Job Displacement and Crime: Evidence from Danish Microdata*

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Abstract

This paper matches a comprehensive Danish employer-employee data set with individual crime information (timing of offenses, charges, convictions, and prison terms by crime type) to estimate the impact of job displacement on an individual's propensity to commit crime. We focus on displaced individuals, i.e. high-tenure workers with strong attachment to their firm, who lose employment during a mass-layoff event. Pre-displacement data suggests no evidence of endogenous selection of workers for displacement during mass-layoffs: displaced workers' propensity to commit crime exhibits no significantly increasing trend prior to displacement; and the crime rate of workers who will be displaced is not significantly higher than the crime rate of workers who will not be displaced. In contrast, displaced workers' probability to commit any crime increases by 0.52 percentage points in the year of job separation. The effects are driven by the propensity to commit property crime, which increases by 0.38 percentage points, or about 26% of the population-wide average. The substantial post-displacement earnings losses, coupled with the effects on property crime, are consistent with Becker's (1968) economic theory of crime. Marital dissolution is more likely post-displacement, and we find small intra-family externalities of adult displacement on younger family members' crime. The impact of displacement on crime is stronger in municipalities with higher capital and labor income inequalities.

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1 Introduction

The causal estimation of the determinants of crime is a central focus of economics. Such determinants are a key input for policymaking, as crime causes significant private and social costs (Anderson 1999), and affects voters' perceptions of politicians' effectiveness (Arnold & Carnes 2012). Additionally, understanding the drivers of crime is a litmus test for behavioral theories, such as Becker's (1968) theory of crime, which argues that a core motive of criminal behavior is an individual comparison of benefits and opportunity costs. There is, however, disagreement on crime's specific drivers. While descriptive statistics suggest a broad coincidence of the timing of the peaks in unemployment and the peaks in crime rates (see Figure 1 for Denmark), Levitt (2004) lists the economy as one of the factors that have too small an effect on crime to explain the 1990s crime rate spike and decline. On the other hand, a substantial body of literature (Gould, Weinberg & Mustard 2002, Öster & Agell 2007, Fougère, Kramarz & Pouget 2009) finds economically significant impacts of unemployment on crime using credible instrumental variable strategies that predict unemployment rate fluctuations at the area-level: U.S. states and counties, Swedish municipalities, and French departements.[1]

Area-wide estimates of the impact of the unemployment rate on aggregate crime are policy-relevant, as such estimates capture spillover effects as well as direct effects; an important challenge is to identify what, in such area-wide estimates, is due to the direct impact of individual unemployment on individual crime. Indeed, explaining changes in aggregate crime rates through changes in individual criminal activity is an active area of research (Cook, Machin, Marie & Mastrobuoni 2013). Individual estimates nevertheless require a combination of longitudinal data on unemployment spells, employment spells, and criminal activity with an identification strategy that uses arguably exogenous determinants of job separations.

This paper estimates the impact of job separations on the propensity to commit crime using a unique 1985-2000 employer-employee panel of all prime-aged male individuals in Denmark born from 1945-1960, matched with crime records (offenses, charges, convictions, and prison terms), with the timing of unemployment and social assistance spells, and with family identifiers. Given that

[1] Previous aggregate studies have found significant and modest impacts of unemployment on total (Gould et al. 2002, Öster & Agell 2007) and property (Raphael & Winter-Ebmer 2001, Lin 2008, Fougère et al. 2009) crimes, where a one percentage point increase in the unemployment rate increases total crime by around 5-6% and property crime by around 3-7%. Fougère et al. (2009) finds a one percentage point increase in the youth unemployment rate increases burglaries by 16-35% and auto theft by 22-25% while Falk, Kuhn & Zweimüller (2011) finds a one percentage point increase in the unemployment rate increases right wing extremist crime by 10-20%.

unemployment and social assistance records follow more than 99% of individuals in Denmark, the longitudinal panel provides a comprehensive, almost balanced, panel of individuals since 1985. The data measures recipiency of unemployment benefits at weekly frequency, and crime events at daily frequency. This allows us to determine the specific timing of job separations and criminal activity to single out criminal events happening after the job separation within a given year. Further the paper records the day of the offense separately from the day of the charges and the day of the conviction, which is key in eliminating observations for which crime drives job separations rather than separations driving crime.

We focus on job separations for *displaced* workers: high-tenure workers who experience job separation during a mass-layoff event, i.e. an event in which a firm loses more than 30 or 40% of its workers relative to either the firm's peak employment in 1985-1990, the firm's average employment in 1985-1990, or relative to a firm-specific trend in 1990-1994 predicted using 1985-1990 employment levels. Using year-to-year declines in firm size of more than 30% relative to firm-specific employment trends allows this paper to consider firm size changes that are arguably sudden and unexpected. As the longitudinal panel follows individuals over time and across municipalities, this paper's identification strategy can additionally control for individuals' non-time-varying unobservables that drive crime and are correlated with displacement, and for municipality-level confounders such as spatial variation in crime-related expenditures or spatial variation in crime-reporting levels.

Displaced workers experience no significant upward trend in their propensity to commit crime prior to displacement; estimated pre-trends display neither statistical nor economic significance. Additionally, displaced workers' propensity to commit crime prior to displacement, in 1985-1989, is not significantly higher than for individuals with similar tenure who will not be displaced. Such placebo tests therefore do not provide evidence of endogenous separation of high tenure workers during mass-layoff events.

The paper finds that job displacement leads to significant impacts of displacement on the probability of committing crime. The probability of committing crime increases by 0.52 percentage points in the year of displacement, by 0.5 percentage points a year after displacement, and by 0.46 percentage points four years after displacement. Such displacement events thus raise displaced individuals' crime rates from below the national average (1.33% for high-tenure workers vs. 1.6% for the national average of males in 1989) to a crime rate above the national average (1.85% post-displacement vs. a national average of 1.6%). Results are driven by workers with at most a high school education: post-displacement, high-tenure displaced workers with a low level of education typically do not regain employment with similar duration, and experience short-, medium-, and long-run earnings losses of up to 69% of a standard deviation. In line with Becker's (1968) theory of crime, empirical results suggest that the paper's main effects are driven by property crime: the probability to commit property crime increases by 0.38 percentage points in the year of displacement, by 0.36 ppt a year after displacement, and by between 0.25 and 0.56 percentage points two to seven years after displacement.

The results are robust to a variety of alternative specifications: specifications with a saturated set of individual, municipal, and year fixed effects; with time-varying controls for changes in marital status, income, and the number of children; and using alternative mass-layoff definitions. In particular, a potential concern with using a 30% decline in employment relative to the firm's 1985-1990 peak or average employment is that firm size may be declining slowly rather than in a sudden and unexpected way. A corresponding robustness check defines a mass-layoff event as a 30% reduction in firm size relative to a firm-specific trend in employment, estimated on 1985-1990 firm size data, and extrapolated to the 1990-2000 decade. While the use of such firm-specific trend halves the number of measured mass-layoff events, estimates of the impact of displacement on crime are left virtually unchanged. Results are robust to either a 30% or a 40% threshold for firm size changes as a definition of mass-layoff events, which mitigates concerns that 30% declines may capture idiosyncratic firm size changes; and results are robust to considering firms larger than 50 workers.

After estimating the direct impact of job displacement on individual crime, a natural extension is to understand how displacement events interact with the individual's family. In particular, the paper estimates whether displacement affects family structure, and whether family structure mitigates the effects of displacement on crime. The paper's baseline results are significant regardless of family structure, i.e. regardless of whether the individual is single, has one or more children, and is in a couple in 1989. However, the impact of displacement on crime is about three times higher for individuals who were single male adults, a finding consistent with the hypothesis that intra-family income pooling may offer partial consumption insurance post-displacement. Evidence does not suggest a change in spousal work hours or income in response to the individual's displacement. The probability of marital dissolution increases post-displacement, with a decline of about 0.9 percentage points in the probability of being married in the year of displacement and of 3.5 percentage points seven years after displacement. The data suggest some potential but small impact of adult family members' displacement on son's crime with a lag, a year after the displacement event.

The importance of municipality-level factors such as income inequality and poverty are also examined. Tax data on wage and capital income enable the construction of income inequality and poverty concentration measures for each of the 270 municipalities.[2] While Denmark as a country exhibits low income inequality (Atkinson & Søgaard 2013, Piketty 2014), within Denmark the five most unequal municipalities have a Gini coefficient about twice the Gini in the five least unequal municipalities. In a cross section, municipalities' Gini coefficients are not significantly correlated with either total or property crime rates. Idiosyncratic job displacement causes a post-displacement decline in the individual's income percentile at the municipal level of about 2.8 percentile points in the year of displacement, and of 3.3 percentile points seven years after displacement. Moreover, displaced workers residing in municipalities in the upper quartile of the Gini distribution are about twice as likely to commit crime post-displacement than workers residing in the lower quartile of the Gini distribution. Importantly, workers in the Copenhagen city area, i.e. those in the municipalities of Copenhagen and Frederiksberg, are not driving the results.

The findings at the individual, family, and municipality levels should be relevant to policymakers and researchers alike. As the data links the employee with his corresponding peers in the family and the municipality, the paper allows an estimation of the impact of job separations beyond its impact on the employer-employee pair. The paper's results suggest that firms' mass-layoffs lead to an increase in the probability of offenses, charges, convictions, and prison terms, which have corresponding social costs for victims, as well as policing, prosecution, and incarceration costs. As such, job separations are unlikely to be efficient (Blanchard & Tirole 2008). Further, higher incarceration rates likely worsen individuals' employment prospects: earnings losses for displaced individuals committing a crime *and* convicted to prison are substantially higher than earnings losses for similarly displaced individuals committing a crime but whose conviction does *not* lead to incarceration.

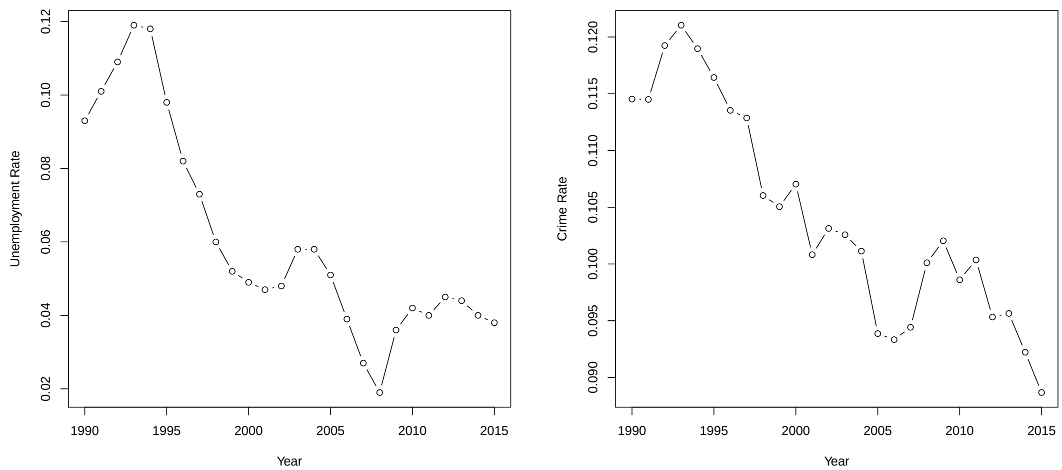
The paper contributes to at least three literatures. First, seminal papers have estimated the impact of job displacement on earnings (Jacobson, LaLonde & Sullivan 1993), health (Black, Devereux & Salvanes 2012), mortality (Sullivan & von Wachter 2009), family structure (Charles &

[2] The paper uses the pre-2007 definition of municipalities, which yields areas of average size $155km^2$, smaller than the average size of a U.S. Census Zip Code (ZCTA), $228km^2$.

Figure 1: Descriptive Comparison – Unemployment and Crime Rate

Crime Rate: total number of reported crimes over total Danish population. Source: combined register data, supplemented with Statistics Denmark's STRAF20 statistic. Unemployment rate: male and female as a fraction of labour force as of January 1, from AULAAR. Population on 1. January, from FOLK2.

(a) Unemployment Rate, 1990–2015 (b) Crime Rate, 1990–2015



Stephens Jr. 2004), children's school performance (Rege, Telle & Votruba 2011), and regional mobility (Huttunen, Moen & Salvanes 2015). To our knowledge, crime is a yet unexplored outcome of job displacement, as records of criminal events, such as data from the FBI's Uniform Crime Reports (UCR) or the National Incident-Based Reporting System (NIBRS), are typically hard to match with employer-employee data sets. Denmark's collection of multiple sources of comprehensive registry data, linked together by individuals' Central Person Register numbers, is a unique opportunity to understand the timing of crime and employment spells. In Denmark unemployment benefits and social assistance recipiency data cover more than 99% of Danes after a job separation, which provides an almost balanced panel data set that bridges the typical data gap between employment spells. The paper combines the job displacement literature and economics of crime as results suggest that earnings losses in the formal sector cause increased property crime. Denmark and other countries studied in prior literature, in particular the United States, differ in their judicial and labor market institutions. But the paper's findings of significant impacts of displacement on crime in a country with relatively high unemployment benefits (Andersen & Svarer 2007) and lower crime rates (Lavrsen & Pedersen 2013) suggest that the paper's results are a lower bound of the impact of job displacement on crime. Finally, the paper presents novel results that document costs of incarceration above and beyond the direct costs of incarceration and parole supervision. Indeed, higher incarceration rates may lead to more substantial earnings losses: Individuals who are convicted to prison experience earnings losses of up to 14,000 Danish Kroner (constant 2000 Danish Kroner, 8% of an S.D.) higher than individuals who are convicted to another outcome than prison (suspended sentence, fine, or settlement).

The paper also contributes to the literature on the economics of crime. While prior literature has estimated the impact of changes in the unemployment rate due to changes in the industrial structure of states or counties (Gould et al. 2002, Lin 2008, Fougère et al. 2009), this paper uses a subset of idiosyncratic job separations to estimate the impact of individual displacement on individual crime probabilities.[3] The Appendix presents a simple job search model[4] that formalizes the difference between the area-wide approach based on unemployment rate fluctuations and the approach based on individual separations. The former literature uses variations in the unemployment rate that correspond to simultaneous changes in the arrival rate of offers, the separation rate, and the distribution of offered wages. The paper's approach instead focuses on isolating the impact of job separations during a mass-layoff event on crime. As our treatment effects are estimated relative to other workers' crime rates in the same year and the same municipality (includes year, municipality, and individual fixed effects), the paper isolates the impact of job separations from the impact of changes in the demand for labor (changes in the economy-wide arrival rate of offers and changes in the wage distribution).

Third, the paper speaks to the literature on the consequences of income inequalities on an area's crime rate. Brush (2007) and Choe (2008) show that, in the cross-section and in first-differenced panel, income inequality is significantly correlated with crime. Kelly's (2000) results suggest a significant correlation between inequality and violent crime, but not between income inequality and property crime. Bertrand & Morse (2013) finds that exposure to higher consumption levels by

[3]In addition to literature examining the impact of unemployment on crime, previous studies have examined the link between other economic conditions and crime such as wages (Grogger 1998, Machin & Meghir 2004), time spent in unemployment (Bindler 2015), and the impact of graduating during a recession on crime (Bell, Bindler & Machin 2014).

[4]The mechanism is formalized in a framework as in Stigler (1961) and McCall (1970). The mechanism could be introduced in a Lucas & Prescott (1974) or Mortensen & Pissarides (1994) model.

higher-income households leads to higher individual consumption levels. Such results are consistent with economic theories of interpersonal comparison and envy (Veblen & Mills 1958), which, when confronted with Becker's (1968) economics of crime, should predict across-municipality variation in crime rates driven by differences in income distribution. The availability of data on the geographic location of residence together with income tax data allows us to combine individual trajectories with area-wide data. The paper's results suggest that, as individuals experience displacement, they are more likely to engage in crime post-displacement if they live alongside higher-income peers. Another implication is that crime prevention policies should be both based on individuals and place-based, as displacement impacts on crime are twice as high in municipalities in the upper quartile of the Gini distribution.

The paper proceeds as follows. Section 2 describes the merged employer-employee-unemployment and crime data set from 1985 to 2000. Section 3.1 presents the identification challenges when correlating job separations with crime. Section 3.2 then describes the identification strategy using displaced workers as a subset of idiosyncratic job separations. Section 3.3 introduces the pre- and post-displacement econometric specification, and Section 3.4 shows the paper's main results. Section 4 then analyzes (i) how family structure affects the impact of displacement on crime, (ii) whether displacement leads to marital dissolution, and (iii) whether fathers' displacement affects children's criminal activity. Section 5 measures local income distribution to identify whether displacement has a greater impact on crime in more unequal municipalities. Finally, Section 6 concludes.

2 Data Set

The Employer-Employee, Unemployment, and Crime Data Sets

An estimation of the impact of job displacement on crime at the individual level requires multiple sources of individual longitudinal panel information: employer-employee data, crime data, data on unemployment and social assistance spells, and demographic data. First, while employer-employee data has detailed information on wages, payroll, firm and worker identifiers, it typically does not capture time periods where an individual is either outside the labor force or looking for a job. Individuals may commit crime during these unemployment spells. Second, estimating the impact of job displacement on crime requires information on the criminal history of each individual throughout the criminal justice system, from the offense to potential convictions and prison time. Third, estimating the impact of job losses on children's criminal activity requires matched household members and their age, marital status, and family ties.

