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Causal Effects of Retirement Timing on Subjective Physical and Emotional Health

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Objectives. This article explores the effects of the timing of retirement on subjective physical and emotional health. Using panel data from the Health and Retirement Study (HRS), we test 4 theory-based hypotheses about these effects—that retirements maximize health when they happen earlier, later, anytime, or on time.

Method. We employ fixed and random effects regression models with instrumental variables to estimate the short- and long-term causal effects of retirement timing on self-reported health and depressive symptoms.

Results. Early retirements—those occurring prior to traditional and legal retirement age—dampen health.

Discussion. Workers who begin their retirement transition before cultural and institutional timetables experience the worst health outcomes; this finding offers partial support to the psychosocial-materialist approach that emphasizes the benefits of retiring later. Continued employment after traditionally expected retirement age, however, offers no health benefits. In combination, these findings offer some support for the cultural-institutional approach but suggest that we need to modify our understanding of how cultural-institutional forces operate. Retiring too early can be problematic but no disadvantages are associated with late retirements. Raising the retirement age, therefore, could potentially reduce subjective health of retirees by expanding the group of those whose retirements would be considered early.

Key Words: Depressive symptoms—Instrumental variables—Mental health—Retirement timing—Self-reported health.

THIS article explores the effects of the timing of retirement on subjective physical and emotional health. Given increased attention paid to raising the retirement age around the world and the economic benefits often associated with delaying retirement, it is important to develop a better understanding of how recent retirees self-assess their overall physical and emotional health depending on the timing of their retirement transitions. The timing of retirement can have implications not only for an individual's retirement income, the economy, and the social security system, but also for an individual's subjective physical and emotional health.

Although the research on health effects of retirement has proliferated (Ekerdt, 2010; Kim & Moen, 2002), this research paid relatively little attention to the potentially moderating role of timing. Prior empirical research and theoretical arguments can be organized around four competing approaches on how the timing of retirement might influence health. Early retirement may be associated with subjective physical and emotional health gains immediately following retirement, as individuals experience a reduction in employment-related stress and greater opportunities for leisure and exercise (Jokela et al., 2010; Westerlund et al., 2009). Conversely, later retirement may protect health because working longer provides individuals with greater financial resources (Alavinia & Burdorf, 2008; Dave, Rashad, & Spasojevic, 2008). It could also be that retirement timing plays a rather minor role in shaping how subjective physical and emotional health responds to retirement (Butterworth et al., 2006; van Solinge, 2007). Yet, retiring around the expected age may be optimal because the retirement experience is then congruous with the broader cultural and institutional context (Börsch-Supan & Jürges, 2009).

Empirical research on the effects of retirement timing sheds little light on these disagreements as it is scarce and suffers from important methodological limitations that have resulted in mixed findings. A major shortcoming is the dearth of studies adjusting for endogeneity bias—that is, potential reverse causality in the relationship between retirement timing and health as well as confounding effects of unobserved factors on health. In addition, prior research has not explored a potentially curvilinear relationship between timing and health.

We fill these gaps by using panel data and instrumental variable fixed and random effects regression models focusing on variation in retirement timing that is exogenous to health to test the four competing explanations—that retirements maximize subjective physical and emotional health when they happen earlier, later, anytime, or on time. Specifically, we use two variables—early retirement window offer and changes in the U.S. Social Security regulations—to instrument retirement timing and assess its effect on health.

We begin by outlining the theoretical and empirical orientation of this study. Next, we describe the panel data

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from the Health and Retirement Study (HRS) that we use to assess the effects of retirement timing on health. We use self-reported subjective health as a measure of physical health, and the number of depressive symptoms (reversed) as a measure of emotional health. After describing variables and methods, we present the results of fixed and random effects models with instrumental variables that estimate short and long-term causal effects of retirement timing on the two health outcomes. Finally, we conclude by considering implications of our findings for the theoretical models of retirement timing as well as for social policy promoting labor force participation at older ages.

Literature Review

Four Approaches to the Relationship Between Retirement Timing and Health

The effects of retirement timing on health are not well understood. Prior studies on retirement and health span a variety of scientific disciplines, but few of them focus on the *timing* of the retirement transition or pose explicit hypotheses about the effects of timing. Despite these limitations, prior work provides arguments, propositions, and empirical evidence that can be organized around four competing approaches on how timing of retirement might influence health. We summarize these approaches in Figure 1. Here, we specify the position of each of these approaches on three issues: (a) when one should retire to maximize health, (b) what type of explanation justifies that ideal timing, and (c) what mechanisms are responsible for these effects. This categorization does not intend to reflect all of the nuances of any particular work but rather to highlight broad approaches to understanding the effects of retirement timing on health.

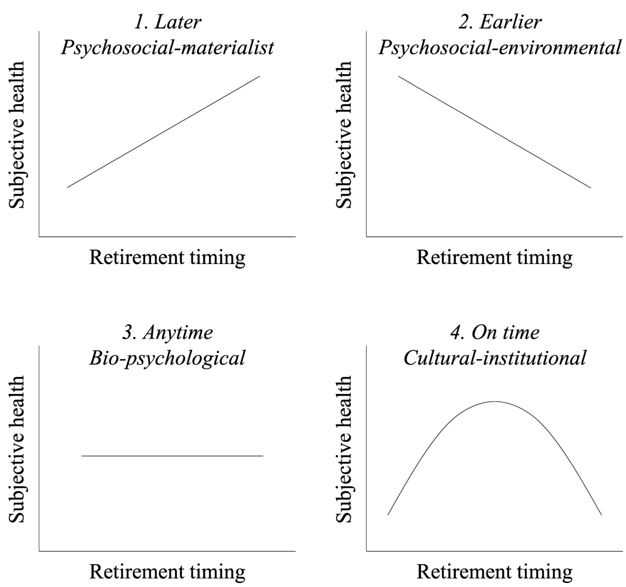

Figure 1. Expected functional relationship by theoretical approach.

