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Essays on the Earned Income Tax Credit

by

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Committee in charge:

Professor Jesse Rothstein, Chair
Professor David Card
Professor Avi Feller
Professor Hilary Hoynes

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Abstract

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This dissertation investigates three questions related to the Earned Income Tax Credit, the largest cash-based, means-tested program in the United States. I study whether the EITC changes how much (as opposed to whether) workers choose to work, whether increasing awareness of the program can improve participation, and to what extent eligible households take up the California supplement to the federal credit.

In Chapter 1, I propose a new strategy for identifying workers' intensive-margin labor supply elasticity using within-year variation in anticipated year-end tax rates. I modify the standard non-linear budget set approach to include uncertainty about future employment. With uncertainty, households must forecast their annual income in order to anticipate the average and marginal tax rates that apply to their earnings. Using survey and administrative data, I find that low-income households' labor supply responds more to expected tax rates at the end of the year, when certainty about annual income is greatest. I use the excess sensitivity to tax incentives near the end of the year, relative to other periods, to estimate an intensive margin labor supply elasticity between .08 and .18. This response is identified largely from non-linearity in the EITC schedule and implies a larger intensive margin response to this program than previous estimates.

In Chapter 2, my co-authors and I summarize six pre-registered, large-scale field experiments involving over one million subjects testing whether "nudges" could increase take-up of the Earned Income Tax Credit (EITC). Despite varying the content, design, messenger, and mode of our messages, we find no evidence that they affected households' likelihood of filing a tax return or claiming the credit. We conclude that even the most behaviorally informed low-touch outreach efforts cannot overcome the barriers faced by low-income households who do not file returns.

In Chapter 3, my co-authors and I use administrative data from California on the population of Supplemental Nutrition Assistance Program (SNAP) recipients, linked to state tax records, to estimate the number of households who are eligible for California's supplement to the federal EITC but do not claim it. We find that nearly half a million households who receive SNAP benefits and

who were eligible for the state EITC in 2017 did not receive the credit. This includes approximately 42,000 eligible households who claimed the federal EITC but not the state credit; 110,000 eligible households who filed a state tax return but did not claim the state credit; and 290,000 eligible households who did not file a state tax return. The corresponding take-up rate for the CalEITC among eligible SNAP-enrolled households was 53%. Altogether, these households left on the table a total of $75 million in state EITC funds. If received, these credits would have increased incomes among these households by 2.6% and increased total state EITC outlays by 38.8%.

To Jacqueline and Abigail

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Chapter 1

Beating the Clock: Using Year-end Changes to Identify Intensive Margin Labor Supply Responses to Taxation

1.1 Introduction

Identifying the effect of taxes on the labor supply of individuals who would work regardless has been a longstanding empirical challenge. Despite the incredible policy relevance of this parameter (see, e.g., Saez, 2002), the micro literature lacks reliable estimates of the intensive margin response to tax incentives. Identification is challenging, in part, because it is difficult to isolate exogenous variation in marginal tax rates within non-linear tax schedules, and in part because extensive margin responses to tax policy reforms can introduce selection bias. Further, even credible average estimates mask important heterogeneous responses. We would expect larger responses to short-term variation in tax rates that can be avoided via intertemporal substitution than to longer-term variation, and it is not clear which are identified by many existing studies.

Concretely, understanding the impact of the Earned Income Tax Credit (EITC) on labor supply is an important topic for policy purposes. The EITC is the largest means-tested cash assistance program in the U.S. and largely shapes the tax policy facing lower-income workers. Many studies demonstrate the program's substantial effects on workers' extensive margin labor supply decision, but few find any effect on the intensive margin.[1]

A possible explanation for the lack of a response, and an important unresolved issue in the labor supply literature, relates to workers' information structure. Labor supply models of non-

[1]One notable exception to the consensus finding regarding the extensive margin response is Kleven (2019), who argues, in part, that welfare reform waivers were responsible for labor supply increases observed in the early 1990s, as opposed to the EITC expansion. If true, these waivers would also confound studies of intensive margin responses to reforms of the EITC, even if those reforms did not have an extensive margin effect. See Schanzenbach and Strain (2020) for a response to Kleven. The common finding regarding minimal intensive margin responses is also not wholly convincing if large extensive margin responses to the EITC changes the composition of the workforce (Nichols and Rothstein, 2016).

linear budget sets (**?**) estimate individuals' responses to their estimated tax rates, but it is not clear that workers are aware of the rates that they face. This is a particular concern with non-linear, annual tax schedules: The marginal tax rate on an individual's labor supply one day depends on their total earnings throughout the year, and for many periods in a year, those earnings have not yet been realized. It seems likely that many low-wage workers, whose earnings are disproportionately volatile and whose income tax schedule is highly non-linear, have trouble forecasting their annual income and thus their average and marginal tax rates.

In this paper, I propose a new strategy for identifying short-term labor supply responses to taxation that exploits this uncertainty. At the beginning of the year, workers' forecasts of their annual earnings and their marginal tax rates are imprecise, but they gain more information with each work day, so they can make more accurate estimates of the tax rate that will ultimately apply to their earnings. As a result, we might expect workers' labor supply to depend more strongly on their expected annual tax rates in the fourth quarter of the tax year, when their employment history is nearly realized, than in the first.

I use this idea to obtain a new measure of the intensive margin Frisch elasticity. Leveraging the difference in awareness of true tax incentives between the beginning and end of the tax year, I distinguish workers' independent response to tax policy from standard serial correlation in earnings. I interpret the excess sensitivity of earnings to likely tax incentives in the fourth quarter, relative to other calendar quarters, to reflect workers' intentional reallocation between labor and leisure in response to those tax incentives.

I evaluate whether workers make these year-end adjustments, and I measure the size of this response, using two data sources: the Survey of Income and Program Participation (SIPP) and administrative earnings records for lower-income Californians enrolled in the Supplemental Nutrition Assistance Program (SNAP). Both datasets contain within-year earnings and information needed to identify households' tax rates. I construct likely tax units from SIPP households and SNAP cases, measure each workers' total earnings through multiple within and cross-year periods, and identify predicted marginal and average tax rates for each tax unit in each of those periods using National Bureau of Economic Research's (NBER) TAXSIM program. To measure households' labor supply response to their expected tax rates, I relate earnings in each period to the average tax rate that would apply to that period's predicted earnings. I distinguish apparent year-end responses to tax policy from other serial earnings patterns by separately identifying this response in each calendar quarter, and evaluating whether the relationship appears strongest in the final quarter of the tax year. To account for any lingering omitted variable bias concerns, I identify this response within the same workers and households over multiple consecutive years.

I conclude that household labor supply is indeed more sensitive to expected tax incentives at the end of the tax year. For a 10 percentage point increase in their predicted net of tax wage rate (i.e., a 10 percentage point decrease in their predicted tax rate), households increase earnings in the fourth quarter by 1 to 2 percent on average. This response is largely driven by households facing the most negative tax rate at year's end. When the same household expects to have especially modest annual earnings, such that they would expect to be within the phase-in segment of the EITC schedule, they tend to increase their earnings in the following quarter. This response is most pronounced at the end of the tax year, suggesting that this adjustment is not due to mean reversion or an attempt to maintain a minimal level of earnings. I find no evidence that households facing steeply positive marginal tax rates – e.g., those with earnings within the phase-out segment of the EITC – scale back their earnings.

My primary empirical strategy assumes that labor supply choices early in the year are not made based on worker's expectation of their end-of-year tax rate. While uncertainty about annual earnings makes this likely to be close to accurate, it may not hold exactly. To assess the importance of this assumption, I estimate an alternative model in which I test whether households adjust earnings earlier in the year based on their expectation of year-end tax incentives. I find that my main result still holds; earnings responses are more sensitive to forecasted tax incentives nearer the end of the tax year. This supports the interpretation of my main estimates, in that they reflect a real response to tax policy rather than bias.

My approach boasts several advantages over other strategies for measuring labor supply elasticity. First, I overcome classic econometric issues that plague most other empirical approaches (See Keane, 2011, for a summary). I address the "taste for work" bias by controlling for to-date earnings and measuring adjustments within the same household over multiple years. I address simultaneity by instrumenting for workers' current net of tax wage using their predicted year-end after-tax income. This instrument also addresses concerns about mismeasurement of hours and earnings. Second, my approach overcomes the selection and composition issues that potentially bias difference-in-difference evaluations (Nichols and Rothstein, 2016); I use panel data to identify responses within the same households within and across tax years. Third, I address external validity concerns inherent to investigations of unique tax reforms by studying a common and regular setting faced by many different workers in a variety of circumstances over many different years. Fourth, I do not rely on a structural model with strong parametric assumptions to interpret cross-sectional data.

To make this approach more tractable, I propose a finite, multi-period model of labor supply in which a representative agent, who aims to maximize utility over consumption and leisure, decides at the start of each period how much to work. The agent makes this decision given its to-date earnings, uncertainty about being employed in this and future periods, a non-linear tax schedule, and parameters governing preferences and probability of employment. In the first period, the agent chooses a preferred bundle of work and leisure based on its expectation of future employment. As the agent realizes its employment history and gains certainty about its expected tax rate, its optimal labor supply choice becomes more sensitive to previous earnings. I show that agents facing a less positive tax rate tend to increase labor supply in the final period in order to maximize post-tax income, and workers facing a more positive tax rate will work less. These adjustments result in greater bunching in proposed annual income as the tax year progresses.

This paper makes a number of important contributions to several literatures in labor and public economics.

First, this paper contributes to a substantial literature measuring labor supply and taxable income elasticity using non-linear budget sets (e.g., Burtless and Hausman, 1978; Hausman, 1985; Moffitt, 1990; MaCurdy, Green and Paarsch, 1990; Keane and Moffitt, 1998; Blundell and MaCurdy, 1999; Blundell et al., 2000, 2009), and a related literature using "bunching" at kink points in tax schedules to measure the same elasticities (Saez, 2010; Kleven and Waseem, 2013; Blomquist et al., 2021). The non-linear budget set is the starting point for my model; I extend it by permitting agents to adjust their labor supply choice over time. I provide important context to Saez's 2010 well-known result, arguing that some workers are aware of their tax incentives and try to move toward kink points in the tax schedule, but are precluded from perfectly bunching by numerous frictions.

Second, this paper adds to a newer literature measuring the size and relevance of those frictions. Chetty et al. (2011) and Gelber, Jones and Sacks (2020) both measure the importance of adjustment frictions in mitigating labor supply responses to tax and transfer policy. One friction is incomplete knowledge of tax incentives. Tax complexity and salience both matter to workers' labor supply choices (Chetty, Friedman and Saez, 2013; Feldman, Katuščák and Kawano, 2016; Miller and Mumford, 2015), and workers must decide whether to optimize over expected marginal or average tax rates (Liebman and Zeckhauser, 2004; Rees-Jones and Taubinsky, 2019). I contribute to this literature by measuring the relevance of uncertainty about future employment – an oft-cited but under-studied type of an optimization friction – to labor supply choices. I also document that workers appear more responsive to their expected average tax rate, as opposed to their expected marginal tax rate.

Third, this paper contributes to the literature investigating labor supply effects of the EITC. Much of the non-linearity in tax incentives that I study is due to the EITC's structure. Accordingly, this paper can largely be understood as a study of households' response to this program. Most work finds that the EITC draws individuals into the workforce (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001; Grogger, 2003; Hotz and Scholz, 2006; Gelber and Mitchell, 2011; Schanzenbach and Strain, 2020), but that consensus has come under some question (Kleven, 2019). Most of the same studies find limited evidence of an intensive-margin response, despite clear theoretical predictions that the program should have such an effect (Meyer, 2002; Hotz, 2003; Eissa and Hoynes, 2006; Saez, 2010).

Similar to Chetty, Friedman and Saez (2013) and Chetty and Saez (2013),[2] I use differences in workers' awareness of their tax incentives to identify their labor supply response to the EITC. Like these studies, but in contrast to much of the literature, I find a non-zero intensive margin response to the program. Further, I find that this effect is driven entirely by households who expect to have earnings within the phase-in segment of the EITC, but no evidence of labor supply reduction by households predicted to face a highly positive marginal tax rate due to the EITC's phase-out segment.

Fourth, this paper contributes to a large literature studying both the extensive and intensive margin Frisch elasticity (MaCurdy, 1981; Altonji, 1986; Pencavel, 1986; Angrist, 1991; Pistaferri, 2003; Card and Hyslop, 2005; Kimball and Shapiro, 2008; Manoli and Weber, 2016; Blundell, Pistaferri and Saporta-Eksten, 2016) and how transitory changes in wages and tax policy affect

[2] Chetty, Friedman and Saez (2013) estimate an average intensive margin earnings elasticity of .14 in the phase-out region and .31 in the phase-in region of the EITC, identified from apparent variation in local knowledge about the EITC schedule and labor supply changes to the birth of a child. Chetty and Saez (2013) evaluate an experiment in which they inform some taxpayers about the EITC's structure to test how awareness of the EITC's incentives affects labor supply. They find a small (3%) increase in average earnings among treated subjects, implying a labor supply elasticity of .075 (Nichols and Rothstein, 2016).

labor supply choices (Camerer et al., 1997; Fehr and Goette, 2007; Farber, 2008; Crawford and Meng, 2011; Stafford, 2015; Powell, 2015; Martinez, Saez and Siegenthaler, 2021). Much of this work focuses on the relevance of those shocks in life-cycle models (Heckman and MaCurdy, 1980; Keane, 2011; Keane and Rogerson, 2012). I study how idiosyncratic wage shocks affect workers short-run labor supply behavior. My estimate of the Frisch elasticity is similar to those in the micro-literature summarized by Reichling and Whalen (2012).

Fifth, this paper extends a small literature that documents how workers' labor supply can vary within the tax year. Yang (2018) and Powell (2020) both study labor supply effects of receiving a lump sum cash payment (i.e., EITC disbursements and 2008 stimulus payments). Each of these authors document an important extensive margin response due to the income effect. In contrast, this paper studies the relevance of the substitution effect on households' within year labor supply behavior. More similar to this study, Wilson (2020) uses the panel-nature of the CPS to document how EITC expansions decrease workforce exits and increase overall months worked among single mothers. Looney and Singhal (2006) study how losing a dependent exemption from aging children affects households' labor supply behavior in the short-term.

Finally, this paper makes an important contribution to policy-relevant discussions about potential reforms to tax-based means-tested programs like the EITC. Policymakers continually express interest in reforming the EITC so that it subsidizes households' earnings throughout the tax year instead of via one lump sum (Jones, 2010; Maag, 2019). A concern about the viability of such reforms is how to project annual household earnings and tax liabilities, and whether households might be asked to repay paid-out assistance if those projections are inaccurate. This study provides evidence about how households exhibit large changes in expected EITC eligibility across and within years. I also demonstrate for which households these predictions are likely to be most wrong.

The paper proceeds as follows. In Section 1.2, I describe my economic model. In Section 1.3, I describe the SIPP and the California administrative data, how I convert households in these data to tax units, and how I estimate households' tax rates using TAXSIM. In Section 1.4, I use these data to document the non-linear tax policy facing low-income households as well as variation in household income and tax rates within and across tax years. In Section 1.5, I describe my empirical analysis. In Section 1.6, I summarize my results. In Section 1.7, I conclude.

1.2 Model

Consider the quasi-linear and isoelastic labor supply utility model proposed by Saez (2010). A worker with ability n aims to maximize utility over consumption c subject to a cost from working, $f(z)$. z denotes workers' pre-tax earnings, which are the product of the worker's wage w and labor supply h. e indexes the worker's compensated elasticity. Consumption c equals net of tax earnings:

$z - \tau(z)$. The worker faces a single-kinked tax schedule: $\tau(z) = \tau_0 \min(z, T) + \tau_1 \min(0, z - T)$.[3]

$$\begin{aligned} u(c, z) &= c - f(z) \\ &= c - \frac{n}{1 + \frac{1}{e}} \left(\frac{z}{n}\right)^{1+\frac{1}{e}} \end{aligned}$$

Saez's (2010) model assumes each worker makes a single labor supply choice h^* to maximize $u(c, z)$. I modify the model to incorporate sequential choices of labor supply and uncertainty about future earnings. Suppose a worker chooses a labor supply level h in each of D periods that comprise the tax year, and in each period, the worker is actually employed at h with probability p. The worker earns pre-tax income $z_d = wh_d$ in period d if employed and 0 if not. She only pays the cost of working, $f(h)$, if she's employed. She faces the same single kinked tax schedule, $\tau(z)$, but the tax is applied on total earnings across all D periods, meaning her consumption is a function of total earnings across all D periods: $c = \sum_{d=1}^{D} z_d - \tau\big(\sum_{d=1}^{D} z_d\big)$.

The worker chooses h_d^* in each period d to maximize expected total utility across all D periods, treating h_t as fixed for periods $t < d$, and knowing that she will adjust h_t for $t > d$. Given values for parameters $p, \tau_0, \tau_1, n,$ and e, I can identify the agent's labor supply choice h for any level of y_{D-1} via dynamic programming. I solve the agent's labor supply decision recursively, beginning in period D and ending with period 1.

In period D, the agent chooses h_D^* to maximize total consumption subject to $f(h)$ and given actual hours worked in each period to date. h_d^* denotes an optimal labor supply choice; $\bar{h}_d$ denotes realized hours in period d. The agent takes an expectation over total consumption and the cost of the optimal hours choice in the final period, given the $(1 - p)$ probability that she might not be employed.

$$\begin{aligned} \max_{h_D} E[U(c, \bar{h}_1, \bar{h}_2, \ldots, \bar{h}_{D-1}, h_D)] = &\underbrace{E[c(\bar{h}_1 + \bar{h}_2 + \ldots + \bar{h}_{D-1} + h_D)]}_{\text{total consumption}} \\ &- \underbrace{f(\bar{h}_1) - f(\bar{h}_2) - \ldots - f(\bar{h}_{D-1}) - E[f(h_D)]}_{\text{cost from working in each period}} \end{aligned}$$

This yields optimal labor supply choice $h_D^*(\bar{h}_1 + \bar{h}_2 + ... + \bar{h}_{D-1})$. The agent chooses h_D^* to maximize utility given to-date hours. The agent solves this final period maximization problem in nearly the same way an agent would solve the single period problem, using an expected value calculation for their hours choice in period D.

Now, I move back one period to $D - 1$. The agent maximizes h_{D-1} given to-date earnings and an expectation of her choice in the following period.

[3] Saez (2010) shows that, with a continuous ability parameter n, the size of the mass in the earnings distribution clustered around z^* is determined by the compensated elasticity e. Saez (1999) shows that if workers actually earn $z + \epsilon$, this bunching would appears more as a dispersed mass as opposed to an atom, but the model still predicts taxpayers cluster at the kink T.

$$\max_{h_{D-1}} E\big[U(c, \bar{h}_1, \ldots, \bar{h}_{D-2}, h_{D-1}, h_D)\big] = \underbrace{E\big[c(\bar{h}_1 + \ldots + \bar{h}_{D-2} + h_{D-1} + h_D)\big]}_{\text{expected total consumption}}$$
$$- \underbrace{f(\bar{h}_1) - \ldots - f(\bar{h}_{D-2}) - E[f(h_{D-1})] - E\big[f(h_D)\big]}_{\text{cost from working in each period}}$$

The agent chooses h^*_{D-1} to maximize expected utility. Both optimal labor supply and expected utility can be expressed as functions of to-date earnings: $h^*_d = g(h_{D-1})$, and $EU_d(h_{D-1})$. This gives a general expression for expected utility EU_{d+1} in any period d given optimal labor supply choices in subsequent periods. The agent chooses h^*_d to maximize EU_{d+1} for every period d.

Since EU_{d+1} is non-linear, I solve this model using grid search for a particular set of values $D, p, \tau_0, \tau_1, z^*$, and e.[4] Figure 1.3 presents the labor supply choices in each period d for an agent with $n = 1$. In each period, and for any hours worked to date, the agent has a unique optimal labor supply choice h^*. In the second period, the worker is fairly insensitive to hours worked in the first. As time progresses, the worker becomes more sensitive to average hours worked to date. If average number of hours worked to-date is low, meaning the worker experienced a number of unemployed periods and its earnings will likely be low enough that she will not face the higher tax rate, she proposes to work her maximum number of hours in the next period. If average hours worked is high, the worker scales back her hours choice.

Panel A in Figure 1.4 presents the same results but for three agents with different levels of ability n. This means these agents incur different costs from work and will have different optimal labor supply choices. Though levels and sensitivities vary, the overall pattern remains the same. Regardless of $h_{d=1}$, agents choose roughly the same $h^*_{d=2}$ given an expectation of future employment, but labor supply choices adjust in each subsequent period as their uncertainty is resolved. If average hours to date are sufficiently low, all three agents choose to work their $h_{\max}$. And if to-date hours are sufficiently high, they work their optimal number of hours under the higher tax rate.

Panel B in Figure 1.4 presents results for three agents with the same ability n but three different elasticities, e. Changes in e reflect agents' sensitivity to the change in the cost of work. Again, though levels and sensitivities vary, the overall pattern is the same. Agents choose $h^*_{d=2}$ given their expectation of future employment, but labor supply choices change in each period as that uncertainty is resolved. All three agents choose to work the maximum number of hours if average hours to date are low, meaning they have a low chance of having a total income above T and facing tax rate τ_1.

Next, I solve the model for 500 levels of n.[5] For each n, I solve the full dynamic programming model, saving all possible earnings and choices in each period. Then I select a single earnings

[4] I present results from a model in which I use $D = 12$, $p = .8$, $\tau_0 = 0$, $\tau_1 = .3$, and $e = .5$, but results are qualitatively similar for alternative values of each parameter. For each period, I construct a grid of discrete levels of possible to-date earnings and hours choices, $z \times h$, where $z = [0, d \times wh_{\max}]$ and $h = [0, wn(1-\tau_0)^e]$. Specifically, I use 200 possible hours choices and 5000 earnings possibilities. Using a coarser or richer grid would not affect my main results.

[5] I sample random levels of n from $\sim \mathcal{N}(1, .01^2)$.

sequence for each agent. I select a sequence by first identifying each agent's $h^*_{d=1}$. For this period and all others, the agent's actual h_d equals h^*_d with probability p, and h_d equals 0 with probability $1 - p$. I recover each worker's proposed hours choice and actual hours in each subsequent period given realized to-date earnings. This simulation yields one employment history for all levels of n. I calculate each agent's predicted annual earnings as of each period d, where $d < D : \hat{Y}_d = \frac{D}{d} \sum_{t=1}^{d} y_t$. I repeat this process for the same levels of e used above.

Figure 1.5 presents the kernel densities for $\hat{Y}_{d=4}$ and $\hat{Y}_{d=8}$, as well as actual total earnings, $Y_1 2$. There are three main takeaways from this simulation. The first is that agents exhibit greater bunching in predicted annual earnings at the end of the year than earlier in the year. As of the fourth period, the distribution exhibits limited mass at the kink point,[6] but as time progresses and labor supply decisions move toward each agent's extreme, bunching increases. The second takeaway is that the amount of bunching is affected by workers' elasticity. Workers with a higher elasticity are more sensitive to tax policy and exhibit greater bunching, as predicted. The third takeaway is that, with a low elasticity, bunching is not conspicuous enough in any period to be identified from the standard bunching estimator. However, the earnings distribution at year's end[7] is distinguishable from the distribution of predicted annual earnings as of period 4, and the difference between these distributions is due to the agent's sensitivity to tax policy.

1.3 Data

SIPP

I use the 1996, 2001, 2004 and 2008 panels of the Survey of Income and Program Participation (SIPP).[8] The SIPP is a nationally representative sample of approximately 25,000 to 45,000 primarily low-income households administered by the Census Bureau over the course of three to four years. Respondents are interviewed about their employment, hours worked, wages, earned and unearned income, household composition, and participation in government programs, among much else, every four months. Most important for my purposes, SIPP respondents are asked to report employment and earnings for the survey month and each of the three previous months. The SIPP is the only survey that captures individual and household earnings at multiple continuous periods across multiple calendar years.

[6]The distribution is bi-modal because some workers will propose to work more early in the year, anticipating that they might become unemployed at some point, but in fact are employed in each period. This adjustment captures a negative intensive margin response to τ_1. An alternative set of parameter values could yield a mass of workers with low proposed earnings who increase labor supply as the year progresses.

[7]One can think of this distribution as the total earnings that researchers observe in annual tax data.

[8]I do not use the most recent SIPP panel, which was first fielded in 2014. In the newly redesigned SIPP, respondents are asked to recall employment, earnings, and program participation for each month in the calendar year. I find that this reform tends to worsen cross-calendar year seam bias, which poses a unique threat to my approach. I find that employment rates exhibit a distinct discontinuity within households between December and January that I do not observe in other panels.

I identify likely tax units from SIPP survey units and narrow my sample to households for whom I can credibly identify tax liabilities, who are likely to face non-linear tax policy and who are firmly attached to the labor force. First, I limit my analysis to households that contain only one family, following Yang (2018). I further restrict to households for whom I have complete information about earnings, employment, and household configuration throughout each calendar year for all household members. This restriction ensures that my estimates of tax liabilities are not confounded by either changes in household composition or missing earnings information. I also restrict to households that include at least one working-age member between the ages of 25 and 55, and I exclude households where siblings and adult children have earnings, as opposed to just the household head and spouse, since these households may contain one tax unit. Finally, I restrict to households who have non-zero earnings in every calendar quarter, and whose total earnings in any set of three consecutive quarters is between $2,000 and $75,000. The purpose of these restrictions is to focus only on the intensive margin response and limit attention to households who are more likely to face some uncertainty in their tax rates.

My final sample includes earnings information for approximately 12,000 unique households and 18,000 unique tax units (i.e., households by tax year). Table 1.1 summarizes key demographic characteristics in my sample and how my restrictions affect the composition of my sample.

For each tax unit, I identify each head and spouses' total earned income in each month from both wages and self-employment. I sum both sources of earnings within each wave and quarter for each tax year. I also identify each households' unearned income each month (e.g., Social Security and unemployment insurance benefits).

SIPP households also complete various supplemental interviews in each panel. Two of these topical modules ask respondents about variables particular to tax filing, including: their property tax bill, amount of itemized deductions, retirement account contributions and deductions, capital gains and losses, and child and dependent care expenses. When available, I associate each adult in each tax unit with tax-relevant variables from these modules.

California Administrative Data

I start with program rosters for California's instantiation of the Supplemental Nutrition Assistance Program (SNAP), known as CalFresh, between 2014 and 2017. These records capture every recipients' per month enrollment in the program, the cases in which they were enrolled, and their demographic characteristics. In 2017, I observe approximately 5.6 million unique individuals across 2.9 million unique SNAP cases. Of these 5.6 million individuals, 2.5 million were younger than 18. Total caseloads are fairly constant over the four years in my sample.

I associate each adult enrolled in SNAP with their quarterly employer-reported earnings records. These records are collected by the California Employment Development Department (EDD), which administers the state's unemployment insurance program. I observe the earnings of each individual for six quarters prior to their enrollment in SNAP, every quarter in which they're enrolled, and 18 months after their last month enrolled. This means that even if an adult is only enrolled for a handful of months in 2016, I still observe their earnings for most, if not all of, 2015 and 2017.

I then match each individual in the SNAP program rosters to their California state tax returns from 2015 to 2017. For each return, I observe basic information about the composition of the tax unit, as well as all variables on the primary state tax form (Form 540). For e-filed returns, I also observe all variables on the Form 1040. I also observe select variables from individuals' information returns, including total wages reported on the W2. Together, these forms allow me to observe all the relevant tax information (i.e., unearned income, deductions, capital gains and losses, etc) necessary to identify income tax rates.

Between 30 and 35 million individuals appear on a state tax return in each tax year. Of the 5.6 million individuals enrolled in SNAP in 2017, about 3.7 million appear across 1.9 million unique state returns in tax year 2017. Roughly 38% of those 3.7 million were a head or a spouse on a return, and the remainder were dependents. These counts and fractions are fairly stable over the three years in my sample. Of the 1.4 million individuals who enrolled in SNAP and appear on a return as a head or spouse, 77 percent have positive EDD wages.

For my primary sample, I implement similar restrictions to those I applied to my SIPP sample, again, in order to focus attention on working-age households firmly attached to the labor force and for whom I can confidently estimate likely tax rates. I limit to households with an adult between the ages of 25 and 55, who have non-zero earnings in all quarters in each tax year, and whose earnings in any consecutive sequence of three quarters is between $2,000 and $75,000. I further restrict to California tax units in which all members were enrolled in SNAP for at least one month in the respective tax year and whose reported AGI matches their total quarterly earnings. I limit my analysis to these households in order to ensure that I can infer changes in true tax incentives only from changes in quarterly earnings. Table 1.2 summarizes how these restrictions affect the characteristics of my sample.

TAXSIM

I use NBER's TAXSIM program to identify SIPP and SNAP households' average and marginal income tax rates and income tax liabilities (Feenberg and Coutts, 1993). TAXSIM allows users to input key tax-related information for a given household, and returns income tax calculations using federal and state income tax policies for any year between 1960 and 2023. I input filing status, state, number of dependents, ages, earned and unearned income, and a variety of possible deductions. The program returns federal and state income tax liabilities and marginal tax rates (inclusive and exclusive of FICA taxes), federal and state EITC amounts, and more for all households. I use TAXSIM to identify how households' likely tax rates and liabilities change over the course of the tax year by summing household earnings over various periods and inputting these sums into TAXSIM. For example, to identify the likely tax rate on a household's fourth quarter earnings, I sum the household's earnings through the first three quarters of a tax year plus a predicted amount for the fourth quarter, and input this sum, as opposed to a households' true annual income, into TAXSIM.

1.4 Motivation

Non-linear Tax Policy

I use output from TAXSIM to document cross-sectional variation in average and marginal tax rates for households in the SIPP and SNAP samples.

Figure 1.6 illustrates how average and marginal tax rates vary by household income in 1997 versus 2012 for married SIPP households with 0, 1, 2 or 3+ dependents. I group households into bins of $2,500, and within each bin, I identify the average marginal and average income tax rates (combining federal and state income taxes as well as payroll taxes) that all households within that bin face on their annual earnings.

Households with children and very low earnings tend to face a steeply negative marginal and average tax rate on annual income. In 2012, the average household with children and annual earnings below $10,000 faced a marginal tax rate between negative 30 and 50 percent. Income taxes boosted these households' net income by 30 to 40 percent. When pre-tax household income eclipses about $45,000, the average tax rate settles to around 30 percent, regardless of the household's number of children.

In Figure 1.7, I plot the marginal and average tax rates by number of dependents and annual income for single and married SNAP households in California in 2017. The patterns are roughly the same as in the SIPP sample. For households with children and incomes below roughly $10,000, marginal and average tax rates are steeply negative. Thanks to California's supplement to the federal EITC, they are even lower than the national averages. Households with children and income in the phase-in portion of the EITC face both a marginal and average tax rate between negative 50 and 75 percent.

Both figures make clear how important the EITC is for eligible households: Negative marginal tax rates align with the phase-in part of the EITC schedule, and the highest marginal tax rates align with the phase-out range. When household income exceeds the maximum eligible income for the EITC, average and marginal tax rates appear to converge and hold steady at around 25 to 30 percent, regardless of household type. Figure 1.6 illustrates the impact of reforms that increased the maximum credit amount. Households with dependents and incomes below about $10,000 benefited from program expansions in 2001 and 2009, as well as the introduction of numerous state supplements.

The key takeaway, which is clear in both figures, is that similar households within a fairly narrow income range can face starkly different tax incentives as a function of their to-date earnings. As earnings rise from around $15,000 to $30,000, households quickly face steeply positive marginal tax rates and positive tax liabilities.

These different tax rates will only impact labor supply if households appreciate that tax policy is non-linear. Surveys and interviews of low-income workers suggest widespread awareness that tax filing is often associated with receiving a refund (Halpern-Meekin et al., 2015; Edin, Tach and Halpern-Meekin, 2014; Smeeding, Phillips and O'Connor, 2000), but only half can recognize the EITC by name (Bhargava and Manoli, 2015) and a minority are aware of the program's benefit structure (Smeeding, Phillips and O'Connor, 2000; Romich and Weisner, 2000; Chetty, Friedman and Saez, 2013). Still, these surveys and other ethnographic evidence also document an appreciation among EITC-eligible taxpayers that income tax policy boosts income for households with modest incomes (Halpern-Meekin et al., 2015). For income volatility to imply tax uncertainty, households must only grasp that these benefits are reduced when earnings exceed some level. Caldwell, Nelson and Waldinger (2021) present some evidence of this awareness, showing that households update their expectation of their likely refund amount using current year earnings.

Income variation

Next, I illustrate how pre- and post-tax income can vary significantly within and across tax year for a substantial share of households. In the SIPP, I identify household income through each month of the calendar year, and in the California administrative data, through every quarter. I project households' annual income as of each period assuming earnings in future periods equal their to-date average. Using these income projections, I also identify each household's predicted year-end average tax rate as of each period.

In Table 1.3, I report the share of SNAP and SIPP households for whom the absolute difference between their predicted annual income, predicted average and marginal income tax rates, and predicted total EITC amounts as of the end of each calendar quarter are more than particular values away from their year-end values. For one-third of households, predicted annual income as of the end of the first quarter is more than \$5,000 from their actual annual income. By the end of September, however, this share falls to just 5%. Only three percent of households have a total EITC amount that is more than \$1,000 different than their predicted EITC value as of the end of the third quarter. Table 1.3 also reports the average standard deviation for each variable within each tax year across all SNAP households. Figure 1.11 plots the distribution of standard deviations in predicted earnings within tax years across all SNAP households.

Table 1.4 reports differences between, as opposed to within, tax years. Cross-year variation is more significant than within-year variation, which reflects both how volatile earnings can be over longer time periods and how households gain clarity about likely earnings within tax years. The various panels in Figure 1.8 plot the distributions of these differences. The red dotted lines indicate the median of the absolute value of all the differences. Half of all SNAP households experience a year-over-year change in wage earnings of at least \$4,500. This corresponds to half of households experiencing an average tax rate one year that is more than seven percentage points different than what they faced the year before.

This variation does not necessarily imply unexpected volatility for all workers. Workers might be able to anticipate future spikes and dips in work hours and wages, which a researcher cannot observe (Card, 1991; Pistaferri, 2003). Workers might also choose to substitute when and how much they work in response to changes in the personal opportunity cost of work, unrelated to tax incentives. Still, it is reasonable to expect that, especially for lower-income households, these variations do reflect some volatility. Unemployment spells can be unanticipated, both in their occurrence and their length, which is why we have a large social program to insure against those risks. Firms also exert significant control over many workers' schedules, which translates into volatile hours worked and total earnings (Golden, 2015; Maag et al., 2017; Gerstel and Clawson, 2018; Schneider and

Harknett, 2019). Even if wages are stable, year-end wage bonuses or unexpected dividends can also affect annual income and tax liability (Saez, 1999). I cannot distinguish for which households earnings changes reflect unanticipated volatility as opposed to intentional reallocations between labor and leisure. I rely on others' work demonstrating that volatility is common enough that a significant share of workers in both samples experience these idiosyncratic wage shocks.

Figure 1.9 provides suggestive evidence that households do shift in the direction of maximizing their after-tax income. I plot the distribution of predicted annual earnings among SNAP households with two dependents in 2017 as of the end of the first and third quarter, alongside their actual earnings. The distributions look fairly similar, reflecting that average distribution of quarterly earnings in the SNAP sample is fairly constant over time. However, note that a greater mass of SNAP households are predicted to have very modest earnings at the beginning of the year than later in the year. This mass appears to shift towards the center of the distribution, where households would maximize their total EITC receipt, by the end of the year. This shift mirrors the predictions from my model. It is suggestive of an increase in labor supply over the course of the tax year on the part of households facing a tax incentive to work more. Further, a greater mass of taxpayers appears to be near the second kink point of the EITC schedule as of the end of Q3, but that mass decreases in the following quarter. There is limited if any evidence of any shift on the part of households with earnings predicted to be in the phase-out portion of the EITC, however.

Figure 1.10 provides additional evidence that the change in the distribution in annual earnings reflects a shift along the EITC schedule. For all SNAP households, I identify their predicted state and federal EITC amounts as of the end of the third quarter, assuming that fourth quarter earnings equal the average of the first three. I subtract this amount from the households' actual EITC amount, and plot this difference over predicted annual income as of the end of the third quarter. Households whose predicted earnings would place them within the phase-in part of the EITC schedule exhibit the greatest difference between their actual and predicted EITC amounts.

Of course, the shifts identified in Figure 1.9 and Figure 1.10 might be due to mean reversion: households with low past earnings bounce back to a more normal earnings level over time. They might also reflect households with very low past earnings increasing their labor supply in order to achieve a minimal level of earned income. They might also be noise. I distinguish these explanations from an intentional response to tax incentives in my empirical analysis.

1.5 Empirical Framework

Consider the simple cross-sectional estimation of the elasticity of labor supply with respect to net of tax wages:

$$\underbrace{l_i}_{\substack{\text{labor}\\\text{supply}}} = \alpha + \underbrace{\beta w_i}_{\substack{\text{net of tax}\\\text{wage rate}}} + \underbrace{\gamma y_i}_{\substack{\text{non-labor}\\\text{income}}} + \underbrace{X_i'\delta}_{\substack{\text{demographic}\\\text{controls}}} + \varepsilon_i \tag{1.1}$$

β captures uncompensated wage effects on labor supply l, and γ captures income effects. Estimates of β using cross-sectional variation in w are biased due to omitted variable and simultaneity: "taste for work" is correlated with w, and changes in l affect w via the tax rate the agent faces. To overcome both concerns, recent work tends to estimate β by measuring employment responses to tax reforms that affect similar workers differently and where changes in w are plausibly exogenous. These analyses regularly use repeated cross-sectional surveys to identify how employment rates change between treated and untreated workers. Since tax reforms can affect the composition of these groups of workers, one generally cannot rely on these settings to identify intensive margin effects. That these reforms might also affect the equilibrium wage raises additional identification concerns (Rothstein, 2010).

I identify β by measuring how earnings change in response to changes in expected tax rates faced by similar workers within the same tax year and the same worker across subsequent tax years. I estimate this response across multiple specifications, but each approach borrows from Equation 1.1 in that I regress a measure of labor supply, y, in a given period on an observation's expected net of tax earnings rate for that period, ω, along with a variety of controls. The models differ in how I define and instrument for ω and how I test for varied earnings responses across the tax year.

First, I define my key independent variable: ω, the net of tax wage rate, or the fraction of the next period's projected earnings that the household expects to retain post-tax. The variable is a function of household i's earnings from the three previous quarters, z,[9] and tax policy in year y and state s.[10] A positive one unit increase in this ratio equals a 100 percentage point increase in the observation's net of tax predicted earnings.

$$\omega_{iyq} = \underbrace{\left(\frac{1}{3}z_{iyq}\right)^{-1}}_{\substack{\text{average to-date}\\ \text{earnings}}} \Bigg(\underbrace{f(\frac{4}{3}z_{iyq})}_{\substack{\text{post-tax predicted}\\ \text{quarter's earnings}}} - \underbrace{f(z_{iyq})}_{\substack{\text{post-tax previous}\\ \text{three quarter's earnings}}} \Bigg),$$

$$\text{where } f(y) = y - \tau_{ys}(y) \text{ and}$$

$$z_{iyq} = \sum_{q=-3}^{-1} y_{iyq}$$

Figure 1.12 summarizes the distribution of $\omega_{q=4}$ for all SIPP and SNAP tax units.

Effect in Q4

In the first model, I identify how each households' earnings in the fourth quarter vary with their predicted net of tax earnings rate in the fourth quarter. I control for each household's earnings from

[9]I predict a household's ω assuming earnings in the final period equal its to-date average. The advantage of this approach is it's straightforward, but it clearly reflects only one method for predicting households' net of tax wage rate in the next period.

[10]Note that I use y to denote quarterly earnings, as well as the tax year. When y is in a subscript, it denotes a tax year. Otherwise, it represents earnings.

the three preceding quarters, z, as well as state-by-year, household type, and demographic fixed effects.[11] The parameter β is a measure of households' earnings elasticity.

$$\underbrace{\ln y_{iy,q=4}}_{\text{log Q4 earnings}} = \beta \underbrace{\omega_{iy,q=4}}_{\substack{\text{predicted}\\\text{net of tax}\\\text{wage rate}}} + \gamma \underbrace{\ln z_{iy,q=4}}_{\substack{\text{log Q1-Q3}\\\text{earnings}}} + \underbrace{\alpha_i}_{\substack{\text{household}\\\text{fixed effect}}} + \underbrace{\theta_{ys} + \theta_h + \theta_x}_{\substack{\text{year} \times \text{state,}\\\text{household type,}\\\text{demographic}\\\text{fixed effects}}} + \varepsilon_{iy,q=4} \tag{1.2}$$

This approach addresses the simultaneity problem, because I identify how earnings in a given period vary with predicted tax incentives in that period. I also account for unobserved and fixed characteristics of each household by measuring this response within the same household over subsequent tax years. However, since ω is a function of previous earnings and despite controlling for z, my estimate of β is confounded by relationship between earnings and lagged earnings. Households with lower past earnings are likely to have lower future earnings. Moreover, the correlation between past and future earnings likely varies over levels of z and ω. For example, households with especially low z or high ω might exhibit higher future earnings if they are reverting back to average earnings level or working to achieve a certain minimal level of earnings.

"Rolling Window" Approach

I distinguish households' response to tax policy from standard serial correlation in earnings by comparing the response in the actual tax year to those that in simulated tax years that end in the first, second or third calendar quarter. I construct simulated tax years comprised of all possible sequences of four consecutive quarters, and I identify ω for each sequence.[12] Figure 1.1 illustrates how these sequences are constructed.

I stack these sequences for each household and estimate the following variation of Equation 1.2.

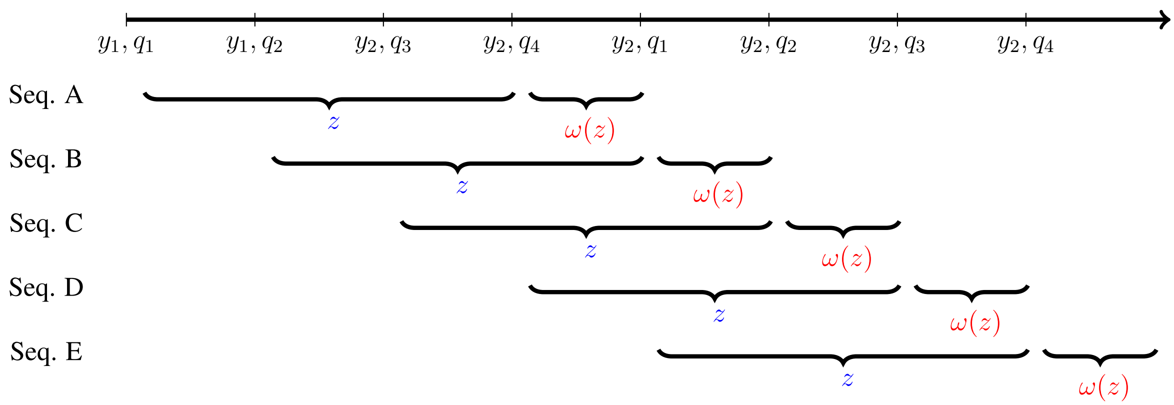
$$\underbrace{\ln y_{iyq}}_{\text{log earnings}} = \beta\omega_{iyq} + \pi_q \underbrace{\omega_{iyq} * Q}_{\substack{\text{predicted net of}\\\text{tax wage rate} \times\\\text{quarter dummy}}} + \gamma \underbrace{\ln z_{iyq}}_{\substack{\text{log sum of last}\\\text{three quarters'}\\\text{earnings}}} + \underbrace{\alpha_i}_{\substack{\text{household}\\\text{fixed effect}}} + \underbrace{\theta_{ys} + \theta_h + \theta_x}_{\substack{\text{year} \times \text{state,}\\\text{household type,}\\\text{demographic}\\\text{fixed effects}}} + \varepsilon_{iyq} \tag{1.3}$$

I interact ω with an indicator for calendar quarter, Q. The coefficient on this interaction, π, captures the unique relationship between $\ln y$ and ω, or the excess sensitivity of $\ln y$ to ω, in calendar quarters 2, 3, and 4 relative to quarter 1. If serial correlation in earnings does not have a systematic seasonal pattern, then each π_q should equal zero. If households adjust earnings to predicted tax incentives when those incentives are more binding at year's end, then $\pi_{q=4}$ will be

[11] Household type is the interaction between filing status (single versus married) and number of dependents (0, 1, 2 and 3+). Demographic fixed effects are the interaction of the household head's race, binned age levels (25-34, 35-44, 45-54), language, and gender.

[12] This means that I have four measures of ω and z for each tax year, one for each quarter, for every tax unit. If I observe a household for three tax years, I have twelve observations for that household.

Figure 1.1: Constructing sequences of four consecutive quarters



Notes. Figure 1.1 illustrates how I construct simulated tax years. Each sequence of four consecutive quarters with earnings information is a simulated tax year, and I use the sum of the first three quarters' earnings in this simulated tax year z to predict earnings in the next quarter, and I identify ω as a function of z. Note that Seqs. A and E align with true tax years.

positive. Indeed, my preferred estimate of labor supply elasticity will be my estimate of $\pi_{q=4}$ from this model. This coefficient identifies the relationship between log earnings and predicted net of tax wage rate in the fourth quarter, netting out the standard period-to-period relationship between past and future earnings captured by β.

I estimate four additional variations of Equation 1.3.

First, the linear parameters π_q mask important heterogeneity in the response across both earnings and values of ω. To identify whether responses estimated in Equation 1.3 are driven by households facing particular levels of ω, I re-estimate Equation 1.3, but replace the continuous measure of ω with binned levels of ω. I group households into those with ω less than .55, greater than 1.1, and increments of .05 in between.

$$\ln y_{iyq} = \beta\Omega_{iyq} + \pi_{q\omega}\Omega_{iyq} * Q + \gamma \ln z_{iyq} + \alpha_i + \theta_{ys} + \theta_h + \theta_x + \epsilon_{iyq} \tag{1.4}$$

Next, to illustrate how this response varies with income as opposed to tax rates, I replace binned values of ω with binned values of predicted annual income, ζ, where predicted annual income equals earnings from the previous three quarters, z, plus predicted income in the next quarter, $\frac{4}{3}z$. I group households into $2,000 bins. I interact values of ζ with indicators for calendar quarters 2, 3 and 4.

$$\ln y_{iyq} = \beta\zeta_{iyq} + \pi_{q\zeta}\zeta_{iyq} * Q + \alpha_i + \theta_{ys} + \theta_h + \theta_x + \epsilon_{iyq} \tag{1.5}$$

To evaluate whether households respond to their expected average tax rate or their expected marginal tax rate, I re-estimate Equation 1.3 but replace ω with households' predicted marginal tax rate on the last predicted dollar earned in the subsequent quarter.

Finally, to test whether Equation 1.3 identifies a change in the amount worked, as opposed to earned, I use log hours as the outcome variable, instead of log earnings. I only observe reported hours in the SIPP, meaning I can only estimate this response in that sample.[13]

Instrumenting for $\omega_{q=4}$

Households can make reasonable estimates of their annual income and their likely net of tax wage rate on January 1st using other information, some of which is observed by the researchers (e.g., household composition, state, year, occupation, age) and some of which is not (e.g., full work histories, preferences, understandings of their local labor market, or agreements with employers). I test whether households exhibit this behavior by estimating the relationship between labor supply in each calendar quarter, y_q, and households' expected year-end net of tax wage rate $\omega_{q=4}$ as of each quarter. I instrument for each household's $\omega_{q=4}$ with similar households' actual $\omega_{q=4}$. I regress each households' actual $\omega_{q=4}$ on a vector of household characteristics (age, race, gender, state, year) and their predicted year-end income given their to-date earnings within the tax year, $\tilde{z}$, as of four different periods: the start of Q1, the end of Q1, the end of Q2 and the end of Q3.[14] Figure 1.2 illustrates how I construct these sequences.

