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Does Retirement Drive Health or Health Drive Retirement?

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Abstract

While health shocks can induce retirement, retirement may also affect health. Using a panel of individuals aged 51-65 and a staggered difference-in-differences design, we examine retirement choices for people who experience health shock, and health outcomes for people who retire. Retirements are differentiated by whether they occur earlier than respondents' initial planned retirement age, and whether respondents cite health as a factor. Health shocks are associated with a 40 percent increase in the probability of retiring, a more than 100 percent increase in the probability of retiring for health reasons, and a 56 percent increase in the probability of retiring early. Spousal health shocks are associated with a smaller increase in the probability of retiring early. Non-health related retirements, as well as retirements that do not occur earlier than planned, are associated with a temporary improvement in self-reported health and an increase in vigorous exercise.

Keywords: retirement, health, difference-in-difference, causal estimation, spousal health

JEL codes : J14, J26, J12, J31, C23

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Introduction

Empirical evidence indicates that health shocks are often a driver of retirement. A health shock may increase the cost of effort or reduce productivity and earnings, thereby causing a decrease in labor supply. But the causation runs in the other direction as well: a large literature suggests that retirement appears to have an impact on health. Theoretically, the impact of retirement on health is ambiguous. Retirement may reduce the value of investing in health to boost one's productivity and earnings. However, the post-retirement increase in leisure may raise the value of heath as a consumption good, while also reducing the time cost of engaging in behavior to promote health. In this paper, we use panel data from the Health and Retirement Study (HRS) to examine the endogenous relationship between retirement and health.

We begin by estimating the relationship between observed health shocks (self-reported declines in health) and retirement (using several alternative definitions). Using the staggered difference-in-differences approach of Callaway and Sant'Anna (2021), we compare the dynamics of retirement behavior for individuals who experience health shocks at age 65 or earlier with those of never-treated controls. We show that a self-reported decline in health is followed by a 40 percent increase in the probability of retirement. Individuals who retire following a health shock are also likely to retire earlier than their anticipated retirement age, and to cite health as a driving factor in their retirement. Specifically, compared to the never-treated controls, respondents who experience a health shock are 56 percent more likely to retire before the age they initially provided as their planned retirement age, and more than twice as likely to retire and cite health reasons for doing so. Meanwhile, again compared to the never treated controls, individuals who experience health shocks are *not* statistically significantly more likely to retire at or after their anticipated retirement age or to retire and cite reasons non-health reasons as the driving factor.

Next, we use the same staggered difference-in-differences methodology to examine the dynamics of health outcomes and behavior for individuals who retire at or before age 65 with those of a never-treated control group. In this analysis, we distinguish between individuals who cite health as a reason for retirement and those who do not. We also distinguish between individuals who retire before their anticipated retirement age and those who retire at or after their planned retirement age. Splitting the sample in this way allows us to deal with the endogeneity of retirement. Individuals who cite health as a reason for their retirement, as well as those who retire earlier than anticipated, are likely to be responding endogenously to health shocks. In contrast, retirements for which health is not cited as a driving factor, as well retirements that do not occur earlier than anticipated, are plausibly exogenous. We find that these non-early and non-health related retirements are associated with a temporary improvement in self-reported health, as well as an increase in the probability of vigorous exercise.

Compared to existing studies, which largely focus on one direction of causation or the other, a key contribution of this work is to illustrate — in a transparent way and using a consistent sample — the endogenous relationship between retirement and health. Moreover, previous studies on the health effects of retirement tend to rely on an instrumental-variables approach. Specifically, they deal with the endogeneity of retirement by using public or private pension eligibility (which is based on age and plausibly uncorrelated with health shocks) as an instrument for retirement. Under the assumption that the instrument is valid, this strategy produces estimates of the health effects of retirement *for individuals who are induced to retire because they became eligible for a pension*. Our approach arguably produces more generalizable estimates because we examine the health effects of all observed, non-health related retirements, not just those that are driven by pension eligibility. An additional contribution of this paper is to validate responses to HRS questions about reasons for retiring and planned retirement age. While these questions rely on stated preference, or may be vulnerable to ex-post rationalization, our analysis suggests that responses to them are informative in the sense that they are correlated, in a plausible way, with retirement and health outcomes.

Prior Research

Does health drive retirement? As discussed by Sammartino (1987) and Dwyer and Mitchell (1999), poor health has a theoretically ambiguous impact on retirement. For example, poor health may reduce productivity and earnings. It may also increase the marginal utility of leisure by increasing either the direct disutility of work or the value of spending non-work time caring for one's health. Together, these two changes would be expected to accelerate retirement. On the other hand, the out-of-pocket health costs associated with poor health may increase the marginal utility of consumption, which may incentivize delayed retirement. Despite the theoretical ambiguity, empirical evidence suggests that, overall, health shocks increase the probability of retirement (Loprest et al. 1995; McClellan 1998; Charles 2003; Wu 2003; Disney et al. 2006; Lindeboom and Kerkhofs 2009; García-Gómez et al. 2013; Garcia-Gomez et al. 2017; Bound et al. 1999; Currie and Madrian 1999; Dwyer and Mitchell 1999; French 2005; Disney et al. 2006).

While there is consensus that a person's retirement choices are influenced by their *own* health, there is less evidence on how these choices might be influenced by a *spouse*'s health. Spousal health shocks could have either a positive or a negative impact on retirement. Individuals may delay retirement to replace the lost income of a spouse, or to pay out-of-pocket health care costs. However, they may also be more likely to retire if they plan to serve as a caregiver for the spouse. The existing evidence on this question is mixed (Berger and Fleisher

1984; Blau and Riphahn 1999; Wu 2003; McGeary 2009; Fadlon and Nielsen 2021; Newmyer et al. 2023).

Does retirement drive health? As discussed by Dave et al. (2008), retirement has a theoretically ambiguous impact on health. In the human capital model of Grossman (1972), health is both an investment good (affecting work productivity) and a consumption good (directly affecting utility). Retirement may affect both the consumption and investment aspects of health. On one hand, retirement decreases the marginal benefit of investing in health to boost work productivity, inducing less investment in health. On the other hand, if health as a consumption good is complementary with leisure, then retirement may lead to improvements in health. Retirement may also lower the opportunity cost of time spent maintaining one's health, such as through exercise or doctor visits (Lucifora and Vigani 2018).

