Interventions to Bolster Benefits Take-Up: Assessing Intensity, Framing, and Targeting of Government Outreach

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Abstract

Behaviorally-informed "nudges" are widely used in government outreach, but face criticism for being too modest to address poverty at scale. Indeed, when used to increase the take-up of social safety net programs, results are often mixed. In this study, we test adjustments to behavioral interventions that are hypothesized to increase their effectiveness. In four field experiments over two years (*n* = 542,804), we examine whether more proactive communication, variations in message framing, and more precise targeting increase take-up of critical anti-poverty benefits in California, above and beyond traditional light-touch approaches. Our interventions targeted extremely vulnerable low-income households, many of whom have no prior-year income and so were at risk of missing out on large cash transfers during the Covid-19 pandemic, namely the Child Tax Credit and economic stimulus payments. We find that light-touch interventions significantly and consistently increase take-up by 0.14 to 2 percentage points – a 150% to over 500% relative increase – regardless of message, sample, timing, or modality, resulting in over $4 million disbursed to low-income families. Importantly, we show that light-touch outreach was extremely cost-effective in this context: every $1 spent yielded from $50 to over $8,000 in benefits disbursed. However, higher-touch proactive outreach, refined messaging, and precise targeting yielded minimal additional benefits, with proactive outreach delivering a negative return on investment. These findings suggest an urgent policy need to rethink what outreach strategies – if any – can do better than a "nudge" if we are to close the take-up gap in anti-poverty programs.

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One of the most pervasive social challenges in lifting people out of poverty is how to ensure that those who are eligible for government assistance receive it. Although means-tested programs have positive long-term impacts on economic, health, and education outcomes (Aizer & Persico, 2022; Chetty et al., 2013; Hoynes et al., 2016), 20% to 50% of Americans do not take up programs for which they are eligible (Giannarelli, 2019; IRS, 2024b; Vigil, 2022). Leaning on evidence from other domains, policymakers are increasingly turning toward behavioral approaches to increase take-up, including proactive government outreach, navigation assistance, and process simplification (Benartzi et al., 2017; OES, 2024b; OES, n.d.; The White House, 2023a). Yet, experimental research examining the impact of these strategies within the social safety net has yielded mixed results and smaller-than-anticipated effect sizes (Bird et al., 2021; De La Rosa et al., 2021; Finkelstein & Notowidigdo, 2019; Linos et al., 2022).

To explain why light-touch behavioral approaches have often failed to deliver desired effects in these contexts, three hypotheses emerge. First, it is possible that the barriers to action in these contexts are simply too high for light-touch outreach to move behavior; higher-touch approaches may be necessary, especially for the most vulnerable households. But while some studies have found significantly larger effects from higher-touch methods that provide direct assistance in navigating complex administrative processes (Bettinger et al., 2012; Finkelstein & Notowidigdo, 2019; Schanzenbach, 2009), others have not (Cook et al., 2015; Grommon et al., 2013). Second, drawing on evidence that message framing matters, it is commonly hypothesized that where light-touch outreach has been ineffective, it is because it has used the "wrong" message. Extant literature documents ways in which subtle variation in framing or messaging can affect behaviors from voting to vaccines to 401k enrollment (Milkman et al., 2022; Nickerson & Rogers, 2010; Thaler & Benartzi, 2004), although more recent evidence suggests that the impact of varying theoretically-driven and well-calibrated messages may be smaller than previously assumed (Avery et al., 2021; Buttenheim et al., 2022; Milkman et al., 2024). In the context of the social safety net, existing evidence is especially mixed: some studies have found that the framing of light-touch outreach significantly affects behavior; others have found that it affects initial engagement but not necessarily target behavior; and still others have found that it has no effect – even within similar programmatic contexts (Bhargava & Manoli, 2015; De La Rosa et al., 2021; Linos et al., 2022). Third, mass outreach, by definition, reaches many people who are not eligible to take action. Thus, it is often hypothesized that for light-touch outreach to be effective, it needs to be more well-targeted at people who are both eligible and "movable." Indeed, some evidence suggests that improved targeting of interventions to those who are most likely to be eligible and movable translates into larger effect sizes (Allcott, 2015; Evans et al., 2016; Saccardo et al., 2022).

We directly test these three hypotheses in four pre-registered, large-scale field experiments (n = 542,804) in California in the context of the Covid-19 stimulus payments (Economic Impact Payments, EIPs) and the expanded federal Child Tax Credit (CTC). Our results cast doubt on all three hypotheses as important explanations for the failure of outreach to have larger effects in the context of the social safety net. First, we find that light-touch interventions consistently and significantly influence behavior, with effect sizes that range from 0.14 percentage points (pp) to 2 pp, compared to no-communication comparison groups.

Conservatively, we estimate that sending light-touch outreach to the full eligible population in each study would have yielded about $4.3 million in additional benefits disbursed to low-income households – and likely much more. Higher-touch interventions were, at most, only slightly more effective than more traditional light-touch approaches, which was not nearly enough to offset the cost. We find no consistent difference in the effect of different behaviorally-informed messages, nor do we find that outreach is significantly more effective among a better targeted sample. These results underscore the potential and limitation of behavioral “nudges” and point toward an urgent need to rethink the design of outreach strategies that aim to close the take-up gap in anti-poverty programs.

