Match Rates and Savings: Evidence from Individual Development Accounts

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Abstract

How do people respond to matched-savings incentives? Studies of 401(k) plans find that matching increases participation but that higher match rates do not increase—and may decrease—the level of savings. This paper analyzes saving by low-income people in Individual Development Accounts (IDAs), a new savings incentive that matches withdrawals if used for home purchase, post-secondary education, or self-employment. The model controls for several sources of bias common in estimates of match-rate effects: unobserved heterogeneity among firms and among participants, censoring of savings at the match cap, and an inverse relationship between match rates and match caps. In IDAs, higher match rates are associated with an increased probability of continued participation but also with a decreased level of savings.

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1. Introduction

The federal government spends billions each year on tax breaks for Individual Retirement Accounts (IRAs) and 401(k) plans. These incentives, however, are weak for low-income people in low tax brackets. Individual Development Accounts (IDAs) are a new savings incentive targeted to the poor (Sherraden, 1991). IDAs provide matches on withdrawals for home purchase, post-secondary education, or self-employment. How do people respond to the increased rate of return in this matched-savings structure?

For the level of savings, theory is ambiguous; the substitution effect and the income effect work against each other. For participation, the income effect vanishes, so higher rates of return should increase participation.

Empirical research takes two tacks. The first looks at the use of 401(k) plans and/or IRAs and total savings. Perhaps one-third of contributions is new savings due to incentives, with the strongest effects for low-income people (Engen and Gale, 2000; Bernheim, 1997; Hubbard and Skinner, 1996). Conclusive measurement has been thwarted by a lack of data on all assets and debts for people with exogenous differences in access to savings incentives.

The second tack looks at rates of return for specific types of savings. The empirical record suggests that household savings does not respond much (if at all) to the rate of return, perhaps because much saving is precautionary (Bernheim, 1999;

Deaton, 1992). Although the data preclude conclusive measurement, again effects seem strongest for poor people (VanDerhei, Copeland, and Quick, 2000).

This paper takes this second tack; it looks at rates of return (match rates) and a specific type of savings (IDAs). It avoids some (but not all) of the technical weaknesses of past work on 401(k) plans.

Higher match rates are associated with an increased likelihood of continued participation in IDAs. If the goal is to include more poor people in savings incentives, then higher match rates worked. A higher match rate, however, was also linked with decreased savings. This decrease might be real (income effects or target-saving effects) or apparent (endogeneity of match rates and saving capacity). If the decrease was real and if the goal was to boost aggregate personal savings, then higher match rates for IDAs did not work; decreased savings more than offset increased participation. Even though higher match rates decreased savings, they still boosted asset accumulation (savings plus match). Thus, they may have facilitated the purchase of a big-ticket item that might have led to discrete improvements in long-term well-being and capacity.

Section 2 describes IDAs. Section 3 reviews the measurement of match-rate effects in the presence of match caps, endogeneity, unobserved heterogeneity, and censoring. Section 4 presents the data, model, and results, and Section 5 discusses potential policy implications.

2. Individual Development Accounts

Individual Development Accounts (IDAs) are matched-savings structures for the poor. Withdrawals from IDAs are matched if used for home purchase, post-secondary education, or self-employment. Participants also receive financial education and support from peers and program staff. IDAs aim to include the poor in savings incentives, to increase savings by the poor, and to boost ownership by the poor.

Sherraden (1988) proposed IDAs. The concept of asset-based policy has since gained intellectual momentum (Shapiro and Wolfe, 2001; Ackerman and Alstott, 1999; Conley, 1999; Stoesz and Saunders, 1999; Oliver and Shapiro, 1995). It has also attracted broad political support. For example, IDAs are part of policy in most states, and Bill Clinton proposed national IDA-like accounts. Both George W. Bush and Al Gore had IDA proposals in their platforms, and a current bill (the Savings for Working Families Act) would budget up to $10 billion for IDAs. The government of Canada will sponsor an IDA demonstration in ten cities, and the government of the United Kingdom has proposed IDA-like accounts.

IDAs are unique in that they are aimed at the poor, they subsidize non-retirement savings, and they provide an explicit match. The rest of this section compares and contrasts IDAs with Roth IRAs and 401(k) plans, two other common matched-savings structures (Table 1, non-Roth IRAs are omitted to save space).

Eligibility for IDAs is income-tested. Like Roth IRAs (which have universal eligibility), deposits are supposed to come from earned income. Unlike 401(k) plans, access and eligibility to IDAs are not provided through an employer.

Roth IRAs and 401(k) plans provide matches indirectly via tax breaks. Tax deductions are worth little to poor people, so IDAs make direct matches. Matches in IDAs are typically much higher than in 401(k) plans or Roth IRAs. Funds for IDA matches and administrative expenses may come from private or public sources, whereas the government pays for matches in Roth IRAs and 401(k) plans.[1] Savers bear administrative expenses for Roth IRAs, and employers bear them for 401(k) plans.

IDAs are held in standard passbook savings accounts in banks, whereas Roth IRAs and 401(k) plans are held in restricted-access accounts, often in mutual funds. IDA deposits may be withdrawn at will and come from after-tax dollars (like Roth IRAs but unlike 401(k) plans) and earnings are taxed (unlike Roth IRAs or 401(k) plans).[2] Withdrawals from IDAs for purposes other than home purchase, post-secondary education, or self-employment are not matched but are not otherwise penalized; early distributions from Roth IRAs are subject to an excise tax, and early distributions from 401(k) plans are subject to income tax as well as an excise tax.

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[1] Matches from employers in 401(k) plans are not subsidies but part of compensation.

[2] The Internal Revenue Service has not yet ruled on the tax status of IDA matches.

IDAs—unlike Roth IRAs and 401(k) plans—are meant primarily for pre-retirement asset purchases rather than retirement consumption. This is the aspect in which IDAs differ most from Roth IRAs and 401(k) plans.[3]

IDA programs (so far at least) are administered by not-for-profit organizations. The not-for-profit sets the match cap, which may be annual or for a span beyond one year. In contrast, 401(k) plans are sponsored by employers and have annual match caps set by the employer and/or by law. Roth IRAs are administered by financial-service organizations and have annual match caps set by law. Payroll deduction is required for 401(k) plans and is possible—but not required—for IDAs and Roth IRAs. Finally, financial education is required in IDAs but is voluntary (if available) in 401(k) plans and Roth IRAs.

[3] IDAs and their subsidies for savings for asset purchases in the near term resemble a program that subsidized savings for a down payment on a home in Canada from 1974 to 1985. Unlike IDAs, the Canadian program matched through tax breaks, so most participants were in high tax brackets (Engelhardt, 1996).

3. Measurement of match-rate effects

The ideal test of the effects of match rates would measure participation, savings, and all personal and program characteristics for people with exogenous differences in match rates. No study (including this one) achieves the ideal. This section reviews past work and argues that their estimates of match-rate effects are biased because they fail to control for match caps, censoring, endogeneity, and unobserved heterogeneity.

3.1 Research on matched savings in 401(k) plans

3.1.1 The first studies

Early studies of match rates use firm-level data. GAO (1988, cited in Poterba, Venti, and Wise, 1994) tabulates participation and savings by match rate and find that both outcomes increase with the presence of a match (perhaps at a decreasing rate) and with the match rate. Neither study uses statistics nor controls for variables that might cause spurious correlations between the match rate, participation, and/or savings.

