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PAYING YOUR FAIR SHARE: PERCEIVED FAIRNESS AND TAX COMPLIANCE

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ABSTRACT

We provide evidence on the role of fairness for tax compliance: households are willing to pay more in taxes if they believe that other households are contributing their fair share. We conducted an information-disclosure natural field experiment in the context of property taxes in the United States. We induced exogenous shocks to households' perceptions about the average tax rate paid by other households. We find that a higher perceived average tax rate decreases the probability of filing a tax appeal. Translating our estimates into a money metric, we find that for each additional $1 contributed by the average household, a taxpayer is willing to pay an extra $0.43 in his or her own taxes.

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1 Introduction

Why do individuals and firms pay their taxes? One natural explanation is that the decision to pay taxes is the result of self-interest: subjects conduct rational cost-benefit calculations. For example, taxpayers can weigh the savings from under-reporting income against the expected punishment if caught cheating (Becker, 1968; Allingham and Sandmo, 1972; Hoopes, Mescall, and Pittman, 2012). Beyond self-interest, individuals may pay their taxes due to tax morale reasons, such as believing that it is the fair thing to do.[1] This paper examines the role of fairness considerations in tax compliance. Specifically, we test the hypothesis that households are more willing to pay their taxes when they believe that other households are paying their fair share.

We designed and implemented a pre-registered, natural field experiment in the context of property taxes in the United States. This is an important context, as property taxes are a significant source of revenue for governments around the world. In 2018, property taxes were the second largest source of tax revenue in the United States, at an estimated $547 billion. This was more than twice the revenue generated by the corporate income tax at about $230 billion.[2] The setting of property taxes offers several key advantages for studying fairness considerations as a driver of the decision to pay taxes. First, there is significant variation across households in effective property tax rates (hereafter, we use the term "tax rate" to refer to a household's effective tax rate, calculated as the total property tax amount divided by the home's market value). While the tax schedule is proportional to the home value, different districts have different tax rates, and there is a large number of exemptions and caps that make some households pay effective tax rates that are much lower or higher than the average. Second, many households have large misperceptions about whether their own tax rate is below or above the average. These misperceptions allow us to induce exogenous information shocks through an information-disclosure experiment. Last, our setting allows us

[1] The fairness mechanism belongs to a set of motives referred to as "tax morale" (Luttmer and Singhal, 2014; Slemrod, 2019), which embraces a variety of non-deterrence motives such as altruism and civic duty.
[2] For reference, the federal income tax generated $1.7 trillion (Tax Policy Center, 2021).

to study the willingness to pay taxes through revealed-preferences even in a high-enforcement context, via the decision to file a tax appeal.[3]

To test our fairness hypothesis, an ideal experiment would be to randomize the average tax rate paid by others while holding constant the household's own tax rate. According to our fairness hypothesis, we would expect that, conditional on the household's own tax rate, a higher average tax rate would increase the household's willingness to pay taxes.[4] That is, while households would always have the temptation to free-ride on the taxes paid by others, they may feel a stronger moral obligation to pay their own taxes if they believe that other households are contributing their fair share.

However, the ideal experiment would not be feasible because randomizing tax rates would be impractical, and potentially illegal and unethical. Our research design does the next best thing, by disclosing information to induce exogenous shocks to households' *perceptions* of the average tax rate. As part of the research design, we pre-registered a complementary survey experiment, with subjects recruited via an online platform (hereafter, we refer to this survey as the "Survey Experiment"). We elicit beliefs about the average tax rate both before and after we disclose information on the average tax rate. The main goal of this survey is to validate the design of our information-disclosure field experiment. We show that homeowners have large misperceptions about the average tax rate that other households pay. More precisely, households tend to believe that the average household pays the same rate as themselves. When provided with accurate information about the average tax rate, subjects update their beliefs accordingly. That is, households with below-average tax rates learn that the average tax rate is higher than they thought and revise their beliefs about the average rate upwards, while households with above-average tax rates learn that the average rate is lower than they thought and revise their beliefs downwards.

[3] For other studies that leverage the context of property tax appeals, see Jones (2019), Nathan et al. (2023) and Giaccobasso et al. (2023). For more examples of studies in accounting and economics that use field experiments, see Floyd and List (2016).

[4] Our hypothesis is related to what in other settings has been called "reciprocal fairness" (Luttmer and Singhal, 2014) and also "conditional cooperation" (Fischbacher et al., 2001; Gachter, 2007).

In the field experiment, we provide misinformed households with accurate information on the average tax rate.[5] Specifically, we sent letters to a sample of homeowners and randomly varied whether the letters included information about the average tax rate or not. Because the information on property tax appeals is public, we can then observe in the administrative data how homeowners' behavior is affected in response to the treatment in the letter. Furthermore, we conducted a complementary field survey to which subjects could respond by visiting a URL contained in the letters (hereafter, the "Field Survey"). We leverage these survey data in multiple ways. For instance, we included a specific question to provide direct evidence on the "proportionality norm:" i.e., that most taxpayers prefer a proportional tax schedule.[6]

The field experiment took place in Dallas County, which is the second largest county in Texas with 2.6 million inhabitants, making it larger than 15 of the 50 U.S. states (U.S. Census Bureau, 2021). Property taxes work almost identically in all 241 counties in Texas and work similarly in other states and countries (Dobay et al., 2019; World Bank, 2019; Nathan et al., 2023). The average household in Dallas County pays about $5,916 in property taxes, which corresponds to an effective tax rate of 2.01% of its home value. This average tax rate masks substantial heterogeneity between households: for example, a household in the 10th percentile pays a tax rate of 1.33%, while a household in the 90th percentile pays a tax rate of 2.73%.[7]

The main outcome of interest is whether a household files a tax appeal. Filing an appeal is a consequential action that households can take to legally reduce their tax burden. The tax appeal process works as follows. Each year, the Dallas Central Appraisal District (DCAD) determines a proposed assessment of the market value of the property, which we refer to as the "proposed value." Property taxes are calculated based on this value. Given that the appraisal process involves significant ambiguity and subjectivity, households have

[5] The field experiment included an additional treatment arm, designed to study the role of filing frictions, which is reported in a different paper (Nathan et al., 2023).

[6] While some taxpayers prefer either progressive or regressive taxes in our context, the preference for tax progressivity may be stronger in other contexts, such as the income tax (e.g., Ballard-Rosa et al., 2017).

[7] This variability is driven by a number of factors, such as special exemptions, a homestead cap, and variation in tax rates across within-county geographical jurisdictions.

the opportunity to contest the counties' assessed values of their homes by filing property tax appeals, also known as tax protests. Many households file tax appeals, which often result in a significant reduction in their tax burden. For example, in the control group of our experiment, 11.08% of the households filed an appeal.[8] Around 76.6% of these appeals resulted in a lower assessed value of the home. Among these successful appeals, tax savings were on average $446 in the first year alone.

We sent letters to a sample of 50,394 households. All letters included information on the household's own taxes. Furthermore, we randomly chose whether the letter also disclosed information on the taxes paid by the average household in the county. This additional information was provided to induce an information shock. We show that the information shocks have a significant effect on real-world behavior and in the expected direction. When a household learns that the average tax rate is higher (or lower) than it thought, this causes a reduction (or increase) in the probability of filing a tax appeal, as measured via administrative records.

This estimate of the intention-to-treat effect that we find in the field experiment, however, suffers from severe attenuation bias due to multiple forms of non-compliance. For example, a large fraction of households may not have opened the letter or may not have paid close attention to the information contained in the letter (e.g., Perez-Truglia and Cruces, 2017; Gerber et al., 2020; Bottan and Perez-Truglia, 2020). To address this challenge, we leverage the responses from 1,888 subjects who participated in the Field Survey. We have strong confirmation that survey respondents paid close attention to the content of the letter, otherwise they could not have known about the personalized survey code needed to complete the survey. As expected, the information shocks have substantially larger effects in the sample of survey respondents. Additionally, we need to address a second form of non-compliance: in information-disclosure experiments, typically subjects do not fully update their beliefs when provided with new information. Our specification that corrects for both forms of non-

[8] Households in the control group are those who received a letter but for whom the letter did not include information on the proposed value and tax amount for the average household in Dallas County.

compliance indicates that a 0.1 pp increase in the perceived average tax rate decreases the probability of a tax appeal by 2.75 pp.

Our interpretation is that the effects of the information shocks on tax appeals operate through the fairness channel. The Field Survey included a question on whether the respondent feels that his or her property taxes are fair. Using these data, we show that after households learn that the average tax rate is higher (lower) than they initially thought, they are more likely to perceive their own taxes as fairer (less fair). In addition, we show that households who perceive their own taxes as fairer are less likely to protest. Finally, we provide evidence that our results are not driven by the households' direct monetary incentives.

We can translate our estimated fairness effect into a money metric. To do this, we take advantage of quasi-experimental evidence from Nathan et al. (2023), which estimates the effect of expected tax savings on the probability of filing a tax appeal. Our findings suggest that a 0.1 pp increase in the perceived average tax rate (equivalent to an increase of $295 in the average tax amount) increases a household's willingness to pay taxes by about $128. In other words, for each additional $1 contributed by the average household, a household, on average, is willing to pay an additional $0.43 in his or her own taxes. The magnitude of this effect is consistent with the results of laboratory experiments of the public goods game, according to which an additional $1 in average contributions increases the individual's own contribution by $0.45 (e.g., Fischbacher et al., 2001).

Our study contributes to the literature on tax morale (Luttmer and Singhal, 2014). The accounting and economics literature has documented that taxpayer behavior aligns with the predictions of cost-benefit analyses, such as research showing that higher audit rates or harsher penalties reduce evasion (Hoopes, Robinson, and Slemrod, 2018; Baugh, Ben-David, and Park, 2018; Belnap, Welsch, and Williams, 2023).[9] However, there is limited compelling evidence to support the role of tax morale (Slemrod, 2019; Giaccobasso et al.,

[9] More broadly, this study is related to an accounting literature on tax planning decisions (Hanlon et al., 2022).

2023).[10] There are a few exceptions. Most related to our experiment, Hallsworth et al. (2017), Del Carpio (2022), and Tanner and Manwaring (2023) provide evidence suggesting that providing information on descriptive norms (e.g., a message that "9 out of 10 households pay their taxes on time") can reduce late payments and tax delinquency. We contribute to this literature by providing novel evidence on the role of fairness concerns in tax compliance. In addition, we are the first to document evidence on a "proportionality norm." Our findings are also consistent with, and complementary to, evidence on other normative considerations such as benefit-based taxation or reciprocity (Giaccobasso et al., 2023; Chow et al., 2023).[11]

Our study also contributes to the broader literature on the role of fairness considerations for economic decision-making (see e.g., Andreoni, 1995; Fehr and Schmidt, 1999; Fehr and Schurtenberger, 2018).[12] In the context of laboratory games, multiple studies document conditional cooperation: i.e., despite individuals' incentives to free-ride and the absence of apparent private benefits from cooperation, individuals want to contribute more to public goods when they believe others are contributing too (Andreoni, 1995; Fehr and Schmidt, 1999; Gachter, 2007; Cappelen et al., 2013; Fehr and Schurtenberger, 2018). There is also survey evidence that fairness considerations may play an important role in stated preferences for redistribution (e.g., Alesina and Angeletos, 2005; Kuziemko et al., 2015; Hvidberg et al., 2020). We contribute to this literature by providing evidence that fairness considerations are significant in the natural, real-world context of property taxes and based on revealed preferences with high-stakes behavior. We find evidence that fairness plays a role in households' tax compliance. Moreover, we provide a measurement of fairness concerns using a money

[10] For example, Antinyan and Asatryan (2019) compared the role of institutions and tax morale using a meta-analysis of approximately 1,000 treatment effects from 45 randomized control trials. They conclude that "interventions pointing to elements of individual tax morale (...) are on average ineffective in curbing tax evasion, while deterrence nudges (...) are potent catalysts of compliance."

