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Retiree Health Benefits and the Decision to Retire

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Abstract

We estimate the effect of employer offers of retiree health benefits (RHBs) on the timing of retirement using a sample of men observed over a period of up to 12 years in the Health and Retirement Study (HRS). Our main concern is that such estimates may be contaminated by unobserved heterogeneity—workers with a taste for early retirement sort into jobs offering RHBs. We attempt to address this concern by using a fixed-effects estimator, which yields substantially smaller estimates of the effect of RHB offers than estimators that do not attempt to control for unobservables. The findings suggest that an RHB offer increased the probability of retirement by 14 percent on average for men born between 1931 and 1941.

JEL classification: J26; I18; D14
Keywords: Retirement; Health Insurance; Employee Benefits; Unobserved Effects

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I. Introduction

An unusual feature of labor markets in the United States is that employers provide health insurance to both currently employed and retired workers. The link between employment and health insurance of current workers has consequences for economic policy and the functioning of labor markets that are of longstanding interest to economists: The labor supply effects of employer-provided health insurance (EPHI), compensating differentials for EPHI, the possibility of "job lock" or reduced mobility due to EPHI, and "crowd-out" of EPHI by public health insurance like Medicaid (for reviews, see Rosen 2000, Gruber and Madrian 2004, Madrian 2006, and Garrett and Chernew 2007).

Interest in the causes and consequences of employer-provided health benefits to retirees is more recent, in part for lack of appropriate data. However, retiree health benefits (RHBs) also raise research and policy issues, including their influence on the probability and timing of retirement, the implied consequences for employers, and their ramifications for the Social Security system and Medicare (Employee Benefit Research Institute 2009, chapter 26). During the last 15 years, RHB offers have become less common, especially for early retirees—those under age 65 who are not yet eligible for Medicare (Fronstin 2001, 2005; Marton and Woodbury 2007). Because early retirees may rely on RHBs for health insurance coverage, the decline in RHBs has been among the pressures to expand Medicare to individuals younger than age 65, and indeed to move toward universal health coverage (Johnson 2007).

Our goal in this paper is to add to the evidence on the effects of RHBs on retirement. We do this along two lines. First, we estimate unobserved effects models of retirement probability using 12 years of data (1992–2004) on older men from the Health and Retirement Study (HRS), a major longitudinal survey sponsored by the National Institute of Aging and conducted at the

University of Michigan (Institute for Social Research 2009). In contrast to earlier work, we identify the effect of RHBs from variation over time in RHB offers to individual workers, rather than from variation across individuals. This approach arguably controls for, or at least reduces, the correlation between RHB eligibility and unobserved factors affecting the probability of retirement. Second, because it is important to know whether RHBs influence the retirement decision of some workers more than others, and whether the influence of RHBs varies over the business cycle, we estimate models that allow the estimated effect of RHBs to vary among different subgroups of men and over time.

The main estimates suggest that an RHB offer increased the average retirement probability of older men by about 1.5 percentage points (or 14 percent) during 1992 through 2004, although this estimate is very imprecise and could be zero. The estimate is roughly half the estimated effect of RHBs in models that do not control for unobserved heterogeneity. The estimates of RHB effects on subgroups and for different time periods suggest that the retirement effects of RHBs were concentrated almost entirely on men in their early 60s and were largest during the slack labor market of 2000–2002.

Section II briefly reviews the existing literature on RHB coverage and retirement and discusses how our analysis contributes to this literature. Section III describes the retirement model we estimate and the HRS data we use, and section IV gives details of the variables we use to specify the model. Section V describes the empirical findings, and section VI discusses implications of the findings.

II. Previous Research

Early estimates of the effect of health insurance coverage on retirement used data from the Retirement History Survey, conducted mainly during the 1970s (Rust and Phelan 1997), the Survey of Income and Program Participation (Karoly and Rogowski 1994, Madrian 1994), the Current Population Survey (Gruber and Madrian 1995), and the National Medical Expenditure Survey (Madrian 1994). These studies uniformly conclude that availability of RHBs (or continuation coverage in the case of Gruber and Madrian) significantly increases the probability that an older worker will retire.

Hurd and McGarry (1993), Rogowski and Karoly (2000), Blau and Gilleskie (2001), Strumpf (2007), and Congdon-Hohman (2008) all estimate the effects of RHBs on retirement (or retirement expectations in the case of Hurd and McGarry) using HRS data. Hurd and McGarry (1993) examine wave 1 (1992) of the HRS and find that workers eligible to receive RHBs that are partly or fully paid by the employer are significantly less likely than other workers to report that they expect to work past age 62. Rogowski and Karoly (2000) and Blau and Gilleskie (2001) each take advantage of two waves of the HRS and find that workers with an offer of RHBs are significantly more likely to retire than workers without. In particular, Rogowski and Karoly (2000) find that workers with RHBs in 1992 were about 11 percentage points more likely to be retired in 1996 than those without. Blau and Gilleskie (2001) emphasize the importance of cost-sharing on the estimated effect of RHBs on retirement. They examine retirement transitions during 1992–1994 and find that RHBs increased the probability of retirement by 6 percentage points if the employer paid the full RHB premium, but only by 2 percentage points if retirees had to contribute to the RHB's cost. Johnson, Davidoff, and Perese (2003) also highlight the importance of RHB premium costs to the retirement decision, and Congdon-Hohman (2008) focuses on the health insurance of wives as a factor in husbands' retirement decisions.

Some recent studies of RHBs have obtained estimates of the effect of RHBs on retirement mainly as a byproduct of more comprehensive analyses. Ambitious papers by Blau and Gilleskie (2008) and Strumpf (2007) are in this vein. Blau and Gilleskie (2008) estimate a dynamic structural model of retirement, using the first four waves (1992–1998, or three transitions) of the HRS, with the goal of evaluating reforms in health policy. Strumpf (2007) focuses on RHBs' effects on health and health care costs; her estimates of the effect of RHBs on retirement are similar to those of Rogowski and Karoly (2000).

Concerns about the endogeneity of RHBs are a frequent refrain in this literature—see especially Blau and Gilleskie (2008). As McGarry (2004) points out, a fixed-effects estimator would be a natural way to handle unobserved heterogeneity in retirement decisions because it takes advantage of within-individual variation to identify the effects on retirement of factors like RHBs, pensions, housing wealth, and non-housing wealth, all of which could be associated with unobserved tastes for retirement. However, mainly because only two or three waves of the HRS data were available when it was conducted, previous research has not applied a fixed-effects estimator to eliminate unobserved heterogeneity. In the next section, we outline an approach that allows us to apply a fixed-effects estimator to the HRS data.

