Heard it through the Grapevine: The Direct and Network Effects of a Tax Enforcement Field Experiment on Firms[☆]

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Abstract

Tax enforcement may have deterrent effects that extend beyond directly treated taxpayers, but evidence of such deterrent effects for major sources of revenue is limited. This paper studies the effects of a large-scale field experiment on employer deposits that make up most U.S. tax collections. In-person visits by Revenue Officers have a large direct effect on visited firms' tax deposits. The other clients of visited firms' tax preparers also deposit more tax, a network effect that suggests preparers disseminate information. Aggregating over all links, this network effect accounts for 1.2 times as much revenue as the direct effect. Letters conveying the same message are found to have much smaller direct effects and no measurable network effects.

Keywords: Tax Enforcement, Randomized Experiments, Networks
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1. Introduction

The effects of tax enforcement directed at one taxpayer are not limited to that taxpayer's behavior. Increased enforcement can deter evasion in the canonical Allingham and Sandmo (1972) model of tax evasion by changing other taxpayers' perceptions of the probability that evasion will be detected and punished. Deterrence may be general to all taxpayers or limited to those who receive information about the level of enforcement from the treated taxpayer through a shared network connection. Beginning with the audit threat letters discussed in Blumenthal et al. (2001) and Slemrod et al. (2001) and continuing with a large recent literature surveyed Hallsworth (2014) and Slemrod (forthcoming), field experiments in cooperation with tax authorities have provided substantial insights into the effects of feasible tax enforcement initiatives. The literature on tax enforcement still contains understudied issues and important gaps, which this paper begins to fill using results from a large-scale field experiment done in partnership with the Internal Revenue Service (henceforth the IRS).

The first gap this paper addresses is a lack of attention to compliance and enforcement for collecting what we call employment taxes, which include payroll taxes and employee income taxes withheld and remitted by employers. The lack of attention to collecting employment taxes is surprising given that they bring in a large amount of revenue. In fiscal year 2017, US FICA[1] payroll tax revenue was $1.05 trillion and individual income tax withheld by employers was $1.33 trillion, comprising 31.6 percent and 38.9 percent of total collections, respectively, or together over 70 percent of taxes collected by the IRS[2].

This paper also provides evidence about how the effects of tax enforcement spill over to firms connected to treated taxpayers through networks. We examine the response of firms connected to treated taxpayers through several distinct, sometimes-overlapping networks. The network effects capture responses driven by financial ties or by information about enforcement spread by word-of-mouth. The spillover effects contribute to the total revenue impact of the enforcement initiatives. Understanding network effects could improve the cost-effectiveness of enforcement policy; for example, treating the most-connected taxpayers increases voluntary compliance in the agent-based model of Andrei et al. (2014), and degl'Innocenti and Rablen (2019) find large simulated revenue gains from using some network information to target enforcement. Taking a broader perspective, the network effects are a crucial link between the specific deterrence effects (i.e., on the treated taxpayers) of an enforcement initiative and the general deterrent effect of changing all actual and prospective evaders' perceptions of the likelihood that evasion will be detected and punished. While we are not the first to note the potential network effects of tax enforcement, existing field experiments focus primarily on geographic connections between households. One example is the study of the spillover effects on nearby households of in-person visits by Austrian TV tax inspectors in Rincke and Traxler (2011) and Drago et al. (2015), as opposed to the inter-firm links studied in this paper; neither the professional preparer nor parent-subsidiary links for

[1]FICA is the Federal Insurance Contributions Act, which covers contributions toward Social Security and Medicare. This number does not include other employment taxes: SECA, unemployment insurance, or railroad retirement.

[2]From Table 1 of the IRS Data Book 2017.

which we find evidence have previously been examined. In contrast, Meiselman (2018) finds no evidence that sending letters to Detroit city income tax non-filers leads their neighbors to file. Pomeranz (2015) is a notable exception to the focus on household ties, in which an experiment shows that an audit threat increases the VAT declarations of the treated firms' suppliers, but not treated firms' clients. This pattern is consistent with the incentives greater VAT enforcement provides for treated firms to insist that transactions with suppliers are reported, and for treated firms' suppliers to match reports with the treated firm, and is not informative about word-of-mouth diffusion of information in a payroll tax or income tax setting like the one we study[3].

Finally, this study contributes to the literature that examines to what extent the delivery mechanism of an enforcement intervention matters. Ortega and Scartascini (2018) show that in the context of Colombian taxes visits are more effective than emails, which are more effective than letters, and Ortega and Scartascini (2015) show that phone calls are more effective than letters, but this pattern has not been demonstrated for taxes in advanced economies.

We study both direct and network effects in a large-scale field experiment conducted in partnership with the IRS, in which 12,172 firms suspected of failure to remit all of the tax they owe, but not subject to any compliance intervention by the IRS, were assigned either to one of two treatment arms or to a control group. One treatment was an informational letter, while the other was a much more dramatic intervention, an in-person visit to the place of business by an IRS Revenue Officer.

We find that in-person visits have large, persistent direct effects on tax payments, while letters have small, fleeting direct effects. A visit from a Revenue Officer causes firms to remit an average of $3,686 in additional tax one quarter after the visit. This effect slowly diminishes to $1,652 four quarters after the visit. The visit also raises the probability of remitting any tax by 12.9 percentage points and log (tax remitted) by 13.2 log points one quarter after treatment. Receiving a letter does not cause firms to remit more tax on average, but it does increase the probability they remit any tax by three percentage points one quarter after treatment.

We also find evidence of network effects. Firms whose tax preparers' other clients receive an in-person visit eventually remit more tax, a network effect that lags the direct effect by three quarters. On average, firms in the experimental group share a tax preparer with 23 other firms. These 23 other firms each remit an average of an additional $243 four quarters after the visit, an effect that is highly statistically significant. This effect takes time to develop. Point effects on tax remitted in the first two quarters after treatment are $86 and $52 and statistically insignificant, while the point effect three quarters after treatment is $156 and is statistically significant at only the ten percent level. This phasing-in is consistent with an informational story, in which tax preparers pass information to their clients only during infrequent contacts. Taking into account the large number of linked firms, the aggregate tax preparer network effect summing over the four quarters following the visit is 1.2 times the

[3]Alstadster et al. (2018) study how information about a legal tax avoidance scheme diffuses. Perez-Truglia and Troiano (2018) study how the visibility of shaming affects the rate of payment of tax delinquencies.

direct effect.

The paper proceeds as follows. In Section 2 we describe the experimental setting and treatments. In Section 3 we present the direct effects of our two tax enforcement interventions, the in-person visit and the letter. In Section 4 we describe the network effects. In Section 5 we discuss the economic significance of the estimates. Section 6 presents a conceptual framework to think about the welfare effects of the interventions and the consequences for policy design, and Section 7 concludes.

2. Setting and Treatments

More than 6.5 million U.S. firms deposited federal income tax withheld from wages and salaries, federal unemployment insurance taxes, and FICA taxes between the fourth quarter of 2013 and the fourth quarter of 2014. Firms report these tax remittances using Form 941, "Employer's Quarterly Federal Tax Return". Most employers are required to make semi-weekly or monthly Federal Tax Deposits (FTDs) of these employment taxes.

The IRS uses an algorithm to identify and prioritize firms at risk of falling behind on their required deposits in each quarter. The IRS assigns at-risk firms into categories called FTD Alerts. For firms with high priority alerts (Alert A or B status), the IRS assigns a Revenue Officer to contact the firm within fifteen days of the alert's issuance. The experiment we study was carried out on a third group of firms, designated as having Alert C status. These are firms for which the algorithm indicates a higher risk of falling behind on their deposits than the general population, but not as high a risk as firms designated Alert A or B. In some quarters prior to the experiment, Alert C firms may have received a letter about their deposits. Some, but by no means all, firms receive the same FTD Alert designation for more than one consecutive quarter[4]. It is especially relevant from a tax enforcement policy standpoint to understand the behavior of Alert C firms, because these firms are at the margin of enforcement action from the IRS, and are therefore the most relevant population when considering whether to expand or contract the set of firms the IRS contacts.

This paper uses a randomized experiment to study the effects of sending letters to and visiting at-risk firms at the margin of enforcement action. There were 12,172 such firms assigned Alert C status by algorithm based on payments before and during the fourth quarter of 2014. These firms were randomly assigned to one of three groups. A control group received no FTD Alert-related contact. A second group received an informational letter[5] early in the first quarter of 2015. The letter notes that the firm's deposits have decreased, discusses the firm's deposit responsibility and potential penalties, and provides information and resources about federal tax deposits and their payment. The third group of firms received an initial

[4]Due to high turnover from quarter to quarter (e.g., only 28 percent of control group firms continue to have the Alert C designation after one quarter), we expect that a few of the firms randomly assigned in the experiment we study would have received an enforcement action prior to the experiment because of an earlier Alert status. Random assignment makes this fact unlikely to bias our results, although it is relevant when considering how our results generalize to other contexts.

[5]A copy of the letter is included in the online appendix. If a taxpayer has filed a form giving a representative power of attorney, the representative also receives a copy of any written correspondence.

in-person contact at the place of business from an IRS Revenue Officer[6]. Initial contact procedures emphasize providing the taxpayer with information about the collection process, discussing the taxpayer's deposit compliance status, and gathering basic information. In some cases, a Revenue Officer may use information from an initial contact to determine that further investigation or contact is warranted, following collection procedures.

Alert C firms show signs of noncompliance before treatment. As Table 1 shows, compared to the average firm filing a quarterly employment tax return, firms with Alert C status as of the fourth quarter of 2014 had more employees but remitted less tax[7] and were less likely to have remitted any tax. As expected due to randomization, the treatment groups are similar before treatment.

Table 1: Descriptive Statistics One Quarter Before Treatment

	Form 941	Alert C	Control	Letter	Visit
Tax Remitted	21,604	10,683	10,499	11,024	10,523
	[57,279]	[20,554]	[20,022]	[21,265]	[20,342]
Any Tax Remitted	0.686	0.570	0.570	0.573	0.567
	[0.464]	[0.495]	[0.495]	[0.495]	[0.496]
Employees	15.0	27.6	27.5	27.4	27.9
	[32.6]	[39.1]	[39.3]	[38.4]	[39.5]
Median Tax Remitted	2,650	2,846	2,841	2,793	2,899
Median Employees	4	14	14	14	14
Number of Firms	6,489,930	12,172	3,894	4,069	4,209

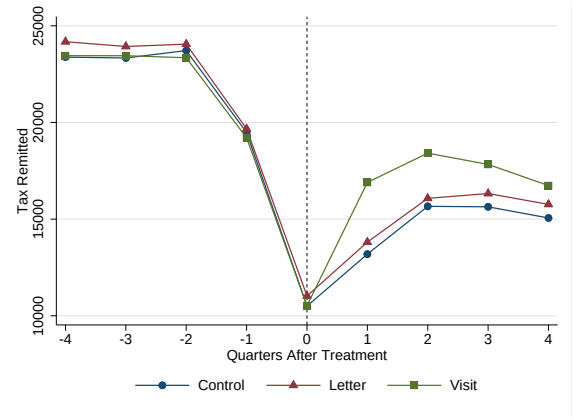
Notes: Means reported except where otherwise indicated. Sample standard deviations in brackets. Form 941 statistics are from a ten percent random sample of all firms filing Form 941 at any point in the prior year. Employees is the number of Forms W-2 filed in the calendar year before treatment. Tax remitted and employees are winsorized at the 98th percentile.

All three groups' tax remittances, probability of remitting any tax, and log (tax remitted), depicted in Figure 1 fall sharply over the four quarters prior to treatment. Control firms' remittances also rebound to an extent one quarter after treatment, a pattern analogous to the "Ashenfelter dip" discussed by Heckman and Smith (1999) in the context of labor market interventions, wherein those who qualify for job training often have temporarily depressed earnings that tend to revert upward toward their longer-term mean even absent treatment. Without an experimental control group, it would be difficult to construct a control group from observational data that would not underestimate the control group's rebound in compliance and thus tend to overestimate the effect of treatment.

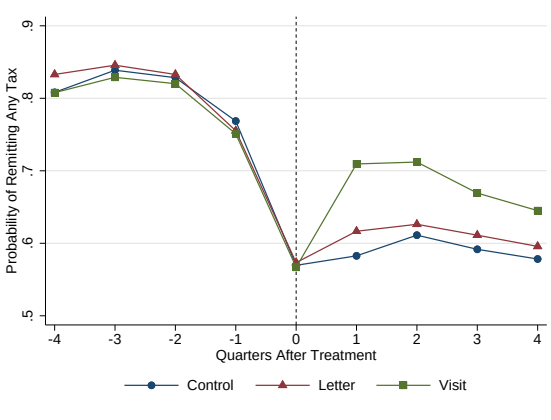
[6]IRS records indicate that Revenue Officers dedicated time to contacting nearly all assigned firms.
[7]Winsorized at the 98th percentile.

Figure 1: Outcome Means by Treatment Group

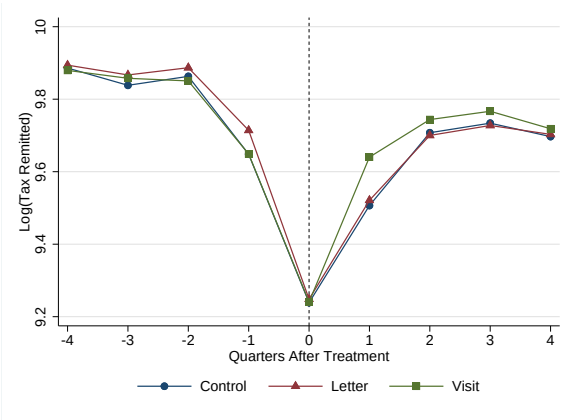
(a) Tax Remitted



(b) Any Tax Remitted



(c) Log(Tax Remitted)



Notes: Tax remitted winsorized at the 98th percentile. Log(tax remitted) excludes firms remitting no tax.

2.1. Follow-up Treatment

Recent work by Bhargava and Manoli (2015) and Guyton et al. (2017) has shown that many enforcement initiatives have short-lived effects on taxpayer behavior and that reminders, essentially follow-up rounds of treatment, can boost the persistence of the policy's effect. This inspired a novel (in the context of tax administration research) feature of the design of this experiment, drawing on practice in medicinewhere patients who are initially unresponsive to treatment may receive continued treatment[8].

At the end of the quarter during which treatment took place, the algorithm that determines whether firms are designated high risk (Alert C) ran again, and some of the 12,172 firms in the experiment were again designated high risk. Firms that were again designated high risk received a second dose of their assigned treatment in the following quarter. Thus, each firm assigned, for example, to the visit group received one visit early in Q1 2015 and, if the firm remained at high risk based on its payments through week twelve of Q1 2015, it received a second visit in the second quarter of 2015. The same procedure was followed with the letter treatment. After the second quarter, no firm received further experimental treatment, although some businesses in the experiment might have been assigned to very high risk (Alert A or B) status and thereby been subject to routine enforcement action. Table 2 presents a treatment timeline.

