Non-Monetary Sanctions as Tax Enforcement Tools: Evaluating California's Top 500 Program

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ABSTRACT**:** Many U.S. states and countries around the world use non-monetary sanctions to encourage tax compliance, including public disclosure, license suspension, and withholding of other government-provided benefits or privileges. Little is known about the effectiveness of these programs. Using administrative tax microdata from California's "Top 500" program, we study whether notices warning of the imminent publication of a taxpayer's personal information and potential license suspension affect payment and other compliance outcomes, as well as whether these notices affect subsequent reported earnings. Exploiting variation over time in the cutoff balance for program eligibility we find evidence of strong positive compliance responses to the program, with no evidence of an impact on subsequent reported earnings. We also develop estimates of the deadweight loss caused by publication of non-compliers, and conclude that the program generates positive net social welfare. Together, these results suggest that non-monetary sanctions can be efficient tax enforcement tools, at least among the relatively high-income population we study.

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1. Introduction

Given the limited resources available to tax enforcement authorities around the world, tax agencies are always in search of cost-effective methods for collecting revenue and assuring tax compliance. In addition to traditional financial tools such as liens and levies, many countries and U.S. states use non-monetary tools to encourage increased tax compliance and collection of outstanding tax debt. These tools take various forms, including public disclosure of tax debtors' personal information (sometimes known as "name and shame" lists), driver's or professional license suspension, and restricted passport access. To date, relatively little is known about whether these programs are effective. Only two existing studies have shed light on the effectiveness of name and shame lists (Perez-Truglia and Troiano 2018, Dwenger and Treber 2019), and the evidence on other non-monetary tools is limited as well, though growing (Organ, et al. 2021, Holz, et al. 2020, Kenchington and White 2021). We add further evidence to the existing literature as well as answering new questions by studying California's "Top 500" program, using restricted-access administrative tax data covering five years of twice-yearly list publications.

Non-monetary sanctions are a theoretically useful tool for a tax authority to consider (Blank 2014, Kuchumova 2018, Kuchumova 2021). Typically, monetary penalties dominate alternatives because they result in transfers, rather than deadweight loss (Polinsky and Shavell 2000). Almost by definition, however, persistent tax debtors are relatively insensitive to monetary penalties: if the authority has not been able to initially collect the tax liability, it is unclear why it would be better able to collect any additional penalty. Optimal enforcement theory suggests that multiple enforcement instruments can be desirable when the target population has heterogeneous sensitivity to each instrument (Slemrod and Gillitzer 2013), and this can hold even for transferless instruments in some cases (Galle and Mungan 2021). Imprisonment, although practiced in the United States as a means of securing some public debts, may lose money on net through its high cost and negative impact on earnings. Disclosure and other non-monetary sanctions appear to be a low-cost alternative, if effective. As we discuss later, however, the total measured cost of the program should include not just administrative costs, but also the potentially significant disutility costs incurred by those whose information is published.

To gain traction on some of these questions, we study several components of the California "Top 500" program, a semi-annual internet posting of California's largest tax debtors in which published individuals also face potential suspension of drivers and professional licenses.[1] We observe outstanding balance, payments, and other administrative outcomes for California taxpayers with outstanding tax debt of at least $100,000 (well below the threshold for Top 500 publication, which ranges from about $150,000 to $230,000 during our study period). In addition, we link these data to individual CA tax return information. This linkage allows us to condition responses on observed taxpayer characteristics, and also to measure the extent to which publication and other administrative steps aimed at collection of old tax debts affect reported income in subsequent tax years. Although California posts both individual and corporate debtors, we limit our focus to individuals.

Tax debtors in California receive several notifications before their names can be posted publicly. Taxpayers with unpaid balances above $25,000 are contacted individually by California tax-collection personnel. In addition, the "pre-letter," an initial notification about the existence of the Top 500 list, is sent semi-annually to all $100,000+ debt households who do not fall into a statutory exception, usually arriving shortly after the publication of the most recent list. Several months later, and two months before posting the final list, a second letter (referred to by staff as the "official letter") is sent to the 500 publication-eligible taxpayers with the highest debts outstanding at that time, informing these taxpayers that if they do not take action, their information will be published and they may be subject to license suspension. Finally, there is the published list itself, which provides names, addresses, unpaid balance amount, and professional license information for all taxpayers who received the official letter and did not take sufficient action to avoid publication.

Our main analysis exploits the random variation in Top 500 balance cutoffs to estimate the impact of receiving the official letter. Over the 10 list publication cycles in our study period, the lowest balance we observe receiving an official letter each cycle ranges from roughly $150,000 to $230,000. This lowest balance is determined by the 500th-largest eligible balance, which varies with each cycle. In this specification, identification is based on observing two taxpayers with similar balance, one who is mailed a letter and the other who

[1] The program includes taxpayers owing the top 500 liened delinquencies of personal income tax (PIT) or business entity (BE) tax in excess of $100,000. We study individual taxpayers.

is not. Because the cutoff is stochastic, even taxpayers who are aware of the program cannot perfectly predict whether they will receive a letter, reducing any concern about selection into treatment status.

We take this identification method to a set of compliance outcomes. Most simply, we look at the extent to which treated taxpayers make any payment, or pay more or less than others. We also examine other behavior of interest to tax administrators, namely entering into an Installment Agreement to pay down the total balance over time, or taking steps necessary to qualify for other statuses leading to exemption from publication, such as filing for bankruptcy or documenting other significant financial hardship.

We find that taxpayers are responsive to receiving an official letter, and thus that the threat of publication and license suspension can be useful tools for tax enforcement. Our preferred identification strategy suggests that, over the two-year period following treatment, recipients pay an average of about $7,200 more than non-recipients, yielding about $2.8 million in added revenues for each year we observed the program in operation. Because this specification is restricted to observations with balances in the range of cutoff values (roughly $150,000 to $230,000), this number omits the highest-balance households. Estimates using our full sample suggest total added revenues of $7.2 million annually. In the Appendices, we draw on inferences from the behavior of marginal avoiders to put bounds on the private costs of the program, including the welfare costs of being subject to publication. Combining these with our revenue estimates allows us to further estimate that this revenue results in net social welfare gains of at least $1 million per year.

The official letter also encourages other compliance behavior. A large fraction of treated households take steps to make themselves ineligible for publication, with an increase in ineligibility of eight percentage points relative to the control households in the three months after treatment. Over the two-year window, the increase rises to twenty percentage points. This is on a baseline that by construction has zero ineligible households. Of these, two percentage points enter into Installment Agreements in the three months after treatment, and over two years there is a 12 percentage point rise in such agreements.

Our paper builds primarily on the two existing studies about delinquent taxpayer disclosure. Perez-Truglia and Troiano 2018 (henceforth "PTT") used randomized letters sent by the researchers to highlight the salience of published tax delinquents' information being

public. Letters increased the probability that low-balance tax debtors (those with debts below $2,500) would leave the list but had no effect on high-balance tax debtors.[2] Dwenger and Treber 2019 (henceforth "DT") study the first year of a delinquent taxpayer disclosure program for corporations and self-employed individuals in Slovenia. Studying responses after the program was announced but before it was implemented, DT find that the threat of publication leads both corporations and the self-employed to reduce tax debt. Publication itself led to further reductions in tax debt, though of a smaller size.

We make several new contributions. First, we offer the best evidence to date that disclosure programs sustainably increase taxpayer remittances. Because PTT only observe publication, and not payments directly, they cannot establish that threats of publication induce payment, nor if so how much. We find that many taxpayers are able to escape publication through avenues other than immediate full payment, so that PTT's evidence does not clearly establish that any taxpayer they treated actually paid.[3] DT find evidence of payment, but because their study covers only the initial rollout of a program, when public attention may be unusually high, the external validity of their results for ongoing programs is unclear.

Next, by combining payments data with individual income tax returns, we observe whether the treatment has any effects on (reported) subsequent income, as well as conditioning other responses on reported income, self-employment, and tax filing status. This allows us to begin to untangle *why* the studied households fail to make timely payments. The self-employed are much more responsive to treatment, suggesting that at least for these individuals, failure to pay assessed taxes is tax avoidance, rather than the result of budget pressure. Ability to pay, measured by reported income, plays a role, too. We find that the effect of the official letter on most of the compliance actions we study, including the total payment triggered by treatment, is considerably higher among households reporting the highest adjusted gross income. Because we argue that non-monetary sanctions are generally

[2] Other field studies find that mailings can increase compliance by raising the salience of detection or sanction (Bott, et al. 2019, Cranor, Goldin and Homonoff 2020, Dorrenberg and Schmitz 2017, Gemmell and Ratto 2018, Holz, et al. 2020, Iyer, Reckers and Sanders 2010).

[3] PTT do note that tax officials in states they contacted indicated that the officials believed the share of individuals who escape publication other than through payment was small in those states (Perez-Truglia & Troiano 2018:127). Our evidence raises questions about whether that holds true generally.

welfare-increasing for intentional tax avoiders, while they may be welfare-decreasing for those who lack ability to pay, this finding has important consequences for assessing the net social welfare of disclosure and license suspension.

We also find a significant and economically substantial positive correlation between failure to file any return and non-compliance. This could reflect a high subjective cost of compliance, perhaps due to filing complexity, or that some households may simply have a relatively strong preference for avoiding contact with California tax authorities. We thus help to confirm prior work finding that complexity is an important contributor to non-filing behavior (Bhargava and Manoli 2015).

California's relatively high-balance debtors allow us to provide new findings on the efficacy of disclosure among populations not previously subject to study. We offer evidence on a program targeting only high-balance individual tax debtors, in contrast to PTT's study of three states using relatively low balance thresholds for publication (publishing all tax debtors with debts above $250, $2,500, or $5,000, depending on the state) or DT's study of only self-employed individuals.

Lastly, we shed some new light on the effectiveness of license suspensions as a tax enforcement tool, another area in which the existing literature is limited. One study of tax debt-related professional license suspensions in Missouri found that suspensions were concentrated among lower-income professions (Kenchington and White 2021). Another study, of the IRS' program of restricting passport access to tax debtors, found that the program induced taxpayers to take positive tax compliance actions (Organ, et al. 2021). We add to this literature by demonstrating that license suspensions can be useful even when targeted only at the highest-balance taxpayers, as we find that license holders are more likely to enter installment agreements.

The rest of the paper proceeds as follows. Section 2 describes California's Top 500 program in further detail, and Section 3 describes the data available for this study. Section 4 is the main section of the paper, describing our analysis of the effects of the official letter notifying taxpayers of their imminent publication. Section 5 then describes a brief analysis of the effects of publication and license suspension, and offers some preliminary evidence on the relative contributions of publication and license suspension to compliance outcomes. Section 6 concludes.

2. Overview of the Top 500 program

California imposes a progressive income tax with a top rate of 13.3%. The state generally follows federal rules for defining the tax base, with certain exceptions. The state's income tax is administered by the California Franchise Tax Board ("FTB"). One tool FTB uses for collecting unpaid tax liabilities is its "Top 500" program. Legislation enacted in 2006 (AB 1418) mandated that FTB annually make public a list of its 250 largest debtors. AB 1424, enacted in 2011, expanded the list to the top 500 debtors, increased the frequency to twice a year, and added additional sanctions for listed debtors, including provisions for the suspension of professional licenses and a prohibition on being awarded state contracts.[4]

California begins assembly of its Top 500 list with a preliminary list of all taxpayers with current unpaid balances of more than $100,000, a group that typically numbers about 6,000 households. FTB staff then scrutinize this preliminary list in an effort to identify taxpayers who are statutorily exempt from being included in the Top 500. The most common exempt categories are for taxpayers who have entered into a payment agreement with FTB or been found to suffer from financial hardship.[5] Others include deceased individuals, "innocent spouses" not responsible for the household's debts, and taxpayers who have commenced federal bankruptcy proceedings.

The winnowing process typically leaves approximately 3,000 eligible individual taxpayers. We call this group the "pre-letter list." At this point FTB prepares a mailing list. Taxpayers who remain on the pre-letter list and have never previously been included in the Top 500 receive a letter (Appendix, Figure 13) informing them of the existence of the Top 500 program and the potential to be included in the Top 500. The letter also states that if a taxpayer is found to be in the Top 500, they will be notified at that time.

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[4] The two pieces of legislation created parallel, but separate, programs for California's top income tax debtors and its top sales tax debtors. California's Sales and Use Tax is administered by a different agency; we do not have access to their data. The change from publishing the top 250 to the top 500 is also of research interest, but because our data begin in 2013 we are unable to study that in this paper.

[5] FTB defines financial hardship as net assets that are less than necessary to "provide for the [taxpayer's] health and welfare" or the "reasonable costs … of the [taxpayer's] trade or business." California Code of Regulations § 19195-2. In addition, the relevant statute requires FTB to remove a taxpayer from the list promptly when it determines that the delinquency is "uncollectible." CA Rev. & Tax Code § 7063(f)(4). FTB cannot collect debts more than 20 years old, with certain exceptions. CA Rev. & Tax Code § 19255.

In general, taxpayers with unpaid balances of this magnitude have already been the subject of extensive collections efforts. Debts are only counted towards the delinquent total if they have been delinquent more than 90 days, and the state has filed a notice of tax lien. All taxpayers with balances above $25,000 and who have failed to reach compliance voluntarily are assigned to an individual agent at FTB, who attempts to contact the taxpayer and work with them personally to collect the outstanding obligation. Taxpayers who do not comply at this point are also subject to wage garnishment. This accounts for the relatively large share of accounts with high balances being excluded from the pre-letter list due to payment agreements, hardship findings, or other ineligibility.

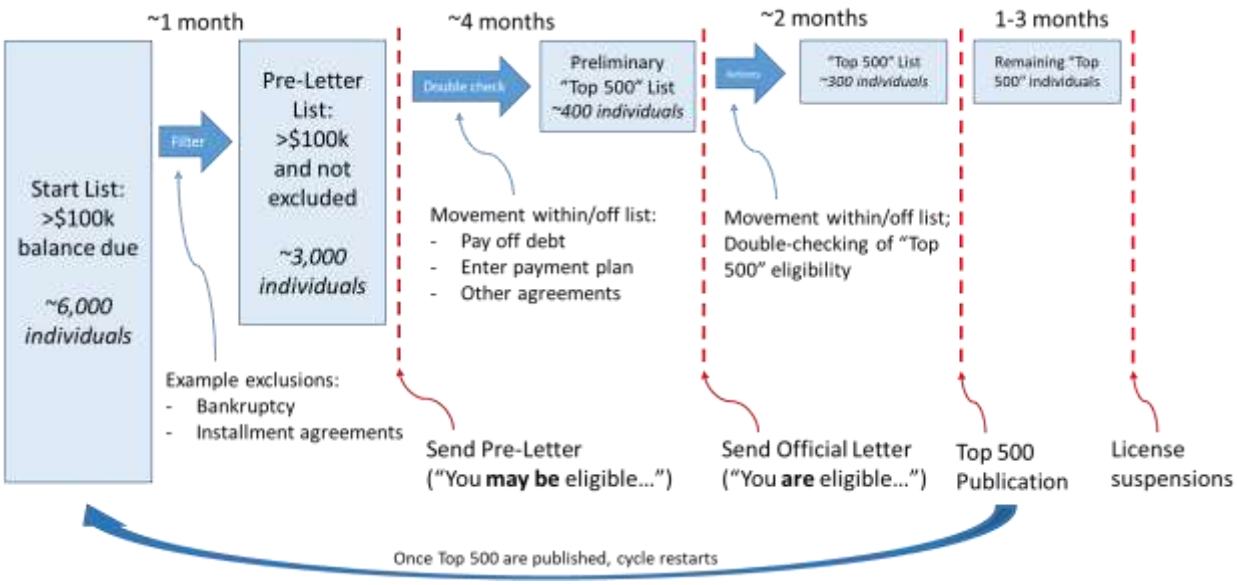
After sending the pre-letter, FTB staff then begin a more thorough review of the potential set of taxpayers who will become the Top 500. As more information is gathered about taxpayers, some of those who received pre-letters are subsequently deemed ineligible for publication. After receiving a pre-letter, taxpayers also may take actions which lead to ineligibility for publication, including entering into payment agreements. Following this review, taxpayers who are still eligible for publication are ranked from highest to lowest balance, and a second mailing list is prepared.

The 500 taxpayers with highest outstanding balances are then sent a letter (the "official letter") informing them that if full payment or other resolution is not made in the next 60 days, they will be included in the Top 500 list posted online, and face potential license suspension. Because the Top 500 is based on all balances due (among both individuals and businesses) the number of individuals sent this letter is less than 500; typically, about 400 individuals receive the official letter. The letter provides taxpayers with information about how to contact FTB to resolve their tax debt, and FTB's web site provides a link taxpayers can click to initiate contact.

Sixty days following the official letter, after a final check to confirm all individuals remaining are eligible for publication and have not taken actions that would lead to ineligibility, FTB posts the remaining taxpayers' information online. FTB does not replace taxpayers who become ineligible between the time of the letter and the list, so that taxpayers that pay their balances or enter into payment agreements during this time reduce the size of the list. In practice, most posted lists include about 300 individuals, providing some initial evidence that the official letter is fairly effective.

This process is carried out twice a year. Figure 1 summarizes the Top 500 timeline. Immediately after the Top 500 List posts, the process repeats with another gathering of taxpayers who then have $100,000 or more in debt.

Figure 1: Top 500 timeline



Notes: This figure describes the timeline of a typical Top 500 publication cycle, from start to finish. The Top 500 list is published twice per year, in April and October.

Appearing on the list triggers additional penalties on top of public disclosure of the taxpayer's name and debt. In most cases listing triggers suspension of professional, occupational, and even driver's licenses. Most professional associations and licensing agencies cooperate with the FTB to suspend licenses, with one notable exception. A license to practice law is not automatically suspended, but the State Bar of California may recommend suspension at its discretion (CA Business & Professions Code § 494.5). In practice, the State Bar does not suspend licenses to practice law for nonpayment of taxes. State agencies cannot enter into contracts with taxpayers who appear on the list.

There is an additional process that licensing entities must follow before suspending a license. The licensee must receive a separate notice of license suspension within thirty days of appearing on the list. The licensee can obtain a temporary license (if in the application period) for ninety days, but at between ninety and one hundred twenty days after mailing of the suspension notice, the license is suspended. For individuals who are published on the Top 500 list, we observe whether a licensing entity has notified FTB that a licensed individual is

on a list, but we do not have additional information on whether the licensing entity complies with the additional notice procedures.

3. Data

Our data comprise a merged set of payment, balance due, and other individual-level tax information for every California taxpayer who has incurred a balance due of at least $100,000. We observe each time a taxpayer appears on FTB's initial list, receives a pre-letter, receives an official letter, or makes the Top 500 list. We also observe all payments made, as well as status and activity code data that allow us to observe other outcomes of interest, such as entering into an installment agreement.

We can also match these payment and status records with tax filing information for each taxpayer who ever appeared on the initial FTB lists (i.e., those who at one point had a delinquency of at least $100,000). This provides us with selected fields on the taxpayer's California (not federal) individual income tax returns, stretching from 2009 to 2019; however, about forty percent of the household-years we observe in the payments data lack a timely return for the two years before observation.[6]

Summary statistics for the individuals receiving the official letter, across all 10 cycles we study, are shown in Table 1.[7] The mean balance among letter recipients is about $859,000, while the median is lower at about $324,000. When restricting to first-time letter recipients only, we see slightly lower balances, with a mean of $606,000 and median of $300,000. Slightly more than half of letter recipients have filed returns for the years just prior to their letter. Among those filers, there is a wide range of income. About half of those with filings report some business income. Nearly all of the letter recipients are California residents.

[6] A taxpayer who fails to file any return may still show a balance owed to California as a result of information return reporting from third parties. Non-filers are likely over-represented among the group of major tax delinquents because they fail to report any potential offsetting deductions, inflating their tax due.

[7] To maintain anonymity with respect to data that are not disclosable under the Top 500 statute, we report descriptive and other statistics in bins large enough to prevent individual identification.

