Pensions and Household Wealth Accumulation

Gary V. Engelhardt
Anil Kumar

ABSTRACT

Economists have long suggested that higher private pension benefits "crowd out" other sources of household wealth accumulation. We exploit detailed information on pensions and lifetime earnings for older workers in the 1992 wave of the Health and Retirement Study and employ an instrumental-variable (IV) identification strategy to estimate crowd-out. The IV estimates suggest statistically significant crowd-out: each dollar of pension wealth is associated with a 53–67 cent decline in nonpension wealth. With less precision, we use an instrumental-variable quantile regression estimator and find that most of the effect is concentrated in the upper quantiles of the wealth distribution.

I. Introduction

Economists have long suggested that higher private 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. While there has been substantial interest in the extent to which targeted savings incentives, such as 401(k) plans and Individual

Gary V. Engelhardt is a professor in the Department of Economics and Center for Policy Research in the Maxwell School of Citizenship and Public Affairs of Syracuse University. Anil Kumar is a senior economist in the regional group of the Federal Reserve Bank of Dallas. *All research with the restricted-access data from the Health and Retirement Study was performed under agreement in the Center for Policy Research at Syracuse University and the Federal Reserve Bank of Dallas. The authors thank Rob Alessie, Dan Black, Raj Chetty, Chris Cunningham, Alan Gustman, Jeff Kubik, Annamaria Lusardi, Tim Smeeding, and seminar participants at NETSPAR, Rand Corporation, and Syracuse University for helpful discussions and comments. They are especially grateful to Bob Peticolas and Helena Stolyarova for their efforts in helping us understand the HRS employer-provided pension plan data. They thank the TIAA-CREF Institute for graciously supporting this research. The opinions and conclusions are solely those of the authors and should not be construed as representing the opinions or policy of any agency of the Federal Reserve, TIAA-CREF, or Syracuse University. The authors claim all errors as their own. The data used in this article can be obtained beginning from August 2011 through July 2014 from Gary V. Engelhardt, 423 Eggers Hall, Syracuse University, Syracuse, NY, 13244, gvengelh@maxwell.syr.edu.*
[Submitted April 2009; accepted December 2009]
ISSN 022-166X E-ISSN 1548-8004

Retirement Accounts (IRAs), raise retirement saving, there have been comparatively fewer studies of the extent of crowd-out across all private pension types in the United States, and the empirical evidence is mixed.[1] 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 one dollar of nonpension wealth with respect to each dollar of private pension wealth.[2] In contrast, other studies suggested much smaller offsets of 0–33 cents.[3] 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.

There are four problems that plague identification in this literature. First, pension wealth may be measured with error, which would impart bias to empirical studies of the effect of pensions on saving. Second, for many pension plans, demographics, employment characteristics, and career earnings map into benefits in a nonlinear manner. If these factors also independently affect nonpension wealth accumulation nonlinearly, then estimates of pension crowd-out will suffer from omitted-variable bias. Third, the presence of unobserved heterogeneity in household saving behavior can bias crowd-out estimates. In particular, some households may have a high taste for saving (patience). These households may accumulate more wealth in all forms, including pensions, so that it is difficult to identify the impact of pensions on nonpension 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. This may be compounded by the fact that these households may also have higher lifetime earnings—if patient individuals invest in human capital to a greater degree—which also are correlated with pension benefits, yet most household surveys do not measure career or lifetime earnings, a key explanatory variable in crowd-out specifications. Finally, workers may sort to jobs based on pension generosity, which is another dimension on which unobserved heterogeneity might bias crowd-out estimates.

In this paper, we depart from the recent literature focused on the specific effects of targeted subsidies to saving, such as 401(k)s and IRAs, and give new estimates of crowd-out across all pension types. Specifically, we exploit detailed administrative data on pensions and lifetime earnings for older workers in the 1992 wave of the Health and Retirement Study (HRS) and an instrumental-variable (IV) approach to attempt to circumvent these difficulties and identify the extent to which pension wealth crowds out nonpension wealth.

1. Studies on targeted saving incentives include Poterba, Venti, Wise (1995, 1996), Hubbard and Skinner (1996), Engen, Gale and Scholz (1996), Madrian and Shea (2001), Choi, Laibson, Madrian, and Metrick (2002, 2003, 2004), Choi, Laibson, and Madrian (2004), Duflo, Gale, Liebman, Orszag, and Saez (2006).
2. For example, Munnell (1974, 1976), Feldstein and Pellechio (1979), Diamond and Hausman (1984), and, more recently, Khitatrakun, Kitamura and Scholz (2001).
3. 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).

Our primary innovation is to 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-benefit calculators, to construct an instrumental variable 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 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). To help ensure that the SPD-based instrument is uncorrelated with household-level heterogeneity and omitted variables that are nonlinear functions of individual demographics, employment characteristics, and earnings, we construct the instrument using a fixed set of demographic and employment characteristics and sample mean earnings. When this is done, the variation in our instrument is due to cross-plan differences in generosity, not to worker characteristics that independently might determine nonpension wealth accumulation.

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 two additional empirical innovations. 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 1980–91 provided by the Internal Revenue Service (IRS) and Social Security covered earnings records for 1951–91 from the Social Security Administration (SSA). In addition, we use the Instrumental Variable Quantile Regression (IVQR) estimator of Chernozhukov and Hansen (2004, 2005) to examine crowd-out at different points in the nonpension wealth distribution.

We have three primary findings. First, our estimates of crowd-out suggest that each dollar of pension wealth is associated with 53–67 cents less in nonpension wealth. Second, the OLS estimates are biased upward, so much so that they indicate that pension wealth *crowds in* nonpension wealth accumulation by 23 cents. About one-half of the difference between the OLS and IV estimates can be attributed to bias from measurement error, with the other half due to nonlinearities and unmeasured heterogeneity. Finally, with less precision than the IV results, the IVQR estimates suggest considerable differences in crowd-out at different points in the nonpension wealth distribution: no crowd-out at or below the median, but crowd-out of 30–75 cents in the upper quantiles.

Overall, our results suggest that policies that raise private pension wealth also will raise household wealth. However, the impact will be 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.

This study is organized as follows. In Section II, we describe the HRS data on pensions and earnings. Section III discusses the regression specification, identification, and baseline estimates. Section IV presents extensions and robustness checks, and Section V presents additional results that suggest sorting is not confounding our IV estimates. The sixth section discusses the IVQR results. There is a brief conclusion.

II. Data Description

We use detailed data from the first wave of the HRS, a nationally representative random sample of 51–61 year olds and their spouses (regardless of age), which asked about wealth, income, demographics, and employment in 1992. Questions on employment were asked for the job (if any) held at the time of the interview, referred to hereafter as the "current job," as well as up to three previous jobs that lasted five years or longer. For each of these jobs, individuals were asked first if they were included in a pension, retirement, or tax-deferred savings plan. We consider those who answered "yes" to be in pension-covered employment on that job. In addition, we consider those who indicated that they were eligible for, but not participating in a plan, to be pension-covered.

For those included in a plan, the survey followed up with a question about plan type for each plan (up to three plans) for that job: formula-based or defined benefit (DB) plan; account-based or defined contribution (DC) plan; or a combination plan (with a mix of DB and DC features). Based on the answer, respondents were routed through different question sequences, one for DBs, one for DCs, and one for combination plans, with each sequence asking about features unique to that plan type, for example, plan balances, number of years included in the plan, the amount of the employer contribution, the amount of the employee contribution, etc., for DC plans, and early and normal retirement dates, number of years in the plan, expected benefits, etc., for DB plans. The respective responses allow for the calculation of pension wealth for each plan type that then can be summed across plans for each job to yield a pension-wealth measure for the job.

A unique feature of the HRS is that the study used the job rosters from the interviews and attempted to collect SPDs from employers of HRS respondents for all current and previous jobs in which the respondent was covered by a pension. We use these to form our instrument, detailed in the next section.

Specifically, the HRS asked all respondents who reported being in a (current or previous) 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) per se, 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, for example, union status, method of pay (hourly, salaried, commission, piece rate), occupation, tenure, etc. Ultimately, in the 1992 wave, 65 percent of those with pensions on the current job and 35 percent for pension-covered previous jobs held five years or longer have an associated SPD.

There are a number of important reasons for the failure to match an SPD to the respondent. The respondent may not have given the correct employer name and address. Alternatively, the HRS may have failed to receive the SPD because the employer may not have complied 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. Finally, 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. For these reasons, the subgroup of individuals in pension-covered employment with an associated SPD may be nonrandom, a point to which we return below.

A total of 5,607 households have an individual employed at the time of the interview. In the next section, we estimate crowd-out on the subsample of 2,728 households who either were working but not in a pension-covered current job, or were in a pension-covered current job and had a matched SPD. Therefore, the 2,879 households (that is, $2{,}879 = 5{,}607 - 2{,}728$) who are omitted are those who were in a pension-covered current job, but for whom the HRS failed to match an SPD. Hence, selection bias is a potential problem for our sample.

