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ABSTRACT

Social Security eligibility begins at age 62, and approximately one third of Americans immediately claim at that age. We examine whether age 62 is associated with a discontinuous change in aggregate mortality, a key measure of population health. Using mortality data that covers the entire U.S. population and includes exact dates of birth and death, we document a robust two percent increase in male mortality immediately after age 62. The change in female mortality is smaller and imprecisely estimated. Additional analysis suggests that the increase in male mortality is connected to retirement from the labor force and associated lifestyle changes.

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1. Introduction

We examine whether there is a change in aggregate mortality in the United States at age 62. This is when most Americans become eligible for Social Security Retirement Insurance ("Social Security"), the primary source of income support for most seniors. Age 62 therefore represents one of the most important age-related policy thresholds in the U.S., and is associated with major changes in work activity and lifestyle.

Many Americans choose to claim Social Security as soon as possible. Figure 1 shows the fraction of males and females between ages 59 and 67 who have ever claimed any type of Social Security; some claiming occurs before age 62 because the data include those claiming Disability Insurance or Survivors Insurance, the two Social Security programs available at earlier ages.[1] The most striking feature is the large increase in new claims at 62: approximately 31 percent of all Americans begin claiming Social Security in their first month of eligibility, with males and females doing so at similar rates.

Using a regression discontinuity (RD) design, we document a discontinuous increase in the age-mortality relationship at age 62. We use restricted-use versions of the National Center for Health Statistics' Multiple Cause of Death (MCOD) data, which are compiled from all death certificates in the U.S. and include exact dates of birth and death. We show that aggregate mortality increases by 1.5 percent in the month individuals turn 62. This is driven by a two percent increase in male mortality that is statistically significant, robust to a wide variety of specification choices, and substantially greater than at nearby ages with known policy discontinuities (e.g., ages 60 and 65). Importantly, using earlier mortality data, we do not find any increase in mortality at age 62 when it was not the Social Security eligibility threshold. We estimate that there is a one percent increase in mortality for females in the month they turn 62; however; it is neither robust nor larger than mortality estimates at nearby ages.

We interpret these increases in mortality at age 62 as the intent-to-treat effects of Social Security eligibility for those on the margin of retiring once eligible for benefits. Our approach follows other studies using age-based eligibility thresholds to understand the intent-to-treat impact of policies on health outcomes (e.g., Card, Dobkin and Maestas, 2008, 2009; Carpenter and Dobkin, 2009, 2015, 2017; Anderson, Dobkin and Gross, 2012, 2014). The focus on mortality

[1] Figure 1 uses data from a one percent extract of administrative data that covers the full population. Data are for the 1921-1948 birth cohorts. Including all types of Social Security avoids mechanical jumps when claims are converted to Retirement Insurance at the Full Retirement Age. We explain more about these data in the next section and in a data appendix.

means we cannot restrict our analysis to the subpopulation who will retire upon claiming Social Security at age 62, as we do not know the claiming intentions of those who die before age 62.[2]

We examine the changes in retirement and related outcomes at age 62 in order to understand what causes this increase in male mortality. Using Health and Retirement Study (HRS) data, we first show that the increase in claiming leads to an immediate increase in retirement: approximately one-tenth of males retire from the labor force in the month they turn 62. We then document that the demographic subgroups with the largest increases in mortality at age 62 are also the ones with the largest increases in retirement from work at that age. In contrast, the change in mortality is only weakly related to Social Security receipt itself, or changes in health insurance status and household income. If we scale the reduced-form mortality estimate for males by their "first stage" change in male retirement at age 62, we estimate that induced retirement increases their mortality by approximately 20 percent. We cannot rule out similarly sized effects for females, as they experience only a small change in retirement at age 62. Moreover, we cannot rule out that Social Security receipt, income and health insurance contribute to our reduced-form effects, although the evidence suggests their impact is likely to be minor.

We contribute to the literature on retirement and health by documenting a clear link in the U.S. between a widespread retirement incentive and an objective measure of health. Recent European studies using mortality as an outcome have found mixed results: Hallberg, Johansson and Josephson (2015) and Bloemen, Hochguertel and Zweerink (2017) find retirement reduces mortality, Hernaes et al. (2013) find no mortality effects, and Kuhn et al. (2015) find higher mortality after retirement.[3,4] Within the broader literature on how retirement affects health, the most consistent relationship is that subjective health and wellbeing measures are improved by retirement. However, many of these same studies find no effect of retirement on objective health outcomes (e.g., Neuman, 2008; Coe and Zamarro, 2011).[5] Our study provides evidence that, for a large

[2] MCOD files have limited information, and no information on retirement intentions or outcomes. This type of limitation is common in such studies: e.g., Card et al. (2009) face similar issues when examining if mortality changes with Medicare eligibility at 65, as do Carpenter and Dobkin (2009) when examining if mortality changes with legal alcohol access at 21.
[3] Bloemen et al. (2017) find that early retirement decreased the mortality of Dutch public sector workers, while Hallberg et al. (2015) find a similar result for Swedish military officers induced to retire early. Hernaes et al. (2013) find no mortality effects from an early retirement scheme in Norway. Kuhn et al. (2015) examine the effects of unemployment insurance extensions that enabled older blue collar workers in Austria to retire early, finding it led to higher mortality among males.
[4] A related study by Snyder and Evans (2006) examines how income affects mortality using the "Social Security Notch," which reduced payments to beneficiaries born after 1916. The authors find that the lower income led to lower mortality. They argue that increased part-time work is the likely reason for this result, which is consistent with our findings.
[5] Prominent studies include Charles (2004), who uses HRS data and age discontinuities in incentives to find that retirement increases subjective wellbeing, and Coe and Lindeboom (2008), who use the HRS and workers subject to "early retirement

group of workers in the U.S., retirement may have an immediate, negative relationship on an important and objective health outcome. Our findings also contribute to the broader literature on the economic determinants of mortality (Cutler, Deaton and Lleras-Muney, 2006).[6]

It is important to note that the RD design allows us to identify only the immediate, contemporaneous effect of retiring on mortality. Economic theories commonly model health as a stock variable that is both susceptible to sudden shocks and dependent on long-term investments and depreciation (e.g., Grossman 1972; Scholz and Seshardi, 2013; Yogo, 2016). As such, there may be different effects of retirement on health in the short run (driven mostly by shocks) and the long run (affected by the combination of investments, depreciation, and shocks accruing over time). Understanding both is important for optimal retirement policy design, and the former – which our results speak to – may be quite different to the latter.

Our estimates apply to those claiming Social Security upon reaching 62, who differ from later claimants in terms of their socioeconomic and health characteristics. For example, those claiming at 62 are more likely to report being in poor health and retiring due to health-related reasons. They are more likely to report having worked in a job that required physical activity, and less likely to have had a job involving stress much of the time (Gustman and Steinmeier, 2005; Li, Hurd and Loughran, 2008). This may limit the generalizability of our findings to the rest of the population. However, age-62 claimants represent approximately one third of the U.S. population and a higher fraction of Social-Security-covered workers, so understanding their immediate health effects is important in its own right. Age-62 claimants are also the group who would be most directly affected by any changes to the Early Eligibility Age (EEA) of 62, which – unlike the Full Retirement Age (FRA) – has not been changed to increase labor force participation and improve the fiscal outlook of Social Security. If there are also negative mortality effects

windows," finding no detrimental effects on physical health and a temporary increase in self-reported wellbeing. Other studies using the HRS include Neuman (2008), who finds retirement improves subjective wellbeing and has no change on objective outcomes; Dave et al. (2008), who rely on individual fixed effects to conclude that retirement negatively affects mental and physical health; and Insler (2014), who uses retirement expectations as instruments and finds that some ailments decrease with retirement. Studies using European data include Bound and Waidmann (2007), who use public pension rules to examine the effect of retirement on subjective and objective health measures using the English Longitudinal Survey on Aging (ELSA). They find no effects, except some results that suggest that males experience a small and temporary improvement in physical health. Behncke (2012), using pension rules and similar ELSA data to find that retirement increases the probability of being diagnosed with a chronic condition. Coe and Zamarro (2011) use cross-country data from the Survey of Health Aging and Retirement in Europe (SHARE) and variation in retirement rules; they estimate a small positive effect of retirement on self-reported health. In reviewing these and other studies, Eibich (2014) notes that "existing empirical evidence is inconclusive" and that "the mechanisms behind these effects remain unclear" (p. 1).

[6] Interest in mortality determinants is especially strong because the mortality rates of some demographic groups – such as middle-aged white males – have been rising (Case and Deaton, 2015). Our paper does not directly address these findings, but in both cases there is suggestive evidence that declining labor force participation may lead to higher mortality.

of retiring and claiming Social Security in the longer term, then a higher EEA may increase life expectancy, although more research is needed to determine whether this is likely to be the case.

The rest of the paper is organized as follows. In Section 2, we provide background about Social Security and claiming at age 62. In Section 3, we describe the data and sample. In Section 4, we analyze the change in mortality upon reaching age 62. In Section 5, we examine the reasons for the increase in mortality at age 62. We conclude in Section 6.

2. Age 62 and Social Security Eligibility

Social Security is available to workers with ten or more years of covered employment.[7] Social Security claiming at age 62 was introduced for women in 1956 and men in 1961; claiming previously began at age 65. Age 65 then became the FRA, the age at which workers would receive the full "Primary Insurance Amount" (PIA) due under the Social Security earnings-payment formula. Benefits were reduced by 5/9 of one percent for each month before the FRA that an individual began claiming Social Security, so that workers claiming in the month they turn 62 received 80 percent of their PIA. Cohorts born after 1942 have a FRA of 66 and receive 75 percent of their PIA if they claim immediately upon reaching age 62.[8]

Social Security is also available to qualifying workers' dependents and survivors. These are most commonly a worker's spouse, divorced spouse, or widow(er).[9] Dependents' base level of benefits is up to 50 percent of the worker's PIA. Spouses (or former spouses) can receive Social Security benefits at age 62, when they receive 70-75 percent of their PIA-based benefits (depending on their FRA). Widow(er)s can receive benefits from age 60; again, there are reductions for claiming early. For our identification strategy, it is important to emphasize that only the claimant's age matters for benefit collection, not the age of the person upon whose record the benefit is being claimed. Therefore, there is no incentive to remain alive until age 62 so that one's spouse and/or dependents can collect benefits.

Individuals can file an application for with the Social Security Administration (SSA) several months before turning age 62, and become eligible for benefits either in the month they turn

[7] For more details about the policies discussed in this section, see SSA (2012).
[8] Starting with the 1938 cohort, the FRA increased in two month increments, so the 1938 cohort has a FRA of 65 and 2 months, the 1939 cohort has a FRA of 65 and 4 months, etc. There is now an early claiming penalty of 5/9 of one percent per month for the first 36 months of early claiming and 5/12 of one percent for each additional month. There are also "Delayed Retirement Credits" for delaying Social Security claiming up to age 70.
[9] Other dependents include children, adult children disabled before age 22, and parents who are reliant on the worker for income support. These groups are more commonly dependents of Social Security Disability Insurance beneficiaries.

62 or the following month.[10] As shown in Figure 1, many people claim Social Security at the first possible opportunity. These are population-level rates, so the rates among workers with sufficient covered employment and their dependents are even higher. Men and women claim at similar rates at age 62, although women are more likely to claim as dependents and have an average monthly benefit that is 25 percent lower than male age-62 claimants (SSA, 2012).

Social Security beneficiaries are not required to stop working, although they are subject to a "Retirement Earnings Test" until they reach the FRA. It reduces benefits by one dollar for every two dollars of earnings above a threshold, which is currently close to $17,000 per annum. Even though beneficiaries later receive the benefits that are withheld, many beneficiaries appear to view the Retirement Earnings Test as a tax and reduce their labor force participation as a result (Gelber, Jones and Sacks, 2013). Most individuals do substantially decrease their employment after claiming Social Security at age 62, and also report immediate changes in retirement status, income and health insurance (Rust and Phelan, 1997; Gustman and Steinmeier, 2005; Li et al., 2008). We examine those changes in Section 5.

Payments to Social Security beneficiaries cease the month after death. Family members, funeral homes and government agencies report deaths to SSA, and there are limited opportunities to delay reporting. Dependents can receive a lump-sum death benefit of $255 if they live with the beneficiary or receive benefits from the beneficiary's record (SSA, 2012). Any change driven by such incentives should be most pronounced for married decedents. (In Section 4.4, we show that non-married males have a larger change in mortality when they reach 62 than married males.)

Any change in mortality can be attributed to Social Security eligibility only if there are no other concurrent policy rules that affect health. To our knowledge, no other federal programs or pension policies have rules that change discontinuously at age 62. Private sector employers have largely switched to defined contribution pensions that provide little incentive to retire at a specific age (Dushi, Iams, and Lichtenstein, 2011). For the defined benefit programs used in the public sector, pension eligibility is typically based on years of service (Friedberg, 2011). Although there may be some state-level policies with specific rules at age 62 in Section 4.4, we show our mortality estimates do not depend on a particular state or region.

3. Data and Sample Characteristics

[10] Individuals born on the 1st or 2nd of the month are eligible in the month they turn 62, while other individuals are eligible in the month after they turn 62. See Olson (1999) and Evans and Moore (2011) for more details.

We use restricted-use versions of the National Center for Health Statistics' Multiple Cause of Death (MCOD) files for 1979 to 2012. MCOD files are based on death certificates compiled by state registries. They include decedents' dates of birth and death; demographic information, including age, sex, marital status and education (since 1989); and cause and place of death. Sex and race in these data closely match survey data (Sorlie, Rogot and Johnson, 1992), while education levels are slightly higher than reported in other data (Sorlie and Johnson, 1996). Cause of death is based on the International Classification of Disease (Versions 9 and 10).

Dates of birth and death are reported to the exact day. There is no odd heaping in the distributions of these dates, suggesting that any patterns in mortality counts based on age at death should not reflect reporting differences related to data collection. Such heaping in data is important to consider before implementing a RD design (Barreca, Lindo and Waddell, 2016).

There may be a concern that the measurement of mortality in the MCOD files changes with the receipt of Social Security, thereby introducing bias into our analysis. Two reasons suggest this type of measurement error is not present in the MCOD data. First, the Centers for Disease Control and Prevention (CDC) regard MCOD data as having universal coverage because state laws require death certificates for the disposition of bodies and certificates are needed for legal purposes (e.g., estate settlement) (CDC, 1989). Second, the MCOD files are not derived from SSA information; in fact, SSA pays states' vital statistics bureaus – the source of MCOD data – to provide it with death records in order to manage SSA program payments.[11]

In order to have sufficient bandwidth and the opportunity to conduct placebo tests at nearby ages, we focus on individuals born between 1921 and 1948 (i.e., turning 62 between 1983 and 2010). In Table 1, we show the number and composition of deaths in this group at ages 61 and 62 for all decedents, and separately for males and females. There are 65,489 deaths per month at age 61 and 70,727 deaths per month at age 62. Males account for approximately 60 percent of the deaths at both ages, well above their proportion of the population at these ages.

To supplement our main analysis, we use four other datasets. We briefly describe them here; more details are provided in a data appendix. First, we use the HRS to describe the characteristics of older Americans and document retirement-related changes at age 62. The HRS is a

[11] SSA also receives death reports from other sources (e.g., family members, funeral homes). The Social Security Act stipulates that SSA can only share its full mortality data with benefit-paying federal agencies (see 42 U.S.C. § 405(r)(3)). There is no evidence that the Social Security Master Death File, the public file, is used by state vital statistics bureaus or the NCHS. See Hill and Rosenwaike (2002) and the General Accountability Office (2013) for more information.

nationally representative panel survey of Americans aged 50 and older that contains information on demographic characteristics, employment, retirement plans, income and health outcomes. We use the RAND HRS File (Version N), a compilation that contains data from 1992 to 2012 on cohorts born between 1924 and 1959 (Chien et al., 2014).

Appendix Table A1 shows the summary statistics for HRS sample respondents aged 61 and 62. Apart from the disproportionate fraction of decedents who are male, the biggest differences between the composition of the HRS sample and the characteristics of MCOD decedents reported in Table 1 are in terms of marital status and educational attainment. High school dropouts account for 17 percent of 61 and 62 year olds in the HRS sample, but close to 27 percent of all deaths at ages 61 and 62. Non-married people represent 29 percent of 61 and 62 year olds in the HRS sample, but make up 40 percent of those who die at ages 61 and 62. These differences are in line with previous research showing that higher mortality at relatively young ages is correlated with characteristics related to low socioeconomic status, including not completing high school and not being married (Sickles and Taubman, 1997).

We also use the Social Security Death Master File (SSDMF), a database of deaths reported to SSA since 1962 that includes dates of birth and death.[12] The most common sources of this information are relatives of deceased individuals, funeral directors, financial institutions and postal authorities. The SSDMF provides incomplete coverage of deaths in the U.S., and the probability a death is included may vary with Social Security eligibility (Hill and Rosenwaike, 2002; Government Accounting Office, 2013). This does create a potential bias in mortality counts at age 62 due to measurement error. There are also a smaller number of deaths in the SSDMF with dates of death from before 1962.[13] This is before men had an EEA of 62, so these deaths should not be subject to systematic measurement error at age 62.

There are two additional datasets. One is the one percent extract of SSA claiming data, used to create Figure 1. The other is accelerometer data from the National Health and Nutrition Examination Survey (NHANES) 2003-2006, a survey that measures the health of the civilian population via interviews and physical examinations. See the data appendix for more details.

[12] Another possible source of mortality data is the Numident File. The Numident consists of the SSDMF data and adds records from 33 state vital statistics agencies. However, the extra data from the vital statistics agencies is exactly what is contained in the MCOD, except the MCOD has it for all 50 states and DC. In other words, the SSDMF is a subset of the Numident, which is in turn a subset of the MCOD. Therefore, using the Numident, we would expect to find an increase in mortality at age 62 that is somewhere between what we find using the SSDMF and what we find using the MCOD data.
[13] These are generally individuals whose Social Security accounts were active in 1962 (e.g., a deceased husband's account may be active because his wife receives benefits on the basis of it). About 95 percent of these earlier records are of males.

4. Assessing the Change in Mortality at Age 62

To estimate the mortality effects due to Social Security eligibility, we use a RD design to compare the number of deaths of individuals aged just older than 62 to slightly younger individuals. In this context, the ideal experiment would be to randomly assign some workers to retire and others to remain employed, and then compare the mortality of the two groups. Such an experiment is infeasible, especially as large sample sizes are required to estimate mortality effects.

We therefore use exogenous rules regarding Social Security eligibility to compare the deaths of those newly eligible to those who are nearly eligible.[14] We follow other studies using age-based RD designs by seeking to estimate the effect of an anticipated change in program eligibility in an intent-to-treat framework.[15] Comparing deaths above and below age 62 will generate estimates of the average treatment effect of Social Security eligibility on mortality as long as other factors affecting mortality do not change discontinuously when individuals turn 62. Since the people whose decisions are most influenced by Social Security eligibility are different from the broader population, our estimate is specific to that population. We estimate the immediate intent-to-treat effect of Social Security eligibility on mortality.

4.1 Graphical Evidence

We show the number of deaths for each month of age in Figure 2. We include counts for 12 months on each side of age 62 (i.e., ages 61 years, 0 months to 62 years, 11 months). Overall mortality counts are shown in Panel A. There is a large increase in mortality in the month individuals turn 62, with 1,580 more deaths in the month after turning 62 than the month before (a 2.4 percent increase). By comparison, the next largest difference in month-on-month mortality is less than one half of this magnitude. We also show lines estimated for each side of the age-62 cutoff using quadratic polynomials. These indicate a discontinuous increase in mortality at 62; we formally test for such an increase in the next section. In the other panels of Figure 2, we show that this increase in monthly mortality occurs at age 62 for both males (Panel B) and females (Panel C), although there is a clearer and larger increase in mortality for males than females.

