Remote Authority:
A Field Experiment with Out-of-state Taxpayers

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Abstract. New laws that expand governments' taxing rights are fundamentally altering the relationship between taxpayers and tax authorities. In today's more global and digital economy, taxpayers increasingly find themselves subject to a distant, "remote" authority, in which they have no physical presence. Little is known about taxpayers' views and compliance behavior under these new circumstances. We address this issue by partnering with the Texas tax authority to conduct a randomized field experiment that targets out-of-state firms' sales tax filings. We find that most correspondence types from the tax authority increase compliance among out-of-state firms, although the effects appear to be short-lived. The response to deterrence messages from out-of-state firms is larger than the response from a matched sample of in-state firms, consistent with remote authority correspondence substantially revising upward the prior perception of detection risk. Cross-sectional tests suggest that remote authority outreach is most effective when it reduces the administrative burden of compliance. These results shed light on tax behavior under remote authority and can inform agencies that must administer new tax laws with limited or costly enforcement options.

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1. Introduction

Traditional tax compliance models assume a standard relationship between a taxpayer and the tax authority: taxpayers are subject to a tax authority that has unequivocal jurisdiction and taxing rights (Allingham and Sandmo, 1972). But today, firms increasingly find themselves subject to *remote* authority—where a government has legal right to constrain or restrain the behavior of a person or business located outside of the government's physical borders. Under these circumstances, it is unclear how taxpayers perceive the legitimacy of remote authorities' taxing rights and enforcement threats. Despite the increasing prevalence of remote authority, a rich literature of tax field experiments focuses exclusively on taxpayers with a physical presence in the tax authority's jurisdiction. Hence, we know little about how taxpayers view and respond to communications from a remote authority. In this paper, we conduct a randomized field experiment to address two questions: (1) does correspondence from a remote tax authority affect firms' tax compliance? and (2) what factors drive their responses to such correspondence?

Understanding the effectiveness of remote authority correspondence is important for several reasons. First, changes in the nature of the economy mean that businesses are becoming more multijurisdictional, products are increasingly digital, and labor and capital no longer need be proximate to serve customers. Firms therefore have increasing compliance demands from more remote or distant jurisdictions. Second, lawmakers and regulatory authorities are passing increasingly creative laws and regulations asserting their authority remotely and extraterritorially, thereby pushing the boundaries of sovereignty. The expansion of taxing rights to market jurisdictions (OECD, 2021), automatic information sharing under the Foreign Accounting Tax Compliance Act (FATCA), and extraterritorial regulation of foreign investment and internal controls under the Foreign Corrupt Practices Act (FCPA) are examples of this

phenomenon. In short, governments are now asserting remote authority across many regulatory fronts and at many jurisdictional levels.

We test our specific research question of how remote authority outreach affects tax compliance using a multistate sales taxes setting. We partner with the Office of the Texas Comptroller of Public Accounts (the Texas tax authority) to conduct a taxpayer outreach field experiment in the wake of recent developments in multistate tax law that expanded states' remote taxing rights. In 2018, the U.S. Supreme Court ruled in *Wayfair v. South Dakota* that an "out-of-state seller can be held responsible" to collect and remit sales tax (U.S. Supreme Court, 2018, p. 6). This decision overturned decades of practice applying case law that stipulated a firm must have a "physical presence" for a state to require sales tax collection and remittance. Following *Wayfair*, every state that imposes a sales tax enacted provisions that require firms with transactions above certain thresholds to collect and remit sales taxes.

There are several ways firms might not fully comply with sales tax requirements. First, firms might not collect the appropriate amount of sales tax either deliberately or inadvertently. Firms might choose not to collect if they perceive a competitive price advantage over their competitors by not charging sales tax (e.g., Hoopes et al., 2016). Further, the sales tax is inherently complex and administratively burdensome due to unique tax rates, definitions, tax bases, filing methods, and payment methods that span over 11,000 sales tax jurisdictions in the U.S. The growth of economic nexus rules after *Wayfair* has exacerbated the complexity and cost of full compliance (Borens et al., 2020). A recent survey found that small and midsized businesses spend on average 163 hours and more than $17,000 per month on sales tax compliance (Cohn, 2021). Taxpayers may therefore focus their compliance efforts in states where they have the greatest familiarity and collection requirements and accept the risk of non-

compliance in remote or less economically meaningful states. Second, firms may collect sales taxes but not remit them; however, this is less likely given that firms typically face stronger penalties than simply not collecting the tax.[1] Third, firms might collect the appropriate amount but fail to remit them to the state by the required monthly or quarterly due date.

Despite the legal authority to expand their taxing rights to out-of-state firms, states face challenges in administering and enforcing these rules due to the firms' distance from the taxing authority and lack of physical presence in the state. State taxing 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, enforcement actions such as property liens and asset seizures are relatively straightforward in a local setting, but they can become much costlier in a remote setting due to the burdensome process of enforcing judgments in the courts of another state (Thimmesch et al., 2018). Beyond enforcement, other features of the remote setting, such as taxpayers' familiarity with the state's rules, lack of perceived benefit from the public goods, or greater sense of social distance (Braithwaite, 2003), add to the complexity and nuances of remote compliance.

Remote tax authority outreach could have either a positive or negative effect on tax compliance for several reasons. To the extent that receiving remote authority correspondence reduces the administrative costs of compliance, increases perceived detection risk, or decreases the perceived social distance between the taxpayer and the remote authority, then taxpayer compliance could increase. For example, in non-remote settings, prior research has shown that deterrence messages are particularly effective at increasing compliance (e.g., De Neve et al.,

[1] In Texas, the criminal penalty for intentionally or knowingly failing to remit sales taxes that were collected can range from a misdemeanor to a first degree felony depending on the amount of unremitted amount (Texas Tax Code, Section 151.7032).

2021; Fischer et al., 1992; Mascagni et al., 2018; Perez-Truglia and Troiano, 2018). This effect could be even greater if firms' ex ante expectation of detection risk from a remote authority is low. Further, given the high complexity and administrative burden of sales tax in the U.S., correspondence that makes salient the filing responsibility and impending deadline in a remote jurisdiction could play a significant role in reducing this friction. However, if receiving remote authority correspondence increases feelings of social distance between the taxpayer and the remote authority or makes salient how costly and unlikely remote tax enforcement would be, then taxpayer compliance could decrease.

To help identify why firms respond to remote authority, we send several different versions of correspondence. The first version merely 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 version contains the same information but also indicates that returns will be closely examined and reminds taxpayers of penalties and interest associated with non-compliance (*Deterrence*). The third version contains the *Inform* language and a reminder that tax payments help to support the well-being of customers in Texas (*Fiscal Exchange*). The final version of the letter contains the *Inform* language, then indicates that a high percentage of Americans pay their taxes and feel it is a civic duty (*Social Norm*). We also include a control group of firms that did not receive tax authority correspondence.