[11] Giaccobasso et al. (2023) use experimental data to show that households are more willing to pay property taxes if they perceive that the government uses their tax dollars to provide government services that benefit them. Furthermore, using data from companies' 10-K statements, Chow, Fan, Huang, Li, and Li (2023) demonstrate companies' tax planning is negatively related to the amount of ground-level ozone pollution, suggesting a tax-public goods reciprocity between firms and the government.

[12] Anthropologists often consider fairness, cooperation, and reciprocity as features that are present among all people, or "human universals" (Brown, 1991).

metric.

Lastly, our study also contributes to a small but growing literature on normative considerations in taxation. Although normative considerations played a key role among the first tax theorists (e.g., Seligman, 1908; Musgrave, 1959), the modern literature has largely ignored normative considerations to focus solely on efficiency (Weinzierl, 2018; Scherf and Weinzierl, 2020). More recently, some studies have tried to incorporate normative considerations into the design of tax policy (Mankiw and Weinzierl, 2010; Weinzierl, 2014; Saez and Stantcheva, 2016; Weinzierl, 2017). The existing studies are mostly theoretical, and empirical evidence is limited to survey data, for example, on stated preferences over hypothetical tax policies. We fill this gap in the literature by providing evidence of normative considerations via revealed-preferences and in a natural, high-stakes context.

The remainder of the paper proceeds as follows. Section 2 presents the Survey Experiment. Section 3 discusses the design of the field experiment, while Section 4 presents its results. The last section concludes.

2 Survey Experiment

In this section, we discuss the complementary Survey Experiment, which we created to validate the design of the information-disclosure mailing intervention and the econometric model that we use in the field experiment. Specifically, we use the Survey Experiment to measure misperceptions about the average tax rate and study how subjects update their beliefs when provided with accurate information.

2.1 Survey Design

We provide the full survey instrument that we used in the Survey Experiment in Appendix G. In the Survey Experiment, we first elicit prior beliefs about the market value and property taxes of the average home in subjects' county of residence in 2018. Then, we

inform all subjects that they will be randomly selected to receive or not receive accurate feedback about these variables. Next, we share the feedback with those randomly selected to receive the feedback (i.e., treatment group) and we do not share this information with the rest of the subjects (control group). Finally, we elicit posterior beliefs about both variables from the treatment and control groups. To avoid asking the same question twice, and following the design from other information-disclosure experiments (e.g., Cavallo et al. (2017)), we elicit posterior beliefs and prior beliefs using slightly different questions: we elicit priors about 2020 and posteriors about 2021.[13]

2.2 Implementation

We conducted the Survey Experiment on around the same dates as the field experiment, from June 5$^{\text{th}}$ to June 15$^{\text{th}}$, 2020. We pre-registered this complementary survey as part of the same AEA RCT registration as the field experiment. We recruited participants on Amazon Mechanical Turk, following the best practices for recruiting individuals from online platforms.[14] We restricted participation in the Survey Experiment to respondents located within the United States. In the survey announcement, we explicitly noted that the survey was restricted to homeowners. We also included screening questions at the beginning of the survey.[15]

We collected responses from 2,065 U.S. homeowners. The median respondent took about 7 minutes to complete the survey. At the end of the survey, we included an attention check, as used in previous studies (e.g., Bottan and Perez-Truglia (2022, 2020)). A total

[13] We also include other questions in the Survey Experiment to evaluate norms about the fair distribution of taxes and to obtain information on subjects' characteristics.

[14] We created a restriction so that each Mturk worker ID could only complete the survey once. We filter out respondents who did not complete the entire survey. To address potential concerns with outliers, we followed the standard practice of filtering out the upper and lower 0.1 percentiles in prior beliefs as well as winsorizing the upper and lower 0.1 percentiles in posterior beliefs (Fuster et al., 2022).

[15] Using these filters, we exclude respondents who are not homeowners, respondents under the age of 18 and respondents who live with their parents. For these respondents, as well as those who lived in counties for which we did not have data from the American Community Survey (ACS), the survey ended immediately after the screening questions. Respondents who passed these filters were allowed to continue with the rest of the survey.

of 99% of the respondents passed the attention check. According to their self-reports, the respondents did not find the survey difficult: 87% said that it was "easy to understand", 12% said that it was "neither easy nor difficult," and the remaining 1% found it "difficult to understand."

2.3 Misperceptions about the Average Tax Rate

In this section, we define the learning model that we use in the Survey Experiment and present the results. Let $\overline{\tau}_i^{prior}$ and $\overline{\tau}_i^{post}$ represent subject i's prior and posterior beliefs about the average tax rate, before and after the information-provision stage, respectively. We elicit $\overline{\tau}_i^{prior}$ and provide the actual average tax rate $\overline{\tau}^{feed}$ as feedback. Subjects' responses allow us to learn about the degree of households' misconceptions about the average tax rate, as represented below:

$$\overline{\tau}^{feed} - \overline{\tau}^{prior} \tag{1}$$

The information from the Survey Experiment also allows us to study how subjects learn when given feedback. Specifically, we use a simple learning model:

$$\overline{\tau}^{post} - \overline{\tau}^{prior} = \alpha \cdot \left(\overline{\tau}^{feed} - \overline{\tau}^{prior}\right) \tag{2}$$

where α captures the degree of learning. The parameter α captures how much individuals ignore or adjust to the feedback given.

Figure 1 shows how the information treatment affected the posterior beliefs in the Survey Experiment. In panel (a), the x-axis represents the respondent's *actual* relative tax rate: i.e., the difference between the *actual* average tax rate in the county and the household's own tax rate. In turn, the y-axis represents the respondent's *perceived* relative tax rate at the start of the survey: i.e., the difference between the *perceived* average tax rate and the household's own tax rate. In a world of perfectly accurate perceptions, we would expect the observations to align precisely along the 45-degree line, corresponding to a slope of 1.

On the opposite end of the spectrum, if subjects have no idea about the county's *actual* average tax rate to be able to compare their own tax rate, we would expect a slope of 0. We find that households' perceptions are further from the extreme of being accurate: the coefficient estimate (0.237) falls significantly short of 1. More precisely, Figure 1.a shows that misperceptions are systematically skewed toward the middle: individuals who pay more than average tend to underestimate how much they pay relative to others, and individuals who pay less than average tend to overestimate how much they pay. As a result, when accurate information is provided, we expect individuals toward the left end of the x-axis in Figure 1.a to update their beliefs downward and individuals toward the right end of the x-axis to update their beliefs upward.

This type of systematic bias is expected if individuals have little or no information about the average rate, so they use their own tax rate as their best guess for what others pay. In fact, this systematic bias has been found in other contexts. For example, when employees must guess the average salary of their peers, they typically use their own salary as their best guess (Cullen et al., 2020). Additionally, when guessing their position in the income distribution, people have a systematic tendency to believe that they are in the middle of the distribution (Cruces et al., 2013).

Figure 1.b shows how subjects in the treatment and control groups update their beliefs. Just as in panel (a), the x-axis in panel (b) is the respondent's *actual* relative position with respect to the average tax rate. However, the y-axis in panel (b) is different from that in panel (a): instead of showing the prior beliefs, panel (b) presents the posterior beliefs. In other words, the y-axis represents the respondent's *perceived* relative tax rate at the end of the survey (i.e., after the information-provision experiment). The blue dots correspond to subjects in the control group (i.e., subjects who were not shown feedback about the actual average tax rate). For this group, the relationship between perceptions and reality continues to be weak, just as in panel (a). This is to be expected: since these respondents did not receive any new information, we would not expect their posterior beliefs to become any more

accurate. In contrast, the red diamonds correspond to the treatment group (i.e., subjects who were shown the feedback). These red diamonds show that the correlation between perceptions and truth becomes markedly stronger when individuals are provided with accurate feedback. For a more formal test, we compare the slope between perceptions and truth in the control group (0.154) versus the corresponding slope in the treatment group (0.609). Consistent with significant learning, the difference between the two is not only large (0.455) but also highly statistically significant (p-value$<$0.001).

Figure 1.b illustrates a key fact that motivates the experimental design for the field experiment. When individuals receive accurate information on the average tax rate, their reaction depends on their prior beliefs. For individuals with accurate prior beliefs, they should not react to the information because it does not contain any news. For individuals whose prior beliefs under-estimate the average tax rate, they should react to the information by increasing their posterior beliefs. For individuals whose prior beliefs over-estimate the average tax rate, they should react to the information by lowering their posterior beliefs. In the field experiment, we do not observe the prior beliefs of the subjects. However, we can still capture the heterogeneous effects of the information, by leveraging whether the individuals have tax rates that are above or below the county average (i.e., whether the observation falls toward the left or right on the x-axis). On the one hand, individuals with tax rates above the average tend to systematically underestimate the average tax rate and, when shown the information, they update their perceptions upward. On the other hand, individuals with tax rates below the average tend to overestimate the average tax rate, so when shown the information, they will systematically update their perceptions downward. In sum, in the field experiment we can capture the heterogeneous effects of the information shock even though we do not observe prior beliefs.

3 Field Experiment

3.1 Institutional Context

We conducted our field experiment in Dallas County, a large county with a population of approximately 2.6 million as of 2020 (U.S. Census Bureau, 2021). The county is diverse along various dimensions, including ethnicity and representation of political parties. For example, in the 2012 presidential election, Barack Obama received 57% of the votes, while Mitt Romney received 42% (Ansolabehere et al., 2014). In Dallas County, property taxes are collected to fund a variety of services such as schools, parks, roads, and the police and fire departments. Although the county collects property taxes for residential and business properties, our study focuses specifically on single-family residential homes.

To conduct our research, we use publicly available administrative data provided by the Dallas Central Appraisal District (DCAD, 2020). These data include information on the names and mailing address of the property owners, property address (which may not coincide with the mailing address of the owners), and details about the properties, such as their characteristics (e.g., number of bedrooms, square footage), taxable values, tax rates, and history of property tax appeals. We use the administrative data to include a rich set of control variables in the regression analysis.

