Job Displacement and Crime[*]

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Abstract

We use a detailed employer-employee data set matched with detailed crime information (timing of crime, fines, convictions, crime type) to estimate the impact of job loss on an individual's probability to commit crime. We focus on job losses due to displacement, i.e. job losses in firms losing a substantial share of their workers, for workers with at least three years of tenure. Displaced workers are more likely to commit offenses leading to conviction (probation, prison terms) for property crimes and for alcohol-related traffic violations in the two years following displacement. We find no evidence that displaced workers' propensity to commit crime is higher than non-displaced workers before the displacement event; but it is significantly higher afterwards. Displacement impacts crime over and above what is explained by earnings losses and weeks of unemployment following displacement.

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1. Introduction

In the last decade, Europe has experienced a "*reversal of misfortunes*": as crime rates have reached historical lows in the U.S., Europe on the contrary currently experiences historically high crime rates (Buonanno et al. 2011). Crime, arrests, and convictions generates large social costs, and what causes crime has been the focus of an extant literature (Benson & Zimmerman 2010; Freeman 1999).

Descriptive statistics suggest that in the United States, the peak of crime of the early 1990s approximately matches to the peak of the U.S. unemployment rate in 1994. Gould, Weinberg, and Mustard (2002) uses trade instruments to estimate that wage trends explain more than 50% of the variation in property crime over their sample period (1979–1997), and that the unemployment rate of non-college-educated men after 1993 lowered crime rates. Lin (2008) uses union membership rates and a state's industrial structure to estimate that a one percentage point increase in the unemployment rate leads to a 4 percent increase in property crime.

Prior literature has indeed uncovered convincing causal estimates of the impact of unemployment on crime in a number of countries including Sweden (Oster & Agell 2007), and France (Fougère et al. 2009). However, prior literature relies on state or municipality level data. In particular it is hard to pinpoint exactly what mechanism, at the individual level, generates the relationship between the regional-level unemployment rate and the regional-level crime rate.

This paper estimates the impact of mass layoffs on the individual probability of committing a criminal offense in Denmark. Focusing on Denmark allows us to use a detailed employer-employee-unemployment matched individual-level data set with crime information. The data set includes information on convictions (prison terms and probations), broken down by crime type – property crime, violent crime, narcotics crime, alcohol-related traffic offenses, and sexual crime – as well as individual earnings, weeks of unemployment, age, marital status, and the municipality of the individual. We focus on displaced workers, i.e. male individuals aged 18-45 that have been in employment for at least 3 years in the same firm and move into unemployment when the firm experiences a mass-layoff event, i.e. loses a substantial fraction of its employees compared to peak employment in a five year window prior to the time period of analysis. Focusing on such displaced workers is likely leading to, arguably, more causal estimates of the impact of job loss on the probability to commit crime than a simple correlation between employment and crime.

We find statistically and economically significant impacts of displacement on the probability of an offense leading to a prison term for property crime and alcohol-related traffic crime; as well as on the probability of an offense leading to probation for property and violent crime. We find that although displaced workers are no more likely to commit crime at any point prior to displacement, displaced are substantially more likely to commit crime *after displacement*. Results are robust to the inclusion of individual fixed effects, controls for changes in marital status, the number of children. Displaced workers experience substantial earnings losses and a higher probability of unemployment after the displacement event. We find that there is an effect of displacement on crime over and above what is predicted by its effect on earnings and unemployment. Results are robust to alternative definitions for mass-layoffs, using mean firm employment as the reference point, or identifying mass layoffs as large deviations from a firm-specific trend in employment. Results are also robust to focusing on larger-sized firms, for which large proportional changes in employee numbers are less likely.

Results by education level suggest that individuals with higher education are more likely to be affected by the displacement event. As individuals with higher education levels display very low levels of criminal offenses prior to displacement, they experience proportionately larger increases in the probability to commit an offense post-displacement.

This paper makes contributions to two different literatures. First, the paper provides individual-level estimates of the impact of job losses on crime using detailed employer-employee data. Previous literature (Gould et al. 2002; Oster & Agell 2007; Fougère et al. 2009) used regional-level data (such as county-level or state-level data) to estimate such impacts. Although the literature uses credible instrumental variable estimates, individual level evidence of a mechanism relating unemployment and crime remains to be established. In particular, no U.S. data set matches individuals with their employers *and* includes crime data. Focusing on Denmark allows such analysis.[1]

Of course, focusing on individual level data give estimates that capture a different effect than regional level estimates. Individual-level estimates helps document the mechanisms: in particular a discrepancy between individual level estimates and area-level estimates suggests either that regional level estimates are confounded or that there are social interactions in crime within states or municipalities: as unemployment rises, both individual incentives and social incentives to commit crime increase (Glaeser et al. 1996), and area-level estimates may be larger than individual level estimates.

The paper also makes a contribution to the literature on the wider impacts of job displacement. Jacobson, Lalonde, and Sullivan (1993) has documented the short-run and long-run earnings losses of displaced workers using U.S. Social Security data. Sullivan and von Wachter (2009) has shown arguably causal evidence that job displacement leads to

[1] Although this is only anecdotal evidence, the peak of property and violent crime rates in Denmark matches the peak of unemployment in 1994.

higher mortality rates. In this paper, we define displacement in a similar way as in Jacobson et al. (1993). We also use three alternative definitions leading to similar results.

Results should be useful to policymakers: by establishing a link between individual-level displacement and crime, the paper thus finds that a job separation is likely to have impacts on other parties than the firm and the employee. Job displacement may thus lead to increased policing costs, and overall negative welfare externalities for the municipality. Following theoretical framework, neither employers nor workers may fully internalize the social cost of the job separations, which may justify either additional taxation of employers and/or active labor market policies that incentivize or help unemployed individuals to go back to formal employment.

The paper proceeds as follows. Section 2 presents the rich Danish employer-employee data set. Section 3 presents the identification challenges and the paper's identification strategy. Section 4 describes our empirical findings, for overall convictions, by crime types. Section 5 concludes.

2. The Employer-Employee-Crime Data

To analyze the impact of job displacement on crime, we utilize detailed employer and employee data contained in Danish Register Data made available by Statistics Denmark. Danish Register Data is a database of every individual residing in Denmark from 1980-present which is collected from various governmental and administrative sources. We follow individuals over time and across different data sources via an anonymous personal identification number derived from the central personal register (CPR, Det Centrale Personregister), and are able to match individuals to their employer using a unique firm identification number. We focus solely on males as males are overwhelmingly those who commit crime. We construct an individual level panel of every male residing in Denmark from 1985-2000 by combining registers on employment, employer, crime, and education into the population register which includes demographic factors such as age, gender, municipality of residence, and immigrant and marital status.

Employee and employer information is taken from the Integrated Database for Labor Market Research (IDA, Integrerede Database for Arbejdsmarkedsforskning), a combination of many employment registers, which contains information on an individual's employment as well as any firm that exists in Denmark in a given year. The employee data set provides information such as an individual's employment status (recorded at the end of November), the number of weeks in the year an individual was unemployed, information on part time or full time status, salary earned in the job, and a workplace (plant) identification number as well as a firm identification number. The employer data contains variables such as the number of employees in a workplace and the number of workplaces in a firm. We use this information to construct a firm level dataset, in order to exclude the possibility of intra-firm mobility. We consider only an individual's primary job, according to a criteria set by Statistics Denmark which follows the definitions of the International Labour Organization (ILO).[2]

All of the employee and employer data contained in IDA is observed annually. However, making use of weekly data on the level of welfare and unemployment insurance payments received contained in the Central Register of Labour Market Statistics (CRAM, Det Centrale Register for Arbejdsmarkedsstatistik), we are able to construct semi-annual measures of an individual's unemployment status by measuring the exact week in a given year an individual goes from receiving no benefits to receiving some form of benefits.[3] The higher frequency of semi-annual data allows us to explore the precise timing behind an individual's displacement and criminality more so than annual data, and as we match individuals to their firm, we know exactly when employees transition in and out of employment with that specific firm.

