How Do Pensions Affect Household Wealth Accumulation?

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Abstract

Most analyses of pension effects on saving behavior use an empirical specification derived from a very simple version of the life cycle model, with a fixed retirement age, a perfect capital market, isoelastic or quadratic preferences, and a “unitary” specification of household preferences. My empirical analysis relaxes several of these strong assumptions. The results indicate that estimates of the effects of pensions on household wealth accumulation are somewhat sensitive to specification. For example, the effect of the accumulated balance in Defined Contribution plans for women is around .20 and significantly different from zero, while the effect for men is about .02. This difference is obscured when the household DC balance is used. The effects of Defined Benefit pension wealth and Social Security wealth are estimated to be quite different from the effects of Defined Contribution balances in some cases. Overall, the results are fairly consistent across specifications in showing very small effects of pensions on household wealth.

1. Introduction

Employer-provided pensions are a form of "backloaded compensation," or forced saving. Economic reasoning suggests that workers will respond to forced saving in a pension plan by saving less in other forms. Thus pensions may "crowd out" household saving. The extent of crowd out may be limited by imperfections in the capital market (inability to borrow against future benefits or earnings), the tax-favored treatment of pension savings, and legal restrictions and penalties on early access to pension benefits. Alternatively, it has been suggested in the behavioral economics literature that pensions may serve as a commitment device, providing a means to avoid the problems of self control associated with time-inconsistent preferences (Thaler and Benartzi, 2004). In this scenario, pension saving could supplement rather than crowd out private saving.

Understanding the effects of pensions on household wealth accumulation is important for at least two reasons. First, there has been a major shift away from *Defined Benefit* (DB) pension plans in the past quarter century in the United States (Poterba, Venti, and Wise, 2007). DB pensions provide a benefit that is a function of the worker's age, years of service, and earnings history at the time of separation from the firm. DB pensions have been rapidly supplanted by *Defined Contribution* (DC) pensions such as 401(k) plans, in which the retiree receives a lump sum distribution equal to the balance in his individual pension account when he claims the pension. This balance is determined by the amounts contributed to the worker's pension account by the worker and the firm, and the returns earned on the assets held in the account. These very different types of plans may have different savings incentives, so it is important to understand how this shift affects saving behavior. Second, the effects of pensions on saving behavior can provide insight into the potential effects of Social Security reform. Social Security in the U.S. currently resembles a DB plan, but some proposed reforms would change it to be more like a DC plan, with personal accounts and asset allocation determined in part by the worker.

Most analyses of pension effects on saving behavior are guided by the simplest version of the life cycle model, with a fixed retirement age, a perfect capital market, isoelastic or quadratic preferences, and a "unitary" specification of household preferences. In this model, an analytical solution for wealth can be derived, and the model predicts perfect crowd out over the life cycle. Under the assumptions of the model, one can compute a measure of the capitalized value of the future benefits from a pension to use as an explanatory variable in a wealth regression.

It is well understood that these assumptions are quite strong and that simple intuition about crowd out may not hold when these assumptions are relaxed. Feldstein's (1974) analysis of the effect of Social Security on saving demonstrates that when the timing of retirement is a choice, the sign of the effect of Social Security wealth on private saving is ambiguous. In the presence of a borrowing constraint, the effect of future pension benefits on current household saving likely depends on the liquidity of the pension and on the severity of the borrowing constraint (Rust, 1998). Uncertainty about future earnings, asset returns, and medical expenditures could induce precautionary savings, which could affect the magnitude of pension crowd out. Other savings motives such as for bequest purposes could affect the extent of crowd out. Finally pension effects on saving in married-couple households are usually analyzed under the assumption of "unitary" preferences, implying that pension wealth can be summed over spouses, but the "collective" model of household behavior with limited commitment implies that this approach can be misleading (Mazzocco, 2007).

In this paper, I provide an empirical analysis of the effects of pensions on saving behavior in which the influence of several of the strong assumptions made in most previous studies can be investigated. I discuss the implications of a more general life cycle framework for the specification and interpretation of an empirical model of pensions and saving. These implications are compared to those of the framework that has guided most studies. I use data from the Health and Retirement Study (HRS) to estimate models that can be though of as approximations to the wealth accumulation decision rules implied by a fairly general model. I then impose the restrictions of the simpler framework and examine the influence of these restrictions on the results.

I find that estimates of the effects of pensions on household wealth accumulation are somewhat sensitive to specification. For example, the effect of the DC account balance for women is around .20 and significantly different from zero, while the effect for men is about .02. This difference is obscured when the household DC balance is used. The effects of DB wealth and Social Security wealth are estimated to be quite different from the effects of the DC balance in some cases. Overall, however, the results are fairly consistent across specifications in showing small effects of pensions on household wealth, and in some cases positive effects, i.e. *crowd in*. These findings are consistent with the results of several previous studies that use data and methods similar to those used here (Alessie, Kapteyn, and Klijn, 1997; Engelhardt and Kumar, 2007; Gustman and Steinmeier, 1999).[1] However, some studies that use similar approaches find

[1] The analysis reported here uses a median regression estimation approach because of the extreme skewness of the wealth distribution. Several of these studies report results from mean regressions and Instrumental Variables regressions, with results that are in some cases quite different from the median regression results.

evidence of substantial crowd out (Gale, 1998). Some studies that have used public pension reforms, which provide a plausibly exogenous source of variation in public pension wealth, have also found evidence of substantial crowd out (Attanasio and Rohwedder, 2003, Attanasio and Brugiavini, 2003). This suggests that unobserved heterogeneity may have an important influence on the results of studies such as mine that rely on potentially endogenous variation in pension wealth across households. However, Kapteyn, Alessie, and Lusardi (2005) find that public pension reform led to only modest crowd out in Holland. I attempt to reduce the influence of unobserved heterogeneity by controlling for a large number of potentially confounding factors that can be measured in the HRS. However, the main findings are not sensitive to the specification of control variables, and it likely that remaining unobserved heterogeneity affects the estimates. Engelhardt and Kumar (2007) prevent evidence that measurement error in pension wealth may have a strong influence on crowd out estimates, and I am not able to deal with this potential problem. This could bias my estimates toward zero.

The following section of the paper discusses the life cycle model and its implications for empirical specification. The data are described in section 3, and the empirical results are presented and discussed in section 4. The final section concludes.