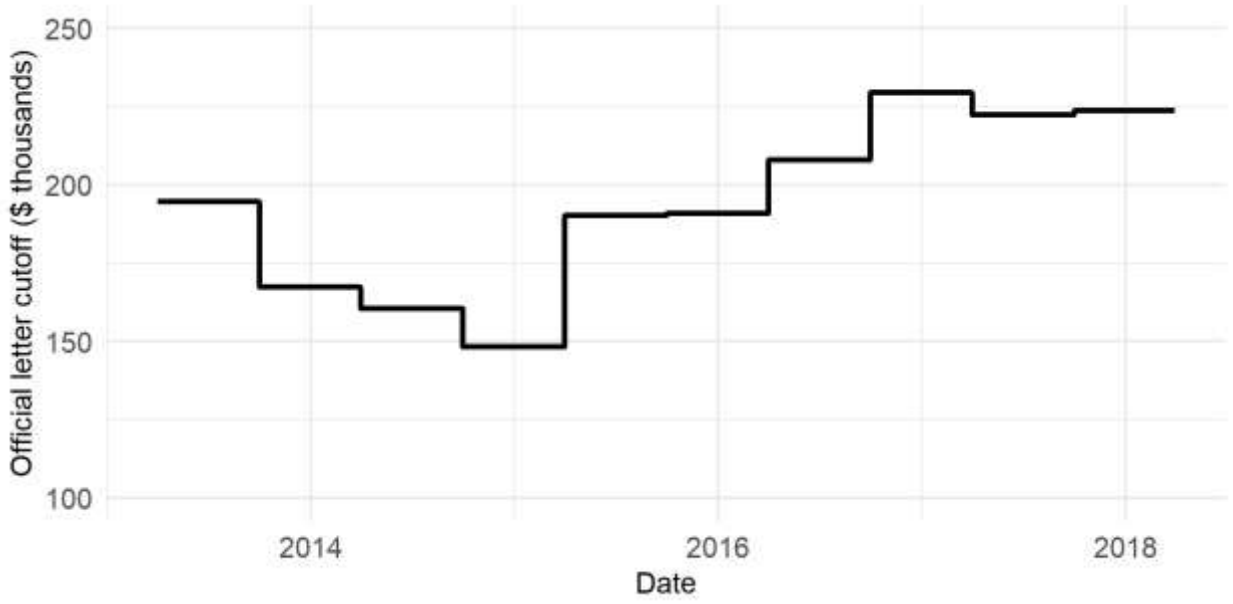
Table 1: Summary statistics for official letter recipients

	Mean	Std. Dev.	P5	P25	Median	P75	P95
Panel A: Among all official letter recipients							
Balance due as of official letter ($ thousands)	859	7,069	181	245	324	539	1,796
Filed on-time return for two-years prior tax year (1/0)	0.57	0.50	0.00	0.00	1.00	1.00	1.00
Filed return for prior tax year (1/0)	0.55	0.50	0.00	0.00	1.00	1.00	1.00
Among those with filed returns for prior tax year:							
AGI ($ thousands)	-250	3,913	-1,100	2	40	152	884
Wages ($ thousands)	71	699	0	0	0	36	213
Has business income (1/0)	0.49	0.50	0.00	0.00	0.00	1.00	1.00
CA resident (1/0)	0.96	0.19	1.00	1.00	1.00	1.00	1.00
Panel B: Among first-time official letter recipients							
Balance due as of official letter ($ thousands)	606	2,012	172	229	300	493	1,512
Filed on-time return for two-years prior tax year (1/0)	0.56	0.50	0.00	0.00	1.00	1.00	1.00
Filed return for prior tax year (1/0)	0.53	0.50	0.00	0.00	1.00	1.00	1.00
Among those with filed returns for prior tax year:							
AGI ($ thousands)	-111	3,509	-825	2	41	165	983
Wages ($ thousands)	98	943	0	0	0	43	272
Has business income (1/0)	0.49	0.50	0.00	0.00	0.00	1.00	1.00
CA resident (1/0)	0.97	0.17	1.00	1.00	1.00	1.00	1.00

Notes: This table presents summary statistics for official letter recipients, across all 10 cycles included in our study.

For each publication cycle, we observe the lowest balance among the official letter recipients for the cycle, and we call this the "cutoff" value for that cycle. Cutoffs for the ten cycles we observe range between $150,000 and $230,000, as illustrated in Figure 2.

Figure 2: Variation in official letter cutoffs over time



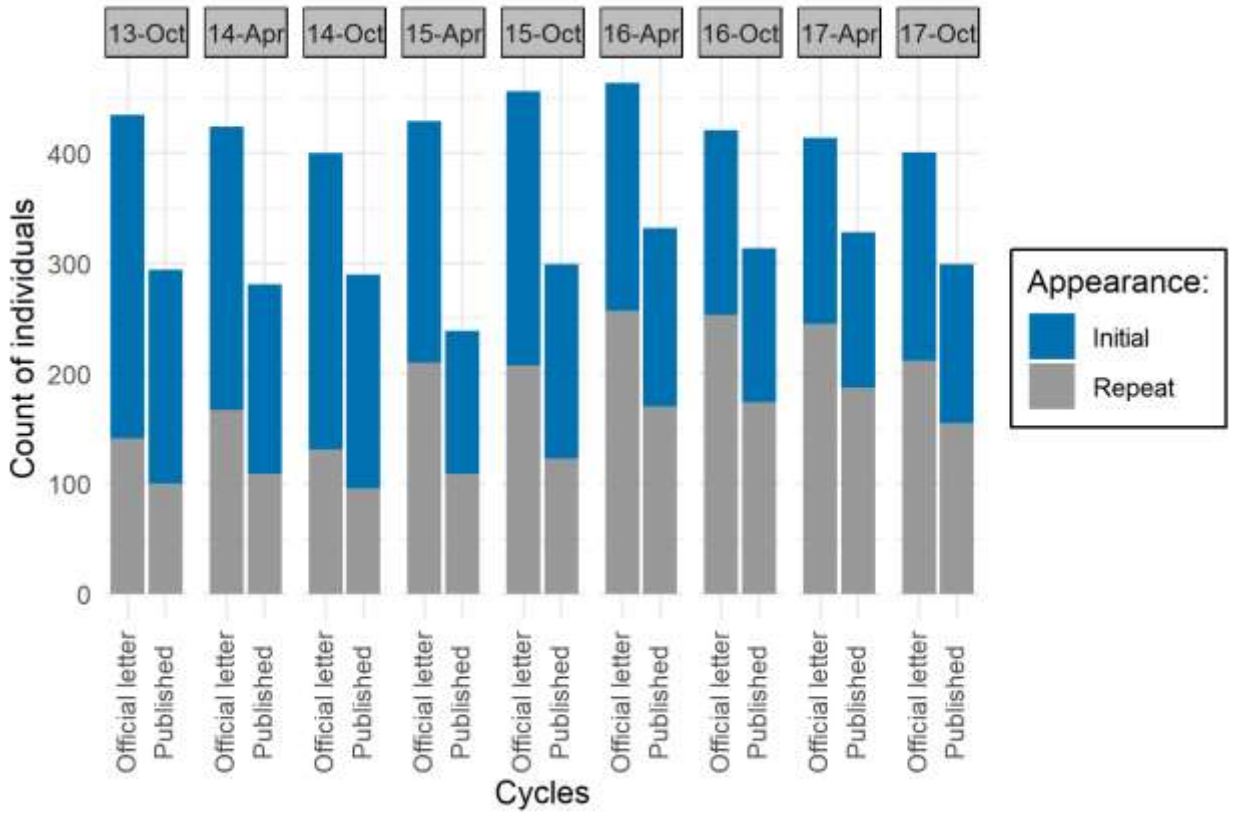
Notes: This figure plots the lowest observed balance among official letter recipients each cycle. Because the official letter is sent to the limited number of taxpayers likely to be in the Top 500 balances, and the set of tax debtors changes over time, the cutoff for official letter receipt also changes over time, as shown here.

In our reported results, we screen out households we can identify as statutorily ineligible for publication. By definition, our "treated" households have been found eligible. To help ensure comparability between treated and control households, we attempt to similarly limit the control population to those who would be eligible if their balance were high enough. Our data do not directly report eligibility. We do, however, observe the "status codes" that FTB uses to determine eligibility, such as whether a taxpayer has entered into an installment agreement, a bankruptcy, or has established that they are an innocent spouse. We thus omit households with one or more of these observed status codes prior to treatment. After screening, we still observe some taxpayers with balances above the cutoff who do not receive letters, suggesting that our screen does not map perfectly onto ineligibility.

A subset of taxpayers remain on the Top 500 list persistently. On average, a little under half of those receiving the official letter received one for a prior cycle, and again slightly less than half of those ultimately published each cycle were published on a prior cycle as well. This pattern can be seen below in Figure 3. Households already published in a prior cycle may be especially unlikely to respond to treatment, as by definition they have already failed

to do so once before. Including them in our control (treatment) population might therefore bias our measured results upwards (downwards). Unless otherwise noted, our reported results thus omit households published in a prior cycle.

Figure 3: Official letter and publication counts, initial vs. repeat appearance



Notes: This figure plots the count of individuals receiving the official letter, and the number ultimately published, each cycle from Oct. 2013 to Oct. 2017. Individuals are counted separately by whether or not it is their first time receiving an official letter or getting published.

4. Effects of the official letter

The central treatment we study in this paper is the official letter, by which taxpayers are notified that they are slated to have their information published on the Top 500 list and may be subject to license suspension. This notice is sent four months after the pre-letter mailing, and two months before list publication, and is sent only to the 500 taxpayers with the highest balances among those eligible to appear on the list. The official letter is a credible and time-sensitive notification, and so we expect it to have the largest impact. Because our data include

codes for a bad mailing address, and we omit these observations, we can verify that we are measuring true treatment effects and not simply intent to treat.[8]

We note that the sample available for analysis of the official letter treatment has already been selected on their being non-responsive to earlier collections efforts; by definition, these are taxpayers who have failed to respond to a series of prior notifications, including a notice of tax lien. In addition, all of these households have received a "pre-letter" notifying them that they may qualify for the Top 500, giving those that are most responsive an opportunity to take action prior to the official letter. Our results in this section can thus be understood as a lower bound on the effect of disclosure on populations that have not been as thoroughly pre-selected.

As we describe in more detail in Appendix E, while we face some data issues in studying the pre-letter population, our best available evidence is that the impact of the pre-letter on individual households is very small. We expect that the pre-letter does not provide new information to most taxpayers. For one, it simply restates the statutory criteria, which are public information, together with the taxpayer's individual balance, which the taxpayer knows from prior contact with FTB staff. It expressly declares that taxpayers will get another warning before they can be published, undermining any sense of urgency to respond. Further, FTB staff are assigned to work individually with taxpayers with balances in excess of $25,000, and collectors are likely to inform tax debtors of the existence of the Top 500 list as part of their collection efforts well before the delivery of the pre-letter.

Consistent with this expectation, our estimates can rule out individual pre-letter impacts of any economically meaningful size, so that any selection effects are minor. With 95% confidence, we can rule out increases of 1% or more in a household's likelihood of entering a

[8] Typically, fewer than 10% of the individuals on the "start list" of individuals considered for the pre-letter have such bad address codes. This represents a much lower share than in other reported tax authority mailing interventions (e.g., Goldin, Homonoff, et al. 2021), consistent with our treated population already having been the subject of close human interaction. Another concern with letter studies is that some taxpayers may fail to open or read the letter (e.g., Perez-Truglia and Cruces 2017, Bottan and Perez-Truglia 2020, Nathan, Perez-Truglia and Zentner 2021). To the extent this is true, our estimates reflect a lower bound on the treatment effect. Our setting is somewhat distinctive from pure mailing interventions, however, in that letter recipients who fail to read the letter are treated later through a non-mail treatment, namely publication. As described below, we find little incremental impact of publication, suggesting that most of those who are susceptible to treatment are reading the letter.

payment agreement or other ineligibility status. We estimate a mean increased monthly payment of about $179, with a 95% confidence interval ranging from just above $0 to $357. Thus, the pre-letter on average results in households paying no more than about 1% of their $100,000+ balance in the three months between pre-letter and official letter. In the aggregate, though, these results suggest the pre-letter brings in meaningful revenues, with our point estimate implying a total of about $3.2 million in additional payments annually across the 6,000 or so pre-letter recipients.

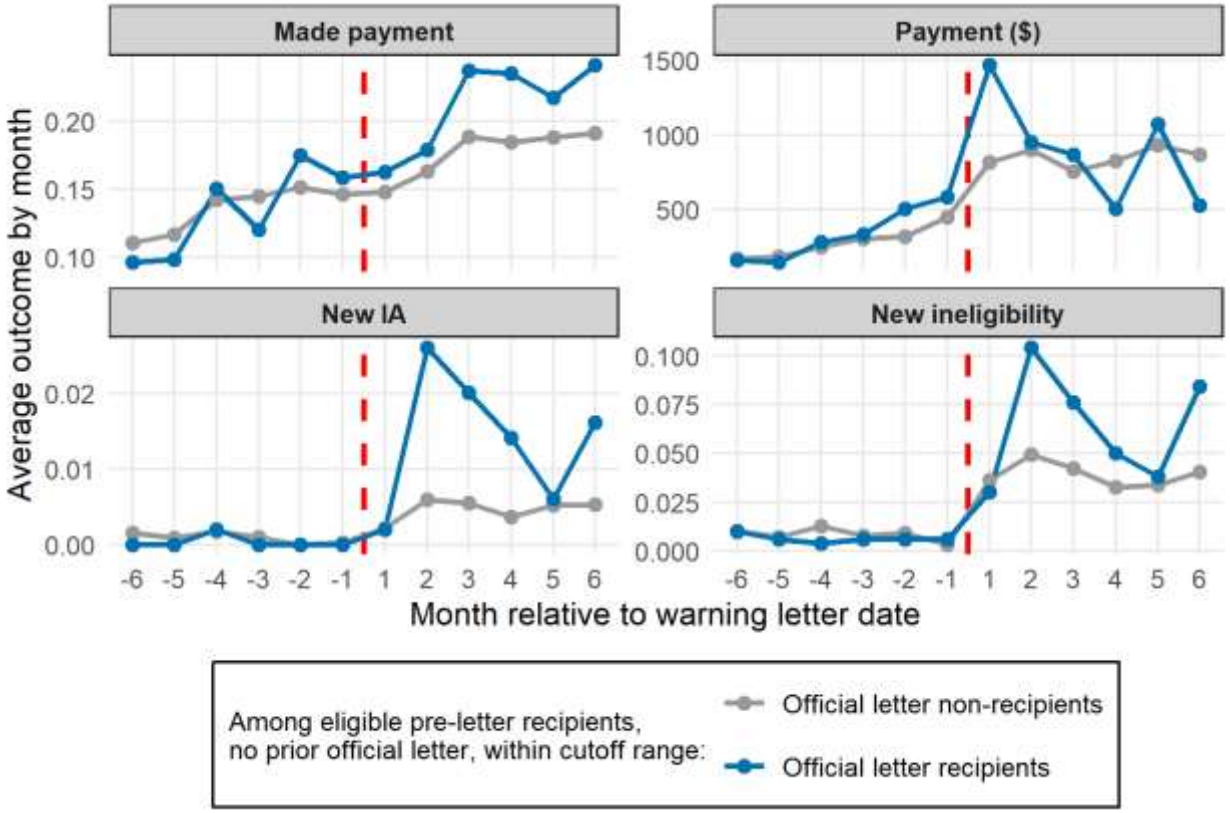
4.1. Graphical analysis

To evaluate the effects of the official letter, we start with a simple graphical analysis showing behavior before and after the official letter dates, for letter recipients and non-recipients. In this section we focus our attention on the taxpayers who are eligible for publication on the Top 500 list and who have not yet received an official letter (that is, for taxpayers who remain on the list for multiple cycles, only their first observation is included).[9] This allows us to present a visual test of the effect on a taxpayer of encountering, for the first time, the letter and its notification that the taxpayer will be published if action is not taken quickly.

Figure 4 below compares official letter recipients and non-recipients on four behaviors: making a payment, average payment amount, starting an Installment Agreement, and entering into a status that makes one ineligible for Top 500 publication. In this figure, we restrict to those within the cutoff range (roughly $150,000 to $230,000), where (as we describe more below) the argument for quasi-random treatment is strongest.

[9] Although we have administrative data only for Top 500 list appearances starting in 2013, we additionally have compiled lists of the names of individuals appearing on the Top 250 list before 2013. None of these individuals appear to have received a warning letter in our sample period.

Figure 4: Behavior before and after the official letter, recipients vs. non-recipients (only those within cutoff range)



Notes: This figure compares the behavior of two groups of taxpayers around the date of the official letter. In gray are taxpayers who are eligible for publication and received a pre-letter, but did not receive an official letter. In blue are similar individuals (eligible pre-letter recipients) who did receive an official letter. In this figure we exclude individuals who previously received an official letter (i.e., we focus on first-time recipients), and we limit to those with balances between $150,000 and $230,000, the range of cutoff values inside of which treatment is quasi-random.

The figure suggests that the official letter has a strong effect. Average payment amounts jump after treatment among treated households. There is also a sharp rise in the share of treated taxpayers entering into new Installment Agreements, and more generally taking actions leading to ineligibility for publication. As for the share making payments, the above-trend but delayed effect (the gap between treated and untreated widening after three months) could be related to the Installment Agreements taking time to set up and first payments to begin.

The small break in trend after the letter for the control group is a mechanical effect: inclusion in the control group, like inclusion in the treatment group, is conditional on

eligibility for the letter, which necessarily means no compliance actions have been taken in the preceding months. Conditioning on an action not happening in the past means that the probability of it occurring in the next period is likely to jump; the patterns here show this, with the effect of the letter demonstrated by the additional increase in activity for the treated group above and beyond the mechanical effect for the control group.

We find similar, and in fact stronger, effects when examining the full sample (see Figure 17 in the Appendix). Seeing stronger effects relative to the effects among those in the cutoff range suggests that the official letter has larger effects on those with higher balances. Because we cannot compare treated high-balance debtors directly to untreated taxpayers with similar balances, however, we cannot fully rule out the possibility that full-sample results are caused by some unobserved phenomenon that happens to affect only high-balance debtors at just the time of treatment.

We also test the sensitivity of these patterns to our eligibility and other data filters. Figure 18 in the Appendix shows that the patterns are consistent when removing the pre-letter, initial receipt, and eligibility filters that are applied in our main specification above.

4.2. Random cutoff analysis

To develop a more precise, quantitative estimate of the effect of the official letter, we now turn to a regression analysis that exploits the variation in the official letter cutoff. Across the ten cycles in our data, the lowest balance receiving an official letter ranges from approximately $150,000 to $230,000, as shown in Figure 2. Because the cutoff is determined by the 500th highest eligible balance, the cutoff dollar value cannot be predicted precisely in advance. As individuals are accruing and paying down balances over time, independently, influenced by myriad factors unrelated to the publication program (for example, volatile income, liquidity constraints, and fluctuating asset values, to name a few), the ranking of balances and value of the 500th highest eligible balance changes such that individuals who are close to the range of historic cutoff values cannot know for sure whether they will be on the list or not. A publication-eligible taxpayer with $175,000 of balance as of the official letter date in one cycle might receive the letter, while in another cycle a taxpayer with the same balance would not. In effect, taxpayers randomly assigned to a cycle in which they do not receive a letter serve as controls for taxpayers with the same balance who are randomly

assigned to a cycle in which that balance does trigger a letter. This mitigates the possibility of selection into treatment.

We use this quasi-random variation to estimate the effect of official letter receipt. We start with a pooled difference-in-differences approach, as follows:

$$Outcome_{itc} = \alpha + \beta_1 \cdot Post_{itc} + \beta_2 \cdot Treat_{itc} + \beta_3 \cdot Post * Treat_{itc} + \gamma \cdot Balance_{ic} + \delta \cdot April_c + \varepsilon_{itc}$$

In this specification, i indexes individuals, c the cycle in which we observe them, and t indexes the month relative to the official letter mailing date, ranging from -6 to 6. $Post$=1 for months after the letter, $Treat$=1 for individuals receiving the letter, and $Post * Treat$ is their interaction. *Balance* does not vary over a given cycle and is measured at the date the official letter list is determined.[10] We also control for whether the observation is for an April or October Top 500 list cycle (to address the potential for seasonality in payments and other actions). We run this specification including only those taxpayers who have balances in the range of the cutoffs and who have never received an official letter before.[11] We test several outcomes, including three monthly binary variables: (1) starting a new Installment Agreement, (2) entering into any status that makes one ineligible for publication, and (3) making a positive payment. We also test the non-binary outcome of monthly payment amount, in dollars.

Table 2 below reports the results of this specification for the period between zero and three months after treatment. In these initial estimates, we limit the sample to observations with balances between $150,000 and $230,000, so that every treated unit has at least one untreated control with a similar balance. The strongest measurable effects are on new installment agreements, and new ineligibility status more generally. By construction, there are zero households in these statuses at the time of treatment. During the three months after treatment, the official letter led an average of an additional 1.2 percent of treated households to enter new Installment Agreements each month, relative to untreated households. For the more general outcome of new ineligibility, the effect is an average additional 2.8 percent each month. Although we observe a positive coefficient, we do not find a statistically significant

[10] Because we observe compliant households only in one cycle, we omit individual-unit fixed effects.
[11] We conduct a variety of robustness analysis to confirm that the results are not sensitive to our data filters, regression specifications, or other choices. Appendix D includes these results.

effect on payment amounts. Our point estimate for the probability of making a payment is positive but not significant at traditional levels.

