Where Do My Tax Dollars Go?

Tax Morale Effects of Perceived Government Spending

Matias Giaccobasso
University of California, Los Angeles

Brad Nathan
The University of Texas at Dallas

Ricardo Perez-Truglia*
University of California, Berkeley

Alejandro Zentner
The University of Texas at Dallas

Abstract

Do perceptions about how the government spends tax dollars affect the willingness to pay taxes? We designed a field experiment to test this hypothesis in a natural, high-stakes context and via revealed preferences. We measure perceptions about the share of property tax revenues that fund public schools and the share of property taxes that are redistributed to disadvantaged districts. We find that even though information on where tax dollars go is publicly available and easily accessible, taxpayers still have significant misperceptions. We use an information-provision experiment to induce exogenous shocks to these perceptions. Using administrative data on tax appeals, we measure the causal effect of perceived government spending on the willingness to pay taxes. We find that some perceptions about government spending have a significant effect on the probability of filing a tax appeal and in a manner that is consistent with the classical theory of benefit-based taxation. We discuss implications for researchers and policy makers.

JEL Classification: C93, H26, I22, K34, K42, Z13.
Keywords: taxes, protest, public services, education, redistribution.

*This Draft: February 15, 2022. Giaccobasso: mgiaccobasso@ad.ucla.edu. Nathan: brad.nathan@utdallas.edu. Perez-Truglia: ricardotruglia@berkeley.edu. Zentner: azentner@utdallas.edu. We are thankful for excellent comments from several colleagues. This project was reviewed and approved in advance by the Institutional Review Board at The University of Texas at Dallas. The field experiment was pre-registered in the AEA RCT Registry (#0007483). To prevent contamination of the subject pool (e.g., that subjects or the media could read about the hypotheses), we posted the RCT pre-registration immediately after the deadline to file a protest had passed, but before conducting any analysis of the data. After the study is accepted for publication, we will share all the code and data through a public repository. Xinmei Yang provided superb research assistance.

1 Introduction

Perceptions about how the government spends tax dollars may affect the willingness to pay taxes. We test this hypothesis using a natural field experiment in a high-stakes context and via revealed-preferences.

Property taxes represent an important source of revenue for governments in the United States and around the world. For instance, U.S. property tax revenues in 2019 were estimated at $577 billion (Tax Policy Center, 2021a).[1] In the United States, virtually all counties rely heavily on property taxes to fund key government services such as schools, parks, and roads, and school funding typically makes up the largest component of property taxes.

When studying attitudes towards taxation, economists and other social scientists rely primarily on survey data (Alesina and Giuliano, 2011; Cruces et al., 2013). However, survey data have some well-known limitations, such as social desirability bias. For example, some individuals may *say* that they are willing to pay more in taxes but would *choose* otherwise when facing real stakes. We study the willingness to pay taxes via revealed preferences, using households' decisions to file tax appeals (Nathan et al., 2020). Filing an appeal is a consequential action that households can take to reduce the amount they have to pay in property taxes.

Taxpayers' perceptions about government spending can affect their willingness to pay taxes. For example, early tax policy scholars focused on benefit-based taxation (Seligman, 1908; Musgrave, 1959). The logic of benefit-based taxation is that individuals are willing to pay more taxes when they believe that they benefit from the government services funded via those taxes. In assessing the benefits of public education, our research design leverages the fact that some households benefit directly from this government service because they have children enrolled in local public schools, whereas others do not benefit directly because they have no children enrolled in public schools. For the sake of brevity, we refer to households with children enrolled in public schools as "households *with* children" and those without children enrolled in public schools as "households *without* children."

We conducted our field experiment in Dallas County, Texas. We focus on this county primarily because, from a logistical perspective, it is more practical to implement a field experiment in a single location. However, as property taxes and tax appeals work similarly in most of the United States and in other countries around the world (Nathan et al., 2020; Dobay et al., 2019; World Bank, 2019), our experimental design could be easily replicated in other locations. Dallas County is the second-largest county in Texas, with an estimated population of about 2.6 million in 2020 (U.S. Census Bureau, 2021) – indeed, Dallas County

[1] For reference, the 2019 federal income tax generated $1,717 billion and corporate income tax $230 billion (Tax Policy Center, 2021b).

has a larger population than 15 of the 50 U.S. states. The county also is diverse along many dimensions, including ethnicity, political affiliation, and others.[2] We designed a survey to be completed by homeowners immediately before they face the opportunity to file a tax appeal. The survey elicits whether the household has children enrolled in public schools to identify which subjects benefit directly from public school spending and which do not. Most importantly, we conduct an information provision experiment informing a random sample of survey participants about where their tax dollars go, and then we observe their behavior using publicly available administrative data.

In the first treatment arm, we measure respondents' perceptions about the share of their own property taxes that corresponds to school taxes and thus funds public school spending. For the sake of brevity, in the remainder of the paper we refer to this percentage as the household's "school share". For reference, the school share is around 49.78% for the average household in Dallas County.[3] We can measure the respondents' misperceptions about where their tax dollars go by comparing their guesses about the school share to the true estimates from administrative records. To study the causal effect of beliefs about government spending, the survey embeds an information-provision experiment. After eliciting respondents' prior beliefs, we inform a random half of them about the true value of their respective school shares. By doing so, we can assess how that information affects their posterior beliefs, as measured in our survey data, and their decisions to file a tax appeal, as measured by administrative data.

The information-provision experiment creates exogenous variation in respondents' posterior beliefs about the fraction of their property taxes that funds local schools. For example, a subject who perceives her or his school share amount to be 30% may be informed that the actual share is 50%. According to the benefit-based framework and as noted in the randomized control trial (RCT) pre-registration, the expected effects of the information shock on subjects' willingness to pay taxes depends on whether their households have children enrolled in public schools. When subjects learn that their respective school shares are higher, those from households *with* children should become less likely to file a tax appeal because they will update their beliefs after learning that they benefit more from government services than they originally believed. Conversely, households *without* children enrolled in public schools should become more likely to file a tax appeal because they learn that they benefit less from government services than they originally thought.

[2] For example, in the 2012 presidential election, Barack Obama received 57% of the votes, whereas Mitt Romney received 42%, and the remaining 1% of votes went to third-party candidates.

[3] This average is calculated over 400,192 properties in Dallas County and excludes properties such as commercial properties and non-owner-occupied residencies; for more details, see Appendix B.2. Unless explicitly stated otherwise, all statistics about Dallas County are based on this sample.

The benefit-based taxation criterion could have implications for the ability to redistribute taxes. For instance, when taxpayers learn that their tax dollars are being spent in communities other than their own, they may be less willing to pay taxes. We explore this additional hypothesis using a second treatment arm and leveraging the significant redistribution of property taxes that occurs in some states, including Texas. In 1993, Texas enacted legislation to redistribute a significant fraction of property taxes from property-wealthy districts to property-poor districts to make funding more equitable across school districts.[4] The media often refer to this legislation as the "Recapture Plan" or the "Robin Hood Plan." Due to the high stakes involved, the recapture system has been a topic of heated debate among politicians and the general public (Dallas Morning News, 2018).

In the second treatment arm, we measure perceptions about the share of a household's school taxes that will be redistributed towards less wealthy school districts. For the sake of brevity, in the remainder of the paper we refer to this as the "recapture share." For example, a recapture share of 50% would imply that half of the school tax revenue will be transferred to disadvantaged school districts. We can measure respondents' recapture share misperceptions by comparing their guesses about the recapture share in their own district to actual estimates from administrative records. Again, we measure the causal effects of the perceived recapture share using an information-provision experiment. According to the framework of benefit-based taxation, the effects of the recapture share should depend on whether the household has children. For example, households *with* children should be more likely to protest upon learning that their school taxes are being diverted to other districts because these households value and use the government service. In contrast, households *without* children should not be more likely to protest because the diverted funds are being used for a service that do not benefit them directly.

We sent a letter to a sample of households in Dallas County inviting them to participate in an online survey. Our main subject pool comprises 2,110 respondents who completed the survey. We conducted the survey in April and May of 2021, a period during which subjects could file a protest of their property taxes with the county.[5] We then matched survey responses to administrative records from the county assessor's office. The rich administrative data allowed us to determine, among many other things, if the survey respondent subsequently filed a tax appeal.

Prior to any adjustment resulting from protests, the average subject in our sample owns a home worth $349,988 and pays $7,738, on average, in annual property taxes. Households

[4] For a history of property tax recapture in Texas, see for example Villanueva (2018).

[5] In 2021, households in Dallas County could file a protest between April 16, 2021, and May 17, 2021. For more details about how tax protests work, see Section 2.3. For more discussion of the context of tax appeals, see for example Nathan et al. (2020) and Jones (2019).

show significant variation in the extent to which they benefit from public education, which is important for our research design. For example, households *with* children accounted for 25.5% of the sample and households *without* children accounted for the remaining 74.5%. We also find significant variation in how the recapture system affects school districts in our sample, with some diverting as much as 57% of their districts' property taxes and others receiving as much as 23% additional funds from other districts.[6] In terms of protest behavior, for reference, 30.1% of the homeowners in the control group (i.e., those who did not receive any information on school taxes or on recapture) filed a tax appeal in 2021.[7] These tax protests are consequential. For instance, 65.4% of protests lead to a decrease in assessed home value, resulting in average tax savings of $579 in the first year alone.

The results from the first treatment arm indicate that even though the information is publicly available and easily accessible, most households have misperceptions about their respective school shares. When provided with factual information, we observe that households strongly update their beliefs. We use a simple two-stage least squares (2SLS) model that leverages information shocks from the experiment to estimate the causal effects of these beliefs. We find effects that are consistent with predictions of the benefit-based taxation framework. Upon learning that their school shares are higher, households *with* children become *less* likely to protest, whereas households *without* children become *more* likely to protest. The effects of the perceptions about government spending are statistically significant and significant in magnitude. Our baseline estimates imply that if increasing the (perceived) school share by 10 percentage points (pp) would cause a drop of 3.67 pp in the probability of filing a protest among households *with* children and an increase of 2.78 pp in the probability of protesting among households *without* children. The effects amount to 11% and 10% of the corresponding baseline protest rates, respectively. These results are robust to a host of alternative specifications and falsification tests.

To assess the novelty of the results, we conduct a forecast survey using a sample of 56 experts, most of whom are professors doing research on these topics. After receiving a brief explanation of the experiment, the experts are asked to forecast the experimental findings. Notably, only a small minority of experts accurately predict the experimental findings. Most predict null effects, expressed uncertainty about their own predictions, and some do not even correctly identify the sign of the effects.

The second treatment arm explores beliefs about recapture. We find that respondents have significant misperceptions about the recapture share and that they update their beliefs

[6] The reported numbers refer to net transfers. The gross transfers are even larger in magnitude, but all districts get some of their school taxes "back" as transfers from the state pool.

[7] Owners can protest directly on their own, which is the main focus of this paper, or they can hire an agent to protest on their behalf. In our control group, 30.1% of owners protest directly and 4.8% use an agent.

significantly when provided with information in the experiment. Both the levels of misperception and updating, however, are smaller relative to the corresponding findings for the school share, implying that the information shocks for school recapture are not as strong as those for school share. We do not find any statistically significant evidence that perceptions about the recapture share affect the probability of filing a tax appeal, either for households *with* children or *without* children. However, these null effects must be taken with a grain of salt due to their limited statistical precision.

Our study provides causal evidence on the roots of tax compliance and state capacity. Additionally, our results have some direct implications for fiscal policies. First, our results suggest that governments may be able to boost tax compliance by improving perceptions about how taxpayers benefit from their tax dollars, such as through transparency and accountability policies. Second, our evidence suggests that normative considerations, such as benefit-based taxation, should be incorporated into tax policy design. Regardless of its efficiency, a policy that is perceived as unfair by voters and policy makers is unlikely to be feasible in practice. A better understanding of why taxpayers perceive tax policies as fair or unfair can aid in the design of efficient and viable policies.

This paper relates to a recent and growing literature on the role of tax morale (Luttmer and Singhal, 2014). To improve causal identification, this literature uses field experiments consisting of sending messages to taxpayers and then measuring the effects of these messages on tax compliance (Slemrod, 2019).[8] For instance, Blumenthal et al. (2001), Castro and Scartascini (2015), Bott et al. (2020), and Bergolo et al. (2021) each include a treatment arm with a message related to government services. More precisely, these moral suasion messages highlight the importance of taxes to the provision of community services. Evidence on the effects of such messages is mixed.[9] Carrillo et al. (2021) conducted another experiment related to government spending in which 400 randomly selected taxpayers from an Argentine municipality are treated with a joint intervention recognizing them publicly for their good behavior and awarding them with the construction of a sidewalk near their homes. They

[8] Beyond field experiments, related evidence from a quasi-experiment (Cullen et al., 2020) and a laboratory experiment (Huet-Vaughn et al., 2019) shows that partisan alignment with the government affects tax compliance.

[9] Blumenthal et al. (2001) find no significant effects of their message on tax evasion. Bergolo et al. (2021) and Bott et al. (2020) find significant negative effects in the first year, but the effects do not persist after a year. Castro and Scartascini (2015) find insignificant effects on average, although they provide evidence of heterogeneous effects.

provide evidence that this intervention decreases subsequent tax delinquency.[10]

We provide three main contributions to the literature. All three contributions are possible due to the novel research design linking data from a survey experiment to administrative tax data at the individual level, which is new to this stream of literature and rare even in broader economics research (Bergolo et al., 2020). The first contribution is to measure and study *perceptions* of government spending. By collecting survey responses from taxpayers, we can precisely measure their beliefs about where their tax dollars go and how those beliefs change in response to accurate feedback.[11]

Our second contribution is the precise study of the causal mechanisms at play. The experimental interventions in the literature are designed as nudges, combining features that complicate interpretations of the effects. For example, the message in the previously mentioned study by Bergolo et al. (2021) includes normative language describing factual information about government spending, as well as factual information about tax evasion, and the intervention from Carrillo et al. (2021) awards some taxpayers with the construction of a sidewalk near their homes, as well as a social reward recognizing them publicly for their good behavior. Our experiment, which revolves around factual information on government spending, is designed to test more precisely the mechanism of benefit-based taxation.

Third, we make a methodological contribution. While previous studies measure intention-to-treat effects, our methodology allows us to measure treatment effects on the treated. This approach is especially important because, as highlighted by Luttmer and Singhal (2014), the null effects of tax morale messages on behavior may be mechanical due to a null effect on beliefs. Consider an intervention consisting of informing taxpayers about their school share. Whether we expect them to react positively, negatively, or not at all depends on whether they underestimate, overestimate, or correctly estimate their school shares. Our research design measures beliefs prior to the information provision, allowing us to fully disentangle reactions based on updating beliefs upwards, downwards, or not at all.[12] Moreover, the effects of this intervention would depend on whether the taxpayers have children enrolled in

[10] In a related result, Cait et al. (2018) show evidence from a laboratory experiment that tax compliance increases when individuals have the opportunity to voice their preferences for how taxes are to be spent. Also related to our study, Nathan et al. (2020) provides evidence that, conditional on their households' own tax rates, taxpayers' perceptions about the average tax rate affects their decisions to file a protest. Although this result does not pertain to perceptions of government spending, it constitutes consistent evidence that fairness concerns play significant roles in the decision to file a tax appeal.

[11] Beyond tax compliance, recent quasi-experimental evidence demonstrates how the salience of government expenditures can affect electoral outcomes (Huet-Vaughn, 2019; Ajzenman and Durante, 2022).

[12] Perhaps the most notable exception in this regard is Castro and Scartascini (2015), who provide a heterogeneity analysis exploiting differences in the quality of public services across different areas.

public schools.[13] We can disentangle those effects because we observe whether the taxpayer has children enrolled in local public schools.[14]

Our study also relates to a small but growing literature on the interplay between tax policy and normative considerations. Benefit-based reasoning was a prominent, and at times leading, approach among tax theorists in the early twentieth century (Seligman, 1908; Musgrave, 1959). However, it has been largely ignored by the modern optimal taxation literature, which instead focuses exclusively on efficiency (Weinzierl, 2018; Scherf and Weinzierl, 2020). Besides benefit-based taxation, a growing body of work is trying to incorporate other normative considerations into tax policy design (Mankiw and Weinzierl, 2010; Weinzierl, 2014; Saez and Stantcheva, 2016; Lockwood and Weinzierl, 2016).[15] This literature is new and mostly theoretical, with empirical evidence limited to survey data, such as asking individuals to choose between hypothetical tax policies (Weinzierl, 2014; Saez and Stantcheva, 2016). We contribute to this literature by providing the first revealed-preference evidence from a natural, high-stakes context.

The rest of the paper proceeds as follows. Section 2 describes the institutional context. Section 3 presents the conceptual framework. Section 4 discusses the experimental design and implementation. Sections 5 and 6 present the results. The last section concludes.

2 Institutional Context

2.1 Property Taxes and Public Schools

In Dallas County, property taxes fund various public services, such as schools, parks, roads, and police and fire departments. In 2021, the average home in Dallas County was worth $327,690, and the average estimated property tax bill was $6,370, implying an effective tax rate of 1.94%. Texas does not have a state income tax. Compared to other states, revenues from property taxes fund a greater share of local government services. Schools receive the largest share of a household's property tax, accounting for nearly half (49.78%) of the average total property tax bill.[16] The second-highest component is the city tax (accounting for roughly 28% of property taxes), followed by hospital (10%), county (8%), college (4%), and

[13] Indeed, an additional contribution of our study is that it focuses on public school spending, which is arguably one of the most important government services but receives no attention in this literature. For example, in fiscal year 2019, the U.S. government spent $752.3 billion on its 48 million children in public schools (2019 Public Elementary-Secondary Education Finance Data, U.S. Census Bureau).

[14] The use of survey data is crucial in this regard because this information is difficult to obtain from government records due to data privacy laws regarding children.

[15] For instance, the normative considerations related to equality of opportunity or poverty alleviation.

[16] Variation in the school share across households ranges from 13.2% (1st percentile of the distribution) to 90.8% (99th percentile).

special district (<1%) taxes.[17]

Dallas County has 14 main Independent School Districts (ISDs).[18] Homeowners who live within the geographical boundaries of a given ISD jurisdiction are subject to the tax rate for that ISD.[19] Households also have the right to send their children to the K–12 public school(s) in their ISD. All households must pay school taxes, regardless of whether they have children enrolled in public schools. The public schools in Dallas County are generally of good quality, although significant differences can occur.[20] Alternatively, homeowners can send their children to private schools, conduct homeschooling, or enter a lottery for a chance to send their children to a charter school.[21] Sending children to private schools can be expensive, however. The average tuition cost for private schools in Dallas County is $12,374 per student as of 2022.[22] According to data from the 2020 U.S. Census, 89% of kindergarten students and 92.5% of students in grades 1–12 attend a public school in Dallas County.

2.2 Property Tax Recapture

To make public school funding more equitable across school districts, Texas enacted a redistribution system in 1993 to divert school tax funds from "property-wealthy" districts to "property-poor" districts.[23] The recapture system has been amended several times since its inception, including a change in 2019 reducing what would have been a very large increase in the amount recaptured due to rising property values. Nevertheless, redistribution amounts remain substantial under the current recapture formula (Texas Education Agency, 2021c).[24]

In this paper, we focus on the *net* redistribution, which is the difference between the taxes recaptured by the state and whatever amount "comes back" from that state pool to the school district (for specifics on the recapture formula and this calculation, see Appendix B.1). Wide variation in the recapture share occurs across the 14 ISDs that we study. Four ISDs are net givers: the highest giver is Highland Park ISD, which diverts 57.3% of its school taxes. The remaining ten districts are net receivers: the highest receiver is Mesquite ISD, which receives

[17] See Appendix B.1 for more details.

[18] The total number of ISDs is sixteen, but two of them are extremely small and thus are excluded of the analysis. See Appendix B.2 for more details.

[19] School districts in Texas can set their own tax rates, but they must abide by certain state regulations. See Appendix B.1 for more details.

[20] For example, according to `www.GreatSchools.org`, 100% of the schools in the Highland Park ISD have above-average ratings in Texas, whereas 43% of schools in the Mesquite ISD have below-average ratings (data accessed on November 4, 2021).

[21] Charter schools are tuition-free public schools that receive funding directly from the state and do not receive funding from property taxes.

[22] Data accessed from https://www.privateschoolreview.com/ exas/dallas-county on January 5, 2022.

[23] This system was the result of poor school districts legally challenging the system of state school finances in the late 1980s and early 1990s on state constitutional grounds.

[24] See Appendix B.1 for more details.

an additional 23.3% in funding from property taxes diverted from other districts.

2.3 Tax Protests

Each year, the Dallas Central Appraisal District (DCAD) conducts market value appraisals for all homes in the county. Each appraisal results in a "proposed value" for the home, which is an estimate of the home's market value as of January 1 of that year. The DCAD makes this information available to all homeowners through its website or by mail.[25] The notice includes additional information, such as the estimated tax due based on the property's proposed values and how property taxes are allocated by jurisdiction type (e.g., school taxes, city taxes). Homeowners have one month from the date of the notice to protest if they disagree with the proposed assessment value. After the notifications are sent, households have a month from the notification date to file a protest. In 2021, the DCAD presented the proposed values on April 16; as a result, the deadline to protest was May 17.

Homeowners can file a protest by mail using a form included with their mailed notice, or they can file a protest online using a simple tool called uFile.[26] After reviewing the argument, the DCAD can (and often does) inform the homeowner by mail or phone that it will reduce the assessed home value and corresponding tax due. If the homeowner refuses the offer or the DCAD does not offer a settlement, the appeal proceeds to a formal hearing with the Appraisal Review Board.[27] Once protests are resolved, the new tax amount becomes payable either immediately or at some later date (e.g., on October 1st in 2021). Any unpaid taxes eventually become delinquent (e.g., unpaid 2021 property taxes became delinquent on January 31, 2022).

A key feature of this setting is the difficulty in estimating home market values for homes that have not been sold recently, a process involving significant ambiguity and subjectivity. To avoid costly in-person appraisals, the DCAD uses statistical models and large datasets (e.g., recent home sales) to formulate an estimated market value for each property. However, even multibillion-dollar companies like Zillow and Redfin have a hard time estimating market values using statistical models (Parker and Friedman, 2021). This ambiguity in home value

[25] A sample notification, called the "Notice of Appraised Value", is shown in Appendix G. This notification is available online for every household, and it is also sent by mail to some households (e.g., households with proposed values that increased since the previous year).

[26] To protest online, homeowners need to look up their account (e.g., searching for their own names or addresses) and then follow some straightforward steps in the uFile system. To protest by mail, households who received a notification from the DCAD can use the protest form included with the notification, and households that did not receive a notification can file by mailing a printed form that can be obtained online on either the DCAD's or the Texas Comptroller's website. In 2020, about 75% of direct protests were filed online while the remaining 25% were filed by mail (Nathan et al., 2020).

[27] Homeowners can contest the Appraisal Review Board's decision in court.

is important for the interpretation of our results because it implies that households are not trying to "correct" estimates from the DCAD. Instead, they are presenting a data point (e.g., the sale price of a neighboring home) to support their protest. This distinction is consistent with what was expressed in our conversations with officials from some of the county appraisal districts in Texas. Their prevailing view is that households use the subjective nature of the appraisal process as an excuse to complain about their taxes being too high (for more details, see Nathan et al., 2020) and not necessarily to complain about the county's estimate of their home value.

3 Conceptual Framework

To formalize the benefit-based logic, we introduce a simple model of how the provision of government services and redistribution affects the decision to file a protest. Let subscript $j \in \{C, NC\}$ represent the two types of households: those with children enrolled in public schools ($j = C$) and those without ($j = NC$). Let P_j be the outcome of interest: the probability that the household files a tax protest, which is a proxy for its (un)willingness to pay taxes. Let B_j be how much households in group j benefit from each dollar spent in government services. Consider the following relationship:

$$P_j = \gamma \cdot B_j \tag{1}$$

Motivated by the theory of benefit-based taxation, we assume $\gamma < 0$: that is, households benefiting more from government expenditures are less likely to protest their taxes. Let S be the government expenditures in the local public school district and NS be the government expenditures in other local government services (e.g., police, parks, and roads). The two types of households benefit from the two types of government expenditures in the following manner:

$$B_C = \alpha^S \cdot S + \alpha^{NS} \cdot NS \tag{2}$$

$$B_{NC} = \alpha^{NS} \cdot NS \tag{3}$$

where parameters α^S and α^{NS} capture how households benefit from different types of expenditures. The parameter α^S denotes how much a household *with* children enrolled in public school benefits per each dollar spent in public schools. α^{NS} denotes how much households (regardless of whether they have children) benefit per each dollar spent in non-school government expenditures. The key assumption is that households *with* children in public schools benefit more from school expenditures than from non-school expenditures: $\alpha^S > \alpha^{NS}$. This

assumption is meant to represent the fact that unlike the benefits from non-school expenditures (e.g., police, roads), which are spread over the entire community, the benefits from school expenditures are concentrated on a subset of the population (households *with* children enrolled in public schools) and thus the members of that subset enjoy them more.

Next, we conduct a simple normalization. Let $G = S + NS$ denote total expenditures and $s = \frac{S}{G}$ denote school expenditures as a fraction of total expenditures, which we previously defined as school share. For the sake of simplicity, we do not incorporate misperceptions into this simple framework. In practice, however, the "s" that matters is the one perceived by the taxpayer when deciding whether to protest. We thus can re-write equations (2) and (3) as follows:

$$B_C = G \cdot (\alpha^S \cdot s + \alpha^{NS} \cdot (1 - s)) \tag{4}$$

$$B_{NC} = G \cdot \alpha^{NS} \cdot (1 - s) \tag{5}$$

Combining equations (1), (4), and (5), we obtain the following:

$$P_C = \gamma \cdot G \cdot (\alpha^S \cdot s + \alpha^{NS} \cdot (1 - s)) \tag{6}$$

$$P_{NC} = \gamma \cdot G \cdot \alpha^{NS} \cdot (1 - s) \tag{7}$$

Using equations (6) and (7), we can see what happens to protest rates if the school share increases:

$$\frac{\partial P_C}{\partial s} = \gamma \cdot G \cdot (\alpha^S - \alpha^{NS}) < 0 \tag{8}$$

$$\frac{\partial P_{NC}}{\partial s} = -\gamma \cdot G \cdot \alpha^{NS} > 0 \tag{9}$$

The intuitions are straightforward. Households *with* children benefit most from school expenditures. Thus, an increase in s implies that they benefit more from government services and that their probability of protesting decreases. In contrast, households *without* children do not benefit from school expenditures. Thus, when s increases, their benefits from government services go down and their probability of protesting goes up. Moreover, if we subtract equation (9) from (8), we obtain the following:

$$\frac{\partial P_C}{\partial s} - \frac{\partial P_{NC}}{\partial s} = \gamma \cdot G \cdot \alpha^S < 0 \tag{10}$$

In other words, the difference in the effect of s between households *with* children versus those *without* children can be tracked to a key parameter of interest, α^S, which is how much

households *with* children benefit from school expenditures.

Prediction 1: *An increase in the school share should negatively affect the protest probability of households with children in public schools and positively affect the protest probability of households without children in public schools.*

This setup corresponds to the simplest case and is based on two simplifying assumptions. First, it assumes that households are entirely selfish and that households *without* children do not benefit at all from school spending, although in practice these taxpayers may feel good about helping other parents in the community. Second, it assumes that benefits from non-school services are the same for households *with* children as for households *without* children in public schools. We choose this setup due to its simplicity, but in Appendix A, we show that our prediction still holds under more general assumptions.

It is straightforward to extend this simple model to include redistribution of school taxes. For the sake of brevity, we consider the analysis from the perspective of a household in a wealthy school district whose school taxes are redistributed to disadvantaged school districts.[28] Let $r \in [0, 1]$ represent the fraction of school taxes that are transferred from the household's own school district to other school districts, which we previously defined as the recapture share. For instance, $r = 0.4$ would indicate that 40% of school taxes are redistributed to other school districts. We can extend equations (2) and (3) to incorporate recapture into the model:

$$B_C = \alpha^S \cdot S \cdot (1 - r) + \alpha^{NS} \cdot NS \tag{11}$$

$$B_{NC} = \alpha^{NS} \cdot NS \tag{12}$$

We normalize equations (11) and (12) by total expenditures, combine them with equation (1), and then rearrange them as follows:

$$P_C = \gamma \cdot G \cdot (\alpha^S \cdot s \cdot (1 - r) + \alpha^{NS} \cdot (1 - s)) \tag{13}$$

$$P_{NC} = \gamma \cdot G \cdot \alpha^{NS} \cdot (1 - s) \tag{14}$$

Using these equations, we can see what would happen if we increase the recapture share:

$$\frac{\partial P_C}{\partial r} = -\gamma \cdot G \cdot s \cdot \alpha^S > 0 \tag{15}$$

[28] The forces at play are similar from the opposite perspective, wherein a disadvantaged district receives funds from more advantaged districts.

$$\frac{\partial P_{NC}}{\partial r} = 0 \tag{16}$$

The intuitions are straightforward. For households *with* children, more recapture means fewer benefits for their local school district and thus less willingness to pay taxes. Households *without* children in the school district do not benefit from school taxes, regardless of which school district receives the funding, so their willingness to pay taxes is unaffected by recapture.

We can also subtract (16) from (15) to show the following:

$$\frac{\partial P_C}{\partial r} - \frac{\partial P_{NC}}{\partial r} = -\gamma \cdot G \cdot s \cdot \alpha^S > 0 \tag{17}$$

Again, the difference in effects between households with children and without children is determined by parameter α^S.

Prediction 2: *An increase in the recapture share should increase the protest probability for households with children in public schools, but it should not affect the protest probability for households without children in public schools.*

This framework assumes that households are totally selfish and care only about how they benefit from government services. In practice, households may appreciate that their tax dollars help the community. In Appendix A, we provide an extension of this framework that incorporates such altruism. Prediction 2 thus must be taken in context because it no longer holds once we allow for altruism.

4 Experimental Design and Implementation

4.1 Subject Recruitment

We mailed our letters so that they would be delivered close to the time that homeowners in Dallas County could start filing tax appeals. Appendix C shows a sample envelope, and Appendix D shows a sample letter. We included several features to indicate the legitimacy of the letters. For example, the letters were sent on behalf of researchers at The University of Texas at Dallas, a well-known institution in Dallas County. The envelope featured the school's logo, the name of a professor from that university, and non-profit organization postage. The letter itself included a physical address for the researcher and a link to the study's website (see Appendix E for a screenshot of the website). It also provided contact information for the researchers and institutional review board. The letter salutation included each recipient's name, and recipients' names and addresses were printed at the bottom of the second page so that they appeared through the envelope window. In cases where properties were jointly

owned by multiple individuals (typically, husband and wife), we sent one letter to the address but listed all owners on the letter. As previously mentioned, the letter also mentioned the recipient household's proposed value and estimated property tax amount for 2021.

Most importantly, our letters included an invitation to participate in an online survey and included the URL of the survey. Each subject was asked to enter their unique survey code, which was included in the letter right next to the survey URL. This survey code allowed us to identify survey respondents and link their responses to the appropriate administrative records. In addition to the opportunity to contribute to a research study, we included two additional incentives for survey participation. First, the letters indicated that detailed, step-by-step instructions on how to file a protest online or by mail would be provided at the end of the survey.[29] As a second incentive, subjects were informed that survey respondents would be entered into a raffle for 20 prizes worth $100 each.[30]

4.2 Survey Design

A sample of the full survey instrument is attached as Appendix H. In this section, we summarize the main features of the survey. We start by asking a critical question, that is, whether the respondent's household has children enrolled in grades K–12 at their local public school district, and if so, how many. This critical information is missing from administrative records of the tax agency and thus the analysis would be impossible without this question, particularly the heterogeneity analysis concerning the benefit-based taxation framework, which is the main form of heterogeneity that we anticipate in the RCT pre-registration.

The module about school taxes can be summarized as follows:

- **Step 1 (Elicit Prior Belief):** We begin by providing the estimated total property tax amount of the respondent's home in 2020 (based on administrative records). We then explain that this total amount is the sum of different components, such as school, city, and hospital taxes. We ask respondents to guess their school share in 2020, using any amount between 0% and 100%.

- **Step 2 (Information-Provision Experiment):** For every subject, we calculate the

[29] This walkthrough included hyperlinks to relevant websites and screenshots of a sample protest using information for a fictitious household for added clarity. To access these instructions, subjects were provided with a URL and a code on the final screen of the survey. A copy of the web instructions is included in Appendix F. Nathan et al. (2020) show that these instructions have a significant positive effect on the probability of protesting.

[30] All respondents were entered into the same raffle, but only a random half of respondents were informed about the raffle in the letter (i.e., before deciding whether to participate in the survey). This randomization aimed to assess the effectiveness of raffle prizes in increasing response rates, which can be useful information for future researchers conducting similar field experiments. For more details, see Appendix B.5.

"correct" answer to the previous question based on administrative records. We then randomize whether the subject sees the correct answer. Each subject faces a 50% probability of being shown this information. To avoid respondents making inferences from the act of receiving information, we make the randomization explicit. On the first screen, we inform respondents that some participants will be randomly chosen to receive the information and that they will find out on the next screen if they are selected. On the following screen, we inform subjects whether they are chosen to receive the feedback.

- **Step 3 (Elicit Posterior Belief):** We give all subjects the opportunity to revise the guess they provided Step 1. To avoid asking the exact same question about their 2020 taxes (i.e., the year prior to our intervention), we instead ask about their 2021 taxes (i.e., the most recent year). To avoid subjects making inferences based on the opportunity to re-elicit their guesses (e.g., subjects inferring that we ask again only if their answer in step 1 is incorrect), we explicitly inform them that all survey participants have this opportunity, regardless of their initial guesses.

To learn about the causal effects of beliefs, it is critical to leverage information on prior beliefs. When provided with feedback during the information-provision experiment, individuals who underestimate may update their beliefs upward and those who overestimate may adjust their beliefs downward. Some individuals may have accurate priors and thus may not make any updates. Whether an individual's probability of protesting increases, decreases, or remains the same depends on the individual's beliefs before receiving the information. For this reason, we conduct the information-provision experiment within the survey, as opposed to providing the information directly in the letter, to measure beliefs prior to information provision. To leverage the effect of the information on prior beliefs, we use the same econometric models used in other information-provision experiments (see e.g., Cullen and Perez-Truglia, 2021; Bottan and Perez-Truglia, 2020b).

The following module is about the recapture share.[31] Some subjects may not know about or understand recapture. Thus, we start with a couple of short paragraphs summarizing the recapture system. The rest of the module follows the same structure as previously described for steps 1 through 3. We elicit beliefs about the recapture share in two steps. First, we ask respondents to guess if their school district will receive more, the same, or less in taxes than what households in their district paid in school taxes. The second step is quantitative in nature. If the respondent selects "More" (or "Less") in the first question, we ask them to guess how much *more* (*less* funding their school district will receive as a share of the

[31] Note that the recapture share is ISD-specific, whereas the school share is household-specific.

district's school tax revenues due to recapture, using any amount between 0% and 100%. We then conduct step 2 (information-provision experiment) and step 3 (elicitation of posterior beliefs).

We cross-randomize subjects to receive the two pieces of information about school taxes and recapture, respectively, with a 50% probability for each. Thus, roughly 25% of the sample receives both pieces of information, 25% receive the first piece of information only, 25% receive the second piece of information only, and 25% receive no information at all.

These questions comprise the core of the survey. We also include a series of additional questions, including one question that serves as a secondary outcome in the analysis of the effects of beliefs. We ask respondents whether they plan to file a protest this year in a 1-4 likelihood scale. This outcome allows us to pick up short-term effects on the *intention* to protest, even if those effects do not materialize into actual protests. For descriptive purposes, we include questions asking respondents' gender, age, ethnicity, education, and political party. To provide supplemental evidence, towards the end of the survey, we include additional questions that are described in more detail in the following sections.

4.3 Subject Pool

We sent the previously described letters to 78,128 households representing a sub-sample of the universe of all households in Dallas County, Texas. We arrived at this sub-sample by applying several filters (e.g., excluding commercial properties and non-owner-occupied residences.)[32] When selecting this sample, we stratified the randomization at the ISD level to ensure wide representation of the beneficiaries and contributors of the recapture system.[33] We can link each survey respondent to rich sources of administrative data, including whether the subject protested in any year from 2016 to 2020, as well as detailed information on property ownership, address, number of bedrooms and other features, exemption amounts, taxable values, and tax rates.

We timed the intervention so that our letters would arrive early enough before the protest deadline to influence the recipient's decision. We created the letters on April 16th, 2021, as soon as the administrative data, including 2021 proposed values, became available. To accelerate delivery, we used a mailing company in Dallas County (i.e., the same county as all recipients). The mailing company dropped off the letters at the local post office on April 20, 2021, and estimated that most would be delivered in the next couple of days. Consistent with this projection, we began to receive survey responses and visits to the study's website

[32] For the full inclusion criteria, see Appendix B.2.
[33] For more details, see Appendix B.3.

on April 22, 2021.[34]

Of 78,128 households invited to the survey, 2,966 answered the first two questions and 2,821 completed the two key modules on posterior beliefs about the recapture share.[35] The implied response rate of 3.6% ($= \frac{2,821}{78,128}$) is comparable to the response rate of 3.7% from a previous study in this same context and with a similar recruitment method (Nathan et al., 2020). Moreover, the response rate of 3.6% is on the same order of magnitude as the response rate of surveys that use this recruitment method (4.7%, as reported in Sinclair et al., 2012).[36] Among respondents, the median time to complete the survey was 11.3 minutes. Towards the end of the survey, we included an attention check similar to the one used in other studies (Bottan and Perez-Truglia, 2020a), which 92.1% of respondents successfully passed. This passing rate is high in the context of survey studies, particularly given that the attention check was located at the very end of the survey when fatigue was likely at its highest.

Of the 2,821 survey responses, we drop responses that, as explained in the RCT pre-registration, could not be excluded ex ante because of data availability. We drop 36 responses from subjects who, according to DCAD records, had already filed a protest before starting our survey and 23 additional subjects who responded to the survey after the deadline to file a protest, as the survey information could not have affected their decisions to protest. We similarly drop 185 subjects who, according to DCAD records, had already hired an agent to file a protest on their behalf.[37]

When studying perceptions via survey data, it is important to deal properly with outlier beliefs. Some individuals may provide guesses that are wildly inaccurate not because they truly hold such extreme beliefs but because they misunderstand the question, make a typo, or just do not pay attention to the question. The "information shocks" for these individuals will be large but meaningless, which can create a significant attenuation bias. To reduce sensitivity to outliers, we follow the standard practice in information-provision experiments and drop respondents with the most extreme misperceptions in their prior beliefs (see e.g., Fuster et al., 2021; Cullen and Perez-Truglia, 2021; Bottan and Perez-Truglia, 2020a). For the baseline specification, we use a conservative definition of outliers that drops 467 subjects from the bottom 5% and top 5% of the distribution of prior misperceptions.[38] After applying these filters, 2,110 respondents remain, constituting our main subject pool. We also exclude subjects based on pre-treatment variables (e.g., prior beliefs), so these filters should not

[34] More details about the timing of survey responses are provided in Appendix B.6.

[35] See Appendix B.3 for more details about the sample and Appendix B.6 for more details about attrition rates and balance tests.

[36] The 4.7% response rate corresponds to a mailing of a personally addressed postcard inviting a household to complete a web-based survey using a unique alphanumeric code.

[37] For more details, see Appendix B.2.

[38] For more details on the distribution of outlier observations, see Appendix B.7.

compromise the validity of the experimental variation. As a robustness check, we reproduce the analysis with more lax definitions of outliers (results presented in Section 5.5). Finally, we provide several sharp falsification tests to address any potential concerns about the internal validity of the results, such as event-study analyses.

Column (1) of Table 1 presents descriptive statistics about the subject pool. Prior to any adjustment resulting from protests, the average subject owns a home with an assessed market value of $349,988 and property taxes of $7,738 (corresponding to an average tax rate of 2.21%). Around 25.5% of respondents have children enrolled in a local public school, 42.9% are women, 44.3% self-identify as White, 38.3% have a college degree, and on average they are 49.6 years old.

