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The effect of job loss and unemployment duration on suicide risk in the United States: a new look using mass-layoffs and unemployment duration*

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Abstract

We examine the link between employment status and suicide risk using a panel of US states from 1996 to 2005 with monthly data on suicides, the duration of unemployment spells and the number of job losses associated with mass layoff events. The use of aggregate data at the monthly level along with the distribution of unemployment duration allows us to separate the effect of job loss from the effect of unemployment duration, an important distinction for policy purposes, especially for the timing of potential interventions. Our results are consistent with unemployment duration being the dominant force in the relationship between job loss and suicide. Nevertheless, mass layoffs may be powerful localized events where suicide risk increases shortly afterward. Implications for the design of unemployment insurance are discussed.

Keywords

Suicide; Unemployment; Mortality; Aggregate panel data

I. Introduction

Accounting for over 33,000 deaths annually, suicide is the 11th leading cause of mortality in the United States and the second most common cause of death among adults age 24 to 35 years old. A decade ago, the Surgeon General of the United States, David Satcher, issued a call to action to "… address suicide as a significant public health problem and put into place national strategies to prevent the loss of life and the suffering suicide causes." It was acknowledged then that a vital component of any comprehensive national prevention strategy should be to understand the risk factors associated with suicide. Many of these factors—alcohol or substance abuse, mental illness, gun ownership—may only be observed by the at-risk individual or a few close acquaintances. Other influential factors, such as job loss or divorce, may be readily identifiable by government or public health authorities. Since the latter set of events often requires interaction with government agencies, e.g. through unemployment insurance or the family court system, in these instances it may be possible to cost-effectively assess suicide risk and intervene with appropriate care when advisable. With these policy implications in mind, the current paper examines the link between job loss and suicide risk, a long-standing empirical question in social science research.

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Methodologically, it departs from the majority of previous work by joining a small, yet growing literature that uses aggregate longitudinal data.[1] But, it is unique in several important respects. First, previous work has utilized the unemployment rate, which is a rather crude measure that conceals the turbulent nature of the labor market, as unemployment is the interaction between distinct processes: job loss, which determines the start of unemployment, and job matching, which determines the duration of unemployment. This is a non-trivial distinction since the set of policy interventions to prevent suicide will vary depending upon which aspects of unemployment are responsible for increased risk of suicide. If suicide risk increases soon after a job separation, then early intervention when receipt of unemployment insurance begins may be most effective; if unemployment duration is the primary contributor, however, then intervention may be most cost-effective when unemployment insurance benefits are set to expire. To separate the effect of the former (*job loss*) from the latter (*unemployment duration*), we use information on the number of workers at different durations of unemployment (less than 5 weeks, 5 to 14 weeks, 15 to 26 weeks and more than 26 weeks) provided by the Bureau of Labor Statistics (BLS). Changes in the number of workers who have been unemployed less than 5 weeks will identify the immediate effect of job loss, while the other three duration lengths will identify the effect of longer unemployment spells. We also use data on mass layoff events–when more than 50 workers apply for unemployment insurance benefits from the same employer within a 30 day span— to examine how large scale job loss that is plausibly exogenous to aggregate mental health affects suicide risk.

Finally, unlike previous work that has only considered variation in annual rates of suicide and unemployment across geographic regions, we exploit variation in monthly rates. Doing so greatly increases the amount of variation within a narrow time span, thereby reducing concerns that structural shifts occur during the period under consideration. Furthermore, it allows for a richer illustration of the dynamic response to job loss that is otherwise obscured by aggregating over long time periods. In addition, the annual unemployment rate is likely correlated with other business cycle phenomena that independently affect suicide risk. For example, sustained levels of high unemployment will tend to be associated with lower tax revenue and potentially fewer resources allocated to mental health outreach. Since state budgetary decisions are typically made on an annual basis, month-to-month variation in our unemployment measures is less likely to be correlated with state fiscal policy.

Our results suggest that moderate to long unemployment spells are a significant risk factor for suicide, but in general, the probability of committing suicide does not respond in the period immediately following job loss. For both males and females, an increase in the number of individuals who are unemployed less than 5 weeks is not associated with the number of suicides. In contrast, increasing the number of workers who are unemployed 15 to 26 weeks tends to increase the number of suicides among both males and females Thus, for most instances of job loss the duration of unemployment is a more important determinant of suicide risk than job loss in itself. However, an increase in the mass layoff rate is positively associated with changes in the suicide rate, a result that is particularly strong for males. In terms of policy, interventions aimed at the newly unemployed, say through the UI system are probably misguided. Instead, future research that examines the cost-effectiveness of interventions directed toward workers who have lost their jobs as part of a mass-layoff or have exhausted their UI benefits would be worthwhile.

[1]These studies consider a variety of geographies including: US states (Markowitz, Chatterji and Kaestner, 2003; Kunce and Anderson, 2002; and Ruhm, 2000); German Länder (Neumayer, 2004); European nations (Andrés, 2005); Taiwanese cities and counties (Chuang and Huang, 1997; Lin, 2006); Asian nations (Lin, 2006); transitioning nations in Europe and the former Soviet Union (Brainerd, 2001); and a mix of developed and developing nations (Neumayer, 2003).

