

# Why are Benefits Left on the Table? Assessing the Role of Information, Complexity, and Stigma on Take-up with an IRS Field Experiment\*

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## Abstract

We address the puzzle of incomplete take-up with a unique field experiment in collaboration with the IRS. Specifically, we test the role of program information (regarding benefits, costs, and rules), informational complexity, and stigma on response to experimental mailings notifying 35,050 eligible individuals of \$26m in unclaimed EITC benefits. We find residual increases in take-up due to the mere receipt of a mailing (response of 0.14); simplification (+0.09 relative to mere receipt); and the display of benefits (+0.08 relative to mere receipt plus simplification). Surveys affirm pervasive low awareness and misconstrual of program incentives among eligibles. Our estimates suggest that the tested interventions could reduce incomplete EITC take-up from 25% to 22%. (JEL D03 C93 H24 M38)

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# 1 Introduction

A well-documented, and perhaps surprising, feature of transfers to the economically and socially disadvantaged is that many targeted individuals fail to take-up their benefits (Currie 2006). The Earned Income Tax Credit (EITC), the nation's largest means-tested cash transfer program, is a prime example with an estimated 25 percent rate of incomplete take-up that amounts to 6.7 million non-claimants each year (Plueger 2009).<sup>1</sup>

The consequences of incomplete take-up are significant. A typical EITC non-claimant forgoes credits equivalent to 33 days of income.<sup>2</sup> Moreover, non-claimants sacrifice other advantages, such as those related to family health, education, or consumption, that are linked to transfers (Hoynes, Miller, and Simon 2011; Dahl and Lochner 2011; Smeeding, Phillips, and O'Connor 2001).<sup>3</sup> The problem, according to many accounts, is even more severe for other means-tested programs.<sup>4</sup>

Several explanations have been proposed for incomplete take-up: lack of information, stigma, transaction costs, and complexity. Yet, despite considerable research, the determinants of take-up remain poorly understood. In a recent survey of the topic, Currie characterized the phenomenon of incomplete take-up as an academic puzzle and advanced experiments as a means of illuminating its causes (2006). In this paper, we test the effect of a set of novel interventions on take-up with a unique field experiment administered in collaboration with the Internal Revenue Service (IRS). Beyond shedding light on leading explanations as to its causes, we identify strategies through which to improve take-up. Specifically, we test the role of information (regarding program benefits, costs, and rules), the complexity of such information, and program stigma on the take-up of the EITC.

To experimentally assess different theories of incomplete take-up, we modify the informational content and complexity of IRS tax mailings and distribute these to the universe of over 35,000 tax filers from California who failed to claim their TY 2009 EITC despite presumed eligibility and the receipt of an initial reminder notice. Each mailing communicates likely eligibility for the program, and includes a worksheet which a recipient can

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<sup>1</sup>The take-up estimate attempts to improve upon estimates of earlier studies by using Census data linked to administrative tax records. A highly cited take-up rate in the academic literature is the 80 to 86% figure reported by Scholz (1994). Note that the Scholz estimate is from a period, TY 1990, prior to the introduction of a credit for childless individuals (who have lower take-up rates) and prior to the cessation of an IRS practice to automatically send a check to any filing non-claimant who appears eligible.

<sup>2</sup>These calculations are based on author calculations from IRS statistics for TY 2005. For the day of work equivalence, we assume 250 work days each year.

<sup>3</sup>Dahl and Lochner (2011) find increases in child test scores in only the short-run. These effects are strongest for children from particularly poor families.

<sup>4</sup>Among other major transfer programs, 42% take-up the Temporary Assistance for Needy Families Program, 55% take-up the Food Stamp Program, and 46% take-up the Supplemental Security Income program. These figures are estimated for 2004 and are reported in a 2007 report to Congress available at <http://aspe.hhs.gov/hsp/indicators07/report.pdf>.

complete and return to claim a credit. We use the differential rate of return across mailings to evaluate the importance of each tested mechanism. To maximize statistical efficiency, and to permit tests of treatment interactions, we independently randomized the three physical components of the mailing—that is, the reminder notice, claiming worksheet, and an experimental envelope—across the sample by blocks defined by zip code and dependent status. The packets were published, assembled and mailed by the IRS in a single batch in mid-November of 2010. We collected and collated all responses received by mid-May 2011.

The current study builds on the existing research in three substantive ways. To our knowledge, this study represents the first collaboration between the IRS and academics in a field experiment on take-up that involves actual tax forms and real benefits. All told, our study informs individuals of \$26 million in unclaimed government benefits, of which about \$4 million is ultimately claimed due to the mailings. Second, beyond yielding broad insights into the question of take-up, our findings apply to a population—eligible, filing non-claimants—whose responsiveness is of direct interest to policy-makers. Indeed, the outcome of this research offers explicit prescriptions for the redesign of federal notices that could substantially increase the take-up rate of the 35% of EITC eligible non-claimants who file taxes (i.e., 2.4 million individuals) but fail to collect \$1.01 billion in benefits.<sup>5</sup> Finally, the scale of the study provides the statistical power to simultaneously test different explanations for incomplete take-up in the same setting.

We supplement the field experiment with two randomized survey instruments, and a rich set of micro-level tax return data. A first survey of low to moderate income tax filers at free tax preparation sites provides novel insight as to how eligible claimants construe incentives associated with the EITC. A second psychometric survey, administered online, assesses how the experimental interventions alter beliefs of program costs and benefits. Together, the experiment and two surveys permit us to make inferences regarding the average, as opposed to marginal, causes of low take-up, and to illuminate the possible psychological mechanisms that underlie the observed response.

As an initial result, the surveys document a widespread lack of awareness (e.g., 43% of eligibles are not aware of the program) and misconstrual of EITC rules and incentives including eligibility (e.g., 33% of aware eligibles believe they are ineligible), benefit size (e.g., 61% of aware eligibles underestimate benefit size, and those who underestimate benefits do so by an average of 83%), and the likelihood of an audit (e.g., the median belief of an audit is 15% while actual incidence is 1.8%). Low awareness and misinformation affects even those who appear to be program-eligible and is only modestly mediated by the size of the

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<sup>5</sup>This estimate assumes that the average filing non-claimant is owed \$420 (which is the typical amount paid to CP notice respondents in the US for TY 2009) and that there are 2.4 million such non-claimants. The latter figure is based on Plueger’s (2009) estimate of a 35% filing rate, amongst non-claimants, and a total eligible population of 27 million (see discussion of Tables 3 and 10).

potential benefit. Although these survey results are from individuals awaiting assistance from a tax preparer, we cross-validate the results in a second survey sample. This evidence suggests a channel through which information, and its transparency, might shape behavior, and we test this channel explicitly via the field experiment.

The experiment yields five main findings. First, we observe that the mere receipt of a “control” notice, just months after the receipt of near identical, initial IRS notice, prompts 0.14 of the residual non-respondents to take-up (this compares to an initial notice response of 0.41). The robust response to the repeat mailing is consistent with the low program awareness evidenced in the surveys.

Second, the experiment suggests that informational complexity influences response. Relative to the textually dense control notice (0.14), a notice with a simplified layout and less repetition improves take-up by 0.06 ( $p < .01$ ). As a second test of complexity, a worksheet featuring the addition of criteria that do not substantively screen for eligibility, reduces response by 0.04 ( $p < .01$ ) relative to the control worksheet. The combination of the simple notice and worksheet produces a response of 0.23 (the baseline for subsequent treatments).<sup>6</sup> Importantly, the basic program information conveyed by the control notice and worksheet, and the complexity treatments, is equivalent.

Third, providing benefit information also raises take-up. Displaying the upper limit of the potential benefit range improves take-up by 0.08 relative to the 0.23 response elicited by the baseline notice in which no figure is displayed ( $p < .01$ ). Intriguingly, the influence of benefit information on response is not monotonically related to the magnitude of the displayed figure which, for some part of the sample, is randomized to show either a medium sized number (e.g., for someone with one qualifying dependent, the maximum benefit of \$3,043) or larger sized number (i.e., the maximum overall program benefit of \$5,657). The sensitivity of take-up to benefit information is consistent with survey evidence that shows that many are not aware of and systematically underestimate benefit size.

Fourth, we find that attempts to clarify the time and penalty costs associated with completing and returning the worksheet do not improve response. In one treatment, a notice headline indicates that the worksheet requires less than 60, or 10, minutes to complete, while in a second treatment, a message displayed on worksheets offers assurance against penalty for unintentional errors. Neither treatment significantly increases take-up. The former result is not surprising given survey evidence of fairly accurate perceptions regarding the time required to complete the worksheet. Directionally, however, the estimate is consistent with research that the salience of costs may negatively impact response (e.g., Chetty and Saez 2009; Finkelstein 2009). The latter result is notable in light of evidence

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<sup>6</sup> “Non-discriminating” refers to the addition of questions that do not screen for one’s eligibility as per our observation of tax records. However, the added questions may affect the reader’s beliefs of eligibility.

that individuals severely overestimate the likelihood of an audit.

Finally, our attempts to reduce program stigma—either by communicating the high level of peer response or by emphasizing that the credit is a reward for “hard work” rather than a transfer—do not improve take-up. In fact, our effort to reduce stigma by invoking a social norm actually reduces response by 0.05 ( $p < .01$ ). We explore the surprising negative impact of this language given both the psychological evidence as to its efficacy (Cialdini and Goldstein 2004), as well as recent demonstrations of ironic or non-effects in the field (Costa and Kahn 2010; Fellner, Sausgruber and Traxler 2011).

Beyond these main results, we document several additional findings. First, data on benefit claims for TY 2010 reveals that the salutary effects of receiving a second notice on claiming continue to affect take-up the EITC in the following year. The persistence of the effect is consistent with updated information on the EITC as being a mechanism of behavioral influence. Second, the influence of complexity appears subject to a threshold effect in that the detrimental influence of complex notices (or informational flyers) and complex worksheets are not fully additive. This effect is imprecisely measured but implies that even a trace of complexity in program information can dampen response. Third, the differentially lower response to mailings in counties with high Hispanic populations, as well as the higher response in the same regions to the treatment featuring a Spanish language envelope, suggests that language may be a barrier to take-up. Finally, in an analysis of heterogeneity by gender, age, benefit size, and income, females are significantly more likely to respond to the mailings than males, and are more sensitive to the complexity and stigma interventions, which is consistent with the evidence on gender differences in receptivity to informational interventions in other contexts (e.g., Liebman and Luttmer 2011). Of greater policy relevance, those of lower income are also more susceptible to the negative effects of complexity.

By integrating the results from the tax center surveys, and the experimental findings, we can adjudicate between competing frameworks through which to explain incomplete claiming in this context. Overall, we interpret the evidence as difficult to rationalize in a standard model in which individuals balance the costs and benefits of take-up even allowing for the inclusion of costs associated with stigma. In particular, such a model would suggest that interventions would be most impactful for the marginal recipient who’s cost of completing the worksheet outweighs the sizable benefits of claiming. Our findings, including the absence of moderation of the effects by benefit size, implicate factors such as low program awareness, misconstrual of program rules and incentives, and the complexity of claiming documents. The possibility that small, non-informational, changes to the appearance or complexity of claiming forms can yield substantive changes to claiming behavior is in keeping with a model of “hassle costs” described by Bertrand, Mullainathan and Shafir (2006).

A set of second psychometric surveys offers additional mechanistic insight into why take-up appears so sensitive to modest changes in informational content and presentation. The evidence suggests that information, and informational complexity, may shape behavior by prompting both direct and indirect inferences regarding program parameters, and, possibly, by changing the degree to which readers attend to the information.

To organize the competing explanations under consideration, we present a model of the decision to take-up benefits in the Appendix. We first outline a standard framework in which the decision to take-up is a function of the administrative and social costs of the take-up decision and the magnitude of the benefit. We then permit individuals to misperceive benefits and costs in a manner consistent with the underlying psychology and institutional detail of the take-up.

Overall, the potential policy impact of the tested interventions is large. We calculate that the most effective experimental treatments, if applied to the entire population of filing non-claimants, could reduce incomplete take-up among filers from 10% to 7%, and overall incomplete take-up from an estimated 25% to 22%. The increase in response due to our context-based interventions is equivalent to that which would be produced by expanding benefits 101% for this population. This impact would be further augmented, depending on assumptions of efficacy, if notices were distributed to the larger population of non-filing non-claimants. The interventions point to a non-traditional channel through which policy-makers can shape policy (particularly aimed at the poor) in a cost-effective, and possibly scalable, manner (Bertrand, Mullainathan and Shafir 2004). We note that the modest administrative and compliance costs of the interventions, coupled with their anticipated impact on the income distribution, suggest that scaled-up interventions would likely be welfare improving.

Our study should be viewed as augmenting existing research on the question of who takes-up social benefits (see Currie 2006 for a comprehensive survey).<sup>7</sup> Our findings also relate to a recent literature that investigates how information regarding benefit programs affects economic decisions such as labor supply choice and reported earnings (Chetty and Saez 2012; Chetty, Friedman, and Saez 2012; Liebman and Luttmer 2011). Related research has shown that the salience of information, such as sales taxes or road tolls, can also affect behavior (Chetty, Looney and Kroft 2009; Finkelstein 2009). Like these studies, we find that the provision of program information influences decision-making, but this time, at the very basic level of taking-up an owed benefit of potentially large magnitude.

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<sup>7</sup>This literature has traditionally stressed the detrimental role of social stigma (e.g., Moffitt 1983), concrete transaction costs (e.g., Currie and Grogger 2001), and the lack of information (e.g., Daponte, Sanders and Taylor 1998). More recent research implicates the role of non-monetary factors on social and private benefit take-up, such as the transparency of information (e.g., Saez 1999; Jones 2010), costs of inconvenience (Ebenstein and Stange 2010), as well as the actions of one's peers (e.g., Duflo and Saez 2003).

Our finding that simplification improves response is in the spirit of a burgeoning literature on the beneficial impact of simplified information on decisions.<sup>8</sup> Methodologically, the closest analogue to the present research is a study in which direct mail varying the economic terms and the informational presentation of loan offers are randomized by a South African lender (Bertrand et al. 2009). Lastly, our survey findings are related to other studies that have documented that those eligible for benefit programs may lack awareness of, or may misconstrue, program incentives (e.g., Liebman and Zeckhauser 2004; Chetty and Saez 2009; Maag 2005). The misconstrual of incentives has implications for tax code design as well as for considerations of welfare (Congdon, Kling, and Mullainathan 2009).

## 2 Background on EITC and Take-Up

### 2.1 Program Structure and Summary

The EITC, (or more recently, the “Earned Income Credit,” or EIC), was conceived in 1975 as a small offset to payroll taxes and as “an added bonus or incentive for low-income people to work.”<sup>9</sup> As a result of five subsequent expansions, notably in 1986, and then again in the 1990s, the EITC distributes \$58B in refundable credits to nearly 27 million working people of low to moderate income (TY 2009).

The program can be characterized by a small number of parameters—a negative, phase-in, tax rate, a plateau tax rate, the income at which the tax supplement is phased-out, and the positive, phase-out tax rate—specific to one’s number of qualified dependents and filing status. Eligibility for the credit requires a valid SSN, earned income below a specified threshold, minimal investment income, and a failure to have been excluded from the program due to past negligence. Having met these criteria, the size of one’s benefit is determined by one’s income and family structure. While a credit of up to \$457 is available to earners with no dependents, those with qualified dependents—based on a complicated set of relationship, age, and residency tests—command much larger credits of up to \$5,667 (TY 2009).<sup>10</sup> The credit begins to diminish at an income of \$21,500 (for a family with 3 children), and is fully exhausted for earned incomes above \$48,321 (TY 2009). Individuals

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<sup>8</sup>These studies have shown that the transparency and clarity of information may affect parental school choice (Hastings and Weinstein 2008), applications for college financial aid and college enrollment (Bettinger et al. 2009), health care choices (Kling et al. 2011), and savings/investment decisions (e.g., Beshears et al. 2010; Madrian and Shea 2001; Choi, and Laibson, and Madrian 2009).

<sup>9</sup>Quotation cited from a 1975 Senate Committee Report. For an excellent historical review, see Ventry, D. Jr., (2001): “The Collision of Tax and Welfare Politics: The Political History of the Earned Income Tax Credit,” in “Making Work Pay: The Earned Income Tax Credit and its Impact on America’s Families,” edited by B. Meyer and D. Holz-Eakin, pp.15-66, New York: Russell Sage Foundation Press.

<sup>10</sup>While those without dependents must be 25 to 65 years old, there is no age restriction for those with qualified dependents, so long as the enrollee is not a qualified dependent of someone else.

in 21 states may accrue additional local credits, from 3.5% to 43% of the federal credit.<sup>11</sup> Appendix Figure A1 displays the benefit schedule for single and married filers.

Because the program, unlike other anti-poverty programs, is administered through the tax system, to receive a credit, eligible individuals must file taxes. Those with no qualified dependents must file a 1040, 1040A, or 1040EZ, and indicate their benefit amount or simply write “EIC” when prompted. In the case of qualified dependents, eligible individuals must file a 1040 or 1040A along with a supplementary, one-page, tax addendum (the Schedule EIC).<sup>12</sup>

The first two columns of Table 1 describe the average benefit and demographic characteristics of EITC recipients. In TY 2009, the typical recipient received \$2,185 from the EITC (13% of adjusted gross income). This compares to a typical benefit of all non-claimants of \$1,096 (12% of adjusted gross income) (calculated from Plueger (2009)). Approximately balanced with respect to gender, 77% of claimants had at least one qualified child, and only 34% of claimants prepared their own taxes.

## 2.2 Take-Up in the EITC

Despite considerable interest in the question, accurately measuring take-up of the EITC (i.e., eligible claimants / eligible individuals) is difficult. The difficulty stems from the unknown rate of ineligible claiming, the unobserved attributes that govern eligibility, and the unreliability of simple imputations that equate eligible non-claimants with eligible claimants (Berube 2006).

A recent analysis by the IRS, which informs assumptions used in this study, suggests an overall program take-up rate of 75% (with a confidence interval of 73% to 77%) based on data for TY 2005 (Plueger 2009).<sup>13</sup> This estimate attempts to improve upon earlier academic studies including Scholz’s oft cited estimate of 80% to 86% for TY 1990 (Scholz 1994).<sup>14</sup> Plueger estimates that of the 25% who do not take-up, 16% do not file taxes while 9% file taxes but fail to claim a benefit on their return, implying an overall rate of take-up among eligible tax-filers of 90%. Take-up appears to further vary with observable

<sup>11</sup>Figures do not include the District of Columbia or 2 localities (Montgomery County, and New York City) that provide benefits at the municipal level. All data is as of June 2011 and reported on the IRS website: <http://www.irs.gov/individuals/article/0,,id=177866,00.html>.

<sup>12</sup>Claimants must file a tax return even if they fall below the filing requirement threshold.

<sup>13</sup>Plueger’s estimate is based on an exact match of tax records and census data. Specifically he estimates eligible claimants from the Survey of Income and Program Participation (SIPP), and IRS studies of EITC compliance, and estimates the number of total eligible from the American Community Survey, SIPP, and the CPS Annual Social and Economic Supplement (Plueger 2009).

<sup>14</sup>As Plueger (2009) notes, the Scholz analysis was for a period in which apparently eligible, filing non-claimants were automatically mailed a benefit by the IRS, and in which there was no credit for those without a qualified dependent (a group for whom incomplete take-up may be particularly low). The estimate from Scholz (1994) is based on SIPP data for TY 1990.



demographic and tax characteristics including benefit size, the number of one’s dependents (i.e., 56% if no dependents, 74% for those with 1 dependent, and 86% for those with 2 or more dependents). Others have characterized non-claimants as being primarily male, and having lower income, larger families, and lower education than claimants (e.g., Blumenthal, Erard and Ho 2005).

Take-up in the EITC is relatively high compared to other major transfer programs, in part, perhaps, because it is administered through the tax system. Researchers have estimated that, of those eligible, 42% take-up benefits in the Temporary Assistance for Needy Families Program, 55% take-up benefits in the Food Stamp Program, and 46% take-up benefits in the Supplemental Security Income program.<sup>15</sup>

### 2.3 CP Reminder Notification

The IRS mails reminder notices and claiming worksheets, (the CP09 targets those with dependents, and the CP27 targets those without dependents), to anyone who files a tax return and neglects to claim their credit despite appearing eligible based on administrative screens such as filing status, age, earned income, investment income and foreign income.<sup>16</sup> However, Plueger notes that the filters may also screen out some fraction of eligible filing non-claimants (Plueger 2009).<sup>17</sup> These reminder notices consist of a one page (double-sided) letter summarizing the program, detailing eligibility requirements and directing the reader to an attached worksheet. The one-page (single or double-sided, depending on the inferred presence of qualified children) worksheet confirms eligibility into the program with a series of screening statements. Those who sign and return the worksheet, if approved, receive a benefit check within three months.

The response to the CP mailings varies over time, as well as by state, but has ranged from 41% to 52% nationally for TYs 2006 to 2009.<sup>18</sup> The second set of columns of Table 1 suggests that CP notice recipients, in comparison with EITC claimants, on average, have a lower benefit (\$412) and adjusted gross income (\$10,448), and are more likely to be male (69%), childless (76%), and self-preparers (70%).

<sup>15</sup>These figures are estimated for 2004 and are included in a 2007 Health and Human Services report to Congress available at <http://aspe.hhs.gov/hsp/indicators07/report.pdf>.

<sup>16</sup>“CP” refers to “Computer Paragraph” and denotes the varied missives that the IRS routinely sends to taxpayers after a tax-filing.

