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. This paper exploits detailed information on pensions in the 1992 wave of the Health and Retirement Study (HRS) and employs a novel empirical strategy that combines two instrumental-variable approaches to identification. The instrumental-variable estimates suggest a significant pension-saving offset: each dollar of pension wealth is associated with about 45- to 60-cent decline in non-pension wealth at the mean. There is evidence of substantial heterogeneity in the pension-saving offset across the wealth distribution, with zero or very small positive offsets at quantiles at or below the median, but significant offsets of 70 cents to dollar-for-dollar in the upper quantiles. Overall, about 80 percent of estimated crowd-out is associated with business equity. Pensions do not appear to crowd out housing equity.

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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 variation in estimates not only reflects the fact that the impact of liquidity constraints, bequest motives, uncertainty, distortionary taxation, among other factors well-established in the literature, make the theoretical extent of pension crowd-out ambiguous, but almost surely also 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

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[1] For example, Munnell (1974, 1976), Feldstein and Pellechio (1979), and Diamond and Hausman (1984).
[2] For example, Hubbard (1986), Cagan (1965), Katona (1965), Kotlikoff (1979), Blinder, Gordon, and Wise (1980), Avery, Elliehausen, and Gustafson (1986), Gustman and Steinmeier (1999), and Khitatrakun, Kitamura and Scholz (2001).

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 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 pension information in household surveys may give inaccurate measures of pension wealth and impart bias to empirical studies of the effect of pensions on household saving.[3]

This paper contributes to the crowd-out literature by exploiting detailed information on pensions and lifetime earnings in the 1992 Health and Retirement Study (HRS) and a novel empirical strategy, which marries two instrumental-variable approaches used in other contexts, to attempt to circumvent these difficulties and identify the extent to which pension wealth—defined as the sum of employer-provided pensions and Social Security throughout the analysis—crowds out non-pension wealth. First, pension Summary Plan Descriptions (SPDs)—legal descriptions of pensions written in plain English—are used 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

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

(such as transcript data). Second, to insure that the SPD-based instrument is uncorrelated with any household-specific fixed effect, it is constructed 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, so that this "synthetic" instrument is by construction uncorrelated with measurement error in the self-reported pension wealth variable and unobserved heterogeneity. When this is done, all of the variation in the instrument 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 policy changes to circumvent the identification issues outlined above and estimate pension-saving offsets in Italy and the United Kingdom, respectively. To the best of our knowledge, ours is the first paper to use instrumental-variable techniques to attempt to identify the extent of pension crowd-out in the United States.

Empirically, the paper makes four additional contributions. First, to help circumvent difficulties with measuring lifetime earnings that have plagued previous studies, administrative data from two sources are used: 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). Second, the instrumental-variable strategy uses the employer-provided SPDs in the HRS, which are available only for a non-random sub-sample of HRS respondents, and, unlike previous pension studies that have used the SPDs to measure pension incentives, the estimation in this paper uses 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 derived from Internal Revenue Service (IRS) Form 5500 administrative pension-plan filings. Third, to explore heterogeneity in crowd-out at different points in the wealth distribution, the paper gives estimates based on the Instrumental Variable Quantile Regression (IVQR) estimator of Chernozhukov and Hansen (2004, 2005). Finally, the analysis exploits the richness of the HRS and uses in the estimation 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 be correlated with pension generosity. These include fringe benefits, plan characteristics, an extensive set of employment characteristics, and measures of the household's planning horizon.

There are five primary findings. First, the instrumental-variable estimates using a sample of 2,717 households from the HRS suggest economically important and statistically significant crowd-out: each dollar of pension wealth is associated with a 45- to 60-cent decline in total non-pension wealth. A comparison of the OLS and IV estimates suggests that the failure to instrument results in crowd-out estimates that are biased upward, toward zero, so much so that many such specifications indicate that pensions *crowd in* saving. Second, the IVQR estimates indicate considerable heterogeneity in crowd-out at different points in the wealth distribution: zero offsets at or below the median, but offsets of 70 cents to dollar-for-dollar in the upper quantiles. Third, there is no statistically significant evidence of selection bias using the HRS data. Fourth, 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 10-30 cents per dollar of pension wealth and weakens the precision of the estimates. However, the inclusion of the household's planning horizon has no impact on the crowd-out estimates. Finally, in an analysis of particular components of total wealth, 80 percent of the estimated crowd-out is associated with business equity. Pensions do not appear to crowd out housing equity.

This study is organized as follows. Section II describes the analytic framework, section III discusses the econometric specification and measurement issues confronted in estimating crowd-out, and Section IV describes the identification strategy and sample selection. The estimation results are discussed in Section V. There is a brief conclusion.

II. ANALYTIC FRAMEWORK

To help set the backdrop for the empirical analysis, consider the following basic continuous-time life-cycle model formulated by Gale (1998). 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,

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

exhibits constant relative risk aversion (CRRA), and ρ 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 δ 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

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

is Gale's Q, which, when $x \neq 0$, 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

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

III. ECONOMETRIC SPECIFICATION AND MEASUREMENT

The empirical analysis uses remarkably detailed data from the 1992 wave of the HRS to estimate the relationship between household pension and non-pension wealth. The HRS is a nationally representative random sample of 51-61 year olds and their spouses (regardless of age) and collected detailed data on employment, wealth (including IRA, business, housing, and taxable assets), pensions, Social Security, demographics, and spousal characteristics. It has been used extensively to study saving and retirement behavior.

