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Measuring the Health Cost of Prolonged Unemployment: Evidence from the Great Recession

Timothy K. M. Beatty*   Joseph A. Ritter†

July 2018

Abstract: How much does a year of unemployment affect a person's health? Previous studies estimate the health effects of job loss after a follow-up period, but the length of unemployment spells within the follow-up is an implicitly variable treatment. Thus estimates based on a fixed follow up average over unemployment spells of different lengths, which implicitly depend on macroeconomic conditions. We estimate the effects of time unemployed and find robust negative effects of duration on men's self-assessed health. For women the estimated effects are smaller and less precise. We use an instrumental variables approach to account for dynamic selection driven by feedback from health to duration via search intensity or reservation wages. Combining these effects with prior estimates of the relationship between self-assessed health and specific-cause mortality suggests the effects correspond to large social costs.

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1 Introduction

Previous research has established that job loss has negative health consequences. We ask a related but different question: How much does it impair a person's health to be unemployed for a year? To our knowledge this question has not been directly answered. Instead, previous research has answered a question along the lines of: how much does it hurt your health to have lost your job a year ago? An answer to that question is certainly valuable, but to fully understand the health burden of unemployment there are three things wrong with substituting the second question for the first.

First, for an individual, the answer almost surely depends on how much of the year you were unemployed, but people remain unemployed for different amounts of time, and the distribution of spell length shifts from recession to recession and place to place. In that sense these estimates lack external validity. In other words, in job-loss studies, people experience a lower average treatment intensity when labor markets are tight than during recessions. This is important, for example, when trying to understand the health effects of Great Recession, which was characterized by an unprecedented increase in unemployment duration.

Second, the estimates are contaminated by possible rebound effects for people who are reemployed during the year, which also vary with macroeconomic conditions.[1] Third, even if the distribution of spell lengths within the year were known, dynamic selection driven by feedback from health to search intensity and or reservation wages makes it impossible to recover the health effect of a given length of unemployment.

This paper directly examines the link between unemployment *duration* and self-assessed

[1] This is not to say that reemployment eventually returns workers to their pre-displacement health, only that any long-term effects probably vary with the length of the unemployment spell.

health. Rather than viewing job loss as a binary indicator of an implicitly variable treatment, we model unemployment as a variable-intensity treatment. The 2005-2015 period provides unprecedented variation in treatment intensity (figure 1), allowing much sharper identification than would have previously been possible.

Estimating the effect of unemployment duration on health presents different methodological challenges than studying job loss: health deterioration potentially reduces the intensity and efficacy of search and/or raises the reservation wage – a kind of survival bias – making unemployment duration endogenous. Consider a hypothetical experiment wherein a group of workers randomly lose their jobs. Now imagine that unemployment does not affect health and consider those who remain unemployed after some fixed time, say 26 weeks. If healthier individuals are able to engage in more intense search and/or have lower reservation wages, this subsample will be less healthy on average than the population or those unemployed for a shorter time, not because their unemployment interfered with their health, but because their health interfered with search. Stewart (2001) provides evidence that this is more than a hypothetical concern. One aspect of this problem is that people who are less healthy when they enter unemployment may immediately have lower search intensity and therefore longer unemployment spells. We take an instrumental variables approach to this issue. Our instruments for duration are based on the average duration of unemployment for individuals sharing certain characteristics with the respondent. The instrumental variables approach also addresses the likely error in reported unemployment duration.

As mentioned, research on the effects of job loss has asked a different type of question. One branch of the literature uses panel data to observe change in health status for individuals who lose jobs between survey waves (Linn et al., 1985; Burgard et al., 2007; Strully, 2009; Schaller and Stevens, 2015). A second series of papers uses administrative data to study the effect of displacements on long-run health outcomes (Sullivan and Von Watcher, 2009; Eliason and Storrie, 2009; Browning and Heinesen, 2012). A common feature of both approaches is that they do not take into account the length of unemployment spells; the timing of health assessments is determined by a pre-determined follow-up or by the timing of the next wave of a panel. In fact, most unemployment spells are short, and significant time may elapse between the end of a spell and subsequent assessment of health outcomes, allowing for possible health rebound. Papers from both branches find that job loss leads to negative health effects, but estimates vary, at least in part, we argue, because macroeconomic conditions differ.[2]

The research designs of many of these papers are informed by the possibility of health-based selection into unemployment. Since our estimates are based on comparing the health of individuals who have been unemployed for different amounts of time, differences between employed and unemployed workers do not affect our results.

The main contribution of this paper is to provide estimates that address the three concerns raised above. There are two additional contributions. In order to avoid health-based selection into unemployment, most research has studied only job losses from establishment closings, which comprised only 18 percent of gross job losses during the Great Recession.[3] Our main estimates apply to all displaced workers, both men and women, who made up 54 percent of the unemployed during 2009-2010.[4] Second, much of the recent re-

[2]A third branch of the literature is concerned with the effects of business cycles on overall population health, not just the health of the unemployed (Ruhm, 2000; Ruhm 2015).

[3]Authors' calculations using Business Employment Dynamics of job losses due to closings relative to gross losses, 2007:Q4 (NBER peak) to 2009:Q2 (NBER trough).

[4]Displaced workers are those who lost a job permanently or are on temporary layoff. The remaining 46

search has used data from Scandinavian countries, but the U.S. population differs in many ways from that of Scandinavian countries and, perhaps more crucially, so do the social safety net and health care finance system, a point made previously by Sullivan and von Wachter (2009).

Using data from the Current Population Survey, we find practically and statistically significant negative effects of time unemployed on men's health. Our estimates for women are less definite: though negative in almost all specifications, they are smaller and less precise. Estimates by Benjamins et al. (2004) of the relationship between self-assessed health and mortality suggest that the effects we estimate likely translate into important objective health consequences.

