NHH

INSTITUTT FOR SAMFUNNSØKONOMI

DEPARTMENT OF ECONOMICS

SAM 32 2018

ISSN: 0804-6824

December 2018

Discussion paper

Job Displacement, Unemployment, and Crime: Evidence from Danish Microdata and Reforms

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Job Displacement, Unemployment, and Crime: Evidence from Danish Microdata and Reforms*

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Abstract

This paper estimates the individual impact of a worker's job loss on his/her criminal activity. Using a matched employer-employee longitudinal data set on unemployment, crime, and taxes for all residents in Denmark, the paper builds each worker's timeline of job separation, unemployment, and crime. The paper focuses on displaced workers: high-tenure workers who lose employment during a mass-layoff event at any point between 1990 and 1994 (inclusive). Initial local industrial specialization suggests that the growth of manufacturing imports and the Nordic financial crisis in the early 1990s explain a significant share of mass layoffs. Placebo tests display no evidence of trends in crime prior to worker separation. Using Denmark's introduction of the Act on an Active Labor Market at the end of 1993, we estimate the impacts of activation and of a reduction in benefit duration on crime: crime is lower during active benefits than during passive benefits and spikes at the end of benefit eligibility. We use policy-induced shifts in the kink formula relating prior earnings to unemployment benefits to estimate the separate impacts of labor income and unemployment benefits on crime: the results suggest that unemployment benefits do not significantly offset the impact of labor income losses on crime.

*We thank the editor Imran Rasul and three anonymous referees for key suggestions. We also thank Decio Coviello, Maria Guadalupe, Birthe Larsen, Nicola Persico, Steve Pischke, Jesse Rothstein, Jay Shambaugh, Ahmed Tritah, Kjell Salvanes, Stephen Machin, as well as the audience of the Society of Labor Economists, the London School of Economics, the Rockwool Foundation, the IZA/CEPR Symposium, the Nordic Summer Institute in Labor Economics, the IMF-OCP Workshop, the Montreal Applied Microeconomics workshop, the Quebec Political Economy Conference, the Petralia Conference for fruitful comments. The authors acknowledge support from Copenhagen Business School, the Norwegian School of Economics, Ecole polytechnique, New York University, INSEAD, HEC Montreal, and the Research Council of Norway. This work was partially supported by the Research Council of Norway through its Centres of Excellence Scheme, FAIR project No 262675. The usual disclaimers apply.

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1 Introduction

Do layoffs cause crime? Becker (1968) describes criminal behavior as the outcome of a comparison between the gains from legal employment and the gains from criminal activity. From this Beckerian perspective, both job losses and unemployment benefit transfers should impact the expected returns of crime and thus the individual crime rate. Key papers (Raphael & Winter-Ebmer 2001, Gould et al. 2002, Öster & Agell 2007, Lin 2008, Fougère et al. 2009) estimate economically significant impacts of area-level unemployment on crime rates using robust instrumental variable strategies. An important remaining question is to what extent are area-level findings due to the impact of individual unemployment on crime as opposed to local economic conditions, declines in government revenue, or peer effects. Measuring the causal impact of worker-level job loss on crime is an empirical challenge. First, this causal analysis requires a representative employer-employee-crime data set. Second, isolating the individual causal mechanism requires an exogenous source of job loss at the worker level. Third, estimating the importance of unemployment insurance (UI) requires policy-based identification strategies.

This paper addresses these three key challenges to estimate the impact of individual job separations on crime. First, the paper builds a longitudinal panel that matches the employer-employee, unemployment, crime, and tax records of all Danish residents. The panel measures unemployment and social assistance (SA) benefits weekly and criminal events (offenses, charges, convictions, and prison terms) daily. Such frequency enables the construction of a timeline of job separation, benefit recipiency, subsequent offense, and subsequent conviction. Second, the paper uses firm-level mass layoffs as an exogenous source of job separations: sudden, unexpected firm downsizing events occurring at any point between 1990 and 1994, inclusive. In the early 1990s, Danish firms experienced the Nordic financial crisis and the liberalization of manufacturing imports. As in Jacobson et al. (1993), the paper follows high-tenure workers, who are unlikely to separate in the absence of a mass-layoff event. Third, the paper estimates the potential impact of UI using successive reforms to (i) the length and conditionality of benefits and (ii) the amount of benefits.

Prior to 1994, unemployment benefits were generous and passive, with a de facto maximum of 9 years. In January 1994, Denmark introduced the Act on an Active Labor Market, affecting all workers' rights to benefits. This reform reduces the length of passive benefits to four years from the initial 9 years and introduces a second active period that expires after three years. During the active period, the receipt of benefits is conditional on a mandatory individual action plan that intended to help a worker transition back into permanent employment. The second reform occurred in 1996 and reduced the length of the passive period from 4 to 2 years while leaving the active period unchanged. The total length thus decreased from 7 to 5 years. The paper uses these two unexpected reductions in benefit duration to identify workers' response to passive and active benefit eligibility.

Finally, the paper separates the impact of labor income losses from the impact of unemployment benefits on crime. During the passive period, benefits follow a simple mechanical formula: a flat 90% replacement rate with a maximum amount, as in Card et al. (2015). The maximum amount changed 3 times during the period of analysis, while the 90% rate remained unchanged. Such exogenous shifts separate the impact of unemployment benefits from the impact of labor income on crime.

The paper's main finding is that job displacement significantly increases the probability of committing crime: by 0.57 percentage points (ppt) in the year of displacement and 0.39 ppt in the following year. These are substantial effects: in the year of displacement, crime increases by 32%. The impact is driven primarily by property crime, in line with prior contributions. The impact on crime is non monotonic: three separate spikes in criminal activity occur in the short, medium and long run.

The paper suggests that the resurgence in criminal activity is likely driven by the design of unemployment benefits. First, the end of active unemployment benefits triggers a +0.30 ppt increase in crime. Displaced workers' transition out of active benefits results in a sudden drop in welfare transfers and, depending on the year of the mass layoff, occurs 7, 6, or 5 years after job loss. As the law reduces the total length of benefits from 7 to 5 years, the spike in crime also shifts from 7 to 5 years after job loss. Second, workers commit significantly more crime during the passive benefit period than the active benefit period: the crime rate is +0.31 ppt higher during passive benefits than in the pre-displacement year, and only +0.03 ppt and non-significant during active benefits. Third, the observed increase in crime 4 years after job displacement is due to an unintended consequence of the 1994 reform on workers displaced in 1990. For those displaced in 1990, the unexpected binding reform leads to the exhaustion of eligibility for passive benefits. According to the reform, workers who accumulate more than 26 weeks of employment in the last 3 years become eligible for a new unemployment spell with benefits based on their new, typically lower, level of income. This gives part-time workers who accumulate less than 26 weeks of employment an incentive to drop out of the labor force and remain in unemployment until the exhaustion of their former rights, as they are better off receiving benefits based on their 1989 income level than based on their 1994 part-time employment income. Consistent with this, the fraction of full-time unemployed workers *doubles* 4 years post-displacement for workers laid off in 1990 but not for workers laid off in the period 1991-1994. Workers laid off in 1990 experience a large increase in property crime 4 years after displacement.

The last set of results pertains to the impact of income losses and benefit transfers on crime. The paper finds substantial impacts of labor income losses on crime. Moving down from the 80th to the 20th percentile of labor earnings in the year following job displacement corresponds to a 0.7 ppt increase in the crime rate. Crime is at its maximum during labor income declines and welfare transfer increases. However, there is no mitigating impact of marginal shifts in the amount of unemployment benefit transfers. Using reform-induced changes in benefits, we estimate that shifts in the amount of unemployment benefits have no statistically significant impact on crime.

Results are robust to a number of alternative approaches and sensitivity tests. First, we control for local, municipality-specific labor demand shocks by including municipality x time fixed effects, and the results are unaffected. Second, using plant closures, which affect all workers at a given location, also yields similar results. Third, placebo tests suggest no evidence of endogenous selection of firms or workers into displacement during a mass-layoff event: firms experience no significant pre-mass-layoff trends; and workers who will be displaced do not experience higher crime than similar workers at the same firm who will not be displaced.

This paper's key results should have broader impacts beyond the specific case of displaced workers in Denmark. First, Denmark is the prime example of the *flexicurity* model, which combines generous unemployment benefits with low employment protection. Second, Denmark is not a particularly low-crime country: data from the European Sourcebook of Crime and Criminal Justice Statistics suggests that while Denmark ranks 22nd in homicides, 21st in robberies, it ranks 1st in theft, and 2nd in motor vehicle theft. Third, workers are aged between 20 and 39 in the sample, an age range that accounts for approximately 2/3 of offenses. Fourth, displaced workers commit similar types of crimes as the overall population of male offenders.

This paper contributes to four literatures. First, the paper contributes to the economics of crime (Grogger 1998, Machin & Meghir 2004, Kling et al. 2005, Barbarino & Mastrobuoni 2014, Damm & Dustmann 2014, Aizer & Doyle 2015, Bindler 2015, Bhuller et al. 2016, Doleac & Hansen 2016). The paper isolates the *individual channel* of the impact of layoffs on crime. In prior studies, a one ppt increase in the unemployment rate leads to a 3-7% increase in the crime rate. In this paper, job displacement leads to a 30% increase in the probability of committing crime, suggesting that the individual channel may account for the majority of the aggregate effects.

Second, the paper contributes to the literature on the welfare effects of job displacement (Jacobson et al. 1993, Charles & Stephens Jr. 2004, Sullivan & von Wachter 2009, Rege et al. 2011, Black et al. 2015, Huttunen et al. 2018, Gathmann et al. 2018). A key feature of our paper is that we exploit the joint dynamics of crime and earnings: we test whether crime is the outcome of post-displacement earnings losses that tilt an individual's cost-benefit analysis towards crime.

Third, the paper contributes to the analysis of the welfare consequences of concentrated financial and trade shocks (David et al. 2013, Autor et al. 2014, Hummels et al. 2014, Bloom et al. 2016, Pierce & Schott 2016). First, the Nordic financial crisis triggered bank failures and branch closures. The share of employment in finance and insurance in 1989 is a significant predictor of mass layoffs. Second, the early 1990s witnessed a decline in tariffs and the end of the Uruguay Round. A municipality-level Bartik predictor of the growth of import volumes is strongly correlated with mass layoffs.

Finally, the results of this paper should help policymakers in the design of optimal UI policies (Farber et al. 2015, Card et al. 2015). We find that benefit transfers without conditionality do not typically mitigate the impact of earnings losses on crime. Crime is at its lowest during the active benefit period. Crime spikes at the end of benefits; thus, reductions in potential benefit duration lead to corresponding shifts in crime spikes at the end of benefits.

The paper proceeds as follows. Section 2 describes the longitudinal panel. Section 3 describes the identification strategy: using workers displaced during mass layoffs. Section 4 describes the main results. Section 5 uses the active labor market reforms as a source of variation in potential benefit duration. Section 6 estimates the impact of labor income and unemployment benefits on crime. Section 7 concludes.

2 Data Set

Anyone residing in Denmark for more than 3 months is assigned a Central Person Register (CPR) number. This paper builds a longitudinal panel from six data sources linked by a CPR number from the following Danish agencies: the police and courts, the tax authority (SKAT), the unemployment funds (A-Kasse), the public registration offices, and the Ministry of Education.

2.1 Crime

Individuals who are formally charged following a citation or arrest are matched to a unique police case number. The police station matches the charged individual to his CPR number. If multiple people are charged with the same crime, we observe each of the multiple co-offenders matched to the same case number. The data on criminal charges include the reported day of the offense and the day charges were filed. Charges are classified according to Danish offense codes. Crimes belong to one of the three most frequent crime categories: property crimes, violent crimes, and crimes related to driving under the influence (DUI).

The police case number follows an individual from charges to court convictions. The Central Police Register includes the conviction date and conviction outcome such as incarceration, suspended sentence, fine, settlement, no charge/warning, or another less frequent decision such as a youth program or military punishment. We focus on the first four conviction outcomes in what follows, eliminating criminal events leading to no charges or other infrequent outcomes. Start and end dates of incarceration (when applicable) are recorded and linked to the police case number.

2.2 Employer-Employee

Employers are legally required to report information for every worker: part- and full-time employment status, the pre-tax annual salary earned, and workplace and firm identification numbers. In this paper's period of analysis, SKAT collects these reports and Statistics Denmark aggregates them into an Integrated Database for Labor Market Research (IDA, *Den Integrerede Database for Arbejdsmarkedsforskning*). Firm and workplace identifiers are consistent over time. As such, a new identifier corresponds to the creation of a new firm or workplace, described in detail in Appendix C.