The econometric specification is based on (10):

$$W_i = \beta_1 Y_i^{0A} + \beta_2 P_i + \beta_3 Y_i^{AR} + \alpha \mathbf{x}_i + u_i\,, \tag{11}$$

where i indexes the household and u is the disturbance term. The dependent variable, W, is total non-pension household net worth 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, and 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 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 offset 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, ρ, is from Barksy, Juster, Kimball, and Shapiro (1998); r is the SSA intermediate forecast for real interest long-term rates in 1992; δ 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 third term on the right-hand side of (10) is Q multiplied by the present value of future household earnings from retirement back to the present. For shorthand, denote the present value of future earnings for the *j*th adult in the *i*th household as y_{ij}^{AR}, but the household-level explanatory variable in (11), which is adjusted by Q, as Y^{AR}. Following the methodology laid out in Engelhardt, Cunningham, and Kumar (2006), this study forecasts future earnings until the individual's expected retirement age using the parameter estimates from a detailed earnings equation, estimated on the administrative earnings panel just described, to construct the present value of future earnings for each adult in the household, y_{ij}^{AR}. This is described in detail in the data appendix. The household-level explanatory variable, Y^{AR}, is made by aggregating the individual present values:

$$Y_i^{AR} \equiv \sum_j y_{ij}^{AR}(Q_{ij}) . \tag{14}$$

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 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

$$P_i \equiv \sum_j p_{ij} Q_{ij} , \tag{15}$$

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. In (11), $\beta_2 < 0$ and is interpreted as the extent to which pension wealth crowds out non-pension wealth. The vector $\mathbf{x}$ is a set of additional control variables, discussed below.

IV. IDENTIFICATION STRATEGY AND SAMPLE SELECTION

As described in the introduction, there are two important obstacles to identifying an estimate of β_2. First, if there is unobserved heterogeneity in saving behavior such that some households are "savers" and others are not, and savers accumulate more wealth in all forms, including pensions, then, for example, OLS estimates of β_2 will be biased and inconsistent, and the bias will be upward, toward zero, 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.[4] One reason is that, during the course of the survey, respondents may

[4] 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

report their pension plan type incorrectly; for instance, a worker who really has a DB plan may report having a 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. To the extent this is correlated with pension wealth, measurement error will render biased and inconsistent crowd-out estimates. To the extent the measurement error in self-reported pension wealth is classical, and because $\beta_2 < 0$, 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.[5]

To circumvent these econometric difficulties, the empirical analysis exploits two instrumental-variable approaches used in other contexts 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 unobserved heterogeneity. First, the SPDs, which describe in detail all plan rules and features, including eligibility, employer contributions, benefit formulas, vesting, etc., are used to

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did not give consent to match SSA administrative earnings and whose Social Security wealth was calculated by Gustman, Mitchell, Samwick, and Steinmeier (1999).

[5] More complicated measurement-error processes could make the sign of the bias ambiguous (Bound et al., 2001).

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 the 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 estimated the return to schooling using instrumental variables when there are two measures of years of education, one self-reported and one administrative (transcript data). In this application, the plan rules laid out in the SPDs, individual data on sex, age, earnings histories, and years of service, and three pension-benefit calculators—the *HRS Pension Estimation Program* for defined benefit plans described in Curtin, Lamkin, Peticolas, and Steinmeier (1998), *HRS DC/401(k) Calculator* for defined contribution plans developed by Engelhardt, Cunningham, and Kumar (2006), and the Social Security calculator developed by Coile and Gruber (2000)—are used to generate the present value of entitlements to defined benefit and defined contribution plans on the current job and Social Security, respectively, for each adult in each household in the sample. These entitlements are summed to yield the instrument, Z_i^P.[6]

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 individual-specific variation, using the

[6] 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 (2006). 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, 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.

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 (birth cohort 1936) for each plan, regardless of whether the plan was a defined benefit or defined contribution plan. To formulate the real annual pay for a synthetic worker, each individual in the sample was placed into a cell determined by educational attainment, race, sex, age, and public-sector status, and the cell mean pay was used; this is described in detail in the data appendix. Importantly, this "synthetic" instrument is by construction uncorrelated with measurement error in the self-reported pension wealth variable and with unobserved heterogeneity.

Overall, there are three sources of identifying variation. First, there is variation across individuals in pension coverage. Second, there is variation across pension plans in generosity. As in all of the previous literature, these sources are assumed to be exogenous.[7] Third, there is variation within plans across different types of synthetic workers. This occurs because some plans, in particular, DBs, have benefit schedules that are non-linear in pay, and there are some plans in the SPD database that are large enough to have multiple workers in the HRS sample. Social Security also has a non-linear benefit schedule. The instrument is denoted as $Z_{\bullet}^{P}$, where the subscript $\bullet$ indicates a synthetic measure.

Sample Construction and Descriptive Statistics

[7] 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.

The 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.[8]

Table 1 shows the descriptive statistics for selected variables used in the empirical analysis. Column 1 presents the summary statistics for the full HRS sample of 12,321 households and Column 2 shows the sample mean for the primary variables in the analysis sample, with the standard deviation in parentheses, and the median in square brackets. Overall, the 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 $240,000. The mean of the instrument, Z^P, was $61,025. A comparison between those without and with pension coverage in columns 3 and 4, respectively, indicates that individuals with pension 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 median and the difference in the median non-pension wealth between these two groups was $14,000.

[8] 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.

Sample Selection

A final issue is that the sample is possibly non-random because it is based on individuals for whom the HRS was able to obtain an SPD. Following the methodology laid out in Engelhardt and Kumar (2006), two exclusion restrictions based on IRS Form 5500 data were used to estimate the model to correct for selection. 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.[9] 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 SPD.[10] The second exclusion is the incidence of pension-plan consolidation due to mergers and acquisitions by cell from 1988-1992. 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. The construction of the exclusions are discussed in detail in data appendix.

[9] 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.

[10] 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.

V. ESTIMATION RESULTS

Table 2 shows the crowd-out estimates from the specification in (11) for the ordinary least squares (OLS) and instrumental-variable (IV) estimators.[11] Each cell of the table represents a crowd-out estimate ($\hat{\beta}_2$) from a different regression. Standard errors are shown in parentheses. All specifications include the present-value earnings measures, Y^{0A} and Y^{AR}, described above and a baseline set of controls in $\mathbf{x}$ 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.

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 (row 1) of the offset is 0.16, statistically significantly different from zero, 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 (row 2) flips sign, is -0.95, and statistically significantly different than zero. The *F*-statistic from the first-stage regression is 16.24, so that there is a 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

[11] The complete set of parameter estimates is available from the authors.

dollar. Furthermore, a comparison of the OLS and IV estimates suggests that the OLS estimates are upward biased, as argued above.