In terms of observable characteristics (e.g., home value, number of bedrooms, or tax rate), the subject pool is similar to the universe of households in the county. Differences between survey respondents and non-respondents are statistically significant but small (see Appendix B.3). However, one significant difference is that, relative to the universe of households, households that responded to the survey are substantially more likely to file a protest in 2021 and in previous years. By design, our study targets individuals who would seriously consider protesting, which increases statistical power by securing more variation in the outcome variable.[39] Moreover, our letter describes tax protests, so subjects considering filing a protest in 2021 are likely to pay attention to the letter and thus also likely to notice the survey link included in the letter.[40]

Columns (2) through (5) of Table 1 break down the average characteristics in each of the four treatment groups. All characteristics shown in Table 1 are determined pre-treatment and thus should not be affected by the treatment assignment.[41] Column (6) reports p-values for the null hypothesis that the average characteristics are equal across the four treatment groups. Table 1 shows that, consistent with successful random assignment, the observable characteristics are balanced across treatment groups.[42] In Appendix B.6, we present alter-

[39] Specifically, when selecting households to participate in the survey, we over-sample those most likely to protest, such as households with a history of increased estimated taxes. For more details, see Appendix B.2.

[40] Indeed, this higher propensity to protest among survey respondents is consistent with results from Nathan et al. (2020), who use a similar recruiting method to collect survey responses in this same context. Moreover, our letter promises instructions on how to file a protest as a reward for participation, so it is natural that interested respondents would be more likely to participate. Additionally, these instructions likely make it easier for survey respondents to file an appeal, as documented in Nathan et al. (2020).

[41] Some questions, such as gender of the respondent, are asked after the information-provision stage. However, treatment assignment should not affect these responses. For instance, we do not expect information on school spending to change responses regarding gender or education level.

[42] The difference is statistically significant for one of the variables (owner protest in 2020). Given the large number of tests conducted, a few differences may be statistically significant just by chance. To be safe and to follow best practices in field experiments (Athey and Imbens, 2017), we include this variable in the set of control variables in all regressions.

native versions of the randomization balance tests, such as breaking the sample down by households *with* and *without* children. We also show that participation in the survey and attrition among participants is orthogonal to treatment assignment, which is expected given that subjects randomly receive a treatment after they start the survey.

4.4 Outcomes of Interest

As stated in the RCT pre-registration, the main outcome of interest is a dummy variable indicating whether the household protested on its own in 2021.[43] To get a sense of the baseline protest rate, we consider subjects in the control group (i.e., those who do not receive any information on school taxes nor recapture). Approximately 30.1% of those owners file a tax appeal in 2021. These tax protests are consequential: 65.4% lead to a decrease in the assessed home value, among which the average tax savings were of $579 in the first year alone.[44]

Owners can file their own protests, which is the main focus of this paper, or they can hire an agent to file a protest on their behalf. In addition to the 30.1% of owners who protest directly, 4.8% use an agent. Due to the nature of the setting and the timing of the protest process, and as stated in the RCT pre-registration, we expect our information to primarily affect whether households file their own protests. It is unlikely that the information provided in our survey would influence whether households hire an agent as these households prefer not to protest on their own. Also, because we provide information to the households and not to their agents, our experiment should not affect the agent's behavior. Moreover, according to anecdotal accounts, households typically sign contracts with agents well in advance of the date when the proposed values are announced, which is when we provide the information. Some agents even offer long-term contracts to represent the owner over may years. For these reasons, we study protests through tax agents as a separate outcome, in a falsification spirit. For the sake of brevity, in the rest of the paper we use the term protest as shorthand for direct protests by the homeowner, unless explicitly stated otherwise.

4.5 Expert Prediction Survey

To assess whether the experimental results are surprising, we conduct a forecast survey with a sample of experts. A sample of the full survey instrument is attached as Appendix I. In this survey, which follows best practices (DellaVigna et al., 2019), we describe the experiment

[43] The protest variable is based on data downloaded from the DCAD website on June 22, 2021.

[44] These calculations are based on data downloaded from the DCAD website on December 2021. The remaining protests are either unresolved by December 2021 (12.2%) or resolved with no change in the assessed home value (22.4%).

and ask experts to forecast the key results in a way that is comparable to the experimental estimates. More precisely, we elicit the effect of a 10-pp shock to the belief about the school share, separately for households *with* and *without* children. We then conduct the corresponding elicitations for beliefs about the recapture share.

We collected responses from experts in two ways. First, we posted the survey in the Social Science Prediction Platform from July 13, 2021, to December 31, 2021. Second, on November 2021, we emailed an invitation to the prediction survey directly to a list of 238 professors with publications related to our experiment. The final sample includes 56 experts. Of these, 21.4% responded to the survey through the Social Science Prediction Platform, and the remaining 78.6% responded through our email invitation.[45] The final sample thus comprises 82.1% professors, 12.5% PhD students, 3.6% post-docs, and 1.8% researchers. Most (78.6%) are from the field of economics; 66.1% report having done research on taxation and 25% on preferences for redistribution.

5 Perceptions about School Spending

5.1 Accuracy of Prior Beliefs

Transparency and accountability efforts have made information about property taxes publicly available. Each year in Dallas County, the Dallas Central Appraisal District (DCAD) provides each homeowner with a Notice of Appraised Value, which is detailed break-down of property taxes by tax jurisdiction, including the share of their property taxes that funds public schools. See Appendix G for a sample of this notice, with the breakdown by tax jurisdiction shown on the second page).[46] We acknowledge that the ease of access to this information does not mean that everyone searches for it or uses it. Many other contexts show that individuals often misperceive easily obtained information, such as the official inflation rate (Cavallo et al., 2017) or recent trends in national home prices (Bottan and Perez-Truglia, 2020a).

Figure 1(a) shows a histogram of the degree of misperceptions about the school share.[47] The x-axis corresponds to the difference between the actual school share (i.e., potential feed-

[45] Among the responses from the Social Science Prediction Platform, we exclude respondents who are not academics, who do not have a PhD, or who are not pursuing a PhD.

[46] The county uses the prior year's jurisdictional tax rates to estimate taxes due in the Notice of Appraised Value because the tax rates for the current year are set later in the year. In practice, tax rate changes are uncommon, so approximation errors are typically negligible. In our study, we use the same definition of estimated taxes because these are the relevant object of study and they represent the subjects' best approximation at the time of deciding whether to protest.

[47] All results are based on the final survey sample, which excludes the outlier misperceptions (i.e., the bottom and top 5%). Including the extreme observations would increase the degree of misperceptions; for more details, see Appendix B.7.

back) versus that perceived by respondents. For the sake of brevity, we use the term feedback to refer to potential feedback. A minority of subjects have accurate perceptions: more precisely, 32.6% of subjects guess the school share to be within ± 5 pp of the actual school share. Misperceptions are quite large on average: the mean absolute error is 16.57 pp. The large degree of misperceptions implies sufficient scope for the information provision experiment to shock beliefs. Another interesting feature of prior beliefs is that the misperceptions show a systematic bias: on average, subjects underestimate the school share by 13.08 pp, as indicated by the mean error. This systematic bias is quite noticeable in Figure 1(a), which has many more observations on the right half of the histogram (corresponding to underestimation) than on the left half (corresponding to overestimation).

5.2 Belief Updating

We find that taxpayers update their inaccurate beliefs when provided with accurate feedback. To model belief updating, we use a simple Bayesian model that has been shown to accurately represent belief formation in other information-provision experiments on a wide range of topics, such as inflation expectations (Cavallo et al., 2017), salary expectations (Cullen and Perez-Truglia, 2021), and home price expectations (Fuster et al., 2021).

We use the subscript i to index the subjects. We use the variable s_i^{prior} to represent subject i's belief right before the information-provision experiment. We use the variable s_i^{feed} to represent the value of the feedback that the subject can potentially receive in the experiment. We define the variable T_i^S as a binary variable that equals 1 if subject i is selected to receive the information and 0 if not. We define variable s_i^{post} as the posterior belief. Specifically, s_i^{post} represents the perceived school share after the taxpayer sees, or does not see, the feedback.

An individual will form posterior belief (s_i^{post}) as a weighted average between the prior belief s_i^{prior} and the feedback (s_i^{feed}), weighted by a parameter α. This parameter, which captures the degree of learning, can range between 0 (individuals ignore the feedback) and 1 (individuals fully adjust to the feedback), and it is a function of the relative precision of the prior belief versus that of the feedback.[48] This Bayesian updating can be summarized by the following linear relationship:

$$s_i^{post} - s_i^{prior} = \alpha \cdot \left(s_i^{feed} - s_i^{prior} \right) \tag{18}$$

Intuitively, Bayesian learning predicts that, when shown feedback, respondents who over-

[48] These results assume normal distribution of priors and feedback and assume that the variance of the prior and the variance of the feedback are independent of the mean of the prior. For more details, see Hoff (2009).

estimate the school share would revise their beliefs downward, whereas respondents who underestimate the school share would revise their beliefs upward. Figure 1(b) estimates this Bayesian learning using a binned scatterplot. The x-axis corresponds to the gaps in prior beliefs ($s_i^{feed} - s_i^{prior}$), and the y-axis denotes the belief updating ($s_i^{post} - s_i^{prior}$). Intuitively, the x-axis shows the maximum revision we would expect if the respondent were to fully react to the information, and the y-axis shows the actual revision. In the case of no updating, the observations should form a horizontal line; in the other extreme, full updating, the observations should form a 45-degree line. The red circles in Figure 1(b) correspond to subjects who are shown the feedback about the school share. Consistent with significant updating, there is a strong relationship between the belief revisions and prior gaps: an additional percentage point (pp) in perception gap is associated with an actual revision that is 0.809 pp higher.

The gray squares in Figure 1(b) correspond to the subjects who do not receive information about the school share. In the absence of feedback, these subjects should not update their beliefs. However, in practice, individuals might revise their beliefs in the direction of the feedback for spurious reasons even when receive no feedback. For instance, respondents may reassess their answers or correct typos when asked a question a second time, leading to an answer that is closer to the truth. The gray squares indicate a weak relationship between belief updating in the group that was not shown the feedback: an additional 1 pp in the prior gap is associated with an actual revision that is 0.052 pp higher. This effect is statistically significant (p-value <0.001) but economically very small. This result of a significant, but small, degree of spurious revisions is consistent with other information-provision experiments (e.g., Fuster et al., 2021; Cullen and Perez-Truglia, 2021).

We can exploit the random assignment from the information-provision experiment to control for spurious learning:

$$s_i^{post} - s_i^{prior} = \tau + \alpha \cdot \left(s_i^{feed} - s_i^{prior}\right) \cdot T_i^S + \beta \cdot \left(s_i^{feed} - s_i^{prior}\right) + \epsilon_i \tag{19}$$

In this model, parameter α represents true learning arising from the information provision (not spurious learning), whereas parameter β captures spurious learning. Parameter α can be computed from the estimates in Figure 1(b). Specifically, the α parameter corresponds to the difference in the regression slopes between the subjects who are or are not shown the feedback. This parameter captures the effect of the exogenous shocks induced by the information-provision experiment. For this reason, it can be used as an excluded instrument in the econometric model below. The estimated α is large ($0.757 = 0.809 - 0.052$) and highly statistically significant (p-value<0.001). This difference suggests that a 1-pp information shock induces a 0.757 effect in the subject's posterior belief. In other words, although subjects did not update fully to the feedback, they were close to updating fully. The fact that subjects

did not update fully is consistent with other information-provision experiments and likely due to some subjects mistrusting the source of the feedback or simply not paying enough attention to the survey.

Appendix B.6 provides some additional results and robustness checks. For instance, this appendix shows that learning from the feedback is compartmentalized (i.e., subjects do not use the information about school share to update beliefs about the recapture share). This appendix also shows that the belief updating results are similar for households *with* and *without* children.

5.3 2SLS Model

Let P_i^{2021} denote the main outcome of interest: an indicator variable that equals 100 for individuals filing a protest in 2021 (i.e., post-treatment) and 0 otherwise. As discussed in the conceptual model in Section 3 and as noted in the RCT pre-registration, the effects are expected to have different signs depending on whether the household has children enrolled in public schools. Let $C_i \in 0, 1$ be an indicator variable that equals 1 if the household has a child enrolled in a local public school and 0 otherwise. We use the following econometric specification:

$$P_i^{2021} = \beta_0 + \beta_C^S \cdot C_i \cdot s_i^{post} + \beta_{NC}^S \cdot (1 - C_i) \cdot s_i^{post} + \beta_1 \cdot C_i + \epsilon_i \tag{20}$$

The term ϵ_i represents the error. The two parameters of interest are β_C^S and β_{NC}^S. The conceptual model from Section 3 predicts that $\beta_C^S < 0$ and $\beta_{NC}^S > 0$. Moreover, the difference between these two parameters, $\beta_C^S - \beta_{NC}^S$, is of special interest because it captures the benefit-based motivation behind public schools. As posterior beliefs (s_i^{post}) are endogenous and thus could suffer from a host of omitted variable biases, we estimate equation (20) using 2SLS, exploiting the exogenous variation in posterior beliefs induced by the information-provision experiment. More precisely, we estimate the following model:

$$\begin{aligned} P_i^{2021} = &\beta_0 + \beta_C^S \cdot C_i \cdot s_i^{post} + \beta_{NC}^S \cdot (1 - C_i) \cdot s_i^{post} + \beta_1 \cdot C_i + \\ &+ \beta_2 \cdot C_i \cdot (s_i^{feed} - s_i^{prior}) + \beta_3 \cdot (1 - C_i) \cdot (s_i^{feed} - s_i^{prior}) + X_i \beta_X + \epsilon_i \end{aligned} \tag{21}$$

The endogenous variables are $C_i \cdot s_i^{post}$ and $(1 - C_i) \cdot s_i^{post}$, for which we use the excluded instruments $C_i \cdot T_i^S \cdot (s_i^{feed} - s_i^{prior})$ and $(1 - C_i) \cdot T_i^S \cdot (s_i^{feed} - s_i^{prior})$.[49]

We can demonstrate the intuition behind the model using a simple example. Consider a pair of households with children enrolled in public school and that share the same bias about

[49] Note that equation (21) controls for the prior gaps in beliefs ($C_i \cdot (s_i^{feed} - s_i^{prior})$) and ($1 - C_i$)$\cdot (s_i^{feed} - s_i^{prior})$). These control variables ensure that the excluded instruments isolate the information shocks that are driven purely by the random assignment of the feedback (T_i^S).

the school share: both underestimate the actual school share by 20 pp. Suppose we randomly assign information about the true school share to one of these two households. We expect that, relative to the subject who does not get the information, the subject who receives the information adjusts their perceived school share upward. For the sake of argument, assume that the subject who does not receive the information continues to underestimate the actual school share by 20 pp and that the subject who does receive the information reacts to it by underestimating the school share by just 10 pp. The information provision is thus equivalent to a +10-pp shock to the perceived school share. We can then check the behavior of this pair of households in the weeks after they receive the information. For example, the +10-pp shock to the perceived school share could translate to a lower probability of filing a protest. Again, for the sake of the argument, assume that the +10-pp shock to the belief causes a 2-pp drop in the probability of protesting. Combining these two results, we can estimate that $\beta_C^S = -0.2$, that is, each 1-pp increase in the perceived school share lowers the probability of protesting by 0.2 pp.[50]

The term X_i in equation (21) corresponds to a set of additional control variables. In principle, the 2SLS model leverages the experimental variation, so there is no need to include additional control variables for identification. However, the inclusion of additional control variables can be helpful, for instance, to reduce the variance of the error term and thus improve precision (McKenzie, 2012). The vector of control variables includes basic pre-treatment information, such as the household's prior history of tax appeals.[51]

Following the regression specification we use to study the effects of the school share (equation (20)), it is straightforward to define the regression specification to study the effects of recapture. Indeed, as these two information treatments are cross-randomized for the same sample, we estimate all effects simultaneously in a single 2SLS regression. See Section 6.3 for a discussion of the recapture share estimates.

[50] Typically in 2SLS models, if treatment effects are heterogeneous, the estimates identify the local average treatment effects of beliefs (Imbens and Angrist, 1994). More precisely, in our study, our estimates would give a higher weight to subjects whose beliefs are more affected by the information-provision experiment. By construction, this weight will be higher for subjects with larger prior misperceptions and, conditional on the misperceptions, those who react more strongly to feedback.

[51] The full set of additional control variables includes the log of total market value in 2021, the growth in total market value between 2021 and 2020, an indicator for positive growth, an indicator of whether the property value was re-evaluated in 2021, the 2021 estimated property taxes (in logs), a dummy for homestead exemption in 2021, an indicator for homestead binding in 2021, the household's effective tax rate, a dummy variable for multiple owners, a dummy variable for condos, the total living area, the number of bedrooms, the number of full baths, the building age, a set of dummies for school districts, the survey start date, and indicator variables for whether the household protested in each pre-treatment period since 2016 (one set for direct protests and another set for protests through agents).

5.4 2SLS Estimates

The 2SLS estimates for school share are presented in the top half of Table 2.[52] In column (1), the dependent variable is the main outcome of interest: an indicator variable that equals 100 if the subject protests directly in 2021 and 0 otherwise. According to the hypothesis of benefit-based taxation, an increase in the perceived school share should decrease the probability of protesting for households with children (because they find out that they benefit from government services more than they thought), whereas this information should have the opposite effect on households without children. The results are consistent with this hypothesis. The coefficient for households *with* children is negative (-0.367) and statistically significant (p-value=0.096). The coefficient for households *without* children is positive (0.277) and statistically significant (p-value=0.032). Most importantly, the difference between the two coefficients (-0.367 and 0.277) is statistically significant (p-value=0.012).

As a thought experiment, consider what would happen if the perceived school share increases by 10 pp. The estimates from column (1) of Table 2 indicate that this change would cause a drop of 3.67 pp ($= 0.367 \cdot 10$) in the probability of filing a protest for households *with* children and an increase of 2.77 pp ($= 0.277 \cdot 10$) in the probability of protesting for households *without* children. These effects would be roughly equivalent to 11% and 10% of the baseline protest rates (33.86 pp and 28.83 pp, reported at the bottom rows of Table 2).

Column (2) of Table 2 is identical to column (1), except that it uses a different dependent variable: an indicator variable that equals 100 if at the end of the survey the subject responds "very likely" to the question on the likelihood to protest in 2021 and 0 otherwise. This outcome measures the intention to protest and allows us to measure if the effects of the information lead to an intention to protest immediately after the information is provided. For reference, at the time of the survey, 45.4% report that they are very likely to protest (this corresponds to the baseline rate, combining subjects *with* and *without* children who do not receive any feedback). It is important to note that the stated intention to protest correlates significantly with whether the individual actually files a protest, but that correlation (0.410) is far from perfect (the correlation coefficient is 0.410 for the no-feedback group, p-value<0.001).[53] Due to this imperfect correlation, the effects on the intention to protest at the time of answering the survey should not expected to be "mechanically" the same as the effects on actual protests.

[52] We present the 2SLS estimates directly because they can be interpreted more easily. Nevertheless, due to the strong first stage (i.e., high belief updating), the 2SLS estimates are similar (in terms of magnitude and statistical significance) to the reduced-form estimates. For more details, see Appendix B.8.

[53] This correlation (0.410) is computed among subjects who do not receive any feedback. Among respondents who report being very likely to protest, 56.8% end up protesting directly or through an agent. On the other hand, among the individuals who do not report being very likely to protest, 16.8% end up protesting.

The results from column (2) of Table 2 are consistent with the results from column (1). In column (2), the coefficient for households with children is negative (-0.408) and similar in magnitude to the corresponding coefficient from column (1) and statistically significant (p-value=0.080). The coefficient for households *without* children is positive (0.269), on the same order of magnitude as the coefficient from column (1), and statistically significant (p-value=0.062). Again, most importantly, the difference between the coefficients for households with children versus those without children (-0.408 and 0.269) is statistically significant (p-value=0.014).

5.5 Robustness Checks

To probe the robustness of the school share results, columns (3) and (4) of Table 2 provide two falsification tests. The first falsification test uses a dependent variable to indicates whether the household protests through an agent. As explained in Section 4.4, it is highly unlikely that the information provided in our survey would affect protests through an agent. The results are reported in column (3), which estimates the same regression from column (1) but using protests conducted by agents as the dependent variable. As expected, the coefficients from column (3) are close to zero (-0.028 and -0.033) for both households *with* and *without* children), precisely estimated, and statistically insignificant (p-values of 0.816 and 0.518). Moreover, the difference between the coefficients for households *with* and *without* children is close to zero (0.006), precisely estimated, and statistically insignificant (p-value=0.966).

For the second falsification test, we exploit the timing of the information intervention in an event-study fashion. In column (4) of Table 2, we estimate the same baseline regression from column (1), except that it uses as a dependent variable the protest decision in a pre-treatment year (2020), rather than in the post-treatment year (2021). Intuitively, the information-provision experiment should not affect behavior in this pre-treatment year because the individuals are not yet exposed to the information. We thus expect the coefficients from this falsification exercise to be close to zero and statistically insignificant. The results from column (4) confirm our expectations: the coefficients from column (4) are close to zero (0.110 and -0.065, for both households *with* and *without* children), precisely estimated, and statistically insignificant (p-values of 0.545 and 0.504); moreover, the difference between households *with* children and *without* children is also close to zero (0.175) and statistically insignificant (p-value=0.398).

We can extend this same falsification test to other pre-treatment years for which we have readily available data. For ease of exposition, the results are presented in graphical form in Figure 2(a). The x-axis denotes the year of the dependent variable (i.e., whether the owner protests directly in years 2016 through 2021). This figure focuses on the main result,

corresponding to the difference between households *with* children versus *without* children. As expected, this difference is close to zero and statistically insignificant for each pre-treatment year (2016–2020) and then negative and statistically significant in the post-treatment year (2021).

One usual concern with 2SLS estimation concerns weak instruments (Stock et al., 2002). Given the strong belief updating documented in Section 5.2, weak instruments should not be a concern in our setting. Nevertheless, for a more rigorous assessment, Table 2 reports the Cragg-Donald F statistic, which is commonly used to diagnose weak instruments. The value of this statistic in each regression is well above the rule of thumb of $F > 10$ proposed by Stock et al. (2002): it equals 30.10, 30.22, 30.10, and 30.02, respectively, in columns (1)–(4) of Table 2.[54]

The 2SLS model used for the results in equation (21) assumes a linear relationship between school share and the probability of protesting, a natural starting point due to its simplicity and because it is common practice in the literature on information-provision experiments. To probe that assumption, Figure 2(b) presents a binned scatterplot representation of the reduced-form effects of the information-provision experiment (i.e., not accounting for how the information provision affects prior beliefs). The x-axis corresponds to the interaction between the potential information disclosure and the prior gap (i.e., the excluded instrument). The y-axis corresponds to the probability of protesting in 2021. This binned scatterplot includes all the same control variables used in the 2SLS model. Figure 2(b) seeks to assess whether the relationship between the interaction term on the horizontal axis and the protest probability on the vertical axis is linear, and the figure shows that a linear fit is a reasonable functional form assumption for this context. Additionally, this figure shows that the previously discussed regression results are not driven by outliers.

Table 3 presents additional robustness checks. Columns (1) and (2) of Table 3 reproduce the baseline specification given by columns (1) and (2) of Table 2 for reference. Columns (3) through (10) of Table 3 present the results under alternative specifications. The specification from columns (3) and (4) is identical to the specification from columns (1) and (2), except that we include some additional control variables: the respondent's age, a dummy for individuals that self-identify as White, a dummy for gender, a dummy for college degree, and a dummy for political party (which equals 1 for individuals who self-identify as Democrat).[55] The results from columns (3) and (4) are similar to the baseline results from columns (1) and (2). If anything, these coefficients are slightly larger (-0.714 vs. -0.644 and -0.744 vs. -0.678).

[54] For the results of the first-stage regressions, see Appendix B.8.

[55] These variables are measured at the end of the survey, and some respondents did not finish the full survey. Thus, the inclusion of these additional controls reduces the number of observations, which is the main reason why we exclude these variables from the baseline controls.

In columns (5) through (8) of Table 3, we try alternative definitions of outliers in prior misperceptions. The baseline specification is already conservative in that it excludes the extreme top and bottom 5% of the distributions. In columns (5) and (6), we use a less stringent definition of outliers based on the upper and bottom 2.5% instead of 5%. The results from columns (5) and (6) are similar to those from the baseline specification of columns (1) and (2), although the coefficients are slightly smaller. In columns (7) and (8), we consider an even more lax definition of outliers, excluding only the upper and bottom 1% of misperceptions. The coefficients from columns (7) and (8) remain consistent with those from the baseline specification of columns (1) and (2), although again the magnitudes are somewhat smaller. These results are consistent with the arguments in Section 4.3 that we should be cautious when including extreme misperceptions because they probably reflect a lack of attention or mistakes, rather than legitimate misperceptions. To explore this further, columns (9) and (10) are identical to the baseline specification from columns (1) and (2), except they exclude respondents who do not pass the attention check included at the end of the survey. Consistent with the attention argument, when we focus on subjects who pass the attention check, the coefficients increase somewhat.

5.6 Comparison to Expert Predictions

Next, we compare our experimental results to expert predictions, as shown in Figure 3. Panel (a) presents the predictions of experts for households *with* children, and panel (b) presents the predictions for households *without* children. The histograms correspond to the distribution of expert predictions estimating the effect of a 1-pp increase in the school share.[56] The solid vertical red line in each panel represents the corresponding estimate from the baseline 2SLS model (column (1) of Table 2), and the red shading denotes the corresponding confidence intervals.

Figure 3 shows that our experimental findings are not obvious to the sample of experts. Our experimental results are consistent with experts who predicted that the school share belief would have a negative effect on the protest rate for households *with* children (panel (a)) and a positive effect for households *without* children (panel (b)). They also are consistent with the mean of the experimental estimates in these two panels. However, the forecasts of most experts are inconsistent with the experimental results: most forecasts indicate effects that would be zero or that have the opposite sign compared to the experimental findings. Only a few expert predictions are close to the experimental estimates, even if we account for

[56] To make the elicitation easier, in the prediction survey, we ask subjects to predict the effects of a 10-pp increase in the school share. In Figure 3, we divide those predictions by 10 to obtain the effect per 1 pp, so that it can be compared directly to the 2SLS estimates.

the sampling variation in the experimental estimates. More precisely, for households *with* children, only 41.1% of predictions are within the 90% confidence interval of the experimental estimate. For households *without* children, just 17.9% of predictions are within the 90% confidence interval of the experimental estimate.

At the end of the survey, we ask the experts to express how confident they feel about their forecasts. One notable finding is that experts do not feel confident about their predictions: on a scale of 1 to 5, where 1 is "not confident at all" and 5 is "extremely confident", the average confidence is 2.07.[57] In any case, we find that the comparison between the forecasts and experimental estimates is similar if we weight the forecasts by the confidence of the experts (results reported in Appendix B.9).

5.7 Non-Experimental Evidence

We complement this experimental evidence with some non-experimental evidence by including a survey question asking individuals to choose between hypothetical tax policies, in the spirit of Weinzierl (2014) and Saez and Stantcheva (2016). More specifically, we include a question about public school taxes. We present the respondent with a hypothetical situation in which two households (A and B) own homes worth $200,000 each. Both households are identical except that household A has two children enrolled in the public school district and household B has no children enrolled in the public school district. The respondent has to levy a total tax of $8,000, which can be spread across the two households in any way (e.g., assign all the burden to household A, all the burden to household B, or anything in the middle). According to the hypothesis of benefit-based taxation, respondents will want the household *with* children to pay more in taxes than the household *without* children, because the former benefits more from that government service. We find that most (58.8%) of the respondents behave according to the theory of benefit-based taxation, that is, they assign a higher tax burden to the household *with* children even though both homes are worth the same.[58] This evidence suggests that the logic of benefit-based taxation resonates with most taxpayers.

A feature of property tax policy in the state of Texas is suggestive of benefit-based reasoning. Texas homeowners who are older than 65, most of whom do not have school-aged children, qualify for an exemption that limits their school taxes to the amount paid in the year that the owner turned 65, regardless of future increases in the home's proposed value (Texas Comptroller, 2021).[59] This exemption policy for households unlikely to have children

[57] More precisely, 25.0% of experts selected "not confident at all," 51.8% selected "slightly confident," 19.6% selected "somewhat confident," 3.57% selected "very confident", and 0% selected "extremely confident."

[58] For detailed results, see Appendix B.10.

[59] The tax amount paid can increase if property improvements are made beyond maintenance and repairs. Homeowners also must apply to receive this benefit.

is consistent with benefit-based reasoning.

6 Perceptions about Recapture

6.1 Accuracy of Prior Beliefs

Unlike the information on the school share, the information on recapture is not readily available in the Notice of Appraised Value from the DCAD. However, households may be informed about the recapture system through its media coverage. Homeowners also may deduce that recapture redistributes from more to less advantages districts; thus, knowing whether one lives in a more or less advantaged district may be enough to form a decent guess about their share of school taxes that are recaptured.

Figure 4(a) shows a histogram of the degree of misperceptions about the recapture share. The x-axis corresponds to the difference between the actual recapture share versus that perceived by respondents. A minority of subjects have accurate perceptions: around 20% of subjects guess the recapture share to be within $\pm$ 5 pp of the actual share. Misperceptions are significant in magnitude: the mean absolute error is 11.36 pp. However, the mean absolute error for the recapture share (11.36 pp) is somewhat less pronounced than that of the school share (16.57). The fact that misperceptions for the recapture share are somewhat smaller than those for the school share implies that there is less scope for the information provision experiment to update beliefs and thus less statistical power for the 2SLS estimates.

Unlike misperceptions about the school share, misperceptions about the recapture share have no systematic bias. Figure 4(a) shows roughly as many observations in the left half of the histogram (corresponding to overestimation) as in the right half (corresponding to underestimation). On average, subjects overestimate the recapture share by just 0.28 pp, as indicated by the mean error.

6.2 Belief Updating

Next, we summarize how subjects update their beliefs in reaction to the information provision about the recapture share. Figure 4(b) shows the results as a binned scatterplot. The x-axis corresponds to the gaps in prior beliefs, and the y-axis denotes the belief updating. As in the figure we used to show belief updating for the school share, the x-axis in Figure 4(b) shows the maximum revision we would expect if the respondents were to fully react to the provided information, and the y-axis shows the revision observed in practice. The red circles from Figure 4(b) correspond to subjects who are shown the feedback about the recapture share. Consistent with significant learning, there is a strong relationship between the belief

revisions and prior gaps: an additional percentage point (pp) in perception gap is associated with a revision that is 0.632 pp higher. The gray squares from Figure 4(b) correspond to the subjects who do not receive information about the school share. In turn, the gray squares indicate a statistically significant (p-value<0.001) but economically small (0.099) degree of spurious revision. Most importantly, the degree of true learning corresponds to the difference in slopes between subjects who are shown the feedback and subjects who are not shown the feedback. This difference is large ($0.533 = 0.632 - 0.099$) and highly statistically significant (p-value<0.001). This difference suggests that a 1-pp information shock induces a 0.533 effect in posterior beliefs. Though large, this rate of information pass-through (0.533) is smaller than the corresponding rate for the school share (0.757).

The weaker effect of information shocks about recapture share, relative to school share, will result in less exogenous variation in posterior beliefs and thus less precisely estimated 2SLS coefficients. Many reasons help explain the weakly updated beliefs about recapture. For example, respondents may feel more confident in their prior beliefs about recapture, or they may trust the feedback on recapture less. Indeed, the recapture estimates are based on a number of assumptions, so subjects may naturally find the recapture feedback less persuasive. Last, subjects may pay less attention to the recapture feedback due to survey fatigue, as this information appears later in the survey.

6.3 2SLS Estimates

Let r_i^{post} be the posterior belief about the funds recaptured from individual i's own school district, in percentage points. Positive values indicate that individual i's district is a net contributor to the recapture system; in other words, $r_i^{post} = 40$ means that 40% of school taxes from household i's district are redistributed to disadvantaged school districts. Negative values, on the contrary, represent situations where individual i's school district benefits from recapture: $r_i^{post} = -30$ means that the school district receives an additional 30% over the amount of its own school taxes from taxpayers in other school districts.[60] We use the following econometric specification:

$$P_i^{2021} = \beta_0 + \beta_C^R \cdot C_i \cdot r_i^{post} + \beta_{NC}^R \cdot (1 - C_i) \cdot r_i^{post} + \beta_1 \cdot C_i + \epsilon_i \tag{22}$$

The two parameters of interest are β_C^R and β_{NC}^R for households *with* and *without* children, respectively. The benefit-based framework from Section 3 predicts that $\beta_C^R > 0$ and $\beta_{NC}^S = 0$. Again, the difference between these two parameters, $\beta_C^R - \beta_{NC}^R$, captures the benefit-based

[60] The negative values can be lower than -100 because an ISD can receive more than 100% of the amount of its own school taxes in redistributed tax.

motivation behind public schools. As in the estimation of the change in perceptions about the school share, we estimate equation (22) using 2SLS to exploit the variation in r_i^{post} induced exogenously by the information provision experiment. As mentioned in Section 5.3, we estimate the effects of school share and recapture share jointly in the same 2SLS regression.

The 2SLS estimates for the recapture share are presented at the bottom panel of Table 2. In column (1) of Table 2, the dependent variable indicates if the subject protests directly in 2021. The coefficient for households *with* children is positive but close to zero (0.076) and statistically insignificant (p-value=0.875). For households *without* children, the coefficient is also positive (0.498) and large but borderline statistically insignificant (p-value=0.101). The difference between the coefficients for households with versus without children (0.076 and 0.498) is large but statistically insignificant (p-value=0.454).

Perhaps Information about recapture share affects the intention to protest, but these effects do not materialize into behavior changes. To assess that possibility, column (2) of Table 2 is identical to column (1), except that it uses as a dependent variable the intention to protest instead of whether the household actually files a protest. As in column (1), the estimates from column (2) are all statistically insignificant. That is, the information on share recapture does not affect the intention to protest, even in the short term. The only coefficient from column (1) that is borderline significant (for households *without* children, with a p-value=0.101), is not even close to being statistically significant in column (2), and furthermore has the opposite sign.

One important caveat to keep in mind, however, is that the coefficients for the recapture share are less precisely estimated. For example, the standard errors of the coefficients for recapture share are more than twice the size of the corresponding standard errors for school share. As a result, the effects for recapture share should be more than twice as high as the effects of school share (which are already quite sizable) to have enough power to detect statistically significant effects. The less precise estimation for the coefficients for recapture share occurs for two reasons, both of which are difficult to anticipate ex-ante in the experimental design. First, as explained in Section 6.1, the misperceptions about recapture share were smaller (mean absolute difference of 11.36 pp) than those about school share (mean absolute difference of 16.57 pp). Second, as documented in Section 6.2, conditional on a level of misperceptions, subjects updated their beliefs more strongly in response to the feedback about school share than in response to the feedback about recapture share.

To assess the robustness of the (null) effect of recapture share on protest behavior, we proceed with two falsification tests. Column (3) of Table 2 uses the dependent variable that indicates whether the household ever protested through an agent. As expected, the coefficients are statistically insignificant (p-values of 0.249 and 0.359 for households *with* and

without children, respectively); the difference between the two coefficients (-0.207) is also statistically insignificant (p-value=0.486). For the second falsification test, column (4) of Table 2 uses the protest decision in a pre-treatment year (2020) as a dependent variable. As expected, the coefficients from column (4) (0.164 and -0.039 for households *with* and *without* children, respectively) are both statistically insignificant (p-values of 0.694 and 0.867); the difference between the two (0.203) is also statistically insignificant (p-value=0.664). We find similar results if we expand this falsification test to other pre-treatment years. Using binned scatterplots, we show that the (null) effects are not driven by non-linearities or outliers (results presented in Appendix B.8).

Table 3 presents additional robustness checks. Columns (1) and (2) of Table 3 reproduce the baseline specification given by columns (1) and (2) of Table 2 for reference, and columns (3) through (10) of Table 3 present the results under alternative specifications. In columns (3) and (4), we include additional control variables. In columns (5) through (8), we employ less stringent definitions of outliers. In columns (9) and (10), we exclude respondents who did not pass the attention check. The baseline results from columns (1) and (2) are consistent with the results from all the alternative specifications from columns (3) through (10).

We can also compare our 2SLS estimates to the expert predictions. However, as the experimental estimates are not estimated with sufficient precision, the results are not very informative (results presented in Appendix B.9).

In sum, we do not find any statistically significant evidence that perceptions about recapture affect the probability of filing a tax appeal. Although the null effects seem to conflict with predictions of the benefit-based framework, they must be considered in the context of limited statistical power. With those caveats in mind, one potential interpretation for the null effects would be respondents' altruism, as exemplified in the model extension presented in Appendix A. Intuitively, households may be willing to sacrifice government services if they believe those tax dollars will help the most disadvantaged communities.[61]

7 Conclusions

We present evidence that perceptions about where tax dollars go can have a significant effect on the willingness to pay those taxes. We conduct a high-stakes, natural field experiment with 2,110 homeowners who pay property taxes and have the opportunity to appeal their property tax assessment. We find that even though accurate information is publicly available and easily accessible, the homeowners have large misperceptions about how tax dollars are

[61] This interpretation is consistent with non-experimental evidence presented in Appendix B.10. Using a hypothetical question, we find that most households demand redistribution of school taxes across districts.

spent. Specifically, they do not know how much of their property taxes correspond to school taxes, and they misperceive the fraction of school taxes that are redistributed to school districts other than their own. The results from the field experiment are consistent with a basic prediction of benefit-based taxation. On the one hand, after learning that a higher share of property taxes funds public schools, households *with* children enrolled in public schools become less likely to appeal their property taxes. On the other hand, upon learning the same thing, households *without* children become more likely to appeal their property taxes. Beliefs about the school share have significant effects on the decision to file a protest, but beliefs about the recapture share do not have a significant effect, although this result must be considered in the context of its limited statistical power.

We implement our field experiment in Dallas Country, the second-largest county in Texas. Property taxes work almost identically in other counties in Texas and similarly across the country (Nathan et al., 2020), implying that our results are generalizable to other U.S. counties . Moreover, conducting the same field experiment in other U.S. counties would be straightforward. For instance, property taxes provide a significant source of school funding in most of the U.S. (Chen, 2021), and other states also redistribute property taxes across school districts similar to Texas' recapture system (Youngman, 2016). Finally, we provide detailed accounts of the implementation and data sources that other researchers can follow, and we are happy to share data, code, tips, and additional resources.

Though the main goal of this study is to provide causal evidence on the roots of tax compliance, our results also have some direct implications for tax and fiscal policies. First, our evidence suggests that governments may be able to design policies to increase their tax capacity. For example, governments may leverage transparency and accountability to improve perceptions about how tax dollars benefit taxpayers, which could boost tax compliance and increase taxpayer support of new taxes. We find evidence of large misperceptions about government spending, even when such information is publicly available. Governments thus may need to make an extra effort beyond just posting information on a website to reach constituents and educate them regarding where their tax dollars go. Relatedly, governments may want to simplify the connection between the taxes they collect and the government services provided, such as introducing new taxes that are earmarked for specific services. Local governments tend to do this well in that they typically break down property taxes into a school tax, a hospital tax, and so on. Even in the simple context of property taxes, however, we still find that taxpayers have large misperceptions about where their tax dollars go. In the case of state and federal governments, for which tax dollars follow a complicated path on their way to becoming public services, there is probably much room for improvement.

Our evidence suggests that normative considerations, such as benefit-based taxation, can

have significant implications in practice. Modern tax theory largely disregards these types of normative considerations to focus on efficiency (Weinzierl, 2018).[62] However, taxpayers and politicians do not think like economists. The general public cares about efficiency, but they also care a lot about normative considerations, such as fairness. Taking these normative considerations into account can be helpful when designing efficient tax policies in a way that makes them more palatable to the general public. These normative considerations also may help clarify some puzzling observations indicating that some policies are chosen even though they are inefficient (Stantcheva, 2020).

References

Ajzenman, N. and R. Durante (2022). Salience and Accountability: School Infrastructure And Last-Minute Electoral Punishment. *Economic Journal, forthcoming*.