The "psychosocial-materialist" approach suggests that retiring later benefits health because work forms a key part of the identity of older individuals and provides them with financial, social, and psychological resources. Such hypothesis is supported by empirical work showing that postponing retirement by just 2 years substantially increases retirement wealth (Munnell & Sass, 2008). Research also showed that working longer can provide opportunities to remain active and socially engaged (Taylor & Bengtson, 2001). In addition, a large body of empirical studies have identified various adverse health effects and behaviors related to retirement (Behncke, 2012; Dave et al., 2008; Kim & Moen, 2002). Moreover, focusing specifically on timing, some studies found that early retirement was associated with poorer perceived health (Alavinia & Burdorf, 2008) as well as lower happiness (Börsch-Supan & Jürges, 2009); however, because these studies were cross-sectional, this association might be due to an underlying health shock that encouraged the act of retirement.

Whereas a psychosocial-materialist perspective stresses the positive aspects of work, a "psychosocial-environmental" approach emphasizes job-related stress and occupational risks. In this context, early retirement transitions may be associated with better health outcomes. This perspective relies on studies finding lower stress among retirees compared with working adults (Coursolle, Sweeney, Raymo, & Ho, 2010; Midanik, Soghikian, Ransom, & Tekawa, 1995; Westerlund et al., 2009) as well as increased opportunities for exercise and physical activity after retirement (Jokela et al., 2010; Midanik et al., 1995). In support of this perspective, some studies found associations between retirement and good mental health (Mein, Martikainen, Hemingway, Stansfeld, & Marmot, 2003), although these studies only used cross-sectional data and have not examined the timing of retirement.

A third competing view, which we call a "biopsychological" approach, sees retirement timing as not affecting health, which is viewed as strongly determined by genes and personality. This approach is supported by evidence showing ambiguous health effects of retirement (Butterworth et al., 2006; Mein et al., 2003). For example, looking at a panel of Dutch older workers, van Solinge (2007) shows variation in the health consequences of retirement across individuals and across health measures. This approach also may reflect elements of rational choice theory, which suggests that individuals voluntarily choose to exit the labor force. Rationally, workers will select optimal timing for this transition given their health stock, psychological predispositions, and economic circumstances. Thus, whereas this perspective sees individual physical and emotional health as affecting retirement timing, it suggests no causal effect in the other direction—that is, no effect of retirement timing on health.

Finally, the "cultural-institutional" approach suggests that retirement transitions that happen on time—that is,

at culturally and institutionally expected ages—produce better health outcomes than transitions that happen off-time. A cultural-institutional approach is typically found in life course literature (Dannefer, 2011); when applied to retirement, it emphasizes the role of norms and policies in shaping the health effects of retirement timing. Settersten (1998), for example, argues that age transition norms, sometimes termed cultural timetables, constitute widely shared constructs that use chronological age to order the timing of role transitions over the life course, such as the beginning and end of formal schooling, childbearing, and retirement. In accordance with such cultural timetables, retirement transitions can be defined as "late," "early," or "on time" based on one's age at the start of the transition (Börsch-Supan & Jürges, 2009). There is some evidence that individuals tend to have better physical and emotional health outcomes when their experiences are congruous with the cultural and institutional milieu (George, 2010). Transitions adhering to age norms may elicit less stress and more peer support compared with "off schedule" transitions (Van Solinge & Henkens, 2007). A cultural-institutional approach, thus, suggests that retiring around the same time as one's peers (likely around statutory retirement ages) may be associated with better health outcomes. Alternatively, retirement transitions that deviate from the cultural and institutional milieu related to retirement timing may be associated with worse health outcomes.

The four aforementioned approaches provide testable hypotheses on the impact of retirement timing on subjective physical and emotional health after retirement (see Figure 1): (1) the psychosocial-materialist hypothesis suggests that later retirements cause better health outcomes than early retirements, (2) the psychosocial-environmental hypothesis suggests that earlier retirements cause better health outcomes than later retirements, (3) the biopsychological hypothesis suggests that retirement timing has no significant causal effect on health, and (4) the cultural-institutional hypothesis suggests that retirements that happen around culturally and institutionally expected ages cause better health outcomes relative to both very early and very late retirements.

Evaluating the Causal Effects of Retirement Timing on Health

Existing empirical assessments of the effects of retirement on self-reported physical and emotional health yield mixed findings in regards to the potential role of timing. Numerous studies report an association between retiring later and better subjective health and emotional health outcomes (Dave et al., 2008; Jaeger & Holm, 2004); others, however, find that earlier retirement has advantageous health effects (Coursolle et al., 2010; Jokela et al., 2010; Mojon-Azzi, Sousa-Poza, & Widmer, 2007; Westerlund et al., 2009). A few studies support the possibility that retirements that happen on time maximize subjective health and mental health (e.g., Börsch-Supan & Jürges, 2009). Yet there are as many studies finding no significant relationship between retirement and health (Lindeboom, Portrait, & van de Berg, 2002; van Solinge, 2007).

