

Psychological Frictions and the Incomplete Take-Up of Social Benefits: Evidence from an IRS Field Experiment[†]

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We address the role of “psychological frictions” in the incomplete take-up of EITC benefits with an IRS field experiment. We specifically assess the influence of program confusion, informational complexity, and stigma by evaluating response to experimental mailings distributed to 35,050 tax filers who failed to claim \$26 million despite an initial notice. While the mere receipt of the mailing, simplification, and the heightened salience of benefits led to substantial additional claiming, attempts to reduce perceived costs of stigma, application, and audits did not. The study, and accompanying surveys, suggests that low program awareness/understanding and informational complexity contribute to the puzzle of low take-up. (JEL C93, D03, H24, M38)

A well-documented, and perhaps surprising, feature of transfers to the economically and socially disadvantaged is that many individuals fail to take-up the benefits for which they are eligible (Currie 2006). The earned income tax credit (EITC), the nation’s largest means-tested cash transfer program, is a prime example, with an estimated incomplete take-up rate of 25 percent, amounting to 6.7 million non-claimants each year (Plueger 2009).¹ The consequences of incomplete take-up can be significant. The typical EITC non-claimant forgoes an estimated \$1,096,

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¹ Throughout the paper we use “incomplete take-up” to describe the failure to fully, or partially, claim a credit by an eligible individual.

equivalent to 33 days of income.² These non-claimants sacrifice other advantages, such as those related to health, education, or consumption, that may be linked to transfers (Hoynes, Miller, and Simon 2015; Dahl and Lochner 2012; Smeeding, Phillips, and O'Connor 2000). The problem of low take-up, according to many accounts, is even more severe for other social programs beyond the EITC such as food stamps, Social Security, and health insurance.

For many policymakers, improving the take-up of means-tested social programs such as the EITC is an unequivocal objective. In speaking of the program in 2007, the acting IRS Commissioner declared that the agency "... wants all eligible taxpayers to claim the EITC"³ However, the rationale for such improvement is often less obvious to economists due to the ambiguous link between higher take-up and welfare. If existing barriers to claiming a credit—such as the time and effort required to learn about, and then apply for, a benefit—discourage applications from those of low economic need, then such barriers may be efficient. On the other hand, if these barriers reduce claiming by those with high need, then policies eliminating such barriers may enhance welfare. Critical for assessing the welfare implications of low take-up is a deeper understanding of why exactly those who are eligible for benefits fail to claim.

Economic models have traditionally recognized three types of costs that might deter take-up: the transaction costs of applying for a benefit, the costs involved with learning about eligibility and application rules, and the stigma associated with enrollment (Currie 2006). Recent work, however, has challenged whether individuals sensibly compare the expected costs and benefits of claiming due to cognitive, motivational, or emotional limits to decision-making. In the context of benefit programs, these limits imply that the failure to claim may be a consequence of low program awareness (e.g., Chetty, Friedman, and Saez 2013; Chetty and Saez 2013; Smeeding, Phillips, and O'Connor 2000), confusion regarding program rules or incentives (e.g., Liebman and Zeckhauser 2004), procrastination (e.g., Madrian and Shea 2001), inattention (e.g., Karlan et al. 2015), or psychological aversion to program complexity or the small "hassles" often involved in claiming (e.g., Bertrand, Mullainathan, and Shafir 2006). As an example of how alleviating a minor procedural hassle can lead to a larger change in behavior than that predicted by economic costs alone, one study documented a significant increase in the take-up of an influenza vaccination when a prompt, asking individuals to note the date of their intended clinical visit, was added to an informational mailer (Milkman et al. 2011).

If existing barriers to claiming deter take-up, particularly among those of high economic need, because of "psychological frictions" associated with low program awareness, confusion, or an aversion to program complexity or hassles, then encouraging take-up by reducing these barriers would likely improve social welfare. In such a scenario, low take-up would reflect a failure of policy to deliver benefits to those who most need them, rather than an optimal use of application ordeals to screen recipients by need. To the extent that policymakers view raising take-up as a

² Estimates of expected benefit size and income for eligible non-claimants are based on author calculations from results reported in Plueger (2009) for tax year (TY) 2005. For the day of work equivalence, we assume 250 work days each year.

³ Statement retrieved in 2014 from <http://www.irs.gov/uac/Paulson,-IRS-Launch-Campaign-to-Help-Low-Income-Taxpayers-Take-Advantage-of-Tax-Credit,-Free-Tax-Help>.

policy objective, clarifying the causes of non-claiming may also provide insight into the design of policies aimed at groups not highly responsive to traditional incentives. Despite the importance of understanding why eligible individuals do not claim, in her seminal review of the topic, Currie (2006) characterized incomplete take-up as a continuing puzzle and advanced experiments as the means to solve it.

In this paper, we report findings from a large policy field experiment, in collaboration with the IRS, designed to investigate the causes of low take-up of the EITC. Our field study focused on a setting where the failure to claim is especially puzzling given that conventional costs of claiming appear to be low and the benefits, for many, are substantial. Specifically, we strategically modified the content and appearance of IRS tax mailings and distributed these to the universe of 35,050 tax filers from California who failed to claim their 2009 Tax Year EITC credit despite presumed eligibility and the receipt of a first reminder notice. Each mailing, consisting of a reminder notice, claiming worksheet, and a return envelope, communicated program eligibility and offered recipients an additional opportunity to claim.

We use the differential response to these mailings to draw inferences about the relative importance of three explanations for non-claiming: the misconstrual of program incentives and/or lack of credit awareness (“Confusion”), the informational complexity of claiming, and program stigma. We define the latter as including both the “social” stigma conventionally discussed by economists, as well as the more identity-driven “personal” stigma recognized by psychologists as potentially important even in the absence of needing to claim the credit in public. To our knowledge, our study represents the first field experiment, conducted with a federal government agency, to investigate the psychological and economic factors that influence program take-up. All told, we informed individuals of \$26 million in unclaimed government benefits, of which about \$4 million was ultimately claimed due to the mailings.

Two features of our setting make it appealing for study. First, because it is a domain where we can precisely target a population of known statutory eligibility, we need not worry that observed increases in enrollment are driven by ineligible applicants. Second, our setting is one in which many of the traditional costs of take-up—transaction costs of claiming, the costs of program learning, and social stigma—are particularly low. Indeed, the mailing provides recipients with a short summary of program and eligibility rules, and claiming a credit requires only that a recipient sign and return a one-page worksheet in a provided stamped envelope. Moreover, social stigma, as it is usually defined, is likely to be minimal. Given that a typical recipient is owed a credit of over \$500 and has an income of about \$14,000, traditional economic models would predict that recipients should claim unless such claiming entails high unobserved costs (e.g., those involving time or stigma), or recipients suffer from the decision-making frictions that our study was designed to test.

Overall, the experiment provides evidence that claiming is sensitive to the frequency, salience, and simplicity with which information is provided. Merely receiving a second opportunity to claim, just months after the receipt of an initial notice led 0.22 of the sample to take-up. Comparing across experimental interventions, simplification, either through a visually more appealing notice, or a shorter worksheet in which select eligibility screens satisfied by all recipients are eliminated, significantly raised take-up from 0.14 (control mailing) to 0.23. Displaying the

generic range of potential benefits in the headline of the simplified notice further improved take-up from 0.23 to 0.31. Intriguingly, the influence of benefit information was not monotonically related to the magnitude of the benefit displayed in the headline which, for some part of the sample, was randomized to show either a medium (\$3,043) or large (\$5,657) amount. Attempts to lower program stigma (social or otherwise), or to inform individuals about the low costs of claiming (i.e., time-costs of filling out the claiming worksheet, or penalties associated with erroneous claiming) did not impact take-up. Finally, an analysis of heterogeneity indicates that simplification disproportionately helped low earners, among those with dependents, and, females, among single filers, while language barriers may have reduced take-up among Hispanic households.

To gain deeper insight into the mechanisms underlying response to the interventions, we conducted a first survey with approximately 3,000 low to moderate income subjects online, many of whom were eligible for the EITC. Participants reviewed one of the experimental interventions, after which we assessed beliefs about program rules, incentives, and stigma. The survey suggests that interventions shaped behavior by influencing beliefs about eligibility and benefit size, and increasing attention paid to forms, but not by reducing perceptions of program stigma or the time and penalty costs of claiming, which respondents judged to be fairly low. Together, the findings from the field study and survey point to the conclusion that confusion, program complexity, and lack of program awareness play a significant role in the failure to take-up, while stigma, and high perceived economic costs of claiming, do not.

The possibility that psychological frictions shape the take-up decision in this setting has implications for welfare and policy. First, so long as the presence of such frictions is not negatively correlated with economic need, low take-up likely reflects a failure to deliver benefits to those who value the benefits most highly. While we cannot directly observe economic need, this interpretation is supported by the fact that the poorest among our sample, a fairly poor group to begin with, were most harmed by the complexity of program mailings. Second, the experimental findings suggest that inexpensive marketing interventions offer a scalable, and potentially more effective, strategy for improving take-up among groups of policy interest than traditional program incentives. Indeed, in our, admittedly unrepresentative, sample, we find a low elasticity of response with respect to benefit size. How might our interventions practically impact overall program take-up? We estimate that the most effective experimental treatments, if applied to the entire population of tax filing non-claimants—approximately 35 percent of all non-claimants overall (Plueger 2009)—could reduce incomplete take-up from 10 percent to 7 percent, among tax filers, and from 25 percent to 22 percent, overall. This would result in an estimated increase in annual disbursements of \$503 million.

While the welfare of the approximately 1.3 million non-claimants who file taxes is of independent policy interest, our experimental sample differs from the broader population of EITC non-claimants across a range of dimensions. Most notably, the recipients of our mailings had two prior opportunities to claim their credit (e.g., at the point of filing, and when they received a first mailed reminder), and, as such, might have especially high unobserved costs of claiming. In comparison to the typical claimant, our sample is owed a smaller average benefit, is less likely to have a qualified dependent, and is less likely to have used a tax preparer (Plueger 2009). To

explore the generalizability of our findings, we report results from a second survey of several hundred low-income subjects from tax preparation clinics who, on several dimensions, more closely resembled the typical EITC eligible individual. The survey assessed program awareness as well as perceptions of program rules, incentives, claiming costs, and stigma.

Jointly, the two surveys we administered, while each subject to its own limits, suggests that a broad population of low-income individuals, including eligible claimants and non-claimants, exhibit low program awareness, and a propensity to underestimate eligibility and benefit size. Respondents do not perceive the EITC to be highly stigmatizing, nor do they perceive the claiming worksheets to be very time-consuming to complete. We interpret the survey evidence as consistent with the possibility that the findings from the experiment extend to eligible non-claimants beyond the experimental sample. Intriguingly, asking survey respondents directly why eligible individuals might not claim a credit, identified several of the same mechanisms implicated in our study—misperceptions of eligibility and confusion about program rules.

Beyond the significance of these results for policy, our findings have implications for the literature on benefit take-up (see Currie 2006 for a review).⁴ The outsized influence of small and largely noninformational changes to program mailings is difficult to explain with economic models in which individuals are assumed to sensibly weigh accurately perceived costs and benefits of claiming. The evidence from this study is instead more consistent with alternative models which not only permit biased beliefs about eligibility and program incentives, but reflect even sharper departures from the standard framework. Such models predict that individuals might avoid or postpone the take-up decision altogether due to psychologically aversive “hassle costs” (Bertrand, Mullainathan, and Shafir 2006), limits to self-control (e.g., O’Donoghue and Rabin 1999) or other cognitive resources (e.g., Mullainathan and Shafir 2013), or because of heuristic-choice strategies of the sort that have been suggested as explaining inefficient health-plan decisions (Ericson and Starc 2012; Bhargava, Loewenstein, and Sydnor 2015).

Our paper additionally builds upon and augments several other literatures including that which investigates how information (e.g., Chetty and Saez 2013; Liebman and Luttmer 2015; Karlan et al. 2015), as well as its salience (e.g., Chetty, Looney, and Kroft 2009; Finkelstein 2009) and complexity (Hastings and Weinstein 2008; Bettinger et al. 2012; Kling et al. 2012; Bhargava, Loewenstein, and Sydnor 2015) affects economic decisions.⁵ We find that the very basic, and consequential, decision of claiming an owed benefit is highly sensitive to the manner, and frequency, with which program information is presented. Methodologically, the closest analogue to our field experiment is a study in which direct mail varying the economic terms and

⁴This 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 1999). More recent research implicates the role of nonmonetary factors on social and private benefit take-up, such as the transparency of information (e.g., Saez 2009; Jones 2010), costs of inconvenience (Ebenstein and Stange 2010), as well as the actions of one’s peers (e.g., Duflo and Saez 2003).

⁵Studies in the latter category 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. 2012), health care choices (Kling et al. 2012; Bhargava, Loewenstein, and Sydnor 2015), and savings/investment decisions (e.g., Beshears et al. 2013; Madrian and Shea 2001; Choi, Laibson, and Madrian 2009).

the informational presentation of loan offers were randomized by a South African lender (Bertrand et al. 2010).

I. Background on EITC and Take-Up

A. Program Structure and Summary

The EITC, (or 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.”⁶ As a result of five subsequent expansions, notably in 1986, and then again in the 1990s, by TY 2009 the EITC distributed \$58 billion in refundable credits to nearly 27 million working people of low to moderate income.

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. Credit eligibility requires a valid Social Security number, 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, benefit size 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 larger credits of up to \$5,667 (figures reflect TY 2009 unless otherwise stated). 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 (see online Appendix Figure A1 for benefit schedules). Individuals in 21 states, as of 2011, could have accrued additional local credits from 3.5 percent to 43 percent of the federal credit.

Critically for the present study, the EITC, unlike other anti-poverty programs, is administered through the tax system. Those with no qualified dependents must file a 1040(A/EZ) and indicate their benefit amount or simply write “EIC” when prompted. In the case of qualified dependents, eligible individuals must file a 1040(A) along with a supplementary, one-page, tax addendum called the Schedule EIC.⁷ 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 percent of adjusted gross income, and amounting to \$2,770 for those with qualified dependents and \$259 for those without). This compares to a typical estimated benefit of \$1,096 (12 percent of adjusted gross income) for non-claimants (calculated from Plueger 2009). Of claimants, 77 percent had at least one qualified child, and only 34 percent of claimants prepared their own taxes. While less is known of non-claimants, estimates suggest that 63 percent had at least one qualified dependent and 56 percent of single filers were female (Plueger 2009).