Alesina, A. and P. Giuliano (2011). Preferences for redistribution. In *Handbook of Social Economics*, Volume 1, pp. 93–131. Elsevier.

Athey, S. and G. W. Imbens (2017). The econometrics of randomized experiments. In *Handbook of economic field experiments, Vol. 1*, pp. 73–140.

Bergolo, M., M. Leites, R. Perez-Truglia, and M. Strehl (2020). What Makes a Tax Evader? *NBER Working Paper No. 28235*.

Bergolo, M. L., R. Ceni, G. Cruces, M. Giaccobasso, and R. Perez-Truglia (2021). Tax Audits as Scarecrows: Evidence from a Large-Scale Field Experiment. *American Economic Journal: Economic Policy, forthcoming*.

Blumenthal, M., C. Christian, and J. Slemrod (2001). Do Normative Appeals Affect Tax Compliance? Evidence From a Controlled Experiment in Minnesota. *National Tax Journal 54*(1), 125–138.

Bott, K. M., A. W. Cappelen, E. Ã. Sørensen, and B. Tungodden (2020). You've Got Mail: A Randomized Field Experiment on Tax Evasion. *Management Science 66*(7), 2801–2819.

Bottan, N. and R. Perez-Truglia (2020a). Betting on the House: Subjective Expectations and Market Choices. *NBER Working Paper No. 27412*.

Bottan, N. L. and R. Perez-Truglia (2020b). Choosing Your Pond: Location Choices and Relative Income. *The Review of Economics and Statistics, forthcoming*.

Cait, L., N. Jan-Emmanuel De, and Michael I. Norton (2018). The Power of Voice in Stimulating Morality: Eliciting Taxpayer Preferences Increases Tax Compliance. *Special Issue: Marketplace Morality*, 310–328.

[62] Tax theorists have only recently explicitly integrated redistributive utilitarianism considerations into their models (Mankiw and Weinzierl, 2010; Scherf and Weinzierl, 2020).

Carrillo, P. E., E. Castro, and C. Scartascini (2021). Public good provision and property tax compliance: Evidence from a natural experiment. *Journal of Public Economics 198*, 104422.

Castro, L. and C. Scartascini (2015). Tax Compliance and Enforcement in the Pampas Evidence From a Field Experiment. *Journal of Economic Behavior & Organization 116*, 65–82.

Cavallo, A., G. Cruces, and R. Perez-Truglia (2017). Inflation expectations, learning, and supermarket prices: Evidence from survey experiments. *American Economic Journal: Macroeconomics 9*(3), 1–35.

Chen, G. (2021). An Overview of the Funding of Public Schools. *https://www.publicschoolreview.com/blog/an-overview-of-the-funding-of-public-schools*.

Cruces, G., R. Perez-Truglia, and M. Tetaz (2013). Biased perceptions of income distribution and preferences for redistribution: Evidence from a survey experiment. *Journal of Public Economics 98*, 100–112.

Cullen, J., N. Turner, and E. Washington (2020). Political alignment, attitudes toward government and tax evasion. *American Economic Journal: Economic Policy, forthcoming*.

Cullen, Z. and R. Perez-Truglia (2021). How Much Does Your Boss Make? The Effects of Salary Comparisons. *Journal of Political Economy, forthcoming*.

Dallas Morning News (2018). Do your schools get your property tax dollars? *July 4, 2018*.

DellaVigna, S., D. Pope, and E. Vivalt (2019). Predict science to improve science. *Science 366*(6464), 428–429.

Dobay, N., F. Nicely, A. Sanderson, and P. Sanderson (2019). The best (and worst) of international property tax administration. Technical report, Council On State Taxation.

Fuster, A., R. Perez-Truglia, M. Wiederholt, and B. Zafar (2021). Expectations with Endogenous Information Acquisition: An Experimental Investigation. *Review of Economics and Statistics, forthcoming*.

Hoff, P. D. (2009). *A first course in Bayesian statistical methods*. Springer Science & Business Media.

Huet-Vaughn, E. (2019, 2). Stimulating the Vote: ARRA Road Spending and Vote Share. *American Economic Journal: Economic Policy 11*(1), 292–316.

Huet-Vaughn, E., A. Robbett, and M. Spitzer (2019). A taste for taxes: Minimizing distortions using political preferences. *Journal of Public Economics 180*, 104055.

Imbens, G. W. and J. D. Angrist (1994). Identification and Estimation of Local Average Treatment Effects. *Econometrica 62*(2), 467–475.

Jones, P. (2019). Loss Aversion and Property Tax Avoidance. *Working Paper*.

Lockwood, B. B. and M. Weinzierl (2016). Positive and normative judgments implicit in U.S. tax policy, and the costs of unequal growth and recessions. *Journal of Monetary Economics 77*, 30–47.

Luttmer, E. F. P. and M. Singhal (2014). Tax Morale. *Journal of Economic Perspectives 28*(4), 149–168.

Mankiw, N. G. and M. Weinzierl (2010). The optimal taxation of height: A case study of utilitarian income redistribution. *American Economic Journal: Economic Policy 2*(1), 155–176.

McKenzie, D. (2012). Beyond baseline and follow-up: The case for more T in experiments. *Journal of Development Economics 99*(2), 210–221.

Musgrave, R. (1959). The Theory of Public Finance. *McGraw-Hill*.

Nathan, B., R. Perez-Truglia, and A. Zentner (2020). My Taxes are Too Darn High: Why Do Households Protest their Taxes? *NBER Working Paper No. 27816*.

Parker, W. and N. Friedman (2021). Zillow Quits Home-Flipping Business, Cities Inability to Forecast Prices. *The Wall Street Journal, November 2 2021*.

Saez, E. and S. Stantcheva (2016). Generalized social marginal welfare weights for optimal tax theory. *American Economic Review 106*(1), 24–45.

Scherf, R. and M. Weinzierl (2020). Understanding Different Approaches to Benefit-Based Taxation. *Fiscal Studies 41*(2), 385–410.

Seligman, E. R. A. (1908). Progressive Taxation in Theory and Practice. Technical Report 4.

Sinclair, M., J. O'Toole, M. Malawaraarachchi, and K. Leder (2012). Comparison of response rates and cost-effectiveness for a community-based survey: postal, internet and telephone modes with generic or personalised recruitment approaches. *BMC Medical Research Methodology 12*(1), 132.

Slemrod, J. (2019). Tax Compliance and Enforcement. *Journal of Economic Literature 57*(4), 904–954.

Stantcheva, S. (2020). Understanding Tax Policy: How Do People Reason? *NBER Working Paper No. 27699*.

Stock, J. H., J. H. Wright, and M. Yogo (2002). A survey of weak instruments and weak identification in generalized method of moments. *Journal of Business and Economic Statistics 20*(4), 518–529.

Tax Policy Center (2021a). Amount of Revenue by Source. *https://www.taxpolicycenter.org/statistics/property-tax-revenue*.

Tax Policy Center (2021b). Amount of Revenue by Source. *https://www.taxpolicycenter.org/statistics/amount-revenue-source*.

Texas Comptroller (2021). Frequently Asked Questions. *https://comptroller.texas.gov/taxes/property-tax/exemptions/age65older-disabled-faq.php*.

Texas Education Agency (2021a). Excess Local Revenue. *https://tea.texas.gov/finance-and-grants/state-funding/excess-local-revenue*.

Texas Education Agency (2021b). Texas Public School Finance Overview: Biennium 2020-2021.

Texas Education Agency (2021c). What is House Bill 3? *https://tea.texas.gov/about-*

tea/government-relations-and-legal/government-relations/house-bill-3.

U.S. Census Bureau (2021). Population, Dallas County, Texas. *https://www.census.gov/quickfacts/fact/table/dallascountytexas/POP010220*.

Villanueva, C. (2018). What is Recapture? *Center for Public Policy Priorities Report, August 30, 2018*.

Weinzierl, M. (2014). The promise of positive optimal taxation: normative diversity and a role for equal sacrifice. *Journal of Public Economics 118*, 128–142.

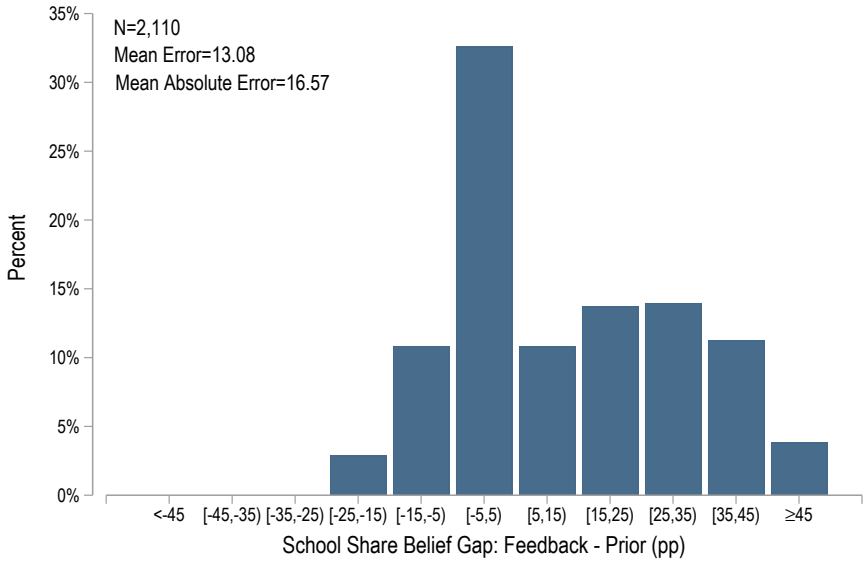
Weinzierl, M. (2018). Revisiting the Classical View of Benefit-based Taxation. *The Economic Journal 128*(612), F37–F64.

World Bank (2019). The Administrative Review Process for Tax Disputes: Tax Objections and Appeals in Latin America and the Caribbean.

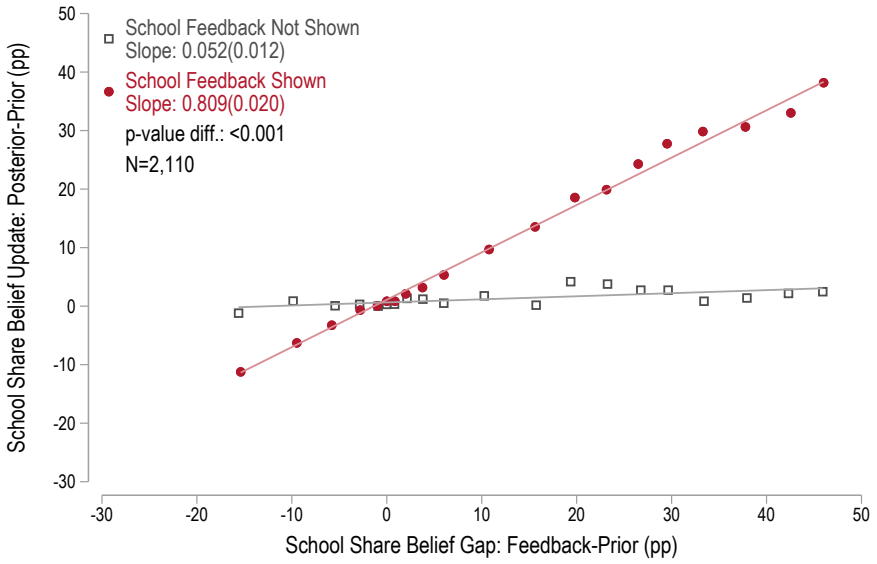
Youngman, J. (2016). *A Good Tax*. New York: Columbia University Press.

Figure 1: Perceptions about the Share of Property Taxes going to Public Schools

(a) Gap in Prior Beliefs



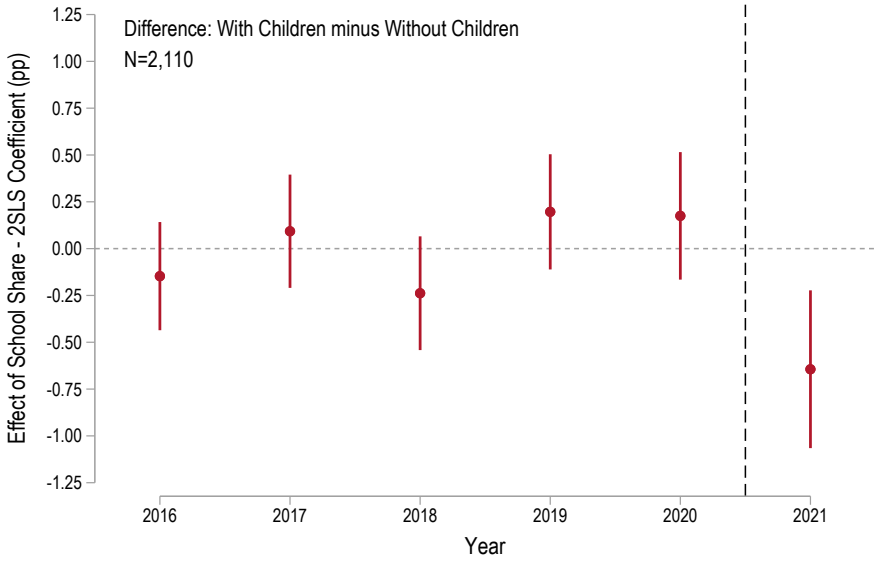
(b) Belief Updating



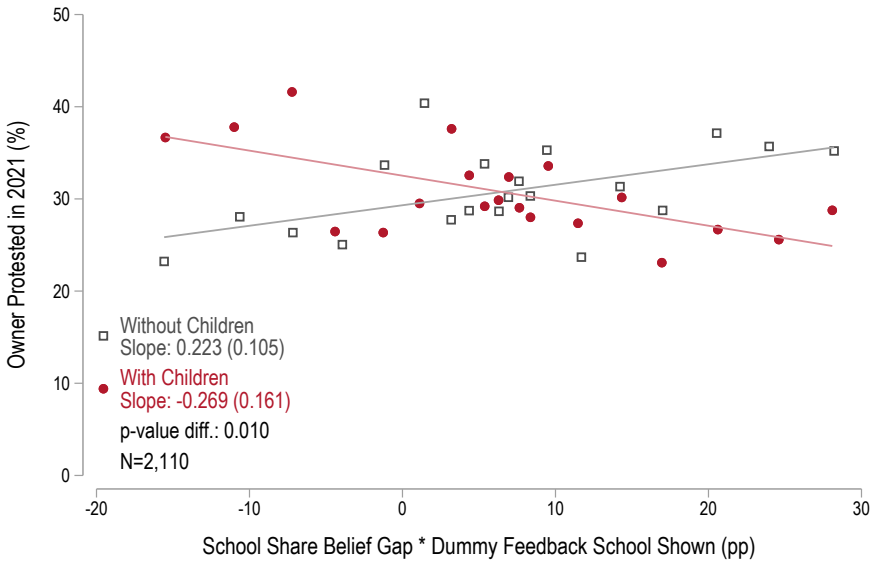
Notes: Panel (a) shows the gap in prior beliefs. The x-axis reports the difference between the actual school share and respondents' prior beliefs about the school share in 10pp width bins. The y-axis reports the percentage of survey respondents in each bin. The upper left corner reports the total number of observations, the average error, and the average absolute error. Panel (b) shows how respondents update their beliefs using a binned scatterplot (using 20 equally sized bins). The x-axis reports the difference between the actual school share and respondents' prior beliefs about the school share. The y-axis reports the difference between posterior and prior beliefs (i.e., belief updating). Red circles (gray squares) represent the average update within each bin for the group of homeowners that were selected (were not selected) into the school share treatment. Each line corresponds to the fitted values from separate OLS regressions where the dependent variable is the actual update and the independent variable is the school share belief gap. The coefficients associated to the gap variable are reported in the upper left corner, as well as their robust standard errors (in parentheses), the p-value of the difference in the slopes, and the number of observations included in the analysis.

Figure 2: The Effects of School Share on Protests: Additional Robustness Checks

(a) Event-Study Analysis



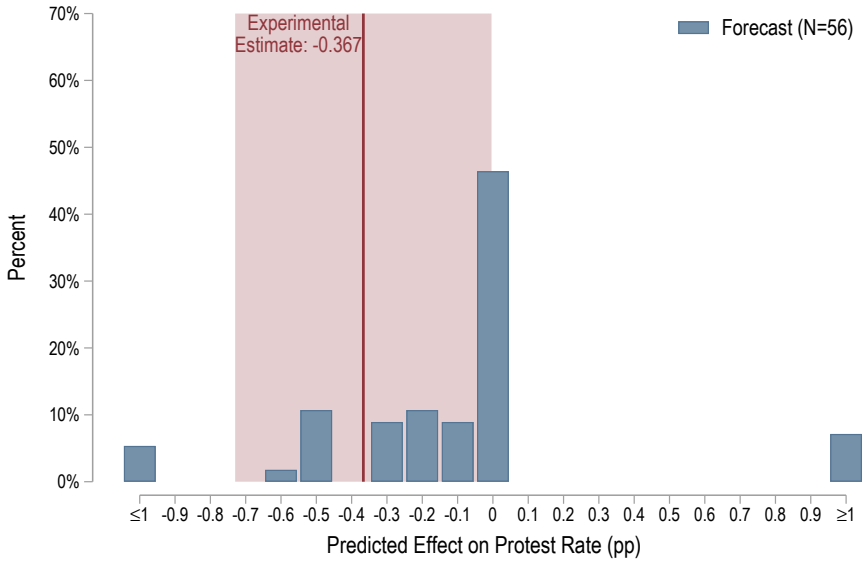
(b) Binned Scatterplot (Reduced Form)



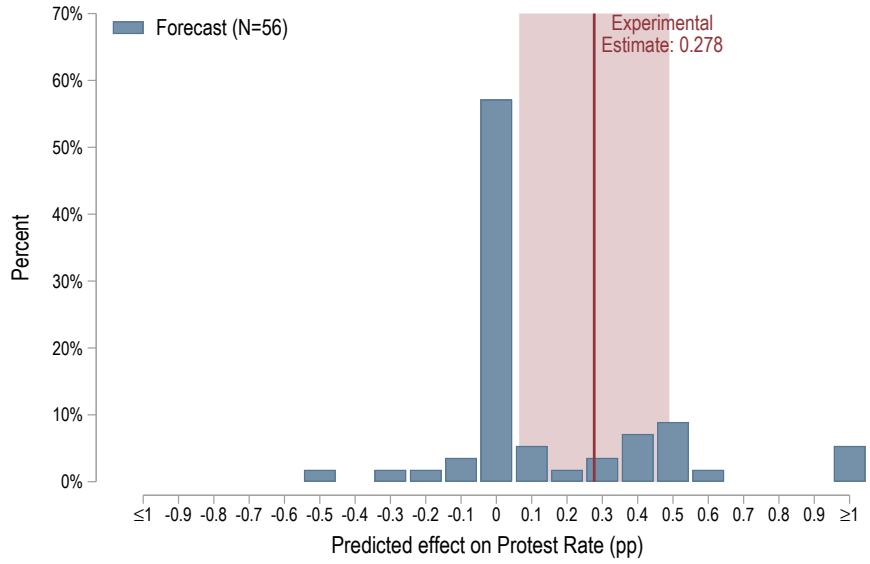
Notes: Panel (a) reports an event-study analysis of the differential effect of the school share belief on the protest probability for households *with* children versus *without* children. The estimates plotted in this figure correspond to the 2SLS point estimate based on equation (21), with 90% confidence intervals based on robust standard errors. The coefficient plotted for 2021 is the coefficient reported in the "difference" row of panel (a), column (1) of Table 2. The remaining coefficients come from similar regressions but using the outcomes in pre-treatment years as falsification tests and restricting the pre-treatment controls to the relevant years. The vertical dashed line separates the post-treatment year (2021) from the pre-treatment years (2016-2020). Panel (b) depicts a 20 equally-sized binned scatterplot representation of the reduced-form effect for households *with* and *without* children separately, using red circles and gray squares respectively. The x-axis corresponds to the interaction between the prior school share belief gap (defined as the difference between the actual school share and the prior belief about the school share) and a dummy variable that indicates if the homeowner was selected into the school share treatment group. The y-axis corresponds to the probability of a homeowner protesting directly in 2021. Each line corresponds to a separate OLS binned scatterplot regression, including the same control variables used in the 2SLS specification. The coefficients reported in the lower left corner and their (robust) standard errors are based on a unique regression that interacts the key variables with a dummy for having children at school (for the results in table form, see Table B.6). In addition we report the p-value of the difference in the effect for the two groups and the number of observations used in the estimation.

Figure 3: The Effects of School Share on Protests: Comparison to Expert Predictions

(a) With Children in Public School



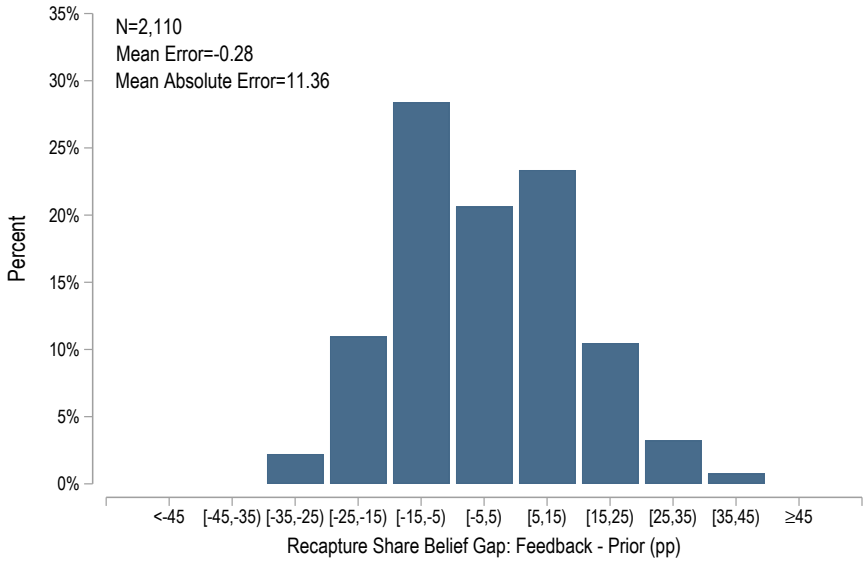
(b) Without Children in Public School



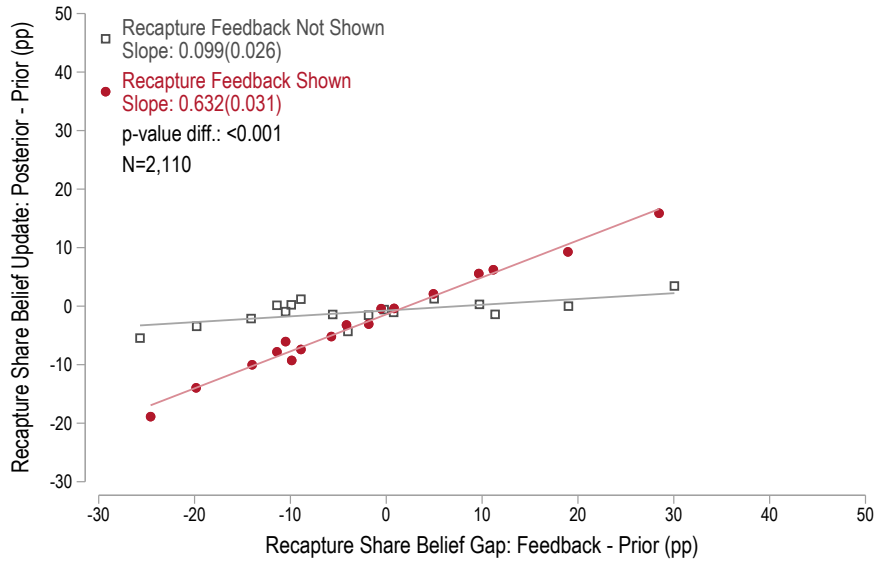
Notes: This figure shows the distribution of expert predictions about the effects of a 1 pp increase in school share beliefs on the probability that a homeowner fills a protest directly for households *with* children enrolled in the public school district (panel (a)) and households *without* children enrolled in the public school district (panel (b)), based on the data collected in the forecast survey. To make the elicitation easier, in the prediction survey we asked subjects to predict the effects of a 10 pp increase in beliefs about school share. For this figure, we divide those predictions by 10 and we obtained the effect per 1 pp to be able to compare these coefficients directly to the 2SLS estimates. In both panels, we pooled responses that were greater than 1 in absolute value into the extreme bins. The vertical red solid line corresponds to the experimental estimate based on the 2SLS specification reported in Table 2, panels (a) and (b). The pink shaded area corresponds to the 90% confidence interval. In the upper right (left) corner of each figure we report the number of answers obtained in the forecast survey that were used for this figure. The entire questionnaire for the prediction survey can be found in Appendix I.

Figure 4: Perceptions about the Share of School Taxes Affected by Recapture

(a) Gap in Prior Beliefs



(b) Belief Updating



Notes: Panel (a) shows the gap in prior beliefs about the recapture share. The x-axis reports the difference between the actual recapture share and respondents' prior beliefs about the recapture share in 10pp width bins. The y-axis reports the percentage of survey respondents in each bin. The upper left corner reports the total number of observations, the average error, and the average absolute error. Panel (b) shows how respondents update their beliefs using a binned scatterplot (using 20 equally sized bins). The x-axis reports the difference between the actual recapture share and respondents' prior beliefs about the recapture share. The y-axis reports the difference between posterior and prior beliefs (i.e., belief updating). Red circles (gray squares) represent the average update within each bin for the group of homeowners that were selected (were not selected) into the recapture share treatment. Each line corresponds to the fitted values from separate OLS regressions where the dependent variable is the actual update and the independent variable is the recapture share belief gap. The coefficients associated to the gap variable are reported in the upper left corner, as well as their robust standard errors (in parentheses), the p-value of the difference in the slopes, and the number of observations included in the analysis.

Table 1: Balance of Homeowners Characteristics across Treatment Groups

		Treatment Arm				
	All	No Feedback	Recapture Feedback	School Feedback	Both Feedback	p-value test
	(1)	(2)	(3)	(4)	(5)	(6)
a. Admin. Records Variables:						
2021 Home Value ($1,000)	349.988	365.355	330.631	365.198	340.088	0.163
	(6.774)	(14.907)	(10.302)	(16.461)	(12.037)	
2021 Property Tax Amount ($1,000s)	7.738	8.018	7.448	7.960	7.546	0.292
	(0.129)	(0.296)	(0.218)	(0.287)	(0.228)	
School Share (%)	50.726	50.603	50.566	50.701	51.029	0.140
	(0.079)	(0.155)	(0.160)	(0.155)	(0.158)	
Recapture Share (%)	1.622	1.852	1.054	2.505	1.130	0.351
	(0.325)	(0.678)	(0.633)	(0.672)	(0.622)	
2020 Owner Protested (%)	18.057	23.121	14.815	19.883	14.684	0.000
	(0.838)	(1.852)	(1.530)	(1.764)	(1.527)	
2020 Agent Protested (%)	1.659	1.156	2.407	1.754	1.301	0.375
	(0.278)	(0.470)	(0.660)	(0.580)	(0.489)	
2019 Owner Protested (%)	13.365	15.029	10.926	14.035	13.569	0.238
	(0.741)	(1.570)	(1.344)	(1.535)	(1.478)	
2018 Owner Protested (%)	13.460	13.680	12.407	14.815	13.011	0.697
	(0.743)	(1.510)	(1.420)	(1.570)	(1.452)	
2017 Owner Protested (%)	10.853	11.561	11.111	11.891	8.922	0.400
	(0.677)	(1.405)	(1.354)	(1.430)	(1.230)	
2016 Owner Protested (%)	7.773	8.478	6.667	8.187	7.807	0.705
	(0.583)	(1.224)	(1.074)	(1.212)	(1.158)	
Multiple Owners (%)	24.645	22.929	24.444	25.146	26.022	0.693
	(0.938)	(1.847)	(1.851)	(1.917)	(1.893)	
Living Area (1,000s Sq. Feet)	2.313	2.317	2.302	2.331	2.302	0.959
	(0.022)	(0.046)	(0.042)	(0.046)	(0.040)	
Number of Bedrooms	3.428	3.432	3.398	3.423	3.459	0.609
	(0.016)	(0.032)	(0.033)	(0.034)	(0.031)	
Number of Baths	2.273	2.274	2.272	2.292	2.253	0.883
	(0.017)	(0.034)	(0.033)	(0.039)	(0.032)	
b. Survey Variables:						
With Children (%)	25.498	24.470	25.370	26.316	25.836	0.918
	(0.949)	(1.889)	(1.874)	(1.946)	(1.889)	
Female (%)	42.898	44.922	43.774	40.990	41.887	0.574
	(1.086)	(2.200)	(2.157)	(2.191)	(2.145)	
Age	49.608	49.711	49.381	50.438	48.945	0.146
	(0.234)	(0.470)	(0.481)	(0.461)	(0.460)	
Race: White (%)	44.300	44.727	47.818	44.422	40.265	0.103
	(1.092)	(2.200)	(2.178)	(2.220)	(2.134)	
Education: Grad. Degree (%)	38.309	39.844	37.761	38.446	37.240	0.841
	(1.069)	(2.166)	(2.114)	(2.173)	(2.104)	
Prior Belief: School Share (%)	37.642	37.741	37.186	37.935	37.726	0.918
	(0.394)	(0.804)	(0.760)	(0.790)	(0.800)	
Prior Belief: Recapture Share (%)	1.910	1.799	1.372	2.945	1.570	0.216
	(0.287)	(0.632)	(0.505)	(0.593)	(0.564)	
Observations	2,110	519	540	513	538	

Notes: This table lists pre-treatment characteristics averages. Statistics are based on the 2,110 homeowners that comprise the subject pool. Standard errors are reported in parentheses. The statistics in panel (a) are based on administrative records available at the DCAD website. The statistics in panel (b) are based on survey responses. Column (1) is based on the entire subject pool. Column (2) is based on homeowners not selected to receive any information. Column (3) is based on homeowners selected to receive information on the recapture share. Column (4) is based on homeowners selected to receive information on the school share. Column (5) is based on homeowners selected to receive information on both the recapture share and the school share. Column (6) reports the p-value of a test of equal means across the four treatment groups.

Table 2: Main Regression

			Falsification Tests	
	P_D^{2021}	I^{2021}	P_A^{2021}	P_D^{2020}
	(1)	(2)	(3)	(4)
a. Effects of School Share:				
With Children	-0.367*	-0.408*	-0.028	0.110
	(0.221)	(0.234)	(0.118)	(0.181)
Without Children	0.277**	0.269*	-0.033	-0.065
	(0.129)	(0.144)	(0.051)	(0.097)
Difference (Children - No Children)	-0.644**	-0.678**	0.006	0.175
	(0.256)	(0.275)	(0.129)	(0.207)
b. Effects of Recapture Share:				
With Children	0.076	-0.313	-0.321	0.164
	(0.485)	(0.541)	(0.278)	(0.417)
Without Children	0.498	-0.101	-0.114	-0.039
	(0.303)	(0.325)	(0.124)	(0.234)
Difference (Children - No Children)	-0.422	-0.212	-0.207	0.203
	(0.563)	(0.620)	(0.297)	(0.468)
Cragg-Donald F-Statistic	30.10	30.22	30.10	30.02
Mean Outcome (Baseline):				
With Children	33.86	47.20	7.09	25.98
Without Children	28.83	44.87	4.08	22.19
Observations	2,110	2,090	2,110	2,110

Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. This table reports 2SLS estimates of equation (21) discussed in Section 5.3. Panel (a) reports the estimates corresponding to the school share treatment effect. We present the coefficients for households *with* children and households *without children* separately, as well as the difference between these two types of households. Panel (b) reports analogous results but for the recapture share treatment effects. The dependent variable in column (1) is an indicator variable that takes the value 100 if the subject protested directly in 2021. The dependent variable in column (2) is an indicator variable that takes the value 100 if the subject answered "very likely" to the question on the likelihood to protest in 2021. Columns (3) and (4) report the results of falsification tests. The dependent variable in column (3) is an indicator variable that takes the value 100 if the subject used an agent to protest in 2021. The dependent variable in column (4) is an indicator variable that takes the value 100 if the subject protested directly in 2020. Mean outcomes at baseline correspond to the mean of the dependent variables computed using the group of subjects who did not receive feedback about the school share nor recapture share.

Table 3: Main Regression: Robustness Checks

	P_D^{2021} (1)	I^{2021} (2)	P_D^{2021} (3)	I^{2021} (4)	P_D^{2021} (5)	I^{2021} (6)	P_D^{2021} (7)	I^{2021} (8)	P_D^{2021} (9)	I^{2021} (10)
a. Effects of School Share:										
With Children	-0.367*	-0.408*	-0.429*	-0.457*	-0.330*	-0.250	-0.226	-0.088	-0.369	-0.418*
	(0.221)	(0.234)	(0.225)	(0.235)	(0.190)	(0.205)	(0.168)	(0.191)	(0.237)	(0.247)
Without Children	0.277**	0.269*	0.285**	0.286**	0.196*	0.321**	0.197*	0.256**	0.301**	0.324**
	(0.129)	(0.144)	(0.133)	(0.146)	(0.119)	(0.132)	(0.116)	(0.130)	(0.139)	(0.153)
Difference (Children - No Children)	-0.644**	-0.678**	-0.714***	-0.744***	-0.525**	-0.571**	-0.423**	-0.344	-0.671**	-0.743**
	(0.256)	(0.275)	(0.262)	(0.278)	(0.224)	(0.244)	(0.203)	(0.231)	(0.274)	(0.290)
b. Effects of Recapture Share:										
With Children	0.076	-0.313	0.141	-0.222	0.166	0.135	0.065	0.013	0.231	-0.059
	(0.485)	(0.541)	(0.478)	(0.536)	(0.417)	(0.451)	(0.330)	(0.373)	(0.442)	(0.492)
Without Children	0.498	-0.101	0.436	-0.125	0.414	-0.129	0.247	-0.051	0.473	-0.051
	(0.303)	(0.325)	(0.307)	(0.325)	(0.273)	(0.291)	(0.243)	(0.265)	(0.318)	(0.338)
Difference (Children - No Children)	-0.422	-0.212	-0.295	-0.098	-0.248	0.264	-0.182	0.063	-0.242	-0.009
	(0.563)	(0.620)	(0.559)	(0.616)	(0.500)	(0.536)	(0.394)	(0.438)	(0.527)	(0.579)
Cragg-Donald F-Statistic	30.10	30.22	29.68	29.68	35.26	35.34	47.35	47.55	34.35	34.35
Mean Outcome (Baseline):										
With Children	33.86	47.20	34.68	47.58	35.00	47.10	33.11	47.95	36.27	50.00
Without Children	28.83	44.87	29.12	45.10	29.77	45.64	29.53	46.09	29.33	44.28
Observations	2,110	2,090	2,070	2,070	2,335	2,309	2,482	2,454	1,807	1,807
Additional Controls			√	√						
2.5% Outliers					√	√				
1% Outliers							√	√		
Attention Check									√	√

Notes: * Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. This table reports 2SLS estimates of equation (21) discussed in Section 5.3. Panel (a) reports the estimates corresponding to the school share treatment effect. We present the coefficients for households *with* children and households *without children* separately, as well as the difference between these two types of households. Panel (b) reports analogous results but for the recapture share treatment effects. For reference, Columns (1) and (2) are identical to those in Table 2. The rest of the columns in this table use the same dependent variables as in Columns (1) and (2). Columns (3) and (4) add additional control variables collected in the survey: age, gender, college degree, and political party. Columns (5) and (6) drop 2.5% of the outliers at each tail of the distribution, instead of 5% which is the baseline specifications. Columns (7) and (8) drop 1% of the outliers at each tail. Columns (9) and (10) restrict the estimates to the subjects who passed the attention check included in the questionnaire. The question for attention check can be found in the survey questionnaire in Appendix D. Mean outcomes at baseline correspond to the mean of the dependent variables computed using the group of subjects who did not receive feedback about the school share nor recapture share.

Online Appendix (For Online Publication Only)

Giaccobasso, Nathan, Perez-Truglia and Zentner, "Benefit-Based Taxation: Evidence from a Field Experiment"

February 15, 2022

A Extensions to the Conceptual Framework

In this section, we discuss how the predictions of the conceptual framework change under more general assumptions. Recall the relationship between benefits and the probability of protesting:

$$P_j = \gamma \cdot B_j \tag{A.1}$$

As in the simpler model from Section 3, we allow the two groups of households *with* and *without* children to have different preferences over government expenditures in local public schools and in other local government services. However, this time we do it in a more flexible way:

$$B_j = \alpha_j^S \cdot S + \alpha_j^{NS} \cdot NS \tag{A.2}$$

Where parameters $\alpha_j^k > 0 \ \forall j \in \{C, NC\}, \ \forall k \in \{S, NS\}$ capture how much different households benefits from different types of expenditures. The parameter α_C^S denotes how much a household *with* children enrolled in public school benefits per each dollar spent in public schools, while α_{NC}^S denotes the corresponding parameter but for households *without* children in public schools. Likewise, α_C^{NS} denotes how much a household *with* children enrolled in public school benefits per each dollar spent in non-school services, while α_{NC}^{NS} is the corresponding parameter for households *without* children in public schools.

As in Section 3, we still assume that households *with* children in public schools benefit more from school expenditures than from non-school expenditures: $\alpha_C^S > \alpha_C^{NS}$. Now we introduce the equivalent assumption for the other group: households *without* children in public schools benefit more from non-school expenditures than from school expenditures ($\alpha_{NC}^{NS} > \alpha_{NC}^S$). Note that we do not need to assume that households *without* children do not benefit at all from school expenditures. Instead, this assumption even allows household *without* children in public schools to benefit somewhat from the provision of public schools (e.g., they may get a warm glow from helping other parents in the community), just not as much as from the non-school expenditures (from which they do benefit directly).

Next, we can follow the same steps from Section 3. We can normalize equation (A.2) by total expenditures:

$$B_j = G \cdot (\alpha_j^S \cdot s + \alpha_j^{NS} \cdot (1 - s)) \tag{A.3}$$

Combining equations (A.1) and (A.3) and re-arranging terms, we obtain the following:

$$P_j = \gamma \cdot G \cdot (\alpha_j^{NS} + s \cdot (\alpha_j^S - \alpha_j^{NS})) \tag{A.4}$$

Using this equation, we can see what would happen if we were to increase the share of government expenditures that go to fund public schools:

$$\frac{\partial P_C}{\partial s} = \gamma \cdot G \cdot (\alpha_C^S - \alpha_C^{NS}) < 0 \tag{A.5}$$

$$\frac{\partial P_{NC}}{\partial s} = \gamma \cdot G \cdot (\alpha_{NC}^S - \alpha_{NC}^{NS}) > 0 \tag{A.6}$$

That is, the results from Prediction 1 still hold under the more general assumptions: For households *with* children in public schools, the predicted effect is negative, while it is positive for households *without* children.

Next, we include redistribution of school taxes. As in Section 3 we consider the analysis from the perspective of a household in a wealthy school district. As before, $r \in [0, 1]$ represents the fraction of school taxes that are transferred from the household's own school district to other school districts. We need to extend equation (A.2) to include preferences over spending in schools in disadvantaged school districts:

$$B_j = \alpha_j^S \cdot S \cdot (1 - r) + \alpha_j^{NS} \cdot NS + \alpha_j^R \cdot S \cdot r \tag{A.7}$$

The difference with respect to Section 3 lies in parameters $\alpha_j^R > 0 \ \forall j \in \{C, NC\}$, which capture how much households in this school district benefit from the expenditures in disadvantaged school districts. When $j = C$, $\alpha_C^R > 0$ is a parameter representing how much households *with* children enjoy giving money away to poor school districts, while when $j = NC$, $\alpha_{NC}^R > 0$ is the corresponding parameter for households *without* children. We can follow the same steps as before. We normalize equation (A.7) by total expenditures, combine it with equation (A.1) and then we re-arrange:

$$P_j = \gamma \cdot G \cdot (\alpha_j^S \cdot s \cdot (1 - r) + \alpha_j^{NS} \cdot (1 - s) + \alpha_j^R \cdot s \cdot r) \tag{A.8}$$

Using this equation, we can see what would happen if we were to increase the recapture share:

$$\frac{\partial P_C}{\partial r} = \gamma \cdot G \cdot s \cdot (\alpha_C^R - \alpha_C^S) \tag{A.9}$$

$$\frac{\partial P_{NC}}{\partial r} = \gamma \cdot G \cdot s \cdot (\alpha_{NC}^{R} - \alpha_{NC}^{S}) \tag{A.10}$$

Under this more general model, the predictions from Prediction 2 no longer hold. For households *with* children in public schools, the sign of the effect is ambiguous. For example, if households *with* children care equally about their own school districts and disadvantaged school districts (i.e., $\alpha_{C}^{S} = \alpha_{C}^{R}$), the predicted effect would be zero. If parents are selfish and they care more about their own children, the predicted effect would be positive. But if parents are altruistic and they care more about children in disadvantaged school districts, the predicted effect would be negative.