Tax appeals in Dallas County operate in a similar manner as in all 241 counties in Texas. Although there may be some variations, there are many similarities in how tax appeals work in other states within the United States and even in other countries (Dobay et al., 2019; World Bank, 2019). Each year, homeowners have the opportunity to appeal their taxes if they disagree with their home's market value as proposed by the DCAD. Homeowners are notified of their proposed values through the DCAD's website, and some may also receive a notification by mail.[16] Homeowners can challenge the tax assessment if there are errors in the public records (e.g., incorrect square footage), if they believe that the market value

[16] The DCAD mails notifications to households with increased appraised values or those meeting certain criteria (for more details, refer to Nathan et al. (2023)). See Appendix F for a sample notification.

of their property is lower than the proposed value, or if they believe similar households in terms of market value received lower proposed values than their own. It is worth noting that home market values are subjective, except perhaps for homes that have recently been sold and thus have a recent transaction price. For example, a quick search on websites such as Zillow.com and Redfin.com shows wide discrepancies in market values for the same home.[17]

Homeowners have the option of filing a tax appeal on their own, either online or by mail, which we refer to as "direct protests" following previous research (Nathan et al., 2023; Giaccobasso et al., 2023). As in Nathan et al. (2023) and Giaccobasso et al. (2023), direct protests are the main outcome of interest in our study. However, homeowners can also choose to hire an agent to file a protest on their behalf. These agents typically charge a combination of a flat fee and a percentage of tax savings, sometimes reaching up to 50% of the savings. However, protests conducted through agents are less relevant to our study for several reasons. First, since we mailed the letters directly to the homeowners, it is unlikely that the information provided to the homeowners would influence the decisions of the agents. Furthermore, the timing of our intervention and the setting make it more difficult for protests through agents to be affected. According to anecdotal accounts, households often enter into contracts with agents months before the proposed values are announced. Agents have an incentive to mechanically protest, since their cost of protesting is low. The decision to protest through an agent may have been made years ago, since agents offer long-term contracts to automatically protest on the owner's behalf every year. In addition, homeowners in all counties in Texas are required to complete and submit a form to their appraisal district to discontinue the services of an agent. This creates a stickiness for households, which implies that agents often handle protests on behalf of owners every year. For these reasons, we focus on direct protests.

Homeowners have a one-month window from the notification date to protest their taxes. In 2020, the DCAD notified the proposed values on May 15th, and the deadline to file

[17] This issue recently caused financial problems for Zillow (Parker and Friedman, 2021).

a protest was June 15th. Once protests are initiated, they go through a resolution process that may involve a proposed settlement by the DCAD, informal negotiations between the owner and the DCAD typically conducted online, via email, or by phone, or a formal hearing with a quasi-judicial entity known as the Appraisal Review Board. As a result of this process, the taxable value of the property is finalized (thereon called the "certified value"), unless it is further challenged in court.

As our experiment took place in 2020, it is important to assess the impact of the COVID-19 emergency on the external validity of our findings. It should be noted that the emergency did not alter the procedure for households to protest their property taxes. For example, the uFile tool that the DCAD uses for online tax protests had been in use for several years prior to our intervention in 2020. Furthermore, the emergency did not affect the way informal settlements were conducted, as they had already been offered by email or telephone prior to 2020. The only change implemented in response to the emergency was that formal hearings were conducted over the phone with a single board member. However, it is worth mentioning that even after 2020, the DCAD continued to provide homeowners with the option to conduct Appraisal Review Board (ARB) hearings over the phone. The Field Survey in our letters included an open-ended question that asked respondents to explain why they decided to protest or not to protest. Only a small minority (3.9%) of the respondents mentioned the pandemic as a factor influencing their decision. Taken together, these factors indicate that while the COVID-19 emergency presented unique circumstances in 2020, it did not significantly alter the core processes of property tax protests.

3.2 Main Hypothesis

Our research question can be summarized in a simple equation. Let WTP_i be household i's willingness to pay taxes – in the field experiment, we use the the decision to file a tax appeal as a proxy for the (un)willingness to pay taxes. Let $\hat{\tau}_i$ be household i's perception about the average tax rate paid by other households in the county. We are interested in the

following relationship:

$$WTP_i = \mu_0 + \mu_1 \cdot \hat{\bar{\tau}}_i \tag{3}$$

The key hypothesis is that $\mu_1 > 0$: i.e., when a household believes that other households pay a higher tax rate, the household's own willingness to pay taxes goes up. In the context of property taxes, households will always have the temptation to free-ride on the taxes paid by their neighbors. In other words, completely selfish households should be willing to pay \$0 in taxes and not be affected by how much other households pay (i.e., $\mu_1 = 0$). If fairness considerations matter, households may still want to pay taxes; for example, they may consider it the right thing to do. More precisely, our hypothesis is that households feel a heightened moral obligation to pay their own taxes when they perceive that other households also contribute their fair share.[18]

3.3 Experimental Design

We sent letters to a sample of Dallas County homeowners and randomized some of the information contained in the letter. See a sample of the first page of the letter in Figure 2. We included attributes in the design of the letters to ensure recipients of its legitimacy. For example, the letters were signed by a professor from The University of Texas at Dallas and the letters and envelopes included the official logo of The University of Texas at Dallas, a well-known institution in Dallas County.[19] We tailored the letters to each recipient, using their names and addresses.[20] The letters also included contact information.[21]

All letters included information to help subjects with the process of filing a tax appeal,

[18] This hypothesis is based on the implicit assumption that households care about the average tax *rate* paid by other households. In theory, households could care about the average tax *amount* paid by other households. In Section 4.3, we provide direct evidence in support of our implicit assumption.

[19] See Appendix C and D for a sample of the entire letter and of the envelope.

[20] For properties owned by multiple individuals (typically, husband and wife), we listed all owners.

[21] The letter included a physical address that recipients could contact and a URL of the study's website. This website provided basic information about the study (without discussing the study's hypotheses), step-by-step instructions on how to file a protest online and by mail, and contact information for both the Institutional Review Board and the researchers. Appendix E shows screenshots of the entire website.

such as a link to step-by-step instructions on how to file a property tax online or by mail. This aid leveled the playing field for all individuals, providing them with the necessary tools to file a protest if they choose to do so. In fact, a portion of this aid was part of another treatment arm, designed to study the role of filing frictions. The results of this second treatment arm are reported in Nathan et al. (2023), which documents that the aid had a significant and positive effect on the probability of filing a tax appeal.[22]

All of our letters include a table in the middle of the first page, as shown in Figure 2. In Figure 2, this table is highlighted inside a red box with dashed lines (the box is for explanatory purposes and was not included in the actual letters sent to subjects).[23] In all letters, the table includes the new information released by the DCAD on the household's own proposed value and estimated taxes for 2020 (i.e., the information in the second column of the table). The key randomization is whether the table also includes a third column with information about the proposed value and estimated taxes for the average household in Dallas County.

More precisely, we randomized (with a $\frac{2}{3}$ chance) whether the table includes a column that shows these figures for the average home in Dallas. This column allows recipients to compare whether their households' tax rates are above or below the average Dallas home and by how much. Randomly including this additional column is meant to provide a shock to households' perceptions of the tax rate that other households pay. We also cross-randomized an additional row that makes the tax rates explicit.[24]

We created the letters as soon as administrative data became available (on May 16$^{\text{th}}$, 2020), mailed them on May 20$^{\text{th}}$, and started receiving responses to the Field Survey on May

[22] As part of that treatment arm which is the focus in Nathan et al. (2023), we cross-randomized whether the letter included an "extra aid message" with additional information on how to file a tax appeal. The "extra aid message" was included in the second page of the letter. Because this intervention was cross-randomized, our regressions include an indicator variable for the extra aid message among the control variables.

[23] See Appendix C for a sample of the letter without the red boxes added.

[24] Since we cross-randomized two aspects of the tables, in total there were four possible types of tables, which are summarized in Appendix Figure B.1.

21st.[25] The post office scanned more than 90% of our letters by Friday, May 22nd, 2020, indicating that they reached the last mile before delivery by then. From analyzing data from previous years, we know that most subjects file their protests near the deadline. Therefore, there was sufficient time between receipt of the letter and the protest deadline for the letter to influence most recipients' decisions to protest.[26]

When our letters inform subjects that the average tax rate in Dallas County is 2.01%, subjects may update their beliefs upward or downward based on whether their prior beliefs about the average tax rate were higher or lower than 2.01%. Given that in the field experiment we do not observe subjects' prior beliefs about the average tax rate, we leverage the systematic misperceptions documented in the Survey Experiment. Specifically, we use the fact that homeowners who pay below-average tax rates tend to underestimate the average tax rate and those who pay above-average tax rates tend to overestimate the average tax rate.

3.4 Econometric Model

In this section, we discuss the econometric model we use to assess the impact of the average tax rate information that we delivered to subjects, through letters mailed to their homes as part of the field experiment. Let Y_i^{post} be the outcome of interest. For example, this outcome could be an indicator variable that takes the value 100 if the household protested directly in 2020 (the post-treatment period). Let τ_i be the household's own tax rate and $\overline{\tau}^{feed}$ be the *actual* average tax rate in the county. Let D_i be an indicator variable that takes the value 1 if the information on the average tax rate was shown to the subject in the experiment. The regression of interest is as follows:

$$Y_i^{post} = \nu_0 + \nu_1 \cdot D_i \cdot (\overline{\tau}^{feed} - \tau_i) + \nu_2 \cdot (\overline{\tau}^{feed} - \tau_i) + \nu_3 \cdot D_i + X_i^{pre} \nu_X + \varepsilon_i \tag{4}$$

[25] In terms of execution, to swiftly create and mail the letters using the newly-released data is a challenging step.

[26] Figure A.10 in Nathan et al. (2023) shows the timing of the survey responses, of the visits to the study's website, and of the protests.

The variable X_i^{pre} corresponds to the vector of additional control variables. Unless stated otherwise, we use the following set of control variables in all regressions: the proposed value, dummies for multiple owners, school districts and special districts, the number of years since the household's last property tax appeal, a dummy for homestead status, a dummy for the extra aid message treatment,[27] growth in the proposed value relative to the previous year and, for each year from 2015 to 2020, dummies indicating whether the household appealed in each year and the outcome of the appeal. We note that since this is an experiment, the goal of using controls is to gain statistical power by reducing the variance of the error term (McKenzie, 2012). The coefficient ν_2 measures the relationship between the outcome and the potential information shock (i.e., $\overline{\tau}^{feed} - \tau_i$) when the information is not disclosed. The key coefficient, ν_1, measures the effects of the information shock: i.e., how much stronger the relationship is due to the disclosure of feedback.

3.5 Subject Pool

Our subject pool of interest is composed of a sample of 50,394 subjects to whom we sent a letter as part of the mailing intervention.[28] The main outcome of interest in the field experiment is whether the household filed a tax protest in 2020. Among households in the control group (i.e., those who received a letter that did not include information on the average household in Dallas County), 11.08% filed a direct protest. An additional 5.85% protested using an agent.

Column (1) of Table 1 presents average pre-treatment characteristics of the subject pool. The average subject owns a home worth $343,000 and pays an effective tax rate of 2.10%. Columns (2) and (3) show that, consistent with successful random assignment, the pre-treatment characteristics are mainly balanced between the treatment and control

[27] We study the effect of the extra aid treatment in Nathan et al. (2023).

[28] See Appendix B.1 for the sample selection criteria. We sent letters to 50,983 households. However, we removed 589 households from the sample because they had already protested by the time we sent the letters. This left us with a final sample of 50,394 households.

groups.[29] In Appendix B.1 we further show that although the subject pool is not completely representative of the universe of 423,607 single-family residential properties in Dallas County, it is close to being so.

3.6 Complementary Field Survey

The letters in the field experiment include a URL to an online survey with a unique five-letter code for access to our Field Survey. This code ensures that only letter recipients can answer the survey and allows us to match the survey responses to the administrative records.

