The Best of Times, the Worst of Times:
Understanding Pro-cyclical Mortality

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ABSTRACT

It is well known that mortality rates are pro-cyclical. In this paper, we attempt to understand why. We find little evidence that cyclical changes in individuals' own employment-related behavior drives the relationship; own-group employment rates are not systematically related to own-group mortality. Further, most additional deaths that occur when the economy is strong are among the elderly, particularly elderly women and those residing in nursing homes. We also demonstrate that staffing in nursing homes moves counter-cyclically. These findings suggest that cyclical fluctuations in the quality of health care may be a critical contributor to cyclical movements in mortality.

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I. Introduction

Why do death rates rise when the unemployment rate falls? Pro-cyclical mortality rates in the United States (and elsewhere) are well-documented, but the causes of this association remain poorly understood. The most frequently cited explanation is that the business cycle affects individuals' time use, stress levels, and related health investments through its effects on hours of work. In this paper, we show that pro-cyclical mortality in the United States is *not* driven by changes in individuals' own time use associated with their own employment changes. Instead, we find evidence that alternative mechanisms are at work, including cyclical variation in the quality of health care.

The negative relationship between the unemployment rate and the mortality rate has been documented in a series of influential papers by Christopher Ruhm (2000, 2003, 2005a, 2005b, 2007). A typical estimate suggests that a one-percentage point increase in a state's unemployment rate leads to a 0.54% reduction in that state's mortality rate (Ruhm, 2000). When applied to U.S. mortality counts from 2006, this implies that a one percentage point increase in unemployment would lead to about 13,000 fewer annual deaths. Ruhm's findings are widely cited in the health economics literature and have been echoed in work by Dehejia and Lleras-Muney (2004) who find that infant health outcomes and economic downturns are positively linked. The most common interpretation throughout this literature has been that good economic times have a negative impact on individuals' health because of an increase in work hours, the opportunity cost of time, and resulting changes in individuals' decisions about how to allocate their time. Related studies find that obesity and smoking both exhibit a pro-cyclical pattern, and that diet and exercise also improve when the unemployment rate rises – patterns that are consistent with changes in the value of time associated with work (Ruhm, 2005b).[1] Business cycle induced variation in mortality rates may also be driven by other factors, however, that have not been fully explored.

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[1] Reference to changes in health behaviors as the primary (or only) mechanism behind pro-cyclical mortality is especially common in the news media. See for example, New York Times articles, "Good Economics Times Can Mean Health Risk." May 30, 2005, or "Are Bad Times Healthy?" October 6, 2008.

This paper delves into the mechanisms behind procyclical mortality in the United States. We are particularly interested in separating the effects of changes in individual behavior that result from changes in one's own employment status from the effects of other factors that fluctuate with the unemployment rate. This distinction is important because of differences in the associated policy prescriptions. In addition, understanding the underlying mechanisms will shed light on a well-known empirical puzzle--while mortality rates are pro-cyclical, job loss is known to have negative effects on individuals' health. Sullivan and von Wachter (2009), for example, find that individuals who experience a job loss via a mass-layoff experience a substantial increase in their mortality hazard that lasts over the next 20 years. As Ruhm (2008) notes, the estimated impact of individuals' own job loss can be reconciled with the aggregate patterns only if the aggregate fluctuations in mortality are concentrated among those who do not change employment status. This suggests that the mechanisms driving pro-cyclical mortality are more complex than a simple connection between own-employment and health.

We find strong evidence that this is the case. Using state-year panel data models similar to Ruhm's, we find that own-group labor market indicators are not positively related to that group's mortality, and that cyclical variation in nursing home deaths among those over age 65 – a group with very low labor force attachment—can more than account for the total cyclical variation in mortality. We also find that states in which a higher fraction of the elderly population reside in nursing facilities exhibit more cyclical variation in mortality. Our analyses suggest that nursing home deaths' pivotal contribution may be driven by cyclically induced impediments to staffing in health-care occupations.

In the next section, we describe our data and econometric approach. In section III, we present our results on the cyclicality of mortality, focusing on analyses that are disaggregated by age, gender, and place of death. Our estimates show that any explanation of cyclical mortality must look beyond the working age population and beyond motor vehicle deaths. The next section focuses on deaths among the elderly and identifies the important role played by deaths that occur in nursing homes. We conclude in section IV.

II. Data and Methodology

We begin by estimating a specification that has become standard in the literature. Specifically, our main regression takes the following form:

$$H_{jt} = \alpha_t + X_{jt}\beta + E_{jt}\gamma + S_j + S_j t + \varepsilon_{jt} \quad (1)$$

where H is the natural log of the mortality rate in state *j* and year *t*, E is a measure of the state's economic health (usually the state unemployment rate), X is a vector of demographic controls including the fraction of the population who are: less than five years old, 5 to 17 years old, 18 to 30 years old, greater than 65 years old, high school dropouts, with some college, college graduates, black and Hispanic. The vector of year specific fixed effects, α_t, captures year effects, and the vector of state specific indicator variables, S_j, controls for time-invariant state characteristics. State-specific time trends are also included ($S_j t$). This specification is identical to Ruhm's, and when we use his data we are able to produce nearly identical estimates, which suggest that a one percentage point increase in the unemployment rate is associated with a 0.5 percent decrease in the predicted death rate (see Appendix Table A.1).

To conduct the most thorough analysis possible, our study exploits both new data sources and additional years of data. We pool monthly CPS files to construct state unemployment and employment rates. This means we begin our analysis in 1978, the first year all individual states are identified in the CPS files, but are able to extend it beyond 1998 (the last year in Ruhm's (2007) data set). We also use the CPS to create state-year unemployment measures for specific demographic groups and to create the demographic controls described above. Our main results are based on data from 1978 through 2006.

Our state-year mortality rates are based on death counts from Vital Statistics' micro-record "multiple cause of death" files (numerator), and state by age population counts collected by the National Cancer Institute's Surveillance Epidemiology and End Results (Cancer-SEER) program (denominator). We age adjust these data to create a measure of the mortality rate that holds the age distribution constant over time. We describe our age adjustment in Appendix 1 where we also document how these and other changes affect Ruhm's (2000) results. The age adjustment is important given the longer time period we study, and tends to increase the size of the estimated coefficient on the unemployment rate. In contrast, adding years after the early 1990s tends to diminish the estimated degree of cyclicality. Taken as a whole, however, the modifications that we make have limited impact on the estimated association between macroeconomic fluctuations and health. Our baseline estimate of γ is -0.0033, which suggests that a one percentage point increase in the unemployment rate is associated with a 0.3 percent decrease in the predicted death rate.

III. Why are Recessions Good for your Health?

III.A. Mortality Patterns by Age, Gender and Cause

Having confirmed that mortality is pro-cyclical through the mid 2000s, the major question we explore is *why* the probability of dying increases when economic times are good. As a starting point, Ruhm (2000) proposes four possible mechanisms. First, leisure time declines when the economy improves, making it more costly to undertake health-producing activities that are time-intensive. Second, health may be an input into the production of goods and services. Hazardous working conditions, job related stress and the physical exertion of employment, for example, may all have negative effects on health, and are expected to increase when the economy is expanding. Both of these mechanisms work through changes in individuals' own work hours or opportunity cost of time. A third explanation is that the relationship reflects the impact of external factors that fluctuate with the economy. In particular, when more people are working, roadways are more congested, and this leads to an increase in the probability of being involved in a fatal auto accident. There is a fair amount of direct evidence in support of this mechanism. For example, Evans and Graham (1988) and Ruhm (1996) show that drinking and driving exhibit a pro-cyclical pattern, and Ruhm (2000) shows that motor vehicle fatalities are more sensitive to the business cycle than any other cause of death. Similarly, pollution may vary over the business cycle and contribute to mortality fluctuations.[2] Ruhm's fourth hypothesis is

[2] Chay and Greenstone (1999) have established an important connection between pollution and cyclical mortality. This effect seems to operate primarily through infant mortality, however. Chay, Dobkin and Greenstone (2003) show little evidence that pollution has powerful effects on adult mortality. We argue below that only mechanisms having a substantial effect on mortality at older ages are likely to play a substantive role in explaining the overall relationship between mortality and the business cycle. Nevertheless, the role of pollution (including cyclical changes in pollution) on mortality at older ages deserves further study.

that business cycles affect geographic mobility, which may increase crowding or otherwise bring transition costs that impact mortality.

These hypotheses are not easily reconciled with broad patterns in the data, however. For example, Table 1 shows the estimated relationship between the state mortality rate and unemployment, after dividing our sample into four age by cause-of-death cells. We divide deaths into those due to motor vehicle accidents and those arising from all other causes, and we focus on two age groups: those under age 65 and those age 65 and over. We see that for both age groups, deaths related to motor vehicle accidents are strongly cyclical: coefficient estimates are on the order of 10 times the estimated coefficients in the "all other causes" category. In contrast, non-motor vehicle deaths among the non-elderly---the group most likely to be attached to the labor force---do not exhibit statistically significant cyclical fluctuations. This is one piece of evidence that own-work behavior is unlikely to provide the key explanation.

Given that the distribution of deaths across age (and cause) groups is not uniform, the estimated coefficients do not provide sufficient information to determine which cells drive the overall effect.[3] We investigate this further by noting that the overall coefficient can be decomposed into the weighted sum of the group-specific coefficients, where the weights are the number of deaths occurring in each cell. For each cell in Table 1 we show the annual average number of deaths, and, using the number of deaths as weights, we compute the fraction of the overall cyclicality that can be attributed to each of the four cells.[4] Motor vehicle accidents comprise only about two percent of all deaths, and thus variation in motor vehicle accidents explains only about 18 percent of the total cyclical movement in mortality. Further, the vast majority (73 out of the remaining 82 percent) is driven by cyclical variation among those over 65. This broadly disaggregated analysis does not tell the full story, and obscures important cyclical responses among more narrowly defined subgroups, which we will explore more fully

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[3] This point has been made previously (see Ruhm 2007, for example, focusing on cardiac deaths), but has not been applied to understand and decompose the overall cyclicality of mortality.