III. Approach to Estimation

Clearly, a key issue vexing past research on RHBs and retirement behavior is whether RHB-eligibility is correlated with unobserved individual characteristics associated with early retirement. It is plausible that workers with a taste for early retirement would sort into jobs offering health benefits to early retirees. Indeed, workers generally need to make such a selection with some foresight because employers often base RHB eligibility on age and service requirements. Typically, a worker must have reached age 55 and have five years of service to be eligible for RHBs (Employee Benefit Research Institute 2009). Estimators that do not take account of this unobserved heterogeneity would not identify the effect of RHBs on the probability of retirement and would be biased upward.

We address the problem of unobserved effects by taking advantage of well-known panel data methods. The HRS data we examine have information on six discrete two-year time intervals (seven interviews, each separated by about two years) starting in 1992, so we model the probability of worker i being retired at time t+1 as a function of observables and unobservables at time t:

$$P(retired_{i,t+1} = 1 \mid \bullet) = \mathbf{x}_{it}\boldsymbol{\beta} + \eta_t + c_i \tag{1}$$

where $\mathbf{x}_{it}$ is a vector of person-specific characteristics capturing the observed heterogeneity in the sample (these may be either time-varying or constant over time), η_t denotes transition-specific fixed effects (to account for economic and labor market conditions), and c_i denotes unobserved worker-specific effects. We specify $\mathbf{x}_{it}\boldsymbol{\beta}$ as follows:

$$\mathbf{x}_{it}\beta = \beta_1(rhb_{it}) + \beta_2(pension_{it}) + \beta_3(wealth_{it}) + \beta_4(demog_{it}) + \beta_5(spouse_{it}) + \beta_6(health_{it}) + \\ + \beta_7(jobchar_{it}) \tag{2}$$

where rhb_{it} denotes a set of dummies modeling whether worker i had an RHB offer in year t, $pension_{it}$ and $wealth_{it}$ are sets of indicators of the pension and nonpension wealth of worker i in year t, $demog_{it}$ denotes variables indicating age, race, and level of education, $health_{it}$ is a set of health indicators, $spouse_{it}$ is a set of dummies indicating whether worker i was married in year t and whether his spouse was working, and $jobchar_{it}$ is a set of job characteristic indicators.

Equation (1) is an unobserved-effects model for panel data, and we face a number of choices in estimating it. A computationally undemanding and easily interpreted approach is to estimate it as a linear probability model (LPM):

$$retired_{i,t+1} = \mathbf{x}_{it}\boldsymbol{\beta} + \eta_t + c_i + u_{it} \qquad (3)$$

where $retired_{i,t+1}$ equals 1 if individual i is retired at interview t+1, conditional on being a full-time worker in 1992 and not having retired before time t, and u_{it} is an idiosyncratic error. A key objection to the LPM—predictions of the retirement probability outside the unit interval—does not apply in this case because we estimate a fully saturated model, so fitted retirement probabilities are cell frequencies and cannot fall outside the unit interval (Wooldridge 2002, pp. 509–510). The other main objection to the LPM—heteroskedasticity—can be handled by computing Huber-White standard errors to correct for heteroskedasticity.

In keeping with past efforts to estimate the effect of RHBs on retirement, we could (and do) estimate equation (3) by pooled OLS; however, this poses two problems. First, if the individual fixed effects c_i are correlated with the observable characteristics $\mathbf{x}_{it}$, then estimates of $\boldsymbol{\beta}$ (β_1 in particular) will suffer from heterogeneity bias due to the omitted individual fixed effects. Second, pooled OLS combines the individual fixed effects c_i and the idiosyncratic error u_{it} into a single composite error, v_{it}, which will be serially correlated. This latter issue can be resolved by imposing structure on v_{it} and applying a random-effects estimator, but random-effects will still be biased for $\boldsymbol{\beta}$ if the individual fixed effects c_i are correlated with the observable characteristics $\mathbf{x}_{it}$.

A possible solution to the first (more serious) problem of heterogeneity bias, and the solution we adopt, is to apply a fixed-effects estimator to equation (3). This is feasible, at least in a linear model, because we have time-varying observations of rhb_{it} and other independent variables for the same workers. The fixed-effect estimator identifies the effect of RHBs on the timing of retirement from individual-specific variation over time in RHB eligibility.

It also would seem natural to apply nonlinear fixed-effects estimators, such as probit or logit, to the model, but as Wooldridge (2002, chapter 15), Cameron and Trivedi (2005, chapter 23), and Imbens and Wooldridge (2007) discuss, these are computationally difficult and, in the case of probit, inconsistent.[1] Mundlak (1978) and Chamberlain (1982) have suggested nonlinear "correlated random-effects" estimators for panel data that have many of the desirable features of fixed-effects estimators. However, the set up of the sample we use differs from that envisioned by the Mundlak-Chamberlain approach (because predictors at time t influence a decision observed at time t+1), so the application is not straightforward. Accordingly, we rely on the linear fixed-effects estimator.[2]

Many interesting questions about RHBs pertain to whether they have different effects over time or on different types of workers, but equation (3) restricts the estimated effect of RHBs on the probability of retirement to be the same for all workers. Accordingly, we also estimate variants of the model in which rhb_{it} is interacted with other characteristics. It is then straightforward to obtain estimated RHB effects for subgroups by differentiating with respect to rhb_{it} at the sample mean. We discuss such estimates below.

[1] The computational difficulty in fixed-effects probit and logit arises because the fixed effect for the latent propensity to retire—equation (1)—perfectly classifies anyone whose response does not vary over the panel. For example, in this application, any worker who never retires has a latent fixed effect of negative infinity. The problem does not arise in the LPM because the fixed effect is a direct effect on the probability of retiring (as in equation [3]), so the worker-specific fixed effect need not be infinite for a worker who never retires.

[2] In an earlier draft of this paper, we approached the estimation problem in the framework of survival or duration analysis. The usual way of handling heterogeneity (or "frailty") in this literature is analogous to random effects—see, for example, Cameron and Trivedi (2005, chapters 17–19) and Wooldridge (2002, chapter 20). The fixed-effects estimator is not well developed in the survival literature, so we take the more straightforward panel data approach outlined in the text, which is similar to that taken by Dave, Rashad, and Spasojevic (2008) in estimating the effect of retirement on health outcomes.

