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Employer matching and 401(k) saving: Evidence from the health and retirement study ☆

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Abstract

Employer matching of employee 401(k) contributions is often touted as a powerful incentive to save for retirement and is a key component in pension-plan design in the United States. Using detailed administrative contribution, earnings, and pension-plan data from the Health and Retirement Study, this analysis formulates a life-cycle-consistent econometric specification of 401(k) saving and estimates the determinants of saving accounting for non-linearities in the household budget set induced by matching. The participation estimates indicate that an increase in the match rate by 25 cents per dollar of employee contribution raises 401(k) participation by 5 percentage points. The parametric and semi-parametric

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estimates for saving indicate that an increase in the match rate by 25 cents per dollar of employee contribution raises 401(k) saving by $365 (in 1991 dollars). Overall, the analysis reveals that the 401(k) saving response to matching is quite inelastic, and, hence, matching is a rather poor policy instrument with which to raise retirement saving.

Keywords: Employer matching; Saving; Taxation; Private pensions

1. Introduction

As 401(k)s have come to dominate the private pension landscape in the United States, researchers and policy makers have given increased attention to the impact of plan characteristics on retirement-saving decisions.[1] One important characteristic is whether and to what extent the employer matches employee contributions. A typical match might be 50 cents for each dollar of contribution, up to a maximum percentage of pay, say, 6%. Although much of the discussion by the popular press and policy makers presumes employer matching raises saving, there is actually strikingly little consensus among researchers. Some studies have found that increases in the match rate raise 401(k) saving (Papke and Poterba, 1995; Clark and Schieber, 1998; Vanderhei and Copeland, 2001; Choi et al., 2002). Others have found that it is not the match rate *per se* that matters, but whether the firm offers a match at all (Even and Macpherson, 1996; Bassett et al., 1998; Papke, 1995; Kusko et al., 1998). That is, providing a match raises 401(k) saving, but an increase in the level of the match rate (conditional on providing a match) does not. Finally, still other studies (Employee Benefit Research Institute, 1994; Andrews, 1992; Munnell et al., 2002; General Accounting Office, 1997) have suggested that, conditional on being eligible for a match, an increase in the match rate *lowers* 401(k) contributions, which, when interpreted in the context of a simple two-period model of saving, suggests that the income effect dominates the substitution effect from the higher rate of return matching provides. Overall, this ambiguity has emerged as an important empirical puzzle in the literature on saving behavior (Bernheim, 2003).

Unfortunately, previous studies have had three important shortcomings. First, they have not couched their analyses in formal models of intertemporal choice, even though saving involves the substitution of resources across time. This means that previous estimates cannot be interpreted as estimates of life-cycle-consistent determinants of 401(k) saving necessarily, because the empirical specifications may not have been consistent with underlying utility maximization. So, while the existing literature has provided quite informative descriptive analyses, it has said little about how 401(k) saving may respond to prospective changes in employer matching or what the optimal match rate should be to achieve a saving target.

Second, with the exception of Choi et al. (2002), Mitchell et al. (2005), and Vanderhei and Copeland (2001), previous studies have failed to exploit the fact that multiple-match-rate schedules and caps on matching induce kinks in the budget set. As has been long recognized in the study of taxation on labor supply, reduced-form estimates of behavioral elasticities are biased and inconsistent unless the non-linearity is accounted for explicitly (Hausman, 1985; Moffitt, 1986, 1990; Blundell and MaCurdy, 1999). Indeed, the presence of budget-set kinks may reconcile some of the findings of previous studies: for example, the provision of a match may raise 401(k) saving if

[1] This includes work on automatic enrollment (Madrian and Shea, 2001; Choi et al., 2002, 2004), investment in company stock (Poterba, 2003; Brown et al., 2006; Mitchell and Utkus, 2002), portfolio choice and trading in 401(k) plans (Benartzi and Thaler, 2001; Agnew et al., 2003).

the substitution effect dominates, but variation in match rates may not matter if employees are bunched at kinks.[2]

Finally, previous research primarily has used nationally representative, individual-level survey data, such as the Current Population Studies (CPS) and Surveys of Consumer Finances (SCF), which are plagued by measurement error. In particular, even though the researcher must know the entire match schedule for a plan to account for the individual's full opportunity set, as well as whether the match is discretionary or through profit-sharing, the typical survey respondent has great difficulty in accurately conveying even relatively simple pension provisions to interviewers, no less detailed matching schedules. Self-reported contribution data suffer from substantial reporting error as well.[3]

Unlike previous studies, this paper uses the necessary conditions for optimal tax-deferred saving to derive a life-cycle-consistent econometric specification for 401(k) participation. As an alternative to the maximum-likelihood piecewise-linear-budget-set estimation summarized in Hausman (1985)–and the recent, related non-parametric extensions by Blomquist and Newey (2002)–and the maximum likelihood differentiable-budget-constraint methodology of MaCurdy et al. (1990), this paper employs instrumental-variable techniques that linearize the budget set at the observed outcome to calculate the price and virtual-income terms and then instruments to correct for endogeneity, which also has a long history, but a recent example of which is Ziliak and Kniesner (1999). To calculate budget-set slopes and virtual income in a neighborhood around kink points, kernel regression is used to smooth the budget set non-parametrically. We also estimate a censored regression model of 401(k) saving to decompose the overall 401(k) saving response between the extensive and the intensive margin, where the instrumental-variable Tobit estimator of Newey (1986, 1987b) and an instrumental-variable symmetrically censored least squares (SCLS) estimator based on Powell (1986) and Newey (1986) are used.

Empirically, the paper makes two additional contributions. First, to circumvent difficulties with measurement error in 401(k) contributions and matching incentives that have plagued previous studies, administrative data from three sources are used: contributions from W-2 earnings records provided by the Social Security Administration (SSA) and Internal Revenue Service (IRS); detailed matching formulas from pension Summary Plan Descriptions (SPD) provided by the employers of Health and Retirement Study (HRS) respondents; and, a combination of Social Security-covered-earnings histories for 1951–1991 and W-2 earnings for 1980–1991, pension SPDs, and pension-benefit calculators to construct public and private pension entitlements and accruals. The sample consists of 1042 individuals in 1991 eligible for 401(k) plans in the HRS.

Second, unlike previous pension studies that have used the employer-provided SPDs in the HRS, which are available only for a non-random sub-sample of HRS respondents, the estimation uses methods laid out in Vella (1992) and Das et al. (2003) to correct for potential sample selection bias using a set of plausible exclusion restrictions derived from Internal Revenue Service (IRS) Form 5500 administrative pension-plan filings. The exclusions have substantial predictive power for determining who is in the analysis sample. There is statistically significant evidence of selection, but the economic impact of the selection on the estimates is mixed: the bias

[2] To compare the non-linear budget set approach with that from the previous literature, we estimated a series of *ad hoc* reduced-form specifications similar in spirit to those in the literature, using the same estimators as used in the literature, but with the HRS data. We were able to replicate the puzzling array of results.

[3] See Mitchell (1988), Starr-McCluer and Sunden (1999), Johnson et al. (2000), Gustman and Steinmeier (2004), and Rohwedder (2003a, 2003b).

is small in the censored regression specifications of saving, but larger in the discrete choice participation specifications.

The estimates from the life-cycle-consistent discrete choice regression specifications for participation indicate that the estimated marginal effect of an increase in the employer match rate by 25 cents per dollar of employee contribution raises 401(k) participation by 5 percentage points. In addition, the parametric and semi-parametric estimates from the two-limit censored regression specifications indicate that the estimated marginal effect of an increase in the employer match rate by 25 cents per dollar of employee contribution raises 401(k) contributions by \$365 dollars (in constant calendar year 1991 dollars), with just under one half of this effect on the intensive (contributions conditional on participation) margin. Comparing the Tobit and SCLS estimates using the Hausman-type test in Newey (1987a), the validity of the Tobit model cannot be rejected.

