Discussion Paper Series

IZA DP No. 18776

July 2026

The Mitigating Effects of Unemployment Insurance on Short-Run Mortality

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ISSN: 2365-9793

IZA DP No. 18776 July 2026

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Abstract

Unprecedented increases in unemployment insurance (UI) claims and dramatic changes in UI delivery across the U.S. in recent years stress the importance of understanding the wide-ranging effects of UI receipt. We estimate the short-run effect of UI on suicide and drug overdose mortality following mass layoff events. Using quarterly county-level data from 1995-2010 and leveraging interactions between state-specific UI generosity and mass layoff events, our two-way fixed effects models and event studies show that more generous UI reduces suicide at the county level. These mitigation effects coincide with otherwise large spikes in mortality following mass layoff events in local communities. We find no effect on drug overdose deaths.

JEL classification

J65, H53, I38

Keywords

unemployment insurance, job loss, mortality, suicide, drug abuse

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1 Introduction

Unemployment insurance (UI) plays a key role in assisting workers displaced by job loss. Current research indicates that UI provides the largest transfers to job losers relative to other social safety net programs in the U.S. (East and Simon, 2022). Moreover, the documented follow-on effects of UI receipt extend beyond those captured by standard economic indicators. For instance, recent evidence has shown that UI affects health (Kuka, 2020), housing (Hsu, Matsa, and Melzer, 2018), college enrollment (Barr and Turner, 2015), fertility, and family stability (Lindo, Regmi, and Swensen, 2023).

In this study, we investigate the degree to which UI benefits offset the documented spike in mortality following job loss. Specifically, we estimate the effect of state-specific UI generosity on death by suicide and drug overdose following mass layoff events over the period 1995 to 2010. These causes of death have been shown to increase with job displacement, and have been the primary drivers of increased death rates among White working class individuals over recent years. We measure these deaths using restricted-use individual mortality records. Our empirical approach utilizes two primary sources of variation: mass layoff events at the county level and state-level UI generosity. We leverage the timing of mass layoff events interacted with state-specific UI generosity to explore the extent to which UI mitigates the adverse effect of mass layoffs on county-level suicides and overdose deaths in communities across counties in the U.S. We show results from both difference-in-differences-type models and event studies, the latter confirming timing that points strongly to a causal relationship.

Consistent with findings in the existing literature, our estimates show large spikes in suicide for prime-age workers in the quarter following a mass layoff event. Of primary interest, our estimates show that UI mitigates the increased mortality: a one standard deviation increase in maximum weekly UI benefits (approximately $100) offsets more than a quarter of the mean increase in county-level suicide rates caused by a mass layoff event. Similar spikes and subsequent mitigation effects are not apparent for drug overdose deaths suggesting different short-run causal pathways from job loss to these two mortality outcomes.

Our dynamic analysis demonstrates that the effects on suicides are apparent following, but not prior to, the mass layoff events. We also find that these effects are most evident for males, White, and middle-aged individuals, a demographic group for which there are established suicide mortality concerns. We also document particularly stark effects on suicide in counties with more intense mass layoff events.

Our results add to the thesis that there are moderating effects of income support policies on the relationship between local area labor market disruptions and short-run mortality. Our results reflect the direct impacts on individuals and households affected by layoffs and UI policies but also capture potential spillover effects on the local community associated with these layoff events. We view this approach as complementary to individual-level analysis of those directly affected by job loss; the approach in our paper captures wider-ranging effects, which are potentially more revealing when considering more broadly targeted policy interventions. In a recent example at the state-level, Dow et al. (2020) analyze the effects of the minimum wage and the earned income tax credit on suicide and drug overdose deaths. Similar to our approach, they use variation in state labor market policies and find mitigating effects on suicide but not drug overdose deaths. This is the same pattern of effects on these two outcomes in our analysis of UI.

Our findings are also consistent with the existing evidence that UI can reduce the often-devastating effects of job loss. In particular, our results add credible evidence that policies intending to help households maintain stable incomes during economic hardships can also help avoid the costliest adverse consequence that follows job loss.

In a related paper, Wu and Evangelist (2022) explore the effects of UI generosity on opioid overdose mortality following job loss. Our study is similar to theirs in that we also leverage county-level layoff events interacted with state UI generosity to identify mortality effects, but differs in important ways. First, we consider two mortality outcomes: death by suicide and death by drug overdose. Second, we consider finer granularity in the timing of effects. Specifically, we use quarterly mortality and layoff data, while Wu and Evangelist (2022) use annual mortality data and rolling two-year mass layoff rates. This allows us to focus more attention on the timing of mass layoff events and use event studies at the quarterly frequency to make claims about the causal nature of the effects. Unlike Wu and Evangelist (2022), when looking at drug overdose mortality, we find neither direct effects of layoff nor mitigating effects of UI using our quarterly data. Our findings for suicide mortality are new, and provide evidence of a previously-undocumented channel through which UI mitigates negative outcomes associated with job loss.

In earlier work, Cylus, Glymour, and Avendano (2014) leverage variation in the annual state unemployment rate interacted with state-level UI generosity to estimate effects of UI on suicide. They find that the interaction is negatively related to suicide rates, though estimated effects are small in magnitude. Importantly, the unemployment rate is likely endogenous to suicide as it may reflect labor supply decisions that also affect the probability of suicide. For example, an individual experiencing serious health challenges may be both more likely to be unemployed and have a higher likelihood of suicide. Our approach instead exploits plausibly exogenous variation in the quarterly timing of mass layoff events measured at the county level.

2 Background

Our work contributes to a large literature documenting the detrimental effects of economic turmoil on health and well-being. Research in this area explores the links between health and macroeconomic cycles (e.g. Ruhm 2000; Stevens et al. 2015; Ruhm 2015; Lindo 2015 and Miller et al. 2009), economic crises (e.g. Ruhm 2016), initial labor market conditions (e.g. Von Wachter 2020), import competition (e.g. Adda and Fawaz 2020), and other broad and localized economic shocks.

Within this literature, our study is closely related to a large number of studies analyzing the effects of job loss on individuals and households. Existing evidence highlights that job loss is an extremely stressful life event that has both short- and long-term consequences for health and well-being. Most well-identified studies in this area leverage involuntary job loss typically due to mass layoffs or plant closures—demand-side factors that are arguably exogenous to physical and mental health outcomes that may also affect labor supply-side decisions.[1] Such studies document adverse effects of job loss on mortality (Sullivan and Von Wachter 2009; Eliason and Storrie 2009; Classen and Dunn 2012; Browning and Heinesen 2012; Bloemen, Hochguertel, and Zweerink 2018), mental health (Tefft 2011; Kuhn, Lalive, and Zweimüller 2009) and self-reported health (Burgard, Brand, and House 2007; Kuka 2020).[2]

In a recent study investigating the relationship between unexpected job loss and health outcomes in the Netherlands, Been, Suari-Andreu, and Knoef (2024) identify two competing factors underpinning this relationship. First, in line with findings from the studies mentioned above, unemployment may cause financial stress and uncertainty, both of which are likely to deteriorate health. The second factor, however, pulls in the opposite direction: the loss of a stressful job may actually improve health outcomes if the negative effects of job-induced stress outweigh the financial concerns. This is perhaps most salient outside of the U.S. UI generosity varies substantially across countries, and in most developed countries, benefits are available for one to two years, whereas in the U.S. they are typically available for less than six months during normal times (Moffitt et al. 2024). Interestingly, Been, Suari-Andreu, and Knoef (2024) find more evidence supporting short-term improvements in health following unexpected job loss. As just noted, this may be due to the Netherlands' larger social safety net, as such outcomes are not typically observed in the U.S., but it provides evidence of the potential positive effects of more generous unemployment insurance (UI) studied in our paper.

Focusing specifically on the effects of job loss on mortality, several studies use detailed administrative data from European countries to offer insight into these effects. Notably, direct extrapolation to the U.S. context may be limited by stark institutional differences, especially

[1] There is some evidence that workforce dynamics may lead to those with poorer health selecting into firms that are more likely to experience plant closure (Bloemen, Hochguertel, and Zweerink 2018). This concern is partly mitigated in panel studies such as ours that use a dynamic framework to exploit the timing of the disruption.

[2] See also McKee-Ryan et al. (2005) for a review of the mental health effects of unemployment within the psychology literature.

with regard to existing social safety nets. Bloemen, Hochguertel, and Zweerink (2018) find that job loss due to firm closure increases the probability of death in the subsequent two years by 34 percent in the Netherlands. They also find evidence that this effect is driven, in part, by stress and changes in lifestyle as suggested by increases in alcohol and smoking related deaths. Using Austrian data, Kuhn, Lalive, and Zweimüller (2009) find that job loss increases expenditures for antidepressant and hospitalizations for mental health. Similarly, Browning and Heinesen (2012) analyze data from Denmark and find that plant closures lead to increases in suicide, mental illness and alcohol-related disease. Finally, Eliason and Storrie (2009) find large increases in suicide and alcohol-related mortality following plant closures in Sweden.[3]

In the U.S. context, Sullivan and Von Wachter (2009) find large effects of mass layoffs on mortality in the 1970s and 1980s over an extended time frame, and show that these effects are more pronounced for those in their middle ages. Classen and Dunn (2012) find that unemployment duration is a primary factor when considering the relationship between job loss and suicide, although mass layoffs also increase suicide in the short run. While not using variation in job loss, Pierce and Schott (2020) find increases in suicide and overdoses in counties with labor markets that are negatively affected by international trade policy. The effects are particularly stark among White individuals.

Turning to the evidence on drug use following job loss, the most rigorous studies in the US exploit variation in general economic downturns. For instance, Carpenter, McClellan, and Rees (2017) find evidence that downturns increase the intensity of prescription pain medication use and substance use disorders involving opioids. These effects are concentrated among working age White males with low educational attainment. Also related, Schwandt and Von Wachter (2019) and Cutler, Huang, and Lleras-Muney (2015) show that poor initial labor market conditions are correlated with drug use and overdose, among other detrimental health consequences.[4]

[3]While our focus is on suicide and drug overdose deaths, note that our definition of drug overdose includes alcohol poisoning. The short-term nature of the effects we study precludes an analysis of deaths related to chronic alcohol consumption, such as deaths related to liver disease.

[4]Notably, supply-side factors such as drug prices and availability — rather than macroeconomic conditions — are primary drivers of the drug epidemic (Ruhm, 2019; Currie, Jin, and Schnell, 2019), which is consistent with

Taken together, these studies suggest that involuntary job loss can lead to adverse health and mortality consequences.[5] Moreover, despite the varied and complex risk factors associated with suicide and drug overdose death, job displacement is a clear cause of drug use, drug overdose, correlates of suicidal ideation, and realized suicide. Existing evidence also points to more pronounced effects among working class White males, which is consistent with the longer-run increases in these deaths of despair identified by Case and Deaton (2015, 2017), and motivates the investigation of heterogeneous effects across demographic subgroups in our paper.

The adverse consequences of job loss highlight the potential for UI to serve a crucial mitigating role. The related literature provides clear evidence that UI is effective in reducing various types of layoff-induced negative shocks. For instance, Hsu, Matsa, and Melzer (2018) find that more generous UI reduces mortgage delinquency and Lindo, Regmi, and Swensen (2023) provide evidence that laid-off workers with access to more generous UI benefits see reductions in the divorce and fertility effects of job loss.

Focusing on non-mortality health outcomes: Tefft (2011) documents decreases in depression and anxiety as the number of individuals claiming UI increases in a state; Kuka (2020) reports that higher state-level UI benefits can improve health insurance coverage and self-reported health; Chen et al. (2022) exploit variation in the extended UI benefits provided during the great recession to show that UI generosity improves self-reported mental health among the unemployed; and Ahammer and Packham (2023) use detailed administrative data from Austria to show that a 9-week extension to UI duration primarily affects women through fewer opioid and anti-depressant prescriptions and a lower probability of filing a disability claim.[6]

The latter two papers use individual-level data to show that expanded unemployment insurance improves self-reported mental health and reduces opioid prescriptions, respectively.

our null drug overdose findings in this short-run context.

[5] Existing evidence also shows that job displacement affects spousal health (Marcus 2013; Jolly 2020).

[6] In several related papers, Koltai et al. (2021) show evidence that reforms to the unemployment benefit system in Italy led to improved measures of self-reported health, while Voßemer et al. (2018) show correlations that suggests higher benefit generosity across 26 countries helps mitigate the adverse consequences of unemployment on well being but not health.

These analyses provide solid evidence that expanding the social safety net improves health outcomes for unemployed individuals. Our aggregate-level analysis complements these studies by focusing on short-term mortality effects on households and communities in counties across the U.S. Given that the mortality and overdose outcomes we study are generally rare, they are not suitably studied with the repeated cross-sectional survey data used by Chen et al. (2022). We therefore consider our study of the effects of UI generosity on mortality at the county-quarter level to be a strong complement to Chen et al. for understanding effects in the US context. Ahammer and Packham (2023) observe mortality in the Austrian administrative data they study, but they do not find a statistically significant effect and it is not the focus of their analysis. The higher levels of social support in developed countries outside of the US (such as Austria) may also mitigate effects on the extreme outcomes we study.

With respect to mortality outcomes, Shahidi and Parnia (2021) find that all-cause mortality in Canada is lower among unemployed individuals that receive UI relative to unemployed individuals that are not UI recipients. Gryzbowski et al. (2021) explore the fraction of laid-off mothers interacted with state UI changes. They find that more generous state-level UI correspond to small decreases in child mortality. As discussed in the introduction, Cylus, Glymour, and Avendano (2014) and Wu and Evangelist (2022) provide initial evidence that UI benefits may reduce suicide and overdose mortality.

3 Data

We assemble data from several sources to measure county-specific UI claims and layoff events, county-specific mortality outcomes, and state-specific UI details. First, we use county-level layoff data from the Bureau of Labor Statistics' (BLS) Mass Layoff Statistics Program, which began in 1995 and was discontinued in 2012. The program collected detailed information on mass layoffs, defined as events where at least 50 laid-off workers from the same private-sector place of employment claim UI benefits in a five-week window.[7] As these layoffs are the direct result of an establishment being downsized or closed, they are plausibly unrelated to the individual characteristics of the workers or supply-side labor market factors. The publicly available data report mass layoffs at an annual frequency. In this paper, we use a richer version of the data from Lindo, Schaller, and Hansen (2018) obtained from the BLS that provide quarterly layoff information at the county level from 1995 to 2010. This is important for identification. While local economic conditions are likely to affect the probability of a mass layoff event, the specific quarter in which the mass layoff event occurs is plausibly random. Comparing quarters before and after the event in the context of our specified model can reinforce a causal interpretation of effects.

We restrict the sample to prime working-age individuals (aged 25 to 54), a group that has a strong attachment to the labor force and is more likely to be eligible for and collect UI benefits (Cullen and Gruber 2000).[8] This restriction is also intended to focus on individuals who are well established in the labor market, while avoiding potential confounding issues from younger individuals still making early career choices between schooling and work, as well as from older individuals who may be transitioning into retirement or have weaker labor market attachment. With that said, we also explore potential effects on other age groups in our robustness analyses.

Figure 1 displays the mean mass layoff rate across counties during our study period, where the layoff rate is defined relative to the county population.[9] Mean layoff rates tend to be higher in what has been refereed to as the Rust Belt region, particularly Michigan, Indiana, Ohio and Pennsylvania, which have experienced declines in their manufacturing base since the 1950s. South-western coastal areas and western coastal states (California, Oregon, and Washington) also have relatively high layoff rates. Note that a higher mass layoff rate in a county-quarter

[7] To be clear, the mass layoff events in our study are identified by UI claims, establishing the link between these mass layoff events, unemployment, and subsequent UI claims based on county of residence.

[8] To be clear, these are workers that likely satisfy the work history and wage documentation eligibility standards that vary across states.