Table 2: Official letter difference-in-difference results, observations within cutoff range

	Dependent variables:			
	Made payment	Payment amount ($)	New IA	New ineligibility
Official letter * Post	**0.0223***	**155.77**	**0.0119*****	**0.0283*****
	(0.0118)	**305.11**	**(0.0032)**	**(0.0066)**
Official letter	0.0010	73.73	-0.0004	-0.0004
	(0.0146)	173.93	(0.0003)	(0.0022)
Post	0.0192***	467.612***	0.0041***	0.036***
	(0.0028)	77.52	(0.0005)	(0.0016)
Balance	-0.0002	0.93	0.0000	0.0000
($ thousands)	(0.0002)	2.03	(0.0000)	(0.0000)
April publication	-0.0441***	-253.07***	-0.0014***	-0.0001
	(0.0049)	83.20	(0.0005)	(0.0015)
Intercept	0.1974***	319.08	0.0030	0.0099
	(0.0440)	356.70	(0.0022)	(0.0066)
Observations	37,848	37,848	37,848	37,848
R2	0.0047	0.0014	0.0041	0.0157
Mean dep var.	0.1583	606.2506	0.0029	0.0258

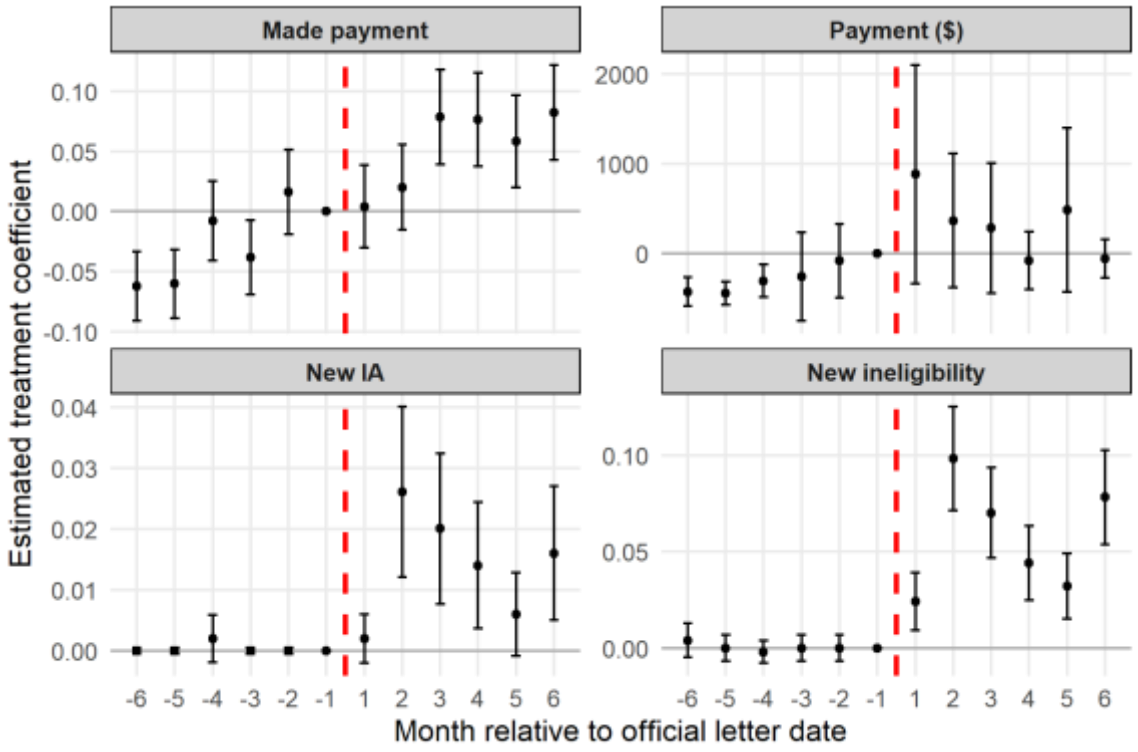
Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by taxpayer are shown in parentheses. This table reports the regression results for the main difference-in-difference specification. The underlying data include only those taxpayers eligible for publication who received a pre-letter for a given cycle and have balance within the range of cutoffs (roughly $150,000 to $230,000). The dollar value cutoff for official letter receipt depends on the 500th highest balance for a given cycle, and this varies across the ten cycles in our data as shown above in Figure 2. We thus argue that, within the range of cutoffs observed in our data, letter receipt is random.

We obtain similar results using an event study design, as follows:

$$Outcome_{itc} = \alpha + \sum_{t=-6}^{t=6} \beta_t Treat_{itc} + \gamma \cdot Balance_{ic} + \delta \cdot April_c + \varepsilon_{itc}$$

The results from this specification, run on the same population as for the diff-in-diff above, are shown graphically in Figure 5 below, with full results presented in Table 5 in Appendix C.

Figure 5: Official letter event study treatment coefficients



Notes: This figure plots the estimated coefficients on official letter receipt (treatment) dummies by month, from the event study regression approach. Coefficients are normalized relative to month t-1. Months are defined relative to the official letter date (month t+1 is defined as beginning on the letter date and ending 29 days later; month t-1 includes the 30 days prior to the letter date).

4.3. Full sample analysis

A difficulty with the design we have pursued so far is that it does not allow us to test the effects of treatment for balances above the highest historic cutoff. Because balances are rightward skewed, the bulk of the unpaid debt lies in this region, and so responses by these households are of considerable policy interest. We therefore repeat our analysis with these taxpayers included. In this set of specifications, we cannot ensure that each treated unit has a matched control, but we can at least control parametrically for (real) balance.

Because treatment is not randomly assigned for the upper tail of the distribution, we are more likely to have selection into treatment. That is, taxpayers with high balances are treated because they chose not to comply between the pre-letter and official letter. If anything, though, this selection effect reinforces our result. Those who do choose to be treated are those who are less apt to comply. We therefore are estimating the effects of treatment on a relatively unresponsive subset of the population. We cannot rule out the possibility, though, that some unobserved confounding event affects only high-balance treated households around the same time as the official letter.[12]

Table 3: Official letter difference-in-difference results, full range of observations

	Dependent variables:			
	Made payment	Payment amount ($)	New IA	New ineligibility
Official letter * Post	**0.0392*** (0.0063)**	**1621.5*** (471.06)**	**0.013*** (0.0018)**	**0.0271*** (0.0035)**
Official letter	-0.05*** (0.0073)	(220.47) (216.96)	-0.0005*** (0.0001)	0.0007 (0.0012)
Post	0.0193*** (0.0016)	590.31*** (82.80)	0.004*** (0.0003)	0.0379*** (0.0009)
Balance ($ thousands)	0*** 0.0000	0.2771 0.1811	0*** 0.0000	0.0000 0.0000
April publication	-0.0414*** (0.0027)	-497.45*** (99.07)	-0.0007** (0.0003)	-0.0001 (0.0009)
Intercept	0.196*** (0.0047)	629.87*** (69.95)	0.001*** (0.0002)	0.0066*** (0.0006)
Observations	126,444	126,444	126,444	126,444
R2	0.0048	0.0018	0.0042	0.0167
Mean dep var.	0.1803	842.2733	0.0031	0.0268

[12] When we estimate including households that are above the cutoff but ineligible for publication, we obtain essentially the same results. In this specification, high-balance ineligible households serve as an additional control for the high-balance treated households. To be sure, there are reasons to believe that ineligible and eligible households would respond differently to treatment. But what we can say is that any unobserved confounder that is driving our results would have to affect only those high-balance households that are treated, and do so at around the time of treatment.

Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by taxpayer are shown in parentheses. This table reports the regression results for the main difference-in-difference specification. The underlying data include only those taxpayers eligible for publication who received a pre-letter for a given cycle, but without the restriction on balance from the main specification. This allows us to now include the higher balances that make up most of the Top 500 list and are of greater policy interest.

The effects on average payment amounts in the three months after treatment are much larger in the full sample population, with a 95% confidence interval for the mean monthly treatment effect that runs from about $700 to $2,500. New Installment Agreements and new ineligibility determinations are very close to the restricted-sample estimates.

4.4. Heterogeneous effects

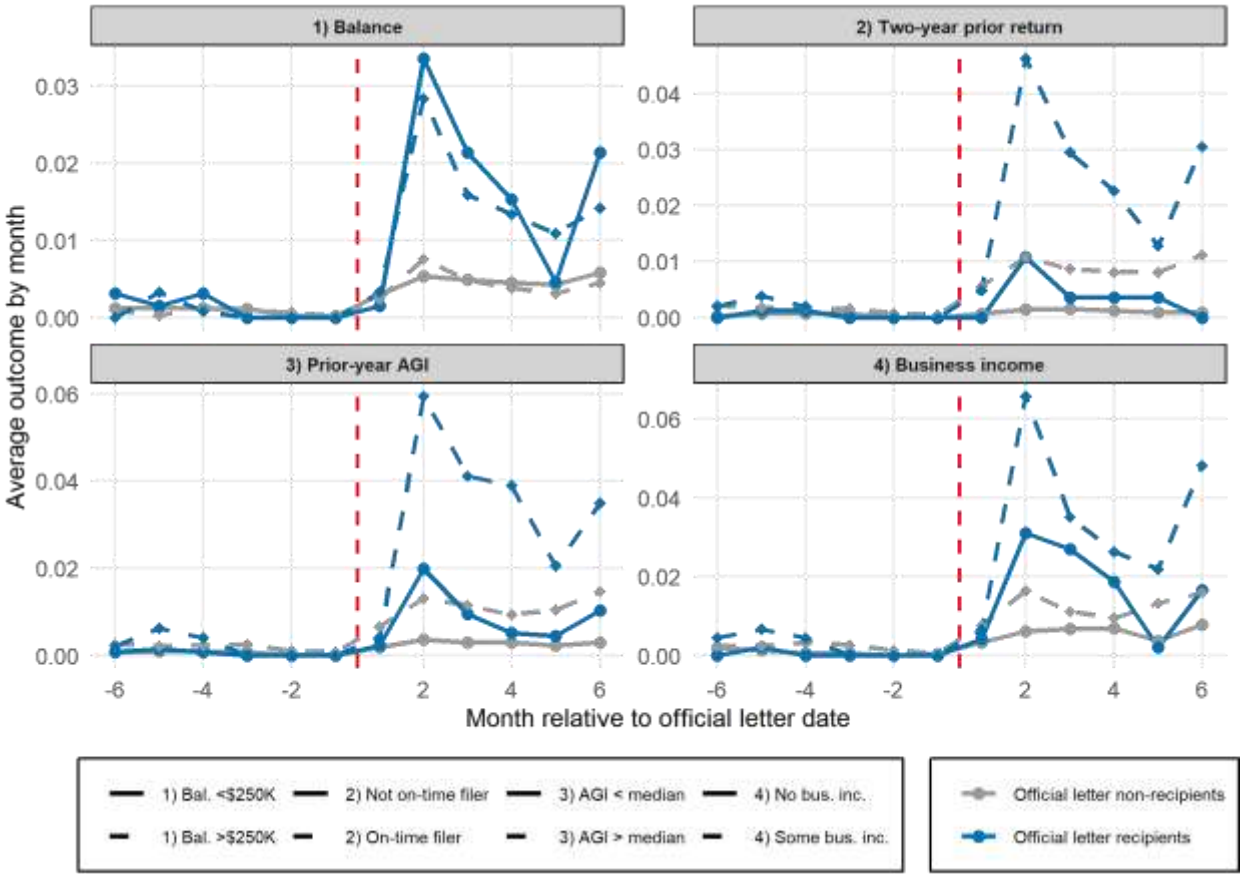
It is also of interest to explore whether treatment effects vary based on observable taxpayer characteristics. For example, as Kuchumova (2021) argues, disclosure is more likely efficient if it disproportionately affects high-earning households, as we expect based on the results in DT. We similarly expect to see larger results for filers with business income: businesses are likely more subject to reputational pressure, and non-business filers are more likely to have been subject to withholding or wage garnishment, leaving less room for them to change behavior in response in treatment. Holding these other factors equal, taxpayers who have already exhibited a relatively high subjective cost of compliance, such as by failing to file any tax returns at all, are also likely to be less responsive.

We therefore re-estimate our regressions from prior sections, this time conditioning on three key data points from our linked individual income-tax data: reported income levels (using CA AGI), the presence of business income, and whether the household filed a return for the prior tax year. For AGI and business income, we use values from the tax return filed in the same year as treatment (and thus exogenous to treatment, reflecting actions from the year prior). Thus, for the April and October 2015 cycles, we use income reported for the 2014 tax year, and we record the household as having filed on-time if they filed a return in 2014 for the 2013 tax year.[13]

[13] We use filing for the prior year as a marker of on-time filing because we cannot observe exactly when in a year a return is filed. Thus, for April 2015, we would not be able to tell if a return for the 2014 tax year was filed in April 2015 or instead in December 2015.

We first summarize the results graphically in Figure 6. The Figure plots results by sub-group: balances above and below $250,000; on-time filers and non-filers; AGI above and below median (roughly $40,000); and whether the taxpayer reports any business income. The installment agreement response is stronger for the sub-groups we predict: high AGI, on-time filing, and business income all appear to correlate with larger effects on the probability of starting an installment agreement. For ease of reading, we report only the Installment Agreement outcome in this Figure. Appendix Figure 20 shows all four outcomes; treatment-effect differences for average payments and ineligibility status are similar to those reported here. High balance is correlated with higher post-treatment payments, but unrelated to the other outcomes.

Figure 6: Installment agreement behavior around official letter, splitting on tax characteristics



Notes: This figure compares the behavior of taxpayers around the date of the official letter, here showing only the installment agreement outcome (Appendix Figure 20 shows all four outcomes). In gray are taxpayers who are eligible for publication and received a pre-letter, but did not receive an official letter. In blue are similar

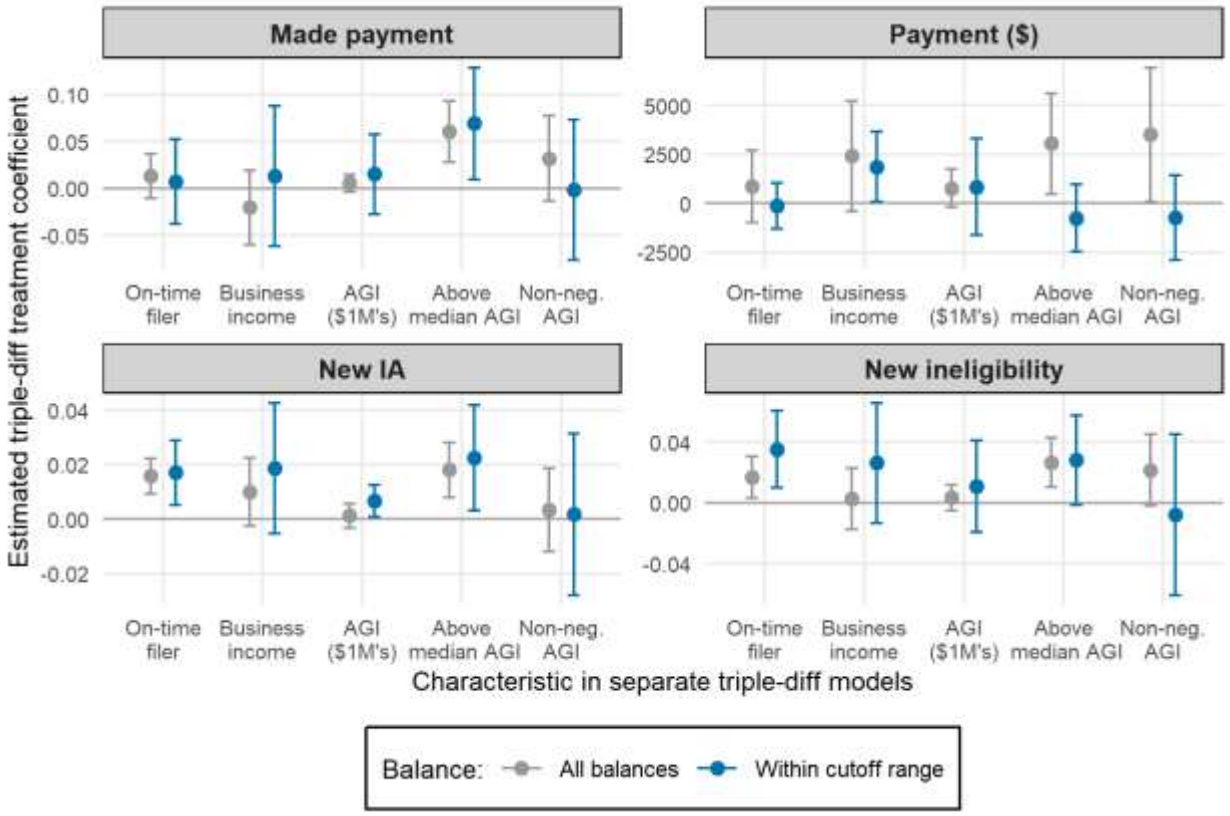
individuals (eligible pre-letter recipients) who did receive an official letter. In this figure we exclude individuals who previously received an official letter (i.e., we focus on first-time recipients). Taxpayers are further split based on balance or tax return characteristics. Dashed lines represent one of the sub-groups, solid lines the other.

We next examine these relationships in a regression framework. We repeat the analysis both restricting to households with balances within the cutoff range, and also over the full sample. The estimates thus take the form:

$$Outcome_{itc} = \alpha + \beta_1 \cdot Post_{itc} + \beta_2 \cdot Treat_{itc} + \beta_3 \cdot Post * Treat_{itc} + \beta_4 \cdot Characteristic_{itc} + \beta_5 \cdot Post * Characteristic_{itc} + \beta_6 \cdot Post * Treat_{itc} * Characteristic_{itc} + \gamma \cdot Balance_{ic} + \delta \cdot April_c + \varepsilon_{itc}$$

where *Characteristic* is one of the four sub-groupings by balance, AGI, filing status, and business income. The coefficient of interest is β_6, the continuous incremental effect of treatment per unit of AGI (in millions here, for coefficient comparability), or the discrete incremental effect of having above-median AGI, non-negative AGI, the presence of business income, or on-time filing. Regression results are summarized in Figure 7 and tabulated in more detail in Appendix C.

Figure 7: Estimates of treatment by sub-groups



Notes: This figure presents the coefficient estimates for separate triple difference models testing four characteristics (i.e., the estimate for the coefficient on treatment X post X characteristic). 95% confidence intervals are shown around the point estimates. In blue are the estimates using only observations within the cutoff range. These can be compared to the estimates in gray, from models using the full range of balance observations. Corresponding tables can be found in Appendix C.

Reported income level appears to play an important role in responses to treatment. Treated households reporting above-median AGI are more likely to make a payment, enter into an Installment Agreement, or otherwise establish ineligibility.[14] These effects become insignificant when considering non-negative AGI instead of above median AGI, suggesting it is indeed higher AGI amounts that are driving this effect. In the full sample, average monthly payments after treatment are also much higher among households with above-median AGI,

[14] We find no significant effects of an interaction between treatment and a linear and continuous measure of AGI. This is not surprising, as there is no particular reason to expect that the impact of AGI will be linear in AGI. As an alternative, we also include interactions with AGI quintiles. Although less precisely measured, results for the upper quintiles, particularly the topmost quintile, are similar to those for the above-median results we present in the main text (see Appendix C, Figure 19).

though our point estimate is close to zero when we limit only to balances in the historic cutoff range.

In addition, we see a relatively large and statistically significant increase in the impact of treatment for filers with business income, with a point estimate about ten times larger than for all filers, although still moderately sized in economic terms, about $1,900 in additional payments each month. To be sure, we can only observe these outcomes for the subset of households for which we have tax filings, and so they may not be fully representative of all households. What we can say for certain is that conditional on filing, business income predicts a greater response.