In our field experiment, we operationalize remote authority by selecting a random sample of 2,000 registered remote taxpayers that had signs of delinquency during calendar year 2020.[2] We then randomly assign each firm to one of our five conditions listed above (400 firms per

[2] Specifically, we randomly selected 1,000 firms from three populations of taxpayers: late filers, extra-late filers, and zero filers. Zero filers are firms that reported zero taxable sales, zero total sales, and zero taxable purchases during all of 2020. Because the vast majority of these firms maintain their zero-filer status, we exclude these firms from our main tests of compliance. Our main inferences are similar if we include these firms.

condition) and we examine measures of tax compliance before and after the experiment using a difference-in-differences specification. In addition to our sample of remote firms, we also create a matched sample of 2,000 comparable local firms that have a physical presence in Texas, and we estimate a triple-differences specification that compares the effects of our conditions across remote firms and local firms.

We first compare the pre- and post-period tax compliance between taxpayers that received correspondence from the remote tax authority and those that did not. We examine two compliance-related outcomes: the taxable base (the base on which the sale tax is levied) and timely tax filing. Hence, our measures reflect the amount of tax owed and whether the return is filed in a timely manner. We assume that any increase in the reported taxable base reflects a reduction in underreporting of the true taxable base.

We find evidence that correspondence causes a significant and economically meaningful short-term increase in the taxable base, and that the effect weakens over time. Over the full sample period, the point estimates for the four outreach strategies suggest higher reported taxable bases relative to the no-letter control group, although the estimates are not statistically significant at conventional levels. We find no evidence that any of the letters affected the number of firms that file on time, either in the short-run or long-run.

We next compare the effectiveness of sending letters to out-of-state taxpayers versus sending letters to a matched pair sample of in-state taxpayers. This test helps us directly compare the effect of receiving correspondence from an authority that is remote versus one that is local.[3] We find that out-of-state firms receiving a deterrence-themed letter report a significantly higher taxable base than in-state firms. This finding could suggest that deterrence messages from a

[3] In our ideal experiment, we would also be able to randomize whether firms are local or remote. Since that is not feasible, we consider this approach as second-best.

remote authority cause firms to significantly revise upward their priors of detection risk rather than to dismiss the deterrence threat as not credible.

Next, we examine the role of correspondence from a remote authority in reducing the administrative costs (e.g., information frictions and hassle costs) of compliance. Taxpayer outreach should have a greater positive impact on taxpayer compliance when these costs are high. We proxy for administrative costs by separating firms in the retail industry from firms in other industries. Compared to non-retail firms, retail firms have greater familiarity with sales taxes, pay them more regularly, have more resources dedicated to sales tax compliance, and have more straightforward tax bases. Therefore, non-retail firms likely face higher compliance hurdles and may benefit more from tax authority outreach than retail firms.

We partition the data and rerun our main analysis and find no positive (and sometimes a negative) response to taxpayer outreach among out-of-state retail firms. Conversely, and as expected, we find taxpayers in non-retail industries substantially and significantly increase the reported tax base in response to taxpayer outreach. Together with our main results, the evidence is consistent with correspondence from a remote authority improving tax compliance the most when it reduces information frictions and hassle costs.

Finally, we conduct other cross-sectional and robustness tests to examine whether factors such as distance (conditional on remote status) or firm size might affect compliance responses. We find little evidence that greater distance or size affect compliance.

Our paper makes several contributions. Among the body of tax compliance-related field experiments, our study provides several important "firsts." Our paper is the first to conduct a field experiment with a remote authority and to explicitly examine the influence of remoteness on taxpayer compliance, which is timely given the rapid growth in taxation of remote firms.

Second, we are among the first to examine firms' compliance related to sales tax, which is responsible for approximately one-third of U.S. states' tax revenues and is the largest source of tax revenue for many states. Because the firms themselves are not responsible for paying sales taxes—only collecting and remitting them—taxpayer attitudes towards them may be different. Further, by exploring firms' sales tax compliance, our paper answers the call for more research on non-income taxes (Dyreng and Maydew, 2017). These innovations help advance the frontier of tax field experiments along several different fronts, and our study could serve as a benchmark for subsequent studies that expand along these dimensions.

Second, our study offers practical insights to tax administrators grappling with incentivizing compliance. Our study suggests that tax authority correspondence is most useful when it reduces compliance hurdles. In an increasingly digital and multijurisdictional economy, these findings should help agencies tasked with administering remote laws craft strategies and regulatory tactics to induce compliance.

Finally, our study contributes to the literature on the expanding reach of government influence. Christensen et al. (2021) argue that portions of the FCPA allows the U.S. to be the "policeman for the world." Similarly, the Foreign Accounts Tax Compliance Act (FATCA) coerces foreign financial institutions to share private banking and investment information about U.S. clients with the IRS via threat of a 30% withholding tax (Belnap et al., 2021; De Simone et al., 2020). In contrast with these efforts that have been criticized as being very heavy-handed, this study shows that compliance with remote authority can be achieved in some settings with a lighter touch and relatively friendly form of enforcement.

2. Motivation, Setting, and Hypothesis Development

2.1. Motivation

Governments at all levels are increasingly claiming the ability to enforce tax rules on businesses located outside of the jurisdiction. At the international level, 137 jurisdictions recently agreed to expand taxing rights on multinational enterprises for market jurisdictions (OECD, 2021). At the U.S. state level, many governments impose economic nexus rules that require corporations with no physical presence in the state to pay income taxes. Further, on June 21, 2018, the U.S. Supreme Court ruled in *South Dakota v. Wayfair* that a state could impose sales and use tax collection obligations on vendors without a physical presence in the state, subject to certain de minimis thresholds (e.g., less than $100,000 of sales to in-state customers). Since *Wayfair*, all 45 states that impose a general sales tax have enacted rules requiring remote sellers to collect and remit sales tax.[4] Both foreign and U.S. state governments also seek to tax the digital economy by either expanding the sales tax base or imposing taxes on digital services.[5]

Despite the legal authority to tax remote firms, states face practical challenges in compelling remittance or enforcing tax judgments on out-of-state taxpayers (Hellerstein, 2003; Swain, 2010). Although states have several available enforcement actions for non-compliance and underreporting (e.g., jeopardy assessments, negligence penalties, property liens, referrals to collection agencies, referral for criminal action to the state's Attorney General, notices to the public regarding the tax delinquencies, and/or deductions from other state tax refunds),[6] these

[4] The following states do not impose a general sales tax: Alaska, Delaware, Montana, New Hampshire, and Oregon.
[5] As of November 2021, Austria, France, Hungary, Italy, Poland, Portugal, Spain, Turkey, and the United Kingdom have implemented a digital services tax. Belgium, the Czech Republic, and Slovakia have published proposals to enact a digital services tax, and Latvia, Norway, and Slovenia have either officially announced or shown intentions to implement such a tax (Asen and Bunn, 2021). Maryland became the first U.S. state to enact a digital advertising services tax, which goes into effect on January 1, 2022 (Maryland Senate Bill 787). Connecticut, Massachusetts, Montana, and Texas have similar proposals.
[6] See https://www.bdo.com/insights/tax/state-and-local-tax/wayfair-faqs-%E2%80%93-top-questions-about-the-u-s-suprem).

actions may be more costly or less effective when applied to out-of-state taxpayers. If initial enforcement actions do not work, states also have the authority to seize certain assets, such as inventory, customer receivables, or bank accounts. If the seller does not have assets in the state, the state can follow the procedures of the Uniform Enforcement of Foreign Judgments Act to enforce its judgments in the courts of another state, although this process may be overly burdensome (Thimmesch et al., 2018).

