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The Influence of Retiree Health Benefits on Retirement Patterns

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The Influence of Retiree Health Benefits on Retirement Patterns

ABSTRACT

We estimate the effect of employer offers of retiree health benefits (RHBs) on the timing of retirement using a sample of Health and Retirement Study (HRS) men observed over a period of up to 12 years. We hypothesize that the effect of RHBs differs for workers of different ages—a hypothesis we can test now that the main HRS cohort has aged sufficiently. We apply three well-known panel data estimators and find that, for men in their 50s, RHBs have little or no effect on retirement decisions; however, a substantial effect emerges for men in their early 60s. We use simulations to illustrate how RHBs alter retirement patterns.

JEL Classification Codes: J26; I18; J32

Keywords: Retirement; Health Insurance; Employee Benefits; Panel Data

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I. Introduction

In 2009, 28 percent of large private employers offered health insurance coverage to their early retirees—former employees under age 65 who were not yet eligible for Medicare—down from 48 percent in 1993 (Fronstin 2010). Health insurance for early retirees represents a substantial benefit to workers and a potential incentive to retire early: Workers with such retiree health benefits (RHBs) can retire before age 65 and retain their former employment-related health insurance at relatively low cost, whereas workers without RHBs who retire before age 65 face costly options if they want health insurance coverage—they must either pay for the former employer’s health benefits at cost or purchase private health insurance.

How do RHBs influence retirement patterns? In particular, does the influence of RHBs differ for workers at different ages? These questions are important because, if RHBs influence the retirement decisions only of eligible workers who are nearing age 65, their implications for labor supply and employer costs would be quite different than if they affected the retirement of all eligible workers equally, as is often assumed. Also, the effect of RHBs on retirement has implications for the labor supply effects of health care reforms—such as universal single-payer insurance—that would break or weaken the link between health insurance and employment that now exists in the United States (Blau and Gilleskie 2001). For example, we would make quite different predictions about the implications of health care reform for the exit of older workers from the labor force depending on whether RHBs encourage retirement of all eligible workers equally, or only of workers in their early 60s.

Our goal in this paper is to add to the existing evidence on the effects of RHBs on retirement along two lines. First, because it is important to know whether the influence of RHBs on retirement decisions is heterogeneous across workers of different ages and whether the

influence of RHBs changes over the business cycle, we estimate a model that allows the effect of RHBs to vary for different subgroups of workers and over time. We do this 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). Second, because workers with RHB offers may have unobserved characteristics that lead to earlier retirement, we apply a fixed effects estimator to the RHS panel (in addition to pooled OLS and random effects) in an attempt to better identify the effect of RHBs on retirement.

Our analysis proceeds in the following steps. After a brief review of the existing literature on RHB coverage and retirement (section II), we describe our approach to estimation (section III). The basic model we specify is similar to other reduced-form models in the literature, but in addition to a restricted model with a single average treatment effect, we specify a less restrictive model in which the effect of RHBs varies across different groups of workers and over time. We also describe our rationale for applying a fixed effects estimator, in addition to pooled OLS and random effects estimators. Section IV describes the HRS data we use and gives details of the variables we use to specify the models.

We then describe the empirical findings from the restricted model in which the estimated effect of RHBs is averaged over all eligible workers (section V). The pooled OLS and random effects estimates are similar to previous findings in the literature and suggest that a worker with an RHB offer is about 3 percentage points (11 percent) more likely to retire than a worker without. But the fixed effects estimates suggest that the effect of RHBs on retirement is about half as large (and statistically insignificant). The attenuated RHB effect under the fixed effects estimator has three possible explanations—first, the pooled OLS and random effects estimators

suffer from heterogeneity bias (that is, workers with RHB offers would have retired earlier even without those offers); second, the fixed effects estimates are plagued with measurement error and the associated attenuation bias; and third, the restricted model is misspecified (for example, the effect of RHBs on retirement should be allowed to vary across age groups). In the Appendix, we follow up on the concern with measurement error by implementing sample-restriction tests and bounding techniques. These tests cannot dispose of the concern that measurement error is responsible for part of the attenuation in the RHB effect estimated by fixed effects, but they do suggest that the attenuation is caused mainly by functional form misspecification and/or failure to control for unobserved heterogeneity.

Section VI presents empirical findings from the unrestricted model, in which RHB effects are disaggregated for subgroups of workers and over time. We begin by performing statistical tests that reject the restricted model (in which the RHB effect of retirement is the same for workers at all ages) in favor of the unrestricted model (in which the retirement effect varies with age). We then describe the estimates, which strongly suggest that RHBs do not affect the retirement behavior of workers aged 50–56, have at most a modest effect on workers aged 57–59, and have a quite substantial effect on workers aged 60–64. This pattern holds with all three estimators (pooled OLS, random effects, and fixed effects), and suggests that the choice of estimator is less important than freeing up the functional form of the model so that RHB effects are allowed to differ for different groups of workers.

Section VII presents simulated survivor functions based on the empirical findings. These survivor functions show that the retirement patterns (and by implication the RHB costs) implied by a model with disaggregated effects differ sharply from those implied by a restricted model. In particular, survivor functions based on estimates of the unrestricted model (and random effects

and fixed effects estimators) suggest that RHBs increase a cohort's cumulative person-years of retirement by 6 to 11 percent by the time cohort reaches age 65. In contrast, survivor functions based on the restricted model and estimators comparable to those used in earlier work (pooled OLS and random effects) suggest that RHBs increase a cohort's cumulative person-years of retirement by 37 to 57 percent. The implication is that the effect of RHBs on retirement behavior is probably more modest than previous work has suggested.

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 (Gustman and Steinmeier 1994, 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). With the notable exception of Gustman and Steinmeier (1994), these studies concluded that RHB availability (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 for RHBs 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.

At least two studies of RHBs have obtained estimates of the effect of RHBs on retirement mainly as a byproduct of more comprehensive structural 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. However, mainly because only two or three waves of the HRS data were available when the work was performed, existing 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.

The literature examining the effect of RHBs on retirement is a small fraction of the economic literature on the determinants of retirement. We return to additional aspects of this literature below when we describe the approach to estimation.

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 base RHB eligibility on age and service requirements. For example, in 2009, 37 percent of large private employers required workers to be at least age 55 and have at least 10 years of service to be eligible for RHBs, and most required at least 10 years of service for eligibility (Fronstin 2010). 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 upward-biased.

Well-known panel data methods offer a possible way to address the problem of unobserved effects, although as we will see, these methods are by no means a panacea. 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 :

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

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:

$$\begin{aligned} \mathbf{x}_{it}\boldsymbol{\beta} = & \beta_1(rhb_{it}) + \beta_2(pension_{it}) + \beta_3(wealth_{it}) + \beta_4(age_{it}) + \beta_5(demog_{it}) + \beta_6(health_{it}) + \\ & \beta_7(spouse_{it}) + \beta_8(jobchar_{it}) \end{aligned} \quad (2)$$

where rhb_{it} denotes a set of indicators modeling whether worker i had employer-provided health insurance (EPHI) and 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 , age_{it} is a set of age indicators, $demog_{it}$ denotes variables indicating 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. The rationale for including these variables in models of retirement behavior is well established in the literature—see for example Ruhm (1990a) and Quinn, Burkhauser, and Myers (1990)—although different retirement models specify these variables in different ways. In particular, the specification of pension wealth in models of retirement has been an active field of research during the past 25 years—see Coile and Gruber (2007), Friedberg and Webb (2005), and Gustman and Steinmeier (2001/2002) for insightful guides. We return to these points below.

