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REMINDERS & RECIDIVISM:
EVIDENCE FROM TAX FILING & EITC PARTICIPATION AMONG LOW-INCOME NONFILERS

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Reminders & Recidivism: Evidence from Tax Filing & EITC Participation among Low-Income Nonfilers
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ABSTRACT

This project examines how reminders affect tax filing among lower-income nonfilers (individuals who did not appear on a filed tax return but had income reported by third parties to the Internal Revenue Service). We present novel data on this population and results from two randomized controlled trials. The results demonstrate that one-time reminders increase tax filing, both to claim tax refunds based in part on withholdings and Earned Income Tax Credit benefits, as well as to voluntarily pay balances owed to the IRS. However, these effects do not persist. Consistent with recency effects, individuals who owe a balance due appear more likely to recidivate into nonfiling than those who receive refunds. Follow-up reminders continue to increase tax filing, particularly among individuals who previously had to pay balances to the IRS instead of receive refunds.

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I. Introduction

Common intuition suggests that attention plays a crucial role in individuals' decisions. If individuals are not attentive to certain benefits from taking an action, it seems unlikely that they will take that action. While traditional economic theories have assumed that individuals make decisions as fully informed agents who take all available information into account (Mas-Colell, Whinston and Green 1995 and Varian 2014), a large and growing literature in psychology and economics has developed theories of limited attention or inattention to characterize individuals' decisions (see DellaVigna 2009 for a survey of this literature). These theories highlight how reminders and other informative or persuasive cues can affect a variety of individuals' decisions. For example, studies have examined how simplified information and reminders affect college enrollment (Castleman and Page 2014), exercise (Royer, Stehr and Sydnor 2015 and Calzolari and Nardotto 2015), savings for retirement (Karlan et al 2015), school choice (Hastings and Weinstein 2008), vaccination use and medical treatment adherence (Mauer and Harris 2014 and Raifman et al 2014), and electricity usage (Allcott and Taubinsky 2015).

While many studies of inattention have focused on static settings, there are still many open questions about inattention in dynamic settings. Do inattentive individuals learn and form habits in dynamic settings? Will a one-time reminder have permanent effects on knowledge and behavior, or does inattention inherently require repeated reminders to get individuals to repeatedly take actions and make decisions? Do individuals become inattentive to repeated reminders? While the answers to these questions are likely to vary across different contexts, understanding the factors relevant to these questions is important for academic researchers as well as policymakers. For example, policies requiring actions each year may need to repeat nudges each year to generate sustained changes in individuals' decisions.

In this project, we examine the effects of reminders in the context of a low frequency repeated behavior. In particular, each year, individuals must decide whether or not to file their tax returns independent of having filed a tax return in a previous year. Relative to daily, weekly, or monthly decisions, or settings with little time to get information between decisions, tax filing represents a low frequency decision. More specifically, we focus on potentially EITC-eligible individuals and examine tax filing decisions across multiple years. We present evidence on one-time and repeated reminders to file tax returns, where one-time reminders refer to reminders sent only in a single year and repeated reminders refer to reminders sent in multiple years.

The empirical analysis is based on outreach pilots conducted by the United States Internal Revenue Service. The field experiment setting for these outreach pilots is relevant both for the general academic insights into inattention and for the insights into this particular population. In contrast to some laboratory settings, this field experiment setting involves significant real money stakes for both individuals and the government. For example, for tax year 2012, there were roughly 39 million nonfilers, and about 4 million of these were estimated to be potentially EITC eligible. In this nonfiler population, there are billions of dollars of potential tax refunds *and* potential tax revenue at stake.[1] We present novel, publicly available data on the spatial distribution of the nonfiler population across multiple years below.

[1] See Table 1 and the corresponding discussion of these summary statistics on this nonfiler population below.

Using a stratified random sample of roughly 360,000 potentially EITC eligible nonfilers from tax years 2011 and 2012, we conducted randomized controlled trials (RCTs) in 2014 and 2015. A sample of 200,000 individuals was randomly selected to receive EITC and filing information from the IRS during the 2014 tax filing season (generally, January 15 through April 15 in a given year). The second RCT built upon the first. The experimental sample for the second RCT consisted of individuals in the first RCT who filed tax returns during the 2014 filing season. Of these roughly 130,000 tax returns, 30,000 were randomly selected to receive one reminder notice from the IRS during the 2015 tax filing season. For each RCT, a control group did not receive any experimental reminders. Importantly, the treatment group for the 2015 RCT included some individuals who received treatment from the 2014 RCT and some who did not.

The results from the experiments provide multiple insights about inattention, reminders and recidivism. First, the results demonstrate that, in a setting with real money at stake, reminders appear to reduce inattention. In particular, in both RCTs, the treatment group had higher filing rates than the control group. The treatment group was more likely to file both current and prior-year tax returns, and was more likely both to claim refunds and pay taxes owed. Notably, the increased filing rates across all income levels, potential benefit levels and filing histories.

Second, in this setting with low frequency decisions, one time reminders generated one-time effects: the effects of reminders in 2014 did not persist to 2015. These results suggest that the reminders reduced inattention when they were received, but individuals did not permanently learn from the reminders. Third, the repeated reminders reduced recidivism. Specifically, individuals who received reminders in both the 2014 and 2015 RCTs had a higher filing rate than individuals who received reminders in the 2014 RCT only or not at all. Consistent with a lack of learning from reminders in this setting, the follow-up reminder did not have diminished effectiveness as individuals who received reminders in the 2014 and 2015 RCTs had a similar filing rate as individuals who received reminders in the 2015 RCT only.

A fourth result is that past experiences were influential and interacted with the effectiveness of the follow-up reminders for one group. Individuals who had a balance due on their 2013 tax return were less likely to file a 2014 return compared to individuals who received a refund on their 2013 return. Further, the follow-up reminder appeared to be particularly effective at reducing recidivism among those who previously owed taxes compared to among those who previously received refunds. This result suggests that some low-income nonfilers may follow an intuition of expecting to pay a balance due in the future if they file a tax return, and this expectation is based on the experience of having had to pay a balance due when they most recently filed a tax return. This intuition indicates some low-income nonfilers may suffer from recency bias or a law of small numbers bias. However, the reasoning may be flawed because, at least for these individuals and perhaps true more broadly, tax situations appear to change significantly from year to year: of the people in the 2015 RCT control group who owed a balance due on their 2013 tax return, roughly 50% of them qualified for a refund on their 2014 tax return.

Overall, the results suggest a model of inattentive behavior in which informational nudges can affect behavior, but individuals do not appear to learn from the informational nudges themselves. Instead, individuals do appear to, perhaps mistakenly or suboptimally, draw inferences from recent realizations of uncertain outcomes. Thus, when considering the impacts of informational

interventions on individuals' well-being, it is important to consider whether or not the set of available policy instruments includes follow-up reminders or only one-time reminders, and the potential for individuals' mistaken or suboptimal inferences to affect future actions.

These insights regarding inattention, reminders and recidivism are broadly relevant to the literatures on consumer choice, psychology, economics, and public finance. While recent work in behavioral economics has aimed to develop dynamic models of consumer choice with inattention (see Taubinsky 2013), the current results help inform such models by providing evidence of how attention and reminder effects decay and how reminder effects can be repeated with follow-up reminders. The current results also relate to a literature in psychology and individual choice on belief adjustment and recency effects (see Hogarth and Einhorn 1992 for a discussion of theoretical foundations for recency effects and Davelaar et al 2009 for more recent evidence). The current results present evidence consistent with possibly suboptimal recency effects or recency bias in a field setting that involves potentially billions of dollars of tax refunds and tax revenue. Furthermore, while other studies have considered settings with high frequency decisions and high frequency reminders (e.g. daily or weekly decisions to put effort at school or go to the gym accompanied by daily or weekly reminders about these decisions as in Calzolari and Nardotto 2015 and Bergman 2015), the current results demonstrate that inattention can be persistent in a dynamic setting with low frequency (annual) decisions. Related to the consumer choice literature, recent literature has highlighted the impacts of various policy nudges on a variety of behaviors (see Thaler and Sunstein 2008, Chetty 2015, Bhargava and Manoli 2015, Manoli and Turner 2015, and the Social and Behavioral Science Team 2015). Results from our study suggest that policy nudges may be effective at generating one-time effects, but they may not generate permanent learning. Thus, to create permanent changes in behavior, nudges may have to be repeated each year or whenever consumers face the relevant decisions.

Our experimental analysis is also relevant for the literature on incomplete take-up of benefits. Incomplete take-up is a widespread phenomenon with potentially severe consequences (Haveman et al 2001, Jones et al 2003). Even for large-scale, popular social programs including the Earned Income Tax Credit, there is significant incomplete take-up of benefits (Currie 2006, Kleven and Kopczuk 2011, Scholz 1994, Hotz et al 2003, Plueger 2009, Bhargava and Manoli 2015). This incomplete take-up across social programs is particularly concerning for policymakers and researchers because many of these programs are intended to provide benefits to individuals with limited resources, and those with the most limited resources may be the ones who face the most barriers to claiming benefits for which they are eligible (Bertrand, Mullainathan and Shafir 2004). Our experimental design highlights that, while the motivation for third-party information reporting to collect tax revenue is typically based on reducing tax administration costs and tax evasion through improved monitoring and detection (Slemrod 2007), third-party information reporting also creates the opportunity for the tax collection agency to apply behavioral economics and advance public policy (Remler and Gleid 2003, Congdon, Kling, and Mullainathan 2009 Baicker, et al 2012, and Chetty 2015).

The remainder of this paper is organized as follows. Section II provides a conceptual framework. Section III discusses the data and experimental design. Section IV presents the empirical analysis and results, and Section V summarizes the findings and discusses potential future studies that can build on this research.

II. Conceptual Framework

We present a simple framework to organize thoughts about reminders and recidivism. In particular, this framework is intended primarily to clarify hypotheses about inattention, reminders and recidivism. Karlan et al (2015) and Taubinsky (2013) provide more complete dynamic models of choice with inattention, and DellaVigna (2009) and Mullainathan, Schwartzstein and Congdon (2012) present static models of inattentive choice.

For simplicity, we present a two-period setting in which an individual is making a decision whether or not to take an action in each period. The action may be an occasional purchase, or as in our experimental setting, filing a tax return. We will assume that the action chosen in the first period does not affect the decision in the second period and vice versa. For example, if an individual files a tax return in the first period, the person still has to make a decision about filing a tax return in the second period. For this purpose, we assume that in each period the individual will take action if and only if expected benefits exceed costs. The cost in each period is denoted by c.

Following the literature in behavioral economics, we assume that benefits in each period are the sum of a visible component, v, and an opaque component o. The individual is fully attentive to the visible component, but inattentive to the opaque component. Using $\theta \in [0,1]$ to denote the degree of inattention, the perceived benefit in each period is $v + (1-\theta)o$, so $\theta = 0$ corresponds to having full attention. We assume that benefits have some uncertainty and that this uncertainty applies only to the visible component, so v is drawn from some random variable. Given this set up so far, the individual will take action in each period if expected benefits exceed the cost,$E(v) + (1-\theta)o > c$.

Next we introduce reminders in each period. In particular, we let $r \in \{0,1\}$ denote a reminder, where $r = 1$ indicates having received a reminder, and $r = 0$ indicates not having received a reminder. Starting with the first period, we assume that the reminder affects the degree of inattention so that $\theta_1 = \theta_1(r_1)$ with the subscript referring to period 1. With this reminder, the individual will take action in period 1 if

$$E_1(v) + [1 - \theta_1(r_1)]o > c.$$

We now turn to the second period. In this period, the individual receives the second reminder r_2, also has the reminder from the first period r_1 and the realized value of the visible component (filed a return) from period 1, v_1. We model the degree of inattention in the second period as being a function of these three components so that $\theta_2 = \theta_2(r_2, r_1, v_1)$. As we discuss in more detail below, a primary motivation for this conceptual framework is to explain our hypotheses regarding how inattention in period 2 relates to each of these three components.

Before turning to the hypotheses, we also introduce notation to account for possible learning that is independent of inattention. Specifically, we allow the realized value of the visible component from period 1 to affect beliefs about the visible component in period 2. In this case, the

expectation of the visible component in period 2 is given by $E_2(v|v_1)$. Thus, the individual will take action in the second period if

$$E_2(v|v_1) + [1 - \theta_2(r_2, r_1, v_1)]o > c$$

Within this framework, we define recidivism as not taking action in period 2 conditional on taking action in period 1. In other words, recidivism refers to not filing a tax return in the current year conditional on having filed a tax return in the previous year.

Using this framework, we now discuss multiple hypotheses about reminders.

1. Reminders reduce inattention: $\theta_1(1) < \theta_1(0)$ and $\theta_2(1, r_1, v_1) < \theta_2(0, r_1, v_1)$.
The decision rules specify that the reminders affect decisions through the inattention parameters, and that these parameters are distinct from beliefs. Because beliefs govern expectations over the visible components of benefits, reminders could reduce inattention without affecting expectations or beliefs about possible benefits. To check this, one could survey reminder recipients before and after receipt of the reminders to see if the reminders affected their beliefs and/or expectations.

2. Persistence: $\theta_2(r_2, 1, v_1) < \theta_2(r_2, 0, v_1)$.
If reminders from period 1 have a persistent impact on inattention, then we expect the degree of inattention in the second period to go down based on having received a reminder in the first period. In this case, we expect less recidivism (inaction) in period 2 based on having received a reminder in period 1. On the other hand, if there is no persistence, the degree of inattention in the second period will be independent of having received a reminder previously or not, i.e. $\theta_2(r_2, 1, v_1) = \theta_2(r_2, 0, v_1)$ so individuals that received reminders in the first period will have the same degree of recidivism in the second period as individuals who did not receive reminders in the first period. To test for persistence, one can test for differences in the likelihood of taking action in the second period based on having received a reminder previously or not.

3. One-time vs. repeated reminders: $\theta_2(1,1, v_1)\ vs.\ \theta_2(1,0, v_1)$.
If repeated reminders are more effective than a one-time reminder, then we expect that there will be less inattention in the second period (i.e. less recidivism into inaction in the second period) if the individual has previously received a reminder in the first period. Thus, this hypothesis is related to the persistence hypothesis, but it is more specific because it is conditional on receiving a reminder in the first period, whereas the persistence hypothesis is unconditional. To test this hypothesis, one can therefore condition on individuals receiving a reminder in the second period and test for differences in the likelihood of taking action in the second period based on having received a reminder in the first period or not.

4. Recency effects
The role of past experience in expectations of current beliefs is captured within the term $E_2(v|v_1)$. This term refers to the expectations of benefits in period 2 given the realization of the uncertain outcome in period 1. Individuals may use their previous outcome from period 1 to update their beliefs about period 2 benefits. More specifically, individuals may use their most recent outcome from period 1 to anchor their beliefs about the potential outcome in period 2.

Following Hogarth and Einhorn's (1992) belief adjustment model with anchoring and recency effects, we refer to this anchoring based on most recent outcomes as recency effects. We note that these recency effects could be a mistaken or suboptimal way of updating beliefs if benefits in period 2 are actually independent from period 1 outcomes.

To check for evidence of recency effects, one can examine the likelihood of taking action in period 2 conditional on period 1 realizations. For example, evidence of people with lower period 1 realizations being less likely to take action in period 2 could be evidence of recency effects. In examining this evidence, however, it is also important to assess the extent to which lower realizations of period 1 outcomes are actually correlated with lower realizations in period 2. This distinction is important for distinguishing heterogeneity across individuals with different beliefs or expectations from actual biases in beliefs across similar individuals who had randomly different outcomes.