Because there are well-known differences in pay, pension coverage, and generosity correlated with unions, firm size, and region, dummy variables for these characteristics are added to the specification in columns 2 and 3. The IV crowd-out estimate falls from 95 at baseline to 62 cents, but is still statistically significant, so that controlling for these factors has an important economic impact on the estimated crowd-out.

The remaining columns in the table give estimates using three alternative measures of non-pension wealth: one that excludes business equity, one that excludes housing equity, and one that excludes both business and housing equity. Whereas the exclusion of housing equity does not affect the IV estimates (column 5), the exclusion of business equity has a pronounced effect on the estimated offset, which falls from about 65 cents per dollar to 18 cents per dollar (column 4) and is no longer statistically significant. The estimated offset to non-business, non-housing net worth is 21 cents (column 6) and is marginally significantly different from zero in a one-tailed test.

Quantile Estimates

There are two key drawbacks of the estimates in Tables 2. First, they are based on mean-regression estimators. It is well known that these estimators can be sensitive to outliers in the distribution of wealth, for which, for example, the mean greatly exceeds the median. Second, the response of household wealth accumulation to pensions is summarized in a single number. There is no allowance for heterogeneity in response across households.

In Table 3, 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, each cell of the table shows the estimated pension crowd-out from a separate quantile regression, with the respective quantile given in the panel heading. Standard errors are in parentheses. For example, panel A shows quantile regression estimates of crowd-out ($\hat{\beta}_2$) for the 10th percentile (or quantile) of the household non-pension net worth distribution. The first row in that panel shows the estimate using the ordinary quantile regression (OQR) estimator; the second row shows the estimate using the instrumental-variable quantile regression (IVQR) estimator of Chernozhukov and Hansen (2004, 2005).[12] Panels *B-H* show results for other quantiles commonly examined in empirical work. The specification in each column of the table corresponds to the respective specification and column in Table 2, so that the quantile regression results can be readily compared to the mean regression results.

Across quantiles, the OQR estimates of the pension offset in Table 3 are analogous to the OLS estimates in Table 2 in that the estimated offsets are positive and significantly different from zero at almost all points of the wealth distribution, indicating that pensions *crowd in* saving. In contrast, the IVQR estimates in Table 3 indicate considerable heterogeneity in the estimated offset. At lower wealth quantiles, the offsets are not statistically different than zero, and even at the median the estimated offset is zero. The IVQR estimated offset for total net worth turns negative at the 60th percentile

[12] All estimates in Table 3 are corrected for potential non-random sample selection.

and by the 75th percentile is -0.5, or 50 cents per dollar, in column 3, and at the 90th percentile, the results are stronger and the estimates indicate dollar-for-dollar crowd-out. Like the mean-regression IV estimates, the IVQR estimates in columns 4-6 also suggest that about two-thirds of the estimated crowd-out in the upper quantiles is associated with business equity. Pensions do not appear to crowd out housing equity. There is no statistically significant evidence of selection bias in the IVQR specifications (*p*-values not shown, but available upon request). Overall, the striking differences in the IVQR and OQR estimates underscore the importance of using instrumental variables as well as letting the effect of pensions be heterogeneous across the wealth distribution.

Figures 1-3 summarize the estimates in Table 3 by plotting the IVQR point estimates of the pension-saving offset and the 95-percent confidence intervals obtained from the estimates presented in columns 3-5, respectively. In Figure 1, where total non-pension wealth is the dependent variable, the confidence intervals around the estimated offsets do not include zero for quantiles above the 60th percentile and both bounds of the interval are in the negative territory. The downward sloping middle line in each of the figures shows that the estimated offsets are increasing in quantiles of the wealth distribution. When we exclude business equity in Figure 2, the offsets become weaker and the 95-percent confidence interval now includes zero until the 75th percentile. In Figure 3, when the housing equity is excluded, the picture is little changed from Figure 1 as the offsets are significantly different from zero immediately following the median.

Further Robustness Checks

Because the instrument is based on variation in coverage and across-plan generosity as measured in the SPDs, an important practical concern with the estimation that falls outside of the scope of the simple theoretical framework is that firms that offer pensions—and, moreover, relatively more generous pensions—may also have other characteristics that might 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.

One of the great strengths of the HRS is that it 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 these potential other influences. To this end, Table 4 examines the robustness of the baseline mean-regression results in Table 2 by progressively adding to the vector $\mathbf{x}$ in the model richer sets of explanatory variables that control for a variety of additional factors. In column 1, *fringe benefits* are added. 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 for total net worth falls from -0.62 (column 3, Table 2) to -0.45, indicating that the omission of fringe benefits did impart some bias to the baseline crowd-out estimates in Table 2.

In columns 2 and 3 of Table 3, second and third sets of variables are added: *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; and *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 accumulation decisions. The results in columns 2 and 3 indicate that the inclusion of these additional variables does not materially affect the IV estimates of crowd-out.

Columns 4-6 of Table 4 show how crowd-out varies with the definition of wealth. Qualitatively, the same pattern as in Table 2 emerges: most of the pension offset occurs with respect to business equity; pensions do not appear to offset housing equity. Quantitatively, whereas the crowd-out estimates for measures of wealth including business equity remain statistically significantly different from zero, those excluding business equity become less precise and are no longer statistically different from zero. Column 7 adds measures of the head's and spouse's planning horizon. The inclusion of these has no impact on the crowd-out estimates.

Table 5 shows a parallel set of OQR and IVQR estimates for the richer specifications in columns 1-6 of Table 4. The results are similar to those in Table 3: there is no crowd-out at or below the median, but the estimates rise with higher quantiles of the distribution, peaking at 60 cents at the 95th percentile. Figures 4-6 summarize the estimates in columns 3-5 of Table 5 by plotting the IVQR point estimates of the pension-saving offset and the 95-percent confidence intervals, respectively.

VI. CONCLUSION

Because of increased concerns about retirement saving adequacy of older Americans, the effect of pensions on household saving has come under sharper focus among researchers and policymakers. This analysis exploited detailed information on pensions in the 1992 wave of the Health and Retirement Study (HRS) and employed a novel empirical strategy that combines two instrumental-variable approaches to identification.