Equation (2) follows the existing literature in restricting the effect of an RHB offer on retirement to be the same for all workers—that is, β_1 is a “main effect” or “average treatment effect” that does not vary over workers. This assumption is unduly strong, especially because we suspect the influence of RHB offers may differ for workers of different ages and over the

business cycle. For example, Coile and Levine (2007) find that labor market downturns increase the probability of retirement, particularly for workers eligible for Social Security.

To allow the effect of RHBs to vary with individual characteristics and over time, we respecify equation (2) by fully interacting rhb_{it} with all explanatory variables and η_t (the transition indicators):

$$\begin{aligned} \mathbf{x}_{it}\boldsymbol{\beta} = & \beta_1(rhb_{it}) + \beta_2(pension_{it})(rhb_{it}) + \beta_3(wealth_{it})(rhb_{it}) + \beta_4(age_{it})(rhb_{it}) + \\ & \beta_5(demog_{it})(rhb_{it}) + \beta_6(health_{it})(rhb_{it}) + \beta_7(spouse_{it})(rhb_{it}) + \beta_8(jobchar_{it})(rhb_{it}) + \\ & \eta_t(rhb_{it}) \end{aligned} \quad (3)$$

In equation (3), $(age_{it})(rhb_{it})$ denotes age indicators by themselves *and* age indicators fully interacted with the health insurance-RHB indicators (with β_4 the vector of coefficients on these indicators), and similarly for the other terms in the equation. Retrieving estimated subgroup effects from this fully interacted model is straightforward if tedious: After substituting equation (3) into equation (1), we differentiate with respect to rhb and evaluate the derivative for a given subgroup at the sample mean (that is, substituting sample mean characteristics for variables other than those in the given subgroup).¹

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):

$$retired_{i,t+1} = \mathbf{x}_{it}\boldsymbol{\beta} + \eta_t + c_i + u_{it} \quad (4)$$

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

¹ Each estimate is a linear combination of coefficient estimates and sample means, so implementation is straightforward using Stata's "lincom" command, which produces both a point estimate and its standard error.

not apply in this case because we estimate a 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.

In keeping with past efforts to estimate the effect of RHBs on retirement, we could (and do) estimate equation (4) 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 β (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 β 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 is to apply a fixed effects estimator to equation (4). This is feasible, at least in a linear model, because we have time-varying observations of rhb_{it} and other independent variables for each worker. The fixed-effects estimator identifies the effect of RHBs on the timing of retirement from individual-specific variation over time in RHB eligibility, which is less likely to be correlated with unobservables than is eligibility for RHBs at a point in time. However, a potential drawback of the fixed effects estimator, which we address below, is that it will be downward-biased if the regressors—in this case, deviations from individual means—are measured with error.

It might seem natural to apply nonlinear fixed effects estimators, such as probit or logit, to equation (1), 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.² 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.³

IV. Data and Variable Specification

We estimate equation (4) using a sample of men born between 1931 and 1941 from the Health and Retirement Study (HRS).⁴ 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 aged 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,

² 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.

³ 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.

⁴ 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).

unemployment, partial retirement, became disabled, or left the labor force (the “other” category in Figure 1).⁵ 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, 226 (11 percent) had retired by 1996, and 235 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 Ruhm (1990b, 1995) and Maestas (2004) have shown, this is not entirely realistic, but it is a simplification that makes sense if the model describing the original decision to retire differs from that describing subsequent retirement decisions.

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 of Table 1 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 mutually exclusive dummy variables for the following four states:

⁵ Out of the labor force and retired are separate categories in the HRS, although a case could be made for counting men who were out of the labor force as retired. See Gustman and Steinmeier (2002) for a helpful discussion of this point.

- the worker had EPHI but *no offer of RHBs*⁶ (the reference category)
- the worker had EPHI and *would receive health benefits if he retired*
- the worker had no EPHI but was *covered by some other type of health insurance*
- 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.

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 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 indicators of four levels of DB pension wealth:

- not included in a DB plan, hence no DB pension wealth (the reference category)
- positive DB pension wealth up to \$100,000

⁶ 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.

- DB pension wealth of \$100,000 to \$200,000
- 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 every 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: not included in any DC plan (the reference category); positive DC accumulation up to \$100,000; DC accumulation of \$100,000 to \$200,000; and DC 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 accumulations greater than \$100,000.

Our specification of pension incentives for retirement is similar to that used in early research on the effect of pensions on retirement, which specified pensions with variables for pension eligibility, current pension wealth, and the change in pension wealth from postponing retirement by one year (see the review by Quinn, Burkhauser, and Myers 1990). Important papers by Lazear and Moore (1988) and Stock and Wise (1990) noted that optimal retirement decisions require workers to be forward-looking and to consider the “option value” of continued work. The reduced-form empirical work that has evolved from this approach has used the ideas of pension wealth accrual (Gustman and Steinmeier 2001/2002, Samwick 1998) and pension peak value (Coile and Gruber 2007, Friedberg and Webb 2005).

We have not attempted to include such forward-looking measures of pension wealth in the models we estimate for three reasons: Doing so is computationally quite demanding, the needed data are not available in the public-use version of the HRS, and our main focus is on RHBs.⁷ Accordingly, it is important to acknowledge that our use of pension wealth *levels* (as opposed to a more complicated construct) represents a potential misspecification that could bias our estimator and lead to *overstatement* of the effect of RHBs on retirement. DB pension plans in particular create a strong incentive for a worker to retire at the plan's normal retirement age, and many DB plans also create an incentive to retire shortly after reaching the plan's early retirement age—see for example Kotlikoff and Wise (1989) and Samwick (1998). (DC plans do not create such incentives.) To the extent these ages are correlated with eligibility for RHBs, we could attribute to RHBs an effect that should be attributed partly or wholly to a pension plan.

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 is reasonable.

⁷ If we did include a forward-looking measure of pensions wealth, we would in principle want to include a similarly constructed measure of RHB wealth. This in turn would pose two problems: first, the HRS includes only indicators of RHB eligibility; second, it is more difficult to place a pecuniary value on health insurance than on pensions.

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).⁸

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 three-quarters of the workers in the sample were overweight or obese 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 between more and less serious conditions. Accordingly, we also include a dummy variable equal to 1 for respondents who

⁸ 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.

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 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 27 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 include a set of mutually exclusive dummies capturing the marital status of each man and the employment status of his wife in year t :

- not married (the reference category)
- married to a woman working full-time
- married to a woman working part-time
- married to a woman who did not work (unemployed, 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). We have checked the sensitivity of the main estimates to inclusion or exclusion of these variables and find that the results are essentially unchanged.

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 more than 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 (Fronstin 2010).

Comparison of columns 2 and 3 in Table 1 shows how those who retired 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 from the Restricted Model

Table 2 displays estimates of equation (4) in which the effect of RHBs on retirement is restricted to be the same for all workers in the HRS sample described above—that is, $\mathbf{x}_{it}\beta$ is specified as in equation (2). We apply three estimators to the equation—pooled OLS, random effects, and fixed effects. The parameter of main interest is the coefficient on EPHI with RHB coverage (“employer-provided and RHB”). The pooled OLS and random effects 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 EPHI 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 OLS and random effects estimators do not attempt to control for unobserved heterogeneity; rather, they assume that the composite error term ($c_i + u_{it}$) in equation (4) is

uncorrelated with the observable characteristics \mathbf{x}_{it} included in the model. The fixed effects estimator 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 about 14 percent relative to the average retirement probability of 11 percent). This point estimate is economically substantial, but it is imprecise and statistically insignificant at conventional levels (p -value = 0.13). The fixed effects point estimate is roughly half that estimated by pooled OLS and random effects.