[4] Specifically, we calculate four counterfactual weighted averages in which we set each of the four individual coefficients from Table 1 to zero. Each cell on the right side of Table 1 shows the percentage by which the overall cyclical coefficient is reduced when each cell's contribution is set to zero.

below. The key point, however, is that any story with the potential to explain a substantial portion of the overall effect must go beyond motor vehicle deaths and must not apply primarily to the working age population.

Table 2, which presents the contribution of more narrowly defined age and gender groups to the overall coefficient estimate, lends further support to this argument. The first row of Table 2 presents estimates for the population as a whole, and separately for men and women. We find that while a one percentage point increase in the unemployment rate decreases the female mortality rate by 0.4 percent, it decreases the male mortality rate by only 0.25 percent.[5] The remaining rows of Table 2 show comparable estimates for five year age groups by gender,[6]and Figure 1 summarizes the coefficient estimates for each year of age. Like Ruhm, we find that mortality among young adults is more sensitive to the business cycle than mortality among other working age adults. For example, we estimate that a one percentage point increase in the unemployment rate reduces the mortality rate among 20 to 24 year olds by about 2 percent. Since young adults' employment fluctuates more than other workers', this finding at first appears to be consistent with the hypothesis that pro-cyclical declines in health are driven by changes in individuals' own behavior. Closer inspection of Table 2, however, reveals several patterns that are not consistent with such a story. First, while we estimate a large semi-elasticity among 20 to 24 year olds, estimates for individuals between the ages of 25 and 59 are substantially smaller and for many prime-age workers, the point estimates are near zero. Second, some of our biggest coefficient estimates are associated with age groups that are certain not to be working, such as 0-4 year olds. Third, the coefficient estimates among those who are over 65 tend to be more negative than the estimates among those 35 to 64.

[5] This gender difference only emerges when we add the later years of data, however. Appendix Table A.1 shows that when we focus on the years of data analyzed by Ruhm (2000), the estimated impact on men is actually larger than it is for women. This suggests that the factors that are driving the pro-cyclical pattern may be changing over time.

[6] In Table 2, we perform the age-adjustment mentioned above within the five-year age groups, so that even within these relatively narrow age bands we are holding the age distribution constant.

The last three columns in Table 2 show the additional number of deaths generated by a 1 percent decrease in the unemployment rate, using as a base the number of deaths that occurred within each age and gender group in 2006.[7] Echoing the results in Table 1, we see that, even though the coefficient estimates are largest among young people, young people are unlikely to have much impact on the cyclical behavior of fatalities overall because deaths among children and adolescents are rare. As in Table 1, we weight the age-specific coefficients by the number of deaths in each age group and see immediately that most business cycle induced deaths occur among those with relatively weak labor force attachment: fewer than 10% of the additional deaths occur among those between the ages of 25 and 64.[8] In fact, we predict that improvements in the unemployment rate lead to more additional deaths among 0-4 year olds than among 30-50 year olds. In contrast, 70% of the additional deaths from a decline in the unemployment rate are among those *over age 70*. These results strongly suggest that the mechanisms at play must go beyond changes in individuals' own work behavior.

Using the "additional deaths" metric makes clear that cyclical mortality is particularly strong among elderly women. The coefficient estimates are notably larger for elderly women than they are for men in the same age range: women age 65+ account for 55% of the roughly 6700 additional deaths (across all ages and genders) that are predicted to result from a 1 percentage point drop in unemployment. In contrast, only 12% of the additional deaths are

[7] These numbers are derived by taking the age-group-specific coefficients and multiplying them by the number of deaths occurring in that age group in 2006. Note that the overall estimated effect (-.0029) is slightly smaller than in Table 1 (-.0033). The overall effect given at the bottom of Table 2 is based on a weighted average of the age-specific coefficients, where the weights are the age-specific number of deaths across all years in our sample. The difference between the overall estimates in Tables 1 and 2 arises because the estimate in Table 2 is based on a less restrictive set of assumptions, using estimates produced by separate regressions for every age group. In particular, we find that allowing different state-specific trends and fixed-effects for each age group generates some differences in the overall estimate. Table 2 is our preferred specification, primarily because of the greater flexibility it allows.

[8] This calculation is slightly complicated by the fact that some age groups have positive (but always statistically insignificant) coefficient estimates associated with the unemployment rate. Thus, we can calculate the total number of additional deaths across narrow age groups as a gross number (in which cells with positive coefficients are ignored) or as a net number, by simply adding up all the positive and negative predicted deaths. In this example, it matters relatively little: 25 to 64 year olds account for 9 percent of all additional deaths using the net number of deaths, and 11 percent using the gross number. In the remaining calculations in this section, we use the net numbers—adding and subtracting across all age categories.

among working age men and women. This result further emphasizes the likelihood that something other than changes in individuals' own work behavior is generating pro-cyclical fluctuations in mortality.

The overall effect can also be decomposed by cause of death. Miller et al. (2009) have done this using an empirical strategy similar to ours and their analyses provide further evidence against the own-work-behavior hypothesis. Specifically, they document that cyclical deaths are concentrated among the elderly and are driven by cardiovascular deaths, respiratory deaths, degenerative brain diseases, and deaths due to infections. We have expanded on these results and improved the categorization of causes, reducing the number of deaths previously assigned to the "other" category. The new categorization is documented in Appendix 2 and Appendix Table A.2, and new by-age-and-cause estimates are presented in Appendix Table A.3. Our by cause estimates are similar to those reported in Miller et al. (2009).[9] As in that paper, none of the largest "by cause" contributors to cyclicality is obviously related to work or time use, which reinforces the need to look for alternative mechanisms.

One alternative explanation that is consistent with these patterns is that business cycle changes lead to changes in the quantity or quality of purchased health inputs, which in turn affect mortality rates. This process would be external to both individual work-related decisions and outcomes, and should be most salient among the elderly, who are relatively intensive users of health care. The remainder of this paper explores the viability of this potential mechanism, which we conclude may be critical.

III.B Relative Importance of "Own-Group" vs. "Other-Group" Employment Opportunities

In this section we explicitly estimate the importance of own versus other responses to the business cycle by looking at how mortality rates for different subgroups respond to variation in that subgroup's unemployment rate relative to variation in other groups' unemployment rates. If

[9] The classification we report in Appendix 2 results in many fewer deaths classified as "Residual", and so in Table A.3 this is a less important category than in Miller et. al. (2009). Also, Miller et. al (2009) do not provide details of the classification scheme used. We present these details in Appendix 2 and Table A.2. Finally, Miller et. al. (2009) do not report measures of statistical precision for the estimates, and present by-age results for only six major categories. Table A.3 presents results for all causes, and also shows standard errors for the semi-elasticities.

most of the mortality effect is driven by changes in "own" behaviors then a group's own unemployment rate should have the strongest impact on that group's mortality.[10] To investigate this possibility we use the Current Population Survey (CPS) to calculate unemployment and employment rates for broad age/gender groups. We then re-estimate equation (1) separately for men and women who are 25-44 years old, 45-61 years old, and older than 62. By creating broadly defined age groups that correspond to different parts of individuals' working lives, we are able to maintain sample sizes that are large enough to produce precisely estimated state-level labor market measures, while retaining the ability to distinguish whether it is one's own group employment status that matters or that of other groups.

The unemployment rate may not fully capture differences in unemployment among the elderly, so for this part of the analysis, we replace the unemployment rate with the age-group-specific employment-to-population ratio. Thus, our expectation is that the estimated coefficient on the regressor of interest will be positive. To facilitate comparison with earlier results, the first column of Table 3 shows estimated coefficients on the aggregate unemployment rate. The second column substitutes the aggregate employment-to-population ratio, and the third column for each group uses the three age-specific employment-to-population ratios. The estimated coefficient on the own-group employment-to-population ratio is shaded in gray.

Table 3 produces little support for the notion that an individual's own group employment rate is driving cyclical fluctuations in mortality. Few of the own group employment coefficients are statistically significant, and most of the estimates on own group employment that are statistically significant are in the opposite direction of our population estimate.[11] Only for women between the ages of 45 and 61 is the own group employment-to-population ratio significantly positively associated with mortality. Note also that the employment rate among women 45 to 61 years old (a group likely to supply labor as paid caregivers) has a positive, statistically significant effect on

[10] Miller and Paxson (2006) estimate the impact of "relative income" using specifications similar to this, using cross-section and decadal-difference variation in the data.

[11] One potential concern with this exercise is that the lack of substantive "own group" estimates might be driven by classical measurement error when we move from using an overall state unemployment rate to a sub-group specific measure. We are skeptical that measurement error is driving these results, however, because measurement error would not be expected to impact the magnitude of "own group" estimates differently from "other group" estimates. In addition, classical measurement error should not lead to sign changes.

older women's death rates. We will return to this point when we discuss the potential role of the supply of health care providers. We also see that mortality fluctuations among the oldest group (over age 62 in this table) are primarily driven by employment changes among younger individuals. This provides an important insight into our earlier finding that pro-cyclical mortality is mainly driven by the top end of the age distribution. It is not this critical group's own employment status that drives the relationship, but rather the employment status of the younger groups.

We have conducted a similar analysis in which we further disaggregate by race (black or white), and then include age- and gender-specific employment rates both for one's own racial group and the other racial group.[12] Once again, we find that own group mortality is, if anything, negatively correlated with one's own group employment rate (defined now by age, gender, and race). In only one case out of 18 is the estimated coefficient on own group employment positive and statistically significant.

