Mass Layoffs and the Dynamics of Local Crime Rates

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Abstract

I study the effect of mass layoffs in the manufacturing sector on local labor market conditions and crime rates using firm-level administrative data on workers applying for Trade Adjustment Assistance from 1990 to 2010. I find that, on average, a mass layoff event that constitutes 1% or more of a county's labor force causes a persistent decline in employment up to 5 years after the layoff. The initial decline in employment is driven evenly by a temporary decline in the employment rate and a decline in labor force participation, but persists due to a lasting decline in labor force participation. While per capita government transfer payments increase, the decline in per capita wages leads to a persistent decline in per capita income as well. The decline in economic conditions is associated with, on average, an increase in the violent crime rate of 6.04% and property crime rate of 6.10%, leading to a persistent increase in the total crime rate of 6.02%. Heterogeneity analysis reveals that the increase in the violent crime rate can be completely attributed to counties experiencing layoffs in relatively low wage manufacturing sectors. Furthermore, while the number of crimes per police officer increases, the proportion of crimes cleared by police does not decline, suggesting that changes in police deterrence as a result of resource constraints are not responsible for the persistent increase in crime.

1 Introduction

Crime inflicts a negative externality on society. As a result, the causes of crime and mitigating polices have long been of interest to economists, particularly the role of labor market conditions in determining crime rates. In the canonical Becker (1968) model, individuals choose strictly between participating in crime or legal work. An individual chooses crime when the expected payoff, a product of the monetary reward and the probability of not being arrested, exceeds the opportunity cost of forgoing legal work, given as a guaranteed wage. Extensions to this model can be made to allow individuals to allocate time between crime and legal work Ehrlich (1973) and for wage to be to be conditional on an individuals human capital Lochner (2004). With this model in mind, an aggregate labor demand shock in the form of a mass layoff can increase the crime rate in a labor market by lowering both the opportunity cost of forgoing legal work and the probability of arrest.

The opportunity cost of legal work is lower for workers displaced in a mass layoff, who suffer a complete loss of wages(disregarding transfer payments), but also for continuously employed and reemployed workers, who through spillover effects, may also become displaced or receive lower wages. Thus ex-ante, it is unclear how the dynamics of the aggregate crime rate will evolve over time. If more workers are marginally induced to commit crimes as a result of direct displacement, a large income shock to a relatively small number of workers, then the aggregate crime rate may initially rise, but decline in the long run as displaced workers become reemployed. On the other hand, as a result of negative spillovers to employment and wages, which may involve smaller income shocks on average, but to a larger number of workers, the aggregate crime rate could remain high or continue to grow in the long run. Regarding the probability of arrest, income shocks may cause local municipalities to suffer lower tax revenue and be unable to invest additional resources in public safety in concurrence with rising crime rates Feler and Senses (2017). This in turn may cause the police to reallocate resources to more serious crimes, lowering the probability of arrest for certain types of crime.

While this model provides a clear rational for participating in economically motivated crimes, the relationship to violent crime is less clear. If violent crime acts as a complement to property crime, then then an increase in violent crime could be purely driven by an increase economically motivated crime. However, there are non-economic channels that could also influence violent crime participation. As noted by Britto et al. (2022), the emotional distress from job loss and worsening economic conditions may change the propensity to commit non-economically motivated violent crimes.

Using firm level administrative data on manufacturing mass layoffs from Trade Adjustment Act petitions, I exploit heterogeneity in the timing and size of mass layoff events across U.S. counties, to estimate the causal effect of a manufacturing labor demand shock on aggregate employment, wages, crime, and police efficiency in the local labor market. Using an event study specification from Callaway and Sant'Anna (2021), I am able to recover an average treatment effect on the treated, but also observe how treatment effects evolve over time. I find that on average a mass layoff of 1% or larger causes a decline in employment of 2.34% that persists fives years after the layoff event. This decline is driven in the years immediately after the layoff by a temporary decline in the employment rate, but persists in the long run as a result of falling labor force participation. Correspondingly, per capita wage declines by 3.32% and the average wage by 0.71% on average. This decline in earnings is partially offset by an increase in government transfers in the form of social security and disability payments of 1.34%, income assistance transfers of 0.60%, and Medicaid transfers of 2.34% leading to a smaller decline in income per capita of 0.87% on average. In turn, I find that, on average, the total crime rate increases by 6.02% relative to the pre-treatment baseline mean and grows over time. This increase is driven by both persistent increases in violent crime of 6.04% and property crime of 6.10% on average.

This paper contributes to a broad empirical literature exists that studies the relationship between labor market conditions and crime, of these there are two general groups that relate to this paper.

The first, exploits administrative data on workers involved in mass layoffs to study the impact of displacement on the probability of committing crime. Workers involved in a mass layoff, defined by the BLS as 50 or more individuals displaced from a single establishment, are less likely to be negatively selected on any particular characteristic. As a result, mass layoffs have been used as a source of exogenous variation to study the effect of job loss on various worker outcomes including earnings Jacobson et al. (1991), mental and physical health Ahammer et al. (2020), and mortality Sullivan and Wachter (2199). Britto et al. (2022) uses administrative data on male workers displaced in Brazil, Bennett and Ouazad (2020) in Denmark, and Rege et al. (2019) in Norway, to study the impact of job loss on committing crime. They all find that workers displaced in mass layoffs are more likely to commit economically motivated property crimes compared to similar non-displaced workers, and Britto et al. (2022) and Rege et al. (2019) also find increases in the likelihood of committing violent crime. Notably, all three papers focus only on male workers, and in particular Britto et al. (2022) find that increases in crime are driven primarily by young low-tenure male workers. I contribute to this literature by studying the effect of large mass layoff events on aggregate crime rates in the wider context of a local labor market. In doing so, I am able to capture the combined effect on crime both for workers directly displaced in a mass layoff and for workers indirectly suffering from negative spillovers to employment and wages. While previous papers find that the propensity of a displaced worker to commit crime peaks in the first year of displacement before levelling off Britto et al. (2022) or decreasing Bennett and Ouazad (2020), Rege et al. (2019), I find that the treatment effects for both property and violent crimes are smallest within the first year of a mass layoff event and instead increase over time up to five years after the mass layoff. These differing treatment dynamics suggest that the role of employment and wage spillovers to non-initially displaced workers plays an important role in determining the long run trajectory of the aggregate crime rate in the local labor market.

The second group of papers, estimate the effect of aggregate labor demand shocks on crime rates in local labor market(e.g. county, commuting zone) using Bartik-type instruments that interact local industry composition with exogenous national level shocks to employment and wages. Beach and Lopresti (2019), Che et al. (2018), and Feler and Senses (2017) follow the design of Autor et al. (2013) and construct shift-share instruments by combining variation in the industry composition of local labor markets with instruments for changes in industry specific US imports from China. These papers are particularly relevant because they study a similar time period in the U.S. from 1990 to the mid 2000s and focus on trade induced aggregate demand shocks to the manufacturing industry. Autor et al. (2013) find that that labor markets experiencing increased import competition suffer persistent decreases in employment, labor force participation, and wages, all of which may induce workers to participate in crime. Beach and Lopresti (2019) and Che et al. (2018), both find significant increases in the rate of economically motivated property crimes and violent crimes, but focus on different channels. Exploiting variation in state Unemployment Insurance (UI) generosity, Beach and Lopresti (2019) find that import competition induced crime increases are completely mitigated in local labor markets in states with the most generous UI programs compared to labor markets suffering similar levels of import competition in less generous states. This supports a story where the main channel driving an increase in crime is through the income shock to directly displaced workers. Che et al. (2018) consider increases in psychological stress, alcohol consumption, and regional income inequality as possible channels, but are unable to point to any one channel as being dominant. Feler and Senses (2017) only find an increase in economically motivated property crimes, but do not directly investigate mechanisms behind increasing crime rates. However, they do find that municipalities experiencing greater import competition suffer decreased tax revenue and respond by cutting expenditures for public goods, but notably not for public safety. Other papers that use shift-share instruments to study the effect of economic conditions on crime include Gould et al. (2002), Raphael and Winter-Ebmer (2001), Öster and Agell (2007), Fougère et al. (2009),Dix-Carneiro et al. (2018), and Dell et al. (2019), of particular interest to this paper are Gould et al. (2002) and Dix-Carneiro et al. (2018). Gould et al. (2002) contemporaneously consider both the unemployment and wage channels by interacting industry specific wage shocks and employment shocks with local industry composition in the US from 1979-1997. They find that the effect of the wage rate, particularly for low-skilled men, is larger and more salient in determining the long-term trajectory of local crime rates compared to changes in the unemployment rate. Dix-Carneiro et al. (2018) consider the channels of labor market conditions (employment and earnings), public good provision, and income inequality in determining short run and long run homicide rates. Using an instrument combining industry specific tariff changes in Brazil in the 1990s with local industry composition, they find that the employment rate is the more important driver of the homicide rate than earnings, public good provision, or income inequality.

I contribute to this literature in a number of ways. First, compared to Bartik-type instruments, exploiting the precise timing of mass layoff events in a difference-in-difference framework allows me to measure the treatment dynamics of crime rates in concurrence with the treatment effects on employment, income, and wages. I find that while the treatment effect on the employment rate dissipates within three years of the mass layoff, the effect on the average wage persist and continues to grow five years after the layoff with the total crime rate. Second, I explore treatment effect heterogeneity by the relative I find that increases in violent crime rates are driven by counties experiencing mass layoffs in low wage manufacturing sectors, which has comparably lower wages to other manufacturing industries. Finally, I investigate the effect on public good provision specifically in terms of police numbers and police clearance rates. I find that while employment in local government declines, police officer numbers do not decline. This is consistent with Feler and Senses (2017) finding that municipalities suffering revenue losses from a labor demand shock do not decrease public safety expenditures. I further explore whether police officer efficiency is affected, and find that while the number of crimes per police officer increases significantly, police clearance rates of property and violent crimes do not decline.

2 Data

To estimate the effect of a mass layoff on labor market conditions and crime, I combine firm-level administrative data on the size and timing of layoffs with economic and aggregated police agency data at the county level. I choose to define local labor markets at the county level because of the highly localized nature of crime. Previous work looking at the effect of aggregate demand shocks on crime also conduct their main analysis at the county level Che et al. (2018), Beach and Lopresti (2019), Gould et al. (2002). I also show results for analysis at the commuting zone level (still need to do this).

2.1 Data Sources

I use firm level administrative data from petitions submitted under the Trade Adjustment Assistance (TAA) program to construct county level mass layoff events. The TAA is a federal transfer program established in 1962 to provide economic assistance to workers displaced by trade. The program has undergone modifications over the decades, but since 1988 benefits have come in two forms: coverage of training costs for every year a qualified worker is retraining, up to a statutory maximum of three years and expanded unemployment insurance (UI) benefits while training Hyman (2022). Evaluating the effectiveness of the TAA as a transfer program is not the focus of this paper, however I do show robustness to certification status in table X. The primary reason I use TAA data to identify mass layoff events is the extensive national coverage of the program (1974-2022) and the rich administrative data provided by each petition on the number of workers laid off at the firm, the address of the firm, the 4 digit SIC or 6 digit NAICS code of the firm, and the date the petition was submitted.

I assign workers from each petition to a county based on the firm address. Given that not all workers live in same county as they work, this will lead me to over estimate the number of workers laid off in the firm address county for a given year and underestimate the number of workers laid off in neighboring counties. To the extent that this causes the firm address county to be classified as treated and neighboring counties as not treated, this would bias my estimate of mass layoff effects towards zero. I cannot observe the exact date a layoff occurred on the petition and therefore use the year of the investigation date as an approximation of the year of displacement. This has the potential to cause some layoffs to be defined as treated a year later than they occurred. I address this issue in section 3 by allowing for treatment anticipation to occur a year before the treatment date. To allow for heterogeneity in mass layoff event by sector, I crosswalk the the SIC or NAICS code on each petition to a 2-digit 2012 NAICS code and then sum the number of workers in a 2-digit NAICS code in a county year. A mass layoff event is then defined as the count of workers in a 2-digit NAICS sector laid off in a county in a single year. TAA petition data inherently under counts the number of workers involved in mass layoff events by being mostly representative of layoffs in the manufacturing sector and only account for workers for whom a petition is filed. This under-counting could attenuate my estimates by classifying counties that do experience large layoffs not accounted for in the TAA petition data as not treated. Therefore estimates should be thought as conservative. Counties with relatively larger numbers of workers filing for TAA benefits, may differ from other counties demographically, economically, and on crime levels. I explore these differences in subsection 2.3 and address the potential for endogenous selection into treatment in section 3.

Crime rates, crime clearance rates, and police officer variables are constructed using two data sets from the FBI Uniform Crime Report (UCR), the Law Enforcement Officers Killed and Assault (LEOKA) data and the Offenses Known and Clearances by Arrest (OKCA) data. Both data sets consists of annual reports submitted to the FBI by individual police agencies. A police agency can be a city police department, a county sheriffs departments, or a specialized state department such as a university police department or a port authority. The LEOKA data contains the count of police officers and non-officer police employees employed at each agency. The OKCA contains data on 7 index crimes–homicide, rape, robbery, aggravated assault, burglary, theft, and motor vehicle theft. In accordance with the FBI definition, I define a violent crimes as homicide, rape, robbery, or aggravated assault, and property crimes as burglary, theft, or motor vehicle theft. I assign a police agency to a county if a majority of the population covered by the agency lives within the county. County crime rates are constructed as the population weighted averages of crime rates from agencies within the county.

A notable issue with the OKCA data set is that reporting is not mandatory and for a specific year, agencies may not submit a report at all or may only submit crime counts for a subset of months. In this last case, it is unclear if an agency is reporting aggregates in larger sub-year intervals (e.g. bimonthly, quarterly) or missing data during certain months Kaplan (2021). To address the issue of missing data I define an agency as missing a year of data if an agency reports less than 9 months of data in a year and only include agencies missing 2 years or less of data from 1990-2010. If an agency reports between 9 and 11 months in a year, I multiply the annual crime rate by 12/(12 - number of months missing). This imputation introduces additional error because crime rates are not homogeneous across months, but increases the geographic coverage. I show that my main results are robust to only including data from agencies reporting all 12 months every year (table X). Another issue common to all police reported crime data is under counting from individuals selectively reporting crimes to police. I address this issue by working with the log-transformation of crime rates, this minimizes the multiplicative measurement error observed in police recorded crime rates by transforming it into a less harmful additive mechanism Pina-Sánchez et al. (2023). I also weight all crime outcome regressions by the total population covered by the agencies since smaller police agencies are likely to have higher reporting errors.

