Scaring or Scarring? Labor Market Effects of Criminal Victimization

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Little is known about the costs of crime to victims. We use unique and detailed register data on victimizations and monthly labor market outcomes from the Netherlands and estimate event study designs to assess short- and long-term effects of criminal victimization. Across offenses, both males and females experience significant decreases in earnings (up to $-12.9\%$) and increases in benefit receipt (up to $+6\%$) after victimization. The negative labor market responses are lasting (up to 4 years) and accompanied by shorter-lived responses in health expenditure. Heterogeneity results suggest that most groups of victims, including the noninjured, suffer nontrivial losses.

I. 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

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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 spillover effects on their families—the same cannot be said for victim-related costs.¹ Yet this existing knowledge gap could possibly lead to an underestimate of the overall social costs of crime: sizeable population shares around the world are exposed to crime directly as victims, and many more are exposed indirectly through their family and neighborhood relations to victims.² One important reason for this gap in the literature is the lack of high-quality micro-level victimization data, with the existing literature relying on small-scale survey data, aggregate crime data, or (more selective) hospitalization data to measure or proxy criminal victimization. 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 victimization on individuals’ labor market outcomes, including earnings (labor income) and social benefit receipt? Second, are labor market effects temporary, or do they persist over time? Specifically, we consider short-run ("scaring") effects, after which labor market outcomes would return to previctimization levels, and long-run ("scarring") effects, which would lead to a more persistent change in labor market trajectories. Third, why do these effects exist? We shed some light on potential mechanisms by studying health-related expenditures, heterogeneities by gender, and offense characteristics as well as other life events after victimization. We overcome the data limitations previously met in the literature by exploiting unique administrative data on victimization from Dutch police records that can be linked to an 18-year-long panel of labor market register data. Finally, when the offender is known to the police, we observe whether the victim and the offender live (or have lived) in the same household. This allows us to separate out domestic violence cases, which are not registered as a separate offense by the police. This is, to our knowledge, the first study that uses victimization register data to study these questions.

To date, there is scarce empirical evidence identifying the causal impacts of victimization and their relative contribution to the overall costs of crime.

¹ The emphasis on offenders compared with victims of crime in the empirical economics of crime literature (for recent reviews, see, e.g., Nagin 2013; Draca and Machin 2015; Chalfin and McCrary 2017) is paralleled by comparably more attention on (potential) offenders and their decision-making in theoretical economics of crime work, often based on the seminal Becker (1968) model.

² For reviews on the cost of crime, see, e.g., Heaton (2010), Chalfin (2015), or Soares (2015).
(see the discussion in Bindler, Hjalmarsson, and Ketel 2020). The small existing literature focuses on questions that differ from ours and addresses topics such as behavioral responses to criminal victimization, risk perceptions, and changes in mental health and subjective well-being. This literature mostly relies on fixed effects approaches to identify the effects of victimization on these outcomes. Two quite recent studies estimate the effect of crime on mental health. Using four waves of Australian survey data, Cornaglia, Feldman, and Leigh (2014) find that violent (but not property) crime has a negative impact on mental health for both victims and nonvictims. In contrast, Dustmann and Fasani (2016) find for the United Kingdom that local property crimes cause mental distress, with stronger effects for females. The literature also suggests negative effects of victimization on subjective well-being and life satisfaction (e.g., Cohen 2008; Johnston, Shields, and Suziedelyte 2018). Currie, Mueller-Smith, and Rossin-Slater (2022) use data on crime incidents to study the effects of violent assaults during pregnancy. They report evidence of lower birth weights and a higher likelihood of preterm births. Koppensteiner and Menezes (2020) study a similar question using administrative data on victimizations and birth outcomes from Brazil. To the best of our knowledge, this is the only other paper using administrative victimization data to estimate causal consequences of crime. In this paper, we build on the above-described literature: if victimizations change behavior, risk perceptions, mental health, and subjective well-being, then questions about their labor market impacts naturally follow. Our study is among the first to address the effect of victimization on these core economic outcomes.

We are aware of only two studies that investigate related questions. Ornstein (2017) uses Swedish register data to study the effect of an assault on mortality, health, and labor market outcomes. Victimization is measured using hospital records, which is selective on both offense type and severity (i.e., assaults severe enough to result in a hospital visit). Velamuri and Stillman (2008) estimate the effect of criminal victimization on labor market outcomes (and well-being) with four waves of Australian survey data; of the resulting analysis
sample (42,945 observations), just 725 and 2,490 observations are for victims of violent and property crime, respectively. Our main contribution compared with these two studies is to credibly assess the impact of criminal victimization on labor market outcomes for a number of separate offenses in a unified framework. By conducting analyses for (specific) offenses—instead of using broad categories (violent and property in Velamuri and Stillman 2008) or only one offense (assaults in Ornstein 2017)—we can increase the external validity of the results and address interesting heterogeneities. Furthermore, as our data encompass all victims of crime (who report them to the police), we can provide more general estimates with a potentially different policy relevance compared with Ornstein (2017): we show that most victims suffer nontrivial losses (not only those likely to be hospitalized). Finally, the monthly frequency of our data allows us to study the timing of the labor market effects in greater detail.

We address the main empirical challenges—selection, omitted variables, and simultaneity—by leveraging large and unique panel data on more than 600,000 victims of crime in the following ways. First, the population of victims of crime may differ from the population of nonvictims. To avoid resulting selection biases, we restrict our sample to individuals who have been victimized at least once during our sample period (2005–16) and conduct all analyses separately by gender and offense, focusing on the first observed victimization. Second, unobservable characteristics may correlate with both the outcome and the probability of becoming a victim. To avoid resulting omitted variable bias, we exploit the long panel of labor market outcomes and estimate event study designs with individual fixed effects controlling for any time-invariant individual traits. This approach is similar to that of Grogger (1995), who studies labor market effects of being arrested using a distributed leads and lags model, or, more recently, that of Dobkin et al. (2018), who study the economic consequences of hospital admissions. Third, labor market outcomes may impact the chance of victimization at the same time that victimization affects labor market outcomes. We address this simultaneity concern by explicitly studying the timing of potential effects to identify sharp changes in labor market trajectories at the time of victimization. Our labor market outcomes are available at the monthly level, allowing us to zoom in close to the victimization date. Finally, we look at correlated shocks (moving, divorce) to rule out potential confounders and show the robustness of our results to new methods for event studies with staggered adoption.

