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Remote tax authority

Andrew Belnap*, Anthony Welsch, Braden Williams

University of Texas at Austin, USA

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ABSTRACT

Tax enforcement and compliance are critical features of any tax system. One way that prior studies examine these topics is by measuring taxpayer responses to different types of letters from a tax authority. We extend this research by focusing on 'remote firms' – firms with no physical presence in the tax authority's jurisdiction – and examining sales tax compliance. Specifically, we partner with the Texas Comptroller's office to conduct a randomized field experiment that examines whether and why firms comply with remote tax authority. Using a sample of remote firms with a recent history of late filing, we find that tax authority correspondence increases the average reported sales tax base by 105 percent and tax payments by $3,711, although the effects are short-lived. Our evidence suggests that the effect is driven by increased salience of firms' tax obligations. Our study offers key insights given the growth in cross-border transactions and remote taxation.

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1. Introduction

Historically, most governments only taxed firms that established a physical presence within their borders. These tax systems, however, originated in a brick-and-mortar environment and are in many ways unsuited for the modern economy where firms can sell their products globally without needing to locate labor and capital near their customers (Huddleston and Sicilian, 2009; OECD, 2020). In response, many lawmakers and regulators are expanding their taxing rights by asserting *remote* tax authority— where a government claims the legal right to tax a person or business located outside of the jurisdiction's physical borders.[1] Despite the growing prevalence of remote tax authority, little is known about tax enforcement and compliance under these new circumstances.

In this study, we partner with the Texas Comptroller's office (the Texas tax authority) to conduct a randomized field experiment using remote firms' sales tax filings. We use a multi-version letter campaign to understand the determinants of remote firms' compliance and how remote tax authorities can encourage compliance. Specifically, we use our experiment to answer two questions: (1) does correspondence from a remote tax authority affect firms' tax compliance? and (2) what explains taxpayers' compliance with a remote tax authority?

* Corresponding author.
E-mail addresses: andrew.belnap@mccombs.utexas.edu (A. Belnap), anthony.welsch@mccombs.utexas.edu (A. Welsch), brady.williams@mccombs.utexas.edu (B. Williams).

[1] "Remote tax authority" refers to both this legal right as well as the agency tasked with administering the tax law on remote firms. "Remote firms" are firms that are subject to tax in a particular jurisdiction but do not have a physical presence there. "Local firms" are firms with a physical presence in a jurisdiction. We use "remote firms" interchangeably with "out-of-state firms" and we use "local firms" interchangeably with "in-state firms."

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Sales taxes are economically meaningful, but understudied (Dyreng and Maydew, 2018). In 2019, state and local governments collected $434 billion in sales tax revenues (Urban Institute, 2021)—roughly double the $217 billion the federal government collected in corporate income tax revenues (St. Louis Fed, 2022)—and the sales tax accounted for approximately 31 percent of states' total tax revenues (Andersen et al., 2020). In Texas, the sales tax accounts for approximately 60 percent of total tax revenues (Texas Comptroller of Public Accounts, 2021).

Further, sales tax collections from *remote* firms have increased dramatically in recent years. Based on survey data, the GAO found that 33 states collected $23.1 billion in remote sales taxes in 2021, up from $6.7 billion collected by 28 states in 2019 (GAO, 2022).[2] In Texas, remote firms account for 18 percent of the sales tax base. The recent growth in remote taxation in the U.S. stems largely from a 2018 Supreme Court decision (South Dakota v. Wayfair, 2018), which drastically changed one of the primary tenets of interstate taxation. *Wayfair* overturned long-standing case law stipulating that firms must have a physical presence for a state to require sales tax collection and remittance. In response, each of the 45 states that impose a general sales tax rapidly adopted new laws that expanded tax collection obligations to remote firms.

Although a growing literature uses tax field experiments to better understand tax enforcement and compliance in local settings (see Mascagni (2018) and Slemrod (2019) for reviews), several key features of remote taxation highlight its unique incentives, costs, and challenges. First, because of information acquisition costs and compliance complexity, tax obligations may be less salient to remote firms.[3] The sales tax is inherently complex (e.g., Murray, 1995; Afonso, 2019) and administratively burdensome due to unique filing thresholds, tax rates, definitions, tax bases, and filing and payment methods that span over 11,000 sales tax jurisdictions (e.g., Yesnowitz, 2020). A recent survey found that small and midsized businesses spend on average 163 h and more than $17,000 per month on sales tax compliance (Cohn, 2021). Because of limited attention and resources, firms may focus their compliance efforts on more economically meaningful and salient states (e.g., local states) and accept the risk of non-compliance in others. For example, Expedia Group Inc. indicated in its 2019 10-K that rather than analyze all rules, it obtains the "advice of state and local experts with respect to tax laws of certain states and local jurisdictions that represent a large portion of [its] hotel revenue" and then posts reserves for potential adjustments required by other jurisdictions.[4]

Second, remote taxpayers may perceive a lower likelihood of detection and enforcement than local taxpayers. State tax authorities have limited resources, and they face higher costs and potentially lower expected payoffs if they pursue non-compliant out-of-state taxpayers rather than in-state taxpayers (Hellerstein, 2003; Swain, 2010). For example, if initial enforcement actions do not work, states have the authority to take further action, such as seizing assets (BDO, 2018). However, this legal process is more costly in a remote setting because states must follow the procedures of the Uniform Enforcement of Foreign Judgments Act to enforce judgments in the courts of other states (Lennihan, 2009; Thimmesch et al., 2018). Further, tax authorities must incur additional costs to find and seize property located in another state.

Third, remote taxpayers have different non-pecuniary incentives for tax compliance. Prior work has recognized that factors such as social norms or the receipt of public goods in exchange for tax dollars help explain rates of compliance (e.g., Erard and Feinstein, 1994; Hashimzade et al., 2013; Traxler, 2010; Luttmer and Singhal, 2014). But when firms are distant from the taxing jurisdiction or employees do not have voting power in the government, a feeling of greater social distance (Braithwaite, 2003) or a lack of perceived benefit from the public goods may affect compliance decisions. In sum, the unique features of the remote setting suggest the determinants of remote tax compliance may be fundamentally different from the determinants of local tax compliance.

We follow prior literature and use taxpayer correspondence (i.e., letters) as a way to understand and affect several key determinants of taxpayer compliance. Correspondence from a remote tax authority could increase compliance if it increases the salience of the tax obligation, increases perceived detection risk, or decreases the perceived social distance between the taxpayer and the remote authority. Conversely, tax authority correspondence sometimes leads to lower compliance or reduced payments among certain types of taxpayers (e.g., Slemrod et al., 2001; Gangl et al., 2014; Fellner et al., 2013; John and Blume, 2018; De Neve et al., 2021). For example, in multijurisdictional tax settings, some firms exhibit a "natural tendency … towards compliance" by paying "without thoroughly examining the strength of their legal position" (GAO, 2017 (p. 23)). Correspondence from a remote tax authority could weaken this natural tendency if it causes firms to re-evaluate their legal positions, detection risk, or the expected costs of non-compliance.

To answer our research questions, we first randomly select 2000 firms from the population of remote firms across the U.S. that (1) are registered to collect and remit Texas sales tax and (2) have at least one late filing in the past year.[5] To help identify the determinants of remote tax compliance and the most effective remote tax correspondence strategies, we randomly assign

[2] This estimate represents a lower bound of all remote sales tax collections because not all states are included in the report. Further, the estimate includes actual collections rather than theoretical obligations (i.e., it ignores the reporting gap due to non-compliance).

[3] We view information acquisition costs as a subset of compliance costs that include the cost of extracting and synthesizing all information necessary to comply with each state and local sales tax rule. Our definition is analogous to the definition of acquisition costs that Blankespoor et al. (2020) use in a disclosure and valuation setting.

[4] Appendix A contains several additional examples from firms' disclosures and other interested parties about the costs of complying with sales tax collection and remittance requirements.

[5] "Registered" firms have previously applied for and obtained a Texas sales tax permit. At the encouragement of the Texas Comptroller's office, we chose this sample because the firms were readily identifiable and arguably more likely to respond to the letters. Firms might also be non-compliant by making sales into Texas but never registering. Although this sample of firms would be interesting to examine and potentially economically meaningful, there is no clear way to identify these firms.

firms to one of five groups. Firms in the first four groups receive a different version of a letter from the Texas tax authority (treatment groups), while firms in the fifth group do not receive any letter (control group). The first treatment group receives a letter that is intended to increase the salience of the tax and reduce information acquisition costs. The letter informs the recipient they are registered to collect and remit sales tax in Texas, reminds them of the upcoming deadline, and provides a link to file and pay the tax online (*Inform*). The second treatment group's letter is intended to increase perceptions of detection risk and costs. The letter contains the *Inform* language, then indicates that tax returns will be closely examined and reminds taxpayers of penalties and interest associated with non-compliance (*Deterrence*). The third and fourth groups each receive a different letter that highlights a distinct non-pecuniary compliance incentive. The third treatment group's letter contains the *Inform* language, then indicates that the firm's tax payments help support the well-being of customers in Texas (*Fiscal Exchange*). The fourth treatment group's letter contains the *Inform* language, then indicates that a high percentage of Americans pay their taxes and believe it is a civic duty (*Social Norms*).

We first examine the effect of remote tax authority correspondence on firms' tax compliance. We employ a difference-in-differences test that compares the pre- and post-period tax compliance of out-of-state firms that received correspondence from the tax authority to those that did not. We examine two compliance-related outcomes: the taxable base (the amount on which the sales tax is levied) and timely tax filing. These measures reflect tax collections and whether the return is filed in a timely manner.

We find evidence that each correspondence strategy caused a significant short-term increase in remote firms' taxable bases. More specifically, the point estimates for all four treatment conditions suggest higher reported taxable bases relative to the no-letter control group, but only in the first full filing period after treatment. To assess the potential determinants of compliance, we examine differences across conditions. We find that the *Inform* strategy generated the largest increase in the reported tax base (141 percent); however, the incremental differences between *Inform* and the other treatment conditions are not significant.[6] We find limited evidence that any of the letters affected the likelihood of firms filing on time. The equality of coefficients on all treatment indicators suggests that remote firms respond to strategies that simply increase the salience of the tax, and that additional efforts by the tax authority to reference economic consequences or non-pecuniary factors provide no incremental benefit. The short-term nature of the effect also points to the letters perhaps having more of a salience effect rather than a learning effect. While we cannot directly observe firms' compliance processes, we would expect letters that induce real learning about tax laws and obligations to cause firms to make permanent changes to their systems (Manoli and Turner, 2014).

We next perform a cross-sectional test to better understand the role of salience in remote firms' responses to the tax authority. We repeat our difference-in-differences tests in retail firms and non-retail firms. Compared to non-retail firms (e.g., firms that provide web hosting or data processing services), retail firms have greater familiarity with sales taxes, pay them more regularly, have more resources dedicated to sales tax compliance, and face more consistent tax base rules across states (Hageman, 2012; Cohn, 2021; Airi and Sammartino, 2021). Therefore, tax authority correspondence could have stronger salience effects on remote non-retail firms than retail firms. We find that remote taxpayers in non-retail industries significantly increase the reported tax base in response to the letters. Conversely, we find no positive response to the correspondence among remote retail firms. Together with our main results, the evidence seems to suggest that salience is the primary channel through which correspondence from a remote tax authority improves compliance.

We also compare the experimental outcomes of our sample of remote firms to those of a matched pair sample of local firms, which helps isolate the effect of remoteness.[7] We find that out-of-state firms receiving *Inform*, *Deterrence*, and *Fiscal Exchange* letters report a significantly larger increase in the taxable base in the first full filing period after receiving the letters than paired in-state firms that receive similar letters. Again, the differences across letters are not statistically significant. Because local firms should be more familiar with the tax obligation, the incremental positive effect for remote firms across treatment groups provides corroborating evidence that salience is a key determinant of remote tax compliance.

Our paper makes several contributions to the academic literature and practice. First, in our remote sales tax setting, deterrence strategies and appeals to tax morale provide no incremental compliance benefit over simple reminders. Published field experiments in local settings that examine other tax types generally find that a specific taxpayer correspondence strategy is superior to others. Collectively, these studies suggest that deterrence or non-pecuniary factors are significant drivers of local tax compliance (Mascagni, 2018; Slemrod, 2019).[8] In contrast, our findings are most consistent with the notion that simply increasing the salience of a tax obligation has the largest effect on reported tax bases in a remote setting.