There are three obvious limitations of these findings. First, the HRS focuses on older workers. Whether the estimates apply to younger workers is an open question. Second, this study has nothing to say about the broader question of to what extent 401(k) saving constitutes new private saving, a point of substantial debate in the literature. Third, to keep the model tractable, we have assumed full rationality in choices, perfect information, no fixed costs, and ignored other behavioral anomalies, such as inertia and passivity, that may be important determinants of 401(k) participation (Choi et al., 2002, 2003a,b,c, 2004a,b, 2005).

The paper is organized as follows. Section 2 describes the data and sample selection, and Section 3 provides selected descriptive statistics on employer matching. Section 4 lays out the econometric framework and construction of the key variables. The estimation results are discussed in Section 5. There is a brief conclusion.

2. Data and sample selection

The data are drawn 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 detailed questions about wealth, demographics, and spousal characteristics in 1992, and household income, tax information, and IRA contributions in 1991. The HRS collected SPDs, which are legal descriptions of pensions written in plain English, from employers of HRS respondents for all current and previous jobs in which the respondent was covered by a pension. The SPDs give the employer matching formula, which measures the exact incentives to contribute and helps to sidestep the problems with measurement error in the previous literature.[4]

Specifically, the job in which the respondent was employed in 1991 was identified and then SPDs associated with that job that had dates of adoption after 1991 were excluded. In addition, the date of last amendment and dates for changes in plan provisions indicated in the text of the SPD were used to exclude plans that were in existence in 1991 but whose features changed between 1991 and the time the SPD was collected.

The HRS also 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 1951–1991 and W-2 earnings records for jobs held from 1980–1991. They are the basis for two critical measures in the analysis dataset. First, the W-2s provide administrative data on earnings and 401(k) contributions for 1991. Unlike the contributions data used in previous studies, these data are not subject to measurement error, as they are the

[4] Engelhardt and Kumar (2006) explain in detail why self-reported pension information was not used and some other data limitations in the HRS.

employer's official report to the government on annual earnings and elective deferrals (Cunningham and Engelhardt, 2002). Second, when combined, the W-2 earnings histories, Social Security-covered-earnings histories, and self-reported earnings histories, allowed for the construction of complete earnings histories from 1951–1991 for each member of our sample. When used with Social Security and pension-benefit calculators, which are described in the Appendix, these data allowed for the calculation of the public and private pension wealth, accruals and changes in accruals, for 1991 and 1992, respectively, which are inputs into the net wage measure used below.

Overall, when all of the sources are combined, the data are a comprehensive description of the household's financial situation and exact pension incentives in 1991 and 1992 and a significantly richer data source than previous studies. The sample consists of 1042 HRS individuals eligible to contribute to a 401(k) in 1991.

One potential concern with this sample is that it may be non-random, because it is based on individuals for whom the HRS was able to obtain an SPD for the plan and administrative earning records. Although previous pension studies using these SPDs and earnings records have not corrected for selection because of the lack of plausible exclusion restrictions, two exclusion restrictions based on IRS Form 5500 data were used to estimate the specifications below using a number of methods 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. 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. 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. Finally, two other variables based on HRS data were used as exclusions in the selection equations: dummies for whether the individual left the job because the business closed or was laid off, respectively. These help to measure whether the employer possibly was in financial difficulty at severance, which, if that resulted in a business failure, would have made it more difficult for the HRS to have obtained an SPD. The construction of the exclusions and the selection equation are discussed in detail in the Appendix.[5]

3. Descriptive statistics on employer matching

Based on these data, Tables 1, 2 and 3 provide selected descriptive statistics on employer matching. First, many plans limit the amount of the match. These caps are usually expressed as a percent of pay in the SPD, but also can be a percent of contributions, and even a fixed-dollar amount. Table 1 shows the distribution of matching caps in the analysis sample, expressed as a percent of annual pay. About 19% of these plans had caps on employer matching that were less than 4% of pay. The median cap was 6% of pay, but 15% of plans had higher caps.

[5] The exclusions must be correlated with the likelihood of being in the sample, but uncorrelated with saving behavior. Because it is difficult to come up with exclusions that explain who consented to have their earnings records included in the HRS, we focused on exclusions that explain the likelihood of being linked to an SPD. Even though our exclusions focus on just one dimension of the selection, these variables are valid exclusions.

Table 1
Cap on matching contributions, as a percentage of pay, for plans that offer employer matching in the analysis sample

Cap on employer matching contributions as a percentage of pay	(1) Number of plans	(2) Percent of plans	(3) Number of individuals	(4) Percent of individuals
Less than 2%	7	3.3	10	2.7
2	11	5.3	12	3.2
2.5	1	0.5	1	0.3
3	19	9.1	24	6.5
3.75	1	0.5	4	1.0
4	23	11.0	40	10.8
5	17	8.1	53	14.2
5.5	1	0.5	1	0.3
5.7	1	0.5	1	0.3
6	56	26.8	109	29.3
Greater than 6%	32	15.3	57	15.3
No cap	41	19.6	60	16.1
Total	209	100.0	372	100.0

Note: Authors' calculations from the HRS restricted-access pension plan data for the 209 plans associated with the 372 of the 1042 HRS individuals in the analysis sample in plans with matching provisions.

Plans also vary according to the match rate. Table 2 shows the distribution of "first-dollar" match rates. Columns 1 and 2 indicate that these match rates were clustered at 25, 50, and 100%, where the median match rate was 50%. However, 27% of the plans offered matches of 100%, and three plans offered match rates of 200%.

Descriptive statistics for selected variables used in the empirical analysis are shown in Table 3. Column 1 shows sample means for the full 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 56.4% of the sample actively participated (defined as having made a contribution) in 1991. The sample mean 401(k) contribution in calendar year 1991 was $1377, but among contributors, the average contribution was $2446 (shown in column 4). Only 3% of the sample made the maximum contribution.[6]

A comparison of contributions between those without and with employer matching in row 1, columns 2 and 3, of Table 3, respectively, indicates that individuals with matching contributed just over $400 more on average than those without matching (*i.e.*, $1640–$1232 = $408). The difference in the median contributions between these two groups was $800. Therefore, just based on a comparison of means, it would appear that there is a small response of 401(k) saving to matching.

Columns 2 and 3 for the remaining rows in Table 3 also indicate that plans with employer matching differ along other dimensions that may make saving attractive. For example, if there is an employer match, the individual is much more likely to be able to borrow against the plan balance, direct the investment of plan balances, less likely to have another traditional pension

[6] After laying out the individual's entire budget set, we also explored whether they bunched at kinked points. We examined the distribution of 401(k) contributions for the match-eligible individuals in the sample in intervals of $200 relative to the kink amount. We found evidence of moderate bunching of contributions at the kink (measured as the value 0 on the horizontal axis). About 25% of match-eligible individuals locate at or within $400 of the kink on their budget sets. Considering that $400 is about 25% of mean 401(k) contribution, this interval is quite large and the amount of bunching not very significant relative to contribution. Since elasticities should be proportional to the amount of bunching (Saez, 2002), this level of bunching is not inconsistent with low elasticities that we estimate.