III.C. Place of Death and Health Care Inputs

Given the evidence thus far that 1) mortality fluctuations over the business cycle are concentrated among the elderly, and 2) mortality fluctuations are not driven by "own" labor market opportunities, we devote the rest of our paper to exploring possible mechanisms that do not involve changes in individuals' own employment status or time use. An obvious possibility is that variation in mortality rates is driven by cyclical changes in the quality, quantity or nature of health care inputs that are relatively heavily utilized by those over age 65. Previous studies have found evidence that employment in the health care sector is lower during expansions than during recessions. Goodman (2006), for example, estimates a strong negative correlation between changes in hospital employment and changes in aggregate employment and concludes that "… at

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[12]Because of concern about small cell sizes, we include only blacks and whites in the by-race analyses. The frequency of negative own-group coefficients (though they are typically not statistically different from zero) might be viewed as consistent with studies using individual level data to study the effects of individual job loss on health or mortality. We do not emphasize this point, however both because of the lack of statistical significance, and because most individual level studies are based on empirical models with quite different structures and assumptions about the dynamics of job loss and health effects.

times of peak U.S. hiring, when the labor shortage in hospitals may be particularly intense, hospitals with staffing shortages may face restrictions on the volume of business that can be performed at a particular time." Similarly, it is frequently claimed that nursing homes experience especially severe shortages of nursing aides when the economy is strong. Yamada (2002) notes that during the late 1990's very low unemployment rates exacerbated already severe labor shortages for direct care workers—nursing aides, home health workers, and other paraprofessional caregivers, and cites a study from the state of New York that suggests that between 70 to 90% of home health care agencies and nursing homes indicate shortages of direct care workers. If such shortages become particularly acute during good economic times, then we might expect higher mortality among the elderly to follow.[13]

This possibility is particularly relevant given that cyclical mortality among older women appears to be greater than among older men (Table 2). Women tend to marry older men, and men have a shorter life expectancy than women, so older women are relatively more likely to have a market-based caregiver than are older men, and they are more likely to reside in nursing homes at the end of their lives (Murtaugh, et al. 1990). Older men and older women may therefore be differentially affected by fluctuations in the quality of health care.

All of this suggests that changes in health care inputs over the business cycle might be part of the story. We explore this possibility by comparing mortality patterns in nursing homes to mortality patterns elsewhere, and by comparing estimated business cycle effects across states whose elderly have different types of living arrangements. We also look at how occupations that are typically associated with nursing home care vary with fluctuations in the economy.

III.D.1. Direct Evidence from Vital Statistics Place of Death Data

We begin by examining mortality patterns among individuals living in nursing homes using the place of death information that is provided by the Vital Statistics mortality files after 1983. Death certificates indicate whether the death occurred in a hospital, nursing home,

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[13] Yamada (2000) also refers to a long list of studies that point to macroeconomic conditions as an important factor driving these labor shortages.

residence, or other location. While this is only a weak proxy for where an individual was living prior to death (many nursing home residents will die in a hospital after being transferred there for an illness, for example), information on place of death is readily available in the Vital Statistics files, and so it is a natural place to start.

Unfortunately, the Vital Statistics place of death codes are available only after 1982 and, over the period we analyze, they have experienced two substantial changes. First, in 1989 death certificates were changed such that physicians no longer filled out an open-ended question regarding the deceased individual's place of death. Instead, they began to fill out boxes indicating whether the death occurred in a hospital, residence or nursing home. For years prior to 1989, the categories listed in the Vital Statistics codebook include hospitals (and several subsets), "other institutions providing patient care", and all other reported places. We assume that "other institutions providing care" are primarily nursing homes. Second, in 2003 the categories associated with the boxes were changed slightly. "Nursing home" was replaced by "nursing home/long-term care" and a separate category was added for deaths in a hospice. We have recoded the categories across years into: nursing homes, hospitals, and "other." Appendix Figure A.2 shows the fraction of deaths occurring in each category by year and shows that these changes do not appear to have had an impact in the mortality-by- place-of-death time series. The timing of the coding changes does not appear to be associated with any breaks in any of the series.

Table 4 presents results based on data from the years 1983 -2006 and 1983-2002. Our dependent variable is the age-adjusted mortality rate among those age 65 and over.[14] When we use data covering the longer time period, the estimated impact of the unemployment rate on elderly mortality (-0.002) is similar to our main results. When deaths are divided between nursing homes and non-nursing homes, the estimated coefficient is not statistically significant for either sub-group, although among deaths that occur in nursing homes the estimated coefficient is

[14] We include the second set of regressions to show that the main patterns with respect to place of death are not sensitive to the change in place of death coding starting in 2003. We have also restricted the sample to the years 1989 through 2002 (the period for which there are no changes in place of death coding). For the shorter period 1989 to 2002, however, we find no statistically significant relationship between the unemployment rate and overall mortality, or the unemployment rate and mortality by place of death.

very large (-.03) and negative, which suggests that nursing home deaths may play an important role. When we drop deaths in nursing homes and focus on all other deaths (which comprise nearly 80 percent of all deaths) the estimated coefficient on the unemployment rate changes from negative and significant to positive and not statistically significant. Mortality in nursing homes thus seems to be a critical part of overall cyclicality, which is, again, suggestive of mechanisms that have little to do with work, time use, or health behaviors.

In the next panel we drop the years after 2002, when the second change in the place of death code took place. As noted in Section II, dropping the last four years of observations substantially increases the magnitude of the estimated coefficient for the full sample. The estimate continues to be driven by deaths in nursing homes, where we observe a statistically significant coefficient estimate of -.056. In contrast, among deaths taking place elsewhere, the estimated impact of the unemployment rate is positive, at 0.005 (but not statistically significant). This pattern holds for both men and women. Focusing on this shortened period gives relatively more weight to the late 1990s, when unemployment rates were particularly low and labor shortages within low-skilled health care occupations may have been particularly acute. Nevertheless, in both sample periods, nursing home deaths are associated with an estimated coefficient that is *an order of magnitude* larger than the coefficient that is estimated among deaths taking place elsewhere.

This finding motivates us to break down the estimates in Table 1, where we initially stratified by age and cause of death, even further. Limiting the sample to the years when we can observe place of death (1983 through 2006) we repeat the analyses in Table 1 stratifying further by whether or not the death occurred in a nursing home. This yields a coefficient estimate of -0.032 (0.020) for non-motor vehicle deaths occurring in nursing homes and 0.004 (0.003) for deaths taking place elsewhere. Given the positive and insignificant effect of the unemployment rate on non-elderly mortality outside of nursing homes, and small numbers of deaths in the non-elderly categories, this suggests that *all* of the aggregate coefficient is explained by elderly nursing home deaths.

III.D.2. Interactions with Institutionalized Fraction of the Elderly

Because place of death is not a perfect indicator of where a person was living prior to death, we supplement our Vital Statistics analyses with a different approach that makes use of information on residence in institutional group quarters that is available in the Census. The Census does not report residence in nursing homes specifically, but it does indicate whether individuals are living in group quarters, which typically include military barracks, nursing homes, college dormitories and prisons. For individuals over age 65, it is likely that the vast majority of residents in group quarters are living in nursing homes. The fraction of older individuals who live in group quarters thus provides a reasonable approximation of the fraction of the elderly in each state who reside in nursing homes. If nursing home staffing or quality of care is an important component of cyclical mortality we should expect to see greater cyclicality in states with larger nursing home populations.

To investigate this possibility, we begin by using data from the 1980, 1990, and 2000 Census files to calculate the fraction of individuals over age 65 living in group quarters in each state and Census year.[15] In 1980, between 2 and 4 percent of men, and 5 to 7 percent of women, over age 65 lived in group quarters. The lower panel of Table 5 shows the extent to which these fractions vary across states. Going from the 25th to the 75th percentile of the distribution of (state-level) nursing home residence moves the fraction by one to two percentage points for both men and women.

In the top half of Table 5 we show what happens when we add to our main specification an interaction between the unemployment rate and the state's fraction of women (or men) over age 65 living in group quarters. The dependent variable in these regressions is the log of the age-

[15] The following census document makes clear that nursing homes are included in "group quarters" but retirement homes (without skilled nursing care) are not.
http://www.census.gov/acs/www/Downloads/data_documentation/GroupDefinitions/2010GQ_Definitions.pdf

adjusted mortality rate for women (or men) over age 65. For simplicity, we present results holding the fraction of the elderly living in group quarters constant at its 1980 Census value.[16]

Among women, the interaction between the unemployment rate and the fraction in nursing homes is negative and statistically significant, suggesting that mortality's pro-cyclical nature is stronger in states where larger fractions of the elderly reside in nursing homes. The estimate suggests that increasing the unemployment rate by 1 percentage point decreases mortality rates by 0.4 % in a state whose nursing home population is at the 25th percentile of the distribution and by 0.5% in a state at the 75th percentile. For men, the estimated coefficient on the interaction term is smaller and not statistically significant, though the point estimate is consistent with a much stronger cyclical response in states with more nursing home residents. The estimated magnitudes for women are consistent with the estimates produced by our analyses using place of death from the mortality files. If we calculate an out-of-sample prediction (using the results from Table 5) of the implied coefficient on the unemployment rate for an area in which *all* elderly residents live in nursing homes, we predict that the effect of a one-percent increase in the unemployment rate is approximately -.03 to -.06, a similar magnitude to the estimates in Table 4.

Finally, the interaction between the unemployment rate and the fraction in nursing homes also helps to explain the difference between elderly men and women shown in the lower rows of Table 2. Using the estimated coefficients on the unemployment rate and its interaction with the fraction in nursing homes in Table 5, evaluated at the gender-specific median fraction in nursing homes (also shown in Table 5), implies a total effect of the unemployment rate of -.0046 for women, and -0.0020 for men.

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[16] The choice to use only the 1980 fractions here also differentiates these results from the previous section since it removes time series variation in the likelihood of entering or living in a nursing home from the specification. In theory, the results in Table 5 could be driven either by a greater sensitivity to the business cycle within nursing homes, or by an increase (or compositional change) in the flow into nursing homes associated with an improving business cycle. Focusing on the time-invariant state-level fraction in nursing homes thus provides stronger evidence on the first possibility.

