DISCUSSION PAPER SERIES

IZA DP No. 7157

Unemployment and Mortality: Evidence from the PSID

Timothy J. Halliday

January 2013

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

Unemployment and Mortality: Evidence from the PSID

Timothy J. Halliday
University of Hawai'i at Mānoa
and IZA

Discussion Paper No. 7157
January 2013

IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

IZA Discussion Paper No. 7157
January 2013

ABSTRACT

Unemployment and Mortality: Evidence from the PSID[*]

In this paper, we use the death file from the Panel Study of Income Dynamics to investigate the relationship between county-level unemployment rates and mortality risk. After partialling out important confounding factors including baseline health status as well as state and industry fixed effects, we show that poor local labor market conditions are associated with higher mortality risk for working-aged men. In particular, we show that a one percentage point increase in the unemployment rate increases their mortality hazard 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.

JEL Classification: I0, I12, J1

Keywords: recessions, mortality, health, aggregation

Corresponding author:

Timothy J. Halliday
University of Hawai'i at Mānoa
2424 Maile Way
Saunders Hall 533
Honolulu, HI 96822
USA
E-mail: halliday@hawaii.edu

[*] 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.

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) and shows that job displacement is strongly associated with increased morbidity. Overall, 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), Miller, Page, Huff-Stevens, and Filipski (2009), and Huff-Stevens, Miller, Page, and Filipski (2011). At first glance, there does appear to be a tension between these two literatures.

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. 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.

We show 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 mortality hazard of working-aged men by 6%. The effects of this increase decline monotonically as we move away from baseline. We also show that a one percentage point increase in the unemployment rate increases 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.

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.

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

We employ data on people between the ages of 25 and 80 (inclusive) from the PSID survey years 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 at 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. 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. Additional details concerning how the sample was constructed can be found in the appendix.

Information on mortality comes from the PSID's mortality file which contains the death years of any 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 from our sample had died prior to 2005. 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 in age; dummy variables for educational attainment; and a dummy variable for being Caucasian.[1] 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

[1] Estimating a logit model is consistent with similar work on mortality and economic conditions that uses micro-data *e.g.* Sullivan and von Wachter (2009).

j years of baseline. 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.

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. Hence, in the absence of any biases, the two estimates should be similar.

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.

The approach that we and others in this literature adopt hinges on our ability to completely control for all confounding variables. As such, there may be some bias in our estimates. To help us to better establish if any effects that we find are real, we will estimate the model on different subsamples with varying degrees of attachment to the labor market. The groups are men and women 60 and under and men and women over 60. Clearly, working-age men will have the largest attachment to the labor market and if there truly are effects running from unemployment to health, the estimates should be the largest for them. On the other hand, both men and women over 60 have the weakest attachment to the labor market and so we should expect to see no effects for them. In other words, if the unemployment rates are mostly picking up omitted variables then we would expect to see a significant relationship between unemployment and mortality for people who have weak labor market attachments.[2] It is also important to emphasize that we can, to some extent,

[2] Note that there is a lingering issue associated with out-migration from high unemployment areas. Halliday (2007) shows that people who migrate to insure against business cycle fluctuations tend to be healthier and, so this would tend to bias our estimates upwards. This is an issue that impacts any study of unemployment and health since people who choose to remain in locations with deteriorating economic conditions and/or increasing numbers of factory closures will be different

control for baseline health status using the SRHS variable; we will show that doing so does have a non-trivial effect.

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 resulting from a one percentage point increase in the unemployment rate.[3] 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

than people who choose to leave. Fortunately, we do have information on SRHS which we can use to control for some of this selection. We also conduct a robustness check where we estimate the model only on a subset of "non-movers" i.e. people who do not change states while they are in the sample.

[3] Note that $p \equiv P(d_{it}^{j}|u_{it}, X_{it})$ is the mortality hazard.

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 1 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).[4]

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, Huff-Stevens, and Filipski (2009) who use aggregate data and

[4] Calculations based on the Actuarial Life Table from the Social Security Administration (available at http://www.ssa.gov/oact/STATS/table4c6.html) indicate the mortality rate for men ages 30 to 60 is 402.40 per 100,000 and 6% of this is 24.14.