Table 2
Distribution of first-dollar match rates as a percentage of contributions

(1) First-dollar match Rate (%)	(2) Number of plans	(3) Percent of plans	(4) Number of individuals	(5) Percent of individuals
0 to 24	9	4.3	11	3.0
25	23	15.3	43	11.6
26 to 49	5	2.4	9	2.4
50	90	43.1	143	38.4
51 to 99	22	8.1	34	12.4
100	57	27.2	116	31.2
200	3	1.4	4	1.1
Total	209	100.0	372	100.0

Note: Authors' calculations from the HRS restricted-access pension plan data for the 209 plans associated with the 372 of the 1042 HRS individuals in the analysis sample in plans with matching provisions.

plan, more likely to have the plan annual contribution limit lower than the federal limit, and more likely to be allowed to make after-tax contributions to the plan.

Overall, there is substantial variation in incentives to contribute based on employer matching. The next section outlines how this variation is used to estimate the impact of employer matching on 401(k) saving using a theoretically coherent, non-linear budget-set econometric framework that accounts for the kinks induced by the caps on the generosity of the match detailed above.

4. Econometric framework

The two forms of tax-deferred saving for retirement available to workers are IRAs and 401(k)s. In each period t, there are minimum- and maximum-contribution constraints on tax-deferred saving (with multipliers in square brackets), respectively,

$$Q_t^{401(\mathrm{k})} \geq 0, \quad [\eta_t^0] \tag{1}$$

$$Q_t^{401(\mathrm{k})} \leq L_t^{401(\mathrm{k})}, \quad [\eta_t^L] \tag{2}$$

$$Q_t^{\mathrm{IRA}} \geq 0, \quad [\upsilon_t^0] \tag{3}$$

and

$$Q_t^{\mathrm{IRA}} \leq L_t^{\mathrm{IRA}}, \quad [\upsilon_t^L] \tag{4}$$

where $Q^{401(\mathrm{k})}$ denotes voluntary 401(k) contributions, Q^{IRA} denotes IRA contributions, and $L^{401(\mathrm{k})}$ and L^{IRA} are the upper limits on 401(k) and IRA contributions, respectively.

Now, consider the following marginal choice: the employee can contribute an additional dollar to the 401(k) financed by contributing one less dollar to the IRA. For an employee of a firm that does not offer matching contributions, the necessary condition for optimal tax-deferred saving is

$$p_t^{401(\mathrm{k})} V_y + (\eta_t^L - \eta_t^0) = p_t^{\mathrm{IRA}} V_y + (\upsilon_t^L - \upsilon_t^0), \tag{5}$$

where Vy is the marginal utility of income, y, and $p^{401(\mathrm{k})}$ and p^{IRA} are the tax prices of 401(k) and IRA saving, respectively. Intuitively, this equation says that the marginal value of an additional dollar of 401(k) saving must be just equal to that of a dollar of IRA saving at the optimum. In

Table 3
Sample means of selected variables in the empirical analysis sample, standard deviations in parentheses, medians in square brackets

Variable	(1) Full sample	(2) Subsample without matches	(3) Subsample with matches	(4) Subsample with positive contributions	(5) Subsample with zero contributions
401(k) contributions	1377	1232	1640	2446	0
(in 1991 dollars)	(1920)	(1895)	(1938)	(1982)	(0)
	[500]	[100]	[900]	[1892]	[0]
Match rate (in %)	23	0	65	28	17
	(37)	(0)	(32)	(38)	(33)
	[0]	[0]	[50]	[0]	[0]
After-tax wage	10.04	10.09	9.96	10.91	8.91
(in 1991 dollars per h)	(5.55)	(5.56)	(5.54)	(5.96)	(4.75)
	[8.92]	[9.12]	[8.51]	[9.66]	[8.23]
Age (years)	54.9	54.9	54.8	54.7	55.1
	(5.2)	(5.1)	(5.4)	(5.0)	(5.5)
	[55.0]	[55.0]	[55.0]	[55.0]	[55.0]
Education (years)	13.3	13.5	13.0	13.8	12.7
	(2.7)	(2.7)	(2.6)	(2.5)	(2.7)
	[13.0]	[13.0]	[12.0]	[14.0]	[12.0]
Percent female	47	47	47	48	45
Percent White	82	81	85	86	78
Number of dependents	0.70	0.68	0.75	0.71	0.70
	(0.93)	(0.93)	(0.94)	(0.95)	(0.91)
	[0.0]	[0.0]	[0.0]	[0.0]	[0.0]
Percent married	80	79	82	81	79
Spouse's Education (years)	10.6	10.6	10.6	11.0	10.1
	(5.5)	(5.7)	(5.2)	(5.5)	(5.5)
	[12.0]	[12.0]	[12.0]	[12.0]	[12.0]
Percent with plans that allow borrowing	36	19	68	42	29
Percent with plans that allow hardship withdrawals	4	4	5	6	2
Percent with plans that allow self-directed investment	63	46	92	66	58
Percent with other pensions at the firm	47	53	34	45	48
Percent with plan limit less than federal limit	80	73	92	76	85
Percent with plan that allows after-tax saving	23	9	47	26	18
Percent that had employer-sponsored retirement seminar	23	23	23	25	20
Percent with a spouse who has a pension	39	39	38	42	35
Percent in a union	34	39	27	28	43
Number of observations	1042	670	372	588	454

Note: Authors' calculations based on the sample of 1042 HRS individuals working in 1991 with matched employer-provided pension plan data and W-2 data, excluding those in plans with discretionary and profit-sharing-based employer matching provisions, as described in the text.

particular, on the left-hand side, an additional dollar contributed to the 401(k) costs $p^{401(\mathrm{k})}$, the tax price, and is valued (in utils) according to the marginal utility of income, V_y, plus the shadow value of the contribution constraints if the employee is at one of the two 401(k) corner solutions. On the right-hand side, one dollar contributed to the IRA costs p^{IRA}, the tax price, valued by the marginal utility of income, plus the shadow value of the contribution constraints if the employee is at one of the two IRA corner solutions. For an employee of a firm that offers matching contributions with a marginal match rate of m, this equation is modified slightly,

$$\left(\frac{1}{1+m_t}\right)[p_t^{401(\mathrm{k})}V_y + (\eta_t^L - \eta_t^0)] = p_t^{\mathrm{IRA}}V_y + (\upsilon_t^L - \upsilon_t^0), \tag{6}$$

to account for the fact that, with matching, only $1/(1+m)$ of a dollar must be foregone by the employee to get an incremental dollar in the 401(k) account. In the simplifying case of interior solutions for 401(k) and IRA saving, $\eta_t^0=0$, $\eta_t^L=0$, $\upsilon_t^0=0$, and $\upsilon_t^L=0$, Eq. (6) reduces to

$$\frac{p_t^{401(\mathrm{k})}}{1+m_t} = p_t^{\mathrm{IRA}}, \tag{7}$$

which indicates that what matters for the optimal decision are the relative prices (or, equivalently, relative net returns) to the two types of saving.