A Hausman test of whether the random effects and fixed effects estimates are equal (not reported) strongly rejects equality. One interpretation of this finding is that the individual unobserved effects are correlated with an explanatory variable such as RHB offers. 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 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 to RHB offers as a determinant of retirement. Accordingly, one interpretation of the difference between the random effects estimate of the RHB effect (3 percentage points) and the fixed effects estimate (1.5 percentage points) is that the random effects estimator suffers from heterogeneity bias, and that half of the RHB effect estimated by random effects is due to unobserved heterogeneity rather than an RHB offer per se.

We now discuss two alternative explanations for the difference between the random effects and fixed effects estimates—measurement error and functional form misspecification. In moving to the fixed effects estimator, any measurement error in the key RHB variable is amplified. For example, Freeman (1984) showed clearly that, in the presence of modest errors in

the measurement of union-status changes, the fixed effects estimator could lead to substantial downward bias in estimating the union wage effect. This occurs because, with the fixed effects estimator, changes in union status are the source of variation that allows estimation of the union wage effect, and these changes are relatively rare, so modest error in measuring union-status change leads to large attenuation bias. It follows that the reduction in the estimated RHB effect when we move to the fixed effects estimator could merely be a case of Hausman’s (2001) “Iron Law of Econometrics”— measurement error leads to smaller-than-expected estimates. We examine this issue in some detail in the Appendix and conclude that measurement error is not likely to be the primary cause of the attenuation observed in moving to fixed effects.

A second alternative explanation for the difference between the random effects and fixed effects estimates is functional form misspecification. For example, either estimator would be biased if it restricted the effect of RHBs to be equal across demographic groups when the effects in fact differed among those groups. We examine this issue next.

VI. Subgroup Effects

Our main concern is that RHBs may have different effects on different subgroups of workers and at different points in the business cycle. In particular, knowing whether and how the retirement effect of RHBs varies by age would offer insight into the costs of RHBs and how changes in the age of eligibility for Medicare or other government-provided health insurance would affect retirement. It is also important to know whether workers are more likely to take advantage of RHBs when the labor market is tight or slack (Coile and Levine 2007). To address these questions, we replace the restrictive specification of $\mathbf{x}_{it}\boldsymbol{\beta}$ represented by equation (2) with the fully interacted specification represented by equation (3).

Table 3 displays selected subgroup effects estimated by pooled OLS, random effects, and fixed effects applied to equation (4). 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 EPHI but no RHBs*.⁹

To begin, we note that statistical tests¹⁰ of whether the estimated RHB effects are equal within various subgroups reject equality in three cases: RHB effects by age subgroup, by job tenure, and across the two-year transitions. (To avoid clutter, we do not attempt to display these test results in Table 3.) Accordingly, we reject the restricted model in favor of the unrestricted model.

The most striking finding in Table 3 pertains to the effect of RHBs at different ages. The pooled OLS, random effects, and fixed effects estimates all suggest that *the effect of RHBs on retirement behavior is substantially larger at ages 60–64 than at younger ages*. Indeed, the effect on younger workers is modest or nonexistent. For men aged 50–56, RHBs have no statistically significant effect on retirement decisions. For men aged 57–59, the pooled OLS and random effects estimates suggest that RHBs increase the two-year retirement probability by about 20 percent (about 2.5 percentage points on a base of 11.7 percent), although the fixed effects estimates suggest no effect for these workers. But for men aged 60–64, the pooled OLS and random effects estimates suggest that RHBs more than double the probability of retirement (by 7.5 percentage points on a base on 6.1 percent), and the fixed effects estimates suggest that RHBs increase the retirement probability by more than 75 percent (4.7 percentage point on a base of 6.1 percent). Accordingly, RHBs affected the retirement decisions mainly of men who

⁹ Section III describes computation of the subgroup effects. 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.

¹⁰ These are *F* tests in the case of the pooled OLS estimator and Chi-squared tests in the cases of random effects and fixed effects.

were within five years of Medicare eligibility. This suggests in turn that, when employer-provided RHBs affected retirement behavior, they provided a bridge to Medicare of at most five years. We illustrate the importance of these differential age effects using simulations in section VII.¹¹

Second, the pooled OLS and random effects estimates suggest that RHBs have a substantially stronger effect on workers with long job tenure (more than 15 years) than those with short job tenure (15 or fewer years). It is difficult to know how much to make of this finding, both because relatively few workers with 15 or fewer years of job tenure are eligible for RHBs, and because the fixed effects estimates fail to confirm it.

Third, the estimates in Table 3 are consistent in suggesting that RHBs had a substantially greater effect on retirement during the 2000–2002 transition than during other periods. Because 2000–2002 was a period of slack labor markets, the finding suggests that RHB offers create an added inducement to retire during a downturn. This makes sense in light of Coile and Levine’s (2007) evidence that retirements tend to increase during economic downturns for workers who are eligible for Social Security.¹²

Finally, it is worth remarking that the overall differences between the pooled OLS and random effects estimates (on one hand) and the fixed effects estimates (on the other) are less striking in the fully interacted model (Table 3) than in the restricted model (Table 2). In

¹¹ Counter to any logic, all three sets of estimates suggest that RHBs *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 controlling for determinants of retirement behavior that are correlated with RHB offers.

¹² The evidence is inconsistent regarding the estimated effect of RHBs during the 1992–1994 transition, which was a period of labor-market expansion. The pooled OLS estimate suggests a slight positive effect during 1992–1994, the random effects estimate (which is a weighted average of the pooled OLS and fixed effects estimates) suggests no effect, and the fixed-effect estimate suggests a negative effect, which would be consistent with the notion that RHBs are a deterrent to retirement during an expansion. We read the evidence on this point as too weak to be conclusive.

particular, the estimated RHB effect in the restricted model is 0.03 for pooled OLS and random effects, but only half that (and very imprecise) for fixed effects. But in the fully interacted model, the three sets of estimates are consistent in suggesting that the estimated RHB effect is concentrated on men aged 60–64, and much weaker for younger men. We read this as tending to confirm our conjecture that the restricted model (Table 2) is misspecified, and that functional form misspecification may be an important reason for the differences between Table 2’s pooled OLS/random effects estimates and the corresponding fixed effects estimates. The next section will give further evidence tending to support this interpretation.

VII. Implications for Retirement Patterns

A complete interpretation of the estimates described above requires us to check the implications of those estimates for retirement patterns. To do this, we simulate survivor functions based on each of the six sets of estimates. These survivor functions offer important insights into how RHBs influence retirement.

Table 4 shows simulated survivor functions based on the six sets of estimates in Tables 2 and 3. Each simulation starts with 1,000 full-time workers at age 50 with the following characteristics: DB pension wealth, DC pension accumulation, housing wealth, and non-housing wealth all between \$1 and \$100,000; white; a high-school education; BMI between 25 and 30 (overweight); fewer than two chronic health conditions; good or better self-reported health; married to a spouse who works full time; more than 15 years of job tenure; not self-employed; and not blue collar. For a given set of estimates, we calculate the conditional probability (or hazard) of retirement at each age. We then apply the retirement hazard for age t to the workers

still working full time at that age (the risk set). The resulting survivor function shows the number of men who remain working full time at each age.