2. Analytical Framework and Empirical Specification

I begin by describing the framework that has guided most previous studies and developing its empirical implications. In order to provide a reasonably close link between the theory and the empirical analysis, which uses longitudinal data and a dynamic specification, it is useful to specify a discrete time analytical framework, which I borrow from Alessie, Kapteyn, and Klijn (1997).[2] Consider an employed individual of age t who will work until age R-1 > t, retire at age R, and remain retired until death at age T+1. There is no uncertainty and a perfect capital market with a rate of return r in each period. The individual will earn salary W_j, $j = t,...,$ R-1, and at age R will begin receiving fixed real annuities b from a DB pension and s from Social Security. In addition, at age R he will receive a lump sum distribution D_R from a DC plan. The individual and his employer each contribute a proportion c of his salary to his DC account in each period. And the individual pays a proportional payroll tax p to finance Social Security. The individual chooses consumption C_t to maximize the Present Discounted Value (PDV) of remaining lifetime utility given a rate of time preference δ. If preferences over consumption are intertemporally additive and homothetic, the consumption decision rule derived by Alessie et al. (1997) is

$$C_t = \kappa_t[(1+\mathrm{r})(A_{t-1} + D_{t-1} + 2cW_{t-1}) \;+\; \sum_{j=t}^{R-1} W_j(1 - c - p)(1+r)^{t-j} + \sum_{j=R}^{T}(b + s)(1+r)^{t-j}]$$

where A_{t-1} is private wealth held at the end of period t-1, and $\kappa_t \in (0, 1)$ is an adjustment factor for time until death that depends on r, δ, T, and the form of the utility function.[3] The term in square brackets is total remaining lifetime wealth, which consists of private wealth plus the DC balance at the beginning of period t (the first term), the present value of future net earnings (the

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[2]Most previous studies use a single cross section or a time series of cross sections. A continuous time analytic framework provides a natural basis for an empirical analysis that uses such data. The key implications of the analysis do not depend on this choice. See Gale (1998) for an exposition of the continuous time framework. See Attanasio and Rohwedder (2003) for a discrete time framework used as the basis for an empirical analysis using a time series of cross sections.

[3]κ_t is the analogue of Q_t in Gale (1998). Alessie et al. do not include a DC plan. The perfect capital market assumption implies that the DC balance is perfectly fungible. Tax incentives for saving in DC pensions are ignored.

second term), and the present value of future pension and Social Security benefits (the third term). Using the budget constraint for period t

$$A_t + D_t =(1+r)(A_{t-1} + D_{t-1} + 2cW_{t-1}) + W_t(1 - c - p) - C_t$$

to solve for wealth in period t, we obtain an equation for private wealth plus the DC balance held at the end of period t:

$$A_t + D_t = (1-\kappa_t)[(1+r)(A_{t-1} + D_{t-1} + 2cW_{t-1}) + W_t(1 - c - p)] - \kappa_t[E_t + B_t + S_t],$$

where E_t is the PDV of remaining lifetime earnings, B_t is the PDV of DB pension benefits, and S_t is the PDV of Social Security benefits, discounted to t.

The implication of this simple framework is that over the lifetime, private wealth accumulation is crowded out dollar for dollar by public and private pensions (κ_t approaches 1 as t approaches T). κ_t can be computed under assumptions about r, δ, T, and the form of the utility function. The model thus implies a regression specification of the form

$$A_t = X_{1t} + X_{2t} - X_{3t} - X_{4t} - X_{5t} - X_{6t}$$

where

$$X_{1t} = (1-\kappa_t)[(1+r)(A_{t-1} + D_{t-1} + 2cW_{t-1})], \qquad X_{2t} = (1-\kappa_t)W_t(1 - c - p),$$

$$X_{3t} = \kappa_t E_t, \qquad X_{4t} = \kappa_t B_t, \qquad X_{5t} = \kappa_t S_t, \qquad X_{6t} = D_t.$$

As specified, there is nothing to be estimated: the model implies that the regression coefficients are equal to 1 or -1. This is obviously not a very interesting specification, so the typical specification in the literature is

$$A_t = \beta_1 X_{1t} + \beta_2 X_{2t} + \beta_3 X_{3t} + \beta_4 X_{4t} + \beta_5 X_{5t} + \beta_6 X_{6t}.$$

A null hypothesis of interest is that β_4, β_5, and β_6 are equal to -1, and coefficient estimates greater than -1 are taken as evidence of incomplete crowd out.

There are several well-understood problems with this approach. (1) Alternative versions of the life cycle model in which complete crowd out is not built in by assumption would not necessarily imply a regression model of the form given above (Gale, 1998). Thus, a finding that the β's of interest are not equal to -1 may not be interpretable as evidence of incomplete crowd out, but rather a mis-specified model. (2) Measuring the explanatory variables requires assumptions about *r*, δ, *T*, and the form of the utility function in order to compute κ_t. (3) The assumption of a fixed retirement age closes off a channel through which individuals can adjust to forced saving (Feldstein, 1974; Crawford and Lilien, 1981). (4) The assumption of no uncertainty implies no precautionary motive for saving, which could be a serious mis-specification. The above specification is consistent with some sources of uncertainty if preference are risk neutral or quadratic. (5) In many applications, the effects of DC pensions, DB pensions, and Social Security are assumed to be the same (the regressor of interest in this case is ($X_{4t} + X_{5t} + X_{6t}$)). There are several reasons why this is likely to be false, including differential tax treatment and differential liquidity. There have been few tests of this assumption (Alessie et al., 1997, and Hubbard, 1986, are exceptions). (6) Finally, when the model has been extended to deal with multiple member households, the usual assumption about household behavior is the "unitary" model, which implies that each of the variables in the above specification can be calculated as the sum over household members.

I estimate several alternative linear specifications of a model of wealth accumulation, in which various restrictions are relaxed. The least restrictive specification allows different parameters for the husband and wife in two-person households, does not impose a fixed retirement age or a specific form of preferences, replaces unknown values of future earnings with variables that predict earnings[4], and replaces unknown values of the future DB pension benefit and Social Security benefit with variables that help predict them:

$$A_t = \beta_{1\mathrm{m}} D_{mt} + \beta_{1\mathrm{w}} D_{wt} + \alpha_{1\mathrm{m}} D^*_{mt-1} + \alpha_{1\mathrm{w}} D^*_{wt-1} + \alpha_2 A^*_{t-1} + \eta_{\mathrm{m}} W_{mt-1} + \eta_{\mathrm{w}} W_{wt-1} + \gamma_{\mathrm{m}} X_{mt} + \gamma_{\mathrm{w}} X_{wt} + \varepsilon_{\mathrm{t}} \quad (1)$$

where an *m* subscript is for a man and a *w* subscript for a woman, $A^*_{t-1} = A_{t-1}(1 + r_t)$ and similarly for the lagged DC balances, and the *X*'s are state variables that help predict future earnings, asset returns, pensions, and Social Security benefits, and control for preferences (the specific variables included in the *X*'s are discussed below).[5] This specification imposes few restrictions but is uninformative about the effects of DB pensions and Social Security. The β coefficients measure the effects of contemporaneous DC balances on household net worth. The simple life cycle model discussed above predicts values of -1, and does not require adjustment for κ_t, thus avoiding the need to make assumptions about preferences, the interest rate, and remaining length of life.

