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NUDGES AND LEARNING:
EVIDENCE FROM INFORMATIONAL INTERVENTIONS FOR LOW-INCOME TAXPAYERS

Dayanand S. Manoli
Nicholas Turner

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ABSTRACT

Do informational interventions create one-time nudges or permanent changes in behavior? We study how taxpayers respond to informational interventions that alert them of their eligibility for the Earned Income Tax Credit using population-level administrative tax data. The empirical analysis is based on a natural experiment in 2005, a randomized experiment in 2009, and quasi-random audits between 2006 and 2009. The evidence from each of these settings indicates that the informational interventions cause economically significant increases in EITC take-up in the short-term, but there are little to no long-term increases in EITC take-up.

Dayanand S. Manoli
Department of Economics
University of Texas at Austin
2225 Speedway Stop C3100
Bernard and Audre Rapoport Building Room 1.116
Austin, TX 78712
and NBER
dsmanoli@austin.utexas.edu

Nicholas Turner
Office of Tax Analysis
U.S. Department of the Treasury
1500 Pennsylvania Avenue, NW
Washington, D.C. 20220
Nicholas.Turner@treasury.gov

I. Introduction

Many studies have examined short-term effects of informational interventions in a variety of settings, but few studies have examined the long-term effects of these interventional interventions.[1] Do informational interventions create short-lived effects or permanent effects? The answer to this question has important implications for theories of incomplete take-up (Currie, 2006) and for interpreting the impacts of informational interventions. If providing individuals with benefit information results in permanent increases in take-up, this suggests that many individuals may not have been aware of their eligibility for benefits. In this case, a single intervention may be sufficient to increase take-up. On the other hand, if informational interventions only have short-lived effects, this suggests inattention may drive incomplete take-up and that repeated interventions may be necessary to reduce incomplete take-up.[2]

We study both short-term and longer-term effects of informational outreaches by the United States Internal Revenue Service (IRS). In an attempt to increase take-up of the Earned Income Tax Credit (EITC), the IRS sends notices to all taxpayers who appear eligible for the credit but who fail to claim it on their tax return. Exploiting two unique experimental settings, we quantify the impact of receiving a notice as well as the effects of different information presentations in the notices. In both settings, we study how taxpayers respond to these notices in the year that they receive the notice and in subsequent years. We distinguish between inattention and unawareness by examining both short-term effects which we term "nudge effects" separately from longer-term effects which we call "learning effects." To our knowledge, this analysis is the first to focus on longer-term effects of an informational intervention designed to increase benefit take-up.[3]

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[1] For examples, see Madrian and Shea (2001), Choi, Laibson, Madrian and Metrick (2002) and Thaler and Benartzi (2004), Hastings and Weinstein (2008), Brown, Kapteyn and Mitchell (2011), Chetty and Saez (2012), Bettinger, Long, Oreopoulos and Sanbonmatsu, (2012), Hoxby and Turner (2013).

[2] The IRS intervention is designed specifically to increase take-up of the Earned Income Tax Credit, a goal we take as given. We discuss prior work that suggests that incomplete take-up may be optimal and result from rational behavior in Section II.

[3] Prior work examines the longer-run impacts of interventions targeted at impacting behavior, such as electricity and water use (Allcott and Rogers 2013; Ferraro and Price 2013), academic performance (Levitt, List and Sadoff 2010) and smoking (Gine, Karlan and Zinman 2010), though not specifically take-up of a benefit program. Gallagher (2014) studies the take-up of flood insurance in a setting where prior floods serve as a key piece of information that affects agents' take-up behaviors.

Using population-level administrative income tax data, we provide multiple pieces of empirical analysis. First, we present a descriptive analysis that characterizes individuals who fail to claim the EITC when they appear eligible and therefore receive a notice from the IRS. Cross-sectional results show: (1) the likelihood of filing and not claiming the EITC does not decrease in the potential benefit amount as predicted by some theories of incomplete take-up (Moffitt 1983, Besley and Coate 1995, Blundell, Fry and Walker 1988, Kopczuk and Pop-Eleches 2007, Kroft 2008, and Kleven and Kopczuk 2011), and (2) the likelihood of responding to a notice does not increase in the potential benefit. Together, these findings suggest that individuals are unaware of the EITC benefit formula generally. Building on prior work (Chetty, Freidman and Saez, 2013) that finds regional differences in understanding of the EITC benefit formula, we document two additional findings across higher and lower knowledge areas: (1) individuals in higher EITC-knowledge areas are more likely to file and claim the credit and hence not receive notices, and (2) conditional on receiving a notice, individuals in higher EITC knowledge areas are less likely to respond to notices. This pattern suggests that areas with greater knowledge of the benefit schedule are also more aware of the EITC generally.

Second, we exploit a natural experiment to estimate the causal effects of receiving a notice on EITC take-up. In 2005, as a result of a computer glitch, some taxpayers who should have received a notice were omitted from the notice population. This glitch allows us to distinguish between a treatment group of taxpayers who received notices in 2005 and a control group of taxpayers who did not receive a notice in 2005 even though they should have received one. Using a difference-in-differences research design, we compare outcomes across these groups over time. The empirical results suggest that the IRS notices have a meaningful nudge effect, increasing EITC take-up by 30 percentage points for taxpayers with kids and by 60 percentage points for taxpayers without kids in 2005. For taxpayers who respond to the notice and claim the EITC in 2005, the credit represents on average roughly 4 percent of labor earnings. Despite this relatively large benefit, learning effects from the notices are much smaller than the nudge effect and quickly attenuate over time. Learning effects in the year after the notice are roughly 7 percentage points for returns with kids and 5 percentage points for returns without kids and three years later these learning effects are not substantively different than zero.

Third, the empirical analysis uses a randomized experiment where the IRS sent different types of notices to EITC-eligible taxpayers in California who filed 2009 tax returns but who did not claim the EITC or respond to an initial notice. This experimental setting allows us to quantify nudge effects and learning effects from notices that vary in their informational content among individuals who all receive a second IRS notice. Bhargava and Manoli (2014) study this setting and examine short-run responses to the different notices. We build on this work by studying the effects of the notices in the longer-run, allowing us to quantify learning effects from these additional notices. Like Bhargava and Manoli (2014) we find that there are meaningful short-term nudge effects for notices that make benefit amounts salient. Consistent with the results from the 2005 natural experiment, we find that learning effects are much smaller and quickly fade out over time. The results suggest that the most effective notice in this setting has a nudge effect on EITC take-up of roughly 13 percentage points relative to the baseline notice, but that the learning effect is less than 2 percentage points the following year.

Fourth, we use audit data to examine the impacts of audits on subsequent EITC participation. In particular, we study audits in which taxpayers initially filed tax returns and did not claim EITC benefits but following the audit were found to be eligible for EITC benefits and subsequently received EITC benefits. Intuitively, the audits may be more heavy-handed informational treatments than notification letters and hence may be more likely to lead to longer-term increases in EITC participation. However, consistent with the notice results, there is little evidence for long-term increases in EITC participation following the audits.

The remainder of this paper is organized as follows. Section II describes institutional background of the notification letters sent by the IRS and also the administrative tax data used in the empirical analysis. Section III presents a conceptual framework for filing and EITC claiming decisions in the presence of post-filing notification letters. Sections IV, V, VI and VII present the components of the empirical analysis, and Section VIII concludes.

II. Institutional Background & Data

A. Literature Review

One view of incomplete take-up is that it results from rational behavior. Individuals may optimally decline their benefits due to stigma or transaction costs (Moffitt, 1983, Currie and Grogger, 2001, Hernanz, Malherbet, Pellizzari 2004, and Currie 2006). Kleven and Kopczuk (2011) suggest that policymakers may enact complex program rules and difficult enrollment procedures to balance rejecting illegitimate claims with legitimate program use. These theories generally assume that agents are both perfectly rational and have complete information about their potential benefits.

There is also a large and growing literature on the effects of informational interventions on benefit take-up across a wide variety of settings. This work relaxes the assumption of complete information. Intuitively, if individuals are not perfectly informed about their benefits then we would expect the informational interventions to impact take-up. Consistent with this idea, many studies find that relatively low-cost informational interventions have meaningful effects on outcomes such as applying for federal student aid and enrolling in college (Bettinger, Long, Oreopoulos and Sanbonmatsu, 2012), applying to more selective colleges (Hoxby and Turner 2013), parents' decisions to send their children to higher-achieving schools (Hastings and Weinstein 2008), labor supply and earnings (Chetty and Saez, 2012), and social security claiming (Brown, Kapteyn and Mitchell 2011). There is also strong evidence that individuals are unaware of important retirement savings benefits (Madrian and Shea, 2001; Choi, Laibson, Madrian and Metrick, 2002; Thaler and Benartzi, 2004). In this paper, we build on this strand in the literature by quantifying the impact of IRS notices that inform taxpayers of the eligibility for the EITC on take-up in the notice year. We also measure the extent to which the notices teach individuals about the EITC generally by examining take-up in subsequent years.

B. The EITC & IRS Notices

The EITC is the largest cash assistance, anti-poverty program in the United States. Prior research has highlighted that the EITC has had positive impacts on labor force participation (Eissa and Hoynes 2006), positive impacts on earnings (Chetty Friedman and Saez 2013), positive impacts on consumption and food expenditures (Patel 2011, Goodman-Bacon and McGranahan 2008, and McGranahan and Schanzenbach 2013), positive impacts on infant health (Hoynes, Miller and Simon 2012), positive impacts on education (Dahl and Lochner 2012 and Manoli and Turner

2014), and small or negligible effects on marriage and fertility (Ellwood 2000, Dickert-Conlin and Houser 2002, and Baughman and Dickert-Conlin 2003). In 2013, eligible families can qualify for a refundable tax credit of just over $6,000. Eligibility is determined based on taxpayers' earned income, adjusted gross income, age, filing status and number of qualifying children. Appendix Figure 1 shows the corresponding EITC benefit schedules for two key tax years that we study, 2005 and 2009. EITC benefits phase in as earnings increase up to the first kink point, where taxpayers realize the maximum credit for the minimum amount of earnings. As earnings increase beyond the first kink point, the EITC amount stays constant until the second kink point. This second kink point is determined by filing status and number of qualifying children. (As shown in Panel B, families with three children received a larger EITC in 2009, which was the result of a policy change that took effect in that year.) As income increases beyond the second kink point, benefits phase out.[4]

To claim the EITC, taxpayers must file a tax return and taxpayers with qualifying kids must complete a Schedule EIC as part of their tax return to confirm information about their qualifying children. If taxpayer appears to be eligible for the EITC yet fails to claim credit, then the IRS mails a notice to the taxpayer.[5] These notices are sent within three to four months after receiving an eligible tax return.[6] The IRS estimates that roughly 20 percent of the notices are undeliverable due to incorrect addresses and/or the result of taxpayers moving to new addresses. Unfortunately, we are not able to identify which notices were undeliverable. To determine the notice population each tax year, the IRS applies a series of filters to ensure that the tax returns meet the EITC eligibility criteria. Plueger (2009) provides details on the filters applied by the IRS. Generally, IRS filters based on taxpayer and dependent ages, filing status and income eligibility criteria, as well as criteria such as having a valid SSN and having no prior disallowance of the EITC. Appendix Table A1 outlines the filters. We refer to taxpayers who

[4] The EITC is a function of both earnings (generally W2 earnings and self-employment income) and adjusted gross income. The EITC phases out once taxpayers have AGI above the second kink point; for these taxpayers the EITC is calculated as the minimum of the credit determined by AGI and the credit determined by earnings.
[5] Using tax data matched to data from the CPS, Plueger (2009) estimates that EITC take-up is roughly 75 percent. Of the 25 percent that fails to claim the credit, Plueger (2009) estimates that 9 percent are individuals who file taxes but do not claim the EITC.
[6] These notices are named CP09 and CP27 notices, with CP09 notices getting sent to taxpayers with children and CP27 notices getting sent to taxpayers without children.

appear eligible based on these filers as eligible taxpayers, even though some taxpayers who pass the screening filters may not be eligible.

The IRS mailings consist of a letter to inform taxpayers of the EITC and a worksheet to confirm the taxpayers' eligibility for the credit. (Appendix Figures 2 and 3 present examples of notices for taxpayers with kids and taxpayers without kids respectively.) Eligible taxpayers can complete the worksheet and return it to the IRS to claim their EITC. Taxpayers do not need to file an amended return to receive their benefits. For taxpayers with children, the information requested on the notice replicates information that would have been reported on Schedule EIC if the taxpayer claimed the EITC on the tax return. This additional information helps the IRS to accurately determine that individuals are truly eligible for the EITC.[7] (Appendix Figure 4 presents an example of a Schedule EIC.) For returns without kids, information on the tax return is almost sufficient to determine EITC eligibility, but IRS still sends simplified notices to these taxpayers, in part to have taxpayers validate the information on the tax form in order to reduce non-compliant credits and to verify residence requirements.[8]

Claiming the EITC in response to the notice may result both from the reminder nudge by the IRS and also from reduced transaction costs. After filling in the demographic information on the notice, taxpayers effectively have the IRS determine their EITC. This saves the taxpayer up to seventeen steps compared to claiming the credit on the tax return and figuring the value of the credit themselves.[9] In order to increase EITC take-up in subsequent years, the IRS also provides taxpayers with a lower transaction cost strategy for claiming the EITC on the tax return.[10] As described in the "what you can do next year" section of the notices, the IRS advises taxpayers that they can fill in schedule EIC and then write "EIC" on the tax return line. This strategy equalizes the transaction costs in claiming the EITC on the tax return or in response to the IRS notice—effectively saving the taxpayers the same steps as the notice. To the extent that

[7] Although the IRS observes that these returns have dependent children, the definition of qualifying children for the EITC is different from the definition used for personal exemptions. The additional information provided to IRS allows the determination of eligibility for the EITC.
[8] The IRS works hard to limit the number of improper EITCs awarded, and the IRS notices help insure access to the EITC for individuals who are eligible to receive the credit.
[9] The number of necessary steps to calculate EIC benefits is based on EIC Worksheet B, and it varies with or without self-employment income and with income above or below the second EITC kink point.
[10] The IRS also provides this guidance on the instructions for the 1040 form.

taxpayers adopt and understand this strategy, differences in EITC claiming in the notice year versus following years would be attributable to the reminder nudge of the notice.

C. 2005 Glitch

In order to quantify the impact of receiving a notice, we exploit an inadvertent error by the IRS that omitted some returns from the notice population. Due to this error, we are able to address selection into the notice population by examining the impact of receiving a notice *among returns that actually received a notice, or that should have received a notice absent the error*. The inadvertent error by IRS occurred only in 2005 and was the result of a computer glitch. In this year, taxpayers who used computer software to generate their returns but then mailed in paper versions of the return were omitted from the notice population. We refer to this filing method as "computer-paper." Taxpayers file computer-paper returns to utilize the benefits of tax software but avoid e-filing fees.[11] In a typical year, roughly 10 percent of returns are files in this way. Based on the 2005 computer glitch, we create a treatment group of taxpayers who received notices and a control group of computer-paper taxpayers who did not receive notices but who would have absent the glitch. We identify the causal effects of receiving a notice based on comparing outcomes across the treatment and control groups over time. Figure 1 shows the effect of this glitch in 2005, where control taxpayers (computer-paper returns) did not receive an IRS notice and treatment taxpayers (not computer paper returns) did receive notices. Figure 1 also shows that some taxpayers in both the treatment and control groups received notices in the years before and after the glitch. To control for different histories of IRS notices, we present results separately for taxpayers first in the notice population in 2005 (including those who should have received a notice).

D. 2009 CA Experiment

We also study the impact of receiving different information in the IRS notification by exploiting an experiment set up by the IRS and Bhargava and Manoli (2014). Typically, taxpayers in the

[11] Taxpayers commonly move into and out of computer-paper filing over time, though taxpayers who used computer-paper filing in 2005 are generally more likely to file in this way in other years. Among returns with kids who filed computer-paper in 2005, 47% filed this way in 2004 and 43% filed computer-paper in 2006. Among returns without kids using computer-paper in 2005 these shares were 44% in 2004 and 43% in 2006. By comparison, among returns with kids that did not file computer-paper in 2005 only 4% filed computer paper in 2004 and 4% filed computer-paper in 2006. Among returns with no kids who did not file computer computer-paper in 2005, 7% used computer-paper in 2004 and 6% used computer-paper in 2006.

notice population receive only a single notice from the IRS each tax year. In this experiment, the IRS sent second notices to taxpayers in California who had received an initial IRS notice for 2009 but did not respond. This analysis includes the following treatments: (1) simplified notices which aimed to reduce complexity by clarifying eligibility conditions and making response worksheets shorter and easier to read, (2) benefit notices which aimed to increase the salience of maximum credit amounts, (3) social influence notices which aimed to use information on peer take-up to influence responses, and (4) claiming time notices which aimed to reduce perceptions of the necessary time to respond to the notices.[12] Bhargava and Manoli (2014) discuss the experimental design in greater detail and analyze the short-term effects of the experimental notices. We extend this analysis by considering additional years and by differentiating between EITC claiming on the tax return and EITC claiming in response to IRS notices.

E. Audit to Claim EITC

To examine a potentially more aggressive informational intervention than the notification letters, the empirical analysis below also considers individuals who were audited in tax years 2006, 2007 2008 or 2009.[13] More specifically, the Audit Sample is restricted to individuals who were audited in these years and (1) did not initially claim the EITC pre-audit and (2) did claim the EITC post audit. The IRS selects tax returns for audit review quasi-randomly. It is not possible for us to explain the audit selection criteria; the specific details of the audit selection are not made publicly available by the IRS so that taxpayers cannot figure out how to evade audit selection. Because audit reviews can involve significant time with an IRS auditing agent and tax preparer, as well as significant time spent reviewing financial records and tax rules, the audit may represent a more aggressive informational intervention that teaches taxpayers more about the EITC than notification letters.

F. Data

[12] The treatment notices involved variations in the notice headlines. The simple notice headline was "You may be eligible for a refund"; the benefit notice headline was "You may be eligible for a refund up to $5,657"; the social influence notice was "You may be eligible for a refund. Usually, 4 out of every 5 people claim their refunds"; the claiming time notice headline was "You may be eligible for a refund. Claiming your refund usually takes less than 10 minutes."

[13] We restrict the sample to audits in these years only since consistent audit data is only available for these years. Data from earlier years is not comparable to data on from these years, and in some cases, not digitized. Audit data is made available internally 4 years after a given tax year. More recent audits may be ongoing.

We use population level administrative income tax data from the United States Internal Revenue Service for the empirical analysis. We construct the 2005 Analysis Sample by identifying individuals who received an IRS notice for tax year 2005. Next, we add individuals who filed computer-generated but paper-filed returns for tax year 2005 to this initial sample. These individuals should have received notices for tax year 2005 but did not because of an administrative computer glitch at the IRS. We identify just over 130,000 omitted tax returns, which is close to the estimate by the IRS that between 100,000 and 140,000 taxpayers were omitted as a result of the glitch (Plueger 2009). To study the impacts of the IRS notices over time, we construct a balanced nine year panel ranging from 2001 to 2009 based on each primary taxpayer in the notice sample in 2005.[14] Table 1 presents summary statistics for the analysis sample and for two comparison samples: (1) notice populations in 2004 and 2006 when there was no glitch and computer-paper returns were included in the notice population; and (2) a random sample of the full EITC population in 2005. (We discuss the summary in more detail in the next section.)