Second, this study's field experiment methodology is a powerful way to draw causal inferences, which are required for evidence-based policy- and decision-making (Leuz and Wysocki, 2016; Floyd and List, 2016; Mascagni, 2018). This design is also advantageous because it allows us to generate experimental data. Many studies in the tax literature are constrained by data availability, and therefore focus largely on public firms' income tax behavior. By partnering with a tax authority and

[6] Several unique features in our setting help explain the large taxpayer response (e.g., opportunities for misreporting, low and weak ex ante expectations of enforcement, etc.). We discuss these more in Section 6.

[7] In an ideal experiment, we would be able to randomize whether firms are local or remote. Because this type of assignment is not feasible, we consider this approach as second-best and note this as a potential limitation.

[8] Some studies in this area compare the effects of a specific letter strategy (e.g., deterrence) to a no-letter control group (e.g., Slemrod et al., 2001; Fellner et al., 2013). However, a more common approach is to compare the effect of a specific strategy on compliance incremental to the effect of an inform-only letter or generic reminder (e.g., Bott et al., 2020). Generally, the latter studies present evidence that an inform-only letter increases compliance relative to a no-letter control group, but that some specific strategy has an incremental effect above receiving an inform-only letter.

conducting a field experiment, we shed light on the determinants of remote sales tax compliance, which is a timely issue given the rapid growth of remote taxation following the *Wayfair* decision and the economic significance of sales taxes. Hence, our study uniquely advances the nascent literature on "non-income-based taxes" and "the taxation of business activities that span multiple jurisdictions" (Dyreng and Maydew, 2018).

Our study also offers practical insights to tax administrators that are reevaluating enforcement tactics in the post-*Wayfair* world. Specifically, we show that low-cost interventions can improve remote sales tax compliance—particularly when interventions target firms whose sales tax obligations are less salient. The returns to these interventions are significant. For the mean remote firm that received any letter, sales taxes paid to Texas increased by $3711 in the first filing period after the treatment, yet it only cost the state $0.55 in postage per letter plus a nominal amount of overhead to send the letters.[9]

2. Prior literature

An existing stream of literature utilizes field experiments to better understand tax compliance.[10] In these studies, researchers typically partner with tax administrators and alter taxpayer correspondence (e.g., letters) in one or more ways to create variation in their research instrument. Together, the variation in the instruments and taxpayer responses help identify determinants of taxpayer compliance, which can inform future models, research, and policy.

Many field experiments are motivated by Allingham and Sandmo's (1972) model of tax compliance, in which rational taxpayers consider the economic returns to evasion, taking into account the expected costs (i.e., the probability of detection and the magnitude of potential penalties) and benefits (i.e., based on the amount of income and the tax rate). These studies examine the influence of pecuniary-based "deterrence" strategies (i.e., the expected costs of non-compliance in the Allingham and Sandmo (1972) model). These strategies could include telling taxpayers that their tax returns will be closely examined (e.g., Slemrod et al., 2001), reminding taxpayers about penalties and interest (e.g., Perez-Truglia and Troiano, 2018), or providing taxpayers with information about audit probabilities (e.g., Kleven et al., 2011). The results generally support the hypothesis that increasing the perceived probability of detection or the salience of penalties reduces non-compliance (e.g., Fischer et al., 1992; Kleven et al., 2011; Mascagni et al., 2017; Perez-Truglia and Troiano, 2018; De Neve et al., 2021; Doerrenberg et al., 2022). However, in some circumstances or for some taxpayers, deterrence strategies do not work (e.g., Iyer et al., 2010; Ariel, 2012) and can even backfire, if for example, taxpayers believe an audit will not always result in a tax deficiency or view their filing as an opening bid in a negotiation (e.g., Slemrod et al., 2001; Gangl et al., 2014).[11]

Empirical studies present strong evidence that the Allingham and Sandmo (1972) economic model over-predicts non-compliance and that most taxpayers are not merely basing compliance decisions on the risk of detection and degree of punishment (e.g., Porcano, 1988; Erard and Feinstein, 1994; Andreoni et al., 1998). Researchers have updated models of non-compliance to include non-pecuniary variables (e.g., Traxler, 2010; Hashimzade et al., 2013; Luttmer and Singhal, 2014). Collectively, they refer to these factors as "tax morale" and offer several channels through which tax morale can manifest. Two common tax morale strategies used in field experiments include "fiscal exchange" and "social norms" appeals. The fiscal exchange strategy highlights the importance of taxes to fund public goods in the hopes that taxpayers will appreciate the goods provided by the government and reciprocate by contributing their legally-owed taxes. The social norm strategy highlights a particular group's typical behavior or beliefs about acceptable behavior as a form of peer pressure (e.g., that the majority of taxpayers are compliant or believe paying taxes is the right thing to do). Several field experiments find that tax morale-themed correspondence have positive effects on compliance (Del Carpio, 2014; Hallsworth et al., 2017; Shimeles et al., 2017; Mascagni et al., 2017; Perez-Truglia and Troiano, 2018; Bott et al., 2020), while other field experiments find zero effects (Blumenthal et al., 2001; Castro and Scartascini, 2015; Cranor et al., 2020) or even negative effects (Ariel, 2012; Fellner et al., 2013; John and Blume, 2018; De Neve et al., 2021).

Overall, the literature that uses field experiments has provided a growing, but still incomplete, understanding of the determinants of tax compliance. These studies reveal that tax compliance may be highly dependent on context and setting; hence, inferences drawn from the behavior of local taxpayers may not generalize to remote taxpayers. Specifically, in a *remote* setting, tax obligations may be less salient to taxpayers, detection and enforcement probabilities are likely lower, and tax payments may be less tied to tangible benefits of compliance (e.g., public services). Mascagni (2018) also cautions against comparisons across field experiments due to differences in jurisdictions and specific attributes of the manipulation. We therefore examine and test the factors that drive tax compliance in a remote setting.

[9] We detail this calculation in Section 6. We caution that results from this field experiment may not scale when interventions are applied to larger populations, which is an inherent issue in many field experiments (List, 2022). Relatedly, our study does not speak to the potential effect of repeated interventions.

[10] Mascagni (2018) provides a detailed review of this literature.

[11] Slemrod et al. (2001) sent a deterrence letter to various taxpayers and found that low- and middle-income taxpayers increased reported taxable income after the letter, relative to a control group. High income taxpayers, however, reported less income. Ex post, the authors create a model that could rationally explain this unexpected result if high income individuals believe that (1) the audit will not detect or punish all tax evasion, and (2) the final outcome is influenced by the initial reported income.

3. Setting and hypothesis development

3.1. Setting

Texas is a large economy that depends heavily on sales taxes (the sales tax accounted for approximately 60 percent of the state's tax revenue in 2020—nearly double the national average of approximately 31 percent).[12] Texas imposes a 6.25 percent state sales tax on all retail sales, leases and rentals of most goods, as well as certain services. The 6.25 percent state rate generally applies to all taxable sales. Local jurisdictions, such as cities or counties, can impose up to an additional 2 percent for a maximum combined rate of 8.25 percent. Businesses that are subject to tax in the state (i.e., those that have "nexus") must collect and remit sales taxes on taxable goods and services provided to Texas customers.[13] Taxpayers generally must file and pay either monthly or quarterly, depending on the amount of their taxable sales (i.e., taxpayers who have $1500 or more in state sales and use tax per quarter must file monthly). Reports are due on the 20th day of the month following the reporting period. For example, the report due July 20th covers the June period for monthly filers and the April, May, and June period for quarterly filers. Texas offers a variety of ways to file and pay, but large taxpayers are required to file and pay online. Online Appendix A provides additional details about Texas sales taxes.

3.2. Hypothesis development

Murray (1995) models a firm's decision to comply with sales tax collection and remittance requirements and notes numerous opportunities for non-compliance. Firms might fail to collect the appropriate amount of sales tax, either deliberately (e.g., to charge lower prices than competitors or avoid consumer backlash (Hoopes et al., 2016; Baugh et al., 2018)) or inadvertently (Murray, 1995). In more extreme cases, firms may collect sales taxes but not remit them (Christian, 2016).[14] As in other crime-based models of tax compliance, the choice of how much income to report is a function of the economic returns to evasion, taking into account the costs and the benefits. As with other taxes, observed levels of sales tax compliance are higher than the model predicts, suggesting that taxpayers comply for reasons beyond expected financial payoffs. In the remote setting, compliance becomes particularly complex and costly because firms face different tax rules and reporting requirements in multiple jurisdictions (GAO, 2017; Afonso, 2019; Yesnowitz, 2020).

Researchers and tax authorities use several general correspondence strategies to influence compliance and understand its drivers. One common form of taxpayer correspondence is a letter that simply informs the taxpayer they have an upcoming filing obligation. The effect of receiving this type of correspondence from a remote authority on tax compliance is not clear ex ante. To the extent that receiving remote correspondence increases the salience of the tax obligation or reduces information acquisition costs, then remote correspondence could have a positive effect on compliance. However, correspondence could cause firms to report less than they were previously reporting. For example, to the extent that remote firms have a tendency for or default to compliance (GAO, 2017), correspondence could cause firms to more rigorously evaluate their behavior and detect overpayments or unnecessary compliance, which would reduce future payments. The letter could also trigger one or more of the backfiring effects found in prior literature. We therefore state our first hypothesis in null form.

H1. *Correspondence from a remote tax authority does not change taxpayer compliance.*

We next discuss hypotheses for specific correspondence strategies that are incremental to simply informing taxpayers of a potential filing obligation. These hypotheses also help us consider the various potential determinants of remote taxpayer compliance.

Deterrence-themed correspondence from a remote authority may cause firms to revise upward their priors of detection probability and the expected cost of non-compliance, which should lead to an increase in compliance. However, a deterrence strategy could also backfire in a remote setting. For example, deterrence correspondence could cause firms to solely base their compliance decision on the likelihood and cost of enforcement, which is relatively low in a remote setting. Rottenstreich and Kivetz (2006) argue that "the relative salience of probabilistic and non-probabilistic cues is an important factor" in the tendency to use a probabilistic or non-probabilistic mindset. Applied to our setting, this could mean that the use of certain deterrent cues could put taxpayers in a probabilistic (i.e., deterrence-based) mindset and thereby dampen the effectiveness of any non-pecuniary incentives that contribute to voluntary compliance. In this vein, Mascagni et al. (2017) note that although deterrence can be highly effective, it faces "limitations in low enforcement environments" and "may even be counterproductive

[12] Texas has the second largest state economy in the U.S. following California. If Texas were its own nation, it would be the world's 9th largest economy based on GDP (Texas Economic Development Corporation, 2021).

[13] Beginning October 1, 2019, all remote firms (those without a physical presence in Texas) must register to collect and remit Texas sales taxes if they have total Texas revenue of at least $500,000 in the preceding twelve calendar months. Prior to October 1, 2019, some firms without a physical establishment in the state may have been required to register for other reasons (e.g., traveling salesperson, equipment rental, etc.). Therefore, our remote sample includes firms both above and below the $500,000 Texas sales threshold. Among all registered *local* firms, firms with at least $500,000 in total annual Texas revenue represent only 18 percent of firms but account for 96 percent of the tax base.

[14] Firms generally face stronger consequences for collecting and not remitting (i.e., stealing) than simply not collecting the tax. For example, in Texas the criminal offense for intentionally or knowingly failing to remit sales taxes that were collected can range from a misdemeanor to a first-degree felony (i.e., jail time and fines) depending on the unremitted amount (Texas Tax Code, Section 151.7032).

… because it may reinforce distrust and taxpayer resistance" (p. 8). The deterrence strategy could therefore crowd out intrinsic motivations (e.g., Del Carpio, 2014) or inadvertently draw attention to the remote authority's lack of coercive power (French and Raven, 1959).

Based on the mixed empirical evidence and the uncertain effect that a remote authority's deterrence strategies can have on compliance, we state the following hypothesis in the null.

H2a. *Deterrence-themed correspondence from a remote tax authority does not change taxpayer compliance.*

In our experiment, we include two common tax morale strategies: fiscal exchange and social norms. Taxpayers might respond positively to tax morale strategies from a remote authority. For example, if the letter upwardly revises the taxpayer's prior beliefs regarding fiscal exchange with the tax authority, compliance may increase. Likewise, social norms could have a positive effect if taxpayers have low priors with respect to their peers' level of compliance.

On the other hand, a remote authority's appeal to tax morale could have a negative effect. Moral appeals might signal that the tax authority has little or no enforcement power and therefore must resort to rhetoric (Bardach, 1989). In the case of fiscal exchange, the letter could focus remote taxpayers' attention on the minimal benefit they receive from paying taxes to a remote government and therefore cause a decrease in compliance. Specifically, the letter could highlight the fact that taxes only support the public goods of a remote state rather than the firm's most direct public goods (e.g., health care, public safety, and transportation for its employees and local stakeholders) and that employees of the remote firm have no direct input on the local government's spending decisions. In the case of social norms, taxpayers may not feel connected to (or even feel aversion towards) the reference group, and therefore decrease compliance. Because of the mixed empirical results from the literature and the theoretical arguments above, we state our next two hypotheses related to tax morale in the null.

