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Essays on Local Labor Markets and Unemployment Insurance

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Essays on Local Labor Markets and Unemployment Insurance

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DISSERTATION

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To my father and mother.

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Abstract

In this dissertation, I explore the impact of both local and national shocks on California's labor market and unemployment insurance (UI) program. Initially, I analyze the consequences of mass layoffs, which serve as a local employment shock, on neighboring firms. Later, I shift my focus to the effect of the COVID-19 shock on California workers, specifically those who receive UI benefits, and how UI assisted these displaced workers.

In the first chapter, I examine the spillover effects of mass layoffs on neighboring establishments, shedding light on the dynamics of agglomeration economies. I employ a difference-in-differences event study framework and leverage comprehensive administrative data encompassing all entities in California to study the indirect effects of mass layoffs on employment, earnings, and the number of nearby establishments. I exploit the geographic coordinates of establishments to define treatment and control areas based on their proximity to instances of mass layoffs. The findings reveal persistent and negative spillover effects on local employment levels, payroll, and the number of operating establishments four years after the events. However, there is no significant change in the average earnings of workers. Moreover, empirical evidence demonstrates that the spillover effects diminish with increasing spatial distance, effectively disappearing after 6km. In summary, a one percent employment shock results in a one percent indirect decrease in employment levels within a 6km radius four years later.

Furthermore, I contribute to the literature on agglomeration economies by assessing the mechanism of agglomeration economies in the observed spillover effects. I use economic distance measures to show the importance of industry linkages, knowledge spillover, and thick labor market as forces behind the spillover effects. My findings show that industries closely tied to the industry of mass layoff establishments experienced a more substantial employment decline, while those economically distant from the events show minimal changes.

In the second chapter, which is joint work with Alex Bell, T.J. Hedin, Peter Mannino, Geoffrey Schnorr, and Till von Wachter, we answer this question: To what extent did jobless Americans benefit from unemployment insurance (UI) during the COVID-19 pandemic? We document geographic disparities in access to UI during 2020. We leverage aggregated and individual-level claims data to perform an integrated analysis across four measures of access to UI. In addition to the traditional UI recipiency rate, we construct rates of application among the unemployed, rates of first payment among applicants, and exhaustion rates among paid claimants. Through correlations across California counties and across states, we show that areas with more disadvantaged residents had less access to UI during the pandemic. Although these disparities are large in magnitude, cross-state analysis suggests that policy can play a salient role in mitigating them.

In the third chapter, coauthored with Alex Bell, T.J. Hedin, Peter Mannino, Carl Romer, Geoffrey Schnorr, and Till von Wachter, we leverage California's administrative longitudinal UI data to introduce two cumulative measures of the labor market health and use them to assess the impact of the COVID-19 pandemic on California's labor market and UI system. First, on the extensive margin, we measure the share of the pre-crisis labor force that applied for UI benefits. Second, on the intensive margin, we calculate the share of UI claimants who have received more than 26 weeks of unemployment benefits in the first year of the crisis. By combining the two measures, we show that the average member of the labor force spent nearly two months receiving regular UI benefits during the first year of the COVID-19 pandemic. Finally, we look into the demographic disparities in receiving UI benefits and show that more vulnerable workers experienced more weeks on UI than the more privileged.

Chapter 1

Spillover Effects of Mass Layoffs on Neighboring Firms

1.1 Introduction

There has been considerable research on the effects of opening large plants on the local economy, primarily started by Greenstone et al. (2010). Alternatively, a few papers have recently emerged to study the effects of plant closures on local economies (Gathmann et al. 2018; Jofre-Monseny et al. 2018; vom Berge and Schmillen 2022; Celli et al. 2023). Plant closures and mass layoffs have been a concern for policymakers, especially in the recent decades after the China shock (Autor et al. 2016; Autor et al. 2021). For instance, President Trump made retaining manufacturing jobs in the US one of the center points of his 2016 electoral campaign. One of his promises was to prevent the closure of the Carrier furnace plant in Indianapolis and the job loss of its 14,000 workers. Four years later, only 800 workers continued to work at the plant. Moreover, more than 20 manufacturing plants in the US had been closed by 2020.[1] Efforts to prevent the closure of large manufacturing plants extend beyond one administration. During the Great Recession, the Obama Administration

[1] Tony Cook, "Trump campaigned on saving jobs at Indianapolis' Carrier plant. This is what it's like now.", IndyStar, October 2020.

allocated a substantial bailout of 80.5 billion dollars to the auto industry. Policymakers express two primary concerns regarding large mass layoffs and plant closures. First, there is direct job loss, which typically includes higher-paying positions. The second concern is the potential domino effect of mass layoffs on other local businesses interconnected with the large plant in various capacities. Throughout American history, there have been notable examples of company towns experiencing devastating consequences when their primary plant closed, affecting the entire community (Crawford, 1995). The negative impact of mass layoffs on directly displaced workers has been extensively studied (Jacobson et al. (1993); Couch 2001; von Wachter et al. 2009; Schmieder et al. 2009; Couch and Placzek 2010; Lachowska et al. 2018; Schmieder et al. 2023). However, the indirect effect on the close-by establishments[2] has been understudied and the evidence is contradictory.

The closure or significant downsizing of a large plant may have local negative effects on other establishments because of agglomeration economies, which refers to the advantages gained when firms and individuals co-locate in urban areas and industrial clusters (Glaeser 2010). Economic activity in most regions is spatially concentrated. In the US by 1992, only 1.9 percent of the land was built up or paved (Burchfield et al., 2006). The automotive industry in the Midwest, finance in New York, and high tech in the Bay area are the most notable examples of agglomeration economies in the US. Agglomeration economies benefit employers and employees through thick labor markets (labor market pooling), knowledge spillovers, and input-output linkages (Marshall 1920; Ellison and Glaeser 1997; Ellison et al. 2010; Combes and Gobillon 2015). When a large establishment experiences closure or mass layoff, on the one hand, local labor markets and industry linkages can be interrupted and decrease other establishments' productivity and employment. Another potential channel of negative spillover is the local multiplier effect, in which the creation or destruction of jobs may create or destroy other jobs in the non-tradable sector through changes in local demand (Moretti, 2010). On the other hand, mass layoffs suddenly increase local labor

[2]Establishment is a business location which can be a part of a firm with multiple establishments (multi-establishment firm), or the single unit of a firm (single establishment firm).

supply and, therefore, may put downward pressure on wages, resulting in more hiring and positive spillover effects. In this paper, I study the spillover effects of large mass layoffs on neighboring establishments in California and shed light on the mechanisms that cause them.

Studying the spillover effects of mass layoffs requires an extensive administrative dataset encompassing establishments in an economy.[3] In this research, I leverage the establishments' longitude and latitude information in California's Quarterly Census of Employment and Earnings (QCEW) from 2000 to 2019. This comprehensive administrative data contains establishments covered by California unemployment insurance, containing over 95% of the state's employees. To define mass layoffs, I adopt a modified version of the definition used by Gathmann et al. (2018). Accordingly, a mass layoff is characterized by an employment reduction of a minimum of 500 employees. Additionally, I employ a 30 percent decline in year-to-year employment, drawing from the literature on mass layoffs and displaced workers.[4] By this approach, I ensure substantial job losses within the local economy and the event establishment.

I employ a difference-in-differences event study approach to assess the causal impact of mass layoffs on neighboring establishments.[5] Given the critical role of the distance between the event and affected establishments[6], I move beyond conventional geographic boundaries (e.g., counties, municipalities) and consider the precise distance between the event establishment and its neighboring counterparts. Consequently, the treatment area is defined as a circular region with the mass layoff establishment at its center. The primary shock in the treatment area is a sharp decline in operations and employment of a large establishment (with at least 500 employees). Suppose changes in labor market thickness, breakage of

[3]While alternative datasets like Dun and Bradstreet also provide location information at establishment-level data, there are concerns about self-reporting and imputation issues. Another concern about such datasets is successors and predecessors of establishments, which are especially important for defining mass layoff events, which are the centerpiece of this study.

[4]In the literature of the effects of mass layoffs on displaced workers, 30% drop in employment level is the gold standard in defining mass layoff events (Jacobson et al. 1993; Couch 2001; von Wachter et al. 2009; Schmieder et al. 2009; Couch and Placzek 2010; Lachowska et al. 2018; Schmieder et al. 2023)

[5]In Section (4), I address and discuss the emerging literature in difference-in-differences.

[6]I do not examine outcomes of neighboring residents of mass layoff events. However, in Appendix 1.C, I examine changes in the prices of single-family homes.

input-output linkages, and a decrease in local demand impact the nearby establishments. In that case, the control area should have a similar establishment at its center to be a viable counterfactual to simulate these economic connections. Thus, the control group is constructed as a circle encompassing a similar-sized and industry-aligned large establishment to the event establishment.

In this paper, I show that a large mass layoff negatively impacts the employment levels of neighboring establishments over the four years following the event. First, I establish that the magnitude of this effect diminishes as establishments are located farther away from the event, eventually reaching zero at a distance of greater than 6 kilometers. Based on these findings, I define treatment and control groups using a radius of 6 kilometers, representing the range within which the spillover effects are most pronounced. Upon analyzing employment dynamics, I find that, excluding the event establishment, the employment level in the treatment area is 6 percentage points lower compared to the counterfactual after four years. Additionally, my results indicate that within the treatment area, there is a reduction of 3 percentage points in the number of establishments compared to the control group. Total paid earnings (payroll) declined by 9 percentage points; however, I do not find statistically significant changes in average earnings per employee.

Furthermore, I employ economic distance indexes to search for the underlying three agglomeration channels driving the spillover effects. Specifically, I utilize industries' input-output index for input-output linkages, occupation correlation between industry pairs for knowledge spillover, and rate of workers' movement between industry pairs (i.e., employment flow) for labor market pooling. Then, I categorize establishments based on their economic proximity to the event establishments. The findings of this analysis reveal intriguing patterns. In all three measures, the industries economically closest to the event establishments experience a notable employment decline in their employment. In contrast, the employment of industries economically furthest away experienced close to zero changes in all measures. These results suggest that interruption in agglomeration economies is an important channel behind the decline in employment. Moreover, I examine the channel local multiplier effect by studying the disparities in spillover effects by tradability of events and affected establishments. My findings show that if the event establishment is in the tradable sector, the non-tradable employment declines by 4.4%. In contrast, a mass layoff in the non-tradable sector has no statistically significant impact on the tradable sector's employment.

The topic of the indirect effect of mass layoffs remains relatively understudied but is gradually expanding. Four papers currently address this topic, each yielding contradictory findings. Two of these papers observe positive spillover effects on local employment, contradicting my findings. Jofre-Monseny et al. (2018) analyze the spillover effect of 45 closures of manufacturing plants in Spain and uncover positive job creation for each lost job within the local economy. Vom Berge and Schmillen (2022) examine German data and reveal a 5% increase in local employment (excluding the event) five years after the mass layoff event. Similar to my work, they include all industries but use a smaller 50-employee threshold to define mass layoff.

Conversely, another paper on the German economy presents evidence of a negative indirect effect on employment. Gathmann et al. (2018) studied West Germany and found a negative employment effect of 2% on the local economy. They use a 500 threshold for the mass layoff definition; however, the area examined for the spillover effect is larger than my setting. The most recent study from Celli et al. (2023) also provides evidence for negative spillover effects, but only in the same industry. Celli et al. (2023) shows that manufacturing mass layoffs result in a 30 percent decrease in the employment level of the same industry in local labor markets but no significant effects in other sectors. Contradictory results suggest that the size of shock and labor market conditions matter in the direction of the effects. Therefore, studying other major economies and more deeply analyzing the potential channels of positive or negative spillovers is necessary.

With this paper, I contribute to the existing literature as the first study investigating the spillover effects of mass layoffs in the United States economy. I employ a novel approach in defining the control group, enhancing the robustness of my analysis, and providing valuable insights into the specific context of the US labor market. Furthermore, for the first time, I move beyond aggregate-level analysis and study establishment-level outcomes to better understand employment effects at intensive margins. Establishment level analysis also enables the analysis of the heterogeneity among different types of establishments, which has not been previously studied. This paper is among just a few papers that use administrative QCEW data, and specifically its information on geographic coordinates, which can be followed by more research at the intersection of labor and spatial economics. Finally, it is the first paper that quantitatively examines each agglomeration channel.

In the bigger picture, my paper contributes to the existing body of research that aims to quantify and comprehend the spillover effects of (mostly positive) local economic shocks through agglomeration forces (Ellison and Glaeser 1997; Rosenthal and Strange 2004; Greenstone et al. 2010; Kline and Moretti 2014). In their canonical work, Greenstone et al. (2010) demonstrate that opening large manufacturing plants leads to a significant and positive increase in productivity within their host counties. Employing a treatment and control group framework, they assess the change in total factor productivity by treating counties that successfully attract large manufacturing plants as the treatment group and comparing them with the control group consisting of counties that were not selected. In a related study, Kline and Moretti (2014) examines the long-term effects of the Tennessee Valley Authority program on local economies. Their findings indicate positive impacts on productivity, employment, and aggregate earnings, suggesting that the local economy benefited from the program. Furthermore, Feyrer et al. (2017) investigate the employment and wage effects at the county level of the new oil and gas production facilities resulting from fracking technology. Their research also reveals positive effects on both employment and earnings.

This paper also contributes to another literature that studies local economic shocks through the lens of local demand changes. Moretti (2010) calls this phenomenon the local multiplier effect and shows that creating tradable jobs in an American city causes more job creation in the non-tradable sector. Moretti and Thulin (2013) show similar multiplier effects for Swedish cities and van Dijk (2017) for US cities. Faggio and Overman (2014) show that in England, the multiplier effect of public job creation on total private employment is zero, but it is similar to other studies on the non-tradable sector. I contribute to this literature by studying the heterogeneity in the tradability of the event and affected establishments.

The remainder of the paper is organized as follows. Section 2 provides a conceptual framework that explains how mass layoffs can impact local labor markets. Section 3 defines mass layoffs and describes the data structure employed in the analysis. Section 4 explains the identification, employing the difference-in-differences approach and main results. Section 5 presents empirical evidence for the spillover channels. Finally, Section 6 serves as the paper's conclusion, summarizing the key findings and offering insights for future research.

1.2 Conceptual and Theoretical Framework

As in many other countries, economic activity exhibits significant concentration in the United States. Marshall (1920) argues that firms and workers derive numerous benefits from agglomeration, primarily through a thick labor market, input-output linkages, and knowledge spillover.

A thick labor market offers advantages to both employers and employees. A larger pool of potential candidates for firms increases the likelihood of finding high-quality matches for job openings and decreases searching time (Andersson et al. 2007; Andini et al. 2013; Abel and Deitz 2015). Conversely, job seekers benefit from the higher chances of finding suitable positions when multiple firms actively hire within the same area. Furthermore, the concentration of firms improves input-output linkages, facilitating access to diverse sellers for necessary inputs (Faggio et al. 2017). This broader range of suppliers results in a higher probability of obtaining higher-quality inputs at lower prices. On the output side, whether intermediate or final goods, the diversity of buyers enhances market opportunities for establishments. Additionally, the geographic proximity of upstream and downstream firms leads to cost and time savings in transportation, further boosting efficiency within the agglomerated region. Knowledge spillover represents another source of agglomeration benefits. As the concentration of workers increases in a local economy, interactions and the flow of employees between firms become more frequent. Increased collaboration and knowledge-sharing among workers lead to increased human capital within the workforce, ultimately driving higher productivity levels (Black and Lynch 1996; Combes and Duranton 2006; Serafinelli 2019).

What are the possible scenarios in which plant closures or mass layoffs can affect neighboring establishments through agglomeration economies? When such events occur, they can disrupt input-output linkages within the local economic network. A large firm could be a part of the production chain in the area, with other related firms positioned either upstream or downstream relative to the event firm. Upstream firms supply goods and services to the event firm and a reduction in the size of the firm results in decreased demand for the final products of these upstream firms. Conversely, some downstream firms rely on the goods and services provided by the event firm. The absence of the event firm's products necessitates sourcing from geographically distant firms, increasing production costs for the downstream firms. Such interruptions in input-output linkages can profoundly affect neighboring establishments' profit and employment levels. The demand reduction and increased production costs can lead to decreased profitability and potential job losses in the affected establishments.

Large mass layoffs also affect the total factor of productivity. First, large mass layoffs decrease the size of the area's labor market, resulting in lower quality employer-employee matches and lower productivity. Second, a reduction in the number of workers reduces the flow of workers between firms and different sorts of interactions and affects knowledge spillover among workers. This is important for industries with higher levels of technology and innovation (Moretti 2021; Saxenian 1996).

Agglomeration forces are not the only reasons that mass layoff events can have a spillover effect on other establishments. The second channel is local multipliers. After a mass layoff, local establishments lose some demand for their products. The employees who used to buy local goods and services during workdays are no longer in the area, which translates to a reduction in local demand and can cause more layoffs. The magnitude of the local multiplier varies between tradable and non-tradable sectors. In the non-tradable sector, the demand comes from the local market, meaning the goods and services the laid-off workers purchased have at least partly vanished. The demand effect on the tradable sector is more limited since the affected establishments can find customers outside the local market.

1.2.1 A Simple Agglomeration Model

I use a simple model developed by Gathmann et al. (2018) from Glaeser and Gottlieb (2009) to formalize the spillover effect of mass layoffs on local employment earnings. I assume that all establishments have a Cobb-Douglas production function in which there are two types of capital, fixed ($\overline{K_j}$) and fully flexible (K_j) with the share of μ:

$$Y_j = f_j A_r L_j^{\alpha} \overline{K}_j^{(1-\alpha)(1-\mu)} K_j^{(1-\alpha)\mu}, \tag{1.1}$$

where f_j is the productivity shifter of firm j, $A_r = L_r^{\lambda}$ is the productivity shifter of the local area, with λ representing local productivity links (input-output linkages, knowledge spillover, local labor pool, etc.). By taking the first order condition of capital and labor, I can derive the aggregate demand curve:[7]

$$\log L_r = \log \sum_j L_j = \log \sum_j f_j^{\frac{1}{(1-\alpha)(1-\mu)}} + \frac{\log A_r}{(1-\alpha)(1-\mu)} - \frac{1-(1-\alpha)\mu}{(1-\alpha)(1-\mu)} \log w_r + \kappa. \tag{1.2}$$

Following Gathmann et al. (2018), I can study the overall effect on aggregate local labor

[7] $\kappa = -(\mu/(1-\mu)) \log i + \log \overline{K} + (1-(1-\alpha)\mu)/((1-\alpha)(1-\mu)) \log \alpha + (\mu/(1-\mu)) \log[(1-\alpha)\mu]$

demand by total differentiation of (2):

$$d\log L_r = \underbrace{\frac{df_{\text{event}}}{J(1-\alpha)(1-\mu)}}_{\text{direct effect }(-)} + \underbrace{\frac{\lambda}{(1-\alpha)(1-\mu)} d\log L_r}_{\text{agglomeration spillover }(-)} \underbrace{- \frac{1-(1-\alpha)\mu}{(1-\alpha)(1-\mu)} d\log w_r}_{\text{endog. wage adjustment }(+)} . \tag{1.3}$$

Excluding the unambiguously negative direct effect on the event establishment, there are two opposite forces that affect the aggregate local employment:

1. Agglomeration spillover on nearby establishments (< 0): The magnitude of agglomeration effect depends on the economic closeness (λ) of the industry of the event establishment and other firms. Industries that are economically closer to the event establishment would be affected the most.

2. Local wage adjustment due to the increase in available labor from the mass layoff establishment (> 0): The magnitude of the wage adjustment depends on the relative size of the mass layoff to the size of the workers' commuting zone, and how mobile are workers in response to unemployment to move to more prosperous areas. If the size of employment reduction is small relative to the commuting zone, and/or workers are highly mobile, decrease in earnings would be small and the positive effect of the wage adjustment can go to zero.

Thus, the direction of the spillover effect is ambiguous and depends on the local labor market and industry composition. I focus on the spillover effect on this paper, and do not quantify each section of equation (1.3); however, I use it to explain my findings and contrast them with the results from the existing literature.

1.3 Data

The primary dataset that I use is the administrative Quarterly Census of Employment and Wages (QCEW) of California which includes comprehensive information on establishments that are covered by the California Unemployment Insurance (UI) system and federal entities covered by the Unemployment Compensation for Federal Employees (UCFE) program. QCEW reports monthly employment and quarterly total paid earnings of each establishment. Two crucial pieces of information in QCEW make it possible to study the spillover effect of mass layoffs. The first is geographic coordinates (longitude and latitude) of establishments. By having the coordinates information on establishments, I can find the exact location of the mass layoff establishments and their distance to other establishments. About 90% of establishments have proper geocoded information, and the other 10% are dropped from the sample.[8] The second piece of information which is essential for any mass layoff study is observing successors (and predecessors) of establishments. With observing successors and predecessors in QCEW, I do not treat establishments that only experience a change in their identification without employment loss, as a mass layoff event.[9] QCEW also contains 6-digit NAICS industry codes, essential to analyze agglomeration forces and heterogeneity in Section 1.5.

The second dataset I utilize is the Quarterly Earnings (QE) of California, which are employer-employee matched data showing the quarterly earnings of workers covered by UI from each employer. QE is at the firm level, unlike the QCEW, which is an establishment-level dataset. Therefore, I cannot directly match all workers to their workplace other than single establishment firms. I employed data from both sources from 2000 to 2019 to ensure the analysis remains unaffected by the pandemic era shocks. Finally, I transformed both data sources into annual longitudinal datasets to prevent treating seasonal layoffs as mass

[8]The distribution of employment, earnings, and industry of excluded establishments are not different from the rest of the sample.

[9]Change in establishment identification number can be due to various reasons such as a change in ownership, merges, divergence, or simply accounting reasons.

layoff events.[10]

1.3.1 Mass Layoff Definition

To identify an employment reduction incident as a mass layoff, two key restrictions must be met: (1) There must be a 30 percent reduction in the annual employment level at the event establishment. This benchmark is borrowed from existing literature on the effect of mass layoff on displaced workers to ensure that it reflects a sizable reduction in economic activity (Jacobson et al. 1993; Couch 2001; von Wachter et al. 2009; Schmieder et al. 2009; Couch and Placzek 2010; Lachowska et al. 2018). (2) The mass layoff must involve a minimum of 500 employees within a year, as defined by Gathmann et al. (2018). It is important to note that in the QCEW data, we do not distinguish between full-time and part-time workers; the employment count encompasses all worker types.

Furthermore, establishments in the agricultural sector are excluded from mass layoff establishments. However, they are still considered part of the sample for assessing the impact on the local economy. The sample of mass layoff establishments is confined to the period from 2004 to 2015. This duration allows for a four-year observation window, enabling the analysis of pre- and post-event trends. Once an event establishment experiences a mass layoff, it should not recover its employment levels to the pre-event period. In situations where an establishment experiences multiple mass layoff incidents, we only consider the first.

1.3.2 Statistics of the Mass Layoff Establishments

Following the definition in 3.1, 132 mass layoffs occurred between 2003 and 2015. Among all events, 53 eventually get closed by 2019, which on average takes 3.5 years from the event year. Similar to economic activities, mass layoffs are also concentrated in a few areas, mainly in the greater Los Angeles area, Bay Area, and, to a lesser extent, San Diego and Sacramento

[10]Seasonal layoffs do not occur as productivity shock but due to the nature of the industry. Thus, not including them in an analysis based on productivity shocks is preferred.

counties. These establishments are larger than a typical one in California, and also, as we can see in Figure 1.1, pay higher wages to their employees. Higher wages suggest that these establishments have a higher share of skilled workers, which is essential for regional productivity.

One assumption in 2.1 was that the mass layoff shock represents a decline in firm-specific productivity. However, there is a concern that mass layoffs are due to local-industry shocks. In Appendix 1.A I show that local economic conditions and state-level industry shocks do not predict large mass layoff incidences.

Previous studies examining both positive and negative employment shocks on the local economy have primarily focused on the impact on the tradable sector (e.g., Greenstone et al. 2010; Moretti 2010; Jofre-Monseny et al. 2018; Gathmann et al. 2018). However, my study delves into the effects of mass layoffs in all sectors on the local economy. Table 1.1 displays the wide range of industries where these mass layoffs have occurred.

To use mass layoff events as a productivity shock to nearby establishments, it is vital to ensure persistent employment decline in event establishments. Figure 1.2 panel (a) presents the average employment level eight years before and after the event. Notably, the employment level shows an increasing trend before the event, but at the time of the event, there is a sudden and mechanical decline in employment levels. Even eight years after the event, the employment level (excluding closures) has not fully recovered to the pre-event period, indicating that, on average, the mass layoffs in the sample have resulted in permanent job losses. Figure 1.2 panel (b) illustrates the log of the average earnings per employee. Before the event, the average earnings per employee remained relatively stable. However, in the post-event level, we observe an increase in earnings, suggesting that, on average, a higher share of lower-skilled workers were affected and laid off during the mass layoffs.

1.4 Identification and Results

In this paper, I employ a difference-in-differences approach, which requires a control area ideally identical to the treated one before the mass layoff event. The critical element in each treated area is the event establishment and its economic linkages with nearby establishments before the mass layoff. To construct a counterfactual, I undertake a simple matching process. These counterfactuals must meet four criteria: (1) be in the same industry as the event (at least 2-digits NAICS code), (2) exhibit persistent employment trend over nine years of observation with no mass layoff, (3) be in a different commuting zone (CZ), and at least 30 km away from the event, and (4) have at least 300 employees at the time of the event.[11]

While traditionally, studies on the spatial spillover effect of local shocks have treated space as a discrete concept (Jofre-Monseny, Sánchez-Vidal, and Viladecans-Marsal 2018; Gathmann, Helm, and Schönberg 2018), I take a different approach by treating space as continuous, leveraging the geocoded data. This choice is crucial because the impact of mass layoffs depends on the spatial distance between the event establishment and the affected ones rather than being limited by administrative boundaries. To achieve this, I consider the treated area a circle with the event establishment at its center. Similarly, the control region is a circle around the counterfactual establishment.

In Section 4.2, I discuss how the radius for treated and control regions (R^T and R^C) are chosen, but first, I explain the overall structure of the difference-in-differences design. I have a staggered difference-in-differences[12], in which there are 132 pairs of treatment and control[13] regions at the industry level, with events occurring at different times.

In recent years, there has been a growing body of literature expressing concerns regarding staggered difference-in-differences designs (de Chaisemartin and D'Haultfœuille 2020; Sun

[11] 16 establishments are counterfactual for more than one event. There are 97 unique counterfactuals for 132 mass layoff events.

[12] staggered design refers to settings in which observations in the treated sample are assigned treatment at different points in time

[13] Some control regions are duplicated, but they are not necessarily at the same event time.

and Abraham 2021; Callaway and Sant'Anna 2021; Gardner 2021).[14,15] In summary, the main issues are heterogeneity in the effects by time of the event (or policy adoption) and group, as well as contamination of coefficients by effects from other periods. For example, de Chaisemartin and D'Haultfœuille (2020) demonstrate that regression coefficients may appear negative even when all the average treatment effects (ATEs) are positive, and Sun and Abraham (2021) argues that coefficients can be influenced by the effects of other time periods. To address these potential issues, I have implemented several precautionary measures. Firstly, in cases where treatment circles overlap, I have excluded establishments that received treatment more than once. Secondly, I have removed all establishments within regions with overlap between treatment and control areas. Consequently, the regression sample exclusively comprises establishments treated only once in the treatment group and establishments that have never been treated in the control group. Lastly, I have restricted comparisons to treated and control industries within the same cohort, ensuring that problematic comparisons[16] are avoided. Therefore, for each mass layoff case, an industry in the treatment region is compared with the same industry in treatment regions. While these steps mitigate some methodological concerns, I also demonstrate in Section 4.3 that my baseline regression results align qualitatively with the methods suggested in recent papers.

1.4.1 Spatial Decay of Spillover Effect

The first two questions to answer are: On average, is there a spillover effect post-event, and the relationship between distance and potential spillover effects? To answer these two questions, I employ a methodology involving creating five concentric "donut" treatment areas around each event establishment, ranging from 0 to 10 kilometers, and utilize a circular region surrounding the counterfactual establishment as the control group (Figure 1.3). This

[14] For a comprehensive overview of the current developments in this literature and practical recommendations, please refer to Roth et al. (2023).

[15] In Section 1.5.3, I examine the difference-in-differences results using suggested methods provided by Sun and Abraham (2021), Gardner (2021), and Callaway and Sant'Anna (2021) and compare them with the baseline results from my main identification.

[16] Problematic comparisons are cases such as comparing treated with not yet treated or already treated.

framework allows me to estimate the following difference-in-differences regression for each treatment "donut" :

$$Y_{irt} = \beta_1 Treatment_r + \beta_2 Post_t + \beta_3 Treatment_r * Post_t + \mu_i + \delta_r + \gamma_t + \epsilon_{irt}, \qquad (1.4)$$

where Y_{irt} is the log employment of industry i in region r at year t. $Treatment_t$ indicates being in the treatment region (2 km donuts in this case), and $Post_t$ indicates being after the event year. β_3 is the coefficient of interest representing the potential spillover effects.

I use three different control radii of 5, 6, and 7km and estimate equation (1.4) to ensure the results are not sensitive to the control radius. Figure 1.4 displays the average spillover effect of mass layoffs by distance for three control radii. A negative spillover effect is observable irrespective of the control area radius, with its magnitude diminishing as the distance from the event increases. Beyond the 6 km threshold, the spillover effect approaches zero and becomes statistically insignificant. This suggests that, on average, the spillover effect is present within a 6 km radius of the mass layoff event.

Comparison of Treatment and Control Regions Following the results of equation (1.4), from now on, all the analyses in this paper use a 6 km radius as the radius around events (and counterfactual) for treatment (and control) regions. Tables 1.2 and 1.3 compare treatment and control regions. On average, establishments in treatment regions are slightly larger and older but pay lower earnings. The industry structure of treatment and control areas is almost identical. The only difference comes from information and other services sectors where treatment areas have lower and higher shares, respectively, compared to control regions. These summary statistics ensure that treatment and control areas are comparable.

1.4.2 Event Study

I employ a difference-in-difference event study approach to estimate the spillover effect of the mass layoff by using the following reduced-form regression:

$$Y_{ir\tau t} = \sum_{\tau=-4}^{-2} \alpha_\tau Event_{r\tau t} + \sum_{\tau=0}^{4} \beta_\tau Event_{r\tau t} + \mu_i + \gamma_t + \delta_r + \lambda_\tau + \epsilon_{ir\tau t}, \tag{1.5}$$

where Y_{irt} is the labor market outcome of interest, and τ represents the time relative to the year of mass layoffs ($\tau = 0$). $Event_{r\tau t}$ is a binary variable that is 1 for the treatment region at time τ and 0 otherwise. This regression controls for industry, region, year, and relative time fixed effect. The year fixed-effect control general shocks such as business cycles, together with relative time fixed effects, guarantee that changes in the outcome of interest in the treatment group are compared with the control group at the same calendar and relative year. Time-invariant differences among regions and industries are controlled by region fixed effect (δ_r) and industry fixed effect (μ_i). The standard errors are clustered at the regional level.

In difference-in-differences models, the parallel trends is a key assumption, and parameters α_{-4} to α_{-2} show if the parallel trends assumption holds in this empirical setting. The parameters of interest are β_0 to β_4 that indicate the percentage change of the dependent variable for each relative year after the event.

Baseline Results

Employment and Earnings.[17] Figure 1.5 presents the baseline results of regression (1.5) for the key labor market outcomes: employment, total paid earnings, and earnings per employee.[18,19] The parallel trends assumption holds for all outcomes, as the point estimates

[17] In Appendix 1.D, I examine the impact of mass layoffs on housing prices.

[18] Earnings per employee is calculated at the establishment level by dividing total paid earnings by total employment.

[19] In Appendix 1.B, I study the effects of mass layoffs on directly displaced workers. The results are consistent with previous literature showing persistent drop in employment and wages of displaced workers.

are close to zero and statistically insignificant. In panel (a), we observe that employment in treated regions begins to decline in the year of the event, with a more pronounced drop one year after the event. Subsequently, employment continues to decline at a lower rate in the post-event years, showing 6 percentage points decline four years after the event. As expected, the same pattern is observed for total earnings in panel (b), as it is a function of the number of employees. The total earnings follow a similar trend, declining in the year of the event and continuing to decrease at a reduced rate in the post-event years.

In panel (c), the average earnings per employee results shed light on the theoretical ambiguity discussed in section 2.1 regarding the direction of spillover effects in which a decrease in earnings could have a positive spillover effect on employment. Interestingly, the point estimates of earnings per employee in the post-event years are consistently lower in treated regions compared to control regions. However, they are less than 1 percentage point and statistically insignificant. This finding suggests that the local wage effect of mass layoffs is negligible in the sample, indicating the absence of a positive spillover channel.

Finally, in Table 1.4, I summarize the event study results along with various alternative controls. In columns (4), (8), and (12), I exclude Industry fixed effect, but the overall post-event trends are consistent with the main model in columns (1), (5), and (9). Furthermore, the results are robust to including year-industry and region-industry interactions in the model.

Employment Decline at The Extensive Margins. Is the decline in the employment of treated areas relative to control due to net layoff in existing establishments, increase in closures, decrease in openings of businesses, or a combination of all? I investigate the changes in the net employment change of existing establishments in 4.2.2. However, I can analyze changes in the number of establishments to understand the role of business formation and closures in the decline of employment in local areas. Figure 8 displays the results of regression (5) with the number of establishments as the dependent variable. Prior to the event, there is an upward trend indicating openings exceeded closures more rapidly in the treatment areas compared to the control. However, after the event, the trend inverses, and four years later, the number of establishments is 3.1 percentage points lower. While changes in the number of establishments do not estimate the exact extensive margins of employment change, it is a proxy providing evidence for that.

Establishment Level Results

Up to this point, I have conducted an aggregate-level analysis of the spillover effects within a 6km radius around the event. However, a crucial dimension of the mass layoff shock that remains unexplored in the existing literature pertains to the spillover effects at the establishment level. To exclude factors related to establishment openings and closures and concentrate solely on the shifts occurring within existing establishments, I limit the sample to include only those establishments present in the data one year before the event and up to four years afterward. Moreover, I modify regression (5) into the following:

$$Y_{eir\tau t} = \sum_{\tau=-4}^{-2} \alpha_\tau Event_{r\tau t} + \sum_{\tau=0}^{4} \beta_\tau Event_{r\tau t} + \mu_i + \gamma_t + \delta_r + \lambda_\tau + \omega_e + \epsilon_{eir\tau t}, \tag{1.6}$$

where ω_e is the establishment fixed effect.

Figure 1.8 and Table 1.5 present the findings of the equation (1.6). In Panel (a), Columns (1) and (2), the results indicate that for firms that survived up to four years after the event, employment begins to decrease in the year of the event and continues to decline by 2.4 percentage points three years after that. Although there is a subsequent employment increase in the fourth year, it remains 2.1 percentage points below the control group. Notably, the extent of employment reduction is less than half of the aggregate results observed. Moving to Panel (b) and Columns (3) and (4), the pattern for total paid earnings is less persistent compared to employment. Total paid earnings experience a decline until the second year, but they begin to rebound by the third year and become statistically insignificant by the fourth year. The different post-event patterns of total paid earnings compared to employment would make sense by examining earnings per employee in Panel (c), Columns (5) and (6). A modest upward trend is noticeable, albeit statistically insignificant, two years post-event. These outcomes suggest that these establishments tended to lay off lower-skilled employees and likely hired more higher-skilled workers following the shock.

The establishment-level analysis allows for a more nuanced examination of heterogeneity based on establishment characteristics. Firstly, I investigate disparities in spillover effects by the industry of the event and affected establishments. Although the aggregate findings indicate persistent employment loss in nearby establishments, different industries may exert varying effects on local areas and respond differently to mass layoff events. Figure 1.9 presents the results of equation (1.6) broken down by the industry of the event establishment.[20] To address suppression requirements, industries with similarities are grouped. In sectors where agglomeration economies play a significant role, such as mining-utilities-construction-manufacturing and professional services, a decline in employment is observed following the event year. Conversely, mass layoffs in the health and education sector do not lead to significant employment declines. Notably, the entertainment and food sector results are intriguing; since these industries heavily rely on local demand, the closure of a large establishment can create opportunities for other establishments in the same sector to expand their local market share. After a mass layoff event in this sector, employment increases by 2.5 percentage points in the first year, with subsequent point estimates remaining positive albeit insignificant.

Moving on to disparities in affected industries, Figure 1.10 demonstrates that regardless of the sector of the affected establishments, employment declines after the event. While mining-utilities-construction-manufacturing, trade-transportation, and food-entertainment

[20]Given that there are only 132 mass layoff events, I combine industries with similarities into groups to comply with suppression requirements of the Employment Development Department. NAICS codes 21-23, and 31-33 are combined into Mining, Utilities, Construction, and Manufacturing; 42, 44, 45, 48, and 49 are trade and transportation; 51-56 are Office and Professional Services; 61, and 62 are health and education; 71, and 72 are entertainment and food; 81, and 92 are public and other services.

sectors experience recovery four years later, establishments in other sectors do not bounce back post-event.

Secondly, I employ different measures to explore how establishment quality can determine resilience towards exposure to mass layoffs. Three notable establishment characteristics serve as proxies for establishment quality: firm size[21], single vs. multi-establishment firms, and firm age. Firm size, often regarded as a proxy for firm quality[22], is depicted in Figure 1.11 panel (a). The data show that surviving establishments associated with small firms (1-9 employees) have experienced the hardest hit. Medium-sized firms (10-100 employees) experienced slightly less impact than small firms, although the difference is not particularly noticeable. In contrast, changes in employment among large firms (more than 100 employees) were insignificant and less than half of the impact observed in small and medium-sized firms. A similar pattern is evident when establishments are categorized as single or multi-establishment firms. Single-establishment firms experience nearly double the employment decline compared to multi-establishment firms, which aligns with the results based on firm size.

The third measure of establishment quality is firm age, calculated one year before the event. Figure 1.11 panel (c) demonstrates that young firms (1-5 years) were the most affected, while older firms (more than 6 years) fared better. Interestingly, firms over 11 experienced greater employment loss than those aged 6 to 10 years. One possible interpretation is that the oldest firms had established strong connections with event establishments and were more dependent on them. In contrast, younger firms, as previous studies have suggested, were more sensitive to the shock.

[21] I specifically choose firm size over establishment size because a small establishment can be associated with a large firm and benefit from its resources, and perform differently from a small single establishment firm.

[22] See Productivity in SMEs and large firms in OECD Countries.

1.5 Channels of Spillover Effects

In Section 2, the discussion revolved around four key channels of spillover effects on neighboring establishments: thick labor markets (or labor market pooling), knowledge sharing, input-output linkages, and local multipliers. In the subsequent section, I comprehensively examine these mechanisms, verifying their presence or absence with empirical evidence. Given these channels' complex and intertwined nature, it is not feasible, at least with the available data, to precisely decompose the magnitudes of each mechanism. Instead, the focus is on leveraging concepts and indexes established within the economic clustering literature to shed light on the importance and existence of these mechanisms.[23, 24]

1.5.1 Economies of Agglomeration Channels

Labor Market Pooling. Mass layoffs inherently lead to a direct reduction in the thickness of the local labor market. This contraction in the labor pool potentially impacts both the pace and quality of job matches within the region. To explore this hypothesis, I employ a data-driven approach by calculating the share of employment flow between industry pairs. The analysis is conducted at the 3-digit NAICS industry level, utilizing a 5 percent sample of employer-employee matched data from 2000 to 2019. First, I construct a sample of workers changing employers between years $t-1$ and t, and then I calculate the proportion of workers in industry i who move to industry j. A higher share of employment flow between industry pairs indicates a higher share of using the same labor pool between the industries. I categorize each combination of a mass layoff event and an affected establishment into three industry and skill proximity tiers based on the distribution of employment flow. These tiers are divided into industry pairs' lower, middle, and upper thirds.

[23] See Delgado et al. (2012), Delgado et al. (2016), Ellison et al. (2010), Glaeser and Kerr (2009), Porter (2003), Duranton and Overman (2005).

[24] Delgado et al. (2016) and Delgado et al. (2012) provide a comprehensive overview of literature on economic closeness and clustering sectors, and I have used their definitions and insights extensively for this section.

Figure 1.12's top panel presents the outcomes of the difference-in-differences estimation (equation (1.4)), computed for three sub-samples ranked by their labor market pooling proximity. The findings indicate that establishments closer to the event regarding sharing the same labor market exhibit a more pronounced drop in employment. While the spillover effects are negative across all three groups, the spillover effect for the industries least related to each other is not statistically significant. In contrast, moderately and highly related industries demonstrate statistically significant spillover effects, and highly related industries experienced 28 percent more employment drop than moderately related industries. This observation supports the hypothesis that labor market pooling is a channel through which mass layoff events extend their impact to nearby establishments.

Input-Output Linkages. Mass layoffs influence the input-output linkages within the local area, impacting upstream and downstream establishments. To quantitatively assess this channel of spillover, I turn to the widely used Benchmark Input-Output (I-O) accounts prepared by Bureau of Economic Analysis[25] which document the flow of intermediate goods and services between industry pairs. I leverage this data to quantify how mass layoffs influence these intricate inter-industry relationships.(Delgado et al., 2016). I follow Ellison et al. (2010) suggestion of creating a symmetric I-O index as follows:

$$IO_{ij} = Max[input_{i \to j}, input_{i \leftarrow j}, output_{i \to j}, output_{i \leftarrow j}], \tag{1.7}$$

where $input_{i \to j}$ is the share of industry i's total input value which is bought from industry j, and $output_{i \to j}$ is the share of industry i's total output value which is sold to industry j.

