Forthcoming AEJ: Economic Policy

Details Matter:

The Impact of Presentation and Information on the Take-up of Financial Incentives for Retirement Saving

Emmanuel Saez, UC Berkeley*

February 2008

Abstract

We examine the effects of presentation and information on the take-up of financial subsidies for retirement saving in a large randomized experiment carried out with H&R Block. The subsidies raise take-up and contributions, with larger effects when the subsidy is characterized as a matching contribution rather than an equivalent-value tax credit (or cash back), and when filers are informed before the tax season about the subsidy. The results imply that both pure incentives and the presentation of those incentives affect consumer choices.

* Saez: Department of Economics, University of California, 549 Evans Hall, Berkeley CA 94720, saez@econ.berkeley.edu. We gratefully acknowledge the assistance of H&R Block employees, including Mary Beth Granger, Mike Lammers, Scott McBride, Andrew Olson, Paul Peterson, Yvette Ruiz, John Thompson, Jeremy White, and Bernie Wilson, as well as the district and office managers and the 600 tax professionals who implemented the experiment. We thank James Poterba, Joel Slemrod, and two anonymous referees for very helpful comments and discussions. The views expressed are those of the author alone and do not necessarily reflect the views of H&R Block.

A growing body of evidence shows that individuals respond not only to the pure economic incentives they face but also to the manner in which such incentives are presented and framed. Such behavior appears to be particularly relevant in analysis of saving choices. Brigitte Madrian and Dennis Shea (2001), for example, show that changing the default rules for 401(k) enrollment has a significant impact on employee participation rates. Esther Duflo and Emmanuel Saez (2002) show that the behavior of one's colleagues has a significant causal influence on workers' participation in 401(k) plans. Marianne Bertrand, Dean Karlan, Sendhil Mullainathan, Eldar Shafir, and Jonathan Zinman (2005) show that changes in the wording of a loan offer have a significant effect on borrowers' choices.

Esther Duflo et al. (2006) used experimental data covering 14,000 tax filers at 60 H&R Block tax preparation offices in St. Louis in 2005, and showed that the matching of IRA contributions can significantly raise take-up and contributions. Raising the match rate from zero to 20 percent to 50 percent raised IRA take-up from 3 percent to 8 percent to 14 percent. We also showed using non-experimental difference-in-difference analysis that increasing the effective match rate in the federal saver's credit from 25 percent to 100 percent raised take-up by at most 1.3 percentage points.[1] Contributions to retirement accounts, conditional on take-up, were also significantly more sensitive to variations in effective matching rates in the controlled experiment than in the saver's credit data. We concluded that taxpayers were more responsive to the incentives in our experiment because our subsidy offer was presented transparently, whereas the similar incentives in

[1] The variation in the federal saver's credit rate comes from differences in income as the credit rate depends (discontinuously) on Adjusted Gross Income. This variation, however, is not experimental.

the federal tax system are difficult to perceive.[2]

All of these findings imply that information and presentation, factors that are often ignored in conventional analysis of public policy, can have first-order effects on individuals' choices and hence on the efficacy of policy interventions, even holding economic incentives constant. Because optimal policy choices hinge sensitively on the size of behavioral responses, and because the presentation and informational details of public policies can often be altered at low cost, a crucial new direction for empirical research is to move beyond merely estimating the size of behavioral responses and instead to analyze how various "details" can shape the size of the behavioral response. It is important to note that some "details" can be altered at low cost (for example the match versus credit framing) while other "details" are most costly to change (such as providing information about a program).

This paper presents new evidence on the importance of presentation and information alternatives in analyzing tax filers' responses to financial incentives for retirement saving contributions. We use data from a large field experiment conducted during the 2006 tax season in collaboration with H&R Block in St Louis.[3] The basic experimental design is simple. H&R Block customers in St. Louis who filed returns in the same office in 2006 as in 2005 were randomly assigned to a treatment group, which received a 50 percent match on IRA contributions made at the time of filing, or a control

[2] A large number of experimental studies have shown, in the context of charitable contributions, that matching subsidies can increase contributions. Many studies focus on laboratory experiments. See Douglas D. Davis (2006), Douglas D. Davis and Edward L. Millner (2005, 2006), Douglas D. Davis and Edward L. Millner, and Robert J. Reilly (2005), and Catherine C. Eckel and Philip J. Grossman (2003, 2005b, 2006a, b). There have been fewer field experiment studies: Dean Karlan and John List (2007) study the case of contributions to a political interest group. Stephan Meier (2007) studies longitudinal aspects in the context of contributions to a social scholarship fund by students. Eckel and Grossman (2005a) analyze the case of Public Radio contributions in Minnesota.

[3] The new experiment expands upon the matching experiments conducted in 2005 and analyzed in Duflo et al. (2006). The new experiment tests how a number of "details" affect IRA contribution decisions.

group, which received no match. Both groups had account set-up fees waived. On top of this basic design, several variations were introduced.

First, a randomly selected subset of treatment group members was presented with a 33 percent credit rebate (cash back) rather than a 50 percent match. While these two subsidies are economically equivalent, previous experiments in the context of charitable giving have shown that a match presentation generates higher take-up than a credit presentation.[4] This raises important issues because the saver's credit could be restructured to offer matching contributions at relatively low cost.

Second, a different randomly chosen subset of filers received advance notification of the fee waiver (if in the control group), or of the match and the fee waiver (if in the treatment group) via a phone call and letter. Advance notification gives tax filers the chance to plan ahead to take advantage of the match. In addition, because public policies can be advertised and tend to be in place for multiple years, the results with advance notification may be more relevant for predicting the likely effects of a real-world government program.

Third, a random subset of treatment group members was offered the match for regular monthly IRA contributions, as well as for contributions made at the time of filing. Recent research on 401(k) plans, where employees make monthly contributions, suggests that employer-provided matching rates raise take-up and contributions (see, for example, Gary Engelhardt and Anil Kumar 2004, Gur Huberman, Sheena Sethi-Iyengar, and Wei Jiang 2007). The extent to which those effects are due to pure incentives versus some combination of peer effects, automatic payroll deduction, or other factors is unclear.

[4] See Davis (2006), Davis and Millner (2005, 2006), Davis, Millner and Reilly (2005), and Eckel and Grossman (2003, 2005a, b, 2006a, b). We discuss below how our findings in the savings case can cast new light on the explanations put forward in the charitable contribution case.

Testing the sensitivity of IRA take-up to whether monthly contributions are matched can provide evidence on this issue because peer effects and payroll deduction are not applicable to the IRA choice that filers in our experiment faced. Our principal findings can be summarized simply. The matching offer raises take-up and contributions to IRAs. Even after controlling for a variety of taxpayer characteristics, returns filed earlier in the season generally exhibited smaller responses to all of the various subsidies offered than those filed later in the season. The match raised take-up of retirement saving by almost 6 percentage points overall, and by more than 10 percentage points among returns filed between March 5th and March 31st, 2006. [5]

The match offer generated significantly higher take-up and contribution levels than the economically equivalent credit rebate offer. Hence, the form of the subsidy and the presentation of information appear to matter significantly, even when the underlying incentives are identical. Moreover, we can explain why people respond differently to these two different framings; almost all of this effect occurred because filers made a specific type of sub-optimal choice, leaving money on the table.

Advance notification more than doubled IRA take-up rates among filers who were assigned to the match treatment. In sharp contrast, it had no effect on take-up among filers who were not assigned to the match treatment. This suggests there are important interactions between information and incentives: each can work more effectively in the presence of the other.

The opportunity to receive matches on future monthly IRA contributions had little effect on take-up or contributions. The difference between these results and the positive

[5] The quantitative magnitude of the results is quite robust with respect to the effects of the 2005 experiment analyzed by Duflo et al. 2006, which took place only from March 5 to April 5, once the date of filing of the tax return is controlled for.

match effects in 401(k) plans is likely to be due to differences in the available information and choice context, rather than with any difference in underlying economic incentives, since the match rate in the experiment, 50 percent, is also the most common employer matching rate in 401(k) plans.

The remainder of the paper is organized as follows. The next two sections describe the experiment and the results of the basic design. The following three sections examine the effects of the three variations on the basic design described above. The final section discusses conclusions and policy implications.

I. Background

The experiment centers around the Express IRA (X-IRA), a product through which H&R Block offers clients the chance to make IRA contributions at the time of tax preparation. X-IRAs can be funded with the forthcoming tax refund, a personal check or cash. X-IRAs can be either traditional IRAs, where the contribution is tax-deductible and withdrawals are taxed, or Roth IRAs, where contributions are not deductible and qualified withdrawals are untaxed. The minimum annual X-IRA contribution is $300 and can be made on a one-time basis or via automatic monthly deductions of at least $25 from a bank account. In the basic experimental design, however, only one-time contributions are matched. There is a $15 fee for opening or re-contributing to the account at the time of tax preparation (contributions and withdrawals by mail are free) and a $25 account termination fee. A $10 annual maintenance fee is waived for accounts with balances over $1,000 or for those using automatic deductions. Until the balance reaches $1,000, the only investment option is an FDIC-insured money market bank account. Take-up of the

X-IRA has been modest. In tax season 2005, the nationwide X-IRA take-up rate (including opening and re-contributions) was approximately 1.3 percent.

The experiment was conducted in 60 H&R Block offices in St Louis between January 20th and March 31st, 2006. Any client coming to prepare taxes at one of the relevant offices, who had also prepared their taxes at the same office in 2005, received a waiver of the $15 X-IRA set-up fee and was randomly assigned to either the control group, which received no match, or the treatment group, which received a 50 percent match on X-IRA contributions up to $1,000.[6] Because IRAs are individually owned, the same offer was extended to each spouse in married couples filing jointly.