H2b. *Fiscal-exchange-themed correspondence from a remote tax authority does not change taxpayer compliance.*

H2c. *Social-norm-themed correspondence from a remote tax authority does not change taxpayer compliance.*

4. Sampling and randomization

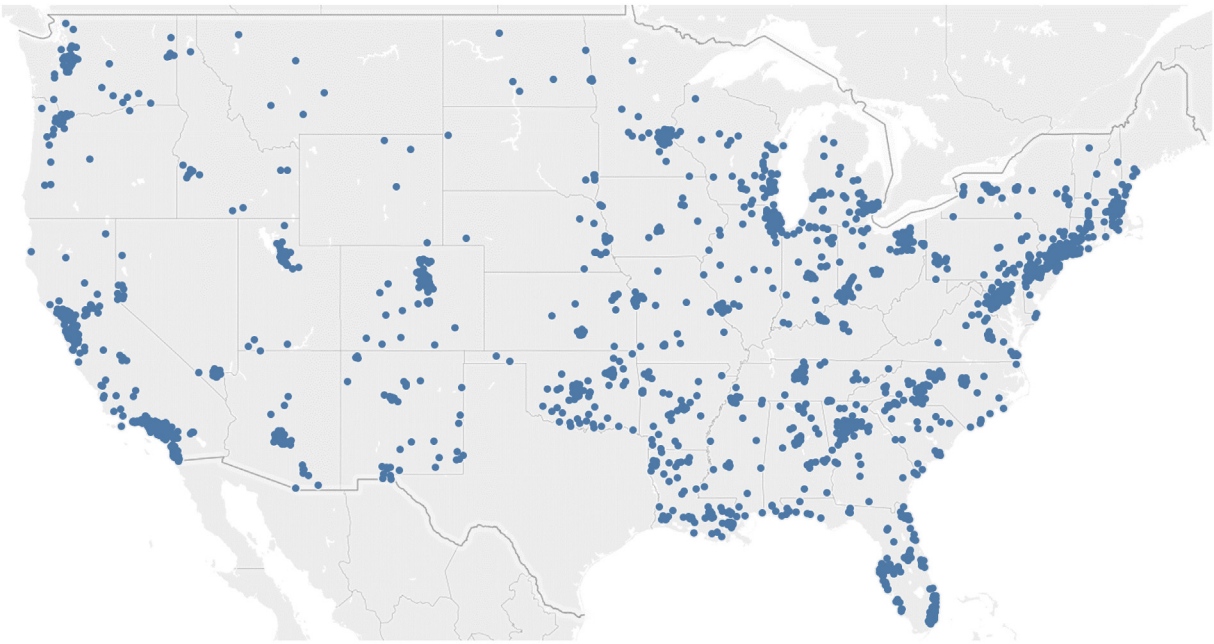
In the spring of 2021, we coordinated with the Texas Comptroller's office to identify a sample of 2000 remote taxpayers that were registered to pay sales taxes but had no reported physical presence or mailing address in Texas. The Comptroller's office randomly selected 1000 out-of-state firms from two distinct groups that showed signs of sales tax delinquency in the past year—late filers and extra-late filers—for a total out-of-state sample of 2000 firms. Late filer taxpayers are those that submit their return or payment after the due date, but no later than 20 days (45 days) late for monthly (quarterly) taxpayers. Extra-late filer taxpayers are monthly (quarterly) taxpayers that fail to submit their return or payment within 20 days (45 days) of the due date.[15] The groups were defined by the Comptroller's office and were based on firms' sales tax filings during calendar year 2020. 61 percent of firms in our sample are monthly filers and 39 percent are quarterly filers.

We present several figures and descriptive statistics to provide context for our sample and illustrate how it fits into the full population of firms registered for Texas sales tax. Fig. 1 presents the location of each remote firm in our sample and shows that firms are distributed across the country. Table 1 presents the composition of our sample by industry and shows that sample firms are also distributed across industries (e.g., less than 30 percent of the sample firms are in retail industries). In Fig. 2, Panels A and B, we show the Texas tax base and number of firms, broken out by the full population of registered in-state and out-of-state firms.[16] Out-of-state firms comprised seven percent of the population of registered firms and a material portion of the Texas tax base, at 18 percent. In Panels C and D, we show the Texas tax base and the number of firms, respectively, broken out by out-of-state late filers (including both late and extra-late) and timely filers. 35 percent of out-of-state firms had at least one late filing in 2020 and comprised 6 percent of the total reported out-of-state tax base.

We randomly assigned each remote firm to one of five conditions: (1) control; (2) inform; (3) deterrence; (4) fiscal exchange; and (5) social norms. In performing the randomization, we block randomized by filing status and industry, which ensures that our outcomes are not driven by a filing status or industry being overly represented in any particular condition, and increases power (Athey and Imbens, 2017). The control firms in condition (1) did not receive any letter. The firms in conditions (2) through (5) received letters that reminded them of their sales tax obligation and upcoming deadline. The *Inform* letter reminded firms of the upcoming deadline and provided a link to file and pay online. The remaining letters were identical to *Inform*, except that they also included a brief message related to deterrence, fiscal exchange, and social norms, respectively. The purpose of the *Inform* condition was to isolate the salience effect of receiving any letter from the tax authority, given that simple reminders can account for some of taxpayers' behavioral response (Del Carpio, 2014). The

[15] The Comptroller's classification of extra-late filers also includes firms that do not file at all. These observations represent fewer than 1 percent of the total filings in our sample and are coded as missing for our empirical tests.

[16] We note that the out-of-state population in Fig. 2 includes a number of very small firms that file annually, and that the Comptroller's office excluded these firms from the study population because of their size and the long gap between when they would have received the letter (July 2021) and when they are required to file (every January). The population of registered out-of-state quarterly and monthly late filers in 2020 consisted of approximately 10,000 firms. The sample size of 2000 represents approximately 20 percent of this population and was the maximum number of firms that the Comptroller's office was willing to include, given anticipated resource constraints.



Notes: This figure plots the location of remote sample firms. To preserve the scale of the image, we omit two out-of-state state firms in Hawaii from this figure.

Fig. 1. Location of sample firms.

Deterrence letter indicated that sales tax filings would be closely examined, and reminded the taxpayer of penalties and interest for past-due taxes. The *Fiscal Exchange* letter highlighted that sales tax revenues fund a significant portion of public services and contribute to the economic and physical well-being of the firm's customers in Texas. The *Social Norms* letter informed the firm that the majority of Americans believe that paying taxes is a civic duty and that the U.S. is consistently among the world leaders regarding voluntary tax compliance. The exact content of the letters is included in Appendix B.

Because we randomly assign firms to conditions, the only expected difference between groups is whether or not they receive treatment. In Table 2, we present descriptive statistics and examine sample balance on observable characteristics. Panel A contains the full sample; Panel B breaks out the full sample into separate treatment and control groups; and Panel C presents tests of pre-existing differences in outcomes between groups. In each panel there is one observation per firm, representing that firm's aggregate 2020 filing information, which is the period from which sample firms were selected. Our dependent variables in regression analyses are the natural logarithm of one plus the taxable base (*Log(1 + TaxBase)*) and an indicator variable for whether a return is filed on time (*Timely*).[17] *TaxBase* is computed as the sum of Texas taxable sales and Texas taxable purchases, and is the base upon which the 6.25 percent state tax rate (plus the applicable local rate) is applied to compute firms' tax liabilities.[18]

In Panel A, we show that remote firms have a mean annual *TaxBase* of $500,942 and mean annual total Texas sales (*TotalSales*) of $1,275,781. *TotalSales* is the amount of firms' total sales in Texas, including both taxable and non-taxable sales. This variable represents the best available proxy for size, which we use in subsequent analyses for matching purposes. The mean (median) of *Timely* is 0.66 (0.75), which indicates that the average firm usually filed on time. In Panel C, we test the differences between each treatment group and the control group for *TaxBase*, *TotalSales*, and *Timely*. Generally, the groups are balanced. However, we do find that firms in the *Social Norms* treatment group report lower sales.[19] Our research design, discussed in Section 5, should alleviate concerns about pre-existing differences biasing estimates.

[17] Because the distribution of *TaxBase* is right-skewed and contains a non-trivial amount of zeroes, in an alternative specification we measure the dependent variable using the inverse hyperbolic sine transformation (instead of the natural log of one plus the amount) and draw similar inferences (untabulated).

[18] We use *TaxBase* as opposed to tax payments because the payments data contains payments/adjustments from other periods that cannot be identified. "Taxable purchases" include taxable items that the firm purchased, leased or rented for use in Texas on which sales tax was not paid. Taxable purchases are included in the tax base because firms have to remit use taxes on these purchases (the use tax rate is the same as the sales tax rate). Remote taxpayers may have taxable purchases if, for example, they have a traveling salesperson who purchased items for use in Texas or if the taxpayer purchases or rents equipment for use in Texas. Approximately 2.55 percent of firms in the sample have taxable purchases, and taxable purchases represent 0.85 percent of *TaxBase* (untabulated).

[19] Random assignment, by its nature, implies that some differences between groups occur by chance. Further, as Hallsworth et al. (2017) notes, "imbalances are an example of the inherent challenge of working with existing government processes in a live public environment."

Table 1
Industry composition.

NAICS code	NAICS description	Count	Percent
11	Agriculture, forestry, fishing and hunting	2	0.10%
21	Mining, quarrying, and oil and gas extraction	3	0.15%
23	Construction	178	8.90%
31	Manufacturing	75	3.75%
32	Manufacturing	50	2.50%
33	Manufacturing	193	9.65%
42	Wholesale trade	284	14.20%
44	Retail trade	193	9.65%
45	Retail trade	395	19.75%
48	Transportation and warehousing	1	0.05%
51	Information	86	4.30%
52	Finance and insurance	12	0.60%
53	Real ESTATE AND RENTAL AND LEASING	44	2.20%
54	Professional, scientific, and technical services	269	13.45%
55	Management of companies and enterprises	1	0.05%
56	Admin, support, waste mgmt, and remediation services	91	4.55%
61	Educational services	6	0.30%
62	Health care and social assistance	2	0.10%
71	Arts, entertainment, and recreation	15	0.75%
72	Accommodation and food services	20	1.00%
81	Other services (except public administration)	79	3.95%
92	Public administration	1	0.05%
Total		**2000**	**100.00%**

Notes: This table shows industry composition (by 2-digit NAICS sector) of out-of-state firms in our sample.

The Texas Comptroller's office sent out letters in early July 2021 via common carrier. Firms should have received the letter at least ten days prior to the filing deadline of July 20th for both monthly and quarterly filers. We then tracked our dependent variables of interest over the next three quarters (through January 2022), giving us seven post-period observations for monthly filers and three post-period observations for quarterly filers.

