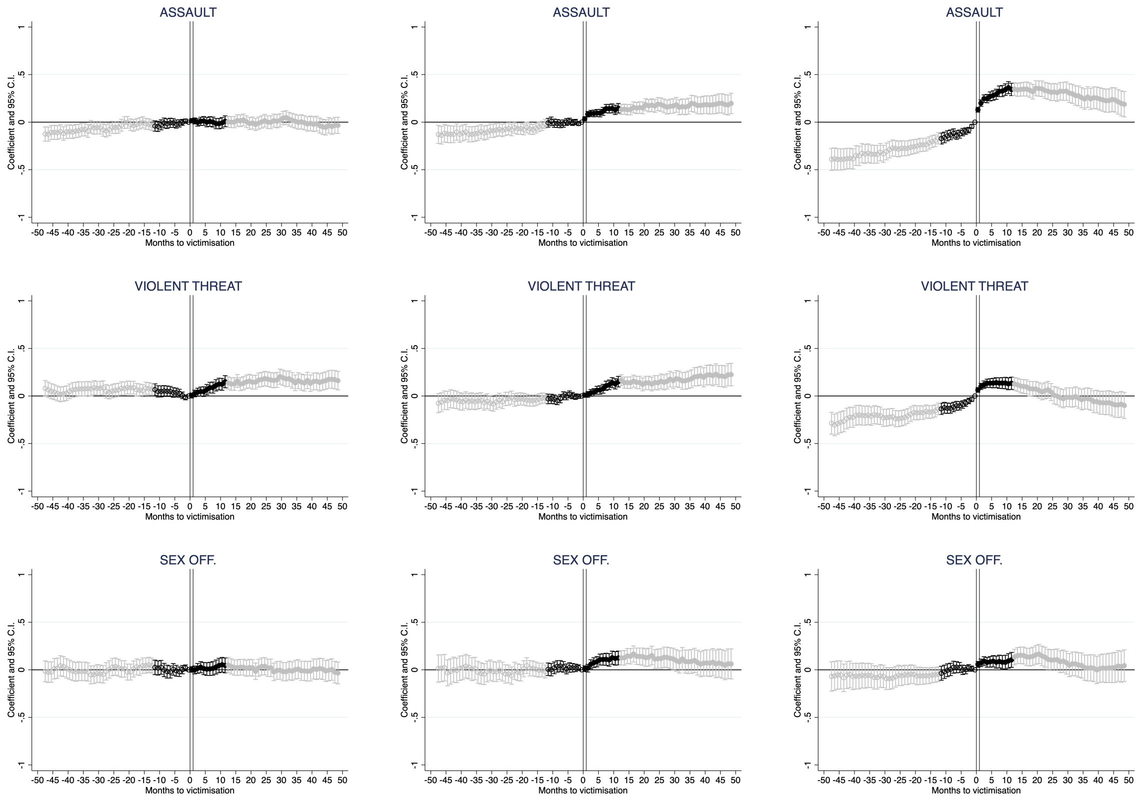


NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with from left to right unemployment insurance, disability insurance and welfare (all measured in days per month) as the dependent variable. Panel A shows results for assault and violent threat while panel B shows the results for burglary and robbery. The two solid vertical lines mark the start and end of the victimisation month. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

Figure B3: **Split by Benefit Type - Females**

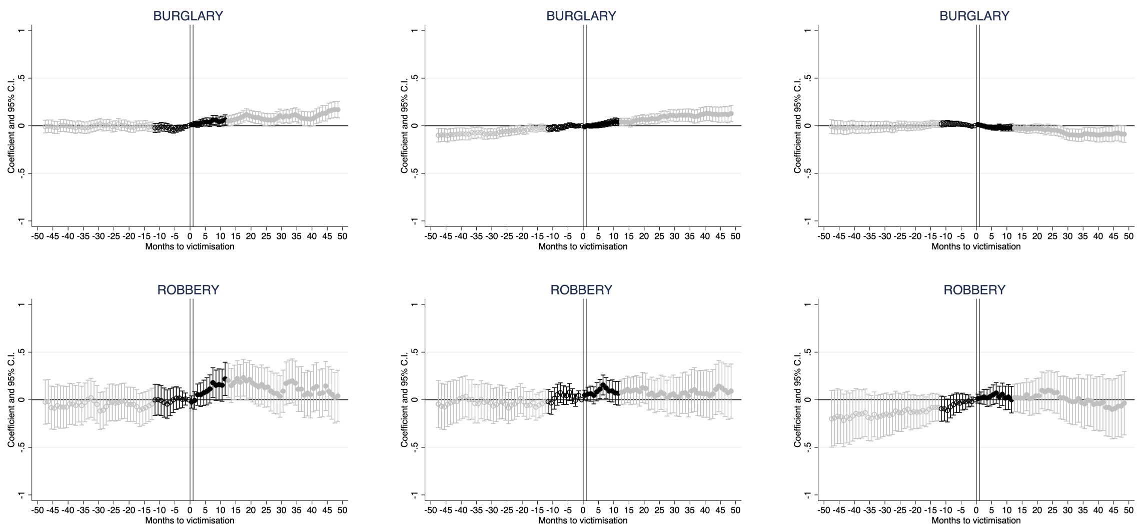
Panel A. Days of Benefits - Violent Crimes