Danish Register Data, made available by Statistics Denmark, is a database of every individual formally residing in Denmark from 1980-present, which is collected by government agencies. The employer-employee data set is then constructed through five primary sources: (i) the Integrated Database for Labor Market Research known in Danish as *Den Integrerede Database for Arbejdsmarkedsforskning (IDA)*, which follows a worker in employment spells, (ii) the Central Register of Labour Market Statistics, which follows individuals during weekly unemployment spells, known in Danish as *Det Centrale Register for Arbejdsmarkedsstatistik (CRAM)* (iii) the Central Police Register, which compiles information from the police and the courts, (iv) the Population Registers, with demographic information and household structure, and (v) the Danish Student Register, which provides information on the highest level of education completed and current student status. Individuals are linked across these different data sources using anonymized individual Central Person Register (CPR) numbers, present across all data sources.[5]

The Integrated Database for Labor Market Research (IDA) compiles data reported annually by employers both at the workplace, firm, and employee levels. The employer-level data contains firm identification numbers, unique workplace identifiers, the number of workplaces in a firm, and the number of employees in each workplace. This is matched to the employee data at the firm level which provides information such as information on part- or full-time employment status, annual salary earned in the position, information on secondary employment, as well as the workplace identification number. Annual salary is measured as pre-tax earnings resulting from the employer-employee relationship and is annually collected from employers who are required to report all salaries paid to all employees to the Danish tax authority SKAT.[6] All of the employee and employer data contained in IDA is observed annually, as in the French (Abowd, Kramarz & Margolis 1999) and Pennsylvania (Sullivan & von Wachter 2009) employer-employee data sets.

Danish registry also addresses a common issue with employer-employee data, i.e. the lack of data in-between employment spells. As identifying precisely when an individuals flows into unemployment

[5] An individual's CPR number is a national identification number used when interacting with public services, similar to a social security number in the US.

[6] This is a short name for *Skatterådet*, i.e. Danish tax board.

is crucial for our empirical design, we supplement the IDA file with data on social assistance and unemployment insurance (UI) benefits received by the individual, from the CRAM registry described above. Importantly, all individuals in the sample constructed for the purposes of estimation are eligible to receive either social assistance or UI benefits. Unemployment statuses are observable at a weekly frequency, allowing us to know the first week an individual is receiving either social assistance or unemployment insurance benefits as well as for how long an individual receives these payments. This paper's annual data set thus aggregates this weekly data on unemployment, and matches them with the individual's annual data. This allows us to (i) measure the number of weeks of unemployment per year, (ii) measure whether individuals commit an offense during an unemployment spell.

Crime data contained in the Central Police Register is a compilation of police and court records. Individuals who are cited or arrested are then formally charged and assigned a police case number. When such a case number is allocated, it is then matched to the charged individual's CPR number and to the Danish police station within a district that charged the individual. If multiple people are charged with committing the same crime, we observe that each of the multiple co-offenders are matched to the same police case number. The data on criminal charges include the day of the offense and the day charges were filed.

Charges are assigned a code corresponding to the Danish classification of offenses. We sort offenses into three broad categories: property crimes, violent crimes, and crimes related to driving under influence (DUI), but also examine total crimes which comprises these three most frequent crime types as well as less frequent crime categories: sexual, narcotics, firearms, tax, unknown and other crimes, and crimes against special legislation. Crimes "against special legislation" include health-related crimes, environmental crimes, violations of construction and housing laws, crimes related to defense laws. Table 1 panel (iv) shows that about 2.27% of the sample is charged in any given year from 1985-2000. A majority of charges translate into a conviction, as 1.91% of the sample is convicted of any crime. The majority of convictions are driving under influence convictions (0.67% of the sample, which is about 0.67/1.91=35% of convictions), and property crime convictions (0.65/1.91=34%). A minority of convictions are related to violent crimes or other types of offenses. Figure 2 presents a breakdown of crime by subcategory within the broad crime categories. We focus here on the overall observations (blue points). A majority of DUI offenses are labelled as "high blood alcohol contents", i.e. more than 1.2g of alcohol for every dL of blood. Among property crimes, theft is the largest category, and among violent crimes, minor violence, i.e. violence not resulting in death or injury, is the largest violent crime category. Together with 'violence against an innocent', these represent more than 2/3 of violent crime, 67.2%.

The police case number follows an individual from charges to courts' convictions. The Central Police Register includes conviction date and conviction outcome. Such outcome can be either incarceration, a suspended sentence, a fine, a settlement, no charge/warning, or another less frequent decision such as a youth program or military punishment. While all of these are possible conviction outcomes, the majority of convictions in Denmark result in a suspended sentence, followed by a fine, and incarceration. In what follows, we focus on the first four conviction outcomes, that is incarceration, suspended sentences, fines, and settlements.[7] Eventual incarceration dates are recorded in a similar fashion, with start and end dates, linked to the police case number. Table 2 describes the timeline from the day of the offense to the day of the charges (upper panel), from the charges to the conviction (middle panel), and from the conviction to the start of the prison term (bottom panel). Multiple charges are typically filed for a single offense, hence the large number of charges (3,729,636) and convictions (1,882,930). In the sample with a least one conviction, charges are filed the same day as the offense for the median observation. For charges that are not filed the same day as the offense, the median is at 42 days. About 50.5% of charges translate into a conviction (second line of the middle panel). Such conviction rate is substantially lower than in other countries such as the United Kingdom, where the conviction rate stood at 82% in 2014 (of Justice of the United Kingdom 2014).

Section 4 below will estimate effects within families and by education. We obtain family and individual demographic data from the *Population Register*. Such register is an administrative data set of all individuals in Denmark, regardless of their labor market status or criminal records. The data include age, gender, municipality of residence, the date the individual's residence last changed, his immigrant status, marital status, and the mother's and father's CPR number. Family members are assigned a family identification number. A household is defined as a set of individuals residing at the same address including any children living at home, with no upper age limit on the children.

[7] Settlements are described in paragraph 723 of the Danish criminal code, at https://www.retsinformation.dk/eli/ft/199112K00184.

To be considered a family, two adults residing together must be registered as a cohabiting couple, as a married couple, in a registered partnership, or have a common child,[8] such that two individuals sharing a housing unit with no such connection will be considered two families. When either an individual or family move, their move is self-reported to the Public Registration Office (*Folkeregisteret*). Individuals have significant incentives to report their address changes as these are connected to public services and welfare payments.

The Danish Student Register contains education data such as an individual's educational qualification and educational institution as well as information of any ongoing schooling. For Danes, educational institutions in Denmark are required by law to report this information to the Ministry of Education. We use such administrative education level to categorize a worker's educational attainment into three categories: high school or less education, vocational education, and university education or beyond. The share of non-natives in the early 1990s is relatively small hence measurement error in education is unlikely to be a substantial concern (Dustmann, Frattini & Preston 2013).

Merged Longitudinal Data Set

This paper's merged longitudinal data set links the five above-mentioned sources of longitudinal information to estimate the correlation between job separations and crime. Further sample restrictions are introduced in section 3.2. As endogenous exit and/or reentry in the sample could be an issue, we focus on individuals who remain in the sample in 1985-2000. In a given year, 0.64% of individuals are not in the data set in the next year, and 0.35% have no observation in the previous year. Results are robust to the inclusion or exclusion of individuals for whom we do not observe all annual data points.[9] We focus on a longitudinal panel of native men in 1985 to 2000. Indeed, following well-established prior evidence (Freeman 1999), the data set suggests that the majority of crimes are committed by men, as in Denmark, 86% of all 1985-2000 convictions are given to males.[10]

The paper estimates the impact of job loss on criminal activity, and focusing on a subset of prime-aged individuals for which labor market participation rates are high. Figure A of the Appendix

[8]The definition of the statistical concept of a family is provided by the Act on Statistics Denmark §6, Legislative Decree no. 599 of 22 June 2000.

[9]Sullivan & von Wachter's (2009) results indicate that job displacement causes increases in mortality rates. Given the robustness of our results to the inclusion of individuals without a full 1985-2000 set of observations, it is likely that post-displacement mortality results are relatively unrelated to criminal activity.

[10]Source: StatistikBanken at Danmarks Statistik (http://www.statistikbanken.dk/); convictions for all types of offenses in the period 1985-2000 (STRAF40).

suggests that birth cohorts from 1945 to 1960 are birth cohorts for which the employment rate is high and relatively stable. The employment rate increases for cohorts up to the 1943 birth cohort. While 72% of males of the 1926-1944 cohorts are employed in 1990, such employment rate is 87% for the 1945 birth cohort in 1990. On the other end of the age distribution, for younger individuals, focusing on the 1960 and prior cohorts also focuses on individuals with stable attachment to the labor force. While only 78% of males in the 1961-1972 cohorts are employed in 1990, the employment rate is 83% for the 1960 birth cohort.

Table 1 presents descriptive statistics for our merged longitudinal data set of Danish males born in 1945-1960 continuously in the panel from 1985 to 2000; the variables are broken down into each of the five sources. The total number of individual×year observations is 8,830,448 or 551,903 observations per year. The median individual earns a wage of 247,029 Danish Kroner (37,310 USD in 2016),[11] and works in a firm with 183 employees. The weekly unemployment data set, collapsed at the annual level, provides the number of weeks of unemployment. The average number of weeks of unemployment is 2.88, which is about a 5.5% year-round-equivalent unemployment rate. Highest education levels achieved are recorded in the data set for 98.51% of the data set, with 1.49% missing. The median individual contributes 53.76% of his family income, with a median household size of 3 composed of 2 adults and 1 child in the family.

Panel (v) of Table 1 presents data on weeks spent receiving unemployment insurance and social assistance payments for individuals with at least one week of unemployment. The median unemployed individual spends 12 weeks on benefits. Although joining an unemployment insurance fund is voluntary, more than 90% of workers aged 30-45 (our 1945-1960 cohorts) were part of a fund in 1990-1995 (Parsons, Tranaes & Lilleør 2015). But Parsons et al. (2015) reports that there are significant adverse selection effects into unemployment funds, whereby the generosity of social assistance benefits tends to lower enrollment rates in unemployment insurance funds. Thus, in this paper, we count an individual as unemployed in a given year either if he receive unemployment benefits or if he receives social assistance benefits. Social assistance (first line of panel (v)) is a means-tested social safety net for individuals not enrolled in an unemployment insurance fund. The panel reports the number of weeks on social assistance for individuals with at least one week of social assistance

[11] Individuals with no wage from employment, excluding all other sources of income enter as zero on this line of the descriptive statistics table.

recipiency. In 1995, less than half a percent (0.45%) of the workforce were looking for work but did not receive unemployment compensation nor were registered at the unemployment office (Parsons et al. 2015). Such 0.45% of the workforce are individuals who are not eligible because of high capital income, family assets or spousal earnings, those who voluntarily choose not to claim the benefits even though they are eligible, or those who have been on the benefits so long their eligibility has expired. As the majority of the workforce is covered by either scheme, together with the employer-employee data provides a longitudinal sample of individuals in the Danish workforce.

3 Empirical Strategy

This section documents the endogeneity of individual transitions into unemployment (Section 3.1), focuses on firms experiencing mass-layoffs and on displaced workers as an identification strategy to estimate the impact of job separations on crime (Section 3.2), presents this paper's main econometric specification (Section 3.3), and presents its estimation results (Section 3.4).

3.1 Sample Correlations and Confounding Factors

Table 3 presents correlations between transitions into unemployment and crime using the sample described in Section 2. This table is purely descriptive, and will help in defining the identification challenges when estimating the impact of job loss on criminal activity.

Column (1) of the table presents the OLS regression of a *Crime* indicator variable on a set of annual pre- and post-transition into unemployment dummies.[12] We focus on the individual's first transition into unemployment in the 1985-2000 period. As we observe unemployment status at the weekly level the data set provides the year in which the individual first experienced unemployment. The specification includes all year-level unemployment dummies and thus the average value of the unemployment dummy coefficients will be equal to the average impact of unemployment on crime.

The *Crime* indicator variable is defined as follows. It is set to 1 in year $t = 1985, 1986, \ldots, 2000$ if the individual commits a crime (offense) in year t that will then lead to a conviction in any year $t' \geq t$. The date of the offense is entered by police staff either at the time a crime is reported to the

[12] All throughout the paper, and in particular in the displacement regressions of subsequent Section 3.3, we use linear probability models for the sake of clarity. Linear probability models yield in this paper results that are very similar to the marginal effects of logit regressions, with or without individual fixed effects.

police, or, at the latest, when charges are brought.

Focusing on the timing of the offense rather than the timing of the conviction helps alleviate concerns of reverse causality, that is an offense which results in job loss. Using the timing of the conviction could indeed lead to capturing cases where the individual commits an offense, which lead to both a change in the worker's employment status and to a conviction. We thus only consider the timing of offenses.

Focusing on offenses leading to a conviction rather than simply offenses also helps alleviate issues related to the measurement of a large volume of minor crimes or due to differences in reporting behavior across police districts. Table 3, columns (1) and (2) use total crimes, including property, violent, and D.U.I. crimes as well as the other crime types discussed in Section 2, as a dependent variable. Columns (3) and (4) set $Crime = 1$ when an individual commits and is convicted for a property crime only. Standard errors are two-way clustered at both the year and the individual levels (Cameron, Gelbach & Miller 2012).

Columns (1) and (3), which do not include an individual fixed effect, suggest that, while crime is statistically and economically significantly higher post-transition into unemployment, the probability of committing crime is also higher pre-transition into unemployment. Columns (2) and (4) include an individual fixed effect in the regression. Such an individual fixed effect captures non–time-varying unobservables that cause both transitions into unemployment and criminal activity. In columns (2) and (4) as well, significant pre-transition-into-unemployment effects are observed.

Overall, columns (1)–(4) strongly suggest that any identification strategy aiming at identifying the *causal* impact of job loss on crime should address the issue of both non–time-varying and time-varying unobservable confounders.

Table 4 correlates a simple set of observable characteristics with the transition into unemployment indicator variable (column (1)) and the total crime variable (column (2)). Four characteristics (marital status, tenure, firm size, age) are time-varying observables. For the observables of this table, the sign of the correlation with the transition into unemployment is the same as the sign of the correlation with criminal activity. This suggests that the unobservable characteristics are also likely correlated in the same way with displacement and crime; and thus the results of table 3 are likely overestimating the impact of job loss on crime.

3.2 Displaced workers

This paper addresses the issue of the endogeneity of job separations by directing attention on displaced workers, in a similar way as in Jacobson et al. (1993) and Sullivan & von Wachter (2009). In this paper, a displaced worker is a high-tenure individual losing employment during a firm's mass-layoff event. This section defines both high-tenure individuals and mass-layoff events, leading to a sample of an arguably idiosyncratic set of job separations.

Mass-Layoff Events

In the period of analysis (1985-2000), prior literature has described evidence of the impact of the Nordic Financial Crisis (Jonung 2008) and of import competition on employment in Denmark (Ashournia, Munch & Nguyen 2014).

In this paper, we use three different approaches to pinpoint firms experiencing a mass-layoff event. All three approaches consider *sudden* and *unexpected* changes in firm[13] employment relative to a reference point. What differs across these definitions is the reference point: (i) the peak of firm employment in the pre-displacement period 1985-1989 as in Jacobson et al. (1993), (ii) the average firm employment in 1985-1989, (iii) a firm-specific trend to predict firm employment levels in 1990-2000 given the annual employment levels of each firm in 1985-1989. A mass-layoff event occurs when firm employment is 30% below its reference point, (i)–(iii), depending on the definition. 30% is a threshold that corresponds to the 10th percentile of the distribution of year-to-year change in log firm size.[14] We also consider a higher 40% threshold later in this paper, to avoid capturing idiosyncratic fluctuations in firm size. The analysis proceeds with private sector firms, for which a mass-layoff event is more likely to be driven by firm-specific factors than for public-sector firms.

One concern with using peak employment or average employment in 1985-1989 (Definitions using (i) and (ii) as the reference point for employment) is that some firms may be shrinking in size across time and that the 30% change may not be unexpected. Using a firm-specific trend as reference point (definition (iii)) helps alleviate such concern by building a predicted firm size for firms whose employment is declining. We build the firm-specific trend as follows. Note $n_{j,t}$ the employment of firm j in year $t = 1985, \ldots, 1989$, and for each $j = 1, 2, \ldots, J$ estimate the regression

[13] The paper considers firm level downsizing rather than plant-level downsizing as in Jacobson et al. (1993).

[14] Most log firm size changes are between ±8%: the lower quartile of year-to-year changes in firm size is 6.9%, the median firm experiences no change in employment, and the upper quartile is +8%.

$n_{j,t} = \alpha_j + \beta_j \cdot t + \varepsilon_{j,t}$. When using (iii) as the reference point, firm j experiences a mass-layoff in year $t = 1990, 1991, \ldots, 2000$ if $n_{j,t}$ is 30% lower than the predicted value $\widehat{n_{j,t}} = \widehat{\alpha_j} + \widehat{\beta_j} \cdot t$ when $\widehat{\beta_j} < 0$ (declining firm), and is 30% lower than $n_{j,1989}$ when $\widehat{\beta_j} > 0$.

Table 5 presents the regression of firm size on a set of indicator variables for each year pre- and post-mass-layoff event. Such regression tests whether firm-size decline trends lead to mass-layoff events, and whether mass-layoffs are the prelude to larger declines or firm closure. In this table the reference point is the firm's peak employment in 1985-1989. If a firm experiences multiple mass-layoff events, the first such event is considered but the entire set of observations of the firm is part of the regression. Column (2) includes year fixed effects, and columns (3) and (4) focus on firms with between 10 and 1,000 employees inclusive in 1989. Standard errors are clustered two-way at the firm and year level, and the regressions are performed on 573,860 firm$\times$year observations. Overall, pre-mass-layoff annual indicator variable coefficients suggest that using mass layoff and firm-specific trends in employment substantially alleviates concerns about pre-trends in firm employment, while post-mass-layoff annual indicator variable coefficient suggest that firms are, on average, 14 to 62-employee smaller post-mass-layoff. The coefficients for Year +1 to Year +5 also suggest that a substantial share of the shock is permanent, but that the magnitude of the downward shock does not increase over time.