The Field Survey instrument is provided in Appendix H. The Field Survey has multiple goals. For instance, it allows us to deal with non-compliance. Many subjects may not have paid attention to the unsolicited mail that we sent to them or may not have opened the letter. We can use the response to the survey as a proxy for the households that paid close attention to the content of the letter. Lastly, we included some questions in the Field Survey that could be used as outcome variables. Specifically, we asked a question about the likelihood that respondents will protest in 2020, on a scale from 1 (very likely) to 4 (very unlikely). The purpose of this question is to measure the "immediate" effects of the information in the letter on the intention to protest, shortly after the information is provided. Another question asks whether the respondent feels that his or her own taxes are unfair on a 1-10 scale: "Relative to the other households in the county, do you think your household pays a fair amount in property taxes?" This question is designed to provide a test of the fairness mechanism. The hypothesis is that someone who finds out that the average tax rate is higher than he or she believed is more likely to think that his or her own taxes are fair.

We received responses to the Field Survey from 1,888 households. The median respondent took about 6.3 minutes to complete the survey. Approximately 88.3% of the respondents

[29] The difference is statistically significant for one the pre-treatment variables: the home value. However, while the difference is statistically different due to the large sample size, the difference is small in magnitude. Moreover, we report 10 tests in column (4) – by chance, one of them is likely to be statistically significant at the 10% level.

said that they found the survey "easy to understand," 10.3% found that it was "neither easy nor difficult" and the remaining 1.4% found it "difficult to understand." We included a question about the household's own tax amount, to serve as an attention check since we share this information in the letter we mailed. The responses confirm that the respondents were paying close attention to the survey. The vast majority of respondents (80.8%) provided an answer that is very close (within 5%) to their true tax amount. Regarding the rest of the responses, they were off primarily because they were rounded up or down, or because the respondent confused the assessed home value amount and the tax amount.[30]

The implied response rate is 3.7%. This response rate is of the same order of magnitude as the rate in other studies using a similar delivery method (a survey link through letters): for example, 3.6% in Giaccobasso et al. (2023) and an average of 4.7% in the meta-study by Sinclair et al. (2012). When looking at the results for the subsample of respondents to the Field Survey, it is important to keep in mind that they are not a random sample. In terms of household characteristics, such as home value, number of bedrooms, and tax rate, the differences between survey respondents and non-respondents are statistically significant but small in magnitude.[31] However, there is a substantial difference in the share of subjects who protested in 2020: 50.52% in the subsample of survey respondents versus 11.08% in the overall sample of the field experiment. This difference has a natural interpretation: the subjects who paid the most attention to our letter were very likely those who were on the margin regarding whether to protest in 2020 or not.[32]

Column (5) of Table 1 shows the average pre-treatment characteristics for the Field Survey sub-sample. The survey respondents own more expensive homes than the owners in the entire subject pool. The tax rates are similar across these two groups. Importantly, in the treatment year the differences in the protest rate for the Field Survey sample will be

[30] More precisely, among the 1,885 respondents who answered this question, 173 provided a value for the tax amount value that was within 10% of the assessed home value instead of their tax amount.

[31] Results reported in Appendix B.2.

[32] Our letter makes it explicit from the start that it is about tax protesting, so individuals who were considering filing a tax protest are more likely to keep reading. Additionally, subjects who paid attention to our letter may have found the information provided in our letter helpful in submitting a protest.

about five times as high compared to the protest rate for the full subject pool (we discuss this difference in more detail in Section 4.1 below). Since subjects self-select to answer the survey, it is important to look at the balance test within the subsample of survey respondents. The results are presented in columns (6) and (7) of Table 1. Consistent with successful random assignment, the pre-treatment characteristics are mostly balanced between the treatment and control groups.[33]

4 Results from the Field Experiment

4.1 Main Results

Table 2 presents the results of the field experiment from estimating equation (4). In column (1), the dependent variable uses the administrative data and takes the value 100 if the household protested directly in 2020 and 0 otherwise. This analysis is based on the sample of 50,394 subjects in the field experiment who were randomly selected to receive a letter. The coefficient on the *Information Shock* (the term $D_i \cdot (\overline{\tau}^{feed} - \tau_i)$ from equation (4)) indicates that finding that the average tax rate ($\overline{\tau}$) is 0.1 pp higher causes a decrease in the probability of protest of 0.093 pp. This effect is statistically significant (p-value=0.066). Next, we present checks to assess whether this effect is robust.

One basic concern with experimental studies is that the effects may be spurious. That is, maybe the treatment group happened to include people who are generally more prone to protesting, by sheer chance, despite randomization. To address this concern directly, we performed an event study analysis. Intuitively, if we happened to select individuals more prone to protesting into the treatment group to receive the information shock compared to the control group who received the letter but not the information shock, we should observe differences in protest rates not only in the post-treatment period, but in the pre-treatment

[33] The difference is statistically significant for one the pre-treatment variables: the property tax rate. However, while the difference is statistically significant due to the large sample size, the difference is rather small in magnitude. Moreover, we report 10 tests in column (8) – by chance, one of them is bound to be statistically significant at the 10% level.

years as well. The results are presented in Figure 3.a. The rightmost coefficient in this figure shows the effect of the information shock on the probability of protesting in 2020, corresponding to the coefficient reported in column (1) of Table 2. The rest of the coefficients in Figure 3.a are estimated with the same regression specification, but instead of using protests in 2020 as the dependent variable, they use protests in the pre-treatment years 2015–2019.[34] Since our letters had not been sent yet, the information shocks could not have an effect on protests in any of the years before 2020. As expected, the coefficient in each pre-treatment year is close to 0, statistically insignificant, and precisely estimated.

Another potential concern with the results from column (1) of Table 2 is that they may be driven by non-linearities or by outliers. To address this concern, Figure 4.a presents the results in binned scatterplot form. This figure shows that the linear specification is a reasonable approximation and also that the results do not seem to be driven by outliers.

One challenge in interpreting the magnitude of the coefficient on *Information Shock* is that it is an intention-to-treat effect, due to multiple sources of non-compliance. An important source of non-compliance is that a significant share of households may not have read our piece of unsolicited mail, which is a common concern for mailing interventions more generally (e.g., see Perez-Truglia and Cruces, 2017; Gerber et al., 2020; Bottan and Perez-Truglia, 2020). Furthermore, even for households who opened the letters on time, many may have skimmed through them without paying attention to the information on average taxes listed in the table. To illustrate how significant this concern can be, some studies have attempted to calculate the share of subjects who paid attention to the information included in mailing interventions. Using data from a follow-up survey, Perez-Truglia and Cruces (2017) estimates that only 21.5% of the subjects who received a letter actually learned the relevant information contained in it. Gerber et al. (2020) study readership from various sources, including results from the USPS Household Diary Survey and a study conducted by the

[34] The regressions in this figure use data from rolling periods as controls. For example, when the dependent variable is the decision to protest in 2019, we control for a set of indicator variables corresponding to the history of protests between 2015–2018. We included controls on protest history up to 2015, because at the time we ran the field experiment the DCAD provided protest data on its website from 2015 onwards.

U.S. Environmental Protection Agency, and argue that 50% of the recipients in their mailing experiment may have read the relevant information.[35] Nathan et al. (2023) estimates that around 26% of subjects may have not received the letter, not opened it, or opened it after the deadline to submit a protest had passed.[36].

We use two approaches to account for non-compliance that leverage the responses to the Field Survey. The first strategy consists of focusing on the type of individuals who are more likely to respond to the Field Survey. Intuitively, since the invitation to the Field Survey is at the bottom of the letter, individuals who respond to the survey are likely the ones who paid the closest attention to the letter. This strategy consists of two steps. In the first step, we use a simple probit model to estimate the ex-ante probability that a household responds to the Field Survey based on a host of pre-treatment characteristics such as the household's protest history, proposed value, tax rate, and homestead status.[37] This model has decent predictive power: the out-of-sample area under the receiver operating characteristic (AUC) is 0.66.[38] In the second step, we split the sample based on whether the ex-ante probability of responding to the survey is above or below the median value, and estimate the same model as in column (1) of Table 2 separately for each half of the sample. Table 2 presents the results from this first approach, with column (2) corresponding to subjects with an above-median probability of response, and column (3) corresponding to subjects with below-median probability. Subjects in column (2) are two times more likely to end up responding to the survey relative to subjects in column (3) (their response probabilities are 5.6% and 1.9%, respectively). We also observe that households who are more likely to respond to the survey

[35] For more details, see Appendix A.5 of Gerber et al. (2020).

[36] The estimates in Nathan et al. (2023) are based on Mazzone and Rehman (2019) and Bottan and Perez-Truglia (2020).

[37] Specifically, this list of variables includes all of the control variables used in column (1), as well as race dummies (White, Black, Asian dummies), the actual difference between the average tax rate in the county and the household's own tax rate (both in the level and using twenty quintile dummies), the difference between the household's 2020 proposed value and the potential homestead cap, this difference interacted with a dummy for a homestead in 2020, and the history of protests by type (direct and agent protests).

[38] Intuitively, this value of the AUC means that if you were to randomly select a subject who responded to the letter and a subject who did not respond to the survey, the model has a 66% probability of guessing which of the two individuals responded to the survey.

are more likely to end up protesting: the average protest probability is 15.6% in column (2) versus 6.5% in column (3). This is natural, as individuals who were more interested in our letter were probably the ones who were contemplating protesting. The comparison between the results from columns (2) and (3) of Table 2 suggests that, as expected, the effects of the information shock are stronger for individuals who are more likely to pay attention to the letter. More precisely, the coefficient is -2.131 (p-value=0.082) in column (2) versus -0.528 (p-value=0.313) in column (3). However, this result must be taken with a grain of salt, as the coefficients are not very precisely estimated and thus their difference is statistically insignificant (p-value=0.228).

As a second (and more direct) approach, we can focus on the subjects who responded to the Field Survey. The advantage of this approach is that for these households we have confirmation that they paid attention to the letter; otherwise they could not know about the survey link and code needed to complete the survey, as that information was only available in their personalized letter. Indeed, assuming that subjects read the letter from top to bottom, these subjects probably noticed the information about the average tax rate, since the survey link was shown below the table that contained the information shock about the average tax rate. However, there is one disadvantage of this approach: since the decision to respond to the survey is made in the post-treatment period, conditioning on this post-treatment variable may contaminate the experimental variation. For this reason, when restricting to the sample of survey respondents, it is important to conduct proper falsification tests, such as the event-study analysis.

Column (4) of Table 2 uses the same specification as column (1), except that column (4) is restricted to the sub-sample of 1,888 households who responded to the Field Survey. The coefficient from column (2) is negative (-12.566) and statistically significant (p-value=0.021). This coefficient is much larger in magnitude than the corresponding coefficient reported in column (1). This difference is to some extent mechanical. Since the baseline protest rate is much higher for the survey respondents (50.52% in column (4) versus 11.08% in column

(1)), it is natural that the effect sizes are also higher. However, this mechanical difference is far from being the only factor at play: while the average outcome is 4.5 times as large (50.52 vs. 11.08), the effect of information is 13.4 times as large (-12.566 vs. -0.937). Thus, our preferred interpretation is that, to a large extent, the effects are stronger in column (4) because those households paid more attention to the information included in the letter.