The considerable variation in findings, we argue, might stem from methodological limitations of much of this research. A major shortcoming is the scarcity of analyses adjusting for endogeneity bias—that is, potential reverse causality in the relationship between retirement timing and health. Retirement is a bounded choice, and health is one of the most important factors that can have an effect on the timing of retirement, generating an endogeneity bias. In addition, various unobservable factors such as personality traits or genetic predispositions may further confound the relationship between retirement timing and health. Although the bulk of previous research has acknowledged endogeneity, it has not addressed it adequately. Many studies use cross-sectional data to evaluate the association between retirement and self-reported health and depression outcomes (e.g., Butterworth et al., 2006). Other studies use longitudinal data but only deal with endogeneity by controlling for baseline levels of health and modeling change (Lindeboom et al., 2002; Mein et al., 2003; Westerlund et al., 2009). Such studies do not ensure that only the variation in retirement timing that is exogenous to health is used as a predictor of health. Some longitudinal studies also lack nationally representative samples (Mein et al., 2003; Midanik et al., 1995). Finally, as previously mentioned, prior studies on retirement and health do not explicitly focus on the role of retirement timing as the main variable of interest.

To help address these limitations, we use nationally representative panel data employing instrumental variable approach to focus on variation in retirement timing that is exogenous to health. Several studies have used an instrumental variable approach to obtain estimates of the causal effects of the *act* of retirement on health (Behncke, 2012; Bound & Waidmann, 2008; Charles, 2004; Coe & Zemarro, 2011; Neuman, 2008), but, to our knowledge, only two studies have examined the effects of retirement *timing* using an instrumental variables approach. Rohwedder and Willis (2010) used cross-sectional data from 13 countries and employed cross-national variation in public policies as an instrument for retirement timing; they found that early retirement has a detrimental effect on cognitive ability but did not examine measures of health. Coe and Lindeboom (2008), using unexpected offers of early retirement windows as an instrument, found no significant effect of retirement timing on a variety of physical and psychological health outcomes for men. They acknowledged, however, that early retirement may have a positive but temporary beneficial impact on self-reported health and activities of daily living (ADL) limitations for highly educated workers. Neither of these two studies evaluated the possibility of a curvilinear effect of retirement timing on health as suggested by the cultural-institutional approach.

Taking into account the limitations of prior research, this article examines the effects of retirement timing on subjective physical health and emotional health by using panel data and instrumental variable regression models to evaluate whether the causal effects of retirement timing on health are linear, curvilinear, or nonexistent.

Method

Data and Sample

We use data from the HRS, a nationally representative, biennial panel survey of older Americans and their spouses who began in 1992 and has data available through 2010 (University of Michigan, 2012). We selected our sample from the 9,753 individuals, born between 1931 and 1941, that became HRS cohort respondents in 1992. Given our interest in the effects of the transition from the labor force into retirement, we used a labor force status variable (combining information from self-reported retirement status, working for pay, hours of work, and several other indicators) to omit 1,640 individuals who were partly or fully retired at the first wave and 1,489 individuals who were out of the labor force for reasons other than retirement at all observed time points or at least at all the observed time points prior to their retirement (if they retired). The resulting sample includes 6,624 individuals. We excluded all preretirement records when a respondent was not in the labor force currently, as well as those records when a respondent reported being retired currently but was out of the labor force at the wave directly preceding retirement. As a result, we used 56,796 records in our analyses. Only 210 individuals died before their transition to retirement; thus, mortality is unlikely to introduce a substantial selection bias into our results. Selection bias is also minimized because our sample includes those 333 individuals who are still not retired and remain in the labor force in 2010.

Dependent Variables

Our dependent variables are based on self-reports of health and self-assessments of depressive symptoms. Our measure of subjective (or self-reported) health is based on a question, “Would you say your health is excellent, very good, good, fair, or poor?” where 1 = “poor” and 5 = “excellent.” Our measure of emotional health assesses presence of depressive symptoms; the raw variable ranges from 0 for individuals who reported no depressive symptoms to 8 for individuals who reported all depressive symptoms included in the reduced Center for Epidemiologic Studies Depression (CESD) scale, which asks: “Now think about the past week and the feelings you have experienced. Please tell me if each of the following was true for you much of the time this past week: you were happy; you enjoyed life; you felt lonely; you felt depressed; you felt sad; you could not get going; you felt that everything you did was an effort; your sleep was restless.” As we used CESD as a measure of emotional health, we reversed the scale so that higher numbers indicate better health.

Retirement Timing and Instrumental Variables

Retirement status is measured with a dichotomy indicating whether a person has completed a transition to retirement, defined as reporting being either “partly retired” or “completely retired” at the current or at least one prior wave. Some individuals reported going back to labor force after retirement; we ignored such repeated transitions and considered a person to be retired once they have reported such a transition. To measure retirement timing, we use interactions of retirement status with current age (divided by 10 and centered at 60) and its squared term (used to test for potential curvilinear effects). Retirement status and its interactions with age and age squared are the core endogenous independent variables.

To avoid endogeneity bias and obtain estimates of the causal effects of retirement timing on self-reported health and depressive symptoms, we use two instrumental variables: changes to Social Security’s full retirement age and unexpected early retirement window offers. Using two instrumental variables adds variation in the timing of retirement that is exogenous to health.

Changes to the full retirement age are represented by a set of dichotomies indicating the number of extra months required to be entitled to full benefits depending on one’s year of birth (0, 2, 4, 6, or 8 months, with 0 months being the reference category). The Social Security Amendments of 1983 gradually increase the full retirement age from 65 to 67 between 2000 and 2027 to create incentives for continued employment at older ages and increase system financing (Gustman & Steinmeier, 2009). Although the 1931–1937 birth cohort is entitled to full benefits at age 65, each birth year starting with 1938 adds 2 months to one’s eligibility age. These changes to Social Security rules introduced exogenous variation in retirement timing (Mastrobuoni, 2009) because we can expect sharp jumps in benefits and retirement, but not sharp changes in subjective health for those specific birth years (Neuman, 2008). That makes these dichotomies to be effective instruments, although they introduce exogenous variation mostly at the higher end of the distribution of retirement ages.