We have also repeated this exercise, using age-adjusted deaths among those ages 0 to 45 as the dependent variable. This specification relates mortality at younger ages to the fraction of elderly in nursing homes. We expect that the interaction terms will be be zero. Unfortunately, the placebo test is uninformative, with wide confidence intervals that contain both zero and meaningfully large effects.[17]

In sum, both our Vital Statistics and Census analyses indicate that mortality fluctuations over the business cycle cannot be explained without focusing on the elderly, particularly elderly persons who reside in nursing homes. This raises further questions about *why* nursing home status influences cyclical variation. Suppose that family members are on the margin between caring for an ailing parent themselves and moving that parent to a nursing home. In good economic times, these family members may be more likely to move a parent into a nursing home as a result of the high demands on, or market value of, their own time. If this is a common response then low unemployment rates should be associated with greater flows into nursing homes, which might in turn increase the number of deaths through crowding, or through changes in the composition of nursing home residents. In other words, the business cycle may simply relocate elderly patients—and potential deaths—into nursing homes. If marginal nursing home entrants are sicker than the average nursing home resident, then the average health of nursing home residents will fall during good economic times, contributing to pro cyclical movement in death rates.

We explore these possibilities using restricted-use data from the Health and Retirement Study (HRS), which allow us to examine how individual transitions into nursing homes are related to the state unemployment rate. Specifically, we use waves 1 through 8 of the HRS data, covering years 1992 through 2006, linked to state identifiers and state unemployment rates, and use a linear probability model to estimate the probability that an individual moves into a nursing home(logit hazard models yield similar results). The model controls for state, year, and single-

[17] The fact that neither interaction is close to statistical significance can support an argument that the placebo test "passes." On the other hand the point estimates for the interaction are larger than those for the 65+ group, which argues that the test "fails." Our interpretation is that the very large standard error estimates make this test uninformative. We note that these large standard error estimates do contrast with those for the 65+ group.

year of age fixed-effects, state-level trends, and the same state-level demographic averages as our main regression models.[18] If caregivers' own-employment and time use are behind the higher degree of cyclicality observed in nursing homes, we would expect to see a negative coefficient on the unemployment rate, particularly for less healthy subsets of the elderly population.

Table 6 summarizes these results, which do not support the idea that cyclical transitions into nursing homes are part of the story. The first column shows that transitions into nursing homes actually increase when the unemployment rate is high. The estimated coefficient is significantly different from zero. In the remaining columns of the table we repeat this exercise for groups that may be considered less healthy: those with more than 2 diagnosed chronic conditions; those who could be considered either "frail elderly" due to a low BMI, or obese, and those reporting their health as fair or poor. None of these groups are more likely to enter nursing homes when economic times are good. If anything, transitions among less healthy groups exhibit even more counter-cyclicality than the overall population. This could be consistent with the work cited above, which suggests that the quality of care in nursing homes deteriorates when the economy is strong, which might discourage individuals or their caregivers from entering nursing homes during such periods.[19] In the final section, we present evidence confirming that changes in nursing home staffing levels over the business cycle are consistent with counter-cyclical variation in the quality of care.

III.D.3. Evidence From Institution-level Measures of Health Care Labor Inputs

The patterns in our data could be generated by difficulties that nursing homes face with respect to hiring and retaining paid caregivers when the economy is strong. We are unable to look directly at how the *quality* of nursing home staff changes with the business cycle, but data from the Online Survey Certification and Reporting Database (OSCAR) allow us to estimate how the *number* of hospital and nursing home workers changes with the unemployment rate.

[18] Given our finding that deaths in nursing homes are important, the HRS is not the ideal dataset to employ in our initial analyses. This is because the HRS initially samples only the *non-institutionalized* population ages 50 to 62. Over time, however, the HRS tracks transitions into nursing homes, making it well suited to this analysis.

[19] It is also possible, of course, that nursing homes themselves limit new entrants during periods in which they are constrained by staff shortages.

OSCAR includes data on any institutional healthcare provider that is certified to provide services under Medicaid or Medicare. The dataset covers 97% of all hospital facilities in the US, and contains detailed information on staffing levels. OSCAR's standard analytical files are available from 1991 through 2007. From 1992 forward, OSCAR's Hospital Service Area file includes information on total patient caseloads and days of care, for several types of facilities, including skilled nursing facilities.

We use these data to examine changes in skilled nursing facility staffing levels over the business cycle. Table 7 shows estimates of the relationship between the log of employment (or occupation-specific employment, including physicians, registered nurses and licensed practical nurses, certified aides, and "other") in skilled nursing facilities and the unemployment rate. The estimates are produced by regressions that include either state-level fixed effects or provider-level fixed effects, along with state-specific trends. The regressions are weighted by the provider size, or by the total number of beds.[20]

We find that staffing levels in nursing homes rise during periods of high unemployment.[21] A one percentage point increase in the unemployment rate raises total full-time employment at skilled nursing facilities by approximately three percent. There is no statistically significant increase in the number of physicians, but there are significant increases in nurses, certified aides, and other occupations. Because physicians are an extremely small part of total employment in nursing homes, the non-MD categories all rise by approximately the same amount as total employment; around 3% for a one percentage point increase in the unemployment rate.

While these estimated effects are statistically significant, their substantive importance is less clear. In order to gauge this, we combine our estimates with estimates of the relationship between nursing home staffing and residential outcomes produced by two quasi-experimental

[20] Unfortunately, the files with staffing levels in skilled nursing facilities do not contain patient counts; patient count levels are provided only for the hospital files.

[21] We have conducted a similar exercise for hospitals, but find little evidence that changes in hospital staffing levels are related to the business cycle.

studies. Tong (2011) uses a regulatory change in California to examine the relationship between nursing staff and nursing home mortality, and finds that the increase in certified nurse assistants (CNAs) that resulted from the new staffing regulation was 0.26 hours per resident-day, and that this was accompanied by a decrease in patient mortality of 4.6%. If we interpret this reduction in mortality as coming solely through the CNA channel, this implies an elasticity of -0.35.[22] When this mortality-staffing elasticity is combined with the estimated staffing-business cycle elasticity above (approximately equal to 3), we predict that a 1 percentage point increase in the unemployment rate will lead to a 1 percent reduction in mortality. Thus, approximately a third of the total mortality effect could be due to the counter-cyclical nature of nursing home staffing.

Extrapolating from a study by Konetzka et. al. (2008), who use a facility fixed effects model to estimate the impact of nurse and nurse assistant staffing on the incidence of bed sores and urinary tract infections, produces even higher estimates. Their elasticity estimates are -1.3 (bed sores) and -0.9 (urinary tract infections), and these estimates become larger when they instrument for staffing levels with a variable that captures the introduction of a prospective payment system. If we take Konetzka et. al.'s elasticity estimates as rough proxies for the impact of staffing on mortality rates, and combine them with our estimates of the relationship between staffing and unemployment rates, the resulting estimate suggests that the entire correlation between mortality and unemployment could be driven by cyclical fluctuations in the number of nursing staff. It is also possible, of course, that the elasticity of mortality in this setting could be quite different than Konetzka's estimates for bedsores and infections. Nevertheless, existing evidence suggests that the magnitude of cyclical changes in nursing home staffing that we find could play a substantively important role in explaining cyclical movements in mortality.

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[22] We acknowledge that other factors may have contributed to the impact of the policy change. For example, there was a concurrent change in federal regulations that could have improved quality. In addition, the regulatory change did not impact CNA hours only. It was also accompanied by a modest decline in RN hours and modest increases in licensed vocational nurse hours. Also, Matsudaira (2011) does not find improvements in other patient outcomes resulting from this reform. Finally, the regulatory change impacted California nursing homes with low baseline levels of staffing, and we do not have information as to the external validity of the estimates with respect to other locations or types of nursing homes.

To supplement our OSCAR analysis we have also used the CPS to construct measures of employment, and employee characteristics, for several health care occupations. Specifically, we construct measures of the fraction of the population in each state-year cell who are MDs, Nurses (which can be further divided into registered nurses (RNs) and licensed practical nurses (LPNs), and Aides (also divided into Health Aides and Nursing Aides).[23] For occupations other than MD, where there may be some flexibility in the level of education required, we also calculate the fraction of each occupation with particular levels of education. Unfortunately, in 2003 there are changes in the occupational coding of some of the key health care occupation categories. As a result, this analysis only includes years 1983 through 2002.

We use these measures to examine how the employment in these occupations fluctuates with the business cycle. Specifically, we regress the fraction of employment in each of these occupations on a set of state and year fixed effects, state-specific trends, and the same set of state-year demographic controls that are included in our main analyses. The results of this exercise, summarized in Table 8, confirm that employment in low-skilled health occupations moves counter-cyclically. Each row of Table 8 shows results from a separate regression in which the dependent variable is the fraction of a state's employment in a given occupation. This dependent variable is different from that used in our OSCAR analyses because it captures employment in the given occupation across all types of employers (not only nursing homes). Also unlike our OSCAR analyses, occupational categorization in the CPS is determined by workers rather than employers. The CPS regressions are also based on a different set of years (1983-2002 instead of 1990-2006).

The first rows of Table 8 show results for the fraction employed as doctors and nurses. The estimates provide evidence that employment in these more skilled occupations is pro-cyclical. In the lower half of the table we show results for health and nursing aides, and then all aides (the sum of the previous two rows). We find evidence that nursing aides exhibit counter-cyclical patterns; for all aides combined and nursing aides, a higher unemployment rate is

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[23] Specifically, our category of nursing aides corresponds to CPS occupation 447 (prior to 2003), "nursing aides, orderlies, and attendants" and health aides corresponds to CPS occupation 446 "health aides, except nursing."

associated with a statistically significant increase in employment. A one percentage point increase in the unemployment rate raises the fraction of employment in the nursing aide occupation by more than 1 percent. Our finding that aides exhibit countercyclical employment is consistent with our earlier evidence on nursing home deaths, since nursing aides are heavily concentrated in skilled nursing facilities (GAO, 2001).[24]

IV. Conclusion

This study provides strong evidence that mortality's pro-cyclical nature is not driven by changes in health related behaviors that are related to individuals' own labor market opportunities. We motivate our approach by showing that variation over the business cycle is mainly driven by non-motor vehicle related deaths among the elderly. Motor vehicle accidents account for just 18 percent of all cyclically induced deaths, and cyclical variation in non-elderly mortality due to other causes explains a very small fraction of the overall variation. We also estimate effects separately by demographic group and find that own-group labor market indicators are not systematically related to that group's mortality.