[14] We are not the first to consider using Social Security eligibility at age 62 for a RD design: Angrist and Pischke (2014) open their RD chapter with three examples, including: "The Social Security Administration won't pay you a penny in retirement benefits until you've reached age 62" (p.147). Ayyagari (2016) also uses the age 62 threshold for Social Security as an instrument to examine the link between retirement and smoking.

[15] E.g., Card et al., 2008, 2009; Carpenter and Dobkin, 2009, 2015, 2017; Anderson et al., 2012, 2014; Barcellos and Jacobson, 2015; Dillender, 2015; and Carpenter, Dobkin and Warman, 2016.

It is possible that this change in mortality is related to having a birthday or generally becoming one year older. To check this, Appendix Figure A1 shows the same age-in-months mortality counts in relation to ages 61 and 63. We are not aware of any important age-based policy rules at these ages. There are no obvious changes in mortality at either of these ages, either for the full sample or for separate subsamples of males and females.

4.2 Regression Estimates

We implement several global parametric and local nonparametric RD specifications to assess the results under different assumptions about the data generating process. The parametric regressions contain polynomials that control for the underlying age-mortality relationship and a dummy variable to estimate the mortality change at age 62. The basic form of the regression is:

$$\log(Mortality_a) = f(a) + Post62_a\,\beta + \varepsilon_a \tag{1}$$

The dependent variable is the natural log of mortality counts for age at death a, so the change in mortality is measured in percentage terms. We use monthly counts, which is the level of aggregation suggested by RD tests of excess smoothing (Lee and Lemieux, 2010). The function $f(a)$ represents the polynomials used to control for the age-mortality relationship. We separately use quadratic, cubic and quartic polynomials, as there is a debate about the correct polynomial length (Lee and Lemieux, 2010; Gelman and Imbens, 2014). The dummy variable $Post62_a$ is equal to one above age 62 and zero otherwise, and the coefficient β gives the percentage change in mortality at age 62 (our coefficient of interest). We allow the relationship between age and mortality to vary on either side of the discontinuity by interacting $Post62_a$ with all of the polynomial terms. The error term is ε_a. We use robust standard errors clustered on age of death.

In column 1 of Table 2, we present the estimated change in mortality at age 62. Using a bandwidth of 12 months, the point estimates for the different specifications are 1.35 percent (quadratic), 1.97 percent (cubic) and 1.93 percent (quartic). All are statistically significant at the one percent level. The underlying relationship between age and log mortality increases slightly after age 62, although the relevant coefficients are not large or precisely estimated.[16]

We also use local nonparametric specifications to relax the functional form assumptions and place more weight on observations near the cutoff. We use the local linear and quadratic specifications of Calonico, Cattaneo and Titiunik (CCT) (2014a, 2014b), whose "data driven" bandwidth selection procedure is based on minimizing the mean square error of the point esti-

[16] The full set of coefficients from the quadratic specification is presented in Appendix Table A2.

mate. We start with 24 months of data on either side of age 62, as policies that take effect at age 60 could separately affect mortality. We report estimates that use a triangular kernel, CCT's procedures to correct for bias due to bandwidth choice, and plug-in residuals for standard error estimation, which are equivalent to Huber-White robust standard errors in the parametric context.

The results from these regressions are also presented in the first column of Table 2, together with the bandwidths used. The estimates are similar to the parametrically generated results, with point estimates of 1.42 percent (local linear) and 1.94 percent (local quadratic) that are statistically significant at the one percent level. Across the five specifications, and in accord with the visual evidence, we find a statistically significant increase in mortality at age 62.

In Table 2, we also present estimates of the change in mortality in the month individuals turn 62 separately for males (in column 2) and females (in column 3) using the three global parametric and two local nonparametric specifications. For males, the estimated increase in mortality is between 1.85 and 2.43 percent and always statistically significant at the one percent level. For females, the estimated increase in mortality is between 0.58 and 1.38 percent. All except the quadratic estimate are statistically significant at the one percent level; the quadratic estimate of 0.58 percent is not statistically different from zero at conventional levels. The results are in line with the visual evidence of a larger and clearer change in mortality for males than for females.

We verify that the estimates are robust to additional regression controls and other choices (Appendix Table A3). First, we show that the global parametric estimates are similar when year-of-birth or month-of-death fixed effects are included. Second, we show that the estimates are similar using daily or weekly mortality counts. Third, we present estimates where the cutoff is defined by the precise Social Security eligibility rules. These rules grant individuals born on the 1st or the 2nd of the month eligibility in the month they turn 62, while others become eligible the following month (Olson, 1999). Our estimates are qualitatively similar throughout.

We next assess the robustness of the estimates to different bandwidths by using bandwidths of between six and 24 months. Having established that the results are generally similar with and without higher-order terms, in this and following exercises we focus on the local linear and global quadratic specifications. Appendix Figure A2 shows the estimates and 95 percent confidence intervals from both of these specifications for the full sample, for males and for females. For the full sample and for males, the local linear and global quadratic estimates are between 1.5 and 2.4 percent and statistically significant for all bandwidth values. The results for females are more sensitive to the choice of bandwidth. The estimated change in mortality declines as the bandwidth increases and loses statistical significance at the five percent level at 11 months in the local linear regression and 12 months in the global quadratic regression. At larger bandwidths, the estimates for females are smaller and imprecisely estimated.

There could be a concern that the change in male mortality is spurious and equally sized estimates could be found at nearby ages. We first deal with this via a permutation test based on randomization inference. We generate "placebo" estimates at monthly intervals for 60 months before and after age 62 (i.e., from age 57 to 67). Note that this range includes ages at which there are policy eligibility thresholds that could affect mortality, such as ages 60 and 65. In Figure 3, we compare the estimate for the month males turn 62 to the empirical cumulative distribution functions of the placebo estimates using the local linear (Panel A) and global quadratic (Panel B) specifications. Despite including estimates possibly not from true placebo locations, for both specifications the largest estimate is for the month males turn 62. In contrast, as shown in Appendix Figure A3, our age-62 estimates for females are not distinct from other estimates: approximately one quarter of the local linear estimates and one half of the global quadratic estimates are larger in absolute value than the estimate for the month females turn 62. A second approach, based on comparing overall model fit in the month individuals turn 62 to nearby ages, reinforces that there is something distinct about the age-62 change in mortality for males, but not females.[17]

The estimated increase in male mortality is extremely robust. The results suggest there a substantial number of "excess" deaths for males after age 62. For example, the 2.15 percent local linear estimate translates into 10,746 additional male deaths in the 12 months after turning 62 and 22,029 additional male deaths in the 24 months after turning 62 (relative to a counterfactual of no discontinuous change at age 62). These translate into averages of 384 and 787 extra deaths per cohort, respectively.

The results for females are smaller in magnitude and sensitive to bandwidth and specification choice. There could be various reasons for this. First, as we show below, although men and women have similar patterns of claiming Social Security at age 62, estimates of the changes in women's labor supply are much smaller and less precise than for men. Second, the effect we estimate is local to those near the margin of death at age 62. The marginal relationship between

[17] We compare R-squared when the discontinuity is at age 62 to placebo locations. Using the global quadratic specification and 12 months of data on each side of age 62, we show that the R-squared value is maximized at age 62 for males but not for females (where it is fourth-largest in the 24-month window). The results are presented in Appendix Figure A4.

age and death differs by sex: many more men than women die at age 62, and the life expectancy of women conditional on reaching 62 is much higher than for men (Arias, 2014). While this may account for the differences across men and women, the imprecision of the female labor supply estimates means that we cannot rule out a similar link between retirement and mortality for both women and men. More generally, we hesitate to place too much weight on the results for women, or the differences between men and women, because the results for women are noisy and therefore difficult to interpret. Henceforth, we focus on the increase in male mortality.

Apart from varying the bandwidth over a range with no other policy discontinuities, it is difficult to determine how "local" this effect is. In particular, some might wonder if the visual evidence in Figure 2 suggests a dip in mortality just before age 62 and a corresponding increase just after age 62. We conduct three additional exercises to assess this. First, we test whether the underlying age-mortality relationship changes at age 62. In Appendix Table A2, which shows the coefficients for the quadratic specification, we provide p-values from the joint hypothesis test that the linear and quadratic terms interacted with the age-62 dummy are equal to zero. The p-value is 0.65 for males, suggesting that the underlying mortality trend does not change at age 62.

Second, we add dummy variables to the global quadratic regression to understand the extent to which the results are driven by observations close to age 62. If we add individual dummy variables to the three months before reaching age 62 and the three months after, the coefficient (standard error) is 0.0247 (0.0145). This is equivalent to a so-called "donut hole" RD where three months on either side of the age 62 discontinuity are dropped. If we drop the observations instead of use dummy variables, the coefficient (standard error) is 0.0247 (0.0126).

Third, we use weekly counts and more dummy variables to provide finer information about the changes in mortality close to age 62. We do find some evidence of an additional increase in male mortality in the first week after turning 62. However, even with these additional controls and using the quadratic, cubic or quartic specifications, the overall change in male mortality at age 62 remains approximately two percent. (These results are shown in Appendix Table A4.) In combination with the stability of the estimates to bandwidth choice, these results suggest that the increase in male mortality persists for some time after males turn age 62.

4.3 Estimates and Placebo Tests using the Social Security Death Master File

We now use the SSDMF, a file of deaths reported to SSA. As already discussed, the SSDMF provides limited insight into how much mortality changes at age 62 because the probability a death is included may vary with Social Security eligibility. However, it is useful for two reasons. First, the measurement bias favors finding an increase in mortality at age 62, so finding no increase would raise some doubt about the analysis using the MCOD data. Second, and more importantly, the SSDMF contains deaths from before men could claim Social Security at age 62. Evidence that deaths in this sample increase at age 62 would suggest that the increase in mortality at age 62 is not related to Social Security and retirement.

To assess whether there is an increase in mortality when Social Security eligibility occurs at age 62, we use a sample of decedents born between 1930 and 1948.[18] Figure 4 shows the number of deaths by age for the 12 months on each side of age 62 for all deaths (in Panel A) and male deaths (in Panel B). Both show a large increase in mortality when individuals turn 62. There is also a large increase in females' mortality at 62, as shown in Appendix Figure A5.

We estimate the changes in mortality at age 62 using RD specifications, focusing on the estimates from the local linear regression with a CCT bandwidth and global quadratic regression using a bandwidth of 12 months. Results are presented in Appendix Table A5. Column 1 shows the results using all mortality counts. The estimated increases are 4.3 percent using the local linear regression and 4.7 percent using the global quadratic regression; both are statistically significant at the one percent level. The results for males and females are shown in columns 2 and 3, respectively. The respective local linear and global quadratic estimates are slightly higher for males (5.5 and 5.0 percent, both $p<0.01$) than females (3.5 and 4.0 percent, both $p<0.05$).

It is difficult to interpret the magnitudes of these estimates, as they likely combine changes in underlying mortality and measurement differences that occur as individuals begin to receive Social Security. Larger increases than in the main analysis and statistically significant increases for females are consistent with the direction of the bias due to reporting differences. Importantly, the results are not inconsistent with the previous analysis.

We next use the SSDMF for placebo tests. First, we check whether the SSDMF has mechanical jumps in mortality related to birthdays by showing the changes in mortality at ages 61 and 63 in Panels A and B of Appendix Figure A6, respectively. There is no obvious change in

[18] Exact dates are generally not available for deaths that occurred before 1988, which prevents us from using the exact same set of birth cohorts as in the MCOD analysis (i.e., 1921-1948). See the data appendix for more details.

mortality at either age. This is confirmed by the regression estimates, shown in columns 4 and 5 of Appendix Table A5.[19] These results are consistent with our results using the MCOD data.

Second, we use deaths occurring before the male EEA was introduced in 1962. These are generally from Social Security accounts that were active when the SSDMF data collection began in 1962 (e.g., a widow claiming on her deceased husband's earnings record). We use deaths occurring between 1940 and 1961; approximately 95 percent are males. While this is a small and selected sample of deaths over this period, there is no reason to expect selection to change at age 62. Day of death is not available, so age of death is based on month and year information.

For this sample, the mortality counts around age 62 are shown in Panel C of Figure 4. There is no visually apparent change in mortality at age 62. This is also the case for male mortality counts, which are shown in Panel D. The regression results for these samples are presented, respectively, in columns 6 and 7 of Appendix Table A5. The coefficients are small and statistically insignificant at conventional levels, suggesting that there was no increase in mortality at age 62 before it was the EEA. The absence of day-of-death information adds measurement error to these placebo estimates: to assess the importance of this, we estimate regressions for the 1930-1948 cohorts without using day of death.[20] We show these estimates in columns 8 and 9 of Appendix Table A5. For all deaths, the local linear and global quadratic estimates are 4.1 and 4.0 percent, respectively (both $p<0.05$). For male deaths, the respective estimates are 4.2 and 5.1 percent (both $p<0.01$). Estimates of the mortality change are slightly attenuated without day of death, but we still detect statistically significant increases at age 62. In summary, these results support the analysis using the MCOD files.

4.4 Heterogeneity of Mortality Effects

We now return to the main MCOD sample and analyze heterogeneity across demographic subgroups. We continue to focus on estimates produced from the local linear regression with CCT bandwidth and global quadratic regression using a bandwidth of 12 months. Note that any difference across these groups could reflect differences in Social Security claiming or labor force participation, or how such changes affect mortality through, for example, healthy behaviors or access to health insurance. We will discuss the heterogeneity in the mortality effects in this section, and then seek to understand the likely cause of the observed differences in the next.

[19] The global quadratic estimate for age 61 of -1.5 percent is statistically significant at the ten percent level. However, the local linear estimate is not statistically significant at any level and there is no visual evidence of a change at age 61 (or 63).
[20] Day of month is now not needed, which adds 12 percent to the overall number of deaths and six percent to male deaths.

We first check if the increase in male mortality is driven by a specific state or region by omitting observations from, in turn, each of the four Census Regions of the United States. These results are presented in Appendix Table A6. For each of the samples, the coefficients are similar to the main estimates and statistically significant at least at the five percent level. These results rule out state- or region-specific factors accounting the change in male mortality at age 62.

We next examine the effects across cohorts. Recall that the change in the FRA from 65 to 66 years increased the penalty due to claiming at age 62 from 20 to 25 percent, lowering average Social Security payments. However, as shown in Appendix Table A7, there is no discernible difference in the estimates across cohorts with different FRAs. This also shows that the increase in male mortality at age 62 is a persistent phenomenon that remains present in recent years.

We next consider demographic characteristics, presenting these results in Table 3. In addition to the local linear and global quadratic estimates, we also report the fraction of total deaths that each group represents. Marital status is presented first. Given that 65 percent of male decedents were married, we initially divide the sample into married and non-married males. For married males, the local linear estimate is 1.30 percent ($p<0.05$) and the global quadratic estimate is an imprecise 0.81 percent ($p\geq0.05$). In contrast, estimates for non-married males are substantially larger, with a local linear estimate of 4.15 percent ($p<0.01$) and a global quadratic estimate of 3.77 percent ($p<0.01$). We can reject the null hypothesis that the coefficients for married and non-married males are the same ($p<0.05$). We examine the non-married group in further detail by looking separately at single, divorced and widowed males. The local linear and global quadratic point estimates for single males are the largest (5.14 and 5.58 percent), followed by divorced (3.05 and 3.37 percent) and widowed males (2.62 and 3.30 percent). Estimates for single and divorced males are statistically significant (at least) at the five percent level, but not for widowed males, although they only account for 6.2 percent of the sample. The only statistically significant difference between coefficients is between the single and married male coefficients ($p<0.01$).

We next present results based on differences in educational attainment. Note that the sample is smaller because educational attainment has only been available in the MCOD file since 1989. The increase in mortality upon turning 62 is largest for males who did not complete high school, with a local linear estimate of 3.03 percent ($p<0.01$) and a global quadratic estimate of 2.75 percent ($p<0.05$). The estimates for those who completed high school and those who completed college are smaller and not statistically significant at any level, although the standard errors for college males are large and 95 percent confidence intervals do not rule out large increases in mortality at age 62. We cannot reject the null hypothesis that the coefficients are the same.

Race is defined broadly as either white or non-white, as non-white males only account for 16.5 percent of the sample. For white males, the estimates are large (2.09 and 2.37 percent) and statistically significant (both $p<0.01$). For non-white males, the estimates are smaller (0.68 and 0.78 percent) and not statistically significant; however, the confidence intervals are wide and do not rule out a similar increase in mortality as for white males.

We examine if there are differences by place of death, using three categories: in-hospital deaths; deaths in nursing homes/institutions; and other deaths (including deaths classified as "dead on arrival" at hospital). The largest increase upon turning 62 occurs for deaths outside of hospitals/institutions, with estimates of 3.23 and 3.39 percent (both $p<0.01$). Deaths in hospitals are estimated to increase by 1.24 and 1.30 percent (both $p\geq0.05$), while deaths in nursing homes and other institutions change by -0.22 and -0.79 percent (both $p\geq0.05$). The lack of any change in mortality in long-term care facilities is comforting, as an increase in this group would have suggested a mechanism unrelated to changes in Social Security.

The heterogeneity across demographic groups may represent underlying differences in Social Security claiming or associated changes in individuals' work, income and health insurance coverage. We next combine the mortality evidence with information on retirement-related changes to understand why mortality increases once males turn 62.

5. Why Does Male Mortality Increase at Age 62?

Claiming Social Security is not an isolated event; many other changes commonly occur at the same time. First, individuals frequently stop working or reduce work levels. Second, health insurance status can change as – upon stopping employment – employer-sponsored health insurance ceases or is more expensive under continuing-coverage mandates. (Medicare is unavailable until age 65.) Third, income can change at age 62 due to the arrival of Social Security income and the reduction in earnings. Each change potentially has detrimental health consequences.

Given the lack of direct information in the MCOD data, we use complementary data sources and previous literature to provide descriptive evidence as to which of these are likely to be important in explaining the rise in mortality at age 62. Our analysis proceeds in three steps. First, we document the retirement-related changes that occur at age 62. Second, using the heterogeneity in the mortality estimates across demographic subgroups, we examine which of these retirement-related changes at age 62 are most strongly correlated to changes in mortality. As a result of these exercises, decreasing labor force participation emerges as the most likely reason mortality rises at age 62. Finally, we examine cause-of-death results and information on health behaviors and show it is plausible that labor force exit could explain our mortality results.

5.1 What Changes at Age 62 as a Result of Social Security Claiming?

We examine retirement-related changes using the HRS, which is focused on aging and retirement information. We first establish what changes occur at age 62 by plotting various outcomes in Figure 5. Age is measured in quarters and rates are calculated using person-specific sample weights. Claiming is self-reported and retrospective. In Panel A and B, we show the respective Social Security claiming rates for male and females. As for the mortality data, we show lines estimated for each side of the age-62 cutoff using quadratic polynomials. We also show the coefficient (standard error) from the global quadratic RD regression used previously, which is 0.206 (0.007) for males and 0.204 (0.009) for females. If we estimate the change using the SSA data for Figure 1, we find similar, albeit slightly higher discontinuities in overall Social Security claiming for males and females.[21]

We present other outcomes in the rest of Figure 5. We show the fraction of males and females retired in Panel C and D, respectively, where retirement is defined by labor force status (i.e., not working due to retirement). Male retirement is estimated to increase discontinuously at age 62 by 10.9 p.p. ($p<0.01$), compared to 4.7 p.p. for females ($p\geq0.05$). Two other employment outcomes are shown: labor force participation (in Panels E and F) and the fraction working for pay (in Panels G and H). Male labor force participation is estimated to change discontinuously at age 62 by -8.2 p.p. ($p<0.01$), while there is no change in female labor force participation (-0.5 p.p., $p\geq0.05$). The fraction of males working for pay is estimated to change discontinuously at age 62 by -8.2 p.p. ($p<0.01$) and remain unchanged for females (-0.9 p.p., $p\geq0.05$). For all three measures of labor supply, males experience large and statistically significant changes upon turning 62, while the estimated changes for females are small and imprecisely estimated.