To derive the econometric specification for the determinants of 401(k) saving, let $\Delta\eta_t \equiv \eta_t^L - \eta_t^0$ and solve Eq. (6) for $\Delta\eta_t$ to yield

$$\Delta\eta_t = \Delta p_t \cdot V_y + p_t^m \Delta\upsilon_t, \tag{8}$$

where, $\Delta\upsilon_t \equiv \upsilon_t^L - \upsilon_t^0$, $p_t^m \equiv 1 + m_t$ and $\Delta p_t \equiv p_t^m p_t^{\mathrm{IRA}} - p_t^{401(\mathrm{k})}$. The tax price for IRA saving is defined as $p_t^{\mathrm{IRA}} \equiv 1 - \tau_t\zeta_t$, where τ is the marginal tax rate, and ζ is the fraction of an IRA contribution that is deductible. Importantly, ζ itself is a function of 401(k) participation, because 401(k) contributions reduce adjusted gross income (AGI), and AGI helps to determine the extent of IRA deductibility.[7] Consequently, the tax price for 401(k) saving is defined as $p_t^{401(\mathrm{k})} \equiv 1 - \tau_t (1 - \zeta_t' Q_t^{\mathrm{IRA}})$, where ζ' is the change in the fraction of an IRA contribution that is deductible for an additional dollar of 401(k) contribution.[8]

Next, let the indirect utility function take the following form,

$$V(\mathbf{q}, y; \mathbf{z}) = \Psi(\mathbf{z}) \cdot \frac{\ln(y) - \ln[a(\mathbf{q})]}{\ln[b(\mathbf{q})]}, \tag{9}$$

which is a member of the class of the PIGLOG indirect utility functions (Muellbauer, 1976), that has been used extensively in the literature on consumption. Following Blundell et al. (1994), Ψ is

[7] Specifically, wage and salary income that is electively deferred is not counted in AGI. For single individuals, IRA contributions in 1991 remained fully deductible if AGI was less than \$25,000, were linearly phased out for incomes between \$25,000 and \$35,000, and completely non-deductible for AGI above \$35,000. For married couples, contributions remained fully deductible if AGI was less than \$40,000, were linearly phased out for AGI between \$40,000 and \$50,000, and completely non-deductible for AGI above \$50,000.

[8] Eqs. (6) and (8) and the tax prices are derived formally from a very detailed dynamic stochastic model of consumer behavior that incorporates bequests, uncertainty, liquidity constraints, and asset allocation and location decisions. See the longer working-paper version of this article, Engelhardt and Kumar (2006), for details.

a utility scaling factor that is a function of exogenous demographic characteristics, $\mathbf{z}$. In Eq. (9), b is homogeneous of degree zero and modeled as a Cobb–Douglas price aggregator

$$b(\mathbf{q}) = \prod_k q_k^{\gamma k}, \tag{10}$$

across the k goods that enter the direct utility function, where $\sum_k \gamma_k = 0$. Because we assume only two goods enter direct utility, leisure (k=1) and consumption (k=2), respectively, this implies $\gamma_2 = -\gamma_1$, so that Eq. (10) can be re-written as

$$b(\mathbf{q}) = \omega^{\gamma k}, \tag{11}$$

where $\omega \equiv q^l/q^c$ is the real relative price of leisure. In addition, a is homogeneous of degree one. From Eq. (9), the marginal indirect utility of income, V_y, is

$$V_y = \frac{\Psi(\mathbf{z})}{y\ln[b(\mathbf{q})]}. \tag{12}$$

The scaling factor Ψ is modeled as

$$\Psi_i(\mathbf{z}) = \sum_m \psi_m z_{i,m}, \tag{13}$$

where $\mathbf{z}$ is an $m \times 1$ vector that includes a constant.

The second term on the right-hand side of Eq. (8) is zero when IRA saving is at an interior solution, positive when constrained by the upper IRA limit, and negative when at the lower IRA limit (of zero). So, let

$$\kappa \equiv p^m (D^{L^{\mathrm{IRA}}} - D^0), \tag{14}$$

where $D^{L^{\mathrm{IRA}}}$ is a dummy variable that is one if IRA contributions are at the upper limit and zero otherwise, and D^0 is a dummy variable that is one if IRA contributions are zero and zero otherwise.

Because, as illustrated above, there is a very small percentage of workers in the sample whose contributions equal the 401(k) plan maximum in the data, we first consider just the participation decision in the empirical analysis, so that $\Delta\eta$ collapses to $-\eta^0$, and Eqs. (8), (12), (13), and (14) combine to yield the following discrete choice econometric model

$$-\eta_{ijt}^0 = \sum_m \delta_{1m} z_{i,m} \frac{\Delta p_{ijt}}{y_{ijt}\ln(\omega_{ijt})} + \delta_2 \kappa_{ijt} + \alpha \mathbf{x}_{ijt} + u_{ijt} \tag{15}$$

and

$$D_{ijt}^{401(\mathrm{k})} = 1 \text{ if } Q_{ijt}^{401(\mathrm{k})^*} > 0 \Leftrightarrow -\eta_{ijt}^0 = 0$$

$$D_{ijt}^{401(\mathrm{k})} = 0 \text{ if } Q_{ijt}^{401(\mathrm{k})^*} = 0 \Leftrightarrow -\eta_{ijt}^0 < 0, \tag{16}$$

where $\delta_{1m} \equiv \psi_m/\gamma_1$, $Q_{ijt}^{401(\mathrm{k})*}$ denotes the desired 401(k) contribution, i and j index individuals and 401(k) plans, respectively, and u is the error term. Alternatively, this framework lends itself naturally to a censored regression model of the determinants of the dollar amount of 401(k) saving (*i.e.*, contributions), where at the upper contribution limit, $\Delta\eta>0$, $Q_{ijt}^{401(\mathrm{k})*}\geq L_{ijt}^{401(\mathrm{k})}$, but $Q_{ijt}^{401(\mathrm{k})}=L_{ijt}^{401(\mathrm{k})}$, and at the lower contribution limit $\Delta\eta<0$, $Q_{ijt}^{401(\mathrm{k})*}\leq 0$, but $Q_{ijt}^{401(\mathrm{k})}=0$. If an increase in the employer match raises participation, then the null hypothesis $\delta_0=\delta_{11}=\delta_{12}=\delta_{13}=\delta_{14}=\delta_{15}=\delta_2=0$ should be rejected, and the estimated marginal effects of participation and saving to the match rate should be positive, respectively.

In the empirical analysis, $\mathbf{z}$ includes a constant, the worker's education (in years), age, and dummy variables for whether the worker was married, White, and female, respectively. These demographic characteristics enter Eq. (15) parsimoniously and allow the impact of employer matching to be heterogeneous across demographic groups. The last term on the right-hand side of Eq. (15) includes $\mathbf{x}$, a vector that contains a constant and exogenous employer and employment characteristics. These are additional factors, explained below, that fall outside of the scope of the theoretical framework, but may affect contributions.

To estimate the parameters in Eq. (15), we employ an instrumental-variable technique that linearizes the budget set at the observed outcome to calculate the price and virtual income terms and then instruments to correct for endogeneity. In particular, for all observed 401(k)-contribution outcomes in the dataset, the tax and match prices, p^{IRA}, p^m, and $p^{401(\mathrm{k})}$, the net wage, ω, and income, y, must be calculated in order to construct the explanatory variables in Eq. (15). Because budget-set slopes are not defined at kink points, a variant of the method of MaCurdy et al. (1990) was used to calculate p^{IRA}, p^m, and $p^{401(\mathrm{k})}$ for each individual in the sample. Specifically, the matching formulas in the SPDs, tax-rate information from NBER's TAXSIM calculator, and detailed household financial and demographic characteristics were used to lay out the budget set in detail, then the kinks in the budget set were smoothed non-parametrically using kernel regression of the implicit (negative) tax rate from the employer matching and tax subsidy to contributions on AGI over the federal legally allowable range of 401(k) contributions of 0 to \$9500 using a second-order Gaussian kernel, with bandwidth chosen by Silverman's rule of thumb.[9] This regression was done on an individual-by-individual basis, so that the smoothing is individual-budget-set specific, and the estimates allow for budget-set slopes to be measured for those individuals located at kink points.