Displaced Workers

Employees leaving a firm that is experiencing a mass-layoff event may not separate at random. In particular, individuals losing employment during a mass-layoff event may differ in unobservable dimensions from individuals staying in employment in that same firm. Gibbons & Katz (1991), Lengermann, Vilhuber et al. (2002) and Abowd, McKinney & Vilhuber (2009) argue that workers experiencing a mass-layoff event are systematically selected.

The second step of our identification strategy is thus to focus the analysis on individuals with a strong attachment to their firm by imposing a set of sample criteria on the sample from Table 1, and in Section 3.3 to design a test for pre-separation differences across workers. Four criteria are used to define strongly-attached individuals: first, individuals need to be continuously employed with the same firm from 1987 to 1989, i.e. they have been employed for at least three consecutive years by the firm in 1989. Individuals can have longer tenure in that particular firm. Second, we focus on individuals in full-time employment, who are less likely to transition in and out of employment for endogenous reasons. Third, the individual needs to be employed in a firm with 10 or more employees in 1989. This firm-size requirement avoids the problem that percentage changes in firm size are much larger for small than for large firms, for the same corresponding absolute change in employment. The firm size requirement also eliminates self-employment. Individuals with two or more jobs between 1987 and 1989 are not considered as their choice of employment may be driven by the comparison of alternative employment options. Fourth, we consider individuals who were not enrolled in education at any point between 1987 and 1989. Imposing these four criteria on the sample of Table 1 results in a final sample of 102,376 high tenure individuals over the period 1985-2000. As mentioned in Section 2, we consider native males belonging to the 1945-1960 birth cohorts, i.e. who are at least 30 years old in 1990, which is likely to yield a lower bound for the true impact of job separations on crime.

Individuals with a strong attachment to their firm, who have high tenure and are older than 30, are among the least likely to lose employment during a mass-layoff event. Indeed, the correlation between tenure in the previous year and unemployment in the subsequent year in Table 3 is statistically and economically significant ($-0.108***$), and the correlation between such lagged tenure and crime is also statistically and economically significant ($-0.073***$). Similarly, the correlation between age and both transitions into unemployment and total crime are negative and significant at 1% ($-0.084***$ and $-0.039***$ respectively, last line of Table 4).

The mass-layoff event will be a decidedly unique event in such high-tenure workers' history if their probability of job separation increases substantially at the time of the firm's mass-layoff event. The mass-layoff is a firm-wide event that affects all workers regardless of their tenure. To understand the impact of mass-layoffs on the probability of job separation for high-tenure individuals, we estimate the probability of job separation in 1989 (rather than 1990) for individuals with at least three years of tenure in 1988 (rather than 1989) and who will be experiencing a mass-layoff event in 1990. The probability of job separation in 1989 for such high-tenure individuals who will experience mass-layoff in 1990 was 3.93% in the year prior to mass-layoff. The probability of job separation for high tenure individuals in their 1990 mass-layoff event was 4.72%, as compared to, by definition a 30% or higher probability for the average worker. As the firm's employment declines by 30% or more, the rate at which high-tenure individuals with a strong attachment to their firm leave their position increases by 0.8 percentage points.

Displaced individuals are thus individuals with a strong attachment to their firm who are losing employment during a mass-layoff event. Figure 3 describes the displacement rate in 1990-2000. In this paper, we estimate the impact of displacement events occurring in the first 5 years of the decade, in 1990-1994 (solid line of Figure 3). The firm trend approach delivers the lowest displacement rate, while the peak employment approach delivers the highest displacement rate. In that sense the firm trend approach is more conservative in that it focuses on firms whose employment changes are sudden relative to the firm-specific trend. Displacement rates range between 0.5% (1990, firm trend definition) and 1.5% (1993, peak definition). The rise in displacement rates in the early 1990s matches the rise in unemployment rates, from 9.61% in 1990 to 12.3% in 1993. The decline in displacement rates from 1993 to 1995 is however steeper than the decline in the unemployment rate in the 1993-1995 period: the unemployment rate declines to 10.2% in 1995 only. This matches the finding that displaced individuals, who have at least three years of tenure prior to displacement, transition to employment with shorter spells and longer durations of unemployment.

A placebo test can estimate whether considering the subset of displaced workers addresses some of the identification concerns that were highlighted in the previous subsection 3.1 and in Tables 3 and 4. Specifically, Table 6 presents a set of regressions of the criminal activity, in 1985 to 1989, of individuals with high tenure in 1985–1989 who will be displaced in 1990–1994. If individuals who will be displaced differ in their unobservables from individuals who will not be displaced, e.g. from individuals who will stay in employment during a mass-layoff event, we should observe that the criminal activity (an offense leading to a conviction) of such future displaced workers is higher than the criminal activity of individuals who will not be displaced. Column (1) of Table 6 presents such regression for property crime, in OLS and with no additional controls. Column (2) includes municipality fixed effects and additional controls: indicator variables for education (less than high school, high school, vocational education, university education or greater), for marital status, control for tenure, firm size, and age. Columns (3) and (4) are the corresponding regressions for violent crime. Columns (1)–(4) are cross-sectional regressions in 1989, where $Future\, Displaced\, Worker = 1$ if the individual will be displaced in any year in 1990-1994. Columns (5)–(8) are regressions pooling all observations in 1985-1989, with a similarly defined right hand side indicator variable. Columns

(5)-(8) add year dummies to control for differences in the crime rates across 1985-1989. Overall the table suggests that future displaced workers are not more likely than non-future displaced workers to commit a crime in 1985-1989. The coefficients, ranging from 0.00 to 0.08 percentage points, are not statistically significant at 10%, regardless of the set of controls, year fixed effects, and municipality-level effects.

The set of crimes leading to a conviction are also similar for the population of displaced workers and for the overall population. Figure 2 presents the distribution of crime types for the overall sample (blue points) and for the sample of displaced workers. Such breakdown is performed for the overall 1985-2000 data set. D.U.I. offense types are very close for both displaced and overall workers. For property crime, crime types are ordered by percentage in a similar way among displaced workers and for all workers. Overall results suggest that displaced workers types of property crimes are similar to the overall population.[15]

3.3 Econometric Specification

The following baseline specification estimates the impact of displacement on the post-displacement probability of committing crime, controlling for individual-level, municipality-level, and year-level unobservables.

$$\begin{aligned} Crime_{it} \quad = \quad & \sum_{k=-5}^{+7} \delta_k \cdot \mathbf{1}(Displaced\, in\, year\, t-k) + Individual_i \\ & + Year_t + Municipality_{m(i,t)} + \mathbf{x}_{it}\beta + Constant + \varepsilon_{it} \end{aligned} \tag{1}$$

where i indexes the $N = 102,376$ individuals, and t indexes years running from 1985 to 2000. $Crime_{it} = 1$ if the individual i commits a crime in year t *and* the crime led to a conviction in the current year or any subsequent year $t' \geq t$. This is defined as in Section 3.1. Focusing on the timing of the offense, rather than the timing of the conviction or prison term is key to address the problem of crime affecting separation decisions during a mass-layoff event. Specifically, because we observe the day of the offense and the week of unemployment, we set $Crime_{it} = 1$ so that the day of the offense always follows the week of displacement.

[15] The breakdown of violent crime by subcategory for displaced workers is prevented by Statistics Denmark's confidentiality policy.

While $Crime_{it} = 1$ implies that an offense leading to conviction has been recorded, charges and convictions typically occur later. Table 2 provides, for individuals convicted, the average, mean, lower and upper quartiles of the duration in days between the day of the arrest or citation and the day of the conviction. On average the lag is less than a year (172 days) between the offense and the conviction. Table 2 displays a similar table for displaced, suggesting that the lag for displaced workers is slightly shorter, at 150 days. Combining this timeline of crime with the breakdown of crime by subcategory for displaced and overall (Figure 2) suggests that displaced workers have criminal histories post-displacement that are similar to the overall population.

In specification 1, the coefficient δ_k, for $k = 1, 2, ...7$, is the impact of displacement in previous year $t - k$ on the probability of committing crime in year t. The specification thus allows for the estimation of short-, medium-, and long-run impacts of displacement on crime, the estimation includes up to effects 7 years after displacement. The +0 to +6 year effects are identified on all individuals displaced in our sample, i.e. displaced between 1990 to 1994 inclusive, as our data set covers year up to 2000 inclusive. The coefficient δ_{+7}, 7 years after displacement, is identified on individuals displaced in 1990-1993. With the presence of a constant in the specification, one of the displacement coefficients is conventionally set to zero; and we choose to set the coefficient δ_{-1}, a year prior to displacement, to zero. Thus, as in Table 3, all estimated effects are relative to that crime rate in the year prior to displacement.

The coefficients for years prior to displacement, $\delta_{-5}, \ldots, \delta_{-2}$ are placebo coefficients; they test whether high-tenure workers had changes in their propensity to commit crime prior to the displacement event. Statistically significant negative coefficients would be a sign of reverse causality: for unobservable reasons, future displaced workers would experience an increase in their propensity to commit crime immediately prior to displacement, and such increase in the propensity to commit crime would be correlated with the probability of losing employment during a mass-layoff event. Statistically significant positive coefficients, on the other hand, would indicate a dip in crime rates in the year prior to displacement. Thus checking the absence of economic and statistical significance of the $\delta_{-5}, \ldots, \delta_{-2}$ is a test of the existence of time-varying unobservable confounders, or, in other words of the existence of dynamic selection into displacement. Another way to see such identification assumption is to use Wooldridge's (2010) insight that panel models such as 1 are identified under the assumption of strict exogeneity of the residuals : future and prior year-specific unobservables ε_{it}

that cause crime should not be correlated with future and prior displacement events. The inclusion of placebo coefficients δ_{-k} can range up to 5 years before displacement, as our data set goes back to 1985 inclusive and follows displacement events from 1990 inclusive.

In subsequent sections we also report cumulative effects: the expected number of years with at least one criminal event in any year $[0; +k]$ is the sum of the probabilities $\delta_0 + \delta_1 + \delta_2 + \cdots + \delta_k$. As a criminal event in year t is largely disjoint from a criminal event in year $t' \neq t$, such cumulative coefficient is also the probability of committing crime at least once in the k years following displacement.

Specification 1 controls for individual-level non–time-varying unobservables through the fixed effect $Individual_i$. Such unobservables cause crime and may be correlated with the probability of displacement. For instance, drug consumption is mentioned by Levitt (2004) as a driver of crime; and literature has presented results suggesting a causal impact of psychiatric disorders on job loss (Kessler & Frank 1997). Average drug use over the time period of analysis 1985-2000, could thus be an unobservable confounding factor that causes crime and that is correlated with job losses, leading to an upward bias on our estimates of displacement. The individual fixed effect controls for the non–time-varying part of the confounders, and the placebo dummies for the time-varying pre-trends in unobservables. Also, literature has shown that the propensity to commit violent acts can cause job separation (LeBlanc & Kelloway 2002, Grandey, Dickter & Sin 2004), and, separately, that individuals have predispositions to violence (Frisell, Lichtenstein & Långström 2011), potentially causing both job separation and reported violent crime offenses. $Individual_i$ fixed effects also capture individual predispositions to violence.

In our estimation, results suggest that $Individual_i$ is negatively correlated with age, education, and tenure in 1989 and is lower as well for married individuals. As the individual effect $Individual_i$ is positively correlated with the probability of displacement ($+0.010{*}{*}{*}$), this suggests that $Individual_i$ captures the selection into displacement of crime-prone individuals. The variance of individual effects is only about 9.6% of the total variance of the crime dependent variable ($0.061ppt/0.636ppt$), suggesting that dynamic selection into displacement, i.e selection driven by time-varying unobservables is a substantial concern that will be tested by the placebo coefficients $\delta_{-5}, \ldots, \delta_{-2}$.

$Year_t$, for $t = 1985, \ldots, 2000$ are a set of year indicator variables that control for national trends in the crime rate. Including such year indicator variables is key: such national trend may, in particular, be correlated with the displacement rate and may confound our estimates of the impact of displacement on crime. Results suggest that year effects are not statistically significant until 1995, and capture a declining crime rate from 1995 till 2000. Results without year dummies suggest that not including such national controls tends to bias estimated effects upwards, as the spike in displacement rates (Figure 3) corresponds to the early part of the sample where crime rates were higher than in the later part of the 1990s.

$Municipality_{m(i,t)}$ is a municipality fixed effect, for each of the 270 municipalities,[16] where $m(i,t) = 1, 2, \ldots, 270$ is the municipality of individual i in year t. Municipality fixed effects control for the existence of municipality-level confounders such as spatial differences in police force numbers that may be correlated with the occurrence of mass-layoffs; changes in victims' reporting behavior at the municipality level; changes in the availability of criminal opportunities that may be correlated with municipality-level displacement rates. The identification of both individual fixed effects $Individual_i$ and municipality fixed effects $Municipality_m$ is possible given the substantial amount of individual mobility across municipalities in 1985-2000.

Finally, residuals ε_{it} are clustered at the individual level. Results accounting for individual-level autocorrelation of errors yield similar results and are available from the authors.

Several alternative identification strategies and alternative econometric specifications have been used in the displacement literatures. Dehejia & Wahba (2002) displays an example where propensity score matching can be as a good as a randomized experiment and Heckman, Ichimura & Todd (1997) introduces the Differenced Average Treatment on the Treated strategy. We can apply such strategy by applying propensity score matching to year-to-year changes in criminal activity $Crime_{it-1} - Crime_{it}$ across displaced and non-displaced individuals in the current year; and by matching on observables: year of displacement, age, tenure, average income pre-displacement, birth year, education, and marital status. Such matching yields estimates of the impact of displacement on crime ranging between 0.3 percentage points and 0.7 percentage points depending on the matching observables, and are consistent with the magnitude, timing, and longevity of our main baseline results.

Another approach to the estimation of the impact of displacement on crime is to make use a logit

[16] Municipalities were consolidated into 98 larger municipalities on January 1, 2007, a phenomenon studied by Amore & Bennedsen (2013). In this paper we use consistent pre-2007 municipality definitions, which provides a more granular geographic division of the country.

regression model with individual fixed effects instead of a linear probability model with fixed effects. The logit approach is particularly appropriate when one is interested in predicting the probability of crime, as the estimation approach ensures that probabilities lie in $(0, 1)$. However literature has shown that logit models with individual fixed effects tend to suffer from a lack of consistency of the estimators due to an incidental parameter problem (Lancaster 2000). The logit approach models the probability of crime as $P(Crime_{it} = 1) = \Lambda(\sum_{k=-5}^{+7} d_k \cdot \mathbf{1}(Displaced\, in\, year\, t-k) + controls)$. The estimated marginal impacts $\Lambda(d_k + controls) - \Lambda(controls)$ of displacement in year t on crime k years later are very similar to the estimated impacts δ_k the main specification 1.

3.4 Baseline Results

The results of the estimation of specification 1 are described in Table 7. All regressions of this table include year, municipal, and individual fixed effects. Column (1) presents the impact of displacement on total crime, i.e. $Crime_{it} = 1$ for any crime type. Columns (2)-(3) are for property crime, Columns (4)-(5) for violent crime, and Column (6) for driving under influence (DUI) offenses. Columns (1), (2), (4), (6) are the annual impacts δ_k while columns (3) and (5) are cumulative impacts measuring the impact of displacement on the probability of committing crime in any of k years post-displacement. All specifications feature the $102,376$ individuals over 16 years of the balanced sample of Danish workers described in Section 3.2. Point estimates are probability increases relative to the year prior to displacement, so that 0.005 corresponds to a 0.5 percentage point increase in the probability of committing crime.

The table suggests statistically significant impacts of job displacement on the probability of committing a crime leading to a conviction. The probability for all crimes increases by 0.52 percentage points in the year post-displacement, and a 0.5 percentage point increase the year following displacement. The effect represents about $0.5/1.91 = 26\%$ of the average probability of a conviction in the overall population (see Table 1, panel (iv)). Such increase in the probability of committing crime is almost entirely driven by the increase in the probability of committing a property crime. The impact of displacement on crime in the year of displacement is 0.38 percentage points (column (2)), with no discernible impact on the probability of committing a violent crime (Column (4)), and a nonsignificant impact on the probability of a DUI crime (+0.32 percentage points). Figure 2 suggests that for displaced workers these property crimes are mostly theft (62.3%), fraud (9.6%), forgery (5.5%), and vandalism (5.5%). Together these four subcategories represent 82.9% of all property crimes committed by displaced workers.

Results suggest that the impact of displacement on property crime last beyond the first year: the impact in the year following displacement is +0.36 percentage points, two years after +0.22. Seven years after the displacement event, the probability of committing property crime is still +0.42 percentage points higher than in the year prior to displacement. Figure 4 presents a graphical depiction of the pre- and post-displacement coefficients of Table 7. While the graph suggests a spike in DUI crimes as well, the curve of total crime and the curve of property crime follow very close trends and levels two years after displacement.

