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University of Hawai`i at Mānoa Department of Economics Working Paper Series

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Working Paper No. 14-13

Unemployment and Mortality: Evidence from the PSID

By

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April 2014

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March 4, 2014

Abstract

We use micro-data to investigate the relationship between unemployment and mortality in the United States using Logistic regression on a sample of over 16,000 individuals. We consider baselines from 1984 to 1993 and investigate mortality up to ten years from the baseline. We show that poor local labor market conditions are associated with higher mortality risk for working-aged

*I would like to thank Takao Kato, Peter Kuhn, Edward Lazear, Doug Miller, Till von Wachter and participants at the Trans-Pacific Labor Seminar in Honolulu for useful comments. I would also like to thank Chris Paxson for some useful conversations prior to the writing of the paper. Address: 2424 Maile Way; Saunders Hall 533; Honolulu, HI 96822. Tele: (808) 956 - 8615. Email: halliday@hawaii.edu. All errors are my own.

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men and, specifically, that a one percentage point increase in the unemployment rate increases their probability of dying within one year of baseline by 6%. There is little to no such relationship for people with weaker labor force attachments such as women or the elderly. Our results contribute to a growing body of work that suggests that poor economic conditions pose health risks and illustrate an important contrast with studies based on aggregate data.

Key words: Recessions, Mortality, Health, Aggregation, Unemployment

JEL Classification: I0, I12, J1

1 Introduction

Recent work has used micro-data to establish a robust relationship between job displacement and adverse health outcomes. For example, Sullivan and von Wachter (2009) use administrative data from Pennsylvania and show that job displacement is associated with an increased mortality risk of 10-15% within about 20 years of baseline. In a similar paper, Strully (2009) uses the Panel Study of Income Dynamics (PSID), a household-level panel that has been run by the University of Michigan since 1968, and shows that job displacement is strongly associated with increased morbidity. On the whole, these papers suggest that there are negative health consequences to losing one's job.

This would suggest that poor macroeconomic conditions should be associated with higher mortality as well since the odds of job displacement will increase as the economy worsens. However, as it turns out, studies based on aggregate data actually suggest the opposite, namely, that worsened macroeconomic conditions are associated with *lower* mortality *e.g.* Ruhm (2000), Gerdtham and Ruhm (2006), Miller, Page, Stevens, and Filipski (2009), and Stevens, Miller, Page, and Filipski (2011). However, according to Ruhm (2013), this pro-cyclical relationship does not appear to hold in more recent years except for mortality from vehicular accidents and cardiovascular disease. Nevertheless, at first glance, there seems to be a tension

between these two literatures since the micro-evidence appears to indicate a counter-cyclical relationship. The only paper of which we are aware that finds better health during poor economic times using micro-data is Ruhm (2003), although he does not look at mortality.

The conflicting findings in the micro and the macro literatures have distinct implications for the mechanisms linking macroeconomic conditions to mortality. As pointed out by Ruhm (2000) and Catalano, Goldman-Mellor, Saxton, Margerison-Zilko, Subbaraman, LeWinn, and Anderson (2011), different mechanisms suggest that the relationship can be either pro-cyclical or counter-cyclical. The former accord with the aggregate studies, whereas the latter accord with the micro-studies.

One set of mechanisms involves how recessions affect the cost of time and the affordability of vices. For example, during recessions the cost of time is lower which may facilitate investments in health such as exercise or routine check-ups. The effects on consumption of vices can theoretically be pro- or a counter-cyclical since, on one hand, recessions should reduce the consumption of normal goods such as cigarettes while on the other hand, recessions may create stresses that are dealt with by increased prevalence of smoking or drinking. Some evidence such as Evans and Graham (1988), Ruhm (1995) and Adda, von Gaudecker, and Banks (2009) indicates that smoking and drinking increase during good economic times suggesting that this

mechanism should generate a pro-cyclical relationship. Although, evidence from Dee (2001) and Davalos, Fang, and French (2012), which in contrast to the previous studies uses micro-data, suggests that binge drinking is more common during downturns.

Other commonly discussed mechanisms involve vehicular accidents and stress. First, when the economy is doing well, car accidents may increase due to increased driving; evidence from Ruhm (2000), Miller, Page, Stevens, and Filipski (2009), and Stevens, Miller, Page, and Filipski (2011) supports this. Finally, as pointed out by Catalano, Goldman-Mellor, Saxton, Margerison-Zilko, Subbaraman, LeWinn, and Anderson (2011), stress can vary over the business cycle. This may generate a pro- or a counter-cyclical relationship depending on whether or not working is more stressful than the exigencies of unemployment or the threat of losing one's job.

In this paper, we see if we can resolve this tension by offering an alternative look at the relationship between macroeconomic conditions and mortality that is based on micro-data. In doing so, we hope to provide some insights into the mechanisms linking the two. Specifically, we add to the literature by investigating the relationship between mortality and county-level unemployment rates using use the PSID's death file. An important feature of our study is that it delivers a similar parameter as the aggregate studies.