Crime data comes directly from police records, and is available for charges (individuals charged by the police with a crime), convictions, as well as incarcerations. After a crime is reported, if the police suspect someone of committing this crime, this individual is formally charged with the crime. After this, we observe whether the individual was tried for the crime, and the ultimate conviction outcome of the trial, which is categorized into incarceration, suspended sentences, fines, settlements, no charges/warnings, and other decisions (for example youth programs and military punishment). While all of these are possible conviction outcomes, the overwhelming majority of convictions in Denmark result in either suspended sentences or fines followed by imprisonments. In the estimation that follows we focus solely on convictions, specifically whether an individual was convicted of a suspended sentence and whether an individual was convicted to a prison sentence. These criminal proceedings are linked not only to the individual charged or convicted of the crime, but also across databases through a unique police case number, so we can track a specific crime an individual was charged with throughout the judicial process. We can also track the timing of events across databases, making use of charge, conviction, and incarceration dates.

As with any administrative crime data, a limitation of this type of crime data is that we only observe individuals as criminals if they are apprehended for the crime committed. This could be problematic if, for example, displaced individuals are more likely to be apprehended and convicted than non-displaced individuals, as we would attribute differences in

[2] See www.dst.dk/kvalitetsdeklaration/848 for an explanation.

[3] In what follows, we use the terms receiving unemployment benefits and weeks of unemployment interchangeably. This could be problematic in that we would misclassify someone as non-displaced if they were displaced, but did not claim any benefits. Past studies have found this to be an unimportant factor, and this is particularly the case for us, as the high tenure individuals we identify are all eligible to receive social assistance or unemployment insurance (if they are a member) following job loss.

apprehension probabilities to effects of displacement on crime. There is little that we can do to explore whether this is a problem in our data, as alternative measures of criminal behavior such as self-reported crime data are unavailable. In what follows, we focus on whether an individual was convicted of a crime or not, as we wish to identify the effects of displacement on criminal activity, and an individual charged with a crime may not ultimately be found guilty of the crime.

Across all three crime databases, we also observe a detailed crime code, corresponding to the Danish classification system of offenses. While in principle we could focus on specific offenses (burglary, assault, etc), in what follows we focus on types of offenses (total, property, violent, etc). Total crimes are comprised of: sexual, violent, property, alcohol related traffic, narcotics, firearms, tax, unknown, and other crimes, as well as crimes against special legislation.[4] In Denmark, sexual, property, and violent crimes are against the penal code, while there are special laws against offenses such as narcotics and firearms crimes.

In order to ensure the unexpected nature of job displacement and following Jacobson, Lalonde, and Sullivan (1993), we implement numerous sample restrictions: that an individual has more than 3 years of tenure,[5] is employed in a firm that has at least 7 employees in 1989, is employed in full time work (30 hours a week or more), and experiences no weeks unemployed.[6] As such, our non-displaced (control) sample is composed of, at any point in time, males who have been employed for more than 3 years in a firm of at least 7 employees calculated at the end of 1989 who are also working full time. For the displacement sample, we impose similar restrictions, which are discussed in the following section. In the results that follow, we focus on workers who are members of an unemployment insurance fund prior to displacement. For a detailed discussion of the unemployment system in Denmark as well as the level and type of benefits available during unemployment, see Appendix A.

3. Econometric Model of Job Displacement and Crime

A number of identification challenges make the identification of the effect of the loss of employment on crime difficult. In particular, individuals typically do not leave their firm for exogenous reasons. Individuals may choose to leave the labor force altogether, or choose

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[4] Excluding offenses such as traffic fines and accidents, which are also reported in the police data.

[5] Because our tenure measure is annual, this restriction is an individual has 3+m years of tenure, where the individual is employed for an unobservable m months between two years.

[6] While 7 employees may seem a very small number, the Danish economy is composed of many small firms. 5 employees is a number which has been used in past studies using Danish data and job displacement. We use 7 employees in order to ensure that a reduction of 2 employees does not cause a firm to suer employment losses of greater than 30%, which would be the case with firms with either 5 or 6 employees in 1989.

to leave their current job and start an unemployment spell to look for a better match. If individuals leave their firm because of opportunities in the informal sector, we may observe a positive correlation between changes in employment status and the probability of crime that do not reflect a causal relationship between employment loss and crime; in such a case, current unobservables would drive both the probability of job loss and the probability of committing crime.

The literature has identified a firm-level cause of job losses that should be arguably independent of the worker's individual dynamics. In this paper, we focus on high-tenure workers whose firm experiences a mass layoff event, as in Jacobson, Lalonde, and Sullivan (1993).

We focus on individuals that are employed full-time, with high-tenure, i.e. more than 3 years in the firm, i.e. experiences no weeks of unemployment from year y-1 to y-3 and stays in the same firm. Individuals with high-tenure are more likely to have accumulated firm-specific human capital, more likely to be enjoying a more favorable employer-employee specific match, and are thus more likely to face relatively worse outside options as compared to low-tenure workers. Such workers are also less likely to leave a firm during a mass layoff event.[7] In our detailed employer-employee dataset, an individual loses employment between semester t and semester t-1 if the individual was in employment in semester *t-1* and has more than one week of unemployment in semester *t*. We ensured that weeks of unemployment are not furloughs, and thus a positive number of weeks of unemployment indicates that the worker either lost his employment or switched to another employer. The data set does not include individuals that leave the workforce as (i) a majority of employees receive either unemployment benefits or social assistance (counted as unemployment) and thus (ii) individuals not in employment in semester *t* are individuals that separated from their firm voluntarily.

An individual is displaced if he loses employment (following the above definition) during a firm-level mass-layoff event. The restrictions we put on the sample of workers ensure that those individuals we classify as displaced are high tenured workers with strong ties to their firms, for whom displacement is likely to be sudden and unexpected. The sample focuses on mass layoffs which occur from 1990-2000, such that an individual can be first displaced in 1990 and last displaced in 2000. The focus on the 1990-2000 period allows us, later in the paper, to compare the magnitude of our results to the results presented for the U.S. (Raphael & Winter Ebmer 2001; Gould et al. 2002).

[7] By focusing on high-tenure individuals, we are likely to get an underestimate of the impact of job losses on crime given the negative correlation between the probability of committing crime and job tenure.

We start by considering the set of firms with more than 7 employees in 1989 (later we will focus on firms more either more than 10 or more than 15 employees, to check the results' robustness). A firm experiences a mass-layoff event in semester *t* if the firm experiences a decline in employment higher than 30% from that firm's peak of employment from 1985-1989 (before the displacement period).[8] According to this definition, displacement occurs somewhere between semester t-1 and t for the individual level, while the firm level data is only observed annually. This implies that the firm level decline in employment for an individual displaced in either semester 1 or semester 2 will be calculated as the percentage decline from 1989 to the end of year y.[9]

Within a firm that experiences the 30% or greater reduction in employment, individuals who lose employment may be specific individuals in observable and unobservable dimensions. Specifically, a firm and a set of employees may agree on voluntary layoffs. If such voluntary or selective layoffs affect workers that are more likely to commit crime, results correlating displacement events with criminal outcomes will be upward biased. Workers who are less productive, or whose nominal wage is high compared to the firm's outside options, may be more likely to experience job separations. This is where the availability of a longitudinal dataset of individuals with wage and crime in every time period, with individual identifiers, allows us to control for an individual-specific fixed effect. For instance, childhood experiences, dimensions of educational achievement that are not controlled for, will be absorbed by the worker fixed effect.