We examine changes in health insurance coverage at age 62 in Panels I and J of Figure 5. For both males and females, the fraction with health insurance is steady and the estimated changes are statistically insignificant estimates of -1.4 and -1.7 p.p., respectively. Finally, we present

[21] Using the SSA data, the estimated discontinuities in claiming at age 62 for males and females are, respectively, 23.3 and 25.1 percentage points. It is not surprising that there is some measurement error in self-reported Social Security claiming rates in the HRS; it is unlikely this differs by sex. We also use alternate measures of claiming in the demographic analysis.

annual household income in Panel K and L of Figure 5. HRS respondents are asked about their income in the prior calendar year, so we cannot examine it on a quarterly basis. We convert income to 2014 dollars using the CPI-U and plot it in terms of the respondents' age in the previous year (e.g., 63 year old respondents have their income for the previous year assigned to age 62 using CPI for that previous year). There is no change in household income trends at age 62 or at 63, the first full year that Social Security is a major income source for those claiming at age 62.[22]

The increases in claiming Social Security at age 62 are similar for males and females, making it unlikely to be the reason why there is a large and robust increase in male mortality. However, males reduce their labor force participation more than females, making it a possible explanation for why males' mortality increases upon turning 62.

5.2 The Relationship between Changes at Age 62 and the Mortality Effects

We make use of demographic information common to both MCOD and HRS data to further explore the role of Social-Security-related changes at age 62. In Section 4.4, we estimated male mortality for subgroups based on marital status (married, single, divorced and widowed), educational attainment (completed less than high school, high school graduate and college graduate) and race (white and nonwhite). Using these characteristics, we create 13 distinct male subgroups with reasonable sample sizes in both the MCOD and HRS files.[23] For 14 groups (females plus 13 male groups), we compare the changes in retirement-related outcomes upon turning 62 to changes in mortality. We use the global quadratic RD specification.

In Figure 6, we examine the relationship between the mortality estimates and the same six retirement-related outcomes used in Figure 5: Social Security claiming, fraction retired, fraction in labor force, fraction working for pay, fraction with health insurance and average household income. For each of the 14 groups, the mortality coefficients are plotted in relation to the horizontal axis and the estimated changes in the retirement-related outcomes are plotted in relation to the vertical axis. The linear relationship between the coefficients is also plotted.

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[22] The timing of income can affect mortality, although it is unlikely to plays a role here. While Evans and Moore (2011) find that there a short-term increase in mortality following income receipt (including for Social Security income), it is primarily present in the first week after receipt and largely offset by declines in mortality three or four weeks afterwards. Our use of monthly counts should smooth out fluctuations in deaths due to income receipt, as well as any short-term variation in mortality due to the within-month mortality cycle (Evans and Moore, 2012).

[23] While the combination of these characteristics provides a possible 24 male subgroups (4 marital x 3 education x 2 race types), sample sizes in some subgroups are very small. Instead, we aggregate some of these groups so there at least 200 HRS respondents within 24 months of age 62. This is achieved by creating six married subgroups (split by both education and race), three subgroups each for single and divorced males (split by education only), and a widower subgroup.

The strongest correlations are between the size of the mortality estimates and the three work-related outcomes. Mortality changes are positively correlated with the changes in retirement and negatively correlated with changes in labor force participation and working for pay. We assess the strength of these relationships by separately regressing the mortality estimates on each of these outcomes. We do this with and without weighting by the average number of deaths in each group to reflect their contribution to the aggregate mortality effects (Solon, Haider and Wooldridge, 2015). Weighting also deals with the potential influence of outliers, which tend to be due to small subgroups. As shown in Appendix Table A8, the coefficients are similar with and without weighting, and all are statistically significant at the five percent level. The results suggest that decreases in labor force participation are correlated with increases in mortality. The point estimates suggest that a one percentage point decrease in the three employment outcomes raise mortality by approximately 0.07-0.1 percent.

In contrast, the relationship between the changes in Social Security claiming and mortality at age 62 appears weak visually and is not statistically significant at conventional levels in either regression. To ensure this is not due to the specific way claiming is measured in the HRS, as alternate measures we consider mean differences in claiming rates between ages 61 and 62 and the receipt of Social Security payments.[24] As shown in Appendix Table A8, there is no relationship between the mortality estimates and either of these Social Security measures.

We also consider the role of changes in health insurance and income, which are shown respectively in Panels E and F of Figure 6. They suggest there is no relationship between these outcomes and the mortality estimates.[25] This is unsurprising given that, as was shown in Figure 5, these outcomes do not change at age 62. In the regression analysis in Appendix Table A8, we also show that there is no relationship with alternate measures, including the number of health insurance plans a respondent has and household income when it is restricted to respondents interviewed between January and March (whose calendar income will more closely match their age-related income). Finally, we jointly assess the relationship between mortality and the various retirement-related changes by regressing the mortality estimates against the combined estimates

[24] There is measurement error in the HRS in self-reported Social Security claiming; the increase at 62 observed in the SSA data is spread slightly over nearby months. As a result, as can be observed in Panel A of Figure 6, the estimated discontinuities for the 14 groups are smaller than might be estimated if administrative data had been available. As an alternative, the difference in the average claiming rates at ages 61 and 62 overstates the change immediate after turning 62, but yields similar results. The receipt of Social Security payments is another, albeit coarse measure that is available in the HRS.

[25] Annual income data means it is not possible to use the RD specification, so we report the average percentage change in income between ages 61 and 63 on the vertical axis.

of each type of change (i.e., retirement, Social Security claiming, health insurance and income). These regressions, which are designed to account for the correlations between the different changes that occur at age 62, are presented in Appendix Table A9. The results do not change once we account for such correlations: the various measures of retirement retain a statistically significant relationship to the mortality results, while all of the other measures continue to have a weak relationship with them.

Another way to consider the potential role of health insurance and household income is to calculate the effect sizes needed to explain the rise in male mortality at age 62 and compare them to estimates from previous studies. For health insurance, the point estimate for the change in males' health insurance at age 62 is -1.4 p.p., so losing health insurance would need to elevate males' mortality by approximately 140 percent to explain the overall increase. Studies find the mortality of uninsured individuals is as much as 43 percent higher than individuals with health insurance (McWilliams et al., 2004). However, Black et al. (2017) argue that this overestimate the size of the effects. For income, the change in male mortality with respect to household income would need to be -3 to -4 as an elasticity in order to explain the increased male mortality. Estimates have generally been much closer to zero than this, even for poor and unhealthy individuals like old-age pensioners in Russia (-0.94 in Jensen and Richter, 2004) and Union Army veterans in the early 1900s (-0.57 in Salm, 2011). Synder and Evans (2006) find a mortality-income relationship for U.S. Social Security beneficiaries going in the other direction that translates into an elasticity of approximately 0.60. The available evidence suggests it is unlikely that health insurance and income account for a large part of the increase in mortality at age 62.

Retirement from work is therefore the most likely driver of the mortality increases at age 62. However, there are several caveats. First, the evidence is descriptive and not causal in nature. Second, we cannot rule out some role for other retirement-related changes, even if any single alternate channel is unlikely to solely explain the change in mortality. Third, there may be changes we do not observe that affect mortality. For example, there might be a change in health insurance quality as people move from employment to retirement. Fourth, interactions in retirement-related changes at the individual level may be important (e.g., stopping work may matter more if you also lose health insurance than if you do not). Although we think there is supporting evidence for the hypothesis that our mortality results are driven by shifts in labor supply, the evidence is far from conclusive. As a final step in considering the plausibility of our estimates and the likely reasons for higher mortality at age 62, we now consider what causes of death increase at 62 and their consistency with other studies on the work-mortality relationship.

5.3 Underlying Causes of Death and Changes in Health Behaviors

To further examine the plausibility of retirement leading to higher mortality, we examine which causes of death increase when males turn 62 and consider the connection between those and decreased labor force participation. We use underlying causes of death across International Classification of Disease Versions 9 and 10, defining four groups and examining subgroups as sample sizes allow. The groups are: (i) heart and lung conditions; (ii) cancers; (iii) external causes; and (iv) all other causes. More details about this classification are provided in the data appendix. Results are provided in Appendix Table A10.

For heart and lung conditions, the local linear and global quadratic RD estimates are 1.35 and 2.50 percent, with the latter statistically significant ($p<0.05$). This group consists of heart attacks, other heart disease, chronic obstructive pulmonary disease (COPD) and strokes. When it is separated into heart attacks, COPD and other heart/lung causes, what is striking is the increase for COPD (4.96 and 6.96 percent, $p<0.05$ and $p<0.01$ respectively). In contrast, the point estimates for heart attacks and other heart/lung causes are smaller and not statistically significant.

For cancers, there is a large, statistically significant increase (2.62 and 2.63 percent, $p<0.01$ for both specifications). While it may seem strange that there is a discontinuous change in cancer at age 62, there can be many other causes of death in addition to the primary cause. When we restrict the sample to cases where cancer was the only identified cause of death, the estimated change is smaller and not statistically significant (0.69 and 1.78 percent, both $p\geq0.05$). Once we separately estimate the change for lung cancer and other cancers (whether or not they were the only identified cause), it is clear that the increase at age 62 is due to a large increase in lung cancer deaths (5.10 and 5.31 percent, both $p<0.01$). In contrast, the estimated increase in other cancers is 0.99 and 1.06 percent (both $p\geq0.05$).

External causes, which consists of accidents, murders, and suicides, increase at age 62 (3.14 and 3.99 percent, $p<0.01$ and $p<0.05$ respectively). This is due to a large, statistically significant increase in motor vehicle fatalities (14.21 and 15.26 percent, both $p<0.01$). However, it is important to note that this group accounts for 1.3 percent of all male deaths at these ages, so that traffic fatalities account for no more than 10 percent of the overall increase in male mortality at age 62. There is no increase in non-motor-vehicle external cause deaths (-0.88 and 0.07 percent, both $p \geq 0.05$). Finally, other deaths (i.e., not classified as heart-/lung-related, cancers or external causes) account for 22 percent of all deaths. There is a small, statistically insignificant increase in deaths at age 62 within this group (1.09 and 1.13 percent, both $p \geq 0.05$).

The coefficient estimates and fraction of deaths suggest that the three causes with statistically significant increases in the month males turn 62 – COPD, lung cancer and traffic accidents – account for slightly less than half of the overall increase in male mortality at age 62. Increases in COPD and lung cancer suggest that males' respiratory-related mortality risks change at age 62.[26] As with the initial mortality analysis, there is a question as to whether there is a transitory or longer-term increase. To understand this, in Appendix Figure A7 we plot male mortality at the weekly level around age 62 for all causes (Panel A); for deaths where the primary cause is COPD or lung cancer (Panel B); for traffic fatalities (Panel C); and for deaths where the primary cause is not COPD, lung cancer or a traffic accident (Panel D).[27] An increase in the level of COPD plus lung cancer deaths appears immediately after age 62. Moreover, the underlying mortality trend remains similar: the quadratic best-fit lines are similar on either side of the age-62 discontinuity, and the coefficients on the linear and quadratic terms interacted with the age-62 dummy are not jointly statistically significant at conventional levels. However, the increase in traffic fatalities may be a short-term phenomenon, as there are statistically significant differences in the age-mortality relationship on either side of age 62.

Important risks for respiratory conditions include smoking (Godtfredsen et al., 2008) and a lack of physical activity (Lee et al., 1999); these can affect mortality immediately, even among those who already have the condition (Young et al., 2009; Waschki et al., 2011). There is some evidence that these risk factors change with retirement. For smoking, a directly relevant study is Ayyagari (2016), who uses HRS data and Social Security eligibility at age 62 to estimate the effect of retirement on smoking. She finds smoking increases at 62 among those who had ever smoked, although the results are sensitive to the specification used. The increase is largest among those working before age 62, which is consistent with labor force participation being a relevant change at age 62. Furthermore, Falba et al. (2005) and Black, Devereux, and Salvanes (2015)

[26] COPD and lung cancer have many common risks, and their presence is highly correlated; 50–70 percent of patients diagnosed with lung cancer suffer from COPD, and COPD often precedes a lung cancer diagnosis (Young et al., 2009).
[27] Weekly counts produce similar estimates of the mortality change at age 62. The local linear and global quadratic estimates are: for COPD, 8.5 and 5.4 percent ($p<0.01$ and $p<0.05$); for lung cancer, 4.6 and 4.1 percent (both $p<0.05$); and for traffic, 14.9 and 11.9 percent (both $p<0.01$). The estimates for COPD + lung cancer are 4.8 and 5.2 percent (both $p<0.01$).

show that smoking increases after job loss, providing further evidence that smoking behavior is related to labor force participation.

Evidence on how physical activity changes after retirement is mixed, although a consistent result is that retirees become more sedentary, often watching more television (Evenson et al., 2002; Barnett et al., 2014). Time being sedentary is positively correlated with mortality, even when accounting for exercise levels (Koster et al., 2012, Matthews et al., 2012). In Appendix Figure A8, we show that males increase the time they are sedentary during work hours increases by approximately 15 percent at age 62 (while there is no change in sedentariness of females).[28]

Our findings are consistent with evidence that job loss increases respiratory-related mortality and traffic fatalities.[29] Eliason and Storrie (2009a) use administrative data to examine the mortality effects of large layoffs in Sweden. They find that male mortality, but not female mortality, increases after job loss. The largest increases are for lung cancer, external causes, and heart conditions. While they do not examine traffic fatalities, in related work on hospitalizations they find traffic-related hospitalizations rise among males who lost their job (Eliason and Storrie, 2009b). Browning and Heinesen (2012), using Danish administrative data, find that male mortality increase after large layoffs. The largest increases are for traffic fatalities, circulatory conditions and suicides, but not cancers. Bloemen et al. (2017) find that, in the Netherlands, males laid off due to plant closures have a higher probability of dying within five years as a result of rises in cardiovascular diseases and smoking-related cancers. Among the studies investigating the mortality effects of retirement, only Kuhn et al. (2015) find differences by cause of death. They find that increases in male mortality resulting from unemployment-insurance-related retirements in Austria are due to increases in heart attacks, traffic fatalities, and diseases related to excessive alcohol use and smoking. In combination, these studies show that the causes of death that change discontinuously at age 62 are similar to those that increase when males lose their jobs.

5.4 Evaluating the Mortality Effects of Retirement at Age 62

[28] We use accelerometer data on the intensity of physical activity from NHANES 2003-2006. Males were sedentary 59 and 60 percent of the time at ages 60 and 61, respectively, then 67 and 65 percent at ages 62 and 63. If we focus on 8am-6pm Monday to Friday (i.e., standard work times) the fraction of time males are sedentary increases by 15 percent at age 62, compared to changes of one and five percent at ages 61 and 63. There is no change at age 62 for males out of these hours, or in the fraction of time females are sedentary. A caveat is that the sample sizes are small (92 males and 87 females).

[29] Some relevant studies, such as Snyder and Evans (2006) and Sullivan and von Wachter (2009), either do not examine different causes of death or do not have precise estimates by cause of death.

Supporting evidence suggests decreased labor force participation at 62 is a key factor in a rise in male mortality, although it is not possible to establish causality or rule out other factors.[30] If decreased labor force participation is the reason, then it is important to adjust our estimates to account for the fact that only some males stop work at age 62. The aggregate mortality results are intent-to-treat estimates; we now use observed changes in work to estimate the treatment effect of retirement on mortality at age 62. We do so by dividing the RD estimate for the percentage change in male mortality upon turning 62 by equivalent RD estimates of the percentage point change in work activity. We use the global quadratic RD specification as before and calculate standard errors for these ratios using the delta method (e.g., similar to Anderson et al., 2014).

As with our previous analysis, we use the HRS to estimate the change in retirement, labor force participation and working for pay at age 62. Results are reported in Appendix Table A11. If the 1.9 percent increase in overall male mortality is attributed to the 11 percentage point increase in retirement, then this discontinuous change in retirement results in a 17 percent increase in mortality. Similarly, males have an 8.2 percentage point decline in both labor force participation and the fraction working for pay at age 62. Attributing the increase in aggregate male mortality to either of these measures suggests that stopping work elevates mortality by 23 percent. All three of these estimates are statistically significant at the one percent level.

We estimate similar ratios for females. The estimated changes in females' retirement, labor force participation and the number working for pay when they turn 62 are, respectively, 4.2, -0.1 and -0.8 percentage points. Attributing the 0.6 percent rise in overall female mortality to these measures leads to imprecise estimates of the increase in mortality of 14 percent (for retirement), 402 percent (for labor force exit) and 72 percent (for stopping work). The standard errors on these estimates are extremely large; it is possible there is no relationship between these work activity measures and female mortality, but it is also possible the relationships are of a similar size to what we precisely estimate for males. The availability of Social Security at age 62 does not generate a "first stage" effect on females' employment, making it impossible to say whether or not the connection between retirement and mortality is specific to males.

[30] There are other ways that exiting the workforce could affect health. For example, social isolation can increase upon retiring and has been shown to be correlated with higher mortality (Pantell et al., 2013). The relatively large increase in the mortality of non-married males is certainly consistent with this type of change, although social connections are not the only difference between married and non-married males (e.g., Ayyagari (2016) finds that smoking at age 62 increases more among non-married than married HRS respondents).

Results for the different work-related outcomes suggest that retirement in this setting increases males' mortality risks by approximately 20 percent. This is comparable to related studies. For example, among Austrian blue collar UI recipients in their late 50s, Kuhn et al. (2015) estimate that labor force exit elevates mortality by 13 percent. Sullivan and von Wachter (2009) examine the mortality effects of involuntary unemployment on males using plant closures in Pennsylvania. They estimate that mortality risks increase by over 50 percent in the year following a layoff, and are 10-15 percent higher even 20 years after displacement. The studies examining involuntary job loss in Europe that were discussed in the previous section find mortality increases of 30-80 percent in the first year after job loss, and persistent effects in subsequent years.[31] We do not expect exactly the same estimates, as these studies generally examine involuntary job loss over a longer period of time, whereas we consider the immediate effects of voluntary exit induced by Social Security eligibility. However, these studies demonstrate that large and immediate changes in mortality are possible when there are abrupt changes in labor force participation.

6. Conclusion

Mortality is an important, well-measured, objective health outcome. The availability of detailed administrative data on the entire U.S. population and a strong claiming response to Social Security eligibility provides an opportunity to examine how retirement-related behaviors affect an objective health outcome. We present evidence of a two percent increase in overall male mortality at age 62. This change is statistically significant and robust to different modeling choices, including the range of mortality data used.

The estimated increase is largest for unmarried males and males with low education levels. While these demographic groups do not necessarily experience the largest rates of claiming Social Security at age 62, they do have the largest changes in terms of labor force exit. The causes of death with the clearest increases at age 62 are traffic accidents and two lung-related conditions: COPD and lung cancer. These causes of death have previously been found to be related to job loss, and there is also suggestive evidence that males engage in more unhealthy behaviors once they retire. In combination, the results suggest decreased labor force participation upon

[31] Browning and Heinesen (2012) estimate that the mortality of Danish males laid off increases by 80 percent in the first year and is 10 percent higher over a 20-year span. Eliason and Storrie (2009a) estimate that males laid off in Sweden increases their mortality by 44 percent over the first four years, and nothing thereafter. Bloemen et al. (2017) find that males laid off in the Netherlands have a 34 percent higher probability of dying within five years.

turning 62 as a key reason for a discontinuous increase in male mortality, although other factors may also play a role.