[9] Using the county labor force as the denominator yields findings similar to our main results. We use the county population as the denominator given that the county labor force is endogenously determined and may be affected by UI generosity.

may be an indication of multiple mass layoff events occurring in a county and quarter; we do not distinguish between many small mass layoff events and one large mass layoff event when computing the layoff rate. We provide summary statistics in Table 1 which shows a mean county layoff rate of 94 per 100,000 county residents.[10]

Second, we obtain the restricted-use national vital statistics micro-data from the Centers for Disease Control and Prevention (CDC), which contains monthly mortality incidents with county identifiers broken down by various demographic groups and underlying causes. These are aggregated up to quarterly counts in our analysis. Mortality causes are classified using the International Classification of Disease (ICD) codes.[11] Again restricting our focus to those aged 25 to 54, we construct mortality rates using the estimates of the county-level population from the Surveillance, Epidemiology, and End Results (SEER) Program. We follow the general practice in the literature of expressing deaths per 100,000 county population specific to each demographic group. The mean suicide rate per quarter in our sample is 3.8 and the mean overdose death rate per quarter is 3.5, shown in Table 1. This table also shows mortality outcomes by gender and race. Suicide rates and overdose death rates are highest for males and White individuals.

We present mean county-level suicide rates during the study period in Figure 2, and mean county-level overdose death rates in Figure 3. A comparison of Figures 1 and 2 shows that, in the cross-section, counties with high mean suicide rates are not typically counties with high layoff rates. Suicide rates are higher in the mountain states of Colorado, Nevada, Wyoming, and Montana, but are not as high in the Rust Belt region, on average. Drug overdose death rates are generally correlated with suicide rates, although there are some stark differences, such as southern New York and Connecticut, where we observe high rates of drug overdose death but low rates of suicide death.

[10]Figure A1 displays the number of mass layoff events in each county over the study period. This shows that mass layoff events occur in the majority of US counties, although, as expected, there are more of them in more populated counties.

[11]In these data, mortality causes are identified with ICD-9 (1995-1998) and ICD-10 (1999+) codes. Table A1 provides the ICD codes used to classify mortality outcomes.

Third, we collect maximum weekly unemployment insurance (UI) benefit levels, our measure of UI generosity, from the "Significant Provisions of State UI Laws: Benefit and Tax" reports published biannually by the BLS. The benefit schedules—providing a cap on amount an eligible laid-off worker can collect in a state—are published twice a year, in January and July. States decide both the levels and changes in maximum UI, leading to substantial differences across states and over time. For example, in 2010, Alabama had the least generous UI benefits program in nominal terms with maximum weekly benefits of $200 and Massachusetts had the most generous UI benefits program in nominal terms with maximum weekly benefits of $721. Figure 4 displays the cross-sectional variation in mean maximum weekly UI benefits during the study period. Table 1 reports a mean maximum amount of weekly unemployment benefits of $356 across all counties during our study period, and a standard deviation of $105.[12]

Consistent with standard practice in the literature (e.g Hsu, Matsa, and Melzer 2018, Krueger and Mueller 2010), we use maximum weekly UI rather than the actual benefit receipt because the latter measure may be confounded by contemporaneous county or state characteristics that are related to the incidence of suicide or drug overdose death. For instance, county-quarters or state-quarters with a higher volume of UI payments may be experiencing weaker underlying labor market conditions and more challenging socio-economic backgrounds, generating a mechanical relationship between adverse economic indicators and actual UI receipt, which would make it difficult to disentangle cause and effect. We also explore an alternative measure of UI generosity, UI duration, in a subsequent sensitivity analysis. Finally, we collect labor force statistics from the Local Area Unemployment Statistics (LAUS) program, and obtain various other county-level characteristics from U.S. census data.[13]

In addition to the means of the key variables already discussed above, Table 1 reports

[12] We note that maximum UI and other state-level social safety net generosity parameters may be correlated; in other words, some states may be more generous than others. However, our subsequent empirical approach identifies key parameters from within-state changes in maximum UI over time rather than cross-state comparisons of maximum UI levels. This reduces the concern that our parameter of interest bundles effects of other state-specific social safety net programs.

[13] These data can be found at https://oui.doleta.gov/unemploy/DataDashboard.asp; and https://www.bls.gov/lau/

descriptive statistics of these variables separately for counties without mass layoff events and with mass layoff events as these breakdowns will be used in our empirical analyses. Other than the layoff rate and the overall population count, which vary mechanically across these three categories, there are no striking differences in UI benefits, suicide rates, or overdose death rates, providing initial support for interpreting the specific timing of mass layoff events as exogenous.

4 Empirical Strategy

We consider a variety of empirical approaches to identify whether UI mitigates the effect of job loss on suicides and overdose deaths. In our first model, suicides or overdose deaths per 100,000 in state s, county c, year y, and quarter q are modeled as a function of the statewide maximum amount of weekly UI benefits (measured in hundreds) that year-quarter, $MaxUI_{syq}$; whether the county experienced a mass layoff in the previous quarter, $I[Layoff_{sc,yq-1} > 0]$; and the interaction between these two variables.[14] This interaction is the key variable as it allows for the possibility that the direct effect of job loss on suicides or overdose deaths is dampened by more generous UI benefits.

$$\begin{aligned} Mortality_{scyq} \quad = \quad & \delta_1 MaxUI_{syq} + \delta_2 I[Layoff_{sc,yq-1} > 0] + \\ & \delta_3 MaxUI_{syq} \times I[Layoff_{sc,yq-1} > 0] + \\ & \theta_{sc} + \theta_y + \theta_q + \epsilon_{scyq} \end{aligned} \tag{1}$$

This baseline model includes county fixed effects θ_{sc}, quarter fixed effects θ_q, and year fixed effects θ_y to control for fixed county characteristics, seasonal effects, and time-varying national shocks. After showing baseline results, we systematically add flexibility leading to our preferred model that also accounts for shocks that are specific to any given state in any given year (state-by-year fixed effects θ_{sy}), national time-varying shocks affecting health outcomes

[14] We use nominal measures of UI benefits; results are qualitatively unchanged if we use real UI benefits. The results are reported in Table A3 in the Appendix.

(year-by-quarter fixed effects θ_{yq}), and time-varying county demographic measures including the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, married, and aged 25-54. Finally, we use population weights and cluster standard errors at the state level.[15,16]

The first two terms in the above model deal with baseline factors that may affect suicides and overdoses. The association between state UI generosity and county suicide or overdose rates, described by the parameter δ_1, may be positive or negative, and does not have a causal interpretation. For example, increasing the generosity of UI within a state may improve the general quality of life for individuals prone to suicide and drug overdoses in that state, reducing the likelihood of suicides and overdoses, but it may also be a policy response to poor economic conditions in the state that reduced the general quality of life for these individuals and increased the probability of suicides or overdoses.

Mass layoff events are known to increase suicides and overdoses (Sullivan and Von Wachter, 2009). We therefore expect a positive estimate of δ_2, the coefficient on the county layoff indicator. This would indicate that county-quarters with positive layoff rates the previous quarter experience more suicides or overdose deaths than county-quarters with zero layoff rates the previous quarter, holding other factors constant.

We now turn to how this estimating equation deals with the focus of this paper, the extent to which the generosity of statewide UI mitigates the effect of job loss on suicides and overdose deaths. This effect is measured by δ_3, the coefficient on the interaction between maximum UI benefits in the state and an indicator for a mass layoff event. Negative estimates would indicate that for county-quarters that experienced a mass layoff event, more generous statewide

[15]Specifically, we weight all estimates by the county population aged 25-54, and as suggested by Abadie et al. (2023), we cluster standard errors at the level at which UI generosity is determined. Clustering at a broader level (e.g., states rather than counties) guards against bias arising from serial correlation between UI benefits and the error term within states and across counties (Cameron and Miller, 2015).

[16]We also explore specifications that include either state-by-quarter fixed effects or county-by-quarter fixed effects to account for seasonal differences that are area-specific. Likewise, we also estimate models with state-by-year-by-quarter fixed effects. The results for these exercises, which can be seen in Table A4, are similar to our main estimates.

UI benefits reduce the corresponding rate of suicides or overdose deaths in the county. The magnitude of δ_3 relative to δ_2 would provide an indication of how much the maximum amount of weekly UI benefits would need to increase to compensate for a mass layoff event, on average.

The identifying assumption underlying a causal interpretation of this parameter is that changes in state UI generosity are not correlated with other changes that differentially affect county populations recently exposed to a mass layoff event relative to county populations that were not recently exposed to a mass layoff event. We also note that while state-specific UI generosity may be a function of state-specific factors that could affect mortality, our fully specified model includes state-by-year fixed effects which absorb state variation in specific policy changes over time.[17] As such our identifying variation comes primarily from the plausibly exogenous timing of county-level mass layoff events interacted with state UI generosity levels.[18]

In addition, interpreting δ_3 as a causal parameter requires that an increase in a states' weekly maximum UI benefits increases UI receipt following mass layoff events. This relationship is supported by existing research on UI and from the fact that our mass layoff data is derived from UI applications.

The above specification captures mean differences between counties that experience mass layoff events and counties that experience no mass layoff events. In ancillary analyses, we also allow the intensity of layoff events to vary by splitting county-quarters into those with zero layoff rates, below-median layoff rates $I[BelowMedianLayoffRate_{sc,yq-1}]$, and above-median

[17] We note that our preferred specification with state-year fixed effects is best suited to capture short-lived, within–state-year treatment effects. Persistent effects or those with heterogeneous timing may be attenuated relative to models with separate state and year fixed effects. Accordingly, we also report estimates without state-year fixed effects for comparison.

[18] Nonetheless, we report in Table A2 that state-wide maximum UI levels are not correlated with other state-level economic conditions including GDP, employment rates, and unionization rates. We also regress state-wide maximum UI benefits on the county-level layoff rate, and the estimated coefficient is 0.06 (SE= 0.33).

layoff-rates $I[AboveMedianLayoffRate_{sc,yq-1}]$.[19]

$$\begin{aligned} Mortality_{scyq} &= \mu_1 MaxUI_{syq} + \mu_2 I\left[BelowMedianLayoffRate_{sc,yq-1}\right] + \\ &\quad \mu_3 I\left[AboveMedianLayoffRate_{sc,yq-1}\right] + \\ &\quad \mu_4 MaxUI_{syq} \times I[BelowMedianLayoffRate_{sc,yq-1}] + \\ &\quad \mu_5 MaxUI_{syq} \times I[AboveMedianLayoffRate_{sc,yq-1}] + \\ &\quad \theta_{sc} + \theta_y + \theta_q + \epsilon_{scyq} \end{aligned} \tag{2}$$

The models shown in equations (1) and (2) assume that previous quarter mass layoff events only affect current quarter suicides or overdose deaths. Since mass layoff events could occur at any time during the quarter, our focus on previous-quarter layoffs avoids the possibility of loading the effects of a mass layoff event occurring near the end of a quarter on suicides or overdose deaths that occur earlier in the quarter. We also anticipate that there may a lag between mass layoff events and mortality consequences. Ultimately, we need not rely on speculation about the timing of effects, as we can explore the dynamics of the relationship directly with an event study approach. Specifically, we estimate models that take the following form:

$$\begin{aligned} Mortality_{scyq} &= \gamma_1 MaxUI_{syq} + \sum_{k=-3}^{3} \gamma_k I[LayoffRate_{sc,yq+k} > 0] + \\ &\quad \sum_{m=-3}^{3} \lambda_m MaxUI_{syq} \times I[LayoffRate_{sc,yq+m} > 0] + \\ &\quad \theta_{sc} + \theta_y + \theta_q + \epsilon_{scyq} \end{aligned} \tag{3}$$

This specification allows the suicide or overdose death rate in quarter t to be affected by lags and leads of indicators for mass layoff events in the county, $I[LayofRate_{sc,yq+k} > 0]$, and interactions between the statewide maximum UI benefits and lags and leads of the county layoff

[19]The median is defined based on county-quarters with non-zero layoff rates, so the below-median and above-median indicators will be switched on for a similar number of county-quarter observations, but there will be many more county-quarters with zero layoff rates. The omitted category is county-quarters with zero layoff rates.

rate indicators. This is essentially a 7-period snapshot centered on the event to explore the dynamics surrounding the short-term effects of the mass layoff relative to periods outside this window.[20]

Our hypothesis is that suicide or overdose rates in year-quarter yq should only be affected by job losses in contemporaneous or prior quarters, and not future job losses. Estimates of γ_k should therefore only be non-zero for $k \leq 0$. Similarly, we expect the coefficients on the interactions of the layoff indicators with the state maximum UI benefits λ_m should only be non-zero for $m \leq 0$. We also estimate variations of this model in which each lag or lead of the county layoff rate enters individually into the specification as a sensitivity check.

These event study estimates speak to the timing, persistence, and robustness of the relationship between mass layoff events and suicide or overdose death rates. In particular, as discussed above, although state-specific UI generosity is likely to be correlated with other state-specific factors that may affect mortality, these factors are likely constant across the specific quarters before and after the mass layoff event. So, given the plausible exogeneity of the timing of the mass layoff event within a set of adjacent quarters, any mortality changes across these quarters can be attributed to the mass layoff event and not other state-specific factors, and, more specifically for the purposes of this paper, so can any mitigation of these mortality changes due to the generosity of state UI.

We also consider a version of Equation 1 that restricts the outcomes to gender-specific or race-specific suicide and overdose death rates within the county. This is motivated by recent studies that document more pronounced suicides and overdoses among certain demographic groups of the US population. In particular, there have been documented increases in these outcomes for White males, most often in their middle ages (e.g. Case and Deaton 2015, 2017; Pierce and Schott 2020). We continue to rely on population-wide mass layoff events in these

[20]This approach is necessary given the repeated and overlapping windows of mass layoff dynamics. Note that in this regard, we do not model the event as a binary persistent treatment in which we omit the period just before treatment as is often the case in policy analyses of a "treatment" that switches on at some point in time. Rather, this is a repeated shock that allows us to evaluate short-term effects.

specifications so economic shocks remain the same and are comparable across specifications.

Finally, we explore the sensitivity of our findings along a variety of dimensions. In particular, we include event studies that attempt to address recent concerns with two-way fixed effects estimates.

5 Results

5.1 Main Results

In Table 2, we report the results from estimating Equation 1, the model with a binary indicator for a mass layoff event the previous quarter. The effects on suicide rates are reported in Panel A and the effects on drug overdose death rates are reported in Panel B. Column 1 reports estimates from a baseline model with county, year, and quarter fixed effects; Column 2 replaces the year and quarter fixed effects with year-by-quarter fixed effects; Column 3 adds time-varying county demographic controls; and Column 4 includes state-by-year fixed effects.

The estimates in Panel A show that counties with mass layoff events experience an increase in suicides in the subsequent quarter, which is consistent with existing evidence on the relationship between job loss and suicide. Given the mean county suicide rate of 3.8 suicides per 100,000 (Table 1), the increase of 0.23*** (0.08) reported in Column 4 reveals that when a county-quarter experiences a mass layoff event, there is a 6 percent increase in suicide rates.[21]

The aggregate nature of our analysis does not allow us to explore whether it is the actual individuals that are laid off who experience an increase in the probability of suicide, their family members, or if the mass layoff events cause negative spillovers in the community which affect the suicide risk of other individuals in the county. However, the immediacy of the effects indicate

[21] On average, 94 out of every 100,000 individuals in a county are laid off every quarter because of mass layoff events (Table 1). The estimated increase in suicides is 0.2 per 100,000 individuals aged 25-54. Under the assumption that the increase in suicides comes solely from laid-off individuals, it is worth noting that this 6 percent increase in the suicide rate corresponds to a small share of laid off workers committing suicide for the typical mass layoff event.

that it is likely the laid off individuals or people with very close ties to these individuals who experience the increase in mortality. This interpretation is consistent with some of the existing literature investigating the effect of job loss on mortality using individual-level data in European contexts (Eliason and Storrie 2009; Browning and Heinesen 2012). Ultimately, the social cost of the increased risk of suicide is high regardless of who experiences it.