Individuals may also experience negative productivity shocks right before the firm's layoff. For instance, the loss of a relative (Bennedsen et al. 2006), changes in marital status (Korenman & Neumark 1991), and other time-varying shocks have been shown to affect either worker pay or worker productivity. Such unobservable time-varying life events in the 1990-2000 period, that are correlated with worker productivity or pay and also with the propensity to commit crime may confound the estimates of the impact of displacement on crime. To test for such possibility, we observe the criminal outcomes of individuals in all semesters prior to displacement and all semesters post-displacement. If displacement is truly exogenous to the individual's prior unobservables – including propensity to commit crime – we should not observe that displaced individuals display a more crime-prone history before displacement than other workers.

Another identification issue is that causality could flow from crime to job displacement if, for example, an individual is convicted of or incarcerated for a crime, an employer may

[8] We also consider two alternative definitions of mass-layoff events in Section.

[9] The percentage decline from 1989 is the same for individuals in semester 1 and semester 2.

fire the worker as a result. Importantly, such individuals would not be displaced according to our definition as they would be ineligible for benefits if they were fired with cause.

Further, the timing of both the displacement event and criminal activity are crucial to our results to be interpreted as the effects of job displacement on crime. A criminal event – left-hand side of the regressions – is either an offense that will lead to a conviction to a prison term, or an offense that will lead to a conviction to probation. Because the dataset includes the time of the offense, and a unique identifier for the charges that is linked to the judicial outcome and the sentencing, for each prison term and probation we trace that outcome back to the date of the corresponding offense. In that way the data focuses only on severe offenses, i.e. with convictions, but avoids the problem of reverse causality that would occur if we defined a criminal event as the start of a prison term or a conviction to probation. For instance, an individual could commit a crime prior to displacement, but due to lags between when the offense is committed and being charged and convicted, may not ultimately be convicted until after displacement. Table 3 presents the typical time between when an offense is committed, is charged, is convicted, and is incarcerated for the overall population as well as our displaced sample. The lag between when the crime is committed and when an individual is finally convicted can be quite substantial. Crime is thus an offense which results in a conviction. Doing so ensures we focus on the offense date, corresponding to exactly when the crime was committed, rather than the date of conviction.

Noting *Crime*$_{i,t}$ such a criminal event (offense leading to a conviction to prison or probation), we estimate the full dynamics of criminal events pre- and post-displacement event. As such the main specification of this paper considers the regression of criminal events *Crime*$_{i,t}$ on the full set of dummy variables. As both criminal events and displacement events are relatively rare events, the propensity to commit crime is modeled in a logit latent variable framework:

$$Crime^*_{i,t} = F\left(\alpha_i + \underbrace{\textstyle\sum_{k=0}^{k=\overline{K}} \delta_k \, \mathbf{1}(Disp.\, in\ t-k)}_{Post-displacement} + \underbrace{\textstyle\sum_{k=\underline{K}}^{k=2} \delta_{-k} \, \mathbf{1}(Disp.\, in\ t+k)}_{Pre-displacement} + Year_t + X_{i,t}\beta\right) + \varepsilon_{i,t} \quad (1)$$

where $P\big(Crime_{i,t} = 1\big) = P(Crime^*_{i,t} > 0)$ is the probability of individual *i* committing an offense in semester *t*. Specification (1) uses the timing of an individual's criminal events *relative* to the individual's displacement event to identify the impact of job displacement on crime. $\mathbf{1}(Disp.\, in\ t-k)$ is a set of post-displacement dummy variables, with k ranging from 1 to $\overline{K} = 12$ semesters equal to one if the individual was displaced in semester *t-k* and 0 otherwise. Thus the coefficient δ_k measure the impact of displacement on crime for the 12 semesters prior to displacement. The variables. In particular δ_k measures both the short-run and the long-run impact of displacement on crime. δ_0 is the coefficient of the impact of displacement on the probability of crime in the semester of displacement.

The specification includes a fixed effect α_i that absorbs individual-specific non-time-varying observables and unobservables that affect an individual's propensity to commit crime. As such the coefficient δ_{-1} is normalized by convention to 0 so that all effects are measured relative to the semester prior to displacement.

For values $k<0$, the coefficients δ_k measure pre-displacement propensity to commit crime. If the event of displacement is truly exogenous with respect to individual time-varying unobservables, the timing of criminal offenses leading to conviction will always be such that, for each individual, the odds of committing an offense are significantly higher after displacement than before displacement. As such the propensity to commit crime k semesters prior to displacement should not be different from the propensity to commit crime in the semester right before displacement. Formally, their estimates $\widehat{\delta_k}$ of pre-displacement or so-called placebo dummies δ_k should not be statistically significantly different from 0 if displacement is truly independent of the individual's unobservables prior to displacement. In particular if individuals who experience negative family events are more likely to commit crime in the semesters prior to displacement and are also more likely to be laid off during a mass-layoff event, specification (1)'s placebo coefficients δ_k will be significantly above zero.

$Year_t$ is a year dummy that captures national trends in the evolution of crime. Such control is key as the fraction of individuals committing offenses declines over the time period of analysis. Unemployment declines too, but the country-level correlation between the decline in crime in Denmark and the unemployment rate may be spurious. Hence the dummy-variables controls. $Year_t$ which capture country-level changes in the propensity to commit crime.

$X_{i,t}$ it is a vector of individual characteristics that capture the change in the individual's marital status, the change in the individual's number of children. As such, as individuals that transition from unmarried to married are less likely to commit crime (in correlation) and are less likely to be displaced, not including the time-varying marital status Xi,t in the vector of covariates may lead to an overestimation of the impact of displacement on crime. Similarly if an increase in the number of children reveals an individual's unobservable propensity to be displaced in future semesters, including the number of children in the regression should dampen the estimate δ_k of the impact of displacement on crime.

In specification (1), residual unobservables $\varepsilon_{I,t}$ are assumed robust and clustered by individual, so that the unobservables in the propensity to commit crime can experience some degree of autocorrelation $Cov(\varepsilon_{it}, \varepsilon_{it'}) \neq 0$ for any two periods t, t'. For examining the impact of a shock on an individual's outcomes over time, controlling for autocorrelation typically leads to substantially wider standard errors.[10]

Specification (1) faces a number of estimation challenges. With upwards of 289,000 displaced individuals and more than 4.5 million non-displaced individuals, and with a crime rate of 0.4% (Table 1), the inclusion of a full set of individual fixed effects, as well as longitudinal pre- and post-displacement dummies renders specification (1) not estimable using a traditional logit approach. Chamberlain (1980) introduced the conditional logit, which provides an estimator that controls for individual fixed effects α_i and is a consistent estimator of the displacement effects δ_k. The conditional logit conditions on the individual's number of offenses to eliminate the individual's fixed effect from the regression. As such, the conditional logit compares, for individuals with at least one offense leading to a conviction, whether the timing of offenses "corresponds" to the timing of displacement. If, on average, the probability of committing crime is higher after displacement than before displacement, the coefficients δ_k will be strictly positive for k greater or equal than 0 and not statistically different from 0 for $k<0$. Such an estimation technique is feasible even for large registry databases. The coefficients δ_k are not directly interpretable but the impact of displacement on the *odds* of committing an offense leading to crime are.