We next describe the data we use. Section 3 describes our instrumental variables strategy and discusses health-based selection into unemployment. Section 4 presents our main results and various sensitivity checks. Section 5 addresses the relationship between our estimates based on self-assessed health and objectively measured health outcomes.

2 Data

Our primary data source is the Annual Social and Economic Supplement (ASEC) of the Current Population Survey for 2005-2015. The ASEC asks respondents to rate the health of household members on a five-point scale from poor to excellent. Self-assessed health has been found to be a consistent predictor of mortality, future diagnoses, and future use of medical services (Latham and Peek, 2013; Becchetti et al. 2015; Idler and Benyamini,

percent were new entrants, reentrants, job leavers, or those for whom a termporary job ended. Job losses through establishment closings are a subset of permanent job losses, but are only separately identified in the January Displaced Worker Supplements.

1997; Doiron et al., 2015; Benjamins et al., 2004). Although there is extensive evidence that self-assessed health predicts concrete health outcomes, changes in a self-reported measure may also reflect psychological factors that may not be directly tied to physical health. Indeed, Browning et al. (2006) and Kuhn et al. (2009) find significant effects of layoffs on mental health outcomes.

We discard observations with imputed health data. Except where otherwise noted, we also drop observations with proxy responses for health status (more than half of responses for unemployed individuals).[5] Finally, we recode the health variable to dummy variables in order to be consistent with previous literature, and because it is difficult to handle an endogenous regressor (duration) with an ordered dependent variable.

The distributions of self-assessed health in our primary regression samples are shown in the first two columns of table 1. Characteristics of our sample of unemployed individuals are summarized in table 2.

The period covered by our sample includes three years before the onset of recession and continues through 2015. As one might expect from figure 1, the distribution of unemployment duration shifted dramatically to the right during and after the recession, ensuring substantial variation in our variable of interest. Figure 2 shows the distribution of unemployment duration for three years in our sample period. The 2010 distribution is characterized by fewer short spells and more long spells compared to 2005 and 2015. Within these distributions, women and workers on temporary layoff tend to have shorter durations.

For supplemental analyses, we match individuals in adjacent ASECs and also match individuals in the even year ASECs to their responses on the respective January displaced

[5] In the CPS, one member of the household responds for the entire household.

worker supplements.[6]

3 Research Design

3.1 Instrumental variables strategy

As noted earlier, the central identification problem we face is a form of survival bias. If healthier individuals engage in more intense search or have lower reservation wages, their exit-to-employment hazard will be higher. This generates a dynamic selection process: the people who remain in the unemployment pool after $n+1$ weeks are on average less healthy than those who have been unemployed only n weeks, even if unemployment does not affect health.

The problem of identifying a link between unemployment duration and health thus requires an instrumental variables strategy. A second problem addressed by the use of instruments is that unemployment duration is probably reported with significant measurement error, which likely biases OLS estimates in the opposite direction as the dynamic selection. Since the CPS duration question is retrospective, measurement error is likely to be larger for longer durations. The duration data are also top-coded at 99 weeks.

We estimate linear probability models of the form:

$$H_{it} = \alpha + \beta D_{it} + X_{it}\gamma + W_{st}\eta + \tau_t + \xi_s + \delta_s t + \varepsilon_{it} \tag{1}$$

where H_{it} is an indicator of the health status of an unemployed individual i in year t. X_{it} is a vector of individual characteristics: (1) a quadratic in age; (2) educational attainment

[6] Our matching algorithm follows that suggested by Madrian and Lefgren (2000).

indicators; (3) race indicators; (4) whether Hispanic; and (5) indicators for marital status interacted with the employment status of the respondent's spouse or unmarried partner, if present. W_{st} is a vector of time-varying state characteristics, ξ_s are state fixed effects, and $\delta_s t$ are state-specific trends. Our main interest is the effect of D_{it}, the duration of unemployment, measured in years, at the time of the survey.

The time-varying state characteristics, W_{st}, are real personal income per capita and the employment-population ratio. These merit particular attention. Duration of unemployment is markedly counter-cyclical and a number of studies have found links between population health and business cycles (for example, Ruhm, 2000), although the link has apparently weakened (Ruhm, 2015). Since we want $\hat{\beta}$ to capture the effect of prolonged individual durations, we include these controls to partial out any general connection between business cycles and health. They are similar to the business cycle variables used by Ruhm (2000), though we use the state employment-population ratio instead of the unemployment rate because of the importance of labor-force exits during the Great Recession.

Additionally, the availability of extended unemployment insurance (UI) benefits is triggered by high unemployment rates. The employment-population ratio controls for this as well. Cross-sectional differences in state UI generosity are captured by state fixed effects.

We now turn to the construction of our instruments for D_{it}. CPS respondents report the reason an individual became unemployed: she was laid off permanently or temporarily, finished a temporary job, quit, had reentered the labor market, or was a new entrant. We consider only the first two (displacements). The value of our primary instrument for a particular individual is the average duration of unemployment for others who were unemployed for the same reason she was (temporary layoff or permanent job loss), in her state, and in the year she was in the ASEC.

The logic behind this instrument is to capture differences in the duration distributions experienced by the respondents. Part of this is captured by differences across states and years. In addition, the two why-unemployed categories predict different duration distributions within states; people on temporary layoff have much lower average observed duration than those who lost their jobs permanently.