Many studies report that retirement is associated with improvements in self-reported health (Gall et al. 1997; Jokela et al. 2010; Gorry et al. 2018; Coe and Zamarro 2011; Rose 2020; Messe and Wolff 2019; Barschkett et al. 2022; Johnson and Lee 2009). However, other studies find insignificant or mixed results, particularly regarding more objective health biomarkers and mortality (Gayman et al. 2013; Mein et al. 2003; Fitzpatrick and Moore 2018; Gorry and Slavov 2021). Studies also reach different conclusions regarding the impact of retirement on health behavior, including physical exercise and health care utilization (Chung et al. 2009; Nooyens et al. 2005; van Solinge 2007; van der Heide et al. 2013; Godfrey et al. 2014; Kämpfen and Maurer 2016; van der Heide et al. 2013; van Solinge 2007; Westerlund et al. 2009; Motegi et al. 2020; Coe and Zamarro 2015; Insler 2014; Gorry and Slavov 2023; Kesavayuth et al. 2018; Celidoni and Rebba 2017; Tran and Zikos 2019; Eibich 2015; Frimmel and Pruckner 2020).

A key challenge in this literature is that retirement is endogenous: workers may retire upon experiencing a health shock, leading researchers to incorrectly conclude that retirement worsens health. Most prior studies have deal with the endogeneity between health and retirement using instrumental variable (IV) or regression discontinuity (RD) approaches. Common instruments and discontinuities used in these analyses include age- and cohort-specific retirement probabilities (Becchetti et al. 2012), age-based retirement incentives in public and private pension systems (Behncke 2012; Charles 2004; Rose 2020; Coe and Zamarro 2011, 2015; Fitzpatrick and Moore 2018; Horner 2014; Johnston and Lee 2009; Neuman 2008; Eibich 2015; Gorry et al. 2018; Barschkett 2021; Motegi et al. 2020; Insler 2014; Gorry and Slavov 2023; Kesavayuth et al. 2018; Celidoni and Rebba 2017; Tran and Zikos 2019; Eibich 2015; Frimmel and Pruckner 2020; Kämpfen and Maurer 2016), and early retirement offers (Coe et al. 2012).

However, these models face some validity challenges. First, in studies that use private pension eligibility or early retirement offers as instruments, there is a concern that people may select into jobs based on unobservable characteristics that are correlated with both health and pension eligibility ages (or early retirement offers). Second, in studies that use variation in public pension eligibility ages across countries or over time, there is a concern that changes to public pension eligibility ages may be endogenous to the general health of the population. Third, even in studies that examine within-person changes in health across a fixed, universal public pension eligibility threshold (e.g., the Social Security early eligibility age of 62), there is a concern that becoming eligible for a public pension may disproportionately induce those experiencing health problems to retire. Fourth, individuals anticipating pension income in the future may adjust their pre-retirement behaviors, violating exogeneity assumptions (Behncke 2012). Finally, the generalizability of results based on the IV and RD approaches may be a concern. Workers who are induced to retire by public or private pension incentives may be systematically different from those who retire at other ages. Similarly, retiring at a pension eligibility age may be associated with changes in income that are absent at other potential retirement ages. These challenges underscore the complexity of identifying causal effects of retirement on health.

Data and Methods

We use data from the Health and Retirement Study (HRS) to address to explore the endogenous relationship between health and retirement. The HRS is a panel survey that is intended to be representative of the US population aged 51 and older and their spouses (Sonnega et al. 2014). It began in 1992 with a cohort aged 51-61 and their spouses, and additional cohorts have been added at regular intervals. The HRS is sponsored by the National Institute on Aging (grant number NIA U01AG009740) and is conducted by the University of Michigan. We primarily rely on the cleaned version of the data produced by the RAND Center for the Study of Aging that includes HRS waves from 1992-2020 (Bugliari et al. 2023). This file contains information on respondents' demographic and socio-economic characteristics, as well as their health insurance coverage, health status, and labor force status in each wave. We also merge in a set of variables from the raw HRS data. These variables include respondent-provided reasons for retiring or changing jobs. We select a sample consisting of individuals who entered the survey between the ages of 51 and 59, were working full time during their first wave, and had not recently experienced a health shock (defined as a self-reported worsening of health over the past two years). We focus on health shocks and retirements that occur at or before age 65, and we drop individuals who are not observed through age 65. In our sample, the majority of people retire at or before age 65. However, the age-65 cutoff still gives us a large never-treated group:

individuals who do not experience a health shock and are not observed to retire at or before age 65. Thus, our estimates apply to health shocks and retirements that occur at or before age 65.

We consider several alternative definitions of retirement. All definitions derive from a detailed labor force indicator that classifies individuals as working full time, working part time, unemployed, partly retired, retired, disabled, or not in the labor force. The partly retired and retired statuses are based on individuals self-identifying as retired; within this group, those who work part time are classified as partially retired, while those who do not work for pay are classified as retired. The first definition of retirement we consider is any labor force status other than full time work (whether that involves part time or no work). The second definition is any labor force status that does not involve working (i.e., any employment status other than working full time, working part time, or being partially retired). A final definition of retirement is self-reporting a retired status (i.e., either retired or partly retired). Our baseline results in the main text of this paper use the third definition — self-reporting a retired status. Results for the other two definitions are provided in the Appendix.

Does health drive retirement? To examine the causal effect of health on retirement, we need a measure of health shocks. In each wave, respondents are asked how their health has changed over the past two years. Possible answers include 'better,' 'about the same,' or 'worse.' We define a health shock as a self-report of 'worse' health. We examine the dynamics of retirement around the *first* reported health shock. For this analysis, an individual is coded as untreated before the wave of their first health shock; during and after this wave, they are coded as treated. The never treated comparison group consists of individuals who do not experience a health shock at or before age 65. For married people, spousal health shocks may also drive retirement. To explore this avenue, we construct an analogous spousal health shock variable that is equal to one starting in the wave of the first observed spousal health shock. The never treated comparison group for this analysis includes individuals whose spouses do not experience a health shock, as well as individuals who are not married.

The dependent variable in this analysis is an indicator that takes on a value of one if the respondent transitions from a non-retired status to a retired status between the previous wave and the current wave. In other words, this variable measures the unconditional probability of shifting from work to retirement (under each of the three definitions above). Individuals may have multiple retirement transitions if they 'unretire' and retire again (e.g., Maestas 2010); however, less than 20 percent of the sample reports multiple transitions (by any of the three definitions).