For households *without* children in the school district, the effect is ambiguous too. Again, if this group cares equally about their own school districts and other disadvantaged school districts (i.e., $\alpha_{NC}^{S} = \alpha_{NC}^{R}$), or if they don't care about either of them, the predicted effect would be zero. It is possible that households *without* children still care more about their own school district (i.e., $\alpha_{NC}^{S} > \alpha_{NC}^{R}$), due to an attachment to their own community, in which case the predicted effect would also be positive. On the other hand, it is plausible that households *without* children care more about the more disadvantaged school districts (i.e., $\alpha_{NC}^{R} > \alpha_{NC}^{S}$) – after all, if they are going to support the education of other people's children, they may as well direct that support to the children who need it the most. In that case, the predicted effect would be negative. In sum, the direction of the effect of an increase in the recapture share is unclear under more general assumptions.

B Further Details and Results

B.1 More Details about the Institutional Context

In this appendix, we provide more details about the institutional context. The property tax system in Texas is comprised of four main parts, which are separated to lessen conflicts of interest. The first part is formed by local taxing units that are in charge of setting tax rates. This includes counties, cities, school districts, and special districts. The second part is the County Appraisal Districts, which are tasked with appraising the market value of each property in the county (a key input upon which property taxes are based) and notifying the owner(s) about the estimated property taxes due each year. The total property tax bill is the sum of a series of taxes (e.g., a school tax, a city tax, a county tax), which is collected by the third part: the County Tax Offices. The County Tax Offices collects property taxes on behalf of the specific jurisdictions to which the household belongs. They send households a bill, process and track payments, and enforce penalties for delinquencies. The last part of

the property tax system is the Appraisal Review Board, a quasi-judicial entity that settles disagreements about property values and how property taxes are assessed between property owners and the County Appraisal Districts.[63]

To make public school funding more equitable across school districts, Texas enacted a system that redistributes school taxes collected from "property wealthy" districts to "property poor" districts. This redistribution system was implemented in 1993. This system was the result of legal challenges to the prior system of state school finances by poor school districts in the late 1980s and early 1990s on state constitutional grounds. While there have been several changes to the recapture system since its inception, the amount of redistribution is still substantial under the current recapture formula (Texas Education Agency, 2021c). Specifically, House Bill 3 passed in 2019 substantially altered the recapture formula in Chapter 41 of the Texas Education Code, effectively reducing the extent of redistribution from wealthy districts to poorer districts immediately and slowing its growth in the future. Prior to the passage of House Bill 3, the amount of redistribution due to recapture was projected to increase rapidly due to growth in property values.

Under the current recapture formula (Texas Education Agency, 2021a), there are two ways (called "entitlements") in which ISDs get access to school property tax dollars. First, there is a basic allotment per student (e.g., $6,160 per student for the school year 2020–2021). This amount may be adjusted based on student and school district characteristics to compute the ISD's "tier one entitlement level". Districts that collect more tax revenues than the tier one entitlement level must transfer the excess revenues into the recapture system.[64] The second entitlement (the "tier-two enrichment entitlement") rewards school districts with higher tax rates by distributing additional funds to ISDs that set tax rates above $0.9164 (but below $1.0864) per $100 in property values.[65]

The recapture system is funded by other state taxes in addition to the funds recaptured from property taxes collected in property wealthy districts. The state places all recaptured funds from ISDs in a large bucket (called the state aid M&O funds) together with several other state revenue sources such as sales taxes. Then, the state computes the amount of

[63] ISDs in Texas can set their own tax rates, but must abide by certain state regulations. Regulations surrounding the flexibility that ISDs have to adjust tax rates have changed substantially in recent years (Texas Education Agency, 2021b).

[64] ISDs' tax rates are constituted by two separate tax rates: (1) the Maintenance and Operations (M&O) tax rate, which provides funds for the maintenance and operation costs of each district, and (2) the Interest and Sinking (I&S) tax rate, which provides funds for payments on the debt that finances a district's facilities. ISDs' M&O taxes *are* subject to recapture, while the revenues from I&S taxes are not subject to recapture (since I&S can only be used for facilities and capital needs which require approval from voters).

[65] The formula used for the second entitlement features a graduated schedule, which allows ISDs to keep all of the money for the first eight pennies above $0.9164 of tax per $100 of total property value and a fraction of the money for the nine pennies from $0.9964 to $1.0864 of tax per $100 in property values.

state aid (i.e., M&O State Aid) going to each ISD. Districts that do not reach the first tier entitlement level per student receive transfers from the state that are funded with funds recaptured from property-wealthy districts.

B.2 Definition of Sub-Samples: Universe, Letter, and Survey Samples

The DCAD listed 844,258 records in 2021, including real commercial and residential property and business personal property. Along the paper we define four different samples that we use with different purposes: Universe, Letter Sample, Subject Pool and No Response Sample. These are the samples included, for instance, in Table B.1.

The "Universe" sample contains 400,192 properties in Dallas County. These comprise the universe of non-commercial, owner-occupied residences that pay property taxes, which is the type of properties in which we are interested in for the purpose of this paper. In addition to these, which are related to specific characteristics of the properties, we introduce some additional filters to facilitate the implementation of the experiment and to have information from previous fiscal years. These are described next:

- Exclude business personal property accounts (102,164 records). These are defined as tangible personal property that is used for the production of income and includes furniture and fixtures, equipment, machinery, computers, inventory held for sale or rental, raw materials, finished goods, and work in process.
- Exclude properties that were not included in the 2020 records (6,395 records).
- Exclude properties with missing owner name or flagged as private per Texas Property Code (1,607 records).
- Exclude properties that are not single-family residences, townhouses or condominiums (138,542 records). These are properties that have the following DCAD STPD Codes: A11, A12 or A13. Examples of the properties excluded are: mobile home, commercial or industrial plots, railroads, pipelines, agricultural land, among others.
- Exclude properties that were formally registered with the county to have owners with different equity stakes (24,440 records). These are the properties included in a file called by the DCAD as "multi_owners."
- Exclude properties that included at least one of a group of keywords that suggest that the properties are not used for residential purposes or are not owned by individuals

(68,921 records). Examples of keywords are: trust, llc, investments, realty, revocable, partner, farm, inc, assoc, ltd, limited, holding, fund, among others.

- Exclude properties where mailing and property street numbers or street names were different. For street numbers we checked for exact matches. For property address we used fuzzy matching and set a high similarity threshold (75,539 records).
- Exclude properties that paid less than \$0.5 in property taxes (12,215 records).
- Exclude properties that have "NO CITY" assigned as city jurisdiction (1,069 records).
- Exclude properties that did not match these criteria in the 2020 DCAD records (13,161 records).
- Drop properties that have two "&" characters in the owner name (13 records).

The "Letter Sample" contains the 78,128 properties that were selected to receive a letter with an invitation to participate in the survey. In order to arrive to this group of properties we proceeded in two steps: first we applied an additional set of filters mostly related to home characteristics, their property taxes, and history of protest through tax agents. Then, with the remaining 210,289 properties we conducted the actual randomization process. The additional filters applied to the sample are:

- Exclude properties with missing information about the number of bedrooms, bathrooms or total living area; that are worth less than \$50,000 and more than \$7,500,000 in 2021; or that have an estimated tax rate of less than 1% (99,458 records).
- Exclude properties that were selected for a related experiment in 2020 (Nathan et al., 2020) but did not respond the corresponding survey and do not belong to school districts that are "property wealthy" (i.e., they redistribute part of the school property tax collected towards other districts). We explain this criterion with more detail later in this section (42,570 records).
- Exclude properties with one bedroom or less (11,680 records).
- Exclude properties with tax ceilings in the school district component of their tax bill (e.g., Age 65 or Older or Disability exemptions) (54,376 records).
- Exclude properties whose homeowners protested taxes through an agent in 2020 (or in 2021 before the assessed values were notified) and do not belong to school districts that are "property wealthy" (16,196 records).

- Exclude properties that protested taxes before notified values were sent – i.e., protests due to reasons unrelated to assessed values (3 records).
- Exclude properties whose owner notified a change of address (5,996 records).

After all these restrictions, the pre-randomization dataset contains 210,289 properties, out of which 78,128 were selected to receive the letter that invited the homeowners to participate in the online survey. We designed this randomization to over-sample households from the ISDs that contribute the most to the recapture system, in order to increase variation in the recapture share. We designed the randomization to also over-sample households who experienced increases in their estimated taxes, which are those who typically are most likely to file a protest (Nathan et al., 2020). We did this because, relative to previous years, the DCAD increased the proposed value for a smaller share of households, and for that reason we expected fewer households to file a protest in 2021. The randomization also under-sampled households from larger ISDs, to reduce sensitivity to the largest ISDs (e.g., Dallas ISD). More specifically, the selection to the survey was stratified as follows. First, for properties in the three ISDs with the highest recapture shares (Carrolton, Coppell, and Highland Park ISDs), all homeowners were selected to receive the letter. For all the remaining ISDs, we sorted the data by an index comprised by the estimated %-increase in the property tax bill (relative to 2020) plus a randomly generated number. We then selected the first 5,200 properties within each school district to be invited to the survey.[66] This selection results in the 78,128 properties that we call "letter sample."[67] It is important to note that the DCAD appraises values of properties located in 16 ISDs. However, 2 of these 16 ISDs had very few households in 2021 that fall under the DCAD: Ferris ISD has 240 properties and Grapevine-Colleyville has 191 properties within the DCAD. Moreover, many of these properties are excluded from the study because they are Business Personal Property and not households. Due to the small number of households in these 2 ISDs that fall under the DCAD, we excluded them from the survey.

Within the "letter" sample, randomization for each of the possible treatment arms was conducted separately. First, half of the homeowners were randomly selected into the school share treatment. Then, regardless the school share treatment assignment status, half of the homeowners were selected to receive the recapture share treatment. This results in four type of homeowners based on their treatment status who are distributed as follows: 1) 19,513 were

[66] For Sunnyvale and Lancaster ISDs, since the number of properties in the pre-randomization dataset were less than 5,200, all properties were selected to receive the invitation letter.

[67] In addition, half of these properties were selected into the prize message, that included explicit references to monetary incentives to participate in the survey. In Section B.5 we describe the effects of this explicit mention to monetary incentives in terms of the response rate.

selected to receive no feedback, 2) 19,551 were selected to receive only school share feedback, 3) 19,551 were selected to receive only recapture share feedback, and 4) 19,513 were selected to receive both school and recapture share feedback.

The data collected from the online platform contains 3,020 observations from homeowners who started the survey. Even though we were very careful in setting up the survey such that it prevents people filling the survey multiple times, 80 of these responses are duplicated observations who correspond to the same homeowner that started filling the survey in multiple devices, or in different internet browsers. To avoid any possibility of contamination in pre-treatment responses, we only kept the observation corresponding to the first time that a homeowner entered the validation code in the survey link. After dropping duplicate answers, the total number of survey answers (complete and incomplete) is 2,980. In addition, we excluded 159 responses that did not include answers for the key variables in the survey i.e., both prior and posterior beliefs about the school and recapture shares. We also dropped 185 responses that corresponded to homeowners that registered a tax agent before the start of the tax protest season, 36 responses in which the homeowner started the survey after the protest date registered in the administrative records, and 23 responses that were started after the deadline to protest taxes. As explained in Section 4.3, our subject pool also filters the sample to homeowners that do not have extreme misperceptions about the school or recapture share (top and bottom 5%). Hence, we dropped 218 individuals with outlier misperceptions about the school share , 215 with outlier misperceptions about the recapture share, and 34 with outlier misperceptions of both. The resulting number of observartions included in the main analysis is 2,110.

B.3 Descriptive Statistics

In this Appendix, we describe the main characteristics of the properties/homeowners included in our analysis (subject pool). We also compare them with the different samples referred to throughout the paper to show whether there are differences between the type of homeowners/properties that answered our online survey, the universe of properties in Dallas County and the sample of homeowners that were invited to the survey but did not respond.

Table B.1 presents descriptive statistics for some key variables, and for each of the samples used in this paper. Column (1) corresponds to the universe of non-commercial, owner-occupied residences that pay property taxes in Dallas County, Texas as defined in Appendix B.2. The average home in this sample was assessed at $327,690 and paid $6,370 annually in property taxes – equivalent to a tax rate of 1.94% in this sample. Of the total amount of property taxes paid, 49.78% go to School Taxes and the average recapture share is -1.96%. This means that the average property is located in a school district that, for each $100

collected in school taxes, receives an additional of $1.96 from other property richer school districts. In terms of the protest history, in 2020, 7.99% of the homeowners in this sample filled a tax protest directly and 8.04% filled a protest through an agent. In 2021, both the owner and agent protest rates (3.81% and 7.58%) were lower compared to 2020. Our survey provided detailed instructions about how to protest property taxes which likely affected the direct protest probability (see Nathan et al. (2020) for results from a related field experiment). In terms of the property characteristics, the average home has a living are of 2,050 squared feet, 3.12 bedrooms, and 2.06 full bathrooms. In addition, 22.17% of all properties are owned by two or more individuals (e.g., couples who share the ownership of a home).

Column (2) of Table B.1 shows the same characteristics but for homeowners that were selected to receive the invitation letter. Because of the filters used to build the letter sample (see Section B.2 for more details), properties included in this group are more expensive, and consequently pay more in property taxes, although the share of property taxes that correspond to school taxes remains the same (50.60%). Instead, because we over-sampled properties in school districts that are property richer, the average recapture share becomes positive (1.23%), which means that the average property included in the letter sample is located in a school district that transfers part of their school taxes revenues to other property poorer school districts. In terms of protest history, the homeowners selected to receive the letter seem slightly more likely to fill a protest directly (e.g., in 2020, the direct protest rate was 8.84% for this sample vs. 7.99% in the universe sample).[68] The owner protest rate in 2021 for the letter sample is also lower compared to 2020 (5.99% versus 8.84%), although the protest rate in 2021 is higher in the letter sample than in the universe by design since we intentionally over-sampled properties that were more likely to protest as explained in Section B.2.

Column (3) of Table B.1 shows the same characteristics but for the 2,110 homeowners that comprise our subject pool, while column (4) does the same for invited homeowners that did not answered the online survey.[69] Relative to those who were invited but did not respond, the homeowners included in the subject pool are very representative in a host of characteristics such as property value, property tax amount, share of school taxes, recapture

[68] While the agent protest rate is smaller for the letter sample compared to the universe of properties (6.31% vs 8.04% respectively, agent protests in the letter sample correspond only to protests filled by homeowners in school districts that are net givers in the recapture system. This is because some of the filters that we used to select the letter sample were not applied to properties in richer school districts in order to increase variation in the recapture share. More details are described in Section B.2).

[69] It is important to recall that the sample described in column (3) is comprised by individuals who filled the online survey and satisfy the requirements to be included in the analysis as explained in Section 4.3 and Appendix B.2 (e.g., answered the key questions, do not have extreme prior misperceptions, among others). For this reason the sum of the number of observations in columns (3) and (4) is not equal to the number of observations in column (2).

share, number of bedrooms and square footage. Due to the large sample sizes, the pairwise differences in characteristics are often statistically significant, but economically small.

There is one meaningful difference, though, in the past protest behavior of survey respondents relative to non-respondents. Survey respondents are more likely to have protested in the past (e.g., the owner protest rate in 2020 was 18.10% for the subject pool while only 8.51% in the no-response sample). The difference is even larger if we consider protest rates in 2021. This was in large extent by design, since when selecting the subjects to be invited to participate in the survey, we over-sampled households who experienced increases in their estimated taxes. Those households are typically the most likely to file a protest (Jones, 2019; Nathan et al., 2020). Moreover, since our letter was about tax protests, subjects who were considering to file a protest in 2021 were probably the ones most likely to pay attention to the letter and, therefore, most likely to notice the survey link included in the letter. Moreover, our letter promised instructions on how to file a protest as a reward for participation, so it is natural that respondents who are considering to protest are more likely to participate. Additionally, these instructions likely made it easier for survey respondents to file an appeal, as documented in Nathan et al. (2020). Indeed, the higher propensity to protest among survey respondents is consistent with the results from Nathan et al. (2020), which used a similar recruiting method to collect survey responses in this same context.

Because of the importance of the school and recapture share variables for our analysis, next we report some additional information about its distribution. Figure B.1 shows the distribution of school share in panel (a) and the distribution of recapture share in panel (b). Each panel reports the distribution for the three most relevant samples separately: universe of households (green bars), letter sample (yellow bars) and subject pool (red bars). Overall, The distribution of school share shown in Figure B.1(a) for any of the samples considered illustrates that there are differences across households in how much of their taxes are destined to funding public schools. Some of these cross-household differences are because of differences in tax rates across ISD and other jurisdictions (e.g., city). Other differences are due to non-linearities produced by caps and exemptions in the tax schedule. For instance, one source of variation is the over-65 exemption: owners over 65 years of age can apply for this exemption for their primary residence, which has the added benefit of "freezing" the dollar amount of school taxes so they do not increase in the future. For households who were granted this exemption many years ago, the savings from this exemption can get to be quite substantial. In addition, Figure B.1(a) shows that the subject pool is very representative of the individuals invited to the survey in terms of the school share. The subject pool is also representative of the universe of households around the center of the distribution. However, it under-represents the extremes of the distribution (i.e., school share below 44% and above 60%). Figure B.1(b)

also shows that there is quite a bit of variation across the 14 ISDs we study.[70] Four of those districts are net givers, with the highest giver being Highland Park ISD, for which 57.3% of their school taxes are redistributed away. The remaining ten districts are net receivers, with the highest receiver being Mesquite ISD, which receives an additional 23.3% of school taxes from other districts in the state. Figure B.1(b) also shows that survey respondents are very representative of the individuals invited to the survey in terms of the recapture share. The subject pool is also representative of the universe of households, except that it under-represents the Dallas ISD and over-represents the Carrolton, Coppell and Highland Park ISDs – this happened by design, due to the algorithm used to select the households that would be invited to partake in the survey as explained in Appendix B.2.

B.4 Details about the Feedback Provided in the Survey

When providing the feedback about the share of school taxes, the screen included a button labeled "methodological notes". Upon clicking that button, the respondent was shown a box with the following details about how the feedback was constructed:

> "Notes: Property tax data was provided by the Dallas Central Appraisal District (DCAD). Amounts paid to the state by each school district for recapture, along with state maintenance and operations funding (state M&O), were obtained from the Texas Education Agency (TEA). The DCAD estimated the market value of your home as of January 1st, 2021. Estimated Tax Amount is our estimate of taxes due for 2021 using the latest tax rates available (some exemptions might not be included). Recapture funds go to a pool (called state M&O) which is redistributed by the state of Texas to economically disadvantaged school districts. In this survey, we use fiscal year 2021 state funding data to estimate recapture and state M&O for the 2021-2022 school year, by school district. State M&O also has other state funding sources, such as sales taxes. Thus, we estimate the portion of state M&O provided to school districts that came from recapture as the sum of total recapture across all school districts, divided by total state M&O funding. We assume the proportion of total state M&O paid out in recapture in the 2021-2022 school year is similar to this ratio. For each school district, we then multiply the estimated percentage of state M&O that came from recapture by the amount of state M&O provided to the district to get the estimated dollar amount of state M&O from recapture provided to the district. Next, we subtract the recapture paid by the school district from the recapture funding received by

[70] See Appendix B.1 for more details about how the recapture share is computed.

the district to get the net recapture amount. Finally, we divide the net recapture amount by the district's property tax collections in fiscal year 2021 to estimate the net dollars paid or received by the district."

When providing the feedback about the share of recaptured taxes, the bottom of the screen also included a button labeled "methodological notes". Upon clicking that button, the respondent was shown a box with the following details about the construction of the feedback:

"Notes: Property tax data was provided by the Dallas Central Appraisal District (DCAD). Amounts paid to the state by each school district for recapture, along with state maintenance and operations funding (state M&O), were obtained from the Texas Education Agency (TEA). The DCAD estimated the market value of your home as of January 1st, 2021. Estimated Tax Amount is our estimate of taxes due for 2021 using the latest tax rates available (some exemptions might not be included). Recapture funds go to a pool (called state M&O) which is redistributed by the state of Texas to economically disadvantaged school districts. In this survey, we use fiscal year 2021 state funding data to estimate recapture and state M&O for the 2021-2022 school year, by school district. State M&O also has other state funding sources, such as sales taxes. Thus, we estimate the portion of state M&O provided to school districts that came from recapture as the sum of total recapture across all school districts, divided by total state M&O funding. We assume the proportion of total state M&O paid out in recapture in the 2021-2022 school year is similar to this ratio. For each school district, we then multiply the estimated percentage of state M&O that came from recapture by the amount of state M&O provided to the district to get the estimated dollar amount of state M&O from recapture provided to the district. Next, we subtract the recapture paid by the school district from the recapture funding received by the district to get the net recapture amount. Finally, we divide the net recapture amount by the district's property tax collections in fiscal year 2021 to estimate the net dollars paid or received by the district."

B.5 Monetary Incentives for Participation

The invitation letters informed a random half of the recipients that they would be entered into a raffle if they completed the survey. This randomization was meant to assess if raffle prizes provide a significant bump to the response rates, which would be useful to inform researchers

wanting to conduct similar field experiments in the future. The results are presented in Table B.2. The first row shows that including the mention to the raffle in the letter had an effect on response rates that is positive, statistically significant (p-value=0.047), but economically small (0.2 pp, or 5.4% of the response rate of the control group). This small effect suggests that while raffles may help boost response rates, it may not be cost-effective. The other rows in this table show the average characteristics of respondents in the samples with versus without the raffle message. The characteristics are quite similar between the two groups, meaning that the inclusion of the raffle did not change the composition of the respondents either.

B.6 Additional Tests for the Validity of the Experimental Design

In this section we provide further evidence about the implementation and validity of the experimental design.

Figure B.2 reports the timing of the survey responses and protests for the subject pool. Panel (a) reports the cumulative distribution of the day in which the homeowners in the subject pool started to fill the online survey. Invitation letters where created as soon as the DCAD proposed values became available (on April 16th, 2021). The mailing company dropped off the letters at the local post office on April 20th and estimated that most of them would be delivered in the next couple of days. Consistent with this projection, we observed the first survey response on April 22th, which is labeled in the figure as day 0 on the horizontal axis. More than half of the survey responses (57%) have a start day within the first week (starting on April 22th), and more than 80% of the subject pool had already started the survey by the end of the second week. Panel (b) reports the evolution of the cumulative direct protest rate, starting also on April 22th. The value of the y-axis in the last day on the horizontal axis (+25) represents the direct protest rate for the subject pool (30.76%).[71] Unlike the cumulative distribution of the start date of the online survey, most of the protests were filled close to the deadline (May, 17th). By the end of the first week (starting on April 22th), the protest rate was only about 6%. Only by the end of the third week the protest rate reached 16.9%, which is slightly more than half of the final protest rate.

To address potential concerns about imbalances in the response rate or attrition, in Table B.3 we report the number of homeowners that reached different parts of the survey by treatment arm, as well as a description of the final subject pool. For reference, panel (a) shows the number of homeowners invited to the survey by treatment arm (N=78,128). Panel (b)

[71] It is important to note that the formal deadline for protests was on May 17th, however, there are 94 protests that are dated between May 18th and June, 19th. For simplicity, we included all these answers within the April, 17th bin.

reports the number of subjects that answered the first two questions of the survey (N=2,966). Since the treatment messages were displayed several questions after the subject started the survey, the treatment messages should not affect whether the subjects started the survey or not. Hence, as expected, the response rates – defined as subjects who answered the first two questions of the survey – are statistically indistinguishable between the four treatment arms: 3.76% for the no feedback group, 3.78% for the school feedback group, 3.88% for the recapture feedback group, and 3.76% for the school and recapture feedback group. The p-value of a test of equality in the response rate across the four groups is 0.912.

Panel (c) in Table B.3 focuses on homeowners that reached (and answered) the key questions of the survey, i.e., prior and posterior beliefs about the school and recapture shares. Because the posterior beliefs were asked after the information treatment was displayed, it is theoretically possible that there is differential attrition (i.e., some people get assigned to a treatment and that makes them more or less likely to finish the survey). However, as observed in panel (c) the evidence suggests that the share of homeowners that answered the key questions of the survey was balanced across treatments: 3.54% for the no feedback group, 3.59% for the school feedback group, 3.70% for the recapture feedback group, and 3.62% for the school and recapture feedback group. The p-value for the equality of these shares across groups is 0.858.

Finally, panel (d) in Table B.3 shows the balance across treatment arms after all the filters were applied to the survey data, as explained in Appendix B.2. This is the actual sample used in the empirical analysis. Panel (d) complements the previous evidence of no differential response rate or attrition by showing that the filters applied to the data were not correlated with the treatment status. In particular, panel (d) shows that 2.66% of the subjects invited to the survey answered the key questions and passed the filters used to define the subject pool, while this share is 2.62% for the school feedback group, 2.76% for the recapture feedback group, and 2.76% for the school and recapture feedback group. All these shares are statistically indistinguishable (p-value=0.377).

Table B.4 presents complementary evidence of potential selection in the survey responses by showing the conditional dropout rate by treatment arm for each question in the survey. We define conditional dropout rate as the probability of dropping the survey conditional on having answered the previous question. Table B.4 shows that even though there is some attrition (i.e. some people leave the survey before reaching the very end), the attrition is orthogonal to the treatment assignment, which suggests that homeowners may drop the survey, but the reasons for this behavior are not related to the treatment assignment. More specifically, of the 22 questions reported in the table, only one seems to have differences in the conditional dropout rate by treatment arm: the question that asks the prior beliefs about the

recapture sign (i.e., whether the own ISD is a net giver or receiver in the recapture system) seems to be larger for the no feedback treatment group. We believe that this result is most likely by chance given the large number of hypotheses being tested in this table, and also because until that point in the survey, all treatment arms had received identical information (the experimental information is displayed after that point in the survey – after "Posterior: Recap (%)").

Finally, Table B.5 reports a similar balance test to the one reported in Table 1. However, rather than showing that the balance test for the entire sample, it performs the balance test separately for the households with children (columns (1) through (5)) and for households without children (columns (6) through (10)). We do this because our main results are also based on breaking down the analysis in these two types of households, so it is natural to look at the balance table for these groups separately. As reported in Table 1, Table B.5 is also consistent with successful random assignment since most of the variables seem to be statistically indistinguishable within each row. Again, the difference is statistically significant for the variable "owner protest in 2020", but this is true both for households with and without children. In addition, there are some few other variables that present imbalances (School Share (%) and Race: White, for household without children; and Owner Protest in 2019 for Household With Children), but given the large number of tests being conducted, it is expected that a few differences will be statistically significant just by chance. Furthermore, the table does not suggest a clear pattern of imbalances that could introduce concerns about issues with the experimental design.

B.7 Robustness of Misperceptions and Belief Updating

In this section we report additional results about the distribution of misperceptions, heterogeneous results in belief updating, and cross-learning.

When studying perceptions via survey data, it is important to deal properly with outlier beliefs. Some individuals may provide guesses that are wildly inaccurate not because they truly hold such extreme beliefs, but because they may have misunderstood the question, made a typo or were just not paying enough attention to the question. The "information shocks" for these individuals will be large but meaningless, which can create a significant attenuation bias. As explained in Section 4.3, following the standard practice in information-provision experiments, to reduce sensitivity to outliers, we drop the households with the most extreme misperceptions in their prior beliefs (see e.g., Fuster et al., 2021; Cullen and Perez-Truglia, 2021; Bottan and Perez-Truglia, 2020a). For the baseline specification, we use a conservative definition of outliers and we drop the top and bottom 5% from the school share belief gap distribution, and we drop the the top and bottom 5% from the recapture share belief gap

distribution. This translates into 467 homeowners: 218 who had extreme misperceptions about school share, 215 that had extreme misperceptions about recapture share, and 34 that had extreme misperceptions about both school and recapture shares. Figure B.3 shows the distribution of misperceptions including the outliers for the school share (panel (a)) and recapture share (panel (b)). By construction, the inclusion of outliers will result in higher degrees of misperceptions. Panel (a) shows that the mean absolute error for the school share belief gap would increase 13.7% (from 16.63 to 18.92) with the inclusion of outliers, while the positive bias would remain about the same (13.20 and 13.06, respectively). Panel (b) shows that, similarly, the mean absolute error for the recapture share belief gap would increase by more than 30% with the inclusion of outliers (from 11.46 to 15.01) while the mean error would remain about the same, with no indication of systematic positive or negative bias.

In Sections 5 and 6, we presented the distribution of misperceptions and the degree of belief updating for the entire sample. In this section, we show that the results are similar when we break them down for households with children versus households without children. We start with the distribution of misperceptions. Figure B.4(a) corresponds to the misperceptions about the school share. This panel is identical to Figure 1(a), but broken down by households *with* children in public schools and households *without* children in public schools. The x-axis corresponds to the difference between the actual school share versus that perceived by respondents. This figure shows that the distributions of misperceptions are quite similar for both groups. A minority of subjects in either of the groups have accurate perceptions, i.e., have beliefs within 5pp of the actual school share. The differences between the two types of households seem to be small: about 30% for households *with* children and 34% for households *without* children. Misperceptions are quite large on average for both groups: the mean absolute error is 18.17pp for households *with* children, and 16.02pp for households *without* children. Finally, school share misperceptions also show a systematic bias in both cases: on average, homeowners in households *with* children under-estimate the school share by 15.28pp, while homeowners in households *without* children underestimate the school share by 12.33pp. This is also evident at first glance, since there are much more observations on the right half of the histogram (corresponding to under-estimation) than on the left half of the histogram (corresponding to overestimation).

Figure B.4(b) presents the corresponding results but for the recapture share instead of the school share. While there is a bit more dispersion among the households *with* children than for the households *without* children, the two distributions are quite similar. In both cases, very few homeowners provide an answer that is within 5pp of the actual recapture share for their school district (around 20%) and the mean absolute error is 13.16 for households *with* children and 10.75 for households *without* children. Unlike the case of the school share, there

is no evidence of a systematic bias in beliefs about school share: for households *with* children, the mean error is 0.72, while for households *without* children, the mean error is -0.62. While the sign of the mean error is the opposite for the two groups, the difference between the two is just 1.3 pp which is very small and consistent with our interpretation of very similar distributions of the misperceptions for both types of households.

Next, we look at the belief updating. Figure B.5 is analogous to Figures 1(b) and 4(b), but broken down by households *with* and *without* children in public schools. Figure B.5(a) and B.5(b) show that the school share belief updating is quite similar for households *with* children as for households *without* children. Panel (a) shows that for households *with* children, for each 1 pp in the school share belief gap, homeowners that were not shown the school feedback update their posterior beliefs by 0.043, while those who were shown the school feedback do it by 0.774, with the difference between the two groups being 0.731 (p-value<0.001). This is very similar to the updating patterns observed for households *without* children shown in panel (b) where the update for the control group is 0.055 and for the treatment group is 0.826, with the difference being 0.771 (p-value<0.001). We obtain a similar result when considering the recapture share belief updating, but with a smaller overall update rate for the recapture share than for the school share as explained in Section 6.2 for the aggregated analysis: the difference between the update rates of households that were shown the recapture share feedback and households that were not shown the feedback is 0.541 for households *with* children (p-value<0.001) and 0.533 (p-value<0.001) for households *without* children.

Finally, we focus on the possibility of cross-learning, i.e., homeowners may update their recapture share beliefs as a response to the school share feedback. It is important to note that cross-learning from recapture share feedback to school share beliefs is technically impossible because survey participants receive the feedback about recapture share after they finish the school share beliefs section of the survey. However, for completeness we also report these results.

For reference, Figure B.6(a) reproduces Figure 1(b) which shows the relationship between the actual school share belief gap and the school share belief update separately for homeowners that were shown the school feedback and homeowners that were not. As explained before, the figure clearly shows the positive effect of receiving the school feedback treatment on the update about school share beliefs. On the other hand, Figure B.6(b) also breaks down the analysis by receiving the school feedback message, but in this case it depicts the relation between the recapture share belief gap and the recapture share belief update. Since the school feedback message does not contain any information related to the recapture share, we do not expect any differences in recapture share updating behavior between the two groups. Figure B.6(b) shows that there is a positive relationship between the recapture share belief

gap and the recapture share belief update for both groups. The reason why we observe a positive relationship between the two variables is that both groups (school feedback shown and school feedback not shown) are comprised by some homeowners that were selected into the recapture feedback treatment and some others that were not. This is because both school and recapture feedback treatment assignments were implemented independently. However, we observe that the difference between the two update coefficients for school feedback shown versus not shown is 0.043 and statistically indistinguishable from 0 (p-value<0.322). This clearly indicates that the school feedback treatment did not have an effect on the updated beliefs about recapture share. In sum, Figure B.6(b) rules out the possibility of cross-learning.

Figures B.6(c) and B.6(d) replicate the analysis but splitting the subject pool into homeowners that received the recapture share feedback and homeowners that did not. As explained before, cross-learning from recapture share feedback to school share beliefs is technically impossible because of the design of the survey questionnaire, but we report the results for completeness and to rule out any concerns. Similar to the results reported in Figure B.6(b) about cross-learning from school share feedback to recapture share beliefs, there are no differences in the relation between the school share gap and the school share belief update between homeowners that received the recapture share treatment and homeowners that did not. Again, this is re-ensuring about the lack of cross-learning effects for any of the treatment arms. Finally, Figure B.6(d) is identical to Figure 4(b) and it is shown for reference. As explained before, Figure B.6(d) shows the positive effect of receiving the recapture share feedback on the recapture share belief updating.

B.8 Further Results about 2SLS Estimates

In this Appendix we provide additional evidence to complement the baseline results. We focus on reporting a more comprehensive set of estimates based on alternative specifications, heterogeneous effects, and additional results on other outcomes based on the survey responses.

First, Table B.6 reports the baseline 2SLS estimates (columns (1) and (2)), the reduced form estimates (columns (3) and (4)), and the first stage estimates (columns (5) and (6)). Columns (1) and (2) and panels (a) and (b) replicate the baseline estimates reported in Table 2 and are included to facilitate the comparison. The reduced form specification is based on a OLS regression of the outcome of interest over the instrument, without accounting for imperfect learning.[72] In other words, it represents the effect of the information included in the feedback message with respect to the prior beliefs without taking into account whether homeowners fully incorporate this information into their posterior beliefs. The estimates

[72] The OLS regression also includes all the control variables included in the 2SLS baseline specification as explained in section 5.3

from the reduced form regression are qualitatively similar to the baseline 2SLS estimates. The only difference is in the magnitude of the treatment effects, which are smaller in the reduced form specification. For instance, while in the 2SLS specification the effect of an additional percentage point in the posterior belief about the school share was -0.367, in the reduced form this coefficient is -0.269. The same can be observed for all the treatment effects and for the two outcome variables (direct protest and intention to protest from the survey responses). The differences in the magnitude of the coefficients between the 2SLS and the reduced form estimates are expected since the first stage results show that homeowners did not fully incorporate the information provided in the treatment. For instance, for each additional percentage point informed about the school share feedback with respect to their prior beliefs, households *with* (*without*) children increased on average 0.736 (0.808) their posterior beliefs about school share. For the recapture share feedback and beliefs, the posterior update was 0.529 (0.550).[73]

Second, Figure **??** presents additional robustness tests about the effect of the recapture share feedback on owner protests, analogous to Figure 2 presented in the main text for the school share. Panel (a) exploits the longitudinal nature of the data and reports the 2SLS coefficient for 2021 – i.e., the treatment year –, but also for five pre-treatment years in the spirit of a falsification test (2016-2021). The x-axis denotes the year of the dependent variable: i.e., whether the owner protested in years 2016 through 2021. This figure focuses on the main result, corresponding to the difference between households *with* children versus *without* children. Figure **??**(a) is re-ensuring about the validity of the experimental design since the treatment does not seem to have any effects on the outcome of interest during the pre-treatment years and it also shows the null effect observed for the year of the treatment. Figure **??**(b) reports the binned scatterplot representation of the reduced-form effect of the recapture information-provision experiment. The x-axis corresponds to the interaction between the potential information disclosure and the prior gap (i.e., the excluded instrument). The y-axis corresponds to the probability of protesting in 2021. This binned scatterplot includes all the same control variables used in the 2SLS model. As for the school information-provision experiment, the figure shows that a linear fit is a reasonable functional form assumption in this context and there is no reason to be concerned about the linear relationship assumed by the 2SLS baseline model.

Tables B.7 and B.8 replicate the baseline analysis reported in Table 2 for the two main outcomes of interest – owner protest in 2021 and intention to protest – but splitting the subject pool by some basic characteristics of the subjects. In Table B.7 we split the sample

[73] It is important to note that while the first stage results explain that 2SLS estimates are larger than the reduced form estimates for the treatment effects on households *with* and *without* children, this does not necessarily holds for the difference, since this is the subtraction of two scaled coefficients.

by gender (female versus male) – columns (1), (2), (7) and (8) – and age (younger than fifty years old versus older than fifty years old) – columns (4), (5), (10), and (11). In columns (3), (6), (9), and (12) we report the p-value of the test of equality of coefficients based on the assumption that coefficients are uncorrelated across the 2SLS regressions. Either by splitting the sample by gender or age, the results are qualitatively similar across groups. In particular, we observe a consistently negative effect of the school share treatment on the probability of protest for households with children compared to households without children. The coefficients associated to the difference between these two groups is negative across the eight columns that report the heterogeneity results. When comparing the coefficients by age of the homeowner, we do not observe major differences in the value of the coefficients between younger and older individuals (-0.826 versus -0.633, and -0.733 versus -0.959) and for the two outcomes we cannot reject the equality of coefficients hypothesis (p-value=0.810 and p-value=0.801). However, differences seem to be slightly larger when comparing male and female homeowners, being the effects larger for males (-0.429 versus -0.911 and -0.209 versus -1.210). While we cannot reject the null hypothesis of equality of coefficients for the owner protest variable (p-value = 0.373), we can reject the equality of coefficients hypothesis for the intention to protest variable at a 10% significance level (p-value=0.090). These differences between male and female behavior may be considered as preliminary evidence of heterogeneous responses, but must be taken with a grain of salt due to the limited statistical power: in some cases we cannot rule out modest or even large differences. Regarding the effect of the recapture share treatment, the sign of the effects oscillate depending on the variable and group, and there are no clear patterns of heterogeneous effects or even any effects: out of the 24 coefficients estimated in this Table for the recapture share treatment only one of them is statistically significant at a 10% level. This is consistent with our interpretation that the recapture share treatment had a null effect on the probability of filling a protest or the intention to protest.

Table B.8 replicates the analysis reported in Table B.7, but in this case splitting the subject pool by education and self-identification with political party of the homeowners. As reported in Table B.7, estimates about the differential effect of the school share treatment between households with children and households without children are consistent across groups and outcome variables, but the effects of the recapture share oscillate and suggest null overall effects. While statistically we cannot reject the equality of effects in most of the cases, there are some small differences in the magnitude of the school share treatment effect that could suggest that less educated and republican homeowners could be slightly more responsive compared to their more educated or democratic party-identified counterparts. However, again we should note that these results are only suggestive and must be taken with a grain

of salt given the lack of statistical power.