Income, y, is measured as "full income" (Blundell and MaCurdy, 1999). Because of the non-linear structure of matching and marginal tax rates, the tax and match prices, p^{IRA}, p^m, and $p^{401(\mathrm{k})}$, change depending upon the budget-set segment (either because the marginal match rate or tax rate changes), and, hence, the implicit tax liability from the employer-matching and tax subsidies will change depending upon the budget-set segment. Therefore, income is actually measured as "virtual" full income, y^v, according to the respective budget segment. The associated implicit tax liability used to virtualize income is calculated by numerically integrating the estimated kernel-smoothed implicit tax function described above.[10]

Unfortunately, the explanatory variables in Eq. (15) have components based on choice variables. Therefore, the instrument set, $\mathbf{Z}$, includes the vector of demographics, $\mathbf{z}$, and three additional variables, Z_{t-1}^{FC}, $p^{mz}\cdot p^{\mathrm{IRA}z}$, and $p^{401(k)z}$: the first is a dummy variable if the household was in poor financial condition in 1990, and the second and the third are based on "first-dollar" match and marginal tax rates for a synthetic taxpayer in 1989. There are two primary sources of variation in the instruments. First, p^{mz} varies by plan, j. That is, it is assumed that the variation in matching schedules across plans is exogenous. Second, $p^{\mathrm{IRA}z}$ and $p^{401(\mathrm{k})z}$ vary across synthetic

[9] See the Appendix for details.

[10] See Engelhardt and Kumar (2006) for details.

individuals because the tax function is non-linear in income and marital status. The Appendix gives a detailed description of the construction of the instruments and discusses the sources of identification.

5. Estimation results

The parameters in Eq. (15) are estimated by maximum likelihood, where $u \sim N(0, \sigma^2)$. Panel A of Table 4 presents parameter estimates with standard errors in parentheses.[11] Panel B lists the set of additional explanatory variables in the models. The marginal effect for a one unit change in the match rate represents an increase of one dollar of match per dollar of employee contribution and is shown in Panel C. The 95% confidence intervals for the marginal effects are given in square brackets.[12]

The choice of the baseline specification was guided by two practical concerns in the estimation that fall outside of the scope of the theoretical framework. First, firms may offer employer matching contributions as a way to try to avoid failing federal pension non-discrimination rules because they have low-saving employees (McGill et al., 1996). This would tend to bias downward the estimated impact of matching on participation. Second, firms that match may adopt other plan features to stimulate employee saving (*e.g.*, allow for borrowing against plan balances, self-directed investment, offer after-tax saving options, offer retirement seminars, *etc.*) or offer different fringe benefit packages that might affect saving behavior than firms that do not match. This would tend to bias upward the estimated impact of matching on participation.

The reduced-form relationship between employer match rates and these factors using the HRS data was examined in a companion paper, Engelhardt and Kumar (2004). As described there, the non-discrimination rules are set up so that employers with a greater proportion of workers with earnings large enough to be deemed “highly compensated employees” under federal law face greater pressure to meet non-discrimination rules if they offer a 401(k). In particular, a variable that measured the share of workers with earnings above the federal threshold for the definition of a “highly compensated” employee under federal non-discrimination regulations in the respondent’s Census-region-by-employment-size-category-by-one-digit-SIC-code-by-union-status cell in 1989 was constructed from the March CPS. This measure was then weighted by the difference in combined federal and state marginal tax rates on earnings for the median highly and non-highly compensated workers in the cell to reflect the value a highly compensated worker would put on a dollar of tax-deferred salary through a 401(k) relative to that for a non-highly compensated worker. This tax-difference-weighted share was used as a measure of the non-discrimination “pressure” faced by the typical employer in the respondent’s cell in a reduced-form model of the determinants of match rates in the HRS data.

The estimation results in Engelhardt and Kumar (2004) showed that the measure of pressure and other plan characteristics were highly significant. For example, the greater the pressure (tax-difference-weighted share) the more likely the respondent’s plan offered a match and the higher the match rate. Also, plans that allowed borrowing, self-directed investment, had other traditional

[11] The standard errors account for the presence of the estimated selection-correction and the use of instrumental variables.

[12] To calculate these intervals, we followed a nonparametric re-sampling procedure (Efron and Tibshirani, 1986) in the following steps. (1) Draw n observations randomly with replacement where n is the sample size. (2) Calculate the bootstrap version of the statistic of interest for each bootstrap sample. (3) Repeat steps (1) and (2) B times, where B was set to 199, to obtain an estimate of the bootstrap distribution. (4) Then construct the bias-corrected confidence intervals. Bias-corrected confidence intervals have desirable properties in finite samples (Efron and Tibshirani, 1986).

Table 4
Selected parameter estimates, marginal effects, and elasticities of 401(k) participation, standard errors in parentheses, bootstrapped 95% confidence intervals in square brackets

Explanatory variable	(1) IV probit with selection	(2) IV probit with selection	(3) IV probit with selection	(4) IV probit with selection	(5) IV Tobit with selection
A. Parameter estimates					
$\Delta p/y\ln(\omega)$	−7.209	−7.486	−8.926	20.056	−12087
	(8.004)	(7.504)	(6.933)	(44.878)	(15,811)
$Age \times \Delta p/y\ln(\omega)$	−0.010	0.044	0.059	−0.432	32
	(0.093)	(0.092)	(0.088)	(0.633)	(194)
Female $\times \Delta p/y\ln(\omega)$	0.857	0.913	1.054	−1.486	−2692
	(1.076)	(1.030)	(1.014)	(4.215)	(2173)
White $\times \Delta p/y\ln(\omega)$	1.057	1.006	1.233	0.700	2430
	(1.209)	(1.166)	(1.151)	(4.405)	(2523)
Married $\times \Delta p/y\ln(\omega)$	0.848	0.745	0.913	2.136	398
	(1.234)	(1.192)	(1.047)	(6.454)	(2501)
Education $\times \Delta p/y\ln(\omega)$	0.677	0.438	0.444	0.434	1290
	(0.188)	(0.190)	(0.190)	(0.668)	(405)
κ	0.293	0.129	0.024	4.757	1712
	(0.478)	(0.500)	(0.500)	(7.921)	(1064)
Selection term	−2.1514	−1.7861	−1.7494	−1.7549	−4172
	(1.2562)	(1.0074)	(1.0128)	(1.0136)	(2104)
p-Value for test of null of					
No match effect	0.0001	0.0001	0.0050	0.0100	0.0001
No selection	0.0868	0.0762	0.0841	0.0834	0.0476
Selection-equation exclusions:					
Plan outsourcing	−0.1173	−0.1321	−0.1320	−0.1321	−0.1200
	(0.0275)	(0.0278)	(0.0279)	(0.0278)	(0.0300
Plan consolidation	−0.1910	−0.1868	−0.1851	−0.1868	−0.1900
	(0.0402)	(0.0400)	(0.0400)	(0.0400)	(0.0400)
Business closure	−0.0530	−0.0481	−0.0483	−0.0481	−0.0500
	(0.0170)	(0.0172)	(0.0172)	(0.0172)	(0.0200)
Laid off	−0.0067	0.0018	−0.0013	0.0018	−0.0100
	(0.0240)	(0.0239)	(0.0239)	(0.0239)	(0.0200)
B. Additional controls					
Fringe benefit, and plan characteristics?	Yes	Yes	Yes	Yes	Yes
Interaction of firm size with fringe benefits and plan characteristics	Yes	Yes	Yes	Yes	Yes
Occupation and interactions of occupation with demographics, fringe benefits, plan characteristics, and other employment characteristics?	No	Yes	No	Yes	Yes
Liquidity constraints	No	No	Yes	No	No
Interactions of κ with demographics	No	No	No	Yes	No
C. Marginal effects					
Match rate (additional \$1 per \$1 contributed)	0.1833 [0.0017, 0.6552]	0.1997 [0.0173, 0.7522]	0.1830 [0.0178, 0.7800]	0.2251 [0, 1.0220]	1459 [829, 2749]

Table 4 (*continued*)