2.3 Unemployment Insurance (UI) and Social Assistance (SA) Benefits

The Labor Office follows workers throughout their unemployment spells and is responsible for the management of benefits. Workers contribute to sector-specific unemployment funds (*arbejdsløshed-skasser*) through an annual membership fee that is 8 times the maximum daily UI benefit, 3,272 DKK in 1990. Workers aged 16–65 may join if they work in the industry of the UI fund, have been employed for at least 300 hours in the last 10 weeks, or have completed vocational education in the industry of the fund.[1] Approximately 85% of the active population are UI fund members. Non-members are younger and either in the lower or the upper percentiles of the income distribution.[2] Non-members are eligible for SA (*kontanthjælp*) and the Labor Office also follows them.

After job separation, any eligible worker is legally entitled to claim UI benefits. Eligible workers are those who have been members of a fund for at least a year and worked the full-time equivalent of 26 weeks in the last three years. After the displacement years considered here, this changed to 52 weeks in 1997 (Chapter 10, Section 53, of LBK 27 of January 13, 1997). Below this 26-week threshold, the worker is eligible to means-tested SA. The worker registers as unemployed with the local Labor Office. A record appears in the *Central Register of Labour Market Statistics* (CRAM), which records any received benefits, both UI and SA, for each worker in each week of the year.

A worker separating due to a layoff or discharge is immediately entitled to UI benefits. It is in the worker's best interest to register as unemployed: the administrative overhead for claiming UI benefits is low and any eligible worker qualifies. Hence, while in the United States the number of unemployment claimants is typically lower than the number of unemployed workers, in Denmark the number of benefit claimants is higher than the number of unemployed workers (Madsen 1999).

2.4 Additional Data

Three additional sources provide data on income, location, family, demographics, and education.

For those with a tax obligation in Denmark, the Income Register records data on income other than annual salary, compiled from various registers by SKAT. The data largely comprise what is stated on an individual's annual tax return (*årsopgørelse*) that is automatically generated

[1] The full list of eligibility criteria is presented in Paragraph 41 of Chapter 8 of Law 586.

[2] Additionally, younger workers below the age of 25 are also subject to stricter regulations. The results that follow are robust to excluding younger workers.

by SKAT. Total annual income is defined as the sum of labor income (salary and self-employed earnings), public transfer income, and residual income not in one of these categories.[3]

Individuals who move report their new address to the Public Registration Office (*Folkeregisteret*). Public services and welfare payments depend on these self-reported address changes. These demographic records make up the Population Register. The data include age, gender, municipality of residence, the date of last move, immigrant status, marital status, and the mother's and father's CPR numbers. 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. To be considered a family, two adults residing together must be registered as a cohabiting couple, a married couple, in a registered partnership, or having a common child. Two individuals sharing a housing unit with no such connection will be considered two families.

All Danish educational institutions are legally required to report information on completed and ongoing schooling to the Ministry of Education. We classify a worker into one of three education categories: high school or less, vocational, and university education. As this paper focuses solely on natives, measurement error due to self-reported education is unlikely to be a substantial concern.

3 Empirical Strategy

There are at least two major identification issues that prevent a causal interpretation of a positive correlation between job separation and crime. First, individual-level observables and unobservables cause both unemployment and crime. Table 1 shows that individuals who transition into unemployment are younger, less likely to be married, less educated, and more likely to be male. Second, crime may cause job separation rather than the converse. The Salaried Employees Act (*Funktionærloven*)[4] does not provide a general statutory prohibition against unfair dismissal. Crimes committed within the firm can lead to summary dismissal with no notice period, while crimes committed outside the firm, i.e., unrelated to the employee's work, can lead to dismissal with a notice period of between one and six months. A correlation between job separation and crime therefore

[3] This includes income such as income from abroad, certain tax-free income, and insurance payments.

[4] "Bekendtgørelse af lov om retsforholdet mellem arbejdsgivere og funktionærer", or Act on the Legal Relationship between Employers and Employees, LBK number 516 of July 23, 1987, applies in the period of analysis. The report of the Directorate General of Employment, Social Affairs, and Equal Opportunities (2006) suggests that Denmark's dismissal rules are more flexible than those of other European countries.

captures both the impact of separation on crime *and* the impact of crime on job separation.

This paper addresses these two central identification issues by focusing on high-tenure workers that separate during sudden, arguably unexpected firm-level mass-layoff events.

3.1 Mass-Layoff Events

Firm Downsizing

Firms experience a mass-layoff event whenever their size experiences a statistically large decline relative to a reference point. We begin with a simple reference point similar to Jacobson et al.'s (1993) approach: a 30% decline in firm employment compared to the peak of firm size in the previous five years, for firms with 30 workers or more.[5] A concern with using peak employment is that some firms may be on a downward trend and that the 30% change may be anticipated.

A second approach considers a 30% decline relative to a firm-specific trend, built as follows. $n_{j,y}$ is the employment of firm j in year y. For each firm $j = 1, 2, \ldots, J$ we estimate the regression $n_{j,y} = \alpha_j + \beta_j \cdot y + \varepsilon_{j,y}$, using the ten years of observations $y = 1980, \ldots, 1989$ prior to the first year of analysis 1990. This yields firm-specific estimates of the constant $\widehat{\alpha_j}$ and the slope $\widehat{\beta_j}$. Firm j experiences a mass-layoff in year $y_0 = 1990, 1991, \ldots, 1994$ if n_{j,y_0} is 30% lower than the firm's predicted value in year y_0, $\widehat{n_{j,y0}} = \widehat{\alpha_j} + \widehat{\beta_j} \cdot y$.

The Spatial Dispersion of Mass-Layoff Events:
the Local Impacts of Imports and of the Nordic Financial Crisis

The map presented in Appendix Figure A suggests that mass layoffs per 1,000 workers are spread across Danish municipalities rather than concentrated in one single area. Such geographic dispersion provides an opportunity to consider two potential causes of mass layoffs relevant during the sample period: (1) the Nordic banking crisis and (2) trade liberalization.

Denmark experienced a wave of bank closures between the late 1980s and the early 1990s due to the Nordic banking crisis, termed by Drees & Pazarbasioglu (1998), Jonung (2008), and Andersen (2011). According to Andersen (2011), 47 Danish banks and savings banks (out of a total of approximately 225) closed involuntarily between 1985 and 1994, more than in all of the previous 40 years combined. In our employer-employee registry, the finance and insurance industry experienced

[5] Appendix Section A.1.1 shows that results based on a 50 employee threshold are similar.

an employment decline of 10% between 1989 and 1993. Employment in construction, which is arguably dependent on external financing according to a Rajan & Zingales (1998) measure, also declined substantially (13% between 1989 and 1993).

As in other countries during the 1990s, Denmark experienced a decline in manufacturing employment, especially in "textiles, clothing, leather", "stone, clay, glass", "food, beverage, and tobacco", "wood, paper, and graphic industry," and "furniture". These declines match the timing of the liberalization of trade, culminating with the end of the Uruguay round of negotiations of the General Agreement on Tariffs and Trade (GATT) in 1994. Cline (1995) mentions that the liberalization of the textile sector was one of the focuses of the negotiations, with the effects of the liberalizations affecting participants as early as 1992. Textiles experience the largest employment decline (-17% between 1989 and 1993) among the 27 industries in the Danish classification.

We match each firm j in the Danish employer-employee registry to its industry $s(j)$'s import values, volumes, and weights from United Nations Comtrade data.[6] The difference between the log change in import values and the log change in import volume or weight provides an average import price, as

$$\underbrace{\Delta \log(p \cdot q)_{sy}}_{\Delta \log \textit{import value}} - \underbrace{\Delta \log q_{sy}}_{\Delta \log \textit{volume or weight}} = \underbrace{\Delta \log p_{sy}}_{\textit{Implicit price change}}. \tag{1}$$

where y indexes years $1990, \ldots, 1993$, and results are similar examining differences over a longer time horizon. Figure 3 relates these shifts in import volumes/weights (resp. shifts in import prices) to changes in log industry employment. Figure 3(a) suggests that industry-level increases in import volume/weight are correlated with employment declines. Figure 3(b) suggests that industry-level declines in import prices are correlated with employment declines.

These impacts of trade and the Nordic banking crisis on Danish industries translate into local geographic impacts that explain part of the spatial distribution of the mass-layoff events built earlier in the section. Indeed, the upper panel of Table 2 shows that at the municipality level, there is a significant positive correlation between the number of mass layoffs in the period 1990-1993 and the initial share of employment in finance and insurance in 1989 (column (1)). A one ppt increase in employment in finance and insurance in 1989 is correlated with an additional 13

[6] Each firm is classified according to the Danish 27-group nomenclature, corresponding to 2-digit NACE levels. The World Integrated Trade Solution (WITS) crosswalks from NACE to the product classifications enable a high-quality match with product-level import data at the 6-digit H0 (pre-1996) or H1 level (post-1996).

workers laid off per 1000 employees. There is also a separate and positive correlation with the initial share of employment in manufacturing in 1989 (column (2)). Column (3) uses a Bartik et al. (1991) predictor of employment shocks based on the product of the national growth of imports by industry and the initial industrial specialization of a municipality m. Formally,

$$Predicted\,\Delta\log(imports)_{my} = \sum_{s=1}^{S} \omega_{m,s,1989} \cdot \Delta\log Imports_{s,1989,y}, \tag{2}$$

where $m = 1, 2, \ldots, 270$ indexes municipalities for mainland Denmark, and s indexes the S industries. $\omega_{m,s,1989}$ is the 1989 share of employment in industry s in municipality m. At the national level, $\Delta\log Imports_{s,1989,y}$ is the growth of imports between 1989 and y. This is an exogenous predictor of the demand for labor in municipality m, provided that the local industry shares are exogenous to national import shocks. The $Predicted\,\overline{\Delta\log(imports)}_m$ is the average of the Bartik predictor over the years $y = 1990, \ldots, 1993$.

Column (3) of Table 2 displays a strong and significant correlation between the Bartik import predictor and the total number of mass layoffs in the municipality, thus strengthening the argument that the decline in manufacturing has been partly driven by the liberalization of the imports of goods. Is import competition a stronger driver of mass layoffs than the Nordic banking crisis? The Table's bottom panel runs a horse race between the two mechanisms. Column (4) suggests that, when controlling for the initial share of employment in manufacturing, the impact of the initial share in finance and insurance becomes non-significant. Column (5) makes a similar point: including the Bartik import predictor makes the share in finance and insurance non-significant.[7] This analysis suggests that these external, arguably exogenous, shocks explain a significant share of the geographic dispersion of mass layoffs across Denmark.

Firm Size Dynamics around Mass Layoffs

Table 3 presents the regression of firm size on a set of indicator variables for each year before and after the mass-layoff event, for 326,157 firm×time observations. The regression tests three key hypotheses. First, whether firm size declines are statistically large in the year of the mass layoff, i.e., if −30% is a common log firm size change. Second, the regression's pre-mass-layoff indicator

[7] The concentration of mass layoffs may be a source of concern for the identification of the impact of displacement on crime. Section 4.2 addresses this point.

variables test whether there are any remaining trends leading to each mass-layoff event. Third, the regression estimates whether mass layoff events have long-lasting, and potentially permanent, impacts on firm size.

In this table, the reference point is the firm's peak employment in the 5 years prior to mass layoff. 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. Standard errors are clustered at the firm level. Column (1) conditions on firm survival following mass layoff while column (2) includes firms which exit the data following a closing, assigning a firm size of 0 to these firms.[8]

Post-mass-layoff indicator variables are significant at the 1% level in both columns (1) and (2), implying that a 30% decline is an uncommon and statistically large event. Pre-mass-layoff indicator variables are not significant at the 10% level suggesting that even the peak-firm-size approach yields relatively unexpected downsizings. Effects are significant up to 5 years after the event, where firms remain smaller by 72 employees when including firm closings (32% of the average firm size immediately prior to mass-layoff).

3.2 Displaced Workers

The paper focuses on workers with strong attachments to their firm, as in Jacobson et al. (1993). Such workers do not endogenously transition in and out of the firm prior to the mass layoff. As in Davis & Von Wachter (2011), we construct 5 separate samples, one for each of the mass-layoff years $y_0 = 1990, ..., 1994$.