The I-O index serves as a metric for quantifying the linkages between two industries, capturing the extent of buying and selling activities between them. Ranging from zero to one, a value of zero indicates no transactions occurring between the two industries. As with the previous measures, the middle panel of Figure 1.12 displays the spillover effects

[25] I use 2016 data at 3-digits NAICS from Bureau of Economic Analysis.

categorized by the degree of linkage between the industries of the event establishment and affected establishments. The results highlight that industries with closer linkages experience more substantial employee losses. Among the three channels examined, input-output linkages yield the most robust and pronounced results, emphasizing the importance of I-O linkages.

Knowledge Spillover. Knowledge sharing among workers from different firms can occur through two primary pathways: formal and informal interactions between workers and workers' movement to new firms, facilitating knowledge exchange through interactions with new colleagues. While quantifying personal interactions among workers from different firms is not feasible within a quantitative framework, we can proxy potential knowledge spillover by comparing shared skills between industry pairs. Labor occupations have commonly served as a metric for assessing the degree of similarity in skills shared between various industries (Glaeser and Kerr 2009; Gabe and Abel 2011). In my analysis, I leverage data from the OES Survey conducted by the Bureau of Labor Statistics in 2016. This dataset encompasses occupations within the non-governmental sector and offers insights into the prevalence of each occupation within different industries at the 4-digit NAICS code. Specifically, for each occupation, OES provides the proportion of that occupation relative to the total occupational employment within the industry. Utilizing this dataset and following the approach outlined by Glaeser and Kerr (2009) and Delgado et al. (2016), I calculate the pairwise correlation between the occupational compositions of any two industries:

$$Occ_{ij} = Correlation(Occupation_i, Occupation_j), \tag{1.8}$$

where $Occupation_i$ is a vector of the share of occupations in industry i, a higher correlation indicates that the two industries share more skill sets. The top panel of Figure 1.12's bottom panel represents the results for sub-samples of labor occupation. Evidently, industries with a higher rate of shared occupation with the event industry lost more employment.

All three measures consistently suggest that industries that are economically closer experience more employment decline. Table 1.8 shows the correlation between each of these measures. While they are positively correlated, their weak correlations suggest that each of them mostly captures a different channel.

Finally, to summarize the agglomeration channels, I merge the measures that exhibited spillover effects - input-output linkages and employment flow to estimate comprehensive spillover impacts. The affected establishments are categorized into three groups based on their economic proximity to the event establishment: the least related (lowest 50 percent in both measures), modestly related (top 50 percent in one measure), and highly related (top 50 percent in both measures). The results in Table 1.6 reveal insignificant spillover effects for the least related industries; however, establishments with even moderate economic association, as indicated by either input-output linkages or industry transitions, exhibit significant negative spillover effects. This highlights the intricate nature of economic connections and their role in influencing the consequences of mass layoffs.

1.5.2 Tradability and Local Multiplier Effect

At a higher level of categorization, industries can be divided into two broad sectors: tradable and non-tradable. Tradable industries produce goods and services that can be sold in national or international markets and thus are not constrained by the local economy's market. Conversely, non-tradable industries rely on local market demand, as their products are not transferable to other markets. I segment the event and affected industries into tradable and non-tradable sectors, resulting in four sub-samples.[26] Table 1.7 presents the outcomes of equation (1.4) for them.

Column (1) displays the negative spillover effect of the tradable events on tradable industries. In this case, neither the event nor the affected establishments were limited to the local market. Hence, the demand for the affected establishments is maintained, and even if

[26]I use Delgado et al. (2016) results to define tradable sectors. They use multiple measures of economic distance and choose 778 sectors (at 6-digit NAICS) as tradable sectors.

it is, they can sell their final products to new buyers outside of the local market. Therefore, the primary mechanism behind the 4.9 percentage points drop in employment of tradable establishments is the agglomeration economies, discussed in section 5.1.

Column (2) delves into the impact of tradable events on non-tradable establishments, showing 4.4 percentage points decline in non-tradable sector employment. Here, the local multiplier effect is a key channel driving the negative spillover. Given that the sectors of the event and affected establishments are different, the local multiplier effect is the substantial driver of the spillover effect. The decline in the number of workers reduces the demand for non-tradable goods and services and emerges as a decline in employment.

Column (3) presents the effect of non-tradable events on tradable establishments. The estimation is comparatively smaller than in Columns (1) and (2) by more than 35 percent, which could be attributed to two potential reasons. Firstly, compared to Column (1), economic closeness is weaker due to the establishments belonging to different sectors than the event. Secondly, unlike Column (2), the affected establishments are not reliant on local demand, causing the local multiplier effect to be less influential.

Lastly, in Column (4), I fail to reject the null hypothesis concerning the impact of non-tradable events on non-tradable establishments. While local multiplier effects and forces of agglomeration economies push employment levels down, there is a positive channel at play as well. When both event and affected establishments are in non-tradable sectors, closure or downsizing of the event establishment opens up opportunities for competitors to fulfill the local demand. Therefore, establishments in the same industry will expand and hire more workers.

1.5.3 Sensitivity Analysis

Robustness Check

While the results are robust to various fixed effects and radius of control regions, in this section, I provide three distinct robustness analyses for the baseline results.

Alternative Difference-in-Differences Methods. As discussed earlier in this section, researchers have introduced updated methods to estimate difference-in-differences regressions. I use Sun and Abraham (2021), Gardner (2021), and Callaway and Sant'Anna (2021) that primarily deal with staggered designs to check if the pre-event parallel trends and the post-event negative spillover effect still hold. Figure 1.C1 represents the point estimate of my baseline results with these three alternative methods.[27] The pre-event trend is very similar to Sun and Abraham (2021), and the other two show better parallel trends than the baseline method. Moreover, we can see that the decline in employment is persistent among all methods. Therefore, I can conclude that my results are robust to these alternative methods.

Alternative Identification. The main identification is centered around finding the best possible counterfactual to the event establishment. Here, I introduce an alternative approach in which the treatment regions are unchanged (i.e., 6km around the mass layoff establishment), but the control regions differ. For each event, the control region is a ring around the mass layoff establishment with a smaller radius of 15km and a larger radius of 20km. A 15km radius is chosen to minimize the potential spillover to the control area. To have a comparable control region, I use inverse propensity score weighting (IPW). The control is re-weighted based on pre-event employment trends and industry (2-digit NAICS) composition. Figure 1.C2 displays the re-weighting method alongside the main identification. The parallel trends assumption holds even better for the alternative method, and in the post-event period, we see a similar trend with larger effects in years two and three. In the alternative method, the control regions are mostly within the same CZ as the event, suggesting that some workers get reemployed within the CZ.

[27] Table 1.C1 represents the point estimates, standard errors, and significance of estimates. After the second year, all measures are statistically significant.

Falsification Test. What if the drop in employment levels is not due to the mass layoff shock but is a local-specific decline in the economy? To address this concern, I use a falsification test. First, I randomly select a 10 percent sample from the main sample. Second, I define 100 similar studies to the main analysis. In each of these analyses, there are 132 events, and each fake event simulates one real event. Each fake event is drawn from a sample with the same year, the same industry (2-digit NAICS), and the same commuting zone as the real event. Third, I follow the same structure as the main identification to define treatment and control areas, and finally, I run equation (1.5) 100 times. Figure 1.C3 shows the visualization of the fake analysis in grey lines and the actual regression line in red. The pre-trend does not deviate from the simulation results, but the post-trend completely deviates from the simulation after the first year following the event.

Do Size of Mass Layoffs Matter?

The results from this paper and prior research on this issue suggest discrepancies regarding the effects of mass layoffs on local areas. In this section, I aim to address these disparities by examining various thresholds for defining mass layoffs and comparing them with findings from other studies.

While Jofre-Monseny et al. (2018) and vom Berge and Schmillen (2022) indicate positive spillover effects, Celli et al. (2023) and Gathmann et al. (2018) show negative and neutral spillover effects, respectively. One potential explanation could be the differences in the industry of the event establishments experiencing mass layoffs. Due to stronger input-output linkages and higher average wages, manufacturing and tradable sectors, in particular, tend to yield more adverse indirect effects. Some evidence supports this notion, as demonstrated in 1.7, the magnitude of spillover effects appears greater following tradable events. However, it's worth noting that Jofre-Monseny et al. (2018) and Gathmann et al. (2018) report opposing indirect effects despite both exclusively studying manufacturing mass layoffs.

Another explanation could be the differing national and local labor market conditions across the studies, including variations in the timing of the business cycles in the country of study. Conducting a comparative analysis of local labor market conditions for each study is beyond the scope of this paper. However, a quantifiable approach involves examining heterogeneity in the size of mass layoffs. This paper, along with Gathmann et al. (2018), focuses on very large mass layoff events at flagship plants, whereas other studies with neutral or positive results consider events with much smaller layoffs.[28] To evaluate disparities in spillover effects based on mass layoff size, I conducted the baseline analysis across three groups: mass layoffs of 200 to 300 employees, 500 to 1000 employees, and over 1000 employees. Figure 1.13 illustrates that mass layoffs resulting in 200 to 300 employment losses exhibit an indirect employment gain four years later. This finding aligns with the results of Jofre-Monseny et al. (2018) and vom Berge and Schmillen (2022), which also report positive spillover effects with similar average employment losses. Large mass layoffs of 500 or more employment loss show negative spillover effects like Gathmann et al. (2018). Additionally, very large mass layoffs of over 1000 employees lead to a greater decline in indirect employment compared to layoffs of 500 to 1000 employees.

1.6 Conclusion

In this study, I leveraged extensive administrative data, including precise geographic coordinates of all establishments in California, to assess the spillover effects of large-scale mass layoffs on nearby establishments. My findings present compelling evidence that large mass layoffs cause a persistent, negative impact on nearby establishments' employment levels, with clear indications of spatial decay. Spillover effects diminish to insignificance beyond a 6 km radius of the event establishment. Within this 6 km radius, the average employment shock across 132 events is 5.5 percent, resulting in a 6 percentage point decline in employment four years later. In other words, a 1 percent employment shock caused 1.1 percentage point

[28] The average number of layoffs in this paper is 792, while in Gathmann et al. (2018), it stands at 1702. In contrast, Jofre-Monseny et al. (2018) analyzes events with a median of 264 employment losses, and the average employment loss in vom Berge and Schmillen (2022) is 101.

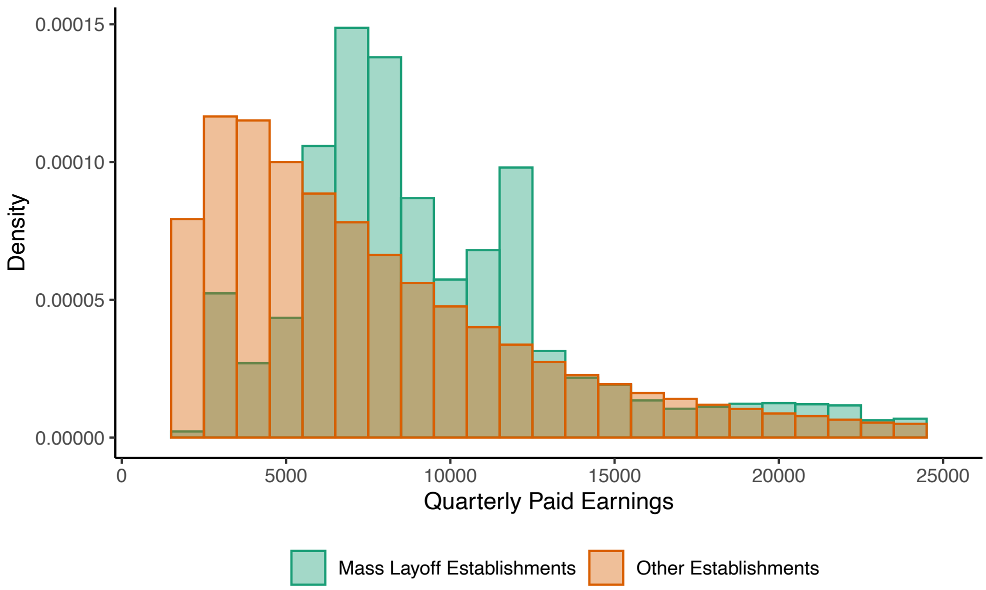
spillover effects on employment within 6km of the event. Moreover, treated areas experienced a 9.8 percentage point decline in total earnings and a 3 percentage point decline in the number of operating establishments. However, there is no tangible alteration in the average earnings per employee. For the first time in the literature, this paper explores and tests the importance of all three channels of agglomeration (labor market pooling, knowledge spillover, and input-output linkages) on spillover effects. I show that when the industry of event establishments and affected establishments are closer in terms of any of these agglomeration channels, the impact of mass layoffs on employment intensifies.

Furthermore, for the first time in the literature, I show the spillover effects of mass layoffs at the intensive margins by employing a balanced sample of surviving establishments after the events. At the intensive margins, employment levels of neighboring establishments decline by 2 percentage points four years later. Using the establishment level results, I also show heterogeneity in the effects of mass layoff by type of firm. Overall, establishments that belong to younger and smaller firms experience greater employment decline compared to their larger and older counterparts.

These findings can provide insights for policymakers seeking to respond optimally to large mass layoffs. Policymakers can use QCEW datasets to identify and target potentially affected nearby establishments by distance. Moreover, adopting a more targeted approach by focusing on younger and smaller establishments, as well as those economically closer to the event establishment, may prove effective in mitigating the adverse effects of mass layoffs.

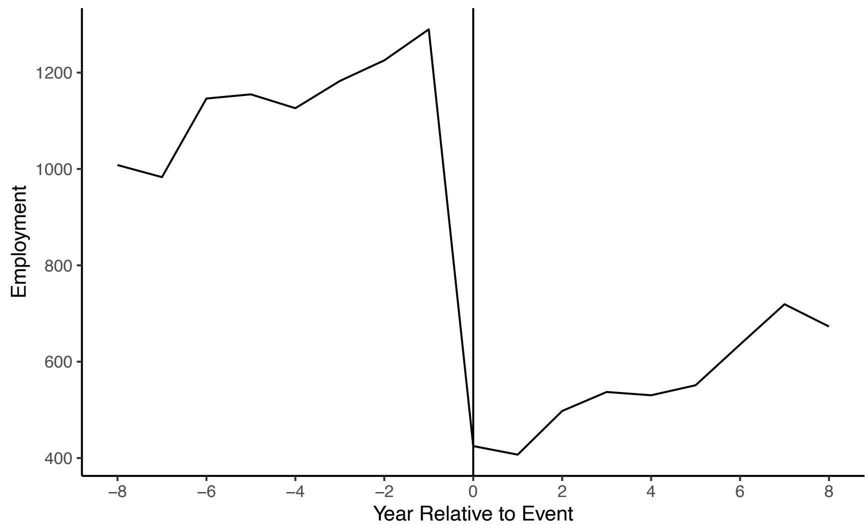
1.7 Figures and Tables

Figure 1.1: Distribution of Average Quarterly Earnings in 2003

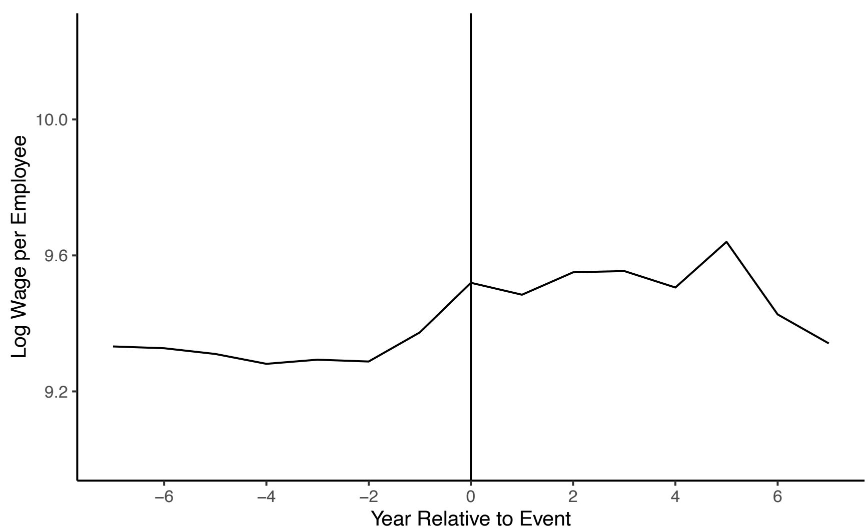


Note: This figure shows the average quarterly paid earnings distribution at the establishment level for firms associated with mass layoff and non-mass layoff establishments in 2003. Earnings include both part-time and full-time pays. The mass layoff establishment sample includes all establishments of firms with at least one mass layoff establishment in 2004-2015. A mass layoff is defined as 30 percent decline in employment and a reduction of 500 employees within a year.

Figure 1.2: Mass Layoff Establishments Over Time



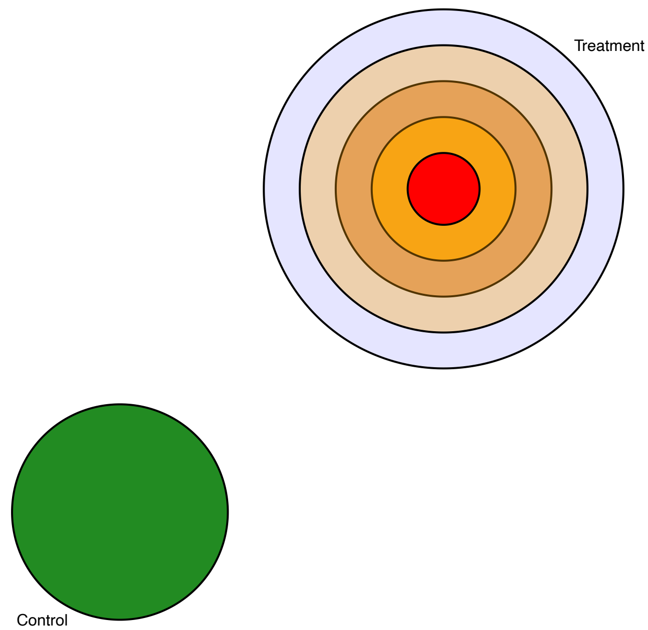
(a) Employment Level



(b) Average Quarterly Earnings per Employee

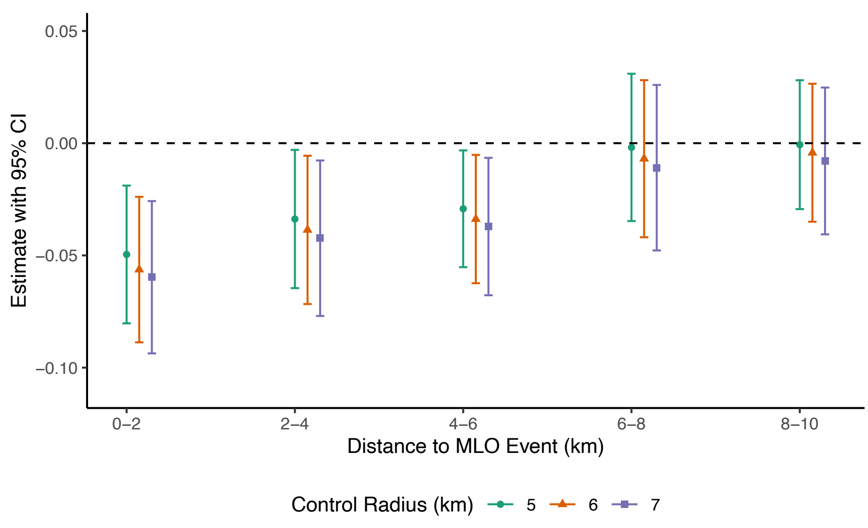
Note: Panel (a) shows the annual employment level of mass layoff establishments conditional on being operational. Panel (b) shows the log mean of quarterly earnings per employee of mass layoff establishments conditional on being operational. In both panels, the sample is an unbalanced panel data, in which closed establishments are dropped.

Figure 1.3: Schematic of Treatment and Control Areas for Spatial Decay Analysis



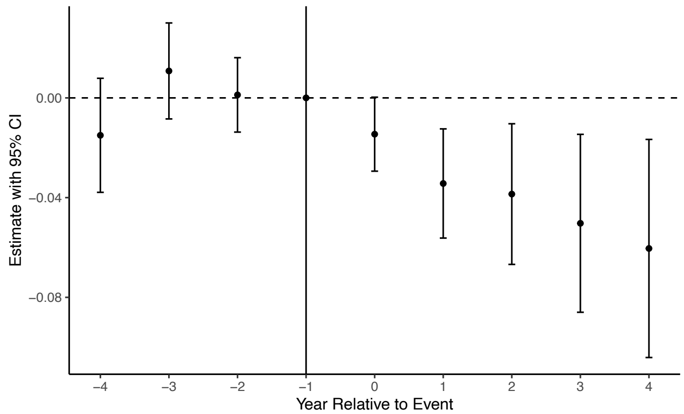
Note: This figure represents the schematic of treatment and control areas for spatial decay analysis. There are five treatment regions in the shape of sequential 2km donuts around the event establishment. The control area is a circle around the counterfactual establishment that can take radii of 5, 6, and 7km.

Figure 1.4: Spatial Decay in Employment Spillover Effects of Mass Layoffs

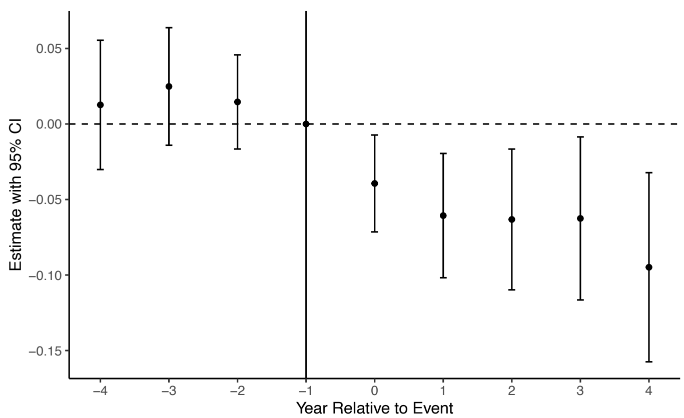


Note: This figure represents the results of equation (1.4) for various rings with radii varying by 2km and different circles radii. Each regression controls for region, year, relative time, and industry fixed effects. The standard errors are clustered at the region level.

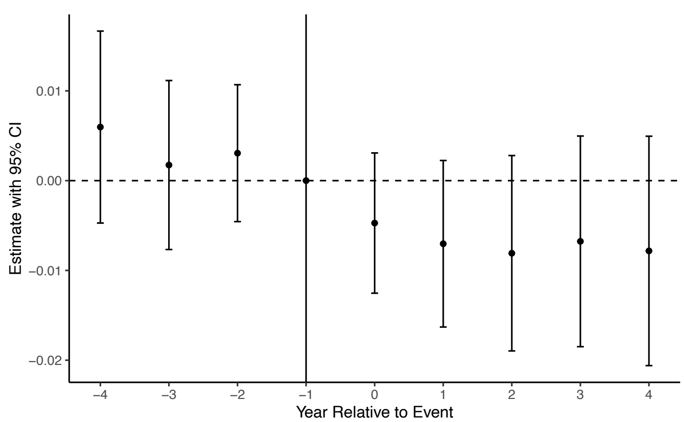
Figure 1.5: Spillover Effects of Mass Layoffs



(a) Employment



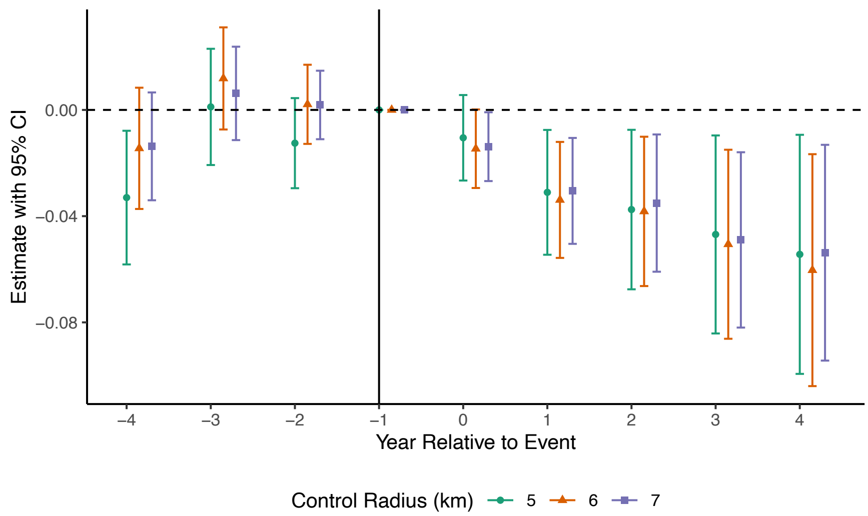
(b) Total Paid Earnings



(c) Earnings per Employee

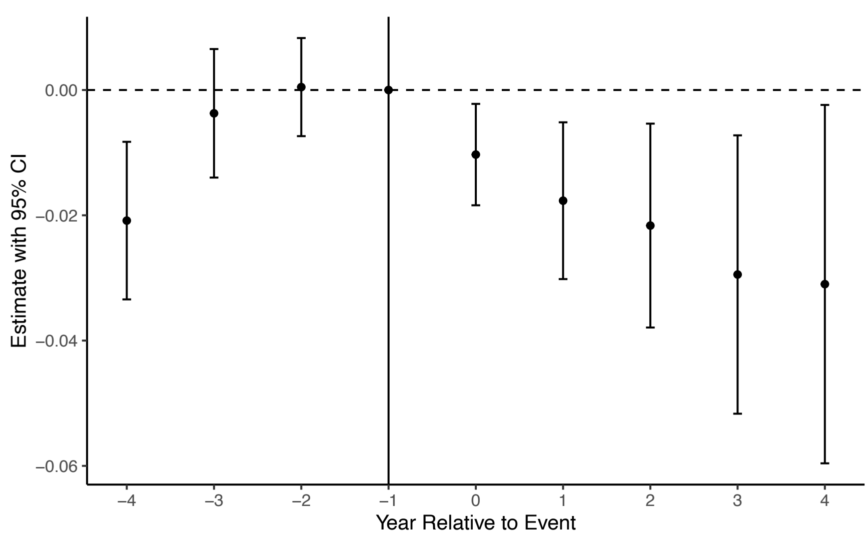
Note: This figure represents the results of equation (1.5) for log(employment), log(total paid earnings), and log(earnings per employee). Each regression controls for region, year, relative time, and industry fixed effects. The standard errors are clustered at the region level.

Figure 1.6: Employment Spillover Effects by Various Control Radii



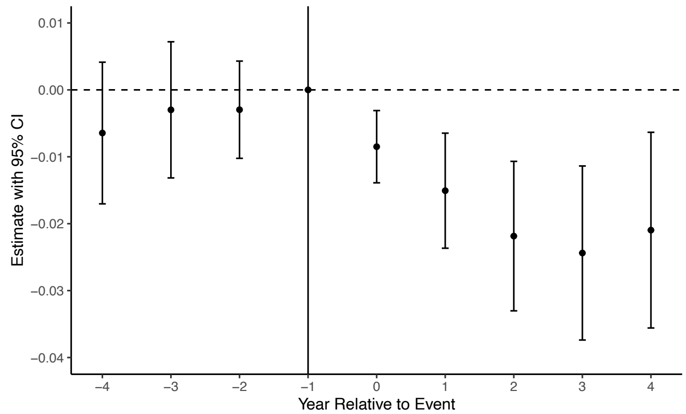
Note: This figure represents the results of equation (1.5) for log(number employment) using 3 different radii for control regions, while keeping the treatment radius at 6km. It suggests that the results are robust to changing size of control regions. The standard errors are clustered at the region level.

Figure 1.7: Spillover Effects on The Number of Operating Establishments

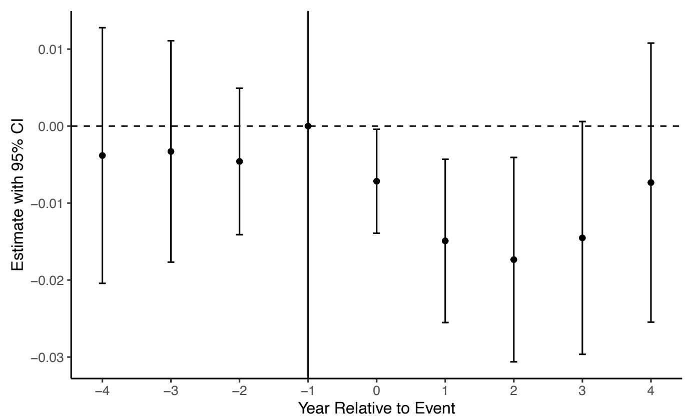


Note: This figure represents the results of equation (1.5) for log(number of establishments). I control for region, year, relative time, and industry fixed effects. The standard errors are clustered at the region level.

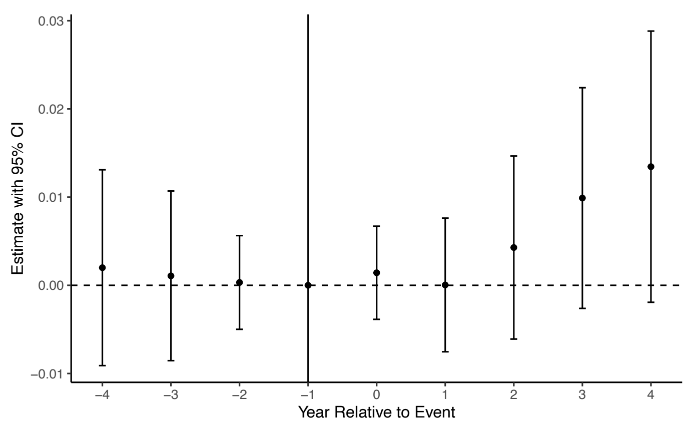
Figure 1.8: Spillover Effects of Mass Layoffs at Establishment Level



(a) Employment



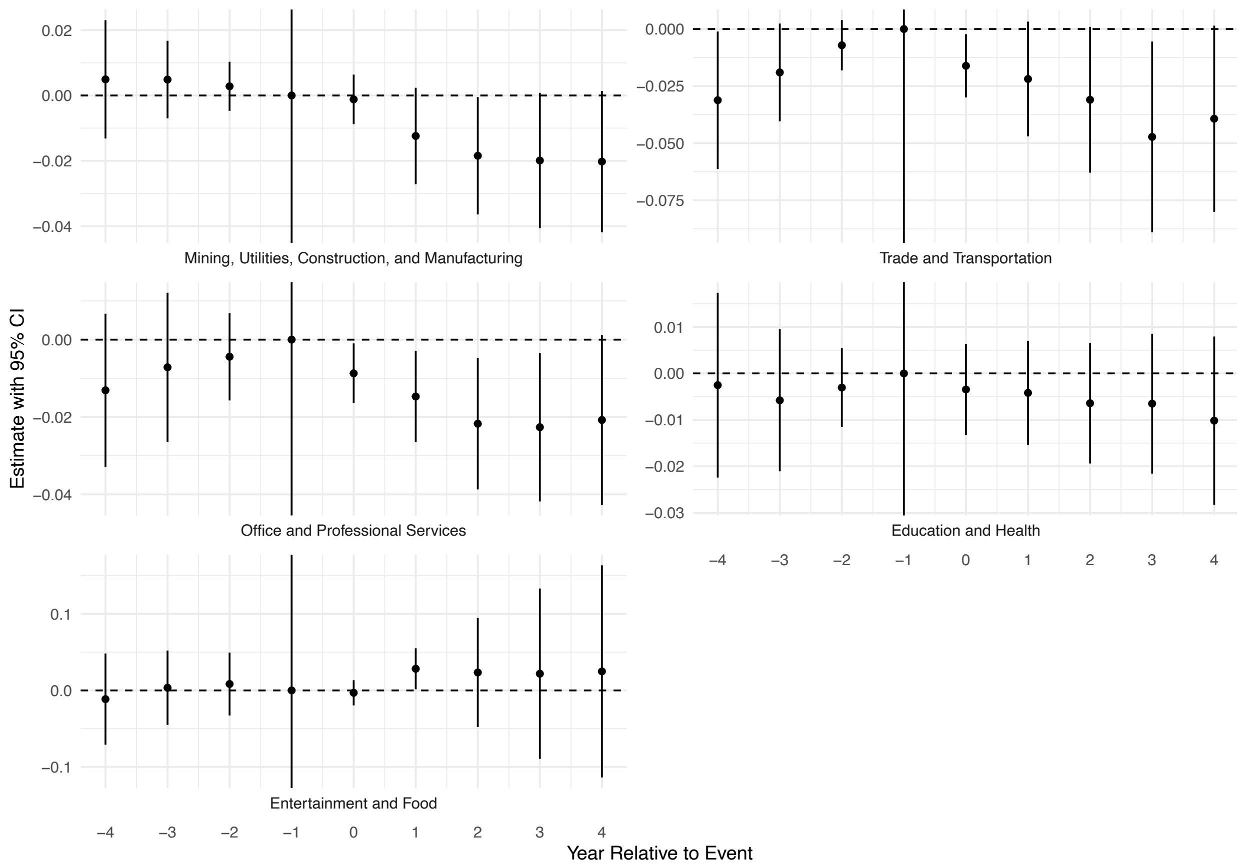
(b) Total Paid Earnings



(c) Earnings per Employee

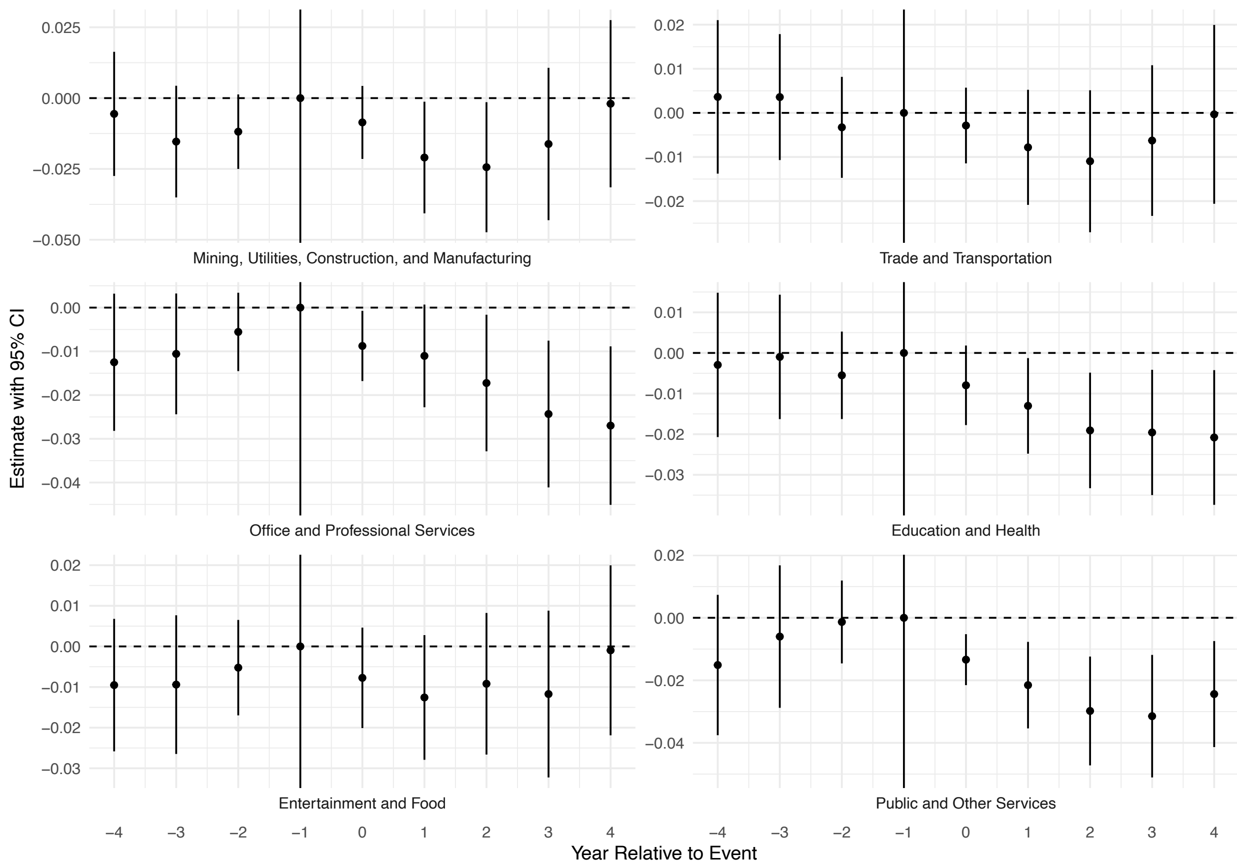
Note: This figure represents the results of equation (1.6) for log(employment), log(total paid earnings), and log(earnings per employee). Each regression controls for establishment ID, region, year, relative time, and industry fixed effects. The standard errors are clustered at the region level.

Figure 1.9: Employment Spillover Effects by Industry of Event Establishments



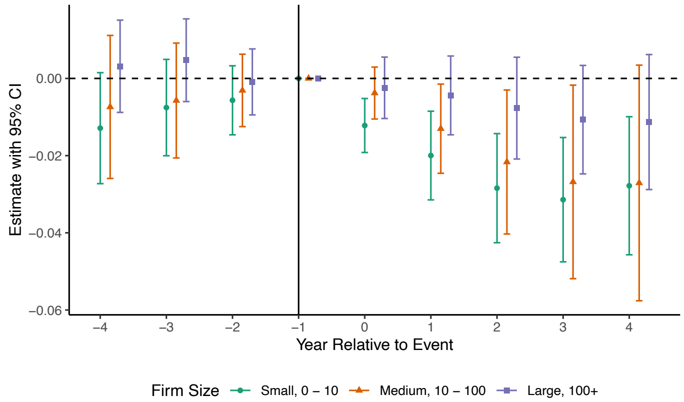
Note: This figure represents the results of equation (1.5) for log(employment) for 5 sub-samples divided by the industry of mass layoff establishments. Each regression controls for establishment ID, region, year, relative time, and industry fixed effects. The standard errors are clustered at the the region level.

Figure 1.10: Employment Spillover Effects by Industry of Affected Establishments

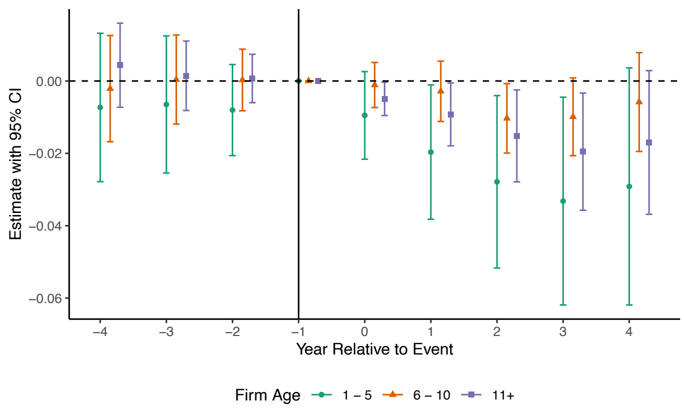


Note: This figure represents the results of equation (1.5) for log(employment) for 6 sub-samples divided by the industry of affected establishments. Each regression controls for establishment ID, region, year, relative time, and industry fixed effects. The standard errors are clustered at the region level.