Our early retirement window measure is a dichotomy coded 1 for respondents whose employers ever offered them a special incentive to retire. Such offers are typically unexpected, open for a short period of time, targeted to entire units or divisions rather than specific workers, and legally required to be unrelated to the health status of workers. Early retirement windows are strong predictors of retirement (Coe & Lindeboom, 2008). Because they may be offered before age 65 or even 62, this variable introduces exogenous variation to retirement timing beyond that produced by increases in the full retirement age.

As mentioned earlier, in order to assess potential linear and curvilinear effects of retirement timing on health, we use interactions between retirement status and age and age squared; in order to estimate models with such interactions, our analyses also include interaction terms between each of the instrumental variables and both age and age squared.

Controls

To isolate the effect of retirement timing on health and to take into account interindividual differences that may be causing older adults to disproportionally self-select or be channeled into earlier or later retirement transitions, we control for the following time-varying and time-invariant sociodemographic characteristics.

Time-varying controls include wealth, income, marital status, and spouse employment status. Total household wealth is the sum of all the assets minus all debts. Assets include the net value of primary and secondary residence, other real estates, vehicles, businesses, individual retirement accounts (IRAs), Keogh accounts, stocks, mutual funds, and investment trusts, as well as the value of checking, savings, or money market accounts, certificates of deposit (CDs), government savings bonds, Treasury bills, bonds and bond funds, and all other savings; debts include the value of all mortgages/land contracts on primary and secondary residence and the value of other home loans on primary residence. Wealth was measured in $1,000 units, adjusted by the Consumer Price Index (CPI) to 2007 real dollars, topcoded at $10 million, centered at $200,000, and then logarithmically transformed.

Total individual income was calculated as the sum of the individual earnings income, individual income from employer pension or annuity, from Social Security Disability Insurance (DI) or Supplemental Security Income (SSI), and from Social Security retirement; individual unemployment or workers' compensation; and individual income from other government transfers. Like wealth, total individual income was measured in $1,000 units, adjusted by CPI to 2007 real dollars, topcoded at $300,000, centered at $40,000, and then logarithmically transformed.

Marital status and spouse employment status were measured with a joint set of dichotomies indicating no spouse, a nonemployed spouse, and an employed spouse (the reference category).

Time-invariant controls include gender, race/ethnicity, education, and occupation type. Gender is coded 1 for women and 0 for men. Race/ethnicity is measured with dichotomies indicating non-Hispanic Black (including Black/African American), Hispanic/Latino/a, and other race/ethnicity (including American Indian/Alaskan Native, Asian/Pacific Islander, Brown/combination, and other), with the reference category being non-Hispanic White/Caucasian. Education is measured in years and centered around 12, which is roughly equivalent to high school education. Occupation type is measured for the job with the longest reported tenure, with dichotomies indicating blue-collar occupations (including farming/forestry/fishing, mechanics/repair, construction trade/extractors, precision production, operators, and members of armed forces) and clerical/sales/service occupations (including sales, clerical/administrative support, and services occupations such as cleaning, protection, food preparation, health services, and personal services), with professional/managerial occupations being the reference category.

Descriptive statistics for health outcomes, retirement timing, instruments, and control variables are presented in Table 1.

Analytic Strategy

The panel nature of the HRS is well-suited for assessing the effects of retirement timing on health. For many years, research used cross-sectional designs to study this topic; such designs, however, raise serious concerns about self-selection and endogeneity biases. We take advantage of the longitudinal nature of the HRS by employing fixed and random effects regression models with instrumental variables to estimate the causal effect of retirement timing on self-reported health and depressive symptoms shortly after retirement.

We use the instrumental variables approach because it is considered to be one of the best ways to estimate causal links in the presence of an endogeneity bias problem, provided that appropriate instrumental variables can be identified (Gangl, 2010). The key endogeneity bias problem when estimating effects of retirement on health is that, to the extent that health shocks drive individuals out of the labor market, the negative effects of retirement might be overestimated. By using instrumental variables, we are able to eliminate potential reverse causation effects from our estimates—that is, we ensure that the estimated effects of retirement timing are not due to health-related selection into retirement. Specifically, we limit our estimates of the effects of retirement timing to the effects of the portion of variation in retirement timing that is caused by variation in our instrumental variables; any other effects of retirement timing (the ones possibly contaminated by reverse causal processes) are not included in the results we are presenting. That is, because our instrumental variables are not linked to health, the effects of retirement timing that we are reporting also cannot be due to impact of health on retirement decisions.

Our analyses included two sets of models: One set focused on the short-term effects of the retirement transition and thus dropped all records subsequent to the wave when an individual reported a transition to retirement for the first time. We used all the available records prior to the first transition to retirement except for time points when respondents were out of the labor force. The second set focused on the more long-term effects of retirement and included all the records subsequent to the first transition to retirement.