Workers with strong attachments to their firm in year y_0 fulfill five criteria. First, workers need at least 3 years of tenure in $y_0 - 1$,[9] defined as continuous, full-time employment with the same employer. Second, the sample focuses on individuals aged 19–39 in $y_0 - 1$ who are aged 20–40 at the time of displacement. Men aged 20–39 committed 65.5% of offenses in the same year; thus, this paper's age range focuses on the subpopulation that accounts for the largest share of committed crimes. Third, individuals with two or more jobs between $y_0 - 3$ and $y_0 - 1$ are excluded as their employment choice may be driven by the comparison of alternative employment options. Fourth,

[8] Firms may experience a mass-layoff event which is a false closing. We exclude false closings from our definition of mass layoff. A false closing is defined as 30% or more of the workers in the closing firm transitioning to the same firm.

[9] Section 4.2 suggests that focusing on high-tenure workers does not cause a significant upward bias in the impact of displacement on crime.

individuals who are enrolled in education between $y_0 - 3$ and $y_0 - 1$ are excluded. Fifth, the sample includes workers of private firms only. Mass layoffs by private firms are more likely to be driven by sector-specific demand shocks than those of public-sector firms. Appendix Section A compares this paper's sample to the job displacement literature.

Finally, this paper follows the crime literature by focusing on males. Indeed, the majority of crimes are committed by males in the U.S. (Freeman 1999, Campaniello & Gavrilova 2018) and in Denmark 87.2% of those convicted in 1989 were male. However, there is growing interest in the drivers of female crime (Cano-Urbina & Lochner 2017, Beatton et al. 2018), and Appendix Section G presents results that suggest that job displacement also causes crime for female workers.

Imposing the criteria above results in a final sample of 154,694 high-tenure individuals over the 5 samples $y_0 = 1990 - 1994$. Within the set of high-tenure workers, individuals are *displaced* if (a) their firm experiences a mass-layoff event in y_0 and (b) they transition from employment in $y_0 - 1$ to unemployment in year y_0, measured as the receipt of unemployment benefits.[10] Each sample thus includes high-tenure workers, both displaced and non-displaced, subject to the same sample restrictions in $y_0 - 1$. To the extent that a non-displaced high-tenure worker fulfills the above criteria in each of the 5 samples, the worker would appear in the control group in all 5 samples.

Figure 1 describes the displacement rate thus obtained between 1990 and 1994, as a proportion of high-tenure workers. In this paper, we estimate the impact of displacement events occurring during the first 5 years of the decade, 1990–1994 (the solid line of Figure 1), which allows us to focus on a set of workers who are differentially impacted by the substantial reforms to the unemployment benefit system described in Section 5. Displacement rates range between 0.3% (1990, firm trend definition) and 1.2% (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. 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.

[10] Appendix Section E discusses the difference between the receipt of unemployment benefits and the Eurostat definition of unemployment.

3.3 Placebo Test: Endogenous Selection Into Displacement

Workers may not be selected at random for dismissal during a mass-layoff event (Gibbons & Katz 1991, Lengermann et al. 2002, Abowd et al. 2009). A concern is that dismissed workers have an unobservably higher propensity to commit crime. We test for such dynamic selection on unobservables by comparing the current crime of observationally identical workers who *will* be displaced to the future crime of workers who *will not* be displaced. If the unobservable propensity to commit crime is positively correlated with the probability of future displacement, we should observe that the crime participation (offenses leading to a conviction) of such future displaced workers is higher than the crime of individuals who will not be displaced.

Table 4 regresses the criminal activity of high tenure individuals between $y_0 - 5$ and $y_0 - 1$ on an indicator variable equal to 1 if the worker will be displaced in any year y_0 1990–1994. The dependent variables are property crime (resp. violent crime) in the upper panel (resp. the lower panel). The coefficients of $Future\,Displaced\,Worker$, ranging between 0.00 and 0.17 ppt, are not statistically significant at the 10% level. Overall the table suggests that future displaced workers are not more likely than future non-displaced workers to commit a crime between $y_0 - 5$ and $y_0 - 1$. This suggests no evidence of endogenous dynamic selection of workers into displacement. Further tests of self-selection are described in Sections 4.2 and 4.3.[11]

3.4 External Validity: The Distribution of Crime Types

The types of crimes leading to a conviction are similar for the population of displaced workers and for the overall population. Figure B compares the distribution of crime types for the entire Danish registry of males in the same age range (blue points) and for the sample of displaced workers. This breakdown is performed for the entire 1985–2001 data set. The numbers of D.U.I. offenses are similar for displaced and all 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 commit crimes that are similar to the other workers.[12]

[11] While the placebo tests suggest that displacement is an idiosyncratic shock, there remains the possibility that displaced workers are unobservably more responsive to layoffs. This is akin to an issue of essential heterogeneity in treatement effects (Heckman et al. 2006).

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

4 The Impact of Job Displacement on Crime

4.1 Econometric Specification

The following specification estimates the impact of job displacement on the probability of committing crime, controlling for individual-, municipality-, and time-specific unobservables:[13]

$$\begin{aligned} Crime_{it} &= \sum_{k=-5}^{+7} \delta_k \cdot \mathbf{1}(k \text{ years after displacement}) + Individual_i \\ &\qquad + Municipality_{m(i,t)} + \mathbf{x}_{it}\beta + Time_{t=y-y_o} + \varepsilon_{it} \end{aligned} \tag{3}$$

where $t = y - y_0$ is the year y relative to the displacement year y_0, which ranges from -5 to $+7$. i indexes the $N = 154,694$ individuals. The coefficient δ_k, for $k = +1, +2, ..., +7$, is the impact of displacement on the probability of committing crime k years after displacement. It measures effects up to 7 years after displacement. The coefficient δ_{-1}, a year prior to displacement, is conventionally set to zero. Thus all estimated effects are relative to the crime rate in the year -1. The regression is estimated by pooling the displacement year samples $y_0 = 1990, \ldots, 1994$.

$Crime_{it} = 1$ if individual i commits an offense and is charged at time t *and* the crime led to a conviction in any year $t' \geq t$. 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. Furthermore, using the timing of the *offense*, rather than the timing of the charge, conviction, or prison term, ensures that the crime does not occur prior to displacement.

Some criminal events occur prior to job displacement and should not be coded as occurring simultaneously. We recode offenses occurring on a day prior to the first week of unemployment as a crime occurring in $t = -1$. We can do so because we observe the day of the offense and the first week of post-displacement unemployment.

Charges and convictions typically occur a few weeks after the offense. Table B shows that, on average, the lag between offense and conviction is less than a year (172 days). Table C suggests that the lag for displaced workers is comparable but slightly shorter, at 150 days.

[13] A logit approach or a matching estimator yields results that are similar in terms of magnitude, timing, and duration.

The placebo coefficients $\delta_{-5}, \ldots, \delta_{-2}$ test whether workers had pre-displacement trends in their propensity to commit crime. The statistical significance of $\delta_{-5}, \ldots, \delta_{-2}$ is thus a test of dynamic endogenous selection into displacement. The fixed effect $Individual_i$ captures a variety of unobservable drivers of crime, for instance predispositions to violence (Frisell et al. 2011), which are correlated with job separation (LeBlanc & Kelloway 2002, Grandey et al. 2004). $Individual_i$ is negatively correlated with age, education, tenure, and the marriage variable in 1989 and positively correlated with the probability of displacement ($+0.010^{***}$). The variance of individual effects is only approximately 9.6% of the total variance of the crime dependent variable ($0.061ppt/0.636ppt$), suggesting that time-varying unobservables are a larger driver of crime than constant unobservables. $Time_{t=y-y_0}$ controls for pre- and post-displacement trends in the crime rate. They typically capture the secular decline in the crime rate throughout the 1990s. There are 270 $Municipality_{m(i,t)}$ fixed effects.[14] They control for the existence of municipality-level confounders such as spatial differences in police force numbers or police tactics; 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. Workers who move across municipalities identify both $Individual_i$ and $Municipality_m$. Regressions with $Municipality \times Year$ fixed effects yield similar results (Appendix Table D). $\mathbf{x_{it}}$ includes age and age squared.[15] Residuals ε_{it} are clustered at the individual i and base firm level, the firm employing the worker at $t = -1$. Results accounting for individual-level autocorrelation of errors yield similar results.

4.2 Key Results

The Impact of Job Displacement on Crime

Table 5 presents this paper's first set of key results following specification (3). All regressions in this table include time, municipal, and individual fixed effects. Columns (1)–(2) present the impact of job displacement on property or violent crime, denoted $Crime_{it} = 1$. Columns (3)–(4) are for property crime, columns (5)–(6) for violent crime, and columns (7)–(8) are for DUI offenses. Odd columns, (1), (3), (5), and (7), report the annual impacts δ_k, while even-numbered columns, (2), (4),

[14] 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.

[15] Including additional covariates such as education (Lochner & Moretti 2004, Machin et al. 2011, Hjalmarsson et al. 2015), marital status, and family size (Farrington 1986) yields similar results.

(6), and (8), report cumulative impacts, measuring the impact of displacement on the probability of committing crime in any of k years post-displacement. All specifications feature the same 154,694 individuals over the 17 years of the balanced sample of Danish workers described in Section (3). Point estimates reflect increases in probability relative to the year prior to displacement.

The table suggests that the probability of committing any crime increases by 0.57 ppt in the year of displacement and by 0.39 ppt the year following job displacement. The effect represents approximately $0.57/1.8 = 32\%$ of the average probability of a conviction in the population 19–39 in 1989. This 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 property crime in the year of displacement is 0.54 ppt (column (3)), with no discernible impact on the probability of committing a violent crime (column (5)) and a nonsignificant impact on the probability of a DUI crime (+0.32 ppt). Additionally, cumulative effects presented in even-numbered columns show long-lasting increases in property crime following displacement, with no discernible long-term impact for violent or DUI crimes. Appendix Figure (B) suggests that for displaced workers, these property crimes are most commonly 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.

Pre-Displacement Trends in Crime

The table's pre-displacement placebo coefficients δ_{-5} to δ_{-2} are jointly insignificant when testing, crime type by crime type, whether all placebo coefficients are jointly equal to zero. F-statistics range from 0.1 (p=0.98) for any crime to 0.9 (p=0.46) for violent crime. In the years leading up to a mass-layoff event, displaced workers' propensity to commit crime is not statistically different from what national trends suggest (captured by the year fixed effects) and also not different from the crime rate in the year before displacement.

Post-Displacement Patterns: Noise or Genuine Responses?

Figure 2 plots the coefficients. It suggests a spike in crime in the year of displacement, then 3 to 4 years after displacement, and again 7 years after displacement. Section 5 will use sudden unemployment reforms to explain the timing of these patterns, but here a first key concern is that these patterns might be statistical noise rather than the consequence of genuine economic incentives to commit crime in +4 and +7. We thus design three statistical tests: a test of joint significance, assessing whether all post-displacement are equal to zero; a test of joint equality, assessing whether the estimates are not different from a constant, flat impact of displacement on crime throughout years +0–+7; and finally, a test of monotonicity, to assess whether the estimates are not different from a simple straight declining probability of committing crime. Formally,

$$\begin{aligned} \text{Joint Significance:} \quad & H_0: \quad \delta_{+0} = \delta_{+1} = \cdots = \delta_{+7} = 0 \\ \text{Joint Equality:} \quad & H_0: \quad \delta_{+0} = \delta_{+1} = \cdots = \delta_{+7} \\ \text{Linear Monotonicity:} \quad & H_0: \quad \delta_{+k} = Constant + Slope \cdot k \end{aligned}$$

The first two tests performed are F-tests, the statistics of which are presented in the upper panel of Table 6. Given the number of degrees of freedom, both p-values are substantially below 0.01, implying that the null hypothesis is rejected at 1%. All p-values are computed with the two-way, base-year firm and individual clustering. The third test is performed by running specification (3) with three sets of covariates: (i) a post-displacement constant, (ii) a time trend, (iii) deviations from such time trend, as each of the individual post-displacement indicator variables. If the paper's main series of coefficients is best represented by a linear decreasing time trend, none of the indicator variables that measure deviation from the time trend will be significant. This is not the case, as four annual indicator variables are statistically significant. This suggests that a linear trend for post-displacement crime is not a sufficient statistic for the results. Thus, the non-monotonic patterns, including the spike in year +4, depicted in Figure 2 arguably reflect genuine shifts in crime propensities.