⁶Quotation cited from a 1975 Senate Committee Report.

⁷Claimants must file a tax return even if they fall below the filing requirement income threshold.

TABLE 1—EITC SUMMARY STATISTICS FOR TY 2009

| Variable name | EITC claimants | | CP notice recipients | | Experimental sample | | |
|--|----------------|-----------|----------------------|----------|---------------------|----------|----------|
| | US mean | CA mean | US mean | CA mean | Mean | Median | SD |
| <i>Panel A. Overall</i> | | | | | | | |
| 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 |
| Benefit w/o qualified dependents | \$2,770 | | | | \$1,870 | \$1,528 | \$1,535 |
| Benefit w/ qualified dependents | \$259 | | | | \$256 | \$262 | \$148 |
| Total EITC paid | \$58.1b | \$6.4b | \$111m | \$13.0m | \$4.0m | — | — |
| <i>Panel B. Descriptive and tax variables (all sample)</i> | | | | | | | |
| Descriptive variables | | | | | | | |
| 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 dependents | 0.77 | 0.76 | 0.24 | — | 0.33 | — | — |
| Tax variables | | | | | | | |
| 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 | — | — |
| <i>Panel C. Descriptive and tax variables (claimants only)</i> | | | | | | | |
| Descriptive variables | | | | | | | |
| 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 dependents | 0.77 | 0.76 | 0.14 | 0.14 | 0.21 | — | — |
| Tax variables | | | | | | | |
| 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.

B. 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 presence of unobservable factors that determine eligibility, such as qualified dependent status, and because one cannot simply assume that eligible non-claimants and claimants, even conditioned on observable characteristics, are otherwise similar (Berube 2006).

An analysis by the IRS based on data for TY 2005, which informs assumptions used in this study, suggests an overall program take-up rate of 75 percent (with a confidence interval of 73 percent to 77 percent), including 56 percent for those without qualified dependents and 81 percent for those with at least one such dependent (Plueger 2009).⁸ After accounting for changes in program eligibility over time, namely the expansion of the credit to those without eligible dependents, Plueger's estimate is similar to that of Scholz (1994), whose take-up estimate of 80 percent to 86 percent (TY 1990), is commonly cited by academics (1994).⁹ Plueger estimates that of the 25 percent who do not take-up, 16 percent do not file taxes while 9 percent file taxes but fail to claim a benefit on their return, implying an overall rate of take-up among eligible tax filers of 90 percent. Take-up appears to further vary across demographic and tax characteristics with generally lower take-up for men, and those with low income and education (e.g., Blumenthal, Erard, and Ho 2005). The participation rate in the EITC compares favorably with other major transfer programs which has been estimated at 42 percent in Temporary Assistance for Needy Families (TANF), 55 percent in the Supplemental Nutrition Assistance Program (SNAP), and 46 percent in Supplemental Security Income (SSI).¹⁰

The IRS mails reminder notices and claiming worksheets—the “CP09” is sent to those with dependents, and the “CP27” is sent to those without—to anyone who files a tax return and neglects to claim their EITC credit despite appearing eligible based on administrative screens such as filing status, age, earned income, investment income, and foreign income.¹¹ However, Plueger (2009) points out that the filters may also screen out some fraction of eligible filing non-claimants.¹² CP 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

⁸ 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.

⁹ As Plueger (2009) notes, the Scholz (1994) analysis was both 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 with presumably lower take-up).

¹⁰ 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>.

¹¹ “CP” refers to “Computer Paragraph” and denotes the varied missives that the IRS routinely sends to taxpayers after a tax filing.

¹² See Plueger (2009) for a discussion of the divide between eligible filing non-claimants and those receiving the CP notification, and specifically Table 10 of Plueger (2009) for an accounting of nationwide filing non-claimants for TY 2005. In brief, some filing non-claimants do not receive a CP reminder notice due to a variety of factors including the exclusion of various filing groups (e.g., taxpayers who file electronically but print and mail their returns, or returns submitted after April 15th may not generate a notice), and a policy designed to avoid missives to anyone with ambiguous eligibility (e.g., taxpayers with dependent children older than 18 whose school enrollment status cannot be verified). We obtained further details of this accounting from interviews with D. Plueger (August 2011).

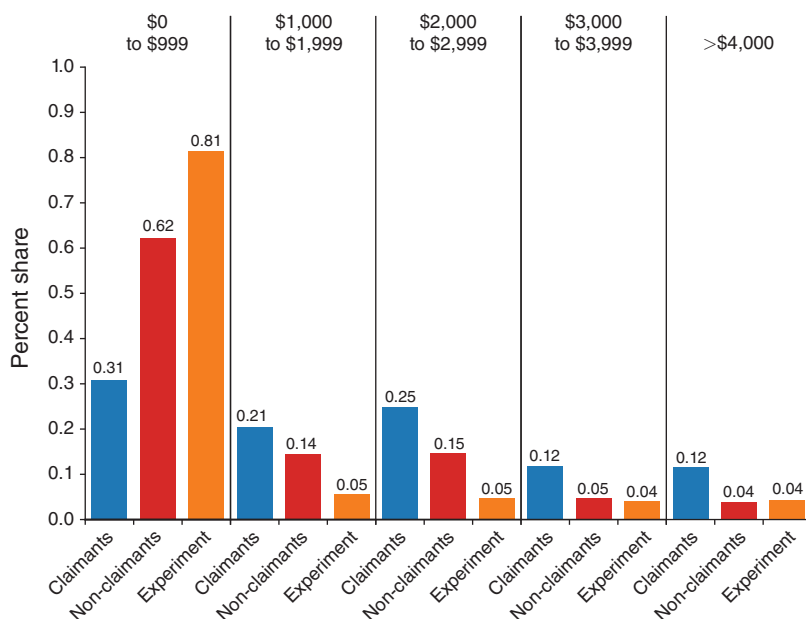


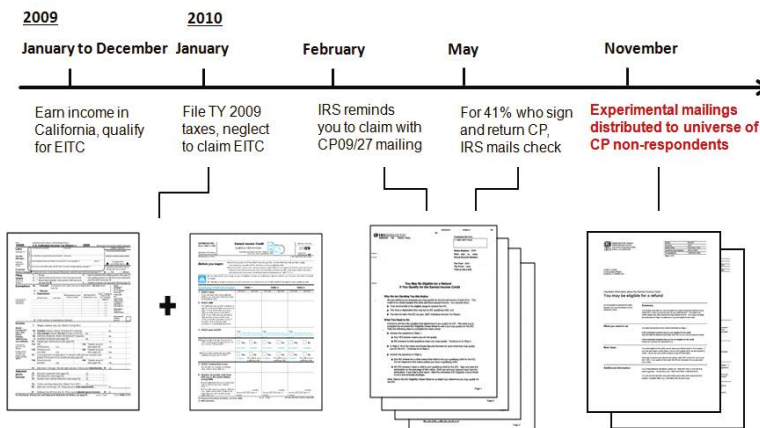
FIGURE 1. BENEFIT DISTRIBUTION FOR EITC CLAIMANTS AND ELIGIBLE NON-CLAIMANTS

Notes: This figure compares the distribution of EITC benefits for claimants, eligible non-claimants, and the experimental sample. Data for the former two groups is for TY 2005 and is estimated from Plueger (2009), while the experimental data is for TY 2009.

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 has ranged from 41 percent to 52 percent nationally for TYs 2006 to 2009.¹³ The experimental sample, discussed below, comprises those who failed to respond to a first CP mailing. Table 1 suggests that the experimental sample, in comparison with EITC claimants more generally, were characterized by a lower average EITC benefit (\$511 versus \$2,185). This difference was due to a lower average benefit for those with dependents (\$1,870 versus \$2,770) and a lower share of such claimants (33 percent versus 77 percent), but not by a significant difference in benefit for those without dependents (\$256 versus \$259). Experimental subjects also had a lower average adjusted gross income (\$15,852 versus \$17,002), and were more likely to have self-prepared their returns (62 percent versus 34 percent) than claimants overall. Figure 1 plots the distribution of expected benefits for EITC claimants and non-claimants, estimated from Plueger (2009), as well as for the experimental sample.

¹³ Author calculations from internal statistics from the IRS.

Panel A. Screening timeline for experimental sample



Panel B. Organization of experimental treatments by mailing component

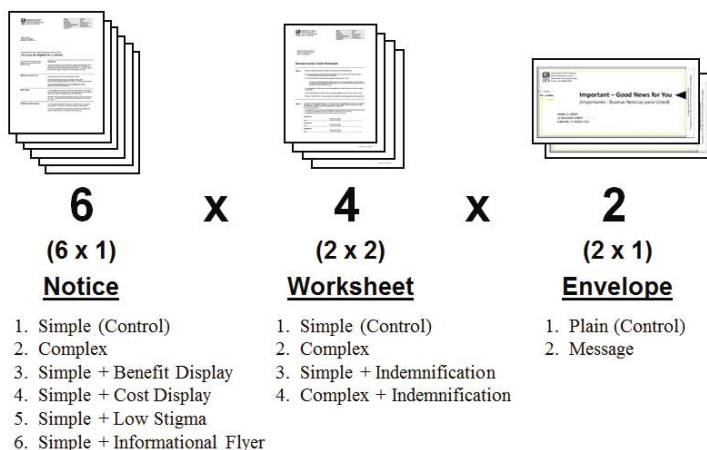


FIGURE 2. SAMPLE SCREEN AND PHYSICAL ORGANIZATION OF EXPERIMENTAL TREATMENTS

II. Research Design

A. Experimental Sample

The sample for the field experiment consists of individuals from California who satisfy the following conditions.¹⁴ First, the taxpayer filed a tax return for TY 2009 but failed to claim an EITC credit. Second, the taxpayer satisfied a set of eligibility screens, enumerated above, that resulted in the receipt of a CP09 or CP27, and finally, the taxpayer neglected to respond to this CP notice. Figure 2 depicts the set of screens that led to the experimental sample (panel A), while Table 2 describes

¹⁴The choice of California as a setting for the study was dictated to us by the IRS.

TABLE 2—STEP-BY-STEP ACCOUNTING TO GENERATE EXPERIMENTAL SAMPLE

| Incremental steps | Eliminated returns | | Remaining returns | | Notes and assumptions |
|-------------------------------|--------------------|---------------|-------------------|----------------|---|
| | Step share | Overall share | Overall share | Overall number | |
| Start: Total CA EITC Eligible | — | — | 1.00 | 3m | 3.0m filed; 26% noncompliance (TIGTA 2011, TY 2009); 25% incomplete take-up (Plueger 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 did not file taxes (Plueger 2009, TY 2005) |
| 3. Did not receive CP 09/27 | 0.75 | 0.06 | 0.03 | 76,440 | ~75% may not have received CP (Plueger 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 | 35,050 | Exclude 2,953 due to mistagging of dependents, and 7,096 due to incorrect address |
| End: experimental sample | | | | 35,050 | |

Notes: This table traces the generation of the experimental sample from an estimate of all EITC eligibles in California for TY 2009. Bolded figures indicate exact figures. Remaining figures are estimated or inferred. Noncompliance estimate assumes that all overclaiming is on the extensive margin (i.e., is by ineligible individuals).

Sources: The source for the noncompliance estimate (TIGTA 2011) is the report titled *Treasury Inspector General for Tax Administration*, Ref. No. 2011-40-023. Filing and CP statistics are either from the IRS website or from internal IRS documents.

the step-wise exclusions that generated the sample from the approximately 3.0 million individuals eligible for the EITC in California for TY 2009 (figures in bold are exact). Of those eligible, an estimated 263,000 filed taxes but did not claim the EITC, and 76,440 received a reminder notice indicating a possible unclaimed benefit of which 45,099 taxpayers failed to respond. A further 7,096 individuals were excluded by the IRS, in part, because of an incorrect mailing address, and 2,953 were excluded due to an inaccurate inference regarding the number of dependents during the randomization stage.¹⁵ The experimental sample featured the 35,050 remaining individuals: 23,618 with no dependents, and 11,432 with at least one dependent.

B. Experimental Conditions

Structure of Mailings.—Subjects in the experiment were either sent a control or one of several treatment mailings. Mailings consisted of three physical components: a one page, two-sided notice; a one-page, two-sided eligibility worksheet, and an envelope in which the notice and worksheet were contained.¹⁶ The notice informed the recipient of possible program eligibility, briefly explained the purpose of the

¹⁵ During the randomization when interventions were assigned to each anonymized taxpayer, our inference of dependents relied on the presence of a child Social Security number. 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.

¹⁶ Each mailing also included an addressed, stamped envelope so that the recipient could return the worksheet. This did not vary across any of the mailings.

TABLE 3—EXPERIMENTAL INTERVENTIONS BY MECHANISM

| Mechanism | Intervention | Description | Sample |
|------------------------------|------------------------------------|---|--------|
| <i>Complexity</i> | | | |
| Complexity (design) | 1. Complex notice | Relative to simple notice, complex notice is two pages, features denser textual layout, and repeats eligibility information included in the worksheet | 3,676 |
| Complexity (length) | 2. Complex worksheet | Relative to simple worksheet, complex worksheet includes additional, nondiscriminatory, questions regarding eligibility | 10,979 |
| <i>Program information</i> | | | |
| Benefit and cost information | 1. Benefit display (low and high) | Simple notice reports upper bound of potential benefit (up to “\$457,” “\$3,043,” “\$5,057,” or “\$5,567”) | 6,761 |
| | 2. Transaction cost (low and high) | Simple notice provides guidance as to worksheet completion time (less than 10 or 60 minutes) | 3,475 |
| Penalty/audit information | 1. Indemnification message | Bold message on worksheet indemnifies against penalty for unintentional error | 17,027 |
| General program information | 1. Envelope message | Envelope message indicates that enclosure communicates “good news” | 17,044 |
| | 2. Informational flyer | One page flyer offers program information and trapezoidal benefit schedule | 4,019 |
| <i>Stigma</i> | | | |
| Personal stigma reduction | 1. Emphasis on earned income | Simple notice emphasizes that credit is earned reward for hard work | 1,844 |
| Social stigma reduction | 2. Social influence | Simple notice communicates that similarly situated peers are also claiming | 1,753 |

program, directed recipients to verify eligibility via the accompanying worksheet, and offered instructions for additional assistance. The eligibility worksheet featured a series of eligibility screening statements (e.g., “My Social Security card reads ‘Not Valid for Employment’ ...”). For those with children, the worksheet additionally asked recipients to report each child’s name and Social Security number. Eligible recipients were asked to sign, date, and return the last page of the worksheet. Finally, the notice and worksheet were enclosed in a standard number-10 sized envelope (4.125 inches \times 9.5 inches). Figure 2 summarizes the treatment conditions by physical component (panel B). Table 3 organizes the interventions by tested mechanisms. Selected examples of notices, worksheets, and the envelope are depicted in the Appendix.