Table 2 presents summary statistics for the 2009 California Experiment sample and the Audit Sample. The 2009 California Experiment sample includes individuals who received an initial IRS notice but did not respond to it. For this sample, we construct a balanced seven year panel ranging from 2005 to 2011 for each primary taxpayer in the notice sample in 2009.[15] Table 2 presents summary statistics for this sample and for two comparison samples: (1) the 2009 California notice population; and (2) the 2009 California EITC population. (We discuss these summary statistics in more detail below.) For the Audit Sample, there are 100 individuals who meet the sample selection criteria. Overall, taxpayers in the Audit Sample tend to be lower income than the EITC population and the notice population. Similar to the notice population, the Audit Sample has more single taxpayers than the EITC population. Additionally, 57 taxpayers in the Audit Sample used paid tax preparers, which is slightly higher than the notice population and more similar to the EITC population.[16]

[14] We track this taxpayer over time, even if they are no longer a primary taxpayer.
[15] We track this taxpayer over time, even if they are no longer a primary taxpayer.
[16] 33 out of the 100 taxpayers in the Audit Sample also show up in the notice population since they received notices between 2005 and 2013. 32 out of the 33 taxpayers in Audit Sample who received notices received notices in the year they were audited.

III: Conceptual Framework

A. EITC-Claiming Decisions

In this section, we describe a simple conceptual framework for EITC-claiming decisions to provide context for the empirical analysis. The key assumptions in the framework are that taxpayers have imperfect information on EITC eligibility and on their potential benefit. Prior evidence indicates that individuals may be partly aware of the existence of the EITC, but they may not fully understand the benefit schedules (Romich and Weisner 2000, Smeeding, Phillips, and O'Connor 2000, Chetty and Saez 2013, and Bhargava and Manoli 2014).

Let $eitc_t$ denotes a taxpayer's true EITC benefit amount in year *t*, and suppose that $\widehat{eitc_t}$ denotes the taxpayer's perceived benefit amount in year *t*. We focus taxpayers who are eligible for positive EITC benefits in year *t*. We assume that taxpayers claim their EITC benefits if $\widehat{eitc_t} > c$ where *c* denotes transaction or stigma costs in dollars.[17] For simplicity, we assume that taxpayers who are ineligible for EITC benefits are not able to claim benefits.

We assume that there is *imperfect knowledge of eligibility*. With this assumption, there are two important cases for taxpayers who are actually eligible for EITC benefits: one, taxpayers correctly perceive that they are eligible, and two, taxpayers incorrectly perceive that they are ineligible. Let E_t denote the fraction of taxpayers with $eitc_t > 0$ who correctly perceive themselves to be eligible for EITC benefits for tax year *t*. Then, $1 - E_t$ denotes the fraction of taxpayers who incorrectly perceive that they are ineligible for EITC benefits, so $\widehat{eitc_t} = 0$ for this group.

We also assume that *conditional on perceived eligibility, taxpayers do not know their true EITC benefit amounts*. As a result of this assumption, we consider taxpayers who correctly perceive themselves to be EITC-eligible to have perceived EITC benefits drawn from a distribution *F(.)* defined on $(0,\overline{eitc})$ where $\overline{eitc}$ denotes the maximum perceived EITC benefit amount. Given the distribution *F(.)*, let θ denote the probability of drawing a perceived benefit greater than the transaction cost *c*.

Next, we introduce notices into the framework to illustrate how they may impact taxpayer behavior. Notices are only sent to eligible taxpayers who did not claim the credit. This group

[17] We include a positive transaction or stigma cost parameter *c > 0* to distinguish between incomplete take-up due to transaction costs and incomplete take-up due to misperceptions of eligibility.

includes eligible taxpayers who incorrectly perceive their eligibility ($\widehat{eitc_t} = 0$), or who correctly perceive their eligibility ($\widehat{eitc_t} > 0$) but who perceive the costs to be higher than their benefit ($\widehat{eitc_t} < c$). Let n_t denote the fraction of recipients who are attentive to the notices so that $n_t\theta$ reflects the overall effectiveness of the notices for both types of taxpayers who opt to respond. The additional benefit take-up due to the notices is then $(1 - E_t)n_t\theta$ and total EITC take-up in year *t* is $E_t\theta + (1 - E_t)n_t\theta$.

Beyond characterizing the initial response to the notice, we also incorporate dynamics across time with *potential decay in eligibility knowledge.* For simplicity, we assume that all eligible taxpayers in year *t* continue to be eligible in year *t+1*. In this case, the fraction of taxpayers who correctly perceive their eligibility in tax year *t+1* will be positively related to (1) persistence amongst taxpayers who initially correctly perceived their eligibility in tax year *t* and (2) persistence amongst taxpayers who received notices and learned about eligibility. More formally, the law of motion for the fraction correctly perceiving eligibility is given by

$$(1)\ E_{t+1} = \alpha E_t + \gamma(1 - E_t)n_t + \varepsilon_{t+1}$$

where the parameter α reflects the degree of persistence in eligibility perceptions for those correctly perceiving their eligibility and the parameter γ reflects the degree of persistence in learning from the notices, and ε_{t+1} drawn from a distribution *G(.)* defined on (0,1) captures random shocks in eligibility perceptions.[18]

B. Predictions

The main objective of the conceptual framework is to highlight several empirical predictions that we test in the data.

Prediction 1: Some EITC-eligible taxpayers will not claim their EITC benefits when filing their tax returns.

This prediction results from the assumption of imperfect information regarding eligibility and the costs of claiming the EITC. Formally the fraction of eligible taxpayers claim EITC benefits is given by $E_t\theta$, but all other eligible taxpayers will not claim the credit. In other words, taxpayers

[18] In this specification of the law of motion, we assume that the degrees of persistence for each group vary only by eligibility perceptions and not additionally based on whether or not EITC benefits are actually claimed. To allow for separate degrees of persistence for claimants and non-claimants within each eligibility-perception group, the law of motion can be generalized to be $E_{t+1} = \alpha_1 E_t\theta + \alpha_2 E_t(1 - \theta) + \gamma_1(1 - E_t)n_t\theta + \gamma_2(1 - E_t)n_t(1 - \theta) + \varepsilon_{t+1}$.

with $\widehat{eitc_t} < c$ will not claim the credit on the tax return even if their actual credit exceeds the costs of claiming ($eitc_t > c$) regardless if they correctly perceive themselves as eligible ($\widehat{eitc_t} > 0$). The framework also allows for heterogeneity in benefit and cost perceptions. Specifically, knowledge of eligibility and the benefit schedule may be more accurate in certain neighborhoods. The notices may have a larger effect where the gap between *c* and *c'* is largest. In these areas, the notices will cause relatively more people to respond to the notices. Baseline EITC take-up rates may result in heterogeneity in cost perceptions across neighborhoods. Intuitively, if more people claim the EITC in a given region, then there may be less stigma associated with claiming the credit.

Prediction 2: Some attentive taxpayers will respond to the notice, by claiming the EITC.

IRS notices could impact taxpayers in two ways. First, notices may cause some individuals with $\widehat{eitc_t} = 0$ to update their perceptions to $\widehat{eitc_t} > 0$. Such taxpayers should respond to the notices by claiming the credit as long as $\widehat{eitc_t} > c$. Second, notices may cause some taxpayers to update their beliefs on the cost of claiming the EITC because claiming the credit in response to the notice may be easier than claiming and figuring the EITC on the tax return. Let *c'* be the updated cost that taxpayers perceive after receiving a notice. Taxpayers who had an initial cost *c>c'* and that with $\widehat{eitc_t} < c$ but $\widehat{eitc_t} > c'$, will also respond to the notices by claiming the credit. Empirically, we will not be able to distinguish these two cases, but we can test the extent to which reduced transaction costs may drive the responses to the notices.

Prediction 3: There will not be a positive correlation between notice responses and potential benefit amounts if the perceived costs of claiming are small.

This prediction results from the assumption of imperfect information about the potential credit, even after taxpayers update their beliefs about eligibility. Intuitively, if the notices only provide taxpayers with the information that they are eligible ($eitc_t > 0$) then taxpayers should update their beliefs on eligibility ($\widehat{eitc_t} > 0$), but these taxpayers will still draw their perceived benefit from the distribution *F()*. As the costs of claiming the EITC approach zero, then all attentive taxpayers will respond to the credit, both those with high values and those with low values. As a result, there will not be a strong positive correlation between the potential EITC and the likelihood of responding to the notice.

Prediction 4: Some eligible individuals who claim the EITC in a given tax year will fail to claim the credit the next year even though they continue to be eligible.

Intuitively, decay in EITC knowledge could be driven by multiple factors such as switching between more and less knowledgeable tax preparers, forgetfulness, or because taxpayers perceive the notice as providing information only for year *t*. If eligible taxpayers who claim the EITC on Form 1040 continue to claim the EITC on Form 1040 and rarely end up receiving notices, then α will be close to 1 in Equation (1). However, if many taxpayers who claim the EITC in tax year *t* end up receiving notices in tax year $t+1$, then the parameter α will be closer to 0. In addition, if notices only get recipients to learn about their eligibility in tax year *t* and not in future years, then γ will be closer to 0. On the other hand, if notices effectively teach taxpayers about program eligibility rules, as opposed to just getting taxpayers to learn about their eligibility in a single tax year, then the parameter γ will be closer to 1.

IV: Empirical Analysis 1: The IRS Notice Population

A. Cross-Sectional Description

Consistent with the first prediction of the model, Table 1 shows that between 645,000 and 775,000 taxpayers receive IRS notices for tax years 2004 to 2006, reflecting that not all eligible taxpayers claim the credit. Column (1) shows the 2005 Analysis Sample, which includes taxpayers who received notices as well as the taxpayers with computer-paper returns who should have received notices. The sample means in Table 1 are for a single year, denoted in each column heading (with all dollar amounts in $2011). The summary statistics highlight that the notice populations have lower wages and adjusted gross income relative to taxpayers who claim the EITC on their tax return in 2005 (Column (4)). Much of this difference is attributable to the fact that notice recipients are far less likely to have EITC qualifying children and the EITC for taxpayers with kids is available at relatively higher income levels (see Appendix Figure 1).[19] Notice recipients in 2005 are also less likely to be self-prepared compared to taxpayers who claim the EITC on their tax return in that year, though a sizeable percentage (roughly 20%) of notice recipients still use paid tax preparers. Table 1 also shows that the 2005 Analysis Sample

[19] Plueger (2009) estimates take-up rates to be 56%, 74% and 86% amongst tax returns with 0, 1 or two or more qualifying children respectively.

has similar observables as the notice populations in 2004 and 2006 (Columns (2)-(3)). In particular, the fraction filing computer-paper is very similar across the three years.

There are meaningful differences across the treatment and control groups in the 2005 Analysis Sample because the computer glitch did not impact a random sample of taxpayers. Table 1 columns (5) and (6) show summary statistics for 2005 for the treatment and control groups separately. On average, taxpayers in the control group have higher wages, are more likely to have self-employment income, are more likely to be married, are more likely to have kids, and are more likely to have paid tax-preparation compared to the control group (all of these differences are significant at the 0.05 percent level). Although not shown, we find a similar pattern across the computer-paper and not-computer-paper groups in 2004 and 2006. Our identification allows for *level differences* in key outcomes including EITC take-up and EITC amounts across the treatment and control groups, but requires that these groups have *parallel trends* in the years prior to 2005. (We find support for this assumption and we discuss this point further in Section IV.) We also find support for parallel trends in a key observable characteristic. Figure 2 plots earnings (wages plus Schedule C income) over time for the treatment and control groups separately. In the years prior to 2005, the figures suggest that the treatment and control groups had similar trends in earnings.[20]

Roughly 35,000 taxpayers received a second IRS notice as part of the 2009 California experiment.[21] Table 2 presents summary statistics based on 2009 data for these taxpayers (Column (1)), for all California taxpayers who received notices for 2009 (Column (2)), and for California taxpayers who claimed the EITC in 2009 (Column (3)). Table 2 has many of the same patterns as in Table 1. In particular, taxpayers in the notice samples are less likely to have kids, compared to the EITC population and therefore have on average lower incomes and lower wages. Yet, individuals in the California Experimental sample who received a second notice have similar observables to individuals the notice sample in California in 2009. As assignment to

[20] Although not shown, we find support for parallel trends in observable other characteristics, including earnings, using a regression framework to explicitly test for different trends across groups in the years prior to the glitch.
[21] As discussed in Bhargava and Manoli (2014), the initial target population for the California experimental included 45,099 taxpayers. However, some taxpayers were excluded due to potentially inaccurate information on mailing addresses (7,096 excluded) and the number of possible qualifying children (2,953 excluded). Thus, the resulting experimental sample consists of 35,050 taxpayers. One additional observation was dropped for the analysis in this paper because it was not possible to match the taxpayer identifier to other administrative data.

a given group is random, there are no meaningful differences across treatment groups within the 2009 Experimental Sample for these observable characteristics. Table 2 columns (4) through (8) show summary statistics for 2009 across the various treatment groups.

We do not find evidence that failing to claim an EITC on the tax return, and thus receiving a notice, decreases in the potential benefit. This pattern contrasts with some theories of take-up that suggest individuals have a (noisy, but monotonic) signal of their actual benefit so that take-up increases in potential benefit (Moffitt 1983, Besley and Coate 1995, Blundell, Fry and Walker 1988, Kopczuk and Pop-Eleches 2007, Kroft 2008, and Kleven and Kopczuk 2011). Figure 3 plots the number of notice recipients divided by the number of taxpayers who claim the EITC on the tax return in each potential EITC benefit bin for the 2005 Analysis Sample.[22] For returns with kids (Panel A), this ratio increases with potential benefits. For returns without kids (Panel B), the ratio is virtually flat, suggesting that the likelihood of receiving a notice is not related to potential benefits. In both Panels A and B of Figure 3, the patterns are not consistent with taxpayers who are fully informed of their benefits but fail to claim an EITC because it is not worth much money.

Figure 4 shows the likelihood of responding to a notice does not increase with potential benefits. This figure plots the fraction of notice recipients responding to a notice by potential benefit bin for the 2005 Analysis Sample.[23] For returns with kids (panel A), the likelihood of responding to a notice decreases with potential benefits. Though the likelihood of response does not increase with potential benefits as a traditional model of take-up would predict, the selection evidence from Figure 3 Panel A makes the plot in Figure 4 Panel A difficult to interpret. For returns without kids (Figure 4, Panel B), the likelihood of responding to a notice does not change with potential benefits. These patterns are consistent with the second and third predictions from the conceptual framework. Specifically, because of inattention regarding eligibility and imperfect information about benefits, some attentive taxpayers will respond to the notices and responses will not be correlated with potential benefit amounts.

[22] We scale the counts of notices by the counts of EITC taxpayers to take account of the underlying income distribution of taxpayers.
[23] Although not shown, we find the patterns in Figures 4 & 5 hold in other years.

Chetty Friedman and Saez (2013) document substantial heterogeneity in understanding of the EITC benefit schedule across geography. To explore the extent to which these neighborhood knowledge differences impact notice receipt and responses, Figures 6 and 7 show the relationships between notices and EITC knowledge in 2005.[24] These figures use the Chetty, Friedman and Saez (2013) measure of knowledge, defined as the number of EITC recipients with kids that have self-employed earnings and who have earned income close to the first kink point in the EITC schedule ("sharp bunchers") divided by the number of EITC recipients in each three-digit ZIP code. This measure proxies for understanding of the EITC benefit schedule, as sharp bunchers receive the maximum EITC by reporting the minimum level of income.

Figure 5 shows the amount of sharp bunching on the x-axis and the share of notices on the vertical axis using a binned scatterplot.[25] This figure demonstrates that the likelihood of receiving a notice declines with the knowledge measure. Intuitively, individuals in high knowledge areas may be more likely to claim the EITC on their tax returns, and as a result are less likely to receive a notice. Figure 6 plots the probability of responding to a notice on the vertical axis and the share of sharp bunchers on the x-axis using a binned scatterplot. [26] This figure shows that, conditional on receiving a notice, the likelihood of responding declines with the EITC knowledge measure. One explanation for this pattern may be that EITC-eligible taxpayers who fail to claim EITC benefits on the tax return in high knowledge are particularly inert or inattentive given that they failed to gleam information about the EITC in a relatively higher knowledge area.

These empirical results are consistent with the conceptual framework. Specifically, the conceptual framework can account for neighborhood effects by assuming that benefits are more accurately perceived in more knowledgeable areas, and claiming costs may be distributed with a flat tail so that non-claimants in high knowledge areas are particularly inert and the notices are more likely to get marginal individuals in less knowledgeable areas to claim benefits.

[24] Although not shown, we find that the patterns in Figures 6 & 7 also hold in other years.
[25] The figures plot the average Chetty, Freidman and Saez (2013) sharp bunching measure and the average count of notices per EITC claimed on the F1040 for 50 equally sized groups based on the amount of sharp bunching. The fitted line estimates the relationship from the underlying ZIP3 level data.
[26] The figures plot the average Chetty, Freidman and Saez (2013) sharp bunching measure and the average probability of responding to the notice for 50 equally sized groups based on the amount of sharp bunching. The fitted line estimates the relationship from the underlying ZIP3 level data.

B. Panel Description

One reason that many taxpayers receive a notice is that they are not typically EITC eligible, so claiming the credit is not a standard part of their typical tax return. Figure 7 plots the probability that a taxpayer is eligible for the EITC over time for the 2005 Analysis Sample (the notice population) and for taxpayers who claimed the EITC on the tax return in 2005 (the EITC population).[27] Eligibility is measured as having a valid EITC claim on a 1040 or receiving a notice. In 2005, all of these taxpayers are eligible for the EITC by construction of the sample. In 2004, roughly 40 percent of taxpayers with kids who are in the 2005 Analysis Sample are eligible. By comparison, among taxpayers with kids who claim the EITC on the tax return in 2005, roughly 80 percent are eligible in 2004. A similar pattern holds for returns with no kids in 2005. Changes in income and family structure account for some of the changes in eligibility. Figure 8 shows average AGI over time for the 2005 Analysis Sample and for EITC returns in 2005. For the notice population, taxpayers have relatively lower AGI in 2005 compared to earlier or later years, though this pattern is roughly comparable to taxpayers who claimed the EITC on the tax return in 2005. Figure 9 plots the number of dependent children claimed on the tax return over time for the 2005 Analysis sample and for the 2005 EITC population. This figure demonstrates that family structure changes impact eligibility. For example, among returns with no kids in the 2005 Analysis Sample, the average number of kids in 2003 and 2004 is greater than zero. Taxpayers who had kids in earlier years but who do not claim kids in 2005 may not be aware of their eligibility for the no-child EITC.[28] Among taxpayers with kids in 2005, the average number of kids in 2005 is different than preceding years, a pattern is consistent with Tong (2014) who finds that a relatively high fraction of EITC eligible children change tax units over time.[29]

V. Empirical Analysis 2: Effects of Getting a Notice

A. Identification Strategy & Regression Specification

[27] In this section we focus on the 2005 Analysis Sample. The patterns are similar in other years and for the 2009 CA sample.

[28] Take-up rates across families with kids suggest different patterns of EITC awareness. Plueger (2009) reports that take-up of the no-child EITC is 56 percent, compared to 74 percent for taxpayers with one child and 86 percent for taxpayers with two children.

[29] Tong (2014) finds that 20 (50) percent of EITC qualifying children experience a tax unit change over two (six) years.

To identify the causal effects of receiving a notice on taxpayer behavior, we use a difference-in-differences identification. We define the treatment group as individuals who received notices for 2005 and the control group as individuals who should have received notices for 2005 but did not (taxpayers with computer-paper returns).

We estimate the following regression specification

$$y_{it} = \beta_0 + \beta_1 T_i + \sum_{k=2004}^{2009} \beta_{2k}[1(Year_t = k)] + \sum_{k=2004}^{2009} \beta_{3k}[1(Year_t = k) * T_i] + \delta' X_{it} + \varepsilon_{it}$$

in which T_i is an indicator equal to 1 if taxpayer i is in the treatment group (defined by 2005 return method) and $1(Year_t = k)$ is an indicator equal to 1 if *Year t* is equal to k. The sample includes a balanced panel of nine years centered at the glitch year (the glitch year is 2005 and the panel covers from 2001 through 2009). In this specification, the effects in 2004 through 2009 are measured relative to the outcome averages for 2001 through 2003. We include coefficients to look at the effects for 2004 through 2009 to confirm that the notice effects emerge in 2005 and not in prior years (we consider additional combinations of omitted/included years in Appendix Tables A2 & A3). We consider multiple outcome variables y_{it}, including claiming EITC, EITC amounts, and EITC eligibility. In all cases, we measure outcomes in 2005 as responses to the IRS notice, but for other years (2001-2004 and 2006-2009) we separately consider EITC claiming on the 1040 form and any EITC claiming (either on the 1040 or in response to IRS notices). X denotes a vector of covariates, including dummies for $1000 earnings bins, 5 year age bins (primary taxpayer age), number of kids, joint filing status, paid tax preparation, $100 bins of tax refunds and 3-digit ZIP code fixed effects. The covariates are based on characteristics from the 2005 tax returns.

