Labour Economics 61 (2019) 101761

Contents lists available at ScienceDirect

Labour Economics

journal homepage: www.elsevier.com/locate/labeco

Job displacement and crime: Evidence from Norwegian register data

Mari Rege[a,b], Torbjørn Skardhamar[b,c], Kjetil Telle[b,d,*], Mark Votruba[e,b]

[a] University of Stavanger, Stavanger, Norway
[b] Research Department, Statistics Norway, Oslo, Norway
[c] University of Oslo, Oslo, Norway
[d] Norwegian Institute of Public Health, Oslo, Norway
[e] Economics Department, Weatherhead School of Management, Case Western Reserve University, Cleveland, OH, United States

ARTICLE INFO

JEL classification:
J12
J63
J65
K42

Keywords:
Crime
Plant closure
Plant downsizing
Displacement

ABSTRACT

We estimate the job displacement effect on criminal behavior for young adult Norwegian men separated from their plant of employment during a mass layoff. Displaced workers experience a 20 percent increase in criminal charge rates in the year of displacement, with effects declining thereafter. Effects are particularly large for property crimes, consistent with the idea that displaced workers turn to acquisitive crimes to replace lost earnings. However, effects are also sizable for violent and alcohol/drug-related crimes, indicating other mechanisms at work. We find strong evidence that displacement increases crime effects through the increased availability of time, and supportive evidence that psychological factors (mental distress, self-control) also play a role.

1. Introduction

A lack of employment and job opportunities are often considered important causes of criminal behavior (Bell et al., 2018; Fishback et al., 2010). Recent world-wide recessions, with particularly high unemployment rates among traditionally crime-prone groups like young and low educated men, have accentuated the importance of understanding relationships between work and crime (Hoynes et al., 2012; Hauser and Baker, 2008). In this paper we use individual level data from Norway to study how the criminal behavior of employed men is affected by job displacement.

There is a rich economic literature exploring the links between labor market conditions and crime.[1] Much of the empirical work draws on US data sources to estimate the relationship between area (usually state) unemployment rates and crime, with the general finding that unemployment has a modest but statistically significant positive effect on property crime rates, with little or no effect on violent crime rates.[2] These findings are consistent with traditional economic rational choice theories of crime, which predict that a reduction in licit earnings opportunities increases the allocation of time toward crime for profit (Ehrlich, 1973; Becker, 1968). The reliance on aggregate data has limited the ability of previous studies to investigate the mechanisms through which labor market conditions may affect criminal behavior. Moreover, when relying on area-level variation, typically in unemployment rates across US states, it is hard to credibly identify causal effects since a number of other factors co-vary with unemployment rates.

We contribute to the existing literature by using individual-level crime data to provide individual-level estimates of the effects of job displacement on crime under a transparent identification strategy (similar to the strategy of e.g. Huttunen et al., 2018 or Black et al., 2015). Specifically, we investigate the impact of job separation associated with mass layoffs on the displaced workers' engagement in crime. Workers suffering involuntary job loss represent an important subset of individuals through which weakening labor markets might affect crime. Focusing on job separations associated with plant mass layoffs allow us to investigate the impact of involuntary job loss while circumventing the most obvious forms of omitted variable bias: the select firing of specific workers based on unobserved attributes. Moreover, we can analyze a richer set of crime categories than others have, including alcohol/drug offenses and serious traffic offenses, and we can date crimes to the

* Corresponding author at: Research Department, Statistics Norway, Akersv. 26, 0177, Oslo, Norway.
E-mail addresses: mari.rege@uis.no (M. Rege), ska@ssb.no (T. Skardhamar), kjetil.telle@ssb.no (K. Telle), mark.votruba@case.edu (M. Votruba).

[1] See for example Mustard (2010) for a review or Bell et al. (2018) for a recent contribution.

[2] Most of the early work in this area suffered from endogeneity and attenuation bias issues, though some studies utilizing more reliably exogenous variation have emerged (Mustard, 2010). For example, Raphael and Winter-Ebmer (2001) and Lin (2008) employ instrumental variable (IV) methods to address measurement error problems and endogeneity of state unemployment rates, and they find that a one percentage point increase in unemployment raises property crime rates 4-6 percent.

https://doi.org/10.1016/j.labeco.2019.101761
Received 7 July 2018; Received in revised form 28 August 2019; Accepted 4 September 2019
Available online 4 September 2019

day-of-week they are committed, allowing us to discuss how displaced workers' variation in time availability on work days versus weekends may affect crime. Our analysis draws on Norwegian register data that include a rich array of socioeconomic and demographic variables for the entire resident population, as well as all criminal charges brought against any resident from 1992 through 2008. Individual employment spell records allow us to calculate employment counts by plant and year, from which we can identify separations and mass layoffs. Our main analytic sample consists of 361,385 different men, 18–40 years old, who were employed with at least two years of tenure in the baseline year. Our difference-in-differences (DID) approach compares the evolution of criminal charge rates in a "treated group" of male workers who were separated from their plant of employment during a period of mass layoff (*the displaced*), to the evolution in charge rates of similar workers employed in plants that did not undergo a mass layoff (*the comparison group*). Pre-separation employment rates are similar across the two groups, however pre-separation crimes rates are somewhat higher in the displaced group, necessitating the DID approach.[3] Our estimated effects are unbiased under the assumption that the difference in crime rates observed *pre-displacement* would have continued in equal magnitude into the post-displacement period had the displacements not occurred. The fact that pre-displacement crime rate differences appear stable throughout the pre-displacement period lends credibility to this assumption.

We find that job displacement leads to a sizable increase in criminal charge rates of about 20 percent in the year of displacement, with declining effects in the subsequent years. Job displacement increases crime for all studied crime categories. In a relative sense, estimated effects are most pronounced for property crimes. Our estimate indicates that job displacement raises the likelihood of property crimes by about 60 percent in the year of displacement. The relative size of effects appears smaller for other crime categories (violence, alcohol/drug, serious traffic violations), but significant effects are estimated throughout, and with similar (though small) *level* effects across all categories.[4]

The effect of displacement on crime presumably operates, at least in part, through workers' labor market detachment. Based on rational choice theories of crime (e.g. Becker, 1968), a displaced worker has incentives to shift the allocation of time toward illicit earnings opportunities (i.e. property crime) since displacement reduces legal earnings opportunities. Additionally, displacement lessens the opportunity cost of a worker's time during the period of unemployment (or underemployment), with implications for both property and non-property crimes (Ehrlich, 1973). Our analysis finds that displacement reduces employment earnings over the immediate years following displacement by 10–15 percent, and displacement substantially increases the likelihood of being unemployed or of working less than full-time. As in prior studies, the particularly large increase in property crimes provides support for rational choice theories emphasizing the role of *earning replacement* as a motivation for crime by the displaced. On the other hand, the significant effects on non-property crimes indicate broader mechanisms are also at work, including a potential role for *time availability*.

Our analysis sheds further light on the *time availability* mechanism by exploiting data we have on the *exact date* each recorded crime occurred. Except for property crimes, we find more dramatic increases in crimes committed on work days (Monday-Friday) than on weekends. This suggests that not having to go to work, associated with a decline in structured daily routines and a reduced opportunity cost of time, is an important channel through which displacement affects non-property crimes. The effects we observe for violent crimes and drug/alcohol-related offenses are also in line with theories that highlight the importance of self-control, financial concerns, frustration and mental distress in determining criminal and counterproductive behavior (Mani et al., 2013; Agnew, 1992; Gottfredson and Hirschi, 1990).

These findings make novel contributions to the existing empirical literature on job loss and crime. We find credible evidence that displacement increases violent (as well as property) crime rates, a fact that has only weak support from most of the area-level studies. We find credible evidence that displacement also affects crimes like traffic offences and drugs/alcohol-offences, an area where no other credible evidence currently exists. The large effects on alcohol/drug crimes may be particularly noteworthy in the economics literature, since they are not straightforwardly explained by the rational crime theory and thus likely speak to psychological effects of job displacement. Our day-of-week analysis is also novel to the literature and provides empirical support for the importance of time availability.

The remainder of the paper is structured as follows. Section 2 discusses theoretical mechanisms through which plant closure could affect criminal behavior, and relates them to the Norwegian context. Section 3 presents the empirical strategy, and Section 4 describes the data. Section 5 presents our results, including robustness checks, and Section 6 explores mechanisms. Section 7 concludes.

2. Mechanisms and the Norwegian context

In the seminal rational crime model of Becker (1968), individuals commit crime when the expected utility from doing so exceeds the expected utility of not doing so. While Becker (1968) was primarily interested in optimal law enforcement, a number of economic studies have extended his model of criminal behavior (see, e.g., overview in Levitt and Miles, 2007). Of particular relevance are the extensions of Ehrlich (1973), who introduces a time constraint whereby individuals divide their time between licit and illicit activities.

Insights from the models of Becker (1968) and Ehrlich (1973) suggest two complementary mechanisms through which involuntary job loss can increase criminal behavior. To the extent job displacement reduces future earnings and employment (Huttunen et al., 2011; Rege et al., 2009; Stevens, 1997; Jacobson et al., 1993), we would expect displaced workers to experience an increase in the marginal utility associated with illicit earnings (the *earnings replacement* mechanism) and a decrease in the opportunity cost of spending time in such activities (the *time availability* mechanism). These mechanisms would anticipate a higher likelihood for acquisitive crime as a result of displacement.

These rational choice-based models provide somewhat weaker predictions for non-acquisitive crime, which fail to compensate for the reduction in licit earnings. Nonetheless, non-acquisitive crimes may still be affected by the reduction in the time costs. Criminologists frequently cite *time availability* as an important determinant of criminal behavior. Felson (1998), for instance, argues that individuals motivated to commit crime cannot do so unless an opportunity is present. Less structured daily routines and increased idleness provide greater opportunities and lower time-costs for criminal activity. Increased idleness may also increase one's exposure to criminogenic settings, where alcohol and drugs may be present and social norms against deviant behavior are weaker (Hirschi, 1969). These theories (including Ehrlich, 1973) suggest the crime effects of displacement could extend to non-acquisitive crimes, and would predict those effects to be largest on days a displaced worker would otherwise have been working. Thus, we would anticipate larger effects on *work day* crimes than on *weekend* crimes.

In their widely cited "general theory of crime", the criminologists Gottfredson and Hirschi (1990) argue that the association between unemployment and crime can be explained by variation in individuals' capacity for self-control, which affects individuals' ability to succeed in

[3] Substantial effort was exerted in attempting to construct more finely-matched samples of displaced and comparison group workers, so that estimates could be based on samples with (near) identical pre-displacement crime rates. These efforts were unsuccessful, but those samples consistently produced DID estimates similar to those reported here.