In this application, compliers are those whose unemployment has been prolonged because of the labor market conditions they face. The monotonicity condition (Angrist and Imbens, 1995) is highly plausible here: worsening labor market conditions will not lower the likelihood of unemployment being extended by one more week. Understanding the LATE requires one more step because we observe unemployment spells that are censored at the time of the ASEC. Thus, even in the worst state-year labor market we observe many spells that just began; see Figure 2. People with short observed durations in a bad labor market are effectively never-takers; their observed duration has not yet been affected by labor market conditions. It turns out this is not a problem: the instrument produces very high F-statistics. Evidently, the Great Recession provides powerful help identifying the effect of unemployment duration by providing us with many compliers with different treatment intensities.

It is possible that the health of permanent job losers differs from that of temporary layoffs at the time of displacement, which would violate the exclusion restriction. We address this concern in section 3.2.

The rotating panel design of the CPS suggests the possibility of controlling for individual fixed effects by differencing equation (1). We report these results, but do not emphasize this approach because it greatly reduces sample size leading to very imprecise estimates.

At most half of ASEC respondents are shared between consecutive ASEC supplements, and all of those who transition to employment during the intervening year must be excluded since we observe their health for the second time some unknown number of weeks after their unemployment spell ended, and we do not observe the length of the completed spell. On the other hand, those who transition from employment or out of the labor force to unemployment are included.

3.2 Health-based selection into unemployment

A different source of potential bias has concerned previous work: individuals who become unemployed may be less healthy on average than those who don't and are possibly following worse health trends. This concern is not relevant here because we are not comparing unemployed people to employed people, but a closely related issue concerns the instrument described in the previous section: are there systematic differences in health trends across reasons for unemployment?

While we can control for some possible demographic or geographic reasons for differences in health trends, a potential channel which we cannot directly control, is that health deterioration might reduce productivity and in turn lead to displacement. This is a special case of the adverse selection mechanism described by Gibbons and Katz (1991). Gibbons and Katz argue that workers displaced by plant (establishment) closings are not affected by this adverse selection because, when plants close, layoffs are not selected on the basis of individual productivity. Since plant closing implies permanent job loss, the adverse selection mechanism could be stronger on average for workers on temporary layoff. In that case, an instrument based on the reason for unemployment might not satisfy the exclusion restriction.

Previous evidence on the importance of this adverse selection channel is inconclusive. Sullivan and von Wachter (2009) find no evidence that their results on mortality were affected by this type of selection. Other studies look for evidence of the same mechanism of productivity-linked displacements (though not specifically linked to deterioration of health) by comparing wage losses for workers displaced by plant closings to losses for workers displaced for other reasons. Gibbons and Katz (1991) find clear evidence that workers displaced by plant closings suffered smaller wage losses, but Hu and Taber (2011), using more years of displaced worker supplements, only find this effect for white males.

We address this concern in two ways. First, we present (less precise) results using an instrument that does not use reason for unemployment. Second, in this section we look for direct evidence of the mechanism by comparing the health of individuals who experience displacement from plant closing to the health of those experiencing other types of displacement.

Since the ASEC does not distinguish plant closings from other reasons for job loss, we construct a longitudinal match between the January displaced worker supplements for 2006, 2008, 2010, 2012, and 2014 with the respective ASEC responses (1-3 months later).[7] Then we ask whether the health of individuals who experienced a plant closing differs from the health of individuals who were displaced due to "insufficient work" or "position or shift abolished." Specifically, we regress the health variable on indicators for the year of job loss relative to the survey, the interaction of a plant closing dummy with year of job loss, and controls.

[7] This match does not in general allow us to establish whether or not a current (i.e., at the time of the ASEC) spell of unemployment was initiated by an plant closing since an individual may have been employed between the spell triggered by a plant closing and the current spell.

If workers laid off due to insufficient work or position abolished had worse health on entry to unemployment and/or were on a steeper downward trend, the coefficients on the plant closing interaction terms in table 3 should be positive. In fact, the interaction coefficients are individually and jointly insignificant, and only two of six interaction coefficients are positive. Similar results obtain using an indicator of good or better health as the dependent variable. We conclude that there is no evidence for this selection mechanism during the period we study.

If the health effects of unemployment are sufficiently large, an individual might choose to exit the labor force and thus be absent from our sample. This would bias our estimates towards zero. However, among the 11 ASEC samples we use, only 1.7 to 2.9 percent of non-employed individuals who worked at least a week during the previous year report they cannot work due to health or disability. Many of these work limitations would be due to injury or illness not related to unemployment. Thus it appears unlikely that this kind of selection has much effect on our estimates

4 Results

Our main results are reported the second row of table 4.[8] For men the effect of a year of unemployment is a 0.15 decrease in the probability of reporting very good to excellent health. For women the effect is a decrease of 0.08. When the dependent variable indicates good to excellent health, the effects are smaller and, for women, not statistically distin-

[8]Complete regressions, first-stage regressions, and reduced-form regressions are reported in appendix tables A-2, A-3, and A-5.

guishable from zero.[9] For brevity we discuss mainly the first set of estimates and refer to them as our main estimates; the most important difference between the two sets is that the good-or-better estimates generally show little effect for women.

OLS estimates are much smaller than the instrumental variables estimates. The difference captures the net effect of dynamic selection, measurement error, and the fact that the LATE estimates the average effect for compliers only. In this context that means the LATE excludes many individuals whose short unemployment durations have not (yet) been affected by labor market conditions.[10]

These impacts of a year of unemployment are an order of magnitude larger than some estimates of the effect of having lost a job a year ago. For example, Schaller and Stevens (2015) find that displacement decreases the probability of being in good to excellent health by 0.014. A key difference is that Schaller and Stevens' health data is measured six to twelve months after job loss (during 1996-2011), so their estimates average over the net effects of varying spells of unemployment, most of which would have ended at some time before the second health assessment (again, because most spells are short). It is impossible make our estimates exactly comparable to theirs, but the following calculation gives a sense of how important this difference is. We know how many weeks individuals in the CPS have been exposed to unemployment at a given time (i.e., duration). If we consider only individuals who were displaced within the last year, the average for 2006 was 11.8

[9]Differences between results for the two dependent variables cannot be interpreted as evidence that healthier people are more vulnerable to effects of unemployment because the health variable is ordinal. Respondents may see the difference between good and fair may be wider (or narrower) than how they see the difference between very good and good. Also, note that when the cutoff is changed, estimates change partly because the fractions of people in the adjacent categories changes. We report estimates that treat the health variable as numeric in table A-4.