We now turn to our primary question of interest: to what extent is this spike in suicides following job loss mitigated by UI generosity? The coefficient in Column 4 on the interaction between maximum UI benefits and the indicator for a mass layoff event in the county-quarter is -0.06*** (0.02), which is clear evidence of a mitigating role played by UI. More specifically, a one standard deviation increase (approximately a $100 increase) in weekly maximum UI benefits would mitigate more than a quarter of the spike in suicide rates following a mass layoff event.[22], [23]

While we see the same pattern of effects in Panel B for drug overdose death rates, the estimates for the layoff event and the interaction in Column 4 are not statistically different from zero at conventional levels of significance, although the mitigating effect of UI is observed in Columns 1 to 3 with fewer controls. The absence of a short-run effect of layoffs on drug overdose deaths contrasts with the findings of Wu and Evangelist (2022). Recall that their study used annual mortality data and rolling two-year mass layoff rates while our study uses quarterly data. In addition, the inclusion of state-by-year fixed effects is shown to be important in our study (comparing the estimates in Columns 1 and 2). Wu and Evangelist (2022) rely on time-varying state-level economic characteristics and year fixed effects; they are unable to use state-by-year fixed effects as that is the level at which UI generosity varies in their study. Beyond methodological differences, the increase in suicide rates following job loss is plausibly driven by acute financial and psychological distress — mechanisms that UI directly addresses.

[22] Note that since MaxUI is measured in hundreds in our analysis, a $100 increase corresponds to 1 unit. The calculation is therefore $(0.06 \times 1)/0.23 = 0.27$.

[23] Note that the effects in columns 3 and 4 are very similar, suggesting that our arbitrary choice of including state-year fixed effects does not lead to meaningful differences in estimated effects or the ratio of UI mitigation to baseline effects on suicide of layoff.

Drug overdose mortality, by contrast, may depend on longer-run patterns of substance use and supply-side factors such as drug availability and pricing (Ruhm, 2019; Currie, Jin, and Schnell, 2019). Consistent with this, the suicide effects we document are concentrated in the single quarter immediately following the layoff, whereas pathways to overdose plausibly unfold more gradually.

In both panels, we observe a positive association between statewide UI generosity and suicides or drug overdose deaths; county-quarters in states with more generous UI also tend to be county-quarters with higher baseline levels of suicide and overdose mortality. This finding is consistent with a hypothesis offered in the Empirical Strategy section: increases in maximum UI within a county over time may be a policy response to poor economic conditions, and the probability of suicides or overdoses may also be higher during economic downturns. In other words, this positive correlation may be driven by omitted variables.

We next explore the extent to which the mitigating effects of UI are a function of the intensity of the mass layoff event. Table A5 reports results from Equation 2, which introduces additional parameters to capture the intensity of local mass layoff events, but retaining the tractability of the indicator treatment model. Recall that in this model, counties with positive layoff rates (counties that experienced a mass layoff event) are classified as above-median or below-median rather than being identified by a single indicator. The omitted category remains county-quarters with no layoff events. The estimates in Panel A show that counties with below-median layoff rates experience larger increases in suicide rates following a mass layoff event relative to counties with zero layoff rates, and counties with above-median layoff rates experience an even larger spike in suicide rates following a mass layoff event. We again observe clear mitigating effects of UI on suicides; the estimates on the interactions are negative, with slightly larger coefficients following above-median mass layoff events. Similar to Table 2, we observe no effects on drug overdose mortality in our fully specified model shown in Panel B.

To further unpack the relationship between mass layoff intensity and suicide, Table A6 presents estimates based on tercile, quartile, and quintile groupings of mass layoff rates. We acknowledge that increasing the number of treatment parameters involves arbitrary groupings and may introduce risks of over-fitting and reduced statistical power. Nevertheless, the estimated interactions between UI and layoff intensity in columns 1 and 2 indicate a stronger mitigating effect on suicide as mass layoff intensity increases. The estimates in Column 3, which divide treatment parameters into quintile groups are less precise and more difficult to interpret. Overall, we interpret these findings as evidence that the effect sizes generally rise with the severity of the mass layoff event.

5.2 Event Studies

We unpack the timing of our estimated effects by considering event studies taking the form of Equation 3. We focus on suicide mortality in this component of analysis given the unambiguous effects reported for this outcome in the regression analysis above. The suicide event studies are shown in Figure 5. The event studies for drug overdose mortality are reported in Figure A2 in the appendix for completeness.

In Figure 5 Panel A, we first plot the direct effects of job loss on suicide for the three quarters leading up to and following mass layoff events. The figure shows a stark spike in suicide rates in the quarter immediately following the layoff event. There is no evidence of effects prior to the event nor in the quarter of the event (recalling that the mass layoff event could occur near the end of the quarter), and the effect does not persist beyond the quarter immediately following the event. Under the assumption that the specific timing of the shock is not correlated with other changes affecting employment, this panel provides strong support for the causal relationship between job loss and mortality that has been documented in other contexts. Our study adds to this evidence with an emphasis on shorter-run effects on suicides consistent with the impulsive nature of suicide ideation.

Panel B speaks to the focus of our paper, the mitigating effect of UI on these layoff-induced increases in county-level mortality. In this panel, we plot the estimated interactions between the maximum UI benefit and lags and leads of the indicators for a mass layoff event. They follow a similarly striking pattern to Panel A. We observe no non-zero effects in the quarters leading up to the layoff; no evidence of an effect in the quarter of the layoff; a clear negative effect in the quarter after the layoff (the mitigating effect of UI), and no further effects for subsequent quarters.

Overall, Figure 5 Panels A and B provide compelling evidence of the spike in county-level suicide rates following job loss and the mitigating role played by UI in reducing county-level suicide responses to layoffs. Importantly, the timing of effects supports a causal interpretation; even if mass layoff events are functions of local economic conditions that may affect mortality through other channels, the specific quarter in which the mass layoff event occurs is plausibly exogenous. We do not observe any pre-trends in suicide mortality that would be indicative of county-specific factors other than the mass layoff event causing the mortality spike relative to adjacent quarters.[24]

In Figure A2 Panels A and B in the appendix, we plot the corresponding estimates for drug overdose deaths. The patterns in both panels are consistent with the null effects on drug overdose deaths that we document in Tables 2 and A5. Given the absence of effects on drug overdose mortality in both the regression and event study analyses, we focus exclusively on suicide mortality in the remainder of our study.

5.3 Identification Issues in Two-way Fixed Effects Models

A recent econometrics literature has cautioned against interpreting the estimates derived from a conventional two-way fixed effects regression (De Chaisemartin and d'Haultfoeuille 2020; Goodman-Bacon 2021; Callaway and Sant'Anna 2021; Sun and Abraham 2021; Borusyak, Jaravel, and Spiess 2024). The primary concern stems from potential contamination of the control

[24] Appendix Figure A6 presents event-study results from the model without state-year fixed effects. The pattern is similar, suggesting that the lack of persistence is not driven by our use of state-year fixed effects.

group in which previously-treated units serve as controls. These papers, along with other recent advances, propose alternative estimators designed to address problems with two-way fixed effects in settings with staggered treatment timing. Our setting differs from the standard staggered-adoption design with unit and time fixed effects and a binary treatment variable. Our variable of interest is the interaction term between two treatments—changes in UI generosity, which is continuous, and mass layoff events. Additionally, the mass layoff event is non-absorbing, with the counties' status switching from experiencing a layoff to no layoff. To provide reassurance that our findings are robust to some of these concerns, we modify two well-known approaches for estimating event studies that mitigate some of these issues.

First, we show an event study in the spirit of Sun and Abraham (2021). In our setting, we recognize that layoff events may last longer than one quarter and there may be overlapping events occurring within a county over our event study window. Such circumstances could potentially lead to contaminated pre- and post-treatment "control" quarters. To address this, we consider several sample restrictions. In order to avoid excessive overlapping treatment events, we focus exclusively on relatively large mass layoff events (i.e. above median layoff events).[25] To account for mass layoff events that last more than one quarter, we exclude all but the first observed quarterly observation of the layoff event. In other words, if county c experiences a layoff event in periods t, $t+1$, and $t+2$, we drop the observations for that county in periods $t+1$ and $t+2$. Prior to dropping these periods, we replace the dependent variable in the first treated quarter with the mean of the dependent variable over the multiple quarters during the layoff event, although we find that not doing so yields nearly identical estimates.[26] Finally, we restrict the sample to those events that do not overlap with another layoff event for at least two county-quarters leading up to or following the quarter of the event. These restrictions mean that we are focusing on a subset of clean mass layoff events in which there is no contamination of pre-treatment and post-treatment periods. These sample restrictions result in a subsample of 105,899 observations

[25]To be clear, small layoff events are considered untreated in this exercise.

[26]The results are shows in the Appendix Figure A5.

for 2,515 counties.[27] The results of this exercise are shown in Appendix Figure A4. These graphs show similar patterns of pronounced spikes and mitigation effects in the quarter immediately following a layoff event, although the 95% confidence intervals slightly overlap with zero in these models.

Second, we extend the method of Gardner (2022) that uses a two-stage approach. In the first stage, we restrict the sample to county-quarter observations that do not experience layoffs and estimate a regression that includes only fixed effects (county, state-by-year, and year-by-quarter) and county demographic measures. We use the parameters from this model to obtain residualized mortality outcomes for the full sample. In the second stage, we estimate treatment effects using these residuals as the dependent variable in our main model including the same fixed effects and individual controls. We report standard errors calculated using the wild cluster bootstrap approach. The results are shown in Appendix Figure A6. Similar to our initial results, these figures show similar patterns of pronounced spikes and mitigation effects in the quarter immediately following a layoff event.

Taken together, these event studies provide clear evidence of a short-run mitigating effect of UI generosity on suicide following mass layoff events, and show no evidence of extraneous effects before the mass layoff event that would question the validity of the model.

5.4 Heterogeneity

Table 3 explores heterogeneity by gender and race. Specifically, we report results in which gender- or race-specific county suicide rates are regressed on the same set of explanatory variables as in Table 2. We observe that the direct effect of job losses on suicide and the corresponding mitigating role played by UI are most evident for males and White individuals. As can be seen from the table, these are also the groups with much higher baseline suicide rates. There are no clear effects on females, Black individuals, or Hispanic individuals. We discuss this heterogeneity

[27] More specifically, 30 percent of our layoff events last longer than one quarter and nearly half of our events overlap another county layoff event within the two quarters before or after the event.

below.

Using a similar approach to Table 3, Table 4 considers heterogeneity by age. In addition to a breakdown of our main focus on prime-age workers (ages 25-54), we show estimates for younger (18-24) and older (56-64) workers. These results reveal no evidence for effects on these groups, which is consistent with a higher likelihood of being unemployed due to schooling or retirement. Among our main sample of prime-age workers, the effects of job loss on county-level suicide rates are largest among later-career individuals between the ages of 45 and 54 years old, with the pattern of estimates showing increasing effect sizes as individuals increase in age. Tables A7 and A8 consider heterogeneous effects by age separately for males and females, respectively. These estimates reinforce our findings that late-middle-aged males have particularly pronounced spikes in suicide and mitigation effects following mass layoff events.[28]

The concentration of mortality effects from job losses on White individuals, males, and individuals between the ages of 46 and 55 is broadly consistent with existing evidence. Case and Deaton (2015, 2017) find that suicides and drug overdoses have been primary drivers of mortality for White Americans in recent years, and Carpenter, McClellan, and Rees (2017) finds substance use disorders increase after economic downturns particularly among White individuals.[29] The observation that the mitigating effects of UI are concentrated among a subpopulation with elevated risks of suicide more generally suggests that a targeted policy response of increasing UI benefits may be especially effective for this group.[30]

We also explore heterogeneity across urban and rural settings. While mass layoff events occur more frequently in urban settings, the relatively limited labor market opportunities in rural settings may exacerbate the negative impacts of a layoff event.[31] We use the 2003 urban-

[28]In other results, we have explored estimates for subpopulations defined by gender, age, and race. These are noisy, which is unsurprising given the increased granularity of the groupings, but the pattern of the findings also point to effects being most pronounced among late-middle-age White men.

[29]The gender differences we observe are also in line those reported in Lindo, Regmi, and Swensen (2023), in which UI generosity reduces the probability of divorce after a job layoff for men more than women.

[30]Notably, this subpopulation is also the group with the highest UI take-up rate and the group for which the UI cap is most likely to be binding.

[31]Despite urban settings being more than twice as likely to experience a mass layoff event, the mass layoff rate

rural continuum codes constructed by the United States Department of Agriculture (USDA) to partition counties into either urban or rural, designating urban counties as those which are in a metro area or adjacent to a metro area and have a population greater than 20,000.[32]

Table 5 shows the results of this breakdown. We observe that in urban counties there is both an increase in suicide following a mass layoff event and a clear mitigating effect of UI, but this is less evident in rural counties. As mentioned above, we may have expected that job loss in urban areas would have a smaller effect on suicide and that the mitigating force of UI would be diminished given the potential of more outside employment options in urban compared to rural counties, but that is not what we observe. Urban settings do not appear to offer an alternative form of unemployment insurance through potentially thicker labor markets.

5.5 Additional Robustness and Sensitivity

We provide a series of robustness and sensitivity checks. First, we consider whether mass layoff events affect other causes of death with the same immediacy (and whether these effects are mitigated by UI). Second, we explore alternative approaches to measuring the intensity of mass layoff events. Third, we consider the sensitivity of our models to controlling for non-layoff unemployment. And, finally, we briefly investigate the potential role of UI duration in mitigating the effects of job loss.

We report effects of mass layoff events on other common causes of mortality in Table 6. We explore deaths related to cardiovascular disease, cancer, respiratory disease, long-term alcohol diseases, and motor vehicle crashes.[33] Although there may be causal pathways from job loss to some of these causes of mortality, we anticipate that any such mortality effects would be muted in the quarter after a mass layoff event, which is what our model investigates. In particular,

driven by these events is nearly identical across urban and rural settings. Notably, the maximum weekly benefits are also similar across urban and rural designations in our sample.

[32] The USDA designates metro areas as "broad labor-market areas that include central counties with one or more urban areas with populations of 50,000 or more people" (https://www.ers.usda.gov/data-products/rural-urban-continuum-codes/documentation/).

[33] ICD codes for these categories are provided in Table A1.

cardiovascular disease, cancer, and respiratory disease are often related to chronic, underlying conditions. Effects on these mortality causes are investigated in columns 1, 2, and 3. Mostly consistent with our priors, the coefficients in columns 1 and 3 show no clear evidence of an effect on these common causes of death. On the other hand, Column 2 shows an imprecisely estimated spike in cancer and some evidence for a mitigation effect. While this may due to chance given our multiple hypothesis testing, we come back to this in the subsequent paragraph. In Column 4, we see no clear evidence of an effect of job loss on deaths caused by long-term alcohol disease. Finally, Column 5 shows some evidence that motor vehicle deaths decline after job loss, but no mitigating effect of UI. We speculate that this may be because workers reduce commute times after losing their jobs. Given this behavioral response to job loss is unlikely to be affected by UI benefits, it is not surprising that there is no mitigating effect of UI generosity on these deaths. With the exception of cancer-related mortality, the absence of effects in Table 6 reinforces the validity of our empirical approach and suggests a unique short-run relationship between job loss and suicide.

In light of the estimated effect on cancer-related mortality, we separately examine the nine main types of cancer in Appendix Table A9. These results suggest that the effect on cancer is largely driven by an apparent effect on lung cancer, which is the leading cause of cancer-related deaths in our data. Again, while this may be due to multiple hypothesis testing, several salient features of lung cancer may given rise to the possibility of a short term response to unanticipated job loss. This includes the prevalence of late-stage diagnoses, low screening rates, and extremely low survival rates (Allemani et al., 2019; Bandi et al., 2024); potential relationships between exposure to pollutants or high rates of smoking in industries with relatively high layoff risk; the link between stressful life events and cancer outcomes (Chida et al., 2008); and evidence of lung cancer responding to economic conditions (Michaud, Crimmins, and Hurd, 2016; Wilson and Walker, 1993; Maruthappu et al., 2016). While we view this background as establishing the plausibility of UI affecting lung cancer following mass layoff events, we leave a more thorough exploration of our finding on cancer mortality to future research.