There are a number of important findings. First, the instrumental-variable estimates suggest a significant pension-saving offset: each dollar of pension wealth is associated with about 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, but substantial larger than the crowd-out estimates of Gustman and Steinmeier (1999) and Khitatrakun, Kitamura, and Scholz (2001), who used the HRS and found little to no crowd-out. [13]

[13] Although this paper focuses on crowd-out in the United States, the results herein are, broadly speaking, 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.

Second, instrumenting seems to matter a great deal. Although the econometric specifications are not identical, the OLS and OQR crowd-out results here are similar to results in Gustman and Steinmeier (1999) and Khitatrakun, Kitamura, and Scholz (2001) using the HRS, showing little to no crowd-out, and, in some cases, crowd-in. In contrast, the IV estimates flip sign and are precise enough to show substantial crowd-out of non-pension wealth. Similarly, for the upper quantiles of the wealth distribution, the IVQR estimates flip sign and indicate crowd-out. Gale 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.

Third, there was substantial heterogeneity in the estimated crowd-out across the wealth distribution, with zero offsets at or below the median but significant offsets close to a dollar-for-dollar at the upper quantiles. 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 an increasing degree of substitution of 401(k) for other assets using data from the Survey of Program Participation (SIPP).

Overall, there are a number of implications for future research from this analysis. First, the analysis suggests that pensions provided to lower-wealth households do not result in a decrease in other household wealth accumulation. However, higher-wealth households do offset their pensions by reducing their non-pension saving. For the wealthiest households this offset approaches a dollar-for-dollar. Therefore, the aggregate impact of federal and employer pension policies on saving will depend on the composition of households affected. Second, the IVQR results showed substantial heterogeneity in response that was not apparent using just median regression. This suggests that moving away estimation techniques that yield a single summary measure of crowd-out is a fruitful avenue for future studies in this area (Bitler, Gelbach, and Hoynes, 2006). Finally, the bulk of the pension crowd-out is associated with business equity. This suggests a better understanding of the pathways to crowd-out requires a better understanding of the role of business equity in wealth accumulation (e.g., Hurst and Lusardi, 2004).

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 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, occupation, and planning horizon. While this is a maintained assumption for all existing studies of the impact of pensions on saving, this assumption 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, this analysis says little directly about 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.[14] 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. 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.

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

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DATA 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 the HRS 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, Cuuningham, and Kumar (2006) 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:

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

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. Earnings were forecast for years beginning with the entry year and future years up until the quit date, producing a real earnings history from age 20 until the quit date. 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. Last, earnings were forecast from the survey entry year to the quit date. This stream of forecast earnings was discounted into present value terms as of 1992 to make the present value of future earnings measure used in the specifications.

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, 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	Non-Pension Covered	Pension-Covered
Total Non-Pension Net Worth	230,000	220,000	250,000	180,000
	(500,000)	(490,000)	(530,000)	(450,000)
	[98,300]	[95,000]	[86,735]	[100,000]
Non-Business Net Worth	190,000	180,000	190,000	170,000
	(370,000)	(380,000)	(370,000)	(390,000)
	[93,000]	[89,000]	[77,600]	[99,900]
Non-Housing Net Worth	170,000	160,000	200,000	120,000
	(470,000)	(470,000)	(490,000)	(430,000)
	[40,400]	[40,325]	[38,500]	[42,425]
Non-Business Non-Housing Net Worth	130,000	130,000	140,000	110,000
	(330,000)	(350,000)	(330,000)	(360,000)
	[36,500]	[36,600]	[30,250]	[41,175]
Q-Adjusted Pension Wealth	200,000	200,000	150,000	260,000
	(190,000)	(190,000)	(130,000)	(220,000)
	[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
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: Author's calculations from the HRS data.

Table 2: Baseline Ordinary Least Squares (OLS) and Instrumental-Variable (IV) Estimates of the Pension-Saving Offset, with Selection Correction, Standard Errors in Parentheses

	(1)	(2)	(3)	(4)	(5)	(6)
Estimator	Total Net Worth	Total Net Worth	Total Net Worth	Non-Business Net Worth	Non-Housing Net Worth	Non-Business, Non-Housing Net Worth
A. Parameter Estimates						
OLS	0.16	0.20	0.20	0.23	0.15	0.18
	(0.06)	(0.06)	(0.06)	(0.04)	(0.05)	(0.04)
IV	-0.95	-0.65	-0.62	-0.18	-0.65	-0.21
	(0.18)	(0.19)	(0.19)	(0.14)	(0.18)	(0.13)
B. Diagnostics						
F-statistic on the instrument from the 1st stage	16.24	16.42	17.74	17.74	17.74	17.74
p-Value for Test of the Null of No Selection	0.94	0.57	0.58	0.40	0.60	0.33
C. Additional Controls						
Union, Firm Size	No	Yes	Yes	Yes	Yes	Yes
Region	No	No	Yes	Yes	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: Baseline Ordinary Quantile Regression (OQR) and Instrumental-Variable Quantile Regression (IVQR) Estimates of the Pension-Savings Offset, With Selection Correction, Standard Errors in Parentheses

Estimator	(1) Total Net Worth	(2) Total Net Worth	(3) Total Net Worth	(4) Non-Business Net Worth	(5) Non-Housing Net Worth	(6) Non-Business, Non-Housing Net Worth
		A. 10th	*Percentile*			
OQR	0.10	0.10	0.10	0.10	0.03	0.04
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
IVQR	0.15	0.15	0.20	0.20	0.05	0.05
	(0.07)	(0.08)	(0.06)	(0.05)	(0.08)	(0.06)
		B. 25th	*Percentile*			
OQR	0.15	0.14	0.14	0.14	0.06	0.06
	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
IVQR	0.20	0.20	0.15	0.20	0.05	0.10
	(0.08)	(0.08)	(0.10)	(0.07)	(0.10)	(0.07)
		C. 40th	*Percentile*			
OQR	0.15	0.16	0.15	0.17	0.08	0.09
	(0.03)	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)
IVQR	0.05	0.10	0.10	0.15	-0.05	0.05
	(0.10)	(0.12)	(0.12)	(0.09)	(0.11)	(0.08)
		D. 50th	*Percentile*			
OQR	0.18	0.17	0.17	0.18	0.08	0.11
	(0.03)	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)
IVQR	0	0	0.05	0.15	-0.10	0
	(0.11)	(0.13)	(0.12)	(0.10)	(0.12)	(0.09)