As an important contribution of this paper is the instrumental-variable strategy for the pension-wealth estimation based on the SPDs, and the highest SPD rate was for current jobs (65 percent), we focus on those with current jobs. Due to the age-based sampling frame of the HRS, respondents' current jobs are likely to be their career jobs, which generate the bulk of their lifetime private pension benefits (Gustman and Steinmeier 1999).

Finally, in addition to the extensive pension data, the HRS asked respondents' permission to link their survey responses to administrative earnings data from SSA and IRS. These administrative data include Social Security covered-earnings histories from 1951–91 and W-2 earnings records for jobs held from 1980–91, and were made available for use under a restricted-access confidential data agreement. These data are the basis for our measure of lifetime earnings.

III. Regression Specification, Identification, and Estimation Results

Let i index the household, then to determine the extent of crowd-out, we estimate the following econometric specification:

$$(1)\quad W_i = \beta P_i + \varphi Yi + \boldsymbol{\alpha}\mathbf{x}_i + \boldsymbol{\gamma}\boldsymbol{\kappa}_i + \rho\hat{\lambda}_i + u_i$$

in which the dependent variable, W, is nonpension household net worth, and P and Y are functions of pension wealth and lifetime earnings, respectively. The specification includes a vector of variables that proxy for future earnings, $\mathbf{x}$, plus a set of controls for demographic and employment characteristics, $\boldsymbol{\kappa}$, and the disturbance term, u.[4] The estimated selection-correction term, $\hat{\lambda}$, accounts for the possibility that

4. The appendix shows how this specification can be derived from a simple life-cycle framework.

those with a matched SPD are not a random sample of pension-covered workers. The primary objective is to obtain consistent estimates of β, the impact of an additional dollar of pension wealth on nonpension wealth, holding lifetime earnings and other factors constant.

A. Variable Definitions

Column 1 of Table 1 shows the means for the primary variables used in the empirical analysis, with standard deviations in parentheses, and medians in square brackets, for the analysis sample. Columns 2 and 3 show similar statistics for the two subsamples: those not in a pension-covered current job and those in a pension-covered current job with an associated SPD, respectively. Column 4 shows statistics for the omitted observations, described in the previous section—people who were in a pension-covered current job, but for whom there are no associated SPDs.

The outcome variable of interest is W, nonpension 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, vehicle net equity, and other assets less other debts, and includes imputed values based on HRS public-use imputations of missing asset and debt information taken from the HRS website. The sample mean nonpension wealth is $220,000. However, the median was $95,000, which illustrates the well-known fact that the distribution of wealth is right-skewed.

The variable Y is a function of the present value of lifetime earnings for the household. As already described, we use administrative earnings data from SSA and IRS that include Social Security covered-earnings histories from 1951–91 and W-2 earnings records for jobs held from 1980–91 to construct this measure. The details are described in the appendix.

The vector $\mathbf{x}$ accounts for factors that affect the present value of future earnings. It contains indicator variables for whether the head and spouse, respectively, expected real earnings growth in the future, 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 and one if the individual expected real earnings to go up. Similarly, we define a dummy variable for whether the head (spouse) expects real earnings to decline. 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 $\boldsymbol{\kappa}$ in Equation 1 contains controls for demographic and employment characteristics. The controls for demographics 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. The employment controls are dummy variables for union status, firm-size category, and Census region.

The focal explanatory variable is P, which is a function of pension wealth, and is defined as the sum of two components. Its basis is self-reported private pension wealth calculated by Venti and Wise (2001). Because some private pensions are structured so that their benefits are integrated with Social Security benefits, we also include Social Security wealth, as constructed by Mitchell, Olson, and Steinmeier (1996) and Gustman, Mitchell, Samwick, and Steinmeier (1999), in our measure of P, so that hereafter "self-reported pension wealth" refers to the sum of public and private pension wealth. P is constructed to take into account the time the household has had since the introduction of each pension plan to adjust the lifetime consumption stream using Gale's Q (Gale 1998). This is detailed in the appendix as well.

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 57 percent of the sample was employed in a current pension-covered job in 1992.

B. OLS Estimates

In Figure 1, we collapse the analysis sample into age $\times$ education $\times$ race $\times$ marital status cells and plot nonpension wealth versus pension wealth, illustrating the basic (noninstrumented) relationship. Contrary to theory, the relationship is strongly positive, suggesting that pension wealth *crowds in* private saving.

Column 1 of Table 2 shows the OLS crowd-out estimate, $\hat{\beta}$, in (1), where $\hat{\lambda}$ is the estimated inverse Mills' ratio from a Heckman selection correction, with standard errors in parentheses. We use two exclusion restrictions developed in Engelhardt and Kumar (2007) in the selection equation. The first is derived from IRS Form 5500 data and is the incidence of pension-plan outsourcing by Census region, employment-size category, one-digit SIC code, and union status (union plan vs. nonunion plan) cell in 1992, where outsourcing means the plan was administered by an entity other than the employer. 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.[5] The second is a dummy variable based on the interviewer's perception of the respondent's cooperation during the interview that takes on a value of one for individuals with excellent cooperation, who would be more likely to give the correct name and address of the employer used in the SPD matching process, and zero otherwise. All standard errors and confidence intervals presented in the analysis below were based on 331 bootstrapped replications, which was the optimal number of replications for this sample based on the method in Andrews and Buchinsky (2000). The selection equation was re-estimated for each bootstrap sample.

The OLS crowd-out estimate, $\hat{\beta}$, in Column 1 is 0.23, with a standard error of 0.15, and indicates that an additional dollar of pension wealth *raises* nonpension net

5. 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.

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

	(1) Analysis Sample	(2) Subsamples of the Analysis Sample	(3)	(4) Omitted
Variable	Not Pension-Covered Plus Those with Matched SPDs	Not Pension-Covered	Pension-Covered with a Matched SPD	Pension-Covered without a Matched SPD
Nonpension wealth	219,945	253,440	183,044	239,958
	(494,145)	(527,916)	(451,375)	(562,348)
	[95,000]	[86,735]	[102,000]	[94,724]
Pension coverage on the current job	0.48	0	1	1
Pension coverage on previous job	0.35	0.37	0.34	0.34
Private pension wealth	75,407	0	129,480	66,176
	(161,937)		(193,598)	(157,952)
	[11,412]		[65,921]	[9,813]
Social security wealth	123,417	119,206	128,055	123,269
	(62,141)	(61,791)	(62,219)	(61,857)
	[122,667]	[117,829]	[133,114]	[123,654]

Head's Age	56.2	56.5	55.8	56.1
	(4.2)	(4.4)	(4.0)	(4.2)
	[56.0]	[56.0]	[55.0]	[56.0]
White	0.81	0.80	0.82	0.82
Female	0.21	0.22	0.21	0.21
Married	0.69	0.68	0.70	0.70
Widowed	0.07	0.08	0.07	0.07
Divorced	0.19	0.20	0.18	0.18
Head high school	0.34	0.34	0.34	0.34
Head some college	0.19	0.18	0.20	0.18
Head college graduate	0.23	0.18	0.29	0.21
Any resident children	0.44	0.43	0.45	0.45
Number of resident children	0.67	0.65	0.70	0.69
	(0.94)	(0.94)	(0.94)	(0.96)
	[0]	[0]	[0]	[0]
Present value of lifetime earnings	464,794	338,117	604,353	496,927
	(505,762)	(432,808)	(542,448)	(560,359)
	[332,370]	[209,423]	[476,700]	[343,187]
Sample size	2,728	1,298	1,430	2,879

Notes: Authors' calculations from the HRS data. Columns 2 and 3 show descriptive statistics for the two subsamples of the analysis sample. Column 4 shows statistics for those who were omitted from the analysis sample because of the failure of the HRS to match an SPD. Private pension wealth on the current job, social security wealth, and the present value of lifetime earnings are all Q-adjusted, based on Gale (1998) as described in the appendix. Column 1 shows descriptive statistics for the analysis sample.

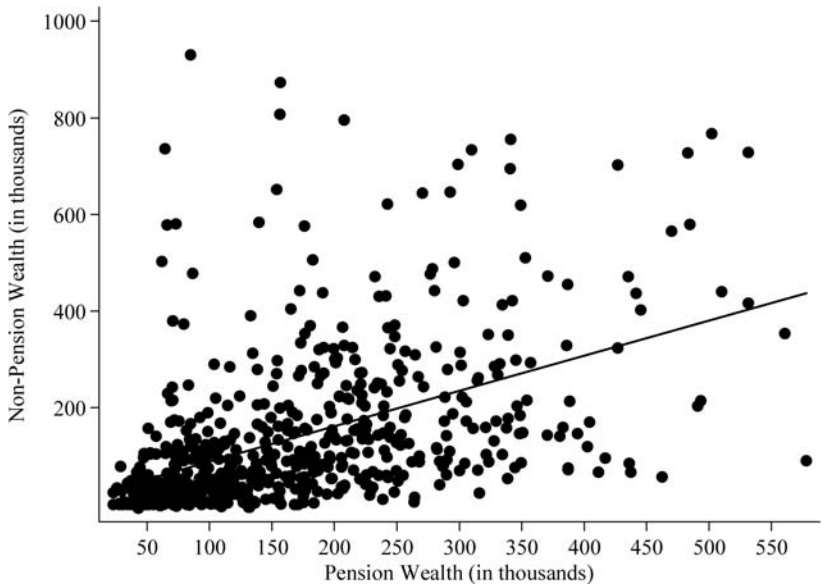

Figure 1
Nonpension Wealth and Pension Wealth

Note: This figure shows a scatter plot of cell mean nonpension wealth versus pension wealth, for cells defined by age, education, race, and marital status. It depicts the basic (noninstrumented) crowd-out relationship.

worth by 23 cents. Taken at face value, this suggests that pensions *crowd in* household saving.[6]

The *p*-value for the test of the null hypothesis that there is no selection is 0.01. However, Column 2 of the table shows the OLS estimate without selection correction. The crowd-out estimate is 0.20, very similar to the selection-corrected estimate in Column 1. This suggests that while correction for potential selection may be important from a statistical standpoint, it has little economic impact on the estimates. This turns out to be the case for the IV estimates as well.