Because of the uncertainty and costs associated with out-of-jurisdiction tax enforcement, we examine whether taxpayers respond to traditional tax enforcement strategies and efforts from remote authorities. Prior studies examine the effectiveness of various types of tax authority correspondence, such as deterrence (making audits and penalties more salient), social norms (making the behavior of a relevant comparison group more salient), and fiscal exchange (making the benefit of paying taxes more salient), among others. These studies, conducted in numerous jurisdictions all over the globe using various designs, focus exclusively on taxpayers that have a physical presence in the tax authority's jurisdiction and have produced somewhat mixed results.[7] Although each type of message (usually in the form of a letter) has proven effective in at least several instances, the lack of consistent findings highlights the importance of understanding what strategies are most effective for governments increasingly interested in shifting the tax burden from local taxpayers to remote taxpayers. Whereas most of the studies examine individual income tax compliance for taxpayers situated within a jurisdiction, we focus on sales tax compliance for out-of-jurisdiction businesses.

[7] To test the effectiveness of tax authority correspondence, many field experiments send letters to non-compliant or delinquent taxpayers. For summaries, see Fischer et al., (1992) and Mascagni (2018).

2.2. Setting

We examine the compliance of Texas sales and use tax filers. The Texas sales tax is a primary source of public funds, accounting for approximately 60 percent of the state's tax revenue in 2020, which is nearly double the national average of approximately 31 percent.[8] Texas imposes a 6.25 percent state sales and use tax on all retail sales, leases and rentals of most goods, as well as taxable services. Local jurisdictions (cities, counties, special purpose districts and transit authorities) in Texas can impose up to an additional 2 percent for a maximum combined rate of 8.25 percent. Businesses with nexus in the state that sell taxable goods and services to Texas customers must collect and remit sales taxes to the state.[9] Taxpayers generally must file and pay either monthly or quarterly, depending on the amount of their taxable sales. 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.[10]

2.3. Hypothesis Development

Allingham and Sandmo (1972) follow a Becker (1968) crime-based approach to model a taxpayer's decision of how much income to report to the tax authority, and by extension, how much tax they pay. In their model, the choice of how much tax to pay is a function of the economic returns to evasion, taking into account the costs (i.e., the probability of detection and the magnitude of potential penalties) and the benefits (i.e., based on the amount of income and

[8] See https://comptroller.texas.gov/about/media-center/news/20210901-texas-comptroller-glenn-hegar-announces-revenue-for-fiscal-2021-august-state-sales-tax-collections-1630511193264 and https://taxfoundation.org/sales-tax-base-reliance-2020/.

[9] Remote sellers (those without a physical presence in Texas) must collect and remit taxes if they have total Texas revenue of at least $500,000 in the preceding twelve calendar months.

[10] See https://comptroller.texas.gov/taxes/sales/ for more information on the Texas sales and use tax.

the tax rate). The model implies that taxpayers focus exclusively on the expected financial payoffs when deciding whether to comply or evade. Murray (1995) applies a similar framework when modeling sales tax compliance and notes that sales taxes can present numerous opportunities for evasion.

Empirical studies present strong evidence that Allingham and Sandmo's standard 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., Andreoni et al., 1998; Erard and Feinstein, 1994; Porcano, 1988). Researchers have updated models of non-compliance to include non-pecuniary variables, such as social norms and taxpayers' sense of fiscal exchange with the government (e.g., Hashimzade et al., 2013; Traxler, 2010). We refer to strategies that highlight something outside of standard economic models collectively as appeals to taxpayer morale.

Correspondence from a remote authority could increase compliance for several reasons. First, if remote taxpayers experience high administrative costs related to compliance, correspondence could reduce this friction by making salient the filing obligation, upcoming deadline, and instructions for filing and paying. Second, if taxpayers initially perceived little-to-no enforcement threat, remote authority correspondence would likely increase taxpayers' prior beliefs about detection risk and therefore increase compliance. Third, remote authority correspondence could decrease the perceived social distance between the taxpayer and the remote authority. If the correspondence increases taxpayers' beliefs about their sense of fiscal exchange with the government or the extent of voluntary compliance by their peers, taxpayers may increase compliance.

On the other hand, remote authority correspondence could decrease compliance for several reasons. First, the correspondence could make the low likelihood of enforcement and detection more salient to the taxpayer, thereby decreasing compliance. Second, the correspondence could make salient the lack of any real connection to the remote government and low degree of fiscal exchange. Taxpayers may therefore perceive a lack of fairness and decrease compliance.

Based on the arguments above, a remote authority's sales tax enforcement efforts could have a positive or negative effect on compliance. We therefore state our first hypothesis in null form.

H1*: A remote tax authority's enforcement efforts do not change a taxpayer's compliance.*

We next turn to hypotheses for specific enforcement strategies. Many studies examine the influence of deterrence initiatives, such as threats of enforcement or reminders about penalties and interest. These studies generally support the hypothesis that increasing the probability of detection reduces non-compliance (e.g., De Neve et al., 2021; Fischer et al., 1992; Mascagni et al., 2018; Perez-Truglia and Troiano, 2018). If deterrence correspondence from a remote authority causes taxpayers to revise upward their priors of detection probability, deterrence letters may have a positive effect on compliance.

However, in some circumstances, deterrence strategies do not work and can even backfire (e.g., Ariel, 2012; Blumenthal et al., 1998; Gangl et al., 2014; Kleven et al., 2011). If remote authority correspondence makes salient the fact that remote tax authorities face significant obstacles in enforcing the payment of proper tax liabilities, taxpayers may not change their behavior or reduce compliance in response to a deterrence message. The deterrence strategy could even crowd out intrinsic motivations (e.g., Del Carpio, 2014). Relatedly, Silverman et al.,

(2014) suggests that individuals are more likely to contribute to public projects when the authority has both expert knowledge and legitimate coercive power. Deterrence strategies could make salient the remote authority's lack of coercive power (French and Raven, 1959), and therefore decrease compliance.

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*: A remote tax authority's deterrence efforts do not change a taxpayer's compliance.*

Many researchers have posited that deterrence incentives and penalties imposed by a tax authority are inadequate to describe taxpayer behavior (Hashimzade et al., 2013; Skinner and Slemrod, 1985; Weber et al., 2014). Luttmer and Singhal (2014) and Mascagni (2018) suggest that other factors outside of the traditional model could influence taxpayer behavior. Collectively, they refer to these factors as "taxpayer morale" and offer five main channels through which tax morale can manifest: (1) intrinsic motivation, (2) reciprocity, (3) peer effects and social influences, (4) culture and (5) information imperfections and deviations from expected utility. We focus on fiscal exchange and social norms, which have been commonly studied in the literature. 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 thus contribute their fair share of taxes. The social norm strategy highlights a particular group's typical behavior or beliefs about acceptable behavior (in this case, that the majority of taxpayers are compliant or that the group believes paying taxes is the right thing to do).

Several field experiments have found positive effects of tax morale strategies on compliance (Bott et al., 2020; Del Carpio, 2014; Hallsworth et al., 2017; Mascagni et al., 2018; Perez-Truglia and Troiano, 2018; Shimeles et al., 2017), while other field experiments find zero

or negative effects (Ariel, 2012; Cranor et al., 2020; De Neve et al., 2021; Fellner et al., 2013; John and Blume, 2018; Slemrod et al., 2001). However, most of these studies examine individual income tax behavior, which may not generalize to business sales tax compliance.

