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Young adult job loss and criminal activity

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YOUNG ADULT JOB LOSS AND CRIMINAL ACTIVITY*

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Abstract: This paper uses data from the Panel Study of Income Dynamics to study the relationship between job displacement and the probability of arrest and incarceration among young adults. Results show that displacement is associated with increases in the probability of arrest and incarceration. The increase in arrest probabilities is associated with crimes related to robbery and alcohol and traffic violations. Access to financial resources from the government to cope with earnings losses associated with displacement does not mitigate the effect of job loss on the probability of arrest. Finally, we find that the increased probability of arrest is concentrated among male and non-white job losers. Results are robust to different sample selection criteria.

JEL Codes: J63; J65; K42
Keywords: job displacement; young adults; criminal behavior

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I. INTRODUCTION

It is unsurprising that dislocated workers change their labor market behaviors to adjust to the negative shock of an involuntary job loss. Displaced individuals are more likely to involuntarily hold part-time jobs relative to their nondisplaced counterparts (Farber 1999, 2013). Those who lose their job through no fault of their own are more likely to migrate not only across geographic regions (Huttunen et al. 2018; Neffke et al. 2018), but also across industries (Neffke et al. 2018). Displacement can also affect a spouse's labor market behavior. Stephens (2002) finds a significant added worker effect after displacement. A growing literature, however, shows that involuntary job loss is associated with other behavioral changes that are not necessarily related to mitigating lost earnings. For example, the probability of divorce increases after involuntary employment separation (Charles & Stephens 2004; Banzhaf 2018). Displacement is also associated with decreased fertility (Lindo 2010; Amialchuk 2013) and an increase in consumption of alcohol, cigarettes, and some hard drugs (Jolly & Davis 2023).

In this paper, we study another behavioral change potentially associated with job displacement: participation in activities that lead to arrest and incarceration. Here, we focus on a sample of young adults between 17 and 28 years old and analyze the potential effect involuntary job loss has on the probability of arrest and incarceration. To do so, we use the 2005 to 2019 survey waves of the Panel Study of Income Dynamics and employ a difference-in-differences estimator with individual-level fixed effects to compare the evolution of arrest and incarceration probabilities for a sample of young adult displaced workers to those in a comparison group of never-displaced individuals. Understanding the relationship between involuntary job loss and crime is important, particularly for young adults. If job loss leads to criminal behavior resulting in arrest and incarceration, then young adults will have a difficult time acquiring important human capital through on-the-job training and formal education. This disruption to human capital accumulation will impede earnings growth throughout the entire life cycle. Moreover, the stigma associated with being arrested or incarcerated may depress employment opportunities or limit those opportunities to jobs characterized by low wage growth. In fact, Jacob & Lefgren (2003) note how incarceration is associated with a 10 to 30 percent reduction in earnings.

Results from the analysis show that involuntary job loss increases the probability of being arrested by 7.2 percentage points and increases the likelihood of incarceration by 2.6 points. When focusing on the reason for arrest, displacement tends to increase the probability of being arrested for such crimes as theft, robbery, and burglary, along with alcohol-related reasons, such as driving under the influence. Additional results show that young adult job losers who have access to financial resources such as savings accounts or help from relatives prior to displacement have lower probabilities of arrest when compared to those displaced workers without access. Since job loss is associated with deep and persistent earnings losses and employment instability, these results are consistent with theoretical models relating economic incentives to criminal behavior, such as Becker (1968) and Ehrlich (1973).

This paper contributes to the literature in two ways. First, little research on displacement's effect on crime exists. To our knowledge, only four papers have studied the relationship between job loss and crime (Rose 2018; Rege et al. 2019; Bennett & Ouazad 2020; Khanna et al. 2021). Therefore, results presented here will add to a growing body of evidence on this important relationship. More importantly, however, this is the first paper to use a nationally representative dataset from the United States. Research using administrative records studied Norway (Rege et al. 2019), Denmark (Bennett & Ouazad 2020), and Colombia (Khanna et al. 2021). There is no reason to expect results for the US to be comparable to those from these countries. Rege et al. (2019) note that Norway has strong employment protection, with workers requiring at least three-month's notice before displacement. Furthermore, workers can receive unemployment benefits for up to two years. Similarly, Bennett & Ouazad (2020) state that individuals from Denmark can qualify for unemployment benefits for approximately five years. Therefore, the social safety nets in the European countries studied to date are significantly more generous relative to the US. This generosity alters the economic incentives to engage in criminal behavior. Moreover, Rege et al. (2019) note that Norway has markedly lower incarceration rates and shorter prison sentences for some crimes relative to the US. These differences in the criminal justice system should create lower costs of committing crimes in Norway relative to the US. Khanna et al. (2021) analyze mass layoff events that occurred in the city of Medellín, Columbia in 2010. The authors note that Medellín has a reputation for being one of the most violent cities in the world. Finally, while Rose (2018) uses administrative data from the US, the data come from Washington state. Moreover, Rose (2018) uses a sample of formerly incarcerated individuals and examines displacement's impact on recidivism.

The second contribution of this study is its focus on young adults. As noted in Jolly & Davis (2023), the job displacement literature traditionally focuses on individuals with at least a few years of tenure on the lost job. Doing so typically results in relatively older samples. This is true of the previous literature mentioned above. The average age in Rege et al. (2019) is 34; Bennett & Ouazad (2020) use a sample with an age range of 20 to 39, and the average age in Rose (2018) is 31. The average age in our sample is 21 years. Khanna et al. (2021) do present separate estimates for those aged 20 to 25 and 26 to 60. However, the sample used in their study is highly selected with a focus on displacements occurring in one year in a very violent city. Relative to older individuals, less is known about how displacement affects young adults. Some research on labor market outcomes, non-wage fringe benefits, and substance abuse following displacement does exist (Kletzer & Fairlie 2003; Jolly & Phelan 2015, 2017; Barnette et al. 2020; Krolikowski et al. 2020; Jolly & Davis 2023). Therefore, our results will add to the expanding literature on displacement's effects on younger individuals.

The paper proceeds by providing context and reviewing the findings in the relevant literature in section II. We discuss the data and empirical methodology in section III and present the results in section IV. Section V provides results from a series of robustness checks. Finally, the paper provides concluding remarks in section VI.

II. CONTEXT AND PREVIOUS LITERATURE

Theoretical models relating economic incentives to criminal activity (Becker 1968; Ehrlich 1973) predict that involuntary job loss should lead to behavioral changes that increase the probability of arrest and incarceration. Earnings decline considerably around the year of employment separation and remain permanently depressed relative to what they would have been had the job loss never occurred (e.g., Jacobson et al. 1993; Couch & Placzek 2010). The Becker (1968) and Ehrlich (1973) models both state that this reduction in legitimate earnings should lead to an increase in crime. Becker (1968) states that the reduction in licit earnings reduces the utility received from legal activity relative to the expected utility received from participating in criminal acts. Ehrlich (1973) notes that the reduction in legal earnings increases the differential in the marginal returns to participating in illegal versus legal activities, thereby increasing the time allocated to illicit acts. With earnings permanently depressed, the cost of participating in criminal activities falls since the opportunity cost of incarceration is lower than before.

Job loss resulting in unemployment increases the amount of leisure time. To the extent that criminal behavior is a type of leisure activity, increases in non-market-based time will lead to increases in activities that may result in arrest and incarceration. Ehrlich (1973) gives theoretical support for this prediction. Jacob & Lefgren (2003) provide evidence that property crime committed by juveniles decreases by 14 percent on days when school is in session due to juveniles' leisure time being incapacitated by the structured nature of school days.