Specifically, we investigate the effects of unemployment fluctuations over the period 1984-1993 on mortality in the United States. To provide some details about how the unemployment rate varied over this period and how it compares to other time periods, we present Figure 1 in which we plot the unemployment rate in the US over the period 1970-2010. The period that we consider in the study is inside the vertical lines and includes the recovery from the deep recession of the early 1980's as well the peak of the recession of the early 1990's. Note that the relationship between the macroeconomy and mortality may change depending on the severity of the recession. Because we do not consider fluctuations that are as severe as the "great recession," we caution the reader not to extrapolate too much from our results to other periods.

It is important to emphasize, however, that this is not the first study that uses individual-level data to investigate the effects of business cycles on mortality. In particular, Gerdtham and Johannesson (2005) uses Swedish micro-data to look at the same question using very similar methods albeit with Probit models in lieu of Logit models and some alternative measures of business cycles. Overall, our results are very much in accordance with theirs.

There are two contributions of our study to the Swedish study. First, we are able to replicate their qualitative findings using American data. Second, we estimate a

parameter that is more directly comparable to parameters that have been estimated by Sullivan and von Wachter (2009) as well as Ruhm (2000), Miller, Page, Stevens, and Filipski (2009), and Stevens, Miller, Page, and Filipski (2011).

The balance of this paper is organized as follows. In the next section, we discuss our data. After that, we discuss our results and some of their implications. Finally, we conclude and offer some insights into how measurement issues can possibly explain the divergence between the results at the micro and macro levels.

2 Data

Our sample selection works as follows. First, we begin with 20,338 individuals from the PSID waves 1984 to 1993; each of these survey years constitutes a separate baseline. Our sample starts at 1984 because the Self-Reported Health Status (SRHS) question is not available prior to that year. The sample ends in 1993 because county level unemployment rates are not available beyond then. In addition, in any study of mortality, it is useful for the baseline to be far enough in the past so that researchers can investigate how the magnitude of any effects change as we move away from baseline *e.g.* the effects of poor economic conditions on mortality one, five or ten years from baseline; since we do not have mortality information beyond 2005,

this suggests that the most recent baseline should not be beyond 1995. Next, we dropped people with incomplete information on SRHS. This lowers the sample size to 20,222. Next, we further restricted the sample to people who were between ages 25 and 80 (inclusive). This brought the sample size to 18,440. Next, we dropped individuals whose ages declined by more than one year or increased by more than two years. The reason why age fluctuates like this in the PSID is that it is measured in each wave of the survey making it prone to errors. We also estimated our model without dropping people whose ages fluctuated so much and the results were not affected. After dropping these individuals, the sample size becomes 16,769. Of these individuals, 8045 are male and 8724 are female.

The covariates that we use are county level unemployment rates, SRHS which is a categorical variable between 1 (excellent) and 5 (poor) that respondents use to rate their own health, age, educational attainment, gender and race. Summary statistics are reported in Table 1. We also employ information on state-of-residence and industry for some robustness checks.

Information on mortality comes from the PSID's mortality file which contains the death years of all PSID members who died on or before 2005 as well as the primary cause of their death. The cause of death was coded according to the International Classification of Death (ICD). For deaths that occurred prior to 1999, the ICD9 was

used. After 1999, the ICD10 was used.

A total of 1932 individuals which is 11.52% of our sample had died prior to 2005. Of these, 996 were men and 936 were women which corresponds to 12.38% of the male sample and 10.73% of the female sample. The two biggest broadly defined causes of death in our data were diseases of the circulatory system, which correspond to ICD9 codes 390 to 459 or ICD10 codes beginning in I and neoplasms, which correspond to ICD9 codes 140 to 239 or ICD10 codes beginning in C. There were 691 deaths from circulatory diseases, which includes heart attacks, and 444 deaths from cancer in our sample. Because these causes of death are very frequent, we will consider them separately in our analysis. Other causes of death are too infrequent to consider on their own.

3 Methods

To shed light on the relationship between unemployment and mortality, we consider the following model:

$$P(d_{it}^j|u_{it}, X_{it}) = \Lambda\left(\alpha^j + u_{it}\beta^j + X_{it}'\theta^j\right) \text{ for } j = 1, ..., 10$$

where $\Lambda(.)$ is the logistic CDF, u_{it} is the unemployment rate in the individual's county of residence and X_{it} is a column vector that includes dummies for SRHS being equal to one, two, three, or four; a quadratic function of age; dummy variables for educational attainment; and a dummy variable for being Caucasian. In some robustness checks, we also include fixed effects for state, year and industry. The subscript t corresponds to the survey year which is the year of the baseline. The dependent variable is an indicator that is turned on if the individual has died within j years of the survey year in which the independent variables were measured which is what we refer to as the baseline in this paper. So, $d_{it}^5 = 1$ if the individual has died within 5 years of the baseline at year t. Note that because the variables d_{it}^j were constructed using the same variable on the death year from the PSID that the sample sizes for regressions using different d_{it}^j as the dependent variable will be the same. As in Meghir and Pistaferri (2004), we adjust all standard errors for clustering on individuals which is the standard procedure in the PSID; Gerdtham and Johannesson (2005) also cluster by individual.

It is informative to compare our parameter to those from macro-based studies. If we let p denote the mortality probability given above, then

$$\beta^j = \frac{\frac{\partial p}{\partial u_{it}}}{p(1-p)} \approx \frac{\frac{\partial p}{\partial u_{it}}}{p} = \frac{\partial \log p}{\partial u_{it}}.$$

This holds because p will be small. The right-hand side is what obtains from a regression of log-mortality rates on the unemployment rates which is what is typically done when using macro-data.