The coefficients β_{3k} are the coefficients of interest as they capture the difference between the treatment and control groups in 2004 and later years, relative to earlier years. These differential effects in 2005 can be interpreted as the immediate impact of the notice, which we refer to as the "nudge" effect. Intuitively, the notice directly informs taxpayers of their eligibility in that year and allows them to claim the EITC simply by responding to the notice. Beyond giving the taxpayer information on their eligibility for the EITC in 2005, the notice tries to teach the

taxpayer about the EITC more generally and provides taxpayers with a strategy to make EITC claiming on the tax return easier (see section IIA and Appendix Figures 2 and 3). The differential effects in 2006 through 2009 test whether taxpayers adopt this proposed strategy and/or whether the notice translates into learning about the EITC generally, which we refer to as "learning" effects.

Figure 10 highlights variation in EITC claiming that we use in our identification. This figure plots the mean likelihood that taxpayers claim the EITC (on either the tax return or in response to an IRS notice) over time for the treatment and control groups respectively. The patterns in Figure 10 suggest that IRS notices have an effect on EITC use in both the notice year and in following years. The empirical results build on this figure, by quantifying the difference in EITC claiming across the treatment and control groups in later years, compared to earlier years. Figure 10 also suggests that taxpayers in the treatment and control groups satisfy a key assumption of the difference-in-differences research design, that these groups have common time trends in the years prior to the glitch. (We find support for this assumption using several tests and we discuss these results in the Appendix.)

B. 2005 Glitch Estimation Results

Consistent with the fourth prediction of the conceptual framework, the empirical results suggest that the notices have meaningful nudge effects and that learning effects are small and quickly fade out over time. Figure 11 shows this pattern by plotting the regression coefficients and standard errors of the estimated differential effects in 2004 and later years for EITC claiming on either the tax return or in response to IRS notices. (Note that the shaded region shows the 95 percent confidence interval of the estimate.) In Panel A of Figure 11, the nudge effect for taxpayers with kids is roughly 33 percentage points, compared to a learning effect of about 7 percentage points in 2006. In Panel B for returns without kids, the nudge effect is more than 60 percentage points, compared to a learning effect in 2006 of about 5 percentage points.[30] The differential effects are virtually zero in 2004, which confirms that these groups did not have

[30] These effects represent the lower-bound of the response to the notices among the eligible population for two reasons. First, roughly one-in-five notices are not deliverable. Second, some taxpayers who appear eligible for the EITC are actually not eligible. We cannot differentiate non-responses from undelivered mail or from taxpayers with private information about their ineligibility who do not respond.

meaningful differences in EITC take-up prior to the 2005 glitch.[31] Tables 3 and 4 present the baseline results for taxpayers with and without kids respectively for various outcomes including claiming the EITC on the tax return (Take EITC F1040), any EITC claiming including both responses to IRS notices and claiming on the tax return (Take EITC total), EITC benefits on Form 1040 (EITC Amount 1040) and EITC amount on the return or in response to the notice (EITC Amount Total). In Table 3, the learning effects go from about 7 percentage points in 2006 to 3 percentage points in 2007 and are not statistically different from zero in 2008 or 2009. A similar pattern holds in Table 4, where the learning effect is 3-5 percentage points in 2006 and quickly fades out in later years. This implication also holds when using the EITC amount rather than a 0-1 measure of EITC claiming. For example, in Table 3 the nudge effect is roughly $430, whereas the learning effect in 2006 is about $125, declining to about $50 in 2007.

To make sure that the full sample results in Tables 3 and 4 are not confounded by differences in notice histories prior to 2005 between the treatment and control group, we also present results in columns 3 and 4 or Tables 3 and 4 in which the sample is restricted to individuals first in the notice population (i.e. not having received prior notices) in 2005. These results are nearly identical to the full sample results, thereby suggesting that differences in notice histories prior to 2005 do not confound the main results.

Additionally, we highlight the 2006 results for the sample first in the notice population in 2005. Intuitively, EITC claiming on the 1040 in 2006 for taxpayers first in the notice population in 2005 provides a relatively cleaner test of learning effects, compared to using the entire 2005 Analysis Sample and all later years. Limiting the sample to taxpayers first in the notice population in 2005 insures that the treatment group received their first notice in 2005 and the control group did not receive a notice in 2005 or in any prior year. Furthermore, claiming the EITC on the 1040 in 2006 excludes any notice effects for the control group, so it directly tests the extent to which the notice in 2005 for the treatment group translates into EITC claiming on the tax return the following year. As shown in Tables 3 and 4, these learning effects are comparable to those of the notice population a whole. While the effect in 2006 is relatively clean

[31] Appendix Table A2 includes interactions prior to 2004 and also confirms that the notice effects first emerge in TY2005.

to interpret as learning from the 2005 notice alone, the differential effects in later years should be viewed as learning effects from the running count of notices, where the treatment group has on average a higher count of total notices received.

We next turn to considering whether the notices teach taxpayers that waiting for a notice may be a strategic way to claim EITC benefits. The notice does not inform the taxpayer that they will receive a notice in each year that they are eligible, but taxpayers who receive a notice in 2005 may come to believe that they can only claim EITC benefits if they receive a notice. If taxpayers who receive a notice in 2005 learned to wait for a notice to claim their EITC, then we would expect to see a relatively larger learning effect for any EITC claiming in 2006 compared to claiming the EITC on the 1040 in 2006 since the any EITC claiming outcome includes notice responses and claiming on the tax return whereas the 1040 claiming outcome does not. Intuitively, taxpayers who strategically wait for a notice should respond to the notice but not take the EITC on the 1040. In both Tables 3 and 4 for all taxpayers in the 2005 Analysis Sample as well as for taxpayers in the notice population for the first time in 2005, the differences across the learning effects in 2006 for EITC claiming on the 1040 relative to any EITC claiming are relatively small. This suggests that few taxpayers anticipate subsequent notices as a method of claiming the EITC.

We also study how receiving a notice in 2005 affects subsequent notice receipt. Intuitively, some of the differences between the treatment and control group in claiming EITC benefits may be due to differences in subsequent notice receipt. In Table 5 we present results on the likelihood that taxpayers receive a notice in later years. While these results are statistically significant, they are too small to account for the differences in EITC claiming. For example, for the returns with kids, the 2005 treatment group is roughly 8 percentage points more likely to claim EITC benefits (Table 3 column 2, 2006 interaction), but the difference in the probability of receiving a notice is only about 2 percentage points (Table 5 column 1, 2006 interaction). For the returns with no kids, the control group is more likely to receive subsequent notices, but these results are also very small relative to the EITC take-up results in Table 4.

Lastly, we examine whether the effects of the EITC notices vary across neighborhoods with different knowledge of the EITC. To explore this possibility, we examine differential effects of the notices using the EITC self-employed knowledge measure from Chetty, Friedman and Saez (2013). Using this measure at the three-digit zip code level, we split the analysis sample into knowledge quintiles.[32] Figure 12 plots the estimated regression coefficients for the highest and lowest knowledge groups. (Appendix Table A10 presents the estimated regression coefficients for all knowledge groups.) For returns with kids, the nudge effects and learning effects are strongest in the lowest knowledge areas. This finding, in combination with Figure 5 that shows there is a negative relationship between the bunching knowledge measure and notice receipt, suggest that taxpayers who have not been nudged by local knowledge of the EITC are less likely to be nudged or informed by IRS notices. Intuitively, if taxpayers in high knowledge areas are immune to the diffusion of EITC knowledge within their neighborhood, they are likely hard to reach and/or convince that EITC claiming is beneficial. Yet, the pattern across knowledge areas does not hold for returns without kids. (We show the regression results in Table A10, which suggest the differential nudge effects range from 0.64 in the least knowledgeable areas to 0.61 in the most knowledgeable areas.) One reason for this pattern may be that the Chetty Friedman and Saez (2013) knowledge measure is based on returns with kids and may not accurately reflect knowledge differences across geographic areas for taxpayers without children.[33]

VI. Empirical Analysis 3: Effects of Different Notices

A. Identification Strategy & Regression Specification

To further investigate the causal effects of IRS notices on taxpayer behavior, we consider the effects of receiving simpler notices with more salient benefit information versus more complicated and less salient notices. (See Bhargava and Manoli (2014) for a more detailed discussion of this experiment.) If the causal effects of notices are concentrated only in the nudge effect, then one would expect that the differences between groups receiving simpler and more salient notices versus more complicated notices are also concentrated in the notice year. To test

[32] Specifically, all of the three-digit ZIP codes are ranked based on the bunching measure, and then the ZIP codes are split into five equally-sized groups. Based on their three-digit ZIP codes off of their 2005 tax returns, individuals are then sorted into these knowledge groups, and the same regression specification from above is separately estimated for the sample within each group.

[33] We explored generating a comparable bunching measure using only returns with no kids, but there is no meaningful bunching at the first kink point for returns without kids.

this hypothesis, we exploit the randomized variation in notice messaging from the 2009 CA experiment. The identification strategy compares outcomes for taxpayers who were randomly assigned to different treatment groups with simpler and more salient notices or more complicated notices as described in Bhargava and Manoli (2014). Compared to the earlier analysis there are two key differences in this experiment. First, the sample includes individuals who did not respond to the initial notice, which may include relatively less responsive taxpayers. Second, the results can differentiate the effect of more versus less complicated and salient notices, rather than simply the effect of receiving a notice versus not receiving a notice.

To test the effects of the different treatments, we estimate the following regression specification

$$y_{it} = \beta_0 + \beta_{1k} \sum_k 1(T_i = k) + \sum_{s=2008}^{2011} \beta_{2s}[1(Year_t = s)] + \sum_k \sum_{k=2008}^{2011} \beta_{3k,s}[1(Year_t = s) * 1(T_i = k)] + \delta' X_{it} + \varepsilon_{it}$$

where T_i captures the taxpayer's 2009 treatment group k = simple, benefit, social, or time. The variable y_{it} denotes outcome y for taxpayer i in tax year t, including take-up of EITC benefits as well as EITC amounts claimed. The key coefficients of interest are the coefficients on the interactions between the year dummies and the treatment group dummies, $\beta_{3k,s}$. These coefficients capture the differences between the treatment groups across the different tax years. As with the 2005 glitch regression specification, we include interactions for one-year prior to the experiment (the experiment was in 2009) to confirm that the experimental effects emerge in the experimental year and not prior.

B. CA Experiment Estimation Results

Consistent with the results examining the impact of notice receipt, the results for the 2009 CA experiment suggest that the nudge effect from a simpler or more salient notice is larger than the learning effects. Figure 13A illustrates the main results from the analysis of the CA experiment, with evidence that there are different nudge effects across the experimental groups in 2009. (Note that this figure does not plot the confidence intervals. Only the benefit notice has a

significant non-zero effect and then only in 2009. All other notices in all years are not significantly different than zero.) As reported in Bhargava and Manoli (2014), the benefit salience treatment has the largest positive effect on take-up of EITC benefits in the notice year (2009), while the remaining treatments all have similar effects on EITC take-up. The plot highlights that the differences between the treatments did not persist beyond the notice year of 2009, suggesting that even notices with a stronger nudge did not translate into meaningful learning about the EITC generally and also that the effects are not present in 2008. Table 6 shows the regression results that correspond to the coefficients in Figure 13A as well as the results for other outcome variables. Consistent with the take-up results, the benefit salience notices increased EITC amounts in TY2009, but not in subsequent tax years relative to the other treatments.

VII. Empirical Analysis 4: Effects of Audits

A. Audit Event Study

In addition to examining effects of notices which may be considered relatively soft touches or passive interventions, we also examine effects from potentially more invasive interventions. The more aggressive interventions we consider are audits by the IRS, focusing on a group of taxpayers who are randomly selected for an audit. In particular, the analysis focuses on taxpayers who filed a tax return and did not claim EITC benefits, but was found to be eligible for EITC benefits following the random audit.[34] This analysis allows us to test whether providing taxpayers information about their EITC eligibility through an audit has a relatively stronger effect than learning about eligibility through an IRS notice. The more heavy-handed intervention from an audit may be more likely to teach taxpayers about the EITC and have more persistent impacts that notification letters.

Audit data is available for tax years 2006 through 2009 from the IRS National Research Program (NRP) database.[35] In these data, there are 100 taxpayers who meet our criteria of: filing a tax return in the audit year and not claiming an EITC; ruled eligible for the EITC following the

[34] We do not examine audits in which a taxpayer initially claimed EITC benefits but was then later found to be ineligible for EITC benefits because such taxpayers may face 2- or 10-year bans for claiming EITC benefits as a result of IRS enforcement and non-compliance rules. This would mechanically affect and hence undermine an event study research design.

[35] We are very grateful to Melissa Vigil for help with the audit event study analysis.

audit; received the EITC as a result of the audit. For these taxpayers, we create a panel dataset based on 2001 through 2013. We then implement an event study research design to examine behavior before and after the audit. Specifically, we define event time as year since the audit, *evtime=year-audit_year*, and we estimate the following specification,

$$y_{it} = \sum_{k=2001}^{2013} \beta_k 1(year_t = k) + \sum_{k=-5}^{5} \delta_k 1(evtime_t = k) + \varepsilon_{it}$$

where y_{it} denotes claiming EITC benefits at time t for individual i. We also pool pre- and post-audit years and estimate the following specification,

$$y_{it} = \sum_{k=2001}^{2013} \beta_k 1(year_t = k) + \delta^{pre} 1(evtime_t < 0) + \delta^0 1(evtime_t = 0)$$
$$+ \delta^1 1(evtime_t = 1) + \delta^{post} 1(evtime_t > 1) + \varepsilon_{it}.$$

With this specification, we can test compare δ^1 and δ^{post} to δ^{pre} to test if there are short-term and long-term effects following the audit.

B. Event Study Results

Figure 13B presents the average fraction of individuals in the audit sample who claim EITC benefits by event time. Prior to the audit year, the average fraction of taxpayers claiming EITC benefits is roughly 0.33. In the year of the audit, the fraction of individuals who claim EITC benefits on the initially filed tax returns and the fraction of individuals who ultimately received EITC benefits following the audit are 0 and 1 respectively because of how the sample is defined. Following the audit, the fraction of individuals receiving EITC benefits is slightly higher than the pre-audit fractions, though there is still some suggestive evidence of fade-out from 1 year following the audit to more than 1 year after the audit. This pattern is consistent with the results of the IRS notices, though the learning effects appear to be relatively larger.

Table A11 presents the regression evidence corresponding to the specifications above and the graphical evidence. The regression specifications control for calendar year fixed effects in addition to looking at the patterns by event time. The regression results shows that 1 year after the audit, the fraction of individuals claiming EITC benefits does appear to be higher than just before the audit. Specifically, the estimated fraction of individuals claiming EITC benefits one year prior to the audit is 0.366, and the estimated fraction one year after the audit is 0.459. These point estimates indicate a relatively large impact of the audit on subsequent claiming since they

suggest roughly a 25% (=(0.459-0.366)/0.366) increase in the fraction claiming EITC benefits. However, we cannot reject that these fractions are equal (and that there are no short-term effects) given the relatively large standard errors. Any effects of the audit appear to fade out as there does not appear to be a significant difference in the fraction of individuals claiming EITC benefits prior to the audit and more than 2 year after the audit. While this evidence is somewhat suggestive given the small sample size, it does appear to indicate little evidence of substantial learning about EITC benefits following the audit.

VIII. Conclusion

We find consistent evidence across multiple settings that many individuals forgo substantial benefits from the tax system because they are unaware of their eligibility and they do not appear to permanently learn about eligibility rules. While there does not appear to be much evidence of learning, the empirical results suggest that informing taxpayers of their eligibility for the EITC has meaningful effects on short-run take-up. The results from the 2005 natural experiment suggest that the IRS notices have a meaningful effect on EITC take-up in the notice year. Among returns that got the notice in that year, over 55 percent responded to the notice, thereby receiving on average $200 from the EITC for 2005. This translates into roughly an additional $120 million in EITC for more than 320,000 families. The results from the 2009 California experiment suggest that clear eligibility notices with salient benefit information could increase the number of EITCs claimed in response to the notices by an additional by 90,000 each year if they were used nationally. Yet in both experimental settings, as well as in an event study based on audits, we find that nudges and audits from the IRS do not translate into learning about the EITC more generally, despite the fact that the EITC represents a potentially large increase in after-tax income. This suggests that informing individuals of their immediate eligibility may be easier than teaching them about their potential benefits and the tax code generally. Prior work (Chetty and Saez 2013) also finds that teaching individuals about potential benefits from the tax system is difficult and that individuals do not have a full understanding of the tax system (Chetty Looney and Kroft 2009; Feldman, Katuscak and Kawano 2014). Determining how individuals learn about benefit programs and the federal income tax code and designing experiments and interventions to test these ideas remain important areas for future research.

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Table 1: Sample Means, 2005 Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Notice Samples			**EITC Sample**	**2005 Analysis Sample**	
	2005 (Analysis Sample)	2004	2006	2005	Computer-Paper (control group)	Not Computer-Paper (treatment group)
Wages	10437	11494	10418	15965	12623	10125
	[10772]	[11539]	[10885]	[11811]	[13093]	[10523]
Has positive wages	0.81	0.81	0.81	0.91	0.71	0.83
	[0.39]	[0.40]	[0.40]	[0.30]	[0.46]	[0.37]
Schedule C Income	1015	1079	1038	1564	2346	709
	[4110]	[4295]	[4173]	[15794]	[6215]	[3376]
Has Schedule C Income	0.20	0.19	0.19	0.20	0.35	0.15
	[0.36]	[0.37]	[0.37]	[0.40]	[0.48]	[0.35]
Adjusted Gross Income	11787	12826	11659	17580	14699	11118
	[10944]	[11606]	[10667]	[36724]	[12738]	[10374]
Joint Tax Filer	0.17	0.17	0.16	0.23	0.26	0.15
	[0.38]	[0.38]	[0.37]	[0.42]	[0.44]	[0.35]
Has Kids	0.34	0.39	0.33	0.78	0.65	0.26
	[0.80]	[0.84]	[0.79]	[0.36]	[1.06]	[0.70]
Number of Kids	0.38	0.45	0.38	2.34	0.77	0.28
	[0.80]	[0.84]	[0.80]	[1.12]	[1.04]	[0.70]
Paid Tax Preparer	0.19	0.22	0.21	0.58	0.56	0.11
	[0.80]	[0.44]	[0.40]	[0.35]	[0.50]	[0.31]
Computer-paper	0.19	0.19	0.18	0.16	1	0
	[0.39]	[0.39]	[0.38]	[0.25]	[0]	[0]
Notices	0.81	1	1	0	0	1
	[0.39]	[0]	[0]	[0]	[0]	[0]
Observations	712,498	775,600	645,280	4,431,231	133,239	579,709

Notes: Table shows mean values with standard deviations in brackets. Column (4) is based on a 20% random sample.