Increasing take-up of the Child Tax Credit and Economic Impact Payments

In response to the Covid-19 pandemic, the United States government disbursed a range of income-based stimulus payments, including an expanded CTC and three rounds of EIPs, to provide economic aid quickly to middle- and low-income Americans. These payments were disbursed automatically through the tax system, meaning families who had filed a tax return in the prior year received them without needing to take any action. However, families who had not filed taxes in prior years, typically because their incomes fell below the tax filing threshold, were at risk of not receiving the benefits. Importantly, under the expanded eligibility criteria, many of these families were also *newly* eligible for the CTC; they had not previously had any reason to file tax returns, but now stood to gain thousands of dollars if they did. As such, the informational barriers to claiming benefits were higher than in other contexts that have studied the take-up of long-standing tax-based benefits (e.g., Bhargava & Manoli, 2015).

Recognizing this challenge, the Internal Revenue Service (IRS) created a “non-filer tool” that offered a way for families to claim the CTC or EIPs without completing full returns, thus also reducing compliance hurdles associated with accessing benefits. Additionally, Code for America (CfA) partnered with The White House and the US Treasury Department to create a separate non-filer tool, GetCTC.org, that could be used for the same purpose. CfA also developed and runs a separate platform, GetYourRefund.org, that offers a streamlined way to file regular taxes for low-income individuals. These tools were available during specific claiming periods each year, loosely aligned with IRS’s annual tax filing and extended tax filing deadlines.

From 2021 to 2022 we conducted four randomized experiments in collaboration with the California Department of Social Services aimed at increasing claiming of the CTC and EIPs (see Table 1). Prior research estimated that around one-quarter of Californians enrolled in CalFresh (California’s Supplemental Nutrition Assistance Program) or CalWORKs (California’s Temporary Assistance for Needy Families program) were at risk of not receiving both the pandemic stimulus payments and the expanded CTC due to not filing returns (Augustine et al., 2021; Ramesh et al., 2021). These children and families were some of California’s most vulnerable – they tended to live in households with little to no earnings, often headed by a single adult. The average household with children could receive over $3,000 from claiming available credits at this time. Nearly 90% of households in our sample that received a refund had no earnings in 2020, meaning these benefits had a real and tangible impact on household income.

In all four experiments, CDSS sent outreach to low-income Californians to encourage them to use a non-filer tool or file a full tax return to claim the CTC or other available EIPs. The sample for each study was drawn from CDSS program enrollment data for CalFresh and CalWORKs, which includes households that were likely income-eligible to claim the expanded CTC and EIPs. These data were also merged with state tax filing data from the California Franchise Tax Board (FTB) to identify adults and children who were at risk of not receiving the credit during the pandemic due to their past state tax return filing history – so-called "previous non-filers."

Study 1 and **Study 2** focused on previous non-filers only and tested the impact of light-touch outreach through recorded voice messages ("robocalls") and emails, respectively. In each study, we also tested different message framings. In Study 1, households were randomly assigned to receive a *standard* recorded voice message that provided basic information about the CTC or EIPs or a *salient assistance* message that provided the same information, as well as the phone number for a CfA-run national hotline that individuals could use to receive filing assistance from a live person. In Study 2, households were randomly assigned to receive a *psychological ownership* message, which emphasized that available tax credits "belong" to recipients or a *simplified process* message, which emphasized that the process of claiming available tax credits had been simplified.

Study 3 tested text message outreach. Similar to Study 2, households were randomly assigned to receive a text message that emphasized *psychological ownership* or a *simplified process*, or were assigned to a no-communication control group (20%). In Study 3, we also expanded the sample to include previous filers in addition to non-filers, in order to test the impact of more precise targeting. Because benefits were distributed automatically to individuals who had filed a tax return in previous years, our hypothesis was that previous non-filers were more likely to need to take action – and should thus be more "movable" – in this context.

In **Study 4**, our sample again focused only on previous non-filers (like Studies 1 and 2) and included higher-touch outreach by leveraging a CDSS hotline that was staffed by approximately 35 workers who were trained to answer questions about the tax filing process and the simplified filing tool, and could connect callers to other resources such as VITA sites. Households were randomly assigned to a *passive assistance* group, which was sent a text message with the phone number for the tax filing assistance hotline; an *opt-in assistance* group, which was sent a text message that asked recipients to reply "yes" if they wanted to receive a call from the assistance hotline; or an *opt-out assistance* group, which was sent a text message that informed recipients that they would also receive a call from the assistance hotline, but could opt-out via text. The *passive assistance* group in this study resembles the salient assistance condition in Study 1 and reflects one of the most common approaches governments use to connect residents with navigation assistance. We conceptualize proactive opt-out calls as higher-touch outreach.

The outcomes for Studies 1, 2, and 4 are return initiations and submissions via GetCTC.org. Because GetCTC.org was a tool intended specifically for disbursing the expanded CTC and EIPs, we can assume that any household that had a return accepted through this filing channel did receive benefits. In Study 3, which was conducted during the regular tax filing season, our outcomes are return initiations and submissions via GetYourRefund.org, as well as California state tax return filing. In this manuscript, we focus primarily on reporting results on return submissions, but all outcomes are included in the Supplement.