I use economic and demographic data from a number of sources to measure effects on labor market conditions and to control for differences between treated and controls counties. To estimate the effects on county wide employment, labor force participation and population, I combine data on employment and labor force levels from the Burea of Labor Statistics Local Area Unemployment Statistics (LAUS) with population data from the Surveillance, Epidemiology, and End Results program (SEER). To estimate effects on county transfer payments and income per capita, I use regional accounts data from the Bureau of Economic Analysis (BEA). To estimate the effect of on wages and further decompose the effect on employment and wages by sector, I use annual employment and wage data from the Quarterly Census on Employment and Wages (QCEW).

2.2 Sample Selection and Defining Treatment

While data on TAA petitions are available starting in 1974 and UCR data on crime in 1960, I limit my sample to 1990 to 2010 for two reason reasons. First, county level data on employment rates and labor force size are only available from the LAUS starting in 1990. Second, the number of workers filing for TAA certification drops off markedly in 2010 and continues post TAA eligibility and funding cuts in 2011 Guth (2017). This raises the potential for increased attenuation bias if workers become less likely to file TAA petitions become less representative of the total number of mass layoffs occurring.

To define treatment, I use a similar framework to other papers that use mass layoffs as an aggregate labor demand shock to local labor markets, by choosing a layoff cutoff size p to define a binary treatment Carlson (2015), Foote et al. (2019). Let p be the percent of employees displaced in a mass layoff event as a percentage of the county labor force, then treatment for a county c turns on and stays on if a layoff larger than p occurs. Assuming that the treatment effect is weakly monotonically increasing with the size of the layoff, then as the treatment size cutoff p increases, the treatment effect for the treatment group will increase, but this effect will be attenuated by counties with layoffs slightly smaller than p being now included in the control group.

I empirically choose a cutoff size using a non-paramtric approach in a similar vein to Muralidharan and Prakash (2017) and Kelly et al. (2020). I estimate the ATT for the total crime rate for my main specification, defined in section 3, for a set of cutoff sizes ranging from the 20th to 90th percentile of the layoff size distribution, approximately (0.25% to 2%). I find that the treatment effect becomes consistently significant for cutoffs 1% and larger as shown in figure A1. I therefore choose a cutoff size of 1%.

2.3 Descriptive Statistics

Figure 1(a) displays the number of workers filing for TAA each year from 1990 to 2020. The number of workers filing increases during economic recessions such as 2001 and 2009, but remains relatively high before 2011. Figure 1(b) plots the number of counties experiencing a layoff larger than 1% for the first time each year. The number of counties treated each year is declining on average over time, but there is still sufficient variation to allow for sizable treatment groups each year. Figure A2 depicts the geographic variation in treatment and treatment timing across the U.S. Treated counties occur more frequently in certain states, but there is still variation across regions. Counties not included in the sample have too much missing crime data.

3 Empirical Specification

The variation in timing of mass layoff events lends itself naturally to a staggered difference in difference (DiD) specification. Goodman-Bacon(2021) shows that the standard Two-Way Fixed Effects (TWFE) specifications suffers from "bad comparisons" between treated and not-yet treated units that may bias the TWFE parameter. To avoid this issue I use Callaway Sant'anna (2021) (CS) to estimate group-time average treatment effects. The ability of CS to allow for limited anticipation and for the parallel trends assumption to hold conditional on baseline covariates are particularly useful for the TAA data. As a result of the imprecision of the treatment timing measure combined with the potential for firms and workers to react to the news of a mass layoff means, treatment may appear to be or actually be anticipated. To meet the assumption of limited anticipation required for identification in CS, I allow treatment to be anticipated the year before the event. Treated counties may also be endogenously selecting into treatment based on their industry composition and labor force characteristics, and therefore parallel trends may only hold after conditioning on baseline covariates. I condition on the percent of manufacturing workers, percent of non-college educated individuals, median household income, and percent of individuals living in rural areas in 1990 for each county.

My main specification is as follows, define county, c, as belonging to treatment year group g, if county c experiences a layoff larger than 1% in year g. Then for each treatment year g and each $t \geq g$ the following 2x2 DiD specification is estimated,

$$Y_{ct} = \alpha + \beta_{ct}\mathbb{1}[G_c = g] \times \text{Post}_t + \gamma\mathbb{1}[G_c = g] + \delta\text{Post}_t + \sigma X_c + \epsilon_{ct} \tag{1}$$

where Y_{ct} is the outcome of interest in county c in year t, $\mathbb{1}[G_c = g]$ is an indicator for whether county c belongs to treatment group g, Post_t is an indicator for whether year $t \geq g$, β_{ct} is the 2x2 DiD parameter of interest, X_c a vector of covariates conditioned on, and ϵ_{ct} the error term. CS proposes a number of aggregations methods for summarizing the recovered β_{ct}'s. Given that the persistence of the treatment effect is unknown, I choose to summarize the group-time average treatment effects using the following event-study style aggregate proposed by CS,

$$\hat{\theta}(e) = \sum_{g=1992}^{2005} \sum_{t=1992}^{2010} \mathbb{1}[t - g + 1 = e] P(G_g = 1 | t - g + 1 \geq e) \hat{\beta}_{gt} \tag{2}$$

Here $\hat{\theta}(e)$ identifies the treatment effect for units that have been exposed to treatment for e years. Finally, to identify the overall ATT after 5 years of treatment,

$$\hat{\theta}^O = \frac{1}{6} \sum_{e=0}^{5} \hat{\theta}(e) \tag{3}$$

This allows for the dynamics of the treatment effect to be observed for five years after the mass layoff. Following the recommendation of Callaway and Sant'Anna (2021), I excludes counties treated between 2006 and 2010 from the treatment group to ensure that dynamic treatment effects are estimated from the same sample of units in all post periods. I also drop counties treated in 1990 and 1991 from the sample given that they have no pre-treatment years that are not anticipated. Following the recommendation of Miller (2023), I choose a pre-treatment length of six years to show that there are no significant treatment effects on outcomes in the years leading up to the layoff event.

4 Effects on Labor Market

To motivate the use of mass layoffs as an aggregate labor demand shock, in this section I illustrate the relationship between mass layoffs, employment, wage, and transfers in the local labor market. A mass layoff can decrease the opportunity cost of legal work for the marginal worker through multiple channels. Workers who are displaced directly in the layoff and drop out of the labor market suffer a complete loss of wages (excluding transfer payments) and those who eventually become reemployed may do so at a lower wage. Through spillover effects, workers in other sectors can also become displaced or receive a lower wage. The event study figures help visualize how the treatment dynamics for these channels differ over time. I begin by estimating the effect on employment. In similar fashion to Foote et al. (2019), I decompose the effect into the effects on the employment rate, labor force participation rate, and population level. I next explore the effect on income, wage, and transfer payments. Finally, I show the effect on employment and wage by sector to demonstrate the spillover effects.

Table 2 shows the event-study aggregated ATT (equation 3) for the effect of a mass layoff on employment, labor force participation, and population. I find that, on average, employment declines by 2.34% after 5 years. Decomposing the effect, roughly two-fifths comes from a from a 1.07% decline in labor force participation. The remaining two-fifths can be approximately divided between a decrease in the employment rate and population, of roughly 0.42% and 0.63% respectively.

Figure 2 shows the corresponding estimated treatment effects in the years before and after the layoff. For all outcomes, the pre-treatment periods, excluding the year before the layoff, are indistinguishable from a null effect, lending credence to the identifying assumption that employment, labor force participation, and population in treated counties would have evolved similarly to control counties in the absence of a mass layoff. The negative effect on employment begins developing the year before the mass layoff and persists over the next five years. The significant effect the year before the layoff is indicative of the imprecision of treatment timing, but also suggests that other firms may layoff workers or marginally attached workers may exit the labor market in anticipation of the mass layoff. The negative effect on the employment rate peaks the year after the layoff and begins diminishing over the next two years, after which the estimated coefficients become statistically indistinguishable from 0. The negative effect on labor force participation manifests the year after the mass layoff and persists after leveling off after two years. The effect on population while negative and appearing to continuously decline after the mass layoff is imprecisely estimated. Overall the event studies suggest that mass layoff events were not preceded by significant long term labor market declines. The initial drop in employment is driven evenly by workers exiting the labor force and filing for unemployment, however the persistence of the decline can be attributed to the reduction in labor force participation. The dissipation of the effect on the employment rate implies that workers who initially file for unemployment eventually exit the labor force, become reemployed, or migrate.

Table 3 displays the event-study aggregated ATT for a 1% mass layoff on income, total wages per capita, and average wage. Income and wage differ in that income includes wages and rents received for provision of labor, land, and capital as well as personal current transfer receipts. Total wages per capita measures total wages averaged over the whole county population, while average wage measures total wages averaged just over those employed. I find that, on average income per capita declines by 1.03%, a decline of \$264 per capita in terms of the pre-treatment mean. This can be attributed to an average decline in per capita wages of 3.32%, $286 per capita in terms of the pre-treatment mean, and an average increase in transfers per capita of 1.14%, an increase of $58 per capita in terms of the pre-treatment mean. This demonstrates that government transfers only partially insure for lost wages. The average wage among employed workers declines on average by 0.71%. This suggests that the decline in wages per capita is driven both by lost wages from displaced workers, but also by declining wages among employed workers. This is consistent with firms responding to a decline in aggregate labor demand by lowering wages. However, it is important to note that the change in the composition of workers post mass layoff may bias the estimated effect on the average wage. If workers that are initially displaced in the mass layoff have a lowerhigher wage than the county average wage, then ceteris paribus, the average county wage would increasedecrease post layoff. In section 6.1, I look at the effect on average wage in the manufacturing sector by relatively low versus high wage sub-sectors to estimate the size of the bias caused by changes in composition. It's also important to note that these effects are averages across the entire distribution of incomes in the labor market, and that the effects may driven by one segment of the income distribution.

Figure 3 displays the corresponding estimated treatment effects for income and wage. For all outcomes, the pre-treatment periods are insignificant, lending support to the identification assumption that income and wage would have evolved similarly to control counties in the absence of a mass layoff. The negative effect on income per capita appears the year of the layoff and peaks and stabilizes one year after. In contrast, the negative effect on wage per capita manifests the year of the layoff and continues to decline fives years afterwards. The continued decline in wages per capita is partially driven by the decline in average wage, which appears to begin declining the year of the layoff, but does not become significant until five years after the layoff. Furthermore, the stabilization of the treatment effect on income per capita the first year after the layoff, despite the continued growth of the negative treatment effect on wage per capita, suggests that government transfers may be mitigating a continued decline in income.

There are multiple government transfer programs that workers may take up after being displaced or experiencing indirect income shocks. Table 4 shows the event-study aggregated ATT for a 1% mass layoff on log government transfers per capita broken down by four of the largest transfer program categories: unemployment insurance (UI), Social Security retirement and disability, income maintenance programs (Supplemental Security Income, Earned Income Tax Credit, Supplemental Nutrition Assistance Program), and Medicaid.

I find that unemployment transfers increase by 1.8% on average, though this effect is imprecise. The corresponding event-study in figure 6a reveals that this effect is driven by an increase of approximately 7% in the year of and after the mass layoff, but dissipates after two years. Given that state unemployment benefit lengths do not exceed more than a year and take up of extended unemployment benefits through TAA has been documented to be relatively low Hyman (2022), it's not surprising that the treatment effect on UI transfers dissipates quicker than the effect on employment rate. This suggests that a non-insignificant number of unemployed workers experience an income shock from a loss of unemployment benefits and may be induced on the margin to participate in crime as a result. Workers who respond to displacement or decreasing wages by exiting the labor force may take up Social Security (SS) retirement and disability benefits to supplement their income. I find that on average, retirement and disability transfers increase by 1.34%. Figure 6b shows that the corresponding treatment effects appear grow over time. Federal income assistance transfers(Supplemental Security Income, Earned Income Tax Credit, Supplemental Nutrition Assistance Program), increase by 0.60% over the baseline. Looking at the corresponding event-study in figure 6c, the effect peaks the year after the mass layoff and persists fives years out, but is imprecisely estimated. I find a significant positive effect on medicaid transfers of 2.34%. Figure 6d shows that the treatment effects, while individual imprecisely estimated appear to grow over time. Overall, these results evidence that on average post mass layoff, displaced workers initially take up unemployment insurance to supplement lost income, but in the long-run both displaced workers and indirectly effected workers take up long-term income supplementing transfers in the form of social security, income assistance transfers, and medicaid. These results also suggest that the negative effects on income are salient among lower earners of the income distribution.

As an aggregate labor demand shock, a mass layoff can cause income shocks and lower the opportunity cost of legal work in the form of displacement and declining wages for non-directly displaced workers. Table 5 shows the event-study aggregated ATT for a mass layoff on employment and wage by NAICS sector groups. The average effect on total employment is larger using the QCEW measure of employment compared to the effect found using LAUS data in table 2. This can possibly be attributed to the QCEW excluding self-employed individuals, who may be more likely to remain employed post layoff compared to individuals working at large firms. On average, I find no significant effect on local government employment. This implies that on average local governments are not responding to potential revenue losses by laying off workers. Now focusing on good producing sectors, I find that on average employment in manufacturing declines significantly by 11.1% compared to baseline. The size of the effect is consistent with the fact that the median mass layoff composing 1.8% of the county labor force and that the manufacturing sector on average composing 11% of the labor force. There are null effects on employment in both the mining and energy and construction sectors. Turning to the service sectors, on average compared to the baseline, employment declines significantly by 2.48% in finance and real estate and 2.17% in hospitality and food sectors. The negative effect on employment in the professional services sector is relatively large, 2.55%, but imprecisely estimated. There are null effects on employment in the retail and transportation and education and health secorts. The corresponding event studies in figure A3 suggest that for service sectors experiencing significant declines in employment, the effect grows slowly over time compared to the immediate effect on employment in the manufacturing sector. This is consistent with a model, as in Autor et al. (2013) where labor demands shocks to goods-producing sectors cause declines in demand for tradable-services in service sectors, but this effect takes time to manifest.

Looking at the effect on the average wage, I find a null effect for manufacturing. This is consistent with Autor et al. (2013) finding that aggregate labor demand shocks to manufacturing, in the form of increased imports from China, did not effect the average manufacturing wage. One explanation they provide for this null effect is that if firms retain the most productive workers, the effect on the average wage will be biased upwards. I further explore this explanation by looking at heterogeneity in the wage effect across manufacturing sectors in section 6. By contrast, I do find significant negative effects on average wage for construction 1.27%, finance and real estate 1.10% (<10%), and education and health 1.16%. The positive effect on mining and energy is also sizable, 1.23%, but imprecise. Notably, for the sectors where I do find significant negative effects on average wage, the corresponding effects on employment are negative or nulls. This implies that the negative effect is not being mechanically driven by firms in these sectors laying off the least productive workers. Overall the spillover effects on employment and wage to certain non-manufacturing sectors evidences that mass layoffs can lower the opportunity cost of forgoing legal work for non-directly displaced workers in the form of displacement and lower wages.