Job Displacement and Recidivism

The impact of job displacement on crime could be driven by separate individuals or repeat offenders. In particular, it might be that job displacement creates a new set of chronic offenders: displacement could permanently induce a worker to shift from seeking employment opportunities in the formal sector to opportunities in the criminal sector. However, if individuals become "career criminals" (Weis 1986), who learn to avoid apprehension after their first offense, we should observe little recidivism.

To isolate the effect of job displacement on recidivism, Appendix Table E reestimates the baseline specification in 3 by altering the dependent variable to estimate the impact of displacement on the first instance of crime. $FirstCrime_{it}$ is equal to one when $t = t^{first}$, where t^{first} is the time period an individual first commits a crime post-displacement and equal to zero for any subsequent crime. Pre-displacement crime remains unchanged compared to the $Crime_{it}$ variable. The impact of displacement on crime in the year of displacement also remains unchanged by construction.

Column (1) of Table E suggests that for any crime, focusing on the first crime dampens the crime spike in year +4. In contrast, for property crime in column (2), the crime spike in +4 remains statistically significant at the 5% level. Comparing the magnitude of the +4 spike in property crime in the baseline Table 5 (+0.0042) to the +4 spike in Table E (+0.0036) suggests that, at least for property crime, most of the spike is driven by first-time offenders. Given the possibility that repeat offenders are more skilled at avoiding apprehension, the baseline results may be interpreted as underestimates of the true impact of displacement on crime for all years following the initial displacement year.

Robustness

Four sets of identification concerns can be addressed at this stage. First, the focus on high-tenure workers (3+ years of continuous employment in the firm) may jeopardize the external validity of our findings, if high-tenure workers have invested in more firm-specific human capital and thus commit more crime after a job separation. Appendix Table M interacts post-displacement crime with the tenure length. We also split the sample into two subsets of tenure ranges with equal numbers of workers. In both cases, there is no evidence of a specific relationship between tenure length and post-displacement crime. The second identification concern is that early leavers may anticipate the mass layoff. Appendix Section B.1 suggests this does not explain the paper's main results. A third concern is one of reverse causality: crime in the year of displacement may cause layoffs in the same period. Appendix Section B.2 designs a robustness check that yields similar results. A final concern is that workers may anticipate mass layoffs if a firm's employment is downward trending. Results in Appendix Figure C reclassify a displaced worker as not displaced when his firm does not satisfy the firm-specific trend criteria described in Section 3.1. Results are unchanged when eliminating predictable mass layoffs.

4.3 Plant Closings

Important contributions in the job displacement literature use plant closings rather than firm downsizings as mass-layoff events (Browning et al. 2006, Eliason & Storrie 2006, Oreopoulos et al. 2008, Rege et al. 2011, Del Bono et al. 2012). When a *firm* downsizes, managers may have discretion regarding which workers to lay off. Requiring a larger decline in firm size alleviates partially, but not fully, such concerns over worker selection. Considering plant closings, where firms lay off all workers at a given physical location, may restrict the scope of endogenous selection. A unique feature of the Danish employer-employee data set is that it includes both plant and firm identifiers and thus offers an opportunity to compare the estimates of each of the two approaches. Appendix Section C constructs a plant closing sample that eliminates false or early closings, administrative code changes, and mass transfers of workers from one physical location to another. Figure 1 in this paper presents the displacement rate thus obtained, which is slightly higher than the displacement rate with the baseline approaches. Appendix Table N presents results on property crime that are similar to the paper's main baseline results.

5 Unemployment Insurance Reforms and Post-Displacement Crime

5.1 Danish Labor Market Reforms throughout the 1990s

Workers displaced in any year between 1990 and 1994 experienced a series of reforms to the UI system. The key reform is the 1993 Act on an Active Labor Market, which took effect at the beginning of 1994.

Unemployment Insurance in 1990

Prior to this 1993 reform, a member of an unemployment fund who was displaced could claim UI benefits for a maximum of 9 years (Jespersen et al. 2008). A worker was able to reach this 9-year maximum through occasional participation in job-offer, education, or job-training schemes. These light programs enabled an individual to extend his passive unemployment benefit period. Workers received benefits both during and after these schemes.

The legal framework is described by law Lbk 510 of July 19, 1989. The law specifies workers' eligibility for unemployment benefits: 26 weeks of full-time employment in the last three years (Chapter 10, Section 55) and a minimum of one year of membership in an unemployment insurance fund (Chapter 10, Section 53, §1). The replacement rate is high, at 90% (Chapter 9, Section 51, §1), with a maximum of 2454 DKK per week. The associated annual maximum was about 124,488 DKK per year, or 46% of the median wage (cf. descriptive statistics in Appendix Table A). The law does not formally set a maximum duration for benefits, which is deferred to a joint decision of the National Labor Board (*Landsarbejdsnævnet*) and the Director of Unemployment Insurance (*Arbejdsdirektøren*) (Section 55, §5, and Sec 59, §1).

Unsurprisingly, Madsen (1999) reports that job offer and training schemes were unsuccessful at promoting labor market participation. Denmark consistently ranks low on the strictness of job search monitoring and the lowest in the strictness of sanctions for refusing a job among OECD countries (Venn 2012, figure 4, p.18). The 1989 law spells out conditions for benefit receipt as follows: workers should not turn down "appropriate work," and the member should have "sufficient ground" to refuse the employment offer (Section 63, §1). The law also provides that if the member has retained an important connection to his previous field of study, it is important to preserve this connection (Section 65). Empirically, such conditionality rules seem not to be a significant obstacle to long periods of unemployment insurance receipt. Unemployment funds were in deficit throughout the period 1989–1993: their expenditures were between 3 and 3.6 times their total income from contributions (Danish Ministry of Labor 1994). As the national unemployment rate reached levels above 11% (Figure 1), political pressure to reform UI policies increased.

The 1993 Act on An Active Labor Market

A key reform, the Act on an Active Labor Market, at the end of 1993, introduced a significant change in the duration of benefits (Lov 434 of *June* 30, 1993). The reform formalized the notion of separate passive and active benefit periods, 'subperiods' 1 and 2, respectively (Lov 434, Chapters 11 and 12), yet at the time of passage, workers were unaware what these subperiods would entail until the passage of the Executive Order on Unemployment Insurance in January 1994. This Executive Order established a combined maximum benefit duration of 7 years: 4 years in the passive period and 3 years in the active period (Lbk 16 of January 11, 1994, Section 55, §1 and §2, respectively).

During the passive period, the worker is entitled to the maximum unemployment benefit of 90% of previous earnings, with a maximum of 2635 DKK per week, or 132,340 DKK per annum. At the beginning of the active period, a binding individual action plan (*handlingsplan*) is proposed, which is intended to improve opportunities for permanent employment. This includes job training opportunities and participation in education, including specially organized education programs. These reforms also removed the possibility that activation measures can extend the benefit period through regained eligibility and strengthen the formal requirements for activation. When income is earned through the individual action plan, the amount of unemployment benefits is reduced by the same amount (Chapters 4–6 of lov 434). Importantly, the wording of the Act is significantly different from previous laws: in the active period, the unemployed worker *"must*, as far as possible, have employment opportunities for an average of 20 hours a week." This shift in wording is the "right and duty" principle: individuals have a right to unemployment benefits but also have a duty to participate in activation measures with a willingness to seek and accept work. At the end of the 3-year active period, the individual loses the right to unemployment benefits unless he/she completes 26 weeks of full-time employment. The worker is then eligible for SA.

Finally, at the start of 1996, the Executive Order on Unemployment Insurance was amended to reduce the length of the passive period to 2 years (Lbk 29 on January 23, Section 55 replaces Lbk 16, same section).

5.2 Using the Timing of Reforms to Identify the Impact of Unemployment Insurance on Crime

The two UI reforms of 1993 and 1996 provide shifts in the duration of passive and active benefit periods that can be used to identify the impact of the reforms on the propensity to commit crime. The identification strategy relies on two essential features: (i) the potential duration of benefits was reduced by the 1993 and the 1996 reforms, and (ii) the transition to active benefits was unanticipated by workers displaced in 1990 or in 1993.

Figure 4 describes the impact of the two reforms in 1993 and 1996 on workers' rights to benefits after displacement. Importantly, the laws described in the previous subsection became effective immediately, affected all workers in unemployment, and differentially impacted different displacement cohorts. Each row of the figure is a displacement cohort, from 1990 (bottom row) up to 1994 (top row). The different shades of blue in Figure 4 correspond to the 5 periods relevant to a displaced worker: (i) the pre-displacement period, (ii) the passive UI period prior to the 1993 reform, (iii) the passive UI period after the passage of the 1993 reform, (iv) the active UI period, and (v) the SA period. We follow each cohort from -5 years pre-displacement to $+7$ years post-displacement.

Following job displacement, the passive period lasts 4 years for workers displaced in 1990, 1991, or 1992. For the 1990 cohort, the 1993 law suddenly ended passive benefits (dashed line), and workers transitioned to the active period. The 1993 cohort enjoyed up to 3 years of passive benefits, but as the 1996 reform (dotted line) reduced passive benefit eligibility to 2 years, they suddenly transitioned into active benefits. Hence, both the 1990 and 1993 cohorts unexpectedly transitioned to active benefits. The 1994 cohort is the first cohort whose passive and active periods match the lengths prescribed by the 1996 reform: 2 years of passive benefits followed by 3 years of active benefits. When eligibility for active benefits expires, workers are eligible to transition to SA.

The spike in crime at $+4$ seen in the baseline results comes at the end of passive benefits for the 1990–1992 displacement cohorts, suggesting that reform Lov 434 of 1993 may have increased crime. We formalize the identification strategy using six indicator variables for each benefit period (passive, active, SA) and each transition between these three periods. For instance, the variable $\mathbf{1}(\mathit{Employment} \rightarrow \mathit{Passive})$ is equal to 1 in the year when the worker is displaced, i.e. transitions from employment to passive benefits. In this transition, the worker experiences a decline in total income, and receives benefits without conditionality. At the transition from passive to active benefits, $\mathbf{1}(\mathit{Passive} \rightarrow \mathit{Active})$, the individual starts the individual action plan. At the transition from active benefits to SA, $\mathbf{1}(\mathit{Active} \rightarrow \mathit{Social\ Assistance})$, the individual experiences a sudden decline in welfare transfers and the end of the individual action plan.

The identification strategy uses *potential* benefit periods instead of the *actual* time spent in each unemployment period. The latter would be endogenous to the worker's unobservables correlated with both employment and the propensity to commit crime. Indeed, workers can flow back to employment, and re-join the pool of unemployed workers in later years. Using the potential length of periods instead of the actual length in unemployment exploits the exogenous timing set by law. The specification identifies the impact of each unemployment insurance period by estimating the impact of the transitions and the periods using all displacement cohorts:

$$
\begin{aligned}
Crime_{it} \quad = \quad & \delta_{e\to p}\mathbf{1}(Employment \to Passive) + \delta_p\mathbf{1}(Passive) \\
& +\delta_{p\to a}\mathbf{1}(Passive \to Active) + \delta_a\mathbf{1}(Active) \\
& +\delta_{a\to sa}\mathbf{1}(Active \to Social\ Assistance) + \delta_{sa}\mathbf{1}(Social\ Assistance) \\
& +Individual_i + Time_{t=y-y_0} + Municipality_{m(i,t)} + \mathbf{x}_{it}\beta + \varepsilon_{it}, \qquad (4)
\end{aligned}
$$

where the specification is otherwise the same as in the baseline approach.

The results are presented in Table 7. We focus here on property crime. Column (1) suggests that crime increases at the transition between employment and passive benefits (as expected), during passive benefits, and at each transition between periods but not during active benefits. Thus, the spike in crime at +4 in the main results is likely explained by the transition from passive to active benefits: the magnitude of the spike in crime at the transition between passive and active benefits is similar to the spike in crime at +4 in the baseline results table.

The spikes in crime at the transition from employment to passive benefits and at the transition from active to SA are in-line with intuition: workers experience a potentially significant loss of earnings at these 2 transitions. In addition, the spikes in crime at these 2 transitions are robust: they occur for each of the 1990–1994 displacement cohorts.