<sup>17</sup>Based on the analysis of TY 2005 returns detailed in Plueger (2009), we believe the incongruity between the population of CP recipients and number of filing non-claimants may be due to ambiguity in perceived eligibility (e.g., taxpayers with dependent children older than 18 may not be sent a notice since the IRS cannot infer the dependent’s school enrollment status) or to a variety of procedural rules governing the processing of returns (e.g., returns submitted after April 15th and sufficiently outside the normal processing year may not generate a notice). See Plueger (2009) for a detailed discussion.

<sup>18</sup>Author calculations from internal statistics from the IRS.

### 3 Survey Evidence on Perceptions of the EITC

We preface the experiment with novel evidence from an initial survey instrument, hereafter the “Chicago Survey.” Understanding awareness and (mis)construal of the costs and benefits of the EITC, among eligibles, may point to channels that affect take-up. While others have measured awareness as well as comprehension of marginal incentives, we believe that our survey is the first to gauge how accurately low income filers perceive various EITC cost and benefit parameters. We administered a paper survey to approximately 1,200 clients at low-income tax-help clinics from February to April 2011 during the “intake” period when clients wait to be seen by a volunteer preparer. Further details of the survey design and implementation, as well as possible limitations, are provided in the Appendix.

**Program Awareness.** A first finding of the survey is a widespread lack of awareness regarding EITC existence. As reported in Panel A of Appendix Table A1, across the sample of 877 responses, only 54% claim to be aware of the EITC (referred to as both the “Earned Income Tax Credit” as well as the “EITC”). Even amongst individuals deemed program eligible, based on self-reported characteristics, awareness is only 56%. These figures, which may overestimate awareness if those who did not respond to the item are disproportionately unaware of the program, are on the lower end of the range established by other survey evidence on EITC awareness (Maag 2005; Romich and Weisner 2002; Ross Phillips 2001; Smeeding, Ross Phillips and O’Connor 2000).

**Perception of Benefits and Eligibility.** A second finding from survey, reported in Panel B of the table, is evidence of pervasive misinformation regarding program costs and benefits. While 65% of the sample appears eligible based on self-reported data, only 45% of respondents believe themselves to be “definitely” or “probably” eligible. We characterize 33% of the response sample as “under-eligible,” in that they believe themselves to be ineligible when they, in fact, appear eligible; this compares to 12% of the sample which over-estimates eligibility.<sup>19</sup>

Beyond misconstruing eligibility, recipients often mistake the magnitude of owed benefits. Among those who, correctly, believe themselves eligible, 61% underestimate benefit size. While the median ratio of expected to actual benefits is .76, among those who underestimate their benefit, this ratio falls to .17. 41% of respondents believe their benefit is less than 50% its actual magnitude.<sup>20</sup>

**Perception of Costs.** One cost whose distorted perception might affect take-up is the perceived time required to complete and return an EITC claiming worksheet (from the

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<sup>19</sup> Amongst the under-eligible, 56% believe they fail eligibility due to the income test.

<sup>20</sup> In order to keep the survey brief and simple, we could not elicit the full set of information required to determine exact eligibility and benefit size. For example, we do not ask about investment income or an invalid Social Security Number which may disqualify an individual. However, we believe that for the large majority of individuals, our inferences regarding eligibility and benefit size are accurate.

CP notice or our experimental mailings). Beyond time required to gather administrative records, such as a social security number, we hypothesize that completing a worksheet requires less than 10 minutes.<sup>21</sup> Indeed, after reading a sample notice and worksheet, the mean estimate of claiming time is 24 minutes, 93% of respondents anticipate spending less than 60 minutes to complete the worksheet, and 92% are not willing to pay more than \$100 to outsource the task to a third party. These data suggest that perceptions of worksheet claiming may not be strongly miscalibrated (in absolute terms) or, in the least, may not be an important deterrent to response.

Survey respondents do overestimate a second cost of claiming: the likelihood of an audit or penalty. The median respondent believes 15% of all EITC claims will be subject to audit which amounts to 14 times the overall audit rate of 1.1% and 8 times the 1.8% audit rate of EITC claimants (the mean response is 25%). We also find evidence consistent with document complexity—low notice comprehension (40% answer a comprehension question incorrectly), partial ignorance of instructions (20% fail to follow rounding instruction on income reporting), and high non-response (41% do not complete last page of survey). Finally, the table reports modest to mixed evidence for the presence of stigma associated with the program (i.e., 32% disagree or strongly disagree that people “respect anyone who receives an EITC benefit”).<sup>22</sup> Further data, documented in the appendix, suggests that the low awareness, and misperception of various benefit and cost parameters is only mildly moderated by an eligible individual’s benefit size.

**Perception of Take-Up.** Intriguingly, much of the surveyed sample, recognizes the prevalence of incomplete take-up. That is, 76% of respondents believe incomplete take-up is at least 20% while 35% accurately estimate the quintile within which actual incomplete take-up rate falls (i.e., 20% to 40%). Given recognition of non-claiming, one strategy to illuminate the determinants of take-up is to simply ask potential claimants. Appendix Table A2 indicates that, conditioned on awareness, the surveyed sample, as well as just surveyed eligibles, attribute failure to claim to confusion over eligibility, or more general confusion over program rules, but not to program stigma or fear of penalties.

Overall, the survey evidence suggests low program awareness, as well as pervasive misinformation regarding eligibility, benefit size, and certain cost parameters. This misconstrual of program incentives suggests possible channels through which to increase take-up. We

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<sup>21</sup>The worksheets inquire as to the validity of one’s social security number, as well as the number of one’s qualified children based on age and residency.

<sup>22</sup>The surveys indicate that 14% of readers strongly disagree, (and another 18% simply disagree), with a statement claiming that people generally “respect” anyone who receives an EITC benefit and 11% strongly disagree, (and another 29% simply disagree), with a statement stating that an individual “would not care” if their friends were aware of the benefit. While it is difficult to construe the strength of this evidence, we interpret this as, at most, an indication that a small to moderate fraction of individuals are stigmatized by the program.

next describe a field experiment through which we investigate such channels.

## 4 Experimental Design

### 4.1 Sample

The sample for the field experiment consists of individuals from California who satisfy the following conditions. First, the taxpayers filed a tax return for TY 2009 but failed to claim an EITC credit. Second, the taxpayers satisfied a set of screens, enumerated above, that resulted in the receipt of a CP09 or CP27 notice indicating likely EITC eligibility. Finally, the taxpayers neglected to respond to this CP notice.<sup>23</sup>

Table 2 traces the experimental sample from the original population of eligible non-claimants through a series of step-wise eliminations. (figures in bold are exact). Of the approximately 3 million eligible individuals in CA, for TY 2009, an estimated 263,000 filed taxes. Of this group, 76,440 received a reminder notice indicating a possible unclaimed benefit. The large divide between eligible filing non-claimants and those receiving the CP notification is due to a variety of factors which include a policy of minimizing notices sent to possibly ineligible individuals, the exclusion of various filing groups (e.g., taxpayers who file electronically but print and mail their returns), and, possibly, imprecision with which the eligibility figure itself is estimated.<sup>24</sup> Of the 45,099 taxpayers that failed to respond to the CP notification mailing, a further 7,096 individuals are excluded by the IRS, in part, because of an incorrect mailing address, and 2,953 are excluded due to an inaccurate inference regarding the number of dependents during the randomization stage.<sup>25</sup> The 35,050 remaining individuals—23,618 with no dependents, and 11,432 with at least 1 dependent—constitute the experimental sample.

### 4.2 Interventions

A first component of each experimental mailing is a one-page, two-sided, notice. The notice informs the recipient of possible program eligibility, briefly explains the purpose of the program, provides instructions as to how to verify eligibility via the accompanying worksheet, and offers sources for additional assistance. The second component is a one-page,

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<sup>23</sup>The choice of tax year was motivated by a desire for recency, while the choice of state, as well as the decision to target filing non-claimants was dictated by the IRS.

<sup>24</sup>See Table 10 of Plueger (2009) for a detailed accounting of nationwide filing non-claimants for TY 2005. We obtained further details of this accounting from interviews with D. Plueger (August 2011).

<sup>25</sup>During the randomization when interventions were assigned to each anonymized taxpayer, our inference of dependents relied on the presence of a child SSN. We later obtained explicit data on number of dependents and learned that our earlier inference was a noisy one. Of the 2,953 mischaracterizations, 2,324 are dependent-free individuals who received dependent worksheets, and 629 are individuals with dependents who received a dependent free worksheet. We ignore these individuals in the remaining analysis.

two-sided, eligibility worksheet featuring eligibility screening statements and accompanying check boxes (e.g., “My Social Security card reads ‘Not Valid for Employment’...”). If eligible, the recipient is asked to sign, date, and return the last page of the worksheet. Finally, the notice and worksheet are enclosed in a standard #10 sized envelope (4.125 inches x 9.5 inches).

We generate the treatment mailings by first creating a simplified version of the initial CP notice and worksheet (which we retain as a control intervention) and, from this “baseline” mailing, introducing further modifications in the notice headline and summary text or in the messaging above the worksheet header. The informational and stigma treatments can then be measured against the “baseline” mailing with the simplified notice and worksheet. Finally, envelopes are either plain or feature a prominent line of text extending from the center to the right margin. Table 3 organizes experimental treatments by the intended mechanism to be tested, while Figure 1 presents the interventions by mailing component (i.e., notice, worksheet, and envelope). Examples of notices, worksheets and the envelope are provided in the Appendix.<sup>26</sup>

**Informational Complexity.** A first category of interventions tests whether the complexity with which information is presented affects take-up. Recent research suggests how informational complexity may influence important economic decisions across a variety of contexts (e.g., Bettinger et al. 2009; Hastings and Weinstein 2008; Beshears et al. 2010).

We manipulate complexity via two interventions. In the aforementioned baseline notice (or “simple notice”), we reduce the volume and “design complexity” of the information relative to the original/initial notice. While the initial notice is a textually dense, two-sided document that emphasizes eligibility requirements repeated later in the worksheet, the new notice occupies a single side, features a larger and more readable font (“Frutiger”), a prominent headline, and does not repeat eligibility information (Appendix Panel A1).<sup>27</sup> A (slightly modified) version of the initial CP notice is included as a control to permit a direct test of the format simplification of the baseline notice (i.e., front side displayed in Appendix Panel A2). Importantly, the basic informational content across the simple and complex notice, when coupled with the accompanying worksheet, is unchanged.

A second intervention manipulates the “length complexity” of the worksheet. While everyone in the sample receives a worksheet with a simplified design and layout, those assigned to the complex worksheet treatment receive a worksheet lengthened with additional eligibility statements that, critically, do not serve as substantive screens of eligibility. That

<sup>26</sup>While the experiment follows a 6 notice x 4 worksheet x 2 envelope design, because some content must be customized to reflect the number of dependents, and due to alternate versions of some select notices, the number of distinct mailings is quite large.

<sup>27</sup>The simplified notice is adapted from a layout originally designed by a third party firm retained by the IRS and pre-tested for “readability” in a test lab.

is, the additional statements communicate criteria that, by our observation of tax records, will not impact eligibility. Specifically, in Step 1 of the worksheet, we present additional screens for earned income, foreign earned income, investment income, citizenship and filing status which the reader has already satisfied (Appendix Panel B). For those with no dependents, the experimental worksheet features a new section that elicits more detailed information on earned income for the recent tax year. All notices offer clear instructions as to how to seek further assistance or clarification by phone or online.

**Information on Program Incentives.** A second set of five treatments tests for whether information regarding program existence or perceived benefits and costs influence take-up. Psychologists have long recognized the limited attentional or processing capacity of decision-makers (e.g., Kahneman 1986), while economists have recently documented the impact of incentive information (e.g., Liebman and Luttmer 2011; Chetty and Saez 2009), or the increased salience of such information (e.g., Chetty, Looney and Kroft 2009; Finkelstein 2009) on economic choice.

We test for the influence of benefit information by prominently reporting the upper bound of one’s potential benefit (we did not receive permission to print the exact figure) in the headline of the simplified baseline notice. Treated recipients without a dependent receive a notice indicating eligibility for a benefit “...of up to \$457.” In order to generate variation in the magnitude of perceived benefits, for those with either 1 or 2 dependents, we additionally randomize the amount reported to either reflect the maximum dependent specific benefit (i.e., \$3,043 for 1 dependent, and \$5,028 for 2 dependents) or for the program as a whole (i.e., \$5,657) (Appendix Panel C). For example, for recipients with 1 dependent in this treatment arm, the notice either declares that the recipient may be eligible for a refund of up to \$3,043 or \$5,657.<sup>28</sup>

We similarly test how perceptions of transaction costs affect response by offering varying guidance, in the notice headline, as to the time required to complete and return the eligibility worksheet. As an example, we communicate that worksheet completion requires “...less than 60[10] minutes” where the specific magnitude, (i.e., 60 or 10), is again randomized among those assigned to this treatment (Appendix Panel D).

A third informational intervention shapes perceptions of costs by offering an assurance that recipients will not face punitive consequences if they mistakenly report incorrect information. We implement this intervention with bold messaging, placed above the headlined, designed to indemnify readers against the fear of reporting incorrect eligibility information on one-half of all worksheets: “Complete to the best of your ability—you will NOT be penalized for unintentional errors.”

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<sup>28</sup> Recipients with 2 dependents receive a notice displaying a maximum benefit of either \$5,028 or \$5,657. Those with 3 or more dependents receive a notice indicating a maximum benefit of \$5,657.

Fourth, to test the influence of additional general program information on response, we attach a one-page flyer, adapted from that used by Chetty and Saez (2009), to select baseline notices. The flyer displays benefit information and marginal incentives through an annotated graphical display (customized by estimated number of dependents; figures are for single, as opposed to married, filers). We believe that this is the first instance in which the trapezoidal benefit schedule has been depicted on IRS documentation. The flyer also contains a section on “Myths and Realities of the EITC” intended to clarify potentially confusing aspects of eligibility rules and requirements (an example of a “myth”: “I need to have a bank account to receive EIC benefits”) (Appendix Panel D).

Finally, to assess whether inattention to the mailed information leads to non-response, we display a prominent message on the experimental envelope, relative to an unmarked control, indicating that the enclosed contents may benefit the recipient: “Important — Good News for You” (Appendix Panel E). By IRS request, the treatment envelopes also include a parenthetical Spanish translation of the message.<sup>29</sup>

**Stigma.** A final set of treatments tests for whether program stigma influences response. While early economic models of take-up featured social stigma as a primary cost (Moffitt 1983), recent scholars have made the distinction between social stigma, and the related construct of personal (or internal) stigma (e.g., Stuber and Schlesinger 2006; Manchester and Mumford 2010). Personal stigma occurs when an individual internalizes existing negative beliefs or stereotypes that others hold towards the stigmatized target.

We test for the role of stigma by providing cues meant to lessen the personal and social stigma associated with the program. A first notice headline, aimed at reducing personal stigma, emphasizes that the benefit is an earned consequence of “hard work” rather than a welfare transfer: “You may have earned a refund due to your many hours of employment.” Past research in the lab has suggested that the framing of government benefits may affect the resultant behavioral response (Epley, Mak and Idson 2006). A second notice headline, aimed at reducing social stigma by invoking a social norm, communicates that a high fraction of peers claim their benefit: “Usually, 4 out of every 5 people claim their refund” (e.g., Cialdini 1989; Cialdini and Goldstein 2004; see Lindbeck, Nyberg and Weibull 1999 for a discussion of social stigma and social norms).<sup>30</sup>

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<sup>29</sup>Due to IRS rules governing messaging outside the envelope, we had little latitude in choosing the precise verbiage. We can disentangle the effects of including Spanish language from the envelope messaging indirectly by examining differential responses for subpopulations in the sample that we believe may be Spanish speaking.

<sup>30</sup>While it is possible that some recipients have ex-ante beliefs about the rate of take-up higher than the figure we provide, the Chicago Survey suggests that our statistic raises the belief of most filers regarding the take-up rate.

### 4.3 Randomization

We randomly assign subjects to an experimental notice (including a condition with the baseline notice plus the informational flyer), worksheet, and envelope in three independent assignments. Conditioned on assignment to a notice displaying benefits (with at least 1 dependent), stigma, or claiming cost, we subsequently randomize recipients into one of the available treatment variations. All randomizations are conducted within blocks defined by zip-code and the presence of eligible dependents generating a total of 3,483 blocks. In this way, our blocking design is constructed to reduce experimental variance and produce more efficient estimates than a simple randomization.

Treatments are randomized with equal sample weights with three exceptions. First, the baseline notice is over-sampled (x 4) in order to maximize the statistical power of tests between pair-wise comparisons. Second, we also over-sample the benefit information notice (x 3) so as to power tests of differentiation across listed benefit amounts and heterogeneity tests by actual benefit size. Finally, at the behest of the IRS, we limit the lengthier worksheets to 25% (rather than 50%) of the sample.

Balancing tests, implemented through a series of regressions, ensure that the treatment samples are similar across key observables such as earned income, adjusted gross income, benefit size, filing status, and past EITC claiming behavior. The analysis, outlined in the Appendix, suggests that the randomization was successful (Appendix Table A3).

## 5 Results

### 5.1 Overall Response

Table 4 reports a first key result of the field experiment—the magnitude of the overall response. The overall response to the mailing is 0.22 with an average disbursed benefit of \$511 (0.25 response and \$247 for those without dependents, and 0.16 response and \$1,531 for those with).<sup>31</sup> Relative to the response to the initial CP notice of 0.41, the experimental treatments augmented response by 32% (i.e.,  $[0.22*(1-0.41)] / 0.41$ ). The additional response does not appear to be driven by denied claims and involves benefits comparable in magnitude to those received by earlier respondents.

A plausible skeptic might point out that the second notices were mailed in mid-November of 2010, at which point the responses to the first notices may not have yet been exhausted.

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<sup>31</sup>Throughout the analysis we either report results for recipients with and without dependents separately or account for the presence of dependents in pooled analysis. We take this approach because the two groups are characterized by markedly different benefit levels and response rates and may therefore be subject to very different selection processes, and because the informational content of the mailings, and, in some cases, the design of the interventions is specific to the presence of dependents.



In the absence of the experimental notices, how many additional respondents would have returned their initial notice? Figure 2 plots the processing date for any initial notice returned since July 2010 and for all experimental notices.<sup>32</sup> The plot suggests that the counterfactual response to the initial notice would have been minimal.<sup>33</sup>

How is it that the mere receipt of a second notice, just months after the receipt of a first notice could prompt such substantive additional response? Our favored explanation, and one which finds support in the survey, is that the experimental mailings help to combat the effects of inattention or low program awareness either through repeat exposure, or by improving the likelihood that the information is attended to carefully.

Alternative explanations exist. It is also possible that the simpler and more informative experimental designs heighten response. However, the control condition, featuring the duplicated initial notice, still commands a response of 0.14. Notably, while our control worksheet is lengthier than that of the original CP mailing, it does feature, like all the treatments, the simpler design.<sup>34</sup> While we cannot rule out that the new worksheet design alone is responsible for this positive response, given the magnitude in question, this seems unlikely. A second alternative is that the receipt of the second notice prompts recipients to modify their beliefs regarding eligibility or some other program parameter. However, the basis for any such inference regarding eligibility cannot be information contained in the second notice control as it contains no new information. Finally, high response may be due to lost or unopened mail that is, at least partially, stochastic in nature.<sup>35</sup>

A second finding addresses whether language is a barrier to response in this context. Table 4 reports an adjusted response rate that accounts for the high density of potentially non-English speaking households throughout CA. We approximate this language neutral take-up rate by modeling response using ZIP code level data on the Hispanic population density from the Census Bureau (2010).<sup>36</sup> Appendix Figure 3 depicts the negative correlation (statistically significant) of response and ethnic density for the Los Angeles area. We predict that overall response would rise from 0.22 to 0.25, (i.e., 0.26 without dependents, and 0.21 with dependents), assuming that response rates, conditional on covariates, were

<sup>32</sup>According to interviews with the IRS, processing dates fall days to a couple of weeks after the receipt of a worksheet. There is a period in early January, as depicted in Figure 2, when the IRS does not process EITC claims.

<sup>33</sup>A more formal model, assuming response follows an AR(1) process, suggests that the adjustment would yield an additional 22 responses without a dependent, and only an additional 2 responses with a dependent.

<sup>34</sup>As we were limited by the IRS to testing a single experimental worksheet design, we chose to test the influence of question length complexity rather than testing changes in response due to worksheet layout.

<sup>35</sup>We were unable to get information on the rate of returned mail for either the initial notice or the experimental mailings. The baseline rate of unopened mail, from the surveys, is 14%.

<sup>36</sup>Specifically, we estimate the regression  $Response_{ij} = \alpha + \theta HispDens_j + X_i\beta' + \varepsilon_{ij}$  where  $Response_{ij}$  is a binary indicator of a returned worksheet for person  $i$  in zip code  $j$ ,  $HispDens_j$  is the fraction of Hispanic households in zip code  $j$ , and  $X$  is a vector of controls including a variety of tax, benefit, and demographic variables for which we have data.  $\hat{\theta}$  is the statistic of interest.

equal across areas of varying Hispanic density. While unobserved cultural factors might also account for this pattern of results, the disproportionately positive, and statistically significant, response in Hispanic regions to envelopes with a Spanish translation, discussed below, also points to language as a meaningful predictor of overall take-up.<sup>37</sup>

Finally, the table compares the response rate for the control condition—that is the condition with the original CP notice and the complex worksheet—with the average response across the three categories of treatments. This comparison suggests a large net positive effect of simplification on response (from 0.14 to 0.23), as well as of information (0.23 to 0.28), but not of the attempted reduction of stigma (0.23 to 0.22).<sup>38</sup> We now examine the specific response to each of the experimental interventions.