Among young adults, labor market instability may weaken connections to social networks such as family and friends that promote positive behavioral growth (Hartnagel 1996), which could lead to delinquent behavior. Jolly & Davis (2023) find evidence that young adult job loss is associated with higher probabilities of smoking and drinking and increases in the intensity of drinking and consuming marijuana. Intense consumption of drugs and alcohol could lead to a loss of self-control and, therefore, behavior that results in arrest. The mental strain associated with involuntary job loss could also lead to a loss of self-control and criminal behavior (Rege et al. 2019). These arrests could be due to individuals committing property crimes or non-property crimes, such as driving under the influence.

Weakening ties to positive peer networks could result in stronger connections to negative peer groups. The crime literature points to strong peer effects regarding criminal behavior. For example, Billings & Hoekstra (2023) present evidence that childhood exposure to peers whose parents have been arrested are significantly more likely to commit crimes in early adulthood.

A large literature exists linking local area unemployment rates and crime. Rose (2018), Rege et al. (2019), and Bennett & Ouazard (2020) provide thorough reviews of this literature, and so we will not repeat that here. Broadly speaking, the literature on macroeconomic conditions and crime finds that a roughly 1 percentage point increase in the unemployment rate is associated with a 3 to 7 percent increase in property crimes, with relatively no effect on violent crimes. While the literature on the relationship between macroeconomic conditions and crime has produced important findings, it lacks in explaining potential underlying mechanisms as to exactly what leads to changes in criminal behavior during poor economic times.

Due to the macroeconomic-crime literature's inability to isolate the causal mechanism of how labor market outcomes lead to increases in property crime, a new literature has developed linking individual job displacements to changes in criminal behavior (Rose 2018; Rege et al. 2019; Bennett & Ouazad 2020; Khanna et al. 2021). This literature finds immediate increases in criminal behavior upon involuntary employment separation. The effect of displacement on the probability of arrest declines as time since job loss increases. This relationship between time-since-displacement and the probability of arrest is expected given that displaced workers' earnings, while depressed relative to a control group of non-job losers, recover over time.

Not only do earlier papers agree on the directional relationship between criminal behavior and displacement, but also they agree that displaced workers commit property crimes at rates higher than their non-displaced counterparts do. There is less agreement, however, on whether job loss leads to other types of crimes. Rose (2018) finds evidence of increases in domestic violence upon employment separation, and Rege et al. (2019) present results showing increases in crimes related to alcohol/drugs and traffic violations. In contrast, Bennet & Ouazad (2020) and Khanna et al. (2021) find that displacement is unrelated to violent crimes, and Bennet & Ouazad (2020) find no relationship between job loss and arrests for driving under the influence. Since each paper studies a different country, the differences in results regarding the type of criminal behavior may be due to different country-specific factors.

Finally, the earlier literature tries to separate the importance of economic incentives versus leisure time on displaced workers' decision to engage in criminal behavior. In trying to assess the importance of economic incentives, Rose (2018) and Bennet & Ouazad (2020) exploit exogenous variation in unemployment insurance (UI) schemes; however, the authors come to differing conclusions. Rose (2018) provides evidence showing that UI significantly reduces the propensity to commit both property and violent crimes, whereas Bennet & Ouazad (2020) find that UI plays only a minor role in mitigating criminal behavior. Khanna et al. (2021) find that having access to retail consumption credit nearly eliminates the relationship between displacement and the probability of arrest, suggesting a strong role for incentives.

Bennet & Ouazad (2020) use the structure of Denmark's UI system and provide evidence suggesting that the change in the amount of leisure time is important for explaining criminal activity after involuntary job loss. Denmark has two parts to its UI scheme, a passive part that allows workers to receive benefits without a legally binding commitment to find employment and an active portion where a re-employment plan is required. The authors find that the probability of arrest falls when workers transition into their active period of UI receipt.

Rege et al. (2019) note that the importance of incentives versus leisure may differ depending on the type of crime. To do so, the authors estimate the effect of displacement on crime during different days of the week. Rege et al. (2019) argue that if time incapacitation is important, then the effect of job loss on crime should be larger during weekdays, when individuals are traditionally working, than on weekends. The authors find that displacement's effect on crimes related to drugs/alcohol, traffic violations, and violent offenses is larger on weekdays relative to weekends. However, they note how the effect of displacement on property crimes is similar on weekdays and weekends, suggesting an important role for economic incentives for that particular type of criminal activity.

III. DATA AND METHODOLOGY

Data for this study come from the 2005 to 2019 survey waves of the Panel Study of Income Dynamics (PSID). The PSID has been a biennial survey since 1997. Therefore, this study uses eight survey waves. During this period, the PSID administered a supplemental survey called the Transition to Adulthood Supplement (TAS). The TAS gathers information on those between the ages of 18 and 28. An individual can be 17 and participate in the TAS as long as that person turns 18 during the calendar year. Originally, to be included in the TAS, young adults must have been a member of a family that responded to the main 1997 PSID survey. Starting in 2017, all young adults between 18 and 28 years old are in the TAS. Each individual included in the main sample used here must have at least two usable observations.

The general form of the estimated equation used throughout the analysis is:

$$y_{it} = x'_{it}\beta_1 + \delta_1 D_{it}^{k\geq 0} + \alpha_i + \theta_r + \gamma_t + u_{it} \qquad (1).$$

In the main analysis, y_{it} is a binary variable equaling one if respondent *i* reports ever being arrested by period *t*. To investigate whether displacement leads to incarceration, we also estimate equation (1) after re-defining y_{it} to equal one if the respondent reports ever being incarcerated by period *t*. Restricting y_{it} to equal one if the respondent reports *ever* being arrested/incarcerated, as opposed to being arrested/incarcerated each year, is a design of the PSID. The PSID only asks respondents if they have ever been arrested/incarcerated. We restrict the sample to only those individuals who do not report an arrest during the first survey wave in which we observe them. We impose this sample selection criterion to ensure that each respondent has some variation in the dependent variable. We re-estimate equation (1) twice, once for each dependent variable from the main analysis, after removing this restriction. The results are in Appendix table 1 for arrest probabilities and 2 for the probability of incarceration. The main qualitative results from the analysis remain unchanged.

In equation (1), $D_{it}^{k\geq 0}$ is a dummy variable equaling one if person *i* reports their first displacement in period *t* and zero otherwise. The superscript k indexes time relative to the initial job loss, with period 0 being the survey year of the report. Therefore, $D_{it}^{k\geq 0}$ equals one in every year after the first job loss, including the year of separation. We define displaced workers as those who separate from their employer between survey waves because they were laid-off/fired or their plant closed. This definition of displacement is common in literature using the PSID (e.g., Stevens 1997; Stephens 2002; Lindo 2010). The PSID asks about employment for up to five jobs, and we record a displacement from any job the individual reports a separation. We require all displaced workers to be in the sample for at least one survey wave before job loss. The comparison group consists of those who never report a displacement. Estimates of δ_1 show the average annual effect displacement has on the probability of being arrested or incarcerated.

The vector x_{it} contains a quartic in age, the γ_t is a set of year fixed-effects, the θ_r is a set of fixed-effects for the Census region of residence, and the u_{it} is the random error term.[1] Finally, the α_i is an individual-specific fixed-effect that accounts for any time-invariant factors that may be correlated with the probability of being arrested and the probability of experiencing displacement. Accounting for individual fixed effects is important since the displacement literature suggests that those who suffer from involuntary job loss are inherently different from those who do not (e.g., Gibbons & Katz 1991). Furthermore, it is reasonable to expect that those

[1] Results are unchanged if we replace region dummy variables with state dummies.

who engage in activity leading to arrest and incarceration may have a higher risk tolerance relative to those who do not (see Becker 1968). These individuals may sort themselves into occupations or industries that face a relatively higher risk of involuntary job loss. Incorporating individual fixed-effects into the analysis accounts for these two factors.