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 the fiscal exchange with the tax authority, taxpayers may increase compliance. Likewise, social norms could have a positive effect if taxpayers have weak 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, remote taxpayers could also realize they get very little benefit from paying taxes to a remote government and therefore decrease compliance. For example, the letter could make salient the fact that taxes do not support the firm's most direct public goods (e.g., for its employees and local stakeholders) and that they have no direct input on the 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*: A remote tax authority's appeal to fiscal exchange does not change a taxpayer's compliance.*

H2c*: A remote tax authority's appeal to social norms does not change a taxpayer's compliance.*

We further ask whether the hypothesized effects differ for remote versus local taxpayers. Remote taxpayers may respond more positively than local taxpayers for several reasons. First, remote taxpayers likely face higher information frictions and hassle costs of compliance. Mascagni (2018) notes that complexity and compliance costs may be an important determinant of non-compliance. Murray (1995) examines the sales tax compliance game using data from Tennessee and notes that tax law complexities can drive both inadvertent and deliberate non-compliance. Because the sales tax is inherently complex and administratively burdensome (e.g., each state has different tax base rules and methods for filing and paying, in addition to a potentially large number of local tax rules, filings, and payments), taxpayers may focus their compliance efforts in states where they have the greatest familiarity and collection requirements.[11] Tax authority correspondence may therefore benefit remote taxpayers more than local taxpayers and elicit a stronger response from remote taxpayers.

Further, tax correspondence may revise remote taxpayers' beliefs more than they revise local taxpayers' beliefs. Castro and Scartascini (2015) examine individual property taxes in Latin American and find that both deterrence and reciprocity messages produce positive and larger effects on compliance for individuals with property in the taxing jurisdiction but who live outside the jurisdiction than for the overall population. One explanation is that individuals outside the jurisdiction react more to the information about deterrence and public services than individuals living in the jurisdiction, who can directly witness behaviors of the jurisdiction and have stronger prior beliefs about the likelihood of detection and of public goods provisions. Therefore,

[11] Cohn (2021) reports that midsized businesses face significant sales tax complex costs. Borens et al., (2020) discuss a recent Congressional hearing focused on the compliance burden imposed on businesses in light of the *Wayfair* decision. Afonso (2019) analyzes state sales tax complexity in a post-Wayfair world.

communications from a remote authority may alter taxpayers' prior beliefs and increase compliance more than communications from a local authority.

On the other hand, correspondence could decrease compliance more for remote taxpayers than local taxpayers. Braithwaite (2003) notes that the psychological concept of 'social distance' influences taxpayers' attitudes towards tax authorities. Compared to local taxpayers, remote taxpayers naturally have greater social distance with the tax authority, which can cause a lower sense of duty or obligation for taxpayers to comply. Taxpayers with a greater social distance may also not care as much to participate in the tax system and therefore be more willing to engage in non-compliance (Braithwaite, 2003). Correspondence from a more distant tax authority could therefore exacerbate taxpayers' sense of social and psychological distance. Relatedly, tax authority correspondence could increase the perceived detection risk more for local taxpayers than remote taxpayers, because the tax authority is much closer to local taxpayers and faces lower costs of enforcement.

Because the effects from traditional enforcement strategies could theoretically be either exacerbated or attenuated for remote taxpayers, we state our third and final hypothesis in the null form.

H3*: A remote tax authority's enforcement efforts have the same effect as a local tax authority's enforcement efforts.*

3. Sampling and Randomization

In the spring of 2021, we coordinated with the Texas Comptroller's Office to identify a sample of 2,000 remote taxpayers that had registered to pay sales and use taxes but had no reported physical presence in the state of Texas. The Comptroller's Office randomly selected 1,000 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 2,000 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 submit their return or payment later than 20 days (45 days) after the due date, or not at all.

The groups were defined by the Texas Comptroller's Office based on firms' sales tax filings during the 12-month period beginning January 1, 2020 and ending December 31, 2020. The total remote population we drew from consisted of 5,734 late filers, and 4,245 extra-late filers. Hence, our sample represents approximately 17 percent and 24 percent of the population of remote late and extra-late filers, respectively. The total number of out-of-state firms in the population, including those with no signs of delinquency in the past year, is 24,350.

We then matched each remote firm with a comparable local firm (a firm with a physical presence in Texas), using information available from sales tax filings. Specifically, we required local firms to match on filing status (late/extra-late), two-digit NAICS industry, and filing frequency. We also required local matches to have Texas taxable sales and total Texas sales within 10 percent of the remote firm.[12] Lastly, we matched on an indicator variable for whether the firms reported Texas taxable purchases, which are purchases made by the firm that are taxable but for which sales or use tax was not paid. This results in a total sample of 4,000 firms (2,000 remote, 2,000 local) to be assigned to treatment and control groups. Table 1 shows the sample selection, including the number of remote and local firms broken out by two-digit NAICS industry. The overall sample size chosen represents the maximum number of firms that the Texas Comptroller's Office was willing to treat, given resource constraints.

[12] "Total Texas sales" is the firm's reported taxable and non-taxable sales to Texas customers.

We then randomly assigned each pair of remote and local firms to one of our five conditions: (1) control; (2) inform; (3) deterrence; (4) fiscal exchange; and (5) social norms. In performing the randomization, we stratified (or block randomized) by filing status and industry. Stratifying by filing status allows us to examine the effects of our interventions on zero filers, late filers, and extra-late filers separately within an experimental framework. Stratifying by industry ensures that industry representation is balanced across our treatment and control groups, which is important in our setting. Specifically, because we examine remote taxpayers, certain industries (e.g., digital services or online retailers) could drive our results if that industry is correlated with our outcomes and is overrepresented in one treatment or control group. This approach also increases the power of our tests.

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 letters were identical except that conditions (3) through (5) also included a brief message related to deterrence, fiscal exchange, and social norms, respectively. The deterrence condition indicates that sales and use tax filings will be closely examined, and reminds the taxpayer of penalties for past-due taxes. The fiscal exchange condition highlights that sales tax revenues fund a significant portion of public services and contribute to the economic and physical well-being of customers in Texas. The social norms condition informs 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 letters received by taxpayers are included in Appendix B.

The purpose of the inform group (condition (2)) is to isolate the effect of receiving any letter from the tax authority. This group is also known as a “control letter” group, and including

this condition follows best practices in tax field experiments (Mascagni, 2018). Recent work shows that in some cases, simple reminders from the tax authority can account for a substantial portion of taxpayers' behavioral response (Del Carpio, 2014; Dunning et al., 2016).

The Texas Comptroller's Office sent out letters in early July 2021, such that firms should have received the letter at least ten days prior to the filing deadline for both monthly and quarterly filers of July 20th. We then tracked our dependent variables of interest over the next two quarters, giving us four post-period observations for monthly filers and two post-period observations for quarterly filers.

Table 2 presents descriptive statistics for the remote and local samples used in the regression main regression analyses.

4. Research Design and Results

Our research design permits us to make a number of important comparisons. We begin by examining the effects of our interventions on remote firms (Section 4.1), then explore whether the effects of our interventions are different in remote firms compared to the same interventions in local firms (Section 4.2).

