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INFORMATION ON THE TAKE-UP
OF FINANCIAL INCENTIVES FOR
RETIREMENT SAVING**

Emmanuel Saez

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Emmanuel Saez, University of California, Berkeley and CEPR

ABSTRACT

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

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.

Emmanuel Saez
Department of Economics
University of California
549 Evans Hall, 3880
Berkeley
CA 94720
USA
Email: saez@econ.berkeley.edu

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I. Introduction

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. Madrian and Shea (2001), for example, show that changing the default rules for 401(k) enrollment has a significant impact on employee participation rates. Duflo and Saez (2002) show that the behavior of one's colleagues has a significant causal influence on workers' participation in 401(k) plans. Bertrand et al. (2005) show that changes in the wording of a loan offer have a significant effect on borrowers' choices.

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 raising 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. 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 the federal tax system are difficult to perceive.

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.

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. The new experiment builds on the matching experiments conducted in 2005 and analyzed in Duflo et al. (2006). 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 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.[1] This raises important issues because the saver's credit could be restructured to offer matching contributions at relatively low cost.

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[1] See Davis and Isaac (2005), Davis and Millner (2005, 2006), Davis, Millner and Reilly (2005), and Eckel and Grossman (2003, 2005, 2006a, b, c).

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, Engelhardt and Kumar 2004, Huberman et al. 2003). 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. 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.

Fourth, some of the 2006 participants also took part in the 2005 experiment analyzed in Duflo et al. (2006) and some did not. Hence, we can explore a number of interactions and dynamic effects of the matching experiment in 2005.

Our principal findings can be summarized simply. The matching offer raises take-up and contributions to IRAs. The results are quite robust with respect to the effects of the 2005 experiment (Duflo et al. 2006), once the date of filing of the tax return is

controlled for. 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. As in the 2005 experiment, contribution rates were well below 100 percent, even with a very generous match.

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 appears 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 suboptimal choice, leaving money on the table.

Advance notification more than doubled IRA take-up rates among filers who were eligible for the match. In sharp contrast, it had no effect on take-up among filers who were ineligible for the match. 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 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.

Lastly, we find relatively modest interactions between the 2005 experiment on outcomes in 2006.   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 are only slightly more likely to take-up the match in 2006. 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.

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 four sections examine the effects of the four variations on the basic design described above. The final section discusses conclusions and policy implications.

II. 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 recontributing 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.[2] 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.[3] 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). The credit reflects the same incentive as the match, since a credit at rate t is equivalent to a match at rate $t/(1-t)$. For example, those offered a rebate received $200 in cash if they contributed $600 to an X-

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[2] 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.

[3] 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.

IRA. Hence, the net out-of-pocket cost of having $600 in the X-IRA would be $400, which makes the 33 percent credit rebate equivalent to a 50 percent match. Because the match was offered for contributions of $300 to $1,000, 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 higher contribution levels coupled with a rebate.

Advance notification was implemented in 20 offices. In these offices, 20 percent of 2005 filers, divided 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.

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.[4]

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 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. To ensure that the results are applicable to a broad population of H&R Block clients, we typically 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).

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

The experiment ran from January 20, 2006 to April 2, 2006. Matching offers were deposited in the X-IRA accounts in mid-April, if the tax filer had not withdrawn his or her contributions by that time. Credit rebate checks were mailed to tax filers about two weeks after tax preparation. With the exception of the advance notification calls and letters described above, the experiment was not advertised in advance.[5] 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 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 1 shows descriptive statistics for the treatment and control groups in the three sets of offices. None of the differences between the treatment and control groups are significant at the 5 percent level, which suggests that the randomization was

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[5] Pre-tax-season training sessions were led 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.

successful. About 13 percent of the 2006 sample also participated in the 2005 experiment

(the 2005 experiment ran during only the last month of the tax season). 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.