To investigate the potential intertemporal relationship between displacement and arrest probabilities, we estimate the following event history model:

$$y_{it} = x'_{it}\beta_1 + \sum_{k\geq -m} D_{it}^{k}\delta_k + \alpha_i + \theta_r + \gamma_t + u_{it} \qquad (2).$$

In equation (2), the D_{it}^{k} is a set of event time dummy variables. For example, D_{it}^{0} equals one during the year of the reported initial displacement and zero otherwise. The estimates of δ_k show the potential inter-temporal effect displacement has on arrest probabilities. The identifying assumption for equations (1) and (2) is that the trends in the probability of arrest for displaced and non-displaced workers should be equal in the absence of job loss. We assess the validity of this assumption by examining the pre-job loss coefficients from equation (2) in the next section.

Even if the parallel trends assumption holds, there are other potential threats to identification that we investigate in the robustness section. We introduce these briefly here. First, the definition of displacement includes not only plant closures, but also layoffs/firings. While this is consistent with the earlier literature using the PSID, it is true that firms have discretion over who to layoff and fire. Therefore, the definition of job loss used here may not be exogenous. To this end, we provide estimates after restricting the treatment group to those who report plant closure only. Second, young adults between 17 and 28 years old may have various degrees of labor market attachment that change over time. We provide estimates below that restrict the treatment and control groups to have similar labor market attachments in the baseline survey. Third, reverse causality is a potential concern. Those who are arrested and/or incarcerated may be more likely to experience layoffs. This elevated propensity of layoff may arise because of a lack of human capital accumulation (i.e., instead of experiencing on-the-job training/education, the worker is in jail) or because of a negative stigma associated with being arrested. Finally, it is possible that unobservable characteristics that are correlated with criminal behavior and job loss evolve over time. To this end, we use a stacked estimator that incorporates individual fixed-effects and propensity score matching similar to Schmieder et al. (2023).

IV. RESULTS

Descriptive Statistics

Table 1 presents descriptive statistics (means and proportions) separately for displaced and non-displaced workers. We calculate these statistics using only the first observation for each individual. This is for two reasons. First, the TAS is a cohort-based survey for the majority of the years used in the analytical sample (2005 through 2015). Second, we restrict the treatment group of displaced workers to have at least one usable observation before the first reported job loss. Therefore, calculating the descriptive statistics using only the baseline observation for each respondent should make the treatment and control groups as comparable as possible.

Table 1 shows that displaced and non-displaced young adults are similar in some dimensions, with some notable exceptions. The table shows that the displaced tend to earn less and work slightly more hours per week pre-job loss relative to their non-displaced counterparts. The displaced tend to have less education relative to the control group of non-displaced workers, and job losers are also less likely to be white. Importantly, these pre-job loss differences between the treatment and control groups justify the use of individual fixed-effects in the analysis. When focusing on criminal behavior, we present calculations for whether someone ever reports an arrest or incarceration during their time in the sample. This is because we select our sample such that no one can report an arrest during the first observation and all statistics in table 1 use only the first observation. Table 1 shows that the eventually displaced have a substantially higher probability of reporting ever being arrested relative to the control group (25% versus 10%). Unsurprisingly, this higher arrest probability translates into displaced workers also having a higher proportion of ever experiencing incarceration.

To examine the inter-temporal evolution of the probability of arrest, Figure 1 presents the average of the arrest variable by displacement status over time relative to the year of job loss. The control group does not have a year of job loss. To circumvent this, we assigned each worker in the control group a pseudo-displacement year such that the calendar year distribution of fake displacements matches the yearly distribution of actual job losses following Jolly & Phelan (2015; 2017). For the control group, figure 1 shows that the probability of arrest remains stable over event time with no discrete changes around the year of pseudo-displacement. For the group of job losers, figure 1 shows that arrest probabilities are relatively stable pre-displacement, mainly ranging from 0.22 to 0.24. This lends some support to the parallel trends assumption. However, the year of job loss is associated with a discrete increase in the average arrest rate that never returns to pre-displacement levels. While these are simple averages, figure 1 suggests that displacement may be associated with increases in the probability of arrest.

Labor Market Outcomes

Since earnings losses and lack of employment are the two main theoretical reasons as to why displacement would lead to increases in criminal activity, the analysis starts by examining the relationship between displacement and labor market outcomes for young adults. To this end, we estimated equation (1) when using real annual labor earnings, the log of annual earnings, and an employment binary variable as dependent variables. Using the level of earnings allows the ability to incorporate observations of those young adults who withdraw from the labor market in response to job loss. Results are in table 2. The table presents the coefficient associated with the displacement dummy variable, which is labeled *After*.[2] Estimates show that displacement is associated with earnings losses amounting to approximately $4,600 (52 percent when using the log of earnings) and a drop in employment probabilities amounting to 13 percentage points.

Figure 2 plots the coefficients and the associated 95% confidence intervals for the displacement dummy variables from the event history analysis presented in equation (2). When estimating equation (2), we use two years before job loss as the omitted period. Figure 2 shows that earnings and the probability of employment decline significantly starting with the year of displacement and remain depressed, relative to the control group, for the entire follow-up period. Importantly, the pre-displacement coefficients for log earnings and employment are statistically insignificant. While the pre-displacement coefficients from the real earnings regression are significant, they are stable and positive, suggesting that those who eventually lose their job tend to earn more than their never displaced counterparts do pre-job loss.

Displacement's Relationship with Arrest and Incarceration

The significant earnings and employment losses presented in table 2 imply that displaced workers should experience a higher rate of arrest and incarceration relative to the control group. The reduction in labor earnings lowers the incentive to engage in legal, market-based activities and reduces the cost of engaging in illicit behavior by reducing the opportunity cost of incarceration. The idle time associated with non-employment increases the time available for committing crimes. To this end, table 3 presents estimates of displacement's effect on the

[2] The remaining coefficients are available upon request.

probability of arrest and incarceration from equation (1). Results indicate that young adult job loss increases the probability of being arrested by 7.2 percentage points. This estimate is significant and economically large when compared to the pre-job loss average. Unsurprisingly, since displaced workers experience a higher probability of being arrested relative to the comparison group, those who suffer an involuntary job loss also face a higher probability of being incarcerated after displacement, equaling 2.6 percentage points.

Interestingly, earnings and employment status at the time of separation do not influence the effect of displacement on the probability of experiencing an arrest. We re-estimated equation (1) after limiting the treatment group to those displaced workers who were non-employed during the wave of reported job loss. The estimated coefficient (standard error) associated with the *After* dummy variable equaled 0.076 (0.020). We also re-estimated equation (1) after limiting the group of displaced workers to those who are in the upper 25th percentile of the displaced worker earnings distribution during the year of reported job loss. The coefficient (standard error) associated with the *After* dummy equaled 0.082 (0.027).

To explore the inter-temporal relationship between job loss and criminal activity, we present estimates from equation (2) in figure 3. For both the probability of arrest and incarceration, the results in figure 3 suggest that the effect of displacement starts the year of job loss and is persistent. The post-displacement coefficients are stable in magnitude, and while estimates during periods $\geq t+6$ lose significance, they remain elevated relative to the pre-job loss coefficients. For arrest and incarceration, the evidence in figure 3 suggests that the relationship between young adult job loss and criminal activity may be causal. The pre-displacement coefficients are relatively stable, and none is statistically significant at the 5% level. Moreover, the signs of the pre-displacement coefficients are negative, which provides transient evidence that reverse causality is not a main driver of the results.

