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Where Do My Tax Dollars Go? Tax Morale Effects of Perceived Government Spending

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Abstract

Do perceptions about government spending affect willingness to pay taxes? We test this hypothesis with a natural field experiment that focuses on the allocation of property taxes to public schools. Our results show that taxpayers often misperceive the destination of their tax dollars. By introducing shocks to households' perceptions via an information-provision experiment, we find that perceptions of how tax dollars are used significantly affect the probability of filing a tax appeal. Moreover, the effects are consistent with reciprocal motivations: individuals are more willing to pay taxes if they believe that the government services funded by those taxes will provide greater personal benefit.

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1 Introduction

Do individuals' perceptions of how the government spends tax dollars influence their willingness to pay taxes? From an individual's point of view, the taxes that a single person pays have a negligible impact on the quantity or quality of government services that the individual receives. In other words, one individual can free-ride on the taxes paid by all the other taxpayers. However, we hypothesize that perceptions about government spending may still affect tax compliance through tax morale. More precisely, if individuals perceive that their taxes are being used to fund government services that are of personal benefit to them, they may feel a heightened *moral obligation* to pay their taxes. From hereon, we refer to this as the "reciprocal motivation" hypothesis.[1] The reciprocal motivation channel may help explain differences in tax morale, such as why some individuals are more willing to pay taxes than others. This channel could also be relevant for policymakers, as government communications about public finances might influence tax morale. In this paper, we test the reciprocal motivation hypothesis using a natural field experiment in a high-stakes context and through revealed preferences.

Our experiment leverages the context of property taxes, which represent an important source of revenue for governments in the United States and around the world.[2] For instance, U.S. property tax revenues in 2019 were estimated at $577 billion (Tax Policy Center, 2021a), nearly three times higher than the corporate income tax.[3] In the United States, virtually all counties rely heavily on property taxes to fund key government services such as schools, parks, and roads. School funding typically makes up the largest component of property taxes.

This setting offers two key advantages to test our hypothesis. First, our research design leverages the straightforward path between property taxes and the government services they fund, allowing us to identify who benefits from what. In particular, households with children enrolled in local public schools benefit directly from education funded by property taxes, whereas households without children enrolled in local public schools do not. For brevity, we hereinafter refer to households with children enrolled in local public schools as "households *with* children" and those without as "households *without* children."

The second advantage of this setting is that we can study the willingness to pay taxes

[1] This channel is similar to what Luttmer and Singhal (2014) call *reciprocal motivation*: "the willingness to pay taxes in exchange for benefits that the state provides to them (...) even though their pecuniary payoff would be higher if they didn't pay taxes." This channel is also related to a normative principle known as *benefit-based taxation*, which can be briefly described as the "idea of basing tax liabilities on how much an individual benefits from the activities of the state" (Weinzierl, 2018).

[2] For other studies on property taxes, see for example Cabral and Hoxby (2012), Jones (2019), Avenancio-Leon and Howard (2022), Nathan et al. (2020) and Dzansi et al. (2022).

[3] For reference, the 2019 federal income tax generated $1.717 trillion in revenue and corporate income tax generated $230 billion (Tax Policy Center, 2021b).

via revealed preferences using households' decisions to file property tax appeals, also known as tax protests (Nathan et al., 2020). Filing an appeal is a consequential, high-stakes action that households can take to reduce the amount they have to pay in property taxes.[4] In a nutshell, households can use the subjective nature of the property appraisal process in their favor. If they feel their taxes are too high, they can file a tax appeal to reduce their tax burden. The majority of these tax protests are successful, and they typically save households hundreds of dollars per year (Nathan et al., 2020).

We conducted the field experiment in Dallas County, Texas. We focus on one county because, from a logistical perspective, it is more practical to implement a field experiment in a single location. With an estimated population of about 2.6 million in 2020 (U.S. Census Bureau, 2021), Dallas County is the second-largest county in Texas and has a larger population than 15 of the 50 U.S. states. The county is diverse along many dimensions, such as ethnicity, and has a relatively even distribution of Democrat and Republican supporters.[5]

We sent an invitation letter to a sample of 78,128 households, encouraging them to participate in an online survey. The response rate was 2.7%, which aligns with the expected rate based on the recruitment method (Sinclair et al., 2012; Nathan et al., 2020). Our main subject pool comprises 2,110 respondents who completed the survey a few weeks before the deadline to file a property tax appeal between April and May of 2021. Our survey elicited key characteristics of the household, such as whether it has children enrolled in public schools. We match survey responses to administrative records from the county assessor's office. The rich administrative data allows us to determine, among other things, if the survey respondent subsequently filed a tax appeal.

Our experimental design can be summarized as follows. First, we measure respondents' perceptions about the share of their own property taxes that corresponds to school taxes and thus funds local public schools. For brevity, in the remainder of the paper, we refer to this percentage as the household's "school share." The school share for the average household in Dallas County is about 49.78%. We can assess respondents' misperceptions about the allocation of their tax dollars by comparing their estimates of the school share to the actual figures from administrative records. To study the causal effect of beliefs about how the government spends its tax dollars, the survey embeds an information-provision experiment. After eliciting respondents' prior beliefs, we provide a random half of them with information about their respective true school shares. We can assess whether the provision of information

[4] When studying attitudes towards taxation, social scientists rely primarily on survey data. 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.

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

influences their posterior beliefs, as indicated by survey responses, and their decisions to file a tax appeal, as reflected in administrative data.

The information-provision experiment creates exogenous variation in respondents' posterior beliefs about the fraction of their property taxes that funds local schools. To illustrate, a subject who perceives her or his school share amount to be 30% may be informed that the actual share is 50%. According to our hypothesis and as noted in the randomized control trial (RCT) pre-registration, the expected effects of the information shock depend on whether the household has children enrolled in public schools. Upon learning that the school share is higher than originally thought, households *with* children should become less likely to file a tax appeal because they learn that they benefit more from the allocation of 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 the allocation of government services than they originally thought.

Our experiment also included a second treatment arm providing information on how the county redistributed part of the tax revenues from richer to poorer school districts (also known as "recapture system"). Before conducting the experiment, we expected that both treatment arms would be adequately powered to detect effects. However, we found that we were underpowered in the second treatment arm. For transparency, we still report the full analysis for the second treatment arm in the appendix.

Before any adjustment resulting from tax appeals, the average subject in our sample owns a home worth $349,988 and pays $7,738 in annual property taxes. There is significant variation in the degree to which households benefit from public education, which is important for our research design: households *with* children account for 25.5% of the sample, and households *without* children account for the remaining 74.5%. Owners can protest "directly" on their own (the main focus of this paper) or they can hire an agent to protest on their behalf. For reference, 30.1% of homeowners in the control group (i.e., those who did not receive any information treatment) protested directly in 2021.

The results indicate that even though the information is publicly available and easily accessible, most households have misperceptions about their respective school shares. When provided with accurate information, we observe that households strongly update their beliefs. Moreover, consistent with the reciprocal motivation hypothesis, the information affects the probability of tax appeals differently for households *with* and *without* children. We start by measuring the average treatment effects of the information. On average, both households *with* and *without* children increase their perception about the share of taxes going to schools. Households *with* children become *less* likely to protest, while households *without* children become *more* likely to file an appeal.

To better assess the magnitude of these effects, we use a Two-Stage Least Squares (2SLS) model. This model estimates the causal effect of beliefs about school share on the probability of protesting by leveraging the exogenous shocks to beliefs induced by the information-provision experiment. We find that the effects are not only statistically significant but economically significant too. Our baseline estimates imply that increasing the (perceived) school share by 10 percentage points (pp) would cause a drop of 4.09 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. These effects amount to 12.1% and 9.6% of the corresponding baseline protest rates, respectively. Furthermore, we show that the effects on protests were consequential in that they subsequently affected the assessed home values. These results are robust to a host of alternative specifications and falsification tests.

Property taxes function in much the same way across counties in Texas and similarly throughout the country (Dobay et al., 2019; World Bank, 2019; Nathan et al., 2020).[6] On one hand, these similarities suggest that our results from Dallas County may be reasonably generalizable to other U.S. counties. On the other hand, there are some unique features of our sample that should be considered when extrapolating to other contexts. First, there are some significant differences between our survey respondents and the broader population of homeowners in Dallas County – for example, survey respondents are more likely to file a tax protest. As a result, the average effects of the information in the subject pool may differ from those among the general population. Second, we conducted our experiment in an area with relatively high-quality public schools. The effects of information about school spending might be much weaker, or even null, in areas where public goods are of lower quality or where local governments are perceived as corrupt. Third, while we examined the effects on tax appeals, the effects may be different for other margins of tax compliance, such as tax delinquency or tax evasion. More broadly, in the language of List (2020), our results can be seen as a wave-1 insight that establishes initial causality and provides the first tests of theory.

Our study relates and contributes to the literature on tax compliance.[7] To explain why tax compliance varies among taxpayers and countries, there are two schools of thought: institutions and tax morale. Abundant research shows that institutions have large effects on tax compliance (Slemrod, 2019). For example, the introduction of withholding and third-party reporting caused a massive increase in tax compliance (Bagchi and Dušek, 2021). In comparison, compelling evidence supporting the role of tax morale is more limited (Luttmer and Singhal, 2014; Slemrod, 2019). Our study provides novel evidence showing that tax

[6] For instance, property taxes provide a significant source of school funding in most of the U.S. (Chen, 2021).

[7] The role of tax morale in shaping individuals' attitudes toward taxation, and in particular the reciprocity mechanism, has also been studied in the lab or using "stated" preferences in the tax aversion literature (e.g., Sussman and Olivola, 2011; Kessler and Norton, 2016; Douenne and Fabre, 2022).

morale *can* be a significant driver of tax compliance and provides evidence on a specific mechanism: reciprocal motivation.

There are a few related studies providing suggestive evidence that are consistent with our results which find that rewarding taxpayers with public services has a positive effect on their subsequent tax compliance.[8] Cullen et al. (2021) provides quasi-experimental evidence that tax evasion decreases when the political party of the taxpayer is in control of the presidency.[9] Carrillo et al. (2021) conducted an experiment in which 400 taxpayers from an Argentine municipality were randomly selected to be publicly recognized for their tax compliance and were awarded the construction of a sidewalk near their homes. They found that their intervention had a positive effect on subsequent tax compliance. Krause (2020) found that tax payments increased 27% as a consequence of an intervention that increased municipal garbage removal in some randomly selected census blocks in Carrefour, Haiti. Lastly, Kresch et al. (2023) provides non-experimental evidence from Manaus, Brazil, showing that households with access to the city sewer system are more likely to pay property taxes. We contribute to the literature in two ways. First, we provide experimental evidence to identify causal effects. Second, we identify and quantify more precisely the causal mechanisms at play. For instance, ours is the first study to measure taxpayers' perceptions about the destination of their tax dollars. Moreover, interventions from previous studies combine multiple features, making it challenging to identify the precise mechanisms at play. For example, the intervention in Carrillo et al. (2021) jointly awards taxpayers with social recognition and the construction of a sidewalk near their homes. The bundled nature of this experimental intervention makes it impossible to identify whether the effects on tax compliance are due to social recognition, the construction of the sidewalk, or a combination of both.

Our setting has some differences with respect to the existing literature on tax morale that are worth discussing. We study taxes in a high enforcement context. The existing institutions, such as tax payments via escrow (Cabral and Hoxby, 2012), tax lien sales, foreclosure of delinquent properties, and wage garnishments, lead to extremely high rates of property tax compliance. In our subject pool, for example, only 0.42% failed to pay their property taxes.[10] In comparison, the related studies were conducted in developing countries such as Argentina, Brazil, Haiti, and Malawi, where more than 50% of households failed to pay their property taxes. In low enforcement contexts, there is scope to study tax morale by looking at outcomes such as tax evasion or tax delinquency. However, in high enforcement

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

[9] In the context of a laboratory study, Huet-Vaughn et al. (2019) provides related laboratory evidence showing that partisanship can affect attitudes towards taxation.

[10] For the property taxes from the 2021 fiscal year, which were due on January, 31st of 2022, 9 out of the 2,110 households (0.42%) of our subject pool had not paid their property taxes by October 22nd, 2022.

contexts, households end up paying their taxes whether they like it or not. By looking at a different margin of tax compliance (legal tax avoidance via appeals), we can study tax morale even in a context of high enforcement. However, it is important to note that these differences may limit the direct comparability of our findings to those of studies focusing on tax evasion. For instance, illegal tax evasion, legal tax avoidance, and legal property tax protests are likely viewed through different moral frameworks by taxpayers: tax evasion is probably viewed in a much more negative light than tax protests.

Our findings can also speak to a literature that uses correspondence experiments to study tax morale (Slemrod, 2019). These studies typically randomize a message of moral suasion, such as a reminder that paying taxes is the right thing to do, and then measure the effects of that message on subsequent tax compliance.[11] Antinyan and Asatryan (2019) conducted a meta-analysis of about 1,000 treatment effects from 45 randomized control trials and concluded that "interventions pointing to elements of individual tax morale (...) are on average ineffective in curbing tax evasion, while deterrence nudges (...) are potent catalysts of compliance."[12] In a review, Slemrod (2019) reaches a similar conclusion.[13] We want to highlight two advantages of our experimental design that enabled us to detect tax morale effects in ways that would have been difficult with traditional correspondence studies. First, because we randomized the information provision within the survey, we can confirm that subjects actually saw the information. In traditional correspondence studies, the vast majority of subjects may not read the correspondence carefully or might discard it without even opening it (Bottan and Perez-Truglia, 2020; Nathan et al., 2020). Second, it is crucial to consider the possibility of highly heterogeneous effects of information. The same piece of information can have opposite effects on different groups, such as households *with* or *without* children or those with prior beliefs that under- or over-estimate the truth.[14] These opposing effects across different groups may, on average, cancel each other out, leading to the erroneous conclusion that tax morale is irrelevant to tax compliance decisions. Our methodological approach allows us to disentangle these sources of heterogeneity. We hope these methodological insights aid researchers to better explore tax morale in other settings.

[11] For a seminal contribution, see Blumenthal et al. (2001).

[12] The messages of moral suasion used in these studies sometimes, but not always, include information related to government services (see e.g., Castro and Scartascini, 2015; Hallsworth et al., 2017; Bowers et al., 2020; De Neve et al., 2021; Carrillo et al., 2021; Bergolo et al., 2023). When looking specifically at the group of interventions including information on government services, Antinyan and Asatryan (2019) arrive at the same conclusion. As for the broad group of moral suasion messages, interventions including information on government services are also ineffective. However, despite the average findings suggesting null effects, there are a few exceptions (e.g., Del Carpio, 2014; Bott et al., 2020).

[13] Slemrod (2019) concludes, "In sum, a plethora of studies have failed to find evidence that appeals to tax morale, defined broadly, affect taxpayer behavior in the short run when delivered via a one-time mailing."

[14] For related evidence on the importance of treatment heterogeneity in the context of tax morale, see Castro and Scartascini (2015).

The remainder of the paper proceeds as follows. Section 2 describes the institutional context. Section 3 discusses the data, experimental design and implementation. Section 4 presents the results. The last section concludes.

2 Institutional Context and Conceptual Framework

2.1 Property Taxes and Public Schools

In Dallas County, property taxes fund various public services, such as schools, roads, parks, and police and fire departments.[15] In 2021, the average home in Dallas County was worth \$327,690. The average estimated property tax bill was \$6,370, implying an effective tax rate of 1.94%.[16] Texas does not have a state income tax. To compensate, revenues from property taxes fund a greater share of local government services in Texas than in many states. School taxes comprise the largest share of property taxes, accounting on average for nearly half (49.78%) of the total property tax bill. There is variation in the share of school taxes between households. For example, in our subject pool, the school share is 41.57% for the 10th percentile and 57.01% for the 90th percentile.[17] The second largest component is the city tax (accounting for approximately 28% of property taxes), followed by the hospital (10%), county (8%), college (4%), and special district (<1%) taxes.

Dallas County has 16 major Independent School Districts (ISDs). Homeowners who live within the geographical boundaries of a given ISD jurisdiction are subject to the tax rate for that ISD. Households also have the right to send their children to their district's K–12 public schools. 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 great quality.[18] Alternatively, homeowners can send their children to private schools, opt for homeschooling, or enter a lottery for the chance to send their children to charter schools, which are tuition-free public schools that receive state and federal funding and do not receive funding from the district's property taxes.[19]

[15] In this sub-section, we present the most important features of the institutional context. More details on the definition of the samples of interest and additional information on the property tax system in Texas are reported in Appendices A.1 and A.2.

[16] There is heterogeneity in the effective tax rate that households pay, with some households paying a rate that is as much as 1 pp below or above the average rate – for more details, see Nathan et al. (2023).

[17] These differences are due to a host of factors such as differences in jurisdictional tax rates across districts and household-specific exemptions such as the homestead cap – more details in Nathan et al. (2020).

[18] 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).

[19] ISDs in Texas can contract with charter schools (Senate Bill 188). No such contract exists in Dallas County.