I stack each sequence for each tax unit and regress each tax unit's actual $\omega_{q=4}$ on earnings as of the start of each period, as well as various fixed effects.

$$\underbrace{\omega_{iy,q=4}}_{\substack{\text{household } i\text{'s} \\ \text{true } \omega_{q=4} \\ \text{in year } y}} = \underbrace{\tilde{z}_{iyq}}_{\substack{\text{earnings as of} \\ \text{the start of} \\ \text{qtr } q \text{ in year } y}} + \underbrace{\theta_{ys} + \theta_h + \theta_x}_{\substack{\text{year} \times \text{state,} \\ \text{household type,} \\ \text{demographics}}} + \epsilon_{iyq} \tag{1.6}$$

Using estimates from Equation 1.6, I predict each tax unit's year-end $\hat{\omega}$ for every sequence. I then estimate Equation 1.7.

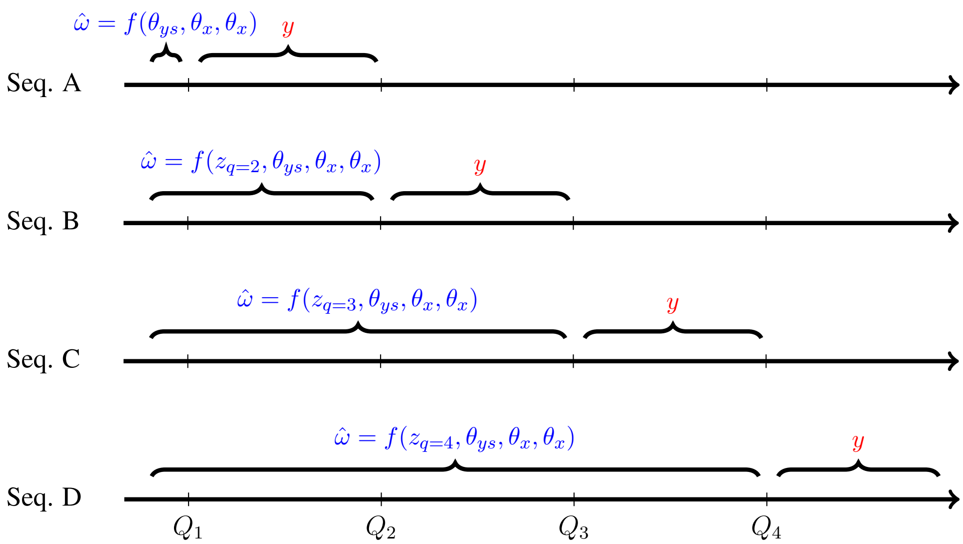
$$\ln y_{iyq} = \beta\hat{\omega}_{iyq} + \pi_q\hat{\omega}_{iyq} * Q + \gamma \ln z_{iyq} + \alpha_i + \theta_{ys} + \theta_h + \theta_x + \varepsilon_{iyq} \tag{1.7}$$

As in Equation 1.3, I control for the log of the three previous quarter's earnings to account for dynamic earnings processes. The coefficient, π_q, on the interaction of ω and the quarter Q, identifies whether there is a unique relationship between one's expected year-end net of tax wage rate and one's earnings in the following quarter. If workers can forecast their year-end $\omega_{q=4}$ accurately, we might expect each value of π_q to be zero. Households can respond to their year-end tax incentives but that response does not have to principally occur at year's end.

[13] SIPP respondents only report average hours worked in a representative week in the calendar month.

[14] Since households have no earned income as of the beginning of the tax year, I do not include a control for earnings when predicting ω at the start of the tax year.

Figure 1.2: Instrumenting for $\hat{\omega}$ as of the start of each quarter



Notes. Figure 1.2 illustrates the sequences for which I identify $\hat{\omega_q}$. In Seq. A, I use non-earnings information observable before the tax year begins, including indicators for filing status by number of dependents, year by state, and demographic characteristics. I identify the household's likely $\omega_{q=4}$ from these variables, and then identify whether $y_{q=1}$ positively covaries with this prediction. In Seq. B, I use the same information as in Seq. A and add in predicted earnings information from quarter 1, and so on.

1.6 Results

Main Results

Table 1.5 and Table 1.6 summarize estimates of β and π across different versions of Equation 1.3 among SIPP and SNAP households, respectively. When I do not account for previous earnings, I find that, in both samples, households with lower previous earnings tend to have low earnings in the final quarter as well. When I control for z, earnings response is positive in the SNAP sample, but statistically insignificant in the SIPP sample. When I include the household fixed effect, I estimate an earnings elasticity in the SNAP sample to be .07; the counterpart estimate in the SIPP sample is negative, but insignificant.

These estimates are likely biased because Equation 1.2 does not account for serial correlation issues. Table 1.7 and Table 1.8 summarize estimates of β and π across different versions of Equation 1.3 among SIPP and SNAP households, respectively, which aims to address this concern. Though magnitudes differ, overall patterns are similar in both samples. When I do not account for z, I find that households with higher ω have lower earnings in the subsequent quarter, which captures the serial correlation issue raised above: Households with lower past earnings are expected to continue to have low earnings, despite their high ω. If I include z but only account for heterogeneity across households by controlling for demographics, household type, and state-by-year effects,

θ_x, θ_h, and θ_{ys}, I find almost no relationship between short-term tax incentives and earnings. When I include a household fixed effect, α_i, I recover the predicted pattern. When the same household faces a higher ω, they tend to earn more in the next period, and this relationship is strongest at year's end when ω most binds. Recall that my preferred estimate of the intensive margin labor supply elasticity is $\pi_{q=4}$; this parameter identifies the excess sensitivity of earnings to ω at the end of the real tax year, netting out the period-to-period relationship between earnings levels captured by β. In the SIPP sample, I estimate an intensive margin labor supply elasticity of .08. In the SNAP sample, I estimate the same elasticity to be .18.

Figure 1.13 plots estimates of $\pi_{q\omega}$ from Equation 1.4. The positive relationship between earnings and the predicted net of tax wage rate is driven largely by households facing an ω greater than one, and this relationship is strongest in the fourth quarter. This response is observed in both samples, though it's clearest in the larger SNAP sample, where earnings are measured with greater accuracy and there are a larger share of households with especially low earnings.

Figure 1.14 plots estimates from Equation 1.5 and further illustrates that this response is largely driven by incentives created by the EITC. I overlay these estimates on the EITC schedule for single filers with two dependents in 2017. For Panel B, I use the combined federal and state EITC schedule. The non-linear relationship between $\ln y$ and levels of predicted income, ζ, suggests that households who expect to be in the phase-in range of the EITC are especially likely to increase earnings in the following quarter. That this effect is particularly pronounced in the fourth quarter suggests it is a response to tax incentives and not due to other serial patterns.

Table 1.9 and Table 1.10 summarize estimates from Equation 1.7. Results are similar across the four models, because ω is a function of these controls. Despite the alternative definition of ω, my results are quite similar to those summarized in Table 1.7 and Table 1.8. Households still appear to increase subsequent labor supply when they expect to face a positive ω and this response is strongest at the end of the year.

Supplementary Results

Response to Marginal Tax Rate

I estimate another version of Equation 1.3 in which I use households' predicted combined federal and state marginal income tax rate as my independent variable. Results are summarized in Table 1.11 and Table 1.12. In the SIPP, I find no relationship between earnings and marginal tax rate. In every model, the effect is near zero. This is likely due to my SIPP sample having too few households with low enough earnings to face steeply negative tax marginal rates. My preferred estimate in the SNAP sample, however, recovers the expected negative relationship, and the effect is again clearest at the end of the year. The response is much more limited, however, which suggests that households are more responsive to their expected average tax rate.

Change in Hours Worked

Table 1.13 summarize estimates of Equation 1.3 when I use log hours as my outcome variable in Equation 1.3, as opposed to log earnings. Results are similar to those summarized in Table 1.7, but I cannot rule out that the fourth quarter response is the same as the response in the second or third quarter. I interpret these findings to suggest that labor supply plays a role in the earnings elasticity reported in Table 1.7, but measurement issues in the SIPP make it difficult to draw a strong conclusion.

Subgroups

Next, I test whether these responses vary by filing status and presence of dependents. Estimates from analyses within these subgroups are summarized in Table 1.14 and Table 1.15. I find that married households are more sensitive to their predicted net of tax wage at year's end, which is consistent with previous literature. Results differ between the SIPP and administrative data for households with or without children.[15]

Table 1.16 summarizes results from additional estimates of Equation 1.3 restricting to the following subgroups in the SIPP.

- **Hourly workers.** We might expect this response to be clearer among hourly workers, since they are more subject to scheduling volatility and have greater flexibility in adjusting shifts and schedules in a particular quarter. Column 1 in Table 1.16 presents estimates of Equation 1.3 limited to households in which the head or spouse is an hourly worker. Though the effect is slightly higher, the estimates are not dramatically different.

- **Self-employed workers.** Saez (2010) shows that self-employed workers exhibit much more significant bunching at the first kink-point in the EITC schedule than wage-earners, which implies they have a higher taxable income elasticity. To test whether this bunching is due to a true earnings response or just tax manipulation, I estimate my model among the small number of households in my SIPP sample in which a head or spouse reports having some self-employment earnings in the tax year. I find that the response to year-end tax incentives is indeed much higher among households with a self-employed workers. My estimate of $\pi_{q=4}$ is .24, which is three times larger than the main result reported in Table 1.7. However, when I test whether that response is driven by changes in self-employed earnings, the effect disappears (Column 3). Together, these estimates paint a mixed picture about whether greater bunching among self-employed workers reflects a real labor supply response. That said, my sample size is small and there are reasonable concerns about measurement of self-employment earnings in the SIPP.

- **Excluding retail workers.** One concern with my approach is that there are significant changes in labor demand and supply around national holidays at the end of the calendar

[15]Recall that ω accounts for the significantly different tax incentives facing parents versus single adults. This analysis is comparing the earnings response between parents and non-parents who are facing similar tax incentives, and not those with similar levels of earnings.

year. For example, if lower-wage retail employees are more likely to work overtime, receive higher wages, or work more frequent shifts in the holiday season independent of tax incentives, this increase in earnings could bias my result. By controlling for occupational fixed effects in the SIPP, as well as household and worker fixed effects in both samples, I should account for this concern. To the extent those concerns remain, however, I also re-estimate Equation 1.3 in the SIPP excluding retail workers entirely from the sample. My results do not change.

- **Addressing seam bias.** Responses in the SIPP suffer from well-recognized seam bias. Changes in hours worked and wages are reported at the time of each survey. Respondents tend to project their current employment situation backwards, instead of accurately recalling each month's unique values. I address this concern, in part, by restricting my sample to households whose waves align with the tax year, meaning a wave does not stretch from Q4 of one year to Q1 of the next. Results from this model are reported in Column 5 of Table 1.16. Results are nearly identical to those from Table 1.7, suggesting this concern is not a significant problem.

Dynamic Panel Bias

My approach raises standard concerns with studies involving dynamic panels and lagged dependent variables. Even though neither of my models include an actual lagged value of y as a regressor, both ω and z are functions of lagged values of y. My preferred version of Equation 1.3 uses panel fixed effects, which risks introducing a mechanical relationship between the lagged dependent variable and the error term, biasing my estimated coefficients on ω and $\ln(z)$ (Nickell, 1981).

Alternative estimation strategies involving dynamic panel data account for heterogeneity across households without using fixed effects. Estimating these models in my setting presents some challenges. First, available tools for implementing the GMM estimators assume the endogenous term is a single lagged dependent variable. My approach involves five separate endogenous regressors: ω and the interactions with each calendar quarter, plus z, the log of the three quarters' earnings. Instrumenting for each of these variables with their lagged levels and differences yields not only a proliferation of instruments, but also involves differencing lagged values across tax years, as opposed to subsequent quarters. Second, even though I have information about earnings for up to twelve periods for some households, I am interested in the unique effect in particular calendar quarters. This means I only have a maximum of three periods for each household. GMM estimators are more useful when more past and future realization of the dependent variable are available.

Notwithstanding these issues, I estimate the Anderson-Hsiao estimator, which does not require using multiple lagged and future values of the dependent variable (Anderson and Hsiao, 1982).[16] I instrument for $\Delta\omega_{iyq}$ and $\Delta \ln z_{iyq}$ – the difference between each household's value of ω and $\ln z$ in the final tax year and the value from the penultimate tax year – with the furthered lagged values

[16] The "Difference GMM" and "Systems GMM" estimators promise increased efficiency over the Anderson-Hsiao estimator by leveraging additional information about the evolution of the lagged dependent variable from additional past of future values of that variable. Given my short panel, I am unable to implement these more popular estimators.

of ω_q and $\ln z$. I then relate the first difference between $\ln y_{iyq}$ in the final and previous period with the instrumented values of $\Delta\omega_{iyq}$ and $\Delta \ln z_{iyq}$.

$$\Delta \ln y_{iyq} = \pi_q \tilde{\omega}_{iyq} + \gamma \tilde{\ln} z_{iyq} + \theta_{ys} + \theta_h + \theta_x + v_{iyq} \tag{1.8}$$

Table 1.17 summarizes the results from this estimation among SIPP and SNAP households. The results differ from those presented in Table 1.7 and Table 1.8. Among SNAP households, despite noisy estimates, it remains the case that even instrumented earnings tend to increase as the tax year progresses, though the largest response now appears in the third quarter as opposed to the fourth. Among SIPP households, my estimates are different. The earnings response are substantially lower in the fourth quarter, the opposite of my main result. However, none of the estimates are statistically significant. In addition to the lack of statistical significance, and all of the issues about applying this estimator in my setting, there are other reasons to be cautious in interpreting these results. First, the true values of each parameter should fall between the biased estimates from the OLS and the fixed effects models (Bond, 2002), but all of my estimates in the SIPP sample, and all but my estimate of γ in the SNAP sample, are outside these bounds, suggesting my instruments are not valid. Second, I fail the over-identification test in both versions.

1.7 Conclusion

This paper studies the impact of tax policy on household labor supply using differences in uncertainty about annual income and tax incentives. Using survey and administrative data, I document significant within and cross year variation in household earnings and implied tax rates on those earnings. I use the fact that uncertainty about tax rates is resolved over the course of the tax year to identify the effect of tax policy on labor supply. I relate household earnings in the final quarter of the tax year to the share of the households' predicted earnings they expect to retain after taxes. I distinguish this response to tax incentives from standard serial correlation in earnings by comparing this response in other quarters, as though those quarters were the end of a tax year. I also use household fixed effects to account for omitted variable bias. I interpret households' excess sensitivity to their predicted net of tax wage rate in the fourth quarter as a measure of their labor supply elasticity. I conclude that households exhibit a small but non-zero intensive margin response to tax policy. My preferred estimate of the intensive margin labor supply elasticity is between .08 using the SIPP and .18 in the SNAP sample. Finally, I conclude that this effect is driven largely by the steeply negative tax rates created by the phase-in part of the EITC.

This study makes an important contribution to the academic literature studying labor supply response to tax policy. The most common approaches to identifying intensive margin labor supply elasticity suffer from important identification challenges, which my approach overcomes. Leveraging unique panel data, I identify how the same household adjust earnings when tax incentives change within and across tax years. I conclude labor supply elasticity is small, consistent with the rest of the micro literature.

My findings also provide useful guidance to policymakers on two policy issues related to the EITC. For policymakers interested in reforming the EITC to pay out benefits in advance of the tax filing season, I provide evidence about how likely those forecasts are to be wrong. For policymakers interested in encouraging eligible non-filers to claim the EITC or interested in pre-filling their tax returns, I provide evidence about how well within-year earnings can be used to identify likely eligible households and predict their year-end EITC amounts.

Second, I provide evidence about the EITC's effect on intensive margin labor supply. The negative marginal tax rate created by the EITC's phase-in is supposed to increase households hours choice via the substitution effect, and minimize negative labor supply distortions driven by the program's income effect. If the program does not have the intensive margin effect, then the consequence of the program's structure – that the lowest-income households receive limited to no assistance – is less justified. This paper provides evidence that the EITC's phase-in does increase labor supply, as intended. Whether this pro-work effect warrants limiting redistribution to the lowest income households, and whether a basic credit could be efficiently incorporated into current policy, remains an open question. Finally, I find limited evidence that households facing steeply positive marginal tax rates reduce their labor supply. This finding suggests that concerns about significant work disincentives created by some means-tested programs for a small subset of workers may be overstated.

1.8 Tables

Table 1.1: Demographic characteristics for household head in SIPP sample

	Full SIPP Sample	Wages>0	Restricted Sample
	mean	mean	mean
Age	45.9	41.5	39.6
Female	0.52	0.49	0.41
Non-white	0.19	0.19	0.21
College grad	0.33	0.36	0.34
Married	0.45	0.49	0.49
Have kids	0.63	0.72	0.52
Annual wages (2017 $)	28,432	36,506	43,827
Observations	274,899	214,287	134,594

Notes. Table 1.1 summarizes the average value for select characteristics of the primary filer in each SNAP household/tax unit pooled over the three tax years in our sample, 2015-2017. Wages are reported in 2017 dollars.

Table 1.2: Demographic characteristics for primary taxpayer in SNAP sample

	SNAP + Tax Filer	Wages>0	Restricted Sample
	mean	mean	mean
Age	35.5	34.5	36.1
Female	0.57	.58	0.67
Non-white	0.78	.79	0.79
Married	0.19	.20	0.14
Have children	0.53	0.59	0.79
Annual wages ($)	11,700	22,358	21,223
Observations	2,102,483	1,227,227	106,636

Notes. Table 1.2 summarizes the average value for select characteristics of the primary filer in each SNAP household/tax unit pooled over the three tax years in my sample, 2015-2017. Wages are reported in 2017 dollars.

Table 1.3: Share of households whose predicted income, average and marginal tax rates, and EITC amounts as of each quarter differ from their year-end actual values by more than identified ranges

	$\lvert\hat{z}-z\rvert > \$5k$	$\lvert\hat{\tau}-\tau\rvert > 10pp$	$\lvert\widehat{\text{EITC}}-\text{EITC}\rvert > \$1k$	$\lvert\widehat{\text{MTR}}-\text{MTR}\rvert > 10pp$
		SIPP		
March	.28	.06	.13	.15
June	.18	.03	.03	.11
September	.06	.01	.01	.06
$\bar{\sigma}$	$2,321	1.4	$93	2.5
		SNAP		
March	.36	.25	.13	.32
June	.21	.15	.08	.22
September	.05	.06	.03	.12
$\bar{\sigma}$	$2,651	4.1	$240	7.1

Notes. Table 1.3 summarizes the share of households in each sample, as of the end of each quarter, whose: (1) predicted annual income z is more than $10,000 from their year-end income, (2) predicted annual tax rate τ is more than 10 percentage points from their actual year-end average tax rate, (3) predicted total EITC refund is more than $1,000 from their year-end amount, and (4) predicted marginal tax rate is more than 10 pp from their year-end marginal tax rate. I limit to households who have positive earnings and no more than $75,000 in annual income through each listed quarter. The final row reports the mean standard deviation for each predicted value over all tax units.

Table 1.4: Share of households whose predicted income, average and marginal tax rates, and EITC amounts as of each quarter differ from their year-end actual values by more than identified ranges

	$\lvert\hat{z} - z\rvert > \$5k$	$\lvert\hat{\tau} - \tau\rvert > 10pp$	$\lvert\hat{\text{EITC}} - \text{EITC}\rvert > \$1k$	$\lvert\hat{\text{MTR}} - \text{MTR}\rvert > 10pp$
		SIPP		
Year over year	.46	.13	.08	.23
Min to max	.59	.19	.11	.31
$\bar{\sigma}$	\$6,298	5.70	\$210	5.35
		SNAP		
Year over year	.46	.41	.27	.44
Min to max	.53	.46	.31	.50
$\bar{\sigma}$	\$4,818	9.57	\$620	12.6

Notes. Table 1.4 summarizes the share of households whose earnings, average and marginal tax rates, and EITC amounts in one year are particular values different than in other years. In Row 1, I report the share of households whose (1) maximum annual income z is more than \$10,000 from their minimum year-end income, (2) maximum annual tax rate τ is more than 5 percentage points from their minimum year-end average tax rate, (3) maximum total EITC refund is more than \$1,000 from their lowest EITC amount, and (4) maximum marginal tax rate is more than 10 pp from their lowest marginal tax rate. In Row 2, I report the same shares but compare differences between subsequent years, meaning I count households that appear in multiple tax years more than once in the denominator. I limit to households who have at least positive earnings and no more than \$75,000 in annual income through each listed quarter. The final row reports the average standard deviation of each variable among households who appear for at least two tax years.

Table 1.5: Log earnings response in Q4 to predicted net of tax earnings, given Q1-Q3 earnings, SIPP sample

	(1)	(2)	(3)	(4)
	$\ln y$	$\ln y$	$\ln y$	$\ln y$
ω				
	-1.959***	-2.008***	0.023	-0.074
	(0.037)	(0.040)	(0.033)	(0.128)
z				
			0.950***	0.367***
			(0.008)	(0.044)
Observations	18,309	18,289	18,289	10,614
Households	12,321	12,312	12,312	4,649
Demographics		X	X	X
# of deps $\times$ marital status		X	X	X
State $\times$ year		X	X	X
Household FE				X
R^2	0.24	0.38	0.72	0.88

Notes. Table 1.5 summarizes estimates of Equation 1.2 in the SIPP sample. I limit to sequences that coincide with the actual tax year. Standard errors are clustered at the household-level, and I use household-level weights. The final row reports the p-value from the F-test that estimates of π_q are equal to each other.

Table 1.6: Log earnings response in Q4 to predicted net of tax earnings, given Q1-Q3 earnings, SNAP sample

	(1)	(2)	(3)	(4)
	$\ln y$	$\ln y$	$\ln y$	$\ln y$
ω				
	-1.368***	-1.491***	0.019	0.073**
	(0.007)	(0.007)	(0.012)	(0.022)
z				
			0.821***	0.432***
			(0.006)	(0.013)
Observations	175,953	175,951	175,951	119,804
Households	106,638	106,637	106,637	50,490
Demographics		X	X	X
# of deps × marital status		X	X	X
State × year		X	X	X
Household FE				X
R^2	0.22	0.35	0.45	0.76

Notes. Table 1.6 summarizes estimates of Equation 1.2 in the SNAP sample. I limit to sequences that coincide with the actual tax year. Standard errors are clustered at the household-level.

Table 1.7: Log earnings response in each quarter to predicted net of tax earnings, simulated using three previous quarters' earnings, SIPP sample

	(1)	(2)	(3)	(4)
	$\ln y$	$\ln y$	$\ln y$	$\ln y$
ω				
	-1.867***	-1.908***	0.027	-0.070
	(0.029)	(0.030)	(0.017)	(0.047)
$\omega\times$				
Q2				
	0.019***	0.019***	0.009	0.035***
	(0.005)	(0.005)	(0.005)	(0.004)
Q3				
	0.028***	0.027***	0.006	0.057***
	(0.006)	(0.006)	(0.007)	(0.006)
Q4				
	0.043***	0.043***	0.006	0.081***
	(0.006)	(0.006)	(0.007)	(0.006)
z				
			0.883***	0.073***
			(0.005)	(0.018)
Observations	73,239	73,239	73,239	73,239
Households	12,321	12,321	12,321	12,321
Demographics		X	X	X
# of deps × marital status		X	X	X
State × year		X	X	X
Household FE				X
R^2	0.24	0.38	0.70	0.81
P-value from F-test	0.00	0.00	0.79	0.00

Notes. Table 1.7 summarizes estimates of Equation 1.3 in the SIPP sample. Standard errors are clustered at the household-level, and I use household-level weights. The final row reports the p-value from the F-test that estimates of π_q are equal to each other.

Table 1.8: Log earnings response in each quarter to predicted net of tax earnings rate in that quarter, simulated using three previous quarters' earnings, SNAP sample

	(1)	(2)	(3)	(4)
	$\ln y$	$\ln y$	$\ln y$	$\ln y$
ω				
	-1.165*	-1.368	-0.001	-0.051***
	(0.004)	(0.004)	(0.007)	(0.009)
$\omega\times$				
Q2				
	0.039	0.034***	0.015***	0.105***
	(0.002)	(0.002)	(0.002)	(0.002)
Q3				
	0.034	0.024***	-0.008***	0.150***
	(0.002)	(0.002)	(0.002)	(0.002)
Q4				
	0.038	0.027***	-0.015***	0.180***
	(0.002)	(0.002)	(0.003)	(0.002)
z				
			0.667***	0.040***
			(.004)	(.006)
Observations	703,792	703,792	703,792	703,791
Households	106,636	106,636	106,636	106,635
Demographics		X	X	X
# of deps × marital status		X	X	X
State × year		X	X	X
Household FE				X
R^2	0.22	0.37	0.46	0.65
P-value from F-test	0.00	0.00	0.00	0.00

Notes. Table 1.8 summarizes results from estimations of Equation 1.3 in the SNAP sample. Standard errors are clustered at the household-level. The final row reports the p-value from the F-test that estimates of π_q are equal to each other.

Table 1.9: Log earnings response in each quarter to predicted net of tax earnings rate in final quarter, SIPP sample

	(1)	(2)	(3)	(4)
	$\ln y$	$\ln y$	$\ln y$	$\ln y$
$\hat{\omega}$				
	-4.032***	-6.236***	-1.525***	-0.074
	(0.041)	(0.045)	(0.057)	(0.041)
$\hat{\omega}$				
Q2				
	0.003	0.017**	0.019***	0.024***
	(0.005)	(0.006)	(0.004)	(0.003)
Q3				
	0.016**	0.032***	0.014**	0.037***
	(0.006)	(0.007)	(0.005)	(0.004)
Q4				
	0.003	0.057***	0.024***	0.055***
	(0.006)	(0.007)	(0.005)	(0.004)
z				
			0.660***	0.126***
			(0.008)	(0.010)
Observations	102,191	102,168	102,052	99,458
Households	17,840	17,826	17,826	15,238
Demographics		X	X	X
# of deps × marital status		X	X	X
State × year		X	X	X
Household FE				X
R^2	0.31	0.55	0.70	0.82
P-value from F-test	0.00	0.00	0.03	0.00

Notes. Table 1.9 summarizes estimates of Equation 1.7 in the SIPP sample. Standard errors are clustered at the household-level, and I use household-level weights. The final row reports the p-value from the F-test that estimates of π_q are equal to each other.

Table 1.10: Log earnings response in following quarter to predicted year-end net of tax earnings rate, SNAP sample

	(1)	(2)	(3)	(4)
	$\ln y$	$\ln y$	$\ln y$	$\ln y$
$\hat{\omega}$				
	-2.312	-2.941	-1.639***	-0.021*
	(0.007)	(0.008)	(0.009)	(0.010)
$\hat{\omega}\times$				
Q2				
	0.138	0.152	0.101***	0.122***
	(0.002)	(0.002)	(0.002)	(0.002)
Q3				
	0.198	0.220	0.109***	0.161***
	(0.002)	(0.002)	(0.002)	(0.002)
Q4				
	0.225	0.251	0.112***	0.179***
	(0.002)	(0.002)	(0.002)	(0.002)
z				
			0.352***	0.072***
			(.003)	(.002)
Observations	847,139	847,102	703,792	833,627
Households	135,871	135,856	135,856	125,926
Demographics		X	X	X
# of deps $\times$ marital status		X	X	X
State $\times$ year		X	X	X
Household FE				X
R^2	0.18	0.35	0.41	0.61
P-value from F-test	0.00	0.00	0.00	0.00

Notes. Table 1.10 summarizes results from estimations of Equation 1.3 in the SNAP sample. Standard errors are clustered at the household-level. The final row reports the p-value from the F-test that estimates of π_q are equal to each other.

Table 1.11: Log earnings response in each quarter to predicted federal and state marginal income tax rate, SIPP sample

	(1)	(2)	(3)	(4)
	$\ln y$	$\ln y$	$\ln y$	$\ln y$
MTR				
	0.016***	0.016***	-0.001***	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)
MTR ×				
Q2				
	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Q3				
	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Q4				
	0.002***	0.002***	0.002***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
z				
			0.885***	0.089***
			(0.005)	(0.017)
Observations	73,239	73,239	73,239	73,239
Households	12,321	12,321	12,321	12,321
Demographics		X	X	X
# of deps × marital status		X	X	X
State × year		X	X	X
Household FE				X
R^2	0.19	0.33	0.70	0.81
P-value from F-test	0.00	0.00	0.00	0.00

Notes. Table 1.11 summarizes estimates from a version of Equation 1.3 in which I replace household's ω_q with the households' predicted marginal income tax rate in that quarter. Standard errors are clustered at the household-level, and I use household-level sampling weights. The final row reports the p-value from the F-test that estimates of π_q are equal to each other.

Table 1.12: Log earnings response in each quarter to predicted federal and state marginal income tax rate, SNAP sample

	(1)	(2)	(3)	(4)
	$\ln y$	$\ln y$	$\ln y$	$\ln y$
MTR				
	0.867***	0.891***	-0.276***	-0.004
	(0.006)	(0.006)	(0.007)	(0.008)
MTR				
Q2				
	0.273***	0.274***	0.280***	0.012*
	(0.007)	(0.007)	(0.007)	(0.005)
Q3				
	0.450***	0.444***	0.407***	-0.016*
	(0.007)	(0.008)	(0.007)	(0.007)
Q4				
	0.521***	0.505***	0.425***	-0.020***
	(0.008)	(0.007)	(0.007)	(0.007)
z				
			0.689***	0.098**
			(0.003)	(0.007)
Observations	703,815	703,815	703,815	703,815
Households	106,640	106,640	106,640	106,639
Demographics		X	X	X
# of deps × marital status		X	X	X
State × year		X	X	X
Household FE				X
R^2	0.21	0.32	0.43	0.63
P-value from F-test	0.00	0.00	0.00	0.00

Notes. Table 1.12 summarizes estimates from a version of Equation 1.3 in which I replace household's ω_q with the households' predicted marginal income tax rate in that quarter. Standard errors are clustered at the household-level. The final row reports the p-value from the F-test that estimates of π_q are equal to each other.

Table 1.13: Log hours response in each quarter to predicted net of tax earnings, simulated using three previous quarters' earnings, SIPP sample

	(1)	(2)	(3)	(4)
	$\ln h$	$\ln h$	$\ln h$	$\ln h$
ω				
	-0.626***	-0.597***	-0.078**	-0.001
	(0.024)	(0.024)	(0.027)	(0.030)
$\omega\times$				
Q2				
	0.010*	0.010*	0.008	0.011**
	(0.004)	(0.004)	(0.004)	(0.004)
Q3				
	0.011	0.011*	0.006	0.015**
	(0.006)	(0.006)	(0.006)	(0.005)
Q4				
	0.005	0.006	-0.004	0.013*
	(0.005)	(0.005)	(0.005)	(0.005)
z				
			0.238***	0.046**
			(0.008)	(0.014)
Observations	70,876	70,876	70,876	70,817
Households	12,224	12,224	12,224	12,165
Demographics		X	X	X
# of deps $\times$ marital status		X	X	X
State $\times$ year		X	X	X
Household FE				X
R^2	0.05	0.25	0.30	0.70
P-value from F-test	0.39	0.52	0.06	0.58

Notes. Table 1.13 summarizes estimates of Equation 1.3 in the SIPP sample, in which I replace the outcome variable with the log of the average hours worked per week, summed over the respective quarter. Standard errors are clustered at the household-level, and I use household-level weights.The final row reports the p-value from the F-test that estimates of π_q are equal to each other.

Table 1.14: Earnings response in each quarter to predicted net of tax earnings rate, simulated using three previous quarters' earnings, by household type, SIPP sample

	Marital Status		Presence of Children	
	(1) Single	(2) Married	(3) No Kids	(4) Have Kids
ω				
	-0.077	-0.044	0.101	-0.052
	(0.068)	(0.061)	(0.159)	(0.048)
$\omega \times$				
Q2				
	0.020**	0.054***	0.026***	0.044***
	(0.006)	(0.006)	(0.006)	(0.006)
Q3				
	0.035***	0.086***	0.049***	0.067***
	(0.008)	(0.008)	(0.007)	(0.009)
Q4				
	0.062***	0.105***	0.073***	0.091***
	(0.008)	(0.009)	(0.008)	(0.009)
z				
	0.046	0.095***	0.049	0.080***
	(0.028)	(0.021)	(0.030)	(0.022)
Observations	37,539	35,084	35,751	37,488
Households	6,287	5,925	6,001	6,420
Demographics	X	X	X	X
# of deps $\times$ marital status	X	X	X	X
State $\times$ year	X	X	X	X
Household FE	X	X	X	X
R^2	0.84	0.76	0.82	0.81
P-value from F-test	0.00	0.00	0.00	0.00

Notes. Table 1.14 summarizes results from estimations of Equation 1.3 in the SIPP sample. Standard errors are clustered at the household-level, and I use household-level weights. The final row reports the p-value from the F-test that estimates of π_q are equal to each other.

Table 1.15: Log earnings response in each quarter to predicted net of tax earnings rate, simulated using three previous quarters' earnings, by household type, SNAP sample

	Marital Status		Presence of Children	
	(1) Single	(2) Married	(3) No Kids	(4) Have Kids
ω				
	-0.058	-0.031	-0.194	-0.039
	(0.064)	(0.052)	(0.050)	(0.042)
$\omega \times$				
Q2				
	0.102***	0.125***	0.157***	0.097***
	(0.006)	(0.004)	(0.006)	(0.002)
Q3				
	0.145***	0.180***	0.205***	0.143***
	(0.007)	(0.005)	(0.007)	(0.002)
Q4				
	0.178***	0.192***	0.229***	0.174***
	(0.007)	(0.005)	(0.007)	(0.002)
z				
	0.041***	0.046***	-0.029*	0.048***
	(0.005)	(0.016)	(0.026)	(0.007)
Observations	602,837	100,692	149,859	553,956
Households	92,599	13,906	26,984	82,103
Demographics	X	X	X	X
# of deps $\times$ marital status	X	X	X	X
State $\times$ year	X	X	X	X
Household FE	X	X	X	X
R^2	0.62	0.64	0.62	0.62
P-val from F-test	0.00	0.00	0.00	0.00

Notes. Table 1.15 summarizes results from estimations of Equation 1.3 among married vs. single households and households with and without children in the SNAP sample. Standard errors are clustered at the household-level. The final row reports the p-value from the F-test that estimates of π_q are equal to each other.

Table 1.16: Earnings response in each quarter to predicted net of tax earnings rate, simulated using three previous quarters' earnings, by various subgroups, SIPP sample

	(1) Hourly workers	(2) Self-employed	(3) SE income	(4) Exclude retail	(5) Seam check
ω					
	-0.067	0.021	1.162	-0.063	0.039
	(0.044)	(0.270)	(2.060)	(0.048)	(0.080)
$\omega\times$					
Q2					
	0.042***	0.132**	0.053	0.035***	0.038***
	(0.005)	(0.048)	(0.398)	(0.005)	(0.009)
Q3					
	0.067***	0.173**	-0.123	0.059***	0.054***
	(0.007)	(0.064)	(0.492)	(0.006)	(0.012)
Q4					
	0.086***	0.261***	0.036	0.084***	0.078***
	(0.007)	(0.061)	(0.508)	(0.006)	(0.012)
z					
	0.039*	-0.366**	-0.872	0.069***	0.103**
	(0.020)	(0.130)	(0.894)	(0.018)	(0.031)
Observations	54,811	832	832	71,571	22,116
Households	9,485	178	178	12,063	3,375
Demographics	X	X	X	X	X
# of deps $\times$ marital status	X	X	X	X	X
State $\times$ year	X	X	X	X	X
Household FE	X	X	X	X	X
R^2	0.81	0.82	0.42	0.81	0.79
P-val from F-test	0.00	0.05	0.88	0.00	0.00

Notes. Table 1.16 summarizes estimates of Equation 1.3 for particular subsets of the SIPP sample. Column 1 reports estimated responses among households in which either the head or spouse report being an hourly worker. Column 2 reports estimates among households in which either the head or spouse report earnings any income from self employment in the tax year. Column 3 reports estimates among the same subset of self-employed workers, but I replace the outcome variable with the log of self-employment earnings, as opposed to all earned income. Column 4 reports estimates from the SIPP sample after I drop all households in in which either the head or spouse report working in the retail industry. Column 5 reports estimates from the SIPP sample after I restrict the sample to households whose survey waves align with the tax year. Standard errors are clustered at the household-level, and I use household-level weights. The final row reports the p-value from the F-test that estimates of π_q are equal to each other.

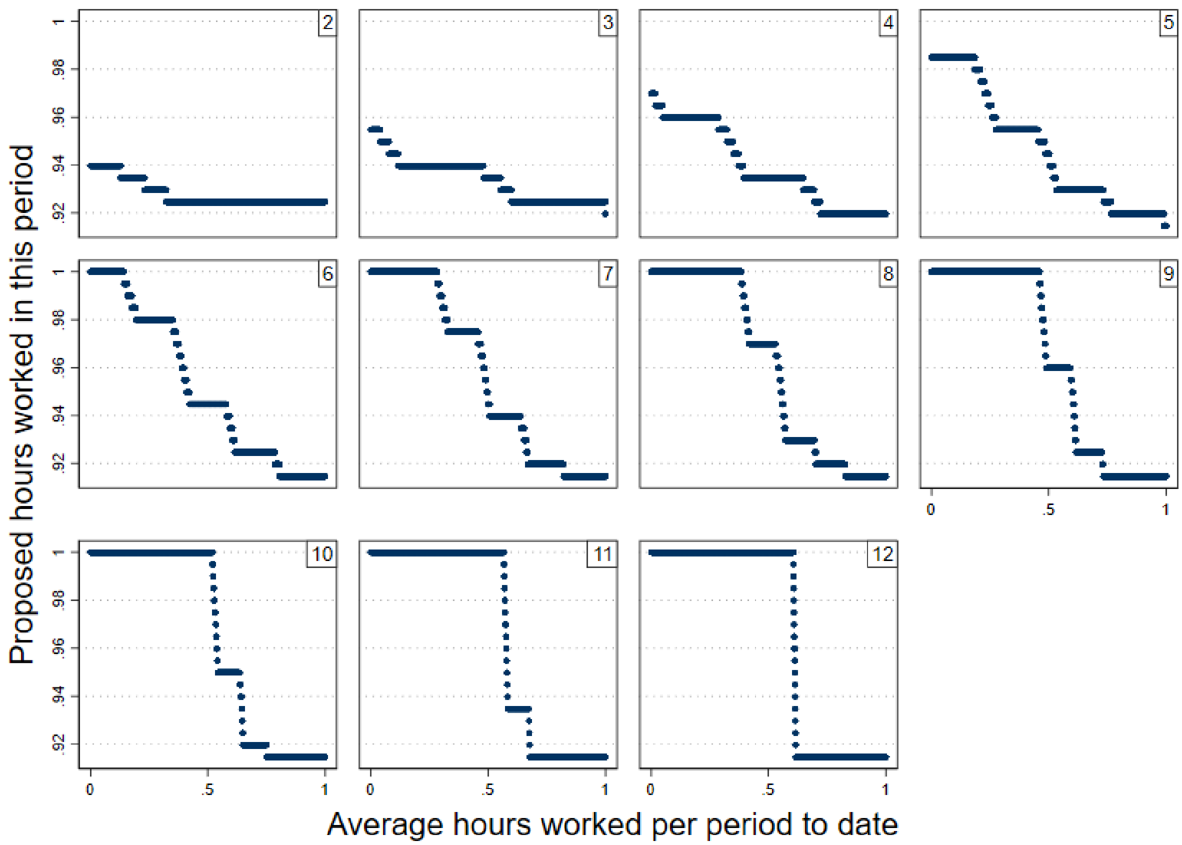
Table 1.17: Anderson-Hsiao estimator for earnings response in each quarter to predicted net of tax earnings

	SIPP	SNAP
$\tilde{\omega}_{q=1}$		
	1.086*	-.381
	(1.466)	(0.111)
$\tilde{\omega}_{q=2}$		
	0.346	0.118***
	(1.436)	(0.126)
$\tilde{\omega}_{q=3}$		
	2.217	0.521***
	(4.193)	(0.113)
$\tilde{\omega}_{q=4}$		
	-2.161	0.126***
	(5.800)	(0.126)
$\tilde{z}$	.977	.630
	(.312)	(.067)
Observations	5,288	75,300
Households	1,322	18,825
Demographics	X	X
# of deps × marital status	X	X
State × year	X	X
Household FE		
R^2	0.22	0.03
P-value from F-test	0.87	0.00
Hansen J-stat	0.00	0.00

Notes. Table 1.17 summarizes results from Equation 1.8. In both models, I cluster standard errors at the household-level. In the SIPP, I apply household weights. The final row reports the p-value from the F-test that estimates of π_q are equal to each other.

1.9 Figures

Figure 1.3: Agent's predicted hours choice based on average hours worked to date

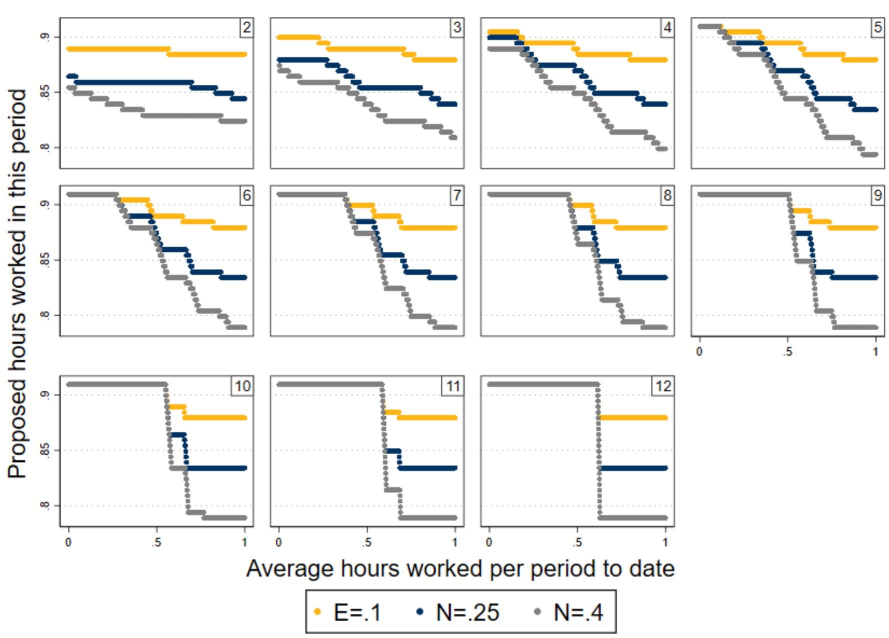


Notes. Figure 1.3 plots proposed hours in each of 11 periods given average hours worked per period to date for a representative agent with ability $n = 1$ and elasticity $e = .25$.

Figure 1.4: Agent's predicted hours choice based on average hours worked to date, by n and e

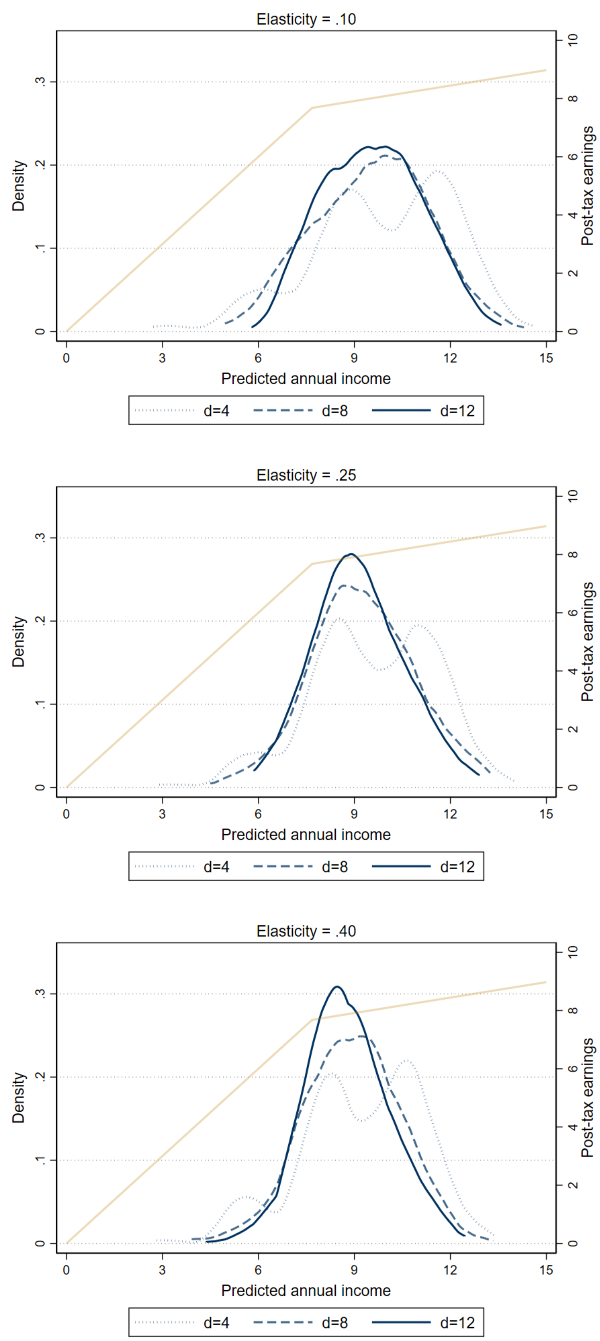
(a) By ability n

(b) By elasticity e



Notes. Figure 1.4 plots proposed hours in the subsequent period given average hours worked per period to date for a set of representative agents. In Panel A, I plot choices for three workers with different ability parameters n. These workers have the same elasticity $e = .3$ and face the same tax policy ($\tau_0 = 0, \tau_1 = .3$). In Panel B, I plot choices for three worker, all with $n = .9$, but with elasticities of .1, .25, and .4.