We distinguish retirements by whether the individual cites health reasons for retiring, and by whether the retirement occurs earlier than anticipated. Individuals who retire or leave a job are asked about their reasons for the transition. A 'health related' retirement transition occurs if during the wave of transition, the respondent reports either retiring or changing jobs for health reasons. A 'non health related' retirement transition occurs if, during the wave of transition, the respondent does not report either retiring or changing jobs for health reasons. In each wave, respondents are also asked for their anticipated retirement age. We capture the value of this variable during each individual's first wave. A transition to retirement is defined as 'early' if the respondent's age is strictly below this anticipated retirement age. A transition to retirement is defined as 'not early' if the respondent's age is greater than or equal to this anticipated retirement age. Individuals who do not report a planned retirement age are excluded from this analysis.[1]

We estimate the following specification for individual i in wave t:

$$y_{it} = \sum_{s} \beta_s \cdot I(shock_{it-s} = 1) + \sum_{a} \theta_a \cdot I(age_{it} = a) + \gamma_i + \delta_t + \epsilon_{it} \qquad (1)$$

In this equation, $I(shock_{it-s} = 1)$ is an indicator that takes on a value of 1 if individual i (or individual i's spouse experienced a health shock s waves ago. The first summation therefore captures the lags and leads of the health shock variable. We control flexibly for age using a set of dummies (the second summation); here, $I(age_{it} = a)$ is an indicator that takes on a value of 1 if individual i is aged a during wave t. The equation also includes a separate intercept for each individual (γ_i) and each wave (δ_t). The dependent variable, y_{it}, may be an indicator for transitioning to retirement (by any of our three definitions). It may also be an indicator for transitioning to retirement early, not early, for health reasons, or for non-health reasons.

Because the treatment — the first observed health shock — is staggered, estimating the equation above using ordinary least squares with two-way-fixed effects results in bias. Bias occurs because the parameters are estimated based on comparing newly treated individuals to three comparison groups: never-treated individuals, not-yet-treated individuals, and earlier-treated individuals. If the impact of health shocks varies across treatment cohorts or time, then the third comparison — of newly treated individuals to earlier-treated individuals — introduces bias (Callaway and Sant'Anna 2021; de Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021; Sun and Abraham 2021; Borusyak et al. 2024).

To address this problem, we estimate the model using Callaway and Sant'Anna's (2021) approach. This method involves splitting treated individuals into cohorts based on the wave in which they experience a health shock. Equation (1) is estimated separately, using two-way fixed effects, for each treatment cohort compared to the never-treated control group. In each regression, we use the wave before the health shock as the base period. For example, the first regression involves estimating the equation using individuals who never experience a health shock and those who experience a health shock in wave 2. The second regression uses the never treated and those who experience a health shock in wave 3 (and so on). In all, fourteen separate regressions — for the cohorts treated in waves 2 through 15 — are estimated. Within each of these regressions, treatment timing is not staggered and thus the equation can correctly be estimated using a standard two-way fixed effects approach. Once the equation is estimated for each cohort, estimates of the dynamic treatment effects, β_s, are derived by aggregating across treatment cohorts.[2,3]

Does retirement drive health? To assess the impact of retirement on health, we examine the dynamics of health around the *first* wave in which an individual is observed to be retired, based on any of the three definitions outlined above. Individuals are coded as untreated until the wave in which they report being retired; during and after this wave, they are coded as treated. The never treated comparison group consists of individuals who are not observed to retire at or before age 65.

The dependent variables in this analysis are measures of the respondent's health. First, respondents are asked in each wave to rate their overall health on a scale ranging from one (excellent) to five (poor). We normalize this variable by subtracting its mean, dividing it by its standard deviation, and reversing its sign so that higher values signify better health. The normalized variable therefore captures the individual's subjective health measured in standard deviations above the mean. Second, as a measure of health care utilization, we consider the self-reported number of doctor visits in the past two years. Third, we construct an index of the number of major health conditions that the individual has ever been diagnosed with. This index includes eight conditions: high blood pressure, diabetes, cancer, lung disease, heart disease, stroke, psychiatric problems, and arthritis. Accordingly, the index ranges from zero to eight.

Fourth, we consider several health behaviors, including an indicator for regular vigorous exercise and indicators for currently drinking alcohol or smoking (Motegi et al. 2020; Coe and Zamarro 2017; Kämpfen and Maurer 2016; Insler 2014; Gorry and Slavov 2023).[4]

For this analysis, we further split the treatment group based on whether individuals cite health as a reason for retirement, and by whether they retire earlier than anticipated. First, we separate individuals into two groups based on whether they ever cite health as a reason for retirement or a job transition at or before the age of 65. Second, we categorize individuals based on the timing of their first observed retirement (by any of our definitions). Those who retire strictly before their anticipated retirement age form the 'early' retirement group, while those who retire at or after their anticipated retirement age form the 'not early' group. Based on the health-causes-retirement analysis above, we argue that the results for the 'not early' and 'non-health' samples can be interpreted causally.

We estimate the following specification for individual i in wave t:

$$y_{it} = \sum_{s} \mu_s \cdot I(retire_{it-s} = 1) + \sum_{a} \lambda_a \cdot I(age_{it} = a) + \phi_i + \eta_t + \epsilon_{it} \qquad (2)$$

In this equation $I(retire_{it-s} = 1)$ is a relative time indicator that takes on a value of 1 if individual i's first observed retirement (by any of our definitions) took place s waves ago, and $I(age_{it} = a)$ is a dummy variable indicating whether individual i is aged a. Our specification also includes individual- and wave-specific intercepts, ϕ_i and η_t. Due to the staggered nature of the treatment, we use Callaway and Sant'Anna's (2021) approach, which involves estimating the equation above separately for the cohort that retires in each wave (as compared to the never-treated control group). Estimates of μ_s, the dynamic treatment effects, are aggregated across treatment cohorts.[5]

Results

Summary statistics for the key variables used in the analysis are shown in Table 1. The top panel suggests that our overall sample contains more men than women (likely due to selecting individuals who are working full time during their first wave). The second panel, which focuses on the variables used in the health-causes-retirement analysis, suggests that almost half the sample experience a health shock at age 65 or earlier (i.e., are in the treatment group). Retirement transitions between each pair of waves occur for approximately 10-13 percent of the sample, depending on the definition of retirement. However, only a small fraction (2-3 percent of the sample) report poor health as a reason for retiring. Early retirement, defined as a retirement occurring before the respondent's initially anticipated retirement age, is common and constitutes the majority of observed retirement transitions. The bottom panel of the table, which focuses on the variables used in the retirement-causes-health analysis, suggests that almost three fourths of respondents stop full time work at some point during the sample period. However, only 56 percent of respondents stop all work, indicating a significant portion engage in partial retirement or other flexible forms of employment. Only 20 percent of the sample ever reports a health-related job transition (either retirement or departure from an employer), while 53 percent of people at some point in the survey report retiring before their initial planned retirement age. As noted above, self-reported health is normalized to have a mean of zero and a standard deviation of one for this sample; higher numbers indicate better health. The average number of diagnosed health conditions is 1.25 on a scale ranging from zero to eight, suggesting that respondents manage relatively few chronic conditions on average.