5. Past experience and interactions with reminders

$$\underbrace{\left|\theta_2\left(1, r_1, v_1^{salient}\right) - \theta_2\left(0, r_1, v_1^{salient}\right)\right|}_{\text{reminder effect with salient experience}} > \underbrace{\left|\theta_2\left(1, r_1, v_1^{non-salient}\right) - \theta_2\left(0, r_1, v_1^{non-salient}\right)\right|}_{\text{reminder effect with non-salient experience}}.$$

Independent of inattention, individuals may learn from period 1 experiences, and some period 1 experiences may be more salient than others. One can examine evidence of learning by studying how the likelihood of action in period 2 relates to realized values (experiences) from period 1. Evidence of systematic patterns may suggest some learning. Moreover, more salient or intense past experiences may make individuals more susceptible or sensitive to reminders. For example, an individual may be more sensitive to a reminder to buy eggs if the eggs were used to make a delicious cake. In this case, the delicious cake is not only a good outcome, but it may also be very memorable or salient the next time buying eggs comes up. Similarly, in the context of filing a tax return, a balance due in the first period may be more salient or memorable than receiving a refund. Thus, reminders may be more effective at reducing inattention among people who had a balance due as opposed to individuals who received refunds. It's also possible that receiving a communication from the IRS, even a simple informational postcard, may be a strong incentive to file a required return.

In addition to the interaction between past experience and reminders, we note that past experience can also affect behavior in period 2 through beliefs, i.e. through $E_2(v|v_1)$. These learning effects are independent of the effects of reminders. Intuitively, even absent any reminders in period 1 or 2, individuals who file tax returns and receive a refund in period 1 may learn about benefits from tax filing and may be more likely to file tax returns in period 2.

Heterogeneous Treatment Effects

Thus far, the framework has focused on the effects of reminders on attention to potential benefits. Nonetheless, it is also possible to consider the impacts of reminders on attention to potential costs. For some individuals, the costs of not filing a tax return may involve perceived costs and opaque costs, and a reminder to file may increase attention to potential penalties or other costs from not filing a tax return. Moreover, the same reminder may have different effects

on different individuals: some individuals may perceive the reminder in terms of attention to potential benefits from filing, while others may perceive the reminders in terms of attention to potential penalties from not filing. We acknowledge this potential for heterogeneous treatment effects here and discuss this in more detail below, specifically in relation to the effects of the follow-up reminders.

III. Experimental Design

A. Data & Experimental Population

The experiment was based on administrative tax data from the IRS. To construct the experimental population, we first created a dataset of potentially EITC-eligible nonfilers for Tax Years (TY) 2011 and 2012. The steps to create this dataset were as follows. First, a population of individuals is identified using the set of all information returns (e.g., Forms W-2 and 1099MISC, as well as other forms) for TY2011 or TY2012. This population was checked against data on filed tax returns for the relevant year. Individuals who filed a return or who were claimed as a dependent on a return for the relevant year were removed from the experimental population. Individuals who were deceased as of January 1, 2014, claimed as a dependent, did not have positive earned income based on information return data, or were not US citizens or residents were filtered out of the experimental population.

Figure 1 presents novel, publicly available data on the spatial distribution of nonfilers.[2] The maps plot the fraction of nonfilers in each county from TY2005 through TY2013. Specifically, we calculate the fraction of nonfilers in a given county by computing the total number of nonfilers divided by the sum of nonfilers and filed tax returns. While the maps highlight significant variation across and within states, the overall patterns indicate higher nonfiling fractions concentrated in southern or southeastern states. Additionally, the plots indicate that places with high degrees of nonfiling generally continue to be high nonfiling areas across the years. The one exception is TY2007 in which nonfiling decreased across all areas. This year corresponds to the Economic Stimulus Act of 2008 which required individuals to file TY2007 returns in order to receive stimulus checks. The maximum amount of the stimulus rebates was $600 per individual.[3] This suggests that many nonfilers may be responsive to financial incentives or information on financial incentives for filing tax returns. The plots for subsequent tax years highlight significant recidivism as the higher nonfiling fractions across areas re-emerge.

Next, potential EITC benefits were estimated for the remaining nonfiler population. Individuals were matched to potential qualifying children (QCs) using Social Security Administration birth record data. Potential EITC benefits were estimated based on income information and the

[2] Data for these plots will be publicly available on the IRS Tax Stats webpage in early 2016. Nonfilers for TY2014 have not yet been identified since the late TY2014 tax returns could be filed toward the end of calendar year 2015. To create the data for these plots, we first compute the number of nonfilers and filed returns in each five-digit zip code. Information returns are used to determine the zip codes for nonfilers, and home addresses on tax returns are used to determine the zip codes for filed returns. Five-digit zip codes with less than 100 nonfilers or less than 100 returns are dropped to preserve confidentiality. The five-digit zip codes are then matched to counties, and sums of nonfilers and returns are computed within each county. Finally, the nonfiler fraction is computed as the total number of nonfilers divided by the sum of nonfilers and filed tax returns.

[3] See Ramnath and Tong (2015) for an analysis of the impacts of the economic stimulus on tax filing.

number of QCs. An individual's child who was under the age of 19 and not claimed as a dependent on anyone else's return for the study year was considered a QC. Individuals who had positive estimated EITC benefits were retained for the experimental population. Lastly, other filters were applied (such as receipt of combat pay (assumed extension to file), a past or present EITC ban, or any current enforcement action). The final database of potentially EITC-eligible nonfilers consisted of about 2.2 million nonfilers with no QC and just over 600,000 nonfilers with one or more QCs for TY2011, and about 2.5 million single nonfilers and 1 million nonfilers with potential QCs for TY2012.

Table 1 presents summary statistics on the population of potentially EITC-eligible nonfilers from TY2011 and TY2012. Average wage income and total gross income are roughly $6,000 and $7,500 for this population, so these nonfilers have lower income levels than typical EITC recipients (see Manoli and Turner 2015). About 75% of the nonfilers do not appear to have any potential qualifying children. Thus, the average estimated EITC benefits for the nonfiler population is on the order of $500 dollars because most individuals in the data have benefits computed based on having no qualifying child which has a maximum benefit of $464 and $475 for TY2011 and TY2012, respectively. About 25% of the experimental population had an estimated positive tax liability before refundable credits of roughly $150. These summary statistics indicate that there are potentially billions of dollars of tax refunds and tax revenue at stake with this nonfiler population.

The summary statistics on individuals' filing histories for TY2001 through TY2010 tax returns indicate that roughly 14% of these nonfilers never filed a tax return for these years, and a similar fraction always filed a tax return for these years. Furthermore, for the individuals who filed one or more returns, the fractions are roughly constant at 8%. This indicates that, for this population, many individuals move in and out of filing over multiple years.

To determine the experimental sample, a stratified random sample of 400,000 nonfilers (200,000 from each tax year) was drawn from the experimental population. National Change of Address (NCOA) data from the United States Postal Service was used to obtain updated address information for this sample. Updated address information was unavailable for about 10% of the sample. From the remaining 360,000 individuals, a stratified random sample of 200,000 individuals (100,000 from each tax year) were assigned to the treatment group, and the remaining roughly 160,000 individuals were assigned to the control group.[4] Table 1 presents the summary statistics for the experimental sample. Overall, the sample is comparable to the experimental population, though income is slightly lower.

B. Experimental Treatments

The analysis consisted of two RCTs, with the first in 2014 and the second in 2015. The 2014 experimental treatments were as follows. The control group received no intervention. Individuals in the treatment group were randomly assigned one of six treatments: (1) an early postcard, (2) an early brochure, (3) an early postcard and a late brochure, (4) an early brochure and a late postcard, (5) an early postcard and a late postcard, or (6) an early brochure and a late brochure.

[4] At the time of mailing, 10 individuals in the treatment group were determined to not have up-to-date address information, so these observations were dropped from the sample.

Figures 2A-C present examples of the postcard and brochure respectively. The early mailing occurred on March 21, 2014, and the late mailing occurred on April 2, 2014. The mail was tracked so that undeliverable mail was identified and recorded.

The second RCT was conducted during the 2015 tax-filing season, and it built upon the 2014 RCT. In particular, the experimental sample for the 2015 RCT was individuals in the 2014 RCT who filed their TY2013 tax returns during the 2014 tax-filing season after the 2014 experimental mailings were sent out. Out of this sample, 30,000 individuals were randomly selected for treatment and the remaining were retained as a control group. This treatment group received two identical experimental postcards during the 2015 tax-filing season. Specifically, the postcards were mailed on February 23, 2015 and March 23, 2015. The experimental postcards for the 2015 RCT were the same as those sent out in the 2014 RCT, but the wording was updated for the 2015 tax-filing season and deadlines. Figure 3 presents an example of the postcard for the 2015 RCT.

IV. Empirical Analysis

A. Methods

We first analyze and provide results for the 2014 RCT and then turn to the 2015 recidivism RCT. For the main empirical analysis presented here, we pool all of the treatment groups and present a comparison to the control group. Angrist and Pischke (2008) and Cameron and Trivedi (2010) provide textbook discussions of the empirical methods used here. We assess the impacts of the experimental outreach on a variety of outcomes that we describe in more detail below. To fix ideas, we denote an outcome of interest for individual *i* as y_i. Using this measure, we estimate the following regression specification

$$y_i = \alpha + \beta received_i + \varepsilon_i$$

where $received_i$ is an indicator equal to 1 if individual *i* received treatment and 0 otherwise. To account for some individuals not receiving intended treatment due to printing or mailing issues, we present treatment-on-the-treated (TOT) results. In particular, in the regression specification above, we instrument for receiving treatment using the original random assignment. Thus, we estimate the regression above using Two Stage Least Squares (Instrumental Variables), and the first stage is given by

$$received_i = \pi + \rho treated_i + u_i$$

where $treated_i$ is an indicator equal to 1 if individual i was randomly assigned to treatment and 0 otherwise. Intuitively, the TOT estimation strategy re-scales the raw difference in outcomes between the treatment and control groups by the fraction of the treatment group that actually received treatment.

Using these regression results, we present the estimated coefficient $\hat{\alpha}$ as the mean outcome for the control group and $\hat{\alpha} + \hat{\beta}$ as the mean outcome for the treatment group. We also separately present the estimated difference $\hat{\beta}$.

B. Results

1. 2014 RCT

Filing

Table 2 presents the first empirical result from the 2014 RCT.[5] The first result is that the outreach increased tax filing for both prior-year tax returns and current-year tax returns. The table presents the filing fractions for both prior-year tax returns (TY2010-TY2012) and current-year tax returns (TY2013) for the treatment and control groups separately. The table highlights that receiving treatment increased filing rates by roughly 0.5% to 1%. These treatment effects may seem small in absolute terms, but they may be viewed as large relative to the baseline filing rates for the control group, particularly for the prior-year returns, which had much lower baseline filing rates. Consistent with having randomly selected the treatment group, the results in the table demonstrate that the treatment effects are robust to controlling for a variety of covariates. This result relates to the reminders reducing inattention, the first implication discussed in the conceptual framework.

The table indicates that all filing rates are higher for the treatment group. Separate results not shown here indicate that the treatment group was more likely to file for two or more years rather than just the current-year return. The lower baseline filing rates for prior-year returns may be driven by individuals facing relatively high barriers to obtaining tax documents for prior years compared to obtaining tax documents for the current year. Nonetheless, these results indicate that the specific informational content of the experimental reminders reduced inattention not only to filing current-year tax returns, but also to filing unfiled prior-year returns.

Figure 4 presents evidence on internal validity and the timing behind the effects of the reminders. This figure plots the hazard rates for filing TY2013 tax returns (i.e. the fraction of individuals filing a TY2013 tax return at a given date, conditional on not having filed earlier) for the treatment and control groups. The figure strengthens the case for a causal interpretation of the experimental mailings as it illustrates that, prior to the experimental mailings being sent out, the filing hazards for the two groups appear similar, and after the experimental mailings were mailed, the filing hazards for the treatment group increase relative to those of the control group. Thus, it appears that some responses to the experimental treatment occurred almost immediately upon receipt of the experimental mailings. Note that the plot also illustrates that, after the experimental mailings were sent, the filing hazards for the treatment group appear consistently higher than those of the control group for all subsequent dates.

Table 3 presents evidence on the differential effects of the different treatments. The results on the individual treatments (Panel A) and comparisons of the postcard versus the brochure (Panel B)

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[5] For the first stage regression, we estimate $\hat{\pi} = 2.25e\text{-}12$ with a standard error of 0.0008 and $\hat{\rho} = 0.7290$ with a standard error of 0.0011. These results indicate that roughly 73% of individuals randomly selected for treatment ended up receiving the intended treatment. The treatment-on-the-treated (TOT) results therefore re-scale the reduced-form differences in the outcome means for the treatment and control groups by $\frac{1}{0.7290} = 1.3717$. Intuitively, for a given outcome, the treatment effect is scaled up from the reduced form difference between the treatment and control groups because only 73% of the treatment group actually received treatment.

and one contact versus two contacts (Panel C) indicate that there were no statistically significant differences due to presentation or the number of contacts. These results suggest that the postcard and brochure were equally as effective at reducing inattention to the tax-filing decision. Moreover, a single reminder may be as effective as reminders separated by less than 2 weeks.

Prior-Year Returns

Table 4 presents an analysis of filing prior-year returns and the characteristics of these filed returns. Analyzing these characteristics provides evidence on which particular types of individuals responded to the treatment. In these results, we focus on TY2011 returns for TY2011 nonfilers, and TY2012 returns for TY2012 nonfilers. Consistent with the earlier results in Table 2, the treated individuals had higher likelihoods of filing their previously unfiled returns. To test if the treatment only induced filing among those who would receive refunds as opposed to those who may owe a balance due, we examine the fractions of the treatment and control groups that received refunds on the prior-year returns, and the fractions that paid a balance due. If the treatment only induced responses among those who would receive refunds, then we would expect that the treatment group would have a higher fraction of receiving a refund than the control group, and that the fractions owing a balance due would be equal across the treatment and control groups. These results in Table 5 indicate that, for both nonfiler groups, the treatment group had higher fractions receiving a refund and paying a balance due. These results indicate that the treatment increased filing among both nonfilers who qualified for refunds as well as those who owed a balance due.

We next turn to examining the characteristics of the prior-year returns conditional on filing to see if there were specific types of nonfilers who responded to the treatment more so than others. Overall, the results indicate that the likelihood of claiming the EITC, having self-employment income, and the use of paid preparers, appear similar for the treatment group and control group. These results suggest that the treatment did not appear to induce filing among specific groups along these dimensions.

The results on the fraction claiming EITC benefits in Table 4 are of particular interest given that the experimental sample focused on identifying potentially EITC-eligible nonfilers for TY2011 and TY2012. The results indicate that roughly 50% of filed prior-year returns claimed EITC benefits. The analysis of these prior-year returns indicates that many of these returns had earned income that was higher than the earned income EITC eligibility thresholds for the corresponding filing status and number of qualifying children reported on the return. This was driven primarily by two factors. First, some filed a joint return, and the spouse's income led to total earned income being beyond the EITC eligibility thresholds. Second, some individuals reported self-employment income that led to total earned income being beyond the EITC eligibility thresholds. Indeed, after conditioning on earned income on the filed return being below the EITC eligibility thresholds, the fraction of prior-year returns that claimed EITC benefits was roughly 80% for TY2011 returns and just under 90% for TY2012 returns. These percentages are comparable to the percentage of individuals claiming EITC benefits when filing current-year returns and appearing to meet EITC eligibility conditions (see Manoli and Turner 2015).[6]

[6] Appendix Table 1 examines whether or not nonfilers that were associated with a potential qualifying child filed with a qualifying child. Overall, there was a low rate of predicting an association to a qualifying child for returns

Interestingly, the EITC claim rate on the filed prior-year returns was similar across the treatment and control groups. This indicates that, while the treatment did increase tax filing and hence EITC participation, conditional on filing, the treatment did not increase the likelihood of claiming EITC benefits. These results suggest that the informational content of the mailings may have primarily increased attention to filing and not just to EITC benefits. For example, in the extreme case that the informational content only made EITC benefits salient but did not make tax filing salient, we may have expected filing rates to be constant for the treatment and control groups but EITC take-up rates to be higher for the treatment group than for the control group.