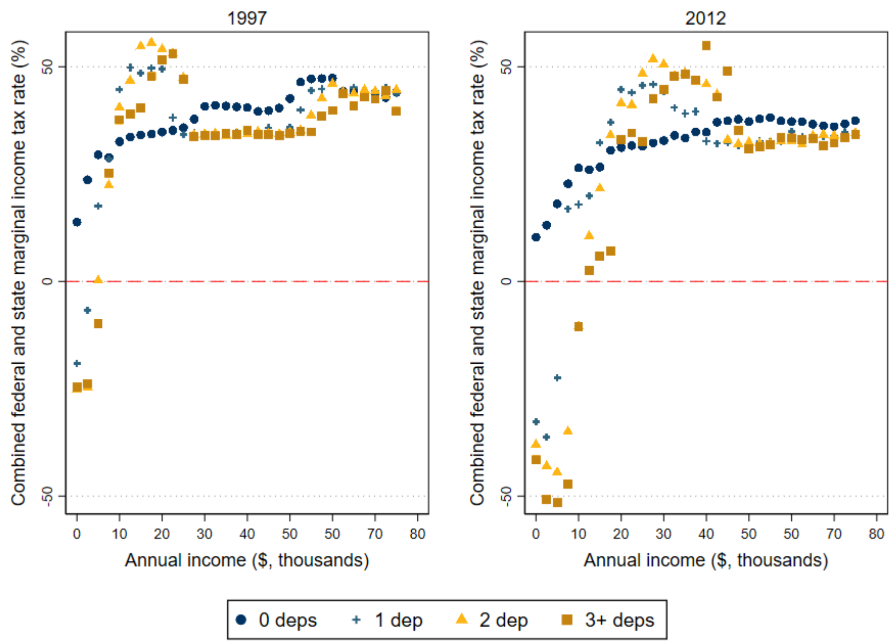
Figure 1.5: Distribution of proposed annual income as of three periods



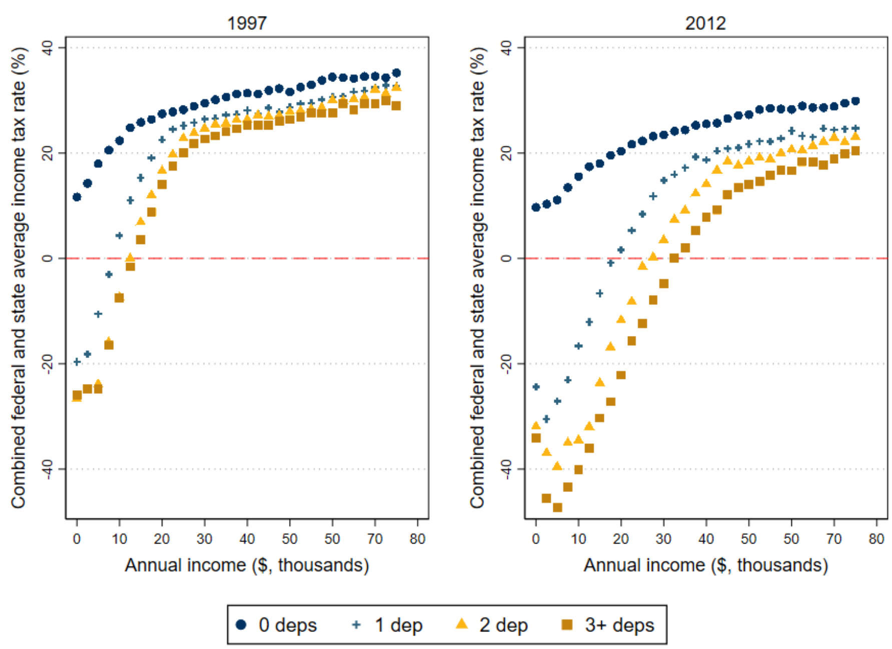
Notes. Figure 1.5 presents results from a three simulations of the model summarized in Section 1.2. I plot the distributions of predicted annual income as of three periods for 500 agents with various levels of n. For each agent, I simulate responses using an elasticity e of .1, .25 and .4. The other parameter values are: $p = .8, \tau_0 = 0, \tau_1 = .3$, and $z^* = 7.6$. The light dotted line indicates predicted earnings as of $d = 4$, the dashed line is as of $d = 8$, and the solid line is the last period, $d = 12$.

Figure 1.6: Combined marginal and average tax rates for married households, 1997 vs 2012, SIPP sample

(a) Marginal income tax rate



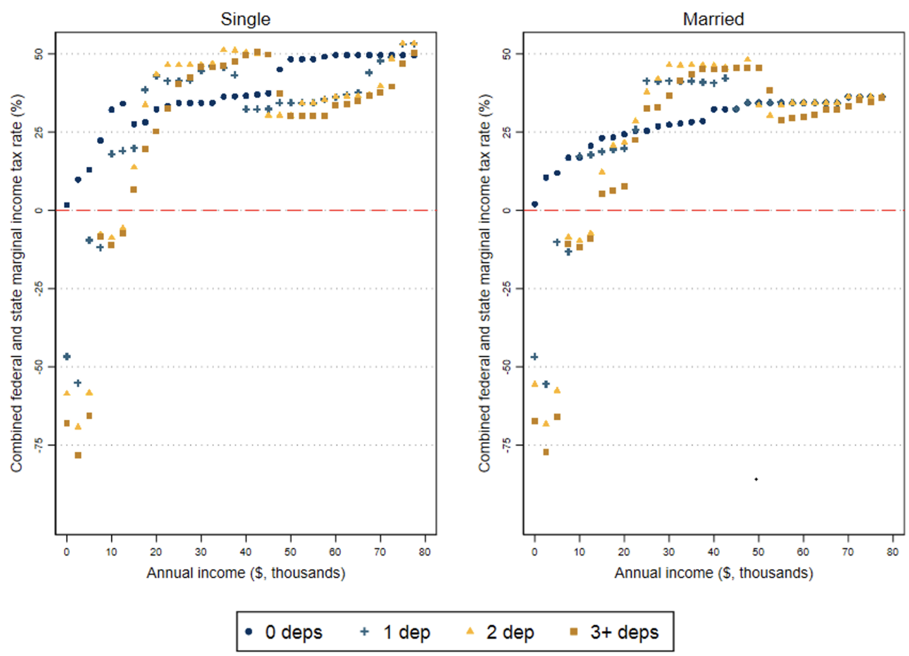
(b) Average income tax rate



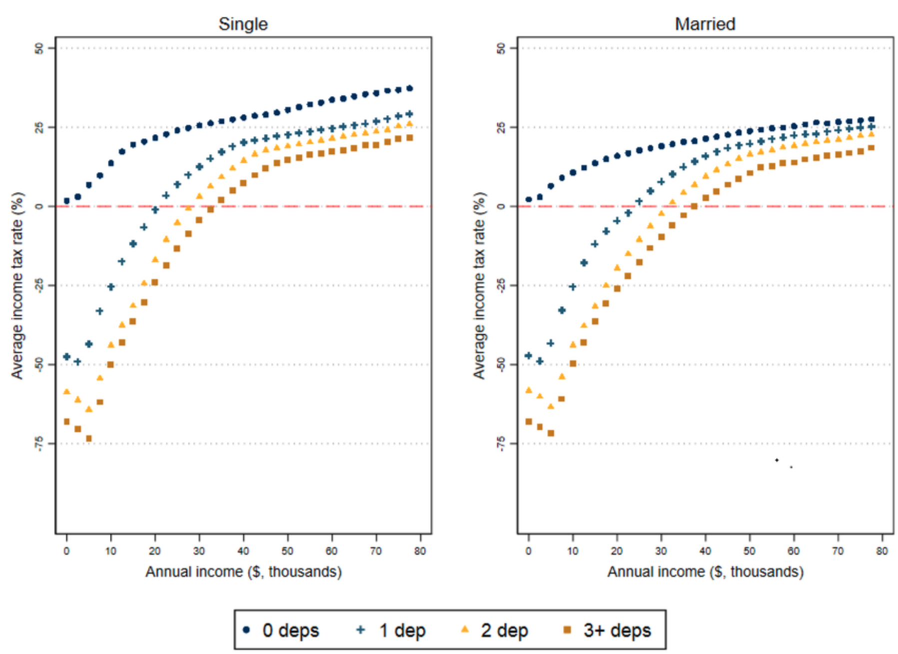
Notes. Figure 1.6 illustrates how marginal and average tax rates vary for married households with 0, 1, 2 or 3+ dependents in 1997 versus 2012. By year and for each number of dependents, I group households into annual income bins of $2,500. In each bin, I calculate the average and average marginal tax rate faced by households at year's end.

Figure 1.7: Combined marginal and average tax rates for married households, by number of dependents and annual income, SNAP households, 2017

(a) Marginal income tax rate



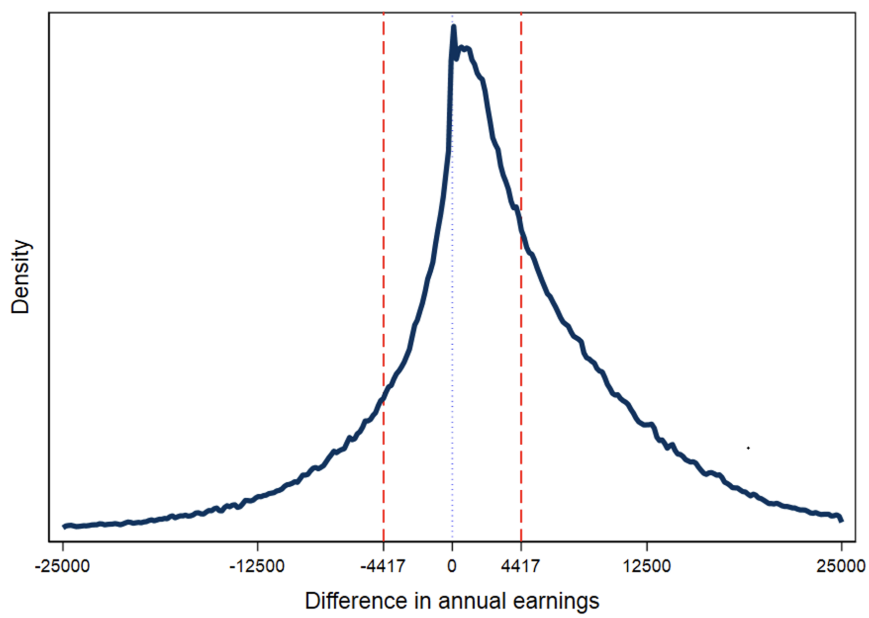
(b) Average income tax rate



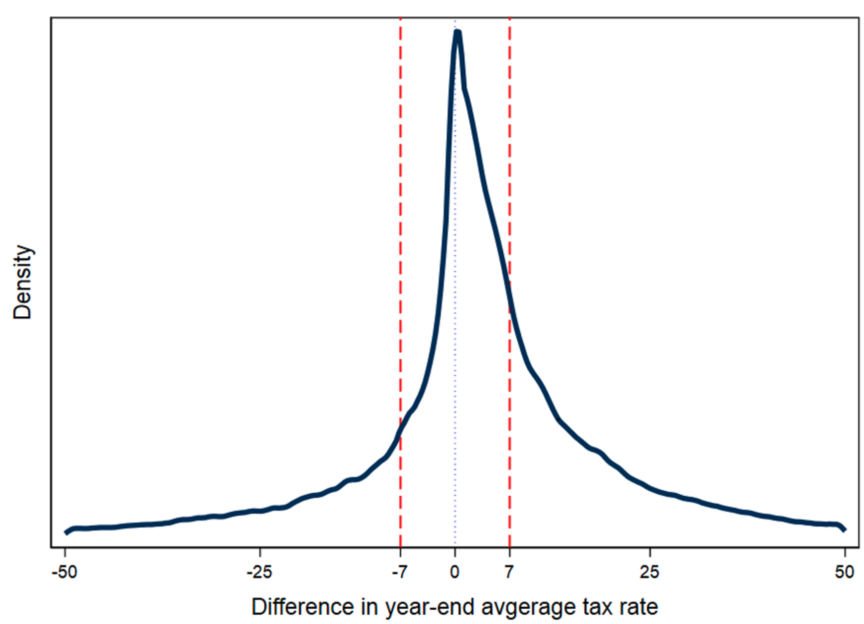
Notes. Figure 1.7 summarizes how average and marginal tax rates vary by household income and number of dependents for single and married filers enrolled in SNAP in California in 2016. I group tax units into bins of $2,000 in annual income by filing status and number of dependents, and within each bin, identify the mean marginal and average tax rate.

Figure 1.8: Distribution of cross-year differences in household income, average tax rate, and predicted ω, SNAP sample

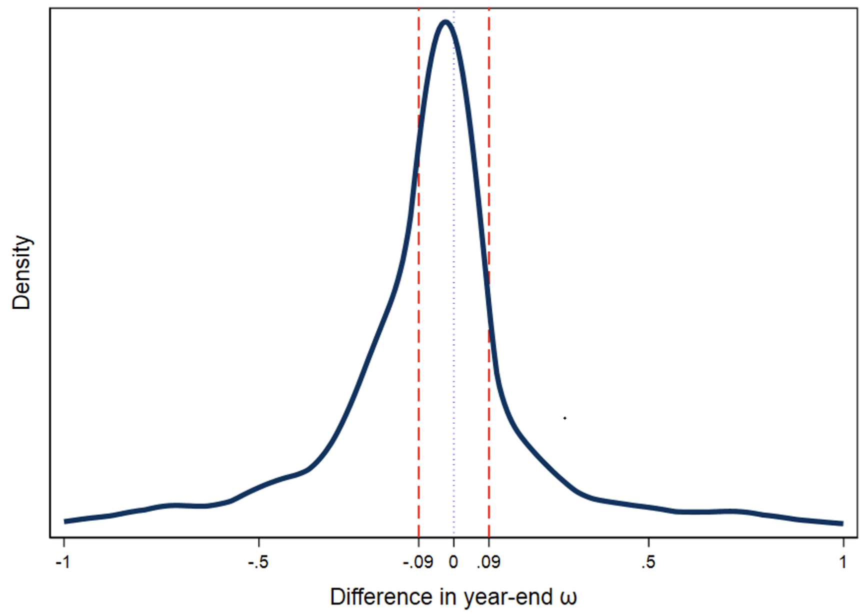
(a) Annual earnings



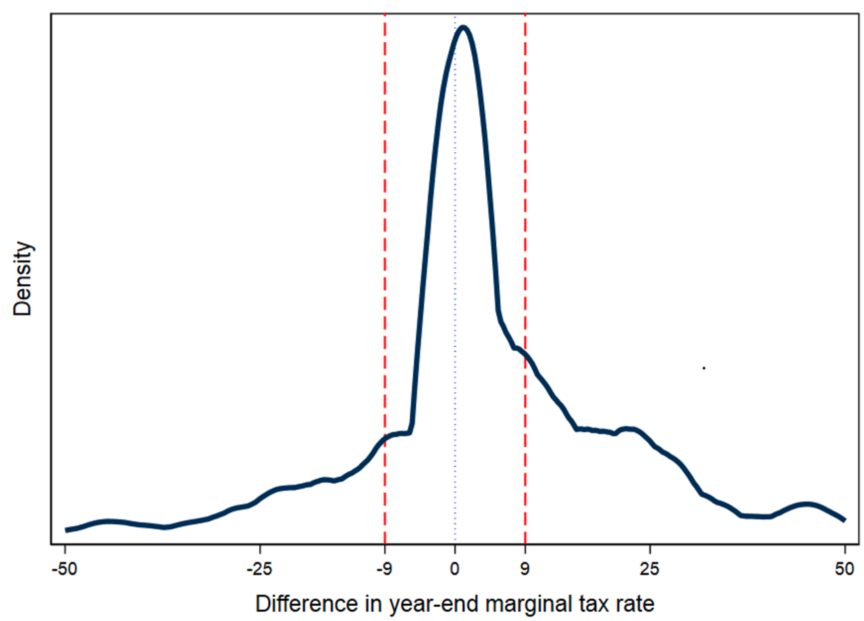
(b) Average tax rate



(c) Predicted final quarter net of tax wage rate

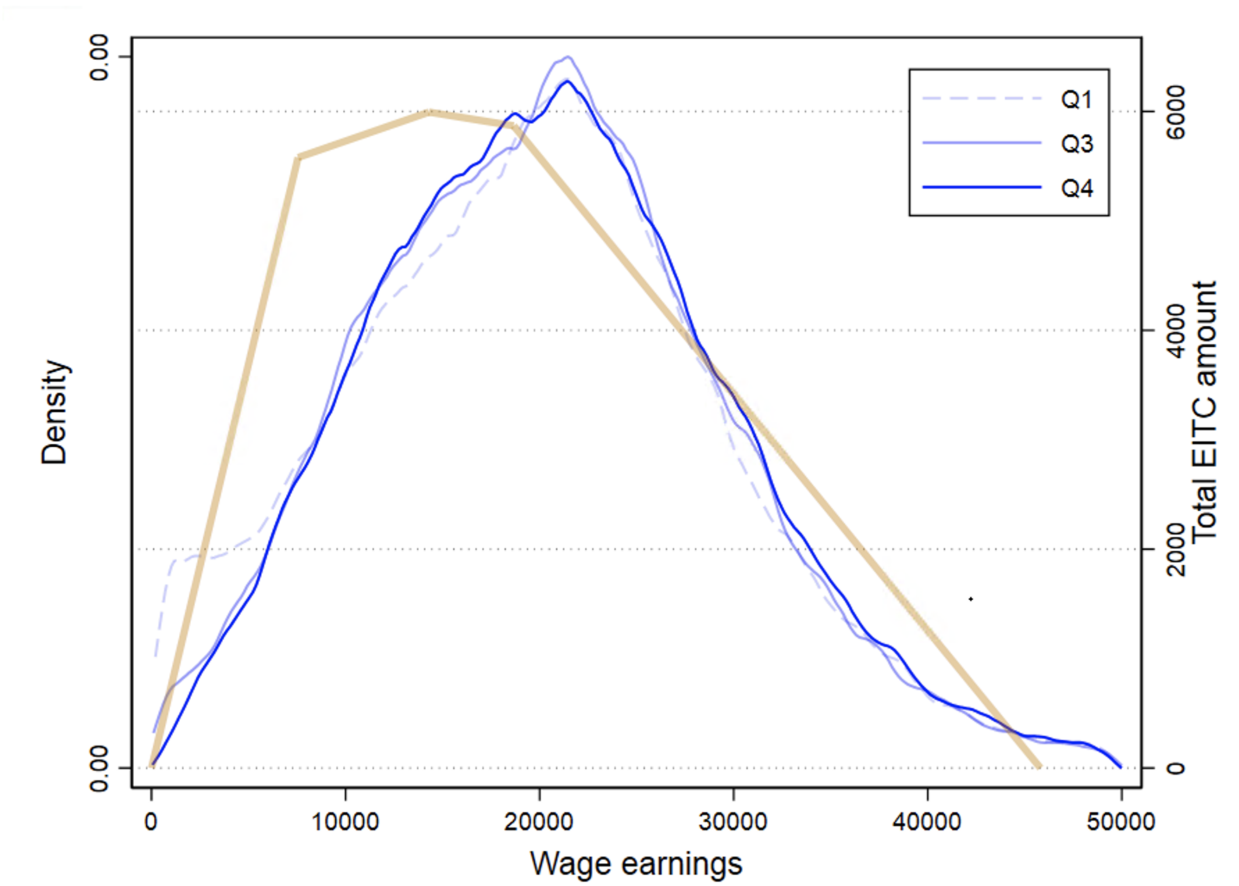


(d) Marginal tax rate



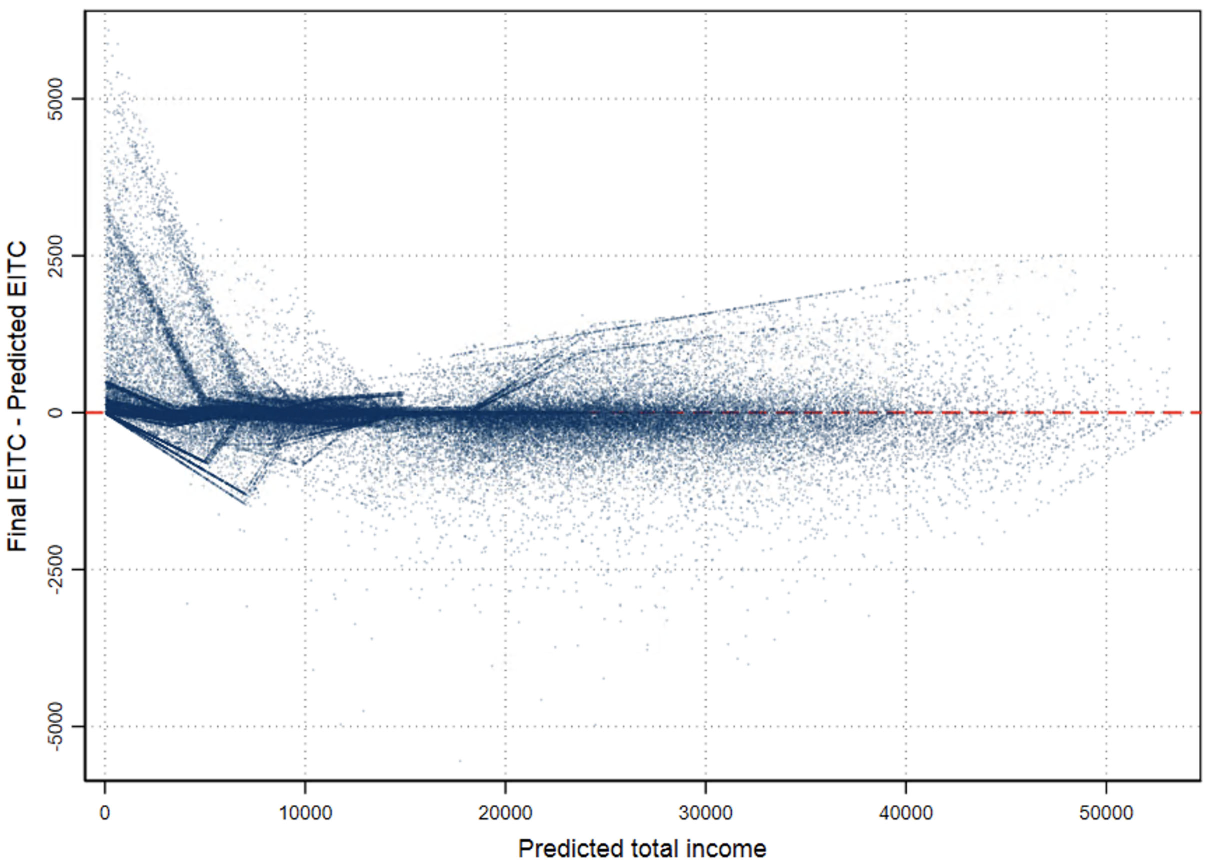
Notes. Figure 1.8 illustrates the differences in household income, average tax rate and ω across tax years within the same SNAP households. For all households, I subtract the value from the value from the previous tax year. I plot the kernel density of all these differences. For household income, I use a bandwidth of $100 and limit to differences within $25,000. For average tax rate, I use a bandwidth of half a percentage point and limit to differences within 50 percentage points. For predicted net of tax wage rate on fourth quarter earnings, I use a bandwidth of 5 percentage points and limit to differences within 75 percentage points. For the marginal tax rate, I use a bandwidth of 2.5 percentage points and limit to differences within 50 percentage points. The red dotted lines indicate the median absolute value difference, meaning half of households exhibit a difference between those bounds and the other half exhibit a difference outside those bounds.

Figure 1.9: Distribution of annual income and predicted annual income as of the end of the first quarter, SNAP households with two dependents, 2017



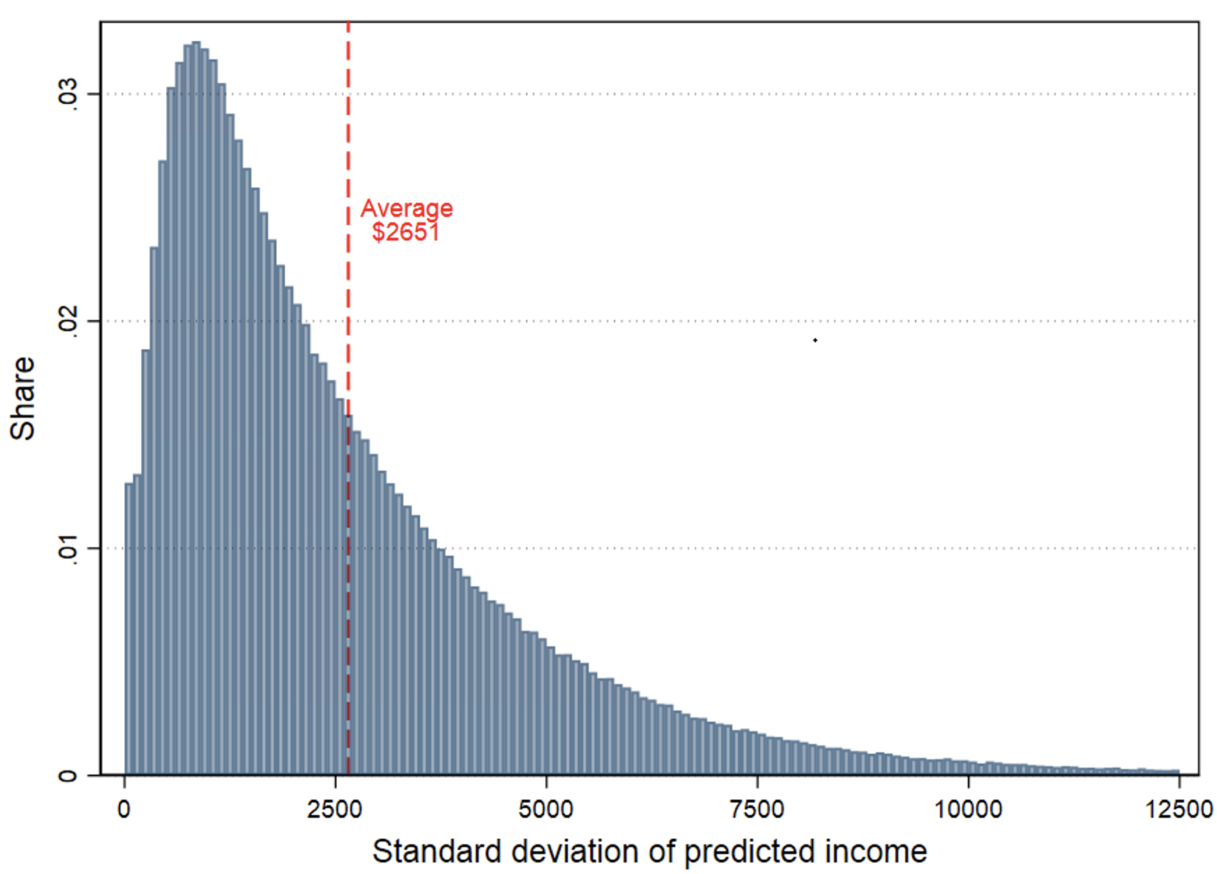
Notes. Figure 1.9 plots kernel density distributions of predicted annual wage earnings as of the end of the first quarter and third quarter, as well as the distribution of actual wage earnings, for SNAP households with two dependents in 2017. I use a bandwidth of $500 and limit to households with a maximum of $50,000. I overlay these distributions on the combined federal and state EITC schedule for a single filer with two dependents in 2017. From the first quarter to the last, the distribution of earnings shifts such that fewer households are located towards the beginning of the phase-in region and more are clustered at the top of the EITC range.

Figure 1.10: Predicted total EITC amounts versus final total EITC amounts, SNAP sample



Notes. Figure 1.10 illustrates how those differences vary by predicted annual income. Each dot represents one SNAP households' predicted and actual EITC amount. I use a five percent sample of SNAP households in tax years 2015 to 2017

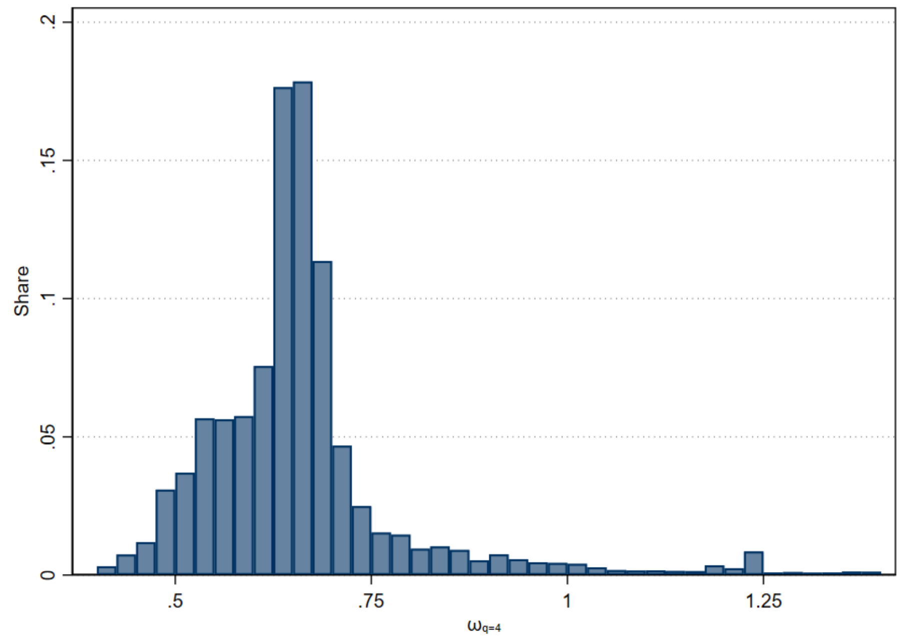
Figure 1.11: Distribution of standard deviations in predicted earnings, SNAP households, 2015-2017



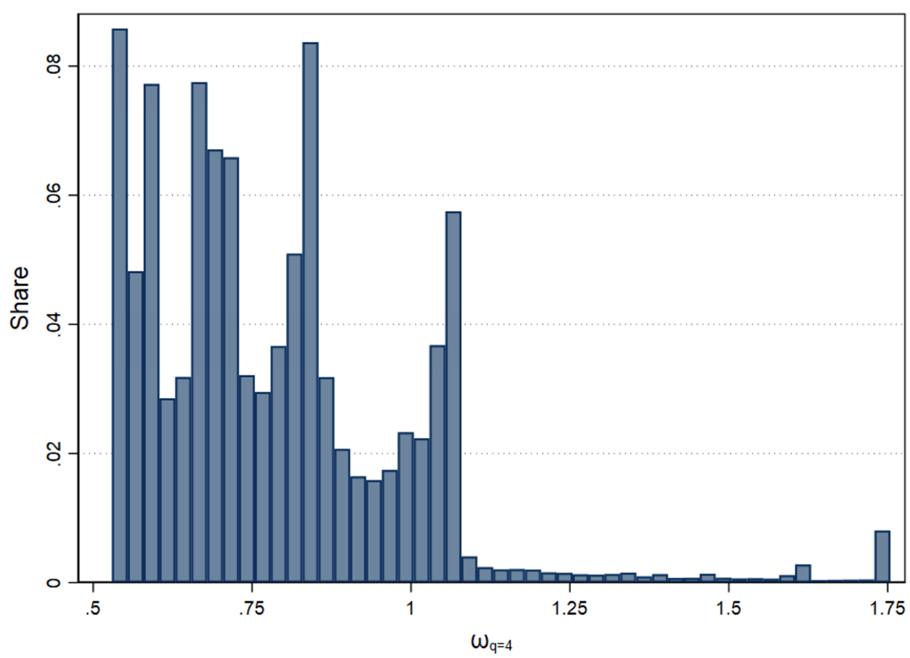
Notes. Figure 1.11 illustrates the distribution of standard deviations across households in their predicted earnings over four quarters of each tax year. I implement the same restrictions as in the rest of my analysis. I predict annual earnings as of each quarter by extrapolating from to-date earnings (i.e., predicted earnings as of June equal double the income earned in the first two quarters). For each household in each tax year, I calculate the standard deviation over the four predicted values, where the final value is equal to the household's actual earnings.

Figure 1.12: Distribution of ω in Q4 in the SIPP and SNAP samples

(a) SIPP sample



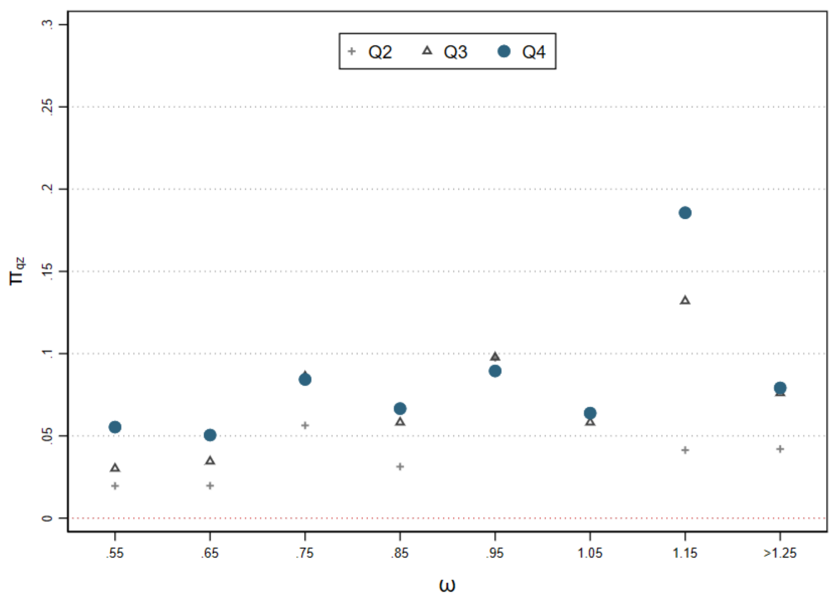
(b) SNAP sample



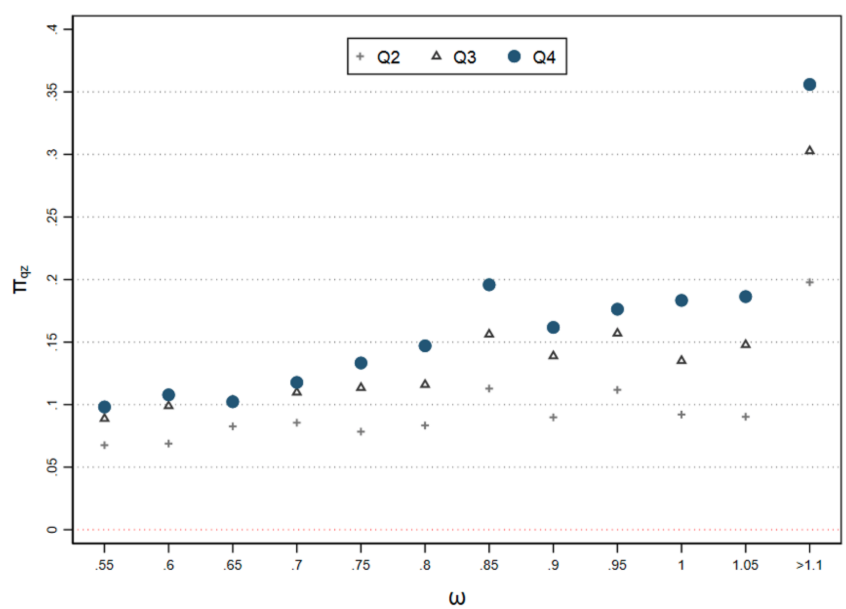
Notes. Figure 1.12 illustrates the distribution of values of ω, the predicted net of tax wage rate for subsequent quarter's earnings, for the fourth quarter in both the SIPP and SNAP sample.

Figure 1.13: Difference in log earnings adjustment between quarters 2, 3, and 4 relative to quarter 1 over predicted net of tax wage rate in subsequent quarter

(a) SIPP sample



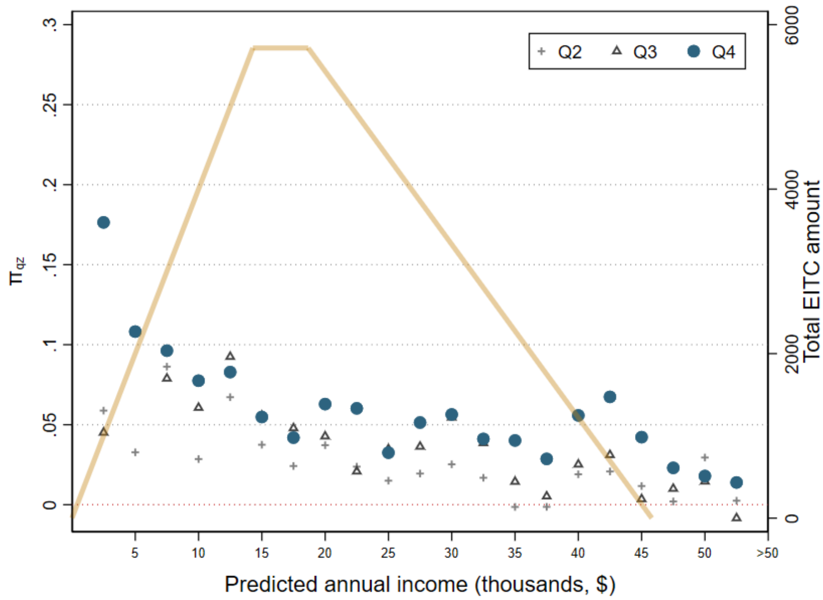
(b) SNAP sample



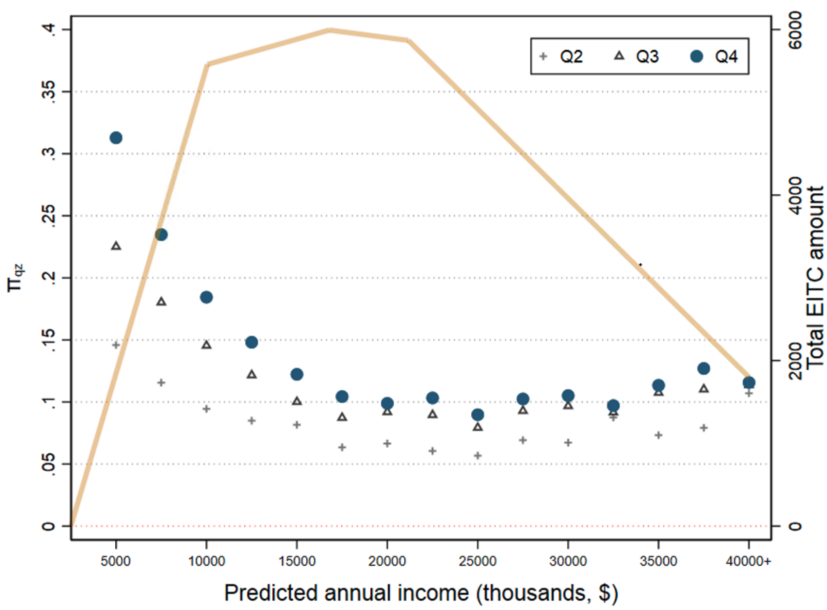
Notes. Figure 1.13 plots estimates of $\pi_{q\omega}$ from Equation 1.4. I bin households into twelve levels of ω: below .55, above 1.1, and in between, groups of .05. I implement the same restrictions described above. I limit to households whose earnings in three previous quarters are between \$2,000 and \$75,000, and with positive earnings in all quarters in a tax year. I also restrict to households whose total EDD wages equal their state AGI. Standard errors are clustered at the household-level.

Figure 1.14: Difference in log earnings adjustment between quarters 2, 3 and 4 relative to quarter 1 over predicted annual income

(a) SIPP sample



(b) SNAP households



Notes. Figure 1.14 plots estimates of π_{qz} from Equation 1.5. The coefficients identify the difference in log earnings by predicted annual income between each calendar quarter relative to the first quarter. I overlay the estimates from the SIPP sample on the EITC schedule for a family with two dependents in 2008. I overlay the estimates among the SNAP households on the combined EITC schedule in California for a household with two dependents in 2017. Standard errors are clustered at the household-level.

Chapter 2

Can Nudges Increase Take-up of the EITC?: Evidence from Multiple Field Experiments

2.1 Introduction

The Earned Income Tax Credit (EITC) is a critical income support for working families. In 2019, 25 million households received about $63 billion nationwide, with an average benefit of approximately $2,500. Numerous studies have documented the EITC's beneficial effects on work, income, and poverty; children's educational achievement and attainment; and adult and infant health (see reviews in Hoynes and Rothstein, 2017; Nichols and Rothstein, 2016). Despite these benefits, the IRS estimates that one in five eligible households do not take up the program (IRS, n.d.*a*). For eligible families with the lowest incomes, take-up may be much lower, with approximately one in two households foregoing their cash benefit (Jones, 2014; Plueger, 2009).

The academic literature in various disciplines has proposed that learning, compliance, and psychological costs can explain incomplete take-up of government benefits (Finkelstein and Notowidigdo, 2019; Herd and Moynihan, 2019; Currie, 2006; Moffitt, 1983). First, to claim the EITC, households must overcome the learning costs associated with discovering the credit exists and determining whether they are eligible. Second, for families aware of the credit, the filing process can be confusing, complex, and burdensome. Previous research suggests that the direct and indirect compliance costs of filing, difficult for the average tax filer to navigate, may be especially burdensome for low-income families (Herd and Moynihan, 2019; Goldin and Liscow, 2018; Bhargava and Manoli, 2015; Currie, 2006). Third, even if learning and compliance costs are overcome, the target population may face psychological barriers that inhibit take-up. Although the EITC is thought to carry less stigma than other benefit programs (Halpern-Meekin et al., 2015), a potential recipient may nevertheless distrust government or face psychological stressors that prevent them from carrying out plans to file a return (Hovland and Weiss, 1951; Pornpitakpan, 2004). Distrust, for example, may be a particular challenge for EITC outreach. Outreach messages often include promises of free cash that can be hard to distinguish from scams to which families are frequently exposed. The relative importance of all these potential explanations for the take-up gap remains largely unexplored.

"Nudges" – small changes to the choice architecture surrounding a decision that aim to alter people's behavior without meaningfully changing incentives – have been used to address many of these barriers, with substantial impacts on enrollment decisions across a wide array of policy contexts (Benartzi et al., 2017; Hallsworth et al., 2017; Thaler and Sunstein, 2009; Thaler and Benartzi, 2004; Madrian and Shea, 2001; Dellavigna and Linos, 2020). For example, nudges have been used to increase take-up of the Supplemental Nutrition Assistance Program (SNAP) (Finkelstein and Notowidigdo, 2019), citizenship applications (Hotard et al., 2019), and even college enrollment through increasing completion of the Free Application for Federal Student Aid (FAFSA) (Bettinger et al., 2012). However, the evidence is not unambiguous; recent studies suggest that nudges may be ineffective in other settings or may fail to scale (Camerer et al., 2018; Bird et al., 2021; Castleman, Patterson and Skimmyhorn, 2019; Bergman, Denning and Manoli, 2019).

Studies on EITC take-up have also yielded mixed results. Bhargava and Manoli (2015) find that IRS notifications to eligible taxpayers increased EITC claiming. Guyton et al. (2017), on the other hand, find positive but much smaller effects of outreach on tax filing rates, with an effect size of about 0.5 percentage points. Kopczuk and Pop-Eleches (2007) find that the availability of tax preparation software increased EITC claiming. Chetty, Friedman and Saez (2013) similarly show that the availability of nearby paid tax preparation services in a neighborhood predicts knowledge about the program and usage. However, Cranor, Kotb and Goldin (2019) find that mandating employers to inform employees about their potential eligibility for the EITC had no effect on EITC take-up.

It is noteworthy that many successful nudge studies focused on populations who already had some interaction with the government. For example, both the Bhargava and Manoli (2015) and Bettinger et al. (2012) interventions were conducted among taxpayers who had already filed a return and only needed to be nudged to complete additional forms covering similar material. Similarly, Meiselman (2018) demonstrates that nudges increase filing of city tax returns amongst those who have already filed federal income taxes. Finkelstein and Notowidigdo (2019) contacted seniors who were already enrolled in Medicaid, but though eligible, failed to enroll in SNAP. It is not clear whether these kinds of nudges would work on those with fewer existing interactions. That is, for those who do not already file or who may have limited positive interactions with government, the learning, compliance, and psychological costs associated with EITC take-up may be much higher.

To contribute to the growing evidence on whether nudges "work" and for whom, and to tease out potential theoretical mechanisms that may explain barriers to take-up, we conducted six large-scale, pre-registered randomized controlled trials in California in 2018 and 2019. These trials were carried out in collaboration with the California tax agency (the Franchise Tax Board, or FTB), state and local agencies that administer SNAP in California (CalFresh), and a large NGO dedicated to statewide EITC outreach (Golden State Opportunity). All of our trials aimed to increase take-up of California's state EITC (CalEITC), introduced in 2015. Because FTB believed that most eligible tax filers already claimed the EITC, and because many families eligible for the CalEITC do not face requirements to file state returns, our efforts focused on reaching families that might not already be filing California tax returns.

The CalEITC supplements the federal credit, as do similar programs in 25 other states (Nichols and Rothstein, 2016). Unlike other states' credits, the California credit does not simply magnify the federal schedule. It is more concentrated at the lowest incomes, where it is worth as much as $3,000 per year. Figure 3.1 shows how the federal EITC and the CalEITC relate to family income for a single parent with two children; schedules vary for other family types but have similar shapes. For families with earnings around $7,000, the combined federal and state credits can be worth as much as $5,500, increasing family resources by close to 80%.

To claim the federal and state EITCs, families must file income tax returns, a potentially complex process. Many families who qualify for the credit are otherwise not required to file returns because their incomes fall below the thresholds that trigger filing requirements. Therefore, to claim the credit for the first time, families might have to engage with a tax system with which they have never interacted. Policymakers in California continue to be concerned that, absent outreach and education, many families will not claim the credits for which they are eligible. Iselin, Mackay and Unrath (2021) find that the take-up rates among eligible CalFresh participants is 50% for the CalEITC and that only about one-third of eligible non-claimants file a state tax return.

Surveys of potentially eligible families demonstrate that many are unaware of the credit and its specific eligibility parameters, validating this concern. One survey found that only two-thirds of families who received food stamps had heard of the federal EITC, and as few as one in three low-income parents who identified as Hispanic had heard of the credit (Phillips, 2001).[1] Much of the evidence on EITC awareness focuses on those low income households that are already filing their taxes. Even among this population, surveys and interviews of filers at low income tax sites and other tax preparation sites find that while most associate filing taxes with a refund (Halpern-Meekin et al., 2015; Smeeding, Phillips and O'Connor, 2000), only about half are aware of the EITC itself (Bhargava and Manoli, 2015). A minority are able to identify the mechanisms as to why they are receiving a refund or the benefit structure itself (Smeeding, Phillips and O'Connor, 2000; Chetty, Friedman and Saez, 2013).

Since it is more difficult to identify and survey non-filers, there is limited evidence about their awareness of, and familiarity with, the EITC. Nevertheless, there is reason to expect that they are less aware of the credit. For example, using a nationally representative sample including both filers and non-filers, Phillips (2001) finds that awareness of the EITC is lower among those with very low incomes, who are unlikely to file tax returns absent the EITC, than among those with somewhat higher incomes. Consistent with this, Blumenthal, Erard and Ho (2005) find that only about 35% of EITC-eligible households who were not required to file tax returns, due to low incomes, in tax year 1988 did so. More recent evidence confirms that substantial majorities of EITC eligible non-claimants did not file tax returns at all (Plueger, 2009; Beecroft, 2012). This suggests that awareness of the EITC is lower among non-filers, but more research is needed both on this question and about how to reduce the burdens that these households face in claiming the EITC. Our study is motivated by the hypothesis that lack of awareness about the credit, the likely benefit amount, and where to seek help play at least some role in explaining incomplete take-up among this population.

[1] While this survey is from 2000, the estimated national EITC participation rate has been remarkably consistent since the 2000s, remaining at roughly 75 to 80 percent (Jones, 2014; IRS, n.d.*a*).

We used six randomized trials, several with multiple treatment arms, to test a range of outreach messages that our partners sent to potentially eligible households. Our messages aimed to inform recipients of their potential eligibility for the federal and state EITCs and to encourage them to file a tax return. They were delivered by postal mail or by text message, and generally resembled the type of outreach efforts that public agencies often make to potential users of their services, though we strove to use best practice in terms of using clear and simple language. Other outreach efforts administered by our partners or other organizations during the same tax seasons, such as billboards, public events or traditional advertisements, were much lower-touch and would have affected our treatment and control groups similarly.

Subjects for our studies were drawn from participant lists for the CalFresh program and from a marketing database that included people with little or no existing interaction with government. None of the studies conditioned on filing taxes, and one conditioned on not having filed taxes in any of the three previous years. Each study was randomized, and we linked treatment and control rosters to FTB tax records. We report estimates for two primary outcomes, measured at the household level: Whether anyone in the household appeared (as filer or spouse) on a California tax return, and whether anyone appeared on a state or federal return including an EITC claim. Results are similar if we examine only CalEITC claiming.