4.1 Remote Taxpayers

Our empirical strategy examines the effect of receiving letters from a remote tax authority on a taxpayer's behavior. Our primary outcomes of interest relate to the amount of the tax liability and whether the return is filed timely. For the first, we use the natural logarithm of one plus the tax base (*TaxBase*), which is Texas taxable sales plus Texas taxable purchases. *TaxBase* multiplied by the sales tax rate equals sales and use taxes owed to the tax authority. We assume that any increase in the taxable base reflects a reduction in underreporting and therefore

an increase in compliance. The second is an indicator for whether the firm files a return by the due date (*Timely*).[13] We examine these effects using the following OLS regression:

$$\begin{aligned} TaxCompliance_{it} = {} & \alpha_{it} + \beta_1 Inform_{it} + \beta_2 Deter_{it} + \beta_3 Fiscal\ Exchange_{it} + \beta_4 Social\ Norms_{it} + \\ & \beta_5 Post_{it} + \beta_6 Inform_{it}{*}Post_{it} + \beta_6 Deter_{it}{*}Post_{it} + \beta_6 Fiscal\ Exchange_{it}{*}Post_{it} \\ & + \beta_6 Social\ Norms_{it}{*}Post_{it} + \gamma_i + \delta_t + \varepsilon_{it} \end{aligned} \quad (1)$$

We employ a difference-in-differences model that compares the changes in our dependent variables for remote firms that receive one of the four letters to those that do not. *Inform*, *Deter*, *Fiscal Exchange*, and *Social Norms* are indicator variables that represent the four letter conditions. *Post* is an indicator variable equal to one for observations in July 2021 and after, and zero for observations before July 2021. The post period covers July 2021 to October 2021, which yields four post-period observations for monthly filers and two post-period observations for quarterly filers. To account for seasonality, we use July 2020 to October 2020 as the pre-period in our main specification. We utilize other pre-periods (such as the two quarters before the intervention) in untabulated analyses and find largely similar results.

Our parameters of interest are the difference-in-differences estimators for each letter condition (*Inform*Post*, *Deter*Post*, *Fiscal Exchange*Post*, *Social Norms*Post*). Because equation (1) includes firm fixed effects γ_i and time fixed effects δ_t (using tax return due dates), the main effects are subsumed.

We present estimates of equation (1) in Table 3. In column 1, we find large point estimates for each letter condition on the *TaxBase*, relative to the no-letter condition, although none of the coefficients are statistically significant. In column 2, we find no evidence that the letter conditions significantly affect taxpayers' likelihood of filing on time.

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

Because the treatment effects may vary based on the time period relative to treatment, we examine the dynamic effects in Table 4. Depending on the column, the pre-period includes data from July, August, September, or October of 2020, and the post period includes data from the same month in 2021. We further break out monthly and quarterly filers because the filing dates represent different reporting periods for these groups. In Panel A, we find some evidence of a large and significant positive effect of remote tax correspondence on the tax base in the second reporting period after treatment. The coefficient of 0.84 on *Inform* × *Post* indicates an increase of 132 percent.[14] The magnitudes are economically meaningful compared to several prior field experiments.[15] The delayed effect is consistent with taxpayers needing time to change their sales tax collection and remittance procedures after receiving the letter. Further, the letter was sent in July, which is after the reporting period of June for monthly filers and April, May, and June for quarterly filers. It is unlikely firms would reach out to consumers to collect sales taxes on previous months' sales. It is therefore not surprising that the positive effects begin in the second reporting period. The estimates for September and October for monthly filers are not significant, which suggests the effect may be temporary. In Panel B, we find limited evidence of remote tax correspondence significantly affecting timeliness.

Tables 3 and 4 do not provide conclusive evidence of a dominating strategy, suggesting that informative correspondence (which was present across all conditions) may be sufficient to increase remote taxpayer compliance. We further explore the possibility that the letters reduced administrative costs in Section 5.2.

[14] Exp(0.84) - 1 = 1.32.

[15] For example, Shimeles et al., (2017) find that Ethiopian businesses that received a deterrence letter increased their reported profit tax payable by 38%, while those that received a persuasion letter increased tax payable by 32%, compared to the control group. Mascagni et al., (2018) found that letters increase reported taxes due in Rwanda by 44 percent. Bott et al., (2019) found that moral suasion and deterrence treatments increased self-reported foreign income by 70-80 percent.

4.2 Remote Taxpayers versus Local Taxpayers

We next explore whether the interventions described above have different effects in remote firms compared to local firms. We parallel the approach used above in examining the effects in remote firms, but we now add the sample of matched local firms and add interaction terms that capture differences between remote and local firms:

$$\begin{aligned} TaxCompliance_{it} = {} & \alpha_{it} + \beta_1 Inform_{it} + \beta_2 Deter_{it} + \beta_3 Fiscal\ Exchange_{it} + \beta_4 Social\ Norms_{it} + \\ & \beta_5 Post_{it} + \beta_6 Remote_{it} * Post_{it} + \beta_7 Remote_{it} * Inform_{it} * Post_{it} + \\ & \beta_8 Remote_{it} * Deter_{it} * Post_{it} + \beta_9 Remote_{it} * Fiscal\ Exchange_{it} * Post_{it} + \\ & \beta_{10} Remote_{it} * Social\ Norms_{it} * Post_{it} + \gamma_i + \delta_t + \varepsilon_{it} \end{aligned} \quad (2)$$

In equation (2) we introduce *Remote,* which is an indicator equal to 1 if the taxpayer is out-of-state, and interact it with each letter condition. Equation (2) is a triple-differences specification where our coefficients of interest are β_7 through β_{10}. That is, the coefficients on the triple interactions estimate whether the change in compliance among treatment firms compared to control firms is different for remote firms versus local firms. Because equation (2) includes firm fixed effects γ_i and time fixed effects δ_t (using tax return due dates), the main effects (β_1 through β_5) are subsumed.

Table 5 presents the results from estimating equation (2) for each outcome. In column 1, we estimate a positive and significant effect of deterrence on remote taxpayers' taxable base, relative to local taxpayers. The remaining coefficients on the triple interaction terms in column 1 are positive, but not statistically significant. We find no evidence of significant differences between remote and local taxpayers' likelihood of filing timely.

Our results provide important insights into firms' perceptions of remote authority. If out-of-state taxpayers viewed correspondence from a remote authority as a less-credible enforcement threat than in-state taxpayers viewed the correspondence, out-of-state taxpayers would have reported lower taxable bases relative to in-state firms–particularly in response to a deterrence

message. In contrast, we find out-of-state firms increase the reported taxable base *more* than in-state firms, which could suggest that deterrence messages from a remote authority cause firms to significantly revise upward their priors of detection risk. This finding could also stem from a ceiling effect if in-state firms are closer to full compliance in the pre-period and simply cannot increase the reported tax base as much as out-of-state firms. In both cases, the results suggest that firms view the remote authority correspondence as a credible enforcement threat, despite significant hurdles in a remote setting.

Taken together, the main results provide evidence that remote tax authority correspondence increases taxpayers' reported tax bases, particularly in the second reporting period after receiving a letter. We also find that deterrence strategies increase remote tax compliance more than local tax compliance. We next examine the potential mechanisms driving the main results.