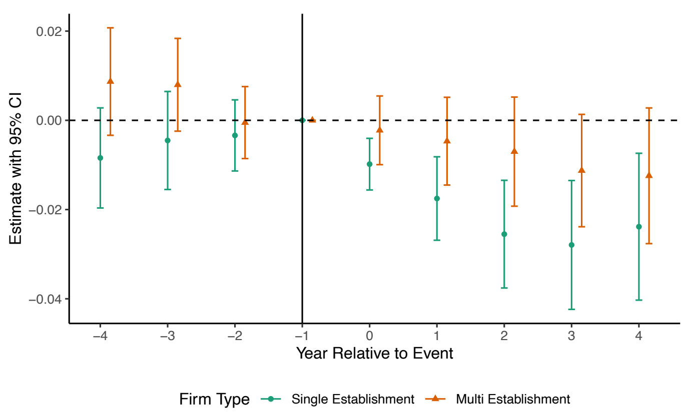
Figure 1.11: Employment Spillover Effects by Firm Type



(a) Firm Size



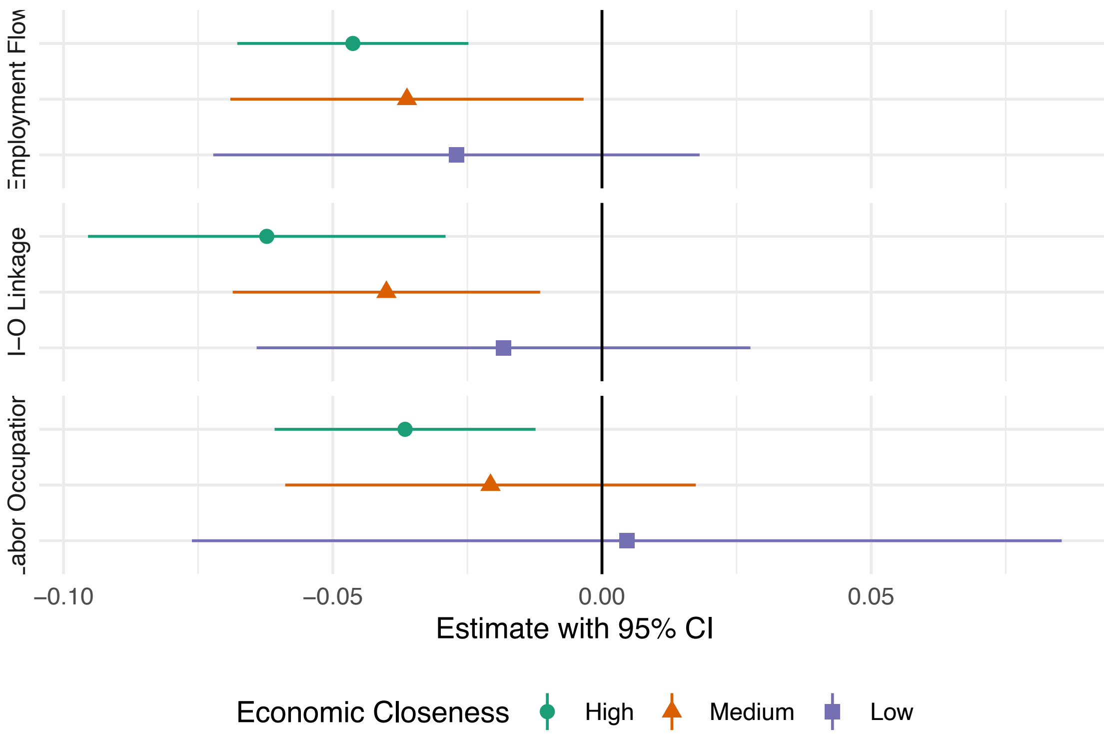
(b) Firm Age



(c) Firm Type

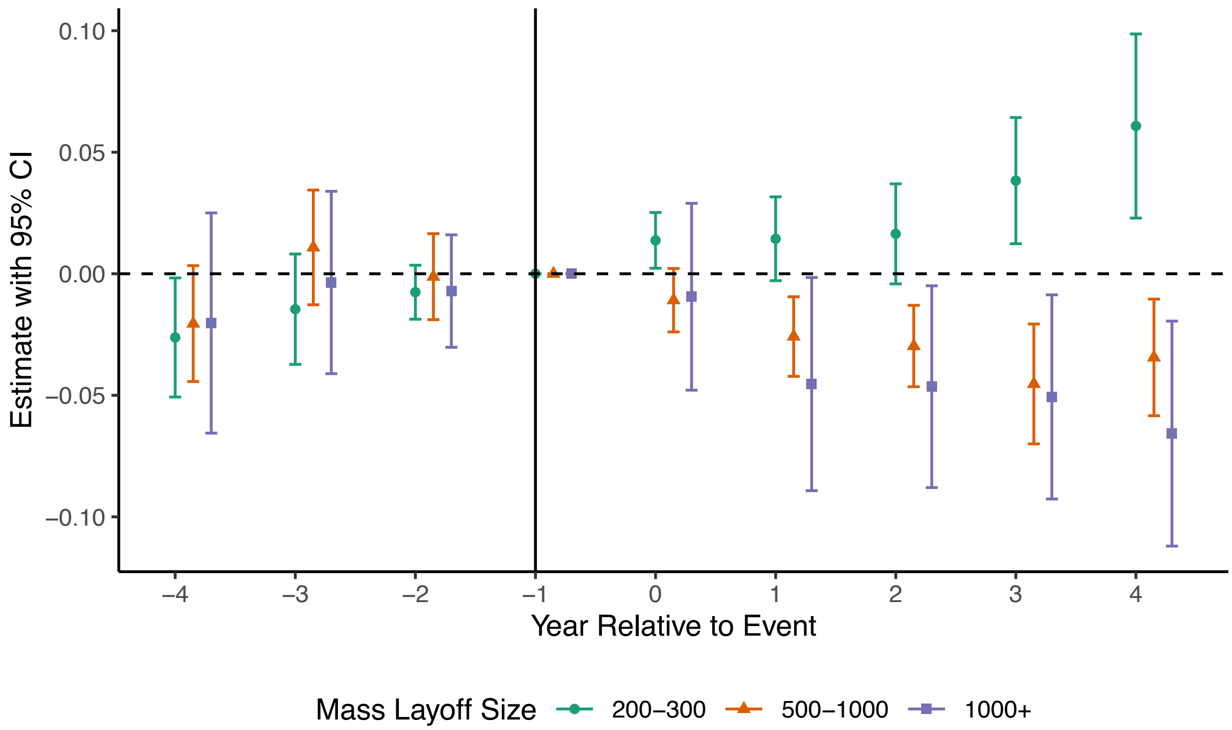
Note: This figure represents the results of equation (1.6) for log(employment). Each panel shows the employment effects by type of establishment. Each regression controls for establishment ID, region, year, relative time, and industry fixed effects. The standard errors are clustered at the region level.

Figure 1.12: Employment Spillover Effects by Economic Closeness



Note: This figure represents the results of equation (1.4) for log(employment). Each panel shows three regression analysis for sub-samples divided by economic closeness indexes. Each regression controls for region, year, relative time, and industry fixed effects. The standard errors are clustered at region Industry level.

Figure 1.13: Employment Spillover Effects by Various Mass Layoff Sizes



Note: This figure represents the baseline results for three groups based on size of mass layoff events.

Table 1.1: Industries of Mass Layoff Establishments

Industry	Number of Mass Layoff Events
Professional and Business Services	27
Finance and Insurance	23
Educational and Health Services	22
Manufacturing	17
Trade, Transportation and Utilities	15
Construction	12
Information	10
Other Sectors	6
Total	132

Note: This table shows the industry of the mass layoff events using QCEW administrative data. A mass layoff is defined as 30 percent decline in employment and a reduction of 500 employees within a year. The industry breakdown is based on super sectors defined by the Bureau of Labor Statistics (BLS), which combines some of the 2-digits NAICS codes. Trade, Transportation, and Utilities is 22, 42, 44, 45, 48, and 49; Financial Activities is 52 and 53; Professional and Business Services is 54, 55, and 56; Educational and Health Services is 61 and 62. Other Sectors are combination of different NAICS codes that are suppressed within one group.

Table 1.2: Summary Statistics for Establishments Within Treatment and Control Regions

	Control	Treatment	Difference
Employment	14.9	16.78	1.82
	(0.97)	(0.72)	(1.19)
Quarterly Earnings	10594.42	9870.53	-721.09
	(709.53)	(281.92)	(768.28)
Firm Age	8.32	8.81	0.49***
	(0.28)	(0.13)	(0.3)

Note: This table shows the mean of employment level and quarterly earnings of event establishments and age of firms associated with the event establishments using QCEW administrative data (standard deviations in parentheses). Treatment and control areas are defined as 6km around the event and counterfactual establishments. All means are calculated at one year before the event year.

Table 1.3: Industry Share of Treatment and Control Regions

	Control	Treatment	Difference
Natural Resources and Mining	0.01	0.01	0.00
	(0.00)	(0.00)	(0.00)
Trade, Transportation and Utilities	0.15	0.15	0.01
	(0.01)	(0.00)	(0.01)
Construction	0.06	0.05	0.00
	(0.00)	(0.00)	(0.01)
Manufacturing	0.05	0.05	0.00
	(0.00)	(0.00)	(0.01)
Information	0.04	0.02	-0.02
	(0.01)	(0.00)	(0.01)
Financial Activities	0.08	0.08	0.00
	(0.00)	(0.00)	(0.01)
Professional and Business Services	0.14	0.14	0.01
	(0.01)	(0.01)	(0.01)
Educational and Health Services	0.12	0.12	0.01
	(0.01)	(0.01)	(0.01)
Leisure and Hospitality	0.07	0.07	0.00
	(0.00)	(0.00)	(0.00)
Other Services (Except Public Admin.)	0.25	0.27	0.03
	(0.02)	(0.01)	(0.02)
Public Administration	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)
Unknown	0.05	0.03	-0.02
	(0.01)	(0.00)	(0.01)

Note: This table shows the industry of the treatment and control areas using QCEW administrative data. Treatment and control areas are defined as 6km around the event and counterfactual establishments. The industry breakdown is based on super sectors defined by the Bureau of Labor Statistics (BLS), which combines some of the 2-digits NAICS codes. Natural Resources and Mining is NAICS codes 11 and 21; Trade, Transportation, and Utilities is 22, 42, 44, 45, 48, and 49; Financial Activities is 52 and 53; Professional and Business Services is 54, 55, and 56; Educational and Health Services is 61 and 62; Leisure and Hospitality is 71 and 72.

Table 1.4: Baseline Results for Spillover Effect of Mass Layoffs

Dependent Variables:	Employment				Total Earnings Paid				Earnings per Employee			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
τ = -4	-0.0150	-0.0130	-0.0184	-0.0085	0.0126	-0.0201	0.0081	0.0221	0.0060	0.0025	0.0056	0.0081
	(0.0117)	(0.0117)	(0.0119)	(0.0116)	(0.0218)	(0.0214)	(0.0220)	(0.0221)	(0.0055)	(0.0051)	(0.0054)	(0.0058)
τ = -3	0.0108	0.0151	0.0079	0.0133	0.0249	0.0161	0.0210	0.0307	0.0017	-0.0001	0.0013	0.0035
	(0.0098)	(0.0098)	(0.0098)	(0.0098)	(0.0198)	(0.0192)	(0.0197)	(0.0201)	(0.0048)	(0.0045)	(0.0047)	(0.0051)
τ = -2	0.0012	0.0041	-0.0014	0.0015	0.0146	0.0131	0.0108	0.0173	0.0031	0.0030	0.0023	0.0046
	(0.0076)	(0.0073)	(0.0077)	(0.0076)	(0.0159)	(0.0153)	(0.0157)	(0.0160)	(0.0039)	(0.0037)	(0.0038)	(0.0040)
τ = -1												
τ = 0	-0.0146*	-0.0208***	-0.0128*	-0.0114	-0.0394**	-0.0419***	-0.0393**	-0.0348**	-0.0047	-0.0030	-0.0058	-0.0037
	(0.0076)	(0.0075)	(0.0076)	(0.0076)	(0.0164)	(0.0154)	(0.0163)	(0.0165)	(0.0040)	(0.0037)	(0.0040)	(0.0042)
τ = 1	-0.0343***	-0.0417***	-0.0301***	-0.0293***	-0.0607***	-0.0603***	-0.0572***	-0.0524**	-0.0070	-0.0062	-0.0085*	-0.0046
	(0.0112)	(0.0114)	(0.0111)	(0.0110)	(0.0210)	(0.0200)	(0.0210)	(0.0211)	(0.0047)	(0.0044)	(0.0047)	(0.0050)
τ = 2	-0.0386***	-0.0437***	-0.0331**	-0.0310**	-0.0632***	-0.0528**	-0.0590**	-0.0515**	-0.0081	-0.0082	-0.0093*	-0.0049
	(0.0144)	(0.0149)	(0.0143)	(0.0138)	(0.0238)	(0.0232)	(0.0237)	(0.0234)	(0.0055)	(0.0051)	(0.0055)	(0.0058)
τ = 3	-0.0503***	-0.0511***	-0.0445**	-0.0360**	-0.0625**	-0.0454*	-0.0585**	-0.0425	-0.0068	-0.0074	-0.0091	-0.0028
	(0.0182)	(0.0187)	(0.0181)	(0.0171)	(0.0275)	(0.0273)	(0.0276)	(0.0265)	(0.0060)	(0.0055)	(0.0060)	(0.0063)
τ = 4	-0.0604***	-0.0528**	-0.0570***	-0.0418**	-0.0949***	-0.0625**	-0.0922***	-0.0682**	-0.0078	-0.0084	-0.0096	-0.0021
	(0.0223)	(0.0226)	(0.0220)	(0.0206)	(0.0319)	(0.0316)	(0.0318)	(0.0303)	(0.0065)	(0.0060)	(0.0065)	(0.0069)
Fixed-effects												
Region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relative Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry (4-digit)	Yes	Yes	Yes		Yes	Yes	Yes		Yes	Yes	Yes	
Region × Industry		Yes				Yes				Yes		
Calendar Year × Industry			Yes				Yes				Yes	
Fit statistics												
Observations	738,479	738,479	738,479	738,479	738,479	738,479	738,479	738,479	712,594	712,594	712,594	712,594
R^2	0.28089	0.55212	0.29100	0.10272	0.21514	0.46773	0.22570	0.09138	0.34184	0.55581	0.35069	0.06325

Clustered (Region) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: The dependent variables for columns (1)-(4) is log(employment), for (5)-(8) is log(total paid earnings), and for (9)-(12) is log(earnings per employee).Columns (1), (5), and (9) are the baseline results presented in Figure 1.5. In columns (2), (6), and (10), the interaction of region and industry are included. In columns (3), (7), and (11), interaction of year and industry are included. In columns (4), (8), and (12), no industry or interaction with industry is included.

Table 1.5: Spillover Effect of Mass Layoffs on Surviving Establishments

Dependent Variables:	Employment		Total Paid Earnings		Earnings per Employee	
Model:	(1)	(2)	(3)	(4)	(5)	(6)
τ = -4	-0.0109*	-0.0064	-0.0115	-0.0038	-0.0008	0.0020
	(0.0065)	(0.0054)	(0.0103)	(0.0084)	(0.0053)	(0.0056)
τ = -3	-0.0065	-0.0030	-0.0112	-0.0033	-0.0027	0.0011
	(0.0058)	(0.0052)	(0.0084)	(0.0073)	(0.0044)	(0.0049)
τ = -2	-0.0036	-0.0030	-0.0080	-0.0046	-0.0021	0.0003
	(0.0035)	(0.0037)	(0.0051)	(0.0048)	(0.0029)	(0.0027)
τ = -1						
τ = 0	-0.0080***	-0.0085***	-0.0068*	-0.0072**	0.0013	0.0014
	(0.0026)	(0.0027)	(0.0035)	(0.0034)	(0.0027)	(0.0027)
τ = 1	-0.0144***	-0.0151***	-0.0142***	-0.0149***	0.0002	0.0000
	(0.0042)	(0.0044)	(0.0052)	(0.0054)	(0.0039)	(0.0038)
τ = 2	-0.0212***	-0.0219***	-0.0167**	-0.0174**	0.0043	0.0043
	(0.0056)	(0.0057)	(0.0068)	(0.0067)	(0.0053)	(0.0053)
τ = 3	-0.0237***	-0.0244***	-0.0136*	-0.0145*	0.0101	0.0099
	(0.0066)	(0.0066)	(0.0080)	(0.0077)	(0.0065)	(0.0064)
τ = 4	-0.0207***	-0.0210***	-0.0072	-0.0073	0.0133*	0.0134*
	(0.0074)	(0.0074)	(0.0096)	(0.0092)	(0.0080)	(0.0078)
Fixed-effects						
Calendar Year	Yes	Yes	Yes	Yes	Yes	Yes
Relative Time	Yes	Yes	Yes	Yes	Yes	Yes
Establishment ID	Yes	Yes	Yes	Yes	Yes	Yes
Industry (4-digit)	Yes	Yes	Yes	Yes	Yes	Yes
Firm Size		Yes		Yes		Yes
Firm Age		Yes		Yes		Yes
Fit statistics						
Observations	3,461,875	3,359,846	3,508,932	3,397,618	3,461,096	3,359,120
R^2	0.95119	0.95364	0.95988	0.96212	0.92094	0.92329

Clustered (Region) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: The dependent variable for columns (1) and (2) is log(employment), for (3) and (4) is log(total paid earnings), and for (5) and (6) is log(earnings per employee). This table displays the results of equation (1.6) with two different sets of controls. Columns (1), (3), and (5) follow the baseline controls, but in columns (2), (4), and (6), firm size and firm age controls are included; however, the results are robust to control changes.

Table 1.6: Spillover Effects of Mass Layoffs by Economic Closeness Based on I-O linkages and Employment Flow

Dependent Variable:	Employment		
Model:	(1)	(2)	(3)
ML × Post Event	0.0025	-0.0546***	-0.0574***
	(0.0114)	(0.0126)	(0.0199)
Fixed-effects			
Region	Yes	Yes	Yes
Industry	Yes	Yes	Yes
Calendar Year	Yes	Yes	Yes
Relative Time	Yes	Yes	Yes
Fit statistics			
Observations	198,472	272,447	258,000
R^2	0.42751	0.39886	0.46170

Clustered (Industry) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: This table presents the results of equation (1.4) for three different subsamples from the main sample based on the economic closeness of affected establishments to the event establishment. The dependent variable is log(employment). Two measures are used in this table: employment flow and input-output linkages, and industry pairs are divided into the top and bottom 50 percent of the distribution of each measure. Column (1) sample is a set of establishments at the bottom half of both measures' distribution. In column (2) sample, affected establishments are at the top half of distribution in one of the measures. Finally, in column (3), affected establishments are at the top half of both measures.

Table 1.7: Spillover Effects of Mass Layoffs by Tradability of Sectors

Dependent Variable:	Employment			
Model:	Traded on Traded	Traded on Non-Traded	Non-traded on Traded	Non-traded on Non-Traded
ML × Post Event	-0.0487***	-0.0435**	-0.0280**	-0.0484
	(0.0124)	(0.0156)	(0.0134)	(0.0376)
Fixed-effects				
Region	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes
Relative Time	Yes	Yes	Yes	Yes
Calendar Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	241,795	193,337	173,057	120,147
R^2	0.49657	0.29148	0.50889	0.30072

Clustered (Industry) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: This figure presents the results of equation (1.4) for four different subsamples from the main sample based on the tradability of the event and affected establishments' industry. The dependent variable is log(employment). To determine industries' tradability, I use Delgado et al. 2016 in which 778 6-digit NAICS codes are categorized as tradable industries.

Table 1.8: Correlation Between Economic Distance Indexes

Index	Employment Flow	I-O	Labor Occupation
Employment Flow	1		
I-O	0.11	1	
Labor Occupation	0.22	0.34	1

Note: This figure presents the correlation between three economic distances. Sources: Employer-employee matched data, BEA I-O tables, OES.

Appendix

1.A Relationship Between Local Economic Conditions and Mass layoff Incidence

To estimate the relationship between local labor market conditions and the probability of mass layoff events, I use two measures of GDP growth and employment growth at two different levels: year-industry (2-digits NAICS) and year-commuting zone-industry (1-digit NAICS) level. The result is four datasets that measure economic health at the industry and local-industry levels. Finally, I add the number of mass layoff incidences (132 total) to the related cells of each dataset. For the employment growth rate, I use QCEW data, and for the GDP growth rate, I use Bureau of Economic Analysis (BEA) data on industry and industry-county GDP. BEA estimates GDP at county, industry, and county-industry levels since 2001. The time period of these datasets is 2004-2015, the same period that we measure mass layoff events.

I use the following regressions to investigate if the decline in local economic conditions can predict large mass layoff events:

$$ML\ Incidence_{it} = \beta X_{it} + \epsilon_{it} \tag{1.9}$$

$$ML\ Incidence_{irt} = \beta X_{irt} + \epsilon_{irt} \tag{1.10}$$

X represents negative GDP growth or employment growth at the industry or CZ-industry level. Table 1.A1 represents the four estimates of 1.9 and 1.10. The only measure that shows a weak correlation is the GDP growth at the industry level. In contrast, the other measures suggest no correlation between the number of mass layoff incidences and economic conditions.

Table 1.A1: Relationship Between Economic Conditions and Mass Layoff Incidences

Dependent Variable:	Mass Layoff Incidence	
Model:	Industry	CZ-Industry
Panel (a)		
-1 $\times$*Employment Growth Rate*	0.0175*	0.0001
	(0.0100)	(0.0005)
R^2	0.18866	0.03607
Panel (b)		
-1 $\times$*GDP Growth Rate*	0.0138	0.0000
	(0.0109)	(0.0001)
R^2	0.18752	0.03607
Observations	228	1,728

Clustered (Industry) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

1.B Directly Displaced Workers

The focus of this paper is spillover effects of mass layoffs on nearby establishments. But, what are the labor market outcomes of the directly displaced workers from the event establishment? What are their chances to reach their pre-displacement earnings? To answer this, I employ a modify version of equation (1.5) at an individual level rather than a regional level for workers from 54 single establishment events:[29]

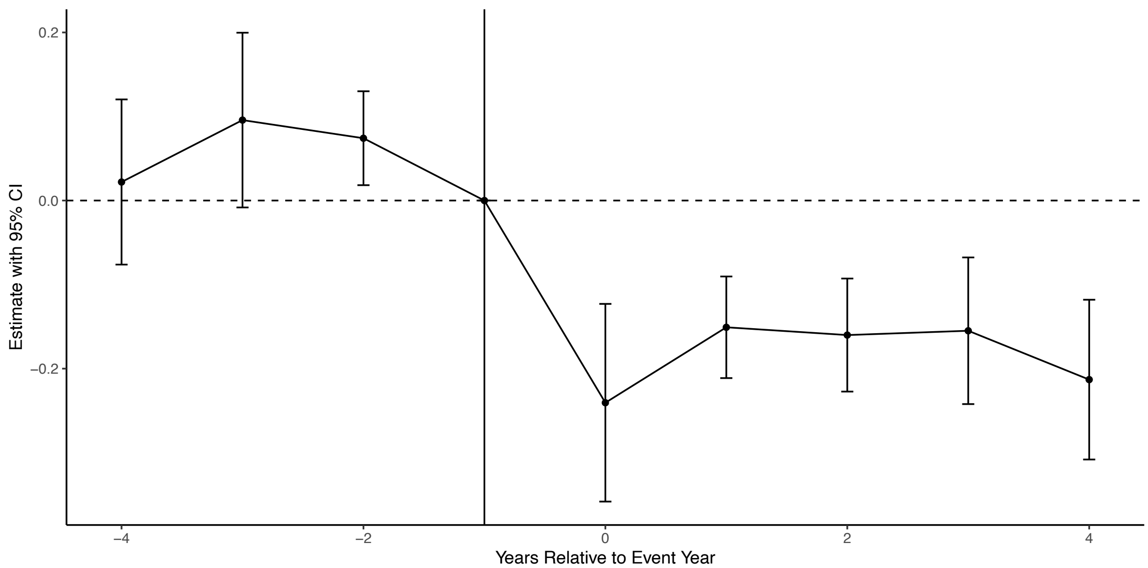
$$Y_{i\tau t} = \sum_{\tau=-4}^{-2} \alpha_\tau Event_{r\tau,t} + \sum_{\tau=0}^{4} \beta_\tau Event_{r\tau t} + \mu_i + \gamma_t + \lambda_\tau + \epsilon_{ir\tau t}, \tag{1.11}$$

where, $Y_{i\tau t}$ is displaced worker i's log of earnings, and I control for individual (μ_i), year (γ_t), and relative time (λ_τ) fixed effects. I cluster the standard errors at year level.

The results are displayed in Figure B.1, suggesting persistent income loss four years after the event, consistent with the displacement literature.

[29] As explained in Data section, Quarterly Earnings (QE) are not at the establishment level but firm level. Therefore, I can only directly find the earnings of the single establishment events.

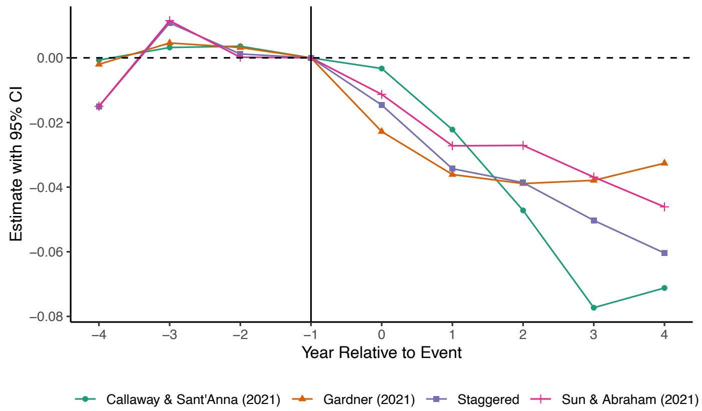
Figure 1.B1: Earnings of Displaced Workers Before and After Mass Layoff



Note: The direct effect of mass layoffs on the earnings of displaced workers. The figure shows a difference-in-difference event study estimate for annual earnings of directly displaced workers using equation (1.11). The control group includes non-displaced workers in firms with at least 500 employees one year before the event. The standard errors are clustered at the year level. Individual, industry, calendar year, and relative year fixed effects are included. In order to be able to match event establishments with employer-employee matched data, Events are limited to single establishment cases.

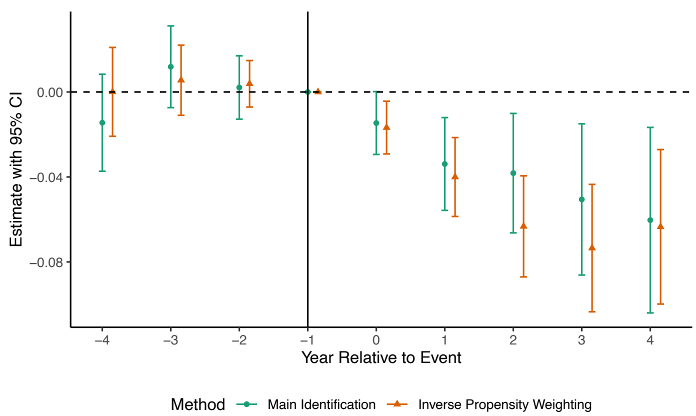
1.C Sensitivity and Robustness Checks

Figure 1.C1: Employment Spillover Effects by Various Difference-in-differences Methods



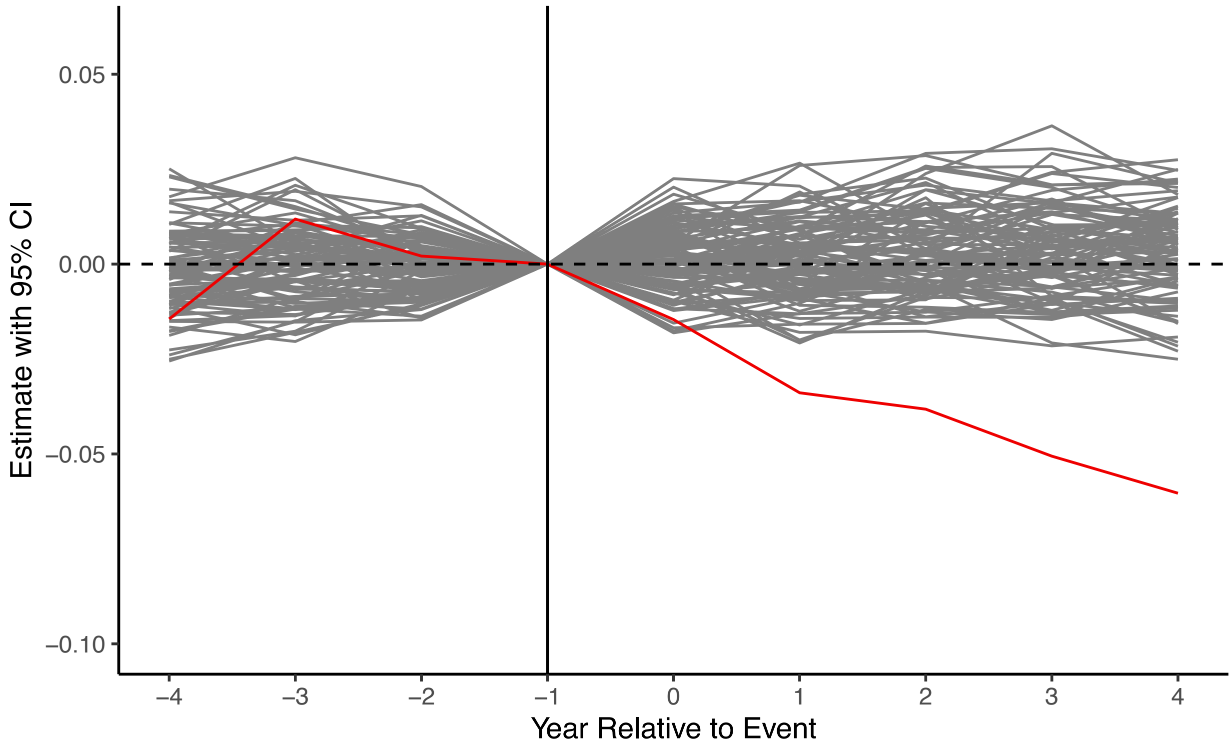
Note: This figure represents the results of equation (1.5) for log(employment), and compare it with proposed methods by Callaway and Sant'Anna (2021), Gardner (2021), and Sun and Abraham (2021). Each regression controls for region, year, relative time, and industry fixed effects. The standard errors are clustered at the region level.

Figure 1.C2: Comparison Between Main and Alternative Identification



Note: This figure represents the results of equation (1.5) for log(employment), and compare it with the alternative method in section 4.3 in which the control region is a ring (15-20km) around the event, and it is re weighted using inverse propensity weighting method. Each regression controls for region, year, relative time, and industry fixed effects. The standard errors are clustered at the region level.

Figure 1.C3: Placebo Regressions vs. Baseline Regression



Note: This figure represents the results of equation (1.5) for log(employment) (red line), and compare it with 100 regressions on randomly selected fake events (gray lines). Each regression controls for region, year, relative time, and industry fixed effects. The standard errors are clustered at the region level.

Table 1.C1: Baseline Results by Various Difference-in-differences Methods

Dependent Variables:	Employment			
Model:	(1)	(2)	(3)	(4)
τ = -4	-0.0007	-0.0020	-0.0150	-0.0150
	(0.0201)	(0.0029)	(0.0117)	(0.0117)
τ = -3	0.0032	0.0046*	0.0115	0.0108
	(0.0188)	(0.0020)	(0.0099)	(0.0098)
τ = -2	0.0036	0.0030	0.0002	0.0012
	(0.0197)	(0.0020)	(0.0075)	(0.0076)
τ = -1				
τ = 0	-0.0033	-0.0228***	-0.0113	-0.0146*
	(0.0186)	(0.0048)	(0.0073)	(0.0076)
τ = 1	-0.0222	-0.0361***	-0.0272***	-0.0343***
	(0.0191)	(0.0072)	(0.0100)	(0.0112)
τ = 2	-0.0472**	-0.0389***	-0.0271**	-0.0386***
	(0.0198)	(0.0097)	(0.0123)	(0.0144)
τ = 3	-0.0773***	-0.0379***	-0.0369**	-0.0503***
	(0.0184)	(0.0121)	(0.0159)	(0.0182)
τ = 4	-0.0712**	-0.0326**	-0.0461**	-0.0604***
	(0.0206)	(0.0148)	(0.0188)	(0.0223)
Fixed-effects				
Region	Yes	Yes	Yes	Yes
Calendar Year	Yes	Yes	Yes	Yes
Relative Time	Yes	Yes	Yes	Yes
Industry (4-digit)	Yes	Yes	Yes	Yes
Fit statistics				
Observations	738,479	738,479	738,479	738,479

Clustered (Region) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: This table presents the results of equation (1.5) for three different alternative methods dealing with staggered difference-in-differences and comparing it with the main identification. The visual representation of point estimates is in Figure 1.C1. The dependent variable is log(employment). Column (1) follows Gardner (2021), column (2) follows Callaway and Sant'Anna (2021), and column (3) follows Sun and Abraham (2021). Column (4) is the main identification result.

1.D Discussion on Spillover Effects on Housing Prices

The main focus of this paper is on studying the neighboring firms to large mass layoffs. However, the spillover effects are not limited to labor market. The people who live close-by and not necessarily work in the same area might also be affected by such a local economic shock.

One way of examining the potential effects on neighboring residents, is by estimating changes of housing prices. Housing is not just a consumption good, it is also a mean of accumulating wealth or speculation for housholds (Gao et al. 2020).

To estimate the spillover effect on housing prices, I utilize a recent dataset introduced by Contat and Larson (2022). This dataset id a balanced panel of annual housing price indexes (HPI) for single-family homes covering this study's time period. I also, use the census tract centroids from US Census Bureau to measure the spatial distance between tratcs. I use a similar approach to the alternative identification in section 4.3. The treatment area includes all census tracts which their centroids lie within 6km of the centroid of the mass layoff establishment's tract. The control area is all tracts within a 20km to 50km ring around the centroid of mass layoff establishment's tract. Finally, I reweight the control using inverse propensity weighting based on trend of pre event HPI, and estimate a modified version of equation (1.5):

$$Y_{c\tau t} = \sum_{\tau=-4}^{-2} \alpha_\tau Event_{c\tau t} + \sum_{\tau=0}^{4} \beta_\tau Event_{c\tau t} + \gamma_t + \delta_c + \lambda_\tau + \epsilon_{ir\tau t}, \tag{1.12}$$

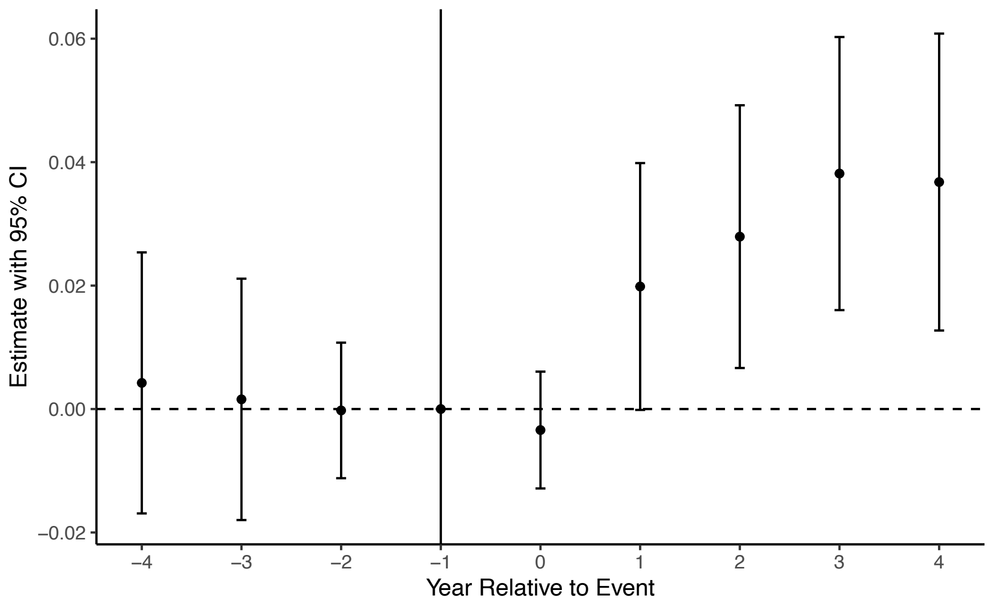
where $Y_{c\tau t}$ is HPI of census tract c at year t and relative time τ. I include census tract along with time fixed effects, and cluster standard errors at region[30] level.

Figure 1.D1 represents the event study results, indicating increase in housing prices in census tracts within 6km of the event census tract. It requires more research in the future to understand the mechanisms behind the changes in housing prices. However, the results

[30] Region here is a treatment and control area pair.

suggest that home owners near large mass layoff events benefit from them, suggesting that plant closures or substantial decrease in economic activity near residential areas increase desirability.

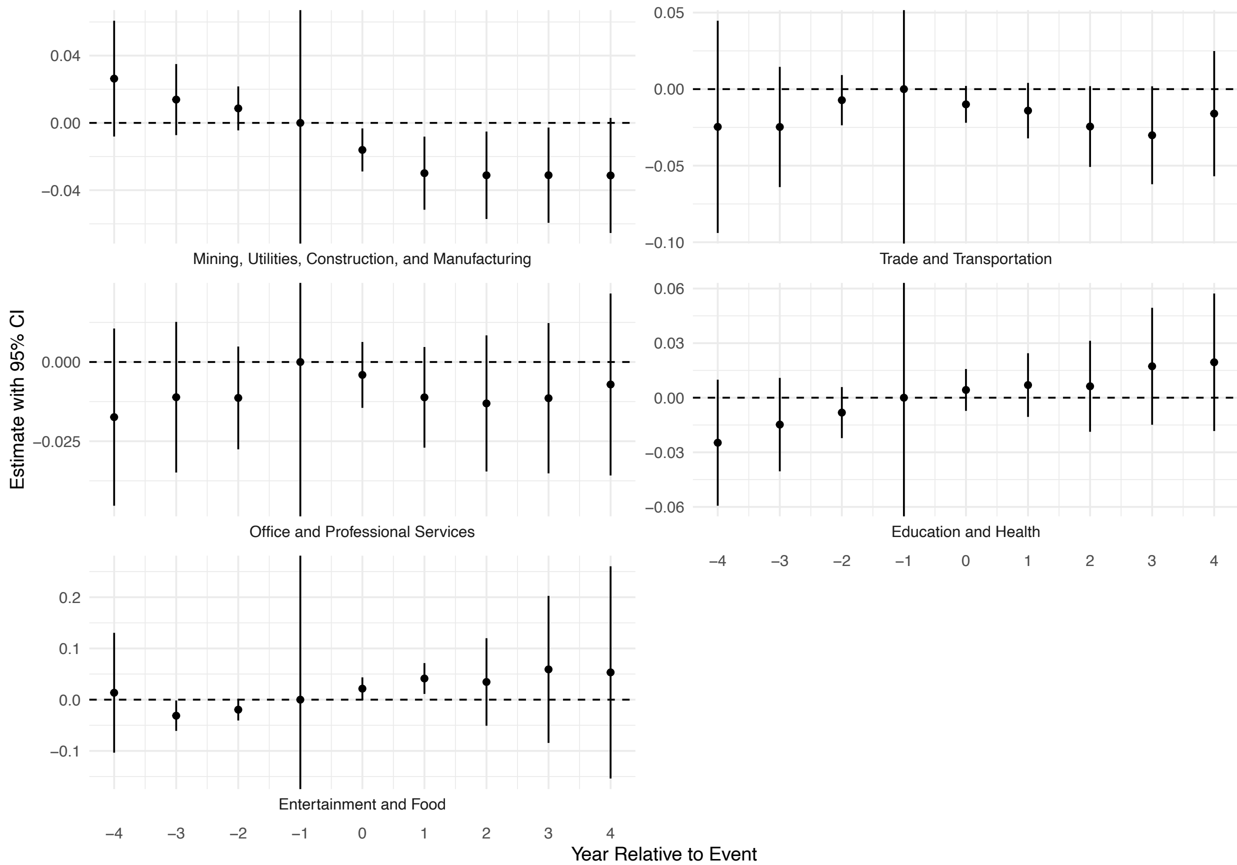
Figure 1.D1: Spillover Effects of Mass Layoffs on HPI



Note: This figure represents the results of equation (3.1) for log(HPI). Each regression controls for region, year, and relative time. The standard errors are clustered at the region level.