Our main results show significant labor market effects of victimization: one year after the incident, earnings decrease by up to 8.4% for males and 12.9% for females, and benefit receipt increases by up to 5% for males and

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4 See Hindelang, Gottfredson, and Garofalo (1978), Cohen and Felson (1979), Miethe, Stafford, and Long (1987), and Miethe and Meier (1990) for discussions of theories of victimization, including the lifestyle exposure and routine activity hypotheses.
6% for females. We find interesting heterogeneities across offenses with respect to the magnitude and the dynamics. For offenses that likely involve physical violence (assault, robbery), the effects are immediate and largest in the short-term, whereas for the other offenses (threat, burglary) there are more gradual changes following victimization. These labor market effects are in many cases accompanied by short-term increases in total and mental health expenditure. Yet they are also seen for victims with no or only modest increases in medical costs—especially among females. Our results reveal further noticeable gender differences. For females, the labor market effects are generally stronger, and the differences in effect sizes between offenses are larger with additional heterogeneities between non-domestic-violence and domestic violence cases. For most offenses, the labor market outcomes do not return to previctimization levels within 4 years. A likely explanation for such scarring effects is path dependency: individuals who become unemployed or leave the labor market may not return to work or remain long-term reliant on benefits. Our extensive margin analyses confirm that employment remains lower 4 years after victimization. An additional explanation is that the victimization is a pivotal event: supplementary analyses suggest that later victimizations and criminal involvement as well as other life events (moves and family outcomes) may contribute to these persistent effects.

II. Data

A. Register Data on Victimization

The victimization register data consist of files of all police-registered victims of crime in the Netherlands (i.e., all victims of an offense reported to the police from 2005 to 2016). They contain (anonymized) social security numbers for 90% of the individuals in the sample, allowing us to link them to labor market and other register data. Individuals without a valid social security number (e.g., tourists) cannot be matched to any other data files. The data contain the reporting date and the offense type, as well as the social security number of the suspected offender(s) whenever there is a known suspect. This allows us to link victims and offenders by household, a feature that we use to identify domestic violence offenses. The share of incidents with a known offender varies by offense and ranges from 72% for assault to 9% for burglary.

To avoid both the issue of measurement error that arises from selective reporting and more general identification problems concerning selection into victimization, we condition our analysis on the sample of crime victims (see sec. III). For our main analyses, we use the month of victimization as the treatment time. We focus on the first observed victimization, and if an individual is victimized more than once, we use the dates of each incident to determine the first. We split the sample by offense to only compare victims of the same offense: each individual is part of one subsample matching the crime category of the first recorded victimization.
We include offenses that are not victimless (and match categories used in the economics of crime literature): assault, threat of violence (including stalking), street robbery, and burglary (theft from a dwelling). Assault and threat are classified as violent crimes while robbery and burglary are classified as property crimes in the Netherlands—but they nonetheless can have a violent component. Burglary includes theft from a dwelling both with and without (the threat of) violence. In the data, we can distinguish the burglaries in which actual violence was used (which is rare: less than 1% of all burglaries), but we do not observe whether there was a threat of violence without actual physical violence. Street robbery is a form of theft with violence in an outdoor setting, and according to police statistics, one in six street robbery victims is injured. Robberies in (commercial) indoor settings or, for example, burglaries in shops are classified differently and do not fall into any of the (personal) crime categories that we consider. If an individual is a victim of multiple offenses during the same incident, only the most severe offense is registered, defined as the one subject to the most severe punishment.

Domestic violence is not recorded as a separate offense by the police. To identify these cases, we use the Municipal Records Database (Gemeentelijke basisadministratie persoonsgegevens [GBA]) to link each victim to their past and current household members at their registered address. We then identify domestic violence offenses as assaults and threats in which the suspected offender is either a current or previous household member: 21.4% of assaults and 10.1% of violent threats are classified as domestic violence. The differentiation between non-domestic-violence and domestic violence offenses is important: table 1 shows descriptive statistics from the Dutch Victimization Survey. Victims of a specific crime in the last 5 years are asked for details of the incident. For assaults, 47% of females report that they knew the offender and 54% report a familiar location, compared with only 24% and 30% for males, respectively. The pattern for threat is similar. The share of women and men knowing the offender or reporting a familiar location is much more equal for the other offenses. This suggests that there are substantial differences in the type of victimization 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 investigate domestic violence offenses separately.

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5 Source: https://www.vraaghetdepolitie.nl/mishandeling/geweld/wat-is-straatroof.html (April 2021).
6 The nationally representative victimization survey is a repeated cross section, and data are available for 2005 to 2016. To keep the sample from the survey comparable to the sample from the register data, we report results only for individuals aged 18–55.
7 Table 1 shows different reporting rates across offenses. Between 54% and 60% of assaults are reported to the police. For threat, this is only 29%–38%, while burglary and robbery have higher reporting rates (up to 72%). Reporting rates for assault are...
This leaves us with subsamples for four offenses by gender and two domestic violence offenses for females. For computational reasons, we draw a 50% random subsample from the sample of burglary victimizations. For domestic violence we only look at females (there are too few incidents with male victims in the data). To further ensure that these subsamples are as homogeneous as possible, we implement a number of sample restrictions. First, we look at the effect of the first observed victimization and restrict the sample to individuals with no registered victimization in at least the two previous years and, therefore, drop those victimized in 2005 and 2006. We allow subsequent victimizations to contribute to the estimated effects; that is, we estimate the combined effect of the first and any future victimizations. Second, we focus on individuals aged 18–55 at the time of observing the (labor market) outcomes for violent crimes and those aged 26–55 for property crimes. Victims of property crime are likely to be positively selected (one needs to own valuable 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, we use a higher age threshold. Third, we exclude individuals who are a registered criminal

higher for females who did not know the offender compared with those who knew the offender: 64% vs. 56%.

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

### Table 1

|                     | Assault (1) | Threat (2) | Burglary (3) | Robbery (4) |
|---------------------|-------------|------------|--------------|-------------|
| **A. Females**      |             |            |              |             |
| Known offender (1/0)| .47         | .32        | .05          | .07         |
| Familiar location (1/0) | .54        | .50        | NA           | .17         |
| Reported to police (1/0) | .60         | .38        | .72          | .67         |
| Observations        | 2,042       | 7,982      | 12,238       | 445         |
| **B. Males**        |             |            |              |             |
| Known offender (1/0)| .24         | .20        | .04          | .04         |
| Familiar location (1/0) | .30        | .35        | NA           | .20         |
| Reported to police (1/0) | .54         | .29        | .72          | .56         |
| Observations        | 3,551       | 11,592     | 11,019       | 385         |

**Source.**—Results based on calculations by the authors using micro-level data from Statistics Netherlands.

**Note.**—This table shows averages of the indicated variables for the four offenses as indicated at the top of each column. The share of missing responses by offense (in the order of the table from left to right, with values for males in parentheses) are as follows: known offender (partner, ex-partner, family, neighbor, work, and other known)—0.20, 0.12, 0.96, 0.56 (0.24, 0.12, 0.96, 0.54); familiar location (home, other dwelling, in a car, at work, at school, sports field/canteen, work/school, elsewhere)—0.19, 0.12, not available, 0.25 (0.24, 0.12, not available, 0.20); reported to police—0.46, 0.53, 0.53, 0.51 (0.52, 0.55, 0.56, 0.49). These are for the large part driven by not all questions being asked in each survey wave. NA = not applicable.
suspect in the years of or before the victimization, to not confound the labor market effects of victimization with those of offending, as documented in the literature. Last, we exclude individuals who are not registered at a valid address in the Netherlands. Overall, this leaves us with 610,854 individuals divided over the offense by gender subsamples. Panel A of table 2 reports the resulting number of individuals for each offense. Burglary (even after taking a 50% random sample; see above) is the most common offense, with 220,867 victims across the male and female samples, while there are fewer reported robberies (23,223 individuals).

