Who Is Hurt by Procyclical Mortality?

Ryan D. Edwards*

February 19, 2008

Abstract

There is renewed interest in understanding how fluctuations in mortality or health are related to fluctuations in economic conditions. The traditional perspective that economic recessions are bad for health and mortality has been challenged by recent findings that reveal mortality is actually procyclical. But the epidemiology of the phenomenon — traffic accidents, cardiovascular disease, and smoking and drinking — suggests that socioeconomically vulnerable populations might be disproportionately at risk of "working themselves to death" during periods of heightened economic activity. I examine mortality by individual characteristic and cause during the recessions and expansions of the 1980s using the U.S. National Longitudinal Mortality Study. I find that procyclical mortality ranges widely in its incidence. There is weak evidence that vulnerable groups may bear a heavier burden. A highly vulnerable group, disabled workers, experiences strongly countercyclical mortality.

JEL classification: E32, I12, J11

Keywords: Business cycles; Mortality; Health; Unemployment

---

* PRELIMINARY. PLEASE DO NOT QUOTE WITHOUT PERMISSION. Assistant Professor of Economics, Queens College, City University of New York. redwards@qc.cuny.edu. An earlier version of this paper was prepared for the 2006 Annual Meeting of the Population Association of America in Los Angeles. I am grateful to Sherry Glied and brownbag participants at Columbia University's Department of Health Policy and Management, and to Jason Schnittker and PAA conference participants for comments. This work was partially supported by NIH grants T32 AG000244 and T32 HD 07329. This paper uses data supplied by the National Heart, Lung, and Blood Institute, NIH, DHHS from the National Longitudinal Mortality Study. The views expressed in this paper are those of the authors and do not necessarily reflect the views of the National Heart, Lung, and Blood Institute, the Bureau of the Census, or the National Center for Health Statistics.

Background and motivation

We typically assume that the downside of the business cycle is bad, especially for individuals of lesser means. The rise of the modern welfare state earlier in the past century was a direct effort to limit the pain inflicted by economic recessions, if not head them off entirely. Similarly, the traditional perspective on the health impacts of economic fluctuations held that recessions were bad, especially for the vulnerable (Brenner, 1971, 1975, 1979).

But a number of recent contributions have cast doubt on this view, at least when applied to developed countries with functioning social safety nets. Ruhm (2000, 2003, 2006a), Laporte (2004), Neumayer (2004), Tapia Granados (2005), and Gerdtham and Ruhm (2006) all report patterns of *procyclical mortality* or health, or the worsening of mortality during expansions and improvement during recessions, in the U.S. and European countries. As reviewed by Ruhm (2006b,c), this finding has become relatively standard, although the recent literature is not completely of one voice. Gerdtham and Johannesson (2005) find countercyclical mortality in Swedish microdata from 1981–1996, while Joyce and Mocan (1993) report no cyclical patterns in infant health.

A central question is precisely who is vulnerable to procyclical mortality. Most of the previous studies in this area have decomposed the phenomenon by age, sex, sometimes race, and cause of death, which is easily done with national vital statistics. Impacts appear to be relatively uniform over age and sex. A key finding is that external causes, in particular traffic accidents, appear to account for a large amount of procyclical mortality. Cardiovascular disease is another commonly identified culprit, as is cirrhosis of the liver and other types of excesses. But an examination focusing solely on the causes of death is limited and somewhat unsatisfactory for researchers and policymakers hoping to improve public health. It is doubtful whether policies designed with current knowledge could effectively ameliorate procyclical mortality without depressing economic activity, or essentially throwing the baby out with the bathwater (Edwards, 2005). We need to know more about the microeconomics of the phenomenon (Ruhm, 2006b).

Who are the victims of procyclical mortality? Are they spread evenly across socioeconomic classes, or are they concentrated in a particular range? Information on causes of death does not offer a concise story here. Traffic fatalities could be suburban commuters or inner-city pedestrians. Heart attack victims could be the hard-living working poor, high-stress executives, or some combination. Edwards (2005) discusses how in the U.S., health insurance coverage is in one sense countercyclical, rising during recessions and falling during expansions. Medicaid, the public medical insurance program for Americans under age 65, is means-tested, so it is naturally countercyclical. We know that a large portion of those without medical insurance in the U.S. are the working poor (Glied, 2001). Given these facts, a key question is to what extent these working poor may be especially vulnerable during periods of rapid economic growth in the U.S.

In this paper, I examine procyclical mortality in the public file of the U.S. National Longitudinal Mortality Study (NLMS), a large dataset of individuals with time-invariant characteristics and a mortality follow-up. After describing the dataset, I first examine aggregate mortality in the NLMS over time. Then I model mortality at the individual level data and test for differential incidence of procyclical mortality. In the last section I discuss the results.

The National Longitudinal Mortality Study

The NLMS is a dataset of individuals who are interviewed once in a Current Population Survey and then subsequently matched to death certificates via the National Death Index. The result is unique among U.S. datasets: a large-scale panel study of mortality connected to a snapshot of covariates such as education, occupation, employment status, and family income. The NDI identifies time of death down to six-hour groupings, along with cause of death and other data from the death certificate. The NLMS is maintained by the National Heart, Lung, and Blood Institute (NHLBI) of the U.S. Census.

For this study, I examine the public version of the NLMS, which follows 637,162 individuals over 3,288 days, or roughly 9 years, starting from around 1980. During the panel, 42,919 total deaths, or 6.7 percent of the sample, were registered through NDI match. In order to maintain confidentiality, the NHLBI has intermingled CPS cohorts without identifying them, leaving precise calendar dates unclear. Thus we do not know the calendar date of death, nor do we know exactly when the initial CPS interview was conducted. As described by Preston and Elo (1995), the NLMS public file contains individuals from five different CPS cohorts sampled between March 1979 and March 1981, the midpoint of which is March 1980. For this study, which requires matching to annual macroeconomic data, I treat all records as starting from the midpoint, March 1980. This introduces potential error in the precise dating of mortality of as much as a year in either direction, or about twice the amount of error we would expect in studies that use annual data averaged over one year.

A more fundamental shortcoming of the NLMS data is the data universe. The CPS is representative only of the civilian noninstitutionalized population, and we know that mortality is higher among nursing home residents. This source of bias will gradually evaporate over time, since the NLMS will match the death of any CPS respondent who later entered a nursing home. But as revealed by Preston and Elo (1995), the NLMS yields lower 5-year mortality than do vital statistics because of undersampled institutional mortality. I can do little but acknowledge this shortcoming and suggest that its primary impact is likely to be observable at older ages, while its effect on temporal patterns is wholly unknown. There is no extant literature on short-term trends in institutional mortality, which is typically difficult to measure except in the cross section, so I have no means of directly assessing how its omission early in the NLMS panel may bias this study.