Table 2.1 describes the six studies; the Appendix includes copies of each of the treatments. Each arm in our studies was designed to test a set of hypotheses, drawn from the literature on administrative burdens and ordeals (discussed above) about why people may fail to take up this available benefit. Specifically, the experiments aimed to reduce learning, compliance, and/or psychological costs associated with EITC participation via scalable, low-touch nudges. All studies included a control group that received no message,[2] and all treatment arms provided information about the program and its value. If eligible households did not know about the program or did not know about the potential amounts they were likely to receive, we hypothesized that receiving this information would reduce learning costs and therefore increase take-up.

Treatment arms in Studies 3, 4, and 5 tested the impact of providing additional information about how to obtain help in filing a return, which targeted compliance costs. Specifically, if people knew about the EITC and understood the potential benefits, but the process was too burdensome, we hypothesized that providing individuals with detailed information about how to obtain help with tax preparation would reduce compliance hurdles and increase take-up. We pointed individuals to existing support services, as these represented the most scalable interventions. In Study 3, we directed people to online resources, to text-based assistance, or to a hotline. In Studies 4 and 5, we provided detailed information about a local Volunteer Income Tax Assistance (VITA) site that provides free, in-person tax preparation assistance to low-income households.

To test whether psychological costs associated with source credibility might affect take-up decisions, treatment arms in Studies 2 and 4 delivered the same information in different formats

[2]It is possible that a member of the control group for one study was in the treatment group for another. The randomization design, discussed below, explicitly stratified on other treatment assignments where possible, to ensure a precisely zero correlation between treatment statuses in the different studies. We present analyses of each study separately. Analyses that use the overlapping samples to explore potential interactions among the different treatments show no evidence of any such interactions.

and via different messengers. Since the notifications informed households that they were eligible to receive free money, we were concerned that households might distrust the information or assume it was a scam. We hypothesized that communication from a government agency and information presented in more formal formats would increase source credibility and reduce distrust. In Study 2, we varied the messenger to test whether receiving information from a government agency was more effective than receiving the same information from a non-profit. We made the difference in messenger salient by changing the logos, signatures, and return addresses on the letters. In both Study 2 and Study 4, we also explored source credibility by sending the same information in both formal and informal formats. The formal treatments adopted the design used for federal tax communications and in other EITC nudging experiments to communicate with taxpayers about the EITC (e.g., Bhargava and Manoli, 2015). In the "informal" format, we used styles, images, and colors similar to those used in marketing materials employed by other statewide outreach efforts. In both cases, we used communication designs that could plausibly be scaled by a government agency. Studies 5 and 6 also aimed to enhance source credibility by delivering messages from local county welfare offices with which recipients regularly interacted, using similar wording as the agencies' other outreach messages.

Despite testing a range of interventions designed to leverage many of the behavioral explanations for incomplete take-up, we found that none of our interventions had substantively or statistically meaningful effects on tax filing or EITC claiming. Our messages were received; we find relatively high engagement with websites linked to in the messages. Nevertheless, in each of our trials, we can reject effects as large as the average nudge effect in a recent meta-analysis of nudge studies reviewed by Dellavigna and Linos (2020).

The existing evidence demonstrates that information-focused "nudge" interventions could have been effective. Our designs resemble interventions studied by other researchers that have found significant positive effects on take-up. For example, Bhargava and Manoli (2015) study an intervention that involved the IRS sending a single mailing to tax filers who had not claimed the EITC, and find that better-designed formats lead to higher rates of claiming. Guyton et al. (2017) also study the effect of a single mailed reminder, while Beecroft (2012) studies the impact of a postcard-sized mailer about the EITC sent to social service program participants. Finkelstein and Notowidigdo (2019) study of SNAP participation involved sending one mailing and one follow-up postcard; their "Information Plus Assistance" group included on these documents a phone number for a non-profit organization which subjects could call for enrollment assistance, which resembles the the offers of assistance we test in our studies 3, 4, and 5. We believe that the difference in our results largely reflects the difficulty of the task that people are being nudged to perform. For low-income households who do not file taxes, the hurdle of submitting a tax return may be too big for a simple outreach effort, no matter how well designed or behaviorally informed, to overcome.[3] Getting families over this hurdle evidently requires more than just information and pointers

[3]While several of our treatment arms directed subjects to VITA sites that could assist with tax preparation, these may not have sufficiently lowered the barriers. Halpern-Meekin et al. (2015) report that EITC recipients experienced volunteer-based tax preparation services as untrustworthy and akin to government bureaucracies with "long lines in drab buildings, impersonal or even rude treatment, and the heavy atmosphere of desperate people soliciting aid", in contrast to the "bright," "neat," more professional for-profit tax preparers, who treat claimants "like a valued customer"

to existing assistance.

It is important for the field to grapple with null findings in the same way it grapples with negative and positive findings. Our study improves our understanding of both the promise and limitations of behavioral interventions for low-income populations. It thereby makes a major contribution to the scholarship on behavioral science and the literature on incomplete take-up of means-tested programs; it should also inform potential policy reforms related to outreach efforts and tax administration. While nudges are a potentially valuable part of the policy toolkit, outreach to hard-to-reach populations will often need to include higher-touch interventions that simplify the underlying processes.

2.2 Methods

This paper encompasses six distinct but partially overlapping randomized controlled trials. The studies were implemented in spring 2018 (Studies 1 and 5) and spring 2019 (Studies 2, 3, 4, and 6), and focused on tax filing for the 2017 and 2018 tax years, respectively. United States income tax returns are typically filed between February and April, and are based on income received during the previous calendar year (the "tax year").

Interventions were delivered by public agencies, the California Franchise Tax Board and the California Department of Social Services, and by a non-governmental organization (Golden State Opportunity) that receives funding from the state to conduct EITC outreach. Some features were chosen to meet agency needs rather than those of researchers.[4]

Sampling frames

A major challenge for EITC outreach is that non-claimers are unlikely to appear in tax records, which are used for most tax-related outreach. Our samples of potential non-claimers drew from two sources. Studies 1-4 used a database purchased from a private marketing firm, TargetSmart. Records were purchased for California low income households, first in spring 2018 and then updated in spring 2019. This yielded approximately 1.3 million records.

From the original sample, we removed individuals younger than 18, older than 70, and those apparently living in group residences (identified by more than four records at the same address). Our eventual sample consisted of 1.2 million individuals in 1 million households.

Many of the individuals in studies 1-3 would have filed taxes even without being nudged. For Study 4, we focused on those households that had not filed taxes in the past three years. To do so, in early 2019 we merged the TargetSmart data to FTB tax filing records for tax years 2015-2017. This was a fuzzy merge, based on names, addresses, and dates of birth, with allowance for apparently legitimate differences between records in the two databases (e.g., misspelled names, alternative ways of recording addresses, potential local moves). The universe for Study 4 was limited to a

(p. 83).

[4]Our analysis of the data from these experiments was overseen by the California State Committee for the Protection of Human Subjects (protocols 2019-021, 2019-002, 2018-037, 2018-194).

subset of approximately 200,000 TargetSmart records that did not appear in the FTB filing records in any of the three preceding tax years.

The second original source of potential non-filers, used in Studies 5 and 6, was administrative records of participants in the CalFresh program, the California instantiation of the federal Supplemental Nutrition Assistance Program (SNAP). We began with approximately 6 million individuals enrolled in CalFresh at any point in calendar year 2017 (Study 5) or 2018 (Study 6), grouped into case (household) units. We excluded cases containing only seniors. We then linked adults to their quarterly earnings records for 2017:Q1 through 2017:Q3 for Study 5 and 2018:Q1 through 2018:Q3 for Study 6 from the California Employment Development Department, which administers the state's Unemployment Insurance program.

We used CalFresh case compositions and earnings records to simulate federal and CalEITC eligibility. Our simulations assumed that the CalFresh case corresponded to the potential tax household; that all children in the CalFresh case would qualify as children for purposes of EITC eligibility; that earnings in 2017:Q4 and 2018:Q4, which were not yet available when we administered treatments, would equal one-third of total earnings over the previous three quarters; and that there would be zero self employment earnings or other income not covered by the earnings records. Based on this simulation, we selected cases with EITC eligibility above \$50. This excluded those with zero or very low earnings and those with earnings too high to qualify for the EITC.

Last, we restricted to participating counties: Sacramento and San Diego in Study 5, and those counties plus San Francisco, San Mateo, and Santa Clara in Study 6. Welfare offices in these counties use text messages to communicate with CalFresh participants about their cases. We limited our sample to individuals with valid phone numbers who had consented to receiving these text messages.

The two data sources each have advantages and limitations. The TargetSmart sample provides a broad cross-section of low-income Californians, including those who interact with government infrequently. However, these data have limited information about earned income or family structure and may contain outdated or incorrect records. The CalFresh enrollee contact information is updated regularly, with detailed and reliable information about income and family composition. However, the CalFresh database only contains households already connected with state social assistance programs, meaning they have exhibited an awareness of and are willing to enroll in these forms of assistance.

Randomization

All six studies were implemented using stratified random assignment. Each included a control group that received no treatment and one or more treatment arms that received text messages or letters. Where there were multiple treatment arms, all were assigned with equal probability, though in several cases the control group was larger than any single treatment arm. Randomization was at the household level, defined by the address in the TargetSmart data and by the case number in the CalFresh data. A single representative was selected from each household to receive the treatment.

Appendix Table 2 provides details about sample sizes and randomization strata. Studies 1-4 were implemented sequentially, with assignment in one used as a stratification variable for the next, as discussed below. This ensured that treatments in each study were perfectly orthogonal to those in the other prior studies. Studies 1 and 2 used varying assignment rates across strata. In Study 1, assignment rates varied across counties and zip codes to meet GSO's needs. In Study 2, rates were set to yield 5,000 observations per treatment arm in Riverside county and 5,000 in the other counties combined. Other studies used constant assignment probabilities across strata.

Procedures

Treatments consisted of sending a single letter or a single text message to an individual chosen from each household. There was no other interaction with subjects. Control group members did not receive the letters or texts, though they may have been exposed to other outreach. Examples of each treatment can be found in the Appendix.

Study 1. Text messages were sent manually by GSO volunteers in March and April 2018, with observations sequenced randomly. Texts informed recipients of their potential eligibility and of the need to file a return in order to claim the credit, and included a link with more information to reduce learning costs. The exact wording of the texts varied over the course of the study.

Study 2. There were four treatment arms, delivered as different letters, that addressed learning costs and psychological costs related to potential mistrust of the messenger or message. Letters varied in two dimensions that both addressed source credibility: the source (GSO or FTB) and the formality. Each sender's letters used the relevant logos, signatures and return addresses. In addition, half were structured as formal letters and half as informal flyers. The front of each letter was printed in English; the back contained the same information in Spanish. Letters were mailed in February 2019.

Study 3. There were four treatment arms, each consisting of a single text message. To target learning costs, each message informed recipients about potential eligibility and the need to file taxes to claim the credit. Treatment arms 2 and 3 also targeted compliance costs by offering assistance through a hotline or via text respectively. Treatment arm 4 included additional information on the average benefit amount to further address learning costs. Texts were sent manually between February and April 2019.

Study 4. There were eight treatment arms, delivered as different letters. As noted above, inclusion in this study was conditioned on not having filed in any of the previous three years. Letters came from FTB and contained one of four different messages: a simple message about the credit; a simple message that also included information about the average value of the credit (addressing learning costs); a message that added information about the location, hours, and contact information of the nearest in-person free tax preparation assistance site (addressing compliance costs); and a message that included both the average value of the credit and tax assistance information. Each message was delivered in a formal and an informal version, with the idea that formal letters might signal more source credibility (addressing psychological costs). The front of each letter was printed in English; the back contained the same information in Spanish. In addition, each letter contained a URL at which recipients could find the letter translated into Korean, Vietnamese, Chinese, or Russian. Letters were mailed in February and March 2019.[5]

Study 5. All treated individuals received the same sequence of text messages, designed to address both learning and compliance barriers. The first message included a personalized benefit amount estimated using the recipients' household composition and quarterly earnings data (see the sampling frames subsection for more information). If the recipient texted "1" for more information, they were provided the URL to an online free tax-preparation software. If they responded "1" again, they were provided the address and hours of the closest VITA site to the client's 9 digit zip code. When that site required appointments, the text also included a link for registration. Texts were sent in English or Spanish, depending on the language indicated in the CalFresh record, and were delivered over two days in March 2018.

Study 6. There were three treatment arms, each delivered by text message. The first treatment arm was a simple text, informing recipients of their potential eligibility, and provided a URL to calculate their credit and a hotline to learn where to file for free. The second treatment arm provided the same information as the first text, along with the average benefit amount. The third treatment arm, as in Study 5, included a personalized credit amount. The three treatments did progressively more to address learning costs. Moreover, the fact that they came from the local CalFresh program should have increased source credibility and reduced psychological costs. Texts were delivered in March 2019 in the language indicated in the recipient's CalFresh record: English, Spanish, Chinese, or Vietnamese. Speakers of other languages received the English message.

A design feature of all of our interventions is that they would all be easily scalable. The vast majority of nudge interventions run by governments at scale in the US are outreach efforts of this nature. A recent paper estimates that 30% are physical letters, 20% are postcards, and almost 40% are emails (Dellavigna and Linos, 2020). Like our interventions, these outreach efforts may link to an in-person service, but directly nudging people in person occurs in less than 1% of cases.

In addition to being the nudge most commonly pursued by government, this type of informational intervention is also one studied by many scholars concerned about incomplete take-up of means-tested programs like the EITC. Several recent papers find that simply informing likely eligible individuals about the existence of a program with a single intervention can yield substantial effects on enrollment decisions (e.g., Bhargava and Manoli, 2015; Goldin, Homonoff and Tucker-Ray, 2017; Armour, 2018; Barr and Turner, 2018; Finkelstein and Notowidigdo, 2019).

While our interventions themselves are low-cost, treatment arms in studies 3, 4, and 5 directed subjects to existing high-touch services, such as in-person assistance with tax returns. This is realistically what intensive interventions look like at scale – offers of help rather than forced provision. Finkelstein and Notowidigdo (2019) also study this type of enhanced intervention; their "compliance" treatment arm contained a phone number for a non-profit that would help seniors walk through their SNAP application. VITA sites offer a similar level of personalized assistance.

[5]Seven of the eight treatment arms were mailed on February 15, 2019. Due to a mailroom error, letters for one arm (a formal letter with benefit amount and in-person free tax preparation site information) were not sent until March 25, 2019.

Data and outcomes

This study is made possible by unprecedented access to an array of administrative data from California, including income tax, wage, and social service records. Our use of social service and wage records is discussed above. We measure impacts of our interventions from actual California income tax filings.

For each study, we attempted to match each member of each household to FTB records, using the same fuzzy match discussed above. We measured whether each individual successfully matched to a California tax return and whether the return included a claim for either the federal EITC or CalEITC. Our primary outcome measures are an indicator for the presence of a matched return and an indicator for the presence of any EITC claim.

We analyze the data at the household level. We consider a household to have filed a return and to have taken up the EITC, respectively, if any member appears on a return and if any member's return includes an EITC claim.

We also track website visits in Studies 2, 4, and 6. Distinct URLs were used for each study and treatment arm. In Studies 2 and 4, we count all hits to these URLs; in Study 6, we count unique users. These measures are not available for the control groups.

Table 2.2 presents summary statistics for our main samples. The TargetSmart sample is relatively old, with a mean age of 60, though nearly half are indicated as having children in the house. 55% are white, 58% are female, and 41% filed taxes in the previous year. In the CalFresh data, individuals are younger (mean age 37) and less likely to be white (23%). A larger share have children, and 74% filed taxes in the prior year. In our CalFresh sample, we also have access to earnings records covering three quarters of the tax year, which we use to simulate EITC eligibility. We include in the study only families eligible for a federal or state EITC of at least $50. In our sample, the average family's estimated annual income is just over $14,000, and the average total (federal and state) EITC eligibility is $2,715. Across all observable characteristics to which we have access, our trial arms are balanced.

Statistical analysis

Our primary analyses examine the effect of any treatment versus control. We construct a single observation for each simulated tax filing unit (household), and estimate regressions of the form:

$$Y_{is} = \alpha + T_{is}\beta + \mu_s + \varepsilon_{is} \tag{2.1}$$

Here, Y_{is} is the outcome for household i in stratum s – either the presence of a tax return for some member of the household or the presence of an EITC claim. T_{is} is an indicator for treatment, and μ_s is a vector of stratum fixed effects. The impact of treatment is β. These are reported as the first estimates for each study (in black) in Figure 2.3.

A second set of analyses examine the effect of each treatment arm separately, where relevant. These are similar, but replace the single treatment effect with a series of separate effects:

$$Y_{is} = \alpha + \sum_j T_{isj}\beta_j + \mu_s + \varepsilon_{is} \tag{2.2}$$

Here, T_{isj} an indicator for assignment to treatment arm j, and β_j is the impact of that treatment relative to control. These are reported as the second and subsequent estimates for each study in Figure 2.3. P-values correspond to the hypotheses that each of the β_js, considered individually, equal zero. We have also tested the joint hypotheses that all of the β_js in a particular study equal zero. Across the four studies for which this is relevant and for both outcomes, we never reject the null hypothesis.

We also use a version of this model to test for baseline balance. For each baseline covariate, we estimate Equation 2.2 separately for each study and report β_js and the p-values for the joint hypotheses that all β_js equal zero in Appendix Tables A.3 to A.8. In Appendix Table A.1, we report a single p-value that aggregates across all studies (1-4 in Panel A and 5-6 in Panel B). This is based on a sample that stacks all observations from the relevant group of studies and includes study-by-stratum and study-by-treatment-arm effects. The p-value is based on an F-test of the joint hypothesis that all study-by-treatment-arm effects equal zero.

Last, in Studies 2 and 4, treatment arms were identified by the presence or absence of particular features. We estimate a separate set of models that examines the impact of each feature.

In Study 2, these take the form:

$$Y_{is} = \alpha + Formal_{is}\gamma_F + FTB_{is}\gamma_M + \mu_s + \varepsilon_{is} \tag{2.3}$$

where $Formal_{is}$ and FTB_{is} are indicators for whether the letter was more formal (vs. informal) and came from the FTB (vs. GSO).

In Study 4, these take the form:

$$Y_{is} = \alpha + Formal_{is}\gamma_F + Amount_{is}\gamma_A + VITA_{is}\gamma_V + \mu_s + \varepsilon_{is} \tag{2.4}$$

where $Formal_{is}$, $Amount_{is}$, and $VITA_{is}$ are indicators for the presence of formal, credit amount, and VITA information features on the letters. Estimates for Equations (2.3) and (2.4) are reported in Table 2.3.

2.3 Results

Figure 2.2 presents the main findings for two types of engagement indicators. In Studies 2 and 4, paper letters included URLs unique to each treatment arm, which allowed us to measure total website visits by each arm. In Study 6, we measure unique click-throughs to URLs embedded in the text messages. Our engagement measures capture only those who click on (or type in) the links included in our messages. We are unable to capture whether our treatments led recipients either to obtain more information via other means (e.g., internet searches for the EITC, CalEITC, or VITA, or other related terms) or to call a VITA site or the 211 number provided in many of our treatments. Thus, they likely understate the number of recipients who received and read our communications.

Nevertheless, engagement is high compared to other estimates of successful online engagement: The average click through rate for our text messages was 10%. Even letters, which required users to type URLs by hand, generated click through rates of around 1%.[6]

Engagement patterns are in line with our main hypotheses: Engagement was higher when the messenger was the government (and therefore perhaps better known); when the message provided useful, personalized, new information (i.e. the location of a VITA site or a personalized credit amount), and when more formal presentation was used to increase source credibility. In the Study 6 text messages, more information about the value of the credit increased engagement (messages including a personalized credit amount exhibited the highest engagement), but letters in Study 4 that listed the average credit amount did not elicit more pageviews.

Figure 2.3 presents our main findings regarding effects on tax filing and EITC claiming. We present the estimated effect of any treatment relative to control for each study, then for each separate treatment arm. Across all trials and each treatment arm, our interventions did not have significant effects on either outcome. Point estimates are uniformly close to zero. Because our sample sizes are large, our estimates are highly precise. In Studies 1, 3, and 4, we can rule out effects of 0.5 percentage point or larger; in the smaller studies, we can rule out effects larger than 1 or 1.5 percentage point.

Studies 2 and 4 were cross-classified to enable us to examine the effects of different features in isolation. We present estimated effects of letter features in Table 2.3: a formal letter format vs. a flyer; a credible messenger (the FTB) vs. an unknown NGO; the inclusion vs. omission of the average value of the credit; and information about the closest VITA site vs. none. As explained above, these features were designed to test particular hypothesized costs. The format and messenger features were meant to reduce psychological costs by increasing credibility. Specifying the credit's value should have reduced learning costs. Finally, VITA information should have reduced compliance costs. We find no evidence that any feature generated non-zero effects.

2.4 Discussion

As has been reinforced by the substantial challenges governments have faced in delivering economic relief during the COVID-19 crisis, the difficulty of accessing public benefits can be a major limitation to the effectiveness of government policy. Yet, it remains unclear whether the low take-up rates for many public programs reflect design choices, lack of awareness, or other factors. In the absence of good understanding of the determinants of take-up, many efforts to increase take-up begin and end with low-cost informational interventions, sometimes called "nudges."

Early successes demonstrated that these interventions can yield significant effects on enrollment decisions (Thaler and Sunstein, 2009; Hummel and Maedche, 2019; Allcott, 2016; Gerber and Rogers, 2009; Beshears et al., 2015). These successes led to the creation of over 200 "nudge units" working in and with governments across the world (OECD, 2017). However, our under-

[6] Irvine (2020) reports that click-through-rates (CTRs) for Facebook ads, the behavioral measure most similar to clicking on an unsolicited text message, are around 1%, while a study by Wozney et al. (2019) obtains a CTR around 0.1%.

standing of what types of nudges work, in what settings, and for whom remains underdeveloped. Most public nudges focus on people who are already interacting with government programs. This makes intuitive sense: It is both practically and theoretically easier to conduct effective outreach to individuals who already have relationships with the government agency providing the nudge. Understanding how to reach people who have not had previous interactions with government is crucial to improving equity in government service delivery and helping the most vulnerable populations escape poverty. This study aimed to do just that. Based on extant theories about behavioral reasons for non-take-up of benefit programs, our messages should have raised participation.

Similar to many behavioral studies, our experiments show substantial engagement, measured by click-throughs or visits to a website. These show expected patterns, indicating that messages were received and that many recipients engaged with the material they received. Yet all of our messages had null effects on the intended behavioral outcomes, filing taxes and claiming the EITC. Our sample sizes are large enough and the effect sizes small and consistent enough that we are confident that our results can be interpreted as precise zeros.

There are several potential explanations for the failure of our outreach efforts.

First, it is possible that the recipients of our messages were already exposed to the relevant information, and that outreach would have been effective in previous years or in other contexts where there are fewer additional outreach efforts. As with any randomized controlled trial, we cannot make strong claims about external validity.

Second, the value of the EITC may not be large enough to warrant filing a return and claiming it. Non-filers in our sample are eligible for smaller credits than are filers, on average (see Appendix Figures A.1 and A.2). Twenty-five percent of non-filers are eligible for credits of \$300 or less, as compared with 9% of filers. For most non-filers, however, potential credits are large relative to plausible financial costs. Three-quarters of non-filers are eligible for credits in excess of \$300 – enough to outweigh the potential financial costs of paying a tax preparer to file returns (U.S. Government Accountability Office 2014, National Society of Accountants n.d.).

Third, our messages – one-time communications that inform low-income households that they are eligible for large sums of cash – may simply feel too good to be true. We tested various message framings that aimed to get past this resistance, varying formatting of the messages and their sources to increase credibility. Nevertheless, it is possible that none of these broke through, but that alternative messages or messengers would be more successful.

Fourth, the particular messages that we sent may have been ineffective, where others would have been more effective. For example, perhaps messages sent earlier in the tax season, or repeated contacts, would have yielded different results. These are potential lines for future inquiry. However, we do not think these are strong candidates for explanations. Our design choices were grounded in behavioral theories drawn from the literature on administrative burden and barriers to take-up, and our various treatments spanned a wide range of potential messages. While it remains possible that we missed the one specific message that would have been effective, this seems unlikely. More plausible is that repeated contacts would have been more effective, but we note that a number of successful interventions in the literature (e.g., Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019) are also based on a single outreach message.

Moreover, our treatments are similar to those that are commonly used in practice. Our partners routinely use one-time, low-cost, low-touch informational campaigns, through targeted text messages, letters, or postcards, to communicate with their clients about benefits and programs. These organizations know that not every recipient interacts with their outreach – but do expect that even if a small percentage take action, the total benefits received for this small group justify the cost of outreach at scale. Our results indicate that this logic may be flawed, as we cannot count on even small effects from this type of outreach. Moving behavior seems to require a larger investment.

Fifth, some eligible filers may face additional direct or psychological costs to interacting with the federal tax agency that may not be addressed simply through information. For example, a family concerned that immigration authorities would use tax returns or claiming behavior to target enforcement efforts or deny citizenship applications might be willing to leave free money on the table. Our interventions were carried out in 2018 and 2019, when such mistrust may have been heightened.

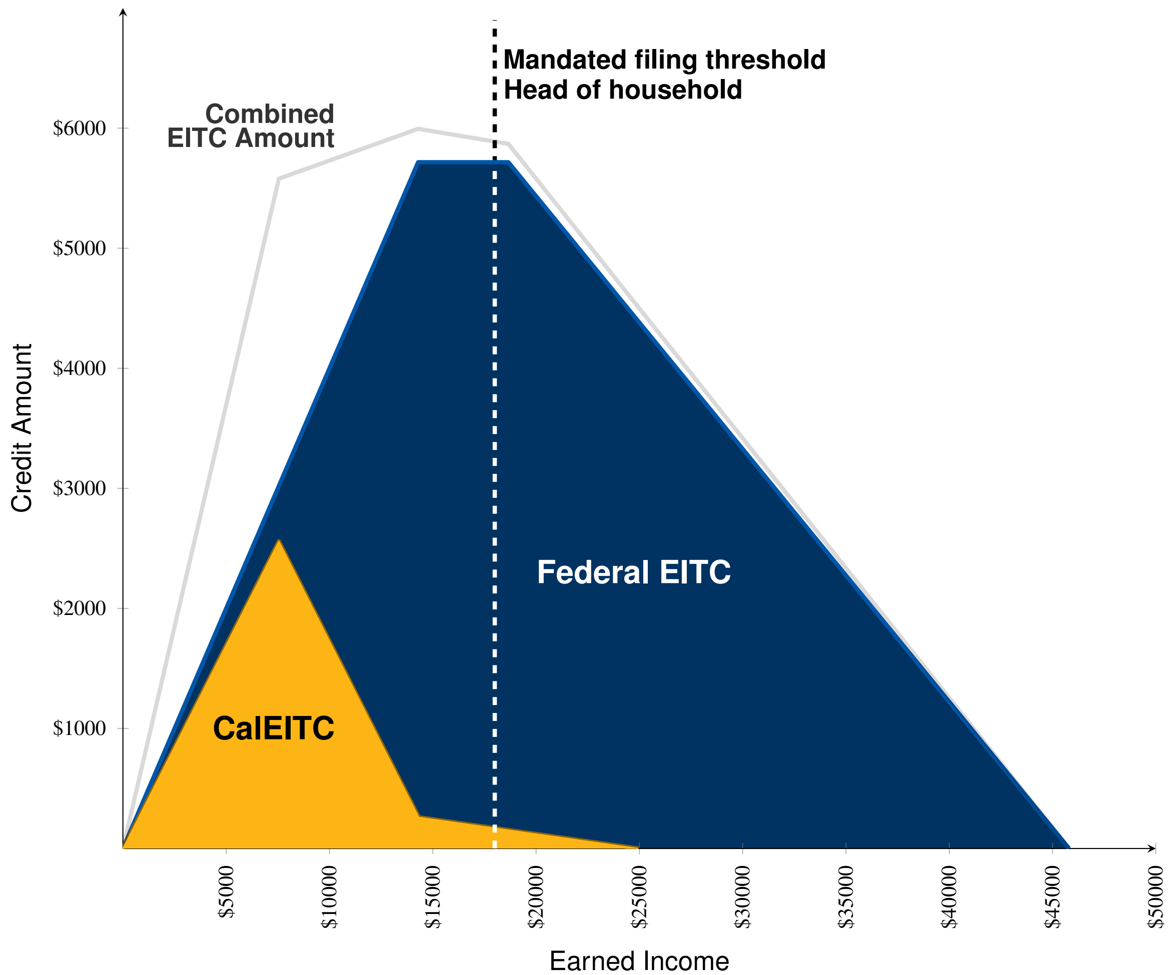
Finally, our treatments do not directly reduce compliance or psychological costs. Instead, we test the effect of messages that use information to reduce perceived learning, compliance and psychological costs. Our evidence suggests that nudges alone may be insufficient, but does not indicate that such costs are absent. The real compliance costs faced by our target population may simply be too high for our messages to overcome. While some trial arms pointed recipients to phone and online support as well as free tax filing assistance, actually receiving that help would have involved seeking out that assistance, going to a VITA site at specified times, compiling all of the required documents, and potentially making an advance appointment. Expanding the availability of this type of free tax support by extending the hours and availability of VITA sites, by increasing access to and quality of free online resources, or by bundling tax services with other services for low-income families may be crucial in helping people overcome real compliance hurdles. Similarly, simplifying the tax filing process itself by using existing administrative data to pre-fill tax returns could reduce real compliance costs.

A first step in understanding the nature of these costs for those who may have limited positive interactions with government is to use linkages of existing data, as we have, to identify potentially eligible households. More research on familiarity with tax filing and awareness of the EITC among non-filers, as well as more qualitative research on the psychological and compliance costs these households face in claiming the credit, would help inform future evaluations of higher-touch interventions that might be able to overcome those costs.

Nudges can provide a low-cost way to achieve many public purposes. They are particularly well suited to bringing marginal people over the threshold into participation, but are less likely to be successful for more inframarginal potential participants. For these populations, higher-touch interventions, particularly design choices that make programs accessible from the beginning, are likely to be required.

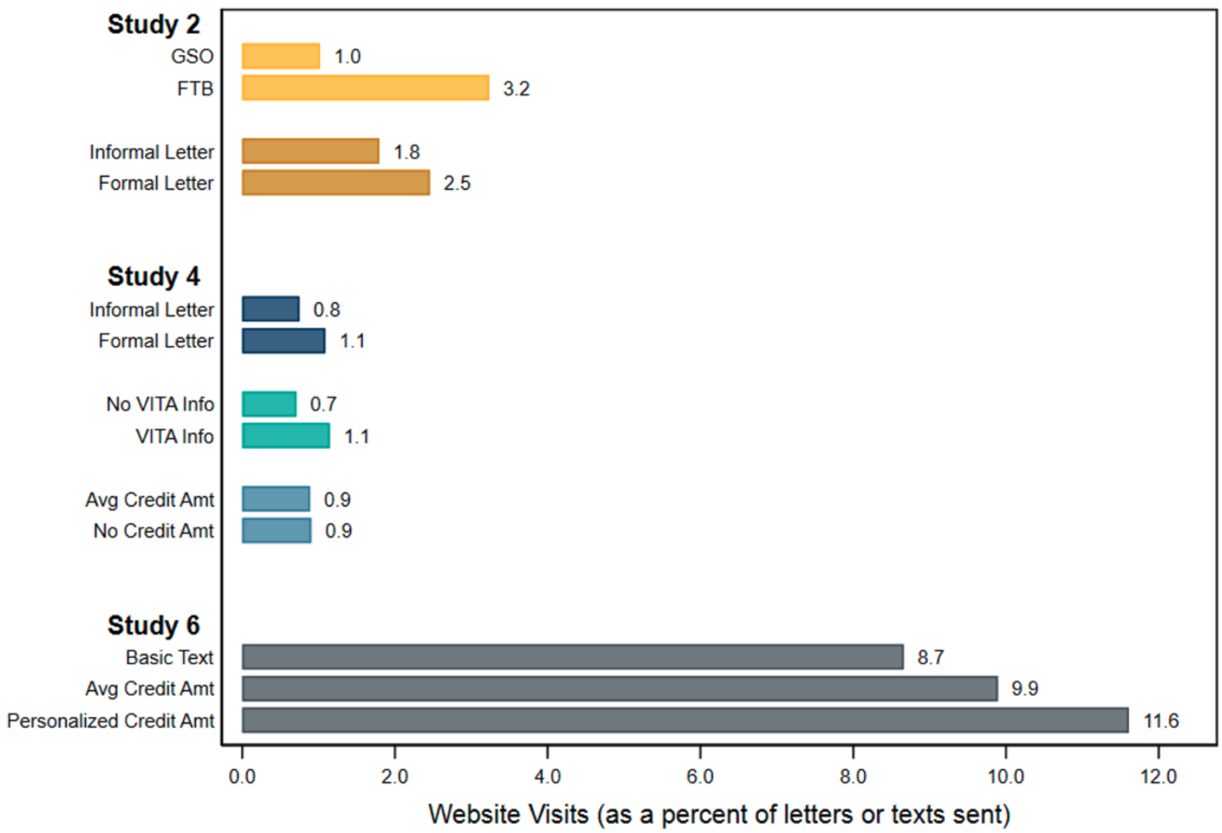
Figures

Figure 2.1: Federal and California EITC schedules for a single-parent family with two children, tax year 2018



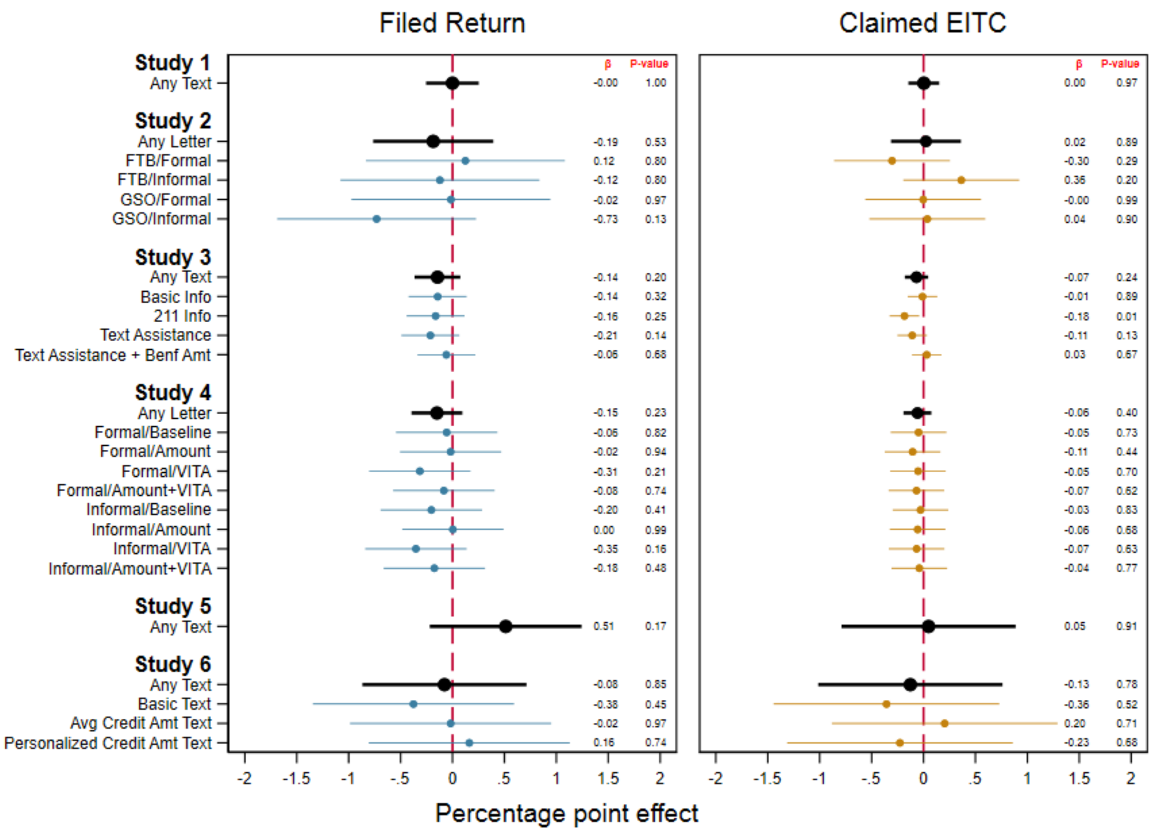
Notes. This diagram illustrates the federal (blue) and state (gold) EITC schedules for a head of household with two children. The gray line illustrates the combined value of the EITC for a filer who claims both credits. The dotted line denotes the filing threshold for a head of household in tax year 2018, which was $18,000; families with incomes below this threshold are generally not required to file returns.

Figure 2.2: Web traffic by pooled treatment features, tax year 2018



Notes. Figure shows measures of engagement as a share of the number of messages sent, by study and treatment feature. In Studies 2 and 4, the features are shared across several treatment arms, and traffic estimates aggregate over all relevant arms. In Studies 3 and 4, engagement measure is visits to a website, hand-entered from treatment-arm-specific URLs included in letters, divided by the number of letters sent. In Study 6, engagement measure is the number of unique visitors to a URL included in the text messages, divided by the number of texts sent.

Figure 2.3: Effects of outreach treatments on tax filing and EITC claiming, by study



Notes. Figure shows estimated treatment effects on tax filing (left) and EITC claiming (right), with 95% confidence intervals. P-values reflect two-sided significant tests. Larger black dots and lines show effects of any treatment vs. control, while smaller, colored dots and lines show effects of each treatment arm individually.

Tables

Table 2.1: Study descriptions

Study	Arms	Treatment mode	Costs addressed
Study 1 N=639,243	1	Text messages sent by NGO	Learning •Simple message
Study 2 N=96,370	4	Letters sent by state government and NGO	Learning •Simple message vs. average benefit amount Psychological •Government vs. NGO messenger •Formal vs. informal
Study 3 N=1,084,018	4	Text messages sent by NGO	Learning •Simple message vs. average benefit amount Compliance •Web vs. text vs. phone-based assistance
Study 4 N=204,285	8	Letters sent by state government	Learning •Simple message vs. average benefit amount Compliance •Local in-person free tax preparation information Psychological •Formal vs. informal formatting
Study 5 N=38,093	1	Text messages sent by county welfare office	Learning •Personalized benefit amount Compliance •Free tax preparation website •Address of local in-person free tax preparation assistance
Study 6 N=47,102	3	Text messages sent by county welfare office	Learning •Average vs. personalized benefit amount

Table 2.2: Summary statistics and balance tests

	Mean	SD	P-value
Panel A: TargetSmart data (N=1,084,018)			
Age	60	21	0.26
Male	0.42	0.49	0.14
White	0.55	0.50	0.20
Married	0.21	0.41	0.66
Have children	0.47	0.50	0.72
College graduate	0.22	0.42	0.53
Filed in previous tax year	0.41	0.49	0.49
Filed in current year, prior to start of study	0.11	0.31	0.83
Panel B: CalFresh participants (N=47,102)			
Age	37	12	0.15
Male	0.36	0.48	0.75
White	0.23	0.42	0.19
Primary language is English	0.75	0.43	0.74
Presence of other adults	0.31	0.46	0.18
Presence of children	0.68	0.47	1.00
Filed in previous tax year	0.74	0.44	0.52
Filed in current year, prior to start of study	0.43	0.49	0.39
Predicted annual income	$14,177	$10,636	0.73
Predicted EITC amount	$2,715	$2,176	0.69

Notes. Summary statistics are for samples used in Studies 2 and 6. The rightmost column shows p-value for a hypothesis test that the indicated characteristic has the same mean across all assignment arms in all relevant studies (1-4 in panel A; 5-6 in panel B).

Table 2.3: Effects of treatment features, identified from across-treatment arm variation

	Filed Return		Claimed EITC	
	Study 2	Study 4	Study 2	Study 4
Baseline	0.377	0.089	0.076	0.024
	(0.002)	(0.001)	(0.001)	(0.000)
Formal letter	0.002	-0.000	-0.002	-0.000
	(0.004)	(0.001)	(0.002)	(0.001)
Messenger: FTB	0.000		0.001	
	(0.004)		(0.002)	
Benefit amount		0.001		-0.000
		(0.001)		(0.001)
VITA referral		-0.002		-0.000
		(0.001)		(0.001)
N	96,370	204,285	96,370	204,285
p-value, $\gamma = 0$	0.89	0.45	0.53	0.88

Notes. These estimates correspond to γ coefficients in Equations 2.3 and 2.4.

Chapter 3

Measuring Take-up of the California EITC with State Administrative Data

3.1 Introduction

The Earned Income Tax Credit (EITC) is the largest means-tested cash transfer program in the United States. In 2019, 25 million households received about $63 billion, with an average benefit of approximately $2,500 (IRS, n.d.*b*). One-fifth of all tax units, and nearly one-half of tax units with children, claimed the EITC. California introduced its own EITC in 2015, joining 27 other states and the District of Columbia that supplement the federal EITC. In 2017, approximately 1.5 million tax units claimed this state supplement, known as the CalEITC, receiving a total of $351 million (Davis and White, 2019). Numerous studies have documented the EITC's beneficial effects on work, income, and poverty; children's educational achievement and attainment; and adult and infant health (see reviews in Hoynes and Rothstein, 2017; Nichols and Rothstein, 2016).

For eligible households to realize the EITC's many beneficial effects, they must file a tax return and claim it. However, the IRS estimates that one in five households who are eligible for the EITC do not receive it (IRS, n.d.*a*). The EITC's take-up rate exceeds that of many other means-tested programs. Nevertheless, this level of non-participation means millions of households fail to receive critical financial assistance that is available to them.

Exactly how many eligible households fail to claim the EITC remains a disputed statistic. The unofficial, but most commonly cited, measure of take-up relies on matching responses from the Current Population Survey's Annual Social and Economic Supplement (CPS ASEC) and the American Community Survey (ACS) to IRS records. While a significant improvement over past approaches, this method relies upon imputations of EITC eligibility from survey data that are prone to error (Jones and Ziliak, 2019). Moreover, this approach cannot be used to assess take-up of state-level EITCs.[1] As such, there exists no estimates of take-up of the California EITC or any other state supplements.

[1] The ACS has a larger sample size and could be used to produce state-level estimates of take-up, but doing so would mean sacrificing precision in the estimation of income and family structure.

To provide a comprehensive look at CalEITC participation and address limitations of the current approach to measuring EITC take-up, we propose a new method to measure eligibility and participation that relies solely on state administrative data. We use enrollment records for the CalFresh program (the state's instantiation of the Supplemental Nutrition Assistance Program [SNAP], or food stamp program) linked to administrative earnings records and California state income tax returns. Importantly, CalFresh program records provide detailed family composition information, which we use to identify likely eligible tax units even for those families who do not file tax returns. We use these program records for all CalFresh participants in 2017, matched to tax return data for that year, to measure eligibility and participation across both filers and non-filers in our CalFresh universe. For each population, we use available information to measure three components of eligibility (filing status, earned income, and number of qualifying children) and then identify which of the seemingly eligible tax units claimed the state credit.

Importantly, these data contain two pieces of information critical to measuring EITC eligibility that tend not to be available in tax records or household surveys. First, we observe the date of birth for each CalFresh recipient, which allows us to identify the ages of dependents on tax returns and identify likely child dependents among non-filers. Second, CalFresh enrollment records provide monthly snapshots of household composition, which enable us to observe whether adults reside with children for a certain number of months in the tax year and to group non-filing adults and children into likely tax units. The novelty of this information and our data match enable us to make important progress on measuring eligibility among filing non-claimants and the larger population of non-filers.

We find that more than 440,000 households who received CalFresh benefits and who appeared eligible for the state EITC did not receive the credit in 2017. This includes roughly 42,000 who claimed the federal EITC but not the state credit; 110,000 who filed a state tax return but did not claim the state credit; and 290,000 who did not file a state tax return. The corresponding take-up rate for these households was 53%. Altogether, these households left on the table a total of almost $76 million in state EITC funds. If received, these credits would have increased incomes among these households by 2.6% and increased total state EITC outlays by 38.8%.

Two-thirds of non-claiming is attributable to eligible households not filing a state return. Among filers and non-filers, the majority of non-claimants are single individuals without dependents. These non-claimants appeared eligible for about $80 on average. Still, we show that tens of thousands of tax units with dependents and non-filing households with children failed to claim the CalEITC. Unclaimed amounts for these households was much higher: an average of $259 for filers and $650 for non-filers. We also present estimated participation rates by individuals' race and filers' preparation method. We find that eligible Black and Hispanic filers were less likely to claim the credit, and that Black, Hispanic, and American Indian or Alaskan Native non-filers were more likely to be eligible. Overall, eligible Asian adults were most likely to claim the CalEITC, and eligible American Indian and Alaskan Native adults were least likely to claim. We also show that eligible households who claimed the federal EITC were much less likely to also claim the state credit if they filed using a paid preparer.

The paper proceeds as follows. In Section 3.2, we describe the federal and California EITC more fully and summarize previous work estimating EITC participation and potential explanations for incomplete take-up. Section 3.3 describes our unique linked data that make our project possible. Section 3.4 describes our methods for simulating EITC eligibility among CalFresh recipients and presents participation estimates. Section 3.5 presents additional results and estimates of take-up for various subgroups. Section 3.6 discusses the relevance of our findings to strategies for increasing take-up. Section 3.7 concludes.

3.2 Background on the EITC

Structure of the federal and state credits

At low earnings levels, the amount of the federal EITC for which households are eligible increases with each dollar of earnings until it reaches a maximum value. The credit is then stable across a range of earnings, before eventually beginning to decline as earnings rise. This structure encourages labor force participation for low earners, who cannot receive the credit unless they work (Nichols and Rothstein, 2016). The basic shape of the schedule is the same for all households, but the quantitative parameters differ. Families with children qualify for much larger credits, at higher earnings levels, than do families without children. Married couples can have higher earnings before the credit begins to phase out than can single filers.

The blue area in Figure 3.1 shows the federal EITC schedule for a single filer with two children in tax year 2017. The maximum credit, available to filers with earnings between $14,040 and $18,340, is close to $6,000. Filers with earnings above or below this range qualify for smaller credits, so long as their earnings are positive and do not exceed $45,007.[2]

In 2015, California joined 27 other states and the District of Columbia in supplementing the federal EITC. Most states with supplements simply offer a partial match for the federal credit, but California adopted its own schedule. The gold area in Figure 3.1 shows the California schedule in tax year 2017. California's schedule is targeted towards households with the lowest earnings. The largest credit is available to families with earnings around $7,500, though households with earnings up to $25,000 can still qualify for small credits. In 2017, approximately 1.5 million tax units claimed the CalEITC, receiving a total of $351 million (Davis and White, 2019).

EITC eligibility depends on a tax unit's filing status, earned income, and number of qualifying children. EITC eligibility depends on the tax unit's Adjusted Gross Income (AGI), investment income, and earned income. A tax unit must have investment income below a set threshold ($3,450 in 2017), and both the tax unit's AGI and earned income must be in the eligible range. The calculation of the actual EITC amount generally depends on the tax unit's earned income and in some cases its AGI.[3]

The EITC uses a different count of children than do other components of the tax code. An EITC qualifying child must pass have a valid SSN; must be under 19, or under 24 if a full-time

[2]This schedule assumes the family does not have investment or other unearned income, which can cause a family to lose eligibility or can reduce their credit amount.

[3]Tax units with income from sources other than earnings and whose AGI is above a certain level are instructed to calculate their EITC amount using their earned income and their AGI, and claim the lesser of the two amounts.

student; must reside in the household for at least half the year; cannot be claimed by a different filer; and must be a near relation or an adopted or foster children of the filer.

