# PENSIONS AND HOUSEHOLD WEALTH ACCUMULATION

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## Abstract

Economists have long suggested that higher pension benefits "crowd out" other sources of household wealth accumulation. Despite the important role public and private pensions play in policy debates in the United States, the empirical evidence on the extent of crowd-out is mixed. We exploit detailed information on pensions and lifetime earnings for older workers in the 1992 wave of the Health and Retirement Study (HRS) and employ a novel empirical strategy that combines two instrumental-variable approaches to identification. The instrumental-variable estimates suggest statistically significant crowd-out: each dollar of pension wealth is associated with a 45- to 60-cent decline in non-pension wealth at the mean. With somewhat less precision, we use an instrumental-variable quantile regression estimator and find evidence of substantial heterogeneity in crowd-out across the non-pension wealth distribution, with no effects at quantiles at or below the median, but crowd-out of 30-50 cents in the upper quantiles. Overall, our results suggest that policies that raise pension wealth also will raise household wealth. However, the impact will be far less for higher-wealth households, for whom crowd-out is the most important. In contrast, policies targeted to increase pension wealth for lower-wealth households will raise overall household wealth accumulation essentially dollar-for-dollar.

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## I. INTRODUCTION

Economists have long suggested that higher pension benefits "crowd out" other sources of household wealth accumulation. If so, then the ability of the government to raise overall household and national saving through pension and tax policies may be limited. Unfortunately, the empirical evidence on the extent of crowd-out in the United States is mixed. Since the seminal time-series studies of Feldstein (1974, 1996), a series of cross-sectional household studies, most notably Gale (1998), have suggested large offsets of 50 cents to 1 dollar of non-pension wealth with respect to each dollar of pension wealth.[1] In contrast, other studies suggested much smaller offsets of 0 to 33 cents.[2] This wide range of estimates likely reflects a variety of differences in empirical methodology across studies, including the time period, household survey, measurement of pension wealth and lifetime earnings, and, perhaps most importantly, the approach to econometric identification.

The fundamental identification problem in this literature is that the presence of unobserved heterogeneity in household saving behavior can bias crowd-out estimates. In particular, some households are "savers," some are not. Savers accumulate more wealth in all forms, including pensions, so that it is difficult to identify the impact of pensions on non-pension wealth separately from tastes for saving. The presence of such heterogeneity would bias upward standard Ordinary Least Squares (OLS) estimates of the pension offset, toward an estimated offset that is too small, perhaps suggesting little crowd out. In addition, there is growing awareness of the fact that many survey

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[1] For example, Munnell (1974, 1976), Feldstein and Pellechio (1979), Diamond and Hausman (1984), and, more recently, Khitatrakun, Kitamura and Scholz (2001).

[2] For example, Hubbard (1986), Cagan (1965), Katona (1965), Kotlikoff (1979), Blinder, Gordon, and Wise (1980), Leimer and Lesnoy (1982), Avery, Elliehausen, and Gustafson (1986), and, more recently, Gustman and Steinmeier (1999).

respondents are unaware of and unable to articulate many key attributes of their pension plans commonly used to measure pension wealth in survey data. This has led to concern that respondent-reported information in household surveys may yield inaccurate measures of pension wealth and impart bias to empirical studies of the effect of pensions on saving.[3]

In this paper, we exploit detailed administrative data on pensions and lifetime earnings for older workers in the 1992 wave of the Health and Retirement Study (HRS), a nationally representative random sample of 51-61 year olds and their spouses (regardless of age). We employ a novel empirical strategy, which marries two instrumental-variable (IV) approaches used in other contexts, to attempt to circumvent these difficulties and identify the extent to which pension wealth crowds out non-pension wealth. Throughout the analysis, we define pension wealth as the sum of the present value of future entitlements to employer-provided pensions and Social Security. First, we use employer-provided pension Summary Plan Descriptions (SPDs)—legal descriptions of pensions written in plain English—matched to HRS respondents, in conjunction with detailed pension and Social Security benefit calculators, to construct an instrument for self-reported pension wealth under the assumption that any error in SPD-based pension wealth is uncorrelated with measurement error in self-reported pension wealth. The basic idea is similar in spirit to that used in recent studies by Kane, Rouse, and Staiger (1999) and Berger, Black, and Scott (2000), who have estimated the return to schooling in the presence of measurement error when there are two measures of years of education, one self-reported and one administrative (such as transcript data). Second, to help insure that

[3] For example, Johnson, Sambamoorthi, and Crystal (2000), Gustman and Steinmeier (1999), Rohwedder, (2003a, 2003b), Engelhardt (2001), Starr-McCluer (1998), and Mitchell (1988).

the SPD-based instrument is uncorrelated with household-level heterogeneity, we construct the instrument using the simulated instrumental-variable approach of Cutler and Gruber (1996) and Currie and Gruber (1996) for a set of "synthetic" workers: individuals with the same pay, age, years of service, hire date, quit date, and survival probabilities for each plan. When this is done, the variation in our "synthetic" instrument primarily is due to cross-plan differences in generosity (not differences in earnings or household characteristics). Two excellent recent studies by Attanasio and Brugiavini (2003) and Attanasio and Rohwedder (2003) have formed instrumental variables by exploiting plausibly exogenous national policy changes to circumvent the identification concerns outlined above and estimate pension-saving offsets in Italy and the United Kingdom, respectively. Our paper is methodologically different than these studies and, to the best of our knowledge, is the first to use instrumental-variable techniques to attempt to identify the extent of pension crowd-out in the United States.

We employ four additional empirical innovations. First, to help circumvent difficulties with measuring lifetime earnings that have plagued many previous studies, we use administrative data for HRS respondents from two sources: W-2 earnings records for 1980-1991 provided by the Internal Revenue Service (IRS) and Social Security covered earnings records for 1951-1991 from the Social Security Administration (SSA).[4] Second, because the employer-provided SPDs in the HRS are available only for a non-random sub-sample of HRS respondents, we use the semi-parametric methods laid out in Newey (1999) and Das, Newey, and Vella (2003) to correct for potential sample selection using a set of plausible exclusion restrictions we derive from IRS Form 5500 administrative

[4] Gustman and Steinmeier (1999) and Khitatrakun, Kitamura and Scholz (2001) also use these data.

pension-plan filings.[5] Third, we exploit the richness of the HRS and in the estimation include additional sets of control variables not found in household surveys used in previous studies for a large set of factors that may affect household wealth accumulation, but also may be correlated with pension generosity. These include fringe benefits, plan characteristics, and an extensive set of employment characteristics. Finally, we use the Instrumental Variable Quantile Regression (IVQR) estimator of Chernozhukov and Hansen (2004, 2005) to examine crowd-out at different points in the non-pension wealth distribution.