It is important to bear in mind that there is little to no measurement error in our dependent variable; we know with a very large degree of accuracy whether or not the respondent is dead. Mortality rates at the state level, on the other hand, are measured with error and if these errors are correlated with local macroeconomic conditions then this will induce a bias in the estimation. Hence, differences in the estimates at the micro- and macro-levels may reflect this aggregation bias. We will flesh this idea out further in the conclusion.

The strategy that we use to identify the effect of unemployment fluctuations on mortality essentially relies on a selection-on-observables assumption, but without a binary treatment. This is somewhat of a common procedure in the literature on unemployment and health (*e.g.* Browning, Dano, and Heinesen (2006), Strully (2009), and Sullivan and von Wachter (2009)). We are careful to control for important confounding factors including health status, education, age and race in a flexible manner. In addition, in some robustness checks, we control for state, year and industry fixed effects. The use of state fixed effects restricts the variation in unemployment rates to intra-state variation; aggregate studies also rely on within state variation.

4 Empirical Results

4.1 Core Results

In Table 2, we report the marginal effects of unemployment on mortality by any cause. In this table and all of the tables that follow, we report the coefficient on the unemployment rate from a Logistic regression. These coefficients can be interpreted as the percentage increase in the mortality hazard within a certain number of years from baseline resulting from a one percentage point increase in the unemployment rate. For men ages 60 and under, we see that higher unemployment rates at the county level predict higher mortality once we partial out important confounding variables including controls for baseline health status. For this demographic group, there is a statistically significant effect of high unemployment on dying within three years of baseline (*i.e.* the year in which the unemployment rates were measured). The estimates for working-aged men indicate that a one percentage point increase in the unemployment rate increases the probability of death within one year of baseline by about 6%, but these effects decline thereafter. This can be visualized in Figure 2 where we plot the point estimates for working-aged men from Table 2 along with their 95% confidence bands. It is also important to note that these effects are of the opposite sign and of a larger magnitude than what we find in the macro literature.

For example, estimates from Ruhm (2000) indicate that a one percentage point increase in the unemployment rate is associated with between 4 and 5 *fewer* deaths per 100,000 (for all demographic groups) whereas ours suggest about 24 *more* deaths per 100,000 (for working-aged men). These calculations are based on information from the Actuarial Life Table from the Social Security Administration which indicates that the mortality rate for men ages 30 to 60 is 402.40 per 100,000; 6% of this is 24.14.

In the same table, when we look at people with a weaker attachment to the labor market such as women and people older than 60, we see a different picture. For these demographic groups, there is no significant relationship between unemployment and mortality.

The bottom line is that we see very significant effects for working-age men for whom labor force attachment is the strongest and no significant effects for women or the elderly for whom there is a smaller attachment. This stands in contrast to results in Miller, Page, Stevens, and Filipski (2009) who used aggregate data to show that there is a significant negative relationship between unemployment and mortality rates at the state level for people who have a weak labor force attachment, particularly the elderly.

4.2 Robustness Checks

We now conduct a comprehensive set of robustness checks for the results on working-aged men in Table 2. In particular, we investigate the robustness of our results to state and year fixed effects. It is important to point out that inclusion of these additional controls greatly reduces the variation in the county level unemployment rates. To illustrate this, we compute the R^2 of regressions of county-level unemployment rates onto state and year fixed effects. We see that with only state fixed effects, the R^2 is 0.2256. Once we include year fixed effects, the R^2 jumps to 0.3399. Controlling for state and temporal variation eliminates about one-third of the variation in the unemployment rates, so this should be viewed as a more stringent test. This is certainly desirable in the sense that it goes a long way towards eliminating confounding variables, but it is less desirable in that it may expunge variation in the county-level unemployment rate that is truly exogenous. This may be especially problematic given that the variation in our dependent variables is not terribly high since only 1.55% of our observations died within one year of the baseline and only 8.39% died within 10 years of the baseline. Finally, in addition to adjusting for state and temporal variation, we also include additional results in which we control for 3-digit industry codes.

We report the results in Table 3 and plot the estimates along with their 95%

error bands in Figures 3-5. For men, when we include state fixed effects only, we see that the point-estimates are basically unchanged from the first column of Table 2. In column (2), we include year fixed-effects as well and the results become slightly larger. In column (3), we further include industry dummies. These estimates are also similar to those in column 1 and are significant up to two years from baseline. Overall, the estimates for working-aged men are robust to the inclusion of state, time and industry fixed effects.

Finally, we estimate the model on a sub-sample of people who did not change states in any year between 1984 and 1993 to investigate if out-migration of healthy people from depressed areas is biasing our results. The idea of this exercise is that by throwing out people who may both tend to be healthier and live in locales with low unemployment, we would expect to see the estimates attenuated if this bias is important. We report the results in columns 4 and 5 without and with state and time dummies. The results are plotted in Figures 6 and 7. Nothing changes suggesting that this mechanism is not strong enough to meaningfully change the results.

We also attempted the following robustness checks. First, we used a Probit regression instead of the Logit; the results were not affected. Second, we included state-specific time trends; unfortunately, the optimization routine would not converge. Third, we included occupation dummies instead of industry dummies; the

results were not affected.