We investigate our sample restrictions in several ways. We exclude 2007 and 2008 victimizations to focus on individuals who have not been victimized in the previous 4 (instead of 2) years. To deal with later victimizations, we look at one-time victimizations only and alternatively control for contemporaneous victimizations. We further restrict the sample to individuals who have not been a registered crime suspect in the years after the victimization, and we discuss the age restrictions for the different samples as well as alternative sample definitions.9

B. Register Data on Outcomes

We link the victimization data to registers of individual labor market outcomes from 1999 to 2016. These contain individuals’ income spells, including wages and earnings from self-employment, unemployment insurance (UI) benefits, sickness and disability insurance (DI) benefits, and welfare benefits. In case of sickness/disability, an individual receives sickness benefits for up to 2 years and, if eligible, then transfers to disability insurance. For simplicity, we pool both types and refer to them as DI benefits. For UI benefits, both eligibility period and level depend on individuals’ labor market histories, and for DI benefits they depend on the degree of disability and again the labor market history. Welfare benefits are provided to households with no or no sufficient means of living. They are means tested (on both

remains stable thereafter (Statistics Netherlands 2013). For violent offenses, 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.

9 A previous version (Bindler and Ketel 2019) also included results for sex offenses and pickpocketing. However, we put little emphasis on these results: there is no evidence of labor market impacts for female victims of pickpocketing. The sample size for males is much smaller (about a third of that for females); there is no short-term effect. For sex offenses, the empirical patterns suggest labor market responses in terms of earnings and benefits following the victimization. We are hesitant, though, to strongly interpret these results, as there is a high degree of underreporting (according to the Dutch Victimization Survey, only 15% of victims report sex offenses).
### Table 2
Summary Statistics

| Offense Comparison: | Assault | Threat | Burglary | Robbery | Comparison: Nonvictims |
|---------------------|---------|--------|----------|---------|------------------------|
|                     | Male    | Female | Male     | Female  | Male       | Female   | Male     | Female   |
|                     | (1)     | (2)    | (3)      | (4)     | (5)        | (6)      | (7)      | (8)      |
| Male                | (9)     | Female (10) |

#### A. Background Characteristics (Measured in the Month of Victimization)

|                     | Male | Female |
|---------------------|------|--------|
| Age at victimization| 30   | 31     |
| Partner (0/1)       | .28  | .25    |
| Children (0/1)      | .19  | .33    |
| Total number of victimizations | 1.28 | 1.41 |
| One victimization   | .81  | .74    |
| Observations        | 126,859 | 83,349 |

#### B. Monthly Labor Market and Annual Health Outcomes

|                     | Male | Female |
|---------------------|------|--------|
| Earnings (0/1)      | .89  | .69    |
| Earnings (in 2015 euros) | 2,063 | 1,104 |
| Benefits (>0)       | .11  | .17    |
| Days of benefits    | 4.49 | 7.99   |
| Total health costs (in euros) | 1,490 | 2,582 |
| Mental health costs (in euros) | 455  | 740   |
| Observations (N × T, in millions) | 15.9 | 11.3 |

**Source.**—Results based on calculations by the authors using micro-level data from Statistics Netherlands.

**Note.**—This table shows the sample means of the indicated variables for each offense/gender subsample as indicated at the top of each column. Panel A reports the (cross-sectional) background characteristics in the month of victimization for all individuals in the respective sample; panel B reports the (longitudinal) monthly labor market and additional outcomes (the health outcomes are at the annual level). NA = not applicable.
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. The benefits registers contain observed benefits spells and thus measure benefit uptake (conditional on eligibility). The benefits spells start at the date of eligibility, but the first payments may be received retrospectively. We use these data to construct our primary labor 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. We measure benefit receipt by the number of days on which individuals receive any social benefits but ignore actual amounts (a function of previous income). Furthermore, we create dummy variables for any positive earnings in a month for an extensive margin analysis.

To proxy for health status, we use annual health insurance data—available from 2009—which contain all medical costs recorded by the standard Dutch health insurance (mandatory for residents of the Netherlands). It covers almost all costs for general health care, such as consulting a general practitioner, hospital treatment, or prescription medication. We construct measures of total annual health expenditure and expenditures specifically for mental health. The latter includes only costs specifically labeled as mental health expenses (such as consultations with a psychologist) and excludes those not separable from general health expenses (such as medication prescribed by a general practitioner).

Finally, we use the GBA as well as registers on crime suspects to measure other life events (moving, family outcomes, and annual offending) and extract demographic information (gender, year of birth, marital status, household composition, and municipality/neighborhood codes).11

C. Descriptives

Table 2 presents summary statistics by gender for all offenses. There are notable differences in the demographic composition (although some are due to the different age restrictions for violent and property crime): victims of assault are younger and less likely to have a partner or children than victims of violent threat or burglary. Victims of burglary are more likely to be a victim only once during our observation window (88% vs. 74%–83% for the

10 To account for inflation, we correct wages by the annual consumer price index provided by CBS Statline, using 2015 as the base year. For our analysis, we use log earnings, replacing zero earnings with a small number. In our robustness section, we alternatively use a hyperbolic inverse sine transformation, which leads to very similar results.