Three other early studies use statistics with firm-level data. Correlation analysis of employees who earned $32,000 a year or less at 24 firms in 1995 showed that participation (but not savings) increased with the match rate (Clancy, 1996).[4] Papke, Petersen, and Poterba (1996) and Papke and Poterba (1995) use regressions with one to five control variables from a 1987 survey of 43 firms. Both studies find that

[4] Clancy (1996) does not explicitly say whether individual or firm data were used.

participation increases with matching. Papke and Poterba (1995) also find that higher match rates—once there is a match—are not linked with savings.

These early studies point in one direction, but they use small samples, simple methods, and firm-level data that may mask individual behavior. A second set of studies uses larger samples, more control variables, and sometimes data on individuals.

3.1.2 Second-generation research

Papke (1995) uses pooled regressions with panel data on 3,565 to 5,363 firms from 1985-1987.[5] With higher match rates, participation increases (at a decreasing rate), and savings increase (at very low match rates) and then decrease. The effects on diminish or vanish with controls for unobserved firm-level heterogeneity.

Bayer, Bernheim, and Scholz (1996) use a panel of 1,100 firms in 1993-94. In pooled regressions with nine control variables, the presence of a match is linked both with increased participation and increased savings. Again, the effects shrink or vanish with controls for unobserved firm-level heterogeneity.

Andrews (1992) regresses participation and savings on the presence of a match and on eleven other variables for 3,884 individuals from the May 1988 Employee Benefit Supplement of the Current Population Survey (CPS). Matching is associated with increased participation but with decreased savings.

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[5] The results are weakened because match rates are imputed from Form 5500 data.

Bassett, Fleming, and Rodrigues (1998) use the April 1993 CPS. Regression with eight controls suggests that participation increases in the presence of a match but that the level of the match rate does not matter. They also show simple tabulations in which contribution rates decrease as match rates increase.

Clark and Schieber (1998) use regressions with five controls for 60,919 people at 19 firms in 1994. Higher match rates increase both participation and contributions. Clark *et al.* (2000) use similar data for 156,376 people at 87 firms in 1995. They find that higher matches increase participation but decrease contributions.

Munnell, Sundén, and Taylor (2000) use the 1998 Survey of Consumer Finances (SCF). In a regression with 8 controls and 1,232 participants, matching is linked with increased savings but, higher match rates are linked with decreased savings.

Like early studies, this second generation finds that the presence of a match increases participation. With their improved data and technique, however, some find that higher match rates do not increase—and may decrease—the level of savings.

3.1.3 Third-generation research

The best, most-recent studies use individual data, control for moderate numbers of other variables, and account for match caps and/or possible endogeneity of match rates with unconditional savings. On the whole, they still find that a match increases participation, but some also find that higher match rates may decrease savings.

Even and Macpherson (2001) use 1993 CPS data and control for 18 variables and for the chance that firms with unconditionally low savers are more likely to introduce matching (or to increase the match rate) to boost participation and savings. Such endogeneity—if it exists—would tend to mask any positive effects of matching. They find much larger match-rate effects on participation than in previous studies.

VanDerhei, Copeland, and Quick (2000) use the best data yet: characteristics for 137 plans and their 163,346 participants. Besides age, wage, and tenure, they control for the match cap (although they do not interact it with the match rate). They find that higher match rates are linked with decreased savings and that higher match caps are linked with increased savings. This suggests, for example, that a total potential match of 4 percent of salary would elicit more savings as a 50-percent match rate on up to 8 percent of salary than as a 100-percent match rate on up to 4 percent of salary.

3.1.4 Summary

The best work on 401(k) plans suggests that the presence of a match increases participation. Once there is a match, participation increases at a decreasing rate. Higher match rates do not increase savings and may even decrease it.

That the presence of a match should increase participation and savings is not a surprise; only the substitution effect is at work. That higher match rates would decrease savings is theoretically possible—the result of income effects or target-saving—but is still somewhat of a surprise. The negative effect, however, may reflect bias from failure

to control for censoring, endogeneity, unobserved heterogeneity, and/or the negative correlation between match rates and match caps.

3.2 Match caps

To show how match caps may affect estimates of match-rate effects, Figure 1 depicts budget sets for a person with an IDA. On the horizontal axis, savings increase from left to right, and consumption increases from right to left. The vertical axis shows asset accumulation (savings plus match). Utility increases to the northwest.

3.2.1 Match rates and match caps

In an IDA, the *match rate* is the number of dollars disbursed to a vendor for each dollar withdrawn by the participant for a matchable purchase.[6] In Figure 1, the match rate is the slope of the budget line minus unity, that is, 0:1 for **OC**, 0.5:1 for **OA**, and 1:1 for **OB**.

Match caps are limits on matchable deposits. IDAs cap the number of matchable dollars, although participants are free to put non-matchable dollars in the same passbook savings account.[7] In Figure 1, **D** is a match cap.

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[6] In a 401(k) plan, the match rate is the number of dollars from the employer for each dollar from the employee. Tax breaks for 401(k) plans are equivalent to matches, even though they are not put in 401(k) plans. In this sense, research on 401(k) plans that ignore tax breaks understate the match rate. This study also ignores taxes, but most IDA participants are in low tax brackets; only people with income under 200 percent of poverty qualified for ADD, and the median participant was at the poverty line.

[7] 401(k) plans have up to two match caps. The employer matches contributions up to the employer-match cap, and the government gives tax breaks on contributions up to

3.2.2 Potential matches and potential asset accumulation

The *potential match* is the product of the match cap and the match rate:

$$\text{Potential match} = \text{Match cap} \cdot \text{Match rate}. \tag{1}$$

In Figure 1, the potential match (match rate 0.5:1, match cap **D**) is **AE**. A given potential match may result from a range of pairs of match rates and match caps. For example, a $500 potential match may result from a 0.5:1 match rate with a $1,000 match cap or from a 1:1 match rate with a $500 match cap. In 401(k) plans, match rates and match caps are negatively correlated (VanDerhei, Copeland, and Quick, 2000; Clancy, 1996; Papke, Petersen, and Poterba, 1996).

Potential asset accumulation is the match cap plus the potential match:

$$\begin{aligned} \text{Potential asset accumulation} &= \text{Match cap} + \text{Potential match}, \\ &= \text{Match cap} + (\text{Match cap} + \text{Match rate}), \\ &= \text{Match cap} \cdot (1 + \text{Match rate}). \end{aligned} \tag{2}$$

In Figure 1, potential asset accumulation with a match rate of 0.5:1 and a match cap of **D** is **AD**. Potential asset accumulation especially matters for target-savers. IDA participants plan to purchase a big-ticket item and so may be particularly likely to target-save,[8] but many retirement savers also target-save (Bernheim, 1999).

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the IRS match cap. Furthermore, specific plans may limit contributions at some point beyond the employer-match cap but before the IRS-match cap. Employer caps limit salary-deferral percentages, and IRS caps limit numbers of dollars.

[8] Bernheim and Scholz (1993) suggest that target-saving predominates for poor people.

Together, match rates and match caps determine the potential match and potential asset accumulation. Theory predicts two types of effects on participation and savings due to changes in match rates and/or match caps: economic effects (substitution and/or income), and behavioral effects.

3.2.3 Economic effects

The economic theory of matched-savings structures discussed below highlights that the analysis of match-rate effects must control for variation in match caps.