4.3 Results by Cause of Death

In Table 4, we look at the relationship between unemployment and mortality due to two common causes of death: cancer and diseases of the circulatory system which includes heart attacks. We plot the estimates for both causes of death in Figure 8. Once again, we restrict our attention to working-aged men.

We see two distinct patterns. First, for neoplasms, we see that the effects are small and insignificant close to baseline, but that they increase as we move away from it. By ten years from the baseline, we see that a one percentage point increase in the unemployment rate increases the mortality hazard by 5.51%; this is significant at the 10% level. In contrast, for diseases of the circulatory system, we see large and significant effects close to baseline which slowly decline as we move away from it. In particular, within one year of baseline, we estimate that a one percentage point increase in the unemployment rate increases the mortality hazard for circulatory diseases by 7.74% but this effect diminishes to 1.35% for ten years from the baseline. The finding for cardiovascular disease is similar to results in Svensson (2007) but differs from Ruhm (2007); the former uses a lower level of aggregation but neither uses individual level data.

4.4 Results with and without Health Status

We conclude by presenting Figure 9 where we plot the marginal effects from the previous figures both including and excluding the dummies for SRHS. First, we plot the marginal effects from Figure 2. Second, we plot the marginal effects from the same model that generated Figure 2 except excluding SRHS controls. The intent is to illustrate the importance of selection on health status. As can be seen, controlling for health status greatly attenuates the estimates. In fact, for one year from baseline, excluding health status increases the marginal effects by 26%. Hence, controlling for health status does appear to matter a great deal.

5 Decomposing the Effects

We now decompose the effects of the unemployment rate on mortality into two constituents. The first effect is the most obvious and operates directly through one's employment status. We call this the employment effect. The second operates independent of employment status. This channel could reflect the stress from the threat of you or your spouse losing your job, the negative effects of slower wage growth on heath, *etc.* We call this second effect the indirect effect.

To fix ideas, we let $L \in \{0, 1\}$ denote a person's employment status where a value

of one indicates that she/he is currently working, $d \in \{0,1\}$ denote whether or not the person is dead with unity indicating death, and U denote the unemployment rate. Next, we define $\pi \equiv P(L=1|U)$ and $\lambda^l \equiv P(d=1|L=l,U)$. Note that U is allowed to affect mortality independently of employment status, so high unemployment rates can be detrimental to your health even if you are currently employed. We can now write the probability of dying conditional on U as

$$P(d=1|U) = \lambda^1 \pi + \lambda^0 (1-\pi).$$

This then implies that the total effect of a 1 percentage point increase in the unemployment rate on the mortality probability will be

$$\frac{dP(d=1|U)}{dU} \equiv TOTAL = EMPLOYMENT + INDIRECT \tag{1}$$

where

$$EMPLOYMENT \equiv \left[\lambda^1 - \lambda^0\right] * \frac{\partial \pi}{\partial U}$$

and

$$INDIRECT \equiv \frac{\partial \lambda^1}{\partial U}\pi + \frac{\partial \lambda^0}{\partial U}(1-\pi).$$

The goal now is to compute the percentage of $TOTAL$ due to $EMPLOYMENT$.

To do this, we will use our estimates in conjunction with estimates from the literature. First, our estimates indicate that the left-hand side of equation (1) is about 24 fewer deaths per 100,000. Next, the best available estimate of $\lambda^1 - \lambda^0$ from the literature is from Sullivan and von Wachter (2009) (column 1 of Table 4) who find that the causal effect of being unemployed on dying within one year of baseline is -0.716 which translates to 288.1 fewer deaths per 100,000. Finally, we need $\frac{\partial \pi}{\partial U}$. One way of obtaining this is to regress the employment-population ratio on the unemployment rate. Doing this, one obtains an estimate of -1.18 which implies that $\frac{\partial \pi}{\partial U} = -0.0118$. So, we obtain that the indirect effects are about 20.7 deaths per 100,000 suggesting that the employment effects are about 14% of the total estimated effects. This calculation indicates that the indirect channels matter relatively more than the employment channel.

This suggests that living in areas with high unemployment may pose health risks even if the individual is currently employed. One possible reason for this is that higher unemployment might be associated with greater economic uncertainty which has been associated with greater risk of myocardial infarction by Lee, Colditz, Berkman, and Kawachi (2004). It is also important to point out that even if the individual has not lost their job, their spouses may have.

Finally, note that there is not a similar estimate of the effect of exogenous job

loss on mortality from the PSID of which we are aware. A useful exercise in the future would be to look at the effects of job displacement on mortality in the PSID. The challenge here is to be able to identify periods of unemployment in the PSID that are involuntary as was done by Sullivan and von Wachter (2009). Provided that there is enough data, this probably can be done but it would entail a careful parsing of the data. As such, we believe that it is best left for a separate paper.