Further details regarding the NLMS can be found in Rogot et al. (1988, 1992). To my knowledge, no previous study has examined temporal patterns in the NLMS. I first construct total age and sex-adjusted annual mortality in the NLMS and examine it relative to official vital statistics and to indicators of the business cycle. We are interested in whether these time-averaged NLMS data look like official annual statistics and how they fluctuate with macroecononomic variables.

Aggregated NLMS data

Levels, trends, and volatility in mortality

Panel A in Figure 1 plots age-adjusted mortality rates for individuals of both sexes at age 10 and over in the NLMS and in data from the Human Mortality Database, drawn from national vital statistics. I truncate both series after 1988 due to noise in the NLMS data. The aggregated NLMS mortality series clearly differs from official data in level, trend, and volatility. The lack of institutional coverage at the beginning of the sample probably accounts for both the reduced level overall and sideways trend, since the NLMS will measure some CPS respondents entering nursing homes and dying. As for the annual variation in mortality around its trend, there is more volatility in the NLMS series, with spikes in 1981 and 1984. This may simply reflect the size of the NLMS sample, or it could mean that institutionalized mortality, which is largely omitted, is less volatile over time than noninstitutionalized mortality.

Procyclical mortality

I posit that the business cycle, indexed by the variable BC_t, impacts mortality m_{it} for individual i at time t, which rises exponentially in age, x_{it}:

$$\log m_{it} = \alpha_i + \beta x_{it} + \delta(x_{it})t + \gamma BC_t + \vec{\theta} \cdot \vec{X}_{it} + \varepsilon_{it} \tag{1}$$

where α_i is an individual fixed effect; $\delta(x_{it}) < 0$ is the rate of temporal decline in mortality at age x_{it}; $\vec{X}_{it}$ is a vector of covariates; and ε_{it} is a white-noise error. Lee and Carter (1992) reveal that δ varies strongly with age but has remained relatively stable over time. Further, we can conceptualize x and $\vec{X}$ as remaining fixed over time for the average individual. With these simplifications, differencing equation (1) produces a relationship for the change in aggregate mortality:

$$\Delta \log \overline{m}_t = \overline{\delta} + \gamma \Delta BC_t + \nu_t, \tag{2}$$

where $\overline{m}_t$ is the age and sex-adjusted mortality rate, $\overline{\delta}$ is the percentage decline in mortality averaged over time and age, and ν_t is the difference in average ε_{it}'s, also a white noise error.

Figure 2 plots annual changes in the age and sex-adjusted log mortality rate along the vertical axis against changes in a key business cycle indicator, the civilian unemployment rate. I juxtapose mortality rates for ages 10 and over from the NLMS alongside those provided by the Human Mortality Database, which are based on official vital statistics and Census estimates. Then I estimate equation (2) using ordinary least squares and overlay the trend lines.

The volatility of the NLMS data is clear. The end result is a strengthening of the simple bivariate relationship between mortality and unemployment, as evidenced by the slopes of the trendlines, $\hat{\gamma}$ in equation (2). In the NLMS data, $\hat{\gamma}$ = –0.0115 with a standard error of 0.0090, while in the HMD data, $\hat{\gamma}$ = –0.0048 with a standard error of 0.0035. That is, a one percentage point increase in the unemployment rate during this period is associated with a decrease in the aggregate NLMS mortality rate of 1.2 percent, and a decrease in the total U.S. mortality rate of about 0.5 percent. By comparison, estimates of $\hat{\gamma}$ in the literature typically range between –0.003 to –0.005 (Ruhm, 2003c). Neumayer (2004) finds $\hat{\gamma}$ = –0.011 in German state-level data, which is very close to the aggregate NLMS result here. This suggests that the micro-level results in the next section are also likely to be larger than typically found, an issue I return to later.

NLMS death probabilities, individual characteristics, and the macroeconomy

With data on individual deaths in the NLMS, I can test for procyclical mortality and examine whether and how it may vary across individuals. In my preferred specification, I model the logit transformation of the individual's death probability, q_{it}, which is highly linear in age (Himes et al., 1994):

$$\log\left(\frac{q_{it}}{1-q_{it}}\right) = \alpha_i + \beta x_{it} + \delta t + \gamma BC_t + \vec{\theta} \cdot \vec{X}_i + \varepsilon_{it}. \tag{3}$$

I pool annual NLMS data on exposures and deaths and merge with annual macroeconomic data. Owing to the limitations of the NLMS design, the covariates in $\vec{X}_i$ are fixed for individual *i* over time, since covariates are only measured once in the initial CPS interview. I specify a fixed linear time trend, δ, but I also split the sample into broad age/sex categories so that δ effectively varies by age. I follow Elo and Preston (1996) in focusing on mortality at ages 25–64 and 65–89 separately by sex because of small sample sizes at advanced ages and incomplete education data below age 25. As robustness checks, I model individual random effects, α_i, and I also estimate an alternative specification using the probit rather than the logit.

Modeling the level rather than the change in mortality is advantageous for two reasons. The logit and probit models are widely used and well suited to this task, and most of the covariates in $\vec{X}_i$ do not vary over time, either by construction or due to data limitations. But the level of mortality is nonstationary, as are many business cycle variables. Including the linear time trend δ addresses the first problem, but the properties of BC_t remain important.

The most common choice for BC_t is the civilian unemployment rate, which is stationary over long periods of time. Researchers have also used real GDP, industrial production, and hours worked, which are not. In my short panel, the unemployment rate appears to be nonstationary, as shown in Panel B of Figure 1. But the change in unemployment, shown in Panel C, appears to be better behaved and is tightly correlated (–0.9580) with the percentage change in real GDP. I therefore specify the change in unemployment as my preferred BC_t variable. Gerdtham and Johannesson (2005) find this transformation produces more consistent results in Swedish microdata as well. To check robustness, I also run regressions with either the level of unemployment or the percentage change in GDP.

Results by age and sex

Table 1 presents the results of estimating equation (3) separately for men and women at working ages and in old age. I recover standard errors using the Huber/White/sandwich estimator and clustering across individuals. I use the same array of covariates specified by Elo and Preston (1996), with the addition of the time trend, Hispanic status, and the change in the unemployment rate.

Table 1: about here

The top row of the table reveals procyclical mortality among each age/sex grouping with coefficients on the change in unemployment ranging between $\hat{\gamma}$ = –0.0263 and –0.0418 and significant at the 5 percent level. For working-age males, the coefficient is the largest and also significant at the 1 percent level. Other coefficient estimates and their standard errors are similar to those reported by Elo and Preston. An additional year of age raises the odds of dying by about 8 percent. Being African-American raises the log odds of dying among individuals of working age, but the effect switches sign for elderly males. Hispanic ethnicity is protective for all age/sex groups, but it is not significant for working-age males. Being born outside of the Northeast is associated with lower odds of dying across the board. Education, income, being married, and living in a rural area are all protective. One difference is that there is a significant positive time trend in female mortality above age 65 between 1980 and 1988 in the annual NLMS data, which probably reflects the changing coverage of institutional mortality and greater numbers of elderly women who die in nursing homes.