3.2.3.1 Participation

Participation is all-or-none, so match rates exert only substitution effects. For the example of a match rate of 0:1 (budget **OC** in Figure 1), the northwest-most indifference curve ($\mathbf{I_0}$) is tangent to the budget at **O**; the person does not participate. As the match rate increases and the budget rotates up and left (first to 0.5:1 for **OAF** and then to 1:1 for **OBG**), the likelihood that the tangency will move off **O** (say, to $\mathbf{I_1}$ or $\mathbf{I_2}$) increases. Thus, higher match rates can only increase participation.

The match cap has no economic effects on participation. A shift from **D** to **D´** (and the 0.5:1 budget from **OAF** to **OHJ**) does not change the slope near **O**, and quasi-concave utility implies that no tangencies on **HJ** give greater utility than at **O**.

3.2.3.2 Level of savings

The economic effects of changes in match rates (and/or changes in match caps) depend both on the match cap and on whether savings are at the match cap.

If savings are not at the match cap (say, at $\mathbf{I_1}$ with a match rate of 0.5:1 on budget **OAF**), then an increase in the match rate has both income and substitution effects; either could dominate. For example, a move to a match rate of 1:1 on budget **OBG** could lead to tangency at $\mathbf{I_2}$ (decreased savings) or at $\mathbf{I_3}$ (increased savings).

If savings are not at the match cap, then the match cap has no economic effect. For example, with tangency at $\mathbf{I_1}$ with a 0.5:1 match rate and a match cap of **D**, a shift to a match cap of **D´** (budget **OAF** to **OHJ**) leaves the optimal choice unchanged.

If savings are at the match cap (for example, $\mathbf{I_4}$ on budget **OAF** with a 0.5:1 match rate and a match cap of **D**), then a higher match rate has only an income effect. Savings may decrease (for example, to $\mathbf{I_2}$) or stay the same ($\mathbf{I_3}$).

If savings are at the match cap (say, $\mathbf{I_4}$ at **D** on budget **OAF**), then a higher cap (**D´** on budget **OHJ**) will increase savings (for example, to $\mathbf{I_5}$ or $\mathbf{I_6}$).[9]

3.2.3.3 How the match cap may confound estimates of match-rate effects

The theory above looks at changes in either match rates or match caps, with the other held constant. In 401(k) plans in practice, however, high match rates go with low match caps (and inversely). If estimates of match-rate effects do not hold match caps constant, then higher match rates may seem to decrease savings even if they really increase savings.

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[9] If the indifference curve is exactly tangent to the budget at the original match cap—and this is unlikely—then a higher match cap will not increase savings.

For example, suppose that an increase in the match rate from 0.5:1 to 1:1 (with the match cap of $\mathbf{D}'$ held constant) would increase saving from $\mathbf{I_6}$ to $\mathbf{I_7}$ (Figure 1). If, however, the higher match rate comes with a lower match cap **D**, then matched savings must decrease, say from $\mathbf{I_6}$ to $\mathbf{I_3}$. Failure to control for the match cap would show (incorrectly) that higher match rates decrease savings.[10]

Estimates of match-rate effects should control for the match cap. Only VanDerhei, Copeland, and Quick (2000) have done so. Estimates of the effects of match rates on savings in other studies are biased downwards, perhaps severely.

3.2.4 Behavioral effects

Match rates and match caps can have behavioral effects because people lack total rationality, complete information, and perfect imagination. The costs of decision-making often lead to choices based on habit, culture, rules of thumb, or what public policy seems to suggest. Saving may be particularly subject to these behavioral effects (Beverly and Sherraden, 1999; Caskey, 1997; Thaler, 1994; Sherraden, 1991; Maital, 1986), in part because costs are swift and sure but rewards are distant and uncertain, and in part because people differ in how well they grasp the math of finance.

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[10] This bias affects all who would save more at the lower match rate (0.5:1) and higher match cap ($\mathbf{D}'$) than at the lower match cap (**D**) and higher match rate (1:1). The bias also affects all who, at the higher match rate (1:1), save up to the lower match cap (**D**). In practice, many participants save at the match cap, so the bias could be large.

Furthermore, the human body evolved in a context of extreme scarcity and so may have a bias for short-term gratification even at the expense of long-term well-being.

Bernheim (1999) discusses how the behavioral effects of savings incentives may increase savings. Foremost, people may take the mere existence of incentives—be they IDAs, IRAs, or 401(k) plans—as a suggestion that they can and should save. Likewise, the existence of a match signals that saving is a good idea; without much in the way of a personal benefit-cost analysis, people may assume that they would be fools not to take advantage of "free money". Furthermore, restrictions on the use of matched savings may highlight goals (such as home ownership, college education, or retirement security) that people might not focus on otherwise. People may also regard matched savings (even if fully liquid, as in IDAs) as "off limits", and this curbs temptations to consume. Finally, people may turn match caps into goals and so may try to save more if match caps are higher. In sum, behavioral theory predicts greater participation and greater savings with higher match rates and with higher match caps.

Can behavioral effects be distinguished from economic effects? Only economic (income) effects can explain decreases in participation and savings. If savings and/or participation increase with higher match rates, however, the cause could be behavioral or economic (substitution). The only sharp test involves changes in the match cap. Economic theory predicts no effect on participation (nor savings, if not at the match

cap), but behavioral theory predicts changes in both participation and savings. Studies on 401(k) plans have not distinguished between behavioral and economic effects.

3.3 Endogeneity

In 401(k) plans, match rates may depend on unconditional savings in two ways. On the one hand, high savers may demand high match rates. On the other hand, firms with low savers may introduce matching or boost match rates to fulfill non-discrimination requirements. Estimates of match-rate effects are biased upwards by the first type of endogeneity and downwards by the second type.

Only one paper convincingly controls for endogeneity. Even and Macpherson (2001) find that firms do add matching or raise match rates in response to low participation and/or low savings. Thus, estimates of match-rate effects that do not account for endogeneity may be biased downwards.

3.4 Unobserved heterogeneity

Unobserved characteristics of firms or participants may be correlated both with savings and with match rates. This unobserved heterogeneity could bias estimates of match-rate effects either upwards or downwards.

The few studies of 401(k) plans that do deal with unobserved heterogeneity find that firm-level controls weaken otherwise-positive associations between match rates and participation and savings (Bayer, Bernheim, and Scholz, 1996; Papke, Petersen, and Poterba, 1996; Papke, 1995). No studies control for participant-level heterogeneity.

3.5 Censoring

Even if *desired* savings responds to changes in match rates, *actual* savings may change little or not at all, due to the match cap. Estimates of match-rate effects that do not control for this censoring are attenuated toward zero.

For example, suppose that desired savings with a $500 match cap and a 1:1 match rate is $450. With a 2:1 match rate, desired savings may exceed the $500 cap. Actual savings, however, is capped at $500, so the effect on actual savings is smaller than the effect on desired savings.

This paper asks about effects on desired savings. This is the appropriate question for counterfactual policy analysis. Given changes in desired savings, changes in actual savings, given a specific match cap, are straightforward to compute.

How often do desired and actual savings diverge? In studies of specific 401(k) plans, Kusko, Papke, and Poterba (1998) and Yakoboski and VanDerhei (1996) find that about 20-30 percent of participants saved up to the highest match cap. In the IDAs studied here, 26 percent of continuing participants were at the match cap. Thus, bias in match-rate effects that do not control for censoring may be large.[11]

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[11] Match caps censor desired savings, but the need to control for match caps is distinct from the need to control for censoring. Controlling for censoring is also distinct from controlling for kinks. Kinks affect estimates of the effects of matched-savings structures on total savings (Moffitt, 1990); this paper—and the 401(k) literature—look only at effects on matched savings.