6 Conclusions

Summary of Findings In this paper, we showed that higher unemployment rates are associated with higher mortality risk for working-aged men. This finding is robust to the inclusion of state and industry dummies as well as baseline health status. Specifically, we see that a one percentage point increase in the unemployment rate increases the probability of dying within one year of baseline of working-aged men by 6%. The effects of this increase decline monotonically as we move away from baseline. We also showed that a one percentage point increase in the unemployment rate raises the probability of a death caused by diseases of the circulatory system by 7.74% for one year within baseline but once we move out to ten years from baseline, the effect diminishes to 1.35% and is no longer significant. In contrast,

the corresponding effect for cancer-related deaths is 2.01% and not significant for one year within baseline, but increases to 5.51% and becomes significant at the 10% level for ten years within baseline. There is no relationship for working-aged women and people over 60. These results complement findings in Halliday (2012) where earnings shocks have substantial adverse effects on self-rated health for working-aged men but smaller effects for working-aged women.

These findings are very similar to what Gerdtham and Johannesson (2005) found. Specifically, they looked at six business cycle indicators and found that mortality varied counter-cyclically for four of the six indicators for men only. Their effects were also more pronounced for working-aged men. However, unlike our study, no significant effects were found when the unemployment rate was used.

The effects are present for people with the largest labor force attachment: working-aged men. This contrasts with studies that use aggregate data that find a significant and negative relationship between unemployment and mortality for the very young and the very old who should have no attachment to the labor market. Overall, our conclusions appear to be consistent with the previous studies that use micro-data. However, the similarity of the regression models but the divergence in the results between our study and studies that employ macro-data suggests the presence of an aggregation bias in the latter. Future work should investigate this.

Possible Mechanisms While this paper does not explicitly investigate mechanisms, we can offer some speculation. The three main viable mechanisms that would generate a counter-cyclical relationship are that consumption of vices increases during downturns, consumption of medical care declines or recessions induce higher stress levels. Evidence from Evans and Graham (1988), Ruhm (1995) and Adda, von Gaudecker, and Banks (2009) suggests that the first of these is not a viable mechanism. However, other evidence presented in Dee (2001) and Davalos, Fang, and French (2012) indicates that binge drinking does indeed rise during bad economic times, perhaps due to increased stress. It is notable that the studies that find that drinking is counter-cyclical use micro data, whereas the others use aggregate data. Finally, we cannot rule out the latter two mechanisms. The fact that we do find significant effects on cardiovascular-related deaths which have been tied to higher stress levels (see Kubzansky and Kawachi (2000), for example) coupled with findings in Dee (2001) and Davalos, Fang, and French (2012) does appear to suggest that stress may be a viable pathway.

Aggregation Bias It is instructive to consider how our specification relates to the macro work. To fix ideas, we write a linear model at the individual level

$$d^1_{ist} = \alpha + \beta u_{st} + \delta_s + \varepsilon_{ist} \tag{2}$$

where the s subscript denotes states. For the sake of simplicity, we let u_{st} denote the state level unemployment rate. If we take expectations over states at a point-in-time, then we obtain

$$d_{st}^{1} = \alpha + \beta u_{st} + \delta_s + \varepsilon_{st} \tag{3}$$

where we have adopted the notation that $z_{st} = E\left[z_{ist}\right]$. The model in equation (3) is similar to those that are estimated by Ruhm (2000) and others who investigate the relationship between unemployment rates and mortality at the state level while employing state fixed effects. If there are no issues with the aggregation, then estimates of β using either model should be the same. If they are different then this suggests that aggregation biases are present.

In our view, this is a topic worthy of future investigation. Importantly, moving from the micro model in equation (2) to the macro model in equation (3) necessitates an accurate measurement of $d_{st}^{1} = E\left[d_{ist}^{1}\right]$ which is the probability that an individual will die in a given state at a given time. However, if there are errors then we will observe

$$d_{st}^{1*} = d_{st}^{1} + e_{st}$$

which implies that the model that is actually estimated at the state-level is

$$d_{st}^{1*} = \alpha + \beta u_{st} + \delta_s + e_{st} + \varepsilon_{st}.$$

If these errors are correlated with u_{st} then this will cause estimates of β in model (3) to be biased. Specifically, for the Ordinary Least Squares estimator, we will have that

$$p\lim \widehat{\beta} = \beta + \frac{Cov\left(\widetilde{e}_{st}, \widetilde{u}_{st}\right)}{Var\left(\widetilde{u}_{st}\right)} \tag{4}$$

where $\widetilde{e}_{st}$ and $\widetilde{u}_{st}$ represent deviations of e_{st} and u_{st} from their state-specific means over time. The advantage of model (2) is that measurement of d^1_{ist} is trivial whereas the measurement of d^1_{st} is not. This idea that parameter estimates can differ depending on the level of aggregation has deep roots in economics (see Blundell and Stoker (2005) for a discussion).

We suspect that $Cov\left(\widetilde{e}_{st}, \widetilde{u}_{st}\right)$ may be negative for two reasons. First, as pointed out by Blanchard and Katz (1992), local economic shocks are typically dealt with by large out-migrations. Second, measurement of mortality rates can change substantially because of migration. Essentially, out-migration leads to a decrease in the number of deaths that take place in a given locale for purely mechanical reasons. Because the denominators of mortality rates often do not adequately adjust for this, it is possible that mortality rates as measured will decline in the presence of out-migration. Future work should investigate the degree to which migration in response to business cycle fluctuations can explain this paradox.

One way that this could be done is to employ administrative data at the indi-

vidual level. Doing so would yield close to a census of individual deaths that could subsequently be aggregated to the state level. One could then estimate the models in equations (2) and (3) see how the estimates of β compare. This could not be credibly done with the PSID, however, since we only have 2000 deaths covering 50 states and 10 years yielding 4 deaths per state/year on average. Clearly, administrative data is needed.