Our definition of "households *with* children" comprises households with children enrolled in local public schools administered by the ISDs since these are funded with property taxes. The category "households *without* children" includes households without school-aged children as well as those with school-aged children but attending private schools, charter schools, or home-schooling. Note that while charter schools are public institutions, we do not include them in the definition of "households *with* children" because charter schools are not funded with property taxes. In the subject pool, the category "households *without* children" is comprised of 83.1% of households without school-aged children and a small minority of 16.9% of households with school-aged children but attending private schools, charter schools, or home-schooled. Unfortunately, we did not include a survey question to differentiate between the three subcategories. However, based on publicly available statistics, our best guess is that the category "households *without* children" includes approximately 4.1% of households with children in private schools, 6.4% with children in charter schools, and 6.4% with home-schooled children.[20]

2.2 Tax Protests

Each year, the DCAD performs 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 1st. The DCAD makes this information available to all homeowners through its website and by mail.[21] The notice includes additional information, such as the estimated taxes due based on the property's proposed values and how property taxes are allocated across jurisdiction types (e.g., school and city taxes). After the notifications are sent, households have a month from the notification date to file a protest if they disagree with the proposed value. In 2021, the DCAD notified the proposed values on April 16; as a result, the deadline to protest was May 17.

Homeowners can file a protest directly or hire an agent to help them. Agents typically charge a combination of a flat fee and a percentage of the tax savings, which can be as high as 50% of the savings. We explain in Section 3.5 that our main focus is on direct protests. Homeowners can file a direct protest by mail using a form included with their mailed notice, or

[20] These calculations are based on estimates from the Texas Public Policy Fundation (2022) indicating that in the Dallas-Fort Worth-Arlington metropolitan area, 79.3% of students were enrolled in local public schools, 7.9% in charter schools, 5.0% in private schools and 7.8% were home-schooled in the 2020-2021 school year.

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

they can file a protest online using a simple tool called uFile.[22] After reviewing the argument, the DCAD can (and often does) make an offer by mail or phone to reduce the home's assessed value. If the homeowner refuses to pay this settlement value or the DCAD does not offer a settlement, the appeal proceeds to a formal hearing with the Appraisal Review Board. Once the protests are resolved, the new tax amount becomes payable immediately or at the billing date if it is later (i.e., on October 1st, 2021). 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 that involves significant ambiguity and subjectivity. The DCAD uses statistical models and large datasets (e.g., recent home sales) to estimate each property's market value. 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 is important for interpreting our results because it implies that households are not trying to objectively "correct" estimates from the DCAD. Instead, they present 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 details, see Nathan et al., 2020) and not necessarily to complain about the county's estimate of their home value.

Tax protests in Dallas County operate similarly to how they operate across all 241 counties in Texas. Indeed, Appraisal District Boards (e.g., DCAD in Dallas) are political subdivisions of the Texas Comptroller of Public Accounts, and homeowners in every county in Texas can protest using the same form provided by the state. For this reason, the prevalence of appeals is likely similar across counties in Texas. Although some form of property value protests exists in other states in the United States, we are unaware of any compiled statistics at the national level. However, we know there are similarities in how tax appeals work, and therefore, appeal rates in other states may be similar to those of Texas. For instance, 16.0% of all Dallas County households filed a protest in 2020.[23] In comparison, the percentage of households who filed a tax appeal in Cook County, Illinois, ranged from 10% to 21% annually between 2002 and 2015, with a mean of 14.6% (Avenancio-Leon and Howard, 2022).

[22] To protest online, homeowners need to look up their account (e.g., searching for their 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).

[23] See column (1) in Table 1.

Nevertheless, tax appeals may be less common in other states. For instance, some counties charge a processing fee for filing an appeal.[24] In some states, such as California, appraised values are updated only when properties are sold, which provides less incentive to file a tax appeal if the homeowner purchased the home several years ago (Nathan et al., 2020). As a result, we estimate that the rates of tax appeal in San Francisco were below 4% in recent years.[25]

2.3 Conceptual Framework

In this section, we outline the key predictions of the reciprocal motivation channel. For brevity, we discuss the main intuitions here and provide the formal model in Appendix C.

Consider a government that allocates a household's tax dollars into two spending categories. A portion of the tax dollars funds local public schools referred to as *school* expenditures, while the remainder is allocated to a residual category, *non-school* expenditures, which includes services such as police and roads. There are two types of households: those *with* children, who benefit from both school and non-school expenditures and those *without* children, who do not benefit from school spending but do benefit from non-school expenditures in the same way as households *with* children. The reciprocal motivation channel assumes that households are more willing to pay taxes (and thus less likely to file a tax appeal) when they perceive that they benefit more from the government services funded by their tax dollars.

Assume there is an increase in a household's perceived school share.[26] We are interested in how this change in perception affects the likelihood of the household filing a tax appeal. The reciprocal motivation channel makes a key prediction: there should be a differential effect between households *with* children and those *without* children. The intuition is straightforward. On the one hand, when the school share increases, households *without* children are more likely to protest since they do not benefit from school expenditures and thus perceive more of their tax dollars as being used for services they do not consider valuable. On the other hand, for households *with* children, the direction of the effect depends on their preference between school and non-school expenditures. For example, if they prefer school expenditures, households *with* children should be less likely to protest when their perceived school share

[24] For example, San Francisco County charges a fee of $60 and Los Angeles County charges a $46 fee.

[25] According to San Francisco Chronicle (2023), there were 1,878 residential and commercial appeals in San Francisco during the 2022-2023 cycle and 6,836 during the 2023-2024 cycle. Based on the number of residential and commercial properties in San Francisco County (source: `https://www.sfassessor.org/news-information/property-data-0`) we estimate appeal rates of 0.8% and 3.2%, respectively.

[26] This simple model assumes that we can change the belief about the school share while holding other beliefs constant. In practice, however, households might become more pessimistic about how efficiently school funds are used in response to the school share information. In that case, households *with* children might become more likely to protest.

increases.[27] Regardless of this preference, the effect on the probability of protesting should always be lower for households *with* children than for those *without* children. Intuitively, when the perceived school share increases, both types of households lose in non-school expenditures; however, households *with* children at least gain in school expenditures. In the remainder of this paper, we leverage a field experiment to test these predictions.[28]

3 Data, Experimental Design, and Implementation

3.1 Data and Sample Selection

To carry out our experiment, we use publicly available administrative data on property taxes and property tax protests from the Dallas County Appraisal District (DCAD, 2021, 2024b).[29] This information includes details about ownership, address, and property characteristics, like square footage and the number of bedrooms and bathrooms, for the different taxing jurisdictions (community college, hospital, 31 cities, 16 ISDs, 12 Special Districts and the county itself). Additionally, the data include historical yearly records of proposed and certified market values, exemption amounts, taxable values, and tax rates. Furthermore, detailed information is available on property tax protest records, separating protests conducted directly by the owner and protests conducted with the help of an agent. The raw data available on the DCAD website contains information on more than 800,000 residential and commercial properties. We begin with a sample of 400,193 owner-occupied residential properties, which we will refer to as the "Universe" sample.[30] When necessary, we supplement the administrative records with data from other sources, such as REDFIN (RedFin, 2021) and the National Change of Address (NCOA) records (NCOA, 2021).[31]

Of the 400,193 properties, we selected a sub-sample of 78,128 households to receive a letter inviting them to participate in an experimental survey. We will refer to this sample

[27] Or, if they prefer non-school expenditures, they should become more likely to protest.

[28] Appendix C presents several extensions. For instance, it considers cases where households *without* children derive value from expenditures in public schools because they are not selfish, have benefited from public schools in the past, or expect to benefit from public school expenditures in the future.

[29] The latest version of the data is available in https://www.dallascad.org/DataProducts.aspx. We downloaded most of the baseline information on April 16, 2021, the day the DCAD notified the proposed values for 2021. Data corresponding to the post-treatment period was downloaded for the current version of the paper on April 25, 2024. We also obtained auxiliary information on tax rates (DCAD, 2024a) and the recapture system (TEA, 2021).

[30] We arrived at this sample by applying several filters, such as excluding commercial properties, non-owner-occupied residences, and properties in two ISDs – Ferris and Grapevine-Colleyville– from which only a marginal area belongs to Dallas County. See Appendix A.2 for details of the selection criteria.

[31] Replication data and code for this study can be found in the online repository Inter-University Consortium for Political and Social Research (ICPSR) #209122 (Giaccobasso et al., 2024)

as the "Letter Sample." We designed the sample criteria to ensure a wide representation of richer and poorer school districts. More specifically, we oversample households from the richer ISDs in Dallas County (Carrolton, Coppell, and Highland Park). All homeowners in these three districts were selected for the letter sample. We also oversample households who experienced increases in their estimated taxes because they are more likely to consider filing a tax protest (Jones, 2019; Nathan et al., 2020).[32] The sample criteria are explained in more detail in Appendix A.2.

Panel (a) of Table 1 presents descriptive statistics for some key variables based on the information available in the administrative records at baseline. Column (1) corresponds to the universe, while column (2) corresponds to the letter sample.[33] By construction, properties in the letter sample are more expensive and consequently pay more in property taxes, although the share of property taxes that correspond to school taxes is similar for the letter sample (50.60%) and the universe (49.77%). Regarding protest history, the homeowners selected to receive the letter seem slightly more likely to file a protest directly (e.g., in 2020, the direct protest rate was 8.83% for this sample vs. 7.96% in the universe sample).

3.2 Subject Recruitment

We sent a letter to the 78,128 households in the letter sample, inviting them to participate in an online survey. The letter included a URL to access the survey. We mailed our letters so they would be delivered close to the time that homeowners in Dallas County could start filing tax appeals. Appendix D shows a sample envelope, and Appendix E shows a sample letter. We include 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 university logo, the name of a professor from that university, and non-profit organization postage. The letter included a physical address for the researcher and a link to the study's website (see Appendix F for a screenshot of the website). It also provided contact information for the researchers and the 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. The letter also mentioned the proposed value of the recipient's

[32] More precisely, for the 11 remaining districts, we sorted the data by the percentage increase in the estimated property tax bill (relative to 2020) and a randomly generated number. We then selected the first 5,200 properties within each school district to be invited to the survey.

[33] Appendix A.3 contains a more detailed description of each subgroup and a more thorough discussion of the property characteristics.

home and the estimated amount of property tax for 2021.

Importantly, we can link the survey responses to the 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.[34] As a second incentive, some subjects were informed that they would enter a raffle for 20 prizes worth $100 each.[35]

3.3 Survey Design

In this section, we summarize the main features of the survey.[36] We start by asking a critical question: whether the respondent's household has children enrolled in grades K–12 at their local public school district and, if so, how many. This important information is not included in the tax agency's administrative records. The survey can be summarized as follows:

- **Step 1 (Elicit Prior Belief):** We begin by providing the estimated total property tax amount for 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 asked 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 "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 next screen, we inform respondents whether they are chosen to receive feedback.

[34] This walk-through included hyperlinks to relevant websites and screenshots of a sample protest using information from a fictitious household for added clarity. To access these instructions, subjects received a URL and a code on the final screen of the survey. Appendix G includes a copy of the Web instructions. Nathan et al. (2020) show that these instructions have a significant positive effect on the protest probability.

[35] 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. The results are presented in Appendix A.4. Overall, the raffle message slightly increased the participation rate by 0.2 pp, an effect that is statistically significant (p-value = 0.047) but economically small (5.4% of the baseline rate).

[36] A sample of the full survey instrument is attached as Appendix I.

- **Step 3 (Elicit Posterior Belief):** We re-elicit the guess they provided in Step 1, which we do for all subjects, regardless of whether they received information. To avoid asking the exact same question twice, we asked about their 2021 taxes (i.e., the most recent year) instead of their 2020 taxes (i.e., the year prior to our intervention). 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 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 should depend on the individual's beliefs before receiving the information. For this reason, we conducted the information-provision experiment within the survey instead of providing the information in the letter to measure beliefs prior to information provision.

The survey also included a module with a second treatment arm where we provided information about the "recapture share:" i.e., the share of school taxes from one's school district that are redistributed towards poor or from rich school districts. Because we found out that we were underpowered to detect effects, we discuss the design and results related to this second treatment arm in Appendix B. We cross-randomized subjects to receive up to two pieces of information, with a 50% probability for each. Thus, roughly 25% of the sample receives both pieces of information, 25% receives the school share information only, 25% receives the recapture share information only, and 25% receives no information at all.

These questions form the core of the survey. We also include a series of additional questions, including the intention to appeal, which serves as a secondary outcome. For descriptive purposes, we include questions about the respondent's gender, age, ethnicity, and education. To provide complementary evidence, we include some additional questions that are described in more detail below.

3.4 Implementation

We timed the intervention so that our letters would arrive early enough before the deadline to appeal as required 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 received survey responses and visits to the study's website starting April 22nd, 2021.[37] Survey responses were linked to each homeowner's information from the administrative records, including whether the subject protested directly or with the help of an agent in any year from 2016 to 2020, property characteristics, home value, tax amount, and school share.

Of the 78,128 households invited to the survey, 2,966 started the survey (i.e., completed at least the first couple of questions), and 2,821 completed the two key modules (i.e., up to the posterior belief on recapture). 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 the same context and using a similar recruitment method (Nathan et al., 2023). 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).[38] Among respondents, the median time to complete the survey was 11.2 minutes. At the end of the survey, we included an attention check similar to that used in other studies (Bottan and Perez-Truglia, 2020), which 92.7% of respondents successfully passed. This passing rate is relatively high for a survey study, especially 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 dropped responses that, as explained in the RCT pre-registration, could not be excluded *ex-ante* due to data availability. We dropped 36 responses from subjects who, according to the DCAD's 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 dropped 185 subjects who, according to the DCAD's records, had already hired a tax agent before starting our survey (for more details, see Appendix A.2).

When studying perceptions through survey data, it is important to deal with outlier beliefs properly. Some individuals may provide wildly inaccurate guesses, not because they genuinely hold such extreme beliefs, but because they misunderstand the question, make a typo, or do not pay enough attention. The "information shocks" for these individuals can be large but meaningless, which can induce substantial attenuation bias to the causal estimates. To reduce sensitivity to outliers, we follow standard practice in information-provision experiments and drop respondents with the most extreme misperceptions in their prior beliefs (see

[37] Appendix A.3 contains more descriptive information about the sample of homeowners who answered the survey, and Appendix A.5 contains more details about the timing of survey responses and discusses in detail attrition rates and balance tests.

[38] 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.

e.g., Fuster et al., 2022; Cullen and Perez-Truglia, 2022; Bottan and Perez-Truglia, 2020). 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.[39] After applying these filters, 2,110 respondents remain, constituting our main subject pool (an implied 2.7% net response rate). Since these exclusions are based on pre-treatment variables (e.g., prior beliefs), they should not compromise the validity of the experimental variation. As a robustness check, we reproduce the analysis with less strict definitions of outliers. Finally, we provide several sharp falsification tests to address any potential concerns about the internal validity of the results, such as event-study analyses.

Panel (a) of Table 1 shows the average pre-treatment characteristics according to the administrative records (e.g., home value, number of bedrooms). A comparison between columns (1) and (2) shows that the households invited to the survey are largely similar to the universe of households: for most characteristics, the differences are statistically significant (due to the large sample sizes) but typically small in magnitude.[40] The comparison between columns (2) and (3) indicates that the households who responded to the survey are largely similar to the sample of households who were invited to participate in the survey. There is a key difference, however: relative to survey non-respondents, survey respondents are more likely to have filed a protest in the recent past and also more likely to protest in 2021.[41] This is mainly by design, as we crafted the letter to attract the attention of households interested in tax protests. As a result, subjects who were at least considering filing a protest in 2021 are more likely to pay attention to the letter and, thus, more likely to notice the survey link included in the letter. Moreover, our letter promises instructions on how to file a protest as a reward for participation, so it is natural that households who are considering filing a protest would be more likely to participate in the survey.[42] Indeed, this higher propensity to protest among survey respondents is consistent with the results from Nathan et al. (2020), who use a similar recruiting method to collect survey responses in this same context.

Prior to any adjustment resulting from protests, column (3) of Table 1 shows that the average subject owns a home with an assessed market value of $349,988 and property taxes of $7,738 (an average tax rate of 2.21%). Panel (b) of Table 1 reports descriptive statistics based on information collected in the survey. The average respondent is 49.6 years old, 42.9% are women, 44.3% are White, and 38.3% have a college degree. Moreover, the proportion of households *with* and *without* children who answered our survey, 25.5% and 74.5%, respec-

[39] For more details on the distribution of outlier observations, see Appendix A.6.

[40] The households invited to the survey are not exactly representative of the universe of households because, as explained above, we applied some filters and intentionally oversampled certain types of households.

[41] Appendix A.3 presents more details of the differences between survey respondents and non-respondents.

[42] Our instructions likely make it easier for survey respondents to file an appeal (Nathan et al., 2020).

tively, approximately matches the proportion of families who have or do not have children in Dallas County: 32.3% and 67.4%, respectively (Statistical Atlas, 2023).