IV. Data and Variable Specification

We estimate equation (3) using a sample of men born between 1931 and 1941 from the Health and Retirement Study (HRS).[3] The analysis below is restricted to men who were working full-time (at least 35 hours per week) at the time of the first survey in 1992. Available HRS data allow us to follow these men through six transitions: 1992–1994, 1994–1996, 1996–1998, 1998–2000, 2000–2002, and 2002–2004.

Figure 1 summarizes the behavior of the men in the main HRS sample over the 12 years we observe them. The sample starts in 1992 with 3,150 men ages 51–61 who were employed full-time. Between 1992 and 1994, 303 left the study due to attrition (death or other reason), so we consider 2,847 men to have been "at risk" of retirement during the 1994–1996 period. Of these, 225 (8 percent) had retired by 1994, and another 309 moved to part-time work, unemployment, partial retirement, became disabled, or left the labor force (the "other" category in Figure 1).[4] Of the 2,313 employed full-time men still in the sample in 1994, 181 men left the sample through attrition by 1996, so 2,132 men remained "at risk" of retirement. Of these, 235 (11 percent) had retired by 1996, and 226 had moved to the "other" category. The remainder of the figure follows in the same way between each two-year time period. Ultimately, of the 3,150 men, 1,060 had retired by 2004, 766 were lost to the study due to attrition, 925 had moved to the "other" category, and 399 continued full-time employment during the entire 12 years. Note that we treat retirement as an absorbing state—once a worker retires, he is lost to further full-time work and another "retirement event." As Maestas (2004) shows, this is not entirely realistic, but

[3] For the empirical analysis, we started with the RAND HRS Data file, Version F, which is a simplified longitudinal data set based on the HRS data. See St. Clair et al. (2006).
[4] Out of the labor force and retired are separate categories in the HRS. A case could be made for counting men who were out of the labor force as retired.

it is a simplification that makes sense if different models describe the original decision to retire and the decision to return to work following retirement.

The HRS survey allows us to specify equation (3) using a rich set of explanatory variables, displayed in Table 1 and described next. The first column shows sample percentages for each variable, calculated from the 9,657 two-year transitions observed in the HRS sample of 3,150 men who were working full-time in 1992. The second column shows sample percentages calculated from the 1992 (wave 1) observations of these 3,150 men. The third column shows sample percentages calculated from the 1992 observations of the 1,060 men who retired during the subsequent six transitions we analyze.

We model RHB coverage for worker *i* in year *t* (rhb_{it}) using a set of four mutually exclusive dummy variables:

- a dummy equal to 1 if the worker had employer-provided health insurance (EPHI) but *no offer of RHBs*[5] (the reference category)
- a dummy equal to 1 if the worker had EPHI and *would receive health benefits if he retired*
- a dummy equal to 1 if the worker had no EPHI but was *covered by some other type of health insurance*
- a dummy equal to 1 if the worker had *no health insurance* coverage

Fronstin (2005) found that roughly 57 percent of men ages 45–64 reported being covered by RHBs in the 1997 SIPP. As shown in Table 1, the percentage of workers covered by RHBs in the sample we analyze (52 percent) is somewhat lower.

[5] The 1992 and 1994 question reads, "Is the health insurance plan [that currently covers you] available to people who retire?" The 1996 and later waves of the HRS ask explicitly whether the respondent's health benefit plan would cover him if he retired before age 65.

The model includes two sets of indicators modeling the type and amount of pension wealth held by worker *i* in year *t* ($pension_{it}$). The first models the current asset value of any defined benefit (DB) pension the worker expected to receive. Specifically, the HRS collected employer contact information in 1992 and 1998, then obtained information on DB pension plans directly from employers when it was possible (Health and Retirement Study 2006, pp. 3–5). From these data, the HRS either calculated or imputed several values of each worker's DB pension plan for 1992 and 1998. We use "DB value at expected retirement age prorated and discounted" to 1992 or 1998, which approximates the present discounted value of expected future plan benefits, based on the worker's work to date and his self-reported expected retirement age. The amount is intended to be comparable to a defined contribution (DC) pension accumulation, which is why we use it.

From the DB wealth variable, we construct four indicators:

- a dummy equal to 1 if the worker was not included in any DB plan, and hence had no DB pension wealth (the reference category)
- a dummy equal to 1 if the worker had positive DB pension wealth up to $100,000
- a dummy equal to 1 if the worker had DB pension wealth of $100,000 to $200,000
- a dummy equal to 1 if the worker had DB pension wealth greater than $200,000

This set of indicators can change only once during the years we observe; that is, the indicators take one set of values for 1992, 1994, and 1996, then can take another for 1998, 2000, and 2002. Table 1 shows that just over two-fifths of the sample (42 percent) had positive DB pension wealth in 1992 (wave 1).

A second set of pension wealth indicators model the current accumulation (if any) in defined contribution (DC) pension accounts held by the worker. DC pension accumulations were reported by workers in each wave, unlike information on DB pensions, so they can vary fully over time. For DC accumulations, we construct four indicators similar to those for DB pensions:

- a dummy equal to 1 if the worker was not included in any DC plan (the reference category)
- a dummy equal to 1 if the worker had positive DC plan accumulation up to $100,000
- a dummy equal to 1 if the worker had DC plan accumulation of $100,000 to $200,000
- a dummy equal to 1 if the worker had DC plan accumulation more than $200,000

Table 1 shows that, in the first year they were surveyed, about one-third of the sample had a DC plan; however, only 7 percent had DC plans that were worth more than $100,000.

To capture possible effects of non-pension assets on decisions to retire, we include two sets of conventional wealth indicators (Farnham and Sevak 2007). The first captures worker *i*'s housing wealth at each interview, defined as the net value of the primary residence. (The net value of any secondary residence is available only starting in 1998. Accordingly, the estimates leave out any consideration of the value of a secondary residence.) The second set of wealth indicators gives the value of worker *i*'s non-housing wealth at each interview, defined as the sum of financial wealth (stocks, checking accounts, CDs, bonds, and other financial assets) plus the value of real estate other than primary and secondary residences, vehicles, and businesses. Note that this variable includes IRAs and Keoghs, which are nominally forms of retirement wealth; however, because many households draw on these assets before retirement (even though they suffer a tax penalty), treating them as nonretirement wealth seems reasonable.

For both housing and non-housing wealth, we construct separate sets of dummy variables with categories similar to those constructed for DB and DC pension wealth—no wealth (the reference category), positive wealth up to $100,000, wealth between $100,000 and $200,000, and wealth greater than $200,000. Table 1 shows that in the first year they were interviewed, 60 percent of the sample had positive housing wealth up to $100,000, and 55 percent had positive non-housing wealth up to $100,000.