Table 2: Sample Means, 2009 California Sample & EITC Audit Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		(9)
	California Notice Samples		**California EITC Sample**	**California 2009 Experimental Sample**						**Audit Sample**
	2009 Experimental	2009	2009	Complex (omitted	Simple	Benefit	Social	Time		
Wages	13476	12255	15355	13780	13778	13725	13824	13610	Wages	6172
	[14823]	[13886]	[13014]	[14930]	[14889]	[14803]	[14754]	[14795]		[10435]
Has positive wages	0.82	0.84	0.84	0.85	0.85	0.85	0.85	0.84	Has positive wages	0.48
	[0.38]	[0.37]	[0.36]	[0.36]	[0.36]	[0.36]	[0.36]	[0.36]		[0.50]
Schedule C Income	1297	1182	2551	1234	1125	1218	1132	1095	Schedule C Income	-9
	[6992]	[10623]	[38641]	[6171]	[6809]	[6306]	[8534]	[6348]		[26809]
Has Schedule C Income	0.25	0.25	0.28	0.24	0.23	0.22	0.23	0.23	Has Schedule C Income	0.61
	[0.44]	[0.44]	[0.44]	[0.42]	[0.42]	[0.42]	[0.42]	[0.42]		[0.49]
Adjusted Gross Income	14336	11121	18124	13836	12829	14111	12959	13360	Adjusted Gross Income	6651
	[54504]	[68771]	[197372]	[28834]	[51520]	[23801]	[43340]	[30461]		[30744]
Joint Tax Filer	0.27	0.25	0.29	0.27	0.27	0.27	0.28	0.27	Joint Tax Filer	0.12
	[0.44]	[0.44]	[0.46]	[0.44]	[0.44]	[0.44]	[0.45]	[0.44]		[0.33]
Has Kids	0.35	0.26	0.71	0.33	0.33	0.33	0.33	0.33	Has Kids	0.24
	[0.48]	[0.44]	[0.46]	[0.47]	[0.47]	[0.47]	[0.47]	[0.47]		[0.43]
Number of Kids	0.72	0.48	1.25	0.68	0.70	0.69	0.70	0.69	Number of Kids	0.49
	[1.08]	[0.95]	[1.12]	[1.08]	[1.09]	[1.07]	[1.09]	[1.08]		[1.11]
Paid Tax Preparer	0.38	0.35	0.72	0.38	0.37	0.38	0.38	0.36	Paid Tax Preparer	0.57
	[0.48]	[0.48]	[0.46]	[0.48]	[0.48]	[0.49]	[0.48]	[0.48]		[0.50]
Computer-paper	0.19	0.19	0.15	0.18	0.18	0.18	0.18	0.18	Fraction Audited in 2006	0.49
	[0.39]	[0.39]	[0.36]	[0.39]	[0.38]	[0.38]	[0.38]	[0.39]	Fraction Audited in 2007	0.18
									Fraction Audited in 2008	0.18
Notices	2	1	0	2	2	2	2	2	Fraction Audited in 2009	0.15
	[0]	[0]	[0]	[0]	[0]	[0]	[0]	[0]		
Observations	35,049	76,907	2,991,886	3,676	17,541	6,761	3,596	3,475	Observations	100

Notes: Table shows mean values with standard deviations in brackets.

Table 3: Diff-in-Diffs Estimates, 2005 sample returns with kids

	Analysis Sample				Sample with First Notice in 2005			
	Take EITC F1040	Take EITC Total	EITC Amount F1040	EITC Amount Total	Take EITC F1040	Take EITC Total	EITC Amount F1040	EITC Amount Total
Treatment*2004	0.0152	0.0294	17.57	33.42	0.0196	0.0207	23.15	24.79
	[0.00257]	[0.00270]	[5.728]	[6.168]	[0.00292]	[0.00293]	[6.718]	[6.809]
Treatment*2005	0.344	0.339	429.4	422.4	0.361	0.361	429.1	428.9
	[0.00563]	[0.00565]	[10.15]	[10.15]	[0.00588]	[0.00591]	[10.79]	[10.82]
Treatment*2006	0.0739	0.0819	124.4	133.7	0.0759	0.0841	121.9	128.2
	[0.00353]	[0.00363]	[8.577]	[8.840]	[0.00402]	[0.00412]	[9.740]	[9.971]
Treatment*2007	0.0343	0.0358	54.09	51.08	0.0321	0.0353	44.91	42.12
	[0.00350]	[0.00361]	[8.979]	[9.105]	[0.00402]	[0.00407]	[10.19]	[10.26]
Treatment*2008	-0.000454	-0.00367	-8.788	-19.47	-0.00433	-0.00548	-20.33	-29.34
	[0.00361]	[0.00367]	[9.499]	[9.566]	[0.00394]	[0.00397]	[10.20]	[10.28]
Treatment*2009	-0.0110	-0.0138	-23.34	-36.34	-0.0143	-0.0139	-33.97	-43.21
	[0.00389]	[0.00394]	[10.43]	[10.58]	[0.00412]	[0.00415]	[10.98]	[11.19]
2004	-0.0113	-0.000479	-71.78	-54.70	0.00953	0.0101	-43.49	-42.56
	[0.00234]	[0.00252]	[5.628]	[5.942]	[0.00245]	[0.00243]	[6.191]	[6.189]
2005	-0.251	-0.256	-472.7	-480.3	-0.266	-0.268	-507.9	-511.2
	[0.00275]	[0.00278]	[6.505]	[6.554]	[0.00298]	[0.00298]	[7.072]	[7.099]
2006	-0.0100	0.00503	-58.55	-31.69	-0.0118	0.00407	-67.01	-39.79
	[0.00292]	[0.00302]	[7.081]	[7.299]	[0.00328]	[0.00339]	[7.968]	[8.157]
2007	0.0164	0.0264	33.06	53.81	0.0109	0.0226	19.01	41.38
	[0.00283]	[0.00285]	[7.600]	[7.695]	[0.00310]	[0.00309]	[8.165]	[8.182]
2008	0.0355	0.0433	91.63	108.8	0.0266	0.0365	71.55	91.13
	[0.00312]	[0.00313]	[8.168]	[8.238]	[0.00337]	[0.00336]	[8.565]	[8.620]
2009	0.0798	0.0858	246.1	264.1	0.0690	0.0772	226.0	246.0
	[0.00307]	[0.00313]	[8.786]	[9.028]	[0.00310]	[0.00316]	[9.040]	[9.344]
Treatment	0.0323	0.0375	104.6	116.5	0.0451	0.0455	137.7	141.0
	[0.00290]	[0.00291]	[7.185]	[7.228]	[0.00316]	[0.00318]	[7.913]	[7.960]
Constant	0.221	0.227	387.6	364.0	0.229	0.230	409.5	397.3
	[0.00959]	[0.00958]	[26.06]	[26.62]	[0.0105]	[0.0106]	[28.43]	[28.80]
Individuals	151,687	151,687	151,687	151,687	121,293	121,293	121,293	121,293
Observations	1,365,183	1,365,183	1,365,183	1,365,183	1,091,637	1,091,637	1,091,637	1,091,637

Notes: All specifications include controls based on the 2005 tax return, including indicators for $1000 earnings bins, 5 year age bins (primary taxpayer age), number of kids, joint filing status, paid tax preparation, $100 bins of tax refunds and 3-digit zipcode fixed effects. Standard errors are clustered at the 3-digit zipcode level and reported in brackets.

Table 4: Diff-in-Diffs Estimates, 2005 sample returns with no kids

	Analysis Sample				Sample with First Notice in 2005			
	Take EITC F1040	Take EITC Total	EITC Amount F1040	EITC Amount Total	Take EITC F1040	Take EITC Total	EITC Amount F1040	EITC Amount Total
Treatment*2004	-0.00871	0.0220	4.809	13.07	-0.0175	-0.0164	0.0670	0.844
	[0.00163]	[0.00163]	[2.527]	[2.499]	[0.00181]	[0.00181]	[2.847]	[2.865]
Treatment*2005	0.632	0.624	167.0	164.9	0.641	0.642	160.2	160.5
	[0.00241]	[0.00242]	[3.248]	[3.274]	[0.00258]	[0.00259]	[3.691]	[3.715]
Treatment*2006	0.0289	0.0516	14.06	19.55	0.0294	0.0502	11.49	16.51
	[0.00203]	[0.00210]	[3.028]	[3.028]	[0.00231]	[0.00236]	[3.584]	[3.550]
Treatment*2007	0.0128	0.0282	-1.348	1.921	0.0133	0.0293	-4.865	-1.224
	[0.00204]	[0.00216]	[3.225]	[3.262]	[0.00227]	[0.00235]	[3.772]	[3.791]
Treatment*2008	0.00306	0.0140	-14.52	-13.31	0.00450	0.0153	-16.01	-14.56
	[0.00220]	[0.00221]	[3.512]	[3.529]	[0.00235]	[0.00236]	[4.010]	[4.035]
Treatment*2009	-0.00609	0.000760	-36.19	-35.56	-0.00395	0.00531	-36.04	-34.69
	[0.00230]	[0.00231]	[3.958]	[3.996]	[0.00247]	[0.00247]	[4.351]	[4.369]
2004	0.0248	0.0660	-42.41	-32.57	0.0466	0.0464	-35.95	-36.37
	[0.00156]	[0.00156]	[2.424]	[2.406]	[0.00175]	[0.00176]	[2.726]	[2.755]
2005	-0.177	-0.192	-173.2	-177.7	-0.183	-0.184	-192.4	-193.4
	[0.00214]	[0.00221]	[3.532]	[3.570]	[0.00239]	[0.00243]	[4.035]	[4.070]
2006	0.0766	0.128	-24.89	-11.37	0.0790	0.130	-32.11	-18.67
	[0.00193]	[0.00208]	[2.965]	[2.965]	[0.00214]	[0.00224]	[3.478]	[3.440]
2007	0.0871	0.114	11.04	18.44	0.0823	0.112	4.642	12.79
	[0.00197]	[0.00205]	[3.203]	[3.239]	[0.00217]	[0.00218]	[3.761]	[3.771]
2008	0.0748	0.0851	30.47	34.63	0.0654	0.0835	21.42	27.52
	[0.00220]	[0.00217]	[3.594]	[3.605]	[0.00232]	[0.00228]	[4.135]	[4.163]
2009	0.0817	0.0845	73.29	75.57	0.0705	0.0823	64.75	69.31
	[0.00216]	[0.00213]	[4.063]	[4.074]	[0.00236]	[0.00233]	[4.471]	[4.462]
Treatment	-0.0323	-0.0247	-0.424	1.477	-0.0248	-0.0264	13.18	12.80
	[0.00176]	[0.00177]	[3.372]	[3.372]	[0.00191]	[0.00193]	[3.881]	[3.882]
Constant	0.141	0.166	85.86	86.65	0.224	0.221	253.9	253.1
	[0.00289]	[0.00303]	[4.173]	[4.214]	[0.00270]	[0.00280]	[4.658]	[4.683]
Individuals	561,261	561,261	561,261	561,261	398,013	398,013	398,013	398,013
Observations	5,051,349	5,051,349	5,051,349	5,051,349	3,582,117	3,582,117	3,582,117	3,582,117

Notes: All specifications include controls based on the 2005 tax return, including indicators for $1000 earnings bins, 5 year age bins (primary taxpayer age), number of kids, joint filing status, paid tax preparation, $100 bins of tax refunds and 3-digit zipcode fixed effects. Standard errors are clustered at the 3-digit zipcode level and reported in brackets.

Table 5: Diff-in-Diffs Estimates, 2005 sample other outcomes

	Notice Receipt	Notice Receipt
Treatment*2004	-0.0195	0.00844
	[0.00269]	[0.00178]
Treatment*2005	0.977	0.965
	[0.00180]	[0.000921]
Treatment*2006	0.0168	-0.00267
	[0.00216]	[0.00177]
Treatment*2007	-0.00549	-0.00406
	[0.00128]	[0.00139]
Treatment*2008	-0.0131	-0.00703
	[0.00154]	[0.00121]
Treatment*2009	-0.0112	-0.00976
	[0.00147]	[0.00104]
2004	0.0954	0.0750
	[0.00272]	[0.00186]
2005	-0.0385	-0.0690
	[0.00235]	[0.000922]
2006	0.0440	0.0797
	[0.00174]	[0.00170]
2007	0.0120	0.0161
	[0.00105]	[0.00127]
2008	-0.00509	-0.0152
	[0.00140]	[0.00113]
2009	-0.0109	-0.0314
	[0.00144]	[0.000950]
Treatment	0.0371	0.0366
	[0.00101]	[0.000924]
Constant	0.00721	0.0974
	[0.00530]	[0.00164]
Individuals	151,687	561,261
Observations	1,365,183	5,051,349

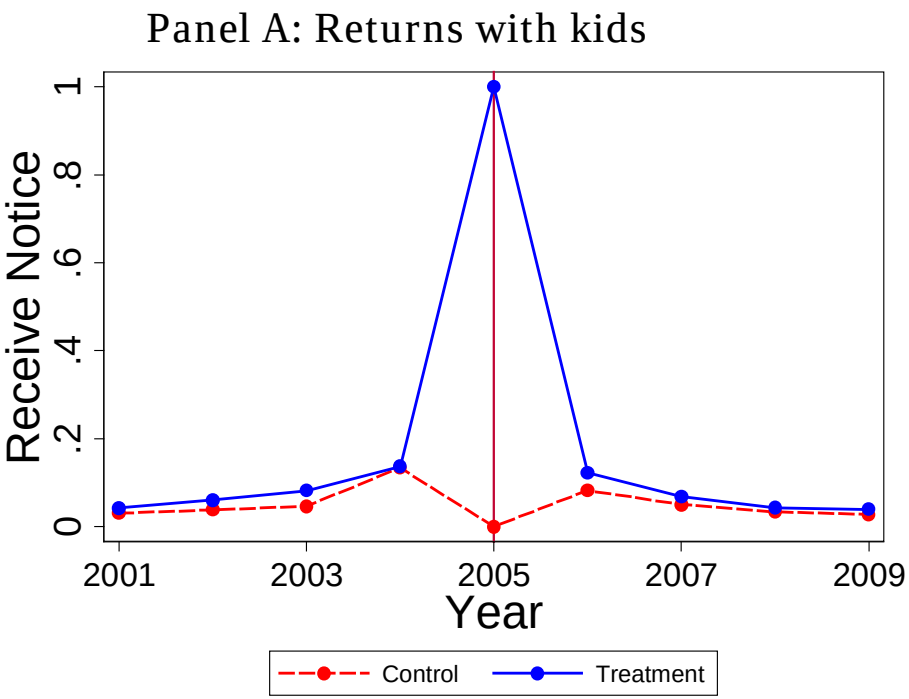
Notes: All specifications include controls based on the 2005 tax return, including indicators for $1000 earnings bins, 5 year age bins (primary taxpayer age), number of kids, joint filing status, paid tax preparation, $100 bins of tax refunds and 3-digit zipcode fixed effects. Standard errors are clustered at the 3-digit zipcode level and reported in brackets.

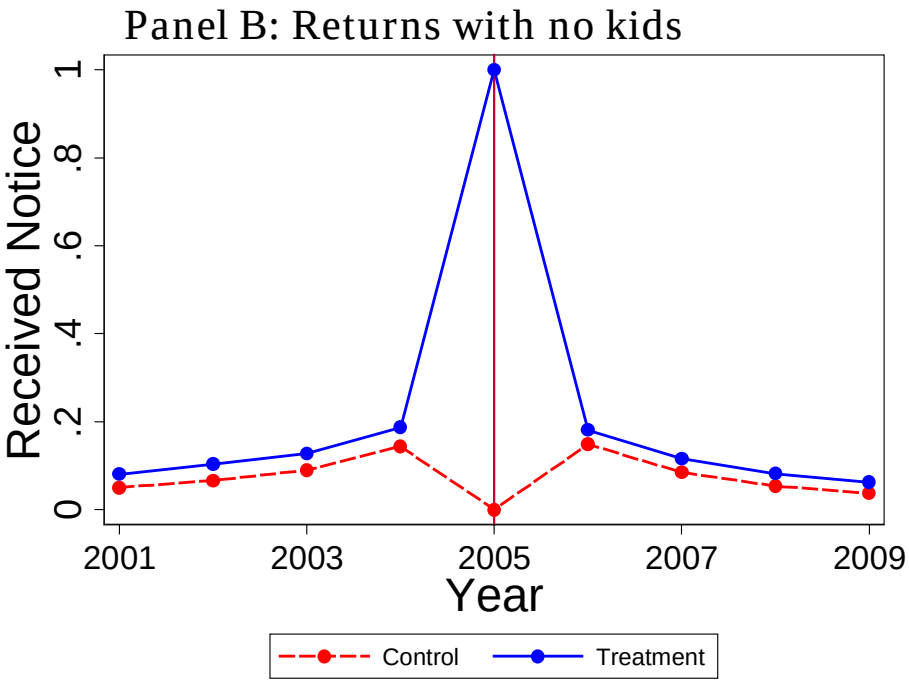
Table 6: Diff-in-Diffs Estimates, 2009 California Sample

	Take EITC F1040	Take EITC Total	EITC Amount F1040	EITC Amount Total
Simple*2008	-0.00105	-0.00684	-19.94	-22.35
	[0.00710]	[0.00700]	[10.83]	[10.58]
Simple*2009	0.0447	0.0454	15.94	15.86
	[0.00782]	[0.00789]	[12.82]	[12.59]
Simple*2010	-0.000500	0.000708	1.652	3.284
	[0.00913]	[0.00883]	[13.44]	[13.44]
Simple*2011	-0.00712	-0.00755	-21.38	-23.09
	[0.00791]	[0.00820]	[16.62]	[15.39]
Benefit*2008	-0.00344	-0.00877	-31.61	-35.46
	[0.00816]	[0.00744]	[12.44]	[12.39]
Benefit*2009	0.127	0.128	54.46	54.38
	[0.0111]	[0.0112]	[13.90]	[13.61]
Benefit*2010	0.00703	0.0129	-11.50	-2.726
	[0.00927]	[0.00958]	[13.80]	[13.12]
Benefit*2011	-0.0193	-0.0151	-38.58	-35.85
	[0.00961]	[0.00978]	[18.46]	[17.06]
Social*2008	0.00151	0.00168	-13.13	-18.56
	[0.00940]	[0.00886]	[13.69]	[13.40]
Social*2009	0.0127	0.0172	-16.95	-14.86
	[0.0116]	[0.0122]	[19.36]	[19.37]
Social*2010	-0.0189	-0.0158	-34.26	-32.30
	[0.0119]	[0.0119]	[19.68]	[19.28]
Social*2011	-0.0253	-0.0233	-42.24	-44.01
	[0.0116]	[0.0125]	[22.79]	[21.95]
Time*2008	-0.00865	-0.0106	-24.40	-27.54
	[0.00902]	[0.00912]	[15.59]	[14.76]
Time*2009	0.0302	0.0312	4.869	4.623
	[0.0113]	[0.0117]	[16.97]	[16.42]
Time*2010	-0.0128	-0.00886	-24.31	-16.27
	[0.0111]	[0.0114]	[19.97]	[17.78]
Time*2011	-0.0247	-0.0247	-43.30	-43.87
	[0.0112]	[0.0120]	[27.05]	[25.29]
2008	0.00250	0.0256	-43.81	-33.48
	[0.00566]	[0.00565]	[9.235]	[9.162]
2009	0.0221	-0.00157	-110.8	-120.1
	[0.0104]	[0.0108]	[16.22]	[16.11]
2010	0.102	0.107	46.19	51.31
	[0.00824]	[0.00868]	[12.68]	[13.52]
2011	0.0879	0.0920	78.88	82.93
	[0.00721]	[0.00824]	[16.66]	[15.71]
Simple	0.00692	0.00635	16.45	16.27
	[0.00493]	[0.00475]	[10.12]	[9.824]
Benefit	0.00883	0.00798	27.13	27.02
	[0.00627]	[0.00641]	[12.50]	[12.43]
Social	0.0155	0.0114	23.45	21.73
	[0.00646]	[0.00720]	[15.70]	[15.73]
Time	0.0145	0.0132	24.12	24.11
	[0.00736]	[0.00738]	[15.34]	[14.74]
Constant	0.207	0.220	331.9	332.7
	[0.0167]	[0.0174]	[43.75]	[43.52]
Individuals	35,049	35,049	35,049	35,049
Observations	245,343	245,343	245,343	245,343

Notes: All specifications include controls based on the 2009 tax return, including indicators for $1000 earnings bins, 5 year age bins (primary taxpayer age), number of kids, joint filing status, paid tax preparation, $100 bins of tax refunds and 3-digit zipcode fixed effects. Robust standard errors are reported in brackets.