We summarize our empirical findings with four important conclusions. First, based on a sample of 2,717 households from the HRS, the IV estimates suggest economically important and statistically significant crowd-out: each dollar of pension wealth is associated with 45-60 cents less in non-pension wealth. In contrast, the OLS estimates are biased upward (toward zero), so much so that they indicate that pension wealth *crowds in* non-pension wealth accumulation. Second, although our exclusion restrictions have substantial power in explaining who is included in our sample, there is no statistically significant evidence of sample selection bias from using the SPD data. Third, the use of richer sets of control variables not found in other studies—fringe benefits, plan characteristics, and an extensive set of employment characteristics—reduces measured crowd-out by about 15 cents per dollar of pension wealth, but weakens the precision of the estimates. Finally, although less precise than the IV results, the IVQR estimates suggest considerable differences in crowd-out at different points in the

[5] The IRS Form 5500 is the annual return that must be filed for each employee benefit plan. This report is required to be filed on a periodic basis with the U.S. Department of Labor by the 1974 Employee Retirement Security Act (ERISA) and includes basic plan organization, participation, benefit, and financial information.

non-pension wealth distribution: no crowd out or below the median, but crowd-out of 30-50 cents in the upper quantiles.

Overall, our results suggest that policies that raise pension wealth also will raise household wealth. However, the impact will be far less for higher-wealth households, for whom crowd-out is the most important. In contrast, policies targeted to increase pension wealth for lower-wealth households will raise overall household wealth accumulation essentially dollar-for-dollar for this group.

This study is organized as follows. In Section II, we discuss the econometric specification and measurement issues confronted in estimating crowd-out. In Section III, we describe the identification strategy. In section IV, we describe the sample construction. Our estimation results appear in Section V, and then we briefly conclude.

## II. ECONOMETRIC SPECIFICATION AND MEASUREMENT

To keep our analytical framework comparable to previous studies and to help motivate the linear-in-parameters econometric specification we estimate in the empirical analysis, we consider the following basic continuous-time life-cycle model formulated by Gale (1998).[6] The consumer lives from time 0, until known death in time $T$; retirement occurs at time $R$. Utility is derived from consumption, $C$, is based on the isoelastic form,

$$U(C_t) = \frac{C_t^{1-\rho}}{1-\rho}, \tag{1}$$

[6] For example, Khitatrakun, Kitamura and Scholz (2001), Attanasio and Brugiavini (2003), and Attanasio and Rohwedder (2003) all derive linear-in-parameters econometric specifications based on discrete-time models that have similar assumptions about consumer behavior and the economic environment.

exhibits constant relative risk aversion (CRRA), and $\rho$ is the coefficient of relative risk aversion. The consumer maximizes lifetime utility

$$\max_{C_t} \int_0^T \frac{C_t^{1-\rho}}{1-\rho} e^{-\delta t} dt \,, \tag{2}$$

subject to the intertemporal budget constraint

$$\int_R^T B_t e^{-rt} dt + \int_0^R E_t e^{-rt} dt = \int_0^T C_t e^{-rt} dt \,, \tag{3}$$

where $E$ is labor-market earnings, $B$ is real public and private pension benefits, $r$ is the real rate of return, and $\delta$ is the rate of time preference.

Because the empirical analysis focuses on the extent to which pension wealth crowds out non-pension wealth, it is convenient to note that non-pension wealth at any given age $A$ while working can be written as

$$W_A = \int_0^A (E_t - C_t) e^{r(A-t)} dt \,. \tag{4}$$

The left-hand side of (4) is the typical dependent variable in an econometric specification for crowd-out. To express the right-hand side in terms of the present value of future pension entitlements, the typical explanatory variable of interest in a crowd-out equation, note that the first-order conditions from (2)-(3) imply

$$C_t = C_0 e^{[(r-\delta)/\rho]t} \,. \tag{5}$$

Let $x \equiv [(r-\delta)/\rho] - r$, then (3) and (4) can be used to solve for $C_0$,

$$C_0 = \frac{x}{e^{xT} - 1} \left( \int_R^T B_t e^{-rt} dt + \int_0^R E_t e^{-rt} dt \right), \tag{6}$$

which can substituted back into (5) and then (4) to yield

$$W_A = \int_0^A E_t e^{r(A-t)} dt - Q\int_R^T B_t e^{r(A-t)} dt - Q\int_0^R E_t e^{r(A-t)} dt\,, \tag{7}$$

where when $x \neq 0$,

$$Q = \frac{e^{xA} - 1}{e^{xT} - 1}\,, \tag{8}$$

is Gale's $Q$, which takes into account the time the consumer has had since the introduction of the pension to adjust the lifetime consumption stream; when $x = 0$, $Q = A/T$. Because the last term on the right-hand side of (7) can be expressed as

$$Q\int_0^R E_t e^{r(A-t)} dt = Q\int_0^A E_t e^{r(A-t)} dt + Q\int_A^R E_t e^{r(A-t)} dt\,, \tag{9}$$

equation (7) simplifies to

$$W_A = (1-Q)\int_0^A E_t e^{r(A-t)} dt - Q\int_R^T B_t e^{r(A-t)} dt - Q\int_A^R E_t e^{r(A-t)} dt\,. \tag{10}$$

Equation (10) is a convenient representation of non-pension wealth at a point in time, and it is consistent with the following linear-in-parameters econometric specification:

$$W_i = \phi Y_i^{0A} + \beta P_i + \alpha \mathbf{x}_i + \gamma \kappa_i + u_i\,, \tag{11}$$

where $i$ indexes the household and $u$ is the disturbance term. The dependent variable, $W$, is non-pension household net worth in the HRS and is defined as the sum of cash, checking and saving accounts, certificates of deposit, IRAs, stocks, bonds, owner-occupied housing, business, other real estate, vehicle net equity, and other assets less other debts.

The first term on the right-hand side of (10) is $1-Q$ multiplied by the present value of household earnings to date. For shorthand, denote the present value of annual earnings to date for the *j*th adult in the *i*th household as $y_{ij}^{0A}$, but the household-level explanatory variable in (11), which is adjusted by $1-Q$, as $Y^{0A}$. Most previous studies have constructed proxies for earnings histories for households based on predicted values from reduced-form regressions of current earnings on demographics and employment characteristics (e.g., Hubbard, 1986) or simply have entered these characteristics directly into the specification to estimate a reduced-form wealth equation (e.g., Gale, 1998). However, a unique feature of the HRS is that it asked respondents' permission to link their survey responses to administrative earnings data from SSA and IRS that include Social Security covered-earnings histories from 1951-1991 and W-2 earnings records for jobs held from 1980-1991. This study uses these data to construct the present value of 1951-1991 earnings for each adult in the household, $y_{ij}^{0A}$. This is described in detail in the data appendix. In addition, the factor $Q$ is defined as

$$Q_{ij} \equiv (e^{x_{ij}S_{ij}} - 1)/(e^{x_{ij}T_{ij}} - 1), \tag{12}$$

where $x_{ij} \equiv [(r-\delta)/\rho_{ij}] - r$; the coefficient of relative risk aversion, $\rho$, is from Barksy, Juster, Kimball, and Shapiro (1998); $r$ is the SSA intermediate forecast for real interest long-term rates in 1992; $\delta$ is taken from Hurd (1989); $S$ is the number of years the individual has participated in the plan (Gale, 1998); and $T$ is the individual's expected lifespan based on age and subjective probabilities of living beyond 75 and 85, reported in the survey, respectively. Then the household-level explanatory variable, $Y^{0A}$, is made by aggregating the individual present values:

$$Y_i^{0A} \equiv \sum_j y_{ij}^{0A}(1 - Q_{ij}) \,. \tag{13}$$

The primary variable of interest in (10) is the second term on the right-hand side, Gale's *Q*-adjusted pension wealth. Denote the pension wealth, which is the present value of future entitlements (*B*), for the *j*th adult in the *i*th household as $p_{ij}$, but the household-level explanatory variable in (11), which is adjusted by $Q$, as $P$. Then $P$ is defined as

$$P_i \equiv \sum_j p_{ij} Q_{ij} \,, \tag{14}$$

where $p$ is defined as the sum of Social Security wealth based on the administrative covered-earnings histories described above and measured by Mitchell, Olson, and Steinmeier (1996), or, if there were no matched administrative earnings, calculated by Gustman, Mitchell, Samwick, and Steinmeier (1999), and self-reported private pension wealth measured by Venti and Wise (2001), which includes the account balance in defined contribution (DC) plans and the present value of entitlements to defined benefit (DB) plans on the current job, as well as pensions from past employment in current-payment status.