[10]More precisely, with variable treatment intensity the LATE weights individual treatment effects proportionally to "the number of people who, because of the instrument, change their treatment from less than j units [weeks] to j or more units [weeks]" (Angrist and Imbens, 1995).

weeks for men, 11.9 weeks for women. Rescaling the main estimates from table 5 gives us a health impact of $-0.150 \times (11.8/52) = -0.03$ for men and -0.020 for women, which are much closer to Schaller and Stevens' estimates. However, average weeks of exposure to unemployment was nearly 70 percent higher during 2010, which highlights that the average health impact of job loss depends critically on *when* the job was lost.

Table 5 considers several variations on the main specification. Row (a) repeats the estimation without state-specific trends.

Row (b) uses an alternate instrument, the average duration of unemployment of others in the individual's state-year cell, which does not rely on reason for displacement (i.e. permanent vs. temporary layoff). The results are much less precise, but similar to the main specification except in column 2. We attribute the loss in precision to the loss of cross-sectional variation in the instrument coming from reason for unemployment.[11]

Rows (c) and (d) address a technical issue with the design of the CPS and ASEC. Halpern-Manners and Warren (2012) found strong evidence of panel conditioning in CPS labor force status, specifically that individuals become less likely in later months to be categorized as unemployed. Rows (c) and (d) address this possibility in a simple way by splitting the sample by month in sample. The month 5–8 effects are closer to zero in all four columns, but the differences are small except in column 2. We have no explanation for the large difference in column 2.

Rows (e) reconsiders our decision to exclude proxy responses, including them while still excluding imputations. In these regressions we include a dummy variable for whether the observation is a self-report in order to remove the average bias of proxy reports (relative

[11] We do not perform over-identification tests because the instruments correspond to different sets of compliers, and heterogeneous treatment effects are to be expected in this application.

to self reports). To the extent that proxy responses are less reliable, the added variability changes the variance of the error; heteroskedasticity-robust standard errors account for this. These estimates are more precise but not dramatically different than the main specification.

Finally, row (f) implements a first-difference specification using all individuals who have valid data for two ASECs and were unemployed at the time of the second ASEC. We assign change in unemployment duration as follows. If an individual was not unemployed in the first year or was unemployed in both years, but reported a duration of less that 50 weeks in the second ASEC, we use the duration reported at the second ASEC. If the individual was unemployed in both years and reported a duration of 50 weeks or more in the second year, we set the change in duration to 52 weeks. This adjustment removes a great deal of evident measurement error, presumably due to the retrospective nature of the duration question.[12] The instrument is the average duration for the respondent's reason unemployed-state-year cell for the second year in the ASEC. To increase sample size, we include any individual for whom both health responses were self-reports or both were proxies. Nevertheless, sample sizes are still reduced by 70 percent for men and 79 percent for women compared to the main specification. As one might expect, the resulting estimates are very imprecise.

It is natural to wonder whether duration affects health nonlinearly. However, nonlinearities in our regressions would not correspond closely to nonlinearity in the effect of duration on an underlying continuous health variable. Our regressions describe whether

[12]Although we do not know in which month a household responds to the ASEC, it is always in the same month in both years. The instruments remain valid as long as memory lapses are not correlated with reason for unemployment or state of residence, but when we do not make the adjustment to durations, the first-difference specification yields much larger negative impacts with much larger standard errors.

individuals are on the good or bad side of a threshold on a coarse health assessment; beyond that they do not track people's deteriorating health. We did explore the possibility of nonlinear effects on these discrete transitions in several ways (polynomial specifications, dummy variable specifications, and linear splines). We found no evidence of nonlinear effects. The coefficient for squared duration, for example, was of meaningful size, but with a large standard error; we found analogous results with other functional forms. Thus, though we cannot rule out nonlinearity, the data are not very informative about it.

Our overall assessment is that there is clear evidence of an important negative health effect of prolonged unemployment for men: every coefficient in table 5 is negative and all are statistically significant except in the first-difference specification. For women the evidence is weaker. The point estimates, though negative in all but two cases, are generally closer to zero than those for men, and they are less precisely estimated. Table A-4 reinforces this overall conclusion.

5 Discussion and Conclusion

How do estimates based on self-assessed health translate to more objective outcomes? As discussed in section 2, there is extensive evidence connecting self-assessed health with objective health outcomes. Benjamins et al. (2004) estimate the relationship between self-assessed health and mortality and present results in a convenient way for making the translation. We combine their estimates with ours to translate effects for self-assessed health to mortality effects. We offer these calculations as an illustration that objectively measured health effects of unemployment can be important, not as reliable estimates.

Benjamins et al. use data from the 1986–1994 rounds of the National Health Interview

Survey (NHIS) with a follow-up link to the National Death Index to estimate the extent to which self-assessed health predicts mortality. The average follow-up period was 7 years. The self-assessment question in the NHIS is identical to that in the ASEC.