The paper is organized as follows: Section II presents theoretical arguments for a causal connection between employment status and suicide risk. Section III discusses methodological issues associated with empirically identifying these causal links. Section IV describes the data. Section V presents an econometric specification that accommodates state-specific fixed effects, linear trends and cycles. Section VI presents results. Section VII discusses and concludes.

II. Theoretical motivation

There are several pathways through which job loss can influence the decision to commit suicide. Job loss can be a profound psychological shock leading some to develop depressive mental illness and then to commit suicide. Similarly, prolonged unemployment can challenge an individual's conception of self-worth, triggering depressive mental illness.[2]

In addition, there are several indirect pathways through which job loss can increase suicide risk. Unemployment reduces the level of household resources and financial stress can be a trigger for depression (Vinokur et al., 1996 and Theodossiou, 1998). Lower income might also result in the interruption of treatment for pre-existing mental illness as households allocate their limited resources away from health care toward other necessities such as food and housing. Financial stress and job loss can also strain relationships between household members by increasing the probability of domestic violence, substance abuse and divorce (Bowlus and Seitz, 2006; Tauchen and Witte, 1994). In this sense, job loss can affect the suicide risk of all household members, not just the individual who suffered the job loss.

Moreover, there is increasing recognition that episodes of involuntary unemployment lead to lower lifetime earnings; greater volatility in earnings and increased probability of future unemployment over the course of an individual's career (Ruhm, 1991; Jacobson, Lalonde and Sullivan, 1993; Stevens, 1997; and Chan and Stevens, 2001). An even larger literature documents the positive association between health status and income.[3] Indeed, using job histories of workers in Pennsylvania, Sullivan and von Wachter (2009) find that mortality risk doubles in the year of displacement; is 75% higher in the year after displacement; is 50% higher in years two and three after displacement; and eventually settles down to 15% higher in each subsequent year.

In the United States, these direct and indirect pathways interact through the health care system. The majority of working-age Americans receives health insurance coverage through employer provided plans, often paying only a small share of the premium. Even though unemployed workers can maintain their insurance coverage for up to 18 months under the provisions of the Consolidated Omnibus Budget Reconciliation Act (COBRA), they must assume responsibility for the entire premium. And while the loss of employer-based insurance coverage certainly affects the affordability of health care, moving from one health insurance plan to another because of job loss can also affect continuity of care, which may be particularly important in mental health treatments, such as that for depression (Melfi et al., 1998).

To summarize, increased probability of depressive illness, financial strain, decreased affordability of mental health care and disruption of current treatment regimes all suggest plausible theoretical pathways through which job loss and subsequent unemployment would have an effect on suicide rates. Using unemployment duration data, we hope to better

[2]See Cavanagh et al. (2003) for a thorough review of these literatures.

[3]Ettner (1996) provided some of the first estimates of the causal influence of income on health. Smith (1999) and Deaton (2002) offer valuable reviews.

understand when in the course of unemployment these forces are most relevant for suicide risk.

III. Issues in empirical analysis

As Jones, Forster and Hassanyeh (1991) point out, a statistical association between employment status and suicide could reflect 1) a direct causal connection; 2) an indirect pathway with unemployment acting as a negative shock that increases the probability of other events that lead to suicide, such as divorce or financial strain; or 3) a spurious relationship driven by an omitted attribute that affects both unemployment and suicide risk, such as mental health or education level. When using individual-level data, investigators face the trade-off between the low statistical power of comprehensive data-sets and the lack of important explanatory variables in larger census and mortality data-sets. While a handful of researchers have linked nationally collected mortality data with other large datasets, these surveys lack adequate questions about mental health state—both diagnosis and treatment.[4] It is difficult, therefore, to assign a causal interpretation to the results since an underlying mental illness may independently explain both unemployment and suicide.[5]

Given the above challenges when analyzing individual level data, many researchers have relied upon aggregate data, using variation in suicide and unemployment rates across regions and time. By defining a sufficiently large geographic area over a sufficiently long period of time, it is possible to observe enough variation in suicide rates to generate the statistical power required for precise estimates. But cross-sectional studies can still suffer from their own variety of omitted variables bias since the unemployment rate is likely associated with other socio-demographic variables that influence suicide. Using longitudinal data can overcome this issue by including fixed-effects and trends in the empirical specification.

While the effect of data structure on the ability to infer causal relationships is widely acknowledged in the suicidology literature, consideration of alternative explanatory labor market variables other than the unemployment rate is largely absent. To our knowledge, studies with aggregate data use the unemployment rate as the sole measure of joblessness. But, the unemployment rate is a function of both the job separation rate and the job finding rate. Consider a region with a labor force, L, of 10,000 workers; a job finding rate, φ, of 95%; and a job separation rate, σ, of 5%. The steady-state unemployment rate is $\sigma/(\varphi+\sigma)=5\%$; 9500 σ=475 employed workers lose their job each period; and 500 φ =475 unemployed workers find new jobs each period. If the job separation rate jumped to 6% from the steady-state, then the number of newly unemployed workers would increase to 570 and the total number of unemployed would be $9500\,\sigma+500(1-\varphi)=595$. If instead the job separation rate remained at 5% while the job finding rate fell to 76%, then the number of new unemployed workers would remain at 475, but the number of workers who are unemployed for an additional period would increase from 25 to 120. The total number of unemployed would also equal 595, but the cause of the increase in unemployment would come entirely from workers facing longer unemployment spells. Using only the unemployment rate, it is impossible to disentangle the effect of job loss and the effect of unemployment duration on suicide risk. This result motivates using the distribution of unemployment duration to separately identify the effect of job loss and the effect of unemployment duration.