3.6 Summary

Estimates of the effects of match rates should control for bias due to negative correlations between match caps and match rates, censoring, endogeneity, and unobserved heterogeneity among programs and among participants. No estimate in the 401(k) literature controls for more than one of these five possible sources of bias. This paper (below) controls for four of them.

4. Data, model, and results

The analysis here controls for match caps and for a wide range of characteristics of programs and participants. It also addresses censoring and unobserved heterogeneity, but not endogeneity. For IDAs examined here, higher match rates were associated with increased continued participation but with decreased savings.

4.1 Data from the American Dream Demonstration

The American Dream Demonstration (ADD) comprises 14 IDA programs across the United States. Enrollment started in July 1997, and 2,378 participants had opened an IDA by June 30, 2000 (Schreiner *et al.*, 2001).

Data on programs and participants in ADD come from management-information software used by programs (Johnson, Hinterlong, and Sherraden, 2001). The system records account-structure parameters, demographic and socio-economic data on participants, and monthly IDA cash flows. The cash-flow data are accurate and complete; they come from bank records, satisfy accounting identities, and were extensively cross-checked. It may be the best (or only) high-frequency data on matched savings by the poor.

4.1.1 Type of match-cap structure

Participants in ADD face either annual or lifetime match caps. *Annual match caps* limit matchable dollars in a participation year. *Lifetime match caps* limit total matchable dollars over a span of multiple years.

Of the 2,378 participants in ADD, this paper analyzes the 807 who had an annual match cap and who exited before the end of their first year or who completed at least 12 months as of June 30, 2000. Participants are analyzed just after their twelfth month for two reasons. First, like all other permanent-access matched-savings structures, any permanent-access IDA would have an annual match cap, in part to prevent abuse. Second, some participants in IDAs—as in IRAs—make large deposits just before the deadline. Figure 2 shows that net deposits in ADD spike at year-end. Measuring IDA savings before an annual or lifetime deadline would be like measuring IRA savings in October even though most IRA deposits for a tax year are made before April 15 in the next calender year.

4.1.2 Continued participation

There are no data on eligible people who choose not to participate in ADD, so this paper cannot look at the effects of match rates on participation. Instead, it looks at effects on *continued participation* through month 12 after enrollment. The opposite of continued participation is *exit*, that is, leaving ADD without a matched withdrawal.

In theory, continued participation is analogous to participation; just as a non-participant can enroll at any time, a participant can exit at any time. Opportunity costs are the same for exit as for non-participation; factors that increase (decrease) participation should also increase (decrease) continued participation.[12]

[12] In practice, imperfect rationality may make match-rate effects stronger for continuing participation than for participation. Although opportunity costs are the same for exit

About 84 percent of participants continued participation through month 12 (Table 2). The share continuing was higher for match rates of 1:1 and 2:1 than for match rates of 3:1. Theory cannot explain this, but the tabulations do not control for other variables correlated both with continuing participation and with match rates.

4.1.3 Level of IDA savings

IDA savings are the smaller of the match cap or of deposits minus unmatched withdrawals. Unlike research on 401(k) plans, this paper looks at savings in terms of dollars rather than in terms of shares of income. This is because IDA match caps are in terms of dollars and because income data in ADD are noisy.

Average net savings in the first 12 months were $379 (Table 2). Savings decreased with the match rate: $411 for 1:1, $376 for 2:1, and $314 for 3:1. Again, the tabulations do not control for other variables and so may not say much about match-rate effects.

Table 2 also shows that asset accumulation (savings plus match) averaged $1,021 and increased with the match rate. IDA savings as a share of income averaged 3.0 percent and was highest for 3:1 match rates. On average, continuing participants made deposits in two of three months.

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and for non-participation, participants may "feel" exit costs more than non-participants because participants are more likely to know what they are missing.

4.1.4 Variation in match caps and match rates

Match caps in ADD do not vary much (Table 3). This likely precludes reliable estimates of match-cap effects.

Unlike match caps, match rates in ADD vary both between programs and within programs both for continuing participants and for exited participants (Table 4). This allows controls for both match rates and program-level unobserved heterogeneity.

4.1.5 Endogeneity

Two-way causation between the match rate and unconditional savings is the main threat to validity in this study. Although participants could not choose among IDA programs by their match rates (ADD was all there was), unconditional savings may still be correlated with match rates in three ways.

First, programs often targeted specific groups, and they may have set higher (lower) match rates if they expected to serve unconditionally low (high) savers (Sherraden *et al.*, 2000). This would bias the estimated match-rate effect downwards.

Second, Program 1 had a 1:1 match rate except for participants who received Temporary Assistance for Needy Families (TANF); their match rate was 2:1. If TANF receipt was correlated with unobserved characteristics that affect unconditional savings, then linking match rates with TANF produces endogeneity bias. For example, if TANF recipients save less, all else constant, then this will bias estimated effects downwards.

Third, Programs 7a and 7b gave a match rate of 2:1 to home buyers and 1:1 to others. If home buyers were high savers, then the bias is upwards. If home buyers were target-savers—for example, for a fixed down payment—then the bias is downwards.

The data provide no way to control for endogeneity where match rates depend on expected savings. Dummies mark the receipt of public assistance and the intended use of the IDA control for the other two types of endogeneity. These imperfect controls are the best that the data allow. The paper returns to this issue below.

4.1.6 Data caveats

Of course, no data set is perfect, and four points are noted here.[13] First, the demographic and socio-economic characteristics of participants are measured at enrollment, but they may have changed afterwards. Second, despite a strong commitment to evaluation by IDA staff, data quality varies among programs. Third, participant income, assets, and debts are noisy and probably understated, as in most surveys. Fourth, continued participation may be overstated because participants may exit *de facto* even if the program has not marked them as exits.

4.2 Model

With data on a wide range of program and participant characteristics, this paper uses a Tobit model with selection for continued participation to control for match caps, censoring, and unobserved heterogeneity in programs and participants.

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[13] Schreiner *et al.* (2001) discuss the data from ADD at length.

The first step is a Probit for continued participation. For person i, z_i^* is the desire to participate through month 12. Desire to continue is assumed to be a linear function of a vector w_i of independent variables and an error term u_i:

$$z_i^* = \alpha' w_i + u_i. \quad (3)$$

The controls w_i include a set of dummies for the match rate (with 1:1 omitted), a single variable for the match cap, and a set of program dummies to control for program-level heterogeneity. Appendix A describes other independent variables.

Desired continued participation z_i^* is unobserved. Actual continued participation d_i is observed, and it equals unity if z_i^* is positive and zero otherwise.

The second step is a Tobit with savings censored at the match cap (31 percent of continuing participants are censored, Table 2). In standard Heckman fashion, a transformation of the error term from the first-step Probit becomes, in the second-step Tobit, a control for unobserved individual heterogeneity that affects both the likelihood of continued participation and the level of savings for continuing participants. Desired savings y_i^* is a linear function of independent variables x_i and an error term ϵ_i:

$$y_i^* = \beta' x_i + \epsilon_i. \quad (4)$$

Observed savings y_i equals the smaller of desired savings y_i^* or the match cap. As in the first-step Probit, the controls x_i include dummies for the match rate, a variable for the match cap, and dummies to control for program-level unobserved heterogeneity, as well as the transformed Probit error term to control for individual-level unobserved heterogeneity. The error terms u and ϵ are bivariate normal with correlation ρ.