For each set of estimates, we simulate two survivor functions—one for 1,000 workers who do not have RHB offers, and a second for 1,000 men who do. The “RHB-No” columns show the number of full-time workers *without* RHBs at each age t (denoted N_{0t}), and the “RHB-Yes” column shows the number of full-time workers *with* RHBs at each age (N_{1t}). To obtain the number of RHB-induced retirements at age t (R_t), we take the number of retirements of RHB-offered workers at age t and subtract the number of retirements of not-RHB-offered workers at the same age (the counterfactual):

$$R_t = (N_{1,t+1} - N_{1t}) - (N_{0,t+1} - N_{0t}).^{13}$$

For example, for the Table 2 pooled OLS estimates (Table 4, panel 1), we calculate the number of RHB-induced retirements at age 50 as $R_{50} = (1,000 - 977) - (1,000 - 992) = 23 - 8 = 15$. [The survivor figures (N_{kt}) in Table 4 are rounded, so the R_t figures calculated from them occasionally differ from the R_t figures reported in the table. Note that the number of RHB-induced retirements becomes negative at age 60 in panels 1 and 2. This happens because by age 60 the risk set for the RHB-Yes group becomes small enough that it generates fewer retirements than does the RHB-No group, even though the RHB-Yes group has the higher retirement hazard.]

The *cumulative person-years of retirement* experienced by the 1,000 workers in each cohort at the time they reach age 65 can be calculated as $\sum_{t=50}^{64} (N_{kt} - N_{k,t+1})(65 - t)$. We report these figures at the bottom of each “RHB-No” and “RHB-Yes” column. For each simulation, the difference between the cumulative person-years of retirement for the RHB-Yes and RHB-No

¹³ For the purposes of the simulation, we assume that a worker who retires at age t is still working on his t th birthday and retires shortly thereafter. For example, a worker who retires at age 50 will be counted as working on his 50th birthday, will retire shortly after that birthday, and will have 15 years of retirement when he reaches age 65.

cohorts represents the *cumulative person-years of retirement induced by RHBs*. [This can also be calculated as $\sum_{t=50}^{64} R_t(65-t)$.] We report this latter figure at the bottom of each R_t column.

Finally, for each simulation, we report *person-years of RHB-induced retirements as a percentage of person-years of retirement for the counterfactual (RHB-ineligible) cohort*. This last figure gauges the importance of RHBs to the overall retirement experience of the cohort.

In performing each simulation, we set an RHB effect to zero unless the corresponding estimate has a p -value of 0.05 or less. As a result, the RHB-No and RHB-Yes survivor functions based on Table 2's fixed effects estimates (Table 4, panel 3) are identical—RHBs have no estimated effect with this model and estimator. The same is true until age 57 for the survivor functions based on the Table 3 pooled OLS and random effects estimates (panels 4 and 5) and until age 60 for those based on the Table 3 fixed effects estimates (panel 6).

The survivor functions based on Table 2's pooled OLS and random effects estimates (Table 4, panels 1 and 2) suggest that RHBs induce 1,325 and 1,142 person-years of retirement by age 65 in a cohort of 1,000 workers—representing increases in cumulative person-years of retirement of 57 percent and 37 percent. In contrast, the survivor functions based on Table 2's fixed effects estimates (panel 3) suggest that RHBs induce no retirement. We show these simulations as benchmarks, although all three restrict the effect of RHBs to be equal at all ages—a restriction we have rejected—and those based on pooled OLS and random effects may be marred by heterogeneity bias.

The survivor functions based on Table 3's estimates, which allow the effect of RHBs to vary with age, tell quite a different story. Consider first the simulations based on the pooled OLS and random effects estimates (Table 4, panels 4 and 5). These suggest that RHBs induce 530 and 455 person-years of retirement by age 65—increases in cumulative retirement of 19 percent and

11 percent. This RHB-induced increase in retirement is about one-third of the increase seen when we assume the effect of RHBs is constant at all ages (panels 1 and 2). The reason for these differences is clear: RHBs have no effect on retirement until age 57 in panels 4 and 5.

The survivor functions based on Table 3's fixed effects estimates (Table 4, panel 6) suggest that RHBs induce 215 person-years of retirement—a 6.4 percent increase in cumulative retirement, or 30 to 60 percent of the effect on cumulative retirement shown in panels 4 and 5. This occurs both because RHBs have no effect on retirement until age 60 in panel 6, and because the estimated RHB effect on retirement of workers aged 60–64 is smaller in panel 6 than in panels 4 and 5.

An important implication of the simulations is that they make it less important for us to take a stand on which estimator—pooled OLS, random effects, or fixed effects—is most convincing. Why? The differences among the simulations in panels 1, 2, and 3 of Table 4, which result from different estimators, are striking. But Table 4 makes clear that functional form specification—in particular, allowing the effect of RHBs on retirement to vary with age—is more important than the choice of an estimator. If we base our inference on the restricted model, then estimates of the effect of RHBs on cumulative retirement range between 0 and 57 percent (Table 4, panels 1, 2, and 3)—a range too wide to be useful. Rejecting the restricted model and allowing the effect of RHBs to vary with age narrows the range to between 6 and 19 percent (panels 4, 5, and 6). If we then reject pooled OLS because the random effects estimator dominates (by accounting for serial correlation of the composite error term), we further narrow the range to between 6 and 11 percent (panels 5 and 6). Finally, we might favor the upper end of this range if we believe measurement error attenuates the fixed effects estimate, but in any case,

the conclusion would be that RHBs have a significant but relatively modest effect of the cumulative retirement of workers aged 50 to 65.

VIII. Summary and Conclusions

We have used data from the main cohort of the Health and Retirement Study to extend past work on RHBs in two ways. First, we specify an unrestricted model that allows the effect of RHBs to differ among different subgroups of workers—in particular, among workers of different ages—and over time. The findings suggest that RHBs had no effect on the retirement behavior of working men aged 50–56, a modest or no effect on the retirement behavior of men aged 57–59, and increased the retirement probability of men aged 60–64 by 5 to 7.5 percentage points (on a base of 6.1 percent for 60–64-year-old men without RHBs). Also, workers with RHBs were substantially more likely to retire during the slack labor market of 2000–2002. To the extent RHBs have an effect on retirement behavior, then, the effects appear to be for men in their early 60s (that is, workers who are within five years of Medicare eligibility), and during periods of slack labor markets.

Second, because we examine a sample of men from the HRS over a 12-year period, we can apply a fixed effects estimator as a possible way of controlling for unobserved individual effects on retirement. Unobserved effects are a concern because it stands to reason that workers with a taste for early retirement would select (or sort into) jobs that offer RHBs; accordingly, estimators that do not take account of unobserved tastes for retirement will tend to overstate the retirement effect of RHB offers per se. On their face, estimates of the restricted model do suggest the presence of unobserved heterogeneity: Whereas pooled OLS and random effects estimators suggest that RHB offers increase the two-year retirement probability of men aged 60–64 by 0.03

(that is, by 27 percent relative to the average two-year retirement probability of 0.11), the fixed effects estimator suggests that RHB offers increase the retirement probability of these men by 0.015 percentage points (14 percent), and this latter estimate is quite imprecise (p -value = 0.13).

Nevertheless, unobserved heterogeneity is only one possible explanation for the smaller estimated RHB effect when we move from a pooled OLS or random effects estimator to a fixed effects estimator—measurement error in the RHB variable and functional form misspecification are alternative possibilities. In the appendix, we perform two sets of tests to check whether the attenuated fixed effects estimates result from measurement error in the RHB variable. We first restrict the sample to observations in which we are relatively confident that the RHB questions were answered accurately, and find that the estimated RHB effects are similar to those estimated with the full sample. This suggests that measurement error explains little or none of the attenuation in the fixed effects estimates. We then apply a bounding technique, which offers somewhat more support for measurement error as an explanation for the attenuation. Still, the bounding technique suggests that half or more of the attenuation cannot be explained by measurement error. Although these tests for the severity of measurement error are far from conclusive, we believe it is safe to say that measurement error plays a relatively minor role in the attenuation that occurs when we employ fixed effects, and that half or more of the attenuation results from model misspecification (an overly restrictive functional form, for example) or from controlling for time-invariant unobservables that are specific to the individual worker.