11 On average, 90% of registered suspects are convicted (Statistics Netherlands, WODC, and Raad voor de Rechtspraak 2013). In this paper, we will refer to the registered suspects using the terms “criminal record” and “criminal suspects” interchangeably.
other offenses). Panel B of table 2 reports the average monthly labor market outcomes (based on both pre- and postvictimization years). Again, there is heterogeneity in earnings and benefit receipt between offenses, but within-offense differences between males and females are even larger: females are less likely to work, have lower earnings, and are more likely to receive benefits. For comparison, columns 9 and 10 of table 2 show corresponding descriptives for a 5% random sample drawn from the population of nonvictims (with equivalent sample restrictions). They differ especially from the victims of violent crime in terms of household composition, labor market outcomes, and health expenditures. As our individual fixed effects approach does not allow for (time-invariant) controls to account for such (quite large) compositional differences, this highlights the importance of restricting our sample to victims of crime and to conduct the analysis separately by offense and gender.12

### III. Empirical Strategy

Ultimately, we are interested in the causal relationship between criminal victimization and labor market outcomes, for which we have to overcome two identification problems. First, unobserved characteristics may correlate with both the outcome and the probability of becoming a victim. For example, the ability to recognize and avoid risky situations may correlate with labor market success. To address the potential omitted variable bias over and above conditioning the sample on victims (which takes care of unobservables common to all victims compared with nonvictims), we adopt a quasi-experimental approach and use an event study design with individual fixed effects (similar to Grogger [1995] or, more recently, Dobkin et al. [2018]). Intuitively, this compares labor market outcomes for the same individual before and after victimization, controlling for any unobservable and time-invariant individual traits.

Second, labor market outcomes may impact the chance of victimization at the same time that victimization affects labor market outcomes, resulting in simultaneity bias. For example, daily routines may change depending on an individual’s employment situation, affecting the risk of victimization. We address this simultaneity concern by explicitly studying the timing of any effect of victimization on labor market outcomes in our event study design. Importantly, we leverage our rich data to zoom into the monthly level, to identify sharp changes in labor market outcomes at the time of victimization and to closely study other life events around the time of victimization.

12 Table A1 (tables A1–A7 are available online) provides descriptives for female domestic violence victims. On average, they are older, more likely to have a partner or children, and more likely to have multiple victimizations than victims of non-domestic-violence offenses. Even though on average older than non-domestic-violence victims, their earnings are lower, they are less likely to work, and they are more likely to receive benefits.
Let \( Y_{i alta} \) 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 \( \beta \) varies with time to and since victimization \( s \):

\[
Y_{i alta} = \sum_{s=-49}^{49} \beta_s V_{i i m l}^s + \theta_i + \theta_{t m} + \theta_{x a} + \theta_l + \nu_{i alta}.
\] (1)

The term \( V_{i i m l}^s \) is a dummy equal to 1 if the difference between the calendar month minus the victimization month equals \( s \) (months). For instance, \( V_{i i m l}^0 \) is equal to 1 in the month of victimization and equal to 0 otherwise, \( V_{i i m l}^1 \) equals 1 in the month after victimization, and so on. The omitted period is defined as the month preceding the victimization (\( s = -1 \)). The dummies \( \beta_{-49} \) and \( \beta_{49} \) capture 4 years or more before and 4 years or more after victimization. We include fixed effects for individuals \( \theta_i \), year by month \( \theta_{t x m} \) (to account for time trends and seasonality), and municipality \( \theta_l \). To nonparametrically control for age-specific trends (e.g., due to cohorts entering the labor market at different times), we include an age group–specific year fixed effect \( \theta_{x a} \). Standard errors are clustered at the individual level, and we estimate the model separately by offense and gender.

A causal interpretation of the parameters in equation (1) relies on the assumption that the timing of victimization is random, conditional on our sample restrictions (victims only) and control variables (including individual fixed effects). Again, this means that there should be no relevant omitted variables (e.g., time-variant unobservables) and that the victimization 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 results in pretrends.

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

Finally, a recent and quickly developing literature (e.g., Abraham and Sun 2021; Callaway and Sant’Anna 2021; Goodman-Bacon 2021) shows that event study designs may behave poorly in the presence of heterogeneous treatment effects. If these exist, the coefficient corresponding to a given

\[^{13}\text{The age groups are 18–20, 21–25, 26–30, 31–35, 36–40, 41–45, 46–50, and 51–55 years.}\]
lead or lag (a nonconvex average of cohort-specific average treatment effects on the treated) could pick up spurious terms consisting of treatment effects from other periods. Our strategy to conduct all analyses separately by offense and gender (major sources of heterogeneity) alleviates some of these concerns. Still, in our setting cohort-specific treatment effects could arise, for example, if the effect of a victimization during a recession is very different from that in a boom. Regarding heterogeneity over time (across treatment cohorts), we show that excluding specific years or increasing the size of the not-yet-treated group (individuals who become victims in the future but have not yet been victimized) does not change our results. Finally, we implement the estimator proposed by Callaway and Sant’Anna (2021) after collapsing our data to the yearly level. The results show very similar patterns as our main estimation strategy using equation (1) but, given the coarser frequency, mask a lot of detail. We thus use event study designs as the main specification throughout the paper.

IV. Main Results

A. Males

Figure 1 shows the labor market results for male victims. Each graph plots the estimated coefficients $\hat{b}$, and corresponding 95% confidence intervals for equation (1). The two vertical lines mark the month of victimization; the month after that is the first full month of treatment. The graphs combine the estimates for log earnings (circles) and for days of benefit receipt (triangles). For easier reference, the darker-shaded markers correspond to the 12 months before and after victimization, our “short-term” period for which the sample is more balanced and less susceptible to, for example, outliers and sample differences. We will focus on this period first; the longer-termed effects are discussed in section V.

Following a victimization, earnings decrease significantly for all four offenses. In the first month after victimization, the earnings decrease is largest for robbery (−8.4%), followed by assault (−4.5%), violent threat (−1.3%), and burglary (−1.0%). Twelve months after, the point estimates for assault, violent threat, and burglary increase to −7.5%, −7.4%, and −4.3%, respectively. For robbery it remains negative and around −6%, but confidence intervals are wider and include zero (likely because the sample size is much smaller for robbery, about a tenth of that for assault or burglary). The earnings decrease for assault and robbery is very immediate, while it is more gradual for violent threat and burglary—an observation we come back to shortly. Notably, for the offenses with a sufficient sample size there are no pretrends and the earnings effects persist over the next 4 years.

14 Unfortunately, it is computationally not feasible to repeat our analysis at the monthly level using this estimator.
15 We also estimate the coefficients for the tails (4 years or more before/after victimization), as specified in eq. (1), but omit them from the graphs.
We find an almost mirror image of these results when we look at benefit receipt: days of benefit receipt increase for all offenses following a victimization, with differences in timing and magnitudes. For assault and robbery, there is a sharp increase, by 3.5% and 2.7%, respectively, relative to the mean in the month after victimization, that remains stable in the 12 months after (although confidence intervals are again wider for robbery than assault). Splitting by benefit type (fig. B2; figs. B1–B8 are available online), we find a more precisely estimated increase in days of sickness/disability benefits for robbery. For violent threat and burglary, the increase is less immediate. For threat it accelerates from initially 1.1% up to 5% 12 months after victimization, while for burglary point estimates are not significantly different from zero within the first few months after victimization.