We next carry out an analysis to confirm that our results are driven by individuals laid off in mass layoff events rather than other unemployed individuals, following an approach similar to Wu and Evangelist (2022). This exercise also tests the stability of our primary estimate to controls for other sources of unemployment. To do so, we calculate the contemporaneous "non-mass-layoff unemployment rate," which is defined as the unemployment rate net of the mass-layoff rate. We include this measure (denoted by $NonLayoffURate$) and its interaction with maximum unemployment insurance as controls in our primary specification. As shown in Appendix Table A10, this does not meaningfully change our main estimated effects and the coefficient on the two new controls are not statistically significant.

As a final robustness test, we use potential UI duration rather than maximum UI benefits as our measure of UI generosity. We leverage state-specific expansions in UI duration that occurred during and after the Great Recession of 2008. States generally provide up to 26 weeks of regular benefits; however, the duration of benefits was extended up to a maximum of 99 weeks after the beginning of the recession.[34] We collect information on the potential duration of UI benefits using the weekly Trigger Notice reports of Emergency Unemployment Compensation and Extended Benefits from the Bureau of Labor Statistics (BLS).[35] We aggregate these data to the quarterly level, and restrict our analysis to 2008-2010, the period of UI duration expansion that overlaps with our study period. We estimate a model similar to Equation 1, but replace the variable measuring maximum UI benefits with a variable measuring potential UI duration in weeks. As reported in Table A11, we find that increasing the number of weeks of potential UI benefits reduces layoff-induced suicides, while the effects on drug overdose deaths are imprecisely estimated.

[34] See Lindo, Regmi, and Swensen (2023) for more details of the UI duration policies.
[35] The link is https://oui.doleta.gov/unemploy/claims_arch.asp.

6 Conclusion

We document the short-run mitigating effects of UI on suicides and drug overdose deaths following mass layoff events in the United States. Using quarterly county-level mass layoff and mortality data, our estimates show that more generous UI mitigates the spikes in county-level suicide rates that follow mass layoff events. There are no clear short-run effects of UI on drug overdose death rates. The estimates on suicide deaths are robust to various model specifications, and effects are evident following, but not leading up to, mass layoff events. Our dynamic estimates show that both the increase in mortality and the mitigation effects are only apparent in the quarter immediately following the mass layoff event. Broadly consistent with the longer-term trends in suicides and drug overdose deaths identified in the literature on deaths of despair, our estimated effects are larger for White individuals, males, and those in their middle ages. These findings may assist in the design of tailored policy interventions to combat short-term suicide ideation among laid off workers.

Furthermore, a simple back-of-the-envelope calculation speaks to the costs and benefits of increasing UI generosity. Our main estimates suggest there are 3.92 suicides per year due to mass layoff events in the average county without UI benefits.[36] Our estimate on the interaction of layoff events and UI generosity indicates that the average county could mitigate 1.06 suicides with a $100 increase in the weekly maximum unemployment insurance benefits.[37] In other words, an additional $100 of maximum UI would mitigate 1 of the 4 suicides caused by mass layoff events per year in the average county. Given that our layoff data are generated by UI applications, we calculate that there are approximately 4,700 workers that apply for UI in the average county each year. Assuming that these individuals are receiving UI benefits for the annual average duration of 13 weeks, the total annual cost of an additional $100 of UI is approximately $6 million in the average county.[38] The value of a statistical life used by the EPA in policy assessments is

[36] This is calculated using our quarterly estimated effect of layoff (0.23 suicides per 100k) and an average county population aged 25-54 of 425,825 ($4 \times 0.23 \times 4.25825 = 3.92$).

[37] Calculated using the 27 percent mitigation effect identified in footnote 22 ($0.27 \times 3.92 = 1.06$).

[38] The calculation is $13 \times 100 \times 4700 = 6{,}110{,}000$.

approximately $7 million.[39] This means that in the context of mass layoff events, the reduction in suicide by itself is sufficient to justify expanding UI benefits. Even if our estimate of the benefit is overstated, given that there are many other benefits of UI, several of which are documented in the introduction, our calculation suggests that increasing UI generosity would pass a cost-benefit analysis.

The labor market disruptions induced by the COVID pandemic focused attention on UI, and there have been calls for the federal and state governments to revise UI policies. In concert with other recent evidence of spillover benefits generated by UI, our finding that UI reduces the mortality effects of mass layoff events emphasize the importance of considering the full range of social benefits when considering the optimal design of UI policies.

[39] The EPA's recommended central estimate of the value of a statistical life is $7.4 million (see https://www.epa.gov/environmental-economics/mortality-risk-valuation).

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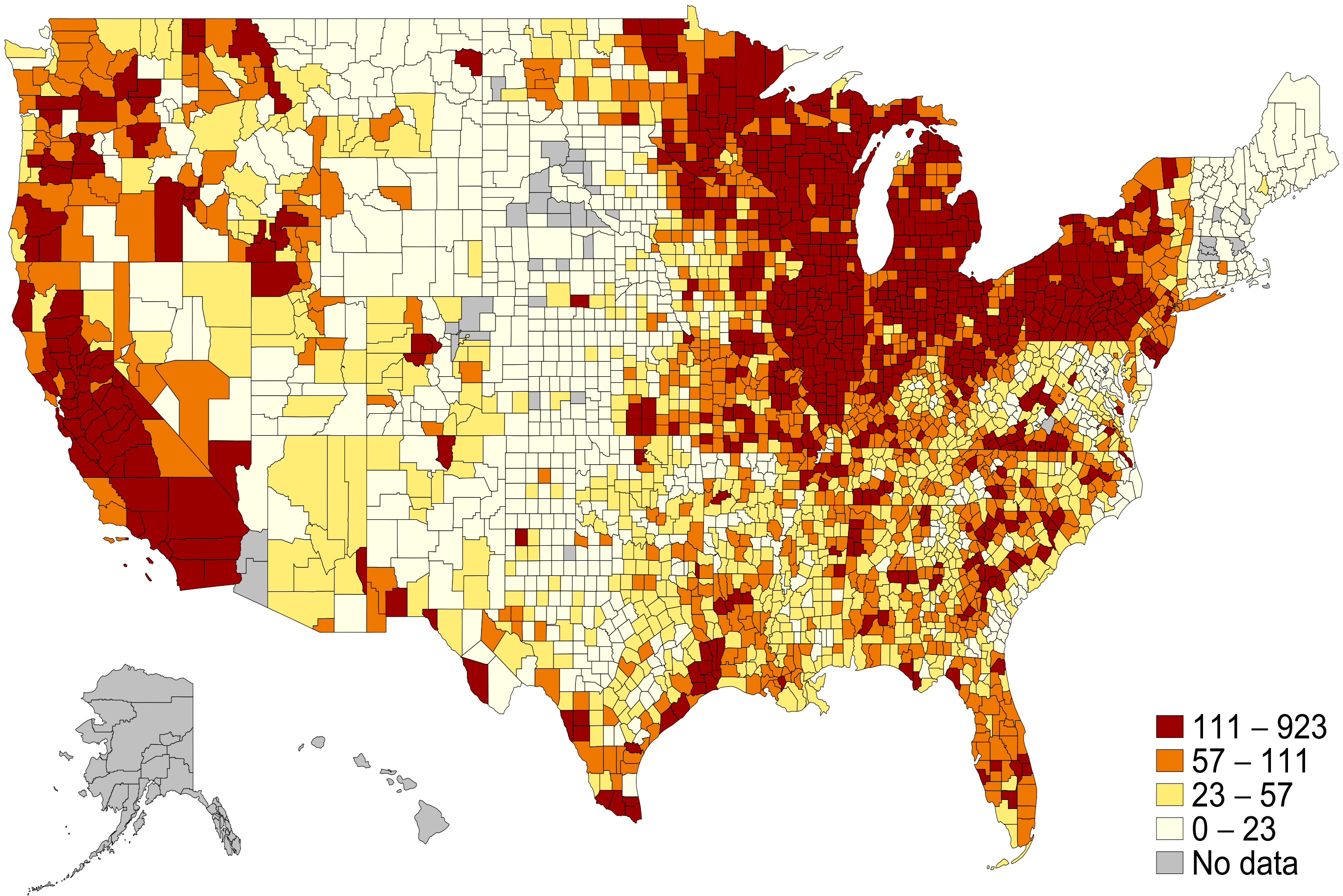
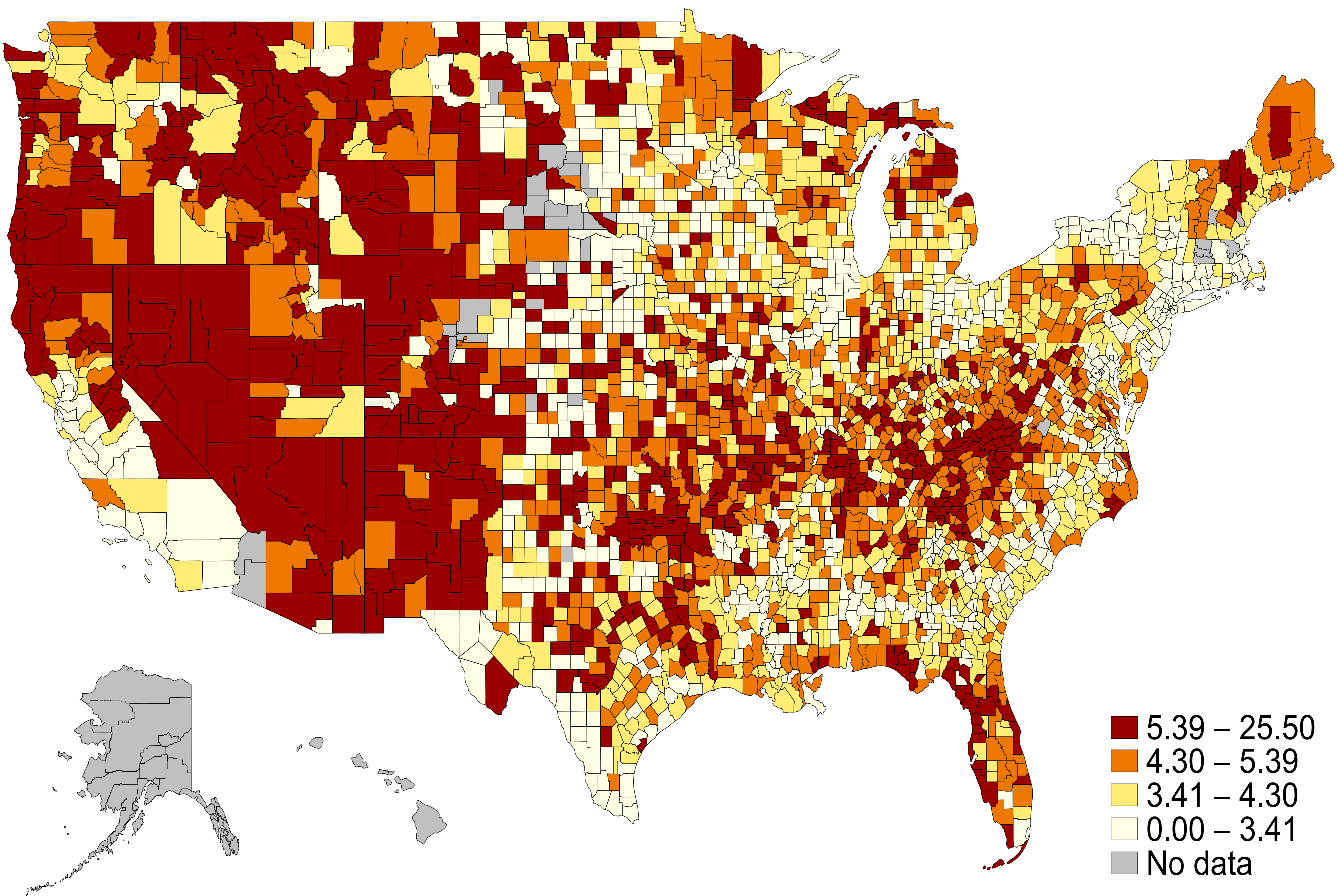
Figure 1: Mean layoff rates across US counties



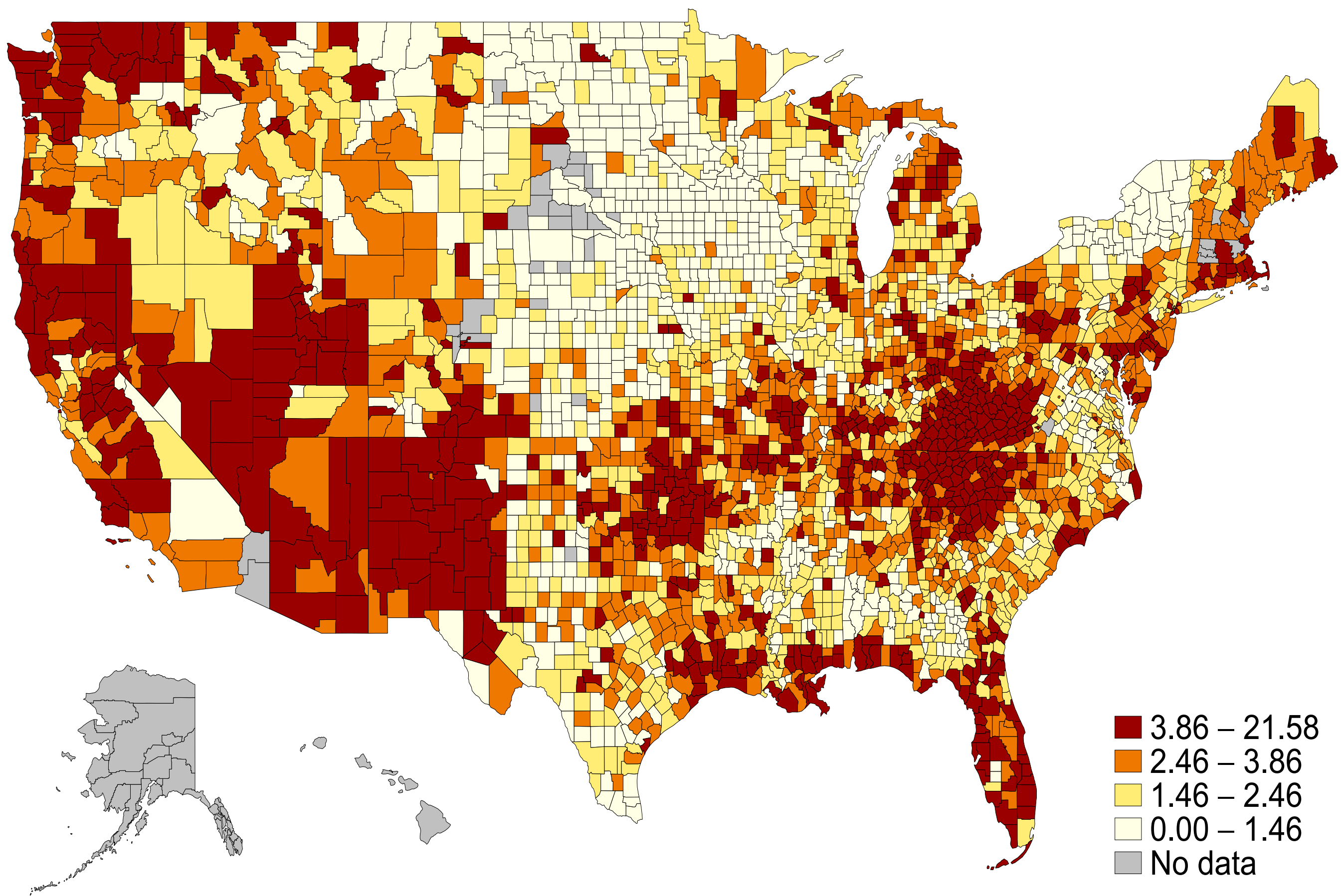
Notes: This figure displays the mean mass layoff rate across counties from 1995-2010, where the layoff rate is calculated as the number of layoffs per quarter per 100,000 county residents. The color breakdowns correspond to quartiles.