The empirical survivor functions reported in section VII clarify the implications of the findings and highlight the importance of allowing the effect of RHBs on retirement to vary by age. Survivor functions based on the restricted model, which forces the effect of RHBs to be equal for all workers aged 50-65, give results that are too dispersed to be useful. Specifically,

these survivor functions suggest that RHBs could have no effect at all on cumulative person-years of retirement at all (if we base the survivor functions on Table 2's fixed effects estimates), or RHBs could increase cumulative person-years of retirement increase by 37 to 57 percent (if we base them on random effects or pooled OLS).

In sharp contrast, when we base the survivor functions on the unrestricted model in which the effect of RHBs varies with age, we find that RHBs increase cumulative person-years of retirement by 6 to 11 percent. This relatively modest effect (and narrow range) results because, in the unrestricted model, RHBs consistently affect the retirement behavior only of men in their early 60s.

RHBs and policies that would expand the availability of government-provided health care to workers under age 65 have been a concern to both employers and policymakers. Employers' concerns have focused on the effects of RHBs on labor costs, and policymakers have focused on the costs and possible labor market effects of expanding Medicare. The findings presented here suggest the importance of distinguishing the effects of these plans and policies on workers of different ages. Although RHBs appear to have a significant effect on the retirement behavior of men in their early 60s, we conclude that their effect on younger men is small to nil.

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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 | n/a | n/a |
| Health Insurance Coverage | | | |
| employer-provided but no RHBs (reference) | 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 |
| Defined Benefit pension wealth (\$) | | | |
| 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 |
| Defined Contribution pension accumulation (\$) | | | |
| 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 |
| Non-housing 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 |

| | Full sample (all two-year transitions) (1) | Full sample (wave 1 values) (2) | Retirees (wave 1 values) (3) |
|--|---|--|---------------------------------------|
| 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 |

Source: Health and Retirement Study sample of 3,150 men aged 51 to 61 who were working full-time in 1992.

Table 2
Estimates of the Restricted Retirement Model

| Independent variable | Pooled OLS | | | Random Effects | | | Fixed Effects | | |
|--|-------------------|----------------|-----------------|-------------------|----------------|-----------------|-------------------|----------------|-----------------|
| | (1) | (2) | (3) | (1) | (2) | (3) | (1) | (2) | (3) |
| | Coef. estimate | Robust s.e. | <i>p</i> -value | Coef. estimate | Robust s.e. | <i>p</i> -value | Coef. estimate | Robust s.e. | <i>p</i> -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 pension wealth (\$) | | | | | | | | | |
| 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 pension accumulation (\$) | | | | | | | | | |
| 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 (\$) | | | | | | | | | |
| <1 (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 |

| Independent variable | Pooled OLS | | | Random Effects | | | Fixed Effects | | |
|--|-------------------|----------------|---------|-------------------|----------------|---------|-------------------|----------------|---------|
| | (1) | | | (2) | | | (3) | | |
| | Coef. estimate | Robust s.e. | p-value | Coef. estimate | Robust s.e. | p-value | Coef. estimate | Robust s.e. | p-value |
| 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 |
| Marital 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 |

| Independent variable | Pooled OLS | | | Random Effects | | | Fixed Effects | | |
|--------------------------|-------------------|----------------|-----------------|-------------------|----------------|-----------------|-------------------|----------------|-----------------|
| | (1) | | | (2) | | | (3) | | |
| | Coef. estimate | Robust s.e. | <i>p</i> -value | Coef. estimate | Robust s.e. | <i>p</i> -value | Coef. estimate | Robust s.e. | <i>p</i> -value |
| 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 | | | 9,657 | | |
| Number of men | 3,150 | | | 3,150 | | | 3,150 | | |
| 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: Figures in the “Coef. estimate” column give estimated average effects on the two-year retirement probability of men in the full sample, as described in Table 1 and Figure 1. Estimates come from applying pooled OLS, random effects, and fixed effects estimators to equation (4), with $\mathbf{x}_{it}\boldsymbol{\beta}$ specified as in equation (2). The dependent variable is an indicator equal to 1 if a man was retired in period $t+1$ (approximately two years after t). Standard errors are robust to heteroskedasticity and serial correlation of errors for each worker over time. The non-white and education variables are dropped from the fixed effects estimator because they do not vary over time.

Table 3
Estimated RHB Effects for Subgroups from the Full Sample

| Subgroup or transition | Pooled OLS | | Random Effects | | Fixed Effects | |
|--|--------------|-----------------|----------------|-----------------|---------------|-----------------|
| | Estimate | <i>p</i> -value | Estimate | <i>p</i> -value | Estimate | <i>p</i> -value |
| Age 50–56 | 0.008 | (0.40) | 0.000 | (0.98) | -0.022 | (0.09) |
| Age 57–59 | 0.027 | (0.02) | 0.024 | (0.04) | -0.011 | (0.47) |
| Age 60–64 | 0.075 | (0.02) | 0.080 | (0.00) | 0.047 | (0.02) |
| Age 65+ | -0.120 | (0.01) | -0.144 | (0.00) | -0.191 | (0.00) |
| White | 0.030 | (0.00) | 0.026 | (0.00) | -0.007 | (0.55) |
| Non-white | 0.012 | (0.47) | 0.013 | (0.46) | 0.004 | (0.86) |
| Less than high school | 0.046 | (0.00) | 0.038 | (0.03) | -0.012 | (0.62) |
| High school only | 0.029 | (0.01) | 0.024 | (0.06) | -0.013 | (0.46) |
| Some college | 0.035 | (0.01) | 0.032 | (0.02) | 0.018 | (0.32) |
| College degree or more | 0.005 | (0.73) | 0.007 | (0.62) | -0.007 | (0.72) |
| Fair or poor self-reported health | 0.045 | (0.03) | 0.042 | (0.06) | -0.006 | (0.82) |
| Good/very good/excellent self-reported health | 0.025 | (0.00) | 0.022 | (0.01) | -0.005 | (0.63) |
| Multiple chronic health conditions | 0.016 | (0.24) | 0.012 | (0.41) | -0.013 | (0.51) |
| Without multiple chronic health conditions | 0.032 | (0.00) | 0.029 | (0.00) | -0.002 | (0.82) |
| Not married | 0.048 | (0.01) | 0.045 | (0.02) | 0.006 | (0.80) |
| Married / spouse works full-time | 0.015 | (0.13) | 0.014 | (0.19) | -0.006 | (0.66) |
| Married / spouse works part-time | 0.024 | (0.12) | 0.017 | (0.30) | -0.004 | (0.82) |
| Married / spouse works < part-time | 0.034 | (0.00) | 0.030 | (0.02) | -0.010 | (0.54) |
| Job tenure at most 15 years | 0.009 | (0.35) | 0.007 | (0.46) | -0.015 | (0.27) |
| Job tenure > 15 years | 0.047 | (0.00) | 0.041 | (0.00) | 0.004 | (0.78) |
| Blue-collar occupation | 0.029 | (0.01) | 0.024 | (0.05) | -0.011 | (0.50) |
| White-collar occupation | 0.027 | (0.00) | 0.025 | (0.01) | -0.001 | (0.94) |
| 1992–1994 transition | 0.022 | (0.05) | 0.013 | (0.28) | -0.054 | (0.00) |
| 1994–1996 transition | 0.019 | (0.14) | 0.013 | (0.32) | -0.023 | (0.15) |
| 1996–1998 transition | 0.028 | (0.07) | 0.029 | (0.62) | 0.024 | (0.13) |
| 1998–2000 transition | 0.014 | (0.47) | 0.014 | (0.47) | 0.017 | (0.39) |
| 2000–2002 transition | 0.081 | (0.00) | 0.088 | (0.00) | 0.095 | (0.00) |
| 2002–2004 transition | 0.041 | (0.29) | 0.056 | (0.16) | 0.068 | (0.10) |
| <i>Estimated main RHB effect</i> (from Table 2): | <i>0.030</i> | <i>(0.00)</i> | <i>0.029</i> | <i>(0.00)</i> | <i>0.015</i> | <i>(0.13)</i> |