Unemployment Insurance	Disability Insurance	Welfare



Panel B. Days of Benefits - Property Crimes

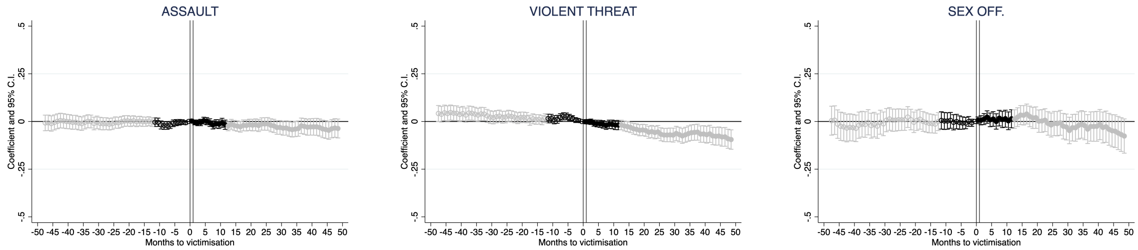
Unemployment Insurance	Disability Insurance	Welfare



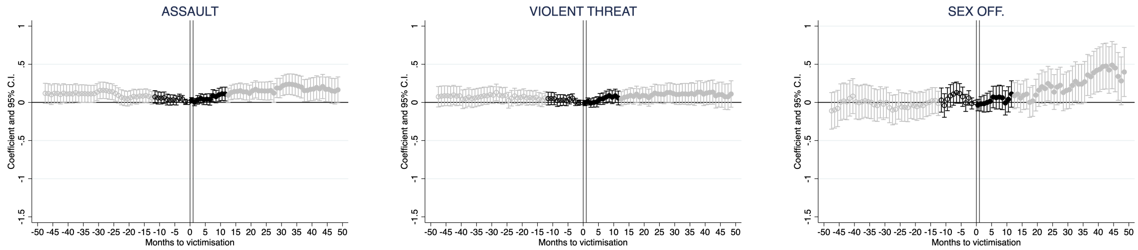
NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with from left to right unemployment insurance, disability insurance and welfare (all measured in days per month) as the dependent variable. Panel A shows results for assault, violent threat and sex offences while panel B shows the results for burglary and robbery. The two solid vertical lines mark the start and end of the victimisation month. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