Table 3 (*Continued*)

	(1)	(2)	(3)	(4)	(5)	(6)
			E. 60th Percentile			
OQR	0.16	0.17	0.17	0.18	0.12	0.14
	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)
IVQR	-0.20	-0.05	-0.15	-0.10	-0.15	-0.05
	(0.13)	(0.14)	(0.15)	(0.11)	(0.12)	(0.09)
			F. 75th Percentile			
OQR	0.14	0.15	0.13	0.15	0.11	0.14
	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)
IVQR	-0.45	-0.50	-0.50	-0.20	-0.40	-0.25
	(0.16)	(0.19)	(0.19)	(0.16)	(0.15)	(0.13)
			G. 90th Percentile			
OQR	0.06	0.06	0.04	0.11	0.04	0.08
	(0.03)	(0.03)	(0.04)	(0.05)	(0.03)	(0.03)
IVQR	-0.95	-1.00	-0.95	-0.65	-0.80	-0.65
	(0.36)	(0.30)	(0.33)	(0.23)	(0.24)	(0.20)
			H. 95th Percentile			
OQR	-0.08	-0.02	-0.01	0.04	-0.06	0
	(0.05)	(0.05)	(0.16)	(0.17)	(0.05)	(0.04)
IVQR	-1.00	-1.00	-1.00	-0.40	-0.95	-0.35
	(0.66)	(0.44)	(0.44)	(0.20)	(0.41)	(0.17)

Table 3 (*Continued*)

	(1)	(2)	(3)	(4)	(5)	(6)
Additional Controls						
Union, Firm Size	No	Yes	Yes	Yes	Yes	Yes
Region	No	No	Yes	Yes	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 4: Additional Ordinary Least Squares (OLS) and Instrumental-Variable (IV) Estimates of the Pension-Saving Offset, Controlling for Fringe Benefits, 401(k) Plan, and Additional Employment Characteristics, with Selection Correction, Standard Errors in Parentheses

Estimator	(1) Total Net Worth	(2) Total Net Worth	(3) Total Net Worth	(4) Non-Business Net Worth	(5) Non-Housing Net Worth	(6) Non-Business, Non-Housing Net Worth	(7) Total Net Worth
A. Parameter Estimates							
OLS	0.28	0.27	0.27	0.27	0.22	0.22	0.26
	(0.06)	(0.06)	(0.06)	(0.05)	(0.06)	(0.04)	(0.06)
IV	-0.45	-0.51	-0.50	-0.05	-0.55	-0.10	-0.50
	(0.26)	(0.32)	(0.29)	(0.22)	(0.28)	(0.21)	(0.29)
B. Diagnostics							
F-statistic on the instrument from the 1st stage	16.24	16.42	17.74	17.74	17.74	17.74	20.96
p-Value for Test of the Null of No Selection	0.38	0.25	0.33	0.76	0.37	0.61	0.34
C. Additional Controls							
Union, Firm Size, Region	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fringe Benefits	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employment Characteristics	No	Yes	Yes	Yes	Yes	Yes	Yes
Plan Characteristics	No	No	Yes	Yes	Yes	Yes	Yes
Planning Horizon	No	No	No	No	No	No	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 5: Additional Quantile Regression (OQR) and Instrumental-Variable Quantile Regression (IVQR) Estimates of the Pension-Savings Offset, Controlling for Fringe Benefits, 401(k) Plan, and Additional Employment Characteristics With Selection Correction, Standard Errors in Parentheses

Estimator	(1) Total Net Worth	(2) Total Net Worth	(3) Total Net Worth	(4) Non-Business Net Worth	(5) Non-Housing Net Worth	(6) Non-Business, Non-Housing Net Worth
		A. 10th Percentile				
OQR	0.11	0.09	0.08	0.08	0.08	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
IVQR	0.25	0.20	0.20	0.20	0.05	0.05
	(0.10)	(0.09)	(0.08)	(0.06)	(0.11)	(0.09)
		B. 25th Percentile				
OQR	0.14	0.13	0.13	0.13	0.13	0.05
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)
IVQR	0.20	0.15	0.15	0.15	0.05	0.10
	(0.10)	(0.15)	(0.13)	(0.13)	(0.15)	(0.11)
		C. 40th Percentile				
OQR	0.17	0.15	0.15	0.16	0.16	0.08
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
IVQR	0.10	0.10	0.10	0.15	-0.05	0.00
	(0.16)	(0.18)	(0.17)	(0.11)	(0.17)	(0.13)
		D. 50th Percentile				
OQR	0.17	0.16	0.16	0.16	0.16	0.11
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
IVQR	0.15	0.05	0.10	0.15	-0.05	0.05
	(0.13)	(0.19)	(0.17)	(0.10)	(0.17)	(0.13)

Table 5 (*Continued*)