## 5.2 Response to Experimental Treatments

We summarize the effects of experimental variations on response, as well as denied claims, in Table 5. The first column depicts marginal effects from a response model described by the following specification estimated with a probit regression:

$$\Pr(\text{Response}_i = 1) = \Phi(\alpha + \sum \text{Notice}_i^j + \sum \text{Worksheet}_i^k + \text{Env}_i + \eta_i)$$

where indicator variables denoting the assigned notice ( $\text{Notice}_i^j$ ), worksheet ( $\text{Worksheet}_i^k$ ), and envelope ( $\text{Env}_i$ ), predict an individual,  $i$ 's, binary response ( $\text{Response}_i$ ). To permit clear pair-wise comparisons, effects are estimated relative to an excluded simple notice, simple worksheet, and the plain envelope.<sup>39</sup> A fixed effect,  $\eta_i$ , is included to control for the presence of dependents. The change in response, relative to the pertinent comparison mailing (i.e., the duplicated initial notice and lengthy worksheet for the simplification treatments, and the simplified mailing for the informational and stigma treatments), is reported in brackets.

The second column estimates the same model but with a rich set of income, benefit, tax, and demographic control variables. The insensitivity of the point estimates to the inclusion of these controls, speaks to the efficacy of the randomization. Since the controls proffer no additional precision, we exclude them in the subsequent analysis. Columns 3 and 4 display the estimated model, without the fixed effect, for the population with and without

<sup>37</sup>In the response model reported in Table 5, the interaction between an indicator for the messaged envelope and the Hispanic household density is a statistically significant and positive 0.030 ( $p < .10$ ). However, the sum of the interaction coefficient (negative) and the coefficient for the indicator variable is positive, but not statistically distinguishable from zero.

<sup>38</sup>Note that to ensure sufficient sample sizes, the table reports figures that are averaged across the envelope and indemnification treatments.

<sup>39</sup>The excluded mailing here is the simplified mailing (which is the baseline notice). This is to permit transparent pair-wise comparisons between various interventions and the baseline notice from which the interventions depart.

dependents. The final two columns provide evidence that any disproportionate increase in denied claims, due to the interventions, are too modest to account for the remaining pattern of response.<sup>40</sup> Figure 4 summarizes the predicted response, with confidence intervals, by intervention as calculated from Column 1.

**Informational Complexity.** Figure 4 indicates that simplification starkly impacts response. The “simple” notice increases response by 0.06 ( $p < .01$ ), or 47%, relative to the control response of 0.14 (i.e., the initial mailing). The inclusion of the simple worksheet increases response by 0.04 ( $p < .01$ ) or 30% relative to the control. The impact of the simple worksheet is driven primarily by those without dependents likely because the implementation of the intervention for this population is substantially “stronger” (due to the additional section of questions) than the intervention for those with dependents.

**Information on Incentives.** Among treatments that provide additional information, benefit information is most efficacious. The inclusion of a benefit range heightens response by nearly 0.08, or 35%, relative to the baseline response of 0.23 ( $p < .01$ ) (i.e., the simple mailing). The table indicates that the increase is roughly equivalent for respondents with and without dependents consistent with the possibility that the effect may be not be due entirely to changes in expectations of benefit size.<sup>41</sup>

Two interventions produce a negative effect on response. First, the inclusion of transaction cost information reduces response by 0.01 (not significant in main specification; weakly significant with controls,  $p < 0.10$ ), or 6% relative to baseline. This result may be due to the minimal role that perceptions of worksheet claiming have on the decision and is also consistent with a small body of research on the aversive effect of making cost incentives salient on economic choice (e.g., Chetty, Looney and Kroft 2009; Finkelstein 2010). Second, the one-page informational flyer dampens response by 0.04 ( $p < .01$ ), or 16% relative to baseline. The negative effect even characterizes the version customized for those with dependents to display the high benefit schedule. The response to the flyer is consistent with the possibility that too much information, or information communicated in a complicated manner, may disengage or confuse the reader.

Another two informational interventions—the envelope message and the indemnity message—have no statistically significant effect on response. One possible explanation for non-positive reaction to the envelope is that the unusual messaging caused some recipients to actually doubt that the legitimacy of the missive as an official IRS mailing.<sup>42</sup>

**Program Stigma.** Finally, we consider the two interventions intended to reduce program stigma. The attempt to reduce personal stigma (emphasizing the role of “hard

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<sup>40</sup>It is, of course, possible that there may unobserved differences in the rate of delinquent returns by treatment category.

<sup>41</sup>A formal statistical test confirms we cannot reject the null that the two coefficients are equal.

<sup>42</sup>This explanation was suggested to us by a number of seminar participants.

work”) does not affect response, while, the social influence treatment (highlighting take-up of peers) decreases response by 0.04, or 19% relative to baseline ( $p < .01$ ). One possible explanation for the ironic effect is that the norm may have been more efficacious among those for whom it lowered the belief in the prevalence of non-claiming (e.g., the Chicago Survey indicates that 25% believe that the rate of claiming is higher than 80%).

### 5.3 Complexity Interactions and Response

We now further scrutinize the role of informational complexity in shaping response. As documented above, the length complexity of worksheets as well as the design complexity of the notices each led to significant reductions in response. Moreover, the presence of an informational flyer also dampened response, and this may, in part, be due to the volume and complexity of information contained therein. One test of policy and of theoretical interest is how readers respond to interactions of these complexity elements—i.e., original notice, and lengthier worksheet, along with the informational flyer.

Formally, we estimate the following probit regression:

$$\Pr(\text{Response}_i = 1) = \Phi(\alpha + \gamma \text{CompN}_i + \theta \text{Flyer}_i + \delta \text{CompWS}_i + \lambda_1(\text{CompN} * \text{CompWS})_i + \lambda_2(\text{Flyer} * \text{CompWS})_i + \pi_1 \text{Indemnity}_i + \pi_2 \text{Env}_i)$$

We estimate the model on a sample restricted to the baseline notice, the complex notice, as well as the flyer, and further confine the analysis to those without dependents, as the effects of the complex worksheets and flyers are largely driven by this group (due possibly to differences in the strengths of the interventions). The coefficients  $\lambda_1$  and  $\lambda_2$  indicate the interaction effect between complexity components.

The estimates,  $\hat{\lambda}_1 = .020$  ( $p = .38$ ) and  $\hat{\lambda}_2 = .022$  ( $p = .36$ ), imply that the negative and significant effects of the complex notice, flyer, and complex worksheet are only partially additive. While estimates of the interaction coefficients are imprecise, they indicate that the combination of complex worksheet and notice result in a predicted response of 15.8 percent and not the 17.4 percent one would expect if component influences were fully additive. Similarly, the flyer and the complex worksheet jointly yield a predicted response of 16.8 rather than 18.5 percent.

The existence of sub-additivity in the influence of complexity interventions is of practical import for a policy maker. Under sub-additivity, even a single component of complexity may result in a large deficit in response. One could imagine alternative explanations for this pattern of results including cognitive or inferential accounts of how individuals engage increasing complexity, or heterogeneity in the types of complexity to which readers are sensitive.

## 5.4 Benefit and Cost Display and Response

A second set of treatments that warrant further inquiry are the benefit and cost displays. Figure 5 plots predicted baseline response and marginal effects from a response model estimated for all display variants on a sample restricted to the relevant baseline and treatment. For the benefit displays, we estimate the model separately by dependent presence and include fixed effects to flexibly control for the number of dependents where appropriate.

The figure confirms that response to the benefit display is not tied to the magnitude of the figure. For those with dependents, who are randomized to receive either a high and low display, the figure suggests that the low benefit display (\$3043) actually produces the largest increase in response of 0.13. This represents an 81% increase relative to the baseline of 0.16. We reject statistical equality of this estimate to the 0.05 increase induced by the \$5028 display ( $p < .01$ ) as well as the 0.06 prompted by the \$5657 display ( $p < .02$ ). The size of the effect, and insensitivity to the magnitude of the display, is consistent with the large marginal effect of 0.09 produced by the \$457 display for recipients without dependents.

The figure also decomposes the modestly aversive effect of the cost displays. There is no statistically significant evidence for a salutary effect on response for either the 10 or the 60 minute advisements, and, further, no evidence that the influence of the two displays can be distinguished ( $p = .70$  w/o dependents, and  $p = .50$  w/ dependents). The isolated effect of the 10 minute display is negative and weakly significant ( $-0.02$ ,  $p < .10$ ). This pattern of results is consistent with survey evidence suggesting that such transaction costs are not an important determinant of the take-up decision, and directionally consistent with the possibility that heightening salience of cost incentives may negatively affect response (Chetty and Saez 2009; Finkelstein 2009).

## 5.5 Persistence and Inertia of Take-Up

Policymakers would be remiss to not ask whether a one-time intervention, such as that implemented in this experiment, leads to a continued pattern of subsequent take-up. The outcome of such a query has implications for policy, welfare and the theoretical interpretation of the findings. Sustained influence of the interventions over periods lends credence to the likelihood that the effects are driven by information acquisition as opposed possibly more transient mechanisms (e.g., short lived attentional or persuasive effects). We assess these dynamics with three distinct approaches that attempt to capture the direct effect of mailing receipt, the relative effects of individual interventions as compared to the baseline mailing, as well as the “inertial” effect of take-up in one period on future take-up.

First we estimate the effect of the mere receipt of an experimental mailing on subsequent

year claiming. An ideal identification would have entailed the presence of a “hold-out” group in the experimental sample that was randomized to not receive a treatment and could then serve as a control for subsequent comparisons. In the absence of an experimental control, under straightforward assumptions, we can still project counterfactual rates of TY 2010 take-up by examining the rate of EITC claiming in the years prior to the experiment.<sup>43</sup> Conditioned on filing but not claiming in time  $t$ , if claiming in proximal years is a white noise outcome, then in expectation, claiming in  $t - 1$  and  $t + 1$  should be equivalent. While many factors produce annual variation in claiming, plausible violations to our assumption such as learning over time or shocks that persist across periods, should actually lead to lower relative claiming in period  $t + 1$ , conditioned on the failure to take-up in period  $t$ . Accordingly, if claiming is not independent across years, our estimation of the causal effect of the experimental mailing is likely to be an upper bound.

Table 6 compares the rate of claiming for TY 2007 through TY 2010 for the experimental sample. Claiming in the year following the experiment (0.245) is significantly higher than in the year preceding the experiment (0.158) ( $p < .01$ ). In support of the identifying assumption, TY 2008 and TY 2007 claiming are not statistically distinguishable ( $p = .15$ ). To account for the possibility that dependents may age a filer out of a credit, we replicate the results on a sample excluding anyone with a dependent at the age threshold in TY 2009. Overall, the table suggests, under the specified assumptions, that the experimental mailings led to an increase in claiming of 55%.

Are there specific experimental interventions that differentially affect subsequent claiming relative to a baseline mailing? Figure 6 plots the marginal effects from a model estimating the direct influence of interventions on TY 2010 claiming evaluated at the mean of the dependent indicator. Two interventions have statistically significant direct effects. The benefit display increases claiming by 0.012 ( $p < .10$ ) relative to baseline claiming of 0.25 (simple mailing). Intriguingly, the aversive effects of the social influence notice persists as it reduces subsequent claiming by 0.02.

Finally, we attempt to estimate the causal effect of higher claiming in one period on subsequent claiming. This exercise aspires to capture an “inertial” parameter which may be of more general interest for policy and welfare. We express the empirical relationship of interest with the following cross-sectional model:

$$Claim_{2010_i} = \alpha + \gamma Claim_{2009_i} + X\beta' + \varepsilon_i$$

where  $Claim_i$  represents the binary claiming decision for the specified tax year of person

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<sup>43</sup> Another strategy would be to identify a control group either through a regression or matched-pair analysis. We do not, however, have micro-data on individuals outside of our experimental sample to construct such a control.

$i$ ,  $X$  represents a vector of available demographic and tax variable controls, and  $\gamma$  is the parameter of interest. An obvious concern in this estimation, with simple OLS, is the endogeneity introduced both by serial correlation in claiming due to stable preferences and beliefs, as well as the possibility of shocks that jointly affect TY 2009 and TY 2010.

We overcome this identification problem by instrumenting for TY 2009 claiming with the experimental interventions. Our main findings can be interpreted as a first stage regression of the causal link between variation in experimental treatments and TY 2009 claiming. The validity of the instrument also depends on its excludability from the main regression, and the approach, therefore, requires that the influence of the experimental mailings, relative to baseline, on subsequent take-up acts only through changes in contemporaneous take-up. If this assumption is violated, our estimates would capture both the direct effect of the interventions and the inertial effect, and should be interpreted as an upper bound of the inertial parameter. Our two-stage least squares design then recovers the effect of higher take-up in TY 2009, induced by variation across the experimental interventions, on TY 2010 take-up.

The lower panel of Table 6 reports both the OLS and IV estimates of  $\hat{\gamma}$  for this model. OLS suggests that induced claiming in one year results in a 0.11 higher likelihood of claiming the subsequent year. The IV estimate, while much less precise, produces a similar effect magnitude of 0.09. Relative to baseline claiming of 0.25, this suggests that inducing take-up in one year leads to a 37% increase in the likelihood of subsequent claiming. We caution that the estimate represents a local parameter averaged across interventions and localized to the given sample (Angrist and Imbens and Rubin 1996). Overall, the three analyses point to significant persistence in the influence of the experimental mailings on future take-up. This is especially notable given that the domain in which the TY 2010 take-up occurs (i.e., on one’s tax return at the time of filing), is very different from that of TY 2009 (i.e., the return of a notice and worksheet mailed in November).

## 5.6 Heterogeneity of Response Effects

**Overall Response.** Table 7 reports cross-tabulations in overall response by dependent status as well as various demographic and tax variables. The table suggests that females and young recipients are more responsive to the mailings than their counterparts. The gender differential in sensitivity to the mailings is consistent with other studies that have documented heightened female response to information regarding incentives (e.g., Liebman and Luttmer 2011). Further, response appears higher for self-preparers, as compared to those who employed third party preparers, across dependent status, and is particularly higher for self-preparers with a history of past-claiming. There is no strong correlation between response and benefit size or earned income. However, one must interpret the

Table 7 with caution. Because the experimental population is the product of considerable selection, it is difficult to interpret findings of heterogeneity without observing how factors differentially select various populations into the sample.<sup>44</sup>

**Experimental Interventions.** We now examine heterogeneity across interventions by benefit size, age, gender, and earned income. Appendix Figure A2 plots the predicted response by intervention from a set of probit regressions estimated separately for those above and below the median benefit level. We confine estimates to those with dependents to achieve a clean comparison and wide ranging benefit levels. Relative to the appropriate baseline, those expecting lower benefit amounts are more responsive to the cost display (+0.05,  $p < .05$ ), as well as the reductions of stigma (+0.05 jointly,  $p < .05$ ).<sup>45</sup> An F-test rejects the equality of the effect across median benefit split jointly for all interventions (F-stat = 5.39,  $p < .05$ ).

We next turn to treatment heterogeneity by age and gender. Appendix Figures A3 and A4 report predicted rates by intervention by median age and gender. The analysis is confined to single filers so as to ensure transparency of gender and age, and to avoid confounding due to the presence of dependents. Response to interventions do not appear to be mediated significantly by age. The results do indicate strong differences in sensitivity to complexity and stigma by gender. Females are less responsive, relative to their respective baseline, to the complexity notice (-0.04, not significant,  $p = .12$ ) the complex worksheet (-0.04,  $p < .05$ ), and the attempted reductions of stigma (-0.06,  $p < .05$ ).<sup>46</sup>

Finally, we examine how response to the interventions varies by income. Appendix Figure A5 displays predicted response by median earned income for those with dependents (i.e., \$33,487). The figure indicates similar baseline levels of response, but that those of lower, as compared to higher, income are relatively less likely to respond to the complex notice (-0.07,  $p < .05$ ), the flyer (-0.04,  $p = .14$ ), and attempted reductions of stigma (-0.04,  $p = .15$ ).

Overall the analysis of heterogeneity by treatment suggests that those expecting lower benefits are less deterred by the cost display or the stigma interventions, females are more negatively sensitive to complexity and manipulations of stigma, and those of lower income are also more negatively sensitive to complexity (via the notice and the flyer) and stigma.

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<sup>44</sup>For example, if those with much higher benefits take-up at the time of filing or the first notice, then the response elasticity with respect to benefit size may not be that meaningful. Indeed other research has found that non-filing non-claimants are more likely to be male, have a lower household income, and qualify for a smaller credit (e.g., Blumenthal, Erard, and Ho 2005).

<sup>45</sup>Tests of statistical difference are from a pooled regression of high and low benefit samples that includes benefit and treatment interactions.

<sup>46</sup>Tests of statistical difference are from a pooled regression across gender that includes gender and treatment interactions.



## 6 Evidence on Underlying Mechanisms

A natural question is why individuals react so sharply to the small contextual changes featured in the experiment. In principal, assuming individuals engage in a rationalizable cost-benefit analysis, one should be able to trace changes in experimental response to the influence of specific interventions on perceptions of cost and benefit parameters, or the attentiveness with which a reader engages the underlying information.<sup>47</sup> A second set of surveys, administered to 2,800 subjects online through Amazon MechTurk, provides psychometric evidence to facilitate such insights (described in the Appendix). Not all interventions were tested due to sample constraints.

**Informational Complexity.** We first consider the mechanisms underlying response to the complexity interventions (again, we include the informational flyer). Appendix Table A4 summarizes a series of regressions of attentional and inferential outcomes following exposure to the various complexity interventions. The excluded category in the regressions, and the baseline for interpretation, is the simple notice and worksheet. As an initial test of experimental efficacy, subjective ratings of complexity (from 1 to 100) indicate that the complex notice is viewed as significantly more “complex” while the worksheet and flyer are not. That the latter elements don’t register on this scale could be because, unlike the notice which is textually dense, the worksheet and flyer feature a simple visual design.

Overall, the evidence from the psychometric surveys suggests that the complexity notice and worksheet dampen response not by increasing perceptions of the “effortfulness” required to navigate the material, but rather by diminishing beliefs of eligibility by 4 to 10% (noisily measured and not significant in the case of the worksheet), and, judging from intent to read and comprehension metrics, lessening attention to the material.<sup>48</sup> The notice and worksheet do not appear to meaningfully raise perceived costs linked to worksheet claiming, stigma, or an audit. The flyer appears to act through multiple channels as it increases pessimism with respect to eligibility and benefits, raises perceptions of claiming costs, and lower comprehension of notice material. The effect of the flyer on expected benefit size is unsurprising given we only test the non-dependent benefit schedule. Finally, consistent with the experimental outcomes, the table suggest that the perception of complexity may be sub-additive across multiple complexity elements.

**Information on Benefits and Costs.** We next examine the response prompted by the display of benefit and transaction cost information. For simplicity and statistical power, we aggregate results for the two \$5k benefit notices (i.e., \$5028 and \$5657). Appendix Table

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<sup>47</sup>There are decisions and sets of preferences that are not amenable to straightforward comparisons of immediate costs and benefits (e.g., a model of “hassle” costs, procrastination). We discuss alternative formulations of the decision to take-up in the next session.

<sup>48</sup>In the case of the notice, changes in beliefs of eligibility may be due to the emphasis the original notices place on eligibility criteria (or the exclusionary language in which the emphasis is rendered).

A5 suggests a possible channel of influence for the benefit display is a change in benefit expectations. Conditioned on belief of eligibility, the high and middle displays (\$5k, \$3043) directly increase belief of benefit size, relative to baseline, by 102% and 114%, respectively. While the low display (\$457) does not significantly alter expectations of benefit size, it does elevate beliefs of eligibility by 31%. What might drive an individual to make such inferences of eligibility is up to speculation. It may reflect statistical inference based on prior experience, or may reflect strategic construal or a self-enhancing bias (i.e., “If the benefit is that large, I must have known of it... therefore, I must not be eligible”). The displays do not lead to a significant change in the perception of various costs (though there is suggestive evidence for an increase in perceived audit rates), and there is mixed evidence for a second channel of influence via changes in attention paid to the notices. Given that the benefit displays appear to prompt inferences about both benefit size and eligibility, a plausible explanation for the more favorable response to the \$3043, relative to the \$5k, notice(s) in the experiment may lie in the comparative degree to which the notices influence these two margins.<sup>49</sup>

In the experiment, individuals respond unfavorably to guidance regarding the time required to complete the enclosed worksheet. The scoring suggests, with some imprecision, that the 10 minute advisement lowers expectations of working time (29 minutes), while the 60 minute guidance raises it very marginally (35 minutes) relative to the baseline (34 minutes).<sup>50</sup> The table suggests that one mechanism through which cost displays might dampen response is by prompting a negative inference regarding eligibility as the 10 minute display lessens beliefs of eligibility by 17% ( $p < .05$ ).

**Program Stigma.** The personal stigma notice was not scored in the survey due to sample constraints. Scoring of the social stigma intervention indicates that while the intervention does decrease perceptions of program stigma by 3% (not significant), relative to already modest baseline perceptions of stigma, it also increased perceptions of document complexity by 8% ( $p < 0.05$ ) and belief in the likelihood of an audit by 5% ( $p < 0.10$ ). The increased perceptions of complexity and likelihood of an audit may account for the ironic effects of the notice on experimental response. While similar language has been impactful in numerous other domains, such demonstrations are typically among subjects and contexts where confusion is less likely.