Results

The effect of light-touch outreach

First, we consider the effect of three different types of light-touch outreach. Results are shown in Table 2. Overall, recorded voice messages, emails, and text messages all had significant and positive impacts on behavior relative to no outreach. In Study 1, individuals that received a recorded voice message were 0.14 pp more likely to submit a return (OLS regression, $SE = 0.03$, $p < .001$, $n = 144{,}139$) during the week following outreach than households that did not receive a message. In Study 2, households that received an email were 1.9 pp more likely to submit a return (OLS regression, $SE = 0.11$, $p < .001$, $n = 47{,}697$) during the week following the first wave of emails than households that did not receive an email.

In Study 3, we tested text message-based outreach among both filers and non-filers – which means the sample differs from Studies 1 and 2, both of which only included non-filers. The Supplement reports results on the subsample of non-filer households, but results do not differ meaningfully. Among the full sample, we see that individuals that received text message outreach were 0.41 pp more likely to initiate a return (OLS regression, $SE = 0.04$, $p < .001$, $n = 278{,}241$), but no more likely to submit a return through GetYourRefund.org than individuals in the no-communication control group (OLS regression, $\beta = -0.01$ $SE = 0.03$, $p = .81$). However, because this study took place during the traditional April tax-filing season, it is possible that outreach recipients chose to file their taxes through other channels, rather than using GetYourRefund.org. Thus, we also examine the impact of outreach on California state tax return filing. We find that individuals that received a text message were significantly more likely to file a state tax return than individuals that did not receive a text message (OLS regression, $\beta = 0.48$ pp, $SE = 0.20$, $p = .02$, $n = 292{,}984$).

Across all three studies, we see that the effect of emails was an order of magnitude larger than recorded voice messages and text messages. However, it is important to note that the baseline filing rate varied across studies. Moreover, the effects of each outreach modality are not directly comparable due to the differences in samples, outcomes, and timing across studies. Despite this, in each study we consistently find that light-touch outreach has a small but positive effect on filing behavior, with effect sizes in line with related meta-analyses on the impact of light-touch government outreach and some studies on tax-based benefits take-up (Bhargava & Manoli, 2015; DellaVigna & Linos, 2022).

The effect of higher-touch outreach

In Study 4, we examined the impact of two higher-touch approaches to navigation assistance – *opt-in assistance* and *opt-out assistance* – compared to light-touch outreach. Given what we learned in Studies 1-3, we used text messages as the mode of delivery in Study 4 – both because they appeared to be more effective than recorded voice messages and because they could reach a larger population than emails.

We find that the method of offering higher-touch assistance significantly affected engagement. Among households assigned to the *opt-in assistance* group, just 72 (0.62%) responded affirmatively, indicating that they wished to receive a call from the tax filing assistance hotline. Of households assigned to the *opt-out assistance* group, only a randomly selected 52% (n = 12,171) ultimately received a live phone call from the tax filing assistance hotline due to time and capacity constraints; the remainder received a recorded voice message. Of the 12,171 live outbound calls made, just 172 (1.6%) opted out and hotline workers were able to reach and speak to people in 1,072 households (8.8%). This is consistent with other research that documents the power of opt-out versus opt-in approaches to enrollment (e.g., Bergman et al., 2020).

Results on our main outcomes are shown in Table 2. Overall, 1.2% of individuals in the *passive assistance* group initiated a return and 0.76% submitted a return via GetCTC.org in the outcome period. Individuals assigned to the *opt-in assistance* group were directionally – but not significantly – more likely to initiate (OLS regression, β = 0.13 pp, SE = 0.19, p = .50, n = 55,838) and submit (OLS regression, β = 0.07, SE = 0.15, p = .64) returns through GetCTC.org than individuals assigned to the *passive assistance* group. Meanwhile, individuals assigned to the *opt-out assistance* group were significantly more likely to initiate a return (OLS regression, β = 0.31, SE = 0.16, p = .05), but no more likely to submit a return (OLS regression, β = 0.11, SE = 0.12, p = .37) than those in the *passive assistance* group. We also find no significant difference in the likelihood of initiating or submitting a return between the *opt-in assistance* and *opt-out assistance* groups.

As a secondary analysis, we evaluate the effect of treatment among the subgroup of households that received a live call as intended (regardless of whether the call was answered). Individuals assigned to the *opt-out assistance* group that actually received an outreach phone call were 0.48 pp more likely to initiate a return (OLS regression, SE = 0.19, p = .01, n = 45,217) and 0.26 pp more likely to submit a return (OLS regression, SE = 0.15, p = .08) than households in the *passive assistance* group.

The effect of varying message framing

Across all studies where we varied message framing (Studies 1-3), we do not see a clear or consistent impact of framing on filing behavior (see Table 3). In Study 1, the s*alient assistance message* – which included the phone number for the CfA tax-filing assistance hotline – significantly increased engagement with the hotline from a baseline of 0.06% (OLS regression, β = 0.11, SE = 0.02, p < .001, n = 144,139). Yet, the *salient assistance message* had no effect on return submissions relative to the *standard message* (OLS regression, β = 0.00, SE = 0.06, p =.94).