A somewhat more restrictive and interesting specification adds the EPDV of future pension and Social Security benefits under the assumptions of a fixed and known rate of return, and a known survival probability schedule:

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[4]Replacing unknown values of future earnings with variables that predict them is common in the literature (Engelhardt and Kumar, 2007; Gale; 1998; Attanasio and Rohwedder, 2003).

[5]The contribution rate, *c*, is included as a separate explanatory variable in *X* rather than restricting it to enter in the form shown above. The Social Security payroll tax, *p*, is not included because the only source of variation across households is from the fact that the tax applies only up to a specified amount of earnings.

$$A_t = \beta_{1m}D_{mt} + \beta_{1w}D_{wt-1} + \beta_{2m}B_{mt+1} + \beta_{2w}B_{wt+1} + \beta_{3m}S_{mt+1} + \beta_{3w}S_{wt+1}$$

$$+ \alpha_{1m}D^*_{mt-1} + \alpha_{1w}D^*_{wt-1} + \alpha_2 A^*_{t-1} + \eta_m W_{mt-1} + \eta_w W_{wt-1} + \gamma_m X_{mt} + \gamma_w X_{wt} + \varepsilon_t \quad (2)$$

The t+1 subscripts on B and S indicate that these variables are computed under the assumption of retirement in t+1.The assumption of retirement following the current period is arbitrary, but it has the virtue of not requiring assumptions about future earnings or preferences. The sample used here contains individuals who are relatively close to typical retirement ages, so the values of future DB and Social Security benefits for alternative retirement dates are highly correlated with the values based on retirement in the next period. Thus in practice they add little additional information. Nevertheless, the parameters in this specification do not have a clear economic interpretation. Also note that there is no adjustment for the stage of the life cycle (κ_t).

A still more restrictive specification uses the observed age of retirement from the pension job, the observed age at which Social Security is claimed, and observed and predicted future earnings to compute benefits to compute B and S[6]:

$$A_t = \beta_{1m}D_{mt} + \beta_{1w}D_{wt} + \beta_{2m}B_{mR} + \beta_{2w}B_{wR} + \beta_{3m}S_{mR} + \beta_{3w}S_{wR}$$

$$+ \alpha_{1m}D^*_{mt-1} + \alpha_{1w}D^*_{wt-1} + \alpha_2 A^*_{t-1} + \eta_m W_{mt-1} + \eta_w W_{wt-1} + \gamma_m X_{mt} + \gamma_w X_{wt} + \varepsilon_t \quad (3)$$

where the R subscript denotes the actual retirement date. This specification imposes the assumptions that retirement age is exogenous and known in advance and that there is no uncertainty about future earnings. The interpretation of the β's remains ambiguous, however, because there is no adjustment for the stage of the life cycle.

The last main specification uses an adjustment factor (κ_t) computed under the

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[6]If the individual has not retired or claimed the Social Security benefit by the end of the observation period, I assume that both events occur at age 65, or, if the individual is already older than 65, in the next period.

assumptions of quadratic preferences, a rate of time preference equal to the interest rate, and standard life table survival schedules (see Alessie, et al., 1997, for details):

$$A_t = \beta_{1m}D_{mt} + \beta_{1w}D_{wt} + \beta_{2m}\kappa_t B_{mR} + \beta_{2w}\kappa_t B_{wR} + \beta_{3m}\kappa_t S_{mR} + \beta_{3w}\kappa_t S_{wR} + \alpha_{1m}(1-\kappa_t)D^*_{mt-1} +$$
$$\alpha_{1w}(1-\kappa_t)D^*_{wt-1} + \alpha_2(1-\kappa_t)A^*_{t-1} + \eta_m(1-\kappa_t)W_{mt-1} + \eta_w(1-\kappa_t)W_{wt-1} + \gamma_m X_{mt} + \gamma_w X_{wt} + \varepsilon_t \quad (4)$$

This specification is similar to typical specifications estimated previously, so the β's have the same interpretation as in previous studies, making comparisons more straightforward. As noted above, the interpretation of the β's in this specification is not entirely clear, since under the assumptions described above there is no reason why they should differ from -1. I follow the literature and interpret the β's in this specification as empirical measures of crowd out. Most studies aggregate pensions across household members and estimate a single parameter on each type of pension for the household as a whole, so I estimate a specification of this type as well. I also estimate specifications in which pension and Social Security wealth are aggregated into a single variable.

3. Data

The Health and Retirement Study (HRS) is a biennial longitudinal survey of a sample of U.S. households with individuals aged over 50. The survey began in 1992 with a sample of individuals born from 1931 to 1941, and their spouses. Additional cohorts have been added over time, but I focus on the original cohort, for which the most extensive data are available. The original cohort sample contains 12,652 individuals residing in 7,067 households. Here, I use a subsample that excludes same-sex couples, married couples in which the age difference between the spouses exceeds 10 years, and cases with missing data on year of birth. This leaves a sample of 10,610 individuals residing in 6,413 households as of the initial interview.

In order to be included in the regression analysis, a household must satisfy the following additional criteria: (1) The individual in a single person household and at least one spouse in a married couple household is employed at the wave 1 interview in 1992. (2) An individual who is employed at the wave 1 interview and is covered by a pension provided by the wave 1 employer remains employed on the wave 1 job at the wave 2 interview. (3) The household's net worth is recorded at the wave 1 and wave 2 interviews. I focus on households in which at least one individual is employed at wave 1 because the relationship between pensions and wealth is likely to depend on whether the individual has claimed the pension benefit or balance.[7] The second criterion is imposed because DC balances are usually claimed as a lump sum, either directly or via roll over to an Individual Retirement Account (IRA). Including households in the sample after such a lump sum transfer would induce the appearance of a large crowd out even if there is no crowd out: at the time of the lump sum transfer the DC balance drops to zero and wealth rises by the amount of the former DC balance. Households that satisfy these three criteria remain in the sample until the spouse with pension coverage leaves the wave 1 job (if both spouses have pension coverage at wave 1, then the household's record is censored when either spouse leaves the wave 1 job). For households in which neither spouse has a pension on the wave 1 job, the household remains in the sample until both spouses have left their wave 1 jobs.[8] Thus households may contribute several observations to the analysis. The estimation sample contains 2,981

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[7]See Gale, Muller, Phillips, and Dworsky (2007) for an analysis of pension effects on wealth in a sample of retirees.

[8]The main findings are robust to alternative sample inclusion criteria.

households, including 2,194 married couples and 787 single individuals. There are 8,200 household-year observations. In order to avoid complications involving changing marital status, I censor single individuals at the time of entry into marriage, and I censor married couples at the time of divorce or death of a spouse. I follow households through the 2004 interview or until they attrit.