A. Experimental Groups

To explore variations on the basic design, the 60 offices were divided into 3 groups.[7] The experimental design is laid out in Table 1. As shown in Panel A, the match versus credit presentation was implemented in 19 offices. In these offices, 80 percent of filers were in the control group, 10 percent were offered the 50 percent match, and 10 percent were offered a 33 percent credit rebate on X-IRA contributions. Randomization occurred at the customer level based on the last two digits of the primary taxpayer's Social Security Number (SSN). As we discuss below in detail, absent any credit constraints or other frictions, the credit generates exactly the same budget set as the match, since a credit at rate t is equivalent to a match at rate t/(1-t). To see this, suppose an individual is offered the 50 percent match and chooses to contribute $400 and hence

[6] We restricted eligibility to prior year customers to protect the external validity of our findings. In particular, we wanted to avoid the possibility that our sample would contain too many people with a higher than average propensity to save – via selection into the sample of people who somehow learned about the matching offer and came to Block specifically to receive the match.

[7] H&R Block offices are grouped into districts of roughly 12 offices each. Because training and management occur at the district level, it was necessary to keep districts intact in dividing the offices into the three groups. We chose the office grouping to balance the income and racial distribution of customers subject to the constraint that we keep districts intact.

obtains a $200 match and a total X-IRA contribution of $600. Under the credit scenario, this allocation can be replicated by making a $600 contribution and receiving a 33 percent rebate of $200 so that the net out-of-pocket cost is also $400, as in the match scenario. Conversely, any allocation chosen under the credit scenario can be replicated under the match scenario. Because the match was offered for contributions of $300 to $1,000,[8] the rebate was offered for contributions of $450 to $1,500, which generate out-of-pocket costs between $300 and $1,000. Notably, those who were offered the credit could make X-IRA contributions between $300 and $450, but such contribution choices did not receive the credit and would be strictly dominated by making a $450 contribution coupled with a $150 rebate. The rebate was provided in the form of a check mailed to the tax filers within two weeks of tax filing.[9] Therefore, a key difference between the match and the rebate is that the rebate required tax filers to advance the match money until they received the rebate check within two weeks. This can be costly if tax filers are severely credit constrained. Most tax filers receive substantial tax refunds which can be used to make X-IRA contributions and might mitigate credit constraints in our specific set-up. The rebate can also be more costly than the match if tax filers face large transaction costs of cashing the rebate check, face the risk of losing the check (or getting it stolen in the mail), or would be required to share the proceeds of a mailed check with household members. Obviously, credit constraints or the friction costs just described break the equivalence between the match and the credit.

As shown on Panel B of Table 1, advance notification was implemented in 20 offices. In these offices, about 20 percent of randomly selected 2005 filers, divided

[8] Contributions above $1,000 received a total match of $500 so that the match was effectively capped at $500. H&R Block requires minimum contributions of at least $300.
[9] The matching funds were deposited to the X-IRA account at the end of the tax season in mid-April.

equally between matched and control groups, were scheduled to be called in November 2005. Those who were reached were informed that, if they returned to the same office in 2006, they would receive a waiver of X-IRA set-up fees. Treatment group members were also informed of the 50 percent match. A marketing company that routinely works with H&R Block attempted up to three calls, if necessary, to reach each filer. Filers who were reached by phone also received a mailing in late December 2005 explaining the offer again. The remaining 80 percent of 2005 filers in those offices were not scheduled to be called; among those, 30 percent received a match.

As shown on Panel C, matching of monthly contributions was provided in the remaining 21 offices. In those offices, 89 percent of 2005 filers were in the control group and the remaining 11 percent received the 50 percent match offer described above. In addition, unlike other groups, treatment group members in these offices could also receive the match for systematic monthly contributions from a bank account of amounts between $25 and $100 through the end of 2006. Once initiated, contributions would continue at the same rate unless the filer actively chose to change the level. This sub-experiment was designed to test the theory that people find it easier to commit to forego future consumption in order to build up savings than to give up immediate consumption.[10]

B. Implementation

The steps taken to implement the experiment were very similar to those described in Duflo et al. (2006). Briefly, to prompt the information required for the tax return, the software used by tax professionals at H&R Block displays a series of screens in a default

[10] A similar hypothesis underlies the "Save More Tomorrow" research of Richard Thaler, and Shlomo Benartzi (2004).

sequence, including a screen for X-IRA participation. Tax professionals can override the default sequence, however, and skip or return to any screen at any time. In the experiment, when the X-IRA screen was activated, a special pop-up window automatically appeared describing the offer corresponding to the client's treatment status. Tax professionals then informed clients that they were receiving a special X-IRA offer as part of a research project and that they were under no obligation to participate. The professionals also provided H&R Block's standard X-IRA explanation that IRAs are not for everyone and that there can be penalties for early withdrawal.

Although random assignment was based on the last two digits of the social security number of the primary filer, tax professionals were not informed of the algorithm, and a client's treatment status was not revealed by the software until after reaching the X-IRA offer screen. Thus, tax professionals' decisions to offer (or skip) the X-IRA screen were independent of treatment status. The decision may, however, have depended on an assessment of whether the client was likely to make a contribution. We analyze take-up and contributions for all tax filers by treatment status, regardless of whether they received an offer (i.e., we present "intent-to-treat" estimates).

The experiment ran from January 20, 2006 to March 31, 2006. With the exception of the advance notification calls and letters described above, the experiment was not advertised in advance.[11] However, some clients may have anticipated the offers since we had run a similar experiment in the same offices during the last month of the

[11] Pre-tax-season training sessions were conducted by an H&R Block implementation manager. Tax professionals are paid $5.50 for each X-IRA account opened or re-contributed to by their clients and this commission structure was in place for our experiment. More generally, tax professionals receive greater compensation for completing more complicated (and therefore more time-consuming) returns. Duflo et al. (2006) show that tax professionals differ substantially in the extent to which their clients sign up for X-IRAs, even after controlling for client characteristics and office.

2005 tax season. H&R Block made the matching payments, which totaled approximately $333,000.

C. Data and descriptive statistics

The analysis is based on data which include information from 2005 and 2006 tax returns, information on X-IRA contributions, and other information collected by H&R Block during tax preparation. We exclude filers with earnings below $300 from the analysis, since they were not eligible to make X-IRA contributions. We prepared statistical programs and sent them to H&R Block, which then sent us back the output of those programs. The output of the programs contains only statistical compilations and no individual information. The Tables and Figures reported in this paper are created using this statistical output.

Table 2 shows descriptive statistics for the treatment and control groups in the three sets of offices. Within each office type, none of the differences between the treatment and control groups are significant at the 5 percent level, which suggests that the randomization was successful. About 13 percent of the 2006 sample also participated in the 2005 experiment (the 2005 experiment ran during only from March 5 to April 5). The average AGI of $34,000 to $40,000 is lower than the national average of around $50,000 (and lower that of the sample in the 2005 experiment because early filers tend to have lower incomes). About half of the sample owns a home. At least 85 percent has a federal refund larger than $500, which would generally allow them to fund a $300 X-IRA out of their refund even if they owed taxes at the state level. The average tax refund is around $2,800. Less than 30 percent of the sample has positive investment income. About 40 percent of filers in the sample receive the EITC. Filers in the match vs. credit group have

somewhat lower income and home ownership rates, and are more likely to be single and receive the EITC. There are about 15,000 tax return observations in each of the three groups. Out of the roughly 48,300 experimental tax filers, about 10,800 received a match (or credit) offer. Appendix Table 1 reports X-IRA participation and contributions by office type.

D. Interaction with 2005 Experiment

The 2005 experiment took place between March 5th and April 5th in the same offices and filers were randomized into a control group, a 20 percent match group, and a 50 percent match group. The 2006 sample consisted of all of those filers and all other 2006 filers in the 60 St. Louis offices, but in each case the filer was only included if the return was filed in the same office in both years. As a result, some 2006 filers were in the 2005 experiment and some were not. The working paper version of this study (Saez, 2007) analyzes in detail the interactions between the two experiments and finds modest interactions. As a result, controlling for those interactions has only a minimal effect on our results. Two points should be noted. First, relative to filers who did not participate in the 2005 experiment, those who participated in 2005 are no more likely to return to the same office in 2006, and they are only very slightly more likely to take-up the match in 2006. Second, relative to filers in the control group in 2005, those who received a match offer in 2005 are only slightly more likely to contribute to an IRA in 2006 suggesting that dynamic effects are modest in the X-IRA environment of this experiment.[12]

II. Effects of the Match

[12] This stands in contrast to findings in the context of 401(k) where inducing individuals to start contributing through a change in default rules (Madrian and Shea 2001) or through committing future pay raises (Thaler and Benartzi, 2004) has very strong dynamic effects.

Table 3 presents the effects of the basic match experiment – that is, the effects of the "pure" 50 percent match with no advance notification. The table combines data from all three groups of offices, but excludes customers who were called in advance or who received a "credit" offer.[13] The average take-up of the X-IRA was 3.07 percent in the control group and 8.91 percent in the 50 percent match group. The raw difference is 5.84 percentage points and is highly significant. The difference is almost the same, 5.72 percentage points, after controlling for all of the variables listed in Table 2 (to control for residual differences between the groups), office-group dummies (to control for the likelihood of being matched), and a dummy for whether the person would have been matched in 2005 (to control for the correlation between match rules in 2005 and 2006).[14] Measured at the tax return level, the effects are larger among married couples (8.78 percentage points) than among singles (4.48 percentage points).[15]

Table 4 explores the heterogeneity in take-up and contribution levels by characteristics in a regression framework, showing the OLS regression coefficients of an X-IRA outcome (take-up in columns (1) and (2), contributions in columns (3) and (4)) on a variety of explanatory factors. Table 4 shows that being married or owning a home increases the effects of the match on take-up significantly. In contrast, being single, having dependents or obtaining a refund in excess of $500 increases take-up without the match. Having higher overall income or investment income raises take-up regardless of

[13] The table does include data from the offices where systematic monthly contributions were eligible to be matched. The net increase in X-IRA take-up due to that option, however, was quite low, as described below, and so has a very small effect on the overall test of the "pure" 50 percent matching incentive.