Studies 2 and 3 both tested the impact of messaging that emphasized psychological ownership versus messaging that emphasized that the process of filing had been simplified. Psychological ownership messaging has been tested in similar contexts and shown to increase interest in claiming the Earned Income Tax Credit (EITC) by about 20% to 128% (De La Rosa et al., 2021). Our results, however, are mixed. In Study 2, we find that individuals who were assigned to receive the *psychological ownership* email were 0.15 pp less likely to submit a return (OLS regression, $SE = 0.15$, $p = .29$, $n = 47{,}697$) than households that were assigned to receive the *simplified process* email, though this difference is not statistically significant. Meanwhile, in Study 3, we find that individuals assigned to receive the *psychological ownership* text message were 0.05 pp more likely to submit a return through GetYourRefund.org (OLS regression, $SE = 0.02$, $p = .04$, $n = 278{,}241$) than individuals assigned to receive the *simplified process* text message. They were also 0.12 pp more likely to file a state tax return, though this difference is not statistically significant (OLS regression, $SE = 0.18$, $p = .50$, $n = 292{,}984$).

The effect of targeting

In Study 3, we examine heterogeneity by previous non-filer status to measure whether the impact of light-touch outreach is larger among a better-targeted – and presumably more movable – sample. Among those assigned to the no-communication control group in this study, 55.1% of primary contacts identified as previous non-filers in our sample filed a 2021 state tax return compared to 73.8% of primary contacts identified as previous filers. While this is a descriptive finding, it suggests that we were able to correctly identify individuals who were less likely to file taxes and for whom outreach should have thus been more relevant.

As shown in Table 4, we find that the effect of text message outreach on filing behavior is directionally – but not significantly – larger among previous non-filers than previous filers (OLS regression, $\beta = .35$ pp, $SE = 0.50$, $p = .48$, $n = 292{,}984$). Examining each subsample separately, we see marginally significant effects of outreach on state tax return filing among both groups, although the magnitude of the effect among previous non-filers is nearly twice as large as among previous filers (previous non-filers: OLS regression, $\beta = .75$ pp, $SE = 0.44$, $p = .09$, $n = 77{,}433$; previous filers: OLS regression, $\beta = .40$ pp, $SE = 0.23$, $p = .09$, $n = 215{,}551$). We also find no interaction between message content and targeting.

Cost-benefit analysis

The marginal cost of light-touch outreach is low: \$0.04 per recorded voice message, \$0.02 per text message, and \$0.003 per email. In Study 1, recorded voice messages cost about \$26 per additional return submitted; in Study 2, emails cost about \$0.15 per additional return submitted; and in Study 3, text messages cost just over \$4 per additional return submitted. This is comparable to – and in some cases, lower than – what others have found. For instance, Finkelstein and Notowidigdo (2019) found that providing information about SNAP enrollment cost around \$20 per additional enrollee. Because benefits were large, this also translates into a high return on investment: For every \$1 spent on recorded voice messages and emails, respectively, roughly \$58 and \$8,884 in benefits were disbursed to low-income families.

However, the marginal cost of higher-intensity outreach that requires labor costs – in this case, staffing a hotline to either receive or place proactive calls – is very high, ranging from \$200,000 to over \$2 million per year, depending on how many staff are hired. In Study 4, factoring in the marginal cost of both hotline staff and text message outreach costs, we find that offering passive assistance cost approximately $142 per additional return submitted, while offering *opt-out assistance* (proactive calls) cost $1,057 per additional return submitted.

Even when including estimated fixed costs (e.g., telecommunications infrastructure and equipment), the benefit-cost ratio in Studies 1, 2, and 3 far exceeds 1 (see Supplement). This is driven, in part, by how large the monetary benefits were in this context. But many social safety net programs have similarly large benefits: the average SNAP recipient receives just under $200 per month or approximately $2,172 per year (DeSilver, 2023), and TANF recipients can receive up to $500 per month, depending on the state and other eligibility requirements (Azevedo-McCaffrey & Aguas, 2024).

At the same time, the benefit-cost ratio of higher-touch outreach in Study 4 – both *opt-in assistance* and *opt-out assistance* – did not clear 1 under most sets of assumptions, once fixed costs are factored in. Larger effect sizes, lower costs, or larger benefits could all influence this calculation.

Discussion

Across four large-scale experiments, we find that light-touch interventions can effectively reduce take-up gaps at the margins – and, when benefits are large, bring millions of dollars to low-income households at a very low cost. Effect sizes are modest in absolute terms, ranging from 0.14 pp to 2 pp (on return submissions), but large in relative terms: these effects represent a 150% to over 500% increase over baseline return submission rates. Overall, these findings are in line with recent studies that have found that behaviorally-informed outreach increases target behaviors by about 2 pp, on average, but that the effect of varying message framing – when content is already theoretically-driven and well-calibrated – is typically less than 1 pp (Milkman et al., 2021; Milkman et al., 2022). Partly because of the nature of the benefit, these light-touch approaches were remarkably cost effective and had a large and tangible real-world impact.