Figure 2: Mean suicide rates across US counties



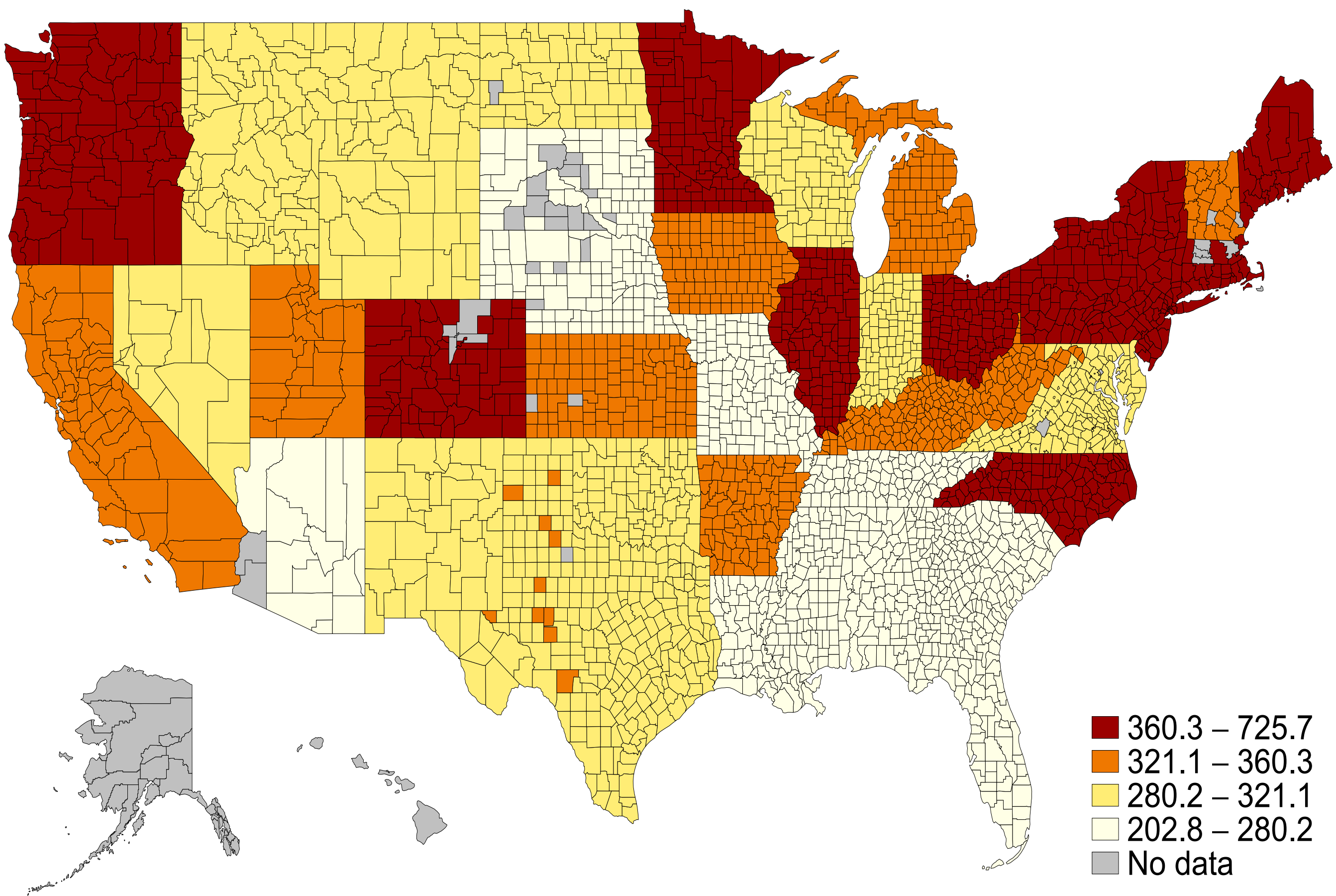
Notes: This figure displays the mean suicide rate for individuals aged 25-54 across counties from 1995-2010, where the suicide rate is calculated as the number of suicides per quarter per 100,000 county residents. The color breakdowns correspond to quartiles.

Figure 3: Mean drug overdose death rates across US counties



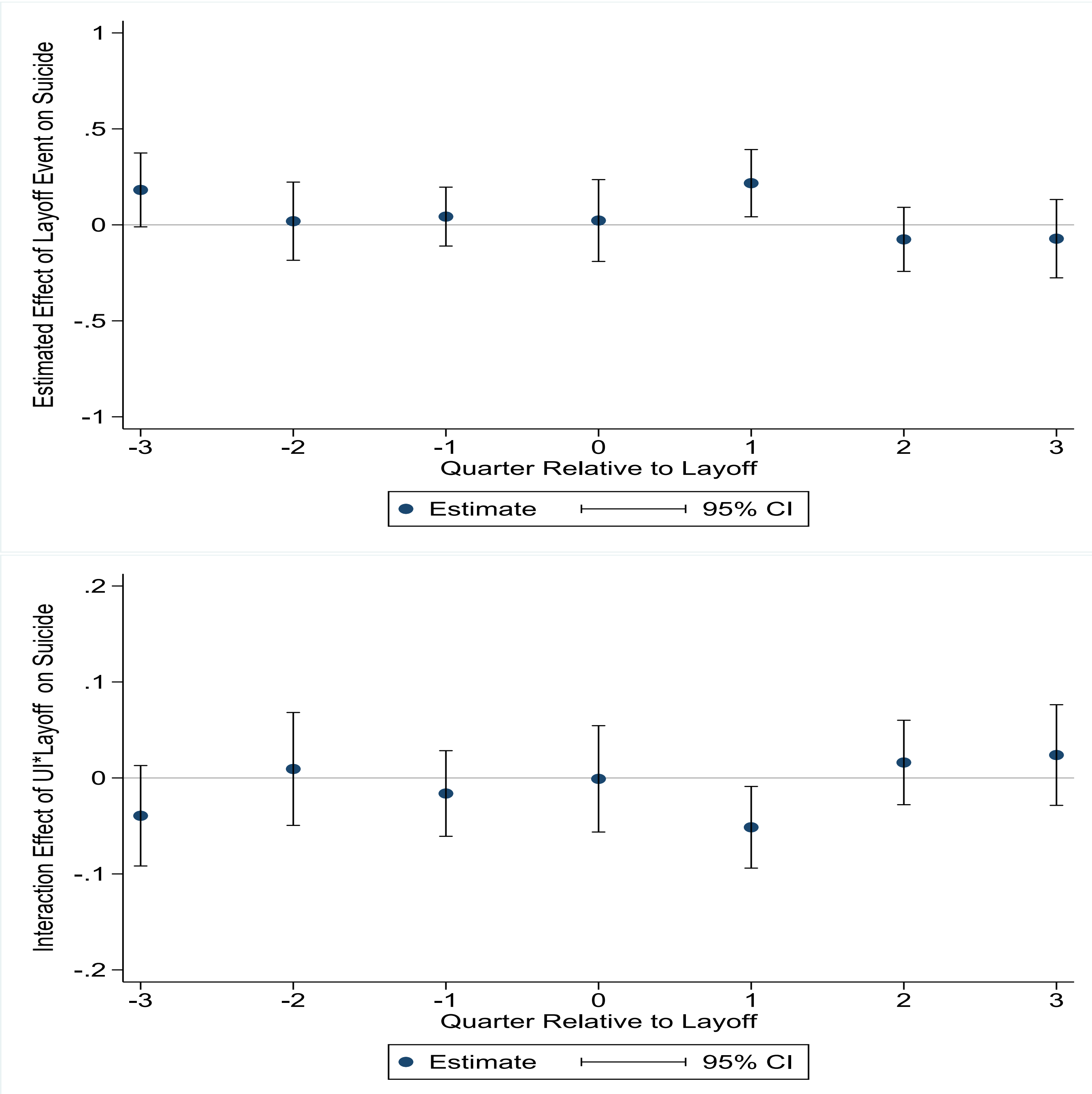
Notes: This figure displays the mean drug overdose rate for individuals aged 25-54 across counties from 1995-2010, where the drug overdose rate is calculated as the number of overdoses per quarter per 100,000 county residents. The color breakdowns correspond to quartiles.

Figure 4: Mean maximum weekly UI benefits across US counties; 1995 to 2010)



Notes: This figure displays the mean maximum weekly unemployment insurance benefits (measured in dollars) across counties from 1995-2010. The color breakdowns correspond to quartiles.

Figure 5: Effect of UI generosity on suicide rates: event study



Notes: Estimates are based on 189,743 quarterly observations spanning 1995–2010 for 3,071 U.S. counties. Both panels plot coefficients and 95 percent confidence intervals from the same regression model which includes an indicator for a mass layoff event in the current quarter. Panel A plots the estimated effect on suicide rates (per 100k aged 25-54) using lags and leads of these indicators. Panel B plots the estimated effect on suicide rates using lags and leads of the indicator interacted with the maximum weekly unemployment insurance benefit (measured in hundreds of dollars). The models also include county fixed effects, state-by-year fixed effects, year-by-quarter fixed effects, and county demographic controls for the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, male, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

Table 1: **Summary Statistics**

	(1)	(2)	(3)	(4)
			Means by Counties	
	Mean	Std. Dev.	Without Mass Layoff Events	With Mass Layoff Events
Maximum Weekly UI	3.544	1.052	3.365	3.581
Layoff Rate	93.594	139.739	0.000	113.023
Suicide Rate	3.764	3.632	4.338	3.645
Male Suicide Rate	5.903	6.384	6.865	5.704
Female Suicide Rate	1.650	3.128	1.815	1.616
White Suicide Rate	4.586	4.587	4.883	4.525
Black Suicide Rate	2.383	75.466	2.413	2.377
Hispanic Suicide Rate	1.994	27.602	2.554	1.878
Overdose Death (OD) Rate	3.483	3.782	3.108	3.561
Male OD Rate	4.734	5.613	4.047	4.876
Female OD Rate	2.266	3.871	2.190	2.282
White OD Rate	4.092	4.616	3.408	4.234
Black OD Rate	3.790	49.666	2.850	3.982
Hispanic OD Rate	1.947	15.272	1.636	2.011
Pop ages 25-54 (10k)	4.257	6.606	0.707	4.994
Frac College	0.276	0.102	0.232	0.285
Frac High School	0.604	0.087	0.641	0.596
Frac Less than High School	0.121	0.058	0.127	0.120
Frac White	0.691	0.206	0.800	0.668
Frac Black	0.129	0.130	0.107	0.133
Frac Hisp	0.128	0.143	0.064	0.142
Frac Other	0.052	0.061	0.029	0.057
Frac Male	0.490	0.013	0.493	0.490
Frac Married	0.634	0.076	0.669	0.627
Frac ages 25-54	0.431	0.034	0.418	0.434
Number of Counties	3,071		2,872	2,800
N	189,759		103,821	85,938

Notes: Summary statistics are calculated using county-by-quarter level data spanning 1995-2010. Suicide and drug-overdose include causes of death identified by International Classification of Diseases (ICD-9 and ICD-10) codes. Mortality rates are calculated per 100,000 county residents aged 25-54. Maximum weekly UI measures the maximum UI benefit level an eligible worker can collect each week (measured in hundreds of dollars). Layoff rates are the number of layoffs per 100,000 county residents.

Table 2: **Effect of UI on mortality following job loss: counties with and without mass layoff events**

	(1)	(2)	(3)	(4)
Panel A: Suicide Death Rates				
MaxUI*I[LayoffEvent]	-0.11***	-0.11***	-0.06**	-0.06***
	(0.03)	(0.03)	(0.02)	(0.02)
I[LayoffEvent]	0.41***	0.42***	0.22**	0.23***
	(0.11)	(0.11)	(0.09)	(0.08)
MaxUI	0.03	0.04	0.04	0.25*
	(0.05)	(0.05)	(0.05)	(0.13)
N	189,759	189,759	189,759	189,759
Panel B: Drug-Overdose Death Rates				
MaxUI*I[LayoffEvent]	-0.35**	-0.35**	-0.14	-0.05
	(0.14)	(0.14)	(0.09)	(0.05)
I[LayoffEvent]	1.17**	1.17**	0.42	0.11
	(0.47)	(0.47)	(0.29)	(0.15)
MaxUI	-0.31	-0.31	-0.17	0.32*
	(0.26)	(0.26)	(0.26)	(0.19)
N	189,759	189,759	189,759	189,759
County FE	Y	Y	Y	Y
Quarter FE	Y	-	-	-
Year FE	Y	-	-	-
StateXYear FE	N	N	N	Y
YearXQuarter FE	N	Y	Y	Y
Demographic Controls	N	N	Y	Y

Notes: Estimates are based on county-by-quarter observations spanning 1995–2010 for 3,071 U.S. counties. The outcome variables are mortality rates per 100k county residents aged 25-54 corresponding to each panel title. The model includes an indicator for a mass layoff event in the previous quarter, the same variable interacted with maximum weekly unemployment insurance benefit (measured in hundreds of dollars), and other fixed effects and controls as indicated. County demographic controls include the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

Table 3: **Effect of UI generosity on suicide following job loss: gender and race**

	Gender			Race	
	Male	Female	White	Black	Hispanic
MaxUI*I[LayoffEvent]	-0.10***	-0.02	-0.06***	0.12	-0.06
	(0.03)	(0.02)	(0.02)	(0.25)	(0.11)
I[LayoffEvent]	0.38***	0.08	0.20**	-0.14	0.13
	(0.14)	(0.07)	(0.08)	(1.04)	(0.47)
MaxUI	0.24	0.25***	0.26	-0.06	0.20
	(0.21)	(0.09)	(0.16)	(0.91)	(0.46)
ymean	6	2	5	2	2
N	189,759	189,759	189,759	179,935	188,414
County FE	Y	Y	Y	Y	Y
StateXYear FE	Y	Y	Y	Y	Y
YearXQuarter FE	Y	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y	Y

Notes: Estimates are based on county-by-quarter observations spanning 1995–2010 for 3,071 U.S. counties. The outcome variables are gender- and race-specific mortality rates per 100k county residents of the same age corresponding to each column title. The model includes an indicator for a mass layoff event in the previous quarter, the same variable interacted with the maximum weekly unemployment insurance benefit (measured in hundreds of dollars) and other fixed effects and controls as indicated. County demographic controls include the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, male, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

Table 4: **Effect of UI generosity on suicide following job loss: age**

	Age					
	25-54	18-24	25-34	35-44	45-54	56-64
MaxUI*I[LayoffEvent]	-0.06***	-0.01	-0.02	-0.03	-0.12***	-0.01
	(0.02)	(0.07)	(0.03)	(0.04)	(0.04)	(0.06)
I[LayoffEvent]	0.23***	0.06	0.11	0.13	0.44***	0.02
	(0.08)	(0.23)	(0.12)	(0.15)	(0.14)	(0.19)
MaxUI	0.25*	0.02	0.19	0.18	0.32	0.19
	(0.13)	(0.21)	(0.21)	(0.17)	(0.20)	(0.26)
Mean of Dep. Var	3.764	3.199	3.392	3.857	4.042	3.484
N	189,759	189,758	189,759	189,759	189,759	189,759
County FE	Y	Y	Y	Y	Y	Y
StateXYear FE	Y	Y	Y	Y	Y	Y
YearXQuarter FE	Y	Y	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y	Y	Y

Notes: Estimates are based on county-by-quarter observations spanning 1995–2010 for 3,071 U.S. counties. The outcome variables are age-specific mortality rates per 100k county residents with ages corresponding to each column title. The model includes an indicator for a mass layoff event in the previous quarter, the same variable interacted with the maximum weekly unemployment insurance benefit (measured in hundreds of dollars) and other fixed effects and controls as indicated. County demographic controls include the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, male married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