5 Effects on Crime

I now measure the effect of mass layoffs on local crime rates. Crime rates can increases as a result of unemployment and declining wages lowering the opportunity cost of participating in legal work. Ex ante, it is unclear how the treatment dynamics of the aggregate crime rate will evolve over time. Both property and violent crime may increase beginning the year of the layoff as a result of the immediate income shock and psychological stress experienced by displaced workers. If only directly displaced workers were induced to participate in crime, following previous work on the criminal participation of workers displaced in mass layoffs Britto et al. (2022),Rege et al. (2019), Bennett and Ouazad (2020), we would expect the effect on the total crime rate to level off or decline in the years after the layoff as displaced workers become reemployed or migrate. However, given that, on average, a mass layoff reduces the employment and wage in some non-manufacturing sectors and that some of these treatment effects do not appear until years after the layoff, non-directly displaced workers may also be induced to participate in crime over time. As a result, the treatment effect on total crime may continue to persist or grow over time. Alternatively, if crime rates do increase and spending on public safety remains constant, as Feler and Senses (2017) find, or declines, the arrest rate for certain types of crime may decline as constrained police departments reallocate resources to the most serious crimes. In this case, if the probability of arrest declines, the treatment effect on crime may also persist or grow overt time. In section 6, I explore these different channels. First though, I present results on the average treatment effect and corresponding dynamics on aggregate crime rates as well as individual types of crime.

Table 6 presents the effect of a mass layoff on log aggregate crime rates. I find that, on average, the total crime rate increases by 6.02% relative to the baseline mean. The property and violent crime rates both increase by similar percentages of 6.10% and 6.04% respectively. Initially this may seem surprising, but it's important to keep two things in mind. One, a 6% increase in violent crime versus property crime represents a relatively lower increase in absolute crime levels, an increase of 4 violent crimes versus 14 property crimes per 10000 people over the baseline mean. Second, as a result of the UCR hierarchy rule, individuals simultaneously committing both property and violent crimes in a single incident, would only be recorded as committing the most serious violent crime. Thus, the increase in property crimes may be under counted if violent crime acts a complement to property crime.

Figure 4 depicts the corresponding estimated treatment effects for the aggregate crime rates in the years before and after a mass layoff. For all outcomes, the pre-treatment periods, are indistinguishable from a null effect, supporting the identifying assumption that crime rates in treated counties would have evolved similarly to similar never-treated counties in the absence of a mass layoff event. The effect on both the property crime rates manifests the year after the layoff and grows and persists 5 years after the layoff, driving the treatment effect on total crime to follow a similar pattern. The treatment effects on violent crime are not individually significant, but suggest that the treatment effect may be growing over time.

To further dissect the types of crime driving the increase in property and violent crime rates, table 5 also exhibits the effect on index property crimes and violent crimes. There are relatively large increases in the burglary and theft rate of 5.72% and 5.97% respectively. The effect of motor vehicle thefts is also relatively large 3.80%, but imprecise. Finding significant increases in the most common non-violent economically motivated crime supports economic channels such as income liquidity constraints and lower opportunity cost of forgoing legal work as the primary driver of crime increases. There are relatively small and insignificant effects on the murder and rape rate, 1.31% and -1.57%, compared to sizable effects on assault and robbery, 5.55% and 4.22%, though only the effect on assault is significant. The small and insignificant effects on murder and rape are not surprising given that these are both rare and extreme crimes (<2% of violent crimes). The relatively larger effect on robbery, while imprecisely estimated (robbery constitutes only 13% of violent crimes), could be explained purely by economically motivated channels. While it is possible that the effect on assault is driven by assault being a complement to economically motivated property crimes, it is also likely the result of non-economically motivated channels too such as psychological stress. Figures A8 and A9 display the treatments effects for index crimes before and after the mass layoff. For all index crimes the pre-treatment effects are insignificant, this further lends support to the identification assumption that crime rates would have evolved similarly in control and treated counties in the absence of a mass layoff. The treatment dynamics for index crimes with significant effects are similar to the corresponding treatment dynamics of the property and violent crime aggregates.

6 Heterogeneity and Mechanisms

So far, the results indicate that a mass layoff causes a decline in manufacturing employment, and negative spillovers to employment and wages in both goods producing and service sectors in the local labor market. This decline in labor market conditions is associated with a persistent increase in both property and violent crime rates. Now, I explore potential mechanisms that could explain these effects.

6.1 Heterogeneity by Wage

Workers displaced in relatively low wage manufacturing sectors are likely to experience larger lifetime earnings losses and higher adjustment frictions compared to displaced workers in relatively high wage manufacturing sectors Autor et al. (2014). As a result, local labor markets experiencing a mass layoff primarily composed of workers in low wage manufacturing sectors may be more likely to experience increases in crime rates through a number of channels. First, low wage workers are likely to face greater liquidity constraints upon displacement, increasing the value of economically motivated crime as a form of subsistence consumption. Second, if low wage workers experience greater adjustment frictions and relatively lower subsequent wage offers, their opportunity cost of forgoing legal work will be lower and thus the propensity to participate in crime will be higher. Third, low wage workers may face greater levels of psychological stress upon displacement, increasing their propensity to participate in non-economically motivated violent crimes. Finally, displaced low wage workers may create larger negative spillovers on employment and wages in other low wage sectors by increasing the labor supply in these sectors as they seek reemployment.

I assess the degree to which the wage of displaced workers causes differential effects on local crime rates by estimating heterogeneous treatment effects for crime rates by the following measure of relative wage. For each mass layoff, I estimate the average wage of displaced workers by matching each firm involved in the mass layoff by it's 2-digit SIC code to the corresponding average state wage for that 2-digit SIC code in 1990. I use state averages because the censorship in QCEW 2-digit SIC wage data at the county level would lead to a substantial reduction in the treatment sample. I use data from 1990 versus two-years before treatment for two reasons: to avoid the potential for earlier layoffs to effect the state average wage estimate for later layoffs in the same state and to allow real wage comparisons to be made in the same base year. To account for differences in cost of living across labor markets, I then take the difference between the weighted average 2-digit SIC wage of the mass layoff firms and the county average wage. I refer to this measure as the relative wage gap. I then classify mass layoffs as high wage or low wage by whether their relative wage gap falls above or below the treatment sample median (-$5174), creating two sub-samples composed of the same controls counties and either low wage or high wage treated counties. Figure A10 displays the distribution of the relative wage gap for treated counties relative to the sample median.

Table 7 shows the results of the heterogeneity analysis by low versus high wage classification. Counties experiencing low wage mass layoff events experience significant increases in both property and violent crime rates. By contrast, counties experiencing high wage mass layoff events experience only significant increases in the property crime rate. The effect on the property crime rate is larger for low wage mass layoffs, the difference in the effect is significant at 4.73%. This suggests that low wage mass layoff events may increase economically motivated crimes relative to high wage mass layoffs as a result of low wage workers facing greater liquidity constraints or lower opportunity costs of forgoing legal work. On the other hand, only counties experiencing shocks to low wage sectors experience a significant increase in the violent crime rate. The difference in the effect is quite large at 15.40%. The size of this difference suggests that low wage mass layoff events disproportionately effect channels that increase violent crime, namely psychological stress. Figure 8 shows the corresponding event studies for the effect on violent and property crime rates for counties experiencing high and low wage layoffs.

6.2 Changes in Police Deterrence

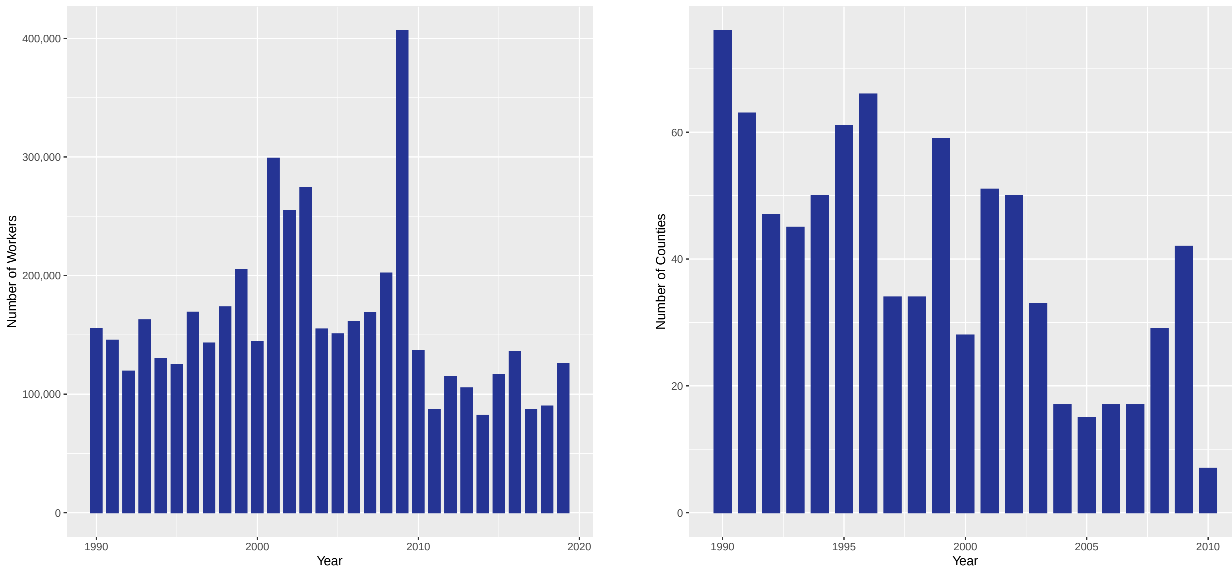
While mass layoffs may induce workers to participate in crime through direct income shocks and decreasing the opportunity cost of forgoing legal work, the benefit of participating in crime may also change if the probability of being arrested declines due to police departments facing binding resource constraints. The negative effect of mass layoffs on average income per capita suggest that local government revenue is also likely declining. Feler and Senses (2017) find that local governments suffering revenue losses as a result of manufacturing layoffs from increased import competition with China, did not decrease spending on public safety, however they importantly note that even if public safety spending remains stable, this may be an inefficient response if crime rates are simultaneously rising. I build upon this analysis by studying if police departments respond to mass layoffs by laying off or hiring more officer or non-officer employees. I then explore the effect on the number of crimes per officer as a measure of police resource constraint, and finally look at the effect on the clearance rate of property and violent crimes to measure if police efficiency in solving crimes is changing as a result.

Table 8 shows the treatment effect of a mass layoff on employment at police agencies. There is no significant effect on employment for officers or non-officer employees. This suggests that police agencies are neither hiring nor firing employees in response to mass layoffs. This is consistent with Feler and Senses (2017) finding that public safety spending remains stable in response to aggregate labor demand shocks in the manufacturing sector. While the number of crimes per police officer increases by 3.5%, the property and violent crime clearance rates do not decrease significantly. This suggests that on average police officers in treated counties are responsible for handling a larger number of crimes per officer, however this tighter resource constraint is not binding and does cause the number of violent or property crimes cleared to decline significantly. Overall these results suggest that individuals are not being induced to commit more crimes on the probability of arrest margin.

7 Conclusion

I study the effect of mass layoffs in the manufacturing sector on local labor market conditions and crime rates using firm-level administrative data on workers applying for Trade Adjustment Assistance from 1990 to 2010. I find that, on average, a mass layoff event causes a persistent decline in employment up to 5 years after the layoff. While the initial decline in employment is driven evenly by a temporary decline in the employment rate and a decline in labor force participation, the persistence of the employment effect can be attributed to the decline in labor force participation. While government transfer payments increase, the decline in per capita wages leads to a persistent decline in per capita income as well. The decline in economic conditions is associated with, on average, an increase in the violent crime rate of 6.04% and property crime rate of 6.10%, leading to a persistent increase in the total crime rate of 6.02%. Heterogeneity analysis reveals that the increase in the violent crime rate can be attributed to counties experiencing layoffs in low wage manufacturing sectors. Furthermore, while the number of crimes per police officer ratio increases, the proportion of crimes cleared by police does not decline, suggesting that changes in police deterrence are not responsible for the increase in crime.

8 Figures



(a) Annual Count of Workers Applying for TAA 1990-2019

(b) Treatment timing of first mass layoff 1990-2010

Figure 1: Timing of Mass Layoff Events

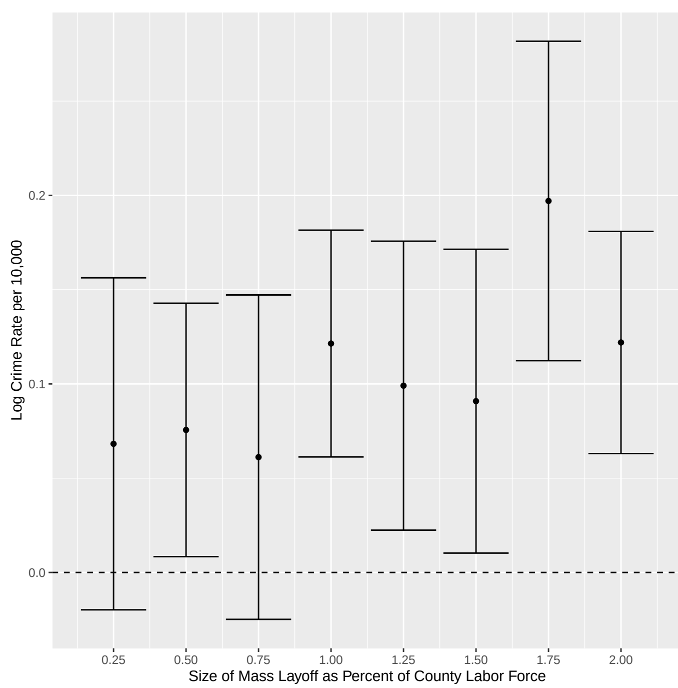


Figure 2: ATT for log total crime rate by layoff treatment cutoff size.