The paper's main results presents the impact of displacement on the odds ratio log(P($Crime_{i,t}$ =1) /(1-P($Crime_{i,t}$ =1))). The estimated impact of displacement on the odds of committing crime is, for instance, equal to 1.5 if the odds of committing an offense are multiplied by 1.5 *relative* to the pre-displacement semester.

Further, specification (1) is estimated over the period 1985 to 2000. The first displacement events occur in 1990, but the inclusion of data from 1985 ensures the identification of the pre-displacement dummies δ_k for $k=-5$ to $k=-1$ starting with the 1990 displacement cohort. As the sample ends in 2000, individuals who are displaced in 2000 are not followed in years 2001—2006 and thus estimates $\widehat{\delta_k}$ of δ_k for years $k \geq 1$ may be biased. This is also true to some extent for individuals displaced in 1999 (for $\widehat{\delta_{-2}}$), individuals displaced in 1998 up to individuals displaced in 1993. To alleviate such concern, the regression also controls for dummies indicating that observations are available for a specific time window. Finally, we focus in the estimation on individuals that are present throughout the sample, i.e. that do not leave and reenter the sample at a later date. Leavers that reenter the dataset represent less than 1% of the sample. As the sample is comprehensive, this is likely individuals leaving Denmark *and* leaving the social security and unemployment benefit system.

[10] On the importance of controlling for autocorrelation or clustering of residuals in the estimation of treatment effects, see Bertrand et al. (2002).

4. Empirical Findings

Table 2 presents the results of estimating the odds ratio impact of displacement on offenses leading to a conviction to either prison or probation. Columns 1—4 are for impacts of displacement on offenses leading to prison, either for all types of offenses (column 1), for offenses leading to prison terms for property crimes (column 2), for offenses leading to prison for alcohol-related traffic offenses (column 3), and similarly for violent crimes (column 4). Authors have suggested, using state- or county-level U.S. data, that unemployment may have a causal impact on *property* crime as individuals' opportunities in the formal labor market are affected by their job separation (Becker 1974; Raphael & Winter Ebmer 2001). This paper's identification strategy focuses on job displacement as individuals who otherwise leave their jobs endogenously may in fact have higher opportunities in the formal sector as they look for a better match (Rogerson et al. 2005). Job displacement may also cause other types of crime such as violent crime that have been explained either by economic mechanisms (Fajnzylber et al. 2002) or social and psychological mechanisms (Fergusson et al. 2001).

Columns 5—7 are for offenses leading to probations for total crime, property crime, and violent crime.[11] Effects displayed here are from 5 semesters before displacement up to 10 semesters after displacement.

The coefficients are multiplicative impacts on odds ratio: the coefficient of 1.773 on the impact of displacement on crime in the semester of displacement indicates the odds of committing crime are 77.3% higher in the semester of displacement than in the semester prior to displacement. The impact of displacement on the odds ratio of offenses leading to prison are driven mostly by property crimes and by alcohol-related traffic offenses. In the semester of displacement, the odds of committing a property crime are 2.74 times the odds in the semester prior to displacement. And for alcohol-related traffic offenses the odds are multiplied by 1.68.

Pre-displacement coefficients are an important indicator of the magnitude of potential dynamic selection effects: if individuals that are more likely to be convicted are also more likely to be displaced, we should observe significant effects for the δ_k with $k<0$. The coefficients are presented in the "Year –2" to "Year –5" rows. In all 7 columns, none of the placebo coefficients for odds ratios are statistically different from 1, suggesting that displaced individuals do not experience a significant increase in their propensity to commit crime prior to displacement, relative to non-displaced individuals. In magnitude, they represent less than half of the coefficient for the impact of displacement on crime. If anything, the coefficients $\widehat{\delta_{-5}}$ to $\widehat{\delta_{-2}}$ fluctuate without a significant discernible pattern.

[11] While there is a substantial number of offenses in all seven categories, there are too few probation sentences for traffic offenses for us to interpret the coefficients of that particular crime type.

The coefficients are graphically depicted in Figure 2 (i) for offenses leading to prison and in Figure 2 (ii) for offenses leading to probation.

Results available on demand also suggest that individual fixed effects α_i (see specification 1) are positively and significant correlated both with the probability of displacement and with the probability of a conviction, indicating that they capture a significant share of the individual unobservables that both affect the likelihood of a job separation and of crime. Year dummies capture the national-level declining trend in crime, as aggregate statistics on crime indicate that all types of crime peak in 1994 before declining all throughout the 1990s.

5. Conclusion

Using a data set of high-tenure male workers from 1985-2000 in Denmark, we explore the relationship between unemployment and crime at the individual level by estimating the impact of job displacement on an individual's probability to commit a criminal offense leading to a prison term or probation. Following Jacobson's (1993) definition of displacement, we find that low educated displaced workers are significantly more likely to engage in property and alcohol related traffic crimes than their non-displaced counterparts following displacement, but importantly, are not significantly different prior to displacement. We find that individuals with a higher education degree are proportionately more affected by displacement than individuals with high school or less. The effects are both long-lived and economically significant, lasting up to four semesters.

Estimating the effects of displacement on log annual earnings and weeks of unemployment, we conclude that these effects of displacement on crime are only partly driven by the impact that displacement has on unemployment and earnings. We explore the possibility that a relationship between marital status, number of children is driving our results, and find that results are robust to the inclusion of such controls – even though the probability of being married is lower after displacement as compared to after displacement. Our results suggest that job separations are likely to have impacts on society which extend beyond employers and employees, which may provide scope for welfare increasing labor market policies.

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Appendix A – Unemployment in Denmark

When an individual becomes unemployed in Denmark, they can receive either unemployment insurance benefits or social assistance. In order to be eligible for unemployment insurance, an individual must, voluntarily, pay monthly membership into a unemployment insurance fund. As a member of an unemployment insurance fund, an individual receives 90% of their original salary, subject to a maximum and eligibility rules. During the time period in this paper, these eligibility criteria were membership into an unemployment insurance fund for a minimum of one year, and employment for at least half a year out of the previous 3 years before unemployment.

If an individual was not a member of an unemployment insurance fund, they still may be eligible for social assistance. Social assistance in Denmark is means-tested, and less clearly defined compared to unemployment benefits. The amount an individual would receive depends on a variety of factors such as household size, marital status, spousal earnings, and household wealth. For example, a single father with kids would receive fairly large welfare payments, while a married father whose spouse earns a high income would receive very little welfare payments. While all these factors go into the level of benefits one would receive, the precise amount received ultimately depends on an assessment determined by a welfare counselor.

Membership in an unemployment insurance fund is relatively common in Denmark, where during the 1990s, membership among employed workers was around 85%. Unemployment insurance membership does vary across socio-economic factors and earnings, due to the fact that some individuals would receive similar payments whether they joined a fund or not, while some individuals would receive nearly no social assistance payments but high unemployment insurance payments.

Table 1 – Displaced Workers and Crime Rates

This table presents the total sample size, the number and fraction of displaced workers in the sample. The male unemployment rate of column (4) is from the net unemployed as a percent of the labor force series of Danmarks Statistik.