Table 1. Descriptive Statistics

Variables	First wave Mean or % (*SD*)	Last wave Mean or % (*SD*)	All waves Mean or % (*SD*)
Health outcomes			
Self-reported health	3.69 (1.04)	3.19 (1.09)	3.40 (1.17)
Emotional health	7.41 (1.11)	6.86 (1.90)	6.92 (2.02)
Retirement			
Retirement status (%)	0.00	93.26	53.02
Current age	55.67 (3.12)	73.64 (3.08)	64.01 (6.48)
Instrumental variables			
Early retirement window (%)	4.78	33.23	20.36
+0 months retirement age (%)	56.84	55.03	56.08
+2 months retirement age (%)	10.19	10.66	10.47
+4 months retirement age (%)	11.24	11.11	11.04
+6 months retirement age (%)	10.81	11.93	11.35
+8 months retirement age (%)	10.92	11.27	11.06
Time-varying controls			
Wealth (in $1,000)	348.09 (710.50)	710.89 (1202.24)	576.11 (1100.43)
Income (in $1,000)	45.99 (41.11)	43.36 (50.00)	44.59 (57.25)
Employed spouse (reference) (%)	49.92	7.84	27.79
No spouse (%)	25.37	40.39	31.68
Spouse not employed (%)	24.70	51.77	40.54
Time-invariant controls			
Women (%)	46.31	48.86	47.34
White non-Hispanic (reference) (%)	74.43	75.61	75.22
Black non-Hispanic (%)	15.93	14.61	15.07
Other non-Hispanic (%)	1.65	1.78	1.72
Hispanic (%)	7.99	8.01	7.99
Years of education	12.48 (3.04)	12.65 (2.99)	12.56 (3.16)
Professional/managerial worker (reference) (%)	31.03	33.06	32.00
Blue-collar worker (%)	31.93	29.72	30.95
Clerical/sales/service worker (%)	37.04	37.23	37.05
Number of observations	6,275	4,938	56,796

Note: Raw values (before transformations and centering) and standard deviations are reported for continuous variables. All statistics are reported for the long-term sample (i.e., for the sample that included all the records subsequent to the first transition to retirement as well as the preretirement records).

In both sets of the models, we use increases to the full retirement age and early retirement window offers as instruments. Both instruments combined are strong predictors of retirement timing, as indicated by the first-stage *F* values larger than 10 as well as by statistically significant values of Anderson–Rubin Wald test (Anderson & Rubin, 1949) and the Stock–Wright LM S statistic (Stock & Wright, 2000), as well as by Kleibergen–Paap rk F statistic values that exceed the 10% maximal instrumental variable bias critical value of 9.85 (Kleibergen & Paap, 2006; Stock & Yogo, 2005).

We utilized both fixed and random effects models as both have distinct advantages. By estimating person-level residuals as fixed effects that are allowed to be correlated with predictors, fixed effects models adjust for all individual-specific, time-invariant sources of endogeneity, both observed and unobserved. These models only utilize data on change within individuals over time and focus on predicting changes in health based on changes in retirement status. Our fixed effects models also include time-varying control variables and use instrumental variables; however, as one of our instruments (changes to the full retirement age) is time-invariant, fixed effects models did not include that instrument itself, only its interactions with age and age squared.

In contrast, random effects models can include both of our instruments along with their interactions with age and age squared; moreover, random effects models are more efficient as they utilize both the information on changes over time and on differences across individuals. However, unlike fixed effects models, they have to assume that person-level residuals are uncorrelated with predictors and that differences across individuals have the same relationship to health outcomes as changes over time within individuals.

Fixed effects models in this analysis were estimated using two-stage least squares and cluster-robust standard errors in order to account for the nested data structure of HRS (individuals within households). Random effects models were estimated using generalized least squares (GLS) and without cluster corrections due to software limitations; however, this is unlikely to be problematic, because the results of the fixed effects models with cluster-robust standard errors were very similar to those obtained without cluster corrections.

In both sets of models, we centered all continuous predictors around specific values to facilitate interpretation of the constant and calculation of predicted values. Prior to estimating multivariate models, we examined the data for univariate normality and bivariate linearity. Our outcome

variables, self-reported physical and emotional health, have significantly nonnormal distributions; however, no transformations can substantially improve these distributions. For independent variables, when necessary and possible, we employed corrective transformations; these were documented in the descriptions of variables earlier. To handle missing data, we performed a multiple imputation with chained equations (MICE; Royston 2004); our model estimates are based on 20 imputations. In total, 7.23% of all the data points used in the estimations were imputed.

Results

Table 2 reports the results of the instrumental variables fixed and random effects regression models for self-reported physical health. As negative coefficients for retirement in all four models (fixed effects and random effects, short- and long-term) show, at 60 years, retirement has a negative effect on self-reported health. However, as the significant interactions between retirement status and age and age squared demonstrate, that negative effect varies depending on retirement timing, and it does so in a curvilinear fashion.

To better illustrate these effects, we examine predicted values of self-reported health for those recently retired and those in the labor force as well as the difference between the two groups (Figure 2). These predicted values were calculated based on the short-term random effects model for an individual who scored 0 on all control variables, that is, for a White man in a professional/managerial occupation, with 12 years of education, $40,000 income, $200,000 wealth, and a nonemployed spouse. (We do not present figures using different configurations of independent variables because that only changes the absolute levels of these curves but not the overall shape or the difference between those retired and those in the labor force. Moreover, figures based on fixed effects results as well as those for long-term models were similar.)

As the upper panel in Figure 2 illustrates, for those not retired, self-reported health declines as they age. In contrast, self-reported health of those who recently retired is at its maximum for those who retire around 67 years—it is significantly lower for those who retire either earlier or later, as indicated by the significant quadratic term for age among those recently retired. However, as the difference score in the lower panel (and the confidence interval [CI] around it) shows, recent retirement produces lower self-reported health only among those younger than 62 years; after that, we find no significant effect of retirement on self-reported health (the CI includes 0, which means

Table 2. Instrumental Variables Regression Results for the Effects of Retirement Timing on Self-reported Physical Health