Columns (4) through (7) 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.[43] Column (8) 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.[44] Appendix A.5 presents alternative versions of the randomization balance tests, such as breaking the sample down by households *with* and *without* children. We also show that response rates to the survey and attrition among participants are orthogonal to treatment assignment, which is expected given that subjects can receive information treatments only after starting the survey.

3.5 Outcomes of Interest

As stated in the RCT pre-registration, the main outcome of interest is a dummy variable indicating whether the household protested directly in 2021.[45] To get a sense of the baseline protest rate, we consider subjects in the control group (i.e., those not selected to receive any feedback). Approximately 30.1% of these owners file a tax appeal in 2021. These tax protests are consequential in that they often reduce the home's assessed value. Indeed, we can use the administrative records to measure whether the protests were consequential. For example, in our sample, 65.4% of the protests lead to a decrease in the home's assessed value – these lower assessments can translate into tax savings in the current or future years.

For brevity, in the rest of the paper, we use the term "protest" as a shorthand for direct protests by the homeowner, unless explicitly stated otherwise. Households also have the option to hire an agent to file a protest on their behalf. In addition to 30.1% of owners who protest directly, 4.8% protest through an agent. Although households can hire an agent to protest on their behalf, we designed our experiment focusing on direct protests. Indeed, when forming the subject pool to invite to the survey, we filtered out households whose owners had protested through an agent in previous years. For completeness, we report the effects on

[43] Some questions, such as the respondent's gender, are asked after the information-provision stage. However, treatment assignments should not affect these responses. For example, we do not expect information on school spending to change responses regarding gender or educational level.

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

[45] Information on whether property owners protested their property taxes was downloaded from the DCAD website for the last time on April 25, 2024.

protests through agents, but we expect our intervention to have no effects on this margin.[46]

We included one question after the information-provision stage to serve as a secondary outcome: We asked respondents if they plan to file a protest this year on a 4-point likelihood scale. We construct 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 allows us to detect short-term effects on the *intention* to protest, even if those effects do not materialize into actual protests. For reference, in the control group, 45.4% report that they are very likely to protest. Most importantly, the stated intention to protest is significantly correlated with whether the individual actually files a protest. However, that correlation is far from perfect: the correlation coefficient is 0.410 in the control group (p-value<0.001).[47]

4 Results

4.1 Average Treatment Effects

As an initial examination of the data, we report the average treatment effects of the information separately for households *with* and *without* children. This approach should be taken cautiously, as it does not account for heterogeneous belief updating, making it a less efficient use of the data. However, its simplicity offers a natural starting point.

The results are presented in Figure 1, which reports both the raw mean difference between treatment and control groups, as well as conditional on a set of baseline control variables.[48] Panel (a) shows the average effects on the posterior belief about school share. This panel shows that providing information has a positive and significant effect on the posterior belief about school share by 10.9 pp (p-value<0.001) for households *with* children and by 10.6 pp for

[46] We did not provide the information to the agents. And while the information provided to the homeowners could potentially affect their decisions to hire or fire an agent, we think this is unlikely because of the characteristics of the contracts between homeowners and agents in this setting (e.g., agents protests mechanically every year, homeowners need to submit a form to the county to remove an agent which creates stickiness). Lastly, consistent with these institutional considerations, Nathan et al. (2020) show that their mail intervention had large effects on direct protests but negligible effects on protests through agents.

[47] Among respondents who report being very likely to protest, 56.8% end up protesting directly or through an agent. Among respondents who do not report being very likely to protest, 16.8% end up protesting.

[48] All the control variables are pre-treatment characteristics: the logarithm of total market value in 2021, the growth in total market value between 2020 and 2021, 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 a binding homestead cap 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).

households *without* children (p-value<0.001). Indeed, the difference between households *with* and *without* children is statistically insignificant (p-value=0.823). This evidence suggests that consistent with the results that we report in the next section, households typically underestimate the share of school taxes.

In turn, Figure 1(b) shows the average effects on the probability of filing a tax appeal. More precisely, the dependent variable is an indicator that equals 100 if the subject protested directly in 2021 and 0 otherwise. This figure shows that the information on school share beliefs *decreases* the probability of filing a tax protest for households *with* children (by about -4.77 pp), while it *increases* for households *without* children (by about 4.79 pp). For households *with* children, this average effect is statistically insignificant when estimated without including control variables (p-value=0.230) but becomes statistically significant when including the set of baseline controls (p-value=0.074). For households *without* children, this effect is statistically significant, excluding and including controls (p-values of 0.039 and 0.011, respectively). Moreover, the difference in the effects between households *with* and *without* children is statistically significant in both specifications (p-values of 0.038 without controls and 0.005 with controls). These results provide preliminary evidence consistent with the reciprocal motivation mechanism.

In the following section, we explore the experiment's results in greater detail. First, we document the initial misperceptions and further examine how individuals updated their beliefs in response to the information. Second, we employ a 2SLS model to assess the causal impact of these beliefs on the decision to file a tax appeal.

4.2 Accuracy of Prior Beliefs

Transparency and accountability efforts have made information about property taxes publicly available. Each year, the Dallas Central Appraisal District (DCAD) provides homeowners in Dallas County with a Notice of Appraised Value, which contains a detailed breakdown of the household's property taxes by tax jurisdiction, including the share of their property taxes that funds public schools.[49] But the ease of access to this information does not mean that everyone searches for it or uses it. Many other contexts show that people often misperceive easily accessible information, such as the official inflation rate (Cavallo et al., 2017) or recent trends in national home prices (Bottan and Perez-Truglia, 2020).

[49] See Appendix H for a sample of this notice, with the breakdown by tax jurisdiction shown on the second page. 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 represent the subjects' best approximation when deciding whether to protest.

Figure 2(a) illustrates the distribution of misperceptions about the school share for the 2,110 observations in the subject pool before the experimental treatment.[50] The x-axis corresponds to the difference between the actual school share (i.e., potential feedback) and respondents' perceptions. For brevity, we use the term feedback to refer to potential feedback. A minority of subjects have accurate perceptions: more precisely, only 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 2(a), where more observations fall in the right half of the histogram (corresponding to an underestimation) than in the left half (corresponding to an overestimation). It is important to note that households *with* children do not have more accurate perceptions about the school share than households *without* children. We discuss this in detail in Appendix A.6.

4.3 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, 2022), and home price expectations (Fuster et al., 2022).

We use the subscript i to index the subjects. We use the variable s_i^{prior} to represent subject i's belief about the school share right before the information-provision stage. We use the variable s_i^{feed} to represent the value of the feedback about the school share 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 that information about the school share and 0 if not. We define the variable s_i^{post} as the posterior belief about the school share: s_i^{post} represents the perceived school share after the taxpayer sees or does not see the feedback.

An individual shown feedback will form her posterior belief (s_i^{post}) as the average of the prior belief (s_i^{prior}) and the feedback (s_i^{feed}), weighted by a parameter that captures the degree of learning. This parameter can range from 0 (individuals ignore the feedback) to 1 (individuals fully adjust to the feedback) and is a function of the relative precision of the

[50] Appendix A.6 contains additional information for the entire survey sample without excluding any outliers.

prior belief with respect to the precision of the feedback.[51] This Bayesian updating model can be summarized by the following linear relationship:

$$s_i^{post} - s_i^{prior} = \quad \cdot \left(s_i^{feed} - s_i^{prior}\right) \tag{1}$$

Intuitively, Bayesian learning predicts that, when shown feedback, respondents who overestimate the school share would revise their beliefs downward. In contrast, respondents who underestimate the school share would revise their beliefs upward. Figure 2(b) estimates this Bayesian learning model using a binned scatterplot. The x-axis corresponds to the gaps in prior beliefs ($s_i^{feed} - s_i^{prior}$), and the y-axis corresponds to the belief updating ($s_i^{post} - s_i^{prior}$). The x-axis shows the maximum revision we would expect if the respondent fully reacted 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, under full updating, the observations should form a 45-degree line. The red circles in Figure 2(b) correspond to the subjects shown feedback about the school share. Consistent with significant updating, there is a strong relationship between the updated beliefs and prior gaps: an additional percentage point (pp) in the perception gap is associated with an actual revision of 0.809 pp higher.

The gray squares in Figure 2(b) correspond to 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 may revise their beliefs in the direction of the feedback for spurious reasons even when they do not receive feedback. For example, respondents may reassess their answers or correct typos when asked a question a second time, leading to an answer closer to the truth. The gray squares indicate a weak relationship between belief updating and prior gaps in the group that was not shown the feedback: an additional 1 pp in the prior gap is associated with an actual revision of 0.052 pp higher. This effect is statistically significant (p-value<0.001) but economically very small. This result is consistent with other information-provision experiments that show evidence of spurious revisions (e.g., Fuster et al., 2022; Cullen and Perez-Truglia, 2022).

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

$$s_i^{post} - s_i^{prior} = \tau + \quad \cdot \left(s_i^{feed} - s_i^{prior}\right) \cdot T_i^S + \quad \cdot \left(s_i^{feed} - s_i^{prior}\right) + \epsilon_i \tag{2}$$

This regression forms the basis for the first stage of the 2SLS model. In this model, parameter   represents true learning arising from the information provision, while param-

[51] In the typical model in the literature, the results assume a 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).

eter   captures spurious learning. The parameter   can be calculated from the estimates in Figure 2(b). Specifically, the parameter   corresponds to the difference in the regression slopes between subjects who receive feedback and those who do not. The estimated   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 causes a change of 0.757 pp in the subject's posterior belief. This shows that, although subjects did not fully update to the feedback, they were close to updating fully. This finding of imperfect updating is consistent with other information-provision experiments, and it is likely due to some subjects mistrusting the source of the feedback or simply not paying enough attention to the survey.[52]

4.4 Econometric Model

To measure the causal effect of posterior beliefs on the probability of filing an appeal, we use the same econometric models used in other information-provision experiments (see e.g., Cullen and Perez-Truglia, 2022; Bottan and Perez-Truglia, 2022).[53] Let P_i^{2021} be an indicator variable equal to 100 for individuals filing a protest in 2021 (i.e., post-treatment) and 0 otherwise. Since the effects of school share are expected to be different for households *with* and *without* children, 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. Consider the following equation:

$$P_i^{2021} = {}_0 + {}^S_C \cdot C_i \cdot s_i^{post} + {}^S_{NC} \cdot (1 - C_i) \cdot s_i^{post} + {}_1 \cdot C_i + \epsilon_i \tag{3}$$

where ϵ_i is the usual error term. The two parameters of interest are ${}^S_C$ and ${}^S_{NC}$. We expect ${}^S_C < 0$ and ${}^S_{NC} > 0$, and ${}^S_C - {}^S_{NC} < 0$. Posterior beliefs (s_i^{post}) could be correlated with a host of omitted variables. Therefore, we estimate equation (3) using a 2SLS model that leverages the exogenous variation in posterior beliefs induced by the information-provision experiment. More precisely, we estimate the following model:

$$\begin{aligned} P_i^{2021} = {} & {}_0 + {}^S_C \cdot C_i \cdot s_i^{post} + {}^S_{NC} \cdot (1 - C_i) \cdot s_i^{post} + {}_1 \cdot C_i + \\ & + {}_2 \cdot C_i \cdot (s_i^{feed} - s_i^{prior}) + {}_3 \cdot (1 - C_i) \cdot (s_i^{feed} - s_i^{prior}) + X_i \, {}_X + \epsilon_i \end{aligned} \tag{4}$$

The endogenous variables are $C_i \cdot s_i^{post}$ and $(1 - C_i) \cdot s_i^{post}$, for which we use the excluded

[52] Some additional results are presented in the Appendix. Appendix A.6 shows that the belief updating is not different between households *with* and *without* children. Appendices A and B show that learning from feedback is compartmentalized (i.e., subjects do not use the information about the school share to update beliefs about the recapture share, or vice versa).

[53] Since the two information treatments were cross-randomized within the same sample, we estimate all effects simultaneously in a single 2SLS regression. For the sake of brevity, however, the econometric model presented below only includes the terms corresponding to the school share arm.

instruments $C_i \cdot T_i^S \cdot (s_i^{feed} - s_i^{prior})$ and $(1 - C_i) \cdot T_i^S \cdot (s_i^{feed} - s_i^{prior})$.[54]

We can illustrate the intuition behind the model using a simple example. Consider a pair of households *with* children that have the same bias about 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 them. We expect that relative to the subject who does not get the information, the subject who receives the information adjusts his or her perceived school share upwards. 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. Therefore, the information provision is 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. Assume that the +10 pp shock to the belief causes a 2 pp drop in the probability of protesting. Combining these two results, we obtain an estimate $\ {}^{S}_{C} = -0.2$. That is, each 1 pp increase in the perceived school share reduces the probability of protesting by 0.2 pp.[55]

The term X_i in equation (4) corresponds to the same set of controls described in Section 4.1. In principle, the 2SLS model leverages experimental variation, so additional control variables are not needed for causal identification. However, including additional control variables can be helpful, for example, in reducing the variance of the error term and thus improving the statistical precision (McKenzie, 2012).

4.5 2SLS Results

Table 2 shows the results from the 2SLS model and the corresponding reduced form and first-stage models. Unlike Figure 1, which reports the average effects of being assigned to the information treatment, estimates in Table 2 account for heterogeneous belief updating. In panel (a) of Table 2, the dependent variable in column (1) is our main outcome variable: protesting taxes directly in 2021. According to the conceptual framework, the difference in the effects of school share between households *with* children and *without* children should be negative. Consistent with this prediction, the difference between the coefficients with

[54] Note that equation (4) controls for the prior gaps in beliefs $(C_i \cdot (s_i^{feed} - s_i^{prior}))$ and $(1 - C_i) \cdot (s_i^{feed} - s_i^{prior}))$. The inclusion of these control variables ensures that the excluded instruments isolate the information shocks that are driven purely by the random assignment of the feedback (T_i^S).

[55] As is typical in 2SLS models, if treatment effects are heterogeneous, the model estimates the local average treatment effects of beliefs (Imbens and Angrist, 1994). In our context, it would mean that the treatment effects would put more weight on the individuals who updated their beliefs the most in response to the information.

and without children is negative (-0.687), large in magnitude, and statistically significant (p-value=0.007). Similarly, we expect that an increase in the perceived school share should decrease the probability of protesting for households *with* children but should have the opposite effect for households *without* children.[56] The results are also consistent with these predictions: the coefficient for households *with* children is negative (-0.409) and statistically significant (p-value=0.062), while the coefficient for households *without* children is positive (0.278) and statistically significant (p-value=0.031).

These coefficients in column (1) of Table 2(a) are not only statistically significant but also economically large. As a thought experiment, consider what would happen if the perceived school share increases by 10 pp – for reference, this is roughly the magnitude of the average update in beliefs due to the information shock. The estimates indicate that this change would cause a decrease of 4.09 pp ($= 0.409 \cdot 10$) in the probability of filing a protest for households *with* children and an increase of 2.78 pp ($= 0.278 \cdot 10$) in the probability of protesting for households *without* children. These effects would be roughly equivalent to 12.1% and 9.6% of the baseline protest rates (33.86 pp and 28.83 pp, reported in the lower rows of Table 3).

To further illustrate the magnitude of these effects, we can convert them into a money metric. We take advantage of the estimates from Nathan et al. (2020). Using a regression kink design, they estimate that an increase of \$100 in the expected tax savings causes an increase of 2.14 pp in the probability of protesting. We can compare the effects of 4.09 pp (for households *with* children) and 2.78 pp (for households *without* children) against that benchmark. For households *with* children, the effect of 4.09 pp would be equivalent to an effect of -\$191 on the expected tax savings ($= \frac{4.09 \cdot 100}{2.14}$). For households *without* children, the 2.78 pp effect on the protest probability would be equivalent to an effect of \$130 on the expected tax savings ($= \frac{2.78 \cdot 100}{2.14}$).

Column (1) of Table 2(b) presents the reduced form specification, i.e., an OLS regression of the outcome of interest on the instrumental variable. Due to the strong first stage (i.e., strong belief updating), the reduced-form estimates are statistically significant and qualitatively similar to the baseline 2SLS estimates. The coefficients are a bit smaller in the reduced form specification than in the 2SLS specification (e.g., -0.296 versus -0.409 for households *with* children, or 0.224 versus 0.278 for households *without* children), which is expected since the first stage results, presented in panel (c) of Table 2, show that homeowners did not fully update their beliefs incorporating the information provided in the treatment. Besides the expected difference in magnitude, the reduced-form coefficients are consistent with the 2SLS results in both direction and statistical significance.

[56] The negative effect for households *with* children is based on the additional assumption that these households prefer school expenditure over non-school expenditure – for more details, see Appendix C.1.

The 2SLS model used in our preferred specification, reported in panel (a) of Table 2, assumes a linear relationship between school share and the probability of protesting. This means that a 1 pp increase in the perceived school share should have the same effect on the probability of protesting regardless of whether we start at a low or a high value of the prior belief. This is a natural starting point because of its simplicity and because it is a common specification in the literature on information-provision experiments. Reduced form estimates are also useful to probe this linearity assumption. Figure 3(b) presents a binned scatterplot representation of the reduced-form effects of the information provision experiment. The x-axis corresponds to the interaction between the 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 3(b) tries 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. In other words, an additional percentage point in the school feedback treatment seems to have the same incremental effect on the probability of protesting, regardless of whether we start from a prior belief that is somewhat below or somewhat above the accurate feedback.[57] Further, this figure shows that outliers do not drive the regression results discussed above.