4.2 Additional Robustness Checks

One potential concern with the results from column (4) of Table 2 for the Field Survey sample is that, despite the random assignment, the endogenous nature of the selection into the survey may introduce an endogeneity bias. To address this concern, Figure 3.b presents the event-study analysis. More precisely, we estimate the same regression as in column (4), but the dependent variables are indicator variables for whether the respondent protested in each of the pre-treatment years. Reassuringly, the effects on the pre-treatment years are close to 0, statistically insignificant, and precisely estimated. A second concern with the results from column (4) of Table 2 is that the larger effects for the Field Survey sample compared to those for the full sample could be driven by non-linearities or outliers. Figure 4.b, which presents the results in binned scatterplot form, rules out non-linearity and outliers as drivers for the differences.

With the subsample of respondents to the Field Survey, we can estimate the effects of the information shock on the survey outcomes. Column (5) of Table 2, which is identical to column (4) except that the dependent variable takes the value 100 if the household reports that it is likely or very likely to protest in 2020, and takes the value 0 if the household reports that it is unlikely or very unlikely to protest. There are reasons why the effects on stated intentions may be different from the effects on actual protests. For example, some individuals may care about the information provided to them, but may forget about it by the time they have to choose whether to protest or not. In addition, the information may increase the desire to file a protest, but the individual may later not act as planned due

to the difficulty of the process or other filing frictions (Nathan et al., 2023). In fact, we do not expect the effects on intended protests and actual protests to be mechanically the same, as their correlation is statistically significant (p-value<0.001) but far from perfect (correlation coefficient of 0.398). We find that the effects on the intention to protest closely mirror the effects on actual protests: the coefficient from column (5) is negative (-11.919) and highly statistically significant (p-value=0.008). This coefficient is close in magnitude to (and statistically indistinguishable from) the corresponding coefficient of -12.566 from column (4).

In Appendix B.3, we present additional robustness checks and results. We show that the effects of the information shock on protest choices were consequential for the households' assessed values and tax amounts. In addition, we show that our results are very similar in terms of both magnitude and statistical significance when we restrict the sample to the subjects who answered on the Field Survey that they considered fair for everyone to pay exactly the same tax rate (i.e., the "proportionality norm").[39]

4.3 Causal Mechanisms: Fairness

The evidence presented above suggests that the information about the average tax rate had a significant effect on the decision to protest. Next, we provide evidence and discuss some of the potential mechanisms at play. We interpret the effect of the information shocks on protests to be due to households changing their perceptions of the average tax rate and, subsequently, the taxes that they deem fair to pay.

We can use the data from the Field Survey to provide direct evidence for the fairness channel. Specifically, we included a question to measure households' feelings of fairness about their own taxes on a scale from 1 (very unfair) to 10 (very fair). Figure 5.a summarizes the distribution of responses to this question. The evidence suggests that most households feel that their taxes are unfair compared to others, but there is substantial variation between households in their feelings of unfairness. In this sample, households lean towards believing

[39] In this appendix, we also show that we do not find any significant differences between effects when the tax rate was made explicit versus not made explicit in an additional row to the table contained in the letter.

that their taxes are unfair: e.g., in the control group, the average fairness score is 4.02, which is closer to the unfair end of the scale.

According to the fairness channel, we would expect that an information shock that increases the perceived average tax rate would have a positive effect on perceived fairness. That is, if you find out that others are contributing their share, it should be fair for you to contribute your part too. The relevant results are presented in column (6) of Table 2, which is identical to column (4) except that the dependent variable is how fair the household perceives its own taxes. As expected, the coefficient on *Information Shock* from column (6) is positive (0.459) and statistically significant (p-value=0.060). Indeed, the effects on perceived fairness are comparable in magnitude to the effects on the probability of protest. For example, the results from column (6) indicate that an information shock of 0.1 pp causes a change in perceived unfairness that is equivalent to 2.15% ($= \frac{0.1 \cdot (-0.459)}{2.13}$) of a standard deviation. In comparison, the results from column (4) indicate that an information shock of 0.1 pp causes an increase in the probability of protest equivalent to 2.48% ($= \frac{0.1 \cdot (-12.566)}{50.52}$) of a standard deviation. Figure 4.c presents these results in a binned scatterplot. While the results are a bit noisy, they do not seem to be driven by outlier observations or by non-linearities.

To complement the above evidence on the role of the fairness mechanism, we provide additional, non-experimental evidence. First, by linking the survey responses to the administrative records, we can measure whether the feeling of fairness is correlated with the probability of filing a tax protest. The results are presented in Figure 5.b. The figure shows a negative, strong and statistically significant (p-value<0.001) association between the perceived fairness and the probability of filing a tax appeal. Increasing perceptions of fairness from very unfair (1) to very fair (10) is associated with a 23.40 pp decrease in the probability of protesting.

Second, we exploit a question from the Field Survey designed specifically to explore what households consider fair when it comes to distributing the property tax burden across taxpayers. Specifically, we asked respondents to distribute a property tax burden of $10,000

between Household A, whose home is worth $100,000, and Household B, whose home is worth $400,000. To make it easy for respondents, we provided a menu of seven options to choose from: Household A pays $0 (and thus Household B pays $10,000), $1,000 ($9,000), $2,000 ($8,000), $5,000 ($5,000), $8,000 ($2,000), $9,000 ($1,000) or $10,000 ($0). Respondents were asked to report the allocation they would consider the most fair. For instance, if a respondent thinks that the fair allocation is that everyone pays the same tax *rate*, then he or she would choose the third option in which Household A pays $2,000 and household B pays $8,000 (which implies a tax rate of 2% for both households). In comparison, if a respondent thinks it is fair for everyone to pay the same tax *amount*, then he or she should choose the middle option in which both households pay $5,000 (implying a tax rate of 5% for Household A and a tax rate of 1.25% for Household B). The responses to this survey question are reported in Figure 6. These results provide evidence supporting what we call the "proportionality norm". Specifically, a strong majority of households (76.5%) choose the option that equalizes the tax rates. In contrast, only a minority of the respondents (5.47%) choose the option that equalizes the tax amounts.[40]

4.4 Alternative Mechanisms

The fairness channel constitutes our preferred interpretation of the effects of the information shock. Next, we provide evidence against a potential alternative channel: subjects might have reacted to the information on the average tax rate because they inferred from that information whether their own protests would likely be successful.

First, a household that receives information indicating that the average household pays a lower tax rate cannot rationally infer that the other households are paying a lower tax rate because those households protested successfully in the past. This is because the average tax rate does not provide any information about whether a household protested in the past

[40] We asked a similar question to subjects across the entire United States in the Survey Experiment and the results from this question also support that the majority of the households chose an allocation of taxes supporting the "property tax's proportionality norm." We present these results in Appendix A.3.

or whether a household's protest was successful. Specifically, the tax rate is calculated by dividing the tax amount by the proposed value of the property. A successful protest reduces the value of both the numerator and the denominator, thus leaving the tax rate roughly unchanged.[41] Therefore, if a household learns that the average household pays a tax rate that is 1 pp higher than its own, it would be irrational for the household to infer anything about its odds of a successful protest. We further show in the appendix that there is no relationship between the protest success rate and the households' tax rates proposed by the DCAD.[42]

Moreover, the magnitude of the alternative channel is at most small and cannot come close to explaining the effect sizes that we document for the information shock. Changes in both individuals' own tax rates and the average tax rate due to protests are limited because, in addition to the fact that tax rates remain (mechanically) roughly unchanged following successful protests as explained before, only a minority of households protest. In contrast, information shocks are often large: households can discover that they pay a tax rate that is 1pp lower or higher than the average.[43]

Columns (7) and (8) of Table 2 present a test of the alternative channel. If individuals make irrational inferences from the information about the average tax rate, we should expect heterogeneous effects on households who are more familiar with and educated about how property taxes and protests operate versus those who are less familiar. Intuitively, households who protested before should have a greater understanding of how property taxes work and have more information about the odds of a successful protest from their past experience, so

[41] In fact, a successful protest can lead to a small *increase* in the tax rate if a household has a binding homestead cap, because the denominator would decrease while the numerator would not change. Appendix Figure B.2.b shows that most successful protests actually lead to a slight increase in the tax rate.

[42] If a higher tax rate indicates that a household has higher odds of success if it protests, then we would expect a positive relation between tax rates and the odds of a successful protest. Instead, we find that there is not a significant association between the proposed tax rate and the odds of a successful protest. For more details, see Appendix Figure B.3.

[43] A household in the 10th percentile pays a tax rate of 1.33%, while a household in the 90th percentile pays a tax rate of 2.73%. For a more quantitative comparison, see Appendix Figure B.2 which shows that the magnitude of the changes in the tax rates due to successful protests (panel (b)) is small relative to the overall differences in tax rates between households (panel (a)).

they should be less prone to making any irrational inference. Columns (7) and (8) of Table 2 divide the sample into subjects who have not protested in the past five years (column (7)) versus subjects who protested at least once in the past five years (column (8)). We do not find evidence of the type of heterogeneity predicted by this alternative mechanism: the coefficients from columns (7) and (8) are similar in magnitude (-13.56 vs. -12.09) and their difference is statistically insignificant (p-value=0.880).

4.5 Magnitude of the Effects

One challenge for assessing the magnitude of fairness concerns is the need to account for two forms of non-compliance. The first form of non-compliance is that some recipients may not have read the letter. As previously explained, we can address this form of non-compliance by focusing on the results from column (4) of Table 2, for the sample of recipients who must have read the letter in order to answer the Field Survey. However, a second form of noncompliance remains: even if they read the letter, subjects may not fully incorporate the feedback into their beliefs. We use the results on pass-through belief updating from the Survey Experiment to correct for this additional form of non-compliance. Because each additional 1 pp in the information shock increased the perceived average tax rate by 0.455 pp (from comparing the slopes in Figure 1.b), we use a scaling factor of 2.19 ($= \frac{1}{0.455}$). Scaling up the coefficient on *Information Shock* from column (4) of Table 2 implies that increasing recipients' perception of the average tax rate paid in the county by 0.1 pp would decrease their protest probability by 2.75 pp ($= 0.1 \cdot 12.566 \cdot 2.19$).

As complementary evidence of the magnitude of the effects, we provide a back-of-the-envelope calculation of the willingness to pay for fairness in dollar terms. To compute this value, we combine the results from this paper with the results in Nathan et al. (2023), which uses a quasi-experiment to measure the effect of monetary incentives on the probability of protest. Specifically, Nathan et al. (2023) leverages a kink in the marginal benefits from protesting that results from the Texas Property Code's homestead cap, which imposes a 10%

cap in the annual increase in assessed values. Nathan et al. (2023) shows that a reduction in expected tax savings of \$100 causes a decrease in the probability of protest of 2.14 pp.[44]. Combining this result with our finding indicating that a 0.1 pp increase in the perceived average tax rate would decrease the protest probability by 2.75 pp, we estimate that a 0.1 pp increase in the perceived average tax rate (equivalent to an increase of \$295 in the average tax amount) increases the willingness to pay taxes by about \$128 ($= \frac{2.75 \cdot 100}{2.14}$).[45]

The evidence discussed above suggests that for each additional \$1 in taxes contributed by the average household, a homeowner is willing to pay an additional \$0.43 ($= \frac{128.00}{295}$) in his or her own taxes. This magnitude suggests that fairness concerns are a significant source of tax compliance. Moreover, this magnitude is roughly comparable to the findings from laboratory experiments on the public goods game. Take, for example, Fischbacher et al. (2001), one of the earliest studies on conditional cooperation. According to Figure 1 from that paper, for each additional dollar in the average contribution of others, the average subject contributes an additional \$0.45. We note that it is not obvious that fairness considerations would hold significant weight in the real-world context of tax compliance. For example, the stakes are orders of magnitude higher for property taxes than for laboratory games; and taxpayers may have strong views about the role of government that are absent in the context of laboratory settings (Huet-Vaughn et al., 2019).