The last term on the right-hand side of (10) embodies the present value of future real earnings. We follow Gale (1998) and control for the impact of future earnings on wealth accumulation in (11) using a flexible, reduced-form approach, where $\mathbf{x}$ is an $m \times 1$ vector that contains an indicator variable for whether the head and spouse, respectively, expected real earnings growth in the future, $D^{\mathrm{Re}\,alWageGrowth}$, based on the following HRS question,

> "Over the next several years, do you expect your earnings, adjusted for inflation, to go up, stay about the same, or go down?"

This variable takes on a value of zero if the individual expected real earnings to stay the same, 1 if the individual expected real earnings to go up, and $-1$ if the individual expected real earnings to go down. In addition, $\mathbf{x}$ contains a quartic in the ages of the head and spouse, expected ages at retirement of the head and spouse, current earnings of the head and spouse, interactions of the age quartics with education and current earnings, the region of birth for the respondent and spouse, and a constant.

The vector $\kappa$ in (11) contains a standard set of demographic controls. These include dummy variables for the race (white), marital status (married, widowed, divorced), gender (female-headed household), any resident children, the number of resident children, and education of the head and spouse (high school, some college, college graduate), respectively.

## III. IDENTIFICATION STRATEGY

In the specification (11), $\beta < 0$ and is interpreted as the impact of an additional dollar of pension wealth on non-pension wealth, holding lifetime earnings and other factors constant. As described in the introduction, there are two important obstacles to identifying an estimate of $\beta$. First, if there is unobserved heterogeneity in saving behavior, then OLS estimates of $\beta$ will be biased and inconsistent. The bias will be upward, toward zero (because $\beta < 0$), which would imply estimated offsets that are too small. Second, whereas the primary advantage of respondent-reported pension wealth is that it can be thought of as reflecting what a household believes its pension(s) to be worth at the time of the survey, substantial measurement error can plague these data.[7] To the extent this is correlated with pension wealth, measurement error will render biased and inconsistent crowd-out estimates. If the error is classical (and because $\beta < 0$), the bias will be upward, toward zero, so that it could be that both the measurement error and the heterogeneity will generate an upward bias and reinforce each other (Bound et al., 2001).

We attempt to circumvent these problems by combining two instrumental-variable approaches to construct an instrument for self-reported pension wealth, $Z^P$, which must be correlated with observed self-reported pension wealth, but uncorrelated with the measurement error and household-level heterogeneity. First, the SPDs, which describe in detail all plan rules and features, including eligibility, employer contributions, benefit formulas, vesting, etc., are used to construct an instrument for self-reported employer-provided pension wealth under the assumption that any error in SPD-based pension wealth is uncorrelated with measurement error in the self-reported pension wealth. In particular, the plan rules laid out in the SPDs, individual data on sex, age, earnings histories, and years of service, and two pension-benefit calculators—the *HRS Pension Estimation Program* for defined benefit plans, described in Curtin, Lamkin, Peticolas, and Steinmeier (1998), and the *HRS DC/401(k) Calculator* for defined

[7] One reason for measurement error in self-reported pension wealth is that, during the course of the survey, respondents may report their pension plan type incorrectly; for instance, a worker who really has a defined benefit (DB) plan may report having a defined contribution (DC) plan (or vice versa); a respondent with a non-401(k) DC plan could report having a 401(k); someone with a DB and a 401(k) plan could report just one plan, etc. Another problem is that even if individuals correctly identify their plan type, they may report plan values inaccurately. This may be particularly true for DB participants, as these plans embody complicated formulas based on salary, age, years of service, early and normal retirement dates, about which the respondent may not be aware. Finally, the respondent-reported data may contain many missing values, which must be imputed by the researcher in order to arrive at pension wealth numbers. Such imputations can result in additional measurement error. Also, it should be noted again that pension wealth in this analysis is defined as the sum of employer-provided pension and Social Security wealth. Because Social Security wealth is measured off of SSA administrative earnings histories for most sample households, measurement error in this component of pension wealth is only a concern for those who did not give consent to match SSA administrative earnings and whose Social Security wealth was calculated by Gustman, Mitchell, Samwick, and Steinmeier (1999).

contribution plans, developed by Engelhardt, Cunningham, and Kumar (forthcoming)—are used to generate the present value of entitlements to employer-provided defined benefit and defined contribution plans on the current job, respectively, for each adult in each household in the sample. We use the Social Security calculator developed by Coile and Gruber (2000) to do a similar calculation for the present value of Social Security entitlements. The resulting employer-provided pension and Social Security wealth figures are summed to yield the instrument, $Z_i^P$.[8]

One obvious difficulty with this approach is that these entitlements are a function of individual pay, age, years of service, and survival probabilities—hence the subscript *i* in $Z_i^P$—all of which may be correlated with unobserved heterogeneity. Therefore, when the instrument is constructed, it is purged of household-specific variation, using the second approach, which is the simulated instrumental-variable methodology of Cutler and Gruber (1996) and Currie and Gruber (1996) for a set of "synthetic" workers: individuals with the same real annual pay, year of birth (1936), hire date (1971), quit date (2001), and survival probabilities (life-table values for the birth cohort 1936) for each plan, regardless of whether the plan was a defined benefit, defined contribution plan, or Social Security. To formulate the real annual pay for a synthetic worker, each individual in the sample was placed into a cell determined by his or her educational attainment, race, sex, age, and public-sector status, and the cell mean pay was used; this is described in

[8] In principle, the earnings histories, SPDs, and benefit calculators can be used to make estimates of pension wealth on the current job that could be used as the primary explanatory variable instead of the self-reported measure by Venti and Wise (2001); see, for example, Engelhardt, Cunningham, and Kumar (forthcoming). However, the Venti-Wise measure is preferred here because it includes the present value of pensions from past jobs in current-payment status and, hence, is more comprehensive. It is not possible to model these prior plans accurately using the SPDs because the HRS was only able to match SPDs for about one-third of reported pensions on past jobs. Because the Venti-Wise measure is the most comprehensive available, it is used as the primary explanatory variable in the analysis.

detail in the appendix. Importantly, this "synthetic" instrument is by construction uncorrelated with measurement error in the self-reported pension wealth variable and with household-level heterogeneity. The instrument now is denoted as $Z_{\bullet}^{P}$, where the subscript $\bullet$ indicates a synthetic measure. Let $\mathbf{X} = (Y^{0A}, \mathbf{x}, \kappa)$, then the identifying assumption is that $Cov(Z_{\bullet}^{P}, u \mid \mathbf{X}) = 0$.