Benjamins et al. report mortality hazard ratios relative to excellent health. Therefore, we first convert these to the hazard ratio for good health relative to very good health (or fair relative to good) by dividing two of their estimates. We then convert that hazard ratio to a percent change (i.e., 1.2 to 20 percent) and multiply by the change in the probability of the corresponding transition from table 4. In the case of overall female mortality, for example, the hazard ratios for very good and good health relative to excellent health are 1.12 and 1.37, implying that the hazard ratio for good relative to very good health is $1.37/1.12 = 1.223$. Thus, for women, table 6 reports $0.083 \times 22.3 = 1.9$ percent. The remaining rows cover causes for which Benjamins et al. find statistically significant results.

Average duration of unemployment of displaced workers increased from about 15 to 31 weeks for both men and women between 2006 and 2010, and the number displaced from 2.6 to 7.8 million, resulting in 3.9 million additional person years of unemployment. Our estimates imply that about 250,000 people (mostly men) switched from very good to good or from good to fair health and were, therefore, exposed to the increased risks shown in table 6. This illustrative calculation does not incorporate person years of unemployment for people whose spells did not begin or end unemployed during 2010, health transitions other than the two shown, morbidity, or medical expenditures, among the factors a complete analysis of health costs would need to account for.

The translation of our estimates to mortality hazards numbers of premature deaths is obviously a crude exercise. However, it emphasizes that point estimates of the magnitude we report are far from trivial when translated into specific health outcomes. Thus the estimated effects of unemployment on self-assessed health appear to correspond to large social costs, especially when, as during the period under study, large numbers of people have been unemployed for prolonged periods.

Although negative effects of job loss have previously been well documented, those estimates average across an implicitly variable treatment intensity: the duration of unemployment. Morever, the distribution of unemployment duration varies with time and place.

We directly estimate the effect of time unemployed, accounting for endogeneity of unemployment duration. Time unemployed has large and statistically significant negative effects on men's self-assessed health. For women the data are more equivocal: estimated effects are smaller and, in many cases, not statistically significant. We link these estimates with prior work connecting self-assessed health with objective health outcomes to illustrate that the estimated effects have considerable practical importance.

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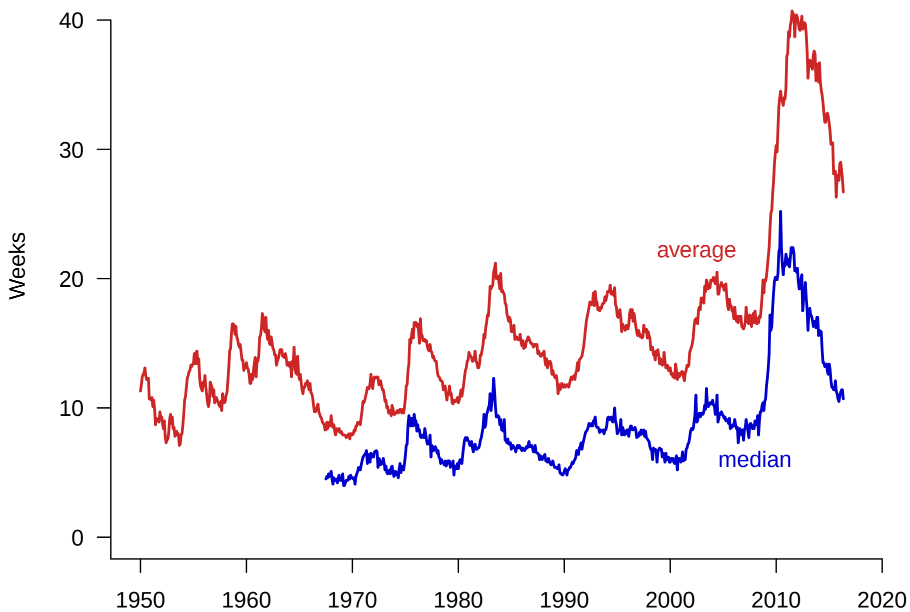
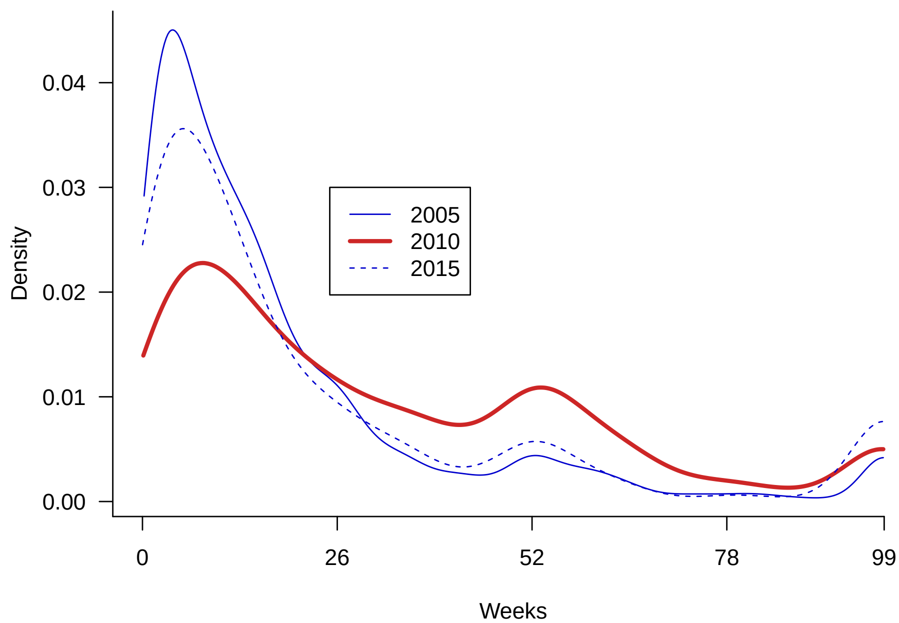
Figure 1: Unemployment duration



Source: Bureau of Labor Statistics

Figure 2: Distribution of unemployment duration



Note: Based on individuals with non-imputed, non-proxy health assessments.