5 Conclusions

This paper provides unique causal evidence on the importance of fairness considerations for tax compliance. We conducted a natural field experiment in the context of U.S. property taxes. We mailed letters to households to introduce exogenous variation in homeowners' perceptions of the average tax rate paid by other households in their county. We

[44] This result is presented in Figure 1 of Nathan et al. (2023).

[45] The average home is worth \$294,846 and pays a tax amount of \$5,916 in Dallas county before any filters. A change in the tax rate by 0.1 pp is equivalent to a change in the average tax amount of \$295 ($= \$294,846 \cdot 0.001$).

examined the impact of this information shock on households' high-stakes decisions to file a tax appeal. Our evidence indicates that when households perceive that other households pay a higher (lower) tax rate, that increases (decreases) the households' willingness to pay taxes, as revealed by a decrease (increase) in their probability of filing a tax appeal. Using responses to a complementary survey included in the letters, we provide evidence about the mechanism driving this result: we find that our treatment affects homeowners' fairness perceptions.

An important factor to consider in any empirical study is the generalizability of the findings. Our study focuses on a single county, although our focal location is the second largest county in Texas, with an estimated population of 2.6 million in 2020, and it has a population larger than 15 states in the U.S.[46] We believe that our field experiment results could be replicated in any of the 241 counties in Texas where property taxes work almost identically. Moreover, property tax protests are also consequential and work similarly in many other U.S. counties and even other countries. Thus, replicating our field experiment for other U.S. counties would be relatively straightforward. In addition, we focus on a single type of tax in a developed country and, despite the fact that property taxes are the second largest source of tax revenue in the United States, each tax and country or subnational government has unique intricacies.[47]

Finally, studying whether empirical results align across different experimental settings and designs can provide clues about the generalizability of experimental results (Levitt and List, 2007). In our case, there is concordance on how fairness affects the willingness to contribute to the provision of public goods: our estimates from the field experiment and the results from laboratory experiments on the public goods game (e.g., Fischbacher et al. (2001)) are quantitatively similar. Although this concordance may lend support to the generalizability of our empirical estimates, given the scant field experimental evidence on the role of fairness considerations in tax compliance more work on these topics is warranted.

[46] We view our results from the field experiment as a wave-1 insight that establishes initial causality and produces first tests of theory in the field in the language of List (2020).

[47] For example, some taxes are withheld limiting the taxpayer from deciding how much taxes to pay.

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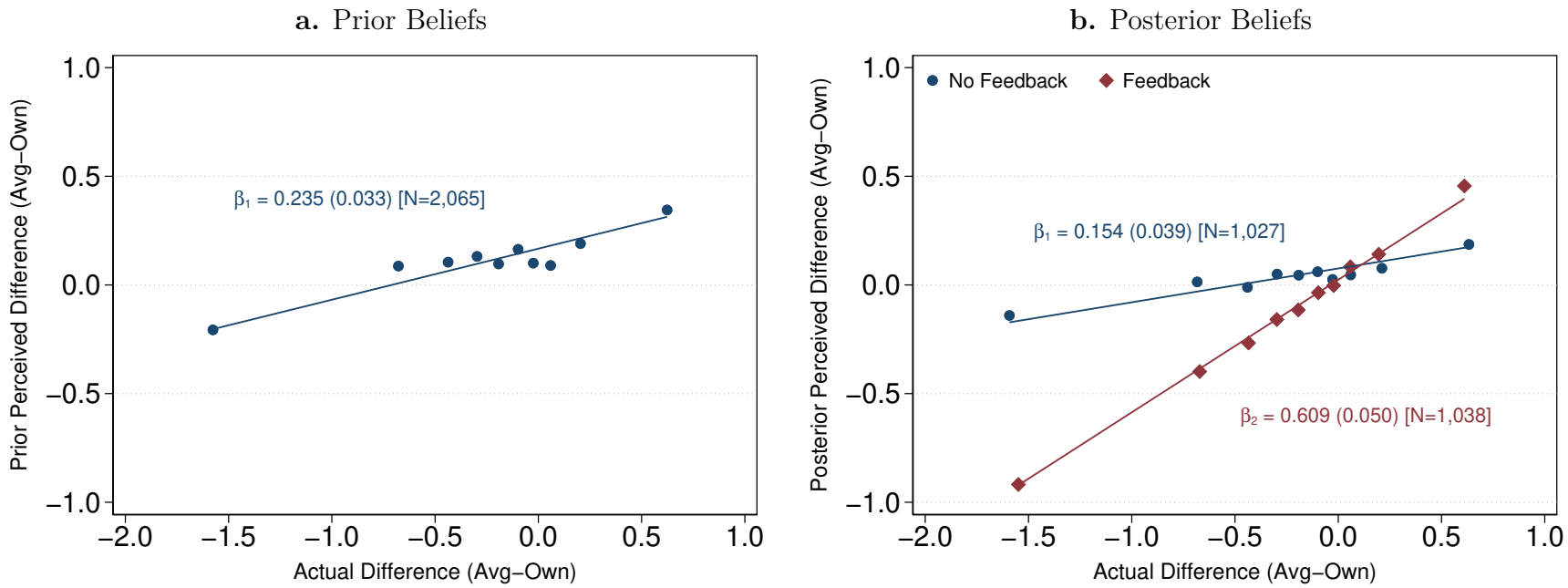
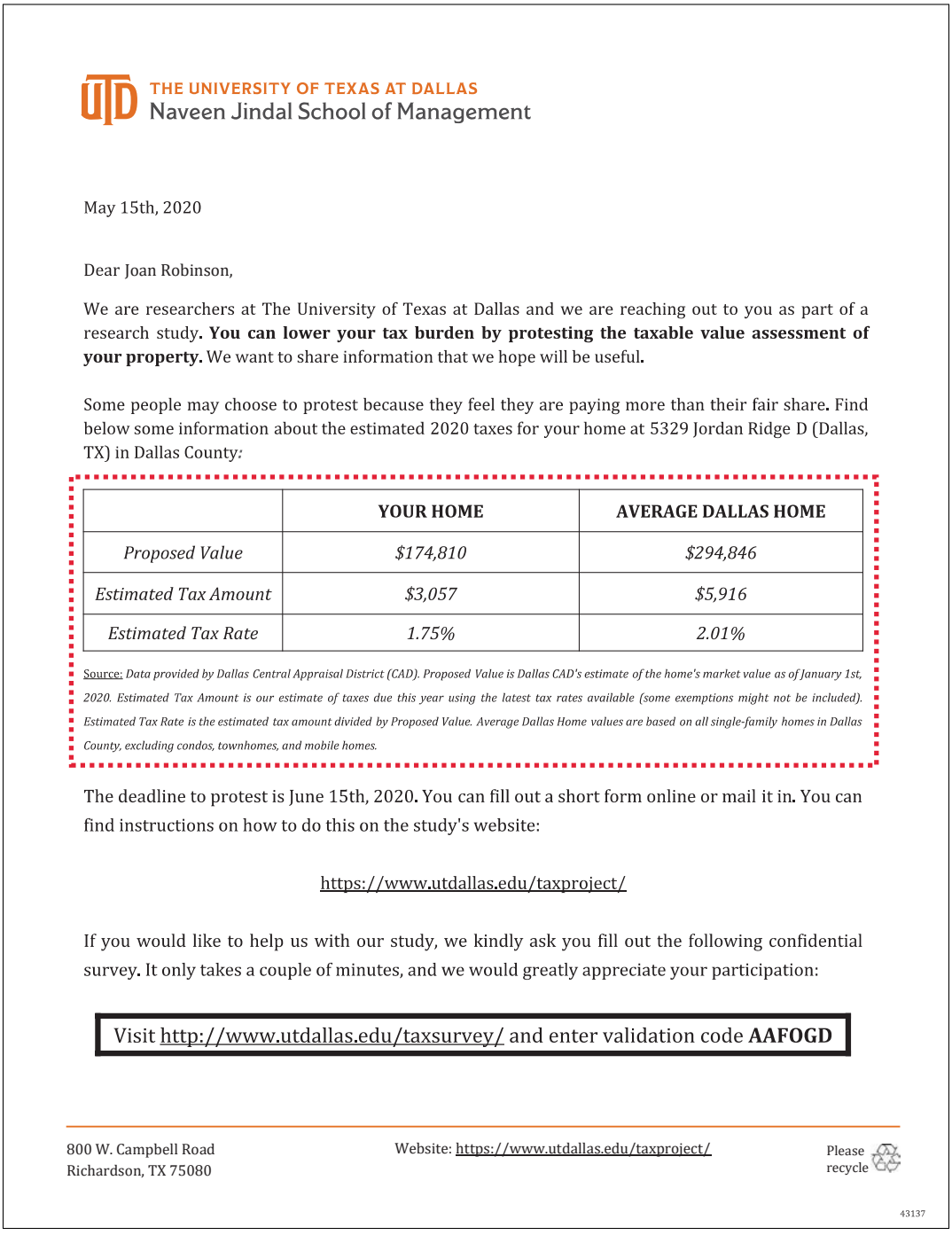
Figure 1: Prior Misperceptions and Belief Updating in the Survey Experiment



Notes: This figure shows how the information treatment in the Survey Experiment affected respondents' perceptions. Each line corresponds to a separate OLS regression, with robust standard errors in parentheses and the number of observations in brackets. The x-axis corresponds with the respondent's *actual* relative position with respect to the county average (i.e., the difference between the actual average tax rate in the respondent's county and the respondent's own tax rate ($\overline{\tau} - \tau_i$)). In panel (a) the y-axis corresponds with the difference between the respondent's *perceived* average tax rate in the county and the household's own tax rate prior to the feedback on the actual county average tax rate could be shown. In panel (b) the y-axis is the respondent's corresponding *posterior* belief at the end of the survey (i.e., after the information-provision experiment). The regressions in this figure include the actual average home value and the actual average tax amount in the respondent's county as controls. The results from panel (b) are broken down by treatment group and response decile within the treatment group: the red diamonds (labeled "Feedback") correspond to deciles of respondents who were shown the feedback while the blue circles (labeled "No Feedback") correspond to deciles of those not shown the feedback.