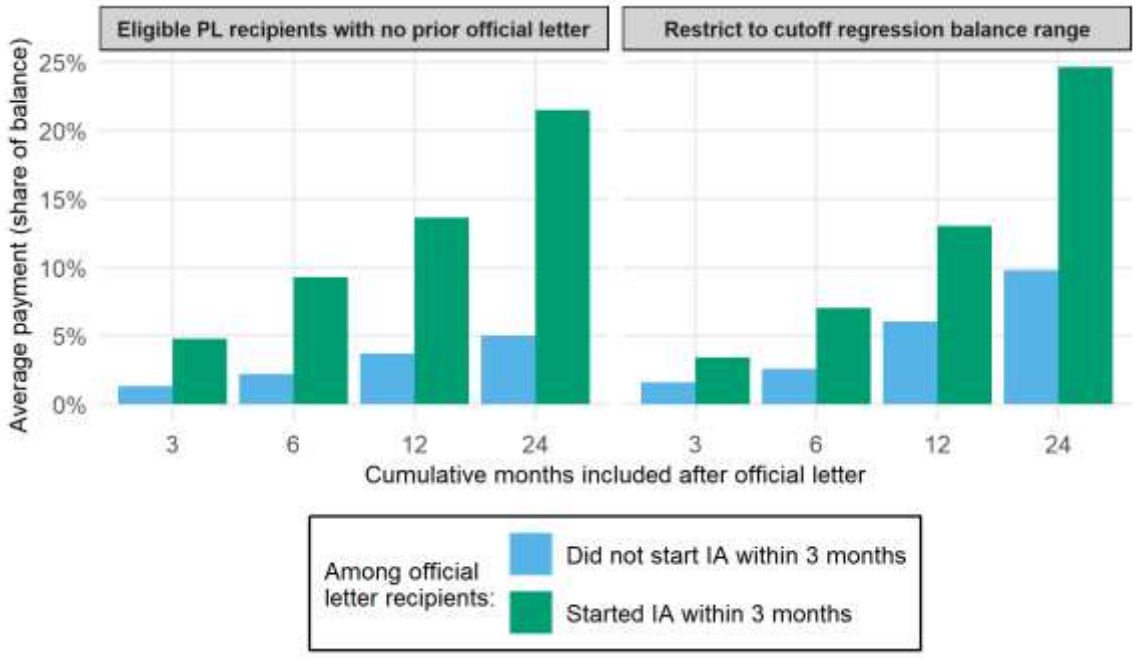
We do not find evidence that filing status affects payment, but we do find it correlated with other compliance outcomes. For average monthly payment, our point estimate for the interaction of filing status with treatment is small and negative. Our confidence interval is fairly wide, however, so that we cannot rule out increases or decreases of $1,000. On-time filing does strongly correlate with an increased propensity to reach an Installment Agreement or other ineligible status, with 95% confidence intervals suggesting that these are both about twice as common among the filer population as among other treated households.

4.5. Long-run results

For purposes of program evaluation, it is useful to know whether treatment leads official letter recipients to remit more money than others over the long term. Among other reasons, one of the main responses we observe is a greater share of taxpayers who enter into payment agreements with FTB. Do these agreements actually bring in more money over time? At a minimum, it would be useful to know whether individuals who reach agreements in order to avoid publication quickly renege.

We can readily rule out the possibility that installment agreements are quickly broken. We sum payments by official letter-recipient households, and compare those who signed new installment agreements within three months of receiving a letter against all other recipients, as summarized in Figure 8. Installment Agreements strongly predict increased payments, whether over six, twelve, or twenty-four months after treatment. This result holds among all recipients, and also when restricting to balances within the cutoff range.

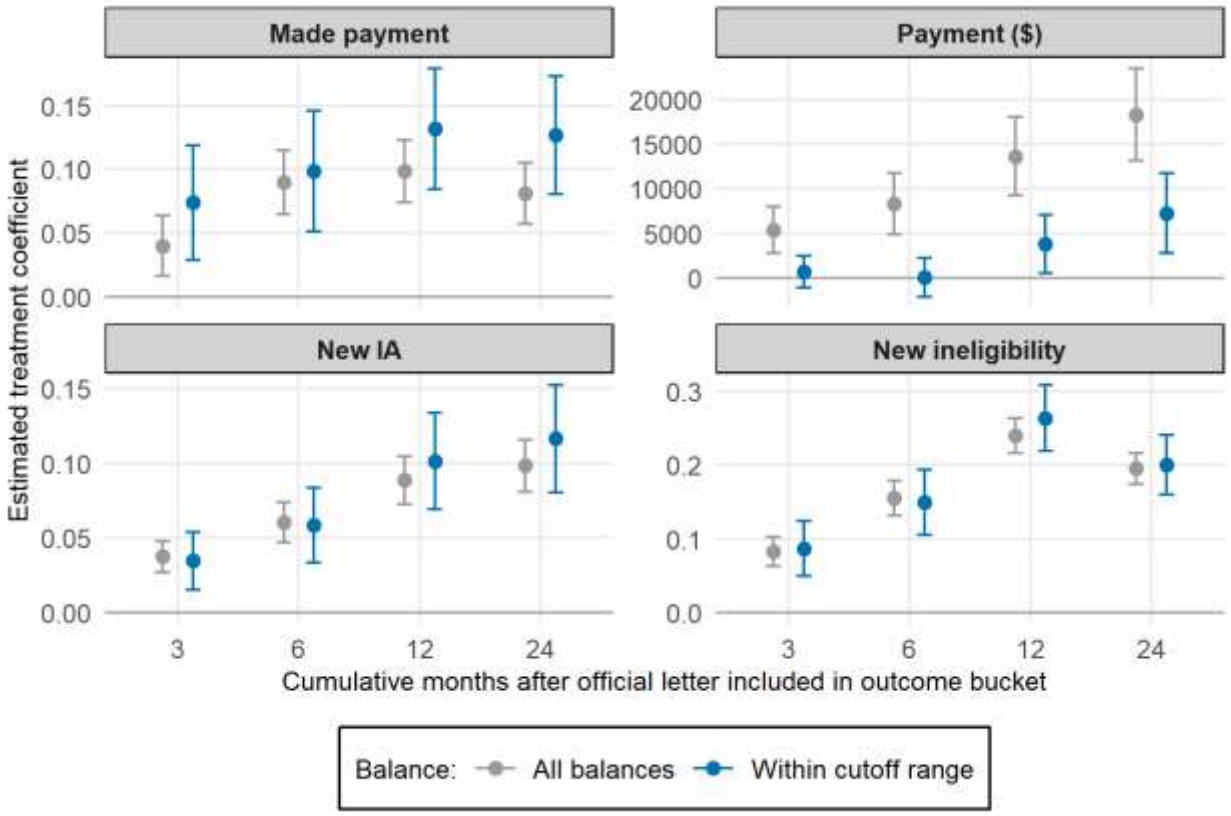
Figure 8: Payment share among official letter recipients, effect of IAs



Notes: The figure summarizes average payments as a share of starting balance by households entering an installment agreement with FTB within three months of receiving an official letter, in green. Blue bars represent averages for all other treated households. The sub-graph on the right restricts observations to those where the unpaid balance at time of the letter falls within the cutoff range.

To provide at least a suggestive sense of other longer-run impacts of treatment, we repeat our regression analyses above, but collect cumulative results over the one- and two-year periods following a letter cycle. Admittedly, though, as we extend our observation window over a longer horizon, it is harder to rule out confounding effects. We thus present just a figure summarizing the main coefficients of interest for most of the outcomes.

Figure 9: Official letter long-term effect estimates



Notes: This figure summarizes estimated coefficients for regressions in which the variable in the grey bar is the outcome, cumulatively defined over the number of months post-official letter noted on the X-axis. Blue markers are for regressions in which we restrict the sample to observations with balances falling in between the lowest and highest observed Top 500 cutoff balances.

Because total payments are of particular interest for our evaluation framework, we present more detail on the long-run impact of treatment on total revenue collected. Although, again, the short-run effects of treatment are not statistically significant when we restrict to between-cutoff balances, over time these units do pay a good bit more, with a point estimate for incremental payments over two years of about $7,200 per household, and a 95% confidence interval ranging from $2,800 to $11,700, as shown in Table 4 below. As above, when we include all households in the analysis, the estimate is again much larger, with a point estimate of about $18,000 and a 95% confidence interval of $12,800 to $23,100 (see Table 8 in Appendix C). As one further alternative approach, we run these tests excluding those we deem "partial controls" – individuals in the control group who receive a letter in a subsequent cycle and whose behavior during the outcome window may thus reflect a response to that

later letter.[15] The estimated effects under this alternative approach are larger, although less precisely estimated (see Figure 21 in Appendix D).

Table 4: Official letter long-term payment effects (observations within cutoff range)

	Dependent variables: Cumulative payment amount post-official-letter ($)			
	3 months	6 months	12 months	24 months
Official letter	**693.15**	**111.26**	**3833.59****	**7260.01*****
	(904.73)	**(1,099.56)**	**(1,658.15)**	**(2,274.17)**
Balance	-0.19	5.89	-10.52	-27.72
($ thousands)	(9.65)	(14.86)	(20.24)	(29.34)
April publication	-1104.84***	-839.11	-25.76	335.50
	(414.60)	(612.57)	(680.92)	(824.58)
Intercept	3064.36*	4472.92*	10583.07***	18379.12***
	(1,700.09)	(2,708.97)	(3,710.08)	(5,332.18)
Observations	6,308	6,308	6,308	6,308
R2	0.0013	0.0003	0.0010	0.0021
Mean dep var.	2,539	5,126	8,980	14,129

Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by taxpayer are shown in parentheses. Outcomes measured as cumulative payments between time of treatment and time following, as listed in column headers. Only observations within the cutoff range are included.

4.6. Subsequent reported earnings

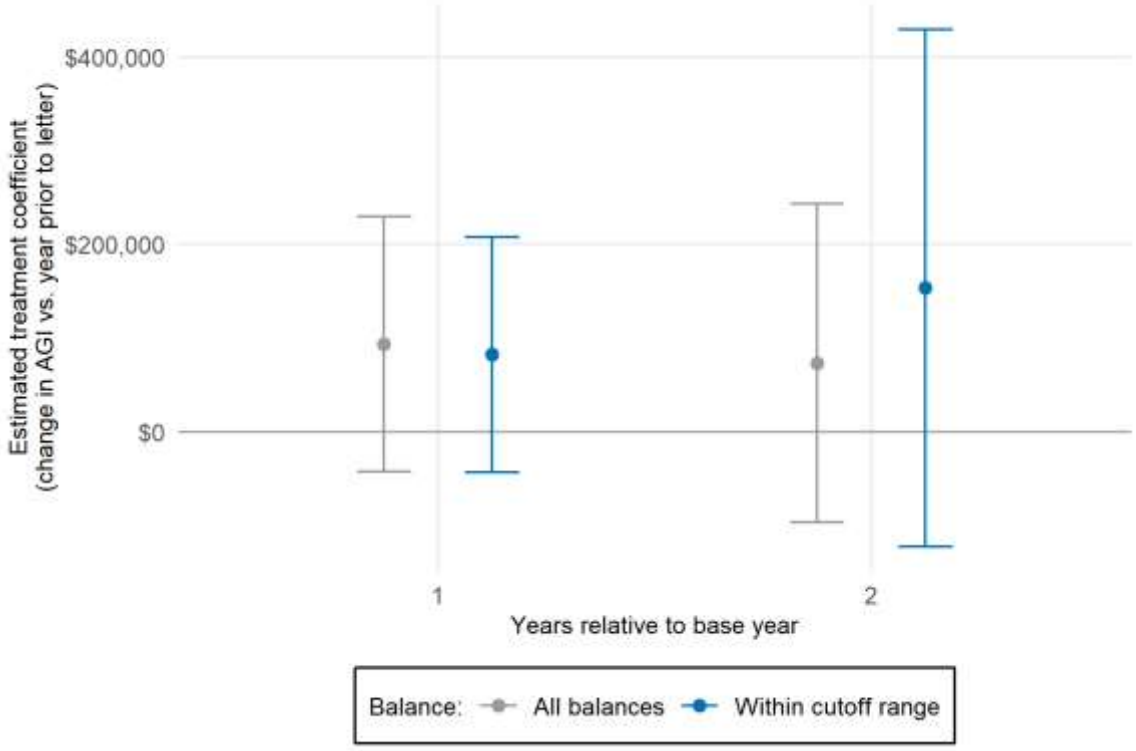
It is possible that disclosure programs are counter-productive if they reduce taxpayer ability to pay in the long run, as findings by DT suggested might be the case for some businesses. Likewise, disclosure might backfire if it crowds out future voluntary compliance efforts, potentially reducing *reported* income (see Luttmer & Singhal (2014) for more

[15] Although retaining some partially-treated units could bias our results downwards, we prefer those estimates because we view them as more conservative. Dropping taxpayers who are subsequently treated from the control could potentially bias results upwards. Taxpayers who are treated in a subsequent cycle are by definition non-compliant for an extended period, and may even have taken actions that deepen their debt. Comparing our treated group to this selected non-responsive group might overstate the effects of treatment relative to the general population of tax delinquents.

discussion). Accordingly, we exploit our ability to link payments data to tax filings to test whether there are any observable long-run impacts on income reported to FTB.

In general, although our point estimates are positive and relatively large, we cannot rule out economically meaningful declines in reported earnings. In our full sample, for instance, the 95% confidence interval for the household's change in AGI between treatment and two years after treatment ranges from -$128,000 to $435,000. Because we have access only to pre-audited income, we cannot tell whether any possible declines might be due to actual reductions in taxpayer earnings or whether they are only a change in reporting.

Figure 10: Effects of official letter on subsequent AGI



Notes: This figure presents the coefficient estimates and 95% confidence intervals for regressions testing the effect of official letter receipt on subsequent changes in AGI.

5. Publication and license revocation

The previous section showed that notifying taxpayers that if they do not take action, their information will be published and they may face license suspension clearly causes a substantial number to take action. Next, we attempt to learn whether publication itself has any effect on the taxpayers who ultimately do get published. As with the official letter, there

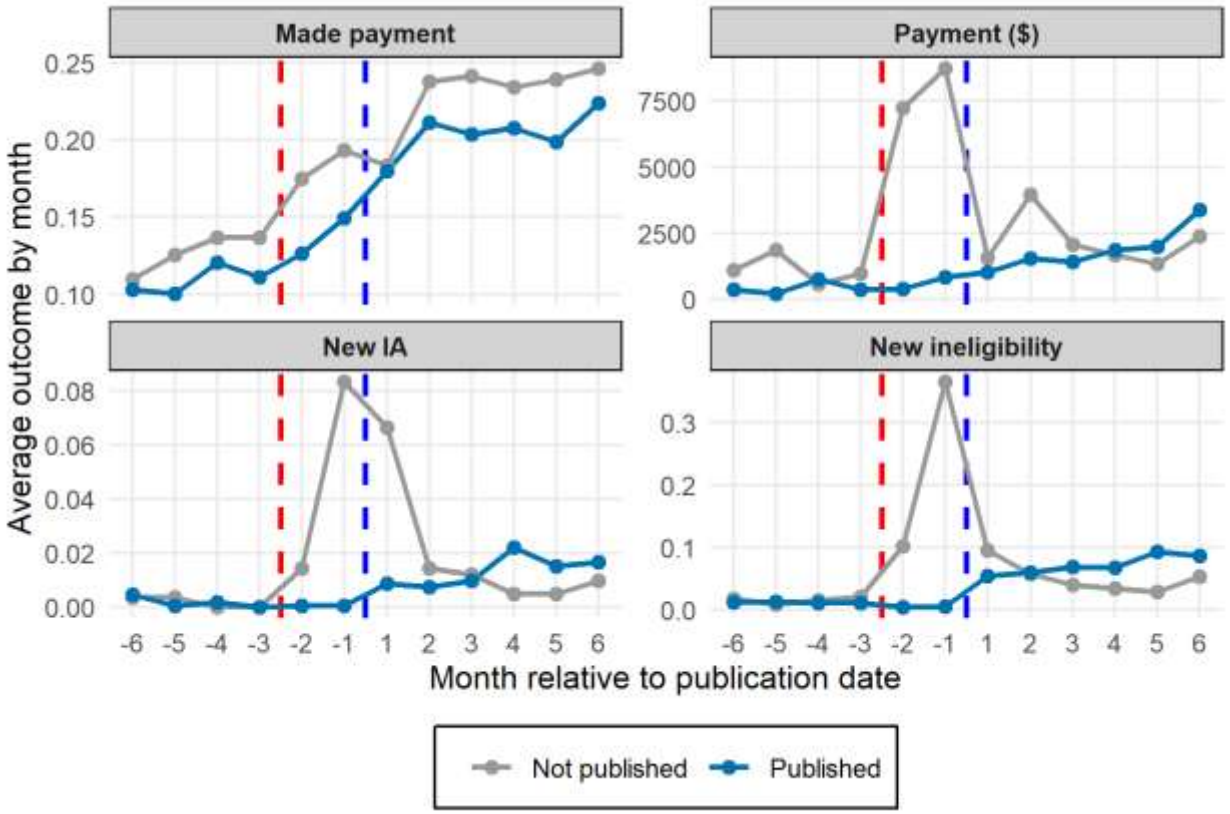
is potential selection before publication, as the individuals who are published on the Top 500 list are the individuals who were given an opportunity to avoid publication, by taking action after the official letter, *and chose not to take such action*. As we do observe a fair bit of response to the official letter, we may not expect publication itself to have much additional impact. DT do find a moderate incremental effect from publication, but their study involved a first-time rollout of a program, such that it may not have been clear *ex ante* whether the government would carry through with its threat or what impact disclosure would have.

Because we aim at the marginal compliance effect of publication over and above receipt of the official letter, we restrict our analysis here to official letter recipients. We then observe the behavior of the two groups before and after publication. As above, we focus on first-time letter recipients to understand the effect of a taxpayer's first encounter with the risk of publication.[16]

Figure 11 below shows the time series comparison of the published and unpublished first-time official letter recipients. The red vertical line indicates the official letter date, while the blue vertical line indicates the Top 500 publication date. The gray series represents the average behavior among the first-time official letter recipients who are *not* published. As expected, we observe larger spikes at the time of the official letter for this group than we plotted in earlier figures, as we are splitting out the subgroup that did not respond.

[16] The findings in this section are similar if we include all official letter recipients, as shown in Appendix D.

Figure 11: Behavior around the publication date among first-time official letter recipients



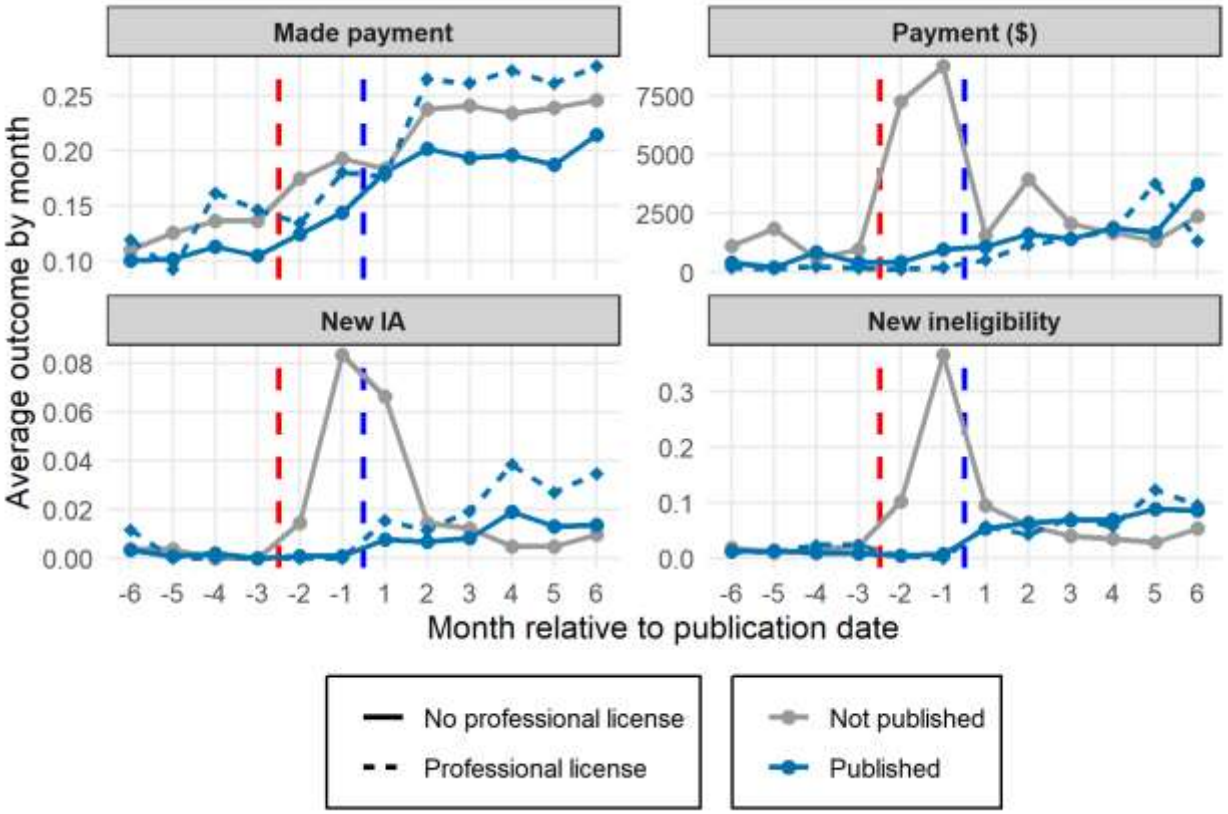
Notes: This figure compares the behavior of first-time official letter recipients, separately showing those that ultimately get published (in light blue) and do not (in gray). The red vertical line indicates the official letter date, and the blue vertical line indicates the Top 500 publication date.