Table 2: Treatment Timeline

By December 31, 2014	Q4 2014 Alert C status determined by algorithm, treatment groups randomly assigned.
January 1-15, 2015	Treatment carried out.
By March 31, 2015	Q1 2015 Alert C status determined by algorithm.
April 1-15, 2015	Firms receive a follow-up round of their assigned treatment if they have both Q4 2014 and Q1 2015 Alert C status.

Turnover in high risk status, detailed in Table 3, is large only 28 percent of control group firms remained in this category one quarter after random assignment. Among firms assigned to receive a letter, 28 percent continued to have high risk status in the following quarter and received a second letter. Among firms assigned to receive a visit, just 19 percent-about one-third less–continued to have high risk status in the next quarter and therefore received a second visit. The lower fraction of firms assigned to receive a visit continuing in high risk status is consistent with the result, detailed below, that the visit increased remittances.

This follow-up treatment allows us to assess the effects of a realistic treatment protocol in which recalcitrant cases receive a follow-up intervention. If the treatment interventions we study were to become standard practice, follow-up treatment of unresponsive firms might well become tax administration procedure. We include firms regardless of follow-up treatment status, but the proper interpretation of our results includes the follow-up treatment

[8]See, for example, Zonder et al. (2003) on leukemia and Diehl et al. (2003) on treatment of refractory Hodgkin's lymphoma with a second course of high-dose chemotherapy.

Table 3: Status One Quarter After Treatment

	Alert A or B	Alert C	No Status
Visit (percent)	2	19	78
Letter (percent)	5	28	66
No Treatment (percent)	5	28	67

Notes: Alert A or B status reflects higher risk than firms in with Alert C status, and Alert C status reflects higher risk than the general population of Form 941 filers. All firms with Alert A or B status receive field contact as part of routine procedure. Firms that continued to have Alert C status one quarter after treatment received a follow-up dose of their initially assigned treatment. Firms with no status one quarter after treatment did not receive a follow-up dose of treatment. Source: Author calculations.

administered to firms whose remittance behavior continued to indicate high risk. Beginning two quarters after treatment, the estimated impacts capture both the persistent component of the initial treatment administered to all firms in the treatment group and the effect of the follow-up treatment administered one quarter later to a subset of treatment group firms.

3. Direct Effects

3.1. Event Study Regression Design

Our preferred specification uses an event-study regression design that reduces residual variance and allows for a flexible time path of the treatment response. This design rests on the assumptions that there are no contemporaneous changes that affect the treatment and control groups differentially, and that absent treatment the time paths of the outcome variables in the treated and control groups would evolve in a parallel fashion. In fact, there were no contemporaneous IRS policy changes that might affect the treatment groups differentially. Figure 1 illustrates that the trends in the outcome variables we study are similar across treatment groups for several quarters prior to treatment, which supports the assumption that these trends would continue to be parallel absent the experiment. We estimate models of the form

$$Y_{it} = \sum_{j} \sum_{q} \beta_{jq} 1(T_i = j) 1(t = q) + \eta_t + e_{it}, \tag{1}$$

where Y_{it} denotes the outcome of interest, e.g. the log amount of employment tax that firm i remitted with Form 941 in quarter t, β_{jq} is the coefficient that indicates the direct effect of treatment j on the outcome q quarters after treatment, $1(T_i = j)$ is an indicator variable

equal to one if firm i received treatment j, $1(t = q)$ is an indicator equal to one if t is q quarters after treatment, η_t is a fixed effect for quarter t, and e_{it} is the regression error term. Standard errors are clustered at the firm level to account for possible serial correlation in the error term. We study how letters and visits affect tax remitted (in dollars, winsorized at the 98th percentile), the probability of remitting any employment tax, for which we use a linear probability model, and log (tax remitted), which omits firms that do not remit any tax.

3.2. Direct Effects Results

We find that in-person visits have large, lasting direct effects on tax payments. Figure 2 illustrates that the effect on tax remitted overall and on the probability of remitting any tax last four quarters after treatment. Table 4 shows the effect estimates. One quarter after a visit firms remit an additional \$3,686. Visited firms are 12.9 percentage points more likely to remit any tax one quarter after treatment; this effect is large relative to the 58 percent of control group firms that remitted any tax one quarter after treatment. This effect shrinks to 6.9 percentage points by four quarters after treatment. The effect on log (tax remitted) lasts only a single quarter. Although control firms' compliance does improve after treatment, which is consistent with mean reversion and the Ashenfelter dip, visited firms' compliance rebounds much more. Control firms rebounding suggests that observational studies comparing firms receiving a visit or letter to firms selected from the general population would likely overstate the effects of the compliance treatments, and further indicates the value of conducting randomized experiments.

Letters have much smaller, and fleeting, direct effects. Letters do not lead to substantially higher average tax payments or increases in log (tax remitted), as shown in Figure 2. Letters have an effect only on the probability that firms remit any tax one quarter after treatment, which rises by three percentage points. This effect is highly statistically significant, but does not persist beyond one quarter after initial treatment, suggesting that follow-up letters have little or no effect.

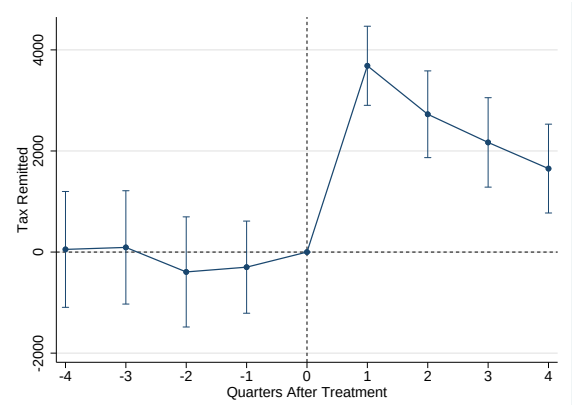
The causal effect of the initial visit beyond one quarter cannot be separated from the combined effect of the follow-up procedure in which continually non-compliant firms receive a second visit, but effects are largest one quarter after treatment, and a second letter appears to have no effect. Section 5 compares the estimated impact of these treatments to their cost to evaluate their impact on net revenue and assess them from a welfare economics perspective.

3.3. Direct Effects and Firm Size

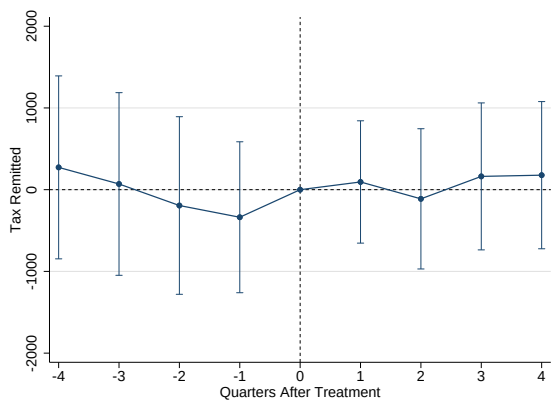
We next explore whether larger firms respond more to treatment using a triple-difference regression specification that compares the direct effect for the largest ten percent of firms to the direct effect for the smallest ten percent of firms. We define size to be the number of employees in the calendar year before treatment, as measured by Forms W-2 filed with the IRS. The largest ten percent of firms have at least 67 employees, while the smallest ten percent of firms have at most two employees.

Figure 2: Direct Effects

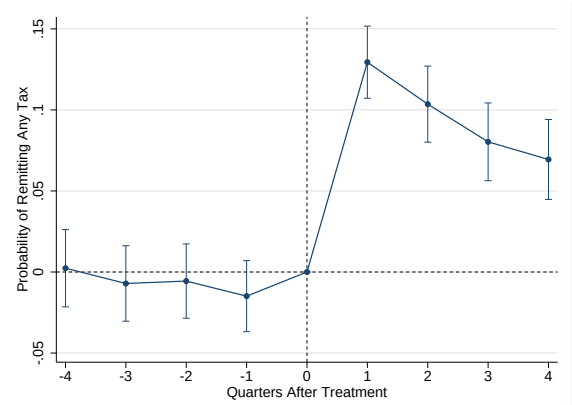
(a) Visit: Tax Remitted



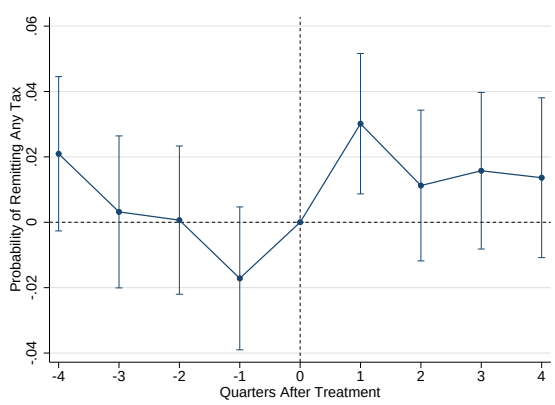
(b) Letter: Tax Remitted



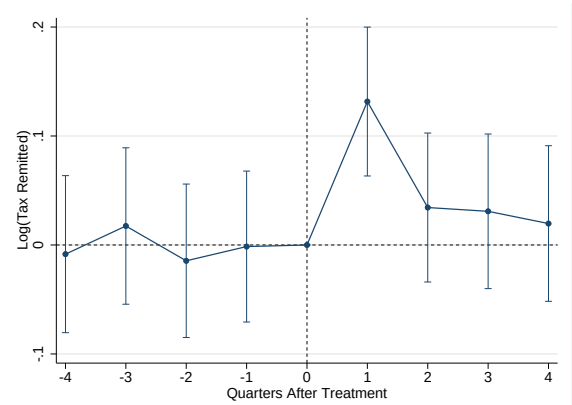
(c) Visit: Any Tax Remitted



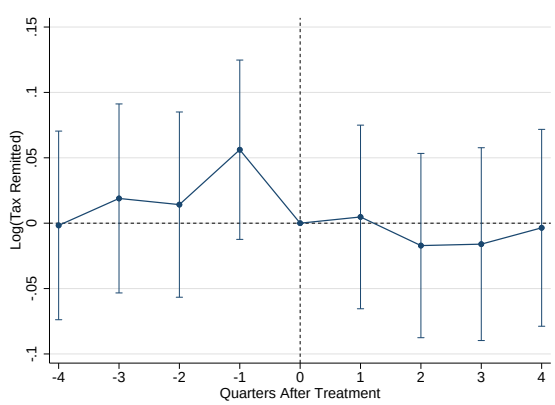
(d) Letter: Any Tax Remitted



(e) Visit: Log(Tax Remitted)



(f) Letter: Log(Tax Remitted)



Notes: figures plot estimates and 95 percent confidence intervals. Tax remitted winsorized at the 98th percentile. Log(tax remitted) excludes firms remitting no tax.

Table 4: Direct Effects

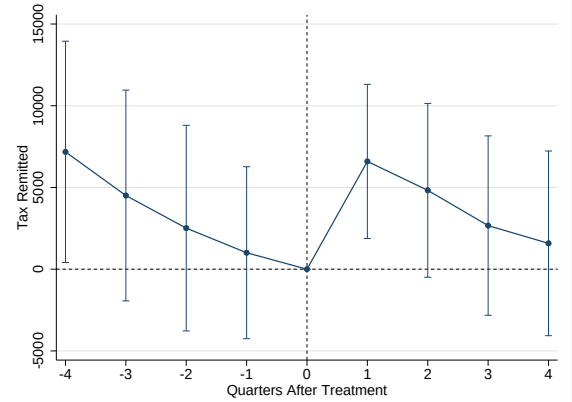
	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
Letter * One Quarter Post	94.4	0.0302***	0.00476
	(382)	(0.0110)	(0.0358)
Letter * Two Quarters Post	-112	0.0112	-0.0171
	(438)	(0.0118)	(0.0359)
Letter * Three Quarters Post	163	0.0158	-0.0160
	(459)	(0.0122)	(0.0376)
Letter * Four Quarters Post	177	0.0136	-0.00353
	(459)	(0.0125)	(0.0384)
Visit * One Quarter Post	3,686***	0.129***	0.132***
	(399)	(0.0113)	(0.0348)
Visit * Two Quarters Post	2,726***	0.104***	0.0344
	(438)	(0.0120)	(0.0349)
Visit * Three Quarters Post	2,169***	0.0803***	0.0309
	(451)	(0.0122)	(0.0362)
Visit * Four Quarters Post	1,652***	0.0694***	0.0197
	(448)	(0.0126)	(0.0364)
Quarter Fixed Effects	Yes	Yes	Yes
Number of Firm-Quarters	109,548	109,548	77,051
R-Squared	0.0281	0.0513	0.0220

Notes: Standard errors (in parentheses) clustered by firm. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Probability results from linear probability model.

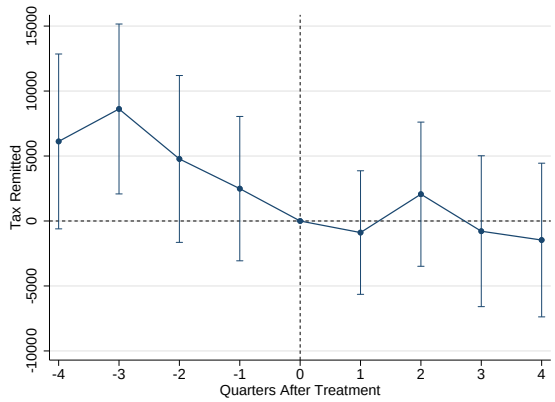
In dollar terms, the direct effect of a visit is much larger for firms in the top ten percent by pre-treatment size than for firms in the bottom ten percent. The direct effect of the visit on tax remitted one quarter after treatment is $6,595 dollars larger for the largest firms than for the smallest firms, as depicted in Figure 3, a difference that is highly statistically significant. The responses of the largest and smallest firms, summarized in Table 5 are otherwise similar, including for the letter. Holding the cost of contacting a firm constant, visiting larger firms uses the same resources to collect more additional revenue than visiting smaller firms.

Figure 3: Direct Effects: Top Ten Percent vs. Bottom Ten Percent by Size

(a) Visit: Tax Remitted



(b) Letter: Tax Remitted



Notes: figures plot estimates and 95 percent confidence intervals. Tax remitted is winsorized at the 98th percentile. Log(tax remitted) excludes firms remitting no tax. Size is the number of W-2 employees in the year before treatment. The largest ten percent of firms have at least 67 employees, while the smallest ten percent of firms have at most two employees.

4. Network Effects

The administrative data we use enable us to study the network deterrent effects of enforcement interventions, which operate through connections between untreated firms and firms directly receiving the enforcement intervention[9]. As discussed earlier, this analysis could provide insight about how information regarding enforcement actions diffuses to alter the generally perceived probability that tax evasion will be detected. Even if the per-linked-firm network effect is small, many linked firms per treated firm can still result in a substantial aggregate effect of network connections on total remittance behavior. Understanding the information network structure could also inform the design of information campaigns, as

[9]Network effects through connections between treated and untreated firms in the randomly assigned group violate the usual assumption in a randomized experiment that the untreated firms receive no treatment. This violation would tend to bias our estimates of the direct effects towards zero, but our direct effects estimates are unchanged when we control for network links.