Figure B4: **Household Spillovers (Violent Crime)**

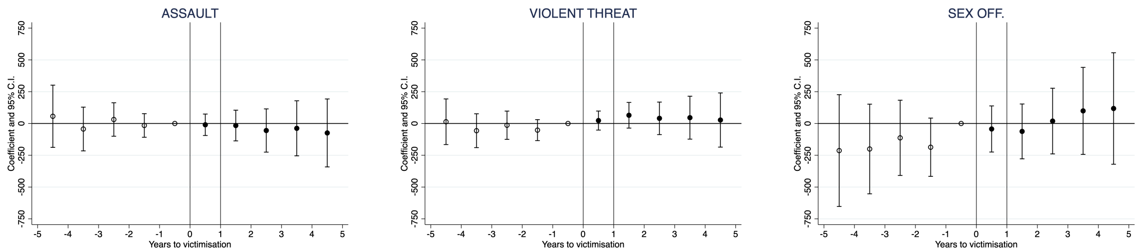
Panel A. Log Earnings



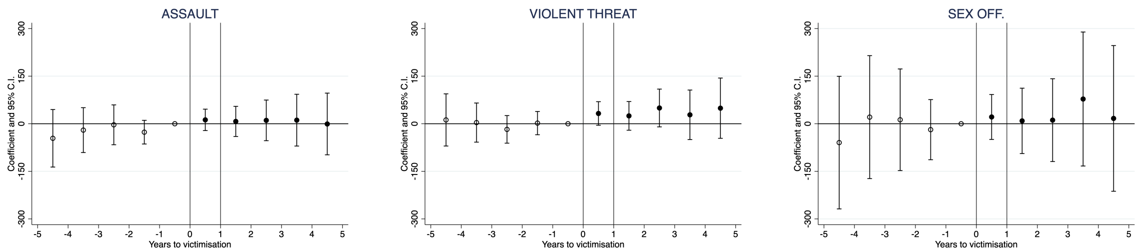
Panel B. Days of Benefits



Panel C. Total Health Expenditure



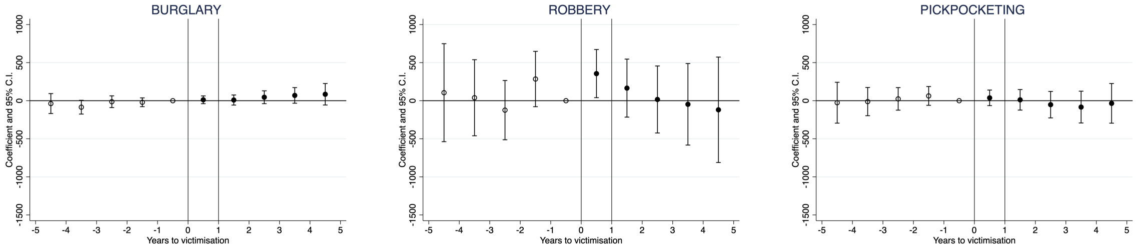
Panel D. Mental Health Expenditure



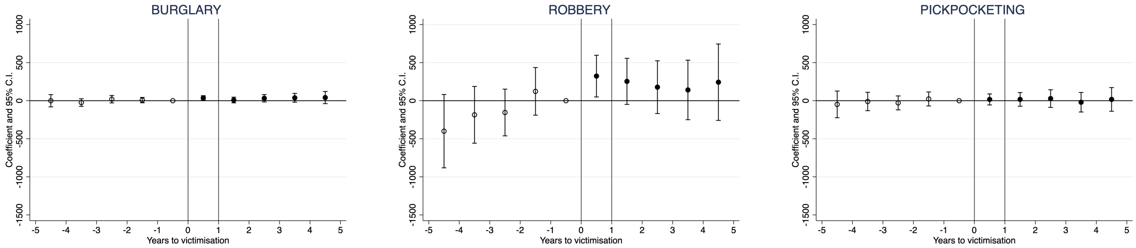
NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the household spillover regressions with log earnings (Panel A), days of benefits (Panel B), total health expenditure (Panel C) and mental health expenditure (Panel D) as the dependent variable. The figures show results from left to right for assault, violent threat and sex offences. The solid vertical lines mark the start and end of the victimisation month/year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.

Figure B5: **Health Expenditure (Property Crime)**

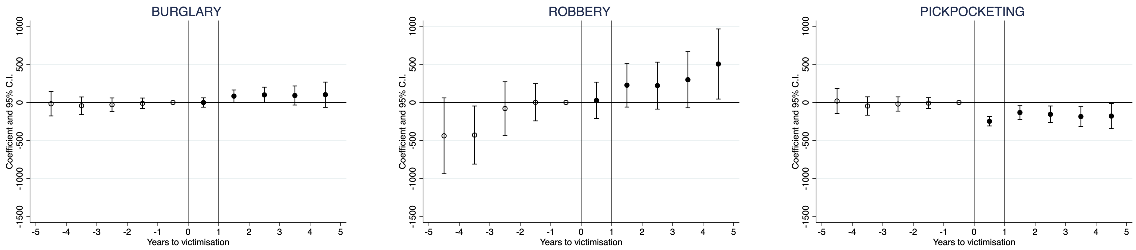
Panel A. Total Health Expenditure - Males



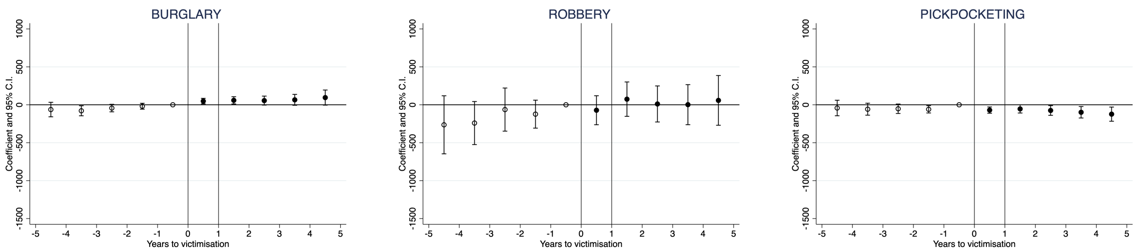
Panel B. Mental Health Expenditure - Males



Panel C. Total Health Expenditure - Females



Panel D. Mental Health Expenditure - Females



NOTE - The figure plots the estimated coefficients and 95% confidence intervals for the regressions corresponding to equation (1) with total health expenditure (Panels A and C) and mental health expenditure (Panels B and D) as the dependent variable. The figures show results from left to right for burglary, robbery and pickpocketing. The solid vertical lines mark the start and end of the victimisation month/year. Standard errors are clustered by individual. SOURCE - Results based on calculations by the authors using microdata from Statistics Netherlands.