Control Condition (Simplicity Interventions).—We created the control mailing by simplifying the initial CP 09/27 notice and worksheet that subjects received just months earlier. While the initial notice was a textually dense, two-sided document that emphasized eligibility requirements repeated later in the worksheet, the new notice was single-sided, featured a larger and more readable font (Frutiger), a prominent headline, and did not repeat eligibility information (“simple notice,” Appendix Figure, panel A1). Similarly, we redesigned the worksheet from the original CP notice by eliminating repetition, changing the font, and using a cleaner layout. The

resulting single page worksheet (two-sided for those with dependents) carries a similar design aesthetic to the simplified notice (“simple worksheet,” Appendix Figure, panel B1).¹⁷

Complexity Interventions.—An initial set of interventions tests whether informational complexity affects take-up. We manipulated complexity via two interventions. Our first intervention, the “complex notice,” was the original CP 09/27 notice that subjects received earlier but with minor changes to standardize information across conditions (Appendix Figure, panel A2). We expected that the difference in response between the control notice (i.e., simple notice) and the complex notice would indicate the role of design and text simplicity in shaping response.

Second, we test whether a modest increase in perceived worksheet complexity—through an additional set of eligibility statements—would lower take-up (“complex worksheet,” Appendix Figure, panel B2). Critically, the additional questions pertained to EITC eligibility criteria which, by our observation of tax records, our recipients had satisfied. Specifically, in Step 1 of the worksheet, we presented additional screens for earned income, foreign earned income, investment income, citizenship, and filing status. For those with no dependents, the complex worksheet featured a new section that elicited more detailed information on earned income for the recent tax year. We expected that the difference in response between the control (i.e., simplified) notice and the complex notice would indicate the role of worksheet length in shaping response.

Information Interventions.—A second set of interventions was designed to test whether information regarding program existence, eligibility, and the costs and benefits of claiming influenced take-up. First, we investigate 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 control notice (“benefit display”). Subjects in this treatment arm received a notice indicating eligibility for a benefit “... of up to \$457” in the case of no dependents and “... of up to \$5,657” in the case of three or more dependents. In order to generate variation in the magnitude of perceived benefits, for subjects in this treatment with either one or two dependents, we additionally randomized the amount reported to either reflect the maximum dependent specific benefit (i.e., \$3,043 for one dependent, and \$5,028 for two dependents) or for the program as a whole (i.e., \$5,657) (Appendix Figure, panels C1 and C2).

Second, we explore how perceptions of transaction costs affected response by offering varying guidance as to the time required to complete and return the eligibility worksheet (“transaction cost display”). That is, we communicated in the notice headline that worksheet completion required “...less than 60[10] minutes” where the specific magnitude, (i.e., 60 or 10), was again randomized among those assigned to this treatment (Appendix Figure, panel D1). Third, we test the importance of perceived penalty costs (e.g., those relating to a possible audit) by assuring recipients, with bold lettering displayed above one-half of worksheet headlines, that mistakenly reporting incorrect information would not result in a penalty (“indemnification

¹⁷The simplified notice is adapted from a layout originally designed by a third party firm retained by the IRS and pretested for “readability” in a test lab.

message”): “Complete to the best of your ability—you will NOT be penalized for unintentional errors.” (Appendix Figure, panel D2).

Fourth, to test the influence of general program information on response, in one condition, we attached a one-page flyer, adapted from that used by Chetty and Saez (2013), to baseline notices. The “informational flyer” displayed 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 contained a section enumerating program “myths and realities” intended to clarify potentially confusing aspects of eligibility requirements (e.g., “I need to have a bank account to receive EIC benefits”) (Appendix Figure, panel E1).

Finally, to assess whether inattention to the mailed information meaningfully contributed to nonresponse, we displayed a prominent envelope message for the treatment group, relative to an unmarked envelope control, indicating that the enclosed contents may benefit the recipient: “Important—Good News for You” (“envelope message,” Appendix Figure, panel E2). By IRS request, the treatment envelopes also included a parenthetical Spanish translation of the message.¹⁸

Stigma Interventions.—A final set of interventions tests whether program stigma influences response. While early economic models of take-up featured the costs of social stigma (Moffitt 1983), psychologists and recent economic research has made the distinction between social stigma, and the related construct of personal (or identity-driven) stigma (e.g., Crocker, Major, and Steele 1998; Manchester and Mumford 2010). The latter occurs when an individual internalizes existing negative beliefs or stereotypes that others hold toward the stigmatized target. We test the sensitivity of response to personal stigma by modifying the notice headline to emphasize that the benefit was an earned consequence of hard work rather than a welfare transfer: “You may have earned a refund due to your many hours of employment.” A second headline tests for the role of social stigma by invoking a stigma-reducing, descriptive social norm: “Usually, four out of every five people claim their refund” (e.g., Cialdini 1989; Cialdini and Goldstein 2004).

C. Experimental Randomization

We assigned subjects to a notice (including a condition with the control notice plus the informational flyer), worksheet, and envelope with three independent randomized assignments. Conditioned on assignment to a notice displaying benefits (with at least 1 dependent), stigma, or claiming cost, we subsequently randomized recipients into one of the sub-treatment variations. All randomizations were conducted within blocks defined by zip code and the presence of eligible dependents yielding a total of 3,483 blocks. In this way, our blocking design was intended to

¹⁸Due to IRS rules governing messaging outside the envelope, we had little latitude in choosing the precise verbiage. We attempt to disentangle the effects of including Spanish language from the envelope messaging indirectly by examining differential responses for subpopulations in the sample that vary in the inferred presence of Spanish speaking households.

minimize experimental variance and produce more efficient estimates than a simple randomization. Treatments were randomized with equal sample weights with three exceptions: The control notice was over-sampled ($\times 4$) to heighten the statistical power for pair-wise comparisons; the benefit display notices were over-sampled ($\times 3$) to power tests of differentiation across listed benefit amounts; finally, at the behest of the IRS, the lengthier complex worksheet was limited to 25 percent of the sample (Table 3 reports sample sizes by intervention). Balancing tests, implemented through a series of regressions, ensure that the treatment samples were similar across key observables such as earned income, adjusted gross income, benefit size, filing status, and past EITC claiming behavior (online Appendix Table A3).¹⁹

D. Survey Instruments

We supplement the field experiment with two large-scale surveys of low to moderate income samples. A first survey was designed to offer a detailed psychometric assessment of how exposure to one of the experimental notices or worksheets altered beliefs regarding the costs—associated with application, stigma, and potential audits—and benefits of claiming. The approximately 10 minute survey was administered to 2,800 subjects online through Amazon Mechanical Turk in the summer of 2011. Subjects in the sample were diverse across gender (62 percent female, 38 percent male), age (median age 27, standard deviation: 11), education (48 percent college, 98 percent high school), earned income (median: $\sim \$24,000$, standard deviation: $\sim \$30,000$), employment status (employed: 60 percent, unemployed at time of survey: 18 percent, student: 17 percent, other: 5 percent), and inferred EITC eligibility (~ 38 percent eligible).

A first segment of the survey elicited basic income and demographic detail which permitted inference of EITC eligibility and estimate benefit size. A second segment of the survey presented respondents with one of the experimental notices and/or worksheets after which respondents were asked about their understanding of program rules, beliefs regarding eligibility, and perceptions of benefit size and a range of claiming costs. Each version of the survey, to which respondents were randomly assigned, featured a distinct experimental mailing (not all conditions were tested due to sample constraints), so that we could attribute differences in program perceptions and beliefs to differences in the content of the interventions. Specifically, respondents were asked to indicate perceptions of program complexity (1 to 100 scale), the carefulness with which they read the information (1 to 100 scale), intent to complete and return the form (yes/no), willingness to pay a preparer to assist in completing the forms (in dollars), and respect for those who decided to claim the credit (1 to 100 scale), and were tested on their comprehension of program information. The survey was distinguished by a near absence of item nonresponse due to built-in forced response mechanisms. A second, paper survey, was administered

¹⁹We implement the balancing tests with individual-level regressions of the following form: $Outcome_{nwe} = \alpha + \varphi_n + \gamma_w + \theta_e + \varepsilon_{nwe}$. 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. Overall, the analysis reported in the table suggests that the treatments were successfully randomized (online Appendix Table A3).

TABLE 4—SUMMARY OF RESPONSE FOR EXPERIMENTAL MAILINGS

| | All sample | | | No dependents | | With dependents | |
|---|------------|--------------|------|---------------|--------------|-----------------|--------------|
| | Response | Benefit size | Deny | Response | Benefit size | Response | Benefit size |
| CP Notice (CA TY 2009) | 0.41 | \$570 | 0.02 | — | — | — | — |
| Overall response | 0.22 | \$511 | 0.01 | 0.25 | \$247 | 0.16 | \$1,531 |
| Simple notice + simple worksheet (Control) | 0.23 | \$514 | 0.01 | 0.27 | \$246 | 0.16 | \$1,616 |
| Complex notice + complex worksheet | 0.14 | \$546 | 0.01 | 0.17 | \$294 | 0.10 | \$1,570 |
| Information notice + simple worksheet | 0.28 | \$531 | 0.01 | 0.31 | \$242 | 0.21 | \$1,643 |
| Stigma notice + simple worksheet | 0.22 | \$452 | 0.01 | 0.25 | \$255 | 0.14 | \$1,330 |
| Predicted language neutral response | 0.25 | \$530 | 0.01 | 0.26 | \$245 | 0.21 | \$1,638 |

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.

in-person to 1139 clients at several low-income tax clinics primarily in Chicago from February to April 2011.²⁰ The survey, which appeared to take about 15 to 25 minutes to complete during an “intake” period when clients waited for a tax preparer, was designed to measure baseline levels of program awareness and literacy in a population beyond the experimental sample. Subjects again reflected a diverse range of gender (56 percent female, 44 percent male), age (median: 44 years, standard deviation: 16), earned income (median: ~\$13,000, standard deviation: ~\$11,000), and education (30 percent college, 90 percent high school). Of the sample, 65 percent of subjects were deemed eligible for the EITC of which 60 percent were female, 41 percent had qualified dependents, and median income was approximately \$9,000. Credit eligible respondents resembled overall EITC claimants more closely than the experimental sample in gender and the presence of dependents, and, of course, nearly all used a tax preparer. Like the first survey, the second survey elicited income and demographic detail, and also gauged program awareness, beliefs of eligibility and benefit size, and perceptions of the various costs of claiming.

III. Results

A. Overall Response

Table 4 reports a first key result of the field experiment: the magnitude of the overall response to a mailed notification. 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). Relative to the response to the initial CP notice of 0.41, the experimental treatments augmented response by 32 percent (i.e., $[0.22 \times (1 - 0.41)]/0.41$). The additional response is not associated

²⁰The 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.

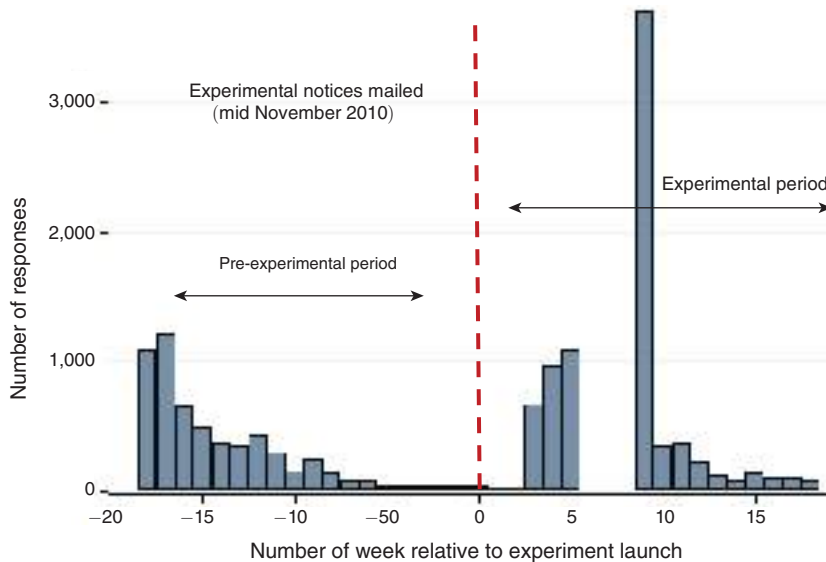


FIGURE 3. EITC CLAIMS PROCESSED BEFORE AND AFTER MAILING OF EXPERIMENTAL NOTICES
(California CP notice recipients)

Notes: This figure depicts the number of TY 2009 EITC claiming worksheets, from California, processed by the IRS, each week from July 2010 to March 2011.

Source: Data was provided to the authors from the IRS.

with a significant increase in denied claims.²¹ The estimated benefit size for nonrespondents was \$788, including \$247 for those without dependents, and \$1,787 for those with, suggesting that response was not driven by the magnitude of anticipated benefits. Figure 3, which plots the IRS processing date for returned worksheets—including response to the initial CP mailings as well as the experimental notices—indicating that the patterns summarized by Table 4 are almost certainly due to receipt of the experimental notices rather than delayed response to older notices.²²

Beyond overall response, the table compares the 0.23 response rate associated with the control condition—that is the mailing with the simple notice and worksheet—with the average response to mailings in each of the three treatment categories (aggregating across the plain and messaged envelopes, and worksheets with and without indemnification messages). The comparison suggests a large net positive effect of simplification on response (from 0.14 to 0.23), as well as of information (from 0.23 to 0.28), but not of the attempts to reduce stigma (from 0.23 to 0.22). These treatment effects are roughly similar for those with and without dependents. 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? While some of the

²¹ A mailed claim is rarely denied, likely because the sample was prescreened for statutory eligibility. Such a denial might arise if the notice recipient filed an amended return which altered eligibility after the CP notification had been triggered, or if a qualified dependent was claimed by another party and such a claim altered the recipient's eligibility.