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 of the results for 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 see illustrate this, we present Table 3 in which we report the R^2 of a regression of county level unemployment rates onto state and year fixed effects. We see that, even 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.[5]

We report the results in Table 4 and plot the estimates along with their 95% error bands in Figures 2-4. 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

[5] In the PSID, the survey enumerator asks the respondent if they have an occupation. This is then followed by asking the respondent what industry their occupation is in. If the individual is not presently "working for money" then occupation or industry is coded as zero. We include these individuals (with an additional dummy for zero) in all of the estimations so as not to induce any selectivity biases.

dummies. The results are plotted in Figures 5 and 6. Nothing changes suggesting that this mechanism is not strong enough to meaningfully change the results.

4.3 Results by Cause of Death

In Table 5, 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 7. 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.

4.4 Results with and without Health Status

We conclude by presenting Figure 8 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 1. Second, we plot the marginal effects from the same model that generated Figure 1 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 Implications

5.1 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) 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.[6] 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.

[6] See column 1 of Table 4 from their paper. Also, note that there is not a similar estimate that we are aware of from the PSID. 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.

5.2 Measuring the Benefits of Macroeconomic Policy

Another implication of these estimates is that one can quantify the effects of macroeconomic policies in a very intuitive way: lives saved.[7] If it is believed that a government policy reduced the unemployment rate by one percentage point then this suggests that 24.1 fewer people per 100,000 died in the following year among the population of working-aged men. If we further consider that the population of men between the ages of 25 and 60 in 2000 was about 68 million this translates to 16,320 fewer deaths in the following year as a consequence of the policy. We can go one step further and value each of these lives at $100,000 per life-year as in Cutler (2004) and obtain that the policy yielded a benefit of $1.6 billion dollars in lives saved the following year.

6 Conclusions

In this paper, we employed mortality information from the PSID to establish a robust positive association between unemployment as proxied by county-level unemployment rates and mortality for working-aged men. In particular, we showed that

[7] I am indebted to Edward Lazear for pointing this out in private communication as a way of measuring the effectiveness of the bail-out of US auto manufacturers in 2008.

a one percentage point increase in the unemployment rate increases the mortality hazard by 6%. There was no evidence of such a relationship for people with weaker labor force attachments such as working-aged women or people older than 60. These results compliment findings in Halliday (2012) that earnings shocks have substantial adverse effects on self-rated health for working-aged men but smaller effects for working-aged women.

Our findings along with Strully (2009) and Sullivan and von Wachter (2009) stand in contrast to results based on aggregate data such as Ruhm (2000) where there is a *negative* relationship between unemployment and mortality. Notably, Miller, Page, Huff-Stevens, and Filipski (2009) show that this relationship holds for the very old and the very young suggesting that the mechanism for the result has to be something that is unrelated to job loss. More recent work by Huff-Stevens, Miller, Page, and Filipski (2011) suggests that these results are the consequences of increases in vehicular accidents and decreases in the quality of medical care for the elderly during boom times.

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

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

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 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. If there are errors in this measurement that are correlated with u_{st} then this will cause estimates of β in model (3) to be biased. The advantage of model (2) is that measurement of d_{ist}^{1} is trivial whereas the measurement of d_{st}^{1} 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 measurement errors might matter 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. 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. 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.

7 Appendix: Sample Construction

We begin with 20,338 individuals. 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. After doing this, the sample size becomes 16,769. Of these individuals, 8045 are male and 8724 are female.

References

Blanchard, O., and L. Katz (1992): "Regional Evolutions," *Brookings Papers on Economic Activity*, (1), 1–61.

Blundell, R., and T. Stoker (2005): "Heterogeneity and Aggregation," *Journal of Economic Literature*, 43(2), 347–391.

Browning, M., A. Dano, and E. Heinesen (2006): "Job Displacement and Stress-Related Health Outcomes," *Health Economics*, 15(10), 1061–1075.

Cutler, D. M. (2004): *Your Money or Your Life*. Oxford University Press, Oxford.

Halliday, T. (2007): "Business Cycles, Migration and Health," *Social Science and Medicine*, 64(7), 1420–1424.