Finally, Table B.9 splits the sample by the direction of the prior misperceptions about school and recapture shares. When we induce the information shocks, some individuals updated their prior beliefs up while some individuals updated them down. The baseline 2SLS model averages the effect of the belief updates across those two groups. However, this could mask some meaningful heterogeneity: perhaps individuals who update up react strongly but individuals who update down do not react at all, or vice versa. To allow for asymmetric responses, we split the sample by whether the individuals were under-estimating (and thus would update up) or whether they were overestimating (and thus would update down). As in Tables B.7 and B.8 there is no significant evidence of asymmetries in the response for any of the groups or outcomes considered although because of the lack of statistical precision we cannot rule out modest or even large asymmetries.

Table B.10 reports additional results based on our main 2SLS specification but focusing on additional survey outcomes. In particular we focus on the following questions that were asked after the information-provision stage and therefore could have been affected by the treatment. These can be useful to understand more about the mechanisms that underlie the effects based on the administrative records. Table B.10 reports the effects on five additional outcome variables based on the following survey questions:

1. "Do you agree or disagree with the following statement? *The local government services that I am provided (e.g., schools, roads, hospitals) justify the amount I pay in property taxes*"

2. "Relative to the other households in your county, do you think your household pays a fair amount in property taxes?"

3. "Do you support the recapture system (Chapter 41 of Texas Education Code) and its redistribution of school property taxes between school districts?"

4. "Do you consider the total amount of property taxes you pay to be too low, about right or too high?"

5. "Which of the following alternatives would you prefer? *Your taxes and the taxes of everyone else decrease but you get worse government services, your taxes and the taxes of everyone else are held constant and so are government services, or your taxes and the taxes of everyone else increase to provide better government services*"

For questions (1) through (3), homeowners had to choose a value on a scale from 1 to 10. In Table B.10, to make interpretation easier and consistent with other outcome variables, we

reversed the scale for these three questions from its original formulation. For instance, while question (3) asks about support of the recapture system, Table B.10 reverses the scale of the question and reports the degree of opposition with the recapture system.

The first thing to notice in Table B.10 is that there are no statistically significant effects of school share on any of these additional survey outcomes, neither for each household type separately nor for the difference between the two. These survey questions were placed at the end of the survey so it is possible that the effects were watered down by survey fatigue, or simply that these estimates are imprecise due to lack of power. For the recapture share, there are three significant coefficients, but only one corresponds to differential effects between households *with* and emphwithout children. Given that we find no effects of changes in the recapture share on the actual behavior on owner protest in 2021, the results on these additional survey questions should be taken with a grain of salt.

B.9 Further Results about Expert Forecasts

In this appendix we provide additional evidence about the experts predictions on the effect of the school and recapture share information treatment on the probability of owners filling a tax protest directly in 2021.

First, we report the distribution of the predictions for the effect of the recapture share. These results are presented in Figure B.7, which is the analog to Figure 3 but for the recapture share instead of the school share. Panel (a) reports the predictions for household with children, while panel (b) presents the predictions for households without children. Figure B.7 shows that most experts predicted non-zero effects for *recapture share*. On the contrary, our estimates suggest a null effect, which at first glance seem to contradict the prediction of the expert. However, we should be cautious about this interpretation because of two reasons: 1) the experts predictions fall within the confidence interval of the actual estimates, and 2) for the reasons mentioned above, our experimental estimates with respect to the effect of the recapture share information treatment are imprecisely estimated.

Second, we also show an alternative specification for the distribution of the experts predictions that takes into account the degree of certainty that they expressed in their answers. Figure B.8 reports the analog figures to Figure B.7 and Figure 3 but in this case it provides weights to the predictions that are increasing in the degree of certainty with respect to the corresponding answer. Overall, the results observed for the weighted distribution are almost identical to the unweighted results. First, for the school share treatment effect the forecasts of most experts were inconsistent with the experimental results: a majority of the forecasts indicate effects that would be either zero or have the opposite sign as the experimental findings. Second, for the recapture share treatment, most of the experts predicted non-zero

effects, while our (imprecisely) estimated effects are null.

B.10 Supplemental Evidence from Hypothetical Questions

In this appendix we report additional evidence about the treatment effects on two questions included in the survey that provided hypothetical scenarios to the survey respondents with the goal of eliciting their views about the relative share of taxes that should be paid by households *with* children compared to households *without* children, and whether they support a redistribution system where property rich school districts fund at least partly property poor school districts. More specifically, we asked:

1. "Imagine the government gave you full power to choose the school property taxes that each household must pay as long as the total school property taxes collected stays the same. The home market value for Household A and Household B is the same: $200,000. However, Household A has two children in public schools, while Household B has no children in public schools. What school property taxes would you choose for each home? These two values must add up to $8,000."

2. "Imagine the government gave you full power to choose how to redistribute property taxes between two hypothetical school districts: School District A and School District B. Both districts have the same number of households, same number of students, and the tax rates are the same too. However, homes in School District A are more expensive ($500,000 each) than homes in School District B ($100,000 each). In School District A, households paid $2,500 each in school taxes. In School District B, households paid $500 each in school taxes. How would you distribute these funds?"

While in the first of these questions the survey respondents had to enter any value they want in a text box, in the second question we provided the following options: A gets: $2,500, B gets: $500), (A gets: $2,000, B gets: $1000), (A gets: $1,500, B gets: $1,500), (A gets $1,000, B gets: $2,000).

Figure B.9(a) reports the distribution of the share of taxes that should be paid by households with children based on the answers to question (1). Instead of reporting the values as entered by the survey respondents (e.g., 2,000 and 6,000) we calculated the percentage of the total tax revenue that the respondents assigned to households with children. On the y-axis we report the percent of answers in a given bar of 10 pp width.[74] According to the hypothesis of benefit-based taxation, respondents will want the household *with* children to

[74] Because 50% is an important reference point for the answers in this question, the middle bin contains answers between 45% and 55%. The extreme bins are of width 15 pp instead of 10 pp.

pay more in taxes than the household *without* children, because the former benefits more from the government services through the public schools. We find that a majority (58.8%) of the respondents behaved as expected by the predictions of the benefit-based taxation theory, that is, preferring a higher tax burden on the household *with* children even though both homes are worth the same. This evidence suggests that the logic of benefit-based taxation resonates with most taxpayers.

Figure B.9(b) focuses on one question that could be useful to understand people's views about the recapture system. One potential interpretation of the null effects of the share recapture treatment is that households may be willing to sacrifice government services as long as those tax dollars go to help the most disadvantaged communities. The x-axis denotes the funds assigned to District B, the district with cheaper houses. The x-axis goes from no redistribution ($500 to District B) to more redistribution moving towards the right of the axis. The vast majority (53.4) of the sample prefers some redistribution. The modal response, with 37.9% of the respondents, is that both districts receive the same funds ($1,500 each). Indeed, a minority of respondents (8.7%) want to go beyond that level of redistribution, so that District B receives more funds than District A (despite District A contributing higher revenues than District B). Due to the hypothetical nature of this data, it is difficult to tell whether these preferences would be significant in a revealed-preferences, real-world context. Perhaps households say that they want to be generous towards disadvantaged school districts simply to appear nice. However, this evidence is at least suggestive that most households demand a significant amount of redistribution through recapture. This could explain why households' do not become more likely to protests after learning that some of their taxes are being recaptured.

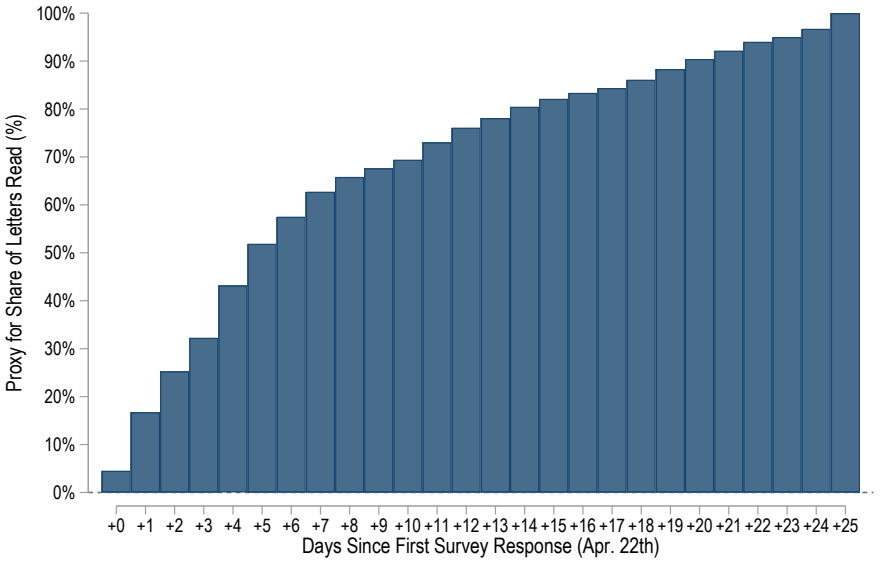
Figure B.1: Distributions of School Tax Share and Recapture Share



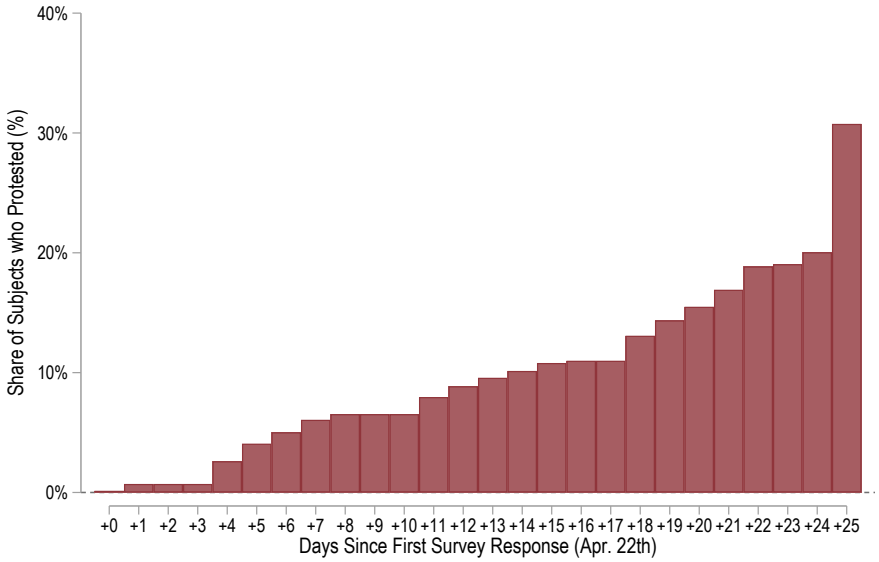
Notes: Panel (a) shows the distribution of school share grouping households in 2 pp bins. Extreme responses are grouped in the ($\leq 40\%$) and ($\geq 60\%$) bins. Panel (b) shows the distribution of properties sorted by their recapture share. Because the recapture share is calculated at the school district level, each of the bars represent a different school district. The solid vertical line separates net giver districts (i.e., part of their school taxes are redistributed towards other property poorer districts) of net receiver districts (i.e., receive money from other richer school districts to fund their public schools). In both panels, each color represents one of the three samples: Universe (green), Letter Sample (yellow) and Subject Pool (red) defined as explained in Appendix B.2. The total number of observations included in each sample is described in notes in the upper right and upper left, respectively. The y-axis reports the percentage of properties within each bin.

Figure B.2: Timing of Survey Responses and Tax Protests

(a) Timing of Survey Responses



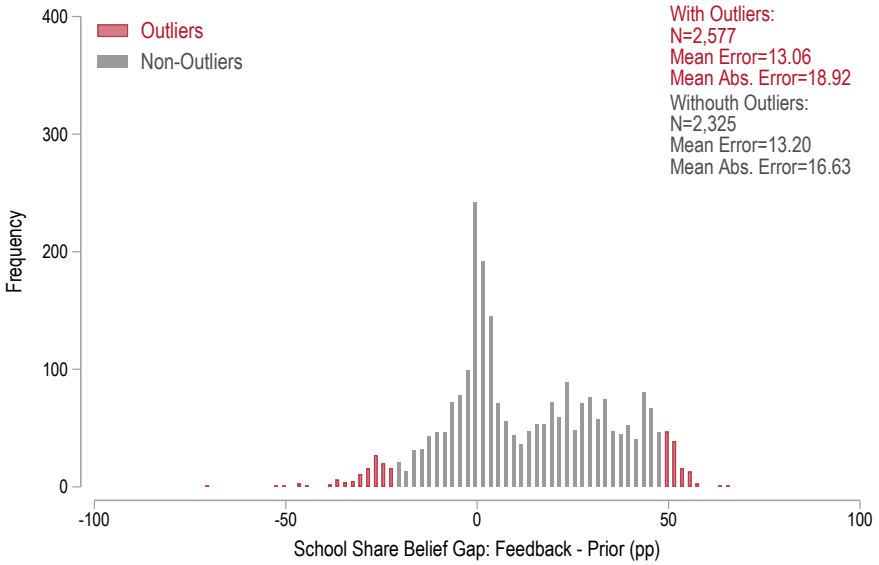
(b) Timing of Subjects' Protests



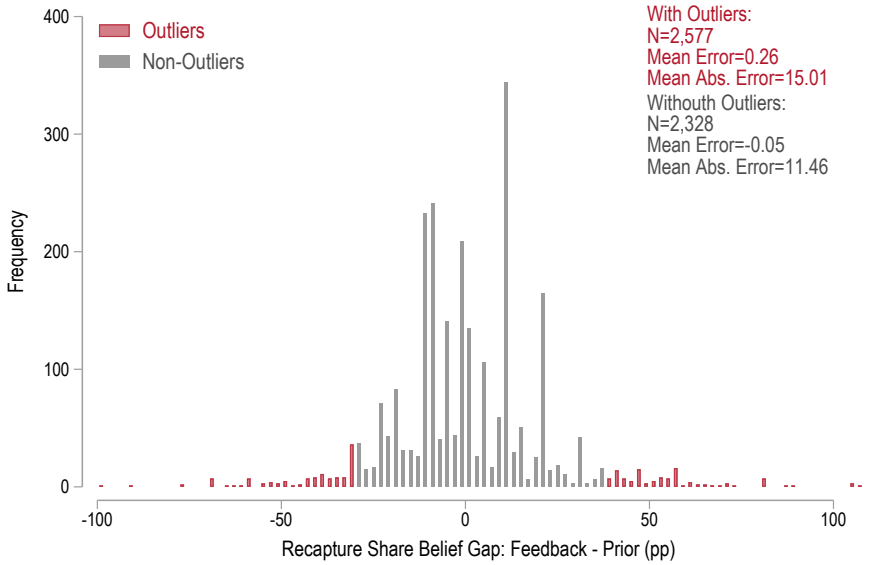
Notes: Panel (a) shows the cumulative distribution of the start day of the survey responses for the subject pool (N=2,110). Day 0 on the horizontal axis represents the day in which we observed the first survey response (April, 22th). The values on the x-axis represent days since April 22th. Day 25 represents the deadline to fill a protest (May, 17th). Panel (b) shows the cumulative protest rate for the subject pool (N=2,110). The protest rate observed in the last day (+25) is 30.76%. Day 25 also includes 94 protests that are dated between May 18th and June, 19th. For simplicity, we included these answers within the April, 17th bin.

Figure B.3: Distribution of the School and Recapture Share Gaps

(a) School Share Gap



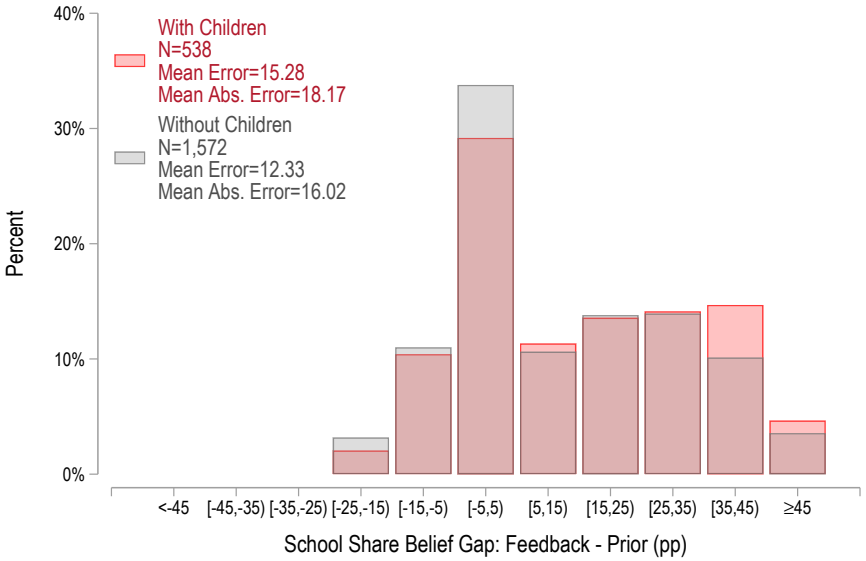
(b) Recapture Share Gap



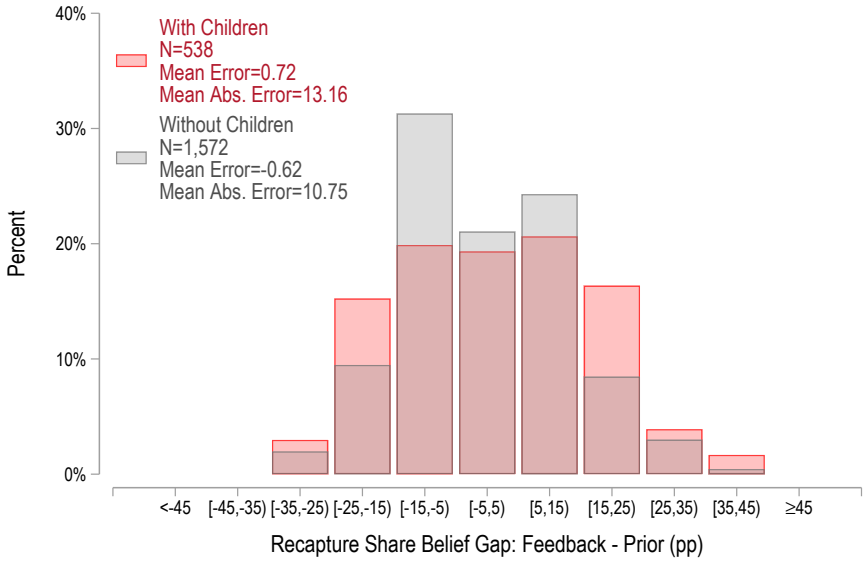
Notes: This Figure reports the distribution of the beliefs gap for the school share (panel (a)) and recapture share (panel (b)). In both cases, the belief gap is defined as the difference between the actual school/recapture share (shown as feedback if selected into the school/recapture share treatment group) and the prior belief reported by respondents of the survey. Gray bars represent the observations observations that are between the 5% and 95% of the school/recapture share distribution. Red bars represent the observations that represent the bars that are in the top and bottom 5% of the distribution of school/recapture share belief gaps. The subject pool used for the main experimental results excludes 467 observations that fall into red bars for at least one of the belief gaps: 218 observations due to extreme misperceptions in school share belief gap, 215 for extreme misperceptions in recapture share belief gap, and 34 observations for extreme misperceptions in both beliefs. The main experimental sample excluded the school share belief gap values $(-\infty$, -20.9] and [48.2, $+\infty]$ and recapture share belief gap values $(-\infty, -29.9]$ and [37.7, $+\infty)$. In the figures in panel (a) and (b) the red bars cover slightly different ranges than the actual filters due to the convenience of having bars of a certain width for illustration purposes (in the figures we use bars of 2pp width).

Figure B.4: Perceptions about the School Share and Recapture Share (With Children versus Without Children)

(a) Gap in Prior Beliefs About School Share



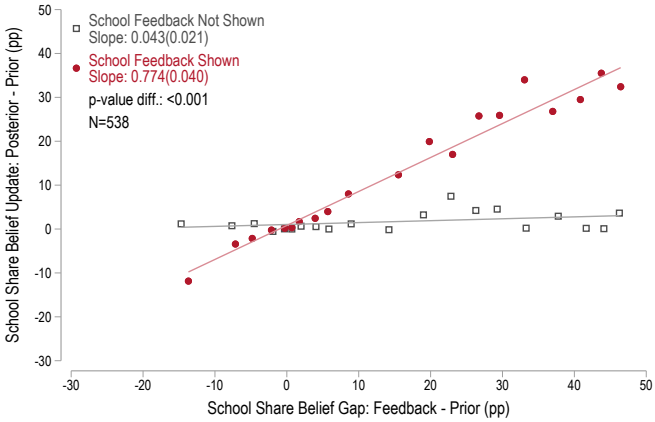
(b) Gap in Prior Beliefs About Recapture Share



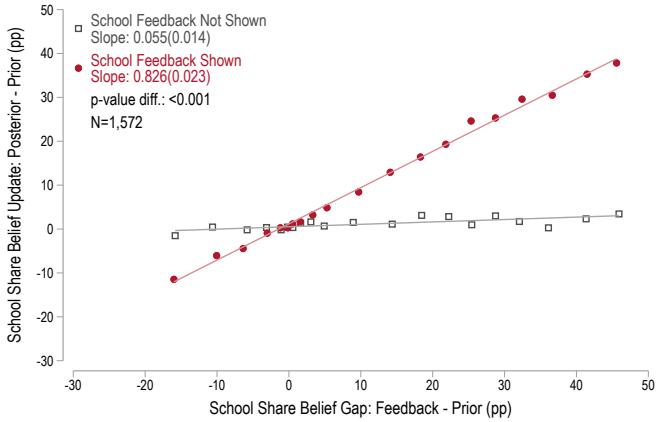
Notes: This figure shows the distribution of the school (panel (a)) and recapture (panel (b)) shares belief gaps broken down by households *with* children in public schools (red bars) and households *without* children in public schools (gray bars), based on the online survey data. In both panels, the belief gap is defined as the difference between the actual school (recapture) share (shown as feedback if selected into the corresponding treatment group) and the prior belief reported by respondents of the survey. The y-axis depicts the percentage of respondents grouped in bins of 10 pp width. In the upper left corner, we report the total number of observations, the average error, and the average absolute error.

Figure B.5: Belief Updating: Households With Children versus Without Children

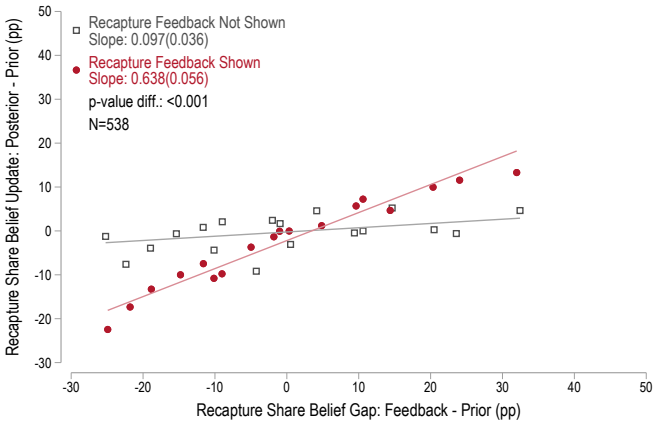
(a) School Share: With Children



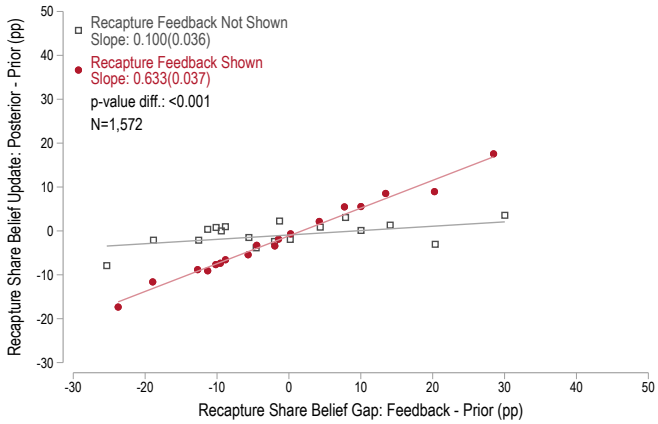
(b) School Share: Without Children



(c) Recapture Share: With Children

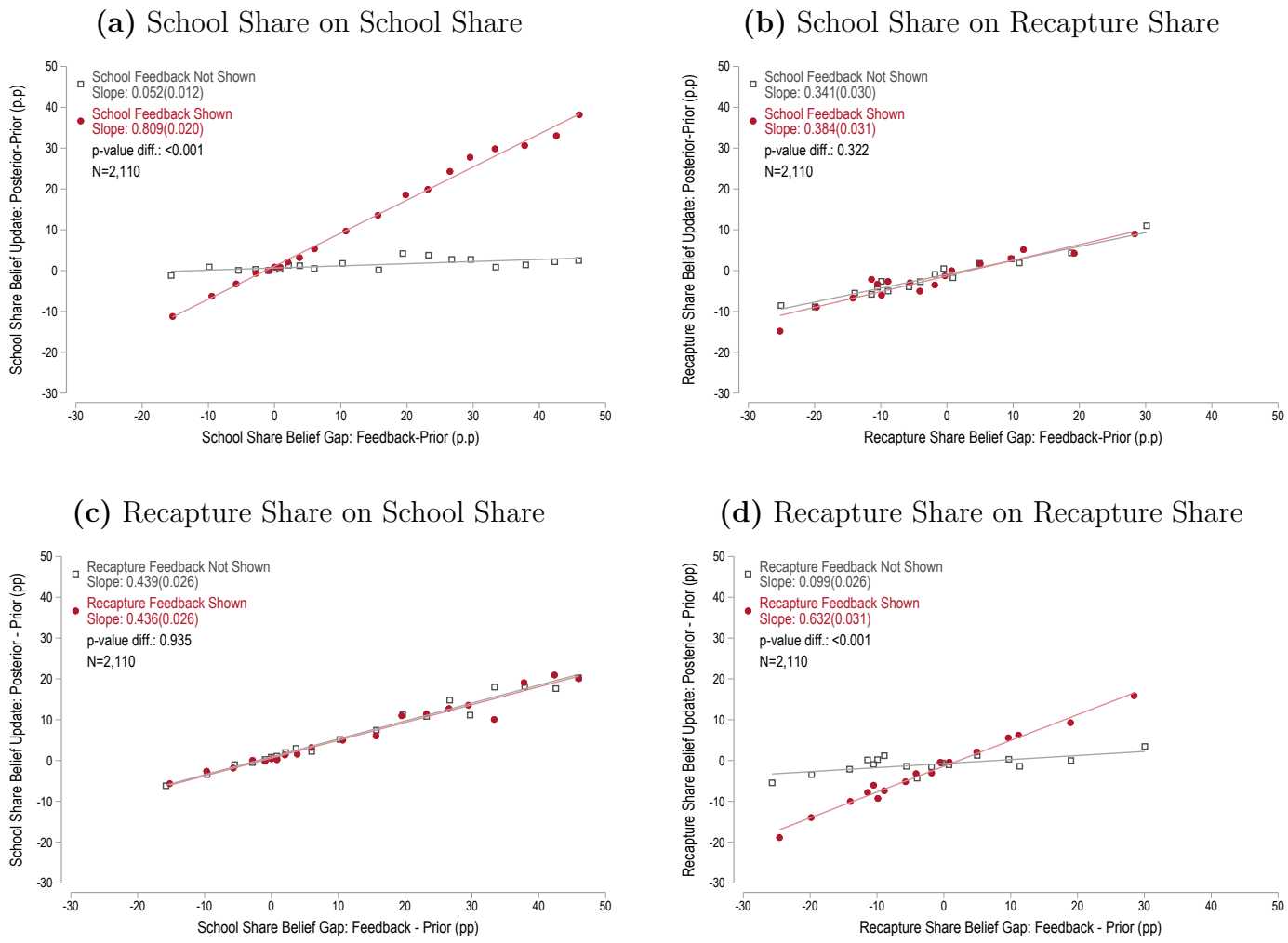


(d) Recapture Share: Without Children



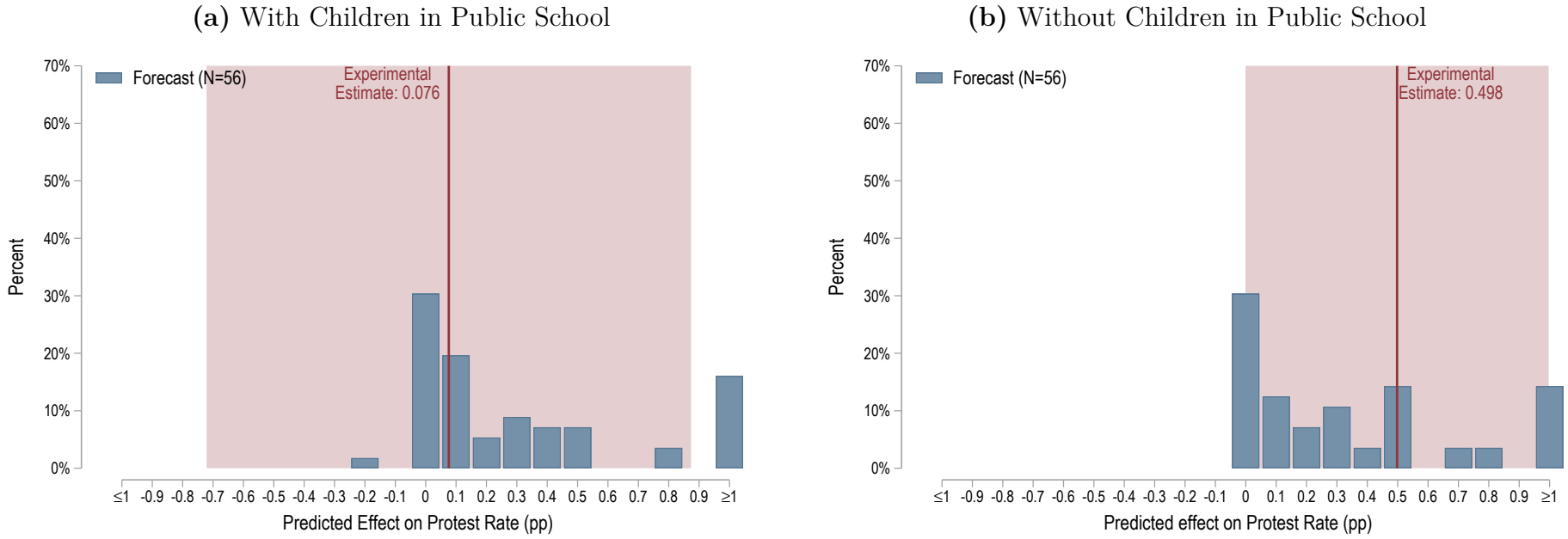
Notes: In panels (a) through (d) we report the relation between the school/recapture shares belief gap - grouped in 20 equally-sized bins - and the size of the update (i.e., posterior belief minus prior belief) by treatment status and broken down by households *with* and *without* children in public schools. Panels (a) and (b) are identical to Figure 1(b), but separating the households *with* and *without* children in public schools. Panels (c) and (d) are identical to Figure 4(b), but separating the households *with* and *without* children in public schools. Gray squares represent the average update within each bin for the group of homeowners that were not selected into the school/recapture share treatment while red circles do the same for homeowners that were selected for treatment. Each line corresponds to the fitted values from separate OLS regressions where the dependent variable is the update variable and the independent variable is the school/recapture share belief gap. The coefficient associated to the gap variable is reported in the upper left corner, as well as the robust standard errors, the p-value of the difference in the slopes, and the number of observations included in the analysis.

Figure B.6: Cross-Learning in the Information-Provision Experiment



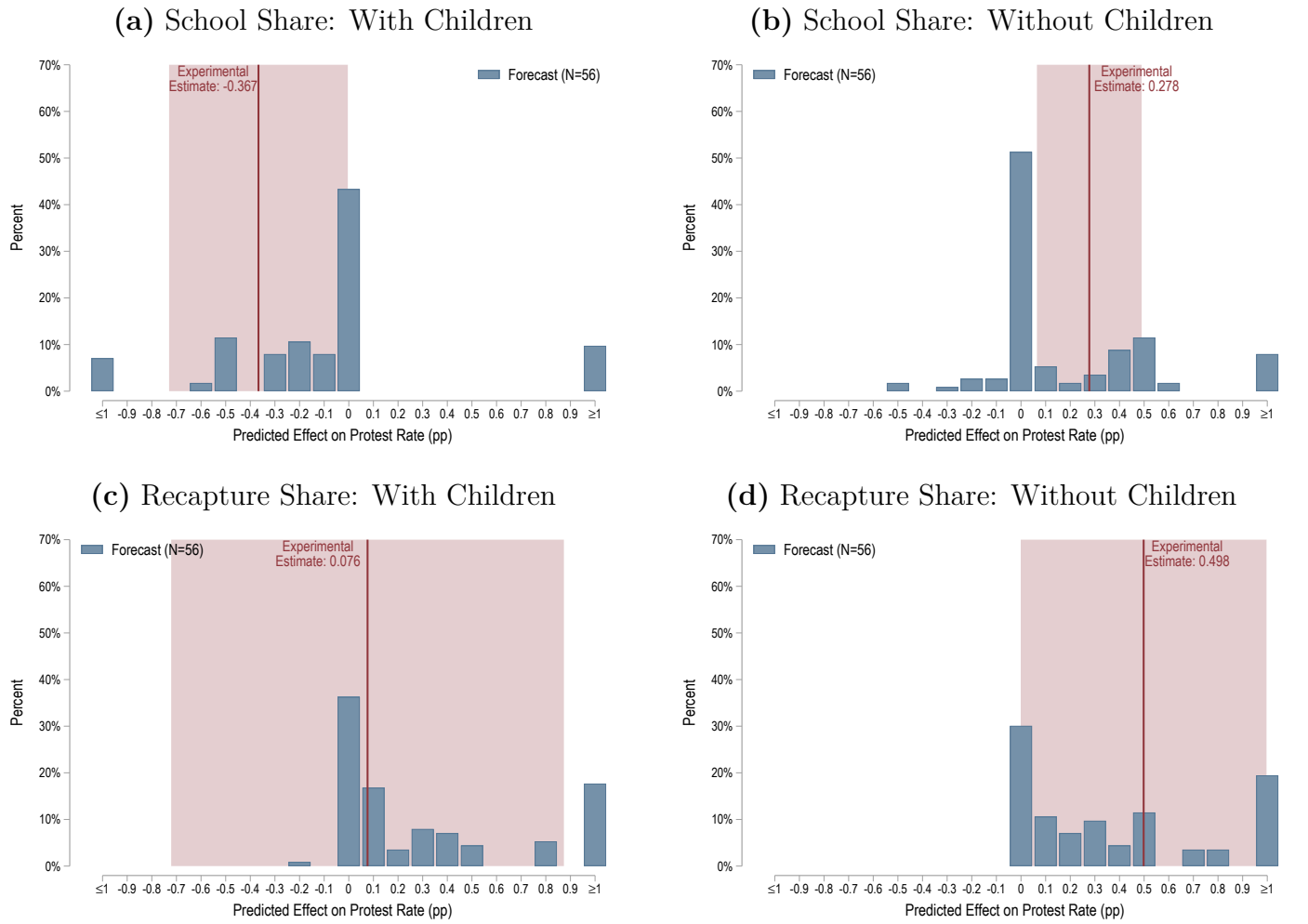
Notes: In panels (a) through (d) we report the relation between the school/recapture shares belief gap - grouped in 20 equally-sized bins - and the size of the update (i.e., posterior belief minus prior belief) by treatment status and broken down by receiving the school/recapture share feedback. Panel (a) shows how the school share feedback affects belief updates about the school share, which is the same as Figure 1(b). Panel (b) shows how the school share feedback affects belief updates about the recapture share. Panel (c) shows how the recapture share feedback affects belief updates about school share. Panel (d) shows how the recapture share feedback affects belief updates about the recapture share, which is the same as Figure 4(b). Gray squares represent the average update within each bin for the group of homeowners that were not selected into the school/recapture share feedback, while red circles do the same for homeowners that were selected. Each line corresponds to the fitted values from separate OLS regressions where the dependent variable is the update variable and the independent variable is the school/recapture share belief gap. The coefficient associated to the gap variable is reported in the upper left corner, as well as the robust standard errors, the p-value of the difference in the slopes, and the number of observations included in the analysis.

Figure B.7: The Effects of Recapture Share on Protests: Comparison to Expert Predictions



Notes: This figure shows the distribution of expert predictions of the effects of a 1 pp increase in the recapture share on the probability that a homeowner fills a protest directly for households *with* children (panel (a)) and household *without* children (panel (b)), based on the data collected in the forecast survey. To make the elicitation easier, in the prediction survey we asked subjects to predict the effects of a 10 pp increase in recapture share. In this figure, we divide those predictions by 10 in order to obtain the effect per 1 pp to make them directly comparable to the 2SLS estimates. In both panels, we group responses that were larger than 1 in absolute value. The vertical red line corresponds to the experimental estimate based on the 2SLS specification reported in rows "With Children" and "Without Children" of panel (b) in column (1) in Table 2, and the red shaded area corresponds to the 90% confidence interval. In the upper left corner we report the number of answers obtained in the forecast survey that were used in this figure. The entire questionnaire for the prediction survey can be found in Appendix I.

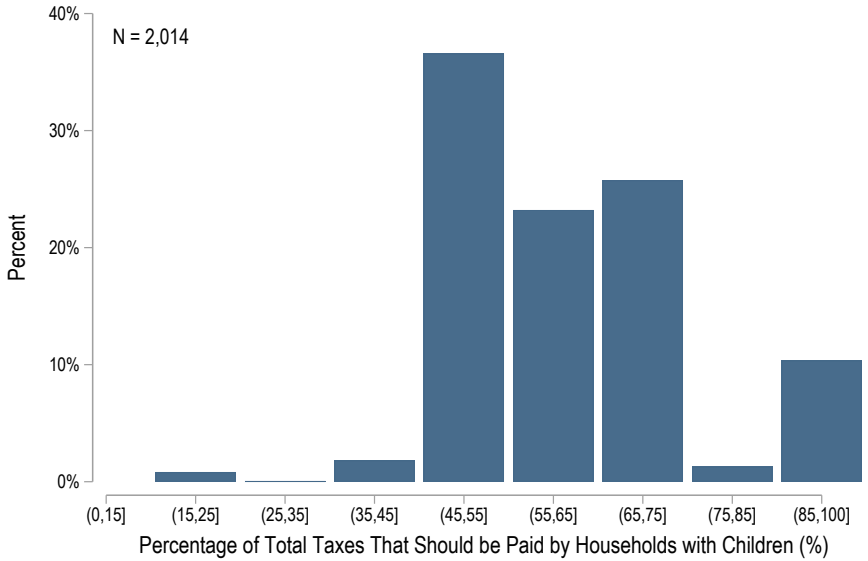
Figure B.8: The Effects of School Share and Net Recapture Information on Protests: Comparison to Weighted Expert Predictions



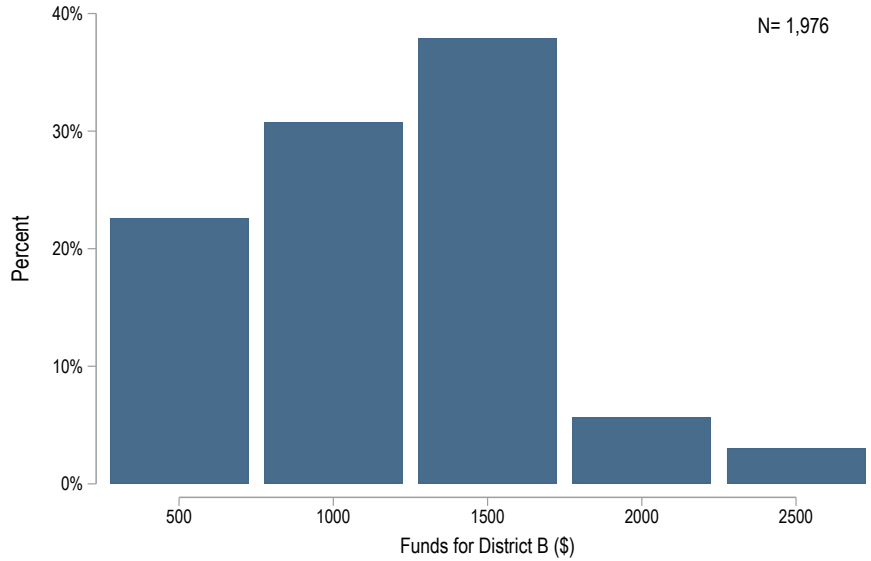
Notes: This figure shows the distribution of the weighted expert predictions of the effects of a 1 pp increase in the school/recapture shares on the probability that a homeowner fills a protest directly for households *with* children – panels (a) and (c) – and household *without* children – panels (b) and (d) –, based on the data collected in the forecast survey. The weights used to build the histograms correspond to the answers to a question about how certain the experts were about their prediction reported in the survey (the confidence variable equals 5 if extremely confident and 1 if not confident at all). To make the elicitation easier, in the prediction survey we asked subjects to predict the effects of a 10 pp increase in recapture share. In this figure, we divide those predictions by 10 in order to obtain the effect per 1 pp to make them directly comparable to the 2SLS estimates. In both panels, we group responses that were larger than 1 in absolute value. The vertical red line corresponds to the experimental estimate based on the 2SLS specification reported in rows "With Children" and "Without Children" of panel (b) in column (1) in Table 2, and the red shaded area corresponds to the 90% confidence interval. In the upper left corner we report the number of answers obtained in the forecast survey that were used in this figure. The entire questionnaire for the prediction survey can be found in Appendix I.