The demographic controls included in the model ($demog_{it}$) are age in year *t* (categories for 50–56, 57–59, 60–64, and 65 and older), an indicator equal to 1 for nonwhites, and four dummies indicating years of schooling (less than high school, high school graduate only, some college, and college graduate or more).[6]

Past research on RHBs using the HRS (for example, Rogowski and Karoly 2000) has captured the worker's health status ($health_{it}$) using one or more indicators constructed from the worker's body mass index (BMI, weight in kilograms divided by height in meters squared) in year *t*. From the reported BMI in each year, we construct indicators for underweight (BMI $<$ 18.5), normal weight (18.5 $\leq$ BMI $<$ 25), overweight (25 $\leq$ BMI $<$ 30) and obese BMI $\geq$ 30). Table 1 shows that roughly half the workers in the sample were overweight by this measure in the first year they were interviewed.

Also following earlier research, we construct a dummy equal to 1 for workers who report having two or more chronic health conditions in year *t*—high blood pressure, diabetes, cancer, chronic lung disease, heart disease, stroke, or arthritis. The latter is only a rough indicator of a respondent's health, in part because it does not distinguish more serious from less serious conditions. Accordingly, we also include a dummy variable equal to 1 for respondents who report being in fair or poor health in year *t*. Longstanding concerns exist about the endogeneity of this variable to retirement decisions—that is, workers who retire report poor health as a way of justifying their decision—although work by McGarry (2004), which recognizes and attempts

[6] Brown (2006) has found that workers tend to retire at the age they regard as "usual" for workers of their type; however, we have not taken advantage of the "usual retirement age" question that is asked of RHS respondents.

to control for this “justification bias,” suggests that self-reported health status is a useful measure of health that does have important effects on retirement. Whereas roughly 20 percent of the workers in the sample reported multiple chronic conditions in the first year they were interviewed, only 12 percent reported being in fair or poor health (Table 1).

Because the labor force status of a spouse is likely to be important to an individual’s decision to retire, we add to the model a set of dummies capturing the employment status of each man’s wife in year *t*:

- a dummy equal to 1 if the worker was *not married* (the reference category)
- a dummy equal to 1 if the worker was *married to a woman working full-time*
- a dummy equal to 1 if the worker was *married to a woman working part-time*
- a dummy equal to 1 if the worker was *married to a woman who does not work* (is unemployed, partly retired, retired, disabled, or not in the labor force)

Couples’ labor supply decisions are likely to be made jointly, and the above set of indicators may be endogenous, although few papers on health insurance and labor supply have addressed the issue (but see Blau and Gilleskie 2006, Kapur and Rogowski 2007, and Congdon-Hohman 2008). Given that our main interest is to obtain an estimate of the effect of RHBs on retirement, we include these indicators and check the sensitivity of the main estimates (the impact of RHBs on retirement) to their inclusion or exclusion.

Finally, we include indicators of two aspects of each worker’s job in year *t*: whether he is in a blue-collar occupation and whether he is self-employed. Blue-collar work tends to be physically taxing, and we expect it to be related to earlier retirement. Self-employed workers tend to have a taste for work, and we expect them to be less likely than others to retire. We also include an indicator of whether a worker has been in his job over 15 years. This is likely to be correlated with eligibility for an RHB offer because RHBs are generally available only to workers with substantial job tenure.

Comparison of columns 2 and 3 in Table 1 shows how those who retire from the HRS sample differed from the full HRS sample. Retirees were more likely to have an RHB offer, positive pension balances, and job tenure exceeding 15 years at wave 1.

V. Empirical Findings

Table 2 displays estimates of equation (3) for the HRS sample described above. We report estimates from a pooled LPM, a random-effects LPM, and a fixed-effects LPM. The estimate of main interest is the coefficient on employer-provided health insurance with RHB coverage ("employer HI and RHB"). The pooled LPM and random-effects LPM estimates (0.03, *p*-values = 0.00) suggest that workers with an RHB offer were 3 percentage points more likely to retire over a two-year interval than otherwise similar workers who had employer-provided health insurance but no RHB offer (the reference group). The mean two-year retirement probability for these workers was 11.0 percent, so the estimated increase in retirement probability (3 percentage points) implies that RHB offers increased the probability of retiring by about 27 percent. This is similar to the estimates obtained by Rogowski and Karoly (2000) and Blau and Gilleskie (2001), who used early waves of the HRS.

The pooled LPM and random-effects LPM make no attempt to control for unobserved heterogeneity; rather, they assume that the composite error term ($c_i + u_{it}$) in equation (3) is uncorrelated with the observable characteristics $\mathbf{x}_{it}$ included in the model. The fixed-effects LPM in Table 2 relaxes this assumption and suggests that an RHB offer increases the probability of retirement over a two-year period by 1.5 percentage points (or by about 14 percent relative to the average retirement probability of 11 percent). This point estimate is economically substantial, although it is imprecise and statistically insignificant at conventional levels (p-value = 0.13). Moreover, the fixed-effects point estimate is roughly half that estimated by the pooled LPM and random-effects LPM.[7]

A Hausman test of whether significant differences exist between the random-effects and fixed-effects estimates (not reported) strongly rejects equality of the two models, suggesting the presence of unobserved heterogeneity in the sample. As a further check for unobserved heterogeneity, we calculate the simple correlation between the estimated fixed effects and RHB offers. (This correlation is the source of the unobserved heterogeneity motivating the fixed effects estimator.) The correlation coefficient is 0.177 (standard error approximately 0.018), suggesting that men with higher probabilities of retiring tend to have sorted into jobs with RHB offers. The random effects estimator suppresses this correlation and attributes too much influence on retirement to RHB offers. Comparison of the random-effects estimate of the RHB effect (3 percentage points) with the fixed-effects estimate (1.5 percentage points) illustrates the heterogeneity bias and leads us to conclude that half the RHB effect estimated by random effects is due to unobserved heterogeneity rather than an RHB offer per se.

Congdon-Hohman (2008) has raised concerns about the reliability of the RHB responses in the HRS. These concerns arise from some households' apparently inconsistent responses to the RHB question, especially during the first three waves of the survey. The inconsistencies appear to arise from two sources: first, a change in the RHB question between wave 2 and wave 3, and second, the possibility that the household member answering the RHB question changed between wave 1 and wave 3. To check for the sensitivity of the findings to these problems, we

[7] Compared with the pooled and random-effects estimates, the fixed-effects estimates come from a smaller effective sample of men (2,313) observed over fewer transitions (6,507). This is because men observed for only the first transition contribute no variation over time, and hence contribute nothing to the fixed-effects estimator.

have dropped the first wave of our analysis sample and re-estimated the models. The findings, which appear in the appendix, are substantially similar to those reported in Table 2.