One final point is that recent work based on macro-data suggests that the relationship between the macro-economy and mortality has changed. For example, Ruhm (2013) shows that, since 2000, the estimates are close to zero except for deaths due to myocardial infarction and vehicular accidents and McInerney and Mellor (2012) shows that over the period 1994-2008, the elderly exhibit counter-cyclical mortality rates. When one considers this evidence in light of equation (4) and Figure 1 which shows that business cycle fluctuations have become more severe in the new millennium, it suggests that the true value of β may have become more negative in recent years. Because estimates of β using macro-data may be biased upwards, this may be responsible for the smaller estimates found in more recent work that uses aggregate data. Extending this analysis to include the "great recession" could shed light on this.

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Table 1: Descriptive Statistics

	Men	Women
County-level Unemployment Rate	6.31	6.38
	(2.53)	(2.51)
SRHS = 1	0.25	0.19
	(0.44)	(0.39)
SRHS = 2	0.31	0.29
	(0.46)	(0.45)
SRHS = 3	0.27	0.31
	(0.44)	(0.46)
SRHS = 4	0.12	0.15
	(0.32)	(0.35)
SRHS = 5	0.06	0.06
	(0.22)	(0.25)
Age	43.49	44.57
	(13.88)	(14.58)
College Degree	0.26	0.19
	(0.44)	(0.39)
More than 12 Years of Schooling	0.45	0.40
	(0.50)	(0.49)
Caucasian	0.70	0.64
	(0.46)	(0.48)

Mean and standard deviation in parentheses.

Table 2: Mortality: Any Cause

Died	Men	Women	Men	Women
	60 and Under		Over 60	
≤ 1	0.06110***	−0.0263	0.0076	−0.0282
	(0.0227)	(0.0350)	(0.0240)	(0.0264)
≤ 2	0.0535**	−0.0351	0.0029	−0.0164
	(0.0222)	(0.0323)	(0.0225)	(0.0245)
≤ 3	0.0403*	−0.0166	0.0009	−0.0165
	(0.0235)	(0.0300)	(0.0219)	(0.0227)
≤ 4	0.0346	−0.0113	0.0010	−0.0175
	(0.0231)	(0.0284)	(0.0211)	(0.0219)
≤ 5	0.0313	−0.0146	−0.0060	−0.0204
	(0.0226)	(0.0272)	(0.0206)	(0.0207)
≤ 6	0.0248	−0.0193	−0.0097	−0.0245
	(0.0214)	(0.0260)	(0.0205)	(0.0201)
≤ 7	0.0213	−0.0235	−0.0101	−0.0261
	(0.0206)	(0.0252)	(0.0203)	(0.0198)
≤ 8	0.0246	−0.0289	−0.0153	−0.0289
	(0.0197)	(0.0245)	(0.0201)	(0.0195)
≤ 9	0.0231	−0.0350	−0.0215	−0.0272
	(0.0193)	(0.0239)	(0.0201)	(0.0193)
≤ 10	0.0247	−0.0381	−0.0265	−0.0309
	(0.0188)	(0.0234)	(0.0199)	(0.0190)
N	6894	7339	1409	1818

Each cell of this table corresponds to a separate Logistic regression. The coefficient on county level unemployment rate is reported along with its standard error clustered by individual. Each row corresponds to a separate dependent variable.
Died <= 1 means that the respondent died within one year of the panel year; died <= 2 means that the respondent died within two years of the panel year; etc.
* sig at 10% level ** sig at 5% level ***sig at 1% level

Table 3: Robustness Checks for Men 60 and Under

Died	(1)	(2)	(3)	(4)	(5)
≤ 1	0.0568**	0.0759***	0.0603**	0.0598**	0.0836***
	(0.0252)	(0.0265)	(0.0282)	(0.0260)	(0.0289)
≤ 2	0.0467**	0.0602**	0.0469*	0.0517**	0.0625**
	(0.0239)	(0.0255)	(0.0266)	(0.0257)	(0.0278)
≤ 3	0.0338	0.0469*	0.0303	0.0399	0.0474*
	(0.0243)	(0.0260)	(0.0269)	(0.0272)	(0.0287)
≤ 4	0.0284	0.0411*	0.0239	0.0378	0.0419
	(0.0231)	(0.0250)	(0.0257)	(0.0264)	(0.0276)
≤ 5	0.0269	0.0415*	0.0249	0.0376	0.0440*
	(0.0221)	(0.0240)	(0.0246)	(0.0252)	(0.0262)
≤ 6	0.0209	0.0374	0.0216	0.0336	0.0405
	(0.0210)	(0.0228)	(0.0231)	(0.0238)	(0.0250)
≤ 7	0.0174	0.0334	0.0176	0.0315	0.0372
	(0.0202)	(0.0218)	(0.0219)	(0.0228)	(0.0241)
≤ 8	0.0212	0.0346*	0.0204	0.0344	0.0372
	(0.0193)	(0.0208)	(0.0206)	(0.0216)	(0.0231)
≤ 9	0.0190	0.0304	0.0167	0.0332	0.0332
	(0.0190)	(0.0205)	(0.0204)	(0.0212)	(0.0229)
≤ 10	0.0218	0.0332	0.0204	0.0350	0.0372*
	(0.0186)	(0.0202)	(0.0201)	(0.0206)	(0.0225)
State Dummies	X	X	X		X
Year Dummies		X	X		X
Industry Dummies			X		
Non-Mover Sample*				X	X
N	6894	6894	6894	4973	4973

Per Table 2.