The TAS gathers information regarding the reason provided for the first arrest experienced by the respondent. The responses include arrests for violent crimes (e.g., domestic violence, battery, assault), severe crimes (e.g., arson, hit and run, robbery), non-severe crimes (e.g., disorderly conduct, liquor violations, resisting arrest), and other (e.g., reckless driving, speeding, driving under the influence, and other). To investigate the reason for arrest, we create binary variables for each potential reason for first arrest and re-estimate equation (1) four times, once for each main reason. Since the TAS asks for the reason of the first arrest and only asks whether the respondent was *ever* arrested, we want to ensure that we are only capturing arrests that may occur after job loss. Therefore, we limit the treated sample to those job losers who are not arrested before displacement occurs.[3] Results are in table 4 and show that displaced workers have significantly higher probabilities of being arrested for severe crimes and other crimes. These categories include robbery, hit and run, reckless driving, and driving under the influence. This is to be expected, as robbery would occur due to reductions in earnings, and driving under the influence occurs during leisure time, both of which displaced workers experience at higher rates relative to the control group of non-displaced individuals.

Like the earlier literature, we find that displacement increases the probability of arrest for property-type crimes, such as robbery. However, our findings for the other categories provide an interesting comparison to the earlier literature. Results in table 4 are similar to those in Rege et al. (2019), who also find displacement increases the probability of alcohol-related crimes and traffic violations. Unlike Rose (2018), who uses administrative records from Washington state,

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[3] Relaxing this assumption does not change the magnitude of the estimates in any meaningful manner. These results are available upon request.

we find no evidence of increases in crimes related to domestic violence. The difference in findings may be due to the samples used. Rose (2018) uses a group of individuals who were formally incarcerated, and he estimates the effect displacement has on the probability of recidivism. These individuals may have a higher predisposition to commit violent acts relative to the non-formerly incarcerated. In contrast, the individuals used in our analysis were never arrested before they enter the analytical sample.

Mechanisms

This subsection investigates the role that financial support aside from an individual's labor earnings plays in the effect of involuntary job loss on arrest. If job losers commit crimes because of reduced earnings, then it is reasonable to expect that the effect of displacement on arrest probabilities should be larger for those who do not have financial support around the time of separation. To investigate the relationship between financial assistance and post-displacement arrest probabilities, we determine whether or not displaced workers received aid during the survey wave before job loss from either parents/relatives, the government, or their own resources. Here, relatives can help by paying tuition, rent, or a mortgage, providing a personal loan, or paying for expenses. Assistance from the government comes from Supplemental Security Income, Temporary Assistance to Needy Families (or other sources of welfare), or unemployment insurance. An individual's own resources include savings/checking accounts, owning stocks, mutual funds, investment trusts, money market accounts, government bonds, or certificates of deposit. We then alter equation (1) as follows:

$$y_{it} = x'_{it}\beta_1 + \delta_1 D_{it}^{k\geq 0} + \sum_j \varphi_j D_{it}^{k\geq 0} * funding_{ij} + \alpha_i + \theta_r + \gamma_t + u_{it} \qquad (3).$$

The variable $funding_j$ is a dummy equaling one if the displaced worker received assistance from source j (j = parents/relative, government, own resources). Results are in table 5. The first column in table 5 lists the estimated full effect for the funding source listed. In other words, for *No Assistance*, the entry in the table is the estimate of δ_1. The other entries in the column are estimates of $\delta_1 + \varphi_j$. The second column in table 5 signifies whether the estimates in the first column are significantly different from receiving no assistance from any source.

The results in table 5 suggest that receiving aid from parents/relatives or having access to one's own financial resources to help smooth consumption lowers the probability of arrest after displacement occurs. The estimates for those who have parental/relative support before job loss are not statistically different from those who have no financial aid. However, those who have their own financial resources to help smooth consumption are statistically less likely to experience arrest relative to those job losers without financial resources. Interestingly, those displaced workers who receive aid from the government before job loss have insignificantly larger arrest probabilities than those who do not. While this result is counter-intuitive, it is in line with Bennet & Ouazad (2020), who find that UI does not mitigate participation in criminal behavior after displacement. That resources such as savings accounts help to mitigate the effect of displacement on criminal behavior, while government or parental resources do not, suggests that the type of financial aid is important.

<u>Heterogeneity</u>

The literature shows that labor market outcomes post-displacement differ by gender (Jahromi & Callaway 2020) and race (Kletzer & Fairlie 1998). Given these differences, there is no reason to expect that arrest probabilities post-displacement will be similar between men and women or whites versus non-whites. To investigate gender and racial differences in arrest probabilities post-job loss, we alter equation (1) as:

$$y_{it} = x'_{it}\beta_1 + \delta_1 D_{it}^{k\geq 0} + \varphi_1 D_{it}^{k\geq 0} * group_i + \alpha_i + \theta_r + \gamma_t + u_{it} \qquad (4).$$

Here, the variable *group* defines the group to which a person belongs. When investigating gender differences, *group* equals one if the person is a female. When examining racial differences, *group* equals one if the respondent reports being white. Estimates of δ_1 show the effect of displacement on arrest probabilities when *group* equals zero (i.e., male or non-white job losers), and estimates of $\delta_1 + \varphi_1$ show the effect of job loss when *group* equals one (either female or white job losers).

As motivation, it is important to show that post-displacement labor market outcomes differ by race and gender before presenting results on differences in arrest probabilities. We estimated equation (4) twice for gender and race. The first estimation uses the level of earnings as the dependent variable; the second uses the employment binary variable. The results are in table 6 and show that female and non-white job losers experience statistically larger earnings losses when compared to their male and white counterparts. Female and non-white job losers also experience larger declines in the probability of employment. While the differences between gender and race are not statistically significant, they are economically large. These larger earnings losses, coupled with the increased time away from legal market activities should lead to higher rates of arrest for women and non-white job losers relative to their male and white counterparts. To investigate this, we present estimates from equation 4 when using the arrest dummy as the dependent variable in table 7.

Results show that male and non-white job losers experience significantly higher probabilities of arrest post-displacement when compared to female and white job losers. In fact, the estimate for white job losers is statistically insignificant, and the one for females is marginally significant at the 10 percent level. Not only are the post-displacement probabilities high for male and non-white job losers when compared to women and whites, but also the probabilities are large relative to the pre-displacement averages.

The heterogeneity by race found here is expected. If non-whites have larger earnings losses and greater reductions in the probability of employment, then it is reasonable to assume that they would have an increased incentive to, and lower cost of, committing crimes and more leisure time in which to do so. By this logic, one would expect the same pattern for women. In other words, female job losers should have larger probabilities of arrest after displacement compared to men. The results by gender do not support this. Campaniello & Gavrilova (2018) find that men are significantly more responsive to economic incentives with respect to crime. In fact, the authors note that changes in economic incentives explain roughly 56 percent of the observed gender gap in criminal activity. By extension, while earnings and employment losses are larger for women than for men in this sample, the fact that men tend to respond much greater to changes in economic incentives than women do could explain the findings in table 7.

To further explore the connection between race, gender, job loss, and arrest probabilities, we estimate equation (4) two additional times. Here, we define *group* to equal one if the respondent is female and estimate equation (4) once for white individuals and once for non-white individuals. The results are in table 8 and show that, regardless of race, displacement has a statistically insignificant relationship with the probability of being arrested for women. For non-white individuals, the gender difference is statistically significant at the 1-percent level. The effect of job loss on being arrested is concentrated solely among the males for both racial categories. Given the discussion in the preceding paragraph, this is unsurprising. When looking at male job losers, table 8 shows that the effect of displacement on white men is similar in magnitude and significance to the baseline estimate in table 3. For non-white men, however, the estimated relationship between job loss and arrest probabilities is more than double the size of the baseline estimate. For non-white men, involuntary job loss is associated with an increase in the probability of arrest by 15.5 percentage points.