[14] Controlling for 2005 assignment status allows us to interpret our impact estimates as the impact of receiving the treatment in 2006 conditional on not receiving a treatment in 2005.

[15] If the saving choices of the two spouses in a married couple were independent it would not be surprising to find twice the effect among married couples as among singles. Because we believe the saving decisions of spouses are highly correlated, we think return-level comparisons (rather than per capita comparison) are more interesting.

match status.

The effects we find in Table 3 are substantially smaller than the effects in the 2005 matching experiment (Duflo et al. 2006), where the difference in X-IRA take-up between the 50 percent match group and the control group was 11.07 percentage points for the whole sample, 15.42 percentage points for married couples, and 8.69 percentage points for singles (Duflo et al. 2006). It turns out that the difference can be primarily attributed to the differences in take-up rates by filing date. Among customers who filed between March 5 and April 1st, 2006 (the time period of the 2005 experiment of Duflo et al. 2006 was March 5 to April 5), the increase in take-up due to the match, in percentage points, is 10.76 for the whole sample, 14.15 for married couples, and 8.31 for singles, as shown in the lowest panel of Table 3. These figures are not significantly different from the 2005 estimates noted above.

The basic 2006 experiment therefore shows that the effect of the match is stable from year to year, but differs appreciably across filing dates.[16] To explore this issue further, Figure 2 plots the coefficients of a regression of an X-IRA contribution dummy on filing time decile dummies interacted with the match treatment dummies (and including also un-interacted filing time dummies in the regression). The bold graph shows that filing a return at a later date, without controlling for any additional variables increases take-up very significantly when a match is offered, from between 2 and 5.5 percentage points at early filing dates to more than 10 points among later filing dates.

[16] It is possible that the apparent stability of results reflects offsetting differences between the 2006 and 2005 experiments. For example, tax professionals were likely to be more comfortable offering the match in 2006 because they had experienced it in 2005. This should have raised client take-up. On the other hand, the training and mid-experiment monitoring and encouragement of tax professionals appeared to us to be less intense in 2006 than in 2005. Regardless of the factors resulting in the stability of the late season results, we believe the within-season differences are real.

The dashed graph displays the filing date effects, but now controlling for all the variables introduced in Table 4.[17] Interestingly and perhaps surprisingly, the matching effects of the filing dates are only about one third lower than without controls. This shows that differences in the income or other observed characteristics of tax filers cannot explain more than a third of the effects of the match by filing date. One plausible explanation is that early filers are more impatient than later filers, even controlling for observable characteristics. After all, by filing earlier they receive their tax refunds earlier. If so, this might be a signal for needing the tax refund money urgently. For example, tax filers who are credit constrained and impatient to buy a large consumption item or tax filers who are indebted and face very high costs of credit would be more impatient to get their refund than tax filers with buffer stock savings. As a result, it is understandable that those constrained and impatient tax filers would be less willing to consider reducing their refund to contribute to an X-IRA.[18] It should be noted that our crude control dummy variable for refund size may not fully control for differences in the size of refunds across filing dates. As a result, some of the residual difference in match take-up behavior might still be due to differences in refund sizes across filing dates rather than differences in preferences.

III. Match versus Credit Presentation

Several lab and field experiments show that matching offers tend to generate more

[17] The control variables are included directly and interacted with the match as in Table 4.
[18] An alternative explanation, which is not inconsistent with the first, is that tax professionals become more competent at explaining the offer as they gain experience with the experiment over the course of the tax season. Tax professionals also become less busy after the February peak filing period is past. Unfortunately, because tax pro experience and filing timing are closely correlated, our data do not allow us to separate those two hypotheses.

charitable giving than economically-equivalent rebates or cash back (see footnote 1). Duflo et al. (2006) provide evidence consistent with those findings, but in the context of retirement saving. They show that variations in effective match rates generate larger responses in a matching experiment than in the existing federal saver's credit. This difference could be due to the difference in taxpayer responses to a match versus a credit, or to a variety of other differences between how the experimental match offer and the federal saver's credit are implemented and perceived. In this section, we present the first experimental evidence on the effects of match versus credit presentations on the take-up of, and contributions to, retirement saving accounts.

Table 5 shows results for the offices where the match and credit options were presented. Both groups have higher take-up rates than the control group. The more interesting comparisons are between the match and credit groups. Relative to the credit group, the match group has a higher take-up rate by 3.68 percentage points; among those who participate, match group members averaged $153 more in contributions (inclusive of match, thus measuring the amount going into the IRA). Both of these effects are statistically different from zero. The results imply that taxpayers do not perceive the match and the credit to be economically identical, even though they are for unconstrained individuals as we explained above. As with the general take-up of the match shown in Figure 1, take-up of the credit and the difference between take-up rates for the match and the credit rise as the tax season progresses (Figure 2).

The difference in take-up rates for the match and credit groups can be attributed almost entirely to the lower probability, in the credit group, of opening an X-IRA with contributions between $300 and $450, with no corresponding increase at higher

contribution levels (Panel A, Table 5). As noted above, contributions below $450 did not trigger the credit rebate. Thus, it appears as though filers correctly perceived that they would not get a credit for small contributions; they are no more likely to open a small X-IRA in the credit group than in the control group. But filers did not realize that if their intent was to have only $300 in out-of-pocket costs, they could achieve this objective with a $450 contribution, which would be offset by a $150 rebate. One possibility is that credit-group participants were severely credit constrained, and thus could not contribute $450 on the spot. However, Panel B of Table 5 restricts the sample to taxpayers with a refund of at least $1,000 and obtains almost identical results, casting doubt on the credit constraint explanation.[19]

Some of the differing take-up responses to the match and credit may arise because filers mistakenly perceive the 33 percent credit as equivalent to a 33 percent match and hence less attractive than a 50 percent match.[20] Evidence on this issue can be gleaned from our 2005 experiment, where 7.7 percent of eligible households took up the 20 percent match, compared to 14 percent who took up the 50 percent match. Among clients in our 2006 sample who filed between March 5th and April 1st, 2006 and who were in the match versus credit offices, take-up rates were about the same for the 50 percent match in 2006 (13.54 percentage points) as in 2005 and were only slightly higher for the 33

[19] It is conceivable that tax filers have already spent their tax refund in advance using expensive credit and hence are not able to contribute into an X-IRA. Another possibility is that clients do not trust a private company to send the rebate check. It is unclear, however, why a promise to mail a future rebate check should be viewed as less trustworthy than a promise to provide a future matching contribution. And, even if the rebate promise were somehow viewed as less believable, the money in the account would still belong to the taxpayer. Thus, someone who would have contributed $300 with no offer, but chose to contribute $450 with the credit rebate offer, could always withdraw the extra $150 (at no cost, if they opened a Roth IRA) if the rebate check was not delivered.

[20] Davis and Millner (2006) makes this point in the context of charitable giving. Some tax professionals, who are on average more financially educated than their clients, argued along those lines and thought that the credit was less advantageous than the match.

percent credit in 2006 (7.92 percentage points) than for the 20 percent match in 2005 (Panel C, Table 4). Hence, the response to the credit seems to be even smaller than what would have been predicted by treating the 33 percent credit as a 33 percent match.

Eckel, and Grossman (2003, 2005a,b, 2006a,b) argue that matching schemes might generate larger effects in the context of charitable giving because individuals perceive the match as sharing the effort whereas the credit feels like the individual shoulders all the contributions. For retirement saving, however, unlike charitable giving, there is no direct external effect of contributions on the common good, so this feeling cannot be the explanation in our context.

Another possibility arises from the fact that, unlike in the charitable giving experiments, in our experiments, filers had to wait a couple weeks for the credit rebate. In other words, contributing $450 out-of-pocket and then receiving $150 back in a few weeks may feel more painful than simply contributing $300 under the match scenario and obtaining the same $450 X-IRA total contribution. This differential effect could be due to loss aversion effects proposed by the prospect theory of Daniel Kahneman, and Tversky, Amos (1979). Alternatively and as discussed above, individuals might face high costs of cashing the rebate check or might face substantial probability of losing the check or having to share the proceeds with household members so that the cost is real rather than psychological. The important point, however, is that such friction costs are generally ignored in economic studies although they might end up having very large effects in real situations.

The difference in contributions levels between the match and credit group is illustrated in Figure 3, which shows the cumulative distributions of X-IRA contributions

across the treatment groups. Contributions are largest in the match group (when including the match) and smallest in the control group. The distributions of contributions in the credit group and the match group (exclusive of the match) are fairly close above $300.

Although the credit rebate in the experiment is somewhat different from the saver's credit – the latter is non-refundable and varies with income level – both the federal saver's credit and our credit rebate provide cash back or a reduction in tax liability, rather than a matching contribution to the account. Thus, as discussed further in the conclusion, the differences in taxpayer responses to the match and credit groups has implications for the design of public policy.