	Not Displaced	Displaced	Difference Displaced – Non-Displaced	t stat
Total Crime - Convicted to Prison	0.0007	0.0023	0.0016	29.53***
Property Crime - Convicted to Prison	0.0001	0.0004	0.0004	21.82***
Violent Crime - Convicted to Prison	0.0001	0.0005	0.0003	13.03***
Traffic Alcohol Crime - Convicted to Prison	0.0005	0.0013	0.0009	19.92***
Total Crime - Convicted to Probation	0.0003	0.0011	0.0007	19.71***
Property Crime - Convicted to Probation	0.0002	0.0007	0.0005	17.40***
Violent Crime - Convicted to Probation	0.0001	0.0002	0.0001	5.19***
Age	35.41	37.64	2.22	172.85***
Weeks Unemployed	0.0074	3.399	3.391	1010.52***
Employed	0.9988	0.8452	0.1536	844.85***
Tenure at firm	7.56	2.74	4.82	689.76***
Semi Annual Salary	138419.47	96177.67	42241.8	378.32***
Less than HS	0.216	0.3055	-0.0896	-112.75***
HS or Voc. Training	0.501	0.5581	-0.0571	-59.63***
University or Greater	0.2427	0.097	0.1456	180.05***
Immigrant	0.0246	0.04	-0.0154	-51.01***
Descendant	0.0024	0.0022	0.0002	+1.94
Native	0.973	0.9578	0.0152	+48.27***
Number of Observations (one per individual and semester)	4531987	289502		

Table 2 – Job Displacement and Crime – Longitudinal Regression

This table presents the main regression of the impact of displacement on crime, with both pre-displacement dummies – for placebo analysis – and post-displacement dummies, which estimate short-, medium- and long-run effects of displacement on crime. Crime is measured either as arrests, citations, fines, or prison terms.

	Total Prison	Property Prison	Traffic Alcohol Prison	Violent Crimes Prison	Total Probation	Property Crime Probation	Violent Crime Probation
				Post-displacement			
+10 sem.	1.564*	1.765	1.474	0.301	1.200	1.778	1.027
	-0.372	-1.261	-0.582	-0.241	-0.439	-0.822	-0.927
+9 sem.	1.437*	2.141	1.234	1.912	0.828	0.815	0.46
	-0.31	-1.525	-0.391	-0.918	-0.31	-0.437	-0.395
+8 sem.	1.440*	2.701*	0.863	2.331*	0.783	1.197	0.209
	-0.31	-1.629	-0.406	-1.028	-0.29	-0.544	-0.216
+7 sem.	1.289	1.851	1.023	1.291	1.281	1.287	1.229
	-0.265	-1.191	-0.35	-0.587	-0.356	-0.519	-0.576
+6 sem.	1.510**	1.602	1.088	1.85	1.469	2.377***	1.319
	-0.288	-1.006	-0.305	-0.739	-0.394	-0.763	-0.688
+5 sem.	0.899	1.378	0.906	0.62	1.159	1.769*	0.35
	-0.187	-0.77	-0.249	-0.331	-0.323	-0.597	-0.267
+4 sem.	1.348	2.867**	1.217	0.897	0.741	1.062	0.201
	-0.249	-1.468	-0.315	-0.413	-0.228	-0.387	-0.206
+3 sem.	1.316	3.410***	1.032	1.374	1.239	1.07	0.818
	-0.232	-1.435	-0.325	-0.52	-0.317	-0.39	-0.428
+2 sem.	1.182	2.545**	1.15	1.448	1.436	1.768*	0.716
	-0.211	-1.058	-0.259	-0.518	-0.336	-0.526	-0.392
+1 sem.	1.380*	4.429***	1.042	0.863	1.426	2.010**	0.846
	-0.227	-1.739	-0.26	-0.362	-0.319	-0.55	-0.414
Displact.	1.773***	2.735**	1.684**	1.24	1.391	1.885**	0.174*
semester	-0.274	-1.14	-0.377	-0.48	-0.304	-0.499	-0.177
-1 sem.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
				Pre-displacement			
-2 sem.	1.129	1.345	0.924	1.228	1.296	1.284	1.648
	-0.187	-0.61	-0.22	-0.451	-0.281	-0.362	-0.662
-3 sem.	0.846	0.872	0.808	1.054	0.932	0.853	1.426
	-0.163	-0.463	-0.224	-0.424	-0.23	-0.279	-0.582
-4 sem.	1.026	0.735	0.935	1.181	0.750	0.878	0.921
	-0.184	-0.436	-0.214	-0.483	-0.208	-0.291	-0.483
-5 sem.	0.798	1.44	0.752	0.986	0.762	0.783	0.381
	-0.161	-0.663	-0.269	-0.458	-0.207	-0.255	-0.29
Controls	Year fixed effects, Individual fixed effects, Time Window Dummies						
Obs.	81347	7023	60921	13796	39022	24219	10239
Pseudo R^2	0.017	0.072	0.019	0.03	0.022	0.051	0.03

Robust and clustered standard errors at the individual level.
: Significant at 10%, **: Significant at 5%, *: Significant at 1%*

Table 3 – Timing of Offense and Charges, Conviction, Start of Prison Term

One charge corresponds to one offense. Multiple individuals can be matched to one offense. One individual can commit multiple offenses in a given year. For confidentiality reasons, the 25th percentile, the median, and the 75th percentile are calculated using the average observations for the 5 individuals around each statistic.

	Time from Offense to Charges (days)					Time from Charges to Conviction (days)					Time from Conviction to Prison (days)				
	Mean	Median	P25	P75	Charges	Mean	Median	P25	P75	Convictions	Mean	Median	P25	P75	Prison terms
Entire sample with at least one conviction	59.6	0.0	0.0	22.0	3,729,636	111.9	70.0	37.0	143.0	1,882,930 (50.5%)[15]	173.0	129.0	53.0	231.0	233,680 (12.4%)[16]
excluding speeding	78.1	1.0	0.0	44.0	2,759,322	136.0	94.0	43.0	180.0	1,172,128	170.6	124.0	47.0	229.0	213,246
excluding zeros	149.1	42.0	10.0	136.0	1,488,564	116.5	74.0	40.0	148.0	1,808,722	187.9	142.0	73.0	244.0	215,268
Displaced with at least one conviction	45.4	0.0	0.0	1.0	21,300	99.6	62.0	39.0	125.0	15,942 (74.8%)[15]	185.5	153.2	92.8	235.8	1,831 (11.5%)[16]
excluding speeding	72.2	0.0	0.0	24.0	12,873	126.6	93.0	49.0	164.0	8,375	187.2	152.0	89.0	239.0	1,551
excluding zeros	171.4	44.0	13.0	162.6	5,642	102.1	68.0	40.0	127.0	15,551	189.8	157.2	96.2	238.2	1,789

[15] Percentage of charges leading to convictions.

[16] Percentage of convictions leading to a prison term.

Table 4 – Job Displacement and Crime – Relaxing Sample Restrictions (Probation)

This table relaxes our sample restrictions and tests the robustness of our displacement definitions to alternative specifications. Including/Excluding individuals on social assistance, changing the minimum number of employees, altering the threshold for mass layoffs (20%, 30%, 40%), taking the average 1985-1989 number of employees instead of the peak 1985-1989 as reference point for mass layoffs. While results are only reported for a dummy combining the initial semester and the +1 semester after displacement, estimation includes the full set of pre and post displacement dummies reported in Table 2.