These findings motivate us to investigate explanations that are likely to be most salient for the elderly population. We find three pieces of evidence in support of the hypothesis that cyclical changes in the quality of health care contribute to pro-cyclical variation in the elderly mortality rate. First, we show that, relative to other places of death, deaths occurring in nursing homes are particularly responsive to the state unemployment rate. Second, we show that mortality is more pro-cyclical in states with a higher fraction of nursing home residents. We also show that these findings are not explained by additional flows into nursing homes when the economy is strong, a –a possibility that would be consistent with cyclically induced increases in family caregivers' opportunity cost of time. In fact, transitions into nursing homes appear to be negatively related to the state of the economy. Finally, we show that employment levels in skilled nursing facilities show statistically significant declines when the unemployment rate falls, findings that are corroborated when we look at occupation data in the CPS.

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[24] The GAO reports that, in 1999, nearly half of all nursing aides worked in nursing homes, with the remainder split relatively evenly between hospitals and home health care agencies.

Taken together, our analyses suggest that the mechanisms driving pro-cyclical mortality have little to do with individual level behavioral changes in time use over the business cycle. Instead, we provide new evidence that staffing difficulties among relatively low-skilled nursing occupations may be an important focus for efforts to improve the quality of health care.

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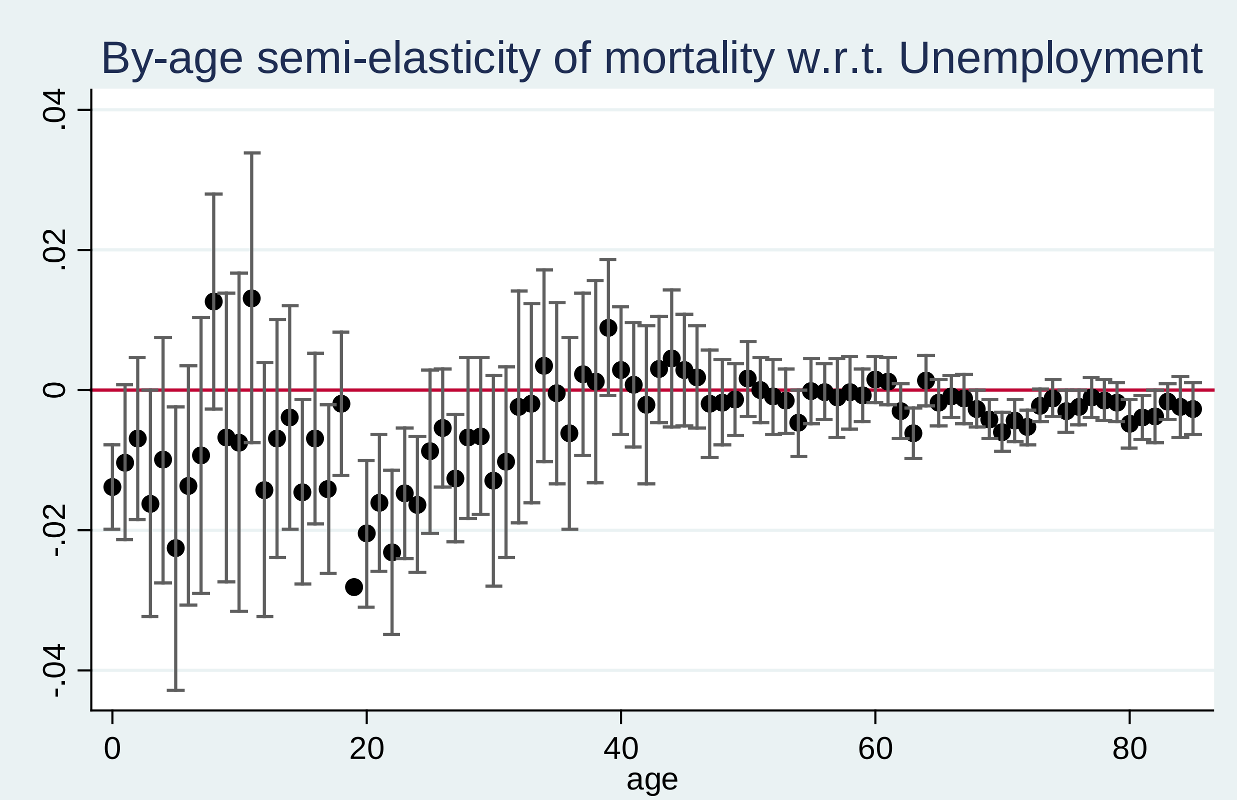
Figure 1



Log-linear models estimated separately for each age. Controls include state + year FEs, state trends, and demographic and education variabels. Bars give 95% CIs. Std. errors clustered by state. For age 19 CI is omitted to preserve vertical scale.

Table 1

Unemployment Rate Coefficients by Broad Age and Cause of Death

1983-2006	Cause/Place of Death		% of overall cyclicality due to cause/age cell	
Age group	MVA	All Other Causes	MVA	All Other Causes
<65	-0.026 ***	-0.001	15.6%	7.7%
	(0.005)	(0.002)		
Mean Deaths per	46,981	702,186		
65 plus	-0.022 ***	-0.003 ***	2.4%	72.8%
	(0.006)	(0.001)		
Deaths per Year	8,412	1,915,572		
			Average deaths per year--total	2,673,151

Parameters are estimated mortality semi-elasticity with respect to the state-year unemployment rate; each cell is a separate regression. Controls include State and Year Fixed effects, State-specific trends, demographic and education controls. Standard errors clustered at the state level. Estimates weighted by state-year population of relevant age group.

*** p < .01
** p < .05
* p < .10

Table 2
Effects of Unemployment on Mortality by Age Coefficients and Additional Deaths

	Coefficients			Additional Deaths from 1% Increase in Unemployment Rate (based on 2006 #s of deaths)		
Age Group	All	Men	Women	All	Men	Women
All ages	-0.003 ***	-0.002 **	-0.004 ***			
	(0.001)	(0.001)	(0.001)			
0 to 4	-0.014 ***	-0.015 ***	-0.012 ***	-464	-278	-176
	(0.003)	(0.003)	(0.003)			
5 to 9	-0.009 *	-0.01 **	-0.008	-25	-16	-9
	(0.005)	(0.005)	(0.009)			
10 to 14	-0.005	0.003	-0.018 *	-17	6	-24
	(0.005)	(0.004)	(0.009)			
15 to 19	-0.014 ***	-0.015 ***	-0.011 **	-192	-149	-42
	(0.004)	(0.005)	(0.005)			
20 to 24	-0.018 ***	-0.018 ***	-0.015 ***	-381	-291	-75
	(0.004)	(0.004)	(0.004)			
25 to 29	-0.008 *	-0.011 **	0.002	-167	-167	11
	(0.004)	(0.005)	(0.004)			
30 to 34	-0.004	-0.008	0.004	-88	-120	28
	(0.006)	(0.007)	(0.006)			
35 to 39	0.001	0.000	0.004	31	0	45
	(0.005)	(0.007)	(0.004)			
40 to 44	0.002	0.001	0.003	103	32	59
	(0.004)	(0.005)	(0.003)			
45 to 49	0.000	0.001	-0.002	0	49	-61
	(0.002)	(0.003)	(0.003)			
50 to 54	-0.002	-0.001	-0.003 *	-212	-66	-119
	(0.002)	(0.002)	(0.002)			
55 to 59	-0.001	0.001	-0.002	-133	81	-103
	(0.002)	(0.002)	(0.002)			
60 to 64	-0.001	-0.001	-0.001	-148	-88	-61
	(0.001)	(0.001)	(0.001)			
65 to 69	-0.002 **	-0.002	-0.002 *	-344	-197	-146
	(0.001)	(0.001)	(0.001)			
70 to 74	-0.004 ***	-0.003 **	-0.005 ***	-873	-358	-494
	(0.001)	(0.001)	(0.001)			
75 to 79	-0.002 **	-0.001	-0.003 *	-595	-154	-430
	(0.001)	(0.003)	(0.002)			
80 to 84	-0.003 ***	-0.003 *	-0.004 **	-1109	-519	-786
	(0.001)	(0.001)	(0.001)			
85+	-0.003	0.000	-0.004 **	-2106	0	-1842
	(0.002)	(0.002)	(0.002)			
Sum of deaths				-6720	-2234	-4224
wtd avg	-0.0029	-0.0022	-0.0035			

First 3 columns ("coefficients") report mortality semi-elasticity with respect to the state-year unemployment rate. Controls include State and Year Fixed effects, State-specific trends, demographic and education controls. Standard errors clustered at the state level. Weighted by population. Last row weighted by age-group-number of deaths over the 1978-2006 period. Last three columns compute "averted deaths" by multiplying the coefficient by the # of 2006 deaths to each age group.

*** $p < .01$

** $p < .05$

* $p < .10$

Table 3

Mortality Response by Age and Gender to Group-Specific Employment-Population Rates

All Sexes	Ages 25 to 44			Ages 45 to 61			Ages 62 and over		
Emp/Pop Ages 25-44			-0.0052**			-0.0012			0.0008*
			(0.0024)			(0.0008)			(0.0004)
Emp/Pop Ages 45-61			0.0047***			0.0006			0.0008**
			(0.0017)			(0.0006)			(0.0004)
Emp/Pop Ages 62+			0.0019			0.0008			0.0001
			(0.0014)			(0.0005)			(0.0003)
CPS Overall Emp/Pop		0.0013			-0.0005			0.0013**	
		(0.0025)			(0.0009)			(0.0006)	
BLS Unemployment	-0.0016			-0.0004			-0.0031***		
	(-0.0042)			(0.0014)			-0.0008		
Men									
Emp/Pop Ages 25-44			-0.0003			-0.0011			0.0010***
			(0.0023)			(0.0010)			(0.0004)
Emp/Pop Ages 45-61			0.0017			-0.0001			0.0002
			(0.0024)			(0.0006)			(0.0003)
Emp/Pop Ages 62+			0.0005			0.0003			-0.0001
			(0.0013)			(0.0005)			(0.0002)
CPS Overall Emp/Pop		0.0026			-0.0019*			0.0007*	
		(0.0031)			(0.0011)			(0.0004)	
BLS Unemployment	-0.0036			0.0008			-0.0020***		
	(-0.0051)			(0.0017)			-0.0006		
Women									
Emp/Pop Ages 25-44			-0.0029*			0.0001			0.0001
			(0.0016)			(0.0006)			(0.0004)
Emp/Pop Ages 45-61			0.0013			0.0010**			0.0009**
			(0.0011)			(0.0005)			(0.0004)
Emp/Pop Ages 62+			-0.0001			0.0003			0.0003
			(0.0013)			(0.0007)			(0.0004)
CPS Overall Emp/Pop		-0.0017			0.0016**			0.0016*	
		(0.0021)			(0.0007)			(0.0008)	
BLS Unemployment	0.0032			-0.002			-0.0041***		
	(-0.0032)			(0.0012)			-0.0012		

N = 1479. Shaded coefficients represent own-group employment/population.