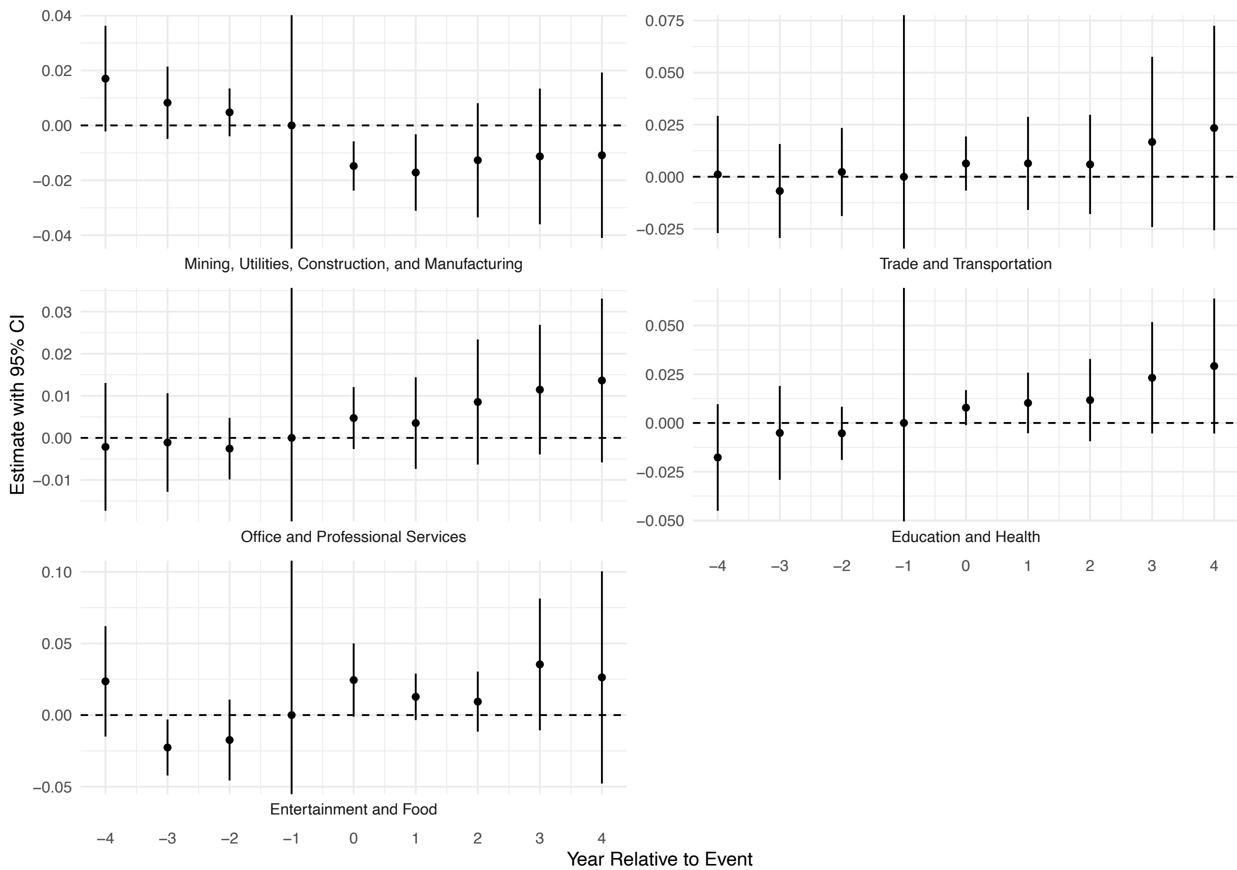
1.D Appendix: Figures and Tables

Figure 1.E1: Total Paid Earnings Spillover Effects by Industry of Event Establishments



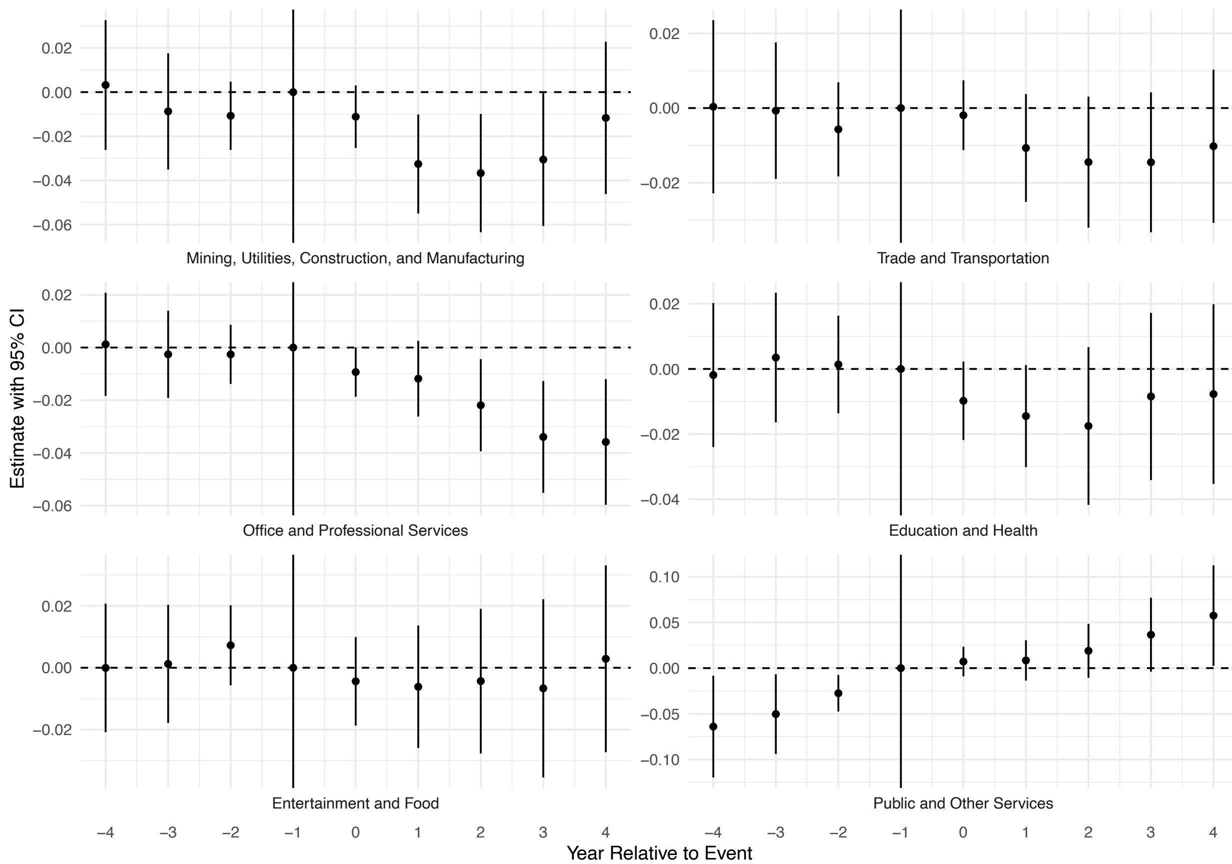
Note: This figure represents the results of equation (1.5) for log(total paid earnings) for 5 sub-samples divided by the industry of mass layoff establishments. Each regression controls for establishment ID, region, year, relative time, and industry fixed effects. The standard errors are clustered at the the region level.

Figure 1.E2: Earnings per Employee Spillover Effects by Industry of Event Establishments



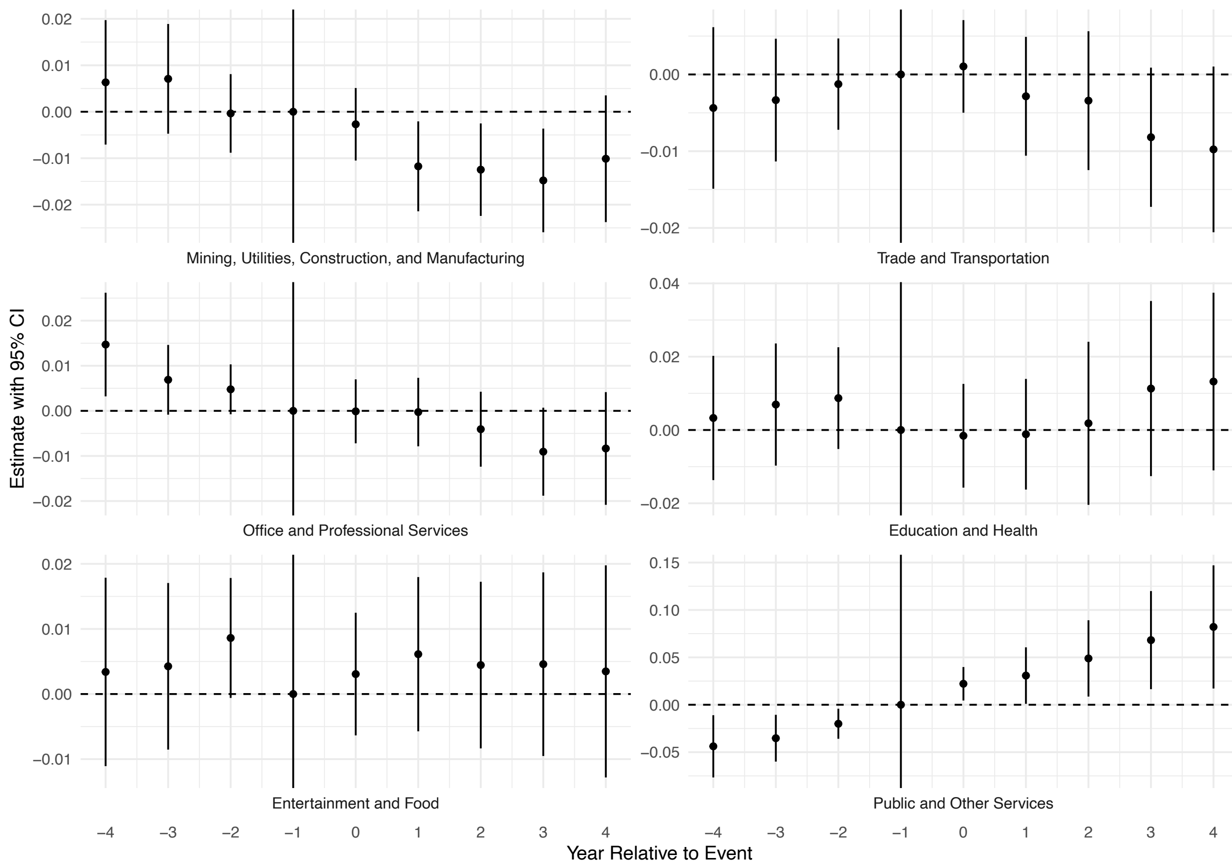
Note: This figure represents the results of equation (1.5) for log(earnings per employee) for 5 sub-samples divided by the industry of mass layoff establishments. Each regression controls for establishment ID, region, year, relative time, and industry fixed effects. The standard errors are clustered at the the region level.

Figure 1.E3: Total Paid Earnings Spillover Effects by Industry of Affected Establishments



Note: This figure represents the results of equation (1.5) for log(total paid earnings) for 6 sub-samples divided by the industry of affected establishments. Each regression controls for establishment ID, region, year, relative time, and industry fixed effects. The standard errors are clustered at the region level.

Figure 1.E4: Earnings per Employee Spillover Effects by Industry of Affected Establishments



Note: This figure represents the results of equation (1.5) for log(earnings per employee) for 6 sub-samples divided by the industry of affected establishments. Each regression controls for establishment ID, region, year, relative time, and industry fixed effects. The standard errors are clustered at the region level.

Table 1.E1: Establishment Level Results for Employment by Firm Size

Dependent Variables:		Small Firms				Medium Firms				Large Firms		
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
τ = -4	-0.0166*	-0.0170*	-0.0074	-0.0196**	-0.0174	-0.0176	-0.0084	-0.0212*	0.0006	0.0024	0.0018	0.0008
	(0.0089)	(0.0091)	(0.0053)	(0.0093)	(0.0108)	(0.0113)	(0.0079)	(0.0114)	(0.0062)	(0.0062)	(0.0062)	(0.0063)
τ = = -3	-0.0113	-0.0124*	-0.0075*	-0.0141*	-0.0104	-0.0124	-0.0056	-0.0137	0.0040	0.0049	0.0048	0.0040
	(0.0071)	(0.0073)	(0.0044)	(0.0077)	(0.0087)	(0.0089)	(0.0063)	(0.0090)	(0.0055)	(0.0056)	(0.0055)	(0.0056)
τ = -2	-0.0067	-0.0073	-0.0069**	-0.0092*	-0.0019	-0.0019	-0.0008	-0.0038	-0.0014	-0.0011	-0.0025	-0.0015
	(0.0042)	(0.0044)	(0.0032)	(0.0049)	(0.0045)	(0.0046)	(0.0036)	(0.0047)	(0.0044)	(0.0044)	(0.0044)	(0.0044)
τ = -1												
τ = 0	-0.0116***	-0.0112***	-0.0102***	-0.0155***	-0.0044	-0.0061*	-0.0030	-0.0054	-0.0017	-0.0013	-0.0016	-0.0020
	(0.0035)	(0.0038)	(0.0032)	(0.0044)	(0.0033)	(0.0033)	(0.0028)	(0.0035)	(0.0039)	(0.0040)	(0.0038)	(0.0039)
τ = 1	-0.0191***	-0.0193***	-0.0132***	-0.0244***	-0.0134**	-0.0149**	-0.0059	-0.0145**	-0.0045	-0.0034	-0.0026	-0.0050
	(0.0056)	(0.0059)	(0.0049)	(0.0067)	(0.0058)	(0.0058)	(0.0046)	(0.0059)	(0.0051)	(0.0052)	(0.0049)	(0.0051)
τ = 2	-0.0271***	-0.0272***	-0.0167***	-0.0327***	-0.0227**	-0.0239**	-0.0052	-0.0240**	-0.0088	-0.0085	-0.0053	-0.0091
	(0.0070)	(0.0073)	(0.0058)	(0.0081)	(0.0098)	(0.0101)	(0.0061)	(0.0099)	(0.0065)	(0.0067)	(0.0059)	(0.0066)
τ = 3	-0.0304***	-0.0307***	-0.0185***	-0.0361***	-0.0276**	-0.0287**	-0.0019	-0.0289**	-0.0110	-0.0112	-0.0057	-0.0112
	(0.0081)	(0.0085)	(0.0067)	(0.0091)	(0.0132)	(0.0138)	(0.0068)	(0.0131)	(0.0071)	(0.0073)	(0.0062)	(0.0072)
τ = 4	-0.0273***	-0.0279***	-0.0172**	-0.0329***	-0.0286*	-0.0309*	0.0005	-0.0299*	-0.0117	-0.0126	-0.0058	-0.0119
	(0.0089)	(0.0094)	(0.0073)	(0.0098)	(0.0159)	(0.0168)	(0.0078)	(0.0157)	(0.0089)	(0.0091)	(0.0078)	(0.0089)
Fixed-effects												
Region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relative Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry (4-digit)	Yes	Yes	Yes		Yes	Yes	Yes		Yes	Yes	Yes	
Region × Industry		Yes				Yes				Yes		
Calendar Year × Industry			Yes				Yes				Yes	
Fit statistics												
Observations	2,247,054	2,247,054	2,247,054	2,247,054	705,014	705,014	705,014	705,014	509,807	509,807	509,807	509,807
R^2	0.86205	0.86770	0.86423	0.85953	0.81604	0.82887	0.82274	0.81231	0.95783	0.96038	0.95982	0.95758

Clustered (group) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: The dependent variables for all columns are log(employment). The sample includes the surviving establishments, measuring the intensive margins. The sample for columns (1)-(4) are limited to establishments of small firms. The sample for columns (5)-(8) are limited to establishments of medium firms. The sample for columns (9)-(12) are limited to establishments of large firms.

Table 1.E2: Establishment Level Results for Total Paid Earnings by Firm Size

Dependent Variables:	Small Firms				Medium Firms				Large Firms			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
τ = -4	-0.0176	-0.0168	-0.0045	-0.0217	-0.0148	-0.0144	-0.0076	-0.0193	0.0035	0.0054	0.0049	0.0036
	(0.0142)	(0.0150)	(0.0084)	(0.0141)	(0.0123)	(0.0126)	(0.0089)	(0.0129)	(0.0077)	(0.0078)	(0.0072)	(0.0077)
τ = -3	-0.0178	-0.0170	-0.0107*	-0.0218*	-0.0106	-0.0127	-0.0031	-0.0146	0.0056	0.0064	0.0062	0.0055
	(0.0111)	(0.0118)	(0.0064)	(0.0114)	(0.0098)	(0.0098)	(0.0070)	(0.0102)	(0.0060)	(0.0061)	(0.0056)	(0.0060)
τ = -2	-0.0125*	-0.0119*	-0.0111**	-0.0160**	-0.0057	-0.0059	-0.0015	-0.0077	0.0016	0.0019	0.0015	0.0014
	(0.0064)	(0.0068)	(0.0046)	(0.0067)	(0.0049)	(0.0051)	(0.0037)	(0.0052)	(0.0047)	(0.0048)	(0.0046)	(0.0047)
τ = -1												
τ = 0	-0.0100**	-0.0096**	-0.0081**	-0.0116**	-0.0012	-0.0029	-0.0025	-0.0022	-0.0045	-0.0047	-0.0050	-0.0047
	(0.0043)	(0.0045)	(0.0035)	(0.0050)	(0.0044)	(0.0045)	(0.0033)	(0.0047)	(0.0046)	(0.0047)	(0.0042)	(0.0046)
τ = 1	-0.0174***	-0.0177***	-0.0110*	-0.0203***	-0.0150*	-0.0158**	-0.0096*	-0.0160*	-0.0083	-0.0082	-0.0066	-0.0087
	(0.0063)	(0.0067)	(0.0056)	(0.0075)	(0.0079)	(0.0079)	(0.0053)	(0.0082)	(0.0061)	(0.0062)	(0.0054)	(0.0061)
τ = 2	-0.0195**	-0.0198**	-0.0144*	-0.0234**	-0.0255**	-0.0260**	-0.0114	-0.0270**	-0.0140*	-0.0145*	-0.0109*	-0.0143*
	(0.0085)	(0.0089)	(0.0076)	(0.0097)	(0.0104)	(0.0105)	(0.0073)	(0.0106)	(0.0072)	(0.0074)	(0.0064)	(0.0073)
τ = 3	-0.0145	-0.0163	-0.0159*	-0.0194	-0.0310***	-0.0315***	-0.0112	-0.0325***	-0.0169**	-0.0178**	-0.0121*	-0.0170**
	(0.0102)	(0.0110)	(0.0085)	(0.0119)	(0.0109)	(0.0112)	(0.0083)	(0.0110)	(0.0073)	(0.0074)	(0.0065)	(0.0073)
τ = 4	-0.0059	-0.0069	-0.0107	-0.0093	-0.0325**	-0.0342**	-0.0080	-0.0339**	-0.0192**	-0.0213**	-0.0138*	-0.0193**
	(0.0121)	(0.0131)	(0.0098)	(0.0138)	(0.0147)	(0.0152)	(0.0097)	(0.0146)	(0.0088)	(0.0092)	(0.0080)	(0.0088)
Fixed-effects												
Region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relative Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry (4-digit)	Yes	Yes	Yes		Yes	Yes	Yes		Yes	Yes	Yes	
Region × Industry		Yes				Yes				Yes		
Calendar Year × Industry			Yes				Yes				Yes	
Fit statistics												
Observations	2,293,575	2,293,575	2,293,575	2,293,575	705,314	705,314	705,314	705,314	510,043	510,043	510,043	510,043
R^2	0.92685	0.92988	0.92936	0.92564	0.88724	0.89536	0.89192	0.88456	0.96467	0.96673	0.96671	0.96447

Clustered (group) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: The dependent variables for all columns are log(total paid earnings). The sample includes the surviving establishments, measuring the intensive margins. The sample for columns (1)-(4) are limited to establishments of small firms. The sample for columns (5)-(8) are limited to establishments of medium firms. The sample for columns (9)-(12) are limited to establishments of large firms.

Table 1.E3: Establishment Level Results for Earnings per Employee by Firm Size

Dependent Variables:	Small Firms				Medium Firms				Large Firms			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
τ = -4	-0.0038	-0.0021	-0.0001	-0.0047	0.0037	0.0041	0.0023	0.0034	0.0033	0.0033	0.0032	0.0032
	(0.0072)	(0.0076)	(0.0057)	(0.0068)	(0.0057)	(0.0059)	(0.0036)	(0.0057)	(0.0041)	(0.0043)	(0.0035)	(0.0042)
τ = -3	-0.0050	-0.0028	-0.0022	-0.0057	0.0001	0.0006	0.0028	-0.0002	0.0012	0.0012	0.0005	0.0011
	(0.0057)	(0.0060)	(0.0040)	(0.0055)	(0.0050)	(0.0052)	(0.0029)	(0.0050)	(0.0031)	(0.0033)	(0.0029)	(0.0031)
τ = -2	-0.0035	-0.0015	-0.0029	-0.0049	-0.0022	-0.0024	0.0010	-0.0024	0.0016	0.0016	0.0022	0.0015
	(0.0039)	(0.0042)	(0.0030)	(0.0036)	(0.0024)	(0.0024)	(0.0019)	(0.0024)	(0.0028)	(0.0029)	(0.0027)	(0.0028)
τ = -1												
τ = 0	0.0017	0.0023	0.0017	0.0041	0.0030	0.0033	0.0004	0.0031	-0.0026	-0.0033	-0.0031	-0.0025
	(0.0037)	(0.0043)	(0.0026)	(0.0035)	(0.0029)	(0.0029)	(0.0018)	(0.0029)	(0.0026)	(0.0026)	(0.0024)	(0.0025)
τ = 1	0.0014	0.0020	0.0016	0.0041	-0.0016	-0.0007	-0.0037	-0.0016	-0.0037	-0.0045	-0.0038	-0.0036
	(0.0050)	(0.0057)	(0.0035)	(0.0052)	(0.0045)	(0.0045)	(0.0024)	(0.0045)	(0.0030)	(0.0031)	(0.0029)	(0.0030)
τ = 2	0.0073	0.0077	0.0017	0.0094	-0.0029	-0.0019	-0.0064**	-0.0030	-0.0051	-0.0059	-0.0055	-0.0051
	(0.0069)	(0.0074)	(0.0041)	(0.0073)	(0.0051)	(0.0052)	(0.0031)	(0.0051)	(0.0035)	(0.0037)	(0.0034)	(0.0035)
τ = 3	0.0158*	0.0150	0.0021	0.0172*	-0.0033	-0.0025	-0.0092**	-0.0034	-0.0057	-0.0065	-0.0062	-0.0057
	(0.0087)	(0.0093)	(0.0046)	(0.0096)	(0.0064)	(0.0067)	(0.0037)	(0.0064)	(0.0041)	(0.0043)	(0.0038)	(0.0041)
τ = 4	0.0211**	0.0214*	0.0061	0.0237**	-0.0041	-0.0033	-0.0086**	-0.0042	-0.0074	-0.0086*	-0.0079*	-0.0074
	(0.0107)	(0.0113)	(0.0058)	(0.0116)	(0.0087)	(0.0090)	(0.0043)	(0.0086)	(0.0048)	(0.0050)	(0.0044)	(0.0048)
Fit statistics												
Observations	2,246,292	2,246,292	2,246,292	2,246,292	705,000	705,000	705,000	705,000	509,804	509,804	509,804	509,804
R^2	0.91075	0.91339	0.91405	0.91041	0.93440	0.93790	0.93601	0.93428	0.94269	0.94483	0.94616	0.94250

Clustered (group) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: The dependent variables for all columns are log(earnings per employee). The sample includes the surviving establishments, measuring the intensive margins. The sample for columns (1)-(4) are limited to establishments of small firms. The sample for columns (5)-(8) are limited to establishments of medium firms. The sample for columns (9)-(12) are limited to establishments of large firms.

Table 1.E4: Establishment Level Results for Employment by Firm Age

Dependent Variables:	Young Firms				Medium Age Firms				Old Firms			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
τ = -4	-0.0071	-0.0045	-0.0073	-0.0067	-0.0023	-0.0046	0.0013	-0.0025	0.0044	0.0048	0.0060	0.0046
	(0.0104)	(0.0107)	(0.0071)	(0.0118)	(0.0075)	(0.0077)	(0.0063)	(0.0074)	(0.0059)	(0.0060)	(0.0044)	(0.0059)
τ = -3	-0.0064	-0.0052	-0.0064	-0.0075	0.0003	-0.0020	0.0037	0.0003	0.0015	0.0014	0.0013	0.0016
	(0.0096)	(0.0099)	(0.0065)	(0.0107)	(0.0062)	(0.0062)	(0.0055)	(0.0062)	(0.0048)	(0.0051)	(0.0031)	(0.0048)
τ = -2	-0.0081	-0.0070	-0.0067	-0.0095	0.0003	-0.0016	0.0025	0.0003	0.0007	0.0007	-0.0014	0.0005
	(0.0064)	(0.0066)	(0.0046)	(0.0070)	(0.0043)	(0.0043)	(0.0039)	(0.0043)	(0.0034)	(0.0035)	(0.0020)	(0.0033)
τ = -1												
τ = 0	-0.0095	-0.0101	-0.0074	-0.0142*	-0.0011	-0.0007	-0.0023	-0.0012	-0.0050**	-0.0052**	-0.0002	-0.0042*
	(0.0061)	(0.0069)	(0.0050)	(0.0072)	(0.0032)	(0.0034)	(0.0030)	(0.0032)	(0.0024)	(0.0023)	(0.0020)	(0.0023)
τ = 1	-0.0196**	-0.0218**	-0.0116*	-0.0242**	-0.0029	-0.0021	-0.0033	-0.0033	-0.0092**	-0.0095**	0.0014	-0.0083*
	(0.0094)	(0.0103)	(0.0070)	(0.0106)	(0.0042)	(0.0045)	(0.0041)	(0.0043)	(0.0044)	(0.0044)	(0.0033)	(0.0044)
τ = 2	-0.0278**	-0.0302**	-0.0131	-0.0330**	-0.0104**	-0.0094*	-0.0067	-0.0110**	-0.0151**	-0.0153**	0.0018	-0.0144**
	(0.0121)	(0.0129)	(0.0086)	(0.0133)	(0.0049)	(0.0051)	(0.0051)	(0.0049)	(0.0065)	(0.0065)	(0.0040)	(0.0065)
τ = 3	-0.0331**	-0.0364**	-0.0153	-0.0376**	-0.0101*	-0.0090	-0.0051	-0.0108*	-0.0194**	-0.0199**	0.0015	-0.0187**
	(0.0146)	(0.0155)	(0.0105)	(0.0158)	(0.0055)	(0.0058)	(0.0059)	(0.0055)	(0.0082)	(0.0085)	(0.0046)	(0.0083)
τ = 4	-0.0289*	-0.0345*	-0.0130	-0.0336*	-0.0060	-0.0044	-0.0027	-0.0067	-0.0169*	-0.0181*	0.0039	-0.0161
	(0.0166)	(0.0175)	(0.0131)	(0.0174)	(0.0069)	(0.0072)	(0.0072)	(0.0070)	(0.0101)	(0.0104)	(0.0051)	(0.0101)
Fixed-effects												
Region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relative Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry (4-digit)	Yes	Yes	Yes		Yes	Yes	Yes		Yes	Yes	Yes	
Region × Industry		Yes				Yes				Yes		
Calendar Year × Industry			Yes				Yes				Yes	
Fit statistics												
Observations	1,013,560	1,013,560	1,013,560	1,013,560	822,368	822,368	822,368	822,368	1,523,918	1,523,918	1,523,918	1,523,918
R^2	0.95827	0.96094	0.95984	0.95711	0.97944	0.98007	0.98004	0.97939	0.96416	0.96570	0.96521	0.96409

Clustered (group) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: The dependent variables for all columns are log(employment). The sample includes the surviving establishments, measuring the intensive margins. The sample for columns (1)-(4) are limited to establishments of young firms. The sample for columns (5)-(8) are limited to establishments of medium age firms. The sample for columns (9)-(12) are limited to establishments of old firms.

Table 1.E5: Establishment Level Results for Total Paid Earnings by Firm Age

Dependent Variables:	Young Firms				Medium Age Firms				Old Firms			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
τ = -4	-0.0249*	-0.0254	-0.0175	-0.0233	-0.0062	-0.0059	-0.0040	-0.0068	0.0134*	0.0147**	0.0124**	0.0135*
	(0.0150)	(0.0159)	(0.0112)	(0.0170)	(0.0125)	(0.0132)	(0.0091)	(0.0123)	(0.0071)	(0.0074)	(0.0057)	(0.0070)
τ = -3	-0.0216*	-0.0229*	-0.0157*	-0.0221	-0.0059	-0.0048	-0.0015	-0.0063	0.0065	0.0075	0.0068*	0.0064
	(0.0127)	(0.0134)	(0.0093)	(0.0146)	(0.0107)	(0.0114)	(0.0076)	(0.0106)	(0.0053)	(0.0057)	(0.0040)	(0.0052)
τ = -2	-0.0166**	-0.0165**	-0.0113*	-0.0174*	-0.0054	-0.0051	-0.0030	-0.0058	0.0013	0.0024	0.0003	0.0010
	(0.0078)	(0.0081)	(0.0065)	(0.0089)	(0.0069)	(0.0074)	(0.0054)	(0.0068)	(0.0041)	(0.0045)	(0.0030)	(0.0040)
τ = -1												
τ = 0	-0.0040	-0.0045	-0.0044	-0.0083	0.0027	0.0019	-0.0008	0.0031	-0.0038	-0.0044	0.0017	-0.0026
	(0.0056)	(0.0061)	(0.0048)	(0.0071)	(0.0046)	(0.0050)	(0.0038)	(0.0047)	(0.0033)	(0.0034)	(0.0024)	(0.0031)
τ = 1	-0.0110	-0.0127	-0.0082	-0.0152	0.0069	0.0059	0.0025	0.0072	-0.0132**	-0.0141**	0.0010	-0.0118**
	(0.0086)	(0.0093)	(0.0078)	(0.0102)	(0.0071)	(0.0076)	(0.0053)	(0.0071)	(0.0054)	(0.0054)	(0.0038)	(0.0054)
τ = 2	-0.0075	-0.0096	-0.0063	-0.0125	0.0047	0.0040	-0.0005	0.0047	-0.0175**	-0.0185**	0.0021	-0.0165**
	(0.0104)	(0.0111)	(0.0104)	(0.0114)	(0.0084)	(0.0089)	(0.0068)	(0.0085)	(0.0073)	(0.0073)	(0.0050)	(0.0073)
τ = 3	-0.0046	-0.0075	-0.0097	-0.0089	0.0128	0.0116	0.0033	0.0126	-0.0183**	-0.0200**	0.0007	-0.0173**
	(0.0114)	(0.0119)	(0.0121)	(0.0122)	(0.0097)	(0.0103)	(0.0072)	(0.0099)	(0.0080)	(0.0081)	(0.0056)	(0.0081)
τ = 4	0.0018	-0.0025	-0.0137	-0.0023	0.0241*	0.0246*	0.0117	0.0240*	-0.0134	-0.0151	0.0052	-0.0122
	(0.0154)	(0.0160)	(0.0148)	(0.0150)	(0.0129)	(0.0137)	(0.0092)	(0.0131)	(0.0099)	(0.0101)	(0.0063)	(0.0100)
Fixed-effects												
Region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relative Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry (4-digit)	Yes	Yes	Yes		Yes	Yes	Yes		Yes	Yes	Yes	
Region × Industry		Yes				Yes				Yes		
Calendar Year × Industry			Yes				Yes				Yes	
Fit statistics												
Observations	1,043,850	1,043,850	1,043,850	1,043,850	827,273	827,273	827,273	827,273	1,526,495	1,526,495	1,526,495	1,526,495
R^2	0.96771	0.96985	0.96862	0.96696	0.98468	0.98509	0.98532	0.98466	0.96832	0.96962	0.96965	0.96824

Clustered (group) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: The dependent variables for all columns are log(total paid earnings). The sample includes the surviving establishments, measuring the intensive margins. The sample for columns (1)-(4) are limited to establishments of young firms. The sample for columns (5)-(8) are limited to establishments of medium age firms. The sample for columns (9)-(12) are limited to establishments of old firms.

Table 1.E6: Establishment Level Results for Earnings per Employee by Firm Age

Dependent Variables:	Young Firms				Medium Age Firms				Old Firms			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
τ = -4	-0.0169	-0.0194	-0.0086	-0.0172	-0.0035	-0.0012	-0.0043	-0.0039	0.0086*	0.0092*	0.0054	0.0087*
	(0.0109)	(0.0119)	(0.0070)	(0.0108)	(0.0079)	(0.0089)	(0.0067)	(0.0079)	(0.0046)	(0.0050)	(0.0038)	(0.0047)
τ = -3	-0.0135*	-0.0149*	-0.0068	-0.0140*	-0.0029	-0.0004	-0.0018	-0.0032	0.0045	0.0054	0.0047*	0.0046
	(0.0081)	(0.0088)	(0.0054)	(0.0081)	(0.0072)	(0.0078)	(0.0055)	(0.0072)	(0.0044)	(0.0046)	(0.0026)	(0.0044)
τ = -2	-0.0054	-0.0053	-0.0013	-0.0059	-0.0038	-0.0021	-0.0034	-0.0041	0.0007	0.0018	0.0016	0.0007
	(0.0048)	(0.0051)	(0.0038)	(0.0047)	(0.0048)	(0.0051)	(0.0041)	(0.0049)	(0.0028)	(0.0031)	(0.0022)	(0.0028)
τ = -1												
τ = 0	0.0042	0.0052	0.0023	0.0054	0.0039	0.0025	0.0016	0.0043	0.0012	0.0009	0.0019	0.0016
	(0.0046)	(0.0052)	(0.0034)	(0.0047)	(0.0032)	(0.0035)	(0.0029)	(0.0034)	(0.0023)	(0.0024)	(0.0016)	(0.0022)
τ = 1	0.0068	0.0088	0.0021	0.0080	0.0096*	0.0076	0.0057	0.0101*	-0.0039	-0.0045	-0.0004	-0.0035
	(0.0067)	(0.0077)	(0.0045)	(0.0068)	(0.0056)	(0.0060)	(0.0042)	(0.0056)	(0.0031)	(0.0031)	(0.0022)	(0.0030)
τ = 2	0.0184*	0.0201*	0.0050	0.0194**	0.0151**	0.0132*	0.0065	0.0156**	-0.0023	-0.0030	0.0003	-0.0020
	(0.0094)	(0.0108)	(0.0055)	(0.0096)	(0.0072)	(0.0077)	(0.0049)	(0.0073)	(0.0039)	(0.0040)	(0.0027)	(0.0039)
τ = 3	0.0271**	0.0292**	0.0042	0.0282**	0.0233**	0.0207**	0.0089	0.0237**	0.0013	0.0001	-0.0006	0.0017
	(0.0111)	(0.0127)	(0.0058)	(0.0114)	(0.0091)	(0.0094)	(0.0056)	(0.0093)	(0.0041)	(0.0043)	(0.0029)	(0.0042)
τ = 4	0.0293**	0.0326**	-0.0013	0.0308**	0.0305***	0.0291**	0.0151**	0.0309***	0.0036	0.0030	0.0013	0.0040
	(0.0134)	(0.0153)	(0.0070)	(0.0136)	(0.0113)	(0.0116)	(0.0066)	(0.0115)	(0.0055)	(0.0058)	(0.0036)	(0.0056)
Fit statistics												
Observations	1,013,242	1,013,242	1,013,242	1,013,242	822,201	822,201	822,201	822,201	1,523,677	1,523,677	1,523,677	1,523,677
R^2	0.94231	0.94536	0.94418	0.94221	0.96260	0.96333	0.96384	0.96257	0.92548	0.92734	0.92719	0.92542

Clustered (group) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: The dependent variables for all columns are log(earnings per employee). The sample includes the surviving establishments, measuring the intensive margins. The sample for columns (1)-(4) are limited to establishments of young firms. The sample for columns (5)-(8) are limited to establishments of medium age firms. The sample for columns (9)-(12) are limited to establishments of old firms.

Table 1.E7: Establishment Level Results for Employment by Number of Establishments

Dependent Variables:	Single Establishment Firms				Multi Establishment Firms			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
τ = -4	-0.0143*	-0.0148*	-0.0065	-0.0173**	0.0075	0.0082	0.0070	0.0075
	(0.0074)	(0.0075)	(0.0045)	(0.0078)	(0.0064)	(0.0065)	(0.0060)	(0.0064)
τ = -3	-0.0089	-0.0103	-0.0058	-0.0117*	0.0073	0.0077	0.0074	0.0074
	(0.0064)	(0.0066)	(0.0039)	(0.0069)	(0.0053)	(0.0055)	(0.0052)	(0.0054)
τ = -2	-0.0042	-0.0049	-0.0045	-0.0064	-0.0009	-0.0008	-0.0010	-0.0009
	(0.0037)	(0.0039)	(0.0028)	(0.0043)	(0.0041)	(0.0041)	(0.0041)	(0.0041)
τ = -1								
τ = 0	-0.0093***	-0.0092***	-0.0080***	-0.0125***	-0.0018	-0.0015	-0.0023	-0.0018
	(0.0028)	(0.0030)	(0.0027)	(0.0037)	(0.0038)	(0.0039)	(0.0037)	(0.0038)
τ = 1	-0.0166***	-0.0169***	-0.0108***	-0.0209***	-0.0051	-0.0042	-0.0050	-0.0052
	(0.0045)	(0.0047)	(0.0041)	(0.0055)	(0.0048)	(0.0050)	(0.0047)	(0.0049)
τ = 2	-0.0243***	-0.0244***	-0.0137***	-0.0290***	-0.0083	-0.0076	-0.0073	-0.0082
	(0.0060)	(0.0062)	(0.0052)	(0.0069)	(0.0061)	(0.0062)	(0.0057)	(0.0061)
τ = 3	-0.0268***	-0.0274***	-0.0142**	-0.0316***	-0.0118*	-0.0117*	-0.0096	-0.0116*
	(0.0072)	(0.0076)	(0.0060)	(0.0079)	(0.0064)	(0.0065)	(0.0059)	(0.0064)
τ = 4	-0.0232***	-0.0246***	-0.0122*	-0.0279***	-0.0130*	-0.0132*	-0.0101	-0.0129*
	(0.0081)	(0.0086)	(0.0066)	(0.0087)	(0.0077)	(0.0079)	(0.0071)	(0.0078)
Fixed-effects								
Region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relative Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry (4-digit)	Yes	Yes	Yes		Yes	Yes	Yes	
Region × Industry		Yes				Yes		
Calendar Year × Industry			Yes				Yes	
Fit statistics								
Observations	2,926,528	2,926,528	2,926,528	2,926,528	535,347	535,347	535,347	535,347
R^2	0.94094	0.94336	0.94201	0.93998	0.95372	0.95625	0.95577	0.95350

Clustered (group) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: The dependent variables for all columns are log(employment). The sample includes the surviving establishments, measuring the intensive margins. The sample for columns (1)-(4) are limited to single establishment firms. The sample for columns (5)-(8) are limited to multi establishment firms.

Table 1.E8: Establishment Level Results for Total Paid Earnings by Number of Establishments (Single vs. Multiple)

Dependent Variables:	Single Establishment Firms				Multi Establishment Firms			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
τ = -4	-0.0157	-0.0155	-0.0040	-0.0196	0.0106	0.0116	0.0101	0.0105
	(0.0120)	(0.0126)	(0.0072)	(0.0120)	(0.0077)	(0.0077)	(0.0071)	(0.0077)
τ = -3	-0.0150	-0.0153	-0.0078	-0.0189*	0.0101*	0.0105*	0.0097*	0.0099
	(0.0097)	(0.0102)	(0.0055)	(0.0099)	(0.0060)	(0.0061)	(0.0056)	(0.0060)
τ = -2	-0.0101*	-0.0099	-0.0081**	-0.0133**	0.0014	0.0016	0.0016	0.0014
	(0.0057)	(0.0061)	(0.0039)	(0.0059)	(0.0047)	(0.0047)	(0.0046)	(0.0048)
τ = -1								
τ = 0	-0.0071*	-0.0071*	-0.0062**	-0.0086*	-0.0052	-0.0056	-0.0064	-0.0051
	(0.0039)	(0.0041)	(0.0029)	(0.0047)	(0.0043)	(0.0044)	(0.0040)	(0.0043)
τ = 1	-0.0153***	-0.0156**	-0.0098**	-0.0176**	-0.0106*	-0.0103*	-0.0105**	-0.0105*
	(0.0057)	(0.0061)	(0.0049)	(0.0070)	(0.0058)	(0.0059)	(0.0053)	(0.0059)
τ = 2	-0.0180**	-0.0183**	-0.0129*	-0.0214**	-0.0159**	-0.0157**	-0.0151**	-0.0157**
	(0.0075)	(0.0079)	(0.0068)	(0.0088)	(0.0069)	(0.0070)	(0.0063)	(0.0069)
τ = 3	-0.0139	-0.0157	-0.0139*	-0.0180*	-0.0208***	-0.0213***	-0.0186***	-0.0203***
	(0.0088)	(0.0095)	(0.0077)	(0.0104)	(0.0066)	(0.0066)	(0.0061)	(0.0066)
τ = 4	-0.0059	-0.0075	-0.0088	-0.0091	-0.0248***	-0.0259***	-0.0214***	-0.0244***
	(0.0106)	(0.0114)	(0.0086)	(0.0120)	(0.0075)	(0.0077)	(0.0071)	(0.0076)
Fixed-effects								
Region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relative Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry (4-digit)	Yes	Yes	Yes		Yes	Yes	Yes	
Region × Industry		Yes				Yes		
Calendar Year × Industry			Yes				Yes	
Fit statistics								
Observations	2,973,323	2,973,323	2,973,323	2,973,323	535,609	535,609	535,609	535,609
R^2	0.95480	0.95657	0.95635	0.95407	0.95961	0.96184	0.96181	0.95943

Clustered (group) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: The dependent variables for all columns are log(total paid earnings). The sample includes the surviving establishments, measuring the intensive margins. The sample for columns (1)-(4) are limited to single establishment firms. The sample for columns (5)-(8) are limited to multi establishment firms.

Table 1.E9: Establishment Level Results for Earnings per Employee by Number of Establishments (Single vs. Multiple)

Dependent Variables:	Single Establishment Firms				Multi Establishment Firms			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
τ = -4	-0.0020	-0.0009	0.0007	-0.0027	0.0029	0.0028	0.0029	0.0029
	(0.0061)	(0.0064)	(0.0046)	(0.0058)	(0.0042)	(0.0043)	(0.0038)	(0.0042)
τ = -3	-0.0040	-0.0027	-0.0010	-0.0046	0.0023	0.0023	0.0015	0.0021
	(0.0050)	(0.0053)	(0.0033)	(0.0048)	(0.0031)	(0.0032)	(0.0029)	(0.0031)
τ = -2	-0.0032	-0.0017	-0.0020	-0.0044	0.0010	0.0011	0.0009	0.0008
	(0.0033)	(0.0035)	(0.0024)	(0.0030)	(0.0025)	(0.0025)	(0.0025)	(0.0025)
τ = -1								
τ = 0	0.0023	0.0027	0.0016	0.0041	-0.0033	-0.0039*	-0.0039*	-0.0032
	(0.0032)	(0.0037)	(0.0020)	(0.0030)	(0.0023)	(0.0023)	(0.0022)	(0.0023)
τ = 1	0.0011	0.0016	0.0007	0.0032	-0.0053*	-0.0059**	-0.0055*	-0.0052*
	(0.0045)	(0.0050)	(0.0027)	(0.0046)	(0.0029)	(0.0030)	(0.0028)	(0.0029)
τ = 2	0.0060	0.0064	0.0005	0.0076	-0.0076**	-0.0080**	-0.0077**	-0.0075**
	(0.0062)	(0.0066)	(0.0033)	(0.0064)	(0.0035)	(0.0036)	(0.0034)	(0.0035)
τ = 3	0.0128*	0.0122	9.15×10^{-5}	0.0139*	-0.0089**	-0.0094**	-0.0088**	-0.0087**
	(0.0077)	(0.0082)	(0.0037)	(0.0083)	(0.0037)	(0.0038)	(0.0036)	(0.0037)
τ = 4	0.0170*	0.0172*	0.0032	0.0188*	-0.0117***	-0.0126***	-0.0111***	-0.0114***
	(0.0094)	(0.0100)	(0.0047)	(0.0101)	(0.0043)	(0.0044)	(0.0041)	(0.0043)
Fixed-effects								
Region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relative Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry (4-digit)	Yes	Yes	Yes		Yes	Yes	Yes	
Region × Industry		Yes				Yes		
Calendar Year × Industry			Yes				Yes	
Fit statistics								
Observations	2,925,752	2,925,752	2,925,752	2,925,752	535,344	535,344	535,344	535,344
R^2	0.91855	0.92072	0.92136	0.91826	0.93874	0.94106	0.94204	0.93855

Clustered (group) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: The dependent variables for all columns are log(earnings per employee). The sample includes the surviving establishments, measuring the intensive margins. The sample for columns (1)-(4) are limited to single establishment firms. The sample for columns (5)-(8) are limited to multi establishment firms.