The transition between passive and active benefits is of a different nature. Indeed, column (2) suggests that the spike at the transition between passive and active benefits is only present for the 1990 displacement cohort, for whom this change was sudden and unanticipated. For this cohort, the effect is 5 times the baseline effect: $+1.55ppt$ compared to $+0.34ppt$. This spike in crime at the transition between passive and active benefits is perhaps surprising, as the worker experiences no loss of income and the beginning of his individual action plan. The next subsection suggests that this spike was an unintended consequence of the Act on an Active Labor Market of 1993.

5.3 Unintended Consequences of the 1993 Act on An Active Labor Market

The 1993 Act on an Active Labor Market (Lov 434) and the 1994 Executive Order on Unemployment Insurance (Lbk 16) set the duration of the passive period to 4 years. Crucially, they affect all workers, regardless of the date at which they were laid off. This leads to unintended consequences for workers who are nearing the end of a passive unemployment period. Workers in the 1990 displacement cohort who stayed in unemployment throughout faced the unexpected end of their passive benefits in 1994. Such workers who, at the time of the Lbk 16, had accumulated 25 weeks of full-time employment or less had an incentive to exhaust their remaining eligibility for full unemployment benefits (for 25 weeks or more) at the previous replacement rate set according to their earnings in 1989. Indeed, workers displaced in 1990 with 25 weeks of full-time employment who stayed in employment would then be eligible for a new unemployment spell based on their new, and on average lower, earnings.

To test this hypothesis, we use a discontinuity design at the 25-week eligibility limit. In column (3) of Table 7, the indicator variable $\mathbf{1}(Passive \rightarrow Active)$ is interacted with three indicator variables for each of 3 bins of weeks of full-time employment in the last 3 years: between 0 and 25 weeks, between 26 weeks and 105 weeks, and between 104 weeks and 250 weeks. The second and third bins are defined based on the new 52-week requirement set by law in 1997. Column (3) presents evidence that the spike in crime at the transition between passive and active benefits is almost entirely driven by individuals with 25 weeks or less of full-time employment in the last three years.

Appendix Figure D presents evidence of large flows back to full-time unemployment for workers in the 1990 displacement cohort when the Act on an Active Labor Market was implemented in 1994. For each of the displacement cohorts, the figure presents the share of workers who received unemployment benefits for more than 80% of their year (the vertical axis), in each of their post-displacement years (the horizontal axis). For the 1991–1994 displacement cohorts, workers flow back gradually to employment: the fraction of workers receiving benefits for more than 80% of the year declines uniformly from 10–15% in year +1 to 1–3% in year +7. For the 1990 displacement cohort, the pattern is different. The cohort faced an unexpected end to its passive benefits and experienced a temporary yet large spike in unemployment in year +4. The spike doubles the 1990 cohort's share in unemployment compared to the 1991 cohort and multiplies it by more than 2.5 compared to the 1992–1994 cohorts.

6 Labor Earnings, Benefits, and Crime

The previous analysis estimated the reduced form impact of job displacement and UI on crime. This section focuses on the impact of labor earnings losses and whether unemployment benefit transfers offset part of their impact on crime.

6.1 Labor Earnings Losses and Crime

Prior literature has provided extensive evidence of the impact of job displacement on short- and long-run labor earnings losses in the United States, starting with Jacobson et al. (1993) and most recently in Lachowska et al. (2018). As in the United States, we observe substantial and long-lasting negative impacts of displacement on labor earnings. Appendix Table F, column (1) shows that individual labor earnings fall by 50.2% in the year of displacement. The impacts are statistically significant at the 1% level, largest one year after displacement (-53.5%) and long lasting: after seven years, the annual earnings losses are 22.3% of pre-displacement earnings.

Figure 5 presents the cross-sectional relationship between crime and labor income in the year following displacement, as in Sullivan & von Wachter (2009). The estimates are obtained as follows. First, we consider the sample of displaced workers in the year after and in the year prior to displacement. Second, the sample is split into 15 equal-sized bins of increasing labor earnings. Finally, we run a regression of crime on each of these indicator variables, a worker fixed effect, and a year fixed effect. As the regression includes observations for the year prior to displacement, the regression conditions on prior labor income. Residuals are here, and throughout Section 6, clustered as in the baseline analysis at the individual and base-firm levels. The figure presents the 15 coefficients of the labor income bins with 95% confidence intervals. The figure suggests that higher post-displacement labor income is correlated with lower crime: moving from the 20th to the 80th percentile of labor earnings corresponds to a 0.7 ppt decline in the probability of committing crime.

Figure 6 presents a longitudinal comparison of changes in year-to-year log labor income (solid red line, left axis) and the propensity to commit crime (dashed line, right axis, as estimated in the paper's baseline section 4.2). Interestingly, the peak of crime in +0 and trough of crime in +2 match the peaks and troughs in year-to-year labor earnings changes. The blue line represents the 1990 cohort. Workers in the 1990 displacement cohort flow back to unemployment in +4 (Section 5.3), with no change in labor income, when other displacement cohorts (red line) see an increase in labor earnings.

6.2 Benefits, Total Income, and Crime

A remaining question is to what extent do benefits mitigate earnings losses and thus the impact of job displacement on crime. To answer this, we construct an exogenous predictor of unemployment benefits and use this predictor in the next subsection to separately estimate the impacts of labor earnings and transfers on crime.

The formula determining UI benefits links pre-job loss earnings to benefits. It is set each year by law (chapter 9, §47 and §51 of the Employment and Unemployment Insurance Act of each year). The formula is strictly followed and similar to that described by Card et al. (2015) in Missouri. The amount b_{it} of benefits is 90% of the previous earnings, with a maximum cap set by law:

$$b_{iy} = \min\{90\% \times earnings_{i,y_0(i)-1}, maximum_y\}, \tag{5}$$

where $y_0(i)$ is worker i's displacement year. $maximum_y$ is the annual maximum amount of unemployment benefits in year y: 127,608 DKK in 1990–1991, 132,600 DKK in 1992, 137,020 DKK in 1993, and 132,340 DKK in 1994–1996. As laws are voted on at different points of the year, we apportion each maximum value according to the number of months of the year during which a law applies. The replacement rate of 90% does not change throughout the period.[16]

Individuals ineligible for UI benefits may be eligible for means-tested SA payments. The level of payments depends on capital income, spousal earnings, and whether the household has children. Parsons et al. (2015) formalizes the provisions of the law from 1994 onwards as reported in Appendix E. Despite the publication of a formal formula for SA, welfare officers have substantial discretion over the awarded level of SA for a specific case. Unlike UI benefits, the level of SA may differ significantly from the formulated amount.

Appendix Table F suggests that benefits offset a sizable portion of the earnings losses from displacement. Workers lose 50% of labor income in the displacement year (column 1), while total

[16] A reduced 80% replacement rate applies for workers in unemployment after vocational education, but it does not apply to this paper's sample as we eliminate individuals in education prior to displacement.

income—the sum of labor earnings, all public transfers (e.g. SA, UI, sickness/disability benefit, early retirement, pensions), and any residual income—declines by only 13% (column 3). This is explained by the results in column (2) of Appendix Table F, which shows that non-labor income—all public transfers and the residual income —is multiplied by 2.4 in the year of displacement. Appendix Figure E suggests that year-to-year changes in non-labor income almost fully compensate year-to-year changes in labor income over the post-displacement period.

While we could use both the formulas for UI benefits (equation (5)) and for SA transfers (Appendix equation E) to assess how much of non-labor income is explained by those two components, the paper predicts benefits using the UI formula. UI benefits are the main public transfer income spanning the post-displacement period, and the formula governing UI benefits is a strict criterion.

The paper predicts non-labor income in the 2 years after displacement. Two years after displacement, all workers eligible for UI have not yet transitioned to SA, and during this passive benefit period, all cohorts are similarly unimpacted by activation measures. When regressing non-labor income on the predicted benefits formula in (5) and its interaction with a post-displacement indicator, predicted benefits strongly and significantly predict non-labor income. The coefficient of the regression of non-labor income on predicted UI benefits is strongly significant, and this first-stage regression has an F-statistic of 687. Non-labor income is thus likely a reasonable measure of welfare transfers when examining the time period close to displacement.

As public transfers account for a large share of annual changes in non-labor income, Denmark's welfare system smooths large drops in labor income. However, the spikes in crime match the spikes in non-labor income and the troughs in labor income. Despite the role of Denmark's relatively generous welfare system in reducing income losses, there remains a strong correlation between labor earnings losses and crime. This strong correlation exists in both the cross-section and longitudinal dimensions, suggesting a small mitigating impact of benefits on crime. The next section presents an identification method to estimate this mitigating impact.

6.3 The Impact of Labor Earnings and Welfare Benefits on Crime

The previous subsections suggest that higher labor income is correlated with lower crime, and that non-labor income is *positively* correlated with crime. There are at least two issues when interpreting these correlations. First, as labor income drops, UI (or SA) benefits increase. Thus, benefits are an omitted variable when regressing crime on labor earnings. Such omitted variable bias leads to an underestimation of the magnitude of the impact of labor earnings on crime. Second, observed benefits are endogenous to the individual's pre-displacement earnings. High benefit payments are received by individuals with high pre-displacement earnings, and high benefits may be correlated with positive unobservable traits that lead to a lower propensity to commit crime. In this case, the impact of benefits on crime may be overestimated.

We estimate the impact of labor and non-labor income on crime in an IV specification as follows. Labor earnings are instrumented by the series of pre- and post-displacement event indicator variables. As described previously, there is a strong first-stage impact of displacement event indicators on labor income, with a first-stage F-statistic of 184. In a simple Beckerian framework, the displacement event has an impact on crime solely through its impact on labor income. If that is the case, the exclusion restriction will be satisfied. However, job loss may have an impact on crime over and above its indirect impact through the decline in earnings; for instance, prior literature has emphasized the scarring impact of unemployment (Clark et al. 2001), and, separately, established a link between idleness and crime (Jacob & Lefgren 2003, Dix-Carneiro et al. 2018). In this case, the estimated impact of income on crime in two-stage least squares is likely an upper bound of the causal impact of labor earnings on crime.

Non-labor income is predicted using the formulas b_{iy} of the Acts on Unemployment Insurance presented in equation 5. When conditioning on the worker fixed effect and thus on pre-displacement earnings, variations in b_{iy} are driven by shifts in the legally prescribed maximum amount of benefits. Workers in-between the lowest maximum $\min_{y=1990,1995}\{maximum_y\}$ and the greatest maximum $\max_{y=1990,1995}\{maximum_y\}$ provide the source of variation that identifies the impact of unemployment benefits on crime. In other words, the estimation compares individuals with similar pre-displacement labor earnings but in different displacement cohorts and who end up earning different levels of benefits. As described previously, the coefficient on predicted benefits is significant, with a first stage F-statistic of 687.

The two-stage least squares regression is similar to the baseline displacement regression :

$$\begin{aligned} Crime_{it} = \delta \cdot \log Labor\ Earnings_{it} + \gamma \cdot \log b_{i,y_0(i)+t} \\ + Individual_i + Municipality \times Time_{m(i,t),t} + \varepsilon_{it} \end{aligned} \tag{6}$$

where i is the worker, $t = -5, \ldots, +7$ is the year relative to displacement $t = y - y_0(i)$, and $m(i,t)$ is the municipality of worker i. Predicted benefits $b_{i,y_{0(i)}+t}$ in year $y = y_0(i) + t$ are determined using pre-displacement labor earnings $Labor\ Earnings_{i,t=-1}$ and the formula (5). The specification includes municipality x time fixed effects to capture year-specific municipality-wide shocks to the demand for labor.

Table 8, column (1) displays the OLS regression with labor earnings. Given the loss of labor income due to displacement (−50.2%), the estimated coefficient of −0.0017 would imply a +0.09 ppt post-displacement increase in crime, approximately 18% of the estimated effect of displacement on crime. Column (2) presents the IV estimation. A 100% loss of labor income would lead to a 0.78 ppt increase in crime, an effect size roughly equal to the baseline impact of displacement on crime given the observed earnings losses in each period.

Such an estimate is confounded by the lack of a control for increasing non-labor income. Indeed, the confounded estimate of column (3) suggests a *positive* correlation between non-labor income and crime. Column (4) is the regression of crime on endogenous log total income. Given the post-displacement loss of total income (−10%), the estimated coefficient implies that income losses predict a 0.02 ppt increase in crime, only a small share (4%) of the impact of displacement on crime (+0.5ppt). In the IV estimation (column (5)), the impact of total income on crime predicts that a 10% decline in total income translates into a 0.11 ppt increase in crime.