		Probation (total):			Probation (total):			Probation (total):
Baseline		odds ratio / SE	**varying:**		odds ratio / SE	**varying:**		odds ratio / SE
		1.408**	from 30%		1.458***	from 30%		1.396
		(0.245)	to 20%		(0.213)	to 40%		(0.285)
	Obs	39022	firm	Obs	45,082	firm	Obs	35,790
	R^2	0.022	layoffs	R^2	0.022	layoffs	R^2	0.020
from only unemp insured		1.346*	From peak emp.		1.542**	from >7		1.624***
to both insured and		(0.222)	To mean		(0.310)	to >15		(0.301)
uninsured	Obs	43,141		Obs	35,968	employees	Obs	34,412
	R^2	0.021		R^2	0.021	minimum	R^2	0.024
					Probation (property)			
With firm-specific		0.976	With firm-spec.		1.285	from >7		1.541**
trend		(0.231)	trend		(0.392)	to >10		(0.271)
	Obs	33,910		Obs	20,449	employees	Obs	37,037
	R^2	0.021		R^2	0.049	minimum	R^2	0.024

Robust and clustered standard errors at the individual level.
: Significant at 10%, **: Significant at 5%, *: Significant at 1%*

Table 5 – Job Displacement and Crime – Relaxing Sample Restrictions (Prison)

This table relaxes our sample restrictions and tests the robustness of our displacement definitions to alternative specifications. Including/Excluding individuals on social assistance, changing the minimum number of employees, altering the threshold for mass layoffs (20%, 30%, 40%), taking the average 1985-1989 number of employees instead of the peak 1985-1989 as reference point for mass layoffs. While results are only reported for a dummy combining the initial semester and the +1 semester after displacement, estimation includes the full set of pre and post displacement dummies reported in Table 2.

		Prison (total):			Prison (total)			Prison (total)
Baseline		odds ratio / SE	**varying:**		odds ratio / SE	**varying:**		odds ratio / SE
		1.582***	from 30%		1.396***	from 30%		1.540***
		(0.206)	to 20%		(0.150)	to 40%		(0.237)
	Obs	81,347	firm	Obs	91,067	firm	Obs	75,670
	R^2	0.017	layoffs	R^2	0.016	layoffs	R^2	0.016
from only unemp insured		1.570***	From peak emp.		1.572***	from >7		1.657***
to both insured and		(0.198)	To mean		(0.238)	to >10		(0.221)
uninsured	Obs	89,947		Obs	75,388	employees	Obs	77,272
	R^2	0.016		R^2	0.016	minimum	R^2	0.017
					Prison (property)			
With firm-specific		1.554**	With firm-spec.		3.147***	From >7		1.777***
trend		(0.248)	Trend.		(1.208)	To >15		(0.249)
	Obs	73,242		Obs	5,948	employees	Obs	4,629
	R^2	0.017		R^2	0.072	minimum	R^2	0.017

Robust and clustered standard errors at the individual level.
: Significant at 10%, **: Significant at 5%, *: Significant at 1%*

Table 6 – Job Displacement and Crime – Robustness to Confounder Controls – Probation

This table presents results of the impact of displacement controlling for income, marital status, family status, municipality effects, and interactions between wage and displacement dummies as well as weeks of unemployment. As such results tests for the robustness to confounders and explore the channels through which displacement affects crime. While results are only reported for a dummy combining the initial semester and the +1 semester after displacement, estimation includes the full set of pre and post displacement dummies reported in Table 2.

	Dependent variable: Probation					
	Controlling for					
	Marital Status	# of Children	Municipality Effects	Log avg. earnings past 3 years	Weeks Unemployed	Wage
Displacement	1.403*	1.410**	1.483**	1.569**	1.030	1.058
	(0.244)	(0.245)	(0.265)	(0.278)	(0.194)	(0.191)
Control	0.802**	0.940		0.806***	1.035***	0.928***
	(0.077)	(0.057)		(0.042)	(0.006)	(0.009)
Displacement x wage						
Observations	39022	39022	39022	38452	39022	39022
Pseudo R^2	0.023	0.022	0.042	0.023	0.024	0.027

Robust and clustered standard errors at the individual level.
: Significant at 10%, **: Significant at 5%, *: Significant at 1%*

Table 7 – Job Displacement and Crime – Robustness to Confounder Controls – Prison

This table presents results of the impact of displacement controlling for income, marital status, family status, municipality effects, and interactions between wage and displacement dummies as well as weeks of unemployment. As such results tests for the robustness to confounders and explore the channels through which displacement affects crime. While results are only reported for a dummy combining the initial semester and the +1 semester after displacement, estimation includes the full set of pre and post displacement dummies reported in Table 2.

	Dependent variable: Prison					
	Controlling for					
	Marital Status	# of Children	Municipality Effects	Log avg. earnings past 3 years	Weeks Unemployed	Wage
Displacement	1.582****	1.582***	1.585***	1.615***	1.441***	1.362**
	(0.206)	(0.206)	(0.210)	(0.214)	(0.200)	(0.183)
Control	0.822**	1.017		0.952	1.010**	0.969***
	(0.052)	(0.046)		(0.047)	(0.005)	(0.007)
Displacement x wage						
Observations	81347	81347	81347	80227	81347	81347
Pseudo R^2	0.017	0.017	-0.266	0.017	0.017	0.018

Robust and clustered standard errors at the individual level.
: Significant at 10%, **: Significant at 5%, *: Significant at 1%*

Table 8 – Heterogeneity of the Impact of Displacement on Crime
Cohort, Education, Gender, Immigrant Status

This table estimates the impact of displacement on crime by cohort (1939-1959, 1960-1981), by education (less than high school, high school, and university), by gender, and by immigrant/native status. While results are only reported for a dummy combining the initial semester and the +1 semester after displacement, estimation includes the full set of pre and post displacement dummies reported in Table 2.

	Dependent Variable:		
	Probation (total)	Prison (total)	Observations
Cohort			
1939-1959	1.795** (0.535)	1.870*** (0.342)	18645/50303
1960-1981	1.296 (0.281)	1.363 (0.258)	20377/31044
Education			
Less than high school	1.604* (0.434)	1.667** (0.339)	17279/31058
High school	1.440 (0.350)	1.396* (0.251)	17825/42057
University	1.236 (1.364)	3.381* (2.290)	1996/5876

Robust and clustered standard errors at the individual level.
: Significant at 10%, **: Significant at 5%, *: Significant at 1%*

Figure 1 – National Unemployment and Displacement Rates

This Figure displays the unemployment rate in Denmark from 1990 to 2000 (right-hand axis) and the displacement rate in Denmark in the same period (right-hand axis). The Figure presents the displacement rate for the three different definitions: using Jacobson Lalonde and Sullivan's (1993) peak firm employment in 1985-1989 (second line from the top). Using the average 1985-1989 firm size (third line from the top). Using a firm-specific trend whose construction is described in the paper (fourth line from the top).