Does health drive retirement? Table 2 presents the overall treatment effects from equation (1). Retirement transitions are based on the self-reported definition. (Results for the other two retirement definitions are similar and presented in Appendix A). Results for the full sample are reported in the first column, while results for various subgroups are reported in the remaining columns. (We provide subgroup results broken down by the socio-economic and demographic factors in the top panel of Table 1. However, we do not include results for the 'Other Race Non-Hispanic' group due to a small sample size.) The first row suggests that, in the full sample, a health shock is associated with a 4.4 percentage point increase in the unconditional probability of starting to self-report a retired status. This magnitude is large relative to the sample means reported in Table 1. In particular, a health shock is associated with a 4.4 / 11 = 40 percent increase in the probability of retiring. The next row suggests that, in the full sample, a health shock is associated with a 3.9 percentage point increase in the probability of a retirement transition for health reasons. This change represents more than a doubling of the average rate shown in Table 1. In contrast, the third row shows that there is no significant impact on the probability of transitioning to retirement for non-health reasons. The fourth row shows that, in the full sample, health shocks are associated with a 3.9 percentage point (56 percent) increase in the probability of an early retirement transition. There is a small and marginally significant association between health shocks and retirement transitions that occur at or after the respondent's anticipated retirement age, which appears to be concentrated among women. For women, therefore, health shocks may play a role in retirement transitions that occur after their initially planned retirement age.

Figure 1 plots the corresponding event study coefficients from equation (1) for the full sample.[6] These event study graphs are largely consistent with the conclusions drawn from the tables. They are also consistent with the raw plots of dependent variables around the health shock, which are provided in Appendix B. Interestingly, Figure 1 suggests a small increase in the probability of *non-health related* retirement two periods before a health shock, indicating possible endogeneity.

Table 3 shows corresponding results from estimating equation (1) for spousal health shocks. The first row of the table suggests that spousal health shocks may be associated with an increase in the probability of a retirement transition. In the full sample, we observe a 1.1 percentage point increase in the probability of starting to self-report a retired status, representing a modest 10 percent increase relative to the sample mean. The coefficient is also only marginally significant at the 10 percent level. Overall, these results suggest that while spousal health shocks may influence retirement decisions, their overall impact is less pronounced than that of personal health shocks. The following rows show no statistically significant relationship between spousal health shocks and retirements driven by health or non-health reasons. There is also no statistically significant relationship between spousal health shocks and either early or non-early retirement transitions. The lack of significance in the more specific retirement transition categories likely reflects a lack of power: we observe fewer retirements in each individual category than we do overall.

Event study versions of these results are provided Figure 2 and provide additional insight. The top right panel of Figure 2 suggests – as we would expect – no statistically significant relationship between spousal health shocks and retiring due to (one's own) health. However, the top right and bottom left panels suggest a small increase in the probability of retiring for non-health reasons and in the probability of retiring early. Both increases occur roughly three waves after the spousal health shock. This timing is plausible. Individuals who initially attempt to manage their spouse's health shock without exiting the workforce may later choose to retire if their spouse's health deteriorates or caregiving demands persist. The bottom right panel suggests no association between spousal health shocks and non-early retirements.

Overall, these findings underscore the complex interplay between spousal health and retirement timing. While spousal health shocks have a more limited immediate effect compared to personal health shocks, their impact on early retirements suggest that caregiving responsibilities play a role in shaping retirement decisions.

Does retirement drive health? Table 4 shows the overall treatment effects from equation (2). The treatment in this table is based on the self-reported definition of retirement. The top left panel suggests that, in the full sample, retirement for non-health reasons is associated with modest but meaningful changes in health outcomes. Specifically, there is a small and marginally significant increase in the number of diagnosed health conditions (a coefficient of 0.029, relative to the sample mean of 1.25) and a 5-percentage point (13 percent) increase in the probability of engaging in vigorous exercise. There is also a 0.031 standard deviation improvement in self-reported health (significant at the 10 percent level). In addition, non-early retirement is associated with a 0.074 increase in the number of diagnosed health conditions and a 6.9 percentage point (18 percent) increase in the probability of vigorous exercise. These findings imply that non-health-related and non-early retirements, which are unlikely to be driven by a health shock, may facilitate healthier lifestyles, such as increased physical activity. In contrast, retirements driven by health reasons or those occurring earlier than planned are (likely endogenously) associated with a more pronounced increase in the number of diagnosed health conditions. Health-related retirements are also associated with an increase in the number of doctor visits, highlighting the reactive nature of health-driven workforce exits. Notably, both health-related and early retirements are associated with a decrease in the probability of smoking, suggesting health shocks may encourage behavioral changes (Kesavayuth et al. 2018; Insler 2014).

We observe some differences across subgroups. The increase in vigorous exercise following non-health related and non-early retirements seems to be concentrated among those with some college education. These point estimates are also larger for female and Black retirees. We find marginally significant decreases in the probability of drinking among Black retirees and in the probability of smoking among White retirees following a non-health related retirement. There is also a significant decrease in doctor visits following non-health related retirements among those with a high school education or less.

Event study plots for the full sample, using the self-reported definition of retirement, are presented in Figures 3 (for those retiring for non-health reasons) and 4 (for those retiring at or after their planned retirement age). Figure 3 suggests that non-health related retirement is associated with a temporary improvement in self-reported health of almost 0.1 standard deviation shortly after retirement. The effect diminishes in subsequent waves, suggesting that the gains may not persist long-term. Figure 4 suggests that non-early retirement has a similar short-term association with self-reported health, although the coefficient is not statistically significant. Both figures confirm the finding that retirement is associated with an increase in the number of diagnosed health conditions. However, this number appears to be trending upwards in the waves prior to retirement, suggesting a possible violation of the parallel-trends assumption. Thus, we hesitate to interpret this finding as causal. Both figures show a clear post-retirement increase in vigorous exercise. However, a comparison of Figures 3 and 4 highlights that non-early retirement is less clearly associated with behavioral changes, possibly because planned retirements reflect continuity rather than abrupt life shifts. Results for the other definitions of retirement are provided in Appendix C and are largely consistent with those discussed here. Appendix D provides all event study figures for those who retire early or for health reasons. These figures suggest that health-related retirements are preceded by a decline in self-reported health and accompanied by an increase in doctor visits. Self-reported health improves following retirement, likely reflecting recovery from the health shock that prompted retirement. A similar pattern is observed for early retirements. These study graphs are consistent with the raw plots of the health outcomes around retirement in Appendix E.