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<sup>49</sup> Additional evidence from the Chicago Survey, not reported here, hints that the \$5k benefit notices significantly reduce beliefs of eligibility though such an effect is not evident in the MechTurk data.

<sup>50</sup> The scoring data from the 60 minute notice is actually from the Chicago Survey since we did not gather psychometric data for this intervention. The estimate of baseline claiming time across both the Chicago Survey and the psychometric survey is an identical 34 minutes.

## 7 Implication of Results for Take-up and Policy

### 7.1 Applying Findings to Theoretical Question of Take-up

Beyond illuminating the channels through which to improve take-up, an original intent of this project is to weigh in on the theoretical discourse over its ultimate causes. We can integrate findings of the surveys and the field experiment to adjudicate between competing frameworks to understand take-up in the specific context at hand. Table 8 compares approximate predictions of a standard cost-benefit analysis, with rational, and fully informed agents (with and without the allowance of stigma), with predictions emerging from models that feature factors such as low program awareness, deficits in understanding of program rules and information, and small claiming “hassle costs”, such as the visual complexity of forms.

Overall, the findings from the study are difficult to rationalize in a traditional cost-benefit analysis with informed agents who hold accurate beliefs about program costs and benefits. Such a model is inconsistent with the lack of program awareness and information deficits collectively evidenced across the two surveys where at least some subjects are not availing themselves of the assistance of preparers. In light of the sizable benefits at stake, and the modest costs of signing and returning the claiming worksheet, the cost-benefit model is additionally inconsistent with the substantial increases in experimental response prompted by repeat mailings and small changes to the content and appearance of the forms.

Consider that the typical non-claimant in the sample fails to claim \$786 in benefits (equivalent to 3 weeks or 5% of annual income), while the median non-claimant forgoes \$326 (1.5 weeks or 3% of annual income). These benefits are weighed against costs of claiming that include the effort associated with reading, signing, dating, and mailing the 1 to 2 page claiming worksheet, as well as possible stigma associated with claiming. The low levels of perceived stigma (and the failure of the experimental stigma reduction to increase response) and the modest perceived time-costs of filling out the form (and the failure of a experimental reduction in such perceived costs to increase response) further support the difficulty of rationalizing the observed behavior. Moreover, a rational cost-benefit analysis would likely predict that the efficacy of the simplification treatments should prompt only marginal users to increase claim, which is inconsistent with the lack of moderation in treatment effects by benefit size. It is worth noting that no novel information is elicited by the forms, as compared to what was elicited in the already submitted tax return, which suggests that expectations of claiming costs should not involve risks linked to the provision of information.

One factor that may better rationalize the pattern of observed findings involves persistent deficits in information and awareness. Such deficits are consistent with the survey

evidence and the sizable experimental effects of the repeat mailings, simplification, and benefit display. The psychometric evidence further suggests that the simplification and benefit provision treatments, in part, shape response by increasing attentiveness to and comprehension of the notices. In support of such an interpretation, Liebman and Zeckhauser (2004) argue that misconstrual of program rules and incentives may dampen the effect of incentives and reduce program participation. Consistent with our analysis of income heterogeneity, the authors point out that the deleterious effects of poor information are particularly pronounced for those with very low incomes.

A second framework that fits the accumulated evidence is a model of “hassle costs.” In such a framework, small obstacles to participation, such as additional, non-discriminating, questions on the claiming worksheet, or a denser textual display of the notice, result in a larger detriment to response than one might expect. The theoretical underpinnings of the model are grounded in the concept of “channel factors” first introduced by psychologist Kurt Lewin (1951). Lewin’s theory posits that small situational changes can result in behavioral changes not necessarily by providing novel information but by facilitating or inhibiting the first step required to complete a multi-step task. Bertrand, Mullainathan, and Shafir (2006) cite a number of laboratory and field examples depicting how the reduction of minor “hassles” can prompt compliance including one classic illustration where persuasive messaging regarding the benefits of tetanus inoculation effectively changed beliefs and attitudes of college students, but only changed behavior when the messages were accompanied by a campus map and advice to pre-specify a route and time for one’s trip to the infirmary (Leventhal, Singer and, Jones 1965). While the behavioral potency of minor modifications in the decision context may be attributable to other psychological frictions (e.g., limited memory, or procrastination), the large response induced from small changes in the forms are difficult to reconcile with a traditional economic model of decisions.<sup>51</sup>

As an alternative framing, Table A6 in the Appendix evaluates explanations of incomplete take-up organized by the three tested mechanistic categories—information, complexity, and stigma. We qualitatively judge the relative importance of each explanation by reporting the ex-ante expectation of the relevant parameter, inferred from surveys, the experimental treatment and resulting change to expectation, and finally the estimated change to overall take-up one might expect if the underlying explanation were to be addressed. The exercise again suggests that three factors—low overall awareness, high complexity, and incomplete benefit information—significantly contribute to incomplete take-up, while beliefs regarding the costs of claiming, or the stigma associated with claiming, appear to have a

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<sup>51</sup>For example, in the presence of time-inconsistent preferences (O’Donoghue and Rabin 1999), because the costs are immediate, and the benefits are due in 6 to 8 weeks, small changes in perceived costs may lead to substantive changes in response.

minimal impact on the decision to take-up.<sup>52</sup>

**The Role of the Preparer.** While our experimental setting mimics the decision setting of response to the first IRS notice, any effort to understand incomplete take-up of EITC among filers invites consideration of the increasingly sizable role played by paid and volunteer preparers. Why would errors occur in the filing of tax forms submitted by certified preparers (particularly since many paid preparers may have incentives to file EITC claims)? First, we can infer that preparers are more efficacious than self-preparers by noting that the experimental sample features only 38% returns that were filed with preparer assistance (compared to 70% of claims nationally, and 65% in CA). Informal interviews with preparers, the IRS, and policy researchers suggests two possible explanations for remaining errors on preparer filed returns. A first is that the sheer size of the preparer population—reportedly over 1 million preparer tax identification numbers were issued from 1999 to 2010—and the ease of the application process, (which requires only a few minutes to complete), suggests severe heterogeneity in preparer quality. Second, the complexity of EITC program requirements (e.g., Publication 596 which describes program rules is 57 pages long), as well as the complexity of other credits for which an individual may qualify (e.g., the Child Tax Credit, the Additional Child Tax Credit, education credits), may lead to errors either due to preparer or claimant confusion.

## 7.2 Projected Implications for Policy

**Optimal Mapping of Notices to Population Sub-Groups.** As a first step in understanding the full policy potential of the experimental findings and the heterogeneity of the effects, we estimate the overall response one would expect if mailing components were customized based on a recipient’s observable attributes. We implement this exercise by defining 16 sub-groups by creating a categorical or median split across four important demographic variables—the presence of dependents, earned income, claiming history, and self versus prepared claiming—and then assorting individuals into the conjoint of each of these sub-groups.<sup>53</sup> We then identify the optimal mailing for each cell from 24 mailing combinations (6 letters x 4 worksheets, combining variations within the same mechanistic category). Appendix Table A6 reports the results of this exercise.<sup>54</sup> The projected overall

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<sup>52</sup>It is of course possible that stronger behavioral changes may have been induced with more potent interventions. As an example, the successful use of automatic defaults, or form pre-population, in other contexts suggests that the ceiling for gains in response due to simplification may extend above the present magnitudes (Madrian and Shea 2001; Bettinger et al. 2009; Beshears et al. 2010).

<sup>53</sup>While one could imagine a more granular partition, in the interest of obtaining cells of sufficient sample size, we restrict ourselves to those variables which conceivable could be used by a policy-maker (and so avoid gender and age) and which may be of theoretical importance.

<sup>54</sup>Of the 16 optimal mailings, 11 include the benefit display, 12 feature the simplified worksheets (all sub-groups free of dependents), and 10 include the indemnity message.

response from this mapping of 0.35 compares favorably to the overall experimental response of 0.22 and the 0.31 response of the most successful mailing (i.e., the benefit display notice and simple worksheet).

**Projected Policy Implications for EITC Filing Non-Claimants.** We next consider the likely policy impact of these findings on take-up if scaled to the broader population of filing non-claimants. Table 9 reports the outcome of calculations which estimate the impact of the experimental mailings on various subsets of filing non-claimants for TY 2009.

The first set of columns reports the average response rates and benefit levels from the field experiment. It is worth noting that the “Original Notice” reflects the complex notice and the simple but lengthier worksheet (as the original CP worksheet was not tested).<sup>55</sup> The “Optimal Notice + Worksheet” is a result of the optimization and mapping exercise described above.

We begin by projecting the effects of the experiment to the national population of non-claimants who also failed to respond to the existing CP 09/27 notification letters (321,340). The second column reports that the mere distribution of a second notice would result in an additional 45k claimants, whereas a more efficacious notice would yield 100k (benefit display) to 112k (optimal mailing) additional claimants.

Next, we project the outcome of replacing the initial CP notices, distributed to 610,904, with the experimental designs. Conservatively assuming that the response rates for the experimental interventions are additively, rather than proportionally, related to the response to the original CP notice, we estimate that the updated mailing would yield an additional 55k to 201k responses (\$28m to \$128m in disbursed benefits).<sup>56</sup>

Finally, we speculatively project experimental response to the expanded population of filing non-claimants which includes both the CP recipients, as well as the estimated 1.8 million individuals who may not have received a CP notice due to a variety of factors.<sup>57</sup> Notably, a large increase in take-up could be had if it were possible to expand the notice program to the entire population of filing non-claimants (1.1 million less the 321k of the current respondents). Again, extrapolating response additively, suggests that the experimental mailings could yield an additional response of 216k to 504k individuals (\$111m to \$321m in additional benefits) as compared to response from the expanded distribution of the original notice. Coupling a widely distributed optimal mailing with an optimal

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<sup>55</sup>Because the original worksheet featured a crowded and textually dense layout, much like the original notice, we believe that our use of the simple but lengthier worksheet as a control may actually underestimate the effects of the treatments.

<sup>56</sup>That is, we project the response to the simplified baseline notice as 56% amongst the CP population, given the response of 47% to the original notice, and the 9% additive response generated by the baseline in the experiment (as compared to 77% under an assumption of proportionality).

<sup>57</sup>This policy, including specifics of the screening mechanism, is discussed in the earlier section describing the experimental sample.

repeat mailing, could lead to an estimated overall program take-up of 0.78 featuring 790k additional filing claimants who collect \$503m in additional benefits.

We parenthetically report the increase in overall program take-up implied by these projections. These calculations suggest a sizable benefit from expanding the recipient population of original notice recipients (+0.04) and also from the contextual changes explored in the experiment (+0.03). Indeed, we estimate that expanding the population of recipients, optimizing documents, and instituting a second mailing to initial non-respondents, could improve take-up from 0.75 to 0.82.<sup>58</sup>

**Comparative Policy Value of Interventions.** One strategy through which to characterize the efficiency of the interventions is to examine the costs of other policies that could lead to equivalent improvements in take-up. An easily calculable alternative policy is to raise benefits in order to induce higher take-up. The last column of Table 9 calculates the increase in benefits that would achieve the equivalent increase in take-up as each of the interventions. For this calculation, we estimate the elasticity of response to a change in benefits with a response model for the experimental sample using a rich set of controls.<sup>59</sup> The estimates indicate that the optimal mailing, coupled with a repeat notice, would lead to a rise in take-up equivalent to that produced from a 101% rise in benefits. While raising benefits has implications for welfare and efficiency beyond take-up, the equivalence calculation highlights the potential role of contextual changes as a viable policy mechanism.

**Projected Policy Implications for EITC Non-Filing Non-Claimants.** We may also speculate as to the implications of these findings if applied to the much larger population of non-filing non-claimants (estimated to comprise 0.16 of the 0.25 incomplete take-up rate). While an ideal test of the applicability of these findings to the non-filing population demands an independent experiment on a sample of non-filers, we can estimate the improvement in overall take-up under varying assumptions regarding the comparability of filers and non-filers. Appendix Table A7 reports the result of this undertaking. For instance, if overall sensitivity amongst non-filers is 50% of that of filers, then a notice with a benefit display distributed to all non-filers alone could improve overall take-up from 0.75 to 0.79 (change in overall take-up reported parenthetically).

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<sup>58</sup>Towards this end, the IRS has indicated an interest in applying the results of this field experiment to the nationwide distribution of CP notices within the next 2 years.

<sup>59</sup>We estimate a regression of response on the log of expected benefit, as well as a rich set of control variables to account for variation in filing status, household size, past claiming behavior, claiming mode, and log of earned income. The regressions suggest that a 1% change in benefits leads to a .3% change in the likelihood of a response.

### 7.3 Cost-Benefit Analysis

A natural question raised by these findings, as well as their projection to broader populations, concerns implications for individual and societal welfare. While a full normative analysis is beyond the scope of this paper, we sketch out the likely costs and benefits associated with the tested interventions.

**Costs of the Policy.** We can organize the costs of the tested interventions as those related to (i) administration (i.e., printing, distributing and processing the mailings), (ii) increases in non-compliance (i.e., ineligible claiming) or monitoring requirements, and (iii) negative externalities or individual disutility attributable to the mailings. While we cannot explicitly calculate these components, the administrative expenses are likely to be minimal if they resemble the present costs of EITC administration estimated at 0.5% of disbursements (IRS 2003). This compares favorably to the typical 16% expense ratio of other transfer programs (Hoynes and Eissa 2011).

Second, while we do not observe true eligibility, there is no strong evidence for increases in observable measures of non-compliance in the experimental sample as compared to national samples of EITC filers or CP recipients. Specifically, the rate of disallowed claims is 0.93% in the experiment which compares to 0.72% nationally, while the experimental audit rate is 1.41% which compares to 0.71% for the national CP sample, and 1.91% amongst all EITC filers.<sup>60</sup>

One might worry that complexity is a useful screening mechanism through which a policy-maker can extract accurate signals of eligibility, and that our efforts at simplification may introduce inefficiency (e.g., Kaplow 1996). However, while ineligibles may be attracted to simply designed interventions, our simplifications in document design and length do not come at the expense of less accurate or less voluminous information. Indeed, the psychometric evidence indicates that our simplification actually appears to improve comprehension and may consequently improve the quality of submitted information.

Finally, other externalities—such as that which may be incurred if mailings reduced taxpayer attention to other IRS mail—or tax-payer disutility associated with the additional mailings must be significant for the total cost of the tested interventions to exceed the modest current costs of EITC administration.

**Benefits of the Program.** One formulation of the potential value of the scaled interventions is signalled by the preference for high take-up expressed by policy-makers. The IRS expends considerable resources on EITC awareness and outreach (e.g., Congress appropriated \$716 million in 1997 over five years for outreach and enforcement), and has a stated objective for all eligible individuals to claim their EITC credit.<sup>61</sup>

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<sup>60</sup>We do not have data on the rate of denied claims among all EITC filers.

<sup>61</sup>In 2008, the acting IRS commissioner Linda Stiff made this goal explicit in stating that



However, we can independently approximate the social impact of scaled interventions by assessing how such interventions would shift the income distribution under the conservative assumption of EITC budget neutrality.<sup>62</sup> We achieve constancy in the size of the program by proportionally reducing the benefits of EITC claimants to fund the new enrollees. Figure 6A depicts both the current distribution of CP notice claimants by income (TY 2008) and the projected distribution under a regime with a nationalized repeat notice.<sup>63</sup> The majority of new claimants fall in the bottom of the income distribution relative to the distribution of CP claimants. Figure 6B depicts the same pattern but with respect to the distribution of benefits. Again, much of the additional benefit is concentrated amongst those in the lower tail of the income distribution. The figure also depicts the distribution of EITC disbursements by income (data is from Hoynes and Eissa 2011 who tabulate returns from 2004 SOI files) which illustrates that the typical CP claimant is poorer than the typical overall EITC claimant. The figure implies that trimming benefits proportionally among existing claimants to fund new claimants would result in redistribution to those with lower incomes. Others have argued that the poor are the most likely to be deterred by costs of complexity and this appears consistent with our evidence (e.g., Bertrand, Mullainathan, and Shafrir 2006; Dynarski and Scott-Clayton 2006).<sup>64</sup>

To evaluate the social welfare consequences of this transfer, one must consider both the change in the individual utilities of those whose income (and thus consumption) is impacted, as well as society’s valuation of such changes. Even under the assumption that individuals have constant marginal utility of income, assuming some curvature in the social welfare function, most formulations of social welfare would judge the depicted shift in transfers to be welfare enhancing.

On a whole, the analysis suggests that scaling the contextual and informational interventions in this study represents a transfer of resources to the poor under this implementation of budget neutrality. The consequence for welfare, given the modest costs of administering these policy interventions, is likely to be positive barring the presence of large, unanticipated negative externalities associated with the mailings.

**Optimal Screening and Rational Non-Claiming.** A possible rejoinder to the

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“The IRS wants all eligible taxpayers to claim this important tax credit.” Available at <http://www.irs.gov/newsroom/article/0,,id=178071,00.html>.

<sup>62</sup> An alternative approach to measuring social benefits would be to allow the size of the EITC to grow via the interventions, but assume that the overall government budget is fixed. However, any welfare calculation from this exercise would rest on knowing the relative efficiency of EITC as compared to other forms of government spending.

<sup>63</sup> For transparency, and in light of the data we have available, we only consider projections associated with a repeat mailing of the baseline notices, applied to nationwide CP recipients, as opposed to the other projections considered in Table 9.

<sup>64</sup> An added consideration is that a reduction in complexity may obviate further compliance costs of the third party agents presently employed by nearly 70% of filers for preparation assistance.

preceding analysis is that welfare gains will necessarily be limited if the receipt of an EITC benefit does not raise individual utility. If compliance costs serve as an optimal screen through which only those with high valuations claim a credit, then failure to claim may reflect high costs related to stigma or other strategic considerations (e.g., a fear of government reprisals due to information disclosure). In such a scenario, the presence of screening mechanisms should improve program efficiency (Akerlof 1978; Dynarski and Scott-Clayton 2006).

A number of factors weigh against such a narrative. As mentioned, the non-claimants in our sample forgo a significant fraction of annual income. Meanwhile, there is no transparent basis for any heterogeneity in costs of claiming in this context. Worksheets elicit less information than recipients previously provide on their tax return (for those without dependents, the worksheet requires two check marks, and a dated signature), there appears to be minimal experimental or survey evidence for stigma, and, claiming in this sample appears only tenuously linked to benefit size. If non-claiming is due to the absence of information or its misconstrual, and if the provision of (more transparent) information—non-normative or non-persuasive in nature—increases response, then, following Liebman and Luttmer (2011), we conclude that benefit receipt, in this domain, is utility enhancing.

## 8 Conclusions

In this paper we use a field experiment, in collaboration with the IRS, to test whether a novel set of interventions can improve the take-up of unclaimed EITC benefits. Our study demonstrates that the provision of basic information, as well as the complexity with which it’s provided can substantively alter the likelihood of claiming an owed benefit. Specifically we find that modest changes to the design of a tax notice or the length of a tax worksheet, as well as the provision of non-specific benefit information, substantially heightens program take-up. Moreover, the mere receipt of information and the opportunity to claim, just months after the receipt of a very similar mailing, also improves response. The influence of the treatments appears to persist and affect subsequent year take-up. We fail to find evidence that better information regarding direct transaction or audit costs, or information designed to reduce perceived program stigma affects response.

We sought to understand why exactly individuals respond as they do with a set of surveys. Even among those likely to be eligible for the EITC, we find that many are unaware of the presence of the credit. Of those who are aware, there is prevalent misconstrual of program incentives and eligibility. In light of individuals having poor information about how a program functions, it is not surprising that better, and clearer, information improves response. Additional psychometric evidence, illuminating how interventions are perceived,

suggests that heightened attention to information as well as inferences regarding eligibility and benefit size may be at the heart of decisions to take-up.

There are implications of the research. First from the vantage of a policy-maker, these interventions could be easily scaled to apply to the broader population of filing non-claimants. We estimate, under various assumptions, that such scaling could improve overall program take-up from 0.75 to 0.78. Larger improvements could be achieved if these interventions are applied to the broader population of eligible non-filers. More generally, while the EITC is an idiosyncratic setting, the sensitivity of individuals to basic information, as well as its complexity and salience, may have scope for improving take-up in other contexts. With respect to welfare, we posit that because of the apparently modest administrative and compliance costs of the interventions, and because the interventions appear to disproportionately enable the poor to claim credits, the net effect of the scaled interventions is likely to be welfare enhancing. The size of the forfeited benefits and the nature of the information required to take-up, among other factors, suggests that low take-up is not the product of optimal screening.

A second implication applies to the literature that seeks to identify the various determinants of take-up. Integrating survey data on typical beliefs, with the experimental findings on marginal behavior, suggests that low awareness, informational complexity (and language barriers), and lack of benefit and eligibility information may be important causes of low levels of take-up. We do not find that misperception of direct transaction costs or program stigma determine low take-up in this context. Overall, the evidence does not appear to rationalize a simple cost-benefit model of take-up, even permitting stigma. Instead, it seems consistent with a model in which the small changes to the appearance and complexity of the paperwork leads to substantive changes in response rates.

Despite the advantages of our research setting, there are potential limits to our findings. A first is that, because the EITC has a number of unique institutional features, findings from our targeted sample may not generalize to other non-claiming populations. A second limit concerns the scalability of strategies identified as benefiting take-up. As illustration, sending a hypothetical bright red letter to individuals may yield an immediate rise in response, but whether such a letter would remain effectual if deployed repeatedly over time or, simultaneously across programs, is an open question. A final limit is more conceptual. While we have causal estimates of the marginal response to various interventions, and survey data on the distribution of beliefs, our claims regarding the determinants of low levels of take-up are subject to assumptions relating average and marginal behavior. Future research may help to construct theories to clarify the feasibility of scaling these interventions across time and programs, how expectations of costs and benefits determine the decision to take-up, and how such expectations are shaped by informational complexity.