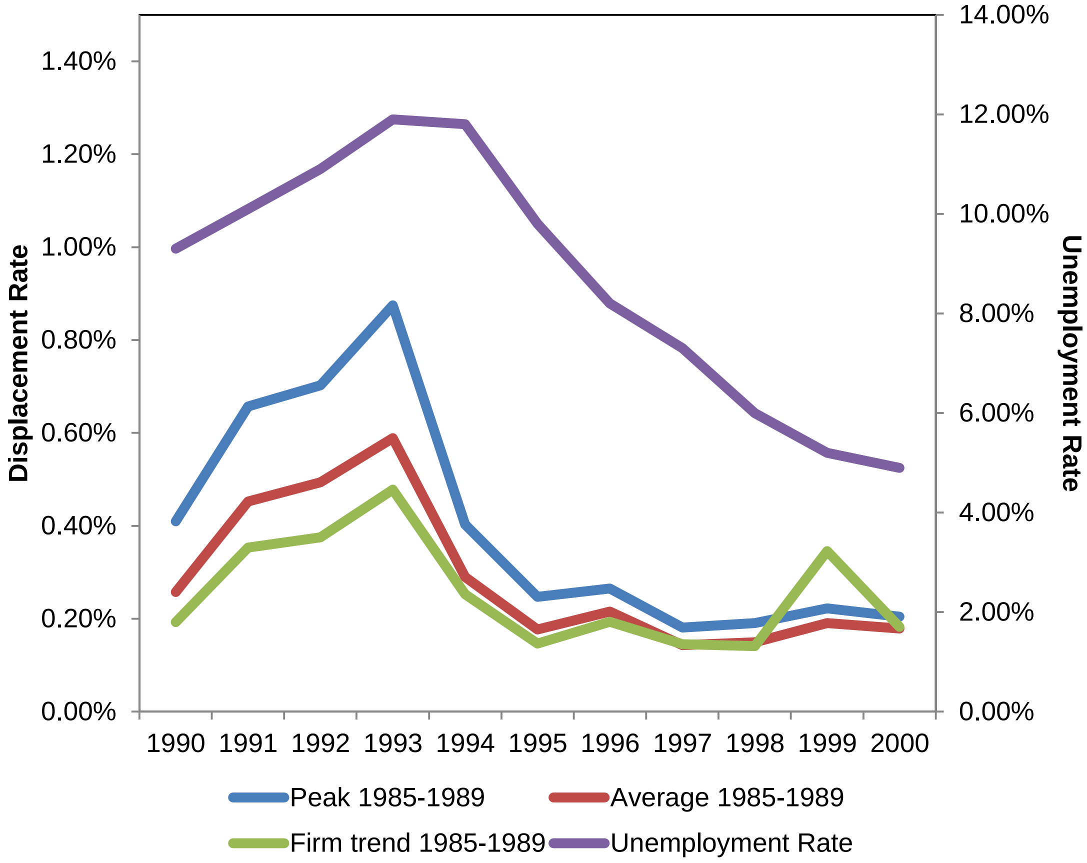
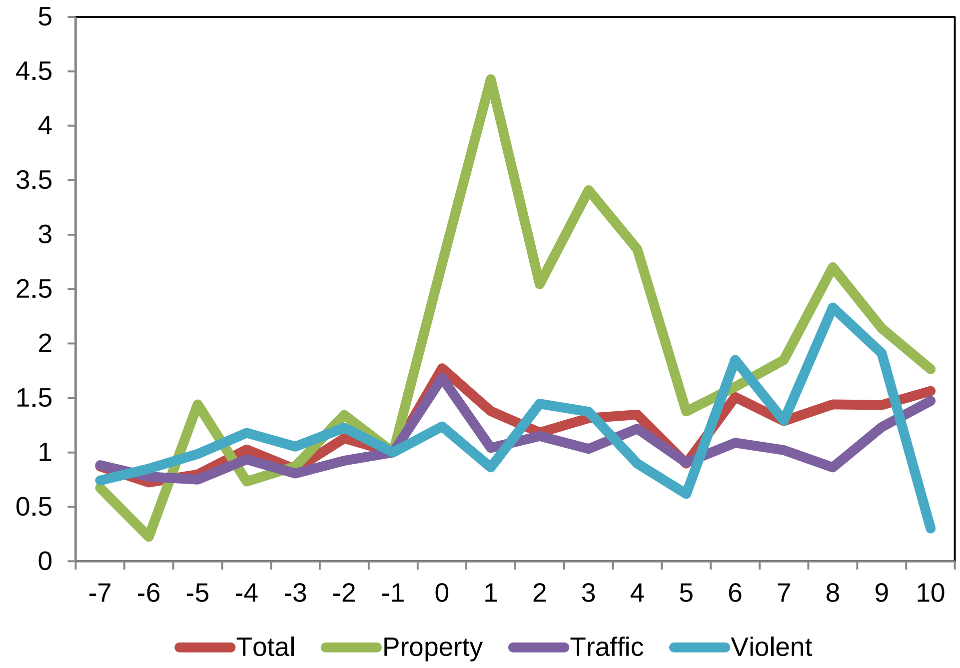


Figure 2– Impact of Displacement on Odds of Crime

This Figure graphically depicts the impact of displacement on the crime rate, i.e. the results of Table 3. Dotted lines indicate 95% confidence intervals.

(i) Prison Sentence



(ii) Probation

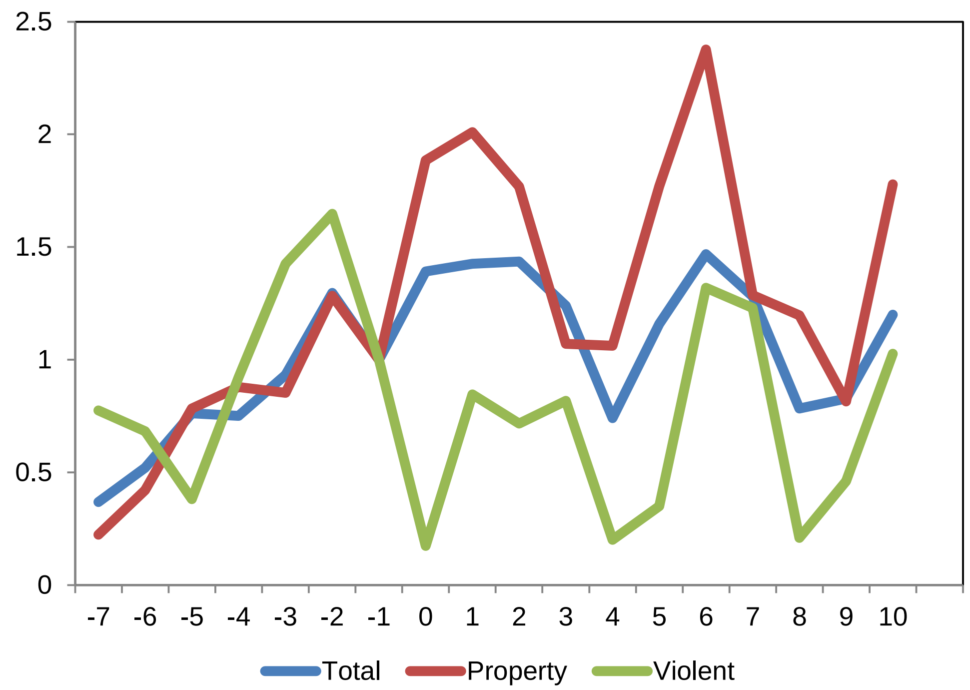
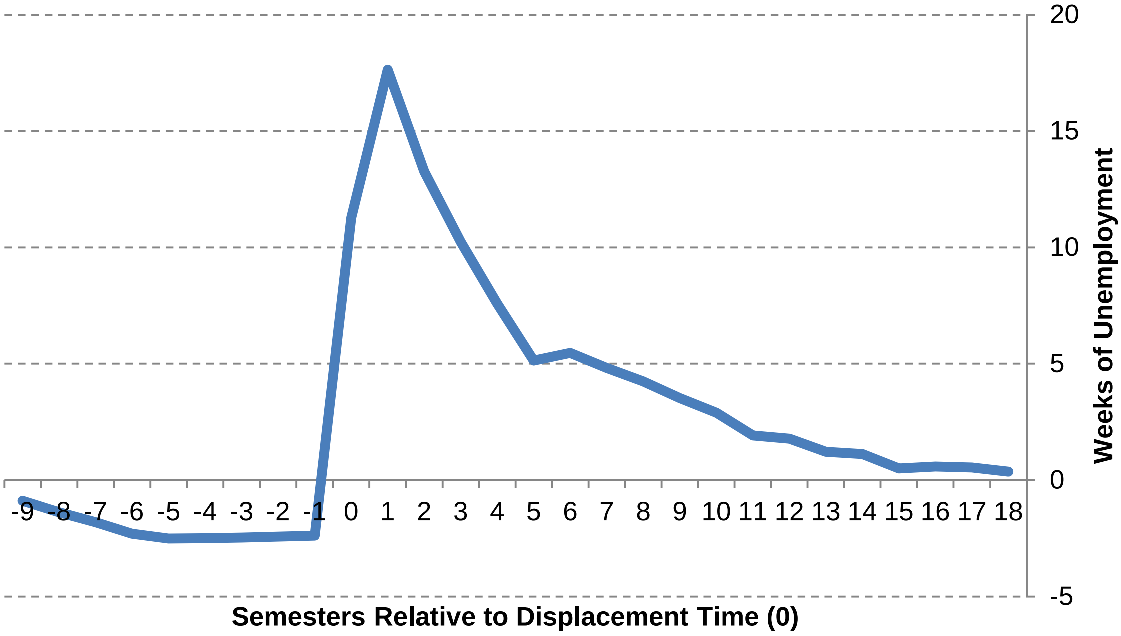