Table 5: **Effect of UI on suicide following job loss: urbanicity**

	Rural Counties	Urban Counties
Panel A: Suicide Death Rates		
MaxUI*I[LayoffEvent]	-0.02	-0.07***
	(0.06)	(0.02)
I[LayoffEvent]	0.11	0.23**
	(0.20)	(0.09)
MaxUI	0.00	0.30**
	(0.23)	(0.13)
Mean of Dep. Var	4.746	3.639
N	109,856	79,903
County FE	Y	Y
StateXYear FE	Y	Y
YearXQuarter FE	Y	Y
Demographic Controls	Y	Y

Notes: Estimates are based on county-by-quarter observations spanning 1995–2010 for 3,071 U.S. counties. The outcome variable is suicide rates per 100k county residents corresponding to each column title. Urban counties include those in a metro area as well as counties that are adjacent to a metro area with over 20,000 residents. The model includes an indicator for a mass layoff event in the previous quarter, the same variable interacted with the maximum weekly unemployment insurance benefit (measured in hundreds of dollars) and other fixed effects and controls as indicated. County demographic controls include the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, male, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

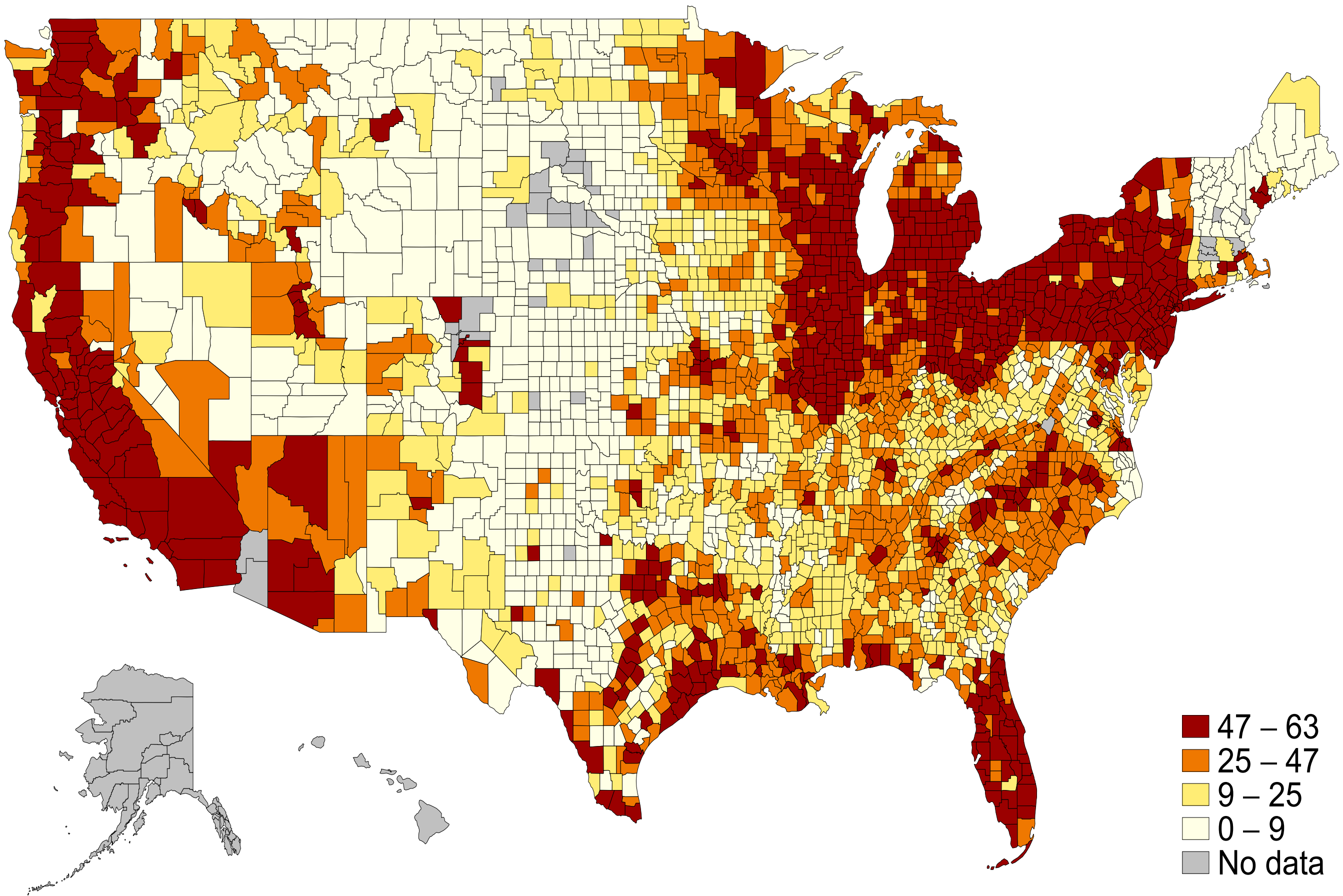
Table 6: **Effect of UI generosity on other mortality outcomes following job loss**

	(1)	(2)	(3)	(4)	(5)
	Cardiovascular disease	Cancer	Respiratory disease	Long-term alcohol-related disease	Motor vehicle crashes
MaxUI*I[LayoffEvent]	-0.01	-0.15**	-0.02	-0.01	0.01
	(0.05)	(0.07)	(0.03)	(0.01)	(0.02)
I[LayoffEvent]	-0.03	0.42*	0.02	0.06	-0.18**
	(0.18)	(0.22)	(0.11)	(0.05)	(0.09)
MaxUI	0.25	0.10	0.05	0.13	0.10
	(0.24)	(0.18)	(0.14)	(0.09)	(0.17)
Mean of Dep. Var	13.43	10.81	2.258	1.265	3.925
N	189,759	189,759	189,759	189,759	189,759
County FE	Y	Y	Y	Y	Y
StateXYear FE	Y	Y	Y	Y	Y
YearXQuarter FE	Y	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y	Y

Notes: Estimates are based on county-by-quarter observations spanning 1995–2010 for 3,071 U.S. counties. The outcome variables are mortality rates per 100k county residents aged 25-54 corresponding to each column title. The model includes an indicator for a mass layoff event in the previous quarter, the same variable interacted with the maximum weekly unemployment insurance benefit (measured in hundreds of dollars) and other fixed effects and controls as indicated. County demographic controls include the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, married, male, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

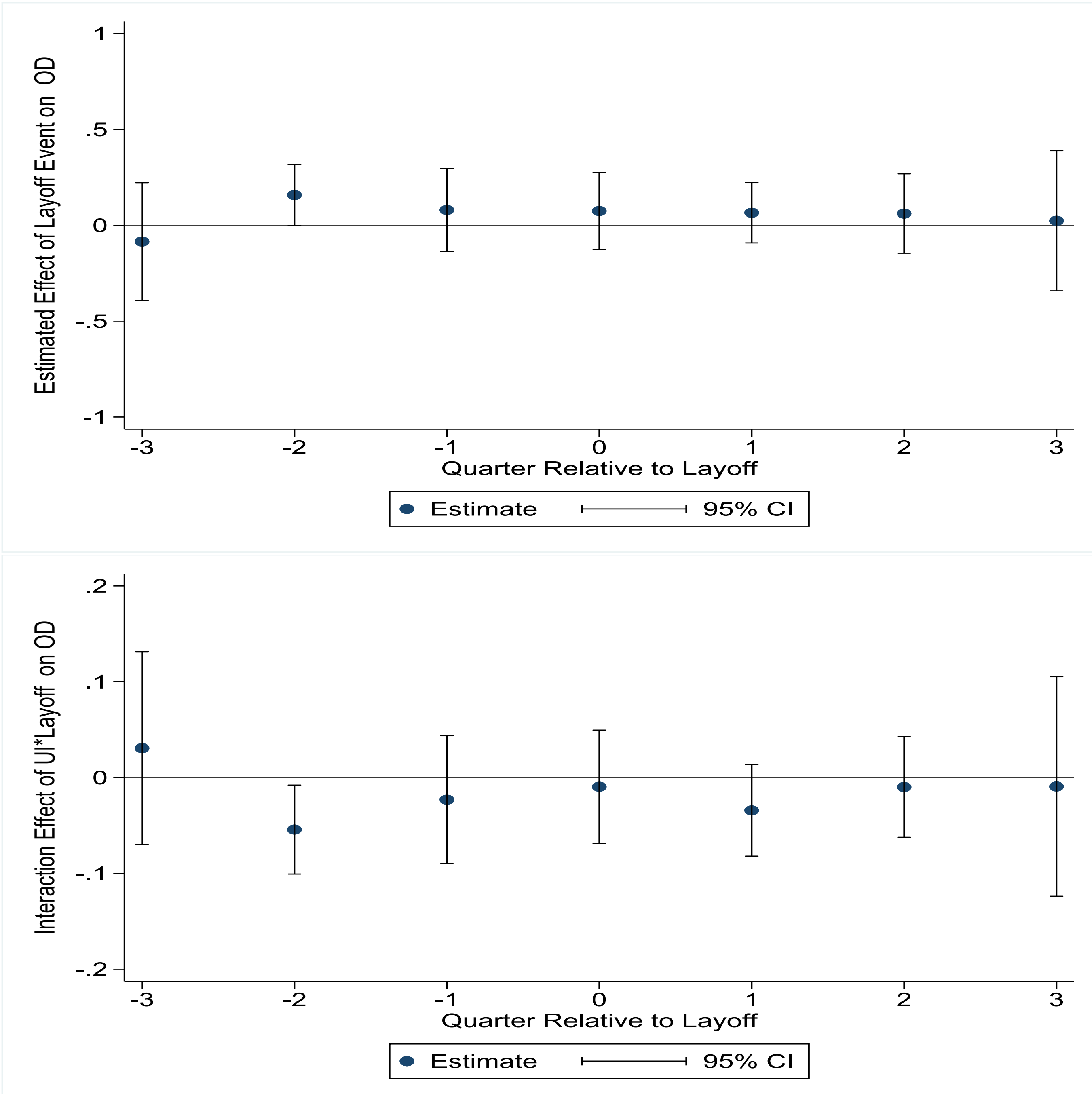
Appendix A

Figure A1: Number of mass layoff events in each county during study period



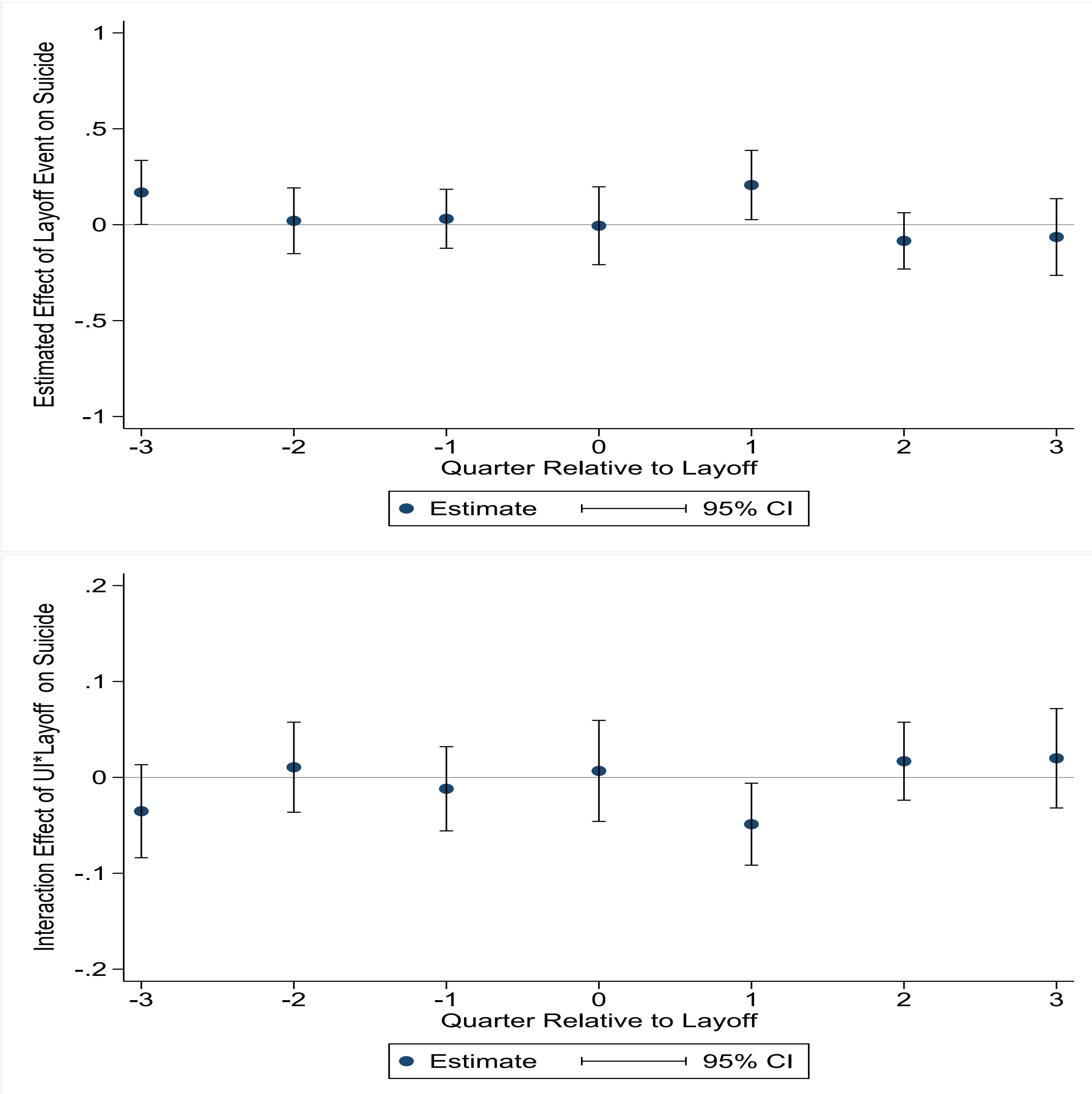
Notes: This figure displays the number of mass layoff events in each county from 1995-2010. The color breakdowns correspond to quartiles.