III. Effects of the Match

Table 2 presents the effects of the basic 2006 match experiments – 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.[6] 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

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[6] 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.

percentage points, after controlling for all of the variables listed in Table 1 (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).[7] Measured at the tax return level, the effects are larger among married couples (8.78 percentage points) than among singles (4.48 percentage points).[8]

These effects 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 entirely attributed to the differences in take-up rates by filing date. Among customers who filed between March 5 and April 1st, 2006, 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 2. 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.[9] To explore this issue

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[7] As we discuss in detail in section VII, assignment status in 2006 was not independent of assignment status in 2005. 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.

[8] 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.

[9] 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

further, Figure 1a plots X-IRA take-up by filing date. For filers with income above the median, and those with income below the median, the effect of the match grows steadily from zero at the earliest filing dates to more than 10 percentage points by the end of the tax season. Figure 1b shows even larger effects of the match over time among married filers, controlling for income group.

Table 3 explores these issues 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. Panel A 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 match status. Panel B shows, consistently with Figures 1a and 1b, that filing a return at a later date, without controlling for any additional variables, reduces take-up when no match is offered but 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. Panel C displays the filing date effects, but now controlling for all the variables introduced in Panel A. Interestingly and perhaps surprisingly, the matching effects of the filing dates in Panel C are almost as strong as in Panel B. This shows that the effects of the match by filing date cannot be explained by differences in the income or other observed characteristics of tax filers. 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

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.

refunds earlier. If so, they would have lower tastes for saving overall and thus would be less likely to respond to saving incentives.[10]

IV. Match versus Credit Presentation

Several lab and field experiments show that matching offers tend to generate more 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 4 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

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[10] 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.

match and the credit to be economically identical, even though they are. As with the general take-up of the match shown in Figures 1a and 1b, 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 4). 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 credit constrained, and thus can not contribute $450 on the spot. However, Panel B of Table 4 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.[11]

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

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[11] 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.

[13] 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.

hence less attractive than a 50 percent match.[13] 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

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.[14]

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

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[14] Eckel and Grossman (2003, 2005, 2006a, b, c) 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 contribution on the common good, so it is not obvious that this feeling would be salient. Another possibility arises from the fact that, unlike in the charitable giving experiments, in our experiments, filers had to wait a few weeks for the credit rebate. This seems to be an unlikely explanation, however, because filers had to wait even longer to receive the matching contribution. Alternatively, 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 Kahneman and Tversky (1979).

the conclusion, the differences in taxpayer responses to the match and credit groups has implications for the design of public policy.

V. Advance Notification

The experiments described above took place without any advance notification of clients.[15] 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.

Table 5 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

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

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 2006 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).[16] Controlling for the covariates,

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[16] 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.

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 subsample 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).[17] 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.[18] In contrast, receiving a call had no 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 5) and to run difference-in-differences regressions. Table 6 shows that all of the key results above about advance notification are robust to this

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[17] 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.
[18] 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." Katz, Kling, and Liebman (2001) discuss the calculation of the control complier mean.

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 1, 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 5. 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 5. 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.

VI. Matches for Monthly Contributions

The results in tables 5 and 6 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 7 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 7, 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 (Choi, et al. 2002, Engelhardt and Kumar 2004, Even and MacPherson 2005, Huberman et al 2003), where matches have positive effects on the take-up of monthly 401(k) contributions.[19] 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

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[19]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.

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. [0]

VII. Interactions Between the 2005 and 2006 Experiments

The 2005 experiment took place between March 5th and April 5th 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. Appendix Figure A shows the relation between 2005 and 2006 experimental status. For offices offering the match versus credit or matching of monthly contributions, filers in the control group or the 20 percent match group in 2005 were controls in 2006. Filers in the 50 percent match group in 2005 and those who were not in the 2005 experiment were in one of the treatment groups in 2006: they could have received the one-time match, the 33 percent rebate, or the monthly contribution option. In the advance notification group, filers in the 2005 control group were in the control group in 2006. All others may or may not have received a match. As noted above, within the match and unmatched groups in these offices, some filers were scheduled to be called. One notable feature of this overall design is that any filer who was in the control group in 2005 was also in the control group in 2006.