More research will be required to understand why there is not a marked change in female mortality at age 62. It may be that women have different risk factors or retirement-related life-style changes than men, although the imprecision of the female labor supply estimates means that we cannot rule out the same link between retirement and mortality for both women and men. Perhaps this puzzle will be resolved as future research is able to examine this question using up-coming cohorts of women with higher labor force attachment, as their responses to retirement incentives at age 62 may be more similar to current responses by men.

Some limitations are important to keep in mind when interpreting the results. First, it is important to reiterate that age 62 is a well-understood eligibility threshold, and therefore we are estimating changes in mortality from a policy that is anticipated. As a result, our estimates are of the effects on mortality net of any anticipatory changes in health investments. Second, it is not possible to establish whether the mortality effects estimated here would apply to individuals claiming Social Security at later ages, especially as age-62 claimants are less healthy, on aver-age, than later claimants. Third, the structure of the empirical approach means that we are esti-mating an immediate change that is difficult to extrapolate too far from age 62. It is not possible to establish whether age-62 Social Security claimants have elevated mortality over the longer term while they are receiving Social Security or if the health effects compound over time, alt-hough the estimates are stable over the range of data we use and evidence from the literature on the mortality effects of involuntary job loss show that persistent effects are possible. Fourth, the increases in mortality may not translate into broader health changes. While there is typically a strong correlation between mortality and other objective health measures (Idler and Benyamini, 1997), factors that mediate the relationship between health and mortality may also change at re-tirement. For example, as a result of social isolation, retirees could have similar health but be less able to get medical assistance in an emergency (Pantell et al., 2013). Finally, any welfare losses related to heightened mortality risks may be offset by the utility gains related to stopping work. It is therefore not possible to know the optimal timing of retirement without knowing the relative values and weights that should be placed on increased mortality and utility gains from retire-ment, and even whether workers correctly anticipate and understand such effects. Further re-search will be required to understand the importance of these issues.

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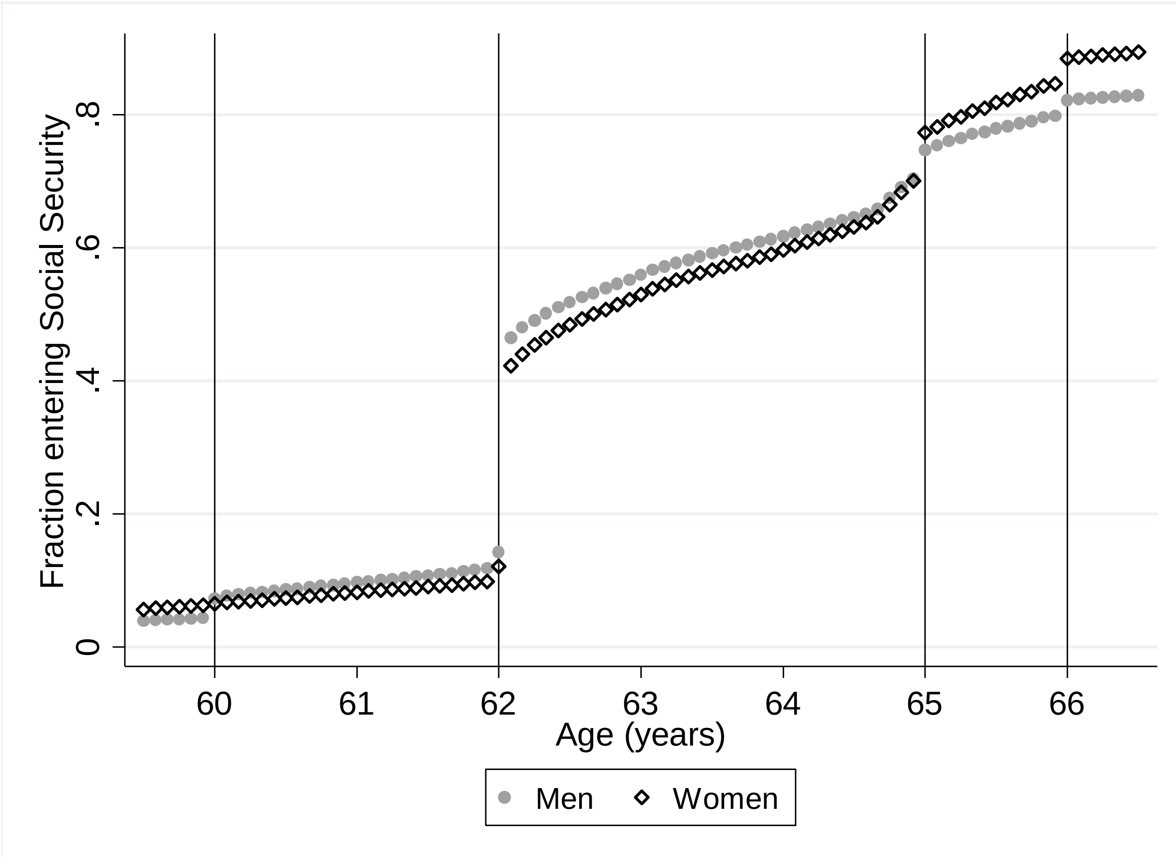
Figure 1 Cumulative Rate for Ever Having Claimed Social Security, by Sex



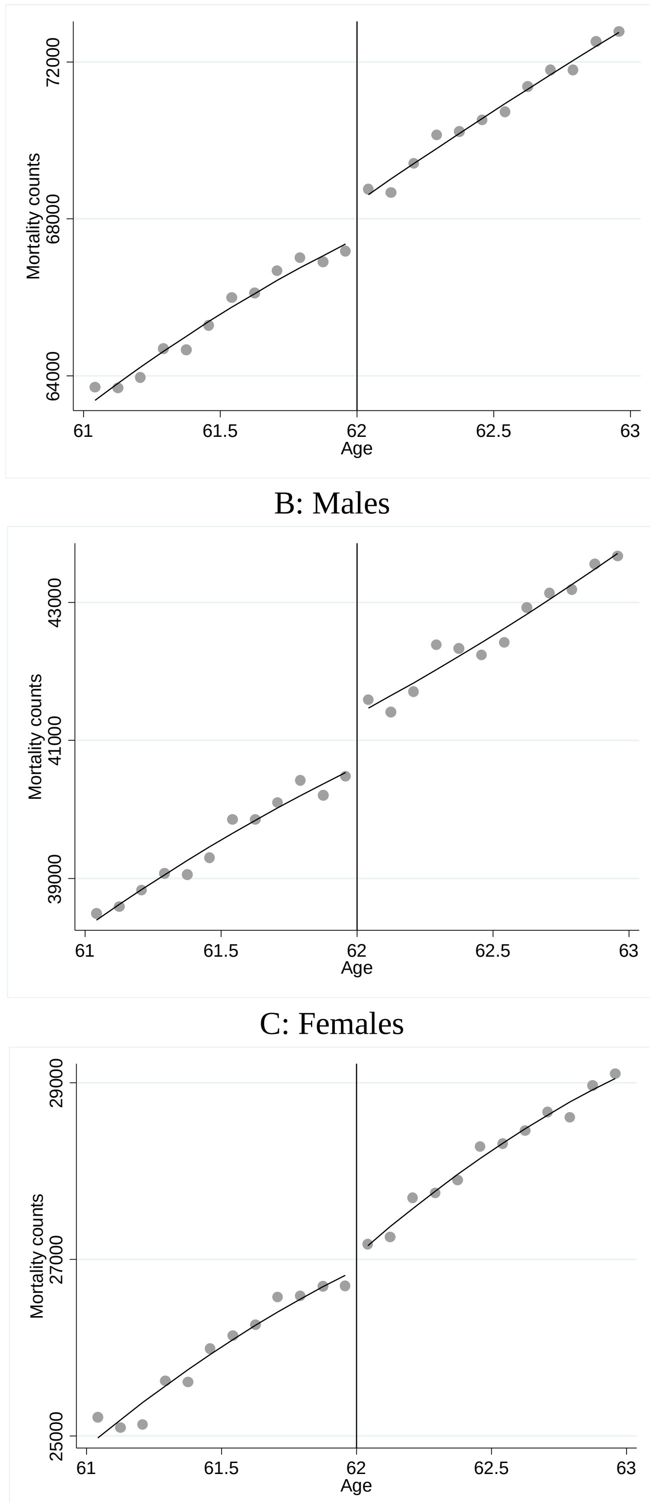
Notes: We use birth cohorts from 1921 to 1948 and include claims by both workers and dependents for all components of Social Security (i.e., Disability, Retirement and Survivors Insurance). Claims data is from a one percent extract of SSA's Master Beneficiary Record and Numident File. Claiming numbers are converted to rates based on population estimates from the Current Population Survey.

Figure 2 Monthly Mortality Counts in Relation to Turning Age 62, Cohorts Born 1921-1948

A: All

B: Males

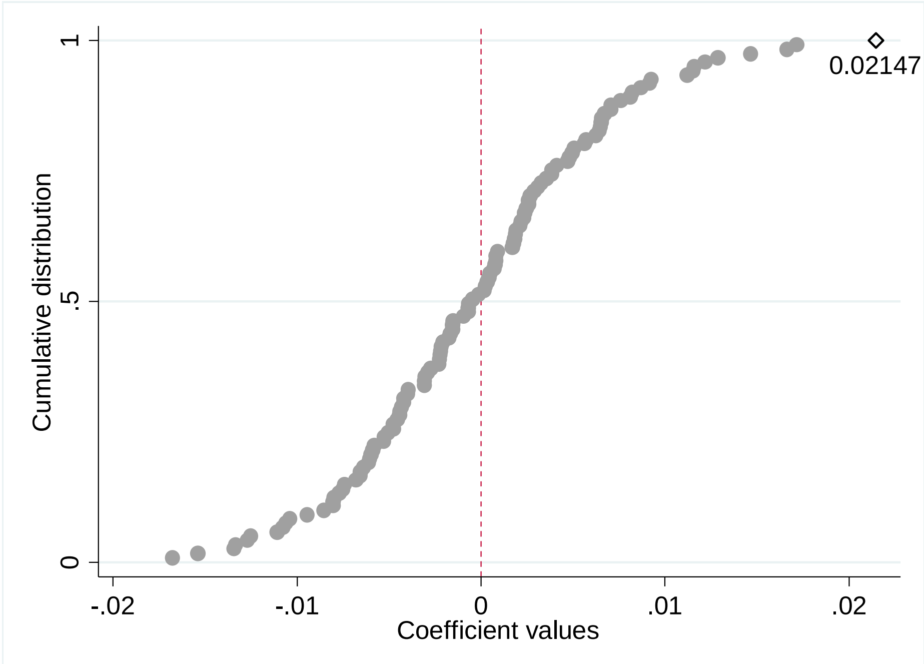
C: Females



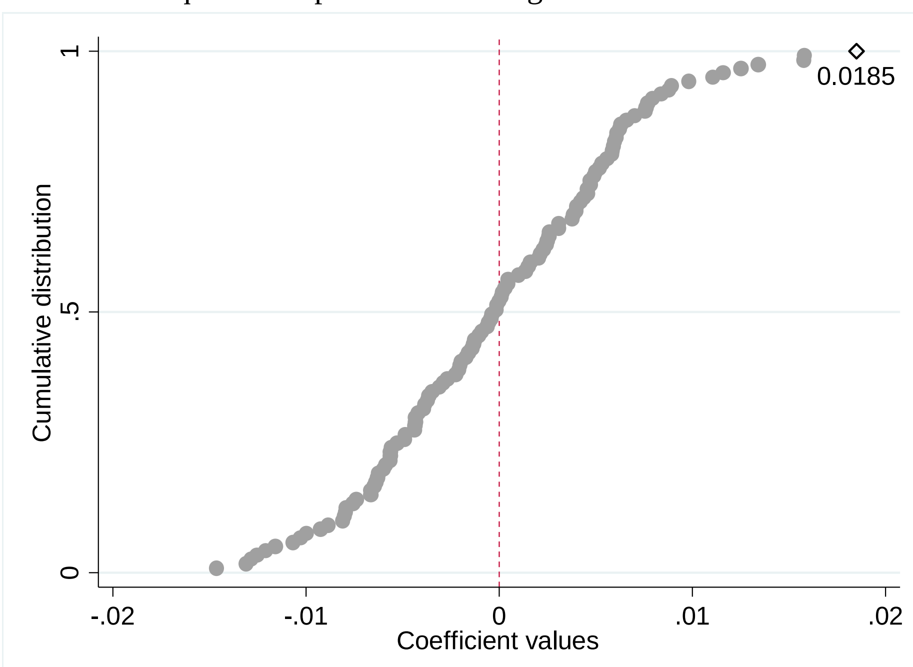
Notes: Data is from restricted-use versions of the Multiple Cause of Death data held at the National Center for Health Statistics. Reported mortality counts are for cohorts born from 1921 to 1948. The figures show the number of deaths by age measured in months. The best-fit lines are quadratic polynomials fitted on each side of age 62.

Figure 3 The Distribution of Placebo Male Mortality Estimates for +/-60 Months of Age 62 Compared to the Estimate at Age 62 *(Diamond, labeled)*

A: Local linear specification using CCT-calculated bandwidths



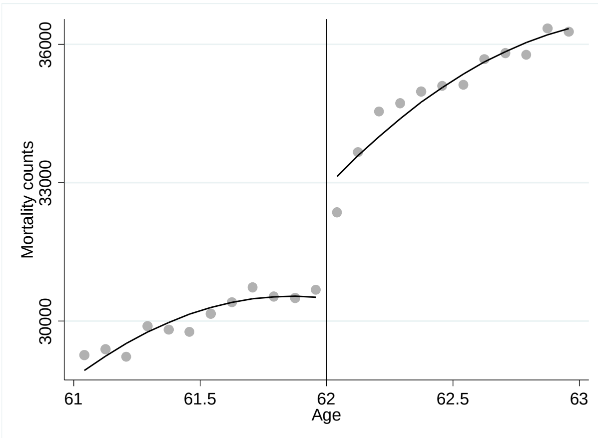
B: Global quadratic specification using a bandwidth of 12 months



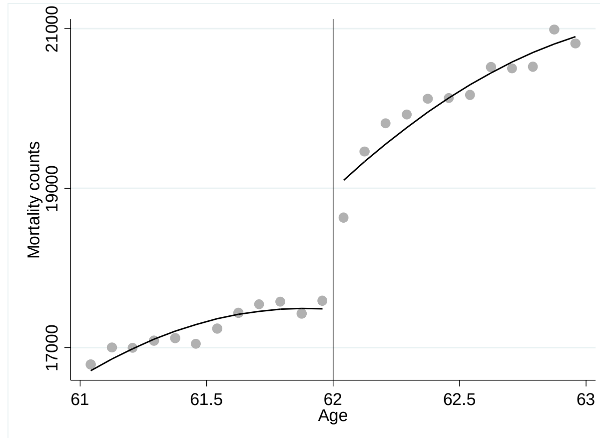
<u>Notes</u>: Data is from restricted-use versions of the Multiple Cause of Death data held at the National Center for Health Statistics, and includes the 1921 to 1948 cohorts. The figures show the conditional density functions of point estimates using each month +/- 60 months before and after age 62 as placebos. The diamond represents the regression discontinuity estimate at age 62. CCT represents the bandwidth procedure of Calonico, Cattaneo and Titiunik (2014a; 2014b). See Table 2 and the text for more details.

Figure 4 Monthly Mortality Counts in Relation to Age 62, Social Security Master Death File

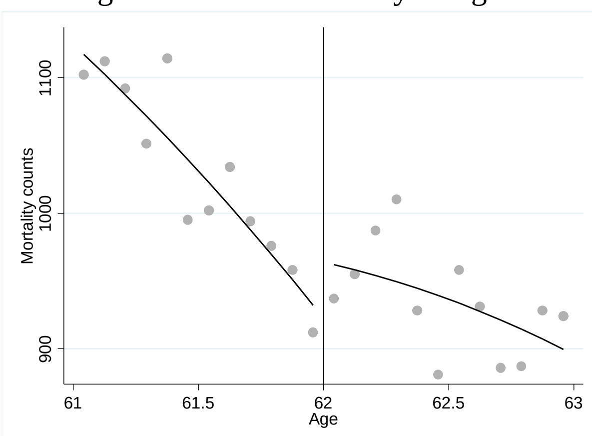
A: All Deaths when Age 62 is the Social Security Eligibility Threshold



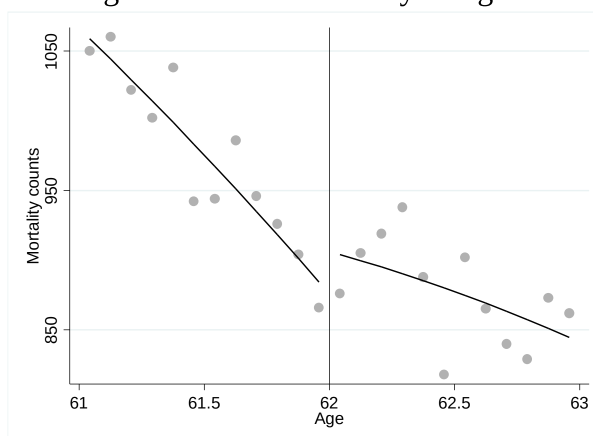
B: Male Deaths when Age 62 is the Social Security Eligibility Threshold



C: All Deaths Prior to 1962: Before Males Eligible for Social Security at Age 62



D: Male Deaths Prior to 1962: Before Males Eligible for Social Security at Age 62



Notes: Data are from the Social Security Death Master File (SSDMF). Panel A and B show reported mortality counts at age 62 using the 1930-1948 birth cohorts. In Panels C and D, reported mortality counts are for deaths from 1940 to 1961 and include deaths without day of month in the death date. All figures show the number of deaths by age measured in months. The best-fit lines are quadratic polynomials fitted on each side of the discontinuity.