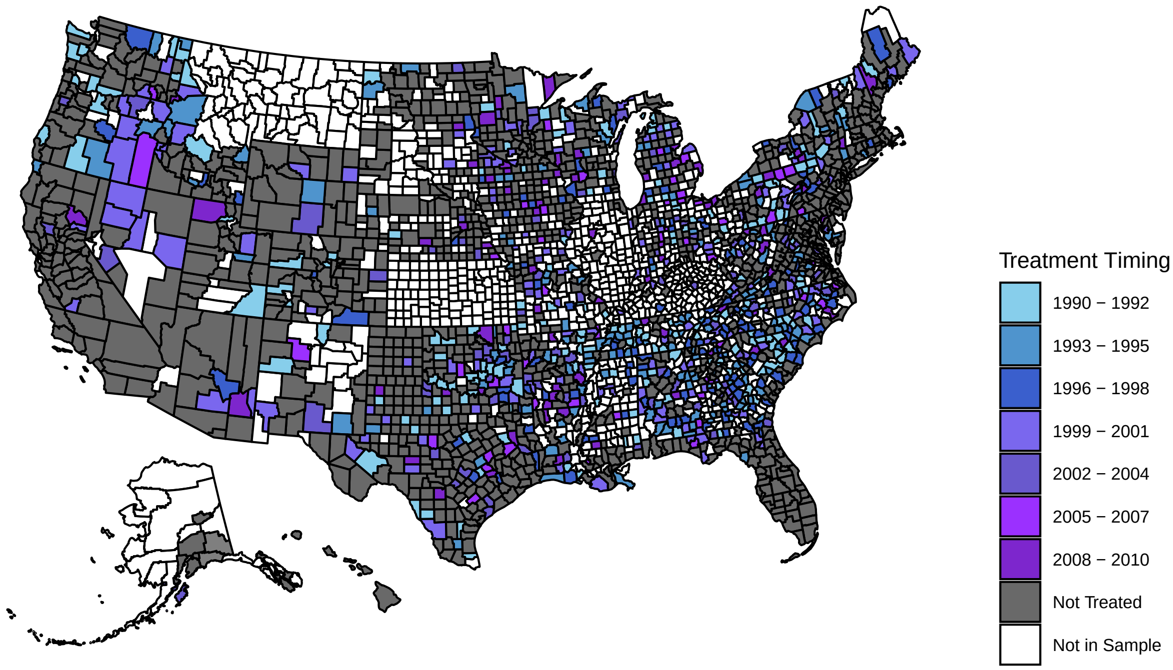
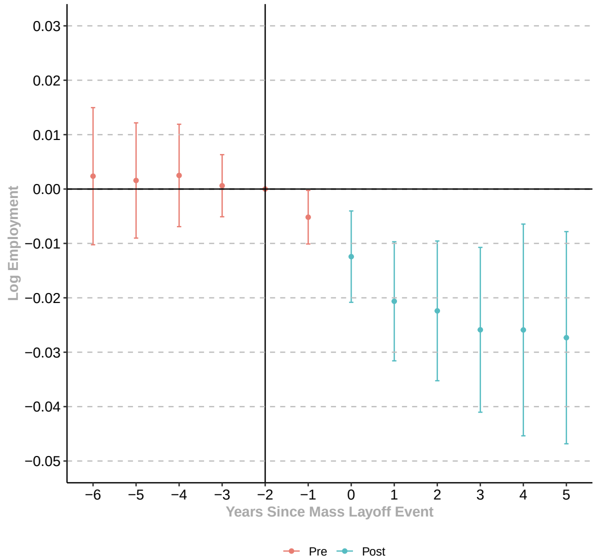
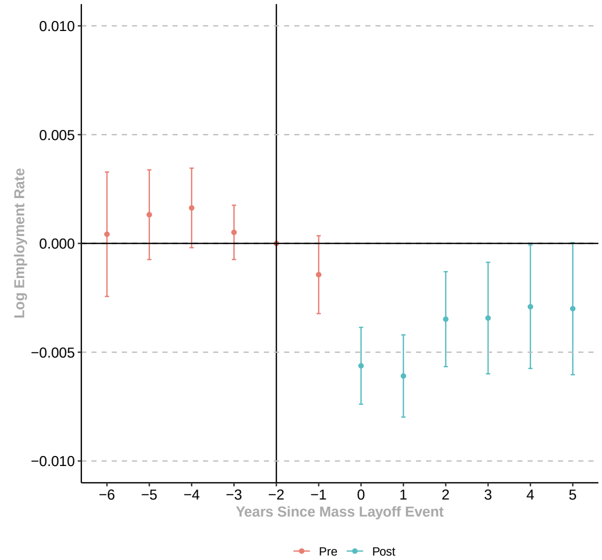


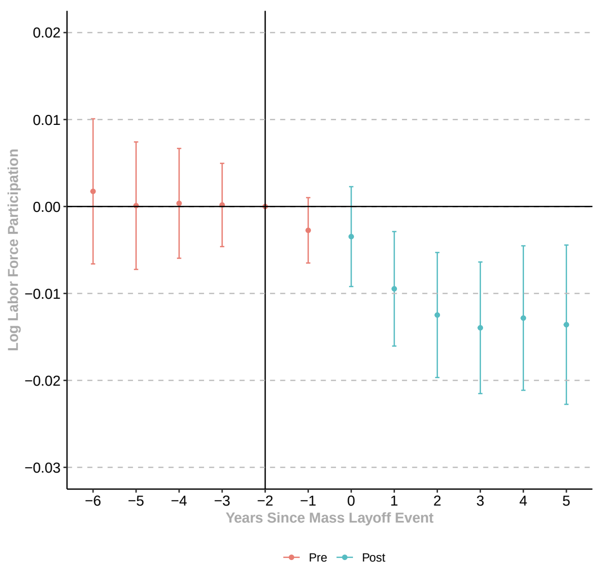
Figure 3: Map of Counties by Treatment Status and Timing of First Mass Layoff



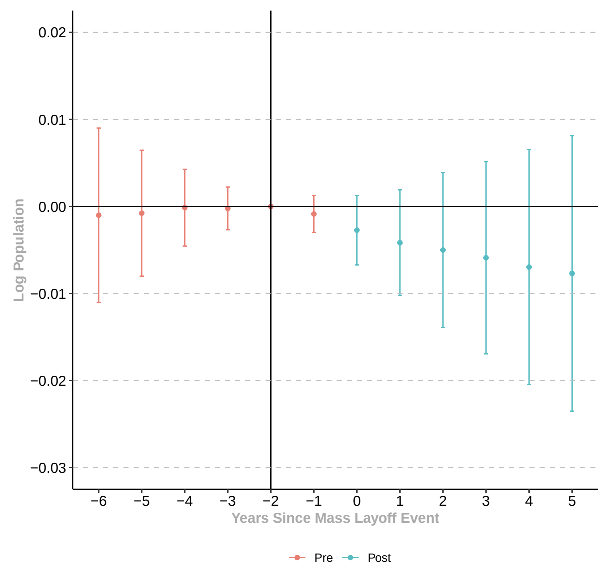
(a) Log Employment



(b) Log Employment Rate

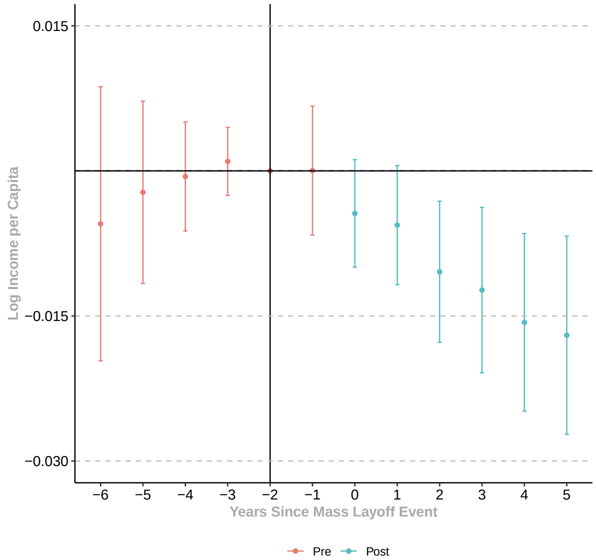


(c) Log Labor Force Participation

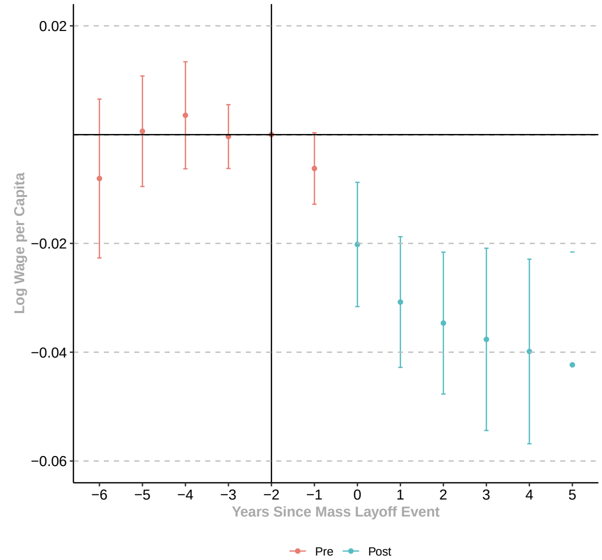


(d) Log Population

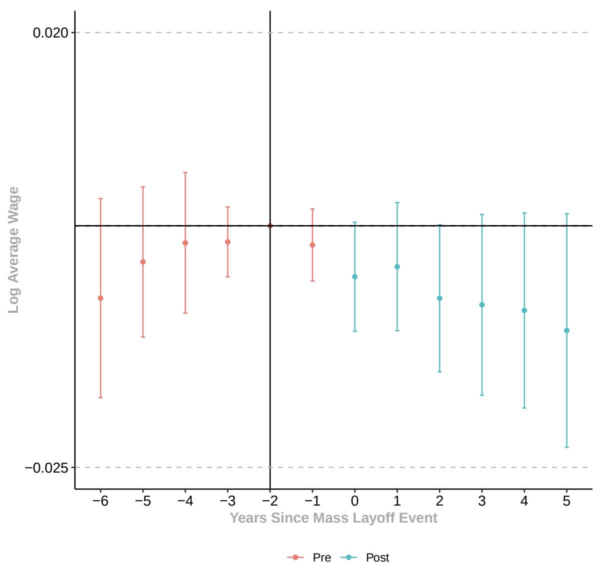
Figure 4: This figures displays the event study treatment effects of Callaway Sant'Anna (2021) (i.e, the average treatment effect for counties that have been treated for e = t-g time periods- see equation(2)). The point estimates measure the effect in relation to year g-2, two years before the mass layoff event. The bars represent a 95% uniform confidence band. The control group includes never-treated counties and counties not treated by year g + 5. Data Sources: BLS Local Area Unemployment Statistics and SEER



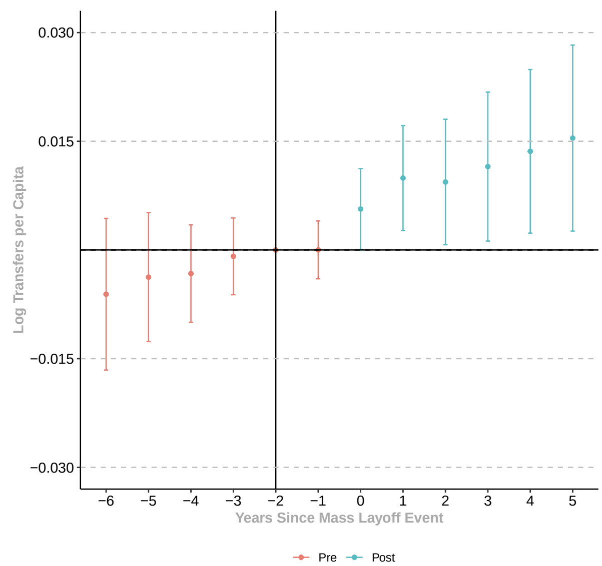
(a) Log Income per Capita



(b) Log Wage per Capita

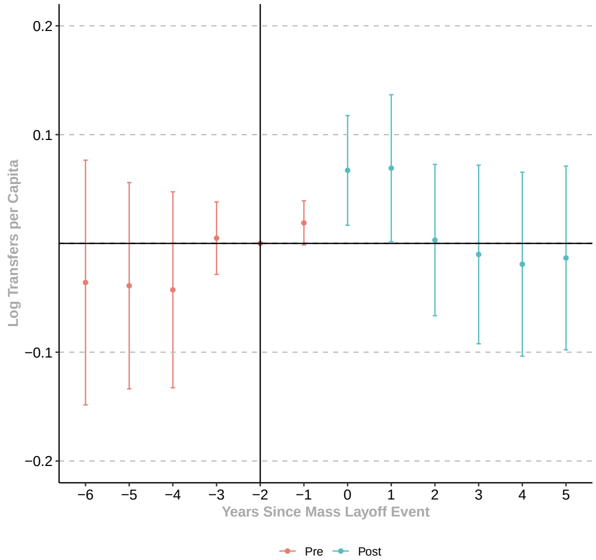


(c) Log Average Wage

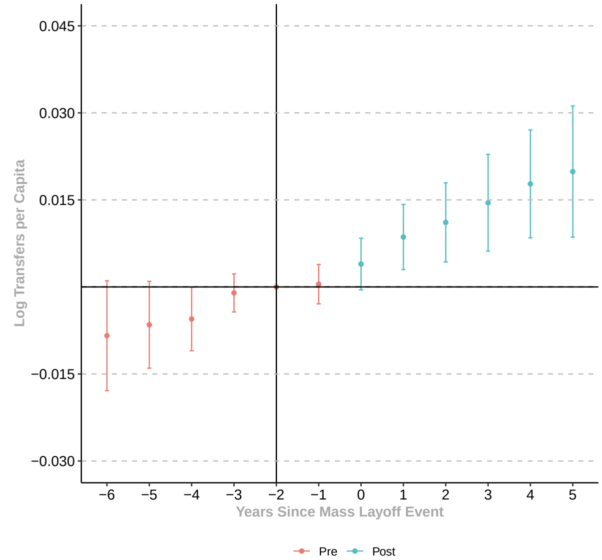


(d) Log Transfers per Capita

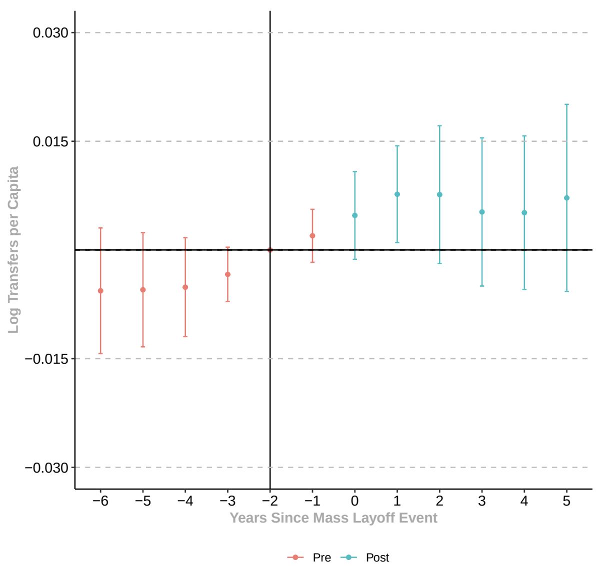
Figure 5: This figures displays the event study treatment effects of Callaway Sant'Anna (2021) (i.e, the average treatment effect for counties that have been treated for e = t-g time periods- see equation(2)). The point estimates measure the effect in relation to year g-2, two years before the mass layoff event. The bars represent a 95% uniform confidence band. The control group includes never-treated counties and counties not treated by year g + 5. Data Sources: BEA Regional Accounts, QCEW Annual Employment and Wage, SEER