5. Research design

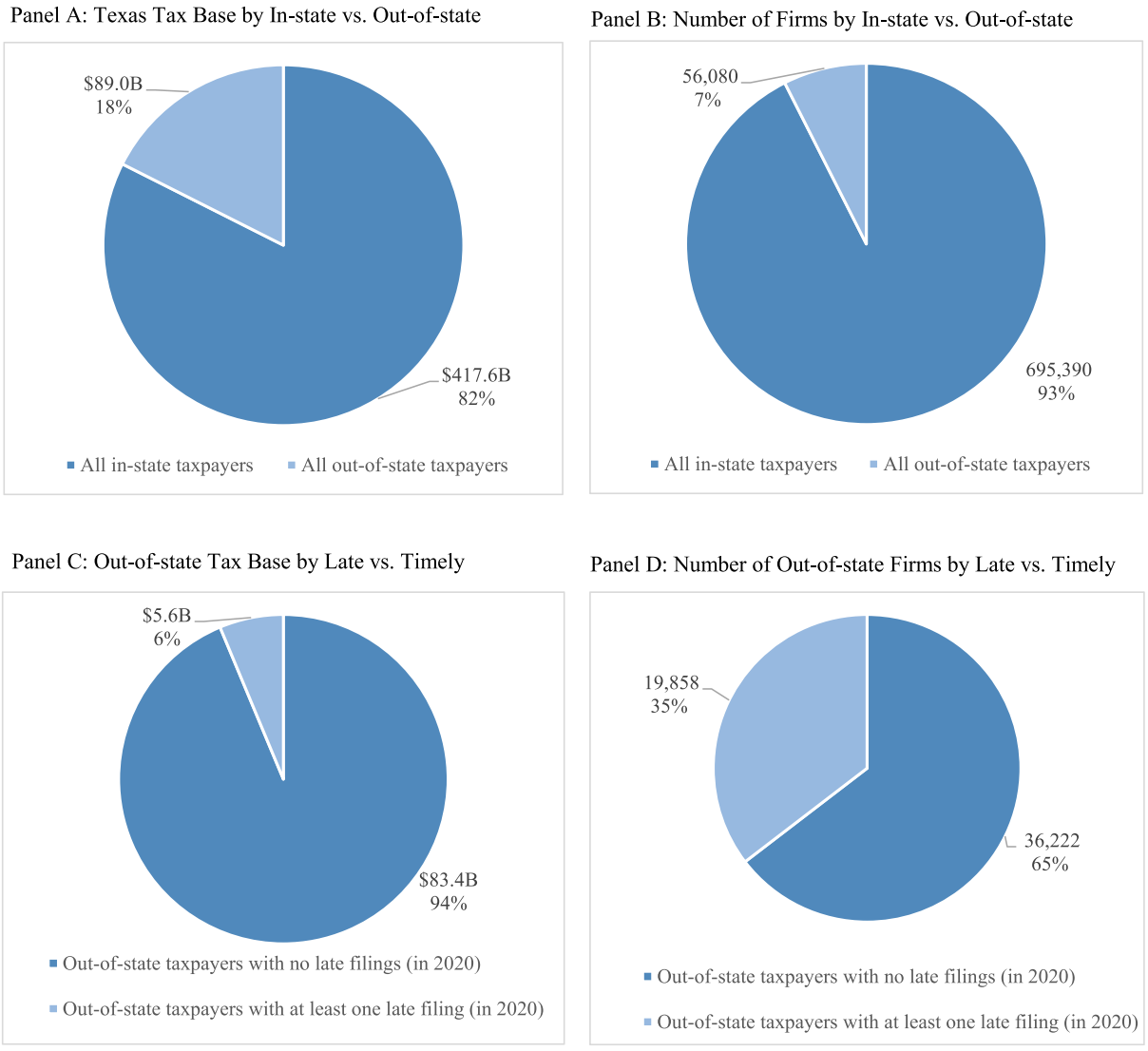
We first examine the effect of receiving letters from a remote tax authority on a taxpayer's behavior using the following difference-in-differences model, which compares the within-firm change in tax compliance for remote firms that receive one of the four letters to control firms that do not receive a letter:

$$\begin{aligned} \text{TaxCompliance}_{it} = {} & \gamma_i + \delta_t + \beta_1 \text{Inform*Post}_{it} + \beta_2 \text{Deterrence*Post}_{it} + \beta_3 \text{Fiscal Exchange*Post}_{it} \\ & + \beta_4 \text{Social Norms*Post}_{it} + \varepsilon_{it} \end{aligned} \tag{1}$$

TaxCompliance is one of our two dependent variables, *Log(1 + TaxBase)* and *Timely*. We follow prior literature and assume that any increase in the taxable base reflects a reduction in underreporting and therefore an increase in compliance (e.g., Slemrod et al., 2001). *Inform*, *Deterrence*, *Fiscal Exchange*, and *Social Norms* are indicator variables that represent the four letter conditions. *Post* is an indicator variable equal to one for observations from July 2021 to January 2022, and zero for observations before July 2021. Because we include firm fixed effects γ_i and time fixed effects δ_t (using tax return due dates), the main effects for each letter condition and *Post* are subsumed. Our parameters of interest are the difference-in-differences estimators for each letter condition (*Inform*Post*, *Deterrence*Post*, *Fiscal Exchange*Post*, *Social Norms*Post*). The coefficients estimate the effect of receiving a letter relative to the control group that did not receive any letter. Appendix C defines all variables.

Although random assignment should minimize differences across conditions, the difference-in-differences design should reduce any remaining concerns that pre-existing differences could drive the results. The coefficients on β_1 through β_4 represent the average difference between pre- and post-period outcomes for a firm receiving a specific treatment letter compared to the average difference between pre- and post-period outcomes for a firm that did not receive a letter. By including firm fixed effects, we control for potential time-invariant observable and unobservable firm characteristics. That is, firm fixed effects allow firms to have different average outcomes and characteristics. For example, it is unlikely that a variable like size could influence our results, even if one group is smaller than the rest (as *Social Norms* is), because differences in size are unlikely to change significantly over the short horizon of our study and are therefore controlled for by the firm fixed effects.

To examine firms' dynamic responses to the treatment over time, we organize our data in event time by tax return deadline ("filing period") and then estimate equation (1) separately for each filing period. We partition the sample into filing periods because our sample contains both monthly filers (which file a sales tax return every month) and quarterly filers



Notes: This figure provides statistics from the Comptroller's office on the universe of registered Texas sales tax filers during calendar year 2020. "In-state" firms include firms that have *any* physical presence in Texas, and is therefore slightly different from the definition of "local" firms used in our analyses (i.e., firms with a primary address in Texas). "OOS" refers to out-of-state taxpayers and is defined the same way as the rest of this study (i.e., firms with no physical presence in Texas).

Fig. 2. Statistics on registered Texas sales tax filers.

(which file a sales tax return in January, April, July, and October). Filing period 0 therefore includes the July 2021 returns for both monthly and quarterly filers because both groups file a sales tax return in July. Filing period 1 consists of the August 2021 returns for monthly filers and the October 2021 returns for quarterly filers. Filing period 2+ covers the remainder of our sample period and includes September 2021 through January 2022 returns for monthly filers and January 2022 returns for quarterly filers. To account for seasonality, the pre-period in each regression is the same month or quarter from the previous year (e.g., July 2020 for filing period 0).

6. Results

We present estimates of equation (1) in Table 3, Panel A. The six columns represent separate regressions for each of our two dependent variables and the three filing periods in which we partition the sample. In period 0 we find positive point estimates for each letter condition on both *Log(1 + TaxBase)* and *Timely*, although none of the coefficients are statistically

Table 2
Descriptive statistics.

Panel A: Remote Firms

Variable	N	Mean	Std. Dev.	p25	p50	p75
TaxBase	2000	500,942	1,194,564	3893	64,535	400,252
Log(1 + TaxBase)	2000	9.58	4.79	8.27	11.07	12.90
TotalSales	2000	1,275,781	3,031,803	31,882	218,580	1,043,139
Log(1 + TotalSales)	2000	12.03	2.48	10.37	12.29	13.86
Timely	2000	0.66	0.31	0.50	0.75	0.92
Filing History (years)	2000	7.78	8.12	2.14	4.55	10.56
Distance (miles)	2000	1078	401	821	1075	1433

Panel B: Remote Firms by Letter Condition

Variable	N	Mean	Std. Dev.	p25	p50	p75
Letter: *Inform*						
TaxBase	400	476,413	1,143,982	6392	98,852	464,423
Log(1 + TaxBase)	400	10.01	4.43	8.76	11.50	13.05
TotalSales	400	1,383,894	3,307,960	29,276	214,292	953,102
Log(1 + TotalSales)	400	12.03	2.50	10.28	12.28	13.77
Timely	400	0.64	0.33	0.42	0.75	0.92
Filing History (years)	400	7.43	7.84	2.11	4.44	9.98
Distance (miles)	400	1109	400	889	1104	1475
Letter: *Deterrence*						
TaxBase	400	461,833	1,019,260	3297	58,044	368,179
Log(1 + TaxBase)	400	9.28	5.01	8.10	10.97	12.82
TotalSales	400	1,173,067	3,115,992	29,577	180,801	892,848
Log(1 + TotalSales)	400	11.90	2.48	10.29	12.11	13.70
Timely	400	0.68	0.29	0.50	0.75	0.92
Filing History (years)	400	8.36	8.75	2.30	4.54	12.04
Distance (miles)	400	1080	400	839	1071	1421
Letter: *Fiscal Exchange*						
TaxBase	400	548,179	1,249,458	5468	72,177	466,533
Log(1 + TaxBase)	400	9.84	4.71	8.61	11.19	13.05
TotalSales	400	1,283,395	2,897,960	38,291	258,094	1,179,531
Log(1 + TotalSales)	400	12.11	2.45	10.55	12.46	13.98
Timely	400	0.66	0.31	0.50	0.75	0.92
Filing History (years)	400	7.71	8.02	2.15	4.72	11.06
Distance (miles)	400	1066	409	816	1059	1404
Letter: *Social Norms*						
TaxBase	400	424,296	1,040,142	4327	53,726	314,636
Log(1 + TaxBase)	400	9.50	4.70	8.37	10.89	12.66
TotalSales	400	1,066,230	2,399,154	29,584	190,413	870,199
Log(1 + TotalSales)	400	11.94	2.39	10.29	12.16	13.68
Timely	400	0.65	0.31	0.48	0.75	0.92
Filing History (years)	400	7.42	7.55	2.12	4.72	10.01
Distance (miles)	400	1068	383	820	1072	1432
Letter: None (*Control*)						
TaxBase	400	593,988	1,462,753	1965	50,157	429,983
Log(1 + TaxBase)	400	9.29	5.07	7.58	10.82	12.97
TotalSales	400	1,472,319	3,337,675	35,893	272,978	1,228,501
Log(1 + TotalSales)	400	12.15	2.60	10.49	12.52	14.02
Timely	400	0.66	0.31	0.50	0.75	0.92
Filing History (years)	400	8.00	8.40	2.14	4.55	10.75
Distance (miles)	400	1066	410	805	1084	1442

Panel C: Tests for Differences

Letter Condition	Diff	T-stat
T-test for differences in *TaxBase* (*Control* relative to):		
Inform	117,576	1.266
Deterrence	132,155	1.483
Fiscal Exchange	45,809	0.476
Social Norms	169,692*	1.891
T-test for differences in *TotalSales* (*Control* relative to):		
Inform	88,425	0.376
Deterrence	299,252	1.311
Fiscal Exchange	188,924	0.855
Social Norms	406,089**	1.976

Table 2 (*continued*)

Panel C: Tests for Differences		
Letter Condition	Diff	T-stat
T-test for differences in *Timely* (*Control* relative to):		
Inform	0.017	0.754
Deterrence	−0.024	−1.123
Fiscal Exchange	−0.002	−0.094
Social Norms	0.012	0.552

Notes: This table presents descriptive statistics for the remote taxpayers in our sample. All panels include full-year data for late filers and extra-late filers. Full-year data for quarterly filers includes aggregate sales tax return figures over the period January 1, 2020 to December 31, 2020. Full-year data for monthly filers includes aggregate sales tax return figures over the period March 1, 2020 to February 28, 2021. These full year-data reflect the period from which sample firms were selected and subsequently matched to local firms. In Panel C, we test for differences between each treatment condition and the control condition. Appendix C defines all variables.

significant. In period 1, the first full period after the treatment, we find large and statistically significant effects of all four treatment conditions on *Log(1 + TaxBase)*, but no significant effects on *Timely*. We find the largest effect in the *Inform* condition—a coefficient of 0.88, which represents an increase of 141 percent—followed by *Deterrence* (116 percent), *Fiscal Exchange* (88 percent), and *Social Norms* (86 percent).[20] The differences across treatment conditions are not statistically significant (untabulated). In filing period 2+, the coefficients become much lower and are not statistically significant. Across all filing periods and conditions, we find little effect of the treatment on *Timely* filing.

In Panels B and C of Table 3, we break out monthly filers separately from quarterly filers. We draw similar inferences for each type of filer, although the significance level on some estimates drop below traditional levels on some variables (e.g., *Fiscal Exchange* and *Social Norms* among monthly files and *Deterrence* for quarterly filers). For the remaining analyses, we only tabulate the grouped specification because it matches taxpayers' normal filing cycles, offers parsimonious interpretation, and maximizes statistical power.[21]

In general, an increase in compliance could reflect one or more of the following mechanisms. If a firm had already been collecting sales taxes from customers but were not remitting them to tax authorities, then the firm could simply start remitting. If a firm had not been collecting sales taxes, then it could (1) try to collect the tax from customers retroactively and remit those taxes, (2) remit taxes due out of cash on hand without charging customers, or (3) begin collecting taxes from customers and remitting in the next filing period.

Our data do not allow us to conclusively determine the extent of each mechanism. However, the letter was sent in July, which is after reporting period 0 ended (i.e., after the period in which the taxable activity actually occurred), but before the filing deadline for reporting period 0. Hence, the delayed effect across Table 3 is most consistent with the average taxpayer increasing compliance by collecting and remitting taxes in the next filing period.