	Short-term model				Long-term model			
	Fixed effects	SE	Random effects	SE	Fixed effects	SE	Random effects	SE
Retirement								
Retirement	−0.37**	0.14	−0.38***	0.10	−0.29*	0.12	−0.34***	0.09
Current age	−0.39***	0.08	−0.44***	0.08	−0.43**	0.14	−0.36**	0.11
Current age squared	0.01	0.13	−0.09	0.11	−0.02	0.16	0.01	0.13
Retirement × current age	1.04*	0.48	1.47***	0.35	0.60**	0.19	0.57***	0.16
Retirement × current age squared	−0.60*	0.28	−0.59*	0.24	−0.23	0.14	−0.26*	0.11
Time-varying controls								
Wealth (in $1,000, logged)	0.05***	0.01	0.11***	0.01	0.05***	0.01	0.09***	0.01
Income (in $1,000, logged)	−0.00	0.01	0.01	0.01	0.01	0.01	0.01	0.01
No spouse[a]	0.12***	0.02	0.01	0.02	0.07*	0.03	0.02	0.02
Spouse not employed[a]	0.02	0.02	−0.01	0.02	−0.00	0.02	−0.01	0.02
Time-invariant controls								
Women	—		0.01	0.02	—		0.03	0.02
Black non-Hispanic[b]	—		−0.28***	0.03	—		−0.26***	0.03
Other non-Hispanic[b]	—		−0.11	0.08	—		−0.13	0.08
Hispanic[b]	—		−0.20***	0.04	—		−0.17***	0.04
Years of education	—		0.07***	0.00	—		0.06***	0.00
Blue-collar worker[c]	—		−0.13***	0.03	—		−0.12***	0.03
Clerical/sales/service worker[c]	—		−0.08**	0.03	—		−0.08**	0.03
Constant	—		3.63***	0.03	—		3.61***	0.03
Number of observations	31,841		32,092		56,544		56,796	
Number of individuals	6,387		6,624		6,372		6,624	
Number of households	5,338		—		5,333		—	
Average number of observations per individual	4.99		4.83		8.87		8.57	
Kleibergen–Paap rk Wald F	10.61		—		20.01		—	
Anderson–Rubin Wald	2.91**		—		7.86***		—	

Notes: SE = Standard error. Statistically significant coefficients are indicated as follows: **p* < .05. ***p* < .01. ****p* < .001 (two-tailed tests).
[a]Reference category is "employed spouse."
[b]Reference category is "White, non-Hispanic."
[c]Reference category is "professional or managerial worker."

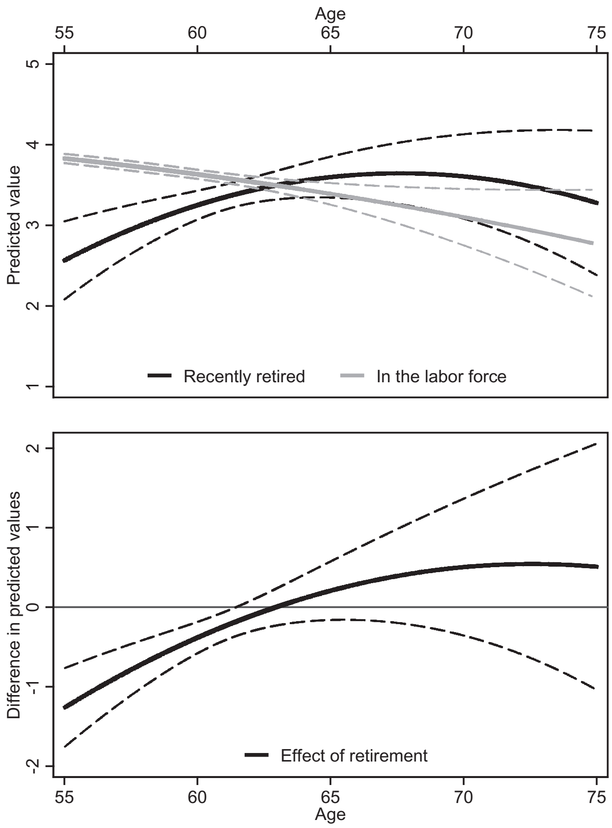

Figure 2. Short-term effects of retirement on self-reported physical health. Predicted values and difference scores are based on the short-term random effects instrumental variable regression model; they were calculated holding all controls at zero. 95% confidence intervals (CI) are represented with dashed lines.

no significant differences between retirees and those employed). Given the uncertainty that CI shows, this lower panel can corresponds to either panel 1 or panel 4 in Figure 1, offering support to either the psychosocial-materialist approach or the cultural-institutional approach. These findings do offer conclusive evidence that individuals get the best subjective health outcomes if they retire after 62 years but not earlier.

Table 3 reports the results of the instrumental variables fixed and random effects regression models for emotional health. Once again, we find negative coefficients for retirement status in all four models, indicating that retirement has a negative effect on emotional health (i.e., it promotes depressive symptoms) at 60 years. However, as the significant positive interaction between retirement status and age shows, the negative effect is reduced if a person retires later.

Figure 3 illustrates these processes using predicted values of emotional health for those recently retired and those in the labor force (upper panel) as well as the difference between the two groups (lower panel). (Like the results presented in Figure 2, these predictions are based on random effects short-term model and are calculated for an individual with all 0 values on controls.) As the upper panel shows, for those not retired, emotional health declines with age and that decline becomes steeper the older one gets. In contrast, emotional health of those recently retired appears to be higher when the retirement age is higher. As the difference score in the lower panel shows, those recently retired and those in the labor force have similar levels of emotional health when retirement occurs "on time," that is, between 62 and 67 years (i.e., the CI around the difference score includes 0 in this age range). Retirement at a relatively young age, however, can substantially lower one's emotional health. In contrast, retirement at a relatively old age can provide a boost to one's emotional health; however, such a boost is only significant in the short-term model; in the long-term model, the CI for those aged 62 and older always includes 0. (We do not present figures for the long-term models because this is their only substantive difference from the short-term figures.) Note that the figure in the lower panel most closely corresponds to panel 1 in Figure 1, offering support to the psychosocial-materialist approach. In sum, these findings suggest that individuals get the best emotional health outcomes if they retire "on time" or later, with late retirements being especially likely to produce a substantial boost to one's emotional health but only in the short run.