*The non-mover sample includes only individuals who remained in the same state from 1984-1993.

Table 4: Mortality by Cause: Neoplasm and Circulatory Disease, Men Under 60

Died	Neoplasm	Circulatory Disease
≤ 1	0.0201	0.0774**
	(0.0614)	(0.0379)
≤ 2	0.0269	0.0629*
	(0.0550)	(0.0376)
≤ 3	0.0369	0.0411
	(0.0480)	(0.0412)
≤ 4	0.0347	0.0473
	(0.0231)	(0.0430)
≤ 5	0.0402	0.0186
	(0.0401)	(0.0412)
≤ 6	0.0445	0.0108
	(0.0370)	(0.0400)
≤ 7	0.0427	0.0100
	(0.0339)	(0.0381)
≤ 8	0.0483	0.0140
	(0.0327)	(0.0364)
≤ 9	0.0478	0.0143
	(0.0322)	(0.0353)
≤ 10	0.0551*	0.0135
	(0.0320)	(0.0342)
N	6894	6894

Per Table 2.

Figure 1

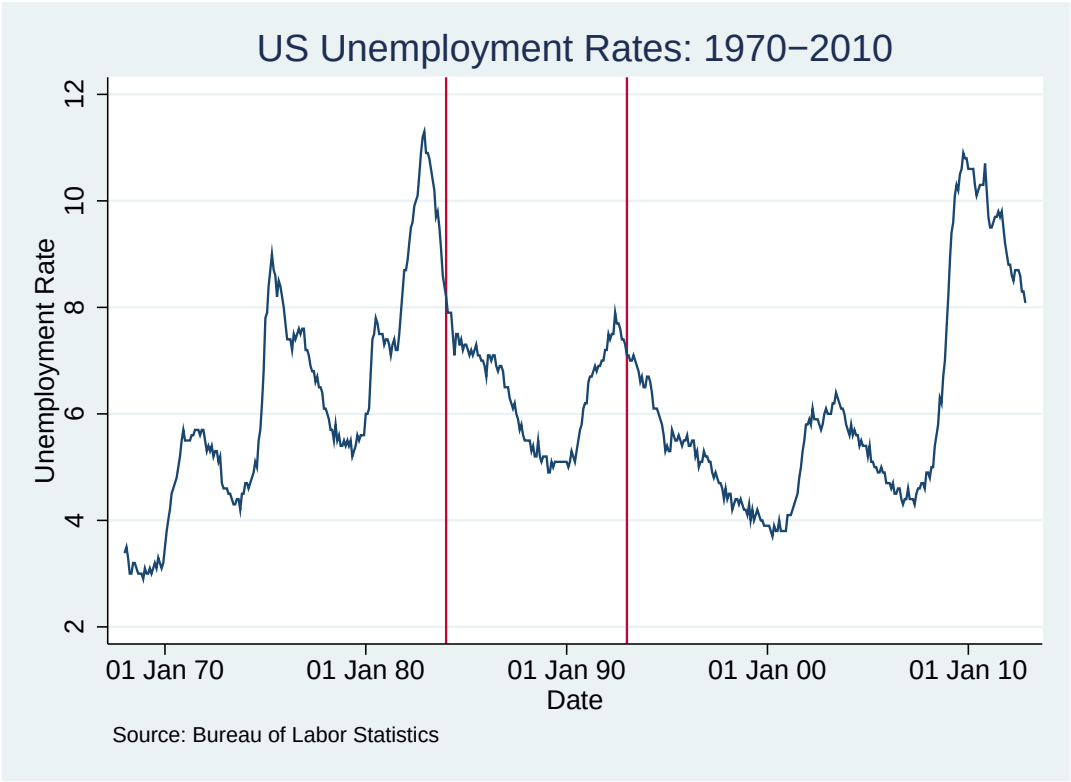
US Unemployment Rates: 1970−2010
Unemployment Rate
2
4
6
8
10
12
01 Jan 70
01 Jan 80
01 Jan 90
01 Jan 00
01 Jan 10
Date
Source: Bureau of Labor Statistics