[4] The 60 percent increase in property crime rates applies to a much lower baseline rate.

school and work.[5] The resource model of self-control posits that the capacity for self-control is limited and can be depleted by cognitive and emotional strains, and this model has found support in the experimental psychology literature (e.g. Inzlicht and Scheichel, 2012; Inzlicht et al., 2006; Baumeister et al., 1994). Empirical findings also suggest that job loss imposes strains and mental distress on affected workers (e.g. Marcus, 2013; Eliason and Storrie, 2009; Dragano et al., 2005; McKee-Ryan et al., 2005; Vahtera and Kivimaki, 1997). If so, the resulting diminishment of self-control could result in counterproductive behaviors (Mani et al., 2013). Therefore, *mental distress/self-control* represents a third mechanism through which we might anticipate a displacement effect on crime, with particular relevance perhaps to non-acquisitive offenses like violence and alcohol/drugs.

The Norwegian context may affect the relevance of each of these theoretical mechanisms. In Norway, strict rules protect employees from being dismissed (Addison and Teixeira 2003), and job displacement is rarely sudden as workers are typically required to receive at least 3 months of advance notice before being dismissed. Moreover, in the recent decades, Norway has been characterized by low unemployment rates, even by Scandinavian standards. In 2007, the survey-based unemployment rate was 2.5 percent, compared with 4.6 percent for the US and 7.1 percent for the European Union (OECD, 2009). With strong demand for workers, the effects of job displacement may not result in prolonged spells of unemployment, or a deterioration of next-job quality, which suggests that we might expect more detrimental effects of displacement on crime in countries with higher unemployment levels.

Moreover, public welfare programs in Norway are generous by international standards. Virtually all Norwegian workers are covered by the state's mandatory unemployment insurance program. The size of the unemployment benefits is typically around two-thirds of the earnings in the previous calendar year, and until 2003 a typical receiver was eligible for unemployment benefits for up to three years (thereafter up to 2 years). Persons not finding a new job when the unemployment benefits run out can get benefits of the same magnitude by participating in medical or vocational training programs or by qualifying for disability pension. The generous welfare benefits available may reduce the incentives to engage in crime for profit compared with other countries where the individual economic consequences of job loss are more severe.

On the other hand, enforcement policies are less punitive in Norway than in the US and the UK (Christie, 2000), which could lessen incentives to avoid crime. Prison terms are substantially shorter for some types of crime in Norway than in countries like US or UK, with remarkable differences in incarceration rates. The US incarceration rate is about 751 per 100,000 inhabitants (BJS, 2009), while the UK rate is about 140 (European Sourcebook, 2006), and Norway's rate is about 91 (Statistics Norway, 2008).

However, for offenses other than murder and robbery,[6] Norwegian conviction rates are similar to those of many other OECD countries. For example, the theft rate per 100,000 is 2860 for Norway, 2182 for the US and 3379 for the UK (UN, 2008). The International Crime Victim Surveys, which might be considered the more reliable data source for cross-national comparisons of crime prevalence, also indicates that crime rates in Norway are similar to those of other OECD countries. Of the 30 countries included in the study, Norway is rated with a medium victimization rate, with lower rates than Ireland (highest rate), England and Wales (next highest) and the US, but higher rates than e.g. France, Germany and Italy (van Dijk et al., 2008). The crime and justice environment in Norway may thus be more similar to other counties in the Western world than suggested by the incarceration and homicide rates.

In summary, over the last decades Norwegian residents have been facing low unemployment rates, generous public benefits, low homicide and robbery rates and a tradition of less punitive law enforcement policies (Pratt, 2008; Christie, 2000). The extent that displaced workers are motivated to replace licit with illicit earnings could thus have been smaller in Norway than in many other Western countries, which may suggest a smaller effect of displacement on crime (especially property crime) in Norway than elsewhere. However, it cannot be ruled out that such a moderating influence of welfare benefits is counteracted by lower expected punishment in Norway compared with many other Western countries. This contextual background should be kept in mind when interpreting the results.

3. Empirical strategy

We estimate the effect of job displacement on crime using a difference-in-differences (DID) approach, which compares the evolution in crime rates in a sample of displaced workers to those in a sample of similar[7] non-displaced workers (our comparison group).

Workers are (potentially) included in the displacement sample if, in a given year, the workers separate from their plant of employment during a period when the plant is undergoing a mass layoff. For such workers, the year of separation is considered the workers' *baseline year*, and we deem the separation to be associated with mass layoff if the worker's plant experienced a reduction in plant employment exceeding 30 percent, either in the baseline year or in either of the two adjacent years.[8] This method for identifying displaced workers largely resembles definitions that have previously been applied in the literature (e.g. Huttunen et al., 2018; Black et al., 2015; Davis and von Wachter, 2011; Couch and Placzek, 2010; Jacobsen et al., 1993).[9] In an attempt to exclude temporary or mis-recorded separations, such as cases of workers relocating within the same firm, we also required evidence that the separation was permanent. To operationalize this, we omit workers from the displaced sample if they had returned to their baseline firm of employment by the end of the second post-displacement calendar year.

In contrast, workers are (potentially) included in the comparison group, for that same baseline year, if their plant-of-employment at

[5] In a meta-analysis Pratt and Cullen (2000) find consistent associations between individuals' criminal behavior and measured levels of self-control. An important methodological concern in estimating effects of job displacement on crime is to rule out spurious associations that might arise from unobserved variation in e.g. capacities for self-control (see Sections 3 and 5.1). Crime, arrests and incarceration might also have causal effects on future employment opportunities, for example, if stigma from a criminal record or human capital depreciation from incarceration, restricts future access to meaningful jobs (Grogger, 1995; Pager, 2003; Mocan and Rees, 2005; Kling, 2006).

[6] As in the other Scandinavian countries, Norway has among the lowest homicide rates in the Western world. Norway has a murder rate of 0.71 per 100,000 inhabitants, compared with rates of 5.62 in the US and 1.41 in England and Wales. The robbery rate per 100,000 inhabitants is 27.9 in Norway, 147.7 in the US and 183.8 in the UK (UN, 2008).

[7] As described below, the workers in both samples are males with at least two years of tenure in their firm of employment at the beginning of a particular baseline year (as well as meeting other inclusion criteria). The samples are, unfortunately, not similar in the pre-displacement crime rates, necessitating the DID approach taken in this paper.

[8] Employment counts are based on full-time equivalents (FTEs) measured at the end of each calendar year. Mass downsizing events in our data are often marked by several consecutive years of high separation rates, which is why we associate separations with mass downsizing even when the major period of employment reduction was a year removed from the year separation occurs.

[9] There are also some smaller differences between our approach and that of (e.g.) Jacobsen et al. (1993) or Couch and Placzek (2010). First, they define mass layoff as a 30 percent decrease relative to the maximum employment level of the plant in the last (6) years before the baseline year, while we define it as a decline of 30 percent relative to the preceding year. Second, while they restrict the sample to plants with at least 50 employees, we restrict our main sample to plants with more than 10 full-time-equivalents, and while they require job tenure of at least 6 years for inclusion, we require only 2 years. Lastly, because of data availability, they exclude workers who do not receive positive earnings in all the years of their data window, while we can follow every worker through time regardless of earnings.

the start of that year did *not* undergo a mass downsizing[10] and the worker remained employed in that plant through the end of that year. Again, this definition of comparison group is similar to definitions that have previously been applied in the literature (e.g. Huttunen et al., 2018; Black et al., 2015; Davis and von Wachter, 2011; Couch and Placzek, 2010; Jacobsen et al., 1993), where the comparison group is often defined to comprise workers who are never separated from their plant of employment. The definitions of the displaced and comparison group imply that workers who remained employed in plants that underwent a mass layoff are excluded from the sample, as are workers who separate from non-downsizing plants.

To demonstrate our empirical model, consider the sample of displaced and comparison workers constructed for baseline year (b) 1997. For these workers, we can observe crimes over calendar years (t) 1992–2008 or, equivalently, over *relative* years τ=−5 to τ=9 (where τ=t-b). Following the literature, we employ various specifications of a distributed lag model, here illustrated by a linear probability model (we will also apply logit models):

$$\Pr\left(c_{i\tau}=1\right)=\alpha x_i+\gamma_\tau+\sum_{k=-5}^{9}\delta_k d_i^k+\varepsilon_{i\tau} \tag{1}$$

where

- $c_{i\tau}$ indicator that worker *i* commits at least one crime in relative year τ (with τ =−5,−4,..,9 for baseline year 1997)
- x_i vector of control variables measured at the beginning of the baseline year (see Appendix B for details)
- γ_τ vector of fixed effects associated with each relative year
- d_i^k dummy variables set to one for *displaced workers* in the k$^{\text{th}}$ relative year, otherwise zero (with k=−5,−4..,9 for baseline year 1997)
- $\varepsilon_{i\tau}$ error term with expectation zero.

The main parameters of interest are the δ_k coefficients which capture the difference in the likelihood of crime between workers in the displacement and comparison group in each relative year, from 5 years preceding the displacement (of the displaced workers) to 9 years after. If displacement increases crime, we would expect δ_0 to be higher than the δ_k in pre-displacement years, i.e. δ_{-5} to δ_{-2}. Estimates of δ_k pertaining to subsequent post-displacement years (δ_1, δ_2, ..) allow us to explore the extent that the crime response to displacement fades (or possibly increases) over time.

Notably, we do not regard estimates of δ_{-1} as (strictly) pertaining to the "pre-displacement period" for two main reasons. First, as mentioned earlier, Norwegian workers are notified in advance of an impending displacement, and they may in many instances foresee and prepare for their plant failing well before layoff (Basten et al., 2016). To the extent *knowledge* of an impending layoff operates on criminal behavior, as it might under either the *earnings replacement* or *mental distress/self-control* mechanisms, estimates of δ_{-1} would capture those effects. Furthermore, it is known that separation dates are not recorded with 100 percent accuracy, with ample evidence that separations sometimes occurred earlier than what is recorded in employment registries.[11] This would also contribute to us estimating a displacement effect that appears to pre-date the displacement event.

To produce specific estimates of the displacement effect, we rely on a standard DID assumption: that any difference in pre-displacement crime rates would have persisted if not for the displacements the displaced workers experienced. Econometrically, this assumption is implemented by modifying our model as follows:

$$\Pr\left(c_{i\tau}=1\right)=\alpha x_i+\gamma_\tau+d_i+\sum_{k=-1}^{9}\delta_k d_i^k+\varepsilon_{i\tau} \tag{2}$$

where d_i is the fixed effect associated with being in the displaced group, and the time-varying effects of displacement are only modeled for relative years −1 going forward. Under the DID assumption, the δ_k coefficients provide causal estimates of the crime effect of displacement in each year relative to the pre-displacement years (−5 through −2).