5. Additional Analyses

We conduct two additional cross-sectional tests within the sample of out-of-state firms to further tease out the mechanisms by which remote authority affects taxpayer compliance.

5.1 Administrative Costs

Several studies suggest that administrative costs (e.g., information frictions and hassle costs of compliance) can explain low tax compliance or the low take up of tax benefits (e.g., De Neve et al., 2021). The *Inform* condition, in part, is intended to identify the effects of a reduction in taxpayers' information frictions relative to the no-letter control group. To provide more direct evidence on this potential mechanism, we partition our sample into groups that we expect to have significantly different administrative costs of compliance: retail firms and other firms. Sales tax calculations and filing requirements are better known and more easily understood by retail firms

than non-retail firms. Compared to non-retail firms, retail firms generally have greater familiarity with sales taxes, pay them more regularly, have more resources dedicated to sales tax compliance, and have more straightforward tax bases. Therefore, non-retail firms likely have relatively higher informational frictions and hassle costs and may benefit more from tax authority outreach than retail firms.

Table 6 presents the results of estimating equation (1) for both tax compliance outcomes within retail industries (columns 1 and 3) and within non-retail industries (columns 2 and 4). We find that the increase in the reported tax base is concentrated among non-retail firms (column 2) for all four different letter conditions, which each contain basic instructions and guidance that should reduce compliance frictions. Results with the sample of retail firms is notably different. Column 1 suggests that remote retail firms did not increase compliance in response to correspondence. This is consistent with remote authority outreach having the strongest effect when it reduces information frictions and hassle costs of complying with remote laws and regulations. To the extent that taxpayers face higher administrative costs to comply with remote authorities than to comply with local authorities, these costs provide one potential explanation for why taxpayers in this study responded more to a remote authority than to a local authority.

5.2. 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 a greater social distance may also have apathy towards the tax system and therefore be more willing to engage in non-compliance. Remote tax authority outreach could decrease compliance if the correspondence makes salient the distance between the taxpayer and

tax authority (i.e., it makes salient 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 7 presents the results of estimating equation (1) for each tax compliance outcome, separated by the distance from the firm's address to Travis County, Texas. Columns 1 and 3 present the estimates from the sample of firms that are located far from Travis County (above the sample median distance), and columns 2 and 4 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 short, we find weak evidence that more distant remote taxpayers increase compliance more than less distant remote taxpayers. Taken together with the results from Table 5, we find no evidence that taxpayers discount correspondence from remote and more distant authority, and they sometimes respond more strongly.

6. 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 nearly at all levels, finite tax administration resources incentivized us to focus on taxpayers with some history of delinquency. Nonetheless, we worked the state tax authority to ensure that the sample 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 more challenging because of the difficulty in identifying a taxpayer that had a physical presence in Texas to match with each sample firm that did not have a physical presence in Texas. The measures upon which we could practically match out-of-state and in-state taxpayers are (1) industry, (2) filing frequency, (3) taxable base, and (4) total Texas sales (both taxable and non-taxable sales to Texas customers). Because we could only match on total Texas sales instead of worldwide sales, it is possible that the out-of-state sample is larger than the in-state sample. One concern is that the stronger results for remote taxpayers compared to local taxpayers could be driven by size, and not some feature of being remote. We attempt to address this concern by testing whether the results from Table 3 vary by size, based on the total Texas sales reported. We re-estimate equation 1 separately for small and large remote taxpayers and find no significant differences between samples (Table 8). This reduces concerns that the results are driven by firm size instead of remoteness.

7. Conclusion

Governments across the world are increasingly seeking to constrain or restrain the behavior of businesses located outside of the government's physical borders. This is especially apparent in the context of taxation, as governments seek to tax companies without a physical

presence in the jurisdiction. Despite the legal authority to impose taxes on out-of-jurisdiction taxpayers, governments face challenges in encouraging and enforcing compliance.

In this study, we conduct a randomized field experiment in a state tax setting to ask whether remote tax authority outreach affects taxpayer behavior. Using a sample of out-of-state sales and use tax filers in Texas, we randomly assign taxpayers to one of five conditions. We send letters with varying information to four of the conditions and do not send correspondence to the remaining condition (control group). On average, we estimate higher reported taxable bases relative to the no-letter control group, although the estimates are not statistically significant. Further analyses suggest that letter correspondence substantially and significantly increases the taxable base in the second reporting period after receipt of the letter, although the effect seems temporary. We find some evidence that letter correspondence increases tax compliance of remote taxpayers more than local taxpayers.

To better understand the potential mechanisms driving the main results, we first test and find that letter correspondence differentially improves compliance for taxpayers more likely to face informational frictions and hassle costs (i.e., non-retail firms). This suggests that remote authority outreach may be most effective when aimed at taxpayers with greater administrative burdens. We find limited evidence that distance or size affects the results.

To our knowledge, we are the first field experiment to examine taxpayers' response to a remote authority, and the first to examine state sales tax collections. We are also one of the first cross-jurisdictional field experiments. Given the rise in taxation of out-of-jurisdiction firms and states' heavy reliance on sales tax revenue, this study provides important and timely insights to the tax field experiment literature that future studies can expand upon.

Our study offers practical insights to tax administrators grappling with incentivizing compliance. Taking all of our results together, we do not find conclusive evidence that one strategy works significantly better than another. This suggests that low costs strategies such as merely informing and guiding remote taxpayers can be effective.

Finally, our study contributes to the literature on the expanding reach of government influence. Recent regulatory changes demonstrate that jurisdictions are increasingly achieving this by passing extra-territorial regulation that exceeds the bounds of traditional sovereignty. The expansion of taxing rights to market jurisdictions (OECD, 2021), the growth in economic nexus provisions post *Wayfair*, automatic information sharing under the Foreign Accounting Tax Compliance Act (FATCA), and extraterritorial enforcement of foreign investment and internal controls under Foreign Corrupt Practices Act (FCPA) are all recent examples of this phenomenon. Our results provide the first evidence that simple correspondence from a remote authority can increase compliance.

Appendix A – Variable Definitions

Variable Name	Definition
Dependent Variables	
TaxBase	The firm's taxable sales plus taxable purchases during the period reported to Texas.
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.
Deter	Indicator equal to 1 if the firm was assigned to the *Deter* letter condition, and zero otherwise. See Appendix B for details.
FiscalExch	Indicator equal to 1 if the firm was assigned to the *FiscalExch* letter condition, and zero otherwise. See Appendix B for details.
SocialNorms	Indicator equal to 1 if the firm was assigned to the *SocialNorms* 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.
Distance	The number of miles from the center of the firm's county (based on the frm'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.
Retail	Indicator equal to 1 if the firm operates in NAICS sector 44 or 45.
Small	Indicator equal to 1 if the firm reports less 2020 *TotalSales* than the median 2020 *TotalSales* of all remote and local taxpayers.

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 follow*s: (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 «DUEDATE».

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."