Table 1: Self-reported health status (percent)

	Unemployed Male	Unemployed Female	Labor Force
Excellent	23.4	20.2	31.8
Very good	32.9	32.1	36.4
Good	32.2	33.1	25.4
Fair	9.8	12.1	5.4
Poor	2.0	2.5	0.9

Note: Based on non-imputed, non-proxy responses.

Table 2: Sample characteristics

	Men		Women	
	Mean	SD	Mean	SD
Weeks unemployed	26.150	28.148	26.847	27.986
Age	43.266	12.613	43.287	12.523
Not high school graduate	0.163	0.369	0.137	0.344
High school graduate	0.375	0.484	0.330	0.470
Some college	0.191	0.393	0.232	0.422
Associate’s degree	0.083	0.276	0.110	0.312
Bachelor’s degree	0.139	0.346	0.139	0.346
Graduate or professional degree	0.048	0.214	0.052	0.222
Hispanic	0.186	0.389	0.179	0.383
Black	0.142	0.349	0.198	0.399
Asian	0.036	0.186	0.028	0.166
Spouse/partner not employed	0.181	0.385	0.118	0.322
Spouse/partner employed	0.383	0.486	0.351	0.477
Married	0.486	0.500	0.410	0.492
Separated or divorced	0.203	0.402	0.276	0.447
Widowed	0.014	0.118	0.041	0.199
Single	0.297	0.457	0.273	0.446
N	9263		7181	

Note: Based individuals with non-imputed, non-proxy health assessments.

Table 3: Displacements and health

	Men	Women
Last year	[omitted]	
(a) Two years ago	−0.013	0.010
	(0.026)	(0.026)
(b) Three years ago	0.012	0.022
	(0.028)	(0.029)
(c) Last year × plant closed	−0.008	−0.004
	(0.033)	(0.032)
(d) Two years ago × plant closed	0.004	0.047
	(0.038)	(0.035)
(e) Three years ago × plant closed	−0.010	−0.056
	(0.038)	(0.038)
R^2	0.063	0.089
N	2741	2733
H_0: (c) = (d) = (e) = 0 (p-value)	0.986	0.251

Dependent variable is very good or excellent health. Standard errors account for clustering at the state level. Controls: (1) quadratic in age; (2) educational attainment indicators; (3) race indicators; (4) whether Hispanic; (5) indicators for marital status interacted with the employment status of the respondent's spouse or unmarried partner, if present; (6) state employment-population ratio; (7) real state personal income per capita; and (8) state fixed effects and state-specific trends. Data are from the 2006, 2008, 2010, 2012 and 2014 January displaced worker supplements linked to the following ASECs.

Table 4: Health and unemployment duration

	very good or better = 1		good or better = 1	
	Men	Women	Men	Women
OLS – linear specification				
Unemployment duration	-0.059^{***}	-0.033^{***}	-0.049^{***}	-0.023^{***}
	(0.012)	(0.011)	(0.007)	(0.007)
2SLS – linear specification				
Unemployment duration	-0.150^{***}	-0.083^{*}	-0.115^{***}	-0.020
	(0.026)	(0.043)	(0.017)	(0.024)
First-stage F-statistic	1348	628	1348	628
N	9261	7181	9261	7181

Significance: $^{***}\ p < 0.01$, $^{**}\ p < 0.05$, $^{*}\ p < 0.1$. Dependent variables are binary indicators of self-assessed health as noted. Standard errors account for clustering at the state level. Controls: race, Hispanic, quadratic in age, education dummies, indicators of employment status of spouse or partner, single, separated/divorced, widowed, state fixed effects, and state-specific trends. (see table A-2 for details).

Table 5: Effect of unemployment duration in alternate specifications

	very good or better = 1		good or better = 1	
	Men	Women	Men	Women
Main specification	-0.150^{***}	-0.083^{*}	-0.115^{***}	-0.020
	(0.026)	(0.043)	(0.017)	(0.024)
(a) Omit state trends	-0.148^{***}	-0.092^{**}	-0.137^{***}	-0.016
	(0.029)	(0.046)	(0.019)	(0.030)
(b) Alternate instrument	-0.180^{***}	0.037	-0.096^{**}	-0.015
	(0.053)	(0.090)	(0.045)	(0.056)
(c) Month in sample 1-4 only	-0.148^{***}	-0.144^{**}	-0.112^{***}	-0.030
	(0.041)	(0.057)	(0.025)	(0.038)
(d) Month in sample 5-8 only	-0.133^{***}	-0.027	-0.102^{***}	-0.014
	(0.033)	(0.059)	(0.025)	(0.028)
(e) Include proxy responses	-0.172^{***}	-0.096^{**}	-0.109^{***}	-0.032^{*}
	(0.020)	(0.042)	(0.012)	(0.018)
(f) First-difference	-0.122^{*}	0.058	-0.002	-0.051
	(0.065)	(0.097)	(0.030)	(0.066)

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Dependent variables are binary indicators of self-assessed health, as indicated. Unemployment duration is measured in years. Standard errors account for clustering at the state level. Controls: (1) quadratic in age; (2) educational attainment indicators; (3) race indicators; (4) whether Hispanic; (5) indicators for marital status interacted with the employment status of the respondent's spouse or unmarried partner, if present; (6) state employment-population ratio; (7) real state personal income per capita; and (8) state fixed effects and state-specific trends. Sample sizes for the first three rows are $N_{\text{male}} = 9621$ and $N_{\text{female}} = 7181$.

(b) Instrument is average duration of unemployment for the state and year.