The dependent variable is household net worth. This is measured at each survey date by summing home equity, financial assets, including IRA balances, and the value of other real estate, businesses, vehicles, and miscellaneous items. I use the net worth measure provided in the RAND version of the HRS, which has been imputed when missing and adjusted to be comparable across survey waves (St. Clair et al., 2007). Note that net worth as measured here does *not* include Social Security wealth, DB pension wealth, or DC balances.

The three main explanatory variables of interest are the balance in the DC pension account, the PDV of future DB pension benefits (DB wealth), and the PDV of future Social Security benefits (SS wealth). These are described in turn.

The DC account balance is reported directly by the respondent. There is a considerable amount of missing data for this variable. In order to avoid the loss of many observations, I keep cases with missing data, and include a dummy variable indicating missing data. DC pensions usually provide quarterly statements with the account balance, so one might expect that respondents could report the balance accurately. Nevertheless, measurement error is a concern and there is no straightforward solution.[9]

[9]Engelhardt and Kumar (2007) use an innovative instrumental variable approach to deal with measurement error in pension wealth. I discuss this approach below.

The HRS asks respondents a substantial battery of questions about DB pensions, including the ages of early and normal retirement, expected benefits at the early and normal retirement ages, and the expected age of retirement and benefit at the expected age. Some studies have used this information to construct a measure of self-reported DB wealth (Engelhardt and Kumar, 2007; Chan and Stevens, 2008). However, the information solicited from respondents is not sufficient to calculate benefits under all possible scenarios about retirement age and future salary. Respondents are also asked for permission to allow the HRS to contact the employer directly in order to request a copy of the written Summary Plan Description and other relevant information about the pension. The benefit formulas and other plan features derived from these documents were coded by the HRS and made available to researchers (under restricted-access conditions) along with pension calculator software. This allows researchers to compute the benefit to which an individual would be entitled under any feasible scenario for age and tenure at exit and the salary profile.

I use this information to compute two measures of DB wealth for each period in which an individual remains on the pension job. The first measure assumes exit from the job in the next period. This measure does not require assumptions about future employment and salary. The two main assumptions required are a rate of return (2% real) and mortality expectations (U.S. life tables). For married individuals I assume that the benefit is taken as a joint and survivor annuity, with the survivor receiving two thirds of the joint annuity. The second measure uses the actual date of exit and observed or imputed future salary up to the exit date.[10] **This draft does not**

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[10]The salary data are reported by respondents at each survey for the previous calendar year. Since the surveys are every other year, salary must be imputed for the off-survey years. I do this by simple averaging when salary is reported in two adjacent survey years, or by assuming

include results for the second measure. Appendix A provides details on these measures. The information required to compute these measures of DB Wealth is missing for a substantial number of cases. As with DC plans, I keep cases with missing data, and include indicators for missing data in the model.

Social Security Wealth is computed using administrative earnings records provided to the HRS by the Social Security Administration for respondents who gave permission for their records to be used. These records provide a measure of Average Indexed Monthly Earnings (AIME) as of the end of 1991. AIME is the lifetime average earnings measure that is the basis for determining the Social Security benefit. With data on AIME it is straightforward to compute the Social Security benefit for alternative claiming ages. As with DB wealth, I compute two measures of SS Wealth, one assuming retirement in the next period and claiming at the earliest possible age (62, or next period if the individual is already older than 62), and the other based on the actual age at which the benefit was claimed (derived from information reported by the respondent) and observed salary. The calculations account for the spouse benefit and survivor benefits. I keep cases with missing data, and include indicators for missing data in the model.

Other important variables included in the analysis are the pension contribution rates of the worker and firm, earnings in the previous calendar year, average lifetime earnings, the AIME, years of work experience, and years of job tenure. Some of these variables are implied directly by the life cycle model, and all help control for earnings expectations. Contribution rates and earnings in the previous year were reported by respondents. Lifetime earnings were

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that salary in year t is equal to salary in year t-1 or t+1 when the data are missing for one of the adjacent years. Salary and other monetary variables are deflated by the Consumer Price Index to 1992 dollars computed from the Social Security earnings records and additional W2 earnings records for 1980-1991.[11]

Table 1 summarizes of the distribution of pension plans on the wave 1 job, by type of plan. Among men employed at wave 1, 39% have no pension coverage on the wave 1 job, 24% have a DB plan only, 18% have a DC plan only, and 19% have both a DB and a DC plan.[12] For women, the figures are 42% with no pension, 25% with a DB plan only, 19% with a DC plan only, and 14% with one plan of each type. DB plans are more common than DC plans in this sample, in contrast to the workforce as a whole, because the major shift away from DB plans had not yet begun when most of the members of these birth cohorts began their pension jobs. Overall, 41% of men have a DB plan, compared to 33% of men who have a DC. The corresponding figures for women are about five percentage points lower in each case. At the family level, one third of married couples have no pension coverage, and 40-42% of singles have no coverage.

Table 2 compares selected characteristics of the sample according to their pension coverage on the wave 1 job. The sample here is limited to individuals who were employed at wave 1 and wave 2 (on the same job), and men and women are pooled. Time-varying variables

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[11]The Social Security earnings records provide capped earnings data when earnings exceed the maximum taxable amount. The W2 records provide uncensored data. The W2 data are used for 1980-1991, and they are used to estimate adjustment factors to "uncap" the censored earnings record for earlier years. Lifetime earnings is set equal to zero when it is missing, and a dummy variables for missing data cases is included.

[12]I use data on up to two pensions per person on the wave 1 job, at most one of which is a DB. It is rare to have more than two plans from the same job, and very rare for an individual to be covered by two DB plans on a given job. Balances and contribution rates for individuals with two DC plans are summed over the two plans. Pension coverage in the sample used here is somewhat lower than in the full HRS sample.

are measured as of wave 2. Workers with pension coverage are better educated, healthier, have longer job tenure and more work experience, work longer hours, and have higher annual and average lifetime earnings than their counterparts without pension coverage. Blacks are no less likely to have a pension than whites, although blacks are under represented among DC holders. Health insurance is much more likely to be provided in jobs that have pension coverage, although non-pension holders often obtain coverage from the spouse's employer. Pension coverage is more common in the public sector, and less common among the self employed. Finally, the most skilled and advantaged workers are most likely to have both DB and DC coverage. The patterns in Table 2 indicate that pension jobs are "good jobs," good jobs usually have more than one good feature, some of which may be observed and others not, and more skilled workers tend to be matched to good jobs. This makes it difficult to disentangle causal effects of pensions from unobserved heterogeneity. I control for most of these variables (and many others, described below) in the regression analysis.