Table 5: Direct Effects: Top vs. Bottom Ten Percent by Size

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
Letter * Top vs Bottom Ten Percent	-889	-0.0300	-0.0297
* One Quarter Post	(2,425)	(0.0483)	(0.175)
Letter * Top vs Bottom Ten Percent	2,063	0.00604	0.102
* Two Quarters Post	(2,829)	(0.0513)	(0.178)
Letter * Top vs Bottom Ten Percent	-786	-0.0486	-0.0462
* Three Quarters Post	(2,961)	(0.0526)	(0.181)
Letter * Top vs Bottom Ten Percent	-1,465	-0.0819	0.0386
* Four Quarters Post	(3,016)	(0.0526)	(0.187)
Visit * Top vs Bottom Ten Percent	6,595***	-0.0311	0.147
* One Quarter Post	(2,404)	(0.0491)	(0.169)
Visit * Top vs Bottom Ten Percent	4,826*	-0.0127	0.191
* Two Quarters Post	(2,712)	(0.0507)	(0.174)
Visit * Top vs Bottom Ten Percent	2,668	-0.0577	0.0624
* Three Quarters Post	(2,799)	(0.0519)	(0.172)
Visit * Top vs Bottom Ten Percent	1,582	-0.0586	0.190
* Four Quarters Post	(2,881)	(0.0527)	(0.173)
Quarter Fixed Effects	Yes	Yes	Yes
Number of Firm-Quarters	24,687	24,687	16,593
R-Squared	0.222	0.0852	0.221

Notes: Standard errors (in parentheses) clustered by firm. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Any tax remitted results from linear probability model. Firms in the bottom 10 percent by size have at most two W-2 employees in the year before treatment, while firms in the top ten percent by size have at least 67 such employees.

models show higher voluntary compliance results from providing information to or about taxpayers with the most links (Andrei et al. (2014), degl'Innocenti and Rablen (2019)). We investigate several types of network, some of which have been examined before (although usually with respect to households rather than firms), and some that have not been heretofore studied. Our rich data set allows us to examine certain links between firms that have not been rigorously studied before.

One connection between the firms in our sample and others is geographic. Geography ties together firms with addresses in the same ZIP Code or, at a more fine-grained level, a shared ZIP+4. The 42,000 five-digit ZIP Codes in the United States indicate a shared postal facility and are assigned to either geographic areas or post office boxes, while a ZIP+4 is a nine-digit designation for a small group of blocks or segment of a postal route (USPS, 2016). Firms in our experimental sample share a ZIP Code with an average of 659[10] other employers filing quarterly employment tax returns, and share a ZIP+4 with an average of just 3 other employers.

Firms also share tax preparers or tax preparation firms. Each individual tax preparer has a unique Preparer Tax Identification Number (PTIN), which that preparer includes on each return he or she prepares. If the preparer is part of a tax preparation firm, the firm's unique Employer Identification Number (EIN) is also included on each prepared return. These identifiers allow us to identify when two firms' returns are prepared by the same individual preparer or by preparers working at the same tax preparation firm. We consider two firms linked to a tax preparer or tax preparation firm if that tax preparer or tax preparation firm prepared at least one Form 941 for that firm in the four quarters prior to treatment; it is plausible that firms might have contact with a tax preparer or tax preparation firm they have used in the past year even if they are no longer using that preparer, especially if they are concerned about IRS enforcement action related to past filings. Each firm in our experimental sample shares a tax preparer with an average of 23 other employers and a tax preparation firm with an average of 98 other employers.

Network effects through shared tax preparers are of interest for two reasons beyond their implications for correctly estimating the revenue impact of enforcement initiatives. First, preparers may be an effective target for expanded information reporting or other enforcement treatments. Second, and related, the fact that the treatment spills over to other firms with the same tax preparer suggest that preparers play a role in firms' decision-making, an issue addressed by Klepper et al. (1991) and recently by Klassen et al. (2015), who analyze confidential data from the IRS to examine whether the party primarily responsible for a firm's tax compliance function-an external auditor or the internal tax department-is related to the firm's tax aggressiveness.

Finally, we investigate links between parent corporations and their subsidiaries. Parent/subsidiary relationships meet one of two sets of criteria in the year prior to treatment assignment. In the first case, the parent corporation files IRS Form 851, "Affiliations Sched-

[10]As some firms are linked to more than one Alert C firm, the sample of firms linked by ZIP code to Alert C firms is somewhat smaller than the number of links per firm times the size of the Alert C sample (536 linked firms instead of 659), and similarly for the other network channels we study.

ule," with a consolidated group annual tax return indicating that the parent owns stock with 80 percent or more of both the total value and voting power of the subsidiary directly or indirectly through other corporations in the consolidated group. In the second case, the parent corporation is a subchapter S corporation and has filed Form 8869, electing to treat a domestic corporation whose stock it wholly owns as a qualified subchapter S subsidiary which is deemed liquidated. This definition implies that firms have at most one parent and that parent firms cannot themselves have a parent, as parents in our sample are either the ultimate parent of a consolidated group or S corporations whose owners are required by law to be individual people. The business operations of the parent and subsidiary are presumably tightly linked, given the degree of ownership and filing of a consolidated annual tax return.

These three sets of networks capture a diverse range of relationships between firms. For example, the network effect per link to a firm visited by a Revenue Officer may be large for one channel but not others, and the network effect per link to a letter firm need not be large for that channel. One might expect that letters have network effects through ZIP and ZIP+4, as these links capture both geographic proximity and shared postal delivery, while visits might have especially strong effects through shared preparers or tax preparation firms, as the preparer or firm may interact directly with the Revenue Officer. Additionally, links to visited firms through a given channel, for example a shared preparer, may affect tax payments overall, only on the extensive margin captured by the indicator for remitting any tax, or only on the intensive margin captured by log (tax remitted).

4.1. Identifying Network Effects with Non-Random Selection into Network Linkages

We aim to identify the causal network deterrence effects of the letter and visit treatments. This causal effect captures the difference between a firm's compliance behavior if its network "neighbors" happen to receive a letter or visit and that firm's behavior if its network neighbors happen to receive no treatment. When estimating these effects, it is important to keep in mind that simply comparing the post-treatment behavior of firms with network neighbors that received a letter or visit to the post-treatment behavior of all firms without treated network neighbors would provide a biased estimate of the network effect. This is because having treated network neighbors requires having network neighbors with high-risk (Alert C) status, so that network links may very well not be random.

Firms with Alert C status are less likely than other employers to have remitted any Form 941-related tax, as Table 1 shows, and so it is natural to suppose that the network neighbors of firms with Alert C status might have systematically different remittance behavior compared to other firms' network neighbors. For example, if adverse local economic shocks make firms in a neighborhood less likely to remit tax payments, firms in that neighborhood are both more likely to have Alert C status themselves and more likely to be linked to firms with Alert C status. The resulting correlation between connections to treated firms and lower tax payments would bias network effects estimates downward. The same concern arises for links through preparer networks; some preparers may be more experienced, or more sympathetic, or condoning, towards at-risk businesses and thus develop clienteles of

such businesses. Parents and their subsidiaries are also likely to share similar compliance behavior.

To address the selection bias concern, we compare firms with the same number of Alert C neighbors. Consider the example of two firms, each sharing its own unique ZIP Code with exactly one Alert C firm in the experimental sample. Prior to random assignment, the likelihood of each firm sharing its ZIP Code with a firm that receives a visit is 1/3. Conditional on the number of links to Alert C firms, network treatment is randomly assigned and thus independent of firms' characteristics and potential compliance outcomes. Comparing firms with the same number of links to Alert C firms allows us to identify an unbiased causal effect of being linked to a treated firm, because before treatment the network treated and control groups are equally likely to have low tax payments on the basis of their similar connections to Alert C firms. The regression approach we implement is a generalized version of the event-study approach used above to study direct effects, where we pool firms with different numbers of links to Alert C firms to produce a single treatment estimate, but control for differential patterns of compliance over time between firms based on their total links to Alert C firms. This approach relies on the assumption that, conditional on the number of total links to Alert C firms, the trends in compliance would be parallel across firms linked to different treatment groups absent treatment. Specifically, separately for each network channel c we run regressions of the form:

$$Y_{it} = \sum_{j} \sum_{q} \rho_{cjq} L_{cij} 1(t = q) + \sum_{l} \theta_{clt} + e_{it}, \tag{2}$$

where Y_{it} is the outcome for firm i in quarter t, ρ_{cjq} is the network effect through channel c of treatment j, q quarters after treatment, L_{cij} is the number of links through network channel c that firm i has to firms that received treatment j, $1(t = q)$ is an indicator equal to one if t is q quarters after treatment, θ_{clt} is a fixed effect common to all firms connected through network channel c to a total of l treated and control firms in quarter t, and e_{it} is the regression error term. Note that, conditioning on a fixed value of the total number of links to Alert C firms, this specification is a standard event-study specification with quarter fixed effects. The specification pools the event-study specifications across different numbers of total links to Alert C firms and constrains the estimated network effect to be linear in the number of links to treated firms. We do this in separate specifications for firms sharing a preparer, preparer firm, ZIP Code, or ZIP+4 with an Alert C firm and for the subsidiaries and parents of Alert C firms. We cluster the standard errors at the level of the channel used in that specification, e.g. ZIP Code, preparer, or parent, which addresses correlation in the error term between firms sharing, e.g., a preparer or parent as well as serial correlation in the error term.

4.2. Tax Preparer Network Effects Results

We find evidence that tax enforcement interventions have network effects transmitted through a shared tax preparer several quarters after treatment, but not immediately. In person visits increase the tax remitted by visited firms' tax preparers' other clients by an

average of $156 three quarters after treatment and by $243 four quarters after treatment, as shown in Figure 4 and Table 6. The effect three quarters after treatment is statistically significant at the ten percent level, while the effect four quarters after treatment is statistically significant at the one percent level. We find that letters increase log (tax remitted) by letter recipients' tax preparers' other clients four quarters after treatment by 1.09 log points. This effect is statistically significant at the five percent level. The time delay between treatment and these tax preparer network effects is consistent with the low frequency with which most firms exchange information with their tax preparers.

Table 6: Preparer Network Effects

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
Preparer Links to Letter Firms	35.3	-0.00183	-0.00437
* One Quarter Post	(72.2)	(0.00152)	(0.00624)
Preparer Links to Letter Firms	27.0	-0.000140	-0.0000449
* Two Quarters Post	(88.6)	(0.00170)	(0.00428)
Preparer Links to Letter Firms	73.0	-0.00201	0.00264
* Three Quarters Post	(92.6)	(0.00216)	(0.00434)
Preparer Links to Letter Firms	146	-0.00111	0.0109**
* Four Quarters Post	(103)	(0.00238)	(0.00455)
Preparer Links to Visit Firms	85.5	0.00188	-0.00236
* One Quarter Post	(61.3)	(0.00122)	(0.00505)
Preparer Links to Visit Firms	52.3	0.000315	-0.00328
* Two Quarters Post	(81.9)	(0.00149)	(0.00353)
Preparer Links to Visit Firms	156*	0.00123	0.00112
* Three Quarters Post	(88.3)	(0.00189)	(0.00368)
Preparer Links to Visit Firms	243***	0.00162	0.000830
* Four Quarters Post	(94.1)	(0.00224)	(0.00357)
Quarter Fixed Effects	Yes	Yes	Yes
Quarter * Total Preparer Links to Alert C Fixed Effects	Yes	Yes	Yes
Number of Preparer Clusters	10,219	10,219	9,357
Number of Firm-Quarters	1,796,994	1,796,994	1,193,501
R-Squared	0.00361	0.00500	0.0120

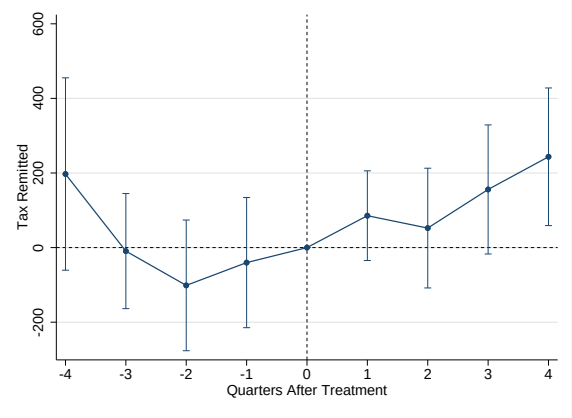
Notes: Standard errors (in parentheses) clustered by Preparer. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Any tax remitted results from linear probability model.