Families with very low earnings levels are typically not required to file tax returns, and only their eligibility for the EITC and other tax credits, as well as overly withheld income taxes being returned, creates an incentive to do so. Prior to the introduction of the CalEITC, there was little reason for many families to file a state return, even if they filed a federal return in order to claim the federal credit. This possibility raises concerns that many eligible families still might not file state returns or, if they do, not know to claim the CalEITC. This issue is relevant to all state supplements, but the the concentration of CalEITC benefits at very low earnings levels heightens the concern in this setting.

Estimates of take-up rates

An accurate estimate of EITC participation requires a sample of the eligible population (including information on household composition, earnings, citizenship status, and more) as well as a tag for EITC receipt. Few administrative or survey datasets contain even proxies for each of these variables, let alone measures that match the IRS definitions.

A number of studies use survey data to simulate eligibility and receipt. Using the Survey of Income and Program Participation (SIPP), Scholz (1994) estimated that take-up in tax year 1990 was between 80 and 86%, and that 1.3 million to 2.0 million eligible taxpayers failed to claim the credit. In 2001, using multiple household surveys, the Government Accountability Office (GAO) estimated EITC participation to be 75% and dollar participation to be almost 90% in tax year 1999. In 2002, IRS researchers estimated that about 15% of eligible households in the CPS and SIPP did not file a tax return, suggesting a maximum take up rate of 85%. Using the CPS, Blumenthal, Erard and Ho (2005) estimated EITC participation to be nearly 90% among legally obligated filers for tax year 1988 , but only 30 to 40% among those not legally obligated to file. Among all eligible households in that year, they estimated take-up to be roughly 70%. A major drawback to the survey-only evidence, however, is that these data typically do not have direct measures of EITC receipt.

In 2004, the IRS and Census Bureau completed the first exact match of tax records and a household survey to estimate EITC take-up. Plueger (2009) describes the match of households in the Individuals Return Tax file to households interviewed for the Current Population Survey Annual Social and Economic Supplement (CPS ASEC) for tax year 2005. With these merged data, researchers estimated take up of the EITC to be 75%. Jones (2014) uses the same match to measure EITC eligibility and participation between 2009 and 2014 and identify factors that predict take-up. The IRS continues to partner with the Census to produce these estimates and to refine this merge. Their results continue to be the most commonly cited measure of take-up.

Though an improvement over previous efforts to measure take-up, this match still suffers from important data challenges. One limitation is that the CPS ASEC collects information about household composition at the time the household is surveyed, but key tax variables depend on households' circumstances at year's end or over the course of the entire year. For example, it is not possible to determine for how long an ASEC child lived with a parent in the relevant tax year, a key consideration when determining whether a dependent is a qualifying child for the purposes of the EITC. Marital relationships may also change between the end of the tax year and the ASEC survey. Further, the matched CPS-IRS data suffers from selection and measurement issues. For example, annual income is measured via respondents' own self-reports, which is prone to error (Meyer and Mittag, 2019).[4] In addition to standard selection concerns from survey non-response issues, researchers were forced to drop almost half of CPS households from the exact match process because they had incomplete survey information, did not complete the ASEC, or did not agree to have information matched to the IRS (Jones and Ziliak, 2019).

Other research on EITC take-up has found that non-filers account for most of the unclaimed dollars. Of households that qualify for the federal EITC but do not claim it, approximately two-thirds do not file any tax return (as cited in Cranor, Kotb and Goldin, 2019). In contrast to non-filers, a large majority (91.5%) of EITC-eligible households that do file a return claim the credit. However, these estimates are based on federal data, where the IRS has developed sophisticated tools for inferring EITC eligibility for those who do not claim it and conducts substantial outreach to identified non-claiming households (Bhargava and Manoli, 2015). There is no available evidence about take-up of state credits.

Similar to this work, Maag et al. (2015) use SNAP enrollment data from Florida, linked with IRS records, to validate EITC eligibility. The authors find that SNAP data could be useful in evaluating which children are qualified children under the EITC, improve enforcement of improper claiming, and to aid with outreach efforts.

Explanations for incomplete take-up

In addition to the narrow literature investigating EITC participation, our paper also contributes to a broader literature investigating the extent of and the reasons for incomplete participation in an array of means-tested programs. Researchers point to three broad explanations for why eligible households may fail to take up benefits for which they're eligible: learning, compliance, and stigma costs (Currie, 2006). In the context of the EITC, households may be unaware of the credit or that they're eligible; they may know the EITC exists but find the process of filing and claiming it too costly or burdensome; or the stigma costs or stresses of tax filing may outweigh the financial benefit of claiming.

There is some evidence about the relative importance of each of these factors in the context of the EITC. Several surveys of eligible households suggest many workers are unaware of the EITC or its structure (Liebman, 1998; Phillips, 2001; Romich and Weisner, 2000; Smeeding, Phillips and O'Connor, 2000; Maag et al., 2015). Awareness also appears to vary across different communities (**?**Chetty, Friedman and Saez, 2013). Some research suggests that providing information to tax filers about the EITC and their likely eligibility can increase EITC claims (Bhargava and Manoli, 2015). Chetty, Friedman and Saez (2013) study the effect of tax preparers explaining the

[4]Of the CPS respondent households who can be matched to a return, the difference between the CPS reported AGI and that on the tax return exceeds $10,000 for 14% of households.

EITC's incentive to taxpayers already claiming the credit and find that taxpayers who received this information are likely to claim higher EITC amounts in the next year.

Evidence on the effect of sending information about the EITC to potentially eligible non-filers is more mixed. Jones (2010) finds small participation effects of an employer-run program designed to increase take-up of an advance on eligible workers' likely EITC amount. Guyton et al. (2016) studies the effect of sending mailings to eligible, non-filing, eligible households, finding small but statistically significant effects on EITC take-up. Cranor, Kotb and Goldin (2019) find that mandating employers to inform employees about their potential eligibility of the EITC had no effect on EITC take-up. Linos et al. (2020) find that a variety of informational nudges sent by both public and non-profit messengers to over one million potentially eligible California households encouraging them to take-up the EITC had no effect on tax filing or EITC claiming. Goldin et al. (2021) find that a single letter from the IRS sent to non-filers increased tax filing a .7 percentage points.

There is limited evidence about the effect of simplifying the tax filing process on EITC claiming. One exception is Kopczuk and Pop-Eleches (2007) who find that the introduction of electronic tax filing increased EITC claims. Goldin and Liscow (2018) argues that efforts to increase take-up should focus on the complexity of tax filing rather than on awareness.

3.3 Data

We draw on data from several California administrative data systems to identify the family structure, income, and tax filing status of households that participated in the CalFresh program (the state's version of the Supplemental Nutrition Assistance Program, or SNAP, formerly known as food stamps) in calendar year 2017.

Our first database contains individual- and case-level CalFresh enrollment records. In 2017, 5.6 million unique individuals enrolled in CalFresh, representing 50.5 million person-months. Forty-eight percent of individuals ever enrolled in CalFresh during 2017 were enrolled for all 12 months, while the other 52% were enrolled for only part of the year. CalFresh cases are defined as groups of people who prepare meals together, and an individual can participate in different cases over the course of a year. Our sample includes 2.9 million distinct cases, and 95% of individuals in our sample are associated with only one case during the year. We link adults' CalFresh enrollment records with their quarterly wage records filed with the Employment Development Department (EDD), which administers the state's unemployment insurance program. This allows us to measure wage and salary earnings (though not cash or self-employment earnings) for all adult CalFresh participants.

As mentioned above, the CalFresh records provide key information for determining EITC eligibility that tends to be unavailable in survey and tax data. First, we observe the date of birth for each CalFresh recipient. Second, CalFresh enrollment records provide monthly snapshots of household composition. We use this information to identify which CalFresh-enrolled dependents on tax returns, as well as non-filing children, would pass the EITC's age and residency test, and to group non-filers into likely tax units according to their ages and common CalFresh cases.

Our second database consists of all California resident personal income tax returns filed with the California Franchise Tax Board for tax year 2017. Roughly 33 million of the 39 million individuals residing in California in 2017 are represented on these returns. This dataset contains all tax returns that could have claimed the CalEITC. For the 87% of California filers who submit their returns electronically (known as "e-filing"), we can associate their state returns with their federal returns. Given the importance of federal tax information to our analysis, all statistics presented below are restricted to this population of e-filers, for whom we can observe a federal return.[5]

For this project, we completed the first-ever individual linkage between these two datasets. We used a fuzzy linking algorithm, identifying both exact and near matches on names, birth dates, and Social Security numbers.[6]

On the FTB side, our linkage is limited to individuals who appear on a state, as opposed to a federal, tax return. The absence of federal-only filers does not bias our estimates of CalEITC participation; CalFresh recipients who do not match to a state tax return cannot have claimed the state EITC. However, we cannot confidently estimate take-up of the federal EITC among CalFresh recipients who do not appear on a state tax return, because some of these individuals may have appeared on a federal return and may have claimed the federal EITC. In Section 3.5, we present take-up estimates for the federal EITC among the narrower population of e-filed tax returns containing a CalFresh-enrolled head or spouse. For these households, we can observe a federal return, a federal EITC claim if there is one, and whether dependents are likely qualifying children.

Table 3.1 presents summary statistics for the FTB tax filer population. Overall, 17% of tax returns include a claim for the federal EITC, and 9% include a claim for the CalEITC. We also present each statistic broken out by number of dependents, as the presence of children is a key criterion for EITC eligibility. Both federal and state EITC claiming shares are larger for returns with dependents than for those without.

Table 3.2 presents statistics for the CalFresh population. We distinguish between three groups of CalFresh cases: those for which all members appear on tax returns, providing complete information about tax unit structure; those for which some but not all members appear on returns, providing partial information; and those for which none of the members file returns. A central piece of our analysis will involve constructing tax units for CalFresh recipients who do not appear on a state return. Overall, of the 5.6 million individuals receiving CalFresh in 2017, 3.1 million (or 66% of CalFresh recipients) appeared on a state tax return. Of these, 1.3 million were heads or spouses and 2.4 million were dependents.

One important sample restriction is that we exclude from our analysis the approximately 1

[5]Given the observed lower incomes of paper filers, and the fact that those who file paper returns are more likely to self-file as opposed to using either paid preparers or volunteer-run tax assistance sites, we anticipate that excluding paper filers from our analysis will bias our estimate of non-claiming downwards. We explore this issue further in Appendix B.2.

[6]The datasets used for the linkage were more expansive than those used for our analysis: All tax returns from tax years 2015-2018, and all individuals enrolled between 2012-2019 in any safety net programs administered by the California Department of Social Services (CDSS). The inclusion of additional years and observations helps us avoid false positive matches in our focal samples. The research team never had access to personally identifying information from either dataset; we used a hashed linking algorithm to identify exact and near matches using hashed identifiers.

million dependents who were claimed on a tax return in which neither the head nor the spouse was a CalFresh recipient. Since we do not observe these filers' casefiles or earnings records in our CalFresh data, we are unable to verify their eligibility for the CalEITC. We also choose not to associate these dependents with other adults who appear on their CalFresh cases, since they were already claimed by these other adults. After this restriction and limiting ourselves only to e-filers, our primary sample includes 2.4 million CalFresh recipients who appear on a tax return in our sample. Of all CalFresh recipients who appear on a state tax return in 2017, 58% (32% of all recipients) include claims for the federal EITC and 38% (21% of the total) include claims for the state credit. Of all 766,000 tax units in 2017 containing a CalFresh-enrolled head or spouse, 69% claimed the federal EITC and 45% claimed the CalEITC (Table 3.3). Their average federal and state EITC claims were $3,081 and $396, respectively.

The Californians who elect to enroll in CalFresh are a subset of the population eligible for the CalEITC. An obvious question is to what extent the CalFresh population is representative of that broader low-income population in the state. In Appendix B.1, we use ACS data to compare a variety of demographic characteristics of households who report receiving SNAP to non-recipients and the wider low-income population in California. We conclude that families in households with at least one SNAP recipient are more likely to be eligible for and to claim the CalEITC due to their lower incomes, larger household sizes, and willingness to interact with government programs. This suggests that our estimates might overstate eligibility, but understate the take-up gap among all Californians.

3.4 Eligibility and participation

Our primary methodological challenge is to measure EITC eligibility for an individual who does not claim the credit. This entails measuring three components of eligibility: the composition of the filing unit on which the individual would appear if he/she filed a return; the unit's AGI and total earned income, and the unit's number of EITC qualifying children (QC).

We measure the extent of eligibility and non-claiming across three distinct populations for whom we have different information about these three components: (1) CalFresh participants who file a state return and claim the federal EITC; (2) CalFresh participants who file a state return but do not claim the federal EITC; and (3) CalFresh participants who do not file a state return.

For those who claim the federal EITC, we can observe nearly all relevant income,[7] and we can infer their number of qualifying children using only information from the tax unit's own return. For those who file but do not claim the federal EITC, we supplement tax unit composition, as well as income and earnings reported on their return, with CalFresh records. Specifically, we use the CalFresh casefiles to test whether dependents pass the qualifying children's age and residency test. For those who do not file a state return, we transform CalFresh cases into likely tax units and measure eligibility using the casefiles, recipients' ages, and merged earnings records.

[7]The only relevant income sources we do not observe are interest and dividend income, which CalFresh-enrolled filers rarely have.

In the following three sections, we describe these processes more fully and present estimates of non-claiming for each population. Table 3.4 provides an overview of the information we use to estimate each component of eligibility across these three populations.

Among filers who claim the federal EITC

For those who file a state return and claim the federal EITC, we observe nearly all of the information needed to simulate eligibility for the CalEITC, including AGI, earned income, and filing status, directly from the filers' state and federal returns.

Qualifying children

The only variable necessary to identify CalEITC eligibility that we do not observe is each unit's number of qualifying children (QCs). As discussed above, a qualifying child for the purposes of the EITC are dependents under the age of 19 (or 24 if the dependent is a full-time student) who reside with the filer for at least six months in the tax year and are near relations or adopted or foster child of the filer. Federal claimants do report their count of QCs on the Schedule EIC, but we do not observe this information as part of the federal return. However, since there is a unique number of QCs that can rationalize the unit's federal EITC claim given their filing status, AGI, and earned income, we can use these variables from the unit's return to infer their number of claimed QCs. In Table 3.5, we compare the number of dependents claimed on the tax return to the count of qualifying children inferred from the federal EITC amount and reported earned income. In the vast majority of cases (97.6.%), the number of QCs from the federal EITC claim and the number of dependents are the same. In just 1.5% of cases, the number of QCs is smaller than the number of dependents on the tax return, and in less than 1% of cases the number of QCs is larger than the number of dependents.

We present two pieces of evidence that help demonstrate that this inference yields trustworthy estimates of each unit's number of qualifying children. First, we compare the count of QCs *inferred* from the federal claim amount to the *actual* number of QCs reported by tax units who claimed the CalEITC (Appendix Table B.6).[8] For nearly all units, the inferred number of QCs exactly matches the number reported on the Schedule 3514. Second, we compare actual CalEITC claims to predicted claim amounts, using the process described here, for the subset of federal EITC claimers who also claimed the CalEITC. We correctly predict the exact CalEITC claim amount for over 97% of CalEITC claimants. See Appendix B.6 for more information about this analysis.

Results

With each unit's inferred number of qualifying children, plus their filing status and reported California earned income, we can identify which appeared eligible for the state EITC in tax year 2017.

[8] We can observe the actual number of qualifying children claimed by these units on the form they submit to claim the state EITC (Schedule 3514). As noted above, we cannot use these counts for all tax units in this population, because we only observe them for those who claimed the CalEITC.

Of the approximately 755,000 e-filed state tax returns containing a head or spouse who enrolled in CalFresh and a federal EITC claim, 71% were eligible for the CalEITC (Table 3.6). Of these eligible units, 8%, or about 42,000 CalFresh tax units, did not claim the CalEITC. These tax units received an average federal EITC benefit of $3,794, but did not claim an additional $233 from the CalEITC. The forgone CalEITC amount for this group totaled nearly $9.8 million, and if received, would have raised annual incomes in this population by 1.6%.[9]

Table 3.6 also summarizes eligibility and take-up of the CalEITC among federal EITC claimants by tax unit type and income levels. A greater share of eligible households with dependents (8-10%), than those without (5%), failed to claim the CalEITC. Eligible households with no dependents forwent an average state credit of $81, and households with dependents forwent an average of $262 to $306, depending on the number of dependents in the household.

The share of eligible households not claiming increases with total earnings: Only 5% of eligible participants with $5,000 to $10,000 in annual earnings did not claim the credit (average credit amount of $571), while 16% of participants in the $20,000-$25,000 income bracket did not claim the credit (average unclaimed credit amount of $28). This difference may be due to eligibility being less certain and the expected return being lower. While take-up was higher in the lower-income categories, there were still many tax units who missed out on a credit for which they were eligible, and the average amounts at stake were substantially larger. Households with incomes below $10,000 and who failed to claim the CalEITC left an estimated $400 to $600 on the table.

Among filers who do not claim the federal EITC

Next, we estimate eligibility among CalFresh-participating tax units who filed a state return but did not claim the federal EITC. As in the previous section, we observe each tax unit's filing status and relevant earned income. However, we do not observe these tax units' number of EITC qualifying children directly, nor can we infer the number from a federal EITC claim. Instead, we impute this number for each unit using other information available on the tax unit's return and in the CDSS records. We describe that process below.

Qualifying children

Beginning with the list of dependents on the return, we interrogate which dependents might be a qualifying child for purposes of the EITC.[10] As mentioned above, QCs must be under 19 (under 24 for full-time students), must reside with the filer for at least six months of the year, must have a valid Social Security number (SSN), and the child can be claimed as a QC on only one return. We

[9] We also identify a small number of tax units (0.1% of all CalFresh units who claim the federal EITC) who do not appear to be eligible for the CalEITC, based on their reported state earnings and federal qualifying children, but who nevertheless claim it. This could indicate an incorrect claim but could also reflect inaccuracies in our simulation. The apparently ineligible claimants have relatively high earnings and an average CalEITC claim of $267.

[10] Although in principle a parent may have a QC who does not qualify as a dependent — for example, a child may be the dependent of a non-custodial parent who provides substantial child support but would be claimable as a QC by the custodial parent – we expect that this is rare and we do not attempt to model it. This rare circumstance might explain the few cases in which there are more inferred QCs in a tax unit than observed dependents.

observe some of this information in our CalFresh records, meaning we can verify which dependents might in fact be qualifying children. Since we cannot verify this information for dependents who do not appear in our CalFresh records, we assume these dependents are qualifying children.

We are unable to identify students, so we only allow dependents age 18 and under to be potential QCs, provided that they satisfy the residency test.[11] We are able to observe whether a dependent has an SSN or an Individual Tax Identification Number (ITIN), and we use this to disqualify children without a valid SSN. See Appendix B.3 for more information about ITIN filers.[12]

To simulate the residency test, we observe the number of months that dependents appeared on the same CalFresh case as the filer and/or spouse on their return. For tax units with single or head of household filing status, or where both members of a married couple appear in the CalFresh records, we use the number of months a child appeared on the same CalFresh case with the head or spouse as a proxy for residential arrangements. A dependent who shared a CalFresh case with the tax unit head or spouse for at least six months is counted as meeting the residency test. If the child and parent were on CalFresh for only part of the year, months in which neither was on CalFresh are allowed to count toward this six-month threshold, on the assumption that the child and parent lived together in this period as well. For married tax units in which only one member appears in the CalFresh data, we cannot track the residency of the non-CalFresh spouse, so we assume the child lives with that spouse and meets the residency test. The effect of these rules is to disqualify dependents as possible QCs if we can observe that they were in different CalFresh cases than the head and/or spouse on their return for more than six months in in the tax year.

Table 3.7 compares the number of imputed EITC qualifying children to the number of dependents claimed on the tax return among units that did not claim the Federal EITC. Since our imputation rule begins with the number of dependents and removes those who do not appear to be qualifying children, the number of QCs is never greater than the number of dependents. For 81% of tax units they are identical. In most of the remainder, we assign one fewer QC than the number of dependents. Of the 43,221 dependents who we deem not to be qualifying children, 29,780 (18% of all dependents) fail the residency test, 13,043 (8% of all dependents) fail the age test, and 11,203 (7% of all dependents) fail the SSN test. A small number of dependents fail more than one test.

To assess the accuracy of our imputation method, we rerun our CalFresh-based imputation process on the set of tax units that claimed both the federal EITC and state EITC, and for whom we can observe actual number of qualifying children. We then compare each tax unit's estimated number of qualifying children according to our CalFresh records against the number reported on those unit's 3514 (Appendix Table B.7). In three-quarters of the cases, our inferred number matches the reported number exactly. When our imputation errs, it most often does so by underestimating the number of QCs, and therefore the family's EITC eligibility and/or credit.

In Appendix Table B.8, we report the same comparison as in Appendix Table B.7 except that we do not allow dependents who did not enroll in CalFresh to be qualifying children. This results in our disqualify many dependents who are claimed as qualifying children. Though allowing non-

[11]Of the 167,240 dependents under age 24 on a return with a CalFresh-enrolled head or spouse and without a federal EITC claim, 4,364 (2.6%) are between the ages of 19 and 24.

[12]In some cases, returns claim more dependents than the number of SSNs we observe. Since cannot link these additional unlisted dependents to CalFresh participants, we do not allow them to count as QCs.

CalFresh dependents to be QCs risks overstating EITC eligibility, restricting our analysis only to dependents observed in CalFresh seems to severely understate the number of actual QCs.

Results

Confident in the imputation method for determining EITC qualifying children, we return to estimating eligibility for and take-up of the CalEITC for this population. Table 3.8 summarizes our results. Nearly 350,000 tax units contain a head or spouse who was enrolled in CalFresh in 2017 and did not claim the federal EITC. Of these units, nearly 113,000 (33%) were eligible for the CalEITC, and among these eligible units, nearly 110,000 (97%) did not claim it. Very few eligible households claimed the state EITC but not the federal EITC. The average forgone CalEITC amount for this group was $84, and totaled nearly $9.3 million. If received, these benefits would have raised annual incomes in this population by 1.1%.

Single filers without QCs were more likely to be eligible than married filers or filers with QCs, which reflects their much lower average earnings. Eligible non-claimants in this group missed out on $82, on average. Though there are far fewer eligible non-claimants among tax units with QCs (just over 3,000), their average forgone credit was higher ($216). Participation rates tended not to vary too significantly with either tax unit composition or total tax unit earnings. Take-up ranged between 2 and 9%. Again, since most non-claimants were adults without QCs, the average forgone credit was fairly low regardless of income level.

Among non-filers

Lastly, we turn to our third population: CalFresh participants who do not appear on a 2017 California tax return. For these participants, we must construct simulated tax units from CalFresh casefiles and simulate eligibility using those casefiles and linked earnings records.

This is the most complex part of our entire analysis. CalFresh cases represent groups of individuals who eat and prepare meals together, while tax units generally reflect immediate families. These two types of households might not coincide. For example, there may be individuals in the CalFresh case (e.g., extended family or unrelated roommates) who are not a part of the tax unit, and individuals in the tax unit (e.g., dependents of non-custodial parents) not on the CalFresh case. We do not observe household relationships, which would allow us to associate filers with their spouses and children and vice versa.[13] That said, measuring eligibility among non-filers (or non-participants across a range of public programs) is not a new challenge, and our administrative data boasts several advantages over commonly used survey data, as discussed in Section 3.2.

The following section details how we construct these simulated filing units, that is, groups of CalFresh recipients who would likely appear on a tax return together if a return had been filed. We proceed in several steps: We disambiguate CalFresh cases, classify individuals as filers or

[13] Using merged IRS and SNAP data from Florida, Maag et al. (2015) show that, among federal EITC claimants, 99% of all claimed qualifying children pass the relationship test and 77% of qualifying children appear to pass the residency test. These findings suggest that the relationship test may be less of a concern for determining EITC eligibility within this population.

dependents, identify married couples, assign dependents to filers, test which of those dependents might be a qualifying child, and construct a measure of earned income.

Reference cases

First, we assign each CalFresh recipient to a single CalFresh case. For the 95% of individuals who appear on only a single CalFresh case in 2017, this is straightforward. For the remaining 5%, we assign individuals to the case they appeared on most frequently. In the rare event of ties, we pick the most recent of the cases. Hereafter, we refer to the assigned CalFresh cases as the individual's *reference case*.

We construct simulated tax units from these reference cases. Some reference cases will include filers and non-filers. Following our general principle that we defer to submitted tax returns when possible, we always respect the tax unit reported on the submitted tax return and construct one or more tax units from the remaining non-filing members of the reference case, as detailed below.

Some reference cases may contain only children, who cannot file a tax return by themselves. Other reference cases may only contain non-filers, but include more individuals than could plausibly appear on a tax return together. Below, we discuss how we address these issues and transform these reference cases into likely tax units.

Assigning heads, spouses, and dependents

We assign each non-filing individual on a CalFresh reference case to be a filer, spouse, or dependent. We assign everyone under the age of 18 or over the age of 80 to be dependents.[14] For those aged between 18 and 80, we predict whether they should be a dependent or a filer using other available information, including their earnings, age, sex, race, language spoken, disability status, number of months on CalFresh, participation in other safety net programs, and prior year status. Specifically, we use the CalFresh observations who do file returns to train a prediction model to classify observations as likely filers/spouses or dependents using these variables. We use cross-validation to select a threshold predicted probability of being a filer/spouse that maximizes out-of-sample accuracy. See Appendix B.4 for a full description of this process.

Filing status

We then identify which of the recipients assigned to be filers would file as a single adult or married couple. This step is only relevant for the subset of reference cases with multiple adults, as adults alone on a reference case must be single filers. To identify likely married couples, we first look to tax returns from the prior tax year. If two individuals on the same 2017 CalFresh case filed as married filing jointly in tax year 2016, we assume they are still married and would still file as part of

[14] In 2017, 99.4% of child CalFresh recipients (aged 17 or younger) and 77.9% of elderly CalFresh recipients (aged 81 years or older) who appeared on a tax return were dependents. Appendix Figure B.2 shows the share of dependents by single-year-of-age among tax filers. There is not an obvious break point at 81, but the size of the population at that age is fairly small, making reliable imputation of an individual's tax status challenging.

the same tax unit. For those remaining adults who do not appear on a tax year 2016 return, we use the relative age of each adult to decide whether the pair is likely a married couple. We marry two individuals in a reference case if they are each older than the 10th percentile of ages among married filers and if the age difference between them is between the 10th and 90th percentiles of within-couple age-differences (also among filers).[15] When there are multiple pairings that would satisfy this rule, we pair the adults who are closest in age. The effect of these rules is that anyone who can be paired to another reference case member of a plausible age is assumed to use a "married" filing status. Anyone who remains unpaired is assumed to use a "single" filing status.[16] Overall, we impute that 13% of the heads or spouses in our non-filing sample are married.

Assigning dependents

The next step is to assign dependents to imputed tax units. We only consider as candidate tax units those that contain an adult in the dependent's CalFresh reference case and contain an adult with whom the dependent might have resided for at least six months in the tax year.[17] When there is only one such tax unit, the assignment is straightforward. When multiple candidate tax units satisfy these criteria, we assign dependents to tax units containing adults with whom the dependent appeared on the prior year's tax return. If there are still multiple candidate tax units, we assign child dependents to tax units with adults who are at least least 16 years older than the child, meaning they could plausibly be the child's parent. If there are still multiple such units, we assign child dependents to the unit with the youngest plausible parent, and in the event of further ties, to the unit with the highest earnings.[18] In the case of adult dependents with multiple candidate tax

[15]The 10th percentile of age among married heads and spouses is 26 and 27 for women and men, respectively. For women, the 10th and 90th percentile of within-couple age differences is 6 and 11 years, meaning women are permitted to marry individuals in their reference case who are 6 years younger or 11 years older than them. The percentiles for men are the inverse. Men are permitted to marry individuals in their reference case who are 11 years younger and 6 years older than them.

[16]We anticipate that this set of marriage rules will underestimate the actual number of married filing jointly tax units that could exist within the non-filing population. Applied over all CalFresh-enrolled filers, our marriage assignment process results in our incorrectly assigning 39,000 individuals who filed a single return to a married couple, reflecting 4% of all single returns with a CalFresh-enrolled head. However, that same process incorrectly assigns the primary taxpayer of 106,000 married tax units to a single return, reflecting 39% of married tax units. We correctly set as married filing jointly only 59% of married tax units. In 90% of cases where we incorrectly label an individual as single, they are a member of mixed tax return, meaning we do not observe their spouse in the CalFresh records. If we assume there is a similar share of mixed-CalFresh status married couples among non-filing CalFresh recipients, then we should expect a similar undercount of married couples there, as well. Under-simulating married units likely biases our measure of EITC eligibility upwards, since some number of simulated eligible single filers might become ineligible if we combined their earnings with a spouse. It is possible that eligibility for the EITC could increase, but that would likely be a function of the qualifying children that are connected to the other adult, and not the marriage itself.

[17]To measure how long adults and children resided together, we count the number of unique months they shared a case in the calendar year, aggregate across multiple cases where needed, and also count months in which the dependent was not enrolled. The effect of this restriction is to rule out candidate tax units containing only adults with whom we can confidently infer from our CalFresh records the child did not reside for more than half the year.

[18]Our process of assigning imputed dependents to potential tax units results in 732,000 imputed dependents (out of a total of 1,067,000) being unassigned to any tax unit. Essentially all of these dependents were assigned to a reference

units, we assign them to the unit with the highest earnings.

Qualifying children

We determine which of the dependents assigned to each imputed tax unit might be a qualifying child using the same procedure described in Section 3.4. We start with the list of dependents assigned to the the simulated tax unit and assess which dependents pass the age and residency test. Table 3.9 compares the number of dependents in each imputed tax unit to the number of simulated QCs. The share of dependents who appear to qualify as QCs is similar to those reported for filing non-claimers of the federal EITC (Table 3.7). Of the 197,375 imputed dependents on a reference case who we deem not to be qualifying children, 122,757 (7% of all imputed dependents) fail the residency test and 81,197 (8% of all imputed dependents) fail the age test.

Earned income

Within each imputed tax unit, we sum all adults' EDD wage earnings over the tax year, and we assume that this total reflects both the AGI and earned income that the tax unit would report on their return if they filed. We do not observe any other form of earnings, like those from self-employment, for this population. This omission means we understate EITC-qualifying earnings. This might lead us to overestimate eligibility if non-filing households have both self-employment and wage earnings, and the combination pushes some households above the maximum eligible earnings limit. It is also possible, and perhaps more likely, that this omission results in our underestimating eligibility and overestimating take-up, because we assume many households with no wage earnings are ineligible, even though they might have some positive self-employment earnings which could make them eligible. See Appendix B.5 for more information about self-employment earnings among CalFresh tax filers.

We also do not observe investment income for non-filers, which might disqualify some families from the EITC. Few CalFresh recipients have investment income, however. According to the ACS 5-Year Sample, only 2% of 18-64-year-old adults with below median income enrolled in SNAP had positive investment income, and less than 1% had investment income above the eligibility threshold (compared to 6.9 and 2.0% for non-recipients, respectively). Among actual tax-units

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case without an adult who was likely a filer. Thirty-six percent of these unassigned dependents are children who do not appear on a CalFresh case with any adult. We do not observe these children's parents or guardians in our CalFresh records; we cannot make any progress in understanding whether they might be eligible for the CalEITC, so we assume they are not. These children likely reflect CalFresh enrollees whose parents or guardians are undocumented adults, who are not eligible for CalFresh, the federal EITC, or, in 2017, the CalEITC. Parents of young children with ITINs became eligible for the CalEITC and certain other tax credits in 2020. We discuss these issues further in Appendix B.3.

Sixty-one percent of the unassigned dependents are adults. One concern is that our process of predicting whether CalFresh recipients are likely to be filers or dependents should have predicted these lone dependents to actually be single filers. However, 96.5% of these adults have no earnings, suggesting they would not file a return by themselves. We discuss this issue and other concerns about this prediction procedure in Appendix B.4. As a robustness check, we reclassify these remaining adult non-filers to be single filers and find that slightly under 15,000 would be eligible for the CalEITC (with a mean credit amount of $71).

with a CalFresh recipient, only 1.3% had positive investment income in tax year 2017, and 0.6% were disqualified from receiving the EITC because their investment income was too high.

We discuss the accuracy of our earnings imputation further in Appendix B.5.

Results

After assigning a filing status, number of qualifying children, and earned income to each simulated tax unit, we can finally test how many appear eligible for the state EITC. Table 3.10 summarizes our estimates of eligibility for this population. We identify nearly 804,000 potential tax units from the non-filing CalFresh population, and we estimate that nearly 290,000 of these households (36%) were eligible for the CalEITC but did not claim it. Their average forgone credit was $196. The total forgone credit for this population was $56.6 million and, if received, would have increased annual income for this population by 3.9%.

Table 3.10 also presents estimates of eligibility and participation by filing status, number of qualifying children, and earnings levels. A large majority of the non-filers would likely be single filers without QCs if they filed return. Approximately 38% of these units are eligible for the CalEITC, but estimated CalEITC amounts are fairly small – just $87 on average. The numbers are similar for married couples, although there are far fewer of these in our data. For tax units with QCs, CalEITC eligibility rates and amounts were notably higher. About two-fifths of single filers with QCs were eligible for the CalEITC and failed to claim on average between $489 and $920. Nearly all households with very low earnings are estimated to be eligible for the CalEITC; only imputed tax units with heads outside the eligible age range are assumed to be ineligible. Most imputed tax units are ineligible because they have no observed earned income.

It is important to recall that if these eligible units represent actual non-filers, as opposed to federal-only filers, their forgone CalEITC amounts like understate the benefits of filing. Many of these units were likely eligible for the federal EITC and other tax credits, not to mention a refund on overly withheld income taxes.

3.5 Additional results

Summary

Table 3.11 brings together our estimates for all three populations – filers who claimed the federal EITC, filers who did not claim the federal EITC, and non-filers. Overall, 49% of CalFresh-participating tax units — either real or imputed -– were eligible for the CalEITC, and among eligible tax units, 53% claimed the credit. This means 441,575 (47%) eligible tax units did not claim the CalEITC, forgoing $172 on average and almost $75 million in total. Among filers, take-up rates were fairly high: 78% of eligible tax units claimed the CalEITC. Nevertheless, roughly 141,000 eligible filers failed to claim the CalEITC. The average forgone credit among filers with children was $259, and for those without children was $81. Two-thirds of non-claimants were non-filers. Among non-filing CalFresh households, 36%, or over 289,000 tax units, were eligible for the CalEITC, forgoing an average of $192. The average forgone credit among non-filers with children was $650, and for those without children was $87.

Take-up of the federal EITC

We are not able to develop a comprehensive estimate of federal EITC claiming among CalFresh households, because some families may have filed federal returns including EITC claims but not state returns, and we do not observe those federal returns. However, we can measure take-up of the federal EITC among those who e-filed a state return.

We measure eligibility for the federal EITC within this population in the same way that we measured eligibility for the CalEITC among those that did not claim the federal EITC (summarized in Section 3.4). We use CalFresh records to test which dependents might be qualifying children, and we use filing status and earned income reported on the state return. Table 3.12 summarizes our estimates of eligibility and participation among all state returns with CalFresh enrollees. We split participation by those who claimed the state EITC and those who did not. Mirroring our count from Table 3.1, about 1.1 million returns contained a CalFresh filer or spouse. Among those, 78% were eligible to receive the federal EITC. Of those, 87% claimed the credit. Those who claimed the CalEITC were overwhelming likely to also claim the federal EITC. A large share of seemingly eligible households who failed to claim the state EITC also failed to claim the federal EITC. Altogether, over 109,000 tax units who were eligible did not claim the federal EITC, forgoing $423 on average.

By tax preparation method

An important question raised by our results is why any eligible filer would fail to claim the CalEITC. We begin to explore this question by investigating how eligibility and participation varies across three methods of tax preparation we observe in our FTB records: self-prepared, prepared by a paid professional, and prepared through the Volunteer Income Tax Assistance (VITA) program (Table 3.13). Among the 42,000 tax units who claimed the federal EITC and were eligible for but did not claim the CalEITC, returns filed by paid preparers are over-represented. While returns filed by paid preparers make up only 60% our sample of tax returns who claimed the federal EITC, they make up 92% of non-claimants. Among tax units on CalFresh who did not claim the federal EITC, we see a higher rate of eligible non-claiming among self-prepared returns (33% for the federal EITC and 32% for the CalEITC) than among returns prepared by VITA or paid preparers (between 23 and 28%). The forgone credit amounts are similar across self-prepared and paid prepared returns, with the exception of forgone CalEITC dollars among federal EITC claimants who used VITA services, which were $100 less than the mean amount forgone among paid and self-preparers. The slightly higher levels of non-claiming among self-preparers that did not claim the federal EITC could be explained by a lack of experience with or information about the tax system, but it is harder to pinpoint what might be causing a higher error rate among paid preparers when it comes to unclaimed CalEITC dollars among tax units who claimed the federal EITC. Certain preparers might have limited experience with a new tax program. Fees charged by preparers might also dissuade eligible filers from claiming the state credit. More research on the role played by the tax preparation industry in accurately and efficiently distributing tax-based benefits to households is needed.

By race

Table 3.14 presents estimates of eligibility and participation by the race of individuals in our pool of CalFresh recipients.[19]

Presenting results at the tax unit level would require that we ascribe results only to the actual or simulated head of each tax unit. Instead, we present results at the individual level.[20] We also report our results separately for heads and spouses and for dependents.

Among filers, eligible Hispanic heads and spouses are less likely to claim the CalEITC than filers from other racial groups represented in our CalFresh sample. Take-up is about 5 percentage points lower for Hispanic filing heads and spouses than White heads and spouses for example. Hispanic dependents in eligible tax units are also 3 to 4 percentage points less likely to receive the CalEITC. Across other racial groups, share of eligible households not claiming the CalEITC is roughly equal (between 19% and 20%).

A greater share of Black, Hispanic, and Native Hawaiian and Pacific Islander (NHPI) non-filing imputed heads and spouses appear eligible for the CalEITC than other imputed heads and spouses. Forty-three percent of Black, 39% of Hispanic, and 36% of NHPI non-filing imputed heads and spouses appeared eligible for the CalEITC, compared to 30% of White heads and spouses. We see similar gaps in participation among imputed dependents. Black, Hispanic and NHPI non-filers are also eligible for higher imputed CalEITC credit amounts. Notably, a much lower share of Asian non-filers appear eligible for the CalEITC (20% of heads/spouses and 22% of dependents).

[19]The race/ethnicity variable we use comes from our CDSS data. This variable combines concepts of race and ethnicity. It is also a combination of self-reporting and social worker visual identification (applicants are asked to provide their self-identified race/ethnicity, but if they do not mark anything the eligibility worker may enter a value based on their own visual assessment). In February 2020, CDSS issued guidance to limit all reporting on race and ethnicity to be self-reported. The demographic distribution of race/ethnicity in the CDSS data is comparable to the distribution of California households enrolled in SNAP by race/ethnicity from the American Community Survey (2019). We exclude the "two or more race" category due to small cell sizes.

Our analysis captures individuals in safety-net programs administered by CDSS who identify as American Indian and Alaska Native but do not live on tribal land and/or earn tribal income. Individuals who earn tribal income are exempt from state tax filing in California and may not appear as having received a payment automatically in our data. However, among those earners who qualify for safety-net programs, most are also likely eligible for tribal safety-net programs (such as the Food Distribution Program on Indian Reservations and Tribal TANF) and would not appear in the MEDS data.

CDSS reports nine ethnicities that are grouped by the US Census Bureau into an "Asian" category (Asian Indian, Cambodian, Chinese, Filipino, Japanese, Korean, Laotian, and Vietnamese), and three ethnicities that are grouped by the US Census Bureau as "Native Hawaiian and Other Pacific" (Guamanian, Hawaiian, and Samoan). Due to small cell sizes, we are unable to report each category uniquely, and use the US Census race/ethnicity categories to best capture the distinct take-up rates across all these categories. CDSS also has a separate category in the data named "Asian or Pacific Islander". We are unable to meaningfully distinguish between each community in that category and so we choose to report it separately.

[20]Accordingly, we do not total amounts of forgone EITC dollars to avoid double-counting.

Altogether, non-participation rates are fairly comparable between Black, NHPI, and White households. Eligible Hispanic and Asian households are more likely to claim. Non-claiming is highest among American Indian and Alaska Native households.

3.6 Discussion

By disaggregating participation rates between filers and non-filers, and highlighting the components of eligibility that tax administrators can and cannot easily confirm, our analysis suggests multiple strategies for increasing take-up of the CalEITC among eligible families.

Increasing take-up among households who file a state return and claim the federal credit would be simplest. California tax administrators observe nearly all the information they need to confirm these households' eligibility and predict their correct CalEITC amount. The state tax agency could verify these households' eligibility on their own, and automatically send them their owed credit. If necessary, the state could also ask these households to attest to the information submitted on their return, so that the state could send them their owed credit.

Increasing take-up among eligible filers who do not claim the federal credit would be more complicated. Estimating likely EITC amounts for this group requires imputing the correct number of qualifying children. As we have shown, tax administrators can use CalFresh records to verify residential arrangements and identify likely eligible households. This process is imperfect, but largely accurate. The tax agency could use this process to focus on those tax units that are most likely eligible, and then reach out to those households to confirm their dependents are qualifying children. While some filers may not respond, others surely will, and the tax agency can then send owed credit amounts to those households. The benefits to taxpayers would surely exceed the cost of this outreach. Successful outreach to these eligible filers might also help them claim the more valuable federal credit.

Those who do not file returns at all are the hardest to reach. We estimate that there were 290,000 potential tax units among the CalFresh enrollees who did not file a state return in 2017 but could have claimed the state EITC if they did, and that this group passed up an opportunity to receive $56 million from the California EITC. We expect that our imputation process yields reliable estimates of average eligibility and participation, but we are not able to ensure accuracy of this imputation at the individual tax unit level. The tax agency would need to engage these recipients and ask them confirm their family structure and earnings.

As mentioned, our study – like all those aiming to measure take-up – suffers from a few limitations. First, we examine only the population of CalFresh participants; we do not estimate eligibility or take-up rates for the much larger population of California families, many low income, who do not receive CalFresh benefits (See Appendix B.1). Second, we are forced to exclude paper filers. We expect that doing so results in our underestimating the size of the take-up gap (See Appendix B.2). Third, we estimate take-up only of the CalEITC. We provide limited evidence of non-claiming of the more valuable federal credit, but since we are unable to observe whether families who did not file state returns did file a federal return, we cannot produce a comprehensive measure of non-participation for the federal credit. Extension of our methods to incorporate federal-only returns would enable an analysis of this form, but this would require data-sharing between CDSS and the IRS.

3.7 Conclusion

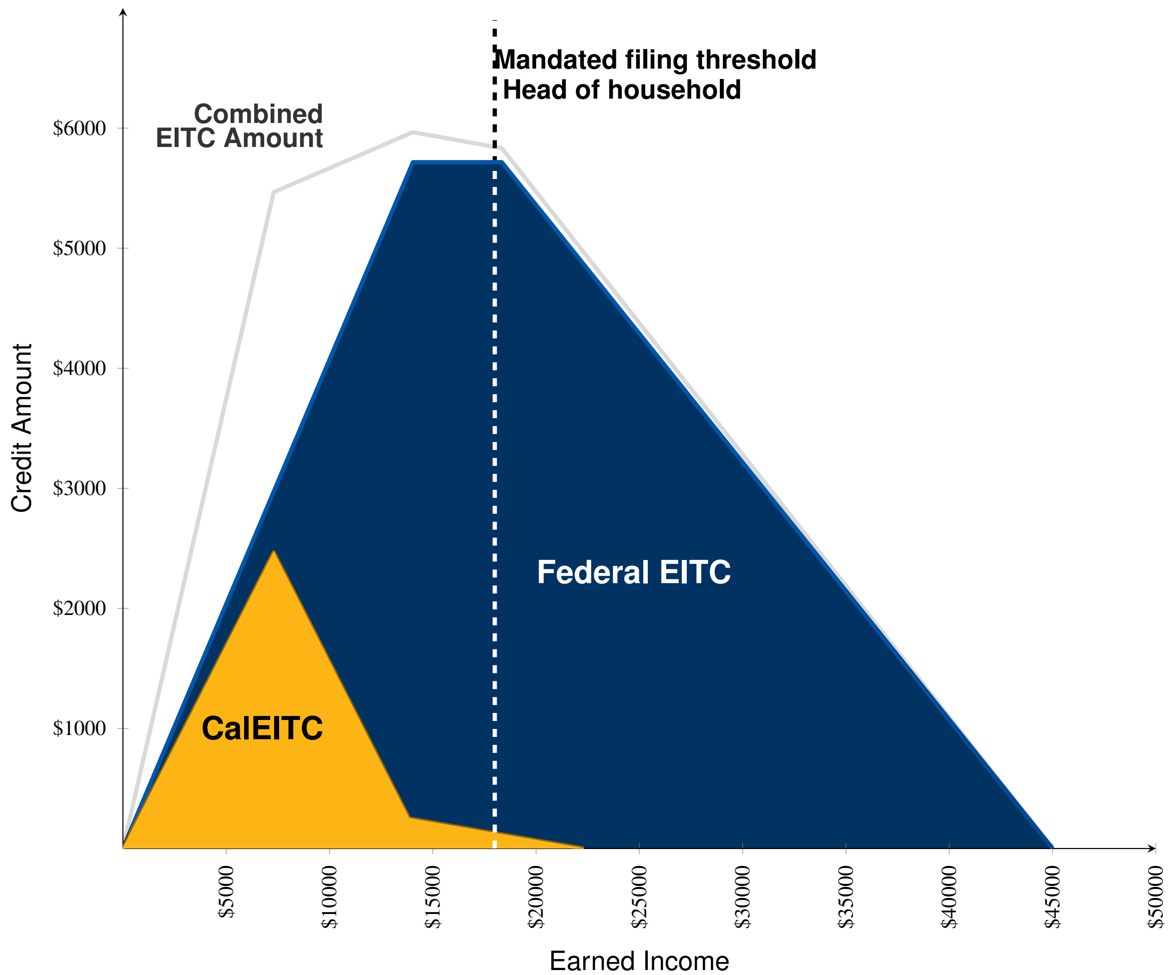
In this paper, we use California administrative data to measure eligibility and participation in the state's Earned Income Tax Credit (CalEITC) program among households who enroll in the state's Supplemental Nutrition Assistance Program (CalFresh). We match state program enrollment data, as well as linked earnings records, to the universe of state tax returns in tax year 2017. We use CalFresh casefiles and earnings records to measure eligibility among non-filers, and these casefiles plus information from households' own returns to measure eligibility and participation among filers. We find that nearly 150,000 actual tax units failed to claim the state EITC, totaling almost $20 million in unclaimed state benefits. We also find almost 290,000 CalFresh households were eligible for the CalEITC but did not file a state return, forgoing an estimated $56 million dollars in CalEITC benefits. The overall household-level take-up rate of the California EITC within the CalFresh population is 53%. The average unclaimed benefit for both filers and non-filers is fairly low. Participation is lower for childless adults, for filers who use paid preparers, and among Black and Hispanic individuals.

All of our estimates pertain only to the population of CalFresh participants. An important avenue for future work would be replicating this analysis using a fuller array of state administrative data – namely, Medicaid case files. Not only are many more households enrolled in Medicaid than SNAP, providing researchers a larger snapshot of the possibly eligible population, Medicaid cases tend to more closely resemble tax units. Using Medicaid case composition information would also improve upon our processes for associating filers with partners and dependents.

In addition to providing the first estimate of take-up of a state EITC program, a principal contribution of this paper is to provide a roadmap to other researchers so they can measure EITC participation in their own states.

Figures

Figure 3.1: Federal and California EITC schedules for a single-parent family with two children, tax year 2017



Notes. This diagram illustrates the federal (blue) and state (gold) EITC schedules for a head of household with two children. The gray line illustrates the combined value of the EITC for a filer who claims both credits. The dotted line denotes the filing threshold for a head of household in tax year 2018, which was $18,000; families with incomes below this threshold are generally not required to file returns.