Figure B.9: Hypothetical Questions about Property Taxes

(a) Taxes and Public Schools



(b) Taxes and Recapture



Notes: Panel (a) shows the distribution of beliefs that survey respondents have about whether households *with* children should pay a larger share of property taxes. The question in the survey was as follows: *"Imagine the government gave you full power to choose the school property taxes that each household must pay as long as the total school property taxes collected stays the same. The home market value for Household A and Household B is the same: $200,000. However, Household A has two children in pubilc schools, while Household B has no children in public schools. What School property taxes would you choose for each home? These two values must add up to $8,000"*. Survey respondents typed the two values on a text entry field. The x-axis reports the share of school taxes that should be paid by households with children according to these responses. The y-axis presents the share of the responses choosing each option, while the note in the upper left corner shows the total number of homeowners that answered this question (96 missing answers). Panel (b) shows the distribution of beliefs that survey respondents have about whether school taxes should be redistributed between school districts. The question in the survey was as follows: *"Imagine the government gave you full power to choose how to redistribute property taxes between two hypothetical school districts: School District A and School District B. Both districts have the same number of households, same number of students, and the tax rates are the same too. However, homes in School District A are more expensive ($500,000 each) than homes in School District B ($100,000 each). In School District A, households paid $2,500 each in school taxes. In School District B, households paid $500 each in school taxes. How would you distribute these funds?"*. In this case, survey respondents had to choose between five options: (A gets: $2,500, B gets: $500), (A gets: $2,000, B gets: $1000), (A gets: $1,500, B gets: $1,500), (A gets $1,000, B gets: $2,000). The x-axis in panel (b) reports each one of these options, while the y-axis presents the share of the responses choosing each option. The note in the upper right corner shows the total number of homeowners that answered this question (134 missing values).

Table B.1: Descriptive Statistics: Comparison Across Samples

	Universe (1)	Letter Sample (2)	Survey Sample (3)	No Response Sample (4)
2021 Home Value ($1,000s)	327.69	359.15	349.99	357.59
	(0.65)	(1.63)	(6.77)	(1.67)
2021 Property Tax Amount ($1,000s)	6.37	7.65	7.74	7.61
	(0.01)	(0.03)	(0.13)	(0.03)
School Share (%)	49.78	50.60	50.73	50.58
	(0.02)	(0.02)	(0.08)	(0.02)
Recapture Share (%)	-1.96	1.23	1.62	1.12
	(0.02)	(0.07)	(0.33)	(0.07)
2021 Owner Protested (%)	3.81	5.99	30.76	5.08
	(0.03)	(0.08)	(1.00)	(0.08)
2021 Agent Protested (%)	7.58	7.24	4.45	7.12
	(0.04)	(0.09)	(0.45)	(0.09)
2020 Owner Protested (%)	7.99	8.84	18.10	8.51
	(0.04)	(0.10)	(0.84)	(0.10)
2020 Agent Protested (%)	8.04	6.31	1.61	6.26
	(0.04)	(0.09)	(0.27)	(0.09)
Multiple Owners (%)	22.17	23.89	24.64	23.82
	(0.07)	(0.15)	(0.94)	(0.16)
Living Area (Sq. Feet)	2.05	2.18	2.31	2.17
	(0.00)	(0.00)	(0.02)	(0.00)
Number of Bedrooms	3.12	3.35	3.43	3.34
	(0.00)	(0.00)	(0.02)	(0.00)
Number of Baths	2.06	2.17	2.27	2.16
	(0.00)	(0.00)	(0.02)	(0.00)
Observations	400,192	78,128	2,110	75,148

Notes: Average for different pre-treatment characteristics of the homeowner properties disaggregated by sample, based on administrative records available at the DCAD website. Column (1) corresponds to the universe of non-commercial, owner-occupied residences that pay property taxes. Column (2) corresponds to homeowners that were selected to receive a letter with the invitation to answer the survey. Column (3) corresponds to homeowners that answered the survey and belong to the subject pool used in our preferred specifications for the main analysis. Column (4) corresponds to the homeowners that did not answer the survey. Observations in column (3) and (4) do not add up to the number of observations in (2) because we excluded some answers based on homeowners responses to the survey (e.g., excluding homeowners that did not answer a set of key questions or had extreme misperceptions). Further details about how each of these samples was constructed can be found in Appendix B.2.

Table B.2: Effects of the Raffle Message on Survey Participation

	Treatment Arm		
	No Raffle (1)	Raffle (2)	p-value test (3)
Started Survey (%)	0.037	0.039	0.047
	(0.001)	(0.001)	
a. Admin. Records Variables:			
2021 Home Value ($1,000)	390.653	404.972	0.326
	(9.764)	(10.723)	
2021 Property Tax Amount ($1,000s)	8.407	8.634	0.381
	(0.177)	(0.187)	
School Share (%)	50.976	51.021	0.762
	(0.103)	(0.106)	
Recapture Share (%)	-3.566	-4.476	0.178
	(0.482)	(0.474)	
2020 Owner Protested (%)	17.692	16.862	0.550
	(1.009)	(0.956)	
2020 Agent Protested (%)	7.203	7.878	0.487
	(0.684)	(0.688)	
2019 Owner Protested (%)	11.678	13.346	0.171
	(0.850)	(0.868)	
2018 Owner Protested (%)	13.217	11.784	0.238
	(0.896)	(0.823)	
2017 Owner Protested (%)	9.650	10.807	0.299
	(0.781)	(0.792)	
2016 Owner Protested (%)	8.112	8.333	0.826
	(0.722)	(0.705)	
Multiple Owners (%)	26.643	24.609	0.205
	(1.169)	(1.099)	
Living Area (1,000s Sq. Feet)	2.397	2.406	0.815
	(0.028)	(0.028)	
Number of Bedrooms	3.445	3.489	0.118
	(0.020)	(0.020)	
Number of Baths	2.345	2.346	0.976
	(0.023)	(0.022)	
b. Survey Variables:			
With Children (%)	24.613	27.630	0.082
	(1.230)	(1.217)	
Female (%)	42.727	43.018	0.882
	(1.427)	(1.357)	
Age	50.049	49.337	0.072
	(0.292)	(0.269)	
Race: White (%)	41.701	43.717	0.307
	(1.425)	(1.361)	
Education: Grad. Degree (%)	38.282	40.105	0.349
	(1.404)	(1.345)	
Prior Belief: School Share (%)	37.042	37.717	0.392
	(0.564)	(0.549)	
Prior Belief: Recapture Share (%)	-2.604	-3.072	0.518
	(0.500)	(0.519)	

Notes: The first row reports the share of homeowners that started the survey (defined as answering at least the first two survey questions). Panel (a) reports averages of pre-treatment characteristics from the DCAD administrative data and panel (b) reports homeowner characteristics and prior beliefs from the survey. Columns (1) and (2) report the statistics for the samples of homeowners that were not selected and were selected to receive the raffle message, respectively. Column (3) reports the p-value of a test in which the null hypothesis is that the mean is equal for the two groups. In the first row, the number of observations is 39,064 for column (1) and 39,064 for column (2) because the starting rate is calculated over the full letter sample. In panels (a) and (b) the averages are calculated over the sample that started the survey (N=2,966). Because the purpose of this table is on the response rate, we did not apply the additional filters we used for the subject pool (N=2,110). In these panels, the number of observations varies depending on missing answers, ranging between N=2,528 and N=2,966. Standard errors are reported in parentheses.

Table B.3: Response Rates by Treatment Arm

	No Feedback (1)	School Feedback (2)	Recap Feedback (3)	Both Feedback (4)	p-value (5)
a. Invited to the survey					
Total	19,513	19,551	19,551	19,513	
b. Started the survey					
Total	734	739	759	734	
%	3.76	3.78	3.88	3.76	0.912
c. Answered Key Part of Survey					
Total	690	702	723	706	
%	3.54	3.59	3.70	3.62	0.858
d. Final Survey Sample					
Total	519	513	540	538	
%	2.66	2.62	2.76	2.76	0.377

Notes: This table describes the participation of homeowners at different stages of the survey as well as the composition of the final survey sample (or subject pool). Panel (a) reports the total number of subjects invited to the survey by treatment arm. Panel (b) reports the number of subjects that started the survey (i.e., answered at least the first two questions of the survey (N = 2,966). Panel (c) reports the number of answers that contained non-missing values for the two key modules of the survey (i.e., prior and posterior beliefs about the school and recapture share (N = 2,821). Panel (d) reports the number of answers by treatment arm that comprise the final survey sample as explained in Appendix B.2 (N=2,110). The percentages reported in panels (b), (c) and (d) are calculated using the number of homeowners in panel (a) as the denominator. Column (1) shows statistics from homeowners selected into the no feedback treatment arm, column (2) from homeowners selected into the school feedback treatment arm, column (3) from homeowners selected into the recapture feedback treatment arm, and column (4)from homeowners selected into both the school feedback and recapture feedback treatment arms. Column (5) reports the p-value of a test of equality of the rates reported in each row.

Table B.4: Conditional Dropout Rate (%) by Treatment Group

	Treatment Group				
	None (1)	Recapture Feedback (2)	School Feedback (3)	Both Feedback (4)	p-value test (5)
Already filled protest	0.445	0.287	0.445	0.149	0.741
	(0.257)	(0.203)	(0.257)	(0.149)	
With Children	0.149	0.000	0.596	0.000	0.033
	(0.149)	(0.000)	(0.297)	(0.000)	
Prior: School Share	1.493	1.153	1.499	0.893	0.708
	(0.469)	(0.405)	(0.471)	(0.363)	
Posterior: School Share	0.455	0.729	0.761	0.901	0.806
	(0.262)	(0.325)	(0.339)	(0.366)	
Prior: Recap (+/-)	1.065	0.294	0.153	0.152	0.027
	(0.401)	(0.208)	(0.153)	(0.152)	
Prior: Recap (%)	0.923	0.884	0.307	0.152	0.142
	(0.375)	(0.359)	(0.217)	(0.152)	
Posterior: Recap (+/-)	1.553	1.189	1.695	1.216	0.828
	(0.488)	(0.418)	(0.507)	(0.428)	
Posterior: Recap (%)	0.473	0.451	0.470	0.154	0.747
	(0.273)	(0.260)	(0.271)	(0.154)	
Satisfaction with Govt. Services	0.158	0.453	0.157	0.308	0.696
	(0.158)	(0.261)	(0.157)	(0.218)	
Support for Recapture	0.158	0.152	0.000	0.309	0.579
	(0.158)	(0.152)	(0.000)	(0.218)	
Fair Amount of Prop. Taxes	0.159	0.152	0.631	0.155	0.265
	(0.159)	(0.152)	(0.315)	(0.155)	
Property Taxes are Too high	0.159	0.000	0.000	0.155	0.568
	(0.159)	(0.000)	(0.000)	(0.155)	
Intention to Protest in 2021	0.318	0.304	0.635	0.156	0.531
	(0.225)	(0.215)	(0.317)	(0.156)	
Taxes-Public Goods Trade-off	0.479	0.458	0.319	0.467	0.970
	(0.276)	(0.264)	(0.226)	(0.269)	
Gender	0.000	0.153	0.000	0.156	0.587
	(0.000)	(0.153)	(0.000)	(0.156)	
Age	0.000	0.000	0.000	0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
Race	0.000	0.461	0.481	0.157	0.283
	(0.000)	(0.266)	(0.277)	(0.157)	
Education	0.000	0.000	0.000	0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
Political party	0.000	0.000	0.000	0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
Govt. Action	0.803	1.235	0.805	1.099	0.822
	(0.358)	(0.434)	(0.359)	(0.413)	
HH with Kids Should Pay More	1.294	1.875	1.461	1.905	0.784
	(0.455)	(0.537)	(0.484)	(0.545)	
Redistribution from Rich ISDs to Poor ISDs	2.131	2.389	2.142	1.456	0.683
	(0.585)	(0.610)	(0.588)	(0.482)	

Notes: The conditional dropout rate (measured in percentage points) is defined as the percentage of missing responses to one question conditional on having answered the previous question in the survey. Each row in the table represents a question of the survey following the order in which they are included. The full survey questionnaire is included in Appendix H. Column (1) shows statistics from homeowners selected into the no feedback treatment arm, column (2) from homeowners selected into the school feedback treatment arm, column (3) from homeowners selected into the recapture feedback treatment arm, and column (4)from homeowners selected into both the school feedback and recapture feedback treatment arms. Column (5) reports the p-value of a test of equality of the conditional dropout rates across groups in each row. Standard errors are reported in parentheses. The starting number of observations for this table is 2,717 – 674 corresponding to the group *No Feedback*, 696 to *Recapture Feedback*, 674 to *School Feedback*, and 673 to *Both Feedback*. These 2,717 subjects are all the homeowners who started the survey, excluding those who had a tax representative before the protest campaign started, that protested already at the moment of starting the survey, or started the survey after the protest deadline.

Table B.5: Balance Checks Survey Sample: By Treatment Arm

	Households Without Children					Households With Children				
	No Feedback (1)	Recapture Feedback (2)	School Feedback (3)	Both Feedback (4)	p-value test (5)	No Feedback (6)	Recapture Feedback (7)	School Feedback (8)	Both Feedback (9)	p-value test (10)
a. Admin. Records Variables:										
2021 Home Value ($1,000)	343.687	305.300	329.996	314.840	0.105	432.236	405.146	463.764	412.564	0.724
	(14.231)	(9.918)	(12.309)	(10.579)		(41.795)	(27.349)	(51.405)	(34.707)	
2021 Property Tax Amount ($1,000s$)	7.693	6.930	7.451	7.034	0.129	9.019	8.972	9.383	9.014	0.968
	(0.323)	(0.212)	(0.277)	(0.220)		(0.676)	(0.573)	(0.756)	(0.601)	
School Share (%)	50.264	50.105	50.461	50.789	0.037	51.650	51.922	51.375	51.717	0.687
	(0.181)	(0.171)	(0.176)	(0.179)		(0.285)	(0.355)	(0.317)	(0.326)	
Recapture Share (%)	0.593	-0.517	1.188	0.147	0.337	5.737	5.676	6.190	3.953	0.752
	(0.689)	(0.669)	(0.678)	(0.670)		(1.733)	(1.466)	(1.671)	(1.423)	
2020 Owner Protested (%)	22.194	15.633	16.931	13.033	0.006	25.984	12.409	28.148	19.424	0.007
	(2.102)	(1.811)	(1.931)	(1.688)		(3.907)	(2.827)	(3.885)	(3.368)	
2020 Agent Protested (%)	1.276	1.985	1.852	1.253	0.776	0.787	3.650	1.481	1.439	0.329
	(0.568)	(0.696)	(0.694)	(0.558)		(0.787)	(1.608)	(1.044)	(1.014)	
2019 Owner Protested (%)	15.816	12.407	12.963	14.536	0.502	12.598	6.569	17.037	10.791	0.060
	(1.845)	(1.644)	(1.730)	(1.767)		(2.956)	(2.124)	(3.248)	(2.641)	
2018 Owner Protested (%)	13.265	11.663	15.079	13.784	0.569	14.961	14.599	14.074	10.791	0.736
	(1.715)	(1.601)	(1.843)	(1.728)		(3.178)	(3.028)	(3.004)	(2.641)	
2017 Owner Protested (%)	10.714	11.414	10.317	9.023	0.729	14.173	10.219	16.296	8.633	0.196
	(1.564)	(1.586)	(1.567)	(1.436)		(3.107)	(2.597)	(3.191)	(2.391)	
2016 Owner Protested (%)	7.398	6.203	7.143	8.020	0.795	11.811	8.029	11.111	7.194	0.495
	(1.324)	(1.203)	(1.326)	(1.361)		(2.875)	(2.330)	(2.715)	(2.200)	
Multiple Owners (%)	21.939	22.829	24.339	22.306	0.867	25.984	29.197	27.407	36.691	0.221
	(2.093)	(2.093)	(2.210)	(2.087)		(3.907)	(3.899)	(3.853)	(4.103)	
Living Area (1,000s Sq. Feet)	2.248	2.192	2.226	2.211	0.846	2.527	2.628	2.625	2.563	0.871
	(0.050)	(0.045)	(0.048)	(0.043)		(0.104)	(0.097)	(0.110)	(0.091)	
Number of Bedrooms	3.360	3.283	3.347	3.393	0.165	3.654	3.737	3.637	3.647	0.712
	(0.036)	(0.036)	(0.037)	(0.034)		(0.064)	(0.068)	(0.072)	(0.070)	
Number of Baths	2.227	2.191	2.222	2.193	0.852	2.417	2.511	2.489	2.424	0.794
	(0.037)	(0.035)	(0.042)	(0.034)		(0.078)	(0.078)	(0.087)	(0.075)	
b. Survey Variables:										
Female (%)	45.361	48.354	43.127	44.557	0.517	43.548	30.370	35.075	34.074	0.160
	(2.531)	(2.518)	(2.575)	(2.504)		(4.471)	(3.973)	(4.138)	(4.094)	
Age	51.057	50.775	51.790	50.071	0.192	45.500	45.304	46.694	45.652	0.526
	(0.547)	(0.582)	(0.550)	(0.567)		(0.807)	(0.709)	(0.746)	(0.636)	
Race: White (%)	45.361	52.041	49.593	42.132	0.028	42.742	35.556	30.075	34.815	0.208
	(2.531)	(2.527)	(2.606)	(2.491)		(4.461)	(4.135)	(3.991)	(4.115)	
Education: Grad. Degree (%)	38.402	35.459	35.230	36.041	0.788	44.355	44.444	47.368	40.741	0.754
	(2.472)	(2.419)	(2.490)	(2.422)		(4.480)	(4.293)	(4.346)	(4.245)	
Prior Belief: School Share (%)	38.534	37.168	38.448	38.178	0.688	35.291	37.237	36.500	36.427	0.870
	(0.924)	(0.862)	(0.903)	(0.915)		(1.624)	(1.599)	(1.619)	(1.643)	
Prior Belief: Recapture Share (%)	0.636	0.625	1.733	0.907	0.528	5.390	3.569	6.341	3.471	0.377
	(0.651)	(0.508)	(0.622)	(0.611)		(1.588)	(1.305)	(1.393)	(1.288)	
Observations	392	403	378	399		127	137	135	139	

Notes: This table lists pre-treatment characteristics averages. Statistics are based on the 2,110 homeowners that comprise the subject pool. Standard errors are reported in parentheses. The statistics in panel (a) are based on administrative records available at the DCAD website. The statistics in panel (b) are based on survey responses. Columns (1) through (5) are based on the sub-group of households that do not have children attending a public school. Column (1) is based on homeowners not selected to receive any information. Column (2) is based on homeowners selected to receive information on the recapture share. Column (3) is based on homeowners selected to receive information on the school share. Column (4) is based on homeowners selected to receive information on both the recapture share and the school share. Column (5) reports the p-value of a test of equal means across the four treatment groups. The statistics in columns (6) through (10) are analogous to those in columns (1) through (5) but for the group of households with children attending a public school.

Table B.6: Main Results: Reduced-Form and First-Stage Estimates

	2SLS		Reduced Form		First Stage	
Dep. Var.:	P_D^{2021}	I^{2021}	P_D^{2021}	I^{2021}	s^{post}	r^{post}
	(1)	(2)	(3)	(4)	(5)	(6)
a. Effect of School Share						
With Children	-0.367*	-0.408*				
	(0.221)	(0.234)				
Without Children	0.277**	0.269*				
	(0.129)	(0.144)				
(Difference Children - No Children)	-0.644**	-0.678**				
	(0.256)	(0.275)				
b. Effect of Recapture Share						
With Children	0.076	-0.313				
	(0.485)	(0.541)				
Without Children	0.498	-0.101				
	(0.303)	(0.325)				
(Difference Children - No Children)	-0.422	-0.212				
	(0.563)	(0.620)				
c. Effect of School Share Belief Gap * Dummy Feedback School Shown						
With Children			-0.269*	-0.288*	0.736***	-0.033
			(0.161)	(0.172)	(0.044)	(0.048)
Without Children			0.223**	0.217*	0.808***	-0.004
			(0.105)	(0.117)	(0.026)	(0.025)
(Difference Children - No Children)			-0.492**	-0.506**	-0.072	-0.028
			(0.192)	(0.208)	(0.051)	(0.054)
d. Effect of Recapture Share Belief Gap * Dummy Feedback School Shown						
With Children			0.061	-0.157	-0.006	0.529***
			(0.237)	(0.267)	(0.055)	(0.067)
Without Children			0.273	-0.060	-0.051*	0.550***
			(0.176)	(0.190)	(0.031)	(0.051)
(Difference Children - No Children)			-0.213	-0.097	0.045	-0.021
			(0.295)	(0.329)	(0.063)	(0.084)
Mean Outcome (Baseline)						
With Children	33.86	47.20	33.86	47.20	36.58	5.78
Without Children	28.83	44.87	28.83	44.87	40.00	-0.02
Observation	2,110	2,090	2,110	2,090	2,110	2,110

Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. Panel (a) shows the estimates corresponding to the school share treatment effect. We present the coefficients for households *with* children and households *without children* separately, as well as the difference between these two types of households. Panel (b) reports analogous results but for the recapture share treatment effects. The dependent variable in column (1) is an indicator variable that takes the value 100 if the subject protested directly in 2021. The dependent variable in column (2) is an indicator variable that takes the value 100 if the subject answered "very likely" to the question on the likelihood to protest in 2021. Estimates reported are based on the 2SLS econometric model given by model 21 and discussed in Section 5.3. These results are the same as the ones reported in Table 2. Panels (c) and (d) report the effect of receiving the school/recapture share feedback interacted with the school/recapture share belief gap. Columns (3) and (4) use the same outcome variables as columns (1) and (2), and therefore can be interpreted as the reduced form estimates (i.e., a regression of the outcome variable over the instrument). Columns (5) and (6) use the school/recapture share posterior beliefs as the dependent variable, and therefore can be interpreted as the first-stage results. Estimates reported in columns (3) through (6) are based on OLS regressions and use the same set of control variables as the main specification discussed in Section 5.3. Mean outcomes at baseline correspond to the mean of the dependent variables computed using the group of subjects who did not receive feedback about the school share nor recapture share.

Table B.7: Heterogeneous Effects by Gender and Age

	Dep.Var.: P_D^{2021}						Dep.Var.: I_D^{2021}					
	Female			Younger than 50 years old			Female			Younger than 50 years old		
	Yes (1)	No (2)	p-value difference (3)	Yes (4)	No (5)	p-value difference (6)	Yes (7)	No (8)	p-value difference (9)	Yes (10)	No (11)	p-value difference (12)
a. Effects of School Share:												
With Children	-0.224	-0.564**	0.471	-0.448*	-0.431	0.982	-0.114	-0.730**	0.229	-0.493*	-0.627	0.874
	(0.375)	(0.286)		(0.246)	(0.720)		(0.393)	(0.328)		(0.266)	(0.809)	
Without Children	0.206	0.347**	0.593	0.378*	0.202	0.513	0.095	0.480**	0.189	0.240	0.332*	0.754
	(0.203)	(0.170)		(0.208)	(0.171)		(0.224)	(0.189)		(0.222)	(0.192)	
Difference (Children - No Children)	-0.429	-0.911***	0.373	-0.826***	-0.633	0.810	-0.209	-1.210***	0.090	-0.733**	-0.959	0.801
	(0.428)	(0.331)		(0.319)	(0.737)		(0.453)	(0.378)		(0.345)	(0.828)	
b. Effects of Recapture Share:												
With Children	0.694	-0.325	0.329	-0.207	0.612	0.380	0.213	-0.993	0.308	-0.686	0.668	0.203
	(0.843)	(0.617)		(0.638)	(0.680)		(0.908)	(0.759)		(0.721)	(0.782)	
Without Children	-0.015	0.774*	0.210	0.407	0.489	0.892	-0.606	0.310	0.158	0.003	-0.322	0.615
	(0.432)	(0.458)		(0.449)	(0.406)		(0.445)	(0.472)		(0.467)	(0.443)	
Difference (Children - No Children)	0.709	-1.099	0.131	-0.614	0.123	0.493	0.819	-1.304	0.106	-0.689	0.989	0.164
	(0.922)	(0.762)		(0.746)	(0.774)		(0.978)	(0.878)		(0.821)	(0.884)	
Mean Outcome (Baseline):												
With Children	38.89	31.43		32.65	42.31		51.85	44.29		44.90	57.69	
Without children	32.95	25.94		32.93	26.24		47.73	42.92		44.31	45.70	
Observations	891	1,186		1,051	1,026		891	1,186		1,051	1,026	

Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. Estimates reported are based on the 2SLS econometric model given by model 21 and discussed in Section 5.3. Panel (a) shows the estimates corresponding to the school share treatment effect. We present the coefficients for households *with* and *without* children separately, as well as the difference between these two types of households. Panel (b) reports analogous results but for the recapture share treatment effects. For each dependent variable we report heterogeneity results based on two demographic variables: a dummy variable that indicates whether the homeowner is female – columns (1), (2), (7), and (8) –, and a dummy variable for homeowners younger than fifty years old – columns (4), (5), (10), and (11). Columns (3), (6), (9), and (12) report the p-value of the difference in the coefficients for the corresponding groups under the assumption that coefficients are uncorrelated across the 2SLS regressions. Mean outcomes at baseline correspond to the mean of the dependent variables computed using the group of subjects who did not receive feedback about the school share nor recapture share.

Table B.8: Heterogeneous Effects by Education and Political Party

	Dep.Var.: P_D^{2021}						Dep.Var.: I_D^{2021}					
	College Degree			Political Party			College Degree			Political Party		
	Yes (1)	No (2)	p-value difference (3)	Dem. (4)	Rep. (5)	p-value difference (6)	Yes (7)	No (8)	p-value difference (9)	Dem. (10)	Rep. (11)	p-value difference (12)
a. Effects of School Share:												
With Children	-0.139	-0.617**	0.292	-0.224	-0.583*	0.412	-0.295	-0.502	0.679	-0.407	-0.376	0.951
	(0.342)	(0.299)		(0.319)	(0.300)		(0.389)	(0.316)		(0.342)	(0.359)	
Without Children	0.340	0.221	0.691	0.054	0.524***	0.076	0.258	0.277	0.955	0.290	0.290	0.999
	(0.251)	(0.161)		(0.175)	(0.199)		(0.285)	(0.173)		(0.193)	(0.226)	
Difference (Children - No Children)	-0.479	-0.839**	0.506	-0.278	-1.107***	0.106	-0.553	-0.779**	0.706	-0.696*	-0.666	0.958
	(0.419)	(0.341)		(0.364)	(0.361)		(0.477)	(0.362)		(0.390)	(0.424)	
b. Effects of Recapture Share:												
With Children	0.347	-0.117	0.667	0.194	0.335	0.886	-0.509	-0.114	0.740	0.538	-1.124	0.143
	(0.507)	(0.953)		(0.690)	(0.705)		(0.543)	(1.059)		(0.769)	(0.837)	
Without Children	0.466	0.466	1.000	0.197	0.606	0.504	-0.034	0.016	0.951	-0.036	-0.314	0.669
	(0.659)	(0.337)		(0.451)	(0.414)		(0.713)	(0.363)		(0.470)	(0.454)	
Difference (Children - No Children)	-0.119	-0.583	0.710	-0.003	-0.271	0.813	-0.475	-0.129	0.801	0.574	-0.810	0.281
	(0.726)	(1.016)		(0.828)	(0.778)		(0.793)	(1.116)		(0.908)	(0.908)	
Mean Outcome (Baseline):												
With Children	30.91	37.68		38.71	30.65		40.00	53.62		53.23	41.94	
Without children	30.87	28.03		30.17	28.23		46.31	44.35		45.81	44.50	
Observations	793	1,277		1,042	1,028		793	1,277		1,042	1,028	

Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. Estimates reported are based on the 2SLS econometric model given by model 21 and discussed in Section 5.3. Panel (a) shows the estimates corresponding to the school share treatment effect. We present the coefficients for households *with* and *without* children separately, as well as the difference between these two types of households. Panel (b) reports analogous results but for the recapture share treatment effects. For each dependent variable we report heterogeneity results based on two demographic variables: a dummy variable that indicates whether the homeowner has a college degree – columns (1), (2), (7), and (8) –, and a dummy variable for homeowners self-identified with the democratic party – columns (4), (5), (10), and (11). Columns (3), (6), (9), and (12) report the p-value of the difference in the coefficients for the corresponding groups under the assumption that coefficients are uncorrelated across the 2SLS regressions. Mean outcomes at baseline correspond to the mean of the dependent variables computed using the group of subjects who did not receive feedback about the school share nor recapture share.

Table B.9: Heterogeneous Effects by Sign of the Prior Belief Gap

	Dep.Var.: P_D^{2021}						Dep.Var.: I_D^{2021}					
	Underestimated Share School			Underestimated Net Recapture			Underestimated Share School			Underestimated Net Recapture		
	Yes (1)	No (2)	p-value difference (3)	Yes (4)	No (5)	p-value difference (6)	Yes (7)	No (8)	p-value difference (9)	Yes (10)	No (11)	p-value difference (12)
a. Effects of School Share:												
With Children	-0.324	-0.431	0.911	-0.087	-0.441	0.480	-0.410*	-0.507	0.933	-0.280	-0.398	0.809
	(0.224)	(0.931)		(0.387)	(0.317)		(0.238)	(1.126)		(0.342)	(0.344)	
Without Children	0.293**	-0.538	0.287	0.312	0.283*	0.918	0.288**	-0.485	0.348	0.207	0.314*	0.723
	(0.131)	(0.770)		(0.227)	(0.164)		(0.145)	(0.811)		(0.233)	(0.190)	
Difference (Children - No Children)	-0.617**	0.107	0.570	-0.399	-0.724**	0.572	-0.698**	-0.022	0.635	-0.488	-0.712*	0.695
	(0.260)	(1.246)		(0.449)	(0.359)		(0.279)	(1.399)		(0.414)	(0.396)	
b. Effects of Recapture Share:												
With Children	0.032	0.129	0.945	-1.930	0.332	0.293	-0.202	-0.807	0.742	-1.626	-0.073	0.439
	(0.512)	(1.288)		(2.100)	(0.467)		(0.570)	(1.746)		(1.941)	(0.513)	
Without Children	0.327	0.934	0.413	1.089**	0.079	0.123	-0.079	-0.250	0.821	0.261	-0.360	0.352
	(0.346)	(0.657)		(0.520)	(0.399)		(0.376)	(0.652)		(0.497)	(0.445)	
Difference (Children - No Children)	-0.295	-0.806	0.755	-3.019	0.253	0.153	-0.123	-0.558	0.832	-1.888	0.287	0.314
	(0.602)	(1.521)		(2.220)	(0.561)		(0.668)	(1.936)		(2.068)	(0.619)	
Mean Outcome (Baseline):												
With Children	32.69	39.13		27.87	39.39		48.04	43.48		42.62	51.56	
Without children	28.15	30.33		27.53	29.91		45.15	44.26		46.02	43.93	
Observations	1,525	585		959	1,151		1,507	583		947	1,143	

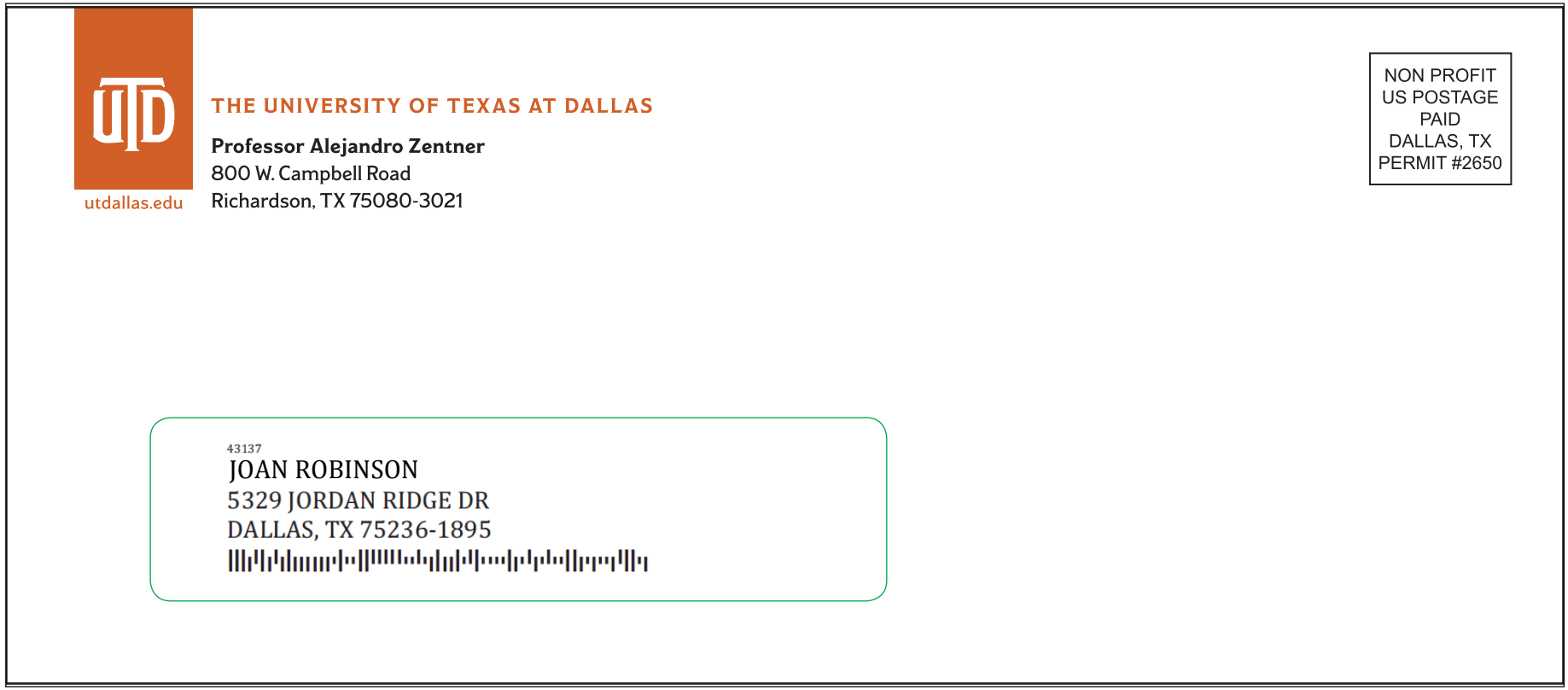
Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. Estimates reported are based on the 2SLS econometric model given by model 21 and discussed in Section 5.3. Panel (a) shows the estimates corresponding to the school share treatment effect. We present the coefficients for households *with* and *without* children separately, as well as the difference between these two types of households. Panel (b) reports analogous results but for the recapture share treatment effects. For each dependent variable we report heterogeneity results based on the direction of misperceptions in prior beliefs: a dummy variable that indicates whether the homeowner underestimated the share of school taxes – columns (1), (2), (7), and (8) –, and a dummy variable for homeowners that underestimated the recapture share – columns (4), (5), (10), and (11). Columns (3), (6), (9), and (12) report the p-value of the difference in the coefficients for the corresponding groups under the assumption that coefficients are uncorrelated across the 2SLS regressions. Mean outcomes at baseline correspond to the mean of the dependent variables computed using the group of subjects who did not receive feedback about the school share nor recapture share.

Table B.10: Effects on Survey Outcomes

	Insatisf. with Govt. Services (1:10)	Unfair Amount of Taxes (1:10)	Against Recapture (1:10)	Taxes Are Too High (-100,0,100)	Less Tax Worse Services (-100,0,100)
	(1)	(2)	(3)	(4)	(5)
a. Effects of School Share:					
With Children	0.000	0.015	-0.002	0.009	-0.252
	(0.013)	(0.012)	(0.013)	(0.177)	(0.288)
Without Children	0.009	0.007	0.011	-0.061	0.138
	(0.007)	(0.008)	(0.008)	(0.101)	(0.161)
Difference (Children - No Children)	-0.009	0.008	-0.013	0.070	-0.389
	(0.015)	(0.014)	(0.015)	(0.204)	(0.331)
b. Effects of Recapture Share:					
With Children	-0.038	-0.008	-0.005	0.330	-0.470
	(0.030)	(0.029)	(0.030)	(0.377)	(0.573)
Without Children	0.032**	0.023	0.043**	0.208	-0.082
	(0.016)	(0.018)	(0.017)	(0.233)	(0.382)
Difference (Children - No Children)	-0.069**	-0.030	-0.048	0.122	-0.388
	(0.033)	(0.033)	(0.034)	(0.436)	(0.678)
Mean Outcome (No Feedback):					
With Children	6.43	6.56	5.84	84.13	3.23
Without children	6.97	6.66	5.88	85.90	6.70
Observations	2,105	2,097	2,101	2,097	2,079

Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. Estimates reported are based on the 2SLS econometric model given by model 21 and discussed in Section 5.3. Panel (a) shows the estimates corresponding to the school share treatment effect. We present the coefficients for households *with* and *without* children separately, as well as the difference between these two types of households. Panel (b) reports analogous results but for the recapture share treatment effects. Column (1) reports the treatment effects on a variable that represent homeowners insatisfaction with government services. Column (2) is based on a question that asks whether relative to other households, the homeowner think they pays an unfair amount in property taxes. Column (3) reports the homeowners views when they are asked explicitly about the support for the recapture system. For these three questions, the potential answers are based on a scale from 1-10. To simplify the interpretation and comparison across the variables included in the Table, these variables were reversed from its original formulation of the question. Column (4) reports the treatment effect on a variable that asks whether the homeowner thinks that the amount of property taxes is too low, about right or too high. Column (5) captures whether individuals would prefer to pay less taxes and receive worse public services, hold everything else fixed, or to increase taxes and the quality of public services. Mean outcomes at baseline correspond to the mean of the dependent variables computed using the group of subjects who did not receive feedback about the school share nor recapture share. The questions used in the survey can be found in Appendix H

C Sample Envelope



D Sample of Full Letter

THE UNIVERSITY OF TEXAS AT DALLAS
Naveen Jindal School of Management

April 19th, 2021

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 2021 property taxes for your home at 123 Fake Street in Dallas County*:*

	YOUR HOME
Proposed Value	*$592,040*
Estimated Tax Amount	*$14,859*

Source: *Data provided by Dallas Central Appraisal District (CAD). Proposed Value is Dallas CAD's estimate of your home's market value as of January 1st, 2021. Estimated Tax Amount is our estimate of taxes due this year using the latest tax rates available (some exemptions might not be included).*

The deadline to protest is May 17th, 2021. If you would like to help us with our study, we kindly ask you to fill out the following short survey:

Visit http://www.utdallas.edu/taxsurvey/ and enter validation code **FF11FF**

Note: Please respond to our survey before you decide whether to file a protest or not.

At the end of our survey, we provide step-by-step instructions on how to file a protest online or by mail, if you wish to do so. It takes only a few minutes to respond to the survey, and your responses will be confidential. As a token of appreciation, everyone who responds to the survey will be entered into a raffle for 20 prizes of $100 each[1].