Overall, there are three sources of identifying variation. First, there is variation across individuals in employer-provided pension coverage. Specifically, whereas almost all workers in the HRS are covered by either Social Security or a public-sector pension provided by the federal, state, or local government, some workers have additional pension coverage through private employer-provided pensions.[9] Second, there is variation across both private employer-provided and public-sector pension plans in generosity. We follow the previous literature and assume these sources of variation are exogenous.[10] Third, there is variation within plans across different types of synthetic workers. This occurs because some plans, in particular, Social Security and defined benefit plans, have benefit schedules that are non-linear in pay, and there are some defined benefit plans in the SPD database that are large enough to have multiple workers in the HRS sample.

[9] Workers need 40 quarters of covered employment to be eligible for benefits under Social Security. Federal employees hired before 1984, railroad workers, and some state and local public-sector workers are exempt from Social Security and covered under their own public-sector pension plans. Federal employees hired after 1983 are covered under Social Security. Public-sector workers who are covered under Social Security may also be eligible for benefits from public-sector pensions.

[10] Although the specter of endogenous sorting is always a potential criticism of empirical analyses in the pension literature, there is scant credible evidence in the existing literature that shows evidence in favor of sorting. Also, because there is only one "plan" associated with Social Security, the across-plan variation in generosity mentioned above comes from variation in generosity of employer-provided pensions, not from Social Security.

## IV. SAMPLE CONSTRUCTION AND DESCRIPTIVE STATISTICS

Our analysis sample consists of all households with an individual employed in the 1992 interview and includes households with individuals who were either in non-pension-covered employment or were pension-covered and had matched SPDs. This implies that all individuals in pension-covered employment for whom the HRS could not obtain a matched SPD for their pension plan were excluded.[11] We discuss below how we account for this potential non-random sample selection.

Table 1 shows the means for the primary variables used in the empirical analysis, with standard deviations in parentheses, and medians in square brackets. Column 1 presents the summary statistics for all individuals in the HRS, whereas column 2 shows the same for our analysis sample. Overall, the analysis sample consists of mostly white, married individuals in their mid-50s, with some college education and relatively few children at home. Only 48 percent of the sample was employed in a pension-covered job. The sample mean non-pension wealth was \$220,000, but the median was \$95,000, which illustrates the well-known fact that the distribution of wealth is right-skewed.

The sample mean of pension wealth, $P$, was \$200,000. The mean of the instrument, $Z^P$, was \$61,025. A comparison between those without and with employer-provided pension coverage in columns 3 and 4, respectively, indicates that individuals with such coverage had \$70,000 less in non-pension wealth on average than those without coverage. However, the pension-covered had higher non-pension wealth at the

[11] The HRS used the job rosters from the household interviews and attempted to collect SPDs from employers of HRS respondents for jobs in which the respondent was covered by a pension, but only successfully obtained SPDs for 1,717 plans that covered 4,503 individuals, or 65 percent of those employed in the first wave in a pension-covered job.

median and the difference in the median non-pension wealth between these two groups was $14,000.

## V. ESTIMATION RESULTS

Before discussing the results, we note that the sample is possibly non-random because it is based in part on individuals for whom the HRS was able to obtain an SPD. All of the specifications below correct for potential sample selection bias using standard Heckman two-step methods for the OLS and IV estimation summarized in Table 2 and Newey's (1999) two-step method for the ordinary and IV quantile estimation summarized in Figures 2 and 3 below.

Specifically, we use two exclusion restrictions developed in Engelhardt and Kumar (forthcoming), based on IRS Form 5500 data, to estimate a semi-parametric selection equation, along the lines of Das, Newey, and Vella (2003). The first exclusion is the incidence of pension-plan outsourcing by Census region, employment-size category, one-digit SIC code, and union status (union plan vs. non-union plan) cell in 1992, where outsourcing means the plan was administered by an entity other than the employer.[12] The intuition is that the HRS is less likely to obtain an SPD from the employer if (on average in its cell) plan administration is outsourced, because more than one contact is needed (first the employer, then the plan administrator) to receive the

[12] There is a restricted-access HRS dataset that provides industry and occupation information at a finer level of detail than the one-digit level. Unfortunately, the Memorandum of Understanding between the Social Security Administration and the University of Michigan concerning the use of restricted-access HRS data prevents the merging of any information based on the more detailed industry and occupation data to the Social-Security-covered-earnings and W-2 earnings files used in this analysis, so that it is not possible to construct these exclusions more finely.

SPD.[13] The second exclusion is the incidence of pension-plan consolidation due to mergers and acquisitions by cell from 1988-1992. Here, the intuition is that the HRS is less likely either to obtain an SPD from the employer or match it to the employee if (on average in its cell) there has been a lot of plan consolidation, because plan names and detail are often changed upon consolidation.[14] Overall, these exclusions have power in explaining who is included in the analysis sample: the null hypothesis that the exclusions jointly do not explain who is in the sample is rejected at the 5% level.

Panels A and C of Table 2 show the selection-corrected crowd-out estimates from the specification in (11) for the ordinary least squares (OLS) and instrumental-variable (IV) estimators, respectively.[15] Each cell in these panels represents a crowd-out estimate ($\hat{\beta}$) from a different regression. Panel B shows the parameter estimate associated with the instrument from the first-stage regression. Standard errors are shown in parentheses.[16]

The results for the baseline specification, in which the dependent variable is total non-pension net worth, are shown in column 1. The OLS estimate of the offset is 0.16 and indicates that an additional dollar of pension wealth *raises* non-pension net worth by 16 cents. Taken at face value, this suggests that pensions crowd in household saving. In contrast, the IV estimate flips sign, is -0.95, and statistically significantly different than zero. In panel D, the *F*-statistic from the first-stage regression is 16.24, so there is a

[13] It may well be that plans that are outsourced are better administered and therefore more likely to return the pension provider survey and SPD. However, this is likely more than offset because the SPD request is significantly less likely to get fulfilled with multiple entities to contact.
[14] The construction of the exclusions is discussed in detail in data appendix.
[15] The complete set of parameter estimates is available from the authors.
[16] All standard errors and confidence intervals presented in the analysis below were based on 99 bootstrapped replications. The selection equation was re-estimated for each bootstrap sample. We checked the sensitivity of the standard errors to the number of bootstrap replications and, for example, found that the standard errors were not appreciably different with 199 replications.

strong first-stage fit, and the instrument passes rule-of-thumb tests for weak instruments (Staiger and Stock, 1997). Based on the *p*-value shown in the table, there is no statistical evidence of selection bias. The IV estimate suggests that an additional dollar of pension wealth reduces household non-pension wealth 95 cents. That is, pensions crowd out saving essentially dollar-for-dollar. Furthermore, a comparison of the OLS and IV estimates suggests that the former are severely upward biased, as argued above.

*Robustness Checks*

Because the instrument is based on variation in coverage and across-plan generosity as measured in the SPDs, an important practical concern that falls outside of the scope of the simple theoretical framework is that firms that offer pensions—and, especially, relatively more generous pensions—might also have other characteristics that affect saving behavior independently from pensions. If these features reduce the need to save, failure to control for them would tend to bias the estimated pension crowd-out downward, away from zero and more negative, implying too much crowd-out.

Because there are well-known differences in pay, pension coverage, and generosity correlated with unions, firm size, and region, we begin in column 2 of Table 2 by adding dummy variables for these characteristics to the specification. The IV crowd-out estimate falls from 95 to 62 cents, but is still statistically significant, so that controlling for these factors has an important economic impact on the estimated crowd-out.