We also present the effects of the treatment on the taxable base visually in Fig. 3, with separate panels for each panel of Table 3 (full sample in Panel A, monthly filers in Panel B, quarterly filers in Panel C). Each point represents the difference-in-differences coefficients from Panel A of Table 3, with two modifications. First, for parsimony, we combine the four treatment conditions into a single variable, "Letter." Second, we add coefficients from separate regressions that examine filing periods −2 and −1 to assess whether pre-existing differences drive the results.[22] The filing period −2 coefficients estimate the difference in *Log(1 + TaxBase)* for treatment and control firms in May 2021 versus May 2020 for monthly filers, and January 2021 versus January 2020 for quarterly filers. The coefficients in filing period −1 estimate the difference in *Log(1 + TaxBase)* for treatment and control firms in June 2021 versus June 2020 for monthly filers, and April 2021 versus April 2020 for quarterly filers.

As expected, the points at filing periods −2 and −1 show that in the months before treatment, there were no significant differences in the reported tax base from 2020 to 2021 for treatment firms relative to control firms. Results are generally consistent across panels. In Fig. 3, Panel A, we find that receiving any letter increases the tax base by 105 percent (exp(0.72) − 1 = 1.05), on average, in the first full filing period after receiving the letter. This estimate implies that firms on

[20] For example, Exp(0.88) − 1 = 1.41.

[21] Specifically, the letter mentions the firm's filing frequency and firms will likely assume the letters apply to the most immediate filings after receiving the letter. Indeed, the observed results suggest that taxpayers' responses are a function of the passage of a filing obligation rather than the passage of calendar time. Both groups exhibit a sharp taper in the estimated coefficients after filing period 1. Hence, we aggregate periods 2+ with no loss of generality other than a one-period positive treatment effect in November among monthly filers. Additionally, results from subsequent analyses are generally consistent across filing types. In Online Appendix Tables OA.1 and OA.2, we present results from Tables 4 and 5 separately for monthly and quarterly filers.

[22] Because our field experiment uses random assignment, pre-existing trends or differences should not be a concern. However, out of an abundance of caution we still present results from the pre-period. Filing periods −2 and −1 effectively serve as placebo difference-in-differences tests to see if treated firms report a higher tax base in 2021 versus 2020 during the months before receiving the letter, relative to control firms.

Table 3
Remote tax compliance.

Panel A: Full Sample

Variable	Reporting Period 0		Reporting Period 1		Reporting Period 2+	
	(1)	(2)	(3)	(4)	(5)	(6)
	Log(1 + TaxBase)	*Timely*	*Log(1 + TaxBase)*	*Timely*	*Log(1 + TaxBase)*	*Timely*
Inform × *Post*	0.39	0.04	0.88***	0.01	0.20	0.02
	(1.21)	(0.90)	(2.79)	(0.39)	(0.86)	(1.02)
Deterrence × *Post*	0.28	0.01	0.77**	−0.06	−0.02	−0.03
	(0.95)	(0.35)	(2.50)	(−1.60)	(−0.07)	(−1.45)
Fiscal Exchange × *Post*	0.43	0.01	0.63**	−0.01	−0.22	−0.04*
	(1.39)	(0.32)	(2.15)	(−0.33)	(−0.96)	(−1.68)
Social Norms × *Post*	0.44	0.05	0.62**	0.03	0.10	0.01
	(1.43)	(1.15)	(2.02)	(0.76)	(0.44)	(0.31)
Firm Fixed Effects	Y	Y	Y	Y	Y	Y
Due Date Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	3716	3716	3650	3650	13,021	13,021
R-Squared	0.820	0.634	0.826	0.626	0.671	0.429

Panel B: Monthly Filers

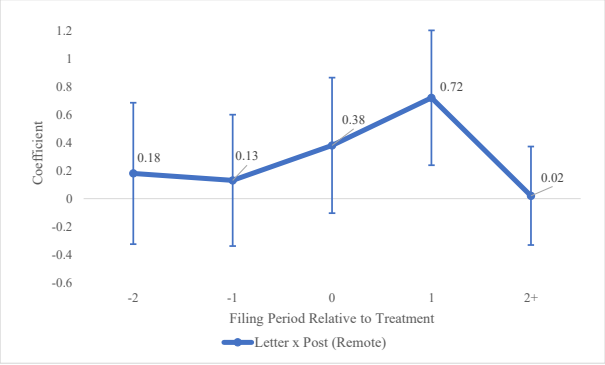
Variable	July		August		September–January	
	(1)	(2)	(3)	(4)	(5)	(6)
	Log(1 + TaxBase)	Timely	Log(1 + TaxBase)	Timely	Log(1 + TaxBase)	Timely
Inform × *Post*	0.39	0.03	0.83**	0.03	0.18	0.03
	(0.94)	(0.73)	(1.99)	(0.65)	(0.72)	(1.19)
Deterrence × *Post*	0.23	0.01	0.89**	−0.04	0.02	−0.03
	(0.57)	(0.19)	(2.11)	(−1.00)	(0.07)	(−1.17)
Fiscal Exchange × *Post*	0.60	0.03	0.43	−0.03	−0.28	−0.03
	(1.47)	(0.76)	(1.11)	(−0.74)	(−1.13)	(−1.41)
Social Norms × *Post*	0.51	0.04	0.54	0.03	0.06	0.00
	(1.24)	(0.91)	(1.32)	(0.68)	(0.26)	(0.09)
Firm Fixed Effects	Y	Y	Y	Y	Y	Y
Due Date Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	2366	2366	2358	2358	11,855	11,855
R-Squared	0.755	0.626	0.760	0.606	0.632	0.390

Panel C: Quarterly Filers

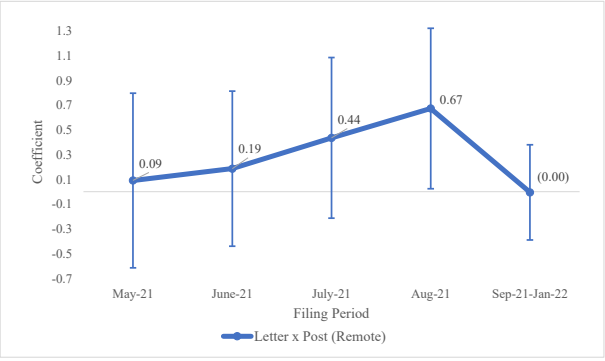
Variable	July		October		January	
	(1)	(2)	(3)	(4)	(5)	(6)
	Log(1 + TaxBase)	Timely	Log(1 + TaxBase)	Timely	Log(1 + TaxBase)	Timely
Inform × *Post*	0.37	0.06	0.96**	−0.01	0.41	−0.01
	(0.78)	(0.73)	(2.09)	(−0.19)	(0.81)	(−0.21)
Deterrence × *Post*	0.37	0.02	0.55	−0.09	−0.30	−0.08
	(0.91)	(0.32)	(1.36)	(−1.26)	(−0.66)	(−1.10)
Fiscal Exchange × *Post*	0.12	−0.02	0.99**	0.03	0.39	−0.07
	(0.27)	(−0.26)	(2.36)	(0.35)	(0.80)	(−0.98)
Social Norms × *Post*	0.32	0.05	0.75*	0.03	0.47	0.06
	(0.72)	(0.72)	(1.71)	(0.39)	(0.93)	(0.91)
Firm Fixed Effects	Y	Y	Y	Y	Y	Y
Due Date Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	1350	1350	1292	1292	1166	1166
R-Squared	0.795	0.601	0.828	0.613	0.832	0.652

Notes: This table presents results from estimating a difference-in-differences model (equation (1)) examining the effect of letter correspondence on remote tax compliance. Panel A includes the full sample; Panel B includes only monthly filers; and Panel C includes only quarterly filers. In Panel A, columns 1 and 2 contain observations from filing period 0 (July 2021) and its pre-period; columns 3 and 4 contain observations from filing period 1 (August 2021 for monthly filers, October 2021 for quarterly filers) and its pre-period; columns 5 and 6 contain observations from filing period 2+ (September 2021–January 2022 for monthly filers, January 2022 for quarterly filers) and its pre-period. The pre-period in all cases is the filing period from the previous year (e.g., July 2020 for columns 1 and 2). *Post* is an indicator variable equal to one for filings July 2021 and later, and zero otherwise. Appendix C defines all variables. Standard errors are clustered by firm and t-statistics are in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1 (two-tailed).

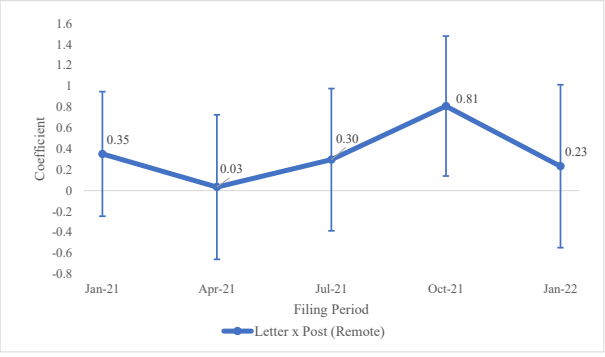
Panel A: Monthly and Quarterly Filers Grouped Together



Panel B: Monthly Filers



Panel C: Quarterly Filers



Notes: This figure plots difference-in-differences coefficients from the regressions in Table 3 using *Log(1+TaxBase)* as the dependent variable, with two modifications to the specification. First, we combine the four treatment conditions into a single variable ("Letter"), which is an indicator variable equal to 1 if the firm received any treatment letter. Second, we include coefficients from additional pre-period regressions (-2 and -1), which show that the differences between treatment and control firms are not statistically significant before the treatment. 95 percent confidence intervals are plotted above and below each coefficient. We present the coefficients next to the point estimate on each figure. The treatment letters were sent in early July 2021, after the June 30 period end and before the July 20 filing deadline. Panel A groups monthly and quarterly filers together. Panel B only includes monthly filers. Panel C only includes quarterly filers.

Fig. 3. Trend in remote firms' reported tax base.

average increased their tax payments by $3711 in response to the treatment.[23] In Panel B (Panel C), we find that the first-period tax base increases among monthly (quarterly) filers by 95 percent (125 percent).[24]

The effect sizes we document are economically meaningful compared to prior studies.[25] However, there are several reasons why benchmarking directly against prior studies may be problematic. First, most prior work focuses on income taxes, whereas we examine sales tax compliance. These two taxes have different compliance and enforcement mechanisms. For sales taxes, firms act as collection agents for the state and only the firm typically files returns (e.g., there is no secondary return to compare the reported tax base to). Sales taxes may also have different incidences than income taxes.[26] Therefore, we may not expect comparable baseline rates of compliance or changes in compliance. Second, we examine a remote setting, where the base rate of non-compliance may be higher due to lower and weaker ex ante expectations of detection and enforcement relative to a local setting, and resource-constrained firms may focus their sales tax compliance efforts on their more meaningful (e.g., local) jurisdictions.

The taxpayer responses across conditions help reveal the determinants of remote taxpayer compliance in our experiment (Mascagni, 2018). Because all conditions have a statistically significant effect and none are statistically distinguishable from one another, additional efforts by the tax authority to highlight the risk and cost of detection or to increase tax morale provide no incremental benefit. Our results suggest that despite the challenges of enforcement and greater social distance between the firm and the tax authority, the act of informing remote firms alone can significantly, albeit temporarily, increase compliance. Although we cannot directly observe why and how firms adjust their sales tax compliance processes after receiving a letter, one potential explanation for our results is that the letters are temporarily making the Texas sales tax more salient to attention- and resource-constrained taxpayers rather than producing long-term learning effects (e.g., Manoli and Turner, 2014).

7. Additional analyses

We conduct additional tests to further tease out the mechanisms by which remote tax authority correspondence affects taxpayer compliance.

7.1. Salience of the tax obligation — retail versus non-retail firms

The results in Table 3 are consistent with letters temporarily increasing salience of the tax obligation. To provide more evidence on this potential mechanism, we partition our sample into groups that we expect to have significantly different information acquisition costs and resources dedicated to remote sales tax compliance: retail firms and non-retail firms. Sales tax calculations and filing requirements are better known and more easily understood by retail firms than non-retail firms, because non-retail firms face greater variation in tax rules across states (e.g., Airi and Sammartino, 2021. See also Appendix A). Compared to non-retail firms, retail firms generally are more familiar with sales taxes, pay them more regularly, have more resources dedicated to sales tax compliance, and have simpler tax bases (Hageman, 2012; Cohn, 2021). Hence, the salience of remote tax obligations should be significantly lower for non-retail firms.

Table 4 presents the results of estimating equation (1) for both tax compliance outcomes within non-retail industries (columns 1, 3, and 5) and within retail industries (columns 2, 4, and 6).[27] In Panel A, we find that the increase in the reported tax base is concentrated among non-retail firms in filing periods 0 and 1 (columns 1 and 3) for all four treatment letters, which each contain basic instructions and guidance that should increase the salience of the tax. Results from the sample of retail firms are notably different. Columns 2, 4, and 6 suggest that remote retail firms did not increase compliance in response to correspondence. In Panel B we find little effect of the treatments on timely filing, either for retail firms or non-retail firms.