Interestingly, one of the few dimensions along which pension coverage is *not* unambiguously associated with higher socioeconomic status is net worth. Mean net worth is higher among individuals without pension coverage than for two of the three pension groups, and almost as high for the third. This is consistent with the crowd out prediction of the basic life cycle framework, as discussed above, although not too much should be made of this simple bivariate association. However, the distribution of assets is highly skewed, so Table 2 shows median [in brackets] as well as mean net worth. Individuals with no pension have median net worth that is similar to the DB only and DC only groups, but lower than the group with both types.

Table 3 provides descriptive statistics on the main variables of interest in the analysis sample. The average DC balance is 71 (in thousands of 1992 dollars) for men and 33 for women. Average DB wealth is 209 for men and 161 for women, assuming exit in the next period. Social Security wealth is measured at the household level, and averages 184 conditional on no further earnings and claiming at the early age. For married couples the mean is 214 (including the spouse benefit, if applicable, and survivor benefits). The means for singles are 112 for men and 89 for women. Worker plus employer contribution rates for DC plans average 10%. Mean annual earnings in the previous year are 39 for men and 22 for women, including positive values only. Average lifetime earnings is computed by dividing total lifetime earnings to date by the number of potential working years (age minus education minus six). The means are 23 for men and 8 for women.

5. Results

Table 4 presents selected coefficient estimates from median regression models of household net worth. Median regressions are more informative then mean regressions for "typical" behavior for a variable like net worth, which has a highly skewed distribution. The other explanatory variables in the model are listed in the note to the table. They include an extensive set of demographic controls, and some of the innovative variables available in the HRS (subjective probabilities of various events, financial planning horizon, risk aversion, and cognitive ability). Dummies for the pension coverage types listed in Table 1 are included as well, in an attempt to control for unobserved characteristics of pension jobs and pension holders that might be associated with saving propensities.

The first column displays results from equation (1), the least restrictive specification. The coefficient estimate on the current DC balance for men is .025, indicating very little crowd out. The coefficient estimate for women is .17, indicating *crowd in*, rather than crowd out. The (bootstrapped) standard error estimate of .11 indicates that the lower limit of the 95% confidence interval for women is -.05. A coefficient estimate of -1 on the current DC balance is a fairly robust prediction of the life cycle model, but it is clearly not supported by these results. Another prediction of the simple model discussed in Section 2 is that the coefficients on the lagged DC balance should be equal to the coefficient on lagged household assets (see equation 1). The results show estimates of -.01 and .01 on lagged DC balances, and a coefficient of .76 on lagged household net worth, thus clearly rejecting this prediction as well. The effects of lagged earnings and average lifetime earnings are positive and significantly different from zero for both spouses. The contribution rate coefficients are also positive, and significantly different from zero for women.

The second column shows estimates of equation (2), which adds DB wealth and SS wealth, computed under the assumption of retirement in the next period, a real rate of return of 2%, and standard life table survival schedules. The effects of DB wealth are -.020 for men and .028 for women. This specification does not use the life cycle adjustment factor, κ_t. Thus, under the null hypothesis of complete crowd out, the coefficients can be interpreted as estimates of the average value of $-\kappa_t$. Under the assumptions used to compute κ_t for the estimates discussed below, the actual sample mean values (and standard deviations) of κ_t are .044 (.005) for married couples, .061 (.008) for men, and .054 (.006) for women. The coefficient estimates clearly fall outside the interval encompassed by the mean plus or minus two standard deviations. The coefficient estimate on SS wealth is -.069, with a standard error of .032. This estimate indicates some crowd out, but the magnitude is quite small. The third column shows results for equation (3), which has the same specification as equation (2) but computes DB wealth and SS wealth using observed retirement age and salary.[13] The estimates are very similar to those in column 2 with the exception that the coefficient on SS wealth is only -.011.

Column 4 reports results for equation (4), which incorporates κ_t. The main change from column (3) is much larger coefficients on DB wealth in absolute value. The coefficient for men increases in absolute value from -.02 to -.28, while the coefficient for women increases from .03 to .56. These changes are not surprising given the mean values of κ_t reported above, but the result for women is nevertheless striking. Adjusting by κ_t cannot change the sign of the effect, so the small positive effect for women becomes a large positive effect after the adjustment, although with a large standard error. The Social Security wealth effect increases in absolute value from -.01 to -.05. When the life cycle adjustment is applied to the Social Security wealth variable used in equation (2), the coefficient estimate changes from -.069 to -.55 (not shown). Thus the results from incorporating κ_t indicate some evidence of crowd out of private saving by Social Security and DB pensions held by men, no evidence of crowd out for DC pensions held by men, and large *crowd in* effects for DB and DC pensions held by women.

In order to compare the findings to results in the literature, I report results from several additional specifications in Table 5. Specification (5) uses household level versions of the pension variables for married couples, summing the husband's and wife's variables. The results

[13]**In this draft, this version of the DB wealth variable is not used. The DB wealth measure computed under the assumption of retirement in the next period is used throughout the paper. The SS variable does use observed retirement age.**

show coefficient estimates that are essentially weighted averages of the spouse-specific coefficients, with greater weight on men since they hold more pensions than women. The estimated effects of DB wealth, DC balances, and SS wealth are small and insignificantly different from zero. Specification (6) sums the DB and SS wealth variables; the DC balance is left as a separate variable as suggested by the discussion above. The coefficient estimate on household DB plus SS wealth is tiny: .0006. Specification (7) sums all three sources of pension wealth. The estimated effect of total pension wealth is also tiny. This specification of pension wealth is similar to the specifications in Engelhardt and Kumar (2007), Gale (1998), and Gustman and Steinmeier (1999), but they exclude lagged wealth, so specification (8) excludes lagged wealth and DC balances. The pension effect remains tiny.

The results in Tables 4 and 5 indicate that estimates of the effects of pensions on household wealth accumulation are somewhat sensitive to specification. For example, the effect of the DC balance for women is around .20 and significantly different from zero, while the effect for men is about .02. This difference is obscured when the sum of DC balances is used. Multiplying by the life cycle stage adjustment factor causes the DB wealth coefficients to increase substantially in absolute value, but this is not surprising. Overall, however, the results are fairly consistent in showing very small effects of pensions on household wealth.

Table 6 summarizes findings from several previous studies of the effects of pensions and Social Security on household saving. The first panel shows results for studies that use household wealth as the dependent variable. The only previous study that exploits longitudinal data to condition on lagged wealth is Alessie et al. (1997). Their OLS estimate of the effect of occupational pensions in Holland is positive and their estimate of the effect of Social Security is close to zero. Alessie et al. interpret these findings as evidence of unobserved heterogeneity and re-estimate the model using a household fixed effect estimator. The positive pension effect persists, while the SS effect changes drastically to -2.10, significantly different from zero but not from -1. I re-estimated specification (5) from Table 5 by OLS and found some very large effects, all with large standard errors. Estimating a wealth model by mean regression seems to produce rather poor results. I also estimated this model by fixed effects with similarly uninformative results.