If instead I use the level of unemployment rather than its change as the BC_t variable, I recover positive but insignificant coefficients at working ages and positive and significant coefficients around 0.04 for elderly men and 0.06 for elderly women (not shown). This sensitivity of results to the form of the business cycle variable probably reflects misspecification bias due to nonstationarity in unemployment. As evidence of this, the time trend is positive and significant for each age/sex group in the levels regressions (not shown), exactly the reverse of what we should see.

Probit regressions (not shown) with the change in unemployment produce essentially identical results. An increase of one percentage point in the unemployment rate is significantly associated with about a 0.01 percentage point decrease in the death probability among working ages and a

0.1 percentage point decrease at ages over 65, or in each case a decrease of about 2 percent in death probabilities. Results were also invariant to modeling individual random effects, either in the logit or the probit, and to dropping the observation for 1984 from the regression.

The coefficient on the change in unemployment declines somewhat with age and is larger for males than for females. Similarly, Tapia Granados (2005) reports gradually decreasing effects through working age that then increase at advanced ages, and a fairly constant male disadvantage. To test the significance of age/sex differences in the procyclical effect, I ran logit regressions with dummy interactions. Likelihood-ratio tests of these results against regressions without interactions produced results that were identical to simple *t*-tests on the dummies themselves. Differences in the procyclical effect, $\hat{\gamma}$, between men and women were significant (*p*-value of 0.002), while those between working and old age were not (*p*-value = 0.164), even after accounting for sex differences.

I briefly explored trends in mortality among teens and young adults in the NLMS. What emerged was noisy and relatively uninteresting: mortality, which is very low for these groups, rises with age, especially for teens, and family income is significantly protective. Although power was low, there was scant evidence of cyclical mortality at ages under 25.

Results by race, SES, employment, and occupation

A key advantage of the NLMS is that it measures many more individual characteristics than age and sex. I can test for differential impacts of procyclical mortality across individual or group characteristics by sequentially interacting covariates with the BC_t variable and testing the significance of these interaction effects. As before, likelihood-ratio tests and *t*-tests produced identical results, so I present the *t*-tests.

Table 2 shows point estimates and standard errors of a wide array of interaction effects, each from a separate logit regression including all the original covariates from Table 1 (not shown) as well as the single interaction effect of interest. The total cyclical effect $\hat{\gamma}$ for the particular subgroup is then the sum of the interaction effect in Table 2 and the coefficient in the top row of Table 1.

Table 2: about here

Point estimates of the interaction effects vary considerably and are sometimes large, but relatively low power hampers inference. The top row of Table 2 explores the interaction of the business cycle variable with race. For males of working age, the interaction with African American is –0.0550, larger than the $\hat{\gamma}$ shown in Table 1. But low power is evident in the large standard error, 0.0344. Although smaller, interaction terms are negative but insignificantly different from zero for African Americans across all age/sex groups. Interactions with Hispanic ethnicity are more noisy and less consistently signed.

The next set of rows show no clear connection between educational attainment and the procyclical effect, again with large standard errors. Only one of the coefficients, for elderly females, is significant, and interactions change sign frequently while progressing from the bottom to the top of the distribution for each age/sex grouping. Similarly, interactions with the income distribution in the next set of rows reveals no simple relationship. Among females, the interaction terms are never significant, while for males, two high-income groups experience opposite effects. Being at the very top of the income distribution is associated with strong countercyclical mortality, while working-age males in the second-highest income group experience amplified procyclical mortality.

Geographic information in the public NLMS dataset is limited. Urban versus rural residence, which we can measure and which could matter for exposure to traffic deaths and air pollution, does not appear to be important for the procyclical effect.

The lower half of Table 2 presents the strongest evidence for differential effects, evident in strong links between employment status and cyclical mortality. Although employment status is not measured over time during the panel and may have changed, significant labor force transitions into or out of disability, retirement, or long-term unemployment, are relatively infrequent. Interactions with employment status should thus provide some insights, although they are admittedly imprecise. For men who reported being employed in the initial CPS interview, the interaction term was –0.10 and significant at the 1 percent level. For women, the employment boost in procyclical mortality is smaller at –0.0637 but still significant at the 5 percent level. These amplifications reverse themselves for ages over 65. Interaction effects are insignificant for labor force participants who were either employed but not working or unemployed at the time of the CPS interview. Housekeeping is weakly associated with a stronger procyclical effect among all four age/sex groups, and with more precision among females, who comprised the majority. Students were such a small share that their interaction terms were extremely noisy. The most stark patterns are evident for individuals who reported being unable to work in 1980, the only marker of health status in the public NLMS data other than death. Mortality for that group, who may be temporarily or permanently disabled, was strongly countercyclical across age and sex, with interaction terms ranging from 0.1561 to 0.5811 and all statistically significant. Although standard errors remain large, we can estimate these interactions with precision because the mortality rate among this subgroup is high, with only 58 percent surviving the full 9 year window of the NLMS, compared to the average survivorship of 0.89. Meanwhile, having been retired at

CPS interview interacts significantly with the business cycle variable for some age/sex groups but reveals no clear pattern. For women, retirement seems to dampen procyclical mortality while amplifying it for men at least at older ages.

The bottom of the table explores how exposure varies across occupation. The top four groups are “white collar” occupations, followed by four “blue collar” categories, two farming categories and finally two categories of service workers. Interactions are significant primarily for working-age males, among whom transport operatives and service workers are estimated to be at greater risk of procyclical mortality. Each group’s interaction term is about –0.10, which represents about a tripling of $\hat{\gamma}$ for that subgroup. That procyclical mortality would be especially high for bus and truck drivers is consistent with findings elsewhere that mortality due to traffic accidents is high during economic expansions.

Results by underlying cause of death

The NLMS codes deaths according to underlying cause of death as reported on the death certificate. I combine deaths classified using the ICD-9 system into 12 functional categories and run separate logit models on each individual cause of death using the full set of covariates listed in Table 1. For brevity, Table 3 lists only the coefficients on the change in unemployment and their standard errors that result from this decomposition, alongside the percentage of total deaths during the panel due to each cause.

Table 3: about here

Virtually all causes appear to vary procyclically in the NLMS data, i.e., with negative coefficients on the change in unemployment, but the reduction in power caused by conditioning on cause hampers inference. What stands out is a broad uniformity in the procyclical effect by cause of death, especially among working-age males. For that group, none of the causes appears to vary countercyclically except the residual category, which is basically acyclical. There is marginally more variation among other age/sex groups, but most causes seem to vary procyclically. Cancer deaths are strongly procyclical for working-age women, with a coefficient of –0.0890. Suicides appear to be only weakly countercyclical in the NLMS data, with a large and significant coefficient for women over 65 but insignificant or even negative for other age groups. Elderly women are also most at risk of procyclical homicides.