4.3 Results

Maximum-likelihood estimates for the Tobit model with sample selection are presented below for the match cap and match rate. Other results are in Appendix A.

The estimated correlation ρ is positive, large (0.56), and statistically significant ($p = 0.01$, Table A5). Unobserved characteristics that make a participant more likely to continue participation also serve to increase desired savings.

4.3.1 Match caps

Consistent with economic theory (but not with behavioral theory), the match cap was not associated with continued participation in IDAs in ADD (Table 5). An increase in the match cap of $100 is estimated to decrease the likelihood of continued participation by 0.04 percentage points, and the p-value of the coefficient is high (0.29).

Expected desired savings decreased by $74 for each $100 increase in the match cap (Table 5). This puzzling effect is both large and statistically significant ($p = 0.01$).

For two reasons, both of these results are probably peculiar to this data set. First, match caps vary little, both between programs and within programs. Second, neither economic nor behavioral theory can explain negative match-cap effects.

4.3.2 Match rates

Higher match rates for IDAs in ADD were associated with a greater likelihood of continued participation. The estimated changes are 0.35 percentage points for the move from 1:1 to 2:1 and 0.87 percentage points for the move from 1:1 to 3:1 (coefficient p-values are 0.04, Table 5). These effects are small compared with the large changes in the match rate. This paper cannot test for the importance of the presence of a match, but perhaps continued participation in IDAs—as research has found for 401(k) plans—depends more on the presence of a match than on the match rate.

Higher match rates decreased desired savings (Table 5). The move from 1:1 to 2:1 was associated with a reduction of \$102 ($p = 0.02$). The move from 1:1 to 3:1 was associated with a reduction of \$232, but the coefficient was not statistically significant ($p = 0.59$). In ADD, income and/or target-saving effects swamped substitution effects.

To recap, higher match rates increased participation a little and decreased desired savings a lot.

4.3.3 Asset accumulation

Do higher match rates increase or decrease asset accumulation (savings plus match)? In the absence of match caps and with a match rate of 1:1 for all continuing

participants, predicted desired savings averages $518. With a 2:1 match rate, the average is $416. Asset accumulation is then $1,036 at 1:1 and $1,248 at 2:1. Thus, decreased savings would offset some—but not all—of the effects of a higher match rate on asset accumulation.

In the presence of match caps, the offset is smaller because some of the decrease in desired savings does not affect actual savings because it takes place above the caps. For example, with a match rate of 1:1 for all continuing participants, average predicted savings with the match caps in ADD is $468 (asset accumulation of $936); with a match rate of 2:1, average predicted savings is $392 (asset accumulation of $1,176).

4.3.4 Bias in past estimates of match-rate effects

This paper addresses some sources of bias ignored in most research on 401(k) plans. Did this care with match caps, censoring, and unobserved heterogeneity matter?

To check, a Tobit with sample selection was run without a variable for the match cap. As might be expected—given that the match cap varies little in this data—effects were unchanged, except the estimate for a 3:1 match rate on desired savings fell to less than $1 ($p = 0.92$).

Without program dummies, results change drastically. Match rates of 2:1 no longer affect participation, and match rates of 3:1 decrease participation. Desired savings still falls (by $84) for match rates of 2:1 compared with 1:1, but it increases $41

for match rates of 3:1 compared with 1:1 ($p = 0.40$). The results without controls for program-level unobserved heterogeneity do not make much sense.

A Probit and a Tobit were run separately to check whether individual heterogeneity mattered. Participation results were virtually unchanged. The desired-savings regression, however, had match-rate effects of essentially zero. This is unlikely.

Censoring also mattered. First-step results for a selection model with ordinary least-squares in the second step (rather than a Tobit) were virtually unchanged. As expected, match-rate effects on desired savings were attenuated toward zero, –$74 for 2:1 match rates and –$181 for 3:1 match rates.

Controls for match caps, censoring, and unobserved heterogeneity did matter, especially for effects on desired savings. Although IDAs are not exactly like 401(k) plans, the measurement issues are quite similar, so the usefulness of work on match-rate effects in 401(k) plans that do not control for these sources of bias is unclear.

Finally, although the model here controls for many sources of bias, it cannot control for all possible forms of endogeneity between match rates and unconditional savings. This weakens the robustness of the results. IDA programs in ADD may have set match rates higher (lower) if they expected participants to be low (high) unconditional savers. This biases estimates of match-rate effects downwards, so the estimated decrease in savings associated with higher match rates may be overstated.

5. Discussion

For IDAs in ADD, higher match rates were linked with increased participation but with decreased savings. Higher match rates may have income effects that swamp substitution effects, and/or people with IDAs may target-save. Endogeneity between the match rate and unconditional savings may also explain at least part of the effect.

What does this mean for policy? The response of savings to match rates matters because of cost. The government spends billions each year on tax breaks for IRAs and 401(k) plans, and IDAs cost even more per dollar saved, both because of higher match rates and because of higher administrative costs (Schreiner *et al.*, 2001).

IDA policy might have three (not necessarily exclusive) goals. The first is to include more poor people in savings incentives (Sherraden, 2001). For IDAs in ADD, higher match rates increased participation and thus served this purpose.

A second goal is to increase aggregate personal savings. Given that the move from 1:1 to 2:1 was linked with an increase in continued participation of less than 1 percentage point and with a decrease of $76 in actual savings and of $102 in desired savings, higher match rates for IDAs in ADD did not serve this purpose.

The third goal is to increase asset accumulation by poor people who would not likely take advantage of tax breaks for retirement savings. This matters in part because ownership of big-ticket items (such as homes) may have broad social benefits and may spark discrete improvements in the long-term well-being and capacity of their owners.

Because matches turn smaller sums of savings into larger sums of asset accumulation, IDAs may facilitate large purchases. All else constant for IDAs in ADD, higher match rates were associated with higher (actual and predicted) asset accumulation.

The broad lesson for policy may be that participation responds to the presence of a match much more than it responds to the level of the match rate. If the goal is to get people to participate in savings incentives, then matching, even a very low rate, may be very effective. As far as the level of savings is concerned, people may respond to higher match rates—once past some still-unknown point—by saving less. Thus, if the goal is to boost saving, perhaps a low match rate is best. For participation, low match rates work as well as high ones; for savings, low match rates work better than high ones.

As a final note, the theory discussed in this paper suggests that the response to matched-savings incentives depends on the interaction of match rates with match caps. The match cap varied little in the data here, but a next step for future research is to map savings for a range of combinations of match rates and match caps to give policymakers and program designers a menu of trade-offs.

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Appendix A: Variables and regression results

This appendix describes 25 independent variables in the Tobit model with selection and discusses collateral results omitted from the main text.

A.1 Program characteristics

A.1.1 Financial education

Unlike IRAs and 401(k) plans, IDAs have mandatory financial education. Education is omitted from the participation equation because exited participants, perforce, have fewer chances to attend classes. The average continuing participant attended 9 hours of class in the first year (Table A1). Each hour in the range of 1 to 6 increased savings by $29, and each hour from 7 to 12 increased savings by $32. The effect leveled off after 12 hours. The large effect for people with zero hours likely reflects programs' letting sophisticated participants miss class.