Gale's (1998) median regression estimate using data from the U.S. Survey of Consumer Finances, a life cycle stage adjustment (κ_t), and a pension measure that combines DB, DC, and SS is -.77, significantly different from zero but not from -1. My comparable estimate in Table 5 (specification 8) is -.0055. The reason for this large difference is not obvious. Engelhardt and Kumar use the 1992 HRS as a cross section to estimate a model like Gale's, using several estimation approaches. Their median regression estimate is roughly .15, significantly different from zero, and their Instrumental Variable median regression estimate is roughly .10, not significantly different from zero.[14] These results are fairly similar to my estimate of the effect of

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[14]Their IV approach uses pension wealth computed from the pension provider data base for a set of "synthetic individuals" as an instrument for the pension wealth regressor, which is based on respondent-reported information. The instrument for a given individual is computed using that individual's pension plan, but mean earnings, job tenure, birth year, and survival probability schedule of the individual's group rather than the individual's own values. Groups are defined by education, race, sex, age, and public versus private sector employment. The authors argue that this IV approach will correct for measurement error bias, since measurement error in the instrument should be uncorrelated with measurement error in the pension wealth explanatory variable. They also argue that the IV approach will correct for endogeneity of the earnings history and job tenure, but not for bias due to unobserved heterogeneity that affects pension coverage and generosity. This approach cannot be applied in my analysis because the pension wealth variable I use is constructed from the information in the pension provider data base. The question of whether to use self reported data or Summary Plan Descriptions to

DC balances (specifications 5 and 6). They estimate regressions for other quantiles as well, and find estimates that are positive at all quantiles using ordinary quantile regression. Using IV-quantile regression, their estimates become negative at quantiles above .65. The estimate at quantile .95 is roughly -0.50, marginally significantly different from zero. I estimated models for several other quantiles, and found some evidence of negative effects at the 90th quantile, not significantly different from zero. Gustman and Steinmeier (1999) used the HRS 1992 cross section to estimate a specification like the one used by Gale (1998) and Engelhardt and Kumar (2007). Their median regression estimate is .012, quite similar to my estimate of specification (8). Kapteyn et al. (2005) report a median regression estimate of -.115 for crowd out of private wealth by Social Security in Holland. This is fairly similar to my estimates of Social Security crowd out in Table 4.

The studies by Attanasio and Rohwedder (2003) and Attanasio and Brugiavini (3003) listed in the lower panel of Table 6 use household expenditure data and estimate models of household saving.[15] Their samples contain a much wider age range than mine, but they allow the effects of pension wealth to differ by age group, so I report results for the age groups that overlap with the age range of my sample. They use public pension reforms in the U.K. and Italy as a source of exogenous variation to identify the effects of public pensions on household saving. The estimates for the U.K. for the effect of the earnings-related public pension (SERPS) at ages 55-64 is -.75 (.24), indicating substantial crowd out. For Italy, the effects are -.49 (.11) for ages

compute the pension wealth explanatory variable is an interesting one: see Chan and Stevens (2008) for a discussion.

[15]The coefficients on pension wealth in their model of the saving rate have the same interpretation as in models in which wealth is the dependent variable.

46-55, -.21 (.14) for ages 56-60, and -.11 (.04) for ages 61-65. Their estimates are more likely than most to be free of bias from unobserved heterogeneity, so the large magnitude of their estimates for some age groups suggests significant crowd out by public pensions.

As noted above, I attempt to eliminate as much unobserved heterogeneity as possible by controlling for a large number of potentially confounding factors that can be measured in the HRS. Some of these variables affect household wealth in the expected way (not shown in the tables). For example, a longer financial planning horizon and a longer expected lifetime are associated with greater wealth accumulation. In order to determine whether these controls helped reduce heterogeneity bias, I re-estimated specification (4) excluding all of the variables listed in the note to Table 4 except age, marital status, and year effects. The results were very similar to those reported above. I estimated another version of specification (4) in which additional controls for industry, occupation, establishment size, and firm size were included. This specification also yielded similar results, with the exception that the coefficient estimate on DB wealth of men increased in absolute value from -.28 (.20) to -.40 (.20), suggesting some evidence of fairly large crowd out.

The positive effects of DB wealth and DC balances that I find in many of the specifications estimated here are difficult to reconcile with the life cycle model. There may be explanations that are consistent with a richer and more realistic version of the life cycle model. For example, net worth can grow through "active savings" decisions, asset allocation behavior, and passive mechanisms such as realization of capital gains. It is difficult to distinguish these in a simple analysis like the one presented here, but a structural approach to estimation may be able to reveal such behavior. Alternatively, saving behavior may be largely governed by "heuristics and biases" (Benartzi and Thaler, 2007). Workers might treat pensions as a form of financial advice from their employers, and respond by saving more on their own. Unfortunately, there is no straightforward way to distinguish among these explanations here.

6. Conclusions

The results of this study indicate that estimates of the effects of pensions on household wealth accumulation are somewhat sensitive to specification. Overall, however, the results are fairly consistent in showing small effects of pensions on household wealth. Whether this is an accurate reflection of household behavior or a result of bias due to unobserved heterogeneity or measurement error is difficult to determine. I am currently working on further analyses to shed some light on the issue of unobserved heterogeneity: (1) Add an individual error component to the specification and allowing it to depend on the individual-specific means of the time-varying variables, as in Kapteyn et al. (2005). This approach can be implemented with quantile regression, unlike a fixed effects approach. (2) Account for the possibility of bias resulting from serial correlation in a specification that conditions on the lagged value of the dependent variable. (3) Use a pension wealth measure derived from respondent-reported information, as in Chan and Stevens (2008) and Engelhardt and Kumar (2007), in order to examine the sensitivity of the results to this important specification issue.

I am also working on specifying a life cycle model of employment and savings that incorporates pensions enter in a fairly realistic way. A model of this type is too complicated to yield analytic results, but can be solved numerically and simulated in order to provide insight on the extent to which the strong assumptions usually employed in empirical analysis can explain deviations from the complete crowd out predicted by the simplest life cycle model. Previous simulation studies of pension effects on savings have accounted for some of these features, but have not allowed for choice of retirement age (Engen, Gale, and Uccello, 1999; Laibson, Repetto, and Tobacman, 1998; Scholz, Seshadri, and Khitatrakun, 2006).

Second, the "natural experiment" approach used by Attanasio and Rohwedder (2003) and Attanasio and Brugiavini (2003) to analyze the effects of public pensions could be applied to study the effects of employer-provided pensions. However, it is not clear where such experiments can be found. Studies of the effects of pension plan changes by individual firms, such as Madrian and Shea (2001), can reveal how saving within pensions respond to such changes. But they are usually limited to data available from company records, and do not provide evidence on household wealth accumulation. Finding such experiments will be difficult, but may have a high payoff.