[4]For example, Blakely, Collings and Atkinson (2003) match suicide reports to the 2001 New Zealand Census and Kposowa (2001) matches suicide reports in the United States to the monthly Current Population Survey.

[5]In an attempt to address this problem, Blakely, Collings and Atkinson (2003) use the relative (mental illness vs. no mental illness) risk of suicide and the relative (unemployed vs. employed) risk of having mental illness to adjust their estimates. Doing so cuts the relative risk of suicide by unemployed males from 2.59 to 1.88, a 47% reduction in relative risk though it is still statistically significant.

IV. Data

Our empirical analysis uses variation in monthly suicide rates and the distribution of unemployment duration for 50 states and the District of Columbia between 1996 and 2005. In addition, we examine the relationship between suicide and jobs lost from mass layoff events. Although data on causes of death are readily available for years prior to 1996, standardized reporting of mass layoff events only begins at that time.

Suicide data come from the National Center for Health Statistics Mortality Files, which report the total number of suicides in each state by age and gender.[6] Because we are interested in the relationship between employment status and suicide, only suicide deaths by adults between the ages of 20 and 59 years are considered.[7] It is widely known that both the suicide rate and the method of suicide differ greatly by gender, leading most researchers to analyze males and females separately. This convention is followed here. The data are thus comprised of 6,120 state-month-gender observations.

Monthly state-level information on the number of mass layoffs—the number of workers who lost their jobs during a mass layoff event—are collected from the Bureau of Labor Statistics. The BLS collects layoff information from establishments with at least 50 initial claims for unemployment insurance filed against them during any 5-week period, including the number of workers involved. An event is categorized as an extended mass layoff if the private sector non-farm employer indicates at least 50 workers were separated from their job for at least 31 consecutive days. To maintain the anonymity of data providers, the BLS does not report mass-layoff information if too few firms in a state record mass-layoffs over the course of the month. Thus, smaller states like Wyoming and Vermont occasionally have missing observations.

Although the BLS reports the average monthly unemployment duration at the national level, due to small state sample sizes in the Current Population Survey (CPS), state-level monthly estimates are not generally available because they do not meet BLS publication standards. Nonetheless, we do have 12-month moving averages of the distribution of unemployment duration for each state, specifically, estimates of the number of individuals (measured in thousands) who were unemployed for: less than 5 weeks, 5 to 14 weeks, 15 to 26 weeks and greater than 26 weeks. These data are estimates from the CPS and values are rounded to thousands. Rounding obviously generates measurement error, though we suspect that it is classical in nature, which would tend to attenuate our coefficient estimates.

As a robustness check, we will also analyze the relationship between new unemployment insurance claims (NUIC) and suicide. The BLS collects NUIC data from state agencies and reports monthly values for each state. Although this is not a census of newly unemployed workers, since some workers are not eligible or chose not to apply, changes in the number of new claims for UI are nevertheless indicative of changes in the number of recent job separations.

Population by gender and age are taken from the annual Census population estimates. Monthly population is interpolated under alternative assumptions of linear and exponential growth. Results are robust to the choice of procedure.

[6]Annual data is readily available online and in print. Monthly data were graciously provided by statisticians at the Centers for Disease Control.

[7]Although we do not have monthly suicide data at the state level by age, repeating our analysis using state-level annual data from 1981 to 2005 revealed that the coefficient estimate on the unemployment rate was robust to the selection of upper and lower limits for the age range.

Previous authors using longitudinal data have included a number of other variables believed to affect suicide rates, including divorce and fertility rates, alcohol consumption and insurance coverage (Andrés, 2005; Markowitz et al., 2004). Others have included religious background and race/ethnicity among the explanatory variables. For many of these variables such as insurance status, monthly data are not reliably available and attempts to impute monthly values from annual data could introduce sizeable measurement error. In addition, these variables tend to be constant over time or subject to broader nationwide trends. Therefore, it is possible to account for them by including state and period fixed effects with state-specific time trends.

Table 1 reports summary statistics for the suicide rate by gender as well as different job loss measures with observations weighted by state population. Among males age 20–59, there was an average of 1.9 suicide deaths per 100,000 individuals each month, almost four times as large as the monthly average for females. In smaller states there were months without any reported suicide deaths. The highest suicide rates for both males and females were reported for Wyoming—January 2002 and April 2005, respectively.

Based on 12-month moving averages, in the average state, i.e. a hypothetical state that accounted for exactly one-fiftieth of total US population, there would be 127,000 workers each month who were unemployed for less than five weeks. Since rounding to thousands of workers implies an error of between ±500 workers, the noise in any one month introduced purely by rounding is at most 0.4%. For the number of workers unemployed between 15 and 26 weeks, the interval with the least mass, the potential noise ratio is 1.0%. This is a good indication that rounding, while introducing some measurement error, should not greatly influence our estimates.