Chapter 2

Disparities in Access to Unemployment Insurance During the COVID-19 Pandemic: Lessons from U.S. and California Claims Data (with Alex Bell, T.J. Hedin, Peter Mannino, Geoffrey Schnorr, and Till von Wachter)

2.1 Introduction

The Unemployment Insurance (UI) system is a key part of the US social safety net. It provides assistance to unemployed workers, and becomes increasingly important during recessions when the number of jobless workers and the time they spend unemployed increase.

Through UI, workers who lose their jobs can receive weekly payments that replace part of their lost income and assistance in finding a new job. However, the program can be difficult to access and unemployed workers frequently do not receive benefits. For example, before the pandemic the share of all unemployed workers that received UI was only around 20% on average across states. Even among workers who filed for UI before the pandemic, nearly a quarter never received benefits (either because they were denied benefits or quickly found re-employment) in California.

Researchers have studied the disparate impacts of both formal and informal barriers to access on different types of workers during periods before the COVID-19 pandemic (Blank and Card 1991; Anderson and Meyer 1997). For example, there are formal eligibility rules that require workers to have earned a minimum level of income to qualify for the program. There are also informal administrative burdens that prevent otherwise eligible workers from receiving benefits such as language or technological barriers. These formal and informal hurdles can represent particular barriers for workers from disadvantaged backgrounds (O'Leary et al. 2021; Shaefer 2010).

The unprecedented surge in job losses and UI claims during the COVID-19 pandemic, and the surge in unemployment among lower-wage workers from sectors directly affected by the pandemic, refocused these long-standing concerns about equity and access to the UI system.[1] In response to the pandemic, states eased certain formal eligibility rules, such as job search requirements, that could improve access for some workers, but public health orders that closed government offices could exacerbate the informal barriers to access for others. Additionally, federal policy makers created new programs that increased the duration and generosity of UI benefits, which could have affected workers differently.

This paper makes three contributions toward measuring disparities in access to UI during the COVID-19 pandemic. First, we introduce a broader conceptual framework to track a jobless worker's access to UI benefits across the main stages in the lifecycle of a potential

[1]See, for instance, January 21, 2021, Executive Order On Advancing Racial Equity and Support for Underserved Communities Through the Federal Government.

UI claim. Second, we use publicly available UI claims data and confidential administrative claims data from California to build and refine measures for each of our four stages of access across states and at more local levels within California. Third, we use these measures to document key patterns of community-level disparities in access to UI during the pandemic by correlating them with state- and county-level attributes reflecting policy regimes and socio-economic characteristics among others.

We find that on average access to UI increased substantially during the pandemic, but that there were large differences in access across states and demographic groups. During the pandemic, the share of unemployed workers receiving UI (called the recipiency rate) reached 60% on average across the US, up from around 20% before the pandemic. However, the pandemic also saw substantial variation in recipiency rates across states ranging from over 90% in California to less than 25% in Florida. We also find that states with higher average incomes and lower Black population shares have higher recipiency rates and that states with more generous UI policies, such as Alternate Base Periods and longer Potential Benefit Durations have higher recipiency rates. The correlation between policy and access indicates that states may have a great deal of discretion in how generous they make access to UI, and that state UI programs could support a larger share of unemployed workers if the state chose to.

We find similar demographic patterns within California where counties with higher incomes saw higher recipiency rates and counties with more Black and Hispanic residents had lower recipiency rates. We provide additional evidence on differences in access across the three other stages of access described in our conceptual framework, but they are broadly consistent with the recipiency rate findings where more advantaged groups have higher access and states with more generous policies have greater access. Despite the disparities in access, the overall increase in recipiency rates in our results and the poverty reduction benefits found in Bitler et al. (this issue) indicates that the UI system responded well to the challenges of the pandemic and effectively provided support to many distressed workers.

To perform this analysis, we utilize public data from the U.S. Department of Labor (DOL) Employment and Training Administration and the Current Population Survey (CPS) as well as our team's unique access to California's UI claims micro data, facilitated by a partnership with the state's Employment Development Department (EDD). We combine these data with detailed demographic, labor market, and public health characteristics across states for the entire U.S. and at the county level in California. We also collected information on state-level differences in the UI programs and states' tax and benefit systems.

The outline for the remainder of this paper is as follows. The next section provides background on the UI program during the pandemic and presents our conceptual framework, the following section describes our methodological approach in detail, and the subsequent section provides descriptive statistics. The next four sections present the results of our analysis of access to UI when applied to rates of recipiency, applications, first payments, and exhaustions, respectively. The final section concludes with a discussion of additional areas for research.

2.2 UI System During the Pandemic and Conceptual Framework

2.2.1 Background on the UI System and the Pandemic

In the US, the UI system is operated by the states within a federal framework. As a result, states can differ in eligibility requirements or benefit generosity. In general, if a worker loses their job through no fault of their own and has earned a minimum level of income (known as the monetary eligibility limit), in a certain base period, they are eligible to receive payments that replace a portion of their previous income (their weekly benefit amount or WBA) for a certain number of weeks (their potential benefit duration or PBD). Some restrictions are universal across programs, for example self-employed workers and undocumented workers are not eligible for UI in any state. Additionally, all states have work search rules that require claimants to prove they are searching for work for each week that they receive benefits.

However, many other aspects of the program are different across states. Eligibility for the UI program can vary on four attributes. First, there can be differences in the minimum amount of income a worker had to earn to be eligible for the program (the monetary eligibility limit). Second, there can be differences in the type of employment covered by the UI system, for example the treatment of agricultural workers differs across states. Third, the types of transitions to unemployment that are covered by UI can differ across states, for example in some states a worker who quit their job to move to the state for their spouses job can be eligible for UI. Fourth, once a worker enters the UI system, the amount of work search activities they have to do to maintain eligibility can vary by state. Finally, as is the case with other social insurance programs, states' UI programs also differ in ways that are less easily quantified but that can influence accessibility, including technology, staffing levels, and internal procedures. In addition to differences in eligibility criteria, other characteristics of the program such as the maximum WBA or the total PBD differ across states and may influence which workers apply to UI.[2]

California (CA) provides a useful example of how the UI system operates. First, a worker had to be in a job that is covered by the UI system, meaning they are not self-employed (small business owners) or contractors (Uber drivers), and they had to be working legally (are not undocumented immigrants). They had to lose their job through no fault of their own, which means they could not quit their job or be fired for cause. As noted previously, the details of who is eligible based on the type of employment and how they lost their job can be different in California than in other states.

Along with these criteria, they further have to meet CA's monetary eligible limit on earnings in a base period to be eligible for UI. In CA, the base period is the first four of the last five completed calendar quarters before application to UI. The monetary eligibility

[2]Each year the Department of Labor publishes a guide to differences in UI programs across states: https://oui.doleta.gov/unemploy/comparison/2020-2029/comparison2021.asp

limits in CA are that a worker either had to earn at least $1,300 in their highest earning quarter, or they had to earn $900 in their highest earning quarter and $1,125 in the whole base period. If they do not meet the criteria in the standard base period, they can use an alternate base period (ABP), which applies the same monetary thresholds to the last four completed calendar quarters. Monetary eligibility limits and whether a worker can use an ABP varies by state. Figure 2.1 shows how monetary eligibility differs by state and which states allow ABPs.

After workers meet these criteria, they are eligible for UI and receive a WBA and a PBD. In California, the WBA is equal to 50% of weekly wages in the worker's highest earning quarter up to a limit of $450. This upper limit varies by state with Massachusetts having an upper limit of $850 and Louisiana having an upper limit of only $221. In California, a worker's PBD will be between 14 and 26 weeks. While the maximum PBD in most states is 26 weeks, some states have substantially lower PBDs with Georgia and Alabama only providing 14 weeks of UI. In order to continue receiving benefits each week, claimants have to report their work search activities. California does not specify the number or type of work search activities that must be taken, but some states do; for example, Utah requires claimants to report 4 job search activities each week.

During the pandemic, federal and state policy makers introduced a large number of temporary changes to the program. Federal policy makers introduced the Pandemic Emergency Unemployment Compensation (PEUC) program that provided additional weeks of UI to claimants who used all their regular UI benefits. They also provided supplemental weekly payments that added either 300*or*600 to claimants' normal WBAs. They introduced a new insurance program called the Pandemic Unemployment Assistance (PUA) program that provided benefits to workers who are normally not eligible for regular UI such as self-employed workers. In addition to federal benefit extensions, in many states workers exhausting their regular UI benefits had access to the Extended Benefits (EB) program. The EB program varies across states but typically provides between 13 and 20 weeks of additional UI benefits when a state's unemployment rate rises above a certain level. See (Bell et al. 2022) for a discussion of the EB program.

State policy makers also made temporary changes to the programs; for example, nearly all states suspended work search requirements at the beginning of the pandemic. While these temporary federal programs had uniform eligibility rules, the ability to access them varied across states, partly due to administrative difficulties in implementing these programs, partly due pre-existing differences in eligibility and access. Moreover, states ended reliance on these programs and re-introduced job search requirements at different times as the pandemic evolved.

2.2.2 Conceptual Framework

To study access to UI, this paper relies on an integrated conceptual framework for measuring community-level access based on four metrics - a traditional measure that considers the stock of workers receiving UI and three new measures that are based on flows of workers entering and exiting the UI system. Figure 2.2 provides a high-level overview of our data-driven framework.

Our framework begins with the traditional measure of UI access, the recipiency rate. The recipiency rate is the share of unemployed (or under-employed) workers in a given week who were collecting regular UI benefits. For this paper, due to issues of data quality, we focus only on measuring the recipiency rate of regular UI, not PUA. Further details on why we exclude PUA and how we implement this and other measures is provided in the Operationalizing the Measures of Access section.

The first of our three flow measures in our framework is the application rate, which begins at the point of a job separation.[3] Upon becoming unemployed, the unemployed worker chooses whether or not to file a new initial claim for UI benefits. The rate at which newly unemployed workers file for benefits is our earliest measure of access. Completion

[3]Not all separations result in a worker being qualified for UI. In robustness checks, we define this event more stringently in terms of layoffs.

of this step requires the worker to know about the UI system, comprehend the language in which the application is written, and in many cases (particularly during the pandemic) perform an identity verification check involving a smartphone with a camera. In general, the recipiency rate will be higher whenever the application rate is higher.

The second flow measure of our model starts after an unemployed worker has filed a new initial claim. We then check to see the rate at which new initial claims are paid at least once. Reasons for a claim to be rejected can be either monetary (e.g., insufficient prior earnings) or non-monetary (e.g., claimant quit their job without good cause). We define this measure of the rate at which new initial claimants receive a first payment as the first payment rate.[4] Although for the limited scope of this paper we refer to the share of claims paid as a measure of access, in future work this measure can be further refined by removing from the denominator any claimants whose claim was not paid because the claimant found alternative work. As with the application rate, the recipiency rate will be higher whenever the first payment rate is higher, all else equal.

While the first two flow measures represent workers entering UI, the last measure represents unemployed workers flowing out of UI. The exhaustion rate measures the share of workers who received UI and used all the benefits for which they were eligible. The exhaustion rate is a useful measure of access because it reflects how fully insured workers were against the length of job loss they experienced. Still, like first payment rates, exhaustion rates are not solely a measure of access since they may also be influenced by claimant decisions around searching for and returning to employment. Future work should examine the reemployment prospects of workers who exhausted benefits during the pandemic. In contrast to the previous two flow variables, the recipiency rate will be higher when the exhaustion rate is lower.[5]

[4]Although the focus of this paper is whether claims are paid, important questions have arisen during the pandemic concerning the timeliness of payments. For information on this dimension, see The Century Foundation's Dashboard: https://tcf.org/content/data/unemployment-insurance-data-dashboard.

[5]In Appendix Table 2.A1, we show that the raw correlations between the Recipiency Rate and other three measures of access are consistent with the mechanisms described in this section.

2.3 Operationalizing the Measures of Access

The data for this paper stems from the DOL and California's EDD. Data from the DOL was taken from its Office of Unemployment Insurance through the publicly available "Data Downloads" portal on the office's website, which is updated daily.[6] The data extracted from this portal dates back to 1984, and it contains state-level employment information for all 50 states. The variables in these extracted datasets are reported on either a weekly or monthly basis. Several of our measures combine variables within the DOL data, such as our first payment rate described in detail below.

For our within-California analysis, we use administrative data from EDD on initial and continuing claims. The initial claims data includes all claims filed in the state of California. For each claim, the dataset has information on the date of claim filing, the benefit amount, and demographics, among other information. The continuing claims data includes payments information for all claims filed in the state of California. The continuing claims data also contains information about the last payment of each claim for all available programs, allowing us to measure exhaustion rates. The administrative data on continuing claims and exhaustions offers several measurement advantages over the publicly available DOL data that we describe in the A1 Measurement Appendix. Table 2.1 describes at a high level how each of the four measures of access are operationalized in the DOL and EDD datasets.

Finally, throughout the paper, the PUA program is excluded from the analysis because the high levels of reported fraud make it difficult to estimate how many workers actually used the program. For example, in California, the PUA program accounted for 95% of all identified fraudulent claims in the state. Additionally, the US DOL has also said that the program was more vulnerable to fraud.[7] How the PUA program impacted access to UI is a very important topic, which we will return to in the conclusion when discussing avenues of

[6]DOL "Data Downloads" portal: https://oui.doleta.gov/unemploy/DataDownloads.asp

[7]For more details on fraud in California, please see: https://edd.ca.gov/siteassets/files/Unemployment/

future research.

2.3.1 Measurement of Recipiency Rates

We measure the UI recipiency rate as the number of people collecting regular UI benefits divided by the number of U-6 unemployed workers in an area. The numerator is the number of people collecting regular UI benefits, and is taken from both the DOL for the state-level analysis and EDD for the within California analysis. The denominator is the number of U-6 unemployed derived from the Current Population Survey.[8]

Our numerator excludes claimants receiving PUA benefits, not only to reduce complications related to reports of fraudulent PUA claims in certain states, but also because some PUA claimants may be working reduced hours for non-economic reasons, and thus would not be included in the denominator.[9] Furthermore, many business owners would be counted as employed if they worked just a single hour during the CPS reference week, but would still be eligible to receive PUA benefits if their business was affected by the pandemic.[10] Thus, by focusing just on claimants receiving regular UI benefits, we are able to form a more "apples-to-apples" comparison. For additional details about the construction of the measures, see the A1 Measurement Appendix.

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[8] If there are a substantial number of workers receiving partial UI for non-economic reasons, the recipiency rate could rise above 100% (as seen in figure 2.3), because these workers can collect UI (and thus be counted in the numerator), but because their reduced hours are for non-economic reasons, they may not be counted as unemployed in the CPS. Furthermore, due to the fact that DOL's continuing claims are reported in the week payments are processed, and not the corresponding week of unemployment, some state-level estimates of recipiency may be artificially high or low, depending on the backlog of claims in the state. This timing issue is discussed in the Measurement Appendix.

[9] For more detail on CPS definitions of unemployment, see: https://www.bls.gov/cps/definitions.htmpter. In addition, certain states had substantial delays in reporting PUA claims, particularly in the first several months of the pandemic.

[10] See California's PUA eligibility criteria here: https://edd.ca.gov/about$_{e}dd/coronavirus - 2019/pandemic - unemployment - assistance.htm$. See the CPS definition here: https://www.bls.gov/cps/definitions.htmemployed.

2.3.2 Measurement of Application Rates

Whereas our analysis of recipiency rates during the pandemic focused on December of 2020, when analyzing application rates we focus on claimants during the first half of 2020. This timing better aligns with when the pandemic-driven surge of unemployment began and peaked.

At present, we are able to measure application rates only at the state level. Our baseline measure of application rates at the state level divides the number of new initial claims in a state[11] by the number of total separations in that state and month as reported by the Job Openings and Labor Turnover Survey (JOLTS) administered by the U.S. Bureau of Labor Statistics. The A1 Measurement Appendix provides details on alternative measures of the application rate that we use in robustness checks.

2.3.3 Measurement of First Payment Rates

Our state-level measure of first payment rates from the DOL data is constructed by dividing the total number of first payments in each state in each month by the total number of new initial claims[12] in each state in each month. In the individual level EDD data, the first payment rate is constructed by measuring the share of new initial claimants in each month who eventually receive a first payment, regardless of when that payment is made. Similar to the application rate, the first payment rate is also measured during the first half of 2020 to align with the surge in new initial claims filed. The A1 Measurement Appendix provides additional detail on two important caveats of this analysis when applied to the DOL data

[11] Importantly, the number of new initial claims has been a very small subset of the number of initial claims during most of the pandemic. For a more detailed investigation of the ways in which initial claims over-state entrances to unemployment, see our June of 2021 report.

[12] For the full definition of a new initial claim in California: https://www.edd.ca.gov/uibdg/Miscellaneous$_{M}I_{5}.htm$initial claims into two main categories: new initial claims and additional claims. New initial claims correspond to "an application for the establishment of a benefit year," and an unemployed person who wants to collect UI benefits must file a new initial claim. Additional claims correspond to claimants who experience an interruption in their benefit certification for one or more weeks due to being employed. Claimants still must be within their benefit year and have remaining benefits in order to file an additional claim. Since additional claims only represent re-entries to UI, we exclude them from our analysis, and only focus on new initial claims.

that can be assessed and remedied with microdata when the analysis focuses on California.

2.3.4 Measurement of Exhaustion Rates

Exhaustion rates have proven particularly difficult to measure, especially in the DOL data. Whereas the term "exhaustion" has at times been used to refer to claimants who exhausted their regular non-extension state UI benefits and moved on to extension programs, in this paper we aim to define exhaustions as those cases in which a claimant has exhausted all available UI benefits (including PEUC and EB), which is a more meaningful measure of access given policy changes during the pandemic.

The numerator of our exhaustion rate is an estimate of the number of claimants in a week who exhausted the final week of regular UI benefits available to them (including PEUC and EB). The A1 Measurement Appendix provides details on how the number of exhaustions is generated in the DOL and EDD data.

Whereas the numerator of our exhaustion rate in either dataset derives from the issuance of final payments, a question remains about what an appropriate at-risk group should serve as the denominator. In the DOL data, we use the number of continuing claimants as a denominator with which to construct an exhaustion rate. This choice of denominator is chosen largely for convenience. The aggregated nature of the DOL data makes it nearly impossible to relate the number of claimants who exhaust in a given week to any other group that is plausibly at risk of exhausting.

In the EDD microdata, we are able to construct two separate measures of exhaustion. In addition to relating the number of individuals exhausting benefits in a given week to the total number of individuals receiving benefits in that week (to compare with DOL results), we are also able to see specifically what share of claimants who established benefit years in a given week have eventually exhausted benefits. We call this measure the cohort exhaustion rate. In calculating the cohort exhaustion rate, we count all exhausted claimants within a cohort and report that number by date of the established benefit year. But in the other measure, we report the number of exhausted claimants (regardless of their cohort) by the week they experienced exhaustion.

2.4 Descriptive Statistics on Measures of Access

Table 2.2 presents descriptive statistics on our four access measures from the EDD and DOL datasets for California. We present means of each measure before and during the pandemic, in the first week of December 2019 and 2020. Since the structure of data in DOL and EDD are different, we did not expect to observe identical estimates. Despite these differences the estimates are in general reasonably close.

The only case in which the EDD estimate is significantly larger (32 percent) is the exhaustion rate in 2020. In this case, we suspect our approach in the DOL data underestimates the exhaustion rate. To calculate the number of claimants exhausting in the DOL data, we use the number of final payments for the program that would be the last one available to most claimants, which was EB in December 2020. This likely misses some claimants who exhausted PEUC and were not eligible for EB.[13]

Aside from exhaustion rates, the remaining EDD estimates are about 5 to 10 percent smaller than DOL. The main differences in estimates for recipiency rates and 2019 exhaustion rates arise from the fact that the DOL data for continuing claims are reported by the processing week while in EDD we use the week of unemployment to count continuing claims. Finally, the basis of discrepancy in the first payment measure is that in the EDD data we link individual-level data for new claimants to payment information to find the first payment rate; however, in the DOL data, we must rely on aggregate monthly numbers.

[13]For more details on EB (FED-ED) eligibility in California see https://edd.ca.gov/en/about$_edd/coronavirus - 2019/fed - ed$.

2.5 Recipiency Rates Among the Unemployed

2.5.1 Recipiency Rates Across the U.S.

Across the United States, we estimate that 60 percent of Americans who were unemployed in December of 2020 collected regular UI benefits.[14] Figure 2.3 shows that the national average masks substantial heterogeneity across states. In some states – such as MN, MA, NY, and CA – the number of UI claimants was essentially comparable to the number of people thought to be unemployed (with a recipiency rate of at least 90 percent). In contrast, TN, ID, NE, and FL all saw recipiency rates of less than one quarter, meaning that even at the height of the pandemic, the vast majority of unemployed workers were not collecting benefits.[15]

To better understand the sources of this state-level variation, figure 2.4 presents correlations of recipiency rates with other state-level policy and socioeconomic factors. On the socioeconomic side, states that experienced higher recipiency rates during the pandemic tended to be wealthier, as evidenced by a strong positive correlation with median household income. States that had a higher Democratic vote share in the last presidential election also had higher recipiency rates. States with higher shares of Black residents had lower recipiency rates during the pandemic. This pattern shines light on racial disparities in access to the UI system documented by a growing historical and qualitative literature (Edwards 2020; Fields-White and Graubard 2020).[16] A number of state-level policies were also very predictive of differences in recipiency rates. States that afforded claimants longer PBD's had

[14] Appendix figure 2.A1 shows that this U6 recipiency rate in December 2020 is a large increase from the pre-pandemic period, when the U6 recipiency rate was around 20%. Appendix figure 2.E1 shows that in December 2020, the average U3 recipiency rate was near 100%. Averaging across the entire year, DOL estimates that the U3 recipiency rate (for the entire country) was 78%, a substantial increase from 28% in 2019, and 24 percentage points above the previous peak of 54%, occurring in 1952. https://oui.doleta.gov/unemploy/Chartbook/a12.asp

[15] Appendix figure 2.A1 demonstrates how this state variation changed over time.

[16] An original aim of this study was to quantify the extent to which racial and ethnic disparities at the national level could be explained by low rates of access in states with certain racial and ethnic demographic compositions. We were unable to answer this question because the race and ethnicity information contained in the DOL data are not comparable with the race and ethnicity information available in the Current Population Survey (from which unemployment estimates are constructed).

substantially higher recipiency rates, as did states which allow the use of alternative base periods to establish monetary eligibility. States with public sick or paid leave programs also had higher rates of recipiency, which in this case could reflect that states with generous UI policies also have other generous labor-related policies. Table 2.E1 in the Data Appendix provides a limited test of this hypothesis by regressing the recipiency rate on a dummy for whether a state has sick or family leave policies and Democratic vote share as a signal for more generous UI policies. After including the vote share control the paid leave coefficient drops from 0.48 to 0.09 and loses significance. This provides some support for the theory that the bivariate correlations between sick/family leave and recipiency rates just reflect more generous labor and UI policies overall. While this is not a causal analysis, the correlations suggest that there is significant scope for state-level policies to affect access to UI, and that states' differing policies have resulted in geographic disparities in access to UI during the pandemic.[17]

Although these findings are correlational, the magnitudes of the correlations of recipiency rates with policy variables are substantial in many cases. Consider, for instance, the cross-state relationship observed between state PBD and recipiency rates. In December of 2020, the state UI maximum PBD in NC was 12 weeks, whereas MA offered up to 30 weeks.[18] Unsurprisingly, recipiency rates were substantially lower in NC than MA – 44 percent vs 102 percent. Suppose for the purpose of a back-of-envelope calculation that the observational correlation between state maximum PBD and recipiency were causal. If all states had a PBD of 30 weeks, the national recipiency rate would grow from 60 percent to 77 percent – a 28 percent increase. This would result in about three million more jobless workers collecting UI benefits each week, totalling about $1.7 billion in benefits. Appendix Table 2.E2 shows that the association between the PBD and recipiency rates is robust to the inclusion of economic, demographic and other policy controls, but nonetheless, such a calculation should

[17]Appendix figure 2.E2 plots the correlations between each covariate and the U3 based version of the Recipiency Rate. The results are nearly identical.

[18]Massachusetts State UI PBD increases from 26 to 30 weeks when unemployment is high.

be interpreted with caution as there are many other factors that differ across states. Still, the magnitude of this difference suggests there was likely great scope for state-level policies to influence recipiency rates during the pandemic.

2.5.2 Insights from Within CA

Measuring recipiency rates for regions within California is an important but difficult task. Although we have precise measures of how many Californians collected benefits from a given geographic unit, estimating the number of unemployed workers in that place at that time is more cumbersome. In this analysis, we rely on official county-level estimates from the Bureau of Labor Statistics Local Area Unemployment Statistics. However, estimating recipiency rates this way is far from ideal because – due to the small sample size of the Current Population Survey – the LAUS estimates for unemployment at the sub-state level rely on certain measures of UI claims themselves.[19] While we have contrasted the LAUS county unemployment rates to comparable estimates based on the CPS microdata and found them to be similar, the fact remains that for many smaller geographic units the estimates are based on small samples and hence are prone to statistical noise. For this reason, the county-level estimates of UI recipiency rates presented below should be interpreted with caution.[20]

Analogous to figure 2.3, figure 2.5 shows how recipiency rates varied within California. Based on the comparisons of UI claimants to LAUS unemployment rates (re-scaled to mirror U-6), Los Angeles County has by far the lowest recipiency rate among large counties in California. Figure 2.5 also demonstrates substantially less variation in recipiency rates across counties than across states.[21] This could be a consequence of the UI program parameters being constant across counties, but substantially different across states.

Figure 2.6 shows county-level correlations of recipiency rates with socioeconomic

[19]For more information, see the LAUS methodology note: https://www.bls.gov/lau/laumthd.htm

[20]In our ongoing series of policy briefs, we have compared geographic patterns of recipiency rates using the LAUS county-level definition of unemployment to the tract-level unemployment estimates near the start of the pandemic of Ghitza and Steitz (2020). We have not detected meaningful differences in the spatial correlations using either measure of unemployment.

[21]Appendix Figure 2.A2 also demonstrates how this county variation changed over time.

indicators. Similar to our findings across states, higher-income counties also saw higher rates of UI recipiency. Counties with higher rates of COVID-19 deaths saw lower rates of recipiency, as did those counties with higher shares of Hispanic residents. We find that counties with more broadband access had substantially higher rates of UI recipiency, which points to the importance of technological gaps in access to UI during the pandemic. We also find that counties with more residents with limited English proficiency had lower rates of UI recipiency, suggesting that language barriers may also have played a role in limiting access. Many of these correlational findings corroborate the more qualitative conclusions of Fields-White and Graubard (2020) on the role that barriers to access during the pandemic have played in widening racial disparities, including stigma, burdens to produce documentation, and the digital divide. Although an authoritative dissection of the roots of these differences is beyond the scope of the current study, a growing body of quantitative and qualitative evidence suggests that both legal eligibility and more nuanced barriers to accessibility of UI have played important roles in determining UI recipiency rates.

Given the stark differences across geographic regions in UI recipiency rates, we next turn to analyzing geographic differences in rates of first payments.

2.6 Application Rates Among the Unemployed

2.6.1 Application Rates Across the U.S.

At the national level, we estimate that 83 percent of workers who were separated from their employer in Q1 or Q2 2020 filed an unemployment insurance claim. The application rate varied substantially across states with an interquartile range of 63 percent to 87 percent. These estimates should be interpreted with some caution as we are relating separations in a month to new initial claims in a month even though the claims filed could be the result of separations in a previous month.[22] One additional note of caution is that the high application

[22]For example, a large increase in separations at the end of a month could lead to a large increase in new UI claims filed at the beginning of the next month depending on how long it takes a worker to file for UI

rates in 2020 could be explained by high levels of fraud that was reported during the pandemic (Podkul 2021). Nevertheless, figure 2.7 shows the spread of application rates across states in the first half of 2020. Among the states that had the highest share of separated workers filing new claims were GA, OK, NY, AL, and LA and among the lowest share was SD, UT, WY, and CO. Interestingly, some of the states with the highest application rates, such as GA[23], OK, AL, and LA also had some of the lowest first payment rates. This pattern is consistent with high levels of fraudulent claims in some states being appropriately rejected and leading to lower first payment rates.

Figure 2.8 explores disparities in application rates by measuring the correlation between application rates and a set of state-level characteristics.[24] Some state-level policies are statistically significantly correlated with application rates. States that either fully or partially suspended work search requirements were correlated with higher application rates. Though we cannot interpret this relationship as causal, one hypothesis that could be tested further is that suspending work search requirements could have encouraged people who were no longer in the labor force to file claims thereby raising the new UI claims without increasing new separations. In contrast to the other three other measures of access, economic affluence was not associated with greater application rates in 2020. Similarly, the share of the state that is Black is actually associated with greater application rates, while it is typically associated with lower access in the other three measures.

after separating from their employer.

[23]Georgia's high application rate is possibly the result of their unique PUA application process. In Georgia applicants who wanted to sign up for PUA benefits had to first apply and be rejected for regular UI benefits before applying for PUA while in other states applicants could directly apply for PUA benefits. This would mechanically increase the application rate and decrease the first payment rate in Georgia.

[24]Appendix figures 2.E3 and 2.E4 depict the same correlations but using the alternative layoffs and recently unemployed denominators discussed in the Measurement Appendix. The pattern of results is very similar.

2.7 First Payment Rates Among Claimants

2.7.1 First Payment Rates Across the U.S.

At the national level, we estimate that about 70 percent of new initial claims filed in the first two quarters of 2020 resulted in first payments. This measure of access varied dramatically across states, although it should be noted that there is noise in this calculation in the DOL data because we are relating first payments issued in a month to new initial claims filed in a month (which are not necessarily the same claims). Still, figure 2.9 shows that states essentially span the entire range from nearly 40 percent to approximately 100 percent.[25] Among the states that paid the highest share of claims in the first half of 2020 were VA, KS, IA, and HI, whereas MT, AZ, and GA were among the lowest.

Figure 2.10 shows how the heterogeneity in first payment rates covaries with our set of state-level covariates. Certain state-level policies appear to relate to first payment rates in the expected directions. In states that allow claims to be established under alternative base period formulas, more claimants get paid. Although states with longer UI durations also see a larger share of claimants paid, we do not detect a significant correlation between the share of claimants paid and monetary eligibility thresholds. This is surprising, since a higher monetary eligibility threshold implies that (all else equal) fewer claimants are monetary eligible and therefore fewer claims will receive a first payment.[26] However, there are other reasons for a claim to go unpaid, including non-monetary eligibility criteria, short unemployment spells, or claimants failing to certify for benefits for other reasons. These scenarios may be less common in states with higher monetary eligibility thresholds. Ultimately, the large variation in first payment rates across states and correlation with policy variables implies that state governments have a great deal of discretion in how generous they

[25]The fact that some states are above 100 percent is an artifact of how DOL reports claims filed in a month and claims paid in a month, but these are not necessarily the same claims. This is a limitation that we face in our cross-state analysis but not for our within-California analysis relying on microdata.

[26]A monetary eligibility threshold is the minimum amount of earnings that a jobless worker must have earned in the base period in order to establish a UI claim. The monetary eligibility threshold in January 2020 ranged from 130$inHawaiito$7,000 in Arizona.

want to make access to UI. Another example of this is the use of facial recognition tools like ID.me for identity verification, which may have helped reduce fraud, but also made it harder for people to legitimately access benefits. In response, some states stopped using ID.me while others continued, illustrating the discretion that states have in making it easier or harder for unemployed workers to access benefits.[27]

In general, states that paid a higher share of claims during the start of the pandemic tended to be more affluent (as measured by median household income or poverty rates) and slightly more economically unequal (evidenced by the negative correlation of first payment rates with the Gini coefficient). States with a higher share of Black workers paid out significantly lower shares of claims, though we did not detect a significant correlation with Hispanic share.

2.7.2 Insights from within CA

Relative to the amount of variation in first payment rates across states, the variation in first payment rates across California's counties is more modest. The sample of the first payment analysis includes claimants with regular new initial claims in the second quarter of 2020. Figure 2.11 plots the rate of first payments in each of California's 58 counties. Trinity County saw the lowest rate of first payments in the second quarter of 2020 (about 68 percent), with low rates also coming from Sierra, Del Norte, and Lake. Among the counties with the highest share of claims paid were Mono, Imperial, and San Benito (83, 83, and 82 percent respectively). Los Angeles County, which ranked among the lowest counties in terms of recipiency rates as benchmarked in relation to LAUS estimates of unemployed people, ranked near the middle in terms of the share of claims from its residents that have been paid.

Figure 2.12 correlates counties' first payment rates with our standard county-level set

[27] For example, Massachusetts stopped using ID.me in early 2020: https://www.bostonherald.com/2022/02/23/massachusetts-unemployment-office-plans-to-drop-facial-recognition-technology-in-coming-weeks/

of covariates. By several measures, more affluent counties saw substantially higher rates of payments. Counties with higher income and fewer SNAP recipients or those in poverty saw higher rates of payments among claimants. We also detect a positive relationship between broadband access and first payment rates.

Having established geographic heterogeneity in the rate at which first payments were issued during and before the pandemic, the final stage of our analysis turns to exhaustion rates.

2.8 Exhaustion Rates

2.8.1 Exhaustion Rates Across the U.S.

We estimate that in the first week of December of 2020, approximately 6 percent of Americans who were claiming UI benefits exhausted their benefits. The exhaustion rate varied substantially across states, with Florida and Georgia seeing more than one-fifth of their claimants exhausting. In contrast, about half of states saw exhaustion rates of 3 percent or less. The top five states with the most exhaustions in December 2020 were Georgia, Texas, Florida, North Carolina, and California, and together they accounted for 52 percent of all exhaustions in the U.S. that month. Figure 2.13 plots a bar graph of exhaustion rates across states.

A wide variety of socioeconomic and policy variables are significant predictors of differences in state-level differences in exhaustion rates during the pandemic. Figure 2.14 presents these correlations. Of the covariates we studied, the strongest predictor of exhaustion rates was the maximum duration of UI benefits. Exhaustion rates were lower in states with more generous benefits (either in terms of duration or levels) and those that provided workers with sick leave programs (which may have functioned as alternatives to UI). In general, exhaustion rates were also substantially lower in more Democratic-leaning states and states with more high-earners. Exhaustion rates were slightly higher in states with more Black residents and older residents.

2.8.2 Insights from within CA

For our within-California analysis, we put forward two distinct measures of exhaustion rates. To mirror the definition of exhaustion rates we were able to operationalize in the DOL data, we first divide the number of claimants who exhausted UI in a given week by the total number of claimants who certified that week. Conceptually, this ratio is difficult to interpret. Although each claimant can count at most once in the numerator (during the week of exhaustion), the same individual would count toward the denominator for multiple weeks (during each week claimed). A more readily interpretable statistic is the share of UI entrants in a given week who will eventually exhaust UI. Because this statistic counts each claimant exactly once in the denominator (during the week of entry), it is more accurate. For the same reason, the more accurate measure tends to be higher than the traditional measure. A potential drawback is that it cannot be implemented nationally with available data.

Figure 2.15 plots how these two definitions of exhaustion rates have evolved in California during the pandemic. Whereas the number of California's claimants exhausting each week has typically amounted to less than 1 percent of that week's continuing claimants (Panels A), a very different story emerges when analyzing exhaustees as a share of the weekly entry cohort (Panel B). Among Californians whose benefit years began during the pandemic, between 10-20 percent of these claimants have already exhausted benefits as of the end of June 2021. However, we anticipate these cohort exhaustion rates to rise considerably as time goes on because this analysis does not take into account the large effects the recent September 2021 benefits expiration had on these cohorts.[28]

[28]We do not estimate the cohort exhaustion rate at the state level. To estimate the cohort exhaustion rate, one needs to find the size of each cohort and the number of exhausted claimants in the related cohort. To calculate such a rate, we need to make assumptions based on PBD. The main reason for avoiding using DOL data to calculate cohort exhaustion rate is the substantial disparities in PBD, especially post COVID with extension programs.

So far, our cohort-level exhaustion rate estimates during the pandemic have been somewhat lower than what prior literature has found during past recessions, though direct comparisons are difficult because our analysis focuses on California whereas other work has estimated national averages. Nicholson and Needels (2006) look at cohort exhaustion rates during recession years between 1970 and 2003. They show that the (national) exhaustion rate for the early 2000s recession was on average 32 percent. In general, it is hard to predict the direction of exhaustion rates during recessions because when unemployment duration increases, the benefit duration also increases due to extension programs.

Mueller et al. (2016) estimated cohort exhaustion during the Great Recession. They show that, at the beginning of the recession, exhaustion rates decreased because of extended benefits, but eventually they started to increase because of the rise of unemployment durations.

Our estimates for cohort exhaustion rates in 2020 must be interpreted with caution because as of June 2021 a vast number of claimants still have remaining benefit durations. Ending extension benefits in September 2021 without a meaningful decrease in unemployment duration will likely increase the cohort exhaustion rates significantly for 2020 cohorts.

In contrast to our cross-state analysis of exhaustions as a share of continuing claimants in December of 2020 in the DOL data, when examining geographic differences in exhaustion rates within California, we analyze the cohort-specific exhaustion rates of claimants who entered UI in March of 2020. Figure 2.16 plots cohort exhaustion rates by county in California. Some of the highest rates of exhaustion among March 2020 entrants were in the counties of Imperial, Kern, and King.

Figure 2.17 describes how exhaustion rates vary across counties in relation to our standard set of county-level covariates. Exhaustion rates have been substantially higher in counties with more limited-English speakers, as well as those that reported more COVID-19 deaths. Poorer counties have also seen higher rates of exhaustion, as have those with higher share of Black or Hispanic residents. Interestingly, whereas states with more elderly residents had higher exhaustion rates, we find within California that counties with more elderly residents have substantially lower exhaustion rates.

2.9 Conclusion

Using a broader set of measures that move beyond and complement the traditional measure of UI recipiency, this paper examines the geographic correlates of access to regular UI during the pandemic. We generated four measures of access to UI that can be operationalized in commonly accessible datasets based on public DOL aggregated data: application rates, first payment rates, recipiency rates, and exhaustion rates. In the context of California, we have validated and explored extensions to these measures using UI claims microdata. We produced these measures for the pandemic period, before the vaccine rollout from March to December 2020.

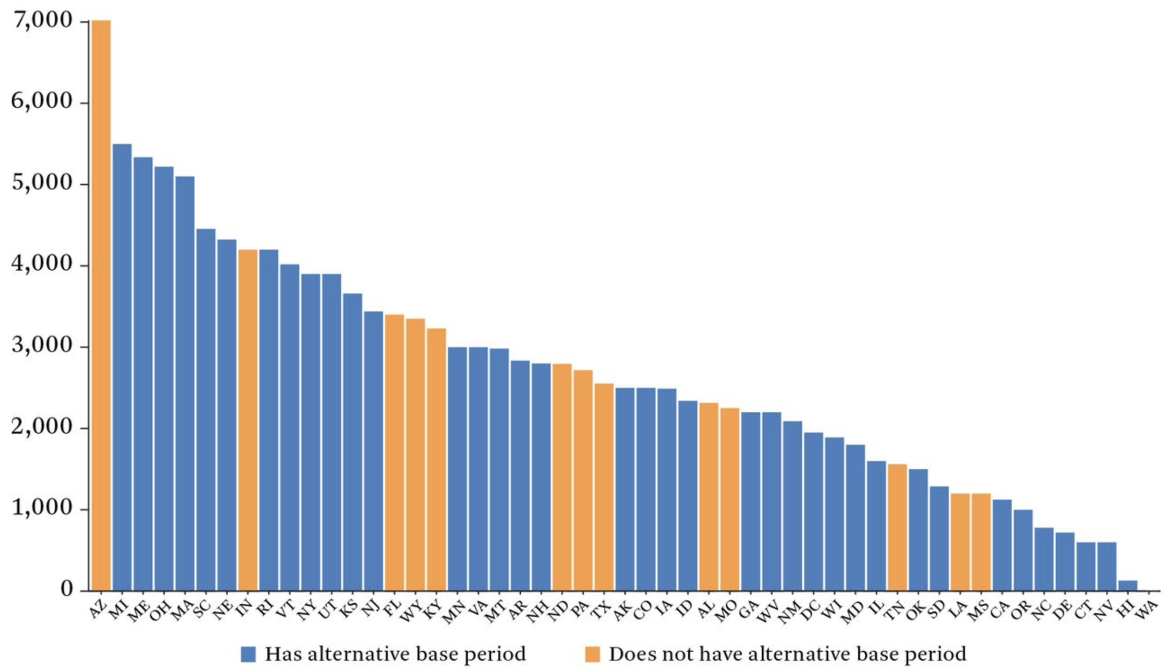
Several key patterns have emerged when comparing our measures of UI access during the pandemic across states and across counties within California. Across states, a clear pattern emerges that residents of states with more generous UI policies have seen higher rates of UI access during the pandemic. Demographic and socioeconomic patterns have also emerged, both across states and within California. Our metrics of access to UI have generally indicated higher access in areas with more affluent residents, more access to broadband internet, and more English-speaking residents, and less access in areas with more Black or Hispanic residents. The findings are strongly suggestive that policy has played an important role in driving disparities in access to UI across states. Further research would be needed to establish a causal link between particular policies, programs or practices and differences in UI access. This is of course a difficult question, since policies may themselves be affected by the fundamental forces helping to determine UI access.