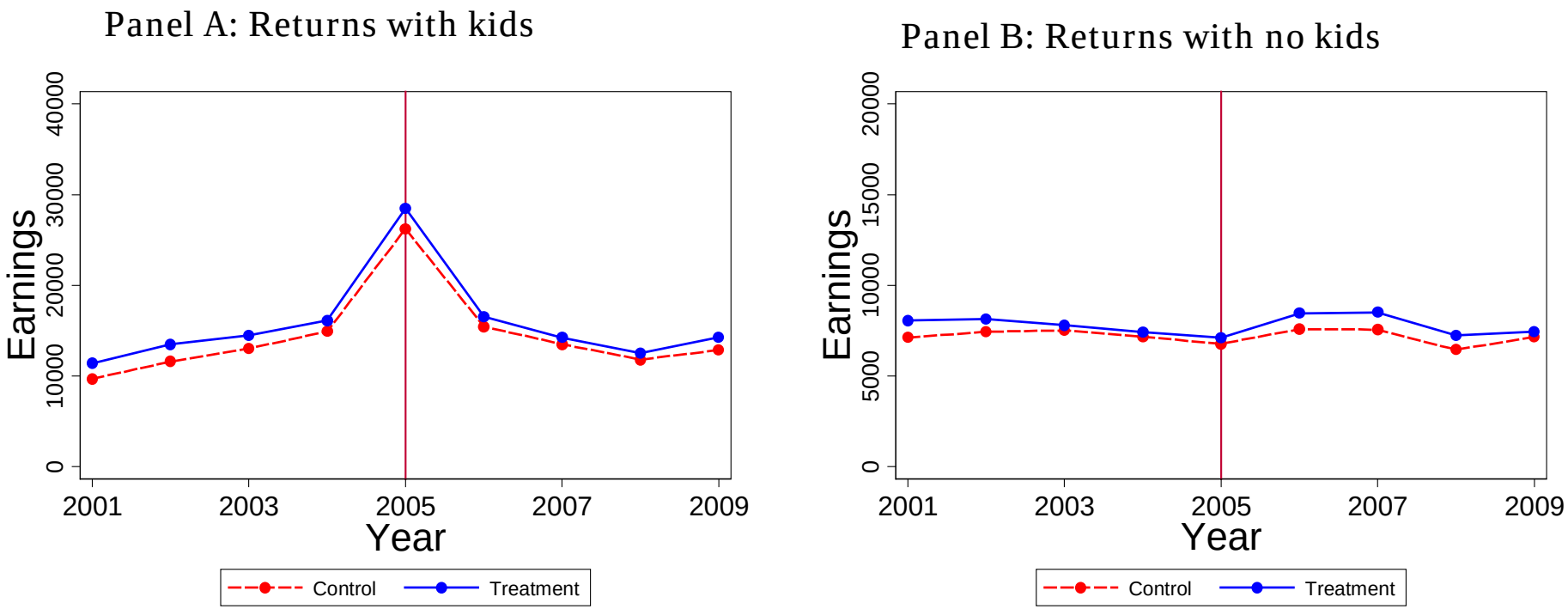
Figure 1: Probability of Notice over time, 2005 Analysis Sample





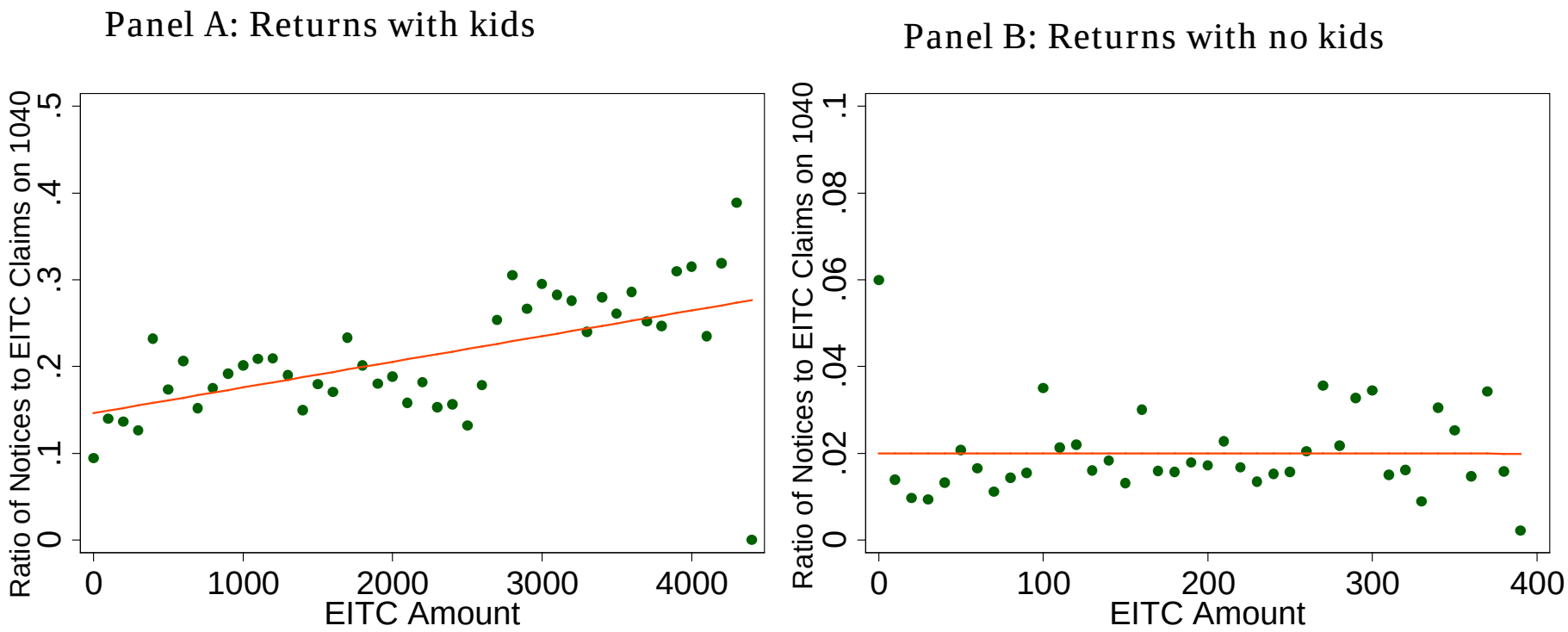
Notes: These figures plot the probability that the primary taxpayers in the 2005 Analysis Sample receive an IRS notice in 2005 and in the four prior tax years or the four following tax years.

Figure 2: Earnings over time, 2005 Analysis Sample



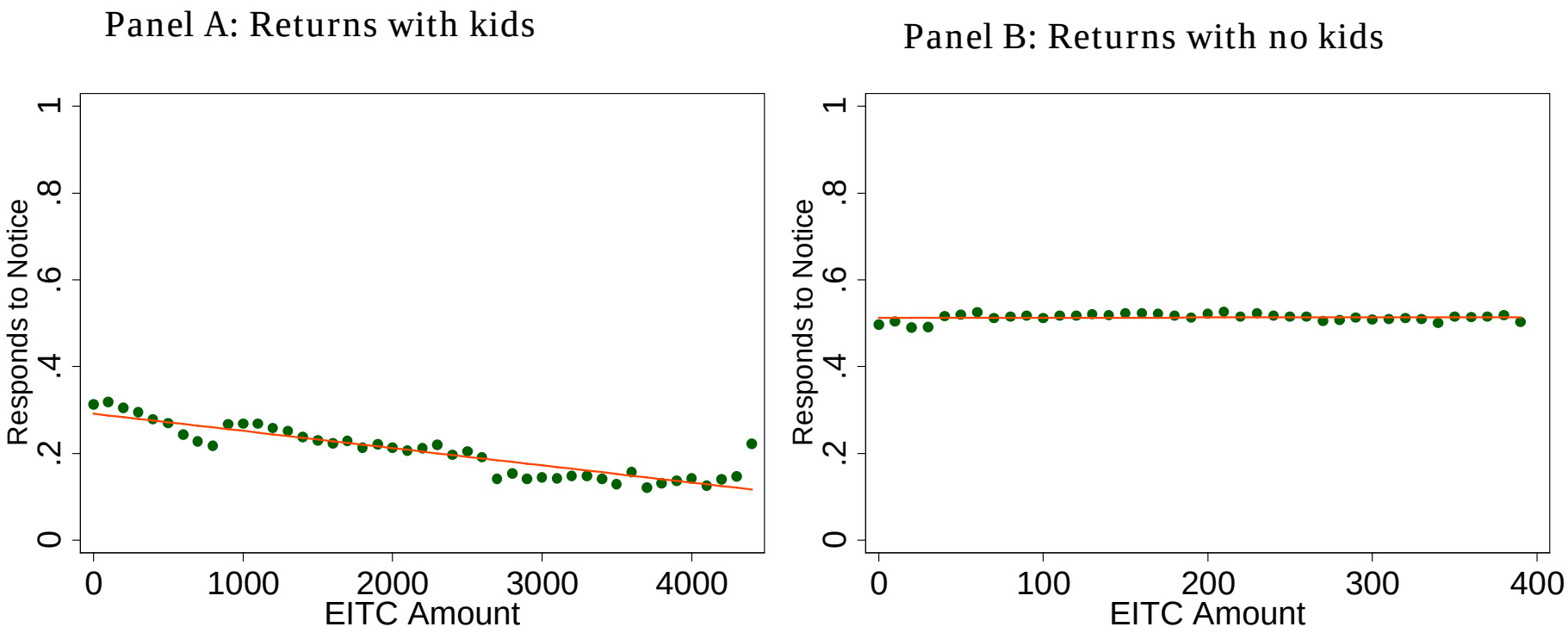
Notes: These figures plot earnings (W2 wages plus Schedule C income in $2011) for primary taxpayers in the 2005 Analysis Sample for tax years from 2001-2009.

Figure 3: Notices Relative to EITC Claims on Tax Returns



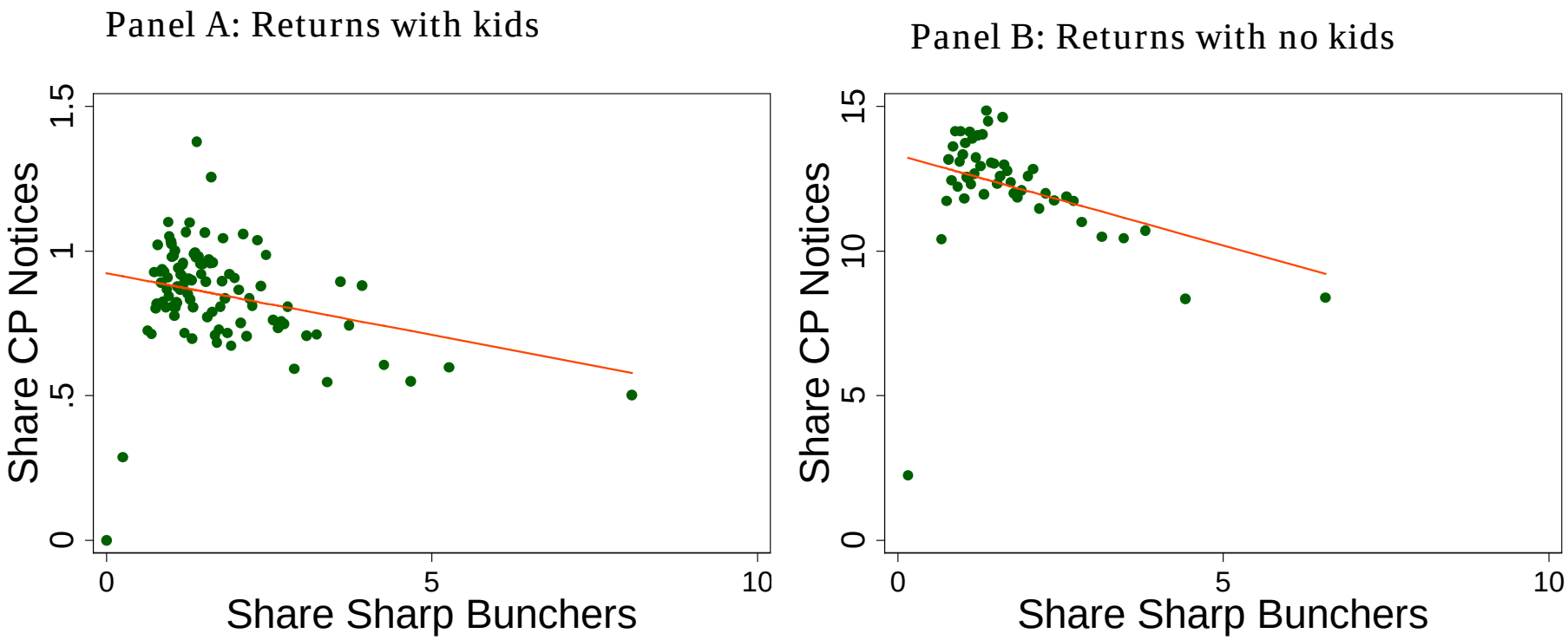
Notes: These figures plot the count of IRS notices relative to the number of EITCs claimed on tax returns (vertical axis) by EITC amount (horizontal axis). Panel A uses $100 bins and Panel B uses $10 income bins. The data are for 2005 though this pattern holds in other tax years. The fitted line and regression coefficients are estimated on the binned data. The regression estimates in Panel A are 2.96e-5[6.87e-7] on EITC and 0.146[0.0175] on the constant and in Panel B are 1.89e-7[1.42e-5] on EITC and 0.0199[0.0032] on the constant.

Figure 4: Response Rate to IRS Notices



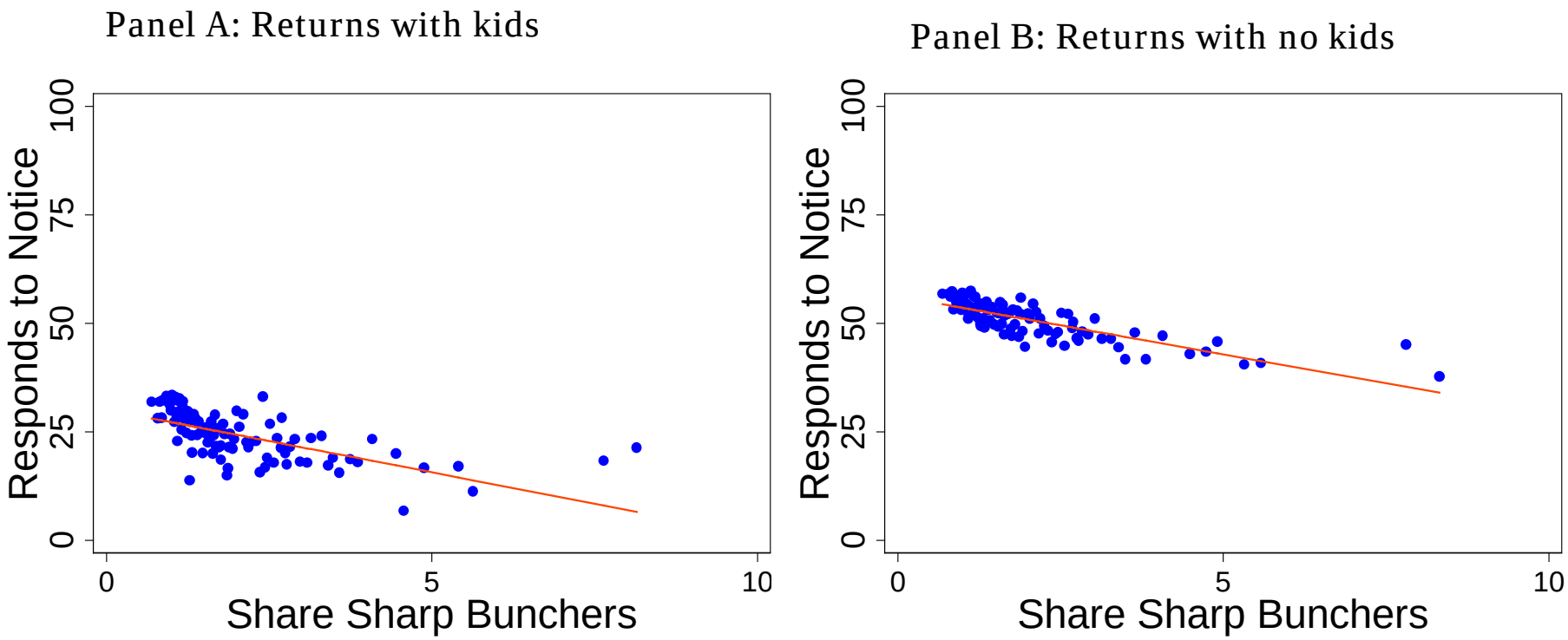
Notes: These figures plot the average response rate to IRS (vertical axis) by EITC amount (horizontal axis). Panel A uses $100 bins and Panel B uses $10 income bins. The data are for 2005 though this pattern holds in other tax years. The fitted line and regression coefficients are estimated on the binned data. The regression estimates in Panel A are -3.97e-7[2.87e-6] on EITC and 0.291[0.0073] on the constant and in Panel B are 5.55e-6[1.16e-5] on EITC and 0.512[0.0026] on the constant.

Figure 5: IRS Notices and EITC Knowledge, by ZIP3



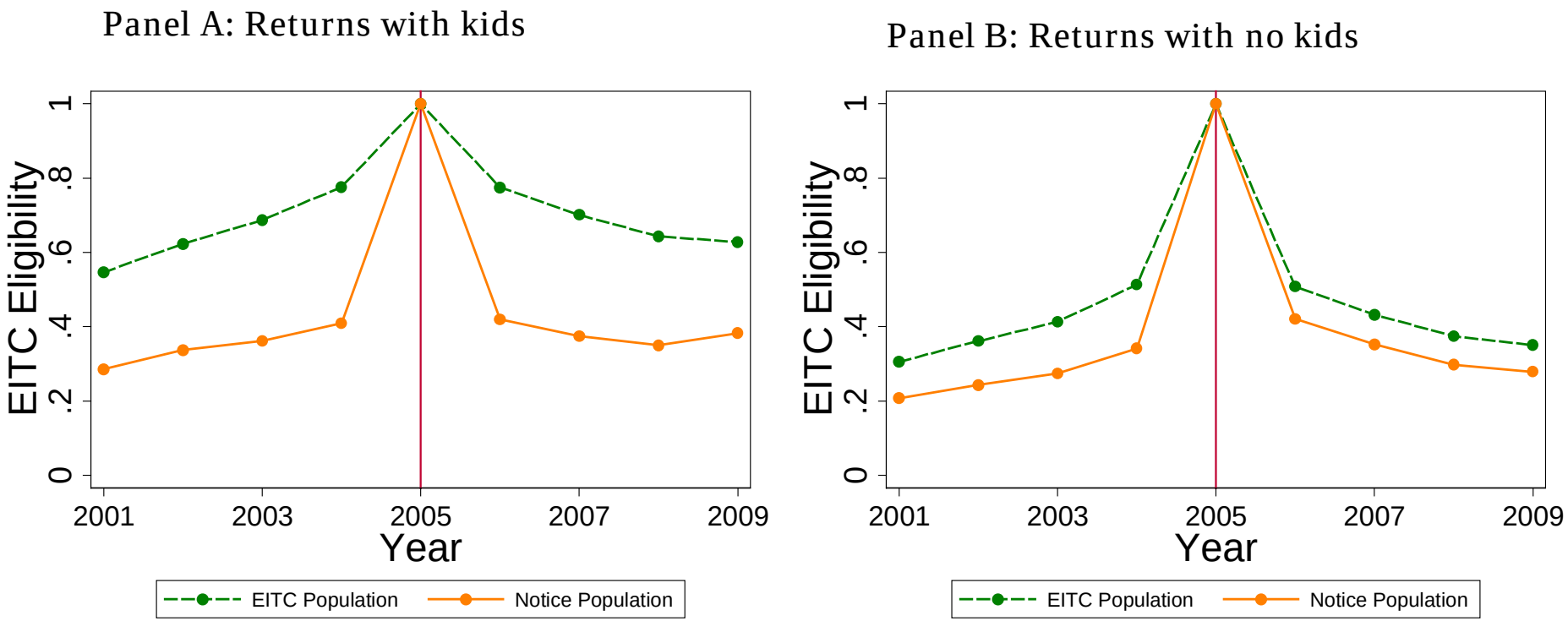
Notes: These figures are binned scatter plots of the Chetty, Freidman Saez (2013) measure of EITC knowledge. The figures show the average share of sharp bunchers (the knowledge measure from Chetty, Friedman and Saez (2013) and the average share of notices for equally sized knowledge measure groups. The fitted lines are estimates based on the underlying ZIP3 level, of a regression of the knowledge measure on the notice rate. The data are for 2005 though this pattern holds in other tax years.