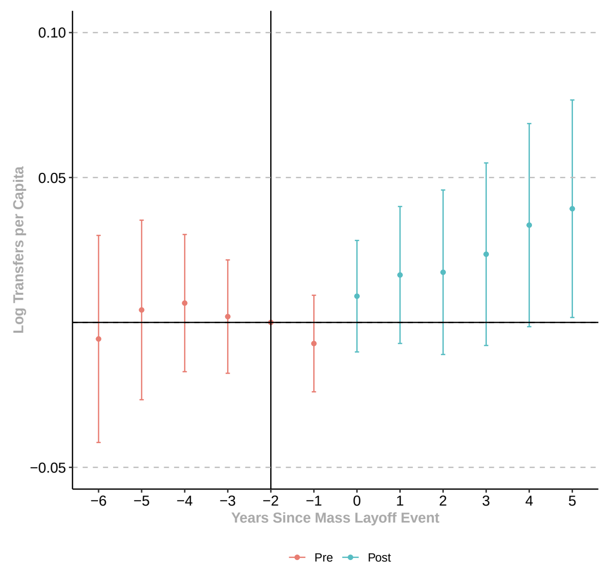
(a) UI Transfers per Capita



(b) Social Security Transfers per Capita

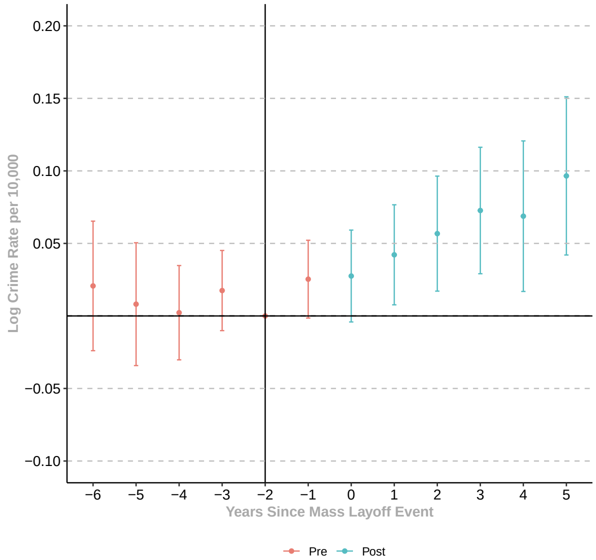


(c) Income Assistance Transfers per Capita

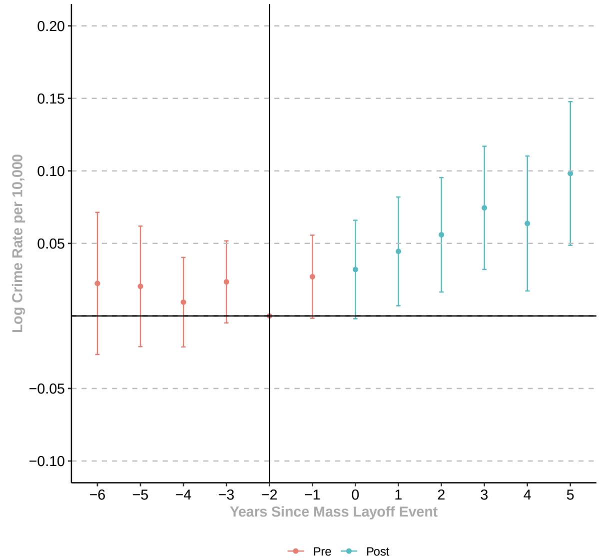


(d) Medicaid Transfers per Capita

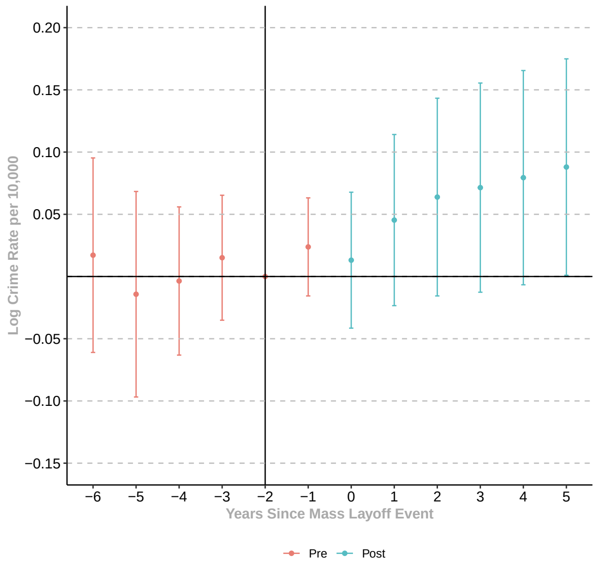
Figure 6: This figures displays the event study treatment effects of Callaway Sant'Anna (2021) (i.e, the average treatment effect for counties that have been treated for e = t-g time periods- see equation(2)). The point estimates measure the effect in relation to year g-2, two years before the mass layoff event. The bars represent a 95% uniform confidence band. The control group includes never-treated counties and counties not treated by year g + 5. Data Sources: BEA Regional Accounts and SEER



(a) Log Total Crime Rate

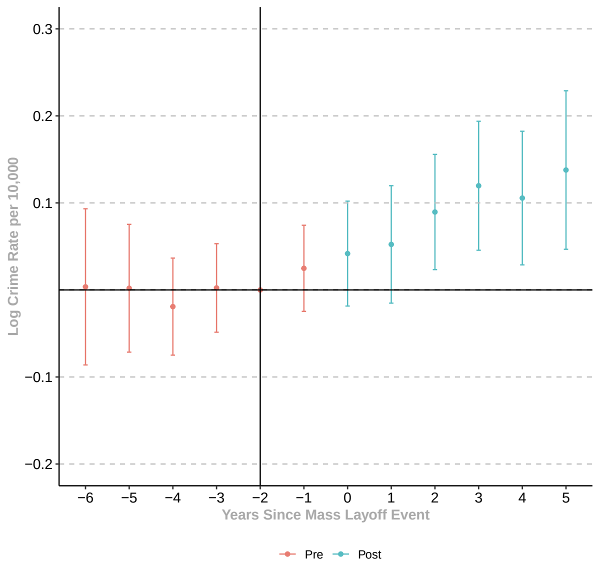


(b) Log Property Crime Rate

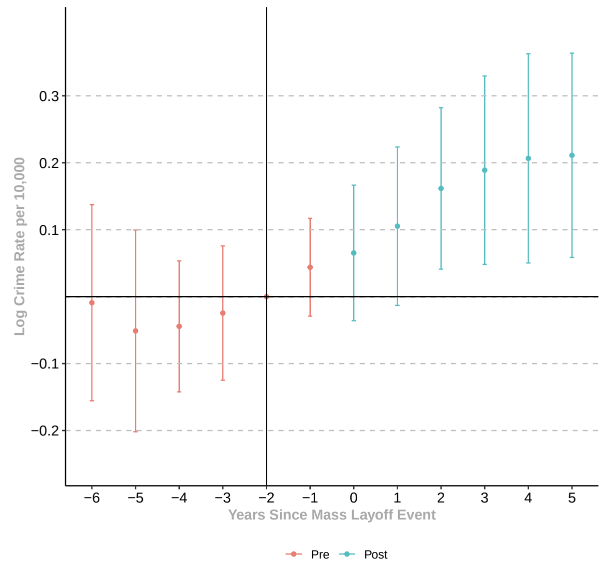


(c) Log Violant Crime Rate

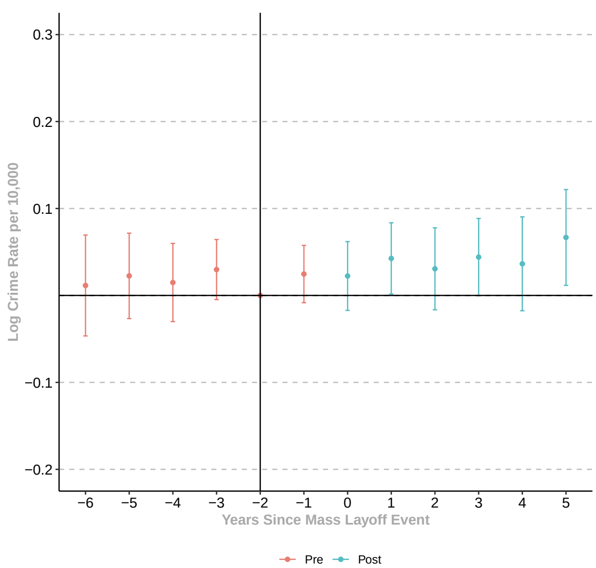
Figure 7: This figures displays the event study treatment effects of Callaway Sant'Anna (2021) (i.e, the average treatment effect for counties that have been treated for e = t-g time periods- see equation(2)). The point estimates measure the effect in relation to year g-2, two years before the mass layoff event. The bars represent a 95% uniform confidence band. The control group includes never-treated counties and counties not treated by year g + 5. Data Sources: UCR Offense Known and Clearances by Arrest and SEER



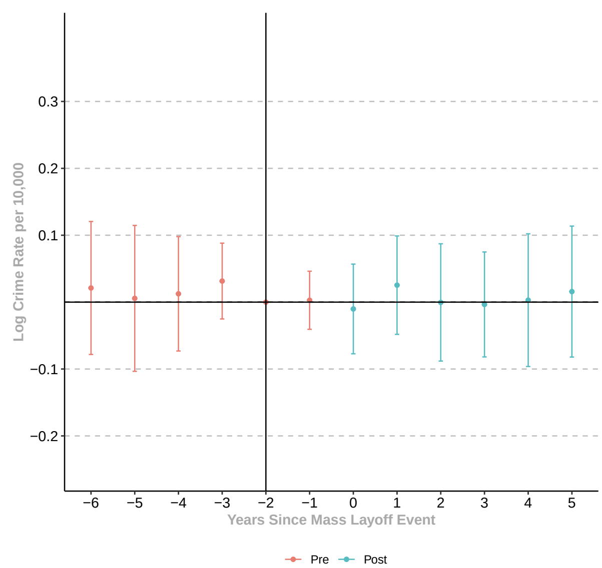
(a) Property Crime Low Wage



(b) Violent Crime High Wage

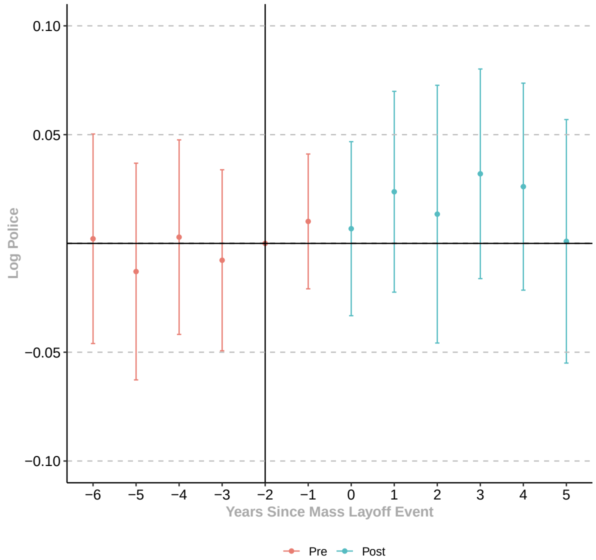


(c) Property Crime High Wage

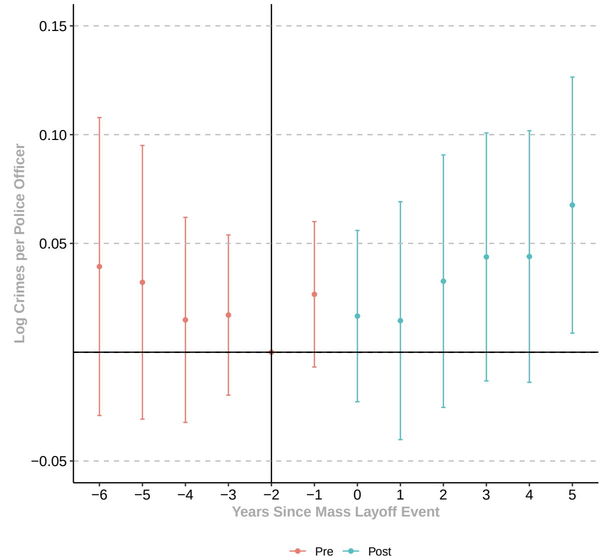


(d) Violent Crime High Wage

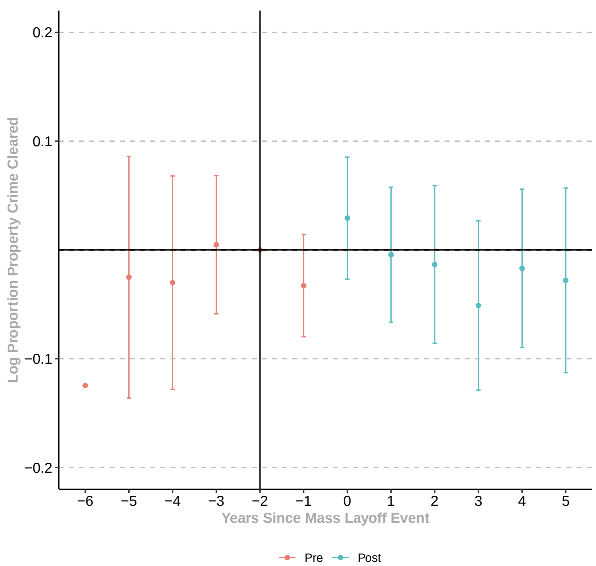
Figure 8: This figures displays the event study treatment effects of Callaway Sant'Anna (2021) (i.e, the average treatment effect for counties that have been treated for e = t-g time periods- see equation(2)). The point estimates measure the effect in relation to year g-2, two years before the mass layoff event. The bars represent a 95% uniform confidence band. The control group includes never-treated counties and counties not treated by year g + 5. The two above median and below median wage gap groups consist of all control counties and treated counties with a baseline wage gap greater than or less than -$5174 in 1990 (see section 6.1 for more details). Data Sources: UCR Offense Known and Clearances by Arrest, QCEW Annual Employment and Wage, and SEER