Overall, the average crime rate of future displaced workers in the years prior to displacement is 1.33% for total crime, and 0.13% for property crime; as our sample focuses on individuals with high tenure, these rates are lower than the average for all male workers from the same cohorts, of 1.91% and 0.65% for total crime and property crime respectively. Displacement brings high-tenure workers' crime rate substantially closer to the average: 7 years after displacement, the crime rates of displaced workers are 1.49% and 0.48% respectively.

Coefficients from Year -5 to Year -2 are not statistically significant at 10% in any of the four specifications of Table 7. More importantly perhaps is the lack of a discernible trend in point estimates for property crime (-0.0005 in year -5, +0.0002 in year -4, -0.0001 in year -2) and for violent crime (-0.0003 in years -5 to -2). These suggest that individuals who will be displaced are not experiencing any systematic trends in their propensity to commit crime prior to displacement. Such lack of evidence of pre-displacement selection effect should alleviate concerns of dynamic selection and possible reverse causality.

The impact of displacement on property crime, and the lack of impact of displacement on either violent or DUI crimes is consistent at the individual longitudinal-panel level with a substantial body of literature in the economics of crime, both theoretical and empirical with state-level data. Becker's (1968) theory of crime predicts that individuals compare the cost and benefit of crime, which has led to a vast literature on the impact of unemployment on property crime. Using state-level data, Raphael & Winter-Ebmer (2001) suggests that the decline in property crime can be attributed to the decline in the unemployment rate in the 1990s.[17] Our results, which focus on mass-layoffs,

[17]Other references showing an impact of unemployment on property crime include Corman & Mocan (2005), Lin

are also consistent with Mocan & Bali (2010), which finds that the impact of unemployment on crime is asymmetric: Figure 3 indeed suggests that the displacement varies asymmetrically with the unemployment rate: increases in the unemployment rate correspond to much more significant increases in the displacement rate than declines in the unemployment rate. A positive impact of displacement may explain why, then, the response of crime to increases in unemployment is greater than the response of crime to declines in unemployment.

Which individuals are driving our estimates?

Seminal work in the economics of crime considers the young, unskilled, and low-educated males as the main groups of interest (Freeman 1995, Grogger 1998, Gould et al. 2002). The sample of high-tenure displaced workers does include a substantial share of low-education workers: in the sample of displaced workers, 28% of individuals have not finished high school, and about 23.9% are categorized as unskilled workers in the Danish occupational categories. This is to be compared with 27.28% of individuals with less than high school education in the longitudinal sample built in Section 2 and described in Table 1.

Given the importance of educational attainment in determining an individual's criminal propensity (Lochner & Moretti 2004, Machin, Marie & Vujić 2011, Hjalmarsson, Holmlund & Lindquist 2015), all else equal, we would expect to see larger effects of displacement on crime for displaced individuals with lower levels of education. Hjalmarsson et al. (2015) uses a credibly exogenous identification strategy relying on changes in compulsory schooling laws to find that one additional year of schooling decreases the likelihood of conviction by 6.7% and incarceration by 15.5%.

While the focus of this paper is to get at an estimate of the causal impact of job displacement on crime rather than estimating the causal impact of education on crime, we can split the sample of displaced individuals to observe which education levels are driving the main results presented in Table 7.

Denmark has two main educational tracks: a general track and a vocational track. In the general track, individuals pursue higher education degrees, while in the vocational track individuals attend schools which are usually combined with apprenticeships. The majority of individuals in the vocational track do not pursue higher education. In the 1945-1960 cohorts that are the focus of this

(2008).

paper, individuals were required to stay in education until grade 7. In 1975, compulsory education was increased from 7 years to 9 years, the minimum level of education required to pursue additional education in either track. Due to this requirement, most students obtained 9 years of education prior to the reform taking place (Arendt 2005). There are thus three natural categories for splitting the sample by the male individual's education in 1989: (i) individuals whose highest educational credentials are vocational, (ii) individuals whose highest degree is from the higher education track, and (iii) individuals who have either finished high school but not followed up with the higher-education track, and individuals who have not completed high school. In Denmark, in contrast to the United States, few individuals leave school at the moment of high school completion. Individuals who complete high school typically move on to university education: while 27.23% of individuals in the longitudinal sample have completed less than high school (Table 1), 4.2% have completed high school exactly, 44.33% have completed a vocational degree, and 22.75% have completed a university degree or more.[18]

Figure D of the Appendix plots displacement rates by three broad categories of individual education. The top line (red) is the displacement rate for individuals with vocational education. The middle line (black) is the displacement rate for individuals who have completed high school or less. The bottom line (green) is the displacement rate for individuals who have completed university or more. The displacement rate for workers with vocational education is more than double the displacement rate for workers with high school or less than high school (1.3% displacement at the peak vs. 0.6% displacement rate for high school or less).

Table 8 presents the results of the estimation of the main specification 1, where observations are grouped by educational qualification in 1989. Thus such specification estimates the impact of displacement on crime separately for individuals with high school or less, individuals who have completed a vocational education, and individuals with university education or more. Results where the displacement dummies are interacted with the education dummies, rather than results obtained by splitting the sample, are similar and available from the authors.

Table 8 suggests that this paper's main results are mostly driven by individuals who have completed high school or less. Indeed, for these individuals, the probability of property crime increases by 0.97 percentage points in the year of displacement, more than twice number for the overall sample

[18]The education variable presents only 14,830 missing observations out of 1.6 million.

of individuals with any education level (+0.38 in Table 7). The effect of displacement is also long lasting for these high-school or less education individuals, remaining above 0.36 percentage points until 7 years after displacement, except for the year +5 effect. Results for other crime categories are consistent with the results with the overall sample results: evidence suggests job displacement impacts property crime rather than the other crime categories.

Interestingly, while displacement rates are highest for individuals with vocational education, the impact of job displacement on crime is non-significant for them. Individuals with vocational education, while experiencing higher rates of job separation during mass-layoff events of the early 1990s, experience substantially lower rates of unemployment: Jørgensen (2014) reports that over the 1994 to 2008 period, vocational-education individuals had the lowest unemployment rate across our three education categories; and that the unemployment rate rose marginally above 5% in only two years out of 15.

In the same Table 8, column (3) suggests that the probability of driving under influence crimes drops in the two years following displacement for individuals with a university degree or more. The longitudinal employer-employee data does not include a variable for the ownership of a car, but data from the European Automobile Manufacturers' Association suggests a strong negative correlation between unemployment and car sales: a one percentage point increase in the unemployment rate leads to a 5% decline in car sales. Such suggestive evidence may imply that the decline in the probability of DUI offenses is partly driven by the decline in car use for individuals with higher education or more.

Convictions, Prison Terms, and Earnings Losses

As in the job displacement and earnings losses' literature, we observe substantial and long-lasting negative impacts of displacement on wage income. Column (1) of Appendix Table C suggests that individuals' earnings fall by about 69,296 Danish Kroner in the year following displacement, or 41% of a standard deviation of annual earnings. The impacts are statistically significant at 1%, maximum one year after displacement (117,268 Danish Kroner, or 69% of a standard deviation) and long-lasting: after seven years, the annual earnings losses are about 50,969 Danish Kroner, or 30% of a standard deviation.

The conviction and incarceration data allows this paper to shed some light on the impact of convictions and incarceration policies on such earnings losses. In the United States, recent work by the Council of Economic Advisers (2016) suggests that the high rates and lengths of incarceration may have large opportunity costs; indeed, the same paper suggests that investments in policies that improve labor market opportunities are likely more beneficial than policies that lead to greater incarceration rates and lengths.

While Denmark has lower incarceration rates and shorter prison sentences than the United States, incarceration is a fairly common occurrence: in our longitudinal sample of male individuals (displaced or non-displaced, described in Table 1), 26.29% of convicted individuals are convicted to a prison term. In addition, regression results displayed in Figure C suggest that individuals convicted to a prison term experience substantially higher earnings losses than individuals convicted with another outcome than a prison term (fine, settlement, and other outcomes described in the data section 2).[19] Part of these higher earnings losses are mechanical: individuals in prison do not enjoy labor market opportunities comparable to individuals outside of the prison system. We thus need to isolate mechanical and non-mechanical impacts of incarceration on earnings losses.

In a next step, we thus estimate a predicted mechanical impact of prison terms on annual earnings losses. Such predicted mechanical impact on earnings is then compared to the observed earnings losses. Assuming no earnings during prison terms, the expected mechanical earnings loss is thus $(1 + P(prison\, in\, year\, t_0 + k)) \cdot E(earnings\, loss\, in\, t_0 + k,\, no\, prison\, conviction)$, where t_0 is the year of displacement, k the number of years after displacement, and $P(prison\, in\, year\, t_0 + k)$ the fraction of the year spent in prison, for individuals convicted to a prison. Actual sentence served can be lower than the sentence length specified in the convictions file.

The comparison of the actual earnings losses for individuals convicted to prison and the predicted earnings losses as given by prison terms is depicted in Figure D. The top line (green) represents earnings losses for displaced individuals who are convicted, with an outcome different from a prison term. The middle line (blue) represents the earnings losses for displaced individuals convicted to prison, as predicted by the median number of days spent in prison. For instance, in year four, the median individual convicted to prison spent 14 days in prison. The bottom line (red) represents earnings losses for individuals convicted to prison. Four years onwards from the displacement event,

[19]Figure C depicts the coefficients of a post-displacement earnings losses regression performed as in Jacobson et al. (1993). The regressions are performed separately for individuals not convicted post-displacement, for individuals convicted post-displacement but with no associated prison term, and for individuals convicted to prison post-displacement.

earnings losses of individuals convicted to prison are at least 14,000 DKK below the predicted earnings losses, suggesting that prison terms are either correlated with unobservable traits that cause lower earnings; or that prison terms have a direct impact over and above the impact predicted by the inability of the individual to be in employment.

4 Family Dynamics

This section matches the displaced individual to his family members to estimate (i) the correlation between family structure and displacement impacts on crime, (ii) the impact of job displacement on marital dissolution, and (iii) the impact of adult displacement on younger family members' crime.

Family Structure and the Impact of Displacement on Crime

Table 9 splits the sample by family structure in 1989. Column (1) considers individuals with at least one child, column (2) individuals with no child, (3) are single families, and (4) have two adults or more, which thus captures married couples, civil partnerships,[20] and cohabiting couples. Results of Table 9 suggests that the effects estimated in our main results (specification 1 and Table 7) are mostly driven by male individuals who are the only adult (born 1945-1960) in the family, and by male individuals with no child.

Family structure is correlated with other dimensions such as education and income: individuals with higher education or higher income are more likely to be married or in a civil partnership. Thus results by family structure reflect both its impact per se and the impact of family income and individual education. We re-estimate main specification 1 by splitting the sample according to whether family income is (i) at more than one standard deviation of family income below the mean, (ii) less than one standard deviation away from the mean, (iii) at more than one standard deviation of family income above the mean.

Results are such that the impacts are large and significant (+0.9 ppt) only for individuals in the low earnings group of family income below the mean minus one standard deviation. Thus we cannot rule out that family structure has an effect over and above what family income predicts. Indeed, table 9 suggests that the impact of job displacement on crime is about +0.97 ppt for single male

[20] Civil unions were introduced in Denmark in October 1989, i.e. before the first displacement events in our sample.

individuals with no child. However, only 59.8% of single male individuals are in such a low-earnings group. Similarly, male individuals with no child see an effect of about 0.53 ppt (column (2) of Table 9), and only about a third (33.9%) of such individuals are in the low-earnings group.

The way family structure affects the impact of displacement on crime is not likely operating through changes in spousal labor supply in response to the male adult's displacement. The loss of earnings following displacement is similar when estimated as in Jacobson et al. (1993) at the individual level, and when estimated at the family level: Table C suggests that the earnings loss in the year of displacement is 69,296 Danish Kroner for the individual regression, and 70,949 DKK in the year of displacement for the family regression (and even closer 69,906 DKK when considering the family earnings for families with at least two individuals born 1945-1960). The male individual's share of income goes from 65.6% of family income pre-displacement to 36.6% in the year right after displacement, and to 46.6% on average over the 7 years post displacement. Such decline in the share of family income is in line with the simple calculation of the impact of earnings losses on family income share. Results thus suggest no specific behavioral response of spouses to the displacement shock.

Displaced Males and Family Dissolution

Job displacement may have an ambiguous impact on the propensity to be married. In a Becker, Landes & Michael (1977) type model, post-displacement earnings losses negatively affect both the value of the outside option for a spouse and the value of staying in the relationship. Using the Panel Study of Income Dynamics, Charles & Stephens Jr. (2004) finds a greater probability of divorce in the year following job displacement. The focus of this paper is on how marital status interacts with displacement to increase the likelihood of crime. We use the multiple years post-displacement to estimate here how the probability of divorce increases, in the short-, medium-, and long-run.

Marriage, civil partnerships, and cohabitation are correlated with lower crime and lower displacement impacts on crime, but column (5) of Table 9 suggests that male individuals are less likely to be married post-displacement than pre-displacement, with a large long-run negative impact of 3.5 percentage points on the probability of being married, 7 years after displacement. And separation is also more likely in non-married families post-displacement, with on average a 1.8 percentage point probability of separation, as adult family size declines by 0.0178 individuals 7 years after displacement. For individuals that remain married, the fact that spousal income remains stable post displacement suggests that spouses practice some degree of pooling that smooths the displaced individual's consumption along the displacement event (Lundberg, Pollak & Wales 1997).

In a next step, we investigate whether the *dynamic* impact of displacement on marital status explains part of the impact of displacement on crime. To do so, we reestimate the baseline regression of Table 7 for the overall sample of displaced workers, including a control for the individual's *current* marital status. While the impact of displacement on crime is larger for individuals who were not married in 1989, the impact of displacement on crime is not significantly affected by the inclusion of the control for the current marital status (results available from the authors).

The Impact of Children's Exposure to Displaced Adults

Literature has suggested that parental job loss may cause increases in the criminal activity of children. For instance, using municipality-level data Öster & Agell (2007) suggests that prime-aged unemployment is robustly correlated with the main categories of youthful crimes. Municipality-level data has the advantage of capturing the impact of the unemployment rate on the aggregate crime rate, including spillovers from fathers to sons. In this paper, we can use the merged longitudinal panel of high-tenure individuals to estimate intergenerational spillovers by matching the displacement event of members of the family born in earlier cohorts to the criminal activity of the later cohorts of the same family.

Multiple channels explain why parental job displacement may cause crime in younger cohorts. Hjalmarsson & Lindquist (2012) provides evidence of the intergenerational correlation of crime. Another channel is evidenced by Oreopoulos, Page & Stevens (2008), who finds that children with displaced fathers have lower annual earnings – about 9% lower than similar children whose fathers did not experience an employment shock. Lower earnings could then potentially translate into higher crime. Finally, Stevens & Schaller (2011) finds that parental job loss leads to lower educational achievement of children; as education is a significant predictor of criminal activity, this is a potential mechanism linking parental displacement to children's crime.

This paper's longitudinal employer-employee-crime data with displacement events provides an arguably exogenous source of parental job displacement to estimate whether parental displacement causes younger household members' crime. As mentioned in Section 2 the family is a subset of a household (individuals living in a given housing unit), which comprises adults in marriage or civil partnership and their children. A first step is to then re-estimate main specification 1 by simply replacing the crime left-hand side outcome variable by the crime of younger family members, i.e. male family members who belong to the 1961 or later birth cohorts. Table 10 considers two ways of tying post-1961 cohort male family members to displaced adults: (i) either, in columns (1)-(2), by tying post-1961 individuals to their current family (measured annually). In such a case, the regression estimates the effect of parental displacement on *current* younger family members' crime. (ii) In contrast, columns (3)-(4) estimate the impact of adults' displacement on younger cohorts even after they have left the family of the displaced individual. The family ties are measured in 1989 and the younger household member is followed through 1990-2000. The tables report coefficients up to four years after the displacement event as the precision of estimates declines in years 5-7.

The results suggest some small impact for property or total crime one year after displacement. In columns (1)-(2) the effects are economically significant (+0.2 ppt for property crime and +0.17 ppt for total crime) but not statistically significant at 10%; in columns (3)-(4), which capture the impact up to 10 years after leaving the family, the effect one year after the adult's displacement is +0.3 ppt and +0.46; which are both economically and statistically significant impacts. In columns (3)-(4) the point estimates remain substantial until 3 years after displacement, at 0.2 percentage points 3 years after the male adult's displacement.

Several robustness checks are in line with the results of Table 10. Considering the sample of single adult households, or the sample of adults born 1951-1960 yields similar impacts. Oreopoulos et al. (2008) found a 9% earnings loss for children whose parents were displaced. Such earnings loss for children is $9/69 \simeq 1/12$ of the earnings losses for the displaced adult. A back-of-the-envelope calculation that assumes that the earnings losses are the sole channel suggests that the impact of children on crime should then be relatively small.

5 Local Income Distribution and Property Crime

Recent literature has shown substantial interest in the impact of local inequalities on economic behavior. In particular, Bertrand & Morse's (2013) state-year analysis suggests that, as top incomes rise, consumption increases at the bottom. Such results are consistent with a variety of mechanisms, including the dependence of individual marginal consumption utility on neighbors' consumption, a theoretical mechanism described at least since Veblen (2007) and Duesenberry et al. (1949).

In the U.S. income inequality is positively associated with violent and property crime in recent decades (Kelly 2000, Choe 2008).[21] Similarly,Fajnzylber, Lederman & Loayza (2002) uses a 1965-1994 panel of countries to find a robust positive correlation between income inequality and property crime within and across countries. Recent work on income inequality highlights the difference between poverty measures and income inequality measures, and Kelly (2000) suggests that poverty concentration is a better predictor of violent crime than inequality.