It is important to recognize that during the ten year period under consideration, two catastrophic events took place: the terrorist attacks in New York, Washington D.C. and Pennsylvania in September 2001 and the destruction accompanying Hurricane Katrina in Louisiana and Mississippi during September 2005. The latter is clearly reflected in the data as the largest reported monthly mass layoff rate was just over 4,200 for Louisiana in September 2005, followed by 1,704 for Mississippi in September 2005. Even when these observations are omitted (as we do in the subsequent analysis), the variation in the mass layoff rate far exceeds that in the unemployment rate. The highest non-Katrina related mass layoff rate is 903 for Alabama in January 2002, more 250 times larger than the smallest value reported by the BLS for New York in March 2000.[8] The coefficient of variation (COV=standard deviation/mean) for the mass layoff rate was 0.99, more than four times larger than the COV of the unemployment rate.[9] The distribution of unemployment duration is evenly distributed between less than 5 weeks (36.9%), 5 to 14 weeks (30.1%) and more than 14 weeks (32.6%). Finally, the weighted mean of new unemployment insurance claims per month is 67,300, or roughly 53% of the mean of workers unemployed less than 5 weeks. This difference is reasonable since not all workers are eligible for unemployment insurance (e.g. the self-employed) and some eligible workers will decide not to apply. It is noteworthy, however, that the population-weighted correlation between the two series is 0.91, suggesting that movements in one series are closely reflected in the other.

Table 1 presents averages over state-year observations, obviously concealing variation that occurs over time. Indeed, it is the change in these rates over time that will be used to identify the parameters of interest, thus Figure 1 plots the population weighted average monthly

[8]Although the 2002 hurricane season was active in the Gulf Coast, Alabama was not appreciably affected by the landfall of a major hurricane.

[9]During the period of this study, the average unemployment rate was 5.1 percent with a standard deviation of 1.1.

suicide rate by gender over the study period along with the six month moving average as deviations from the mean reported in Table 1. From 1996 to 2001, the suicide rate for males exhibited a steady trend downward. After 2001, however, the behavior of the suicide rate changed in two respects 1) its decline stopped and then reversed somewhat and 2) it became highly cyclical. In the second half of the series, the suicide rates in June, July and August are noticeably higher than in surrounding months. The pattern after 2001 is similar for females, though the downward trend in suicide rates before 2001 is less pronounced.

The weighted average monthly mass layoff rate is presented in the top panel of Figure 2. For reference, the weighted average of the unemployment rate is also plotted. The mass-layoff rate is significantly more volatile than the unemployment rate. The within-COV, the ratio of the standard deviation within group to the mean, equals 0.18 for the unemployment rate compared to 0.85 for the mass layoff rate. Notice also that the unemployment rate exhibits trends that are largely absent for the mass layoff rate. For example, there is a general decline in the unemployment rate between 1996 and 2001, while the mass-layoff rate fluctuates around a relatively stable mean. It also appears that the mass-layoff rate is at least weakly seasonal, indicating that the inclusion of month or month-year dummy variables is required. It is also worth pointing out that the two measures are not strongly correlated with each other, either in levels or in first-difference. The correlation coefficient between the mass layoff rate and the unemployment rate is 0.18 and falls to 0.15 when using first-differences. The distribution of unemployment duration between 1996 and 2005 is presented in the bottom of Figure 2, but as these data are reported as 12-month moving averages, month-to-month variation is greatly smoothed. It is evident that while the number of workers who are unemployed less than five weeks is relatively flat over the time period, the number of workers who are unemployed for 15 weeks or more increases greatly, nearly doubling between 2001 and 2004.

V. Estimation

The choice of empirical specification for estimation is motivated by the nature of the available data. First, the plotted suicide rates in Figure 1 exhibit both a trend and cycle. It may thus be important to allow both to be state-specific. Working with year-on-year differences offers a convenient method of incorporating state-specific fixed-effects, linear trends, and cycles. Specifically, consider the following specification:

$$S_{j,t}=\alpha_0+\alpha_j+\beta X_{j,t}+\gamma_t+\delta_j t+\mu_{j,m}+\varepsilon_{j,t} \quad (1)$$

where $S_{j,t}$ is the number of suicides in state *j* in time *t*; α_j is a time-invariant state specific fixed effect; $X_{j,t}$ is a vector of labor market characteristics for state *j*; β is a vector of coefficients that do not vary by state or year; γ_t is a period (month-year) effect that captures shocks affecting all states in the same manner; $\mu_{j,m}$ is a state-specific month effect, the set of which capture state-specific cycles; δ_j is a state-specific linear time trend; and $\varepsilon_{j,t}$ are error terms that either represent time-varying measurement error or omitted variables that vary over time and across state. Estimation of (1) directly involves 50 α_j's, 120 γ_t's, 600 $\gamma_{j,m}$'s and 50 δ_j's. Taking the year-on-year difference of (1) yields:

$$\begin{aligned} S_{j,t}-S_{j,t-12} &= \beta(X_{j,t}-X_{j,t-12})+(\gamma_t-\gamma_{t-12})+12\delta_j+(\varepsilon_{j,t}-\varepsilon_{j,t-12}) \\ &= \beta(X_{j,t}-X_{j,t-12})+\phi_j+\zeta_t+\nu_{j,t} \end{aligned} \quad (2)$$

which removes both the state and state-month fixed-effects, greatly reducing the number of parameters that need to be estimated.10 To allow for serial correlation in the error term, it is assumed that $\nu_{j,t}$ follows an AR(1) process with a correlation parameter, ρ, that is common to all states and must be estimated. To control for changes in population (*ceteris paribus*, an increase in population should increase the number of suicides), the year-on-year change in population is included in the set of explanatory variables.