Appendix A: Calculating Defined Benefit Pension Benefits

Respondents who reported any pension coverage at wave 1, either on the current job or a previous job, were asked for permission to contact the employer to obtain information on the pension plan (this has been repeated in some subsequent survey years for new cohorts added to the HRS). For respondents who gave permission and whose employers provided the requested information, the formulas that determine the pension benefit for each plan were coded by the HRS and provided to researchers along with pension calculation software. These formulas determine the pension benefit for all possible scenarios involving birth date, age, job tenure at the time of exit, and salary history on the job. Rather than use pension calculation software (which is coded in Visual Basic) to directly compute benefits for each individual, I used an approximation approach. This was done so that the benefit calculations could be easily computed for many alternative age-tenure-salary scenarios. This approach is necessary for a related paper in which I am estimating a structural model using a program written in Fortran. Using the same approach in this paper will allow me to compare results more easily with the structural estimates. In the next draft of this paper, I will examine the sensitivity of the findings to the approximation approach.

The approximation approach uses the pension calculation software to compute benefits for each DB and combination DB/DC plan in which any respondent is enrolled at wave 1 (834 plans, some covering more than one HRS respondent), for about 5,000 hypothetical individuals, with alternative combinations of birth date, hire date, and real salary level and growth rate. For each simulated individual and each plan, I compute the monthly pension benefit and the age at which the individual is first eligible for the benefit for every possible age at which the individual could quit from the year after the hire date through age 75.

I then ran three regressions separately for each pension plan, using the 5,000 simulated observations for each plan. The dependent variable in the first regression is a binary indicator for whether the simulated individual will ever be eligible for a benefit, given the age at exit. The dependent variable in the second regression is the age at which the individual is first eligible for the benefit, conditional on ever being eligible. The dependent variable in the third regression is the monthly benefit, conditional on eligibility before age 75. Each regression is specified with a very flexible functional form, with dummies for age at exit, tenure at exit, and combinations of quit-age and tenure. For the benefit regression, the specification includes average salary in the most recent five years, the second most recent five years, and so forth, and interactions of the salary averages with age and tenure dummies. The coefficient estimates from these three regressions for each plan are stored, and used to predict benefits for the actual individuals covered by a given plan, as described below. These regressions are generally very accurate in predicting outcomes. I compared the predictions from the regressions to the "true value" computed directly from the pension calculator. For the "ever eligible" regression, using the rule that the prediction is zero if the fitted value is less than 0.5 and the prediction is one otherwise, the regression predicts every one of the approximately 5,000 observations correctly for 78% of the plans, and never predicts more than 13% incorrectly for any plan. Two thirds of the first-age-of-eligibility regressions predict the correct age exactly for every observation, and the 5th and 95th percentiles of the rounded residual distribution are 1 and -1 respectively. Finally, for the annual benefit regressions, the mean prediction error is -2.7 (in thousands of dollars per year), the median error is -0.6, the 75th percentile of the prediction error is 0.6, and the 25th percentile is

-8.5.

The next step is to use the regression coefficients to predict benefits for actual respondents under alternative scenarios about quit dates. In order to do this, I need salary information for the years in which the respondent works at the pension-providing job. The HRS asks respondents to report their salary at the beginning of the job, at each interview date, and at the time of exit from the job. This is generally insufficient information for computing the pension benefit. Another source of salary data is provided by the respondent's W2 record, which records earnings from each employer in each year from 1981-1991. I use this data source to fill in missing information on salary. In many cases, this still leaves an incomplete salary history, so I estimate a fixed effects earnings regression and use the regression coefficients to fill in missing salary data.

The salary data are then used with the stored regression pension regression coefficients to predict the benefit for each year in which an individual appears in the HRS and is still on the wave 1 job. For each of these eligible dates, I predict the benefit if the individual were to exit the job at that date and for all other possible dates up to age 75. In each case, I use the salary data up to the given period, together with salary forecasts for future periods from the fixed effects earnings model. Finally, I use life tables and an assumed real interest rate of 2.0% to compute the Expected Present Discounted Value (EPDV) of the pension benefit for each scenario. Comparing the benefits predicted from this approach with the actual benefit reported by the respondent given his actual quit date yields a mean prediction error of 3.0 and a median of 2.7.

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Table 1: Percent Distribution of Pension Plans on the Wave 1 Job

<table>
<tr><th colspan="5">A. All Individuals</th></tr>
<tr><td></td><td colspan="2">Men</td><td colspan="2">Women</td></tr>
<tr><td>No pension</td><td colspan="2">39</td><td colspan="2">42</td></tr>
<tr><td>Defined Benefit (DB) only</td><td colspan="2">24</td><td colspan="2">25</td></tr>
<tr><td>Defined Contribution (DC) only</td><td colspan="2">18</td><td colspan="2">19</td></tr>
<tr><td>Both</td><td colspan="2">19</td><td colspan="2">14</td></tr>
<tr><td>Sample size</td><td colspan="2">1,976</td><td colspan="2">1,974</td></tr>
<tr><th colspan="5">B. By Marital Status</th></tr>
<tr><th colspan="5">Unmarried individuals</th></tr>
<tr><td>No pension</td><td colspan="2">42.0</td><td colspan="2">39.7</td></tr>
<tr><td>Defined Benefit (DB) only</td><td colspan="2"></td><td colspan="2"></td></tr>
<tr><td>Defined Contribution (DC) only</td><td colspan="2"></td><td colspan="2"></td></tr>
<tr><td>Both</td><td colspan="2"></td><td colspan="2"></td></tr>
<tr><td>Sample size</td><td colspan="2">205</td><td colspan="2">582</td></tr>
<tr><th colspan="5">Married Couples</th></tr>
<tr><td></td><td colspan="4">Women</td></tr>
<tr><td>Men</td><td>No pension</td><td>DB only</td><td>DC only</td><td>Both</td></tr>
<tr><td>No pension</td><td>33</td><td>8</td><td>6</td><td>4</td></tr>
<tr><td>DB only</td><td>13</td><td>4</td><td>1</td><td>1</td></tr>
<tr><td>DC only</td><td>9</td><td>2</td><td>3</td><td>1</td></tr>
<tr><td>Both</td><td>9</td><td>2</td><td>2</td><td>3</td></tr>
<tr><td>Sample size</td><td colspan="4">2,194</td></tr>
</table>

Table 2: Selected Sample Characteristics by Type of Pension on Wave 1 Job

	No pension	DB only	DC only	DB & DC
Male	.42	.49	.49	.57
Married	.89	.80	.79	.80
Hours of work per week	37	41	41	43
Union member	.05	.43	.16	.32
Black	.13	.18	.11	.10
Years of Education	12.0	12.9	12.8	13.9
Social Security AIME*12	14.0	21.4	22.5	28.3
1991 Annual earnings	22	31	34	44
Average annual lifetime earnings	11.9	18.0	18.5	28.3
Bad health	.20	.11	.10	.07
Federal employee	.01	.05	.03	.08
State and local employee	.05	.31	.13	.20
Self-employed	.24	.04	.07	.02
Work experience (years)	18	24	23	26
Job tenure (years)	8	18	15	20
Health Insurance:				
Own employer	.30	.78	.71	.82
Spouse employer	.31	.17	.22	.15
Net worth [median]	271 [116]	190 [101]	272 [114]	250 [167]
Sample size	2,835	974	725	641

Notes: All figures are means unless otherwise noted. AIME = Average Indexed Monthly Earnings multiplied by 12. Average lifetime earnings are computed by dividing total lifetime earnings to date by the number of potential working years (age minus education minus six). Sample size is smaller for some variables. Monetary variables are in units of thousands of 1992 dollars.