Similar to Table 4 on prior-year returns, Table 5 presents an analysis of current-year (TY2013) returns for the 2014 RCT. These results are similar to those in Table 4 in that they indicate that the treatment increased filing generally, but it did not appear to induce filing more so for any particular group of individuals measured along various tax characteristics.

Refunds Received and Balances Paid

Building on the results in the previous tables, another main result is that the experimental outreach increased both refunds received as well as balance dues paid. To examine the impacts on refunds received and balance dues paid, we compute total refunds received for each individual as the sum of refunds received from prior-year and current-year tax returns. Similarly, total balance dues paid for each individual are computed by summing balances paid from prior-year and current-year returns. The results in Table 6 show that, on average, individuals in the treatment group received $38 per individual more than the control group, and they paid roughly $50 per individual more than the control group. These results are consistent with the reminders possibly having heterogeneous impacts on beliefs for different groups. For example, the reminders may have increased perceived costs of filing for individuals who owed taxes, but may have increased perceived benefits from filing for individuals who could receive a refund.

Persistence

The effects of the outreach do not appear to have persisted to the next year. Table 7 presents the fractions filing 2014 tax returns for the treatment and control groups of the 2014 RCT. These tax returns were filed during the 2015 filing season, one year after the treatment mailing from the 2014 RCT had been sent out. These results indicate that the filing rates for 2014 tax returns are virtually identical across the treatment and control groups. Furthermore, these results demonstrate that there was no evidence of persistence even among individuals who filed 2013 tax returns during the 2014 filing season, owed taxes on these returns, or received refunds on

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that filed without any qualifying children. Many prior-year returns were filed without a qualifying child, and they had been predicted to not be associated with a qualifying child. Appendix Table 2 examines predicted income and income reported on filed tax returns. Overall, there was a strong positive correlation between predicted income and total income reported on tax returns, but there was also significant variation in total income beyond the variation in gross income. These could be driven, for example, by unobserved spousal income and unobserved self-employment income. Comparing predicted wage amounts and wage amounts reported on the 1040s indicates that predicted wages explained more of the variation in 1040 wages than gross income did with total income. However, there was still significant variation in wages reported on 1040s beyond the variation in predicted wages.

these returns. Moreover, there is little evidence of persistence even among individuals who had a tax preparer.

We also note that it is useful to consider the magnitudes of the treatment effects and standard errors when examining persistence. Table 7 indicates that the standard error for the difference in filing rates for TY2014 tax returns is 0.002. With this standard error, we have statistical power to detect a difference of 0.004. Table 2 indicates that the treatment effect on filing TY2013 tax returns is 0.010. Thus, the standard error in Table 7 suggests that less than 40% (0.004 out of 0.010) of the treatment effect persisted into the following year. We conclude that the IRS reminders were effective at increasing filing only in the year that they were sent out, and subsequently there is essentially almost complete recidivism into pre-treatment behaviors. This is consistent with the effects of other tax notices also fading out in subsequent years (see Manoli and Turner 2015).

Heterogeneity

We next turn to examining heterogeneity in the filing rates and treatment effects across multiple dimensions. This analysis of heterogeneity allows for a further characterization of inattention as the potential mechanism behind nonfiling and responses to the experimental treatment. In particular, we examine heterogeneity along the following dimensions: potential EITC benefits from unfiled tax returns, gross income on unfiled returns, and filing histories. Figure 5 presents the analysis of heterogeneity along these dimensions. In particular, these figures present separate series of filing rates for the treatment and control groups across these dimensions of heterogeneity.

Panel A indicates that, with the exception of slightly lower filing rates at very low or very high potential benefit amounts, baseline filing rates for the control group are mostly stable across potential benefit amounts. Additionally, at most levels of potential benefits, the treatment group had slightly higher filing rates, and the difference between the treatment and control group is relatively stable across potential benefit amounts. If individuals were at least partially attentive to their potential EITC benefit amounts , we would expect to see an increase in the baseline filing rates for the control group as actual benefit levels increase. Furthermore, if the treatment reminded them to claim those potential benefits, we would expect to see larger differences between the treatment and control groups at higher potential benefit levels. The relatively stable differences between the treatment and control groups across potential benefit levels suggests that the treatment did not impact beliefs about the potential benefit but instead increased attention to the requirement to file or the opportunity to receive a refund.[7]

Panel B presents filing results across gross income amounts from unfiled returns. Filing rates for the control group appear to increase slightly with gross income, and the treatment group has higher filing rates at each income level. The differences between the treatment and control

[7] The patterns in Panel A are also consistent with individuals knowing their potential benefits for current-year returns and these potential benefits being uncorrelated with potential benefits from unfiled prior-year returns. However, the patterns in Panel A could also be explained by measurement error in potential benefits from unfiled returns so that potential benefits are unrelated to true benefits from unfiled returns. Because some forms of income are unobserved (e.g. cash income), we cannot definitely rule these alternative hypotheses out.

groups appear relatively stable across gross income levels. Thus, although higher-income nonfilers may be more attentive to filing (possibly because they are more likely to be required to file), the reminders appear to affect inattention equally across lower- and higher-income nonfilers.

Heterogeneity related to filing history also speaks to the nature of inattention among lower-income nonfilers. If lower-income nonfilers with limited filing histories are particularly inattentive, we might expect 1) a positive relationship between the number of past returns filed and the likelihood of filing and 2) a positive relationship between the number of past returns filed and responses to the treatment. Panel C presents filing rates for the experimental sample broken down by different filing histories based on TY2001 through TY2010 tax returns. The fact that baseline filing rates increase with the number of past returns filed suggests that individuals with more complete filing histories are more attentive to benefits from tax filing. The plot also shows that, while there are minimal differences between the treatment and control groups, the differences in filing rates are relatively stable in cases where at least five past returns were filed. Thus, it appears that the experimental outreach reduced inattention relatively equally across groups that had a consistent but incomplete connection to the tax system, but not so much for groups with minimal or no previous connection with the tax system. (The latter group may be making an attentive decision not to file.)

2. 2015 RCT

Repeated Reminders

Table 8 presents the results from the 2015 recidivism RCT. A first result from the 2015 follow-up outreach is that a single postcard increased filing and EITC participation, even though the sample population all filed a 2013 return in the prior year. Viewed from the perspective of nonfiling instead of filing, the results for the control group indicate that roughly 47% of the 2013 filers recidivate into nonfiling, and the treatment reduced this recidivism by 4% to 43%. Moreover, the results for the 2014 and 2015 treatment and control groups indicate that there were no statistically significant differences in the effects of the 2015 outreach based on whether or not individuals also received treatment in 2014. The effect of the period 2 reminder did not diminish based on having received a previous reminder. Thus, relating to the third implication in the conceptual framework, repeated reminders appear more effective than one-time reminders.

Recency Effects & Heterogeneous Responses to Reminders

Next, we examine differences in baseline likelihoods of filing 2014 returns and heterogeneity in the responses to the 2015 treatment based on outcomes from filing the 2013 return. These results relate to the fourth and fifth implications in the conceptual framework.

Figure 6 presents graphical evidence on possible recency effects and heterogeneous treatment effects. In particular, the figures plot the likelihood of filing a TY2014 tax return after having a balance due on the TY2013 tax return. Panel A presents the evidence for the full 2015 RCT sample. To account for differences driven by self-employed individuals, Panel B plots the filing patterns for individuals who did not have any self-employment income on the TY2013 tax

returns. Lastly, to account for differences in income and household differences, Panel C presents covariate-adjusted filing patterns for people with no self-employment income on their TY2013 tax returns.[8] These figures highlight noticeable declines in the likelihood of filing as the prior-year balance due increases.[9]

In their belief adjustment model, Hogarth and Einhorn (1992) distinguish between step-by-step and end-of-sequence information processing modes. We note that tax-filing may fall in the end-of-sequence processing mode because individuals working with a tax preparer or preparing their own returns with tax software may only focus on the bottom line refund or balance due on their tax returns. Hence, the tax-filing setting may be conducive to involving recency effects, and the evidence presented in Figure 6 would be consistent with this intuition.

Consistent with the graphical evidence in Figure 6, Table 8 shows that the 2015 outreach appears to have been particularly effective at reducing recidivism for individuals who had a balance due on their 2013 tax return. From the perspective of nonfiling, roughly 52% of individuals who had a balance due on the 2013 return recidivated into nonfiling, but the treatment reduced this recidivism to about 42%. The results for the control group show that individuals with a balance due had a higher likelihood of recidivating into nonfiling. This is consistent with a recency effect and tax aversion. Intuitively individuals may use their most recent past experience paying a balance due to forecast their current tax situation, and then they may seek to avoid paying a perceived balance due by not filing a tax return.

The treatment effects in Table 8 for those who previously received refunds versus those who paid a balance due are consistent with heterogeneous treatment effects. The reminders may have increased attention to potential benefits of filing for those who previously received refunds, and the reminders may have increased attention to potential penalties for not filing for those who had a balance due.

The results in Table 8 also indicate that reminders appear to have been more effective among individuals who used a paid tax preparer to file their 2013 return. Among individuals in the control group, filing rates were similar across individuals who did or did not have a preparer in the previous year. This indicates that tax preparers themselves may not be effective at reducing recidivism among this population. However, comparing the differences between the treatment and control groups for individuals who did and did not have a tax preparer in the previous year indicates that, not only did the treatment increase filing for both groups, but the increase was almost twice as large for individuals who used a tax preparer than for those who had self-prepared returns.

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[8] The covariate-adjusted filing patterns are obtained via the following steps. First, we regress an indicator for filing a TY2014 return on dummies for filing status, number of EITC qualifying children, having a paid tax preparer, deciles of total wage income, and deciles of total income. Next, we obtain the residuals from this regression and add the mean probability of filing a TY2014 return to the residuals. Finally, we created bins based on TY2013 balance due amounts and then computed the means of the residuals within the bins. Figure 6 Panel C plots the mean residuals by the balance due bins.

[9] The graphical evidence should not be interpreted in terms of a regression discontinuity; as indicated by the plotted frequencies in Appendix Figure 1, most individuals receive a refund or have a zero balance due because of withholdings and refundable tax credits.

Table 9 presents an analysis of characteristics of 2014 returns filed by the treatment and control groups, and also for the subsample of returns who paid a balance due on the previous year's return. Similar to the 2014 RCT, the results in Table 9 indicate that the treatment did not induce filing among any particular group with specific tax characteristics on their 2014 tax returns.

Building on this intuition, Figure 7 plots the fraction of individuals getting a tax refund on their TY2014 tax return conditional on filing these returns against the balance due on their TY2013 tax returns. The graph shows that the fractions do not differ noticeably between the treatment and control groups. Moreover, while there is a noticeable decline in the likelihood of getting a refund for those who previously had a balance due, roughly 50% of those who had a balance due on their TY2013 returns received a tax refund on their TY2014 returns. This highlights that tax situations for the experimental sample appear to change significantly from year to year so that many individuals who previously owed a balance due last year may qualify for a refund in the current year. Thus, if individuals have recency effects in their beliefs about their current tax situations based on their most recent filing, it may be erroneous for them to conclude that they are likely to owe a balance due in the current year simply because they owed a balance due in the previous year. Moreover, the possible recency effects also highlight that EITC-eligibile individuals may move toward nonfiling if they previously fail to claim EITC benefits that could have more than offset the balance due on the return.

V. Conclusions

The primary goal of this study has been to provide insights into inattention, reminders and recidivism using a setting of the annual tax-filing decision. We summarize the broad results from the two field experiments conducted by the IRS as follows. First, reminders reduce inattention and increase tax filing. Second, in this setting with annual decisions, reminders to file in a given year have impacts on filing in that year only (i.e. impacts do not persist to future years). Third, repeated reminders reduce recidivism and generate increased attention to tax-filing to the same degree as the first reminder. Fourth, individuals appear to be influenced by past experiences (especially negative ones), and individuals' receptiveness to reminders appears to be affected by these past experiences. These results contribute to the literatures on consumer choice, behavioral economics and tax policy. Even though we acknowledge that results and intuitions from this analysis may be specific to lower-income nonfilers studied here, many of them can also apply to the population more broadly. Individuals who did not file tax returns despite having income may be more likely to be inattentive or receptive to reminders than the population of individuals who regularly file tax returns each year. Nonetheless, the population of low-income nonfilers is large enough that there are potentially billions of dollars of tax refunds and tax revenue at stake.

While the current research has examined inattention with individual-level treatments, future research may examine inattention in a social context and consider the role of peer influence. For example, advertising reminders to broad populations may reduce inattention more effectively than individual-level reminders because individuals may discuss the ad thereby reminding one another about the need to take a given action. On the other hand, advertising reminders to broad populations may be less effective than individual-level reminders if individuals are not attentive to impersonal messaging or if they are not attentive to transmitting reminders to peers.

Future research may also explore conditions under which third parties can address inattention using peer influence and social context. For example, tax preparation firms, employers, or information return issuers (such as banks or universities) could provide reminders about filing requirements and/or tax benefits each time they have contact with those individuals. Similarly, as Cushing and Ahlawat (1996) demonstrate in the context of a firm's financial auditing decisions, future research may test if it is possible to alleviate or mitigate recency effects by providing step-by-step processing, documentation or checklists that increase tax comprehension and draw attention away from just the bottom line of tax refunds or balances owed.

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Table 1: Summary Statistics

	Experimental Population		Experimental Sample		Treatment		Control		Difference (Treatment-Control)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev	Mean	Std. Dev
Wages	6,269.11	7,630.87	5,106.72	7,199.02	5,128.75	7,251.19	5,079.28	7,133.42	49.47	24.13
Has Only Wages	0.59	0.49	0.46	0.50	0.46	0.50	0.47	0.50	-0.01	0.00
Has No Wages	0.02	0.14	0.13	0.34	0.14	0.34	0.13	0.33	0.01	0.00
Gross Income	7,350.12	7,981.84	6,953.15	7,869.89	7,013.99	7,970.53	6,877.34	7,742.03	136.64	26.38
Has QC	0.25	0.44	0.23	0.42	0.23	0.42	0.21	0.41	0.02	0.00
Wage Withholdings	242.04	552.61	216.69	537.14	218.27	542.94	214.71	529.82	3.57	1.80
Estimated EITC Benefits	576.35	990.52	533.42	933.49	548.39	948.05	514.91	914.81	33.48	3.33
Has Possible Tax Liability	0.27	0.44	0.34	0.47	0.34	0.47	0.33	0.47	0.01	0.00
Estimated Tax Liability	143.69	334.28	134.48	328.15	137.14	332.41	131.18	322.77	5.95	1.17
# of Past Tax Returns (TY2001-TY2010) = 1	0.14	0.34	0.10	0.31	0.10	0.31	0.10	0.31	0.00	0.00
# of Past Tax Returns (TY2001-TY2010) = 2	0.09	0.29	0.08	0.27	0.08	0.27	0.08	0.27	0.00	0.00
# of Past Tax Returns (TY2001-TY2010) = 3	0.08	0.26	0.07	0.26	0.07	0.26	0.08	0.26	0.00	0.00
# of Past Tax Returns (TY2001-TY2010) = 4	0.07	0.26	0.08	0.26	0.08	0.26	0.07	0.26	0.00	0.00
# of Past Tax Returns (TY2001-TY2010) = 5	0.07	0.26	0.08	0.27	0.08	0.27	0.08	0.27	0.00	0.00
# of Past Tax Returns (TY2001-TY2010) = 6	0.07	0.26	0.08	0.27	0.08	0.27	0.08	0.27	0.00	0.00
# of Past Tax Returns (TY2001-TY2010) = 7	0.07	0.26	0.08	0.27	0.08	0.27	0.08	0.27	0.00	0.00
# of Past Tax Returns (TY2001-TY2010) = 8	0.08	0.27	0.08	0.27	0.08	0.27	0.08	0.27	0.00	0.00
# of Past Tax Returns (TY2001-TY2010) = 9	0.08	0.27	0.08	0.28	0.08	0.28	0.08	0.28	0.00	0.00
# of Past Tax Returns (TY2001-TY2010) = 10	0.08	0.28	0.09	0.29	0.09	0.29	0.09	0.29	0.00	0.00
# of Past Tax Returns (TY2001-TY2010) = 11	0.14	0.35	0.16	0.37	0.16	0.37	0.16	0.37	0.00	0.00
Age	40.82	11.21	40.54	11.16	40.48	11.15	40.61	11.19	-0.13	0.04
Male	0.58	0.49	0.58	0.49	0.58	0.49	0.58	0.49	-0.01	0.00
N	6313478		360352		199910		160442		360352	

Notes: Dollar values in 2014 dollars.