V. ROBUSTNESS AND REVERSE CAUSALITY

In this section, we explore the robustness of our main findings on displacement's effect on arrest probabilities. Specifically, we focus on changes to sample selection criteria, choice of estimator, and reverse causality. All results are in table 9 and come from equation (1).

Plant Closings

Researchers generally view plant closures as more exogenous than layoffs/firings as firms do not have discretion over who to fire when shutting down. Therefore, it is standard to provide separate estimates after limiting the treatment group to those displaced due to plant closures (e.g., Jolly & Davis 2023). Only 142 young adults in this sample experience a job loss due to plant closure, which makes drawing meaningful conclusions difficult. However, for completeness, we re-estimated equation (1) after re-defining job loss as that resulting from plant closures only. The results in table 9 suggest that plant closures are not statistically related to the probability of being arrested. The magnitude of the coefficient is still large relative to the pre-job loss average in table 3. However, the estimate loses precision with the reduced sample size.

Labor Market Attachment

The sample here contains young adults between 17 and 28 years old. The labor market attachment of teenagers and individuals in their early 20s may differ from those in their late 20s. To ensure that treatment and comparison groups are similar along the dimension of labor market attachment, we separately impose three different restrictions on the sample. First, we restrict the entire sample to those reporting that they are employed during the baseline interview; second, we then require individuals to work at least 25 hours per week at baseline. Finally, recall that we define a displacement from an employment separation that occurs from up to five different jobs. It is traditional in the displacement literature to only examine displacements from the main job. To this end, we re-define the treatment group to only include those who experience displacement from job 1. Results are in columns (2) through (4) of table 9. Here, the estimated effect of job loss on arrest probability is similar to the main finding shown in table 3.

Propensity Score Matching

Here we employ a propensity score matching estimator similar to that in Schmieder et al. (2023). Displaced and non-displaced workers may be inherently different, and these differences may not necessarily be time invariant. Schmieder et al. (2023) note that propensity score matching can be useful if displaced and non-displaced individuals have differential trends in their unobservable characteristics. The authors propose a matching estimator to account for this. The authors further discuss that this estimator accounts for the potential bias that exists in two-way fixed-effects models when treatment varies over time.

Following Schmieder et al. (2023), we construct a matched comparison group by first defining a displacement year as d. We have seven displacement years in the sample, 2007, 2009, 2011, 2013, 2015, 2017, and 2019. For each year d, we match each displaced worker to a non-displaced worker without replacement. This matching is based off of propensity scores calculated from a probit model estimating the probability of job loss in year d. Control variables in this model include dummies for baseline survey year, gender, race, and the following characteristics in period $d - 2$: labor earnings, employment status, a quartic in age, and whether or not someone was arrested. In line with Rege et al. (2019), who also employ propensity score matching, we do not use the eventually displaced as part of the control group in the early displacement years. For example, a worker experiencing displacement in 2019 cannot be part of the control group in displacement year 2007. We then alter equation (1) as follows: $y_{itd} = x'_{itd}\beta_1 + \delta_1 D_{itd}^{k\geq 0} + \alpha_i + \theta_r + \gamma_t + u_{itd}$. Here, the subscript d signifies different displacement-year cohorts. Results from this analysis show that the estimated effect of job loss on the probability of being arrested is only slightly smaller than the main findings in table 3 and is still significant at the 1 percent level.

We also altered the event history analysis in equation (2) as $y_{itd} = x'_{itd}\beta_1 + \sum_{k\geq -m} D_{itd}^{k}\delta_k + \sum_{k\geq -m} c_{itd}^{k}\psi_k + \alpha_i + \theta_r + \gamma_t + u_{itd}$. Here, the c_{itd}^{k} are dummy variables representing time relative to treatment year d. As Schmieder et al. (2023) note, these are important for controlling for trends in the probability of arrest around cohort years that exist for all workers. Figure 4 presents estimates of the parameters for the event time dummies, δ_k. As in the main results in figure 3, there do not appear to be any pre-trends in the periods before displacement. Furthermore, the magnitude and persistence of the effect of displacement on arrest probabilities are similar to the main results.

<u>Reverse Causality</u>

It is possible that being arrested leads to displacement. Since firms have discretion over who to lay off/fire, it is reasonable to expect that those who have been arrested in the past may be more likely to experience layoffs or firings. This higher probability may be due to a stigma associated with being arrested, excessive absenteeism, or the inability of the formerly arrested/incarcerated to develop human capital through formal schooling or on-the-job training. To investigate this reverse causality, we estimate $job\ loss_{it+1} = x'_{it}\beta_1 + \delta_1 arrest_{it} + \alpha_i + \theta_r + \gamma_t + u_{it}$, where $job\ loss_{it+1}$ is a binary variable equaling one if the respondent reports a job loss in the following survey wave and $arrest_{it}$ is the arrest dummy variable. The result from estimating this equation is in the final column of table 9 and shows that being arrested is statistically unrelated to the probability of reporting a job loss in the following survey wave.

VI. CONCLUSIONS

In this paper, we investigate the effect job displacement has on criminal activity for young adults between 17 and 28 years old. Like the earlier literature, results indicate that displacement is associated with sustained earnings and employment losses. Negative labor market shocks may lead to committing crimes due to reduced incentives to remain in legal work, reduced opportunity costs of crime, and an increase in leisure time available. The empirical evidence provided here suggests that displaced young adults do experience an increased probability of being arrested and incarcerated. Additional results show that non-white male job losers experience substantially larger increases in the probability of arrest post displacement when compared to other gender-race groups. Having access to certain financial resources to smooth consumption mitigates the effect displacement has on engaging in criminal behavior. Results are robust to changes in sample selection criteria and choice of estimator.

This paper contributes to the literature by using a nationally representative dataset for the United States. The results presented here aid in the understanding of a potential mechanism through which labor market instability at young ages can lead to economic challenges later in the lifecycle. Being arrested and incarcerated at younger ages could lead to difficulties accumulating human capital through on-the-job training and formal education, which could impede earnings growth over time. Difficulty in finding stable employment may also occur due to the stigma associated with being arrested and incarcerated. Furthermore, employment opportunities after arrest may be limited to those jobs with low earnings growth. Findings here suggest that government policy designed to aid displaced workers should contain provisions to anticipate and respond to negative behavioral changes among younger job losers. While this paper cannot assess the degree to which economic incentives versus time availability contribute to the increased probability of participating in criminal acts, the results do support the idea that both reasons for committing crime are important as displaced workers are more likely to be arrested for both robbery and alcohol-related reasons and traffic violations. Therefore, government policies should aim to not only support individuals through income replacement services, but also through initiatives to re-train individuals formally so that idle time is not used for crime.