We also present the results from Table 4 visually in Fig. 4. As before, we plot a single line that represents the effect of receiving any letter, and we include coefficients from additional pre-period regressions. The figure shows that differences

[23] We compute this by multiplying the average pre-period tax liability by the effect size of 105 percent. The average tax liability in the pre-period is $3534 ($44,173 tax base multiplied by a tax rate of 8 percent). Multiplying by 105 percent yields an increase of $3711 in tax. The 8 percent tax rate is the sum of the state tax rate of 6.25 percent and the "single local use tax rate" of 1.75 percent that remote sellers can use in lieu of collecting and remitting local tax for each jurisdiction where the firm has customers. In an alternative specification, we use actual tax payments as a dependent variable and draw similar inferences, although the magnitude is somewhat lower (e.g., we estimate an increase in taxes paid in filing period 1 of $2356 for the average firm). We do not use this variable in our main specification because it contains adjustments to the current-period liability that are related to other periods (e.g., refunds, pre-payments, prior period payments coupled with current period payments, etc.).

[24] I.e., $\exp(0.67) - 1 = 0.95$ and $\exp(0.81) - 1 = 1.25$.

[25] Shimeles et al. (2017) find that Ethiopian businesses that received a deterrence letter increased their reported profit tax payable by 38 percent, while those that received a persuasion letter increased tax payable by 32 percent, compared to the control group. Mascagni et al. (2017) find that letters increase reported taxes due in Rwanda by 44 percent. Bott et al. (2020) find that moral suasion and deterrence treatments increased self-reported foreign income by 70–80 percent.

[26] Although firms and consumers might bear some of the economic burden for both sales taxes and corporate income taxes, recent working papers that use barcode-level scanner data suggest that 31 percent of corporate income tax incidence falls on consumers (Baker et al., 2020), while the increase in sales tax stemming from *Wayfair* was fully borne by consumers (Fox et al., 2022).

[27] In Online Appendix Table OA.3, we present descriptive statistics for the remote non-retail and retail samples. In short, although retail firms have larger tax bases, there are no significant differences across conditions within the remote retail and non-retail samples.

Table 4
Cross-sectional tests based on industry (non-retail versus retail).

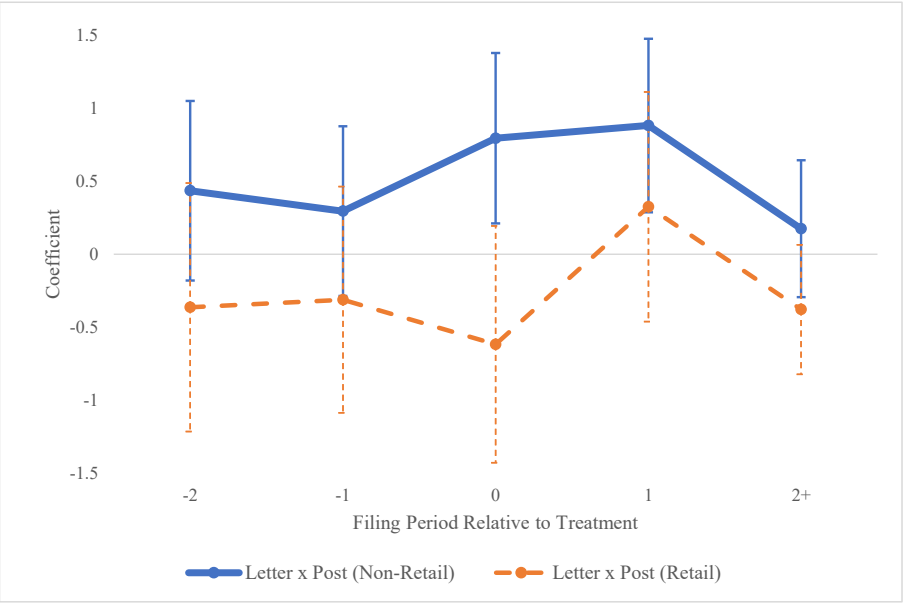
Panel A: Dependent Variable = *Log(1 + TaxBase)*

Variable	Reporting Period 0		Reporting Period 1		Reporting Period 2+	
	(1)	(2)	(3)	(4)	(5)	(6)
Inform × *Post*	0.73*	−0.47	1.03***	0.50	0.38	−0.26
	(1.86)	(−0.94)	(2.59)	(1.03)	(1.29)	(−0.78)
Deterrence × *Post*	0.71*	−0.74	0.95**	0.34	0.15	−0.38
	(1.95)	(−1.45)	(2.44)	(0.72)	(0.48)	(−1.61)
Fiscal Exchange × *Post*	0.84**	−0.57	0.80**	0.16	0.00	−0.85**
	(2.25)	(−1.07)	(2.25)	(0.31)	(0.01)	(−2.30)
Social Norms × *Post*	0.89**	−0.68	0.75**	0.28	0.17	−0.06
	(2.43)	(−1.26)	(2.02)	(0.53)	(0.59)	(−0.17)
Sample	Non-retail	Retail	Non-retail	Retail	Non-retail	Retail
Firm Fixed Effects	Y	Y	Y	Y	Y	Y
Due Date Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	2646	1070	2608	1042	9403	3618
R-Squared	0.808	0.851	0.810	0.868	0.641	0.749
Test for Differences						
Inform × *Post* × *Non-Retail*	1.20*		0.53		0.64	
	(1.89)		(0.84)		(1.44)	
Deterrence × *Post* × *Non-Retail*	1.44**		0.61		0.53	
	(2.31)		(0.99)		(1.37)	
Fiscal Exchange × *Post* × *Non-Retail*	1.41**		0.65		0.85*	
	(2.17)		(1.06)		(1.82)	
Social Norms × *Post* × *Non-Retail*	1.57**		0.47		0.23	
	(2.41)		(0.72)		(0.51)	

Panel B: Dependent Variable = *Timely*

Variable	Reporting Period 0		Reporting Period 1		Reporting Period 2+	
	(1)	(2)	(3)	(4)	(5)	(6)
Inform × *Post*	0.01	0.11	0.02	0.01	0.02	0.03
	(0.12)	(1.47)	(0.40)	(0.16)	(0.69)	(0.84)
Deterrence × *Post*	0.03	−0.02	−0.04	−0.11	−0.03	−0.03
	(0.56)	(−0.23)	(−0.90)	(−1.63)	(−1.24)	(−0.72)
Fiscal Exchange × *Post*	0.02	−0.00	−0.00	−0.03	−0.05*	−0.02
	(0.41)	(−0.05)	(−0.06)	(−0.43)	(−1.72)	(−0.42)
Social Norms × *Post*	0.02	0.12	0.02	0.06	−0.01	0.06
	(0.38)	(1.59)	(0.48)	(0.73)	(−0.50)	(1.42)
Sample	Non-retail	Retail	Non-retail	Retail	Non-retail	Retail
Firm Fixed Effects	Y	Y	Y	Y	Y	Y
Due Date Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	2646	1070	2608	1042	9403	3618
R-Squared	0.634	0.638	0.629	0.622	0.423	0.443
Test for Differences						
Inform × *Post* × *Non-Retail*	−0.10		0.01		−0.01	
	(−1.17)		(0.10)		(−0.26)	
Deterrence × *Post* × *Non-Retail*	0.04		0.07		−0.01	
	(0.51)		(0.82)		(−0.12)	
Fiscal Exchange × *Post* × *Non-Retail*	0.02		0.03		−0.03	
	(0.26)		(0.32)		(−0.54)	
Social Norms × *Post* × *Non-Retail*	−0.10		−0.03		−0.08	
	(−1.12)		(−0.38)		(−1.47)	

Notes: This table presents results from estimating a difference-in-differences model (equation (1)) separately for remote non-retail firms and remote retail firms. The odd-numbered columns include the non-retail sample, and the even-numbered columns include the retail sample. The "non-retail" sample includes remote taxpayers whose 2-digit NAICS code equals anything other than 44 or 45 (see Table 1 for a break-out of our sample by industry). The "retail" sample includes remote taxpayers whose 2-digit NAICS code equals 44 or 45. Panel A uses *Log(1 + TaxBase)* as the dependent variable, and Panel B uses *Timely* as the dependent variable. Columns 1 and 2 contain observations from filing period 0 (July 2021) and its pre-period; columns 3 and 4 contain observations from filing period 1 (August 2021 for monthly filers, October 2021 for quarterly filers) and its pre-period; columns 5 and 6 contain observations from filing period 2+ (September 2021−January 2022 for monthly filers, January 2022 for quarterly filers) and its pre-period. The pre-period in all cases is the filing period from the previous year (e.g., July 2020 for columns 1 and 2). *Post* is an indicator variable equal to one for filings July 2021 and later, and zero otherwise. Below the regression results, we report coefficients and t-stats from a fully interacted model to test for differences in effect sizes. Appendix C defines all variables. Standard errors are clustered by firm and t-statistics are in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1 (two-tailed).



Notes: This figure plots difference-in-differences coefficients from the regressions in Table 4 (non-retail versus retail firms) using *Log(1+TaxBase)* as the dependent variable, with two modifications to the specification. First, we combine the four treatment conditions into a single variable ("Letter"), which is an indicator variable equal to 1 if the firm received any treatment letter. Second, we include coefficients from additional pre-period regressions (-2 and -1), which show that the differences between treatment and control firms are not statistically significant before the treatment for both non-retail and retail firms. 95 percent confidence intervals are plotted above and below each coefficient. The treatment letters were sent in early July 2021, after the June 30 period end and before the July 20 filing deadline.

Fig. 4. Trend in remote firms' reported tax base – non-retail versus retail firms.

between the treatment and control groups were not statistically significant before the treatment, and that the non-retail difference-in-differences coefficients are statistically significant in periods 0 and 1. None of the coefficients for retail firms are statistically significant.[28] Overall, the results are consistent with remote tax authority correspondence having the strongest effect when it increases salience.

7.2. The role of remoteness – remote versus local firms

To examine the role of remoteness in firms' compliance behavior, we compare the treatment effect among remote firms to that of a matched sample of firms with a physical location and primary mailing address in Texas. By examining the differential responses between remote firms and local firms across letter conditions, we can better understand which features of remoteness (e.g., salience and information acquisition costs, detection risk and costs, or intrinsic motivations) explain the main results.

During the sample selection process, we created a matched sample of local Texas firms that were similar to the remote sample. Specifically, we identified a local firm that matched each remote firm in terms of filing status (late/extra-late), two-digit NAICS industry, and filing frequency (i.e., monthly or quarterly). We also required local matches to have Texas taxable sales and total Texas sales within 10 percent of the remote firm. Lastly, we matched on an indicator variable for whether the firms reported Texas taxable purchases.

We next utilize this matched sample to compare the effect of each type of correspondence between remote and local firms by estimating equation (1) separately for each sample, then estimating a fully interacted model in which an indicator for *Remote* is interacted with each variable and fixed effect in equation (1). In Panel A of Table 5 we present the results with *Log(1 + TaxBase)* as the dependent variable. In columns 2, 4, and 6 we find that none of the coefficients for local firms are statistically significant. The three-way interaction coefficients indicate that the effect of *Inform*, *Deterrence*, and *Fiscal*

[28] We note that the unfamiliarity that stems from the remote setting is a key feature of our design and seems necessary for salience to have an effect on compliance. We repeat the retail versus non-retail test among the matched local sample from Section 7.2 and find no significant difference between the two groups in this local setting.

Exchange letters compared to control firms was greater in remote firms than in local firms. In Panel B, we find that the effects of the letters on timely filing was limited in both remote and local firms. In Fig. 5, we present these results visually. We show that there are virtually no differences in the reported tax base between local treated and control firms throughout all periods.

Table 5
Remote versus local tax compliance.