Table 2: 2014 RCT Impacts on Filing

	No Controls			With Controls		
	Treatment	Control	Difference	Treatment	Control	Difference
Fraction Filing TY2010 Return	0.021	0.015	0.006	0.021	0.016	0.005
	(0.000)	(0.000)	(0.001)	(0.016)	(0.016)	(0.001)
Fraction Filing TY2011 Return	0.046	0.037	0.009	0.045	0.037	0.008
	(0.001)	(0.000)	(0.001)	(0.038)	(0.038)	(0.001)
Fraction Filing TY2012 Return	0.112	0.105	0.008	0.110	0.106	0.004
	(0.001)	(0.001)	(0.001)	(0.098)	(0.098)	(0.001)
Fraction Filing TY2013 Return	0.378	0.368	0.010	0.376	0.370	0.006
	(0.002)	(0.001)	(0.002)	(0.139)	(0.139)	(0.002)

Notes: The controls are wages, gross income, withholdings, potential EITC, number of past returns for TY2001-TY2010, age and indicators for having only wages on the unfiled return, having no wages on the unfiled return, having potential qualifying children, having potential tax liability, and gender. Columns 3 and 6 show the difference between treatment and control group. Standard errors are in parentheses.

Table 3: Treatment Variations
Fraction Filing TY2013 Tax Return

Panel A. Individual Treatments			
	Treatment	Control	Difference (Treatment - Control)
Early Postcard	0.384	0.368	0.016
	(0.003)	(0.001)	(0.004)
Early Brochure	0.373	0.368	0.004
	(0.004)	(0.001)	(0.004)
Early Postcard + Late Brochure	0.378	0.368	0.010
	(0.004)	(0.001)	(0.004)
Early Brochure + Late Postcard	0.373	0.368	0.005
	(0.004)	(0.001)	(0.004)
Early Postcard + Late Postcard	0.381	0.368	0.012
	(0.004)	(0.001)	(0.004)
Early Brochure + Late Brochure	0.380	0.368	0.011
	(0.004)	(0.001)	(0.004)

Panel B. Postcard vs. Brochure Comparison		
Postcard	Brochure	Difference (Postcard-Brochure)
0.399	0.394	0.004
(0.002)	(0.002)	(0.003)

Panel C. One vs. Two Contact Comparison		
One Contact	Two Contacts	Difference (Two Contacts - One Contacts)
0.394	0.397	0.003
(0.002)	(0.002)	(0.003)

Notes: The sample size for Panel A, Panel B and Panel C are 360,352, 98,065, and 145,725, respectively. The sample in Panel B consists of treated individuals who only received postcards or brochures (ie the individuals in the Early Postcard + Lat Brochure and Early Brochure + Late Postcard treatments are omitted). In Panel A, the filing rate fo the control group is the same across treatment groups since all of the treatment groups are compared to the control group that did not receive any experimental mailings.

Table 4: Prior-Year Return Characteristics

	TY2011 Nonfilers, N= 175780			TY2012 Nonfilers, N= 184572		
	Treatment	Control	Difference	Treatment	Control	Difference
Fraction Filing	0.070	0.058	0.012	0.159	0.148	0.011
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
Fraction Receiving Refund	0.051	0.042	0.009	0.120	0.112	0.009
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Fraction Paying Balance Due	0.016	0.013	0.002	0.033	0.031	0.002
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Fraction with 0 Balance Due	0.003	0.003	0.001	0.006	0.006	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	Characteristics on TY2011 Returns, Conditonal on Filing			Characteristics on TY2012 Returns, Conditonal on Filing		
Fraction Single	0.462	0.465	-0.003	0.382	0.376	0.006
	(0.007)	(0.008)	(0.012)	(0.005)	(0.004)	(0.007)
Fraction Married Filing Jointly	0.368	0.362	0.006	0.444	0.443	0.001
	(0.007)	(0.007)	(0.011)	(0.005)	(0.004)	(0.007)
Fraction Head of Household	0.120	0.122	-0.003	0.137	0.140	-0.003
	(0.005)	(0.005)	(0.008)	(0.003)	(0.003)	(0.005)
Fraction Other Filing Status	0.050	0.051	0.000	0.037	0.041	-0.004
	(0.003)	(0.003)	(0.005)	(0.002)	(0.002)	(0.003)
Fraction Claiming EITC	0.483	0.483	0.000	0.523	0.515	0.008
	(0.007)	(0.008)	(0.012)	(0.005)	(0.004)	(0.007)
Fraction Claiming EITC with Qualifying Children	0.161	0.152	0.009	0.228	0.227	0.001
	(0.005)	(0.005)	(0.008)	0.004	0.004	0.006
Fraction with Earned Income below EITC Thresholds	0.491	0.489	0.003	(0.534)	(0.531)	(0.004)
	(0.007)	(0.008)	(0.012)	0.005	0.004	0.007
Fraction Claiming EITC, Conditional on Earned Income below EITC Thresholds	0.821	0.812	0.009	(0.877)	(0.873)	(0.004)
	(0.009)	(0.008)	(0.013)	(0.005)	(0.004)	(0.007)
Total Income	26592.77	25919.70	673.08	33121.53	33108.34	13.19
	(409.63)	(417.09)	(638.41)	(346.70)	(319.53)	(511.22)
Wages	19786.99	19138.75	648.24	24925.37	24726.86	198.52
	(340.06)	(346.26)	(529.99)	(284.78)	(262.47)	(419.92)
Fraction with Self-Employment Income	0.270	0.276	-0.006	0.299	0.296	0.003
	(0.007)	(0.007)	(0.010)	(0.004)	(0.004)	(0.007)
Fraction with Refund	0.728	0.723	0.005	0.757	0.753	0.004
	(0.007)	(0.007)	(0.010)	(0.004)	(0.004)	(0.006)
Refund Amount, Conditional on Any Refund	1676.67	1648.11	28.56	1962.88	2039.73	-76.86
	(30.35)	(30.53)	(47.26)	(22.64)	(20.55)	(33.34)
Fraction with Balance Owed	0.224	0.228	-0.004	0.205	0.207	-0.003
	(0.006)	(0.006)	(0.010)	(0.004)	(0.004)	(0.006)
Balance Owed, Conditional on Any Balance	1467.75	1421.03	46.72	1789.74	1823.21	-33.46
	(39.67)	(41.70)	(61.93)	(29.72)	(28.91)	(44.03)
Fraction with 0 Balance Due	0.048	0.049	-0.001	0.039	0.040	-0.001
	(0.003)	(0.003)	(0.005)	(0.002)	(0.002)	(0.003)
Fraction with Preparer	0.631	0.641	-0.010	0.677	0.686	-0.009
	(0.007)	(0.007)	(0.011)	(0.005)	(0.004)	(0.007)
Fraction without Preparer	0.369	0.359	0.010	0.323	0.314	0.009
	(0.007)	(0.007)	(0.011)	(0.005)	(0.004)	(0.007)

Notes: The median value of total income on TY2011 returns and TY2011 nonfilers is roughly $15,200. The median value of wages on TY2011 returns and TY2011 nonfilers is roughly $11,200. The median value of refund amount, conditional on any refund, on TY2011 returns and TY2011 nonfilers is roughly $900. The median value of balance owed, conditional on any balance, on TY2011 returns and TY2011 nonfilers is roughly $800. The median value of total income on TY2012 returns and TY2012 nonfilers is roughly $16,700. The median value of wages on TY2012 returns and TY2012 nonfilers is roughly $11,200. The median value of refund amount, conditional on any refund, on TY2012 returns and TY2012 nonfilers is roughly $11,100. The median value of balance owed, conditional on any balance, on TY2012 returns and TY2012 nonfilers is roughly $1,400. Columns 3 and 6 show the difference between treatment and control group. Standard errors are in parentheses.

Table 5: TY2013 Return Characteristics

	Treatment	Control	Difference
Fraction Receiving Refund	0.322	0.317	0.006
	(0.001)	(0.001)	(0.002)
Fraction Paying Balance Due	0.047	0.043	0.003
	(0.001)	(0.001)	(0.001)
Fraction with 0 Balance Due	0.631	0.640	-0.009
	(0.002)	(0.001)	(0.002)
	Characteristic on TY2013 Return, conditional on filing		
Fraction Single	0.479	0.489	-0.009
	(0.002)	(0.002)	(0.004)
Fraction Married Filing Jointly	0.274	0.266	0.008
	(0.002)	(0.002)	(0.003)
Fraction Head of Household	0.205	0.204	0.001
	(0.002)	(0.002)	(0.003)
Fraction Other Filing Status	0.041	0.041	0.000
	(0.001)	(0.001)	(0.001)
Fraction Claiming EITC	0.558	0.561	-0.003
	(0.002)	(0.002)	(0.004)
Fraction Claiming EITC with Qualifying Children	0.306	0.301	0.005
	(0.002)	(0.002)	(0.003)
Fraction Claiming EITC, Conditional on Income below EITC Threshold	0.882	0.883	-0.001
	(0.002)	(0.002)	(0.003)
Total Income	24712.79	24197.70	515.09
	(120.78)	(101.26)	(176.09)
Wages	20684.02	20344.91	339.11
	(104.89)	(87.94)	(152.92)
Fraction with Self-Employment Income	0.147	0.143	0.004
	(0.002)	(0.001)	(0.003)
Fraction with Refund	0.853	0.859	-0.006
	(0.002)	(0.001)	(0.003)
Refund Amount, Conditional on Any Refund	2403.03	2374.99	28.04
	(12.03)	(9.84)	(17.45)
Fraction with Balance Owed	0.123	0.117	0.006
	(0.002)	(0.001)	(0.002)
Balance Owed, Conditional on Any Balance	938.044	928.500	9.544
	(7.074)	(6.792)	(10.609)
Fraction with 0 Balance Due	0.024	0.023	0.001
	(0.001)	(0.001)	(0.001)
Fraction with Preparer	0.547	0.547	0.000
	(0.002)	(0.002)	(0.004)
Fraction without Preparer	0.453	0.453	0.000
	(0.002)	(0.002)	(0.004)

Notes: The median value of total income on TY2013 returns is roughly $15,300. The median value of wages on TY2013 returns is roughly $13,300. The median value of refund amount, conditional on any refund, on TY2013 returns is roughly $1,300. The median value of balance owed, conditional on any balance, on TY2013 returns is roughly $1,100. Column 3 shows the difference between treatment and control group. Standard errors are in parentheses.

Table 6: 2014 RCT Impacts on Refunds Received & Balances Paid

	Treatment	Control	Difference
Total Balance Due	897.69	861.41	36.28
	(8.24)	(6.41)	(11.81)
N=360352			
Total Balance Due, Conditional on Filing Any Return	2232.63	2215.62	17.01
	(17.55)	(14.85)	(25.65)
N=142014			
Balance Owed to IRS	117.29	108.84	8.45
	(2.74)	(2.13)	(3.93)
N=360352			
Balance Owed to IRS, Conditional on Any Balance Owed to IRS	1788.99	1792.68	-3.69
	(30.97)	(29.81)	(46.48)
N=22588			
Refund to Taxpayer	1014.98	970.25	44.73
	(7.75)	(6.03)	(11.11)
N=360352			
Refund to Taxpayer, Conditional on Any Refund to Taxpayer	2877.46	2833.01	44.45
	(17.44)	(14.50)	(25.40)
N=124877			
EITC Claimed	464.26	439.44	24.81
	(4.12)	(3.21)	(5.91)
N=360352			
EITC Claimed, Conditional on Any EITC Claimed	1949.74	1907.81	41.93
	(13.16)	(10.69)	(19.09)
N=84123			

Notes: Standard errors are in parentheses. Average total tax refers to the average total tax paid on any TY2010-TY2013 returns filed. Average total refund refers to the average total refund received on any TY2010-TY2013 returns filed. Column 3 shows the difference between treatment and control group.

Table 7: Heterogeneity in Persistence from 2014 RCT

	Fraction Filing TY2014 Returns		
Samples	Treatment	Control	Difference
Full Sample	0.322	0.322	-0.001
	(0.001)	(0.001)	(0.002)
TY2013 filers	0.637	0.644	-0.007
	(0.002)	(0.002)	(0.003)
Owed Tax on TY2013 Return	0.483	0.504	-0.021
	(0.006)	(0.006)	(0.009)
Received Refund from TY2013 Return	0.659	0.663	-0.004
	(0.003)	(0.002)	(0.004)
Had Preparer for TY2013 Return	0.638	0.649	-0.011
	(0.003)	(0.003)	(0.005)
Self-Prepared TY2013 Return	0.636	0.638	-0.002
	(0.004)	(0.003)	(0.005)

Notes: Column 3 shows the difference between treatment and control group.
Standard errors are in parentheses.

Table 8: 2015 RCT Impacts on Filing TY2014 Tax Returns

	Fraction Filing TY2014 Return		
	Treatment	Control	Difference
Full 2015 RCT Sample	0.675	0.631	0.044
	(0.003)	(0.001)	(0.003)
Subsamples			
In 2014 RCT Control Group	0.678	0.635	0.043
	(0.004)	(0.002)	(0.005)
In 2014 RCT Treatment Group	0.673	0.629	0.045
	(0.004)	(0.002)	(0.004)
Balance Due on TY2013 Return	0.578	0.477	0.101
	(0.010)	(0.004)	(0.011)
0 Balance Due or Received Refund from TY2013 Return	0.685	0.654	0.031
	(0.003)	(0.002)	(0.003)
Had Preparer for TY2013 Return	0.688	0.632	0.056
	(0.004)	(0.002)	(0.004)
Self-Prepared TY2013 Return	0.661	0.630	0.030
	(0.004)	(0.002)	(0.005)

Notes: Column 3 shows the difference between treatment and control group. Standard errors are in parentheses.