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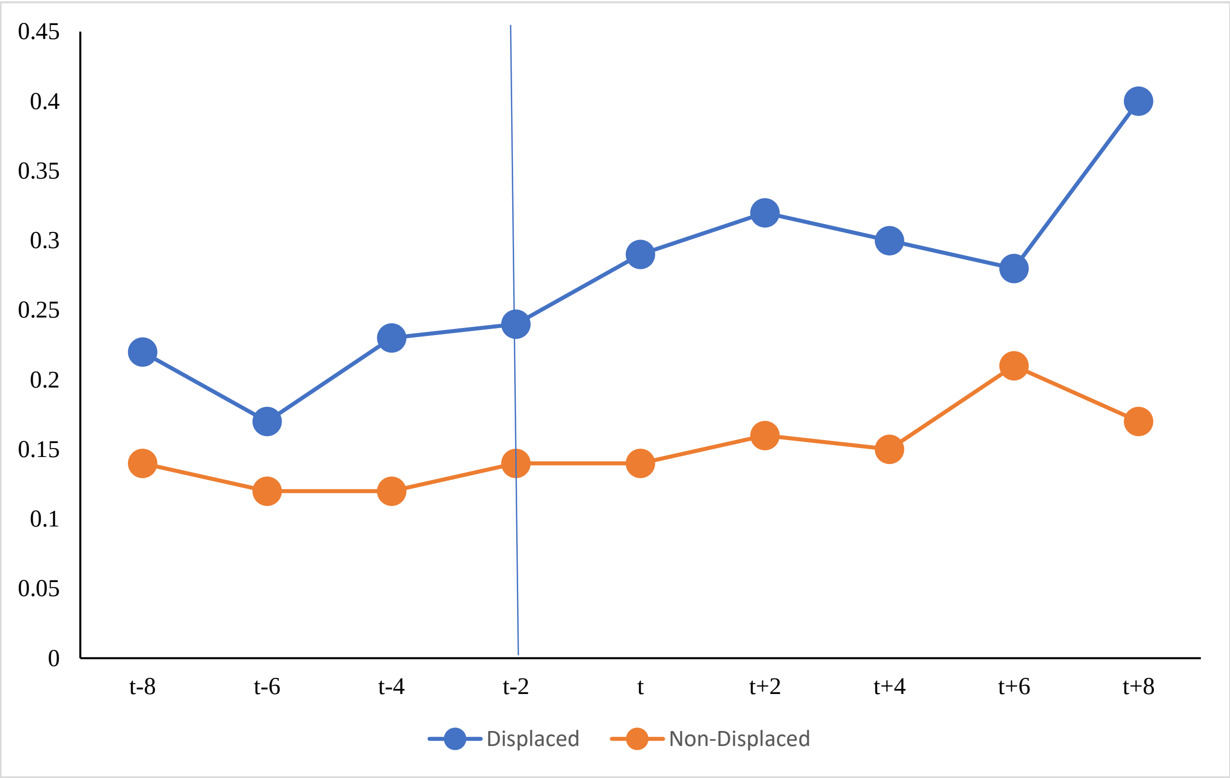
TABLES

Table 1: Descriptive Statistics (means) of Selected Variables

	Non-Displaced	**Displaced**
Total Earnings[a,b]	\$3,410	\$1,965
Hours Worked[b]	16.14	20.57
Employed	55.96%	54.92%
Age	19.01	18.68
Female	57.73%	54.29%
White[c]	53.18%	38.06%
Education		
< High School	13.18%	12.60%
High School Degree/GED	22.80%	27.95%
Some College/Associates Degree	60.95%	59.18%
High School Plus Another Degree (not college)	0.14%	-
Bachelor's Degree	2.65%	0.27%
Master's Degree	0.28%	-
Ever Arrested	10.61%	25.71%
Ever Incarcerated	2.73%	9.37%
Total Number of Individuals	1,980	630

Notes: Sample includes individuals who respond to at least two surveys. Displaced workers must respond to at least one survey prior to job loss. All calculations are means/proportions of selected variables. All years are used in the calculations. However, calculations only use the first observation in which an individual is observed.
a – Real 2018 dollars; b – equals the total across all five jobs; c – represents the modal report for race

Figure 1: Average Arrest Probabilities over Event Time by Displacement Status



Average arrest probabilities by displacement status over event time. The year of displacement for the control group is randomly generated such that the calendar year distribution of pseudo-displacements matches the yearly distribution of actual displacements.

Table 2: Displacement's Effect on Labor Market Outcomes

	Earnings	**Log Earnings**	**Employed**
After	-4,693.579***	-0.744***	-0.131***
	(815.537)	(0.160)	(0.022)
R^2	0.18	0.18	0.09
N	9,323	7,346	9,323
# Individuals	2,610	2,466	2,610

The data come from the 2005-2019 waves of the TAS from the PSID. The sample includes individuals who respond to at least two surveys. Displaced workers must respond to at least one survey prior to job loss. Standard errors clustered at the individual level shown in parentheses. The dependent variables are noted in the column headings. Additional independent variables include a quartic in age and dummy variables for calendar year and Census region of residence.

* p<0.1; ** p<0.05; *** p<0.01

Figure 2: Displacement's Effect on Labor Market Outcomes

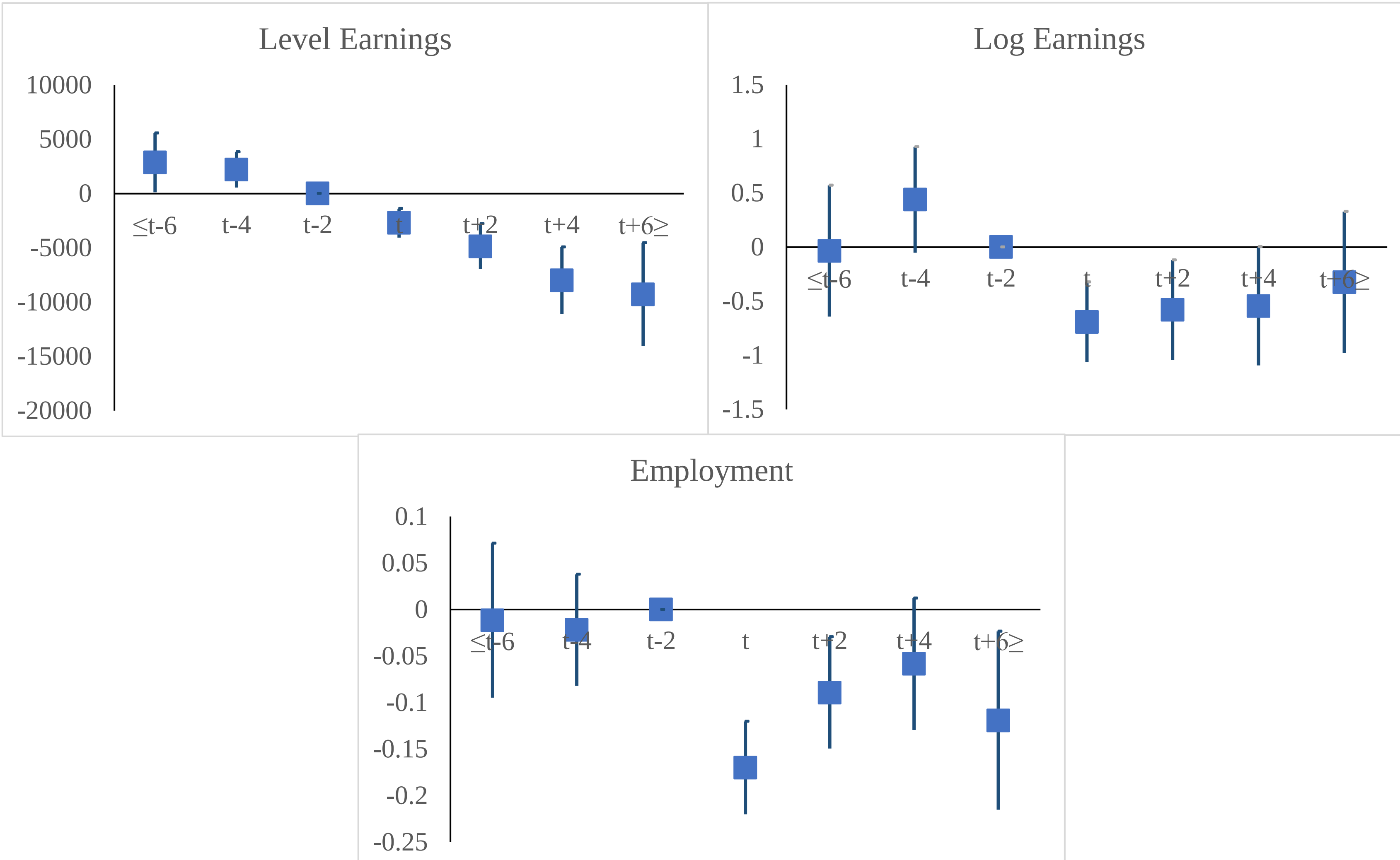


Figure 1 plots coefficients and associated 95% confidence intervals from equation 2. The coefficients are estimated relative to period *t-2*. See notes from table 2 for sample selection criteria and included control variables.