4.3. Tax Preparation Firm Network Effects Results

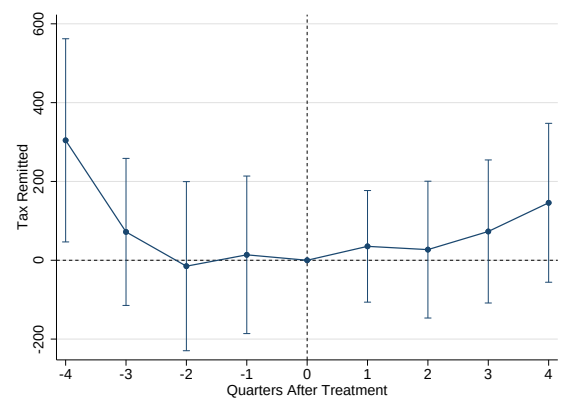
In contrast to the network effects of shared individual tax preparers, we do not find evidence of network effects through shared tax preparation firms. The results shown in

Figure 4: Tax Preparer Network Effects

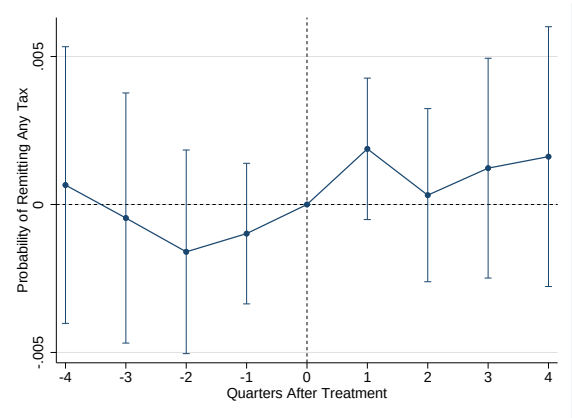
(a) Visit: Tax Remitted



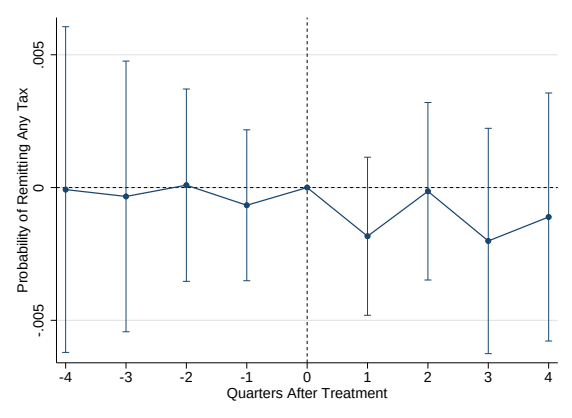
(b) Letter: Tax Remitted



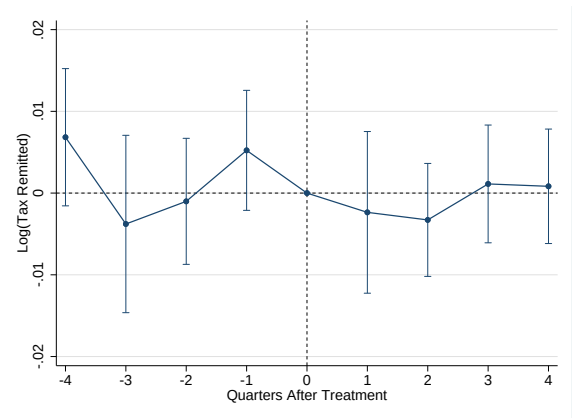
(c) Visit: Any Tax Remitted



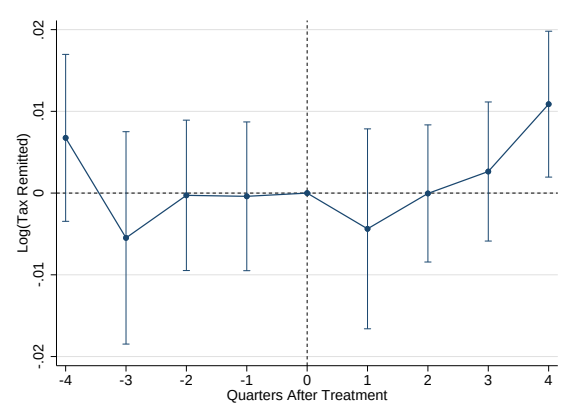
(d) Letter: Any Tax Remitted



(e) Visit: Log(Tax Remitted)



(f) Letter: Log(Tax Remitted)



Notes: figures plot estimates and 95 percent confidence intervals. Standard errors clustered by preparer. Tax remitted is winsorized at the 98th percentile. Log(tax remitted) excludes firms remitting no tax.

Figure 5 and in Table 7, are precisely estimated, ruling out large per-firm spillovers, and are not statistically significant at the five percent level. While Alert C firms on average share a tax preparer with 23 other firms, they share a tax preparation firm with an average of 659 other businesses, limiting the influence of a single contact with one of the preparation firms' clients on the firm's other clients.

Table 7: Preparation Firm Network Effects

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
Preparation Firm Links to Letter	-5.67	-0.00109	-0.000987
Firms * One Quarter Post	(103)	(0.00107)	(0.00380)
Preparation Firm Links to Letter	-116	-0.000442	-0.00361
Firms * Two Quarters Post	(102)	(0.00119)	(0.00317)
Preparation Firm Links to Letter	-67.5	-0.00115	0.000720
Firms * Three Quarters Post	(91.8)	(0.00141)	(0.00331)
Preparation Firm Links to Letter	-132*	-0.00149	0.000841
Firms * Four Quarters Post	(80.2)	(0.00168)	(0.00300)
Preparation Firm Links to Visit	2.47	-0.000349	-0.00154
Firms * One Quarter Post	(110)	(0.000943)	(0.00309)
Preparation Firm Links to Visit	-52.7	-0.000465	-0.00331
Firms * Two Quarters Post	(89.7)	(0.00117)	(0.00248)
Preparation Firm Links to Visit	65.9	0.000129	0.00189
Firms * Three Quarters Post	(79.9)	(0.00148)	(0.00225)
Preparation Firm Links to Visit	22.9	0.000206	0.00246
Firms * Four Quarters Post	(79.8)	(0.00185)	(0.00222)
Quarter Fixed Effects	Yes	Yes	Yes
Quarter * Total Preparation Firm Links to Alert C Fixed Effects	Yes	Yes	Yes
Number of Preparation Firm Clusters	9,759	9,759	9,053
Number of Firm-Quarters	3,563,361	3,563,361	2,468,149
R-Squared	0.00502	0.00620	0.0131

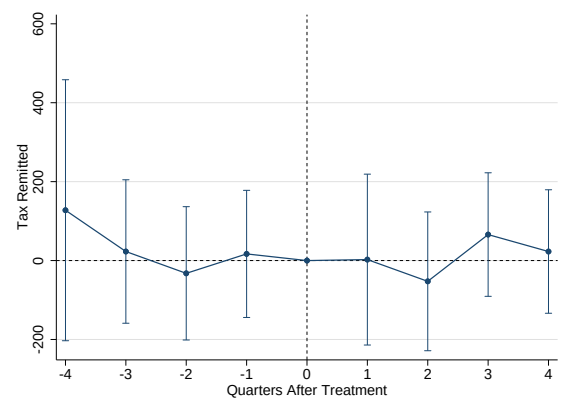
Notes: Standard errors (in parentheses) clustered by Preparation Firm. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Any tax remitted results from linear probability model.

4.4. Narrow Geographic (ZIP+4) Network Effects Results

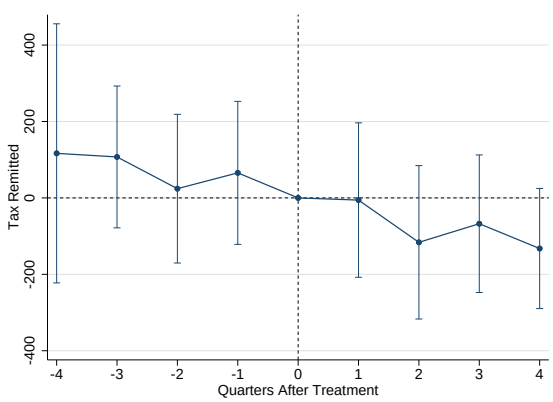
We find mixed evidence of narrow geographic network effects between firms on the same postal route, presented in Figure 6 and Table 8, and no evidence of such network effects on tax remitted overall. One quarter after treatment, log (tax remitted), which excludes firms remitting nothing, rises by 3.26 log points for firms sharing a postal route with a visited firm, and three quarters after treatment the probability of remitting any tax falls by 1.39

Figure 5: Tax Preparation Firm Network Effects

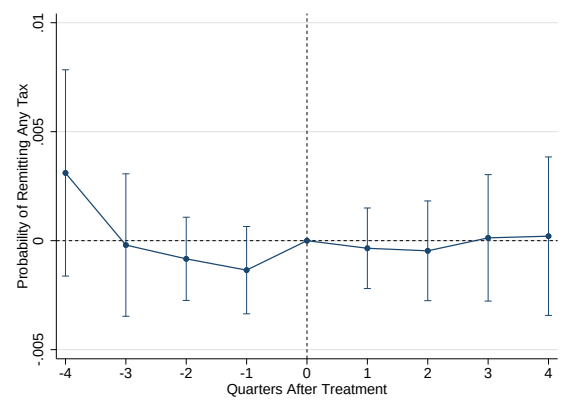
(a) Visit: Tax Remitted



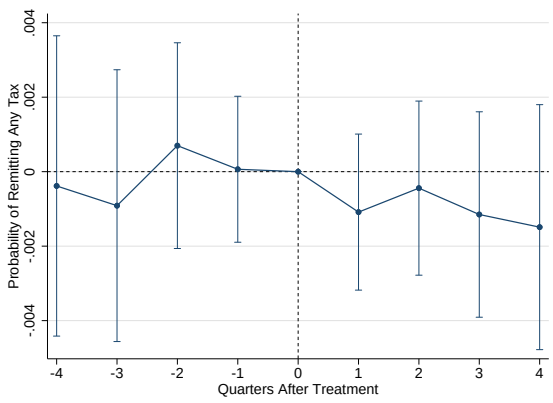
(b) Letter: Tax Remitted



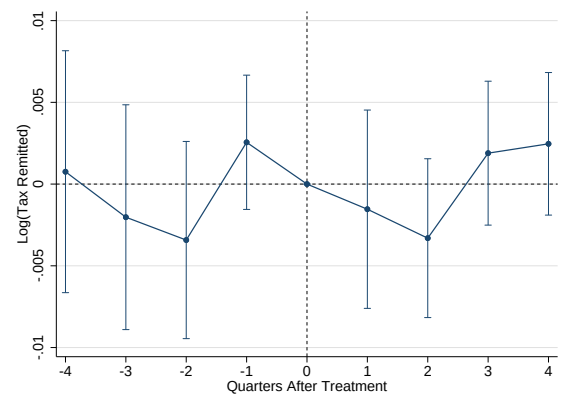
(c) Visit: Probability of Remitting Any Tax



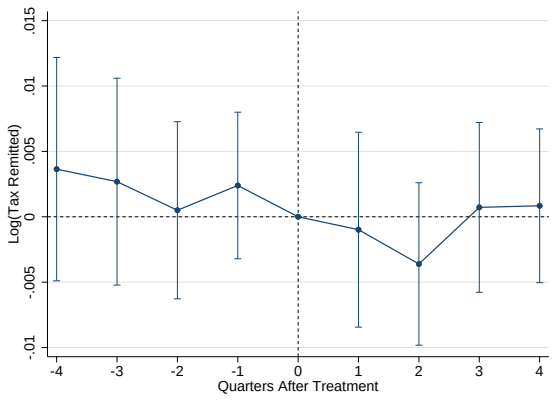
(d) Letter: Any Tax Remitted



(e) Visit: Log(Tax Remitted)



(f) Letter: Log(Tax Remitted)



Notes: figures plot estimates and 95 percent confidence intervals. Standard errors clustered by tax preparation firm. Tax remitted is winsorized at the 98th percentile. Log(tax remitted) excludes firms remitting no tax.

percentage points for firms sharing a postal route with a letter recipient. These effects are statistically significant at the five percent level, and do not accompany substantial changes in tax remitted overall.

Table 8: ZIP+4 Network Effects

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
ZIP+4 Links to Letter Firms	59.5	-0.00469	0.0217
* One Quarter Post	(527)	(0.00619)	(0.0156)
ZIP+4 Links to Letter Firms	-117	-0.00748	0.00782
* Two Quarters Post	(495)	(0.00536)	(0.0120)
ZIP+4 Links to Letter Firms	-545	-0.0139**	-0.00767
* Three Quarters Post	(526)	(0.00628)	(0.0126)
ZIP+4 Links to Letter Firms	-255	-0.0123	0.0129
* Four Quarters Post	(481)	(0.00770)	(0.0131)
ZIP+4 Links to Visit Firms	847	0.00158	0.0326**
* One Quarter Post	(784)	(0.00540)	(0.0162)
ZIP+4 Links to Visit Firms	319	0.00225	0.0131
* Two Quarters Post	(563)	(0.00554)	(0.0131)
ZIP+4 Links to Visit Firms	-41.5	-0.00302	0.00650
* Three Quarters Post	(568)	(0.00631)	(0.0134)
ZIP+4 Links to Visit Firms	84.9	-0.000572	0.0128
* Four Quarters Post	(660)	(0.00734)	(0.0127)
Quarter Fixed Effects	Yes	Yes	Yes
Quarter * Total ZIP+4 Links to Alert C Fixed Effects	Yes	Yes	Yes
Number of ZIP+4 Clusters	5,916	5,916	5,476
Number of Firm-Quarters	290,745	290,745	201,828
R-Squared	0.00326	0.0104	0.00891

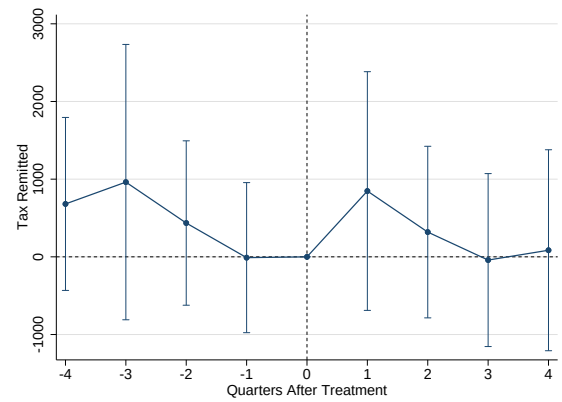
Notes: Standard errors (in parentheses) clustered by ZIP+4. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Any tax remitted results from linear probability model.

4.5. Geographic (ZIP Code) Network Effects Results

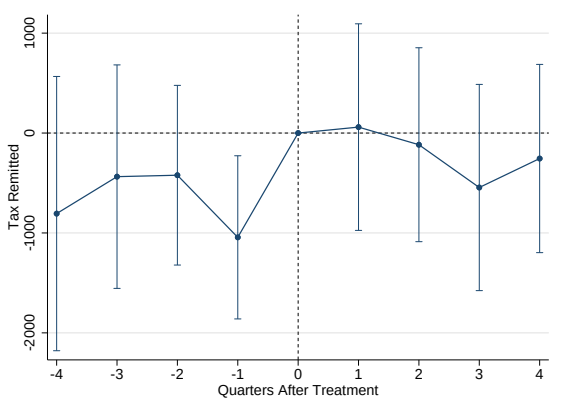
We do not find evidence of spillovers at the ZIP code level on tax remitted, although visits do have small, positive spillovers conditional on remitting any tax. As Figure 7 and Table 9 show, two quarters after treatment, firms in the same ZIP code as a visited firm have log(tax remitted), which is conditional on remitting any tax, that is 0.412 log points higher (with a p-value less than 0.05), and four quarters after treatment the effect on this outcome is 0.666 log points (with a p-value less than 0.01). These effects do not translate to higher tax payments overall.