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 of protesting in 2021 and 0 otherwise. This outcome measures the intention to protest and allows us to measure whether the effects of the information lead to an intention to protest immediately after the information is provided. As discussed above, the correlation between the intention to protest and the actual protests is far from perfect, so the effects should not be expected to be "mechanically" the same across these two outcome variables. The results from column (2) of Table 2 for the stated intention to protest are consistent with the results from actual protests in column (1). In column (2), the coefficient for households *with* children in panel (a) 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). 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). The estimated effects in panels (b) and (c) of column (2) are also consistent with the corresponding

[57] To test the linearity of this relationship, we estimated a model including a quadratic term, which turned out to be statistically insignificant.

coefficients in column (1).

In Table 3, we present some additional results and robustness checks. For reference, columns (1) and (2) present the results from the baseline specification, corresponding to columns (1) and (2) from panel (a) of Table 2. A concern when using 2SLS estimation is the potential for weak instruments (Stock et al., 2002). Given the strong belief updating documented in Section 4.3, weak instruments should not be a concern in our setting. Nevertheless, we computed the Cragg-Donald F-statistic, which is commonly used to diagnose weak instruments. The F-statistics reported in columns (1) and (2) of Table 2, of 30.10 and 30.22, are substantially above the rule of thumb of F > 10 proposed by Stock et al. (2002).

As explained in Section 3.5, it is highly unlikely that the information provided in our survey would affect protests through an agent. Nevertheless, in column (3) of Table 3, we report the results of protests through agents for completeness. In this column, we report estimates from 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 (-0.015 and -0.030) for both households *with* and *without* children, precisely estimated with standard errors smaller than in column (1), and statistically insignificant (p-values of 0.906 and 0.566). The difference between the coefficients for households *with* and *without* children is close to zero (0.044), precisely estimated, and statistically insignificant (p-value=0.743).

Column (4) of Table 3 provides a falsification test. In this column, we exploit the timing of the information intervention in an event-study fashion. Specifically, we estimate the same baseline regression from column (1), except that we use the protest decision in a pre-treatment year (2020) rather than in the post-treatment year (2021) as the dependent variable. Intuitively, since the information was provided in 2021, it could not possibly affect the decision to protest a year earlier (2020). We, therefore, expect the coefficients from this falsification exercise to be close to zero and statistically insignificant. The results reported in column (4) confirm our expectations. The estimated effects are close to zero (0.110 and -0.065, for households *with* and *without* children, respectively), precisely estimated with standard errors smaller than in column (1), and statistically insignificant (p-values of 0.545 and 0.504); the difference between households *with* children and *without* children is also close to zero (0.175) and statistically insignificant (p-value=0.398). Indeed, 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 a graphical form in Figure 3(a). The x-axis denotes the year of the dependent variable (i.e., whether the owner protests directly in the years 2016 through 2021). This figure focuses on the main result, which corresponds to the difference in coefficients between households *with* children versus *without* children. For example, the 2020 coefficient from Figure 3(a), which takes the value 0.175, corresponds to the coefficient from column (4) of Table 3. As expected, for each pre-treatment year (2016–2020), the coefficients are close to zero and statistically insignificant; by contrast, the coefficient is negative and statistically significant in the post-treatment year (2021).

The main focus of our paper is whether the beliefs about government spending affect the household's decision to file a tax appeal insofar as that choice reveals the willingness to pay taxes. However, in some contexts, it may also be helpful to know whether the protests induced or dissuaded by our treatment were consequential, for example, by changing the home's assessed value. To explore that additional hypothesis, columns (5) through (7) focus on downstream outcomes. In column (5) of Table 3, we measure the effects on the probability of a *successful* protest. More precisely, the dependent variable takes the value of 100 if the subject protested directly in 2021 *and* the protest resulted in a reduction of the assessed home value, and 0 otherwise (i.e., either if the owner did not protest or if the owner protest was not successful). The coefficients reported in column (5) are similar to (and statistically indistinguishable from) those in column (1). This similarity suggests that the marginal protests induced by the treatment were largely successful in changing the assessed market value and that the marginal protests dissuaded by the treatment would have been largely successful too. Column (6) illustrates the same result but uses an alternative outcome equal to the percent change in the assessed home value due to the direct protest.[58] The coefficients reported in column (6) are not directly comparable in magnitude to those reported in column (1) because the scale of the outcome is substantially different: e.g., the mean outcome of the percent change in the assessed market value for the reference group is 1.13 for household *with* children whereas for the direct protest variable is 33.86. However, in terms of the direction of the effects and their statistical significance, the conclusions are the same. Column (7) uses a third measure of success, equal to the estimated percent change in the tax liability due to the direct protest. The effects on tax savings in the first year go in the expected direction but are more muted and statistically insignificant. The tax savings during the first year were small because the homestead cap was binding for a significant fraction of the households in our sample.[59] However, the reduction in assessed home values should eventually be reflected in tax savings during subsequent years since it caps future increases in appraised values.

Finally, columns (8) and (9) of Table 3 present estimates on outcomes that incorporate data from the two years after the intervention: 2022 and 2023. Estimating effects on an extended post-treatment period aims to test whether the treatment induces new protests or simply changes their timing and whether treatment effects carry over a more extended period. In column (8) of Table 3, the dependent variable takes the value 100 (200, 300) if

[58] Consistent with the definition used in column (5), this variable takes the value of 0 either when the subject did not protest or in cases where the protest was unsuccessful.

[59] See Nathan et al. (2020) for a detailed explanation of how the homestead cap works.

the subject protested directly once (twice, three times) in the years 2021, 2022, and 2023, and 0 if the subject did not protest at all. In column (9), we use an indicator variable that takes the value 100 if the individual protested at least once during that period and 0 otherwise. If our treatment in 2021 only changed the timing of protests, but not the decision to protest or not, we should not observe any effects on these two longer-term outcomes. Estimates reported in columns (8) and (9) show statistically significant effects that align with our baseline specification and thus rule out that effects are explained only by changes in the timing of protests. Furthermore, when considering the total number of protests for households *with* children (column (8)), the effect is twice as large (-0.856 vs. -0.409), and the same is observed for the difference between the two types of households. This suggests that not only did the information change the protests in 2021, but the effects may have spilled over to subsequent years too.[60]

4.6 Robustness Checks and Additional Results

Table 4 presents additional robustness checks. In panel (a), we report estimates using the direct protest in 2021 as the outcome variable, whereas, in panel (b), we use the intention to protest variable. Estimates reported in column (1) replicate our baseline specification for reference. Column (2) is identical to the baseline specification but includes additional control variables measured in the survey.[61] On the other hand, column (3) presents results from a specification that does not include any control variables. In both cases, the results are similar in direction, size, and statistical significance compared to the preferred specification.

Columns (4) and (5) of Table 4 analyze the robustness of our results to less stringent definitions of outliers for prior beliefs. While in the baseline specification we exclude the top and bottom 5%, in column (4) we exclude the top and bottom 2.5% and in column (5) we exclude the top and bottom 1%. The results are similar to those of the baseline specification in column (1), although slightly smaller in magnitude. These results are consistent with the arguments in Section 3.4 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, column (6) excludes respondents who do not pass the attention check included at the end of the survey. Consistent with the attention argument, the coefficients are slightly larger when we focus on subjects who pass the attention check.

[60] Additional results using the extended post-treatment period data are reported in Appendix A.7.

[61] The additional control variables included are: 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). These variables are measured at the end of the survey, but some respondents did not finish the entire survey. Including these additional controls reduces the number of observations, which is the main reason why we exclude these variables from the set of baseline controls.

To partially address potential sample selection, in columns (7) and (8) of Table 4, we use inverse probability weights to re-weight the observations in the subject pool to match the letter sample and universe sample, respectively. The results are similar to the baseline results. For example, in the baseline estimates, the treatment effects for the difference between households *with* children and *without* children are -0.687 and -0.678 for the protest probability and the intention to protest, respectively. The estimates when re-weighting to match the letter sample (universe sample) are -0.614 and -0.799 (-0.610 and -0.738), respectively.[62]

Some additional results and robustness checks are presented in the Appendix. In the baseline specification, the category households *without* children combines a strong majority of households that do not have children with a smaller fraction that has children but sends them to private school, charter school or home-school. The rationale for this specification is that none of these households benefit from school expenditures financed by their property taxes. However, in theory, there could be meaningful differences between these sub-groups. For instance, households who send their children to charter schools may react differently because even though they do not benefit from the local school taxes, they do benefit from public education financed by state-level and federal-level taxes. For that reason, in Appendix A.7, we report results for a more detailed breakdown of households (i.e., children in public schools vs. no children vs. children not in public schools), and we show that the main findings are consistent. Appendix A.7 also reports estimates from alternative specifications, excluding all variables related to the recapture treatment, event studies for downstream outcomes, and additional year-by-year estimates for the extended post-treatment period (2021-2023). Appendix A.8 uses as dependent variables other questions included after the information provision stage.

Lastly, Appendix A.9 shows results from a forecast prediction survey that we conducted to assess whether the experimental results are surprising.[63] Specifically, we elicited predictions from 56 experts with publications on related topics about the effect of a 10 pp shock to the belief about the school share, for households *with* and *without* children. The results from this exercise suggest that our experimental findings are not obvious to the sample of experts, and only a minority of the experts predict effects close to the experimental estimates.

4.7 Non-Experimental Evidence

We present non-experimental evidence that complements the experimental evidence presented above. Our survey asked respondents to choose between hypothetical policies in the spirit of

[62] The separate effects for households *with* children and *without* children have larger standard errors but are consistent in sign and magnitude with the baseline results.

[63] The survey instrument used for the forecasting prediction survey is replicated in Appendix J.

Weinzierl (2014) and Saez and Stantcheva (2016). More precisely, 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 must levy a total tax of \$8,000, which can be spread between 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 reciprocal motivation, the respondents should want the household *with* children to pay more taxes than the household *without* children because the former benefits more from this government service. Consistent with this prediction, most (58.8%) respondents assign a higher tax burden to the household *with* children even though both homes are worth the same.[64] This evidence suggests that the logic of reciprocal motivation resonates with most taxpayers.

5 Conclusions

We conducted a natural field experiment to examine the reciprocal motivation hypothesis: Are households more willing to pay taxes if they believe they benefit from government spending? We leverage the fact that households have significant misperceptions about how their tax dollars are allocated. We designed a field experiment to induce exogenous changes in households' perceptions by providing households with information on the destination of their tax dollars. The results reveal that perceptions about where tax dollars go significantly influence the likelihood of filing a tax appeal in the direction predicted by the reciprocal motivation channel. Specifically, when households with children enrolled in public schools learned that a larger portion of their taxes supports local schools, they were less inclined to file tax appeals, indicating an increased willingness to pay due to greater perceived personal benefit. In contrast, households *without* children were more likely to protest their taxes after learning the same information, as they perceived less personal benefit. These effects are both statistically significant and economically meaningful, highlighting the impact of perceptions of government spending on tax compliance.

Our evidence contributes to understanding why some individuals are more willing to pay taxes than others and why tax compliance varies across countries. It has been documented that nations with the most effective government services, such as the Scandinavian countries, also exhibit the highest levels of tax morale (Kleven, 2014). The concept of reciprocal motivation offers a natural explanation for this correlation: better government services cause taxpayers to be more willing to pay their taxes. Moreover, this reciprocal mechanism can lead

[64] For more details, see Appendix A.8.

to self-reinforcing cycles. For example, if taxpayers develop a negative bias about how much they benefit from their tax dollars, they may become less inclined to comply with their tax obligations. This reduced compliance can lead to lower government revenue and diminished services, turning the initial bias into a self-fulfilling prophecy. Investigating these implications, both theoretically and empirically, remains an important avenue for future research.

Our results stress the challenges of public communication policies. First, we document large misperceptions about government spending, even when such information is publicly available. For governments interested in educating their citizens on how tax dollars are spent, they should do more than post information on a website. Additionally, governments may want to simplify the connection between the taxes they collect and the government services they support. In fact, local governments in the United States are already doing this by breaking down property taxes into specific components, such as the school tax and the hospital tax. Even in the simple context of property taxes, however, we still find that taxpayers have large misperceptions about how their tax dollars are spent. In the case of state and federal governments, tax dollars follow a complicated path from taxpayers' pockets to the provision of public services. As a result, there is probably much more room for improvement in how the state and federal governments communicate with their taxpayers.

Our experimental intervention was designed to disentangle causal mechanisms, not to increase average tax compliance. Nevertheless, our findings provide hints for policymakers looking to improve tax compliance. Our results underscore the challenges and limitations of transparency policies and information campaigns. For example, a message highlighting a government service (e.g., public schools) can boost tax compliance among individuals who benefit most from that service (e.g., households *with* children). However, it can reduce compliance from taxpayers who do not benefit from that service (e.g., households *without* children). As a result, these effects may cancel each other out, resulting in a null average effect on tax compliance. In some cases, this approach may even backfire. Our findings suggest that governments may be able to use reciprocal motives to boost average tax compliance, but only if they are willing to target information (e.g., informing households *with* children about public school spending). Also, governments could try to persuade taxpayers that their tax dollars are spent efficiently or that their tax payments are not captured by corrupt politicians or wasted by bureaucrats. To the extent that these messages raise the average taxpayers' perceptions that their tax dollars are well-spent, they also may lead to higher overall tax compliance.

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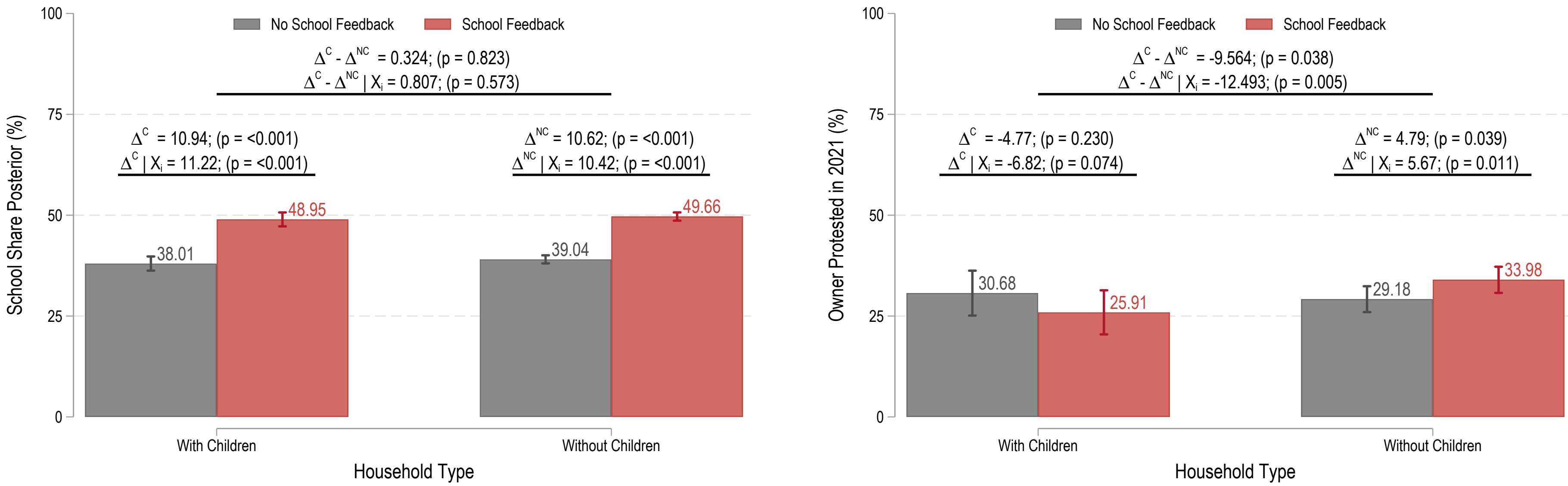
Figure 1: Average Treatment Effects of School Feedback

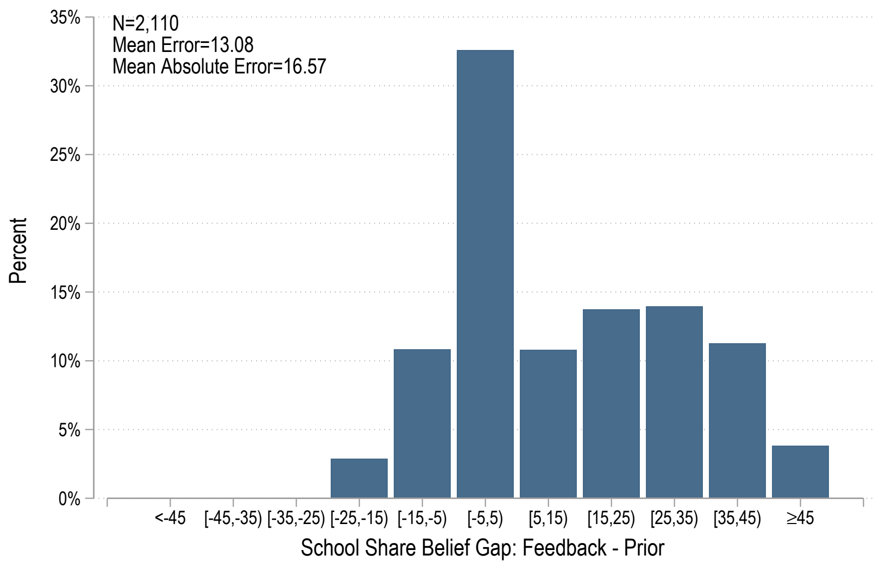
(a) School Share Posterior　　　　**(b)** 2021 Owner Protest