We address three aspects of the interactions between the 2005 and 2006 experiments. The first issue is how participation in the 2005 experiment affected tax filers' willingness to return to H&R Block in 2006. Column 1 of Table 8, Panel A shows

that neither participation in the 2005 experiment nor receiving a match offer in 2005 had any effect on filers' willingness to return to H&R Block the following year.

The second issue is whether exposure to the match in 2005 affected behavior in 2006. To examine this question, the second and third columns of Table 8, Panel A investigate whether, among filers who were in the control group in 2006, those who were matched in the 2005 experiment were more likely to contribute in 2006 than those who did not receive a match in 2005. They are indeed slightly more likely to do so: X-IRA participation rates rise by 1.29 percentage points among those who received a 20 percent match in 2005, and 2.27 percentage points among those who received a 50 percent match (although the difference between receiving the 20 percent or the 50 percent match groups is not significant). Recalling that X-IRA participation in the 50 percent match group was 11 percentage points higher than in the control group in 2005, the results suggest that about one in five (2.27 percent out of 11 percent) of the customers who opened an account in 2005 because of the match continue to contribute to the account even after the match is gone. This result implies that there is a significant effect of having an account already set up on the probability of continuing to contribute. However, the effect is much smaller than the effect of a default participation requirement or contribution level in 401(k) settings (Madrian and Shea 2001, Thaler and Benartzi 2004). Overcoming the barriers to setting up an account is not enough to ensure strong participation; having to make the active decision to contribute also plays a major role.

The third interaction issue we examine is how being in the 2005 experiment affects take-up of the match in 2006. Table 8, Panel B suggests that those who participated in the 2005 experiment are 6 percentage points more likely in 2006 to take

up an X-IRA when offered a match than those who did not participate in 2005. However, this may, in part, be due to the fact that late and early filers have different propensities to take-up the match, as shown in Figures 1a and 1b. To address this problem, we exploit the fact that the matches were introduced on a fixed date in 2005. When we control for the date at which the clients filed in 2005 (and its interaction with being matched), the difference drops to 1.6 percentage points, suggesting relatively little impact of prior participation on 2006 participation.

VIII. 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 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 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.

Finally, this experiment provided an opportunity to test the robustness of results in the 2005 experiment (Duflo et al. 2006) and to test for lasting effects of having received a match in the prior year. The 2006 results closely replicate the 2005 results, after controlling for date of filing. We found only small effects of participation in the 2005 experiment on outcomes in 2006. These results, in combination with the advance notification results, suggest that predictability that the program will be available matters much more than prior exposure to (and hence familiarity with and trust of) the program.

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.

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Figure 1a
Time series of X–IRA Take up by income––all
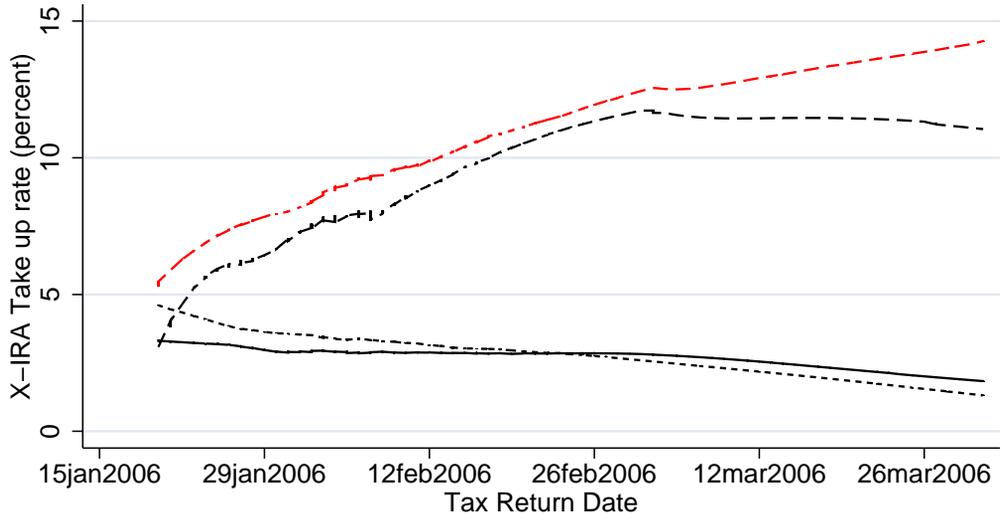
Legend:
- No Match––low income
- No Match––high income
- 50% Match––low income
- 50% match––highincome