²² According to interviews with the IRS, there was a period in early January, 5 to 8 weeks after we mailed the interventions, when the IRS did not process EITC claims.

additional response appears due to the modifications reflected in specific interventions, the complex mailing (notice and worksheet), arguably the closest analogue to the initial mailing received by recipients, still resulted in a response of 0.14.²³ One explanation as to why second exposure to the same information raised take-up is that the experimental mailings helped to combat low program awareness, inattention, or forgetfulness among recipients. Consistent with this explanation, in a subsequent section, we discuss survey evidence indicating low program awareness among those eligible for the EITC. Another alternative is that the receipt of the second notice may have caused recipients to adjust inferences regarding eligibility or some other program parameter. Finally, a small share of the response may be attributable to lost or unopened mail that is, at least partially, stochastic in nature.²⁴

B. Response to Experimental Treatments

We summarize the effects of the individual interventions on response, as well as denied claims, in Table 5. The first column depicts treatment effects from a linear probability model estimated as follows:

$$\Pr(\text{Response}_i = 1) = \alpha + \sum \theta^j \text{Notice}_i^j + \sum \partial^k \text{Worksheet}_i^k + \ell \text{Env}_i + \pi \text{Dep}_i + e_i,$$

where indicator variables denoting experimental notice $j(\text{Notice}_i^j)$, worksheet $k(\text{Worksheet}_i^k)$, and the presence of a messaged envelope (Env_i), predict an individual, i 's, binary response, Response_i . To permit clear pair-wise comparisons, effects are estimated relative to the excluded control condition (i.e., simple notice, simple worksheet, and the plain envelope). A dummy variable, Dep_i , controls for the presence of dependents. We report the change in response relative to the control mailing 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 additional controls speaks to the success of the randomization. We exclude controls, apart from the variable indicating the presence of dependents, in the subsequent analyses. Columns 3 and 4 report the estimated model, without the dummy variable, for the sample with and without dependents while the following column reports p -values testing for coefficient equality across the two groups (estimated from a separate set of pooled regressions with an interaction term). The final two columns provide evidence that any disproportionate increase in denied claims, due to the interventions, are too modest to account for the overall pattern of response. Figure 4 summarizes treatment effects graphically, with confidence intervals, as calculated from column 1. While the comparisons summarized in the table were all preplanned, we note that the five strongly significant interventions reported in the first column survive a Bonferroni correction for multiple comparisons at a family-wise alpha of 0.05.

²³ Importantly, none of the interventions in our study precisely duplicated the initial mailing received by recipients. The complex notice was a near duplicate of the initial notice, and the complex worksheet featured more screening questions than the initial worksheet but had a simpler design.

²⁴ We were unable to obtain information on the rate of returned mail for either the initial notice or the experimental mailings.

TABLE 5—RESPONSE AND DENIAL BY EXPERIMENTAL INTERVENTION

| | Dependent variable: (LPM) | | | | | | |
|--|--------------------------------|--------------------------------|--------------------------------|--------------------------------|------------------------|---------------------|---------------------|
| | Response (yes/no) | | | | | Denial (yes/no) | |
| | Full sample (1) | Controls (2) | No dependents (3) | with dependents (4) | <i>p</i> -value (5) | Full sample (6) | Controls (7) |
| [Simple notice, simple worksheet, plain envelope: excluded] | | | | | | | |
| <i>Complexity</i> | | | | | | | |
| Complex notice | −0.061*** (0.007) [−27%] | −0.060*** (0.007) [−26%] | −0.062*** (0.009) [−23%] | −0.060*** (0.010) [−38%] | <i>p</i> = 0.90 | −0.0014 (0.002) | −0.0014 (0.002) |
| Complex worksheet | −0.040*** (0.005) [−17%] | −0.040*** (0.005) [−17%] | −0.054*** (0.006) [−20%] | −0.012 (0.008) [−8%] | <i>p</i> < 0.01 | −0.0011 (0.001) | −0.0012 (0.001) |
| <i>Program information</i> | | | | | | | |
| Benefit display | 0.077*** (0.007) [+33%] | 0.078*** (0.006) [+34%] | 0.082*** (0.008) [+30%] | 0.066*** (0.010) [+41%] | <i>p</i> = 0.23 | 0.0035** (0.001) | 0.0033** (0.001) |
| Transaction cost display | −0.013* (0.008) [−6%] | −0.015* (0.008) [−6%] | −0.015 (0.010) [−6%] | −0.008 (0.012) [−5%] | <i>p</i> = 0.67 | 0.0025 (0.002) | 0.0027 (0.002) |
| Indemnification message | 0.004 (0.004) [+2%] | 0.005 (0.004) [+2%] | 0.003 (0.006) [+1%] | 0.007 (0.007) [+4%] | <i>p</i> = 0.71 | 0.0010 (0.001) | 0.0010 (0.001) |
| Informational flyer | −0.036*** (0.007) [−16%] | −0.036*** (0.007) [−16%] | −0.045*** (0.009) [−17%] | −0.018 (0.011) [−11%] | <i>p</i> = 0.07 | 0.0001 (0.002) | 0.0001 (0.002) |
| Envelope message | −0.007 (0.004) [−3%] | −0.006 (0.004) [−3%] | −0.009* (0.005) [−3%] | −0.001 (0.007) [−1%] | <i>p</i> = 0.37 | −0.0005 (0.001) | −0.0005 (0.001) |
| <i>Stigma</i> | | | | | | | |
| Personal stigma reduction | −0.007 (0.010) [−3%] | −0.009 (0.010) [−4%] | −0.011 (0.013) [−4%] | 0.001 (0.016) [+1%] | <i>p</i> = 0.57 | 0.0033 (0.003) | 0.0035 (0.003) |
| Social stigma reduction | −0.042*** (0.010) [−18%] | −0.042*** (0.010) [−18%] | −0.045*** (0.013) [−17%] | −0.037** (0.015) [−23%] | <i>p</i> = 0.67 | −0.0023 (0.002) | −0.0021 (0.002) |
| Dummy variable for dependents | X | X | | | | X | X |
| Controls | | X | | | | | X |
| Observations | 35,050 | 35,050 | 23,618 | 11,432 | | 35,050 | 35,050 |
| <i>R</i> ² | 0.02 | 0.04 | 0.01 | 0.01 | | 0.02 | 0.03 |
| Response/deny rate for control (simple <i>N</i> + <i>WS</i>) | 0.23 | 0.23 | 0.27 | 0.16 | | 0.01 | 0.01 |
| <i>p</i> -value of <i>F</i> -test: complexity | 0.00 | 0.00 | 0.00 | 0.00 | | 0.19 | 0.18 |
| <i>p</i> -value of <i>F</i> -test: program intervention | 0.11 | 0.11 | 0.42 | 0.09 | | 0.10 | 0.09 |
| <i>p</i> -value of <i>F</i> -test: stigma | 0.00 | 0.00 | 0.00 | 0.12 | | 0.77 | 0.69 |

Notes: This table summarizes the marginal treatment effects on response and denial estimated from a linear probability model. 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 the response rate of the simple mailing (i.e., the control) 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 with standard errors clustered at each zip code.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

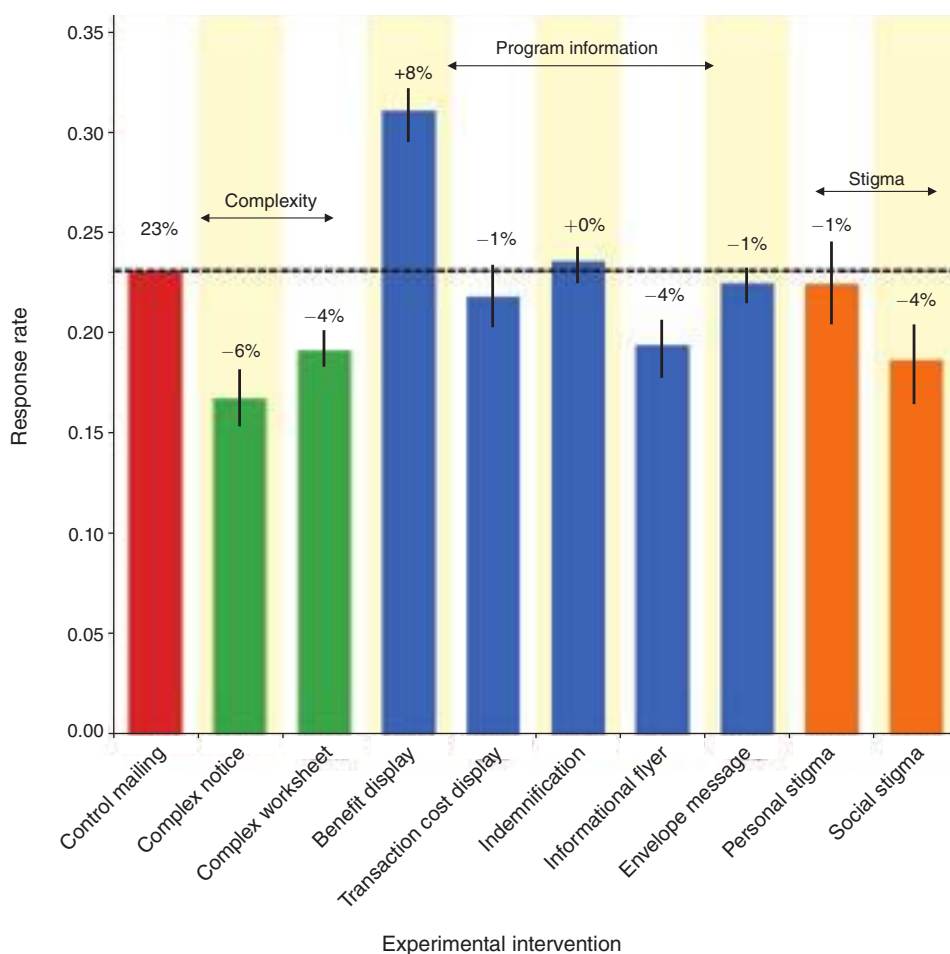


FIGURE 4. RESPONSE AND MARGINAL EFFECTS BY EXPERIMENTAL INTERVENTION

Notes: This figure depicts the response rates, and marginal treatment effects, associated with experimental interventions using estimates reported in column 1 of Table 4. The “Control mailing” refers to the simple notice and simple worksheet and reflects response averaged across the envelope and indemnity treatments.

Complexity Interventions.—The first set of interventions, as depicted in Figure 4, indicates the stark effect of informational complexity on response. The complexity notice decreased response by 0.06 ($p < 0.01$), or 27 percent, relative to the 0.23 response of the control mailing, and the effect magnitude, in absolute terms, did not differ significantly across dependent status. The lengthened worksheet lowered response by 0.04 ($p < 0.01$) or 17 percent. The effect of worksheet complexity appears to be driven largely by those without dependents possibly because the treatment worksheet for this population is substantially “stronger” (due to the additional section of questions) than the same intervention for those with dependents. A separate estimate of the interaction of the two conditions reveals that the joint presence of both complexity elements reduced response by 0.09 ($p < 0.01$).

TABLE 6—PSYCHOMETRIC ASSESSMENT OF EXPERIMENTAL INTERVENTIONS

| Intervention | Complexity | Attention paid | | Inferences of program benefits and costs | | | | |
|--|--------------------------------------|---------------------------------------|------------------------------|--|---------------------------------------|-------------------|--------------------------------|------------------------------|
| | Subjective complexity rating (0–100) | Carefully read? (self-report) (0–100) | Comprehension question (1,0) | Perceived eligibility % (0–100) | Expected benefit size (ln \$, if > 0) | WTP preparer (\$) | Respect for claimants? (0–100) | Perceived audit rate (0–100) |
| [Simple notice, simple worksheet: excluded] | | | | | | | | |
| <i>Panel A. Complexity</i> | | | | | | | | |
| Complex notice | 6.12*** (1.926) | –4.31** (2.025) | –0.18*** (0.036) | –0.3 (2.9) | 0.08 (0.08) | –2.42 (3.77) | –1.02 (1.75) | 1.44 (1.88) |
| Complex worksheet | 0.36 (1.925) | –1.26 (2.023) | –0.03 (0.036) | –6.99** (2.9) | 0.11 (0.08) | –1.32 (3.77) | –0.81 (1.75) | –0.22 (1.88) |
| <i>Panel B. Program information (selected interventions)</i> | | | | | | | | |
| \$457 benefit display | –2.77 (4.152) | 2.179 (3.989) | 0.09 (0.077) | 24.0*** (6.3) | 0.265 (0.18) | 8.24 (8.64) | 13.62*** (3.80) | 4.83 (3.85) |
| \$3,043 benefit display | 7.01** (3.011) | –4.896* (2.893) | –0.066 (0.056) | 2.0 (4.5) | 0.981*** (0.13) | 1.46 (6.26) | 1.89 (2.75) | 3.59 (2.79) |
| \$5,000 benefit display | 5.30* (2.717) | –3.159 (2.611) | –0.051 (0.051) | 5.3 (4.1) | 1.043*** (0.12) | 6.79 (5.65) | 3.07 (2.48) | –0.19 (2.52) |
| 10 minute cost display | 0.07 (3.666) | –1.742 (3.522) | –0.11 (0.069) | –4.3 (5.5) | –0.023 (0.16) | 14.04* (7.65) | 0.58 (3.36) | –3.29 (3.41) |
| Flyer | 3.58** (1.421) | 2.857* (1.577) | –0.048* (0.026) | 3.4 (2.1) | –0.357*** (0.05) | –0.55 (2.88) | 0.09 (1.24) | –0.78 (1.37) |
| <i>Panel C. Stigma (selected interventions)</i> | | | | | | | | |
| Social influence | 6.07 (3.751) | –1.924 (3.604) | –0.042 (0.070) | 2.6 (5.7) | 0.061 (0.16) | 8.48 (7.80) | 4.20 (3.42) | 6.75* (3.48) |
| Average response | 29.7 | 78.5 | 0.69 | 44.2 | 6.8 | 33.0 | 77.2 | 23.1 |

Notes: This table provides output from OLS regressions that capture psychometric assessments of select experimental mailings from an online sample of respondents (Amazon M-turk, total $N = 2,800$). We restricted tests to versions of the mailings without dependents. All regressions include a fixed effect to control for whether the participant had an eligible dependent. Please refer to the text for a description of the sample and design of the survey. Errors are robust.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Mechanisms: We turn to the psychometric survey evidence for insight into why modest, noninformative, changes in the appearance of the mailings lead to such large changes in response. Table 6 presents a series of regressions estimating how exposure to each of the mailing elements, randomized across survey respondents, altered the attention recipients paid to the information as well as inferences made with respect to the costs and benefits of the program. Indicator variables represent each intervention, with the control notice and worksheet excluded, and the model controls for the presence of dependents.