Using year-on-year differences in the number of monthly suicides also introduces a simple method of incorporating the unemployment duration information. Recall that while state-level suicide rates are available for each month, the distribution of unemployment duration is only reported as 12-month moving averages. However, first-differencing a 12-month moving average yields the year-on-year difference. That is, the difference between the 12-month moving average for March 1996 and February 1996 is (one-twelfth) the difference in levels between March 1996 and March 1995.

First-differencing the unemployment duration data is not without issue, however. The BLS does not report monthly duration distributions at the state-level, or indeed any monthly unemployment statistic at the state-level calculated solely from the Current Population Survey,[11] because such estimates would be based on relatively small sample sizes. Year-on-year changes in unemployment duration will therefore exhibit (potentially large) measurement error, though as with the rounding issue described previously, it is likely classical in nature.

Assuming that the measurement error is classical, subsequent coefficient estimates would then be subject to the usual attenuation bias. Since the number of workers who are unemployed less than 5 weeks is highly correlated with the number of new unemployment insurance claims, repeating the analysis using the latter as the measure of short-term unemployment will give some indication of the bias introduced by using the former. As the distribution of unemployment duration is close to uniform across the intervals of less than 5 weeks, 5 to 14 weeks and greater than 14 weeks, we do not expect measurement error with respect to short-term unemployment to be different than the measurement error with respect to longer unemployment durations. Hence, the greater the similarity in results when using the BLS unemployment duration data and the state NUIC data, the more confidence we have that the data reliability issues the BLS outlines are not influencing our findings.

Finally, concern about measurement error leads us to avoid estimation in rates: because of rounding, changes in the rate would in large part be driven by estimated changes in population rather than changes in the number of unemployed in each duration interval. By estimating in levels, we avoid at least one additional source of measurement error.

VI. Results

The first two columns of Table 2 report estimation results of Equation (2) separately by gender when the number of workers at various stages of unemployment duration and the number of mass layoffs are included together in the set of explanatory variables (results when unemployment duration and mass layoffs enter separately are reported in the remaining columns).

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[10]Since the number of suicides is censored at zero, equation (2) describes the latent rather than the observed dependent variable. In our data, only 22 state-month observations report zero suicides for males (.4%) and 362 report zero suicides for females (6.5%). Moreover, when the total number of suicides was used as the dependent variable in a conditional (fixed-effects) negative binomial regression with the mass layoff and NUIC rates as explanatory variables, omitting observations with zero reported suicides did not influence coefficient estimates. The relatively small number of censored observations and the robustness of results to dropping zeros when using count regression models suggests that treating (2) as the observed rather than the latent relationship is not an unwarranted simplification Thus, while a number of papers have investigated the estimation of panel data with censoring (Honoré 1992; Honoré and Powell, 1994; Honoré Kyriazidou and Powell, 2000), no attempt is made to explicitly address censoring here.

[11]Monthly state-level unemployment rates are calculated using data from several sources (the Current Population Survey, the Current Employment Statistics and state unemployment insurance information) and generated using regression analysis.

For both males and females, the coefficient estimate on the number of newly unemployed workers (duration less than 5 weeks) is small and statistically insignificant. As described previously, attenuation bias from measurement error is a potential explanation for the magnitude of these coefficients. However, replacing the number of workers unemployed less than 5 weeks with the number of new unemployment insurance claims yields similar results.[12] This gives us confidence that our empirical results are not driven by measurement issues in the calculation of monthly unemployment duration.

In contrast, the coefficient on the number of workers who have been unemployed between 15 and 26 weeks is large, positive and statistically significant. Recall that if the difference in 12 month moving averages is 1,000 unemployed, the year-on-year difference is 12,000 unemployed. Assuming 60% of these workers are male (males make up roughly 60% of the unemployed), for every 7,200 additional males who are unemployed between 15 and 26 weeks, we expect 0.36 additional suicides or one additional suicide for every 20,200 males unemployed (0.6*12,000/0.36) between 15 and 26 weeks. The coefficient estimate on the number of workers unemployed longer than 26 weeks is positive, but not statistically significant.

For females, the coefficient estimates on each of the unemployment duration lengths beyond 5 weeks are positive and statistically significant. Moreover, the magnitude of the coefficient is increasing in duration length implying one additional suicide for every: 43,600 females unemployed between 5 and 14 weeks; 18,700 females unemployed between 15 and 26 weeks; and 16,600 females unemployed more than 26 weeks.

Although the number of newly unemployed is not associated with the number of suicides, an increase in the number of job losses from mass layoff events is positively related to suicide deaths for both males and females. Increasing the number of individuals losing their job through a mass layoff event in a month by 1,000 increases the number of suicides by males by 0.142, or one additional suicide death per month for every 4,200 males (0.6*1000/0.142) who lose their job as part of mass layoffs. For females, we estimate one additional suicide for every 7,100 mass layoffs (0.4*1000/0.057).

In the regression for males, the estimated correlation coefficient on the error term assuming an AR(1) process is 0.10, implying a relatively small degree of serial correlation. For females, the estimated correlation coefficient is even smaller, 0.07.

Coefficient estimates are similar when the distribution of unemployment duration and the number of mass layoffs enter separately. Indeed, the very small differences between the estimates for mass layoffs strongly suggests that any measurement error in the calculation of the unemployment duration distribution exerts only a minimal influence on the estimated relationship between suicide risk and mass layoff events. This gives us greater confidence that the noise to signal ratio in our unemployment duration measures is relatively small.