Next, we exploit that fact that the HRS has information on fringe benefits, an extensive set of employment characteristics, and plan characteristics from the SPDs—

which are either not available in other household surveys or not exploited by other researchers in existing crowd-out studies—that can be used to account for other potential influences. To this end, we add to the specification richer sets of explanatory variables that control for a variety of additional factors.[17]

In column 3, we begin by adding *fringe benefits*. These include dummy variables for whether the firm offered long-term disability and group term life insurance, respectively, as well as the number of health insurance plans, number of retiree health insurance plans, weeks paid vacation, and days of sick pay. The IV crowd-out estimate falls from 62 to 45 cents, indicating that the omission of fringe benefits did impart some bias to the crowd-out estimates.

In column 4, we add *employment characteristics*—dummy variables for both the worker and spouse for whether the firm offered a retirement seminar, discussed retirement with co-workers, whether responsible for the pay and promotion of others, the number of supervisees, and a full set of occupation dummies. In column 5, we add *other plan characteristics*—dummy variables for whether the firm offered a voluntary saving option like a 401(k), allowed borrowing against plan balances, hardship withdrawals, self-directed investment, had an after-tax saving option, and a voluntary contribution limit less than the federal limit, respectively. These facets of the plan may be correlated with the generosity of the plan and also independently affect non-pension wealth

[17] The impact of these additional controls on the independent variation in the instrument is illustrated in the last row of panel D, which shows the $R^2$ from the auxiliary regression of the instrument on all of the exogenous regressors in the specification. Because the residuals from this regression include that part of the instrument that is independent of the other controls, $1-R^2$ is a measure of the independent variation in the instrument.

accumulation decisions.[18] Although less precise, the results in columns 4 and 5 suggest crowd-out of 50-60 cents.[19]

One concern about this set of results is that one of the assumptions of the IV strategy was that there is no labor-market sorting of individuals to pension-covered jobs based on tastes for saving, a condition that might be most easily violated by the self-employed, who represent both the worker and the firm. To check the robustness of our findings, we follow the existing literature and limit the sample to those who are not self-employed. These results are shown in column 6 for the same specification as in column 5. In this sample, the marginal dollar of pension wealth crowds out non-pension net worth by 57 cents. These results are similar to those in the full sample and provide evidence against the argument that the crowd-out we measure is driven solely by the behavior of the self-employed.[20]

*Impact Across the Wealth Distribution*

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[18] The fringe benefit and employment characteristics are drawn from the respondent survey. The plan characteristics are drawn from the SPDs.

[19] We also added a set of dummy variables for the household's financial planning horizon to the model in column 5 based on the following HRS question, "In deciding how much of their (family) income to spend or save, people are likely to think about different financial planning periods. In planning your (family's) saving and spending, which of the time periods listed is most important to you?" where the possible answers were "next few months, next year, next few years, next 5-10 years, longer than 10 years." Controlling for planning horizon had no impact on measured crowd-out. Finally, as we noted above, there are three sources of identifying variation in the instrument: across-respondent variation in employer-provided pension coverage, across-plan variation in generosity conditional on coverage, and within-plan variation in generosity that is non-linear in pay. The two most important sources of variation are the first two, coverage and generosity. One potential concern with our identification strategy is that most (if not all) of the variation in the instrument might be coming from the extensive margin of coverage, and that little might be coming from the intensive margin of generosity (conditional on coverage). To examine this, we repeated the IV regressions in Table 2, but just using the dummy variable for coverage as the instrument. Those crowd-out estimates are of roughly the same magnitude (slightly larger), but much less precise, than those in Table 2, which suggests that the independent variation in generosity is an important source of identifying variation and that the results are not being driven solely by variation in coverage.

[20] We also explored whether the crowd-out was associated with housing equity and found no such effect. All of the crowd-out is associated with non-housing equity.

There are two key drawbacks to the crowd-out estimates thus far. First, they are based on mean-regression estimators, which are sensitive to outliers in the distribution of wealth. Second, the response of household wealth accumulation to pensions is summarized in a single number. There is no allowance for differential response of non-pension wealth to pension wealth across the wealth distribution (Bitler, Gelbach, and Hoynes, 2006).

To illustrate the potential for differences in response, Table 3 shows the sample means for various measures of wealth and asset ownership for each decile of the non-pension net worth distribution. The patterns in our sample accord with what is known broadly about wealth holdings (Browning and Lusardi, 1996; Hurst, Luoh, and Stafford, 1998). In particular, Americans in the lower part of the wealth distribution have very little wealth beyond pensions and owner-occupied housing. One implication of this is that we might expect a marginal increase in pension wealth to have a very small crowd-out effect in the lower part of the wealth distribution simply because these households have very few other assets to crowd out. Indeed, only in the upper portions of the wealth distribution is non-pension wealth likely large enough for substantial crowd-out to occur.

To examine crowd-out across the distribution more formally, we relax the assumption of a homogeneous response to increases in pension wealth by using a quantile regression approach, which also is robust to the influence of outliers. Specifically, we estimate the richest specification (from column 5, Table 2) using the ordinary quantile regression (OQR) estimator and the instrumental-variable quantile regression (IVQR) estimator of Chernozhukov and Hansen (2004, 2005) for every fifth quantile (from the $10^{th}$ through the $95^{th}$) of the distribution of non-pension net worth. In Figure 1, we illustrate the independent variation in the instrument across the non-pension wealth distribution. Namely, we plot for each decile of the non-pension wealth distribution the standard deviation of the residuals from the auxiliary regression of the instrument on all of the exogenous regressors in the specification. There is variation in the instrument at all parts of the distribution, but as one moves higher in the wealth distribution, the independent variation in the instrument rises.

The OQR and the IVQR estimates and 90% nonparametric bootstrapped confidence intervals are plotted in Figures 2 and 3, respectively.[21] Across quantiles, the OQR estimates of the pension crowd-out in Figure 2 are analogous to the OLS estimates in Table 2 in that the estimated offsets are positive at all points of the wealth distribution, indicating that pensions *crowd in* saving. Although less precise, the IVQR estimates in Figure 3 indicate, in contrast, considerable heterogeneity in crowd-out.[22] At lower wealth quantiles, the offsets actually are positive, and around the median are not statistically different than zero. These results are not inconsistent with our expectation of little crowd-out for lower-wealth households. Then, the IVQR crowd-out estimates turn negative after the 60$^{th}$ percentile and by the 75$^{th}$ percentile is 30 cents. From the 85$^{th}$-95$^{th}$ percentiles, the crowd-out is 40-50 cents.[23] The striking differences in the IVQR and

[21] All estimates in the figures are corrected for potential non-random sample selection bias using the two-step approach of Newey (1999). In particular, in the first-step we estimated the selection equation semi-parametrically, along the lines of Das, Newey, and Vella (2003), using the exclusions outlined in the text, and then included a quartic of the propensity score from the selection equation in the crowd-out equation estimated by OQR and IVQR, respectively. Both the OQR and IVQR results were robust to variations in the order of the selection-correction polynomial beyond a quartic. When bootstrapping the confidence intervals shown in Figures 2 and 3, the selection equation was re-estimated with each bootstrap replication.