The potential impact of state policies and the substantial amount of discretion states have in choosing program parameters and administrative procedure within the federal framework has implications for efforts to improve access to the UI program nationwide. In the past, the federal government has provided monetary incentives to encourage states to make their programs more inclusive. The ongoing disparities provide some support to the notion that stronger federal guidelines, or the establishment of federally managed components (e.g., such as a common application portal), may be required to broaden access to UI throughout the country.

Several important questions remain. A key question for future research will be how UI access changed when several states terminated PEUC and PUA early in the summer of 2021. Similarly, more research will be needed to understand the impacts of the September 2021 benefits expiration. Comparing the magnitudes of these turn-offs to those of the Great Recession would be useful in this context. Additionally, the data used in this paper are also not recent enough to ascertain how vaccination efforts have affected the role of UI in the economy. Also, research into how the PUA program has shaped access to UI during the pandemic would be valuable. Researchers should estimate recipiency rates of PUA, with a focus on self-employed workers and wage workers not eligible for regular UI. Comparisons of the effect of the PUA program on labor supply choices would also be valuable for policy making. Finally, the present analysis is largely cross-sectional in that it compares differences in access across space. Given the vast number of state-level policy changes (e.g., such as changes in benefit levels or durations, changes in monetary and non-monetary eligibility), that have occurred during the decades for which data are available, additional work implementing difference-in-differences strategies would provide policy-relevant estimates of the effects of UI policy changes on various measures of access.

2.10 Figures and Tables

Figure 2.1: Monetary Eligibility and Alternative Base Periods by State



Note: Source: Department of Labor Comparison of State UI Laws. The height of each bar represents the minimum amount of income a worker needed to earn to qualify for UI. The dark bars represent states with Alternative Base Periods and the light bars represent states that do not have alternative base periods.

Figure 2.2: Measuring Access in UI Claims Data

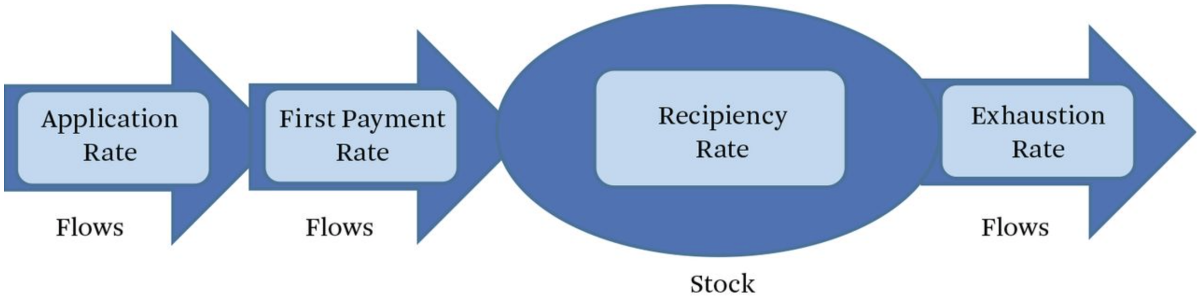
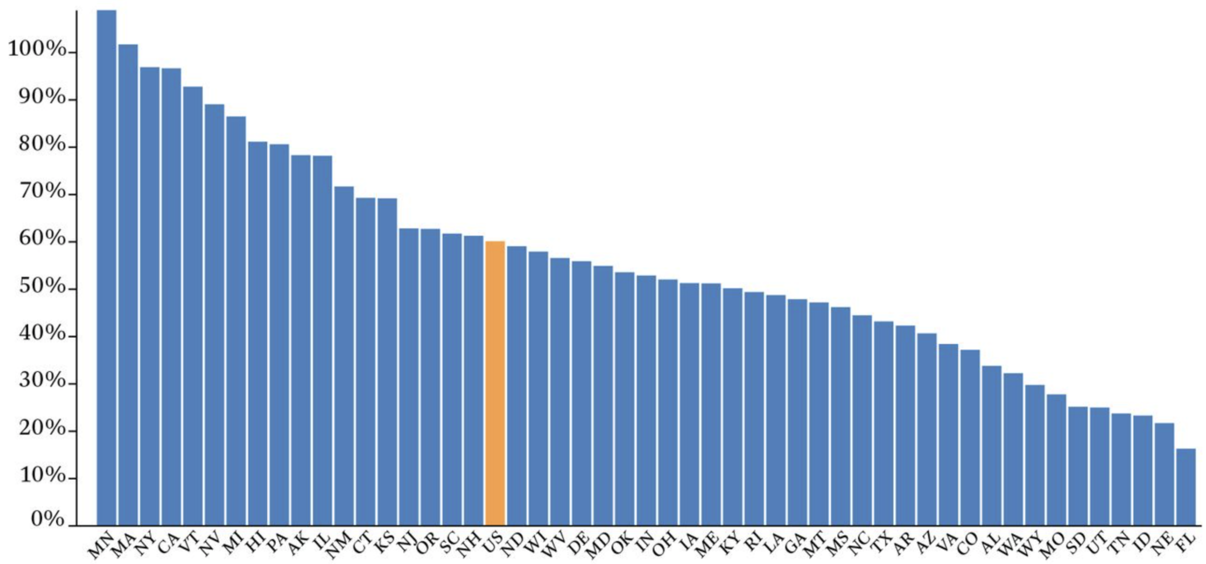
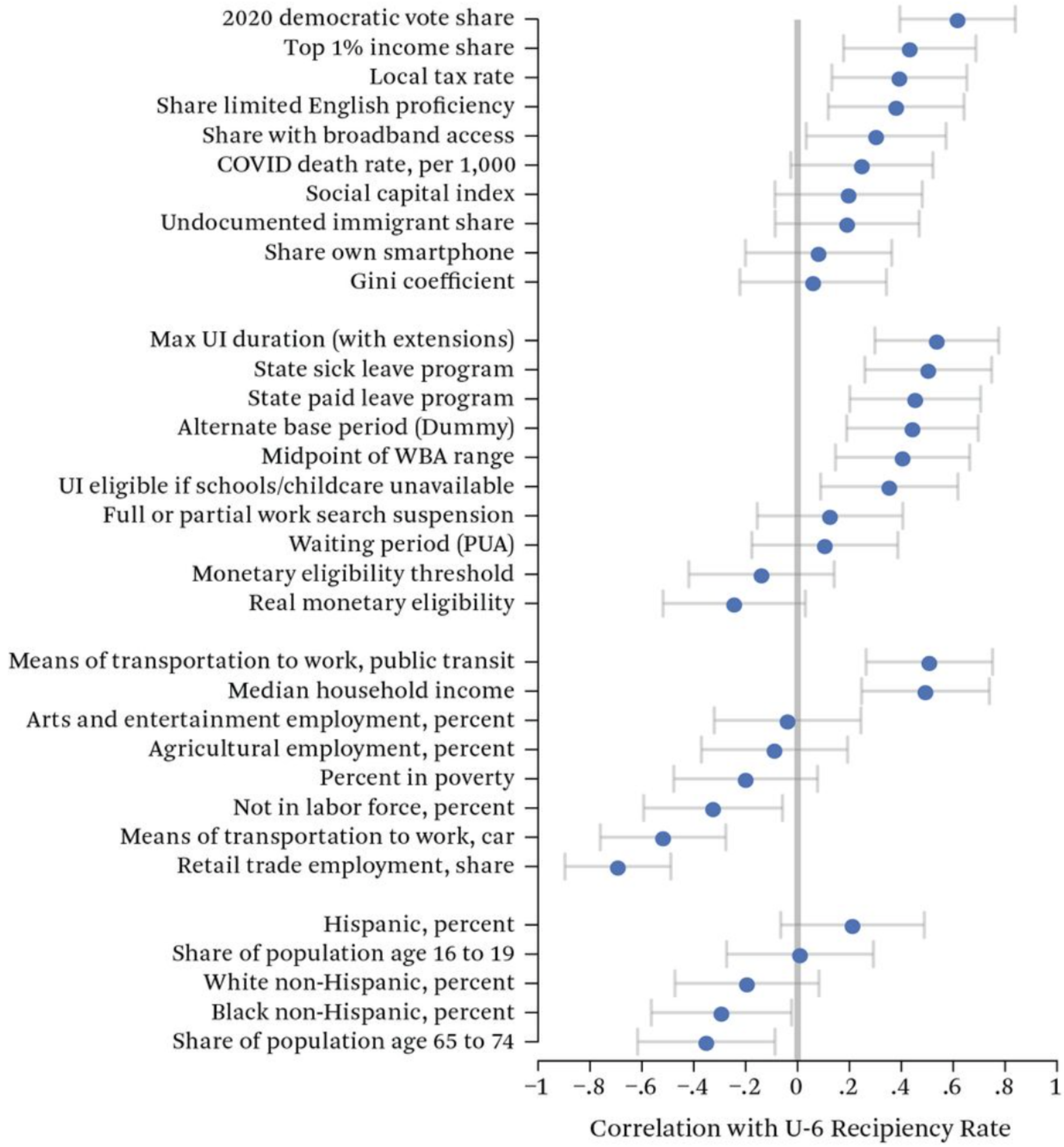


Figure 2.3: Recipiency Rates Across States, Bar Graph



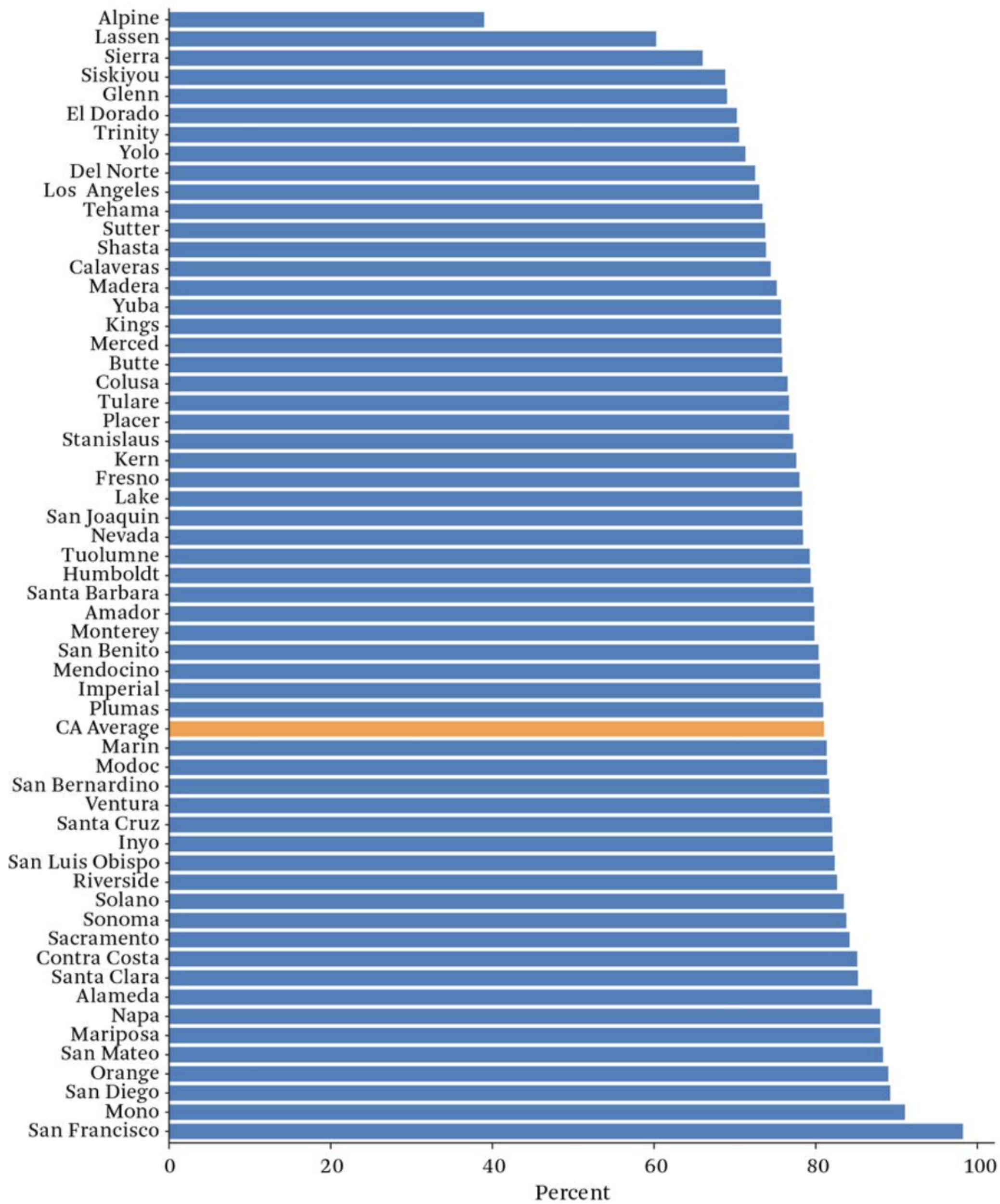
Note: $N = 50$. Source: DOL, CPS, author calculations. The dark bars represent the recipiency rates across states for the week of December 5th, 2020. The light bar represents the US average recipiency rate weighted by population in 2019. The recipiency rate is the number of continuing claims paid from the DOL divided by the number of U6 unemployed from the CPS.

Figure 2.4: Recipiency Rates Across States, Correlations



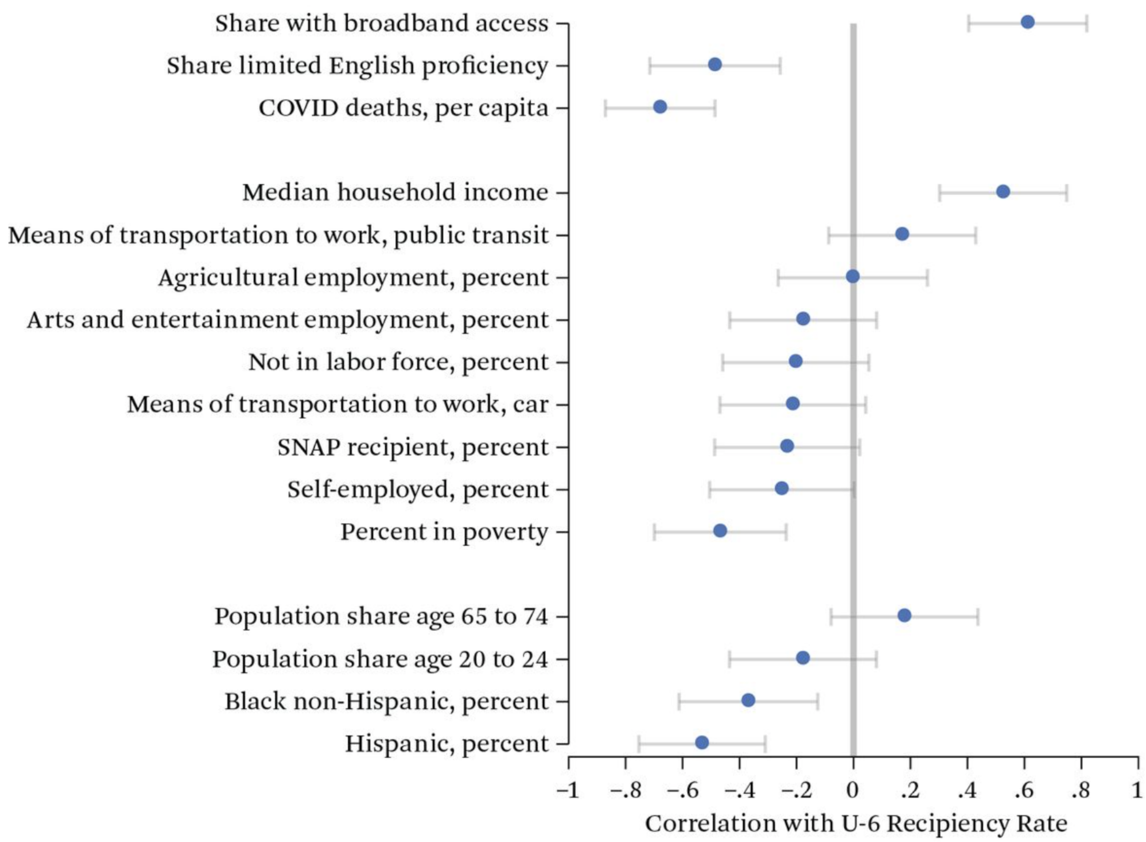
Note: N = 50. Source: DOL, CPS, ACS, author calculations.. Each dot represents the correlation between the covariate and recipiency rate in December 2020 weighted by population in 2019. All variables are measured at the state level. Error bars represent the 95% confidence interval. The recipiency rate is the number of continuing claims paid from the DOL divided by the number of U6 Unemployed from the CPS. For more details of covariates, see Data Appendix.

Figure 2.5: Recipiency Rates Within California, County-Level Bar Graph



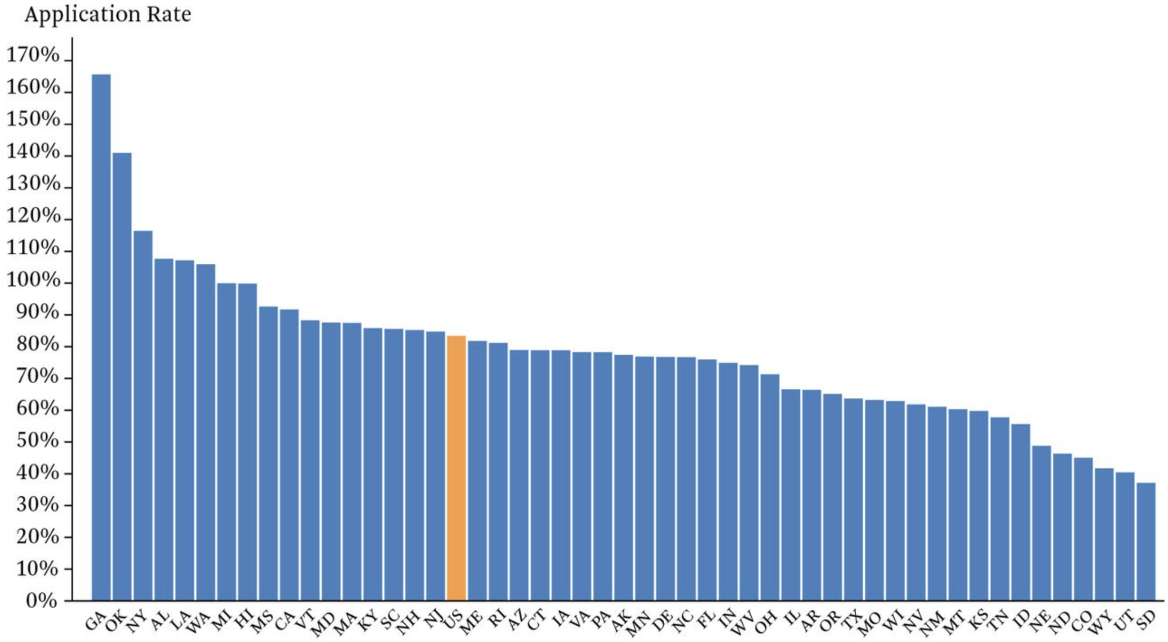
Note: $N = 58$. Source: EDD, CPS, author calculations.. The dark bars represent the recipiency rates for all the counties in December 2020. The light bar represents the California average Recipiency Rate weighted by population. The recipiency rate is the number of continuing claims paid from EDD divided by the number of U6 unemployed from the CPS and LAUS.

Figure 2.6: Recipiency Rates Within California, County-Level Correlations



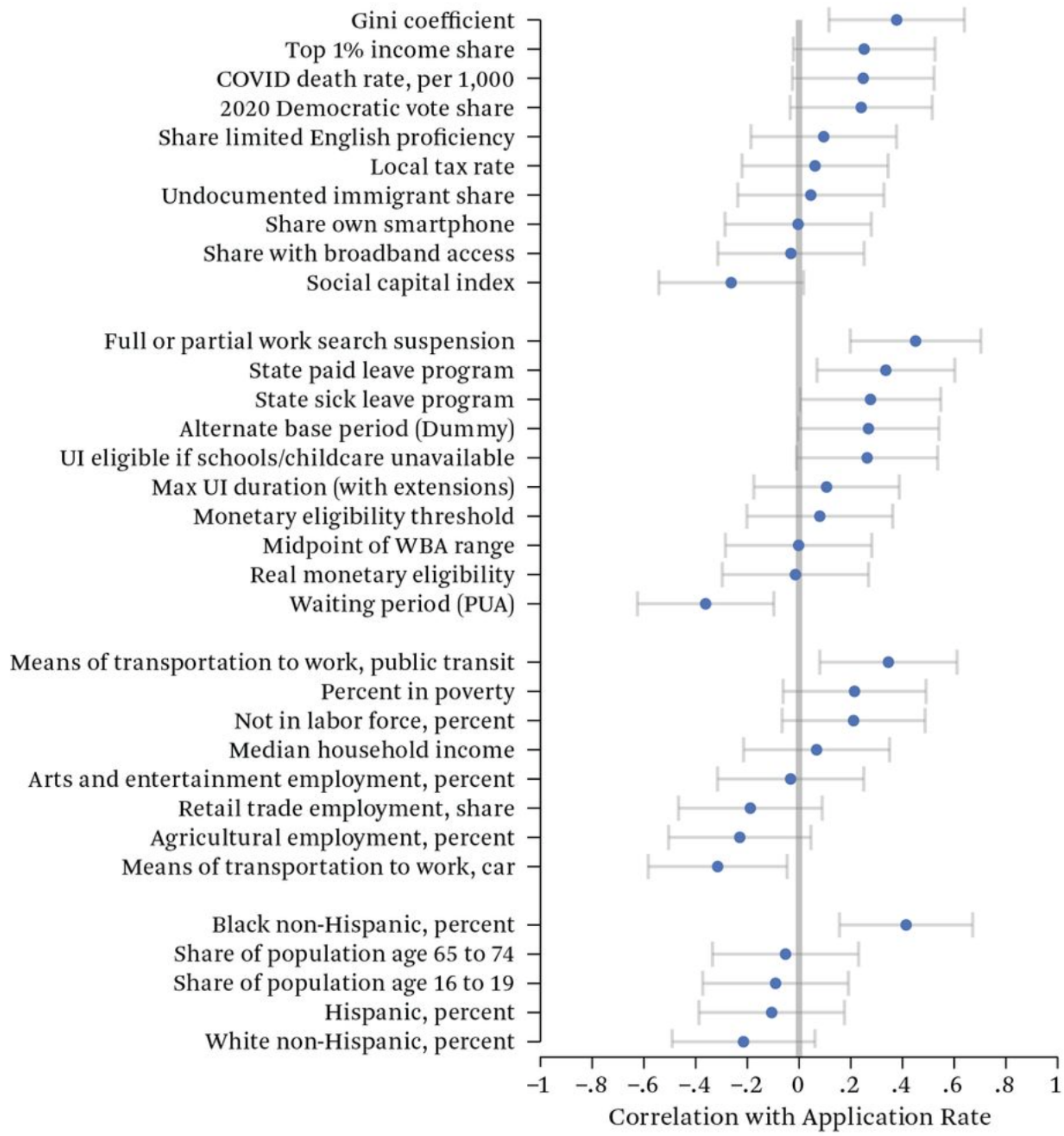
Note: $N = 58$. Source: EDD, CPS, ACS, author calculations. Each dot represents the correlation between the covariate and UI recipiency rate in December 2020 weighted by population in 2019. All variables are measured at the county level. Error bars represent the 95% confidence interval. The recipiency rate is the number of continuing claims paid from EDD divided by the number of U6 Unemployed from the CPS and LAUS. For more details of covariates, see Data Appendix.

Figure 2.7: Application Rate, Across States, Bar Graph



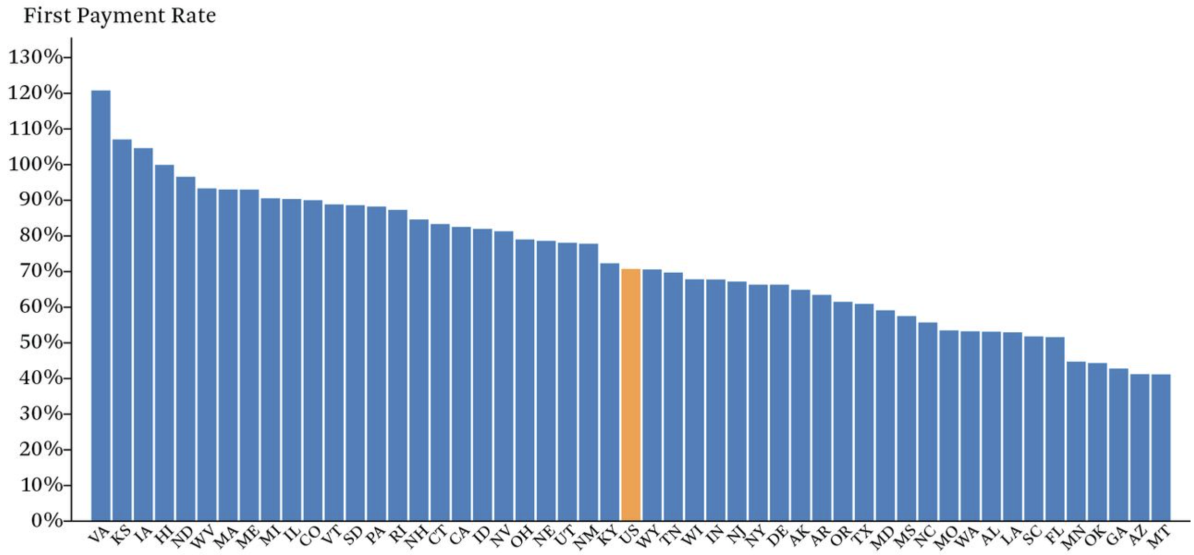
Note: $N = 50$. Source: DOL, JOLTS, author calculations.. The dark bars represent the application rates across states for Q1 and Q2 2020. The light bar represents the US average application rate weighted by population in 2019. The application rate is the number of new UI claims from the DOL divided by the number of separations from JOLTS.

Figure 2.8: Application Rates Across States, Correlations



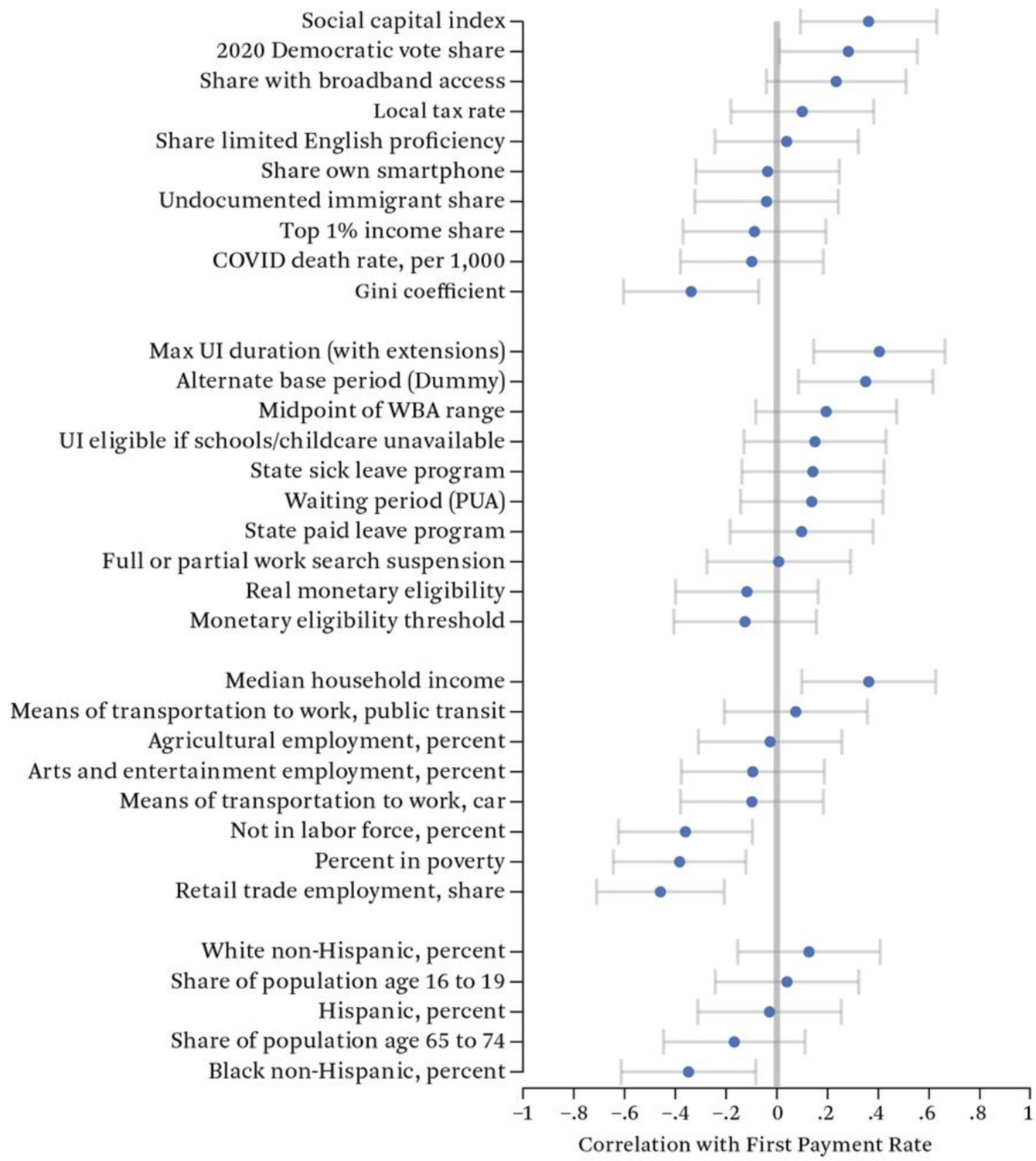
Note: $N = 50$. Source: DOL, ACS, JOLTS, author calculations. Each dot represents the correlation between the covariate and the application rate in Q1 and Q2 2020 weighted by population in 2019. All variables are measured at the state level. Error bars represent the 95% confidence interval. The application rate is the number of new UI claims from the DOL divided by the number of separations from JOLTS. For more details of covariates, see Data Appendix.

Figure 2.9: First Payment Rates Across States, Bar Graph



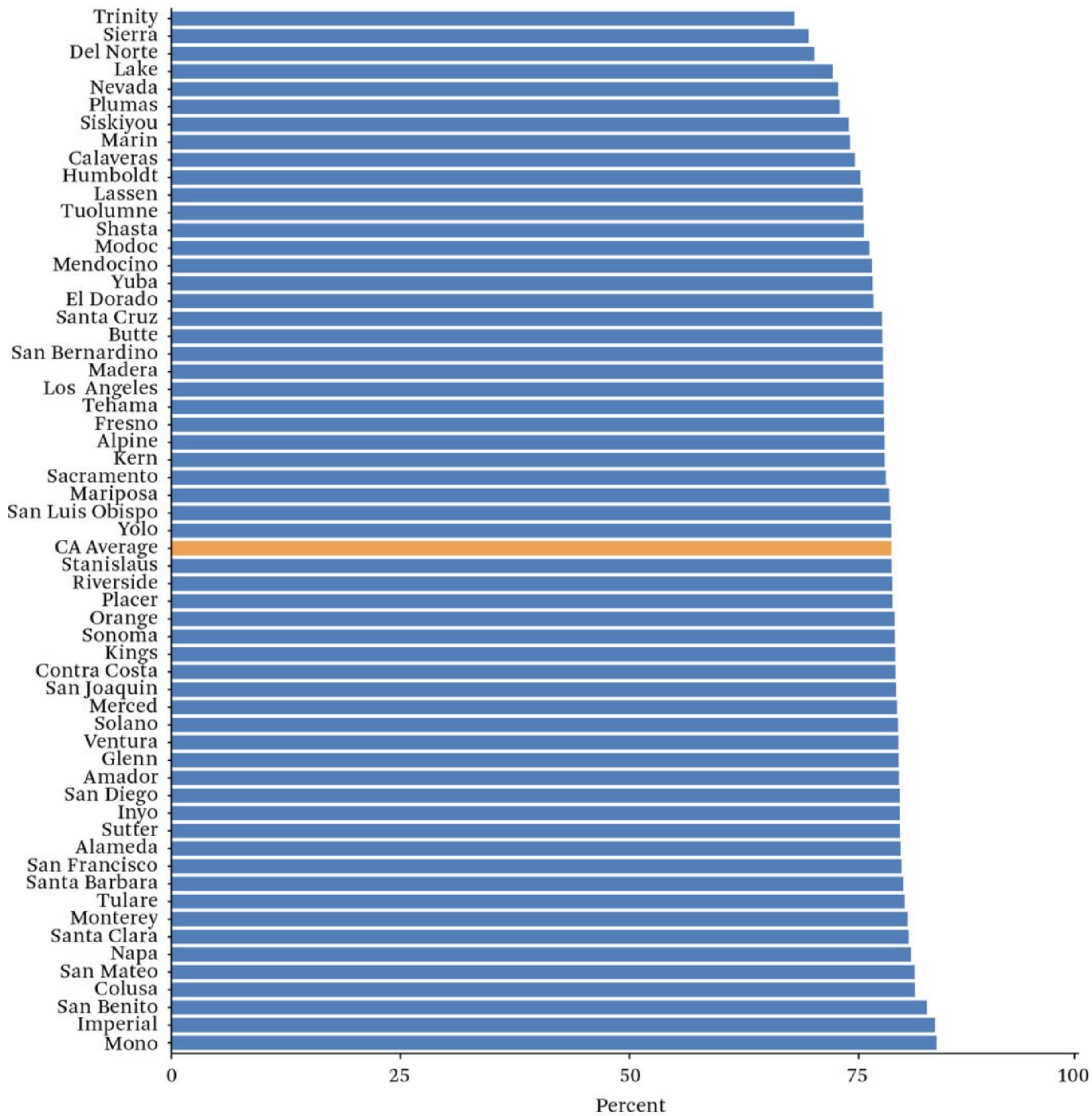
Note: $N = 50$. Source: DOL, author calculations. The dark bars represent the first payment rate across states for 2020Q1 + 2020Q2 (January through June). The light bar represents the US population weighted average. The first payment rate is the number of first claim payments divided by the number of new initial claims.

Figure 2.10: First Payment Rates Across States, Correlations



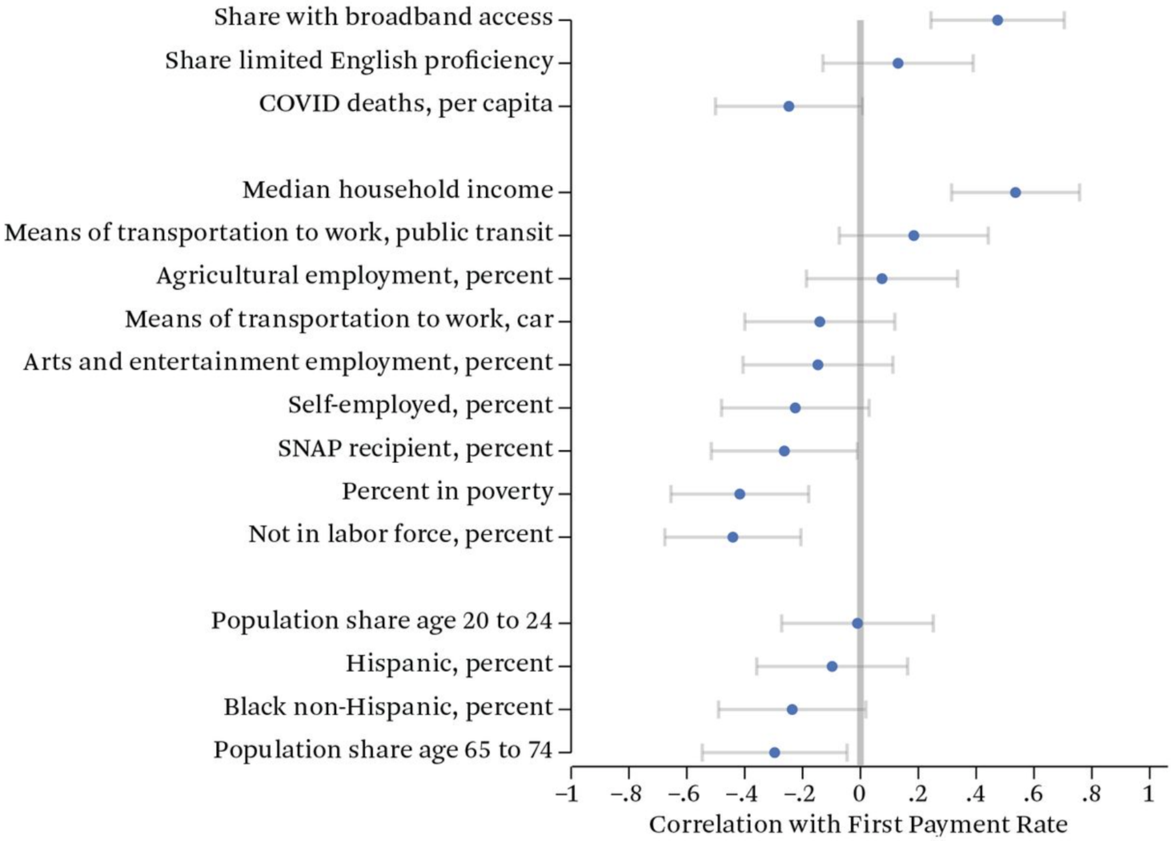
Note: $N = 50$. Source: DOL, ACS, author calculations. Each dot represents the correlation between the covariate and the first payment rate in Q1 and Q2 2020 weighted by population in 2019. All variables are measured at the state level. Error bars represent the 95% confidence interval. The first payment rate is the number of first claim payments divided by the number of new initial claims. For more details of covariates, see Data Appendix.

Figure 2.11: First Payment Rates Within California, County-Level Bar Graph



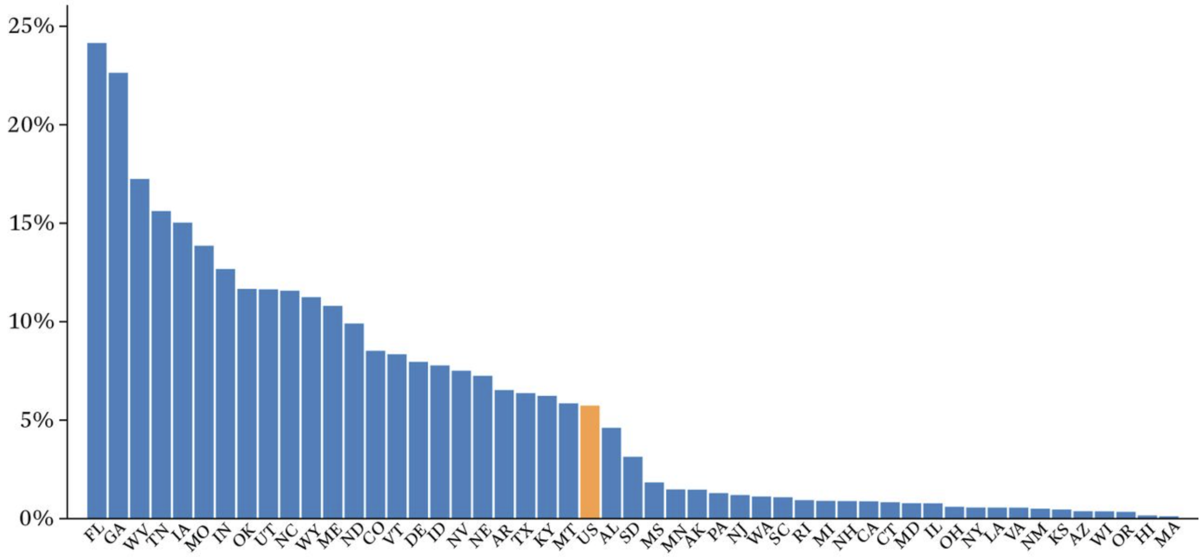
Note: $N = 58$. Source: EDD, author calculations. Each dark bar represents the first payment rate in each county in Q2 of 2020. The light bar represents the California average weighted by population in December 2019. The first payment rate is the number of first claim payments divided by the number of new initial claims.