Why are the labor market responses immediate for assault and robbery but more gradual for violent threat and burglary? To start with, assault and robbery are more likely “one-off” incidents, while threat may be ongoing over a sustained period of time as reflected by a longer average time between the “start” and “end” date of a given offense in the data. Furthermore,
the remarkably immediate decrease in earnings for assault and robbery victims can plausibly be due to job losses. While Dutch employment protection makes it generally not possible to lay off workers directly, temporary or zero-hour employments can terminate more easily. The proportion of assault and robbery victims with such contracts is high in comparison: just under 45% hold fixed-term contracts, compared with about 20% of the general workforce. On top of that, more than 15% of assault and robbery victims are in temporary work arrangements via agencies.\textsuperscript{16} As information on contract types is not available for all outcome years, we instead refer to the extensive margin results in figure B1A (i.e., the likelihood of having any type of employment). In line with the results for log earnings, this likelihood decreases immediately after victimization for assault and robbery victims. In terms of magnitude, the extensive margin can explain some, but not all, of the earnings effects. The implication is that the estimated effects in figure 1 combine changes at both margins, where the extensive margin captures both voluntary and involuntary job separations.

The timing of the changes in benefits is similar to that for earnings. Recall that the benefit measure reflects benefit eligibility (possibly reported retrospectively) and not actual payment dates. The type of benefits can tell more about the mechanisms: figure B2 shows separate analyses for UI, DI, and welfare benefits. There is a sharp increase for DI benefits (specifically for assault and robbery), a more gradual increase for welfare benefits, and not much of a response for UI benefits. The results for DI benefits are consistent with the type of offense: both assault and robbery entail violence, and as victims plausibly suffer from injuries, they may (fully or partially) transition to sickness benefits, which are included in the DI benefits category.

To better understand the effect of victimization on health outcomes, figure 2 shows results for total and mental health expenditure (our best proxies for health status). Most strikingly, there is a spike of 27.7% relative to the mean in total health expenditure in the year of an assault (and a smaller one for mental health). The point estimates remain significantly different from zero afterward but decrease in (relative) magnitude to around 11%. While in the year of a robbery total and mental health expenditures increase by 356 euros (18%) and 323 euros (45%), the confidence intervals are much wider and results are harder to interpret. Our results suggest that assaults (and robberies) cause physical harm and may inflict the need for ongoing medical treatment, which is consistent with the sharp increase in DI benefits for these offenses. The remaining two offenses, threat and burglary, are less likely to lead to physical injuries. Consistent with that, there is no significant

\textsuperscript{16} Descriptives suggest that these contract types matter: 8.2% of assault victims with fixed-term employment at the time of victimization are jobless 1 month later, compared with 3% for those with permanent contracts. The pattern for robbery is very similar.
The increases in mental health expenditure across offenses are in line with results reported in Cornaglia, Feldman, and Leigh (2014) and Dustmann and Fasani (2016).

These results naturally lead to the question of whether the negative labor market consequences of victimization are driven by just some victims who experience large increases in health expenditure or whether most victims suffer from nontrivial losses. For this, we split the sample into three groups based on the increase in medical costs from the year before to the year of victimization: no increase (41%–48% of observations), modest increase, and large increase.17 For easier comparison, we use a modified version of

17 We use the distribution of the changes in medical costs to define modest and large: “large” refers to individuals with a change above the 75th percentile of this distribution in the respective offense and gender subsample, and “modest” refers to the remaining positive changes below the 75th percentile. Using year-to-year
equation (1): instead of estimating monthly coefficients, we average over years and plot the estimated coefficients for the first year after victimization for each of the three subgroups (left graph of fig. B4A; see the figure legend for the estimating equation). The negative impact of victimization on labor market outcomes is largest for victims with a large change in medical costs. This is also true for offenses that do not usually include physical violence (threat and burglary), pointing toward the importance of mental health. Victims with a large change in health costs appear to drive the overall increase in benefit receipt with almost no (significant) increases for the other two groups. Interpreting differences in health cost increases as a proxy for differences in incident severity, this is consistent with DI benefits being the driver of the benefit increases for men. In contrast, assault and threat victims with no or modest increases in health expenditure also experience earnings declines (with a similar but less precisely estimated pattern for robbery). This is interesting, as it means that most victims suffer nontrivial losses, not only the ones with the most severe health consequences. Furthermore, it implies that when victimization data are selected on the severity of the health impact (e.g., hospitalizations), results will not capture the full picture of the consequences of victimization.

B. Females

*Non-domestic-violence offenses.*—Figure 3 shows the results for females excluding all offenses that have been classified as domestic violence, to which we will come back later. For earnings, there are parallels to the results for males: again, there is an immediate change in earnings for assault compared with a more gradual change for threat, with no indication of pretrends. Earnings losses are largest for robbery (−12.9%), followed by threat and assault (−10.4% and −8.8%), and they are smallest for burglary (−2.6%) 12 months after victimization. As for men, confidence intervals are widest for robbery; however, point estimates are more precisely estimated for females and are significantly different from zero in the longer run. In terms changes implies that we lose observations for whom not all years are available. The results for our main analysis are robust to this.

18 For burglary, only those with the largest changes in medical costs are affected. This suggests that the effects of burglary in the main analyses are driven by the most severe and traumatic cases.

19 We further look into heterogeneity along the earnings distribution. We use the highest level of attended education (high school, basic qualification, and college or university) as a proxy for individuals’ earnings potential to avoid conditioning on the outcome (earnings). There are negative labor market effects for all groups—without systematic differences based on education (see right graph of fig. B4A). This means that neither those at the lower nor at the higher end of the earnings potential distribution drive the main results. To put this into perspective, recall, however, that victims in our samples are in general negatively selected in terms of labor market outcomes (see table 2).
of magnitudes, point estimates for assault, threat, and robbery are larger for women (although not always significantly so), while for burglary men show a larger impact after one year. The extensive margin analysis in figure B1 again suggests some transition into nonemployment.