As we mentioned earlier, it is interesting to know whether RHBs have different effects on different subgroups of workers—those closer to or further from retirement, or those in better or worse health. Also, it is important to know whether workers are more likely to take advantage of RHBs when the labor market is tight or slack. To address these questions, we estimate a model similar to equation (3) but with interactions between rhb_{it} and all other explanatory variables. We then compute estimated RHB effects for various subgroups at the sample mean.

Table 3 displays selected subgroup effects estimated from the fixed-effects LPM. We report only fixed-effects estimates because the Hausman test mentioned above rejected equality of the random-effects and fixed-effects estimates. (Also, we do not report estimated subgroup effects for pension wealth, housing wealth, and non-housing wealth subgroups—none of these estimated subgroup effects are statistically significant at conventional levels.) Figures in the "Estimate" column give the estimated effect of an RHB offer on the retirement probability of the specified group, *relative to workers in the same group who had employer-provided health insurance but no RHB.*

In two cases, the subgroup estimates suggest an economically substantial and statistically significant effect of RHBs on retirement. First, RHBs increased the retirement probability of men aged 60–64 by 5 percentage points (relative to 60–64-year-old men without RHB), but had little effect on younger men.[8] This suggests that RHBs affected the retirement decisions mainly of

[8] Counter to any logic, RHBs appear to have *decreased* the retirement probability of men aged 65 and older in the HRS, although this effect is estimated with only about 4 percent of the worker-transitions in the sample. Two observations are relevant here. First, an RHB offer to a worker aged 65 or older is far less valuable than an RHB offer to a younger worker because virtually all retirees are eligible for Medicare at age 65. Accordingly, at age 65, the RHB offer becomes an offer of supplemental health insurance only, and the "treatment" whose effect we are estimating changes. Second, men who are still working at age 65 or older could be a select group who have a taste for work, although this possibility clearly undermines our argument that the fixed-effects estimator succeeds in

men who were within five years of Medicare eligibility; that is, when employer-provided RHBs affected retirement behavior, they provided a bridge to Medicare of at most five years.

Second, the estimates in Table 3 offer evidence that RHBs had different effects on retirement over the six two-year time periods. In particular, the estimates suggest that workers with RHBs were *less* likely to retire during 1992–1996 (a period of labor-market expansion), and *more* likely to retire during 2000–2002 (a recession). We cannot say for certain that these heterogeneous time-period effects are due to differences in the state of the labor market, but the pattern of no (or negative) estimated effects during labor-market expansion, and positive estimated effects during a contraction, does suggest a link to labor-market conditions. The evidence is consistent with RHB offers creating an added inducement to retire during a downturn.

Apart from heterogeneous age and time-period effects of RHBs, the estimates in Table 3 fail to turn up significant estimated subgroup effects of RHBs. Point estimates of RHB effects are close to zero and statistically insignificant for different racial groups, levels of education, self-reported health, marriage and spousal labor force participation, job tenures, and occupational groups. (The same is true for different subgroups by pension wealth, housing wealth, and non-housing wealth, although we do not display these estimates.)

VII. Summary and Conclusions

We have used data from the Health and Retirement Study to extend past work on RHBs in two ways. First, we examine a sample of men from the HRS over a 12-year period, which allows us to apply a fixed-effects estimator to eliminate unobserved individual effects on retirement. The findings suggest these unobserved effects are substantial. Pooled OLS and

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controlling for determinants of retirement behavior that are correlated with RHB offers.

random-effects estimators suggest that RHB offers increase the two-year retirement probability of men aged 50 and over by 3 percentage points (that is, by 27 percent relative to the average retirement probability of 0.11). In contrast, the fixed-effects estimator suggests that RHB offers increase the retirement probability by about 1.5 percentage points (14 percent, with a *p*-value of 0.13). Overall, the evidence is consistent with a process in which many workers with a taste for early retirement select (or sort into) jobs that offer RHBs. As a result, estimates that do not take account of unobserved tastes for retirement tend to attribute a greater retirement effect to RHB offers per se than those offers cause.

Second, we estimate fixed-effects models that allow the effect of RHBs to differ among different subgroups of workers and over time. In view of the generally small estimated main effects of RHB, it is not surprising that few significant subgroup effects emerge from this analysis. However, two substantial subgroup effects do appear. First, for the main HRS sample, RHBs increased the retirement probability of men aged 60–64 by 5 percentage points (relative to 60–64-year-old men without RHB), but had little effect on younger men. Second, workers with RHBs were substantially *less* likely to retire during the expanding labor market of 1992–1996 and substantially *more* likely to retire during the slack labor market of 2000–2002. To the extent RHBs have an effect on retirement behavior, those effects appear to be for men in their early 60s and during periods of slack labor markets.

We have emphasized the importance of the fixed-effects estimator for purposes of estimating the effect of RHBs on the timing of retirement, but estimators that do not eliminate unobserved effects also have a useful role. For example, a firm that wanted to predict the number of employees who would accept RHBs (in order to estimate RHB costs, perhaps) would use estimates from pooled OLS or random effects. These estimators do not identify the causal effect of RHBs on retirement, but they do estimate the difference between the retirement probabilities when RHBs are offered and when they are not. (That is, they combine the causal effect on retirement with the sorting and self-selection effects of RHBs.) In contrast, a firm that wanted to estimate the number of additional retirements induced by RHB offers (or the reduction in retirements following elimination of RHBs) would require fixed-effects estimates.

Given the relatively small estimated effect of RHBs on retirement, another question comes up: Why do employers offer RHBs? Their efficacy as a tool for inducing retirement appears limited, so what is their purpose? A possible (and straightforward) answer is that, from the firm's standpoint, RHBs are mainly a form of deferred or nonwage compensation, rather than a means of managing attrition and labor turnover. If so, they are presumably less costly than other forms of compensation that would be similarly attractive to workers. In addition to quantitative evidence, case studies and interviews could be useful in shedding light on this further issue.