Perhaps more important are the estimates in columns (6) and (7). These columns regress crime on covariates that include labor income and non-labor income separately. In column (6), these are the observed endogenous income levels. In column (7), labor income and non-labor income are predicted by their described instruments. We focus on the sample of observations in a $t = -5, ..., 0, +1$ window. Non-labor income has a small and non-significant impact on crime (column (7)). The impact of labor income on crime is imprecisely estimated given the strong correlation between labor and non-labor income and the high replacement rate. Taken together, these results suggest that non-labor income has, at best, a small mitigating impact on crime.

However, the previous analysis has abstracted from at least potential three concerns. Results in Appendix show that: (i) considering family income instead of individual income yields similar conclusions, suggesting small shifts in spousal labor supply (Appendix Table F); (ii) a comparison between individual and median municipal income plays no role, as the results defining income loss relative to municipality income are similar to results on earnings losses (column (1) Appendix Table G); and (iii) defining income loss relative to pre-displacement earnings also yields similar results as the level of income loss, suggesting little role for the comparison between current and past earnings (column (2) Appendix Table G).

7 Conclusion

Key papers have examined the potential virtues of Danish *flexicurity* (Algan & Cahuc 2006, Algan & Cahuc 2009) that combines low employment protection with generous unemployment benefits. This paper suggests that, at transitions between employment and passive benefits and between active benefits and social assistance, crime spikes, and generates an associated social cost. The paper also suggests that passive unemployment benefits are an imperfect substitute for labor income. Finally, policy reforms can have unintended short-run consequences for crime rates for cohorts that face sudden and unexpected incentives to switch back to unemployment.

Overall, the availability of matched employer-employee-unemployment-crime records should spark research at the intersection of labor search (Mortensen & Pissarides 1994), optimal unemployment insurance (Acemoglu & Shimer 1999) and the microeconomics of crime (Draca & Machin 2015, Bell et al. 2017). Such a *labor search theory of crime* calls for the estimation of the joint dynamics of crime, employment, and unemployment benefits. Exogenous shocks to labor demand, shifts or kinks in the effective marginal tax schedules can identify the distribution of the individual values of crime. Conversely, such estimated distributions of the values of crime could imply an optimal benefit and tax schedule.

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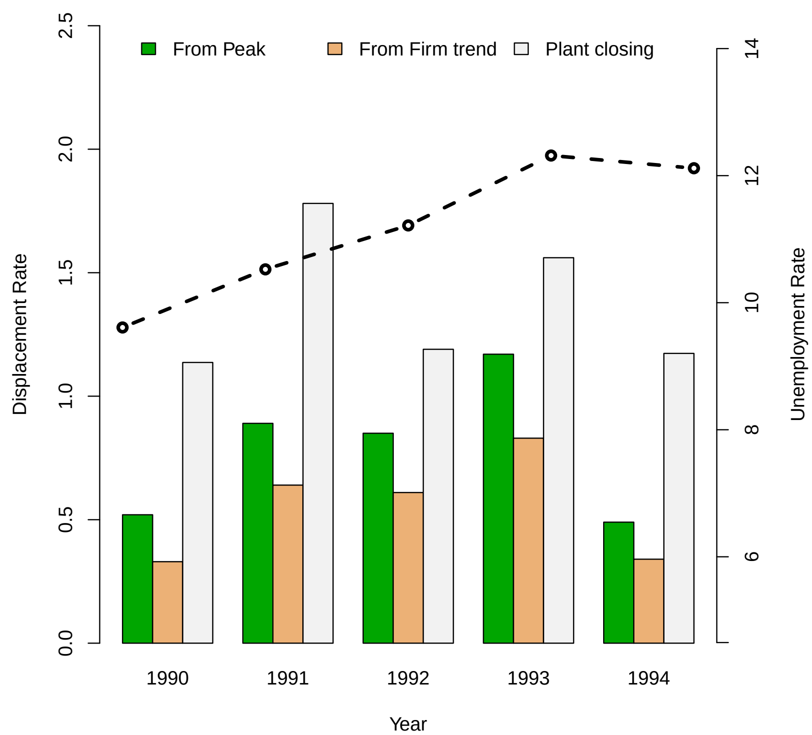
Figure 1: Displacement Rate, 1990–1994

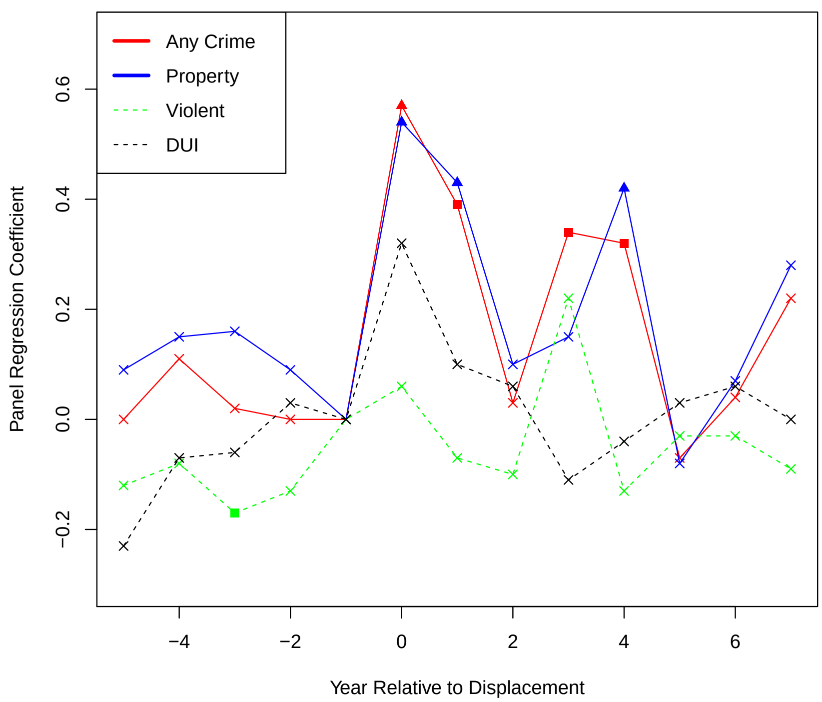
This figure presents the annual displacement rate between 1990 and 1994, using the three different definitions of mass layoffs considered in this paper: (i) for 30% firm downsizings relative to the previous 5 years' peak employment, (ii) for 30% firm downsizings relative to a firm-specific trend, and (iii) using plant closings. The displacement rate is the number of displaced workers as a share of the number of workers in each high-tenure sample, $y_0 = 1990, \ldots, 1994$ *(see Section 3.2).*



The average annual number of workers for either the peak approach or the firm trend approach is 79,498 per year, for a total of 397,486 over the 1990–1994 period. In the plant closing sample described in Section 4.3*, the worker sample has an average of 124,702 workers, for a total of 623,511 workers over the 1990–1994 period.*

Figure 2: Impacts of Displacement on Crime

The figure displays the coefficients of the panel regression estimating the impact of job displacement on crime (Table 5). The horizontal axis is the year $t = y - y_0$ *relative to displacement. The specification is described in Section 4. Each line corresponds to the coefficients of a separate regression, with different crime types as the 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 the 5 or 1% level. Coefficients are in percentage points: in the year of displacement* ($t = 0$) *job displacement increases the probability of property crime by 0.54 percentage points.*

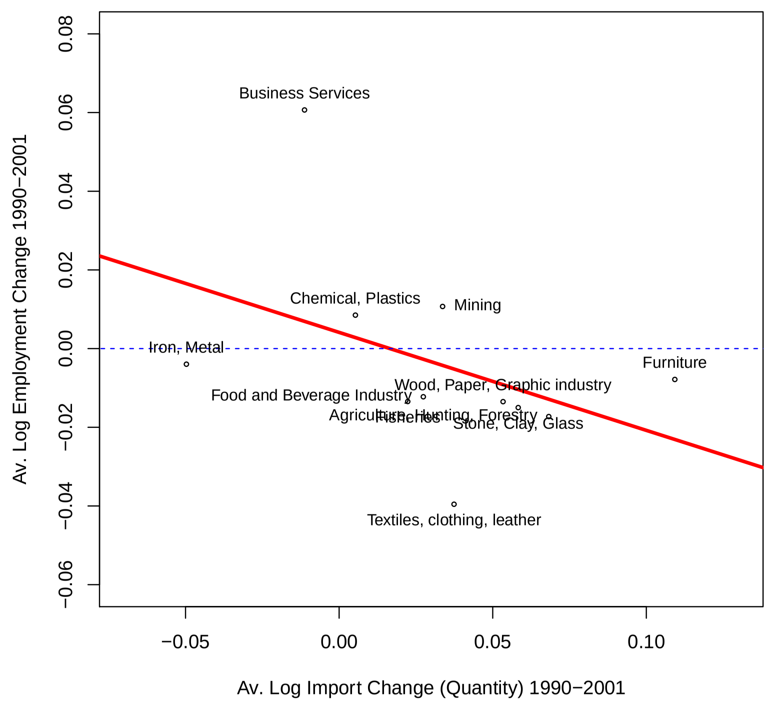


The coefficients' standard errors are displayed in Table 5. Table 6 presents statistical tests of joint equality, joint significance, and monotonicity of the coefficients. Coefficients significant at the 5% level are marked with a triangle. Coefficients significant at the 10% level are marked with a square.

Figure 3: Import Volumes, Import Prices and Employment Declines

At the 2-digit NACE industry level, these two figures relate the log employment change to the log change in import volume (subfigure (a)) and to the change in the implicit import prices. The implicit import price is the difference between the log volume and the log total value of imports. Both figures built using Danish employer-employee registry, UN Comtrade import data, and World Integrated Trade Solution product-to-industry crosswalks.