As initial evidence for whether the interventions successfully manipulated perceived complexity, the first column of the table indicates that subjects rated the complex notice, but not the lengthier worksheet, as significantly more complex than the control (notice: $p < 0.01$). That the latter doesn't register as more complex on this scale could be because, unlike the textually dense notice, the complex worksheet features a simple visual design. Overall, the survey suggests that the complex notice and worksheet may have dampened response not by significantly increasing the perceived effort or time-costs of claiming, as proxied by the willingness to pay a preparer to complete the worksheet (WTP Preparer), but by reducing the degree

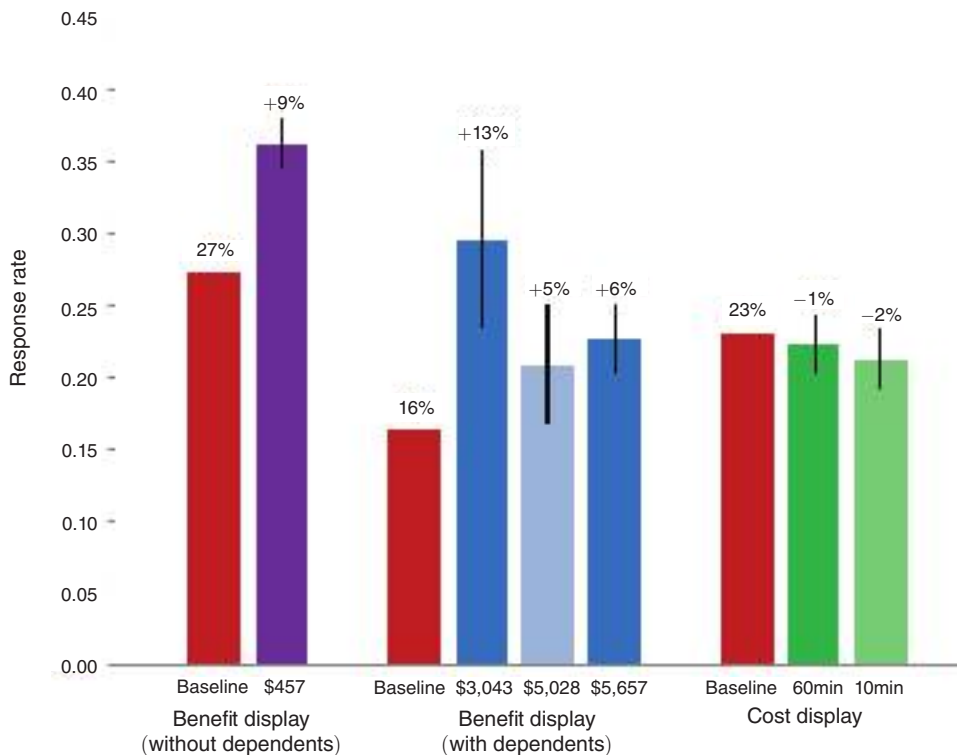


FIGURE 5. RESPONSE AND MARGINAL EFFECTS FOR BENEFIT AND COST DISPLAY INTERVENTIONS

Notes: This figure depicts the response rates, and marginal treatment effects, associated with the Benefit and Cost Display Interventions. Baseline figures refer to the response to the control mailing (simple notice and simple worksheet) for the relevant sample (i.e., those without dependents, those with dependents, and the overall sample, respectively).

to which individuals attended to, and understood, program information. Beliefs of program eligibility, in particular, appeared sensitive to the complexity of the worksheet.

Informational Interventions.—Among treatments that provided information, the display of benefit information was the most potent. The inclusion of a benefit range heightened response by nearly 0.08 ($p < 0.01$), or 33 percent, relative to the control, and its effect was roughly equal for respondents with and without dependents. Figure 5, which plots response separately for each benefit display relative to the appropriate control, investigates whether this increase in response was tied to the magnitude of the displayed figure. For those with dependents, assigned to this treatment arm, the figure reports response after flexibly adjusting for the number of dependents with dummy variables. The figure reveals that response to the benefit display was not tied to the benefit magnitude. For those with dependents, randomized to receive either a high or low display, the low display (\$3,043) actually produced the largest increase in response of 0.13. This represents an 81 percent increase relative to the 0.16 response of the dependent control, and is statistically distinguishable from the 0.04 and 0.06 increases induced by the \$5,028 ($p < 0.05$) and

\$5,657 ($p < 0.01$) displays. Those without dependents randomized into the benefit display treatment (\$457) also exhibited a large and statistically significant increase in response, relative to the control, of 0.08 ($p < 0.01$).

The remaining informational interventions did not significantly improve response. Figure 4 indicates that the inclusion of transaction cost information reduced response by 0.01 ($p < 0.10$), while Figure 5 indicates that the influence of the two cost displays (60 and 10 minutes) cannot be distinguished. The one-page informational flyer, which includes a benefit schedule as well as information regarding eligibility and enrollment, actually dampened response by 0.04 ($p < 0.01$), while the final two informational interventions—the envelope message and the indemnity message—had no statistically significant effect on behavior.

Mechanisms: Table 6 suggests at least two channels through which the benefit display may have altered behavior (the two \$5,000 interventions were coupled to increase power). First, respondents observing notices with the high and middle displays (\sim \$5,000, \$3,043) expected benefits twice as large as the control condition. Second, while the low display (\$457) did not significantly alter expectations of benefit size, it did significantly elevate belief of eligibility by 24 percent. Given beliefs of benefit size and eligibility are both sensitive to the benefit display, a possible explanation for the stronger response to the smaller magnitudes in the experiment may lie in the comparative degree to which the notices influence beliefs across these two margins (i.e., “If the benefit is that large, I must have known of it... therefore, I must not be eligible”). The nonpositive effect of the transaction cost notice on take-up is consistent with survey evidence indicating that respondents did not view the claiming worksheets as overly burdensome to complete. The mean willingness to pay a third party to complete the worksheet was \$33 (median: \$20) while the median expected completion time was 15 minutes (unreported in the table) which suggests perceived economic costs of claiming that were modest in comparison to expected benefits. Consistent with studies of tax salience (e.g., Chetty, Looney, and Kroft 2009), judging from an increased willingness to pay a preparer, the transaction cost notice may actually have heightened the salience of worksheet completion costs and, through this channel, reduced response. Intriguingly, survey respondents saw the informational flyer as more complex, relative to just the baseline notice. The flyer also lowered comprehension and actually decreased expectations of benefit size. These patterns raise the possibility that the flyer significantly lowered response in the field due to its perceived complexity. Finally, while neither the envelope or indemnity messages were tested in the psychometric instrument, the nonpositive reaction to the envelope, coupled with the relatively high share of survey respondents who claim they would open IRS mail (85 percent, not reported in the table) suggests that ignoring mail may not be an important determinant of low take-up in this context. Alternatively, our envelope message may have simply failed to increase the rate at which individuals open mail. The ineffectiveness of the indemnity message in raising response is surprising given survey respondents vastly overestimated the likelihood of an audit (mean belief of 23 percent relative to actual audit rate for EITC claimants of about 2 percent). Again, the lack of observed influence on

response in the field could be due to the treatment not sufficiently shifting recipient beliefs.²⁵

Stigma Interventions.—Finally, we consider the two interventions intended to reduce program stigma. The attempt to reduce personal stigma (emphasizing the role of “hard work”) did not affect response, while the social influence treatment, highlighting take-up of peers, surprisingly decreased response by 0.04, or 18 percent relative to the control ($p < 0.01$).

Mechanisms: The nonpositive impact of attempts to reduce stigma is consistent with survey results suggesting that claiming the EITC may not be highly stigmatizing. To assess perceived stigma, we asked respondents to indicate agreement with the statement “I respect anyone who decides to claim the earned income credit” (scale ranging from 0, strongly disagree, to 100, strongly agree). The mean response was 77 and less than 4 percent of respondents disagreed with the statement, signaled by a score below 50. We can only speculate as to why the social stigma intervention actually decreased response in light of its successful use in other contexts. One possibility, suggested by the psychometric surveys, is that while the intervention marginally increased respect for claimants (not significant), it also directionally increased perceived complexity and belief in the likelihood of an audit. The increase in recipient confusion, coupled with the already low baseline levels of perceived stigma, may have prompted recipients to react negatively to the social stigma intervention.

C. Persistence and Inertia of Take-Up

Policymakers would be remiss not to ask whether a one-time intervention leads to a continued pattern of increased take-up. The persistence of the interventions featured in this study also may offer insight into whether the effects are driven by information acquisition and learning as opposed to more transient mechanisms (e.g., attention-based or persuasion effects). We assess persistence with two distinct approaches that attempt to capture the effect of receiving a mailing on subsequent claiming and 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. Despite the absence of a “hold-out” group, randomized not to receive any mailing, in the experimental sample, we can still project a counterfactual rate of TY 2010 take-up by examining the rate of EITC claiming in the years prior to the experiment under straightforward assumptions. 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. The most plausible violations to this assumption, such as learning over time or shocks that persist across periods, should actually lead to lower relative claiming in period

²⁵ Another intriguing possibility is offered by Engel and Hines (1999) who note that tax behavior may be sensitive to expectations regarding audit rates in the future as well as the present.

TABLE 7—PERSISTENCE OF TREATMENTS AND TAKE-UP INERTIA

| | Claiming rate by year | | |
|---|--|---------------------|-------------------|
| | TY 2007 | TY 2008 | TY 2010 |
| <i>Panel A. Pre and post experiment claiming</i> | | | |
| Experimental sample | 0.162 (0.369) | 0.158 (0.365) | 0.245 (0.430) |
| <i>p</i> -value of claiming equivalence ($t = t - 1$) | | [0.149] | [0.000] |
| Adjusted for dependent age out | 0.16 (0.366) | 0.156 (0.363) | 0.245 (0.430) |
| <i>p</i> -value of claiming equivalence ($t = t - 1$) | | [0.228] | [0.000] |
| | Dependent variable: TY 2010 claiming (1,0) | | |
| | | OLS | IV |
| <i>Panel B. Inertial effect of TY 2009 claiming</i> | | | |
| Claiming 2009 (yes/no) | | 0.108*** (0.006) | 0.090* (0.049) |
| Observations | | 35,050 | 35,050 |
| R^2 | | 0.04 | 0.02 |

Notes: This table summarizes analysis of persistence of the experimental interventions as well as take-up inertia. Panel A compares EITC claiming in years prior to and following 2009. Bracketed figures indicate *p*-values from a *t*-test of the null hypothesis that current year claiming is equivalent to that of the prior year. Panel B reports results of an OLS and IV regression of TY 2010 claiming on TY 2009 claiming as specified in the text. Regressions include flexible controls for the number of dependents, as well as controls for gender, filing status, past claiming, preparation mode, expected benefit size, and earned income. Errors are robust.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

$t + 1$, given the failure to take-up in period t .²⁶ In this sense, if claiming is not independent across years, our estimate is likely to be a lower-bound of persistence.

Table 7 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 ($p < 0.01$). In support of the identifying assumption, TY 2008 and TY 2007 claiming are not statistically distinguishable ($p = 0.15$). 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, relative to the TY 2008 claiming rate, the table implies that the mailings led to a subsequent increase in claiming of 55 percent.

Next, 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: $Claim_{2010,i} = \alpha + \gamma Claim_{2009,i} + \mathbf{X}\beta' + \varepsilon_i$ where $Claim_i$ represents the binary

²⁶There is the possibility that a secular increase in take-up over this period, unrelated to the one-time shock which might have prompted non-claiming in TY 2009, could lead to the spurious appearance of persistence. However, overall take-up rates, reported by the IRS, (and available on the EITC website), suggest that claiming actually decreased in California in 2010 relative to 2008.

TABLE 8—SUMMARY STATISTICS FOR EXPERIMENT

| Variable name | All sample | | No dependents | | With dependents | |
|---------------------------------------|------------|--------------|---------------|--------------|-----------------|--------------|
| | Response | Observations | Response | Observations | Response | Observations |
| Experimental sample | 0.22 | 35,050 | 0.25 | 23,618 | 0.16 | 11,432 |
| <i>Panel A. Demographic variables</i> | | | | | | |
| 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 |
| <i>Panel B. Tax variables</i> | | | | | | |
| 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 2008 | 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 |
| <i>Panel C. Benefit and income</i> | | | | | | |
| Expected benefits: \$0 to \$499 | 0.24 | 26,988 | 0.25 | 23,618 | 0.15 | 3,370 |
| Expected benefits: \$500 to \$1,499 | 0.18 | 2,708 | — | — | 0.18 | 2,708 |
| Expected benefits: \$1,500 to \$2,499 | 0.17 | 1,701 | — | — | 0.17 | 1,701 |
| Expected benefits: \$2,500 to \$3,999 | 0.15 | 2,259 | — | — | 0.15 | 2,259 |
| Expected benefits: ≥ \$4,000 | 0.14 | 1,394 | — | — | 0.14 | 1,394 |
| Earned income: \$1 to \$4,999 | 0.24 | 9,759 | 0.24 | 9,230 | 0.22 | 529 |
| Earned income: \$5,000 to \$9,999 | 0.26 | 8,490 | 0.26 | 7,988 | 0.18 | 502 |
| Earned income: \$10,000 to \$19,999 | 0.23 | 7,895 | 0.25 | 6,400 | 0.16 | 1,495 |
| Earned income: \$20,000 to \$29,999 | 0.15 | 2,275 | — | — | 0.15 | 2,275 |
| Earned income: ≥ \$30,000 | 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 subcategories sum to 35,050 due to either missing data or excluded subcategories.

claiming decision for the specified tax year of person i , \mathbf{X} represents a vector of available demographic and tax variable controls, and γ 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 using the experimental interventions as an instrument for claiming in TY 2009. The resulting two-stage estimate recovers the LATE of higher take-up in TY 2009, induced by variation across the experimental interventions (first stage), on TY 2010 take-up (second stage). If the excludability assumption is violated—that is, the effect of the experimental mailings on subsequent take-up does not act only through changes in contemporaneous take-up—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. Panel B of Table 7 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 (i.e., or 44 percent relative to the 0.25 baseline claiming rate in TY 2010). The less precise IV estimate produces a similar effect magnitude of 0.09 (37 percent relative to baseline).