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## 10 Tables and Figures

**Table 1**  
**SUMMARY STATISTICS (TY 2009)**

	EITC CLAIMANTS		CP NOTICE RECIPIENTS		EXPERIMENTAL SAMPLE		
	US	CA	US	CA	(excluded from other counts)		
Variable Name	Mean	Mean	Mean	Mean	Mean	Median	St. Dev.
<u>Panel A: Overall</u>							
Number	26,742,267	2,975,197	608,233	76,759	35,050		
Response	--	--	--	0.41	0.22		
Share Paid	0.99	0.99	0.44	0.39	0.21		
EITC Benefit if > \$0	\$2,185	\$2,165	\$412	\$415	\$511	\$288	\$838
Total EITC Paid	\$58.1b	\$6.4b	\$111m	\$13.0m	\$4.0m		
<u>Panel B: Descriptive and Tax Variables [All Sample]</u>							
<b>Descriptive Variables</b>							
Age					43	22	13
Gender - Male (Primary Filer)	0.49	0.54	0.69	0.67	0.71		
Gender - Male if Single FS					0.65		
Filing Status = Single	0.26	0.30	0.62	0.60	0.58		
Filing Status = Married Filing Jointly	0.26	0.30	0.26	0.25	0.27		
Filing Status = Head of Household	0.47	0.41	0.12	0.14	0.15		
Share with Qualified Children	0.77	0.76	0.24	--	0.33		
<b>Tax Variables</b>							
Earned Income					\$14,402	\$9,568	\$13,532
Adjusted Gross Income	\$17,002	\$16,964	\$10,448	\$10,368	\$15,852	\$10,538	\$14,044
Total Taxes	\$368	\$463	\$312	\$347	\$352	\$0	\$842
Total Taxes (if > 0)					\$810	\$383	\$1,124
Tax Refund	\$4,080	\$3,874	\$1,338	\$1,342	\$1,246	\$604	\$3,182
Tax Refund (if > 0)					\$1,471	\$802	\$3,409
Share - Self-Preparation	0.34	0.27	0.70	0.65	0.62		
Share - Self-Employ Inc > 0					0.18		
Past Claim - TY 2008					0.16		
Past Claim - TY 2006 to 2008					0.29		
<u>Panel C: Descriptive and Tax Variables [Claimants Only]</u>							
<b>Descriptive Variables</b>							
Number	26,567,446	2,959,339	270,642	31,012	7,423		
Gender - Male (Primary Filer)	0.49	0.54	0.64	0.61	0.65		
Filing Status = Single	0.26	0.30	0.68	0.72	0.70		
Filing Status = Married Filing Jointly	0.26	0.30	0.25	0.20	0.20		
Filing Status = Head of Household	0.47	0.41	0.07	0.08	0.09		
Share with Qualified Children	0.77	0.76	0.14	0.14	0.21		
<b>Tax Variables</b>							
Share - Self-Preparation	0.34	0.27	0.78	0.77	0.76		
Adjusted Gross Income	\$17,002	\$16,964	\$9,793	\$9,083	\$12,352	\$9,179	\$11,442
Total Taxes	\$368	\$463	\$248	\$252	\$285	\$0	\$784
Tax Refund	\$4,080	\$3,874	\$1,061	\$974	\$955	\$504	\$1,602

**Notes:** This table provides summary statistics for various subsets of EITC eligible based on data from the IRS Central Data Warehouse. The data is extracted through end of 2010 except for the experimental data which is through May 2011. The sets of columns report data for US EITC recipients, CA EITC Recipients, US CP recipients, CA CP recipients, and the experimental sample, respectively. Statistics from the first four columns exclude response from the experimental sample. Panel A reports overview statistics, Panel B reports descriptive and tax variables for the full sample, and Panel C reports descriptive and tax variables for those who claim an EITC benefit across each sample. Some of the figures are estimated from author calculations.

**Table 2**  
**STEP-BY-STEP ACCOUNTING TO GENERATE EXPERIMENTAL SAMPLE (CA, TY 2009)**

Eliminated Populations by Step	ELIMINATED RETURNS		REMAINING RETURNS		NOTES
	Step (%)	Overall (%)	(%)	(~N)	
Start - Total EITC eligible	--	--	1.00	3m	3.0m filed; 26% non-compliance (TIGTA 2011; TY 2009); 25% incomplete take-up (Ploger 2009; TY 2005)
1. Program Participants	0.75	0.75	0.25	750,000	
2. Non-Filing Non-Claimants	0.65	0.16	0.09	262,500	65% of non-claimants are non-filers (Ploger 2009; TY 2005)
3. Did Not Receive CP 09/27	0.75	0.06	0.03	<b>76,440</b>	~75% of eligible filers may not have received CP notice (Ploger 2009) [76,440 is exact figure as reported by IRS]
4. Respond to CP 09/27	0.41	0.01	0.015	45,099	
6. Mistagged & Exclusions	0.22	0.003	0.012	<b>35,050</b>	We exclude 2,953 due to mistagging of dependents; IRS excludes another 7,096 individuals due to incorrect address
Experimental Sample				<b>35,050</b>	

**Notes:** This table traces the generation of the experimental sample from an estimate of all EITC eligibles in CA for TY 2009. Bolded figures indicate exact figures. Remaining figures are estimated or inferred. Non-compliance estimate assumes that all overclaiming is on the extensive margin (i.e., is by ineligible individuals). Source for non-compliance estimate (TIGTA 2011) is the report entitled "Treasury Inspector General for Tax Administration, Ref. No. 2011-40-023." Filing and CP statistics are either from IRS website or from internal IRS documents.

**Table 3**  
**EXPERIMENTAL INTERVENTIONS BY MECHANISM**

MECHANISM	INTERVENTION	DESCRIPTION	SAMPLE
<b><u>Informational Complexity</u></b>			
Simplicity / Complexity (Design)	1. Simple Notice	Relative to complex (original CP) notice, "simple" single-sided notice has simplified layout and excludes eligibility information repeated in worksheet	3,676
Simplicity / Complexity (Length)	2. Simple Worksheet	Relative to simple worksheet, a complex worksheet includes additional, non-discriminatory, questions regarding eligibility	10,979
<b><u>Program Information</u></b>			
Benefit and Cost Information	1. Benefits (Low and High)	Simple notice reports upper bounds of benefit range	6,761
	2. Transaction Costs (Low and High)	Simple notice provides guidance as to worksheet completion time	3,475
Penalty/Audit Information	1. Indemnity Message	Worksheet with message to indemnify against penalty for unintentional error	17,027
General Program Information	1. Attention Envelope	Envelope with message indicating enclosed information is "good news"	17,044
	2. Informational Flyer	One page flyer offers program information and trapezoidal benefit schedule	4,019
<b><u>Program Stigma</u></b>			
Personal Stigma	1. Emphasis on Earned Income	Simple notice emphasizes that benefit is reward for hard work	1,844
Social Stigma	2. Social Influence	Simple notice communicates that similarly situated peers are also claiming	1,753

Table 4

## SUMMARY OF RESPONSE FOR INITIAL AND EXPERIMENTAL NOTICE

	ALL SAMPLE			W/O DEPENDENTS			W/ DEPENDENTS		
	Response	Benefit Size	Deny	Response	Benefit Size	Deny	Response	Benefit Size	Deny
CP Notice (CA TY 2009)	0.41	\$570	0.02	--	--	--	--	--	--
Overall Response	0.22	\$511	0.01	0.25	\$247	0.00	0.16	\$1,531	0.03
Overall Response - Hispanic Adjusted	0.25	\$530	0.01	0.26	\$245	0.00	0.21	\$1,638	0.02
Control (Complex N + Complex WS)	0.14	\$546	0.01	0.17	\$294	0.00	0.10	\$1,570	0.02
Simple (Simple N + Simple WS)	0.23	\$514	0.01	0.27	\$246	0.00	0.16	\$1,616	0.03
Simple + Information	0.28	\$531	0.01	0.31	\$242	0.00	0.21	\$1,643	0.04
Simple + Low Stigma	0.22	\$452	0.01	0.25	\$255	0.00	0.14	\$1,330	0.03

**Notes:** This table summarizes the response rate, non-zero benefit size, and denial rate for the CA CP sample and experimental samples of interest. To ensure a sufficient sample, figures in the table represent an average across the envelope as well as the indemnity treatments. The adjustment for the Spanish speaking population is estimated with a response model using ZIP code level data on the density of the Hispanic population and is further described in the text. Dependent specific response data is not available for the CP Notice.

**Table 5**  
**RESPONSE AND DENIAL BY EXPERIMENTAL INTERVENTION**

	DEPENDENT VARIABLE - (PROBIT)					
	RESPONSE (YES/NO)				DENIAL (YES/NO)	
	Full Sample (1)	w/ Controls (2)	w/o Deps (3)	w/ Deps (4)	Full Sample (5)	w/ Controls (6)
[Simple Notice, Simple Worksheet, Plain Envelope - Excluded]						
<b><u>Complexity Interventions</u></b>						
Complex Notice	-0.064*** (0.007) [-47%]	-0.063*** (0.007) [-47%]	-0.065*** (0.010) [-38%]	-0.060*** (0.010) [-60%]	-0.0003 (0.0010)	-0.0002 (.0000)
Complex Worksheet	-0.040*** (0.005) [-30%]	-0.040*** (0.005) [-30%]	-0.054*** (0.006) [-32%]	-0.012 (0.007) [-12%]	-0.0003 (0.0000)	-0.0003 (0.0000)
<b><u>Informational Interventions</u></b>						
Benefit Display	0.080*** (0.007) [+35%]	0.081*** (0.007) [+35%]	0.085*** (0.009) [+31%]	0.066*** (0.011) [+41%]	0.0012** (0.0010)	0.0008* (.0000)
Transaction Cost Display	-0.013 (0.008) [-6%]	-0.015* (0.008) [-6%]	-0.015 (0.010) [-6%]	-0.008 (0.012) [-5%]	0.0008 (0.0010)	0.0007 (0.0010)
Indemnification Message	0.005 (0.005) [+2%]	0.005 (0.005) [+2%]	0.003 (0.006) [+1%]	0.007 (0.007) [+4%]	0.0004 (0.0000)	0.0002 (0.0000)
Informational Flyer	-0.037*** (0.007) [-16%]	-0.038*** (0.007) [-16%]	-0.047*** (0.009) [-17%]	-0.019* (0.011) [-12%]	0.0000 (0.0010)	0.0001 (.0000)
Envelope Message	-0.007 (0.005) [-3%]	-0.007 (0.005) [-3%]	-0.010 (0.006) [-4%]	-0.001 (0.007) [-1%]	-0.0002 (0.0000)	-0.0001 (0.0000)
<b><u>Stigma Interventions</u></b>						
Personal Stigma Reduction	-0.007 (0.011) [-3%]	-0.009 (0.010) [-4%]	-0.012 (0.014) [-4%]	0.001 (0.016) [+1%]	0.0010 (0.0010)	0.0009 (0.0010)
Social Stigma Reduction	-0.044*** (0.010) [-19%]	-0.044*** (0.010) [-19%]	-0.047*** (0.013) [-17%]	-0.037** (0.015) [-23%]	-0.0007 (0.0010)	-0.0004 (0.0010)
Fixed Effects, I(Deps)	X	X			X	X
Controls		X				X
N	35,050	35,050	23,618	11,432	35,050	35,050
Pseudo R-Squared	0.02	0.03	0.01	0.01	0.17	0.22
Baseline Response Rate (Simple N + WS)	0.23	0.23	0.27	0.16		
Control Response Rate (Complex N + WS)	0.14	0.14	0.17	0.10		
P-value of F-Test - Complexity Interventions	0.00	0.00	0.00	0.00	0.32	0.31
P-value of F-Test - Informational Interventions	0.28	0.28	0.69	0.15	0.13	0.12
P-value of F-Test - Stigma Interventions	0.00	0.00	0.00	0.11	0.96	0.77

**Notes:** This table summarizes marginal effects from probit regressions testing the response and denial associated with each experimental intervention. Marginal effects are evaluated at the mean of the dependent indicator, and at the mean of the control variables where relevant. The first column presents the baseline response model, while the second column estimates the model with a full set of controls. Control variables include indicators for filing status, past claiming behavior, mode of tax preparation, gender, as well as expected benefit size and income. The next two columns estimate the baseline model for recipients with and without dependents. The final columns estimate the baseline model of denials without and then with controls. The relative size of the estimated effects, compared to either the response rate of the complex (original) mailing or the response rate of the simple (baseline) mailing, is reported in brackets. P-values report results of F-tests that check for the joint significance of interventions in the specified categories. Errors are robust. \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 6**  
**PERSISTENCE OF TREATMENTS AND TAKE-UP INERTIA**

	Pre and Post Experiment Claiming		
	TY 2007	TY 2008	TY 2010
Full Sample (35,050)	0.162 (0.369)	0.158 (0.365)	0.245 (0.430)
p-value of t-test (t and t-1)		[.149]	[.000]
Responded to Mailing (7,711)	0.16 (0.366)	0.156 (0.363)	0.245 (0.430)
p-value of t-test (t and t-1)		[.228]	[.000]
<b>Inertial Effect of TY 2009 Claiming</b>			
Dependent Variable - TY 2010 EITC (Yes/No)			
	Full Sample		
	OLS	IV	
Claiming 2009 (Yes/No)		0.108*** (0.006)	0.090* (0.049)
N		35,050	35,050
R-Squared		0.04	0.02

**Table 7**  
**SUMMARY STATISTICS OF EXPERIMENTAL RESPONSE**

Variable Name	ALL SAMPLE		W/O DEPENDENTS		W/ DEPENDENTS	
	Response	N	Response	N	Response	N
Full Sample	0.22	35,050	0.25	23,618	0.16	11,432
<u>Panel A: Demographic Variables</u>						
Female, Age < 35	0.29	3,738	0.30	2,061	0.21	677
Female, Age ≥ 35	0.25	6,544	0.28	4,445	0.18	2,099
Male, Age < 35	0.23	7,329	0.25	5,731	0.18	1,598
Male, Age ≥ 35	0.19	17,424	0.22	10,375	0.15	7,049
<u>Panel B: Tax Variables</u>						
Self-Preparation	0.26	21,890	0.27	18,363	0.23	3,527
Paid Preparation	0.16	13,136	0.20	5,235	0.13	7,901
Past Claim from TY 2006 to TY	0.23	10,165	0.27	5,870	0.17	4,295
Past Claim + Self Prep	0.29	5,007	0.30	2,936	0.25	1,071
Past Claim + Paid Prep	0.17	5,149	0.21	1,927	0.15	3,222
Self Employment Income > \$0	0.19	6,427	0.19	4,656	0.18	1,771
Filing Status = Single	0.26	20,317	0.26	20,317	--	--
Filing Status = MFJ	0.18	9,522	0.21	3,134	0.16	6,388
Filing Status = HOH	0.16	5,196	0.13	167	0.16	5,029
<u>Panel C: Benefit and Income</u>						
Expected Benefits: \$0 to \$499	0.24	26,988	0.25	23,618	0.15	3,370
Expected Benefits: \$500 to \$1499	0.18	2,708	--	--	0.18	2,708
Expected Benefits: \$1500 to \$2499	0.17	1,701	--	--	0.17	1,701
Expected Benefits: \$2500 to \$3999	0.15	2,259	--	--	0.15	2,259
Expected Benefits: ≥ \$4000	0.14	1,394	--	--	0.14	1,394
Earned Income: \$1 to \$4999	0.24	9,759	0.24	9,230	0.22	529
Earned Income: \$5000 to \$9999	0.26	8,490	0.26	7,988	0.18	502
Earned Income: \$10000 to \$19999	0.23	7,895	0.25	6,400	0.16	1,495
Earned Income: \$20000 to \$29999	0.15	2,275	--	--	0.15	2,275
Earned Income: ≥ \$30000	0.16	6,631	--	--	0.16	6,631

**Notes:** This table summarizes response statistics by demographic, tax and benefit/income variables for various subsets of the experimental sample. Panel A reports response statistics by age and gender, Panel B reports response by various tax variables, and Panel C reports response by expected benefit size and earned income. Not all sub-categories sum to 35,050 due to either missing data or excluded sub-categories.

Table 8

## EVALUATING COMPETING FRAMEWORKS TO EXPLAIN INCOMPLETE TAKE-UP

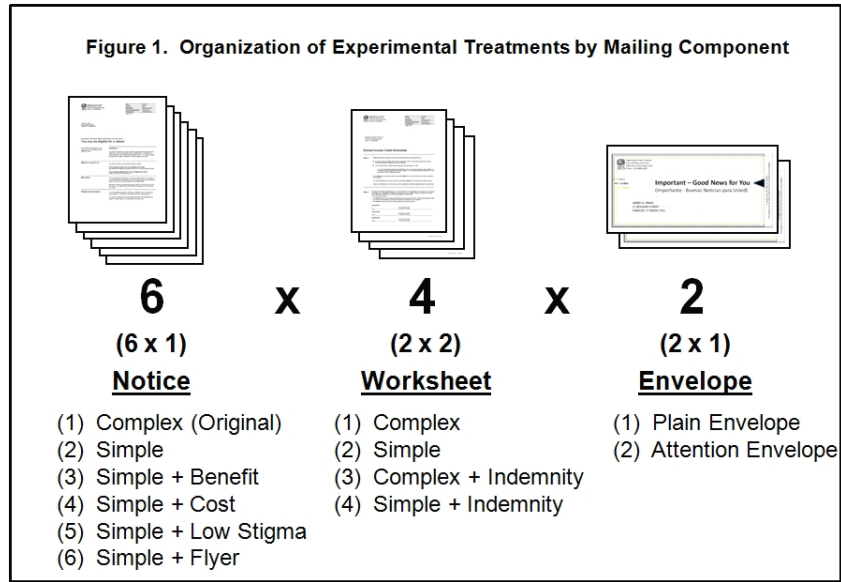
Finding	STANDARD MODEL		BEHAVIORAL MODEL	
	Cost / Benefit Calculation	+ Stigma	Information Deficits	Hassle Cost/ Complexity
<u>Panel A: Survey Results</u>				
1. Lack of Program Awareness	No	No	Yes	Yes
2. Misperception of Eligibility	No	No	Yes	Yes
3. Underestimation of Benefits	No	No	Yes	Yes
4. Overestimate of Audit Rate	No	No	Yes	Yes
5. Modest Baseline Levels of Stigma	--	No	--	--
<u>Panel B: Experimental Findings</u>				
6. Repeat Mailings Improve Response	No	No	Yes	Yes
7. (Suggestive) Language Barrier	--	--	Yes	Yes
8. Simplification Improves Response	No	No	Yes	Yes
9. Benefit Display Improves Response	No	No	Yes	Yes
10. Magnitude of Benefit Display is Not Monotonically Related to Improved Response	No	No	Yes	Yes
11. Cost Information Does Not Improve Response	Yes	Yes	--	--
12. Stigma Reduction Does Not Improve Response	--	--	--	--
13. (Partial) Effect Persistence in 2011	--	--	Yes	Yes
<u>Panel C: Treatment Heterogeneity</u>				
14. No Moderation By Benefit Size	No	No	Yes	Yes
15. Low Income More Sensitive to Complexity	No	No	Yes	Yes

**Notes:** This table adjudicates between standard and behavioral decision-making frameworks for take-up in light of the survey and experimental evidence from the study. The table reports whether each finding is consistent (or indifferent) with (to) the predictions of each framework. Behavioral explanations are organized into two categories approximately drawn from Bertrand, Mullainathan, and Shafir (2006).