Table 9: 2015 RCT, Characteristics on Filed TY2014 Returns

	Full 2015 RCT Sample			Subsample: Balance Due on 2013 Return		
Characteristic on TY2014 Return	Treatment	Control	Difference	Treatment	Control	Difference
Fraction Single	0.471	0.483	-0.013	0.398	0.405	-0.008
	(0.003)	(0.002)	(0.004)	(0.009)	(0.008)	(0.013)
Fraction Married Filing Jointly	0.246	0.003	0.009	0.454	0.438	0.015
	(0.002)	(0.002)	(0.237)	(0.009)	(0.008)	(0.013)
Fraction Head of Household	0.247	0.244	0.003	0.083	0.086	-0.003
	(0.002)	(0.002)	(0.003)	(0.005)	(0.005)	(0.008)
Fraction Other Filing Status	0.036	0.035	0.001	0.065	0.070	-0.005
	(0.001)	(0.001)	(0.001)	(0.005)	(0.004)	(0.007)
Fraction Claiming EITC	0.576	0.576	0.000	0.221	0.230	-0.009
	(0.003)	(0.002)	(0.004)	(0.008)	(0.007)	(0.011)
Fraction Claiming EITC with Qualifying Children	0.355	0.346	0.009	0.091	0.091	-0.001
	(0.003)	(0.002)	(0.004)	(0.005)	(0.005)	(0.008)
Fraction Claiming EITC, Conditional on Income below EITC Thresholds	0.918	0.918	0.000	0.708	0.702	0.007
	(0.002)	(0.002)	(0.003)	(0.016)	(0.014)	(0.023)
Total Income	23853.74	23391.52	462.22	41944.77	40967.44	977.33
	(111.67)	(89.96)	(161.14)	(485.37)	(449.85)	(719.21)
Wages	21333.75	20914.19	419.55	32396.29	31771.82	624.47
	(106.26)	(85.60)	(153.33)	(477.54)	(442.59)	(707.61)
Fraction with Self-Employment Income	0.125	0.123	0.001	0.284	0.298	-0.013
	(0.002)	(0.001)	(0.003)	(0.008)	(0.008)	(0.012)
Fraction with Refund	0.893	0.894	-0.001	0.479	0.470	0.009
	(0.002)	(0.001)	(0.002)	(0.009)	(0.008)	(0.014)
Refund Amount, Conditional on Any Refund	2657.30	2610.79	46.51	1859.64	1949.35	-89.71
	(13.71)	(10.92)	(19.74)	(54.37)	(49.84)	(80.67)
Fraction with Balance Owed	0.093	0.091	0.002	0.502	0.501	0.001
	(0.002)	(0.001)	(0.002)	(0.009)	(0.008)	(0.014)
Balance Owed, Conditional on Any Balance	498.27	502.21	-3.94	560.83	566.71	-5.88
	(3.87)	(3.44)	(5.70)	(5.03)	(4.74)	(7.48)
Fraction with 0 Balance Due	0.014	0.015	-0.001	0.020	0.029	-0.010
	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)	(0.004)
Fraction with Preparer	0.523	0.522	0.002	0.593	0.621	-0.028
	(0.003)	(0.002)	(0.004)	(0.009)	(0.008)	(0.013)
Fraction without Preparer	0.477	0.478	-0.002	0.407	0.379	0.028
	(0.003)	(0.002)	(0.004)	(0.009)	(0.008)	(0.013)

Notes: The median value of total income on TY2014 returns is roughly $16,800. The median value of wages on TY2014 returns is roughly $15,400. The median value of refund amount, conditional on any refund, on TY2014 returns is roughly $1,600. The median value of balance owed, conditional on any balance, on TY2014 returns is roughly $700. Columns 3 and 6 show the difference between treatment and control group. Standard errors are in parentheses.

Appendix Table 1: Qualifying Child Predictions
Frequencies & Column Percentages

All Filed Returns

	TY2011 Returns for TY2011 Nonfilers			TY2012 Returns for TY2012 Nonfilers		
	Not Associated with Potential QC	Associated with Potential QC	N	Not Associated with Potential QC	Associated with Potential QC	N
Filed without Qualifying Child	6,498	2,858	9,356	12,986	8,758	21,744
	94.65	67.58	84.33	91.03	63.13	77.27
Filed with a Qualifying Child	367	1,371	1,738	1,280	5,115	6,395
	5.35	32.42	15.67	8.97	36.87	22.73
N	6,865	4,229	11,094	14,266	13,873	28,139
%	100	100	100	100	100	100

Returns with Head of Household Filing Status

	TY2011 Nonfilers			TY2012 Nonfilers		
	Not Associated with Potential QC	Associated with Potential QC	N	Not Associated with Potential QC	Associated with Potential QC	N
Filed without Qualifying Child	219	251	470	357	371	728
	49.55	27.86	35	33.52	13.06	18.64
Filed with a Qualifying Child	223	650	873	708	2,469	3,177
	50.45	72.14	65	66.48	86.94	81.36
N	442	901	1,343	1,065	2,840	3,905
%	100	100	100	100	100	100

Notes: Column percentages refer to percentages computed based on the frequencies listed in a given column.

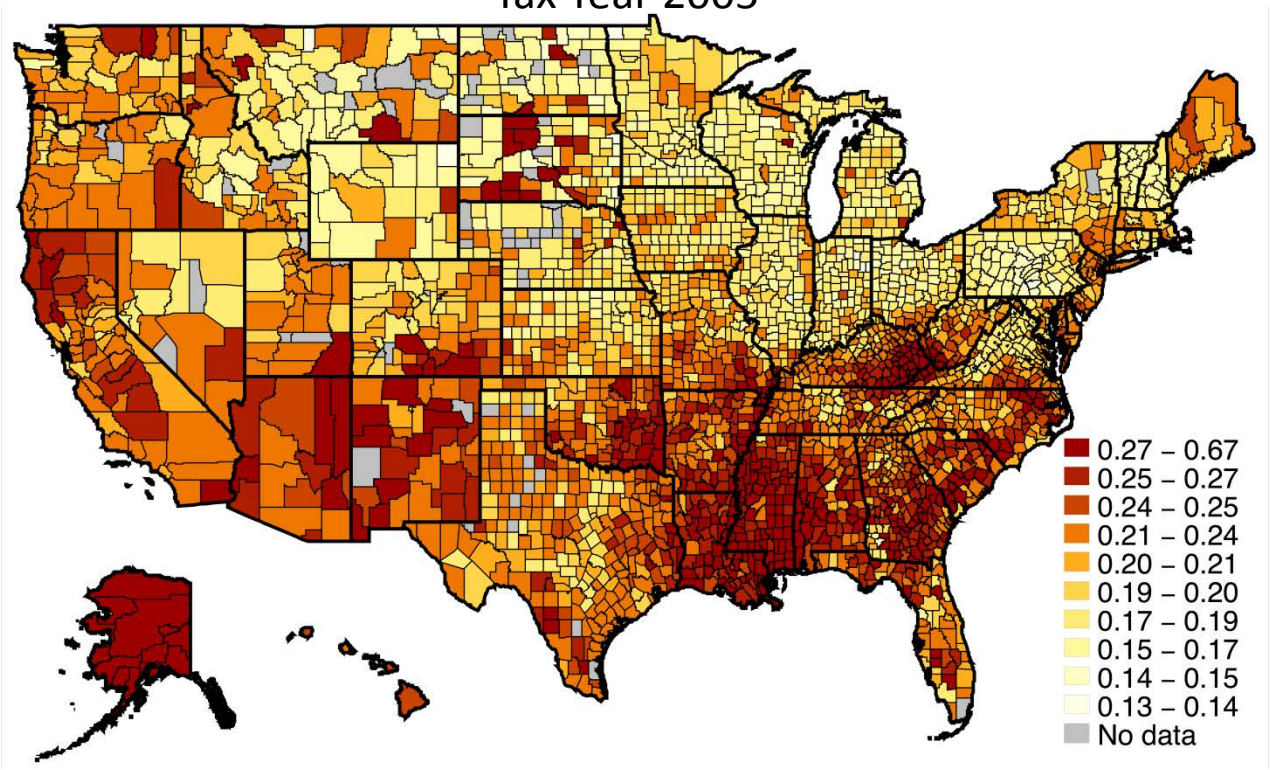
Appendix Table 2: Income Predictions

	TY2011 Returns for TY2011 Nonfilers				TY2012 Returns for TY2012 Nonfilers			
	Dependent Variable = Total Income		Dependent Variable = 1040 Wages		Dependent Variable = Total Income		Dependent Variable = 1040 Wages	
	Full Sample	Restricted Sample	Full Sample	Restricted Sample	Full Sample	Restricted Sample	Full Sample	Restricted Sample
Predicted Gross Income	0.975	0.788			1.311	0.898		
	[0.0261]	[0.0119]			[0.0181]	[0.00904]		
Predicted Wages			0.632	0.660			1.350	0.944
			[0.0247]	[0.0119]			[0.0143]	[0.00478]
Constant	13731.3	2566.4	15122.0	5025.0	17019.0	2020.3	9700.7	833.9
	[416.7]	[167.6]	[271.8]	[128.2]	[296.3]	[125.9]	[220.9]	[61.55]
Observations	11094	5265	11094	5265	28139	11423	28139	11423
R-Squared	0.112	0.455	0.056	0.370	0.157	0.464	0.242	0.774

Notes: Standard errors are in brackets. Each column corresponds to a separate regression with the sample and dependent variales listed in the column heading and the independent variables listed in the row titles. The restricted sample includes returns with single or head-of-household filing status and no self-employment income.

Figure 1. Spatial Distributions of Nonfilers
Fraction of Nonfilers by County

Tax Year 2005



Tax Year 2006

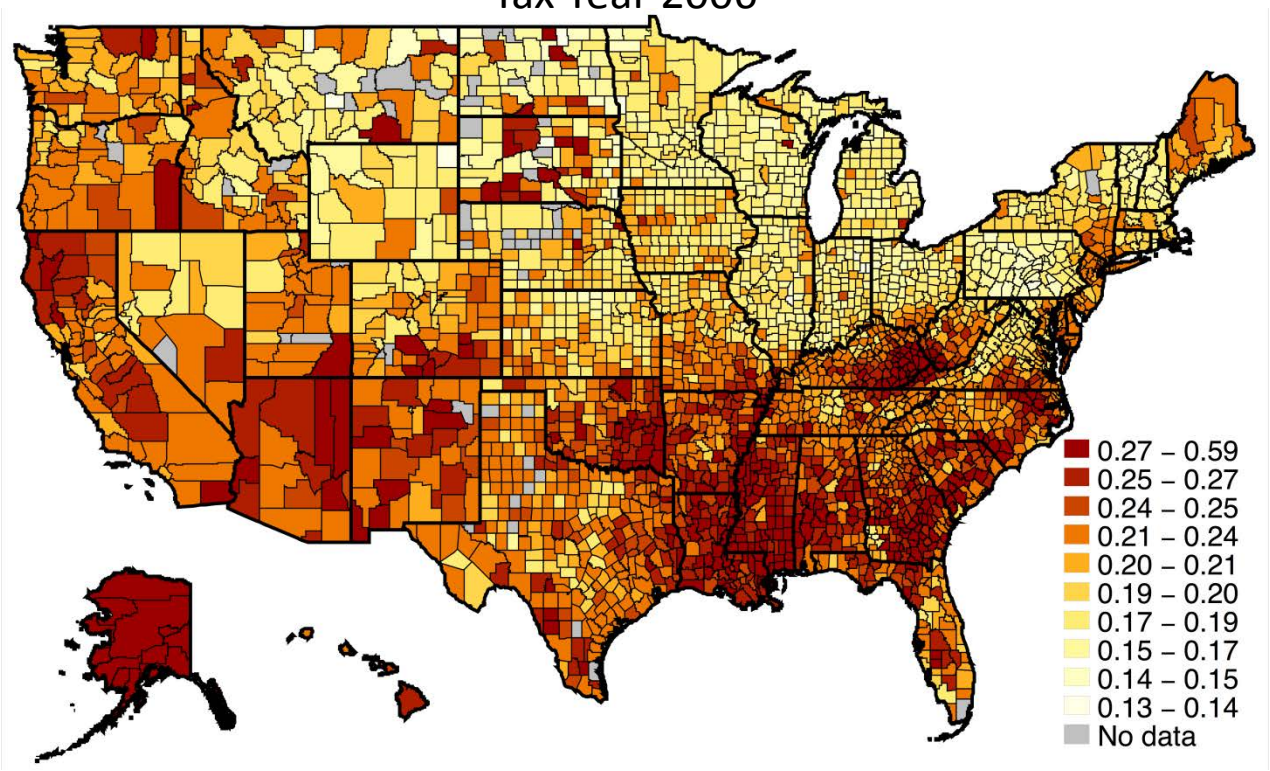
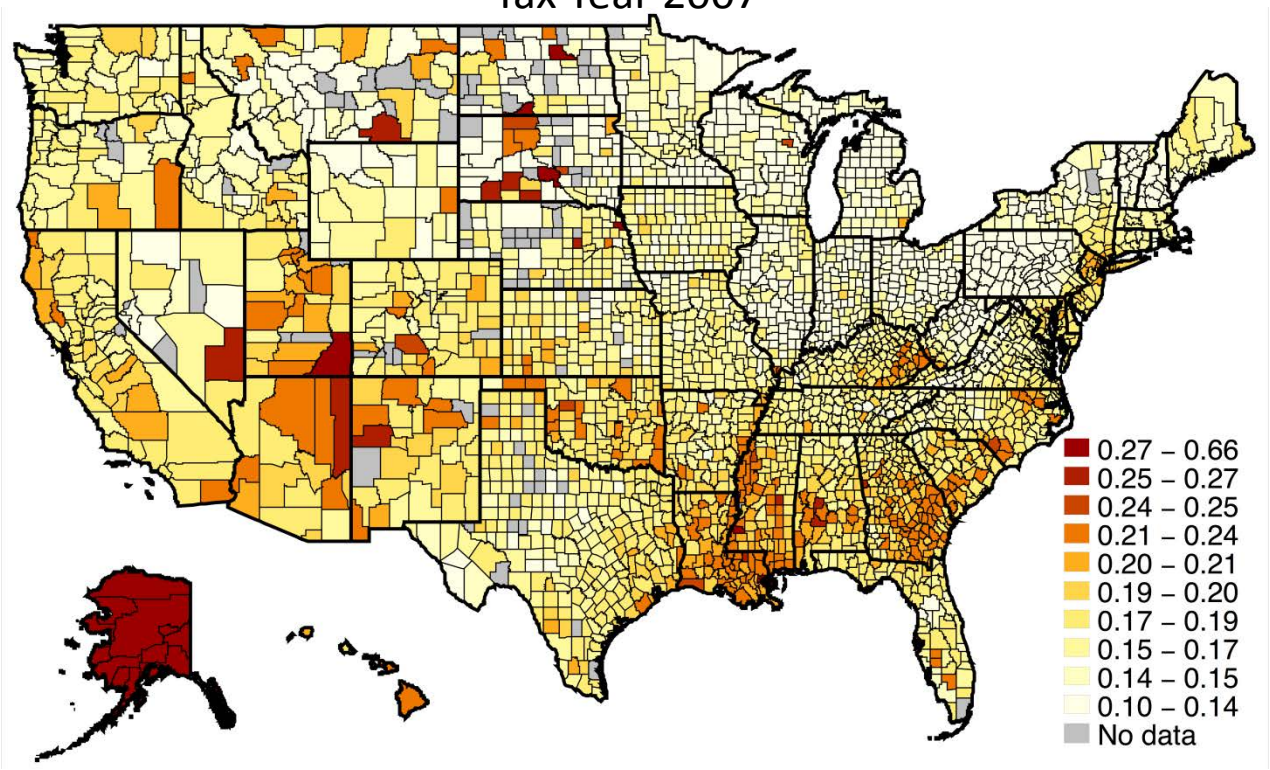


Figure 1 presents the spatial distribution of nonfilers. The maps plot the fraction of nonfilers in each county from TY2005 through TY2013. The fraction of nonfilers in a given county is calculated by computing the total number of nonfilers divided by the sum of nonfilers and filed tax returns.