VII. Discussion and Conclusion

Suicide is one of the leading causes of death among working-age adults in the United States, but the underlying pathologies—depression, substance abuse, other mental illness—tend to be under-diagnosed and under-treated. Moreover, the number of successful suicides is small relative to the population suffering from these diseases. While better diagnosis and treatment

[12]For instance, for females the coefficient estimate on NUIC remains small and statistically insignificant. For males, the coefficient on NUIC is positive, but exceptionally small in magnitude, implying one suicide death for every 120,000 additional newly unemployed males. Moreover, the effect on other coefficient estimates is also small: the coefficient on mass layoffs falls from 0.056 to 0.052 for females and increases from 0.148 to 0.167 for males.

for depression is certainly desirable in its own right, for policy-makers with the specific goal of lowering the number of suicides, interventions broadly directed toward all individuals with depression may be less cost-effective than focusing more narrowly on observable events that trigger suicide, such as job loss. This paper makes several methodological contributions to the literature and offers a number of important results.

First, our measure of recent job loss—the number of workers who have been unemployed less than 5 weeks—exhibits no explanatory power for suicide risk in either gender. Results using monthly new unemployment insurance claims corroborate this finding and mitigate concerns over measurement error in monthly unemployment duration measures. This is suggestive that in general, job loss itself does not increase suicide risk.

Second, a narrower measure of recent job loss based on mass layoff events is positively associated with the suicide rate for males and females. We estimate one additional suicide death for every 4,200 males who become unemployed as part of a mass layoff and one additional suicide death for every 7,100 females who lose their job as a result of a mass layoff. Several plausible explanations exist for this result. It is possible that the nature of the jobs lost because of a mass layoff event is different: the prospects of finding a new job may be worse, or a large number of social networks may be fractured. Indeed, entire towns may experience an emotional trauma from a plant closing, suggesting sociological forces could be at play. Using individual-level data, Sullivan and von Wachter (2009) have used mass layoff events to study the effect of unemployment on long term health and further research focused specifically on suicide could help determine the relevant causal pathways. Nevertheless, our results suggest intervention programs specifically targeted at workers who lose their jobs as part of a mass layoff, their families or their communities may be effective measures to lower suicide risk.

Indeed, our results provide some guidance into what a cost-effective intervention would have to resemble. Using the recent EPA estimate of $7 million as the value of a statistical life and our finding that there is an additional suicide for every 4,000 to 7,000 job losses from mass layoff events implies that the expected cost of a life-saved must be less than between $1,000 and $1,750 per laid off worker for a cost-effective intervention.13 For example, an intervention that reduced suicide risk by 10% cannot cost more than $100 to $175 per person to implement, while one that reduced risk by 50% cannot cost more than between $500 and $875 per person. These do not seem *prima facie* impossible, therefore future work should also focus on both the efficacy of potential interventions and their cost.

Third, long stretches of unemployment are positively associated with suicide risk for both males and females. Increases in the number of workers who are unemployed between 15 and 26 weeks tend to increase the number of suicides among males, while increases in the number of workers who are unemployed longer than 5 weeks tend to increase the number of suicides among females. Combined with our results for the newly unemployed, this suggests that the duration of unemployment rather than job loss itself is the primary determinant of suicide risk.

Because unemployment duration is reported in intervals, it is more difficult to offer specific guidance on the required cost-effectiveness of policy interventions. Our results imply that there is an additional suicide death for every 20,200 males who are unemployed between 15 and 26 weeks. Assuming a homogenous response within this interval, an additional suicide

[13]Obviously, using alternative values of a statistical life will alter this calculation. Nonetheless, we believe the EPA estimate, which is widely used (or at least acknowledged) by government agencies in setting policy, is a useful starting point. We are sympathetic to the argument, however, that future work studying the cost-effectiveness of a particular intervention should consider how heterogeneity in the value of a statistical life among the treatment group influences the calculation of net benefits.

death for 20,000 individuals unemployed between 15 and 26 weeks implies that an intervention that reduced suicide risk by 10% can cost no more than $35 per person while one that reduced risk by 50% can cost no more than $175 per person. It is likely, however, that the treatment effect is not constant across that time range. For example, there may be an additional suicide death for every 40,000 males unemployed 15 weeks compared to an additional death for every 10,000 males unemployed 26 weeks. Obviously, this type of heterogeneity could influence the cost-effectiveness of different interventions, as well as their timing.

Finally, the association between suicides and the number of unemployed between 15 and 26 weeks is much stronger than the relationship between suicide and the number of unemployed between 5 and 14 weeks. For males, the coefficient estimate for the latter is not statistically significant, while for females it is only one-third as large. This may simply reflect that as individuals are unemployed for longer, the psychological cost tends to increase and household resources tend to decline. In this case, one might generally expect a continuous, though not necessarily linear relationship. An alternative explanation is that the structure of unemployment insurance (UI) leads to a discontinuous jump in suicide risk. In many instances, UI benefits can be collected for 26 weeks.[14] It is possible that the increase in suicide risk for those unemployed 15 to 26 weeks is concentrated toward the end of this span as UI begins to run out. Again, without more disaggregated duration data, it is impossible to distinguish these effects.