A.1.2 Intended use

About 49 percent of continuing participants intended to use their IDA for home purchase, 18 percent for post-secondary education, and 14 percent for self-employment. Another 20 percent planned for home repair, job training, or retirement. Intended use was omitted from the first step and had no effect on desired savings.

A.1.3 Program fixed effects

A set of dummies control for unobserved heterogeneity among programs. Unobserved factors that vary across programs matter a lot (Table A1). Not all

coefficients are statistically significant, but that depends in part on which program is arbitrarily omitted, and most estimated coefficients are large in magnitude.

A.2 Program characteristics

Three-fourths of participants were female (Table A2). For all participants, the average age was 36 years, and 83 percent of participants lived in places with 2,500 people or more. Most participants were single; 43 percent were never-married, and 32 percent were divorced, separated, or widowed. Participant households had 1.4 adults and 1.7 children, and 92 percent had only one IDA participant. In this sample, 51 percent were Caucasian, 32 percent were African-American, and 18 percent were of another race/ethnicity (mostly Asian Americans, Hispanics, and Native Americans).

Of these demographic characteristics, only race/ethnicity had statistically significant effects. People who were not Caucasian nor African-American were more likely to continue to participate, and African-Americans had lower desired savings than other groups. Schreiner *et al.* (2001) discuss this result in detail.

A.3 Education, employment, and receipt of public assistance

College graduates (24 percent of participants, Table A3) had higher desired savings than those who went to college but did not graduate (37 percent), who completed high school or got a General Equivalency Diploma (24 percent), or who did not finish high school (15 percent). Education did not affect continuing participation.

Employment status did not affect desired savings, but students and full-time workers were more likely to exit. "Unemployed or not working" includes people laid-off and awaiting a call-back, people seeking a job, as well as homemakers, the retired, and the disabled. About 92 percent of IDA participants in ADD worked full-time or part-time or were students.

About 19 percent of participants owned business assets or had self-employment income. The self-employed were more likely to continue participation.

Data on receipt of Aid for Families with Dependent Children (AFDC), TANF, food stamps, or Supplemental Security Income (SSI) was missing for 70 percent of cases. A dummy marks these missing cases. Of the rest, about 43 percent had received public assistance, with no effect on participation or desired savings.

A.4 Income, assets, and debts

A.4.1 Income

Average annual income was about $12,000. Increased income was linked with a small increase in desired savings but not with any change in participation (Table A4).

About three-fourths of IDA participants likely received the Earned Income Tax Credit (EITC), worth about $1,100 to the average recipient. Although IDA savings spike in tax season (Schreiner *et al.*, 2001), neither EITC receipt nor the imputed level of the tax credit were linked with continued participation or with desired savings.

A.4.2 Assets

About 55 percent of participants reported owning a passbook savings account at enrollment (in addition to the IDA). Continued participation increased with account ownership, and higher balances were associated with increased desired savings.

About 68 percent of participants owned a checking account. Ownership did not affect participation, but higher balances were linked with higher desired savings.

About 16 percent of participants owned a home, and 72 percent owned a car. Neither was associated with participation or savings.

A.4.3 Debts

About 62 percent of participants reported debts at enrollment, be they home mortgages, car loans, business loans, mortgages on land or property, loans from family or friends, credit-card debt, student loans, or overdue bills. Neither the presence of debt nor its value was associated with continued participation or desired savings.

A.5 Other variables and regression parameters

A.5.1 Pre-IDA relationship

Some participants (40 percent, Table A5) received services from the host organization at some point before enrollment in the IDA program. Another 22 percent were referred to the IDA program by a partner organization. An existing relationship with the host or a referral from a partner did not affect participation or savings.

A.5.2 Late enrollees and multiple accounts

In the months before the ADD enrollment deadline of December 31, 1999, some programs scrambled to meet enrollment goals. Table A5 suggests that last-minute enrollees were more likely to exit but did not differ in terms of desired savings.

Program 6 allowed participants to have more than one account. The analysis here aggregates the accounts as if they were a single account. Multiple accounts were weakly associated with an increase in desired savings of about $200.