Overall, the analyses point to some persistence in the influence of the experimental mailings on take-up the following year. This is especially notable given that the

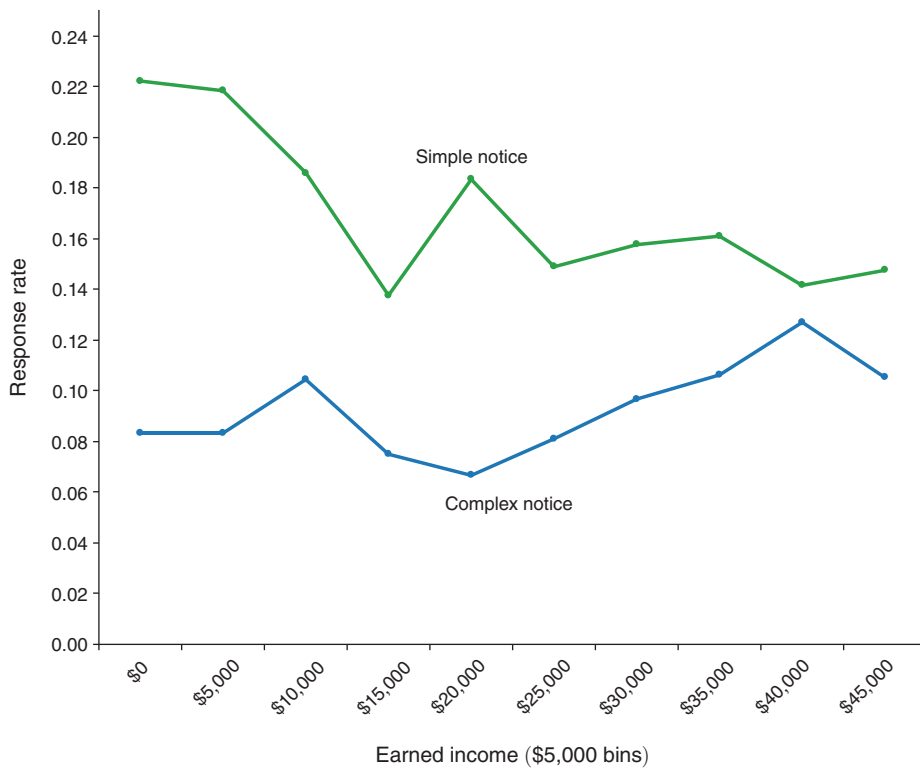


FIGURE 6. HETEROGENEITY IN RESPONSE TO SIMPLIFICATION BY EARNED INCOME
(For recipients with dependents)

Notes: This figure displays the response associated with the simple and complex notice by earned income for recipients with dependents. Response for each notice is averaged across the envelope and worksheet variants.

domain in which 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). This partial persistence speaks both to the possibility that respondents acquire and retain program information from the experimental mailings or to possible habit formation in claiming.

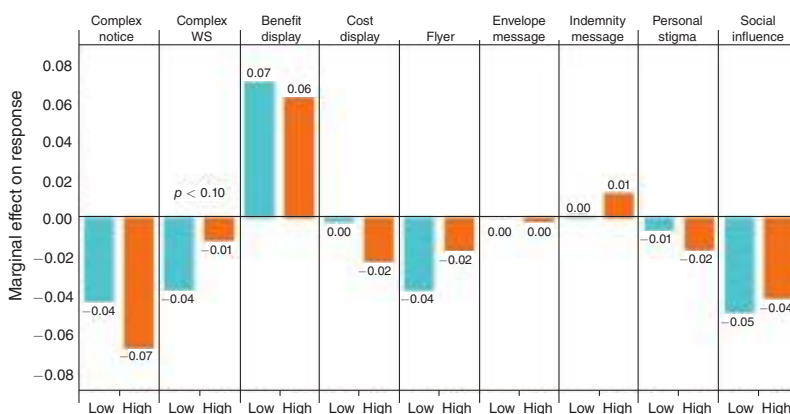
D. Heterogeneity in Response

We explore the heterogeneity in experimental response for potential insights of both theoretical and policy relevance. Looking first at differences in overall response by demographic and tax variables, Table 8 indicates a higher response rate for females, young recipients, and self-preparers for those with and without dependents. The apparent heterogeneity in response by earned income actually reflects differential response by dependent status. However, one must interpret the table with caution since the experimental population is the product of substantial selection that likely differs across the examined subpopulations.

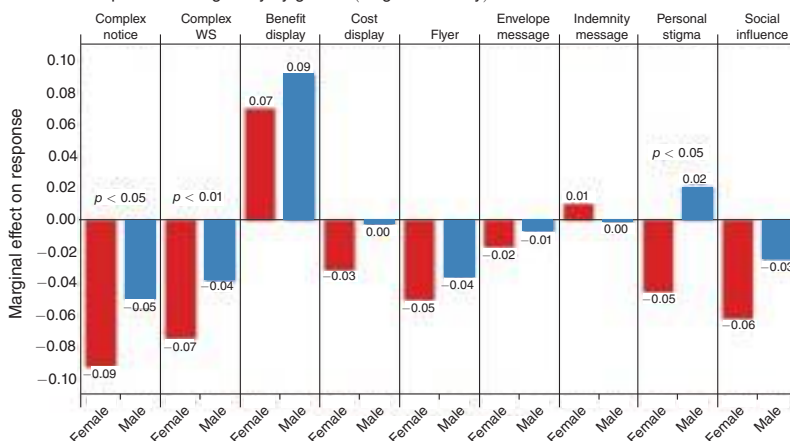
We can more cleanly investigate heterogeneity in the relative response to treatment as compared to control mailings. Our main analysis investigates the sensitivity of response to informational complexity across recipient income. We focus on those

Panel A. Response heterogeneity by benefit size

(filers with dependents, low benefit: < \$678, high benefit: >= \$678)



Panel B. Response heterogeneity by gender (single filers only)



Panel C. Response heterogeneity by age (single filers only, Young: < 39 years, Old: >= 39 years)

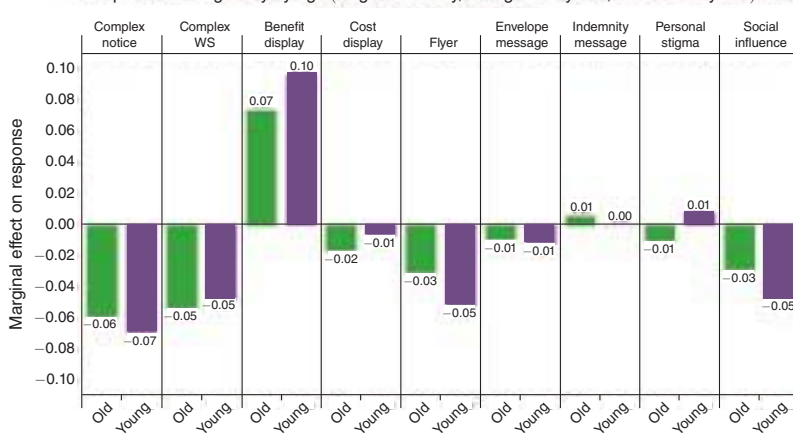


FIGURE 7. RESPONSE HETEROGENEITY BY BENEFIT SIZE, GENDER, AND AGE

Notes: This figure shows heterogeneity in experimental response by estimated benefit size, gender, and age. Each panel reports marginal effects by intervention for the specified sample. The sample for panel A is restricted to those with dependents, while the samples for panels B and C are restricted to single filers. The figure additionally reports *p*-values corresponding to statistically significant between-group differences, estimated separately from pooled regressions.

with dependents in order to examine a wide range of recipient incomes.²⁷ Figure 6 compares the average response by earned income bins of \$5,000 for those receiving either the complex or simple notice. To expand the comparison sample, we average response across the cross-randomized envelope and worksheet variants. The figure indicates that recipients with lower incomes benefited more from simplified notices than did recipients with higher incomes. Specifically, the differential increase in response for those below median income ($b = 0.084$) was more than twice that of recipients above median income ($b = 0.036$) ($p < 0.05$).²⁸ Even among a sample of relatively low earners, informational complexity disproportionately affected the very poor.

We examine heterogeneity in relative response to each treatment across other dimensions of interest—median benefit level, gender, and median age—and report these in Figure 7. We confine the analysis of gender and age to single filers for the purpose of identification. Overall, relative to the control condition, females were more deterred by complexity (notice: $p < 0.05$, worksheet: $p < 0.01$) as well as the attempt to reduce personal stigma ($p < 0.05$), than were men. We do not find clear heterogeneity in response with respect to benefit size or recipient age.

Our results additionally speak to the possibility that language may serve as a barrier to take-up. While we did not experimentally test non-English language notices, we can estimate a language-neutral take-up rate by modeling overall response to the mailings across regions using zip code level census data from 2010.²⁹ Assuming that differences in response, conditional on covariates, across regions of varying density of Hispanic households can be attributed to language, the estimates, as reported in Table 4, imply that overall take-up would rise from 0.22 to 0.25 in the absence of language barriers. While unobserved cultural factors might also account for the observed patterns, the disproportionately positive, and statistically significant, response in Hispanic regions to the messaged envelopes, which included a Spanish translation, also points to language as a meaningful predictor of overall take-up.³⁰

IV. Rationalizing and Generalizing Findings

A. Implication of Findings for Models of Take-up

One may have initially interpreted incomplete take-up of the EITC among tax filers as reflecting costs of claiming—that is, those associated with time, effort, stigma, and potential penalties—which outweigh program benefits. However, the responses documented in the field study, and mechanisms implied from the survey, are difficult

²⁷For those without dependents, the interquartile range in income is \$2,964 to \$10,307. Even for this group, we find that the complex notice is, at least directionally, more detrimental for subjects below ($b = -0.067$), as compared to above ($b = -0.057$), median earnings.

²⁸We find similar results when explicitly controlling for the cross-randomized envelope and worksheet interventions and demographic controls.

²⁹Specifically, we estimate the regression $Response_{ij} = \alpha + \theta HispDens_j + \mathbf{X}\beta' + \varepsilon_{ij}$ 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 \mathbf{X} is a vector of controls including tax, benefit, and demographic variables. θ is the statistic of interest.

³⁰Adapting the main response model by including an interaction between the messaged envelope and Hispanic household density produces a statistically significant and positive interaction coefficient, 0.030 ($p < 0.10$). The sum of the interaction coefficient and the coefficient for the envelope indicator is positive but insignificant.

to rationalize in a traditional model of take-up in which eligible individuals balance accurately perceived expectations of benefits and costs, even allowing for the possibility of program stigma. In particular, the field experiment affirms the sensitivity of take-up to repeated exposure to program information (i.e., simply receiving a second notice), reductions in its complexity (i.e., through the simplified notice, shortened worksheet, and even omission of the informational flyer) or changes to its salience (e.g., benefit display), but not attempts to lower perceptions of program stigma or expectations of the time-costs of claiming. Consistent with this pattern of behavior, the accompanying survey suggests that successful interventions may have influenced decisions by heightening awareness and remedying confusion with respect to eligibility and benefit size (possibly by increasing the attention paid to the mailings), but not by significantly reducing expectations of the economic costs of claiming—which respondents reasonably judged to be low.

The present findings seem more consistent with alternative models of behavior in which psychological frictions play an important role. One candidate model is one in which individuals rationally weigh the costs and benefits of claiming, but suffer from distorted beliefs as to the magnitudes of such costs and benefits. However, the relatively modest baseline assessments of claiming costs from the surveys, and the further fact that the substantial influence of complexity on experimental response is not driven by increases in the perceived economic costs of claiming (Table 6), suggest that informational frictions alone may be insufficient for explaining the low take-up observed in this setting.³¹ Other models, which depart more sharply from conventional models of take-up, may have more success in rationalizing the accumulated evidence. One such example are those models which incorporate the presence of “hassle costs.” First introduced by psychologist Kurt Lewin (1951) and later discussed in the context of financial decisions of the poor by Bertrand, Mullainathan, and Shafir (2006), the framework explains how seemingly minor details can influence behavior to a degree larger than that predicted by economic costs alone by facilitating, or hindering, the psychologically important initial steps of a multi-step task.³² With respect to program take-up, rather than deciding to claim after careful evaluating expected costs and benefits, individuals may instead avoid, or postpone, claiming due to the psychological burden imposed by complicated forms, confusion about program rules, or even a small degree of uncertainty with respect to eligibility. The potential influence of hassle costs on important decisions is consistent with the success of automatic defaults in reshaping retirement savings and organ donation, as well as studies demonstrating the surprisingly large importance of minor logistical detail in improving medical adherence (e.g., Gilovich and Griffin 2010; Milkman et al. 2011). A recent study documented how tax complexity could serve as a psychological hassle in finding that taxpayer aversion for itemizing returns amounted to individuals valuing the time-costs of itemization 4.2 times more than the time-costs associated with other tasks (Benzarti 2015). While the recognition of hassle costs offers one promising account for how minor changes in the decision-setting might

³¹ Given survey respondents had inflated beliefs of the likelihood of an audit, if the indemnification intervention was not effective in assuaging audit concerns, it is possible that a model of take-up with distorted beliefs of penalty costs could explain low take-up in this context.

³² Lewin’s work spoke about the role of small situational forces, or “channel factors,” which caused individuals to move strongly toward, or away from, a particular goal.

lead to significant changes in behavior, the findings of the study may also reflect other models of behavior including those which involve limits to attention (Karlan et al. 2015), self-control (e.g., O'Donoghue and Rabin 1999), or other cognitive resources (Mullainathan and Shafir 2013).