Figure 6: Narrow Geographic (ZIP+4) Network Effects

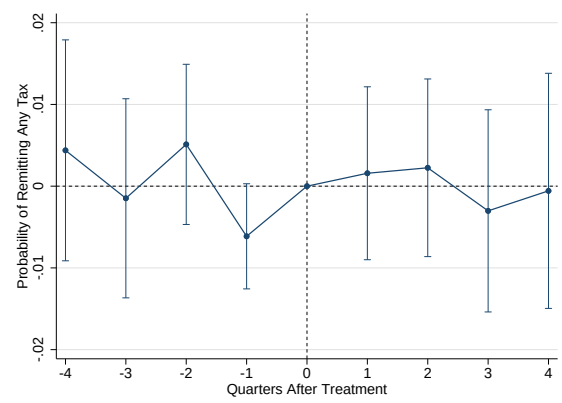
(a) Visit: Tax Remitted



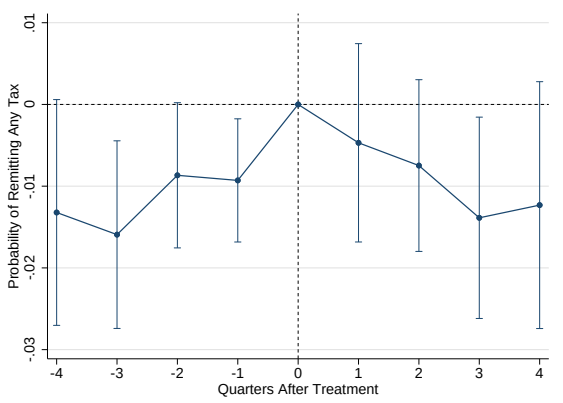
(b) Letter: Tax Remitted



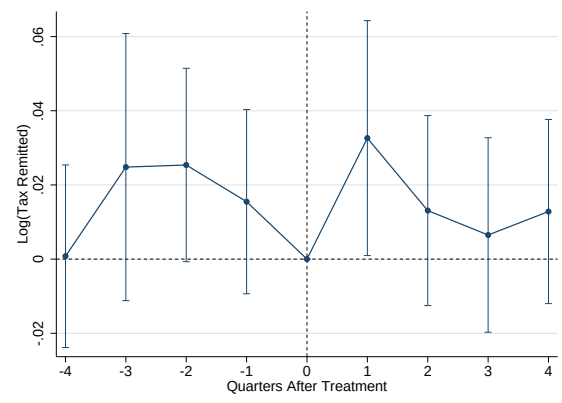
(c) Visit: Any Tax Remitted



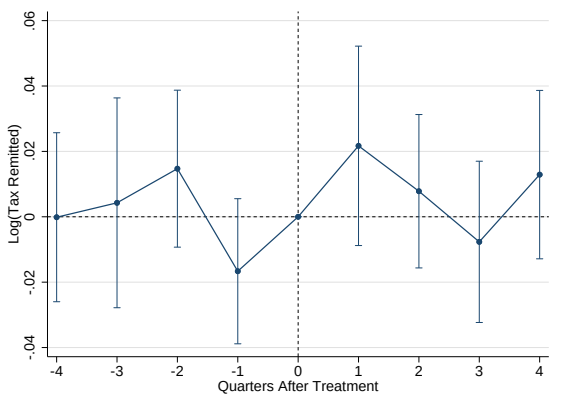
(d) Letter: Any Tax Remitted



(e) Visit: Log(Tax Remitted)



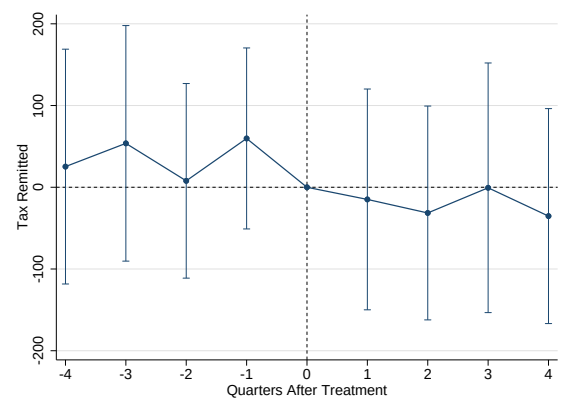
(f) Letter: Log(Tax Remitted)



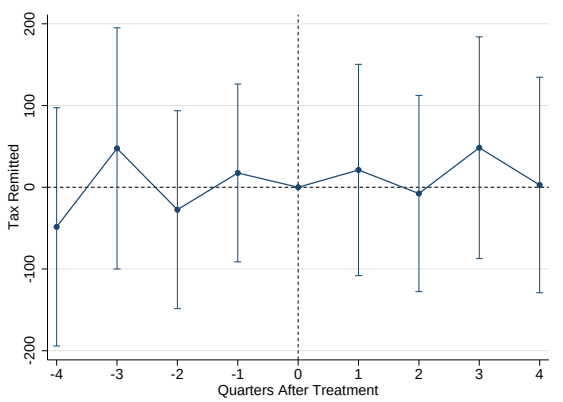
Notes: figures plot estimates and 95 percent confidence intervals. Standard errors clustered by ZIP+4. Tax remitted is winsorized at the 98th percentile. Log(tax remitted) excludes firms remitting no tax.

Figure 7: Geographic (ZIP Code) Network Effects

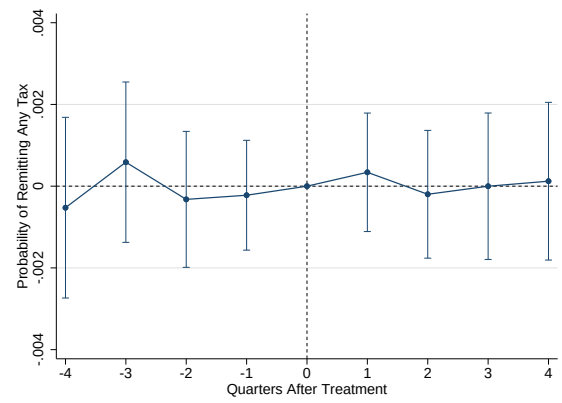
(a) Visit: Tax Remitted



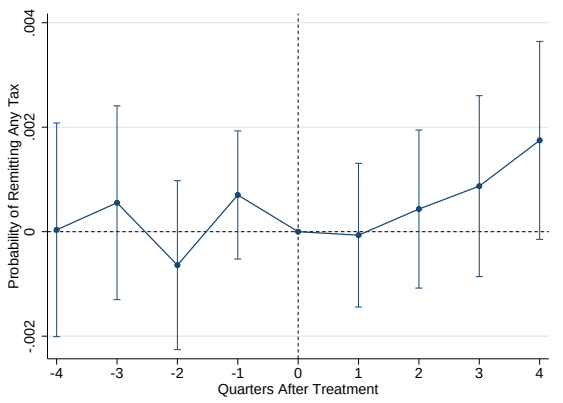
(b) Letter: Tax Remitted



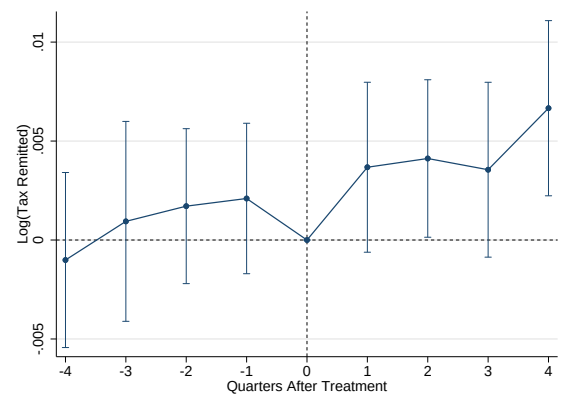
(c) Visit: Any Tax Remitted



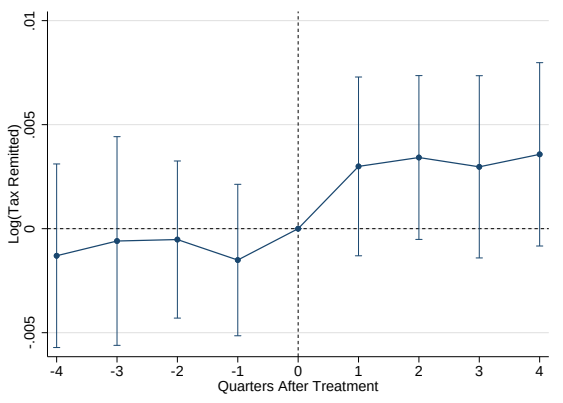
(d) Letter: Any Tax Remitted



(e) Visit: Log(Tax Remitted)



(f) Letter: Log(Tax Remitted)



Notes: figures plot estimates and 95 percent confidence intervals. Standard errors clustered by ZIP Code. Tax remitted is winsorized at the 98th percentile. Log(tax remitted) excludes firms remitting no tax.

Table 9: ZIP Code Network Effects

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
ZIP Code Links to Letter Firms	21.1	-0.0000667	0.00299
* One Quarter Post	(66.0)	(0.000702)	(0.00219)
ZIP Code Links to Letter Firms	-7.65	0.000433	0.00342*
* Two Quarters Post	(61.2)	(0.000772)	(0.00201)
ZIP Code Links to Letter Firms	48.4	0.000872	0.00297
* Three Quarters Post	(69.2)	(0.000883)	(0.00223)
ZIP Code Links to Letter Firms	2.79	0.00175*	0.00357
* Four Quarters Post	(67.3)	(0.000967)	(0.00225)
ZIP Code Links to Visit Firms	-14.9	0.000339	0.00368*
* One Quarter Post	(68.9)	(0.000740)	(0.00219)
ZIP Code Links to Visit Firms	-31.4	-0.000198	0.00412**
* Two Quarters Post	(66.7)	(0.000798)	(0.00203)
ZIP Code Links to Visit Firms	-0.644	-0.00000113	0.00355
* Three Quarters Post	(77.9)	(0.000914)	(0.00225)
ZIP Code Links to Visit Firms	-35.3	0.000122	0.00666***
* Four Quarters Post	(67.1)	(0.000984)	(0.00226)
Quarter Fixed Effects	Yes	Yes	Yes
Quarter * Total ZIP Code Links to Alert C Fixed Effects	Yes	Yes	Yes
Number of ZIP Code Clusters	7,046	7,046	7,008
Number of Firm-Quarters	3,181,959	3,181,959	2,159,992
R-Squared	0.00170	0.00624	0.00949

Notes: Standard errors (in parentheses) clustered by ZIP Code. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Any tax remitted results from linear probability model.

4.6. Parent and Subsidiary Network Effects Results

There is limited evidence that letters and visits have effects on the parents of contacted firms. Few treated firms have parents, and therefore the estimates, presented in Figure 8 and Table 10 are imprecise. Three quarters after treatment, parents of visited firms remit an additional $4.15 million, but this effect is statistically significant only at the ten percent level, and as such is weak evidence. Across other quarters, outcomes, and both treatments, there is little evidence of an effect on treated firms' parents.

Table 10: Effects on Parents of Treated Firms

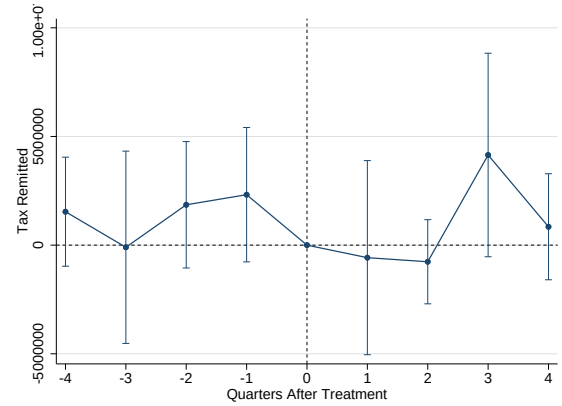
	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
Subsidiary Letter *	-2,862,994	0.0455	-0.955
One Quarter Post	(1,774,428)	(0.0456)	(0.682)
Subsidiary Letter *	-2,431,920	0.0455	-1.00
Two Quarters Post	(1,535,133)	(0.0456)	(0.648)
Subsidiary Letter *	928,559	0.0455	-0.793
Three Quarters Post	(944,069)	(0.0456)	(0.659)
Subsidiary Letter *	758,309	3.68e-13	-0.132
Four Quarters Post	(1,143,241)	(0.000000566)	(0.293)
Subsidiary Visit *	-1,647,927	0.0455	-0.779
One Quarter Post	(2,035,611)	(0.0456)	(0.668)
Subsidiary Visit *	-2,313,524	0.00198	-0.614
Two Quarters Post	(1,534,115)	(0.0631)	(0.751)
Subsidiary Visit *	2,282,318*	0.00198	-0.502
Three Quarters Post	(1,243,672)	(0.0631)	(0.784)
Subsidiary Visit *	150,468	-0.0435	-0.361
Four Quarters Post	(1,199,528)	(0.0437)	(0.444)
Quarter Fixed Effects	Yes	Yes	Yes
Parent Clusters	76	76	36
Observations	684	684	253
R-Squared	0.0201	0.0365	0.0512

Notes: Standard errors (in parentheses) clustered by parent. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Any tax remitted results from linear probability model.

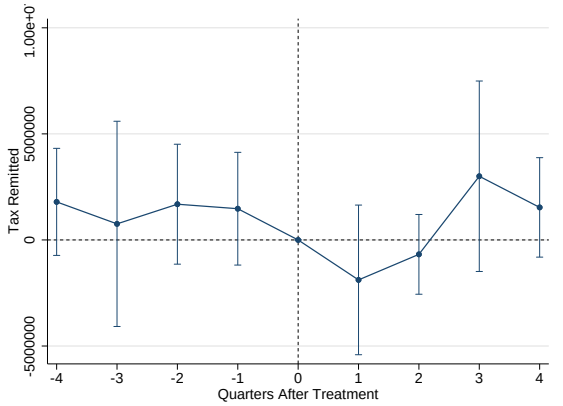
Contacting a parent firm has similarly ambiguous effects on its subsidiaries. Figure 9 and Table 11 show that the in-person visit raises tax remitted by the visited firm's subsidiaries in the quarter after treatment by $915,000, an effect that is highly statistically significant, yet at the same time decreases the probability the subsidiary remits any tax by 0.917 percentage points. The letter has no effects on subsidiaries that are statistically significant at the five percent level. Only 49 treated firms are parents, and there is evidence, shown in Table A.14, of a pre-treatment trend in subsidiaries' tax payments, so these results should be interpreted with caution.