Figure 5 Changes in Social Security Claiming and Related Outcomes at Age 62

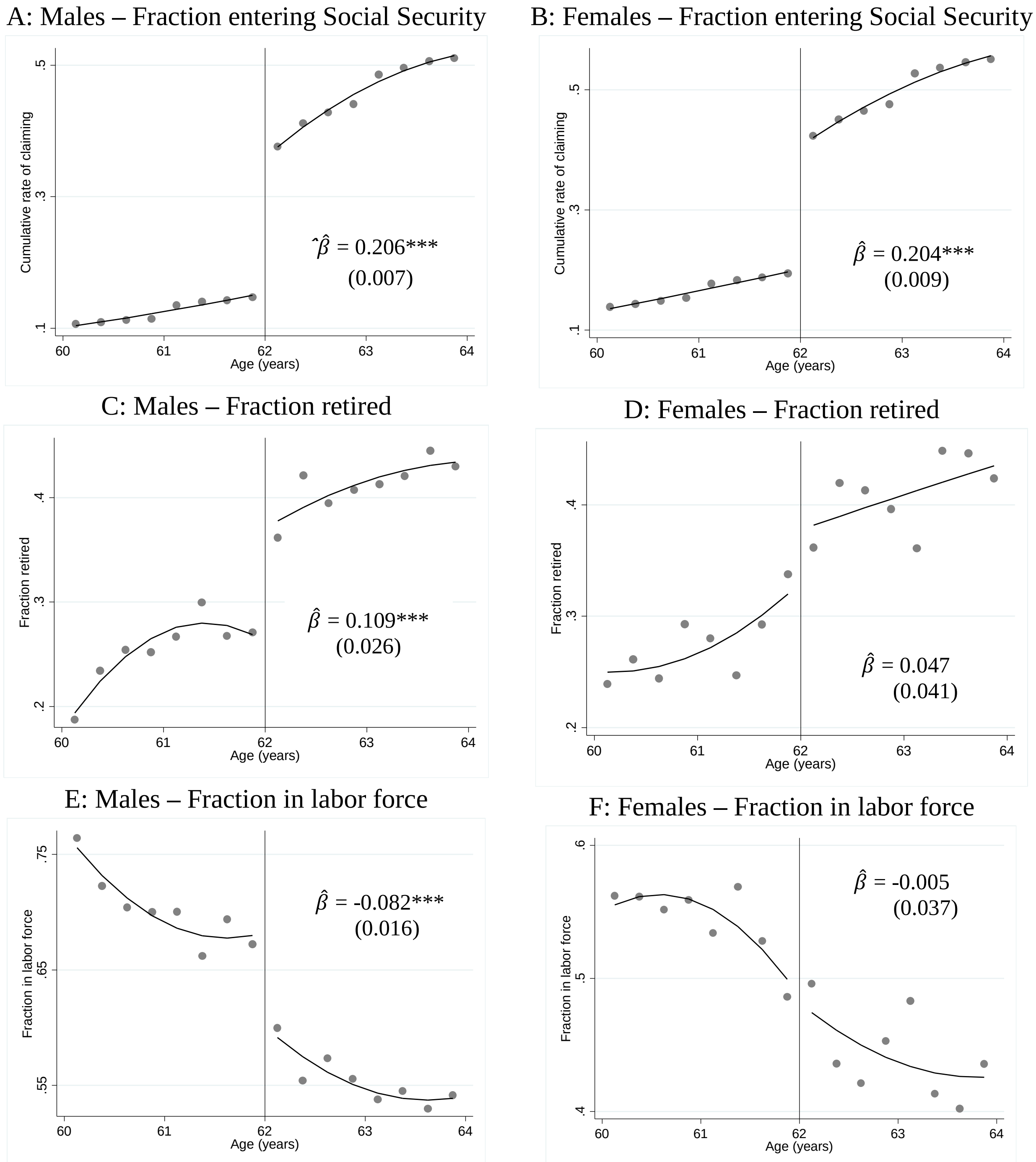
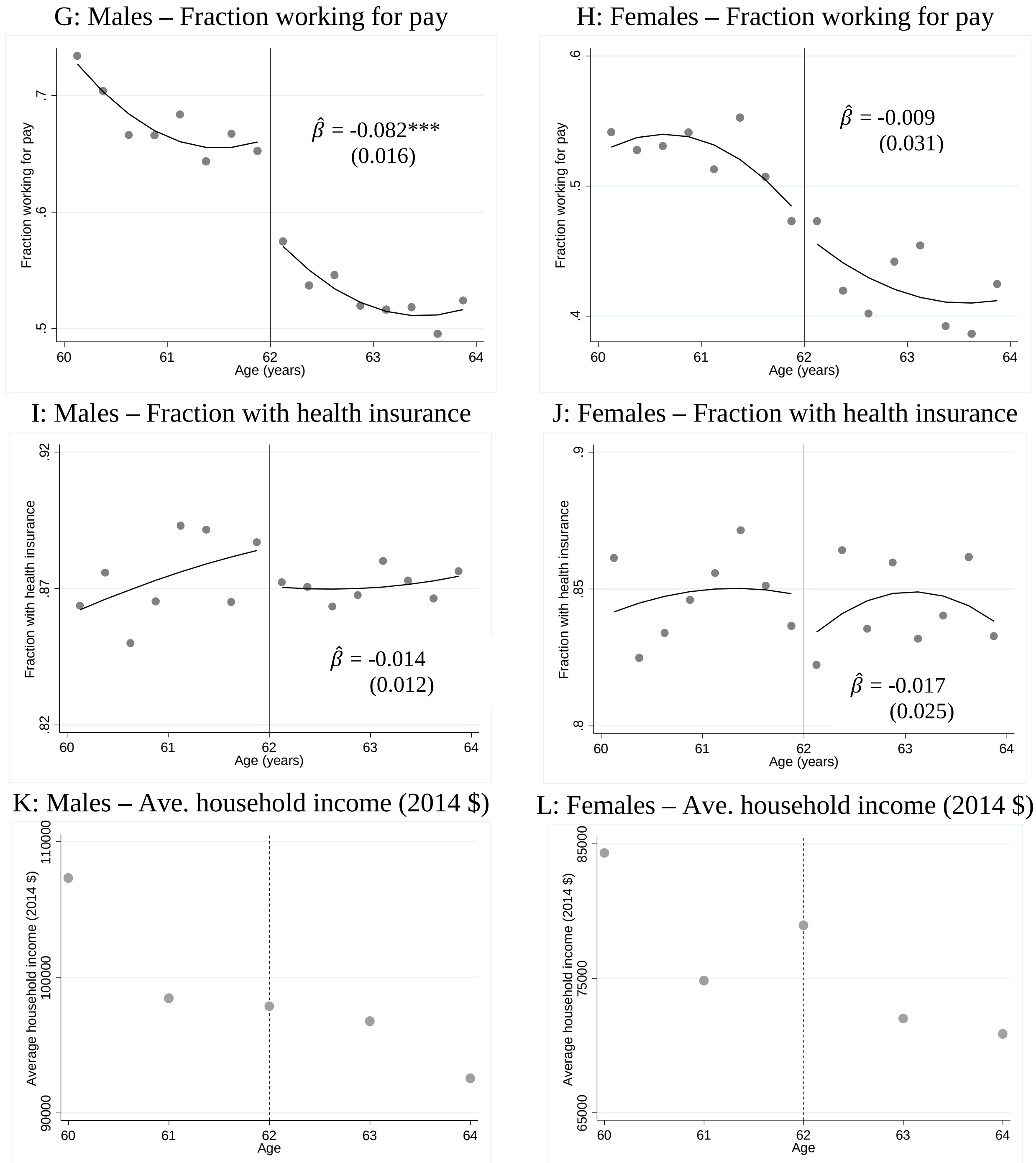
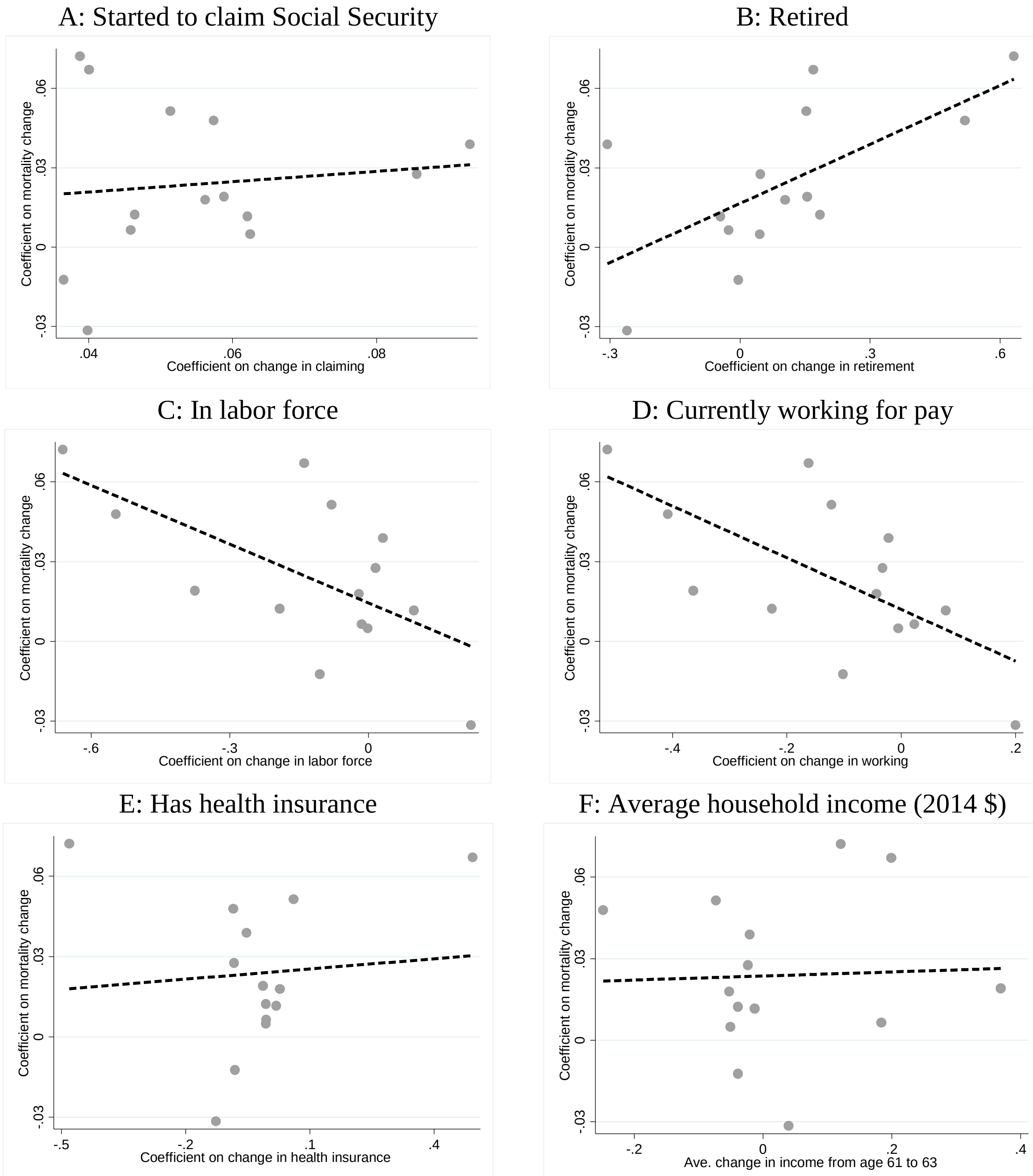


Figure 5 Changes in Social Security Claiming and Related Outcomes at Age 62 (Continued)



Notes: Data are from the RAND Health and Retirement Study Data File (Version N). The variables are described in the appendix. Average household income is shifted back one year, as HRS respondents are asked about income in the previous year (e.g., the income response when interviewed at age 63 is assigned as age 62 income). Income is converted to 2014 dollars using the CPI-U. In Panels A-J, the best-fit lines are quadratic polynomials fitted on each side of age 62. We do not fit lines in Panels K and L because the number of points is too small to accurately do so.

Figure 6 The Relationship Between Changes in Mortality and Retirement Outcomes at Age 62



Notes: These coefficients come from the global quadratic RD regression. Each panel shows the mortality coefficients plotted for 14 distinct demographic subgroups in relation to the horizontal axis and the estimated changes in the retirement-related outcomes for the same 14 groups plotted in relation to the vertical axis. For the non-mortality outcomes, we use data from the RAND Health and Retirement Study Data File (Version N); these variables are described in the appendix. Average household income is shifted back one year, as HRS respondents are asked about income in the previous year (e.g., the income response when interviewed at age 63 is assigned as age 62 income). Income is converted to 2014 dollars using the CPI-U.

Table 1 Summary Statistics, Multiple Cause of Death Data

	All		Males		Females	
	Age 61	Age 62	Age 61	Age 62	Age 61	Age62
Total deaths	785,871	848,728	474,272	510,561	311,599	338,167
Ave. deaths per month	65,489	70,727	39,523	42,547	25,967	28,181
Race						
White (%)	82.7	82.8	83.4	83.5	81.6	81.7
Black (%)	15.3	15.1	14.7	14.5	16.2	16.1
Other race (%)	2.0	2.1	1.9	2.0	2.2	2.2
Marital Status						
Single (%)	9.0	8.9	10.0	9.8	7.5	7.5
Married (%)	60.2	59.5	65.2	65.0	52.5	51.3
Divorced (%)	18.7	18.6	18.9	18.8	18.5	18.4
Widowed (%)	12.1	13.0	5.9	6.4	21.4	22.9
Educational Attainment						
Less than high school (%)	26.4	26.8	27.4	27.6	25.1	25.6
High school graduate (%)	59.3	59.0	56.7	56.5	63.1	62.8
College graduate (%)	14.3	14.2	16.0	15.9	11.8	11.7
Place of Death						
Hospital (%)	59.3	58.6	59.3	58.6	59.4	58.6
Nursing home/institution (%)	6.8	7.4	6.0	6.5	8.0	8.7
Residence or other location (%)	33.9	34.1	34.8	35.0	32.6	32.7
Underlying Cause of Death						
Cancer (%)	36.7	36.6	33.5	33.7	41.4	40.7
Cardiovascular and respiratory conditions (%)	36.2	36.5	39.2	39.1	31.7	32.3
External causes (%)	4.5	4.1	5.3	4.8	3.3	3.1
All other causes (%)	22.7	22.9	22.0	22.2	23.6	24.0

Notes: Data is from restricted-use versions of the Multiple Cause of Death data held at the National Center for Health Statistics, including the 1921 to 1948 cohorts. The sample includes all deaths for cohorts born between 1921 and 1948. Marital status is missing in 0.9 percent of cases and place of death is missing in 0.7 percent of cases. As educational attainment is only available after 1989, it is available for 69.1 percent of all cases. The category "Residence or other location" includes those classified as "Dead on arrival" at the hospital.

Table 2 Regression Estimates of Increase in Mortality at Age 62

Regression type	All (1)	Males (2)	Females (3)
Global parametric regressions (bandwidth = 12 months)			
Quadratic regression	0.0135***	0.0185***	0.0058
	(0.0043)	(0.0049)	(0.0049)
Cubic regression	0.0197***	0.0236***	0.0138***
	(0.0049)	(0.0060)	(0.0047)
Quartic regression	0.0193***	0.0243***	0.0116***
	(0.0051)	(0.0082)	(0.0043)
Local nonparametric regressions			
Local linear using data-driven bandwidth	0.0142***	0.0215***	0.0103***
	(0.0036)	(0.0041)	(0.0030)
Data-driven bandwidth	10 months	7 months	6 months
Local quadratic using data-driven bandwidth	0.0194***	0.0233***	0.0131***
	(0.0039)	(0.0058)	(0.0026)
Data-driven bandwidth	7 months	7 months	8 months

Notes: ** denotes p<0.05, *** denotes p<0.01. Data is from restricted-use versions of the Multiple Cause of Death data and include the 1921 to 1948 birth cohorts. The global parametric regressions allow for the polynomial to vary either side of the discontinuity and we report robust standard errors. The nonparametric regressions are estimated using the "robust data-driven" procedures of Calonico, Cattaneo and Titiunik (2014a; 2014b). We use a triangular kernel, robust standard errors, and their bandwidth selection and bias correction procedures. See text for more details.

Table 3 Estimated Change in Mortality at Age 62, Male Subgroups

	Local linear (1)	Global quadratic (2)	Fraction of deaths (3)
Marital status			
Married	0.0130**	0.0081	65.1%
	(0.0058)	(0.0057)	
Not married	0.0415***	0.0377***	34.9%
	(0.0079)	(0.0107)	
- Single	0.0558***	0.0514***	9.9%
	(0.0056)	(0.0111)	
- Divorced	0.0305***	0.0337**	18.9%
	(0.0111)	(0.0137)	
- Widowed	0.0330	0.0262	6.2%
	(0.0202)	(0.0248)	
Race			
White	0.0237***	0.0209***	83.5%
	(0.0042)	(0.0053)	
Non-white	0.0078	0.0068	16.5%
	(0.0104)	(0.0115)	

	Local linear (4)	Global quadratic (5)	Fraction deaths (6)
Education			
Did not complete high school	0.0303***	0.0275**	27.6%
	(0.0103)	(0.0115)	
Completed high school, not college	0.0087	0.0099	56.5%
	(0.0050)	(0.0066)	
Completed college	0.0146	0.0187	15.9%
	(0.0146)	(0.0181)	
Place of death			
Out of hospital/ institution	0.0339***	0.0323***	34.6%
	(0.0074)	(0.0073)	
In hospital	0.0124	0.0130	58.5%
	(0.0064)	(0.0070)	
In nursing home/ institution	-0.0022	-0.0079	6.2%
	(0.0161)	(0.0194)	

Notes: ** denotes p<0.05, *** denotes p<0.01. We show coefficients and standard errors for local linear (using bandwidths selected using the procedures of Calonico, Cattaneo, Titiunik (2014a; 2014b)) and global quadratic regressions (using a 12-month bandwidth). The fraction of deaths is for male deaths at age 61 and 62. See Table 2 and the text for more details.

Web Appendix for: "The Mortality Effects of Retirement: Evidence from Social Security Eligibility at Age 62"

Maria D. Fitzpatrick and Timothy J. Moore

DATA APPENDIX

A1. Cause of Death Categories in the MCOD Files

The MCOD files are explained in Section 3. Here we provide more details about how we create consistent cause-of-death categories across the 9th and 10th Versions of the International Classification of Disease (ICD-9 and ICD-10). We use the classification of Evans and Moore (2012) that was developed using epidemiological studies and cause-of-death recodes. For underlying cause of death, there is a 34-cause recode for ICD-9 ("Recode34") and a 39-cause recode for ICD-10 ("Recode39"). Underlying causes of death are categorized as follows:

- Heart and lung conditions: Heart attacks (ICD-9: 410; ICD-10: I21); COPD (ICD-9: 490-496; ICD-10: J40-J43, J44.0-J44.7, J44.9, J45-J48); Other heart and lung conditions (ICD-9: 390-398, 402, 404, 411-439; ICD-10: I00-I09, I11, I13, I20, I22-I51, I60-I69);
- Cancers: Lung cancer (ICD-9: 162.2-162.5, 162.8-162.9; ICD-10: C34); Other cancers (ICD-9: Recode34 = 4, 5, 8, 9 or 11 and not assigned to lung cancer; ICD-10: Recode39 = 4-7, 10-13 or 15 and not assigned to lung cancer);
- External causes: Traffic fatalities (ICD-9: Recode34 = 33, Recode39 = 38); Other external causes (ICD-9: Recode34 = 34-36, Recode39 = 39-41).

All other causes of death are included in the "other" category. This accounts for approximately 22 percent of all deaths during the sample period.

A2. Social Security Administrative Data Extract

We use two datasets drawn from SSA program management systems. The Master Beneficiary Record is used to manage Social Security activity. Birth date is taken from the Numident File, which contains vital statistics information for the entire population and is used to manage Social Security Numbers. We use a one percent extract of these datasets that are based on the last two digits of Social Security Numbers, which are essentially randomly assigned.

We focus on the first date that a beneficiary is eligible to receive Social Security and whether the claim is made on their own record (i.e., as a worker beneficiary) or someone else's record (i.e., as a dependent). Date of birth is used to calculate the age of claiming, and year of birth is used to restrict the sample to those born between 1921 and 1948 (the same sample available in the MCOD Files). We combine claiming for Disability Insurance or Retirement and Survivors' Insurance in our figures, as individuals can apply for both simultaneously and commonly transition from one program to another. However, we do confirm that the spike at 62 is due to new Retirement and Survivors' Insurance claims.

To convert this information into rates, we use monthly extracts of the Current Population Survey to calculate the sex- and age-specific population numbers for these cohorts. While we could use mortality information from the Numident File to calculate these numbers, we use survey estimates as a review by the Social Security Administration Office of the Inspector General (2012) suggests that the Numident mortality numbers are subject to measurement error.

A3. Social Security Death Master File (SSDMF) Data

The SSDMF is a dataset of people whose deaths were reported to SSA since 1962. The most common sources of information are relatives of deceased individuals, funeral directors, financial institutions and postal authorities. It was first made available in 1988. Variables in the SSDMF include decedents' name and dates of birth and death, although for deaths prior to 1988 only month and year are generally provided for these dates (Hill and Rosenwaike, 2002).

We use a version of the SSDMF that contains deaths through 2012. Decedents' first names are used to determine their likely sex. We do so by combining the SSDMF with data on the frequency of first names for boys and girls that come from Social Security card applications.[1] For our main analysis we use deaths from 1988, as few deaths prior to 1988 include information on exact dates of birth and death. Therefore, our main sample is for decedents born between 1930 and 1948, which results in a consistent sample for two years either side of the age-62 discontinuity. Approximately 87 percent of deaths have the necessary date information; of those, we can use first name to assign sex for approximately 95 percent of decedents.[2]

The SSDMF contains approximately 675,000 deaths before 1962. Nearly all occur after the introduction of Social Security benefits in 1940. Day of death is generally not available for these deaths, so age of death is based on the month and year information.