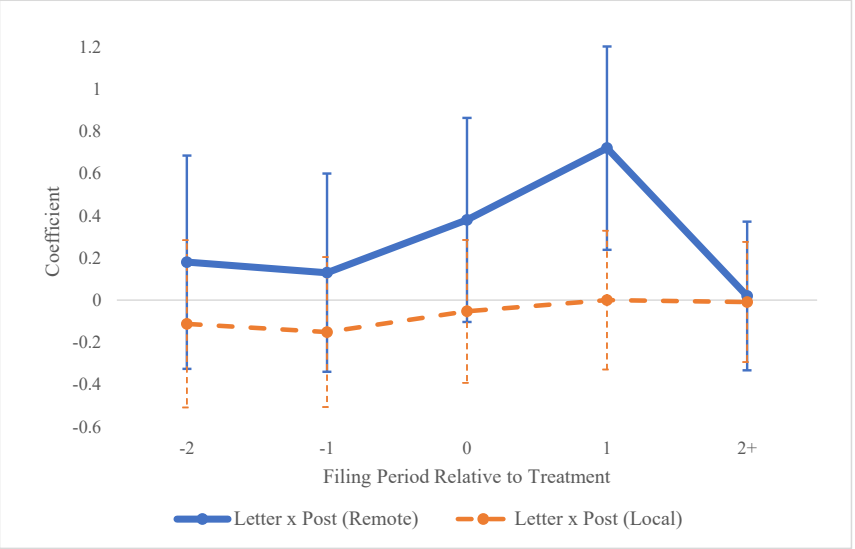
Panel A: Dependent Variable = *Log(1 + TaxBase)*

Variable	Reporting Period 0		Reporting Period 1		Reporting Period 2+	
	(1)	(2)	(3)	(4)	(5)	(6)
Inform × *Post*	0.39	0.01	0.88***	0.01	0.20	−0.03
	(1.21)	(0.04)	(2.79)	(0.05)	(0.86)	(−0.18)
Deterrence × *Post*	0.28	−0.19	0.77**	−0.27	−0.02	−0.10
	(0.95)	(−0.82)	(2.50)	(−1.22)	(−0.07)	(−0.53)
Fiscal Exchange × *Post*	0.43	0.10	0.63**	0.02	−0.22	0.01
	(1.39)	(0.45)	(2.15)	(0.08)	(−0.96)	(0.06)
Social Norms × *Post*	0.44	−0.13	0.62**	0.24	0.10	0.08
	(1.43)	(−0.58)	(2.02)	(1.11)	(0.44)	(0.42)
Sample	Remote	Local	Remote	Local	Remote	Local
Firm Fixed Effects	Y	Y	Y	Y	Y	Y
Due Date Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	3716	3794	3650	3708	13,021	13,021
R-Squared	0.820	0.886	0.826	0.886	0.671	0.778
Test for Differences						
Inform × *Post* × *Remote*	0.38		0.87**		0.23	
	(0.99)		(2.29)		(0.79)	
Deterrence × *Post* × *Remote*	0.47		1.04***		0.08	
	(1.26)		(2.74)		(0.28)	
Fiscal Exchange × *Post* × *Remote*	0.33		0.61*		−0.23	
	(0.89)		(1.68)		(−0.80)	
Social Norms × *Post* × *Remote*	0.56		0.38		0.02	
	(1.50)		(1.01)		(0.08)	

Panel B: Dependent Variable = *Timely*

Variable	Reporting Period 0		Reporting Period 1		Reporting Period 2+	
	(1)	(2)	(3)	(4)	(5)	(6)
Inform × *Post*	0.04	−0.02	0.01	0.00	0.02	−0.00
	(0.90)	(−0.54)	(0.39)	(0.08)	(1.02)	(−0.21)
Deterrence × *Post*	0.01	0.00	−0.06	0.01	−0.03	0.03
	(0.35)	(0.01)	(−1.60)	(0.37)	(−1.45)	(1.12)
Fiscal Exchange × *Post*	0.01	0.01	−0.01	0.04	−0.04*	0.01
	(0.32)	(0.25)	(−0.33)	(0.90)	(−1.68)	(0.35)
Social Norms × *Post*	0.05	−0.00	0.03	0.08**	0.01	0.01
	(1.15)	(−0.04)	(0.76)	(2.02)	(0.31)	(0.27)
Sample	Remote	Local	Remote	Local	Remote	Local
Firm Fixed Effects	Y	Y	Y	Y	Y	Y
Due Date Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	3716	3794	3650	3708	13,021	13,021
R-Squared	0.634	0.627	0.626	0.657	0.429	0.417
Test for Differences						
Inform × *Post* × *Remote*	0.06		0.01		0.03	
	(1.02)		(0.21)		(0.87)	
Deterrence × *Post* × *Remote*	0.01		−0.08		−0.06*	
	(0.23)		(−1.37)		(−1.81)	
Fiscal Exchange × *Post* × *Remote*	0.00		−0.05		−0.05	
	(0.04)		(−0.87)		(−1.45)	
Social Norms × *Post* × *Remote*	0.05		−0.05		0.00	
	(0.83)		(−0.94)		(0.03)	

Notes: This table presents results from estimating a difference-in-differences model (equation (1)) examining the effect of letter correspondence on remote tax compliance relative to local tax compliance. The odd-numbered columns include the remote sample, and the even-numbered columns include the local sample. Panel A uses *Log(1 + TaxBase)* as the dependent variable, and Panel B uses *Timely* as the dependent variable. Columns 1 and 2 contain observations from filing period 0 (July 2021) and its pre-period; columns 3 and 4 contain observations from filing period 1 (August 2021 for monthly filers, October 2021 for quarterly filers) and its pre-period; columns 5 and 6 contain observations from filing period 2+ (September 2021–January 2022 for monthly filers, January 2022 for quarterly filers) and its pre-period. The pre-period in all cases is the filing period from the previous year (e.g., July 2020 for columns 1 and 2). *Post* is an indicator variable equal to one for filings July 2021 and later, and zero otherwise. Below the regression results, we report coefficients and t-stats from a fully interacted model to test for differences in effect sizes. Appendix C defines all variables. Standard errors are clustered by firm and t-statistics are in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1 (two-tailed).



Notes: This figure plots difference-in-differences coefficients from the regressions in Table 5 (remote versus local) using *Log(1+TaxBase)* as the dependent variable, with two modifications to the specification. First, we combine the four treatment conditions into a single variable ("Letter"), which is an indicator variable equal to 1 if the firm received any treatment letter. Second, we include coefficients from additional pre-period regressions (-2 and -1), which show that the differences between treatment and control firms are not statistically significant before the treatment for both remote and local firms. 95 percent confidence intervals are plotted above and below each coefficient. The treatment letters were sent in early July 2021, after the June 30 period end and before the July 20 filing deadline.

Fig. 5. Trend in remote versus local firms' reported tax base.

One ex post interpretation of our results is that the incremental effect of *Remote* on the reported tax base in the *Inform* condition supports the information acquisition and salience channel (i.e., because local firms should be more familiar with the tax obligation, the letters should increase the salience more for remote firms if salience is a key determinant of compliance). Because (1) all letters contain the *Inform* language and (2) the incremental effect of *Remote* in the other treatment conditions is not significantly greater than in the *Inform* condition, these results are consistent with salience being the reason that remoteness moderates compliance.

However, the differential response between remote and local firms could be driven by other potential, non-mutually exclusive explanations (e.g., differential liquidity or financial constraints). Although our data do not allow us to definitively conclude that salience is the sole reason for the differential effect, several ex post analyses do not support alternative channels. First, the result does not appear to be due to a compliance ceiling effect (i.e., the baseline compliance level among in-state firms theoretically could have been high enough to preclude any meaningful increase in compliance in response to the treatments). Untabulated Texas audit data from 2020 reveal that similar proportions of audited out-of-state and in-state firms have deficiencies (64 percent and 61 percent, respectively). Further, the average deficiency for in-state firms is large enough to be inconsistent with the ceiling effect. However, the average deficiency is roughly three times larger for out-of-state firms, which could suggest out-of-state firms have more room to increase compliance than in-state firms. We caveat that these data come from non-random audits, and we are therefore cautious to not over-interpret these statistics.

Second, the result does not appear to be due to the unique cultural or political attributes of Texas firms. In untabulated tests, we examine the moderating role of state tightness/looseness cultural scores (Harrington and Gelfand, 2014) and local political leanings in our remote sample. We find no evidence that these factors moderate remote firms' responses, which partially alleviates concerns about potential spurious Texas-specific confounds driving the remote versus local result rather than remoteness itself.

Third, the result does not appear to be due the potential difference in size of local and remote firms. Because firms' worldwide sales are not available, we match on firms' total Texas sales, which could lead to our local sample being smaller than our remote sample. In untabulated tests using our remote sample, we do not find a difference in the responses of small firms and large firms, which is inconsistent with size being a confound. In sum, we do not find strong evidence that any of the channels beyond salience that we examine explain our results.

Table 6
Cross-sectional tests based on proximity.

Panel A: Dependent Variable = *Log(1 + TaxBase)*

Variable	Reporting Period 0		Reporting Period 1		Reporting Period 2+	
	(1)	(2)	(3)	(4)	(5)	(6)
Inform × *Post*	0.83*	−0.10	1.41***	0.26	0.44	−0.08
	(1.90)	(−0.21)	(3.24)	(0.56)	(1.48)	(−0.21)
Deterrence × *Post*	0.14	0.45	0.79*	0.74*	0.09	−0.13
	(0.33)	(1.05)	(1.81)	(1.72)	(0.31)	(−0.40)
Fiscal Exchange × *Post*	−0.05	0.86*	0.28	0.94**	−0.34	−0.10
	(−0.12)	(1.96)	(0.70)	(2.23)	(−1.15)	(−0.28)
Social Norms × *Post*	0.55	0.32	0.50	0.72*	0.31	−0.11
	(1.28)	(0.74)	(1.13)	(1.72)	(1.00)	(−0.33)
Sample	Far	Near	Far	Near	Far	Near
Firm Fixed Effects	Y	Y	Y	Y	Y	Y
Due Date Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	1868	1848	1844	1806	6657	6364
R-Squared	0.836	0.805	0.834	0.820	0.707	0.634
Test for Differences						
Inform × *Post* × *Far*	0.93		1.15*		0.51	
	(1.45)		(1.81)		(1.10)	
Deterrence × *Post* × *Far*	−0.31		0.05		0.22	
	(−0.52)		(0.08)		(0.50)	
Fiscal Exchange × *Post* × *Far*	−0.91		−0.66		−0.25	
	(−1.50)		(−1.14)		(−0.53)	
Social Norms × *Post* × *Far*	0.24		−0.22		0.42	
	(0.39)		(−0.36)		(0.92)	

Panel B: Dependent Variable = *Timely*

Variable	Reporting Period 0		Reporting Period 1		Reporting Period 2+	
	(1)	(2)	(3)	(4)	(5)	(6)
Inform × *Post*	0.06	−0.00	−0.05	0.08	0.03	0.02
	(1.13)	(−0.01)	(−0.91)	(1.40)	(0.91)	(0.52)
Deterrence × *Post*	−0.04	0.07	−0.16***	0.05	−0.04	−0.03
	(−0.70)	(1.15)	(−3.01)	(0.85)	(−1.26)	(−0.81)
Fiscal Exchange × *Post*	−0.00	0.03	−0.09*	0.07	−0.01	−0.07**
	(−0.04)	(0.51)	(−1.67)	(1.18)	(−0.26)	(−2.17)
Social Norms × *Post*	0.06	0.04	−0.05	0.11**	0.03	−0.02
	(1.00)	(0.63)	(−0.83)	(2.00)	(0.88)	(−0.47)
Sample	Far	Near	Far	Near	Far	Near
Firm Fixed Effects	Y	Y	Y	Y	Y	Y
Due Date Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	1868	1848	1844	1806	6657	6364
R-Squared	0.646	0.626	0.622	0.635	0.424	0.434
Test for Differences						
Inform × *Post* × *Far*	0.06		−0.13*		0.01	
	(0.78)		(−1.65)		(0.26)	
Deterrence × *Post* × *Far*	−0.11		−0.21***		−0.01	
	(−1.31)		(−2.71)		(−0.27)	
Fiscal Exchange × *Post* × *Far*	−0.03		−0.16**		0.06	
	(−0.38)		(−2.01)		(1.35)	
Social Norms × *Post* × *Far*	0.02		−0.16**		0.05	
	(0.28)		(−2.00)		(0.96)	

Notes: This table presents results from estimating a difference-in-differences model (equation (1)) separately for near and far remote taxpayers. The odd-numbered columns include the "Far" sample, and the even-numbered columns include the "Near" sample. The "Far" sample includes remote taxpayers whose mailing address county is further from Travis County, Texas than the median of all remote taxpayers. The "Near" sample includes remote taxpayers whose mailing address county is closer to Travis County, Texas than the median of all remote taxpayers. Panel A uses *Log(1 + TaxBase)* as the dependent variable, and Panel B uses *Timely* as the dependent variable. Columns 1 and 2 contain observations from filing period 0 (July 2021) and its pre-period; columns 3 and 4 contain observations from filing period 1 (August 2021 for monthly filers, October 2021 for quarterly filers) and its pre-period; columns 5 and 6 contain observations from filing period 2+ (September 2021−January 2022 for monthly filers, January 2022 for quarterly filers) and its pre-period. The pre-period in all cases is the filing period from the previous year (e.g., July 2020 for columns 1 and 2). *Post* is an indicator variable equal to one for filings July 2021 and later, and zero otherwise. Below the regression results, we report coefficients and t-stats from a fully interacted model to test for differences in effect sizes. Appendix C defines all variables. Standard errors are clustered by firm and t-statistics are in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1 (two-tailed).