X-axis: Tax Return Date (15jan2006, 29jan2006, 12feb2006, 26feb2006, 12mar2006, 26mar2006)

Y-axis: X–IRA Take up rate (percent) (0, 5, 10, 15)

Figure 1b

Time series of X–IRA Take up by income––married
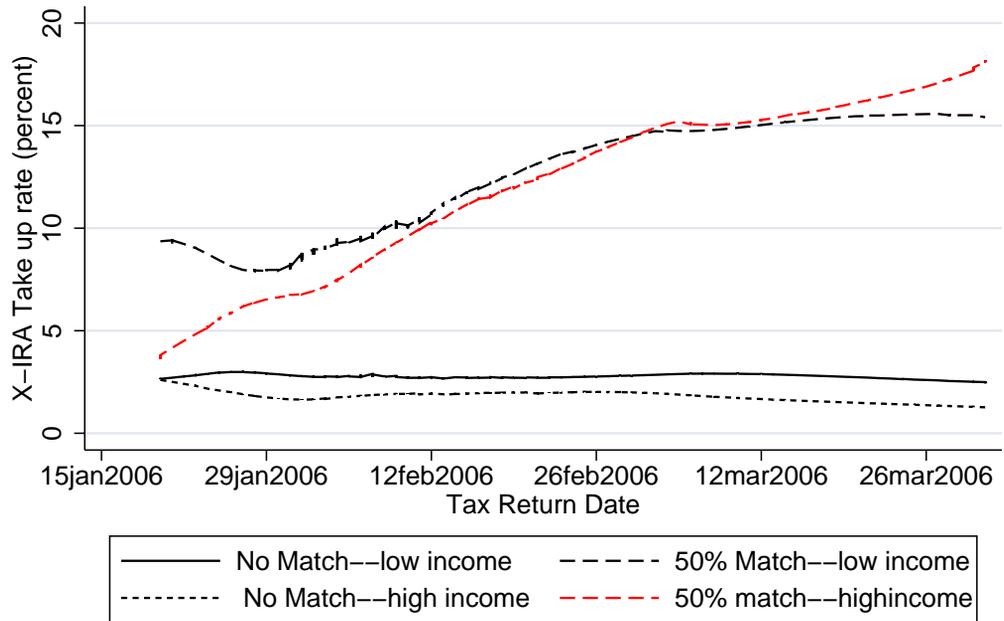
Figure 2

Time series of X–IRA Take up––match and Credit

FIGURE 3
Match vs. Credit presentation cummulative distributions of X-IRA take-up

Appendix Figure A

Match vs. Credit Offices

2005 Status	Not in Experiment, or 50% Match	Control, or 20% Match
2006 Status	50% Match	33% Credit	Control

Systematic Monthly Contributions

2005 Status	Not in Experiment, or 50% Match	Control, or 20% Match
2006 Matched Status	50% Match w/ monthly option	Control

Advance Notification Offices

2005 Status	20% Match, 50% Match, or Not in Experiment	Control
2006 Matched Status	50% Match	Control
2006 Scheduled for Advance Notification	YES "Matched Call" / NO	YES "Control Call" / NO