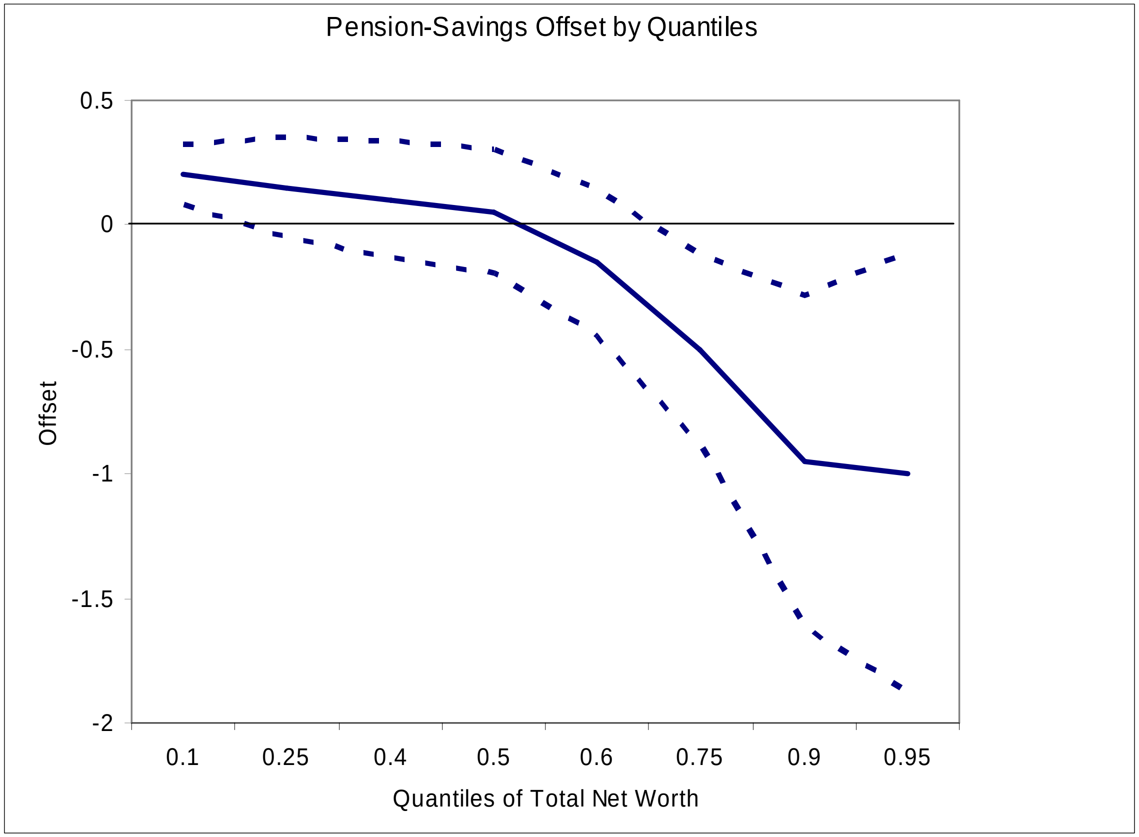
	(1)	(2)	(3)	(4)	(5)	(6)
		E. 60th Percentile				
OQR	0.19	0.15	0.16	0.18	0.18	0.12
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
IVQR	0.00	0.05	0.05	0.10	-0.05	-0.05
	(0.18)	(0.19)	(0.19)	(0.16)	(0.17)	(0.15)
		F. 75th Percentile				
OQR	0.18	0.19	0.21	0.20	0.20	0.15
	(0.05)	(0.07)	(0.07)	(0.07)	(0.07)	(0.04)
IVQR	-0.45	-0.25	-0.35	-0.10	-0.25	-0.20
	(0.26)	(0.24)	(0.25)	(0.22)	(0.20)	(0.19)
		G. 90th Percentile				
OQR	0.18	0.15	0.20	0.22	0.22	0.20
	(0.12)	(0.10)	(0.13)	(0.11)	(0.11)	(0.10)
IVQR	-0.50	-0.35	-0.45	-0.15	-0.55	-0.25
	(0.39)	(0.31)	(0.30)	(0.30)	(0.25)	(0.18)
		H. 95th Percentile				
OQR	0.04	0.33	0.44	0.34	0.34	0.34
	(0.19)	(0.34)	(0.38)	(0.21)	(0.21)	(0.19)
IVQR	-0.70	-0.45	-0.60	-0.20	-0.70	-0.20
	(0.47)	(0.27)	(0.33)	(0.35)	(0.33)	(0.22)

Table 5 (*Continued*)

	(1)	(2)	(3)	(4)	(5)	(6)
Additional Controls						
Union, Firm Size, Region	Yes	Yes	Yes	Yes	Yes	Yes
Fringe Benefits	Yes	Yes	Yes	Yes	Yes	Yes
Employment Characteristics	No	Yes	Yes	Yes	Yes	Yes
Plan Characteristics	No	No	Yes	Yes	Yes	Yes

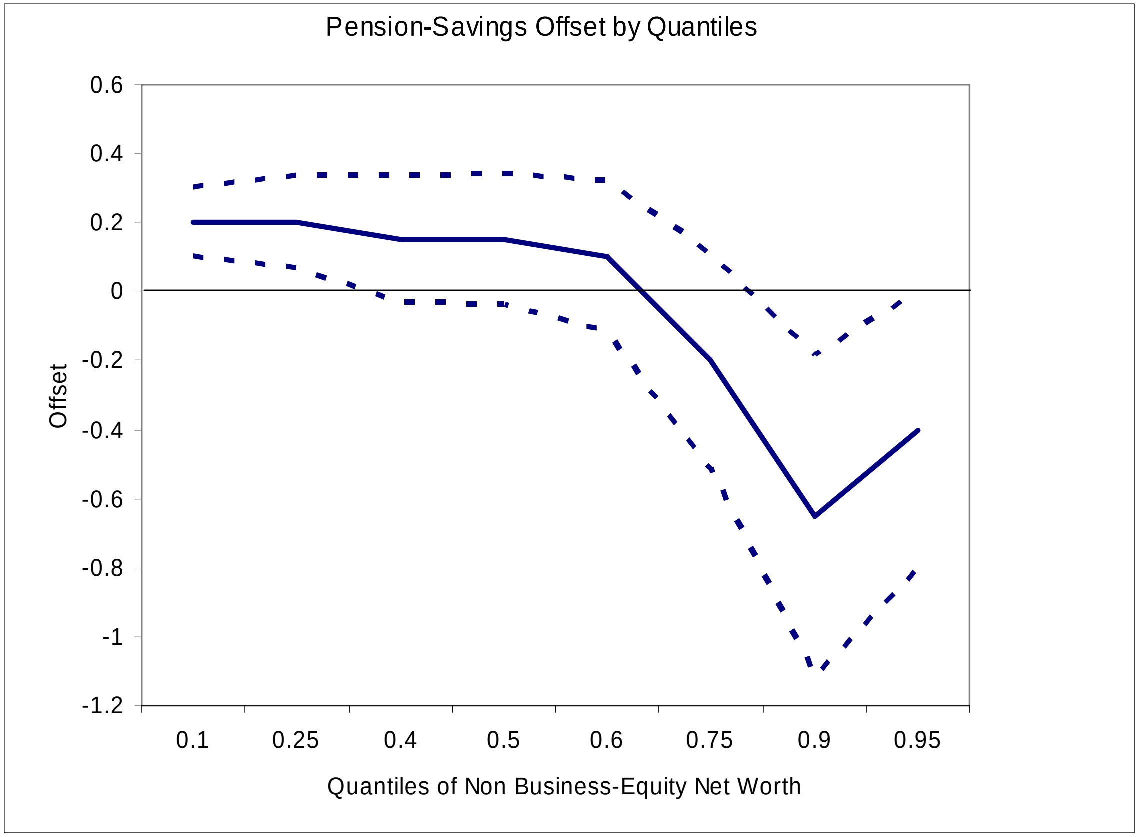
Note: Each cell of the table represents a crowd-out estimate from a different regression. Standard errors are shown in parentheses. All specifications include the present-value earnings measures described in the text and a baseline set of controls in for the race (white), marital status (married, widowed, 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.