[22] There is no statistically significant evidence of selection bias in the IVQR specifications; *p*-values not shown, but available upon request. When we re-estimate the IVQR specifications without selection correction, the crowd-out are qualitatively unchanged, but the estimates are more precise than those shown in Figure 3.

[23] IVQR estimates excluding the self-employed from the sample produced qualitatively similar, but somewhat less precise, offsets across the wealth distribution. In addition, the IVQR estimates for the other

OQR estimates underscore the importance of using instrumental variables and, as stressed by Bitler, Gelbach, and Hoynes (2006), allowing for differential response across the wealth distribution.

## VI. CONCLUSION

In this analysis, we exploited detailed information on pensions and lifetime earnings in the 1992 wave of the Health and Retirement Study (HRS) and employed a novel empirical strategy to identify the extent to which pensions crowd-out other forms of wealth. There are a number of important findings. First, the IV estimates suggest significant crowd-out: each dollar of pension wealth is associated with a 45- to 60-cent decline in non-pension wealth at the mean. Compared to what has been found relatively recently in the previous literature, these estimates are, broadly speaking, similar in magnitude to those by Gale (1998), who found that pensions crowd-out total net worth by 40-83 cents per dollar of pension wealth using median and robust regression estimators on a sample of households of all ages from the 1983 Survey of Consumer Finances, and Khitatrakun, Kitamura, and Scholz (2001) in the HRS.[24] Second, instrumenting matters a great deal, suggesting that unobserved heterogeneity, measurement error, or both, impart substantial bias. The OLS results suggest crowd-in, whereas the IV estimates flip sign and are precise enough to show substantial crowd-out of non-pension wealth. Similarly, the OQR results uniformly suggest crowd-in, but for the upper quantiles of the wealth

specifications in Table 2 gave similar patterns of response across the non-pension wealth distribution as that shown in Figure 3 and, consequently, are not shown here separately.

[24] Although this paper focuses on crowd-out in the United States, the results herein are, broadly speaking, also consistent with the two best recent papers in this area by Attanasio and Rohwedder (2003) and Attanasio and Brugiavini (2003), who found substantial substitutability between pensions and housing saving in the United Kingdom and Italy, respectively.

distribution, the IVQR estimates flip sign and indicate crowd-out.[25] Finally, there was substantial heterogeneity in the estimated crowd-out across the wealth distribution, with zero offsets at or below the median but offsets of 30-50 cents in the upper quantiles.[26]

Overall, our results suggest that policies that raise pension wealth also will raise household wealth and will improve retirement-income adequacy. However, the impact will be far less for higher-wealth households, for whom crowd-out is the most important. In contrast, policies targeted to increase pension wealth for lower-wealth households will raise overall household wealth accumulation essentially dollar-for-dollar.

There are a number of caveats to this analysis. First, the identifying assumptions underlying the instrumental-variable estimates were that across-plan variation in the generosity of pension benefits was exogenous and, more broadly—because the sample included both households with and without employer-provided pensions—that pension coverage was exogenous. Put differently, there was no differential sorting of households to jobs that offer (more generous) pensions based on tastes for saving, conditional on the large set of control variables included in the specifications, including demographics, lifetime earnings, union, firm size, region, fringe benefits, employment characteristics, and occupation. While this is a maintained assumption for all existing studies of the impact of pensions on saving, it has gone untested in a credible manner in the literature, and the results of the current analysis should be interpreted with that in mind. Second,

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[25] Gale (1988) found substantial offset at the median without instrumenting. How much this is due to using an SCF sample with a broader age range, and how much of this is due to the dramatic changes in the pension landscape from 1983, when the SCF data were gathered, to 1992, when the HRS data were gathered, are open questions (Gale and Milano, 1998).

[26] Gale (1998) also documented substantial heterogeneity in response, a theme that emerged in this analysis and many other studies as well, but using sample-splitting techniques that differ from the IVQR approach used here. The IVQR results in this paper are consistent with those of Chernozhukov and Hansen (2004), who found, at higher quantiles, an increasing degree of substitution of 401(k) for other assets using data from the Survey of Program Participation (SIPP).

this analysis says little directly about one of the most important recent trends in pension provision, the impact of automatic enrollment in 401(k) plans on household saving, because none of the 401(k) plans included in this study (circa 1992) had automatic enrollment.[27] Given the rapid adoption of automatic enrollment, assessing the impact of such default policies on wealth accumulation is a first-order question. To the extent that automatic enrollment increases participation among households in the lower part of the wealth distribution (Madrian and Shea, 2001), the results of this analysis would seem to suggest that increased saving through automatic enrollment would increase household wealth and not be undone by a reduction in non-pension wealth, but that is an open question. Finally, this analysis focused on older households from the HRS, and these results may not fully characterize the saving response of younger workers to changes in pension benefits.

[27] This was confirmed through an SPD search done by the HRS staff at our request.

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APPENDIX

*Construction of the Present Value of Earnings Measures*

A unique feature of the HRS that makes it substantially better suited for estimating the pension-saving offset than other datasets is that it asked respondents' permission to link their survey responses to administrative earnings data from SSA and IRS that include Social Security covered-earnings histories from 1951-1991 and W-2 earnings records for jobs held from 1980-1991. When combined with self-reported earnings histories, these data allow for the construction of the present value of earnings to date from 1951-1991.

We follow Cunningham and Engelhardt (2002) and Engelhardt, Cunningham, and Kumar (forthcoming) in using administrative earnings to construct career earnings, based on the parameter estimates from an annual earnings equation using all HRS individuals with matched Social Security earnings histories. The following model is estimated using a two-limit Tobit model to account for the censoring imposed from below by zero earnings from labor force non-participation and from above by the FICA cap on all person-year observations in the Social Security earnings database:

$$\ln(y_{it}) = \kappa_{1t} + \sum_{g=1}^{G} \kappa_{2gt} D_i^{OwnEduc\ g} + \kappa_{3t} Age_{\mathrm{it}} + \kappa_{4\mathrm{t}} Age_{it}^2 + \kappa_{5t} Age_{it}^3 + \kappa_{6t} Age_{it}^4 + \kappa_{7\mathrm{t}} D_i^{White} + \kappa_{8t} D_{it}^{GovtJob} + \boldsymbol{\theta}\mathbf{Z}_i + \eta_{it}$$

The dependent variable, ln($y$), is the natural log of real covered earnings (nominal covered-earnings from the database deflated into 1992 dollars by the all-items Consumer Price Index, or CPI). The earnings equation is estimated separately by sex and employs a flexible functional form that allows for (reading the terms on the right-hand side of the equation from right to left in order) calendar-year effects; time-varying returns to the respondent's education, measured by educational attainment group, $g$ (high school graduate, some college, college graduate, graduate degree); time-varying quartic age-earnings profiles; time-varying white-non-white earnings gaps; and time-varying returns to government jobs. In addition, the specification includes a vector of explanatory variables, $\mathbf{Z}$, which include a large set of time-invariant differences in earnings that are interpreted as part of the individual's human capital endowment: an indicator for whether U.S. born; sets of indicators for mother's and father's education, respectively, measured by educational attainment group (high school graduate, some college, college graduate, education not reported); own Census region of birth; and interactions of race, education, and region of birth.