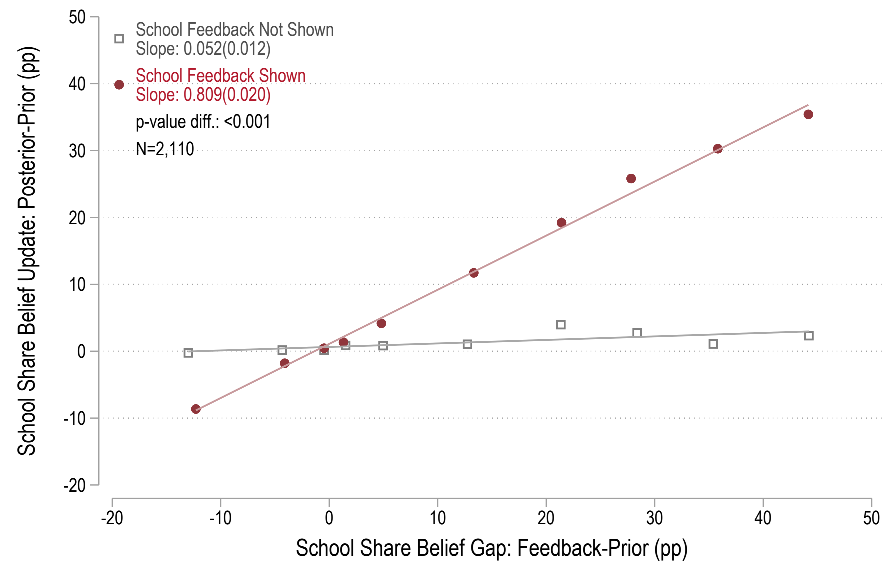
Notes: This figure illustrates the average treatment effects of the school feedback treatment. Results are based on 538 households *with* children and 1,572 households *without* children. Panel (a) reports the mean of the school share posterior by treatment group, separately for households *with* and *without* children. Grey bars represent the average school share posterior for the group that did not receive the school feedback treatment, while red bars represent the average school share posterior for the group that did receive it. In addition, we report: (1) the difference in raw means between the treatment and the control group (Δ^C, and Δ^{NC}), (2) the difference in means conditional on the covariates included in the baseline specification ($\Delta^C|X$ and $\Delta^{NC}|X$), (3) the corresponding p-value of the equality of means test for treated *versus* control groups within each household type, and (4) the p-value corresponding to the comparison of these differences between household types (i.e., a double-difference test). Panel (b) replicates the analysis but uses the probability of protesting as the dependent variable.

Figure 2: Perceptions about the Share of Property Taxes Going to Public Schools

(a) Gap in Prior Beliefs



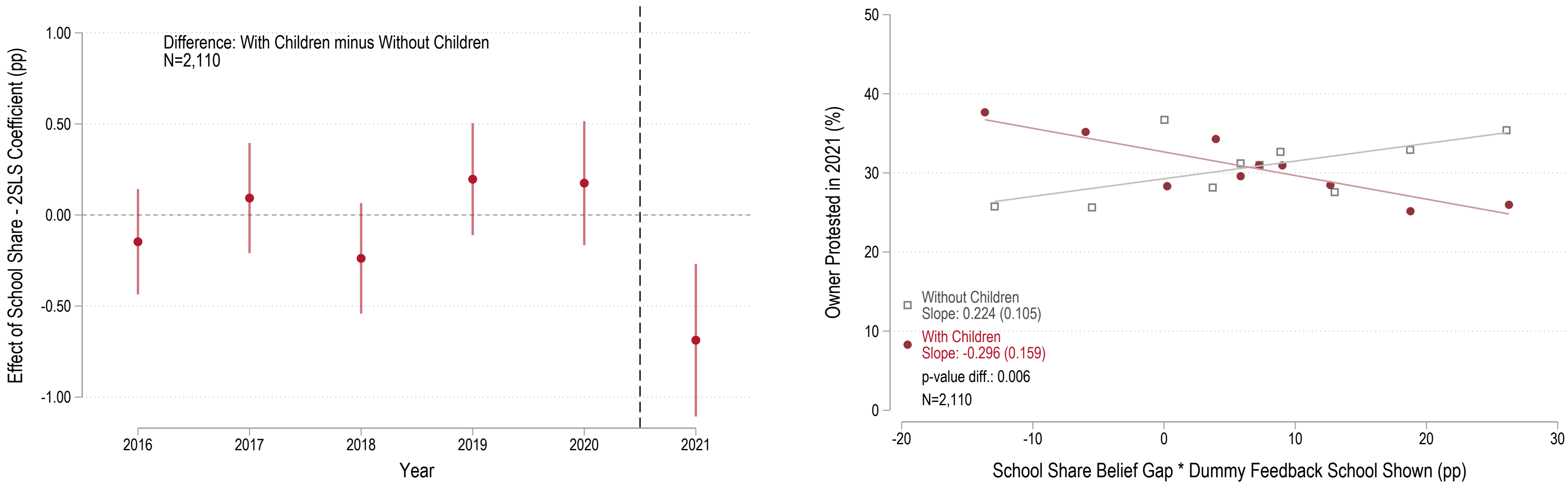
(b) Belief Updating



Notes: Panel (a) shows the gap in prior beliefs about the school share. The x-axis reports the difference between the actual school share and respondents' prior beliefs about the school share in 10 pp-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 mean error, and the mean absolute error. Panel (b) shows how respondents update their beliefs using a binned scatterplot (using ten bins corresponding to each decile of the School Share Belief Gap). 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 with 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 3: The Effects of School Share Perceptions 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 effects of school share beliefs on the probability of protesting for households *with* children versus *without* children. The estimates plotted in this figure correspond with the 2SLS point estimate based on equation (4), 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 3. 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 corresponding years. The vertical dashed line separates the post-treatment year (2021) from the pre-treatment years (2016-2020). Panel (b) depicts a scatterplot representation of the reduced-form effect for households *with* and *without* children separately, using red circles and gray squares respectively and 10 equally-sized bins. 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 direct protest in 2021. Each line corresponds to a separate OLS binned scatterplot regression, including the same control variables used in the 2SLS specification. Control variables for the protest history depend on the year in which the outcome is measured. For instance, if the outcome corresponds to the protest in 2019, the protest history controls include protests in 2016, 2017, and 2018; and so on. 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 2). 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.

Table 1: Balance of Households' Characteristics across Treatment Groups

	Universe (1)	Letter Sample (2)	Subject Pool (3)	Treatment Arm: No Feedback (4)	Recapture Feedback (5)	School Feedback (6)	Both Feedback (7)	p-value test (8)
Panel (a): Admin. Records Variables								
2021 Home Value ($1,000)	327.688	359.145	349.988	365.355	330.631	365.198	340.088	0.163
	(0.651)	(1.632)	(6.774)	(14.907)	(10.302)	(16.461)	(12.037)	
2021 Property Tax Amount ($1,000s)	6.372	7.645	7.738	8.018	7.448	7.960	7.546	0.292
	(0.013)	(0.028)	(0.129)	(0.296)	(0.218)	(0.287)	(0.228)	
School Share (%)	49.777	50.600	50.726	50.603	50.566	50.701	51.029	0.140
	(0.017)	(0.016)	(0.079)	(0.155)	(0.160)	(0.155)	(0.158)	
Recapture Share (%)	-1.957	1.227	1.622	1.852	1.054	2.505	1.130	0.351
	(0.021)	(0.068)	(0.325)	(0.678)	(0.633)	(0.672)	(0.622)	
2020 Owner Protested (%)	7.969	8.832	18.057	23.121	14.815	19.883	14.684	0.000
	(0.043)	(0.102)	(0.838)	(1.852)	(1.530)	(1.764)	(1.527)	
2020 Agent Protested (%)	8.059	6.322	1.659	1.156	2.407	1.754	1.301	0.375
	(0.043)	(0.087)	(0.278)	(0.470)	(0.660)	(0.580)	(0.489)	
2019 Owner Protested (%)	6.055	6.587	13.365	15.029	10.926	14.035	13.569	0.238
	(0.038)	(0.089)	(0.741)	(1.570)	(1.344)	(1.535)	(1.478)	
2018 Owner Protested (%)	5.788	6.443	13.460	13.680	12.407	14.815	13.011	0.697
	(0.037)	(0.088)	(0.743)	(1.510)	(1.420)	(1.570)	(1.452)	
2017 Owner Protested (%)	5.589	5.674	10.853	11.561	11.111	11.891	8.922	0.400
	(0.036)	(0.083)	(0.677)	(1.405)	(1.354)	(1.430)	(1.230)	
2016 Owner Protested (%)	4.412	4.623	7.773	8.478	6.667	8.187	7.807	0.705
	(0.032)	(0.075)	(0.583)	(1.224)	(1.074)	(1.212)	(1.158)	
Multiple Owners (%)	22.173	23.886	24.645	22.929	24.444	25.146	26.022	0.693
	(0.066)	(0.153)	(0.938)	(1.847)	(1.851)	(1.917)	(1.893)	
Living Area (1,000s Sq. Feet)	2.048	2.182	2.313	2.317	2.302	2.331	2.302	0.959
	(0.002)	(0.004)	(0.022)	(0.046)	(0.042)	(0.046)	(0.040)	
Number of Bedrooms	3.119	3.345	3.428	3.432	3.398	3.423	3.459	0.609
	(0.001)	(0.003)	(0.016)	(0.032)	(0.033)	(0.034)	(0.031)	
Number of Baths	2.062	2.171	2.273	2.274	2.272	2.292	2.253	0.883
	(0.001)	(0.003)	(0.017)	(0.034)	(0.033)	(0.039)	(0.032)	
Panel (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	400,193	78,128	2,110	519	540	513	538	

Notes: Average pre-treatment characteristics of homeowners' properties disaggregated by sample. 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) is based on homeowners who were not selected to receive any information (control group). Column (5) is based on homeowners selected to receive information on the recapture share only. Column (6) is based on homeowners selected to receive information on the school share only. Column (7) is based on homeowners selected to receive information on both the school share and the recapture share. Column (8) reports the p-value of a test of equal means across the four treatment groups. Standard errors are reported in parentheses. The statistics in panel (a) are based on administrative records available on the DCAD's website. The statistics in panel (b) are based on survey responses.

Table 2: Main Results: 2SLS, Reduced-Form, and First-Stage

	(1)	(2)
	P_D^{2021}	I^{2021}
a. Effect of School Share (2SLS)		
With Children	-0.409*	-0.408*
	(0.219)	(0.234)
Without Children	0.278**	0.269*
	(0.129)	(0.144)
(Difference Children - No Children)	-0.687***	-0.678**
	(0.255)	(0.275)
	P_D^{2021}	I^{2021}
b. Effect of School Share Belief Gap * Dummy Feedback School Shown (Reduced Form)		
With Children	-0.296*	-0.288*
	(0.159)	(0.172)
Without Children	0.224**	0.217*
	(0.105)	(0.117)
(Difference Children - No Children)	-0.520***	-0.506**
	(0.191)	(0.208)
	s^{post}	s^{post}
c. Effect of School Share Belief Gap * Dummy Feedback School Shown (First Stage)		
With Children	0.736***	0.741***
	(0.044)	(0.044)
Without Children	0.808***	0.805***
	(0.026)	(0.026)
(Difference Children - No Children)	-0.072	-0.064
	(0.051)	(0.051)
Mean Outcome (Baseline)		
With Children	33.86	47.20
Without Children	28.83	44.87
Observation	2,110	2,090

Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. This table shows the 2SLS reduced form, and first-stage 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. In panel (a), 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 of protesting in 2021. Reported estimates are based on the 2SLS econometric model given by model 4 and discussed in Section 4.4. Panel (b) reports the reduced form effects, i.e., it represents the effect of the information included in the feedback message with respect to the prior beliefs without considering how much subjects actually learn from the experiment. In this case, the dependent variables are the same as in panel (a). Finally, the first-stage estimates are reported in panel (c). Estimates in this panel use the school share posterior as the dependent variable and the reported coefficients can be interpreted as the percentage point update in school share posteriors per percentage point of prior school share misperception. Estimates reported in panels (b) and (c) are based on OLS regressions and use the same set of control variables discussed in Section 4.4. Mean outcomes at baseline correspond to subjects who did not receive feedback about the school share nor the recapture share.

Table 3: 2SLS Estimates: Additional Results

	P_D^{2021}	I^{2021}	P_A^{2021}	P_D^{2020}	$P_{D,won}^{2021}$	ΔMV^{2021}	ΔT^{2021}	$\#P_D^{2021-2023}$	$\text{Any}_D^{2021,2023}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Effects of School Share:									
With Children	-0.409*	-0.408*	0.015	0.110	-0.450**	-0.047**	-0.011	-0.856**	-0.439*
	(0.219)	(0.234)	(0.123)	(0.181)	(0.190)	(0.021)	(0.012)	(0.430)	(0.242)
Without Children	0.278**	0.269*	-0.030	-0.065	0.136	0.005	0.003	0.294	0.144
	(0.129)	(0.144)	(0.051)	(0.097)	(0.111)	(0.009)	(0.007)	(0.234)	(0.140)
Difference (Children - No Children)	-0.687***	-0.678**	0.044	0.175	-0.586***	-0.052**	-0.014	-1.150**	-0.583**
	(0.255)	(0.275)	(0.134)	(0.207)	(0.220)	(0.022)	(0.014)	(0.493)	(0.280)
Cragg-Donald F-Statistic	30.10	30.22	30.10	30.02	30.10	30.10	30.10	30.10	30.10
Mean Outcome (Baseline):									
With Children	33.86	47.20	7.09	25.98	20.47	1.13	0.84	74.80	47.24
Without Children	28.83	44.87	4.08	22.19	19.39	1.66	1.04	66.33	45.41
Observations	2,110	2,090	2,110	2,110	2,110	2,110	2,110	2,110	2,110

Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. This table reports 2SLS estimates of equation (4) discussed in Section 4.4. corresponding to the school share treatment effect. We present the coefficients for households *with* children and households *without* children, as well as the difference between these two types of households. The dependent variable in column (1) is an indicator variable that takes the value 100 if the subject protested directly in 2021, and 0 otherwise. 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 subject's protest likelihood in 2021 ("Do you intend to protest this year?"). The dependent variable in column (3) corresponds to an indicator variable that takes the value 100 if the subject used an agent to protest in 2021, whereas in column (4) corresponds to an indicator variable that takes the value 100 if the subject protested directly in 2020. In Column (5), the dependent variable is an indicator that takes the value of 100 if the protest by the owner was successful, and 0 otherwise. Column (6) reports the effects on the market value *savings* from protesting. Negative estimates indicate that post-protest market values are higher. Column (7) uses savings in post-protest estimated tax liabilities as the dependent variable. Again, negative values indicate that post-protest taxes are higher. The dependent variables in columns (8) and (9) consider the protesting behavior in the 2021-2023 period. In column (8), the dependent variable is the total number of protests in 2021-2023 (multiplied by 100 to make it comparable to estimates in column (1)). In column (9), the dependent variable is an indicator that takes the value 100 if the subject protested directly in 2021, 2022, or 2023. Mean outcomes at baseline are computed using the group of subjects who did not receive feedback about the school share nor recapture share (the control group).