Figure 1. Spatial Distributions of Nonfilers
Fraction of Nonfilers by County

Tax Year 2007



Tax Year 2008

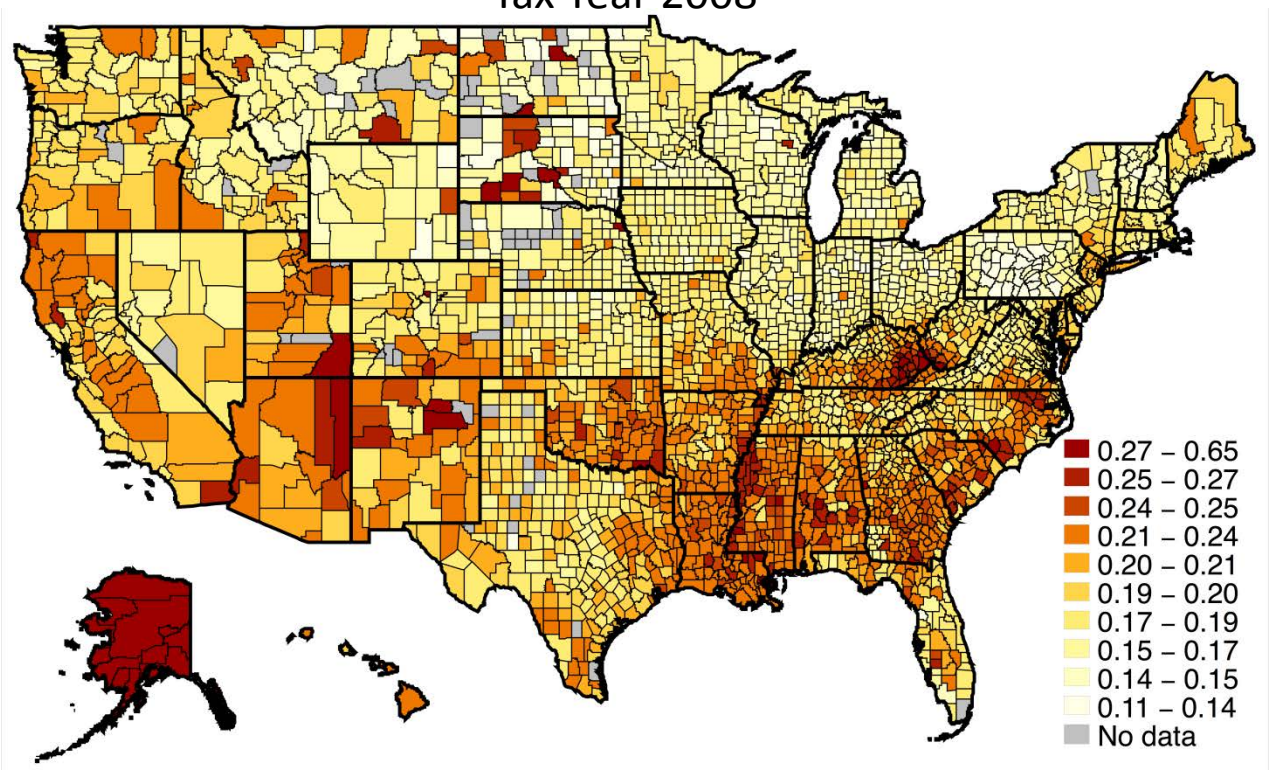
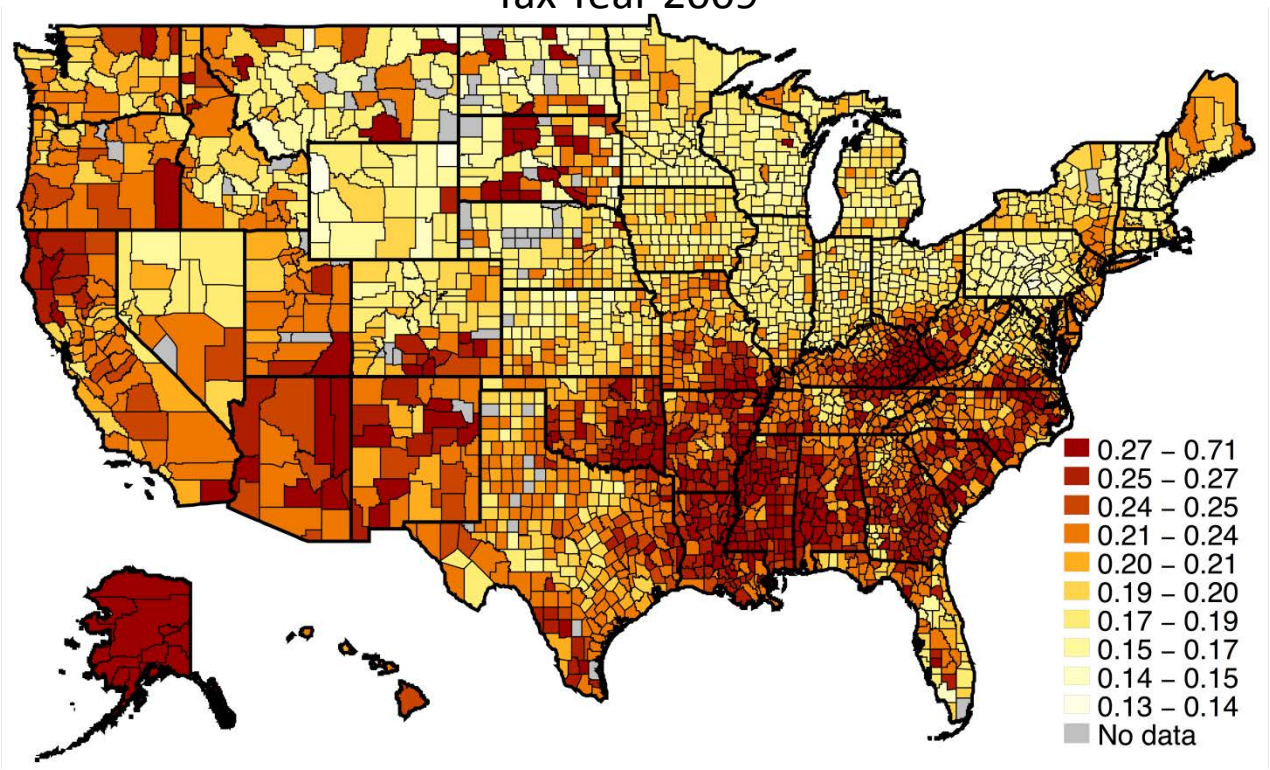


Figure 1 presents the spatial distribution of nonfilers. The maps plot the fraction of nonfilers in each county from TY2005 through TY2013. The fraction of nonfilers in a given county is calculated by computing the total number of nonfilers divided by the sum of nonfilers and filed tax returns.

Figure 1. Spatial Distributions of Nonfilers
Fraction of Nonfilers by County

Tax Year 2009



Tax Year 2010

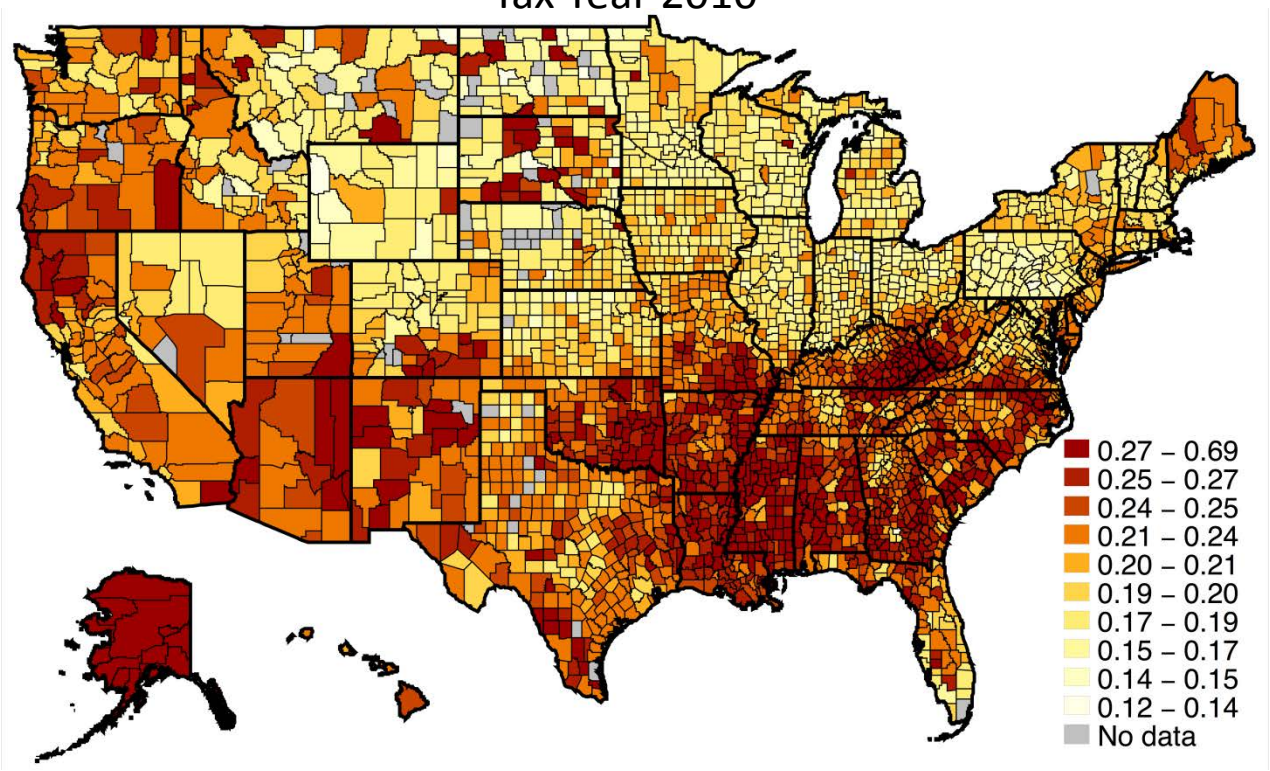
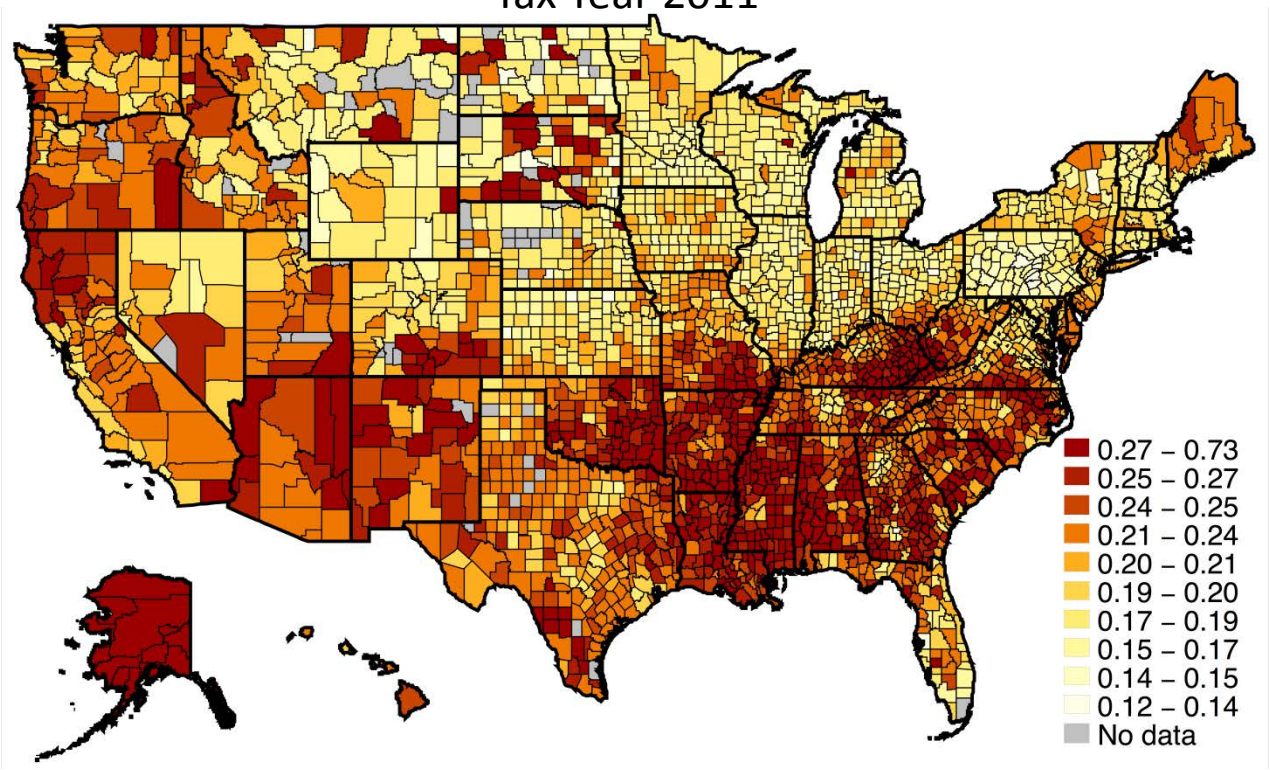


Figure 1 presents the spatial distribution of nonfilers. The maps plot the fraction of nonfilers in each county from TY2005 through TY2013. The fraction of nonfilers in a given county is calculated by computing the total number of nonfilers divided by the sum of nonfilers and filed tax returns.

Figure 1. Spatial Distributions of Nonfilers
Fraction of Nonfilers by County

Tax Year 2011



Tax Year 2012

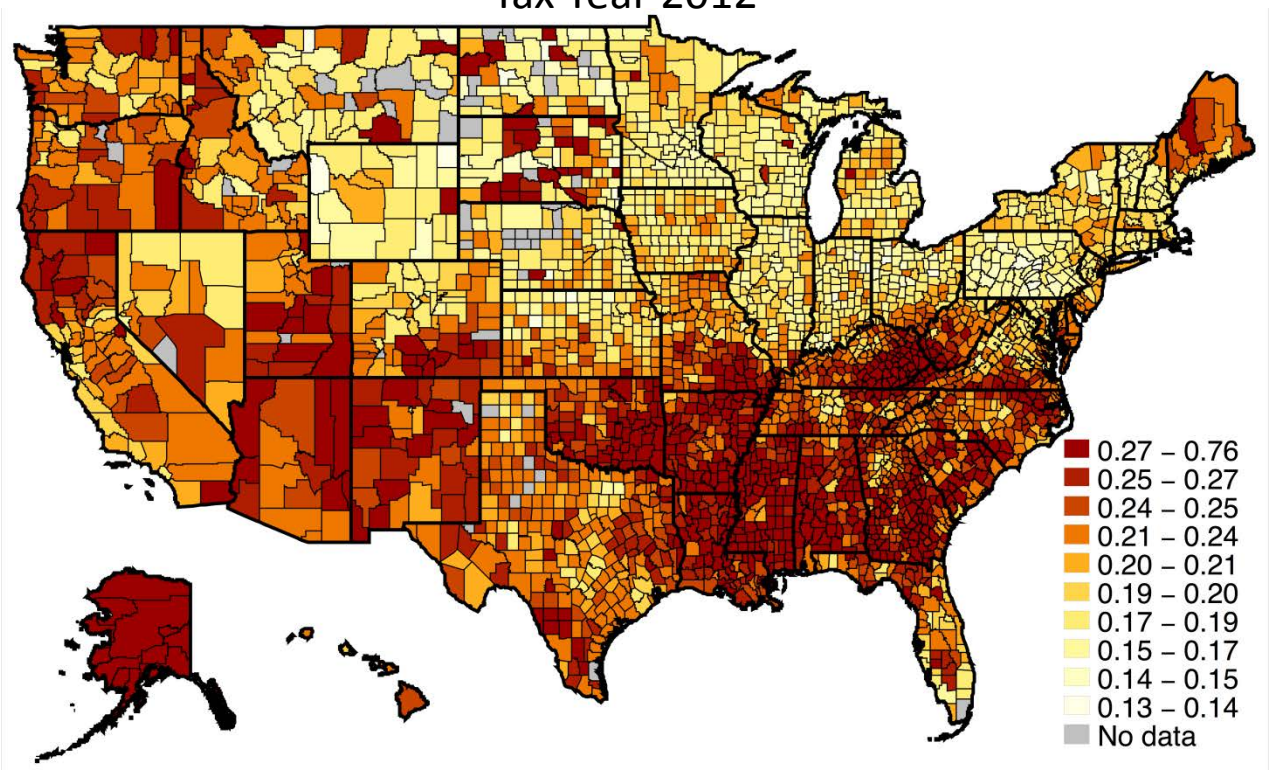


Figure 1 presents the spatial distribution of nonfilers. The maps plot the fraction of nonfilers in each county from TY2005 through TY2013. The fraction of nonfilers in a given county is calculated by computing the total number of nonfilers divided by the sum of nonfilers and filed tax returns.