B. Generalizing Findings with a Survey of Low-Income Tax Filers

A potential drawback of the present study is that because it pertains to a sample which failed to claim the credit on two prior occasions and is also observably different from the overall population of EITC claimants—the typical experimental subject is more likely to be without a dependent, male, and to have self-prepared—the findings may not generalize. One difficulty in assessing generalizability is that while Table 2 reports available characteristics of EITC claimants and non-claiming tax filers, we cannot directly observe the characteristics of non-claimants. Nevertheless, to examine the potential role of psychological frictions in explaining non-claiming in the EITC more generally, we report the results of a second survey, along with additional findings from the first, in order to better understand program awareness and literacy, and perceptions of program stigma, beyond the experiment. The second survey, administered primarily at volunteer tax clinics in Chicago, comprises a diverse sample of 1,139 low to moderate income tax filers. While the survey is itself narrowly limited to subjects who file with preparer assistance, the use of preparers is commonplace among EITC claimants with 66 percent of TY 2009 claims having been filed in this manner. Given estimates from Plueger (2009) indicating an average income of \$8,900 for non-claimants, and further, that a majority of non-claimants had a qualifying dependent (63 percent) and, among single filers, were female (56 percent), eligible survey respondents (\$9,000 median income; 41 percent with dependents; 60 percent female) more closely resemble eligible non-claimants across these dimensions.

The results of the survey, summarized in Table A1 of the online Appendix, indicate widespread deficits in program awareness and misperceptions regarding program benefits and the costs of claiming. Only 54 percent of the sample, including 56 percent of the 65 percent deemed eligible for the program, reported awareness of the EITC.³³ The survey also provides novel evidence that individuals systematically under-estimate eligibility and the magnitude of program benefits. After reading provided program information, one-third of those eligible for the credit did not believe themselves to be eligible (this compares to 12 percent of sample which believed themselves to be eligible when they were not). Among those who correctly judged eligibility, the median ratio of expected to actual benefits was 0.8, 61 percent under-estimated benefit size, and 41 percent under-estimated benefit size by 50 percent or more. Echoing conclusions from the first survey, respondents did not perceive claiming as overly time-consuming, but did substantially overestimate the likelihood of an audit with a median estimate of 15 percent (more than eight times the actual audit rate for EITC claimants). Finally, the table reports, low to moderate

³³ We did not elicit the full set of information required to determine exact eligibility and benefit size such as investment income or an invalid Social Security number. For the large majority of individuals, our inferences regarding eligibility and benefit size should be accurate.

evidence that respondents viewed benefit receipt as stigmatizing.³⁴ Overall the second survey documents low program awareness, a significant degree of under-estimation of eligibility and benefit size, reasonably well-calibrated beliefs about the time-costs of claiming, but high costs associated with potential penalties, and low to moderate perceptions of stigma.

Given concerns that the second survey is unrepresentative, we also report program awareness and literacy from respondents of the original psychometric survey. The 38 percent of this sample which appears eligible for the credit once again resemble non-claimants more closely than the experimental sample with respect to gender (64 percent female) and the share with qualified dependents (57 percent) but has higher income (median: ~\$13,000). More tellingly, the sample includes a significant fraction of eligible non-claimants (i.e., of those deemed eligible, 68 percent applied for the EITC, while 17 percent did not, and 15 percent didn't remember). Taken together, the two survey instruments canvass several thousand low-income respondents—including eligible claimants and non-claimants—and document low levels of program awareness, confusion with respect to program incentives, and low to moderate degrees of perceived stigma. While one must cautiously interpret the findings from these samples, the surveys imply that the psychological frictions implicated in the field study may extend to broader groups of EITC non-claimants.

Lay Theories for Incomplete Take-Up.—An alternative strategy through which to understand the factors responsible for low take-up is to directly ask the target population why they, or their peers, might not claim an EITC credit. The introspections of the surveyed sample, including those eligible for the credit, parallel our other findings in attributing the failure to claim to confusion regarding eligibility and program rules, but not the insufficient size of benefits, low need of government assistance (possibly capturing perceptions of program stigma), or fear of penalties for inappropriate claiming (Table A2 of the online Appendix).

V. Policy Implications

In the introduction we noted that the welfare implications of low take-up hinged on whether the presence of psychological frictions, among those of high need, deterred claiming. The findings of the study, including the observation that the lowest earners in the sample were disproportionately harmed by informational complexity, supports the view, adopted by those who administer the EITC, that improving take-up is normatively desirable. Allowing for the possibility that these findings generalize to all tax filing non-claimants, we can project how the experimental interventions might affect overall program take-up with a series of calibrations.

³⁴The surveys indicate that 14 percent of subjects strongly disagree, and another 18 percent simply disagree, with a statement declaring that people generally “respect” anyone who receives a benefit, while 11 percent strongly disagree, and another 29 percent simply disagree, with a statement stating that an individual “would not care” if their friends were aware of the benefit. We interpret this as indicating a small to moderate share of individuals who may find the program to be stigmatizing.

TABLE 9—PROJECTED POLICY IMPACT OF EXPERIMENT FOR ALL EITC TAX FILING NON-CLAIMANTS

| Mailing type | Policy intervention and target population | | | | | | | |
|---|---|--------------|--------------------------------------|---|---|---|--|---|
| | Second mailing experimental sample | | Second mailing all CP nonrespondents | | Revised first mailing all CP recipients | | Revised first mailing all tax filing non-claimants | |
| | Response | Benefit | Observations | Response (Δ take-up) [\$Benefits] | Observations | Response (Δ take-up) [\$Benefits] | Observations | Response (Δ take-up) [\$Benefits] |
| Complex mailing (also proxy for initial CP mailing) [+2.3m] | 0.14 (0.00) | \$461 | +44,988 | 0.14 (0.00) [+\$24m] | 321,340 | 0.47 (0.01) [\$121m] | 1,128,000 | 0.47 (0.05) [\$520m] |
| Simple mailing | 0.23 (0.00) [+\$4.1m] | \$514 | +73,908 | 0.23 (0.00) [+\$38m] | +54,981 | 0.56 (0.00) [+\$28m] | +216,000 | 0.56 (0.01) [+\$111m] |
| Benefit display | 0.31 (0.00) [+\$5.9m] | \$544 | +99,615 | 0.31 (0.00) [+\$54m] | +103,854 | 0.64 (0.00) [+\$56m] | +408,000 | 0.64 (0.02) [+\$222m] |
| Benefit display + second mailing | — | — | — | — | +172,031 | 0.75 (0.01) [+\$128m] | +675,840 | 0.75 (0.03) [+\$503m] |
| Actual population levels (TY 2009) | 35,050 | | 321,340 | | 610,904 | | ~2.4m | |

Notes: This table projects how expanding the distribution of the experimental mailings to broader populations of filing non-claimants would change overall program take-up and disbursements under the stated assumptions and using figures from TY 2009. Bolded figures are exact and are from IRS while other figures are estimated. Parenthetically, we report the percent change in overall program take-up reflected by the given projection. We project results for the simple mailing (i.e., simple notice and worksheet), the simple mailing with benefit display (with simple worksheet), and the benefit display plus a second mailing (also a benefit display) sent to nonrespondents of the first mailing. The first set of columns reports response for the experimental sample. The second set of columns projects the response of a second mailing distributed to all CP nonrespondents. The third set of columns projects take-up assuming that the original CP notice was replaced by the experimental mailings. The final set of columns projects response in a scenario in which an initial notice, whose design is replaced by an experimental mailing, is distributed to the entire population of non-filing non-claimants rather than just those who received the initial CP notice. The number of total non-filing non-claimants is estimated using take-up rates from Plueger (2009) and assumes 27 million individuals were eligible for the EITC in TY 2009.

A. Projected Effect of Interventions on Overall Take-Up

We can estimate the effect of scaling-up our interventions on overall take-up, as well as disbursements, by projecting the increase in response under various scenarios involving wider distribution of the experimental mailings. Table 9 reports the estimated impact of select experimental mailings on various subsets of filing non-claimants for TY 2009 (bolded figures reflect exact data). For tractability, we interpret the complex mailing, in the first row of the table, as a proxy for repeat distribution of the initial CP 09/27 mailing even though the two mailings feature differences in the worksheet design (as the original CP worksheet was not tested).

The first set of columns reports the average response rates and benefit levels directly from the field experiment while the second set of columns extrapolates the additional response one would expect if experimental mailings were distributed to the national population of 321,340 filing non-claimants who failed to respond to the initial CP mailing. For example, we estimate that the mere distribution of a second mailing, approximately similar to the first reminder notice, would result in an additional 44,988 claimants, whereas a more efficacious notice would yield 73,908 (simple mailing) to 99,615 (benefit display) additional claimants. In the third set of columns, rather than assuming a second round of notices, we project the outcome

of replacing the initial CP notices, distributed to 610,904, with the experimental designs. Conservatively assuming that experimental response rates relate additively, rather than proportionally, to the initial CP response, we estimate that an updated mailing would yield an estimated 54,981 to 103,854 in additional responses, amounting to \$28 million to \$56 million in additional disbursed benefits.³⁵

The fourth set of columns projects the additional claiming that would result from replacing the initial mailings with the experimental mailings across all filing non-claimants—that is, both existing CP recipients as well as the estimated 1.8 million individuals who may not have received a CP notice. Notably, expanding the notice program to all filing non-claimants, even using the original notice, would result in a substantial improvement in take-up. The extrapolation suggests that adopting the experimental mailing designs could yield an additional 216,000 to 408,000 claimants (\$111 million to \$222 million in additional benefits) beyond those brought in from the expanded distribution. Finally, the last row of the table projects response given a combination of a redesigned first notice (Benefit Display) and an identical second notice. This policy intervention, even if targeted only at the existing population of CP recipients, would yield, according to our estimates, an additional 172,000 claimants and \$128 million in benefits. We parenthetically report the increase in overall program take-up implied by these projections. These calculations reveal a sizable benefit from expanding the original population of mailing recipients (+0.05) beyond that achieved through the contextual changes explored in the experiment (+0.03). All told, we estimate that expanding the population of recipients, redesigning documents, and instituting a second mailing to initial non-respondents, could improve take-up from 0.75 to 0.83. Of this projected increase, we attribute a rise in take-up of 0.03, involving \$503m in additional benefits, to the redesigned mailings.

B. Cost-Benefit Analysis

While we interpret the findings of our study to suggest that higher take-up would raise individual, and collective, welfare, a full normative analysis is beyond the scope of this paper. Nevertheless, we can gain insight into the economic consequences of a policy involving simpler and more psychologically informed mailings by sketching out the anticipated costs and benefits of expanding the tested interventions.

Costs of the Policy.—Our experimental interventions are not likely to be costly. While we lack explicit data on costs, we can organize such costs as those relating to (i) administration (i.e., printing, distributing, and processing the mailings); (ii) noncompliance (i.e., ineligible claiming); and other (iii) negative externalities (e.g., disutility of receiving IRS mail). Administrative costs are likely minimal if they resemble the current 0.5 percent expense ratio of the EITC (IRS 2003) which is less than the 16 percent expense ratio of other transfer programs (Eissa and Hoynes

³⁵For example, we project the response to the simplified baseline notice as 56 percent amongst the CP population, given the response of 47 percent to the original notice, and the 9 percent additive response generated by the simple mailing (as compared to 77 percent under an assumption of proportionality). Estimated increases in disbursements are bracketed in the table.

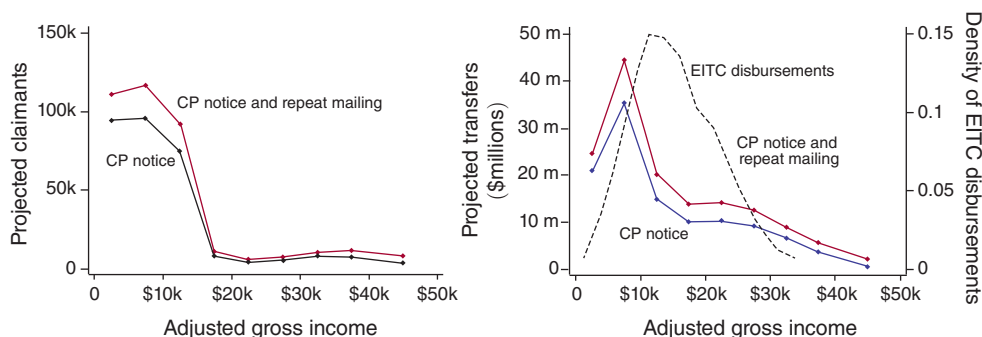


FIGURE 8. PROJECTED CLAIMANTS AND DISBURSEMENTS FOR SECOND MAILING BY INCOME
(Assumes a second simplified mailing to CP nonrespondents)

Notes: This figure depicts the projected shift in the distribution of claimants and disbursements by adjusted gross income attributable to a second, simplified, reminder notice distributed to nonrespondents of the initial CP notice. Distributional data for the CP notice are from TY 2008 (the year prior to the experiment) while projected distributions are extrapolated from the experimental data. The distribution of overall EITC disbursements in the right panel is estimated from TY 2004 data reported by Eissa and Hoynes (2011).

2011). Noncompliance costs are also likely to be minimal given that statutory eligibility can be, at least noisily, inferred from administrative records. Moreover, there is no evidence that the experiment led to an increased rate of ineligible claiming, relative to all program claimants, judging from the relative rates of disallowed claims (0.93 percent in the experiment, versus 0.72 percent nationally) and audits (1.41 percent versus 1.91 percent, respectively). Externalities associated with the mailings—such as those which might be incurred if the mailings reduced attention to other important communications—would need to be significant for the total cost of the interventions to significantly exceed the modest costs of administration.

Benefits of the Program.—One could gauge the social benefits of higher take-up from the revealed preference of policymakers—e.g., congress appropriated \$716 million in 1997 over five years for EITC outreach and enforcement—or, alternatively, by forecasting how our interventions, if scaled to broader populations, would shift the income distribution of beneficiaries. Under the conservative assumption of EITC budget neutrality, we can compare the preexperimental income distribution of CP notice recipients (TY 2008 data) to the projected income distribution under a regime featuring a second, simplified, notice. To achieve budget neutrality, we proportionally reduce the benefits of all EITC claimants to fund new enrollees.