The fact that we have panel data allows us to define displacement and comparison groups for multiple baseline years. To maximize power, we therefore stack the data for each of the baseline years, and run regressions on the pooled data (see Huttunen et al., 2018 or Black et al., 2015 for a similar procedure). Baseline year, relative year and calendar year are thus defined for all workers in both the comparison and the treatment group, which introduces a few additional considerations. First, it is possible for some workers to be displaced in multiple baseline years. To simplify matters, we only consider the first baseline year in which such a worker is displaced. Second, it is possible for a worker to be displaced in one baseline year and belong to the comparison group in another baseline year. To avoid "partly-treated" workers in the comparison group, we do not allow a worker who is in the displacement group to be in the comparison group of any baseline year. Third, to generalize the model to the pooled data, and to account for common calendar year shocks, we include indicators to capture the calendar year effects.[12] Finally, in all regressions we cluster on the individual worker, but we also explore how the estimated standard errors are affected by restricting workers in the comparison group to be present in no more than one baseline year.

Our estimation strategy is a straightforward generalization of the "difference-in-differences" method, and it thus relies on the comparison group to account for changes in crime rates over time that would have occurred in the absence of displacement. The crucial assumption for a consistent estimate of the displacement effect is that the crime rates in the displacement and comparison groups would have evolved similarly over time in the absence of the displacement. This assumption can be tested, to some degree, by comparing the evolution of crime rates in the two groups during the pre-displacement period.

It might be worth drawing attention to a couple of distinctions with respect to what conceptual effects this approach does *not* attempt to estimate. First, it does not estimate the effect of *exposure* to mass layoff on crime. As long as the plant does not close completely, a number of workers are retained in the plant during and after the mass layoff. There are several studies that look at the average effect (on various outcomes) of exposure to plant downsizing over both laid off and retained workers, and some have argued that this average is more policy relevant than the effects specific to laid off workers (e.g. Rege et al., 2011, 2009). Indeed, some studies indicate that adverse effects on the retained workers could be as severe, or even more severe, than the adverse effects on the laid off workers (Vahtera and Kivimaki, 1997). However, what we attempt to estimate in the current study is the effect on the displaced workers only, neglecting possible effects on the workers who remain in the plant through and after the mass layoff.

Second, it does not estimate the effect of *unemployment* on crime. A number of the workers separated from their plant during a mass layoff could be directly entering a new job in another plant. Indeed, some of these workers may not even leave involuntarily, but may have got a better offer elsewhere around the time their plant downsized. What we estimate is therefore the overall effect of job separation in association with a mass layoff over all separating workers, including those who go directly into another job, those who undergo a period of unemployment,

[10] Either in the baseline year, or the adjacent-to-baseline years.

[11] For instance, a fair number of disability program entrants appear to still be employed fulltime (in their prior plant) for a few months after disability entry. For this reason, we always exclude from our sample of "workers" persons on social benefits that should have precluded fulltime work.

[12] We could have alternatively included fixed effects for baseline year with identical results, as baseline year, relative year and calendar year are perfectly collinear.

and those who drop out of the workforce altogether. As discussed in Section 2, the mechanisms through which displacement increases crime are presumably stronger for those undergoing a period of involuntarily unemployment following the displacement.[13]

4. Data

4.1. Data sources

To estimate the effect of job displacement on crime, we combine two register databases provided by Statistics Norway. The databases can be merged using a unique personal identifier provided to every Norwegian resident at birth or immigration. The first database contains complete records of criminal charges for every Norwegian resident over the period 1992–2009. We utilize offenses committed through 2008 to allow for a registration lag between the time an offense is committed and the charges. The database contains all serious crimes, but also misdemeanors like drunk driving, excessive speeding and shop lifting. A person is registered as “charged” if the police perform an investigation and conclude that the person did commit the recorded crime, and the case is considered solved. The investigation may be initiated by the police receiving a report or by an arrest. The registration is independent of the further outcome of the case (filing of formal charges, prosecutions or convictions).[14] Date of crime and detailed codes of “offense type” are also included on charge records. Statistics Norway has constructed subcategories of crime and we rely on these definitions to construct crime categories that correspond to those used by the US FBI (see Appendix A).

The second database is called *FD-trygd*. It is a rich longitudinal database with records for every Norwegian resident from 1992 to 2008 (for most variables), containing individual demographic information (e.g. sex, age, marital status), socio-economic data (e.g. education, income), current employment status, industry of employment, indicators of participation in Norway’s welfare programs, and geographic identifiers of area of residence.

In particular, *FD-trygd* contains records for timing of employment “events” since 1995. These events, captured by individual and date, include entries into and exits out of employment, changes in employment status (full time, part time, minor part time), and changes in plant and firm of employment. The employment records are constructed by data analysts at Statistics Norway from raw employment spell records submitted by employers, and verified against employee wage records to ensure the validity of each spell and to eliminate records pertaining to “secondary” employment spells.[15]

Based on the employment records, we constructed plant-level employment counts at the end of years 1995 to 2008. The counts were constructed as measures of full-time equivalents (FTEs), with part time and minor part time employment measured as 0.67 and 0.33 FTEs, respectively. Excluded from these counts were any person identified in *FD-trygd* as self-employed or receiving assistance that should have precluded full time work (rehabilitation pensions, disability pensions, etc.). The annual plant FTE were then used to identify separations that were associated with a mass downsizing as described in Section 3.

4.2. Defining analytic sample

Our main analytic sample consists of men between 18 and 40 years of age at the beginning of the baseline year. We restrict to non-elderly men because crime rates among women and older men are too low to provide estimates with any precision (Statistics Norway, 2008; Freeman, 1996; Hirschi and Gottfredson, 1983). Moreover, to study effects on crime of job displacement, men in our sample were required to have had reasonable attachment to an established job. Specifically, we restrict the main analytic sample to men who were full-time employed preceding the baseline year, excluding a few cases where the man received assistance that should have precluded full time work, such as disability benefits. We also require the men to have at least two years of tenure in the plant at the beginning of the baseline year, to ensure durable attachment to one’s current plant of employment. As a precaution against the plant downsizing variable being correlated with unobserved individual determinants of crime, we exclude men working in a plant with less than 10 FTEs at the beginning of the baseline year.

We construct our main analytic sample by appending the 10 baseline year datasets (1997–2006) together, yielding a panel dataset with 10,526,937 person-year observations. The dataset consists of 361,385 different men in the ten baseline years, with 83,974 different men in the displacement group and 277,411 different men in the comparison group. As mentioned, the displaced men are present in one baseline year only, while more than 90 percent of the men in the comparison group appear in several baseline years.[16] For all men we can observe crimes over the 17 years 1992–2008, but to avoid that the panel becomes highly unbalanced for the early (1997) and late (2006) baseline years, we only use crime data for the 11 relative years −5 to 5.[17]

4.3. Summary statistics

Variables capturing individual and plant socio-economic characteristics were constructed based on *FD-trygd* records pertaining to the beginning of the baseline year. A number of these variables (x) are included as covariates in our estimation models (see Appendix B for details). Summary statistics are presented in Table 1 for our main analytic sample in the baseline year. About 8 percent[18] of the sample was displaced. The average age in the sample is about 34 years, and 38 percent of

---

[13] It is tempting to imagine the causal effect of *unemployment* could be investigated by using mass downsizing events as an instrument. To our mind, this exercise makes no sense unless we imagine *unemployment* to be the sole mechanism through which downsizing affects criminal behavior. The results we present indicate this isn’t true.

[14] A problem that should be kept in mind when measuring results from any empirical study of crime is the difficulty in measuring latent criminal activity. Self-reports of criminal activity should be interpreted cautiously since they are often impossible to validate and since the extent of truthful self-reporting is lower among subjects with an extensive criminal record than among subjects with little or no criminal history (Kirk, 2006; MacDonald, 2002; Farrington et al., 2003; Hinderlang et al., 1981). Crime data from registries have the advantage that offenders cannot choose not to be registered. The main disadvantage of register data is that crimes which are not reported to the police are not recorded, and crimes left “unsolved” cannot be matched to a specific individual.

[15] If an individual was employed in multiple plants at a given time, primary employment was determined by employment status and recorded income from each source of employment. A plant’s identifier is only supposed to change if at least two of the three following conditions are met at the same time: geographical relocation, change of industry and new owner. In reality, and especially within firms, plant identifiers may change even if a large proportion of the same employees remain working together. Though such measurement issues may attenuate our results somewhat, the most typical cases of restructuring should be captured by utilizing *firm* identifiers in defining permanent displacement (see Section 3).

[16] On average, a worker in the comparison group is present in 3.4 baseline years. As mentioned, we will always cluster on the individual worker, and we also explore how the estimated standard errors are affected if we allow workers in the comparison group to be included in only one baseline year.

[17] This ensures that the panel is fully balanced in the five years prior to the baseline year (1997-5=1992) and up to relative year +2 (2006+2=2008), while for relative year +3 and after it becomes unbalanced (since we do not have crime data after 2008).

[18] As mentioned, workers in the comparison group are present in 3.4 baseline years on average. This implies that the rate of unique men in our main analytic sample who were displaced is much larger than 8 percent, it is 24 percent. Recall that all the men who were *not* separated from a plant in association with a mass layoff (or who e.g. worked in plants with less than 10 FTE, cf. the exclusion restrictions described above) are excluded from our main analytic sample,

Table 1
Summary statistics.

Variable	All	Displaced group	Comparison group	Difference
Displaced	0.08			
Age	33.8	33.3	33.9	−0.54**
	(4.7)	(4.9)	(4.7)	
Compulsory school only	0.07	0.08	0.07	0.01**
High school only	0.65	0.65	0.65	0.00
More than high school	0.28	0.27	0.28	−0.01**
Educ. Missing	0.00	0.00	0.00	−0.00
Earnings	354,600	354,500	354,600	0.81
	(184,200)	(197,200)	(183,000)	
Tenure	5.6	4.7	5.6	−0.99**
	(3.1)	(2.9)	(3.1)	
Married	0.39	0.37	0.39	−0.02**
Children (below 18)	0.55	0.52	0.55	−0.03**
FTE of plant	298.0	224.3	304.8	−80.5**
	(721.8)	(475.8)	(739.6)	
Crime in baseline year	0.019	0.028	0.018	0.009**
# observations	1,019,940	83,974	935,966	

Notes: Variables are measured at the beginning of the baseline year (operationalized as the end of the relative year −1) unless otherwise specified. Standard deviations in parentheses.* and** indicate that the variable is significantly different across the group of displaced and comparison workers at the 5 and 1 percent level (two-sided *t*-test).