IV. Advance Notification

The experiments described above took place without any advance notification of clients.[21] Field observations strongly suggest that the vast majority of those clients did not know about the matching program prior to their visit to the H&R Block office. The potential effects of advance notification are interesting, however, for two reasons. First, such information could alter taxpayer responses. For example, focus group discussions following the 2005 experiment revealed that some filers had turned down the 50 percent match offer because, before coming in to the Block office to file their returns, they had already made plans to use their refunds for specific purposes. Second, federal policies could be redesigned at low transaction and revenue costs to allow people to know in advance that they will be eligible for a particular subsidy rate and hence give them the chance to plan ahead.

[21] Specifically, clients receiving advance notification were excluded from the analyses presented in the earlier sections of this paper.

Table 6 shows descriptive data for offices where some filers were scheduled to be called in advance. The first two columns report information by call status; the two panels report information by match status. The sample excludes taxpayers in the "Do not call" registry, regardless of whether they were scheduled to be called. Even after those filers are excluded, only about 30 percent of those who were intended to be called were actually reached. Although call status was determined randomly based on the last two digits of the customer's SSN, there are small, but statistically significant differences in the percent married and the mean adjusted gross income between those scheduled to receive a call and those with no call scheduled. We attribute these differences to random assignment (also since married taxpayers have higher AGI than other taxpayers, it is not surprising to have both of these variables showing lack of balance at the same time). The fraction reached was similar in the match and no match groups, suggesting that we successfully implemented calling procedures that were blind to treatment status. Perhaps surprisingly, the calls had no impact on the fraction of 2005 H&R Block customers who returned to have their taxes done by Block in 2006 in the match group. In the no match group, the calls seem to have had a small positive impact on the fraction returning to Block, though the t-stat on this impact is slightly below 2.

For the analysis of the impact of the calls (rows 5-7 of each panel), we restrict our sample to clients who return to Block in 2006. This restriction allows our sample to be comparable to the samples used in the earlier sections of the paper (which include only clients who returned to Block in 2006 and not non-returning clients), and it allows us to regression adjust our results using the same specifications used for the other results (most of our covariates come from information collected during the 2005 tax interview and are

therefore not available for clients who did not return in 2006). Our judgment is that given that the calls had little or no impact on who returned, the benefits of presenting comparable results outweigh the potential selection bias that could be present if the calls affected who returned to Block offices.

Among matched filers, being scheduled to be called raised take-up by a statistically significant 2.1 percentage points (column 3).[22] Controlling for the covariates, the coefficient is essentially the same – 1.9 percentage points. These are underestimates of the effect of actually receiving a call, however, since there is no reason why attempting to call a filer would have an effect if the filer was not reached. Among those called, the sub-sample of those reached is not random, so we cannot simply compare those who are reached and those who are not scheduled to be called. However, since the difference in take-up between those who were scheduled to be called and those who were not can be entirely attributed to the effect of the call, the "attempt to call" variable can be used as an instrument for the dummy indicating whether someone has been reached, to generate an estimate of the "effect of the treatment on the treated" (TOT).[23] The TOT effect is large: receiving a call raised take-up by 6.9 percentage points (unadjusted) and 6.1 percentage points (regression adjusted). This effect is larger than the "pure" effect of the match itself, 5.72 percentage points, as shown in Table 2.[24] In contrast, receiving a call had no

[22] As with the earlier results for the "pure" match and the match versus credit comparisons, take-up rates among those who were notified in advance and who received the match rose as the tax season progressed.

[23] The Wald estimate is obtained by dividing the difference between the participation rates in the two groups by the fraction of those who were reached.

[24] The mean X-IRA contribution rate for members of the match group who received calls was 8.9. With a TOT effect of the calls of 6.9, this implies that the mean for these individuals would have been 2.0 percent in the absence of the calls. Thus, the calls more than doubled take-up relative to the match-only outcome (which itself combines the impact of the match and the baseline contribution rate in the absence of the match) for these "compliers." Lawrence Katz, Jeffrey Kling, and Jeffrey Liebman (2001) discuss the calculation of the control complier mean.

effect on take-up among filers who did not receive a match. Also, advance notification had little effect on contribution levels, given take-up, for either the matched or the unmatched group (these estimates are not very precise).

An alternative way to analyze the joint impact of the match and the advance calling is to pool both the match and no match samples (which were analyzed separately in the two panels of Table 6) and to run difference-in-differences regressions. Table 7 shows that all of the key results above about advance notification are robust to this alternative specification. In Panel A, X-IRA take-up rates and contribution levels are regressed on a dummy for whether the filer was scheduled to be called, whether she received a match, the interaction of the two, all of the characteristics in Table 2, and their interaction with being scheduled to be called. In column 1, the coefficient on the interaction between receiving a call and receiving a match is 1.62, quite close to the difference between the regression adjusted impact of calling for those in the match group (1.9) and those in the no-match group (0.2) in column 4 of Table 6. Panel B presents an instrumental variables (IV) regression where the variables "call attempted" and "call attempted*match in 2006" are used as instruments for the variables "reached" and "reached*match in 2006" – a specification comparable to the TOT specifications in Table 6. The phone call had no impact in the unmatched group, but increased take-up in the matched group by 4.93 percentage points; that is, it more than doubled the effect of the match, which was 3.56 percentage points. This is a striking result: the fact that a simple phone call and follow-up letter can more than double the take-up of a generous financial offer underlines the importance of advance notification.

The finding that advance notification raises the take-up of subsidies for saving

may also help shed light on why the saver's credit generates such small take-up. Although people may know of the existence of the credit, the actual rate that will apply is difficult to forecast in advance. The credit is non-refundable, and thus depends on the presence of other deductions and credits. In addition, the rate changes quite abruptly over some ranges of income. As a result, filers may not know in advance whether they will qualify for the credit, let alone which rate will apply. The results for advance notification suggest that knowing these factors in advance can significantly boost the effects of matching incentives. It is also important to note that the call might also be perceived by tax filers as an encouragement to contribute to an X-IRA as the match was presented by callers as an opportunity. Additional experiments varying the format of the call would be required to try and separate pure information effects (just knowing that a match will be offered) from encouragement effects (being told that the match is an opportunity).

V. Matches for Monthly Contributions

The results in tables 6 and 7 suggest that the ability to plan ahead is an important factor in the decision to take advantage of a financial incentive for retirement saving. In this section, we explore the effects of offering an alternative way for taxpayers to plan for the future. Table 8 reports data from the offices where filers were offered matches for monthly contributions to X-IRAs, in addition to matches for one-time contributions. In general, monthly X-IRA contributions are not very popular. As shown in Table 8, only 0.20 percent of filers chose to make monthly contributions in the control group in the offices where the monthly contribution match experiments were conducted. The match increased this proportion to 1.67 percent. However, of the 1.47 percentage point increase

in take-up, 1.41 percentage points occur because of filers who made a matched, one-time contribution as well. As a result, the net effect on take-up of matching monthly contributions is essentially zero.

These results differ from recent studies of 401(k) plans (James J. Choi, David Laibson, Brigitte C. Madrian, and Andrew Metrick 2002, Engelhardt and Kumar 2004, William E. Even, and David C. MacPherson 2005, Huberman et al. 2007), where matches have positive effects on the take-up of monthly 401(k) contributions.[25] The differing results are not due to dissimilar economic incentives; 50 percent is the most common match rate in 401(k) plans. Rather, the differences suggest once again that "details," such as the differences between automatic deduction from payrolls versus bank accounts, whether it is a tax preparer or an employer making the deductions, peer effects, and so on, can play a key role in shaping saving behavior

VI. Conclusion

This paper provides evidence on individuals' saving choices in experiments where variation in informational and presentational characteristics is orthogonal to variation in the pure, underlying economic incentives. The results confirm that economic incentives significantly affect behavior, but also that – holding such incentives constant – relatively small changes in the presentation of an incentive can have first-order effects on the effectiveness of the policy.

Presenting an economically equivalent subsidy as a match rather than credit rebate (or cash back) raises participation by 4 percentage points, from a base of just over

[25]See Choi, Laibson, and Madrian (2004) and Duflo et al. (2006) for discussion of the difficulty of interpreting earlier, mixed findings on the effects of match rates on employee participation in 401(k) plans.

6 percentage points. This shows that a purely presentational change can have a very large effect on the behavioral response elasticity, and hence that it is crucial to account for such presentational issues in estimating the effects of an incentive. Such a large difference cannot be easily explained within the rational model, and we have argued that a mix of confusion about subsidy rates and other factors such as very strong credit constraints must be part of the explanation.

Providing a phone call and letter a few months in advance of tax season more than doubles the effect of a 50 percent match. In contrast, advance notification had no effect on take-up of the IRA among filers who were ineligible for the match. These results show important interactions between incentives and information: each can amplify the effects of the other, and providing one without the other may be ineffective, even in a situation where providing both is effective. These findings suggest that either many tax filers spend their refund in advance and therefore feel severely cash constrained at the time of tax preparation, even if they are receiving refunds, or that prior information helps build interest in the matching program. In either case, the conclusion is that prior information about a program is an important determinant of the size of the behavioral response and therefore that such informational variables should be included in empirical program evaluation.

Offering a matching subsidy for monthly contributions, which appears to be effective in raising take-up in workplace 401(k) plans, appears to have no impact on take-up of X-IRA plans. The differing impact of similar incentives in different environments again points to the importance of other factors, such as peer effects and automatic payroll deductions, that are present in 401(k) plans but not in the X-IRA presentation.

All of these effects represent the first experimental evidence on these issues in the context of retirement saving. The findings are important for several reasons. First, they shed light on decision-making processes that people actually follow and in particular on the relative importance of economic incentives and psychological factors. Second, many of the informational and presentational characteristics of public policies can be changed at relatively low cost. Thus, the results give policy-makers new and potentially powerful dimensions along which to alter public policy. The results also suggest that the content of a public policy can and should be considered to be much broader than just how it affects individuals' budget constraints. Third, optimal taxes and transfers depend crucially on the elasticity of household responses to public policies. Thus, a better understanding of efficient and equitable taxation can be obtained by examining the response to the presentation of incentives as well as the pure economic incentives themselves.