800 W Campbell Rd, Office 3206
Richardson, TX 75080

Please recycle

Your household was randomly chosen to receive this letter. We will not send you any letters in the future. If you have any questions about the study, you can find contact information on the study's website provided at the end of the survey.

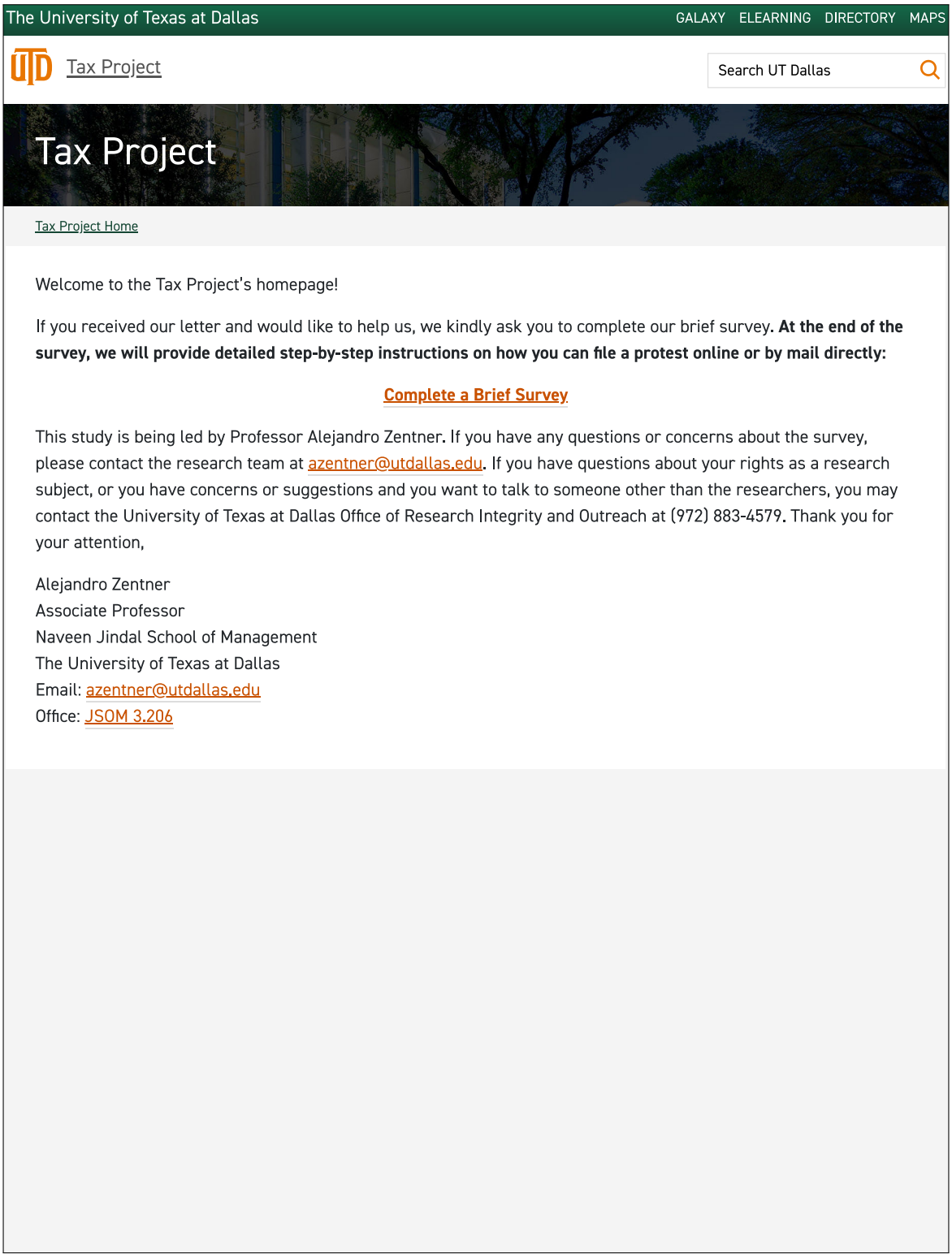
Thank you for your attention!

Alejandro Zentner

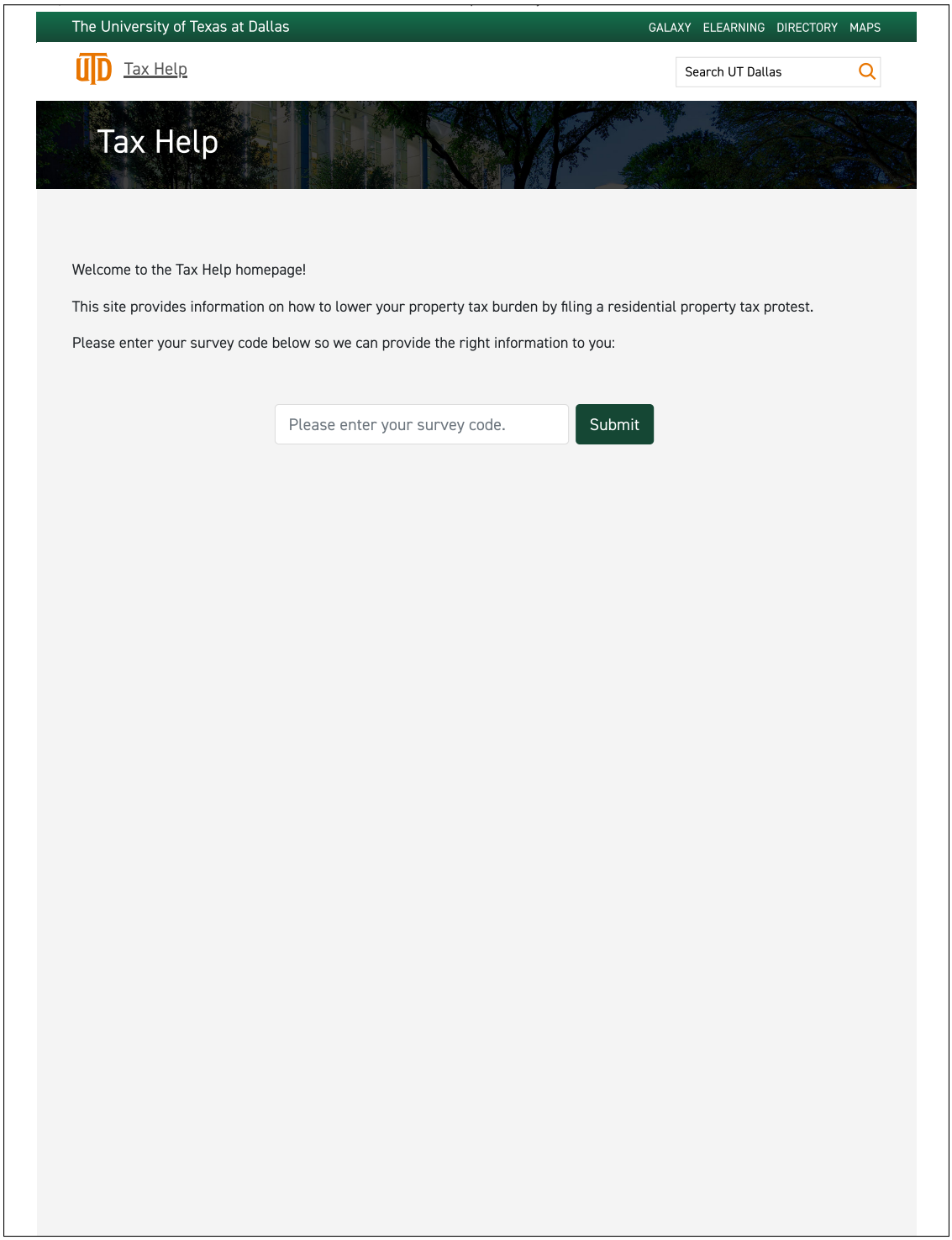
Associate Professor
The University of Texas at Dallas
https://www.utdallas.edu/taxproject/

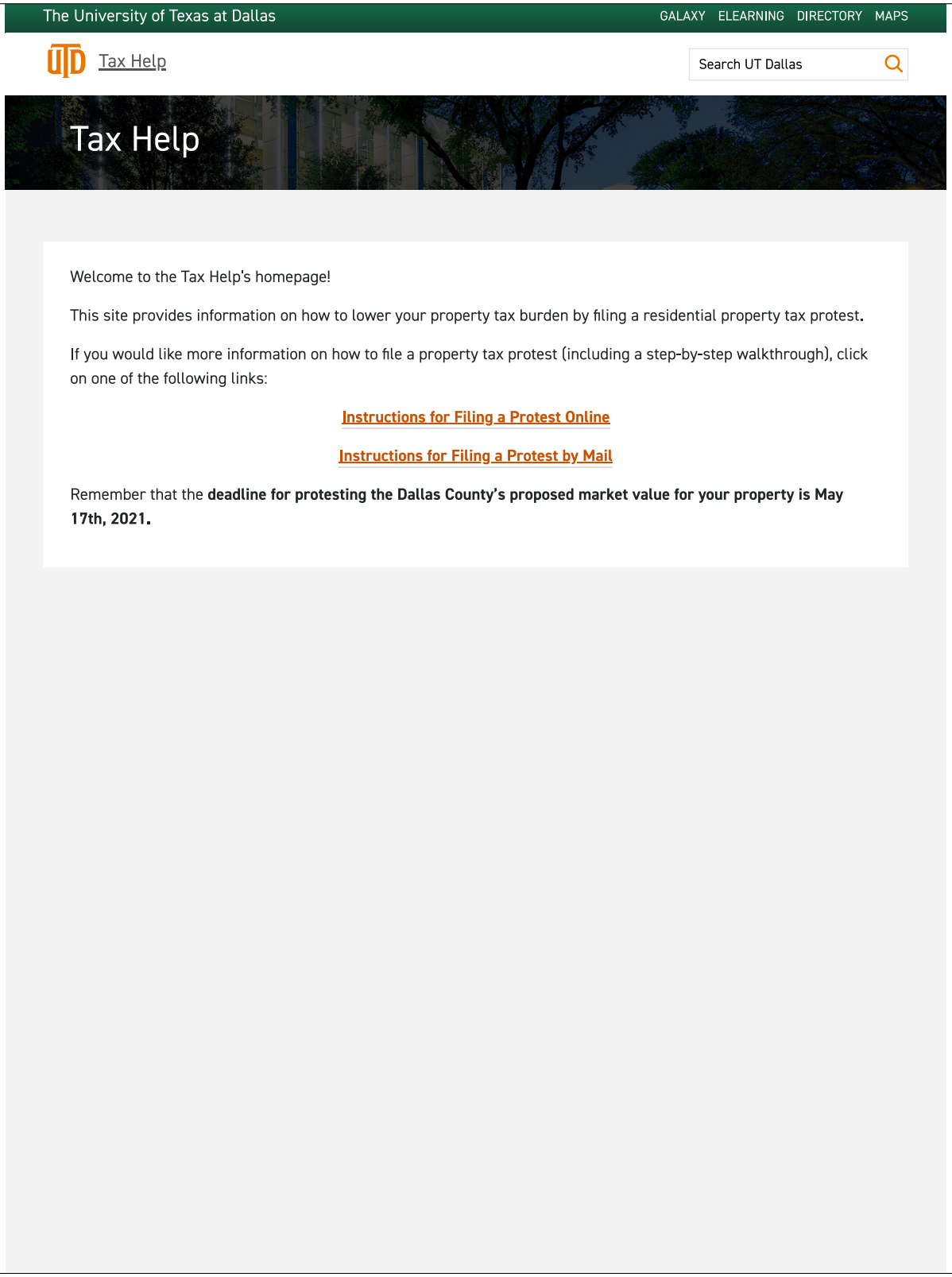
Joan Robinson
123 Fake Street

E Project's Website



F Tax Help Homepage




The University of Texas at Dallas
GALAXY ELEARNING DIRECTORY MAPS
Tax Help
Search UT Dallas
Tax Help
Welcome to the Tax Help's homepage!
This site provides information on how to lower your property tax burden by filing a residential property tax protest.
If you would like more information on how to file a property tax protest (including a step-by-step walkthrough), click on one of the following links:
Instructions for Filing a Protest Online
Instructions for Filing a Protest by Mail
Remember that the deadline for protesting the Dallas County's proposed market value for your property is May 17th, 2021.

The University of Texas at Dallas GALAXY ELEARNING DIRECTORY MAPS

Tax Project

Search UT Dallas

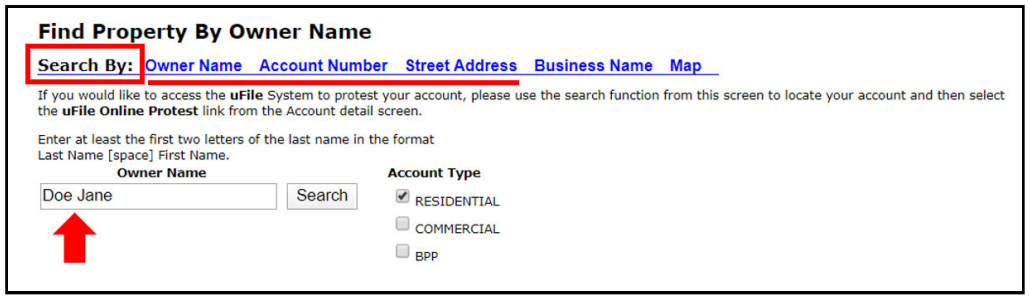
Walkthrough for Filing a Protest Online

Tax Project Home » Walkthrough for Filing a Protest Online

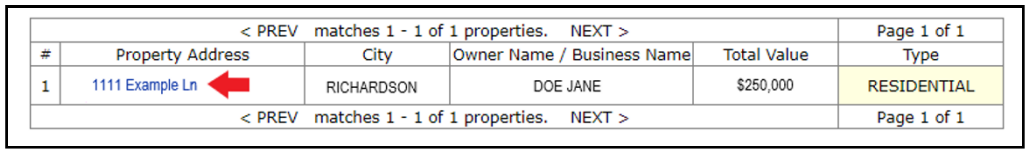
To file an online (uFile) protest related to your property in Dallas County, simply follow the steps below. You only need your property address or your name (account number not required).

Step 1. Enter the following URL into your internet browser. This opens the Dallas CAD Property Search webpage.
http://www.dallascad.org/SearchOwner.aspx

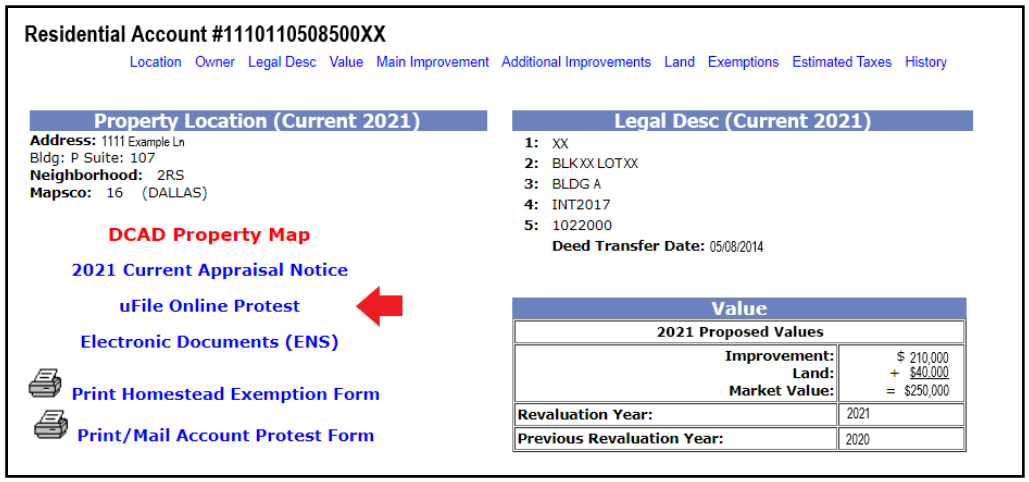
Step 2. Click the link at the top of the webpage to choose how you would like to search for your property. The options include by "Owner Name", "Account Number", or "Street Address". "Owner Name" searches must be done with your *last name* first.



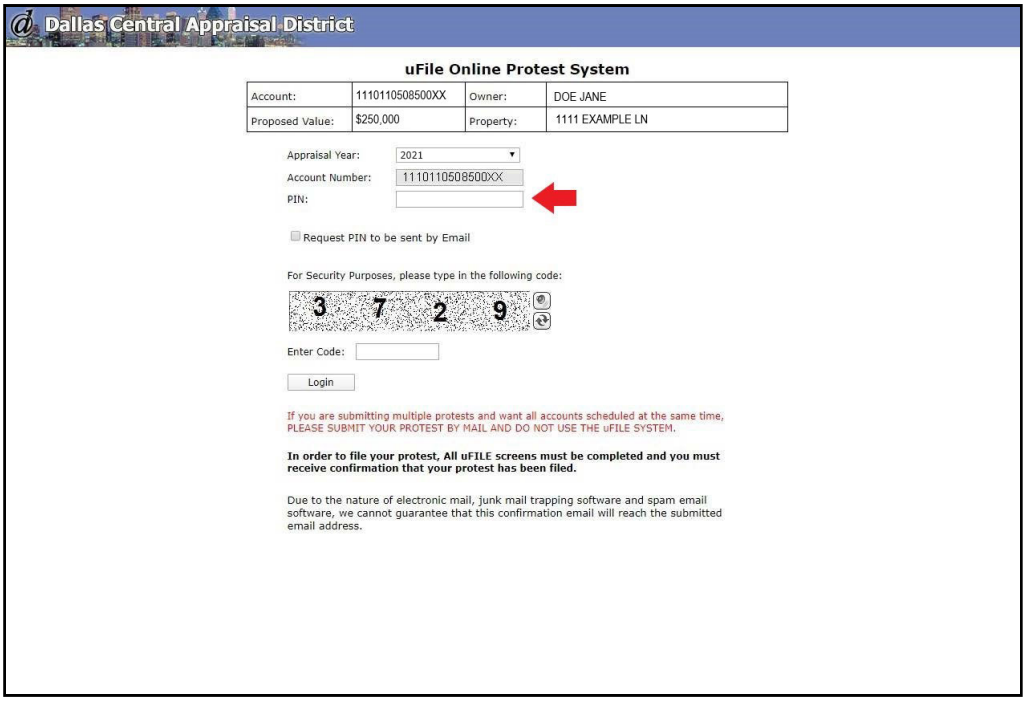
Step 3. Select your property from the results by clicking on your address.



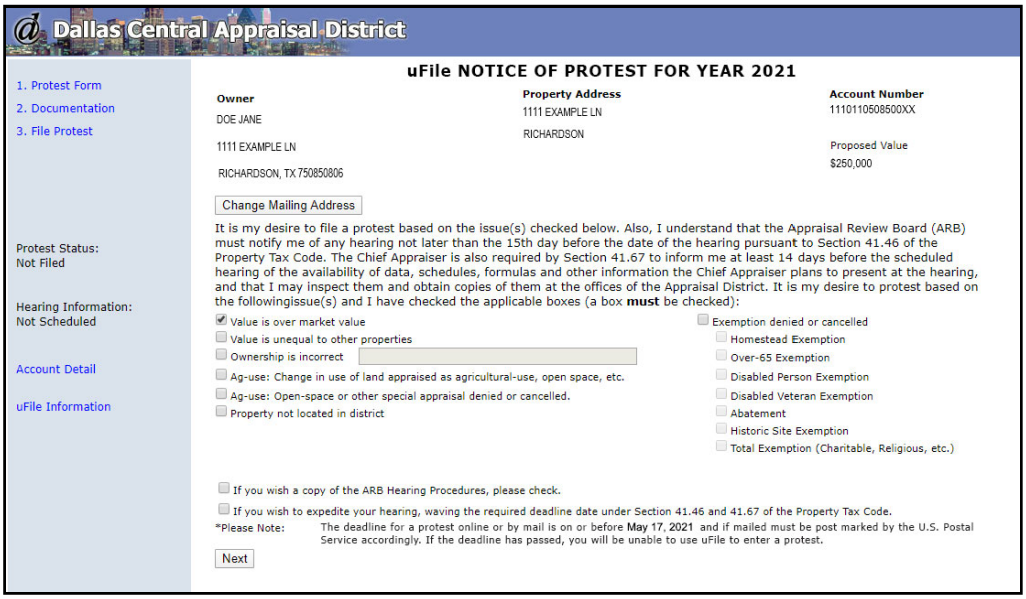
Step 4. You are now on your property's "Residential Account" page. To access the uFile Online Protest system for your property, click the link on the left titled "uFile Online Protest".



Step 5. This brings up the uFile Online Protest System webpage. To access your account, enter the PIN number from the top left corner of the 2021 Notice of Appraised Value that you received in the mail (under the large "@" symbol) into the box labeled "PIN". To finish on this page, enter the large security code shown on the webpage into the box labeled "Enter code", then click "Login". **If you did not receive your 2021 Notice of Appraised Value in the mail (or you lost the letter)**, you can request for your PIN to be sent **instantly** to your email (the online version of the Notice of Appraised Value does not have this PIN). To do this, simply click the box next to "Request PIN to be sent by Email". If you do not receive an email from Dallas CAD in your inbox within two minutes, check your spam or junk mail folders.



Step 6. Now, you will see you are on the "uFile Notice of Protest for Year 2021" page. In the middle of the page on the left and right are boxes you may check to select the reason(s) for your protest. These are explained below. After you select your reason for protest, you may click "Next" at the bottom.



Some of the most common reasons for protesting include:

Value is over market value: DCAD's proposed market value for your home is higher than the recent final selling price of comparable homes. If this applies to your protest, you should mark the box on the form, "Value is over market value." You can find information on recent home sales by searching for your property's address on websites such as www.Zillow.com or www.trulia.com (we provide additional details on how to do this in Step 8 below).

Value is unequal compared with other properties: DCAD's proposed market value for your property is higher than the market values DCAD proposed for other houses that are comparable to yours. If this applies to your protest, you should mark the box on the form, "Value is unequal to other properties." You can find information on the appraised market values of other homes using the search tool on the following website: www.dallascad.org/SearchAddr.aspx. Using this website, you can find the appraised market values of other homes with similar characteristics to your home, such as being located in the same (or a nearby) neighborhood, having the same number of bedrooms, bathrooms, and similar square footage. To do this, on the website www.dallascad.org/SearchAddr.aspx click the "Street Address" link at the top of page, then enter a street name near your property's street (or even the same street as yours) in the "Street Name" box, then click "Search". The search results will show properties on that street and their appraised values. To learn about the characteristics of any of these homes, click on the resulting home's street address to pull up its account page, then, importantly, scroll all the way down to see the home's characteristics. You may click the "Back" button on your browser to return to the search results and click the other results to open their account pages as well. If there is a large number of results, you may click "Next" at the bottom of the search results to see additional pages of results.

Errors in DCAD data: For example, DCAD may be overestimating your home's value if the number of bedrooms or other characteristics are incorrect on their website. It is possible more than one of these reasons applies for your protest, in which case you should check all that apply.

Step 7. Next, you will see the page where you may upload any supporting evidence you would like the county to consider for your protest. This may be a simple hand-written explanation that you can take a photo of (in .JPG format), an explanation you typed into an Excel spreadsheet (.XLS format), or one of the other formats listed on the site.

When you protest you need to provide an argument in a few sentences. For example, you may argue that the appraised market value is too high. As explained in Step 6, you can identify another home that was sold recently and has similar characteristics to your home, such as being located in the same (or a nearby) neighborhood, having the same number of bedrooms, bathrooms, and similar square footage, using www.Zillow.com or www.trulia.com. You can then mention that the comparison home was sold for less than the DCAD's appraised market value for your home and the date it was sold. A sample argument, using the example from the photo below (which assumes that your home is located at 1111 Example Ln and has a proposed value of $250,000), would be:

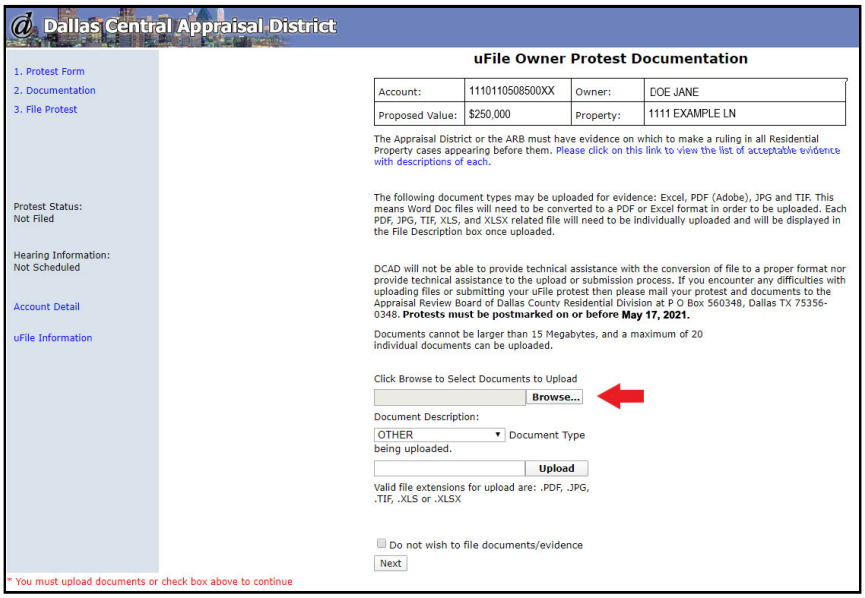
"*I found a home that is similar to mine but was recently sold for less than my home's appraised market value. The property located at 1204 Example Ln (Richardson, TX) is 0.29 miles away from home, and has the same number of bedrooms and a similar square footage. The property was sold on 11/31/2020 for $220,000.*"

You can alternatively search for the values the county proposes for other houses in your neighborhood by typing your street in the following link: https://www.dallascad.org/SearchAddr.aspx

If you do this, you need to check Value is unequal compared with other properties on the form in Step 6. A sample argument, using the example from the photo below (which assumes that your home is located at 1111 Example Ln and has a proposed value of $250,000:

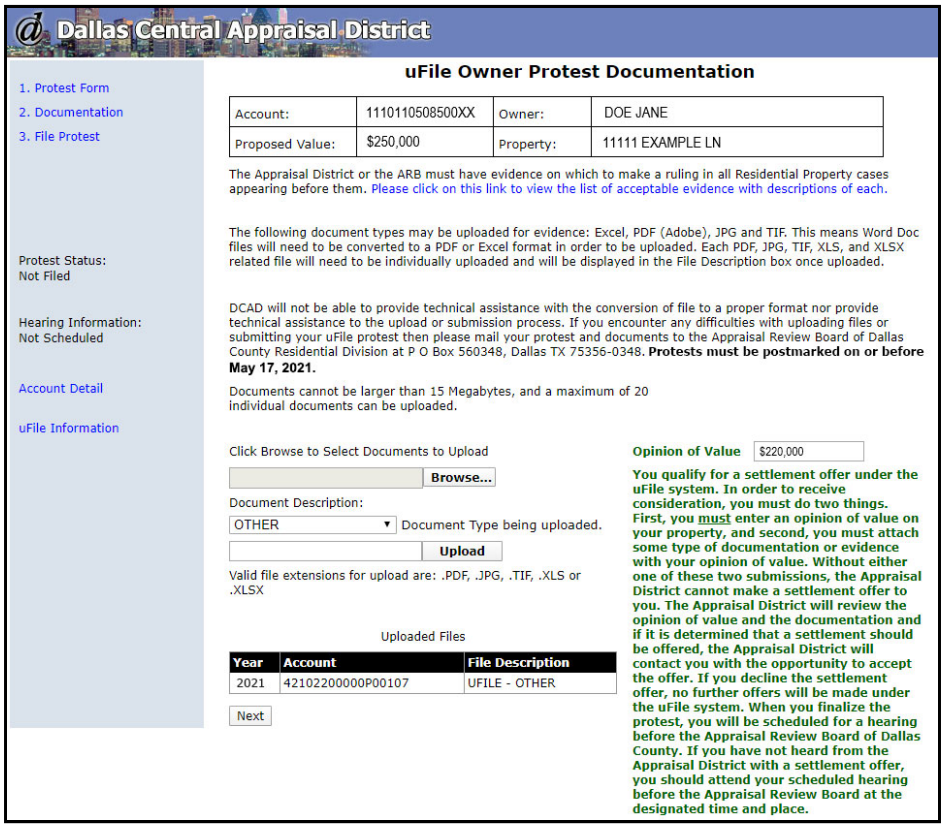
"*I found a home that is similar to mine but that has a proposed value that is lower than that of my home. The property located at 1204 Example Ln (Richardson, TX) is 0.29 miles away from home, and has the same number of bedrooms and a similar square footage. The proposed market value is $220,000.*"

If you wish to receive an informal settlement offer from the county, you must upload a document of some sort. If you do not wish to receive an informal settlement offer, you may simply click "Next".



To upload a document you would like considered by the county for your protest, first click the "Browse" button, then locate the document on your computer. Next, choose the description that you believe best describes your document from the dropdown menu labeled "Document Type". Finally, click straight on "Upload", ignoring the box next to it. **You may repeat this for each document you would like to upload.**

After the first document is uploaded, a green paragraph will appear on the right side of the screen. At the top of the green paragraph, you should enter your "opinion of value", which the county will consider when it makes its informal settlement offer. You must include your "Opinion of Value" on your property to qualify for a settlement offer. Enter the value you believe your property was actually worth as of January 1st, 2021 (this is the date the county's proposed value is for). After you finish uploading any documents you wish, click "Next" to proceed to the last step.



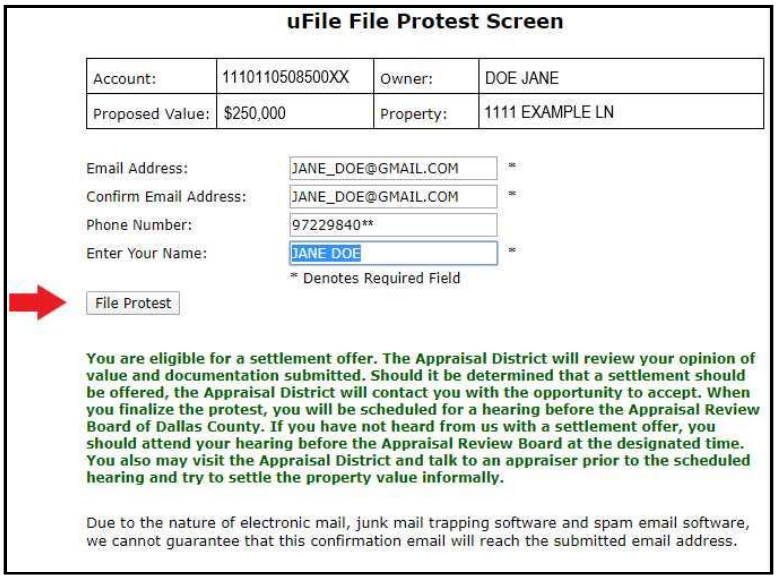
Step 8. Go to this link: https://www.trulia.com/

Once you open the link, there are 3 option: Buy, Rent, Sold. You need to click on Sold. Type your zip code in the search box. You can now use the map to search for houses that are near your home, are similar in terms of number of bedrooms, bathrooms, and similar square footage, and that were sold for a lower price than the value DCAD is proposing for your home. Once you click on a home, you can find the "pending price," which is an approximation of the real transaction price of the recent sale, by scrolling down to find for the price history. Remember that the relevant price for the protest is how much the property was actually worth as of January 1st, 2021. For example, if you see that home prices have increased after January 1st, 2021, that is irrelevant for the protest.

You can alternatively use this link: https://www.zillow.com/homes/recently_sold/

Once you open the link, type your address in the search box (when you start typing your address, the Zillow webpage will likely propose your address and you can just click on it). You can alternatively type your zip code. You can now use the map to search for recently sold houses that are near your home. Look for houses that are similar in terms of number of bedrooms, bathrooms, and similar square footage, and that were sold for a lower price than the value DCAD is proposing for your home. Once you click on a home, you can find the "pending price," which is an approximation real transaction price of the recent sale, by opening the "home details" tab and scrolling down.

Step 9. Final step: On the page that appears, enter your email address, phone number, and name in the boxes provided. These boxes automatically capitalize whatever you type, so do not worry about this. Click "File Protest" when you are ready to submit your protest. A confirmation email will be sent to your email address.



uFile File Protest Screen

Account:	1110110508500XX	Owner:	DOE JANE
Proposed Value:	$250,000	Property:	1111 EXAMPLE LN

Email Address: JANE_DOE@GMAIL.COM *

Confirm Email Address: JANE_DOE@GMAIL.COM *

Phone Number: 97229840**

Enter Your Name: JANE DOE *

* Denotes Required Field

File Protest

You are eligible for a settlement offer. The Appraisal District will review your opinion of value and documentation submitted. Should it be determined that a settlement should be offered, the Appraisal District will contact you with the opportunity to accept. When you finalize the protest, you will be scheduled for a hearing before the Appraisal Review Board of Dallas County. If you have not heard from us with a settlement offer, you should attend your hearing before the Appraisal Review Board at the designated time. You also may visit the Appraisal District and talk to an appraiser prior to the scheduled hearing and try to settle the property value informally.

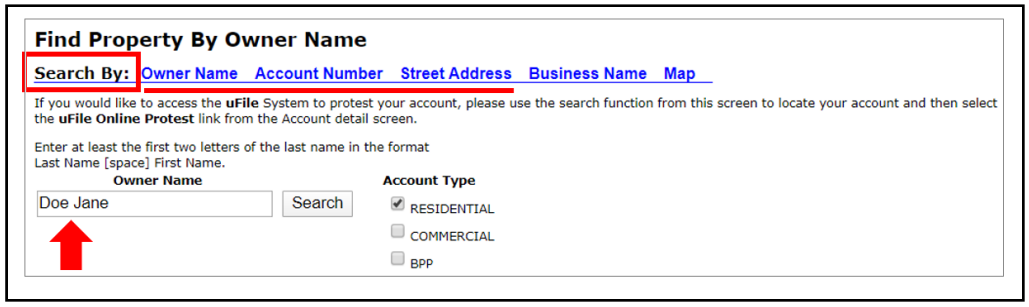
Due to the nature of electronic mail, junk mail trapping software and spam email software, we cannot guarantee that this confirmation email will reach the submitted email address.

Walkthrough for Filing a Protest by Mail

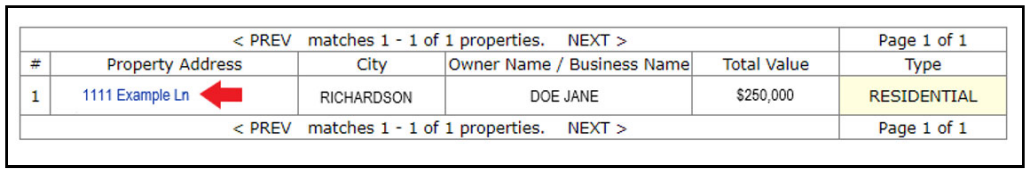
To file a protest related to your property in Dallas County **by mail**, simply follow the steps below: If you received your 2021 Notice of Appraised Value from the Dallas Central Appraisal District (CAD) in the mail, you may use the protest form provided on the third page and you can skip straight to Step 5. If you do not have or did not receive a 2021 Notice of Appraised Value, follow all of the steps below.

Step 1. To retrieve your property protest form, enter the following URL into your internet browser. This opens the Dallas CAD Property Search webpage. http://www.dallascad.org/SearchOwner.aspx

Step 2. Click the link at the top of the webpage that describes how you would like to search for your property. The options include by "Owner Name", "Account Number", or "Street Address". "Owner Name" searches must be done with your *last name*. You only need your property address or your name (your account number will *not* be necessary).

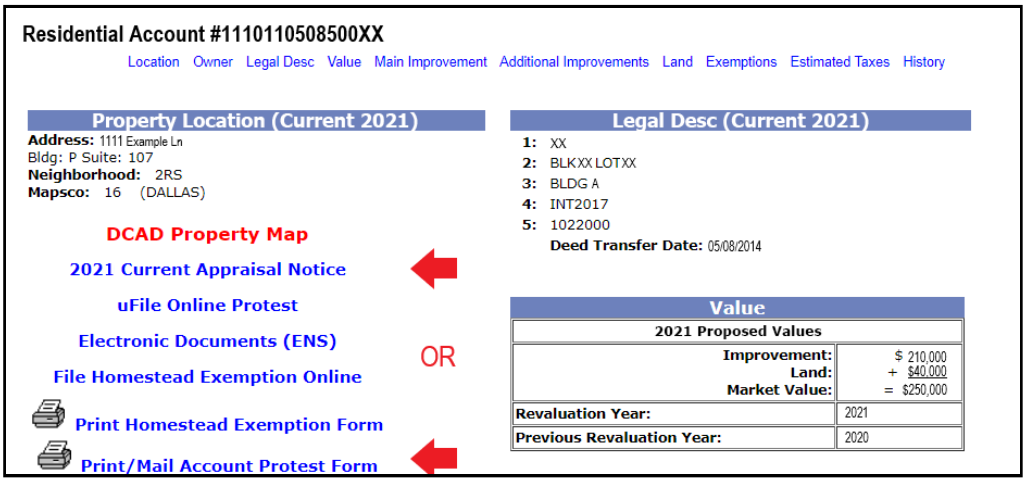


Step 3. Select your property from the results by clicking on your address.



< PREV matches 1 - 1 of 1 properties. NEXT >					Page 1 of 1
#	Property Address	City	Owner Name / Business Name	Total Value	Type
1	1111 Example Ln	RICHARDSON	DOE JANE	$250,000	RESIDENTIAL
< PREV matches 1 - 1 of 1 properties. NEXT >					Page 1 of 1

Step 4. You are now on your property's "Residential Account" page. To access the protest form for your property, click on either "Print/Mail Account Protest Form" or "2021 Current Appraisal Notice" (the protest form is the third page of the 2021 Current Appraisal Notice). You may print this form to mail in your protest.



Residential Account #1110110508500XX

Location Owner Legal Desc Value Main Improvement Additional Improvements Land Exemptions Estimated Taxes History

Property Location (Current 2021)

Address: 1111 Example Ln
Bldg: P Suite: 107
Neighborhood: 2RS
Mapsco: 16 (DALLAS)

DCAD Property Map
2021 Current Appraisal Notice
uFile Online Protest
Electronic Documents (ENS)
OR
File Homestead Exemption Online
Print Homestead Exemption Form
Print/Mail Account Protest Form

Legal Desc (Current 2021)

1: XX
2: BLK XX LOT XX
3: BLDG A
4: INT2017
5: 1022000
Deed Transfer Date: 05/08/2014

Value

2021 Proposed Values	
Improvement:	$ 210,000
Land:	+ $40,000
Market Value:	= $250,000
Revaluation Year:	2021
Previous Revaluation Year:	2020

Step 5. Once you have your protest form handy, it is time to fill it out. An example protest form is shown below. On your protest form, in the middle of the page on the left and right are boxes you may check to select the reason(s) for your protest.



APPRAISAL REVIEW BOARD OF DALLAS COUNTY
NOTICE OF PROTEST - RESIDENTIAL
TAX YEAR 2021

www.dallascad.org (214) 905-9402

Account Number: 1110110508500XX

JANE DOE
1111 EXAMPLE LN
DALLAS, TX XXXXX-XXXX

Property Address:
1111 EXAMPLE LN
DALLAS, TX XXXXX-XXXX

Legal Description:
EXAMPLE
BLK XX LOT XX

Proposed Value: $250,000

Deed Transfer Date:

CHANGE OF ADDRESS: _____

It is my desire to file a protest based on the issue(s) checked below. Also, I understand that the Appraisal Review Board (ARB) must notify me of any hearing not later than the 15th day before the date of the hearing pursuant to §41.46 of the Texas Property Tax Code. At the time your account is scheduled for an ARB hearing, the evidence that the Chief Appraiser will introduce at your hearing will be available on the DCAD website. You may access this evidence on the website by using the property account number and PIN located on your notice of appraised value and hearing notice.