It has long been hypothesized that job loss and unemployment lead to increased suicide risk. The results presented here are consistent with longer unemployment duration spells increasing suicide risk for both males and females. We also find that mass layoffs are associated with increased suicide risk, but we find no evidence that short unemployment spells in general result in more suicides. Although these are important new results in the literature, there are many questions about the mechanism through which employment status affects suicide risk that remain unanswered. Some of these could be addressed with more disaggregated unemployment duration data, particularly those associated with the most cost-effective timing of interventions. Others could assess the costs and benefits of individual policies intended to reduce the risk of suicide among the unemployed.

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[14]Extensions of UI benefits beyond 26 weeks (or 39 weeks with Extended Benefits in high unemployment states) were implemented during the recession beginning in late 2007, but this was not a common feature during the period covered in this analysis.

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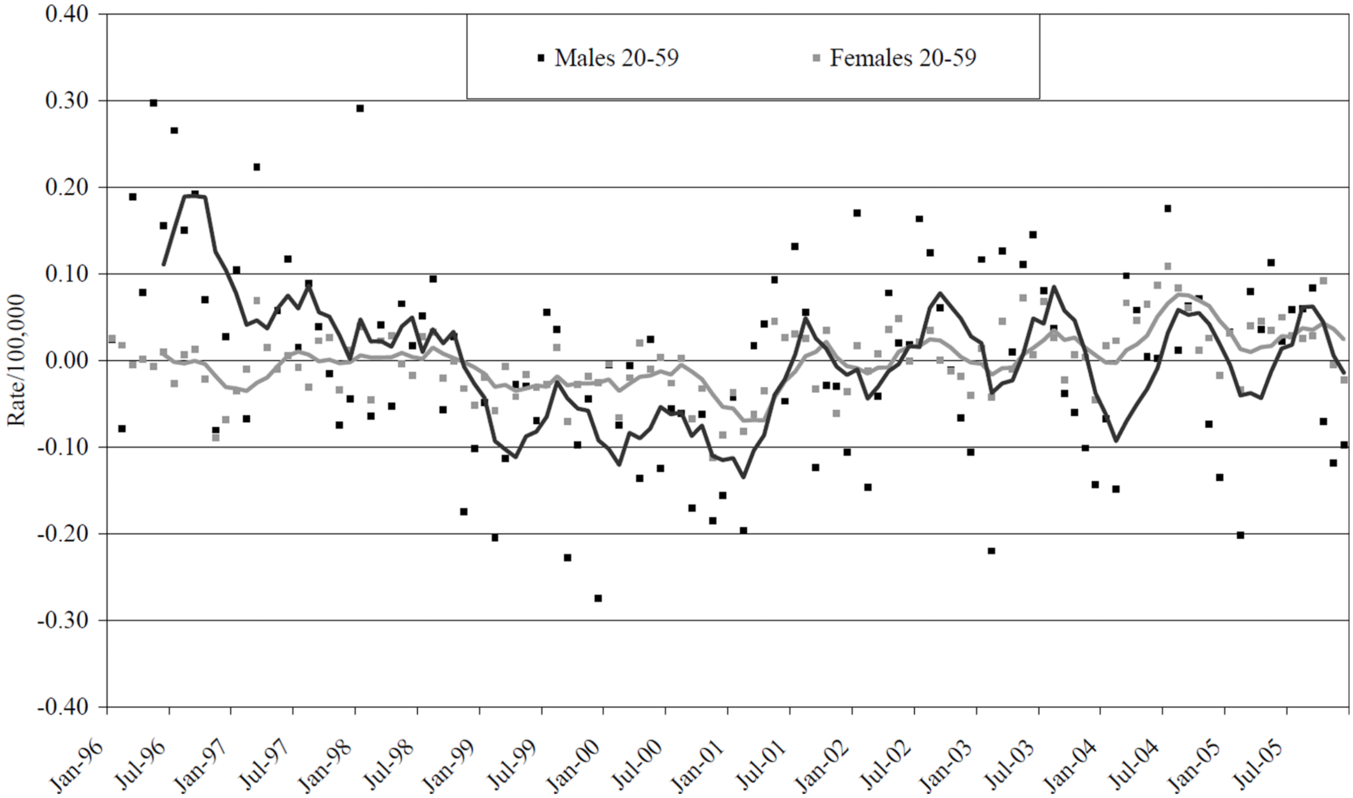

Figure 1. Weighted monthly average suicide rates by gender with 6-month moving average as a deviation from the mean, 1996–2005

Note: Points are monthly rates, lines are 6-month moving average.