Figure 1. Spatial Distributions of Nonfilers
Fraction of Nonfilers by County

Tax Year 2013

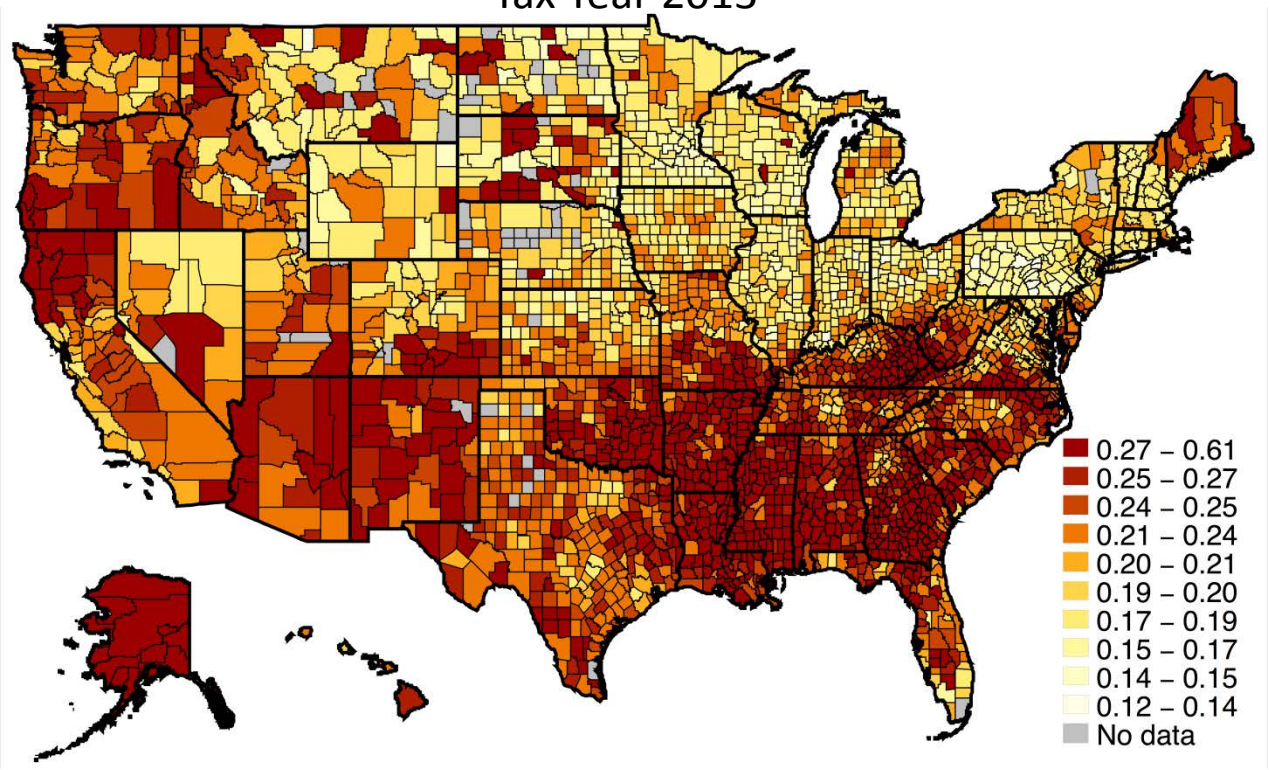
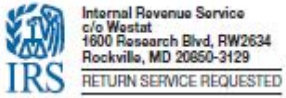


Figure 1 presents the spatial distribution of nonfilers. The maps plot the fraction of nonfilers in each county from TY2005 through TY2013. The fraction of nonfilers in a given county is calculated by computing the total number of nonfilers divided by the sum of nonfilers and filed tax returns.

Figure 2. 2014 RCT Mailing Documents

A. Postcard Example



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RETURN SERVICE REQUESTED

Publication 5143(EN-SP) (3-2014) Catalog Number 68362P Department of the Treasury **Internal Revenue Service** www.irs.gov

It is not too late to file your tax returns for 2010, 2011, 2012, and 2013.

You may be eligible for important tax benefits.

You may be eligible for the **Earned Income Tax Credit**. For 2013, this credit provides **up to $6,044** to households with earnings less than $51,567. For more information about the EITC and eligibility rules, see **www.irs.gov/eitc**.

Many individuals are unaware of valuable tax benefits and miss out because they do not file tax returns. You must file a tax return to claim these tax benefits.

If you have not done so already, please file your tax returns for 2010, 2011, 2012, and 2013.

Additional Information

You can file late tax returns and claim tax refunds up to 3 calendar years after the April filing deadline. For example, you can claim a tax refund for 2010, if you file the tax returns by April 18, 2014.

Todavía no es muy tarde para presentar las declaraciones del 2010, 2011, 2012 y 2013.

Tal vez usted tenga derecho a beneficios tributarios importantes.

Tal vez usted tenga derecho al **Crédito por Ingreso del Trabajo**. En el 2013, este crédito les brinda **hasta $6,044** a unidades familiares que ganaron menos de $51,567. Si desea más información acerca del Crédito y las reglas de elegibilidad, visite **www.irs.gov/eitc**.

Muchas personas no saben de éstos beneficios tributarios valiosos, y los pierden ya que no presentan declaraciones de impuestos. Tiene que presentar una declaración de impuestos para reclamar estos beneficios.

Si todavía no lo ha hecho, por favor presente las declaraciones para los años 2010, 2011, 2012 y 2013.

Información Adicional

Puede presentar declaraciones de impuestos que están tardías y reclamar reembolsos del impuesto hasta 3 años naturales a partir de la fecha límite, en abril, para presentarla. Por ejemplo, puede reclamar un reembolso de los impuestos del 2010, si presenta la declaración de impuestos para el 18 de abril del 2014.

Figure 2. 2014 RCT Mailing Documents

B. Brochure Example, front

IRS

Do You Know About the

Earned Income Tax Credit?

The Earned Income Tax Credit (EITC) is a refundable credit that can help you pay bills, fix up your home, and even save for a rainy day. If you qualify, you can claim the credit even if you aren't required to file a tax return.

WHO QUALIFIES TO CLAIM EITC?

If you have earned income (wages or self-employment), you may qualify to claim EITC if you

- Are a U.S. citizen or resident alien all year
- Have a Social Security Number that is valid for employment
- Are NOT a dependent of another taxpayer
- Do NOT file as married filing separate
- Do NOT exceed certain income limits
- Meet certain other requirements

For more information about who qualifies for EITC, go to **www.irs.gov/eitc**.

Tax Year	EITC Income Limits		Maximum Credit
	Single, Head of Household	Married Filing Jointly	
2013	$46,227	$51,567	$6,044
2012	$45,060	$50,270	$5,891
2011	$43,998	$49,078	$5,751
2010	$43,352	$48,362	$5,666

HOW DO I FILE?

Go to **www.irs.gov/eitc** for free information and to check out the interactive EITC Assistant to see if you qualify and estimate the amount of your EITC.

Visit a Volunteer Income Tax Assistance (VITA) site for free tax help and preparation. Or, call 1-800-906-9887 to find a site.

Use Free File at **www.irs.gov** for free online filing through commercially available tax preparation software.

Find a qualified tax preparer; find tips for choosing on irs.gov, keyword: Choose Preparer.

Call 1-800-829-4059 if you have access to TTY/TDD equipment for the hearing impaired.

WHAT IF I HAVEN'T FILED A PRIOR YEAR RETURN?

If you are due an income tax refund, you can file a return to claim it within 3 years of the return due date.

Return Year	File By
2012	April 15, 2016
2011	April 17, 2015
2010	April 18, 2014

Find tax forms and instructions at **www.irs.gov/Forms-&-Pubs**. You may want to seek help from a tax professional. And you may qualify for other tax benefits.

WHERE CAN I GET A COPY OF MY W-2?

Request a copy of your W-2 or other tax document from the company that sent it to you or use Form 4506-T, *Request for Transcript of Tax Return*, to request a copy of your tax documents from the IRS. Find Form 4506-T at **www.irs.gov/pub/irs-pdf/f4506t.pdf**

eitc earned income tax credit

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Figure 3. 2014 RCT Mailing Documents

C. Brochure Example, back

IRS

¿Sabía Usted Que Existe El

Credito Por Ingreso Del Trabajo?

El Crédito por Ingreso del Trabajo (*EITC*, por sus siglas en inglés) es un crédito reembolsable, que podría usar para pagar sus cuentas, arreglar su casa, y aún ahorrar para tiempos malos. Si reúne los requisitos, puede reclamar el crédito aunque no tenga la obligación de presentar una declaración de impuestos.

¿QUIÉN REÚNE LOS REQUISITOS PARA RECLAMAR EL *EITC*?

Si tiene ingresos del trabajo (sueldos o ingresos por el trabajo por cuenta propia), usted puede reclamar el *EITC* si

- Es ciudadano estadounidense o extranjero residente durante el año entero
- Tiene un Número de Seguro Social válido para trabajar
- NO es un dependiente de otro contribuyente
- NO presenta su declaración con el estado civil de casado que presenta por separado
- Sus ingresos NO superan ciertos límites
- Cumple los demás requisitos

Si desea más información acerca de quién califica para el *EITC*, visite **www.irs.gov/eitc**.

Año Tributario	Límites de Ingresos para el *EITC*		Crédito Máximo
	Soltero, Cabeza de Familia	Casado que presenta una declaración conjunta	
2013	$46,227	$51,567	$6,044
2012	$45,060	$50,270	$5,891
2011	$43,998	$49,078	$5,751
2010	$43,352	$48,362	$5,666

¿CÓMO LO RECLAMO?

Visite **www.irs.gov/eitc** para acceder a información gratuita y conocer el Asistente Interactivo del *EITC* para saber si reúne los requisitos, y ver un estimado del *EITC* que le corresponderá.

Visite un centro de Ayuda Voluntaria al Contribuyente Individual (*VITA*, por sus siglas en inglés) para recibir gratuitamente ayuda y servicios de preparación de impuestos. Puede llamar al 1-800-906-9887 para localizar a uno de estos centros.

Use el servicio de *Free File* en **www.irs.gov/espanol**, el cual le permite presentar su declaración gratis, mediante los software de preparación de impuestos actualmente disponibles en el mercado.

Busque un preparador de impuestos capacitado. Si desea recomendaciones para cómo elegir un preparador, vaya a irs.gov/espanol y entre las palabras claves *Elegir Preparador* en la barra de búsqueda.

Llame al 1-800-829-4059 si tiene acceso al equipo TTY/TDD para personas con incapacidades auditivas.

¿QUÉ PASA SI NO HE PRESENTADO UNA DECLARACIÓN PARA UN AÑO ANTERIOR?

Si se le debe un reembolso del impuesto sobre los ingresos, puede presentar una declaración para reclamarlo dentro de los 3 años a partir de la fecha de vencimiento de la declaración.

Año de la Declaración	Presente para el
2012	April 15, 2016
2011	April 17, 2015
2010	April 18, 2014

Puede encontrar formularios de impuestos y sus instrucciones en **www.irs.gov/Forms-&-Pubs**. Tal vez quiera pedir la ayuda de un profesional de impuestos, y puede que reúna los requisitos para otros beneficios tributarios.

¿DÓNDE CONSIGO UNA COPIA DE MI FORMULARIO W-2?

Usted puede pedir una copia de su formulario W-2 u otro documento de impuestos a la compañía que lo emitió, o puede utilizar el Formulario 4506-T, *Request for Transcript of Tax Return* (Formulario para la solicitud de un trasunto de la declaración de impuestos personales), en inglés, para pedir del IRS una copia de sus documentos tributarios. El Formulario 4506-T está disponible en **www.irs.gov/pub/irs-pdf/f4506t.pdf**

eitc earned income tax credit

Publication 5142 (EN-SP) (2-2014) Catalog Number 66351E Department of the Treasury **Internal Revenue Service** www.irs.gov

Figure 3. 2015 RCT Mailing Document

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Publication 5143-A (EN-SP) (1-2015) Catalog Number 67525U Department of the Treasury **Internal Revenue Service** www.irs.gov

If you have not done so already, you can file your tax returns to avoid losing valuable tax benefits.

It is not too late to file your tax returns for 2011, 2012, 2013 and 2014.

You may be eligible for important tax benefits.

You may be eligible for the **Earned Income Tax Credit**. For 2014, this credit provides **up to $6,143** for households with earnings less than $52,427. For more information about the EITC and eligibility rules, see **www.irs.gov/eitc**.

Many individuals are unaware of valuable tax benefits and miss out because they do not file tax returns. You must file a tax return to claim these tax benefits. In addition, you may be due a refund of some or all of the income tax withheld from your wages or other income.

If you have not done so already, please file your tax returns for 2011, 2012, 2013 and 2014.

Additional Information

You can file late tax returns and claim tax refunds up to 3 calendar years after the April filing deadline. For example, you can claim a tax refund for 2011, if you file the tax return by April 15, 2015.

Si todavía no lo ha hecho, usted puede presentar sus declaraciones de impuestos para evitar perder valiosos beneficios tributarios.

No es demasiado tarde para presentar sus declaraciones de impuestos para los años 2011, 2012, 2013 y 2014.

Usted puede ser elegible para importantes beneficios tributarios.

Usted puede ser elegible para el **Crédito Tributario por Ingreso del Trabajo** (*EITC*, por sus siglas en inglés). Para el año 2014, este crédito ofrece **hasta $6,143** a las familias con ingresos inferiores a $52,427. Para obtener más información sobre el *EITC* y las reglas de elegibilidad visite ***www.irs.gov/espanol***.