——— (2012): "Earnings Growth and Movements in Self-Rated Health," IZA Working Paper.

Huff-Stevens, A., D. Miller, M. Page, and M. Filipski (2011): "The Best of Times, The Worst of Times: Understanding Procyclical Mortality," NBER Working Paper.

Meghir, C., and L. Pistaferri (2004): "Income Variance Dynamics and Heterogeneity," *Econometrica*, 72(1), 1–32.

Miller, D., M. Page, A. Huff-Stevens, and M. Filipski (2009): "Why are recessions good for your health?," *American Economic Review*, 99(2), 122–127.

Ruhm, C. (2000): "Are Recssions Good for your Health?," *Quarterly Journal of Economics*, 115(2), 617–50.

Strully, K. (2009): "Job Loss and Health in the US Labor Market," *Demography*, 46(2), 221–246.

Sullivan, D., and T. von Wachter (2009): "Job Displacement and Mortality: An Analysis Using Administrative Data," *Quarterly Journal of Economics*, 124(3), 1265–1306.

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)
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: Regression of Unemployment Rates on State and Year Fixed Effects

	(1)	(2)
R^2	0.2256	0.3399
State Dummies	X	X
Year Dummies		X

Table 4: 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 5: 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

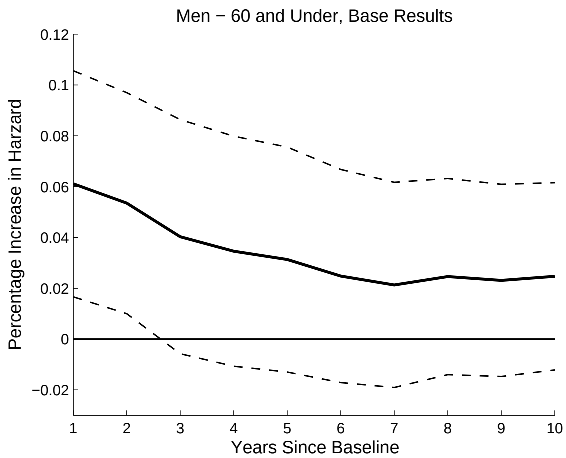


Figure 2

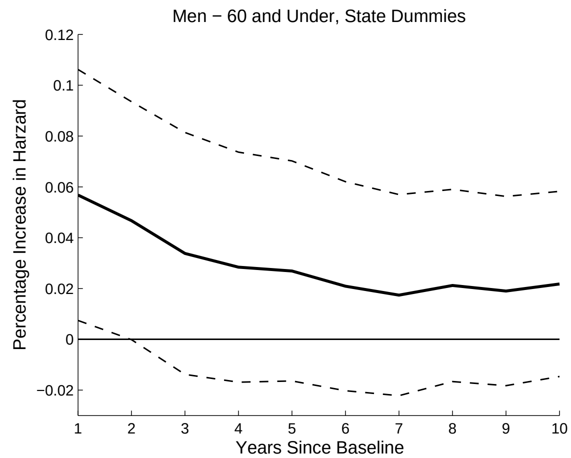


Figure 3

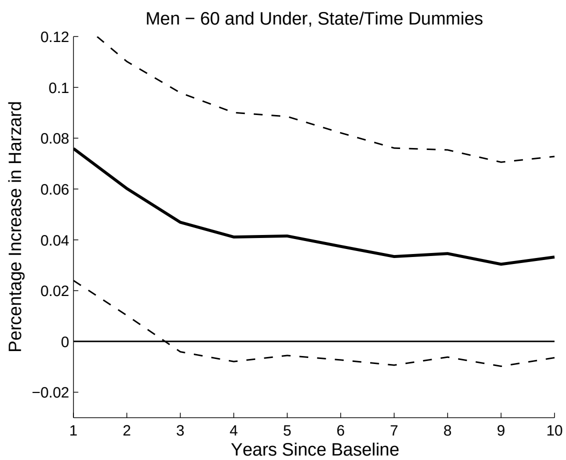


Figure 4

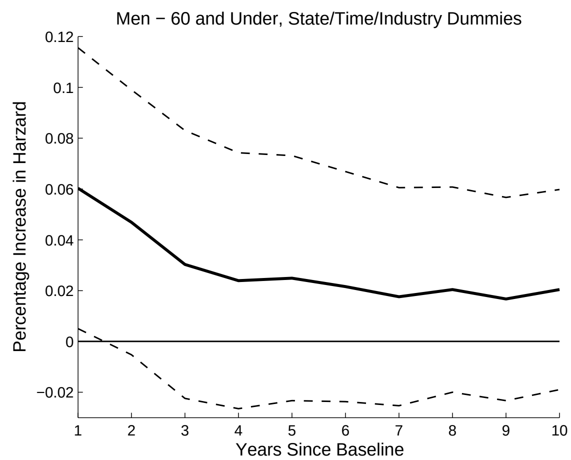


Figure 5

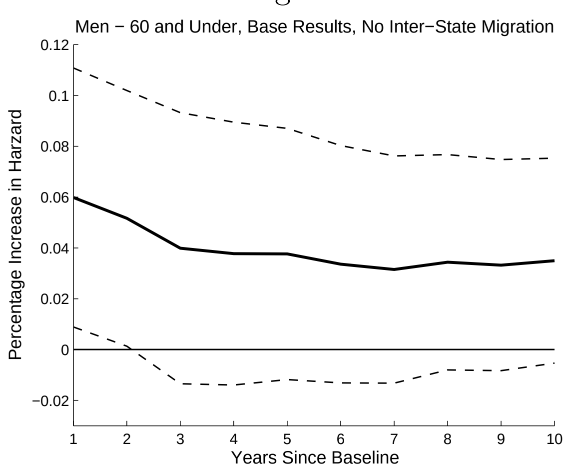


Figure 6

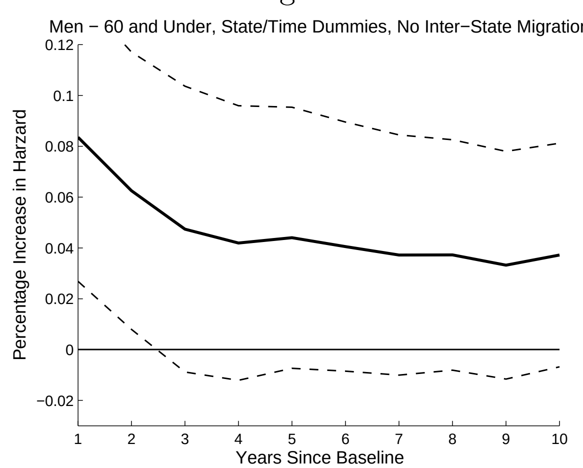


Figure 7

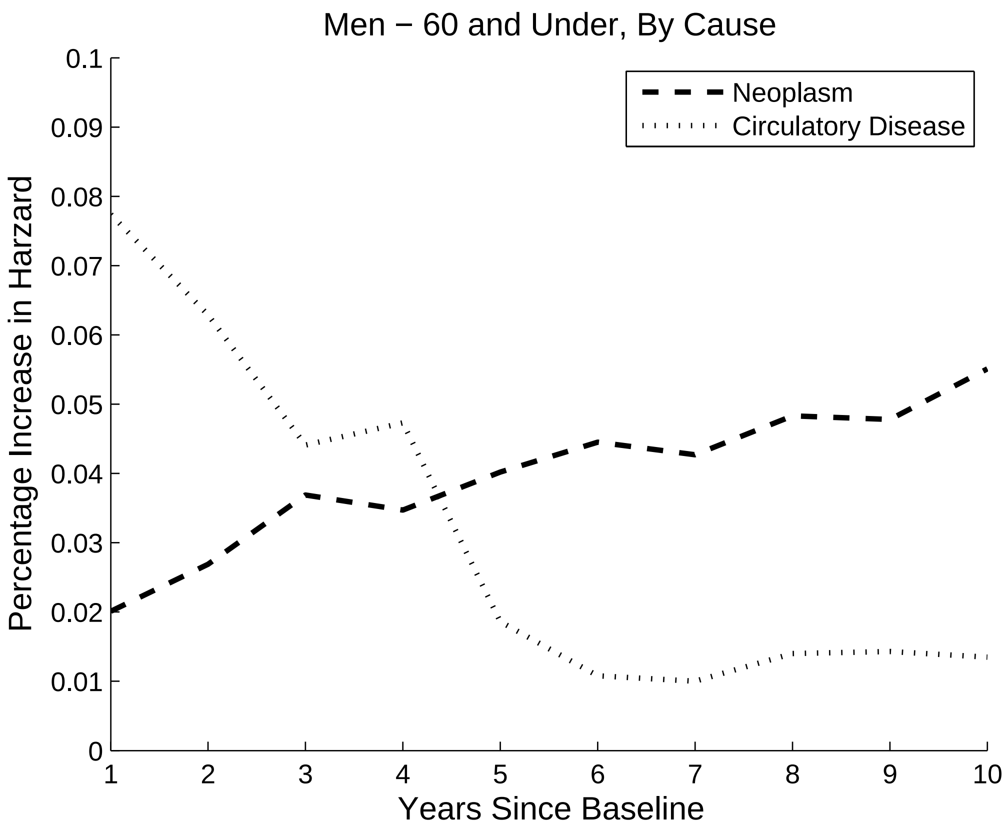
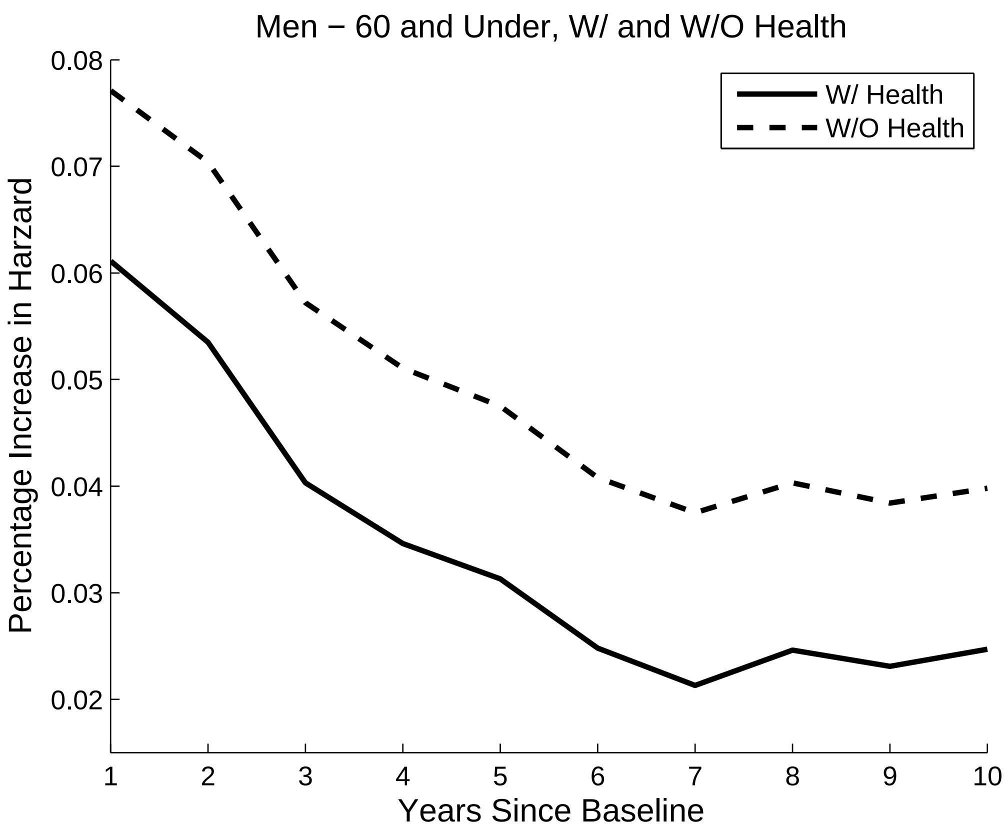

Men − 60 and Under, By Cause
Neoplasm
Circulatory Disease
Percentage Increase in Harzard
0
0.01
0.02
0.03
0.04
0.05
0.06
0.07
0.08
0.09
0.1
1
2
3
4
5
6
7
8
9
10
Years Since Baseline

Figure 8


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