C. Construction of the Instrument

As described in the introduction, there are a number of potential problems with this OLS estimate of β. For instance, a number of studies have documented substantial measurement error in pension data in the HRS, including Johnson, Sambamoorthi, and Crystal (2000), Gustman and Steinmeier (2004), Rohwedder, (2003a, 2003b),

6. The bottom part of the table shows selected estimates from the selection equation. 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.

Table 2
Ordinary Least Squares (OLS) and Instrumental-Variable (IV) Estimates of the Pension Crowd-Out of Nonpension Wealth with and without Selection Correction, Standard Errors in Parentheses

	(1)	(2)	(3)	(4)
	OLS		IV with Fixed Demographics, Employment, and Mean Earnings	
Explanatory Variable	With Selection Correction	Without Selection Correction	With Selection Correction	Without Selection Correction
Pension wealth	0.23	0.20	−0.62	−0.66
	(0.15)	(0.04)	(0.29)	(0.21)
First-stage estimate with respect to the instrument	—	—	0.06	0.12
			(0.03)	(0.08)
First-stage partial *F*-statistic	—	—	79.7	111.9
Selection-equation exclusions:				
Plan outsourcing	−0.08	—	−0.08	—
	(0.13)		(0.13)	
Excellent cooperation	0.29	—	0.29	—
	(0.04)		(0.04)	
p-Value for test of selection	0.01	—	0.28	—

Note: Each cell of the first row of the table represents a crowd-out estimate from a different selection-corrected estimation based on the subsample of 2,728 observations discussed in the text. Block-bootstrapped (by plan) standard errors based on 331 replications are shown in parentheses. All specifications include the present-value earnings measures described in the text and a baseline set of controls 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), a quartic in age of the head and spouse, respectively, and interactions of the age-quartic with education and current-year earnings, plus dummy variables for union, firm-size category, and region.

and Engelhardt, Cunningham, and Kumar (2007).[7] One reason for measurement error in self-reported pensions in the HRS is that respondents may report their pension plan type incorrectly. For instance, a worker who really has a DC plan in which the employer's contribution is a percentage of pay that differs with age may report

7. Starr-McCluer and Sunden (1999) and Mitchell (1988) examine pension data from the Survey of Consumer Finances (SCF).

having a “formula-based” plan, which in the HRS taxonomy means a DB plan. As discussed in Section II, because each plan type has a specific sequence of questions in the survey, this would lead the respondent down the wrong path (in this case, the DB path), confronted by a set of questions inappropriate for their true plan type. This incorrect routing leads to substantial measurement error in reported plan characteristics that then translates into error in the calculation of pension-wealth measures.

Another problem is that even if individuals correctly identify their plan type and have a good sense of the benefits their plan will eventually convey, they may have difficulty in articulating well complex plan characteristics quickly in an interview setting, such as complicated DB formulas based on salary, age, years of service, early and normal retirement dates.[8] This may lead to “don’t know” responses or refusals. Indeed, the respondent-reported data may contain many missing values, which must be imputed by the researcher in order to arrive at pension wealth numbers. Specifically, Venti and Wise (2001) reported almost 40 percent of HRS households had to have had at least one piece of information imputed in order to construct their measure of self-reported pension wealth. Such imputations can result in additional measurement error. Overall, documented problems in measurement have led the HRS to significantly revise and refine the pension sequence in more recent waves of the survey (Gustman, Steinmeier, and Tabatabai 2009).

In addition to these, for many pension plans, demographics, employment characteristics, and career earnings map into benefits in a nonlinear manner. However, most previous empirical analyses only include linear effects for these factors. If these factors also independently affect nonpension wealth accumulation nonlinearly, then previous estimates of pension crowd-out will suffer from omitted-variable bias. Finally, the presence of unobserved heterogeneity would bias OLS estimates.

We attempt to circumvent these problems by constructing an instrument for self-reported pension wealth, P, in Equation 1. The instrument must be correlated with observed pension wealth, but uncorrelated with the measurement error, nonlinearities, and heterogeneity.

Our instrument has two components: the first component is for employer-provided pension wealth; the second component is for Social Security wealth. In the first component, for each individual in pension-covered employment on their current job, we use their actual SPD, which describes in detail all plan rules and features, including eligibility, employer contributions, benefit formulas, vesting, etc., along with information on sex, age, year of birth, earnings histories, and years of service, and pension-benefit calculators—the *HRS Pension Estimation Program* for DB plans, described in Curtin, Lamkin, Peticolas, and Steinmeier (1998), and the *HRS DC/401(k) Calculator* for DC plans, developed by Engelhardt, Cunningham, and Kumar (2007)—to construct a measure of pension wealth based on matched information.

8. For example, based on extensive comparisons of responses with plan characteristics drawn from the SPDs done as background analysis for Cunningham and Engelhardt (2002), Engelhardt, Cunningham and Kumar (2007), and Engelhardt and Kumar (2007), we found more than 75% of discrepancies between self-reported and SPD plan characteristics for DC plans can be explained by three simple problems with the HRS pension question sequence.

A potential concern in constructing the instrument is that when the present value of pension entitlements are modeled as a function of *individual* pay, age, years of service, and survival probabilities, the instrument may be correlated with nonlinearities and unobserved heterogeneity and, hence, invalid. To address this, we constructed the present values using a *fixed* profile of employment and demographic characteristics, corresponding (roughly) to sample mean values: year of birth (1936), hire date (1971), quit date (2001), and survival probabilities (life-table values for the birth cohort 1936). For earnings, we use the sample mean earnings (that is, unconditional mean earnings) for each year from 1951–91 taken from the administrative earnings histories, so that the pension-values calculation is done for a fixed earnings profile, identical for all in our sample. Therefore, the earnings used to form the present value of pension entitlements are not based on actual individual information. For those without pension coverage on the current job, they are assigned a zero for this component of the instrument.

For the second component, we use the Social Security calculator developed by Coile and Gruber (2000) and the same inputs—fixed data on sex, age, year of birth, and years of covered work, and the sample mean earnings histories—to do a similar calculation for the present value of Social Security entitlements as we did for employer-provided pensions. The SPD-based employer-provided pension and Social Security wealth components are summed to yield the instrument, which we denote as Z.

There are two sources of variation in this instrument. First, there is variation across individuals in pension coverage. Second, there is variation across pension plans in generosity.

There are two key identifying assumptions for this instrument to be valid. First, any error in the SPD-based pension wealth is uncorrelated with measurement error in the self-reported pension wealth. Second, differences across workers in pension generosity, as embodied in the instrument, are as good as random, conditional on lifetime earnings, the proxies for future earnings, demographics, and employment characteristics, that is, $Cov(Z,u \mid Y,\mathbf{x},\boldsymbol{\kappa}) = 0$. In particular, there is no sorting of workers to firms that offer pensions based on an unobserved taste for saving subsumed in u. This is identical to what has been assumed in the previous literature on pensions (Gale 1998; Hubbard 1986) and the more narrowly focused literature on the impact of 401(k)s on saving (Chernozhukov and Hansen 2004; Poterba, Venti, and Wise 1995, 1996), in which 401(k) eligibility has been assumed to be conditionally random. We assess the potential impact of sorting in our robustness checks in Section V.

D. Baseline IV Estimation Results

Using the cell-level data underlying the first figure, Figure 2 plots the value of P versus the instrument, Z. This illustrates the first-stage relationship, which is strongly positive. Figure 3 plots the value of nonpension wealth versus the instrument. This illustrates the reduced-form relationship, which is negative.