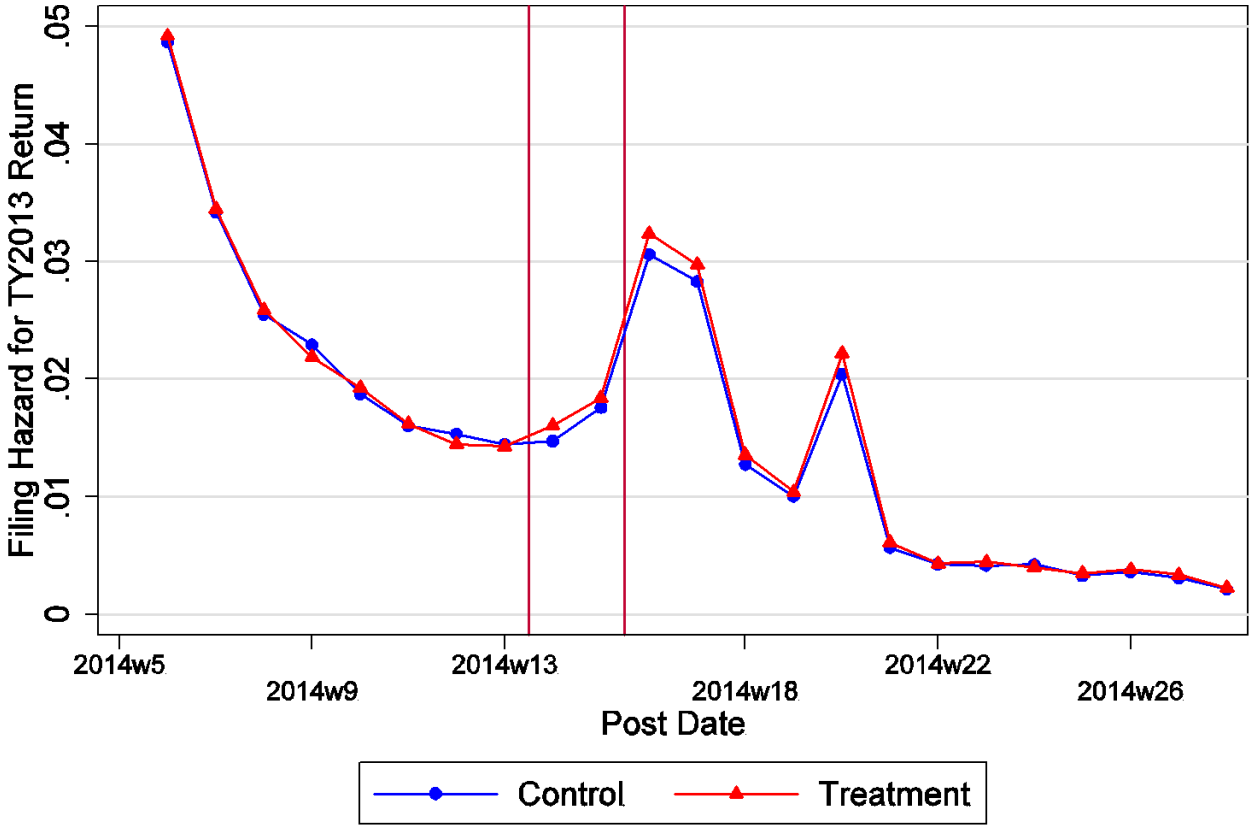
Muchas personas no están conscientes de los valiosos beneficios tributarios y los pierden por no presentar las declaraciones de impuestos. Usted tiene que presentar una declaración de impuestos para reclamar estos beneficios tributarios. Además, usted puede tener derecho al reembolso de una parte o la totalidad de los impuestos retenidos sobre sus sueldos u otros ingresos.

Si todavía no lo ha hecho, por favor presente sus declaraciones de impuestos para los años 2011, 2012, 2013 y 2014.

Información Adicional

Usted puede presentar declaraciones de impuestos tardías y reclamar los reembolsos de impuestos de hasta 3 años calendarios después de la fecha límite de presentación de abril. Por ejemplo, usted puede reclamar un reembolso de impuestos para el año 2011, si presenta la declaración de impuestos para el 15 de abril de 2015.

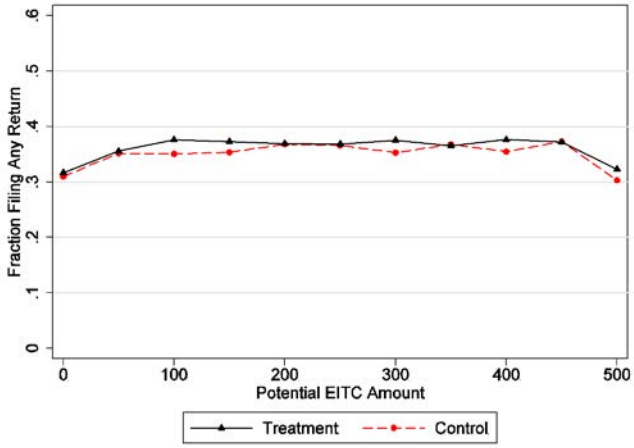
Figure 4. Filing TY2013 Returns by Date



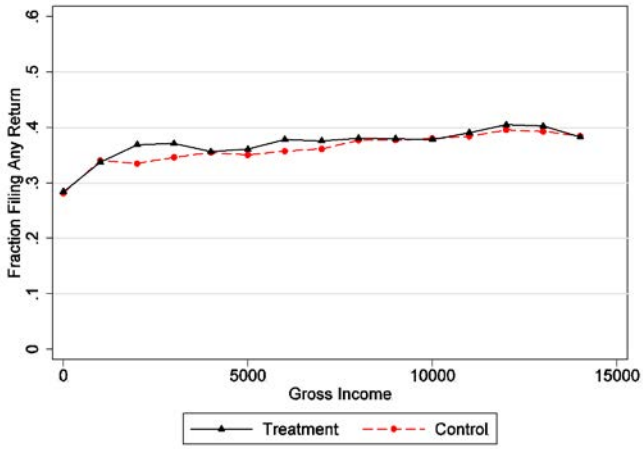
Notes: The filing hazard at a given point in time is defined as the probability of filing a tax at that time conditional on not having filed earlier. For each group, the hazard rate at a given time is computed as the number of individuals who file a return on that date divided by the number individuals in the group who have not filed a return prior to that date. The Post Date refers to the calendar week that the tax return is posted to the IRS database.

Figure 5. Treatment Effect Heterogeneity

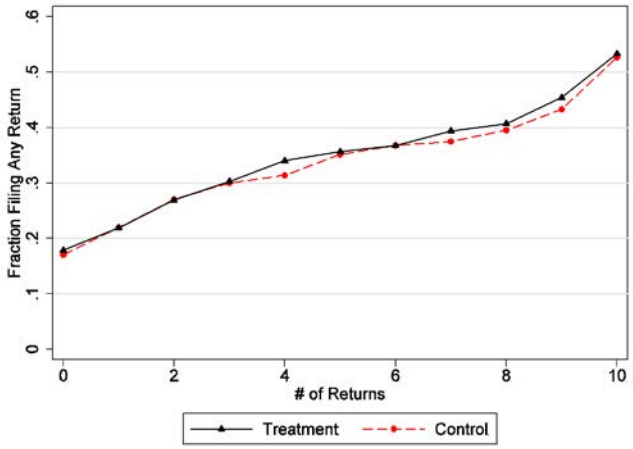
Panel A. Filing Rates by Potential EITC Benefits



Panel B. Filing Rates by Gross Income



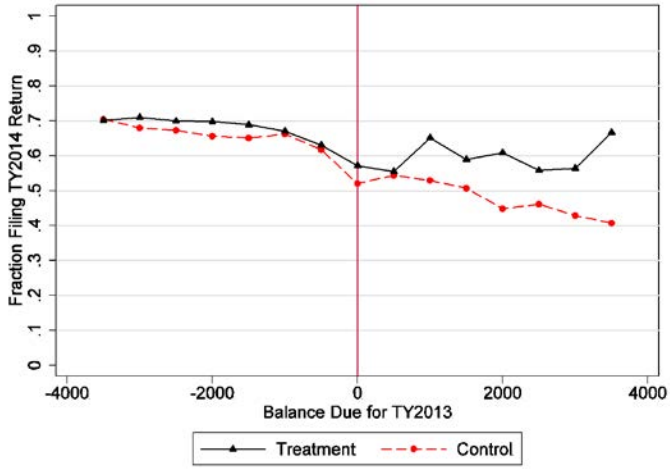
Panel C. Filing Rates by # Returns (TY2001-TY2010)



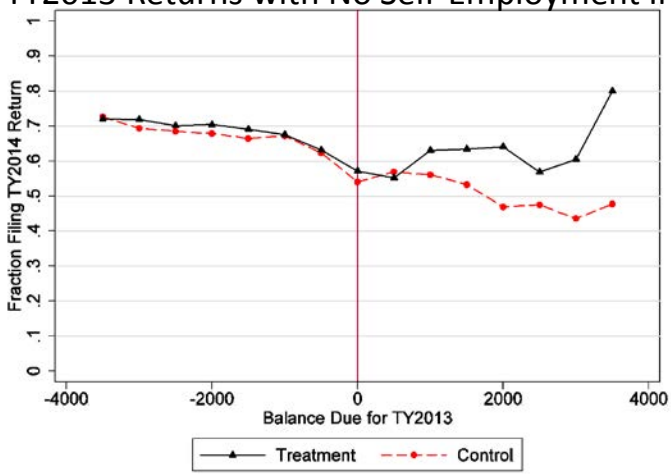
Notes: Filing rates are based on the likelihood of filing a TY2010, TY2011, TY2012 or TY2013. The sample is restricted to nonfilers who were not associated with a possible qualifying child.

Figure 6. Filing TY2014 Returns by TY2013 Balance Due

A. Full 2015 RCT Sample (All TY2013 Returns)



B. TY2013 Returns with No Self-Employment Income



C. TY2013 Returns with No Self-Employment Income, Covariate Adjusted

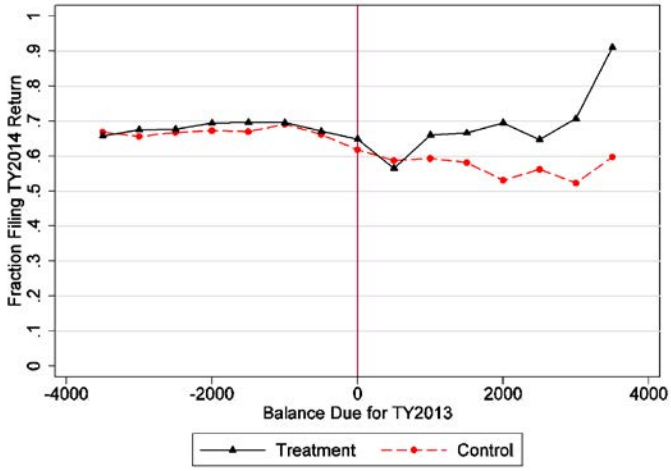


Figure 6 plots the likelihood of filing a TY2014 tax return after having a balance due on the TY2013 tax return for treatment and control group. Panel A plots the fraction filing returns for the full 2015 RCT sample. Panel B plots the filing patterns for individuals who did not have any self-employment income on the TY2013 tax returns. Panel C plots covariate-adjusted filing patterns for people with no self-employment income on their TY2013
tax returns.

Figure 7. Likelihood of Refund on TY2014 Return, Conditional on Filing TY2014 Return

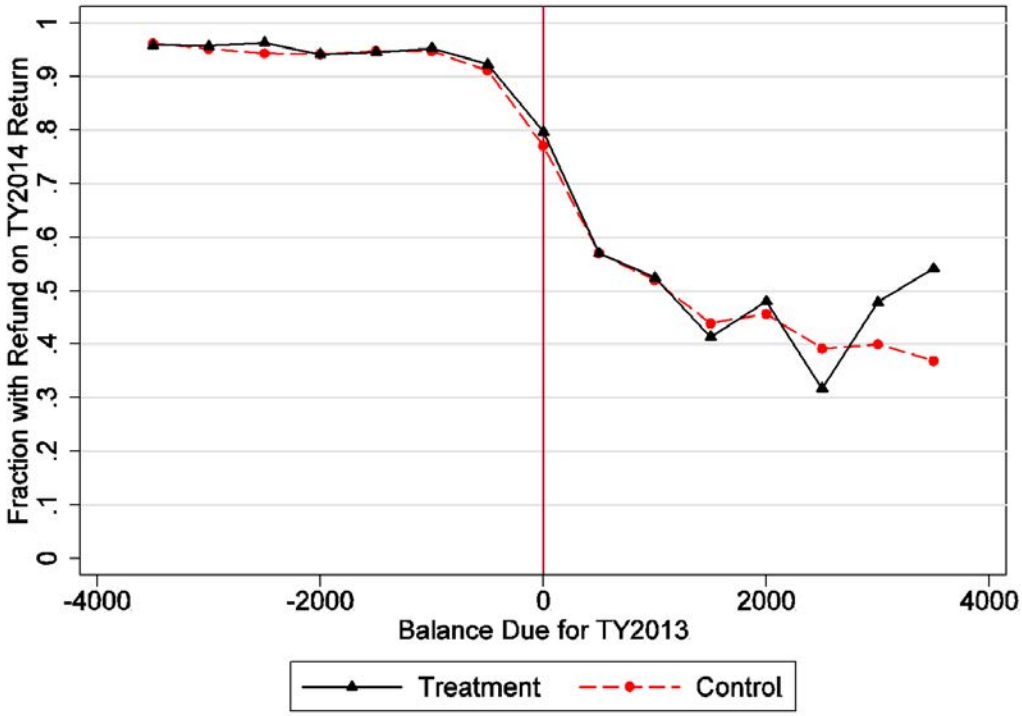
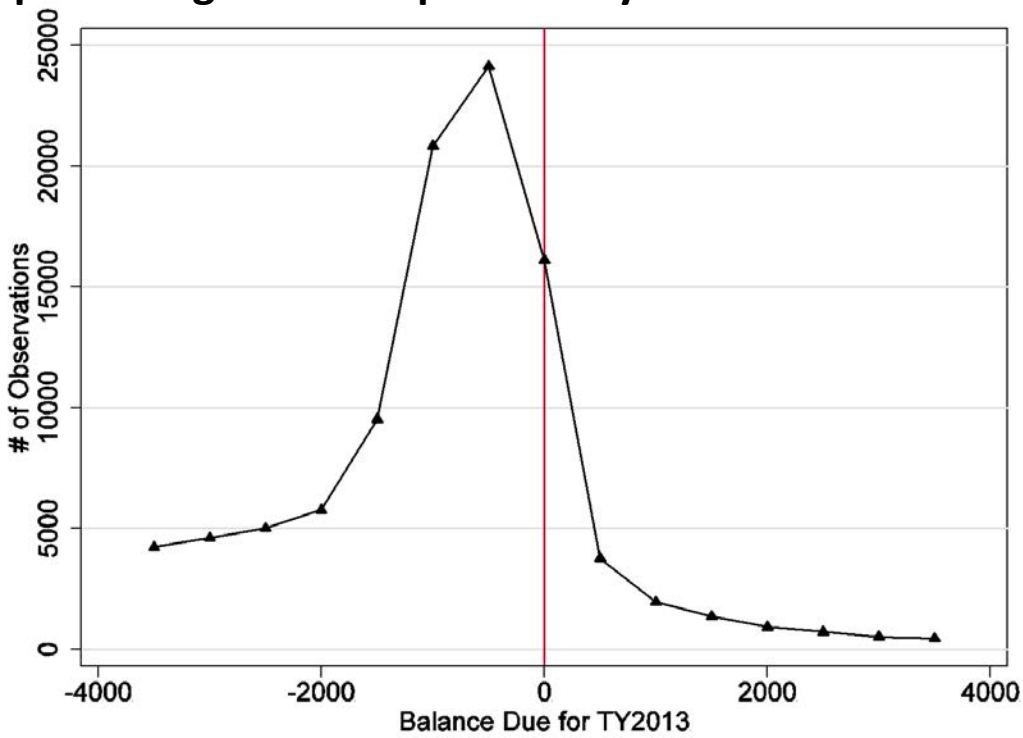


Figure 7 plots the fraction of individuals getting a tax refund on their TY2014 tax return conditional on filing these returns against the balance due on their TY2013 tax returns for treatment and control group.

Appendix Figure 1. Frequencies by TY2013 Balance Due



Appendix Figure 1 plots the number of nonfilers against the balance due on their TY2013 tax returns.