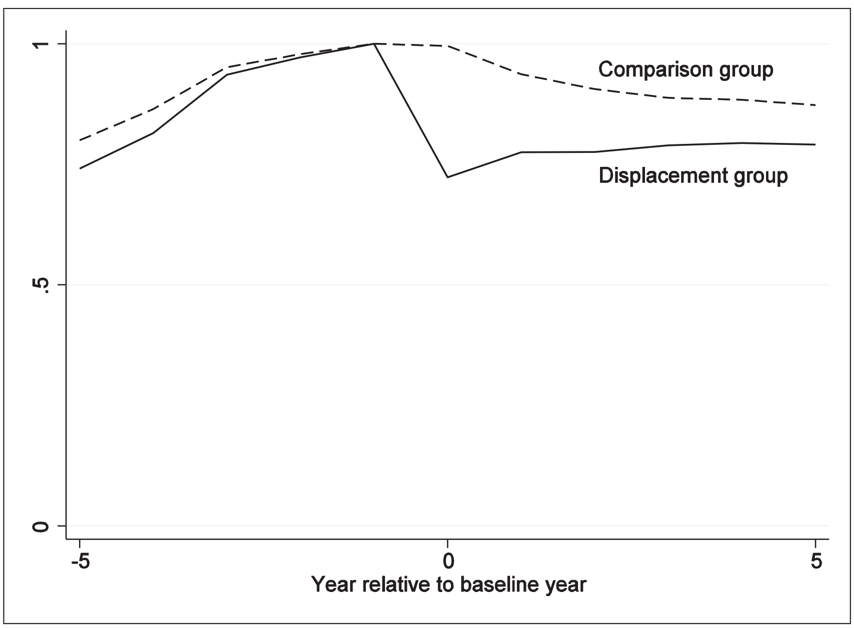

Fig. 1. Proportion full-time employed around the baseline year (+/- 5 years).

the men in the sample were married. The displaced and the comparison group differ on observables, but in general the magnitude of the differences is quite small. The displaced are about half a year younger than the men in the comparison group, and are somewhat less educated, but their (pre-displacement) earnings are similar. We also see that they had somewhat shorter tenure and that they worked in smaller plants. Fig. 1 shows the development over time in the rate of full-time employment for the two groups. By construction of the dataset, everyone is required to be full-time employed at the start of the baseline year, and as expected, we see that full-time employment drops substantially for the displaced in the baseline year; before it starts to converge to the comparison group. Overall, the displacement and comparison groups are fairly similar, but Table 1 shows some deviations which indicate the need to consider robustness to controlling for pre-existing differences across the two groups.

5. Empirical findings

5.1. Main results

The two thick lines in Fig. 2 show the evolution of crime rates, relative to the baseline year, for the displaced (solid) and the comparison (dashed) group. We see that the crime rate of the displaced is generally above that of the comparison group, but the trend in crime for the comparison group is similar to the trend for the displaced during the pre-displacement period. This is illustrated with the thin dashed line, which is calculated by adding the mean pre-displacement (τ<−1) difference in crime rates to the crime rate of the comparison group in every year. It is evident that the relative crime rate of the displaced increases around the time of displacement, while there is no similar

implying that fewer than 24 percent of *all* employed men in Norway experienced a separation in association with a mass layoff over the period.

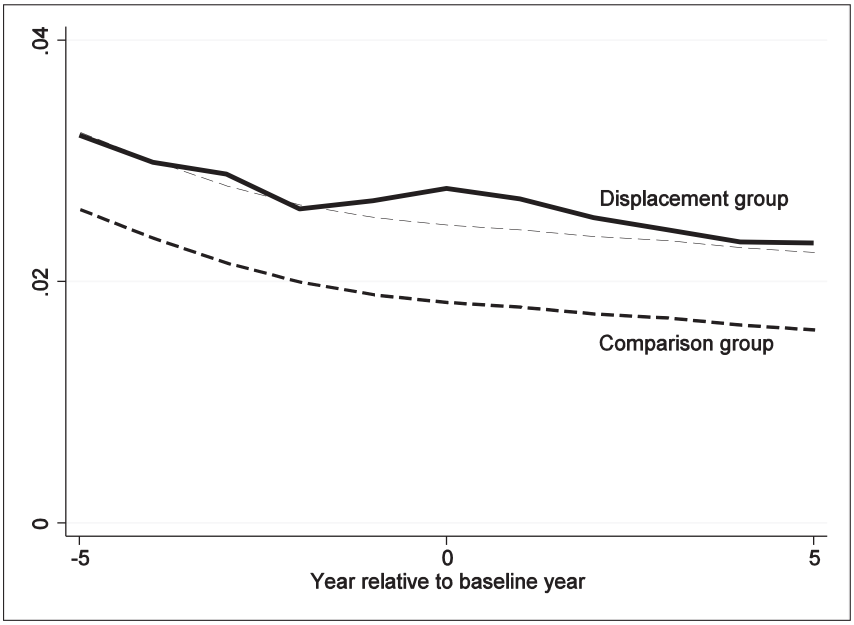

Fig. 2. Proportion charged of crime around the baseline year (+/- 5 years).

increase for the comparison group around the baseline year. This is what we would expect if job displacement results in more crime.

Table 2 presents regression results. Model 1 shows OLS regression results for Eq. (1) with no control variables, which simply provides the differences in crime rates between the displaced and comparison groups, i.e. the difference between the two thick lines in Fig. 2. We see that the difference in the crime rate fluctuates around 0.6–0.7 percentage points over years τ=−5 to τ=−2. Then the difference rises to 0.9 percentage points in the baseline year (τ=0), before declining in subsequent years. Controlling for age and calendar year fixed effects in Model 2 reduces each of the point estimates somewhat, but the differential change from the pre-displacement to post-displacement years is slightly larger.

Model 1 and 2 and Fig. 2 show that the displaced have a higher crime rate than the comparison group over the years preceding the displacement, indicating that the displaced are more crime-prone irrespective of any exposure to job displacement.[19] As discussed in Section 3, this is not a concern for our difference-in-differences identification strategy as long as the crime rate in the displaced group would have evolved similarly over time (in the absence of displacement) as it does for the comparison group. In this respect, it is reassuring that the trend in crime rates is similar for the displacement and comparison groups in the years preceding displacement.

To obtain a difference-in-differences estimator, we include in Model 3 a dummy variable identifying displaced workers and omit the displacement terms pertaining to the pre-displacement period, as in Eq. (2). In doing so, we effectively "difference out" the mean pre-displacement difference in crime rates observed across the two groups. Our estimates in Model 3 therefore capture the effect of displacement under the assumption that pre-existing differences in crime rates across the two groups would have remained unchanged (conditional on the included covariates) in the absence of displacement. Our estimates indicate a pre-displacement difference in crimes rates of 0.44 percentage points between the displaced and comparison group (conditional on age and calendar year dummies). The estimated effect of displacement on crime rates in the baseline year is 0.38 percentage points. This effect estimate is hardly affected by adding a rich array of control variables (Model 4) or individual fixed effects (Model 5), but we note that the individual fixed effects model reveals a more clear-cut decline in the effect of displacement on crime in the years after displacement, and no statistically significant effect remains after 4 years.[20]

The dependent variable is dichotomous with a mean close to zero, which suggest that the logit model is, for example, more efficient than OLS. Models 6–8 present estimated odds ratios from logit models that correspond to the OLS Models 2–4.[21] From the implied marginal effects (reported in square brackets) we see that the logit and the OLS models produce similar estimates, and, more importantly, that the time pattern of the logit estimates also indicate a positive effect of job displacement on crime. The results in Model 4 (OLS) and in Model 8 (Logit) indicate that job displacement increases the probability of committing crime by about 20 percent in the baseline year,[22] with estimated effects that weaken in subsequent years. In the following, we will use Model 8 as our model of reference.

5.2. Robustness of main results

Table 3 presents several robustness checks. First, the results presented in Model 1 are from the exact same model as the one presented in Model 8 of Table 2, but now we only report the estimated coefficients for relative years −1 to +2 and the dummy indicating that the worker is in the displacement group.

One concern with our reference model is that the same man can be present in the comparison group in several baseline years. While this

[19] There could be selection at the plant level, for example if plants with oscillating employment stocks are only able to attract more crime-prone workers. There could also be selection at the individual level, for example if firms are laying off more crime-prone men first in association with mass layoffs. We return to the empirical relevance of these potential sources of bias below.

[20] In the subsequent tables we will restrict attention to the estimates for −1 to +2. We do this for three reasons. First, point estimates that might have been somewhat different or not significant in the individual fixed effects model are not reported. Second, point estimates that might be biased due to unbalanced panel (the panel becomes unbalanced from +3; see footnote 17) are not reported. Third, it succinctly conveys the point estimates of main interest.

[21] Negative binomial models took an excessively long time to converge, and often failed to converge altogether. For models that did converge, like the one corresponding to Model 7 of Table 2, results were qualitatively the same as those reported.

[22] Dividing the marginal effect estimate (0.38 percentage points) from Model 4 by the baseline crime rate (1.96 percent) yields a relative effect of 19.4 percent. The mean marginal effect implied from the logit estimate of Model 8 (provided in square brackets) produces a nearly identical relative effect estimate.

Table 2
Main results: effect on crime of being displaced in relative year 0 (baseline year).

		Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Dependent variable: Any crime in the given relative year									
Displaced (dummy)				0.0044**	0.0038**			1.1933**	1.1713**
				(0.0004)	(0.0004)			(0.0163)	(0.0160)
								[0.0035]	[0.0031]
Estimate of effect of displacement in given relative year	−5	0.0061**	0.0038**				1.1522**		
		(0.0006)	(0.0006)				(0.0244)		
							[0.0028]		
	−4	0.0062**	0.0041**				1.1765**		
		(0.0006)	(0.0006)				(0.0258)		
							[0.0031]		
	−3	0.0073**	0.0054**				1.2446**		
		(0.0006)	(0.0006)				(0.0279)		
							[0.0043]		
	−2	0.0060**	0.0043**				1.2108**		
		(0.0006)	(0.0006)				(0.0284)		
							[0.0038]		
	−1	0.0077**	0.0064**	0.0020**	0.0019**	0.0017**	1.3265**	1.1115**	1.1085**
		(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0308)	(0.0263)	(0.0264)
							[0.0055]	[0.0021]	[0.0020]
	0	**0.0093****	**0.0082****	**0.0038****	**0.0038****	**0.0035****	**1.4367****	**1.2039****	**1.1987****
		(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0329)	(0.0285)	(0.0286)
							[0.0071]	[0.0036]	[0.0035]
	1	0.0089**	0.0080**	0.0036**	0.0035**	0.0032**	1.4319**	1.1999**	1.1912**
		(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0332)	(0.0293)	(0.0293)
							[0.0070]	[0.0036]	[0.0034]
	2	0.0079**	0.0071**	0.0026**	0.0026**	0.0022**	1.3902**	1.1650**	1.1537**
		(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0331)	(0.0293)	(0.0292)
							[0.0065]	[0.0030]	[0.0028]
	3	0.0072**	0.0065**	0.0020**	0.0020**	0.0015*	1.3635**	1.1426**	1.1323**
		(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0345)	(0.0304)	(0.0303)
							[0.0061]	[0.0026]	[0.0024]
	4	0.0068**	0.0061**	0.0017**	0.0017**	0.0010	1.3593**	1.1390**	1.1261**
		(0.0006)	(0.0006)	(0.0007)	(0.0007)	(0.0007)	(0.0371)	(0.0326)	(0.0324)
							[0.0060]	[0.0026]	[0.0023]
	5	0.0071**	0.0065**	0.0020**	0.0021**	0.0012	1.3873**	1.1625**	1.1548**
		(0.0006)	(0.0006)	(0.0007)	(0.0007)	(0.0007)	(0.0411)	(0.0359)	(0.0359)
							[0.0064]	[0.0030]	[0.0028]
Estimation model		OLS	OLS	OLS	OLS	OLS FE	Logit	Logit	Logit
Covariates included (in addition to dummies for crime in comparison group in relative years)		No controls	No controls except dummies for age and calendar year	No controls except dummies for age and calendar year	All observed controls given in Appx. B	No controls except dummies for age, calen-dar year and individual fixed effects	No controls except dummies for age and calendar year	No controls except dummies for age and calendar year	All observed controls given in Appx. B
Mean of dependent variable in comparison group		0.0196	0.0196	0.0196	0.0196	0.0196	0.0196	0.0196	0.0196
R-squared		0.0006	0.0104	0.0104	0.0146	0.1696			
N		10,526,937	10,526,937	10,526,937	10,526,937	10,526,937	10,526,937	10,526,937	10,526,937