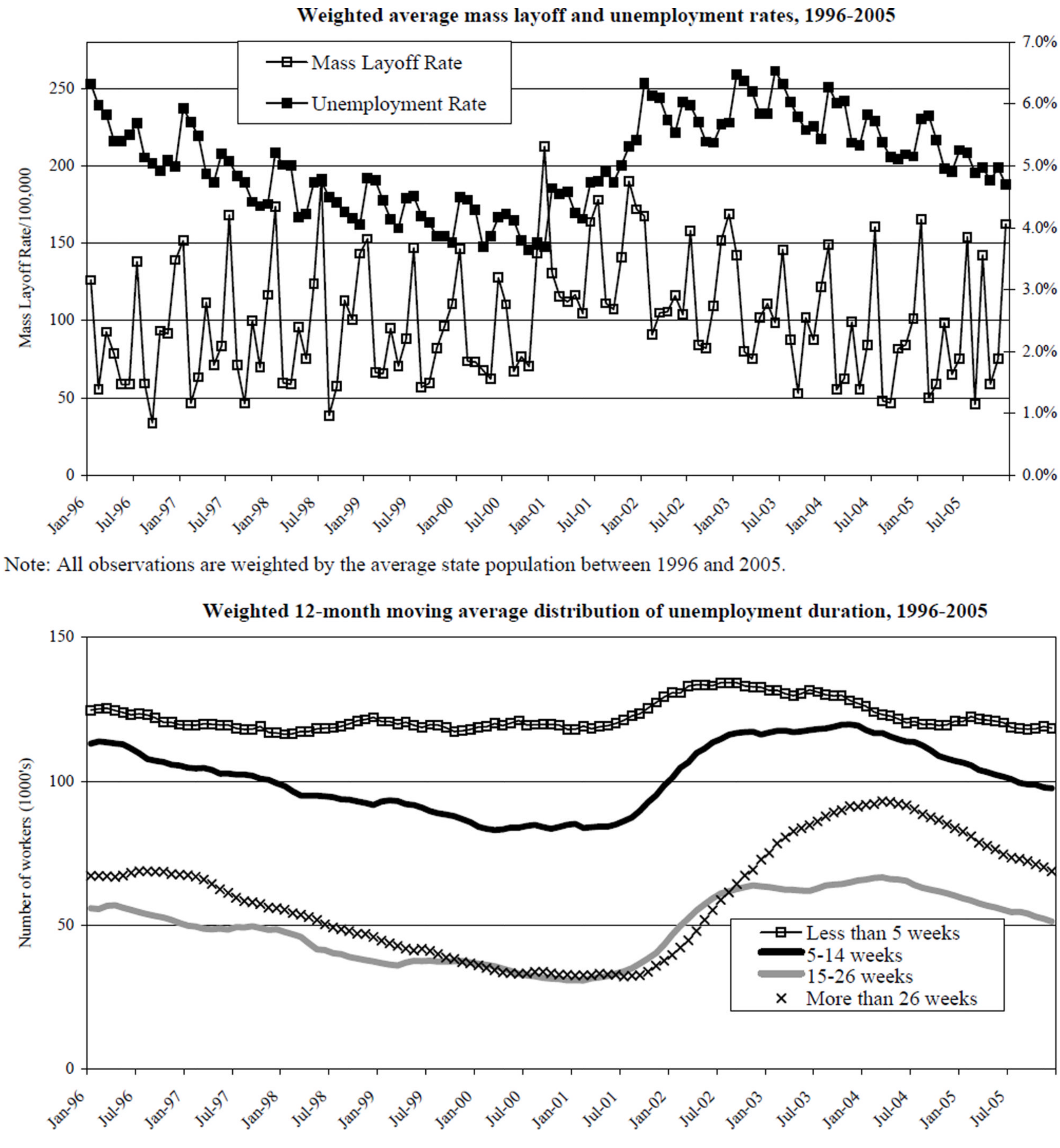

Figure 2. Measures of job loss and unemployment duration, 1996–2005

Table 1

Weighted average summary statistics

	Mean	Standard Deviation	Minimum	Maximum
Suicide Rate/100,000				
Females 20–59	0.50	0.23	0	3.60
Males 20–59†	1.90	0.60	0	7.09
New UI claims	67.3	68.7	0.6	330.9
Mass Layoff Rate/100,000	102.3	101.7	3.3	903.3
Unemployment Duration (12 month moving average in 1,000s of workers)				
Less than 5 weeks	127.3	108.3	4.0	403.0
5 to 14 weeks	105.5	88.7	3.0	367.0
15 to 26 weeks	51.1	47.0	1.0	208.0
More than 26 weeks	61.3	63.7	1.0	286.0

Note: All observations weighted by average monthly state population between 1996 and 2005.

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Table 2

Fixed-effect regression results of year-on-year differences in suicides

	Males	Females	Males	Females	Males	Females
Mass layoffs (year-on-year differences, 1000's of workers)						
Mass layoffs	0.142[a] (0.038)	0.057[a] (0.019)			0.140[a] (0.038)	0.053[a] (0.019)
Unemployment duration (first differences of 12 month moving average, 1000's of workers)						
Less than 5 weeks	−0.051 (0.072)	0.051 (0.036)	−0.026 (0.067)	0.060 (0.034)		
5 to 14 weeks	−0.013 (0.082)	0.110[a] (0.041)	0.000 (0.077)	0.103[a] (0.038)		
15 to 26 weeks	0.356[a] (0.108)	0.257[a] (0.054)	0.377[a] (0.101)	0.251[a] (0.050)		
More than 26 weeks	0.091 (0.092)	0.289[a] (0.046)	0.084 (0.085)	0.273[a] (0.042)		
ρ	0.104	0.073	0.076	0.056	0.111	0.090
Observations	4,758	4,758	5,455	5,455	4,758	4,758

Note: Coefficient estimates from fixed-effect regressions with an error term that is assumed AR(1) with a correlation coefficient that is common to all states. All regressions include year-on-year differences of state population and month-year fixed-effects.

[(a)] denotes significant at 1% level

[(b)] denotes significant at 5% level