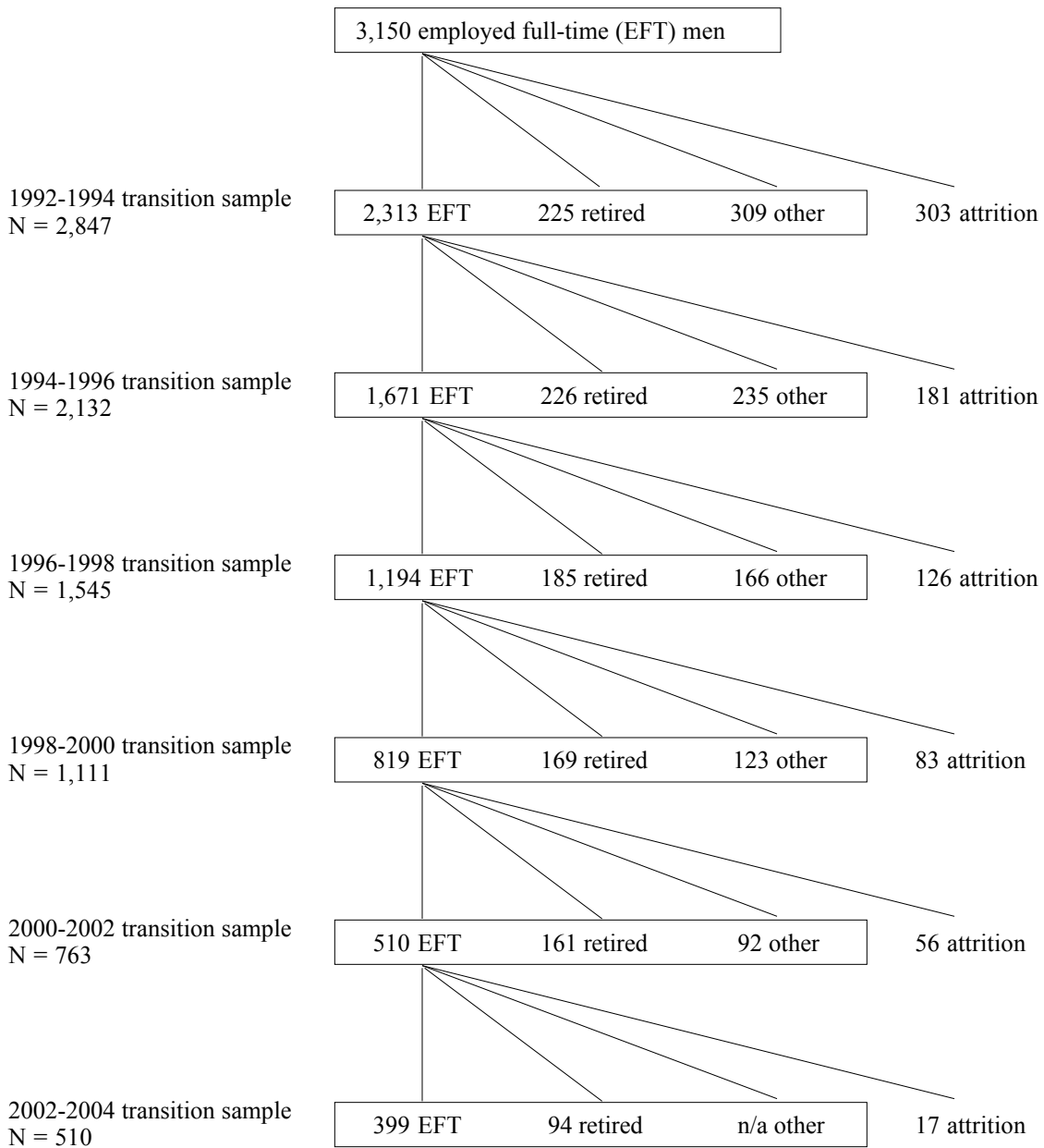
Note: Figures in the “Estimate” column give the estimated effect of an RHB offer on the two-year retirement probability of workers in the specified group, relative to workers in the same group who had employer-provided health insurance but no RHB. Subgroup estimates are obtained by estimating equation (4) with $\mathbf{x}_{it}\boldsymbol{\beta}$ specified as in equation (3)—that is, *rhb* is fully interacted with the other independent variables in the model. Each subgroup estimate is computed by evaluating the derivative of *retired* with respect to *rhb* for the subgroup at the sample mean. Section III describes computation of the subgroup effects from the complete model. Complete model estimates are available from the authors.

Table 4
Survivor functions of men aged 50 to 65 and RHB-induced retirements (R_t) at each age (based on estimates from Tables 2 and 3)

| Age | (1) Table 2 Pooled OLS | | | (2) Table 2 Random Effects | | | (3) Table 2 Fixed Effects | | | (4) Table 3 Pooled OLS | | | (5) Table 3 Random Effects | | | (6) Table 3 Fixed Effects | | |
|---|------------------------------|------|-------|----------------------------------|------|-------|---------------------------------|------|-------|------------------------------|------|-------|----------------------------------|------|-------|---------------------------------|------|-------|
| | RHB | | | RHB | | | RHB | | | RHB | | | RHB | | | RHB | | |
| | No | Yes | R_t | No | Yes | R_t | No | Yes | R_t | No | Yes | R_t | No | Yes | R_t | No | Yes | R_t |
| | | | | | | | | | | | | | | | | | | |
| 50 | 1000 | 1000 | 15 | 1000 | 1000 | 15 | 1000 | 1000 | 0 | 1000 | 1000 | 0 | 1000 | 1000 | 0 | 1000 | 1000 | 0 |
| 51 | 992 | 977 | 15 | 978 | 964 | 14 | 973 | 973 | 0 | 984 | 984 | 0 | 969 | 969 | 0 | 970 | 970 | 0 |
| 52 | 983 | 954 | 14 | 956 | 928 | 13 | 947 | 947 | 0 | 968 | 968 | 0 | 938 | 938 | 0 | 940 | 940 | 0 |
| 53 | 975 | 931 | 14 | 935 | 894 | 12 | 921 | 921 | 0 | 953 | 953 | 0 | 909 | 909 | 0 | 911 | 911 | 0 |
| 54 | 966 | 909 | 13 | 915 | 862 | 11 | 896 | 896 | 0 | 937 | 937 | 0 | 881 | 881 | 0 | 884 | 884 | 0 |
| 55 | 958 | 888 | 13 | 895 | 830 | 11 | 872 | 872 | 0 | 922 | 922 | 0 | 853 | 853 | 0 | 857 | 857 | 0 |
| 56 | 950 | 867 | 12 | 875 | 800 | 10 | 849 | 849 | 0 | 907 | 907 | 0 | 826 | 826 | 0 | 831 | 831 | 0 |
| 57 | 942 | 847 | 10 | 856 | 771 | 8 | 826 | 826 | 0 | 893 | 893 | 12 | 800 | 800 | 10 | 805 | 805 | 0 |
| 58 | 916 | 811 | 9 | 825 | 732 | 7 | 813 | 813 | 0 | 866 | 854 | 11 | 769 | 759 | 9 | 793 | 793 | 0 |
| 59 | 891 | 776 | 8 | 794 | 694 | 6 | 801 | 801 | 0 | 840 | 816 | 10 | 738 | 720 | 8 | 780 | 780 | 0 |
| 60 | 866 | 743 | -1 | 765 | 659 | -2 | 789 | 789 | 0 | 815 | 781 | 26 | 709 | 683 | 26 | 768 | 768 | 18 |
| 61 | 784 | 661 | -2 | 679 | 575 | -3 | 721 | 721 | 0 | 745 | 685 | 21 | 637 | 585 | 19 | 712 | 694 | 15 |
| 62 | 710 | 589 | -3 | 603 | 502 | -4 | 658 | 658 | 0 | 682 | 601 | 16 | 573 | 502 | 14 | 660 | 627 | 12 |
| 63 | 642 | 524 | -3 | 535 | 438 | -5 | 601 | 601 | 0 | 624 | 527 | 12 | 515 | 430 | 9 | 612 | 566 | 10 |
| 64 | 581 | 466 | -4 | 475 | 383 | -5 | 549 | 549 | 0 | 571 | 463 | 8 | 463 | 369 | 6 | 567 | 512 | 8 |
| 65 | 526 | 415 | -- | 421 | 334 | -- | 502 | 502 | -- | 522 | 406 | -- | 416 | 316 | -- | 525 | 462 | -- |
| Cumulative person-years of retirement: | | | | | | | | | | | | | | | | | | |
| | 2313 | 3642 | 1325 | 3491 | 4634 | 1142 | 3281 | 3281 | 0 | 2772 | 3303 | 530 | 4004 | 4458 | 455 | 3385 | 3600 | 215 |
| Person-years of RHB-induced retirements as a percentage of person-years of retirement for the counterfactual (RHB-ineligible) cohort: | | | | | | | | | | | | | | | | | | |
| | | | 57 | | | 37 | | | 0 | | | 19 | | | 11 | | | 6.4 |