7.3. Proximity

Braithwaite (2003) notes that the psychological concept of 'social distance' influences taxpayers' attitudes towards tax authorities. Taxpayers naturally have greater social distance with the remote authorities, which can cause a lower sense of duty or obligation for taxpayers to comply. Taxpayers with greater social distance may also have apathy towards the tax system and therefore be more willing to engage in non-compliance. Remote tax authority correspondence could decrease compliance if it inadvertently draws attention to the distance between the taxpayer and tax authority (i.e., if it causes taxpayers to more thoroughly consider the low detection probability, the lack of representation in voting, the lack of fiscal exchange, etc.). On the other hand, it could increase compliance if it reduces the taxpayer's sense of distance (i.e., it revises prior beliefs upward about detection probability, fiscal exchange, norms, etc.).

Table 6 presents the results of estimating equation (1) based on remote firms' distance from the firm's address to Travis County, Texas (the Texas tax authority's location). Columns 1, 3, and 5 present the estimates from the sample of firms that are located farther from Travis County (above the sample median distance), and columns 2, 4, and 6 present the estimates from the sample of firms that are located closer to Travis County (less than or equal to the sample median distance).

In Panel A, the dependent variable is *Log(1 + TaxBase)*. In filing period 1, we find that firms located further from Travis County exhibit a positive and significant response to *Inform* and *Deterrence* messages, and closer firms exhibit a positive and significant response to *Deterrence*, *Fiscal Exchange*, and *Social Norms* messages. The difference between groups is only significant for *Inform*, which could suggest that *Inform* strategies may be more beneficial when targeted at firms that face greater information acquisition costs due to their distance from the tax authority.

In Panel B, the dependent variable is *Timely*. We find, somewhat unexpectedly, that far firms respond to all conditions by filing less timely than near firms in filing period 1. In short, we find some evidence that a taxpayer's distance from the tax authority matters when estimating the response to tax correspondence. Certain types of correspondence (e.g., *Inform*) may be more effective at increasing reported tax bases as distance increases.

8. Caveats and limitations

We offer several caveats to this study. First, while we randomly assigned sample firms to different conditions within our field experiment, the selection of sample participants was not entirely random. As is customary in tax administration programs at nearly all levels, finite tax administration resources incentivized us to focus on taxpayers with some history of late filing. Although our study cannot directly speak to the effect on firms that always file on time, such firms could be underreporting sales and therefore respond to correspondence from a remote tax authority. Despite this limitation, the state tax authority's random selection of delinquent remote firms produced a sample that had diversity in size, industry, and home business location. Because we randomize treatment conditions within the sample, we alleviate concerns about internal validity. However, we note that there are potential limitations to external validity and generalization.

Second, our experiment was performed by one of the largest state tax authorities in the country. We cannot say conclusively whether the results of our study would be the same if an identical test had been administered by a smaller tax authority. Because it is not practicable to replicate this study in every state, we caution readers of this limitation.

Finally, there were certain aspects of our experiment that we were not able to randomly assign or vary by taxpayer. For example, we cannot randomly assign which firms are located in or out of Texas (see Table 5). Our reliance on observed, rather than assigned, differences in this test was relevant when we identified a local firm to match with each remote firm.

9. Conclusion

In this study, we conduct a randomized field experiment with the Texas tax authority to examine whether and why firms respond to remote tax authority. We find that correspondence increases the short-term tax compliance of remote firms. Specifically, taxpayers increased compliance in response to correspondence that simply informed them of their tax obligation. We find no incremental effect of correspondence that emphasized deterrence or non-pecuniary factors. We find that compliance increased the most for remote non-retail firms and compliance did not change among a matched sampled of local firms that received the same correspondence. Collectively, the results point towards salience as a key determinant of remote tax compliance.

Given the rise in taxation of out-of-state firms and the economic importance of both sales tax and remote sales tax collections, this study provides important and timely insights that future studies can expand upon. Further, our paper answers the calls in Dyreng and Maydew (2018) for more research that addresses both non-income-based taxes and the taxation of business activities that span different jurisdictions.

Finally, our insights about the importance of salience in remote tax settings should be relevant to tax administrators, who increasingly grapple with compliance from out-of-jurisdiction firms. Many U.S. states now aim to tax out-of-state firms (e.g., through economic nexus laws for sales and income taxes, market-based sourcing rules, combined reporting, etc.). At the

international level, about half of all European OECD countries and a handful of others have proposed or implemented digital services taxes, which are based on firms' digital activities in a jurisdiction (Bunn and Asen, 2022; Morris and Brown, 2021). In addition, the OECD/G20 Inclusive Framework on Base Erosion and Profit Shifting proposes expanding taxing rights for market jurisdictions (i.e., typically the jurisdiction where customers are located). The proposal has been agreed to in concept by over 130 nations, but has not yet been formally adopted as of November 2022. Regardless of the future of these proposals, there seems to be a general trend towards expanding the taxing rights held by remote tax authorities.

Although interstate and international tax policy exhibit similar trends in remote taxation, we acknowledge that there are significant differences in the settings. Many factors that are consistent across U.S. states (e.g., federal laws, currency, language, etc.) vary across countries. Nonetheless, our study suggests that international tax policymakers or administrators might benefit by considering how the salience of tax obligations affects remote tax compliance.

Declaration of competing interest

None.

Appendix A. Firm disclosure and other anecdotes on sales tax issues

An online travel shopping company explains the burden of multistate sales tax compliance:

> "We note that there are more than 10,000 taxing jurisdictions in the United States, and it is not feasible to analyze the statutes, regulations and judicial and administrative rulings in every jurisdiction. Rather, we have obtained the advice of state and local tax experts with respect to tax laws of certain states and local jurisdictions that represent a large portion of our hotel revenue … We continue to work with the relevant tax authorities and legislators to clarify our obligations under new and emerging laws and regulations. We will continue to monitor the issue closely and provide additional disclosure, as well as adjust the level of reserves, as developments warrant" (Expedia Group, Inc., FY 2019 10-K, Management's Discussion and Analysis, other long-term liabilities).

A cloud software company on the complexity and nuance in sales tax rules:

> "State, local and foreign jurisdictions have differing rules and regulations governing sales, use, value added and other taxes, and these rules and regulations are subject to varying interpretations that may change over time. In particular, the applicability of such taxes on subscriptions to our platform in various jurisdictions is unclear. Further, these jurisdictions' rules regarding tax nexus are complex and vary significantly. As a result, we could face the possibility of audits that could result in tax assessments, including associated interest and penalties" (Domo, Inc., FY 2021 10-K, Item 1A. Risk Factors).

A video game software development company on jurisdictions adopting laws to tax firms with no physical presence:

> "In response to Wayfair, or otherwise, states or local governments may adopt, or begin to enforce, laws requiring us to calculate, collect and remit taxes on sales in their jurisdictions. Similarly, many foreign jurisdictions have considered or adopted laws that impose value added, digital service, or similar taxes, on companies despite not having a physical presence in the foreign jurisdiction. A successful assertion by one or more states, or foreign jurisdictions, requiring us to collect taxes where we presently do not do so, or to collect more taxes in a jurisdiction in which we currently do collect some taxes, could result in substantial tax liabilities, including taxes on past sales, as well as penalties and interest. The requirement to collect … could also create additional administrative burdens for us … We continually monitor the ever-evolving tax landscape in the jurisdictions in which we operate and those jurisdictions where our customers reside" (Unity Software, Inc., FY 2020 10-K, Item 1A. Risk Factors).

Based on interviews with practitioners and members of the business community, the GAO reports that businesses face significant multistate sales tax compliance costs, including the cost to understand compliance obligations:

> "In November 2017, we reported that if states were given authority to require businesses to collect tax on remote sales, businesses would have to incur costs to understand their new compliance obligations, which could differ by state or tax jurisdiction" (GAO, 2022).

In 2020, testimony on behalf of the AICPA in front of the U.S. House of Representatives highlights the burdens of interstate taxation and *Wayfair* on small businesses:

> "Since Wayfair, it has become even more challenging to meet [sales tax compliance] requirements. Even those small businesses historically compliant in states where they are physically present are resource constrained and face financial limitations to either internally implement or outsource new and complex compliance processes … If these small

> businesses are subject to the new rules, they must determine whether the products they sell are taxable or exempt depending on the existing rules in each state in which they are selling … If the sales are taxable, small businesses must determine the correct state and local tax rates to charge and comply with each state's specific collection and remittance compliance obligations. Taken as a whole, there are often prohibitive costs attached to ensuring that the compliance is performed correctly, whether completed in-house with dedicated tax staff, or completely outsourced" (Yesnowitz, 2020).

Appendix B. Instrument (letter conditions)

The letters were sent from the Texas Comptroller's office using their standard letterhead. Each letter condition contained the same opening paragraph and ending three paragraphs. The second paragraph differed based on letter condition (in brackets below). Italicized portions were italicized in the actual letters.

"«DATE»
«TAXPAYER NAME»
«TAXPAYER ADDRESS»
Re: Taxpayer Number - «TPID»
Dear Taxpayer:

Our records indicate that you are currently registered to collect and remit Texas sales and use tax. Texas law requires registrants to accurately file a Texas sales and use tax return and remit taxes in a timely manner on a «PERIOD» basis.

[**Inform condition**: no additional information.]

[**Deterrence condition**: "Through the end of this fiscal year, we will be closely examining sales and use tax filings. *As a reminder, Texas imposes a statutory penalty on past due taxes, calculated as follows*: (i) a 5 percent penalty if taxes are paid 1–30 days late, and (ii) a 10 percent penalty if taxes are paid more than 30 days late. Statutory interest begins accruing on the 61st day after the due date of a required report. The interest rate is a variable rate determined at the beginning of each calendar year. A taxpayer who fails to file reports on time will be assessed an additional penalty of $50 for each late report."]

[**Fiscal Exchange condition**: "Texas sales tax is a primary source of public funds, accounting for 59 percent of the state's tax revenue in 2020. *By funding education, health care, transportation, and public safety, your taxes contribute to the economic and physical well-being of your customers here in Texas*."]

[**Social Norms condition: "**Ninety-five percent of Americans believe it is every American's civic duty to pay their fair share of taxes, and the U.S. is consistently among the world leaders when it comes to voluntary tax compliance."]

This notice is a reminder that the due date for filing and paying your Texas sales and use tax liability is July 20, 2021.

File and pay online! Webfile is a secure, online tool for filing and paying taxes and fees. Visit https://comptroller.texas.gov/taxes/file-pay/webfile.

For questions regarding this letter, call us toll free at XXX-XXX-XXXX."

Appendix C. Variable definitions

Variable Name	Definition
Dependent Variables	
TaxBase	The firm's taxable sales plus taxable purchases during the period reported to Texas. This variable is winsorized at the 1st and 99th percentiles in descriptive statistics.
Timely	Indicator equal to 1 if the firm filed a timely sales and use tax return for the period, and zero otherwise.
Explanatory and Cross-Sectional Variables	
Inform	Indicator equal to 1 if the firm was assigned to the *Inform* letter condition, and zero otherwise. See Appendix B for details.
Deterrence	Indicator equal to 1 if the firm was assigned to the *Deterrence* letter condition, and zero otherwise. See Appendix B for details.
Fiscal Exchange	Indicator equal to 1 if the firm was assigned to the *Fiscal Exchange* letter condition, and zero otherwise. See Appendix B for details.
Social Norms	Indicator equal to 1 if the firm was assigned to the *Social Norms* letter condition, and zero otherwise. See Appendix B for details.
Remote	Indicator equal to 1 if the firm has no physical location in Texas, and zero otherwise.
Post	Indicator equal to 1 if the sales and use tax return is due July 20, 2021 or thereafter, and zero otherwise.
Non-Retail	Indicator equal to 1 if the firm operates in an NAICS sector other than 44 or 45.
Distance	The number of miles from the center of the firm's county (based on the firm's mailing address) to the center of Travis County, Texas.
Far	Indicator equal to 1 if the firm's *Distance* is greater than the median of all remote and local taxpayers.
TotalSales	The firm's total Texas sales (both taxable and non-taxable) during the period reported to Texas. This variable is winsorized at the 1st and 99th percentiles in descriptive statistics.
Filing History (years)	The number of years the firm has been registered to file and pay Texas sales tax as of July 20, 2021.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jacceco.2022.101570.

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