Table 1. 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
	(7)	(8)	(9)	(10)	(11)	(1)	(2)	(3)	(4)	(5)	(6)
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 2. 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	8.91	3.07	5.84	5.72	11.03	2.26	8.76	8.78	8.04	3.38	4.66	4.48
	(0.35)	(0.09)	(0.26)	(0.29)	(0.71)	(0.15)	(0.46)	(0.51)	(0.40)	(0.12)	(0.31)	(0.35)
Amount Contributed	66	15	51	50	118	18	100	101	45	14	31	29
	(3.71)	(0.75)	(2.36)	(2.61)	(10.32)	(2.03)	(6.47)	(7.14)	(3.00)	(0.68)	(2.03)	(2.25)
Amount Contributed, conditional on contribution	763	540	223	57	1088	868	220	-83	576	456	120	86
	(30.03)	(20.13)	(34.90)	(38.56)	(63.62)	(76.62)	(99.14)	(114.49)	(25.40)	(14.44)	(27.30)	(31.28)
Amount Contributed, inclusive of Match	96	15	81	80	170	18	152	154	65	14	51	50
	(5.09)	(0.75)	(2.82)	(3.12)	(14.21)	(2.03)	(7.82)	(8.62)	(4.09)	(0.68)	(2.37)	(2.63)
Number of Observations	6675	34345			1950	9637			4725	24708		
PANEL B. Was Part of St. Louis Experiment												
Opened XIRA	15.18	3.62	11.56	10.71	17.50	3.14	14.36	14.63	13.66	3.96	9.71	7.84
	(1.46)	(0.29)	(0.95)	(1.02)	(2.46)	(0.43)	(1.46)	(1.57)	(1.80)	(0.40)	(1.24)	(1.35)
Amount Contributed	151	28	123	117	237	39	198	199	95	20	75	60
	(18.32)	(4.14)	(12.81)	(13.89)	(39.86)	(9.12)	(28.35)	(30.43)	(14.72)	(2.91)	(9.39)	(10.23)
Amount Contributed, conditional on contribution	996	808	187	-19	1356	1320	36	-175	693	527	166	68
	(73.84)	(99.82)	(136.37)	(143.38)	(126.17)	(248.63)	(293.01)	(355.48)	(57.85)	(54.34)	(84.51)	(88.21)
Amount Contributed, inclusive of Match	222	28	194	190	349	39	310	314	139	20	119	104
	(26.51)	(4.14)	(14.82)	(16.06)	(57.77)	(9.12)	(32.50)	(34.87)	(21.17)	(2.91)	(11.14)	(12.15)
Number of Observations	606	4008			240	1657			366	2351		
PANEL C. Returns Filed between March 5 and April 1, 2006												
Opened XIRA	12.96	2.01	10.96	10.76	16.01	1.99	14.02	14.15	10.55	2.02	8.53	8.31
	(1.11)	(0.20)	(0.66)	(0.73)	(1.82)	(0.30)	(1.06)	(1.16)	(1.36)	(0.26)	(0.84)	(0.92)
Amount Contributed	159	20	139	136	246	29	218	221	90	14	76	72
	(18.31)	(3.35)	(11.08)	(12.15)	(35.94)	(6.99)	(22.40)	(24.69)	(15.68)	(2.79)	(9.36)	(10.28)
Amount Contributed, conditional on contribution	1240	1108	131	-142	1539	1632	-92	-356	873	756	116	44
	(94.85)	(144.17)	(166.54)	(195.81)	(141.30)	(301.76)	(294.39)	(380.82)	(101.11)	(109.75)	(149.51)	(175.66)
Amount Contributed Inclusive of Match	226	20	205	203	349	29	321	326	128	14	113	110
	(24.43)	(3.35)	(13.05)	(14.30)	(48.26)	(6.99)	(26.47)	(29.15)	(20.34)	(2.79)	(10.82)	(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 1 and a dummy for the office type. Standard errors in parentheses.