Figure 8: Effects on Parents of Treated Firms

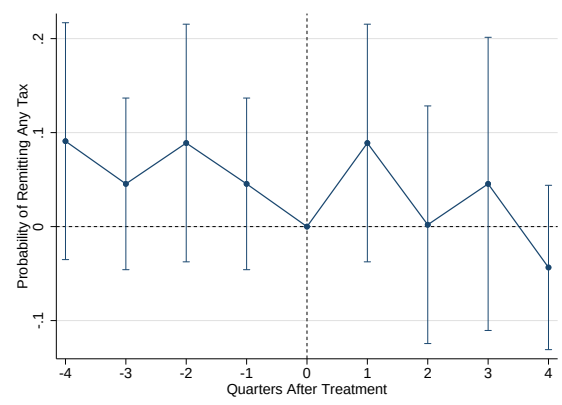
(a) Visit: Tax Remitted



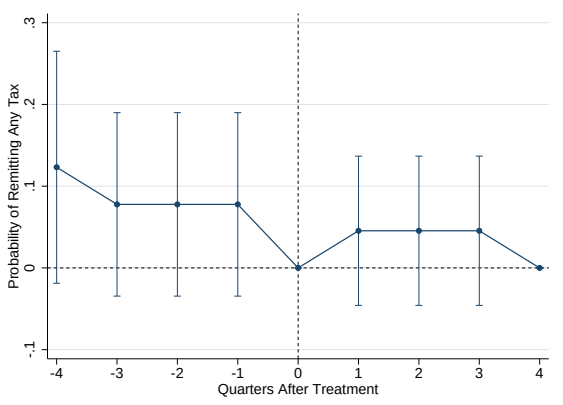
(b) Letter: Tax Remitted



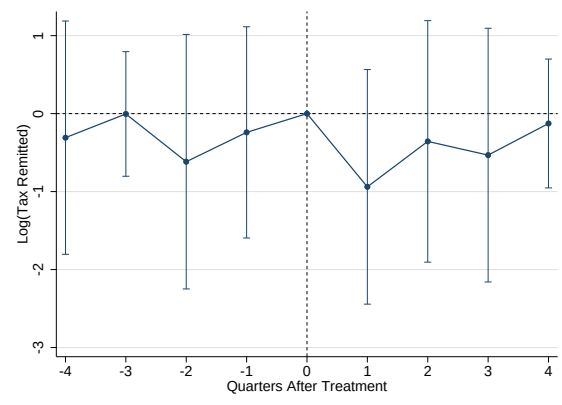
(c) Visit: Any Tax Remitted



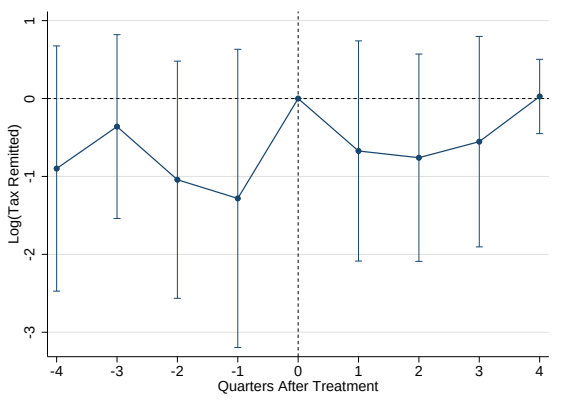
(d) Letter: Any Tax Remitted



(e) Visit: Log(Tax Remitted)



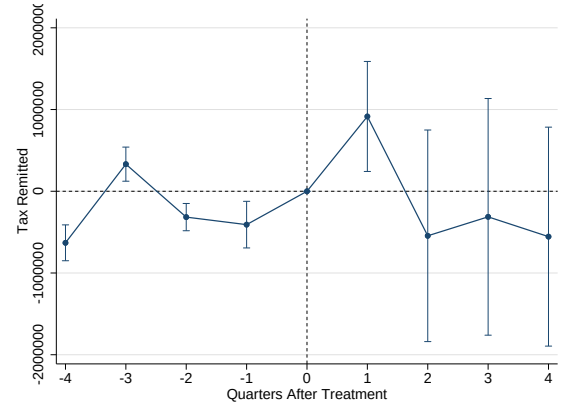
(f) Letter: Log(Tax Remitted)



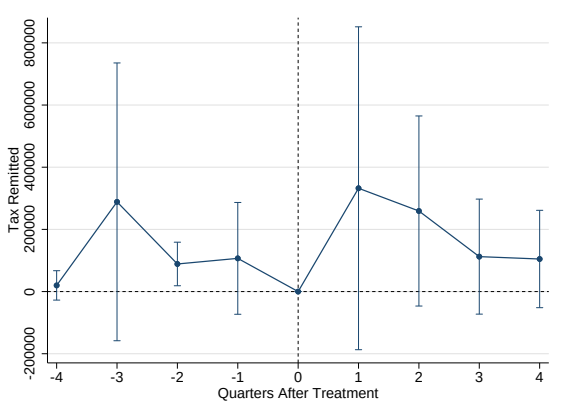
Notes: figures plot estimates and 95 percent confidence intervals. Standard errors clustered by parent firm. Tax remitted is winsorized at the 98th percentile. Log(tax remitted) excludes firms remitting no tax.

Figure 9: Effects on Subsidiaries of Treated Firms

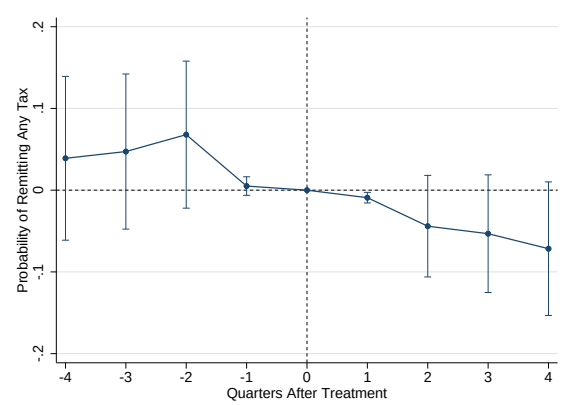
(a) Visit: Tax Remitted



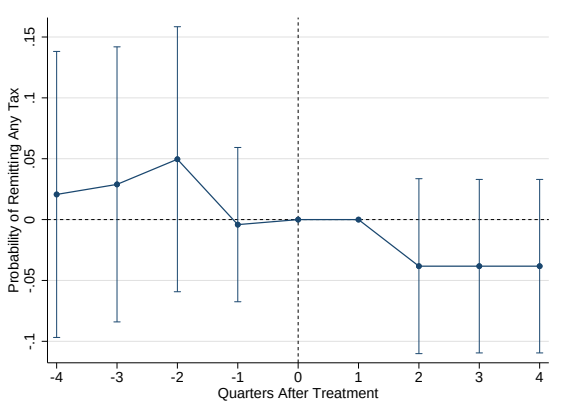
(b) Letter: Tax Remitted



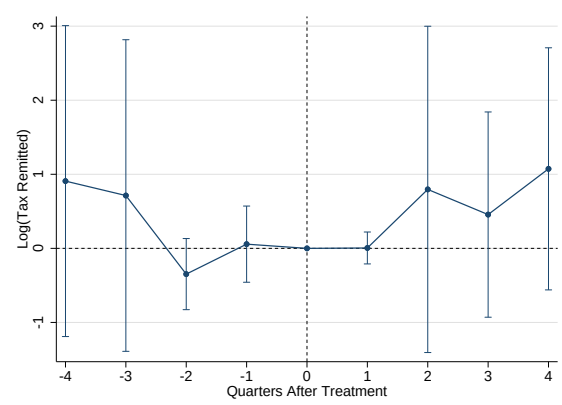
(c) Visit: Any Tax Remitted



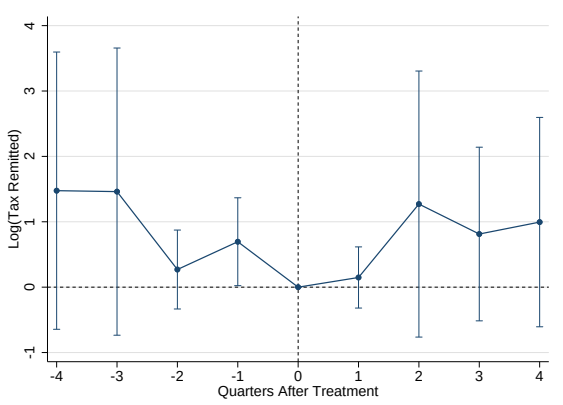
(d) Letter: Any Tax Remitted



(e) Visit: Log(Tax Remitted)



(f) Letter: Log(Tax Remitted)



Notes: figures plot estimates and 95 percent confidence intervals. Standard errors clustered by parent firm. Tax remitted is winsorized at the 98th percentile. Log(tax remitted) excludes firms remitting no tax.

Table 11: Effects on Subsidiaries of Treated Firms

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
Parent Letter *	293,983	0.0259	0.140
One Quarter Post	(216,372)	(0.0236)	(0.300)
Parent Letter *	259,715*	0.00521	1.04
Two Quarters Post	(153,028)	(0.0357)	(0.935)
Parent Letter *	112,997	-0.0165	0.999
Three Quarters Post	(95,483)	(0.0414)	(0.860)
Parent Letter *	103,595	-0.0124	0.885
Four Quarters Post	(80,073)	(0.0412)	(0.858)
Parent Visit *	1,065,494***	-0.00504	0.205
One Quarter Post	(314,180)	(0.00556)	(0.206)
Parent Visit *	83,654	-0.0532	1.24
Two Quarters Post	(658,621)	(0.0358)	(0.953)
Parent Visit *	402,655	-0.0624*	1.37
Three Quarters Post	(784,156)	(0.0341)	(0.882)
Parent Visit *	-110,721	-0.0674	1.16
Four Quarters Post	(644,678)	(0.0404)	(0.859)
Quarter Fixed Effects	Yes	Yes	Yes
Parent Clusters	49	49	26
Number of Firm-Quarters	3,573	3,573	768
R-Squared	0.114	0.0720	0.263

Notes: Standard errors (in parentheses) clustered at the parent level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Any tax remitted results from linear probability model.

To summarize the network effects results, focusing on overall tax remitted, visits have delayed spillover effects on tax remitted through shared individual tax preparers, while there is not strong evidence of effects through the other networks we study.

5. Comparison of Aggregate Network Effects to Direct Effects

Even if the network effect is small *per linked firm*, a large number of network links between firms can imply substantial network effects in the aggregate. To compare the aggregate network effects to the direct effects, we define the network multiplier, equal to the ratio of the aggregate network effect of a treatment to the treatment's direct effect. We multiply the coefficient or sum of coefficients on tax remitted by the average number of links per firm to obtain a per-letter or per-visit effect, which we divide by the direct effect to obtain a network multiplier. Limiting our focus to estimates with 95 percent confidence intervals that exclude zero, we find that the tax preparer network multiplier over the year following the visit is 1.2, and the subsidiary network multiplier one quarter after the visit is 8.1 (although violations of the parallel trends assumption before treatment for subsidiaries make us skeptical of this subsidiary estimate). Multipliers for the quarter after treatment are reported in Table 12 and for the four quarters after treatment in Table 13. Over the following year, each visit leads the visited firm to remit an additional $10,233, and also generates an additional $12,258 from firms sharing a tax preparer with the visited firm.

Table 12: Dollar Values and Network Multipliers: First Quarter After Treatment

	(1) Direct	(2) Preparer	(3) Prep Firm	(4) ZIP+4	(5) ZIP Code	(6) Parent	(7) Subsidiary
Links	1	22.8	98.1	2.84	659	0.00657	0.0326
Letter Effect per Link	94.4	35.3	-5.67	59.5	21.1	-2,862,994	293,983
(se)	382	72.2	103	527	66.0	1,774,428	216,372
Dollars per Letter	94.4	806	-556	169	13,938	-18,817	9,589
(se)	382	1,649	10,116	1,497	43,493	11,662	7,057
Letter Network Multiplier	1	8.53	-5.89	1.79	148	-199	102
Visit Effect per Link	3,686***	85.5	2.47	847	-14.9	-1,647,927	1,065,494***
(se)	399	61.3	110	784	68.9	2,035,611	314,180
Dollars per Visit	3,686***	1,952	242	2,404	-9,800	-10,831	34,752***
(se)	399	1,399	10,837	2,224	45,440	13,379	10,247
Visit Network Multiplier	1	0.530	0.0658	0.652	-2.66	-2.94	9.43

Notes: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 13: Dollar Values and Network Multipliers: Four-Quarter Totals

	(1) Direct	(2) Preparer	(3) Prep Firm	(4) ZIP+4	(5) ZIP Code	(6) Parent	(7) Subsidiary
Links	1	22.8	98.1	2.84	659	0.00657	0.0326
Letter Effect per Link	322	281	-322	-857	64.7	-3,608,046	770,290
(se)	1,483	273	296	1,759	215	2,745,525	502,352
Dollars per Letter	322	6,417	-31,580	-2,434	42,647	-23,714	25,124
(se)	1,483	6,231	29,062	4,993	141,806	18,045	16,385
Letter Network Multiplier	1	19.9	-98.0	-7.55	132	-73.6	78.0
Visit Effect per Link	10,233***	537**	38.6	1,209	-82.2	-1,528,665	1,441,081
(se)	1,495	250	273	2,046	233	3,013,540	2,356,680
Dollars per Visit	10,233***	12,258**	3,789	3,433	-54,207	-10,047	47,002
(se)	1,495	5,702	26,820	5,807	153,582	19,806	76,865
Visit Network Multiplier	1	1.20	0.370	0.335	-5.30	-0.982	4.59

Notes: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

These estimates depend on several simplifying assumptions. Although we multiply the mean effect per link by the mean number of links, both the effect and the number of links are unlikely to be distributed evenly across the population. These calculations also do not account for heterogeneous direct effects by firm size or for non-linear dose response to multiple links to treated firms. The networks we discuss above intersect, as firms for example may share both a neighborhood and a tax preparer, though in unreported results we find that including all of the networks in a single specification does not substantially change the tax preparer network effect of the visit. Despite these assumptions, the network multiplier calculations demonstrate that network effects may be economically substantial.

6. Implications for Policy

How do these findings inform resource allocation decisions? Should each treatment be expanded or cut back? To answer these questions, we need to consider all the costs and benefits of each treatment. Before proceeding, we note that all the estimated effects pertain to revenue remittance and not, as is true in most similar studies, reported tax liability (that may not be remitted in a timely way, or at all).

6.1. Would Net Revenue Rise?

A treatment boosts net revenue if the marginal revenue it raises exceeds its marginal administrative costs. There are three components to the revenue raised: the direct effect on the treated group, the network effect, and the general deterrent effect in the population at large, denoted as r_{Dt}, r_{Nt}, and r_{Gt}, respectively, where subscript t indicates the treatment, either V for visit or L for letter. In this paper, we have estimated the direct effect and the network effect, but not the general deterrent effect. The revenue raised should be compared to the marginal administrative cost, denoted a_t. The calculation for each treatment is simply whether $r_{Dt} + r_{Nt} + r_{Gt} \equiv r_t > a_t$.

To address these questions, we begin by referring to the dollar values for the year following treatment calculated in Table 13, where we show that $r_{DV} = \$10,233$ and $r_{DL} = \$322$. Based on IRS data, $a_V = \$220$ and $a_L = \$4$. Both treatments clearly increase net revenue without taking network or general deterrent effects into account. Assuming the general deterrent effect, which we cannot observe, were negligible, incorporating the statistically significant tax preparer network effect of the visit yields $r_{NV} = \$12,258$. Then $r_V = \$10,233 + \$12,258 = \$22,491 \gg \$220$. Similarly, we can calculate that $r_L = \$322 > \$4$. Even absent general deterrent effects, both treatments easily pass this simple net-revenue-increasing test.

There are, though, other issues to consider. These calculations ignore compliance costs incurred by treated taxpayers, which are likely higher for the visit. In addition, we have ignored any difference between the average effect we have estimated and the marginal effect, although this difference may not be large given that the population of firms we study are not the highest-risk firms routinely subject to treatment, but instead a group of firms that typically are not treated. These calculations should be done on a discounted present-value basis. Given that current interest rates are near zero, discounting itself is not a substantively important issue over the course of a single year. What is not known is whether the estimated

effects would reverse sign if carried out past the year we examine. In other words, we will be overstating the net revenue gain to the extent that the treatments cause payments to accelerate but not increase in total; we see no sign of this over the course of a year but cannot be sure it is not an issue in the longer term.