Figure 1



Pension-Savings Offset by Quantiles
0.5
0
-0.5
-1
-1.5
-2
Offset
0.1
0.25
0.4
0.5
0.6
0.75
0.9
0.95
Quantiles of Total Net Worth

Figure 2


Pension-Savings Offset by Quantiles
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0.4
0.2
0
-0.2
-0.4
-0.6
-0.8
-1
-1.2
Offset
0.1
0.25
0.4
0.5
0.6
0.75
0.9
0.95
Quantiles of Non Business-Equity Net Worth

Figure 3

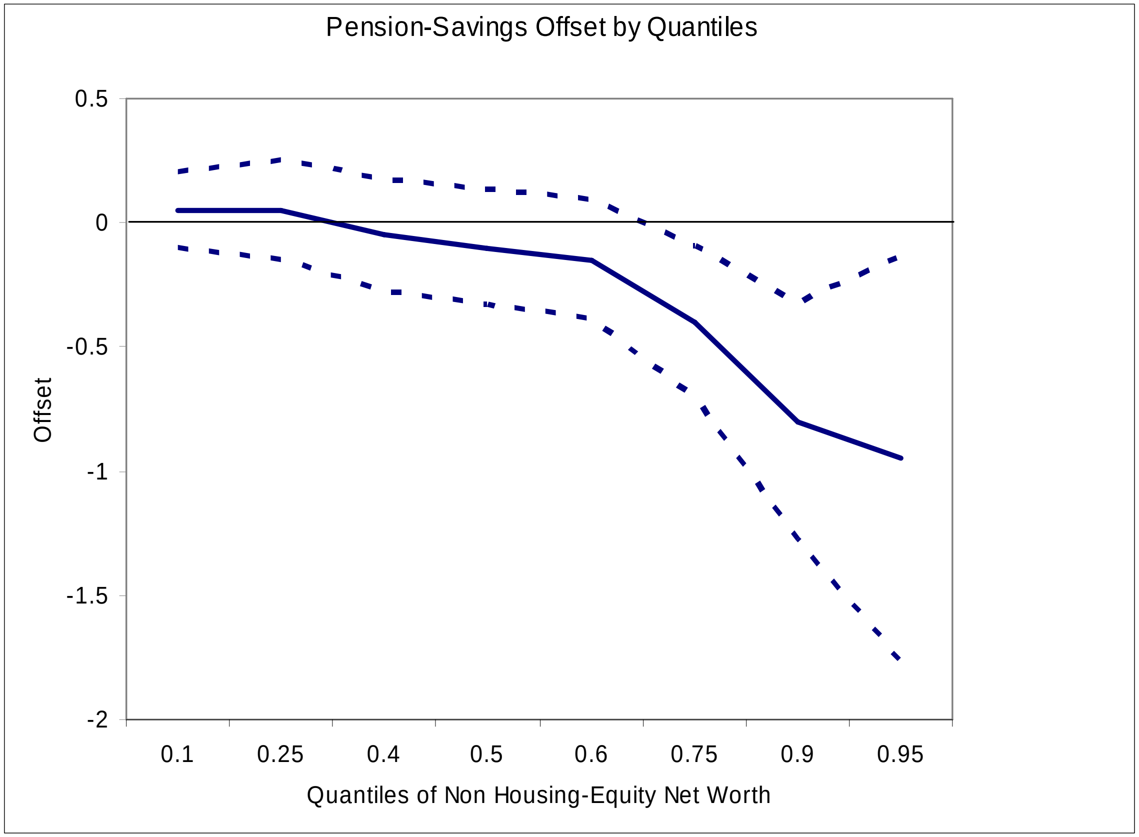
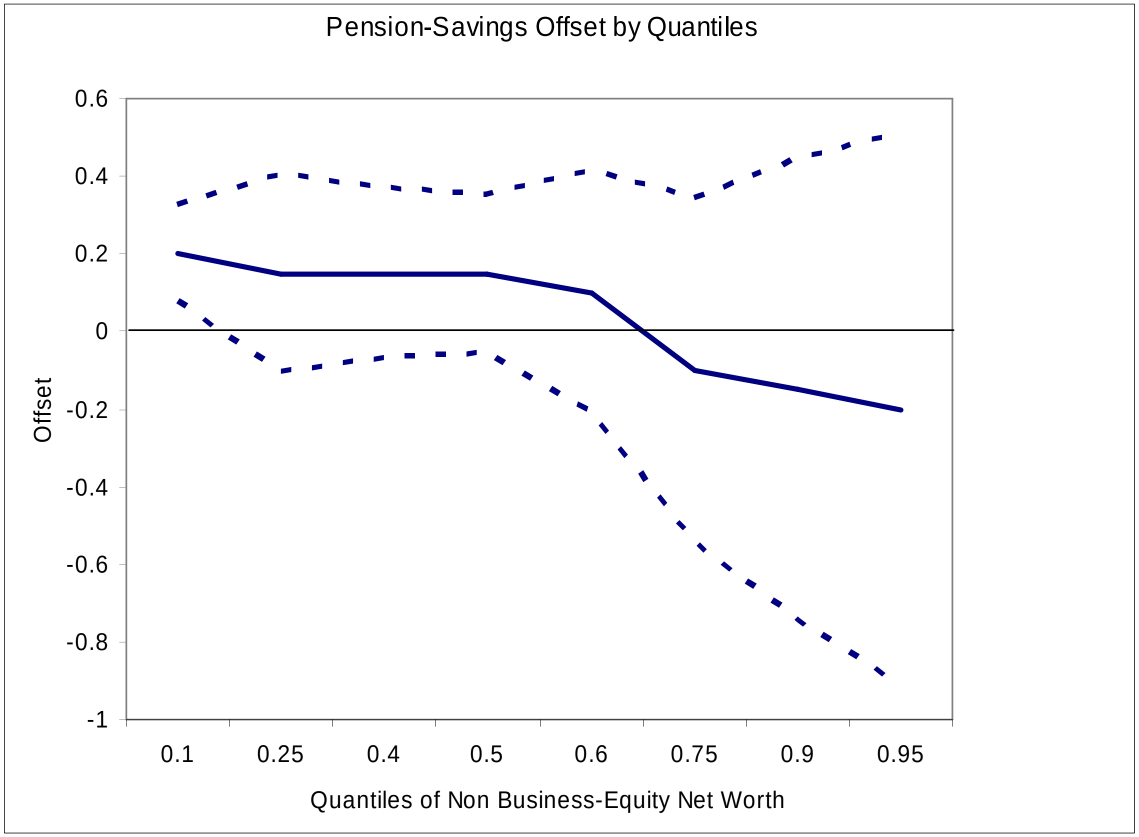