The blue series represents the average behavior among the first-time official letter recipients who *do* get published. Their lack of response to the letter is what leads them to be published. We see little evidence that this group responds to publication, except for a small bump in new installment agreements about four months after publication. This timing coincides with the statutory timing for license revocation notices, which issue 120 days after publication (after an initial warning 30 days following).

To better understand whether the observed response may be related to license suspension, we further segregate the population by whether FTB data show any professional license that might be subject to suspension. FTB only collects these data for published

taxpayers, so we cannot provide a full triple-differences analysis.[17] Graphical analysis is suggestive, though, that license suspension does have some impact, as illustrated in Figure 12. In the figure, there is a noticeable above-trend surge in the likelihood of making any payment for license holders (plotted using the dashed light blue line) around the date of the first license notification, 30 days after publication. We also see a slightly higher share of installment agreements, peaking at the time when license suspension would take effect, 120 days after publication.

Figure 12: Behavior around the publication date, split by license holding



Notes: This figure compares the behavior of first-time official letter recipients, separately showing those that ultimately get published (in light blue) and do not (in gray). The published individuals are further split into those with professional licenses (dashed line) and those without (solid line). The red vertical line indicates the official letter date, and the blue vertical line indicates the Top 500 publication date.

[17] If data on licenses were available for all individuals, it would be interesting to test for differential responses to the official letter based on license-holding and among different license types. It is possible that some of the response to the official letter is driven by concern over future license suspension, rather than publication.

6. Discussion and conclusion

We have examined the economic impact of California's Top 500 program, which exposes individual taxpayers with large unpaid tax debts to the threat of public disclosure and license suspension. Treated taxpayers receiving a warning that disclosure is imminent respond with large increases in their efforts to reach payment agreements with the California tax authority. Treated taxpayers also pay more, with increases in payment particularly concentrated among those with greatest ability to pay and those with business income. In Appendix C, we use these findings as inputs in a cost-benefit analysis of the program. While results are potentially sensitive to assumptions about how to measure deadweight loss, our calculations suggest that, looking only at the most direct effects of the program, it increases social welfare substantially. We also find evidence that a good portion of this gain derives from reduced tax avoidance or evasion.

The California experience therefore suggests that non-monetary tax enforcement tools may be a useful component of a regulatory toolkit. The households we have studied are ones where the government has all but exhausted its ordinary collection efforts. That we identify a relatively sizable additional response from the threat of disclosure and license suspension even from this population suggests that disclosure would likely be quite effective for more-compliant households, especially if such households also attach a higher subjective value to being *seen* as compliant.

Further, our results shed light not only on whether the Top 500 Program improves social welfare, but also more generally on why households appear on the Top 500 list. As expected, ability to pay is likely part of the story. We see that, holding balance constant, households with the highest reported AGI respond to treatment with the greatest payments, the largest increase in likelihood of making any payment, and the greatest propensity to enter into an installment agreement.

But ability to pay is not the full story. If households are able to respond to treatment by paying tens of thousands of dollars on average, why don't they pay when their taxes are due, or when they receive personal contact from an FTB employee, or when they receive a notice of tax lien? At prevailing California income tax rates, a taxpayer would likely have needed to earn at some point in the past in aggregate hundreds of thousands of dollars in taxable (not gross) income in order to accumulate tax debts in excess of our lowest observed list cutoff

value of $150,000. Income volatility may result in large fluctuations in ability to pay over time, but would not explain why treated households are more apt to pay than others. We further see a strong correlation of basic tax compliance through the filing of a return with subsequent willingness to comply. The correlation of *reported* income with compliance outcomes may similarly indicate a household's preferences for tax compliance, rather than only their ability to pay.

The data are therefore consistent with a substantial amount of deliberate tax avoidance among the observed population. As an additional piece of evidence on this front, we note that tax-filing business owners, who are not subject to withholding, are far more responsive to treatment than the average tax-filing household. This is consistent with the hypothesis that business owners are better able to protect their assets from other collection efforts. It is also possible that business-owners are more responsive because disclosure is more costly, such as by affecting the business's relationship with customers or suppliers (as noted by DT). We do not observe any significant differences in post-treatment reported earnings for business owners versus other taxpayers, however. Taken together, these results suggest that non-monetary sanctions may be most effective, and socially desirable, when designed to target groups of noncompliant taxpayers with higher ability to pay and who are more likely to be avoiding or evading taxes, rather than being noncompliant due to financial constraints.

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Appendices

Appendix A: Sample FTB Documents

Figure 13: Sample pre-letter

Information Regarding Public Disclosure of Tax Delinquency

Notice Date:
Taxable Years:

Account Number:
Balance Due:
Pay By:

Revenue and Taxation Code (R&TC) Section 19195 directs the Franchise Tax Board (FTB) to publicly disclose a list of the 500 largest state income tax delinquencies. These delinquencies must total in excess of $100,000 and be subject to a recorded notice of state tax lien. We intend to post this list on our website at **ftb.ca.gov**.

Your account may qualify for this disclosure and Internet posting. If we determine that you are among the 500 largest tax delinquencies, we will send you a notice by certified mail advising you of the inclusion on this list of your name, address, and any occupational or professional licenses with status. At the time of publication, **your occupational, professional, and driver licenses issued by a California agency will be submitted for suspension**. You will be prohibited from contracting with any California state agency for the acquisition of goods or services. **Final determination of the top 500 names eligible for publication is pending confirmation of any resolutions or other qualifying circumstances for exclusion, including payment or arrangement for payment of tax liabilities.**

Pay your debt in full. You may be required to make payments electronically. Go to **ftb.ca.gov** and search for **mandatory epay**. If your estimated tax or extension payment exceeds $20,000 or your tax liability exceeds $80,000 for any taxable year beginning on or after January 1, 2009, you must make all future payments electronically, regardless of the taxable year. Payments made by other means will result in a penalty of 1 percent of the amount paid, unless your failure to pay was for reasonable cause and not willful neglect (R&TC Section 19011.5). If you are not required to pay electronically, enclose the above part of this notice and mail it with a check or money order for the total amount due payable to the Franchise Tax Board. Write your full name and account number on your payment. **Use the enclosed return envelope and mail to:** FRANCHISE TAX BOARD, PO BOX 3065, RANCHO CORDOVA, CA 95741-3065. No additional penalties or interest accrue on the existing liability if we receive full payment within 15 days of the notice date.

Call 888.426.8555 if you have questions, need assistance, think you do not owe this amount, paid the balance due, or filed bankruptcy.

Get FTB 1131, *Franchise Tax Board Privacy Notice,* at **ftb.ca.gov** and search for **privacy notice**.

Get FTB 1140, *Personal Income Tax Collections Information,* at **ftb.ca.gov** and search for **1140**.

Internet and Telephone Assistance

Website: **ftb.ca.gov**
Telephone: 888.426.8555 from within the United States
916.845.7874 from outside the United States
TTY/TDD: 800.822.6268 for persons with hearing or speech impairments

FTB 3703 PIT PC (REV 03-2012)

Figure 14: Sample official letter

Notice of Public Disclosure of Tax Delinquency

Notice Date:
Taxable Years:

Account Number:
Balance Due:
Pay By:

Revenue and Taxation Code (R&TC) Section 19195 directs the Franchise Tax Board (FTB) to publicly disclose a list of the 500 largest state income tax delinquencies. These delinquencies must total in excess of $100,000 and be subject to a recorded notice of state tax lien. We intend to post this list on our website at **ftb.ca.gov**.

Your account qualifies for this disclosure and Internet posting. If you do not pay your tax liability or take other action described below, we may add to a list posted on our website:

- **Your name and address.**
- Your occupational or professional licenses with type, status, and license numbers.
- The liened amount owed and the earliest date a notice of state tax lien was recorded.

Your inclusion on the list may lead to the denial or suspension of your licenses, including driver's licenses, under Business and Professions Code Section 494.5, and will preclude you from entering into contracts for the acquisition of goods or services with California state agencies under Contract Code Section 10295.4.

To avoid public disclosure of tax delinquency, you must do **one** of the following within 30 days of the notice date:

- **Pay your balance due. You may be required to make payments electronically.** Go to **ftb.ca.gov** and search for **mandatory epay**. If your estimated tax or extension payment exceeds $20,000 or your tax liability exceeds $80,000 for any taxable year beginning on or after January 1, 2009, **you must make all future payments electronically**, regardless of the taxable year. Payments made by other means will result in a penalty of 1 percent of the amount paid, unless your failure to pay was for reasonable cause and not willful neglect (R&TC Section 19011.5). If you are not required to pay electronically, enclose the above part of this notice and mail it with a check or money order for the total amount due payable to the Franchise Tax Board. Write your full name and account number on your payment. **Use the enclosed return envelope and mail to:** FRANCHISE TAX BOARD, PO BOX 3065, RANCHO CORDOVA, CA 95741-3065. No additional penalties or interest accrue on the existing liability if we receive full payment within 15 days of the notice date.
- **Arrange to pay your balance due**. To determine if you qualify for installment payments, call us at 888.426.8555.

Partial payment (even a reduction of the balance due below $100,000) will not preclude you from being on the list.
If your name appears on the list, FTB will continue to pursue collection actions. Call 888.426.8555 if you believe you should not be on the list, have questions, paid the balance due, made payment arrangements, otherwise resolved the balance due, think you do not owe the balance due, or filed bankruptcy.

Get FTB 1131, *Franchise Tax Board Privacy Notice,* at **ftb.ca.gov** and search for **privacy notice**.

Get FTB 1140, *Personal Income Tax Collections Information,* at **ftb.ca.gov** and search for **1140**.

Internet and Telephone Assistance

Website:	**ftb.ca.gov**
Telephone:	888.426.8555 from within the United States
	916.845.7874 from outside the United States
TTY/TDD:	800.822.6268 for persons with hearing or speech impairments

FTB 4192 PIT PC (REV 10-2012)

Figure 15: Top 500 website, landing page (10/6/2020)

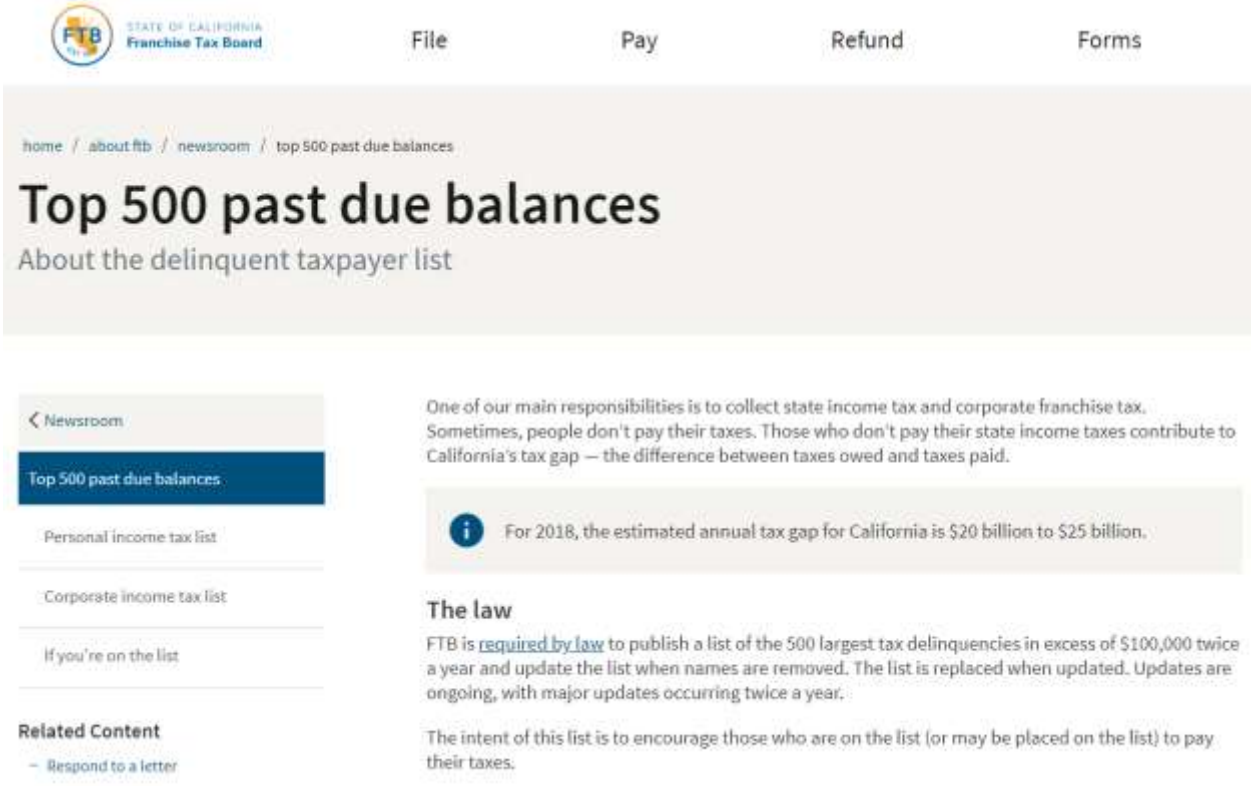

Figure 16: Top 500 list website, top balances (10/6/2020)

This is a list of the 500 largest tax delinquencies over $100,000. By law, we must publish this list at least twice a year.

Last updated: 10/06/2020

Name	Address	Subtotal	Total	Lien	License	Status	Number
Moreland, Peggy J & Terry L	Bakersfield, CA 93306	$5,306,836.86	$5,306,836.86	01/24/2008	Contractor's State License Board	Expired	856954
					Contractor's State License Board	Expired	362166
Cooksey, Jimmy D	Bowling Green, KY 42104	$2,403,194.62	$2,403,194.62	04/25/2008			
Amin, Joseph & Sharona	Beverly Hills, CA 90210	$1,730,698.65	$1,730,698.65	04/14/2014	Board of Pharmacy	Active	0034252
Patrick, William L & Susan K	Cody, WY 82414	$1,648,546.31	$1,648,546.31	05/31/2019			

Appendix B: Social Welfare Analysis

B1. An evaluation framework for tax debt collection and non-monetary sanctions

With these outcomes in hand, we now aim to evaluate the California program through two related frameworks. First, we ask whether taken on its own it likely increases social welfare, relative to a baseline of no added enforcement of any kind. Second, we ask whether disclosure is optimal given alternative supplemental enforcement choices, such as increased fines or penalties on late payers.

Analysis of the first frame is familiar. Tax compliance efforts are not universally welfare-improving. Instead, as Keen and Slemrod (2017) show, the necessary condition for welfare-improving compliance policy α is:

$$\phi(tz_\alpha - a_\alpha) - c_\alpha > 0 \qquad (1)$$

where tz_α is total tax revenue caused by the policy (tax rate t times marginal taxable income z_α), a_α is the administrative cost of the policy, and c_α is the net marginal compliance or concealment cost. ϕ is the weight applied to government revenues, generally the marginal social value of public spending (Meiselman 2018), which we assume to equal the marginal cost of public funds (Hendren and Sprung-Keyser 2020 provides a more comprehensive estimate of MVPEs for an array of spending options). In words, a public expenditure on increased compliance increases welfare when the marginal value of additional public funds, net of marginal enforcement, avoidance, and compliance costs, exceeds zero.

Net private costs c_α can be either positive or negative. Intuitively, when some tax avoiders become compliant, they no longer incur private avoidance costs, but instead must bear the costs of compliance, while infra-marginal avoiders may strictly increase avoidance expenditures. In a simple setting where taxes are not shifted across bases or time, we can infer that, for taxpayers at the margin, the amount of additional tax paid and compliance costs incurred equals the amount of private avoidance costs saved (Feldstein 1999, Chetty 2009). Thus, if marginal compliance costs are small, the marginal revenues from an enforcement effort, weighted by the value of public expenditures, and net of the public expenditures on that effort, offer a reasonable starting point for the social benefit of

enforcement (Keen and Slemrod 2017). In our setting, direct compliance costs over and above those of payment itself are likely minimal.[18] But marginal private costs should also include any measurable impact on avoidance expenditures by infra-marginal avoiders.

Accordingly, we aim to measure what we argue are the two key inputs into this basic welfare analysis: net revenues and infra-marginal avoidance costs. Social planners could then weight our net revenue estimate by their preferred value for the marginal value of public expenditures to assess whether the Top 500 program increases welfare relative to a baseline of no special enforcement policies for the largest debtors.

It is also useful to evaluate whether the Top 500 program is preferable to alternative methods for collecting large tax debts. For example, standard enforcement theory holds that monetary sanctions are usually preferable to non-monetary sanctions such as disclosure (Polinsky and Shavell 2000). While both might be capable of motivating compliance, a non-monetary sanction such as disclosure imposes costs on non-compliers with no offsetting gains, whereas a fine is a transfer and potentially welfare-neutral.[19]

More recent work suggests some potential reasons for preferring disclosure in select instances. The first of these relates to the possible effect of disclosure on tax evasion, as in Kuchumova (2018), which models both driver's license and passport suspension as instruments for reducing evasion. In her framework, the non-monetary sanction potentially serves as a "tag" for higher true earnings. Because driving and international travel are forms of consumption that are correlated with income but largely unobservable by the tax authority, suspension effectively imposes a higher tax on individuals with greater true earnings. Welfare gains from this improved targeting can exceed welfare losses from the deadweight loss the transferless instrument imposes, as in equation (2):

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[18] To the extent that the increased threat of collection discourages avoidance or evasion efforts prior to assessment, collection may affect income shifting and compliance expenses. Our discussion in this paragraph focuses on measurement of the direct social gains from collections, not these kinds of "upstream" effects.

[19] The net welfare effects of a transfer may depend on the relative social welfare weights assigned to transferor and transferee. For example, Galle (2013) discusses informally the importance of the relative weights on transferors and transferees in comparing alternative Pigouvian instruments. Kaplow (2008) argues, however, that transfers should always be considered to be welfare-neutral because any unwanted redistribution can be undone by, or desirable distribution accomplished through, income tax schedules.

W_1 - W_2 > c_n - c_m (2)

which corresponds to Kuchumova's equation (26), but where we have simplified to set W_1 - W_2 as the net social gain caused by transfers to low potential earners from high earners, and c_n - c_m as the incremental compliance costs of the nonmonetary instrument over the monetary instrument. This is a threshold that the non-monetary policy must clear in addition to equation (1). That is, if both non-monetary and monetary sanctions would satisfy cost-benefit analysis, the non-monetary sanction is only optimal if the incremental transfers achievable through the non-monetary instrument exceed its additional deadweight loss. We expect c_n - c_m to be hard to estimate because it will often require comparison to a hypothetical monetary instrument.

This same model can extend to disclosure if it is the case that the evader's taste for being perceived as tax compliant is also valued more highly by those with higher true earnings. DT report, for example, that larger firms in their sample were more responsive to treatment. We test this hypothesis with respect to individuals.

A second possibility is that public disclosure is an efficient component of a "tax systems" approach to compliance (Slemrod and Gillitzer 2013). In this account, taxpayers have a menu of options for minimizing the household tax burden. Government responses may affect each of the taxpayers' margins differently, resulting in varying elasticities of taxpayer response to each government strategy. This results in a Ramsey-type model in which government should employ a variety of enforcement techniques, each weighted inversely to the elasticity of taxpayer response.

Keen and Slemrod (2017) extend the tax systems approach, showing that a similar inverse-elasticity rule holds in the presence of transfers. Extrapolating from their parts 4.1 and 4.2, we can say that:

$$E(z,\alpha_k) = (\alpha_k((1-\mu_k)\, c_{\alpha_k} / \phi) + \alpha_k a_{\alpha_k}) / tz \quad (3)$$

where $E(z,\alpha_k)$ is the elasticity of revenue with respect to the enforcement instrument α_k, $\alpha_k a_{\alpha_k}$ is the cost of administering that instrument, and $\alpha_k((1-\mu_k)\, c_{\alpha_k} / \phi)$ is the net cost of compliance, discounted by the proportion $1-\mu_k$ (with $0 \leq \mu_k \leq 1$) that represents the share of costs that are other than pure transfers. As above, ϕ is the marginal value of public spending and tz is revenue. That is, for any given instrument, the government should invest in

enforcement to the point at which the marginal after-transfer cost-to-revenue ratio is equal to the enforcement elasticity of tax revenue.

Equation (3) suggests that non-monetary instruments can be optimal, but usually only if they are highly effective in returning revenue. A non-monetary instrument is likely to be optimal only when the elasticity of revenue with respect to enforcement is relatively high, as the top term on the right-hand side will be relatively large. In order for this to be the case, the non-monetary sanction would presumably have to affect different margins of response than the monetary sanction, or have a larger elasticity of response per unit of expenditure; otherwise, increasing the monetary sanction would strictly dominate (again, ignoring welfare weights). As Galle and Mungan (2021) show in a Pigouvian setting, it is usually optimal to exhaust monetary sanctions before employing non-monetary options, but non-monetary options can still be optimal when taxpayers are heterogeneous in their sensitivity to sanctions.