The associated IV estimate of β in Equation 1 is shown in Column 3 of Table 2. To account for the fact that the instrument varies by plan, and there are multiple workers in the sample for some plans, we present standard errors that are block-

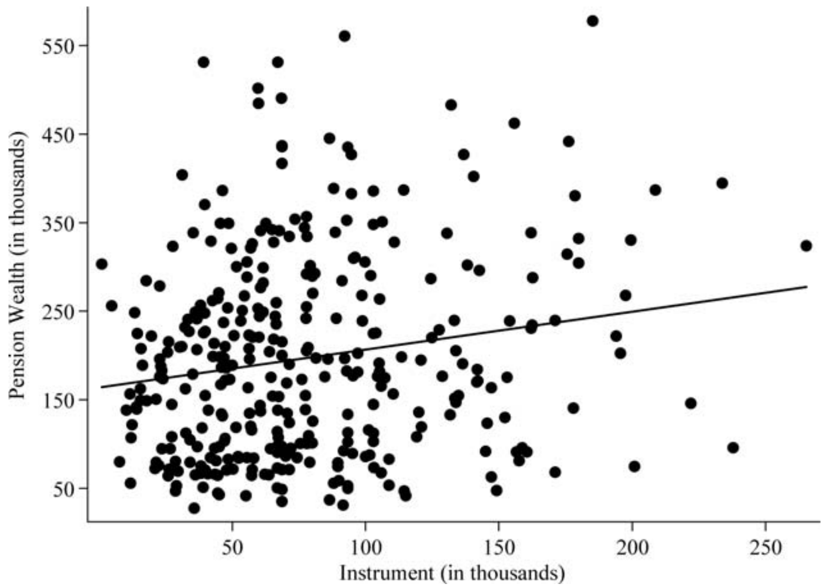

Figure 2

Pension Wealth and the Instrument

Note: This figure shows a scatter plot of cell mean pension wealth versus the instrument, for cells defined by age, education, race, and marital status. It depicts the first-stage relationship.

bootstrapped (by plan) based on 331 replications, the Andrews-Buchinsky optimal number. The selection equation was reestimated for each bootstrap sample. We follow Wooldridge (2002) and include the exclusions from the selection equation in the instrument set; the associated partial F-statistic on the instrument set from the first-stage model is shown in the third row of the table.

In contrast to the OLS result in Column 1, the IV estimate is negative, -0.62, and statistically significantly different than zero, but not different than negative one, full crowd-out. The IV estimate suggests that an additional dollar of pension wealth reduces household nonpension wealth by 62 cents. Furthermore, a comparison of the OLS and IV estimates suggests that the former is severely upward biased, with a p-value for the Hausman test of 0.02. The IV estimate without selection correction (Column 4) is similar, showing crowd-out of 66 cents.

IV. Extensions and Robustness Checks

There is an 85-cent difference between the OLS and IV crowd-out estimates $(0.85 = 0.23 - (-0.62))$. In Column 1 of Table 3, we attempt to decompose how much of this change is due to measurement error vs. unobserved heterogeneity and nonlinearities. Specifically, we recalculated the instrument assuming *actual* demographics and employment characteristics, and *actual* earnings histories. Under the

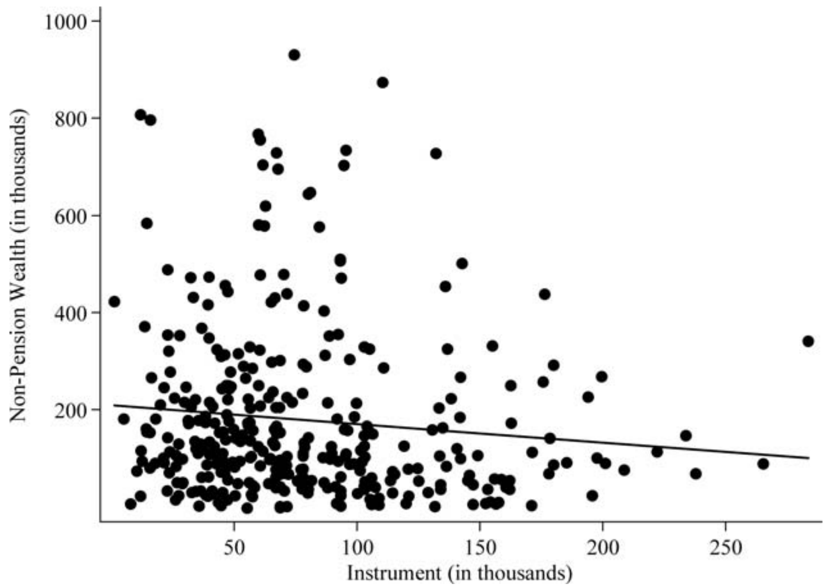

Figure 3
Nonpension Wealth and the Instrument

Note: This figure shows a scatter plot of cell mean nonpension wealth versus the instrument, for cells defined by age, education, race, and marital status. It depicts the reduced-form relationship.

assumption that error in the SPD-based measure of pension wealth is uncorrelated with the error in self-reported pension wealth and fixing characteristics helps to strip away the impact of unobserved heterogeneity, a comparison between the OLS and the IV estimate based on this new instrument should isolate the bias to OLS from measurement error. The IV estimate in Column 1 is -0.20, with a standard error of 0.28. Thus, approximately one-half of the difference in the OLS and IV estimates in Table 2 is due to measurement error in self-reported pension wealth, with the remainder due to the nonlinearities and unmeasured heterogeneity.

We explore robustness in the remaining columns of Table 3. In Column 2, we recalculate our instrument using the worker's own SPD, but randomly assigned demographic and employment characteristics and mean earnings. The assignment was done such that, when aggregated across individuals, the assigned probabilities of having a given profile of demographics match the probability in the overall sample. The crowd-out estimate in Column 2 is 60 cents, similar to that with the basic instrument.

In Column 3, we recalculate our instrument using fixed demographic and employment characteristics and predicted earnings profiles based on the parameter estimates of an earnings equation (that is, conditional mean earnings). Specifically, to calculate the predicted earnings profiles, we follow Cunningham and Engelhardt (2002) and Engelhardt, Cunningham, and Kumar (2007), and use Social Security covered earnings and a two-limit Tobit estimator to account for the censoring im-

Table 3
Additional Instrumental-Variable (IV) Estimates of the Pension Crowd-Out of Nonpension Wealth, Standard Errors in Parentheses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	IV Constructed with						
Explanatory Variable	Actual Demographics, Employment and Actual Earnings	Randomly Assigned Demographics, Employment and Mean Earnings	Fixed Demographics, Employment, and Predicted Earnings	Fixed Demographics, Employment, and Predicted Earnings	Fixed Demographics, Employment, and Predicted Earnings	Fixed Demographics, Employment, Predicted Earnings, and Imputed Past Pension	Fixed Demographics, Employment, Predicted Earnings
Pension wealth	−0.20	−0.60	−0.57	−0.61	−0.58	−0.58	−0.57
	(0.28)	(0.28)	(0.28)	(0.44)	(0.28)	(0.28)	(0.28)
First-stage estimate with respect to the instrument	0.35	0.06	0.11	0.11	0.12	0.12	0.11
	(0.08)	(0.04)	(0.02)	(0.09)	(0.09)	(0.09)	(0.08)
First-stage partial F-statistic	17.3	84.1	79.7	25.1	80.0	79.6	73.3
Additional controls							
Fringe and employment characteristics	No	No	No	Yes	No	No	No
Previous pension coverage	No	No	No	No	Yes	No	No
Risk aversion, planning horizon, and life expectancy	No	No	No	No	No	No	Yes

Note: Each cell of the first row of the table represents a crowd-out estimate from a different selection-corrected estimation based on the subsample of 2,728 observations discussed in the text. Block-bootstrapped (by plan) standard errors based on 331 replications are shown in parentheses. All specifications include the present-value earnings measures described in the text and a baseline set of controls 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), a quartic in age of the head and spouse, respectively, and interactions of the age-quartic with education and current-year earnings, plus dummy variables for union, firm-size category, and region. The additional controls in Column 4 include fringe benefits and other employment characteristics, as described in the text.

posed from below by zero earnings from labor force nonparticipation and from above by the FICA taxable maximum for the following specification:

$$(2) \quad \ln(y_{it}) = \kappa_{1t} + \sum_{g=1}^{G} \kappa_{2gt} D_i^{OwnEduc\,g} + \sum_{h=1}^{H} \kappa_{4ht} Age_{it}^{h} + \kappa_{5t} D_i^{White} + \boldsymbol{\theta}\mathbf{m}_i + \eta_{it}.$$

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 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 ($H = 4$); and time-varying white-nonwhite earnings gaps. In addition, the specification includes a vector of explanatory variables, $\mathbf{m}$, 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.

The parameters in (2) were estimated separately by sex using Social Security earnings data from 1951 to 1991 for *all* HRS individuals with matched earnings histories. These estimates, with actual values of education, age, and the variables in $\mathbf{m}$ for those individuals in the analysis sample, were used to make the predicted log earnings for each year from 1951 to 1991 and then were exponentiated to form an earnings history (in levels) for each individual in the analysis sample. This predicted earnings history, along with the fixed demographic and employment characteristics, was then used as an input into the pension calculators described above to generate the instrument.