Our results suggest that the federal saver's credit could more effectively promote retirement saving if it were designed as a refundable, flat-rate match. The matching provision would raise total contributions (by the tax filer and the government) and could raise take-up relative to the current credit structure. A refundable, flat-rate subsidy would provide certainty, in advance, about the availability of the provision for tens of millions of households. There would still be uncertainty for taxpayers in or near the phase-out range of the credit, however. This could be eliminated by determining a filer's credit rate for the current year based on the taxpayer's situation (income, deductions, credits) in the previous year. That would allow tax filers to know early in the year, when they filed their taxes, the subsidy they would receive the following year. It might also make it more

feasible to administer the subsidy through employers. Because retirement saving is part of a lifetime decision plan and such savings are consumed during retirement, there should be less of a concern for basing a subsidy on last year's income rather than current income than there might be for a means-tested program that provides a current consumption safety net.

Finally, it is worth emphasizing that even when all the details are set in favorable ways – for example, a match presentation with advance notification – the take-up effects of a 50 percent matching offer remain quite modest, in the vicinity of 15 percent at a maximum, and are substantially smaller in the group of early filers. This suggests that even well-advertised and well-presented matching incentives for retirement saving can only address part of the issue of retirement security facing American families.

References

Bertrand, Marianne, Dean Karlan, Sendhil Mullainathan, Eldar Shafir, and Jonathan Zinman. 2005. "What's Psychology Worth? A Field Experiment in the Consumer Credit Market." NBER Working Paper 11892.

Choi, James J., David Laibson, and Brigitte C. Madrian. 2004. "Plan Design and 401(k) Savings Outcomes." *National Tax Journa,l* 57 June: 275-298.

Choi, James J., David Laibson, Brigitte C. Madrian, and Andrew Metrick. 2002. "Defined Contribution Pensions: Plan Rules, Participant Choices, and the Path of Least Resistance." *Tax Policy and the Economy,* 16: 67-113.

Davis, Douglas D. 2006. "Rebate Subsidies, Matching Subsidies and Isolation Effects." *Judgment and Decision Making,* 1(1): 13-22.

Davis, Douglas D., and Edward L. Millner. 2005. "Rebates, Matches and Consumer Behavior." *Southern Economic Journal*, 72: 410-422.

Davis, Douglas D., and Edward L. Millner. 2006. "On Rebate Subsidies, Matching Subsidies and the Methodology of Experiments." VCU Working Paper.

Davis, Douglas D., Edward L. Millner, and Robert J. Reilly. 2005. "Subsidy schemes and charitable contributions: a closer look." *Experimental Economics,* 8: 85-106.

Duflo, Esther, and Emmanuel Saez. 2002. "Participation and Investment Decisions in a Retirement Plan: The Influence of Colleagues' Choices." *Journal of Public Economics,* 85: 121-148.

Duflo, Esther, William Gale, Jeffrey Liebman, Peter Orszag, and Emmanuel Saez. 2006. "Saving Incentives for Low- and Middle-Income Families: Evidence from a Field Experiment with H&R Block." *Quarterly Journal of Economics,* 121(4): 1311-1346.

Eckel, Catherine C., and Philip J. Grossman. 2003. "Rebates Versus Matching: Does How We Subsidize Charitable Contributions Matter?" *Journal of Public Economics,* 87(3-4): 681-701.

Eckel, Catherine C., and Philip J. Grossman. 2005a. "Subsidizing Charitable Contributions: A Field Test Comparing Matching and Rebate Subsidies." Manuscript, Virginia Tech.

Eckel, Catherine C., and Philip J. Grossman. 2005b. "Subsidizing Charitable Giving with Rebates or Matching: Further Laboratory Evidence." *Southern Economic Journal*, 72(4): 794-807.

Eckel, Catherine C., and Philip J. Grossman. 2006a. "Do Donors Care about Subsidy Type? An Experimental Study." In *Research in Experimental Economics*, eds. Douglas Davis and M. Isaac, vol. 11, 163-182.

Eckel, Catherine C., and Philip J. Grossman. 2006b. "Subsidizing Charitable Contributions: Evidence from a Non-Secular Charity." Manuscript, Virginia Tech.

Engelhardt, Gary V., and Anil Kumar. 2004. "Employer Matching and 401(k) Saving: Evidence from the Health and Retirement Study", Center for Retirement Research Working Paper 2004-18, Boston College.

Even, William E., and David C. MacPherson. 2005. "The Effects of Employer Matching in 401(k) Plans." *Industrial Relations*, 44(3): 525-49.

Huberman, Gur, Sheena Sethi-Iyengar, and Wei Jiang. 2007. "Defined Contribution Pension Plans: Determinants of Participation and Contribution Rates." *Journal of Financial Services Research*, 31(1): 1-32.

Kahneman, Daniel, and Tversky, Amos. 1979. "Prospect Theory: An Analysis of Decision under Risk." *Econometrica*, 47(2): 263-91.

Karlan, Dean, and John List. 2007. "Does Price Matter in Charitable Giving? Evidence from a Large-Scale Natural Field Experiment." *American Economic Review*, 97(5): 1774-1793.

Katz, Lawrence, Jeffrey Kling, and Jeffrey Liebman. 2001. "Moving to Opportunity in Boston: Early Impacts of a Randomized Mobility Experiment." *Quarterly Journal of Economics*, 116(2): 607-654.

Madrian, Brigitte, and Dennis F. Shea. 2001. "The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior." *Quarterly Journal of Economics,* 116: 1149-1187.

Meier, Stephan. 2007. "Do Subsidies Increase Charitable Giving in the Long Run? Matching Donations in a Field Experiment." *Journal of the European Economic Association,* 5(6): 1203-1222.

Saez, Emmanuel. 2007. "Details Matter: The impact of Presentation and Information in the Take-up of Financial Incentives for Retirement Savings." CEPR Discussion Paper 6386.

Thaler, Richard, and Shlomo Benartzi. 2004. "Save More Tomorrow: Using Behavioral Economics to Increase Employee Saving." *Journal of Political Economy*, 112: S164- S187.

Table 1. Experiment Lay-Out

	Aggregates across office Types		Split within Office Types	
	Number of Offices	Total Number of Individuals	Percentage in each group	Number of tax filers
	(1)	(2)	(3)	(4)
A. Match versus Credit Presentation	19	15,852		
50% Match Presentation			10	1,561
33% Credit Presentation			10	1,588
Control (no match or credit offered)			80	12,703
B. Advance Notification	20	17,578		
50% Match offered with Advance Notification			10.4	1,831
50% Match offered without Advance Notification			24.1	4,232
No match with Advance Notification			10.2	1,796
No match without Advance Notification			55.3	9,719
C. Match for Monthly Contributions	21	14,878		
50% Match offered			11	1,617
Control (no match offered)			89	13,261
D. Grand Total across office Types	60	48,308		
Match or Credit offered			22.4	10,829
No Match or Credit offered			77.6	37,479

Notes: Panels A, B, C lay out the experimental groups in each of the 3 sets of offices.
Within each set of offices, tax filers were randomized into treatment and control groups based on last 2 digits of Social Security Numbers.
In the match versus credit presentation group (Panel A), tax filers in the treatment groups were offered either a 50% match, or an economically equivalent 33% credit rebate.
In the advance notification group (Panel B), tax filers in the treatment group were offered a match. In both treatment and control groups, a subset of tax filers was scheduled to receive a phone call notifying them of the IRA opportunity and describing the match in the case of the treatment group. Tax filers were also randomized into the advance notification versus no advance notification groups.
In the Match for Monthly Contributions group (panel C), the treatment group was offered a 50% match for a one-time contribution and also a 50% match on monthly contributions (if the tax filer decided to start such monthly contributions).

Table 2. Descriptive Statistics

Office Type	Match Versus Credit Presentation					Advance Notification			Match Monthly Contributions		
	50% Match	33 % Credit	Control	Match vs Credit	Credit vs Control	50% Match	No Match	Difference	50% Match	No Match	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Was in St Louis 2005 experiment	0.13 (0.0086)	0.14 (0.0088)	0.124 (0.0029)	-0.01 (0.0123)	0.02 (0.0088)	0.14 (0.0044)	0.13 (0.0031)	0.006 (0.0054)	0.13 (0.0084)	0.13 (0.0029)	0.004 (0.0088)
Adjusted Gross income	34109 (714)	34150 (767)	33804 (252)	-41 (1048)	346 (761)	39527 (518)	39235 (329)	292 (589)	38929 (883)	37654 (269)	1275 (828)
Adjusted Gross income, Married filling jointly	58676 (1856)	58953 (2338)	57908 (675)	-277 (2971)	1045 (2087)	63237 (1231)	62382 (632)	855 (1248)	66630 (2089)	63757 (590)	2873 (1821)
Adjusted Gross income non Married	25872 (539)	26482 (530)	26270 (200)	-611 (756)	212 (594)	27909 (360)	27776 (304)	133 (494)	26885 (596)	26771 (203)	114 (621)
Fraction Married	0.25 (0.0110)	0.24 (0.0107)	0.238 (0.0038)	0.01 (0.0153)	0.00 (0.0113)	0.33 (0.0060)	0.33 (0.0044)	-0.002 (0.0075)	0.30 (0.0114)	0.29 (0.0040)	0.009 (0.0120)
Overpayment amount	2874 (51)	2762 (50)	2832 (18)	112 (72)	-70 (53)	2764 (27)	2726 (20)	38 (33)	2796 (52)	2761 (18)	35 (54)
Fraction with overpayment>500	0.89 (0.0081)	0.86 (0.0087)	0.879 (0.0029)	0.02 (0.0119)	-0.02 (0.0088)	0.87 (0.0043)	0.87 (0.0032)	0.005 (0.0054)	0.85 (0.0088)	0.87 (0.0030)	-0.015 (0.0090)
Fraction with positive investment income	0.23 (0.0107)	0.26 (0.0109)	0.238 (0.0038)	-0.02 (0.0153)	0.02 (0.0114)	0.29 (0.0058)	0.29 (0.0042)	0.000 (0.0072)	0.27 (0.0111)	0.27 (0.0039)	-0.002 (0.0118)
Fraction home owner	0.47 (0.0126)	0.45 (0.0125)	0.462 (0.0044)	0.02 (0.0178)	-0.01 (0.0133)	0.51 (0.0064)	0.51 (0.0047)	0.002 (0.0079)	0.52 (0.0124)	0.50 (0.0043)	0.016 (0.0132)
Fraction EITC Recipients	0.44 (0.0126)	0.45 (0.0125)	0.448 (0.0044)	0.00 (0.0177)	0.00 (0.0132)	0.38 (0.0062)	0.36 (0.0045)	0.013 (0.0077)	0.39 (0.0122)	0.40 (0.0043)	-0.006 (0.0129)
Number of Observations	1561	1588	12703			6063	11515		1617	13261	