The results for benefits show some similarities but also striking differences compared with those for men. For threat and burglary, relative effect sizes are comparable (about 5% and 2% for men and women, respectively), although absolute effect sizes are significantly larger for women. The robbery point estimate 12 months after is significantly different from zero and larger in relative, but not in absolute, size for women (4.3% vs. 2.5%). The largest differences can be seen for assault: the increase in the number of days with benefit receipt per month 12 month after victimization is significantly different and twice as large as for men (6% vs. 2.9% in relative terms). However, we see a pretrend for this one offense (even though there is no pretrend for earnings). Recall that female assaults stood out in table 1: almost 50% of the victims knew the offender, clearly linking this offense to domestic violence. Also recall that domestic violence is not recorded as a separate offense by the

Fig. 3.—Females: labor market outcomes. This figure plots the estimated coefficients and 95% confidence intervals (CIs) for the regressions corresponding to equation (1) with log earnings (circles) and days of benefits (triangles) as the dependent variable. The graphs show results for assault, violent threat, burglary, and robbery. The solid vertical lines mark the victimization month. Standard errors are clustered by individual. Source: Results based on calculations by the authors using micro-level data from Statistics Netherlands. A color version of this figure is available online.
police. Our own measure underestimates the number of actual domestic violence victims, either when the offender is not known or reported or when they are not registered at the same address as the victim. If domestic violence plays a role, then it is likely that unreported incidents took place earlier, affecting labor market outcomes even before the first reported victimization. We investigate this point in more detail soon and provide results supporting this argument. As an alternative explanation for the pretrend, we study other life events in our discussion of correlated shocks (sec. V) but find that these are unlikely drivers. Regardless, there is a sharp and discrete increase in the point estimate for days of benefit receipt following an assault that is not plausibly explained by a continuation of the pretrend alone.

In terms of benefit types (fig. B3), the immediate increase in welfare benefit receipt is more than twice as large for females (5.5%) as for males (2.2%). In fact, the overall difference in the benefits results across gender appears to be driven by welfare. But as for men, DI benefits also increase following assaults and threats. This is consistent with the results for health expenditure shown in figure 4: total health expenditure increases by 277 euros (10.7%) in the year of an assault. In contrast to males, level increases in mental health expenditure explain large parts of this and are significantly higher for females following an assault. For violent threat, total health expenditure increases by 6.2% in the year following the victimization, with a smaller contribution of mental health costs.

To connect the different results, we repeat the heterogeneity analysis from before and split the sample based on changes in health expenditure (fig. B4B). The patterns for burglary and robbery resemble those for men, with the largest labor market impacts for those with large health cost increases. While this is the case for assault and threat too, the difference in magnitude compared with females with no or modest increases in medical costs is much more pronounced. Furthermore, all of these groups show significant increases in benefit receipt. Above we found that the larger increases in benefit receipt for women relative to men are mainly driven by welfare. As eligibility for welfare is not directly tied to health status, this could also explain the significant benefit effects for females with no increase in medical costs. More generally, we find that the effect sizes are larger for women for both assault and threat. This can be a result of a difference in the response to the victimization and/or in the underlying nature of the offense. One example for the latter are remaining domestic violence cases that we cannot identify in the data.

Domestic violence offenses.—By specifically studying domestic violence offenses, our main goal is to offer a possible explanation for the pretrend seen in the specific case of benefit receipt of assault victims (fig. 3). A secondary interest lies in sketching the labor market responses to domestic violence, adding to the existing literature and ongoing policy debate and providing a comparison for the general results for females (see, e.g., Ornstein 2017; Peterson et al. 2018; Currie, Mueller-Smith, and Rossin-Slater 2022).
We restrict our analysis of domestic violence to cases in which the offender is a partner or ex-partner (for summary statistics, see table A1). Figure 5A shows the case in which the offender is the current partner at the time of victimization. The results are striking: earnings drop immediately for assault and decrease more gradually for threat, while benefits increase sharply. Unlike for assault in figure 3, pretrends are mostly flat. This could be mechanical if reporting to the police leads to household dissolution and the victim (immediately) becomes eligible for household-level means-tested welfare benefits. However, if that was the case, then one would not expect to see a postvictimization impact on earnings; yet this is the case. An alternative explanation for the absence of pretrends is that these reported victimizations are first-time victimizations and that there are no earlier, unobserved incidents (as we argued in the case of fig. 3). In fact, when the offender is an ex-partner and it is plausible that there are earlier, unreported incidents of domestic violence, there is a pretrend leading up to the (reported) victimization (fig. 5B). If such earlier incidents lead to effects such as those seen in the case in which the partner is the offender (fig. 5A), they can reasonably

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**FIG. 4.**—Females: health expenditure. This figure plots the estimated coefficients and 95% confidence intervals (CIs) for the regressions corresponding to equation (1) with total health expenditure (circles) and mental health expenditure (triangles) as the dependent variable. The graphs show results for assault, violent threat, burglary, and robbery. The solid vertical lines mark the victimization year. Standard errors are clustered by individual. Source: Results based on calculations by the authors using micro-level data from Statistics Netherlands. A color version of this figure is available online.
explain the pretrends in the benefit outcome in our main results for assaults (fig. 3). In that sense, our results document underreporting of domestic violence cases, consistent with earlier statistics in table 1.

Despite the pretrends, there is a distinct and large change at the time of victimization. When the current partner is the reported offender (fig. 5A), earnings decrease by 8.9% in the month immediately after an assault (14.4% 12 months later) and gradually decline for violent threat (17.9% 12 months after). At the same time, benefit receipt sharply increases following an assault or threat (almost 42% relative to the mean 12 months after). When the ex-partner is the offender (fig. 5B), there is a sharp change at the time of victimization despite the pretrend: benefit receipt in the month after compared with the month before victimization—that is, precisely around the 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 pretrends—this is of interest in and of itself, and we leave it for future research. However, it is worth highlighting that the relative
magnitudes found for domestic violence victims substantially exceed those for other victims.

V. Extensions and Robustness

A. Long-Term Effects

So far, we focused on the shorter-term responses within the first year after victimization.\textsuperscript{20} Our rich data also allow for a longer-term perspective, which has hardly been studied before but is important from a policy perspective.\textsuperscript{21} The light-shaded markers in figures 1, 3, and 5 represent the estimates up to 4 years after victimization. For victims of all offenses, neither earnings nor benefits (except for robbery) return to previctimization levels within 4 years. One likely explanation for such scarring effects is path dependency: individuals who become unemployed or leave the labor market may not return to work or remain long-term reliant on benefits. Indeed, analyses at the extensive margin show that after 4 years employment remains lower (fig. B1) and the probability of benefit receipt stays higher (results not shown) than before victimization. Other literatures find similarly persistent labor market effects of adverse events: job displacement decreases earnings up to 12 years after displacement in Sweden (Eliason and Storrie 2006) and leads to a 3% loss in earnings 7 years after displacement in Norway (Huttunen, Moen, and Salvanes 2011). Oreopoulos, von Wachter, and Heisz (2012) show similarly lasting effects for graduating from college during a recession (with initial earnings losses of 9%). Importantly, they find that college graduates with lower predicted earnings suffer from larger and more permanent losses. Even though our sample is very different from their sample of college graduates, victims are on average at the lower part of the earnings distribution compared with the general population (see table 2) and may be at higher risk of lasting losses in earnings.

An additional explanation for the long-term effects is that the victimization is a pivotal event with other life events contributing to the labor market impacts. To understand whether this is the case, we study multiple victimizations, address a possible victim-offender overlap, and look into family outcomes as potentially relevant life events.