Now, there are three sources of variation in this instrument based on predicted earnings. First, there is variation across individuals in pension coverage. Second, there is variation across plans in generosity. Third, there is variation within plans across workers with different predicted earnings, which occurs because some plans have benefit schedules that are nonlinear in pay, and there are some plans in the SPD database that are large enough to have multiple workers in the HRS sample.[9]

The associated IV estimate of β is shown in Column 3 of Table 3. The IV estimate is -0.57 and statistically significantly different than zero. It suggests that an additional dollar of pension wealth reduces household nonpension wealth by 57 cents. Once demographic and employment characteristics are fixed as inputs to the pension

9. Technically, the component of the instrument based on Social Security wealth does not account, in general, for any real variation, in the sense that Social Security rules are identical across covered workers. More specifically, the Social Security wealth component will vary across workers occupying different predicted-earnings cells. But within those cells, there is no variation across workers. There is one general exception. Some DB plans are integrated with Social Security, so that even within a predicted-earnings cell, there can be different total pension wealth depending on the extent of variation across DB plans (within that cell) in Social Security integration.

calculators, the similarity in the estimate in Column 3 here and that in Column 3 of Table 2 (-0.57 vs. -0.62) suggests that additional nonlinearities associated with the observed determinants of earnings used in the earnings Equation 2 are not generating substantial omitted-variable bias in the crowd-out estimates.

In Column 4 of Table 3, we perform another robustness check by adding two large sets of controls. First, controls for nonpension fringe benefits on the current job: 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. Second, other controls for 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. Although somewhat less precise, the results in Column 4 suggest crowd-out of a similar magnitude, 61 cents. In fact, the *p*-value for the test of the null hypothesis that crowd-out, β, is equal across Columns 3 and 4 is 0.70.

V. Additional Robustness Checks for Sorting

Because the instruments used thus far are based on the worker's *actual* SPD, a key threat to a causal interpretation from the crowd-out estimates is the potential for endogenous sorting. For example, workers with a high taste for saving may sort themselves to firms that offer pensions (Allen, Clark, and McDermed 1993; Curme and Even 1995; Ippolito 1997; Even and McPherson 1997). If so, then the crowd-out estimates thus far in Tables 2 and 3 are biased toward zero, indicating too little crowd-out. Sorting in this direction is what typically has been discussed in the literature on the saving impact of IRAs, 401(k)s, and pensions—namely, "savers" save more in all forms, including pensions. Alternatively, if workers with commitment problems sort to pension-covered jobs as a way to force saving, then the basic crowd-out estimates are biased away from zero, indicating too much crowd-out. Firms also may have adopted pensions in response to employee interest, especially small firms. In a survey by Buck Consultants (1989), employee interest was cited as a reason for 401(k) adoption by 63.5 percent of firms. Finally, Ippolito (1997) has argued that firms may have used 401(k)s, and employer matching, in particular, to direct additional compensation to workers with a low rate of time preference as part of an optimal employee-retention policy. In any of these cases, sorting that is correlated with the instrument would render our IV crowd-out estimates invalid.

Unfortunately, because the HRS first interviews individuals in their mid 50's and has sparse retrospective information (including no retrospective information on wealth), it is not an ideal data source to explore the impact of sorting over the career. Consequently, we assess whether sorting confounds our IV estimates using a series of approaches that best exploit the HRS data available, but none of which are ironclad.

A key implication of sorting models is that job mobility and changes in coverage should be linked. For example, if workers with high tastes for saving systematically seek pension-covered jobs, then past coverage should be correlated with current coverage for these workers, and because current coverage is an important source of variation in our instrument, then past coverage might confound our IV estimates.

We assess the impact of this in two ways. First, we utilize the retrospective information gathered in the 1992 survey on job histories and prior pension coverage and present in Column 5 of Table 3 the crowd-out estimates from a modified version of Equation 1,

$$(3) \quad W_i = \beta P_i + \varphi Yi + \boldsymbol{\alpha}\mathbf{x}_i + \boldsymbol{\gamma}\boldsymbol{\kappa}_i + \rho\hat{\lambda}_i + \theta_1 D_i^{PrevJob1Cov} + \theta_2 D_i^{PrevJob23Cov} + u_i$$

in which indicators for coverage on the most recent previous job ("previous job 1"), $D^{PrevJob1Cov}$, and the second and third previous jobs ("previous jobs 2 or 3"), $D^{PrevJobs23Cov}$, respectively, have been added. If workers sort, past coverage can be thought of as a proxy for the omitted variable that is the unobserved taste for saving. In particular, if past coverage both predicts current coverage and is correlated with the unobservable, then the IV estimate of β should change substantially from that in Column 3 of Table 3, reflecting the omitted-variable bias from sorting. The estimation results for Equation 3 are shown in Column 5 of Table 3. The IV estimate, $\hat{\beta} = -0.58$, is almost identical to that in Column 3.[10]

Second, we remade the instrument in Column 3 of Table 3 by imputing past pension benefits at the one-digit industry level for those with pension coverage but no matched SPD on the past job. The IV estimates based on this alternative instrument are shown in Column 6 of the table and are very similar, showing about 58 cents crowd-out.

Next, a number of previous studies have suggested that sorting is done on the basis of risk aversion, discount rate, and life expectancy. For example, workers with low discount rates and long life expectancies should be particularly attracted to firms offering deferred compensation in the form of pensions. Thus, in Column 7 of Table 3, we added to our specification from Column 3 variables that proxy for these factors. Our measure of risk aversion is based on Barsky et al. (1998), and our proxy for the discount rate is a set of dummy variables for the household's reported financial planning horizon, 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

10. Unfortunately, panel tests for sorting going forward in time from 1992 are not feasible for two reasons. First, many of the employment transitions that occur after 1992 are retirements. Second, the next year in which the HRS gathered SPDs was 1998. The HRS was able to match far fewer SPDs in 1998 than 1992. For example, we can make our instrument in 1998 for only 711 of the 2,728 households (or roughly 25%) in our analysis sample, so that any type of panel tests would be on a small and highly selected sample.

> "next few months, next year, next few years, next five to ten years, longer than ten years."

Finally, we include two measures of life expectancy based on the following HRS question:

> "Using any number from zero to ten, where 0 equals absolutely no chance and ten equals absolutely certain, what do you think are the chances that you will live to be 75 or more?"

(and an isomorphic question for the chances of living to be 85 or more). The associated IV estimate in Column 7 is 57 cents, in the same general range as the other estimates.

Finally, Column 1 of Table 4 presents the crowd-out estimates for the specification in Column 3 of Table 3 that use an instrument based not on the worker's actual SPD, but based on the mean instrument for similar workers. Specifically, we divided the sample into cells based on industry, firm size, union, and region of birth and then collapsed the instrument from our preferred specification in Column 3 of Table 3 to yield a leave-one-out cell-mean instrument. We then use this as our instrumental variable. This cell-mean instrument should be less susceptible to any concerns about sorting, because for any worker, the cell-mean instrument does not depend on the actual SPD, but on the average of others with similar characteristics in the sample. The IV estimates in Column 1 of Table 4 indicate that an additional dollar of pension wealth reduces household nonpension wealth by 67 cents. This is qualitatively similar to what was found when the instrument was based on the actual SPD.

Column 2 of Table 4 repeats this exercise, but now collapsing only by industry, union, and birth region, to account for potential sorting across firm size. The IV crowd-out now is 56 cents. The specification in Column 3 adds controls for risk aversion, planning horizon, and life expectancy, and the results are similar.

In summary, while each of these approaches has strengths and weaknesses in evaluating the potential impact of sorting, the IV estimates are very similar across approaches and suggest substantial crowd-out, in the 53–67 cent range. The weight of the evidence suggests that sorting is not confounding our IV crowd-out estimates.

VI. Impact Across the Wealth Distribution

There are two key drawbacks to the crowd-out estimates thus far. First, they are based on mean-regression estimators, which are sensitive to skewness 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 nonpension wealth to pension wealth across the wealth distribution (Bitler, Gelbach, and Hoynes 2006).

To illustrate the potential for differences in response, Table 5 shows the sample means for various measures of wealth and asset ownership for each decile of the nonpension net worth distribution. The patterns in our sample accord with what is known broadly about wealth holdings (Browning and Lusardi 1996; Hurst, Luoh,

Table 4
Additional Instrumental-Variable (IV) Estimates of the Pension Crowd-Out of Nonpension Wealth, Standard Errors in Parentheses

	(1)	(2)	(3)
	Cell Mean of the Instrument in (3) of Table 3 by		
Explanatory Variable	Industry, Firm Size, Union, and Birth Region	Industry, Union, and Birth Region	Industry, Union, and Birth Region
Pension wealth	−0.67 (0.30)	−0.56 (0.31)	−0.53 (0.32)
First-stage estimate with respect to the instrument	0.11 (0.02)	0.45 (0.11)	0.47 (0.11)
First-stage partial F-statistic	23.7	24.0	23.7
Additional controls			
Risk aversion	No	No	Yes
Planning horizon	No	No	Yes
Life expectancy	No	No	Yes

Note: Each cell of the first row of the table represents a crowd-out estimate from a different selection-corrected estimation based on the subsample of 2,728 observations discussed in the text. Block-bootstrapped (by cell) standard errors based on 331 replications are shown in parentheses. All specifications include the present-value earnings measures described in the text and a baseline set of controls 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), a quartic in age of the head and spouse, respectively, and interactions of the age-quartic with education and current-year earnings, plus dummy variables for union, firm-size category, and region.