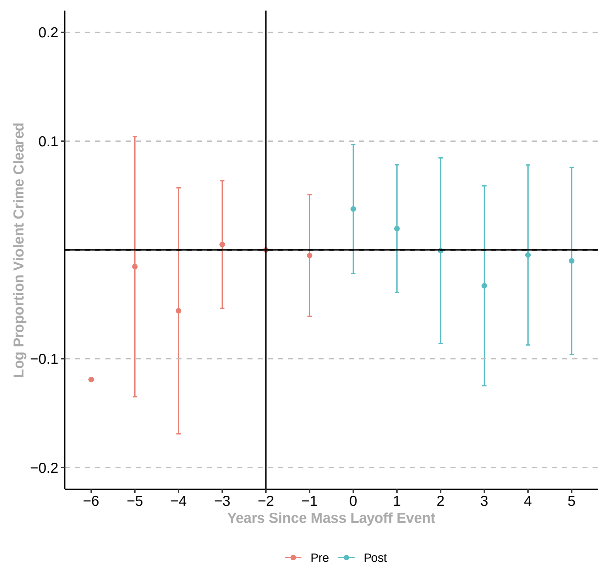
(a) Log Police Officer Employees



(b) Log Crimes per Officer



(c) Log Property Crime Clearance Rate



(d) Log Violent Crime Clearance Rate

Figure 9: This figures displays the event study treatment effects of Callaway Sant'Anna (2021) (i.e, the average treatment effect for counties that have been treated for e = t-g time periods- see equation(2)). The point estimates measure the effect in relation to year g-2, two years before the mass layoff event. The bars represent a 95% uniform confidence band. The control group includes never-treated counties and counties not treated by year g + 5. Data Sources: UCR: Offense Known and Clearances by Arrest and UCR: Law Enforcement Officers Killed and Assaulted

9 Tables

Table 1: Summary Statistics by Treatment Status

	Treated	Not Treated	Difference
Population	36.31	134.26	-97.95^{***}
	(46.22)	(384.58)	(15.89)
Percent White	84.42	87.45	-3.03^{***}
	(16.75)	(12.31)	(0.69)
Percent Black	12.25	7.81	4.44^{***}
	(17.07)	(10.89)	(0.66)
Percent Hispanic	3.41	5.46	-2.05^{***}
	(9.97)	(10.44)	(0.51)
Percent Under 25	36.30	36.05	0.25
	(3.71)	(4.87)	(0.23)
Median Household Income	29557.28	37948.25	-8390.97^{***}
	(6787.86)	(9562.49)	(437.87)
Percent No College	68.51	56.42	12.09^{***}
	(8.91)	(10.37)	(0.50)
Percent Rural	66.19	37.91	28.28^{***}
	(23.22)	(26.23)	(1.26)
Employment Rate	0.93	0.94	-0.01^{***}
	(0.03)	(0.03)	(0.00)
Labor Force Participation	0.74	0.77	-0.03^{***}
	(0.07)	(0.08)	(0.00)
Percent Manufacturing	10.84	6.94	3.90^{***}
	(7.38)	(4.01)	(0.27)
Police per Capita	17.08	16.80	0.29
	(10.91)	(9.94)	(0.51)
Total Crime Rate	376.10	446.62	-70.53^{***}
	(270.05)	(297.07)	(14.41)
Property Crime Rate	290.26	349.07	-58.81^{***}
	(201.70)	(227.29)	(10.95)
Violent Crime Rate	83.72	95.08	-11.36^{**}
	(85.68)	(83.61)	(4.21)
Median Layoff Size (Employees)	244.0	–	–
Median Layoff Size (Percent)	1.76%	–	–
Number of Counties	590	1254	

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$
Data on race, population, education, income, percent of workers in manufacturing, and rurality, is from the 1990 Census. Data on employment and labor force is from the 1990 LAUS. Labor force participation is defined as the labor force divided by the population age 16-64. Police and crime data are from 1990 UCR data.

Table 2: Effect of Mass Layoff on Labor Force Components

	Employment	Employment Rate	Labor Force Participation	Population
ATT	-2.34^{***}	-0.42^{***}	-1.07^{***}	-0.63
	(0.51)	(0.08)	(0.25)	(0.45)
t-2 Mean (level)	11556.46	93.43%	74.23%	26160.35
Num. obs.	39690	39690	39690	39690

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$
Coefficient estimates are given in log points × 100. ATT refers to the event-study aggregated ATT. Data on employment and labor force size comes from the BLS LAUS. Data on population comes from the SEER. Labor force participation is constructed as the labor force divided by population age 16-64. Regressions are weighted by 1990 census population.

Table 3: Effect of Mass Layoff on Log Income, Wage, and Transfers

	Income (per capita)	Total Wages (per capita)	Average Wage	Total Transfers (per capita)
ATT	-1.03^{***}	-3.32^{***}	-0.71^{**}	1.14^{***}
	(0.30)	(0.57)	(0.31)	(0.34)
t-2 Mean (level $1000s)	25.61	8.60	26.64	5.09
Num. obs.	39690	39690	39690	39690

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$
Coefficient estimates are given in log points ∗ 100. ATT refers to the event-study aggregated ATT. Data on income and transfers comes from the BEA Regional Accounts. Data on wages comes from the QCEW.

Table 4: Effect of Mass Layoff on Log Transfers by Program

	Total Transfers (per capita)	Unemployment Benefits (per capita)	SS Disability Retirement (per capita)	Income Assistance (per capita)	Medicaid (per capita)	Medicare (per capita)	Education Training (per capita)
ATT	1.14^{***}	1.80	1.34^{***}	0.60^{*}	2.34^{**}	-0.52	0.58
	(0.33)	(3.51)	(0.30)	(0.31)	(0.99)	(0.67)	(1.53)
Num. obs.	39690	39690	39690	39690	39690	39690	39690

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$
Coefficient estimates are given in log points ∗ 100. ATT refers to the event-study aggregated ATT. Transfer data comes from the BEA Regional Accounts. SS Disability Retirement refers to Social Secuirty benefits received by retired and disabled workers, dependents, and survivors. Income Assistance refers to income maintenance benefits: Supplemental Security Income (SSI) benefits, Earned Income Tax Credit (EITC), Additional Child Tax Credit, Supplemental Nutrition Assistance Program (SNAP) benefits, family assistance, and other income maintenance benefits, including general assistance. Education and training assistance consists of federal fellowships, federal higher education student assistance, state educational assistance. Regressions are weighted by 1990 census population.

Table 5: Effect of Mass Layoff on Log Employment and Log Average Wage by Super Sector

	Employment		Average Wage		
	ATT	(se)	ATT	(se)	N
Total	-3.24^{***}	(0.55)	-0.71^{**}	(0.33)	39690
Government	-0.65	(0.82)	0.45	(0.40)	38283
Good-Producing					
Manufacturing	-11.07^{***}	(1.31)	0.48	(0.58)	38178
Mining & Energy	1.00	(2.72)	1.23	(2.01)	39165
Construction	-1.53	(1.65)	-1.27^{**}	(0.50)	39060
Service-Providing					
Retail & Transport	-1.12	(0.79)	-0.35	(0.49)	39690
Finance & Real Estate	-2.48^{**}	(1.24)	-1.10^{*}	(0.61)	39375
Professional Services	-2.55	(1.66)	-0.18	(0.89)	39396
Education & Health	0.51	(0.91)	-1.16^{***}	(0.44)	39249
Hospitality & Food	-2.17^{**}	(0.94)	-0.09	(0.46)	39564

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$
Coefficient estimates are given in log points $*$ 100. ATT refers to the event-study aggregated ATT. Professional Services refers to professional, scientific, and technical services, management of companies and enterprises, administrative and support and waste management remediation services. Education and Health refers to educational, healthcare, and social assistance. Hospitality and Food refers to accommodation and food services. Regressions are weighted by 1990 census population.

Table 6: Effect of Mass Layoff on Log Crime Rates

	ATT	(se)	t - 2 mean (offenses per 10000)	N
Total Crime	6.02^{***}	(1.42)	298.27	39669
Property				
Total	6.10^{***}	(1.35)	235.80	39669
Burglary	5.72^{***}	(1.71)	55.60	39669
Theft	5.97^{***}	(1.47)	133.75	39648
Motor Theft	3.80	(2.40)	11.91	39627
Violent				
Total	6.04^{**}	(2.47)	70.46	39648
Homicide	1.31	(5.02)	1.10	37632
Rape	-1.57	(3.37)	4.08	39165
Assault	5.55^{**}	(2.58)	64.65	39648
Robbery	4.22	(2.93)	2.57	38451

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$
Coefficient estimates are given in log points $*$ 100. ATT refers to the event-study aggregated ATT. Crime data comes from the UCR Offenses Known and Clearances by Arrest. Crimes rates are per 10000 individuals covered by the agencies. Regressions are weighted by 1990 police agency population covered within county.

Table 7: Heterogeneity of Mass Layoff Effect by Low vs. High Wage Sectors

	Below Median Wage Gap		Above Median Wage Gap		Difference	
	ATT	(se)	ATT	(se)	Diff	(se)
Crime						
Total	10.87***	(2.32)	3.30**	(1.43)	7.57***	(0.73)
Property	9.11***	(2.26)	4.38***	(1.49)	4.73***	(0.72)
Violent	15.64***	(4.53)	0.25	(2.51)	15.40***	(1.38)

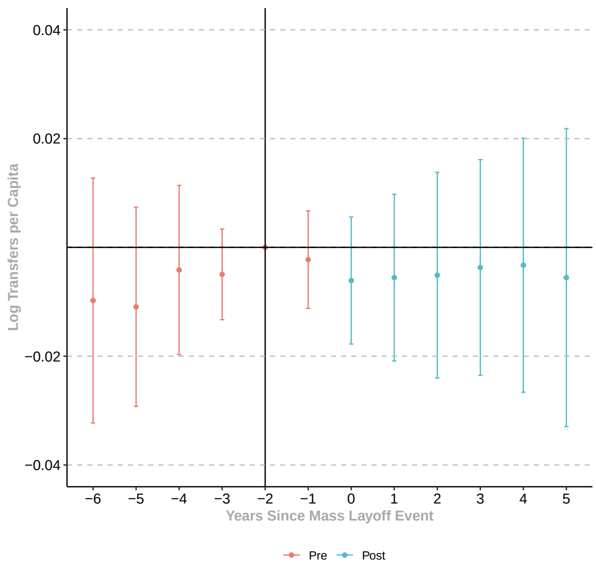
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$
Coefficient estimates are given in log points ∗ 100. ATT refers to the event-study aggregated ATT. Crime data comes from the UCR Offenses Known and Clearances by Arrest. The two groups consist of all control counties and treated counties with a baseline wage gap greater than or less than -$5174. The baseline wage gap is the difference between the average 2-digit SIC wage of firms in the mass layoff and the average county wage in 1990. Regressions are weighted by 1990 police agency population covered within county.

Table 8: Effect of Mass Layoff on Police Capacity

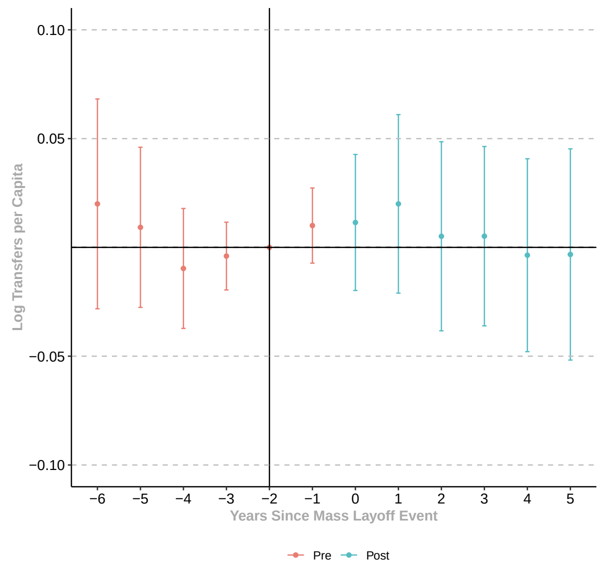
	ATT	(se)	N
Log Police Staffing			
All Employees	0.22	(1.51)	39144
Officers	1.75	(1.57)	39543
Non-Officers	−1.03	(2.92)	39144
Log Crimes per Officer	3.52**	(1.71)	39522
Log Clearance Rates			
Property	−1.38	(2.12)	39627
Violent	0.24	(2.44)	39543

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$
Coefficient estimates are given in log points ∗ 100. ATT refers to the event-study aggregated ATT. Data on police employee counts come from the UCR Law Enforcement Officers Killed and Assaulted. Data on reported crimes and clearance rates come from the UCR Offense Known and Clearances by Arrest. Crimes per Officer is constructed as the total reported crimes divided by the total number of police officers from agencies in the county. Regressions are weighted by 1990 police agency population covered within county.