Taken together, then, this second set of prior studies suggests that disclosure could potentially offer an efficient tool for tax collection. Households with high ability to shield wealth from collections are unlikely to respond to a threat of fines that would be uncollectable. If these taxpayers were not sanctioned, they would have an *ex ante* incentive to shift to uncollectable sources of income. But households may find it more difficult to escape disclosure than a fine, reducing their propensity to earn uncollectable income.

In addition to these direct effects on tax debtors, disclosure systems in particular may affect taxpayer morale. By highlighting state efforts to ensure that everyone pays their fair share, disclosure may contribute to a sense that tax systems are fair, and thereby encourage voluntary compliance (Blank 2014; see Tyler 1999 for evidence that perceptions of systemic fairness affect compliance generally, and Hartner, et al. 2011 and Murphy 2003 for evidence in the tax context). Disclosure may also backfire, however, if it instead spotlights wrongdoing or otherwise causes observers to update their beliefs about compliance downwards (Luttmer and Singhal 2014).

In short, although non-monetary sanctions pose tradeoffs, they also offer a path to a more efficient tax system. Households with the ability to conceal collectable wealth may be relatively insensitive to additional fines or fees, but still relatively sensitive to disclosure or

a lost license. These instruments may therefore both bring in assessed revenues as well as deter behaviors that would prevent the revenues from being assessed in the first instance.

B2. Application of the evaluation framework

To apply the frameworks just set out, we use our estimates in Sections 4 and 5 to pin down values for equation (1), our measure of the social welfare effects of the program, relative to a baseline in which there is no alternative enforcement policy. For easy reference, we repeat equation (1) here in words:

$$\phi(Revenues - Admin\ Costs) - Private\ Compliance\ Costs + Foregone\ Avoidance\ Costs > 0$$

Our estimates suggest the California Top 500 program brings in meaningful amounts of revenue. Our preferred point estimate for the incremental two-year payments of first-time treated households is about $7,200, and there are an average of 400 such households per year. A simple back-of-the-envelope calculation thus suggests the program results directly in at least $2.8 million annually in its steady state. We argue, though, that this figure is too low, because it fails to account for higher-balance households, where most of the outstanding debt is. Although our estimates for these households are not as well-controlled, it is likely that any selection that occurs actually depresses our estimate. When we use estimates for all eligible households with a balance above $100,000, the back-of-the-envelope revenue figure is $7.2 million.[20] In addition, as we describe more fully in Appendix E below, we estimate the pre-letter treatment brings in at least an additional $3.2 million annually, for a total of $10.4 million.

An additional benefit from inducing taxpayer compliance is that taxpayers do not incur deadweight-loss costs of avoiding collection. As discussed, we argue that in a rational-actor sufficient statistics framework, increased payments of $7.2 million in this setting imply as much as $7.2 million in foregone private avoidance.

[20] We emphasize that this figure represents only the estimated mean incremental payments caused by treatment among individuals receiving the official letter in the two years after receipt, and so ignores payments by the thousands of other recipients of the pre-letter, as well as omitting payments by business entities. Total annual revenues brought in by the Top 500 unit are at least an order of magnitude larger, although the exact figure is not public information.

On the cost side, it is not easy to fully separate the costs of the program from general collection costs, but direct estimated administrative costs total somewhere between $1.5 and $2 million per year. This reflects the estimated direct cost of administering both the personal income tax list and the corporate income tax list; the personal income tax cases typically represent about 80% of the total, so the relevant cost for this study is in the range of $1.2 to $1.6 million. To ensure a conservative evaluation of the program, we will use the $1.6 million estimate.

Estimating private compliance costs is a bit more subtle. Non-compliers who are unwilling or unable to pay are posted to the list, and our evidence suggests that at least the complier portion of the population perceives publication to be subjectively costly. Individuals can be removed from the list simply by entering into an installment agreement, which is a commitment to make modest ongoing payments, and do not have to pay off anything like their full balance amount. Assuming, then, that non-compliers have the ability to pay an outstanding balance but choose not to do so, their revealed subjective cost of publication is zero.

We cannot draw this inference for individuals who lack the ability to pay. Although we lack direct evidence of the subjective cost of disclosure for those who cannot pay, there is no reason to think it differs from the distribution in the general population (Goldin and Reck 2020). At maximum, it should be no higher on average than the average disutility among compliers—higher, that is, than among households that reveal themselves to have the greatest subjective costs of publication. We therefore can put a plausible upper bound on the disutility from publication experienced by non-compliers who lack ability to pay. Among compliers, the observable cost of avoiding publication is the amount of incremental payments these households make over our two-year window, approximately $46,000 in the full-sample results. Over the ten observed cycles, we count an average of 161 initial non-compliers per cycle, defined as households that are published for the first time.

The question, then, is what share of these 161 non-compliers lack ability to make any payment. Recall that California does not permit publication of any taxpayer experiencing financial hardship, which FTB defines as net assets that are less than necessary to "provide for the [taxpayer's] health and welfare" or the "reasonable costs … of the [taxpayer's] trade or business." (California Code of Regulations § 19195-2). If this screen is imperfect, or if

taxpayers must bear some subjective cost to qualify for the exclusion, then there may still be some taxpayers whose payments following the official letter do not fully measure their subjective cost of publication and license suspension, because the household lacks ability to pay.

We suggest a conservative measure of taxpayers who may face this situation would be the share of taxpayers whose reported AGI falls below $40,000, which is the median we observe. As described in 4.6, we find evidence that reported income below this level reduces compliance. Since this is a reported value, some households in this group may have greater actual income. But taking reported AGI at face value, the implication is that about half of non-compliers who are published may lack ability to pay, and may experience subjective costs of publication of as much as $46,000. If there are 161 of these per year (half of the 161 per cycle), this yields an estimated ceiling value of $7.4 million in deadweight loss.

To reiterate, when drawing on our full sample results we estimate additional revenue of $10.4m, administrative costs of $1.6m, private compliance costs of $7.4m in disutility of publication and $10.4m in payments (a total of $17.8m), and foregone avoidance costs of $7.2m.[21] This results in net social welfare from the program of approximately:

$$\phi(\$10.4m - \$1.6m) - \$17.8m + \$7.2m = (\phi)\$8.8m - \$10.6m$$

If we assume that the marginal value of public revenue is equivalent to the tax cost of replacement, and use common estimates of the (national) MCPF of 1.5 (Heckman et al. 2010; Cellini et al. 2010 also estimate the marginal value of public expenditures in California at 1.5), we estimate total annual welfare gains of about $2.6 million, ignoring any possible distributive weights. Setting aside the pre-letter, total net welfare of the official letter alone is about $1 million (or a bit higher, given that the bulk of the administrative costs we estimate are connected to the much larger pre-letter program). Again, this estimate relies on an assumption that the actors we observe are rational and on the margin between compliance

[21] Arguably, our calculation of private compliance costs should omit the cost of payments, but we include them to present a conservative estimate. Typically, measures of the efficiency of the tax system will already account for the private costs of payment (Dahlby 2008). If we are estimating the incremental welfare effects of a particular tax collection method, accounting for the costs of payment would thus be double-counting. If payments are omitted from the private compliance costs, our estimate of net social welfare, excluding the pre-letter, increases to $8.2m.

and non-compliance. The ultimate value could be higher (lower) if a larger (smaller) share of non-compliers are infra-marginal as compared to compliers.

We note that netting incremental revenue collection against these private costs likely considerably understates the benefit of the Top 500 program, because we cannot directly measure its upstream impacts on taxpayer behavior. For instance, we do not observe the extent to which the program may contribute to taxpayer morale.

Our results also shed some light on the second possible evaluation framework, namely, whether the Top 500 program is more efficient than other alternative interventions. Testing equation (2), derived from Kuchumova 2018, requires us to make assumptions about the efficacy of a counter-factual regime that relied solely on elevated fines and fees. Given that the treated households we observe had already avoided paying hundreds of thousands of dollars in debts, we think it is reasonable to assume additional fines and fees would have had limited effect. If so, equation (2) is likely satisfied. We can take $\phi(Revenues - Admin\ Costs)$ as the weighted social value of added revenue derived from tagging. And the deadweight loss of disclosure is a fair estimate for c_n - c_m, the incremental compliance costs of the Top 500 program. This again gives us an estimate in the range of $2.6m.

We also can make some observations about equation (3), which requires that the weighted cost-revenue ratio exceed the elasticity of income with respect to the enforcement tool. Plugging our point estimates into the right-hand side of the equation yields:

((($7.4m - $7.2m) / 1.5) + $1.6m) / $10.4m ≈ 0.167

This calculation reflects the net of compliance costs ($7.4m in deadweight loss, none of which is transferred to others, less $7.2m in foregone avoidance expenses), plus the conservatively high estimate of $1.6m in administrative costs, over our revenue point estimate of $7.2m.

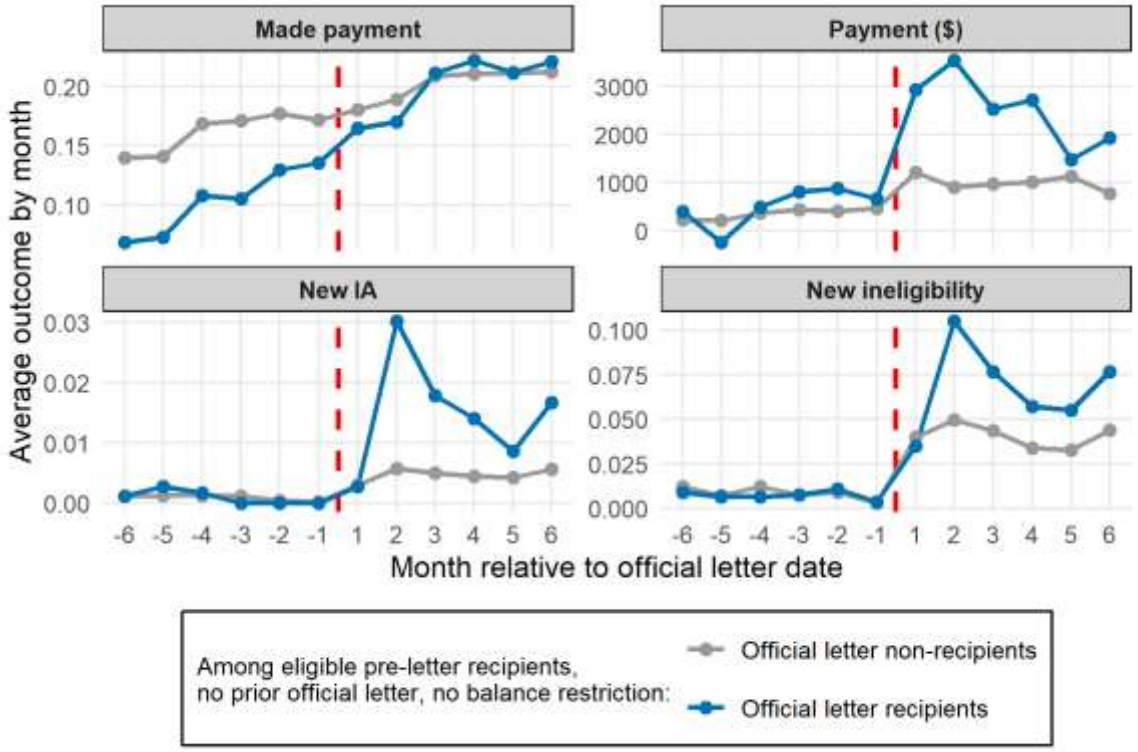
We cannot fully evaluate this result without knowing the opportunity set available to FTB. In general, though, a cost-revenue ratio this small implies that the Top 500 program would be an efficient option even if its elasticity of revenue with respect to enforcement were very low.

Additionally, non-monetary sanctions produce welfare gains if they affect a population that would be able to escape taxation if it were enforced only through a fine. We find a large

relative increase in payments by households with business income, suggesting that the implementation of a disclosure regime helps to reduce the net tax-avoidance payoff to self-employment. To be sure, this mechanism is imperfect: a fairly large fraction of delinquent households did not file any recent return, and non-filers are relatively less likely to respond to treatment. While this could reflect relatively high subjective costs of compliance, it may also reflect evasion behavior that is not curbed by disclosure.

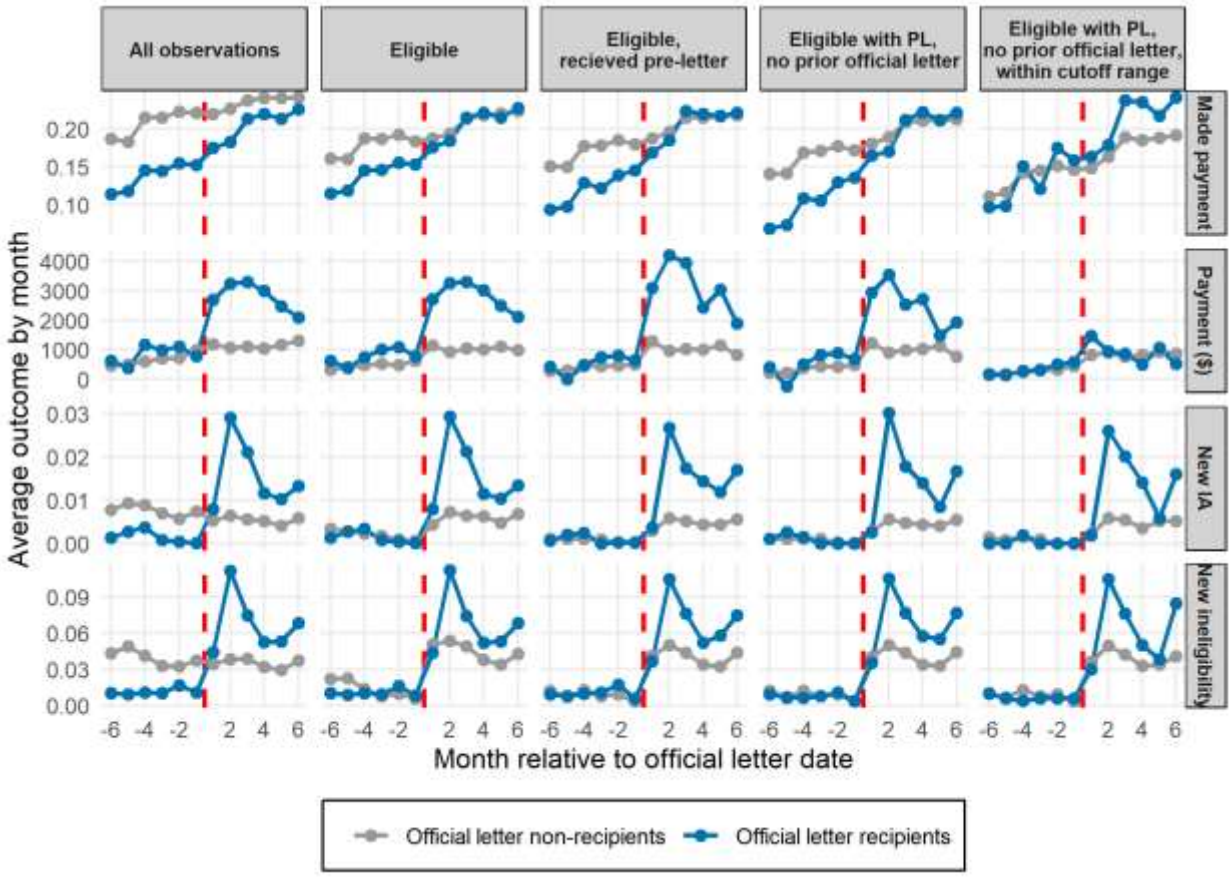
Appendix C: Additional Figures and Tables

Figure 17: Behavior before and after the official letter, recipients vs. non-recipients (including the full range of balances)



Notes: This figure compares the behavior of two groups of taxpayers around the date of the official letter. In gray are taxpayers who are eligible for publication and received a pre-letter, but did not receive an official letter. In blue are similar individuals (eligible pre-letter recipients) who did receive an official letter. In this figure we exclude individuals who previously received an official letter (i.e., we focus on first-time recipients), and we do not restrict to only those within the cutoff range.

Figure 18: Behavior before and after the official letter, testing importance of data filters



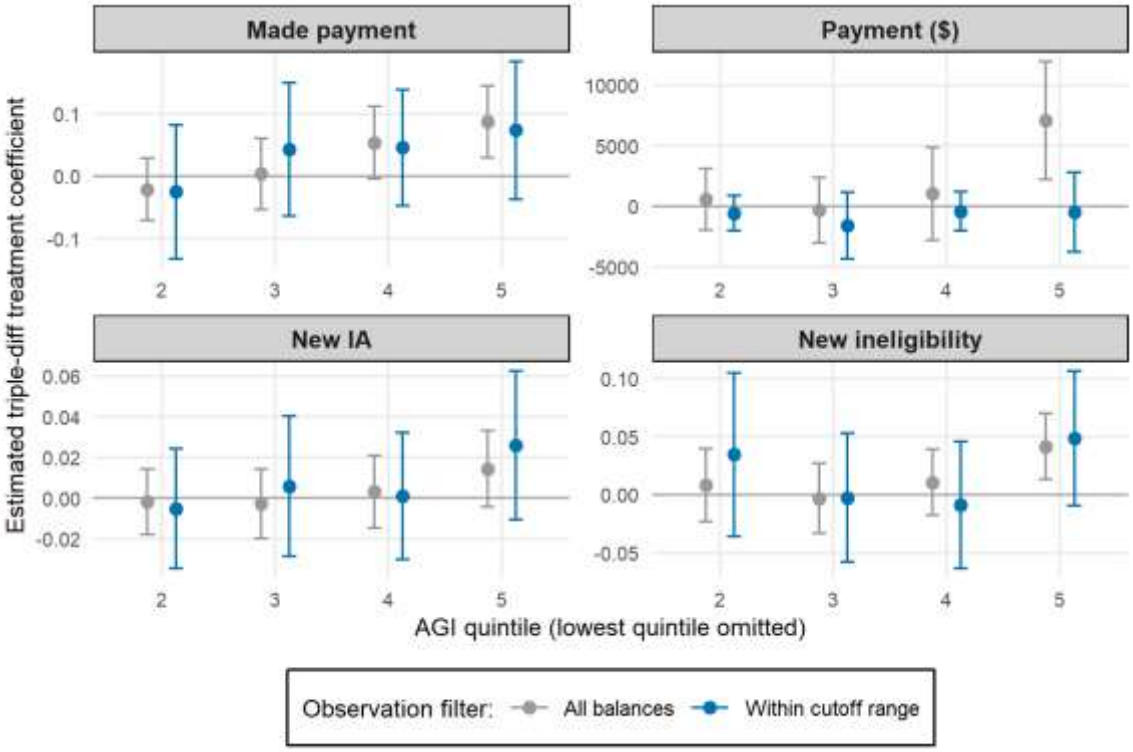
Notes: This figure compares the behavior of two groups of taxpayers around the date of the official letter, when applying various data filters. Going from left to right, the first column includes all observations in the dataset; the second restricts to those eligible for publication (based on their most recent status codes); the third further restricts to those receiving a pre-letter; the fourth further restricts to those who received no prior official letter; and the fifth adds the final restriction that balance fall between $150,000 and $230,000, the range of cutoff values inside of which treatment is quasi-random.