(a) Import Volumes and Employment Declines



(b) Implicit Import Prices and Employment Declines

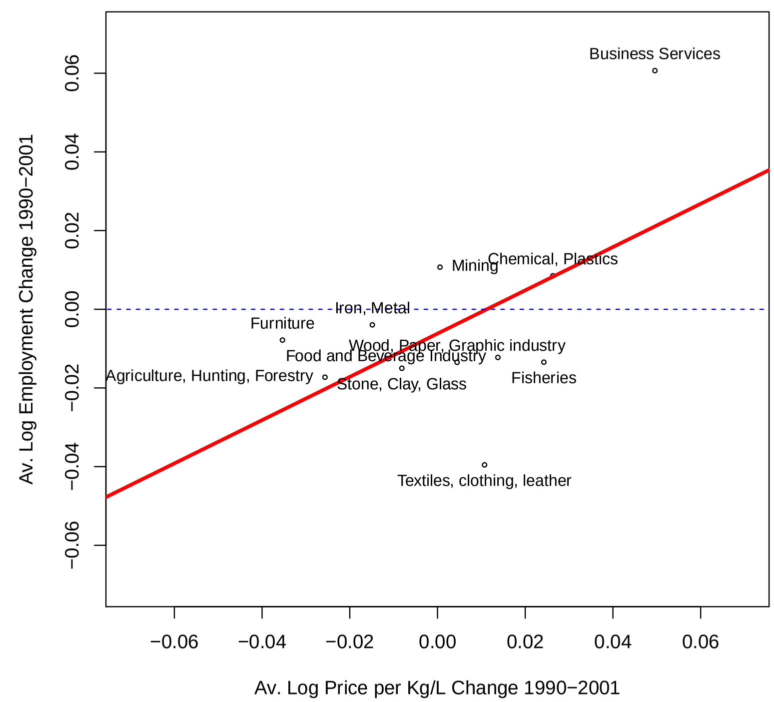


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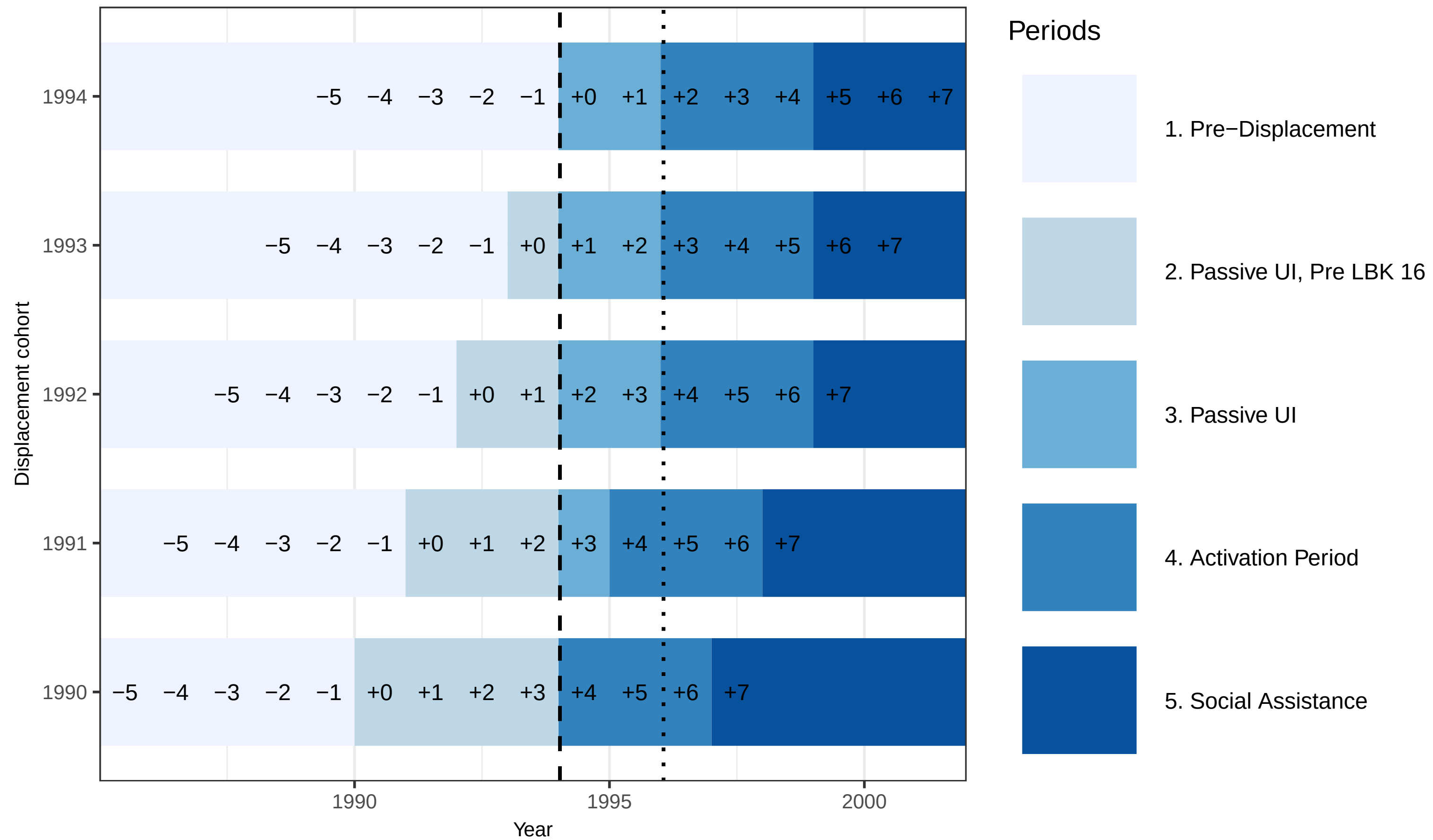
Figure 4: Cohort-to-Cohort Variation in Passive and Active Unemployment Benefits Duration

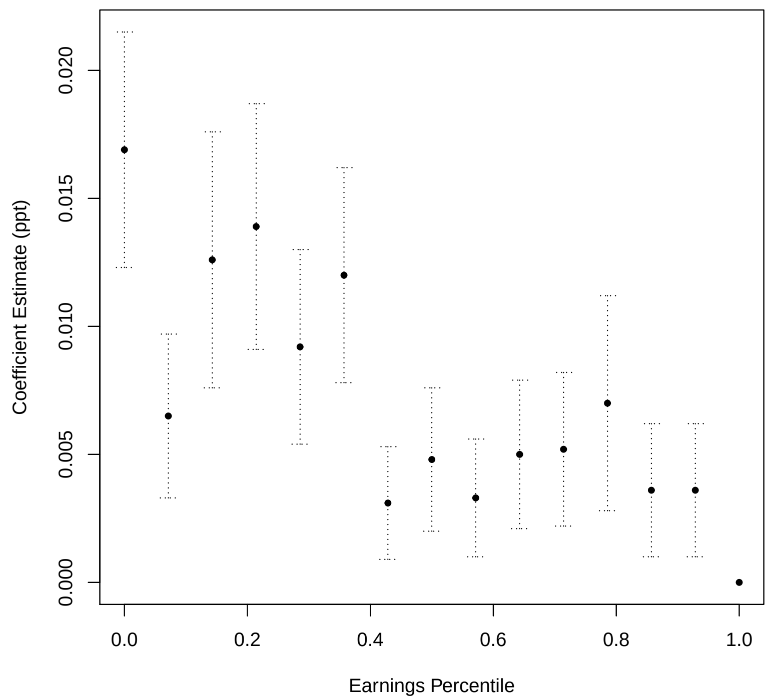
This table presents the impact of key laws and executive orders on the length of both passive and active unemployment benefits for each of the 1990–1994 displacement cohorts (vertical axis). First, the Act on an Active Labor Market, Law number 434 of June 30, 1993 introduces a passive unemployment benefit period and an activation period. It entered into force on September 1, 1993. Executive order LBK 16 of January 11, 1994 sets the duration of the first passive period to 4 years within 5 years (dashed line ‒ ‒ ‒) and the duration of the second active period to 3 years within 4. Second, amendment LBK 29 of January 23, 1996 reduces the length of the first period to 2 years within 3 (dotted line ·····).



The figure plots worker's rights to unemployment benefits and not all workers' potential employment histories. It is thus drawn assuming no reentry into employment. The figure also assumes that an unemployed worker does not choose to pause the receipt of benefits. For instance, the passive period could be extended to 5 years, i.e. with 4 years of actual benefits received within those 5 years. Finally, job loss occurs at the beginning of the year in the figure. UI: Unemployment Insurance.

Figure 5: Comparison of Earnings Losses and Crime across Workers

This figure assesses whether displaced workers with larger earnings losses had a higher propensity to commit crime. Using the sample of displaced workers, we regress crime on 15 bins of labor earnings in the first two years post-displacement, controlling for year and worker fixed effects. The plot below is a graphical representation of the regression coefficients for each of the 15 bins. The bands are 95% confidence intervals.



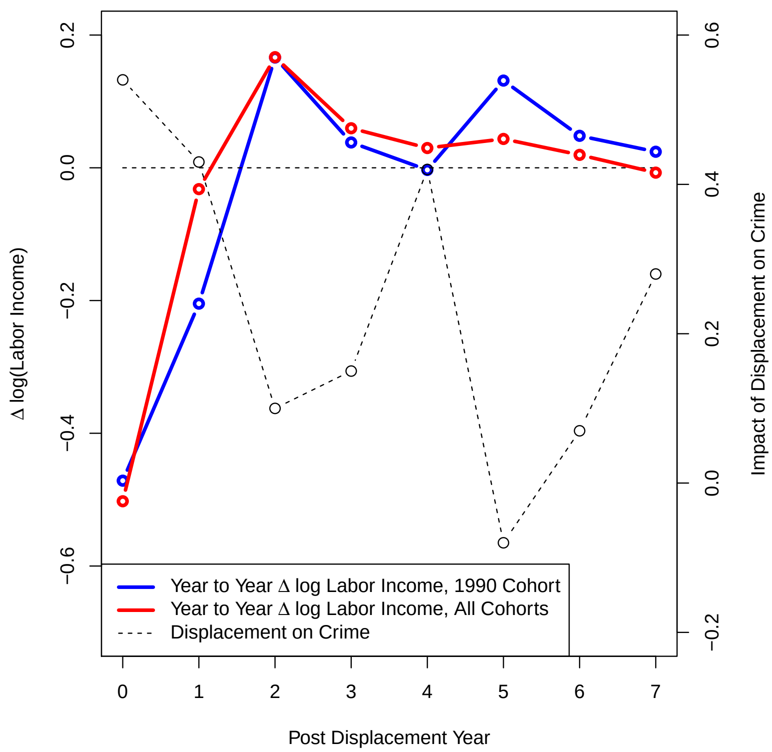
The regression is as follows:

$$Crime_{it} = \sum_{k=1}^{15} \gamma_{k\,quantile} \mathbf{1}(k - th\,percentile\,of\,earnings) + Individual_i + \mathbf{x}_{it}\beta + Time_{t=y-y_0} + \varepsilon_{it}.$$

estimated on displaced workers only. Each point is a $\gamma_{k\,quantile}$. *An instrumental variable identification strategy is presented in Table (8).*

Figure 6: Labor Income after Displacement – Year-to-Year Shifts

This figure plots the year-to-year percentage changes in labor income after displacement. The horizontal axis represents the year $t = y - y_0$ relative to displacement. The blue line represents for the cohort of workers displaced in 1990. The red line is for all cohorts, i.e., workers displaced at any point between 1990 and 1994 inclusive. The dashed line is the impact of displacement on crime (right axis) in percentage points, from Table 5.



The estimate is obtained by regressing log labor income on year-level indicator variables for each of the post-displacement years and taking the first difference of these coefficients. The regression is:

$$\log(Labor\ Income)_{it} = \sum_{k=-5}^{+7} \varphi_k \mathbf{1}(k \text{ years after displacement}) + Individual_i + \mathbf{x}_{it}\beta + Time_{t=y-y_0} + \varepsilon_{it}$$

and the reported coefficient (left axis) is $\varphi_k - \varphi_{k-1}$. The coefficient φ_{-1} is conventionally set to 0. The figure's points are thus net of individual and time fixed effects.

Table 1: Descriptive Statistics – Confounders of Unemployment and Crime

The table presents the correlation of the transition into unemployment separately with (i) a crime indicator variable and (ii) a range of individual observables. This suggests that a regression of crime on unemployment transitions would be confounded. The 8,830,448 observations are those of the comprehensive Danish registry including all workers regardless of tenure.

	(1)	(2)
	Correlation with	
Individual Observable:	Job Separation	Any Crime
Age	−0.084***	−0.039***
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***
Crime in Previous Year	+0.022***	–
Crime in Year $t-5$	+0.016***	–
Individual × Year Observations	8,830,448	

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

Table 2: Mass Layoffs and Industrial Structure

This table relates the total number of workers affected by mass layoffs in the 1990–1993 period of analysis to the initial industrial structure at the municipality-level. The predicted annual increase in log imports is established using a Bartik predictor that is the product of initial municipality level industry shares by manufacturing sector with the national log import growth. Imports are measured in USD values. Import values at the product level (HS) come from United Nations Comtrade data. Crosswalks between HS products and NACE industry codes come from the World Integrated Trade Solution (WITS).