Notes: This table presents the co-variates across treated and control groups for the three groups of offices. Standard errors in parentheses below the coefficients.

Table 3. Effects of the 50 % Match on X-IRA Take-up and Contributions

	All Returns				Married Couples				Singles			
	50 % Match	No Match	Difference	Difference adjusted for Covariates	50 % Match	No Match	Difference	Difference adjusted for Covariates	50 % Match	No Match	Difference	Difference adjusted for Covariates
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
PANEL A. All returns												
Opened XIRA (percentage)	8.91 (0.35)	3.07 (0.09)	5.84 (0.26)	5.72 (0.29)	11.03 (0.71)	2.26 (0.15)	8.76 (0.46)	8.78 (0.51)	8.04 (0.40)	3.38 (0.12)	4.66 (0.31)	4.48 (0.35)
Amount Contributed (dollars)	66 (3.71)	15 (0.75)	51 (2.36)	50 (2.61)	118 (10.32)	18 (2.03)	100 (6.47)	101 (7.14)	45 (3.00)	14 (0.68)	31 (2.03)	29 (2.25)
Amount Contributed, conditional on contribution	763 (30.03)	540 (20.13)	223 (34.90)	57 (38.56)	1088 (63.62)	868 (76.62)	220 (99.14)	-83 (114.49)	576 (25.40)	456 (14.44)	120 (27.30)	86 (31.28)
Amount Contributed, inclusive of Match	96 (5.09)	15 (0.75)	81 (2.82)	80 (3.12)	170 (14.21)	18 (2.03)	152 (7.82)	154 (8.62)	65 (4.09)	14 (0.68)	51 (2.37)	50 (2.63)
Number of Observations	6675	34345			1950	9637			4725	24708		
PANEL B. Was Part of St. Louis Experiment												
Opened XIRA (percentage)	15.18 (1.46)	3.62 (0.29)	11.56 (0.95)	10.71 (1.02)	17.50 (2.46)	3.14 (0.43)	14.36 (1.46)	14.63 (1.57)	13.66 (1.80)	3.96 (0.40)	9.71 (1.24)	7.84 (1.35)
Amount Contributed (dollars)	151 (18.32)	28 (4.14)	123 (12.81)	117 (13.89)	237 (39.86)	39 (9.12)	198 (28.35)	199 (30.43)	95 (14.72)	20 (2.91)	75 (9.39)	60 (10.23)
Amount Contributed, conditional on contribution	996 (73.84)	808 (99.82)	187 (136.37)	-19 (143.38)	1356 (126.17)	1320 (248.63)	36 (293.01)	-175 (355.48)	693 (57.85)	527 (54.34)	166 (84.51)	68 (88.21)
Amount Contributed, inclusive of Match	222 (26.51)	28 (4.14)	194 (14.82)	190 (16.06)	349 (57.77)	39 (9.12)	310 (32.50)	314 (34.87)	139 (21.17)	20 (2.91)	119 (11.14)	104 (12.15)
Number of Observations	606	4008			240	1657			366	2351		
PANEL C. Returns Filed between March 5 and April 1, 2006												
Opened XIRA (percentage)	12.96 (1.11)	2.01 (0.20)	10.96 (0.66)	10.76 (0.73)	16.01 (1.82)	1.99 (0.30)	14.02 (1.06)	14.15 (1.16)	10.55 (1.36)	2.02 (0.26)	8.53 (0.84)	8.31 (0.92)
Amount Contributed (dollars)	159 (18.31)	20 (3.35)	139 (11.08)	136 (12.15)	246 (35.94)	29 (6.99)	218 (22.40)	221 (24.69)	90 (15.68)	14 (2.79)	76 (9.36)	72 (10.28)
Amount Contributed, conditional on contribution	1240 (94.85)	1108 (144.17)	131 (166.54)	-142 (195.81)	1539 (141.30)	1632 (301.76)	-92 (294.39)	-356 (380.82)	873 (101.11)	756 (109.75)	116 (149.51)	44 (175.66)
Amount Contributed Inclusive of Match	226 (24.43)	20 (3.35)	205 (13.05)	203 (14.30)	349 (48.26)	29 (6.99)	321 (26.47)	326 (29.15)	128 (20.34)	14 (2.79)	113 (10.82)	110 (11.88)
Number of Observations	918	5036			406	2114			512	2922		

Notes: This table shows the effect of the basic match across all offices. Clients who received advance notification or a credit rebate offer are excluded from the sample. Columns 4, 8, and 12 report the coefficient on a "match" dummy in a regression that controls for all the variables in table 2 and a dummy for the office type. Standard errors in parentheses.

Table 4. Effects of the 50 % Match by Individual Characteristics

	X-IRA take-up (percentage)		Amount contributed (unconditional, dollars)	
	Effect	Effect*match 2006	Effect	Effect*match 2006
	(1)	(2)	(3)	(4)
Married	-1.05	2.61	0.85	47.98
	(0.27)	(0.66)	(2.42)	(5.90)
Quartile 2	1.57	1.79	6.08	12.25
	(0.34)	(0.83)	(3.02)	(7.44)
Quartile 3	1.80	0.82	7.32	6.93
	(0.33)	(0.82)	(2.98)	(7.37)
Quartile 4	0.81	2.42	4.33	34.64
	(0.35)	(0.87)	(3.16)	(7.87)
Has investment income	0.65	3.12	12.22	61.75
	(0.28)	(0.69)	(2.49)	(6.17)
Own a home	-0.27	2.05	-0.11	17.35
	(0.25)	(0.62)	(2.25)	(5.55)
Overpayment >500	1.78	1.52	11.69	-20.94
	(0.34)	(0.86)	(3.05)	(7.75)
Has dependent	1.47	0.11	3.22	8.52
	(0.23)	(0.58)	(2.10)	(5.22)
Number of Observations	41004		41004	

Notes: Coefficients of the regression of each characteristic (columns (1) and (3)), and each characteristic interacted with the match (columns (2) and (4)). Standard errors in