Under these conditions, where the benefits are significant and the marginal cost of outreach is low, we find that more precise targeting may not be cost-effective. Although Study 3 documents that the effect of outreach is directionally larger among previous non-filers than previous filers, this difference is neither sizable nor statistically significant. That said, our sample was targeted by design, focusing on households that were participating in means-tested programs and thus known to be low-income. Our findings invite further research on the trade-offs between more precise targeting – for example, using cross-agency data linkages – and less costly mass outreach.

Finally, we find that higher-touch outreach that involved staffing a hotline that could proactively reach out to residents, akin to voter registration or vaccination campaigns, was, at best, slightly more effective at closing take-up gaps than light-touch outreach – and was much more costly. These findings challenge the assumption that more resource-intensive interventions necessarily lead to higher impact. Yet, the vast majority of governments offering navigation assistance are using approaches that resemble our *salient assistance* treatment in Study 1 or our *passive assistance* treatment in Study 4. Our findings demonstrate the need for additional research on how to most effectively deliver higher-touch interventions – in particular, how intensive these approaches need to be to have a meaningful impact – as well as whether, for whom, and in what contexts they are cost-effective strategies for closing take-up gaps at scale.

Our studies also have some notable limitations. First, the context of this research – which took place against a backdrop of unprecedented efforts to expand and increase take-up of the CTC – may limit the generalizability of our findings to other programs and populations. We also cannot rule out that other high-touch interventions would have been more effective, or that something specific about our sample makes this population more or less movable than other vulnerable populations. Relatedly, our study designs limit the extent to which we can directly compare effectiveness across samples and modalities. Future research should replicate these interventions in the context of different programs and under conditions that will allow for direct comparison across approaches.

Second, overall utilization of GetCTC.org and GetYourRefund.org were very low. This suggests that at least some percentage of the population used other channels to claim the expanded CTC and EIPs. Since we are not able to measure claims filed through other channels, our findings may be over- or under-estimates of the true effect of outreach depending on whether outreach led people to substitute across tools or motivated people to file through any channel, rather than just the one recommended. Additionally, our focus on short-term behavioral outcomes – tax filing – means we may have not fully captured the potential downstream benefits of these interventions. Proactive government outreach, for instance, may build resident trust over time, thereby proving more effective over multiple interactions or programs, which warrants further exploration.

Taken together, these findings offer both promising and realistic insights that have immediate implications for scholars and policymakers. Nationally, millions of dollars are being invested in interventions like those tested in our field experiments. The strategies tested in our experiments form the core of related playbooks released by civic tech organizations or government digital service and customer experience teams (CfA, 2022; Miller et al., 2023; The White House, 2023b). While our studies reinforce the promise of light-touch outreach as one potential lever through which policymakers can start to close take-up gaps, they also underscore the need for further research to identify more robust approaches, perhaps through cross-enrollment mechanisms or by simplifying the claiming process even more than was the case in our context, to fully support those at risk of missing out on critical benefits.

Methods

Preregistration

All four experiments were pre-registered on OSF. The pre-registered analysis plans (PAPs) for Study 3 and Study 4 (Study 3: https://osf.io/r9vct; Study 4: https://osf.io/axyqf) were updated after randomization, but prior to conducting data analysis, to reflect a change in analytic methods due to unanticipated challenges accessing individual-level outcome data. Our original PAPs for Study 3 and Study 4 contemplated analyzing individual-level data from GetYour Refund.org and GetCTC.org, respectively. After randomization, we were unable to access these data for either study. We developed and adopted an innovative alternative strategy for analysis that permitted valid (though slightly less precise) estimates without linking the individual-level data, described in detail in the Technical Appendix. All estimates presented in this paper for studies 3 and 4 use this method, and standard errors and *p*-values reflect the precision reduction.

Ethics Approval

All studies reported in this manuscript were approved by the California Health and Human Services Agency Committee for the Protection of Human Subjects (FWA# 00000681; IRB# 00000552); IRB project number: 2019-002.

Study Design

Study 1 - Testing the impact of light-touch outreach and message variation via recorded voice messages

Study 1 was conducted from September to October of 2021 and tested outreach aimed at encouraging likely non-filers to claim the 2020 CTC or other EIPs that they had missed out on by November 15, which was when the non-filer tool closed that year (IRS, 2021). In a stratified randomization, non-filer households[1] that only had a phone number on file with CDSS (N = 205,217) were randomly assigned with equal probability to receive one of two recorded voice messages ("robocalls"): (1) a *standard* message that provided basic information about the Child Tax Credit or EIPs and a link to GetCTC.org; or (2) a *salient assistance* message that provided the same information as the *standard* message, as well as the phone number for a CfA-run national hotline that individuals could use to receive filing assistance from a live person.[2] Individuals assigned to the *salient assistance* message group also received a link to GetCTC.org, but their landing page was personalized to include a banner highlighting the phone number for

[1] Households with at least one previous non-filer, based on 2018 and 2019 FTB state tax return filing data (for Studies 1, 2, 3) or 2021 FTB state tax return filing data (for Study 4). See Supplement for details.