Figure 2.12: First Payment Rates Within California, County-Level Correlations



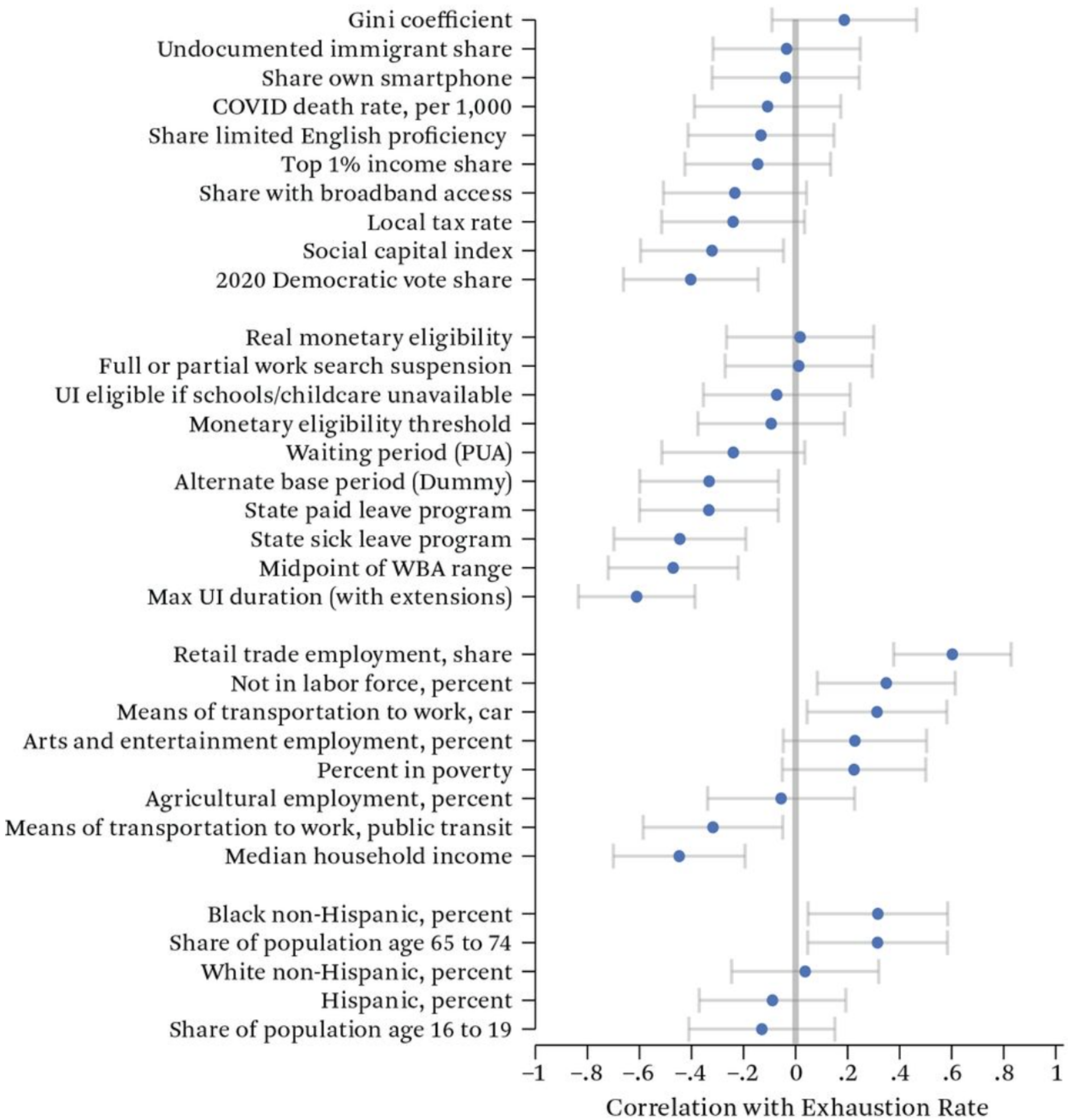
Note: $N = 58$. Source: EDD, ACS, author calculations. Each dot represents the correlation between the covariate and the first payment rate in Q2 2020 weighted by population in 2019. All variables are measured at the county level. Error bars represent the 95% confidence interval. The first payment rate is the number of new initial claimants who received at least one payment divided by the total number of new initial claimants in Q2 2020. For more details of covariates, see Data Appendix.

Figure 2.13: Exhaustion Rates Across States, Bar Graph



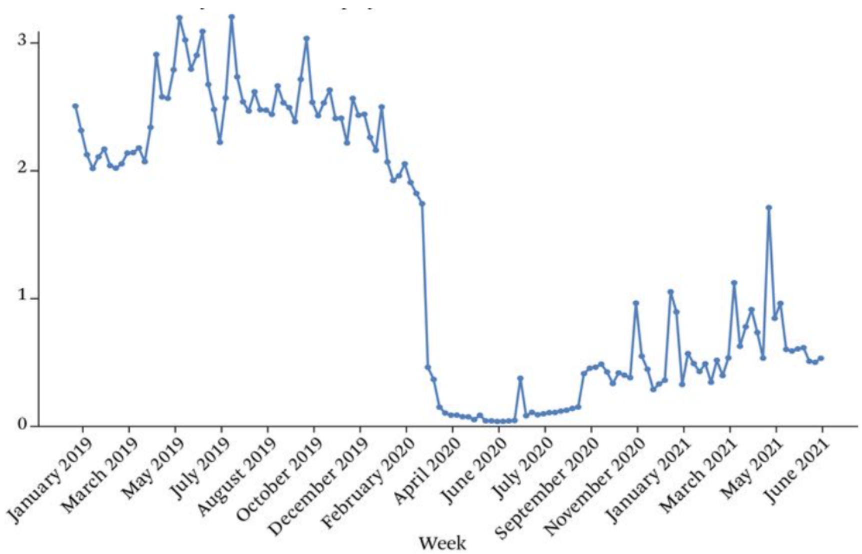
Note: $N = 50$. Source: DOL, author calculations. The dark bars represent the percent of claimants who exhausted their benefits across states for the month of December 2020. The light bar represents the US average weighted by population. The Exhaustion Rate is the number of claimants who exhaust their benefits divided by the number who received payments.

Figure 2.14: Exhaustion Rates Across States, Correlations

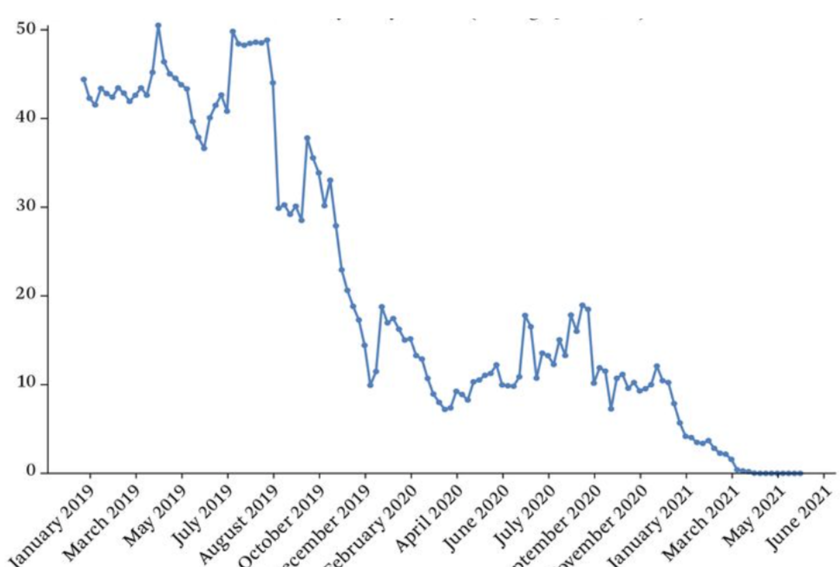


Note: $N = 50$. Source: DOL, ACS, author calculations. Each dot represents the correlation between the covariate and the exhaustion rate in December 2020 weighted by population in 2019. All variables are measured at the state level. Error bars represent the 95% confidence interval. The Exhaustion Rate is the number of claimants who exhaust their benefits divided by the number who received payments. For more details of covariates, see Data Appendix.

Figure 2.15: Exhaustion Rates Within California, Weekly Resolution, 2019-present



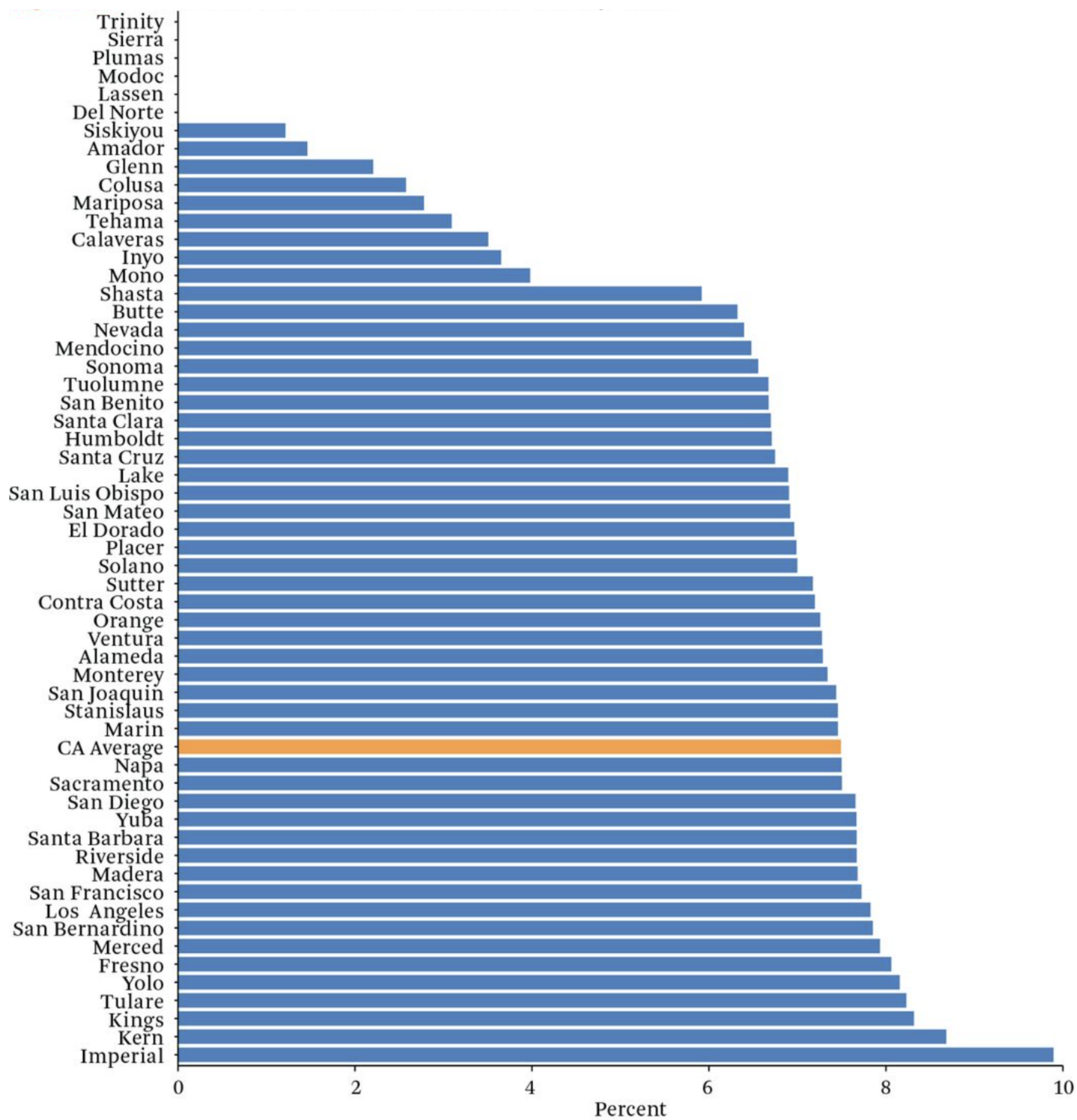
(a) Share of Claimants in California Exhausting as a Share of Weekly Continuing Claimants (from EDD)



(b) Number of Claimants Exhausting as a Share of Weekly Entry Cohort

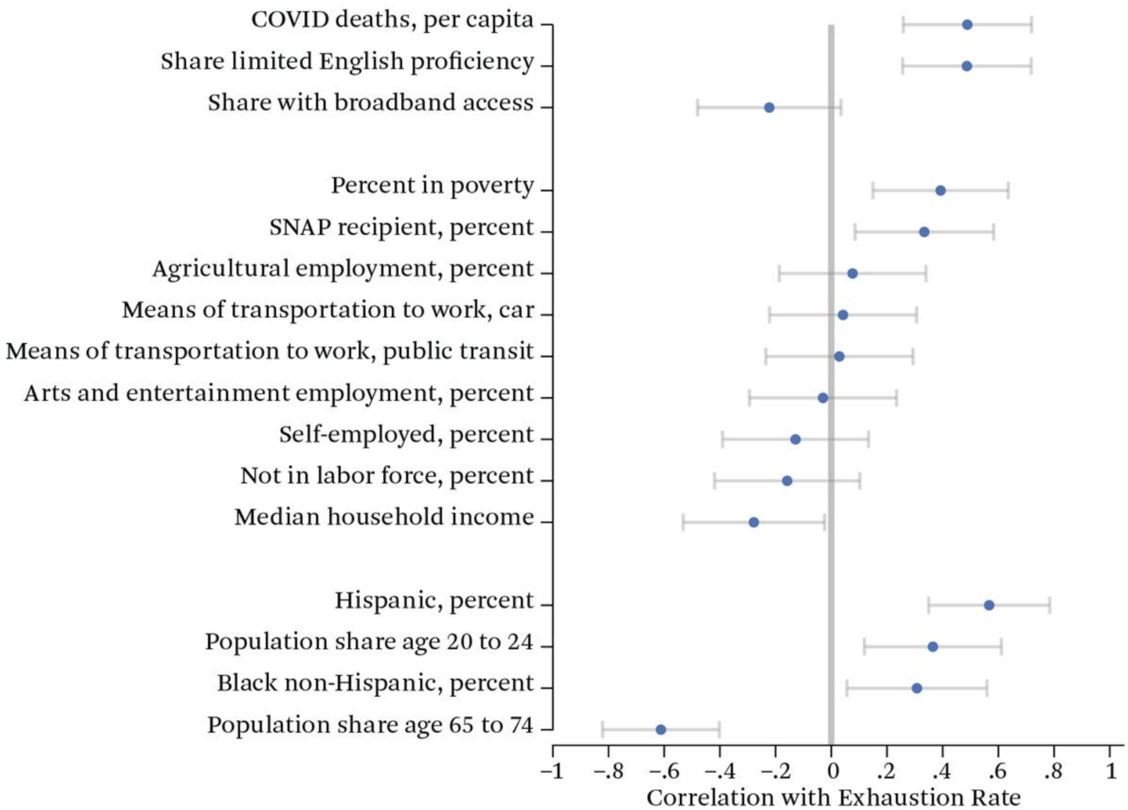
Note: Panel (a): $N = 79$. Source: EDD, author calculations. The line represents the number of claimants who exhausted benefits each week as a percent of the number of continuing claims each week. The figure does not include claimants who only ever received PUA benefits. Panel (b): $N = 79$. Source: EDD, author calculations. The line shows the share of all claimants who entered UI each week and who ultimately received all the benefits they were eligible for before and during the pandemic.

Figure 2.16: Exhaustion Rates Within California, County-Level Bar Graph



Note: $N = 58$. Source: EDD, author calculations. Each dark bar represents the exhaustion rate in each county for claimants whose benefit year began in March of 2020, and who exhausted by the end of Q2 2021. The light bar represents the California average weighted by population in December 2019.

Figure 2.17: Exhaustion Rates Within California, County-Level Correlations



Note: N = 58. Source: EDD, ACS, author calculations. Each dot represents the correlation between the covariate and the exhaustion rate weighted by population in 2019. All variables are measured at the county level. Error bars represent the 95% confidence interval. The Exhaustion Rate is the number of claimants whose benefit year began during the weeks of 3/15/2020 or 3/22/2020 and exhausted benefits by Q2 2021, divided by the number of total claimants whose benefit year began those weeks. For more details of covariates, see Data Appendix.

Table 2.1: Definitions of Key Access Measures, EDD and DOL

Access Measure	Definition in Microdata	Definition in State Aggregates
Application rate	N/A	New initial UI claims in a month divided by the number of newly separated workers in a month.
Initial claims payment rate	Number of regular UI-paid claimants divided by regular claimants at quarterly level. Drop anyone who filed a PUA claim in that quarter from the sample.	First payments for regular UI divided by new regular initial claims, at the monthly level.
Recipiency rate	Number of claimants who claimed regular UI benefits for unemployment experienced in a given week divided by our U6 estimate.	Number of weeks paid across regular UI programs divided by number of (U6) unemployed people in CPS.
Exhaustion rate	Number of exhausted claimants divided by number of people who claimed UI for unemployment in a given week. First, we exclude claimants who have received only PUA payments in the time period of analysis. We code exhaustions when a claimant receives a final payment for a program and does not receive another payment for any UI program for four weeks. For the case of claimants who receive regular and then PUA payments, transitions that occur within four weeks are not coded as exhaustions.	The denominator for exhaustions is calculated by summing the number of people paid in a week for regular UI, including extensions. The numerator is equal to the number of final payments for the final extension in a given time period. During periods with no extension programs, the numerator is final payments for state UI.

Table 2.2: Comparisons of Key Access Measures, EDD and DOL

Period	Measure	DOL Estimate for CA	EDD Estimate
December 2019 (first week)	first payment rate	0.8485	0.78
	recipiency rate	0.2279	0.2098
	exhaustion rate	0.0287	0.0257
	application rate	0.226	N/A
December 2020 (first week)	first payment rate	0.8028	0.75
	recipiency rate	0.9664	0.8500
	exhaustion rate	0.0022	0.0029
	application rate	0.156	N/A

Note: Each cell represents the mean of the measure of access.

Appendices

2.A Measurement of Recipiency Rates

This section details how the recipiency rate is constructed in the DOL and EDD data and describes how they differ. As noted in the main text, we measure the UI recipiency rate as the number of people collecting regular UI benefits divided by the number of U-6 unemployed workers in an area. In the EDD data, the number of people collecting benefits in a week is defined as the number of people who were paid for unemployment experienced in a given week, regardless of when the benefits were paid. This definition more accurately represents the number of unemployed people receiving UI benefits in a given week, and is the natural counterpart to the number of unemployed people as measured in survey data (Bell et al. 2022). In contrast, in the DOL data, the number of people collecting benefits in a week corresponds to the number of payments that were issued that week for regular state UI, PEUC, or EB.[29] Discrepancies can arise when a large number of individuals file and get paid for multiple weeks retroactively. During the crisis, this led to large discrepancies between the two measures, but prior to the crisis, the number of payments issued in a given week was on average similar to the number of individuals receiving payments for unemployment in a given week. See Bell et al. (2022) for further discussion of these two measures.

Our denominator – an estimate of the number of people who experienced unemployment in a week – is derived from CPS microdata. We use the so-called U-6 measure of unemployment, which is broader than the traditional number of unemployed published by the U.S. Bureau of Labor Statistics (BLS), also called U-3. As discussed in our series of unemployment policy briefs, we use this broader measure to account for the fact that workers working part-time involuntarily can receive UI benefits, and that during the crisis, individuals available for work but not actively searching for a job could receive UI benefits.[30]

[29] Georgia and Florida did not report any PEUC claims during 2020.

[30] According to the definition of the U.S. Bureau of Labor Statistics, the U-6 measure of unemployment includes workers who fall under the traditional measure of unemployed (U-3), along with those working part time for economic reasons and with those marginally attached to the labor force. We supplement the U-6

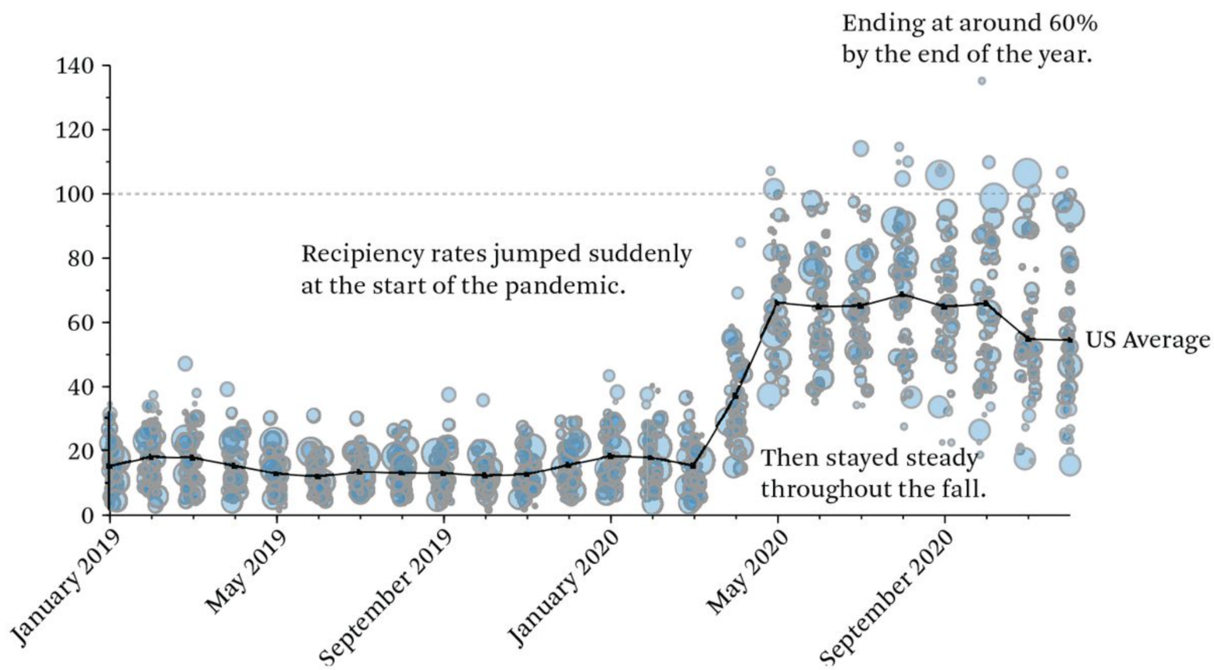
measure to include workers the BLS believes may have been misclassified as employed despite not being at work during the reference week for reasons related to the pandemic (These workers instead should have been classified as "Unemployed on temporary layoff"). We follow the methodology outlined in Question 5 of the December Employment Situation FAQ to adjust our unemployment estimate for these misclassifications https://www.bls.gov/covid19/employment-situation-covid19-faq-december-2020.htm#ques5. In the text, when we refer to using U-6, we are referencing this adjusted version of U-6* which includes these misclassified workers. The BLS does not publish a monthly estimate of U-6 at the state level, so the study team generated a measure of U-6 for California based on the CPS micro data following the definition of the national U-6 measure. Although we use U-6 exclusively for the main analysis, we also calculate state Recipiency Rates using U-3 unemployment and present the figures in the Appendix 2.10. Results using either measure are typically similar and comparisons will be highlighted in the footnotes throughout the Recipiency Rate section.

Table 2.A1: Correlations Among Key Access Measures

	Recipiency Rate	First Payment Rate	Exhaustion Rate
A. Within California (county-level)			
Recipiency rate	1		
First payment rate	0.1589	1	
Exhaustion rate	−0.0149	0.2353	1
B. Across states			
Recipiency rate	1		
First payment rate	0.2884	1	
Exhaustion rate	−0.6394	−0.2551	1

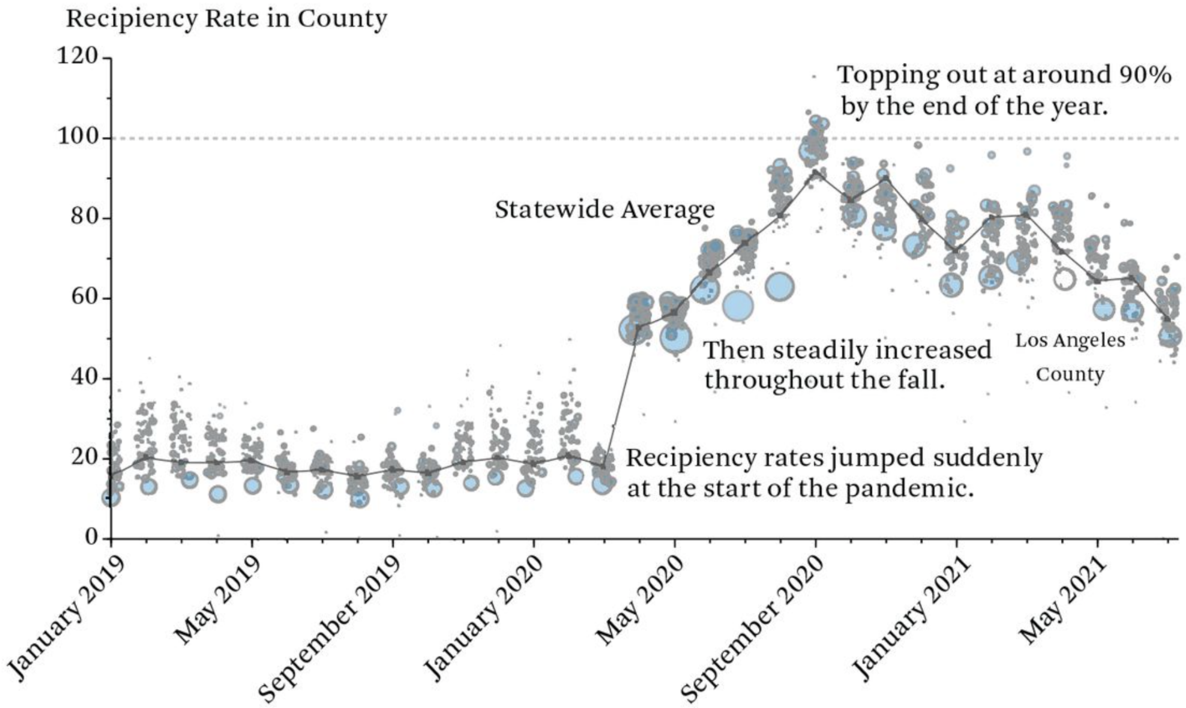
Note: Each cell represents the correlation between the two measures of access, weighted by population in 2019.

Figure 2.A1: Recipiency Rates by State and Month



Note: $N = 1200$. Each dot represents the recipiency rate in each month for each of the 50 US States. The size of the dot corresponds to the population in each state. The line represents the weighted average recipiency rate in the US for each month. The recipiency rate is the number of continuing claims paid from the DOL divided by the number of U6 Unemployed from the CPS.

Figure 2.A2: Recipiency Rates by County and Month



Note: $N = 1798$. Each dot represents the recipiency rate in each month for each of the 58 counties in California. The size of the dot corresponds to the number of U6 unemployed in each county. The line represents the weighted average recipiency rate in California for each month. The recipiency rate is the number of continuing claims paid from EDD divided by the number of U6 Unemployed from the CPS and LAUS.

2.B Measurement of Application Rates

In addition to our baseline specification that normalizes new initial claims by total separations, we also assess robustness of results to two alternative denominators. First, because there are many reasons that an employee would separate from an employer that would not constitute basis for a UI claim – e.g., most quits – we also evaluate robustness to using layoffs from JOLTS as the denominator, rather than the broader category of total separations. Second, whereas the JOLTS data is derived from firm-level surveys, we also constructed an alternative denominator from the CPS worker-level survey. In particular, we evaluated robustness of our correlational results to normalizing new initial claims relative to CPS respondents in a state who reported having been unemployed for less than five weeks. Although the levels of the three measures differ – with total separations showing the largest counts – we did not detect meaningful differences in the spatial correlations when applying different denominator measures.

2.C Measurement of First Payment Rates

In this section we describe two key caveats to the first payment rate measure when applied to the DOL data, and describe how these issues can be remedied with microdata when the analysis focuses on California.

First, in the DOL data, there are substantial payment timing issues. We are only able to look at each state's number of first payments issued in a given month relative to the number of new initial claims filed in that month. To the extent that not all first payments are paid in the month in which the claim was filed, we expect this measure to be relatively noisy at the state level, and this would be a particular problem near the start of the pandemic when long payment lags were common. This timing issue can help to explain the inflated (greater than 100%) first payment rates reported in figure 2.12. This is not an issue in the EDD data where we can see whether each individual received a first payment regardless of when the claim was filed or when the first payment was received.

Second, there are likely cases during the pandemic in which a claim does not result in a first payment under the regular UI program, but the claimant is later able to receive payment under the PUA program. In the DOL data, we are unable to account for these cases as we cannot observe whether the same person applied for, or was paid under multiple programs. In the individual-level analysis from EDD, we drop anyone who ever filed a PUA claim so as to make this measure comparable across time, given that the PUA program did not exist prior to the pandemic. An important avenue for future work, which is beyond the scope of this paper, will be to document the role the PUA program played in expanding access to UI.

2.D Measurement of Exhaustion Rates

This section briefly describes how the count of exhaustions is generated during periods with and without extension programs in the DOL and EDD data. During periods when there are no extensions available, the number of people exhausting is the number of final payments issued for the regular UI program.

During periods when extensions are available, we follow different strategies in the two datasets to count exhaustions. In the DOL data, we infer exhaustions based on the number of final payments made under the program that we believed was the last extension program available to most claimants at the time. For instance, since claimants in California were eligible for Extended Benefits during most of the pandemic, we infer the number of exhaustions based on the number of final payments for EB processed that week.[31] In the EDD data, we improve on this measure by counting exhaustions as the co-occurrence of two separate events. The first event is that a final payment flag was set for a particular UI program, and the second is that another payment does not follow within four weeks.[32] Similar to the other access measurements in this analysis, we only study regular (non-PUA) claimants. However, in the EDD data, in cases where claimants receive their last regular payment and then transit to PUA within four weeks, we do not count them as exhausted because they are still receiving payments – just under a different program. The number of such cases is small, but including them improves the accuracy of our exhaustion rate measurement.

In either dataset, counts of exhaustions should be handled with caution. As pandemic-era extensions have temporarily lapsed and re-started, it is possible that some claimants may be coded as having exhausted, but have in reality been eligible to resume collecting payments after new policies came into effect. Furthermore, even if a claimant exhausts all of

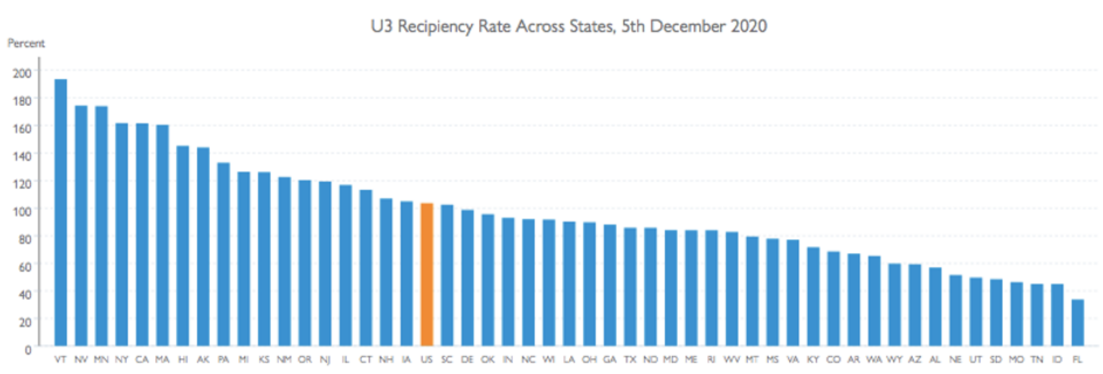
[31] This is a less-than-ideal approximation, as not all claimants are eligible for EB. For instance, our earlier work found that approximately 7 percent of those claimants who would have exhausted regular UI benefits in December of 2020 had PEUC not been extended then would have not been eligible for EB.

[32] In the EDD data, both the final payment flag and gap weeks in payment are based on the week of unemployment.

their benefits available under one benefit year, if their earnings were high enough, they may be able to establish a new claim. Moreover, the data for exhaustion analysis is up to June 2021. Changes in extension programs afterwards will likely affect our estimates.

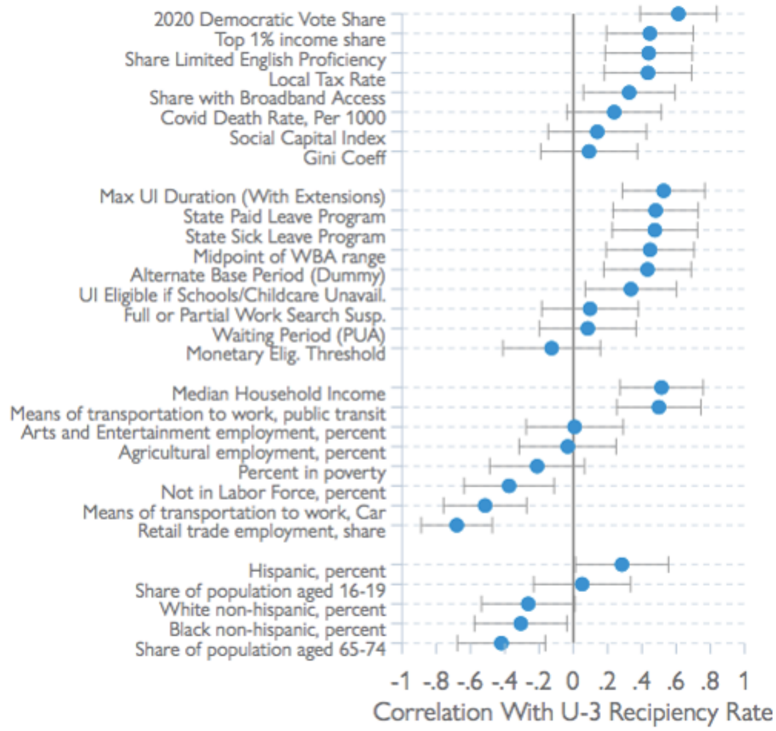
2.E Additional Figures and Tables

Figure 2.E1: U3 Recipiency Rates Across States, Bar Graph



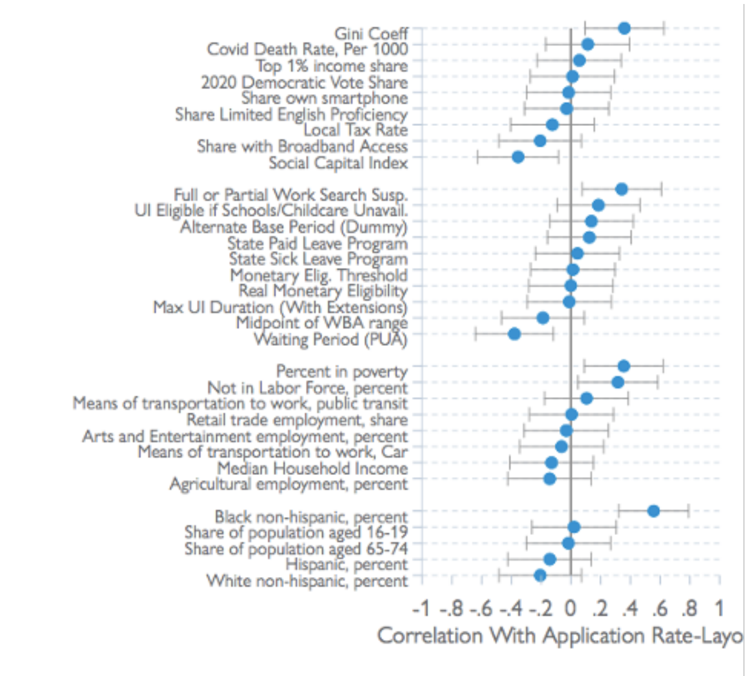
Note: $N = 50$. The blue bars represent the U3 recipiency rates across states for the week of December 5th, 2020. The orange bar represents the US weighted average U3 recipiency rate. The recipiency rate is the number of continuing claims paid from the DOL divided by the number of U3 Unemployed from the CPS. For more details on the recipiency rate please see the text.

Figure 2.E2: U3 Recipiency Rates Across States, Correlations



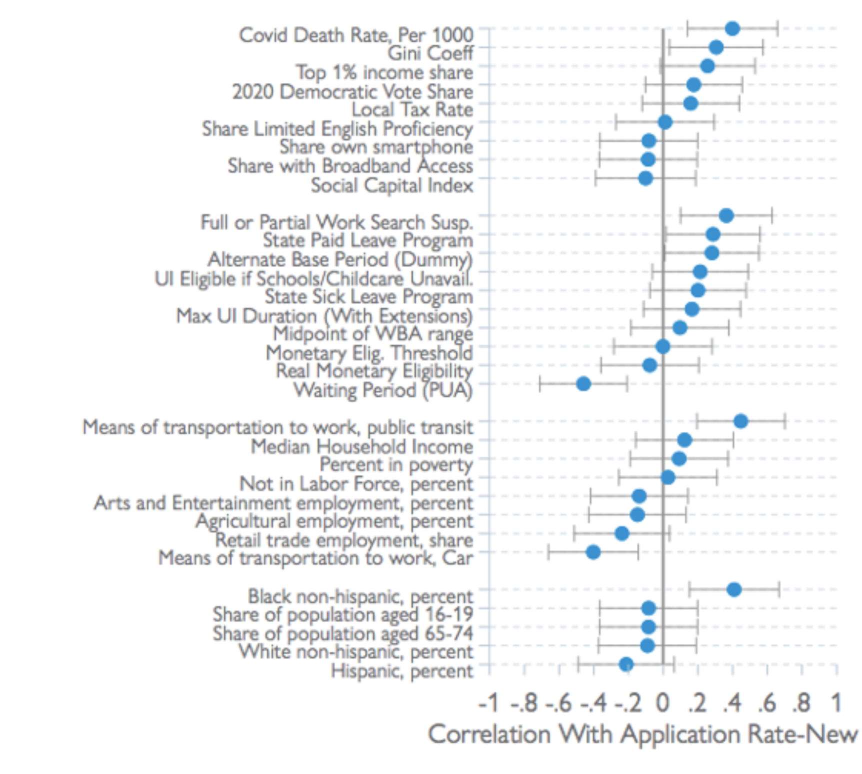
Note: $N = 50$. Each dot represents the correlation between the covariate and the U3 recipiency rate in December 2020 weighted by population in 2019. All variables are measured at the state level. Error bars represent the 95% confidence interval. The recipiency rate is the number of continuing claims paid from the DOL divided by the number of U3 Unemployed from the CPS. For more details on the recipiency rate and the sources of the covariates, please see the text and the Data Appendix.

Figure 2.E3: Layoff Application Rates Across States, Correlations, 2020



Note: $N = 50$. Each dot New UE Application Rates Across States, Correlations, 2020represents the correlation between the covariate and the application rate in December 2020 weighted by population in 2019. All variables are measured at the state level. Error bars represent the 95% confidence interval. The application rate is the number of new UI claims from the DOL divided by the number of layoffs from JOLTS. For more details on the application rate and the sources of the covariates, please see the text and the Data Appendix.

Figure 2.E4: New UE Application Rates Across States, Correlations, 2020



Note: $N = 50$. Each dot represents the correlation between the covariate and the application rate in Q1 and Q2 2020 weighted by population in 2019. All variables are measured at the state level. Error bars represent the 95% confidence interval. The application rate is the number of new UI claims from the DOL divided by the number of people unemployed less than 5 weeks from CPS. For more details on the application rate and the sources of the covariates, please see the text and the Data Appendix.

Table 2.E1: Paid Leave and Liberal Policies

	RR Bivariate	RR Political Controls	EX Bivariate	EX Political Controls
(Intercept)	0.000	0.000	0.000	0.000
	(0.168)	(0.152)	(0.188)	(0.190)
Sick or Paid Leave	0.487 ***	0.090	-0.442 ***	-0.315
	(0.169)	(0.213)	(0.147)	(0.228)
2020 Democratic Vote Share		0.552 ***		-0.177
		(0.155)		(0.146)
N	50	50	50	50
R2	0.237	0.384	0.195	0.210

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

Note: Source: Department of Labor, Cook Political Report.

Table 2.E2: Maximum UI Duration Robustness

	RR Bivariate	RR Econ Controls	RR Econ and Demo Controls	RR Econ, Demo, and Policy Controls
(Intercept)	0.000	0.000	0.000	0.000
	(0.179)	(0.146)	(0.139)	(0.116)
Max UI Duration	0.537 ***	0.362 ***	0.358 ***	0.385 ***
	(0.110)	(0.109)	(0.117)	(0.128)
Economics Index		0.188 ***	0.169 ***	0.003
		(0.052)	(0.055)	(0.101)
Demographic Index			0.070	-0.003
			(0.096)	(0.059)
Real Monetary Eligibility				-0.111
				(0.147)
Midpoint WBA				0.040
				(0.213)
Democratic Vote Share				0.333
				(0.222)
Binary Policy Variables				0.189
				(0.156)
N	50	50	50	50
R2	0.288	0.441	0.455	0.566

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

Note: Source: Department of Labor, American Community Survey.

2.F Data Appendix

To better understand why some areas have benefited more from UI during the pandemic than others, we sourced a variety of county-level and state-level socioeconomic characteristics from public datasets. Our primary source of geographic correlates is ACS 5-year estimate from 2014-2019, the most recent cohort available. The ACS data spans several topics. Variables relating to the economic status of the region include median household income, percent below the Federal poverty line, percent who have broadband internet, percent who do not speak English well, and percent collecting SNAP. Measures of the region's urbanicity include population density per square mile, and median gross rent (either overall or for homes of a specific number of bedrooms). Certain information is available on transportation to work, including the amount of time spent commuting to work as well as the percent commuting via certain modes (such as car, walking, or public transit). We also collected population shares falling in particular age brackets as well as racial categories, and the percent of the labor force employed in each industry (such as food services, retail, finance, etc.). In addition, we collected information on COVID-19 cumulative infections and deaths through early December 2020 in California by county and by state in the U.S. from datasets compiled by the New York Times (New York Times 2021). We collected estimates of the undocumented population as a share of the population from the Pew Research Center (Pew Research Center 2019). Finally, we collected Presidential Democratic vote share from the 2020 election for each state from Cook Political Reports (Cook Political Report 2021).