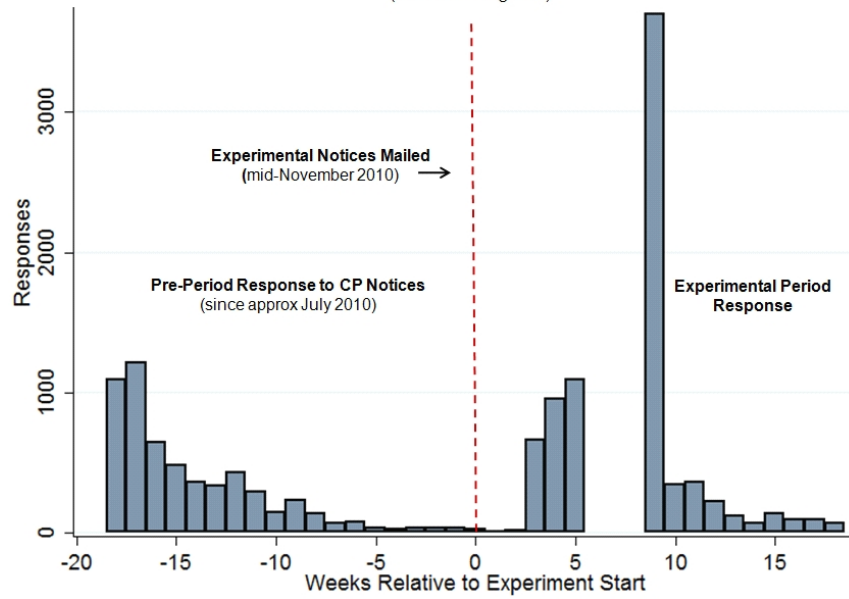
**Table 9**  
**PROJECTED POLICY IMPACT OF EXPERIMENTAL FINDINGS ACROSS EITC FILING NON-CLAIMANTS (CA TY 2009)**

Mailing Type	EXPERIMENT (CA)			REPEAT MAILING (US)			CP RECIPIENTS (US)			FILING NON-CLAIMANTS (CP + NON-CP, US)			
	Response	Avg Ben		N	% TU	Benefits	N	% TU	Benefits	N	% TU	Benefits	% Δ Ben Equiv
Complex Notice	<b>0.14</b>	<b>\$461</b>		+44,988	0.14 (0.00)	+\$24m	<b>321,340</b>	<b>0.47</b> (0.01)	<b>\$121m</b>	1,128,000	0.47 (0.05)	\$520m	--
Simplification	<b>0.23</b>	<b>\$514</b>		+73,908	0.23 (0.00)	+\$38m	+54,981	0.56 (0.00)	+\$28m	+216,000	0.56 (0.01)	+\$111m	+23%
Simplification + Benefit Display	<b>0.31</b>	<b>\$544</b>		+99,615	0.31 (0.00)	+\$54m	+103,854	0.64 (0.00)	+\$56m	+408,000	0.64 (0.02)	+\$222m	+49%
Optimal Mailing	<b>0.35</b>	<b>\$637</b>		+112,469	0.35 (0.00)	+\$72m	+128,290	0.68 (0.01)	+\$81m	+504,000	0.68 (0.02)	+\$321m	+62%
Optimal Mailing + Repeat (Optimal)	<b>0.35</b>	<b>\$637</b>		--	--	--	+200,987	0.80 (0.01)	+\$128m	+789,600	0.80 (0.03)	+\$503m	+101%
			Total	Response	Benefits		Total	Response	Benefits	Total	Response	Benefits	
Actual Category Statistics (CA TY 2009)			<b>321,340</b>	--	--		<b>610,904</b>	<b>0.47</b>	<b>\$121m</b>	~2.4m	0.13 (Assumes Total Eligible: ~27m)	--	

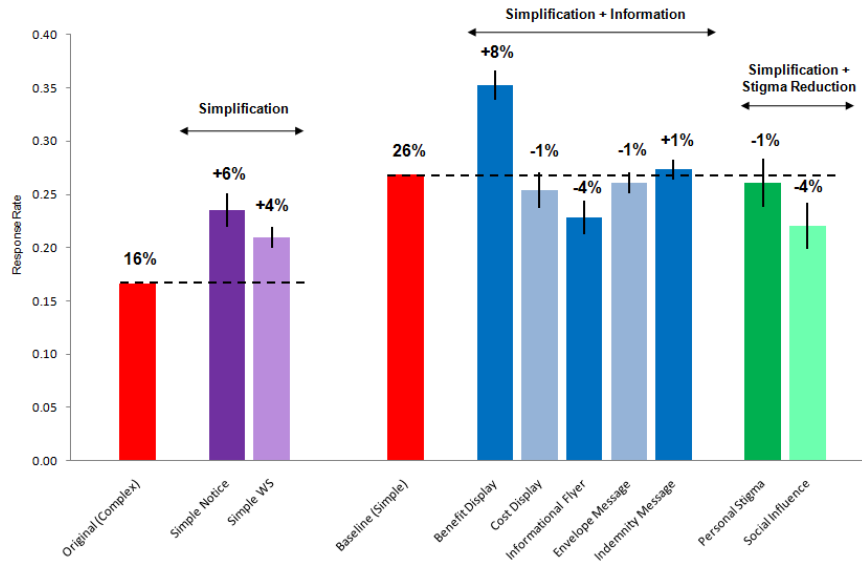
Notes: This table projects the experimental findings to broader populations of filing non-claimants under various assumptions for CA TY 2009. Bolded figures are exact and are from IRS while other figures are estimated. Parenthetically, we report the share of overall program take-up that could be improved if the given projection were to be adopted. We project results for the simplified mailing, the simplified mailing with benefit information, the set of mailings generated from the optimal mapping exercise described in the text, as well as the combination of an optimal mailing and a repeat notice. The first set of columns projects the response of the mailings to the entire population of CP non-respondents. The second set of columns projects the results under the assumption that the experimental mailings replace the CP notice. The final set of columns projects the results in the scenario where an initial notice, based on the experimental mailings, is applied to the entire population of non-filing non-claimants. The number of total non-filing non-claimants is estimated using take-up rates from Pisager (2009). Specifics of the derivation of the last column, which displays the benefit equivalence of the projections, are described in the text.



**Figure 2. CA Notice Response since July 2010**  
(IRS Processing Date)

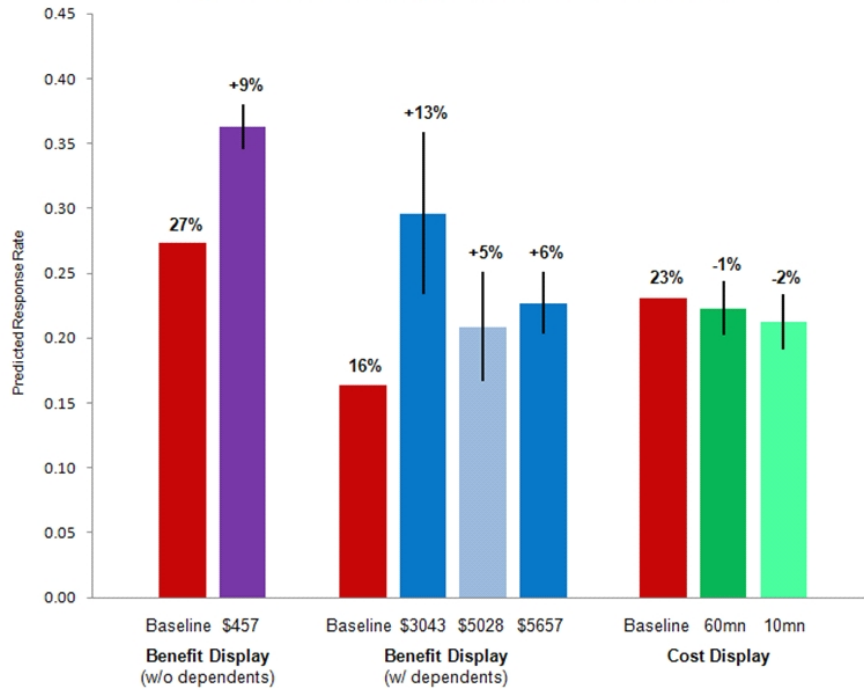


**Figure 3. Predicted Response by Experimental Intervention**  
(evaluated assuming no dependents)



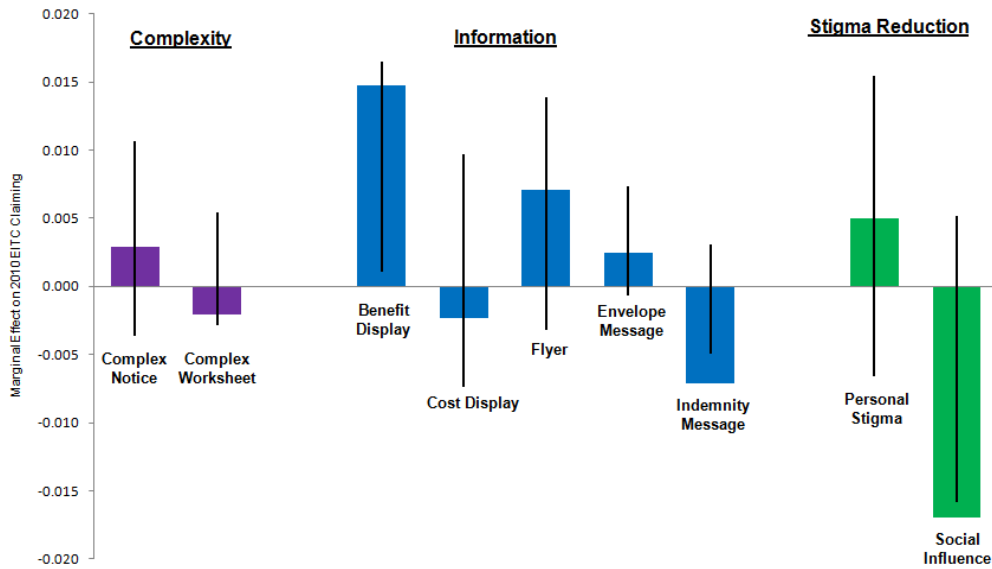


**Figure 4. Predicted Response for Benefit and Cost Notices**

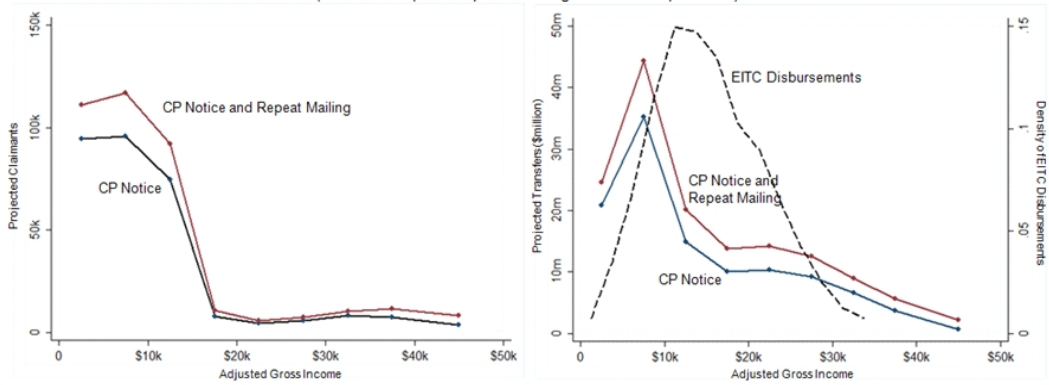


**Figure 5. Marginal Effect on 2010 EITC Claiming by Intervention**

(Relative to baseline notice; controlling for presence of dependents, baseline claiming is .25)



**Figure 6. Projected Claimants and Transfers for Repeat Mailing by Income**  
 (assumes a repeat simplified mailing to CP non-respondents)



## 11 Appendix A – Model of Incomplete Take-Up (Not for Publication)

We attempt to theoretically organize the analysis by considering a simple model of the decision to take-up in the presence of transaction costs and social stigma in the spirit of Moffitt (1983). The model is intended to apply to the population targeted by the present experiment. We then extend the standard model to allow individuals to misperceive the costs and benefits of take-up in a manner consistent with the psychology that underlies the decision. Specifically, we introduce a social planner who first dictates the salience and the complexity with which the program is administered. Program salience and complexity help shape perceptions of program costs and benefits, and, ultimately, the decision of whether to take-up.

### 11.1 Standard Model of Take-Up

Assume an individual is eligible for benefits from a means-tested program. We can specify the individual's utility by the following function:

$$U = U(Y + \theta_1 P b - (\theta_2 + \pi)P)$$

where  $Y$  is income prior to benefits,  $b$  is the non-negative benefit amount, and  $P$  is a binary choice variable describing the individual's decision to participate in the program. The model permits both benefit varying and non-varying costs associated with social stigma, represented by,  $-\theta_1$ , and  $\theta_2$  respectively. A fixed set of administrative (or direct transaction) costs are indicated by  $\pi$ . For tractability, we represent utility with a negative exponential function with some non-positive parameter of risk aversion,  $\ell$ , and that is additively separable in logs. An optimizing eligible agent, with a utility function as specified above, will choose  $P = 1$  if, and only if, the following condition holds:

$$b \geq \frac{\theta_2 + \pi}{\theta_1}$$

In this simple framework, the likelihood that an individual participates in a program increases in benefits,  $b$ , and decreases in costs linked to stigma ( $-\theta_1$ ,  $\theta_2$ ) and administration ( $\pi$ ). Here, the choice to not participate must be rationalized by sufficiently large administrative and/or stigma costs.

## 11.2 Psychological Model of Take-Up

We introduce additional descriptive realism to the model by permitting taxpayers to misperceive benefits ( $\widehat{b}$ ) and costs ( $\widehat{\pi}$ ). The misperception of costs and benefits is determined by the complexity and salience with which policymakers present program and benefit information. For simplicity, we assume that costs associated with stigma are accurately perceived such that a taxpayer has the following utility:

$$U = U(Y + \theta_1 P \widehat{b} - (\theta_2 + \widehat{\pi})P)$$

**Informational Salience.** We first introduce the notion of program salience,  $s_{eic}$ , and benefit salience,  $s_b$ . Our invocation of salience in this model is predicated on research which asserts that the limited attentional or processing capacity of decision-makers (Kahneman 1973) forces individuals to selectively attend to available information (see DellaVigna 2009). We therefore characterize salience as the likelihood that particular information is able to command limited processing resources. We assume that the amount of information, as well as its salience, is set exogenously by some social planner.

Salience enters the model in the following way. The probability that the agent engages in the maximization is some function of program salience,  $s_{eic}$ . For simplicity, assume that if  $s_{eic}$  is less than some awareness threshold,  $k$ , then the recipient is unaware that she faces a maximization problem. In this case, the agent makes no choice and implicitly sets  $P = 0$ . If  $s_{eic} \geq k$ , the agent proceeds with the optimization.

Benefit salience,  $s_b$ , helps to determine both the level and variance of an individual's beliefs regarding the magnitude of owed benefits. That is, in light of prior recipient unresponsiveness, as well as recipient surveys indicating pessimistic beliefs about eligibility and benefit amounts, we allow individuals to have biased, as well as noisy, expectations. Accordingly, benefit salience,  $s_b$ , should influence any such bias in expectations as well as the precision with which such beliefs are held.

More formally, imagine that  $\widehat{b}$  is drawn from some distribution  $N(b - \delta_b, 1/\lambda)$  centered at the true benefit amount,  $b$ , less some pessimism parameter (both motivated by the select nature of the non-claiming population and the survey data),  $\delta_b$ , and with precision  $\lambda$ . If  $\delta_b$  is a negative function of benefit salience, then higher salience leads to a lower bias. We can also write the precision of beliefs as a positive function of benefit salience:  $\lambda = \eta(s_b)$ . Expected benefits can therefore be described as true benefits, less pessimism, perturbed by some error:

$$\widehat{b}(s) = b - \delta_b(s_b) + \varepsilon(s_b)$$

**Complexity.** Second, perceptions of program costs may be shaped by the complexity,  $c$ , with which program and claiming information is presented. Specifically, we view the amount of computational effort required to process and understand a given set of information as our measure of complexity. Like salience, we assume informational complexity is determined exogenously by a social planner.

Our intuition is that individuals have noisy, and possibly biased, expectations regarding the administrative costs of claiming. These costs are a positive function of complexity to the extent that higher complexity in the claiming process demands more effort. The model also permits individuals to hold pessimistic beliefs regarding the claiming costs. The introduction of a pessimism parameter,  $\delta_\pi$ , is, in part, motivated by the past unresponsive of this population and survey results indicating beliefs that the program is complicated. We therefore describe the expectation of direct transaction costs as actual costs, with possible pessimism, perturbed by some normally distributed error:

$$\hat{\pi} = \pi(c) + \delta_\pi + v$$

**Take-Up Decision.** Under this formulation, the take-up problem proceeds in two steps. First, the social planner determines the levels of program and benefit information/salience, and also establishes the complexity of the claiming process based on a variety of considerations. We treat  $s$  and  $c$  as exogenous inputs into the model. Second, if the eligible recipient is aware of the program, then based on a calculation of anticipated benefits and costs, the recipient decides whether to participate. Importantly, we assume that individuals are cognizant of the uncertainty that characterizes their expectations but are naive to any biases to which they may be subject.

Therefore, in the amended model, if  $s_{eic} < k$ , then the individual sets  $P = 0$ . Otherwise, she chooses  $P$  to solve:

$$\max_{\{P \in (0,1)\}} U[Y + \theta_1 P \hat{b}(s_b) - (\theta_2 + \hat{\pi}(c))P]$$

Given additively separable negative exponential utility with parameter of risk aversion,  $\ell$ , perceived benefits,  $\hat{b}$ , and costs,  $\hat{\pi}$ , that are normally distributed, the agent will choose  $P = 1$  if, and only if,  $s_{eic} \geq k$  and the following condition holds:

$$b > \frac{\theta_2 + \pi + \delta_\pi}{\theta_1} + \delta_b + \frac{\theta_1 \ell}{2\lambda}$$

Here, the decision to participate decreases in the magnitude of the bias associated with expectations of benefits,  $\delta_b$ , and costs,  $\delta_\pi$  and rises in the precision of benefit information

λ. Any systematic underestimation of benefits, or overestimation of costs, naturally leads to lower rates of take-up. Consequently, the social planner can provide better or more salient information, or can decide to reduce the complexity such information in order to improve participation. Our experimental interventions are designed to test the link between take-up, perceptions of benefits and costs, and informational complexity.

## 12 Appendix B – Survey Data and Analysis

### 12.1 Chicago Survey

Two surveys offer an important supplement to the field experiment. A first, the Chicago Survey, is a survey of low-income taxpayers which provides motivation for the experimental design and provides a set of baseline distributions regarding cost and benefit parameters which help illuminate the factors that affect average, rather than just marginal, take-up behavior.

**Research Design (Chicago Survey).** The Chicago Survey consists of three segments. A first segment elicits basic income and demographic detail that permit the authors to approximate EITC eligibility and benefit size. A second segment gauges recipient awareness of the program; beliefs regarding eligibility, incomplete take-up, and the likelihood of an audit; and proclivity towards opening IRS mail. The final segment solicits expectations of various program cost and benefit parameters after guiding the reader through a sample, randomized, informational notice and claiming worksheet.

The survey was administered to low-income tax filers at five Chicago tax-centers, as well as one in San Francisco, organized by local organizations (the Chicago sites were managed by the Center for Economic Progress and Ladder-Up) to assist in tax preparation. Approximately 1,200 surveys were distributed from February through April 2011 by the authors and site volunteers.

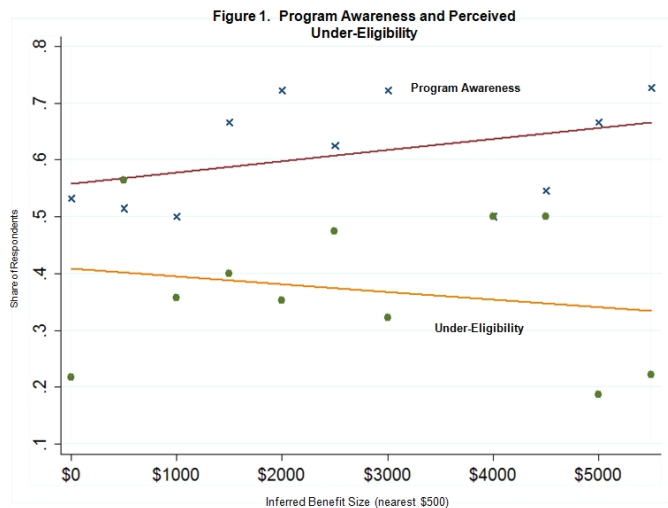
Surveys typically required 10 to 15 minutes to complete and were almost always completed during the “intake process” when clients fill out required forms and wait for a preparer to become available.<sup>65</sup> Though verbal instructions often accompanied survey distribution, and volunteers were available to field questions, as anticipated, both the rate of overall non-response and item non-response is high. We did not hand out surveys to non-English speaking clients.

**Analysis of Heterogeneity.** Does benefit size moderate the incidence of low awareness and program misinformation? Figures 1 suggests that program awareness, for those

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<sup>65</sup>The intake process involved filling out additional forms handed out by the tax-center to facilitate the preparation process. Clients were typically stationed in a waiting room before they met with a preparer. The wait was usually 30 minutes to 2 hours.

who are appear eligible, does modestly increase as a function of inferred benefits (averaged across \$500 increments).<sup>66</sup> Moreover, beliefs of “under-eligibility”, or ineligibility when actually eligible, decrease with larger benefits (both trends are statistically significant).<sup>67</sup>



**Figure 2. Estimated Benefits and Actual Benefits  
(for those eligible)**

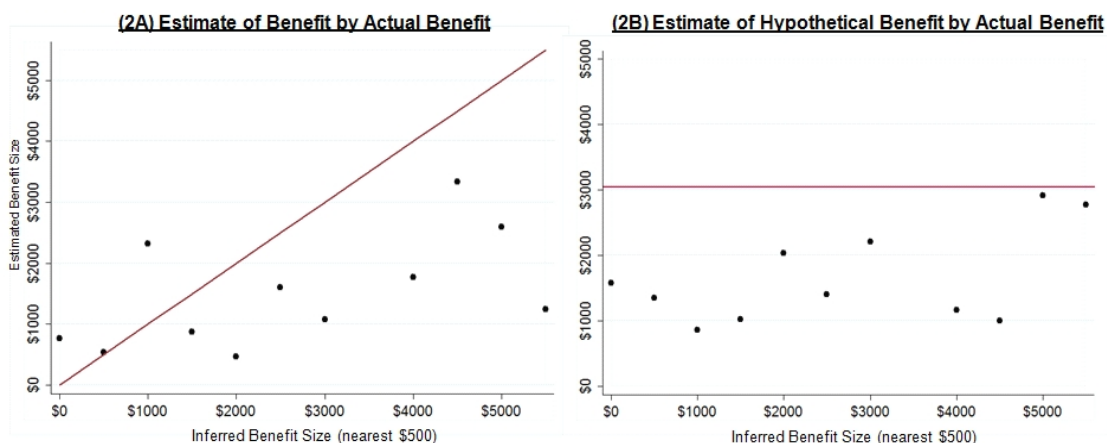


Figure 2 depicts expected benefits as well as benefit estimates from a hypothetical scenario posed in the survey by inferred benefit size. The accuracy of benefits does not appear to increase with benefit size, while estimates of worksheet claiming time do appear to better calibrated for those with larger benefits (not reported here). The accuracy of anticipated audits is also unimproved by benefit size. Overall, despite evidence for higher

<sup>66</sup>The figures omit the point for \$3500 due to a sample of less than 10 individuals.