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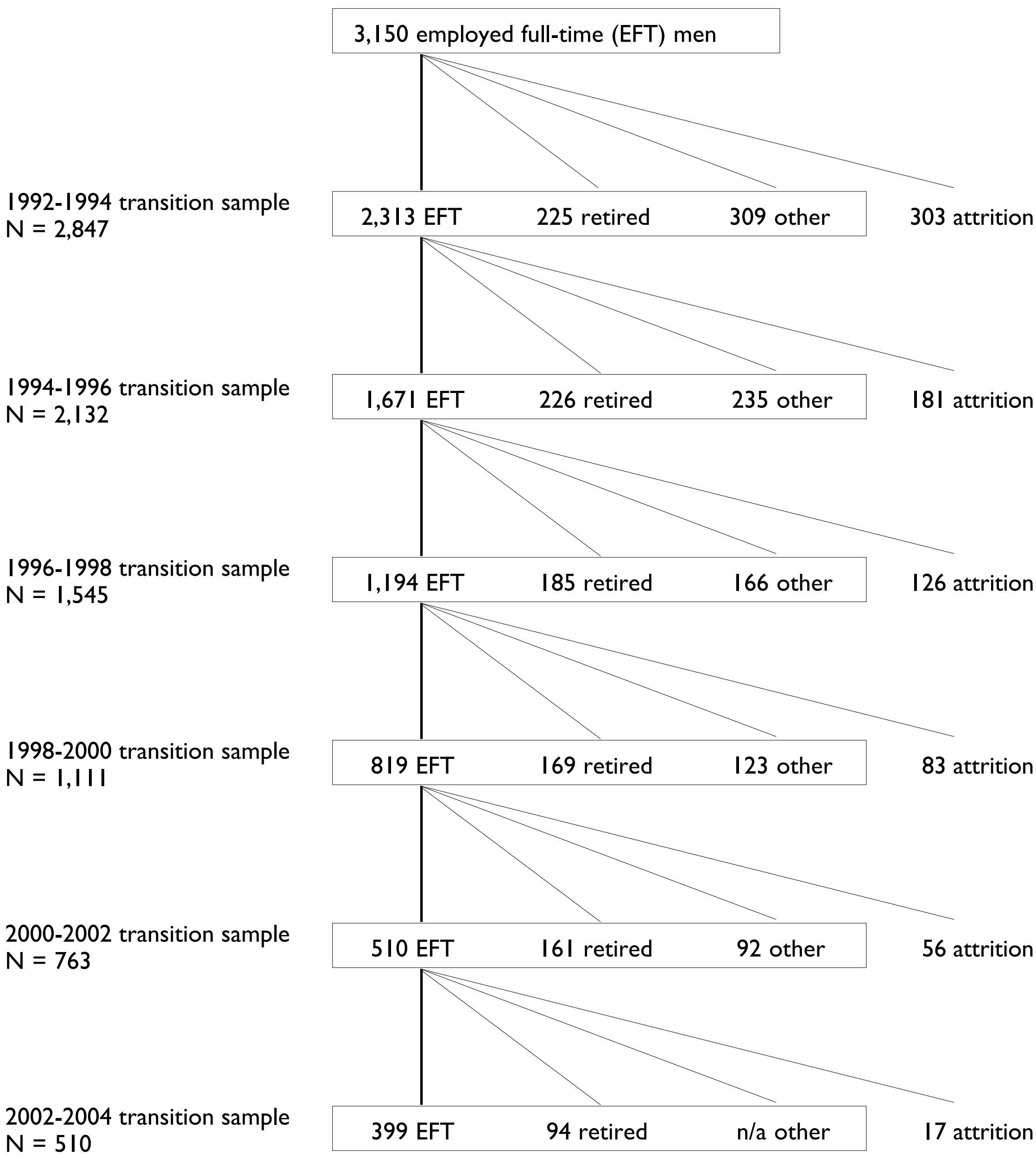
Figure 1
HRS analysis sample transitions illustrated



Notes: EFT refers to employed full-time workers. "Attrition" includes those who were not interviewed or died. "Other" includes part-time, unemployed, disabled, and not in the labor force.
Source: Authors' tabulations of Health and Retirement Study data. See text for discussion.

Table 1
Sample Descriptive Statistics
(percentages except where noted)

	Full sample (all two-year transitions) (1)	Full sample (wave 1 values) (2)	Retirees (wave 1 values) (3)
Number of men	3,150	3,150	1,060
Number of two-year transitions	9,657	3,150	1,060
Health Insurance Coverage			
employer-provided but no RHBs	30.8	24.2	21.6
employer-provided and RHBs	51.6	56.1	67.6
non-employer	8.3	7.8	4.7
none	9.4	11.9	6.0
DB balance ($)			
0 (reference)	58.8	57.6	43.7
1–100,000	23.0	23.9	28.7
100,001–200,000	8.9	9.2	13.5
> 200,000	9.3	9.2	14.2
DC balance ($)			
0 (reference)	64.7	64.7	56.9
1–100,000	28.0	29.7	35.9
100,001–200,000	3.5	3.2	4.9
> 200,000	3.8	2.4	2.3
Housing Wealth ($)			
<1 (reference)	15.8	18.3	11.9
1–100,000	59.5	62.2	68.7
100,001–200,000	18.0	14.9	15.7
> 200,000	6.7	4.6	3.7
Nonhousing wealth ($)			
<1 (reference)	5.1	5.8	3.7
1–100,000	54.9	61.6	64.3
100,001–200,000	14.9	13.4	15.4
> 200,000	25.1	19.2	16.7
Age			
50–56 (reference)	39.0	63.6	54.0
57–59	28.0	24.0	29.8
60–64	29.0	12.4	16.2
65 or older	4.0	0.0	0.0
Nonwhite	14.6	16.2	13.9
Education			
less than high school (reference)	22.5	24.4	25.2
high school only	32.5	32.7	36.6

some college	19.5	19.2	16.7
college degree or more	25.6	23.6	21.5
Body Mass Index			
underweight (BMI<18.5)	0.2	0.2	0.2
normal (18.5≤ BMI < 25) (reference)	27.1	29.6	26/6
overweight (25≤ BMI < 30)	49.6	49.5	50.8
obese (BMI ≥ 30)	23.1	20.7	22.5
Multiple chronic health conditions	26.8	20.1	22.8
Fair or poor self-reported health	12.2	11.9	12.2
Marital status and spouse's employment			
not married (reference)	15.2	15.9	14.8
married/spouse full-time	37.1	37.9	38.2
married/spouse part-time	14.2	14.9	14.5
married/spouse < part-time	33.5	31.2	32.5
Job tenure > 15 years	50.0	49.1	60.8
Self-employed	20.1	19.6	9.3
Blue-collar occupation	43.1	44.4	47.6
Transitions			
1992–1994 (reference)	32.6	100.00	100.0
1994–1996	24.0	0.00	0.0
1996–1998	17.3	0.00	0.0
1998–2000	12.4	0.00	0.0
2000–2002	8.5	0.00	0.0
2002–2004	5.3	0.00	0.0

Note: The HRS sample starts with 3,150 men aged 51 to 61 who were working full-time in 1992.