\textsuperscript{20} We have done so to focus on sharp changes, to limit the risk of confounding events, and to use the most balanced part of the sample. This makes the results less susceptible to outliers and individuals entering and leaving the sample (due to age restrictions).

\textsuperscript{21} Another important component of the social cost of victimization is the potential impact on nonvictimized household members. We pool men and women and use an analogous design to eq. (1) for the cohabiting partner of the victim. We do not find an immediate impact on their earnings, some (albeit comparably small) response in benefits, and no spillovers on health expenditure (fig. B5). Taken together, our results suggest that there are limited (in terms of occurrence and magnitude) spillover effects on the cohabiting partner, which is consistent with Cornaglia, Feldman, and Leigh (2014).
Multiple victimizations.—So far we have focused on the first victimization, but if subsequent victimizations affect labor market outcomes, our results combine the effect of both. To test this, we look at a sample with just one victimization between 2007 and 2016 (74%–88% of victims; table 2). Tables A2 and A3 report the estimated coefficients 6 months before, 1 month after, and 12 months after victimization. If later victimizations matter, we expect to find attenuated coefficients, and this is indeed the case. Taking assault as an example (12 months after victimization), we find a 6.7% (7.6%) decrease in earnings for females (males) compared with 8.8% (7.5%) at baseline and an increase of 0.37 (0.11) days of benefit receipt compared with 0.48 (0.13) at baseline. This suggests that especially for women multiple victimizations contribute to the persistent labor market responses—but given the large share with only one victimization, they are unlikely to capture the full story. Alternatively, we include an additional control for any contemporaneous victimization in equation (1). Again, we find slightly attenuated coefficients and more so for females (table A2). Nonetheless, these results reinforce our strategy of restricting the main samples to individuals with no earlier observed victimization(s) in at least the two prior years to abstract from potential confounding effects. In robustness checks (see sec. V.B), we further this restriction to 4 years.

Criminal record.—We address a potential victim-offender overlap by focusing on individuals with no criminal record after victimization. The likelihood of having a criminal record after victimization varies by offense: around 10% for assault, threat, and robbery and around 5% for burglary. Recall that individuals with a criminal record prior to victimization (including the victimization year and including those listed as both offender and victim in the same incident) are already excluded. If subsequent criminal activity affects labor market outcomes (as suggested by the literature), point estimates should be smaller for a sample of nonoffenders. Table A2 shows that compared with baseline, point estimates are attenuated for all offenses, in particular for earnings, and are more attenuated for males than for females. Given that males constitute the larger share of offenders, the latter may not be too surprising. Twelve months after an assault, earnings decrease by 5.3% (7.7%) for males (females), compared with 7.5% (8.8%) at baseline, and days of benefit receipt increase by 0.11 (0.45) days compared with 0.13 (0.45) at baseline. The smaller estimated impacts of victimization for nonoffenders suggest indeed that later criminal involvement contributes to the long-term effects, in particular for males’ earnings.

Correlated shocks.—Other life events may also contribute to the long-run labor market effects. If the victimization exactly coincides with another major life event, it could be that even in the short run the sharp changes in earnings and benefits that we observe at the time of victimization are not driven by the victimization but by other disruptions in life. We study moves (fig. 6A) and divorce (fig. 6B) as potential correlated shocks and reestimate
Fig. 6.—Correlated shocks. This figure shows the results from estimating equation (1) for moves (A) and divorce (B). For moves, we do not include location fixed effects. The solid vertical lines mark the victimization month. The graphs combine the results for males, for females without domestic violence, and for domestic violence. Source: Results based on calculations by the authors using micro-level data from Statistics Netherlands. DV = domestic violence. A color version of this figure is available online.
equation (1) for these outcomes. We combine the results for males, females, and female domestic violence into one graph per offense and outcome.

For males, there is no change in the likelihood of any of these life events before victimization. After victimization, the likelihood of moving increases, but not by much (around 0.1 percentage points for assault and burglary in the month after victimization). When the crime takes place in the vicinity of a victim’s home, moving may be a natural response. If the move leads to changes in work patterns, it could be one contributing factor to the labor market effects discussed before. The probability of divorce does not change. Taken together, this suggests that there are no large enough changes in other life events that would convincingly explain the labor market effects for males.

There is no noticeable change in the probability of divorce for female victims of nondomestic offenses, but the likelihood of moving increases following victimization (0.8 percentage points for assault, 0.4 for threat). For burglary there was no gender difference in the labor market results, mirrored by a very similar increase in the moving rate. The other magnitudes are larger than for men, consistent with the stronger labor market responses for women. However, they are at best moderate compared with domestic violence: the (monthly) probabilities of moving and divorce peak sharply at 3.6 and 1.1 percentage points for assault and 2.2 and 0.7 percentage points for threat, but they already increase before the victimization. This previctimization increase could plausibly explain the pretend trends in the domestic violence labor market results (fig. 5). We actually observe similar but less strong patterns for assaults and threats that are not identified as domestic violence in our data. As in the labor market analyses (fig. 3), these may be driven by remaining domestic violence cases that we cannot identify as such.

### B. Robustness and Falsification Tests

We conduct a number of robustness tests that can be divided into specification tests related to the event study design with staggered treatment and more general robustness tests. As discussed in section III, event study designs may behave poorly in the presence of heterogeneous treatment effects. We address this concern in several ways. First, we drop two victimization years at a time to show that our results are not driven by specific “cohorts” of victims and to assess whether there are heterogeneous treatment effects across cohorts. Second, we drop the 2016 outcome year to create a never-treated control group of 2,016 victims (see Goodman-Bacon 2021). The results are shown in tables A4–A7. As earlier, to ease comparison we report only the coefficients for 6 months before, 1 month after, and 12 months after victimization and include the baseline estimates as a benchmark. Neither dropping outcomes for 2016 nor leaving out two victimization years at a time changes our conclusions. In other words, we do not find evidence of heterogeneous treatment effects by year of victimization. Nonetheless, we implement the estimator proposed by Callaway and Sant’Anna (2021)
after collapsing our data to the yearly level. These yearly estimates are less precise than our monthly event study estimates (and mask much of the interesting detail) but show very similar patterns and do not overturn our conclusions (see figs. B6, B7).