The parameters are estimated mortality semi-elasticities with respect to the state-year unemployment rate; each cell is a separate regression. Controls include State and Year Fixed effects, State-specific trends, demographic and education controls. Standard error estimates are clustered at the state level. Estimates weighted by state-year population of relevant age group.

*** p < .01

** p < .05

* p < .10

Table 4

Effect of Unemployment Rate on Log Mortality by Place of Death: Nursing Homes vs. All Other

	1983-2006			1983-2002		
	All	Men	Women	All	Men	Women
All places	-0.002*	-0.002*	-0.002	-0.004***	-0.003***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Nursing Homes	-0.029	-0.028	-0.029	-0.056**	-0.058**	-0.055**
	(0.019)	(0.019)	(0.019)	(0.027)	(0.027)	(0.027)
Not Nursing Homes	0.003	0.001	0.005	0.005	0.004	0.006
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)
N=	1224	1224	1224	1020	1020	1020

The parameter estimates represent Mortality semi-elasticities with respect to the state-year unemployment rate. The second and third rows count only deaths in Nursing Homes and all other places--"Not Nursing Homes". Controls include State and Year Fixed effects, State-specific trends, demographic and education controls. Standard errors clustered at the state level. Estimates weighed by population.

*** p < .01

** p < .05

* p < .10

Table 5
Cyclicality of Mortality by Fraction Living in Nursing Homes

	Deaths Ages 65+ (age-adjusted)	
	women	men
BLS_UER	-0.001	-0.001
	(0.002)	(0.001)
UER X % Over 65 Living in Group Quarters	-0.056**	-0.028
	(0.021)	(0.028)
1980 Census measures of % over 65 in group quarters		
25th percentile state	0.059	0.032
median state	0.065	0.037
75th percentile state	0.070	0.040

Parameter estimates are mortality semi-elasticities with respect to the state-year unemployment rate and its interaction with the state-specific fraction of elderly living in group quarters. Controls include State and Year Fixed effects, State-specific trends, demographic and education controls. Standard errors clustered at the state level. Estimates weighed by population.

*** p < .01
** p < .05
* p < .10

Table 6

Effects of the Unemployment Rate on the Probability of Transitioning to a Nursing Home

(HRS Respondents Aged 65 and Over, 1992-2006)

Prob (Transition to Nursing Home)	Full Sample	> 2 Chronic Conditions	BMI < 23	BMI > 30	Health Fair or Poor
Unemployment Rate Coefficient	0.003**	0.003**	0.006**	0.0004	0.006**
	(0.001)	(0.001)	(0.003)	(0.002)	(0.003)
N	90768	53006	37657	32340	28335

Estimates are produced by linear probability models using data from the Health and Retirement Study, waves 1 through 8. Controls include single year of age dummies, sex, state and year fixed-effects and state-specific trends, and state-level demographic and education controls. Standard error estimates are

*** p < .01

** p < .05

* p < .10

Table 7

Cyclicality of Employment by Occupation: Skilled Nursing Facilities

	Total		Physicians		Nurses		Certified Aides		Other	
UE Rate	0.0316*	0.0375**	0.0138	0.0129	0.0214	0.0256*	0.0270**	0.0337**	0.0277**	0.0358**
	(0.0182)	(0.0182)	(0.0255)	(0.0241)	(0.0140)	(0.0132)	(0.0125)	(0.0135)	(0.0133)	(0.0143)
N=	185779	185779	56477	56477	185482	185482	182577	182577	184808	184808
Facility FE	X		X		X		X		X	
State FE		X		X		X		X		X

Linear regressions, dependent variable is log(employment count). Unit of observation is the facility-year, from OSCAR files, 1990 through 2006. All specifications include year and state fixed effects and state-specific trends. All models weighted by # of beds.

*** p < .01
** p < .05
* p < .10

Table 8

Effects of the Unemployment Rate on the Fraction of the State

Population Employed in Health Occupations

Occupation	Mean Fraction of State in Occupation	Coefficient on Unemployment RateX100
MDs	0.0032	-0.015***
		(0.005)
LPNs	0.0022	-0.007**
		(0.003)
RNs	0.0094	-0.002
		(0.010)
Nursing Aides	0.0093	0.018**
		(0.007)
Health Aides	0.002	-0.003
		(0.004)

Parameter estimates indicate the effect of the state-year unemployment rate on the fraction of state-year employment in the given occupation. Controls include State and Year Fixed effects, State-specific trends, demographic and education controls. Standard error estimates are clustered at the state level. Estimates are weighted by population. Data for fraction in occupations from monthly Current Population Survey, 1983-2002

*** $p < .01$

** $p < .05$

* $p < .10$

Appendix 1 – Details on the Differences between Ruhm's Analysis and Ours

We began by replicating Ruhm's analysis with his own data which he generously shared with us. The basic regression equation takes the following form:

$$H_{jt} = \alpha_t + X_{jt}\beta + E_{jt}\gamma + S_j + S_jT + \varepsilon_{jt} \quad (1)$$

where H is the natural log of the mortality rate in state j and year t, E is a measure of the state's economic health (usually the state unemployment rate), X is a vector of demographic controls including the fraction of the population who are: less than five years old, greater than 65 years old, high school dropouts, with some college, college graduates, black and Hispanic. Most of Ruhm's control variables come from the Census decadal counts and are interpolated in between Census years. The vector of year specific fixed effects, α_t, captures national time effects, and the vector of state specific indicator variables, S_j, controls for time-invariant state characteristics. State-specific time trends are also included. State unemployment rates are taken from unpublished statistics put together by the Bureau of Labor Statistics, and mortality rates come from Vital Statistics publications. Ruhm's analysis is based on data from 1972-1991.

Our replication results are presented in the first column of Table A.1. We present estimates produced by both unweighted regressions and regressions weighted by state-year population,[25] although we find that weighting makes little difference in the magnitude of the estimated coefficient on the state unemployment rate, which is between -0.0054 and -0.0056. The estimates, which are nearly identical to Ruhm's, suggest that a one percentage point increase in the unemployment rate is associated with a 0.5 percent decrease in the predicted death rate.

In order to investigate the potential mechanisms behind pro-cyclical mortality, we exploit both new data sources and additional years of data. The remaining columns of Table A.1 show what happens to the estimated relationship between mortality and unemployment as we

[25] The two choices of weights are motivated by distinct conceptual questions. Using population weights is appropriate to estimate the degree to which economic conditions contribute to overall fluctuations in U.S. mortality. On the other hand, the un-weighted regressions address the impact on a typical state's mortality rate.

systematically make these changes. Column 2 shows what happens when we continue to use Ruhm's data but eliminate years between 1972 and 1977. Ultimately, we want to extend our analysis through 2006 so that we can include more recent business cycles in our analysis, but we do not have a consistent measure of the unemployment rate between 1972 and years beyond 2000. Instead, we pool monthly CPS files to construct employment and unemployment rates by state and demographic group between 1978 and 2006 (estimates for all states prior to 1978 are not available in the CPS). Here, we show that eliminating the first six years of Ruhm's data has little impact on the estimated coefficient; the estimated effect of a one percentage point rise in the unemployment rate continues to be approximately -0.005.

Column 3 shows how the estimates change when we replace Ruhm's mortality rate variable with a "new and improved" measure of the mortality rate whose numerator is based on death counts from Vital Statistics' micro-record "multiple cause of death" files and whose denominator comes from population counts collected by the National Cancer Institute's Surveillance Epidemiology and End Results (Cancer-SEER) program. The "multiple cause of death" data are less aggregated than the Vital Statistics data Ruhm uses, and this will later allow us to construct state-level death counts by narrowly defined age groups. We replace Ruhm's population estimates with the Cancer-SEER population counts because the Cancer-SEER estimates are based on an algorithm that incorporates information from Vital statistics, IRS migration files and the Social Security database. As such, they are likely to be more accurate than population estimates that are interpolated between Census years. Changing the dependent variable reduces the estimated unemployment effect by about 20% (from -0.005 to -0.004), but it continues to be strongly statistically significant. Most of this change is driven by the change in the population denominator.

In the fourth column of Table A.1, we replace Ruhm's unemployment variable with the CPS unemployment rate. We also replace some of Ruhm's control variables, which are interpolated between census years, with state-year measures of the same variables calculated from the CPS data. We add in a richer set of covariates to control of the state's age distribution. These changes have little effect on the estimated unemployment effect when the regressions are weighted, although they do increase the magnitude of the estimate in the unweighted regressions from -.004 to -.005.

Next, we extend the data through 2006 (column 5). We find that adding fifteen years of data cuts the estimated coefficient on the unemployment rate in half: the new coefficient estimate is between -0.002 (weighted) and -0.003 (unweighted), which suggests that the overall effect of the business cycle on mortality may not be as large as previously thought. On the other hand, the smaller coefficient estimates may also result from other important changes that have occurred over the past fifteen years. In particular, there have been remarkable increases in longevity — between 1978 and 2006, for example, the fraction of Americans over age 65 grew from 11 to 12.5 percent. If this shift in the age distribution occurred unevenly across states, then, given the tight correlation between mortality and age, controlling for these shifts could prove to be very important. Indeed, the age structure in different parts of the country has evolved quite differently over this time period. In California, the fraction of individuals over age 65 increased by less than a percentage point, from 10.0 to 10.8 percent, but in Michigan the fraction of residents over age 65 increased by 3 percentage points, from 9.5 to 12.5 percent.