Figure A2: Effect of UI generosity on overdose death rates: event study



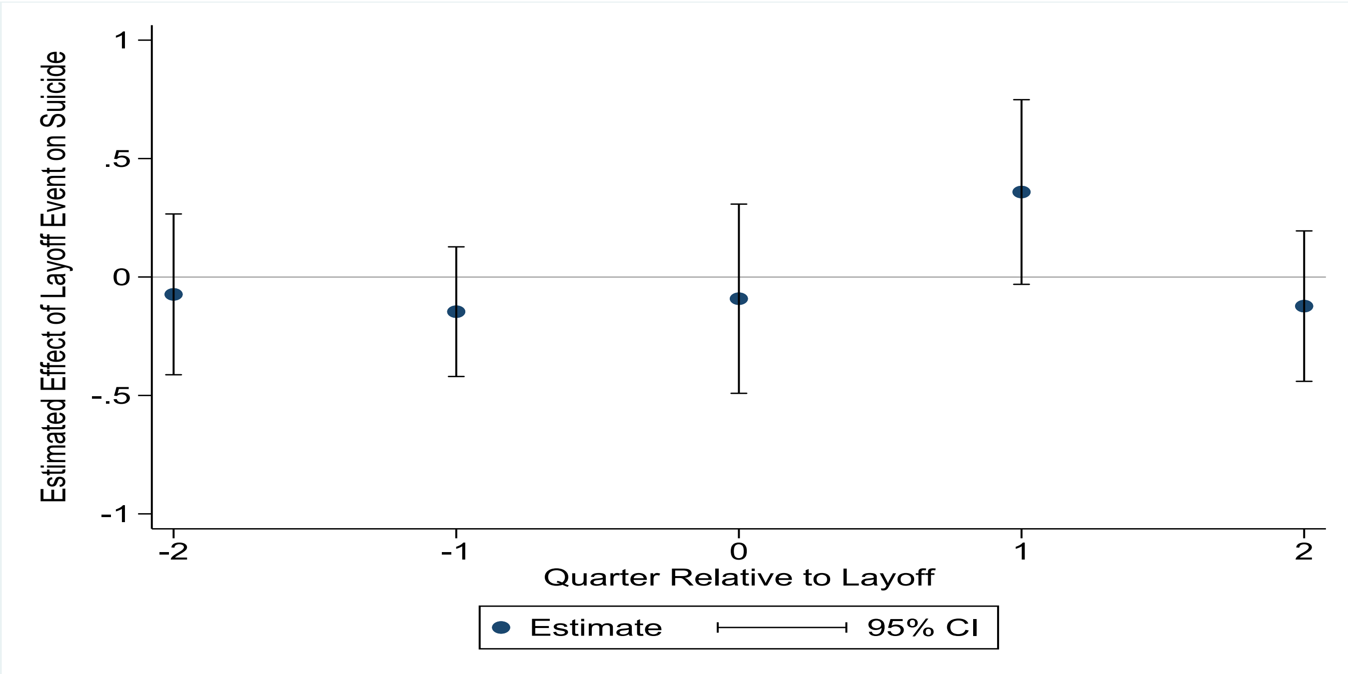
Notes: Estimates are based on 189,743 quarterly observations spanning 1995–2010 for 3,071 U.S. counties. Both panels plot coefficients and 95 percent confidence intervals from the same regression model which includes an indicator for a mass layoff event in the current quarter. Panel A plots the estimated effect on drug OD rates (per 100k aged 25-54) using lags and leads of these indicators. Panel B plots the estimated effect on suicide rates using lags and leads of the indicator interacted with the maximum weekly unemployment insurance benefit (measured in hundreds of dollars). The models also include county fixed effects, state-by-year fixed effects, year-by-quarter fixed effects, and county demographic controls for the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, male, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

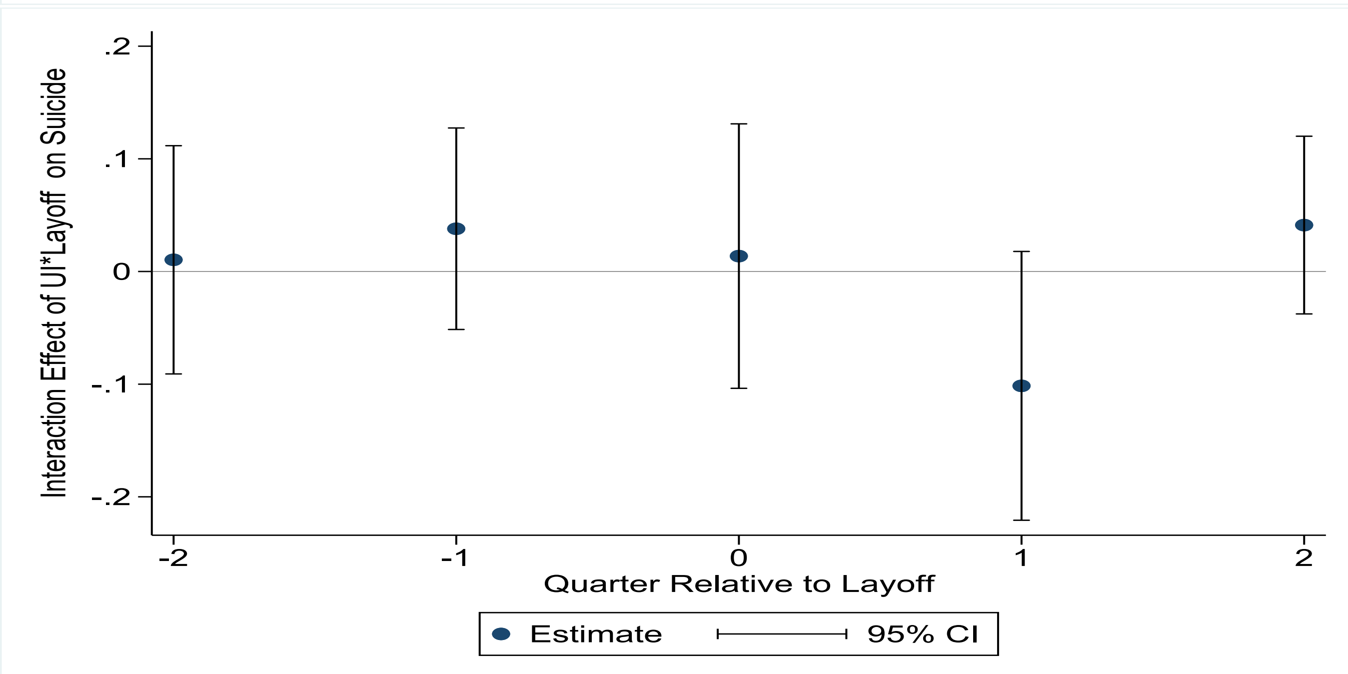
Figure A3: Effect of UI generosity on suicide rates: Without state-by-year fixed effects



Notes: Estimates are based on 189,743 quarterly observations spanning 1995–2010 for 3,071 U.S. counties. Both panels plot coefficients and 95 percent confidence intervals from the same regression model which includes an indicator for a mass layoff event in the current quarter. Panel A plots the estimated effect on suicide rates (per 100k aged 25-54) using lags and leads of these indicators. Panel B plots the estimated effect on suicide rates using lags and leads of the indicator interacted with the maximum weekly unemployment insurance benefit (measured in hundreds of dollars). The models also include county fixed effects, year-by-quarter fixed effects, and county demographic controls for the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, male, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

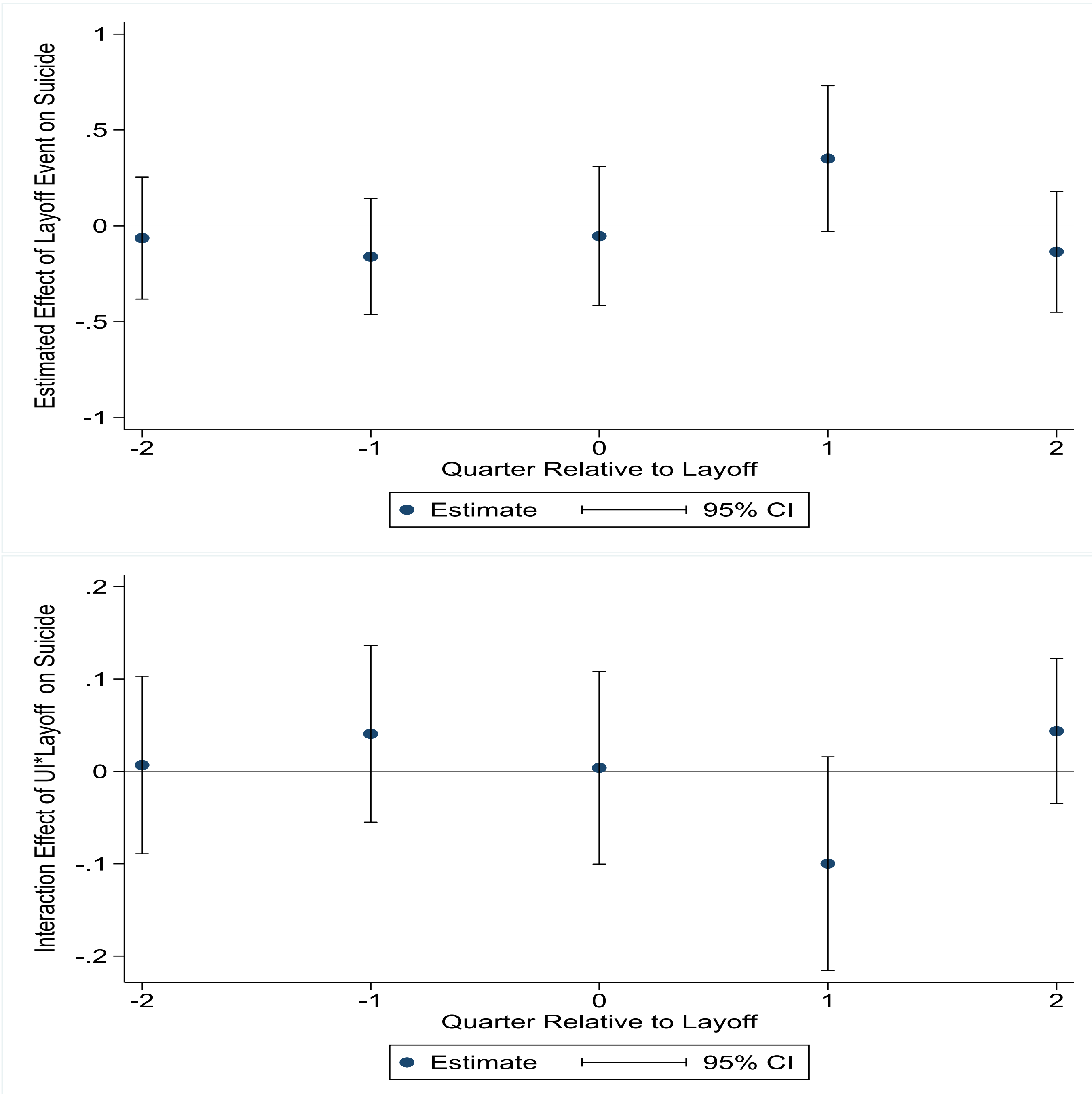
Figure A4: Effect on suicide rates: event study accounting for overlapping layoff events–above median





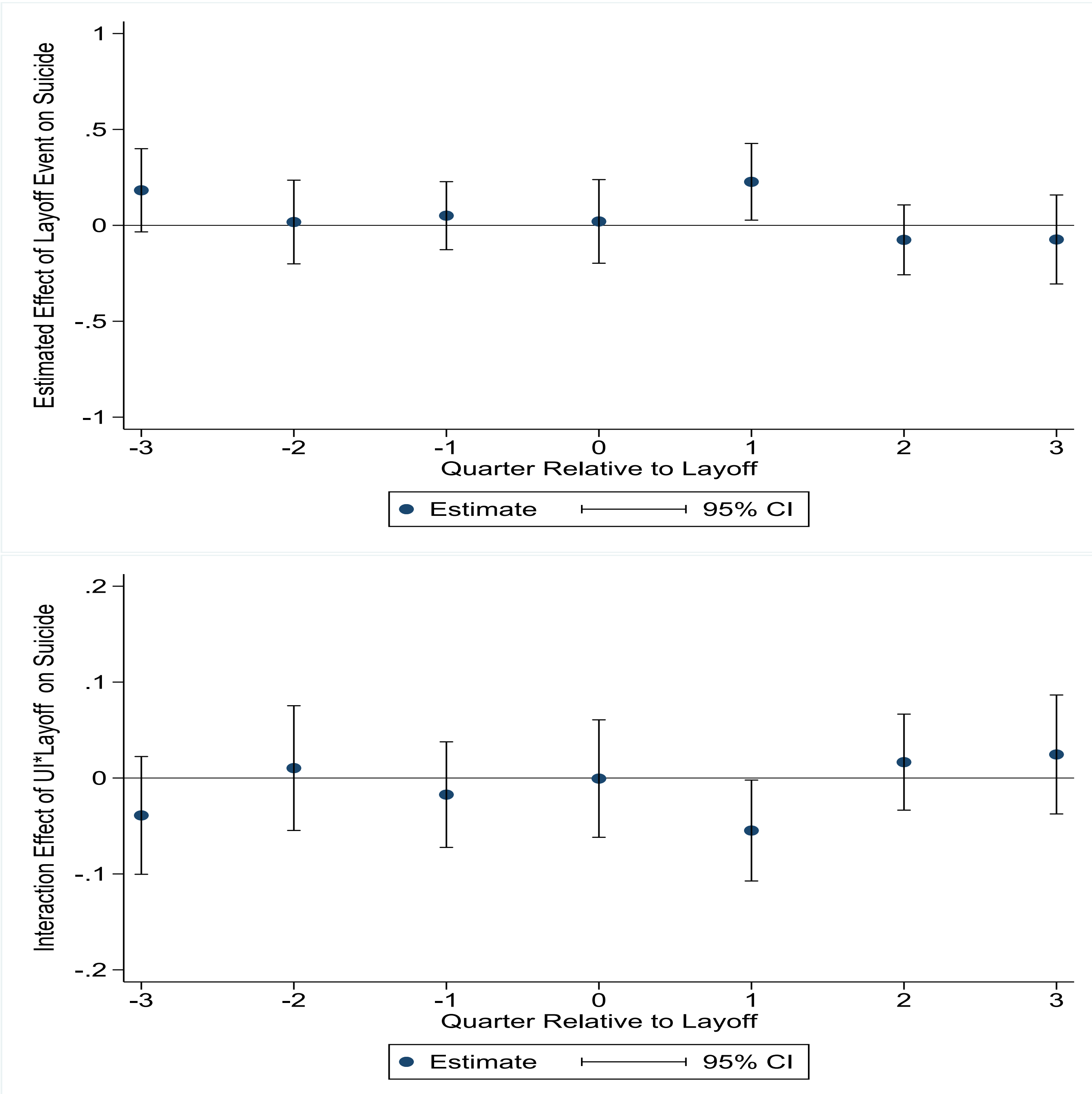
Notes: Estimates are based on 105,743 quarterly observations spanning 1995–2010 for 2,524 U.S. counties. The sample has been restricted to non-overlapping layoff events. Both panels plot coefficients and 95 percent confidence intervals from the same regression model which includes an indicator for being above the median layoff rate. Panel A plots the estimated effect on suicide rates (per 100k aged 25-54) using lags and leads of these indicators. Panel B plots the estimated effect on suicide rates using lags and leads of these indicators interacted with the maximum weekly unemployment insurance benefit (measured in hundreds of dollars). The models also include county fixed effects, state-by-year fixed effects, year-by-quarter fixed effects, and county demographic controls for the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, male, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

Figure A5: Effect on suicide rates: event study accounting for overlapping layoff events–above median (without mean adjustment)



Notes: Estimates are based on 105,743 quarterly observations spanning 1995–2010 for 2,524 U.S. counties. The sample has been restricted to non-overlapping layoff events. Both panels plot coefficients and 95 percent confidence intervals from the same regression model which includes an indicator for being above the median layoff rate. Panel A plots the estimated effect on suicide rates (per 100k aged 25-54) using lags and leads of these indicators. Panel B plots the estimated effect on suicide rates using lags and leads of these indicators interacted with the maximum weekly unemployment insurance benefit (measured in hundreds of dollars). The models also include county fixed effects, state-by-year fixed effects, year-by-quarter fixed effects, and county demographic controls for the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, male, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

Figure A6: Effects on suicide rates: Event study following Gardner (2022)



Notes: Estimates are based on 189,743 quarterly observations spanning 1995–2010 for 3,071 U.S. counties. Panel A plots the estimated effect on suicide rates (per 100k aged 25-54) using lags and leads of these indicators. Panel B plots the estimated effect on suicide rates using lags and leads of these indicators interacted with the maximum weekly unemployment insurance benefit (measured in hundreds of dollars). The models also include county fixed effects, state-by-year fixed effects, year-by-quarter fixed effects, and county demographic controls for the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, male, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

Table A1: Classification of Cause of Death

Death Category	**ICD-10 (1999-2010)**	**ICD-9 (1995-1998)**
Suicide	*113 Category Recode:* 124-126	*72 Category Recode:* 820
Drug-Overdose	X40-X45, X60-X65, X85, Y10-Y15	*282 Category Recode:* 31700, 33800, 35300
Cardiovascular	*113 Category Recode:* 53-75	*72 Category Recode:* 320-490
Cancer	*113 Category Recode:* 19-44	*72 Category Recode:* 160-250
Respiratory	R092 *113 Category Recode:* 76-89	500, 501, 512, 514, 515, 5070, 5109, 5119, 5130, 5168, 5184, 5185, 5188, 5191, 5198, 7991 *72 Category Recode:* 500-580
Alcohol	E244, G312, G621, G721, I426, K292, K700-K704, K709, K852, K860	3050, 3030, 3575, 4255, 5710-5713, 7903
Motor-Vehicle Accidents	*113 Category Recode:* 115-123	*72 Category Recode:* 800

Notes: The ninth revision of the International Classification of Diseases (ICD) corresponds with 1995-1998 data and the tenth revision with 1999-2010 data. To group deaths into the above categories, we use either the actual ICD code or a recode of ICD codes grouped into categories as indicated.