To make the present value of earnings to date measure, actual earnings were used from the calendar year the respondent turned 20 through 1979, for those person-year observations with actual earnings below the FICA cap; for those observations with earnings above the FICA cap, the larger of the predicted value from the earnings equation and the cap was used. For 1980 through the year prior to the entry year, the actual uncapped earnings were taken from the W-2 database for all observations. For respondents who did not give consent, the predicted values from the estimation based on their socio-demographic characteristics were used to calculate an earnings growth rate from each single year of age, starting at 20, to the age in the survey entry year. Then using the respondent-reported annual earnings in the survey entry year, annual earnings were backcast with these growth rates. Real earnings from age 20 until the survey date were then expressed in present value terms in 1992.

*Construction of the Instrument*

The instrument was constructed by calculating the present value of employer-provided pension and Social Security benefits for a set of "synthetic" workers: individuals with the same real annual pay based on the predicted values from the earnings equation above, year of birth (1936), hire date (1971), quit date (2001), and survival probabilities (birth cohort 1936) for each private pension plan associated with a sample member, regardless of whether the plan was a defined benefit or defined contribution plan. This "synthetic" instrument is by construction uncorrelated with measurement error in the self-reported pension wealth variable and with unobserved heterogeneity. All of the variation in the instrument will be attributable to either variation across pension plans in generosity or across individuals in pension coverage, the latter of which, as in all of the previous literature, will be assumed exogenous. In addition, for a defined contribution plan that allows voluntary employee contributions (like in a 401(k) plan), the synthetic worker was assumed to have contributed 6 percent of pay per year since 1978, regardless of when the voluntary saving feature was actually adopted in the plan. This allows variation in the generosity of employer matching contributions to enter into the instrument and provide additional variation in plan generosity across plans.

*Construction of the Exclusion Restrictions*

To understand the exclusion restrictions that were developed, it is useful to note the manner in which the HRS obtained the SPDs. The HRS asked all respondents who reported being in a (current or past) pension-covered job to provide the name and address of the employer. To maintain respondent confidentiality, the HRS attempted to contact the employer, not about the respondent's pension(s), but more generally as part of a survey of pension providers in which the HRS requested copies of SPDs for the universe of pensions the employer provided (to all employees). The HRS then "matched" from this universe the appropriate pension(s) to the respondent based on the respondent's characteristics, e.g., union status, method of pay (hourly, salaried, commission, piece rate), occupation, tenure, etc. The "match" rates were well below 100 percent: 65 percent of those currently working in pension-covered jobs, 66 percent for the last job for those not working, and 35 percent for jobs held five years or longer prior to the current (last) job for those working (not working).

There are a number of important reasons for the failure to match an SPD to the respondent. First, the respondent may not have given correct employer name and address. Second, the HRS may have failed to receive the SPD because the employer may have refused to comply with the pension provider survey, the employer could not be located at the address given, or the employer went out of business or merged with another company and no longer existed under the name given by the respondent. Third, the employer may have submitted an SPD, but the HRS was unable to match the SPD to the respondent based on the plan detail and the respondent's characteristics. This is less likely for union and public sector workers, who are easy to identify and whose plans are easy to obtain, and more likely for workers whose employers had undergone mergers and acquisitions with subsequent plan modifications.

The exclusion restrictions were constructed as follows. First, Form 5500 data for 1988-1992 from the Department of Labor, Employee Benefit Security Administration, on the universe of pension plans with 100 or more participants and a 5 percent random sample of plans with less than 100 participants were obtained. Second, plans were divided into cells defined by Census region, employment size category, one-digit SIC code, year, and union status (union plan vs. non-union plan). The first exclusion is the incidence of pension plan outsourcing by cell in 1992, where outsourcing means the plan was administered by an entity other than the employer (weighted using sampling weights provided by DOL). The intuition here is that the HRS was less likely to have obtained an SPD from the employer if (on average in its cell) plan administration was outsourced, because more than one contact was needed (first the employer, then the plan administrator) to have received the SPD. (It may well have been that plans that were outsourced were better administered and, therefore, employers that outsourced were more likely to have returned the pension provider survey. However, this was likely more than offset because the SPD request was significantly less likely to have been fulfilled when multiple entities needed to be contacted.) The second exclusion was the incidence of pension plan consolidation due mergers and acquisitions by cell from 1988-1992. The intuition here is that the HRS was less likely either to have obtained an SPD from the employer or to have matched it to the employee if (on average in its cell) there had been a lot of plan consolidation, because plan names and detail were often changed upon consolidation.

Table 1: Sample Means for Selected Variables, Standard Deviations in Parentheses, Medians in Brackets

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Variable | Entire HRS | Analysis Sample | Not Covered by an Employer-Provided Pension | Covered by an Employer-Provided Pension |
| Total | 230,000 | 220,000 | 250,000 | 180,000 |
| Non-Pension | (500,000) | (490,000) | (530,000) | (450,000) |
| Net Worth | [98,300] | [95,000] | [86,735] | [100,000] |
| Non-Business | 190,000 | 180,000 | 190,000 | 170,000 |
| Net Worth | (370,000) | (380,000) | (370,000) | (390,000) |
| | [93,000] | [89,000] | [77,600] | [99,900] |
| Non-Housing | 170,000 | 160,000 | 200,000 | 120,000 |
| Net Worth | (470,000) | (470,000) | (490,000) | (430,000) |
| | [40,400] | [40,325] | [38,500] | [42,425] |
| Non-Business | 130,000 | 130,000 | 140,000 | 110,000 |
| Non-Housing | (330,000) | (350,000) | (330,000) | (360,000) |
| Net Worth | [36,500] | [36,600] | [30,250] | [41,175] |
| Q-Adjusted | 200,000 | 200,000 | 150,000 | 260,000 |
| Pension | (190,000) | (190,000) | (130,000) | (220,000) |
| Wealth | [170,000] | [160,000] | [130,000] | [210,000] |
| Head's Age | 55.6 | 56.15 | 56.51 | 55.75 |
| | (5.66) | (4.22) | (4.41) | (3.97) |
| | [56] | [56] | [56] | [55] |
| Spouse's Age | 44.8 | 36.34 | 36.1 | 36.6 |
| | (22.57) | (24.9) | (25.4) | (24.34) |
| | [54] | [50] | [50] | [50] |
| White | 0.8 | 0.81 | 0.8 | 0.82 |
| Female | 0.54 | 0.21 | 0.22 | 0.21 |
| Married | 0.78 | 0.69 | 0.68 | 0.7 |
| Widowed | 0.05 | 0.07 | 0.08 | 0.07 |
| Divorced | 0.11 | 0.19 | 0.2 | 0.18 |
| Head High School | 0.35 | 0.34 | 0.34 | 0.34 |
| Head Some College | 0.18 | 0.19 | 0.18 | 0.2 |

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Head College Graduate | 0.17 | 0.23 | 0.18 | 0.29 |
| Spouse High School | 0.29 | 0.28 | 0.27 | 0.29 |
| Spouse Some College | 0.15 | 0.14 | 0.14 | 0.15 |
| Spouse College Graduate | 0.14 | 0.12 | 0.1 | 0.14 |
| Any Resident Children | 0.44 | 0.44 | 0.43 | 0.45 |
| Number of Resident Children | 0.69 | 0.67 | 0.65 | 0.7 |
| | (.99) | (.94) | (.94) | (.95) |
| | [0] | [0] | [0] | [0] |
| In Current Pension-Covered Employment | 0.35 | 0.48 | 0 | 1 |
| Instrument | --- | 61,025 | 0 | 130,000 |
| | | (120,000) | (0) | (150,000) |
| | | [0] | [0] | [89,855] |

Note: Authors' calculations from the HRS data.