Finally, control variables have essentially the expected effects. White non-Hispanics as well as those with more wealth and education tend to have the best physical and emotional health outcomes; income, however, has no significant effect after controlling for wealth. Women and men do not differ in terms of physical health, but men are better off in terms of emotional health. Those in professional/managerial occupations have the best physical health outcomes, but they do not differ from those in blue-collar or clerical/sales/service occupations in terms of emotional health. Respondents without a spouse have worse emotional health than married individuals (regardless of spouse's employment status), but, surprisingly, as fixed effects models show, those who recently lost a spouse have better physical health than those who are still married.

Discussion

This study used panel data from the HRS, fixed effects and random effects models, and instrumental variable techniques to test four competing hypotheses about the causal effect of retirement timing on subjective physical and emotional health—that retirements maximize health when they happen earlier, later, anytime, or on time. Our results suggest that when both subjective physical health and emotional health are considered, the best outcomes are observed for retirement transitions happening "on time" or later—that is, at or after 62 years, the early eligibility age for claiming Social Security retirement benefits and the usual age of retirement in the United States.

Table 3. Instrumental Variables Regression Results for the Effects of Retirement Timing on Emotional Health

	Short-term model				Long-term model			
	Fixed effects	SE	Random effects	SE	Fixed effects	SE	Random effects	SE
Retirement								
Retirement	−1.90***	0.27	−1.54***	0.20	−1.39***	0.20	−1.26***	0.15
Current age	−0.33	0.21	−0.61**	0.20	−0.26	0.30	−0.38	0.27
Current age squared	−0.54	0.33	−0.63*	0.29	−0.41	0.40	−0.53	0.38
Retirement × current age	4.51***	1.07	4.26***	0.75	2.04***	0.48	2.07***	0.45
Retirement × current age squared	−0.61	0.70	0.14	0.73	−0.38	0.32	−0.24	0.29
Time−varying controls								
Wealth (in $1,000, logged)	0.11***	0.02	0.17***	0.02	0.10***	0.02	0.15***	0.01
Income (in $1,000, logged)	−0.01	0.03	0.03	0.02	−0.01	0.02	0.01	0.02
No spouse[a]	−0.38***	0.07	−0.34***	0.04	−0.35***	0.05	−0.37***	0.04
Spouse not employed[a]	−0.01	0.05	−0.03	0.04	0.01	0.03	−0.00	0.03
Time−invariant controls								
Female	—		−0.15***	0.03	—		−0.16***	0.03
Black non-Hispanic[b]	—		−0.14***	0.04	—		−0.09*	0.04
Other non-Hispanic[b]	—		−0.28*	0.11	—		−0.25*	0.11
Hispanic[b]	—		−0.33***	0.06	—		−0.25***	0.06
Years of education	—		0.07***	0.01	—		0.07***	0.01
Blue-collar worker[c]	—		−0.06	0.04	—		−0.07	0.04
Clerical/sales/service worker[c]	—		−0.07	0.04	—		−0.08*	0.04
Constant	—		7.43***	0.04	—		7.46***	0.05
Number of observations	31,841		32,092		56,544		56,796	
Number of individuals	6,387		6,624		6,372		6,624	
Number of households	5,338		—		5,333		—	
Average number of observations per individual	4.99		4.83		8.87		8.57	
Kleibergen–Paap rk Wald F	10.61		—		20.01		—	
Anderson–Rubin Wald	19.45***		—		22.21***		—	

Notes: SE = Standard Error. Statistically significant coefficients are indicated as follows: **p* < .05. ***p* < .01. ****p* < .001 (two-tailed tests).
[a]Reference category is "employed spouse."
[b]Reference category is "White, non-Hispanic."
[c]Reference category is "professional or managerial worker."

Our findings on subjective physical and emotional health offer partial support to both the psychosocial-materialist hypothesis suggesting that retiring later is preferred and the cultural-institutional hypothesis suggesting that "on time" retirements produce the best outcomes. On the one hand, for those younger than 62 years, work clearly serves to promote better subjective physical and emotional health as it likely serves as a source of identity and resources. After 62 years, however, it appears that physical and emotional health of those who continue to work declines, whereas retirees' physical health only enters a decline trajectory after 67 years; moreover, those who retire get a short-term boost in terms of emotional health. Thus, contrary to what the psychosocial-materialist hypothesis suggests, working past 62 years does not appear to produce any benefits as far as subjective physical and emotional health is concerned.

Therefore, these findings support the cultural-institutional hypothesis as they demonstrate the importance of traditional retirement age in shaping subjective physical and emotional health outcomes. These findings, however, also suggest that we need to modify our understanding of how cultural-institutional forces actually operate. Although we do find that retiring too early can be problematic, we do not find any pronounced disadvantages associated with late retirements.

Importantly, our findings clearly do not support the biopsychological hypothesis characterizing retirement timing as having no effect on subjective health given the persistent influence of genes and personality on the adjustment to new situations. Furthermore, the findings do not support the psychosocial-environmental hypothesis suggesting that retiring earlier maximizes subjective health because work is a source of stress and risks. In fact, we find that retiring early (before 62 years) can be detrimental for both subjective physical and emotional health.