6.2. Would Re-Allocating Resources Raise More Revenue?

Given a fixed resource budget, would more re-allocating resources between visits and letters raise more revenue? If the objective of the tax authority is to maximize revenue net of cost, then the answer depends on whether the following inequality holds: if it does, resources should be shifted from letters to visits[11]:

$$r_{DV} + r_{NV} + r_{GV} > \frac{a_V}{a_L}(r_{DL} + r_{NL} + r_{GL}). \tag{3}$$

In Expression 3, (a_V/a_L) represents the trade-off in the extent of alternative treatments: visiting one fewer firm enables the tax authority to send (a_V/a_L) more letters while staying within the given budget. Now the relative general deterrence effects of the two treatments can matter. If we are willing to assume that the general deterrence effects are proportional to the sum of the direct and network effects, $r_{GV}/(r_{DV} + r_{NV}) = r_{GL}/(r_{DL} + r_{NL})$, then Expression 3 simplifies to:

$$r_{DV} + r_{NV} > \frac{a_V}{a_L}(r_{DL} + r_{NL}). \tag{4}$$

Using our values from above, the left-hand side of Expression 4 is $\$10,233 + \$12,258 = \$22,491$, while the right-hand side is $(220/4) * \$322 = \$17,710$. Because letters deliver about 1/70 of the visit's return for 1/55 of the cost, the average per-dollar-spent return is slightly higher for the visit and thus a fiscally-constrained tax agency would increase revenue by shifting resources from letters to visits at the margin[12]. Given the degree of uncertainty surrounding our estimates of both the direct and network effects, however, we cannot confidently rule out that the per-dollar returns to the two interventions are the same.

6.3. Would Policy Changes Increase Welfare?

The evaluation of whether welfare would rise when a given policy changes is more complicated. For one thing, such an evaluation should account for marginal compliance costs (resource costs borne directly by private citizens in the form of time and expenditure), which are social costs that do not show up in government budgets. Second, the appropriate criterion is not whether revenue net of cost increases, because that ignores the fact that any additional tax remittance is a transfer from private hands, which has social value, to the

[11] All the point estimates have associated confidence bands, and thus the cost-benefit analyses are themselves subject to error.

[12] If the average return in our sample equals the marginal return, and in equilibrium the deterrent effects of the two treatments are not related, as they would be if for example firms that do not respond to a letter are later visited as a result. This possibility is not addressed by the experiments we conduct, because we do not vary the operational procedure in which populations judged to be higher-risk than our sample receive visits.

government that provides services that are of value to the population. As shown in Keen and Slemrod (2017), which draws on Slemrod and Yitzhaki (1987) and Mayshar (1991), the welfare impact of the intervention can be approximated by

$$\Delta W \equiv (v^{\theta} - 1)\Delta R - v^{\theta}\Delta a - \Delta c. \tag{5}$$

In Expression 5, v^{θ} is the marginal social value of an additional dollar of revenue. If the question is whether to increase administrative effort, ceteris paribus, then v^{θ} represents the marginal social value of raising a dollar of net revenue for public spending. If the question is whether to increase administrative effort while reducing, say, the tax rate in a revenue-neutral way, then v^{θ} represents the social cost saved by reducing the tax collected via the tax rate by one dollar, sometimes referred to as the marginal efficiency cost of raising funds. In either case, the first term on the right-hand-side of Expression 5 is the marginal social value of the additional net revenue collected when an administrative policy instrument increases by one unit. Because raising revenue is costly, the value of v^{θ} will exceed one. The other two terms on the right-hand-side of Expression 4 are the marginal social cost of increasing government spending and the marginal compliance cost; the former is multiplied by v^{θ} to reflect that government spending must be funded by raising distortionary, and therefore socially costly, taxation. To see the implications of Expression 5, following Mayshar (1991) we set $v^{\theta} = 1.17$ and assume that the marginal compliance cost is twice the marginal administrative cost. In addition, we assume that the general deterrent effect is zero. Then Expression 5 becomes the following for letters and visits respectively:

$$\Delta W_L = (1.17 - 1) * 322 - 1.17 * 4 - 8 = 42.1 \gg 0, \tag{6}$$

$$\Delta W_V = (1.17 - 1) * 22,491 - 1.17 * 220 - 440 = 3,126 \gg 0. \tag{7}$$

In these calculations, additional letters and visits each enhance welfare. To be sure, these illustrative calculations depend on arbitrary assumptions about the social value of marginal revenue, the marginal compliance cost, and the general deterrent effect of expanding enforcement instruments. The calculations do, though, illustrate the difference between subjecting enforcement initiatives to a net-revenue-maximizing criterion and subjecting enforcement initiatives to a welfare-maximizing criterion.

7. Conclusion

This paper uses a randomized experiment conducted in partnership with the IRS to estimate both the change in employment payroll and withholding taxes remitted caused by receiving a letter noting that the firm's deposits have decreased, discussing the firm's deposit responsibility and potential penalties, and providing general information, or caused by an in-person visit from an IRS Revenue Officer. In addition, we estimate the network, or spillover, effects on taxes remitted by firms linked to letter and visit recipients by geography, tax preparers, and parent-subsidiary relationships. To our knowledge, no previous research has investigated the effects of tax enforcement on firms sharing a tax preparer with the treated firm or on the treated firm's parent or subsidiaries.

We find large, immediate effects of in-person visits on tax remitted that persist for at least four quarters and are transmitted through tax-preparer networks. Although the per-firm-link tax-preparer network effects of the visit are much smaller than the direct effects, their aggregate effect is 1.2 times the size of the direct effect. We find that letters increase the likelihood that firms remit any tax by three percentage points, but this effect lasts only one quarter, and the effect of the letter on tax remitted overall is not statistically significant. There is no evidence of network effects of the letter. Given the empirical results, both visits and letters pass a net-revenue-increasing criterion. With a fixed tax authority budget, net revenue from one additional dollar of resources spent on in-person visits is slightly higher than net revenue from an additional dollar spent to send letters. With some additional assumptions, both treatments also easily pass a welfare-increasing criterion.

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Table A.1: Direct Effect of Letter: All Quarters

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
Letter * Four Quarters Pre	273 (571)	0.0210* (0.0120)	-0.00170 (0.0368)
Letter * Three Quarters Pre	69.7 (570)	0.00317 (0.0119)	0.0189 (0.0369)
Letter * Two Quarters Pre	-194 (554)	0.000663 (0.0116)	0.0142 (0.0361)
Letter * One Quarter Pre	-337 (471)	-0.0172 (0.0112)	0.0562 (0.0350)
Letter * One Quarter Post	94.4 (382)	0.0302*** (0.0110)	0.00476 (0.0358)
Letter * Two Quarters Post	-112 (438)	0.0112 (0.0118)	-0.0171 (0.0359)
Letter * Three Quarters Post	163 (459)	0.0158 (0.0122)	-0.0160 (0.0376)
Letter * Four Quarters Post	177 (459)	0.0136 (0.0125)	-0.00353 (0.0384)
Quarter Fixed Effects	Yes	Yes	Yes
Number of Firm-Quarters	109,548	109,548	77,051
R-Squared	0.0281	0.0513	0.0220

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Direct Effect of Visit: All Quarters

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
Visit * Four Quarters Pre	52.7 (585)	0.00234 (0.0122)	-0.00842 (0.0368)
Visit * Three Quarters Pre	92.1 (572)	-0.00707 (0.0119)	0.0174 (0.0366)
Visit * Two Quarters Pre	-393 (556)	-0.00559 (0.0117)	-0.0145 (0.0359)
Visit * One Quarter Pre	-299 (465)	-0.0149 (0.0112)	-0.00144 (0.0353)
Visit * One Quarter Post	3,686*** (399)	0.129*** (0.0113)	0.132*** (0.0348)
Visit * Two Quarters Post	2,726*** (438)	0.104*** (0.0120)	0.0344 (0.0349)
Visit * Three Quarters Post	2,169*** (451)	0.0803*** (0.0122)	0.0309 (0.0362)
Visit * Four Quarters Post	1,652*** (448)	0.0694*** (0.0126)	0.0197 (0.0364)
Quarter Fixed Effects	Yes	Yes	Yes
Number of Firm-Quarters	109,548	109,548	77,051
R-Squared	0.0281	0.0513	0.0220

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: Preparer Letter Network Effects with Pre-Treatment Quarters as Placebo Test

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
Preparer Links to Letter Firms *	304**	-0.0000760	0.00676
Four Quarters Pre	(132)	(0.00313)	(0.00521)
Preparer Links to Letter Firms *	71.9	-0.000335	-0.00548
Three Quarters Pre	(95.2)	(0.00260)	(0.00663)
Preparer Links to Letter Firms *	-15.0	0.0000893	-0.000276
Two Quarters Pre	(109)	(0.00185)	(0.00469)
Preparer Links to Letter Firms *	13.8	-0.000667	-0.000396
One Quarter Pre	(102)	(0.00145)	(0.00464)
Preparer Links to Letter Firms *	35.3	-0.00183	-0.00437
One Quarter Post	(72.2)	(0.00152)	(0.00624)
Preparer Links to Letter Firms *	27.0	-0.000140	-0.0000449
Two Quarters Post	(88.6)	(0.00170)	(0.00428)
Preparer Links to Letter Firms *	73.0	-0.00201	0.00264
Three Quarters Post	(92.6)	(0.00216)	(0.00434)
Preparer Links to Letter Firms *	146	-0.00111	0.0109**
Four Quarters Post	(103)	(0.00238)	(0.00455)
Firm Fixed Effects	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes
Quarter * Total Preparer Links to Alert C Fixed Effects	Yes	Yes	Yes
Preparer Clusters	10,219	10,219	9,357
Number of Firm-Quarters	1,796,994	1,796,994	1,193,501
R-Squared	0.00361	0.00500	0.0120

Notes: Standard errors (in parentheses) clustered by Preparer. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Probability results from linear probability model.

Table A.4: Preparer Visit Network Effects with Pre-Treatment Quarters as Placebo Test

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
Preparer Links to Visit Firms *	197	0.000657	0.00683
Four Quarters Pre	(132)	(0.00238)	(0.00428)
Preparer Links to Visit Firms *	-9.41	-0.000457	-0.00378
Three Quarters Pre	(78.7)	(0.00216)	(0.00554)
Preparer Links to Visit Firms *	-101	-0.00160	-0.00101
Two Quarters Pre	(89.4)	(0.00176)	(0.00393)
Preparer Links to Visit Firms *	-40.3	-0.000983	0.00523
One Quarter Pre	(89.0)	(0.00121)	(0.00375)
Preparer Links to Visit Firms *	85.5	0.00188	-0.00236
One Quarter Post	(61.3)	(0.00122)	(0.00505)
Preparer Links to Visit Firms *	52.3	0.000315	-0.00328
Two Quarters Post	(81.9)	(0.00149)	(0.00353)
Preparer Links to Visit Firms *	156*	0.00123	0.00112
Three Quarters Post	(88.3)	(0.00189)	(0.00368)
Preparer Links to Visit Firms *	243***	0.00162	0.000830
Four Quarters Post	(94.1)	(0.00224)	(0.00357)
Firm Fixed Effects	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes
Quarter * Total Preparer Links to Alert C Fixed Effects	Yes	Yes	Yes
Preparer Clusters	10,219	10,219	9,357
Number of Firm-Quarters	1,796,994	1,796,994	1,193,501
R-Squared	0.00361	0.00500	0.0120

Notes: Standard errors (in parentheses) clustered by Preparer. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Probability results from linear probability model.

Table A.5: Preparation Firm Letter Network Effects with Pre-Treatment Quarters as Placebo Test

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
Preparation Firm Links to Letter	117	-0.000384	0.00364
Firms * Four Quarters Pre	(173)	(0.00206)	(0.00436)
Preparation Firm Links to Letter	107	-0.000913	0.00269
Firms * Three Quarters Pre	(94.7)	(0.00186)	(0.00403)
Preparation Firm Links to Letter	24.2	0.000699	0.000498
Firms * Two Quarters Pre	(99.3)	(0.00141)	(0.00346)
Preparation Firm Links to Letter	65.6	0.0000650	0.00239
Firms * One Quarter Pre	(95.5)	(0.001000)	(0.00286)
Preparation Firm Links to Letter	-5.67	-0.00109	-0.000987
Firms * One Quarter Post	(103)	(0.00107)	(0.00380)
Preparation Firm Links to Letter	-116	-0.000442	-0.00361
Firms * Two Quarters Post	(102)	(0.00119)	(0.00317)
Preparation Firm Links to Letter	-67.5	-0.00115	0.000720
Firms * Three Quarters Post	(91.8)	(0.00141)	(0.00331)
Preparation Firm Links to Letter	-132*	-0.00149	0.000841
Firms * Four Quarters Post	(80.2)	(0.00168)	(0.00300)
Firm Fixed Effects	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes
Quarter * Total Preparation Firm Links to Alert C Fixed Effects	Yes	Yes	Yes
Preparation Firm Clusters	9,759	9,759	9,053
Number of Firm-Quarters	3,563,361	3,563,361	2,468,149
R-Squared	0.00502	0.00620	0.0131

Notes: Standard errors (in parentheses) clustered by Preparation Firm. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Probability results from linear probability model.

Table A.6: Preparation Firm Visit Network Effects with Pre-Treatment Quarters as Placebo Test

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
Preparation Firm Links to Visit	128	0.00311	0.000758
Firms * Four Quarters Pre	(169)	(0.00241)	(0.00377)
Preparation Firm Links to Visit	22.9	-0.000201	-0.00203
Firms * Three Quarters Pre	(92.7)	(0.00167)	(0.00351)
Preparation Firm Links to Visit	-32.4	-0.000834	-0.00342
Firms * Two Quarters Pre	(86.2)	(0.000974)	(0.00308)
Preparation Firm Links to Visit	16.9	-0.00135	0.00256
Firms * One Quarter Pre	(82.2)	(0.00102)	(0.00210)
Preparation Firm Links to Visit	2.47	-0.000349	-0.00154
Firms * One Quarter Post	(110)	(0.000943)	(0.00309)
Preparation Firm Links to Visit	-52.7	-0.000465	-0.00331
Firms * Two Quarters Post	(89.7)	(0.00117)	(0.00248)
Preparation Firm Links to Visit	65.9	0.000129	0.00189
Firms * Three Quarters Post	(79.9)	(0.00148)	(0.00225)
Preparation Firm Links to Visit	22.9	0.000206	0.00246
Firms * Four Quarters Post	(79.8)	(0.00185)	(0.00222)
Firm Fixed Effects	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes
Quarter * Total Preparation Firm Links to Alert C Fixed Effects	Yes	Yes	Yes
Preparation Firm Clusters	9,759	9,759	9,053
Number of Firm-Quarters	3,563,361	3,563,361	2,468,149
R-Squared	0.00502	0.00620	0.0131

Notes: Standard errors (in parentheses) clustered by Preparation Firm. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Probability results from linear probability model.