Table 2
Estimated Unobserved Effects Panel Models of Retirement

	Pooled OLS (1)			Random Effects (2)			Fixed Effects (3)		
Independent variable	Coef. estimate	Robust s.e.	*p*-value	Coef. estimate	Robust s.e.	*p*-value	Coef. estimate	Robust s.e.	*p*-value
Health insurance coverage									
employer-provided but no RHB (reference)									
employer-provided and RHB	0.030	0.007	0.000	0.029	0.008	0.000	0.015	0.010	0.132
non-employer	0.021	0.012	0.080	0.025	0.013	0.055	0.030	0.016	0.057
none	0.000	0.010	0.998	0.004	0.011	0.686	0.020	0.015	0.196
DB balance ($)									
0 (reference)									
1–100,000	0.023	0.008	0.002	0.019	0.009	0.037	-0.017	0.018	0.340
100,001–200,000	0.082	0.014	0.000	0.087	0.016	0.000	0.040	0.027	0.149
> 200,000	0.093	0.015	0.000	0.109	0.017	0.000	0.088	0.032	0.005
DC balance ($)									
0 (reference)									
1–100,000	-0.019	0.007	0.008	-0.027	0.008	0.000	-0.042	0.009	0.000
100,001–200,000	0.004	0.018	0.836	-0.010	0.020	0.596	-0.031	0.023	0.190
> 200,000	-0.001	0.018	0.950	-0.010	0.020	0.619	-0.015	0.025	0.565
Housing wealth ($)									
<1 (reference)									
1–100,000	0.004	0.008	0.637	-0.005	0.009	0.596	-0.052	0.013	0.000
100,001–200,000	0.005	0.011	0.658	-0.001	0.012	0.921	-0.034	0.018	0.057
> 200,000	0.001	0.015	0.960	-0.003	0.017	0.869	-0.023	0.025	0.359
Non-housing wealth ($)									
<0 (reference)									
1–100,000	0.029	0.011	0.012	0.031	0.013	0.013	0.015	0.017	0.361
100,001–200,000	0.059	0.014	0.000	0.061	0.016	0.000	0.030	0.020	0.143
> 200,000	0.062	0.014	0.000	0.072	0.015	0.000	0.058	0.021	0.007

Age									
50-56 (reference)									
57–59	0.038	0.007	0.000	0.029	0.007	0.000	-0.024	0.011	0.026
60–64	0.173	0.010	0.000	0.181	0.011	0.000	0.119	0.019	0.000
65 or older	0.135	0.022	0.000	0.194	0.025	0.000	0.142	0.036	0.000
Non-white	-0.012	0.009	0.195	-0.012	0.011	0.258		dropped	
Education									
less than high school (reference)									
high school only	-0.009	0.009	0.303	-0.009	0.011	0.404		dropped	
some college	-0.030	0.010	0.003	-0.034	0.012	0.004		dropped	
college degree or more	-0.046	0.010	0.000	-0.054	0.013	0.000		dropped	
Body Mass Index									
underweight	0.121	0.092	0.191	0.121	0.091	0.185	0.102	0.112	0.363
normal (reference)									
overweight	0.008	0.007	0.267	0.004	0.008	0.601	-0.006	0.014	0.643
obese	0.016	0.009	0.074	0.015	0.010	0.135	-0.007	0.021	0.727
Multiple chronic health conditions	0.016	0.008	0.035	0.021	0.009	0.024	0.010	0.017	0.554
Fair or poor self-reported health	0.038	0.011	0.001	0.040	0.012	0.001	0.034	0.016	0.029
Martial status and spouse's employment									
not married (reference)									
married/spouse full-time	-0.034	0.010	0.000	-0.042	0.011	0.000	-0.030	0.023	0.187
married/spouse part-time	-0.048	0.011	0.000	-0.054	0.013	0.000	-0.033	0.024	0.171
married/spouse < part-time	-0.010	0.010	0.325	-0.014	0.012	0.233	-0.002	0.023	0.921
Job tenure > 15 years	0.035	0.007	0.000	0.040	0.008	0.000	0.014	0.014	0.341
Self-employed	-0.067	0.009	0.000	-0.075	0.010	0.000	-0.020	0.017	0.233
Blue-collar occupation	0.015	0.007	0.035	0.011	0.008	0.168	-0.024	0.016	0.144
Transitions									
1992-1994 (reference)									
1994–1996	0.003	0.007	0.638	0.020	0.007	0.004	0.077	0.007	0.000
1996–1998	-0.004	0.009	0.678	0.025	0.009	0.003	0.116	0.010	0.000
1998–2000	-0.004	0.012	0.718	0.033	0.012	0.005	0.154	0.016	0.000

2000–2002	0.021	0.016	0.180	0.064	0.016	0.000	0.204	0.021	0.000
2002–2004	-0.017	0.021	0.399	0.031	0.022	0.155	0.184	0.030	0.000
Constant	-0.016	0.016	0.323	0.000	0.019	0.982	0.018	0.031	0.553
Number of observations	9,657			9,657			8,820		
Number of men	3,150			3,150			2,313		
R^2 (within)	n/a			0.161			0.179		
R^2 (between)	n/a			0.065			0.002		
R^2 (overall)	0.105			0.101			0.051		
ρ	n/a			0.269			0.545		

Note: Estimates come from applying pooled OLS, random-effects, and fixed-effects to equation (3). The models are estimated on the sample described in Table 1 and Figure 1. Figures in the "Coef. estimate" column give estimated effects on the average two-year retirement probability of men in the sample. The dependent variable is an indicator equal to 1 if a man was retired in period t+1 (approximately two years after t).