Explanatory variable	(1) IV probit with selection	(2) IV probit with selection	(3) IV probit with selection	(4) IV probit with selection	(5) IV Tobit with selection
C. Marginal effects					
Virtual full income	−0.0549	−0.0598	−0.0548	−0.0674	−437
(additional $100,000)	[−0.1963, −0.0005]	[−0.2254, −0.0052]	[−0.2337, −0.0053]	[−0.3063, 0]	[−824, −249]
Net wage (additional $1 per h)	−0.0018	−0.0019	−0.0018	−0.0022	−14
	[−0.0063, −0.00001]	[−0.0073, −0.0002]	[−0.0076, −0.0002]	[−0.0099, 0]	[−27, −8]
Marginal tax rate	0.1077	0.1173	0.1075	0.1322	857
(increase of 10 percentage points)	[0.0010, 0.3849]	[0.0102, 0.4419]	[0.0105, 0.4582]	[0, 0.6005]	[487, 1615]
Predicted probability of participation	0.88	0.81	0.82	0.87	0.64

Note: Columns 1–4 of panel A of this table present parameter estimates of Eq. (15) in the text for the discrete choice model of participation. Column 5 presents parameter estimates for the Tobit model of contributions. For all columns, the sample consists of 1042 individuals. All the columns assume prices, net wage, and virtual income are endogenous and correct for endogeneity by instrumenting using the instrumental variables described in the text and Appendix. All columns present selection-corrected estimates using the exclusion restrictions discussed in the text and the Appendix. The parameter estimates for the selection term in the participation equation are shown in panel A. Panel A also presents parameter estimates for the exclusion restrictions from the selection equations. Panel B presents the additional controls included. Marginal effects evaluated at the mean of the regressors are shown in panel C. For the marginal effects, bias-corrected bootstrapped 95% confidence intervals are reported in square brackets.

features, had limits less than the federal limit, and after-tax saving options had significantly higher first-dollar employer match rates, as was suggested in the comparison of unconditional means in Table 3 of the current paper.[13]

With this in mind, three groups of additional explanatory variables were included in the vector $\mathbf{x}$ in (15) for the baseline specification in column 1 of Table 4: 1) *fringe benefits offered*: 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; 2) *other plan characteristics*: dummy variables for whether the 401(k) allowed borrowing, hardship withdrawals, self-directed investment, had an after-tax saving option, a 401(k) contribution limit less than the federal limit, respectively, whether the firm offered other traditional pensions, and the measure of non-discrimination “pressure” described above; and 3) *additional 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, spousal pension coverage, as well as controls for firm size, Census division, and union status. These additional employment characteristics were interacted with the fringe benefit and plan characteristics described above to allow a more flexible functional form for $\alpha\mathbf{x}$ in Eq. (15).

The estimated marginal effects, calculated at the sample mean, for the match rate, income, net wage, and marginal tax rate from this baseline specification are shown in column 1. The estimated marginal effect of an increase in the match rate is an increase in participation of 0.18, or 18 percentage points, which is statistically different than zero, based on the *p*-value for the test of the null

[13] Another potential concern is that high-saving individuals, such as those with long horizons, might sort to firms that offer employer matching contributions (Ippolito, 1997). This would tend to bias upward the estimated elasticity of voluntary contributions to matching. However, the estimation results in Engelhardt and Kumar (2004) showed no correlation of the employer match rate with measures of the demographics and horizon and offered no support for endogenous sorting.

hypothesis of no impact of employer matching shown in panel A. Relative to the sample mean match and participation rates, this marginal effect implies an elasticity of participation with respect to the match rate of 0.048, suggesting that participation is very inelastic. The estimated marginal effect for virtual full income is negative, consistent with consumption and leisure as normal goods, statistically significantly different than zero, but the economic magnitude of the income effect is small.

Panel A also shows the parameter estimates on the exclusion restrictions in the selection model.[14] For the baseline in column 1, the p-value for the test of the null hypothesis that the exclusions jointly do not explain who has a matched SPD, though not shown in the table, is less than 0.01, which indicates that the exclusions have predictive power for who is in the sample. In particular, greater plan outsourcing, consolidation, business closure and layoffs significantly decrease the likelihood of having a matched SPD. The parameter estimate on the selection term in the participation equation is negative, and, based on the associated standard error, the null hypothesis of no selection bias can be rejected at the 8.7 percent level of significance. This indicates some evidence of selection: high savers are less likely to have an employer-provided SPD in the HRS, consistent with the reduced-form analysis of Gustman and Steinmeier (2004).[15]

5.1. Robustness checks and extensions

To allow for a significantly more flexible functional form for $\alpha\mathbf{x}$ in Eq. (15), in column 2, occupation dummies were added, the fringe benefit, plan, and other employment characteristics were interacted with occupation, and the other plan characteristics were interacted with the fringe benefit variables. Hence, the specification in column 2 is essentially fully interactive in the elements of $\mathbf{x}$. The estimated marginal effect to the match rate rises to 0.20. This estimate implies that for an increase in the match rate of 25 cents per dollar of employee contribution–the sample mean match rate–participation would rise by 5 percentage points.

As noted in Section 4, the demographic characteristics enter Eq. (15) parsimoniously, in a manner that allows the impact of employer matching to be heterogeneous across demographic groups. Across columns in Table 4, the demographic group for which the employer match consistently appears to have statistically significant differential effects on contributions is the relatively highly educated. To highlight any differences in responsiveness across groups, Table 5 shows the estimated marginal effect and elasticities for the employer match rate by sex and education group for the specification in column 2 of Table 4. In columns 1 and 2 of Table 5, an increase in the match rate of one dollar increases participation of females and males by an estimated 22.6 and 17.5 percentage points, respectively. However, the estimated 95% confidence intervals around these estimates overlap substantially and these effects are not statistically different from each other. In columns 3–7, the marginal effects rise sharply by education group.[16]

We carried out some further robustness checks. In column 3 of Table 4 we added liquidity-constraint variables to the specification, and in column 4 we present results with κ in Eq. (15)

[14] Parameter estimates for the full selection model are available upon request.

[15] To gauge whether this selection is economically important, we also estimated all specifications with instrumenting but without selection correction. The estimated marginal effect with respect to the match rate was about 50% larger than the selection-corrected one in column 2. This indicates that the bias from estimating the determinants of 401(k) participation on the selected sample of the respondents in the HRS who have matched SPDs has an important effect on the economic magnitude of the employer match estimates.

[16] In addition to these robustness checks, specifications were estimated based on the more general Box-Cox functional form for indirect utility from Blundell, Browning, and Meghir (1994) that nests the PIGLOG function we use. These results were very consistent with the results shown in the tables.

Table 5
Estimated marginal effects by demographic groups, bootstrapped 95% confidence intervals in square brackets

Panel	(1) Female	(2) Male	(3) High school dropout	(4) High school diploma	(5) Some college	(6) College degree	(7) Graduate school
A. Participation marginal effect with respect to the match rate	0.2259 [0.0081, 0.5900]	0.1750 [0.0012, 0.7843]	0.0670 [−0.1326, 0.7177]	0.1725 [0.0071, 0.6336]	0.2177 [0.0075, 0.5620]	0.2532 [0.0146, 0.8546]	0.2633 [0.0147, 0.8736]
B. Contribution marginal effect with respect to the match rate	1153 [742, 3013]	1752 [902, 3480]	396 [−232, 2484]	1178 [611, 3042]	1700 [1080, 3415]	2366 [1545, 3963]	2693 [1788, 4434]

Note: Panel A of this table shows estimated marginal effects for 401(k) participation with respect to the employer match rate by sex and education group based on the parameter estimates from the specification in column 2 of Table 4, evaluated at the sample means. Bias-corrected bootstrapped 95% confidence intervals are reported in square brackets. Panel B of this table shows estimated marginal effects for 401(k) contributions with respect to the employer match rate by sex and education group based on the parameter estimates from the specification in column 5 of Table 4, evaluated at the sample means.