Figure 2

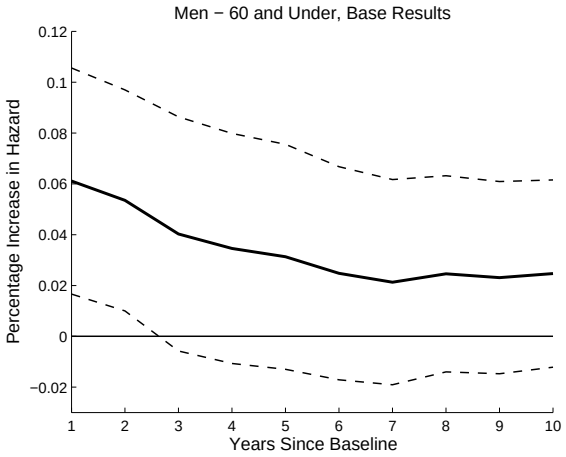

Figure 3

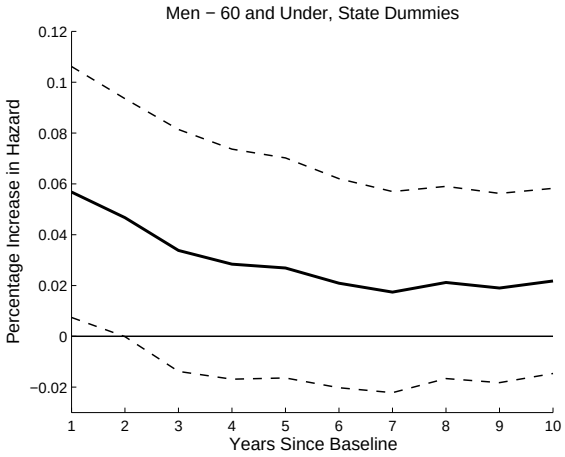

Figure 4

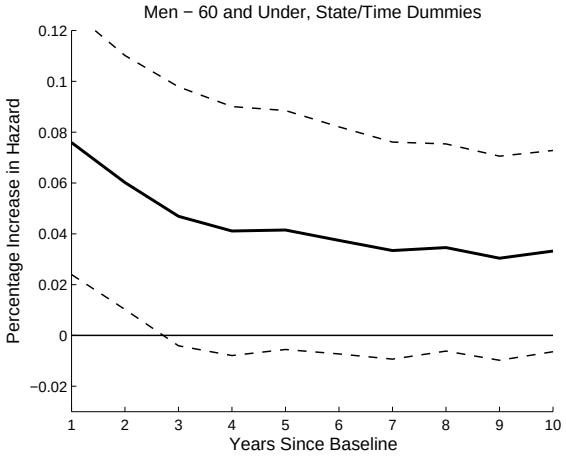

Figure 5

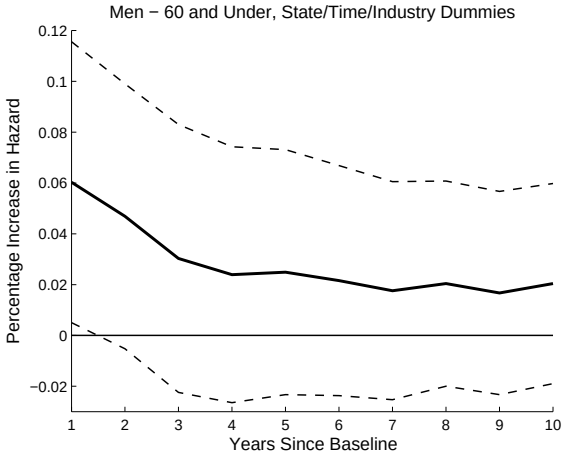

Figure 6

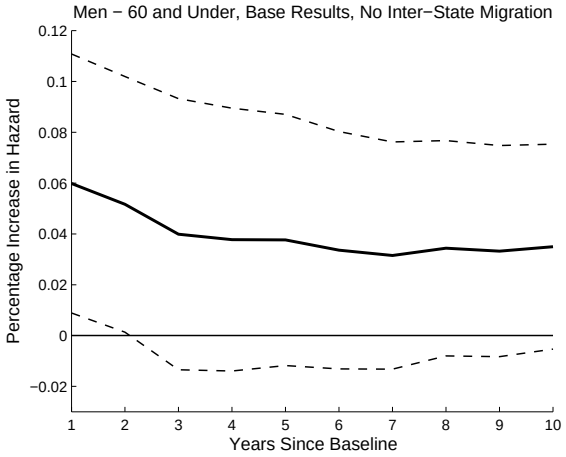

Figure 7

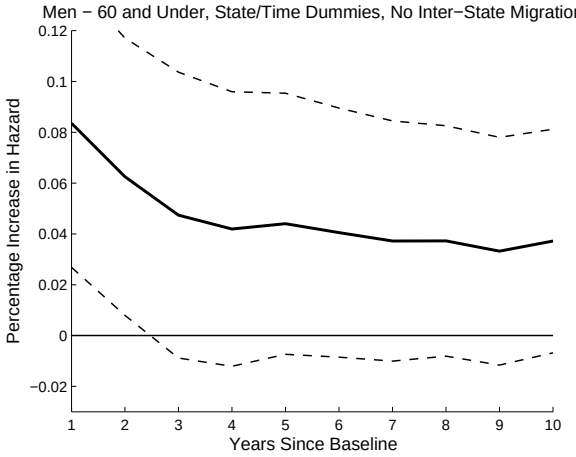

Figure 8


Men − 60 and Under, By Cause
Neoplasm
Circulatory Disease
Percentage Increase in Hazard
0.1
0.09
0.08
0.07
0.06
0.05
0.04
0.03
0.02
0.01
0
1
2
3
4
5
6
7
8
9
10
Years Since Baseline

Figure 9


Men − 60 and Under, W/ and W/O Health
W/ Health
W/O Health
Percentage Increase in Hazard
0.08
0.07
0.06
0.05
0.04
0.03
0.02
1
2
3
4
5
6
7
8
9
10
Years Since Baseline