Table 4: 2SLS Estimates: Robustness Checks

	Dependent Variable: P_D^{2021}							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
a. Effects of School Share on P_D^{2021}								
With Children	-0.409*	-0.473**	-0.336	-0.364*	-0.252	-0.413*	-0.384	-0.556*
	(0.219)	(0.224)	(0.231)	(0.189)	(0.167)	(0.236)	(0.248)	(0.325)
Without Children	0.278**	0.286**	0.292**	0.196*	0.198*	0.303**	0.230	0.054
	(0.129)	(0.133)	(0.136)	(0.119)	(0.116)	(0.139)	(0.143)	(0.153)
Difference (Children - No Children)	-0.687***	-0.759***	-0.628**	-0.561**	-0.450**	-0.716***	-0.614**	-0.610*
	(0.255)	(0.261)	(0.268)	(0.223)	(0.203)	(0.273)	(0.285)	(0.357)
Observations	2,110	2,070	2,110	2,335	2,482	1,807	2,110	2,091
	Dependent Variable: I_D^{2021}							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
b. Effects of School Share on I_D^{2021}								
With Children	-0.408*	-0.457*	-0.322	-0.250	-0.088	-0.418*	-0.420	-0.461
	(0.234)	(0.235)	(0.246)	(0.205)	(0.191)	(0.247)	(0.262)	(0.360)
Without Children	0.269*	0.286**	0.299**	0.321**	0.256**	0.324**	0.379**	0.277
	(0.144)	(0.146)	(0.147)	(0.132)	(0.130)	(0.153)	(0.163)	(0.176)
Difference (Children - No Children)	-0.678**	-0.744***	-0.622**	-0.571**	-0.344	-0.743**	-0.799***	-0.738*
	(0.275)	(0.278)	(0.286)	(0.244)	(0.231)	(0.290)	(0.308)	(0.397)
Observations	2,090	2,070	2,090	2,309	2,454	1,807	2,090	2,071
Baseline Controls	√	√		√	√	√	√	√
Additional Controls		√						
5% Outliers	√	√	√			√	√	√
2.5% Outliers				√				
1% Outliers					√			
Attention Check						√		
Re-weighted (Exp.)							√	
Re-weighted (Univ.)								√

Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. This table reports 2SLS estimates of equation (4) discussed in Section 4.6 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. In the upper panel of the table, we report coefficients corresponding to regressions where the outcome variable is our main outcome of interest, i.e., direct protests, while column (2) reports estimates on the intention to protest. For reference, column (1) corresponds to our preferred specification reported in columns (1) and (2) in Table 3. Column (2) includes additional control variables collected in the survey: age, gender, college degree, and political party. Column (3) reports estimates where no control variables are included at all. In column (4) we drop 2.5% of the outliers at each tail of the distribution (instead of the 5% used in the baseline specification), while in column (5) we drop 1% of the outliers at each tail. Column (6) restricts the sample to subjects who passed the attention check included in the questionnaire (see Appendix I for the survey). Column (7) reports re-weighted estimates where inverse probability weights are used to match the letter sample. Column (8) does the same but for the universe of non-commercial properties. Estimates are based on inverse probability weighted 2SLS regressions. Weights are obtained from a logit model that regresses a response dummy over the variables included in the administrative records reported in panel (a) of Table 1.

Online Appendix (For Online Publication Only)

"Where Do My Tax Dollars Go?"

Giaccobasso, Nathan, Perez-Truglia and Zentner

November 6, 2024

A Further Details and Results

A.1 More Details about the Institutional Context

In this section, we provide more details about the institutional context. The property tax system in Texas is comprised of four main parts, which are separated to avoid conflicts of interest. The first part consists of local taxing jurisdictions tasked with setting property tax rates. These jurisdictions include counties, cities, school districts, and special districts. The second part is the County Appraisal Districts, tasked with appraising the market value of each real property in the county (which are used to calculate property taxes for each property). County appraisal districts also notify property owners about the market values and estimated property taxes due each year. Each property's total property tax bill is the sum of the taxes due to each jurisdiction. The third part is the County Tax Offices, which collect property taxes on behalf of each jurisdiction where the property resides. They send the property owner(s) a bill, process and track payments, and enforce penalties for delinquencies. The final part of the property tax system is the Appraisal Review Board, a quasi-judicial entity that settles any appeals made by property owners of properties' appraised values and other inputs into the property tax calculation, such as exemptions.[65] See Nathan et al. (2020) for additional details.

To make public school funding more equitable across school districts, Texas enacted a law in 1993 that created what is called the "recapture system," which redistributes ("recaptures") school taxes collected from "property wealthy" districts to "property poor" districts. This system was the result of poor school districts mounting legal challenges to the previous system of state school finances in the late 1980s and early 1990s on state-level constitutionality grounds. There have been several changes to reduce the amount recaptured since the system's inception. The most recent, contained in House Bill 3 and 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. House Bill 3 was passed because the amount of recapture was

[65] 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).

projected to increase rapidly due to rampant growth in property values. However, the amount of redistribution is still substantial under the current recapture formula (Texas Education Agency, 2021c).

Under the current recapture formula (Texas Education Agency, 2021a), there are two ways (called "entitlements") by which ISDs get access to school property tax dollars. First, there is the basic allotment per student (this was \$6,160 per student for the 2020–2021 school year), which may be adjusted based on student and school district characteristics to compute what is called 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.[66] The second entitlement, called the "tier-two enrichment entitlement," rewards school districts that implement steeper tax rates. Specifically, it distributes additional funds to ISDs with tax rates above \$0.9164 (but below \$1.0864) per \$100 in property values.[67]

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 into a large "bucket" (called the M&O State Aid funds) together with several other state revenue sources such as sales taxes. Then, the state computes the amount of state aid (e.g., M&O State Aid) going to each ISD. Districts that do not reach the first-tier entitlement level per student receive state transfers funded with funds recaptured from property-wealthy districts.

A.2 Definition of Subsamples: Universe, Letter, and Survey Samples

The DCAD listed 844,258 property account records at the time they mailed households the Notification of Appraised Value in 2021 (April 16th, 2021). These records included commercial and residential properties, as well as business personal property accounts. In this paper, we use four different samples for various purposes: Universe, Letter Sample, Subject Pool, and No Response Sample. These samples are described in Table A.1 and are defined below.

The "Universe" sample is comprised of 400,193 non-commercial, owner-occupied, residen-

[66] 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, requiring voters' approval).

[67] 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 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.

tial properties in Dallas County that paid property taxes in both 2021 and 2020. The filters used to construct the Universe sample were:

- Exclude business personal property accounts (102,164 records). These are defined as tangible personal property used for income production and include furniture and fixtures, equipment, machinery, computers, inventory held for sale or rental, raw materials, finished goods, and work in process.
- Exclude properties not included in the 2020 records (6,395 records).
- Exclude properties with missing owner name or flagged as private per Texas Property Code (1,609 records).
- Exclude properties that are not single-family residences, townhouses, or condominiums (138,542 records). These properties have the following DCAD STPD Codes: A11, A12, or A13. The excluded properties include mobile homes, commercial or industrial plots, railroads, pipelines, and agricultural land, among others.
- Exclude properties 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 include any keyword that suggests they 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, and fund.
- Exclude properties with mailing and property addresses that have different street numbers or street names. For street numbers, we required exact matches. For property addresses, we used a fuzzy matching algorithm and set a high similarity threshold (75,539 records).
- Exclude properties that have $0 estimated property taxes (12,215 records).
- Exclude properties that have "NO CITY" assigned as the city jurisdiction (1,069 records).
- Exclude properties did not also satisfy the above criteria in the DCAD's records for 2020 (13,161 records).
- Drop properties with two "&" characters in the owner name (13 records), indicating three or more listed owners.

Next, we construct the "Letter Sample", which contains 78,128 properties whose owners were selected to receive a letter with an invitation to participate in the survey. To arrive at this group of households, we proceeded in two steps: First, we applied an additional set of filters to the Universe sample, mostly related to households' characteristics, their property taxes, and their history of protesting using tax agents or not. This yielded 210,289 households. Then, we conducted the randomization process. Below, we detail the filters that yield the pre-randomization dataset:

- Exclude properties with missing information about the number of bedrooms, bathrooms, or total living area; that were worth less than $50,000 and more than $7,500,000 in 2021; or that had 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 to the survey in that experiment and do not belong to school districts that are "property wealthy" (i.e., ISDs that redistribute part of the school property taxes collected towards other districts). We discuss this criterion in greater detail later in this section (42,570 records).
- Exclude properties with one or zero bedrooms (11,680 records).
- Exclude properties with tax ceilings in the school district component of their tax bill (i.e., households with Age 65 or Older or Disability exemptions) (54,376 records).
- Exclude properties whose owners protested through an agent in either 2020 or 2021 before the assessed values were notified, as well as households that do not belong to school districts that are "property wealthy" (16,196 records).
- Exclude properties that protested taxes before the notified values were mailed – i.e., protests that occurred due to reasons unrelated to assessed values (3 records).
- Exclude properties whose owner notified a change of address to the post office (5,996 records).
- Exclude properties that show up on REDFIN as recently sold (2,176 records)

The pre-randomization dataset contains 210,289 properties. We selected 78,128 of them and sent a letter to their owners inviting them to participate in our online survey. The randomization procedure was designed to oversample households from ISDs that contribute the most to the recapture system to increase variation in the recapture share. We also designed the randomization to oversample households who experienced increases in their estimated taxes, which are more likely to file a protest (Nathan et al., 2020). We did this because, in 2021, the DCAD increased the proposed value for a smaller share of households compared to 2020, and for that reason, we expected fewer households to file a protest in 2021. The randomization also undersampled households from larger ISDs to reduce sensitivity to the largest ISDs (e.g., Dallas ISD). More specifically, selection into receiving a letter was stratified as follows. First, all households in the three ISDs with the highest recapture shares (Carrolton, Coppell, and Highland Park ISDs) were selected to receive a letter. For all the remaining ISDs, we sorted the data by the percentage increase in the estimated property tax bill (relative to 2020) and a randomly generated number. We then selected the first 5,200 properties within each school district to be invited to the survey.[68] This selection results in the 78,128 households, which we call the "Letter" sample.[69] It is important to note that the DCAD appraises values of properties located in 16 ISDs. However, two of these 16 ISDs had very few property accounts within the limits of Dallas County: Ferris ISD had 240 accounts, and Grapevine-Colleyville had 191 accounts. Moreover, many of these accounts are excluded from the study because they are business personal properties or commercial properties, not households. Because of this, we excluded these two ISDs from our experimental design.

Within the "letter" sample, randomization for each treatment arm was conducted separately. First, half of the homeowners were randomly selected into the school share treatment group. Then, independent of the school share treatment status, half of the households were selected to receive the recapture share treatment. This results in four types of households according to treatment status, which are distributed as follows: (1) 19,513 were 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.

In total, 3,020 observations households started the survey. Even though we were careful to prevent people from filling out the survey multiple times, 80 of these responses are duplicates due to some households starting to fill in the survey from multiple devices or different internet browsers. To avoid contaminating pre-treatment responses, we only kept the observation corresponding to the first time each household entered the validation code in the survey link. After dropping duplicate answers, we have a total of 2,980 survey responses (both complete and incomplete). In addition, we excluded 159 responses that did not include answers for the key survey variables in the study: prior and posterior beliefs about the school and recapture

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

[69] In addition, half of these households were selected to receive the prize message in their letter, which included explicit references to monetary incentives to participate in the survey. In Section A.4, we discuss the effects of this prize message on the response rate.

shares. We also dropped 185 responses by households that registered a tax agent before the start of the protest season, 36 responses where the household started the survey after they filed a protest according to the administrative records, and 23 responses started after the protest deadline. As explained in Section 3.4, our subject pool also filtered out households with extreme misperceptions about the school or recapture share (top and bottom 5%). This resulted in the exclusion of 218 households with outlying misperceptions about the school share, 215 with outlying misperceptions about the recapture share, and 34 with outlying misperceptions of both. These filters yielded a final survey sample of 2,110 responses, which we used in our main analyses.

A.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 used 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 invited to the survey but did not respond.

Table A.1 presents descriptive statistics for some key variables and for each sample 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 A.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% represent School Taxes. 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 $1.96 from other property richer school districts. In terms of the protest history, in 2020, 7.97% of the homeowners in this sample filed a tax protest directly, and 8.06% filed a protest through an agent. In 2021, both the owner and agent protest rates (3.86% and 7.67%) 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 area of 2,050 square 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 A.1 shows the same characteristics but for homeowners selected to receive the invitation letter. Because of the filters used to build the letter sample (see Section A.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 with school taxes remains about the same (50.60%). However, because we oversampled 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 tax revenues to other property poorer school districts. In terms of protest history, the homeowners selected to receive the letter seem slightly more likely to file a protest directly (e.g., in 2020, the direct protest rate was 8.83% for this sample vs. 7.97% for the universe sample).[70] The owner protest rate in 2021 for the letter sample is also lower compared to 2020 (6.06% versus 8.83%), although the protest rate in 2021 in the letter sample is higher than that in the universe by design since we intentionally oversampled properties that were more likely to protest as explained in Section A.2.

Column (3) of Table A.1 shows the same characteristics but for the 2,110 homeowners that comprise our subject pool, while column (4) shows the characteristics for invited homeowners that did not answer the online survey.[71] Relative to those invited but who did not respond, the homeowners included in the subject pool are representative in a host of characteristics such as property value, property tax amount, share of school taxes, recapture share, number of bedrooms and bathrooms, 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 18.06% for the subject pool, while only 8.50% for the no-response sample). The difference is even larger if we consider protest rates in 2021. This was mainly by design since households that experienced increases in their estimated taxes were oversampled when deciding who would be invited to participate. 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 considering filing 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 in the letter. In addition, our letter promised instructions on how to file a protest as a reward for participation, so it is natural that respondents who are

[70] While the agent protest rate is lower for the letter sample compared to the universe of properties (6.32% vs 8.06% respectively), agent protests in the letter sample correspond only to protests filed 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 A.2.

[71] It is important to recall that the sample described in column (3) is comprised of individuals who filled out the online survey and meet the requirements to be included in the analysis as explained in Section 3.4 and Appendix A.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).

considering protesting are more likely to participate. Finally, 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, we will report some additional information about its distribution next. Figure A.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 A.1(a) for any of the samples considered illustrates that there are differences across households in how much of their taxes are destined to fund public schools. Some of these cross-household differences are because of differences in tax rates across ISD and other jurisdictions (e.g., cities). 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 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 be quite substantial. In addition, Figure A.1(a) shows that the subject pool is 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 underrepresents the extremes of the distribution (i.e., school share below 44% and above 60%).

Figure A.1(b) shows a lot of variation across the 14 ISDs we study regarding the recapture share.[72] 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 A.1(b) also shows that survey respondents are 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 A.2.

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

A.4 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 significantly bump the response rates, which would help inform researchers wanting to conduct similar field experiments. The results are presented in Table A.2. The first row shows that including the mention of 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, they 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.

A.5 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 A.2 reports the timing of the survey responses and protests for the subject pool. Panel (a) reports the cumulative distribution of the day when the homeowners in the subject pool started to fill out the online survey. Invitation letters were created when 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 would be delivered in the next couple of days. Consistent with this projection, we observed the first survey response on April 22nd, labeled in the figure as day 0 on the horizontal axis. More than half of the survey responses (57%) have the start day within the first week (from April 22nd), 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 22nd. The value of the y-axis on the last day on the horizontal axis (+25) represents the direct protest rate for the subject pool (30.76%).[73] Unlike the cumulative distribution of the start date of the online survey, most of the protests were filed close to the deadline (May 17th). By the end of the first week (starting on April 22nd), the protest rate was only about 6%. By the end of the third week, the protest rate reached 16.9%, slightly more than half of the final protest rate.

[73] It is important to note that the formal deadline for protests was on May 17th. However, 94 protests were dated between May 18th and June 19th (late protests are sometimes allowed). For simplicity, we included all these answers in the April 17th bin.

To address potential concerns about imbalances in the response rate or attrition, in Table A.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) 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 subjects started the survey, they should not affect the probability of starting the survey. 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 A.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, theoretically, there may be differential attrition (i.e., some people get assigned to a treatment, making 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 A.3 shows the balance across treatment arms after all the filters were applied to the survey data, as explained in Appendix A.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 A.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 A.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. This suggests that homeowners may drop the survey, but the reasons for this behavior are unrelated 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. Given the many hypotheses being tested in this table, we believe this result is most likely by chance

Finally, Table A.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 A.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 a few other variables that present imbalances (School Share (%) and Race: White, for households *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.

A.6 Robustness of Misperceptions and Belief Updating

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

When studying perceptions via survey data, dealing properly with outlier beliefs is important. Some individuals may provide very inaccurate guesses not because they truly hold extreme beliefs but because they misunderstood the question, made a typo, or were innatentive. The "information shocks" for these individuals will be large but meaningless, which can create a significant attenuation bias. As explained in Section 3.4, 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., 2022; Cullen and Perez-Truglia, 2022; Bottan and Perez-Truglia, 2020). 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 the top and bottom 5% from the recapture share belief gap distribution. This translates into 467 homeowners: 218 with extreme misperceptions about school share, 215 with extreme misperceptions about recapture share, and 34 with extreme misperceptions about both school and recapture shares. Figure A.3 shows the distribution of misperceptions, including the outliers for the school share (panel (a)). 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).

Figure A.3.b shows misperceptions about the school share, as in Figure 2(a) in the main text 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 and the respondents' perceptions. 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 5 pp 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 18.17 pp for households *with* children and 16.02 pp for households *without* children. Finally, school share misperceptions also show a systematic bias in both cases. On average, homeowners in households *with* children underestimate 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 underestimation) than on the left half (corresponding to overestimation).