interacted with demographics to allow the impact of IRA participation to vary across individuals.[17] Although the parameter estimates are a little less precise, the estimated marginal effects are similar to the previous specifications, indicating that our results are robust to a variety of additional controls.[18]

5.2. Estimates from the censored regression model

We also estimated Eq. (15) as a two-limit censored regression model of 401(k) saving in an instrumental-variable Tobit framework using the estimator of Newey (1986, 1987b), which allowed us to decompose the overall saving responses between the extensive and the intensive margins, respectively, based on the method of McDonald and Moffitt (1980). Column 5 of Table 4 presents a parallel set of results from the censored specification to those in column 2 of Table 4 for the discrete choice model of participation.[19] Overall, the general pattern of estimated responses is very similar to those from the binary choice specification in column 2. Panel C shows the total marginal effect, which includes the response on the extensive (participation) and intensive (contributions conditional on participation) margins. The estimated marginal effect of the match rate on contributions is \$1459 (column 5, panel C), with more than one half of the total effect on the extensive margin.[20] This marginal effect also implies that an increase in the match rate of 25 cents–the sample mean–raises contributions by just \$365.

Panel B of Table 5 shows the estimated marginal effect of the employer match rate by sex and education group on contributions obtained from this Tobit model. In columns 1 and 2, an increase in the match rate of one dollar increases contributions of females and males by an estimated \$1153 and \$1752, respectively. The results for the education groups echo those from the discrete choice model of participation in Panel A, with marginal effects rising sharply with education.

In addition, we used an instrumental-variable version of the Symmetrically Censored Least Squares (SCLS) estimator of Powell (1986) and Newey (1986, 1987b). The primary advantage of this semi-parametric estimator is that it robust to heteroscedasticity and any departures to normality that were assumed for the Tobit error term. The latent marginal effects from the parametric and semi-parametric estimates were similar, and based on a Hausman-type test (Newey, 1987a), we could not reject the IV Tobit model in favor of the IV SCLS specification. Therefore, we do not present the results from IV SCLS estimation, but they are discussed in detail in Engelhardt and Kumar (2006).

6. Summary and implications

Previous studies have produced a puzzling array of estimates of the impact of employer matching on 401(k) saving. This probably stemmed from the use of less than ideal data and, more

[17] Our set of explanatory variables that measures (to varying degrees) the ability to borrow has been used by others in the previous literature on liquidity constraints and includes: dummy variables for whether the household has no capital income; has access to borrowing against home equity through a home equity line of credit, conditional on being a home owner; has experienced financial distress in the past due to unanticipated medical expenses and unemployment, respectively; and has access to informal private support from friends and family if under financial distress.

[18] In column 4, the 95% confidence interval for the match rate marginal effect now includes zero. However, the 90% confidence interval (not shown) does not, so that the implied marginal effect is significant at the 10% level.

[19] The estimation uses methods laid out in Vella (1992) and Das et al. (2003) for the Tobit and SCLS estimators, respectively, to correct for potential sample selection. Parameter estimates for the full selection model are available upon request. The first-stage regressions for the endogenous variables in Eq. (15) were selection corrected as well.

[20] Relative to the sample mean match and participation rates, this marginal effect implies a total elasticity of contributions with respect to the match rate of 0.12, that can be decomposed into an extensive-margin (participation) elasticity of 0.066 and an intensive-margin elasticity (contributions conditional on participating) of 0.054.

importantly, the failure to incorporate into estimation match-induced kinks in the budget set. In this analysis, based on the life-cycle consistent specification derived, participation and contributions are quite inelastic with respect to employer matching.

Subject to the caveats stated in the Introduction, there are a number of potential implications of these findings. First, because of the estimated inelastic response, the analysis reveals that for employers and policy makers interested in promoting retirement saving by older workers through greater 401(k) participation and saving, matching is a rather poor public policy instrument. However, employers who worry about satisfying non-discrimination criteria may place greater value on these increases in participation. Second, a number of commonly advocated reforms to Social Security call for the introduction of voluntary private accounts, to which individuals could choose to contribute additional funds toward Social Security. Under some proposals, the federal government would match those contributions as an incentive. In designing such a system, it would be instrumental for policy makers to know how individual contributions would respond to the government match. Our analysis suggests that government matching of voluntary contributions to any type of Social Security personal account would be relatively ineffective in promoting personal-account contributions (Engelhardt and Kumar, 2005). Third, beyond personal accounts, there is substantial policy interest in the government provision of matching contributions designed to stimulate targeted forms of saving among lower income households, which has led researchers to evaluate the impact of Individual Development Accounts (IDA) (Mills et al., 2006), federal programs for matching IRA contributions, and the adoption of the Saver's Credit (Duflo et al., 2006). In particular, the Mills et al. and Duflo et al. analyses, which were based on randomized field experiments, have shown substantially more elastic responses from matching contributions on IDA and IRA contributions, respectively.

Our reading of these two studies suggests that their findings are difficult to extrapolate to employer matching contributions in corporate 401(k) plans for three reasons. First, eligible employees in 401(k) plans are not typically lower income individuals. They are more likely to be substantially better off economically. For example, individuals in the Mills et al. analysis had incomes below 150% of the poverty line. Second, institutional features differ substantially. For example, contributions to a 401(k) plan that are matched are typically done through automatic payroll deduction, whereas contributions to IDAs and IRAs in the Mills et al. and Duflo et al. analyses were not. Third, the matching contributions in the Duflo et al. analysis were offered as part of an unannounced, take-it-or-leave-it decision, whereas employees in a 401(k) can respond to a match by contributing at any point in a given calendar year, or even a different calendar year, without foregoing the option of being eligible for a match. Overall, although these are fascinating studies that will have important implications for policy targeted to lower income households and, through randomization, have convincingly addressed important issues in the econometric identification of the causal effects of matching, these studies are of limited use in formulating policy concerning 401(k) plans.

Finally, a number of prominent companies have reduced or eliminated matching contributions in the last few years due to declining profits. Although it remains to be seen if this is a long-term trend, understanding the impact of matching is critical to understanding the impact of these changes on retirement income security for a workforce increasingly dependent on 401(k) plans for retirement. The fact that the estimated response of contributions to the employer match in this paper was quite inelastic suggests that overall 401(k) activity at these firms might not be greatly affected by these changes in matching.

Appendix A

This appendix describes the construction of and gives background on the analysis dataset. The interested reader also should consult the working-paper version of this article, Engelhardt and Kumar (2006). The data are drawn 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 detailed questions about wealth, demographics, and spousal characteristics in 1992, and household income, tax information, and IRA contributions in 1991. So, for the purposes of the empirical analysis, periods t and $t+1$ refer to 1991 and 1992, respectively.

The sample consists of 1042 individuals from wave 1 of the HRS who were employed in 1991, eligible for a 401(k), whose employer provided a SPD for the plan, and who had linked administrative W-2 and Social Security earnings data. The restricted-access employer-provided SPDs are distributed as the *HRS Wave 1 Pension Plan Detail Data Set* (Gustman et al., 1999). This dataset contains plan type, eligibility rules, benefit formulae, employer contribution and matching formulae, early and normal retirement dates, and other information described in the SPD, but not any information for individual employees. The W-2 data are distributed as the *HRS Wages and Self-Employment Income in Covered and Non-Covered Jobs* dataset (Mitchell et al., 1996). The dataset is a cross-section for 1991 because even though there are earnings and deferral data prior to 1991, there are no data on other income and wealth needed to construct full income prior to 1991 in the HRS. Some of the individuals in the sample worked in 1991 but were retired at the time of the first interview in 1992. Exclusion of these individuals had no impact on the estimation results.