Table 5: Official letter event study results

	Dependent variables:			
	Made payment	Payment amount ($)	New IA	New ineligibility
Treatment dummies for each month relative to official letter date				
-6	-0.0622***	-422.6142***	0.0000	0.0040
	(0.0147)	(80.7791)	(0.0003)	(0.0045)
-5	-0.0602***	-437.5042***	0.0000	0.0000
	(0.0146)	(65.0554)	(0.0003)	(0.0035)
-4	-0.0080	-301.9792***	0.0020	-0.0020
	(0.0169)	(92.2779)	(0.0020)	(0.0029)
-3	-0.0382**	-253.4375	0.0000	0.0000
	(0.0157)	(251.9457)	(0.0003)	(0.0036)
-2	0.0161	-79.1456	0.0000	0.0000
	(0.0179)	(207.8576)	(0.0003)	(0.0035)
-1		*Excluded*		
1	0.0040	883.4737	0.0020	0.0241***
	(0.0175)	(620.2508)	(0.0020)	(0.0077)
2	0.0201	369.1572	0.0261***	0.0984***
	(0.0182)	(381.6091)	(0.0071)	(0.0137)
3	0.0783***	284.9205	0.0201***	0.0703***
	(0.0201)	(368.0232)	(0.0063)	(0.0119)
4	0.0763***	-78.6157	0.0141***	0.0442***
	(0.0199)	(163.1266)	(0.0053)	(0.0098)
5	0.0582***	487.2466	0.006*	0.0321***
	(0.0195)	(464.5481)	(0.0035)	(0.0086)
6	0.0823***	-56.2538	0.0161***	0.0783***
	(0.0201)	(109.2293)	(0.0056)	(0.0125)
Balance	-0.0037***	10.9097***	0.0026***	0.0167***
($ thousands)	(0.0002)	(1.4382)	(0.0000)	(0.0000)
April publication	-0.0175***	-90.8046	0.002***	0.0189***
	(0.0038)	(56.5107)	(0.0004)	(0.0010)
Intercept	0.1925***	414.2260	0.008***	0.0475***
	(0.0425)	(259.2547)	(0.0018)	(0.0048)
Observations	75,696	75,696	75,696	75,696
R2	0.0021	0.0003	0.0026	0.0044
Mean dep var.	0.1576	569.4367	0.0031	0.0246

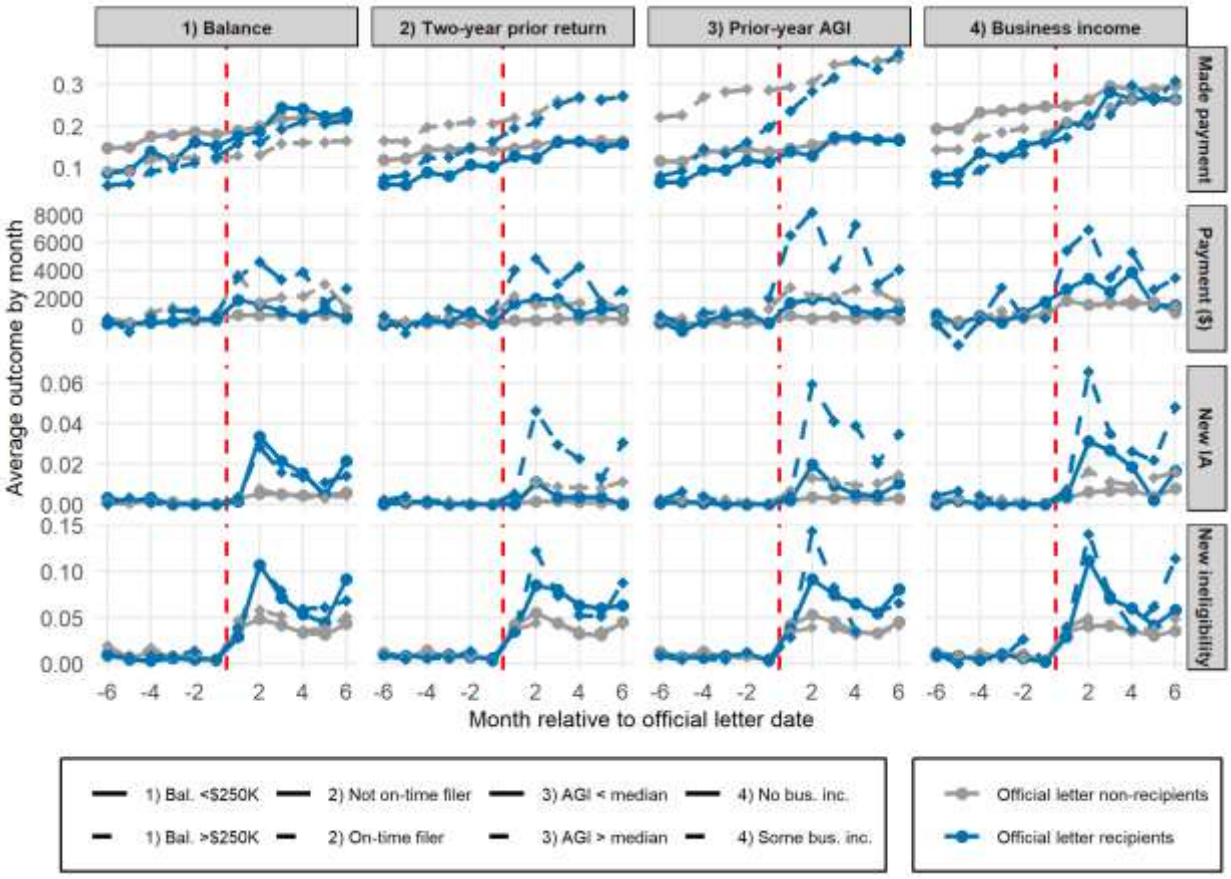
Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by taxpayer are reported in parentheses. Months are defined relative to the official letter date (month t+1 is defined as beginning on the letter date and ends 29 days later; month t-1 includes the 30 days prior to the letter date).

Figure 19: Estimates of treatment by AGI quintiles



Notes: This figure presents the coefficient estimates for separate triple difference models testing AGI quintiles (i.e., the estimate for the coefficient on treatment X post X quintile). The lowest quintile is omitted. 95% confidence intervals are shown around the point estimates. In blue are the estimates using only observations within the cutoff range. These can be compared to the estimates in gray, from models using the full range of balance observations.

Figure 20: Behavior around official letter, splitting on tax characteristics



Notes: This figure compares the behavior of a two x four matrix of taxpayers around the date of the official letter. In gray are taxpayers who are eligible for publication and received a pre-letter, but did not receive an official letter. In blue are similar individuals (eligible pre-letter recipients) who did receive an official letter. In this figure we exclude individuals who previously received an official letter (i.e., we focus on first-time recipients). For each of four outcomes on the Y axis there are four groups on the X axis, each divided into two sub-groups. Dashed lines represent one of the sub-groups, solid lines the other.

Table 6: Official letter triple difference results, heterogeneity tests (within cutoff range)

	Dependent variables:			
	Made payment	Payment amount ($)	New IA	New ineligibility
On-time filer	**0.0074**	**-125.51**	**0.017*** **	**0.0353*** **
(1/0)	(0.0230)	600.10	(0.0061)	(0.0130)
Among those with filed prior-year returns:				
Has business income	**0.0134**	**1864.61****	**0.0187**	**0.0262**
(1/0)	(0.0382)	914.62	(0.0122)	(0.0202)
AGI	**0.0151**	**834.08**	**0.0066****	**0.0109**
($ millions)	(0.0217)	1260.68	(0.0030)	(0.0154)
Above median AGI	**0.0693****	**-759.18**	**0.0224****	**0.0281***
(1/0)	(0.0304)	875.86	(0.0098)	(0.0150)
Non-negative AGI	**-0.0016**	**-725.57**	**0.0018**	**-0.0079**
(1/0)	(0.0380)	1100.72	(0.0151)	(0.0270)

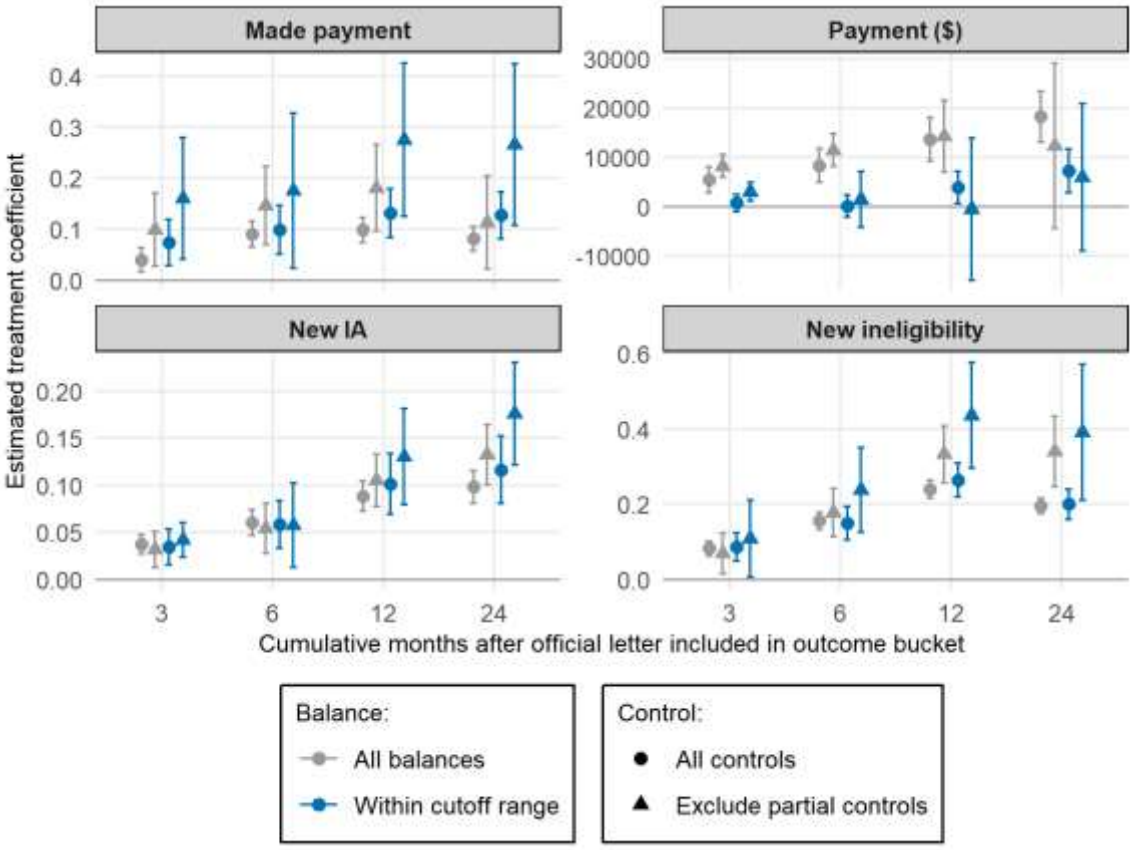
Notes: This table summarize regression results for regressions in which the outcome variable appears in the column headers across the top. Reported coefficients are for a triple-interaction term between post, treatment, and the variable listed in the row labels in the leftmost column. Only those with balance within the range of cutoffs (roughly $150,000 to $230,000) are included. An "on-time filer" is one who filed a tax return the year prior to receiving treatment. AGI = annual gross income; the median in our sample is roughly $40,000. Standard errors clustered by taxpayer. *p<0.1; **: p<.05; ***: p <.01.

Table 7: Official letter triple difference results, heterogeneity tests (full sample)

	Dependent variables:			
	Made payment	Payment amount ($)	New IA	New ineligibility
On-time filer	**0.0130**	**852.36**	**0.0157*** **	**0.0168** **
(1/0)	(0.0122)	940.00	(0.0033)	(0.0070)
Among those with filed prior-year returns:				
Has business income	**-0.0204**	**2410.45***	**0.0099**	**0.0027**
(1/0)	(0.0202)	1432.27	(0.0063)	(0.0103)
AGI	**0.0055**	**772.23**	**0.0012**	**0.0033**
($ millions)	(0.0048)	495.89	(0.0022)	(0.0042)
Above median AGI	**0.0606*** **	**3053.57** **	**0.018*** **	**0.0266*** **
(1/0)	(0.0168)	1318.25	(0.0052)	(0.0083)
Non-negative AGI	**0.0320**	**3508.82** **	**0.0034**	**0.0216***
(1/0)	(0.0232)	1752.72	(0.0078)	(0.0121)

Notes: This table summarize regression results for regressions in which the outcome variable appears in the column headers across the top. Reported coefficients are for a triple-interaction term between post, treatment, and the variable listed in the row labels in the leftmost column. An "on-time filer" is one who filed a tax return the year prior to receiving treatment. AGI = annual gross income; the median in our sample is roughly $40,000. Standard errors clustered by taxpayer. *p<0.1; **: p<.05; ***: p <.01.

Figure 21: Official letter long-term effect estimates (comparing control approaches)



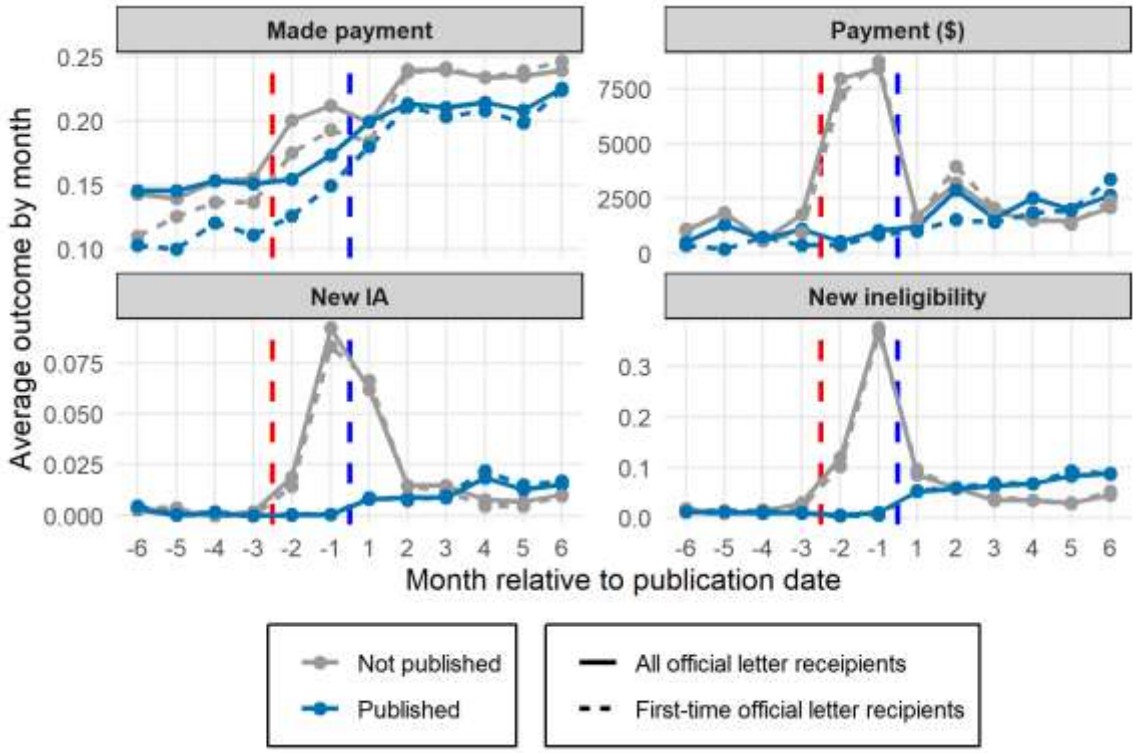
Notes: This figure summarizes estimated coefficients for regressions in which the variable in the grey bar is the outcome, cumulatively defined over the number of months post-official letter noted on the X-axis. Blue markers are for regressions in which we restrict the sample to observations with balances falling in between the historic lowest and highest Top 500 cutoff balances. Estimates shown with circles are the same as those presented above in Figure 9; estimates shown with triangles exclude those deemed "partial controls" (individuals in the control group who receive a letter during the subsequent two years).

Table 8: Official letter long-term payment effects (full range of observations)

	Dependent variables: Cumulative payment amount post-official-letter ($)			
	3 months	6 months	12 months	24 months
Official letter	**5388.73*** (1,318.31)**	**8332.44*** (1,746.36)**	**13608.8*** (2,234.60)**	**18283.3*** (2,628.68)**
Balance ($ thousands)	1.13 (0.74)	1.85 (1.15)	2.61* (1.50)	2.63* (1.59)
April publication	-2048*** (510.48)	-2487.13*** (684.55)	-2406.15*** (740.21)	-2034.9** (831.59)
Intercept	3869.5*** (405.42)	6838.05*** (562.87)	10802.19*** (703.61)	15299*** (835.56)
Observations	21,074	21,074	21,074	21,074
R2	0.0039	0.0049	0.0070	0.0075
Mean dep var.	3,626	6,835	11,519	16,620

Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by taxpayer are shown in parentheses. Outcomes measured as cumulative payments between time of treatment and time following, as listed in column headers. The full range of observations are included.

Figure 22: Behavior around the publication date, among official letter recipients



Notes: This figure compares the publication time series when including all official letter recipients (solid lines) and just first-time recipients (dashed lines). The patterns are similar.

Appendix D: Robustness Analyses

In this section, we test whether the findings discussed above are sensitive to the assumptions and data filters that define our main specification. Beginning with the pooled diff-in-diff approach, we test the sensitivity of the results to the following changes:

- Removing the requirement for same-cycle pre-letter receipt;
- Expanding the ineligibility definition to include any ineligible status during the prior month;
- Removing the eligibility filter entirely;
- Dropping any non-recipients above the cutoff value;
- Including individuals who received official letters for prior cycles;
- Narrowing the range of included values to those within the 2nd highest and lowest cycle cutoff values
- Not controlling for balance
- Including relative month as a control
- Considering different pre/post time windows

Table 9: Official letter difference-in-difference, data filter robustness checks

	Dependent variables:			
	Made payment	Payment amount ($)	New IA	New ineligibility
Main specification	**0.0223***	**155.77**	**0.0119*****	**0.0283*****
	(0.0118)	305.11	(0.0032)	(0.0066)
Robustness to data filters:				
Pre-letter: Don't require same-cycle pre-letter	0.0262**	105.67	0.0111***	0.03***
	(0.0105)	251.18	(0.0031)	(0.0062)
Eligibility: Exclude those with any past-month ineligible status change	0.0251	-98.75	0.0079**	0.0159**
	(0.0153)	316.79	(0.0036)	(0.0075)
Eligibility: Don't apply any eligible status filter	0.0161	221.16	0.0181***	0.0611***
	(0.0117)	303.78	(0.0033)	(0.0067)
Eligibility: Drop any non-recipients above that-cycle cutoff value	0.0216*	75.15	0.0119***	0.0279***
	(0.0119)	311.62	(0.0033)	(0.0066)
Prior Top500 experience: Don't restrict to first-time recipients	0.0165	319.36	0.0119***	0.0273***
	(0.0116)	315.10	(0.0030)	(0.0063)
Balance range: Use narrower range of included balances	0.0193	21.75	0.0138***	0.0307***
	(0.0135)	321.73	(0.0038)	(0.0075)
Balance control: Don't control for balance due	0.0223*	155.77	0.0119***	0.0283***
	(0.0118)	305.11	(0.0032)	(0.0066)
Time trend control: Include relative month covariate	-0.0479*	585.08	-0.0037	-0.0164
	(0.0271)	1010.72	(0.0057)	(0.0158)

Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by taxpayer are shown in parentheses. This table reports the coefficient on the treat*post term from each alternative specification of the difference-in-difference approach. See Table 2 and the accompanying text above for a discussion of the full model.

Table 10: Official letter difference-in-difference, time window robustness checks

	Dependent variables:			
	Made payment	Payment amount ($)	New IA	New ineligibility
Main specification	**0.0223***	**155.77**	**0.0119*****	**0.0283*****
	(0.0118)	305.11	(0.0032)	(0.0066)
Robustness to observation window:				
Restrict to one month pre/post	0.0021	516.19	-0.0001	-0.0090
	(0.0170)	742.71	(0.0021)	(0.0088)
Restrict to two months pre/post	-0.0027	190.21	0.0101***	0.0246***
	(0.0135)	442.42	(0.0038)	(0.0084)
Expand to four months pre/post	0.0273**	28.56	0.0115***	0.0277***
	(0.0116)	233.87	(0.0028)	(0.0055)
Expand to five months pre/post	0.0312***	58.24	0.0095***	0.0232***
	(0.0112)	211.83	(0.0023)	(0.0046)
Expand to six months pre/post	0.0367***	-6.27	0.01***	0.0267***
	(0.0111)	181.18	(0.0021)	(0.0043)

Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by taxpayer are shown in parentheses. This table reports the coefficient on the treat*post term from each alternative specification of the difference-in-difference approach. See Table 2 and the accompanying text above for a discussion of the full model.

Appendix E: Pre-letter analysis

This Appendix reports preliminary results on the effect of the pre-letter, which is the first step in the Top 500 process. The pre-letter is a notification sent to many taxpayers with balances above $100,000 that a taxpayer *may* have their information published if they are (1) eligible for publication and (2) among the Top 500 balances when the list is compiled, several months later. We begin by comparing the behavior of pre-letter recipients and non-recipients over time, before and after the pre-letter. We then use a regression discontinuity approach, taking advantage of the cutoff at $100,000, to study the effect of pre-letter receipt for those just above the cutoff.