Table 3: Displacement's Effect on Criminal Activity

	Arrest	Incarceration
After	0.072***	0.026***
	(0.013)	(0.008)
R^2	0.08	0.02
N	9,310	9,316
# Individuals	2,610	2,610
Avg Pre-Job Loss	0.053	0.013

The data come from the 2005-2019 waves of the TAS from the PSID. The sample includes individuals who respond to at least two surveys. Displaced workers must respond to at least one survey prior to job loss. Standard errors clustered at the individual level shown in parentheses. The dependent variables are noted in the column headings. Additional independent variables include a quartic in age and dummy variables for calendar year and Census region of residence.
* p<0.1; ** p<0.05; *** p<0.01

Figure 3: Displacement's Effect on Criminal Activity

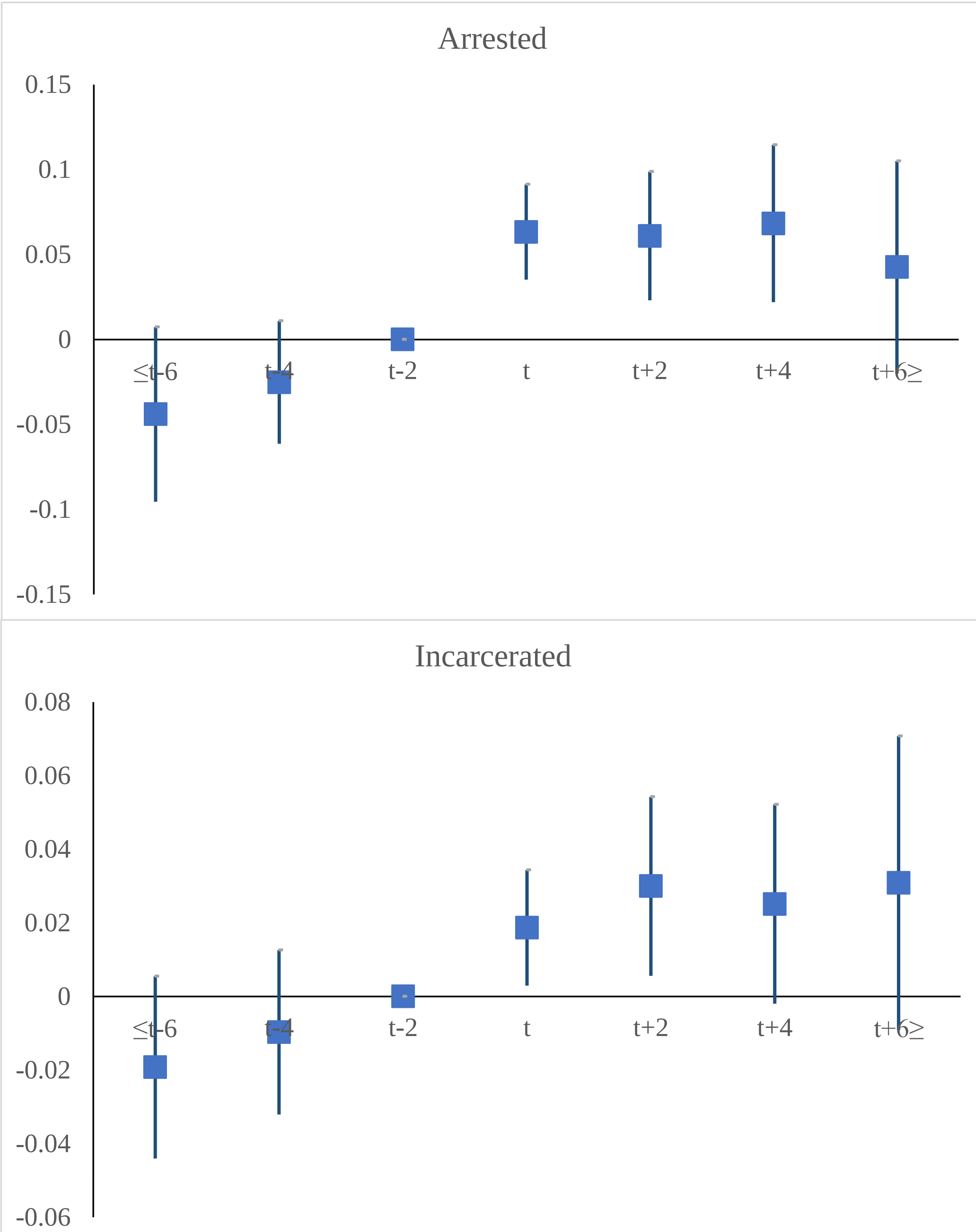


Figure 2 plots coefficients and associated 95% confidence intervals from equation (2). The coefficients are estimated relative to period *t-2*. See notes from table 3 for sample selection criteria and included control variables.

Table 4: Displacement's Effect on Criminal Activity - Reason for First Arrest

	Violent	**Severe**	**Non-severe**	**Other**
After	0.007	0.013**	0.007	0.038***
	(0.005)	(0.006)	(0.005)	(0.008)
R^2	0.01	0.02	0.01	0.03
N	9,066	9,066	9,066	9,066
# Individuals	2,563	2,563	2,563	2,563

The data come from the 2005-2019 waves of the TAS from the PSID. The sample includes individuals who respond to at least two surveys. Displaced workers must respond to at least one survey prior to job loss. Displaced workers also limited to those who did not experience an arrest prior to job loss. Standard errors clustered at the individual level shown in parentheses. The dependent variables are noted in the column headings. Additional independent variables include a quartic in age and dummy variables for calendar year and Census region of residence. Violent crimes include domestic violence, battery, and assault. Severe crimes include arson, hit and run, and robbery. Non-severe arrests include crimes related to disorderly conduct, liquor violations, and resisting arrest. Other crimes include reckless driving, speeding, driving under the influence, and other.

* p<0.1; ** p<0.05; *** p<0.01

Table 5: Heterogeneity by Type of Financial Assistance

	After	**Sig. Diff no Assistance**
No Assistance	0.144***	-
	(0.034)	
Parents/Relatives	0.114***	No
	(0.034)	
Government	0.181***	No
	(0.058)	
Own	0.068***	5%
	(0.022)	
R^2	0.08	
N	9,310	
# Individuals	2,610	

The data come from the 2005-2019 waves of the TAS from the PSID. The sample includes individuals who respond to at least two surveys. Displaced workers must respond to at least one survey prior to job loss. Standard errors clustered at the individual level shown in parentheses. The dependent variable equals one if the individual reports ever being arrested by period t. Additional independent variables include a quartic in age and dummy variables for calendar year and Census region of residence. See text for definition of support from parents, the government, and own support.
* p<0.1; ** p<0.05; *** p<0.01

Table 6: Heterogeneity in Post-Displacement Labor Market Outcomes by Gender and Race

Level of Earnings			
Gender		**Race**	
Men	-2743.244**	Non-White	-6373.16***
	(1155.641)		(861.072)
Women	-6305.27***	White	-2048.087
	(923.373)		(1312.134)
R^2	0.18		0.18
Sig. Different	1%		1%
Employment			
Men	-0.100***	Non-White	-0.150***
	(0.032)		(0.028)
Women	-0.157***	White	-0.101***
	(0.029)		(0.033)
R^2	0.09		0.09
N	9,323		9,323
# Individuals	2,610		2,610
Sig. Different	No		No