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 nonpension 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 skewness. Specifically, we estimate the specification from Column 3 of Table 3 using the ordinary quantile regression (OQR) estimator and the instrumental-variable quantile regression (IVQR) estimator of Chernozhukov and Hansen (2004, 2005) for every

Table 5
Sample Means for Selected Wealth and Ownership Measures by Decile of the Total Nonpension Net Worth Distribution

Variable	(1) Bottom Decile	(2) 2nd Decile	(3) 3rd Decile	(4) 4th Decile	(5) 5th Decile	(6) 6th Decile	(7) 7th Decile	(8) 8th Decile	(9) 9th Decile	(10) Top Decile
Total nonpension 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 percent of total wealth	109	91	79	73	69	64	59	51	42	21
Nonhousing 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 per cent of total nonhousing wealth	108	94	91	87	84	77	72	63	51	24

Note: Authors' calculations from the analysis sample.

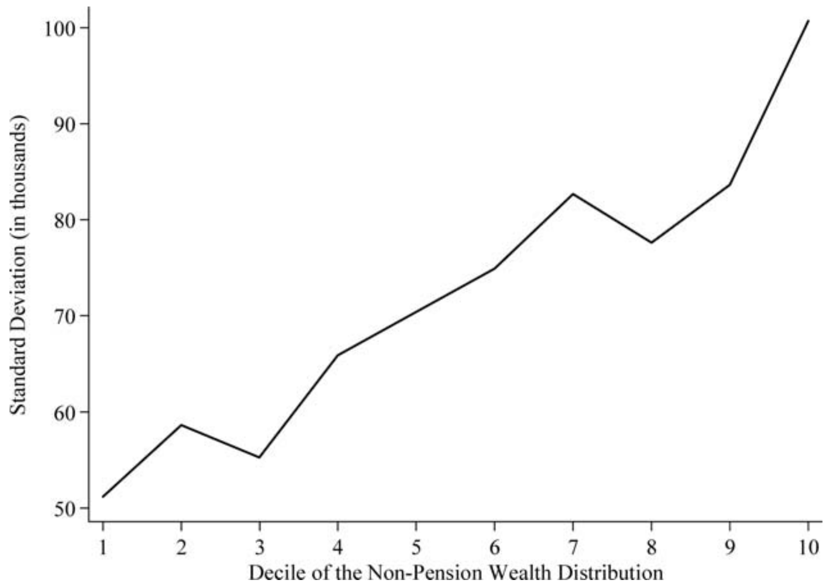

Figure 4

Standard Deviation of the Instrument by Wealth Decile

Note: This figure shows a line graph of the standard deviation of the instrument for selected deciles of the wealth distribution and illustrates there is substantial variation in the instrument by wealth level.

fifth quantile (from the tenth through the 90th) of the distribution of nonpension net worth.[11]

In Figure 4, we illustrate the independent variation in the instrument across the nonpension wealth distribution. Namely, we plot for each decile of the nonpension 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 percent nonparametric bootstrapped confidence intervals are plotted in Figures 5 and 6, respectively. All estimates in the figures are corrected for potential nonrandom 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 two exclusions already outlined, and then in place of $\hat{\lambda}$ in (1)

11. Technically, this estimator requires that the disturbance term, u, contain a ranking variable, call it v that represents heterogeneity in nonpension wealth outcomes for households with the same lifetime earnings, demographics, pension wealth, employment characteristics, etc. The asymptotic properties of the estimator will not hold if there is sorting of workers to firms based on pension generosity, because this ranking requirement will not necessarily obtain. Therefore, the IVQR estimates are identified (in part) in our application under the assumption of no sorting.

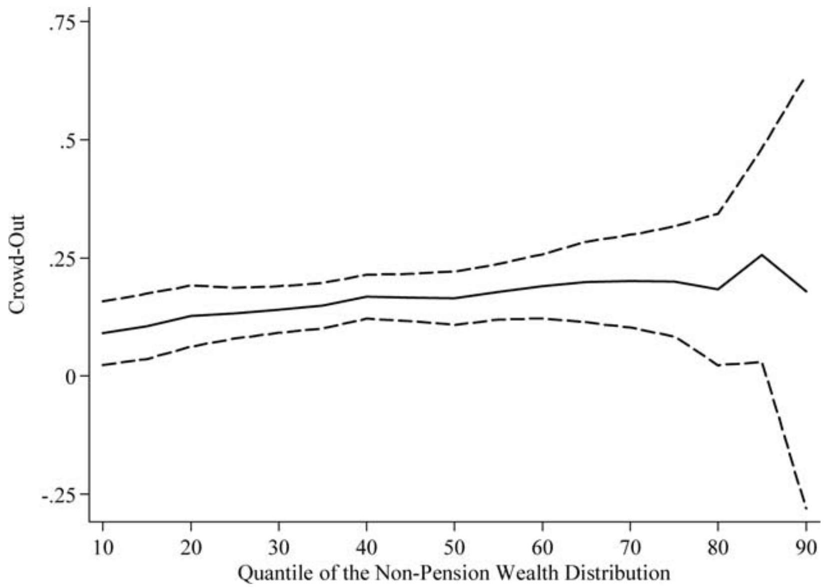

Figure 5

Ordinary Quantile Regression Estimates for Crowd-Out with 90% Confidence Intervals

Note: The solid line in the figure shows the ordinary quantile regression crowd-out estimates for selected quantiles of the nonpension wealth distribution. The dashed lines show the boundaries of the associated 90 percent confidence intervals.

included a quartic of the propensity score from the selection equation in the crowd-out equation estimated by OQR and IVQR, respectively. Newey (1999) proves the consistency of this estimator. 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 5 and 6, the selection equation was re-estimated with each of the 331 bootstrap replications.

Across quantiles, the OQR estimates of the pension crowd-out in Figure 5 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 6 indicate, in contrast, considerable heterogeneity in crowd-out.[12] 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 and may suggest that households in the lower portions of the wealth

12. 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 estimates are qualitatively unchanged, but the estimates are more precise than those shown in Figure 6.

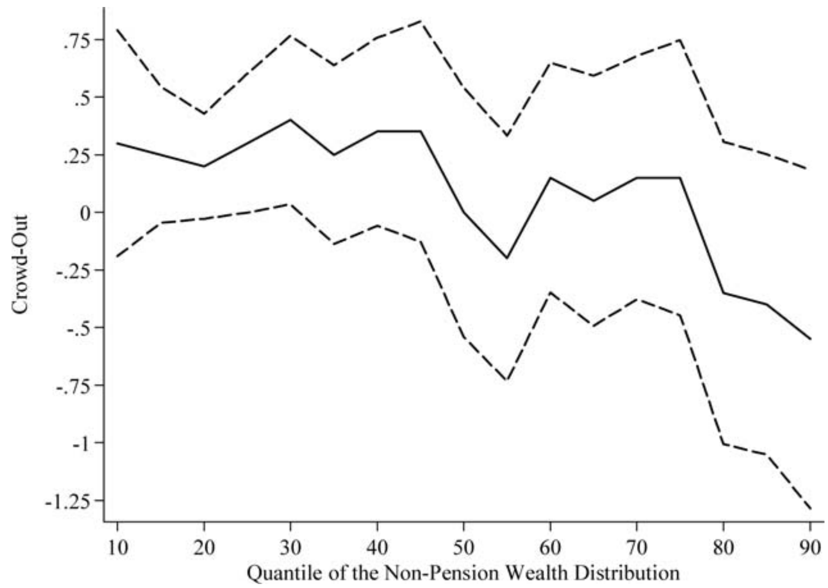

Figure 6
Instrumental Variable Quantile Regression Estimates for Crowd-Out with 90% Confidence Intervals

Note: The solid line in the figure shows the instrumental variable quantile regression crowd-out estimates for selected quantiles of the nonpension wealth distribution. The dashed lines show the boundaries of the associated 90 percent confidence intervals.

distribution may save more as they come in contact with pensions, an important part of the financial system. However, for 80th-90th percentiles, the crowd-out is 30–50 cents. The crowd-out at the 95th percentile (not shown) is -75 cents, but with a very large confidence interval (0, -1.5).

To further examine the extent to which the wealth response to pensions differs across households, we follow Gale (1998) and split the sample along three dimensions, median net worth, median income, and college education.[13] The associated estimation results, which are briefly summarized in Table 6, reinforce the quantile regression results: there is substantial variation across households in the pension offset to saving, with the bulk of crowd-out occurring in the upper levels of the income/wealth distribution.

13. The sample split by net worth is endogenous, but was suggested by a referee. We also have examined the extent to which the crowd-out operates through illiquid wealth, such as business, housing, and vehicles. Crowd-out through these forms is not driving our results.

Table 6
Instrumental-Variable (IV) Crowd-Out Estimates for Selected Sub-Samples Based on Economic Status, Standard Errors in Parentheses

	(1)	(2)	(3)	(4)	(5)	(6)
	Nonpension Net Worth		Income		Education	
Explanatory Variable	Above the Median	Below the Median	Above the Median	Below the Median	College Graduate or Higher	Less than College Graduate
Pension wealth	−0.98	0.16	−1.14	0.01	−0.70	−0.45
	(0.41)	(0.04)	(0.33)	(0.35)	(0.15)	(0.27)
p-value for test of equal crowd-out	0.01		0.12		0.56	

Note: Each cell of the table represents a crowd-out estimate from a different selection-corrected estimation of the specification in Column 3 of Table 3 for the group shown in the Column heading. Block-bootstrapped (by plan) standard errors based on 331 replications are shown in parentheses. The last row shows the p-value for the test of the null hypothesis that the crowd-out estimates in the first row are equal.