E1. Graphical analysis

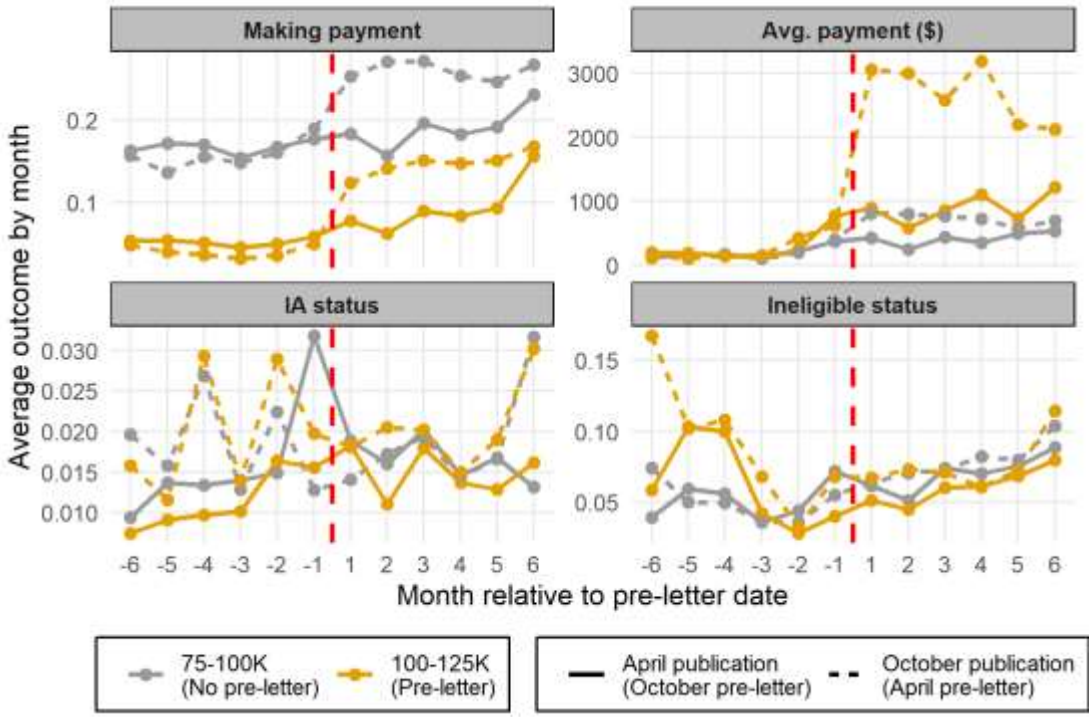
Similar to the graphical analysis above for the official letter effect, here we focus our attention on the taxpayers who are eligible for receiving a pre-letter and who have not yet received one (that is, for taxpayers who remain eligible for a pre-letter for multiple cycles,

only their first observation is included). This allows us to present a visual test of the effect on a taxpayer of encountering, for the first time, the pre-letter informing the taxpayer about the existence of the Top 500 list and that they will be notified in a few months if they are among the top 500 publishable balances at that time.

Figure 23 below compares two groups: a control group (those in eligible statuses but with balances as of the pre-letter date between $75 and $100 thousand, who do not receive a pre-letter); and a treatment group (all those in eligible statuses with balances above $100 thousand and below $125 thousand, who receive a pre-letter). The groups are further split into those observations that are part of an April Top 500 cycle (corresponding to October pre-letter mailing dates), or an October Top 500 cycle (April pre-letter mailing dates). As above, we compare these groups on four behaviors: making a payment, average payment amount, status changes indicating an Installment Agreement, and status changes making one ineligible for Top 500 publication.

This high-level comparison of treated and untreated taxpayers (i.e., pre-letter recipients and non-recipients) suggests that the pre-letter does not have a noticeable effect on the probability of taxpayers who are near the cutoff region starting an Installment Agreement or otherwise entering an ineligible status. However, there may be a small response in payments, with average payment amounts increasing more for treated vs. untreated taxpayers. The large spike for April pre-letters is likely related to tax filing deadlines; the bump up for October pre-letters may be easier to tie to the pre-letter itself.

Figure 23: Pre-letter outcome time series



Notes: This figure compares the behavior of two groups of taxpayers around the date of the pre-letter. In gray are taxpayers below the threshold, with $75-100 thousand in balance due, but who otherwise would be eligible for pre-letter receipt. In orange are similar individuals above the threshold, who receive the pre-letter. Observations are further split by whether they are part of an April Top 500 cycle (with pre-letters sent in October) or an October Top 500 cycle (with pre-letters sent in April).

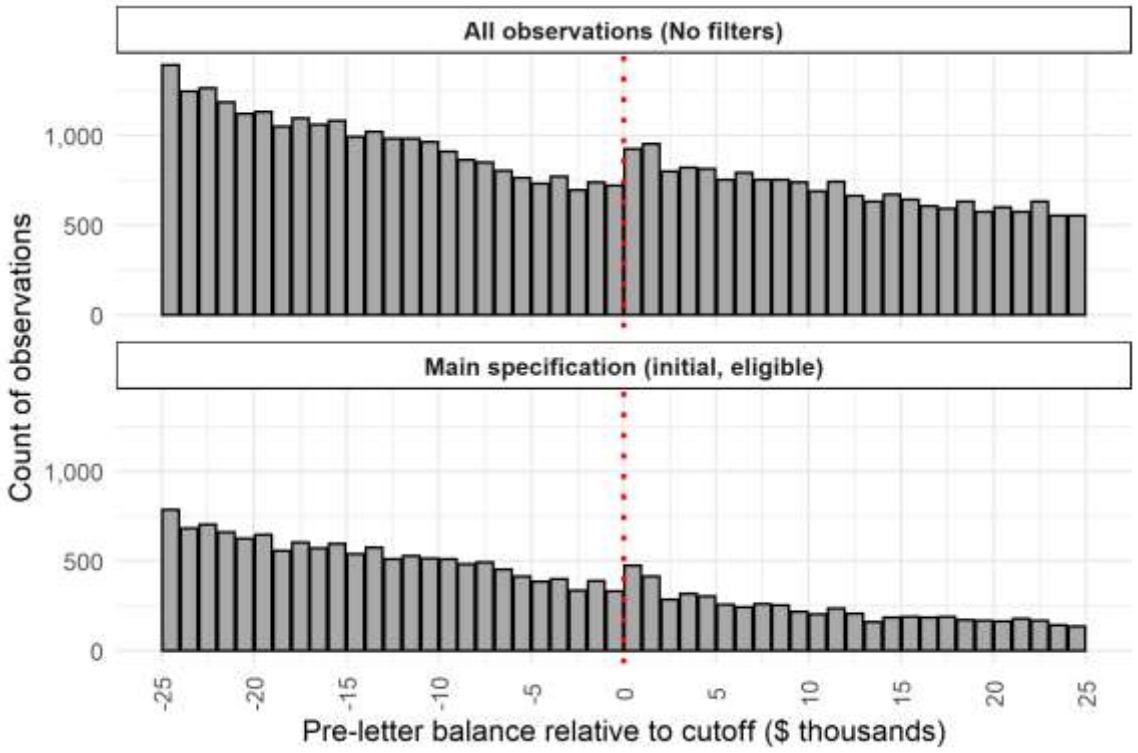
E2. Regression discontinuity analysis

To quantify the effect of pre-letter receipt, we implement a differences in discontinuities design using the $100,000 cutoff for pre-letter eligibility. That is, for our four main outcomes, we compare the extent of any discontinuous jump at a $100,000 balance before and after the date of the letter.

A standard test for the validity of an RD design is that non-outcome data should be distributed smoothly across the discontinuity. Figure 24 plots histograms of the count of taxpayer observations in pre-letter eligible statuses around the $100,000 cutoff. At present, our data includes some evidence of either bunching above the cutoff or missing mass below the cutoff, when including all observations for each taxpayer. However, when we filter to exclude any observations after a taxpayer's first pre-letter receipt, most of that bunching goes away. Taxpayers can of course exercise some control over their balance, and thus whether

they fall above or below the cutoff. However, since there is no practical consequence of receiving the pre-letter, and FTB does not publicize the existence of the $100,000 cutoff, we argue that assignment is effectively random. Because the filter on initial observations removes most of the bunching pattern, we use only the initial pre-letter observations in the following regression discontinuity analyses.

Figure 24: Histograms of taxpayers in pre-letter eligible statuses around the $100K pre-letter cutoff

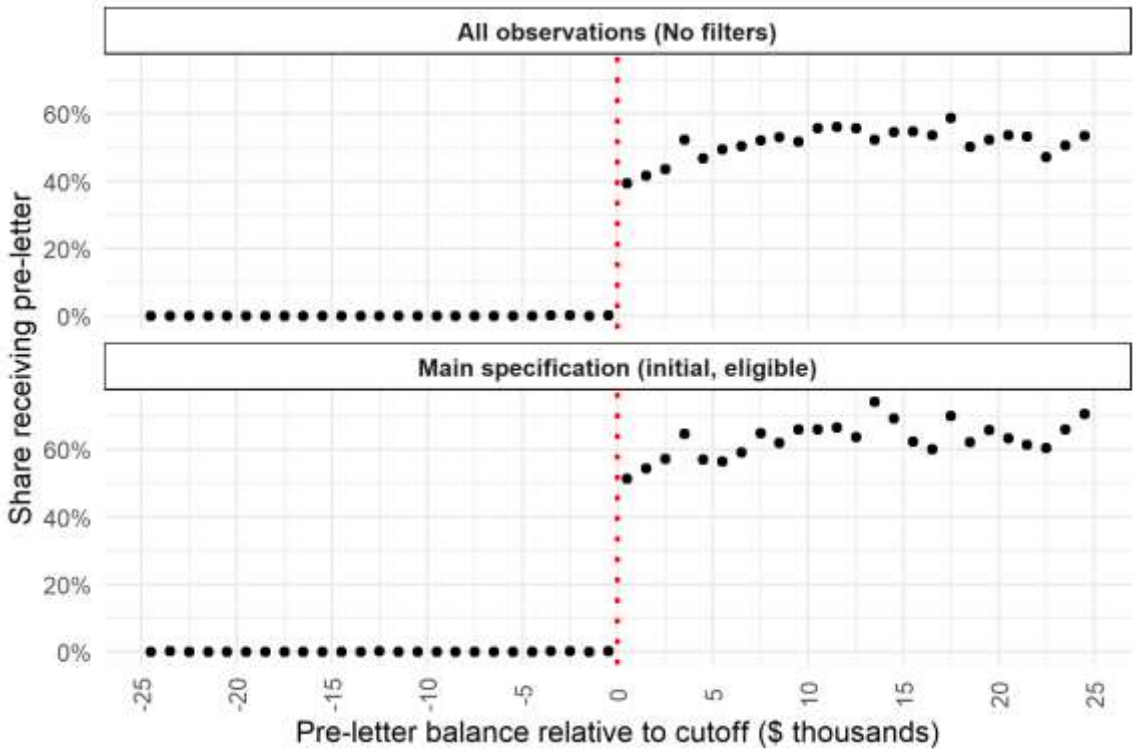


Notes: This figure plots the count of observations in each $1,000 balance bucket around the pre-letter cutoff at $100,000. The top panel shows the count among all observations; the bottom panel shows the count among the filtered observations in our main specification (those eligible for publication who have not previously received a pre-letter).

Applying our eligibility filter in addition to limiting to first-time appearances further improves the correspondence between the $100,000 threshold and observable treatment. As Figure 25 illustrates, there are no households with balances below $100,000 receiving a pre-letter, but only about half of households with balances above that amount are coded as receiving one. When we apply our filter for households legally eligible to receive a letter, this share rises to sixty percent. We are not able to ascertain why the remaining forty percent of

households are not coded as receiving a letter; these observations already exclude households with "bad address" code so it is not a mail delivery problem.

Figure 25: Pre-letter receipt at the threshold



Notes: This figure plots the share of observations in each $1,000 balance bucket around the pre-letter cutoff that receives a pre-letter. The top panel shows the share among all observations; the bottom panel shows the share among the filtered observations in our main specification (those eligible for publication who have not previously received a pre-letter).

While the data are imperfect, to get a sense of the likely magnitude of any pre-letter effect, we estimate a difference-in-discontinuity analysis using the $100,000 threshold as our discontinuity and the date of pre-letter as the treatment. We limit observations to the band of balances within $25,000 of the discontinuity. In equation terms, we estimate:

$$Outcome_{itc} = \alpha + \beta_1 \cdot Post_{itc} + \beta_2 \cdot D_{ic} + \beta_3 \cdot Post_{itc} * D_{ic} + \gamma_1 Below_{ic} * R_{ic} + \gamma_2 Above_{ic} * R_{ic} + \delta \cdot April_c + \varepsilon_{itc}$$

where D is an indicator for balances above $100,000 and R is the linear distance in the running variable, balance. Balance amounts, R, and discontinuity indicators are not indexed

for time because they are defined based on the taxpayer's balance at the time of the pre-letter, and so do not vary within a given cycle.

This framework suggests that any effects of the pre-letter on individual taxpayers are small, though potentially meaningful in the aggregate. Our point estimates for all three binary outcomes are close to zero with confidence intervals relatively tightly bunched around zero. For example, we can rule out with 95% confidence any positive impact on the likelihood of making a payment of more than 2.3%. Above-cutoff households are actually less likely to enter into any ineligible status after the pre-letter than those below, though this effect is tiny, with a 95% confidence interval bottoming out at -1.5%.

We do find a very modest individual increase in average total payments, however, which could result in economically meaningful remittances when summed across all households. Our point estimate is a $179 bump in monthly payments, with a 95% confidence interval ranging from just above $0 to $357. That implies that, of their $100,000+ in debt, the average taxpayer makes good on an additional $537 in the three months between the pre-letter and the official letter. But when multiplied by the roughly 3,000 individuals who receive the pre-letter each cycle, that amount translates to $3,222,000 in incremental annual revenues.

Table 11: Pre-letter diff-in-disc results testing effect of pre-letter receipt

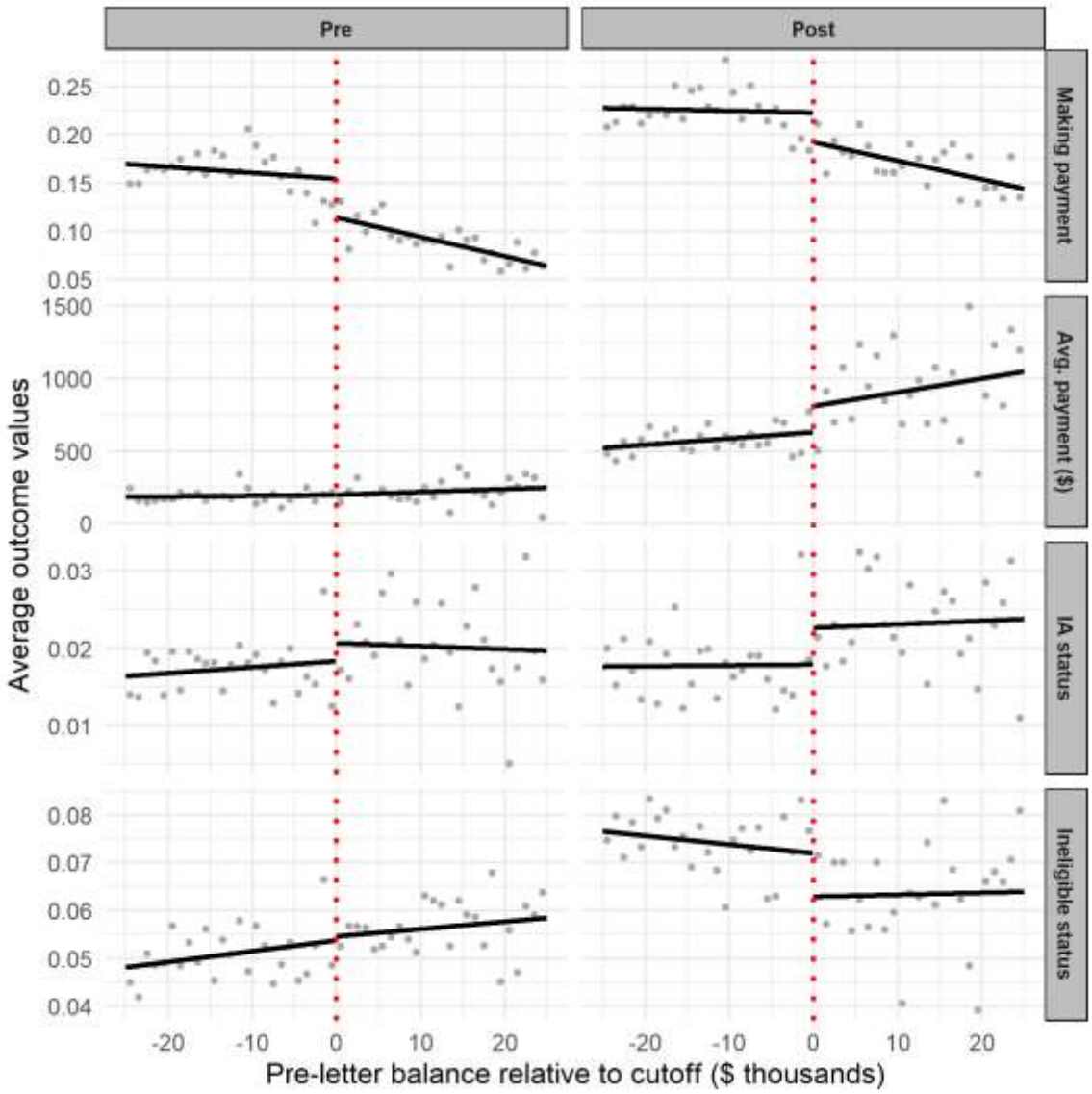
	Dependent variables:			
	Made payment	Payment amount ($)	IA status	Ineligible status
Above * Post	**0.0095**	**178.9906****	**0.0024**	**-0.0099****
	(0.0069)	**(89.9753)**	**(0.0024)**	**(0.0048)**
Above	-0.0395***	3.2943	0.0024	0.0009
	(0.0110)	(40.1620)	(0.0027)	(0.0036)
Post	0.0684***	433.1851***	-0.0004	0.0182***
	(0.0042)	(47.9352)	(0.0014)	(0.0030)
Observations	228,504	228,504	228,504	228,504
R2	0.0156	0.0038	0.0005	0.0020
Mean dep var.	0.1756	433.6749	0.0188	0.0617

Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by taxpayer are reported in parentheses. All regressions include linear controls for balance relative to

the cutoff, separately above/below the cutoff and pre/post pre-letter date. Also included are a control for April vs. October cycles, and an intercept term.

Figure 26 illustrates these results graphically. We present two standard RD plots for each outcome: one (on the left) for the period before the pre-letter, and a second (on the right) for the period after. Notably, the slope of the relationship between outstanding balance and average payment tilts upwards after treatment, suggesting that effects may be slightly larger for higher-balance taxpayers in a way that is not captured by the RD design, which is limited to the fraction of the distribution very close to the discontinuity.

Figure 26: Pre-letter difference-in-discontinuities



Notes: This figure shows the average outcome values within each $1000 balance bucket, calculated separately over the six months before (pre) and after (post) the pre-letter date, in gray. In black are the linear fits estimated in the regression, separately for pre/post and above/below the pre-letter cutoff of $100,000.

E3. Robustness checks

For the most part, the pre-letter results are robust to alternative specifications, as summarized in Table 12. We repeat the analysis using different bandwidths around the discontinuity; when omitting our controls for tax-filing season; and when omitting our filter for legal eligibility. None of these change the null results. Payment amounts are fairly similar in all the alternative specifications, except that when we narrow the window to just the $10,000 band around $100,000, the point estimate for payments drops close to zero with a larger error term. As we mentioned above, we may be missing some observations just below the $100,000 balance, and this may contribute to the instability of the results when we focus most closely on that region.

Table 12: Pre-letter diff-in-disc, robustness checks

	Dependent variables:			
	Made payment	Payment amount ($)	IA status	Ineligible status
Main specification	0.0095	178.9906**	0.0024	-0.0099**
25K bandwidth, eligible	(0.00695)	(89.9753)	(0.0024)	(0.0048)
Robust: 10K bandwidth	0.0093	21.7721	-0.0012	-0.0114
	(0.0110)	(133.0803)	(0.0037)	(0.0076)
Robust: 20K bandwidth	0.0061	234.0725**	0.0019	-0.0084
	(0.0078)	(98.1059)	(0.0026)	(0.0053)
Robust: No April control	0.0095	178.9906**	0.0024	-0.0099**
	(0.0069)	(89.9751)	(0.0024)	(0.0048)
Robust: 25K bandwidth	0.0015	117.8104**	0.0028*	-0.0058*
No eligibility filter	(0.0048)	(54.7849)	(0.0015)	(0.0030)

Notes: This table reports the estimated coefficients on the Above * Post covariate, under various alternative specifications of the diff-in-disc approach. See above for the main specification and corresponding discussion.

In sum, we generally confirm our expectation that the individual-level impact of the pre-letter is extremely modest. Unlike the official letter, which notifies individuals that they will be subject to sanctions with certainty if they do not comply, the pre-letter simply repeats probabilistic information that was likely already conveyed to taxpayers by FTB collection personnel. In the aggregate, though, the pre-letter does seem to add value to the Top 500 program, as it may bring in upwards of $3 million.