If I combine all deaths from infectious disease, which together represent 4.4 percent of all deaths in the sample, I recover a significant coefficient around –0.19 for working age men (not shown). This result echoes the findings of by Tapia Granados, who reports $\hat{\gamma}$ = –0.140 for U.S. deaths due to flu and pneumonia between 1920 and 1996. Accidents and homicides taken together, at about 3.5 percent of all deaths, are also significantly procyclical among working-age men, with a coefficient of about –0.1 (not shown).

Discussion

Data from the National Longitudinal Mortality Study reveals that mortality was procyclical in the U.S. during the 1980s, a period of much macroeconomic volatility. This qualitative result matches up well with the findings of Ruhm (2000, 2003, 2006a), Laporte (2004), Neumayer (2004), Tapia Granados (2005), and Gerdtham and Ruhm (2006), as do my findings concerning the epidemiology of the phenomenon. Employed males of working age are more at risk, especially service workers and transportation operators, and these procyclical deaths are frequently due to infectious disease and accidents.

My results conflict with those of Gerdtham and Johannesson (2005), the only other study that examines mortality trends using individual-level data. They find countercyclical mortality among men and acyclical mortality among women in Sweden. Precisely why my results differ from theirs in unclear. Gerdtham and Johannesson control for fewer covariates that I do, so omitted variables could be biasing their results. The underlying socioeconomic and sociocultural systems are clearly different in the U.S. compared to Sweden, and that may explain some of the divergence.

The size of the procyclical effect that I recover in NLMS data, particularly when measured at the individual level, is considerably larger than other researchers have found in other datasets. Aggregate NLMS data reveal an impact that is equal in size to that found by Neumayer (2004) in German state-level data, $\hat{\gamma} = -0.011$, but which is 2–4 times larger than typically estimated. When I examine individual-level NLMS mortality, I find effects around $\hat{\gamma} = -0.03$, or another 3 times larger. The source of these differences remains unclear and could be a combination of several factors. The 1980s were a period of considerable macroeconomic volatility in the U.S., with two recessions, one short expansion, and one very long expansion, and if the health impacts of business cycles become magnified during periods of rapid swings, the choice of sample period could explain the larger estimates. A second possibility is that the NLMS mortality data are simply more volatile than vital statistics, perhaps due to the smaller sample size or associated with the changing coverage of institutional mortality, if the latter varies differently over the business cycle. Thirdly, since my results focus on ages 25 and over, they will overstate total procyclical mortality to the extent that infant and child mortality is not procyclical, although this effect probably is not large. Finally, it is possible that the data aggregation in other studies may have dampened previous estimates of the true procyclical effect if aggregation combines individuals who experience differential incidence. That is, if incidence were to vary by income, and if between-state variation is small relative to within-state and total variation in income, then procyclical mortality could appear more muted in state-level data.

Unfortunately the evidence concerning differential incidence of procyclical mortality is not strong in the public NLMS file, so it is difficult to assess either this last hypothesis, or indeed, the central question of this paper. As revealed by the large standard errors in Table 2, statistical power is generally too weak to reveal significant differences in the cyclical responses of individual mortality among subgroups in the NLMS, probably because mortality is simply too rare and the sample is not big enough. Based on approximations provided by Hsieh et al. (1998), a sample of around 40 million is required to achieve 90 percent power in a test of a logit coefficient equal to 0.01 when the mortality probability averages 0.003 during working ages.

Still, my results shed some light on the central issue of differential incidence. There is weak evidence that groups with lower socioeconomic status might be relatively more exposed to procyclical mortality than those with higher status. Among African Americans, especially working-age males, the procyclical effect certainly appears to be amplified, although the interaction is statistically insignificant. There is no consistent story for Hispanics, which almost seems fitting given the inversion of traditional socioeconomic disparities in health we often find among Hispanics. But the lack of clear differentials across income or educational gradients is puzzling, as are the bizarre flip-flopping in differentials at the very top of the income distribution among males. One would imagine that the net effect of exposure to potential harm during ups or downs of business cycles ought to be monotonically related to socioeconomic status due to its protective effects. But no clear picture emerges.

What is clearer is that employment status is an important channel for procyclical mortality. Having been employed at the time of the CPS interview is associated with significantly higher risk for men and women of working age. Transport operators appear to be particularly exposed to this risk, which probably reflects the important role played by traffic accidents. But workers in other occupations are also subject to procyclical mortality, albeit less acutely, so job-related stress and healthy behaviors that are common to many occupations, such as diet and smoking, are probably also important. Since most individuals work prior to old age, the significance of employment status is consistent with fairly uniform procyclical mortality across socioeconomic groups. But it does not help us understand why individuals over 65, who are probably retired, are also at risk. More generally, the NLMS data do not reveal much of anything about the epidemiology of procyclical mortality at older ages.

The group most at risk during cyclical fluctuations appears to be individuals who were disabled and out of the labor force at the time of the CPS interview, and their plight provides the most interesting counterpoint here. Regardless of age or sex, these disabled workers experienced mortality that was strongly countercyclical, rising during economic bad times and falling during good times, as originally foreseen by Brenner (1971, 1975, 1979) and others. Thus while procyclical mortality appears to be a broad-based risk borne by workers, countercyclical mortality seems to be a relatively concentrated risk acutely felt by a highly vulnerable group.

But why disabled workers and not other dependent groups such as retirees should experience countercyclical mortality is unclear and somewhat puzzling. Federal Disability Insurance is not generous, but like Social Security it is also acyclical. But if a large share of these workers reporting disability were somehow ineligible for Disability Insurance, which is notoriously difficult to qualify for, unemployment could have a stronger negative effect on their economic and physical well-being. Disabled workers are probably the "last hired, first fired" category of workers; when unemployment rises, they suffer immediately, and when it falls, they stand finally to benefit. Disabled workers might also be dependent on the labor income of a spouse. If so, a rise in unemployment that claims the healthy spouses job could simultaneously be good for the now unemployed spouse, through a reduction in stress or exposure to traffic accidents, but bad for the disabled spouse due to a loss of household income.

Many of the mechanisms underlying the results in this paper remain unclear due to the limitations of the NLMS public file. The public dataset contains a large amount of error in temporal variation, which is the key source of identification, and covariates are only measured at the beginning of the panel and thus do not vary over time. Lagged characteristics should only matter to the extent they are stable over time, and it is odd that unchanging variables like race and education do not seem to matter for the intensity of the procyclical effect while income, which changes quite a bit, may matter. The NLMS data also do not reveal much about why mortality varies procyclically for nearly everyone in the sample. Traffic accidents are clearly key, both for transport operatives themselves and for the general public. But cardiovascular deaths are also important, and neither job stress nor psychosocial stress of becoming unemployed can realistically be operative in the cases of retired individuals or the disabled. It is also unclear why mortality should become more acutely procyclical and then abruptly countercyclical for men at the top end of the income distribution, although these results may not prove to be robust.