<sup>67</sup>The simple linear regression of awareness, assuming eligibility, on benefit size yields a negative slope coefficient that is statistically significant,  $p < .05$ ; the corresponding slope coefficient of the regression of under-eligibility and benefit size is also negative and significant with  $p < .01$ .

awareness for those receiving larger benefits, the analysis suggests that misinformation is fairly pervasive across the population.

**Limits to Survey Evidence.** There are caveats to the interpretation of the survey results. Because the survey sample is from the population of clients at a tax-help center, individuals may have particularly low awareness and knowledge because they rightly anticipate that a preparer will soon apprise them of any relevant information, or they are merely unaware of a large refund’s specific decomposition.<sup>68</sup> Alternatively, because the survey is administered during the middle of the filing season, it may overstate the awareness and knowledge of those receiving the experimental notices in the late fall. A second limitation is that survey elicitation of broach threatening topics, including tax and welfare information, may not produce reliable response.<sup>69</sup> Finally, the survey canvasses opinions of clients who are primarily from Chicago. However, we do not find sharp differences in outcomes between the Chicago sample and the small sample collected from the San Francisco site.

## 12.2 Psychometric Survey

A second survey was administered to approximately 2,800 online subjects in order to understand how readers perceive and attend to the various notice and worksheet interventions utilized in the experiment. We use the data from this survey to generate the analysis which populates Appendix Tables A4 and A5. The notice and worksheet, to which each subject was exposed, was randomized at the individual level. The survey was designed using the Qualtrics software, and subjects, from the U.S., were recruited from an online marketplace, Amazon Mechanical Turk, in August 2011. Subjects were paid a \$1 fee for completing the instrument. The structure of the psychometric survey paralleled that of the Chicago Survey but featured a richer set of questions eliciting program beliefs and perceptions. Beyond featuring a much larger sample than the Chicago Survey, the survey was distinguished by near zero item non-response—that is, of those who began the survey and received payment, due to built-in forced response mechanisms, item non-response is minimal.

## 13 Appendix C – Balancing Checks for Experiment

**Balancing Checks.** A series of regressions ensures that the randomization strategy produced treatments that are balanced across key economic variables of interest. We implement the balancing tests with individual-level regressions of the following form:

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<sup>68</sup> A typical tax refund might consist of a return on an income withdrawal, the Make Work Pay Tax Credit, and possibly, an education credit as well as an EITC credit.

<sup>69</sup> Amongst others, Hessing and Elffers and Wiegel articulate this point (1988).



$$Outcome_{nwe} = \alpha + \varphi_n + \gamma_w + \theta_e + \varepsilon_{nwe}$$

Here,  $n$  indexes the notice,  $w$  indexes the worksheet, and  $e$  indexes the envelope. Indicator variables mark assignment into each of the three components of the mailings and the excluded category consists of the simple notice, simple worksheet and plain envelope. The dependent variables relate to income, expected benefit levels, filing status, and past claiming. Appendix Table A3 reports the results of these regressions. The F-tests, reported at the bottom of Panel A, fail to reject the null hypothesis that any of the outcomes are jointly predicted by the treatment assignments. Additional regressions and the corresponding F-tests, reported in Panel B, confirm that the unique combination of assigned notices, worksheets and envelopes, also do not predict the outcomes of interest. Overall, the analysis suggests that the treatments are successfully randomized.

## 14 Appendix D – Additional Tables and Figures

**Appendix Table A1**  
**SURVEY EVIDENCE: EITC AWARENESS AND CONSTRUCTION OF PROGRAM PARAMETERS**

Parameter	CHICAGO SURVEY (N = 1139)				MECH TURK SURVEY (N = 2800)		Correct Information & Notes	
	Empirical Distribution of Response / Belief				Median			
	Mean	25%	50%	75%				
Panel A. Program Awareness Prior to Sample Informational Notice								
<b>Program Awareness</b>								
Aware of EITC	0.54				0.64 (mean)			
Aware amongst Eligible	0.56				0.66 (mean)			
Open IRS Mail	0.84				0.85 (mean)			
Panel B. Amongst Those with Program Awareness, Either Due to Prior Knowledge, or to Sample EITC Notice								
<b>Program Benefits</b>								
"Definitely" or "Probably" Eligible	0.45				0.43		.65 - Chicago Survey; .30 - Mech Turk Survey	
Benefit Size (if eligible)	920	200	400	1000	584			
Expected / Actual Benefit (if eligible)	3.8	0.3	0.8	1.5	0.6			
Hypothetical Benefit Calculation	\$1,725	\$450	\$1,000	\$2,000	\$500		\$3043	
<b>Program Costs</b>								
Worksheet Claiming Time	~24 mins	< 30 mins	< 30 mins	30 to 60 mins	~15 mins		Worksheet requires < 10 minutes	
Willingness to Pay for Worksheet	\$61	\$25	\$50	\$70	\$20			
Audit Rate (amongst aware)	0.25	0.05	0.15	0.50	0.23		1.1% - All Filers; 1.8% - EITC Filers	
Worry of Penalty/Audit	2.2	2.0	2.0	3.0	--			
(1 - Strongly Disagree to 4 - Strongly Agree)								
<b>Complexity</b>								
Fail to Follow Instruction (Round to nearest \$1k)	0.20				--			
Did Not Complete Page Following Sample Notice	0.41				--			
Correct Answer to Comprehension Question	0.60				0.67 (mean)			
<b>Program Stigma</b>								
Recipients of Benefits Respected	2.6	2.0	3.0	3.0	76			
Don't Care if Friends Know	2.8	2.0	3.0	3.0	(1 to 100 scale)			
(1 - Strongly Disagree to 4 - Strongly Agree)								

Notes: This table provides data on EITC awareness as well as the perception of EITC cost and benefit parameters from respondents of both the Chicago Survey, as well as the Mech Turk Survey. Please refer to the text for a description of the survey design and sample.

Appendix Table A2

SURVEY EVIDENCE: CANDIDATE EXPLANATIONS FOR INCOMPLETE TAKE-UP

EXPLANATION	ALL SAMPLE		ELIGIBLE SAMPLE	
	Why Others Fail to Claim (1)	Why I Will Not Claim (2)	Why Others Fail to Claim (3)	Why I Will Not Claim (4)
Perceived Ineligibility	0.46	0.52	0.43	0.44
Forms too Confusing	0.17	0.19	0.16	0.24
Benefits "Too Small" for Effort	0.09	0.05	0.05	0.07
Fear of Penalty/Audit	0.05	0.06	0.05	0.07
Don't "Need" Government Help	0.01	0.02	0.00	0.02
Non-Response	0.22	0.16	0.31	0.16
N	N = 524	N = 187	N = 357	N = 119

**Notes:** This table reports results of a question from the Chicago Survey regarding respondent beliefs about the causes of incomplete take-up. The survey question included a menu of five responses along with an option to indicate "Other" and write-in a response. Results of the top panel are scaled to reflect results excluding item non-response which is reported separately in the second panel. Eligible sample is estimated from economic and household information elicited in the survey.

Appendix Table A3

## BALANCING CHECK OF EXPERIMENTAL TREATMENTS

## Panel A: Regressions using Indicators of Intervention Categories (Notice, Worksheet, Envelope)

INTERVENTION	DEPENDENT VARIABLE - TAX VARIABLES (OLS)						
	INCOME AND BENEFITS			FILING STATUS			CLAIMING
	AGI (1)	Earned Inc (2)	Exp Benefits (3)	Single (4)	Married (5)	HOH (6)	Past 3 Yrs (7)
[Simple Notice, Simple Worksheet, Plain Envelope - Excluded]							
Complex Notice	322 (698)	46 (199)	-11 (19)	-0.001 (0.007)	-0.001 (0.008)	0.001 (0.006)	-0.003 (0.008)
Benefit Display [Low]	1322** (524)	105 (213)	-5 (20)	-0.003 (0.008)	0.007 (0.008)	-0.004 (0.006)	0.003 (0.009)
Benefit Display [High]	750 (596)	-43 (202)	12 (19)	-0.004 (0.007)	-0.001 (0.008)	0.005 (0.006)	0.001 (0.008)
Program Stigma [Hard Work]	257 (893)	213 (273)	25 (26)	-0.013 (0.010)	0.018* (0.011)	-0.006 (0.008)	0.030*** (0.011)
Social Stigma [4 out of 5]	-560 (1261)	-19 (263)	46* (27)	-0.011 (0.010)	0.011 (0.010)	-0.001 (0.008)	0.011 (0.011)
Transaction Costs [10 min]	-360 (832)	-261 (288)	-2 (25)	-0.005 (0.011)	0.004 (0.011)	0.001 (0.008)	0.015 (0.012)
Transaction Costs [60 min]	932 (784)	-5 (275)	-18 (26)	-0.002 (0.010)	-0.004 (0.011)	0.006 (0.008)	0.010 (0.011)
Complex Worksheet	306 (511)	146 (125)	-14 (12)	-0.001 (0.005)	0.007 (0.005)	-0.006 (0.004)	-0.001 (0.005)
Indemnification Message	-392 (432)	-304*** (117)	-18 (11)	0.005 (0.004)	0.002 (0.005)	-0.006 (0.004)	-0.014*** (0.005)
Informational Flyer	-1106 (1081)	84 (199)	8 (20)	-0.010 (0.007)	0.0138* (0.008)	-0.004 (0.006)	0.007 (0.008)
Envelope Message	-293 (443)	185.5* (108)	14 (11)	-0.004 (0.004)	0.006 (0.004)	-0.002 (0.003)	0.002 (0.005)
Observations	35050	35050	35050	35050	35050	35050	35050
R <sup>2</sup>	0.00	0.00	0.00	0.00	0.00	0.00	0.00
F(11, 2152)	1.34	1.22	1.04	0.53	0.96	0.77	1.72

## Panel B: Regressions using Indicators for each Unique Intervention Bundle

	DEPENDENT VARIABLE - TAX VARIABLES (OLS)						
	AGI	Earned Inc	Exp Benefits	Single	Married	HOH	Past 3 Yrs
Indicators for Unique Treatment	X	X	X	X	X	X	X
Observations	35050	35050	35050	35050	35050	35050	35050
R <sup>2</sup>	0.00	0.00	0.00	0.00	0.00	0.00	0.00
F(71, 2152)	1.23	1.01	1.16	1.01	1.15	0.86	1.45

Notes: This table reports the outcome of a series of regressions meant to test whether the experimental randomization produced balanced treatment groups across a number of observable economic and tax filing variables of interest. The upper panel presents results of regressions on indicators for each mailing component. The lower panel presents results of regressions on dummy variables which indicate each unique treatment bundle. There are 72 unique treatment bundles; this results from the interactions between 9 types of notices (the informational flyer is counted as a notice in the randomization), 4 types of worksheets and 2 types of envelopes. Standard errors are robust and clustered at the zipcode level; there are 2153 unique zipcodes in the sample.

Appendix Table A4

## ATTENTIONAL AND INFERENTIAL RESPONSE TO INFORMATIONAL COMPLEXITY

Intervention	COMPLEXITY	ATTENTIONAL MEASURES			INFERENTIAL MEASURES (BENEFIT AND COST)				
	Complexity (0-100)	Intent to Return (0,1)	Careful Read (0-100)	Comprehension (0,1)	Eligibility (0,1)	Benefit Size (ln \$)	Claiming Costs (ln \$)	Stigma (0-100)	Audit Rates (0-100)
Complex Notice (CN)	5.469** (2.717)	-0.007 (0.045)	-5.001* (2.761)	-0.200*** (0.050)	-4.107 (4.005)	0.257** (0.112)	-0.039 (0.097)	-0.002 (0.038)	1.806 (2.653)
Complex WS (CWS)	-0.695 (2.666)	-0.016 (0.045)	-1.861 (2.665)	-0.062 (0.046)	-9.861** (4.032)	0.286*** (0.107)	-0.014 (0.095)	0.000 (0.041)	0.631 (2.660)
Flyer (F)	-0.716 (2.421)	-0.163*** (0.047)	0.520 (2.374)	-0.210*** (0.047)	-5.791 (3.898)	-0.192** (0.097)	0.208** (0.096)	-0.025 (0.035)	0.314 (2.387)
CN + CWS	0.779 (3.847)	-0.06 (0.066)	1.311 (4.025)	0.057 (0.071)	6.387 (5.718)	-0.344** (0.156)	0.139 (0.137)	-0.026 (0.055)	-1.725 (3.750)
CWS + F	3.647 (3.557)	0.209*** (0.065)	-1.837 (3.663)	0.166** (0.069)	11.100* (5.674)	-0.261* (0.141)	-0.201 (0.129)	0.045 (0.052)	-1.858 (3.524)

Notes: This table provides output from regressions to capture subject perceptions of complexity, attentiveness, and inferences regarding program benefits and costs following randomized exposure to complexity elements of the experimental mailings. Only non-dependent versions of the mailings are tested. All regressions include a fixed effect to control for the presence of dependents. Please refer to the text for a description of the sample and design of the survey. Errors are robust. \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Appendix Table A5

## ATTENTIONAL AND INFERENTIAL RESPONSE TO BENEFIT AND COST DISPLAYS

Intervention	ATTENTIONAL MEASURES			INFERENTIAL MEASURES (BENEFIT AND COST)				
	Intent to Return (0,1)	Careful Read (0-100)	Comprehension (0,1)	Eligibility (0,1)	Benefit Size (ln \$)	Claiming Costs (ln \$)	Stigma (0-100)	Audit Rates (0-100)
\$457 Benefit Display	0.084 (0.075)	5.519 (4.352)	-0.140* (0.082)	0.309*** (0.098)	0.262 (0.192)	0.289 (0.205)	3.254 (4.031)	4.137 (4.347)
\$3043 Benefit Display	0.07 (0.064)	-5.094 (3.744)	-0.180** (0.070)	-0.016 (0.084)	1.135*** (0.166)	0.078 (0.173)	-0.167 (3.468)	3.666 (3.740)
\$5k Benefit Display	-0.069 (0.057)	-3.516 (3.305)	-0.031 (0.062)	0.016 (0.074)	1.015*** (0.146)	0.238 (0.157)	3.828 (3.061)	2.431 (3.301)
10 Minute Cost Display	-0.002 (0.057)	-0.097 (3.280)	0.019 (0.061)	-0.165** (0.071)	0.021 (0.133)	-0.028 (0.155)	0.328 (3.177)	-1.416 (3.034)

Notes: This table provides output from regressions to capture subject attentiveness and inferences regarding program benefits and costs following randomized exposure to the benefit and cost displays of the experimental mailings. Only non-dependent versions of worksheet are used in the survey. All regressions include a fixed effect to control for the presence of dependents. Please refer to the text for a description of the sample and design of the survey. Errors are robust. \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Appendix Table A6

## EXPLANATIONS FOR EITC INCOMPLETE TAKE-UP

MECHANISM	SURVEY EXPECTATIONS		EXPERIMENTAL TREATMENT AND RESPONSE			IMPORTANCE
	MEDIAN	ACTUAL	INTERVENTION	Δ PERCEPTION (Psychometric Survey)	Δ TU	+ Δ TU
<b><u>Informational Explanations</u></b>						
Low Overall Awareness	0.54 aware 0.45 eligible	-- 0.65	Repeat Mailing	.85 Open Envelope	+ .14	HIGH
Low Cost Information	< 30 mns (fill out worksheet)	< 10 mns	60 Minute Claiming 10 Minute Claiming	+ 1 mn -5 mns	-0.01 -0.02	LOW
Low Benefit Information	\$670	\$1266	\$5657 Display \$5028 Display (2 dep) \$3043 Display (1 dep) \$457 Display	+\$701 +\$1400 +\$788 -\$18	+ .06 + .05 + .13 + .09	HIGH
Low Penalty Information	0.25 (share audited)	0.02	Indemnity Message	--	+ .01	UNCERTAIN
<b><u>Complexity Explanations</u></b>						
Complexity	34 (1 to 100 scale)	--	Simple Notice Simple Worksheet Simple Notice + Worksheet	-6.9 -1.7 -9.3	+ .06 + .04 + .10	HIGH
Language	--	--	Spanish Language Envelopes / Differential Response	--	+ .03	MEDIUM
<b><u>Stigma Explanations</u></b>						
Personal Stigma	respect - 77	--	Reward for Hard Work	--	-0.01	LOW
Social Stigma	worthwhile - 77 (1 to 100 scale)	--	Social Influence	+3 respect +5 worthwhile	-0.04	

Notes: This table integrates survey and experimental evidence in order to generate qualitative projections as to the relative importance of candidate explanations in the decision to take-up. We organize explanations into three mechanistic categories (i.e., information, complexity and stigma). The first set of columns reports ex-ante beliefs of relevant parameters from survey evidence. The second set of columns reports the pertinent experimental interventions and their effects on parameter beliefs as indicated from the psychometric evidence. The final column offers a qualitative judgment as to the importance of the designated explanation in explaining overall take-up.

Appendix Table A7

## OPTIMAL MAILING BUNDLES FOR SAMPLE SUB-GROUPS

	W/O DEPENDENTS								W/ DEPENDENTS							
	Low Earnings				High Earnings				Low Earnings				High Earnings			
	No Past Claim	Past Claim	No Past Claim	Past Claim	No Past Claim	Past Claim	No Past Claim	Past Claim	No Past Claim	Past Claim	No Past Claim	Past Claim	No Past Claim	Past Claim	No Past Claim	Past Claim
	No Self	Self	No Self	Self	No Self	Self	No Self	Self	No Self	Self	No Self	Self	No Self	Self	No Self	Self
Optimal Response Rate	0.361	0.39	0.306	0.412	0.323	0.363	0.412	0.508	0.333	0.5	0.429	0.667	0.184	0.403	0.234	0.446
Notice	Ben	Ben	Stigma	Ben	Ben	Ben	Trans	Ben	Ben	Trans	Stigma	Trans	Ben	Ben	Ben	Ben
Worksheet	Simp-I	Simp-I	Simp	Simp	Simp-I	Simp	Simp-I	Simp	Comp	Comp-I	Simp-I	Comp-I	Simp-I	Comp-I	Simp	Simp-I
Sample Size	2066	10156	1218	2924	1155	4164	634	977	331	418	230	181	4450	2145	3076	925

Notes: This table identifies optimal notice and worksheet combination based on analysis of response rates for each mailing group. "Low" and "High" earnings refer to earnings below and above median earnings. "Ben" refers to the benefit display notices, "Stigma" refers to the stigma reduction notices, while "Trans" refers to the claiming cost guidance notices. For worksheets, "Simp-I" refers to the simple worksheet with the indemnity message, "Simp" refers to the simple worksheet without the message, "Comp-I" refers to the complex notice with the message, and "Comp" refers to the complex notice without the message.

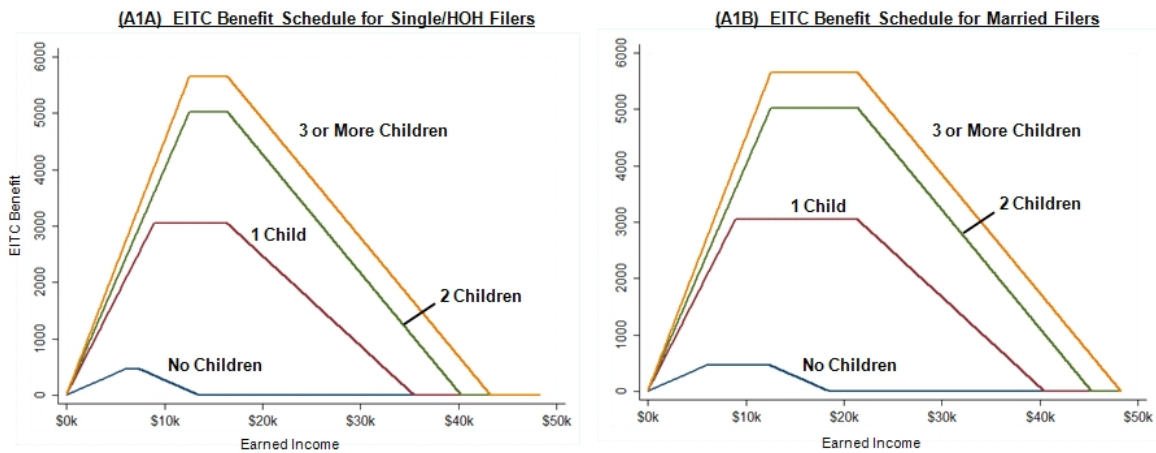
Appendix Table A8

## PROJECTED POLICY IMPACT OF EXPERIMENTAL FINDINGS ACROSS EITC NON-FILING CLAIMANTS (CA TY 2009)

	BASELINE		SENSITIVITY ANALYSIS - EXP BASELINE					SENSITIVITY ANALYSIS - CP BASELINE				
	EXP	CP	5%	10%	25%	50%	100%	5%	10%	25%	50%	100%
CP Original Notice	0.16	0.47	35k \$39m (0.00)	70k \$77m (0.00)	176k \$193m (0.01)	352k \$386m (0.01)	704k \$772m (0.03)	103k \$113m (0.00)	207k \$227m (0.01)	517k \$567m (0.02)	1.03m \$1.13b (0.04)	2.07m \$2.27b (0.08)
Simplification	0.22	0.53	48k \$53m (0.00)	97k \$106m (0.00)	242k \$265m (0.01)	484k \$530m (0.02)	968k \$1.06b (0.04)	117k \$128m (0.00)	233k \$256m (0.01)	583k \$639m (0.02)	1.17m \$1.28b (0.04)	2.33m \$2.56b (0.09)
Simplification + Benefit Display	0.23	0.54	51k \$55m (0.00)	101k \$111m (0.00)	253k \$277m (0.01)	506k \$555m (0.02)	1.01m \$1.11b (0.04)	119k \$130m (0.00)	238k \$260m (0.01)	594k \$651m (0.02)	1.19m \$1.30b (0.04)	2.38m \$2.60b (0.09)
Optimal Mailing	0.31	0.62	68k \$75m (0.00)	136k \$149m (0.01)	341k \$374m (0.01)	682k \$747m (0.03)	1.36m \$1.49b (0.05)	136k \$149m (0.01)	273k \$299m (0.01)	682k \$747m (0.03)	1.36m \$1.49b (0.05)	2.73m \$2.99b (0.10)
								<u>All Non-Claimants</u>		<u>NF Non-Claimants</u>		
								Eligible	N	Avg Ben	N	
Summary Statistics (CA TY 2009)								~27m	~6.8m	\$ 1,096	~4.4m	

**Notes:** This table projects the experimental findings to the broader population of non-filing non-claimants under various assumptions for CA TY 2009. Parenthetically, we report the possible improvement in the overall take-up rate under the given projection. We project results for the simplified mailing, the simplified mailing with benefit information, the set of mailings generated from the optimal mapping exercise described in the text, as well as the combination of an optimal mailing and a repeat notice. The first set of columns projects the response of the mailings assuming experimental response rates. The second set of columns projects the results assuming response rates of the CP notice adapted with the experimental interventions. The total number of filing non-claimants is estimated using take-up rates from Plugger (2009).