Tables

Table 3.1: Summary statistics for tax filer sample

	By number of dependents				
	0	**1**	**2**	**3+**	**Total**
Count of Individuals	13,071,385	6,610,757	7,633,885	6,672,745	33,988,772
Count of Tax Units	10,355,163	2,817,791	2,181,257	1,347,900	16,702,111
Count of Tax Units That E-Filed	8,926,909	2,502,564	1,952,585	1,206,975	14,589,033
% E-Filed	86%	89%	90%	90%	87%
Statistics for e-filers (tax unit level)					
Filing Status					
Single	72%	12%	7%	6%	48%
Married Filing Jointly	26%	42%	60%	64%	37%
Married Filing Separately	1%	1%	1%	0%	1%
Head of Household	0%	45%	32%	29%	15%
EITC Claiming					
% claiming federal EITC	7.4	34.0	32.8	32.1	17.4
Mean federal EITC claim (if positive)	$335	$2,317	$3,516	$3,835	$2,335
% claiming CalEITC	7%	17%	13%	10%	9%
Mean CalEITC claim (if positive)	$76	$290	$526	$573	$266
% claiming either EITC	8%	34%	33%	32%	18%
Mean total EITC (if positive)	$394	$2,455	$3,721	$4,012	$2,464
Income					
Mean earnings	$49,218	$73,065	$101,194	$82,917	$63,053
Mean AGI	$76,165	$90,063	$125,567	$105,820	$87,614

Notes. Universe is all state tax returns in tax year 2017. Other than row 1, all statistics are at the tax unit level.

Table 3.2: Summary statistics for CalFresh sample

	By case-level filing status			
	Everyone files	**Some file**	**No one files**	**Total**
Number of individuals	3,032,005	1,079,949	1,505,795	5,617,749
Row percent	54%	19%	27%	100%
Mean case size				
Total	2.2	3.6	1.4	2.0
Adults	1.1	1.4	1.0	1.1
Children	1.1	2.2	0.3	0.9
Share of adults w. linked wages	68%	53%	28%	51%
Mean total EDD wages (if positive)	$15,919	$13,803	$9,197	$14,229
Tax return linkage				
Linked to tax return	100%	50%	0%	55%
Linked to federal EITC tax return	57%	33%	0%	32%
Mean federal EITC amount (if positive)	$3,017	$3,698	.	$3,094
Mean CalEITC amount (if positive)	$378	$509	.	$392

Notes. Universe is all CalFresh recipients in tax year 2017. Statistics are reported at the reference-case level; see Section 3.4 for more information about how these are defined. Column 1 reports means for cases in which every member appears on a 2017 state tax return. Column 2 reports means for cases in which at least one member, but not all, appear on a 2017 state tax return. Column 3 reports means for cases in which no members appear on a 2017 tax return.

Table 3.3: Claiming of federal and state EITC among all CalFresh tax units

	Total tax units	Fed EITC Claimants			CalEITC Claimants		
	Count	Count	Share	Amount	Count	Share	Amount
For all filers							
Total	1,110,984	766,596	69%	$3,081	504,230	45%	$396
By filing status and number of dependents							
Single							
0 dependents	351,882	113,314	32%	$340	108,696	31%	$88
1 dependent	236,994	215,584	91%	$2,665	159,290	67%	$355
2 dependents	174,204	166,082	95%	$4,124	108,989	63%	$625
3+ dependents	103,867	98,713	95%	$4,581	55,122	53%	$668
Married							
0 dependents	41,221	15,924	39%	$391	11,151	27%	$80
1 dependent	48,269	37,082	77%	$2,649	19,304	40%	$287
2 dependents	69,532	55,387	80%	$4,098	22,306	32%	$523
3+ dependents	85,015	64,510	76%	$4,350	19,372	23%	$555
By total earnings (thousands)							
$0-5	155,215	88,617	57%	$1,004	81,202	52%	$474
$5-10	189,622	143,054	75%	$2,127	136,061	72%	$732
$10-15	222,500	180,767	81%	$3,582	165,994	75%	$300
$15-20	168,681	115,493	68%	$4,659	90,214	53%	$121
$20-25	124,913	87,031	70%	$4,190	30,542	24%	$31
$25-30	85,532	63,023	74%	$3,494	107	0%	$354
$30+	164,521	88,611	54%	$2,240	110	0%	$430

Notes. Universe is e-filed tax returns linked to at least one CalFresh participant. Column 1 reports the total number of tax units in each cell. Column 2 reports the count of those tax units that claimed the federal EITC. Column 3 reports what share of all returns in each cell claimed the federal EITC. Column 4 reports the average claimed amount of the federal EITC for each cell. Column 5s through 7 report the same statistics but for the state EITC.

Table 3.4: Data sources for measuring three components of eligibility across three populations

	Eligibility components		
	Filing Status	**Earned income**	**Qualifying children**
Filers			
Fed EITC claimants	Filing status (540, Lines 1-5)	CA wages (540, Line 12) AGI (540, Line 13) Investment (1040, Lines 13, 14, 17) Self-employment (1040, Lines 12, 17, 27)	CA wages (540, Line 12) AGI (540, Line 13) Fed EITC amt (1040, Line 66a) Fed EITC amt (Sch 3514, Line 3)
Non Fed EITC claimants	Filing status (540, Lines 1-5)	CA wages (540, Line 12) AGI (540, Line 13) Investment (1040, Lines 13, 14, 17) Self-employment (1040, Lines 12, 17, 27)	CA wages (540, Line 12) AGI (540, Line 13) # of deps (540, Line 10) CalFresh casefiles
Non-Filers			
	CalFresh casefiles Participants' ages	EDD wages	CalFresh casefiles Participants' ages

Notes. Table 3.4 summarizes the data sources we use to measure the three components of eligibility across our three populations. Forms and line numbers are applicable to 2017 returns. "CalFresh casefiles" refers to our ability to observe individuals sharing CalFresh cases with each other for a certain number of months in the tax year.

Table 3.5: Relationship between the number of dependents and number of qualifying children among CalFresh households claiming the federal EITC

	Dependents claimed on tax return			
	0	**1**	**2**	**3+**
Number of EITC qualifying children				
0	98.9%	2.1%	0.4%	0.1%
1	0.8%	97.4%	7.9%	1.7%
2	0.2%	0.4%	91.5%	8.8%
3+	0.1%	0.1%	0.3%	89.4%
N	126,642	249,089	218,151	161,068

Notes. Universe is tax units that e-filed their returns, included at least one CalFresh participant, and had a positive federal EITC claim. Cells represent column percentages.

Table 3.6: Eligibility and take-up of the CalEITC among CalFresh filers who claimed the federal EITC

	Total tax units	CalEITC eligible		Eligible non-claimants		
	Count	Count	Share	Count	Share	Amount
For all filers						
Total	754,950	534,406	71%	42,024	8%	$233
By filing status and number of qualifying children						
Single						
0 QCs	114,737	113,701	99%	5,607	5%	$64
1 QC	224,558	176,198	78%	14,289	8%	$220
2 QCs	159,830	113,405	71%	10,134	9%	$326
3+ QCs	85,611	53,829	63%	5,347	10%	$310
Married						
0 QCs	16,676	12,125	73%	620	5%	$71
1 QCs	38,970	21,459	55%	1,817	8%	$151
2 QCs	55,223	23,873	43%	2,218	9%	$247
3+ QCs	59,345	19,816	33%	1,992	10%	$230
By total earnings (thousands)						
$0-$5	83,266	78,551	94%	2,609	3%	$416
$5-$10	140,411	139,194	99%	6,594	5%	$571
$10-$15	179,530	176,178	98%	12,078	7%	$247
$15-$20	114,685	104,297	91%	14,787	14%	$121
$20-$25	86,407	36,186	42%	5,956	16%	$28
$25-$30	62,638	0	0%	0	.	.
$30+	88,013	0	0%	0	.	.

Notes. Universe is e-filed tax returns linked to at least one CalFresh participant that included a claim for a non-zero federal EITC. The number of qualifying children in each tax unit was calculated using the process described in Section 3.4. Column 1 reports the total number of tax units that meet those criteria. Column 2 reports the count of those tax units that were eligible for the California EITC. Column 3 reports what share of all returns were eligible for the CalEITC. Column 5 reports the number of eligible returns that did not claim the CalEITC, and Column 6 reports the share of eligible units that did not claim. Column 7 reports the average imputed amount among those non-claimers for each cell.

Table 3.7: Relationship between number of dependents and imputed number of qualifying children among CalFresh filers that did not claim federal EITC

	Dependents claimed on tax return			
	0	**1**	**2**	**3+**
Number of EITC qualifying children				
0	100.0%	43.7%	17.8%	7.2%
1	0.0%	56.3%	17.5%	7.1%
2	0.0%	0.0%	64.6%	16.9%
3+	0.0%	0.0%	0.0%	68.8%
N	263,865	32,597	22,267	25,659

Notes. Universe is tax units that e-filed their returns, included at least one CalFresh participant, and did not claim the federal EITC. Cells represent column percentages.

Table 3.8: Eligibility and take-up of the CalEITC among CalFresh filers who did not claim the federal EITC

	Total tax units	**CalEITC eligible**		**Eligible non-claimants**		
	Count	Count	Share	Count	Share	Amount
For all filers						
Total	344,388	112,881	33%	109,786	97%	$84
By filing status and number of qualifying children						
Single						
0 QCs	252,388	107,442	43%	105,012	98%	$82
1 QC	12,724	2,196	17%	1,869	85%	$175
2 QCs	5,159	337	7%	228	68%	$318
3+ QCs	2,983	96	3%	70	73%	$434
Married						
0 QCs	31,550	2,493	8%	2,367	95%	$73
1 QC	11,341	162	1%	136	84%	$267
2 QCs	13,560	107	1%	73	68%	$491
3+ QCs	14,683	48	0%	31	65%	$574
By total earnings (thousands)						
$0-5	66,598	35,861	54%	34,814	97%	$140
$5-10	46,568	42,203	91%	41,197	98%	$81
$10-15	41,733	33,136	79%	32,238	97%	$30
$15-20	53,188	1,164	2%	1,048	90%	$87
$20-25	37,882	517	1%	489	95%	$23
$25-30	22,509	0	0%	0	.	.
$30+	75,910	0	0%	0	.	.

Notes. Universe is e-filed tax returns linked to at least one CalFresh head or spouse that did not include a federal EITC claim. The number of qualifying children in each tax unit was calculated using the process described in Section 3.4. Column 1 reports the total number of tax units that meet those criteria. Column 2 reports the count of those tax units that were eligible for the California EITC. Column 3 reports what share of all returns were eligible for the CalEITC. Column 5 reports the number of eligible returns that did not claim the CalEITC, and Column 6 reports the share of eligible units that did not claim. Column 7 reports the average imputed amount among those non-claimers for each cell.

Table 3.9: Relationship between number of dependents and imputed number of qualifying children among CalFresh imputed tax units that did not file return

	Dependents claimed on imputed tax return			
	0	**1**	**2**	**3+**
Number of EITC qualifying children				
0	100.0%	19.8%	7.1%	6.9%
1	0.0%	80.2%	9.5%	1.3%
2	0.0%	0.0%	83.4%	7.3%
3+	0.0%	0.0%	0.0%	84.6%
N	610,644	104,958	52,178	35,924

Notes. Universe is simulated tax returns including only non-filing CalFresh participants. These tax units are constructed using the process described in Section 3.4. Cells represent column percentages.

Table 3.10: Eligibility and take-up of the CalEITC among non-filers

	Total tax units	**CalEITC eligible**		**Eligible non-claimants**		
	Count	Count	Share	Count	Share	Amount
For all filers						
Total	803,724	289,765	36%	289,765	100%	$196
By filing status and number of qualifying children						
Single						
0 QCs	600,388	226,703	38%	226,703	100%	$87
1 QC	83,587	29,018	35%	29,018	100%	$489
2 QCS	40,473	13,431	33%	13,431	100%	$827
3+ QCs	24,691	7,455	30%	7,455	100%	$920
Married						
0 QCs	37,222	7,264	20%	7,264	100%	$85
1 QC	6,030	2,012	33%	2,012	100%	$434
2 QCs	5,645	1,990	35%	1,990	100%	$799
3+ QCs	5,688	1,892	33%	1,892	100%	$873
By total earnings (thousands)						
$0	435,797	0	0%	0	.	.
$1-$5	178,585	175,666	98%	175,666	100%	$180
$5-$10	65,929	64,693	98%	64,693	100%	$305
$10-$15	41,383	40,677	98%	40,677	100%	$109
$15-$20	27,449	6,009	22%	6,009	100%	$117
$20-$25	19,771	2,720	14%	2,720	100%	$29
$25-$30	12,141	0	0%	0	.	.
$30+	22,669	0	0%	0	.	.

Notes. Universe is simulated tax returns including only non-filing CalFresh participants. These tax units are constructed using the process described in Section 3.4. The number of qualifying children in each imputed tax unit was calculated using the process described in Section 3.4. Column 1 reports the total number of tax units that meet those criteria. Column 2 reports the count of those tax units that were eligible for the California EITC. Column 3 reports what share of all returns were eligible for the CalEITC. Column 5 reports the number of eligible returns that did not claim the CalEITC, and Column 6 reports the share of eligible units that did not claim. Column 7 reports the average imputed amount among those non-claimers for each cell.

Table 3.11: Summing up CalEITC take-up among CalFresh recipients

	Total	**CalEITC eligible**		**Eligible non-claimants**			
	Count	Count	Share	Count	Share	Mean Amount	Total Amount
Filers							
Fed EITC claimants	754,950	534,406	71%	42,024	8%	\$233	\$9,791,892
Non Fed EITC claimants	344,388	112,881	33%	109,786	97%	\$84	\$9,275,297
Non-Filers							
All	803,724	289,765	36%	289,765	100%	\$196	\$56,651,396
Total	1,903,062	402,646	49%	441,575	47%	\$171	\$75,718,585

Notes. Table 3.11 compiles information from earlier tables; see those tables for details. The addition is the final column which reports the total unclaimed dollars for each population. Cells represent column percentages.

Table 3.12: Simulated federal EITC eligibility among CalFresh filers

	Tax units	Fed EITC eligible		Eligible non-claimants			
	Count	Count	Share	Count	Share	Mean Amount	Total Amount
Filers							
CalEITC claimant	504,230	501,232	99%	3,069	1%	$714	$2,190,355
Non CalEITC claimant	606,754	369,209	61%	106,433	29%	$415	$44,122,653
Total	1,110,984	870,441	78%	109,502	13%	$423	$46,313,008

Notes. Table summarizes rates of eligibility for the federal EITC among e-filed tax returns with a CalFresh-enrolled head or spouse. Results are separated between units that included a CalEITC claim and those that did not.

Table 3.13: CalEITC take-up among CalFresh recipients by tax preparation method

	Tax Preparation Method			
	Paid	**Self**	**VITA**	**Total**
Number of Tax Returns	671,335	377,819	48,006	1,097,160
Claimed the Federal EITC				
Number of tax returns	474,790	251,232	27,647	753,669
CalEITC				
% eligible	69%	74%	79%	71%
% non-claiming among eligible	12.1%	1.6%	0.4%	7.9%
Mean unclaimed amount	$232	$252	$145	$233
Total unclaimed amount	$8,998,985	$757,609	$12,789	$9,769,383
Did Not Claim the Federal EITC				
Number of Tax Returns	196,545	126,587	20,359	343,491
CalEITC				
% eligible	31%	35%	32%	33%
% non-claiming among eligible	99%	94%	100%	97%
Mean unclaimed amount	$85	$83	$90	$84
Total unclaimed amount	$5,156,249	$3,504,335	$594,014	$9,254,598
Federal EITC				
% eligible	30%	36%	26%	32%
% non-claiming among eligible	100%	100%	100%	100%
Mean unclaimed amount	$425	$426	$372	$423
Total unclaimed amount	$24,871,421	$19,410,396	$1,940,375	$46,222,192

Notes. Universe is e-filed tax returns linked to at least one CalFresh participant. The top panel is restricted to tax returns in which there was a positive federal EITC claim, and the bottom panel is restricted to returns with no federal EITC claim. For each population, we report the number of returns filed via each of the three preparation methods: paid preparer, self-prepared, or VITA. For each method, we report the share of returns that appeared eligible for the CalEITC (and the federal EITC for those that did not claim the Federal EITC), and among those deemed eligible, the share that did not claim. For eligible non-claimers, we also report the mean unclaimed amount and the total unclaimed dollars. The share of eligible non-claimants among tax units who claimed the federal EITC and used VITA is 0.4 percent, which we round down to 0%. A very small number do not claim the state eitc, and the average unclaimed amount for this group was $145 and the total was $12,789.

Table 3.14: CalEITC eligibility and participation by race

	AIAN	Asian	Asian/PI	Black	Hispanic	NHPI	Other/ Unknown	White	Total
Within actual tax units									
Heads and Spouses									
Number of individuals	6,459	81,395	15,912	146,976	589,561	4,275	145,188	276,927	1,266,693
% eligible	58%	55%	51%	67%	55%	53%	55%	55%	56%
% non-claiming among eligible	19%	20%	19%	18%	25%	22%	20%	20%	22%
Mean unclaimed amount	$137	$108	$105	$115	$114	$136	$110	$111	$113
Dependents									
Number of individuals	4,654	53,691	11,500	138,062	652,631	4,287	124,972	191,880	1,181,677
% eligible	57%	52%	48%	67%	51%	47%	55%	53%	54%
% non-claiming among eligible	8%	7%	7%	8%	11%	9%	7%	7%	9%
Mean CalEITC amount	$312	$229	$181	$246	$220	$269	$251	$271	$232
Within imputed tax units									
Heads and Spouses									
Number of individuals	8,959	33,552	8,019	146,689	272,572	3,585	99,644	285,229	858,249
% eligible	31%	20%	26%	43%	39%	36%	34%	30%	35%
% non-claiming among eligible	100%	100%	100%	100%	100%	100%	100%	100%	100%
Mean unclaimed amount	$61	$42	$63	$86	$86	$92	$64	$57	$72
Dependents									
Number of individuals	3,027	9,094	3,117	59,143	142,773	1,610	36,920	79,721	335,405
% eligible	26%	22%	27%	37%	32%	35%	31%	29%	32%
% non-claiming among eligible	100%	100%	100%	100%	100%	100%	100%	100%	100%
Mean CalEITC amount	$160	$144	$188	$261	$226	$262	$204	$184	$217
Overall take-up gap									
Heads and Spouses									
% non-claiming among eligible	54%	30%	36%	50%	44%	50%	44%	49%	45%
Dependents									
% non-claiming among eligible	29%	13%	20%	25%	22%	29%	20%	24%	22%

Notes. The universe for the top panel is e-filed tax returns linked to at least one CalFresh participant. The universe for the bottom panel is simulated tax units only containing non-filing CalFresh participants. Within each race category, we report the share of heads/spouses and dependents (either as reported on their tax return for those who filed or as predicted via the process described in Appendix B.4) within each race category who were eligible for the CalEITC. Among those eligible, we also report the share that did not claim the CalEITC and the mean imputed amount for these eligible non-claimers. Column 1 reports statistics for American Indian and Alaskan Native enrollees. Column 2 reports statistics for Asian enrollees. Column 3 reports statistics for Asian and/or Pacific Islander enrollees. Column 4 reports statistics for Black enrollees. Column 5 reports statistics for Hispanic enrollees. Column 6 reports statistics for Native Hawaiian and/or Pacific Islander enrollees. Column 7 reports statistics for those whose race is reported as Other or Unknown. Column 8 reports statistics for White enrollees. We do not report statistics for the less than 1% of enrollees who are associated with different race categories across copies of our MEDS data. See Section 3.5 for more information about the data we use and how we group enrollees into these categories.

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Appendix A

Can Nudges Increase Take-up of the EITC?: Evidence from Multiple Field Experiments

Appendix Table A.1 lists the assignment rates and stratification used in each study. Appendix Table A.2 lists the minimum detectable effects stated in the pre-analysis plan for each study.

Appendix Tables A.3 to A.8 present full balance checks for each of the available baseline covariates and each treatment. There is one table for each study. In each, the first column shows the control group mean and standard deviation. The next series of columns, one for each treatment arm, shows the estimated difference between that treatment arm and the control group, along with standard errors. These are the β_j coefficients from Equation 2.2, using the baseline covariates as dependent variables. The final column of each table shows a p-value for the hypothesis that the means are the same in each treatment group as in the control; that is, that all of the β_js are jointly zero. Very few of the estimated β_js are statistically significant, evaluated individually, and all of the estimated effects are substantively small. None of our tests indicate joint significance for any study or covariate.

The final columns of Appendix Tables A.3 to A.8 present estimated treatment effects on our primary outcomes, tax filing and EITC claiming. These are again based on Equation 2.2, and correspond to the individual treatment arm effects plotted in Figure 2.3.

Appendix Figures A.1 and A.2 use the CalFresh data from Study 6 to simulate annual incomes and EITC credit eligibility for each family in our sample. We plot the histograms of those two measures, separately for the subsets of the control group who do and do not file tax returns. These figures indicate that filers have much higher incomes and higher credit eligibility than non-filers, on average. Twenty-five percent of non-filers are eligible for credits of $300 or less, as compared with 9% of filers. Nevertheless, 75% of non-filers are eligible for credits in excess of $300 – enough to outweigh the potential financial costs of paying a tax preparer to file returns (U.S. Government Accountability Office 2014, National Society of Accountants n.d.).

The various treatment letters and text messages are reproduced in Appendix Figures A.3 to A.18.

Appendix Table A.1: Randomization details by study

Study	Arms	N (households)	Assignment frequencies / proportions	Strata
1	1	639,244	200,000 control; remainder treatment*	Zip code, dummies for white/nonwhite and college/non-college
2	4	96,370	10,000 to each treatment arm; remainder control	County, zip code, missing DOB, Study 1 assignment
3	4	1,084,018	20% to each treatment arm; 20% control	County, zip code, missing DOB, Study 1 and Study 2 assignment
4	8	204,285	15,000 to each treatment group; remainder control	County, zip code, Study 1 assignment and Study 3 assignment
5	1	38,093	50% treatment; 50% control	County, race, single adult, four bins of income, presence of children
6	3	47,102	25% to each treatment arm; 25% control	County, presence of children, low or moderate income, primary language

Notes. Study 1 used the TargetSmart records updated through spring 2018, and excluded those older than 70. Studies 2 and 3 included older individuals and those added in the 2019 update. Study 2 limited to a subset of records in six counties (Alameda, Contra Costa, Marin, Riverside, San Francisco, San Mateo, and Santa Clara). For Studies 1-4, some zip codes contained too few observations to assign observations to treatment and control with nearly equal probabilities. We grouped observations in these less populous zip codes to a simulated county-level ”zip code”, and used this simulated zip code for stratification. For Study 5, we stratified on four income bins: $0 to $5000, $5000 to $11,500, $11,500 to $20,000 and $20,000 to $50,000. The *race* variable used for stratification had four values: White, Latinx, Black/Asian/PI/American Indian/Alaskan Native. The indicator for *single adult* reflected whether we identified the case as having one working-age adult or more than one working-age adult. For Study 6, we stratified on two broader income bins: $0 to $12,500 and $12,500 to $55,000; primary languages were the four languages in which we distributed the messages, English, Chinese, Spanish, and Vietnamese.

Appendix Table A.2: Pre-registration minimum detectable effect estimates

Study	**OSF ID**	MDE Any treatment vs. control	MDE Individual treatment arm vs. control
1	https://osf.io/ct58w	0.3 percentage point	n/a
2	https://osf.io/z8ebc	0.8 pp	1.4 pp
3	https://osf.io/z8ebc	0.3 pp	0.4 pp
4	https://osf.io/z8ebc	0.6 pp	1.1 pp
5	https://osf.io/msh7t	1.6 pp	n/a
6	https://osf.io/p2q4y	1.4 pp	1.7 pp

Notes. The pre-analysis plan for study 5 was posted before the eventual sample size was known, and does not contain a power calculation. We estimate this MDE in the same way that we estimated the MDE for Study 6, as described in that study's posted pre-analysis plan. Studies 2-4 are all described in the same pre-analysis plan. We used different numbers to identify those experiments than we do here. Study 3 corresponds to what we called in the pre-analysis plan Experiment 1, Study 4 corresponds to Experiment 2, and Study 2 corresponds to Experiment 3.

Appendix Table A.3: Differences in subjects' characteristics by treatment assignment in Study 1

	Control Group Average (mean/sd)	Basic Text	P-value from F-test: β=0
Baseline Characteristics			
Age	47.250***	0.017	0.674
	(13.712)	(0.041)	
Male	0.424	-0.000	0.991
	(0.494)	(0.001)	
White	0.424	-0.000	0.526
	(0.494)	(0.000)	
Married	0.187	-0.001	0.645
	(0.390)	(0.001)	
Have Children	0.586	0.002	0.281
	(0.493)	(0.002)	
College Grad	0.377	-0.002	0.123
	(0.485)	(0.001)	
Filed Early	0.219	-0.001	0.641
	(0.413)	(0.001)	
Treatment Effects			
Filed Return	0.404	-0.000	0.996
	(0.491)	(0.001)	
Claimed EITC	0.087	0.000	0.967
	(0.282)	(0.001)	
N	195,199	444,044	

Notes. Standard errors in parentheses; standard deviations in square brackets. Asterisks indicate statistical significance of individual coefficients; *=10%, **=5%, ***=1%. The p-values in the final column are for tests of the hypothesis that the treatment effect on the indicated characteristic or outcome is zero.

Appendix Table A.4: Differences in subjects' characteristics by treatment assignment in Study 2

	Control Group Average (mean/sd)	FTB formal	FTB informal	GSO formal	GSO informal	P-value from F-test: β=0
Baseline Characteristics						
Age	43.499***	-0.175	0.107	-0.116	0.227	0.169
	(11.484)	(0.140)	(0.140)	(0.140)	(0.140)	
Male	0.628	-0.003	0.004	-0.001	-0.001	0.922
	(0.683)	(0.006)	(0.006)	(0.006)	(0.006)	
White	0.443	0.001	0.001	0.005	0.002	0.901
	(0.497)	(0.005)	(0.005)	(0.005)	(0.005)	
Married	0.241	0.007	-0.003	0.002	0.006	0.188
	(0.520)	(0.004)	(0.004)	(0.004)	(0.004)	
Have Children	1.159	0.008	-0.000	-0.001	-0.002	0.724
	(0.762)	(0.006)	(0.006)	(0.006)	(0.006)	
College Grad	0.213	0.004	0.003	-0.002	0.010	0.484
	(0.409)	(0.006)	(0.006)	(0.006)	(0.006)	
Filed Early	0.115	0.002	0.003	-0.001	-0.007*	0.245
	(0.319)	(0.003)	(0.003)	(0.003)	(0.003)	
Filed in 2017	0.377	0.001	0.004	-0.000	-0.001	0.934
	(0.485)	(0.005)	(0.005)	(0.005)	(0.005)	
Treatment Effects						
Filed Return	0.377	0.001	-0.001	-0.000	-0.007	0.644
	(0.485)	(0.005)	(0.005)	(0.005)	(0.005)	
Claimed EITC	0.077	-0.003	0.004	-0.000	0.000	0.513
	(0.266)	(0.003)	(0.003)	(0.003)	(0.003)	
N	56,370	10,000	10,000	10,000	10,000	

Notes. Standard errors in parentheses; standard deviations in square brackets. Asterisks indicate statistical significance of individual coefficients; *=10%, **=5%, ***=1%. The p-values in the final column are for tests of the hypothesis that all of the treatment coefficients (the βs in equation (2)) are jointly zero.

Appendix Table A.5: Differences in subjects' characteristics by treatment assignment in Study 3

	Control Group Average (mean/sd)	Basic Info	211 Info	Text Assistance	Credit Amt	P-value from F-test: β=0
Baseline Characteristics						
Age	59.930**	-0.042	0.017	-0.038	0.033	0.476
	(21.096)	(0.050)	(0.050)	(0.050)	(0.050)	
Male	0.550	-0.005**	-0.002	-0.002	-0.002	0.055
	(0.640)	(0.002)	(0.002)	(0.002)	(0.002)	
White	0.548	-0.001	-0.001	-0.002	-0.001	0.700
	(0.498)	(0.001)	(0.001)	(0.001)	(0.001)	
Married	0.269	-0.001	0.000	-0.001	-0.001	0.702
	(0.512)	(0.001)	(0.001)	(0.001)	(0.001)	
Have Children	0.946	-0.001	-0.002	-0.002	-0.002	0.666
	(0.821)	(0.002)	(0.002)	(0.002)	(0.002)	
College Grad	0.223	-0.001	-0.001	-0.001	-0.001	0.923
	(0.416)	(0.002)	(0.002)	(0.002)	(0.002)	
Filed Early	0.111	-0.001	-0.000	-0.000	-0.000	0.659
	(0.314)	(0.001)	(0.001)	(0.001)	(0.001)	
Filed in 2017	0.411	-0.001	-0.001	-0.000	0.000	0.875
	(0.492)	(0.001)	(0.001)	(0.001)	(0.001)	
Treatment Effects						
Filed Return	0.390	-0.001	-0.002	-0.002	-0.001	0.583
	(0.488)	(0.001)	(0.001)	(0.001)	(0.001)	
Claimed EITC	0.063	-0.000	-0.002*	-0.001	0.000	0.017
	(0.244)	(0.001)	(0.001)	(0.001)	(0.001)	
N	216,804	216,804	216,804	216,803	216,803	

Notes. Standard errors in parentheses; standard deviations in square brackets. Asterisks indicate statistical significance of individual coefficients; *=10%, **=5%, ***=1%. The p-values in the final column are for tests of the hypothesis that all of the treatment coefficients (the βs in equation (2)) are jointly zero.

Appendix Table A.6: Differences in subjects' characteristics by treatment assignment in Study 4

	Control Group Average (mean/sd)	Formal/ Baseline	Formal/ Amount	Formal/ VITA	Formal/ Amount+VITA	Informal/ Baseline	Informal/ Amount	Informal/ VITA	Informal/ Amount+VITA	P-value from F-test: β=0
Baseline Characteristics										
Age	44.106***	0.081	0.129	-0.027	0.057	-0.216	-0.016	0.293*	-0.059	0.288
	(11.789)	(0.138)	(0.138)	(0.139)	(0.138)	(0.139)	(0.139)	(0.138)	(0.139)	
Male	0.708	-0.003	-0.001	-0.005	0.014**	-0.001	-0.004	-0.005	-0.002	0.116
	(0.735)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
White	0.474	-0.006	0.009*	-0.001	-0.010*	-0.004	-0.001	0.005	-0.001	0.023
	(0.499)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	
Married	0.152	0.002	0.002	-0.000	0.003	-0.001	0.003	-0.002	-0.000	0.709
	(0.460)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
Have Children	1.390	-0.000	-0.007	-0.008	0.002	-0.008	-0.002	-0.010	-0.012	0.501
	(0.759)	(0.006)	(0.006)	(0.007)	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)	
College Grad	0.157	0.010	0.004	0.001	0.003	-0.002	0.012*	-0.003	0.001	0.345
	(0.364)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
Filed Early	0.015	-0.002	-0.000	-0.001	-0.001	-0.000	0.000	0.000	-0.002	0.489
	(0.120)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
Filed in 2017	0.036	-0.003	0.000	-0.001	-0.001	-0.003	-0.002	-0.001	-0.002	0.407
	(0.186)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
Treatment Effects										
Filed Return	0.090	-0.001	-0.000	-0.003	-0.001	-0.002	0.000	-0.004	-0.002	0.874
	(0.286)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
Claimed EITC	0.024	-0.000	-0.001	-0.001	-0.001	-0.000	-0.001	-0.001	-0.000	0.999
	(0.154)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
N	85,355	14,863	14,892	14,880	14,836	14,867	14,862	14,880	14,850	

Notes. Standard errors in parentheses; standard deviations in square brackets. Asterisks indicate statistical significance of individual coefficients; *=10%, **=5%, ***=1%. The p-values in the final column are for tests of the hypothesis that all of the treatment coefficients (the βs in equation (2)) are jointly zero.

Appendix Table A.7: Differences in subjects' baseline characteristics by treatment assignment, Study 5, TY2017

	Control Group Average (mean/sd)	Basic Text	P-value from F-test: $\beta = 0$
Baseline Characteristics			
Age	36.95***	0.01	0.94
	(11.07)	(0.10)	
Male	0.36	0.00	0.96
	(0.48)	(0.00)	
White	0.27	-0.00	0.94
	(0.45)	(0.00)	
English	0.86*	0.00	0.65
	(0.35)	(0.00)	
Presence of Other Adults	0.34	-0.00	0.48
	(0.47)	(0.00)	
Presence of Children	0.73	0.00	0.98
	(0.44)	(0.00)	
Filed Previous Year	0.70	0.01	0.20
	(0.46)	(0.00)	
Filed before treatment started	0.67	0.00	0.75
	(0.47)	(0.00)	
Predicted Total EITC Amount	2915.16	-2.89	0.80
	(2155.56)	(11.24)	
Treatment Effects			
Filed Return	0.79	0.01	0.17
	(0.41)	(0.00)	
Claimed EITC	0.70	0.00	0.91
	(0.46)	(0.00)	
N	19,009	19,084	

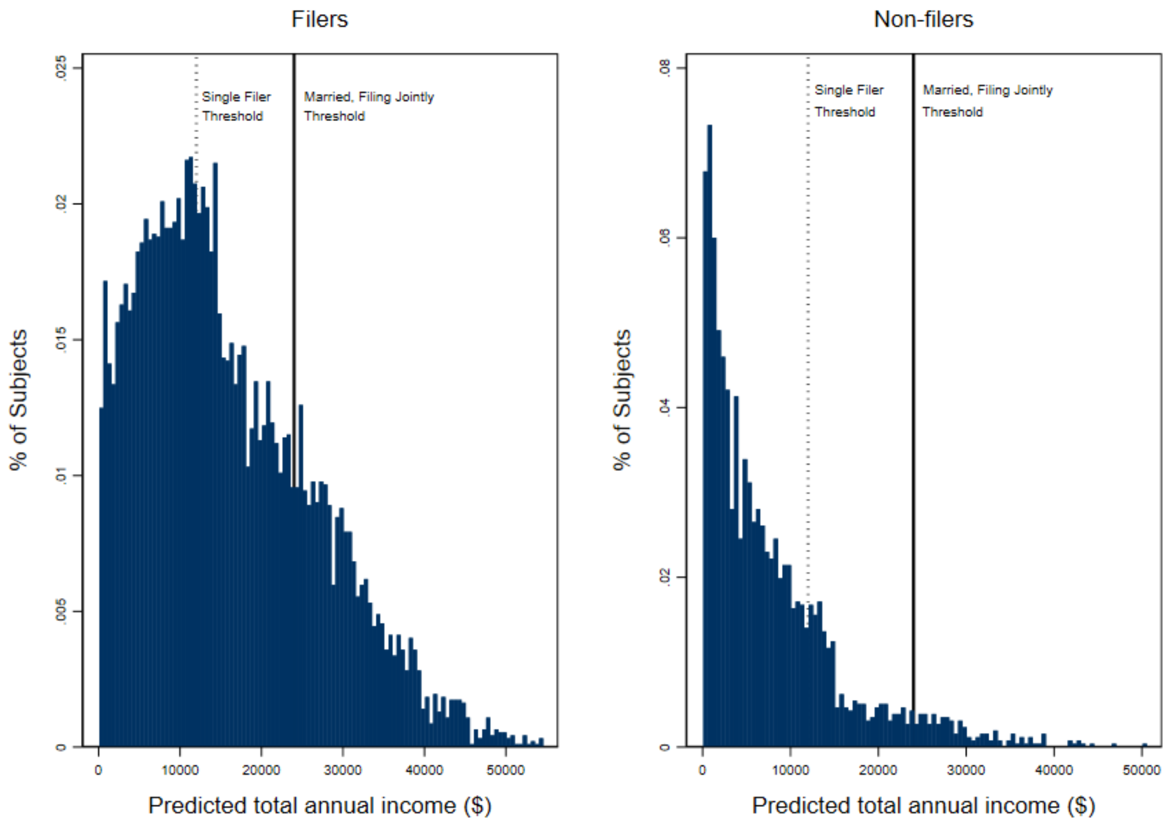
Notes. Standard errors in parentheses; standard deviations in square brackets. Asterisks indicate statistical significance of individual coefficients; *=10%, **=5%, ***=1%. The p-values in the final column are for tests of the hypothesis that the treatment effect on the indicated characteristic or outcome is zero.

Appendix Table A.8: Differences in subjects' baseline characteristics by treatment assignment, Study 6, TY2018

	Control Group Average (mean/sd)	Basic Info	Average Credit Amount	Personalized Credit Amt	P-value from F-test: $\beta = 0$
Baseline Characteristics					
Age	36.98**	-0.31*	-0.01	-0.02	0.10
	(12.38)	(0.15)	(0.15)	(0.15)	
Male	0.37	-0.01	-0.01	-0.01	0.62
	(0.48)	(0.01)	(0.01)	(0.01)	
White	0.24	-0.01	0.00	-0.01	0.27
	(0.43)	(0.01)	(0.01)	(0.01)	
English	0.75	0.00	0.00	0.00	0.59
	(0.43)	(0.00)	(0.00)	(0.00)	
Presence of other adults	0.31	-0.01	-0.00	-0.01	0.31
	(0.46)	(0.01)	(0.01)	(0.01)	
Number of children	1.24	-0.01	0.00	0.00	0.45
	(1.06)	(0.01)	(0.01)	(0.01)	
Filed previous year	0.74	-0.00	-0.01	-0.01	0.66
	(0.44)	(0.01)	(0.01)	(0.01)	
Filed before treatment started	0.43	-0.00	-0.01	-0.01	0.29
	(0.50)	(0.01)	(0.01)	(0.01)	
Predicted annual income	14199.09	-71.70	-22.77	10.58	0.73
	(10641.75)	(78.05)	(78.06)	(78.05)	
Predicted total EITC amount	2702.15	11.99	24.71	16.68	0.61
	(2162.98)	(18.86)	(18.86)	(18.86)	
Treatment Effects					
Filed return	0.78	-0.00	-0.00	0.00	0.74
	(0.41)	(0.00)	(0.00)	(0.00)	
Claimed EITC	0.70	-0.00	0.00	-0.00	0.75
	(0.46)	(0.01)	(0.01)	(0.01)	
N	11,771	11,776	11,773	11,782	

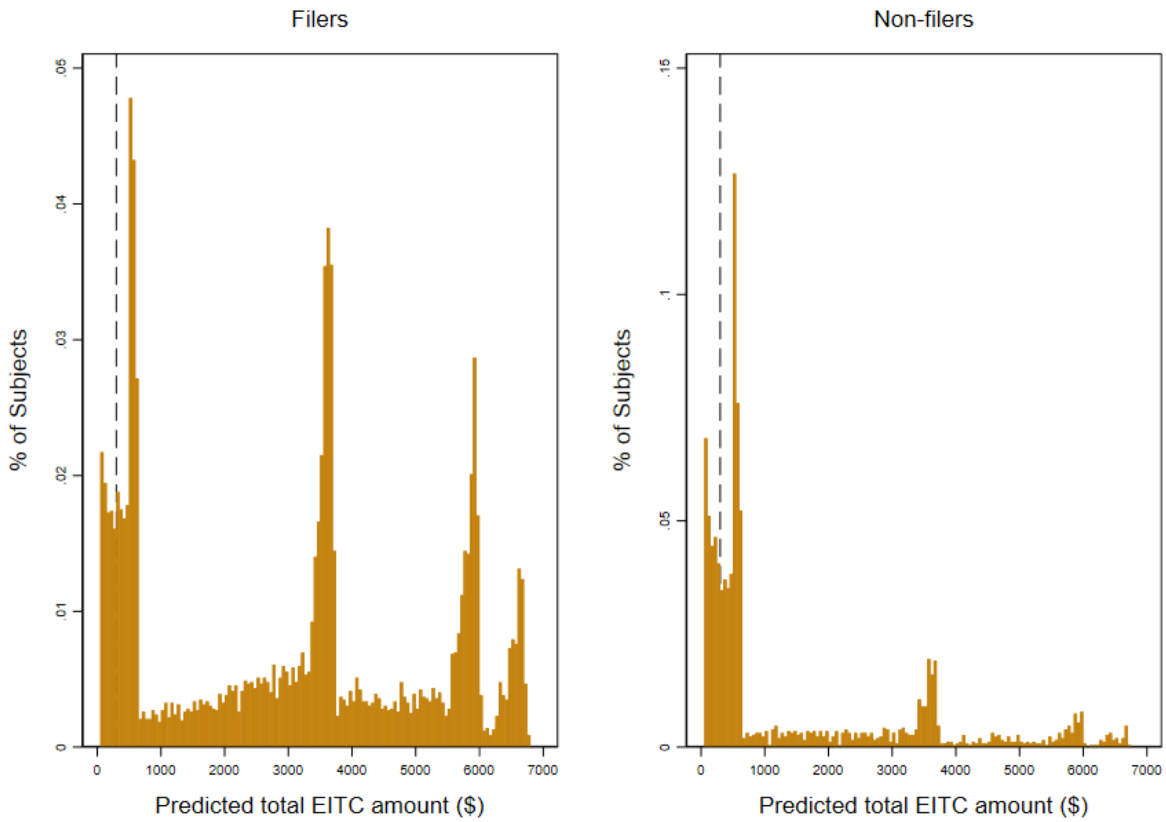
Notes. Standard errors in parentheses; standard deviations in square brackets. Asterisks indicate statistical significance of individual coefficients; *=10%, **=5%, ***=1%. The p-values in the final column are for tests of the hypothesis that all of the treatment coefficients (the βs in equation (2)) are jointly zero. We show balance on the variable for number of children in the household, as opposed to presence of children which we showed in Appendix Table A.7, because we stratified on a dummy for presence of children during randomization for Study 6, ensuring perfect balance on that variable.

Appendix Figure A.1: Estimated income for filers and non-filers in Study 6 control group



Notes. The figure plots the distribution of predicted annual income among subjects in the Study 6 control group separated by those who did and did not file a return. We group households into bins of $500. We also mark, using the dotted and solid vertical lines, the income levels in TY 2018 at which the IRS requires single and married taxpayers must file a return, which are $12,000 and $24,000, respectively.

Appendix Figure A.2: Estimated EITC amount for filers and non-filers in Study 6 control group



Notes. The figure plots the distribution of estimated EITC eligibility amounts among subjects in the Study 6 control group, separated by those who did and did not file a tax return. We group households into bins of $50. The heaps at around $500, $3,500, $6,000 and $6,500 correspond to the maximum EITCs for families with zero, one, two, or three or more children in TY2018, respectively. The dotted line marks predicted EITC amount of $300, representing the approximate cost that families are charged by some for-profit preparers to file their return.

Appendix Figure A.3: Study 1, Golden State Opportunity text messages

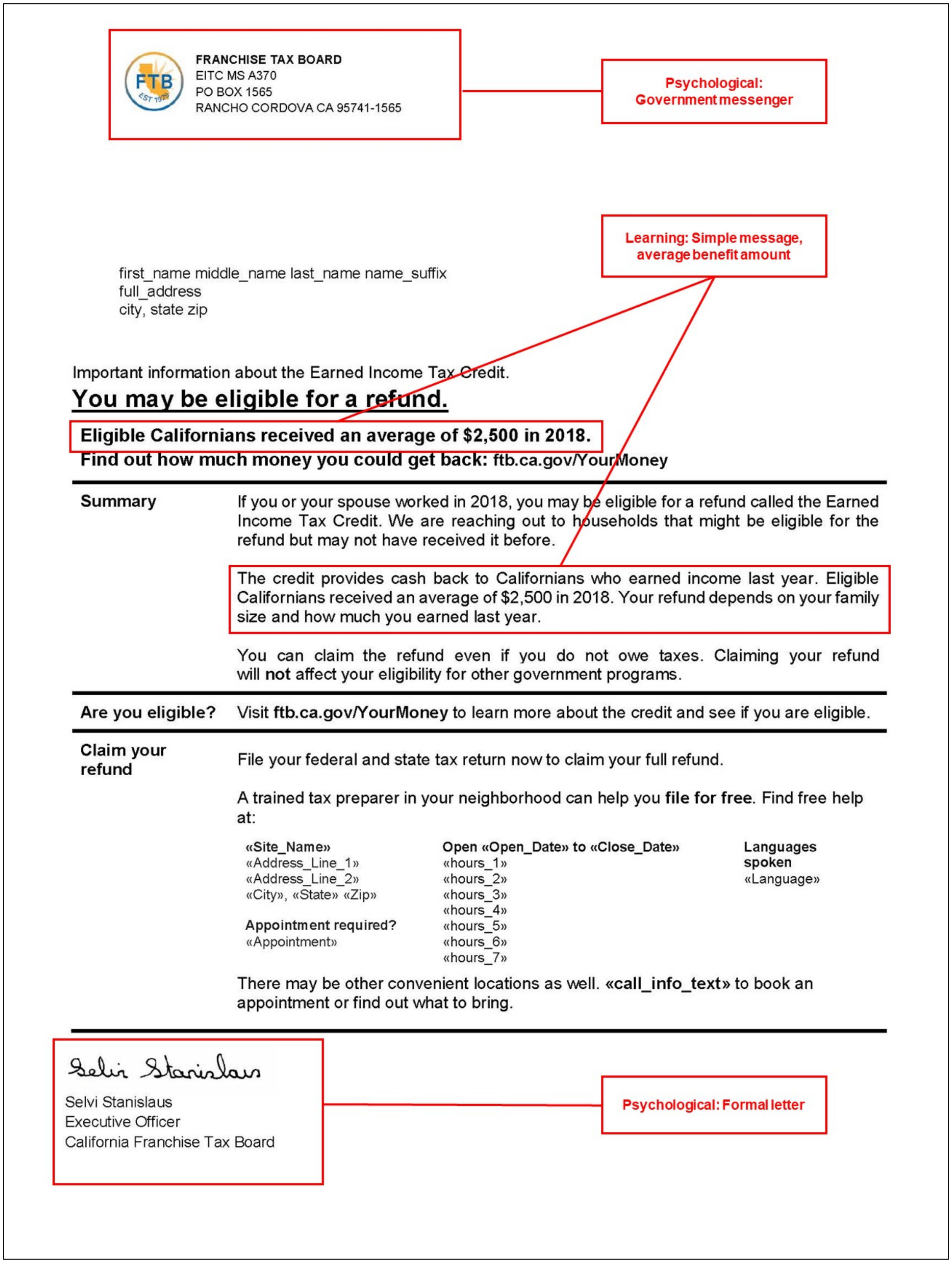
Messages sent in March read:

> *Hello, this is a volunteer from CalEITC4Me, we're texting to let people know they may be leaving up to $6,000 in tax money on the table, visit our website to see if you are eligible for the EITC and free tax preparation! Have you filed your taxes yet? goo.gl/42PR24*

Messages sent in April read:

> *Hello! My name is ¡name¿ with CalEITC4me. Have you filed your taxes yet? You may want to file this year because of the recently expanded California Earned Income Tax Credit! Thousands of Californians are claiming this cash-back credit. You don't want to miss out! If you want to know how to claim it, text me back! We are here to help! Click here! Para Espanol responda con la palabra: Espanol bit.ly/CalEITC4Me*

Appendix Figure A.4: Study 2, treatment arm 1 (FTB messenger, formal letter)



FRANCHISE TAX BOARD
EITC MS A370
PO BOX 1565
RANCHO CORDOVA CA 95741-1565

first_name middle_name last_name name_suffix
full_address
city, state zip

Important information about the Earned Income Tax Credit.