Table 3: Descriptive Statistics for the Main Explanatory Variables

	Men	Women
DC Balance	71 (118) [995]	33 (55) [936]
DB wealth	209 (193) [798]	161 (163) [777]
Social Security (SS) wealth	184 (85) [6932]	
SS Wealth, married	214 (75) [5181]	
SS Wealth single	112 (33) [436]	89 (40) [1315]
DC contribution rate (worker plus firm)	.10 (.09) [701]	.09 (.06) [682]
Annual earnings	39 (47) [4763]	22 (22) [5328]
Average lifetime earnings	23 (13) [5148]	8 (7) [6022]

Monetary amounts are in thousands of 1992 dollars. The sample statistics are computed for the sample with non-zero values of a given variable. The figures shown are means, standard deviations (in parentheses) and sample size [in brackets] for the non-zero values. Average lifetime earnings are computed by dividing total lifetime earnings to date by the number of potential working years (age minus education minus six).

Table 4: Selected Coefficient Estimates from Median Regressions of Household Net Worth

Male variables	1	2	3	4
DC Balance (t)	.025 (.052)	.016 (.053)	.017 (.056)	.018 (.067)
DB Wealth		-.020 (.014)	-.022 (.011)	-.28 (.20)
DC Balance (t-2)	-.012 (.08)	-.006 (.08)	-.005 (.079)	-.011 (.09)
Earnings (t-1)	.46 (.13)	.50 (.17)	.50 (.14)	.52 (.15)
Average lifetime earnings	2.61 (.55)	1.63 (.34)	1.34 (.31)	1.33 (.33)
Contribution rate	17 (35)	31 (43)	34 (31)	32 (33)
Female Variables				
DC Balance (t)	.17 (.11)	.19 (.10)	.20 (.11)	.20 (.10)
DB Wealth		.028 (.016)	.029 (.016)	.56 (.25)
DC Balance (t-2)	.01 (.14)	-.003 (.12)	-.008 (.12)	-.01(.10)
Earnings (t-1)	.23 (.10)	.25 (.10)	.24 (.10)	.25 (.11)
Average lifetime earnings	.50 (.48)	.18 (.24)	-.04 (.22)	-.08 (.20)
Contribution rate	83 (31)	72 (34)	78 (30)	79 (28)
Household Variables				
Social Security Wealth		-.069 (.032)	-.011 (.017)	-.054 (.37)
Net Worth (t-2)	.76 (.04)	.77 (.04)	.77 (.04)	.77 (.04)

Notes: The column headings refer to the equation numbers in the text. Bootstrapped standard errors with 50 replications are in parentheses. There is no adjustment for multiple observations for a given household. Sample size is 8,200.

t refers to the current period, t-1 to the previous period, and t-2 to the twice-lagged period.

Other regressors include age, education, marital status, pension type dummies, the age of enrollment in the pension plan, AIME, number of children, year dummies, union status, race, Hispanic ethnicity, bad health and work limitation dummies, dummies for federal, state and local, and self employment, experience, tenure, the number of pensions on previous jobs, health insurance coverage dummies, the self-reported probabilities of living until age 75, and of receiving an inheritance, self-reported measures of risk aversion, financial planning horizon, cognitive ability, out of pocket medical expenditure in the year prior to the survey and in the year prior to the previous survey, annual earnings in the year prior to the previous survey, and dummies for current employment status for married individuals who do not have pension coverage. For married couples, the individual variables for both spouses are included. Missing value dummies for several of these variables are included as well, with missing cases set to zero.

Table 5: Selected Coefficient Estimates from Additional Models of Household Net Worth

	5	6	7	8
Household DC Balance	.061 (.040)	.062 (.040)		
Social Security Wealth	.02 (.35)			
Household DB Wealth	-.003 (.009)			
Household DB plus SS Wealth		-.003 (.009)		
Household DB plus SS Wealth plus DC Balance			.0007 (.012)	-.0055 (.019)
Net worth (t-2)	.77 (.04)	.77(.04)		
Household DC Balance (t-2)	-.02 (.06)	-.023 (.063)		
Net worth (t-2) plus Household DC Balance (t-2)			.75 (.04)	

Bootstrapped standard errors with 50 replications are in parentheses. See the text for descriptions of the specifications. See the notes to Table 4 for a list of additional regressors. The models were estimated by median regression.

Table 6: Selected Estimates of the Effects of Pension Wealth on Household Net Worth

Study	Estimation approach	Data type and country	Coefficient on Pension wealth	Coefficient on Social Security wealth	Coefficient on Pension plus SS wealth
Dependent Variable: Household Net Worth					
Alessie et al. (1997)	OLS	Longitudinal, Holland	0.28 (.06)	.003 (0.3)	
	Fixed effects		0.16 (.11)	-2.10 (0.81)	
Gale (1998)	Median regr.	cross section, U.S.			-.77 (.24)
Engelhardt and Kumar (2007)	OLS[a]	cross section, U.S.			.27 (.17)
	IV[a]				-.50 (.33)
	Median regr.				~.15*
	IV-median				~.10*
Gustman and Steinmeier (1999)	Median regr.	cross section, U.S.			.012 (.003)
Hubbard (1986)	OLS	cross section, U.S.	-.16**	-.33**	
Kapteyn et al. (2005)	Median regr.	Panel, Holland		-.115 (.020)	
Dependent Variable: Household Saving Rate					
Attanasio and Brugiavini (2003), Table 6 column 4, ages 46-55	IV	time series of cross sections, Italy		-.49 (.11)	
ages 56-60				-.21 (.14)	
ages 61-65				-.11 (.04)	
Attanasio and Rohwedder (2003) ages 55-64, SERPS	OLS	time series of cross sections, UK		-.75 (.24)	

[a]Table 2, column 5. * Standard error estimates are unavailable. See the text for discussion of the statistical significance of these estimates. ** Computed from coefficient estimates that are significantly different from zero.