We control for this phenomenon by replacing the dependent variable with the log of an age-adjusted mortality rate. Consider the mortality rate for state j in a given year t, and note that it can be written as the sum of each age-specific mortality rate weighted by the fraction of individuals in each age interval

$$MR_{jt} = \sum_{a=0}^{85+} MR_{ajt} f_{ajt}$$

In order to abstract from within state-year changes in f_{ajt}, we replace the variable with the 1990 nationwide fraction of individuals in each age category, $f_{a\text{-US-1990}}$. This creates a measure of the state-year mortality rate that holds the age distribution constant and is defined only by the state-year cell's relative number of deaths. Figure A.1, which plots our age-adjusted and unadjusted mortality rates over time, suggests the potential importance of this adjustment; because the U.S. population is aging, the unadjusted series appears to be relatively flat, while the age-adjusted series shows a fairly dramatic decline over time.

Replacing the unadjusted mortality variable with an age adjusted mortality rate turns out to have important effects on our estimates. In column 6, the estimated coefficient on the unemployment rate moves back up to -0.0033. State-specific shifts in the age distribution are clearly correlated with state-level unemployment movements over this period. Note that the need to age-adjust our dependent variable is directly related to our inclusion of additional years of data, which creates a longer period over which states' age distributions can evolve differentially. If we age adjust the mortality rate and repeat the analysis only for the years 1978 through 1991, the estimated unemployment coefficient only moves from -0.0038 to -0.0043 (not shown). [26]

Taken as a whole, our changes have a limited impact on the estimated association between macroeconomic fluctuations and health. Consistent with Ruhm's earlier studies, every entry in Table A.1 is negative, statistically significant, and of substantive magnitude. In the main body of the paper we focus on weighted regressions in which the dependent variable is the age adjusted mortality rate.

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[26] In earlier work (Miller, et al., 2009) we estimate the coefficient on the unemployment rate to be approximately -.005. This estimate is based on data ending in 2004, and we verify that it is sensitive to which years are included. In general, we find that the estimated coefficient on the unemployment rate declines as we add additional years of data after 2000.

Appendix Table A.1

Replication of Ruhm (2000) estimates, different specifications and years

year	Ruhm- all 1972-1991	Ruhm - all 78-91	Ruhm rhs Our mortality 78-91	Our rhs Our mortality 78-91	Our rhs Our mortality 78-06	Age-adjusted mortality 78-06
weight by pop						
coeff	-0.0054	-0.0048	-0.0038	-0.004	-0.0019	-0.0033
se	(0.0010)	(0.0010)	(0.0007)	(0.0008)	(0.0010)	(0.0010)
no weights						
coeff	-0.0056	-0.0053	-0.004	-0.0052	-0.0028	-0.0037
se	(0.0009)	(0.0010)	(0.0008)	(0.0010)	(0.0008)	(0.0007)
weight by pop						
women						
coeff				-0.0034	-0.0023	-0.004
se				(0.0009)	(0.0009)	(0.0011)
men						
coeff				-0.044	-0.0013	-0.0024
se				(0.0009)	(0.0011)	(0.0011)
women 65+						
coeff				-0.0043	-0.0023	-0.0041
se				(0.0009)	(0.0010)	(0.0012)
men 65+						
coeff				-0.0035	-0.0003	-0.0018
se				(0.0009)	(0.0008)	(0.0006)

Parameter estimates represent Mortality semi-elasticities with respect to the state-year unemployment rate. Controls include State and Year Fixed effects, State-specific trends, demographic and education controls. Standard errors clustered at the state level. Weighed estimates use state-year population of relevant group. "Ruhm rhs" has control variables as in Ruhm (2000), "Our rhs" has control variables as described in text.

Appendix 2 – Mortality classifications

We grouped causes of death into 13 categories according to codes and recodes published by the Center for Disease Control for each version of the International Classification of Diseases (ICD). More about the ICD can be found at http://en.wikipedia.org/wiki/International Statistical Classification of Diseases and Related Health Problems. We used the ICD-8, ICD-9 and ICD-10, which correspond to the years 1968-1978, 1979-1998 and 1999-2004. The ICD groups reported causes of death into categories called "recodes." Within a given year, there are several different sets of re-codes. For example, the 1990 data include a 272 cause recode, a 72 cause recode and a 61-cause recode. For deaths occurring in 1968-1978 we use the ICD-8 69-cause recode. We use the ICD-9 72-cause recode for deaths occurring in 1979-1998, and the ICD-10 113-cause recode for deaths occurring in 1999-2004. Column 2 of Appendix Table A.2 shows how we assigned each re-code category into our 13 causes of death groups.

There are some causes of death which the CDC recodes as "other and unspecified" but which seemed to fit our categories. For example, in the ICD8 years, code 782.4 was classified as "other" by the 69-cause recode. Further investigation of this recode, however, made clear that this "other" category was related to cardiovascular deaths, so we reclassified all deaths that had been assigned the 782.4 code as cardiovascular deaths. The third column of Appendix Table A.2 shows which "other" recodes in the ICD were reclassified into each of our 13 causes of death groups.

Appendix Table A.2

Classification of ICD Recodes into Cause of Death Categories

	Cause of Death	Recodes	Raw codes reclassified from “other
1	Cardiovascular	ICD8: 310,320,330,350,360,370,380,390,400, 410,430,440,450,460,470,480 ICD9: 320,330,340,360,370,380,390,400,410, 420,440,450,460,470,480,490 ICD10: 55,56,57,59,60,62,63,65,66,67,68,69,70 ,71,73,74,75	ICD8: 7824
2	Cancer	ICD8:150,160,170,180,190,200,210,22 0,230,240 ICD9: 160,170,180,190,200,210,220,230,240, 250 ICD10: 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, 31, 32, 33, 34, 35, 36, 38, 39, 40, 41, 42, 43, 44	
3	Respiratory	ICD8:490,510,520,540,550,560 ICD9: 500,520,530,550,560,570,580 ICD10: 77,78,80,81,83,84,85,86,87,88,89	ICD8: 450, 510, 5112, 512, 513, 514, 5150, 5151, 5159, 517, 518, 5190, 5191, 5192, 5193, 5199 ICD9: 500, 501, 5070, 5109, 5119, 512, 5130, 514, 515, 5168, 5183, 5184, 5185, 5188, 5191, 5198, 7991 ICD10: R092

4	Infections and immune deficiency	ICD8:10,20,40,50,60,70,80,90,100,110,120,130,280 ICD9: 10,20,40,50,60,70,80,90,100,110,120,130,140,290 ICD10: 1,2,3,5,6,7,8,9,10,11,13,14,15,16,17,18,50	ICD8:5990 ICD9: 2791,2793,5990 ICD10: D849,N390
5	Degenerative brain diseases	ICD10: 51,52	ICD8: 2900, 2901, 299, 3049, 3099, 323, 3304, 340, 341, 342, 3439, 3441, 3449, 3451, 3452, 3459, 3471, 3479, 794, 7330 ICD9: 2900, 2901, 2902, 2904, 2949, 2989, 3109, 311, 319, 3239, 3310, 3314, 3319, 3320, 3352, 340, 3419, 7855, 797, 3440, 3451, 3453, 3459, 3481, 3483, 3485, 3489, 3568, 3580 ICD10: F019, F03, F069, F109, F179, F329, F79, G122, G319, G35, G409, G419, G459, G700, G809, G825, G919, R54, R628, G934, M349
6	Kidney and urethra	ICD8:630,640,650 ICD9: 650,660,670,680 ICD10: 98,99,100,101,102	ICD8:465,592,5931,5932,595,5999,792 ICD9: 5920,5939,5996 N289
7	Nutrition-Related	ICD8:250,260,270 ICD9: 260,270,280 ICD10: 45,46,48,49	ICD8:277 ICD9: 2720,2724,2780 ICD10: E668,E669,E780,E785

8	Motor Vehicle Accidents	ICD8:770 ICD9: 800 ICD10: 114	
9	Other Accidents	ICD8:780 ICD9: 810 ICD10: 115,116,118,119,120,121,122,123	
10	Suicides	ICD8:790 ICD9: 820 ICD10: 125,126	
11	Homicides	ICD8:800 ICD9: 830 ICD10: 128,129,130	
12	Other, Unspecified and ill-defined	ICD8:Remainder of 740,750 ICD9: Remainder of 770,780 ICD10: Remainder of 110,111	
13	Misc: Birth defects; diseases of blood/ bone/ gastrointestinal/ metabolic/ autoimmune; drug abuse; and other external	ICD8:570,580,590,600,610,660,680,690,700,720,730,810 ICD9: 590,600,610,620,630,690,710,720,730,750,760,840 ICD10: 90,91,92,94,95,96,103,104,106,107,108,109,132,133,134,135	ICD8: 251, 2730, 274, 276, 279, 2869, 2871, 288, 2910, 3032, 3039, 3310, 4510, 4519, 453, 4549, 4560, 4589, 5301, 5304, 5309, 5340, 535, 5379, 5621, 5630, 5631, 5670, 5679, 5693, 5694, 5699, 570, 5730, 5739, 5760, 5769, 5770, 7123, 7130, 715 ICD9: 2733, 2762, 2765, 2769, 2770, 2773, 2779, 2866, 2869, 2873, 2875, 2880, 2898, 303, 3049, 3050, 3059, 3334, 3591, 4512, 4519, 4560, 4589, 4590, 4599, 5301, 5303, 5305, 5559, 556, 5570, 5571, 5579, 558, 5621, 5672, 5679, 5698, 570, 5722,

	5724, 5728, 5733, 5739, 5761, 5770, 5771, 5789, 7140, 7330, 7373 ICD10: D65, D689, D696, D70, D869, E854, E859, E86, E870, E872, E875, E878, E889, F101, F102, F141, F191, F192, G10, K222, K224, K509, K529, K550, K559, K562, K566, K572, K578, K579, K631, K639, K650, K659, K729, K759, K760, K767, K830, K85, K860, K861, K922, M069, M321, M819, M869