	(1) Mass Layoffs per 1000 employees, 1990-1993	(2) Mass Layoffs per 1000 employees, 1990-1993	(3) Mass Layoffs per 1000 employees, 1990-1993
1989 Fraction in Finance and Insurance	1336.3*** (263.3)		
1989 Fraction in Manufacturing		2351.4*** (657.9)	
Predicted $\overline{\Delta \log(imports)}$			9796.9*** (2834.6)
R Squared	0.189	0.211	0.203
Municipalities	270	270	270
F Statistic	26	13	12
Mean of Dep. Variable	31.8	31.8	31.8

	(4) Mass Layoffs per 1000 employees, 1990-1993	(5) Mass Layoffs per 1000 employees, 1990-1993
1989 Fraction in Finance and Insurance	499.9 (607.4)	656.1 (524.3)
1989 Fraction in Manufacturing	1647.1** (811.5)	
Predicted $\overline{\Delta \log(imports)}$		6177.0*** (2875.2)
R Squared	0.219	0.221
Municipalities	270	270
F Statistic	11	12
Mean of Dep. Variable	31.8	31.8

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

Table 3: Identification Strategy – 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 in year t if the firm size is lower than 30 % of its peak between $y_0 - 5$ and $y_0 - 1$*, following Jacobson et al. (1993). Alternative definitions of mass layoffs are presented in Section 3.2.*

	(1)	(2)
Dependent variable:	Firm Size (employees)	
Subsample:	All firms	Including Closings
Year +5	−54.852***	−72.084***
	(19.098)	(15.904)
Year +4	−41.682*	−69.423***
	(23.921)	(17.749)
Year +3	−43.492*	−73.737***
	(23.061)	(17.729)
Year +2	−40.679*	−75.835***
	(22.461)	(17.819)
Year +1	−50.991***	−85.000***
	(17.385)	(16.024)
Mass-Layoff Year	−67.808***	−95.842***
	(16.998)	(16.318)
Year −1	Ref.	Ref.
Year −2	12.133	12.133
	(19.212)	(19.212)
Year −3	10.011	10.011
	(19.647)	(19.647)
Year −4	11.024	11.024
	(20.084)	(20.084)
Year −5	14.221	14.221
	(20.766)	(20.766)
Fixed Effects	Time Effects	
R Squared	0.001	0.001
Observations	326,157	371,887
Firms	9,382	9,382
Clustering	Firm	Firm
F Statistic	41	111
Mean of Dep. Variable	227	227

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

Table 4: Identification Strategy – Current Convictions of Future Displaced Workers

A 'Future displaced worker' is an individual observed in the period $y_0 - 5$ and $y_0 - 1$ *who will be displaced in any year* y_0 *between 1990 and 1994, inclusive. The placebo table below estimates whether, for such individuals, the* $y_0 - 5$ to $y_0 - 1$ *crime rate is significantly different than for workers who will* not *be displaced between 1990 and 1994. Crime is defined as equal to 1 if an individual commits an offense at time t and is convicted in the same or any subsequent year.*

	(1)	(2)	(3)	(4)	(5)
Subsample:	All Workers, 1985–1989				
Dependent:	Property Crime				
Dependent:	1990	1991	1992	1993	1994
Future Displaced Worker	−0.0000	0.0014	0.0017	0.0012	0.0007
	(0.0017)	(0.0014)	(0.0013)	(0.0011)	(0.0016)
Fixed Effects	Year, Municipality, Employer				
R Squared	0.015				
Observations	1,973,619				
F Statistic, joint significance	0.850				
p value, joint significance	0.517				
Mean of Dep. Variable	0.016				

	(1)	(2)	(3)	(4)	(5)
Subsample:	All Workers, 1985–1989				
Dependent:	Violent Crime				
Dependent:	1990	1991	1992	1993	1994
Future Displaced Worker	0.0005	−0.0001	0.0012	0.0005	0.0005
	(0.0008)	(0.0004)	(0.0009)	(0.0005)	(0.0008)
Fixed Effects	Year, Municipality, Employer				
R Squared	0.012				
Observations	1,973,619				
F Statistic, joint significance	0.660				
p value, joint significance	0.657				
Mean of Dep. Variable	0.003				

Standard errors twoway-clustered at the individual level and baseline firm.
Controls: age, tenure, annual wage, education, industry, number employees firm, marital status.
***: Significant at 1%, **: Significant at 5%, *: Significant at 10%.

Table 5: Baseline Estimation – Impact of Displacement on Crime

This table presents the main estimates of the impact of displacement on crime. Columns (1), (3), (5), and (7) present the impact of displacement on crime in year k only. Columns (2), (4), (6), and (8) present the cumulative impact of displacement for crimes committed at any point between the year of displacement ($k = 0$) and year k. Crime is defined as equal to 1 if an individual commits an offense at time t and is convicted in the same or any subsequent year.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent:	Any Crime		Property Crime		Violent Crime		D.U.I. Crime	
Coeff.:	Annual	Cumul.	Annual	Cumul.	Annual	Cumul.	Annual	Cumul.
Year +7	0.0022	0.0068	0.0028	0.0117	−0.0009	−0.0038	0.0000	−0.0045
	(0.0020)	(0.0105)	(0.0017)	(0.0089)	(0.0011)	(0.0069)	(0.0024)	(0.0146)
Year +6	0.0004	0.0067	0.0007	0.0105	−0.0003	−0.0056	0.0006	−0.0038
	(0.0018)	(0.0091)	(0.0015)	(0.0077)	(0.0012)	(0.0061)	(0.0022)	(0.0128)
Year +5	−0.0007	0.0078	−0.0008	0.0113*	−0.0003	−0.0020	0.0003	−0.0020
	(0.0016)	(0.0079)	(0.0014)	(0.0068)	(0.0011)	(0.0052)	(0.0024)	(0.0112)
Year +4	0.0032*	0.0100	0.0042**	0.0128**	−0.0013	−0.0012	−0.0004	−0.0005
	(0.0019)	(0.0068)	(0.0018)	(0.0058)	(0.0009)	(0.0044)	(0.0024)	(0.0094)
Year +3	0.0034*	0.0083	0.0015	0.0092*	0.0022	0.0001	−0.0011	0.0001
	(0.0020)	(0.0057)	(0.0015)	(0.0047)	(0.0015)	(0.0038)	(0.0021)	(0.0076)
Year +2	0.0003	0.0071	0.0010	0.0090**	−0.0010	−0.0018	0.0006	0.0032
	(0.0017)	(0.0044)	(0.0014)	(0.0038)	(0.0011)	(0.0028)	(0.0024)	(0.0061)
Year +1	0.0039*	0.0081**	0.0043**	0.0088***	−0.0007	−0.0005	0.0010	0.0040
	(0.0021)	(0.0035)	(0.0018)	(0.0031)	(0.0011)	(0.0020)	(0.0026)	(0.0043)
Disp. year	0.0057**	0.0057**	0.0054***	0.0054***	0.0006	0.0006	0.0032	0.0032
	(0.0022)	(0.0022)	(0.0020)	(0.0020)	(0.0011)	(0.0011)	(0.0026)	(0.0026)
Year −1	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Year −2	0.0000	-	0.0009	-	−0.0013	-	0.0003	-
	(0.0018)		(0.0015)		(0.0010)		(0.0024)	
Year −3	0.0002	-	0.0016	-	−0.0017*	-	−0.0006	-
	(0.0017)		(0.0015)		(0.0010)		(0.0023)	
Year −4	0.0011	-	0.0015	-	−0.0008	-	−0.0007	-
	(0.0020)		(0.0017)		(0.0011)		(0.0025)	
Year −5	0.0000	-	0.0009	-	−0.0012	-	−0.0023	-
	(0.0019)		(0.0016)		(0.0011)		(0.0024)	
Fixed Effects	Individual, Year and Municipality		Individual, Year and Municipality		Individual, Year and Municipality		Individual, Year and Municipality	
R Squared	0.114		0.112		0.093		0.101	
Observations	5,167,318		5,167,318		5,167,318		5,167,318	
Individuals	154,694		154,694		154,694		154,694	
F Statistic	14.912		14.591		2.358		12.579	
Mean of Dep. Variable	0.018		0.016		0.003		0.011	

Any Crime: Property or Violent Crime. DUI: Driving Under the Influence.
Disp. year: Displacement Year. Cumul.: Cumulative coefficient.

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

Table 6: Tests of Significance and Monotonicity – Impact of Displacement on Crime

Subtable (a) tests two statistical hypotheses regarding in the main baseline results of Table 5. The first hypothesis is the joint significance of such coefficient estimates. The second hypothesis is the joint equality of all of the 8 post-displacement coefficients $\delta_{+0}, \ldots, \delta_{+7}$. Subtable (b) reestimates the main baseline results with the addition of a post-displacement constant and a post-displacement time trend. Crime is defined as equal to 1 if an individual commits an offense at time t and is convicted in the same or any subsequent year.

(a) Tests of Joint Significance and Joint Equality

Null hypothesis	F Statistic	p-value
$\delta_0 = 0, \delta_1 = 0, \ldots \delta_{+7} = 0$	$F(8, 9381) = 2.75$	0.0050
$\delta_0 = \delta_1 = \cdots = \delta_{+7}$	$F(7, 9381) = 2.91$	0.0049

(b) Tests of Monotonicity

	Dependent variable: Property Crime	
Post-Displacement	0.0047**	(0.0022)
Time Trend	−0.0044**	(0.0018)
Post-Displacement × Time Trend	0.0003	(0.0004)
Year +7	−0.0035	(0.0022)
Year +6	−0.0053***	(0.0020)
Year +5	−0.0065***	(0.0019)
Year +4	−0.0014	(0.0023)
Year +3	−0.0040*	(0.0021)
Year +2	−0.0043**	(0.0021)
Year +1	−0.0009	(0.0022)
Displacement Year	-	-
Constant	−0.0988**	(0.0499)
Other controls	Worker f.e.	
Clustering	2-way Worker × Year	
R Squared	0.000	
Observations	5,167,318	
Individuals	154,694	
F Statistic	14.250	
Mean of Dep. Variable	0.016	

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

Table 7: Unemployment Insurance Policies and Crime – Transition across Welfare Benefit Regimes

This table presents the impact of displacement on the probability of committing crime along the successive unemployment benefit periods: passive benefits, active benefits, and social assistance. The length of these periods is described in Figure 4. Crime is defined as equal to 1 if an individual commits an offense at time t and is convicted in the same or any subsequent year.

	(1) Property Crime	(2) Property Crime	(3) Property Crime
Social Assistance	0.0028 (0.0021)	0.0026 (0.0021)	0.0028 (0.0021)
Transition Active Benefits → SA	0.0030* (0.0016)	0.0030* (0.0016)	0.0029* (0.0016)
Active Benefits	0.0003 (0.0013)	0.0003 (0.0013)	0.0003 (0.0013)
Transition Passive → Active Benefits	0.0034** (0.0017)		
× First Cohort Affected by 1993 Act	–	0.0155** (0.0069)	–
× Other Cohorts	–	0.0017 (0.0015)	–
× Weeks $\in [0, 26)$	–	–	0.0108* (0.0057)
× Weeks $\in [26, 104)$	–	–	0.0035 (0.0025)
× Weeks $\in [104, 250]$	–	–	0.0008 (0.0016)
Passive Benefits	0.0031** (0.0012)	0.0031** (0.0012)	0.0031** (0.0012)
Transition Employment → Passive Benefits	0.0056*** (0.0020)	0.0056*** (0.0020)	0.0056*** (0.0020)
Pre-displacement Year	Ref.	Ref.	Ref.
Fixed Effects	– Individual and Year Fixed Effects –		
R Squared	0.112	0.112	0.112
Observations	5,167,318	5,167,318	5,167,318
Individuals	154,694	154,694	154,694
F Statistic	15.601	15.105	14.492
Mean of Dep. Variable	0.016	0.016	0.016

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

Table 8: Earnings Losses and Crime

This table estimates the impact of labor and non-labor income on crime using (i) a least squares panel approach with worker and year fixed effects (columns (1), (3), (4), and (6)), (ii) by predicting labor earnings using pre- and post- displacement indicator variables as instruments (columns (2), (5), (7), and (8)), and (iii) by predicting non-labor earnings using the unemployment benefits formula described in Section 6 (column (7)). Crime is defined as equal to 1 if an individual commits an offense at time t and is convicted in the same or any subsequent year.

	Dependent variable: Property Crime							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Specification	OLS	IV	OLS	OLS	IV	OLS	IV	IV
Year Range	−5 to +7	−5 to +7	−5 to +7	−5 to +7	−5 to +7	−5 to +1	−5 to +1	−5 to +1
log(Labor income)	−0.0017***	−0.0078***				−0.0028***	−0.1328*	
	(0.0002)	(0.0027)				(0.0003)	(0.0763)	
log(non Labor income)			0.0002***			0.0001***	−0.0042	
			(0.0000)			(0.0001)	(0.0105)	
log(Total income)				−0.0020***	−0.0110**			−0.0173**
				(0.0003)	(0.0048)			(0.007)
Instruments for Labor earnings	-	Pre- and Post- Displacement Indicators	-	-	Pre- and Post- Displacement Indicators	-	Pre- and Post- Displacement Indicators	Pre- and Post- Displacement Indicators
Instruments for non Labor earnings	-	-	-	-	-	-	Predicted U.I. Benefits	-
Fixed Effects	Municipality×Time, Individual Fixed Effects							
R Squared	0.113	-	0.113	0.113	-	0.174	-	-
Observations	5,167,318	5,167,318	5,167,318	5,167,318	5,167,318	2,782,402	2,782,402	2,782,402
Workers	154,694	154,694	154,694	154,694	154,694	154,694	154,694	154,694
F Statistic	70.8	15.1	73.1	65.2	11.6	25.8	5.5	4.69
Mean of Dep. Variable	0.016	0.016	0.016	0.016	0.016	0.016	0.016	0.016

Standard errors twoway-clustered at the individual level and baseline firm.
R^2 is not reported for the instrumental variable regressions in columns (2), (5), (7), and (8).
***: Significant at 1%, **: Significant at 5%, *: Significant at 10%.

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