Figure 6: Response Rate to IRS Notices and EITC Knowledge, by ZIP3



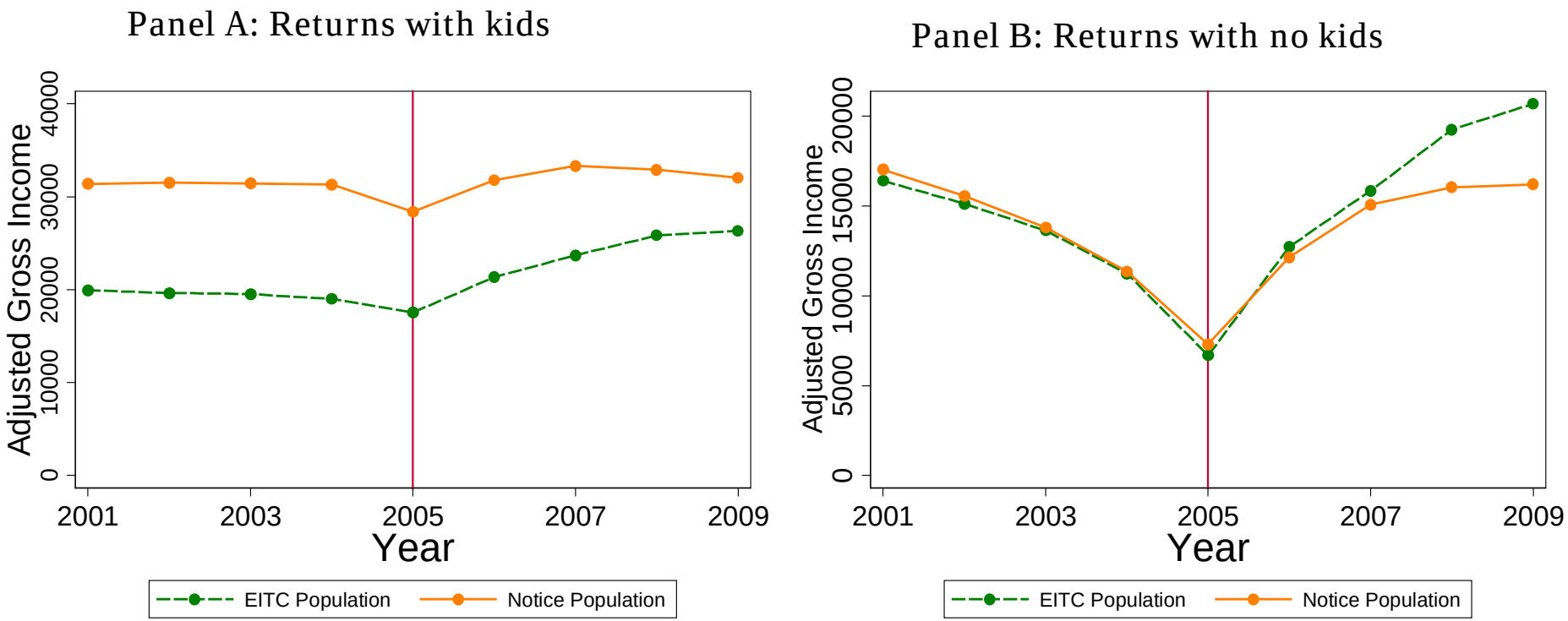
Notes: These figures are binned scatter plots of the Chetty, Freidman Saez (2013) measure of EITC knowledge. The figures show the average share of sharp bunchers (the knowledge measure from Chetty, Friedman and Saez (2013) and the average response rate to notices for equally sized knowledge measure groups. The fitted lines are estimates based on the underlying ZIP3 level, of a regression of the knowledge measure on the notice rate. The data are for 2005 though this pattern holds in other tax years.

Figure 7: EITC Eligibility over time, Notice Population and EITC Returns



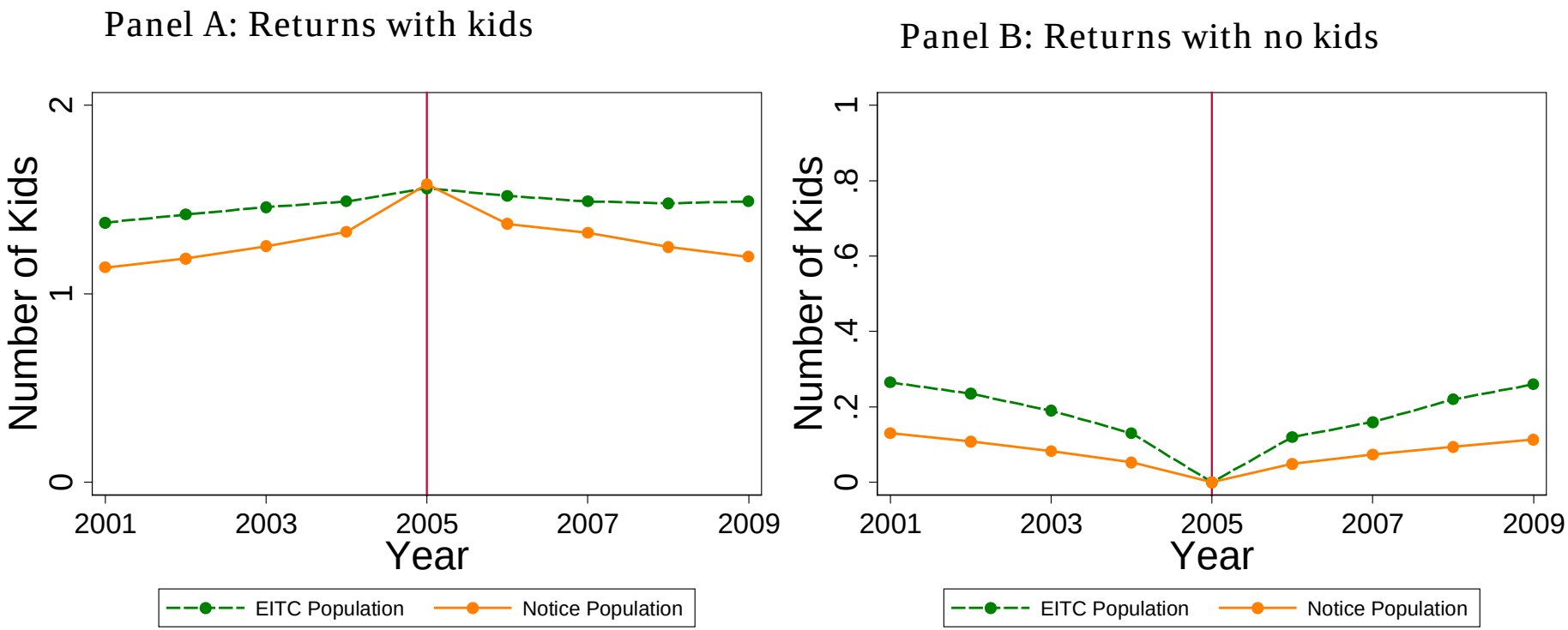
Notes: The EITC population figures are based on a 20 percent random sample of EITC tax filers in 2005. EITC eligibility is measured as having a valid EITC claim on Form 1040 or receiving an EITC notice (CP 09 or 27).

Figure 8: Adjusted Gross Income over time, Notice Population and EITC Returns



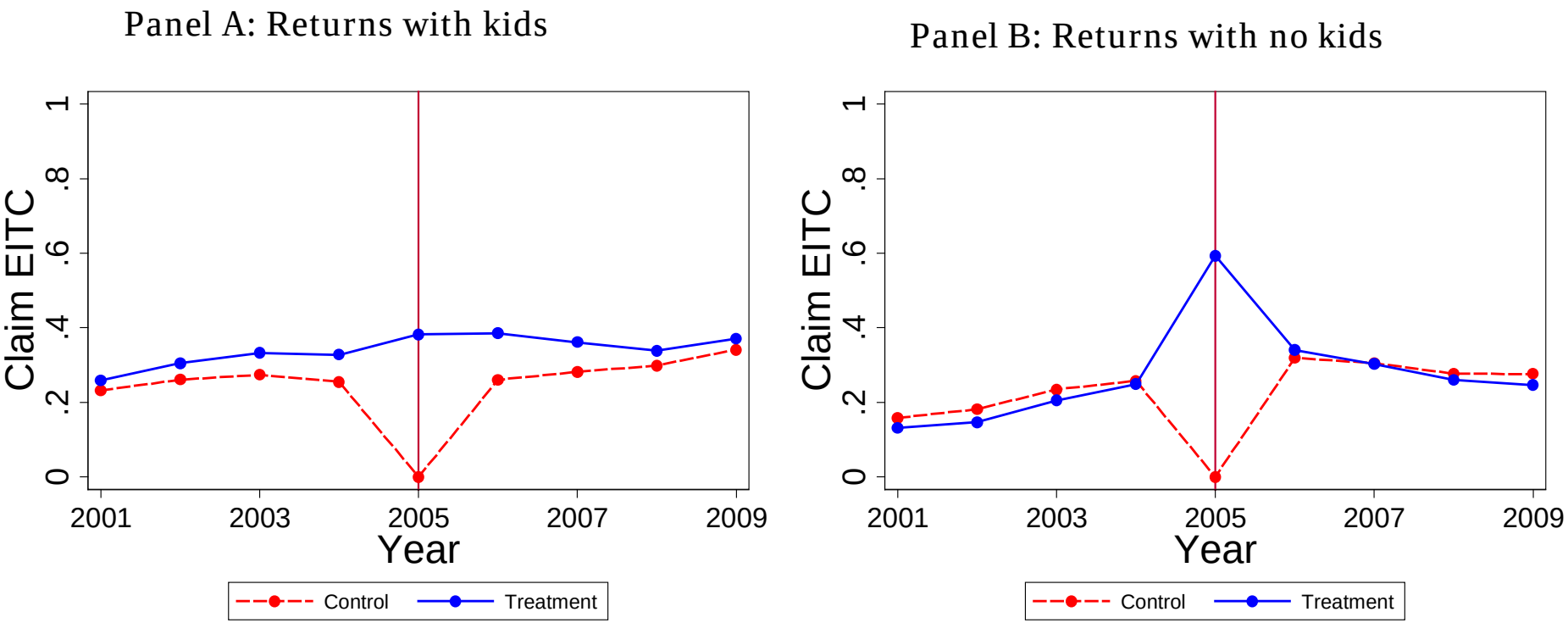
Notes: The EITC population figures are based on a 20 percent random sample of EITC tax filers in 2005. Adjusted gross income is inflation adjusted using the CPI-U to $2011.

Figure 9: Number of Kids over time, Notice Population and EITC Returns



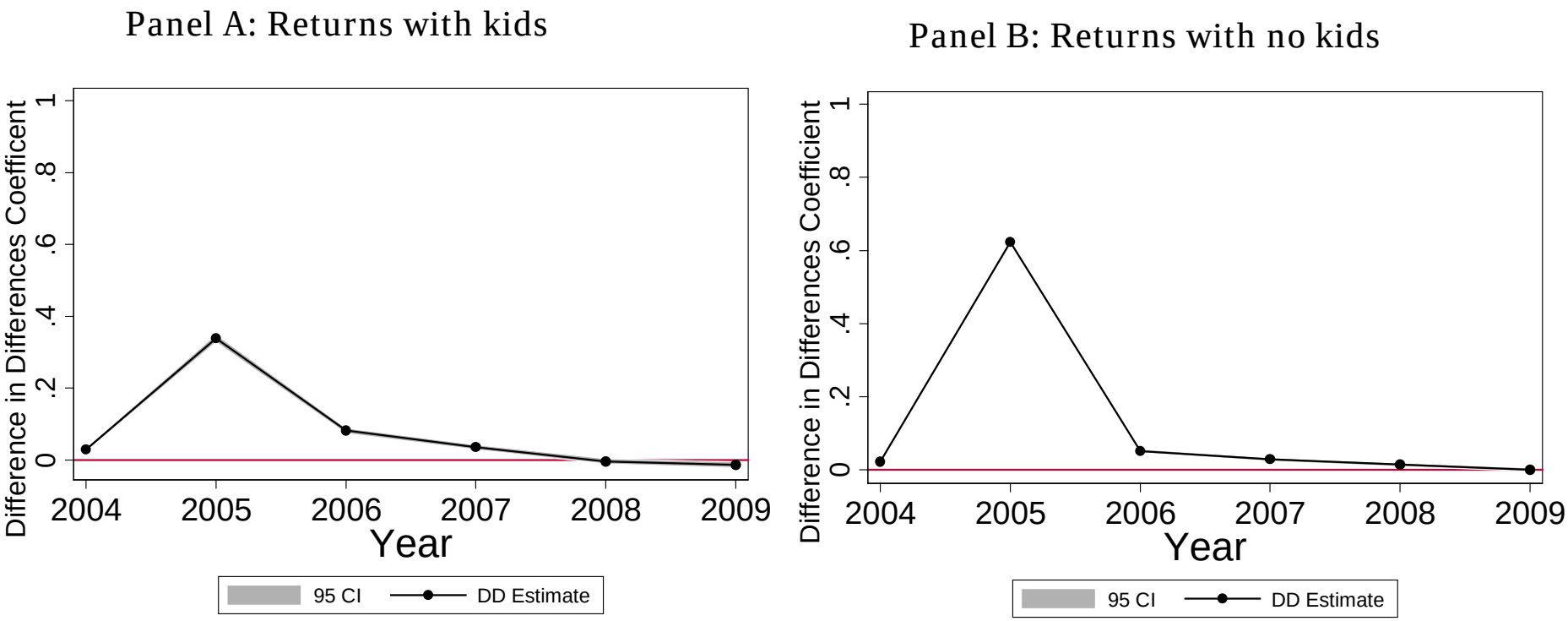
Notes: The EITC population figures are based on a 20 percent random sample of EITC tax filers in 2005. The number of children is based on information from tax returns in each tax year.

Figure 10: EITC claiming over time, 2005 Analysis Sample



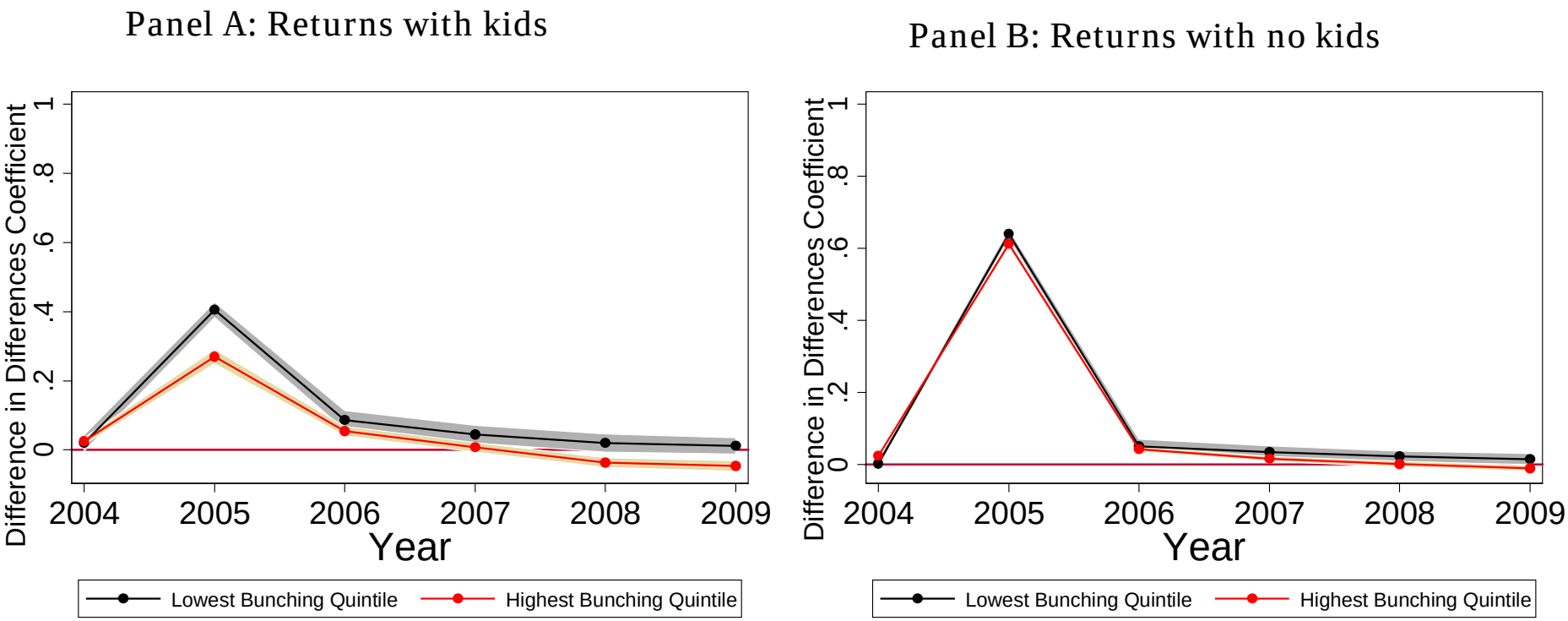
Notes: These figures plot the probability that the primary taxpayers in the 2005 Analysis Sample claim the EITC, either on the tax return or in response to an IRS notice, in 2005 and in the four prior tax years or the four following tax years.

Figure 11: Diff-in-Diffs Results for EITC Claiming



Notes: These figures plot the coefficient and standard errors of the difference-in-differences coefficient for each tax year that give the differential effect of treatment versus control in later years compared to the years prior to 2005 (2001-04). See Tables 3A and 4A for further details.

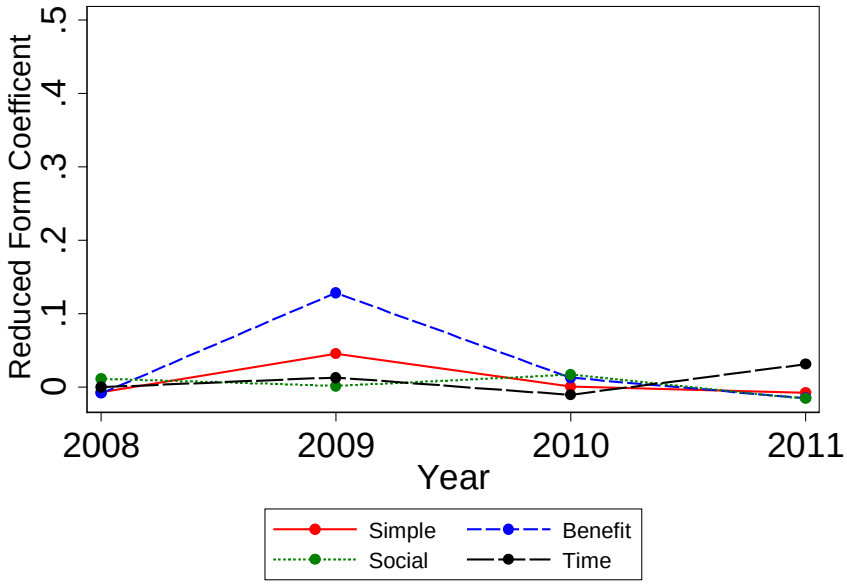
Figure 12: Diff-in-Diffs Results for EITC Claiming by Knowledge Areas



Notes: These figures plot the coefficient and standard errors of the difference-in-differences coefficient for each tax year that give the differential effect of treatment versus control in later years compared to the years prior to 2005 (2001-04) separately for the highest and lowest bunching quintiles across three-digit zip codes as defined in Chetty, Friedman and Saez (2013). See Appendix Table A5 for further details, including results for each bunching quintile.