Next, we look at the belief updating for households *with* and *without* children in public schools. Figure A.4(a) and A.4(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 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 also examine the possibility of cross-learning, i.e., homeowners may update their school share beliefs as a response to the recapture share feedback, which was independently randomized, as explained in Section 3.3, and did not contain any information on school shares.[74] It is important to note that cross-learning from recapture share feedback to school share beliefs cannot occur in principle because survey participants receive the feedback about recapture share after they finish the school share beliefs section of the survey. However, for transparency, we still report these results. Figure A.5(a) reproduces Figure 2(b) from the main text, 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 in the main text, the figure clearly shows the positive effect of receiving the school feedback treatment on the update about school share beliefs. Figure A.5(b) analyzes the same relationship but splits the sample into homeowners who were shown the *recapture* feedback or not. The figure shows no differences in the school share updating patterns between homeowners who received the recapture share treatment and homeowners who did not. This rules out the possibility of cross-learning from the recapture share treatment to school share beliefs. [75]

A.7 Further Results

In this Appendix we provide additional evidence to complement the baseline results using outcomes from administrative records.

First, recall that our baseline specification is based on the original pre-registered experimental design and includes both school share and recapture variables, as described in Section 4.4. In addition, our preferred sample excludes outliers in school feedback priors *or* recapture feedback priors. Hence, Table A.6 presents additional results that show that these specific choices do not drive our main results. For reference, columns (1) and (2) report the baseline estimates presented in columns (1) and (2) of Table 3. Columns (3) and (4) report the estimates of a model excluding the variables related to recapture, i.e., compares individuals who received the school feedback with those who did not, regardless of their recapture feedback status. These two columns use the same definition of outliers from columns (1) and (2). Columns (5) and (6) report estimates excluding the recapture share variables, as in columns (3) and (4), but define outliers only considering school share prior beliefs gaps.

[74] In Appendix B, we conduct the same analysis for the effects of the school share feedback on recapture share beliefs.

[75] It is important to recall that both school and recapture feedback treatment assignments were implemented independently. This means that about half of the households that received the recapture share feedback also received the school share feedback, and the same is true for households that did not receive the recapture share feedback. This explains the positive relation between the school share gap and the school share update observed for both groups. However, the relevant comparison for studying cross-learning is that the difference in the updating coefficients between the two groups is 0.043 and statistically indistinguishable from 0 (p-value$<$0.322). This clearly indicates that the recapture feedback treatment did not affect the updated beliefs about school share.

The results in Table A.6 demonstrate that all estimates are similar in direction, size, and statistical significance.

Second, in the baseline model, the category households *without* children includes both a large majority of households that do not have children and a smaller group that does have children but chooses private schooling, charter schools, or homeschooling. The reasoning behind this classification is that all these households do not directly benefit from public school expenditures funded by their property taxes. To explore whether there are any meaningful differences between these sub-groups, Table A.7 reports estimates analogous to Table 3 but splitting the category of households *without* children into households without school-aged children and households with school-aged children but not attending a local ISD-provided public school. Table A.7 shows that increased school share beliefs have a positive effect on tax protests for both of these two sub-groups. Due to the smaller sample sizes, the coefficients are less precisely estimated. We cannot reject the null hypothesis that the effects are equal among these two sub-groups. The point estimate is somewhat stronger for households with school-aged children but not in public school, but the coefficients are imprecisely estimated so the results must be taken with caution.[76]

Third, in Figure A.6, we replicate the falsification tests analogous to that shown for the protest probability in Figure 3(a), but for the effect on successful protests and changes in home market value in 2021. Similar to the results for the main outcome variable that we show in the main text, the estimated coefficients for these additional variables are small and statistically insignificant during the pre-treatment year (2016–2020). In contrast, for the post-treatment (in 2021), the estimated coefficient is negative and statistically significant.

Finally, Tables A.8 and A.9 present more details about the immediate and subsequent protest behavior. In columns (1) through (6) of Table A.8, for each year, we report estimates of the effect of the treatment on the probability of protesting and on the probability of protesting successfully, i.e., protests that reduced the originally assessed value. As expected, the effects on protesting are strongest in 2021, the year we conducted the experiment. For 2022 and 2023, the effects on the probability of protesting go in the same direction as in 2021 but are weaker. In addition, columns (7) and (8) report effects on the total number of protests (from Table 3) and the total number of successful protests during 2021-2023, and columns (9) and (10) on the probability of protesting (also from Table 3) and protesting

[76] For example, one potential interpretation for these results could be that households who send their children to private schools, who are included in the category with children not attending a local ISD-provided school, may dislike school expenditures because they already pay for their children's schools from their own pockets. On the other hand, households without school-aged children might have more mixed feelings toward public school expenditures. Even though they currently do not enjoy benefits from these expenses, they might benefit from public schools in the future if they expect to have children or might be more sympathetic toward public school expenditures if they benefited from public schools in the past.

successfully in at least one year during 2021-2023. The results are similar in direction and statistical significance and provide two additional insights. First, the fact that the estimated effect on the total number of protests is larger than the 2021 year-specific estimate suggests that effects might have spilled toward subsequent years. Households induced to protest due to the treatment (or dissuaded from protesting) might have acted similarly later. Second, the significant effects observed both in the total number of protests and the probability of ever protesting during the three-year post-treatment period suggest that changes in protesting behavior in 2021, our main outcome of interest, are not simply due to anticipation of protests that were likely to occur in the future in the absence of our intervention. If the reported effects were simply due to households re-timing their protests, we should observe null effects on the probability of protesting at least once during 2021-2023 and on the total number of protests since the protests in 2021 would displace future protests.

In Table A.9, we focus on the year-by-year analysis of downstream outcomes such as changes in assessed values and taxes owed. Columns (1) and (2) reproduce the baseline results for reference. Columns (3) and (4) focus on the percent changes in market values and tax liabilities savings before and after protests in 2021. Intuitively, changes in market value and tax savings can be thought of as intensive-margin measures of success associated with the protesting process. For instance, positive values in market value savings indicate that households saw their property values reduced due to protesting their taxes. Analogously, positive values in tax savings indicate that households pay less taxes after protesting. The effects of the school share treatment on changes in the market value savings reported in column (3) show, as expected, that a reduction in protests is also associated with a reduction in market value savings. For instance, an increase of 1 p.p. in the perceived school share for households *with* children is associated with a reduction of -0.047 % in market value savings. It is important to note that the coefficients reported here are not directly comparable in magnitude to those reported in columns (1) and (2) because the variables are expressed in different units ($ versus percentage points). However, in terms of the direction of the effects, the conclusions are the same. As a reference, we report the mean outcome for the control group in the bottom panel of the table.

Examining responses regarding post-protest tax liabilities is not straightforward because there are several administrative restrictions on how property taxes can be adjusted. In Table A.9, for instance, column (4) shows the expected negative sign on tax savings (-0.011) for households *with* children. However, the effects are smaller in magnitude and statistically insignificant. The more muted effects on tax savings relative to the effects on market value savings can be explained by administrative restrictions, such as the homestead cap. A binding homestead cap implies that lower assessed market values will not be reflected in tax savings in the first year but will most likely affect tax savings in the second year and beyond. Specifically, due to the cap, a lower assessed market value today imposes a limit to the maximum possible tax increases in the future. [77] In our sample, the homestead cap was binding for a significant fraction of the households. It is important to note, however, that regardless of whether the homestead cap is binding in the current year, protests still might benefit homeowners who want to reduce their tax liabilities because they can create savings in the future.

To analyze if the effects of our treatment translate into changes in tax savings in future years when tax liabilities have had more time to adjust, columns (5) through (8) in Table A.9 look at the extended post-treatment period. For instance, column (8) compares the final tax liability in 2023 with the 2021 tax liability notified in the notice of appraisal letter (i.e., pre-intervention). Consistent with the idea that tax adjustments might take longer to materialize, columns (6) and (8) show that the effects on tax savings are stronger when considering an extended period. For instance, for households *with* children, the estimated effect on tax savings when comparing 2022 vs. 2021 is -0.057%, and -0.045% when comparing 2023 vs. 2021. In both cases, the coefficients are larger than the one observed in 2021 (-0.011%). However, these results should be interpreted with caution given that all these measures are also affected by the intrinsic nature of the appraisal process that leads to continuous re-assessments of the value of the properties and, hence, of tax liabilities. In fact, despite being more than three times larger in magnitude compared to the 2021 estimate, this might explain why the longer-term estimates are still statistically insignificant.

A.8 Further Results on Additional Survey Outcomes

In this Appendix we provide additional evidence to complement the baseline results using additional survey outcomes.

Table A.10 reports additional results based on our main 2SLS specification but focuses on additional survey outcomes. In particular, we focus on the following questions that we 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 administrative records. Table A.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.*"

[77] See Nathan et al. (2020) for a detailed explanation of how the homestead cap works.

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 A.10, we reversed the scale for these three questions from its original formulation to make interpretation easier and consistent with other outcome variables. For instance, while question (3) asks about the support for the recapture system, Table A.10 reverses the scale of the question and reports the degree of opposition to the recapture system.

The first thing to notice in Table A.10 is that there are no statistically significant effects of school share on any of these additional survey outcomes for each household type separately or 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.

Figure A.7 discusses the responses to one question included in the survey that provided hypothetical scenarios to the survey respondents to elicit their views about the relative share of taxes that should be paid by households *with* children compared to households *without* children. Specifically, we asked:

"*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.*"

Figure A.7 reports the distribution of the share of taxes that should be paid by households *with* children based on the answers to the question above. 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 percentage of answers in a given bar of 10 pp width.[78] According to the hypothesis of reciprocal motivation, respondents will want the household *with* children to 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 reciprocal motivation channel, 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 reciprocal motivation resonates with most taxpayers.

A.9 Expert Forecasts

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 J. In this survey, which follows best practices (DellaVigna et al., 2020), we describe the experiment and ask experts to forecast key results in a way comparable to experimental estimates. More precisely, we elicit their prediction of 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 invited experts to participate in our survey in two ways. First, we posted the survey on the Social Science Prediction Platform from July 13, 2021, to December 31, 2021. Second, in November 2021 we invited a sample of 238 professors with published research on related topics by email. The final sample includes 56 experts' responses. Of these, 21.4% responded to the survey through the Social Science Prediction Platform, and the remaining 78.6% responded through our email invitation.[79] The final sample is made up of professors (82.1%), Ph.D. students (12.5%), postdocs (3.6%) and other researchers (1.8%). Most of the respondents (78.6%) are economists, 66.1% report having done research on taxation, and 25% have done research on preferences for redistribution.

In Figure A.8, we compare our experimental results with expert predictions. 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 for the effect of a 1 pp increase in the school share.[80] The solid vertical

[78] 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.

[79] Among the responses from the Social Science Prediction Platform, we require that they either are academics, already have a Ph.D. or are currently pursuing one.

[80] 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 A.8, we divide those predictions by 10 to obtain the effect per 1 pp, to compare directly with the 2SLS estimates.

red line in each panel represents the corresponding estimate from the baseline 2SLS model (column (1) of Table 3), and the red shading denotes the corresponding confidence intervals.

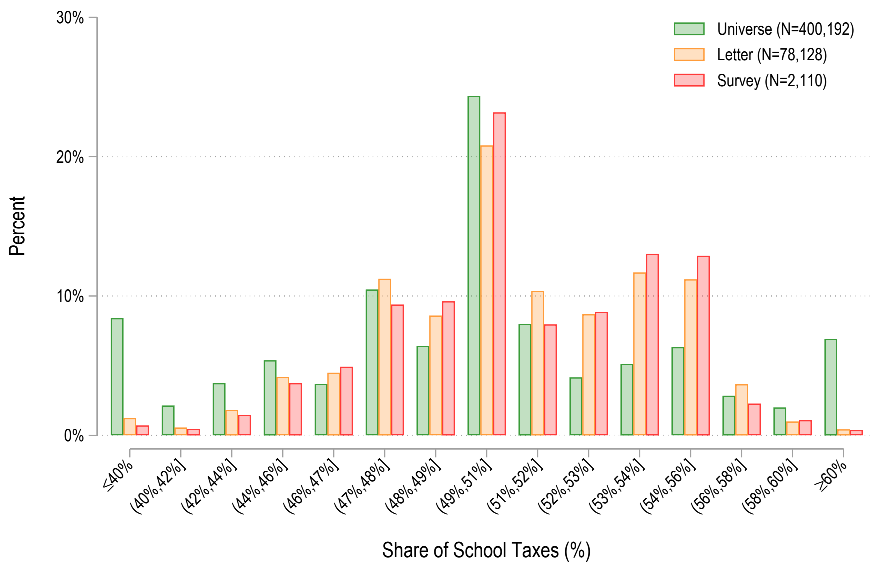
Figure A.8 shows that our experimental findings are not obvious to the sample of experts. Our experimental results are consistent with a minority of experts who predicted that the school share belief would have a negative effect on the protest rate for households *with* children (panel (a)) and a positive effect for households *without* children (panel (b)). They are also 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 experts predict either zero effect or an effect of the opposite sign compared to the experimental findings. In addition, only a few expert predictions are close to the experimental estimates, even if we account for 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, only 17. 9% of the predictions are within the 90% confidence interval of the experimental estimate. That the majority of experts' predictions do not coincide with the experimental findings may not be surprising since their predictions are consistent with the general takeaway from the extant literature on how messages of moral suasion affect tax compliance, which suggests that deterrence nudges are effective, whereas tax morale messages are less effective or have no effects whatsoever (see Antinyan and Asatryan (2019)).

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.[81] In any case, we find that the comparison between the forecasts and experimental estimates is similar if we weigh the forecasts by experts' confidence.

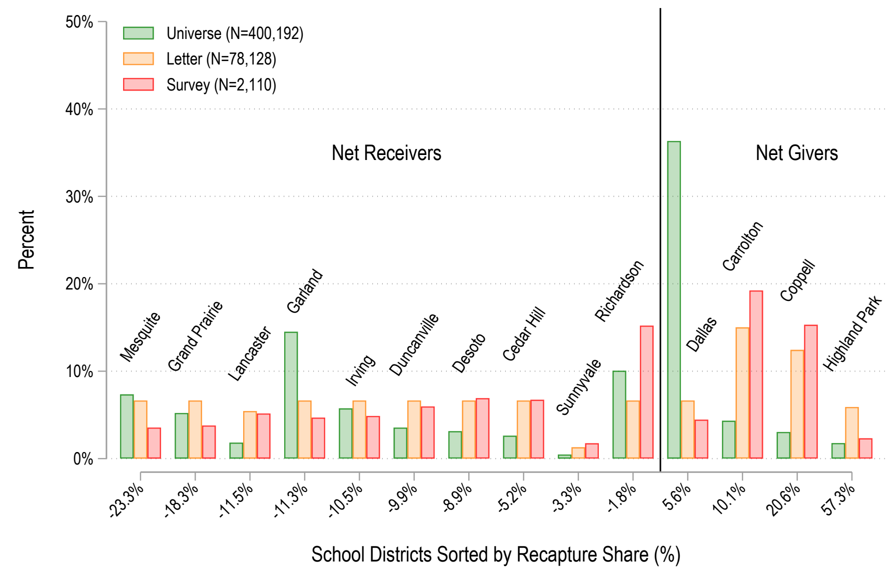
[81] More precisely, 25.0% of experts selected "not confident at all," 51.8% selected "slightly confident," 19.6% selected "somewhat confident," 3.57% selected "very confident," and 0% selected "extremely confident."