It is my desire to protest based on the following issue(s) and I have checked the applicable boxes:

- [X] Value is over market value
- [ ] Value is unequal compared with other properties
- [ ] Property not located in district
- [ ] Exemption was denied or cancelled (Specify _____)
- [ ] Ownership is incorrect (Specify _____)
- [ ] Ag-Use: Change in use of land appraised as agricultural use, open-space, etc.
- [ ] Ag-Use: Open-Space or other special appraisal denied or cancelled
- [ ] Property should not be taxed in district or in one or more taxing units
- [ ] Other: (Specify _____)

Additional Requests: _____ Opinion of Value: $220,000

If you wish to expedite your hearing by waiving the required deadline date under Section 41.46 of the Texas Property Tax Code, please check the following box: [ ]

Signature of Owner (or Agent) | Date Filed | (Agent Registration No., if applicable)

Jane Doe
Printed Name | Daytime/Cell Phone No. | E-Mail Address

DEADLINE FOR FILING A PROTEST: May 17, 2021

Some of the most common reasons for protesting include:

Value is over market value: DCAD's proposed market value for your home is higher than the recent final selling price of comparable homes. If this applies to your protest, you should mark the box on the form, "Value is over market value." You can

find information on recent home sales by searching for your property's address on websites such as www.Zillow.com or www.trulia.com (we provide additional details on how to do this in Step 7).

Value is unequal compared with other properties: DCAD's proposed market value for your property is higher than the market values DCAD proposed for other houses that are comparable to yours. If this applies to your protest, you should mark the box on the form, "Value is unequal to other properties." You can find information on the appraised market values of other homes using the search tool on the following website: www.dallascad.org/SearchAddr.aspx. Using this website, you can find the appraised market values of other homes with similar characteristics to your home, such as being located in the same (or a nearby) neighborhood, having the same number of bedrooms, bathrooms, and similar square footage. To do this, on the website www.dallascad.org/SearchAddr.aspx click the "Street Address" link at the top of page, then enter a street name near your property's street (or even the same street as yours) in the "Street Name" box, then click "Search". The search results will show properties on that street and their appraised values. To learn about the characteristics of any of these homes, click on the resulting home's street address to pull up its account page, then, importantly, scroll all the way down to see the home's characteristics. You may click the "Back" button on your browser to return to the search results and click the other results to open their account pages as well. If there is a large number of results, you may click "Next" at the bottom of the search results to see additional pages of results.

Errors in DCAD data: For example, DCAD may be overestimating your home's value if the number of bedrooms or other characteristics are incorrect on their website. It is possible more than one of these protest reasons applies, in which case you should check all that apply.

Step 6. Before you mail your protest, you must include in the envelope any supporting evidence you would like the county to consider for your protest. This may be a simple hand-written explanation. If you wish to receive an informal settlement offer from the county, you must at least include a brief explanation for your protest on a piece of paper in the envelope.

When you protest you need to provide an argument in a few sentences. For example, you may argue that the appraised market value is too high. As explained in Step 5, you can identify another home that was sold recently and has similar characteristics to your home, such as being located in the same (or a nearby) neighborhood, having the same number of bedrooms, bathrooms, and similar square footage, using www.Zillow.com or www.trulia.com. You can then mention that the comparison home was sold for less than the DCAD's appraised market value for your home and the date it was sold. A sample argument, using the example from the photo above (which assumes that your home is located at 1111 Example Ln and has a proposed value of $250,000), would be:

"*I found a home that is similar to mine but was recently sold for less than my home's appraised market value. The property located at 1204 Example Ln (Richardson, TX) is 0.29 miles away from home, and has the same number of bedrooms and a similar square footage. The property was sold on 11/31/2020 for $220,000.*"

You can alternatively search for the values the county proposes for other houses in your neighborhood by typing your street in the following link: https://www.dallascad.org/SearchAddr.aspx

If you do this, you need to check Value is unequal compared with other properties on the form in Step 5. A sample argument, using the example from the photo above (which assumes that your home is located at 1111 Example Ln and has a proposed value of $250,000), would be:

"*I found a home that is similar to mine but that has a proposed value that is lower than that of my home. The property located at 1204 Example Ln (Richardson, TX) is 0.29 miles away from home, and has the same number of bedrooms and a similar square footage. The proposed market value is $220,000.*"

Step 7. Go to this link: https://www.trulia.com/

Once you open the link, there are 3 option: Buy, Rent, Sold. You need to click on Sold. Type your zip code in the search box. You can now use the map to search for houses that are near your home, are similar in terms of number of bedrooms, bathrooms, and similar square footage, and that were sold for a lower price than the value DCAD is proposing for your home. Once you click on a home, you can find the "pending price," which is an approximation of the real transaction price of the recent sale, by scrolling down to find for the price history. Remember that the relevant price for the protest is how much the property was actually worth as of January 1st, 2021. For example, if you see that home prices have increased after January 1st, 2021, that is irrelevant for the protest.

You can alternatively use this link: https://www.zillow.com/homes/recently_sold/

Once you open the link, type your address in the search box (when you start typing your address, the Zillow webpage will likely propose your address and you can just click on it). You can alternatively type your zip code. You can now use the map to search for recently sold houses that are near your home. Look for houses that are similar in terms of number of bedrooms, bathrooms, and similar square footage, and that were sold for a lower price than the value DCAD is proposing for your home. Once you click on a home, you can find the "pending price," which is an approximation real transaction price of the recent sale, by opening the "home details" tab and scrolling down.

Step 8. On the line marked "Opinion of Value", write the value you believe your property was actually worth as of January 1st, 2021 (this is the date the county's proposed value is for). The county will take your opinion of value into consideration when it makes its informal settlement offer. You must include your "Opinion of Value" on your property to qualify for a settlement offer.

Step 9. Complete the rest of the lines asking for your personal information. **Be sure to provide your signature on the line provided.**

Step 10. Mail your protest to the following address:

Appraisal Review Board of Dallas County
Residential Division
PO Box 560348 Dallas,
TX 75356-0348

Note the envelope must be postmarked by the May 17th, 2021 deadline.

G Sample of Online 2021 Appraisal Notice

DALLAS CENTRAL APPRAISAL DISTRICT
NOTICE OF APPRAISED VALUE - RESIDENTIAL
TAX YEAR 2021

Mailing Address:
Residential Division
PO Box 560348
Dallas, TX 75356-0348

www.dallascad.org (214) 905-9402

Account Number: 325577000F0260000
Ownership:

JOAN ROBINSON
123 FAKE STREET
DALLAS

Property Address:
123 FAKE STREET
DALLAS

Legal Description:

Dear Property Owner:

This letter is your official notice of the **2021** proposed property tax appraisal for the account listed above. The Dallas Central Appraisal District (DCAD) appraises all of the property in Dallas County for property tax purposes. State law requires that appraisal districts appraise all taxable property at its fair market value. Your county, city, school district and other local governments use the appraisal in calculating your property taxes. Property taxes support critical services such as schools, police and fire protection, street maintenance and many others.

As of January 1, 2021, the DCAD appraised your real property at:

2021 Market Value:	$354,800
2021 Appraised Value:	$354,800
2021 Estimated Taxes (using last year's tax rates):	$8,902

DO NOT PAY FROM THIS NOTICE. THIS IS NOT A TAX BILL.

Your current year exemptions are: No Exemptions

The Texas legislature does not set the amount of your local taxes. Your property tax burden is decided by your locally elected officials and all inquiries should be directed to those officials.

The governing body of each taxing jurisdiction decides whether or not taxes on your property will increase. The DCAD only determines the value of the property in accordance with the Texas Constitution and Statutes.

The percentage difference between the 2016 appraised value of $289,262 and the proposed 2021 appraised value is an increase of 22.66% over a 5-year period.

To **PROTEST** the proposed 2021 value or other issues, you must file a protest with the Appraisal Review Board (ARB) by using the **uFile Online Protest System (preferred method)** or by submitting a written protest (form enclosed).

If you agree with the proposed value, no further action is required.

Deadline for filing a protest: May 17, 2021

Location of ARB hearings: 2949 N. Stemmons Fwy, Dallas, TX 75247

ARB hearings will begin: Monday, May 24, 2021

ARB deliberations will end: Mid-July

More information about your appraisal and the protest process is on the back of this notice and on the inserts enclosed.

Homestead "Capped" Limitation: The Texas Constitution provides that property with a homestead exemption may not be increased in value more than 10% per year, excluding any new improvements made. This provision takes effect the first year following the year the owner qualified for a homestead. Because of this constitutional limitation, if you received a homestead exemption on this property in the previous year, it will be "**capped**" at the appropriate limit.

103-1789

DALLAS CENTRAL APPRAISAL DISTRICT
NOTICE OF APPRAISED VALUE - RESIDENTIAL
Tax Year 2021
www.dallascad.org

Owner Name: JOAN ROBINSON
Account Number: 325577000F0260000
Property Address: 123 FAKE STREET

CURRENT YEAR 2021	County and School Equalization	City	School	Hospital	College	Special District	Canceled/ Reduced Exemption
Jurisdictions	Dallas County	City of Irving	Irving ISD	Parkland Hospital	Dallas Co Community College		
Market Value - Land	$ 80,000	$ 80,000	$ 80,000	$ 80,000	$ 80,000		
Market Value - Structure(s)	$ 274,800	$ 274,800	$ 274,800	$ 274,800	$ 274,800		
Market Value	$ 354,800	$ 354,800	$ 354,800	$ 354,800	$ 354,800		
Less Deductions							
Homestead Capped Limitation							
Ag-use Value							
Absolute Exemption							
Appraised Value	$ 354,800	$ 354,800	$ 354,800	$ 354,800	$ 354,800		
Less Exemption Amount							
Homestead							YES
Exemption Amount Subtotal							
Estimated Taxable Value	$ 354,800	$ 354,800	$ 354,800	$ 354,800	$ 354,800		Total
Last Year's Tax Rate	0.249740	0.594100	1.275100	0.266100	0.124000		2.509040
Estimated Taxes Due*	$ 886	$ 2,108	$ 4,524	$ 944	$ 440		$ 8,902

PRIOR YEAR 2020	County and School Equalization	City	School	Hospital	College	Special District
Jurisdictions	Dallas County	City of Irving	Irving ISD	Parkland Hospital	Dallas Co Community College	
Market Value - Land	$ 80,000	$ 80,000	$ 80,000	$ 80,000	$ 80,000	
Market Value - Structure(s)	$ 274,800	$ 274,800	$ 274,800	$ 274,800	$ 274,800	
Market Value	$ 354,800	$ 354,800	$ 354,800	$ 354,800	$ 354,800	
Less Deductions						
Homestead Capped Limitation						
Ag-use Value						
Absolute Exemption						
Appraised Value	$ 354,800	$ 354,800	$ 354,800	$ 354,800	$ 354,800	
Less Exemption Amount						
Homestead	$ 70,960	$ 70,960	$ 25,000	$ 70,960	$ 70,960	
Exemption Amount Subtotal	$ 70,960	$ 70,960	$ 25,000	$ 70,960	$ 70,960	
Estimated Taxable Value	$ 283,840	$ 283,840	$ 329,800	$ 283,840	$ 283,840	

Tax Ceiling: If you received the Age 65 or Older or the Disabled Person homestead exemption, your school, county, and certain city taxes for this year will not be any higher than they were for the year in which you first received the exemption, unless you have made new improvements to your home. If you improved your property by remodeling or adding an addition, your school, county, and certain city taxes may increase for new improvements. If you are the surviving spouse of a person who was age 65 or older or disabled at death and you were age 55 or older at the time of death, you may retain the school, county, and certain city tax ceilings.

APPRAISAL REVIEW BOARD OF DALLAS COUNTY
NOTICE OF PROTEST - RESIDENTIAL
TAX YEAR 2021

www.dallascad.org (214) 905-9402

Account Number: 325577000F0260000

JOAN ROBINSON
123 FAKE STREET
DALLAS

Property Address:
123 FAKE STREET
DALLAS

Legal Description:

Proposed Value: $354,800

Deed Transfer Date: 5/4/2020

CHANGE OF ADDRESS: _____

It is my desire to file a protest based on the issue(s) checked below. Also, I understand that the Appraisal Review Board (ARB) must notify me of any hearing not later than the 15th day before the date of the hearing pursuant to §41.46 of the Texas Property Tax Code. At the time your account is scheduled for an ARB hearing, the evidence that the Chief Appraiser will introduce at your hearing will be available on the DCAD website. You may access this evidence on the website by using the property account number and PIN located on your notice of appraised value and hearing notice.

It is my desire to protest based on the following issue(s) and I have checked the applicable boxes:

- ☐ Value is over market value
- ☐ Value is unequal compared with other properties
- ☐ Property not located in district
- ☐ Exemption was denied or cancelled (Specify _____)
- ☐ Ownership is incorrect (Specify _____)
- ☐ Ag-Use: Change in use of land appraised as agricultural use, open-space, etc.
- ☐ Ag-Use: Open-Space or other special appraisal denied or cancelled
- ☐ Property should not be taxed in district or in one or more taxing units
- ☐ Other: (Specify _____)

Opinion of Value : _____

Additional Requests : _____

If you wish to expedite your hearing by waiving the required deadline date under Section 41.46 of the Texas Property Tax Code, please check the following box: ☐

Signature of Owner (or Agent)	Date Filed	(Agent Registration No., if applicable)
Printed Name	Daytime/Cell Phone No.	Email Address

DEADLINE FOR FILING A PROTEST: May 17, 2021

GENERAL INSTRUCTIONS: Pursuant to §41.41 of the Texas Property Tax Code, a property owner has the right to protest certain actions taken by the appraisal district. There are two options to file a protest, 1) use the **uFile Online System**, or 2) mail a protest form.

uFile ONLINE PROTEST & SETTLEMENT SYSTEM: The preferred method of protesting your property is to use the **uFile Online Protest & Settlement System**. You may access the system by going to **www.dallascad.org**; under the blue Navigation Links box, select Search Appraisals and once you are on the details page of your account select the uFile Online Protest link. For easy access, you may request your individual PIN through this system or use the PIN located at the top left side of your Notice of Appraised Value. Once you utilize the uFile system to protest your property, you may also be eligible to use the settlement program and settle your protest online. If you file a protest using the uFile Online System, please do not file a written or duplicate protest.

uFile is the preferred method of filing a protest in order to expedite and ensure timely delivery of your protest.

PROTEST FORM: This form is for use by a property owner or designated agent who would like the ARB to hear and decide a protest. If you are leasing the property, you are subject to the limitations set forth in Texas Property Tax Code §41.413. Please review the ownership and property information provided on this protest form and make any necessary corrections.

If you wish to mail your protest and supporting documents, the envelope must be postmarked by U.S. Postal Service on or before the deadline.

Appraisal Review Board of Dallas County
Residential Division
PO Box 560348
Dallas, TX 75356-0348

HOW TO SETTLE THE VALUE OF YOUR PROPERTY

Informal Hearing Process: If you disagree with DCAD's proposed market value then **you must first file a protest and provide evidence as to your value position before DCAD's appraisal staff will conduct an informal review of the property's valuation**. DCAD would encourage all property owners to utilize DCAD's uFile Online Protest and Settlement System (uFile System) as this is the most efficient way to file a protest, submit evidence, and to begin the informal review process. Once a protest has been filed and evidence submitted, DCAD will reach out to the property owner prior to the Formal ARB Hearing if the evidence warrants a value change. DCAD appraisal staff will either conduct an Informal telephone review with the property owner or will respond via an email using the DCAD uFile and Settlement System. If you filed a protest, provided evidence, and have not been contacted by DCAD via a phone call or email at least 3 days prior to your scheduled Appraisal Review Board (ARB) Hearing, then please call the appropriate Division to speak with an appraiser who will at that time conduct an Informal telephone review. At any time, DCAD would encourage property owners to call the Appraisal District and speak with staff concerning any issues or questions they may have about their property or the Appraisal Review Board process.

UFILE - PREFERRED METHOD

uFile Online Protest & Settlement System: The preferred method of protesting your property is to use the **uFile Online Protest & Settlement System**. You may access the system by going to **www.dallascad.org;** under the blue Navigation Links box, select Search Appraisals and once you are on the details page of your account select the uFile Online Protest link. For easy access, you may request your individual PIN through this system or use the PIN located at the top left side of your Notice of Appraised Value. **Requesting a PIN does not constitute filing a uFile protest; you must complete the uFile protest process**. Once you utilize the **uFile** system to protest your property, you may also be eligible to use the settlement program and settle your protest online. All uFile protests will eventually be scheduled for an ARB Hearing if the protest issue(s) remain unresolved. Once scheduled for an ARB Hearing, DCAD will post the ARB Hearing Date and Time on your account on our website. The ARB will also mail you an ARB Hearing Notification. **If you file a protest using the uFile Online system, please do not file a written or duplicate protest.**

WRITTEN PROTEST

Protest Form: If you choose not to use the **uFile Online System**, you may use the protest form provided. You should attach to your protest form any documentation that supports your opinion of value or any other protested issue (reference the Standards of Documentation). **If you are protesting more than one account, be sure to staple or bundle together all protest forms and documents to avoid receiving multiple dates and times for your accounts**.

Useful Information: If you have purchased your property within the last three years, please include with your protest form, a copy of your closing statement or other official record that validates the purchase price.

Filing Deadlines: While **May 17** is the deadline to file a protest, a different deadline will apply to you if 1) your Notice of Appraised Value was mailed to you after **April 16**; 2) your protest concerns a change in use of agricultural, open-space, or timber land; 3) the Appraisal Review Board (ARB) made a change to the appraisal records that adversely affects you and you received notice of the change; 4) the DCAD or the ARB was required by law to send a notice about your property and did not; or 5) you had good cause for missing the **May 17** protest filing deadline. Contact the DCAD for questions about your specific protest filing deadline.

Weekends and Holidays: If your deadline falls on a Saturday, Sunday, or legal holiday, it is postponed until midnight of the next business day.

Appraisal Review Board (ARB): Members of the ARB are not employees of the DCAD. They serve as jurors to arbitrate issues brought before them. The Texas Property Tax Code outlines specific duties for the ARB to follow. The goal of the ARB is to ensure that each property owner is given a fair and impartial hearing in the most efficient and timely manner.

Hearing Process and Delivery of Requested Information: Once the Appraisal Review Board (ARB) receives and processes your protest, **your account will be scheduled for an ARB Telephone Hearing. If you do not want an ARB Telephone Hearing but would rather appear in person for your ARB Hearing then please indicate as such under the Additional Request(s) field on the front side of this protest form.** Once scheduled for an ARB Hearing your hearing date and time will be posted on your account(s) on DCAD's website. You will also receive an ARB Hearing Notice by first class mail with your hearing date and time and a phone number to call to start your ARB hearing. **You will be required to call the ARB at the designated ARB Hearing date and time to start the ARB Hearing.** If you do not receive an ARB Hearing Notice please call DCAD to inquire about your ARB Hearing date or check your account on the DCAD website. You may request in writing that your ARB Hearing Notice be sent to you by certified mail but you may be charged for this request. You can also request your ARB Hearing Notice be emailed to you if you provide an email address on the protest form and request this in writing. If you would like for the ARB to send your hearing notice to you by certified mail or by email then please indicate as such on the Protest Form under Additional Request(s). If your property's market value is greater than $50 million dollars then you can request to be heard by a special ARB Panel but you must do so in writing at the time you file a protest and indicate as such on the Protest Form under Additional Request(s). Prior to your ARB Hearing you may request a copy of the evidence DCAD plans to introduce at the hearing to establish any matter at issue. Before any ARB hearing on a protest or immediately after the hearing begins you or your agent and the CAD are required to provide each other with a copy of any materials (evidence) intended to be offered or submitted to the ARB at the ARB Hearing. Evidence may be submitted for any ARB hearing type either on paper or on a small digital portable device (such as a CD, USB flash drive, or thumb drive) which will be kept by the ARB**. Do NOT bring evidence by smartphone**. At the time your account is scheduled for an ARB Hearing, evidence that the Appraisal District will introduce at your hearing will be available on the DCAD website. You may access this evidence by using the property account number and PIN located on your Notice of Appraised Value and Hearing Notice. You may also request this information by calling the DCAD office.

Hearing Postponements: As a property owner, you are entitled to one postponement of the hearing without showing good cause. You are also entitled to postpone your hearing if you or your agent shows reasonable cause for postponement. You must request this postponement to the ARB before the hearing date. The ARB will determine if good cause exists for missing your hearing.

Residence Homestead Exemptions: If the property is your home and you occupy it as your principal place of residence, you may qualify for one or more residence homestead exemptions, which will reduce the amount of taxes imposed on the property. If you are single or a married couple filing together, you may be eligible to **apply online** for the **Homestead Exemption** at **www.dallascad.org**. If you are filing for the Age 65 or Older or Disabled Person exemption or the property is owned by multiple owners, you are *not* eligible to file online. However, you may select the link "Print Homestead Exemption Form" from your account on the DCAD website or you may call 214-631-0910.

Special Service Accommodations: The DCAD offices are wheelchair accessible and parking spaces for the disabled are provided. The DCAD will provide sign interpretation services for the hearing impaired at any scheduled hearing or meeting if at least 72 hours advance notice is given. The hearing impaired can call TDD at (214) 819-2368.

If you desire any special assistance during the hearing process to accommodate any disability you have, please specify on the "Additional Requests" line on the Notice of Protest form:

Additionally, to arrange for any special service to accommodate a disability, you may contact the Assistant Director of Administration at (214) 631-0520, extension 1107.

H Questionnaire: Field Experiment



Welcome to our web-based survey that examines residents' preferences regarding property taxes. Please read the consent form below and click "I Agree" when you are ready to start the survey. **At the end, we will provide detailed step-by-step instructions on how you can file a protest online or by mail directly.**

The study is being conducted by a team of researchers led by Professor Alejandro Zentner of The University of Texas at Dallas, and it has been designated by The University of Texas at Dallas Office of Research Integrity and Outreach as exempt from review by an Institutional Review Board. No deception is involved, and the study involves no more than minimal risk to participants (i.e., the level of risk encountered in daily life). Participation in the study typically takes between 5 and 10 minutes and is strictly confidential. Participants begin by entering the validation code included in the letter received by mail and then answer questions related to property taxes and demographics. All responses are treated as confidential. Data will be pooled and published in aggregated form only. Participants should be aware; however, that the survey is not being run from a "secure" https server of the kind typically used to handle credit card transactions, so there is a small possibility that responses could be viewed by unauthorized third parties (e.g., computer hackers). Many individuals find participation in this study enjoyable, and no adverse reactions have been reported thus far. Participation is voluntary, refusal to take part in the study involves no penalty or loss of benefits to which participants are otherwise entitled, and participants may withdraw from the study at any time without penalty or loss of benefits to which they are otherwise entitled. By filling the survey, you enter a raffle for 20 prizes of $100. Based on the expected number of people that will answer the survey, our best guess is that participants will have a 1 in a 100 odds on the raffle prizes. If participants have further questions about this study, they may contact the Principal Investigator, Alejandro Zentner (azentner@utdallas.edu). Participants who want more information about their rights as a participant or who want to report a research related concern may contact The University of Texas at Dallas Office of Research Integrity and Outreach at (972) 883-4579. If you are 18 years of age or older, understand the statements above, and freely consent to participate in the study, click on the "I Agree" button to begin the survey.

○ Yes, I would like to take part in this study and confirm that I am 18 years of age or older, I understand the statements above, and freely consent to participate in the study.



Please enter the validation code included in the letter (next to the URL of this survey, inside the black box) to begin:



Have you already filed a property tax protest in 2021?

- ◯ Yes
- ◯ No

When did you first read the letter that included the link to this survey? Your best guess is fine.



How many **schooled-aged children (in grades K-12)** do you have?

- ○ 0
- ○ 1
- ○ 2
- ○ 3
- ○ 4 or more



According to the Dallas Central Appraisal District's (DCAD) records, your school district is **Independent School District (ISD)**.

Do any of your children attend a public school (grades K-12) in this district?

- ○ Yes
- ○ No



How many of your children attend a public school (grades K-12) in **Dallas ISD?**

- ○ 1
- ○ 2
- ○ 3
- ○ 4 or more

We want to ask you about the property taxes you paid in the past year (2020 taxes, due on January 31, 2021).

We estimate that you paid **$4,241** in property taxes in 2020 for your property at **924 Pavillion St (Dallas, TX)**. This amount went to cover school taxes, city taxes, hospital taxes, county taxes, college taxes, and other special taxes in the area in which your home resides.

What percentage of your total property taxes in 2020 do you believe corresponded to **school taxes**? Don't worry if you don't know, we just need your best guess.

0 %

Note: Please insert a number between 0 and 100.



Next, a group of individuals participating in this survey will be randomly chosen to receive some data related to the previous question.

Please continue to the next screen to find out if you will be selected to receive the data.



You have been randomly selected to not receive the data.

Please proceed to the next screen to the continue with the survey.

Previously, we asked you about the past year (2020 taxes, due on January 31, 2021). **Now, we want to get a sense of your expectations for this year (2021 taxes, due on January 31, 2022).**

As of today, our best estimate is that you will pay **$7,723** in property taxes in 2021.

What percentage of your total property taxes in 2021 do you believe will correspond to **school taxes**? Please enter your best guess.

0 %

Note: Please insert a number between 0 and 100.

Next, we want to ask you a few questions related to Chapter 41 of the Texas Education Code, typically referred to as "recapture" or "Robin Hood Tax" in the media.

Recapture payments redistribute property taxes collected from “property wealthy” districts to “property poor” districts in Texas. Due to recapture, a school district may receive more, the same, or less in funding than what households in that school district paid in school taxes.

We want to ask you about recapture in the past year (2020 taxes, due on January 31, 2021).

Do you think that **your school district** will receive more, the same, or less in funding than what households in your school district paid in school taxes in 2020?

- ○ More
- ○ The same
- ○ Less



You said your school district will receive more in funding than what households in your school district paid in school taxes in 2020. How much more will it receive, in your opinion? Please enter your best guess.

0 %

more than what households paid in school taxes

Note: Please insert a number between 0 and 100.



Next, a group of individuals participating in this survey will be randomly chosen to receive some data related to the previous question.

Please continue to the next screen to find out if you will be selected to receive the data.



You have been randomly selected to receive the following data:

In a previous question you indicated you believe that your school district will receive 13% more in funding than what households in your school district paid in school taxes in 2020. Based on public records, we estimate that your school district will receive **5.6% less** in funding than what households in your school district paid in school taxes.

Methodological Notes



Previously, we asked you about the past year (2020 taxes, due on January 31, 2021). **Now, we want to get a sense of your expectations for this year (2021 taxes, due on January 31, 2022).**

Do you think that **your school district** will receive more, the same, or less in funding than what households in your school district will pay in school taxes in 2021?

- ○ More
- ○ The same
- ○ Less



You said your school district will receive more in funding than what households in your school district will pay in school taxes in 2021. How much more will it receive, in your opinion? Please enter your best guess.

0 %

more than what households will pay in school taxes

Note: Please insert a number between 0 and 100.

Next, we would like to better understand the views of Dallas County residents about their property taxes.

Do you agree or disagree with the following statement?

The local government services that I am provided (e.g., schools, roads, hospitals) justify the total amount I pay in property taxes.

1 - Strongly disagree	2	3	4	5 - Neither agree or disagree	6	7	8	9	10- Strongly Agree
○	○	○	○	○	○	○	○	○	○

Do you support the recapture system (Chapter 41 of Texas Education Code) and its redistribution of school property taxes between school districts?

1 - Strongly oppose	2	3	4	5 - Neutral	6	7	8	9	10- Strongly support
○	○	○	○	○	○	○	○	○	○



Relative to the other households in your county, do you think your household pays a fair amount in property taxes?

1 - Very unfair	2	3	4	5 - Neither fair nor unfair	6	7	8	9	10- Very fair
○	○	○	○	○	○	○	○	○	○



Do you consider the total amount of property taxes you pay to be too low, about right or too high?

- ◯ Too low
- ◯ About right
- ◯ Too high



You have time until May 17th, 2021 to protest Dallas CAD's proposed value of your property. Do you intend to protest this year?

- ○ Very likely
- ○ Likely
- ○ Unlikely
- ○ Very unlikely

If you can, please explain why you will (or will not) protest in 2021:



Which of the following alternatives would you prefer?

- ○ Your taxes and the taxes of everyone else decrease but you get worse government services
- ○ Your taxes and the taxes of everyone else are held constant and so are government services
- ○ Your taxes and the taxes of everyone else increase to provide better government services



We are almost done. We would like to ask you a few more questions about yourself before finishing the survey.

Please indicate your gender:

- ○ Male
- ○ Female
- ○ Other

How old are you?



Which of the following best describes your race/ethnicity?

- ○ American Indian or Alaska Native
- ○ Asian / Pacific Islander
- ○ Black or African American
- ○ Hispanic or Latino
- ○ White
- ○ Multiple Ethnicity / Other (Please specify)

What is the highest level of education you have completed?

- ○ Less than a high school degree
- ○ High school degree
- ○ College degree
- ○ Graduate degree

do you consider yourself Republican, a Democrat, or an Independent?

- ○ Republican
- ○ Democrat
- ○ Independent

Some people think that the government in Washington ought to reduce the income differences between the rich and the poor, perhaps by raising the taxes of wealthy families or by giving income assistance to the poor. Others think that the government should not concern itself with reducing this income difference between the rich and the poor. Below is a scale from 1 to 7. Think of a score of 1 as meaning that the government ought to reduce the income differences between rich and poor, and a score of 7 meaning that the government should not concern itself with reducing income differences. What score between 1 and 7 comes closest to the way you feel?

1- Government should do something to reduce income differences	2	3	4	5	6	7- Government should not concern itself.
○	○	○	○	○	○	○



Imagine the government gave you full power to choose the school property taxes that each household must pay, as long as the total school property taxes collected stays the same. The home market value for Household A and Household B is the same: $200,000. However, Household A has 2 kids in public schools, while Household B has no kids in public schools. What school property taxes would you choose for each home? These two values must add up to $8,000.

Household A (2 kids in public schools (K-12))	0
Household B (0 kids in public schools (K-12))	0
Total	0

Imagine the government gave you full power to choose how to redistribute property taxes between two hypothetical school districts: School District A and School District B. Both districts have the same number of households, same number of students, and the tax rates are the same too.

However, homes in School District A are more expensive ($500,000 each) than homes in School District B ($100,000 each). In School District A, households paid $2,500 each in school taxes. In School District B, households paid $500 each in school taxes. How would you distribute these funds?

- School District A gets $2,500 per household and School District B gets $500 per household
- School District A gets $2,000 per household and School District B gets $1,000 per household
- School District A gets $1,500 per household and School District B gets $1,500 per household
- School District A gets $1,000 per household and School District B gets $2,000 per household
- School District A gets $500 per household and School District B gets $2,500 per household

Recent research on decision making shows that choices are affected by the context in which they are made. Differences in how people feel, in their previous knowledge, experience, and in their environment can influence the choices they make. To help us understand how people make decisions, we are interested in information about you. Specifically, whether you actually take the time to read the instructions. If you don't, some results may fail to tell us very much about decision making in the real world. To help us confirm that you have read these instructions, please ignore the question about how you are feeling. Instead, only check the "none of the above" option. Thank you very much.

☐ Interested	☐ Hostile	☐ Nervous
☐ Distressed	☐ Enthusiastic	☐ Determined
☐ Excited	☐ Proud	☐ Attentive
☐ Upset	☐ Irritable	☐ Jittery
☐ Strong	☐ Alert	☐ Active
☐ Scared	☐ Inspired	☐ None of the above



One last question.

Feel free to share any comments with us below



You have reached the end of the survey. Thanks for your participation! The raffle will be held on May 17th, 2021. If you are a winner, we will contact you by mail shortly afterward to coordinate payment.

To file a protest in Dallas County, you can fill out a short form online or mail it in. You do not need an agent to protest. You do not need to attend a hearing if you accept an online settlement offered by the county. If the county schedules a hearing and you do not attend it, the protest will simply be dismissed with no penalty.

You can find **step-by-step instructions for filing a protest** at the following website:
https://taxhelp.utdallas.edu/

Please be sure to print or save this URL (https://taxhelp.utdallas.edu/), because you may not be able to return to this page.

I Questionnaire: Prediction Survey



Welcome to our prediction survey for a study that examines Dallas County residents' preferences regarding property taxes. Please read the consent form below and click "I Agree" when you are ready to start the survey.

This survey involves no more than minimal risk to participants (i.e., the level of risk encountered in daily life). Participation typically takes between 5 and 10 minutes and is strictly confidential. Many individuals find participation in this survey enjoyable, and no adverse reactions have been reported thus far. Participation is voluntary, and participants may withdraw from the survey at any time.

○ Yes, I would like to take the survey

→

UCLA Anderson School of Management

Introduction:

We study a sample of homeowners living in Dallas County (Texas) who have the opportunity to file an appeal of their property taxes, which can (legally) reduce the amount they have to pay in property taxes. We conducted a field experiment to understand why homeowners choose to protest their taxes. More specifically, we study two potential drivers of protests:

(1) Whether households are more or less likely to protest when they find out that their taxes go towards the public services from which they benefit (more precisely, public schools).

(2) Whether households are more or less likely to protest when they find out that their taxes are being redistributed towards less wealthy school districts or that they receive part of the property taxes collected from wealthier school districts.

←

→

UCLA Anderson School of Management

School Taxes:

Property taxes are used to fund various services such as schools, parks, and roads. In 2020, the average home in Dallas County was worth $306,000 and the average estimated property tax bill was $6,150. The total property tax bill is the sum of a series of taxes: a school tax, a city tax, a county tax, and others. The school tax is typically the largest component: on average, school taxes account for about 50% of the total property tax bill.

Tax Protests:

Households have the opportunity to file a protest by mail or online free of charge. In their protest form households typically argue that the market value estimated by the Dallas Central Appraisal District was too high. In 2020, around 8.40% of the households filed a protest directly (i.e., *without* a tax agent representing them).[1] Of those, 69.7% were successful. Successful protests resulted in average tax savings of $485.

---

[1] In this project, we focus on protests filed directly. For the sake of brevity, in the remainder of this survey we always refer to direct protests and all the information provided pertains to direct protests.

←

→

UCLA Anderson School of Management

Recapture System:

To make public school funding more equitable between school districts, Chapter 41 of the Texas Education Code, typically referred to as "recapture" or the "Robin Hood Tax" by the media, established a system to redistribute part of the property taxes collected from “property wealthy” school districts to “property poor” school districts. For example, in one of the wealthiest school districts, Highland Park, for each $1 paid in property taxes by households from that district only $0.43 went to the local school district and the remaining $0.57 was transferred to other school districts in Texas.

Subject Pool and Timing:

We invited a sample of about 78,000 households from Dallas County via mail to take part in an online survey. Around 3,000 households completed the survey, which constitute our subject pool. We conducted the survey shortly before the deadline to submit a protest (May 17th, 2021).

←

→

UCLA Anderson School of Management

Experimental Design:

Our survey elicits the respondents' beliefs about **two key variables**:

> **(1) The share of property taxes that corresponds to their household's school taxes**. As a reference, school taxes accounted for 50% of the property taxes for the average subject, with nearly all subjects falling in the 38% - 59% range.
>
> **(2) The share of the property taxes paid by households in the subject's school district that were redistributed to other school districts due to recapture.** For reference, 40% of subjects included in the sample live in districts that are net givers to the recapture system (i.e., part of the school taxes collected from their own school district is redirected towards less wealthy districts), and 60% corresponds to net receivers (i.e., their own school districts receive part of the school taxes collected in other wealthier districts). Among net givers, school districts can give away as much as 57% of the property taxes they raise; among net receivers, a school district may receive as much as 23% in additional funds from other districts due to recapture.

We randomize the provision of accurate information about each of these two variables to subjects. For example, a subject who believed 40% of his/her property taxes corresponds to school taxes may find out that, in fact, 50% of his/her property taxes corresponds to school taxes. To measure whether individuals incorporated the information provided to them into their beliefs, we measured their prior beliefs (i.e., before receiving this information) and their posterior beliefs (i.e., after receiving this information).

UCLA Anderson School of Management

Outcome of interest:

We are interested to hear your predictions about the effects of this intervention on the main outcome: **whether the household filed a tax protest in 2021**. Even if you do not have strong beliefs about the effects of the information provision on this outcome, we are still interested in your best guess. We observe whether the subject filed a tax protest in 2021 using the county administrative records, which are publicly available. In this subject pool (individuals who completed the online survey), 29.4% filed a tax protest in 2021.

←

→

UCLA Anderson School of Management

First, we want to elicit your predictions about households **WITH** kids enrolled in their local public school district.

←

→

UCLA Anderson School of Management

We want your predictions about households **WITH** kids enrolled in their local public school district

Suppose that a respondent's **prior belief** about the share of property taxes that corresponds to school taxes **was 40%**. In response to the information treatment, the respondent **now believes that 50%** of their property taxes corresponds to school taxes.

As a result of this belief updating, would you expect the respondent's probability of filing a protest to go up, stay the same or go down?

- ○ Go up
- ○ Stay the same
- ○ Go down

← →

UCLA Anderson School of Management

We want your predictions about households **WITH** kids enrolled in their local public school district

By how much would you expect the respondent's probability of filing a protest to go up (in percentage points)? Please enter a number between 0 and 100.

0 p.p.

Reference:

In this subject pool (individuals who completed the online survey), 29.4% filed a tax protest in 2021.

← →

UCLA Anderson School of Management

We want your predictions about households **WITH** kids enrolled in their local public school district

Suppose that a respondent's **prior belief** was that their local school district **does not give nor receive** any transfers due to recapture. In response to the information treatment, the respondent **now believes that 10%** of the school taxes raised in their school district are redistributed to other school districts in Texas.

As a result of this belief updating, would you expect the respondent's probability of filing a protest **to go up, stay the same or go down**?

- ○ Go up
- ○ Stay the same
- ○ Go down

← →

UCLA Anderson School of Management

We want your predictions about households **WITH** kids enrolled in their local public school district

By how much would you expect the respondent's probability of filing a protest to go down (in percentage points)? Please enter a number between 0 and 100.

0 p.p.

Reference:

In this subject pool (individuals who completed the online survey), 29.4% filed a tax protest in 2021.

← →

UCLA Anderson School of Management

Now, we want to elicit your predictions about households **WITHOUT** kids enrolled in their local public school district (this includes households without kids at all).

←

→

UCLA Anderson School of Management

We want your predictions about households **WITHOUT** kids enrolled in their local public school district

Suppose that a respondent's **prior belief** about the share of property taxes that corresponds to school taxes **was 40%**. In response to the information treatment, the respondent **now believes that 50%** of their property taxes corresponds to school taxes.

As a result of this belief updating, would you expect the respondent's probability of filing a protest to go up, stay the same or go down?

- ○ Go up
- ○ Stay the same
- ○ Go down

← →

UCLA Anderson School of Management

We want your predictions about households **WITHOUT** kids enrolled in their local public school district

Suppose that a respondent's **prior belief** was that their local school district **does not give nor receive** any transfers due to recapture. In response to the information treatment, the respondent **now believes that 10%** of the school taxes raised in their school district are redistributed to other school districts in Texas.

As a result of this belief updating, would you expect the respondent's probability of filing a protest **to go up, stay the same or go down**?

- ○ Go up
- ○ Stay the same
- ○ Go down

← →

UCLA Anderson School of Management

In a scale from 1 (Not confident at all) to 5 (Extremely confident), how confident are you in your predictions for this study?

- ○ Not confident at all
- ○ Slightly confident
- ○ Somewhat confident
- ○ Very confident
- ○ Extremely confident

← →

UCLA Anderson School of Management

This is the last section of the survey. We would appreciate if you could share some information about yourself.

Are you currently one of the following: graduate student (either Master level or PhD level), faculty, post-doc or non-academic researcher?

- ○ Yes
- ○ No

← →

UCLA Anderson School of Management

Please select your discipline

- ○ Economic
- ○ Business (management, accounting, finance, etc.)
- ○ Political Science
- ○ Psychology
- ○ Sociology
- ○ Other

Have you ever conducted research on preferences for redistribution?

- ○ Yes
- ○ No

Have you ever conducted research on taxation?

- ○ Yes
- ○ No

Before filing this prediction survey, had you ever heard of the Tax Recapture System in Texas?

- ○ Yes
- ○ No

← →