Figure 1: Possible theoretical effects of match rates and/or match caps

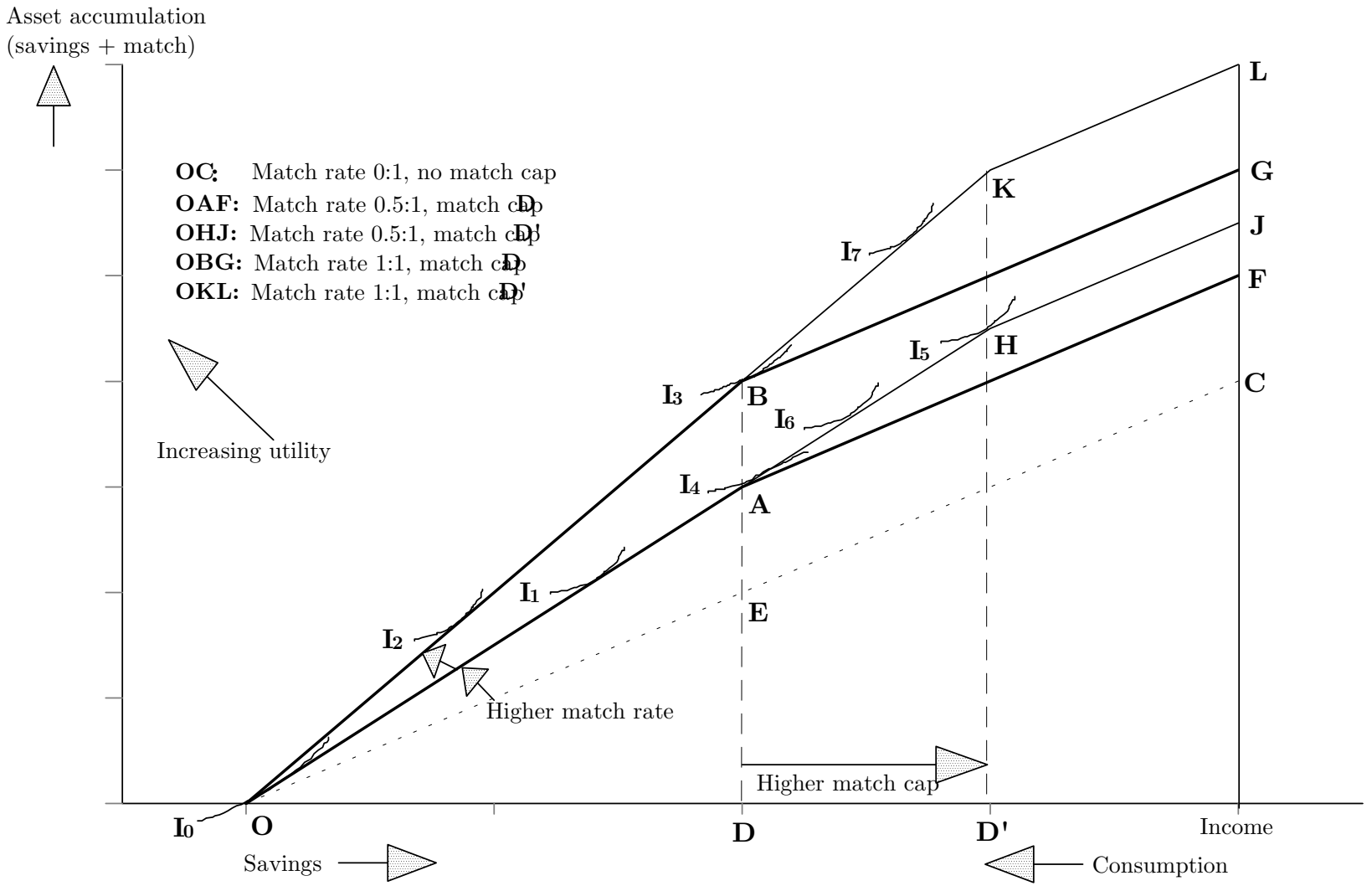

Figure 2: With annual match caps, net deposits spike at year-end

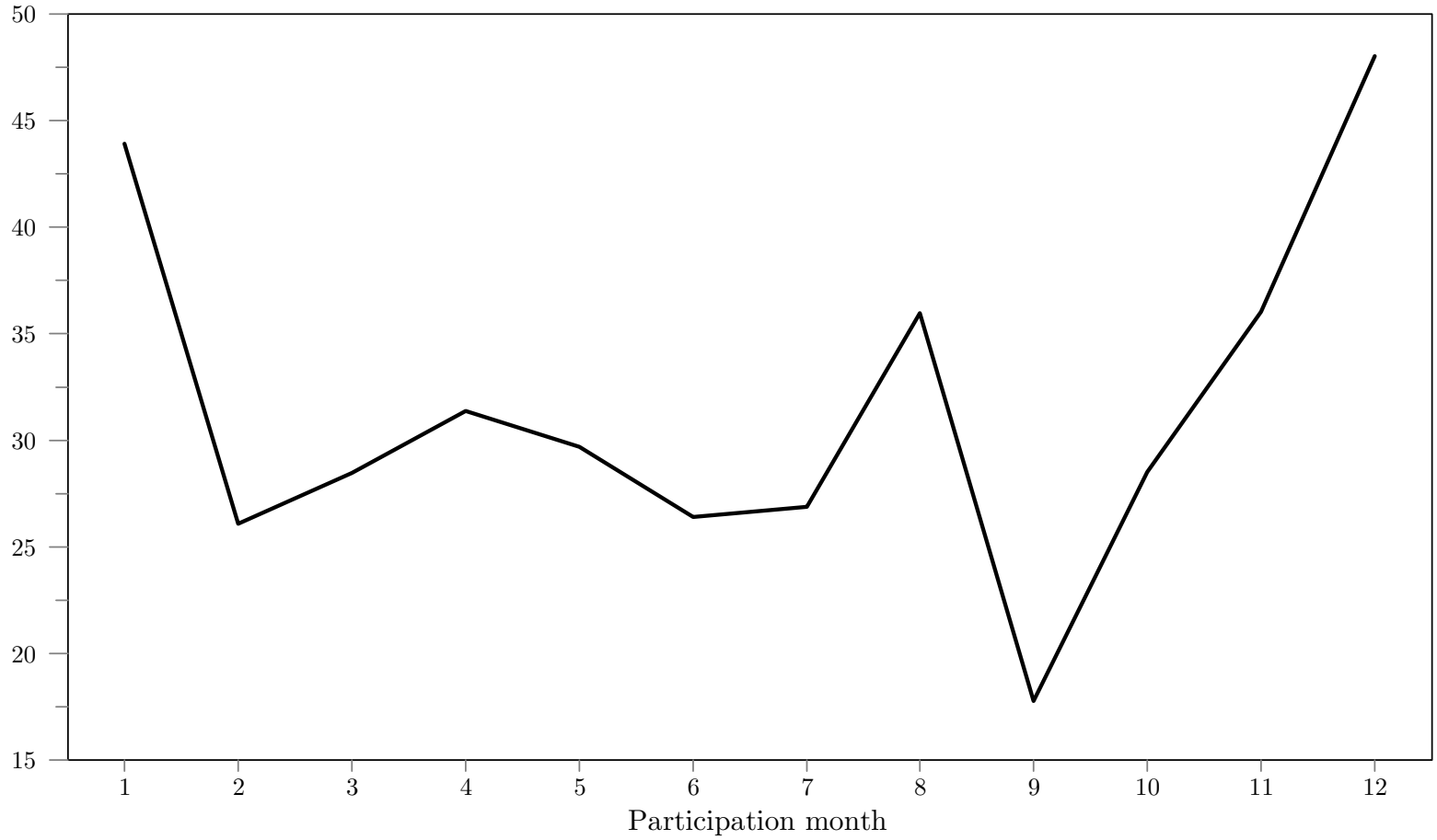

Table 1: Comparison of features of IDAs, 401(k) plans, and Roth IRAs

Feature	IDAs	401(k) plans	Roth IRAs
Eligibility	Income-tested	Employer that offers plan	Universal (earned income, income cut-off)
Subsidy	Direct match	Match (via tax break; employer match is part of compensation)	Match (via tax break)
Source of subsidy	Government or private donor	Government	Government
Type of account	Unrestricted-access, insured passbook savings account in bank	Restricted-access account, often in a mutual fund	Restricted-access account, often in a mutual fund
Tax status	After-tax deposits, earnings taxed, general tax status of match not defined	Before-tax contributions, tax-free accumulation, retirement distributions taxed as income	After-tax contributions, tax-free accumulation, no tax on retirement distributions
Penalty for early distributions	None (but no match)	Current taxes and excise tax	Excise tax
Uses	Purchase of home, post-secondary education, or microenterprise assets	Consumption in retirement (also pre-retirement loans and hardship withdrawals may be allowed)	Consumption in retirement (also allows pre-retirement home purchase, college tuition, or medical hardship)
Administrator/Sponsor	Not-for-profit organizations	Employers	Financial-service organizations
Match cap	Annual, set by program	Annual, set by employer and/or by law	Annual, set by law
Direct deposit	Possible, but not required	Obligatory	Possible, but not required
Financial education	Obligatory	Voluntary (employer may offer)	Voluntary (must be sought out)

Table 2: Continuing participation and savings by match rate for IDAs in ADD

		Match rate		
Measure	**Full sample**	**1:1**	**2:1**	**3:1**
Number of participants				
All	807	330	292	185
Continuing	674	292	252	130
Exits	133	38	40	55
Share continuing (%)	84	88	86	70
For continuing participants only:				
Net savings ($)	379	411	376	314
Asset accumulation (savings + match) (	1,021	823	1,127	1,259
IDA savings as a share of income (%)	3.0	3.2	2.5	3.5
Share of months with a deposit (%)	67	68	64	71
Share of participants at match cap (%)	31	37	25	31

Source: Continuing participants with annual match c
their twelfth month of participation in ADD through June 30, 200(

Table 3: Match caps across programs in ADD for continuing versus exited participants

	Continuing participants			Exited participants		
Program	**1:1**	**2:1**	**3:1**	**1:1**	**2:1**	**3:1**
1	50	38		9	1	
2			47	3	1	51
3	70			18		
4	33			2		
5			79			3
6		73	4	1	30	1
7a	76	63		5	8	
7b	63	78				

Note: Of 807 participants, 674 continued and 133 exited.

Table 4: Match rates across programs in ADD for continuing versus exited participants

	Continuing participants			Exited participants		
Program	**250-300**	**500**	**>=750**	**250-300**	**500**	**>=750**
1		88			10	
2	32	15		10	45	
3		70			18	
4		33			2	
5		79			3	
6	4	54	19	1	23	8
7a			139			13
7b			141			

Note: Of 807 participants, 674 continued and 133 exited.