[2] Other households were assigned to one of two additional conditions which are excluded from this paper and analysis. See Supplement for details.

the assistance hotline. Households were cross-randomized to one of four time cohorts, with messages sent about one week apart in late September and early October 2021.

Our primary outcomes are (a) initiation and (b) submission of returns via GetCTC.org. Data come from Code for America, which manages the GetCTC.org platform. First, we use the randomized variation in timing of messages to examine the effect of outreach in a one-week period, relative to none. Specifically, we compare return initiations and submissions between cohort 1 and cohorts 2, 3, and 4 in the one week after cohort 1 received outreach, but before cohort 2 received outreach. We also compare the effect of the *standard message* versus the *salient assistance* message on return initiations and submissions over a rolling four-week period, calculated as the 30 days following each round of outreach.

Study 2 - Testing the impact of light-touch outreach and message variation via email

Study 2 was conducted in November 2021 (just ahead of the November 15 extended filing deadline) and tested the impact of email outreach that encouraged likely non-filers to claim the CTC or EIPs. In a stratified randomization, non-filer households with an email address on file with CDSS (N = 47,983) were randomly assigned with equal probability to receive one of two messages: (1) a *psychological ownership* message, which emphasized that available tax credits "belong" to recipients, or (2) a *simplified process* message, which emphasized that the process of claiming available tax credits had been simplified. Both messages directed recipients to GetCTC.org. Similar to Study 1, households were cross-randomized into two timing cohorts, with emails sent about one week apart.

Our primary outcomes are (a) initiation and (b) submission of returns via GetCTC.org. As in Study 1, we use the randomized variation in timing of messages to examine the effect of email outreach relative to none by comparing return initiations and submissions between cohort 1 and cohort 2 in the one week after cohort 1 received outreach, but before cohort 2 received outreach. We also compare the impact of the *psychological ownership* and *simplified process* emails by comparing return initiations and submissions in the one week following outreach, pooling both cohorts.

Study 3 - Measuring the impact of targeting

Study 3 was conducted in March-April 2022 and tested the impact of text message outreach, variations in language, and targeting. This study contrasts with Studies 1 and 2 in two key ways. First, the sample included both previous non-filers and filers (per 2018 and 2019 state tax return filing data). We estimate both the overall effect of outreach, as well as the differential effect on previous non-filers, with the prediction that they were likely to be current non-filers as well – meaning they were more likely to need to take action to avoid missing out on the CTC and EIPs. Second, this study took place during the regular tax filing season, which meant that individuals needed to file a traditional tax return to claim their credits at this time. Thus, all messages in this study directed recipients to GetYourRefund.org through which individuals could check whether they were eligible to submit a simplified tax return.

In a stratified randomization, 20% of households were assigned to a no-communication control group (N = 58,596), while the remainder were assigned with equal probability to receive one of two text messages: (a) a *psychological ownership* message (N = 117,195); or (b) a *simplified process* message (N = 117,193). Message content mirrored that of the emails used in Study 2 and all households received two messages: an initial message and a reminder message, sent approximately two weeks later. Both messages had similar content and were specific to the assigned condition.

We examine the impact of outreach on (a) initiations and (b) submissions of returns through GetYourRefund.org in the four weeks following initial outreach. In addition, we also use individual-level administrative data from the FTB to examine the impact of outreach on state tax return filing (via any method) for 2021.

Study 4 - Testing higher-touch outreach

Study 4 was conducted in November 2022 just ahead of a November 15 deadline for claiming available credits that year. As part of this study, CDSS set up a hotline that was staffed by approximately 35 workers who were trained to answer questions about the tax filing process and the simplified filing tool, and could connect callers to other resources such as VITA sites.

In a stratified and clustered randomization, non-filer households were randomly assigned to one of three conditions with equal probability: (1) a *passive assistance* group, which was sent a text message with the phone number for the tax filing assistance hotline; (2) an *opt-in assistance* group, which was sent a text message that asked recipients to reply "yes" if they wanted to receive a call from the assistance hotline; and (3) an *opt-out assistance* group, which was sent a text message that informed recipients that they would also receive a call from the assistance hotline, but could opt-out via text. Though some studies have tested the impact of combining proactive calls with more traditional light-touch outreach (e.g., OES, 2024a), our *passive assistance* treatment reflects the more typical approach to offering navigation assistance (e.g., Finkelstein & Notowidigdo, 2019; Linos et al., 2022). All messages directed recipients to GetCTC.org and we examine the impact of outreach on (a) initiations and (b) submissions of returns in the two weeks following outreach.

Analyses

First, we examine the effect of light-touch outreach – recorded voice messages, emails, and text messages – on filing behavior in Studies 1-3, relative to a comparison condition of no outreach. All outcomes are measured at the individual-level for the primary contact (the recipient of the outreach message).[3]

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[3] Our pre-registered outcomes for Study 1 and Study 2 were household-level measures of filing behavior. However, the alternative analytical approach utilized in Studies 3 and 4 does not allow us to construct comparable household-level outcomes. As such, for ease of interpretability across experiments, all results reported here focus on the individual contacted through the study (the "primary contact"). Results from all pre-registered models are included in the Supplement and do not differ meaningfully.