Note: The “RHB-No” and “RHB-Yes” columns show the number of workers without and with RHB offers who remain working full-time based on retirement hazard functions (conditional probabilities of retiring at each age) calculated from each of the six sets of estimates in Tables 2 and 3 and applied to a cohort of 1,000 workers starting at age 50. The R_t column shows the number of RHB-induced retirements at each age. “Cumulative person-years of retirement” are the accumulated number of years of retirement experienced by each cohort at the time it reaches age 65. See the text for further discussion.

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.

Appendix. Sensitivity of the Findings to Measurement Error

In this appendix, we take two approaches to appraising the extent to which measurement error may be responsible for attenuation of the RHB effect when we move to a fixed effects estimator. First, we follow Hirsch and Schumacher's (1998) sample restriction approach, which involves narrowing the sample to observations in which responses are likely to be accurate, then checking whether the findings differ from those obtained with the full sample. Second, we make use of econometric bounding techniques developed to appraise the seriousness of measurement error.

Table A1 summarizes the findings of two sets of sample restriction tests. For comparison, row 1 of the table repeats the estimated RHB effects from Table 2 (pooled OLS, random effects, and fixed effects). Rows 2a and 2b report estimated RHB effects after dropping the first wave (row 2a) or the first and second waves (row 2b) of our analysis sample and re-estimating the models. The rationale for dropping the first two waves is developed by Congdon-Hohman (2008), who notes that some households' responses to the RHB question appear inconsistent, 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. Dropping the first wave (or first and second waves) should lessen the effects of these inconsistencies. The findings in rows 2a and 2b suggest that all three estimators are fairly robust to dropping the first one or two waves from the sample. In particular, the difference between the pooled OLS and random effects estimates (on one hand) and the fixed effects estimates (on the other) are consistently about 1.5 percentage points. If we believe that measurement of RHB eligibility is likely to be more accurate in later waves of the HRS, then

these findings suggest that something other than measurement error (functional form misspecification or unobserved heterogeneity) is attenuating the fixed effects estimates.

Rows 3a, 3b, and 3c of Table A1 report estimates from a second set of sample restriction tests. The estimates in these rows come from samples in which individuals have been dropped if their reported RHB status changed more than three times (row 3a), more than twice (row 3b), or more than once (row 3c). The rationale here is that workers typically must have several years of tenure with an employer before they become eligible for RHBs, so those who report multiple changes in their RHB eligibility are less likely to be reporting their true RHB status. For example, it is unlikely that a worker would lose and then regain RHB eligibility over the (at most) ten years (1992–2002) for which we observe RHB status. Accordingly, we expect the estimates in rows 3a, 3b, and 3c to be the product of progressively less measurement error.

Rows 3a and 3b suggest that dropping workers who report two or more changes in RHB status has little influence on the estimates, which again suggests that factors other than measurement error are attenuating the fixed effects estimates. In row 3c, we restrict the sample to individuals who are most likely to be giving reliable reports about their RHB eligibility, and the fixed effects estimate of the RHB effect is nil. This last finding would suggest that measurement error plays essentially no role in explaining differences between the random effects and fixed effects estimates.¹⁴

Whereas Table A1 shows estimates of the restricted model using various subsamples of data, Table A2 displays subgroup effects (based on the unrestricted model) estimated from a subsample more likely than the full sample to be reporting their true RHB status. The setup of this table is the same as Table 3; the only difference is that the estimates come from a sample in

¹⁴ We have also examined cross-tabulations of changes in RHB status by job tenure and by change in job, and have attempted to use these joint changes to detect misclassification of RHB status. Samples selected in these ways are almost identical to those used for the estimates in row 3c of Table A1.

which workers changed RHB status at most once during the time we observe them (so the estimates are analogous to row 3c of Table A1). The findings displayed in Table A2 are essentially consistent with those in Table 3, which suggests again that most of the attenuation in the fixed effects estimates can be attributed to unobserved heterogeneity rather than measurement error.

The second approach we take to appraising the seriousness of measurement error is to make use of bounding techniques (see Bound, Brown, and Methiowetz 2001 for an overview). In particular, we take advantage of Bollinger's (1996, 2001) method of calculating the proportion p of "positive misclassifications" that would be consistent with attenuation being fully attributable to measurement error. In our context, a "positive misclassification" is a worker who is classified as RHB-eligible when he is in fact RHB-ineligible. Bollinger's formula is:

$$p < P_x(1 - R^2)(1 - b/d),$$

where P_x is the mean proportion of RHB-eligibles observed in the sample; R^2 is the R^2 computed from the regression of *rhb* on all controls, including a dummy variable for each individual; b is the slope coefficient on *rhb* from the pooled OLS regression of *retired* on *rhb* and all controls; and d is the inverse of the coefficient on *retired* from the reverse regression (pooled OLS regression *rhb* on retirement and all controls). A similar formula gives the proportion q of "negative misclassifications" (classified as RHB-ineligible when in fact RHB-eligible) consistent with attenuation being fully attributable to measurement error.

When we apply Bollinger's method to this problem, we obtain a bound for p of 0.185 $\{= 0.516 \times (1 - 0.641) \times [1 - (0.0305/15.73)]\}$ and a bound for q of 0.173 $\{= (1 - 0.516) \times (1 - 0.641) \times [1 - (0.0305/15.73)]\}$. The interpretation of these findings is that if approximately 17

percent or more of the RHB responses are errors, then we should attribute all of the attenuation in the fixed effects estimates to measurement error.