Conclusions

In this paper, we have applied a staggered difference-in-differences approach to data from the Health and Retirement Study to examine the impact of health shocks on retirement, as well as the impact of retirement on health. Consistent with previous research, we have shown that health shocks are associated with a large increase in the probability of retirement. These health-driven retirements tend to occur earlier than anticipated, with retiring individuals generally citing health as a key factor in their retirement decision. We have also shown that non-health related, as well as non-early, retirements — both of which are unlikely to have been preceded by health shocks — are associated with a temporary improvement in self-reported health and a higher probability of engaging in vigorous exercise. Due to space constraints, we have only examined a limited set of health outcomes and behaviors. It remains to be seen whether non-health related or non-early retirements have an impact on other dimensions of physical and mental health.

Figures

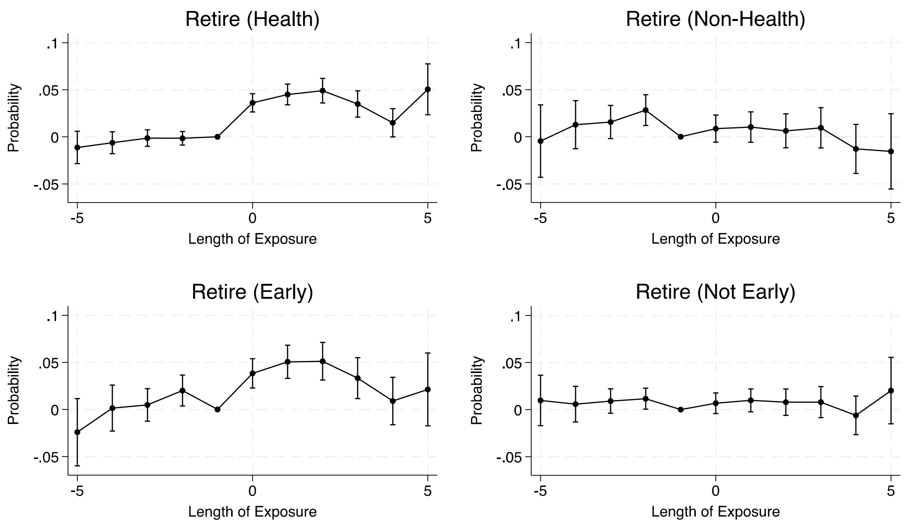

Figure 1. Impact of Health Shocks on Retirement

Notes: Graphs show plots of event study coefficients in equation (1). All models also include age dummies, wave dummies, and individual dummies. Standard errors clustered by individual.

Source: Authors' calculations from HRS data.

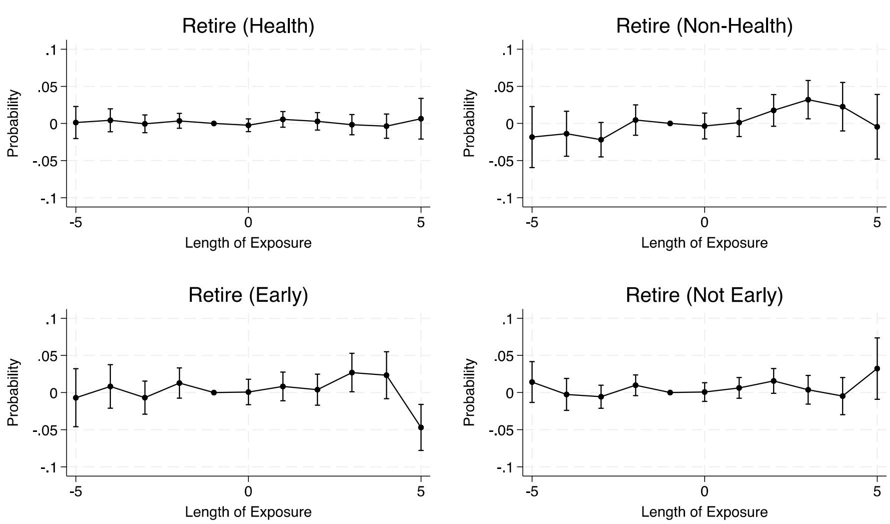

Figure 2. Impact of Spousal Health Shocks on Retirement

Notes: Graphs show plots of event study coefficients in equation (1). All models also include age dummies, wave dummies, and individual dummies. Standard errors clustered by individual.

Source: Authors' calculations from HRS data.