Table 3. Effects of the 50 % Match: Individual Characteristics

	PANEL A					PANEL B					PANEL C			
	X-IRA take-up		Amount contributed (unconditional)			X-IRA take-up		Amount contributed (unconditional)			X-IRA take-up		Amount contributed (unconditional)	
	(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)		(9)	(10)	(11)	(12)
	Effect	Effect*match 2006	Effect	Effect*match 2006	Date-of-filing decile	Effect	Effect*match 2006	Effect	Effect*match 2006	Date-of-filing decile	Effect	Effect*match 2006	Effect	Effect*match 2006
Married	-1.05 (0.27)	2.61 (0.66)	0.85 (2.42)	47.98 (5.90)	2: Jan 25-Jan 27	-0.24 (0.42)	5.55 (0.80)	-0.79 (3.80)	25.04 (7.18)	2: Jan 25-Jan 27	0.16 (0.44)	3.82 (1.08)	0.34 (3.96)	15.04 (9.70)
Quartile 2	1.57 (0.34)	1.79 (0.83)	6.08 (3.02)	12.25 (7.44)	3: Jan 28-Jan 30	-0.39 (0.43)	2.11 (0.81)	-2.23 (3.87)	12.85 (7.25)	3: Jan 28-Jan 30	0.20 (0.45)	0.14 (1.09)	-0.89 (4.04)	-0.40 (9.77)
Quartile 3	1.80 (0.33)	0.82 (0.82)	7.32 (2.98)	6.93 (7.37)	4: Jan 31-Feb 2	-0.81 (0.45)	4.37 (0.89)	-2.46 (4.02)	16.22 (8.01)	4: Jan 31-Feb 2	-0.23 (0.47)	2.58 (1.15)	-1.45 (4.18)	2.63 (10.37)
Quartile 4	0.81 (0.35)	2.42 (0.87)	4.33 (3.16)	34.64 (7.87)	5: Feb 3-Feb 4	-0.88 (0.42)	5.09 (0.78)	-3.20 (3.81)	30.38 (7.04)	5: Feb 3-Feb 4	-0.10 (0.45)	2.99 (1.08)	-2.20 (4.01)	12.30 (9.69)
Has investment income	0.65 (0.28)	3.12 (0.69)	12.22 (2.49)	61.75 (6.17)	6: Feb 5-Feb 8	-0.65 (0.46)	6.14 (0.93)	-0.46 (4.16)	41.92 (8.34)	6: Feb 5-Feb 8	0.35 (0.49)	3.85 (1.19)	0.55 (4.37)	20.36 (10.72)
Own a home	-0.27 (0.25)	2.05 (0.62)	-0.11 (2.25)	17.35 (5.55)	7: Feb 9-Feb 15	-0.72 (0.44)	5.73 (0.82)	2.27 (3.92)	52.81 (7.38)	7: Feb 9-Feb 15	0.48 (0.47)	3.16 (1.13)	3.41 (4.20)	22.45 (10.15)
Overpayment >500	1.78 (0.34)	1.52 (0.86)	11.69 (3.05)	-20.94 (7.75)	8: Feb 16-Feb 25	-0.82 (0.44)	9.03 (0.84)	3.07 (3.92)	95.72 (7.57)	8: Feb 16-Feb 25	0.55 (0.47)	6.60 (1.17)	3.88 (4.26)	62.63 (10.50)
Has dependent	1.47 (0.23)	0.11 (0.58)	3.22 (2.10)	8.52 (5.22)	9: Feb 26-Mar 13	-1.11 (0.43)	10.32 (0.84)	7.12 (3.89)	121.66 (7.56)	9: Feb 26-Mar 13	0.49 (0.48)	7.56 (1.18)	8.48 (4.30)	81.75 (10.64)
					10: Mar 14-Mar 31	-2.07 (0.44)	10.81 (0.86)	2.71 (3.98)	134.04 (7.78)	10: Mar 14-Mar 31	-0.26 (0.50)	7.94 (1.22)	4.28 (4.46)	90.29 (10.98)
Number of Observations	41004				Number of Observations	41020				Number of Observations	41004			

Notes: The first decile of returns were filed between January 20 and January 24. Coefficients of the regression of each characteristic (odd columns), and each characteristic interacted with the match (even columns). Panel C controls for the variables in Panel A, office group, and their interactions with the match. Standard errors in parentheses.

Table 4. 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
	(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)	(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
	(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)	(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
	(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)	(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 1. Standard errors in parentheses.