Table 5. Effect of Presentation: Match versus Credit

	Means			Differences			Differences adjusted for co-variates		
	Match Group	Credit Group	Control Group	Match vs control	Match vs Credit	Credit vs Control	Match vs control	Match vs Credit	Credit vs Control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. All Filers									
Fraction Open an XIRA	10.19	6.42	3.34	6.85	3.76	3.09	6.11	3.68	2.40
(percentage)	(0.77)	(0.62)	(0.16)	(0.53)	(0.98)	(0.50)	(0.64)	(0.97)	(0.61)
Amount Contributed	56.14	41.47	13.55	42.59	14.67	27.92	34.28	14.51	19.85
(unconditional, dollars)	(5.86)	(5.61)	(0.89)	(3.26)	(8.11)	(3.20)	(3.97)	(8.05)	(3.91)
Amount Contributed	82.5	41.5	13.6	68.9	41.0	27.9	60.6	40.9	19.8
(inclusive of match, unconditional)	(8.20)	(5.61)	(0.89)	(3.83)	(9.90)	(3.20)	(4.67)	(9.83)	(3.91)
Amount Contributed	558	672	439	119	-114	233	-10	-109	83
(conditional)	(40)	(63)	(19)	(39)	(71)	(49)	(48)	(60)	(54)
Amount Contributed	820	672	439	381	148	233	246	153	83
(conditional, inclusive of match)	(53)	(63)	(19)	(45)	(84)	(49)	(54)	(69)	(54)
Amount Contributed	558	504	439	119	54	65	-10	59	-81
(conditional, exclusive of credit)	(40)	(50)	(19)	(39)	(64)	(45)	(48)	(55)	(50)
Fraction Open an XIRA	6.09	2.27	2.63	3.46	3.82	-0.36	3.40	3.78	-0.39
with less than $450	(0.61)	(0.37)	(0.14)	(0.46)	(0.71)	(0.42)	(0.56)	(0.71)	(0.52)
Fraction Open an XIRA	4.10	4.16	0.71	3.39	-0.06	3.45	2.71	-0.10	2.79
with $450 or more	(0.50)	(0.50)	(0.07)	(0.28)	(0.71)	(0.28)	(0.34)	(0.70)	(0.34)
Number of Observations	1561	1588	12703						
B. Filers with refund above $1000									
Fraction Open an XIRA	12.29	7.81	4.12	8.18	4.48	3.69	7.37	4.66	2.79
(percentage)	(0.95)	(0.79)	(0.20)	(0.66)	(1.23)	(0.64)	(0.81)	(1.23)	(0.78)
Amount Contributed	63.75	47.43	16.51	47.24	16.32	30.92	37.40	16.75	20.84
(unconditional, dollars)	(6.82)	(6.42)	(1.08)	(3.89)	(9.37)	(3.83)	(4.76)	(9.32)	(4.65)
Amount Contributed	93.3	47.4	16.5	76.8	45.9	30.9	67.0	46.6	20.8
(inclusive of match, unconditional)	(9.37)	(6.42)	(1.08)	(4.50)	(11.43)	(3.83)	(5.50)	(11.35)	(4.65)
Amount Contributed	522	621	428	94	-99	193	-16	-95	66
(conditional)	(39)	(56)	(18)	(37)	(66)	(45)	(48)	(60)	(51)
Amount Contributed	764	621	428	337	144	193	227	148	66
(conditional, inclusive of match)	(49)	(56)	(18)	(42)	(76)	(45)	(53)	(69)	(51)
Amount Contributed	522	465	428	94	57	38	-16	62	-88
(conditional, exclusive of credit)	(39)	(42)	(18)	(37)	(60)	(42)	(48)	(55)	(47)
Fraction Open an XIRA	7.56	2.83	3.27	4.29	4.73	-0.44	4.33	4.87	-0.50
with less than $450	(0.76)	(0.49)	(0.18)	(0.58)	(0.91)	(0.55)	(0.71)	(0.91)	(0.67)
Fraction Open an XIRA	4.73	4.98	0.85	3.89	-0.24	4.13	3.04	-0.21	3.28
with $450 or more	(0.61)	(0.64)	(0.09)	(0.34)	(0.88)	(0.35)	(0.42)	(0.88)	(0.42)
Number of Observations	1204	1165	9691						
C. Filers with return between March 5 and April 1, 2006									
Fraction Open an XIRA	13.54	7.92	1.93	11.61	5.62	5.99	10.66	5.45	5.03
(percentage)	(2.27)	(1.75)	(0.32)	(1.21)	(2.85)	(1.09)	(1.48)	(2.85)	(1.34)
Amount Contributed	110.70	101.13	13.45	97.25	9.57	87.68	77.18	7.23	68.26
(unconditional, dollars)	(23.33)	(28.43)	(3.62)	(13.14)	(36.97)	(14.35)	(15.95)	(36.94)	(17.56)
Amount Contributed	166.0	101.1	13.4	152.6	64.9	87.7	132.3	63.0	68.3
(inclusive of match, unconditional)	(35.00)	(28.43)	(3.62)	(16.07)	(44.90)	(14.35)	(19.53)	(44.83)	(17.56)
Amount Contributed	845	1348	786	59	-503	562	-266	-498	143
(conditional)	(106)	(229)	(162)	(195)	(224)	(275)	(243)	(199)	(349)
Amount Contributed	1268	1348	786	481	-81	562	163	-67	143
(conditional, inclusive of match)	(159)	(229)	(162)	(227)	(272)	(275)	(276)	(233)	(349)
Amount Contributed	845	1010	786	59	-165	224	-266	-160	-195
(conditional, exclusive of credit)	(106)	(197)	(162)	(195)	(205)	(260)	(243)	(183)	(334)
Fraction Open an XIRA	3.49	1.67	1.05	2.45	1.83	0.62	2.07	1.71	0.26
with less than $450	(1.22)	(0.83)	(0.24)	(0.80)	(1.46)	(0.72)	(0.98)	(1.48)	(0.89)
Fraction Open an XIRA	10.04	6.25	0.88	9.16	3.79	5.37	8.59	3.74	4.77
with $450 or more	(1.99)	(1.57)	(0.22)	(0.94)	(2.52)	(0.83)	(1.14)	(2.51)	(1.02)
Number of Observations	229	240	1813						

Notes: The differences in columns (7) to (9) are regression-adjusted for the same co-variates as in table 2. Standard errors in parentheses.

Table 6. Effect of Advance Notification

	Mean, call scheduled	Mean, no call scheduled	Difference	Adjusted difference	Effect of being Reached	Effect of being reached, adjusted
	(1)	(2)	(3)	(4)	(5)	(6)
A. Filers who received the Match in 2006						
Married	0.21	0.19	0.020			
	(0.0062)	(0.0044)	(0.0075)			
2004 Adjusted gross income	33743	31606	2137			
	(491)	(314)	(559)			
Fraction reached	0.28	0.00	0.28			
	(0.0068)	(0.0000)	(0.0050)			
Fraction filing returns with H&R Block	0.64	0.64	-0.002			
	(0.0073)	(0.0054)	(0.009)			
Percent Contributed to XIRA	10.4	8.3	2.1	1.9	6.9	6.1
	(0.71)	(0.47)	(0.83)	(0.99)	(2.69)	(3.24)
Amount Contributed (dollars) (unconditional)	95.3	67.8	27.4	22.5	89.5	73.7
	(9.13)	(5.44)	(10.01)	(11.91)	(32.56)	(38.94)
Amount Contributed (dollars) (conditional)	923.0	846.7	76.4	26.6	182.7	66.0
	(61.59)	(47.51)	(76.84)	(82.76)	(182.29)	(205.25)
Number of Observations	1831	3494				
B. Filers who did not receive the Match in 2006						
Married	0.21	0.20	0.013			
	(0.0062)	(0.0028)	(0.0067)			
2004 Adjusted gross income	33336	31987	1349			
	(466)	(209)	(498)			
Fraction reached	0.32	0.00	0.32			
	(0.0071)	(0.0000)	(0.0033)			
Fraction filing returns with H&R Block	0.65	0.63	0.015			
	(0.0073)	(0.0035)	(0.008)			
Percent Contributed to XIRA	3.0	2.9	0.1	0.2	0.4	0.6
	(0.40)	(0.18)	(0.44)	(0.47)	(1.32)	(1.42)
Amount Contributed (dollars) (unconditional)	18.0	12.7	5.3	6.6	16.1	20.1
	(5.14)	(1.04)	(3.27)	(3.51)	(9.90)	(10.63)
Amount Contributed (dollars) (conditional)	687.3	489.8	197.5	172.5	488.6	432.9
	(171.45)	(23.07)	(93.50)	(98.72)	(229.60)	(246.61)
Number of Observations	1796	8379				

Notes: The variable "call attempted" is 1 for all individuals who were scheduled to be called. The sample for this table excludes all individuals in the "do not call" directory (irrespective of whether they were scheduled to be called). Columns 4 and 6 are instrumental variable regressions, where the endogenous regressor is a dummy equal to 1 if the person was reached, and the instrument is the "call attempted" dummy. Columns 4 and 6 control for the same variables as in table 2 (in column 6, all the control variables are also in the instrument set). There is no adjusted regression for the indicator for whether the taxpayer filed a return with Block, since the control variables are not defined for taxpayers who did not. Standard errors in parentheses.

Table 7. Effect of Advance Notification: Summary Regressions

	Percent Contributed to an X-IRA	Amount Contributed (Unconditional)	Amount Contributed (Conditional)
	(1)	(2)	(3)
A. Reduced Form (Excluding Do Not Call)			
Match	3.56	23.4	318
	(0.32)	(2.06)	(60)
Call sample	-0.19	1.56	147
	(0.40)	(2.61)	(105)
Call sample*Match	1.62	9.8	-82
	(0.60)	(3.92)	(124)
Number of Observations	23214	36291	834
B. IV (Excluding Do Not Call)			
Match	3.56	23.4	318
	(0.32)	(2.05)	(60)
Reached	-0.56	4.94	419
	(1.16)	(8.26)	(297)
Reached*Match	4.93	35.7	-254
	(1.80)	(13.29)	(340)
Number of Observations	23214	36291	834

Notes: This table excludes individuals in the "Do Not Call" directory in panels A and B and controls for all the variables included in table 2. The "call sample" includes all those who were scheduled to be called (based on their social security number); the "reached" dummy indicates whether the individual was reached by phone. In panels A and B, reached and reached*match are endogenous regressors; "call sample" and "call sample*match" are used as instruments. Standard errors in parentheses.

Table 8. Effect of Matches for Automatic Monthly Contributions

	Match	No match	Difference	Adjusted Difference
	(1)	(2)	(3)	(4)
Opened a one-time XIRA (percent)	8.91 (0.71)	2.78 (0.14)	6.13 (0.48)	5.96 (0.55)
Opened a systematic XIRA (percent)	1.67 (0.32)	0.20 (0.04)	1.47 (0.16)	1.47 (0.18)
Opened one-time and systematic XIRA (percent)	1.55 (0.31)	0.14 (0.03)	1.41 (0.14)	1.44 (0.16)
Number of Observations	1617	13261		

Notes: The differences in column (4) are regression-adjusted for the same co-variates as in table 2. Standard errors in parentheses.