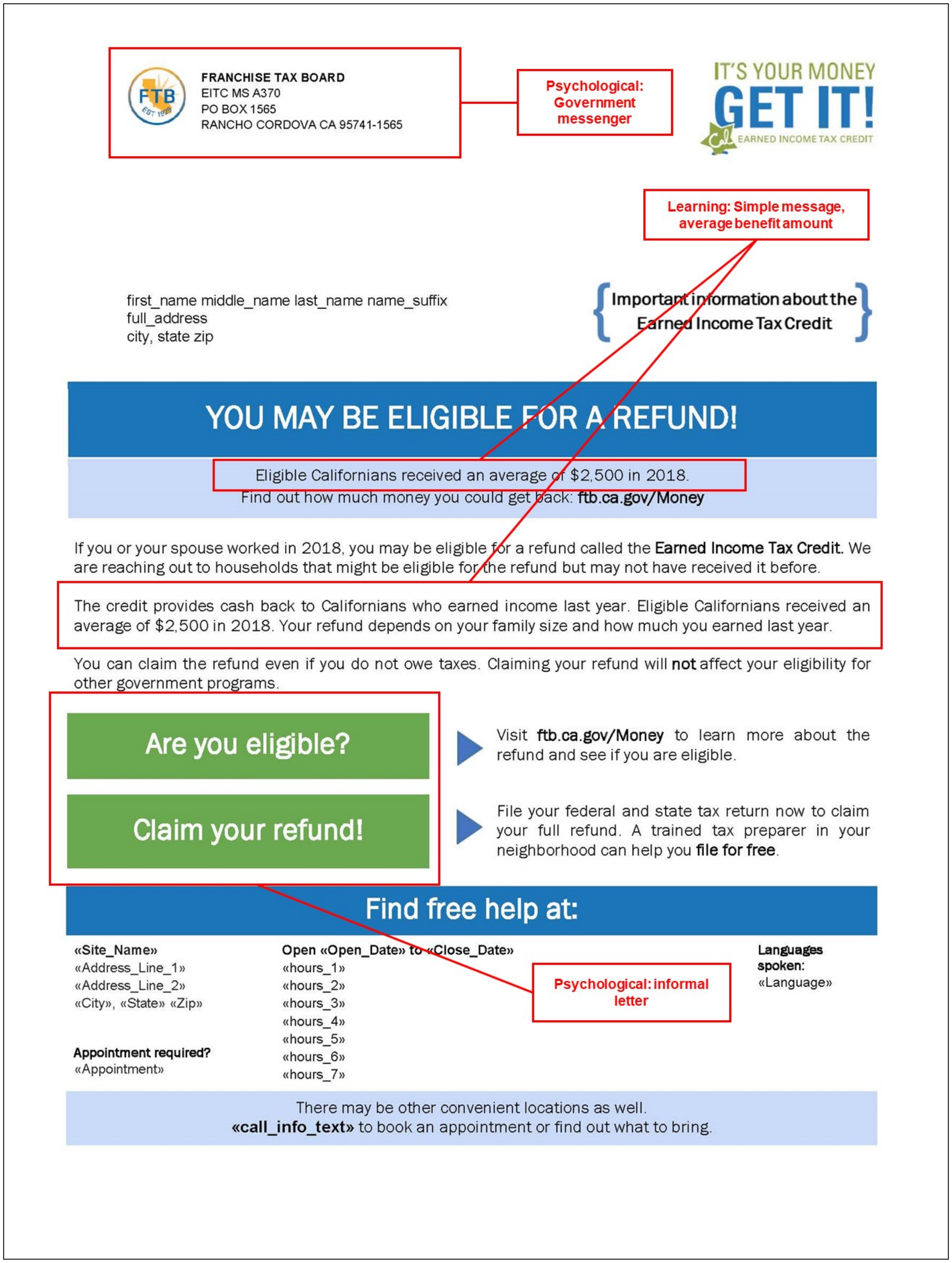
You may be eligible for a refund.

Eligible Californians received an average of $2,500 in 2018.
Find out how much money you could get back: ftb.ca.gov/YourMoney

Summary	If you or your spouse worked in 2018, you may be eligible for a refund called the Earned Income Tax Credit. We are reaching out to households that might be eligible for the refund but may not have received it before. The credit provides cash back to Californians who earned income last year. Eligible Californians received an average of $2,500 in 2018. Your refund depends on your family size and how much you earned last year. You can claim the refund even if you do not owe taxes. Claiming your refund will **not** affect your eligibility for other government programs.
Are you eligible?	Visit **ftb.ca.gov/YourMoney** to learn more about the credit and see if you are eligible.
Claim your refund	File your federal and state tax return now to claim your full refund. A trained tax preparer in your neighborhood can help you **file for free**. Find free help at: **«Site_Name»** «Address_Line_1» «Address_Line_2» «City», «State» «Zip» **Appointment required?** «Appointment» **Open «Open_Date» to «Close_Date»** «hours_1» «hours_2» «hours_3» «hours_4» «hours_5» «hours_6» «hours_7» **Languages spoken** «Language» There may be other convenient locations as well. **«call_info_text»** to book an appointment or find out what to bring.

Selvi Stanislaus
Selvi Stanislaus
Executive Officer
California Franchise Tax Board

Appendix Figure A.5: Study 2, treatment arm 2 (FTB messenger, informal letter)



FRANCHISE TAX BOARD
EITC MS A370
PO BOX 1565
RANCHO CORDOVA CA 95741-1565

Psychological: Government messenger

IT'S YOUR MONEY GET IT! EARNED INCOME TAX CREDIT

Learning: Simple message, average benefit amount

first_name middle_name last_name name_suffix
full_address
city, state zip

{ Important information about the Earned Income Tax Credit }

YOU MAY BE ELIGIBLE FOR A REFUND!

Eligible Californians received an average of $2,500 in 2018.
Find out how much money you could get back: **ftb.ca.gov/Money**

If you or your spouse worked in 2018, you may be eligible for a refund called the **Earned Income Tax Credit.** We are reaching out to households that might be eligible for the refund but may not have received it before.

The credit provides cash back to Californians who earned income last year. Eligible Californians received an average of $2,500 in 2018. Your refund depends on your family size and how much you earned last year.

You can claim the refund even if you do not owe taxes. Claiming your refund will **not** affect your eligibility for other government programs.

Are you eligible? Visit **ftb.ca.gov/Money** to learn more about the refund and see if you are eligible.

Claim your refund! File your federal and state tax return now to claim your full refund. A trained tax preparer in your neighborhood can help you **file for free**.

Find free help at:

«Site_Name»
«Address_Line_1»
«Address_Line_2»
«City», «State» «Zip»

Appointment required?
«Appointment»

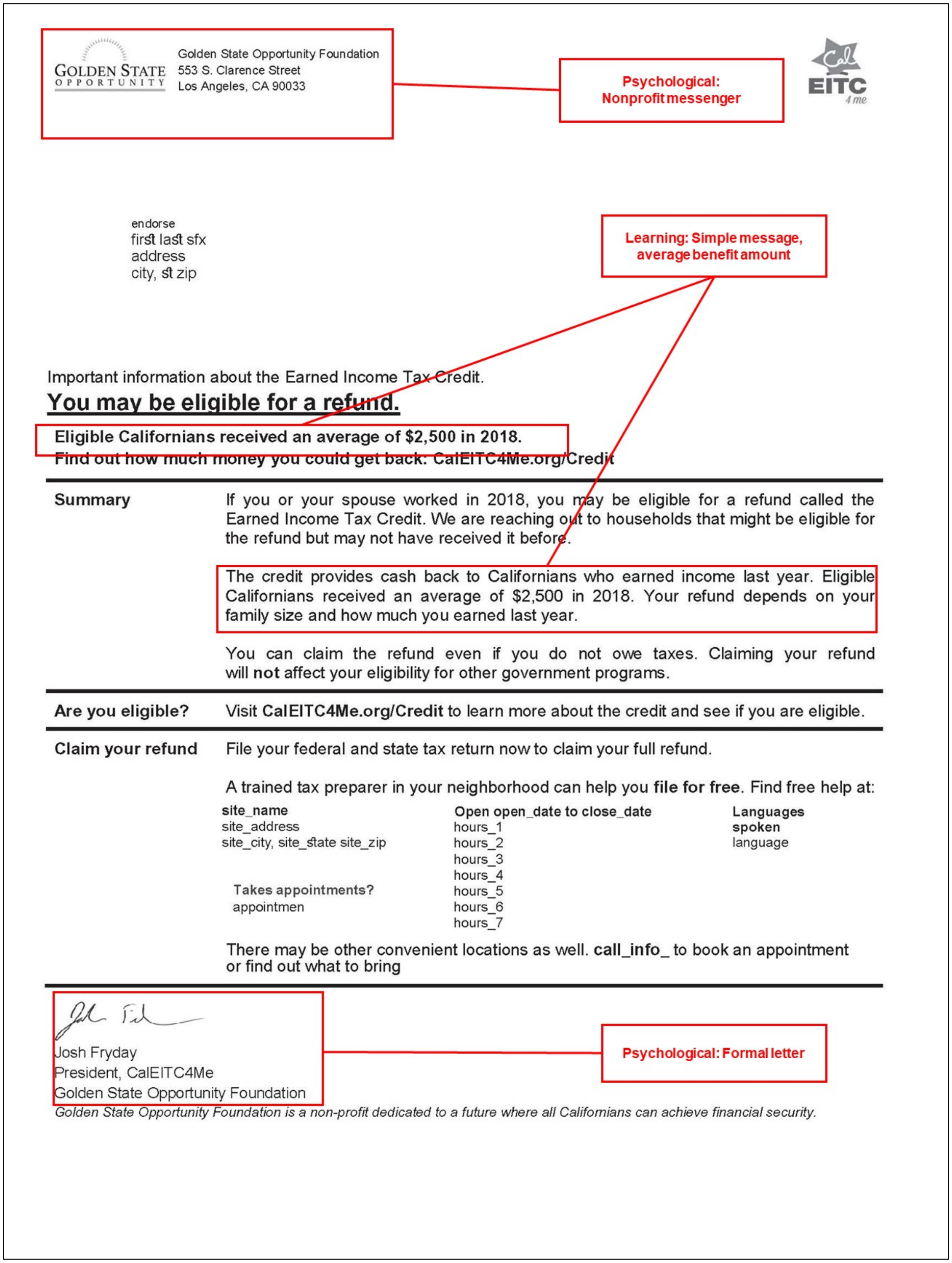
Open «Open_Date» to «Close_Date»
«hours_1»
«hours_2»
«hours_3»
«hours_4»
«hours_5»
«hours_6»
«hours_7»

Psychological: informal letter

Languages spoken:
«Language»

There may be other convenient locations as well.
«call_info_text» to book an appointment or find out what to bring.

Appendix Figure A.6: Study 2, treatment arm 3 (GSO messenger, formal letter)



Golden State Opportunity Foundation
553 S. Clarence Street
Los Angeles, CA 90033

endorse
first last sfx
address
city, st zip

Important information about the Earned Income Tax Credit.

You may be eligible for a refund.

Eligible Californians received an average of $2,500 in 2018.
Find out how much money you could get back: CalEITC4Me.org/Credit

Summary If you or your spouse worked in 2018, you may be eligible for a refund called the Earned Income Tax Credit. We are reaching out to households that might be eligible for the refund but may not have received it before.

The credit provides cash back to Californians who earned income last year. Eligible Californians received an average of $2,500 in 2018. Your refund depends on your family size and how much you earned last year.

You can claim the refund even if you do not owe taxes. Claiming your refund will **not** affect your eligibility for other government programs.

Are you eligible? Visit **CalEITC4Me.org/Credit** to learn more about the credit and see if you are eligible.

Claim your refund File your federal and state tax return now to claim your full refund.

A trained tax preparer in your neighborhood can help you **file for free**. Find free help at:

site_name
site_address
site_city, site_state site_zip

Takes appointments?
appointmen

Open open_date to close_date
hours_1
hours_2
hours_3
hours_4
hours_5
hours_6
hours_7

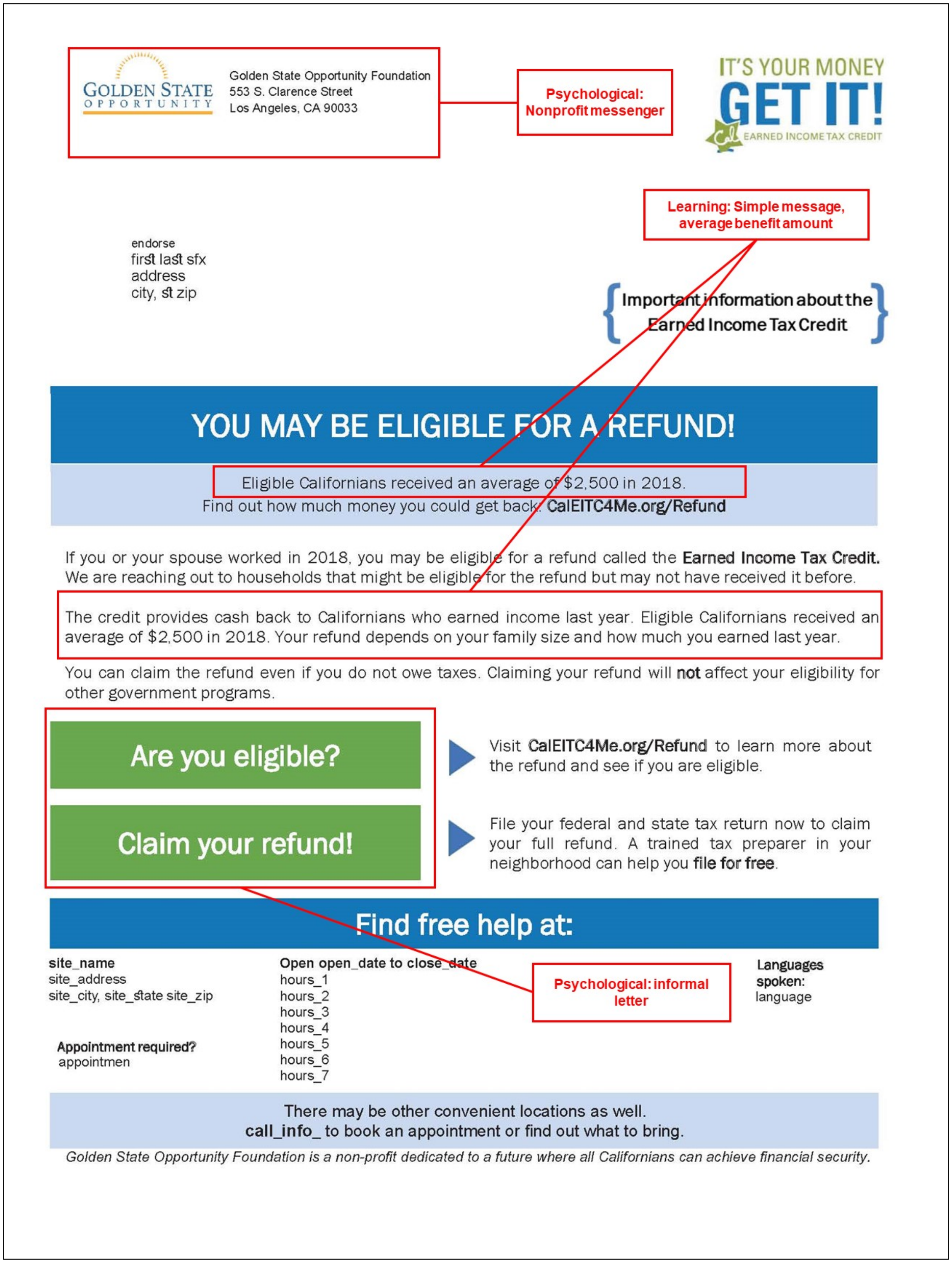
Languages spoken
language

There may be other convenient locations as well. **call_info_** to book an appointment or find out what to bring

Josh Fryday
President, CalEITC4Me
Golden State Opportunity Foundation

Golden State Opportunity Foundation is a non-profit dedicated to a future where all Californians can achieve financial security.

Appendix Figure A.7: Study 2, treatment arm 4 (GSO messenger, informal letter)



GOLDEN STATE OPPORTUNITY

Golden State Opportunity Foundation
553 S. Clarence Street
Los Angeles, CA 90033

Psychological: Nonprofit messenger

IT'S YOUR MONEY GET IT! EARNED INCOME TAX CREDIT

Learning: Simple message, average benefit amount

endorse
first last sfx
address
city, st zip

Important information about the Earned Income Tax Credit

YOU MAY BE ELIGIBLE FOR A REFUND!

Eligible Californians received an average of $2,500 in 2018.
Find out how much money you could get back: **CalEITC4Me.org/Refund**

If you or your spouse worked in 2018, you may be eligible for a refund called the **Earned Income Tax Credit.** We are reaching out to households that might be eligible for the refund but may not have received it before.

The credit provides cash back to Californians who earned income last year. Eligible Californians received an average of $2,500 in 2018. Your refund depends on your family size and how much you earned last year.

You can claim the refund even if you do not owe taxes. Claiming your refund will **not** affect your eligibility for other government programs.

Are you eligible?

Visit **CalEITC4Me.org/Refund** to learn more about the refund and see if you are eligible.

Claim your refund!

File your federal and state tax return now to claim your full refund. A trained tax preparer in your neighborhood can help you **file for free**.

Find free help at:

site_name
site_address
site_city, site_state site_zip

Appointment required?
appointmen

Open open_date to close_date
hours_1
hours_2
hours_3
hours_4
hours_5
hours_6
hours_7

Psychological: informal letter

Languages spoken:
language

There may be other convenient locations as well.
call_info_ to book an appointment or find out what to bring.

Golden State Opportunity Foundation is a non-profit dedicated to a future where all Californians can achieve financial security.

Appendix Figure A.8: Study 3, Golden State Opportunity text messages

- **Treatment 1: Basic Informational Message + Link**
  - Hi-this is *¡volunteer name¿*, a volunteer with CalEITC4Me. I'm contacting households who might qualify for a tax refund. Even if you don't owe taxes, you could get cash back by filing a tax return. Visit caleitc4me.org/CashBack to learn more about your eligibility and to claim your EITC refund.
- **Treatment 2: Phone number/call option**
  - Hi-this is *¡volunteer name¿*, a volunteer with CalEITC4Me. I'm contacting households who might qualify for a tax refund. Even if you don't owe taxes, you could get cash back by filing a tax return. Call *¡local hotline phone number¿* to get free help with filing your return and to claim your EITC refund.
- **Treatment 3: Offer Text-based Assistance**
  - Hi-this is *¡volunteer name¿*, a volunteer with CalEITC4Me. I'm contacting households who might qualify for a tax refund. Even if you don't owe taxes, you could get cash back by filing a tax return. Text "yes" and I can help you claim your EITC refund.
- **Treatment 4: Benefit Value**
  - Hi-this is *¡volunteer name¿*, a volunteer with CalEITC4Me. I'm contacting households who might qualify for a tax refund. Even if you don't owe taxes, you could get cash back by filing a tax return. Eligible families got back an average of $2,000 last year. Text "yes" and I can help you claim your EITC refund.

Appendix Figure A.9: Study 4, treatment arm 1 (formal, simple)

FORMAL –SIMPLE

FRANCHISE TAX BOARD
EITC MS A370
PO BOX 1565
RANCHO CORDOVA CA 95741-1565

English (front)

first_name middle_name last_name name_suffix
full_address
city, state zip

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

Psychological: Formal letter

Summary	If you or your spouse worked in 2018, you may be eligible for a refund called the Earned Income Tax Credit. We are reaching out to households that might be eligible for the refund but may not have received it before. The credit provides cash back to Californians who earned income last year. Your refund depends on your family size and how much you earned last year. You can claim the refund even if you do not owe taxes. Claiming your refund will **not** affect your eligibility for other government programs.
Are you eligible?	Visit **ftb.ca.gov/Credit** to learn more about the credit and see if you are eligible.
Claim your refund	File your federal and state tax return to claim your full refund. You can **file for free.** For free tax preparation help, visit: **ftb.ca.gov/Credit**

URL unique to each treatment arm

Learning: Simple message

Selvi Stanislaus

Selvi Stanislaus
Executive Officer
California Franchise Tax Board

Link to language translations (Spanish translation on back)

中文 | 한국어 | Русский | Tiếng Việt → ftb.ca.gov/Lang1

Appendix Figure A.10: Study 4, treatment arm 2 (formal, credit amount)

FORMAL –BENEFIT

FRANCHISE TAX BOARD
EITC MS A370
PO BOX 1565
RANCHO CORDOVA CA 95741-1565

first_name middle_name last_name name_suffix
full_address
city, state zip

Learning: average benefit amount

Important information about the Earned Income Tax Credit.

You may be eligible for a refund.

Eligible Californians received an average of $2,500 in 2018.
Find out how much money you could get back: ftb.ca.gov/Earn

Summary	If you or your spouse worked in 2018, you may be eligible for a refund called the Earned Income Tax Credit. We are reaching out to households that might be eligible for the refund but may not have received it before. The credit provides cash back to Californians who earned income last year. Eligible Californians received an average of $2,500 in 2018. Your refund depends on your family size and how much you earned last year. You can claim the refund even if you do not owe taxes. Claiming your refund will **not** affect your eligibility for other government programs.
Are you eligible?	Visit **ftb.ca.gov/Earn** to learn more about the credit and see if you are eligible.
Claim your refund	File your federal and state tax return to claim your full refund. **You can file for free.** For free tax preparation help, visit: **ftb.ca.gov/Earn**

Selvi Stanislaus

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Executive Officer
California Franchise Tax Board

中文 | 한국어 | Русский | Tiếng Việt → ftb.ca.gov/Lang2

Appendix Figure A.11: Study 4, treatment arm 3 (formal, VITA info)

FRANCHISE TAX BOARD
EITC MS A370
PO BOX 1565
RANCHO CORDOVA CA 95741-1565

FORMAL – VITA

first_name middle_name last_name name_suffix
full_address
city, state zip

Compliance: local in-person free tax preparation information

Important information about the Earned Income Tax Credit.

You may be eligible for a refund.

Summary	If you or your spouse worked in 2018, you may be eligible for a refund called the Earned Income Tax Credit. We are reaching out to households that might be eligible for the refund but may not have received it before. The credit provides cash back to Californians who earned income last year. Your refund depends on your family size and how much you earned last year. You can claim the refund even if you do not owe taxes. Claiming your refund will **not** affect your eligibility for other government programs.
Are you eligible?	Visit **ftb.ca.gov/Refund** to learn more about the credit and see if you are eligible.
Claim your refund	File your federal and state tax return now to claim your full refund.

A trained tax preparer in your neighborhood can help you **file for free**. Find free help at:

«Site_Name» «Address_Line_1» «Address_Line_2» «City», «State» «Zip» **Appointment required?** «Appointment»	**Open «Open_Date» to «Close_Date»** «hours_1» «hours_2» «hours_3» «hours_4» «hours_5» «hours_6» «hours_7»	**Languages spoken** «Language »

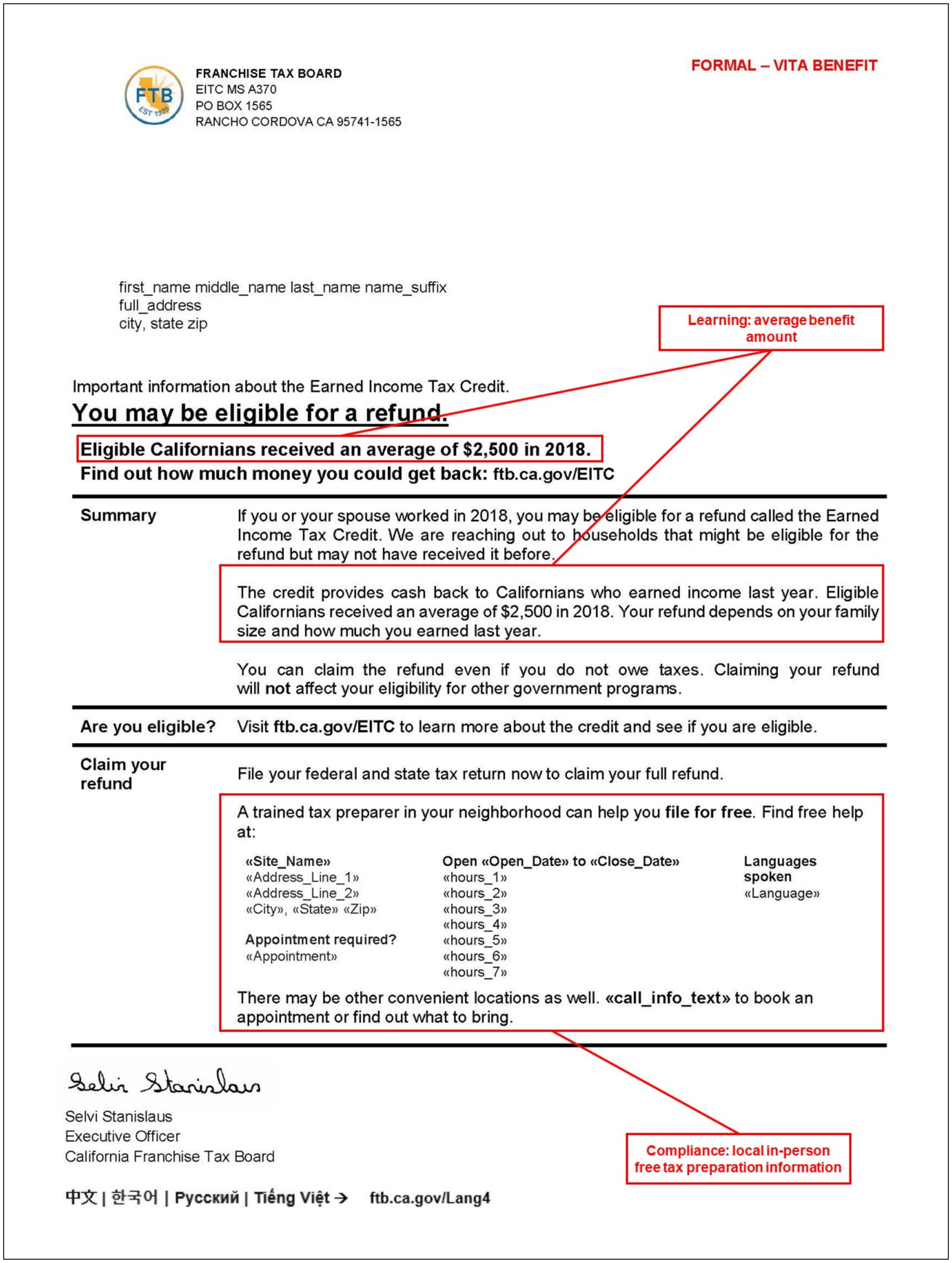
There may be other convenient locations as well. **«call_info_text»** to book an appointment or find out what to bring.

Selvi Stanislaus

Selvi Stanislaus
Executive Officer
California Franchise Tax Board

中文 | 한국어 | Русский | Tiếng Việt → ftb.ca.gov/Lang3

Appendix Figure A.12: Study 4, treatment arm 4 (formal, credit amount + VITA info)



FORMAL – VITA BENEFIT

FRANCHISE TAX BOARD
EITC MS A370
PO BOX 1565
RANCHO CORDOVA CA 95741-1565

first_name middle_name last_name name_suffix
full_address
city, state zip

Learning: average benefit amount

Important information about the Earned Income Tax Credit.

You may be eligible for a refund.

Eligible Californians received an average of $2,500 in 2018.
Find out how much money you could get back: ftb.ca.gov/EITC

Summary

If you or your spouse worked in 2018, you may be eligible for a refund called the Earned Income Tax Credit. We are reaching out to households that might be eligible for the refund but may not have received it before.

The credit provides cash back to Californians who earned income last year. Eligible Californians received an average of $2,500 in 2018. Your refund depends on your family size and how much you earned last year.

You can claim the refund even if you do not owe taxes. Claiming your refund will **not** affect your eligibility for other government programs.

Are you eligible? Visit **ftb.ca.gov/EITC** to learn more about the credit and see if you are eligible.

Claim your refund

File your federal and state tax return now to claim your full refund.

A trained tax preparer in your neighborhood can help you **file for free**. Find free help at:

«Site_Name» «Address_Line_1» «Address_Line_2» «City», «State» «Zip» **Appointment required?** «Appointment»	**Open «Open_Date» to «Close_Date»** «hours_1» «hours_2» «hours_3» «hours_4» «hours_5» «hours_6» «hours_7»	**Languages spoken** «Language»

There may be other convenient locations as well. **«call_info_text»** to book an appointment or find out what to bring.

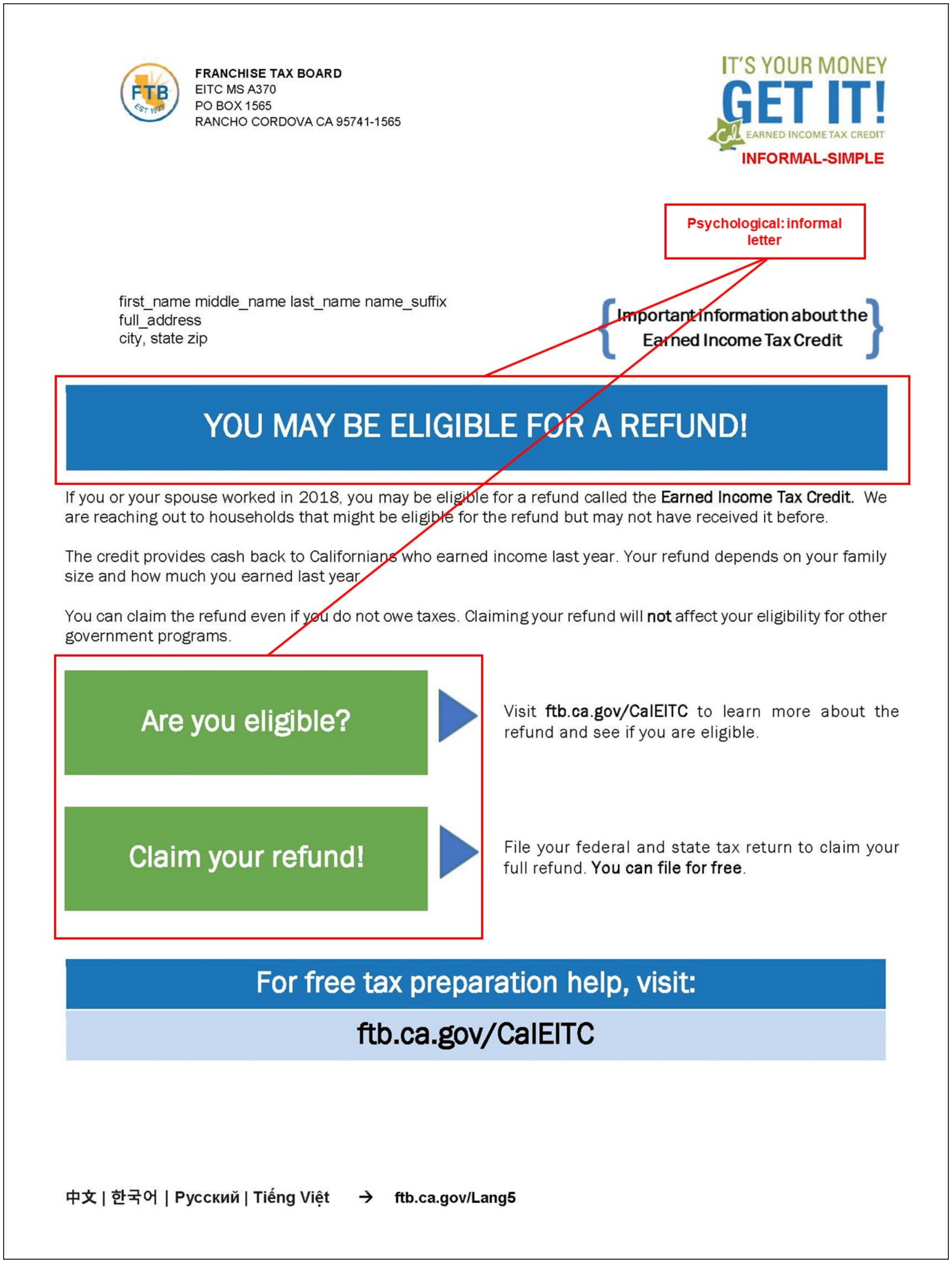
Selvi Stanislaus

Selvi Stanislaus
Executive Officer
California Franchise Tax Board

Compliance: local in-person free tax preparation information

中文 | 한국어 | Русский | Tiếng Việt →   ftb.ca.gov/Lang4

Appendix Figure A.13: Study 4, treatment arm 5 (informal, simple)



FRANCHISE TAX BOARD
EITC MS A370
PO BOX 1565
RANCHO CORDOVA CA 95741-1565

IT'S YOUR MONEY
GET IT!
EARNED INCOME TAX CREDIT
INFORMAL-SIMPLE

Psychological: informal letter

first_name middle_name last_name name_suffix
full_address
city, state zip

Important information about the Earned Income Tax Credit

YOU MAY BE ELIGIBLE FOR A REFUND!

If you or your spouse worked in 2018, you may be eligible for a refund called the **Earned Income Tax Credit.** We are reaching out to households that might be eligible for the refund but may not have received it before.

The credit provides cash back to Californians who earned income last year. Your refund depends on your family size and how much you earned last year.

You can claim the refund even if you do not owe taxes. Claiming your refund will **not** affect your eligibility for other government programs.

Are you eligible? — Visit **ftb.ca.gov/CalEITC** to learn more about the refund and see if you are eligible.

Claim your refund! — File your federal and state tax return to claim your full refund. **You can file for free.**

For free tax preparation help, visit:
ftb.ca.gov/CalEITC

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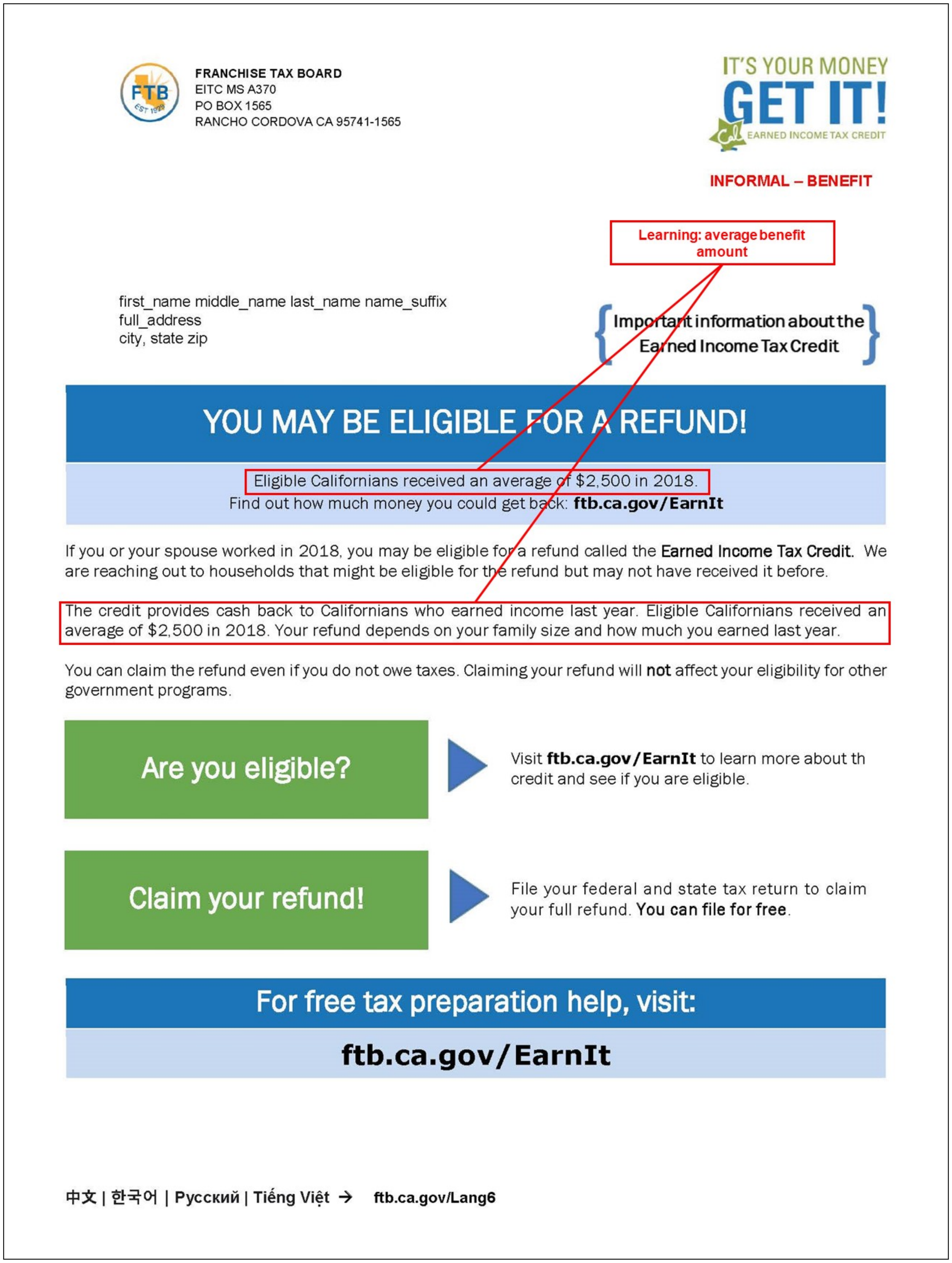
Appendix Figure A.14: Study 4, treatment arm 6 (informal, credit amount)



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EITC MS A370
PO BOX 1565
RANCHO CORDOVA CA 95741-1565

IT'S YOUR MONEY GET IT! EARNED INCOME TAX CREDIT

INFORMAL – BENEFIT

Learning: average benefit amount

first_name middle_name last_name name_suffix
full_address
city, state zip

Important information about the Earned Income Tax Credit

YOU MAY BE ELIGIBLE FOR A REFUND!

Eligible Californians received an average of $2,500 in 2018.
Find out how much money you could get back: **ftb.ca.gov/EarnIt**

If you or your spouse worked in 2018, you may be eligible for a refund called the **Earned Income Tax Credit.** We are reaching out to households that might be eligible for the refund but may not have received it before.

The credit provides cash back to Californians who earned income last year. Eligible Californians received an average of $2,500 in 2018. Your refund depends on your family size and how much you earned last year.

You can claim the refund even if you do not owe taxes. Claiming your refund will **not** affect your eligibility for other government programs.

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Claim your refund! — File your federal and state tax return to claim your full refund. **You can file for free.**

For free tax preparation help, visit:

ftb.ca.gov/EarnIt

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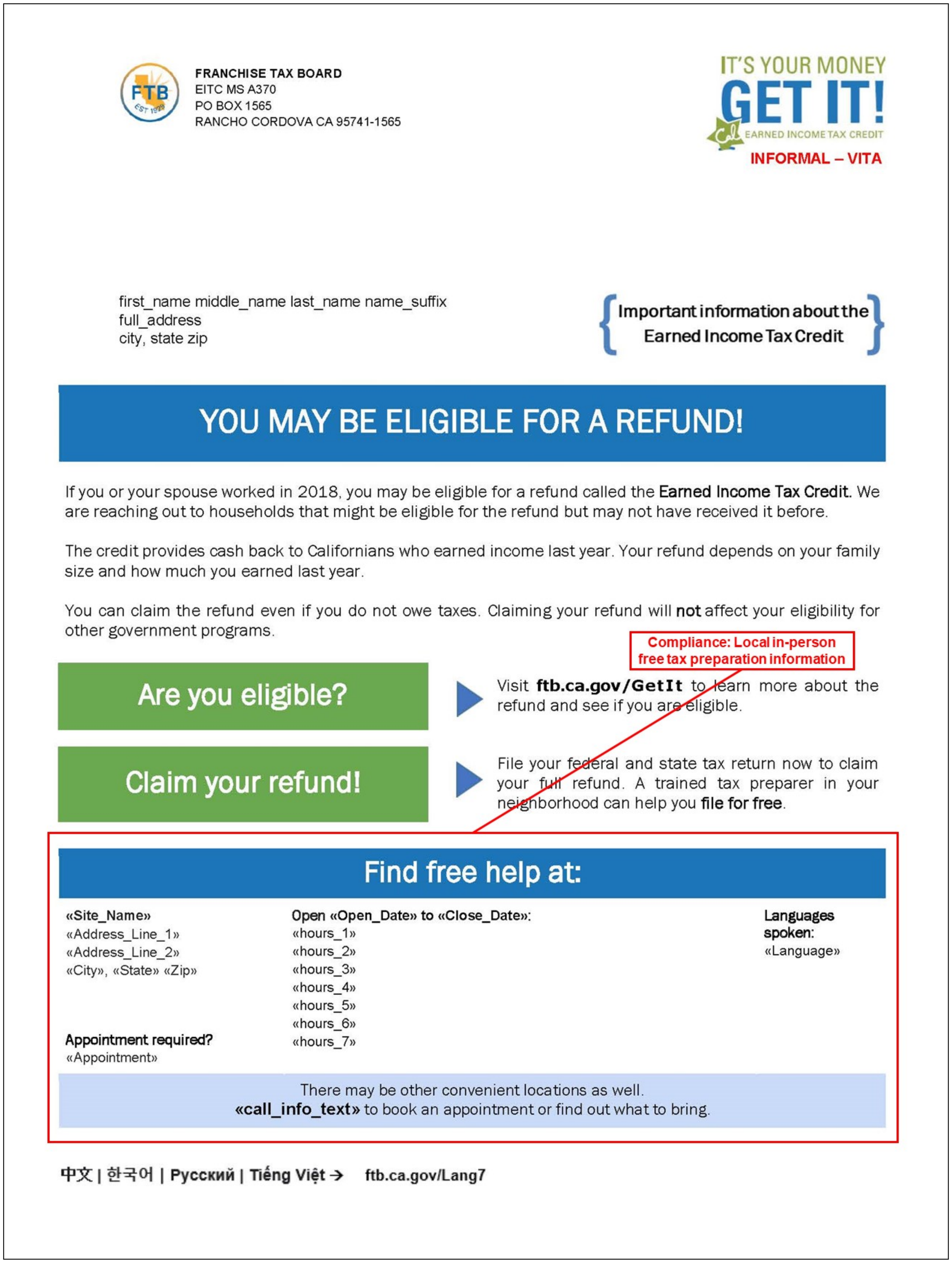
Appendix Figure A.15: Study 4, treatment arm 7 (informal, VITA info)



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IT'S YOUR MONEY
GET IT!
EARNED INCOME TAX CREDIT
INFORMAL – VITA

first_name middle_name last_name name_suffix
full_address
city, state zip

Important information about the Earned Income Tax Credit

YOU MAY BE ELIGIBLE FOR A REFUND!

If you or your spouse worked in 2018, you may be eligible for a refund called the **Earned Income Tax Credit.** We are reaching out to households that might be eligible for the refund but may not have received it before.

The credit provides cash back to Californians who earned income last year. Your refund depends on your family size and how much you earned last year.

You can claim the refund even if you do not owe taxes. Claiming your refund will **not** affect your eligibility for other government programs.

Compliance: Local in-person free tax preparation information

Are you eligible? Visit **ftb.ca.gov/GetIt** to learn more about the refund and see if you are eligible.

Claim your refund! File your federal and state tax return now to claim your full refund. A trained tax preparer in your neighborhood can help you **file for free**.

Find free help at:

«Site_Name»
«Address_Line_1»
«Address_Line_2»
«City», «State» «Zip»

Open «Open_Date» to «Close_Date»:
«hours_1»
«hours_2»
«hours_3»
«hours_4»
«hours_5»
«hours_6»
«hours_7»

Languages spoken:
«Language»

Appointment required?
«Appointment»

There may be other convenient locations as well.
«call_info_text» to book an appointment or find out what to bring.

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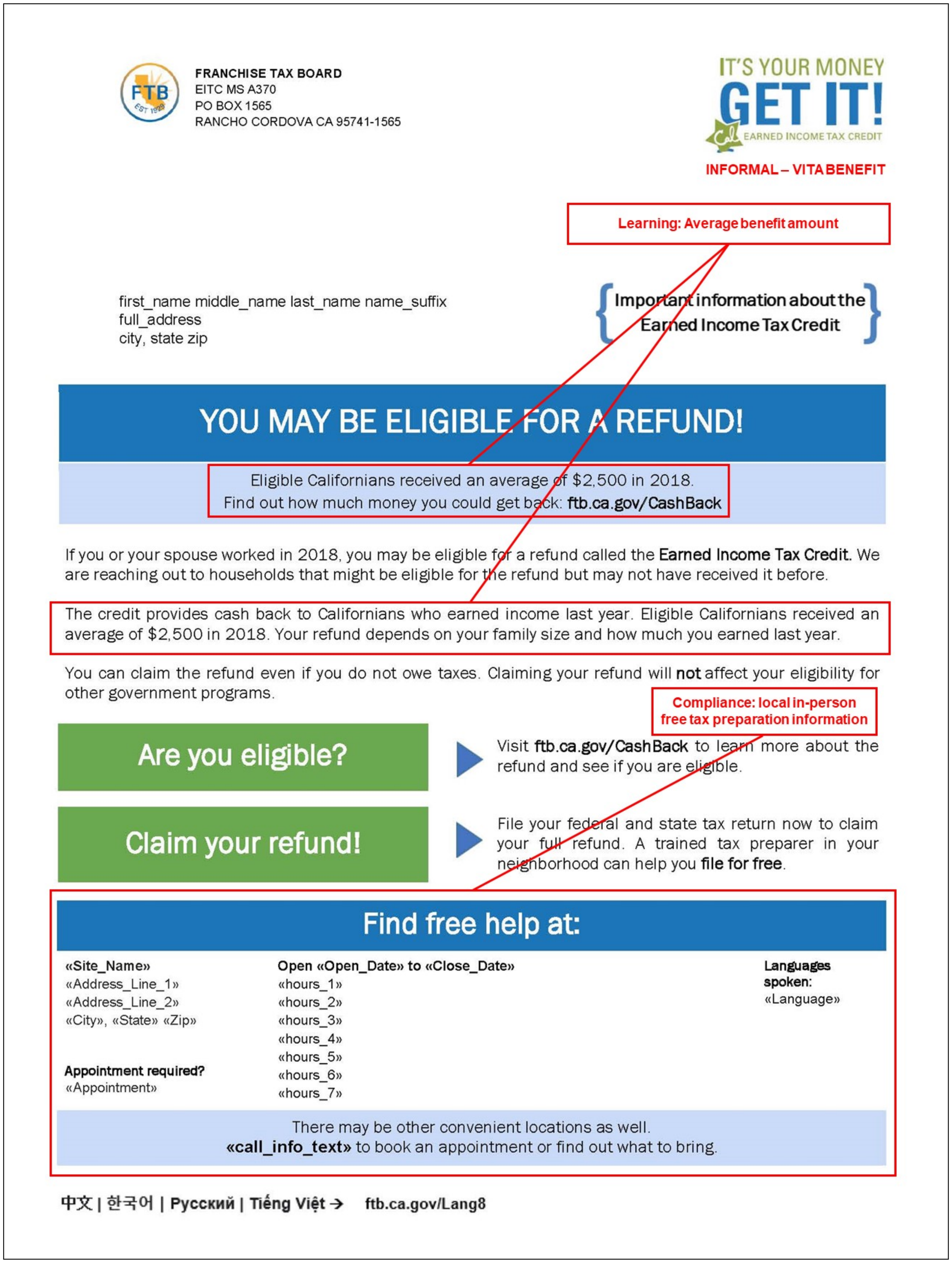
Appendix Figure A.16: Study 4, treatment arm 8 (informal, credit amount + VITA info)



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RANCHO CORDOVA CA 95741-1565

IT'S YOUR MONEY
GET IT!
EARNED INCOME TAX CREDIT

INFORMAL – VITA BENEFIT

Learning: Average benefit amount

first_name middle_name last_name name_suffix
full_address
city, state zip

Important information about the Earned Income Tax Credit

YOU MAY BE ELIGIBLE FOR A REFUND!