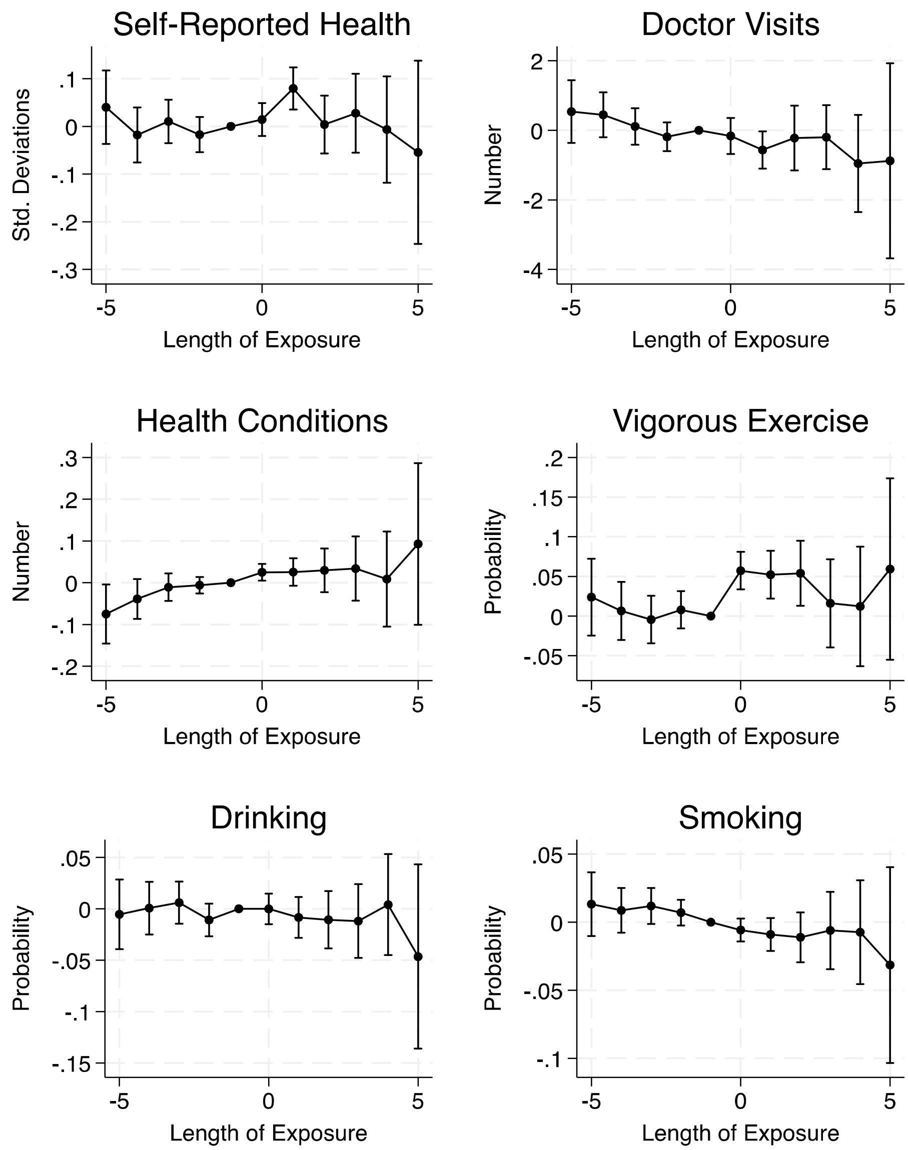

Figure 3. Impact of Retirement on Health Outcomes (Non-Health Reasons for Retiring)

Notes: Graphs show plots of event study coefficients in equation (2). All models also include age dummies, wave dummies, and individual dummies. Standard errors clustered by individual.

Source: Authors' calculations from HRS data.

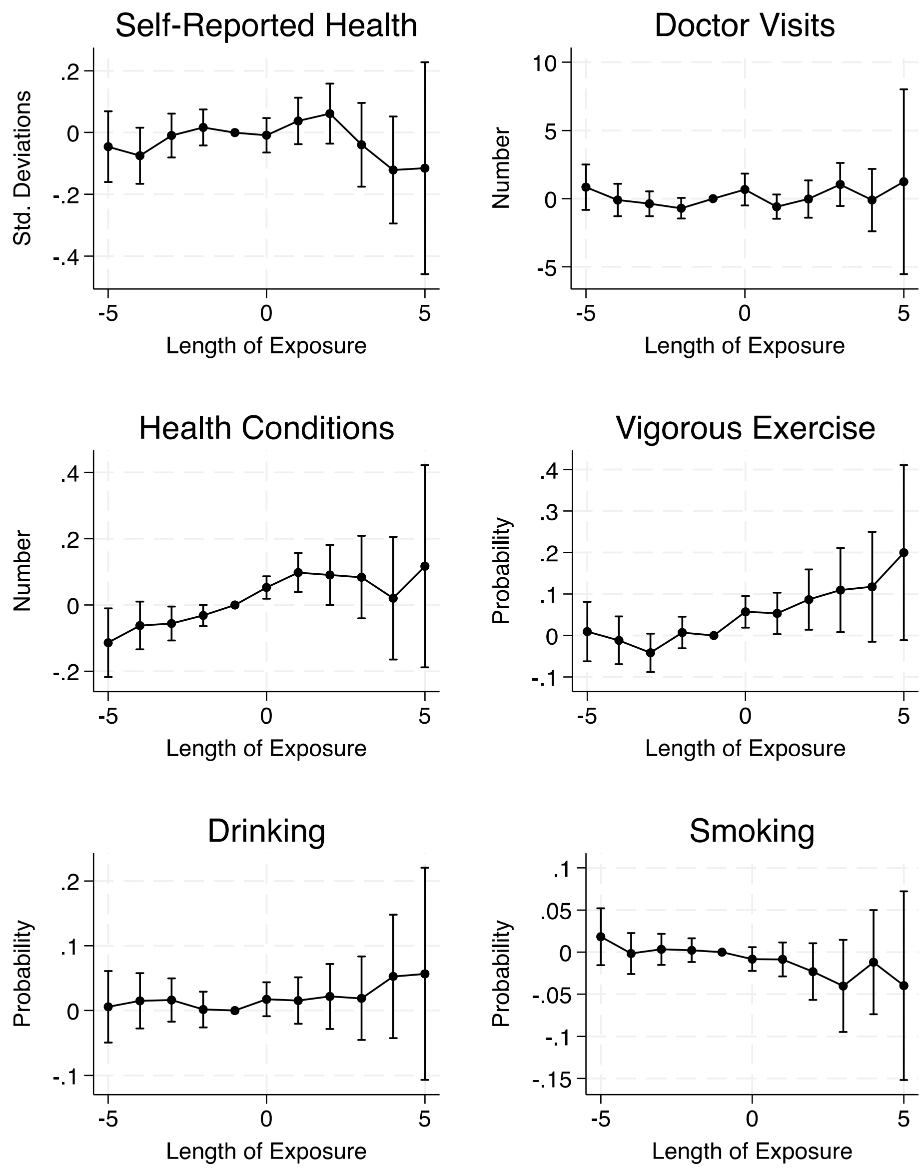

Figure 4. Impact of Retirement on Health Outcomes (Non-Early Retirement)

Notes: Graphs show plots of event study coefficients in equation (2). All models also include age dummies, wave dummies, and individual dummies. Standard errors clustered by individual.

Source: Authors' calculations from HRS data.