(c, d) Only individuals in rotation groups 1-4 or 5-8 are included.
$N_{\text{male}} = 4,738$ and 4,523, $N_{\text{female}} = 3,650$ and 3,531.

(e) Both self and proxy health reports used. Dummy variable for self-report included.
$N_{\text{male}} = 19,341$, $N_{\text{female}} = 11,129$

(f) First-difference specification using why unemployed-state-year instrument from second year in the ASEC. Proxy responses included to increase sample size. $N_{\text{male}} = 2,796$, $N_{\text{female}} = 1,536$

Table 6: Translation of estimated health effects to mortality (percent increase in hazard)

	very good to good		good to fair	
	Men	Women	Men	Women
Overall mortality	4.4	1.9	2.7	0.5
Diabetes	10.9	4.6	3.7	1.1
Heart disease	4.5	1.9	3.3	0.6
Stroke	6.1	1.9	0.0	0.5
Respiratory disease	6.7	2.0	4.8	0.9
Cancer	4.5	1.1	10.4	0.1
Infectious disease	2.8	7.7	−1.8	0.3

Note: Calculated as the product of hazard ratios from Benjamins et al. (2004) and effect sizes from table 4.

Appendix

Table A-1: First-stage F-statistics for Tables 4, 5 and A-4

	Men	Women
Main specification	1348.4	628.0
(a) Omit state trends	1257.3	534.2
(b) Alternate instrument	278.6	114.3
(c) Month in sample 1-4 only	721.7	449.9
(d) Month in sample 5-8 only	646.7	388.0
(e) Include proxy responses	2236.6	548.3
(f) First-difference	467.1	424.5

Table A-2: Complete results for main specification

	very good or better = 1		good or better = 1	
	Men	Women	Men	Women
Unemployment duration	-0.150^{***}	-0.083^{*}	-0.115^{***}	-0.020
	(0.026)	(0.043)	(0.017)	(0.024)
Employment/population	-0.009^{*}	-0.005	-0.007^{***}	-0.002
	(0.005)	(0.006)	(0.002)	(0.004)
Real disposable income per capita	0.000	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Age	-0.014^{***}	-0.019^{***}	-0.004^{**}	-0.013^{***}
	(0.003)	(0.003)	(0.002)	(0.002)
Age^2	0.000^{***}	0.000^{***}	0.000	0.000^{***}
	(0.000)	(0.000)	(0.000)	(0.000)
High school	0.074^{***}	0.087^{***}	0.051^{***}	0.092^{***}
	(0.018)	(0.018)	(0.012)	(0.016)
Some college	0.146^{***}	0.093^{***}	0.054^{***}	0.090^{***}
	(0.021)	(0.020)	(0.017)	(0.015)
Associate degree	0.119^{***}	0.153^{***}	0.054^{***}	0.110^{***}
	(0.022)	(0.019)	(0.016)	(0.023)
Bachelor's	0.265^{***}	0.246^{***}	0.097^{***}	0.145^{***}
	(0.026)	(0.021)	(0.014)	(0.019)
Graduate or professional	0.275^{***}	0.277^{***}	0.124^{***}	0.175^{***}
	(0.031)	(0.031)	(0.023)	(0.020)
Hispanic	-0.020	-0.053^{***}	0.002	0.007
	(0.017)	(0.015)	(0.008)	(0.010)
Black	0.011	-0.075^{***}	-0.007	-0.038^{***}
	(0.017)	(0.012)	(0.012)	(0.012)
Asian	-0.053^{**}	-0.104^{***}	-0.019	-0.033
	(0.026)	(0.035)	(0.017)	(0.027)
Other race	-0.022	-0.085^{***}	-0.013	-0.028
	(0.027)	(0.030)	(0.018)	(0.025)
Spouse unemployed	-0.037^{*}	-0.062^{**}	-0.009	-0.025
	(0.019)	(0.024)	(0.012)	(0.015)
Spouse employed	0.021	0.035^{**}	0.022^{*}	0.008
	(0.019)	(0.017)	(0.012)	(0.016)
Separated or divorced	-0.053^{***}	-0.050^{***}	-0.033^{**}	-0.044^{***}
	(0.020)	(0.017)	(0.013)	(0.015)
Widowed	-0.005	-0.036	-0.040	-0.054^{*}
	(0.046)	(0.032)	(0.038)	(0.031)
Single	-0.028	-0.051^{***}	0.005	-0.035^{**}
	(0.021)	(0.019)	(0.011)	(0.016)
R^2	0.070	0.070	0.043	0.050
N	9261	7181	9261	7181

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Dependent variables are binary indicators of self-assessed health, as indicated. Unemployment duration is measured in years. Standard errors account for clustering at the state level.

Table A-3: First-stage results for main specification

	Men	Women
Duration instrument	0.0171^{***}	0.0157^{***}
	(0.0005)	(0.0006)
Employment/population	-0.0112^{**}	-0.0182^{***}
	(0.0048)	(0.0051)
Real disposable income per capita	0.0000	0.0000
	(0.0000)	(0.0000)
Age	0.0142^{***}	0.0158^{***}
	(0.0022)	(0.0030)
Age^2	-0.0001^{***}	-0.0001^{***}
	(0.0000)	(0.0000)
High school	-0.0072	-0.0336
	(0.0159)	(0.0230)
Some college	0.0131	-0.0331
	(0.0169)	(0.0263)
Associate degree	-0.0061	-0.0450^{*}
	(0.0206)	(0.0267)
Bachelor’s	-0.0370^{*}	-0.0597^{**}
	(0.0218)	(0.0260)
Graduate or professional	-0.0089	-0.0349
	(0.0362)	(0.0291)
Hispanic	-0.0237^{*}	-0.0170
	(0.0129)	(0.0181)
Black	0.0343^{**}	0.0647^{***}
	(0.0167)	(0.0166)
Asian	0.0527^{**}	0.0053
	(0.0255)	(0.0364)
Other race	0.0238	-0.0206
	(0.0296)	(0.0347)
Spouse unemployed	0.0046	0.0515^{**}
	(0.0172)	(0.0246)
Spouse employed	0.0180	0.0070
	(0.0169)	(0.0183)
Separated or divorced	0.0153	0.0049
	(0.0170)	(0.0194)
Widowed	0.0464	-0.0096
	(0.0565)	(0.0269)
Single	0.0514^{***}	0.0384
	(0.0161)	(0.0240)
R^2	0.2315	0.2093
N	9261	7181