Eligible Californians received an average of $2,500 in 2018.
Find out how much money you could get back: **ftb.ca.gov/CashBack**

If you or your spouse worked in 2018, you may be eligible for a refund called the **Earned Income Tax Credit.** We are reaching out to households that might be eligible for the refund but may not have received it before.

The credit provides cash back to Californians who earned income last year. Eligible Californians received an average of $2,500 in 2018. Your refund depends on your family size and how much you earned last year.

You can claim the refund even if you do not owe taxes. Claiming your refund will **not** affect your eligibility for other government programs.

Compliance: local in-person free tax preparation information

Are you eligible?

Visit **ftb.ca.gov/CashBack** to learn more about the refund and see if you are eligible.

Claim your refund!

File your federal and state tax return now to claim your full refund. A trained tax preparer in your neighborhood can help you **file for free**.

Find free help at:

«Site_Name»
«Address_Line_1»
«Address_Line_2»
«City», «State» «Zip»

Appointment required?
«Appointment»

Open «Open_Date» to «Close_Date»
«hours_1»
«hours_2»
«hours_3»
«hours_4»
«hours_5»
«hours_6»
«hours_7»

Languages spoken:
«Language»

There may be other convenient locations as well.
«call_info_text» to book an appointment or find out what to bring.

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Appendix Figure A.17: Study 5, CA Department of Social Services/CalFresh text messages, tax year 2017

Text 1:

> *Hi, this is ¡county¿. Have you claimed your tax refund? We estimate you're owed about $x,xxx from state and federal earned income tax credits. File your taxes to get the refund you earned! Reply "1" to learn how to get your taxes done for free or "2" to stop these texts. Standard messaging rates apply.*

Text 2:

> *You can use free online software to prepare your taxes at www.myfreetaxes.org (sponsored by the United Way). For in-person assistance, find the closest volunteer site at irs.treasury.gov/freetaxprep. Would you like information for a nearby site? If yes, reply "1".*

Notes. Those who responded with a "1" to the first message were sent the second message. Those who responded with a "1" to the second message were sent the address and hours of the closest VITA site to the client's 9 digit zip code. When that site required appointments, the text also included a link for registration. Texts were sent in English or Spanish, depending on the language indicated in the CalFresh record, and were delivered over two days in a single blast in March 2018.

Appendix Figure A.18: Study 6, CA Department of Social Services/CalFresh text messages, tax year 2018

- **Treatment 1: Control Text**
  - *Text 1:* Hi *¡name¿*. This is *¡county name¿* County. You may qualify for cash back thanks to tax credits.
  - *Text 2:* Claim your refund by filing a tax return. See if you're eligible at caleitc4me.org/Cash. Call 211 to file your taxes for free.
- **Treatment 2: Average Benefit**
  - *Text 1:* Hi *¡name¿*. This is *¡county name¿* County. You may qualify for cash back thanks to tax credits. Eligible families got back $2,500 on average last year.
  - *Text 2:* Claim your refund by filing a tax return. See if you're eligible at caleitc4me.org/YourMoney. Call 211 to file your taxes for free.
- **Treatment 3: Personalized Benefit**
  - *Text 1:* Hi *¡name¿*. This is *¡county name¿* County. You may qualify for cash back thanks to tax credits. Based on our records, you could get back *¡$ credit amount¿*.
  - *Text 2:* Claim your refund by filing a tax return. See if you're eligible at caleitc4me.org/Money. Call 211 to file your taxes for free.

Appendix B

Measuring Take-up of the California EITC with State Administrative Data

B.1 Representativeness of CalFresh population

We use the 2017 ACS 5-year sample to investigate how representative CalFresh recipients are of the low-income population in California, and to what extent our estimates of take-up in the CalFresh population might apply to all low-income households in the state. As discussed in Section 3.2, the ACS does not contain all the information needed to accurately estimate EITC eligibility, but we can compare the overall income, demographic, and household characteristics of SNAP enrollees (and those who reside with SNAP enrollees) to those who do not enroll in SNAP (and reside with no other SNAP enrollees).

First, we restrict the ACS sample to households who reside in California. Approximately 5.2 million individuals reside in a household with at least one SNAP recipient, versus 32.9 million in households without a SNAP recipient. We consider a subsample of these data, limited to those 18–64-year-old individuals with family income between $0 and $69,063 (the median family income among this population in 2017), excluding those in group-quarters. Within this group, we contrast those who live in a household with someone claiming SNAP to those who do not (Appendix Table B.1). This sample includes 81% of those 18-64-year-olds in SNAP households, and 46% of those in non-SNAP households.

Overall, adults between 18-64 years old in households with a SNAP recipient (hereafter referred to as SNAP families) tend to belong to larger families than those in households without SNAP recipients (non-SNAP families). SNAP families contain an average of 4.1 individuals, compared to 2.8 in non-SNAP families. The composition of those families is also different. SNAP families tend to contain more children and fewer elderly individuals. Adults in SNAP families are more likely to be Hispanic relative to those in non-SNAP households and to a lesser degree more likely to be non-White.

SNAP families also tend to have lower incomes than non-SNAP families. Median total family income among SNAP families is $11,886 lower than for non-SNAP families. This trend is similar for earned income ($11,917 lower) and wage income ($11,376 lower). Non-SNAP families are slightly more likely to have a positive amount of investment income than SNAP families and are also more likely to have investment income over the EITC cutoff. The lower total, earned, and wage income at the family level among SNAP families suggests a larger share of these families could be eligible for the federal and California EITC than non-SNAP families.

In Appendix Figure B.1, we compare the distribution of income among SNAP families versus non-SNAP families. The difference in the distribution of income between SNAP and non-SNAP families is similar across total, earned, and wage income. The lower means reported in Table A1 seems to be a product of this overall shift in the income distribution, although there is some small evidence of a slightly higher share of SNAP families having $0 in family income (though not wages).

These characteristics indicate that families in SNAP households are more likely to be eligible for the federal EITC and CalEITC, given their lower income and higher number of children. Of course, many eligible families who do not appear in our CalFresh data. It is also likely we miss many eligible non-claimants, because there are many more low-income individuals and families in the non-SNAP population, and the same families who would choose not to enroll in a program like SNAP would also be less inclined to file a return and claim the CalEITC.

B.2 Comparison of e-filers with paper and web filers

Our main analysis does not consider the approximately 13% of California tax units who do not e-file. We exclude these paper filers because we are unable to observe their federal tax returns, and federal tax returns contain information necessary to determine both EITC eligibility and federal EITC receipt. Excluding paper filers does not impact our estimate of EITC eligibility among non-filers, because paper filers are included in the matching process between the tax and social service universe, meaning these filers are not inappropriately included in the non-filing population.

Given the information we do have on paper filers,[1] we believe that excluding these tax units from our analysis results in our underestimating the share of tax units that are both eligible for but do not claim the CalEITC. Appendix Table B.2 compares means of tax filing characteristics between paper and e-filers. Overall, e-filers and paper filers appear similar. E-filers are slightly more likely to file as married filing jointly or as a head of household and have slightly more dependents. Paper and e-filers claim the CalEITC in roughly equal proportions, and e-filers are eligible for slightly higher CalEITC amounts on average. E-filers are slightly more likely to be on CalFresh and are no-more likely to have an ITIN present on their tax return relative to paper filers.

However, there are two substantial differences that lead us to believe that the inclusion of paper and web filers would increase the rate of non-participation among tax filers. First, paper filers are much more likely to self-file tax returns, as opposed to filing with either a paid preparer or through a free tax preparation service like VITA. Among e-filers, 30% of tax units self-prepare their returns

[1] Paper returns – or tax returns that are submitted by mail to the FTB and IRS – make up 94% of the filers that do not e-file. The remaining 6% are web filers, or filers who make use of CalFile, a service where tax filers can submit their state tax return directly to the FTB. We refer to web and paper filers as paper filers in this section.

compared to 73% of paper or web filers. It seems likely that filing with the aid of either a paid or free tax preparation service increases the likelihood that a tax unit claims tax credits for which they are eligible. Second, paper filers have lower wages and adjusted gross income than e-filers, making it more likely that they have income in the EITC-eligible range. Median AGI is $38,009 and median wages are $24,732 for paper filers, versus $42,823 and $31,482 for e-filers. Even if we limit the sample to tax units with a head or spouse on CalFresh, these differences remain. These differences also remain, though the gap decreases, if we limit to the sub-population of tax units that claimed the CalEITC.

To test how these two factors combine to impact EITC non-claiming, we construct a measure of imputed CalEITC eligibility using only characteristics available from the primary state tax form (Schedule 540), which we can observe for all California filers. This measure of eligibility will be less accurate than our measure for e-filers alone. For example, it does not include non-wage earned income and investment income from the federal 1040 form and does not incorporate our efforts to accurately estimate the number of qualified children. But it allows us to roughly compare eligibility rates between e-filers and paper filers using the same information. We only consider CalEITC eligibility, because we cannot accurately determine which paper or filers claim the federal EITC without their 1040 information.

We find that roughly equal shares of e-filers (13%) and paper filers (14%) are likely to be eligible for the CalEITC, but paper filers are 13 percentage points less likely to claim the CalEITC, conditional on imputed eligibility. When limited to tax units on CalFresh, that difference falls to 8 percentage points. Given these results, we anticipate that excluding paper returns likely results in an underestimate of the share of eligible tax units that do not claim their CalEITC amounts.

B.3 Individuals without Social Security Numbers

Valid SSN rule

Individuals without a valid Social Security Number (SSN) can use an Individual Tax Identification Number (ITIN) when filing a return. Though individuals with ITINs are generally not eligible for tax-based benefits, they can receive refunds from overpayment of income taxes. In our FTB data, 5.6% of our full sample – or 1.9 million individuals, including 1 million dependents and 900,000 filers or spouses – filed using an ITIN. Of the 5.1 million individuals on tax returns that include at least one CalFresh recipient, 7%, or 360,000, have an ITIN.

Only tax units in which both the head and spouse have a valid SSN can claim the federal EITC, and only dependents with a valid SSN can be a qualifying child. In 2017, eligibility for the California EITC was also restricted to tax units in which both head and spouse had a valid SSN, though ITIN filers became eligible for the CalEITC in 2020. Individuals with a valid SSN can enroll in CalFresh even if another household member does not have a valid SSN, though the individual without a valid SSN cannot could toward the enrollees' household size and thus the calculation of their benefit amount.

We account for the 2017 SSN rules when assigning CalEITC eligibility to actual tax units. We disqualify any actual tax unit with a head or a spouse who has an ITIN, and we disregard dependents without a valid SSN when determining which might be qualifying children. For non-filers, we assume that our simulated tax units would file returns according to the composition imputed to them. In other words, these simulated tax units would not include a head, spouse or dependent unobserved in the CalFresh records who might not have a valid SSN.

Of the 400,000 CalFresh recipients who are on a tax return with an individual with an ITIN,[2] 94% appear in our data as Hispanic, even though Hispanic individuals represent just 56% of all CalFresh recipients who appear on a 2017 state return. This suggests that that Hispanic tax filers may be more likely to lose eligibility for the EITC due to the documentation status of someone in their tax unit, even conditional on their receipt of CalFresh.

Unassigned dependents

As discussed in Section 3.4, we are unable to assign 732,000 non-filers (including 285,000 children) to an imputed tax unit. For 99.9% of these children, there are no potential tax filers on their reference case, and for 93%, there are no adults. The expectation is that many of these "child-only" reference cases represent households in which parents or guardians are not eligible for CalFresh due to their documentation status.

To test whether this expectation is accurate, we rerun our tax unit imputation procedure on the universe of CalFresh recipients who *do* appear on a state tax return. Of the 2,293,000 individuals assigned to be dependents, we are unable to assign 902,000 dependents (693,000 of which are children) to an imputed tax unit – again, because these individuals are on reference cases without a likely adult filer. Over 87% of these unassigned dependents appear on tax returns in which neither the head nor spouse enrolled in CalFresh. Of the dependent children on reference cases without a likely filer, 43.5% appear on a tax return with at least one individual with an ITIN. In other words, as expected, a large share of child-only reference cases represent households in which an adult is ineligible for CalFresh because they do not have a valid SSN. Hispanic children are over-represented in both the count of unassigned children and the count of unassigned children with ITINs present on their tax returns. Overall, 61% of children enrolled in CalFresh are identified as Hispanic, but Hispanic children comprise 79% of those who are not assigned to an imputed tax unit. Further, 52% of Hispanic children who are not assigned to an imputed tax unit have at least one individual with an ITIN in their actual tax unit, while the average across all other races is 11%. Overall, it appears more likely that we will not be able to match a Hispanic child with their correct imputed tax unit because of issues related to documentation status, relative to children of other races.

[2] We can use the racial data in our CalFresh records to study which recipients are most likely to appear on tax returns with an ITIN filer. We can only see the race of individuals who are enrolled in CalFresh, so we cannot observe the race of the individuals who have an ITIN.

Impact of the modifying the SSN rule

In June 2020, California expanded eligibility for the CalEITC to ITIN filers with young children. To model the impact of this expansion, we consider the set of e-filers in 2017 (not limited to CalFresh recipients) and count the number of households who might be eligible for the CalEITC if not for a filer or spouse having an ITIN.

We take as given the actual tax unit composition and the income reported on the tax return, as we do with our imputations described in the main part of the paper. However, since ITIN filers cannot claim either the federal or state EITC, we cannot use information from those claims to infer which dependents on the return might be qualifying children. We also cannot rely on CalFresh casefiles for the residency test, since not all e-filers with an ITIN appear in our CalFresh records. Instead, we assume that all dependents are qualifying children.

In 2017, 611,607 returns included a filer or spouse with an ITIN (Appendix Table B.4). Of these returns, we estimate that 67% would be eligible for the federal EITC and 25% for the CalEITC if not for the SSN test. The average federal EITC amount for these tax units would be $3,133 and $307 for the CalEITC. If these tax units were allowed to claim either credit, an additional 907,217 children would become eligible for the federal EITC and an additional 231,515 children would become eligible for the CalEITC. If these households were allowed to claim both credits, they would be eligible to receive nearly $47 million from the CalEITC and $1.3 billion from the federal EITC. We also report these statistics separately for single ITIN filers and married filers in which one or both filers have an ITIN. Eligibility rates for the federal and state EITC are highest for single filers, followed by married units in which both filers have an ITIN, and then mixed status couples.

The second group of tax units impacted by the repeal of either the federal or California SSN test are those in which all heads or spouses have SSNs, but at least one dependent has an ITIN. We observe 208,335 such tax units. If these dependents became eligible, the number of qualifying children on the average affected tax unit would increase from .8 to 2.2. Of these returns, 50% would be eligible for the federal EITC (up from 22%), and 13% would be eligible for the CalEITC (up from 8%). In total, an additional 179,404 children would become eligible for the federal EITC, and 39,236 for the CalEITC, and their tax units would be eligible for an additional $304 million from the federal EITC and $9.7 million from the CalEITC. The average federal EITC amount for these households would be over $1,500 (up from $500), while the average CalEITC amount would be just $47 (up from $13).

B.4 Assigning non-filing CalFresh recipients to be heads/spouses or dependents

As part of our construction of simulated tax units among non-filing CalFresh recipients, we predict whether a given recipient is likely to appear as a filer (a head or spouse) or a dependent on their unit's return if one were to be filed. Below, we describe how we use information in our CalFresh files to predict these roles for each individual.

First, we assign all individuals under the age of 18 or over the age of 80 to be dependents. Among CalFresh recipients who appeared on tax returns, 99.4% of individuals under the age of 18 are dependents. Second, we assign all individuals over the age of 80 to be dependents as well. We do this because (1) the vast majority (80%) of CalFresh recipients age 81 plus appear on a tax return as a dependent and (2) there are not enough individuals in this age bracket for our prediction method to produce reliable results.

Second, we take the pool of individuals between the ages of 18 and 80, and we predict whether each are a dependent or filer. While most of these working-age (or near working-age) individuals would likely be filers, a non-trivial share still appear as dependents in the tax records (Appendix Figure B.2). Some of these adults might be too young to work, might still be in school, or are working but still residing with parents. Others could be older, unable to work, and are being cared for by working-age children. Others might have disabilities or other challenges which prevent them from working and qualify as a dependent.

We predict which non-filing adults might be dependents by identifying how characteristics observed in our CalFresh relate to whether filers appear as dependents or heads/spouses on their returns. We randomly assign 70% of individuals on CalFresh who appear on a tax year 2017 return to a training dataset and assign the remaining 30% to a test dataset. We estimate the following logit on the observations in the training set.

$$\ln\left(\frac{p(Y_i = \text{Filer} \mid X_{ij}))}{1 - p(Y_i = \text{Filer} \mid X_{ij})}\right) = \beta_0 + \beta_j \mathbf{X}_{ij}$$

X_i is a vector of characteristics we observe for each individual in the CalFresh data, including: whether English is their primary language; whether they can be merged to EDD earnings records; whether they receive cash assistance or SSI; whether the individual is incarcerated, is a senior, is a non-resident; the size of the individual's reference case; and the individuals' age, interacted with their EDD wage income, number of unique CalFresh cases over the course of the tax year, whether the individual is disabled, a categorical race variable, number of months enrolled on CalFresh, a binary sex variable, and the number of persons on the individual's reference case.

Using estimates from this model, we constructed a predicted probability that each observation is a filer or dependent. We apply the predicted probabilities for each individual to the test set in order to select two probability cutoffs that we use for our final filer or dependent determination. All individuals with a predicted probability over the cutoff are set as filers. The first cutoff (50%) is the cutoff that minimizes error across the entire test dataset, with a prediction accuracy in the test set of 88%. The second set of cutoffs utilizes prior-year tax information for the test-set and allows the cutoff to vary across three groups: Individuals who were a head or spouse on a tax return last year (a 4% cutoff), individuals who were a dependent last year (9%), and individuals who did not file last year (46%). With these cutoffs, we achieved an accuracy rate in the test set of 92%. This second version is used throughout the following paper.

B.5 Accuracy of earnings information

For filers, we can use households' actual reported earnings and income to measure eligibility. For non-filers, our estimates of eligibility hinge on our assumption that the sum of EDD wages is a reliable measure of households' true total earned income. Since we do not observe tax information for non-filers, we cannot test this assumption. Instead, in the following section, we present evidence that EDD wages do often reflect households' total earned income among CalFresh filing households.

For all actual tax units containing only CalFresh-enrolled filers (ie, single filers enrolled in CalFresh or both head and spouse in married filing jointly households enrolled in CalFresh), we compare the sum of UI-covered wages for the head (and spouse, for married couples) to their total reported AGI and California wages. Appendix Figure B.3a presents the distribution of differences between California wages and total EDD wages at the tax unit-level. The median difference is $0, and the mean difference is $300. For 2% of returns, the difference is greater than $20,000 dollars. Appendix Figure B.3b presents the distribution of differences between AGI and total EDD wages at the tax unit-level. The median difference is $0, and the mean difference is $1,800. For 5% of returns, the difference is greater than $20,000 dollars.

Among the households in which the difference between AGI and total earned income is greater than $20,000, 15% report only self-employment income and have no reported EDD earnings. Our inability to measure earned income correctly for this class of tax units does not risk our overestimating EITC eligibility, since we would assume that these households have no earnings and we would classify all of them as ineligible.

Appendix Figure B.4 plots the distribution of reported self-employment income among actual CalFresh tax units. Seventeen percent of CalFresh tax units reported positive self employment income in tax year 2017. Among those with self-employment earnings, the average amount was $9,165 and the median was $8,689. Ten percent (or more than half of those with any self-employment earnings) reported self-employment income less than $10,000. Ninety-five percent of these tax units had self-employment income below $20,500. The two clear masses of filers in Appendix Figure B.4 correspond to the kink points in the federal EITC schedule.

B.6 Predicted versus actual CalEITC claim

In Appendix Table 3.6, we present estimates of CalEITC eligibility and participation among CalFresh tax filers who claimed the federal EITC. Since the number of qualifying children claimed for the purposes of the EITC is not available in our tax records, we use the value of each tax unit's federal EITC claim, plus their earned income, to infer their number of qualifying children. Using this inference, along with earnings information available in their tax return, we determine each tax unit's eligibility for the CalEITC, as well as an estimated credit amount for those units that are eligible. To further validate the reliability of this inference, the following section discusses how our predicted CalEITC amounts compare to actual claimed amount among tax units who claimed the credit.

Of the 752,597 tax units with a head or a spouse on CalFresh and who claimed the Federal EITC, we observe 489,679 units claiming the CalEITC (489,262 eligible claimants and 410 apparently ineligible claimants). Of these, we fail to exactly predict the credit amount received for just 14,019 units, or 2.86% of all claiming units. On average, our predicted credit amount exceeds the credit amount such units actually received by roughly $235 dollars, while the median difference between predicted and actual credit values is $83.

In order to explain the source of these errors, we experimented with systematically varying the inputs to our CalEITC predictions. We begin by substituting each tax unit's earned income with their AGI in our credit calculator. The replacement of earned income with AGI allows us to match our predicted and the actual credit amount for 968 (7%) of the 14,019 tax units where we initially observed errors in our predictions. Though households are supposed to use their earned income to determine their correct eligible CalEITC amount, it is possible that a small number of preparers inputted the incorrect earnings variable.

For the remaining 13,051 units, we experimented with adjusting the number of qualifying children used in our credit amount predictions. We vary the possible number of qualifying children between 0 and 3 for each tax unit and calculate predicted credit amounts. This exercise allows us to recover the actual claimed amounts for an additional 10,925 units. For approximately 68% of these units, increasing or decreasing the count of QC by 1 child yielded accurate predictions of actual credit amounts.[3]

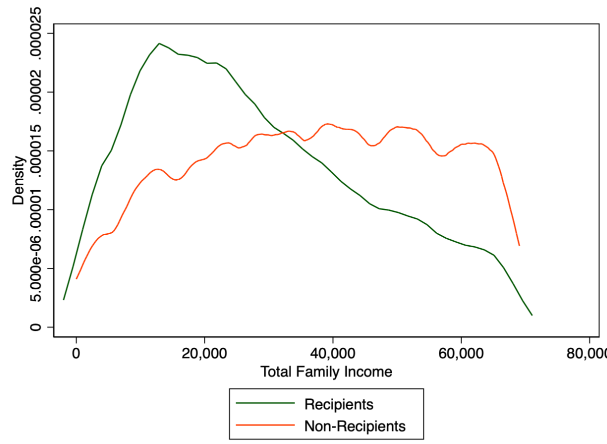
Finally, we experiment with both substituting AGI for earned income and varying the number of qualifying children for the 2,126 tax units for whom the above substitutions did not produce a predicted credit equal to their actual credit. In doing so, we are able to match predicted and actual credit amounts for an additional 285 tax units.

Following the above exercises, we are left with only 1,841 tax units (approximately 0.38% of the full sample of tax units claiming the CalEITC) for which no combination of substitutions above yielded an exact match between predicted and actual credit amounts.

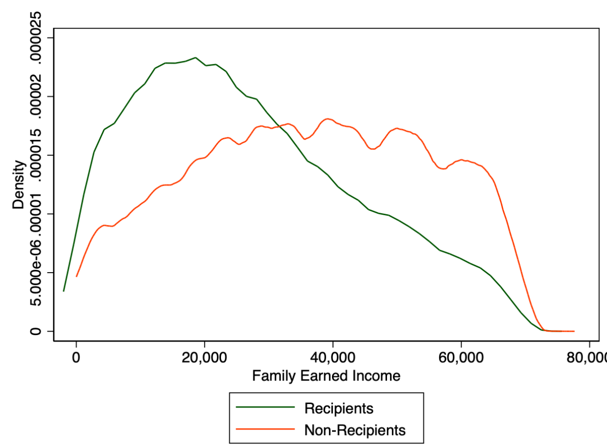
[3]One possible explanation for this pattern of results is that these tax units may have had dependents who did not reside in the state of California for the requisite 6 months required to satisfy the state residency requirement of the CalEITC to be counted as qualifying children. Using CalFresh records, however, we find no evidence that dependents in this subset of tax units were systematically less likely to appear in CalFresh records, either on their own or matched with heads or spouses on their reference case, throughout the year than dependents on the broader sample of tax units included in this analysis. Likewise, such dependents were not more likely to fail at the age test requirement for the CalEITC as compared to the broader sample of dependents.

Appendix Figure B.1: Distribution of income sources for SNAP and non-SNAP families in California, 2017 5-year ACS sample

(a) Total family income



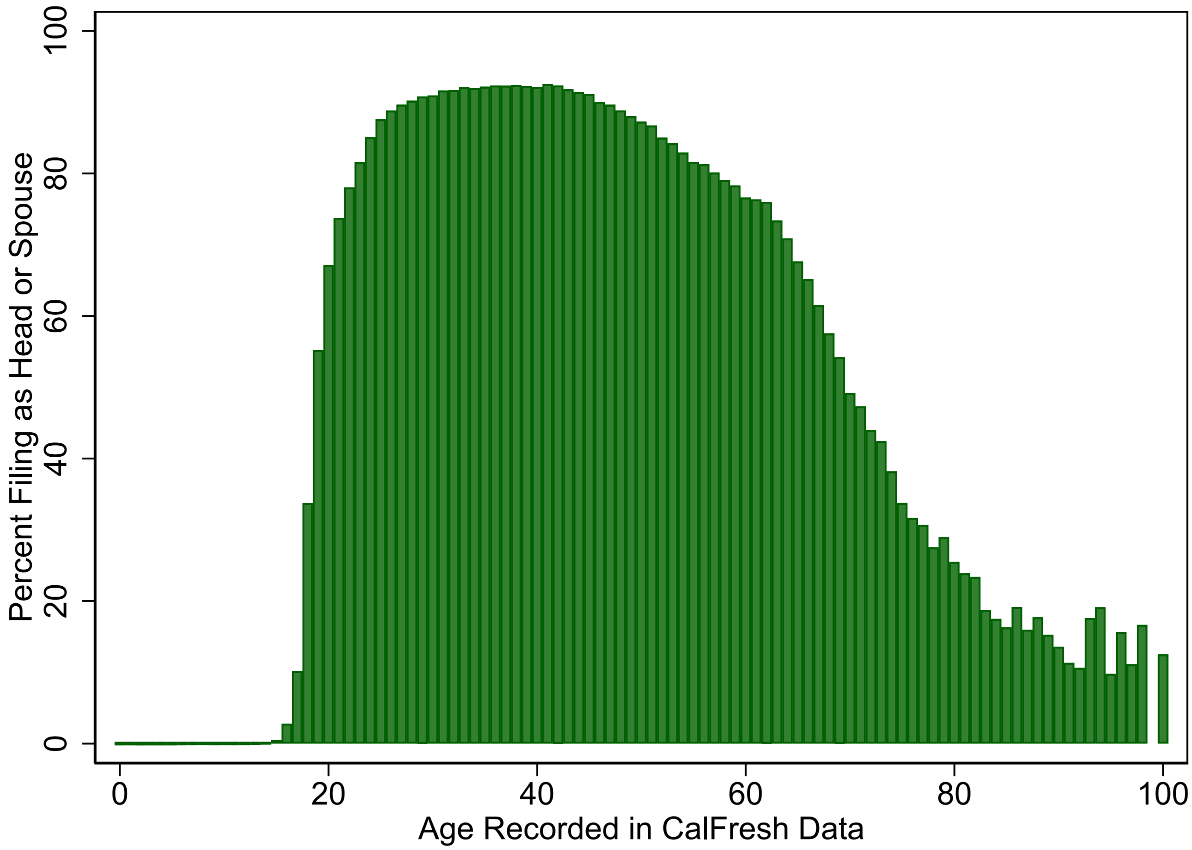
(b) Family earned income



(c) Family wage income

Notes. Constructed using the 2017 ACS 5-Year Sample, restricted to 18–64-year-olds with total family income between $0 and $69,063, using ACS person-weights, excluding individuals in group quarters. Each figure excludes individuals with negative income of the graphed type.

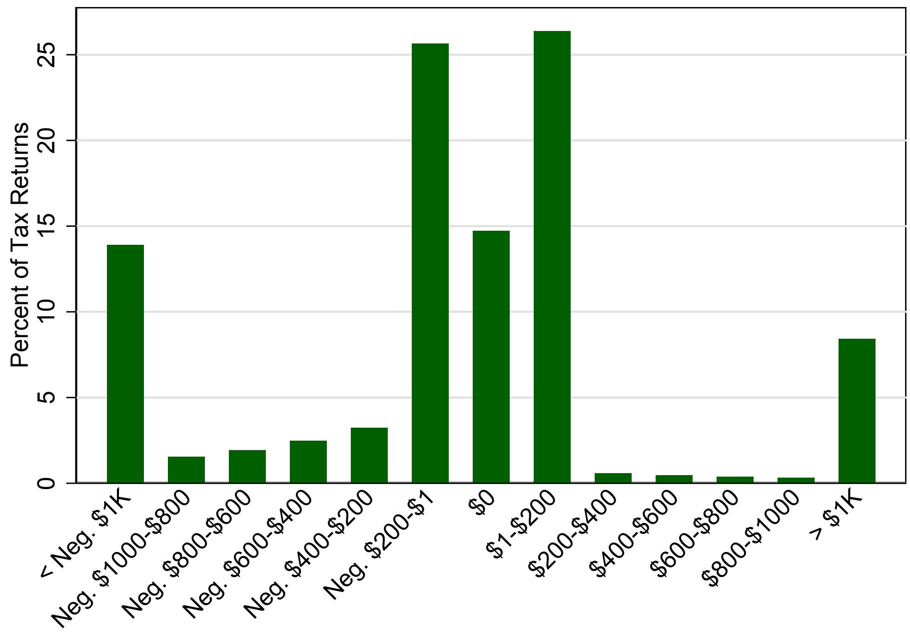
Appendix Figure B.2: Distribution of ages among dependents on CalFresh tax units, TY 2017



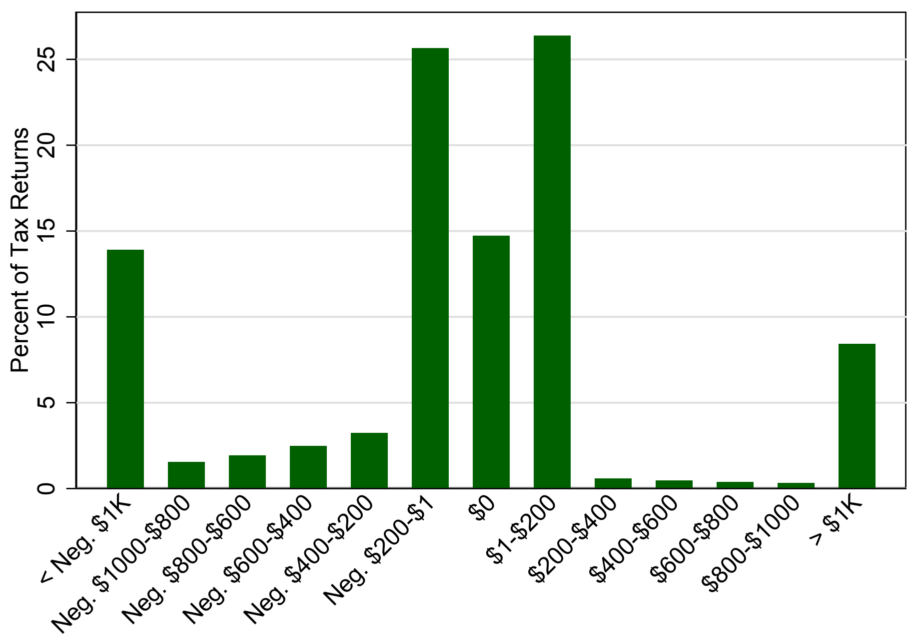
Notes. Universe is all individuals on tax year 2017 returns who also enrolled in CalFresh that year. We group ages into one year bins, according to enrollees' reported date of birth, and report the share in each bin who appear on their tax return as head/spouse as opposed to dependent.

Appendix Figure B.3: Distribution of differences between total EDD earnings and reported California wages among tax units with all CalFresh-enrolled heads and spouses, TY 2017

(a) 540 wages versus EDD wages

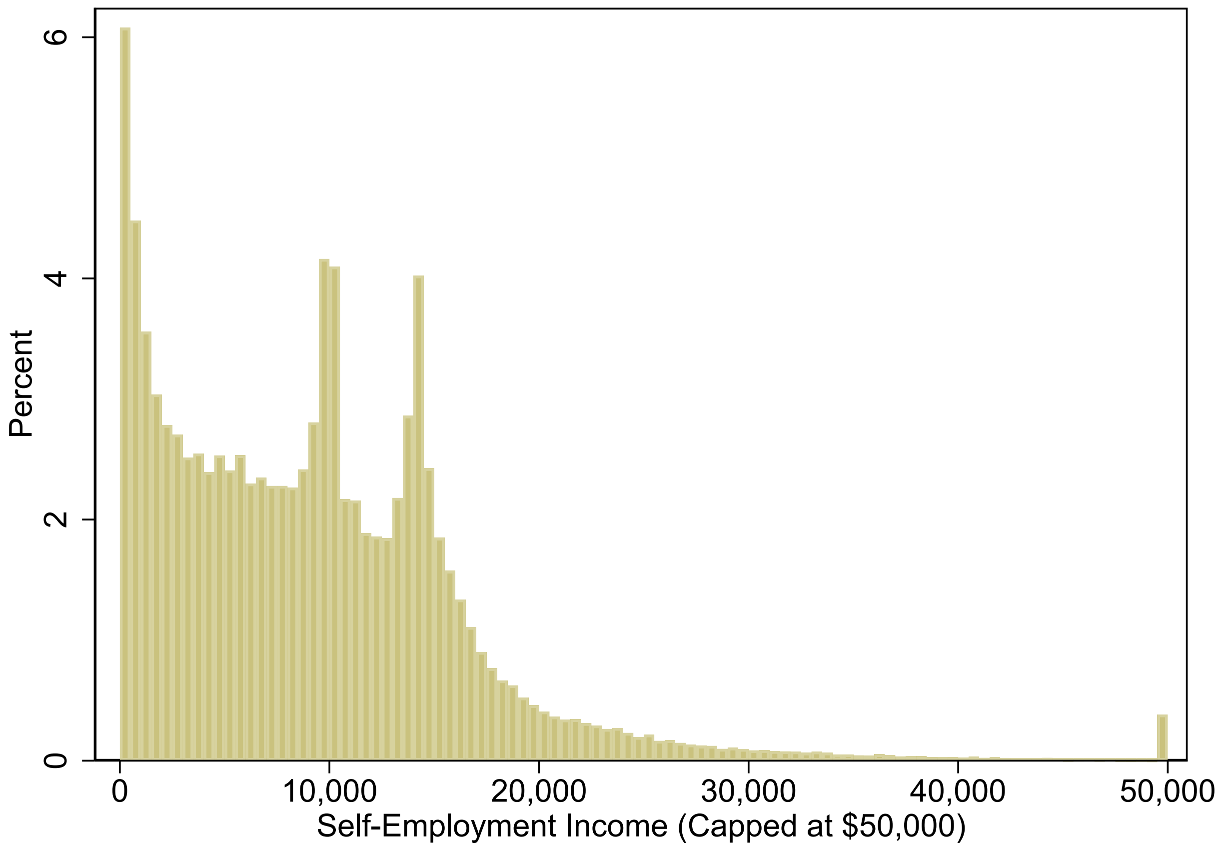


(b) AGI versus EDD wages



Notes. Universe is all tax returns with head and spouse (if present) enrolled in CalFresh. EDD wages are total of all 2017 quarterly wage earnings for head and spouse (if present) on return.

Appendix Figure B.4: Distribution of self-employment income among CalFresh-enrolled tax units with positive self-employment income, TY 2017



Notes. Universe is all tax returns with head and/or spouse (if present) enrolled in CalFresh and positive self-employment income.

Appendix Table B.1: Comparison of means between SNAP recipient and non-recipient families

	Non-recipients	**Recipients**	**Total**
Sample Size	9,677,982	2,345,127	12,023,109
Family composition			
Family size	2.8	4.1	3.0
Count of adults	2.0	2.3	2.0
Count of children	0.6	1.6	0.8
Count of elderly	0.2	0.1	0.2
Demographics (percent)			
Married	38%	34%	37%
Non-white	41%	44%	42%
Black	6%	10%	7%
Hispanic	44%	58%	46%
Income (median)			
Total family income	$35,886	$24,000	$33,094
Family earned income	$31,566	$19,649	$29,474
Family wage income	$28,957	$17,581	$25,885
Family investment income	$0	$0	$0
Income (percent)			
Positive investment income	7%	2%	6%
Investment income over the EITC cap	0%	0%	0%
$0 total family income	5%	4%	5%
$0 family earned income	14%	17%	14%
$0 family wage income	19%	22%	20%

Notes. Constructed using the 2017 ACS 5-Year Sample. Restricted to 18–64-year-olds with total family income between $0 and $69,063, constructed using ACS person-weights, excluding individuals in group quarters.

Appendix Table B.2: Comparison of means between e-filers and paper / web filers

	Tax Preparation Method		
	Paper or Web Filer	**E-Filer**	**Total**
Number of Tax Units	2,113,078	14,589,033	16,702,111
Filing status and number of dependents			
Share single	50%	48%	48%
Share head of household	12%	15%	14%
Share married filing jointly	35%	37%	36%
Mean number of dependents	0.6	0.7	0.7
EITC participation			
Share claiming CalEITC	8%	9%	9%
Mean CalEITC amount	$224	$266	$262
Share CalEITC eligible	13%	14%	14%
Share claiming CalEITC among eligible	41%	54%	52%
Income			
Mean Federal AGI	$64,644	$73,389	$72,282
Median Federal AGI	$38,009	$42,823	$42,239
Mean CA wages	$46,972	$55,341	$54,282
Median CA wages	$24,732	$31,482	$30,708
Other characteristics			
Share on CalFresh	6%	8%	7%
Filed with Paid Preparer	26%	68%	62%
Self-Prepared	73%	30%	36%
Filed with VITA	0%	2%	2%
Share with ITIN on Return	6%	6%	6%

Note: Restricted to head filers on tax returns. For our EITC imputations, we use that California wages and Federal AGI reported on F540 to represent earned income and adjusted gross income, and we assume that no tax unit has investment income. In the income statistics reported above, we top code both California wages and Federal AGI at the 99th percentile (excluding $0s) to avoid the impact of potentially erroneous outliers. Including those outliers does not impact the median amounts but does increase the mean Federal AGI amount to $87,614 for e-filers and $155,117 for paper filers, and the mean California Wage amount to $62,631 for e-filers and $396,269,984 for paper-filers.

Appendix Table B.3: Comparison of means between e-filers and paper filers in 2017, among tax units that claim CalEITC

	Tax Preparation Method		
	Paper or web filer	**E-filer**	**Total**
Count of tax units	144,796	1,317,854	1,462,650
Income information			
Mean AGI	$9,862	$10,989	$10,877
Mean earned income	$9,781	$10,806	$10,705
Mean wage income	$6,885	$8,338	$8,194
Mean investment income	$47	$39	$40
Other characteristics			
Mean number of qualifying children	0.7	0.8	0.8
EITC information			
Mean CalEITC amount (if positive)	$196	$239	$235
Federal EITC amount (if positive)	$1,883	$2,233	$2,198

Notes: Restricted to head filers on tax returns with positive CalEITC amounts reported on the Schedule 3514.

Appendix Table B.4: Estimated EITC eligibility among tax units with a head or spouse with an ITIN

	CalEITC	Federal EITC	Either EITC
Count of tax units	611,607	611,607	611,607
Without SSN test			
Single filers			
% eligible	36%	77%	78%
Number eligible	108,774	232,806	233,702
Mean EITC amount	$289	$3,014	$3137
Total EITC amount	$31,394,302	$701,719,023	$733,113,325
Married filing jointly, one filer has ITIN			
% eligible	10%	43%	43%
Number eligible	15,245	64,009	64,185
Mean EITC amount	$313	$2,875	$2942
Total EITC amount	$4,775,348	$184,036,537	$188,811,885
Married filing jointly, both filers have ITIN			
% eligible	18%	71%	71%
Number eligible	29,114	113,543	113,670
Mean EITC amount	$370	$3,440	$3531
Total EITC amount	$10,768,346	$390,556,317	$401,324,663
Total			
% eligible	25%	67%	67%
Number eligible	153,133	410,358	411,557
Mean EITC amount	$307	$3,110	$3,215
Total EITC amount	$46,937,996	$1,276,311,877	$1,323,269,873

Notes. Universe is e-filed tax returns containing a a head or spouse (if present) with an ITIN. We report four statistics (share and number eligible for either federal or state EITC, plus the mean and total amounts claimable) for three populations (single filers, married joint filers in which one spouse has an ITIN, and married joint filers in which both have an ITIN).

Appendix Table B.5: Estimated EITC eligibility among tax units containing only a dependent with an ITIN

	CalEITC	Federal EITC	Either EITC
Count of tax units	208,335	208,335	208,335
With SSN Test			
Mean count of QC	0.8	0.8	0.8
% eligible	8%	22%	23%
Mean EITC amount	$13	$480	$2,175
Total EITC amount	$2,752,525	$100,071,072	$102,823,597
Without SSN Test			
Mean count of QC	2.2	2.2	2.2
% eligible	13%	50%	50%
Mean EITC amount	$47	$1,462	$3,030
Total EITC amount	$9,769,810	$304,605,333	$314,375,143

Notes. Universe is e-filed tax returns containing a dependent with an ITIN, but both head and spouse (if present) have a valid SSN. Panel A reports average number of QCs, share eligible for either federal or state EITC, and the mean and total amounts claimed. Panel B reports same statistics assuming that dependents with an ITIN could be qualifying children. Dependents must still pass age test, but all are assumed to pass residency test.

Appendix Table B.6: Relationship between number of inferred qualifying children from federal EITC claim and number of qualifying children reported on Schedule 3514 and total number of dependents, among CalFresh households claiming federal EITC and positive CalEITC amounts

	Number of qualifying children inferred from Federal EITC			
	0	1	2	3+
Dependents claimed on tax return				
0	95.4%	0.5%	0.2%	0.2%
1	3.9%	93.5%	0.6%	0.2%
2	0.6%	5.4%	94.4%	0.5%
3+	0.1%	0.6%	4.8%	99.1%
Number of qualifying children reported on Sch 3514				
0	99.9%	5.0%	4.7%	3.8%
1	0.1%	94.8%	2.5%	1.6%
2	0.0%	0.1%	92.6%	1.5%
3+	0.0%	0.0%	0.2%	93.1%
N	119,621	181,479	124,875	66,314

Notes. Universe is e-filed tax returns linked to at least one CalFresh participant and a non-zero CalEITC claim on the Schedule 3514. Columns represent number of EITC QCs inferred using process described in Section 3.4. Rows in Panel A are dependents reported on tax return. Rows in Panel B are number of QCs reported on Schedule 3514 for purposes of CalEITC claim. Cells represent column percentages.

Appendix Table B.7: Relationship between number of inferred qualifying children from CalFresh records and number of qualifying children reported on Schedule 3514 and total number of dependents, among CalFresh households claiming federal EITC and positive CalEITC amounts

	Number of qualifying children inferred from CalFresh records			
	0	1	2	3+
Dependents claimed on tax return				
0	62.2%	0.0%	0.0%	0.0%
1	25.8%	76.9%	0.0%	0.0%
2	9.2%	19.0%	82.7%	0.0%
3+	2.8%	4.1%	17.3%	100%
Number of qualifying children reported on Sch 3514				
0	66.7%	4.6%	4.1%	3.5%
1	24.2%	77.4%	2.9%	1.6%
2	7.2%	15.3%	79.9%	1.9%
3+	1.9%	2.7%	13.2%	93.0%
N	185,820	165,375	96,723	44,371

Notes. Universe is e-filed tax returns linked to at least one CalFresh participant and a non-zero CalEITC claim on the Schedule 3514. Columns represent number of EITC QCs inferred using process described in Section 3.4. Rows in Panel A are dependents reported on tax return. Rows in Panel B are number of QCs reported on Schedule 3514 for purposes of CalEITC claim.

Appendix Table B.8: Relationship between number of inferred qualifying children from CalFresh records (in which we disqualify dependents who are not observed in CalFresh records) and the number of qualifying children reported on Schedule 3514 and total number of dependents, among CalFresh households claiming federal EITC and positive CalEITC amounts

	Number of qualifying children inferred from CalFresh records			
	0	1	2	3+
Dependents claimed on tax return				
0	24.5%	0.0%	0.0%	0.0%
1	35.1%	53.2%	0.0%	0.0%
2	25.7%	31.7%	66.3%	0.0%
3+	14.7%	15.2%	33.7%	100%
Number of qualifying children reported on Sch 3514				
0	28.7%	8.3%	8.7%	6.2%
1	35.3%	54.3%	15.2%	7.0%
2	23.5%	26.2%	53.7%	10.8%
3+	12.5%	11.2%	22.4%	75.9%
N	471,715	17,866	2,339	369

Notes. Universe is e-filed tax returns linked to at least one CalFresh participant and a non-zero CalEITC claim on the Schedule 3514. Columns represent number of EITC QCs inferred using process described in Section 3.4, amended to disqualify any dependents who cannot be matched to our CalFresh records. Rows in Panel A are dependents reported on tax return. Rows in Panel B are number of QCs reported on Schedule 3514 for purposes of CalEITC claim.

Appendix Table B.9: Simulated CalEITC eligibility using CalFresh-based QC imputation process among tax units with CalFresh-enrolled head or spouse and who claimed the federal EITC

	Tax units	CalEITC eligible		Eligible non-claimants		
	Count	Count	Share	Count	Share	Amount
For all filers						
Total	754,950	100%	68%	38,941	8%	$217
By filing status and number of qualifying children						
Single						
No QCs	114,737	15%	99%	5,616	5%	$81
1 QC	224,558	30%	73%	12,687	8%	$208
2 QCs	159,830	21%	68%	9,322	9%	$333
3+ QCs	85,611	11%	61%	5,103	10%	$354
Married						
No QCs	16,676	2%	73%	715	6%	$60
1 QC	38,970	5%	50%	1,468	8%	$115
2 QCs	55,223	7%	42%	2,075	9%	$173
3+ QCs	59,345	8%	33%	1,955	10%	$164
By total earnings (thousands)						
$0-$5	83,266	11%	94%	2,584	3%	$380
$5-$10	140,411	19%	99%	6,509	5%	$568
$10-$15	179,530	24%	97%	11,780	7%	$229
$15-$20	114,685	15%	79%	12,827	14%	$92
$20-$25	86,407	11%	37%	5,241	16%	$10
$25-$30	62,638	8%	0%	0	.	.
$30+	88,013	12%	0%	0	.	.

Notes. Universe is e-filed tax returns linked to at least one CalFresh participant that included a claim for a non-zero federal EITC. The number of qualifying children for each tax unit is calculated using the process described in Section 3.4, as opposed to Section 3.4. To observe the effect of this alternative QC inference process, results reported here can be compared against those reported in Table 3.5. Column 1 reports the total number of tax units that meet those criteria. Column 2 reports the count of those tax units that were eligible for the California EITC. Column 3 reports what share of all returns were eligible for the CalEITC. Column 5 reports the number of eligible returns that did not claim the CalEITC, and Column 6 reports the share of eligible units that did not claim. Column 7 reports the average imputed amount among those non-claimers for each cell.