[1] Counts of names used at least five times in a year are at https://www.ssa.gov/oact/babynames/limits.html. We use birth cohorts from 1880 (the earliest cohort) to 1948 (the youngest cohort used in our main analysis). For names used for both boys and girls, we assign the name to the sex that most commonly use it. This type of overlap is not a major concern, as names are assigned to the wrong sex for less than two percent of individuals.

[2] A high number of deaths are listed as occurring on the 15th of the month: six percent are on that date, which is more than double the fraction on other days of the month. This spike is not evident in the MCOD Files, so is likely that it is due to it being assigned when day of death is missing. We exclude deaths on the 15th, as well as days with missing day or month of birth or death (representing another seven percent of deaths).

A4. Health and Retirement Study (HRS) Data

The HRS provides comprehensive information on Americans in households aged 50 and older, including their demographic characteristics, employment, retirement plans, income sources, health insurance and health outcomes. It began in 1992 with a nationally representative sample of individuals living in households who were born between 1931 and 1941, who have since been interviewed every two years. Older and younger cohorts have since been added and subsequently interviewed very two years. We use the RAND HRS File (Version N), which contains data through 2012 and cohorts born between 1924 and 1959 (Chien et al., 2014). In the RAND HRS File, variables are created that are internally consistent and consistent across waves.

Age at interview is calculated using date of birth and final date of interview, and respondent-specific sample weights are used throughout. The work-related variables are: retired (based on labor force status, where respondents defined as retired mention retirement and currently are not working and or looking for work); currently in labor force (using the variable that follows the Bureau of Labor Statistics definition); and working for pay (if respondent is currently working for pay). Household income and Social Security income are from the previous calendar year and converted to 2014 values using the CPI-U. For these variables, age is calculated for the previous year (i.e., the year the income was earned). The health insurance variables are: has health insurance coverage (if has government health insurance or a health insurance plan); and the number of health insurance plans a respondent reports.

For these variables, we aggregate the different waves and use the fraction of respondents or average response at each age of interview. In the two years before age 62 (i.e., age 60 to 63), we have a sample of 12,632 males and 14,119 females. For Social Security claiming, age of first claiming is retrospective and does not vary across waves. We therefore use a slightly different sample that omits respondents who have not yet reached an age where they eligible for Social Security or who are less likely to accurately recall their exact claiming date. We use respondents whose highest age at interview is between 63 and 80. In this sample, we have a sample of 6,897 males and 7,462 females, with the smaller sample due to not having repeated observations from the same respondents.

A5. National Health and Nutrition Examination Survey (NHANES) Data

NHANES measures the health status of the American civilian population via both interviews and physical examinations. A subset of participants in the NHANES 2003/04 and 2005/06 cycles were asked to wear Actigraph accelerometers on their right hip for seven days during waking hours except when it could get wet (e.g., bathing). Data was recorded in one minute increments. By calibrating accelerometer readings to different activity levels, researchers have used these data to examine sedentary behavior and physical activity levels (e.g., Matthews et al, 2008; Healy et al, 2011).

We examine the fraction of time that participants were sedentary, and follow previous studies by defining being sedentary as whenever activity falls below 100 counts per minute. We focus on NHANES participants aged 60 to 63 years. We include them if they had data judged to be valid between 8am and 6pm on weekdays. This restriction matters because not all participants consistently wore their accelerometers. Following previous studies, we assume an accelerometer is being worn when there was at least two minutes of activity in an hour (Matthews et al., 2008; Troiano et al., 2008). Like these studies, we also exclude data where the device was found to be not calibrated (upon its return) or the data was classified by NHANES as unreliable. There are 179 observations (92 males, 87 females). For the ages that we are interested in, there are not marked differences in non-wear times by age.

We provide evidence that at age 62 there is an increase in the fraction of time that males are sedentary. It is difficult to link that directly to Social Security claiming, as the focus of the NHANES is on health-related outcomes. There is no information on whether people are receiving Social Security. Information about work and retirement is extremely limited, although the fraction of all respondents who are not working because of retirement does go up by 14 percentage points at age 62.

Additional References

Healy, Genevieve N., Charles N. Matthews, David W. Dunstan, et al. 2011. Sedentary Time and Cardio-metabolic Biomarkers in US Adults: NHANES 2003–06. *European Heart Journal*, 32(5): 590-597.

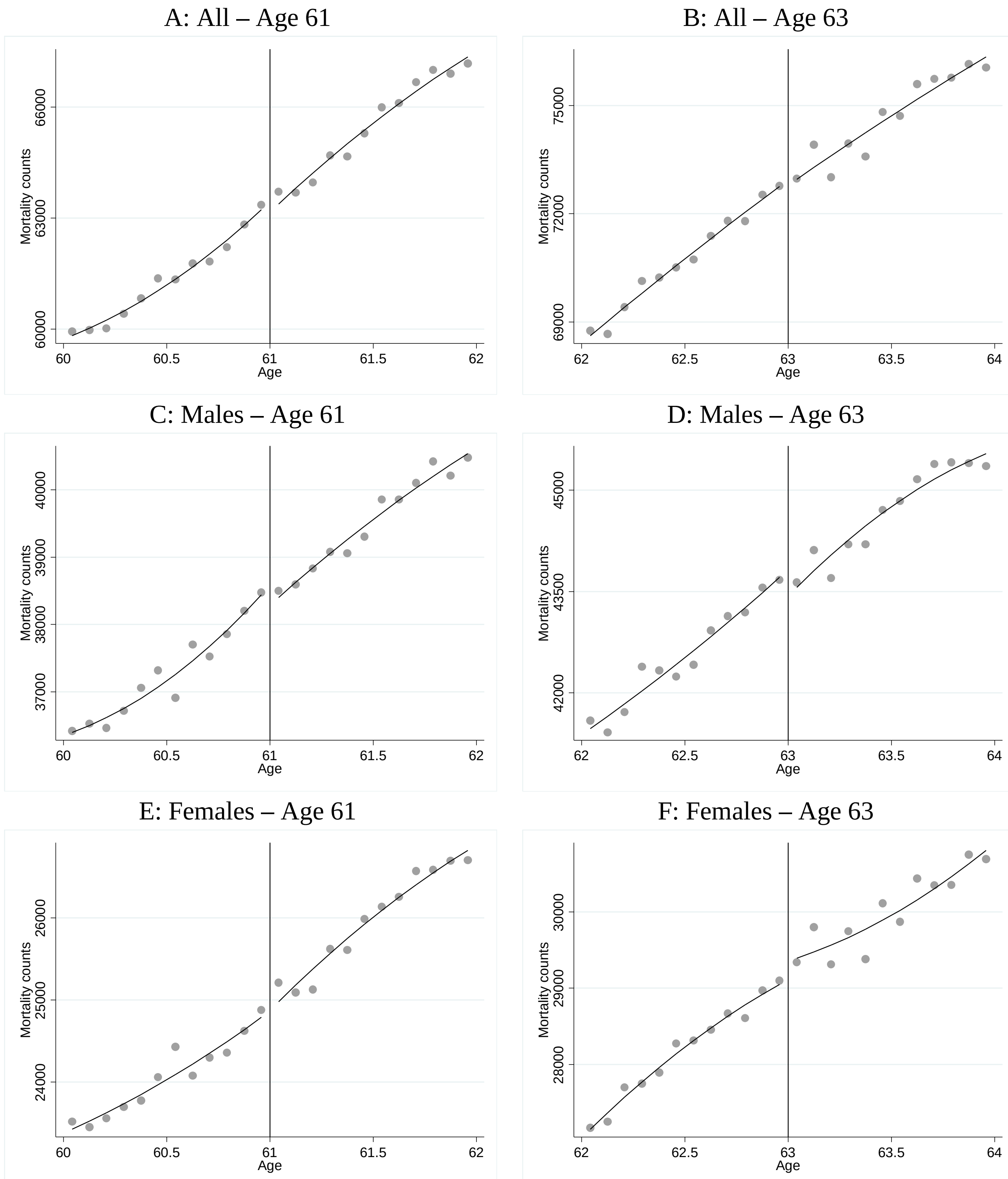
Matthews, Charles E., Kong Y. Chen, Patty S. Freedson, et al. 2008. Amount of Time Spent in Sedentary Behaviors in the United States, 2003–2004. *American Journal of Epidemiology*, 167(7): 875-881.

Social Security Administration Office of the Inspector General. 2012. *Title II Deceased Beneficiaries Who Do Not Have Death Information on the Numident. Audit Report A-09-11-2271*. Social Security Administration Office of the Inspector General, Baltimore MD.

Troiano, Richard, David Berrigan, Kevin Dodd, et al. 2008. Physical Activity in United States Measured by Accelerometer. *Medicine and Science in Sports and Exercise*, 40(1): 181-88.

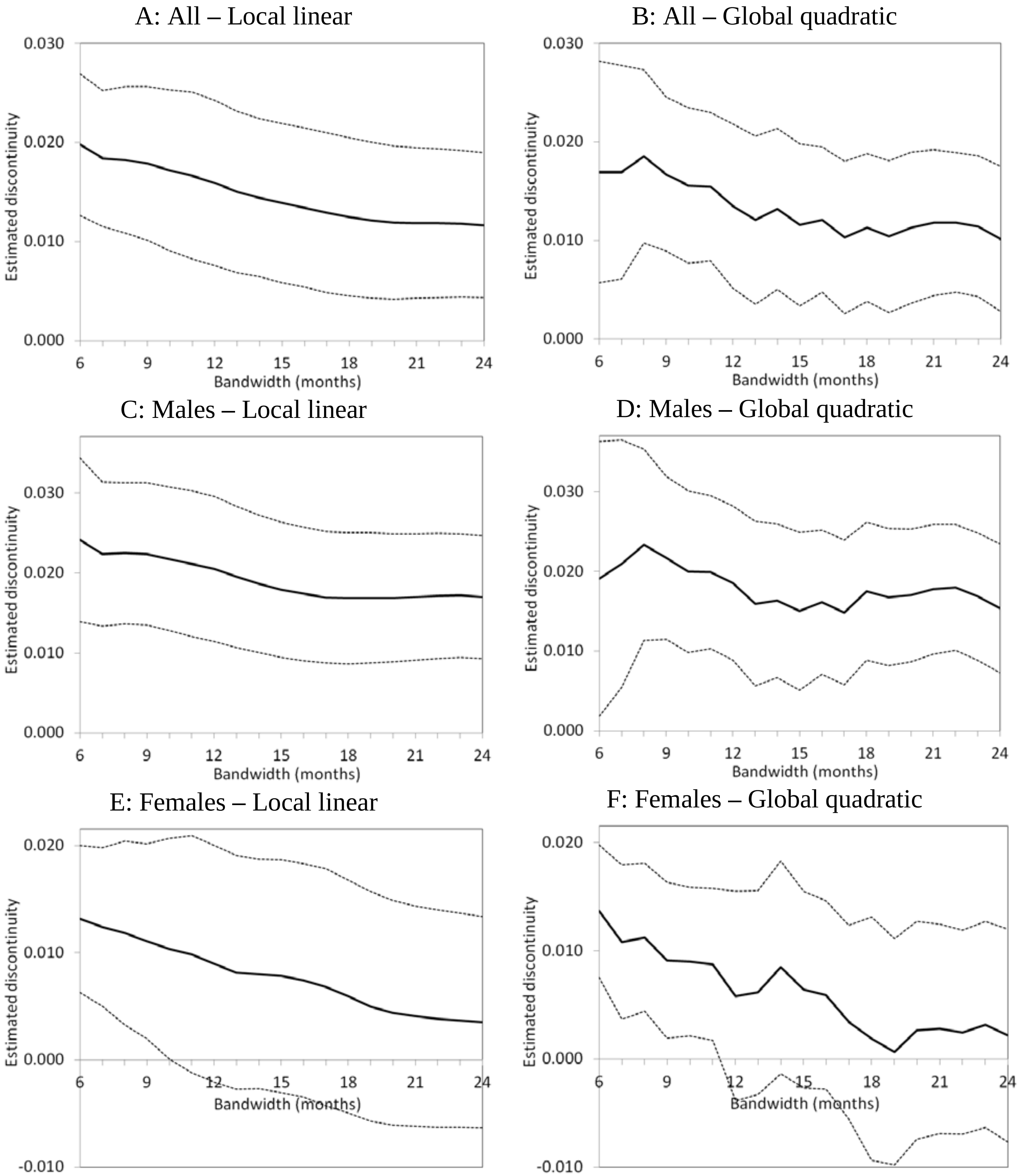
RESULTS APPENDIX

Appendix Figure A1 Monthly Mortality Counts in Relation to Turning Age 61 and 63



Notes: Data is from restricted-use versions of the Multiple Cause of Death data held at the National Center for Health Statistics. Reported mortality counts are for the 1921 to 1948 cohorts. The figures report the number of deaths by age measured in months. The best-fit lines are quadratic polynomials fitted on each side of age 62.

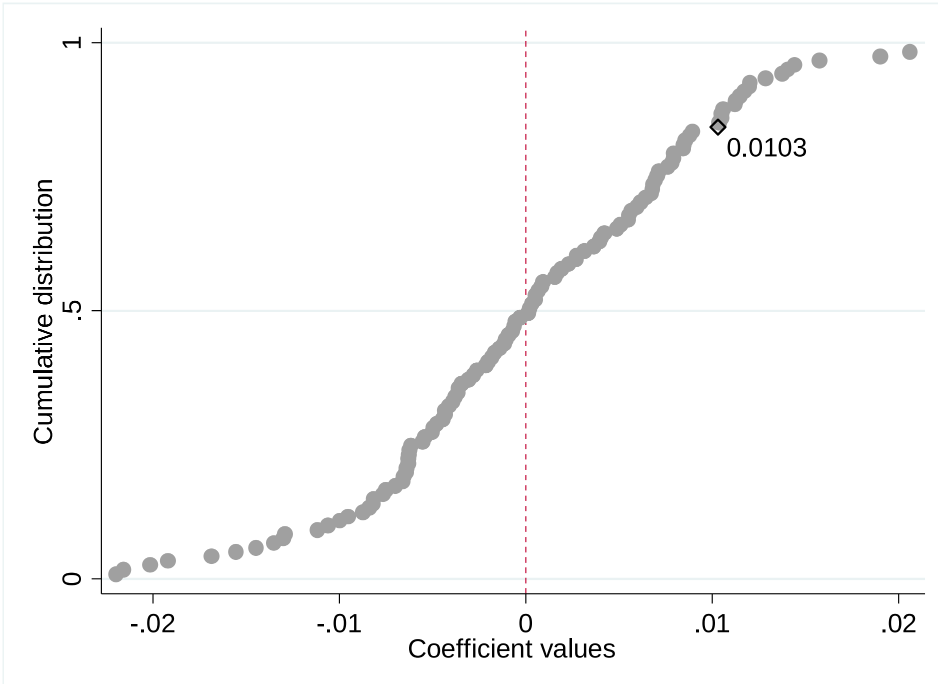
Appendix Figure A2 Robustness of Estimates to Bandwidth



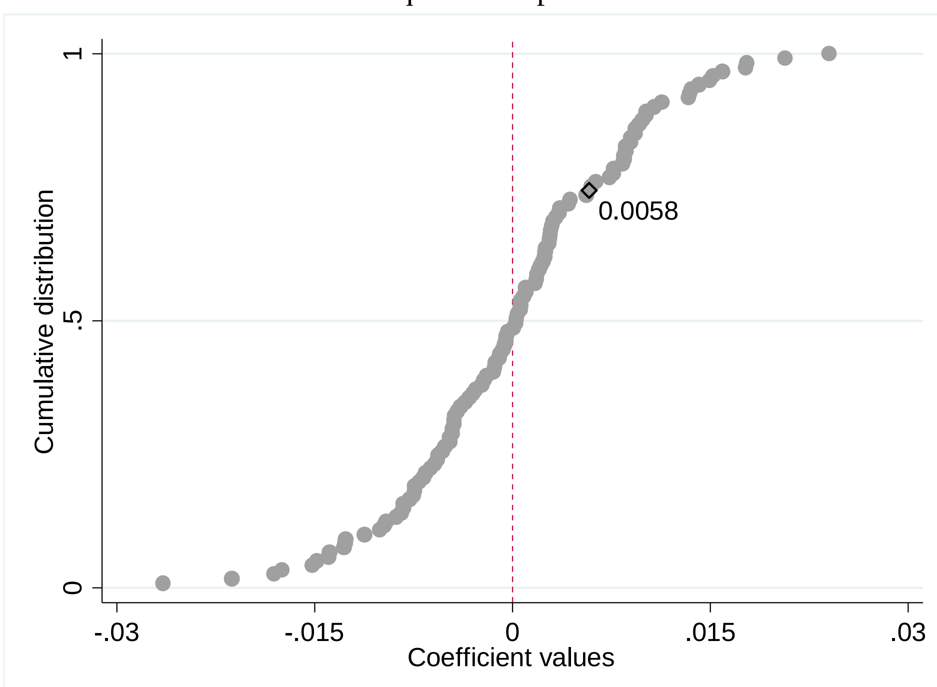
Notes: The figures show the coefficient estimates and 95 percent confidence intervals at different bandwidths between six months and two years. Data is from restricted-use versions of the Multiple Cause of Death data held at the National Center for Health Statistics, including the 1921 to 1948 cohorts. The local linear regressions are estimated using the “robust data-driven” procedures of Calonico, Cattaneo and Titiunik (2014a; 2014b). We use a triangular kernel, robust standard errors, and their bias correction procedures. The global parametric regressions allow for the polynomial to vary either side of the discontinuity and we report robust standard errors. The estimates are stable and statistically significant for the full sample and for males, but not for females.