Table A2: **Correlation between state-level maximum UI benefits and either state-level economic conditions or county-level layoff rates**

	(1)	(2)	(3)	(4)	(5)
GDP	-0.006			-0.007	
	(0.006)			(0.006)	
UnempRate		-0.009		-0.013	
		(0.031)		(0.031)	
UnionDensity			0.012	0.011	
			(0.020)	(0.020)	
LayoffRate					(3.25)
N	1,632	1,632	1,632	1,632	189,711
Year FEs	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	-
County FEs	-	-	-	-	Y

Notes: Estimates are from regressing maximum unemployment insurance (UI) benefits (measured in hundreds of dollars) on major state-level economic conditions in Columns 1-4 and county-level layoff rates in Column 5 for the period 1995-2010. The units of observation in Columns 1-4 are state-years and the unit of observation in Column 5 is county-years. Each column presents estimates based on a separate regression. Standard errors are clustered at the state level.

Table A3: **Effect of UI on mortality following job loss: MaxUI in real terms**

	(1)	(2)	(3)	(4)
Panel A: Suicide Death Rates				
RealMaxUI*I[LayoffEvent]	-0.10***	-0.10***	-0.10***	-0.07**
	(0.03)	(0.03)	(0.03)	(0.03)
I[LayoffEvent]	0.31***	0.32***	0.32***	0.23**
	(0.10)	(0.10)	(0.10)	(0.10)
RealMaxUI	-0.02	-0.01	-0.01	0.30*
	(0.07)	(0.07)	(0.07)	(0.16)
N	189759	189759	189759	189759
Panel B: Drug-Overdose Death Rates				
RealMaxUI*I[LayoffEvent]	-0.27*	-0.27*	-0.27*	-0.05
	(0.14)	(0.14)	(0.14)	(0.04)
I[LayoffEvent]	0.77**	0.77**	0.77**	0.10
	(0.38)	(0.38)	(0.38)	(0.11)
RealMaxUI	-0.48	-0.49	-0.49	0.41*
	(0.37)	(0.37)	(0.37)	(0.22)
N	189759	189759	189759	189759
County FE	Y	Y	Y	Y
Quarter FE	Y	-	-	-
Year FE	Y	-	-	-
StateXYear FE	N	N	N	Y
YearXQuarter FE	N	Y	Y	Y
Demographic Controls	N	N	Y	Y

Notes: Estimates are based on county-by-quarter observations spanning 1995–2010 for 3,071 U.S. counties. The outcome variables are mortality rates per 100k county residents aged 25-54 corresponding to each panel title. The model includes an indicator for a mass layoff event in the previous quarter, the same variable interacted with maximum weekly unemployment insurance benefit (measured in hundreds of dollars), and other fixed effects and controls as indicated. County demographic controls include the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

Table A4: **Effect of UI on mortality following job loss: additional robustness**

	(1)	(2)	(3)
Panel A: Suicide Death Rates			
MaxUI*I[LayoffEvent]	-0.05**	-0.05***	-0.06***
	(0.02)	(0.02)	(0.02)
I[LayoffEvent]	0.20**	0.17**	0.24***
	(0.08)	(0.08)	(0.09)
MaxUI	0.24*	0.24*	
	(0.12)	(0.12)	
N	189,759	189,757	189,759
Panel B: Drug-Overdose Death Rates			
MaxUI*I[LayoffEvent]	-0.05	-0.07	-0.05
	(0.05)	(0.05)	(0.05)
I[LayoffEvent]	0.14	0.18	0.14
	(0.16)	(0.17)	(0.17)
MaxUI	0.35*	0.36*	
	(0.18)	(0.19)	
N	189,759	189,757	189,759
County FE	Y	-	Y
StateXYear FE	Y	Y	N
YearXQuarter FE	Y	Y	N
Demographic Controls	Y	Y	Y
StateXQuarter	Y	N	N
CountyXQuarter	N	Y	N
StateXYearXQuarter FE	N	N	Y

Notes: Estimates are based on county-by-quarter observations spanning 1995–2010 for 3,071 U.S. counties. The outcome variables are mortality rates per 100k county residents aged 25-54 corresponding to each panel title. The model includes an indicator for a mass layoff event in the previous quarter, the same variable interacted with maximum weekly unemployment insurance benefit (measured in hundreds of dollars), and other fixed effects and controls as indicated. County demographic controls include the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

Table A5: **Effect of UI on mortality following job loss: counties with above and below median layoff rates**

	(1)	(2)	(3)	(4)
Panel A: Suicide Death Rates				
MaxUI*I[AboveMedianLayoff]	-0.14***	-0.14***	-0.08***	-0.09**
	(0.04)	(0.04)	(0.03)	(0.03)
MaxUI*I[BelowMedianLayoff]	-0.10***	-0.10***	-0.05**	-0.06***
	(0.03)	(0.03)	(0.02)	(0.02)
I[AboveMedianLayoff]	0.52***	0.53***	0.32**	0.30**
	(0.16)	(0.16)	(0.12)	(0.14)
I[BelowMedianLayoff]	0.37***	0.37***	0.18**	0.22***
	(0.10)	(0.10)	(0.09)	(0.08)
MaxUI	0.05	0.05	0.05	0.25*
	(0.05)	(0.05)	(0.05)	(0.13)
N	189,759	189,759	189,759	189,759
Panel B: Drug-Overdose Death Rates				
MaxUI*I[AboveMedianLayoff]	-0.46***	-0.47***	-0.23**	-0.06
	(0.15)	(0.15)	(0.10)	(0.06)
MaxUI*I[BelowMedianLayoff]	-0.31**	-0.31**	-0.11	-0.05
	(0.14)	(0.14)	(0.09)	(0.04)
I[AboveMedianLayoff]	1.49***	1.52***	0.74**	0.15
	(0.50)	(0.51)	(0.35)	(0.21)
I[BelowMedianLayoff]	1.03**	1.03**	0.29	0.10
	(0.46)	(0.46)	(0.30)	(0.15)
MaxUI	-0.28	-0.28	-0.14	0.32*
	(0.25)	(0.25)	(0.25)	(0.19)
N	189,759	189,759	189,759	189,759
County FE	Y	Y	Y	Y
Quarter FE	Y	-	-	-
Year FE	Y	-	-	-
StateXYear FE	N	N	N	Y
YearXQuarter FE	N	Y	Y	Y
Demographic Controls	N	N	Y	Y

Notes: Estimates are based on county-by-quarter observations spanning 1995–2010 for 3,071 U.S. counties. The outcome variables are mortality rates per 100k county residents aged 25-54 corresponding to each panel title. The model includes indicators for above and below the median layoff rate, the same variables interacted with the maximum weekly unemployment insurance benefit (measured in hundreds of dollars) and other fixed effects and controls as indicated. County demographic controls include the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

Table A6: **Effect of UI on Suicide: varying the intensity of mass layoffs**

	(1) Tercile Groups	(2) Quartile Groups	(3) Quintile Groups
MaxUI*I[Group 1]	-0.05**	-0.05***	-0.06***
	(0.02)	(0.02)	(0.02)
MaxUI*I[Group 2]	-0.07**	-0.05	-0.02
	(0.03)	(0.03)	(0.04)
MaxUI*I[Group 3]	-0.10***	-0.07*	-0.05
	(0.03)	(0.04)	(0.04)
MaxUI*I[Group 4]		-0.08**	-0.10**
		(0.04)	(0.04)
MaxUI*I[Group 5]			-0.05
			(0.04)
I[Group 1]	0.20**	0.20***	0.23***
	(0.08)	(0.07)	(0.07)
I[Group 2]	0.23*	0.18	0.12
	(0.13)	(0.12)	(0.13)
I[Group 3]	0.28**	0.28	0.15
	(0.13)	(0.18)	(0.15)
I[Group 4]		0.23	0.35**
		(0.16)	(0.16)
I[Group 5]			0.08
			(0.18)
N	189,759	189,759	189,759
County FE	Y	Y	Y
StateXYear FE	Y	Y	Y
YearXQuarter FE	Y	Y	Y
Demographic Controls	Y	Y	Y

Notes: Estimates are based on county-by-quarter observations spanning 1995–2010 for 3,071 U.S. counties. The outcome variables are mortality rates per 100k county residents aged 25-54. The model includes indicators for each group within terciles, quartiles, and quintiles of mass layoff rates according to the column title, the same variables interacted with the maximum weekly unemployment insurance benefit (measured in hundreds of dollars) and other fixed effects and controls as indicated. County demographic controls include the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

Table A7: **Effect of UI on male suicide following job loss: by age**

	Age					
	25-54	18-25	26-35	36-45	46-55	56-64
MaxUI*I[LayoffEvent]	-0.10***	-0.04	-0.02	-0.05	-0.22***	0.01
	(0.03)	(0.11)	(0.05)	(0.08)	(0.06)	(0.10)
I[LayoffEvent]	0.38***	0.20	0.15	0.21	0.80***	-0.05
	(0.14)	(0.38)	(0.18)	(0.31)	(0.24)	(0.36)
MaxUI	0.24	-0.10	0.18	0.15	0.37	0.41
	(0.21)	(0.38)	(0.37)	(0.27)	(0.37)	(0.45)
Mean of Dep. Var	5.903	5.376	5.533	5.999	6.204	5.581
N	189,759	189,758	189,759	189,759	189,759	189,759
County FE	Y	Y	Y	Y	Y	Y
StateXYear FE	Y	Y	Y	Y	Y	Y
YearXQuarter FE	Y	Y	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y	Y	Y

Notes: Estimates are based on county-by-quarter observations spanning 1995–2010 for 3,071 U.S. counties. The outcome variables are male suicide rates per 100k male county residents in various age group bins corresponding to each column title. The model includes an indicator for a mass layoff event in the previous quarter, the same variable interacted with the maximum weekly unemployment insurance benefit (measured in hundreds of dollars) and other fixed effects and controls as indicated. County demographic controls include the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, male, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

Table A8: **Effect of UI on female suicide following job loss: by age**

	Age					
	25-54	18-24	25-34	35-44	45-54	56-64
MaxUI*I[LayoffEvent]	-0.02	0.01	-0.01	-0.02	-0.02	-0.02
	(0.02)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)
I[LayoffEvent]	0.08	-0.08	0.05	0.07	0.09	0.07
	(0.07)	(0.15)	(0.15)	(0.11)	(0.14)	(0.11)
MaxUI	0.25***	0.16	0.17	0.22	0.28	0.01
	(0.09)	(0.11)	(0.19)	(0.19)	(0.20)	(0.19)
Mean of Dep. Var	1.650	0.926	1.236	1.742	1.952	1.536
N	189,759	189,758	189,758	189,759	189,759	189,759
County FE	Y	Y	Y	Y	Y	Y
StateXYear FE	Y	Y	Y	Y	Y	Y
YearXQuarter FE	Y	Y	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y	Y	Y

Notes: Estimates are based on county-by-quarter observations spanning 1995–2010 for 3,071 U.S. counties. The outcome variables are female suicide rates per 100k female county residents in various age bins corresponding to each column title. The model includes an indicator for having a mass layoff event in the previous quarter, the same variable interacted with the maximum weekly unemployment insurance benefit (measured in hundreds of dollars) and other fixed effects and controls as indicated. County demographic controls include the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, male married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

Table A9: **Effect of UI on Cancer Deaths Following Mass Layoff**

	(1) Stomach	(2) Colon	(3) Liver	(4) Pancreas	(5) Lung	(6) Breast	(7) Cervix	(8) Ovarian	(9) Prostate
MaxUI*I[LayoffEvent]	0.00	-0.01	-0.01	-0.01	-0.07***	0.01	0.01	-0.02**	0.01*
	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.00)	(0.01)	(0.00)
I[LayoffEvent]	-0.01	0.04	0.03	0.03	0.20**	-0.03	-0.03*	0.05**	-0.02*
	(0.02)	(0.03)	(0.03)	(0.03)	(0.08)	(0.03)	(0.01)	(0.02)	(0.01)
MaxUI	0.02	-0.02	0.03	-0.02	-0.02	-0.03	-0.02	0.00	-0.02
	(0.03)	(0.07)	(0.04)	(0.06)	(0.06)	(0.07)	(0.04)	(0.03)	(0.02)
Mean of Dep. Var	0.273	0.981	0.423	0.510	2.324	1.440	0.280	0.334	0.0894
N	189,759	189,759	189,759	189,759	189,759	189,759	189,759	189,759	189,759
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
StateXYear FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
YearXQuarter FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: Estimates are based on county-by-quarter observations spanning 1995–2010 for 3,071 U.S. counties. The outcome variables are mortality rates per 100k county residents aged 25-54 corresponding to each column title. The model includes an indicator for a mass layoff event in the previous quarter, the same variable interacted with maximum weekly unemployment insurance benefit (measured in hundreds of dollars), and other fixed effects and controls as indicated. County demographic controls include the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

Table A10: **Comparison of effects of UI generosity on mortality: mass-layoff unemployment and other unemployment**

	(1)	(2)	(3)	(4)
MaxUI*I[LayoffEvent]	-0.11***	-0.11***	-0.05**	-0.06***
	(0.03)	(0.03)	(0.02)	(0.02)
I[LayoffEvent]	0.40***	0.42***	0.21**	0.24***
	(0.11)	(0.11)	(0.09)	(0.08)
MaxUI	0.05	0.04	0.06	0.21
	(0.07)	(0.07)	(0.07)	(0.13)
NetUrate	0.03	0.02	0.03*	-0.03
	(0.02)	(0.02)	(0.02)	(0.02)
MaxUI*NetUrate	-0.00	0.00	-0.00	0.01
	(0.01)	(0.01)	(0.00)	(0.01)
N	189,711	189,711	189,711	189,711
County FE	Y	Y	Y	Y
Quarter FE	Y	-	-	-
Year FE	Y	-	-	-
StateXYear FE	N	N	N	Y
YearXQuarter FE	N	Y	Y	Y
Demographic Controls	N	N	Y	Y

Notes: Estimates are based on county-by-quarter observations spanning 1995–2010 for 3,071 U.S. counties. The outcome variables are mortality rates per 100k county residents aged 25-54. The model includes an indicator for a mass layoff event in the previous quarter, the non-layoff unemployment rate in the previous quarter, these two variables interacted with the maximum weekly unemployment insurance benefit (measured in hundreds of dollars), and other fixed effects and controls as indicated. County demographic controls include the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.

Table A11: **Effect of UI generosity measured by UI duration on suicides following job loss: 2008-2010**

	(1)	(2)	(3)	(4)
MaxUI*UIDuration	-0.33***	-0.34***	-0.30**	-0.27*
	(0.12)	(0.12)	(0.12)	(0.14)
UIDuration	0.24	0.31	0.27	0.14
	(0.21)	(0.35)	(0.35)	(0.36)
I[LayoffEvent]	0.41**	0.42**	0.37**	0.33*
	(0.16)	(0.16)	(0.16)	(0.17)
N	33,657	33,657	33,657	33,657
County FE	Y	Y	Y	Y
Quarter FE	Y	-	-	-
Year FE	Y	-	-	-
StateXYear FE	N	N	N	Y
YearXQuarter FE	N	Y	Y	Y
Demographic Controls	N	N	Y	Y

Notes: Estimates are based on county-by-quarter observations spanning 1995–2010 for 3,071 U.S. counties. The outcome variables are mortality rates per 100k county residents aged 25-54. The model includes an indicator for a mass layoff event in the previous quarter, the same variable interacted with the potential duration of the unemployment insurance benefit, and other fixed effects and controls as indicated. County demographic controls include the fraction of the population that are college educated, high school educated, less than high school educated, White, Black, Hispanic, other race, married, and aged 25-54. The regressions are weighted by the county population aged 25-54 and standard-error estimates allow for clusters at the state level.