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Table 1: Sample Selection

Panel A: Selection of Remote Sample

	Randomly-selected sample	Total population	% of Population
Late Filers	1,000	5,734	17%
Extra-late Filers	1,000	4,245	24%
Total	2,000	9,979	20%

Panel B: Remote and Local Sample used in Main Tests

	Firms	Firm-Periods
Remote: Late filers and Extra-late filers	2,000	12,250
Local: Late filers and Extra-late filers	2,000	12,303
Total	4,000	24,553

Panel C: Firms Assigned to Each Letter Condition

	Remote firms	Local firms
Control	400	400
Inform	400	400
Deterrence	400	400
Fiscal Exchange	400	400
Social Norms	400	400
Total	2,000	2,000

Table 1 (continued)

Panel D: Industry Breakout

NAICS Code	NAICS Description	N *(Remote=0)*	N *(Remote=1)*	N (Total)	Percent
11	Agriculture, Forestry, Fishing and Hunting	2	2	4	0.10%
21	Mining, Quarrying, and Oil and Gas Extraction	3	3	6	0.15%
23	Construction	178	178	356	8.90%
31	Manufacturing	75	75	150	3.75%
32	Manufacturing	50	50	100	2.50%
33	Manufacturing	193	193	386	9.65%
42	Wholesale Trade	284	284	568	14.20%
44	Retail Trade	193	193	386	9.65%
45	Retail Trade	395	395	790	19.75%
48	Transportation and Warehousing	1	1	2	0.05%
51	Information	86	86	172	4.30%
52	Finance and Insurance	12	12	24	0.60%
53	Real Estate and Rental and Leasing	44	44	88	2.20%
54	Professional, Scientific, and Technical Services	269	269	538	13.45%
55	Management of Companies and Enterprises	1	1	2	0.05%
56	Admin, Support, Waste Mgmt, and Remediation Services	91	91	182	4.55%
61	Educational Services	6	6	12	0.30%
62	Health Care and Social Assistance	2	2	4	0.10%
71	Arts, Entertainment, and Recreation	15	15	30	0.75%
72	Accommodation and Food Services	20	20	40	1.00%
81	Other Services (except Public Administration)	79	79	158	3.95%
92	Public Administration	1	1	2	0.05%
Total		**2,000**	**2,000**	**4,000**	**100.00%**

Notes: This table shows the number of remote and local firms included in the study, broken out by 2-digit NAICS industry sector.

Table 2: Descriptive Statistics

Panel A: Remote Taxpayers (pre-period)						
Variable	N	Mean	Std. Dev.	p25	p50	p75
Log(1+TaxBase)	6311	6.77	4.78	0.00	8.68	10.55
Timely	6311	0.70	0.46	0.00	1.00	1.00
Distance (miles)	2000	1088.26	392.33	843.55	1084.18	1443.02

Panel B: Local Taxpayers (pre-period)						
Variable	N	Mean	Std. Dev.	p25	p50	p75
Log(1+TaxBase)	6366	7.79	4.28	6.77	9.30	10.70
Timely	6366	0.69	0.46	0.00	1.00	1.00
Distance (miles)	2000	162.22	93.03	132.85	160.98	187.19

Notes: This table presents descriptive statistics for the remote taxpayers (Panel A) and the local taxpayers (Panel B). Both Panels include observations for late filers and extra-late filers during the pre-period. Appendix A defines all variables.

Table 3: Remote Tax Compliance

Variable	**(1)** ***Log(1+TaxBase)***	**(2)** ***Timely***
Inform × *Post*	0.32	0.03
	(1.35)	(1.01)
Deter × *Post*	0.31	-0.03
	(1.30)	(-1.04)
FiscalExch × *Post*	0.16	-0.02
	(0.68)	(-0.68)
SocialNorms × *Post*	0.31	0.03
	(1.33)	(1.18)
Firm Fixed Effects	Y	Y
Due Date Fixed Effects	Y	Y
Observations	12,250	12,250
R-Squared	0.704	0.442

Notes: This table presents results from estimating a difference-in-differences model (equation 1) examining the effect of letter correspondence on remote tax compliance. The pre-period includes data from sales tax returns due in July, August, September, and October of 2020, and the post-period includes data from sales tax returns due in July, August, September, and October of 2021. Appendix A 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).

Table 4: Remote Tax Compliance – Dynamics Effects

Panel A: DV = *Log(1+Tax Base)*

	Filing Date (Monthly Filers)				Filing Date (Quarterly Filers)	
	July	**August**	**September**	**October**	**July**	**October**
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Inform x Post	0.37	0.84**	-0.29	-0.15	0.32	1.22**
	(0.87)	(2.00)	(-0.73)	(-0.38)	(0.67)	(2.39)
Deter × Post	0.23	0.89**	0.01	-0.06	0.25	0.65
	(0.57)	(2.11)	(0.02)	(-0.14)	(0.60)	(1.53)
FiscalExch × Post	0.59	0.43	-0.33	-0.51	-0.01	1.10**
	(1.45)	(1.11)	(-0.82)	(-1.35)	(-0.02)	(2.41)
SocialNorms × Post	0.51	0.60	-0.42	0.24	0.16	1.06**
	(1.26)	(1.45)	(-1.02)	(0.58)	(0.35)	(2.31)
Sample	Monthly	Monthly	Monthly	Monthly	Quarterly	Quarterly
Firm Fixed Effects	Y	Y	Y	Y	Y	Y
Due Date Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	2,358	2,348	2,326	2,302	1,306	1,136
R-Squared	0.755	0.757	0.765	0.769	0.798	0.834

(continued)

Table 4 (continued)

Panel B: DV = *Timely*						
	Filing Date (Monthly Filers)				**Filing Date (Quarterly Filers)**	
	July	**August**	**September**	**October**	**July**	**October**
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Inform x Post	0.03	0.03	0.06	-0.01	0.04	0.02
	(0.75)	(0.66)	(1.30)	(-0.25)	(0.46)	(0.28)
Deter × Post	0.01	-0.04	-0.06	-0.01	-0.00	-0.10
	(0.19)	(-1.00)	(-1.36)	(-0.20)	(-0.00)	(-1.30)
FiscalExch × Post	0.04	-0.03	-0.09**	-0.03	-0.02	0.07
	(0.88)	(-0.64)	(-2.03)	(-0.65)	(-0.34)	(0.90)
SocialNorms × Post	0.05	0.04	-0.00	0.02	0.05	0.06
	(1.02)	(0.79)	(-0.08)	(0.53)	(0.70)	(0.71)
Sample	Monthly	Monthly	Monthly	Monthly	Quarterly	Quarterly
Firm Fixed Effects	Y	Y	Y	Y	Y	Y
Due Date Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	2,358	2,348	2,326	2,302	1,306	1,136
R-Squared	0.626	0.603	0.599	0.603	0.601	0.597

Notes: This table presents results from estimating the dynamic effects of letter correspondence on remote tax compliance. Each Panel uses a different dependent variable. Panel A uses the natural log of 1 + *TaxBase* and Panel B uses *Timely*. Columns 1-4 include only monthly filers, and columns 5-6 include only quarterly filers. The pre-period includes data from 2020 of the month indicated at the top of the column, and the post period includes data from 2021 of the same month. Appendix A 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).