Table 1: Summary Statistics

Variable	Obs	Mean	Std. dev.	Min	Max
Demographic and Socio-Economic Characteristics					
Age	35,755	59.12	3.88	51.00	65.00
Male	35,755	0.57	0.49	0.00	1.00
Female	35,755	0.43	0.49	0.00	1.00
White Non-Hispanic	35,755	0.72	0.45	0.00	1.00
Black Non-Hispanic	35,755	0.16	0.37	0.00	1.00
Other Race Non-Hispanic	35,755	0.03	0.16	0.00	1.00
Hispanic	35,755	0.10	0.29	0.00	1.00
High School or Less	35,755	0.47	0.50	0.00	1.00
College+	35,755	0.53	0.50	0.00	1.00
Does Health Drive Retirement?					
Ever Have Health Shock	35,755	0.47	0.50	0.00	1.00
Post Health Shock	35,755	0.26	0.44	0.00	1.00
Ever Have Spousal Health Shock	35,755	0.34	0.47	0.00	1.00
Post Spousal Health Shock	35,755	0.19	0.40	0.00	1.00
Retirement Transition (Stop Full-Time Work)	35,755	0.13	0.33	0.00	1.00
Retirement Transition (Stop All Work)	35,755	0.10	0.29	0.00	1.00
Retirement Transition (Self-Report)	35,755	0.11	0.31	0.00	1.00
Retirement Transition - Health Reasons (Stop Full-Time Work)	35,755	0.02	0.15	0.00	1.00
Retirement Transition - Health Reasons (Stop All Work)	35,755	0.03	0.16	0.00	1.00
Retirement Transition - Health Reasons (Self-Report)	35,755	0.02	0.15	0.00	1.00
Retirement Transition - Non-Health Reasons (Stop Full-Time Work)	35,755	0.11	0.31	0.00	1.00
Retirement Transition - Non-Health Reasons (Stop All Work)	35,755	0.07	0.26	0.00	1.00
Retirement Transition - Non-Health Reasons (Self-Report)	35,755	0.08	0.27	0.00	1.00
Retirement Transition - Early (Stop Full-Time Work)	30,842	0.09	0.29	0.00	1.00
Retirement Transition - Early (Stop All Work)	30,842	0.06	0.24	0.00	1.00
Retirement Transition Early(Self-Report)	30,842	0.07	0.25	0.00	1.00
Retirement Transition - Not Early (Stop Full-Time Work)	30,842	0.04	0.19	0.00	1.00
Retirement Transition - Not Early (Stop All Work)	30,842	0.03	0.18	0.00	1.00
Retirement Transition Not Early (Self-Report)	30,842	0.04	0.19	0.00	1.00
Does Retirement Drive Health?					
Ever Retire (Stop Full Time Work)	35,755	0.73	0.45	0.00	1.00
Ever Retire (Stop All Work)	35,755	0.56	0.50	0.00	1.00
Ever Retire (Self-Report Retired Status)	35,755	0.62	0.48	0.00	1.00
Ever Retire Due to Health	35,755	0.20	0.40	0.00	1.00
Ever Retire Early	30,842	0.53	0.50	0.00	1.00
Post Retirement (Stopped Full Time Work)	35,755	0.37	0.48	0.00	1.00
Post Retirement (Stopped All Work)	35,755	0.26	0.44	0.00	1.00
Post Retirement (Self-Reported Retired Status)	35,755	0.26	0.44	0.00	1.00
Self-Reported Health	35,746	0.00	1.00	-2.51	1.43
Number of Doctor Visits	35,169	7.23	14.06	0.00	900.00
Number of Health Conditions	35,755	1.25	1.16	0.00	7.00
Regular Vigorous Exercise	35,636	0.38	0.49	0.00	1.00
Currently Drinks	35,747	0.62	0.48	0.00	1.00
Currently Smokes	35,648	0.18	0.38	0.00	1.00

Notes: Sample includes individuals who entered the survey between ages 51 and 59, work full time during their first wave in the survey, and are observed through age 65.

Source: Authors' calculations from HRS data.

Table 2: Impact of Health Shocks on Retirement

	All	Women	Men	White	Black	Hispanic	High School	College+
				Transition to Retirement				
Post-Shock	0.044***	0.055***	0.036***	0.039***	0.068***	0.044***	0.046***	0.041***
	(0.005)	(0.008)	(0.007)	(0.006)	(0.016)	(0.014)	(0.008)	(0.008)
				Transition to Retirement (Health Reasons)				
Post-Shock	0.039***	0.042***	0.036***	0.037***	0.060***	0.034***	0.045***	0.033***
	(0.003)	(0.004)	(0.004)	(0.003)	(0.010)	(0.009)	(0.005)	(0.004)
				Transition to Retirement (Non-Health Reasons)				
Post-Shock	0.005	0.013*	0.000	0.002	0.009	0.009	0.001	0.008
	(0.005)	(0.007)	(0.007)	(0.006)	(0.014)	(0.013)	(0.007)	(0.007)
				Transition to Retirement (Early)				
Post-Shock	0.039***	0.043***	0.037***	0.033***	0.062***	0.051***	0.042***	0.036***
	(0.005)	(0.008)	(0.007)	(0.006)	(0.017)	(0.013)	(0.007)	(0.007)
				Transition to Retirement (Not Early)				
Post-Shock	0.007*	0.014***	0.001	0.007	0.015	0.002	0.005	0.008*
	(0.004)	(0.005)	(0.005)	(0.004)	(0.010)	(0.012)	(0.006)	(0.005)

Notes: Coefficients show overall treatment effects for equation (1). All models also include age dummies, wave dummies, and individual dummies. Standard errors clustered by individual.

Source: Authors' calculations from HRS data.

Table 3: Impact of Spousal Health Shocks on Retirement

	All	Women	Men	White	Black	Hispanic	High School	College+
				Transition to Retirement				
Post-Shock	0.011*	0.012	0.011	0.010	0.016	0.004	0.008	0.009
	(0.006)	(0.010)	(0.008)	(0.008)	(0.018)	(0.016)	(0.009)	(0.009)
				Transition to Retirement (Health Reasons)				
Post-Shock	0.001	0.004	0.000	0.001	0.013	-0.008	-0.004	0.005
	(0.003)	(0.004)	(0.004)	(0.004)	(0.009)	(0.010)	(0.005)	(0.004)
				Transition to Retirement (Non-Health Reasons)				
Post-Shock	0.010	0.008	0.011	0.008	0.003	0.012	0.012	0.004
	(0.006)	(0.010)	(0.007)	(0.007)	(0.018)	(0.015)	(0.008)	(0.008)
				Transition to Retirement (Early)				
Post-Shock	0.007	0.013	0.004	0.004	0.017	0.004	0.008	0.004
	(0.006)	(0.009)	(0.008)	(0.007)	(0.019)	(0.016)	(0.009)	(0.008)
				Transition to Retirement (Not Early)				
Post-Shock	0.007	0.005	0.009	0.010*	0.004	0.005	0.003	0.008
	(0.004)	(0.007)	(0.006)	(0.005)	(0.013)	(0.013)	(0.006)	(0.006)

Notes: Coefficients show overall treatment effects for equation (1). All models also include age dummies, wave dummies, and individual dummies. Standard errors clustered by individual.

Source: Authors' calculations from HRS data.