By specifically excluding victimizations in 2007 and 2008 in the above-described robustness test for cohort heterogeneity, we also take one of our sample restrictions one step further: the baseline restricts the sample to individuals who have not reported any criminal victimization within (at least) the 2 years prior to their observed victimization and assumes that the effect is not confounded by any victimization earlier than that. Excluding 2007 and 2008 victimizations, we restrict our sample to individuals who have not reported any victimization within the previous 4 instead of 2 years, thereby relaxing this assumption. In addition, we alter our sample definition in two more ways: we exclude individuals who only enter the sample at the time of victimization (no preoutcomes observed) to assess whether the unbalancedness of the sample affects our estimates, and we restrict the assault and threat samples to ages 26–55 (as for burglary and robbery at baseline). As further robustness tests, we estimate equation (1) for level earnings and the inverse hyperbolic sine transformation instead of log earnings, we include age specific fixed effects instead of age group fixed effects and we include a linear trend (as in Dobkin et al. 2018).22 Adding the 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 both the inverse hyperbolic sine transformation and for level earnings, we find quantitatively and qualitatively similar results as for log earnings (see tables A4–A7). The results are further robust to the different age restrictions, to including year of age fixed effects, and to clustering standard errors by victimization month (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 more granular neighborhood instead of municipality fixed effects.

To rule out that our baseline specification picks up spurious variation, we conduct a falsification exercise in which we draw a 5% random sample of the nonvictimized population, apply equivalent sample restrictions, and assign a placebo month of victimization. This allows us to test whether the main results are driven by remaining trends (or economic shocks) that 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 spurious variation, we would expect this to be visible in the placebo tests. Figure B8 shows that this is not the case: the coefficients are generally not significantly different from and close to zero.

22 For identification, we have to omit one additional previctimization month (Borusyak and Jaravel 2017).
VI. Discussion

What are the effects of criminal victimization on individuals’ labor market outcomes? Our results document statistically and economically significant impacts on both earnings and benefit receipt, which last up to 4 years. Supplementary analyses suggest that later victimizations and criminal involvement as well as other life events (moves and family outcomes) may contribute to these persistent effects. There is a noteworthy parallel to the crime literature regarding offending as a life-changing event and based on the notion 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).²³

There is considerable heterogeneity across offenses in terms of both timing and magnitude: offenses that involve physical violence (assault and robbery) lead to particularly strong and immediate labor market effects. They are accompanied by increases in total health expenditure and a sharp rise in the likelihood of receiving disability benefits. The labor market impacts for threat and burglary evolve more gradually. There is a corresponding gradual increase in DI benefits, and some evidence for increases in medical costs for mental health. Together, this suggests that individuals who receive DI benefits for mental health reasons could drive the gradual increase.

Comparing results for males and females leads to a number of interesting observations. The labor market effects are often stronger for females (except for burglary), with the largest gender differences for assault. Also, the differences in effect sizes between offenses (assault, threat, burglary, and robbery) appear larger for women, with additional heterogeneities between domestic and nondomestic instances of assault and violent threat. Studying within-offense heterogeneity documents further gender disparities: the differences between victims with no, modest, or large health cost increases are more pronounced for women than for men. In contrast to men, women with no observed health cost increases still experience negative labor impacts. This is aligned with the patterns for the types of benefits: while DI benefits increase for both, women experience stronger increases in welfare following victimization. This is potentially important from a policy perspective: while all social benefits provide transfers to compensate for a lack of income, their generosity varies, with DI benefits typically being more generous than welfare benefits.

²³ The idea of hysteresis has found empirical support in the literature. Bell, Bindler, and Machin (2018) report a lasting effect of entering the labor market during recessions on criminal involvement over the life course; Mueller-Smith and Schneipel (2021) document adverse labor market effects of convictions compared with diversion in the criminal justice system. For further discussion of the life course view of crime, see Sampson and Laub (2005).
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 victimization. As stressed before, evidence on the causal impacts of criminal victimization is scarce. To date, the closest study to ours is that by Ornstein (2017). She finds that earnings decrease on average by 25% for female assault victims (identified by hospitalization records) and by 14% for male assault victims, paralleled by a larger increase in sick leave uptake by women (31 days annually) compared with men (15 days annually). Our estimates of (average) losses in labor income are smaller but encompass those for victims with and without increases in medical costs. This is a more general estimate, especially as we document negative labor market effects for victims with no or only moderate health cost increases. The other closely related study (Velamuri and Stillman 2008) finds negative employment effects 1–2 years after a violent crime victimization (5–8 percentage points). Although the Household, Income, and Labour Dynamics in Australia Survey contains interesting variables not included in register data (such as alcohol consumption and life satisfaction), there are limitations in terms of sample size and frequency. Accessing rich register data with monthly labor market outcomes allows us to go beyond their analysis, to pin down the timing of the effects and unmask important heterogeneities.

Using a simple back-of-the-envelope calculation, we find that the average accumulated loss within the first year following an assault amounts to 1,476 euros for males, 1,743 euros for females, and 1,301 euros (compared with a lower baseline) for female domestic violence victims. In terms of benefits, the respective numbers are 1.4 days for males, 4.9 days for females, and 27.7 days for domestic violence. Combined losses in earnings for all four offenses in the first year after victimization sum up to on average 72 million euros per year. Given the sample restrictions needed for our analysis, these numbers have to be taken with a pinch of salt, yet they provide an indication of the cost of victimization when it comes to labor market outcomes. 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, and victims (of all offenses) received 34.5 million euros in compensation from offenders (Statistics Netherlands, WODC, and Raad voor de Rechtspraak 2013).

The costs of crime literature divides the cost of crime into three types: direct costs (e.g., criminal justice, immediate health costs), which are straightforward to measure; indirect victim costs (e.g., lost productivity, precautionary behavior, long-term health consequences), which are more difficult to measure; and intangible victim costs (e.g., pain and suffering), which are by far the hardest to measure (Bindler, Hjalmarsson, and Ketel 2020). Our
estimates speak to the indirect victim costs. Existing approaches, using contingent valuations and jury awards, provide accurate rankings of the costs but imperfect and not necessarily accurate cost estimates (see Dominguez and Raphael 2015). Our study makes progress in that respect by providing novel estimates for the labor market costs of victimization. While cross-country comparisons come with many known caveats, we use existing cost estimates to put our results into perspective. Donohue (2009) estimates the total costs of an assault to lie between 17,463 and 82,108 euros. Compared with his estimates, just the average earnings losses for the victim in the first year after an assault correspond to 1.8%–8.5% of the total for men and 2.1%–10% for women.

VII. Conclusion

Our findings of persistent labor market costs of criminal victimization have important policy implications. They speak to the ongoing debate concerning the social cost of crime and to the nontrivial question of suitable compensation for victims: are there labor 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, Shields, and Suziedelyte 2018). We consider the results in our study as early causal evidence that victimization 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 victimization, labor market outcomes, and necessary support systems. Moreover, the Netherlands has a comparatively generous welfare system, in terms of both health insurance and social welfare. As our study cannot speak to this directly, one can only speculate whether the negative impacts of victimization in other countries with less generous support systems, more inequality, and/or differing access to health care are larger than what we document here.

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