In Studies 1 and 2, we estimate the average effect of light-touch outreach using the following regression specification:

$$(1)\ Y_{ig} = \beta_1 cohort1_{ig} + \delta + \varepsilon_{ig}$$

where Y_{ig} reflects the outcome of interest for individual *i* in stratum g, $cohort1_{ig}$ is an indicator for individual i's assignment to the first timing cohort, and δ is a vector of controls including randomization strata and county of residence. Outcomes are return initiation and submission via GetCTC.org during the one-week period between the first cohort of outreach and later cohorts, so the latter are effectively not treated.

In Study 3, we estimate the average effect of text message outreach on primary contact return initiations and submissions using the following regression specification:

$$(2)\ Y_{ig} = \beta_1 treat\_pooled_{ig} + \delta + \varepsilon_{ig}$$

where Y_{ig} reflects the outcome of interest for individual *i* in stratum g, $treat\_pooled_{ig}$ is an indicator for individual *i*'s assignment to receive either outreach message (relative to the no communication control), and δ is randomization stratum fixed effects.

Next, in Study 4, we estimate the effect of higher-touch outreach – specifically, offering *opt-in assistance* and *opt-out assistance* – on return initiations and submissions among household primary contacts using the following regression specification:

$$(3)\ Y_{ig} = \beta_1 optin_{ig} + \beta_2 optout_{ig} + \delta + \varepsilon_{ig}$$

where Y_{ig} reflects the outcome of interest for individual *i* in stratum g, $optin_{ig}$ is an indicator for individual *i*'s assignment to the *opt-in assistance* group, $optout_{ig}$ is an indicator for individual *i*'s assignment to the *optout assistance* group, and δ is randomization stratum fixed effects. The omitted treatment condition is a *passive assistance* text message.

To evaluate the effect of varying message framing on filing behavior in Studies 1-3, we estimate the following regression specification among primary contacts:

$$(4)\ Y_{ig} = \beta_1 treat_{ig} + \delta + \varepsilon_{ig}$$

where Y_{ig} reflects the outcome of interest for individual *i* in stratum g, $treat_{ig}$ is an indicator for individual *i*'s assignment to receive the *salient assistance* message (for Study 1) or the *psychological ownership* message (for Study 2 and Study 3), and δ is a vector of controls. For Studies 1 and 2, controls include time cohort, randomization strata, and county of residence. For Study 3, controls include randomization strata when the outcome is GetYourRefund.org return initiations and submissions, or randomization strata and county of residence when the outcome is 2021 state tax return filing. The comparison conditions are the *standard* message in Study 1 and the *simplified process* message in Study 2 and Study 3. Again, all outcomes reported here are measured at the individual-level among primary contacts.

Finally, in Study 3, we examine whether targeting previous non-filers (i.e., individuals who had not filed or been claimed on a filed 2018 and 2019 state tax return) increases effect sizes using the following regression specification:

$$(5)\ Y_{ig} = \beta_1 treatXnonfiler_{ig} + \beta_2 treat_{ig} + \beta_3 nonfiler_{ig} + \delta + \varepsilon_{ig}$$

where Y_{ig} reflects the outcome of interest – 2021 state tax return filing – for individual *i* in stratum *g*, $treat_{ig}$ is an indicator for individual i's assignment to receive outreach, $nonfiler_{ig}$ is an indicator for whether individual *i* was a previous non-filer, and δ is a vector of controls including randomization strata and county. Outcomes reported here are measured among primary contacts only.

Tables

Table 1. Study Overview

Study	Sample	Sample size	Experimental design	Outcomes	Outreach modality
Study 1 (Sept-Oct 2021)	Previous non-filers only	144,125 households	(1) Standard message (2) Salient assistance message	(1) Return initiation via GetCTC.org (2) Return submission via GetCTC.org	Recorded voice messages
Study 2 (Nov 2021)	Previous non-filers only	47,693 households	(1) Psychological ownership message (2) Simplified process message	(1) Return initiation via GetCTC.org (2) Return submission via GetCTC.org	Emails
Study 3 (March-April 2022)	Includes previous filers	292,984 households	(1) No-communication control (2) Psychological ownership message (3) Simplified process message	(1) Return initiation via GetYourRefund.org (2) Return submission via GetYourRefund.org (3) State tax return filing from the Franchise Tax Board	Text messages
Study 4 (Nov 2022)	Previous non-filers only	58,002 households	(1) Passive assistance (2) Opt-in assistance (3) Opt-out assistance	(1) Return initiation via GetCTC.org (2) Return submission via GetCTC.org	(1) Text messages (2) Proactive outbound phone calls

Notes: Sample sizes reported are for the analytic samples for each study. See Supplement for details on post-randomization exclusion criteria.