Table 5: Remote versus Local Tax Compliance

	(1)	(2)
Variable	***Log(1+TaxBase)***	***Timely***
Inform × *Post* × *Remote*	0.30	0.04
	(1.02)	(0.97)
Deter × *Post* × *Remote*	0.50*	-0.05
	(1.70)	(-1.32)
FiscalExch × *Post* × *Remote*	0.10	-0.04
	(0.36)	(-1.13)
SocialNorms × *Post* × *Remote*	0.23	0.01
	(0.77)	(0.27)
Post × *Remote*	-0.13	0.09***
	(-0.61)	(3.45)
Inform × *Post*	0.03	-0.01
	(0.18)	(-0.30)
Deter × *Post*	-0.20	0.02
	(-1.10)	(0.81)
FiscalExch × *Post*	0.05	0.02
	(0.31)	(0.92)
SocialNorms × *Post*	0.09	0.02
	(0.50)	(0.84)
Firm Fixed Effects	Y	Y
Due Date Fixed Effects	Y	Y
Observations	24,553	24,553
R-Squared	0.759	0.432

Notes: This table presents results from estimating a difference-in-differences model (equation 2) examining the effect of letter correspondence on remote tax compliance relative to local tax compliance. The pre-period includes data from sales tax returns due in July, August, September, and October of 2020, and the post-period includes data from sales tax returns due in July, August, September, and October of 2021. The sample includes both remote and local late filers and extra-late filers. Appendix A 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).

Table 6: Cross-sectional Tests Based on Industry (Retail versus Other)

	(1)	(2)	(3)	(4)
Variable	***Log(1+TaxBase)***	***Log(1+TaxBase)***	***Timely***	***Timely***
Inform × Post	-0.29	0.58*	0.06	0.01
	(-0.97)	(1.85)	(1.26)	(0.42)
Deter × Post	-0.49	0.65**	-0.05	-0.02
	(-1.53)	(2.11)	(-1.08)	(-0.54)
FiscalExch × Post	-0.69*	0.49*	-0.02	-0.02
	(-1.95)	(1.71)	(-0.30)	(-0.60)
SocialNorms × Post	-0.40	0.61**	0.12**	-0.00
	(-1.07)	(2.08)	(2.37)	(-0.03)
Sample	Retail	All Other	Retail	All Other
Firm Fixed Effects	Y	Y	Y	Y
Due Date Fixed Effects	Y	Y	Y	Y
Observations	3,474	8,776	3,474	8,776
R-Squared	0.777	0.676	0.469	0.433

Test for Differences	***Log(1+TaxBase)***	***Timely***
Inform × Post × Retail	-0.87**	0.05
	(-2.00)	(0.82)
Deter × Post × Retail	-1.14**	-0.03
	(-2.56)	(-0.60)
FiscalExch × Post × Retail	-1.19***	0.00
	(-2.59)	(0.06)
SocialNorms × Post × Retail	-1.01**	0.12**
	(-2.12)	(2.02)

Notes: This table presents results from estimating a difference-in-differences model (equation 2) separately for retailers and other remote taxpayers. The odd-numbered columns include the "Retail" sample, and the even-numbered columns include the "All Other" sample. The "Retail" sample includes remote taxpayers whose 2-digit NAICS code equals 44 or 45. The "All Other" sample includes remote taxpayers whose 2-digit NAICS code equals anything other than 44 or 45. The pre-period includes data from sales tax returns due in July, August, September, and October of 2020, and the post-period includes data from sales tax returns due in July, August, September, and October of 2021. Below the regression results, we report coefficients and t-stats from a fully interacted model to test for differences in effect sizes. Appendix A 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).

Table 7: Cross-sectional Tests Based on Proximity

	(1)	(2)	(3)	(4)
Variable	***Log(1+TaxBase)***	***Log(1+TaxBase)***	***Timely***	***Timely***
Inform × Post	-0.11	0.72**	0.03	0.02
	(-0.32)	(2.16)	(0.66)	(0.64)
Deter × Post	0.29	0.32	0.04	-0.09**
	(0.86)	(0.97)	(1.12)	(-2.51)
FiscalExch × Post	0.36	-0.06	0.01	-0.05
	(1.10)	(-0.19)	(0.28)	(-1.20)
SocialNorms × Post	0.31	0.32	0.06	0.01
	(0.97)	(0.91)	(1.56)	(0.19)
Sample	Far	Near	Far	Near
Firm Fixed Effects	Y	Y	Y	Y
Due Date Fixed Effects	Y	Y	Y	Y
Observations	6,091	6,159	6,091	6,159
R-Squared	0.68	0.73	0.446	0.442

Test for Differences	***Log(1+TaxBase)***	***Timely***
Inform × Post × Far	0.83*	-0.00
	(1.72)	(-0.02)
Deter × Post × Far	0.04	-0.13***
	(0.08)	(-2.59)
FiscalExch × Post × Far	-0.42	-0.06
	(-0.93)	(-1.06)
SocialNorms × Post × Far	0.00	-0.05
	(0.00)	(-0.91)

Notes: This table presents results from estimating a difference-in-differences model (equation 2) separately for far and near 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. The pre-period includes data from sales tax returns due in July, August, September, and October of 2020, and the post-period includes data from sales tax returns due in July, August, September, and October of 2021. Below the regression results, we report coefficients and t-stats from a fully interacted model to test for differences in effect sizes. Appendix A 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).

Table 8: Cross-sectional Tests Based on Size

	(1)	(2)	(3)	(4)
Variable	***Log(1+TaxBase)***	***Log(1+TaxBase)***	***Timely***	***Timely***
Inform × Post	0.48	0.22	0.02	0.03
	(1.24)	(0.72)	(0.38)	(0.95)
Deter × Post	0.56	0.13	-0.01	-0.04
	(1.42)	(0.45)	(-0.15)	(-1.41)
FiscalExch × Post	-0.08	0.31	-0.00	-0.03
	(-0.21)	(1.10)	(-0.10)	(-0.84)
SocialNorms × Post	0.62*	0.09	-0.01	0.06
	(1.66)	(0.31)	(-0.16)	(1.58)
Sample	Small	Large	Small	Large
Firm Fixed Effects	Y	Y	Y	Y
Due Date Fixed Effects	Y	Y	Y	Y
Observations	5,099	7,151	5,099	7,151
R-Squared	0.584	0.711	0.441	0.426

Test for Differences	***Log(1+TaxBase)***	***Timely***
Inform × Post × Small	0.26	-0.02
	(0.53)	(-0.32)
Deter × Post × Small	0.43	0.04
	(0.87)	(0.75)
FiscalExch × Post × Small	-0.39	0.02
	(-0.82)	(0.43)
SocialNorms × Post × Small	0.53	-0.06
	(1.10)	(-1.19)

Notes: This table presents results from estimating a difference-in-differences model (equation 2) separately for small and large remote taxpayers. The odd-numbered columns include the "Small" sample, and the even-numbered columns include the "Large" sample. The "Small" sample includes remote taxpayers who have less total Texas sales in 2020 than the median amount of all remote taxpayers in 2020. The "Large" sample includes remote taxpayers who have greater total Texas sales in 2020 than the median amount of all remote taxpayers in 2020. The pre-period includes data from sales tax returns due in July, August, September, and October of 2020, and the post-period includes data from sales tax returns due in July, August, September, and October of 2021. The sample only includes remote late-filers and non-filers. Below the regression results, we report coefficients and t-stats from a fully interacted model to test for differences in effect sizes. Appendix A 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).