Figure 8 indicates that the majority of new claimants would fall in the left of the existing income distribution of CP claimants, and further, that the typical CP claimant is poorer than the typical overall EITC claimant (data is from Eissa and Hoynes 2011 who tabulate returns from 2004 Statistics of Income (SOI) files). The exercise implies that redistributing benefits among existing EITC claimants to fund new claimants, through interventions like those used in the experiment, would result in a transfer of incomes to the very poor. Given the modest costs of administration, non-compliance, and externalities, assuming some curvature in a policymaker's social welfare function, the analysis echoes our earlier interpretation that a policy which

leveraged the findings of the study, even under budget neutrality, would be likely to improve welfare.³⁶

VI. Conclusions

In this paper we use a field experiment, in collaboration with the IRS, to better understand the factors that give rise to the incomplete take-up of economically consequential government benefits. Our study demonstrates that the mere receipt of an informational notice and claiming worksheet, just months after the receipt of a very similar mailing, led to higher take-up. More strikingly, the complexity, and salience, of the information in the mailings shaped the likelihood of claiming, but attempts to reduce stigma or perceptions of economic claiming costs did not. We sought to understand the mechanisms underlying the differential responses to the interventions with an accompanying survey. The survey suggested that successful mailings heightened program awareness, improved accuracy of beliefs regarding eligibility and benefit size, and increased attention paid to the notices, and, consistent with the findings from the experiment, did not substantially reduce the perceived costs of claiming. We explored the generalizability of our findings with a second survey of low-income individuals. Together, the two surveys point to deficits in program awareness and understanding that extend beyond the experimental sample.

Our focus on understanding the behavior of non-claimants ignores the potentially critical role of the tax preparers. Given the share of EITC claimants who rely on preparers, an open question is why such preparers would fail to claim the credit for their clients (particularly since many paid preparers may have incentives to file claims)? While the composition of the experimental sample implies prepared claims are less likely to forego an eligible credit as compared to self-prepared claims, one possible explanation, raised during informal discussions with the preparer community, is that the sheer size of the preparer population and the ease of application—reportedly over 1 million preparer identification numbers were issued from 1999 to 2010—has led to significant variation in preparer quality. Given the complexity of the EITC and other credits for which a typical EITC claimant may also be eligible, it is plausible that even a reasonably competent preparer might neglect to claim a credit on behalf of a client who is herself unaware.

Our study has important limitations. Chief among these is that because our experimental and survey samples are nonrepresentative, our findings may not generalize to other non-claiming populations even within the EITC. A second limitation concerns the scalability of the identified strategies for improving take-up. As an 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 a question for future work.


These limitations notwithstanding, we see three primary implications of this work. First, in this setting, and perhaps more broadly, the findings suggest that incomplete take-up should be viewed as a “policy problem” in which those of high economic

³⁶We do note that, in this exercise, the redistribution of marginal dollars from households typically with children to those typically without children may have more complicated implications for welfare. We thank an anonymous referee for this observation.


need do not receive intended benefits. Second, our evidence is not easily rationalized by a simple cost-benefit model of take-up, even one which allows for stigma, but instead seems consistent with models in which small changes to the frequency, appearance, and complexity of information matters. We hope that future research will clarify which of these models best describe take-up in the presence of psychological frictions. A final, practical, implication is that we see our study as identifying a set of specific interventions, and a more general set of principles, that highlight the role of nontraditional policy levers in engaging populations that may not be highly responsive to traditional incentives. To the extent that even the most sensible policy implementation may not overcome decision-making frictions, like those associated with program complexity, there may be a rationale for policies, such as the automatic distribution of payments, that move beyond merely simplifying program information to simplifying the rules and incentives governing such programs.

APPENDIX: SELECTED EXPERIMENTAL INTERVENTIONS


Panel A1. Simple notice (control)

| | | | | | | | | | | | | | | |
|---|--|---|--------|--------|----------|------|-------------|---------------|------------------------|-------------|---------------|----------------|------|--------|
|  Department of the Treasury Internal Revenue Service Submission Processing Center Fresno, CA 93888-0405 | | <table border="1"> <tr><td>Notice</td><td>EC0927</td></tr> <tr><td>Tax Year</td><td>2009</td></tr> <tr><td>Notice Date</td><td>November 2010</td></tr> <tr><td>Social Security Number</td><td>999-99-9999</td></tr> <tr><td>To Contact Us</td><td>1-800-829-1040</td></tr> <tr><td>Page</td><td>3 of 4</td></tr> </table> | Notice | EC0927 | Tax Year | 2009 | Notice Date | November 2010 | Social Security Number | 999-99-9999 | To Contact Us | 1-800-829-1040 | Page | 3 of 4 |
| Notice | EC0927 | | | | | | | | | | | | | |
| Tax Year | 2009 | | | | | | | | | | | | | |
| Notice Date | November 2010 | | | | | | | | | | | | | |
| Social Security Number | 999-99-9999 | | | | | | | | | | | | | |
| To Contact Us | 1-800-829-1040 | | | | | | | | | | | | | |
| Page | 3 of 4 | | | | | | | | | | | | | |
| <p>JAMES Q HINDS 22 BOULDER STREET HANSON, CT 06030-7253</p> | | | | | | | | | | | | | | |
| <p>Important information about the Earned Income Credit You may be eligible for a refund</p> | | | | | | | | | | | | | | |
| <p>Do not discard or overlook this notice because you may be entitled to some additional money.</p> | <p>Summary</p> <p>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 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.</p> | | | | | | | | | | | | | |
| <p>What you need to do</p> | <p>Complete the Earned Income Credit Worksheet on Page 3.</p> <p>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.</p> <p>If the worksheet indicates that you are not eligible for the credit Please do not return the worksheet to us.</p> | | | | | | | | | | | | | |
| <p>Next steps</p> | <p>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.</p> <p>Next year, 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.</p> | | | | | | | | | | | | | |
| <p>Additional information</p> | <p>If you need additional assistance, please call 1-800-829-1040, or visit online at www.irs.gov/eic. For tax forms, call 1-800-764-4674 (1-800-829-3676).</p> <p>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.</p> | | | | | | | | | | | | | |


Panel A2. Complex notice (page 1 of 2)

| | | | | | | | | | | | | | | |
|--|----------------|---|--------|--------|----------|------|-------------|---------------|------------------------|-------------|---------------|----------------|------|--------|
|  Department of the Treasury Internal Revenue Service Submission Processing Center Fresno, CA 93888-0405 | | <table border="1"> <tr><td>Notice</td><td>EC0927</td></tr> <tr><td>Tax Year</td><td>2009</td></tr> <tr><td>Notice Date</td><td>November 2010</td></tr> <tr><td>Social Security Number</td><td>999-99-9999</td></tr> <tr><td>To contact us</td><td>1-800-829-1040</td></tr> <tr><td>Page</td><td>1 of 4</td></tr> </table> | Notice | EC0927 | Tax Year | 2009 | Notice Date | November 2010 | Social Security Number | 999-99-9999 | To contact us | 1-800-829-1040 | Page | 1 of 4 |
| Notice | EC0927 | | | | | | | | | | | | | |
| Tax Year | 2009 | | | | | | | | | | | | | |
| Notice Date | November 2010 | | | | | | | | | | | | | |
| Social Security Number | 999-99-9999 | | | | | | | | | | | | | |
| To contact us | 1-800-829-1040 | | | | | | | | | | | | | |
| Page | 1 of 4 | | | | | | | | | | | | | |
| <p>JAMES Q HINDS 22 BOULDER STREET HANSON, CT 06030-7253</p> | | | | | | | | | | | | | | |
| <p>You May Be Eligible for a Refund If You Qualify for the Earned Income Credit</p> | | | | | | | | | | | | | | |
| <p>Why We Are Sending You this Notice</p> <p>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:</p> <ul style="list-style-type: none"> 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. | | | | | | | | | | | | | | |
| <p>What You Need to Do</p> <p>Income is not the only condition that determines if you qualify for EIC. We need you to complete the enclosed <i>EIC Eligibility Check-Sheet</i> to see if you may qualify for the EIC. Take the following steps to complete the check-sheet:</p> <ul style="list-style-type: none"> Check that you are eligible for the EIC in Step 1. <ul style="list-style-type: none"> 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 <i>EIC Eligibility Worksheet</i> to us in the enclosed envelope. | | | | | | | | | | | | | | |
| <p>Note: Return the EIC Worksheet to us only if you determine you may qualify for the EIC.</p> | | | | | | | | | | | | | | |


Panel B1. Simple worksheet (no dependents)

| | | |
|---|--|--|
|  Department of the Treasury Internal Revenue Service Submission Processing Center Fresno, CA 93888-0405 | | Notice EIC0927 Tax Year 2009 Notice Date November 2010 Social Security Number 999-99-9999 To Contact Us 1-800-829-1040 Page 3 of 4 |
| INTERNAL REVENUE SERVICE Submission Processing Center Fresno, CA 93888-0405 | | |
| <h3>Earned Income Credit Worksheet</h3> | | |
| Step 1 Read each statement listed below, and place a checkmark next to any that describes you. | | |
| <input type="checkbox"/> My Social Security card reads "Not Valid for Employment," AND my Social Security card was issued so I could receive federally funded benefits such as Medicaid. <input type="checkbox"/> I was an Earned Income Credit qualifying dependent of another person in 2009. You may be 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 1-800-829-1040 or visit www.irs.gov/eic 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 _____ Date _____ <input type="checkbox"/> a.m. <input type="checkbox"/> p.m. Primary phone _____ Best time to call _____ Secondary phone _____ Best time to call _____ | | |
| For IRS use only - Code WS4 | | |


Panel B2. Complex worksheet (no dependents)
(page 1 of 2)

| | | |
|---|--|--|
|  Department of the Treasury Internal Revenue Service Submission Processing Center Fresno, CA 93888-0405 | | Notice EIC0927 Tax Year 2009 Notice Date November 2010 Social Security Number 999-99-9999 To Contact Us 1-800-829-1040 Page 3 of 4 |
| INTERNAL REVENUE SERVICE Submission Processing Center Fresno, CA 93888-0405 | | |
| <h3>Earned Income Credit Worksheet</h3> | | |
| Step 1 Read each statement listed below, and place a checkmark next to any that describes you. | | |
| <input type="checkbox"/> My Social Security card reads "Not Valid for Employment," AND my Social Security card was issued so I could receive federally funded benefits such as Medicaid. <input type="checkbox"/> I was an Earned Income Credit qualifying dependent of another person in 2009. You may be 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 1-800-829-1040 or visit www.irs.gov/eic for additional information regarding the above requirements. | | |
| <input type="checkbox"/> My filing status in 2009 was "married filing separately" <input type="checkbox"/> I was not a U.S. citizen (or resident alien) for any part of 2009 <input type="checkbox"/> I filed Form 2555 (Foreign Earned Income) or Form 2555-EZ (Foreign Earned Income Exclusion) in 2009 <input type="checkbox"/> My investment income was greater than \$3,100 in 2009 <input type="checkbox"/> I did not have earned income in 2009 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 1-800-829-1040 or visit www.irs.gov/eic for additional information regarding the above requirements. | | |

Panel C1. Benefit display (high)

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|---|---|--|
|  Department of the Treasury Internal Revenue Service Submission Processing Center Fresno, CA 93888-0405 | | Notice EIC0927 Tax Year 2009 Notice Date November 2010 Social Security Number 999-99-9999 To Contact Us 1-800-829-1040 Page 3 of 4 |
| JAMES Q. HINDS 22 BOULDER STREET HANSON, CT 06030-7253 | | |
| Important information about the Earned Income Credit You may be eligible for a refund of up to \$5,657 | | |
| Do not discard or overlook this notice because you may be entitled to some additional money. Depending on your earnings and eligibility, your benefit can be up to \$5,657. | 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 form. The credit, which can be up to \$5,657, 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, 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 . 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 . | |

Panel C2. Benefit display (low)

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|---|---|--|
|  Department of the Treasury Internal Revenue Service Submission Processing Center Fresno, CA 93888-0405 | | Notice EIC0927 Tax Year 2009 Notice Date November 2010 Social Security Number 999-99-9999 To Contact Us 1-800-829-1040 Page 1 of 4 |
| JAMES Q. HINDS 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. Depending on your earnings and eligibility, your benefit can be up to \$457. | 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 form. The credit, which can be up to \$457, 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, 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 . 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 . | |

Panel D1. Transaction cost notice

Department of the Treasury
Internal Revenue Service
Submission Processing Center
Fresno, CA 93888-0405

Notice EIC0927
Tax Year 2009
Notice Date November 2010
Social Security Number 999-99-9999
To Contact Us 1-800-829-1040
Page 1 of 4

JAMES Q. HINDS
22 BOULDER STREET
HANSON, CT 06060-7253

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 because you may be entitled to some additional money.

To claim your benefit, simply complete and return this form. This usually 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 2009 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 8 to 10 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, 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 www.irs.gov/eic. For tax forms, call 1-800-TAX-FORM (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.

Panel D2. Indemnification worksheet (no dependents)

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INTERNAL REVENUE SERVICE
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Complete to the best of your ability – you will NOT be penalized for unintentional errors
Earned Income Credit Worksheet

Step 1

Read each statement listed below, and place a checkmark next to any that describes you:

☐ My Social Security card reads "Not Valid for Employment," AND my Social Security card was issued so I could receive federally funded benefits such as Medicaid?

☐ I was an Earned Income Credit qualifying dependent of another person in 2009.

You may be 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 1-800-829-1040 or visit www.irs.gov/eic 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.

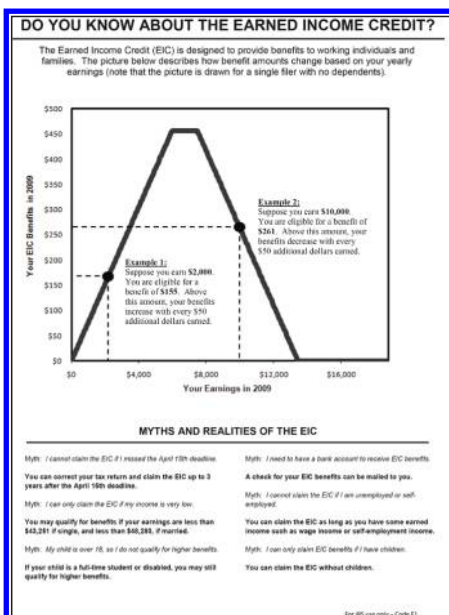
Your signature _____ Date _____

☐ a.m. ☐ p.m.

Primary phone _____ Best time to call _____ Secondary phone _____ Best time to call _____

For IRS use only – Code WS2

Panel E1. Informational flyer



Panel E2. Envelope message

Department of the Treasury
Internal Revenue Service
Submission Processing Center
Fresno, CA 93888-0405

Important – Good News for You
(Importante - Buenas Noticias para Usted)

JAMES Q. HINDS
22 BOULDER STREET
HANSON, CT 00000-7253

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