The data come from the 2005-2019 waves of the TAS from the PSID. The sample includes individuals who respond to at least two surveys. Displaced workers must respond to at least one survey prior to job loss. Standard errors clustered at the individual level shown in parentheses. The dependent variables are listed in the panel headings. Additional independent variables include a quartic in age and dummy variables for calendar year and Census region of residence.
* p<0.1; ** p<0.05; *** p<0.01

Table 7: Heterogeneity by Gender and Race

Gender		Race	
Men	0.126***	Non-White	0.102***
	(0.021)		(0.018)
Women	0.027*	White	0.022
	(0.016)		(0.017)
R^2	0.08		0.08
N	9,310		9,310
# Individuals	2,610		2,610
Sig. Different	1%		1%
Avg Pre-Job loss			
Men	0.107	Non-White	0.096
Women	0.052	White	0.054

The data come from the 2005-2019 waves of the TAS from the PSID. The sample includes individuals who respond to at least two surveys. Displaced workers must respond to at least one survey prior to job loss. Standard errors clustered at the individual level shown in parentheses. The dependent variable equals one if the individual reports ever being arrested by period t. Additional independent variables include a quartic in age and dummy variables for calendar year and Census region of residence.
* p<0.1; ** p<0.05; *** p<0.01

Table 8: Heterogeneity by Gender – Separate Estimates by Race

	White	Non-White
Men	0.065***	0.155***
	(0.024)	(0.032)
Women	0.021	0.016
	(0.022)	(0.021)
R^2	0.06	0.10
N	4,702	4,601
# Individuals	1,292	1,316
Sig. Different	No	1%

The data come from the 2005-2019 waves of the TAS from the PSID. The sample includes individuals who respond to at least two surveys. Displaced workers must respond to at least one survey prior to job loss. Standard errors clustered at the individual level shown in parentheses. The dependent variable equals one if the individual reports ever being arrested by period t. Additional independent variables include a quartic in age and dummy variables for calendar year and Census region of residence.

* p<0.1; ** p<0.05; *** p<0.01

Table 9: Sensitivity Analysis

	Plant Closure	**Employed at Base**	**Hours ≥ 25 at Base**	**Main Job**	**Matching**	**Reverse Causality**
After	0.031	0.070***	0.086***	0.077***	0.062***	-0.033
	(0.021)	(0.017)	(0.023)	(0.023)	(0.015)	(0.032)
R^2	0.06	0.07	0.10	0.07	0.09	0.01
N	7,301	4,966	3,407	9,310	5,121	6,707
# Individuals	2,123	1,454	845	2,610	1,172	2,610

The data come from the 2005-2019 waves of the TAS from the PSID. The sample includes individuals who respond to at least two surveys. Displaced workers must respond to at least one survey prior to job loss. Standard errors clustered at the individual level shown in parentheses. The dependent variable equals one if the individual reports ever being arrested by period t. Additional independent variables include a quartic in age and dummy variables for calendar year and Census region of residence.
* p<0.1; ** p<0.05; *** p<0.01

Figure 4: Displacement's Effect on Criminal Activity – Matched Sample

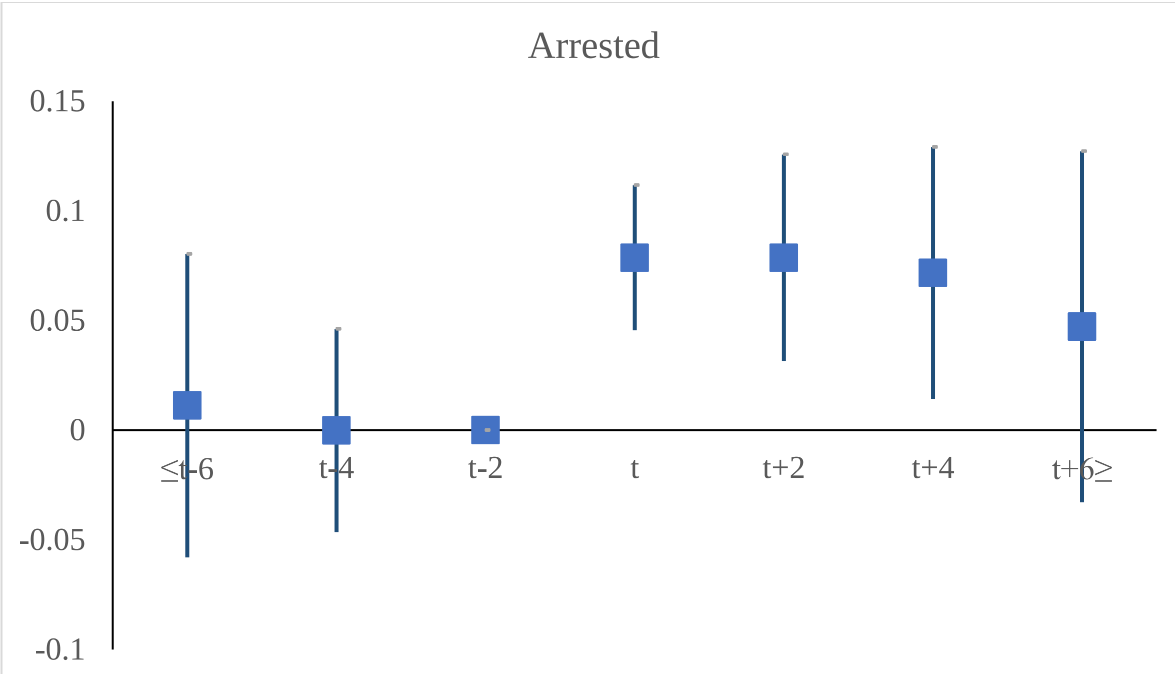


Figure 3 plots coefficients and associated 95% confidence intervals from equation (2) after performing the propensity score matching. The coefficients are estimated relative to period *t-2*. See the main text for sample selection criteria and included control variables.

APPENDIX

Appendix Table 1: Displacement's Effect on Criminal Activity

	Arrest	**Incarceration**
After	0.040***	0.022**
	(0.013)	(0.009)
R^2	0.02	0.01
N	10,871	10,873
# Individuals	3,059	3,059

The data come from the 2005-2019 waves of the TAS from the PSID. The sample includes individuals who respond to at least two surveys. Displaced workers must respond to at least one survey prior to job loss. Standard errors clustered at the individual level shown in parentheses. The dependent variables are noted in the column headings. Additional independent variables include a quartic in age and dummy variables for calendar year and Census region of residence.
* p<0.1; ** p<0.05; *** p<0.01

Appendix Table 2: Displacement's Effect on Criminal Activity - Reason for Arrest

	Violent	**Severe**	**Non-severe**	**Other**
After	0.012**	0.020***	0.008	0.054***
	(0.005)	(0.007)	(0.005)	(0.008)
R^2	0.00	0.01	0.01	0.02
N	9,958	9,958	9,958	9,958
# Individuals	2,848	2,848	2,848	2,848

The data come from the 2005-2019 waves of the TAS from the PSID. The sample includes individuals who respond to at least two surveys. Displaced workers must respond to at least one survey prior to job loss. Displaced workers also limited to those who did not experience an arrest prior to job loss. Standard errors clustered at the individual level shown in parentheses. The dependent variables are noted in the column headings. Additional independent variables include a quartic in age and dummy variables for calendar year and Census region of residence. Violent crimes include domestic violence, battery, and assault. Severe crimes include arson, hit and run, and robbery. Non-severe arrests include crimes related to disorderly conduct, liquor violations, and resisting arrest. Other crimes include reckless driving, speeding, driving under the influence, and other.

* p<0.1; ** p<0.05; *** p<0.01