Table 5. 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)			
% 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 Countributed (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 (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)			
% 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 Countributed (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 (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 1 (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 6. Effect of Advance Notification: Summary Regressions

	% Contributed to XIRA	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 1. 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 7. Effect of Matches for Automatic Monthly Contributions

	Match	No match	Difference	Adjusted Difference
	(1)	(2)	(3)	(4)
Opened an XIRA	12.60	2.10	10.50	10.19
	(2.05)	(0.32)	(1.15)	(1.33)
Opened a one-time XIRA	8.91	2.78	6.13	5.96
	(0.71)	(0.14)	(0.48)	(0.55)
Opened a systematic XIRA	1.67	0.20	1.47	1.47
	(0.32)	(0.04)	(0.16)	(0.18)
Opened one-time and systematic XIRA	1.55	0.14	1.41	1.44
	(0.31)	(0.03)	(0.14)	(0.16)
Amount contributed for one-time XIRA (conditional)	1089	1540	-451	-1037
	(134)	(283)	(337)	(525)
Amount contributed per month for systematic contributions (conditional)	82	49	32	15
	(11)	(7)	(13)	(19)
Number of Observations	1617	13261		

Notes: The differences in column (4) are regression-adjusted for the same co-variates as in table 1. Standard errors in parentheses.

Table 8. Interactions between the 2005 and 2006 Experiments

A. Effect of 2005 treatment status on 2006 behavior among those who were not matched in 2006

	Returned in 2006	Contributed to XIRA in 2006	Amount contributed in 2006	Conditional amount contributed in 2006
		(1)	(2)	(3)
Any match in 2005	-0.002	1.29	19	231
	(0.006)	(0.68)	(9.5)	(230.7)
50% match in 2005	0.007	0.98	9.1	116
	(0.006)	(0.79)	(11.1)	(229.3)
Was in 2005 experiment	0.003			
	(0.007)			
Constant	0.644	0.21	-31	-241
	(0.004)	(1.23)	(17.4)	(544.9)
Number of Observations	41334	4008	4008	138

B. Effects of Participation in the 2005 Experiment on Take-up in 2006

	No Control			Controlling for 2005 return date		
	Contributed to XIRA in 2006	Amount contributed in 2006	Conditional amount contributed in 2006	Contributed to XIRA in 2006	Amount contributed in 2006	Conditional amount contributed in 2006
	(1)	(2)	(3)	(4)	(5)	(6)
Match in 2006	5.1	42.5	71.6			
	(0.30)	(2.72)	(41.3)			
Match in 2006* In 2005 Sample	6.3	80.7	-62.8	1.6	6.0	-297.6
	(0.89)	(8.05)	(87.02)	(1.08)	(9.74)	(115.58)
Number of Observations	41004	41004	1558	40571	40571	1549

Notes: The first column of Panel A uses the whole 2006 sample. The other columns of Panel A restrict the sample to people who participated in the 2005 experiment and did not receive a match in 2006. Panel B examines whether those who were matched in 2005 are more likely to take up the match than those who were not. The regressions in Panel A and in the last three columns of Panel B control for all the co-variates in table 1, the office group, the date in which the return was filed in 2005 and, in columns 4 to 6 of Panel B, the date squared, and the interaction of the date and its square with the match in 2006. Standard errors in

Appendix Table 1

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
	(7)	(8)	(9)	(10)	(11)	(1)	(2)	(3)	(4)	(5)	(6)
Opened XIRA	10.19	6.42	3.34	3.76	3.09	9.12	2.81	6.31	9.09	2.94	6.15
	(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
	(5.86)	(5.61)	(0.89)	(8.11)	(3.20)	(4.59)	(1.24)	(3.74)	(8.01)	(1.62)	(5.41)
Amount Contributed (conditional on contribution)	558	672	439	-114	233	913	555	358	823	677	146
	(40)	(63)	(19)	(71)	(49)	(35)	(37)	(55)	(62)	(47)	(84)
Amount Contributed (inclusive of match)	82.5	41.5	13.6	41.0	27.9	117.2	14.1	103.1	105.1	18.8	86.3
	(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 co-variates across treated and control groups for the three groups of offices. Standard errors in parentheses below the coefficients.