Table 5: Effects of match rates and match caps on continued participation and savings

	Continuing participation			Desired savings		
Variable	**Beta**	**p-value**	**Mean**	**Beta**	**p-value**	**Mean**
Match cap						
Annual cap ($100s)	-0.11	0.29	6.06	-74	0.01	6.16
Match rate						
1:1 (omitted)			0.41			0.43
2:1	0.88	0.04	0.36	-102	0.02	0.37
3:1 or higher	2.19	0.04	0.23	-232	0.59	0.19

Note: Tobit model for savings with selection on continued participation.
Probit marginal effects in percentage-point units are Beta·0.399

Table A1: Program characteristics

	Continuing participatio			Desired savings		
Variable	**Beta**	**p-value**	**Mean**	**Beta**	**p-value**	**Mean**
Hours of general financial ed. attended (spline)						9.01
Zero	(Omitted from first step)			226	0.02	0.05
1 to 6				29	0.03	5.39
7 to 12				32	0.01	3.24
12 to 18 (capped at 18)				2	0.88	0.38
Intended use						
Home purchase (omitted)	(Omitted from first step)					0.49
Post-secondary education				-10	0.80	0.18
Self-employment				44	0.31	0.14
Other use				32	0.47	0.20
Program fixed effects						
7a and 7b			0.36			0.42
1	-0.08	0.88	0.12	-220	0.01	0.13
2	-4.09	0.01	0.13	139	0.75	0.07
3	-0.29	0.62	0.11	-70	0.31	0.10
4	0.45	0.60	0.04	-106	0.22	0.05
5	-1.67	0.15	0.10	101	0.81	0.12
6	-2.86	0.01	0.14	-38	0.59	0.11

Note: Tobit model for savings with selection on continued participa
Probit marginal effects in percentage-point units are Beta·0.399.

Table A2: Participant demographics

	Continuing participati			Desired savings		
Variable	**Beta**	**p-value**	**Mean**	**Beta**	**p-value**	**Mean**
Gender						
Male (omitted)			0.25			0.25
Female	-0.37	0.18	0.75	27	0.41	0.75
Age (spline)			36.1			37.2
0 to 40 years	0.02	0.37	33.7	-3.3	0.18	34.5
41 years or more	0.06	0.16	2.4	2.8	0.30	2.8
Location of residence						
Population of 2,500 or more (omitted)			0.83			0.81
Population less than 2,500	-0.08	0.87	0.17	-46	0.36	0.19
Marital status						
Never-married (omitted)			0.43			0.41
Married	0.21	0.62	0.25	34	0.51	0.26
Divorced, separated, or wido	-0.20	0.48	0.32	27.1	0.40	0.33
Household composition			3.1			3.1
Adults (18 or older)	-0.35	0.22	1.4	35	0.29	1.4
Children (17 or younger)	0.004	0.97	1.7	10.9	0.35	1.7
Participants in household						
One (omitted)			0.92			0.92
More than one	0.13	0.70	0.08	20	0.75	0.08
Race/ethnicity						
Caucasian (omitted)			0.51			0.53
African-American	-0.23	0.39	0.32	-108	0.01	0.29
Other race/ethnicity	0.66	0.05	0.18	6.9	0.85	0.18

Note: Tobit model for savings with selection on continued participa
Probit marginal effects in percentage-point units are Beta·(

Table A3: Education, employment, and receipt of public assistance

	Continuing participation			Desired savings		
Variable	**Beta**	**p-value**	**Mean**	**Beta**	**p-value**	**Mean**
Education						
College graduate (omitted)			0.24			0.26
Attended college but did not graduate	-0.02	0.94	0.37	-67	0.03	0.38
Completed high school or earned GED	-0.15	0.63	0.24	-121	0.01	0.23
Did not complete high school	-0.30	0.39	0.15	-82	0.12	0.13
Employment						
Full-time (omitted)			0.60			0.61
Employed part-time	0.69	0.02	0.24	40	0.24	0.25
Unemployed or not working	0.74	0.09	0.08	-4	0.95	0.08
Student	-0.15	0.72	0.08	-34	0.59	0.06
Self-employed						
No (omitted)			0.83			0.81
Yes	0.79	0.05	0.17	19	0.55	0.19
Welfare receipt						
No (omitted)			0.17			0.16
AFDC, TANF, food stamps, or SSI	-0.05	0.90	0.13	-38	0.36	0.12
Missing	0.86	0.01	0.70	76.7	0.05	0.73

Note: Tobit model for savings with selection on continued participation.
Probit marginal effects in percentage-point units are Beta·0.399.

Table A4: Income, assets, and debts

	Continuing participation			Desired savings		
Variable	**Beta**	**p-value**	**Mean**	**Beta**	**p-value**	**Mean**
Income at enrollment						
Annual (log of $100s)	-0.05	0.80	4.78	29	0.02	4.77
Imputed EITC receipt						
Likely did not receive (omitted)			0.26			0.25
Likely did receive	0.35	0.44	0.74	-39	0.50	0.75
Value (log of $100s)	-0.09	0.55	2.07	4	0.84	2.09
Passbook savings account						
Not owned (omitted)			0.39			0.36
Owned	0.39	0.10	0.55	-18	0.57	0.57
Missing	-0.76	0.19	0.06	50	0.37	0.07
Balance (log of $100s)	-0.22	0.16	0.51	38	0.03	0.53
Checking account						
Not owned (omitted)			0.21			0.15
Owned	0.18	0.54	0.68	-26	0.48	0.74
Missing	-0.30	0.55	0.10	7	0.88	0.11
Balance (log of $100s)	0.08	0.68	0.63	72	0.01	0.69
Home						
Not owned (omitted)			0.84			0.81
Owned	0.30	0.51	0.16	44	0.26	0.19
Car						
Not owned (omitted)			0.28			0.24
Owned	0.24	0.31	0.72	11	0.73	0.76
Debts						
None (omitted)			0.34			0.33
Some	0.16	0.71	0.62	-18	0.69	0.64
Missing	-0.31	0.57	0.04	23	0.72	0.04
Value of debt (log of $100s)	0.03	0.78	2.30	-7.6	0.48	2.44

Note: Tobit model for savings with selection on continued participation.
Probit marginal effects in percentage-point units are Beta·0.399.

Table A5: Other variables and regression parameters

	Continuing participation			Desired savings		
Variable	**Beta**	**p-value**	**Mean**	**Beta**	**p-value**	**Mean**
Existing relationship with host org.						
No (omitted)			0.44			0.47
Yes	0.37	0.29	0.40	-10	0.72	0.41
Missing	-0.69	0.28	0.16	-33	0.60	0.12
Referred by a partner organization						
No (omitted)						
Yes	-0.05	0.91	0.22	12	0.69	0.23
Missing	-1.72	0.01	0.13	-56	0.54	0.09
Enrolled after June 30, 1999						
No (omitted)			0.92			0.95
Yes	-1.03	0.01	0.08	-32	0.57	0.05
Multiple accounts in one household (Program 6 only)						
No (omitted)			0.97			0.97
Yes	0.91	0.31	0.03	202	0.17	0.03
Regression parameters						
Rho	(Omitted from first step)			0.56	0.01	
Intercept	1.84	0.23	1.00	20	0.92	1.00
Sigma	(Omitted from first step)			270	0.01	

Note: Tobit model for savings with selection on continued participation.
Probit marginal effects in percentage-point units are Beta·0.399.