Table 2. Effect of light- and high-touch outreach on filing outcomes

	Initiations	Submissions	State tax returns (study 3 only)
	(1)	(2)	(3)
Panel A. Study 1: Recorded voice messages			
Week 1 cohort	0.0025***	0.0014***	
	(0.0003)	(0.0003)	
Observations	144,139	144,139	
Mean for cohorts 2-4	0.0012	0.0009	
Panel B. Study 2: Emails			
Week 1 cohort	0.0307***	0.0194***	
	(0.0013)	(0.0011)	
Observations	47,697	47,697	
Mean for week 2 cohort	0.0055	0.0044	
Panel C. Study 3: Text messages			
Pooled treatment	0.0041***	-0.0001	0.0048*
	(0.0004)	(0.0003)	(0.0021)
Observations	278,241	278,241	292,984
Mean for control	0.0014	0.0004	0.689
Panel D. Study 4: Higher-intensity approaches			
Opt-in assistance	0.0013	0.0007	
	(0.0019)	(0.0015)	
Opt-out assistance	0.0031*	0.0011	
	(0.0016)	(0.0012)	
Opt-out assistance + rec'd call[1]	*0.0048**	*0.0026+*	
	(0.0019)	*(0.0015)*	
Observations[2]	55838	55838	
Mean for passive assistance	0.0122	0.0076	

[1]Average treatment effect estimates among the subsample of households assigned to the *opt-out assistance* group that received a proactive call as intended (see Section III.D). These estimates come from a separate model.
[2]Number of observations for the model examining the subsample of households assigned to the *opt-out assistance* group that received a proactive call is 45,217.
Notes: Average treatment effect estimates from models described in section III.D. Outcomes for Study 1 (Panel A) and Study 2 (Panel B) are indicators for primary contact return initiations (column 1) and submissions (column 2) through GetCTC.org in the one week following the first wave of outreach. Outcomes for Study 3 (Panel C) are indicators for primary contact return initiations (column 1) and submissions (column 2) through GetYourRefund.org in the four weeks following outreach, and 2021 California state tax return filing (column 3). Outcomes for Study 4 (Panel D) are indicators for primary contact return initiations (column 1) and submissions (column 2) through GetCTC.org in the two weeks following outreach. All models include randomization stratum fixed effects. The models shown for Study 1, Study 2, and Study 3, column 3 also control for county of residence. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05, + p<0.10.

Table 3. Effect of message content on filing outcomes

	Initiations	Submissions	State tax returns (study 3 only)
	(1)	(2)	(3)
Panel A. Study 1: Recorded voice messages			
Salient assistance message	-0.0004	0.0000	
	(0.0007)	(0.0006)	
Observations	144,139	144,139	
Standard message mean	0.0184	0.0132	
Panel B. Study 2: Emails			
Psychological ownership message	-0.0027	-0.0015	
	(0.0018)	(0.0015)	
Observations	47,697	47,697	
Simplified process message mean	0.0404	0.0268	
Panel C. Study 3: Text messages			
Psychological ownership message	0.0017***	0.0005*	0.0012
	(0.0004)	(0.0002)	(0.0018)
Observations	278,241	278,241	292,984
Simplified process message mean	0.0046	0.0001	0.693

Notes: Average treatment effect estimates from models described in section III.B. Outcomes for Study 1 (Panel A) are indicators for primary contact return initiations (column 1) and submissions (column 2) through GetCTC.org in the four weeks following outreach. Outcomes for Study 2 (Panel B) are indicators for primary contact return initiations (column 1) and submissions (column 2) through GetCTC.org in the one week following outreach. Outcomes for Study 3 (Panel C) are indicators for primary contact return initiations (column 1) and submissions (column 2) through GetYourRefund.org in the four weeks following outreach, and 2021 California state tax return filing (column 3). Controls described in section III.B. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05, + p<0.10

Table 4. Effect of targeting: Differential effect of outreach on state tax return filing, by likely non-filer status

	Interaction	Previous Non-filers	Previous Filers
	(1)	(2)	(3)
Panel A: Effect of pooled treatment			
Pooled treatment	0.0040+	0.0075+	0.0040+
	(0.0023)	(0.0044)	(0.0023)
Previous non-filer	-0.2061***		
	(0.0044)		
Pooled treatment X previous non-filer	0.0035		
	(0.0050)		
Observations	292,984	77,433	215,551
Mean for control (+ previous filer)	0.744	0.551	0.739
Panel B: Effect of treatment message			
Control	-0.0040	-0.0050	-0.0041
	(0.0025)	(0.0048)	(0.0025)
Psychological ownership	-0.0002	0.0049	-0.0002
	(0.0021)	(0.0039)	(0.0021)
Previous non-filer	-0.2052***		
	(0.0031)		
Control X previous non-filer	-0.0009		
	(0.0054)		
Psychological ownership X previous non-filer	0.0052		
	(0.0044)		
Observations	292,984	77,433	215,551
Mean for control (+ previous filer)	0.748	0.556	0.743

Notes: Estimates from models described in section III.C. Column 1 reports estimates from a model interacting treatment with previous non-filer status; Column 2 reports estimates for the subsample of previous non-filers; and Column 3 reports estimates for the subset of previous filers. The outcome is an indicator for filing a 2021 California state tax return among primary contacts. All models control for randomization strata and county of residence. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05, + p<0.10

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