We combine our micro-panel identification strategy using displaced workers during mass-layoff events with local measures of income inequality and poverty concentration for each of our 270 municipalities.[22] In particular, high levels of income inequality or poverty concentration could enable an individual's criminal activity, either because of greater private incentives, a larger criminal peer network (Ballester, Zenou & Calvó-Armengol 2010), or because job loss triggers greater anger and anxiety in more unequal or poorer neighborhoods (Aseltine Jr, Gore & Gordon 2000). Displaced individuals experience relative earnings losses as they fall in the distribution of percentiles of income: on average, estimates suggest that a displaced individual drops 2.8 percentile points in the *current* municipality income distribution in the first year, 5.93 percentiles in the second year. In the long-run, the individual loses about 3.3 percentile points in the current distribution 7 years after displacement.

The income inequality measure is constructed as follows. Danish authorities report the current municipality of residence of each individual as well as his total (wage and capital) income, aggregated up to the family level. Following following Gastwirth (1972), annual income inequality across families for each municipality is the ratio of the absolute differences in income across households to total municipal income:

$$Gini_{m,t} = \frac{\sum_{i=1}^{N_m} \sum_{i'=1}^{N_m} |Income_{i,m} - Income_{i',m}|}{2N_m \sum_{i=1}^{N_m} Income_{i,m}} \tag{2}$$

where $Income_{i,m}$ is the total income of family $i = 1, 2, \ldots, N_m$ in municipality m in year t.[23] Total personal income is the sum of wages, transfers, property income, and other income sources attributed

[21] Such robust correlations are obtained when analyzing the FBI's Uniform Crime Report data. Brush (2007) finds, in the longitudinal panel dimension, a negative correlation between changes in income inequality and changes in the crime using two waves of the U.S. Census Office's County and City Data Books.

[22] As described in Section 3, consistent pre-2007 municipality definitions are used.

[23] At *http://www.dst.dk/da/Statistik/dokumentation/Times/personindkomst/perindkialt.*

to the individual before taxes. The Gini measure is relatively robust to outlier observations and differences in population numbers N_m compared to a typical alternative measure of inequality, the ratio of the 90th and the 10th percentile.

The mean Gini (0.361) is consistent with Piketty's (2014) estimate for Scandinavian countries. While Denmark has had low levels of individual income inequality relative to the median country in the OECD Income Distribution Database (*In It Together: Why Less Inequality Benefits All* 2015), the variance of income inequality across municipalities is significant: municipalities with the five highest Gini coefficients have Gini levels above 0.4, and municipalities with the five lowest Ginis have Ginis below 0.31. The ratio of the 90th percentile to the 10th percentile of income, calculated at the municipal level, also experiences variance across municipalities: the municipality at the 25th percentile has a P90/P10 of 7.31, and the municipality at the 75th percentile has a P90/P10 of 7.8.

The poverty concentration measure is derived as follows. Family income after taxes and transfers is equalized across families of different sizes using the modified OECD equivalence scale.[24] Using these equalized measures of family disposable income, we construct a national poverty line for each year from 1985-2000 for Denmark, which is defined as 50% of the national median equivalent family income. Poverty concentration in a municipality is the fraction of families below this national line per municipality and year.

The impact of job displacement on crime depends on local income distribution in the following specification:

$$\begin{aligned} Crime_{it} \quad = \quad & \sum_{k=-5}^{+7} \mathbf{1}(Displaced\, in\, year\, t-k) \cdot \left(\delta_k + \gamma_k \cdot \left(IncomeDist_{m(i,t),t} - \overline{IncomeDist}\right)\right) + Individual_i \\ & + Year_t + Municipality_{m(i,t)} + \xi \cdot IncomeDist_{m(i,t),t} + \mathbf{x}_{it}\beta + Constant + \varepsilon_{it} \quad (3) \end{aligned}$$

which interacts the main specification's displacement timeline indicator variables with one of the two demeaned income distribution measure: either the $Gini_{m,t}$, where $\overline{Gini}$ is the population weighted average municipality-level Gini; or similarly $Poverty_{m,t}$ with $\overline{Poverty}$ defined similarly. $m(i,t)$ is the municipality of individual i in year t. The rest of notations is as in section 3. For instance, if the Gini of municipality m is at the upper quartile $P75$, the impact of displacement on crime

[24] This modified OECD equivalence scale, adopted by Eurostat, scales household income by 0.5 for each additional adult beyond 1 and 0.3 for each child. Results are robust to other common equivalence scales.

in the year of displacement is $\delta_0 + \gamma_0 \cdot (P75(Gini) - \overline{Gini})$. For a municipality with the average Gini or poverty level, the impact in year k is simply δ_k. In the specification, the municipality effect and the time-varying income distribution control for the impact of income inequalities on the individual's propensity to commit crime. As before, the regression captures individual non–time-varying unobservables through the individual effect $Individual_i$. As income distribution is measured at the municipality-year level, standard errors are clustered at both the municipality and the year levels, yielding conservative estimates of the standard errors.

Identification of specification 3 presents at least two challenges. First, income distribution may be correlated with municipality-level unobservables $E(\varepsilon_{it}|m,t)$ that cause crime. Second, as income distribution is time-varying in the specification, job displacement may cause individual mobility either to municipalities where income distribution is higher or lower, e.g. where either the demand or the supply of crime is high. The first confounding factor can be alleviated by considering how changes rather than levels in the income distribution affect criminal activity. The second confounding factor can be alleviated by considering whether using current or the initial (prior to displacement) distribution of income affects econometric estimates.

To understand the impact of such biases on estimated heterogeneity, we first notice that the correlation between municipality-level Ginis and individual crime rates is low, at -0.01, and not significant at 10%. Second, we estimate the impact of job displacement on mobility across municipalities. Previous results already suggest some degree of mobility which is a source of identification for municipality and individual effects in the main specification. Results available on request suggest that an individual's probability of moving to another municipality (compared to the municipality one year prior to displacement) increases by 1.9 percentage points in the displacement year, and by 0.6 percentage points in the year following displacement. About half of the moves are within the same county as the probability of moving to another county increases by only 0.9 percentage points in the displacement and not significantly in following years. We thus reestimate specification 3 by fixing the municipality Gini associated to the individual at the 1989 Gini level, i.e. in the year before the first displacement events. Results are similar, suggesting little evidence that dynamic endogenous mobility confounds the estimates.

Results are presented in Table 11. The first column presents estimates of the coefficients of the base terms $\widehat{\delta_k}$ and the coefficients $\widehat{\gamma_k}$ of the interaction between the displacement indicator variables and the demeaned Gini. The impact of job displacement on property crime is $\widehat{\delta_0} = 0.0031$ in the municipality with the average Gini, consistent with the main baseline coefficient of 0.0038 obtained previously in the main specification. The impact of the Gini on job displacement effects is, significant at 5%, with the estimate of the interaction term $\widehat{\gamma_0} = 0.097$. Columns (2) and (3) translate such coefficients into the impact of job displacement on crime in the municipality with a Gini in the lower quartile of the Gini distribution (demeaned Gini of -0.011, column (2)) and in the municipality with a Gini in the upper quartile of the Gini distribution (demeaned Gini of $+0.012$, column (3)).[25]

While the impact of job displacement on crime in the year of displacement is 0.2 ppt and non-significant in the municipality with the lower quartile Gini, the effect is 0.43 ppt in the municipality in the upper quartile Gini. The results also suggest that the impact of the Gini is to increase the short run effects while lowering the longer term effects of displacement on crime: the interacted coefficient for the years +1, +2, and +3 after displacement are negative although non-significant, while the base coefficients are positive and significant. Overall, over the seven years after displacement, a regression with a 0–7 years post-displacement indicator variable (rather than annual indicator variables) indicates that a higher Gini is associated with stronger impacts of job displacement on crime. Column (4) presents the estimation of the specification excluding the county of Copenhagen. Indeed, the map of municipality Ginis depicted in Figure F suggests different income inequality patterns in the Copenhagen county. Results are similar to the specification with all municipalities. Finally, results using either poverty concentration or Gini measures are similar: in municipalities with 1 percentage point more families below the poverty line, displaced individuals experience a higher immeidate impact on crime, of about 0.16 percentage points.

6 Conclusion

This paper uses a unique merged longitudinal employer-employee crime data set for all male individuals in Denmark since 1985 to estimate the impact of arguably idiosyncratic job separations on the propensity to commit crime. Because job separations are endogenous, the paper considers a subset of high-tenure individuals, with strong attachment to their firm, who lose employment during a mass-layoff event, i.e. during a reduction in firm size of more than 30% compared to either the

[25] The standard errors are calculated using the variance and the covariance of the base and interacted terms, $var(\widehat{\delta_k} + \widehat{\gamma_k} \cdot (Gini_{m,t} - \overline{Gini})) = var(\widehat{\delta_k}) + var(\widehat{\gamma_k}) \cdot (Gini_{m,t} - \overline{Gini})^2 + 2 \cdot cov(\widehat{\delta_k}, \widehat{\gamma_k}) \cdot (Gini_{m,t} - \overline{Gini})$.

peak employment in 1985-1990, the average employment in the same period, or a 30% reduction in firm size compared to a firm-specific trend in employment. Such *displaced* individuals display no significant increasing trend in their propensity to commit crime prior to job displacement; and they also do not exhibit higher crime rates than individuals who will *not* be displaced prior to the first mass-layoff events.

Results suggest that job displacement has a significant impact on the propensity to commit crime: displaced workers' probability to commit a crime (resp. a property crime) increases by 0.52 percentage points (resp. 0.38 ppt) in the year of displacement. These are economically significant impacts, of about 26% of the annual probability of conviction. We observe that the impacts of displacement on crime are mostly driven by the impacts on property crime, in line with Becker's (1968) economic theory of the benefits and opportunity costs of crime: displaced workers, who had at least three years of tenure pre-displacement, experience a transition to shorter employment spells and lower earnings, consistent with the job displacement and earnings losses literature. Results are mostly driven by males with less than high school education.

Results are stronger when focusing on individuals who have no child or are single male individuals. For such workers, the impact of job displacement on crime is an increase of 0.9 percentage points. Job displacement also leads to a higher probability of marital dissolution. There is also suggestive evidence that displacement may impact younger family members one year after a corresponding adult's displacement event.

Using the availability of geographic identifiers for individuals' locations, together with the availability of total personal income (labor and capital income), we combine the job displacement identification strategy with matched annual measures of income inequalities, and find that displacement effects on crime are larger in more unequal municipalities.

Blanchard & Tirole's (2008) dynamic labor market model suggests that the optimality of job separations requires layoff taxes. This paper's results highlight the social costs of job separations, over and above earnings losses borne by the employee. Increases in crime following job separations likely affect children, spouses, crime victims, and the costs of policing beyond the sole employer-employee relationship.

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Figure 2: Breakdown of Crime by Type

This figure breaks down convictions by broad crime category (property crime, violent crime, and driving under influence DUI) and by crime subcategory. The number is the percentage of the crime category, e.g. in the overall sample (blue point) 60.5% of property crimes are theft. Minor violence: violence not resulting in injuries or death. Other violent crimes: aggregated due to Statistics Denmark confidentiality policy, composed of homicide and attempted homicide. Omitted because of such confidentiality policy: riot and disturbances, homicide, attempted homicide, very serious violence, and intentional bodily injury. The confidentiality policy also prevents a breakdown of violent crimes by subtype for displaced workers.

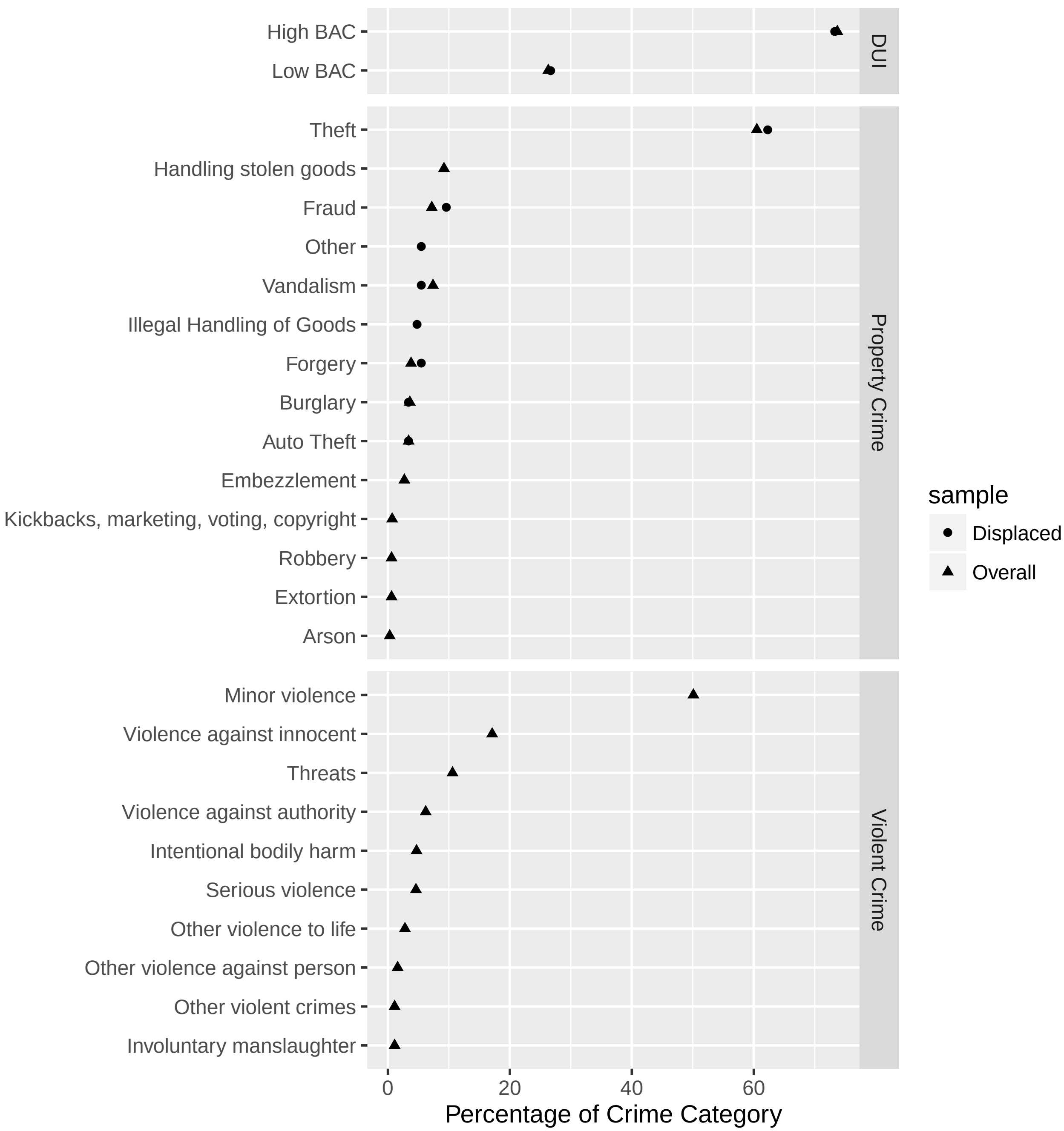
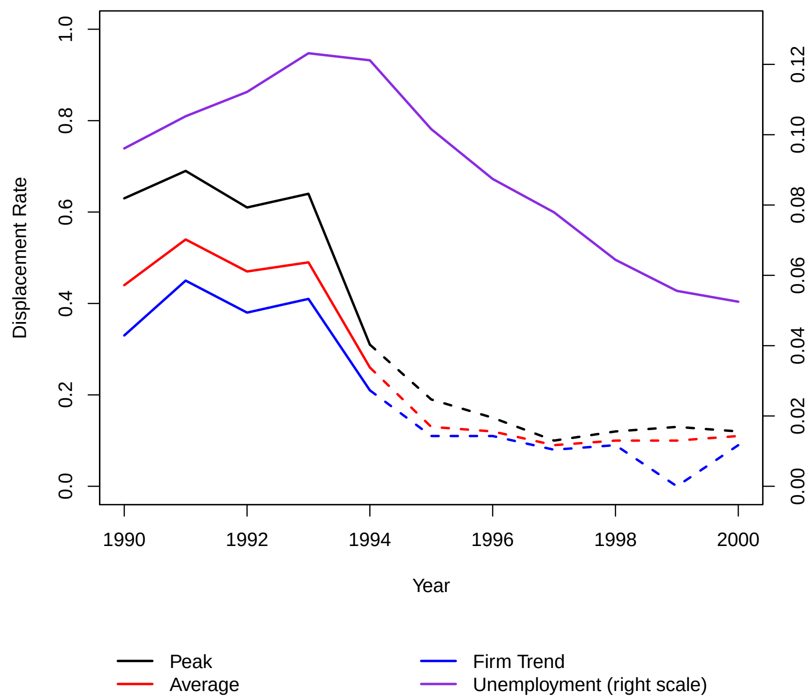


Figure 3: Displacement Rate Along the Business Cycle

This figure presents the annual displacement rate from 1990 to 2000, using the three different definitions of this paper. The paper's estimation results focus on workers displaced in 1990–1994. The displacement rate is the number of male displaced workers (in the current year) divided by the number of male workers. A worker is displaced if he is a high-tenure worker who loses employment during a mass-layoff event (see Section 3.2).