VII. Conclusion

Our results suggest significant crowd-out, ranging from 53 to 67 cents 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.[14]

Instrumenting matters a great deal, suggesting that unobserved heterogeneity, nonlinearities, and measurement error 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 nonpension wealth. Similarly, the OQR results uniformly suggest crowd-in, but for the upper quantiles of the wealth distribution, the IVQR estimates flip sign and indicate crowd-out.[15] Finally, there was substantial heterogeneity in the estimated crowd-out across the wealth distribution, with zero offsets at or below the median, and the bulk of the mean effect concentrated in the upper quantiles.[16]

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 two important caveats to this analysis. 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.[17] 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 house-

14. 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 household saving in the United Kingdom and Italy, respectively.

15. Gale (1998) 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).

16. 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).

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

hold wealth and not be undone by a reduction in nonpension wealth, but that is an open question. In addition, 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.

References

Allen, Steven G., Robert L. Clark, and Ann A. McDermed. 1993. "Pensions, Bonding, and Lifetime Jobs." *Journal of Human Resources* 28(3):463–81.

Andrews, Donald W. K., and Moshe Buchinsky. 2000. "A Three-Step Method for Choosing the Number of Bootstrap Repetitions." *Econometrica* 68(1):23–51.

Attanasio, Orazio P., and Agar Brugiavini. 2003. "Social Security and Households' Saving." *Quarterly Journal of Economics* 118(3):1075–1119.

Attanasio, Orazio P., and Susann Rohwedder. 2003. "Pension Wealth and Household Saving: Evidence from Pension Reforms in the United Kingdom." *American Economic Review* 93(5):1499–1521.

Avery, Robert B., Gregory E. Elliehausen, and Thomas A. Gustafson. 1986. "Pensions and Social Security in Household Portfolios: Evidence from the 1983 Survey of Consumer Finances." In *Savings and Capital Formation: The Policy Options*, ed. F. Gerard Adams and S. M. Wachter, 127–60. Lexington, Mass.: Lexington Books.

Barsky, Robert B., F. Thomas Juster, Miles S. Kimball, and Matthew D. Shapiro. 1998. "Preference Parameters and Behavioral Heterogeneity: An Experimental Approach in the Health and Retirement Study." *Quarterly Journal of Economics* 112(2):537–80.

Berger, M., D. Black, and F. Scott. 2000. "Bounding Parameter Estimates with Non-Classical Measurement Error." *Journal of the American Statistical Association* 95(451):739–48.

Bitler, Marianne P., Jonah P. Gelbach, and Hilary W. Hoynes. 2006. "What Mean Impacts Miss: Distributional Effects of Welfare Reform Experiments." *American Economic Review* 96(4):988–1012.

Blinder, Alan S., Roger H. Gordon, and Donald E. Wise. 1980. "Reconsidering the Work Disincentive Effects of Social Security." *National Tax Journal* 33(4):431–42.

Browning, Martin, and Annamaria Lusardi. 1996. "Household Saving: Micro Theories and Micro Facts." *Journal of Economic Literature* 34(4):1797–1855.

Buck Consultants, Inc. 1989. *Current 401(k) Plan Practices: A Survey Report*. Secaucus, NJ: Buck Consultants, Inc.

Cagan, Phillip. 1965. "The Effect of Pension Plans on Aggregate Saving: Evidence from a Sample Survey." National Bureau of Economic Research Occasional Paper No. 95.

Chernozhukov, Viktor, and Christian Hansen. 2004. "The Effect of 401(k) Participation on the Wealth Distribution: An Instrumental Quantile Regression Analysis." *Review of Economics and Statistics* 86(3):735–51.

Chernozhukov, Viktor, and Christian Hansen. 2005. "An IV Model of Quantile Treatment Effects." *Econometrica* 73(1):245–61.

Choi, James J., David Laibson, and Brigitte C. Madrian. 2004. "Plan Design and 401(k) Savings Outcomes." *National Tax Journal* 57(2):275–98.

Choi, James J., David Laibson, Brigitte C. Madrian, and Andrew Metrick. 2002. "Defined Contribution Pensions: Plan Rules, Participant Decisions, and the Path of Least Resistance." In *Tax Policy and the Economy*, Vol. 16, ed. J.M. Poterba, 67–114. Cambridge, MA: MIT Press.

———. 2003. "Optimal Defaults." *American Economic Review* 93(2):180–85.

———. 2004. "For Better or Worse: Default Effects and 401(k) Savings Behavior." In *Perspectives in the Economics of Aging*, ed. D. A. Wise, 81–125. Chicago: University of Chicago Press.

Coile, Courtney, and Jonathan Gruber. 2000. "Social Security and Retirement." NBER Working Paper 7830.

Cunningham, Christopher R., and Gary V. Engelhardt. 2002. "Federal Tax Policy, Employer Matching, and 401(k) Saving: Evidence from HRS W-2 Records." *National Tax Journal* 55(3):617–45.

Curme, Michael A., and William E. Even. 1995. "Pension Coverage and Borrowing Constraints." *Journal of Human Resources* 30(4):701–12.

Curtin, Richard, Jody Lamkin, Bob Peticolas, and Thomas Steinmeier. 1998. " *The Pension Calculator Program for the Health and Retirement Study (Level 2: Pre-Compiled Version)*." Unpublished.

Das, Mitali, Whitney K. Newey, and Francis Vella. 2003. "Nonparametric Estimation of Sample Selection Models." *Review of Economic Studies* 70(1):33–63.

Diamond, Peter A., and Jerry A. Hausman. 1984. "Individual Retirement and Savings Behavior." *Journal of Public Economics* 23(1):81–114.

Duflo, Esther, William Gale, Jeffrey Liebman, Peter Orszag, and Emmanuel Saez. 2006. "Saving Incentives for Low- and Middle- Income Families: Evidence from a Field Experiment with H&R Block." *Quarterly Journal of Economics* 121(4):1311–46.

Engelhardt, Gary V., Christopher R. Cunningham, and Anil Kumar. 2007. "Measuring Pension Wealth." In *Redefining Retirement: How Will Boomers Fare?* ed. O. Mitchell, B. Soldo, and B. Madrian, 211–33. Oxford: Oxford University Press.

Engelhardt, Gary V., and Anil Kumar. 2007. "Employer Matching and 401(k) Saving: Evidence from the Health and Retirement Study." *Journal of Public Economics* 91(10):1920–43.

Engen, Eric M., William G. Gale, and John Karl Scholz. 1996. "The Illusory Effects of Saving Incentives on Saving." *Journal of Economic Perspectives* 10(4):113–37.

Even, William E., and David A. Macpherson. 1997. " *Factors Influencing Participation and Contribution Levels in 401(k) Plans*." Miami University (Ohio). Unpublished.

Feldstein, Martin. 1974. "Social Security, Induced Retirement, and Aggregate Capital Accumulation." *Journal of Political Economy* 82(5):905–26.

———. 1996. "Social Security and Savings: New Time Series Evidence." *National Tax Journal* 46(2):151–64.

Feldstein, Martin, and Anthony Pellechio. 1979. "Social Security and Household Wealth Accumulation: New Microeconometric Evidence." *Review of Economics and Statistics* 61(3):361–68.

Gale, William G. 1998. "The Effects of Pensions on Household Wealth: A Re-Evaluation of Theory and Evidence." *Journal of Political Economy*106(4):706–23.

Gale, William G., Michael Dworksy, John Phillips, and Leslie Muller. 2007. " *Effects of After-Tax Pension and Social Security Benefits on Household Wealth: Evidence from a Sample of Retirees*." Brookings Institution. Unpublished.

Gale, William G., and Joseph M. Milano. 1998. "Implications of the Shift to Defined Contribution Plans for Retirement Wealth Accumulation." In *Living with Defined Contribution Pensions*, ed. O.S. Mitchell and S.J. Schieber, 115–35. Philadelphia: University of Philadelphia Press.

Gustman, Alan L., Olivia S. Mitchell, Andrew A. Samwick, and Thomas L. Steinmeier. 1999. "Pension and Social Security Wealth in the Health and Retirement Study." In *Wealth, Work, and Health: Innovations in Measurement in the Social Sciences*, ed. J. Smith and R. Willis, 150–208. Ann Arbor: University of Michigan Press.

Gustman, Alan L., and Thomas L. Steinmeier. 1999. "Effects of Pensions on Savings: Analysis with Data from the Health and Retirement Study." *Carnegie-Rochester Series on Public Policy* 50(1):271–326.

———. 2004. "What People Don't Know About Pensions and Social Security." In *Private Pensions and Public Policies*, ed. W. G. Gale, J. B. Shoven, and M. J. Washawsky, 57–125. Washington, D.C.: Brookings Institution Press.