Table 3
RHB Effects for Subgroups, Fixed-Effects Estimates

Subgroup or transition	Estimate	*p*-value
Age 50–56	-0.022	0.088
Age 57–59	-0.011	0.472
Age 60–64	0.047	0.022
Age 65+	-0.191	0.000
White	-0.007	0.547
Non-white	0.004	0.861
Less than high school	-0.012	0.624
High school only	-0.013	0.459
Some college	0.018	0.318
College degree or more	-0.007	0.716
Fair or poor self-reported health	-0.006	0.815
Good/very good/excellent self-reported health	-0.005	0.632
Multiple chronic health conditions	-0.013	0.510
Without multiple chronic health conditions	-0.002	0.824
Not married	0.006	0.795
Married / spouse works full-time	-0.006	0.663
Married / spouse works part-time	-0.004	0.823
Married / spouse works < part-time	-0.010	0.540
Job tenure at most 15 years	-0.015	0.265
Job tenure > 15 years	0.004	0.780
Blue-collar occupation	-0.011	0.496
White-collar occupation	-0.001	0.937
1992–1994 transition	-0.054	0.002
1994–1996 transition	-0.023	0.146
1996–1998 transition	0.024	0.127
1998–2000 transition	0.017	0.388
2000–2002 transition	0.095	0.001
2002–2004 transition	0.068	0.100
Estimated main RHB effect (from Table 2):	*0.015*	*0.13*

Note: Estimates for subgroups come from a single model similar to equation (3) in which rhb_{it} is fully interacted with the other independent variables in the model. Each subgroup estimate is computed as the derivative of $retired_{it}$ with respect to the specified independent variable at the sample mean. Figures in the “Estimate” column give the estimated effect of an RHB offer on the two-year retirement probability of workers in the specified group, relative to workers in the same group who had employer-provided health insurance but no RHB.

Appendix Table
Estimated Unobserved Effects Panel Models of Retirement — Wave 1 Dropped

	Pooled OLS (1)			Random Effects (2)			Fixed Effects (3)		
Independent variable	Coeff. estimate	Robust s.e.	*p*-value	Coeff. estimate	Robust s.e.	*p*-value	Coeff. estimate	Robust s.e.	*p*-value
Health insurance coverage									
employer-provided but no RHB (reference)									
employer-provided and RHB	0.031	0.009	0.001	0.028	0.010	0.003	0.018	0.011	0.099
non-employer	0.015	0.015	0.307	0.016	0.016	0.315	0.029	0.020	0.145
none	-0.015	0.014	0.271	-0.008	0.015	0.585	0.031	0.020	0.113
DB balance ($)									
0 (reference)									
1–100,000	0.037	0.010	0.000	0.032	0.012	0.005	-0.018	0.019	0.355
100,001–200,000	0.096	0.018	0.000	0.099	0.020	0.000	0.038	0.029	0.178
> 200,000	0.107	0.018	0.000	0.118	0.020	0.000	0.089	0.033	0.006
DC balance ($)									
0 (reference)									
1–100,000	-0.022	0.009	0.016	-0.029	0.010	0.003	-0.033	0.013	0.009
100,001–200,000	0.006	0.023	0.800	0.004	0.024	0.879	0.020	0.028	0.477
> 200,000	0.011	0.021	0.608	0.002	0.023	0.925	0.002	0.030	0.943
Housing wealth ($)									
<1 (reference)									
1–100,000	0.003	0.011	0.798	-0.001	0.012	0.959	-0.049	0.020	0.012
100,001–200,000	0.006	0.015	0.681	0.003	0.016	0.867	-0.033	0.024	0.165
> 200,000	0.006	0.019	0.730	0.010	0.021	0.644	-0.014	0.032	0.661
Non-housing wealth ($)									
<0 (reference)									

1–100,000	0.029	0.016	0.067	0.031	0.017	0.073	0.014	0.022	0.515
100,001–200,000	0.055	0.019	0.004	0.053	0.021	0.011	0.015	0.026	0.562
> 200,000	0.058	0.018	0.002	0.065	0.020	0.001	0.045	0.028	0.104
Age									
50-56 (reference)									
57–59	0.042	0.009	0.000	0.033	0.009	0.000	-0.049	0.014	0.001
60–64	0.185	0.012	0.000	0.189	0.013	0.000	0.064	0.026	0.014
65 or older	0.150	0.023	0.000	0.207	0.026	0.000	0.079	0.045	0.080
Non-white	-0.017	0.012	0.138	-0.019	0.014	0.174		dropped	
Education									
less than high school (reference)									
high school only	-0.014	0.012	0.246	-0.017	0.014	0.239		dropped	
some college	-0.038	0.013	0.003	-0.048	0.016	0.002		dropped	
college degree or more	-0.057	0.014	0.000	-0.068	0.017	0.000		dropped	
Body Mass Index									
underweight	0.081	0.110	0.461	0.090	0.110	0.415	0.154	0.124	0.215
normal (reference)									
overweight	0.017	0.009	0.057	0.019	0.010	0.067	0.009	0.019	0.640
obese	0.020	0.011	0.073	0.025	0.013	0.058	0.018	0.028	0.530
Multiple chronic health conditions	0.008	0.009	0.368	0.010	0.011	0.374	0.013	0.022	0.558
Fair or poor self-reported health	0.043	0.014	0.002	0.048	0.015	0.002	0.037	0.019	0.058
Marital status and spouse's employment									
not married (reference)									
married/spouse full-time	-0.027	0.013	0.034	-0.029	0.015	0.048	-0.011	0.034	0.755
married/spouse part-time	-0.036	0.015	0.014	-0.035	0.017	0.040	-0.026	0.035	0.454
married/spouse<part-time	0.003	0.013	0.843	0.003	0.016	0.834	0.003	0.035	0.929
Job tenure > 15 years	0.042	0.009	0.000	0.049	0.010	0.000	0.027	0.019	0.150

Self-employed	-0.075	0.011	0.000	-0.088	0.012	0.000	-0.026	0.024	0.274
Blue-collar occupation	0.024	0.009	0.009	0.023	0.011	0.030	-0.062	0.021	0.003
Transitions									
1994–1996 (reference)									
1996–1998	-0.008	0.010	0.412	0.014	0.009	0.113	0.098	0.009	0.000
1998–2000	-0.010	0.013	0.421	0.026	0.013	0.037	0.164	0.017	0.000
2000–2002	0.013	0.016	0.417	0.060	0.017	0.000	0.236	0.023	0.000
2002–2004	-0.026	0.021	0.214	0.030	0.023	0.187	0.234	0.034	0.000
Constant	-0.032	0.022	0.140	-0.024	0.024	0.333	0.019	0.044	0.671
Number of observations	6,507			6,507			6,507		
Number of men	2,313			2,313			2,313		
R^2 (within)	n/a			0.158			0.188		
R^2 (between)	n/a			0.090			0.020		
R^2 (overall)	0.110			0.105			0.034		
ρ	n/a			0.292			0.590		

Note: These models are estimated on the sample described in Table 1 and Figure 1 after dropping the first transition. Estimates come from applying pooled OLS, random-effects, and fixed-effects to equation (3). Figures in the "Coef. estimate" column give estimated effects on the average two-year retirement probability of men in the sample. The dependent variable is an indicator equal to 1 if a man was retired in period *t*+1 (approximately two years after *t*). See note to Table 2.