Note: Estimates of how much higher the likelihood of crime is among the displaced than the comparison group (and the pre-displacement period of the displaced for Models 3–5 and 7–8) in the given relative year (0 indicates the baseline year). Odds-ratios reported for the logit estimations with implied mean marginal effects in square brackets, and marginal effects reported for OLS regressions. * and ** denote significance at the 5 and 1 percent levels. Robust standard errors in parentheses corrected for non-independent observations for the same individual.

should not bias the point estimates, it raises concerns that the estimated standard errors are too small (but recall that this concern is limited by the fact that we always cluster on the individual level). In Model 2 of Table 3 we have randomly selected no more than one baseline year for each worker in the comparison group.[23] As expected, this produces similar point estimates as in our reference model, but the sample size drops substantially and the estimated standard errors become bigger.

[23] To create a sample representative of our original comparison group sample, this was done as follows. First, each comparison group worker had an $n/10$ probability of being included in the sample, where n represents the number of times the worker was included in the original comparison group sample. (Recall, workers in the comparison group could be included for up to 10 baseline years). Next, for included workers, one of their records was randomly chosen for inclusion. If we had omitted the first step, the restricted comparison group sample would have been over-represented by workers with less consistent employment.

Table 3
Robustness checks of the effect on crime of being displaced in relative year 0 (baseline year).

		Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Dependent variable: Any crime in the given relative year									
Estimate of effect of displacement in given relative year	−1	1.1085**	1.1052**	1.0735	1.0784**	1.1317**	1.1282**	1.0959**	
		(0.0264)	(0.0377)	(0.0368)	(0.0254)	(0.0497)	(0.0373)	(0.0268)	
		[0.0020]	[0.0022]	[0.0014]	[0.0016]	[0.0019]	[0.0021]	[0.0018]	
	0	**1.1987****	**1.2011****	**1.1785****	**1.1336****	**1.1124***	**1.1993****	**1.1930****	**1.1628****
		(0.0286)	(0.0412)	(0.0400)	(0.0266)	(0.0503)	(0.0400)	(0.0294)	(0.0307)
		[0.0035]	[0.0041]	[0.0031]	[0.0026]	[0.0016]	[0.0032]	[0.0034]	[0.0027]
	1	1.1912**	1.1879**	1.1441**	1.1380**	1.2257**	1.1893**	1.1927**	1.1766**
		(0.0293)	(0.0414)	(0.0402)	(0.0275)	(0.0546)	(0.0411)	(0.0303)	(0.0317)
		[0.0034]	[0.0038]	[0.0026]	[0.0027]	[0.0031]	[0.0031]	[0.0034]	[0.0029]
	2	1.1537**	1.1788**	1.1088**	1.1042**	1.2058**	1.1464**	1.1466**	1.1480**
		(0.0292)	(0.0423)	(0.0400)	(0.0275)	(0.545)	(0.0407)	(0.0300)	(0.0316)
		[0.0028]	[0.0037]	[0.0020]	[0.0021]	[0.0028]	[0.0024]	[0.0026]	[0.0025]
Displaced (dummy)		1.1713**	1.1575**	1.1313**	1.1240**	1.1568**	1.1861**	1.1916**	1.1399**
		(0.0160)	(0.0203)	(0.0211)	(0.0149)	(0.0282)	(0.0226)	(0.0170)	(0.0161)
		[0.0031]	[0.0033]	[0.0024]	[0.0024]	[0.0022]	[0.0030]	[0.0034]	[0.0023]
Sample redefinitions		Reference model (i.e. Model 8, Table 2)	Comparison group is a random draw from main sample, which ensures that an individual is never present in more than one base-line year (see Section 5.2. for details)	Displaced if PDR>0.9 (instead of PDR>0.3)	Workers in comparison group remain in plant through −1 (instead of through baseline year)	Tenure≥5 (instead of tenure≥2)	Plant size≥50 (instead of plant size ≥10)	Comparison group if PDR≤0.1 (in-stead of PDR≤0.3)	Excluding workers who committed crime in t-1 (instead of in-cluding them)
Mean of dependent variable in comparison group		0.0196	0.0198	0.0196	0.0212	0.0152	0.0179	0.0191	0.0180
N		10,526,937	1,842,131	10,093,950	12,931,965	5,600,492	6,147,571	7,384,630	9,321,536

Note: Estimates of the effect of displacement (in the baseline year, denoted 0) on crime (dummy) in the given relative year. Estimated using logit models (odds-ratios reported, with implied mean marginal effects in square brackets). All covariates described in Appx. B included in all models (coefficients for them and for effect estimates in years 3–5 not reported). * and ** denote significance at the 5 and 1 percent levels. Robust standard errors in parentheses corrected for non-independent observations for the same individual.

The point estimates, however, remain significant at the one percent level.

Another concern is that less productive workers might be the first to be laid off in association with mass-layoffs. To the extent that laid off workers commit more crime irrespective of displacement, this would not bias our effect estimate of the overall effect of job displacement on crime (since we contrast crime rates after displacement with rates of the same men before displacement). Bias could arise, however, if the criminal behavior of such workers was more responsive to displacement. We can get an idea of this possible bias by restricting the sample to workers separated from plants that closed, since closing plants are not retaining employees and thus have no discretion with respect to whom to lay-off. In Model 3 of Table 3 we restrict the definition of the displaced to workers separated from a plant that downsized by more than 90 percent (and the comparison group remains the same as before). As we can see, this reduces the point estimates somewhat, suggesting some differential selection of more crime-prone workers in our main *displaced worker* sample. Nonetheless, the estimates remain large and highly significant.

In Model 4 we remove the requirement that the workers in the comparison group remain in the plant throughout the baseline year. This requirement could generate selection of less crime-prone workers (on unobservables) into the comparison group. Removing this requirement also implies, however, that the comparison group can now include workers who are separated from a plant in association with a smaller downsizing (e.g. with mass layoffs of 29 percent). One may argue that this results in some partly treated (i.e. separated in association with 29 percent downsizing) workers ending up in the comparison group, thereby attenuating the effect estimate.[24] In line with what we would expect from the attenuation story, we see from Model 4 that the effect estimate of the baseline year declines somewhat under this restriction, but it remains significant.

In Models 5 and 6 we check for robustness to the sample selection criteria related to increasing the requirements for tenure and plant size. The effect estimate for the baseline year is somewhat lower when we require tenure of at least 5 years (instead of 2 years), but the effect estimates are larger in years 1 and 2 (see Model 5). Restricting to plants with at least 50 employees (instead of 10) produces effect estimates (Model 6) that are almost identical to the estimate of our reference model. Finally, we check that the results are not sensitive to the downsizing requirement of the comparison group. In the main specification we required that the plant of employment did not downsize 30 percent or more around the baseline year, while in Model 7 we have changed this requirement to

[24] As noted in Section 3, previous studies have typically required that the workers in the comparison group remain in their plant of employment throughout the observation window (for us that could be through +5). The advantage of this requirement is that effect estimates are not attenuated by the presence in the comparison group of workers who are laid off in association with downsizing events after the baseline year ("partly treated"). The possible disadvantage is that the comparison group then comprises very stable workers who may exhibit different trends in criminal behavior (e.g. steeper declines over time), which might result in upward-biased effect estimates.

Table 4
Effects on labor market attachment of being displaced in relative year 0 (baseline year).

Dependent variable:		Model 1 Any crime	Model 2 FT	Model 3 Unemployed	Model 4 Earnings (100 NOK)
Estimate of effect of displacement in given relative year	−1	1.1085**			64.97**
		(0.0264)			(5.21)
		[0.0020]			
	0	**1.1987****	**0.0141****	**7.5097****	**−39.37****
		(0.0286)	(0.0003)	(0.1478)	(6.47)
		[0.0035]	[−0.3276]	[0.0872]	
	1	1.1912**	0.2948**	4.8932**	−190.51**
		(0.0293)	(0.0036)	(0.0903)	(6.61)
		[0.0034]	[−0.0940]	[0.0687]	
	2	1.1537**	0.4745**	2.8280**	−127.45**
		(0.0292)	(0.0058)	(0.0554)	(7.52)
		[0.0028]	[−0.0573]	[0.0450]	
Displaced (dummy)		1.1713**	0.8632**	1.1469**	−22.65**
		(0.0160)	(0.0067)	(0.0119)	(3.25)
		[0.0031]	[−0.0113]	[0.0059]	
Estimation model		Logit	Logit	Logit	OLS
Mean of dependent variable in comparison group		0.0196	0.910	0.0480	3070
R-squared					0.5623
N		10,526,937	9,506,997	7,760,990	10,526,937

Note: Estimates of the effect of displacement (in the baseline year, denoted 0) on the given labor market attachment variables in the given relative year. Odds-ratios reported for logit models (with implied mean marginal effects in square brackets) and marginal effects for the OLS model. All observed covariates described in Appx. B included in all models (but estimates are not reported). Fewer observations are utilized in (logit) models 2 and 3 because there is no variation in the dependent variable for some categories (for example, we have required all workers to be full time employed in −1). * and ** denote significance at the 5 and 1 percent levels. Robust standard errors in parentheses corrected for non-independent observations for the same individual.

10 percent. We see that this hardly affects the estimates. Overall, our main results appear reasonably robust across variations in data definitions and model specifications.