Figure 2: First Page of the Letter from the Field Experiment



THE UNIVERSITY OF TEXAS AT DALLAS
Naveen Jindal School of Management

May 15th, 2020

Dear Joan Robinson,

We are researchers at The University of Texas at Dallas and we are reaching out to you as part of a research study**. You can lower your tax burden by protesting the taxable value assessment of your property.** We want to share information that we hope will be useful**.**

Some people may choose to protest because they feel they are paying more than their fair share**.** Find below some information about the estimated 2020 taxes for your home at 5329 Jordan Ridge D (Dallas, TX) in Dallas County*:*

	YOUR HOME	AVERAGE DALLAS HOME
Proposed Value	*$174,810*	*$294,846*
Estimated Tax Amount	*$3,057*	*$5,916*
Estimated Tax Rate	*1.75%*	*2.01%*

Source: *Data provided by Dallas Central Appraisal District (CAD). Proposed Value is Dallas CAD's estimate of the home's market value as of January 1st, 2020. Estimated Tax Amount is our estimate of taxes due this year using the latest tax rates available (some exemptions might not be included). Estimated Tax Rate is the estimated tax amount divided by Proposed Value. Average Dallas Home values are based on all single-family homes in Dallas County, excluding condos, townhomes, and mobile homes.*

The deadline to protest is June 15th, 2020**.** You can fill out a short form online or mail it in**.** You can find instructions on how to do this on the study's website:

https://www.utdallas.edu/taxproject/

If you would like to help us with our study, we kindly ask you fill out the following confidential survey**.** It only takes a couple of minutes, and we would greatly appreciate your participation:

Visit http://www.utdallas.edu/taxsurvey/ and enter validation code **AAFOGD**

800 W. Campbell Road
Richardson, TX 75080

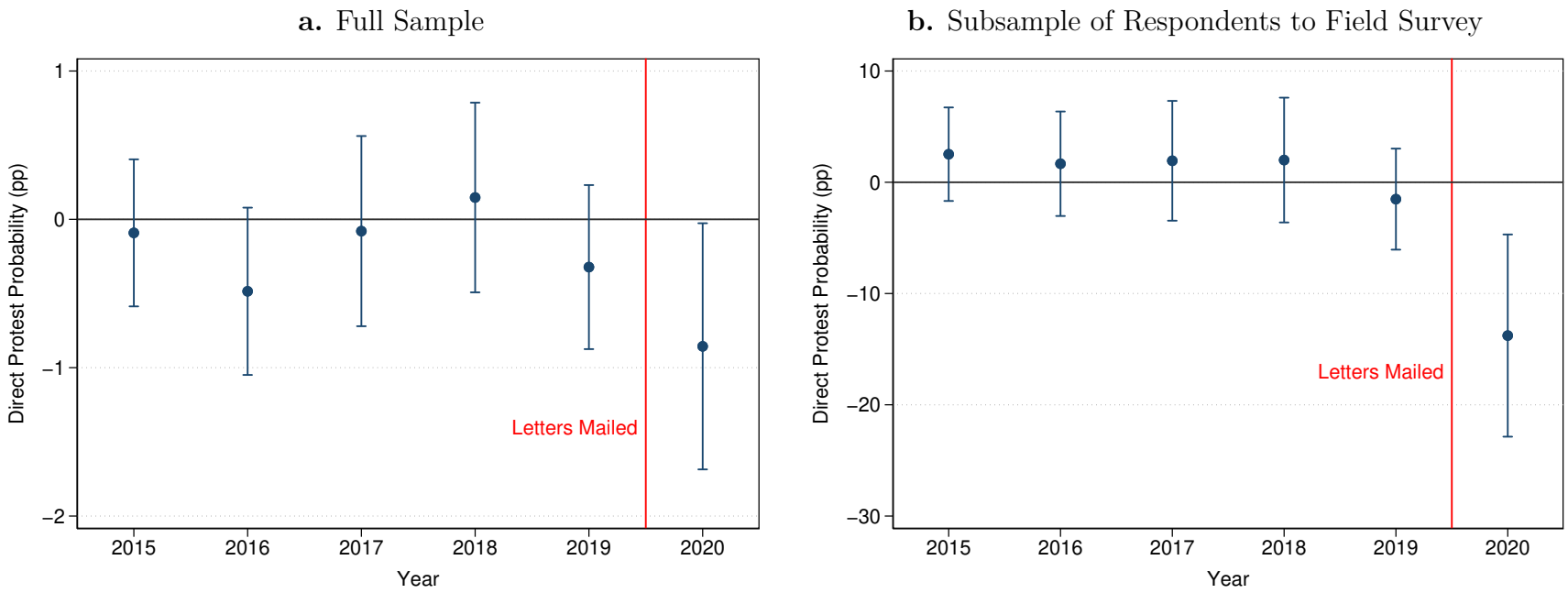
Website: https://www.utdallas.edu/taxproject/

Please recycle

43137

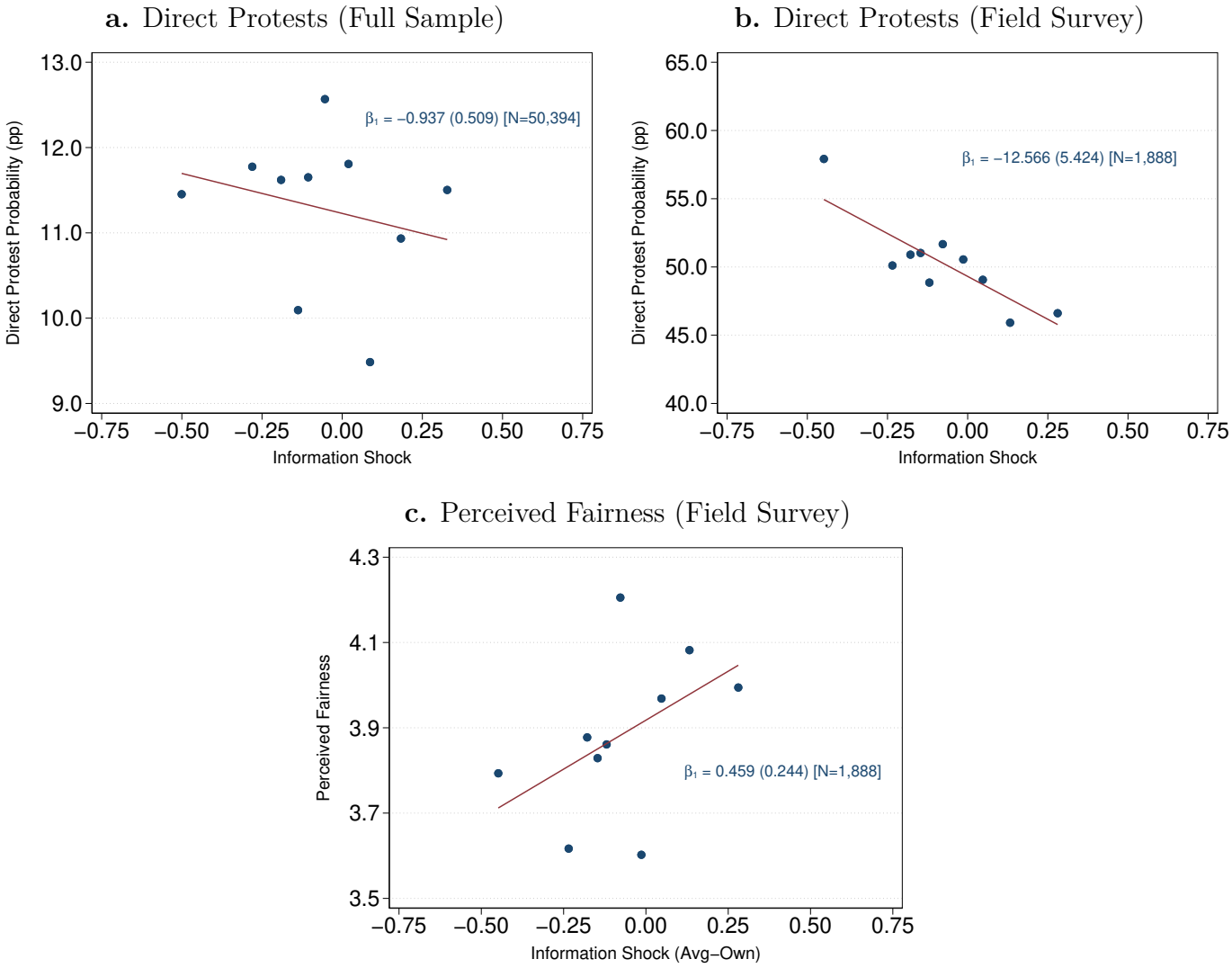
Notes: A sample of the first page of the letter used in the field experiment. The information in the table varied by treatment group. Sample tables for every treatment group are presented in Figure B.1 in the Appendix. The table appears inside a red frame with dashed lines (this frame was added to this figure for emphasis but does not appear in the actual letters).

Figure 3: Results from the Field Experiment: Event-Study Falsification Tests



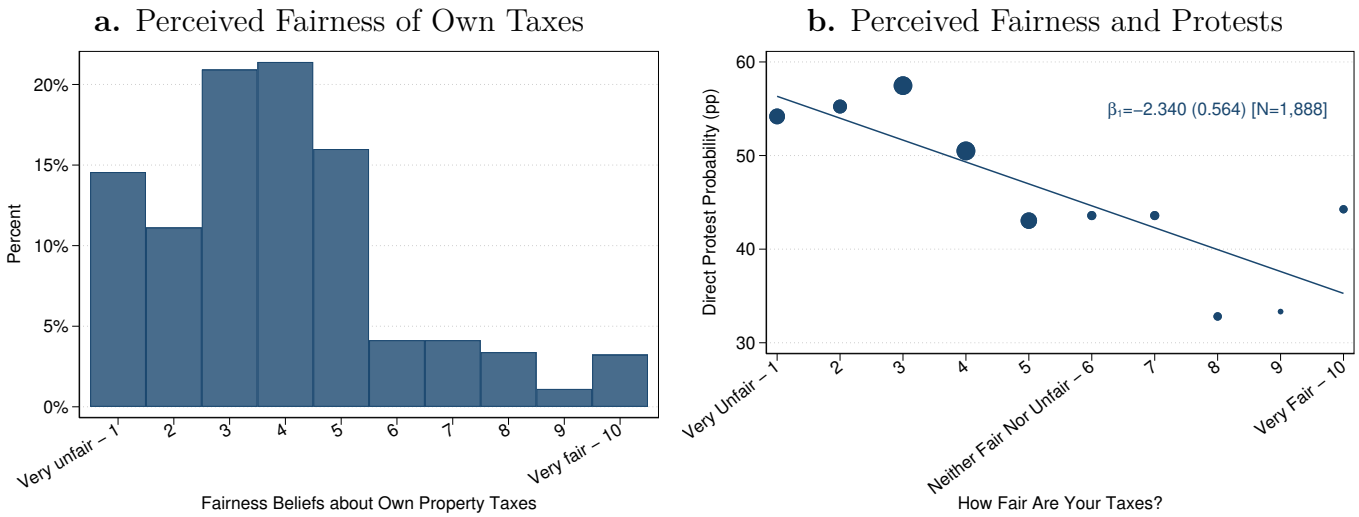
Notes: Point estimates with 90% confidence intervals in brackets, based on robust standard errors. The point estimates are computed in the same way within both panels: the point estimates within each panel only change the focal year. Panel (a): The blue dots represent the coefficient on the information shock ($D_i \cdot (\overline{\tau} - \tau_i)$) based on equation (4) from Section 3.4, with direct protests as the dependent variable. Panel (d): same as panel (b) except that it is based on the subsample of 1,888 subjects who responded to the Field Survey. The regressions in this figure include the following controls: the 2020 proposed value in levels and its annual growth, dummies for multiple owners, school and special districts, number of years since the household's last protest, a dummy for homestead status, a dummy indicating if the household received the extra aid message, and, for each previous year since 2015, a dummy indicating if the household protested in that year and the outcome of the protest (if any) as a percent-reduction in the market value (i.e., the protest history). Control variables for the protest history depend on the year in which the dependent variable is measured. For instance, if the outcome corresponds to direct protests in 2018, the protest history controls include protests in 2015, 2016, and 2017.