Further research with an expanded dataset of time-varying covariates could shed light on these and other unanswered questions. Continued investigation into sub-state variation in temporal mortality would provide additional insights into differential incidence. Disaggregating the data further by time, to sub-annual frequencies, may also prove to be a useful exercise if it can increase statistical power.

I would also argue that the lack of a clear picture regarding the socioeconomic gradient in incidence should motivate us, however tentatively, to revisit our basic conceptualization of cyclical mortality as a bad against which SES should be protective. From a welfare perspective, variation in the quantity of a good, here survivorship, of a given size is painful regardless of sign. But procyclical mortality implies negative covariance between unemployment and mortality, both of which are economic bads. If it provides a hedge against unemployment risk, procyclical mortality should therefore be preferred to countercyclical mortality and perhaps even to acyclical mortality. The evidence presented here provides weak support for the view that the mortality of individuals at both ends of the socioeconomic spectrum are relatively more impacted by the business cycle than those in the middle, and perhaps we should expect as much.

References

Brenner, M. H., 1971. Economic changes and heart disease mortality. American Journal of Public Health 61 (3), 606–611.

Brenner, M. H., 1975. Trends in alcohol consumption and associated illnesses: Some effects of economic changes. American Journal of Public Health 65 (12), 1975.

Brenner, M. H., 1979. Mortality and the national economy. The Lancet 314 (8142), 568–573.

Edwards, R. D., 2005. Commentary: Work, well-being, and a new calling for countercyclical policy. International Journal of Epidemiology 34 (6), 1222–1225.

Elo, I. T., & Preston, S. H., 1996. Educational differentials in mortality: United states, 1979–85. Social Science & Medicine 42 (1), 47–57.

Gerdtham, U.-G., & Johannesson, M., 2005. Business cycles and mortality: Results from swedish microdata. Social Science & Medicine 60, 205–218.

Gerdtham, U.-G., & Ruhm, C. J., 2006. Deaths Rise in Good Economic Times: Evidence from the OECD. Economics & Human Biology 4, 298–316.

Glied, S. A., 2001. Challenges and options for increasing the number of americans with health insurance. Inquiry 38 (2), 90–105.

Himes, C. L., Preston, S. H., & Condran, G. A., 1994. A relational model of mortality at older ages in low mortality countries. Population Studies 48 (2), 269–291.

Hsieh, F. Y., Bloch, D. A., & Larsen, M. D., 1998. A Simple Method of Sample Size Calculation for Linear and Logistic Regression. Statistics in Medicine 17(14), 1623–1634.

Human Mortality Database, 2006. University of California, Berkeley (USA) and Max Planck Institute for Demographic Research (Germany). Available at www.mortality.org.

Joyce, T., Mocan, N., 1993. Unemployment and infant health: Time-series evidence from the state of Tennessee. Journal of Human Resources 28 (1), 185–203.

Laporte, A., 2004. Do Economic Cycles Have a Permanent Effect on Population Health? Revisiting the Brenner Hypothesis. Health Economics 13 (8), 767–779.

Lee, R. D., Carter, & L. R., 1992. Modeling and Forecasting U.S. Mortality. Journal of the American Statistical Association 87 (419), 659–671.

Neumayer, E., 2004. Recessions lower (some) mortality rates: Evidence from Germany. Social Science & Medicine 58 (6), 1037–1047.

Preston, S. H., & Elo, I. T., 1995. Are educational differentials in adult mortality increasing in the united states? Journal of Aging and Health 7 (4), 476–496.

Rogot, E., Sorlie, P. D., Johnson, N. J., Glover, C. S., & Treasure, D., 1988. "A mortality study of one million persons by demographic, social, and economic factors: 1979–1981 follow-up," Bethesda, MD: National Institutes of Health; NIH publication 88-2896.

Rogot, E., Sorlie, P. D., Johnson, N. J., & Schmitt, C., 1992. "A mortality study of 1.3 million persons by demographic, social, and economic factors: 1979–1985 follow-up," Bethesda, MD: National Institutes of Health; NIH publication 92-3297.

Ruhm, C. J., 2000. Are Recessions Good for Your Health? Quarterly Journal of Economics 115 (2), 617–650.

Ruhm, C. J., 2003. Good Times Make You Sick. Journal of Health Economics 22 (3), 637–658.

Ruhm, C. J., 2006a. A healthy economy can break your heart. NBER Working Paper 12102.

Ruhm, C. J., 2006b. Macroeconomic conditions, health and government policy. National Poverty Center Working Paper Series 06-26.

Ruhm, C. J., 2006c. Macroeconomic conditions, health and mortality. In: Jones, A. M. (Ed.), Elgar Companion to Health Economics. Cheltenham, UK: Edward Elgar Publishing, pp. 5–16.

Tapia Granados, J. A., 2005. Increasing Mortality During the Expansions of the US Economy, 1900–1996. International Journal of Epidemiology 34 (6), 1194–1202.

Figure 1: Aggregate variables over time

Notes: Panel A shows age-adjusted mortality rates for both sexes combined at ages 10 and over. Data are drawn from the Human Mortality Database and the National Longitudinal Mortality Study (NLMS). Age-adjusted rates are constructed using age-specific mortality rates and the age distribution in 1990 as provided by the HMD. Panel B shows the civilian unemployment rate as provided by the U.S. Bureau of Labor Statistics. Panel C shows the change in the civilian unemployment rate and the percentage change in real GDP as provided by the U.S. Bureau of Economic Analysis.

Figure 2: Changes in log mortality versus changes in unemployment 1980–1988

Notes: This figure plots changes in log age-adjusted mortality rates for both sexes combined at ages 10 and over from two different sources — the National Longitudinal Mortality Study (NLMS), shown in triangles, and the Human Mortality Database, shown in circles — against changes in the civilian unemployment rate provided by the U.S. Bureau of Labor Statistics. Age-adjusted rates are constructed using age-specific mortality rates and the age distribution in 1990 as provided by the HMD. Trendlines are ordinary least squares fits of the each data series to a constant and the change in unemployment. In the NLMS data, the slope is estimated at $\hat{\gamma}$=-0.0115 with a standard error of 0.0090, while in the HMD data, it is estimated at $\hat{\gamma}$=-0.0048 with a standard error of 0.0035.