Appendix Table A.3

Effects of Unemployment on Mortality by Age & Cause and Additional Deaths

	Coefficients on Unemployment Rate				Additional Deaths from 1% Increase in Unemployment Rate			
	Age Group				Age Group			
Cause of Death	All	0 to 24	25-64	65 and up	All	0 to 24	25-64	65 and up
cardiovascular	-0.0035	-0.0023	-0.0007	-0.0037	-2444	-6	-94	-2105
	(0.0011)	(0.0042)	(0.0014)	(0.0012)				
cancer	0.0026	0.0017	0.0020	0.0027	1345	5	297	1030
	(0.0006)	(0.0042)	(0.0011)	(0.0007)				
respiratory	-0.0085	-0.0228	-0.0104	-0.0064	-1695	-42	-275	-1086
	(0.0028)	(0.0096)	(0.0036)	(0.0031)				
infections & immune deficiencies	-0.0058	-0.0181	-0.0190	-0.0091	-412	-28	-475	-412
	(0.0061)	(0.0079)	(0.0154)	(0.0029)				
degenerative brain diseases	-0.009	-0.0005	-0.0058	-0.0073	-1432	-1	-60	-1066
	(0.0032)	(0.0122)	(0.0034)	(0.0041)				
kidney	-0.0092	-0.0101	-0.0080	-0.0088	-370	-3	-56	-288
	(0.0045)	(0.0128)	(0.0060)	(0.0044)				
nutrition related	-0.0012	-0.0074	-0.0012	-0.0011	-99	-4	-27	-62
	(0.0043)	(0.0122)	(0.0050)	(0.0048)				
MVA	-0.0254	-0.0275	-0.0254	-0.0220	-1156	-364	-652	-150
	(0.0055)	(0.0052)	(0.0062)	(0.0061)				
other accidents	-0.0083	-0.0163	-0.0033	-0.0091	-581	-136	-124	-228
	(0.0038)	(0.0043)	(0.0067)	(0.0056)				
suicide	0.0137	-0.0005	0.0188	0.0097	438	-2	433	51
	(0.0055)	(0.0074)	(0.0061)	(0.0058)				
homicide	-0.0088	-0.0155	-0.0073	0.0110	-177	-109	-90	9
	(0.0079)	(0.0076)	(0.0090)	(0.0089)				
VS "other"	-0.0176	-0.0484	-0.0297	-0.0049	-1068	-336	-551	-171
	(0.0083)	(0.0064)	(0.0130)	(0.0083)				
Residual	-0.0054	-0.0052	-0.0090	-0.0016	-664	-135	-372	-89
	(0.0031)	(0.0033)	(0.0065)	(0.0021)				

First 3 columns ("coefficients") report mortality semi-elasticity with respect to the state-year unemployment rate. Controls include State and Year Fixed effects, State-specific trends, demographic and education controls. Standard errors clustered at the state level. Weighted by population. Last 3 columns compute "averted deaths" by multiplying the coefficient by the # of 2006 deaths to each age group.

Figure A.1

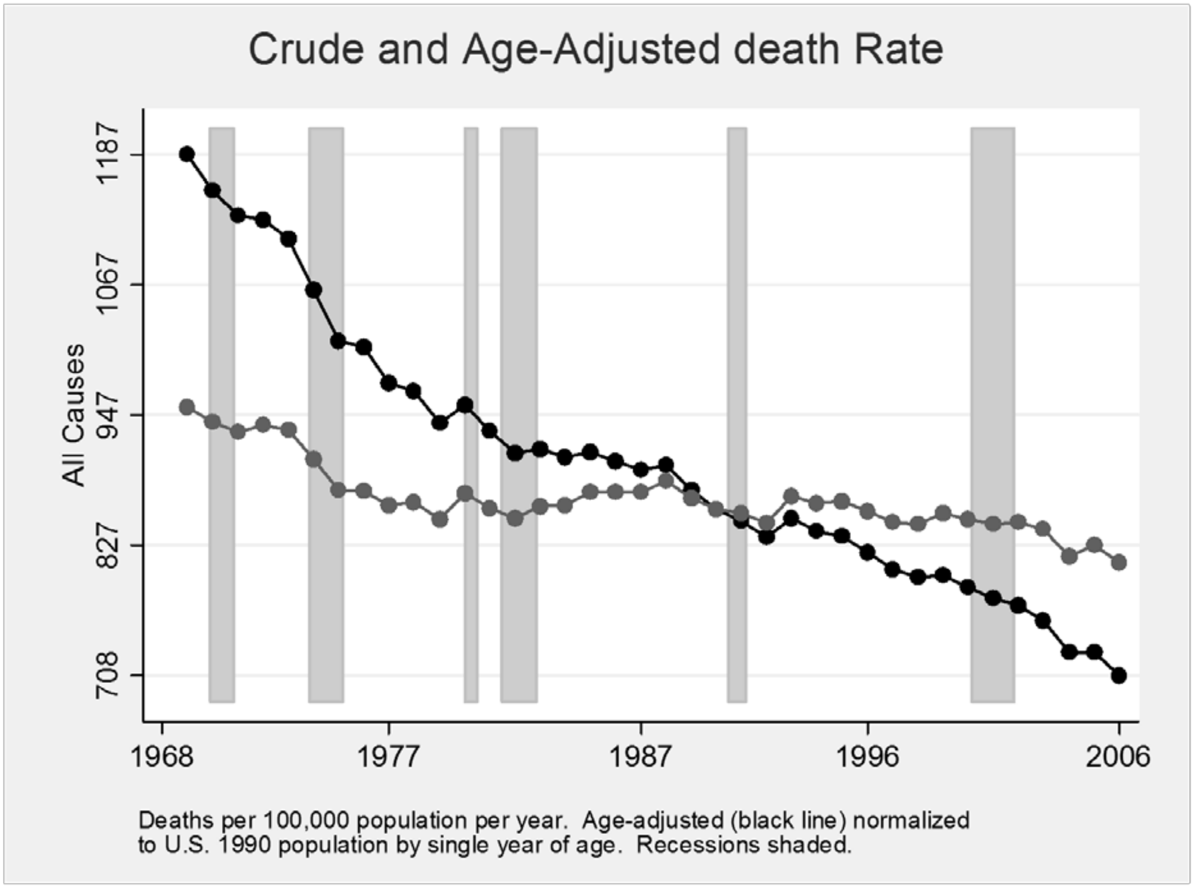
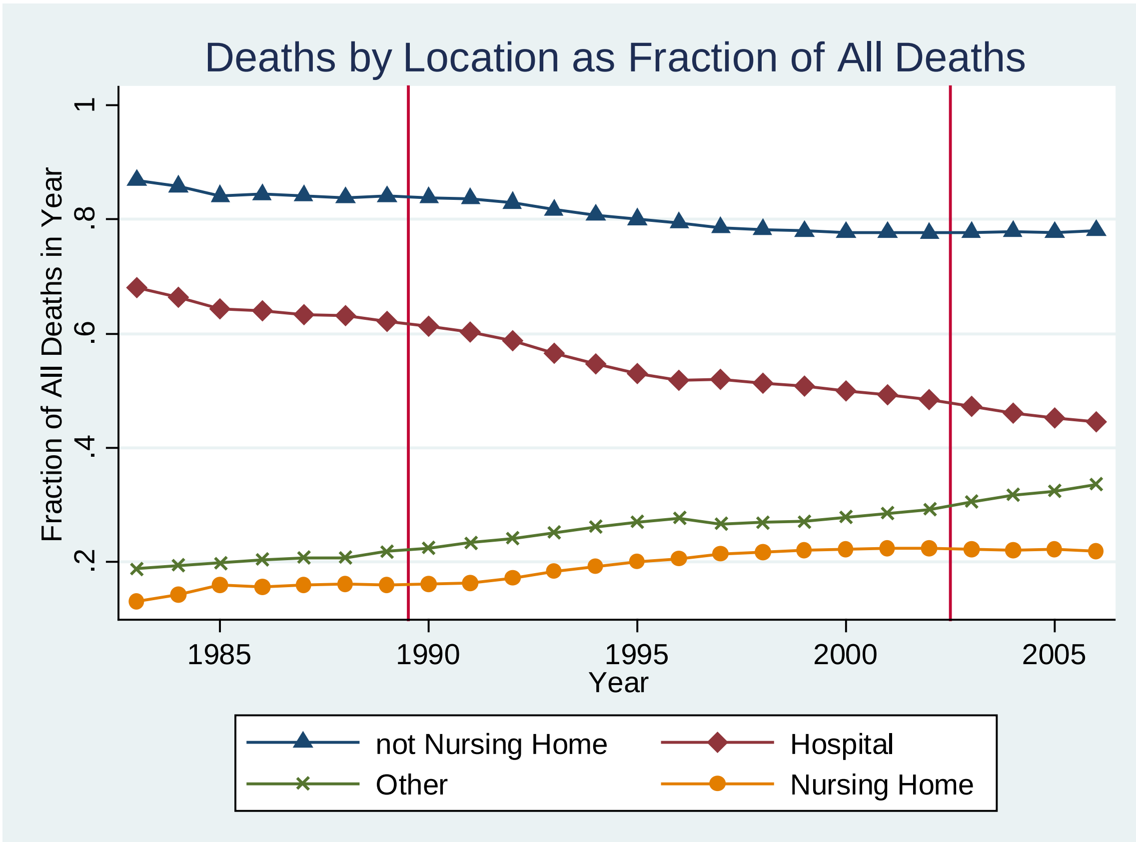

Crude and Age-Adjusted death Rate
All Causes
1187
1067
947
827
708
1968
1977
1987
1996
2006
Deaths per 100,000 population per year. Age-adjusted (black line) normalized
to U.S. 1990 population by single year of age. Recessions shaded.

Figure A.2



Note: Vertical lines at dates of change in death certificate reporting of location.