Appendix Figure A3 The Distribution of Placebo Female Mortality Estimates for +/-60 Months of Age 62 Compared to the Estimate at Age 62 *(Diamond, labeled)*

A: Local linear specification



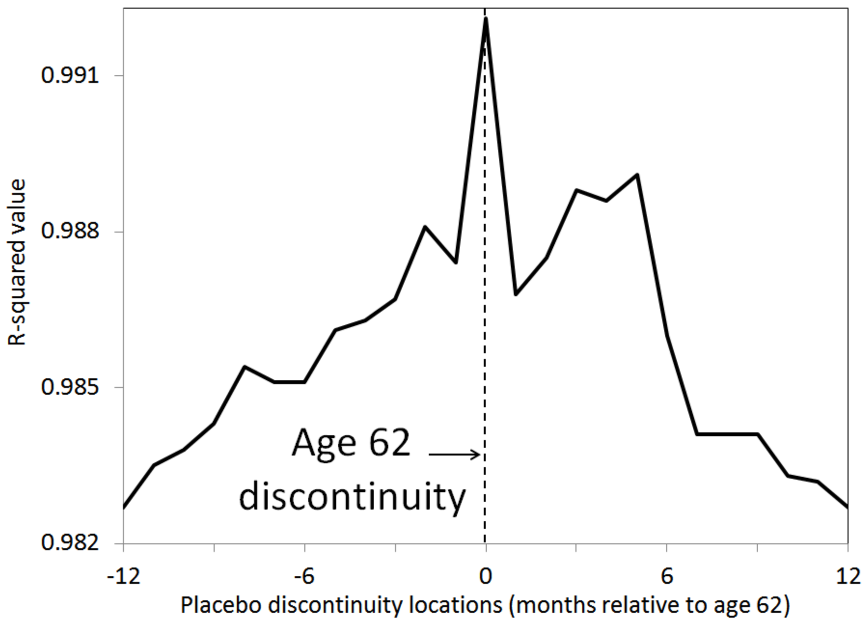
B: Global quadratic specification



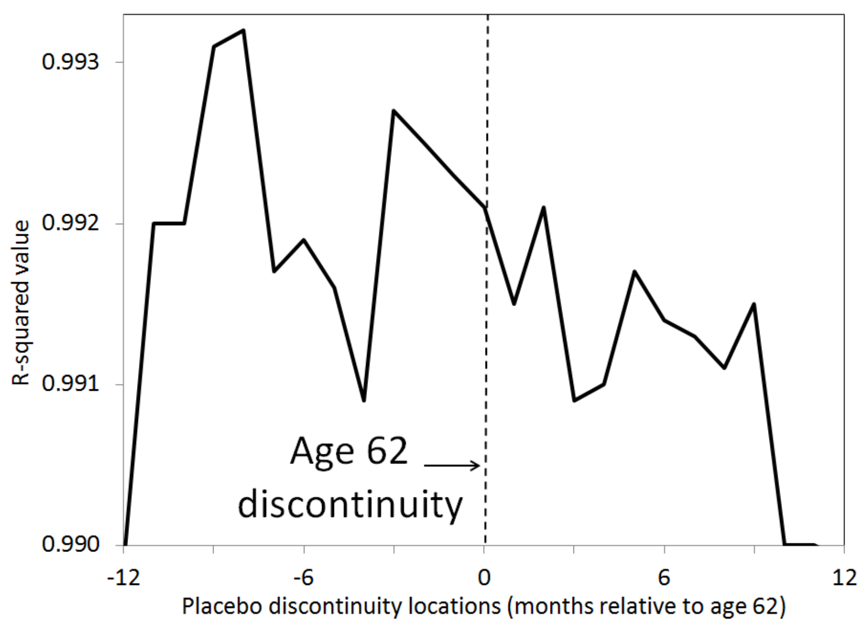
Notes: Data is from restricted-use versions of the Multiple Cause of Death data held at the National Center for Health Statistics including the 1921 to 1948 cohorts. The figures show the conditional density functions of point estimates using each month +/- 60 months before and after age 62 as placebos. The red diamond represents the regression discontinuity estimate at age 62; the values of these estimates are shown in the figures.

Appendix Figure A4 R-squared as Function of Location of the Discontinuity, Global Quadratic

A: Males

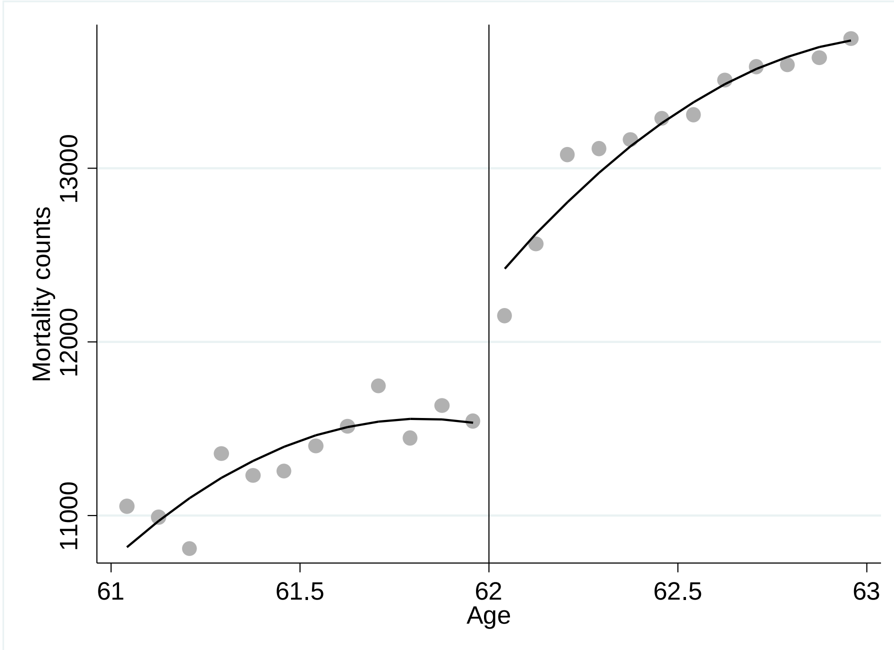


B: Females



Notes: We compare R-squared when the discontinuity is at age 62 to when it is placed at other locations in a 24-month window around age 62. We use the global quadratic specification and data from restricted-use versions of the Multiple Cause of Death data held at the National Center for Health Statistics, which include deaths for the 1921 to 1948 cohorts. The figures show the R-squared statistics as a measure of model fit when the discontinuity is at each age, in months. The dashed line shows the regression discontinuity estimate at age 62.

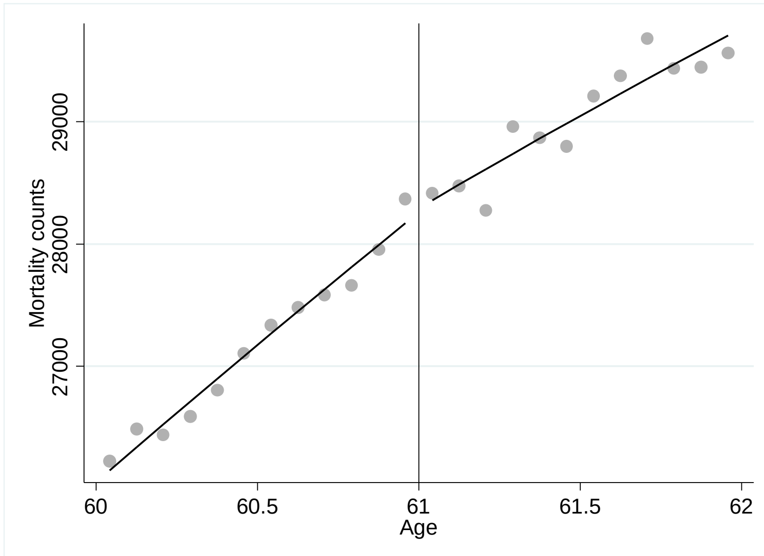
Appendix Figure A5 Female Mortality Counts around Age 62, Social Security Master Death File



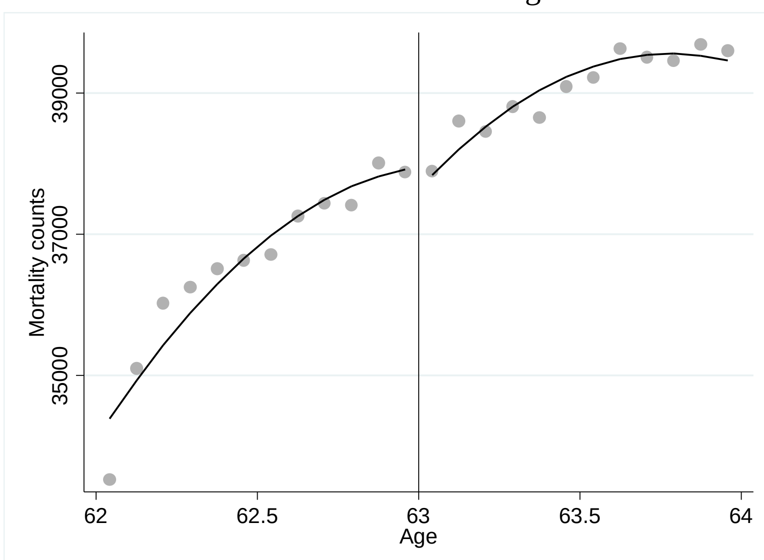
Notes: Data are from the SSDMF, and include deaths for the 1930-1948 birth cohorts. The figures show the number of deaths by age measured in months. Sex is sometimes unknown, so the counts in this figure and Panel B of Figure 4 do not sum to the counts in Panel A of Figure 4. Best-fit lines are quadratic polynomials fitted on each side of age 62.

Appendix Figure A6 Placebo Mortality Counts from the Social Security Master Death File

A: Deaths in Relation to Age 61

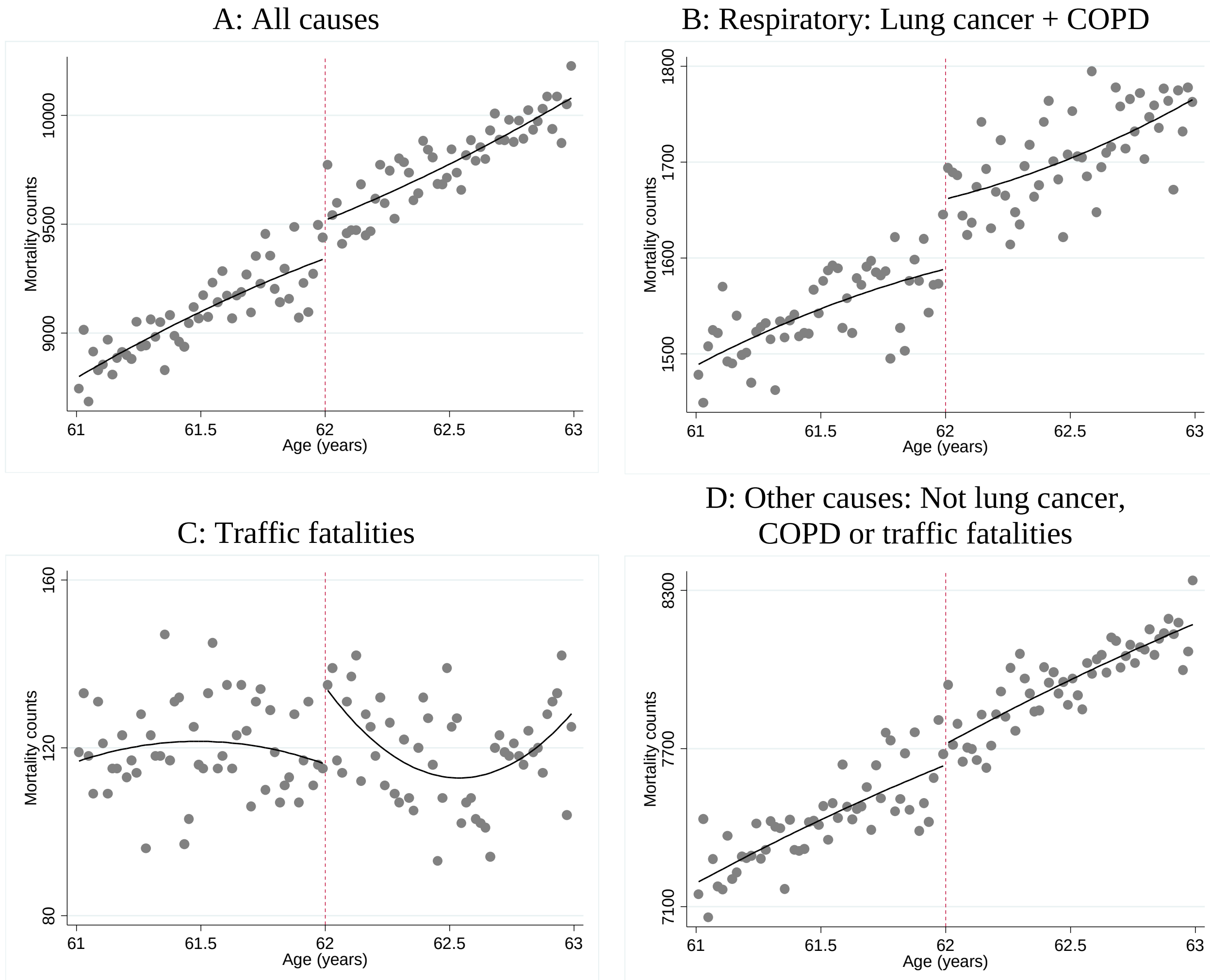


B: Deaths in Relation to Age 63



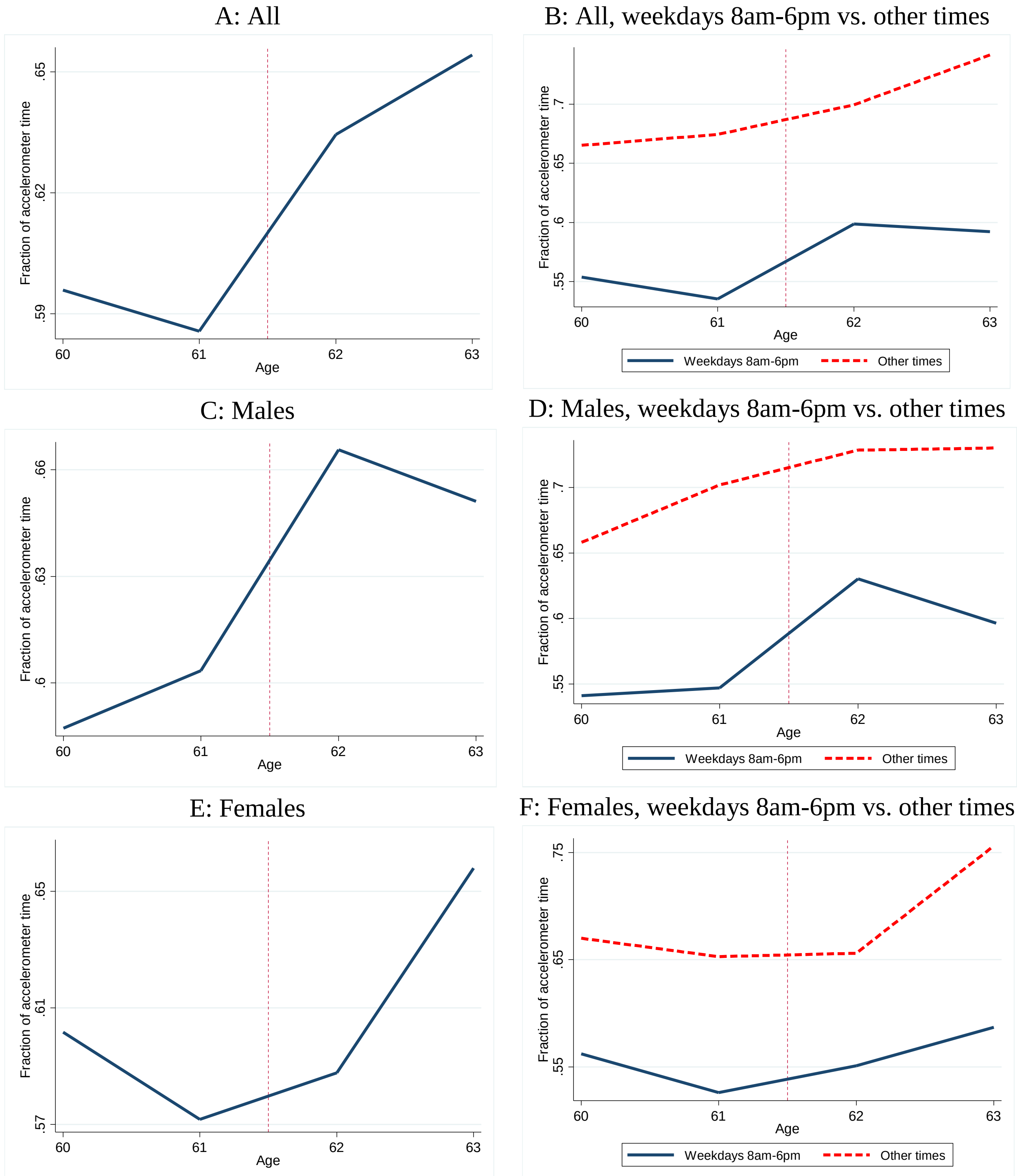
Notes: Data are from the SSDMF. Panel A shows reported mortality counts at age 61 using the 1931-1948 birth cohorts and Panel B shows reported mortality counts at age 63 using the 1929-1948 birth cohorts; these samples reflect exact date of birth generally not being available prior to 1988. Both figures show the number of deaths by age measured in months. The best-fit lines are quadratic polynomials fitted on each side of the discontinuity.

Appendix Figure A7 Weekly Mortality Counts for Males, By Selected Causes of Death



Notes: Data is from restricted-use versions of the Multiple Cause of Death data held at the National Center for Health Statistics. Reported mortality counts are for the 1921 to 1948 cohorts. The figures report the number of deaths by age measured in weeks. The best-fit lines are quadratic polynomials fitted on each side of age 62.

Appendix Figure A8 Sedentary Time by Age and Sex, NHANES 2003-06



Notes: These figures are based on Actigraph accelerometer data collected from National Health and Nutritional Examination Survey (NHANES) 2003/04 and 2005/06 participants. Sedentary is defined as less than 100 counts per minute and outcome measured is the fraction of sedentary time when the accelerometer was worn. Participants are included if they were aged 60 to 63 and wore their accelerometer between 8am and 6pm Monday and Friday. The sample consists of 92 males and 87 females.

Appendix Table A1 Summary Statistics, Health and Retirement Study (HRS) Sample

	All		Males		Females	
	Age 61	Age 62	Age 61	Age 62	Age 61	Age62
Race						
Nonwhite (%)	14.8	15.7	14.1	14.3	15.4	17.1
	(35.5)	(36.4)	(34.8)	(35.0)	(36.1)	(37.6)
Marital Status						
Single (%)	6.8	8.1	7.0	9.3	6.6	7.1
	(25.2)	(27.3)	(25.5)	(29.1)	(24.8)	(25.7)
Married (%)	71.7	70	78.5	77.2	65.9	63.4
	(45)	(45.8)	(41.1)	(42.0)	(47.4)	(48.2)
Divorced (%)	14	13.2	11.4	11.0	16.2	15.2
	(34.7)	(33.9)	(31.8)	(31.3)	(36.8)	(35.9)
Widowed (%)	7.5	8.6	3.1	2.4	11.2	14.1
	(26.3)	(28)	(17.3)	(15.4)	(31.6)	(34.8)
Educational Attainment						
Less than high school (%)	16.8	17.3	15.9	16.7	17.6	18.0
	(37.4)	(37.9)	(36.6)	(37.3)	(38.1)	(38.4)
High school graduate (%)	58.0	58.2	55.3	53.9	60.3	62.0
	(49.4)	(49.3)	(49.7)	(49.9)	(48.9)	(48.5)
College graduate (%)	25.2	24.5	28.8	29.4	22.2	20.0
	(43.4)	(43.0)	(45.3)	(45.6)	(41.5)	(40.0)
Social Security and Economic Characteristics						
Claim Social Security (%)	18.3	47.4	14.8	44.8	21.2	49.6
	(38.6)	(49.9)	(35.5)	(49.7)	(40.9)	(50.0)
Retired (%)	28.3	39.7	27.7	39.6	28.8	39.7
	(45.0)	(48.9)	(44.7)	(48.9)	(45.3)	(48.9)
In labor force (%)	60.0	50.9	68.2	57.1	53.0	45.2
	(49.0)	(50.0)	(46.6)	(49.5)	(49.9)	(49.8)
Working for pay (%)	58.1	48.7	66.1	54.5	51.2	43.5
	(49.3)	(50.0)	(47.3)	(49.8)	(50.0)	(49.6)
Has health insurance (%)	86.8	85.6	88.4	86.8	85.4	84.5
	(33.9)	(35.1)	(32.0)	(33.8)	(35.3)	(36.2)
Number health ins. plans	0.80	0.75	0.81	0.77	0.79	0.73
	(0.56)	(0.56)	(0.54)	(0.56)	(0.57)	(0.56)
Household income (2014 $)	86,069	87,771	86,069	87,771	86,069	87,771
	(133,234)	(132,131)	(133,234)	(132,131)	(133,234)	(132,131)
Number of observations	7,266	6,608	3,191	3,013	4,075	3,595

Notes: Data are from the RAND HRS Data File (Version N), a compilation that contains data from respondents interviewed between1992 and 2012. The variables are described in the data appendix. Average household income is shifted back one year, as HRS respondents are asked about income in the previous year (e.g., the income response when interviewed at age 63 is assigned as age 62 income). Income is converted to 2014 dollars using the CPI-U.