Solid line: displacement rates for the period of analysis; main specification 1 considers the impact of displacement in 1990-1994 on subsequent crime in 1990-2000. Displacement rates on the left-side vertical axis. The three bottom lines (black, red, blue) correspond to three different approaches to defining mass-layoff events. *The 'peak' definition (black line) uses the 1985-1989 firm size peak as the reference point for mass-layoffs. The 'average' definition (red line) uses the 1985-1989 firm size average as the reference point for mass-layoffs. The 'firm trend' definition (blue line) uses a firm-specific trend in employment as the reference point. The unemployment rate is the top curve, and corresponds to the right-side vertical axis.*

Figure 4: Impacts of Displacement on Crime

The figure displays the coefficients of the panel regression estimating the impact of job displacement on crime (Table 7). The specification is described in Section 3.3. Each line corresponds to the coefficients of a separate regression, with different crime types as dependent variable. Bold lines correspond to total and property crimes, for which short- and medium-run impacts of displacement on crime are statistically significant at 5 or 1%. Coefficients are in percentage points: in the year of displacement (year=0) job displacement increases the probability of property crime by 0.38 percentage points. The legend is in the order of the year=0 impact.

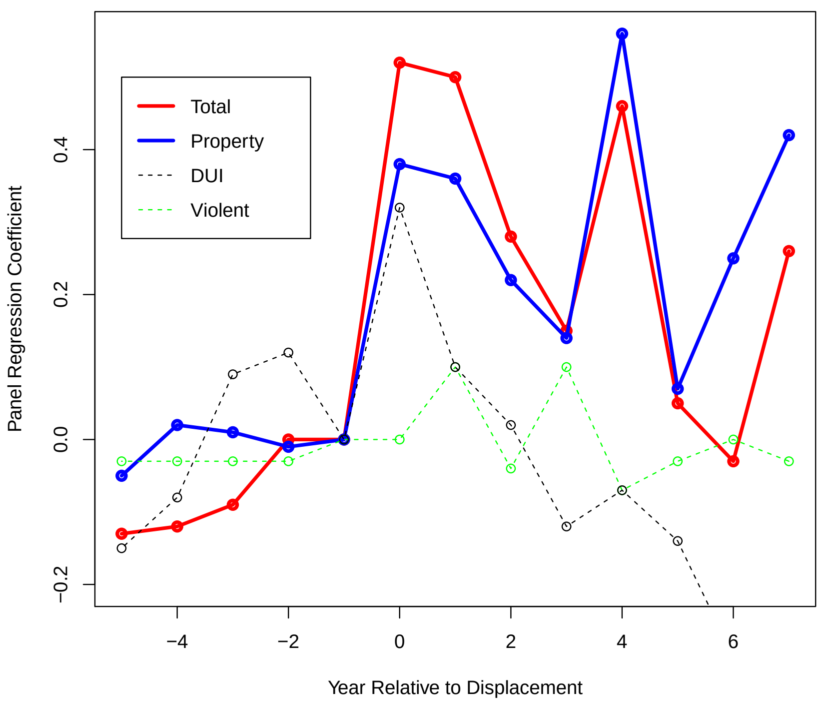


Table 1: The Employer-Employee Crime Data Set

This table summarizes observables from the five merged sources of data used in this paper: the employer-employee, the education data set, the household and demographics data set, the police and court records data, and the unemployment and social assistance files. The sample is Danish males born from 1945-1960 who are continuously in Denmark from 1985-2000. DKK: Danish Kroner. In order to comply with Statistics Denmark's data confidentiality criteria, the 25th percentile, the median, and the 75th percentile are calculated using the average observations of the 5 individuals surrounding each statistic.

(i) Employer-Employee

Variable	Mean	S.D.	P25	P50	P75	Observations
Annual Wage (2000 DKK)	238,170	169,906	141,047	247,029	317,177	8,830,448
Weeks Fully Unemployed	2.88	9.06	0	0	0	8,830,448
Firm size	4124.46	9860.5	20	183	2273	7,494,777

(ii) Demographics and Education

Variable	Mean	S.D.	P25	P50	P75	Observations
Age	39.23	6.56	35	39	44	8,830,448
Birth Year	1952.27	4.67	1948	1952	1956	8,830,448
Less than high school	27.23%	0.4452	1	0	0	8,830,448
High School	4.20%	0.2006	0	0	0	8,830,448
Vocational	44.33%	0.4968	1	0	0	8,830,448
University or beyond	22.75%	0.4192	0	0	0	8,830,448
Missing education	1.49%	0.121	0	0	0	8,830,448

(iii) Family Structure

Variable	Mean	S.D.	P25	P50	P75	Observations
Family income (2000 DKK)	484,396	451,135	323,507	461,747	588,389	8,830,448
Wage as fraction of HH Income	50.47%	29.97	36.11%	53.76%	67.10%	8,830,448
Family size	2.89	1.35	2	3	4	8,830,448
Adults in Family	1.89	0.62	2	2	2	8,830,448
Number of children	1.05	1.14	0	1	2	8,830,448

(iv) Police and Court Records

Variable	Mean	S.D.	P25	P50	P75	Observations
Probability of charge	2.27%	14.89%	0	0	0	8,830,448
Number of charges	1.66	3.34	1	1	1	200,391
Probability of conviction	1.91%	13.69%	0	0	0	8,830,448
Probability of conviction - Property	0.65%	8.06%	0	0	0	8,830,448
Probability of conviction - Violent	0.13%	3.67%	0	0	0	8,830,448
Probability of conviction - DUI	0.67%	8.14%	0	0	0	8,830,448
Number of convictions	2.26	5.89	1	1	2	168,517
Probability of conviction to Prison	26.29%	44.02%	0	0	0	168,517
Length of prison sentence (days)	2341.89	5844.60	14	30	240	44304

(v) Unemployment and Social Assistance

Variable	Mean	S.D.	P25	P50	P75	Observations
Weeks on social assistance	27.1	17.05	12	25	44	150,083
Weeks on UI benefits	16.77	15.02	4	12	26	1,271,574

Table 2: The Timeline from Offense, Charge, to Prison

The upper panel presents the distribution of the number of days from the date of the offense to the date of the charge(s). Multiple charges can correspond to one offense. The date of the offense is recorded at the time the charges are filed. In order to comply with Statistics Denmark's data confidentiality criteria, the 25th percentile, the median, and the 75th percentile are calculated using the average observations of the 5 individuals surrounding each statistic.

	Time from Offense to Charges (days)				
Sample	Mean	Median	P25	P75	Charges
At least 1 charge	59.6	0	0	22	3,729,636
Excluding speeding	78.1	1	0	44	2,759,322
Excluding zeros	149.1	42	10	136	1,488,564
	Time from Charges to Conviction (days)				
Sample	Mean	Median	P25	P75	Convictions
At least 1 conviction	111.9	70	37	143	1,882,930 (50.5%)[1]
Excluding speeding	136	94	43	180	1,172,128
Excluding zeros	116.5	74	40	148	1,808,722
	Time from Conviction to Prison (days)				
Sample	Mean	Median	P25	P75	Prison terms
At least 1 prison term	173	129	53	231	233,680 (12.4%)[2]
Excluding speeding	170.6	124	47	229	213,246
Excluding zeros	187.9	142	73	244	215,268

[1]: Percentage of charges leading to conviction.
[2]: Percentage of convictions leading to a prison term.

Table 3: Correlations Between Unemployment and Crime

This table presents the results of linear probability longitudinal panel regression of crime (defined in Section 3.1) on a dummy for the year of the first transition to unemployment. Columns (2) and (4) include an individual fixed effect.

	(1)	(2)	(3)	(4)
Dependent:	Total Crime		Property Crime	
Specification:	OLS	Fixed Effect	OLS	Fixed Effect
Year +7	0.0156***	0.0012***	0.0064***	0.0012***
	(0.0004)	(0.0004)	(0.0002)	(0.0002)
Year +6	0.0155***	0.0016***	0.0069***	0.0020***
	(0.0004)	(0.0004)	(0.0002)	(0.0002)
Year +5	0.0173***	0.0029***	0.0077***	0.0027***
	(0.0004)	(0.0004)	(0.0003)	(0.0003)
Year +4	0.0196***	0.0049***	0.0094***	0.0043***
	(0.0004)	(0.0004)	(0.0003)	(0.0003)
Year +3	0.0218***	0.0068***	0.0100***	0.0047***
	(0.0004)	(0.0005)	(0.0003)	(0.0003)
Year +2	0.0232***	0.0082***	0.0110***	0.0057***
	(0.0005)	(0.0005)	(0.0003)	(0.0003)
Year +1	0.0249***	0.0098***	0.0110***	0.0058***
	(0.0005)	(0.0005)	(0.0003)	(0.0003)
Unemployment Year	0.0303***	0.0153***	0.0127***	0.0074***
	(0.0005)	(0.0005)	(0.0003)	(0.0003)
Year −1	0.0300***	0.0150***	0.0108***	0.0056***
	(0.0005)	(0.0005)	(0.003)	(0.0003)
Year −2	0.0277***	0.0129***	0.0103***	0.0051***
	(0.0005)	(0.0005)	(0.0003)	(0.0003)
Year −3	0.0252***	0.0108***	0.0098***	0.0048***
	(0.0005)	(0.0005)	(0.0003)	(0.0003)
Year −4	0.0247***	0.0107***	0.0098***	0.0050***
	(0.0005)	(0.0005)	(0.0003)	(0.0003)
Year −5	0.0231***	0.0098***	0.0092***	0.0046***
	(0.0005)	(0.0005)	(0.0003)	(0.0003)
Individual Fixed Effect	No	Yes	No	Yes
R Squared	0.005	0.001	0.003	0.001
Observations	8,830,448	8,830,448	8,830,448	8,830,448
Clusters	551,903	551,903	551,903	551,903

***: Significant at 1%, **: Significant at 5%, *: Significant at 10%.

Table 4: Confounders of Unemployment and Crime

The table presents the correlation of a dummy for transition into unemployment (resp., a dummy for total crime) with individual observables that may confound a regression of crime on transition into unemployment. 'Less than high school', 'High School, 'Vocational education', 'University or Greater': highest level of education completed. Less than 0.01% of observations have missing education information.

	(1) Transition into Unemployment	(2) Total Crime
Less than High School	0.042***	0.070***
High School Education	−0.002***	−0.010***
Vocational Education	0.005***	−0.022***
University or Greater	−0.053***	−0.053***
Missing Education	0.011***	0.034***
Married	−0.069***	−0.073***
Lag of Tenure	−0.108***	−0.073***
Lag Firm Size	−0.043***	−0.012***
Age	−0.084***	−0.039***
Observations	8,830,448	

***: Significant at 1%, **: Significant at 5%, *: Significant at 10%.

Table 5: Pre- and Post-Mass-Layoff Firm Size

This table estimates the impact of mass-layoffs on firm size, and estimates whether there were significant firm size pre-trends prior to the mass-layoff event. A mass-layoff occurs if firm size is lower than 30 % of its 1985-1989 peak, following Jacobson et al. (1993). Alternative definitions of mass-layoffs are presented in Section 3.2.

	(1)	(2)	(3)	(4)
Dependent:	Number employees			
Sample:	All firms	All firms	Size < 1,000	Size < 1,000
Year +5	-18.329***	-17.975**	-15.619***	-15.522**
	(6.341)	(8.566)	(5.017)	(6.880)
Year +4	-18.342***	-20.641***	-15.535***	-17.613***
	(5.815)	(7.519)	(4.629)	(6.003)
Year +3	-18.560***	-22.527***	-15.626***	-19.007***
	(5.806)	(6.952)	(4.742)	(5.638)
Year +2	-18.331***	-24.498***	-15.278***	-20.643***
	(5.521)	(6.726)	(4.411)	(5.428)
Year +1	-17.889***	-25.990***	-14.891***	-21.810***
	(5.411)	(6.596)	(4.428)	(5.359)
Mass-Layoff Year	-17.469***	-27.439***	-14.354***	-22.667***
	(5.640)	(7.350)	(4.691)	(6.041)
Year -1	0.000	0.000	0.000	0.000
Year -2	5.374	-2.131	4.959	-0.945
	(11.355)	(14.157)	(8.761)	(10.828)
Year -3	7.913	-0.410	7.301	0.802
	(9.513)	(11.567)	(7.136)	(8.705)
Year -4	10.592	1.457	9.460	2.130
	(8.361)	(10.171)	(6.059)	(7.549)
Year -5	12.685*	2.471	11.572**	3.275
	(7.574)	(9.577)	(5.431)	(6.925)
Year Dummies	No	Yes	No	Yes
R Squared	0.008	0.013	0.008	0.013
Observations	573,860	573,860	569,971	569,971
Clustering	Firm x Year	Firm x Year	Firm x Year	Firm x Year
Year Clusters	16	16	16	16
F Statistic	24.631	4.867	27.933	5.331

***: Significant at 1%, **: Significant at 5%, *: Significant at 10%.

Table 6: Current Convictions of Future Displaced Workers

Future displaced worker: individual who will be displaced in any year between 1990 and 1995 inclusive. The table estimates the correlation between 1985-1989 criminal activity and the probability of displacement in the subsequent six years.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	1989				1985-1989			
Dependent:	Property		Violent		Property		Violent	
Future Displaced	0.0008	0.0007	0.0005	0.0004	0.0000	-0.0002	0.0003	0.0002
Worker	(0.0008)	(0.0008)	(0.0005)	(0.0005)	(0.0003)	(0.0003)	(0.0002)	(0.0002)
Year Dummies	-	-	-	-	Yes	Yes	Yes	Yes
Municipality Dummies	No	Yes	No	Yes	No	Yes	No	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R Squared	0.000	0.003	0.000	0.002	0.000	0.001	0.000	0.001
Observations	102,360	102,360	102,360	102,360	511,800	509,955	511,800	509,955
Number of Individuals	102,360	102,360	102,360	102,360	102,360	102,360	102,360	102,360
F	1,232	0.315	0.896	0.085	0.011	1.548	1.897	0.507

***: Significant at 1%, **: Significant at 5%, *: Significant at 10%.

Table 7: Impact of Displacement on Crime

This table presents the results of the main specification estimating the impact of displacement on crime, and estimating whether there are pre-displacement trends in criminal activity. The dependent variable is an indicator variable in columns 1–6. Annual coefficients are for crime committed in year k. Cumulative impacts are for crime committed at any point between the year of displacement (year=0) and year k.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent:	Total Crime	Property Crime		Violent Crime		DUI
Coefficient:	Annual	Annual	Cumulative	Annual	Cumulative	Annual
Year +7	0.0026	0.0042***	0.0030***	-0.0003	0.0000	-0.0025
	(0.0024)	(0.0015)	(0.0006)	(0.0004)	(0.0003)	(0.0016)
Year +6	-0.0003	0.0025**	0.0028***	0.0000	0.0001	-0.0030**
	(0.0021)	(0.0012)	(0.0006)	(0.0005)	(0.0003)	(0.0014)
Year +5	0.0005	0.0007	0.0029***	-0.0003	0.0001	-0.0014
	(0.0022)	(0.0010)	(0.0007)	(0.0004)	(0.0003)	(0.0016)
Year +4	0.0046*	0.0056***	0.0033***	-0.0007***	0.0002	-0.0007
	(0.0025)	(0.0016)	(0.0008)	(0.0002)	(0.0003)	(0.0017)
Year +3	0.0015	0.0014	0.0027***	0.0010	0.0004	-0.0012
	(0.0023)	(0.0011)	(0.0008)	(0.0008)	(0.0003)	(0.0016)
Year +2	0.0028	0.0022*	0.0032***	-0.0004	0.0002	0.0002
	(0.0024)	(0.0012)	(0.0009)	(0.0004)	(0.0004)	(0.0018)
Year +1	0.0050*	0.0036**	0.0037***	0.0010	0.0005	0.0010
	(0.0026)	(0.0014)	(0.0011)	(0.0008)	(0.0005)	(0.0019)
Displacement year	0.0052**	0.0038***	0.0038***	-0.0000	-0.0000	0.0032
	(0.0026)	(0.0014)	(0.0014)	(0.0004)	(0.0004)	(0.0021)
Year -1	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*
Year -2	-0.0000	-0.0001	-	-0.0003	-	0.0012
	(0.0022)	(0.0008)		(0.0004)		(0.0019)
Year -3	-0.0009	0.0001	-	-0.0003	-	0.0009
	(0.0022)	(0.0008)		(0.0004)		(0.0019)
Year -4	-0.0012	0.0002	-	-0.0003	-	-0.0008
	(0.0021)	(0.0008)		(0.0004)		(0.0018)
Year -5	-0.0013	-0.0005	-	-0.0003	-	-0.0015
	(0.0021)	(0.0006)		(0.0004)		(0.0017)
R Squared	0.097	0.089		0.074		0.084
Observations	1,638,016	1,638,016		1,638,016		1,638,016
Individuals	102,376	102,376		102,376		102,376
F Statistic	8.465	9.641		1.519		6.517

***: Significant at 1%, **: Significant at 5%, *: Significant at 10%.

Table 8: Education and the Impact of Displacement

Impacts of displacement on crime are estimated as in Table for an individual's pre-displacement education in 1989 for who completed high school or less (first three columns) and for individuals who completed a university degree or more (last three columns).