Table A.7: ZIP+4 Letter Network Effects with Pre-Treatment Quarters as Placebo Test

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
ZIP+4 Links to Letter Firms	-806	-0.0132*	-0.000138
* Four Quarters Pre	(700)	(0.00705)	(0.0132)
ZIP+4 Links to Letter Firms	-436	-0.0159***	0.00427
* Three Quarters Pre	(570)	(0.00585)	(0.0164)
ZIP+4 Links to Letter Firms	-422	-0.00867*	0.0147
* Two Quarters Pre	(459)	(0.00453)	(0.0122)
ZIP+4 Links to Letter Firms	-1,044**	-0.00929**	-0.0167
* One Quarter Pre	(417)	(0.00384)	(0.0113)
ZIP+4 Links to Letter Firms	59.5	-0.00469	0.0217
* One Quarter Post	(527)	(0.00619)	(0.0156)
ZIP+4 Links to Letter Firms	-117	-0.00748	0.00782
* Two Quarters Post	(495)	(0.00536)	(0.0120)
ZIP+4 Links to Letter Firms	-545	-0.0139**	-0.00767
* Three Quarters Post	(526)	(0.00628)	(0.0126)
ZIP+4 Links to Letter Firms	-255	-0.0123	0.0129
* Four Quarters Post	(481)	(0.00770)	(0.0131)
Firm Fixed Effects	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes
Quarter * Total ZIP+4 Links to Alert C Fixed Effects	Yes	Yes	Yes
ZIP+4 Clusters	5,916	5,916	5,476
Number of Firm-Quarters	290,745	290,745	201,828
R-Squared	0.00326	0.0104	0.00891

Notes: Standard errors (in parentheses) clustered by ZIP+4. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Probability results from linear probability model.

Table A.8: ZIP+4 Visit Network Effects with Pre-Treatment Quarters as Placebo Test

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
ZIP+4 Links to Visit Firms	681	0.00438	0.000777
* Four Quarters Pre	(568)	(0.00689)	(0.0126)
ZIP+4 Links to Visit Firms	962	-0.00148	0.0248
* Three Quarters Pre	(904)	(0.00621)	(0.0184)
ZIP+4 Links to Visit Firms	435	0.00511	0.0254*
* Two Quarters Pre	(540)	(0.00500)	(0.0133)
ZIP+4 Links to Visit Firms	-10.4	-0.00614*	0.0155
* One Quarter Pre	(493)	(0.00328)	(0.0127)
ZIP+4 Links to Visit Firms	847	0.00158	0.0326**
* One Quarter Post	(784)	(0.00540)	(0.0162)
ZIP+4 Links to Visit Firms	319	0.00225	0.0131
* Two Quarters Post	(563)	(0.00554)	(0.0131)
ZIP+4 Links to Visit Firms	-41.5	-0.00302	0.00650
* Three Quarters Post	(568)	(0.00631)	(0.0134)
ZIP+4 Links to Visit Firms	84.9	-0.000572	0.0128
* Four Quarters Post	(660)	(0.00734)	(0.0127)
Firm Fixed Effects	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes
Quarter * Total ZIP+4 Links to Alert C Fixed Effects	Yes	Yes	Yes
ZIP+4 Clusters	5,916	5,916	5,476
Number of Firm-Quarters	290,745	290,745	201,828
R-Squared	0.00326	0.0104	0.00891

Notes: Standard errors (in parentheses) clustered by ZIP+4. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Probability results from linear probability model.

Table A.9: ZIP Code Letter Network Effects with Pre-Treatment Quarters as Placebo Test

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
ZIP Code Links to Letter Firms	-48.4	0.0000352	-0.00130
* Four Quarters Pre	(74.4)	(0.00104)	(0.00225)
ZIP Code Links to Letter Firms	47.6	0.000554	-0.000593
* Three Quarters Pre	(75.3)	(0.000946)	(0.00256)
ZIP Code Links to Letter Firms	-27.4	-0.000641	-0.000523
* Two Quarters Pre	(61.8)	(0.000825)	(0.00193)
ZIP Code Links to Letter Firms	17.5	0.000703	-0.00151
* One Quarter Pre	(55.5)	(0.000625)	(0.00186)
ZIP Code Links to Letter Firms	21.1	-0.0000667	0.00299
* One Quarter Post	(66.0)	(0.000702)	(0.00219)
ZIP Code Links to Letter Firms	-7.65	0.000433	0.00342*
* Two Quarters Post	(61.2)	(0.000772)	(0.00201)
ZIP Code Links to Letter Firms	48.4	0.000872	0.00297
* Three Quarters Post	(69.2)	(0.000883)	(0.00223)
ZIP Code Links to Letter Firms	2.79	0.00175*	0.00357
* Four Quarters Post	(67.3)	(0.000967)	(0.00225)
Firm Fixed Effects	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes
Quarter * Total ZIP Code Links to Alert C Fixed Effects	Yes	Yes	Yes
ZIP Code Clusters	7,046	7,046	7,008
Number of Firm-Quarters	3,181,959	3,181,959	2,159,992
R-Squared	0.00170	0.00624	0.00949

Notes: Standard errors (in parentheses) clustered by ZIP Code. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Probability results from linear probability model.

Table A.10: ZIP Code Visit Network Effects with Pre-Treatment Quarters as Placebo Test

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
ZIP Code Links to Visit Firms	25.3	-0.000526	-0.00101
* Four Quarters Pre	(73.3)	(0.00113)	(0.00226)
ZIP Code Links to Visit Firms	53.7	0.000587	0.000942
* Three Quarters Pre	(73.5)	(0.00100)	(0.00258)
ZIP Code Links to Visit Firms	7.88	-0.000323	0.00171
* Two Quarters Pre	(60.7)	(0.000849)	(0.00200)
ZIP Code Links to Visit Firms	59.7	-0.000222	0.00210
* One Quarter Pre	(56.5)	(0.000686)	(0.00194)
ZIP Code Links to Visit Firms	-14.9	0.000339	0.00368*
* One Quarter Post	(68.9)	(0.000740)	(0.00219)
ZIP Code Links to Visit Firms	-31.4	-0.000198	0.00412**
* Two Quarters Post	(66.7)	(0.000798)	(0.00203)
ZIP Code Links to Visit Firms	-0.644	-0.00000113	0.00355
* Three Quarters Post	(77.9)	(0.000914)	(0.00225)
ZIP Code Links to Visit Firms	-35.3	0.000122	0.00666***
* Four Quarters Post	(67.1)	(0.000984)	(0.00226)
Firm Fixed Effects	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes
Quarter * Total ZIP Code Links to Alert C Fixed Effects	Yes	Yes	Yes
ZIP Code Clusters	7,046	7,046	7,008
Number of Firm-Quarters	3,181,959	3,181,959	2,159,992
R-Squared	0.00170	0.00624	0.00949

Notes: Standard errors (in parentheses) clustered by ZIP Code. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Probability results from linear probability model.

Table A.11: Effect of Letter on Parent: All Quarters

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
Subsidiary Letter *	1,669,376	0.123*	-0.947
Four Quarters Pre	(1,226,347)	(0.0709)	(0.721)
Subsidiary Letter *	1,047,398	0.123*	-1.17
Three Quarters Pre	(2,112,822)	(0.0709)	(0.899)
Subsidiary Letter *	1,420,764	0.0777	-1.09
Two Quarters Pre	(1,207,557)	(0.0561)	(0.734)
Subsidiary Letter *	429,299	0.0777	-1.30
One Quarter Pre	(702,842)	(0.0561)	(0.916)
Subsidiary Letter *	-2,862,994	0.0455	-0.955
One Quarter Post	(1,774,428)	(0.0456)	(0.682)
Subsidiary Letter *	-2,431,920	0.0455	-1.00
Two Quarters Post	(1,535,133)	(0.0456)	(0.648)
Subsidiary Letter *	928,559	0.0455	-0.793
Three Quarters Post	(944,069)	(0.0456)	(0.659)
Subsidiary Letter *	758,309	3.68e-13	-0.132
Four Quarters Post	(1,143,241)	(0.000000566)	(0.293)
Subsidiary Visit *	-1,647,927	0.0455	-0.779
Quarter Fixed Effects	Yes	Yes	Yes
Parent Clusters	76	76	36
Observations	684	684	253
R-Squared	0.0201	0.0365	0.0512

Notes: Standard errors (in parentheses) clustered by parent. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Any tax remitted results from linear probability model.

Table A.12: Effect of Visit on Parent: All Quarters

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
Subsidiary Visit *	1,496,364	0.0909	-0.299
Four Quarters Pre	(1,239,504)	(0.0629)	(0.690)
Subsidiary Visit *	100,974	0.0909	-0.674
Three Quarters Pre	(1,659,865)	(0.0629)	(0.740)
Subsidiary Visit *	1,565,585	0.0455	-0.301
Two Quarters Pre	(1,226,307)	(0.0456)	(0.705)
Subsidiary Visit *	1,509,764	0.0455	-0.190
One Quarter Pre	(1,134,349)	(0.0456)	(0.627)
Subsidiary Visit *	-1,647,927	0.0455	-0.779
One Quarter Post	(2,035,611)	(0.0456)	(0.668)
Subsidiary Visit *	-2,313,524	0.00198	-0.614
Two Quarters Post	(1,534,115)	(0.0631)	(0.751)
Subsidiary Visit *	2,282,318*	0.00198	-0.502
Three Quarters Post	(1,243,672)	(0.0631)	(0.784)
Subsidiary Visit *	150,468	-0.0435	-0.361
Four Quarters Post	(1,199,528)	(0.0437)	(0.444)
Quarter Fixed Effects	Yes	Yes	Yes
Parent Clusters	76	76	36
Observations	684	684	253
R-Squared	0.0201	0.0365	0.0512

Notes: Standard errors (in parentheses) clustered by parent. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Any tax remitted results from linear probability model.

Table A.13: Effect of Parent Letter on Subsidiary: All Quarters

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
Parent Letter *	21,784	0.0641	1.18
Four Quarters Pre	(22,310)	(0.0554)	(0.982)
Parent Letter *	234,560	0.0765	1.03
Three Quarters Pre	(171,417)	(0.0529)	(1.04)
Parent Letter *	91,092**	0.0931*	0.0251
Two Quarters Pre	(36,695)	(0.0508)	(0.247)
Parent Letter *	40,538	0.0176	0.234
One Quarter Pre	(39,979)	(0.0236)	(0.334)
Parent Letter *	293,983	0.0259	0.140
One Quarter Post	(216,372)	(0.0236)	(0.300)
Parent Letter *	259,715*	0.00521	1.04
Two Quarters Post	(153,028)	(0.0357)	(0.935)
Parent Letter *	112,997	-0.0165	0.999
Three Quarters Post	(95,483)	(0.0414)	(0.860)
Parent Letter *	103,595	-0.0124	0.885
Four Quarters Post	(80,073)	(0.0412)	(0.858)
Quarter Fixed Effects	Yes	Yes	Yes
Parent Clusters	49	49	26
Number of Firm-Quarters	3,573	3,573	768
R-Squared	0.114	0.0720	0.263

Notes: Standard errors (in parentheses) clustered at the parent level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Any tax remitted results from linear probability model.

Table A.14: Effect of Parent Visit on Subsidiary: All Quarters

	Tax Remitted	Any Tax Remitted	Log(Tax Remitted)
4 Quarters Pre * Parent Visit	-455,328***	0.0390	0.949
	(94,688)	(0.0498)	(0.973)
3 Quarters Pre * Parent Visit	354,676***	0.0514	0.665
	(64,243)	(0.0471)	(1.01)
2 Quarters Pre * Parent Visit	-247,099**	0.0679	-0.292
	(105,832)	(0.0447)	(0.220)
1 Quarters Pre * Parent Visit	-289,645*	-0.00413	0.573**
	(172,415)	(0.00464)	(0.250)
Parent Visit *	1,065,494***	-0.00504	0.205
One Quarter Post	(314,180)	(0.00556)	(0.206)
Parent Visit *	83,654	-0.0532	1.24
Two Quarters Post	(658,621)	(0.0358)	(0.953)
Parent Visit *	402,655	-0.0624*	1.37
Three Quarters Post	(784,156)	(0.0341)	(0.882)
Parent Visit *	-110,721	-0.0674	1.16
Four Quarters Post	(644,678)	(0.0404)	(0.859)
Quarter Fixed Effects	Yes	Yes	Yes
Parent Clusters	49	49	26
Number of Firm-Quarters	3,573	3,573	768
R-Squared	0.114	0.0720	0.263

Notes: Standard errors (in parentheses) clustered at the parent level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Any tax remitted results from linear probability model.

Figure A.1: Letter

Department of the Treasury
Internal Revenue Service

Date:

Dear

Your federal tax deposits
We understand federal tax deposit requirements may be confusing and the resulting penalties can be significant. With this in mind, we reviewed your federal tax deposit history and your deposits appear to have decreased. This may be due to a change in your payroll, because you are a new business owner and are not familiar with deposit requirements, or it may be due to other factors.

Your responsibility as an employer
You, as the employer, have the responsibility of withholding trust fund taxes from employees' paychecks. Trust fund tax is money withheld, by an employer, from employees' wages for FICA (social security and Medicare tax) and income tax held in trust until paid to the Department of Treasury. This money must be paid periodically to the Treasury by making federal tax deposits.

What you need to do
Please tell us about the decrease in your deposits so that your account can be updated. You may do one of the following:

- Call the IRS at 1-866-897-4289 Monday through Friday, 8 AM to 8 PM eastern time, or
- Complete and return the enclosed Form 14143, *Reason for Decrease to Federal Tax Deposit.*

Penalty for failing to pay
Individuals who are required to account for and pay these taxes for the business may be personally liable for a penalty if the business fails to pay trust fund taxes. The penalty is equal to the amount of the unpaid trust fund taxes that the business owes the Treasury. For additional information, see the enclosed Notice 784, *Could You be Personally Liable for Certain Unpaid Federal Taxes?*

Penalty for failing to pay timely
If you do not pay these taxes on time or you do not include the required payment with your Form 941, *Employer's Quarterly Federal Tax Return,* interest and penalties will be assessed on any unpaid balance. Additionally, penalties of up to 15% of the amount not deposited may also be assessed, depending on the number of days the federal tax deposits are late.

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Penalty for failing to file your return timely
In the event you are unable to pay your taxes timely, it is imperative to file your Form 941 Employer's Quarterly Federal Tax Return timely. If the return is filed after the due date, the law provides penalties for filing late unless there is a reasonable cause for the delay.

Additional information
For further information, please see Publication 15, *Circular E, Employer's Tax Guide*, or the Internal Revenue Service's small business employment tax section. Both are available at www.irs.gov. The employment tax section of the small business web page can be accessed by selecting "Businesses" at the home page, then selecting "Employment Taxes" under Business Topics.

Thank you for taking the time to keep up with your employment tax obligations.

Program Manager
Centralized Processing Operation
Philadelphia Compliance Services

Enclosures:
Form 14143
Notice 784