This article contributes to the retirement literature by offering empirical support for the cultural-institutional approach that emphasizes the role of norms and policies in shaping the effects of retirement as well as some support for the psychosocial-materialist approach that stresses potential financial, social, and physical benefits of continued employment. Specifically, we find that at earlier ages (prior to 62 years, i.e., the usual retirement age in the United States), staying in the labor force has better subjective health outcomes than retiring, but at later ages, continued employment has no clear benefits. These findings speak to broader debates about the importance of institutional and cultural expectations for perceptions of health during late-life transitions. In a similar vein, results

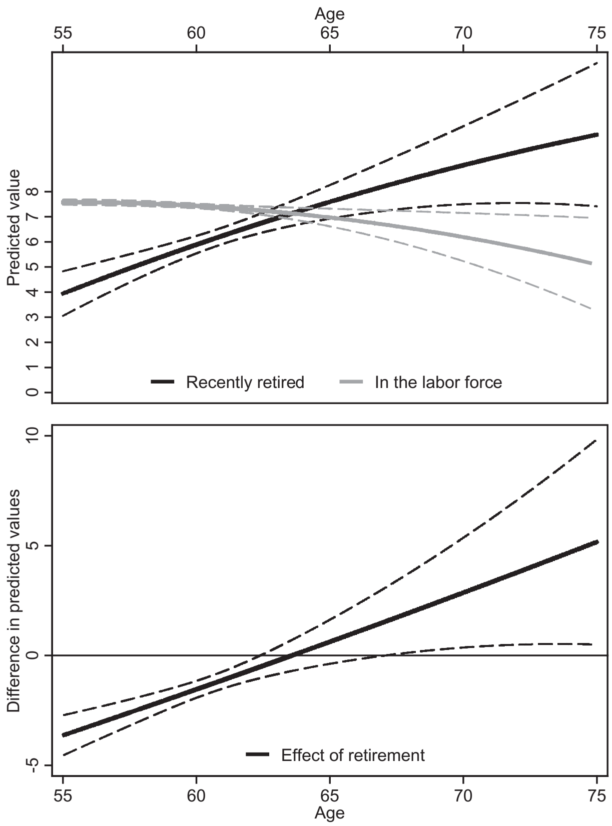

Figure 3. Short-term effects of retirement on emotional health. Predicted values and difference scores are based on the short-term random effects instrumental variable regression model; they were calculated holding all controls at zero. 95% CIs are represented with dashed lines. As linear regression was used, some predicted values fall outside of the range of the original reversed Center for Epidemiologic Studies Depression (CESD) scale (0–8).

offer empirical support for the notion in the life course approach that the timing of transitions is important for how individuals experience them (Elder, Johnson, & Crosnoe, 2003). The results are also consistent with the argument that off-time transitions may be a source of stress, as evident by the decline in subjective physical and emotional health of those who retire too early. To the extent that there is an institutionally reinforced and culturally expected age to begin a retirement transition, it appears to be a one-sided expectation: Earlier transitions have negative consequences but later transitions do not.

This study also makes a methodological contribution. Prior studies on health response to retirement suffer from important methodological limitations that have resulted in mixed findings and contradictory theories. This is the first study to simultaneously adjust for the potential reverse causality in the relationship between retirement timing and health and explore the curvilinear relationship between timing and changes in self-assessed health outcomes suggested by the cultural-institutional approach.

Our findings also have policy salience. In the context of rising longevity, a variety of initiatives encouraging delayed retirement has gained the attention of policymakers (Munnell & Sass, 2008). Although a plethora of research has focused on the impact of delaying retirement on economic well-being (Munnell & Sass, 2008), this study explores retirement timing in relation to subjective physical health and emotional health. When evaluating changes to retirement age and considering labor force policies to promote longer working lives, it is important to take into consideration the effects on both physical and emotional health and their potential costs, such as health care expenses. Our study suggests that waiting a few years after 62 years does not have a substantial detrimental effect on subjective physical and emotional health. Our findings also show that retirements that happen too early are especially problematic: Retiring before 62 years significantly increases the risk of poor subjective physical health and emotional health.

These findings raise the question of how the observed relationship between retirement timing and health would be affected by a change in Social Security's age of early eligibility. This question is increasingly important as policymakers around the globe consider promoting delayed retirement. Institutional definitions of retirement age certainly influence cultural norms and boundaries related to the expected timing of retirement: Tying access to public and private pension benefits to chronological age effectively institutionalizes withdrawal from employment around benefit eligibility ages (Settersten, 1998; van Solinge & Henkens, 2007). However, retirement choices are complex; therefore, other forces that currently lead people to retire at a given age might persist. This can result in a larger fraction of the retiree population to be perceived as retiring early given new regulations and expectations. As we found that early retirements are detrimental for subjective physical and emotional health, this outcome would be troublesome as a larger group of people would experience these negative effects. Thus, insofar as the actual ages at which people retire might lag behind changes in institutional definitions, increases in legal retirement ages could potentially reduce subjective health of retirees by expanding the group of those whose retirements would be considered early (Riley, Kahn & Foner, 1994).

This article has a number of limitations that offer some suggestions for future research. First, future research should include more recent observations collected on the HRS cohort and other birth cohorts. That should make it possible to determine whether the optimal timing for retirement is the same across periods and cohorts. It would also increase the number of observations and therefore boost the precision of estimates, especially among those still in the labor force at late ages. Second, future research should also examine similar processes in other countries to evaluate the effects of cross-national variation in normative retirement ages on health outcomes. Third, future research should examine whether

different groups of people would respond to changing incentives to work longer in different ways. Scholars could explore interactions between retirement timing and demographic characteristics such as race/ethnicity, gender, and occupation. It is possible for the relationship between retirement timing and health to be curvilinear for some groups but linear or nonexistent for others; future research should examine that possibility.

In sum, our study highlights the role of institutional policies and cultural norms in shaping health. Overall, retirement transitions appear to be more beneficial if they happen either on time or late—but certainly no earlier than it is institutionally and culturally prescribed. Deviating from conventional retirement age by retiring early appears to have detrimental effects on self-reported physical and emotional health, whereas late retirements are not associated with subjective health penalties.

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