	(1)	(2)	(3)	(1)	(2)	(3)
Dependent:	Property Crime	Violent Crime	DUI Crime	Property Crime	Violent Crime	DUI Crime
Sample:	High School or Less			University or Greater		
Year +7	0.0051*	0.0004	-0.0015	0.0026	-0.0015	0.0036
	(0.0029)	(0.0014)	(0.0036)	(0.0032)	(0.0010)	(0.0046)
Year +6	0.0057*	0.0003	-0.0008	0.0020	-0.0014	-0.0027*
	(0.0030)	(0.0013)	(0.0035)	(0.0030)	(0.0010)	(0.0014)
Year +5	0.0018	0.0003	-0.0009	0.0020	-0.0015	0.0001
	(0.0022)	(0.0013)	(0.0036)	(0.0030)	(0.0010)	(0.0031)
Year +4	0.0051*	-0.0010*	0.0004	0.0075	-0.0015	0.0059
	(0.0029)	(0.0005)	(0.0036)	(0.0049)	(0.0010)	(0.0051)
Year +3	0.0036	-0.0010*	0.0000	0.0047	0.0013	0.0003
	(0.0028)	(0.0005)	(0.0037)	(0.0040)	(0.0030)	(0.0032)
Year +2	0.0057*	0.0003	0.0008	0.0018	-0.0015	0.0029
	(0.0032)	(0.0013)	(0.0039)	(0.0021)	(0.0010)	(0.0042)
Year +1	0.0070**	0.0039*	0.0043	-0.0009	0.0014	-0.0028**
	(0.0035)	(0.0023)	(0.0044)	(0.0007)	(0.0030)	(0.0014)
Displacement year	0.0097**	0.0002	0.0085*	-0.0008	-0.0014	-0.0028**
	(0.0039)	(0.0010)	(0.0049)	(0.0007)	(0.0010)	(0.0014)
Year -1	Ref	Ref	Ref	Ref	Ref	Ref
Year -2	0.0021	-0.0009*	0.0027	-0.0005	-0.0014	0.0028
	(0.0022)	(0.0005)	(0.0043)	(0.0007)	(0.0010)	(0.0042)
Year -3	0.0023	0.0003	0.0015	-0.0004	-0.0014	0.0001
	(0.0022)	(0.0013)	(0.0042)	(0.0007)	(0.0010)	(0.0024)
Year -4	0.0000	0.0003	-0.0043	-0.0004	-0.0014	0.0057
	(0.0014)	(0.0013)	(0.0032)	(0.0007)	(0.0010)	(0.0051)
Year -5	-0.0001	0.0003	0.0041	-0.0005	-0.0014	-0.0028**
	(0.0014)	(0.0013)	(0.0044)	(0.0007)	(0.0010)	(0.0014)
R Squared	0.092	0.078	0.089	0.081	0.068	0.077
Observations		377024			292256	
Individuals		23564			18266	
F	3.399	1.092	2.750	.	.	1.341

***: Significant at 1%, **: Significant at 5%, *: Significant at 10%.

Table 9: Job Displacement and Family Dynamics

This table estimates (i) the impact of displacement according to the individual's 1989 pre-displacement family structure (columns 1–4), and (ii) whether displacement affects family structure (columns 5 and 6). Estimation uses full set of pre- and post-displacement indicators.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent:	— Property Crime —				Married	Adult Family Size
Sample:	Children	No child	Single Family	2-Adults Or More	All Individuals	All Individuals
Year +7	0.0030*	0.0064**	0.0105**	0.0022	-0.0349***	-0.0178**
	(0.0016)	(0.0030)	(0.0042)	(0.0014)	(0.0057)	(0.0069)
Year +6	0.0024	0.0026	0.0090**	0.0004	-0.0281***	-0.0224***
	(0.0015)	(0.0021)	(0.0037)	(0.0011)	(0.0059)	(0.0068)
Year +5	-0.0003	0.0025	0.0031	-0.0001	-0.0289***	-0.0120*
	(0.0010)	(0.0021)	(0.0025)	(0.0010)	(0.0059)	(0.0070)
Year +4	0.0042**	0.0083***	0.0100**	0.0042**	-0.0270***	-0.0095
	(0.0017)	(0.0032)	(0.0040)	(0.0016)	(0.0060)	(0.0071)
Year +3	0.0009	0.0023	0.0027	0.0009	-0.0234***	-0.0158**
	(0.0013)	(0.0021)	(0.0025)	(0.0012)	(0.0059)	(0.0071)
Year +2	0.0018	0.0031	0.0038	0.0017	-0.0191***	-0.0067
	(0.0015)	(0.0023)	(0.0029)	(0.0014)	(0.0058)	(0.0071)
Year +1	0.0033*	0.0041*	0.0094**	0.0017	-0.0130**	-0.0050
	(0.0017)	(0.0025)	(0.0040)	(0.0014)	(0.0056)	(0.0067)
Displacement year	0.0030*	0.0053**	0.0097**	0.0019	-0.0091*	-0.0023
	(0.0017)	(0.0027)	(0.0040)	(0.0014)	(0.0054)	(0.0066)
R Squared	0.091	0.084	0.093	0.087	0.733	0.596
Observations	1,213,872	424,144	269,840	1,368,176	1,638,016	1,638,016
Clusters	75,867	26,509	16,865	85,511	102,376	102,376
F Statistic	9.807	2.091	3.256	7.600	363.975	38.260

***: Significant at 1%, **: Significant at 5%, *: Significant at 10%.

Table 10: The Impact of Job Displacement on Younger Family Members' Criminal Activity

This table presents results estimating the impact of male adults' displacement on younger male family members' criminal activity. Younger male family members are those family members born after 1961, and the displacement events occur, as in the main specification 1, for male individuals born in 1945-1960. In columns (1)-(2) family ties are the current (annual) family ties. In columns (3)-(4) family ties are the 1989 family ties. Estimation uses full set of pre- and post-displacement indicators.

	(1)	(2)	(3)	(4)
	Sons' Crime, Current Family		Sons' Crime, 1989 Family	
	Property	Total	Property	Total
Year +4	-0.0012	-0.0006	-0.0005	0.0001
	(0.0013)	(0.0017)	(0.0016)	(0.0021)
Year +3	-0.0003	-0.0009	0.0020	0.0019
	(0.0015)	(0.0018)	(0.0018)	(0.0022)
Year +2	0.0010	0.0008	0.0009	0.0022
	(0.0016)	(0.0019)	(0.0018)	(0.0023)
Year +1	0.0022	0.0017	0.0032*	0.0046**
	(0.0017)	(0.0019)	(0.0019)	(0.0023)
Displacement year	0.0005	-0.0011	0.0006	0.0014
	(0.0014)	(0.0016)	(0.0017)	(0.0021)
R Squared	0.120	0.135	0.200	0.178
Observations	1,638,016	1,638,016	1,638,016	1,638,016
Clusters	102,376	102,376	102,376	102,376
F Statistic	18.662	30.943	54.410	98.931

Table 11: Local Income Inequalities and Displacement Effects

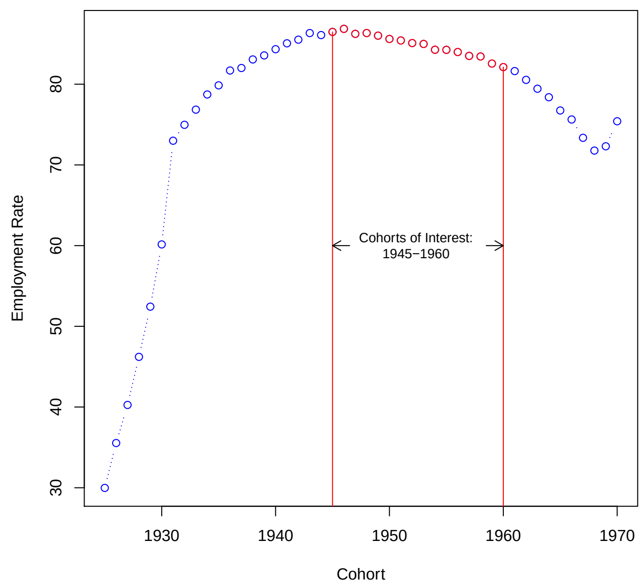
This regression is the main regression where post-displacement year indicator variables are interacted with the municipal Gini coefficient. Estimation uses full set of pre- and post-displacement indicators.

Dependent variable:	Property Crime					
Sample:	All individuals				Excl. Copenhagen and Frederiskberg	
Coefficients:	Annual	× Gini	Annual Effect for		Annual	× Gini
Gini:	-	-	P25	P75	-	
Year +7	0.0039***	0.0477	0.0034**	0.0045***	0.0037**	0.0413
	(0.0015)	(0.0531)	(0.0017)	(0.0015)	(0.0015)	(0.0554)
Year +6	0.0020*	0.0871	0.0010	0.0030**	0.0018	0.0837
	(0.0012)	(0.0806)	(0.0016)	(0.0014)	(0.0012)	(0.0843)
Year +5	0.0007	-0.0019	0.0007	0.0007	0.0006	-0.0041
	(0.0011)	(0.0547)	(0.0014)	(0.0010)	(0.0011)	(0.0557)
Year +4	0.0053***	0.0585	0.0047***	0.0060***	0.0041***	0.0280
	(0.0016)	(0.0679)	(0.0018)	(0.0017)	(0.0015)	(0.0668)
Year +3	0.0014	-0.0031	0.0014	0.0014	0.0008	-0.0216
	(0.0012)	(0.0384)	(0.0014)	(0.0011)	(0.0011)	(0.0379)
Year +2	0.0023*	-0.0060	0.0024	0.0022*	0.0025*	-0.0163
	(0.0013)	(0.0493)	(0.0016)	(0.0012)	(0.0014)	(0.0515)
Year +1	0.0037**	-0.0232	0.0040**	0.0034**	0.0030**	-0.0469
	(0.0015)	(0.0449)	(0.0018)	(0.0014)	(0.0015)	(0.0423)
Displacement year	0.0031**	0.0970**	0.0020	0.0043***	0.0025*	0.0906**
	(0.0014)	(0.0443)	(0.0014)	(0.0015)	(0.0013)	(0.0460)
Demeaned Gini	0.0044*				0.0048*	
	(0.0024)				(0.0025)	
R Squared	0.089				0.089	
Observations	1,638,016				1,526,573	
Individuals	102,376				98,057	
F Statistic	6,952				6.196	

***: Significant at 1%, **: Significant at 5%, *: Significant at 10%.

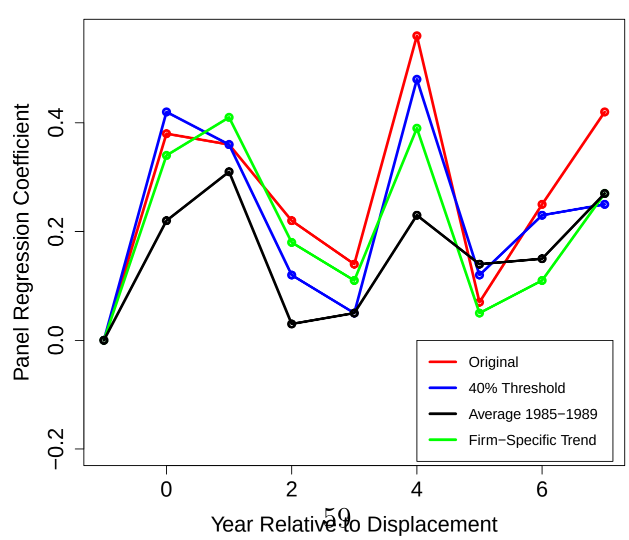
Appendix Figure A: Employment Rate for the 1925 to 1970 cohorts in 1990

Source: authors' own calculations from the employer-employee-unemployment data set described in Section 2.



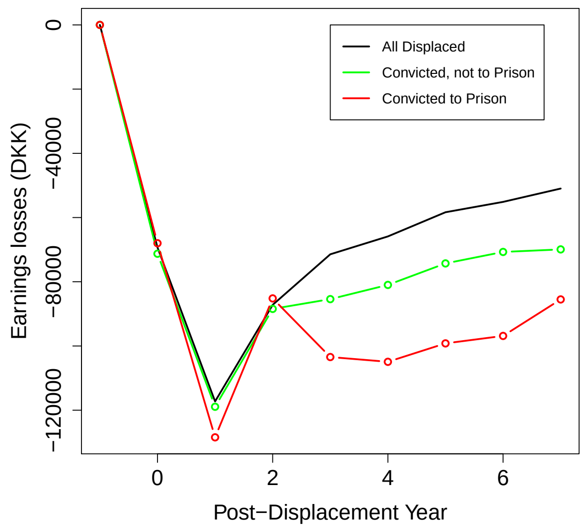
Appendix Figure B: Displacement Impacts with Alternative Definitions for Mass-Layoffs

The graph compares the impact of displacement on crime with three alternative definitions for mass-layoffs: (i) using a 40% threshold for firm size changes rather than a 30% threshold as in the original estimation presented in Table 7 (ii) using the average employment of 1985-1989 instead of the peak employment in that period to define the reference firm size (iii) using a firm-specific trend to correct the mass-layoff definition for any preexisting trend in firm size decline. -1 is the pre-displacement year, as in the main specification.



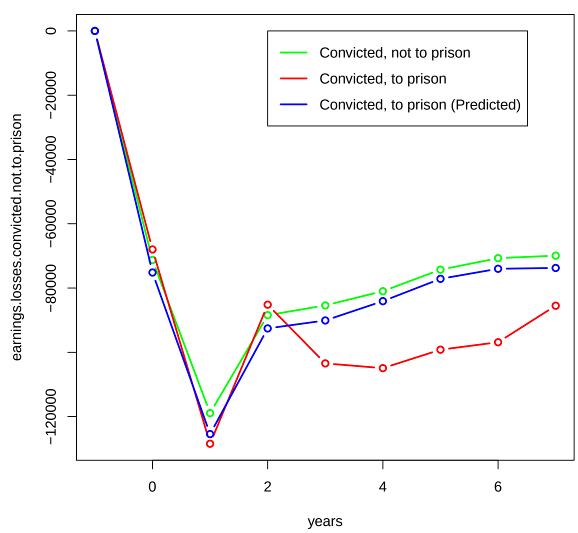
Appendix Figure C: Impact of Displacement on Earnings Losses by Conviction Status

The graph below represents earnings losses post displacement for individuals convicted and for other displaced individuals.



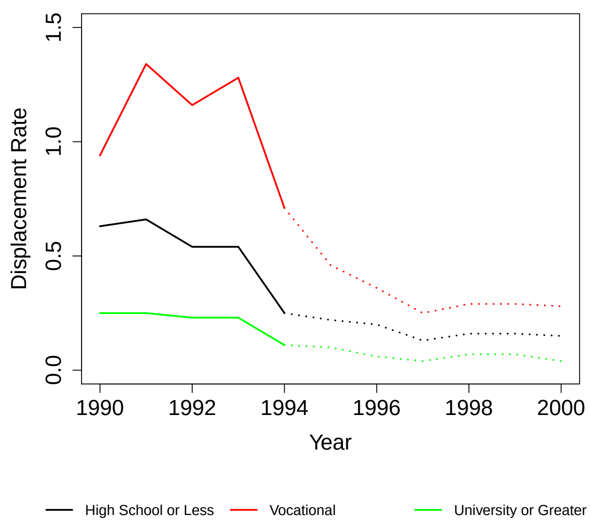
Appendix Figure D: Prison Terms and the Impact of Displacement on Earnings Losses: Predicted vs. Actual

The graph computes earnings losses as predicted by the time spent in prison; and compares such predicted earnings losses to the actual earnings losses of individuals convicted to prison.



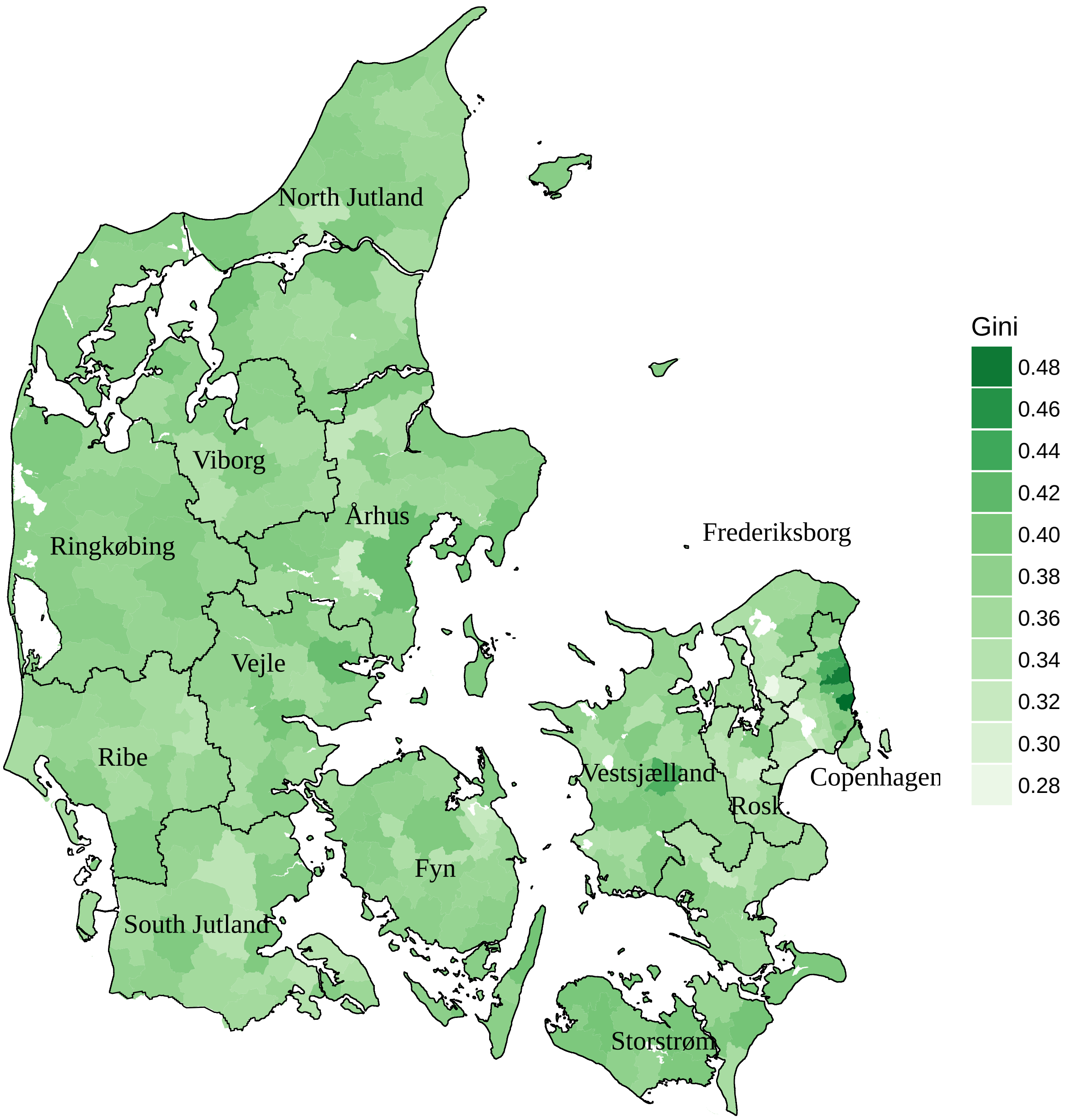
Appendix Figure E: Displacement Rates by 1989 Education Level

This figure presents displacement rates by the highest level of education completed: less than high school, vocational educational, and university or greater. Solid line: displacement rates for the period of analysis; main specification 1 considers the impact of displacement in 1990-1994 on subsequent crime in 1990-2000.