We also gathered additional covariates at the state level. In particular, we obtained each state's UI policies (compiled February 2021) from the Georgetown Center on Poverty and Inequality (Dutta-Gupta 2021), which includes suspension of UI work search requirements, UI eligibility given unavailable schools and child care, and waiting period for PUA (Pandemic Unemployment Assistance). Measures that reflect UI generosity of each state, like weekly UI benefit amount and maximum UI duration, were also available from GCPI, together with each state's policies on benefits other than unemployment insurance, including the availability of state paid leave programs and sick leave programs. In addition, we also gathered data (compiled January 2014) from Opportunity Insights Data Library (Chetty et al. 2014; Chetty et al. 2020) on selected socioeconomic variables, including Gini coefficient (from core sample in tax records, with income topcoded at $100M in 2012 dollars), top 1% income share (computed using core sample in tax records), local tax rate (from 1992 Census of Government county-level summaries), and Social Capital Index at the CZ level, which we later converted to state level data through weighted averages by population. Finally, we extracted information on alternative base period and monetary eligibility threshold of each state from the 2020 Comparison of State Unemployment Laws written by the U.S. Department of Labor (Department of Labor 2020). We have also spot-checked this against earlier years' data collected by (Gould-Werth and Shaefer 2013).

2.G Demographic Differences in Recipiency Rates

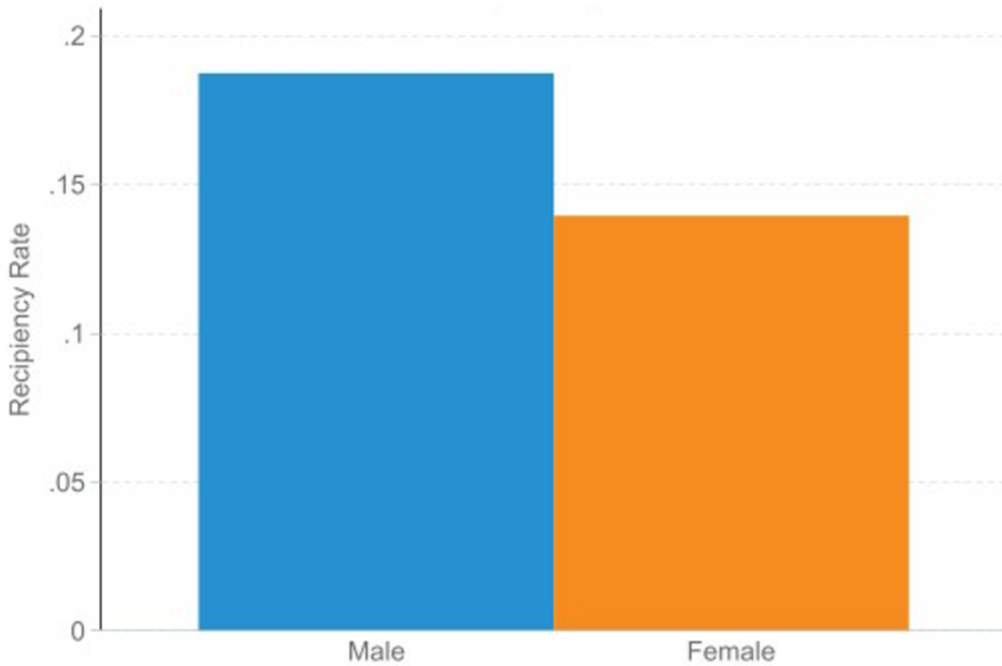
The DOL dataset includes information on the number of claimants by age and gender, and the CPS similarly allows one to measure unemployment by these variables. We are therefore able to combine these two datasets to analyze how recipiency rates, defined as the proportion of the unemployed that is on unemployment insurance, varies by these groups. The DOL claimant data does not contain this information for unemployment insurance extensions, so our analysis must be limited to before the beginning of the pandemic-related extensions that began in March of 2020.

Nationally, some clear differences exist between these demographic groups. Overall, recipiency rates for men tend to be slightly higher than for women, with unemployed men on average in December of 2019 having an 18.73 percent chance of being on UI compared to 13.94 percent of unemployed women.[33] Older unemployed workers tended to have much higher recipiency rates. Those aged 25 to 34 had an average recipiency rate of 16.74 percent, while those aged 55 to 59 were more than double at 33.51 percent. This information is

[33] All national averages for all groups are a population-weighted average across the 50 states.

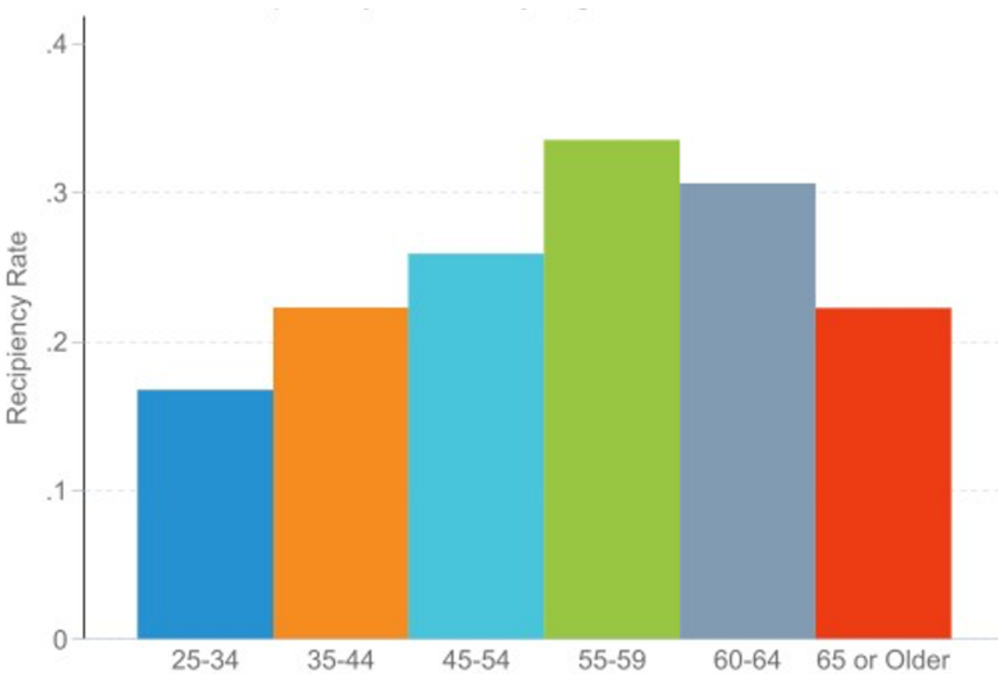
visualized in the bar graphs below.

Figure 2.G1: Male and Female Recipiency Rates, December 2019



Male and Female Recipiency Rates, December 2019

Figure 2.G2: Recipiency Rates By Age, December 2019



These demographic differences can also be analyzed geographically. The below maps display the male and female recipiency rates as well as the recipiency rates by certain age groups per state in December of 2019. These initial results can suggest some interesting regional trends in these recipiency rates, and several hypotheses can be explored that may explain why these geographic differences occur. Due to inconsistencies in how different states ask claimants about their race, our analysis was not able to include an examination of race.

Figure 2.G3: Male Recipiency Rates by State, December 2019

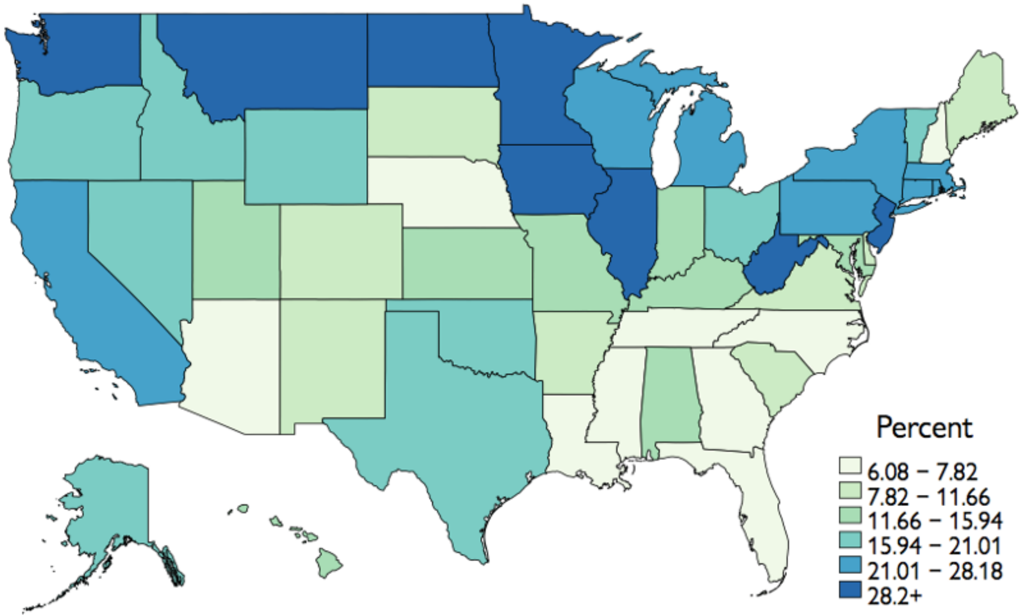


Figure 2.G4: Female Recipiency Rates by State, December 2019

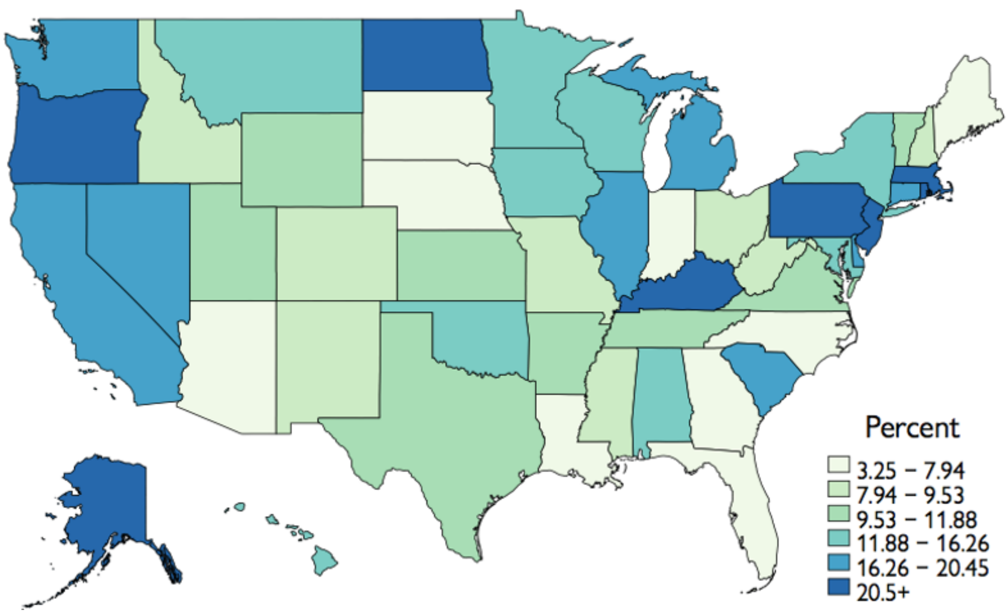


Figure 2.G5: Recipiency Rates Among 25-34 Year Old by State, December 2019

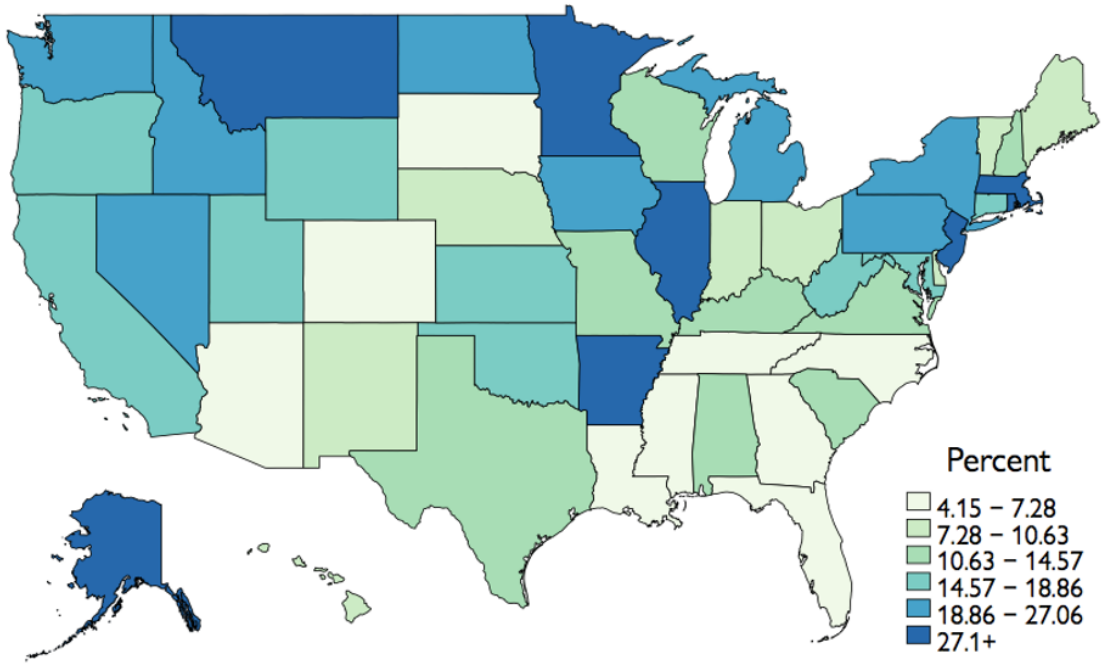
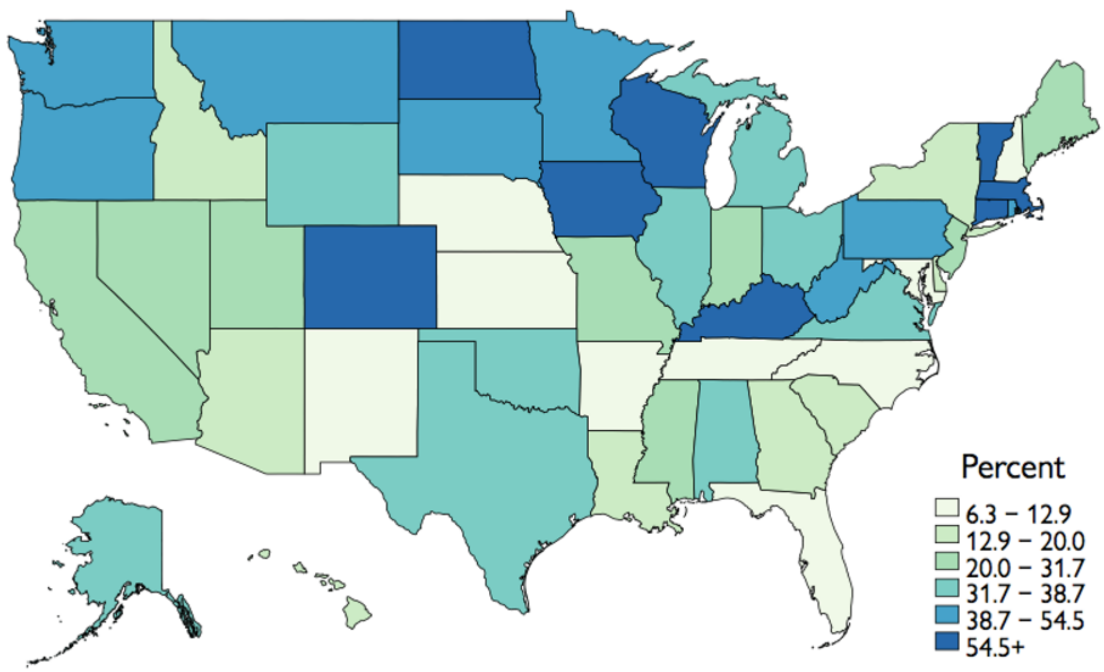


Figure 2.G6: Recipiency Rates Among 55-59 Year Old by State, December 2019



2.H Demographic Differences in First Payment Rates

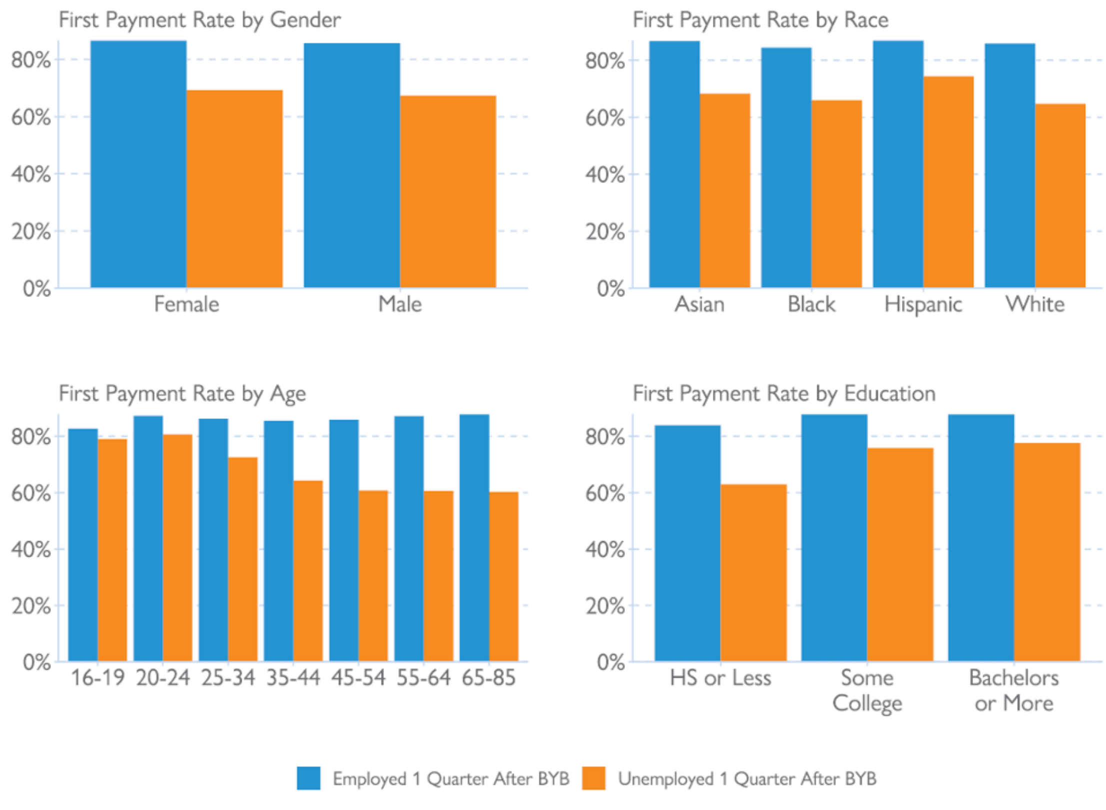
Failure in receiving the first UI payment after unemployment is a challenge for consumption smoothing. This is a greater challenge if the unemployed worker is unable to find a job for extended periods. To further investigate the case, we expanded the first payment measurement within-California to be conditional on employment status after filing the UI claim, and we also derived the first payment rates for various demographics to check for potential unevenness.

The employment status one quarter after the beginning of the benefit year (BYB) of UI claimants is based on the UI Base Wage data, which includes quarterly information on wages and employer firms for UI-covered employees. We follow the employment status of the claimants with new initial claims in the second quarter of 2020 into the third quarter of 2020.

A close look at all of the figures of demographic categories shows that claimants who remained unemployed one quarter after establishing their claim are less likely to be paid. Claimants with insufficient earning history have poorer connections with the labor market. They are less likely to be paid UI benefits, and at the same time, less likely to find a job in the middle of a recession.

Focusing on the heterogeneity of first payment rates within employment status, we do not observe significant disparity among claimants who are employed one quarter after BYB. However, differences within unemployed claimants one quarter after BYB are more outstanding. Particularly, we see that younger unemployed claimants are more likely to receive the first payment compared to older workers. This result is unexpected because even among unemployed claimants, we assume the older claimants to be more likely to receive the first payment due to stronger work history. Understanding these disparities is potential future research.

Figure 2.H1: First Payment Rates by Demographic Group, December 2019



Chapter 3

Estimating the Disparate Cumulative Impact of the Pandemic in Administrative Unemployment Insurance Data (with Alex Bell, T.J. Hedin, Peter Mannino, Carl Romer, Geoffrey Schnorr, and Till von Wachter)

3.1 Introduction

The COVID-19 pandemic had a staggering impact on the labor market in the United States and many other countries, with extensive job loss and long-term unemployment (LTU) affecting particularly less advantaged workers. Standard measures such as the unemployment rate capture the state of the labor market at a point in time, typically in a given month. Yet, research has shown that events such as a job loss or an unemployment spell can affect workers' employment and wage outcomes for a long time, especially in recessions (Davis et al. 2011). As a result, the recent employment history of workers will likely influence their job search activity, layoff risk, labor supply, and training activity even once they are employed. Cumulative measures based on longitudinal data that reflect workers' recent employment history not only better measure which workers and communities were most impacted by a recession but can also provide a more comprehensive view of the state of the labor market and likely employment and earnings dynamics.

Survey datasets typically used to assess the labor market, such as the Current Population Survey (CPS), are limited in their ability to capture workers' labor market histories. Administrative data from the unemployment insurance (UI) system on program use and earnings follow the entire covered workforce over time and hence make it possible to generate cumulative measures of labor market health. These data also allow for statistical analyses across more detailed geographical units and demographic groups. Furthermore, cumulative measures of incidence of UI claims (on the extensive margin) and long-term duration of UI receipt (on the intensive margin) provide import-ant insights into the extent and differences of UI use in the population.

The California Policy Lab (CPL) at the University of California has obtained access to California's administrative UI records through a partnership with the state's Employment Development Department (EDD). These data were used for a series of reports that analyzed the state of the California labor market throughout the COVID-19 pandemic and provided a deeper understanding of the UI system and its data. This article briefly describes the administrative UI data, presents estimates of the cumulative impact of the COVID- 19 crisis over its first year at the extensive and intensive margin, and compares how it differed for workers of various demographic groups. We find that during the first year of the crisis, 30 percent of the labor force filed a UI claim, over 46per-cent of recipients spent more than 6 months on the program, and the mean work time lost was 8 weeks. Less advantaged workers and counties saw much higher rates of claiming and LTU.

3.2 Standard Measures versus Cumulative Measures

Standard measures of employment treat workers who remained employed through-out an entire period the same as workers who lost their job, spent time in unemployment, and were recently reemployed. This would fully capture the state of the labor market if job losers were in the same economic position after jobs loss as they were before, thereby ignoring both search dynamics and "scarring" effects from job loss. Yet, job losers have a higher likelihood to change jobs again (e.g., Krolikowski 2017), switch industries or occupations (e.g., Jackson 2021), or suffer from repeated job loss (e.g., Stevens 1997) and unemployment (Bell et al. 2021). Workers losing stable jobs at good employers experience substantial future earnings declines (Davis et al. 2011), a pattern accentuated for workers experiencing longer unemployment spells (Schmieder et al. 2016) . Hence, the recent work history of the current labor force can aid our understanding of labor market dynamics. By capturing the overall earnings losses afflict-ing a community or group of workers, the total extent of job loss or LTU will also better capture the cost of recessions and can be a use-ful indicator of where government support is most needed. Finally, cumulative measures can be particularly helpful for characterizing the cost of recessions for marginalized groups who often experience above-average levels of unemployment. Differences in unemployment rates at points in time will accumulate over long periods to create larger absolute differences in total unemployment between groups.

Such cumulative measures are particularly salient in the context of the UI program. UI recipients are of interest in their own right, since they can experience large earnings losses and long unemployment spells (e.g., Jacobson et al. 1993) and are often the focus of retraining or job search assistance pro-grams. At the same time, cumulative measures can better characterize the overall reach of the UI system, which has been long criticized for low coverage, particularly among less advantaged workers (e.g., Bell et al. 2023).

3.3 Description of Administrative UI Data

We generate cumulative measures of unemployment over the COVID-19 pandemic using longitudinal UI administrative records from two datasets. The initial claims files contain the universe of new and additional claims for UI submitted by workers in California and include detailed demographic and geographic characteristics. Figure 1 illustrates the large rise in weekly new initial claims in the first months of the COVID-19 pandemic. The figure also shows that starting in mid-2020, a large and growing proportion of initial claims came from UI claim-ants that had found a job, stopped receiving benefits, and then returned to UI via a so-called additional claim.[1] This large amount of churn, discussed in the CPL's UI reports, underscores the importance of deduplicating initial UI claims when assessing the total impact of the crisis.

The continuing claims files contain the records of all UI claims paid each week, including the amounts paid and the weeks of unemployment for which those claims were paid. The continuing claims series published by the Department of Labor (DOL) reports payments by the week they were processed. Due to administrative delays or retroactive claims, payments are often processed several weeks after the week of unemployment that the payment is for. Moreover, not all processed claims are actually paid (for example, if a worker reports earnings above a threshold for the particular week). An advantage of the administrative data is that we can account for differences in timing and fluctuations in benefit denial by counting the number of individuals that received benefits for a given week of unemployment, shown in Figure 2 over the course of the COVID-19 pandemic.[2]

[1] In contrast, the publicly available data from the DOL only provide new and additional claims by month.

[2] The claims-by-week-of-unemployment graph comes with an additional adjustment to account for retroactive claims as outlined in Bell et al. (2021).

3.4 Cumulative Measures of Unemployment

We exploit the longitudinal nature of the administrative data just described to create two cumulative measures of unemployment along the extensive and intensive margins.

3.4.1 Extensive Margin: Unique UI Claimants as a Share of the Pre-pandemic Labor Force

The first cumulative measure of labor market disruption from the COVID-19 pandemic is the share of the pre-crisis labor force that applied at least once for regular state UI between March 2020 and March 2021.[3] We interpret this as a measure of the total number of workers who experienced job loss or hours reductions during the crisis.[4] The advantage of administrative data over surveys like the CPS or the Job Opening and Labor Turnover survey is that the administrative data can be used to get a count of unique individuals affected by a recession over long periods of time. In the publicly available DOL data, summing the monthly new initial claims series could provide a closer estimate of our measure but would still suffer from some duplication from claimants who file multiple claims.[5] Crucially, the publicly available data do not provide the new initial claims series with demo-graphic or detailed geographic breakdowns.

Column 1 of Table1 shows the percent of the February 2020 labor force in California that applied for regular UI benets through March 2021.[6] It also provides this estimate for selected demographic groups. Over the first year of the COVID-19 crisis, a staggering 31

[3]While fraud has been widely reported in the PUA program, this was much less of an issue for the regular UI claims that we focus on here.

[4]This interpretation is reasonable in our context, as UI recipiency rates among the unemployed reached nearly 90 percent in California during the pandemic (Bell et al. 2021). Prior to the pandemic, recipiency rates in California were only around 20 percent, such that our measure would yield an important metric for the extent of UI claims.

[5]This could be the case if the EDD website experienced crashes from high usage and users resubmitted their claims because they were unsure if their first claim was received. Alternatively, it could happen if employers submit claims on an employee's behalf and an employee submits a claim themselves (Cajner et al. 2020). As shown in Figure1, the duplication issue would be substantial for all initial claims.

[6]Unlike in other states, in California PUA claimants did not first need to file and be rejected for regular UI, and hence, they do not affect these numbers.

percent of the California labor force applied for regular UI benets. Table1 also shows that the accumulated disparity between groups is substantial and much larger than what would be implied by a comparison of, say, monthly unemployment or UI claim rates. In particular, we find that less educated workers have been hit hardest over the course of the pandemic, with over half of workers with a high school degree or less having applied for regular UI benets in the first year (over three times the ling rate of workers with a bachelor's degree), and that women led for UI at a higher rate than men.

3.4.2 Intensive Margin: Long-Term Unemployment Rate

The second cumulative labor market indicator we calculate is the share of UI claimants who have received more than 26 weeks of unemployment benefits in the first year of the crisis. To account for the high degree of churn seen in the data, our measure is based on the total time spent on UI in a year, ignoring temporary returns to employment. In contrast, the rate of LTU is defined by the Bureau of Labor Statistics (2022) as the share of the unemployed who have been jobless for over 26 weeks. Again, an advantage of our measure is that it captures the extent of chronic loss of employment over a longer time period. Moreover, the administrative UI data allow us to measure the incidence of long unemployment durations at more detailed geographic and demographic levels.

One potential limitation compared to the CPS is that UI-based measures will depend on the maximum number of weeks available to claimants. During the pandemic, the federal PEUC program and the state Extended Benefits programs made the maximum benefit duration well over 26 weeks. In regular economic times, considering mean total UI duration or a lower threshold is more appropriate.[7]

As a particularly salient use case, Figure3 presents county-level correlations between the LTU rate calculated from the UI data and a series of economic and demographic attributes

[7]7 It is also the case that not all claimants qualify for the maximum benefit duration, which could bias the measure across different types of workers. In principle, the microdata could be used to identify the subset of claimants who qualify for the whole duration, and the rate of LTU could be calculated using that sample.

from the 2019 American Community Survey (ACS). It demonstrates that areas that were already vulnerable before the crisis were still affected by above-average incidence of LTU in the year leading up to March 2021. For example, counties with more-limited English speakers were also counties where claimants were more likely to experience LTU, as were counties with higher population densities.

3.4.3 Combining the Intensive and Extensive Margins: Weeks Lost to Unemployment

Finally, the administrative UI data can be used to generate an indicator of the cumulative labor market impact of the COVID-19 pandemic by combining extensive and intensive margin measures. We calculate the average number of weeks that members of the California labor force spent on UI as

$$E(time\ on\ UI) = Pr(UI\ claim) \times E(time\ spent\ on\ UI \mid UI\ claim), \tag{3.1}$$

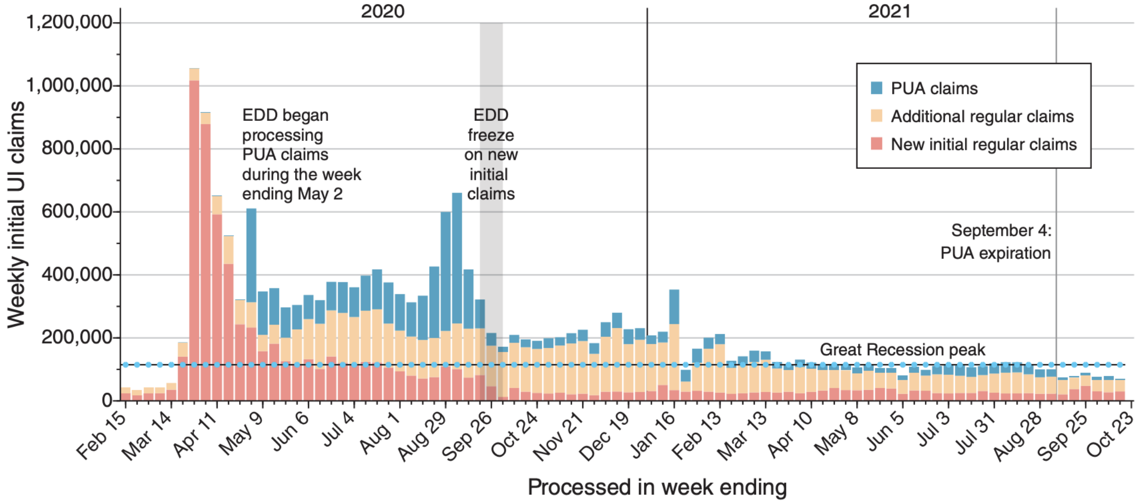
where the $Pr(UI\ claim)$ is the extensive margin measure discussed in Section 3.4.1 and the $E(time\ spent\ on\ UI \mid UI\ claim)$ is an alternate intensive measure representing the mean number of weeks a claimant received UI benefits. In Table1, columns 1–3 show the combined measure and both constituent parts. Column 3 indicates that in the first year of the pandemic, the average member of the labor force spent nearly two months receiving regular UI benefits. Across the demographic groups, less educated, female, and younger members of the labor force experienced more weeks on UI than other demographic groups.

3.5 Conclusion

Administrative datasets from the UI system can be a valuable source of insight into the labor market, but they are not often made available to researchers. In this paper, we put forward three cumulative measures of the labor market that can be calculated from UI administrative data and used them to measure the impact of the COVID-19 pandemic in California. Along with generating these measures from the UI micro-data, the CPL published a series of briefs using the UI data to better understand the UI system.8 Caution should be applied when extending these analyses and results beyond California, as different US states may not only have weathered the pandemic in different ways but also have very different UI systems (e.g., Bell et al. 2021). Future collaboration between state agencies and researchers that unlock these state-level administrative datasets would improve our understanding of labor markets and UI systems across the United States.

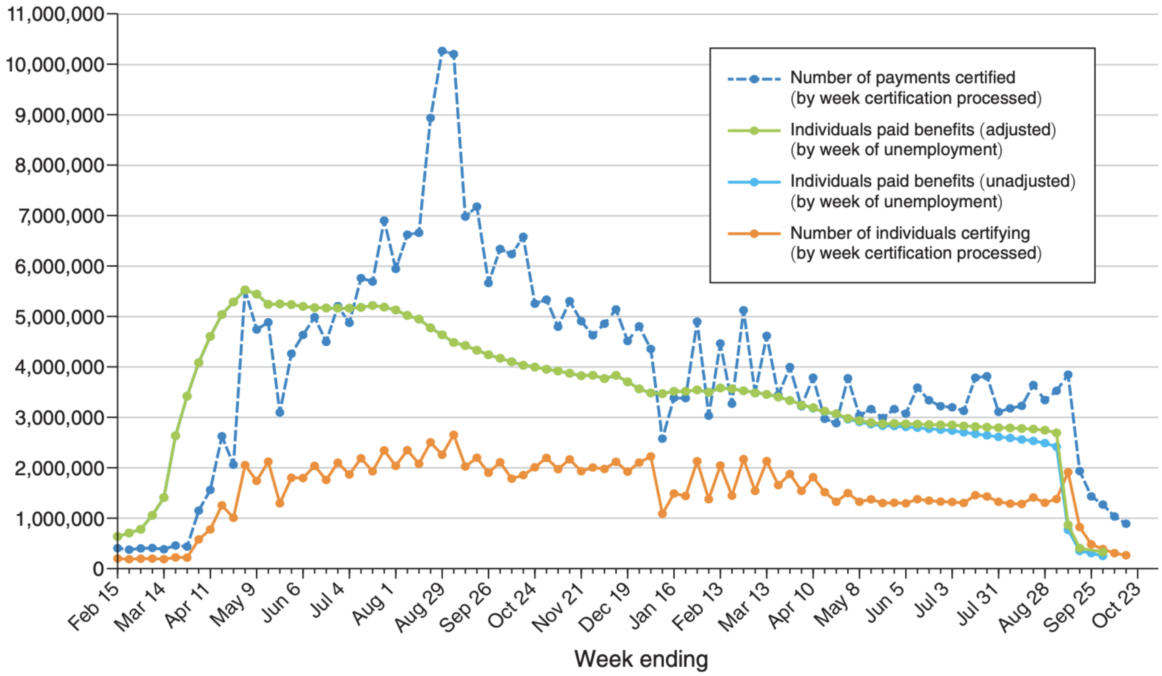
3.6 Figures and Tables

Figure 3.1: Weekly Initial UI Claims (including PUA) during the COVID-19 Crisis in California (February 15, 2020–October 16, 2021)



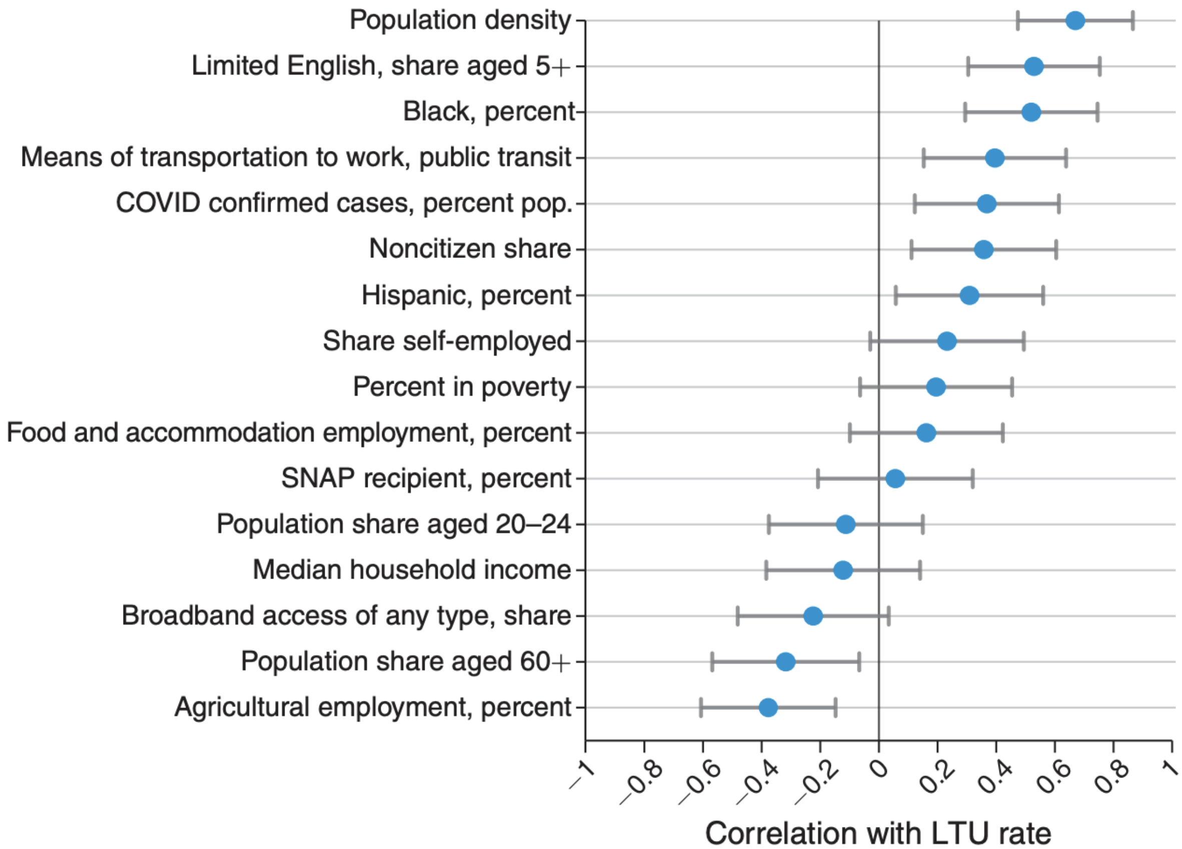
Note: "New initial regular claims" includes new initial claims for regular state UI. "Additional regular claims" includes additional claims for regular state UI and additional claims for extension programs. "PUA claims" are claims filed for the temporary Pandemic Unemployment Assistance (PUA) program that was established to provide benefits to workers not usually eligible for regular UI. This figure does not include transitional claims, as the DOL does not include them in their headline initial claim number, nor do they represent flows into the UI system. Source: California Employment Development Department (EDD)

Figure 3.2: Total Number of Individuals Paid Benefits by Week of Unemployment, Total Number of Individuals Certifying for Benefits by Week of Certification, and Total Number Payments Certified by Week of Certification (all claims)



Note: "Number of payments certified" refers to the number of payments that were certified during a given week (the common definition of continued UI claims). "Number of individuals certifying" refers to the number of people that certify for UI benefits in a given week (which is roughly half of the number of payments because certification is biweekly in California). "Individuals paid benefits by week of unemployment" refers to the number of individuals paid benefits for the week they experienced unemployment either adjusting for historical lags in claiming behavior ("adjusted") or not ("unadjusted"). This figure includes claimants receiving benefits for regular UI, Pandemic Unemployment Assistance (PUA), and Pandemic Emergency Unemployment Compensation (PEUC). Source: California Employment Development Department (EDD)

Figure 3.3: County-Level Correlations between Long-Term UI Receipt in the First Year of the COVID-19 Crisis and County Characteristics



Sources: California Employment Development Department (EDD), ACS data via Ruggles et al. (2021)

Table 3.1: Cumulative Measure of UI Claims at the Extensive and Intensive Margin during the First Year of the COVID-19 Crisis

	Unique claimants as percent of precrisis LF (1)	Mean UI duration among recipients in weeks (2)	Time lost due to pandemic (3)	Long-term unemployed among recipients (4)
Statewide	31.2	25.9	8.1	46.5
Panel A. Age group				
16–24	51.8	24.5	12.7	42.4
25–34	35.0	26.2	9.2	47.3
35–44	25.5	26.0	6.6	46.9
45–54	24.8	25.3	6.3	45.0
55+	27.4	27.0	7.4	49.7
Panel B. Race				
Asian (non-Hispanic)	NA	26.6	NA	48.1
Black (non-Hispanic)	NA	29.2	NA	55.7
Hispanic	NA	24.8	NA	43.2
White (non-Hispanic)	NA	25.2	NA	44.2
Panel C. Education				
High school or less	47.9	27.0	13.0	49.6
Some college	30.6	25.7	7.9	46.8
Bachelor's or more	13.2	23.9	3.2	41.3
Panel D. Gender				
Women	34.6	26.4	9.1	47.9
Men	28.3	25.3	7.2	45.0

The unique claimants and the long-term unemployed are totals from March 2020 to March 2021. The pre-crisis labor force ("LF") was calculated from the February 2020 CPS. Figure excludes all PUA claims. Race/ethnicity are not included in columns 1 and 3, because the data are collected differently between the UI data and the CPS, making comparisons difficult. Source: California Employment Development Department (EDD)

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