The question, of course, is whether as many as 17 percent of the RHB responses in the HRS are in error. In the absence of a validation study of RHB responses, we cannot answer this question with any confidence. However, Mitchell's (1988) validation study of the extent to which workers know about their pension plans suggested that about 10 percent of workers in the 1983 Survey of Consumer Finances were misinformed about whether they were covered by a pension or about the type of pension (DB or DC) for which they were eligible. Whether measurement error is similar in the case of RHBs is an open question; however, RHBs and pensions are similar kinds of benefits, so 10 percent might be a reasonable first approximation to the extent of measurement error of RHB eligibility in the HRS. If so, and if we take the above calculations at face value, then about half the attenuation of the fixed effects estimates (compared with the pooled OLS and random effects) might be attributable to unobserved heterogeneity; the other half would be attributed to measurement error.

Finally, it is worth noting that recent work by Bollinger and van Hasselt (2009) suggests that the bounds we have calculated may be unreasonably wide. The implication is that we should probably not take the above bounding calculations at face value, but should attribute less of the attenuation of the fixed effects estimates to measurement error than those calculations suggest. In this case, we would attribute more than half of the attenuation to functional form misspecification or controlling for unobserved heterogeneity.

Although it would be unwise to draw strong conclusions from these two exercises, each is suggestive. When we restrict the sample in various ways to workers who we believe were surveyed more accurately or consistently, or who gave more accurate self-reports of RHB

coverage, we continue to obtain fixed effects estimates that are roughly half the size of the pooled OLS and random effects estimates. Accordingly the sample restriction sensitivity tests offer evidence that measurement error is not the cause of the attenuated fixed effects estimates. The bounding exercise offers more support to measurement error as an explanation of the attenuated fixed effects estimates, but it suggests nevertheless that half or more of the attenuation has some other cause. Overall, we conclude that most of the attenuation occurs because the fixed effects estimator reduces unobserved heterogeneity, because the restricted model is misspecified, or both.

Table A1
Sensitivity Tests: Estimated Average RHB Effects from Restricted Samples

| Sample | Pooled OLS | | | Random Effects | | | Fixed Effects | | | Number of transitions | Number of individuals |
|--|----------------|-------------|-----------------|----------------|-------------|-----------------|----------------|-------------|-----------------|-----------------------|-----------------------|
| | Coef. estimate | Robust s.e. | <i>p</i> -value | Coef. estimate | Robust s.e. | <i>p</i> -value | Coef. estimate | Robust s.e. | <i>p</i> -value | | |
| 1. Full Sample (from Table 2) | 0.030 | 0.007 | 0.000 | 0.029 | 0.008 | 0.000 | 0.015 | 0.010 | 0.132 | 9,567 | 3,150 |
| 2a. Wave 1 dropped | 0.031 | 0.009 | 0.001 | 0.028 | 0.010 | 0.003 | 0.018 | 0.011 | 0.099 | 6,507 | 2,313 |
| 2b. Waves 1 and 2 dropped | 0.038 | 0.012 | 0.002 | 0.035 | 0.013 | 0.007 | 0.021 | 0.016 | 0.195 | 4,194 | 1,671 |
| 3a. Changed RHB status at most three times | 0.031 | 0.007 | 0.000 | 0.029 | 0.008 | 0.000 | 0.015 | 0.010 | 0.146 | 9,543 | 3,131 |
| 3b. Changed RHB status at most twice | 0.032 | 0.008 | 0.000 | 0.030 | 0.008 | 0.000 | 0.013 | 0.011 | 0.233 | 9,032 | 3,042 |
| 3c. Changed RHB status at most once | 0.024 | 0.009 | 0.001 | 0.024 | 0.010 | 0.016 | -0.000 | 0.015 | 0.986 | 7,485 | 2,740 |

Note: Figures in the “Coef. estimate” column give estimated average effects on the two-year retirement probability of men in various restricted samples, as shown in the “Sample” column. The model estimated is the same as that underlying the estimates in Table 2. Standard errors are robust to heteroskedasticity and serial correlation of errors for each worker over time.

Table A2
Estimated RHB Effects for Subgroups from the Sample Restricted
to Workers Who Changes RHB Status at Most Once

| Subgroup or transition | Pooled OLS | | Random Effects | | Fixed Effects | |
|---|------------|-----------------|----------------|-----------------|---------------|-----------------|
| | Estimate | <i>p</i> -value | Estimate | <i>p</i> -value | Estimate | <i>p</i> -value |
| Age 50–56 | 0.012 | (0.35) | 0.004 | (0.76) | -0.037 | (0.07) |
| Age 57–59 | 0.019 | (0.19) | 0.022 | (0.15) | -0.015 | (0.46) |
| Age 60–64 | 0.062 | (0.00) | 0.076 | (0.00) | 0.051 | (0.06) |
| Age 65+ | -0.107 | (0.10) | -0.143 | (0.04) | -0.201 | (0.01) |
| White | 0.027 | (0.01) | 0.028 | (0.01) | -0.011 | (0.52) |
| Non-white | 0.001 | (0.96) | 0.001 | (0.97) | -0.016 | (0.68) |
| Less than high school | 0.057 | (0.00) | 0.055 | (0.01) | -0.004 | (0.91) |
| High school only | 0.022 | (0.13) | 0.017 | (0.30) | -0.041 | (0.13) |
| Some college | 0.023 | (0.15) | 0.023 | (0.19) | 0.014 | (0.59) |
| College degree or more | -0.004 | (0.83) | 0.006 | (0.77) | -0.001 | (0.98) |
| Fair or poor self-reported health | 0.039 | (0.12) | 0.041 | (0.13) | -0.015 | (0.66) |
| Good/very good/excellent self-reported health | 0.021 | (0.03) | 0.022 | (0.05) | -0.011 | (0.49) |
| Multiple chronic health conditions | 0.010 | (0.53) | 0.009 | (0.63) | -0.018 | (0.50) |
| Without multiple chronic health conditions | 0.028 | (0.01) | 0.030 | (0.01) | -0.009 | (0.58) |
| Not married | 0.061 | (0.00) | 0.063 | (0.01) | 0.021 | (0.59) |
| Married / spouse works full-time | 0.006 | (0.62) | 0.007 | (0.60) | -0.020 | (0.34) |
| Married / spouse works part-time | 0.008 | (0.69) | 0.004 | (0.84) | -0.014 | (0.61) |
| Married / spouse works < part-time | 0.032 | (0.02) | 0.033 | (0.03) | -0.016 | (0.47) |
| Job tenure at most 15 years | 0.010 | (0.40) | 0.011 | (0.39) | -0.015 | (0.42) |
| Job tenure > 15 years | 0.037 | (0.00) | 0.037 | (0.01) | -0.008 | (0.71) |
| Blue-collar occupation | 0.025 | (0.07) | 0.025 | (0.10) | -0.014 | (0.55) |
| White-collar occupation | 0.022 | (0.05) | 0.023 | (0.07) | -0.010 | (0.61) |
| 1992–1994 transition | 0.015 | (0.24) | 0.008 | (0.55) | -0.064 | (0.01) |
| 1994–1996 transition | 0.017 | (0.28) | 0.012 | (0.44) | -0.031 | (0.14) |
| 1996–1998 transition | 0.021 | (0.29) | 0.024 | (0.22) | 0.012 | (0.58) |
| 1998–2000 transition | 0.018 | (0.49) | 0.024 | (0.39) | 0.022 | (0.46) |
| 2000–2002 transition | 0.099 | (0.01) | 0.114 | (0.01) | 0.117 | (0.01) |
| 2002–2004 transition | 0.006 | (0.91) | 0.032 | (0.55) | 0.036 | (0.53) |
| <i>Estimated main RHB effect (from Table A1, row 3c):</i> | | | | | | |
| | 0.024 | (0.00) | 0.024 | (0.02) | -0.000 | (0.99) |

Note: The above estimates come from the same model as that underlying Table 3's estimates, but estimated using a sample of workers whose reported RHB eligibility changed at most once during the years observed. See the note to Table 3.