Pension-Savings Offset by Quantiles
0.5
0
-0.5
-1
-1.5
-2
Offset
0.1
0.25
0.4
0.5
0.6
0.75
0.9
0.95
Quantiles of Non Housing-Equity Net Worth

Figure 4

Pension-Savings Offset by Quantiles
0.6
0.4
0.2
0
-0.2
-0.4
-0.6
-0.8
-1
-1.2
-1.4
Offset
0.1
0.25
0.4
0.5
0.6
0.75
0.9
0.95
Quantiles of Total Net Worth

Figure 5


Pension-Savings Offset by Quantiles
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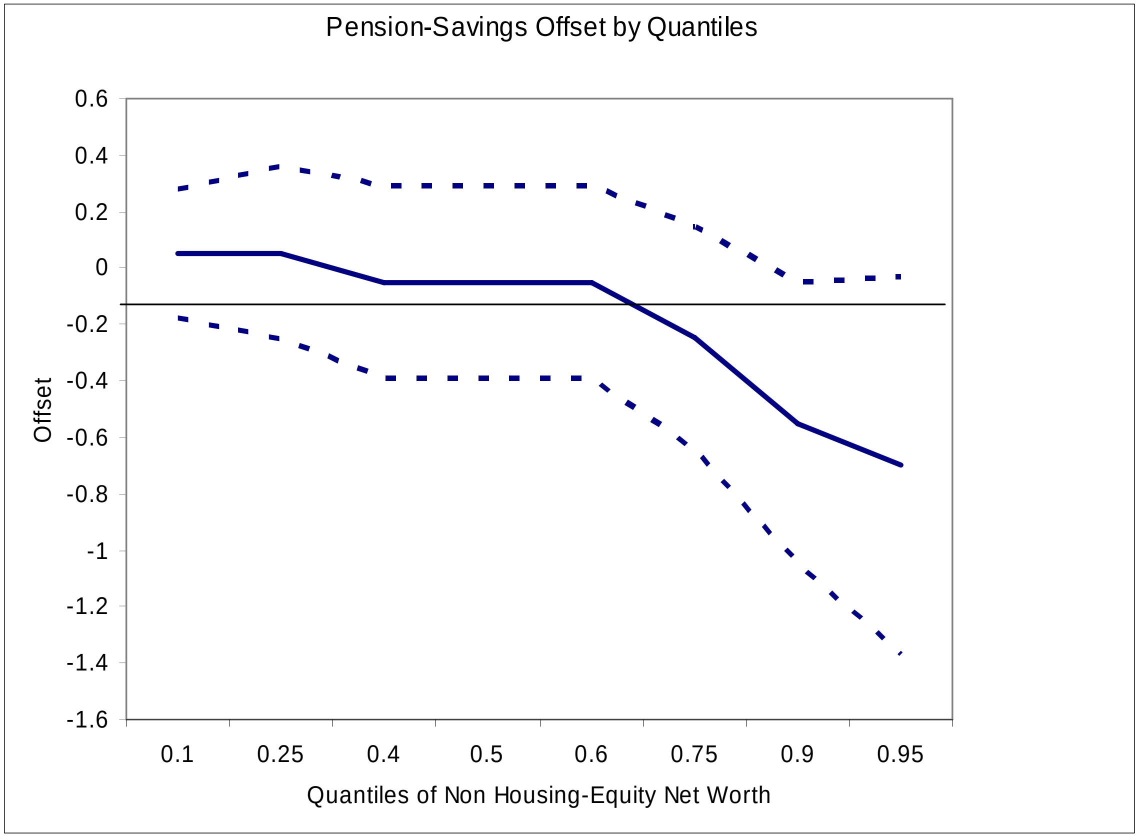
0.4
0.2
0
-0.2
-0.4
-0.6
-0.8
-1
Offset
0.1
0.25
0.4
0.5
0.6
0.75
0.9
0.95
Quantiles of Non Business-Equity Net Worth

Figure 6


Pension-Savings Offset by Quantiles
Offset
0.6
0.4
0.2
0
-0.2
-0.4
-0.6
-0.8
-1
-1.2
-1.4
-1.6
0.1
0.25
0.4
0.5
0.6
0.75
0.9
0.95
Quantiles of Non Housing-Equity Net Worth