Appendix Table 1: Effect of the Match and Credit on X-IRA take-up and contributions

Office Type	Match Versus Credit Presentation					Advance Notification			Match Monthly Contributions		
	50% Match (1)	33 % Credit (2)	Control (3)	Match vs Credit (4)	Credit vs Control (5)	50% Match (6)	No Match (7)	Difference (8)	50% Match (9)	No Match (10)	Difference (11)
Opened XIRA	10.19	6.42	3.34	3.76	3.09	9.12	2.81	6.31	9.09	2.94	6.15
(percentage)	(0.77)	(0.62)	(0.16)	(0.98)	(0.50)	(0.37)	(0.15)	(0.34)	(0.72)	(0.15)	(0.49)
Amount Contributed	56.1	41.5	13.6	14.7	27.9	81.8	14.1	67.7	73.3	18.8	54.5
(dollars)	(5.86)	(5.61)	(0.89)	(8.11)	(3.20)	(4.59)	(1.24)	(3.74)	(8.01)	(1.62)	(5.41)
Amount Contributed	558	672	439	-114	233	913	555	358	823	677	146
(conditional on contribution)	(40)	(63)	(19)	(71)	(49)	(35)	(37)	(55)	(62)	(47)	(84)
Amount Contributed (inclusive	82.5	41.5	13.6	41.0	27.9	117.2	14.1	103.1	105.1	18.8	86.3
of match)	(8.20)	(5.61)	(0.89)	(9.90)	(3.20)	(6.25)	(1.24)	(4.85)	(10.94)	(1.62)	(6.01)
Number of Observations	1561	1588	12703			6063	11515		1617	13261	

Notes: This table presents the X-IRA outcomes across treated and control groups for the three groups of offices. Standard errors in parentheses below the coefficients.

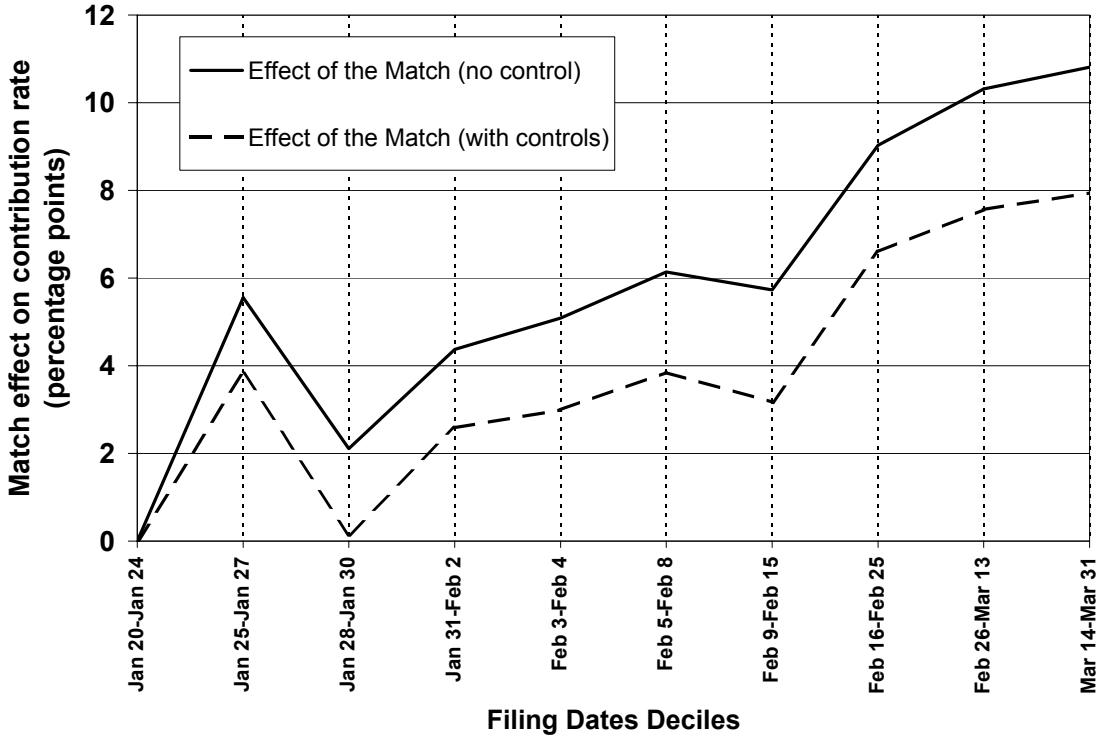
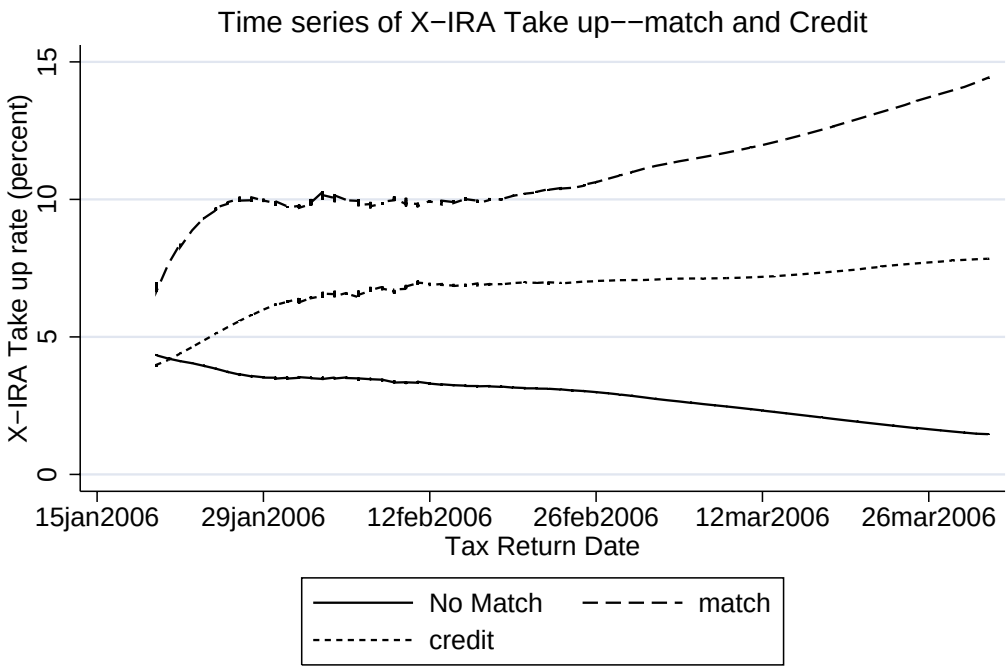


FIGURE 1

Effect of the Match on X-IRA contribution rates over-time

Notes: The figure reports the coefficients of a regression of an IRA contribution dummy on filing dates decile dummies interacted with the match treatment dummy. The regression also includes filing dummies. The omitted decile dummy is the first decile (Jan 20-Jan24).
The bold line does not include any additional controls. The dashed line includes additional control variables (income quartile dummies, presence of investment income, home ownership, tax refund above $500, presence of dependents, and office group) and the interaction of those control variables with the match dummy. Standard errors are around 1 percentage points for each plotted coefficients so that all coefficients (except those for Jan 28-Jan 30) are significant.

Figure 2


Time series of X−IRA Take up−−match and Credit
X−IRA Take up rate (percent)
0
5
10
15
15jan2006
29jan2006
12feb2006
26feb2006
12mar2006
26mar2006
Tax Return Date
No Match
match
credit

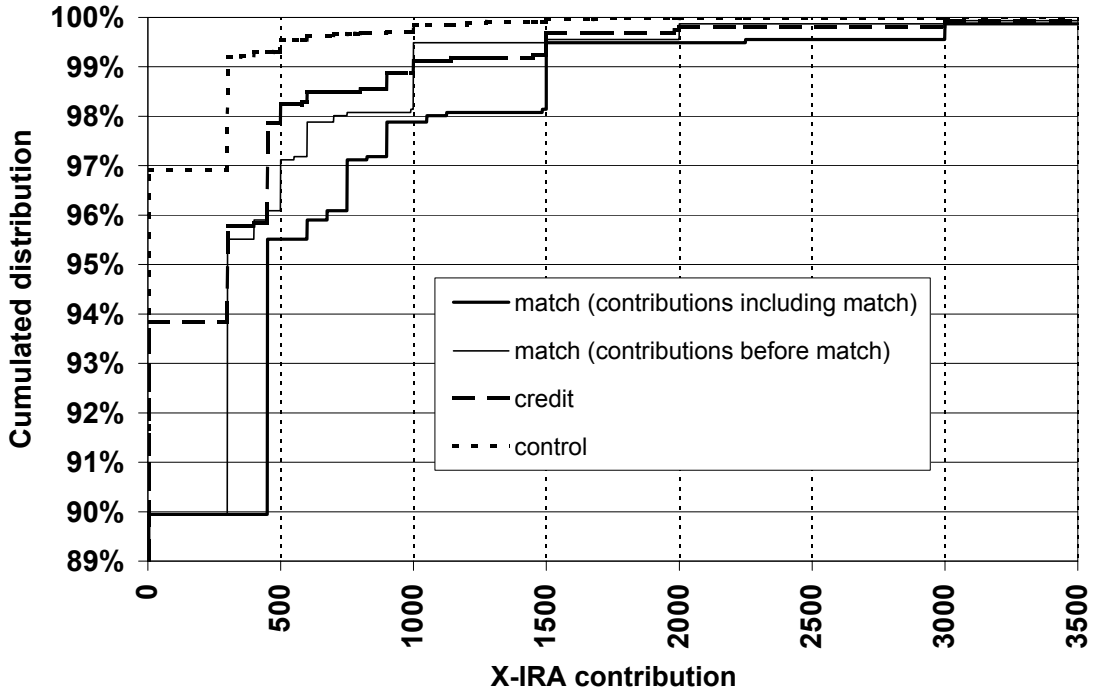


FIGURE 3

Match vs. Credit presentation cumulative distributions of X-IRA take-up

Notes: The figure reports the cumulated distributions of X-IRA take-up (including zeros) for the match presentation group, the credit presentation group, and the control group. In the case of the match group, we report cumulative distributions including and excluding the match.