Notes: $^{***}\ p < 0.01$, $^{**}\ p < 0.05$, $^{*}\ p < 0.1$. Dependent variable is years of unemployment. Standard errors account for clustering at the state level.

Table A-4: Effect of unemployment duration, self-reported health level

	Men	Women
Main specification	-0.410^{***}	-0.201^{***}
	(0.050)	(0.078)
(a) Omit state trends	-0.432^{***}	-0.181^{**}
	(0.055)	(0.087)
(b) Alternate instrument	-0.379^{***}	-0.074
	(0.130)	(0.174)
(c) Month in sample 1-4 only	-0.416^{***}	-0.261^{**}
	(0.083)	(0.111)
(d) Month in sample 5-8 only	-0.353^{***}	-0.156
	(0.065)	(0.110)
(e) Include proxy responses	-0.451^{***}	-0.252^{***}
	(0.043)	(0.080)
(f) First-difference	-0.418^{***}	0.012
	(0.112)	(0.245)

Notes: $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.1$. Dependent variables is self-assessed health (5=excellent,. . . , 1=poor). Unemployment duration is measured in years. Standard errors account for clustering at the state level. Controls: (1) quadratic in age; (2) educational attainment indicators; (3) race indicators; (4) whether Hispanic; (5) indicators for marital status interacted with the employment status of the respondent's spouse or unmarried partner, if present; (6) state employment-population ratio; (7) real state personal income per capita; and (8) state fixed effects and state-specific trends. Sample sizes for the first three rows are $N_{\text{male}} = 9621$ and $N_{\text{female}} = 7181$.

(b) Instrument is average duration of unemployment for the state and year.

(c, d) Only individuals in rotation groups 1-4 or 5-8 are included.
$N_{\text{male}} = 4,738$ and 4,523, $N_{\text{female}} = 3,650$ and 3,531.

(e) Both self and proxy health reports used. Dummy variable for self-report included.
$N_{\text{male}} = 19,341$, $N_{\text{female}} = 11,129$

(f) First-difference specification using why unemployed-state-year instrument from second year in the ASEC. Proxy responses included to increase sample size. $N_{\text{male}} = 2,796$, $N_{\text{female}} = 1,536$

Table A-5: Reduced form estimates (coefficients on instrument)

	very good or better = 1		good or better = 1	
	Men	Women	Men	Women
Main specification	-0.0026^{***}	-0.0013^{*}	-0.0020^{***}	-0.0003
	(0.0004)	(0.0007)	(0.0003)	(0.0004)
(a) Omit state trends	-0.0024^{***}	-0.0014^{**}	-0.0023^{***}	-0.0002
	(0.0005)	(0.0007)	(0.0003)	(0.0005)
(b) Alternate instrument	-0.0025^{***}	-0.0019^{**}	-0.0021^{***}	-0.0003
	(0.0005)	(0.0008)	(0.0003)	(0.0005)
(c) Month in sample 1-4 only	-0.0025^{***}	-0.0022^{**}	-0.0019^{***}	-0.0005
	(0.0007)	(0.0008)	(0.0004)	(0.0006)
(d) Month in sample 5-8 only	-0.0023^{***}	-0.0004	-0.0018^{***}	-0.0002
	(0.0006)	(0.0010)	(0.0005)	(0.0005)
(e) Include proxy responses	-0.0029^{***}	-0.0015^{**}	-0.0018^{***}	-0.0005^{*}
	(0.0003)	(0.0006)	(0.0002)	(0.0003)
(f) First-difference	-0.0008	-0.0005	-0.0003	-0.0008
	(0.0005)	(0.0008)	(0.0004)	(0.0005)

Notes: $^{***}\ p < 0.01$, $^{**}\ p < 0.05$, $^{*}\ p < 0.1$. Dependent variables are binary indicators of self-assessed health, as indicated. Unemployment duration is measured in years. Standard errors account for clustering at the state level. Controls: (1) quadratic in age; (2) educational attainment indicators; (3) race indicators; (4) whether Hispanic; (5) indicators for marital status interacted with the employment status of the respondent's spouse or unmarried partner, if present; (6) state employment-population ratio; (7) real state personal income per capita; and (8) state fixed effects and state-specific trends. Sample sizes for the first three rows are $N_{\text{male}} = 9621$ and $N_{\text{female}} = 7181$.

(b) Instrument is average duration of unemployment for the state and year.

(c, d) Only individuals in rotation groups 1-4 or 5-8 are included. $N_{\text{male}} = 4,738$ and 4,523, $N_{\text{female}} = 3,650$ and 3,531.

(e) Both self and proxy health reports used. Dummy variable for self-report included. $N_{\text{male}} = 19,341$, $N_{\text{female}} = 11,129$

(f) First-difference specification using why unemployed-state-year instrument from second year in the ASEC. Proxy responses included to increase sample size. $N_{\text{male}} = 2,796$, $N_{\text{female}} = 1,536$