A particular concern arises in our context due to the potential for reverse causality. Our identification strategy assumes the difference in crime propensities across the displaced and comparison groups are fixed over time. However, being charged with a crime could increase one's likelihood of being let go during a downsizing. At a minimum, this would lead to upwards biased estimates for the displacement effect in relative year −1. However, as we have noted, there are reasons to expect displacement crime effects to emerge prior to a worker's recorded displacement date, due to the advanced notice workers receive before a layoff and the incorrect (late) recording of some job separations. Nonetheless, the sizable estimates in relative year −1 might also partly reflect reverse causation, which we cannot rule out. And if criminal behavior in relative year −1 indicates a higher propensity for criminal behavior going forward (independent of displacement), the crime effects we estimate for subsequent years would then be upwards biased as well.

To investigate whether reverse causation substantially biases estimates pertaining to subsequent years, Model 8 estimates our main specification excluding from the sample all workers who were criminally charged in relative year −1. This is far from an ideal test. While we eliminate any workers for whom reverse causality is potentially relevant, we also eliminate workers who exhibited a true response to an impending (or mistimed) layoff. Eliminating these true responders, we would anticipate smaller effects estimated over the remaining sample of displaced workers. In light of that, the fact that estimates in Model 8 are only modestly smaller is reassuring. The odds-ratio estimate for relative year 0 is 18 percent smaller under this sample restriction, with even smaller differences (of 8 and 4 percent) in relative years 1 and 2.

The workers who were charged in relative year 0 may to an even larger extent than those charged in relative year −1, be responding to an impending or actual displacement, and thus when we exclude them from the sample the effect estimates (not reported) decline somewhat more (12 and 19 percent, instead of 8 and 4 percent). However, the change is within one standard error, and the main results remain similar and statistically significant.[25]

6. Mechanisms

In this section, we investigate the plausibility of alternative channels that might explain the estimated effects of job displacement on crime. We do so by estimating effects across crime categories (Section 6.1) and days of the week (Section 6.2).

As the labor market effects of displacement are key factors in the *earnings replacement* and *time availability* mechanisms, we first present evidence of them in Table 4. Model 1 replicates the crime effect estimates from our preferred specification. Model 2 demonstrates a substantial reduction (33 percentage points, cf. the implied mean marginal effects in square brackets) in the likelihood of being fulltime employed at the end of the baseline year, though fulltime employment rates recover as one would expect in subsequent years (6 percentage points reduction at the end of year 2). Similarly, Model 3 estimates a steep increase in the likelihood of drawing unemployment benefits, that also peaks in the baseline year and fades over time. The results for both full-time employment and unemployment therefore suggest an increase in time availability for displaced workers which peaks in the year of displacement, before declining in years 1 and 2. Model 4 demonstrates an earnings effect that is more delayed, peaking in year 1 and still larger in year 2 than in the baseline year. Interestingly, earnings for displaced workers in the year before displacement are significantly *larger* than predicted based on their earlier pre-displacement earnings, a result we cannot explain (see similar findings in Couch and Plazek, 2010 and Basten et al., 2016). Nonetheless, the results provide support that the *earnings replacement* and *time availability* mechanisms are both plausibly at work.

We would caution the reader against two unwarranted hypotheses these findings could inspire. First, even if the *earnings replacement*

[25] We have also run the regressions on the crime categories in Table 5 on samples excluding any worker charged in relative year −1 or 0, and the same pattern occurs: There is some decline in effect estimates, but the main results remain.

Table 5
Effects by crime category of being displaced in relative year 0 (baseline year).

Dependent variable:		Model 1 Any crime	Model 2 Traffic	Model 3 Violence	Model 4 Alcohol and drugs	Model 5 Property
Estimate of effect of displacement in given relative year	−1	1.1085**	1.0467	1.2246**	1.1850**	1.5165**
		(0.0264)	(0.0374)	(0.0848)	(0.0573)	(0.1389)
		[0.0020]	[0.0004]	[0.0005]	[0.0007]	[0.0005]
	0	**1.1987****	**1.1730****	**1.1990***	**1.3379****	**1.6339****
		(0.0286)	(0.0411)	(0.0864)	(0.0645)	(0.1483)
		[0.0035]	[0.0015]	[0.0004]	[0.0012]	[0.0005]
	1	1.1912**	1.1783**	1.1204	1.2994**	1.3580**
		(0.0293)	(0.0424)	(0.0844)	(0.0645)	(0.1267)
		[0.0034]	[0.0016]	[0.0003]	[0.0011]	[0.0003]
	2	1.1537**	1.1564**	1.0224	1.2254**	1.3480**
		(0.0292)	(0.0424)	(0.0825)	(0.0643)	(0.1269)
		[0.0028]	[0.0014]	[0.0000]	[0.0009]	[0.0003]
Displaced (dummy)		1.1713**	1.1649**	1.1611**	1.1929**	1.4327**
		(0.0160)	(0.0212)	(0.0457)	(0.0339)	(0.0675)
		[0.0031]	[0.0014]	[0.0003]	[0.0007]	[0.0004]
Mean of dependent variable in comparison group		0.0196	0.0093	0.0022	0.0041	0.0010
N		10,526,937	10,526,937	10,526,937	10,526,937	10,526,937

Note: Estimates of the effect of displacement (in the baseline year, denoted 0) on the given category of crime (dummy) in the given relative year. Estimated using logit models (odds-ratios reported, with implied mean marginal effects in square brackets). All covariates described in Appx. B included in all models (but estimates are not reported). * and ** denote significance at the 5 and 1 percent levels. Robust standard errors in parentheses corrected for non-independent observations for the same individual.

mechanism is operating (e.g. peaking in year 1), we should not necessarily expect the crime effects to follow the same time pattern. Rational actor theories would indicate the utility of illicit earnings increases the moment *expectations* about future income change, not when the lost earnings are realized. Second, even if the *time availability* mechanism is operating, we should not necessarily expect crime effects to decline at a similar trajectory. If those workers most prone to criminal behavior in response to a displacement are also those most likely to remain out of the workforce, high crime effects could persist even as employment rates begin to recover.

We unfortunately have no direct evidence regarding the mental distress that displaced workers experienced (if any), nor any direct evidence pertaining to diminished self-control – factors highlighted by the "general theory of crime" discussed earlier. Nonetheless, we believe the evidence below sheds *some* light on the importance of these mechanisms as potential contributors to the crime effects we estimate.

6.1. Category of crime

We now estimate the effect of plant closure on crimes of different categories. If the effect of job displacement on crime is largely driven by incentives to replace lost employment earnings with illicit earnings (the *earnings replacement* mechanism), we particularly expect to see an increase in crimes for profit, which fall in the category of *property crimes*.[26] On the other hand, if the *time availability* mechanism or *mental distress/self-control* mechanism are at work, we would expect to see a rise in other categories of crime as well.

In Table 5 we report estimates of the effect of displacement on the likelihood that the workers are charged with each of the four aggregate categories *violent* crimes, *property* crimes, crimes related to *alcohol and drugs,* and serious *traffic* violations (see Appendix A for details). We will focus on the relative effects (odds ratios), but since the underlying crime rate differs considerably across crime categories, we also report the implied marginal effects (in square brackets). Displaced workers have a significantly higher probability of being charged with all of the four crime categories. The estimated relative effect on violent crimes and crimes related to traffic, are roughly the same magnitude (Models 2 and 3) in the baseline year. However, in the case of violent crimes, the effect is large in relative years −1 and 0, before diminishing. In contrast, the effect on traffic crimes emerges in the baseline year and remains high through relative year 2. To some degree, the durability of the traffic violation effect might be related to re-employment in jobs requiring a longer commute (Evans and Graham, 1988). We find somewhat stronger relative effects for crimes related to alcohol and drugs (Model 4), which peak in the baseline year before diminishing somewhat in successive years. This observation aligns with Eliason and Storrie (2009) who find that displacement raises hospitalizations due to alcohol-related conditions. The biggest relative effects, however, are on property crimes, with estimates that peak in the baseline year before declining somewhat. In the baseline year, the effect estimate suggests that job displacement raises the likelihood of property crimes by about 60 percent (Model 5). The large effect on property crime suggests an important role for the earnings mechanism. However, the finding of sizable effects for non-acquisitive crimes would suggest other mechanisms are also at work.

The time patterns of these estimates also speak to potential mechanisms. Group-level differences in employment open in the year of displacement before closing somewhat over the years that follow (Fig. 1). If the crime effects were strictly a result of *time availability*, we would expect crime effects to moderate in relative years 1 and 2, as they generally appear to do. However, we would also anticipate only a small estimated effect in relative year −1, as an artifact of any mistimed separations in our data (discussed in Section 3). Instead, aside from traffic violations, the δ_{-1} estimates are sizable and significant, even exceeding the estimate of δ_0 for violent offenses. This would seem to indicate that *knowledge of an impending layoff* plays a meaningful role in contemporaneous criminal behavior. Rational actor theories could explain such a result for property crimes – a forward-looking worker perceives increased utility from illicit earnings once that worker recognizes future employment earnings are imperiled. But they cannot explain the large effects for violent and alcohol/drug offenses. Combined with the nature of these offenses, this would seem to suggest an important role for the *mental distress/self-control* mechanism in explaining these findings.[27]

[26] Not all property crimes, however, can be regarded as crimes for profit (e.g. most cases of *vandalism* and some cases of *arson* are solely destructive in nature).

[27] An alternative possibility is that the positive estimates of δ_{-1} are biased as a consequence of reverse causality. If the initiation of criminal activity *causes* some workers to be selectively displaced, we would expect a displacement "ef-

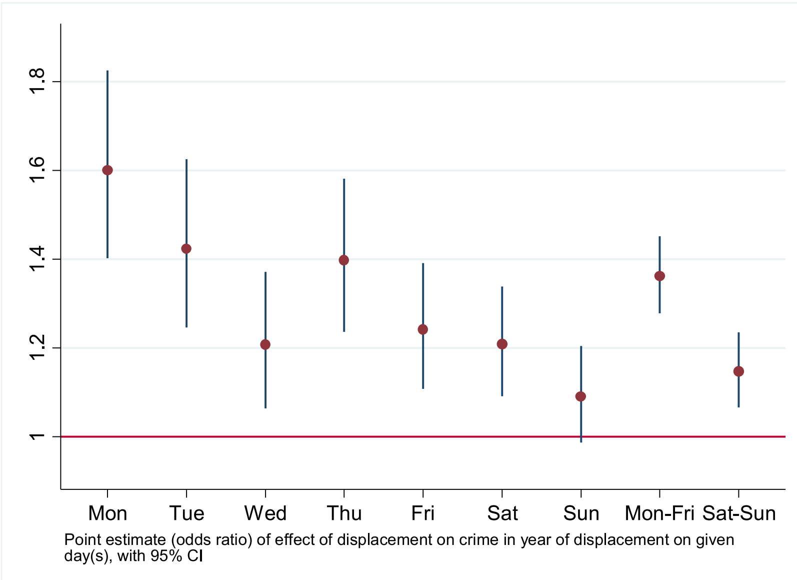

Fig. 3. Effect of displacement on crime (in baseline year) by day of week.