Figure 3 – Earnings Losses and Unemployment Status of Displaced Workers

This figure presents the impact of displacement on earnings and unemployment status in the semesters following the displacement event.

(i) Unemployment



(ii) Earnings

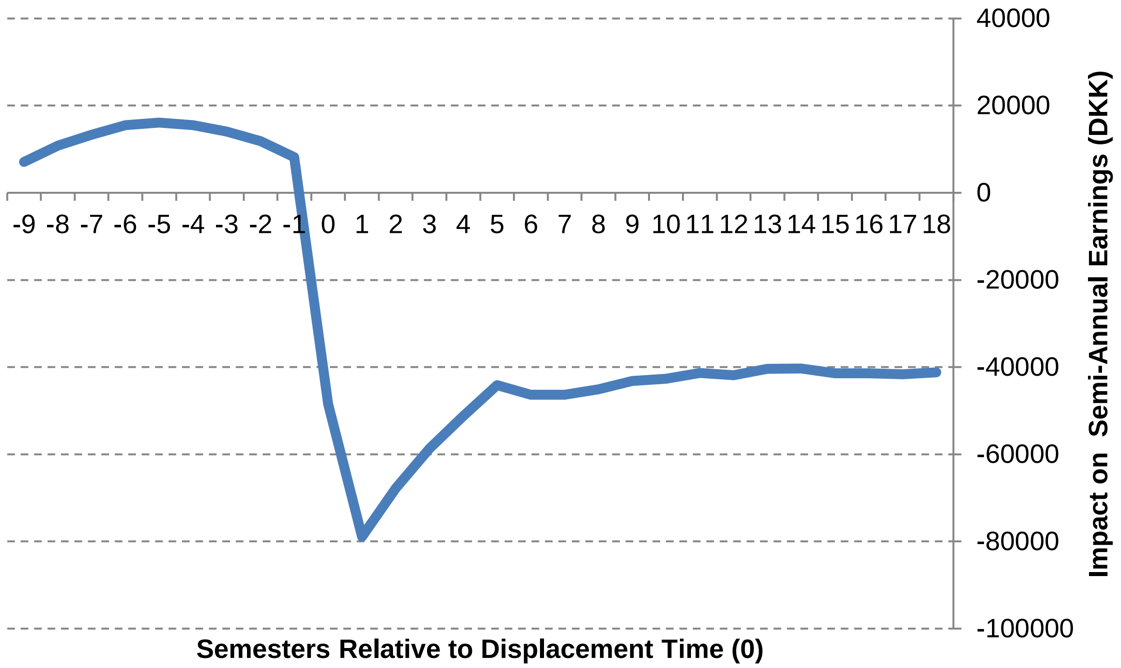
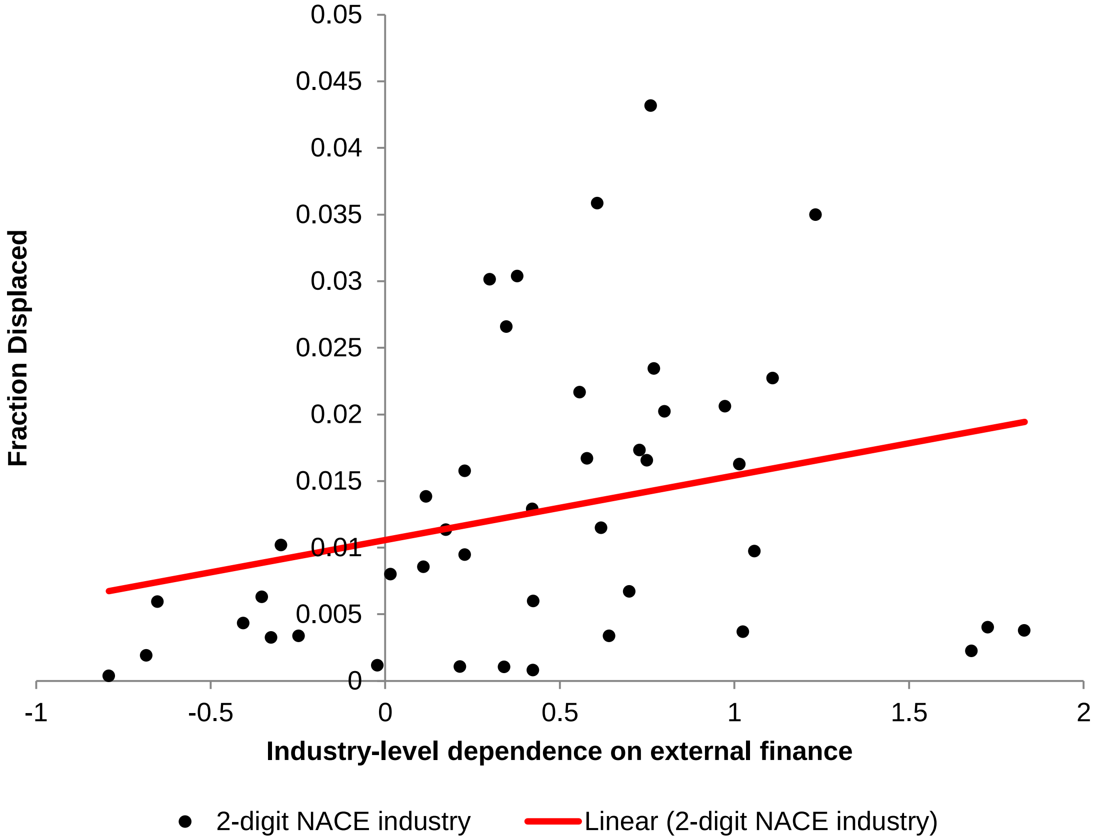


Figure 3 – Job Displacement and the Nordic Banking Crisis

This Figure graphically depicts the relationship between the industry-level fraction of displaced workers (vertical axis, using the JLS peak definition) and the industry-level measure of dependence on external finance, introduced by Rajan and Zingales (1998). Industries are 2-digit NACE, and the measure of dependence on external finance is from Compustat 1990.



$$\textit{Frac. Displaced} = 1.06\% + \underset{(0.26\%)}{0.48\%}\ \textit{Dependence} + \textit{Residual}$$