There are four types of employer matching: fixed-rate, discretionary, profit-sharing, and variable-rate matching. Because the extent of matching is not always known in advance to employees making deferral decisions in profit-sharing and discretionary plans, these plans were not included in our sample. The SPDs were used to construct the complete schedule of employer matching contributions for each individual in our sample and applied all relevant restrictions on plan eligibility in the SPD, including those due to tenure, hours, earnings, age, and vesting of the employer matching contributions.

Because workers' budget sets can have multiple kinks and, therefore, multiple points of non-differentiability, from changes in match and marginal tax rates, a smooth, differentiable budget set around all kink points was constructed, following the methodology of MaCurdy et al. (1990). Specifically, kernel regression of the implicit subsidy from employer matching and tax deductibility on the set of potential contributions from 0 to \$9500 (the federal maximum contribution in 1991) by \$50 increments was used to smooth the budget set, using the Gaussian kernel, $K(z) = (1/\sqrt{2\pi})e^{-z^2/2}$, with bandwidth chosen by Silverman's rule of thumb, $h=1.06m/n^{1/5}$, where $m = \min(\sqrt{\mathrm{var}_x}, \mathrm{iqr}_x/1.349)$ and iqr_x is the inter-quartile range. A smooth marginal implicit subsidy function was constructed from the kernel-regression estimates.

From the notation of the complete theoretical model detailed in Engelhardt and Kumar (2006), full income, y, can be expressed as

$$y_{it} \equiv \Delta A_{it}^{\mathrm{TA}} + w_{it}L^1 + y_{it}^{\mathrm{o}} - Q_{it}^{401(\mathrm{k})} - Q_{it}^{\mathrm{IRAC}} + Q_{it}^{\mathrm{IRAW}} - T_{it}, \tag{A.1}$$

where A^{TA} is taxable assets, w is the gross wage, L^1 is the leisure endowment, y^{o} is other income, Q^{IRAC} is IRA contributions, Q^{IRAW} is IRA withdrawals, and T is taxes paid. The full income measure includes the market value of the leisure endowment. Under two-stage budgeting, the capital income and net (dis-)saving terms embodied in the change in assets, ΔA, are sufficient statistics for the past and the expectations of future variables (Blundell and MaCurdy, 1999). The respondent-reported income in wave 1 of the HRS referred to behavior in calendar year 1991; hence, ΔA was formed by using taxable wealth in 1992 taken from wave 1, taxable wealth in 1991, which was capitalized from 1991 capital income, a technique that is commonly used in the literature, and R^{TA} constructed from a weighted-average gross return based on returns in Ibbotson (2003). Tax rules from 1991 were used to construct an IRA phase-out calculator to determine ζ and ζ', and household income, tax, and demographic data and NBER's TAXSIM calculator (Feenberg and Coutts, 1993) were used to construct marginal tax rates and taxes paid for each household. The estimated kernel-regression function was numerically integrated to obtain the dollar amount of implicit subsidy for every level of potential 401(k) contribution, which, with full income, was used to construct virtual full income along all portions of the budget sets.

The construction of the net wage, ω, is described in detail in Engelhardt and Kumar (2006). For the private and public pension components in ω, individuals were divided into cells based on exogenous demographic characteristics and the Social Security-covered earnings from 1951–1991 and W-2 earnings records from 1980–1991 were used to calculate earnings histories for a synthetic-cell individual. These synthetic earnings histories were input as follows: 1) into the University of Michigan's *Pension Estimation Program* to calculate defined benefit pension wealth, accrual, and change in accrual for additional earnings for individuals with DB plans; 2) into the *HRS DC/401(k) Calculator* (Cunningham et al., in press) developed to calculate for individuals with defined contribution plans their DC pension wealth, non-matching contributions and the effect of additional earnings thereon, respectively; the impact of additional earnings on employer match on voluntary contributions; required 401(k) contributions and the impact of additional earnings thereon, respectively; and, 3) into the Social Security benefit calculator developed by Coile and Gruber (2000) to calculate Social Security wealth, accrual, and change in accrual for additional earnings. The effect of additional earnings on the employer match to voluntary contributions was calculated assuming a 401(k) contribution of 50 dollars for all individuals (regardless of actual contribution level).

Finally, the sample is likely non-random because it is based on individuals for whom the HRS was able to obtain 1) an employer-provided SPD for the 401(k) plan, and 2) permission from the individual to match SSA covered earnings and IRS W-2 earnings histories. To understand the exclusion restrictions that were developed, it is useful to note the manner in which the HRS obtained the SPDs and administrative earnings data. 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%: 65% of those currently working in pension-covered jobs, 66% for the last job for those not working, and 35% for jobs held 5 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. Two other variables were used as exclusions for pensions on past jobs in our selection equations: dummies for whether the individual left the job because the business closed or was laid off, respectively. These helped to measure whether the employer possibly was in financial difficulty at severance, which, if that resulted in a business failure, would have made it more difficult for the HRS to have obtained an SPD.

There were three important considerations in constructing the instruments. First, the instruments were drawn from the information set Ω_t. Because t is 1991, all information from 1989 and 1990 is in the information set and orthogonal to decisions made in 1991 under rational expectations. Second, because the observed marginal match and tax rates depend upon 401(k) and IRA contributions, $p^{mz} \cdot p^{\mathrm{IRA}z}$ and $p^{401(\mathrm{k})z}$ are based on first-dollar measures: the employer match on the first dollar contributed and the marginal tax rate at which the first dollar contributed is deductible (which equals the tax rate on the last dollar of earnings). Third, to minimize dependence on individual-specific income and family size that might be correlated with saving behavior, the first-dollar rates were calculated for a synthetic individual of each marital status assumed to have no capital income, no children, under the age of 65 years, and taking the standard deduction–where marital status is assumed exogenous–with synthetic annual labor earnings constructed as follows: individuals were divided into cells based on exogenous demographic characteristics, and the cell mean gross hourly wage rate, $\overline{\mathrm{w}}_{\bullet t-2}$, was multiplied by 2000 annual hours, $\overline{\mathrm{H}}$. Let the subscript $\bullet$ denote a synthetic measure and the superscript 0 denote a first-dollar measure, then

$$p_j^{mz} \cdot p_{t-2}^{\mathrm{IRA}z} \equiv \left(1 + M^{V0}_{\underset{Q_j}{401\mathrm{k}}\,_j}\right) \cdot \left(1 - T^0_{It-2}\zeta^0_{t-2}\right) \tag{A.2}$$

and

$$p_{\bullet t-2}^{401(\mathrm{k})z} \equiv 1 - T_{I\bullet t-2}^{0}\left(1-\zeta_{y^1\bullet t-2}^{0}\,\overline{Q}^{\mathrm{IRA}}\right), \tag{A.3}$$

where $\overline{Q}^{\mathrm{IRA}}$ was set to one dollar for all individuals.

It is important to note that the tax function for Δp in the endogenous variable is based on the tax system in 1991, but the tax function for the instruments is different because it is based on the tax system in 1989 (indicated by the subscript $t-2$ in Eqs. (A.2)–(A.3) above). For individuals with AGI below $50,000, the functions were essentially the same, but differed for those above this level. Specifically, above this income level in 1989, the marginal tax rate increased from 28 to 33% due to the phase-out of the personal exemption. However, the Budget Act of 1990 raised the top marginal tax rate to 31% and changed the phase-out of the personal exemption. Therefore, the non-linearity in the instruments' tax function differs from that for the endogenous regressor due to the tax-law change, which is taken as exogenous to the individual. About 15% of the sample is affected by this differential non-linearity in the instruments.

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