Table 1. Logit regressions of the annual log odds of dying on the change in the unemployment rate and individual characteristics, individuals aged 25-89

	Males Age 25-64		Females Age 25-64		Males Age 65-89		Females Age 65-89	
Δ Unemployment rate	-0.0418 **	(0.0153)	-0.0381 *	(0.0193)	-0.0277 *	(0.0112)	-0.0263 *	(0.0121)
Time	-0.0054	(0.0068)	0.0089	(0.0088)	0.0016	(0.0049)	0.0308 **	(0.0052)
Age	0.0873 **	(0.0015)	0.0856 **	(0.0021)	0.0750 **	(0.0017)	0.0854 **	(0.0018)
White	---							
African American	0.2349 **	(0.0480)	0.2100 **	(0.0598)	-0.1569 **	(0.0426)	0.0442	(0.0428)
Other race	0.1722	(0.1005)	0.1232	(0.1211)	-0.3610 **	(0.0875)	-0.3073 **	(0.1031)
Hispanic	-0.1359	(0.0816)	-0.5058 **	(0.1154)	-0.4596 **	(0.0755)	-0.3088 **	(0.0842)
Born: Northeast	---							
South	-0.0025	(0.0414)	-0.0051	(0.0537)	0.0240	(0.0314)	-0.0198	(0.0322)
Midwest	-0.0614	(0.0403)	-0.0188	(0.0523)	-0.0036	(0.0290)	-0.0761 *	(0.0305)
West	-0.1184 *	(0.0547)	-0.0286	(0.0700)	-0.1155 **	(0.0430)	-0.0688	(0.0465)
Elsewhere	-0.5142 **	(0.0791)	-0.2976 **	(0.0918)	-0.1977 **	(0.0422)	-0.1637 **	(0.0431)
0-7 years of school	0.1139 *	(0.0538)	0.3251 **	(0.0706)	0.0648	(0.0333)	0.0873 *	(0.0348)
8	0.1785 **	(0.0513)	0.2867 **	(0.0672)	0.1150 **	(0.0313)	0.0748 *	(0.0324)
9-11	0.1429 **	(0.0414)	0.2140 **	(0.0502)	0.0981 **	(0.0329)	0.0581	(0.0334)
12	---							
13-15	-0.0165	(0.0453)	0.0023	(0.0581)	-0.0196	(0.0415)	-0.1100 **	(0.0420)
16+	-0.2872 **	(0.0474)	-0.2323 **	(0.0703)	-0.1767 **	(0.0417)	-0.0724	(0.0464)
Log 1980 family income	-0.3215 **	(0.0194)	-0.2543 **	(0.0254)	-0.1590 **	(0.0145)	-0.0519 **	(0.0151)
Household size (people)	-0.0079	(0.0098)	-0.0245	(0.0134)	0.0501 **	(0.0107)	0.0566 **	(0.0105)
Married	---							
Widowed	0.4133 **	(0.0897)	0.1418 *	(0.0613)	0.1239 **	(0.0324)	0.1208 **	(0.0257)
Divorced	0.3592 **	(0.0474)	0.1459 **	(0.0559)	0.2957 **	(0.0460)	0.2535 **	(0.0497)
Never married	0.4057 **	(0.0485)	0.2149 **	(0.0723)	0.0962 *	(0.0472)	0.1104 *	(0.0489)

Not a metro area		---										
Live in central city	0.1241	**	(0.0367)	0.1863	**	(0.0475)	0.1174	**	(0.0258)	0.0765	**	(0.0266)
Live in metro area	0.0301		(0.0346)	0.1421	**	(0.0444)	0.0272		(0.0251)	0.0705	**	(0.0265)
Constant	-6.3324	**	(0.2093)	-7.6617	**	(0.2755)	-7.1978	**	(0.2011)	-9.7415	**	(0.1983)
N	1,025,616			1,130,961			209,775			294,365		
Chi-square statistic	5,672.12			3,412.75			3,244.39			3,516.04		

Notes: Asterisks denote statistical significance at the 1% (two) and 5% (one). Each column shows the coefficient estimates from a logit regression of the log odds of dying during a calendar year between 1980 and 1988. Standard errors are estimated using the Huber/White/sandwich method and clustering across individuals. The unemployment rate is measured in percentage points and is obtained from the U.S. Bureau of Labor Statistics. All other data are from the National Longitudinal Mortality Study (NLMS) public file. Observations are individuals observed alive and either surviving or dying during each year between 1980 and 1988, where annual observations are pooled. The time variable starts at 0 and indexes years. The default category is a white non-Hispanic individual born in the Northeast with 12 years of education (if 25 or older), who is married and not living in a metro area. Ethnicity (Hispanic) is a separate question from race.

Table 2. Interaction effects from logit regressions of the annual log odds of dying on the change in the unemployment rate and individual characteristics

	Males Age 25-64			Females Age 25-64			Males Age 65-89			Females Age 65-89		
African American	-0.0550		(0.0344)	-0.0426		(0.0424)	-0.0269		(0.0321)	-0.0018		(0.0342)
Hispanic	0.0313		(0.0663)	-0.0794		(0.0935)	-0.0942		(0.0615)	-0.0202		(0.0779)
Educational attainment												
0-7 years	-0.0358		(0.0373)	0.0197		(0.0507)	0.0015		(0.0205)	-0.0027		(0.0234)
8	0.0121		(0.0395)	-0.0315		(0.0497)	-0.0016		(0.0211)	-0.0576	*	(0.0234)
9-11	0.0080		(0.0311)	0.0186		(0.0373)	-0.0064		(0.0234)	0.0171		(0.0253)
12	-0.0235		(0.0257)	-0.0476		(0.0314)	0.0123		(0.0220)	0.0293		(0.0229)
13-15	-0.0044		(0.0360)	0.0502		(0.0474)	-0.0452		(0.0338)	0.0239		(0.0358)
16+	0.0667		(0.0348)	0.0560		(0.0586)	0.0282		(0.0325)	0.0086		(0.0380)
Family income in 1980												
Bottom 10th percentile	0.0266		(0.0364)	-0.0479		(0.0406)	-0.0070		(0.0210)	-0.0325		(0.0202)
10th-25th	0.0429		(0.0345)	-0.0036		(0.0379)	-0.0200		(0.0186)	0.0279		(0.0208)
25th-40th	0.0559		(0.0325)	0.0144		(0.0388)	-0.0189		(0.0220)	-0.0225		(0.0271)
40th-60th	-0.0393		(0.0306)	0.0295		(0.0455)	-0.0088		(0.0314)	-0.0107		(0.0370)
60th-70th	-0.0334		(0.0325)	0.0253		(0.0455)	0.0580		(0.0363)	0.0053		(0.0420)
70th-95th	-0.0759	*	(0.0302)	0.0129		(0.0415)	0.0552		(0.0357)	0.0768		(0.0413)
95th and above	0.1468	*	(0.0658)	-0.0924		(0.1117)	0.1395	*	(0.0651)	0.0395		(0.0845)
Urban residence in 1980	0.0154		(0.0250)	0.0168		(0.0327)	-0.0058		(0.0182)	-0.0159		(0.0210)
Employment status in 1980												
Employed	-0.0999	**	(0.0297)	-0.0637	*	(0.0301)	0.0525	**	(0.0199)	0.0514		(0.0306)
Employed, not working last week	0.0223		(0.0576)	0.1140		(0.0775)	0.0543		(0.0654)	0.1766	*	(0.0824)
Unemployed	0.0445		(0.0534)	-0.0848		(0.0788)	-0.0652		(0.0971)	-0.0185		(0.1395)
Not in the labor force:												
Housekeeping	-0.0686		(0.1860)	-0.0498		(0.0317)	-0.0087		(0.0571)	-0.0899	**	(0.0213)
At school	0.1353		(0.1692)	0.1698		(0.1930)	0.9891		(0.6249)	-0.5246		(0.2983)
Unable to work	0.2611	**	(0.0732)	0.5811	**	(0.1459)	0.1561	*	(0.0625)	0.2211	**	(0.0733)
Other, retired	0.0467		(0.0425)	0.2842	**	(0.0793)	-0.0715	**	(0.0191)	0.0504	*	(0.0257)