Table 4: Impact of Retirement on Health

	Self-Reported Health	Doctor Visits	Health Conditions	Vigorous Exercise	Drinking	Smoking	Self-Reported Health	Doctor Visits	Health Conditions	Vigorous Exercise	Drinking	Smoking
			Full Sample						*Black*			
Non-Health Reasons	0.031*	-0.351	0.029*	0.050***	-0.006	-0.008	0.024	0.868	0.045	0.093***	-0.046*	0.004
	(0.019)	(0.243)	(0.016)	(0.013)	(0.008)	(0.006)	(0.056)	(0.626)	(0.042)	(0.033)	(0.024)	(0.018)
Health Reasons	0.040	2.419***	0.270***	-0.008	-0.007	-0.028***	0.032	5.219*	0.317***	0.077*	0.033	-0.052**
	(0.034)	(0.881)	(0.028)	(0.017)	(0.013)	(0.010)	(0.073)	(2.799)	(0.067)	(0.041)	(0.028)	(0.023)
Not Early	0.007	0.203	0.074***	0.069***	0.019	-0.013	-0.084	2.316**	0.080	0.138***	-0.039	-0.018
	(0.028)	(0.414)	(0.024)	(0.020)	(0.014)	(0.009)	(0.073)	(1.081)	(0.056)	(0.042)	(0.035)	(0.025)
Early	0.052**	0.553	0.102***	0.015	-0.012	-0.016**	0.066	2.570	0.175***	0.055	-0.004	-0.025
	(0.022)	(0.435)	(0.018)	(0.013)	(0.009)	(0.007)	(0.060)	(1.706)	(0.052)	(0.035)	(0.024)	(0.020)
			Women						*Hispanic*			
Non-Health Reasons	0.028	-0.442	0.024	0.076***	-0.012	-0.014	-0.002	-1.010	-0.011	0.081	0.006	0.011
	(0.028)	(0.453)	(0.024)	(0.019)	(0.013)	(0.009)	(0.084)	(0.818)	(0.050)	(0.049)	(0.039)	(0.023)
Health Reasons	0.102**	-0.340	0.207***	0.012	0.006	-0.023	0.038	-0.675	0.219**	-0.007	0.003	-0.044
	(0.047)	(1.135)	(0.040)	(0.024)	(0.019)	(0.014)	(0.101)	(1.294)	(0.105)	(0.055)	(0.049)	(0.027)
Not Early	-0.029	0.818	0.089**	0.079***	0.022	-0.017	-0.069	-1.621	-0.099	0.161*	0.009	-0.004
	(0.042)	(0.774)	(0.035)	(0.029)	(0.022)	(0.014)	(0.136)	(2.163)	(0.107)	(0.089)	(0.050)	(0.033)
Early	0.078**	-0.850	0.076***	0.038**	-0.015	-0.018*	0.066	-1.022	0.084	0.014	0.002	-0.019
	(0.032)	(0.603)	(0.027)	(0.019)	(0.013)	(0.010)	(0.081)	(0.859)	(0.064)	(0.047)	(0.038)	(0.025)
			Men						*High School*			
Non-Health Reasons	0.035	-0.292	0.037*	0.032*	-0.003	-0.005	0.044	-0.758**	0.040*	0.026	0.002	-0.004
	(0.025)	(0.262)	(0.021)	(0.017)	(0.011)	(0.008)	(0.029)	(0.346)	(0.023)	(0.019)	(0.013)	(0.010)
Health Reasons	-0.010	4.670***	0.323***	-0.023	-0.020	-0.034**	0.038	1.433	0.307***	-0.040*	-0.019	-0.030*
	(0.048)	(1.301)	(0.041)	(0.024)	(0.017)	(0.015)	(0.046)	(0.942)	(0.042)	(0.024)	(0.017)	(0.016)
Not Early	0.043	-0.327	0.066*	0.065**	0.022	-0.010	0.062	-0.339	0.094***	0.031	0.025	-0.008
	(0.039)	(0.438)	(0.034)	(0.028)	(0.017)	(0.013)	(0.043)	(0.578)	(0.034)	(0.031)	(0.021)	(0.015)
Early	0.031	1.712***	0.124***	-0.001	-0.013	-0.015*	0.043	0.384	0.122***	-0.014	-0.011	-0.015
	(0.031)	(0.614)	(0.025)	(0.018)	(0.012)	(0.009)	(0.033)	(0.536)	(0.029)	(0.019)	(0.014)	(0.012)
			White						*College*			
Non-Health Reasons	0.035*	-0.432	0.031*	0.042***	-0.004	-0.011*	0.020	0.059	0.017	0.078***	-0.012	-0.011
	(0.021)	(0.293)	(0.018)	(0.014)	(0.009)	(0.007)	(0.025)	(0.339)	(0.022)	(0.017)	(0.010)	(0.007)
Health Reasons	0.039	1.840**	0.251***	-0.024	-0.020	-0.014	0.073	3.394**	0.211***	0.040	0.012	-0.021
	(0.043)	(0.924)	(0.034)	(0.021)	(0.016)	(0.013)	(0.053)	(1.615)	(0.039)	(0.026)	(0.019)	(0.014)
Not Early	0.037	0.020	0.088***	0.052**	0.028*	-0.006	-0.028	0.981	0.051	0.118***	0.016	-0.015
	(0.032)	(0.486)	(0.029)	(0.024)	(0.016)	(0.010)	(0.038)	(0.610)	(0.035)	(0.027)	(0.018)	(0.012)
Early	0.044*	0.272	0.081***	0.008	-0.017*	-0.014*	0.058*	0.763	0.079***	0.049***	-0.013	-0.018**
	(0.025)	(0.429)	(0.021)	(0.015)	(0.010)	(0.008)	(0.030)	(0.656)	(0.025)	(0.018)	(0.011)	(0.008)

Notes: Coefficients show overall treatment effects for equation (2). All models also include age dummies, wave dummies, and individual dummies. Standard errors clustered by individual.

Source: Authors' calculations from HRS data.

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[1] Some individuals report that they ‘never’ plan to retire. Any observed retirements for these individuals are coded as early.

[2] This approach is implemented using Stata’s xthdidregress command with the regression adjustment option. We also employ the common base time option, estimating both pre- and post-treatment coefficients relative to the wave prior to the treatment. The common base time option is not the default. The default is to use the previous period as the base time for estimating pre-treatment coefficients, and the period before the treatment to estimate the post-treatment coefficients. However, as discussed by Roth (2024), this default option can produce misleading event study plots because the base time changes in the post-period relative to the pre-period. In contrast, the common base time option allows us to estimate pre- and post-treatments symmetrically.

[3] For comparison, we also estimate equation (1) using the traditional two-way fixed effects approach. Results are similar and available upon request.

[4] The vigorous exercise indicator has some inconsistencies across waves because of variation in the HRS questions. During the first six waves, respondents are asked a yes/no question whether they engage in vigorous physical activity three or more times per week. Starting in wave 7, respondents are asked whether they engage in vigorous physical activity daily, more than once a week, once a week, 1-3 times per month, or never. Our frequent vigorous exercise indicator is coded as one if the individual answers ‘yes’ to the question in waves 1-6, or if the individual indicates vigorous physical activity more than once per week in wave 7 or later. As the cross-wave differences in these indicators are the same for the treatment and control groups, we do not expect this inconsistency to introduce bias. In other words, it should be absorbed by the wave dummies in equation (2).

[5] For comparison, we also estimate equation (2) using the traditional two-way fixed effects approach. Results are similar and available upon request.

[6] Throughout this paper and Appendix, we only present event study plots for the full sample. Event study results for the subgroups are available upon request.