A Appendix

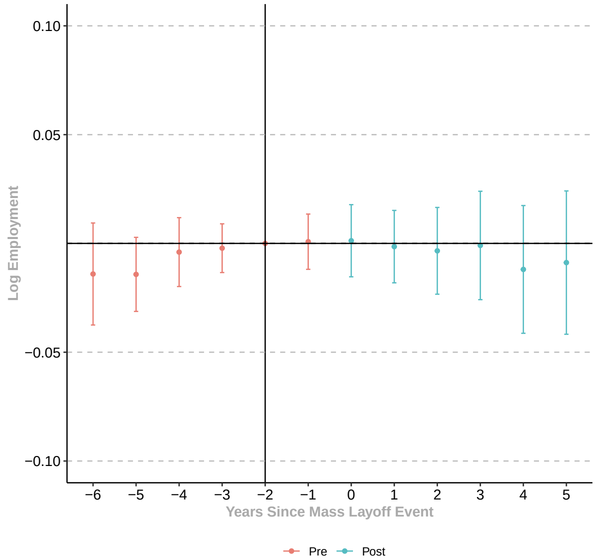


(a) Log Medicare Transfers

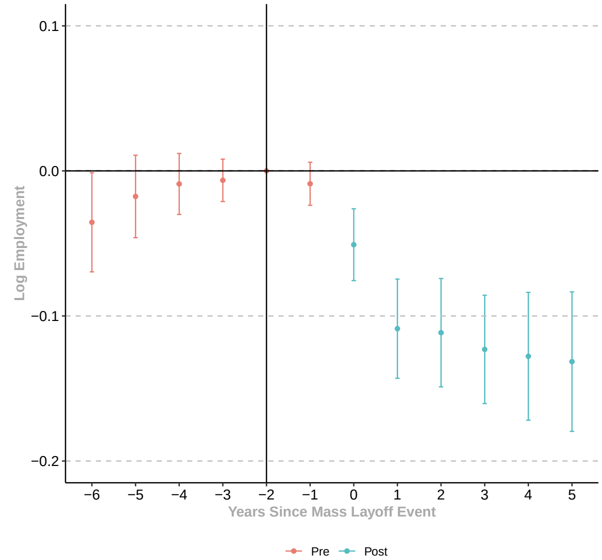


(b) Log Education and Training Transfers

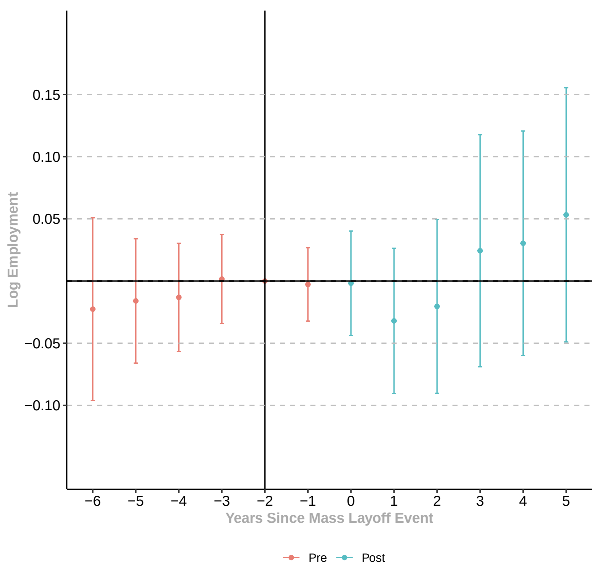
Figure A.1: This figures displays the event study treatment effects of Callaway Sant'Anna (2021) (i.e, the average treatment effect for counties that have been treated for e = t-g time periods- see equation(2)). The point estimates measure the effect in relation to year g-2, two years before the mass layoff event. The bars represent a 95% uniform confidence band. The control group includes never-treated counties and counties not treated by year g + 5. Data Sources: BEA Regional Accounts and SEER



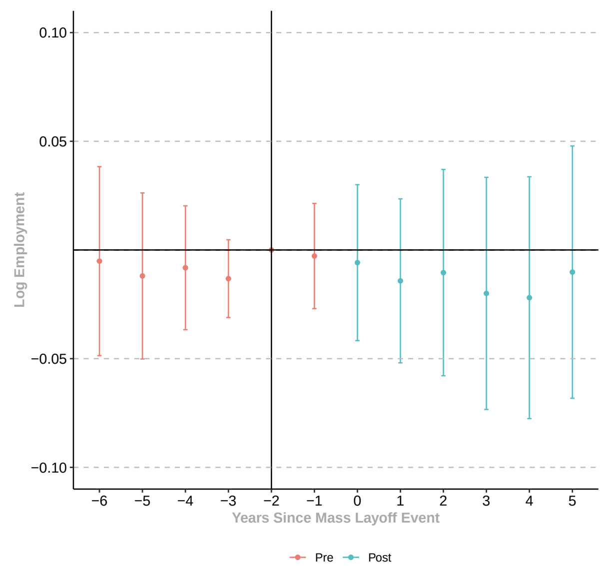
(a) Log Employment Local Government



(b) Log Employment Manufacturing

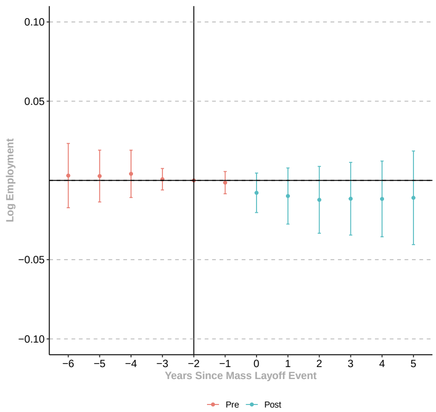


(c) Log Employment Mining and Energy

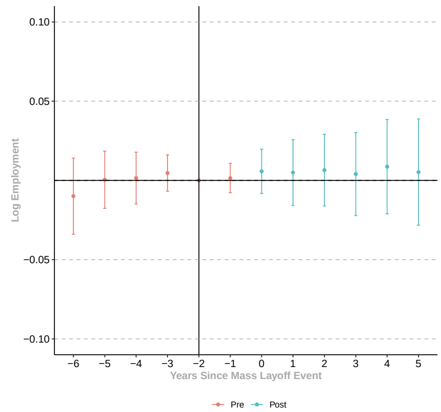


(d) Log Employment Construction

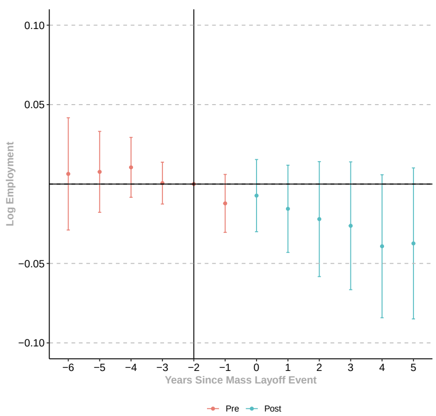
Figure A.2: This figures displays the event study treatment effects of Callaway Sant'Anna (2021)(i.e, the average treatment effect for counties that have been treated for e = t-g time periods- see equation(2)). The point estimates measure the effect in relation to year g-2, two years before the mass layoff event. The bars represent a 95% uniform confidence band. The control group includes never-treated counties and counties not treated by year g + 5. Data Sources: QCEW Annual Employment and Wage



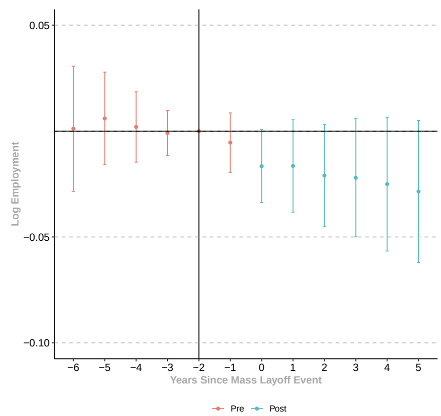
(a) Log Employment Retail and Transportation



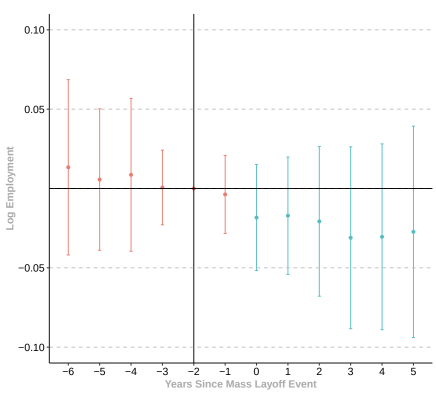
(d) Log Employment Education and Health



(b) Log Employment Finance and Real Estate

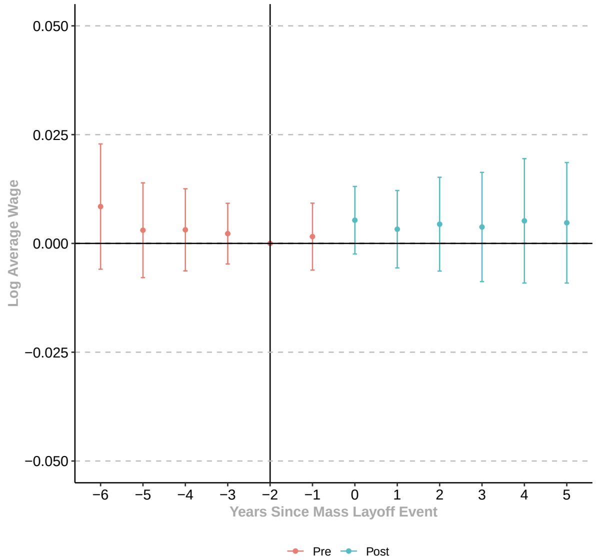


(e) Log Employment Hospitality and Food Services

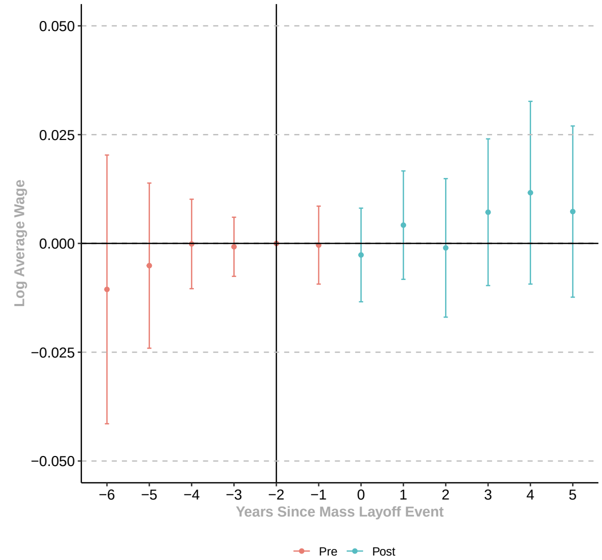


(c) Log Employment Professional Services

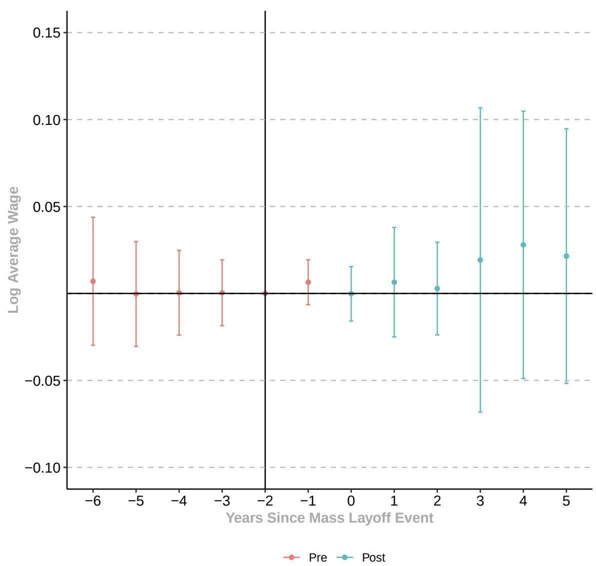
Figure A.3: This figures displays the event study treatment effects of Callaway Sant'Anna (2021) (i.e, the average treatment effect for counties that have been treated for e = t-g time periods- see equation(2)). The point estimates measure the effect in relation to year g-2, two years before the mass layoff event. The bars represent a 95% uniform confidence band. The control group includes never-treated counties and counties not treated by year g + 5. Data Sources:QCEW Annual Employment and Wage



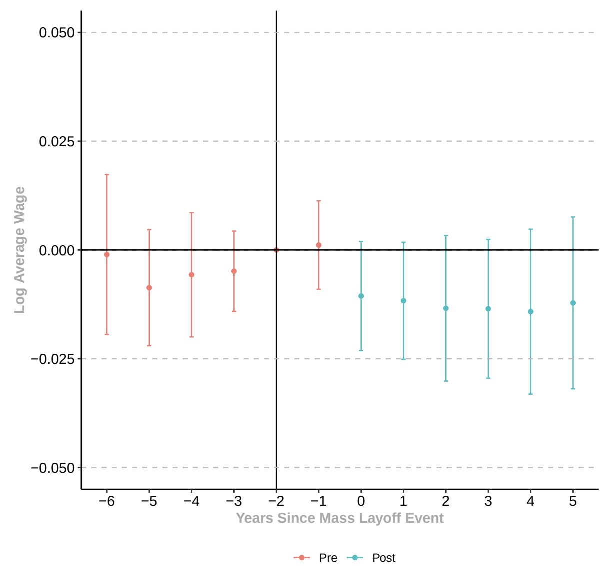
(a) Log Average Wage Local Government



(b) Log Average Wage Manufacturing

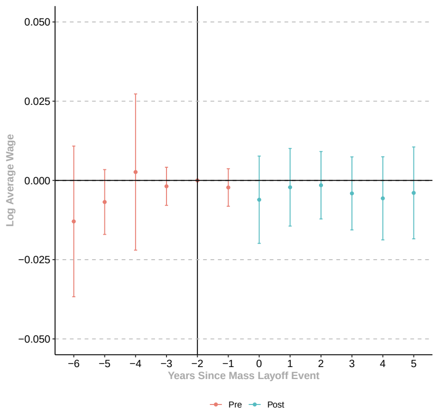


(c) Log Average Wage Mining and Energy

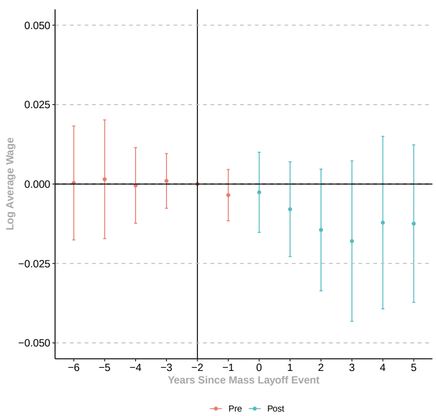


(d) Log Average Wage Construction

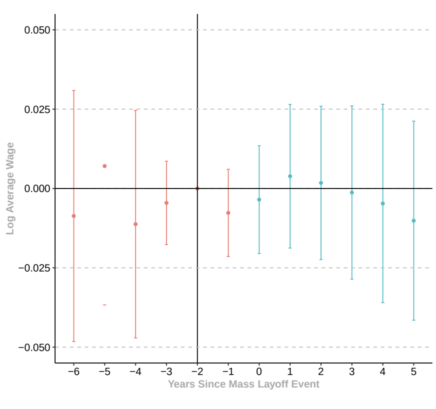
Figure A.4: This figures displays the event study treatment effects of Callaway Sant'Anna (2021) (i.e, the average treatment effect for counties that have been treated for e = t-g time periods- see equation(2)). The point estimates measure the effect in relation to year g-2, two years before the mass layoff event. The bars represent a 95% uniform confidence band. The control group includes never-treated counties and counties not treated by year g + 5. Data Sources: QCEW Annual Employment and Wage