Occupation								
Professional/technical	0.0394	(0.0382)	0.0516	(0.0523)	0.0416	(0.0475)	0.0036	(0.0795)
Managers/administrators	-0.0582	(0.0367)	-0.1267	(0.0714)	0.0395	(0.0400)	-0.0195	(0.0870)
Sales workers	-0.0462	(0.0630)	-0.1200	(0.0637)	-0.0325	(0.0636)	0.1018	(0.0698)
Clerical workers	-0.0956	(0.0550)	-0.0075	(0.0385)	-0.0215	(0.0642)	0.1562 **	(0.0509)
Craftmen workers	0.0403	(0.0284)	-0.0046	(0.1198)	0.0532	(0.0392)	0.0171	(0.1733)
Operatives except transport	-0.0280	(0.0358)	-0.0043	(0.0537)	-0.0152	(0.0577)	-0.0037	(0.0758)
Transport operatives	-0.1155 *	(0.0488)	-0.3501 *	(0.1726)	0.0814	(0.0784)	0.3894 **	(0.0543)
Laborers, no farm	0.0425	(0.0489)	0.1066	(0.1221)	0.0288	(0.0652)	0.2121	(0.2480)
Farmers and farm managers	0.0749	(0.0505)	-0.0111	(0.1983)	0.0580	(0.0393)	0.1226	(0.1563)
Farm laborers/managers	0.0034	(0.0796)	0.0119	(0.1103)	0.0172	(0.0713)	0.0332	(0.1873)
Service workers	-0.1057 *	(0.0429)	-0.0484	(0.0432)	0.0005	(0.0454)	-0.0049	(0.0580)
Private household workers	0.0271	(0.0359)	0.0088	(0.0930)	0.5675	(0.3131)	-0.0362	(0.0717)

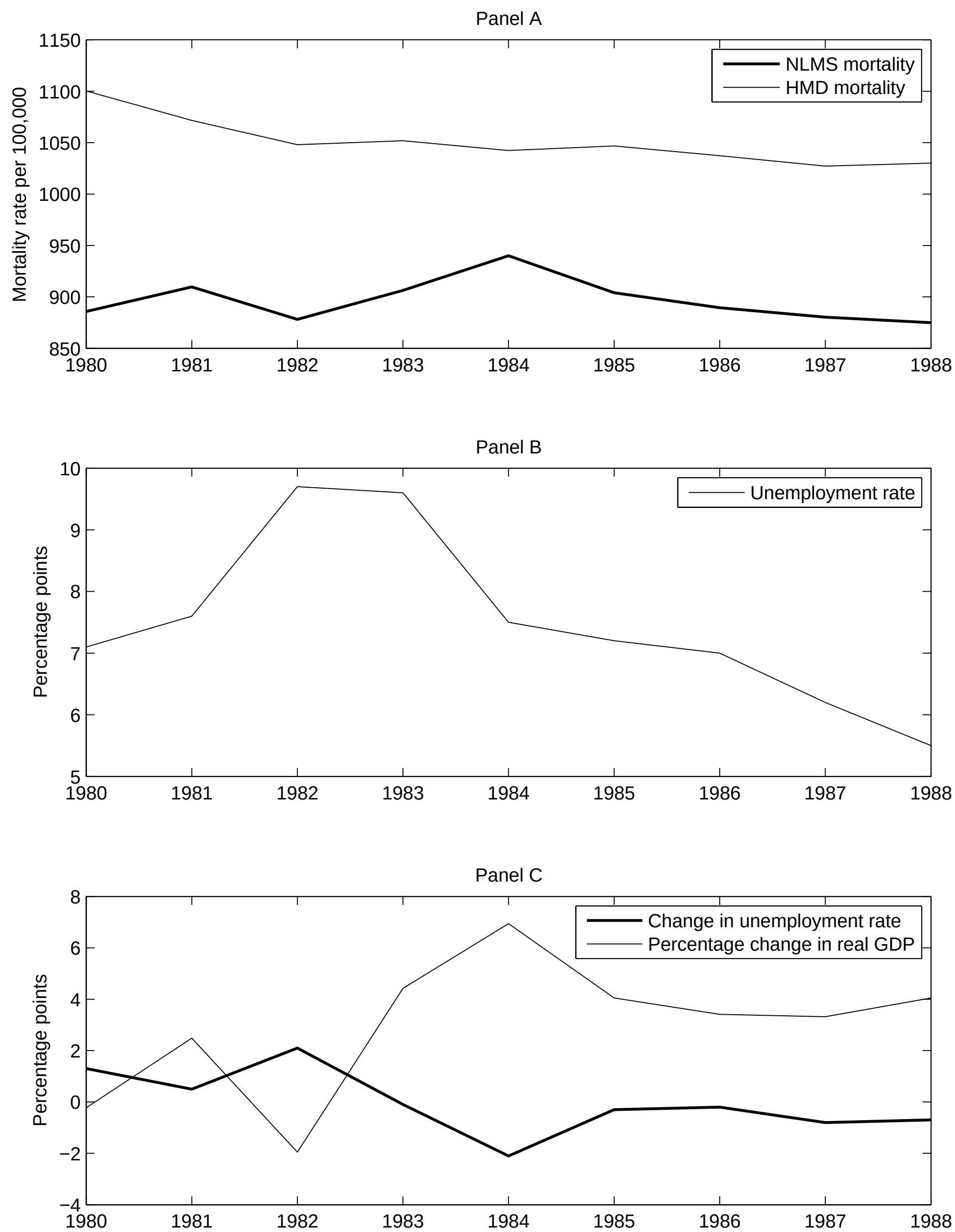
Notes: Asterisks denote statistical significance at the 1% (two) and 5% (one). Each cell depicts an interaction coefficient and its standard error from a separate logit regression of the log odds of dying during a calendar year between 1980 and 1988. Standard errors are estimated using the Huber/White/sandwich method and clustering across individuals. The unemployment rate is measured in percentage points and is obtained from the U.S. Bureau of Labor Statistics. All other data are from the National Longitudinal Mortality Study (NLMS) public file. Observations are individuals observed alive and either surviving or dying during each year between 1980 and 1988, where annual observations are pooled. The time variable starts at 0 and indexes years. The default category in each regression is a white individual born in the Northeast with 12 years of education, who is married and not living in a metro area.