Figure 4: Results from the Field Experiment: Effects of the Average Tax Rate in the County on Direct Protests and Perceived Fairness



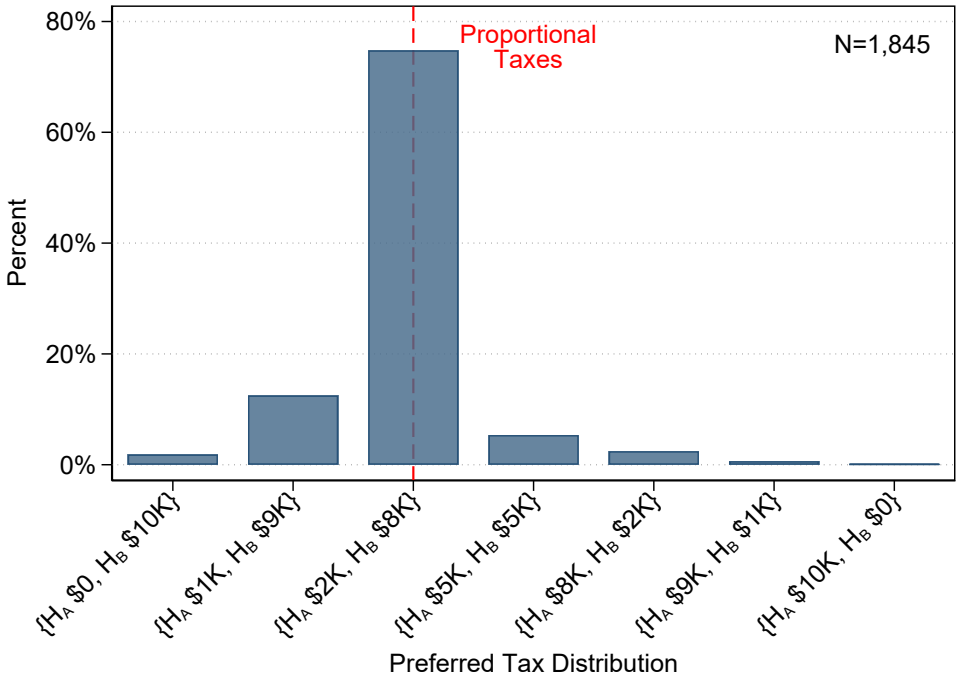
Notes: This figure depicts binned scatterplot representations of the effect of the information on the average tax rate in the county on the probability of protesting directly (panels (a) and (b)) and perceived fairness (panel (c)). Panel (a) uses the full field experiment sample, while panels (b) and (c) use the subsample that responded to the Field Survey. The x-axis corresponds to the information shock, i.e., the interaction between the respondent's *actual* relative position with respect to the county average (i.e., the difference between the actual average tax rate in the respondent's county and the respondent's own tax rate ($\overline{\tau} - \tau_i$)) and a dummy variable that indicates if the household was selected to receive the information on the average tax rate in the county. Each dot corresponds with a decile of the information shock. In panels (a) and (b), the y-axis corresponds to the probability of a direct protest in 2020. In panel (c), the y-axis corresponds to the level of perceived fairness, based on responses from subjects who answered the question, "Relative to the other households in the county, do you think your household pays a fair amount in property taxes?" Subjects could answer on a scale from 1 to 10, with 1 indicating "Very unfair" and 10 indicating "Very fair". The line in each panel corresponds to the linear fit and is shown with the corresponding slope and robust standard error (in parentheses). The 2020 direct protest, perceived fairness, and information shock variables are residualized on the control variables and the sample mean of each variable is added to its residuals before binning and plotting. The control variables are as follows: the 2020 proposed value in levels and its annual growth, dummies for multiple owners, school and special districts, number of years since the household's last protest, a dummy for homestead status, a dummy indicating if the household received the extra aid message, and, for each previous year since 2015, a dummy indicating if the household protested in that year and the outcome of the protest (if any) as a percent-reduction in the market value (i.e., the protest history). Control variables for the protest history depend on the year in which the dependent variable is measured. For instance, if the outcome corresponds to direct protests in 2018, the protest history controls include protests in 2015, 2016, and 2017.

Figure 5: Non-Experimental Evidence on the Fairness Channel



Notes: Non-experimental evidence of the fairness channel based on responses from 1,888 subjects in the Field Survey. Panel (a) features responses from subjects who answered the question, "Relative to the other households in the county, do you think your household pays a fair amount in property taxes?" Subjects could answer on a scale from 1 to 10, with 1 indicating "Very unfair" and 10 indicating "Very fair". Panel (b) presents a binned scatterplot of the association between direct protest rates (observed in the administrative records) and households' perceived fairness of their own property taxes (from a question included in the Field Survey). The size of each circle is proportional to the number of subjects that provided the response indicated. The line corresponds to the linear fit and is shown with the corresponding slope and robust standard error (in parentheses).

Figure 6: Field Survey Evidence on the Proportionality Norm



Notes: This histogram shows the results of the responses to a question included in the Field Survey in Dallas County regarding the fair distribution of tax burden. The horizontal axis shows the options presented to respondents on how to distribute a total tax burden of $10,000 between Household A (which is worth $100,000) and Household B (which is worth $400,000). The vertical axis presents the share of the responses choosing each of the seven options. The dashed red line indicates the choice that provides the same (proportional) tax rate for both households.

Table 1: Randomization Balance Test: Field Experiment

	Full Sample				Field Survey Subsample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Treatment	Control	P-value	All	Treatment	Control	P-value
Home Value ($1,000s)	343.162	345.235	339.001	0.034	394.983	395.021	394.916	0.994
	(1.406)	(1.743)	(2.371)		(6.987)	(8.548)	(12.092)	
Property Tax Rate (%)	2.103	2.104	2.100	0.364	2.107	2.125	2.075	0.014
	(0.002)	(0.003)	(0.004)		(0.010)	(0.012)	(0.016)	
Owner-Protest in 2019 (%)	5.854	5.934	5.692	0.273	10.328	10.370	10.253	0.936
	(0.105)	(0.129)	(0.179)		(0.701)	(0.875)	(1.170)	
Agent-Protest in 2019 (%)	4.598	4.543	4.708	0.407	5.667	5.926	5.201	0.507
	(0.093)	(0.114)	(0.164)		(0.532)	(0.678)	(0.857)	
2020 Homestead Exemption (%)	83.760	83.779	83.722	0.871	92.797	92.346	93.611	0.297
	(0.164)	(0.201)	(0.285)		(0.595)	(0.763)	(0.943)	
Number of Bedrooms	3.304	3.303	3.306	0.554	3.410	3.396	3.435	0.197
	(0.003)	(0.003)	(0.005)		(0.015)	(0.018)	(0.025)	
White (%)	44.275	44.412	44.000	0.380	56.568	57.449	54.978	0.301
	(0.221)	(0.271)	(0.383)		(1.141)	(1.419)	(1.919)	
Hispanic (%)	27.321	27.290	27.382	0.827	15.307	15.556	14.859	0.686
	(0.199)	(0.243)	(0.344)		(0.829)	(1.040)	(1.372)	
Black (%)	18.685	18.579	18.897	0.389	17.691	16.626	19.614	0.110
	(0.174)	(0.212)	(0.302)		(0.878)	(1.069)	(1.532)	
Asian (%)	9.719	9.719	9.720	0.997	10.434	10.370	10.550	0.903
	(0.132)	(0.162)	(0.229)		(0.704)	(0.875)	(1.185)	
Observations	50,394	33,635	16,759		1,888	1,215	673	

Notes: Average pre-treatment (i.e., before the start of letter delivery) characteristics of subjects in the field experiment, with standard errors in parentheses. Column (1) corresponds to the entire field experiment sample, column (2) contains subjects in the field experiment that received the average rate treatment, and column (3) contains subjects in the field experiment that did not receive the information on the average rate. Column (4) reports the p-value of the test of equal means across the two groups shown in columns (2) and (3). Column (5) corresponds to the entire Field Survey sample. Column (6) contains respondents to the Field Survey who received the average rate treatment, and column (7) contains respondents of the Field Survey that did not receive the information on the average rate. Column (8) reports the p-value of the test of equal means across the two groups shown in columns (6) and (7). *Home Value* is the proposed assessment value; *Property Tax Rate* is the ratio of the property tax amount over the home value; *Owner-Protest in 2019* and *Agent-Protest in 2019* indicates whether the subject protested directly or through an agent in 2019, respectively; *2020 Homestead Exemption* indicates an effective homestead exemption. *Number of Bedrooms* is the number of bedrooms in the respondent's home. *White*, *Hispanic*, *Black*, and *Asian* are the fraction of homeowners by each imputed race.

Table 2: Results from the Field Experiment: Effects of the Information on the Average Tax Rate in the County

	Full Sample			Field Survey				
	(1) P^d_{2020}	(2) P^d_{2020}	(3) P^d_{2020}	(4) P^d_{2020}	(5) I_{2020}	(6) F_{2020}	(7) P^d_{2020}	(8) P^d_{2020}
Information Shock ($\overline{\tau}$)	-0.937*	-2.131*	-0.528	-12.566**	-11.919***	0.459*	-13.566*	-12.094
	(0.509)	(1.224)	(0.524)	(5.424)	(4.495)	(0.244)	(7.047)	(8.707)
Predicted Probability:		$Q > Q_{50}$	$Q \leq Q_{50}$					
Protested in Recent Past:							No	Yes
Mean Outcome (Control)	11.08	15.67	6.52	50.52	82.86	4.02	46.26	55.08
Std. Dev. Outcome (Control)	31.39	36.35	24.69	50.03	37.72	2.13	49.93	49.82
Observations	50,394	25,197	25,197	1,888	1,867	1,888	1,008	880

Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. All columns present results from equation (4) in Section 3.4. The variable *Information Shock* ($\overline{\tau}$) corresponds to the information shock term ($D_i \cdot (\overline{\tau} - \tau_i)$). Column (1) reports results for subjects in the field experiment who received a letter. Column (2) shows results for subjects who were predicted by a probit model to have an above-median probability of responding to the Field Survey contained in the mailed letters, while column (3) shows the same for subjects with below-median probability of responding. Columns (4) through (8) report results for subjects who received a letter in the field experiment and responded to the Field Survey. Columns (7) and (8) split the sample used in column (2) in two groups: i) subjects who did not protest during 2015 through 2019 (column (7)) and ii) subjects who protested at least once during 2015 through 2019 (column (8)). The dependent variables are defined as follows: P^d_{2020} is an indicator variable that takes the value 100 if the owner protested directly in 2020 and 0 otherwise; I_{2020} is an indicator variable that takes the value 100 if the household reported to be either likely or very likely to protest in 2020 and zero otherwise; F_{2020} corresponds to a question about whether the taxes of the respondent are fair relative to the taxes of everyone else, on a 1-10 scale. The regressions in this table include the following controls: the 2020 proposed value in levels and its annual growth, dummies for multiple owners, school and special districts, number of years since the household's last protest, a dummy for homestead status, a dummy indicating if the household received the extra aid message, and, for each previous year since 2015, a dummy indicating if the household protested in that year and the outcome of the protest (if any) as a percent-reduction in the market value (i.e., the protest history). Control variables for the protest history depend on the year in which the dependent variable is measured. For instance, if the outcome corresponds to direct protests in 2018, the protest history controls include protests in 2015, 2016, and 2017.