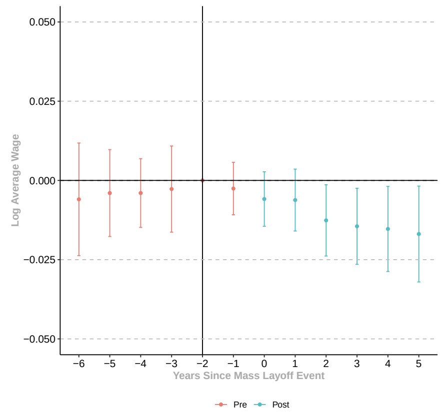
(a) Log Average Wage Retail and Transportation



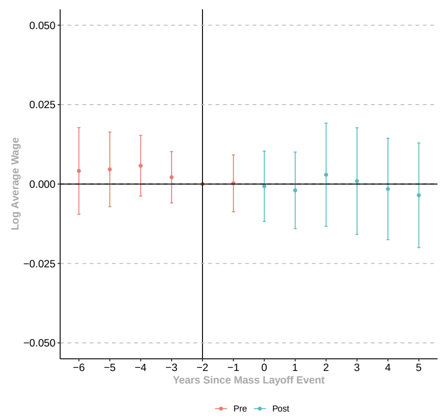
(b) Log Average Wage Finance and Real Estate



(c) Log Average Wage Professional Services

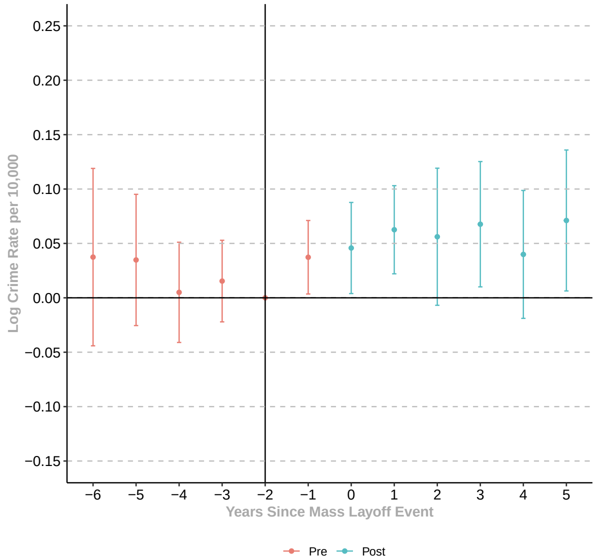


(d) Log Average Wage Education and Health

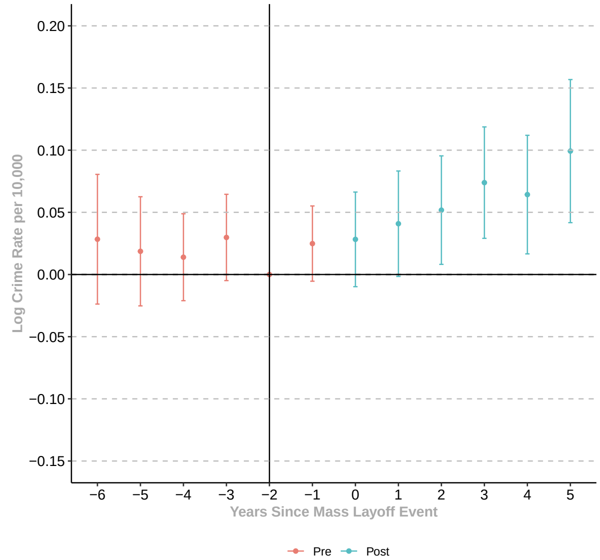


(e) Log Average Wage Hospitality and Food Services

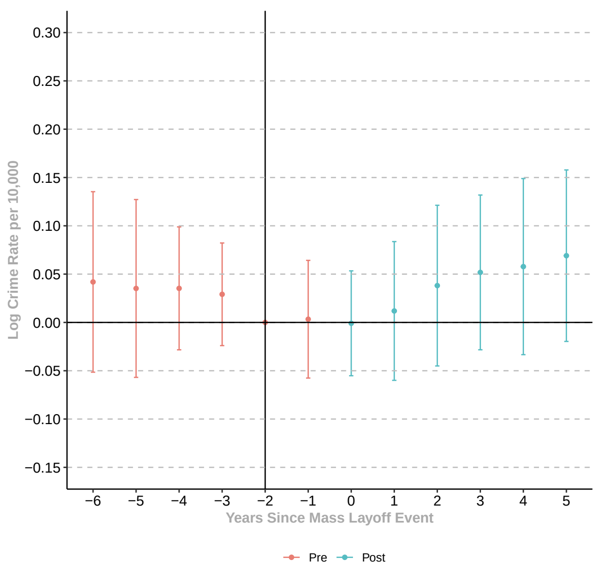
Figure A.5: This figures displays the event study treatment effects of Callaway Sant'Anna (2021) (i.e, the average treatment effect for counties that have been treated for e = t-g time periods- see equation(2)). The point estimates measure the effect in relation to year g-2, two years before the mass layoff event. The bars represent a 95% uniform confidence band. The control group includes never-treated counties and counties not treated by year g + 5. Data Sources:QCEW Annual Employment and Wage



(a) Log Burglary Rate

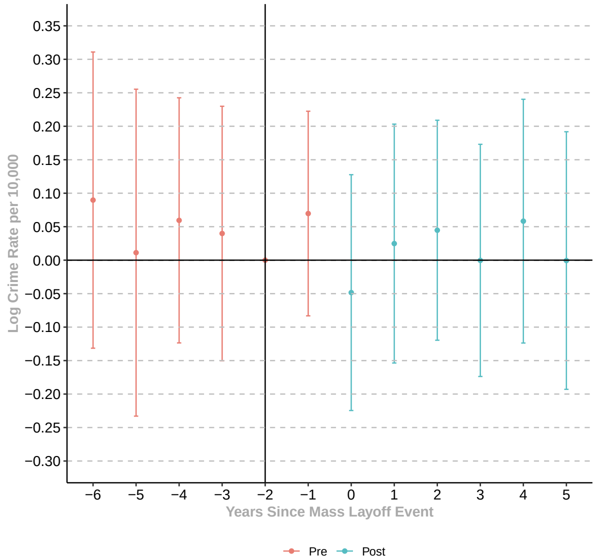


(b) Log Theft Rate

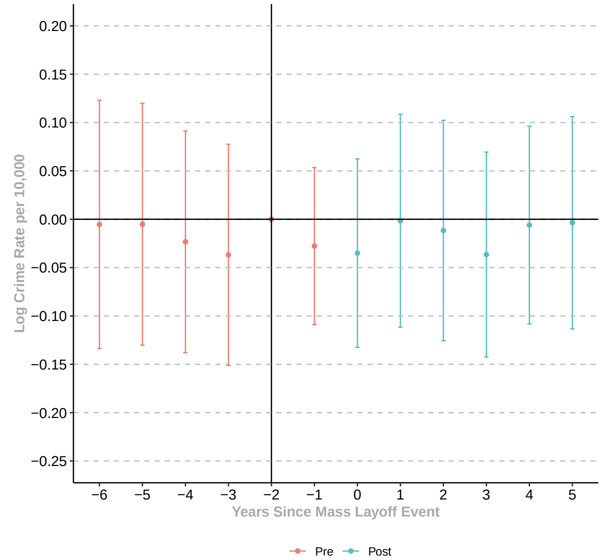


(c) Log Motor Vehicle Theft Rate

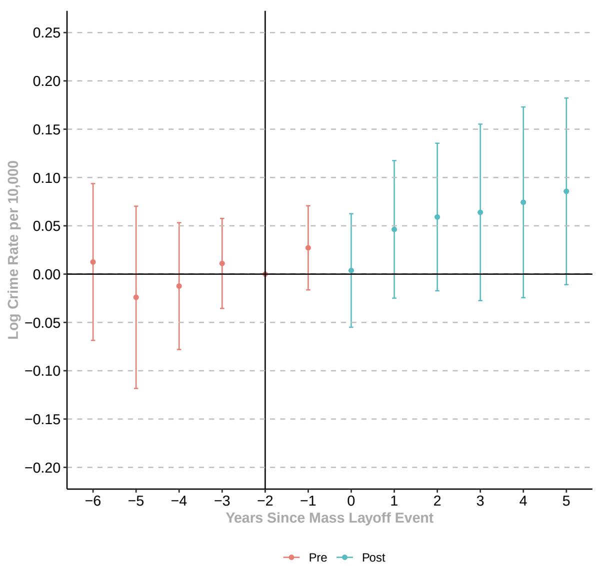
Figure A.6: This figures displays the event study treatment effects of Callaway Sant'Anna (2021) (i.e, the average treatment effect for counties that have been treated for e = t-g time periods- see equation(2)). The point estimates measure the effect in relation to year g-2, two years before the mass layoff event. The bars represent a 95% uniform confidence band. The control group includes never-treated counties and counties not treated by year g + 5. Data Sources: UCR: Offense Known and Clearances by Arrest and SEER



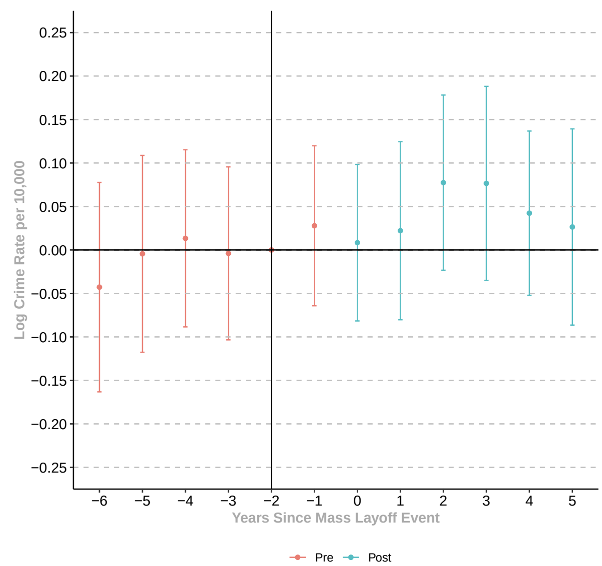
(a) Log Murder Rate



(b) Log Rape Rate



(c) Log Assault Rate



(d) Log Robbery Rate

Figure A.7: This figures displays the event study treatment effects of Callaway Sant'Anna (2021) (i.e, the average treatment effect for counties that have been treated for e = t-g time periods- see equation(2)). The point estimates measure the effect in relation to year g-2, two years before the mass layoff event. The bars represent a 95% uniform confidence band. The control group includes never-treated counties and counties not treated by year g + 5. Data Sources: UCR: Offense Known and Clearances by Arrest and SEER.

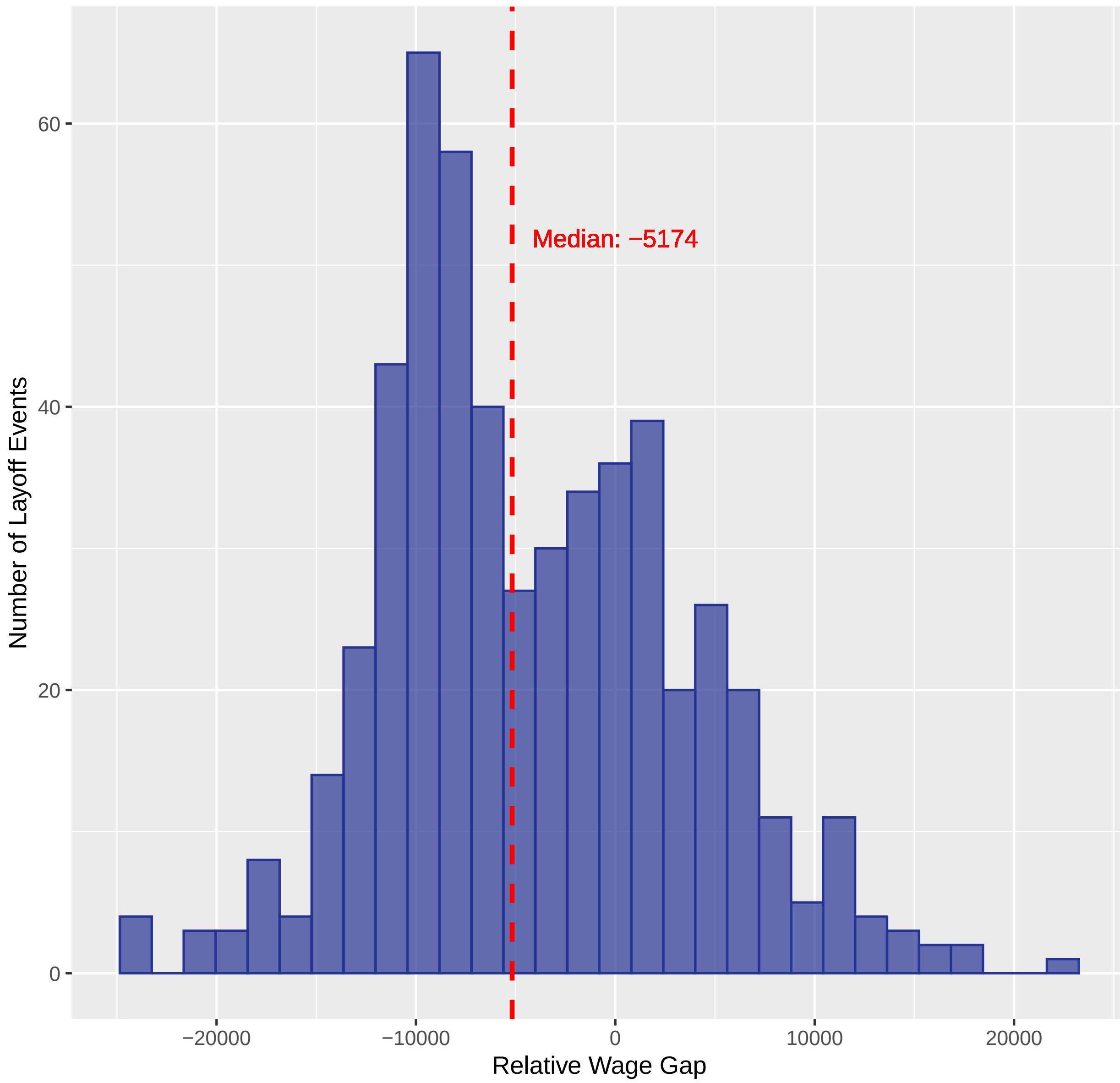


Figure A.8: This figure displays the distribution of the relative wage gap for treated counties. The relative wage gap is defined as the difference between the weighted average 2-digit SIC wage of firms involved in the mass layoff event and the county average wage in 1990. Treated counties are group into above or below the median of $-5174.

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