Table 3. Estimates of the coefficient on the change in unemployment from logit regressions of the annual log odds of dying from the listed cause of death on the change in the unemployment rate and individual characteristics

	Share	Males Age 25-64			Females Age 25-64			Males Age 65-89			Females Age 65-89		
Heart disease	39.2%	-0.0288		(0.0260)	0.0725		(0.0413)	-0.0128		(0.0172)	-0.0175		(0.0184)
Malignant neoplasm	25.4%	-0.0244		(0.0303)	-0.0890	**	(0.0289)	-0.0374		(0.0212)	-0.0201		(0.0251)
Cerebrovascular disease	7.1%	-0.0601		(0.0868)	-0.0370		(0.0904)	-0.0345		(0.0424)	-0.0312		(0.0373)
Influenza and pneumonia	2.7%	-0.2051		(0.1343)	-0.0042		(0.1683)	-0.0221		(0.0679)	-0.1173		(0.0739)
Other infectious disease	1.7%	-0.1765		(0.0944)	-0.0537		(0.1309)	-0.0075		(0.0898)	0.0926		(0.0997)
Motor vehicle accidents	1.1%	-0.0801		(0.0738)	-0.0897		(0.1197)	0.1672		(0.2178)	-0.1741		(0.2055)
Other accidents	1.9%	-0.1003		(0.0690)	0.2567		(0.1492)	-0.1109		(0.0863)	-0.0934		(0.0880)
Suicides	1.1%	-0.0611		(0.0741)	-0.1593		(0.1427)	0.0793		(0.1189)	0.3471	*	(0.1739)
Homicides	0.5%	-0.1737		(0.0960)	-0.2201		(0.1863)	0.4996		(0.4782)	-0.2119	*	(0.0856)
Liver cirrhosis	1.4%	-0.0784		(0.0762)	0.0467		(0.1366)	-0.0587		(0.0839)	0.1619		(0.1600)
Other stomach/liver/kidney	1.6%	-0.1923		(0.1601)	-0.0481		(0.1646)	-0.0468		(0.1001)	-0.1293		(0.0758)
Other causes	16.0%	0.0037		(0.0423)	-0.0817		(0.0479)	-0.0456		(0.0263)	-0.0487		(0.0312)

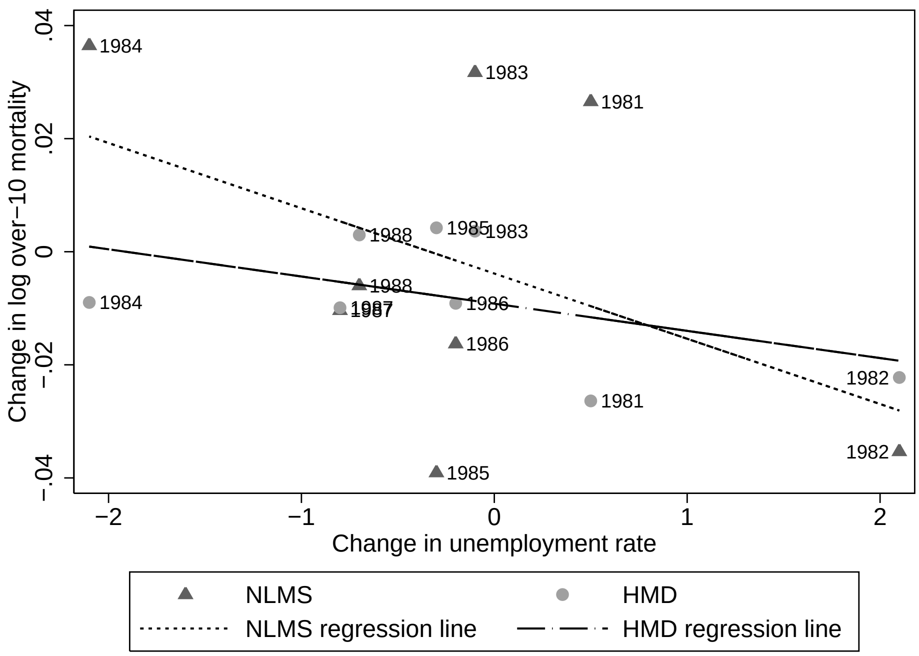
Notes: Asterisks denote statistical significance at the 1% (two) and 5% (one). Each cell shows the estimate on the change in unemployment from a logit regression of the log odds of dying from the cause listed along the row during a calendar year between 1980 and 1988. Each logit regression includes the full set of covariates listed in Table 1. Standard errors are estimated using the Huber/White/sandwich method and clustering across individuals. The unemployment rate is measured in percentage points and is obtained from the U.S. Bureau of Labor Statistics. All other data are from the National Longitudinal Mortality Study (NLMS) public file. Observations are individuals observed alive and either surviving or dying during each year between 1980 and 1988, where annual observations are pooled. The time variable starts at 0 and indexes years. The default category is a white individual born in the Northeast with 12 years of education, who is married and not living in a metro area. Cause of death is coded using these ICD-9 classifications: heart disease, 393-398, 401-405, 410-429; malignant neoplasm, 140-208; cerebrovascular disease, 430-438; influenza and pneumonia, 480-487; other infectious disease, 1-139, 460-466, 490-491, 590, 680-686; motor vehicle accidents, 810-825; other accidents, 800-809, 826-949; suicides, 950-959; homicides, 960-969; liver cirrhosis, 571; other stomach/liver/kidney, 531-533, 535, 555-556, 558, 562, 580-589; other is a residual.

Figure 1: Aggregate variables over time



Notes: Panel A shows age-adjusted mortality rates for both sexes combined at ages 10 and over. Data are drawn from the Human Mortality Database (2006) and the National Longitudinal Mortality Study (NLMS). Age-adjusted rates are constructed using age-specific mortality rates and the age distribution in 1990 as provided by the HMD. Panel B shows the civilian unemployment rate as provided by the U.S. Bureau of Labor Statistics. Panel C shows the change in the civilian unemployment rate and the percentage change in real GDP as provided by the U.S. Bureau of Economic Analysis.

Figure 2: Changes in log mortality versus changes in unemployment 1980–1988



Notes: This figure plots changes in log age-adjusted mortality rates for both sexes combined at ages 10 and over from two different sources — the National Longitudinal Mortality Study (NLMS), shown in triangles, and the Human Mortality Database (2006), shown in circles — against changes in the civilian unemployment rate provided by the U.S. Bureau of Labor Statistics. Age-adjusted rates are constructed using age-specific mortality rates and the age distribution in 1990 as provided by the HMD. Trendlines are ordinary least squares fits of the each data series to a constant plus GDP growth. In the NLMS data, the slope is estimated at $\hat{\gamma} = -0.0115$ with a standard error of 0.0090, while in the HMD data, it is estimated at $\hat{\gamma} = -0.0048$ with a standard error of 0.0035.