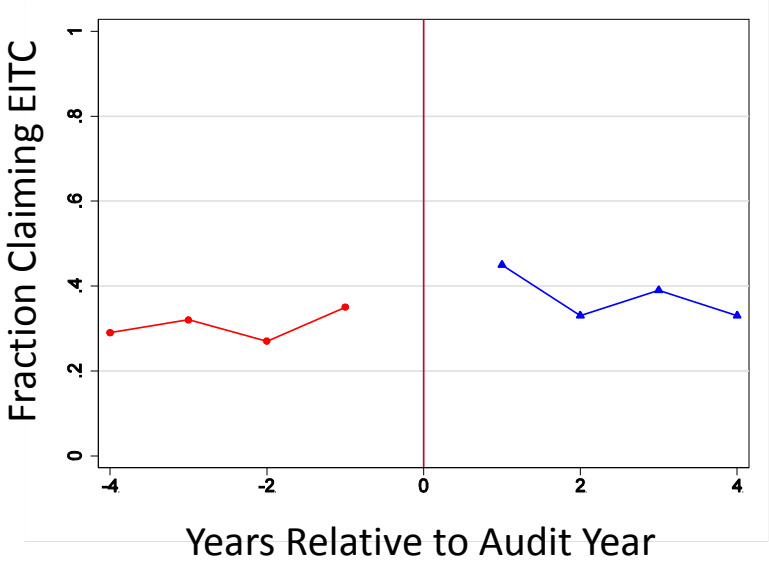
Figure 13: CA Experiment & Audit Event Study

Panel A: California 2009 Experiment, Diff-in-Diffs Results for EITC Claiming



Notes: This figure plots the coefficient and standard error of the difference-in-differences coefficient for each tax year that give the differential effect of treatment versus control in later years compared to the years prior to 2008 (2006-07). See Table 6.

Panel B: Audit Event Study



Notes: This figure plots the probability of claiming EITC benefits by years relative to the year of audit. The sample is restricted to individuals who did not claim the EITC in the audit year but were then found to be eligible for EITC benefits after the audit was complete. Thus, pre-audit, the Fraction Claiming EITC is 0, and post-audit, the Fraction Claiming EITC is 1. The sample is taken from audits in Tax Years 2006 to 2009, and data from tax years 2001 through 2013 are pulled for all individuals in the sample.

ONLINE APPENDIX – NOT FOR PUBLICATION

Identification Tests, 2005 Analysis Sample

As a specification check, we implement several placebo tests using alternate placebo glitch years in the pre-glitch period. Table A2 presents these results, which show small differences between treatment and control groups in earlier, non-glitch years for returns with kids. However, these differences are an order of magnitude smaller than the differences observed in the glitch year for these returns. For example, the largest differential effect in a placebo glitch year in Table A2 is roughly 3 percentage points, compared to over 30 percentage points for the actual glitch year in Table 3. For returns without kids, the differential effects in the placebo glitch years in Table A3 are of the wrong sign and are also substantively small. Additionally, we restrict the sample to pre-glitch years (2001 through 2004) and estimate the regression specifications in which we assign each of the earlier years 2002 through 2004 as glitch years. These results in Table A4 also confirm that the differences observed in 2005 are an order of magnitude larger than any differences observed in the pre-glitch years. Based on these placebo checks, we conclude that the regression specification accurately identifies differences in the glitch year.

A key identifying assumption in the difference-in-differences specification that we use is that the treatment and control groups have common time trends prior to the intervention year. In Table A5, we examine this assumption by restricting the sample to years prior to 2005 (2001 through 2004), and we test whether a linear time trend is different across the treatment and control groups. We implement this test by regressing outcomes on a linear time trend, a dummy for being in the treatment group, an interaction between this dummy and the linear time trend, and covariates. The results in Table A5 show statistically significant coefficients on the interaction terms, but effects that are economically small. For example, in Panel A of Table A5 the interaction term implies there is a 0.864 percentage point difference in the linear trends across the treatment and control groups in total EITC claiming for returns with kids. By comparison, the differential effects in the nudge year range are over 30 percentage points in Table 3. Similarly, for returns without kids the differential trend in total EITC claiming in Table A5 is -0.732 percentage points, compared to more than 60 percentage points in Table 4.

While the differential time trends in Table A5 clearly cannot explain the large differences in outcomes across treatment and control groups in 2005, it is possible that the differential trends impact the learning effects in later years. To test the sensitivity of the learning results, we add flexible time controls and interactions of these time controls with an indicator for the treatment status. Tables A6 and A7 show the baseline results after including controls for a linear trend and an interaction of the linear trend with the treatment indicator. Both the learning and nudge effects are qualitatively similar with differential time trends. For example, among returns with no kids the nudge effect in 2005 and the learning effect in 2006 for total EITC take-up are 62 and 5 percentage points respectively in Table 4, and these figures are virtually the same in Table A6.

Appendix Table A1: IRS Notice Filters
Control Group Construction
Retain returns that meet earnings & investment income restriction
Remove married filing separate returns
Retain returns from US states
Age Restrictions Taxpayer: 25-64 for returns with no kids, 18-80 for returns with kids
Age Restrictions Children: no missing dependent ages, no returns with dependents over age 19
Remove dependent returns
Remove returns with foreign income
Remove returns with primary taxpayers that do not have valid SSN
Remove returns that decline EITC
Remove late filed returns
Remove returns that claim EITC
Remove returns with dependent children that do not have valid SSN
Remove returns with prior compliance issues with EITC
Drop returns that file as single with kids
Source: Plueger (2009).

Appendix Table A2: Placebo Tests of Omitted Years, Any EITC Claiming among Returns with kids

Omitted Years	2001	2001-02	2001-03 (Baseline Results)	2001-04
Treatment*2002	0.0171			
	[0.00237]			
Treatment*2003	0.0319	0.0233		
	[0.00303]	[0.00250]		
Treatment*2004	0.0457	0.0372	0.0294	
	[0.00359]	[0.00307]	[0.00270]	
Treatment*2005	0.355	0.347	0.339	0.331
	[0.00560]	[0.00567]	[0.00565]	[0.00566]
Treatment*2006	0.0982	0.0896	0.0819	0.0745
	[0.00436]	[0.00396]	[0.00363]	[0.00339]
Treatment*2007	0.0521	0.0435	0.0358	0.0284
	[0.00419]	[0.00392]	[0.00361]	[0.00344]
Treatment*2008	0.0127	0.00410	-0.00367	-0.0110
	[0.00423]	[0.00395]	[0.00367]	[0.00354]
Treatment*2009	0.00257	-0.00599	-0.0138	-0.0211
	[0.00422]	[0.00412]	[0.00394]	[0.00386]
2002	0.0289			
	[0.00217]			
2003	0.0420	0.0276		
	[0.00283]	[0.00223]		
2004	0.0232	0.00872	-0.000479	
	[0.00353]	[0.00297]	[0.00252]	
2005	-0.232	-0.246	-0.256	-0.255
	[0.00303]	[0.00285]	[0.00278]	[0.00273]
2006	0.0287	0.0142	0.00503	0.00515
	[0.00394]	[0.00342]	[0.00302]	[0.00271]
2007	0.0500	0.0356	0.0264	0.0265
	[0.00354]	[0.00319]	[0.00285]	[0.00265]
2008	0.0669	0.0525	0.0433	0.0434
	[0.00360]	[0.00338]	[0.00313]	[0.00303]
2009	0.109	0.0950	0.0858	0.0859
	[0.00330]	[0.00323]	[0.00313]	[0.00315]
Treatment	0.0211	0.0297	0.0375	0.0448
	[0.00330]	[0.00309]	[0.00291]	[0.00281]
	0.203	0.218	0.227	0.227
Constant	[0.00962]	[0.00962]	[0.00958]	[0.00954]
Individuals	151,687	151,687	151,687	151,687
Observations	1,365,183	1,365,183	1,365,183	1,365,183

Notes: All specifications include controls based on the 2005 tax return, including indicators for $1000 earnings bins, 5 year age bins (primary taxpayer age), number of kids, joint filing status, paid tax preparation, $100 bins of tax refunds and 3-digit zipcode fixed effects. Standard errors are clustered at the 3-digit zipcode level and reported in brackets.

Appendix Table A3: Placebo Tests of Omitted Years, Any EITC Claiming among Returns with no kids

Omitted Years	2001	2001-02	2001-03 (Baseline Results)	2001-04
Treatment*2002	-0.00835			
	[0.00145]			
Treatment*2003	-0.00238	0.00179		
	[0.00196]	[0.00169]		
Treatment*2004	0.0185	0.0226	0.0220	
	[0.00199]	[0.00182]	[0.00163]	
Treatment*2005	0.620	0.624	0.624	0.618
	[0.00239]	[0.00236]	[0.00242]	[0.00242]
Treatment*2006	0.0480	0.0522	0.0516	0.0461
	[0.00223]	[0.00215]	[0.00210]	[0.00207]
Treatment*2007	0.0246	0.0288	0.0282	0.0227
	[0.00234]	[0.00219]	[0.00216]	[0.00217]
Treatment*2008	0.0104	0.0146	0.0140	0.00848
	[0.00237]	[0.00225]	[0.00221]	[0.00221]
Treatment*2009	-0.00282	0.00136	0.000760	-0.00475
	[0.00243]	[0.00232]	[0.00231]	[0.00235]
2002	0.0232			
	[0.00135]			
2003	0.0763	0.0646		
	[0.00186]	[0.00163]		
2004	0.0991	0.0875	0.0660	
	[0.00195]	[0.00178]	[0.00156]	
2005	-0.159	-0.170	-0.192	-0.208
	[0.00228]	[0.00224]	[0.00221]	[0.00219]
2006	0.161	0.149	0.128	0.111
	[0.00231]	[0.00222]	[0.00208]	[0.00200]
2007	0.147	0.135	0.114	0.0972
	[0.00232]	[0.00216]	[0.00205]	[0.00202]
2008	0.118	0.107	0.0851	0.0686
	[0.00238]	[0.00226]	[0.00217]	[0.00213]
2009	0.118	0.106	0.0845	0.0680
	[0.00232]	[0.00220]	[0.00213]	[0.00214]
Treatment	-0.0211	-0.0253	-0.0247	-0.0192
	[0.00199]	[0.00187]	[0.00177]	[0.00171]
Constant	0.133	0.144	0.166	0.182
	[0.00324]	[0.00318]	[0.00303]	[0.00297]
Individuals	561,261	561,261	561,261	561,261
Observations	5,051,349	5,051,349	5,051,349	5,051,349

Notes: All specifications include controls based on the 2005 tax return, including indicators for $1000 earnings bins, 5 year age bins (primary taxpayer age), number of kids, joint filing status, paid tax preparation, $100 bins of tax refunds and 3-digit zipcode fixed effects. Standard errors are clustered at the 3-digit zipcode level and reported in brackets.

Appendix Table A4: Placebo Tests for EITC Claiming, 2005 Sample

	Returns with kids			Returns with no kids		
Placebo year	2002	2003	2004	2002	2003	2004
Treatment*2002	0.0164			-0.00820		
	[0.00233]			[0.00146]		
Treatment*2003	0.0190	0.0108		-0.0235	-0.0194	
	[0.00293]	[0.00240]		[0.00183]	[0.00156]	
Treatment*2004	0.0270	0.0188	0.0152	-0.0193	-0.0152	-0.00871
	[0.00343]	[0.00293]	[0.00258]	[0.00196]	[0.00181]	[0.00163]
2002	0.0273			0.0205		
	[0.00214]			[0.00136]		
2003	0.0334	0.0197		0.0405	0.0302	
	[0.00269]	[0.00211]		[0.00172]	[0.00148]	
2004	0.00896	-0.00470	-0.0113	0.0452	0.0349	0.0248
	[0.00330]	[0.00275]	[0.00234]	[0.00186]	[0.00174]	[0.00156]
Treatment	0.0258	0.0339	0.0375	-0.0118	-0.0159	-0.0224
	[0.00314]	[0.00292]	[0.00278]	[0.00197]	[0.00185]	[0.00175]
Constant	0.0434	0.0571	0.0637	0.182	0.192	0.202
	[0.00997]	[0.00993]	[0.00993]	[0.00321]	[0.00315]	[0.00305]
Individuals	151,687	151,687	151,687	561,261	561,262	561,263
Observations	606,748	606,748	606,748	2,245,044	2,245,048	2,245,052

Notes: Sample includes years 2001-2004. All specifications include controls based on the 2005 tax return, including indicators for $1000 earnings bins, 5 year age bins (primary taxpayer age), number of kids, joint filing status, paid tax preparation, $100 bins of tax refunds and 3-digit zipcode fixed effects. Standard errors are clustered at the 3-digit zipcode level and reported in brackets.

Appendix Table A5: Common Trends Tests, 2005 Sample

	Dependent Variable				
	EITC Eligibility	Take EITC F1040	Take EITC Total	EITC Amount F1040	EITC Amount Total
Panel A: Returns with kids					
Trend	0.0351	0.0148	0.00957	0.518	7.515
	[0.00101]	[0.00148]	[0.00144]	[3.191]	[3.269]
Treatment*Trend	0.00716	0.0158	0.00864	3.125	2.55
	[0.00115]	[0.00166]	[0.00160]	[3.458]	[3.591]
Mean	0.348	0.482	0.496	934.77	951.73
Individuals	151,687	151,687	151,687	151,687	151,687
Observations	606,748	606,748	606,748	606,748	606,748
Panel B: Returns with no kids					
Trend	0.0460	0.0350	0.0155	-15.70	-10.88
	[0.000806]	[0.000663]	[0.000628]	[1.039]	[1.023]
Treatment*Trend	-0.00330	0.00613	-0.00732	0.155	3.731
	[0.000785]	[0.000673]	[0.000658]	[1.087]	[1.072]
Mean	0.267	0.111	0.15	51.47	61.93
Individuals	561,261	561,261	561,261	561,261	561,261
Observations	2,245,044	2,245,044	2,245,044	2,245,044	2,245,044

Notes: Sample includes years 2001-2004. All specifications include controls based on the 2005 tax return, including indicators for $1000 earnings bins, 5 year age bins (primary taxpayer age), number of kids, joint filing status, paid tax preparation, $100 bins of tax refunds and 3-digit zipcode fixed effects. Standard errors are clustered at the 3-digit zipcode level and reported in brackets.

Appendix Table A6: Robustness to time trends, returns with kids

	Take EITC F1040	Take EITC Total	EITC Amount F1040	EITC Amount Total
Treatment*2004	-0.00374	-0.00249	-2.553	-3.821
	[0.00314]	[0.00324]	[7.240]	[7.338]
Treatment*2005	0.315	0.291	399.3	366.6
	[0.00757]	[0.00769]	[13.86]	[13.92]
Treatment*2006	0.0360	0.0181	84.15	59.20
	[0.00599]	[0.00603]	[13.41]	[13.47]
Treatment*2007	-0.0131	-0.0439	3.785	-42.03
	[0.00737]	[0.00758]	[16.61]	[17.00]
Treatment*2008	-0.0574	-0.0993	-69.15	-131.2
	[0.00879]	[0.00906]	[20.09]	[20.60]
Treatment*2009	-0.0774	-0.125	-93.77	-166.7
	[0.0107]	[0.0111]	[23.70]	[24.35]
Trend	0.0167	0.0210	21.46	27.71
	[0.00134]	[0.00142]	[3.031]	[3.174]
Trend*Treatment	0.00948	0.0159	10.06	18.62
	[0.00146]	[0.00151]	[3.314]	[3.420]
Treatment	0.0203	0.00559	84.44	79.29
	[0.00412]	[0.00426]	[10.25]	[10.52]
Constant	0.190	0.185	344.7	308.5
	[0.00999]	[0.00993]	[26.76]	[27.40]
Individuals	151,687	151,687	151,687	151,687
Observations	1,365,183	1,365,183	1,365,183	1,365,183

Notes: All specifications include controls based on the 2005 tax return, including indicators for $1000 earnings bins, 5 year age bins (primary taxpayer age), number of kids, joint filing status, paid tax preparation, $100 bins of tax refunds and 3-digit zipcode fixed effects. Standard errors are clustered at the 3-digit zipcode level and reported in brackets.

Appendix Table A7: Robustness to time trends, returns with no kids

	Take EITC F1040	Take EITC Total	EITC Amount F1040	EITC Amount Total
Treatment*2004	0.0148	0.0244	11.25	14.01
	[0.00224]	[0.00234]	[2.951]	[2.946]
Treatment*2005	0.668	0.627	176.7	166.4
	[0.00402]	[0.00422]	[4.463]	[4.466]
Treatment*2006	0.0760	0.0564	26.94	21.44
	[0.00427]	[0.00458]	[5.763]	[5.756]
Treatment*2007	0.0716	0.0342	14.75	4.285
	[0.00508]	[0.00548]	[7.050]	[7.083]
Treatment*2008	0.0737	0.0211	4.800	-10.48
	[0.00606]	[0.00642]	[8.483]	[8.434]
Treatment*2009	0.0763	0.00910	-13.64	-32.25
	[0.00699]	[0.00747]	[9.999]	[9.935]
Trend	0.0202	0.0381	-7.444	-2.763
	[0.000858]	[0.000932]	[1.298]	[1.287]
Trend*Treatment	-0.0118	-0.00119	-3.220	-0.473
	[0.000916]	[0.000980]	[1.366]	[1.353]
Treatment	-0.00237	-0.0223	6.017	2.423
	[0.00257]	[0.00264]	[4.936]	[4.899]
Constant	0.156	0.0895	100.7	92.18
	[0.00322]	[0.00380]	[5.313]	[5.313]
Individuals	151,687	151,687	151,687	151,687
Observations	1,365,183	1,365,183	1,365,183	1,365,183

Notes: All specifications include controls based on the 2005 tax return, including indicators for $1000 earnings bins, 5 year age bins (primary taxpayer age), number of kids, joint filing status, paid tax preparation, $100 bins of tax refunds and 3-digit zipcode fixed effects. Standard errors are clustered at the 3-digit zipcode level and reported in brackets.

Appendix Table A8: Cross-sectional notice results, first time notice sample				
	2006	2007	2008	2009
Panel A: Returns with kids				
Treatment	0.0399	0.0187	0.00827	0.00992
	[0.00234]	[0.00139]	[0.00111]	[0.00101]
Mean Notice (control)	0.075	0.040	0.026	0.023
Individuals	13,477	13,477	13,477	13,477
Observations	121,293	121,293	121,293	121,293
Panel B: Returns with no kids				
Treatment	0.0182	0.0150	0.0124	0.0126
	[0.00169]	[0.00138]	[0.00119]	[0.000811]
Mean Notice (control)	0.124	0.073	0.049	0.036
Individuals	44,224	44,224	44,224	44,224
Observations	398,016	398,016	398,016	398,016

Notes: Table shows the results for cross-sectional regressions in each tax year for the sample that recieves, or should receive, their first notice in 2005. The dependent varaible in each regression is 0-1 measure of receiving a notice.

Appendix Table A9: Cross-sectional eligibility results, first time notice sample				
	2006	2007	2008	2009
Panel A: Returns with kids				
Treatment	0.151	0.0988	0.0604	0.0491
	[0.00426]	[0.00386]	[0.00345]	[0.00343]
Mean Eligibility (control)	0.407	0.367	0.345	0.379
Individuals	13,477	13,477	13,477	13,477
Observations	121,293	121,293	121,293	121,293
Panel B: Returns with no kids				
Treatment	0.0198	0.000566	-0.00686	-0.0132
	[0.00315]	[0.00303]	[0.00287]	[0.00280]
Mean Eligibility (control)	0.382	0.321	0.272	0.257
Individuals	44,224	44,224	44,224	44,224
Observations	398,016	398,016	398,016	398,016

Notes: Table shows the results for cross-sectional regressions in each tax year for the sample that recieves, or should receive, their first notice in 2005. The dependent varaible in each regression is 0-1 measure of EITC eligibility.