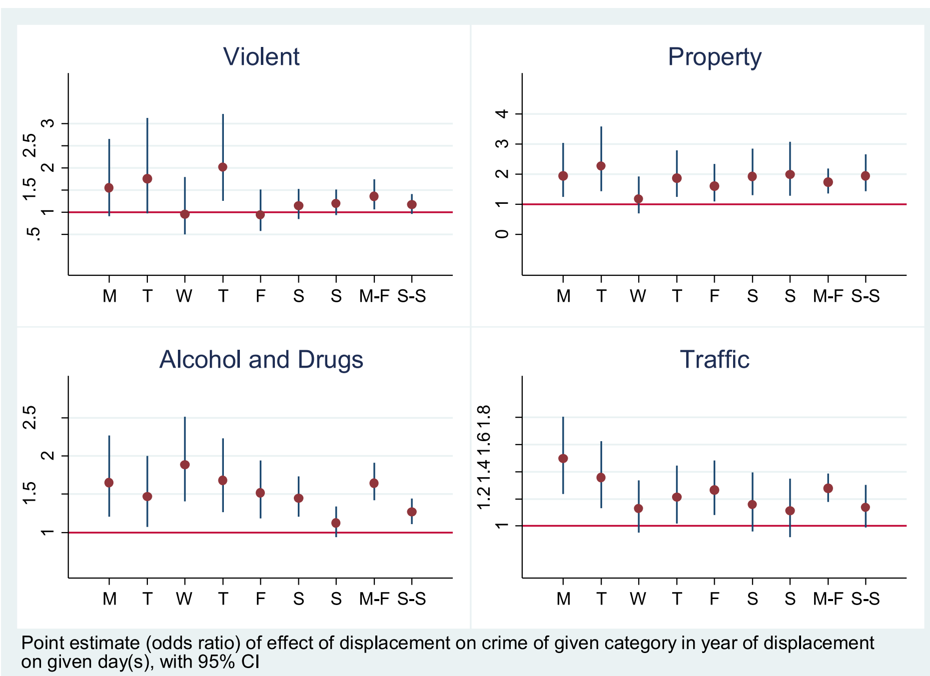

Fig. 4. Effect of displacement on crime (in baseline year) by crime category and day of week.

6.2. Crime by day of week

We now estimate the effect of job displacement on crimes committed on different days of the week. If displacement reduces the opportunity cost of spending time in illicit activities (*time availability* mechanism), we expect crime effects to be more pronounced on work days (when most paid work occurs) than on weekends.

In Fig. 3 we report odds-ratios (with 95 percent confidence intervals) of the relative effect of displacement on crime in the baseline year on the given day of the week, applying our reference model without[28] the large set of control variables (i.e. the approach of Model 7, Table 2) for each day individually. Fig. 3 also includes results from models where work days are grouped together, and where Saturdays and Sundays are grouped together. As we see, the estimated positive effect of displacement on crime holds both for crimes committed on work days and on weekends. However, the magnitude is larger on work days, which suggests that increased time availability (or the upheaval of structured daily routines) contributes to the overall effect of job displacement on crime.

We replicate this analysis by individual categories of crime in Fig. 4, and the results indicate that day-of-week effects differ across crime

fect" to emerge prior to the displacement event, with estimates in subsequent periods likely biased upwards as well. We are unable to fully rule this out this possibility, though the result in Table 3 (see Models 3 and 8, cf. discussion at end of Section 5.2) give us some reassurance that the resulting bias is not large.

[28] We were unable to have some of these models converge when including the full set of covariates (i.e. the approach taken in Model 8 of Table 2). For the models that did converge, the results were very similar to those reported here.

categories. For property crimes, the increase is similar on work days and weekends, suggesting little role for the time availability mechanism in the property crime effect. Instead, it appears that the reduction in earnings associated with job displacement – rather than the increased availability of time – induces the displaced workers to engage in property crime.

However, for crimes related to alcohol and drugs and for traffic violations, the displacement effect is driven by an increase in crimes committed during the work days. For crimes related to traffic, idleness could result in more driving during the work days, with corresponding exposure to being charged with traffic violations. Or it might be that the displaced workers take new jobs where they drive more or that are further away from home (with longer commute, cf. Evans and Graham, 1988), which also make them more exposed to being charged with traffic violations.

Less structured daily routines during work days could result in more consumption of alcohol and drugs. This is in line with a literature that links job displacement and involvement in crime to consumption of alcohol and drugs (Eliason and Storie, 2009; Schroeder et al., 2007; Crawford et al., 2006; Dawkins, 1997). Increased consumption of alcohol and drugs might also reflect mental distress (e.g. Dragano et al., 2005, Vahtera and Kivimaki, 1997). However, if the effect of displacement on alcohol/drug-related crimes was *solely* related to mental distress, we might expect that the effect would have been similar in magnitude on work days and weekends. Our results therefore indicate that displacement affects alcohol/drug crimes by increasing individual propensities for illegal alcohol/drug use, perhaps as a consequence of mental distress, but that the expression of this increased propensity is amplified by less structured daily routines on work days.

The estimated day-of-week effects of displacement on violent crimes are imprecise and somewhat more erratic than for other crime categories. Overall, there is little difference in the violent crime effect across work days and weekends, with only a slightly larger effect on workdays. Thus, time availability seems to play a minor role in explaining the violent crime effects of displacement, though we would add an important caveat to that. Card and Dahl (2011) demonstrate the importance of "victim availability" in the occurrence of violent behavior. Violent crime requires *victims*, who are perhaps more available on weekends than on work days, which could temper any differential work day effects due to time availability.

7. Conclusion

We have estimated the impact of job displacement on crime using a panel data set comprising Norwegian men below the age of 40. Our results suggest that being separated from the job in association with a mass layoff increases the likelihood of being charged of a crime by about 20 percent in the year of displacement, with ongoing effects that weaken over time.

To put the magnitude of this effect into perspective, we can conduct a back-of-the-envelope calculation for the extent job displacements might contribute to aggregate crime levels in Norway. Gorda (2016) provides evidence that the annual rate of worker transitions from employment to non-employment was approximately 5.5 percent in Norway over the 2005–12 period. She also finds that the annual rate of workers with twelve months in employment during two consecutive years who changed employer since last year, was 16 percent. While most of these job-to-job transitions are likely to be voluntary, some of the workers may, like in our sample, have been displaced and even experienced up to 12 months of non-employment. Thus, the rate of workers experiencing a form of displacement in a year – for which our crime estimates may be relevant - is likely to exceed 5 percent and be well below 20 percent (see also Schmieder et al., 2018 or Aurdal and Næsheim, 2015).

In 2010, the Norwegian workforce comprised about 2 million workers, of which roughly 500 000 were males under 40 years of age. If we assume an annual displacement rate of 10 percent for the 500,000 young male adults, this implies 50,000 annual displacements in this population. Applying the linear effect estimates pertaining to years 0 through 5 following displacement (in Table 2, Model 4), we would expect 800 additional individuals to be charged annually as a result of recently experienced displacements. This represents about one percent of the 84,000 persons criminally charged in Norway in 2010, and closer to 2 percent of the 50,000 men and women aged 18–40 charged in the year (Statistics Norway 2019).[29] Thus, while we find substantial effects of job displacement on crime, the importance of job displacement for aggregate crime levels seems moderate, maybe except in situations of extreme youth unemployment shocks with scaring effects (Bell et al., 2018; Fishback et al., 2010).

Bell et al. (2018) find that people who leave school during recessions are significantly more likely to lead a life of crime than those entering a buoyant labor market. If displacement for young males could have similar life-determining effects on crime as graduation in a recessions, the overall effect on national crime rates could of course be higher. Our findings may contribute to our understanding of how to shield displaced workers from entering crime, with possible long-term scaring effects.

In line with the predictions of traditional rational crime theory, as well as the existing literature analyzing the effect of area unemployment on crime rates, we find evidence that job loss especially increases the likelihood of property crime. We document the negative effect of displacement on labor market outcomes – in the form of lower future employment and earnings – which further supports the idea that displaced workers turn to illicit earnings opportunities in response to job loss. The finding that property crimes do not increase more on work days than on weekends, and the fact that this effect starts to emerge in the year *preceding* displacement, seems to indicate that it is the reduction in earnings (or the anticipation thereof) that induces property crime, rather than reductions in the opportunity cost of time.

However, unlike area-level studies (where results have been mixed), we also find compelling evidence that job displacement raises the rate of violent crimes, as well as crime rates for lower-level offenses (alcohol/drug offenses, serious traffic violations) that have received less attention in the literature. These findings imply that forces other than the *earnings replacement* mechanism are operating on the criminal behavior of displaced workers. Effects on non-property crimes are more pronounced on work days than on weekends, which suggest a role for *time availability* and the loss of structured daily routines.

Yet the *time availability* mechanism appears insufficient to explain other aspects of our findings. If this were the only mechanism, we would not expect an increase in non-property crime until *after* the displacement actually occurred. However, for both violent crime and alcohol/drug offenses, we find evidence of substantial increases in the year *preceding* displacement. As we have discussed, this suggests that impending job loss affects the workers, which, along with the nature of these crimes, appear most consistent with the *mental distress/self-control* mechanism. We have no way to directly test whether such a mechanism was operational in our sample of workers, though it seems likely given other published evidence (e.g. Dragano et al., 2005, Vahtera and Kivimaki, 1997).

Our findings in support of the *time availability* and *mental distress/self-control* mechanisms suggest policies targeting these mechanisms could be effective at inhibiting the crime response to displacement. For instance, policies that were successful at helping displaced young males remain engaged in structured daily activities could help reduce crimes arising as a consequence of idleness. However, we would anticipate that the most successful interventions for reducing the crime response of displaced male workers would arise from trying to address all three mech-

[29] Aggregate charged data for only men aged 18-40 is not available at Statistics Norway (2019). Of the overall 83,600 persons charged, more than 15 percent were women. Assuming a similar rate of women at age 18-40, would imply about 42,000 young adult men charged, of which 800 crimes comprise about 2 percent.

anisms simultaneously – as might be achieved through a well-executed retraining (return-to-work) program.

Our findings do highlight a specific target for policy intervention on the basis of the relatively large effects of displacement on drug/alcohol crimes. Given this finding, programs designed to discourage alcohol/drug abuse targeted to displaced young men, could yield sizable welfare improvements for the men, while also reducing crimes stemming from that abuse, which includes violence.[30]

As a concluding remark, we would remind the reader that these results pertain to a specific context (Norway), where employment rates and income levels are relatively high, rates of serious crime and incarceration are relatively low, and a generous social safety net exists which reduces the material hardship resulting from displacement. In countries where the financial implications of job loss are more severe, such as the US, we might anticipate a larger crime response operating through both the *earnings replacement* and *mental distress/self-control* mechanisms. Similarly, the extent that crime effects operate through the *time availability* mechanism presumably depends on the duration of non-employment suffered by the displaced. In conditions when displaced workers find it difficult to quickly regain employment, like during recessions, we should anticipate a larger crime response when workers are displaced.