Appendix Figure A1. EITC Benefit Schedule by Filing Status



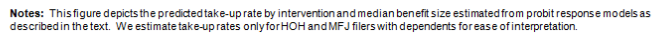
**Fraction of Population that is Hispanic (by zip code, 2007)**

- 100%
- 50%
- 0%

**LA Area Response and Hispanic Density (by zip code, min n=20)**

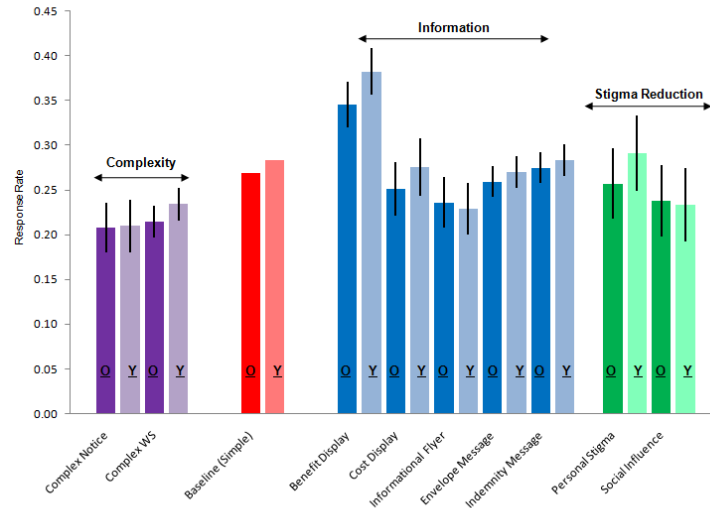
Map showing the distribution of the fraction of the population that is Hispanic by zip code in the Los Angeles area, 2007. The map includes labels for major cities and geographical features.

(HOH or MFJ Filers; High Benefit is  $\geq \$678$ , Low Benefit is  $< \$678$ )



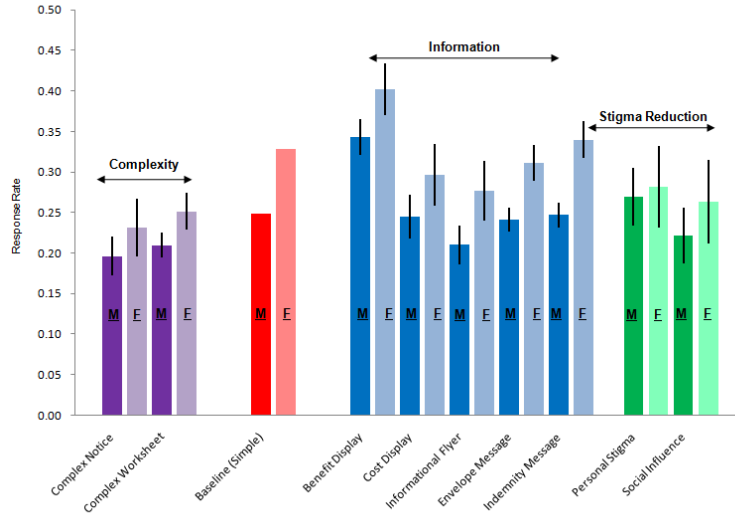


**Appendix Figure A4. Predicted Response by Experimental Intervention by Age**  
(Single Filers; Old is  $\geq 39$ , Young is  $< 39$ )



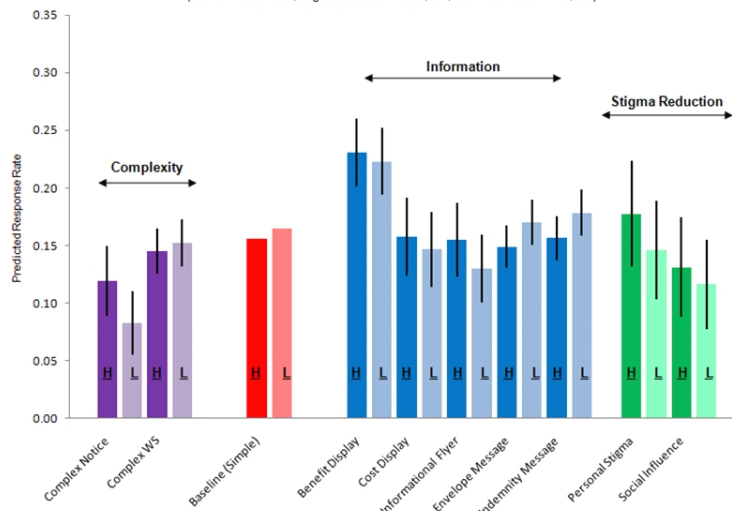
**Notes:** This figure depicts the predicted take-up rate by intervention and median age estimated from probit response models as described in the text. We estimate take-up rates only for single filers for ease of interpretation.

**Appendix Figure A5. Predicted Response by Experimental Intervention by Gender**  
(Single Filers)



**Notes:** This figure depicts the predicted take-up rate by intervention and gender estimated from probit response models as described in the text. We estimate take-up rates only for single filers for ease of interpretation.


**Appendix Figure A6. Predicted Response by Experimental Intervention by Median Income**  
(HOH or MFJ Filers; High Income is  $\geq \$33,487$ , Low Income is  $< \$33,487$ )



**Notes:** This figure depicts the predicted take-up rate by intervention and median income from probit response models as described in the text. We estimate take-up rates only for HOH or MFJ filers with dependents for ease of interpretation.

14.1 Example of Interventions

Panel A1: Baseline Notice



Department of the Treasury  
Internal Revenue Service  
Submission Processing Center  
P.O. Box 1218  
Fresno, CA 93882-0125

Notice  
Tax Year  
Submission Date  
Social Security Number  
To Contact Us

EO 9227  
2009  
09-09-2020  
000-00-0000  
1-800-829-1040

Page 1 of 4

JAMES O HANZIG  
22 BOULDER STREET  
HANSON, CT 06030-7205

Important information about the Earned Income Credit  
**You may be eligible for a refund**

Do not discard or overlook this notice because you may be entitled to some additional money.

Summary

Our records show that you may be eligible for a refund called the Earned Income Credit (EIC), which you did not claim on your 2009 tax return. The amount is for \$1,000.00. You should complete the worksheet on Page 2 to determine if you are eligible for the credit.

What you need to do

Complete the Earned Income Credit Worksheet on Page 3.  
If the worksheet confirms that you are eligible for the credit, sign and date the attached notification, and mail it to us in the enclosed envelope.  
If the worksheet indicates that you are not eligible for the credit, please do not return the notification to us.


Next steps

If you are eligible for the credit, we will send you a refund check in 6 to 8 weeks. If you are not eligible, we will send you a notice explaining why you are not eligible to collect. We will use your credit to reduce or pay off these debts.  
Next step: To receive your refund more quickly, with "EIC" on the EIC line of your form 1040. If you qualify for the credit, the IRS will calculate it for you and send you a check.

Additional information

If you need additional assistance, please call 1-800-829-1040, or visit online at [www.irs.gov/eic](http://www.irs.gov/eic). For tax forms, call 1-800-TAX-FORM (1-800-829-2875).  
You can also find tax forms and other helpful documents which explain the EIC program in greater detail (e.g., Publication 596) at [www.irs.gov](http://www.irs.gov).

Panel A2: "Old Notice" (page 1 of 2)



Department of the Treasury  
Internal Revenue Service  
Submission Processing Center  
P.O. Box 1218  
Fresno, CA 93882-0125

Notice  
Tax Year  
Submission Date  
Social Security Number  
To Contact Us

EO 9227  
2009  
09-09-2020  
000-00-0000  
1-800-829-1040

Page 1 of 4

JAMES O HANZIG  
22 BOULDER STREET  
HANSON, CT 06030-7205

**You May Be Eligible for a Refund  
If You Qualify for the Earned Income Credit**

Why We Are Sending You this Notice

You may qualify for the earned income credit (EIC). The EIC is for certain people who work and have earned income. This tax credit usually means more money in your pocket. It reduces the amount of tax you owe, and may give you a refund. Our records show:

- Your income falls in the eligible range to receive the EIC.
- You have a dependent who may be an EIC qualifying child, and
- You did not claim the EIC on your 2009 Individual Income Tax Return.

What You Need to Do


Income is not the only condition that determines if you qualify for EIC. We need you to complete the enclosed EIC Eligibility Check Sheet to see if you may qualify for the EIC. Take the following steps to complete the check sheet:

- Check that you are eligible for the EIC in Step 1.
- If your Social Security Number is not valid or if you are a qualifying dependent of another person, you do not qualify.
- If your Social Security Number is valid and you are not a qualifying dependent of another person, you may qualify. Continue to Step 2 only if you did not place a check next to any of the eligibility criteria in Step 1.
- In Steps 2 and 3, fill in the name and Social Security number for each child who may qualify you for the EIC and check that each child meets the stated requirements.
- Any NO answer for a child means that child is not your qualifying child for the EIC. Do not respond to this notice unless you have a qualifying child.
- All YES answers mean a child is your qualifying child for the EIC. Sign and date the declaration on the last page of this notice. Mail the completed EIC Eligibility Worksheet to us in the enclosed envelope.

Note: Return the EIC Worksheet to us *only* if you determine you may qualify for the EIC.

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## Panel B1: Low Complexity (No Dependents)



Department of the Treasury  
Internal Revenue Service  
Education Processing Center  
Fresno, CA 93888-0005

Notice: E-07027  
Tax Year: 2020  
Notice Date: November 2022  
Period Security Number: 999-19-9999  
Accession Number: 1 999-19-9999  
Page 3 of 4

INTERNAL REVENUE SERVICE  
Education Processing Center  
Fresno, CA 93888-0005

### Earned Income Credit Worksheet

**Step 1** Read each statement listed below, and check a checkbox next to any that describes you.

☐ My Social Security card says "Not Valid for Employment." AND my Social Security card was issued on or after 12/31/2019. I am not eligible for the credit.

☐ I am an Earned Income Credit-qualifying dependent of another person in 2020.

*Note: You are a qualifying dependent of another person if you live with your parents or other caretaker for more than half the year AND are either under the age of 19, are a student and under the age of 24, or are permanently or totally disabled at any age.*

If you checked any of the above boxes, you are not eligible for the credit. Please do not continue completing this worksheet.

If you did not check any of the above boxes, you are likely eligible for the credit! Please continue to Step 2.

Please call 1-800-828-9005 or visit [www.irs.gov/efc](https://www.irs.gov/efc) for additional information regarding the above requirements.

**Step 2** Sign and date the statement if you are eligible for the credit.

Under penalties of perjury, I declare that this information is true and correct to the best of my knowledge.

Your signature \_\_\_\_\_

Primary phone: \_\_\_\_\_ Best time to call: \_\_\_\_\_ Secondary phone: \_\_\_\_\_


Date: \_\_\_\_\_

\_\_\_\_\_ ☐ a.m. ☐ p.m. ☐ n/a

Best time to call: \_\_\_\_\_

For B1 use only—Circle 9904

## Panel B2: High Complexity (No Dependents) (Page 1 of 2)



Department of the Treasury  
Internal Revenue Service  
Education Processing Center  
Fresno, CA 93888-0005

Notice: E-07027  
Tax Year: 2020  
Notice Date: November 2022  
Period Security Number: 999-19-9999  
Accession Number: 1 999-19-9999  
Page 3 of 4

INTERNAL REVENUE SERVICE  
Education Processing Center  
Fresno, CA 93888-0005

### Earned Income Credit Worksheet

**Step 1** Read each statement listed below, and check a checkbox next to any that describes you.

☐ My Social Security card says "Not Valid for Employment." AND my Social Security card was issued on or after 12/31/2019. I am not eligible for the credit.

☐ I am an Earned Income Credit-qualifying dependent of another person in 2020.


*Note: You are a qualifying dependent of another person if you live with your parents or other caretaker for more than half the year AND are either under the age of 19, are a student and under the age of 24, or are permanently or totally disabled at any age.*

If you checked any of the above boxes, you are not eligible for the credit. Please do not continue completing this worksheet.

If you did not check any of the above boxes, you are likely eligible for the credit! Please continue to Step 2.

Please call 1-800-828-9005 or visit [www.irs.gov/efc](https://www.irs.gov/efc) for additional information regarding the above requirements.

## Panel C1: High Benefit



Department of the Treasury  
Internal Revenue Service  
Business Opportunity Center  
P.O. Box 10300  
Pawnee, CA 95068-0400

Notice  
Tax Year  
Notice Date  
Notice Period  
To Contact Us

EO 20027  
2009  
November 2010  
November 2010  
1-800-829-1040

Page 1 of 4

JAMES O'HANES  
22 BOULDER STREET  
HANSON, CT 06030-7253

Important information about the Earned Income Credit  
**You may be eligible for a refund of up to \$5,028**

Do not discard or overlook this notice because you may be entitled to some additional money.

Our records show that you may be eligible for a refund called the Earned Income Credit (EIC). You may be able to claim on your 2008 tax return the credit, which can reduce your tax liability or increase your refund. The credit, which can be up to \$5,028, is for people with low to moderate income. You should complete the worksheet on Page 3 to determine if you are eligible for the credit.

**What you need to do**

Complete the Earned Income Credit Worksheet on Page 3.


**If the worksheet confirms that you are eligible for the credit**  
Sign and date the attached worksheet, and mail it to us in the enclosed envelope. Please do not return the worksheet to us.

**If the worksheet indicates that you are not eligible for the credit**  
If you are eligible for the credit, we will send you a self-addressed envelope in 6 to 8 weeks. If you are not eligible, we will send you a self-addressed envelope in 6 to 8 weeks. If you are not eligible, we will use your credit to reduce or pay off those debts.

**Next steps**  
To receive your refund more quickly, write "EIC" on the EIC line of your form 1040. If you qualify for the credit, the IRS will calculate it for you and send you a check.

**Additional information**  
If you need additional assistance, please call 1-800-829-1040, or visit online at [www.irs.gov/eic](http://www.irs.gov/eic). For tax forms, call 1-800-TAX-FORMS (1-800-829-3676). You can also find tax forms and other helpful documents which explain the EIC program in greater detail (e.g., Publication 596) at [www.irs.gov](http://www.irs.gov).

## Panel C2: Low Benefit



Department of the Treasury  
Internal Revenue Service  
Business Opportunity Center  
P.O. Box 10300  
Pawnee, CA 95068-0400

Notice  
Tax Year  
Notice Date  
Notice Period  
To Contact Us

EO 20027  
2009  
November 2010  
November 2010  
1-800-829-1040

Page 1 of 4

JAMES O'HANES  
22 BOULDER STREET  
HANSON, CT 06030-7253

Important information about the Earned Income Credit  
**You may be eligible for a refund of up to \$457**

Do not discard or overlook this notice because you may be entitled to some additional money.

Our records show that you may be eligible for a refund called the Earned Income Credit (EIC). You may be able to claim on your 2008 tax return the credit, which can reduce your tax liability or increase your refund. The credit, which can be up to \$457, is for people with low to moderate income. You should complete the worksheet on Page 3 to determine if you are eligible for the credit.

**What you need to do**

Complete the Earned Income Credit Worksheet on Page 3.

**If the worksheet confirms that you are eligible for the credit**  
Sign and date the attached worksheet, and mail it to us in the enclosed envelope. Please do not return the worksheet to us.

**If the worksheet indicates that you are not eligible for the credit**  
If you are eligible for the credit, we will send you a self-addressed envelope in 6 to 8 weeks. If you are not eligible, we will send you a self-addressed envelope in 6 to 8 weeks. If you are not eligible, we will use your credit to reduce or pay off those debts.

**Next steps**  
To receive your refund more quickly, write "EIC" on the EIC line of your form 1040. If you qualify for the credit, the IRS will calculate it for you and send you a check.

**Additional information**  
If you need additional assistance, please call 1-800-829-1040, or visit online at [www.irs.gov/eic](http://www.irs.gov/eic). For tax forms, call 1-800-TAX-FORMS (1-800-829-3676). You can also find tax forms and other helpful documents which explain the EIC program in greater detail (e.g., Publication 596) at [www.irs.gov](http://www.irs.gov).

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## Panel D1: Administrative Costs (Low)



JAMES O'NEILL  
22 BOULDER STREET  
HANSON, CT 06030-7253

Notice	EO 2002-17
Tax Year	2009
Notice Date	November 2010
Notice Period	12/1/10 to 12/31/10
IRS Address	1500 ...
Tax Center	1500 ...

Page 1 of 4

### Important information about the Earned Income Credit You may be eligible for a refund. Claiming your refund usually takes less than 10 minutes.

Do not discard or overlook this notice. It contains information that may be critical to your tax situation. To claim your benefit, simply complete and return this form. This notice takes less than 10 minutes.

#### Summary

Our records show that you may be eligible for a refund called the Earned Income Credit (EIC), which you did not claim on your 2008 tax form. The credit is for certain people who have worked and have earned income. You should complete the worksheet on Page 3 to determine if you are eligible for the credit.

#### What you need to do

Complete the Earned Income Credit Worksheet on Page 3.  
If the worksheet confirms that you are eligible for the credit, Sign and date the attached worksheet, and mail it to us in the enclosed envelope.  
If the worksheet indicates that you are not eligible for the credit, Please do not return the worksheet to us.

#### Next steps

If you are eligible for the credit, we will send you a refund check in 6 to 8 weeks. If you owe back taxes or other debts, such as child support which we are required to collect, we will use your credit to reduce or pay off those debts.

Next year, to receive your refund more quickly, use "EIC" on the EIC line of your form 1040. If you qualify for the credit, the IRS will calculate it for you and send you a refund.

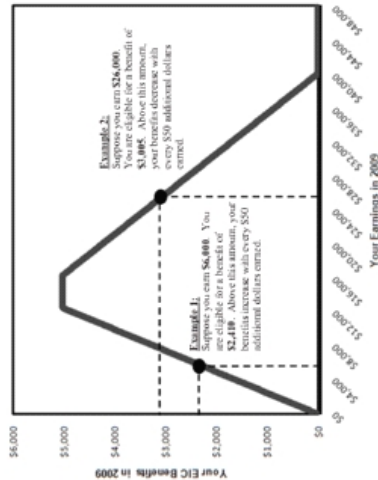
#### Additional Information

If you need additional assistance, please call 1-800-829-1040, or visit online at www.irs.gov/efile. For tax forms, call 1-800-767-4799 (1-800-829-3070). You can also find the forms and other helpful documents which explain the EIC program in greater detail in a Publication 2981 at www.irs.gov.

## Panel D2: InFormational Flyer (2 dependents)

### DO YOU KNOW ABOUT THE EARNED INCOME CREDIT?

The Earned Income Credit (EIC) is designed to provide benefits to working individuals and families. The picture below describes how benefit amounts change based on your yearly earnings (note that the picture is drawn for a single filer with 2 dependents).



### MYTHS AND REALITIES OF THE EIC

- Myth:** I cannot claim the EIC if I exceed the April 15th deadline.  
**Reality:** You can correct your tax return and claim the EIC up to 3 years after the April 15th deadline.
- Myth:** I can only claim the EIC if my income is very low.  
**Reality:** You may qualify for benefits if your earnings are less than \$43,251 if single, and less than \$48,286 if married.
- Myth:** My child is over 18, so I cannot qualify for higher benefits.  
**Reality:** If your child is a full-time student or disabled, you may still qualify for higher benefits.
- Myth:** I need to have a bank account to receive EIC benefits.  
**Reality:** A check for your EIC benefits can be mailed to you.
- Myth:** I cannot claim the EIC if I am unemployed or self-employed.  
**Reality:** You can claim the EIC as long as you have some earned income such as wage income or self-employment income.
- Myth:** I can only claim EIC benefits if I have children.  
**Reality:** You can claim the EIC without children.

## Panel E: Messaged Envelope