Gustman, Alan L., Thomas L. Steinmeier, and Nahid Tabatabai. 2009. "Do Workers Know About Their Pension Plan Type? Comparing Workers' and Employers' Pension Information." In *Overcoming the Saving Slump: How to Increase the Effectiveness of Financial Education and Saving Programs*, ed. A. Lusardi, 47–81. Chicago: University of Chicago Press.

Hubbard, R. Glenn. 1986. "Pension Wealth and Individual Saving." *Journal of Money, Credit, and Banking* 18(2):167–78.

Hubbard, R. Glenn, and Jonathan S. Skinner. 1996. "Assessing the Effectiveness of Saving Incentives." *Journal of Economic Perspectives* 10(4):73–90.

Hurd, Michael D. 1989. "Mortality Risk and Bequests." *Econometrica* 57(4):779–813.

Hurst, Erik, Ming Ching Luoh, and Frank Stafford. 1998. "The Wealth Dynamics of American Families, 1984–94." *Brookings Papers on Economic Activity* 1998(1):267–337.

Ippolito, Richard A. 1997. *Pension Plans and Employee Performance: Evidence, Analysis, and Policy*. Chicago: University of Chicago Press.

Johnson, Richard W., Usha Sambamoorthi, and Stephen Crystal. 2000. "Pension Wealth at Midlife: Comparing Self-Reports with Provider Data." *Review of Income and Wealth* 46(1):59–83.

Kane, Thomas, Cecilia Rouse, and Douglas Staiger. 1999. "Estimating the Returns to Schooling when Schooling is Misreported." NBER Working Paper 7235.

Katona, George. 1965. *Private Pensions and Individual Saving*. Ann Arbor: University of Michigan, Survey Research Center, Institute for Social Research.

Khitatrakun, Surachai, Yuichi Kitamura, and John Karl Scholz. 2001. " *Pensions and Wealth: New Evidence from the Health and Retirement Study*." University of Wisconsin-Madison. Unpublished.

Kotlikoff, Laurence J. 1979. "Testing the Theory of Social Security and Life-Cycle Accumulation." *American Economic Review* 69(3):396–410.

Leimer, Dean R., and Selig D. Lesnoy. 1982. "Social Security and Private Saving: New Time-Series Evidence." *Journal of Political Economy* 90(3):606–29.

Madrian, Brigitte C., and Dennis F. Shea. 2001. "The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior." *Quarterly Journal of Economics* 116(4):1149–87.

Mitchell, Olivia S. 1988. "Worker Knowledge of Pension Provisions." *Journal of Labor Economics* 6(1):21–39.

Mitchell, Olivia S., Jan Olson, and Thomas Steinmeier. 1996. "Construction of the Earnings and Benefits File (EBF) for Use with the Health and Retirement Survey." *Institute for Social Research*, University of Michigan, HRS/AHEAD Documentation Report No. DR-001.

Munnell, Alicia H. 1974. *The Effect of Social Security on Personal Savings*. Cambridge, Mass.: Ballinger.

Munnell, Alicia H. 1976. "Private Pensions and Saving: New Evidence." *Journal of Political Economy* 84(5):1013–32.

Newey, Whitney K. 1999. "Consistency of Two-Step Sample Selection Estimators Despite Misspecification of Distribution." *Economics Letters* 63(2):129–32.

Poterba, James M., Steven F. Venti, and David A. Wise. 1995. "Do 401(k) Contributions Crowd Out Other Personal Saving?" *Journal of Public Economics* 58(1):1–32.

———. 1996. "How Retirement Saving Programs Increase Saving." *Journal of Economic Perspectives* 10(4):91–112.
Rohwedder, Susann. 2003a. "Empirical Validation of HRS Pension Wealth Measures." Rand Corporation. Unpublished.
———. 2003b. "Measuring Pension Wealth in the HRS: Employer and Self-Reports." Rand Corporation. Unpublished.
Starr-McCluer, Martha, and Annika Sunden. 1999. "Workers' Knowledge of Their Pension Coverage: A Reevaluation" *Federal Reserve Board of Governors*. Unpublished.
Venti, Steven F., and David A. Wise. 2001. "Choice, Chance, and Wealth Dispersion at Retirement." In *Aging Issues in the United States and Japan*, ed. S. Ogura, T. Tachibanaki, and D. Wise, 25–64. Chicago: University of Chicago Press.
Wooldridge, Jeffrey M. 2002. *Econometric Analysis of Cross Section and Panel Data*. Cambridge, Mass: MIT Press.

Appendix 1

Technical Appendix

The linear-in-parameters econometric specification we estimate in (1) in the text follows from the basic, frictionless 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,

$$\text{(A1)} \quad U(C_t) = \frac{C_t^{1-\rho}}{1-\rho},$$

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

$$\text{(A2)} \quad \max_{C_t} \int_0^T \frac{C_t^{1-\rho}}{1-\rho} e^{-\delta t} dt \ ,$$

subject to the intertemporal budget constraint

$$\text{(A3)} \quad \int_R^T B_t e^{-rt} dt + \int_0^R E_t e^{-rt} dt = \int_0^T C_t e^{-rt} dt \ ,$$

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 nonpension wealth, it is convenient to note that nonpension wealth at any given age A while working can be written as

(A4) $$W_A = \int_0^A (E_t - C_t) e^{r(A-t)} dt \ .$$

The left-hand side of (A4) 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 (A2)-(A3) imply

(A5) $$C_t = C_0 e^{[(r-\delta)/\rho]t} \ .$$

Let $x \equiv [(r-\delta)/\rho] - r$, then (A3) and (A4) can be used to solve for C_0,

(A6) $$C_0 = \frac{x}{e^{xT}-1}\left(\int_R^T B_t e^{-rt} dt + \int_0^R E_t e^{-rt} dt\right),$$

which can substituted back into (A5) and then (A4) to yield

(A7) $$W_A = \int_0^A E_t e^{r(A-t)} dt - Q\int_R^T B_t e^{r(A-t)} dt - Q\int_0^R E_t e^{r(A-t)} dt \ ,$$

where when $x \neq 0$,

(A8) $$Q = \frac{e^{xA}-1}{e^{xT}-1} \ ,$$

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 $x=0$, $Q = A/T$. We note that this adjustment is not due to any inherent frictions in the model, as Gale's framework is based on a frictionless model, and, as Gale argues, is applicable even for incomplete offset. Because the last term on the right-hand side of (A7) can be expressed as

(A9) $$Q\int_0^R E_t e^{r(A-t)} dt = Q\int_0^A E_t e^{r(A-t)} + Q\int_A^R E_t e^{r(A-t)} dt \ ,$$

equation (7) simplifies to

(A10) $$W_A = (1-Q)\int_0^A E_t e^{r(A-t)} dt - Q\int_R^T B_t e^{r(A-t)} dt - Q\int_A^R E_t e^{r(A-t)} dt \ .$$

Equation (A10) is a convenient representation of nonpension wealth at a point in time, and is the basis for (1) in the text:

(A11) $$W_i = \beta P_i + \varphi Y_i + \boldsymbol{\alpha}\mathbf{x}_i + \boldsymbol{\gamma}\boldsymbol{\kappa}_i + \rho\hat{\lambda}_i + u_i \ ,$$

with $\hat{\lambda}$ added to correct for selection, as described in the text.

The first term on the right-hand side of (A10) 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 jth adult in the ith household as y_{ij}^{0A}, but the household-level explanatory variable in (A11), which is adjusted by $1-Q$, as Y^{0A}. We use the Social Security covered-earnings histories from 1951–91 and W-2 earnings records for jobs held from 1980–91 to construct the present value of 1951–91 earnings for each adult in the household, y_{ij}^{0A}. Then the household-level explanatory variable, Y^{0A}, which is the first term on the right-hand side of (A11) is made by adjusting the individual present values by one minus Gale's Q and aggregating within the household,

$$\text{(A12)} \quad Y_i^{0A} \equiv \sum_j y_{ij}^{0A}(1-Q_{ij}) \ ,$$

to obtain the present value of household earnings.

To make the present value of earnings to date measure, we use administrative earnings data from SSA and IRS that include Social Security covered-earnings histories from 1951–91 and W-2 earnings records for jobs held from 1980–91. When combined with self-reported earnings histories, these data allow for the construction of the present value of earnings to date from 1951–91. Specifically, 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 of (3) in the text 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 back-cast with these growth rates. Real earnings from age 20 until the survey date were then expressed in present value terms in 1992.

The factor Q is defined as

$$\text{(A13)} \quad Q_{ij} \equiv (e^{x_{ij}A_{ij}}-1)/(e^{x_{ij}T_{ij}}-1) \ ,$$

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); 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:

$$\text{(A14)} \quad Y_i^{0A} \equiv \sum_j y_{ij}^{0A}(1-Q_{ij}) \ .$$

The primary variable of interest in (A10) 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 jth adult in the ith household as

p_{ij}, but the household-level explanatory variable in (A11), which is adjusted by Q, as P. Then P is defined as

(A15) $$P_i \equiv \sum_j p_{ij} Q_{ij} \ ,$$

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). We do not tax-adjust our measures. For such an analysis, see Gale, Dworsky, Phillips, and Muller (2007).