Appendix Table A2 Full Set of Coefficients from Global Quadratic Regression

Regression type	All (1)	Males (2)	Females (3)
Post62 [Primary coefficient of interest]	0.0135***	0.0185***	0.0058
	(0.0043)	(0.0049)	(0.0049)
AgeDeath	0.0042***	0.0039***	0.0047**
	(0.0013)	(0.0012)	(0.0019)
AgeDeath^2 (x100)	-0.0111	-0.0085	-0.0150
	(0.0113)	(0.0096)	(0.0175)
Post62 x *AgeDeath*	0.0016	0.0004	0.0035
	(0.0016)	(0.0018)	(0.0021)
Post62 x *AgeDeath*^{2} (x100)	0.0068	0.0126	-0.0021
	(0.0130)	(0.0142)	(0.0188)
Constant	11.120***	10.612***	10.199***
	(0.003)	(0.003)	(0.004)
R-squared	0.995	0.992	0.992
Number of observations	24	24	24
p-value on test of joint statistical significance of the coefficients from the *Post62* interactions	0.59	0.65	0.09

Notes: ** denotes p<0.05, *** denotes p<0.01. Data is from restricted-use versions of the Multiple Cause of Death data held at the National Center for Health Statistics. The regressions allow for the polynomial to vary either side of the discontinuity and we report robust standard errors. Mortality is measured at the monthly level. In order to test whether the age-mortality relationship changes after age 62, we provide the p-value from the joint hypothesis test that the linear and quadratic terms that are interacted with the age-62 dummy are zero (i.e., *Post62* x *AgeDeath* = *Post62* x *AgeDeath*^{2} = 0). See Table 2 and the text for more details.

Appendix Table A3 Robustness of Regression Estimates

	Main estimate (1)	Year-of-birth fixed effects (2)	Month-of-death fixed effects (3)	Using daily Counts (4)	Using weekly counts (5)	Using SS eligibility dates (6)
			A: Full sample			
Global quadratic	0.0135***	0.0132***	0.0133***	0.0132***	0.0137**	0.0134
	(0.0043)	(0.0039)	(0.0041)	(0.0049)	(0.0067)	(0.0084)
Global cubic	0.0197***	0.0195***	0.0213***	0.0204***	0.0200**	0.0254***
	(0.0049)	(0.0043)	(0.0039)	(0.0067)	(0.099)	(0.0068)
Global quartic	0.0193***	0.0187***	0.0208***	0.0224***	0.0235**	0.0333***
	(0.0051)	(0.0043)	(0.0041)	(0.0081)	(0.0114)	(0.0065)
Local linear	0.0142***	--	--	0.0135***	0.0223**	0.0166**
	(0.0036)			(0.0047)	(0.0105)	(0.0074)
Bandwidth	10 months			305 days	18 weeks	8 months
Local quadratic	0.0194***	--	--	0.0221***	0.0239**	0.0359***
	(0.0039)			(0.0077)	(0.0115)	(0.0043)
Bandwidth	7 months			218 days	27 weeks	6 months
			B: Males			
Global quadratic	0.0185***	0.0176***	0.0187***	0.0167***	0.0173*	0.0227**
	(0.0049)	(0.0043)	(0.0045)	(0.0058)	(0.0090)	(0.0092)
Global cubic	0.0236***	0.0220***	0.0251***	0.0252***	0.0243*	0.0348***
	(0.0060)	(0.0051)	(0.0048)	(0.0076)	(0.0129)	(0.0084)
Global quartic	0.0243***	0.0222***	0.0250***	0.0265***	0.0286*	0.0459***
	(0.0082)	(0.0066)	(0.0065)	(0.0092)	(0.0154)	(0.0078)
Local linear	0.0215***	--	--	0.0135***	0.0162*	0.0250***
	(0.0041)			(0.0047)	(0.0084)	(0.0085)
Bandwidth	7 months			225 days	46 weeks	9 months
Local quadratic	0.0233***	--	--	0.0221***	0.0263*	0.0472***
	(0.0058)			(0.0077)	(0.0143)	(0.0051)
Bandwidth	7 months			218 days	36 weeks	6 months
			C: Females			
Global quadratic	0.0058	0.0065	0.0049	0.0071	0.0074	-0.0007
	(0.0049)	(0.0045)	(0.0050)	(0.0078)	(0.0072)	(0.0080)
Global cubic	0.0138***	0.0151***	0.0153***	0.0172	0.0166*	0.0111*
	(0.0047)	(0.0042)	(0.0038)	(0.0107)	(0.0095)	(0.0058)
Global quartic	0.0116***	0.0127***	0.0138***	0.0222	0.0222**	0.0140*
	(0.0043)	(0.0036)	(0.0036)	(0.0137)	(0.0109)	(0.0079)
Local linear	0.0103***	--	--	0.0104	0.0123	0.0246***
	(0.0030)			(0.0083)	(0.0078)	(0.0064)
Bandwidth	6 months			257 days	30 weeks	9 months
Local quadratic	0.0131***	--	--	0.0195	0.0194*	0.0141***
	(0.0026)			(0.0120)	(0.0102)	(0.0052)
Bandwidth	8 months			245 days	33 weeks	7 months

Notes: * denotes p<0.10, ** denotes p<0.05, *** denotes p<0.01. The global parametric regressions use a bandwidth of 12 months or equivalent (i.e., 52 weeks, 365 days) and polynomials that vary on either side of the discontinuity. The nonparametric regressions are estimated using the "robust data-driven" procedures of Calonico, Cattaneo and Titiunik (2014a; 2014b). We use a triangular kernel, robust standard errors, and their bandwidth selection and bias correction procedures. See Table 2 and the text for more details.

Appendix Table A4 Male Estimates with Additional Controls for Mortality Near Age 62

Regression type	Quadratic specification			Cubic specification			Quartic specification		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mortality change at 62	0.0173*	0.0218	0.0217	0.0243*	0.0329	0.0376	0.0286*	0.0308	0.0301
	(0.0090)	(0.0217)	(0.0206)	(0.0129)	(0.0449)	(0.0435)	(0.0154)	(0.0558)	(0.0539)
Dummy variables for before 62									
One week before 62		0.0067	0.0093		0.0225	0.0292		0.0207	0.0269
		(0.0164)	(0.0154)		(0.0375)	(0.036)		(0.0474)	(0.0454)
Prior 4 weeks before 62		-0.0083	-0.0060		0.0031	0.0084		0.0023	0.0073
		(0.015)	(0.0140)		(0.0278)	(0.0264)		(0.0318)	(0.03)
Prior 8 weeks before 62		-0.0016	-0.0018		0.0025	0.0034		0.0027	0.0036
		(0.0097)	(0.0095)		(0.0131)	(0.0128)		(0.0127)	(0.0122)
Dummy variables for after 62									
One week after 62		0.0186	0.0205*		0.0240	0.0255		0.0240	0.0297
		(0.0132)	(0.0126)		(0.0202)	(0.0202)		(0.0214)	(0.0214)
Next 4 weeks after 62		-0.0109	-0.0132		-0.007	-0.0096		-0.0070	-0.0076
		(0.0120)	(0.0111)		(0.0165)	(0.016)		(0.0166)	(0.0161)
Next 8 weeks after 62		-0.0082	-0.0070		-0.0068	-0.0057		-0.0068	-0.0062
		(0.0085)	(0.0080)		(0.0097)	(0.0093)		(0.0099)	(0.0093)
Fixed effects									
Year-of-birth FE	No	No	Yes	No	No	Yes	No	No	Yes
Month-of-death FE	No	No	Yes	No	No	Yes	No	No	Yes

Notes: ** denotes $p<0.10$, ** denotes $p<0.05$, *** denotes $p<0.01$. Data is from restricted-use versions of the Multiple Cause of Death data held at the National Center for Health Statistics. The global parametric regressions allow for the polynomial to vary either side of the discontinuity and we report robust standard errors. See Table 2 and the text for more details.

Appendix Table A5 Estimates of Increase in Mortality at 62, Social Security Death Master File

	Using exact dates					Estimates at 62 without day of month			
	Estimates at age 62			Placebo estimates at other ages		Placebo sample: Pre-1962 deaths		Treatment sample comparison	
	All	Males	Females	Age 61	Age 63	All	Males	All	Males
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Global quadratic	0.047***	0.055***	0.035**	-0.015*	0.004	-0.013	-0.025	0.040**	0.051***
	(0.017)	(0.018)	(0.017)	(0.008)	(0.011)	(0.044)	(0.048)	(0.017)	(0.015)
Local linear	0.043***	0.050***	0.040**	-0.009	-0.008	-0.001	-0.021	0.041**	0.042***
- CCT bandwidth	(0.016)	(0.018)	(0.017)	(0.007)	(0.008)	(0.029)	(0.032)	(0.017)	(0.012)
	6 mths.	6 mths.	7 mths.	6 mths.	7 mths.	9 mths.	8 mths.	8 mths.	5 mths.
Ave. monthly deaths:									
- At age 61	30,026	17,239	11,331	27,168	36,558	1,034	976	34,365	18,377
- At age 62	35,026	20,132	13,226	29,040	39,047	921	862	38,354	21,402

Notes: * denotes p<0.10, ** denotes p<0.05, *** denotes p<0.01. Data are from the SSDMF. Data are reported monthly mortality counts: columns 1-5 are based on exact date of death, while columns 6-9 are based on month and year of death. The global parametric regressions allow for the polynomial to vary either side of the discontinuity, and we report robust standard errors. The nonparametric regressions are estimated using the "robust data-driven" procedures of Calonico, Cattaneo and Titiunik (2014a; 2014b). We use a triangular kernel, robust standard errors, and their bandwidth selection and bias correction procedures. Columns 1 to 3 use mortality counts for the 1930-1948 birth cohorts, and show that there is a statistically significant increase in reported mortality at age 62. Note that the counts in column 1 are not equal to those in column 2 and 3 as sex is identified using names and cannot be identified for all decedents. Column 4 displays results at age 61 using the 1931-1948 birth cohorts and column 5 displays results at age 63 using the 1929-1948 birth cohorts; the samples reflect exact date of birth generally not being included prior to 1988. There is no change in mortality at these ages. Columns 6 and 7 use reported mortality counts for deaths from 1940 to 1961, and show there was no increase in mortality at age 62 in this placebo sample. As a comparison of these results, columns 8 and 9 show that there is a statistically significant increase in mortality at age 62 for the 1930-1948 cohorts even when exact day of death is not used. See text for more details.

Appendix Table A6 Sensitivity of Male Mortality Estimates at Age 62 to Omitting Census Regions

Regression type	Northeast region excluded (1)	Midwest region excluded (2)	South region excluded (3)	West region excluded (4)
Global parametric regressions (bandwidth = 12 months)				
Quadratic regression	0.0183***	0.0236***	0.0190***	0.0160**
	(0.0036)	(0.0055)	(0.0048)	(0.0065)
Cubic regression	0.0194***	0.0298***	0.0175***	0.0196**
	(0.0047)	(0.0071)	(0.0059)	(0.0086)
Quartic regression	0.0230***	0.0304***	0.0183***	0.0205**
	(0.0065)	(0.0086)	(0.0089)	(0.0103)
Local nonparametric regressions				
Local linear	0.0193***	0.0275***	0.0184***	0.0177***
	(0.0035)	(0.0044)	(0.0036)	(0.0060)
Data-driven bandwidth	6 months	7 months	8 months	6 months
Local quadratic	0.0210***	0.0282***	0.0186**	0.0200***
	(0.0046)	(0.0061)	(0.0041)	(0.0077)
Data-driven bandwidth	7 months	7 months	10 months	8 months

Notes: ** denotes p<0.05, *** denotes p<0.01. Data is from restricted-use versions of the Multiple Cause of Death data held at the National Center for Health Statistics. The global parametric regressions allow for the polynomial to vary either side of the discontinuity and we report robust standard errors. The nonparametric regressions are estimated using the "robust data-driven" procedures of Calonico, Cattaneo and Titiunik (2014a; 2014b). We use a triangular kernel, robust standard errors, and their bandwidth selection and bias correction procedures. See text for more details.

Appendix Table A7 Estimates for Male Cohorts Based on Differences in Full Retirement Age

	Local Linear (1)	Global quadratic (2)
Cohorts with a FRA equal to 65 years: 1921-1937	0.0148	0.0170**
	(0.0090)	(0.0068)
	11 months	12 months
Cohorts with a FRA greater than 65 years: 1938-1948	0.0244**	0.0179***
	(0.0099)	(0.0069)
	8 months	12 months
- Cohorts with a FRA between 65 & 66 years: 1938-1942	0.0259	0.0190
	(0.0178)	(0.0151)
	10 months	12 months
- Cohorts with a FRA equal to 66 years: 1943-1948	0.0232**	0.0170***
	(0.0098)	(0.0062)
	7 months	12 months

Notes: ** denotes p<0.05, *** denotes p<0.01. FRA is "Full Retirement Age." For each subgroup, we show the coefficient, standard error and bandwidth used for both the local linear regression (with CCT bandwidth) and global quadratic regression (with 12-month bandwidth). See the notes to Table 2 for more details.

Appendix Table A8 Assessing the Relationship between the Changes in Mortality and Other Outcomes at Age 62 using the 14 Demographic Subgroups

Regressor used	Coefficient (s.e.) from weighted regression (1)	Coefficient (s.e.) from unweighted regression (2)
Social Security outcomes		
(a) Fraction who began claiming Social Security	-0.007	0.195
	(0.491)	(0.449)
(b) Fraction who began claiming Social Security (change in means, ages 62-61)	0.070	-0.041
	(0.130)	(0.169)
(c) Fraction receiving Social Security payments	-0.049	0.050
	(0.039)	(0.056)
Labor force participation outcomes		
(d) Fraction retired	0.073***	0.074**
	(0.021)	(0.029)
(e) Fraction in labor force	-0.060***	-0.074***
	(0.019)	(0.019)
(f) Fraction working for pay	-0.070**	-0.097***
	(0.026)	(0.025)
Health insurance outcomes		
(g) Fraction with health insurance	-0.034	0.013
	(0.071)	(0.067)
(h) Average number of health insurance plans	-0.013	-0.004
	(0.021)	(0.020)
Household income (change in means)		
(i) Average annual change between 61 and 63	0.046	0.007
	(0.046)	(0.046)
(j) Average annual change between 61 and 63, Jan.-Mar. sample	0.047	0.001
	(0.048)	(0.008)

Notes: ** denotes p<0.05, *** denotes p<0.01. The coefficients and standard errors come from regressions with the mortality coefficients as the dependent variable and the particular outcome as the independent variable. The results in column (1) come from regressions weighted by the number of deaths in each group.

Appendix Table A9 Jointly Assessing the Relationship between the Changes in Mortality and Other Outcomes at Age 62 using the 14 Demographic Subgroups

	Varying labor force participation measures			Using Social Security payments and varying labor force participation			Using number of health insurance plans and varying labor force participation		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Social Security outcomes									
Fraction claiming SS, change age 61-62	0.022	0.024	0.010	--	--	--	0.025	0.021	0.010
	(0.108)	(0.124)	(0.114)				(0.089)	(0.110)	(0.103)
Fraction with SS payments	--	--	--	-0.050	-0.025	-0.014	--	--	--
				(0.050)	(0.048)	(0.051)			
Labor force participation outcomes									
Fraction retired	0.078*	--	--	0.079**	--	--	0.068**	--	--
	(0.036)			(0.031)			(0.023)		
Fraction in labor force	--	-0.073**	--	--	-0.068*	--	--	-0.058*	--
		(0.031)			(0.032)			(0.027)	
Fraction working for pay	--	--	-0.079*	--	--	-0.075*	--	--	-0.068*
			(0.037)			(0.040)			(0.033)
Health insurance outcomes									
Fraction with health insurance	0.019	0.025	0.014	0.016	0.020	0.011			
	(0.045)	(0.052)	(0.055)	(0.048)	(0.056)	(0.058)			
Average health insurance plans	--	--	--	--	--	--	-0.024	-0.011	-0.011
							(0.026)	(0.025)	(0.022)
Household income (change in means)									
Average annual change, ages 61-63	0.023	-0.010	-0.011	0.002	-0.021	-0.016	0.055	0.012	0.009
	(0.042)	(0.055)	(0.059)	(0.051)	(0.056)	(0.057)	(0.049)	(0.057)	(0.059)
R-squared	0.448	0.394	0.387	0.507	0.407	0.390	0.490	0.385	0.389

Notes: * denotes p<0.10, ** denotes p<0.05, *** denotes p<0.01. The coefficients and standard errors come from regressions with the mortality coefficients from the MCOD data as the dependent variable and the particular outcomes as independent variables. Each regression has 14 observations and is weighted by the number of deaths in each group. See text for more details.

Appendix Table A10 Estimated Change in Male Mortality at Age 62, By Cause of Death

	Local linear (1)	Global quadratic (2)	Fraction of deaths (3)		Local linear (4)	Global quadratic (5)	Fraction deaths (6)
Cardiovascular and respiratory conditions	0.0250**	0.0135	39.2%	Cancers	0.0262***	0.0263***	33.6%
	(0.0106)	(0.0119)			(0.0072)	(0.0084)	
- Heart attacks	0.0159	0.0072	19.4%	- Single cause cancer	0.0069	0.0178	12.3%
	(0.0138)	(0.0172)			(0.0071)	(0.0087)	
- Chronic obstructive pulmonary disease (COPD)	0.0696***	0.0496**	4.2%	- Lung cancer	0.0531***	0.0510***	13.1%
	(0.0089)	(0.0180)			(0.0097)	(0.0108)	
- Not heart attacks or COPD	0.0064	0.0118	15.6%	- Not lung cancer	0.0099	0.0106	20.6%
	(0.0106)	(0.0115)			(0.0080)	(0.0092)	
External causes	0.0314***	0.0399**	5.0%	All other causes	0.0113	0.0109	22.1%
	(0.0103)	(0.0163)			(0.0109)	(0.0106)	
- Motor vehicle accidents	0.1421***	0.1526***	1.3%				
	(0.0376)	(0.0508)					
- Not motor vehicle accidents	-0.0088	0.0007	3.8%				
	(0.0165)	(0.0159)					

Notes: ** denotes p<0.05, *** denotes p<0.01. We show coefficients and standard errors for local linear (using bandwidths selected based on the procedures of Calonico, Cattaneo, Titiunik (2014a; 2014b)) and global quadratic regressions (using a 12-month bandwidth). Fraction of deaths is for male deaths at age 61 and 62. See Table 2 and the text for more details.

Appendix Table A11 The Impact of Changing Employment Levels on Mortality at Age 62

	Males			Females		
	Retirement (1)	Labor force participation (2)	Working for pay (3)	Retirement (1)	Labor force participation (2)	Working for pay (3)
Percentage change in mortality (a)	0.019***	0.019***	0.019***	0.006	0.006	0.006
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Percentage point change in work (b)	0.110***	-0.082***	-0.082***	0.042	-0.001	-0.008
	(0.023)	(0.022)	(0.022)	(0.030)	(0.041)	(0.036)
Mortality-to-work ratio [(a)/(b)]	0.169***	-0.226***	-0.225***	0.138	-4.02	-0.719
	(0.057)	(0.085)	(0.084)	(0.153)	(114)	(3.26)

Notes: ** denotes p<0.05, *** denotes p<0.01. The first row shows coefficients and standard errors for the estimated change in mortality at age 62 using the global quadratic RD specification. These are from Table 2. The second row shows the coefficients and standard errors for the percentage point changes in employment outcomes at age 62, which are calculated from the HRS using the global quadratic specification. The third row shows the estimated percentage increase in mortality if it is attributed to one of three measures of work levels: fraction retired, labor force participation, and fraction working for pay. It is calculated as the ratio of the first two rows, with the standard errors calculated using the delta method.