Acknowledgements

We are grateful to the editor, reviewers, Gordon Dahl, Paul Devereux, Terje Skjerpen and seminar participants at The Harris School at the University of Chicago, the Northeast Ohio Economics workshop at the Federal Reserve Bank of Cleveland, and the Lunchtime Economic Seminar Series at Case Western Reserve University for comments and suggestions. Financial support from the National Science Foundation (SES-0417418) and the Norwegian Research Council (160965/V10) is gratefully acknowledged.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.labeco.2019.101761.

References

Addison, J.T., Teixeira, P., 2003. The economics of employment protection. J. Labor Res. XXIV (1), 85–129.
Agnew, Robert, 1992. Foundation for a general strain theory of crime and delinquency. Criminology 30, 47–87.
Aurdal, Per Svein and Helge Næsheim (2015). Statistikk om jobbstrømmer. Documents 2015/12, Statistics Norway.
Basten, C., Fagereng, A., Telle, K., 2016. Saving and portfolio allocation before and after job loss. J. Money Credit Bank. 48 (2–3), 293–324.
Baumeister, Roy F., Heatherton, Todd F., Tice, Dianne M., 1994. Losing Control: How and Why People Fail at Self-Regulation. Academy Press, San Diego, CA.
Becker, G., 1968. Crime and punishment: an economic approach. J. Politic. Econ. 76 (2), 169–217.
Bell, B., Bindler, A., Machin, S., 2018. Crime scars: recessions and the making of career criminals. Rev. Econ. Stat. 100 (3), 392–404.
BJS, 2009. The U.S. Department of Justice. Bureau Justice Stat.. http://www.ojp.usdoj.gov/bjs/pub/pdf/pim08st.pdf.
Black, S., Devereux, P., Salvanes, K., 2015. Losing heart? The effect of job displacement on health. Ind. Labor Relat. Rev. 68 (4), 833–861.
Card, D., Dahl, G., 2011. Family violence and football: the effect of unexpected emotional cues on violent behavior. Quart. J. Econ. 126 (1), 103–143.
Christie, N., 2000. Crime Control as Industry: Towards Gulags. Routledge, London.
Couch, K., Placzek, D., 2010. Earnings losses of displaced workers revisited. Am. Econ. Rev. 100 (1), 572–589.
Crawford, Alex, Plant, Martin A., Kreitman, Norman, Latcham, Richard W., 2006. Unemployment and drinking behaviour: some data from a general population survey of alcohol use. Addiction 82 (9), 1007–1016.
Davis Steve and Till von Wachter, Recessions and the costs of job loss, Brookings Papers on Economic Activity, 2011, The Brookings Institution; Washington DC.
Dawkins, Marvin, 1997. Drug use and violent crime among adolescents. Adolescence 32, 395–406.
Dragano, Nico, Verde, Pablo E., Siegrist, Johannes, 2005. Organizational downsizing and work stress: testing synergistic health effects in employed men and women. J. Epidemiol. Commun. Health 59 (8), 694–699.
Ehrlich, Isaac, 1973. Participation in illegitimate activities: a theoretical and empirical investigation. J. Politic. Econ. 81, 531–567.
Eliason, M., Storie, D., 2009. Job loss is bad for your health - Swedish evidence on cause-specific hospitalization following involuntary job loss. Soc. Sci. Med. 68 (8), 1396–1406.
European Sourcebook, 2006. European Sourcebook of Crime and Criminal Justice. Statistics – 2006. The Council of Europe http://www.europeansourcebook.org/.
Evans, W., Graham, J., 1988. Traffic safety and the business cycle. Alcoh. Drugs Driv. 4 (1), 31–38.
Farrington, David P., Jolliffe, Darrick, Hawkins, J.David, Catalano, Richard F., Hill, Karl G., Kosterman, Rick, 2003. Comparing delinquency careers in court records and self--reports. Criminology 41, 933–958.
Felson, M., 1998. Crime and Everyday Life. Pine Forge Press, Thousand Oaks.
Fishback, P., Johnson, R., Kantor, S., 2010. Striking at the roots of crime: the impact of welfare spending on crime during the great depression. J. Law Econ. 53 (4), 715–740.
Freeman, R., 1996. Why do so many young American men commit crimes and what might we do about it? J. Econ. Perspect. 10 (1), 25–42.
Garda, P., 2016. The Ins and Outs of Employment in 25 OECD Countries. OECD Publishing, Paris OECD Economics Department Working Papers, No. 1350Gottfredson, M. & T.
Hirschi, 1990. A General Theory of Crime. Stanford University Press.
Grogger, J., 1995. The effect of arrests on the employment and earnings of young men. Quart. J. Econ. 110 (1), 51–71.
Hauser, C., Baker, A., 2008. Keeping a Wary Eye on Crime as Economy Sinks. New York Times October 9, 2008.
Hinderlang, M., Hirschi, T., Weis, J., 1981. Measuring Delinquency. Sage, Beverly Hills, CA.
Hirschi, T., 1969. Causes of Delinquency. University of California Press, Berkely.
Hirschi, T., Gottfredson, M., 1983. Age and the explanation of crime. Am. J. Sociol. 89 (3), 552–584.
Hoynes, H.W., Miller, D.L., Schaller, J., 2012. Who suffers during recessions. J. Econ. Perspect. 26 (3), 27–48.
Huttunen, Kristiina, Møen, Jarle, Salvanes, Kjell G., 2011. How destructive is creative destruction? Effects of job loss on mobility, withdrawal and income. J. Eur. Econ. Assoc. 9 (5), 840–870.
Huttunen, Kristiina, Møen, Jarle, Salvanes, Kjell G., 2018. Worker displacement and regional mobility. J. Labour Econ. 36 (2), 479–509.
Inzlicht, Michael, McKay, Linda, Aronson, Joshua, 2006. Stigma as ego depletion: how being the target of prejudice affects self-control. Psychol. Sci. 17 (3), 262–269.
Inzlicht, Michael, Schmeichel, Brandon J., 2012. What is ego depletion? Toward a mechanistic revision to the resource model of self-control. Perspect. Psychol. Sci. 7 (5), 450–463.
Jacobson, Louis, Lalonde, Robert, Sullivan, Daniel, 1993. Earnings losses of displaced workers. Am. Econ. Rev. 83 (4), 685–709.
Kirk, D., 2006. Examining the divergence across self-report and official data sources on inferences about the adolescent life-course on crime. J. Quant. Criminol. 22, 107–129.
Levitt, Steven, Miles, Thomas, 2007. Empirical study of criminal punishment. In: Polinsky, Shavell (Eds.). *Handbook of Law and Economics*,, 1. North-Holland.
Lin, M.-J., 2008. Does unemployment increase crime? Evidence from U.S. data 1974-2000. J. Hum. Resour. 43 (2), 413–436.
MacDonald, Z., 2002. Official crime statistics: their use and interpretation. Econ. J. 112 (477), F85–F106.
Mani, A., Mullainathan, S., Shafir, E., Zhao, J., 2013. Poverty impedes cognitive function. Science 341 (6149), 976–980.
Marcus, J., 2013. The effect of unemployment on the mental health of spouses – Evidence from plant closures in Germany. J. Health Econ. 32, 546–558.
McKee-Ryan, Frances, Song, Zhaoli, Wanberg, Connie, Kinicki, Angelo, 2005. Psychological and physical well-being during unemployment: a meta-analytic study. J. Appl. Psychol. 90 (1), 53–76.
Mocan, H.N., Rees, D.I., 2005. Economic conditions, deterrence and juvenile crime: evidence from micro data. Am. Law Econ. Rev. 7 (2), 319–349.
Mustard, D.B., 2010. How do labor markets affect crime? new evidence on an old puzzle. In: Benson, B., Zimmerman, P. (Eds.), Handbook On the Economics of Crime. Edward Elgar, pp. 342–358 Chapter 14.
OECD (2009). OECD.StatExtracts, see http://stats.oecd.org/Index.aspx
Pager, D., 2003. The mark of a criminal record. Am. J. Sociol. 108, 937–975.
Pratt, J., 2008. Scandinavian exceptionalism in an era of penal excess. Br. J. Criminol. 48, 119–137.
Pratt, T.C., Cullen, F.T., 2000. The empirical status of Gottfredson and Hirschi's general theory of crime: a meta-analysis. Criminology 38 (3), 931–964.
Raphael, S., Winter-Ebmer, R., 2001. Identifying the effect of unemployment on crime. J. Law Econ. 44 (1), 259–283.
Rege, M., Telle, K., Votruba, M., 2009. The effect of plant downsizing on disability pension utilization. J. Eur. Econ. Assoc. 7 (4), 754–785.
Rege, M., Telle, K., Votruba, M., 2011. Parental job loss and children's school performance. Rev. Econ. Stud. 78 (4), 1462–1489.
Rehm, J., Room, R., Graham, K., Monteiro, M., Gmel, G., Sempos, C.T., 2003. The relationship of average volume of alcohol consumption and patterns of drinking to burden of disease: an overview. Addiction 98, 1209–1228.
Schmieder, J., von Wachter, T., Heining, J., 2018. The Costs of Job Displacement over the Business Cycle and Its Sources: Evidence from Germany. UCLA, Mimeo.

[30] A large literature in criminology links violence to alcohol and drug use (see e.g. Rehm et al., 2003, Boles and Miotto, 2003).

Schroeder, R., Giordano, P., Cernkovich, S., 2007. Drug use and desistance processes. Criminology 45 (1), 191–222.
Boles, Sharon M., Miotto, K., 2003. Substance abuse and violence: a review of the literature. Aggress. Violent Behav. 8 (2), 155–174.
Statistics Norway, 2008. Crime Statistics 2004. Statistics Norway, Oslo.
Statistics Norway (2019). Charged persons, https://www.ssb.no/statbank/table/09413/
Stevens, Ann H., 1997. Persistent effects of job displacement: the importance of multiple job losses. J. Labor Econ. 15 (1), 165–188.
UN, 2008. Tenth United Nations survey of crime trends and operations of criminal justice systems, covering the period 2005 – 2006. United Nations Office on Drugs and Crime..
Vahtera, J., Kivimaki, M., 1997. Effect of organizational downsizing on health of employees. Lancet 350 (9085), 1124–1128.
van Dijk, J.J.M., van Kesteren, J.N., Smit, P., 2008. Criminal Victimisation in International Perspective, Key Findings from the 2004-2005 ICVS and EU ICS. The Hague, Boom Legal Publishers.