Table 2: Ordinary Least Squares (OLS), First-Stage, and Instrumental-Variable (IV) Estimates of the Extent to which Public and Private Pension Wealth Crowds Out Total Non-Pension Net Worth, Standard Errors in Parentheses

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| *A. OLS* | 0.16<br>(0.13) | 0.20<br>(0.14) | 0.28<br>(0.16) | 0.25<br>(0.16) | 0.27<br>(0.17) | 0.04<br>(0.05) |
| *B. First-stage coefficient on the instrument* | 0.17<br>(0.04) | 0.18<br>(0.04) | 0.19<br>(0.04) | 0.19<br>(0.04) | 0.18<br>(0.04) | 0.16<br>(0.04) |
| *C. IV* | -0.95<br>(0.19) | -0.62<br>(0.24) | -0.45<br>(0.24) | -0.59<br>(0.31) | -0.50<br>(0.33) | -0.57<br>(0.36) |
| *D. Sample and Diagnostics* | | | | | | |
| Sample | Full | Full | Full | Full | Full | Non-Self-Employed |
| *F*-statistic on the instrument from the first-stage | 16.24 | 17.74 | 16.24 | 20.45 | 19.77 | 13.37 |
| *p*-Value for Test of the Null of No Selection | 0.94 | 0.58 | 0.38 | 0.25 | 0.33 | 0.56 |
| *R*-squared from regression of instrument on other controls | 0.34 | 0.39 | 0.42 | 0.43 | 0.49 | 0.51 |
| *E. Additional Controls* | | | | | | |
| Union, Firm Size | No | Yes | Yes | Yes | Yes | Yes |
| Region | No | Yes | Yes | Yes | Yes | Yes |
| Fringe Benefits | No | No | Yes | Yes | Yes | Yes |
| Employment Characteristics | No | No | No | Yes | Yes | Yes |
| Plan Characteristics | No | No | No | No | Yes | Yes |

Note: Each cell of the table represents a crowd-out estimate from a different regression. Standard errors are shown in parentheses. All specifications include the present-value earnings measures described in the text and a baseline set of controls in for the race (white), marital status (married, widowed, divorced), gender (female-headed household), any resident children, the number of resident children, education (high school, some college, college graduate), as well as a quartic in age of the head and spouse, respectively. Interactions of the age-quartic with education and current-year earnings were also included.

Table 3: Sample Means for Selected Wealth and Ownership Measures by Decile of the Total Non-Pension Net Worth Distribution

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| Variable | Bottom Decile | 2nd Decile | 3rd Decile | 4th Decile | 5th Decile | 6th Decile | 7th Decile | 8th Decile | 9th Decile | Top Decile |
| Total Non-Pension Net Worth | -8,990 | 9,490 | 31,949 | 55,959 | 83,236 | 117,612 | 164,192 | 234,856 | 371,804 | 1,262,101 |
| Employer-Provided Pension Coverage (%) | 14 | 27 | 34 | 38 | 42 | 47 | 43 | 42 | 37 | 27 |
| Pension Wealth | 97,660 | 126,485 | 158,163 | 205,187 | 238,607 | 268,606 | 286,827 | 296,130 | 310,974 | 301,877 |
| Pension Wealth as a % of Total Wealth | 109 | 91 | 79 | 73 | 69 | 64 | 59 | 51 | 42 | 21 |
| Non-Housing Net Worth | -7,726 | 5,917 | 12,627 | 21,130 | 37,941 | 62,237 | 92,932 | 144,000 | 263,466 | 1,176,938 |
| Homeownership Rate | 15 | 45 | 82 | 90 | 95 | 95 | 96 | 97 | 97 | 97 |
| Housing Equity | -2,585 | 3,478 | 19,066 | 34,583 | 45,250 | 55,589 | 71,501 | 91,824 | 107,786 | 152,782 |
| Pension Wealth as % of Total Non-Housing Wealth | 108 | 94 | 91 | 87 | 84 | 77 | 72 | 63 | 51 | 24 |

Note: Authors' calculations from the full analysis sample.

Figure 1. Standard Deviation of the Instrument Conditional on the Other Explanatory Variables, by Decile of the Non-Pension Net Worth Distribution

Figure 2. Pension-Savings Offset With 90% Confidence Intervals
Based on Ordinary Quantile Regression

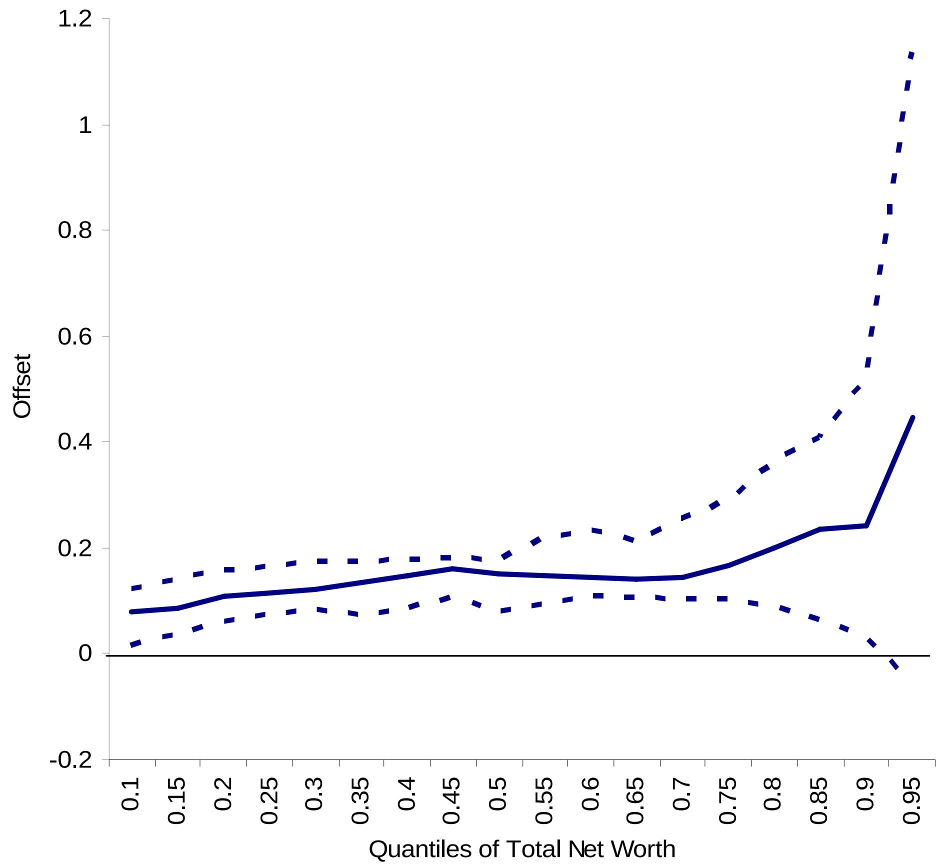

Figure 3. Pension-Savings Offset With 90% Confidence Intervals
Based on Instrumental-Variable Quantile Regression