Appendix Table A10: Diff-in-Diffs Estimates, 2005 Sample Any EITC Claiming

	Bunching Quintile				
	1	2	3	4	5
	Panel A: Returns with kids				
Treatment*2004	0.0196	0.0341	0.0301	0.0329	0.0254
	[0.0121]	[0.00783]	[0.00495]	[0.00558]	[0.00501]
Treatment*2005	0.405	0.425	0.346	0.323	0.270
	[0.0113]	[0.00957]	[0.0102]	[0.0104]	[0.0106]
Treatment*2006	0.0866	0.112	0.0965	0.0852	0.0546
	[0.0139]	[0.0101]	[0.00732]	[0.00738]	[0.00700]
Treatment*2007	0.0452	0.0804	0.0464	0.0350	0.00732
	[0.0131]	[0.00976]	[0.00688]	[0.00644]	[0.00688]
Treatment*2008	0.0203	0.0474	0.00686	-0.00765	-0.0361
	[0.0133]	[0.00823]	[0.00754]	[0.00622]	[0.00697]
Treatment*2009	0.0121	0.0372	-0.00191	-0.0181	-0.0465
	[0.0121]	[0.00894]	[0.00870]	[0.00706]	[0.00760]
Individuals	13,382	20,710	28,917	35,048	48,736
Observations	120,438	186,390	260,253	315,432	438,624
	Panel B: Returns with no kids				
Treatment*2004	0.00355	0.0114	0.0277	0.0217	0.0239
	[0.00565]	[0.00534]	[0.00408]	[0.00341]	[0.00249]
Treatment*2005	0.641	0.636	0.627	0.612	0.612
	[0.00639]	[0.00528]	[0.00497]	[0.00496]	[0.00428]
Treatment*2006	0.0511	0.0552	0.0606	0.0422	0.0432
	[0.00983]	[0.00569]	[0.00496]	[0.00475]	[0.00324]
Treatment*2007	0.0350	0.0331	0.0414	0.0242	0.0162
	[0.00869]	[0.00649]	[0.00503]	[0.00452]	[0.00340]
Treatment*2008	0.0229	0.0191	0.0281	0.00831	0.00238
	[0.00735]	[0.00602]	[0.00434]	[0.00407]	[0.00404]
Treatment*2009	0.0157	0.00963	0.0106	-0.00499	-0.00988
	[0.00800]	[0.00583]	[0.00511]	[0.00434]	[0.00407]
Individuals	57,614	87,395	110,250	134,054	163,716
Observations	518,526	786,555	992,250	1,206,486	1,473,444

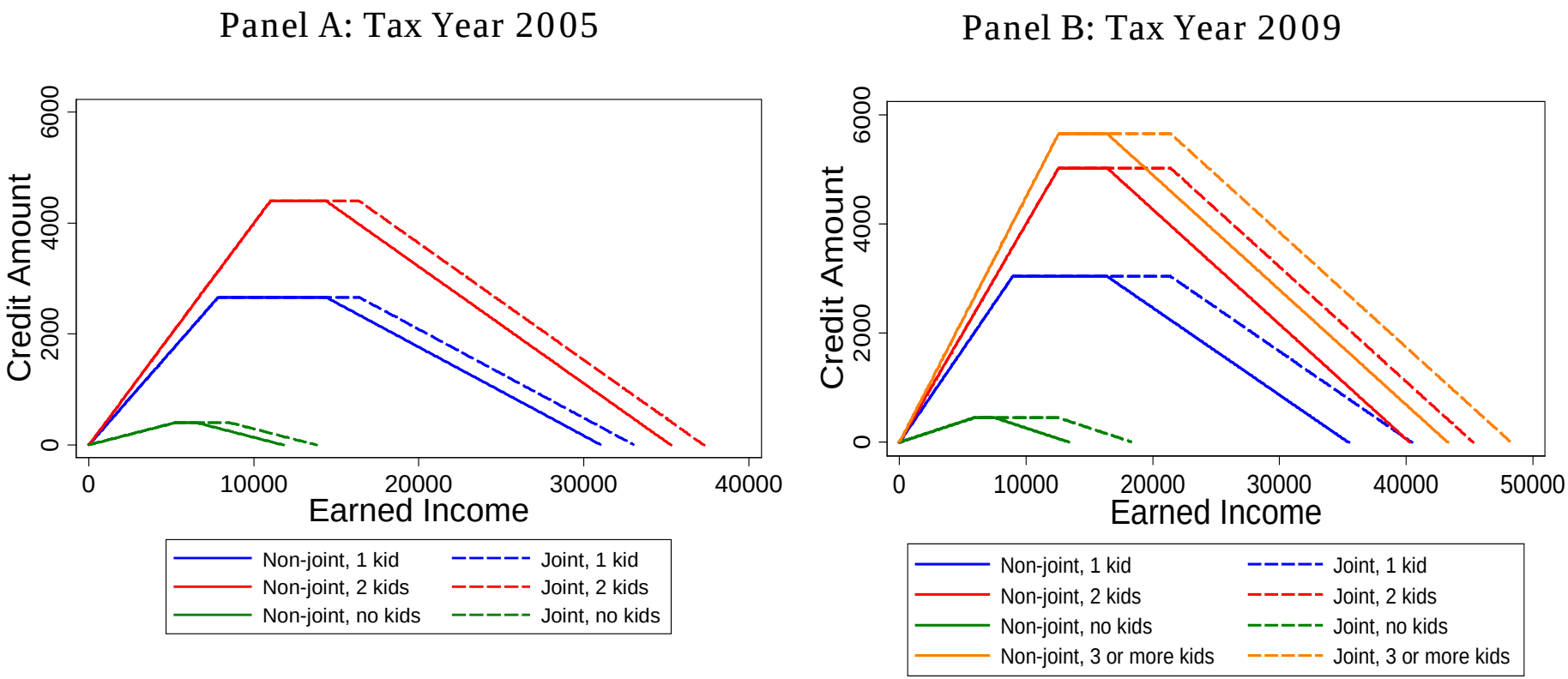
Notes: Quintiles in each column are defined at the 3-digit zipcode level using the Chetty, Friedman and Saez (2013) measure of sharp bunching. All specifications include controls based on the 2005 tax return, including indicators for $1000 earnings bins, 5 year age bins (primary taxpayer age), number of kids, joint filing status, paid tax preparation, $100 bins of tax refunds and 3-digit zipcode fixed effects. Standard errors are clustered at the 3-digit zipcode level and reported in brackets.

Table A11: Event Study Results

pre4	0.311	pre	0.330
	(0.100)		(0.065)
pre3	0.307	post1	0.456
	(0.087)		(0.071)
pre2	0.279	post	0.329
	(0.078)		(0.073)
pre1	0.366		
	(0.072)		
post1	0.459		
	(0.072)		
post2	0.331		
	(0.078)		
post3	0.352		
	(0.086)		
post4	0.314		
	(0.097)		
Observations	900		900

Notes: This table reports results of regression an indicator for claiming EITC benefits on calendar year dummies and event time variables.

Appendix Figure 1: Earned Income Tax Credit Schedules



Notes: For simplicity, these figures show the statutory EITC schedules by earned income. In practice, the EITC is a function of both earnings and adjusted gross income.

Appendix Figure 2A: Example Notice, returns with kids

[redacted] 200504 09

SBV

Department of the Treasury
Internal Revenue Service [redacted]

Customer Service:

Notice Number: CP09
Date: FEB. 7, 2005
Social Security Number:

Tax Year: 2003
Tax Form: 1040
This is not a bill.

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

Why We Are Sending You this Notice

We are writing to you because you may qualify for the earned income credit (EIC). This credit is for certain people who work and have earned income. Our records show:

- Your income falls in the eligible range to receive the EIC,
- You have a dependent who may be an EIC qualifying child, and
- You did not claim the EIC on your 2003 Form 1040 .

What You Need to Do

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

- Answer the questions in Step 1.
  - Any YES answer means you do not qualify.
  - NO answers to both questions mean you may qualify. Continue on to Step 2.
- In Step 2, fill in the name and Social Security number for each child who *may* qualify you for the EIC. Continue on to Step 3.
- Answer the questions in Step 3.
  - 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. Both you and your spouse must sign the declaration, if you filed a joint return. Mail the completed *EIC Eligibility Check-Sheet* to us in the enclosed envelope.

*Note: Return the EIC Eligibility Check-Sheet to us **only** if you determine you may qualify for the EIC.*

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What if a Child is a Qualifying Child of More than One Person

If your qualifying child is also a qualifying child of another person, you and the other person can decide who will claim the credit using that child. If more than one person claims the credit using the same child, the IRS will apply the following tie-breaker rules to determine which person can claim the credit with that child.

If	Then Only
Only one person is the child's parent,	The parent can treat the child as a qualifying child.
Two persons are the child's parents and they do not file a joint return together,	The parent with whom the child lived the longest during the year can treat the child as a qualifying child.
Two persons are the child's parents, the child lived with each parent the same amount of time during the year, and the parents do not file a joint return together,	The parent with the highest adjusted gross income can treat the child as a qualifying child.
Neither person is the child's parent,	The person with the highest adjusted gross income can treat the child as a qualifying child.

What We Will Do

When we receive a completed *EIC Eligibility Check-Sheet*, we will review the information you provide. If you qualify for the EIC we will send you a refund within the next eight weeks (if you owe no other amounts we are required to collect). We will send you a letter of explanation if you do not qualify for the credit.

What You Can Do Next Year

You can get your earned income credit faster, if you are eligible and avoid getting a notice next year by doing one of the following on your next Federal Income Tax return:

- Figure and include the EIC amount on your return. The instruction booklets for Form 1040 and Form 1040A include instructions on how to figure your EIC.
- Ask the IRS to figure the EIC for you by:
  - Writing "EIC" on the earned income credit line.
  - Completing and attaching Schedule EIC for a qualifying child. *Remember, you do not need to claim a child as a dependent to receive the EIC.*
- Write "NO" on the EIC line of your return if you do not want or do not qualify for the credit.

How to Get More Information

You can get more information about the earned income credit by visiting www.irs.gov or by calling 1-800-829-4477 and requesting pre-recorded topic #601. You can also get IRS publications by calling 1-800-829-3676 or by downloading them from www.irs.gov.

- *Form 1040 or Form 1040A* instruction booklets
- *Publication 596, Earned Income Credit (Publicación 596(SP) para la versión en español)*

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Appendix Figure 2B: Example Notice, returns with kids

EIC Eligibility Check-Sheet

Step 1	Read and answer each question below and check the correct box.	YES	NO
Does your Social Security card (or your spouse's, if you filed a joint return) read **Not Valid for Employment** *AND* was the Social Security number (SSN) issued so that you (or your spouse) could get a federally funded benefit, such as Medicaid?		☐	☐
Do you (or your spouse, if you filed a joint return) meet the age, relationship, and residency tests to be an EIC qualifying child of another person? (For more information on these tests, see Step 3.)		☐	☐

Attention	If	Then
	You answered NO to both questions in Step 1,	**Go!** Continue on to Step 2.
	You answered YES to either questions in Step 1,	**Stop!** You do not qualify. Do not answer this notice.

Step 2

Write the name and SSN for no more than two children who lived with you during 2003. Then go to Step 3. Do not list the child if:

- The child has a Social Security card that reads **Not Valid for Employment** *AND* the SSN was issued so that the child could get a federally funded benefit, such as Medicaid.
- The child was married and you are not entitled to claim the child as a dependent on your 2003 return (you can list the child if you are not entitled to the child's exemption under the rules for children of divorced or separated parents).

	First and Last Name	Social Security Number
Child # 1		
Child # 2		

Page 3

Step 3	Read and answer each question for each child listed in Step 2 to decide whether the child is your EIC qualifying child.	Child # 1 YES	Child # 1 NO	Child # 2 YES	Child # 2 NO
	Relationship Test - Is the child your: • Son, daughter, stepson, stepdaughter, or a descendant of any of them (such as a grandchild), *or* • Brother, sister, stepbrother, stepsister, or a descendant of any of them (such as a niece or nephew) whom you cared for as your own child, *or* • Eligible foster child (child placed with you by an authorized placement agency whom you cared for as your own child)?	☐	☐	☐	☐
	Age Test - At the end of 2003, was the child: • Under age 19, or • Under age 24 and a full time student, or • Permanently and totally disabled at any time during the year, regardless of age?	☐	☐	☐	☐
	Residency Test - Did the child live with you in the United States for more than half of 2003?	☐	☐	☐	☐

Attention	If	Then
	You answered YES to *all* of the questions for either child,	**Go!** You have an EIC qualifying child. Sign the declaration below and return this check-sheet in the envelope provided.
	You answered NO to *any* question for a child and you have no other child you answered YES to all of the questions for,	**Stop!** You do not have an EIC qualifying child. Do not respond to this notice.

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

Your Signature _____ Date _____

Spouse's Signature _____ Date _____
(if you filed a joint return)

Your Home Phone # _____ Your Daytime Phone # _____

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Appendix Figure 3: Example Notice, returns with no kids

A000050 [redacted] WI 200352 27

Department of the Treasury
Internal Revenue Service [redacted]

Customer Service
1-800-829-0922
Hours of Operation: 7:00 AM - 11:00 PM

Notice Number: CP - 27
Notice Date: JAN. 5, 2004
Social Security Number: [redacted]
Tax Period: DEC. 31, 2001
Form: 1040EZ

[redacted]

NOTICE SUPPORT
XXXXXXXXXXXXXXXXX
ANYWHERE USA 000000-0000

Helpful Hint: For faster service, try calling us any day except Monday when our call volumes are highest.

According to the information you filed on your 2001 tax return, you may be able to take the Earned Income Credit (EIC). To help us decide if you qualify, please answer the following questions:

1. Does you or your spouse's Social Security Card say "Not Valid For Employment"?
   - ☐ **YES -** YOU DO NOT QUALIFY FOR THE CREDIT
   - ☐ **NO**
2. Can your parents (or someone else) claim you as a dependent on their 2001 tax return?
   - ☐ **YES -** YOU DO NOT QUALIFY FOR THE CREDIT
   - ☐ **NO**
3. Did you live in the U.S. for more than half of 2001?
   - ☐ **YES**
   - ☐ **NO -** YOU DO NOT QUALIFY FOR THE CREDIT

If you do not think you qualify for the credit, DO NOT answer this notice.

If you think you do qualify for the credit, sign this notice in the space provided below and return this entire page in the enclosed envelope. We will determine if you qualify for the credit.

If the information you give us shows you do not qualify for the credit, we will let you know. Otherwise, if you owe no other taxes, we will send you a check within the next 8 weeks.

Under penalties of perjury, I declare that I have examined this return information, and to the best of my knowledge and belief, it is true, correct, and complete.

Your Signature _____ **Date** _____

Spouse's Signature _____ **Date** _____

Your Home Phone # _____ **Your Daytime Phone #** _____

For IRS use only:

EI [[redacted]] AGI [[redacted]]
[ ]

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Appendix Figure 4: Example Schedule EIC

SCHEDULE EIC
(Form 1040A or 1040)

Department of the Treasury
Internal Revenue Service (99)

Earned Income Credit
Qualifying Child Information

Complete and attach to Form 1040A or 1040 only if you have a qualifying child.

OMB No. 1545-0074

2005

Attachment Sequence No. **43**

Name(s) shown on return

Your social security number

Before you begin: See the instructions for Form 1040A, lines 41a and 41b, or Form 1040, lines 66a and 66b, to make sure that **(a)** you can take the EIC and **(b)** you have a qualifying child.

CAUTION

- If you take the EIC even though you are not eligible, you may not be allowed to take the credit for up to 10 years. See back of schedule for details.
- It will take us longer to process your return and issue your refund if you do not fill in all lines that apply for each qualifying child.
- Be sure the child's name on line 1 and social security number (SSN) on line 2 agree with the child's social security card. Otherwise, at the time we process your return, we may reduce or disallow your EIC. If the name or SSN on the child's social security card is not correct, call the Social Security Administration at 1-800-772-1213.

Qualifying Child Information	Child 1	Child 2
1 Child's name If you have more than two qualifying children, you only have to list two to get the maximum credit.	First name Last name	First name Last name
2 Child's SSN The child must have an SSN as defined on page 44 of the Form 1040A instructions or page 48 of the Form 1040 instructions unless the child was born and died in 2005. If your child was born and died in 2005 and did not have an SSN, enter "Died" on this line and attach a copy of the child's birth certificate.		
3 Child's year of birth	Year ___ ___ ___ ___ *If born after 1986, skip lines 4a and 4b; go to line 5.*	Year ___ ___ ___ ___ *If born after 1986, skip lines 4a and 4b; go to line 5.*
4 If the child was born before 1987— **a** Was the child under age 24 at the end of 2005 and a student?	☐ Yes. *Go to line 5.* ☐ No. *Continue*	☐ Yes. *Go to line 5.* ☐ No. *Continue*
b Was the child permanently and totally disabled during any part of 2005?	☐ Yes. *Continue* ☐ No. The child is not a qualifying child.	☐ Yes. *Continue* ☐ No. The child is not a qualifying child.
5 Child's relationship to you (for example, son, daughter, grandchild, niece, nephew, foster child, etc.)		
6 Number of months child lived with you in the United States during 2005 • If the child lived with you for more than half of 2005 but less than 7 months, enter "7." • If the child was born or died in 2005 and your home was the child's home for the entire time he or she was alive during 2005, enter "12."	_____ months *Do not enter more than 12 months.*	_____ months *Do not enter more than 12 months.*

TIP

You may also be able to take the additional child tax credit if your child **(a)** was under age 17 at the end of 2005, **and (b)** is a U.S. citizen or resident alien. For more details, see the instructions for line 42 of Form 1040A or line 68 of Form 1040.

For Paperwork Reduction Act Notice, see Form 1040A or 1040 instructions. Cat. No. 13339M **Schedule EIC (Form 1040A or 1040) 2005**

Page 1

Schedule EIC (Form 1040A or 1040) 2005 Page **2**

Purpose of Schedule

The purpose of this schedule is to give the IRS information about your qualifying child after you have figured your earned income credit (EIC).

To figure the amount of your credit or to have the IRS figure it for you, see the instructions for Form 1040A, lines 41a and 41b, or Form 1040, lines 66a and 66b.

Taking the EIC when not eligible. If you take the EIC even though you are not eligible and it is determined that your error is due to reckless or intentional disregard of the EIC rules, you will not be allowed to take the credit for 2 years even if you are otherwise eligible to do so. If you fraudulently take the EIC, you will not be allowed to take the credit for 10 years. You may also have to pay penalties.

Qualifying Child

A qualifying child is a child who is your . . .

Son, daughter, stepchild, foster child, brother, sister, stepbrother, stepsister, or a descendant of any of them (for example, your grandchild, niece, or nephew)

AND

was . . .

Under age 19 at the end of 2005
or
Under age 24 at the end of 2005 and a student
or
Any age and permanently and totally disabled

AND

who . . .

Lived with you in the United States for more than half of 2005. If the child did not live with you for the required time, see *Exception to "time lived with you" condition* on page 43 of the Form 1040A instructions or page 48 of the Form 1040 instructions.

CAUTION

If the child was married or meets the conditions to be a qualifying child of another person (other than your spouse if filing a joint return), special rules apply. For details, see page 44 of the Form 1040A instructions or page 48 of the Form 1040 instructions.

Do you want part of the EIC added to your take-home pay in 2006? To see if you qualify, get Form W-5 from your employer, call the IRS at 1-800-TAX-FORM (1-800-829-3676), or go to *www.irs.gov*.

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