Appendix Figure F: The Geographic Distribution of Danish Income Inequalities

The map presents the Gini coefficient computed from the income data set. The method behind the Gini coefficient calculation is described in Section 5. We use total personal income at the family level. The list of municipalities' names, median income and population numbers is described in Table D.



Appendix Table A: For Displaced Workers: The Timeline from Offense, Charge, to Prison

The upper panel presents the distribution of the number of days from the date of the offense to the date of the charge(s). Multiple charges can correspond to one offense. The date of the offense is recorded at the time the charges are filed. In order to comply with Statistics Denmark's data confidentiality criteria, the 25th percentile, the median, and the 75th percentile are calculated using the average observations of the 5 individuals surrounding each statistic.

	Time from Offense to Charges (days)				
Sample	Mean	Median	P25	P75	Charges
at least 1 charge	50.9	0	0	0	1,537
excluding speeding	81	0	0	18	922
excluding zeros	232.2	63.2	18.4	226	337

	Time from Charges to Conviction (days)				
Sample	Mean	Median	P25	P75	Convictions
at least 1 conviction	98.8	64.4	39	118.4	1,246 (81.06%)[1]
excluding speeding	129.9	89.6	52	151.4	646
excluding zeros	101.5	67	41	119.8	1,213

	Time from Conviction to Prison (days)				
Sample	Mean	Median	P25	P75	Prison terms
at least 1 prison term	193.5	156.2	98.4	229.8	140 (11.23%)[2]
excluding speeding	203.2	166	106	236.8	117
excluding zeros	196.3	159	101.8	232.8	138

[1]: Percentage of charges leading to conviction.
[2]: Percentage of convictions leading to a prison term.

Appendix Table B: Robustness: Estimation Eliminating Small Firms

The table assesses the robustness of the results of the main specification (Table 7) to considering solely firms with more than 20 employees, more than 25 employees, and more than 50 employees in 1989.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent:	Property Crime	Violent Crime	Property Crime	Violent Crime	Property Crime	Violent Crime
Firm Size:	20+		25+		50+	
Year +7	0.0049***	-0.0008***	0.0054***	-0.0009***	0.0061***	-0.0008**
	(0.0017)	(0.0003)	(0.0019)	(0.0003)	(0.0022)	(0.0003)
Year +6	0.0025*	0.0000	0.0028**	0.0000	0.0023	-0.0001
	(0.0013)	(0.0006)	(0.0014)	(0.0007)	(0.0015)	(0.0006)
Year +5	-0.0001	-0.0004	0.0001	-0.0004	-0.0001	-0.0001
	(0.0009)	(0.0005)	(0.0010)	(0.0005)	(0.0011)	(0.0006)
Year +4	0.0067***	-0.0008***	0.0068***	-0.0009***	0.0070***	-0.0007**
	(0.0018)	(0.0003)	(0.0019)	(0.0003)	(0.0022)	(0.0003)
Year +3	0.0017	0.0012	0.0019	0.0012	0.0023	0.0003
	(0.0013)	(0.0010)	(0.0014)	(0.0010)	(0.0016)	(0.0008)
Year +2	0.0031**	-0.0004	0.0035**	-0.0005	0.0033*	-0.0002
	(0.0015)	(0.0005)	(0.0016)	(0.0005)	(0.0018)	(0.0006)
Year +1	0.0040**	0.0012	0.0044**	0.0013	0.0038**	0.0021*
	(0.0016)	(0.0009)	(0.0017)	(0.0010)	(0.0019)	(0.0012)
Displacement year	0.0046***	-0.0000	0.0046***	-0.0000	0.0046**	-0.0002
	(0.0017)	(0.0005)	(0.0017)	(0.0005)	(0.0020)	(0.0005)
R Squared	0.090	0.075	0.089	0.076	0.087	0.077
Observations	1,472,016	1,472,016	1,407,120	1,407,120	1,201,344	1,201,344
Individuals	92,001	92,001	87,945	87,945	75,084	75,084
F Statistic	9,028	1,605	8,854	1,621	8,164	1,471

***: Significant at 1%, **: Significant at 5%, *: Significant at 10%.

Appendix Table C: Individual and Family Earnings Losses Post-Displacement

The results below present the impact of displacement on individual earnings (column (1)), on family earnings (column (2)), and on family earnings when the family has more than one adult (column (3)). Income in Danish Kroner (DKK): in column (1), wage income as in panel (i) of the descriptive statistics Table 1. In columns (2) and (3), total personal income aggregated to the family level. Total personal income is the sum of wages, transfers, property income, and other income sources attributed to the individual before taxes. The family is a set of individuals living in the same housing unit, either married or in a civil partnership, and their children.

	(1)	(2)	(3)
Earnings l.h.s:	Individual Income	Family Income	Family Income
Sample:	All high-tenure individuals	All high-tenure individuals	Two or more Adults in Family
Year +7	-50,969***	-60,967***	-56,254***
	(1,914)	(2,936)	(3,305)
Year +6	-55,124***	-64,991***	-58,943***
	(1,960)	(2,941)	(3,280)
Year +5	-58,350***	-66,831***	-61,960***
	(2,011)	(2,998)	(3,405)
Year +4	-65,881***	-72,137***	-66,816***
	(2,075)	(2,977)	(3,343)
Year +3	-71,446***	-78,469***	-71,843***
	(2,114)	(3,018)	(3,416)
Year +2	-87,188***	-92,343***	-85,080***
	(2,236)	(3,035)	(3,416)
Year +1	-117,268***	-120,396***	-114,452***
	(2,272)	(2,993)	(3,409)
Displacement year	-69,296***	-70,949***	-69,906***
	(1,852)	(2,592)	(2,894)
R Squared	0.036	0.083	0.113
Observations	1,638,016	1,638,016	1,370,138
Individuals	102,376	102,376	97,140
F Statistic	1,626	1,786	1,893

Appendix Table D: Danish Municipalities by Income Inequality

In the table below, municipalities are ranked by their Gini coefficient, which is displayed alongside the population numbers, the median income. Population from 2006 Statistics Denmark data. Municipalities and Counties per the pre-2007 Danish boundaries. Gini and Median Income: Average 1985-1990 municipality-level measure based on total personal income at the household level.

Rank in Gini	Municipality name	County	Population	Gini	Median Income (DKK)
1	Gentofte kommune	Copenhagen	68,623	0.4893	228,376
2	Søllerød kommune	Copenhagen	31,920	0.4753	288,671
3	Hørsholm kommune	Frederiksborg	24,317	0.4375	304,673
4	Birkerød kommune	Frederiksborg	22,321	0.4319	271,654
5	Stenlille kommune	West Zealand	5,634	0.4309	225,017
...					
266	Ramsø kommune	Roskilde	9,412	0.3153	333,619
267	Gjern kommune	Århus	8,295	0.3115	311,161
268	Vallensbæk kommune	Copenhagen	12,230	0.2949	365,670
269	Ledøje-Smørun kommune	Copenhagen	10,797	0.2871	388,982
270	Ølstykke kommune	Frederiksborg	15,681	0.2778	367,736

Job Flows and Crime

This appendix presents a model that formalizes the difference between, on the one hand, estimates of the impact of unemployment on crime based on simultaneous shifts in the arrival rate of offers, the separation rate, and the wage distribution, as in the unemployment and crime literature; and, on the other hand, estimates based on individual experiences of idiosyncratic job separations, as in this paper.

A Simple Model

Time is continuous and individuals have infinite horizon $t \in [0, \infty)$. There is a density 1 of individuals $i \in [0; 1]$. In each period $[t; t + dt)$, individual i can be in one of three states $\{u, e, c\}$. When unemployed, individuals get utility bdt equal to the utility value of unemployment benefits b. They receive a wage offer with probability δdt and with distribution $f(w; \theta)$, where θ is a productivity parameter. They receive an opportunity to commit crime with probability λdt, and the utility value of criminal opportunities is noted κdt and has distribution $g(\kappa)$. The value of state $s \in \{u, e, c\}$ is noted V_s.

$$\begin{aligned} V_u = bdt + (1 - \beta dt) \cdot [(1 - \delta dt \cdot P(w \geq \underline{w}) - \lambda dt \cdot P(\kappa \geq \underline{\kappa})) \cdot V_u \\ + \delta dt \cdot \int_{\underline{w}}^{\infty} V_e(w) f(w; \theta) dw + \lambda dt \cdot \int_{\underline{\kappa}}^{\infty} V_c(\kappa) g(\kappa) d\kappa] \end{aligned} \tag{4}$$

$$V_e(w) = wdt + (1 - \beta dt) \cdot [(1 - \mu dt) \cdot V_e(w) + \mu dt \cdot V_u] \tag{5}$$

$$V_c(\kappa) = \kappa dt + (1 - \beta dt) \cdot [(1 - \xi dt) \cdot V_c(\kappa) + \xi dt \cdot V_u] \tag{6}$$

From the second and third equations, we obtain the values of employment and crime respectively:

$$V_e(w) = \frac{w + \mu V_u}{\beta + \mu}, \quad V_c(\kappa) = \frac{\kappa + \xi V_u}{\beta + \xi} \tag{7}$$

which are increasing in w and κ respectively. These two equations imply that the reservation wage and the reservation value of criminal opportunities are equal at equilibrium. The probability of accepting an offer in the formal or in the criminal sectors will be however different. We then turn to the value of unemployment:

$$V_u = \frac{1}{\beta + \delta \cdot P(w \geq \underline{w}) + \lambda \cdot P(\kappa \geq \underline{\kappa})} \cdot \left[b + \delta \cdot \int_{\underline{w}}^{\infty} V_e(w) f(w) dw + \lambda \cdot \int_{\underline{\kappa}}^{\infty} V_c(\kappa) g(\kappa) d\kappa \right] \quad (8)$$

Now plug-in the value of employment and crime in the equation:

$$\begin{aligned} V_u \quad = \quad & \frac{1}{\beta + \delta \cdot P(w \geq \underline{w}) + \lambda \cdot P(\kappa \geq \underline{\kappa})} \cdot \\ & \left[b + \delta \cdot \frac{E(w|w \geq \underline{w}) P(w \geq \underline{w}) + \mu V_u}{\beta + \mu} + \lambda \cdot \frac{E(k|\kappa \geq \underline{\kappa}) P(\kappa \geq \underline{\kappa}) + \xi V_u}{\beta + \xi} \right] \end{aligned}$$

which provides a unique V_u for any pair $(\underline{w}, \underline{\kappa})$.

Definition 1. *A steady state equilibrium of the labor market is a reservation wage $\underline{w} \in [0, \infty)$, a reservation value of criminal opportunities $\underline{\kappa} \in [0, \infty)$, a value of unemployment $V_u \in \mathbb{R}$, a value of employment $V_e(w) \in \mathbb{R}$ for each wage $w \in [0, \infty)$, and a value of crime $V_c(\kappa) \in \mathbb{R}$ for each criminal opportunity value $\kappa \in [0, \infty)$ such that:*

- *Individuals in unemployment are indifferent between staying in unemployment and accepting wage offer $\underline{w}$, i.e. $V_e(\underline{w}) = V_u(\underline{w}, \underline{\kappa})$.*
- *Individuals in unemployment are indifferent between staying in unemployment and accepting criminal opportunity $\underline{\kappa}$, i.e. $V_c(\underline{\kappa}) = V_u(\underline{w}, \underline{\kappa})$*

The equilibrium parameters are the job separation rate μ, the crime exit rate ξ, the arrival rate of formal job offers δ, the arrival rate of criminal opportunities λ, the unemployment benefits b, the distribution of wages $f(\cdot)$, and the distribution of the value of criminal opportunities $g(\cdot)$.

Impact of Individual Separations on Crime at Steady-State

The paper estimates the impact of job separations on the probability of committing crime. The simple model outlined above provides predictions regarding the probability to commit crime at any time $t' > t$ given that the individual is in unemployment at time $t = 0$.

Define s_t the state of the individual at t. It is a random variable $s : [0, \infty) \times \Omega \to \{u, e, c\} = \mathbb{S}$, i.e. which is such that $s_0 = u$. Then note $\phi(s, t)$ the probability of state s in time period $[t, t + dt)$, and $\boldsymbol{\phi}(t)$ the stacked vector $[0,\infty) \to [0,1]^3$ of probabilities of each state respectively. Then, the probability of unemployment $\phi(u,t)$ satisfies:

$$\frac{\partial \phi(u,t)}{\partial t} \cdot = -\left[\delta P(w \geq \underline{w}) + \lambda P(\kappa \geq \underline{\kappa})\right] \cdot \phi(u,t) + \mu \cdot \phi(e,t) + \xi \cdot \phi(c,t)$$

performing the same operation for the two other states leads to finding the individual dynamics of transition between unemployment, employment, and crime:

$$\frac{\partial \boldsymbol{\phi}}{\partial t}(t) = \underbrace{\begin{pmatrix} -(\delta P(w \geq \underline{w}) + \lambda P(\kappa \geq \underline{\kappa})) & \mu & \xi \\ \delta P(w \geq \underline{w}) & -\mu & 0 \\ \lambda P(\kappa \geq \underline{\kappa}) & 0 & -\xi \end{pmatrix}}_{Q} \boldsymbol{\phi}(t) \tag{9}$$

where the initial probability is such that the probability of unemployment is $\boldsymbol{\phi}(0) = (1\,0\,0)$. Then the probability of each of the three states is given by:

$$\boldsymbol{\phi}(t) = \boldsymbol{\phi}(0)e^{tQ}, \quad \forall t \in [0,\infty) \tag{10}$$

Proposition 1. *Note $(\phi_u^*, \phi_e^*, \phi_c^*) \in [0,1]$ the steady-state equilibrium fractions of unemployment, employment, and crime. As $t \to \infty$, an individual initially in unemployment ($\phi(u,0) = 1$) experiences a probability of crime that converges to the probability of crime in the economy, i.e. $\phi(c,0) = 0$ and $\phi(c,t') \to \phi_c^*$ as $t' \to \infty$.*

The steady-state crime rate is $\phi_c^* = \frac{\lambda}{\xi} P(\kappa \geq \underline{\kappa}) / \left[1 + \frac{\delta}{\mu} P(w \geq \underline{w}) + \frac{\lambda}{\xi} P(\kappa \geq \underline{\kappa})\right]$. This paper's estimated impact of a job separation on the crime rate corresponds to such ϕ_c^* in the model.

Comparative Statics: Arrival Rate, Separation Rate, and Wage Distribution

In contrast, prior literature on the impact of unemployment on crime has estimated the impact of changes in area-level industrial structure on crime. Changes in the industrial structure affect the unemployment rate through changes in the arrival rate of offers and the distribution of wages. Indeed, the steady-state unemployment rate is $\phi_u = 1/\left[1 + \frac{\delta}{\mu} P(w \geq \underline{w}) + \frac{\lambda}{\xi} P(\kappa \geq \underline{\kappa})\right]$, and the crime rate is: $\phi_c = \frac{\lambda}{\xi} P(\kappa \geq \underline{\kappa}) \phi_u$, where the steady-state reservation value $\underline{\kappa}$ for crime opportunities depends on the labor market's fundamentals. This implies that a 1 percentage point increase ($d\phi_u = +0.01$) in the unemployment rate will lead to a $d\phi_c = \lambda / \left[\xi \cdot P(\kappa \geq \underline{\kappa})\right] \cdot d\phi_u$ increase in the probability of crime. Such impact $\lambda / \left[\xi \cdot P(\kappa \geq \underline{\kappa})\right] \cdot d\phi_u$ is typically smaller than the impact of a job separation on the probability of crime $\phi_c^* = \frac{\lambda}{\xi} P(\kappa \geq \underline{\kappa}) / \left[1 + \frac{\delta}{\mu} P(w \geq \underline{w}) + \frac{\lambda}{\xi} P(\kappa \geq \underline{\kappa})\right]$ derived above.