Figure A.1: Distributions of School Share and Recapture Share

(a) School Share



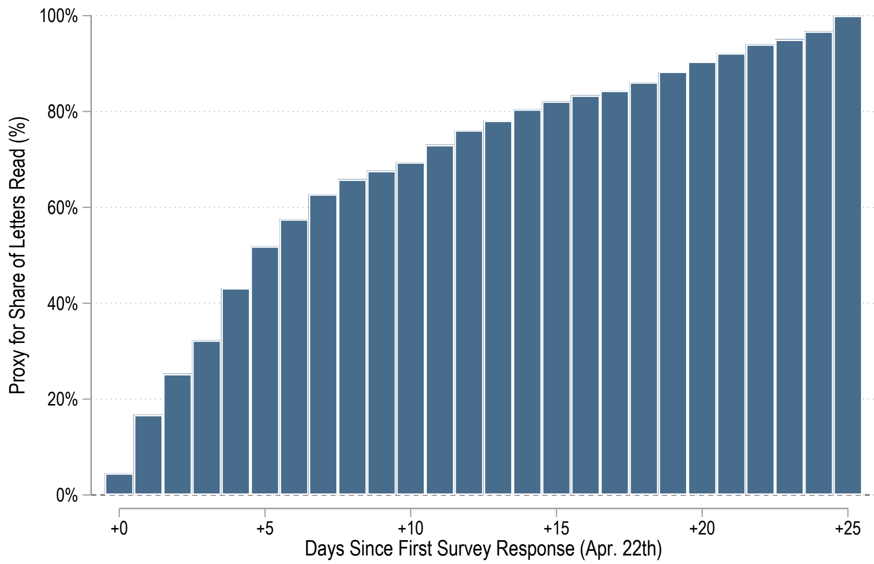
(b) Recapture Share



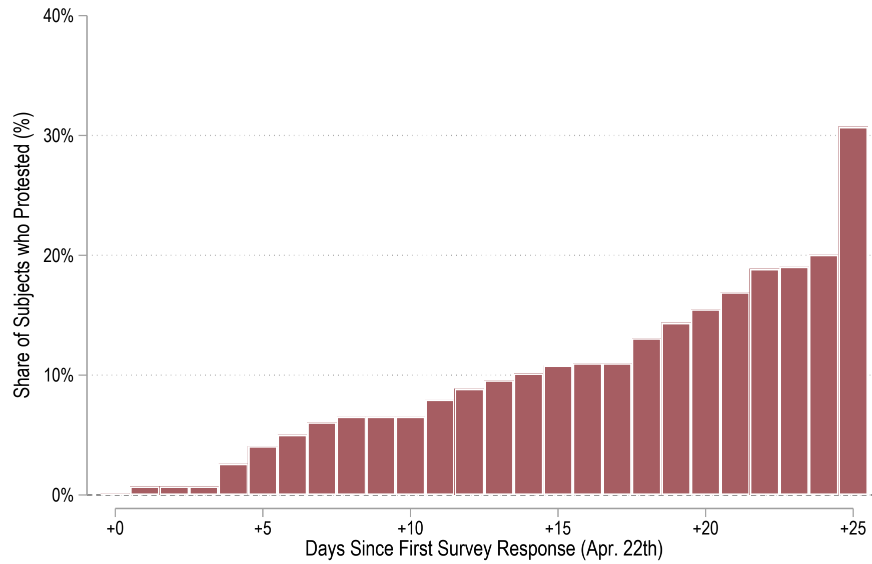
Notes: Panel (a) shows the distribution of school share grouping households in 2 pp bins. Extreme responses are grouped in the ($\leq$ 40%) and ($\geq$ 60%) bins. Panel (b) shows the distribution of properties sorted by their recapture share. Because the recapture share is calculated at the school district 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 A.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 A.2: Timing of Survey Responses and Tax Protests

(a) Timing of Survey Responses



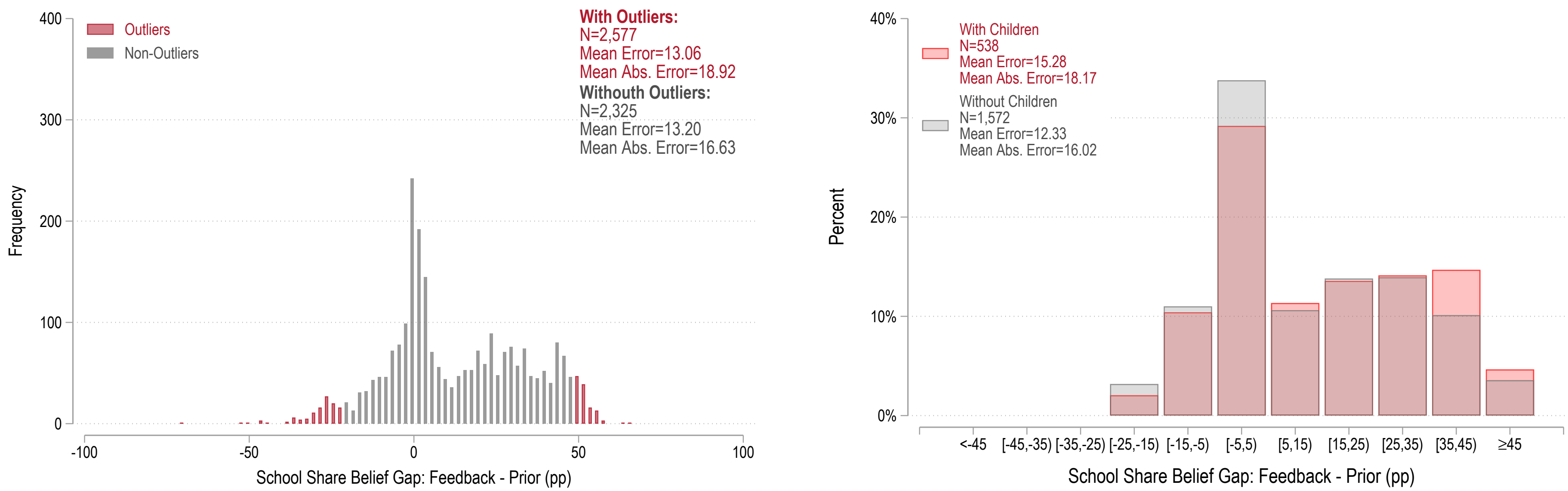
(b) Timing of Subjects' Protests



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 A.3: Distribution of the School Share Gaps

(a) School Share Gap (Full) **(b)** School Share Gap by *With*/*Without* Children

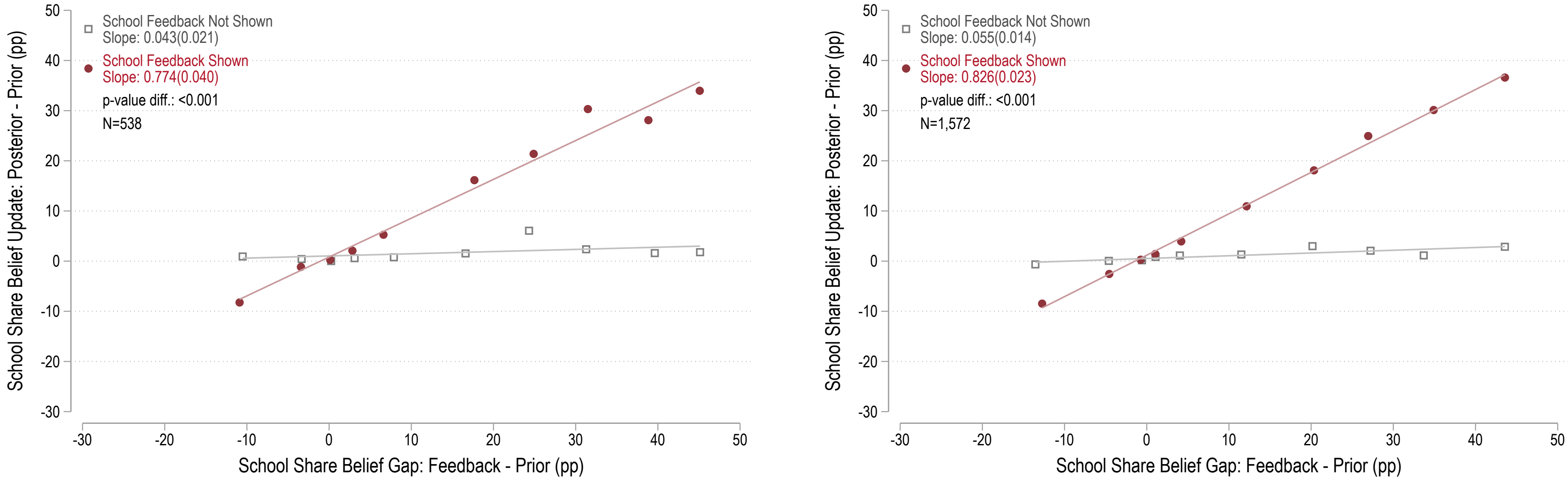


Notes: This figure illustrates the distribution of the school share beliefs gaps, defined as the difference between the actual school share (shown as feedback if selected into the school share treatment group) and the prior belief reported by respondents of the survey. In panel (a), gray bars represent observations that are between the 5% and 95% of the school share distribution. Red bars represent observations in the top and bottom 5%. 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 (see Figure B.2.), and 34 observations for extreme misperceptions in both beliefs. More specifically, the main experimental sample excluded the school share belief gap values $(-\infty, \text{-}20.9]$ and $[48.2, +\infty]$ and recapture share belief gap values $(-\infty, -29.9]$ and $[37.7, +\infty)$. In panel (a) 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 (i.e., bars of 2 pp width). Panel (b) breaks down the analysis by households *with* children in public schools (red bars) and households *without* children in public schools (gray bars), based on the online survey data for the main experimental sample (N=2,110). 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 A.4: Belief Updating: Households *With* Children versus *Without* Children

(a) School Share: *With* Children

(b) School Share: *Without* Children

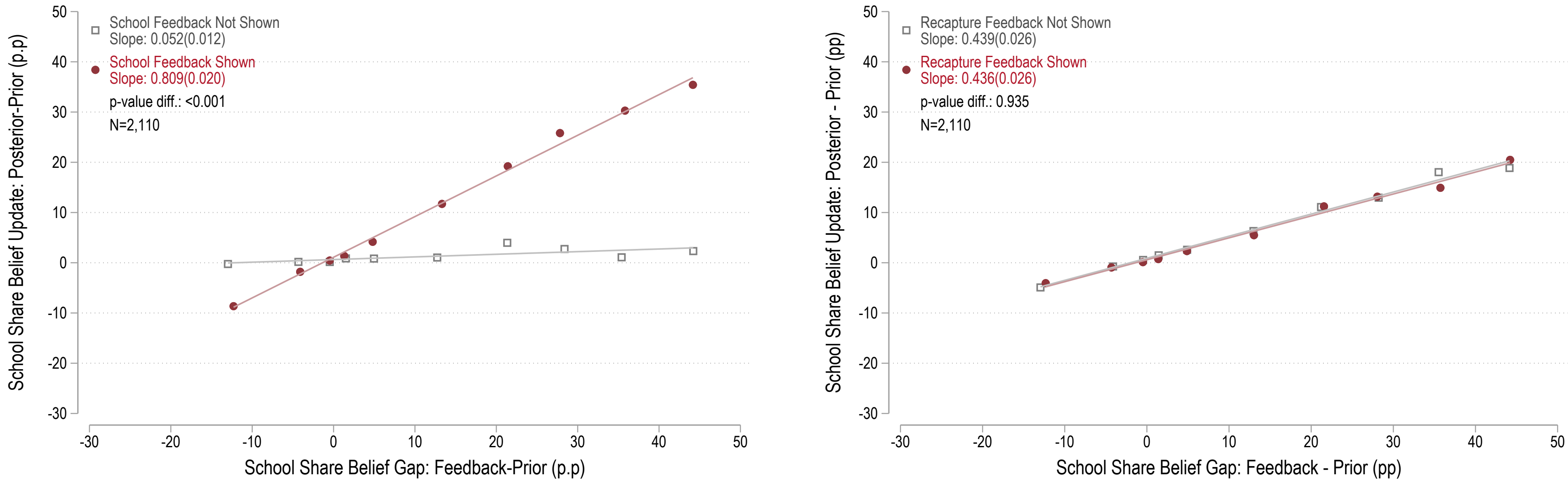


Notes: This figure illustrates the relation between the school shares belief gap and the size of the update (i.e., posterior belief minus prior belief) by treatment status. Both figures are analogous to Figure 2(b), but separating the households *with* and *without* children in public schools. Gray squares 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 with 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 A.5: Cross-Learning in the Information-Provision Experiment

(a) School Feedback on School Share

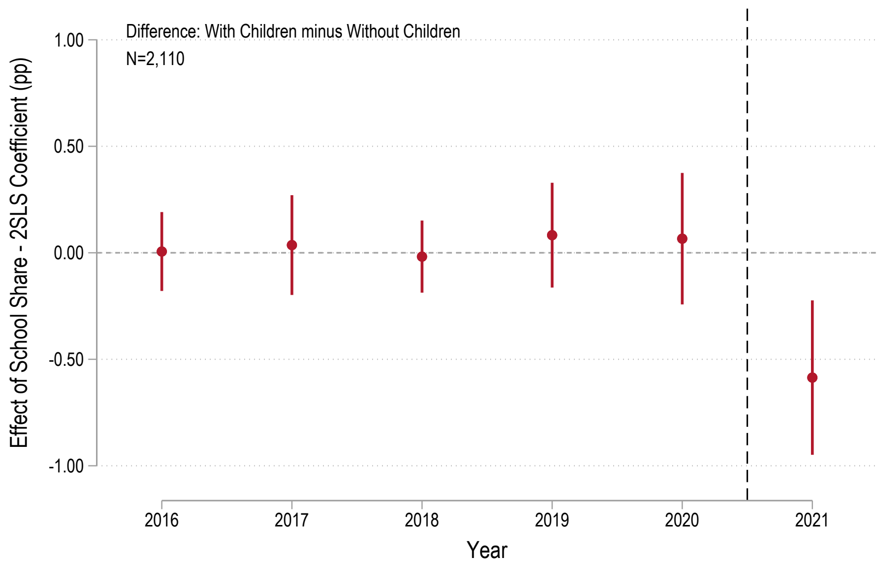
(b) Recapture Feedback on School Share



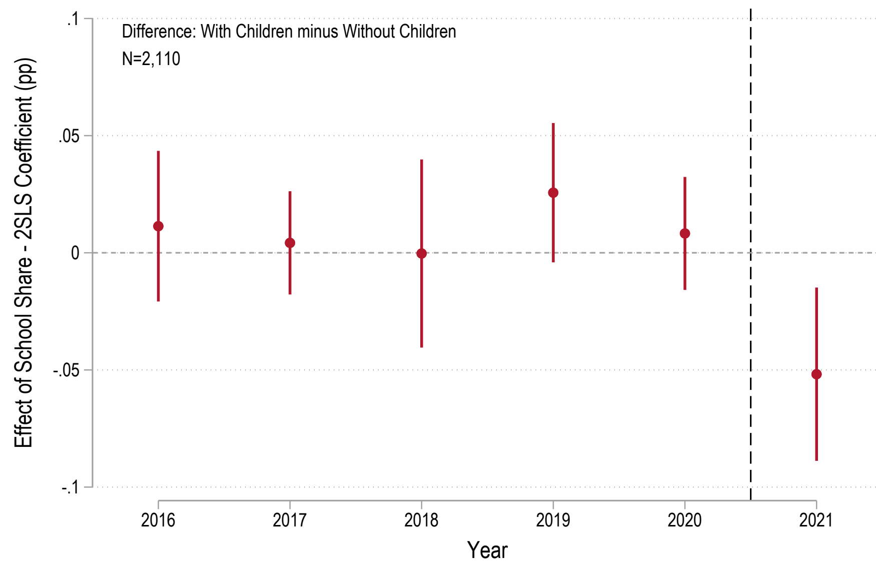
Notes: This figure illustrates the relation between the school shares belief gap and the size of the update (i.e., posterior belief minus prior belief) by treatment status. Panel (a) shows how the school share feedback affects belief updates about the school share, which is the same as Figure 2(b). Panel (b) depicts how people learn about the school share from the recapture share feedback (i.e., cross-learning between experimental arms). 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 with 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 A.6: Treatment Effect of School Share Perceptions on Successful Protests and Market Values

(a) Effect on Succesful Protests

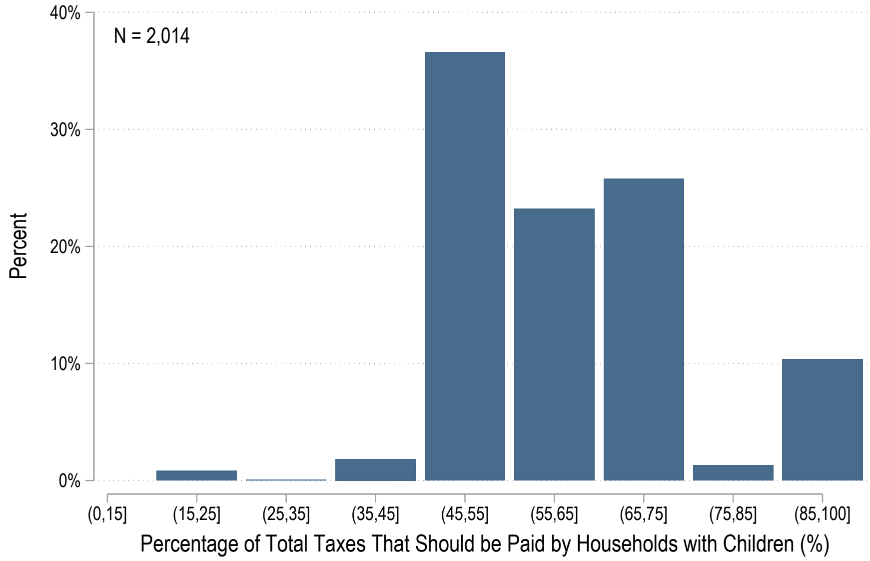


(b) Effects on Market Value Decreases



Notes: This figure depicts an event-study analysis of the differential effect of the school share beliefs for households *with* children versus *without* children. In panel (a), the dependent variable takes the value of 100 if the subject protested directly in 2021 and the protest was successful in reducing the assessed market value, and 0 otherwise (i.e., either if the owner did not protest, or if the owner protest was unsuccessful). In panel (b), the dependent variable represents the percent market value savings as a result of the protest. This is defined as the difference between the post- and pre-protest assessed market value expressed as a percent of the pre-protest value. The estimates plotted in this figure correspond to the 2SLS point estimate based on equation (4), with 90% confidence intervals based on robust standard errors. In panel (a), the coefficient plotted for 2021 is the coefficient reported in column (5) corresponding to the "difference" rows in Table 3. In panel b., it corresponds to estimates reported in column (6) of the same table. 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). The number of observations used in the estimation is reported in the upper left corner.

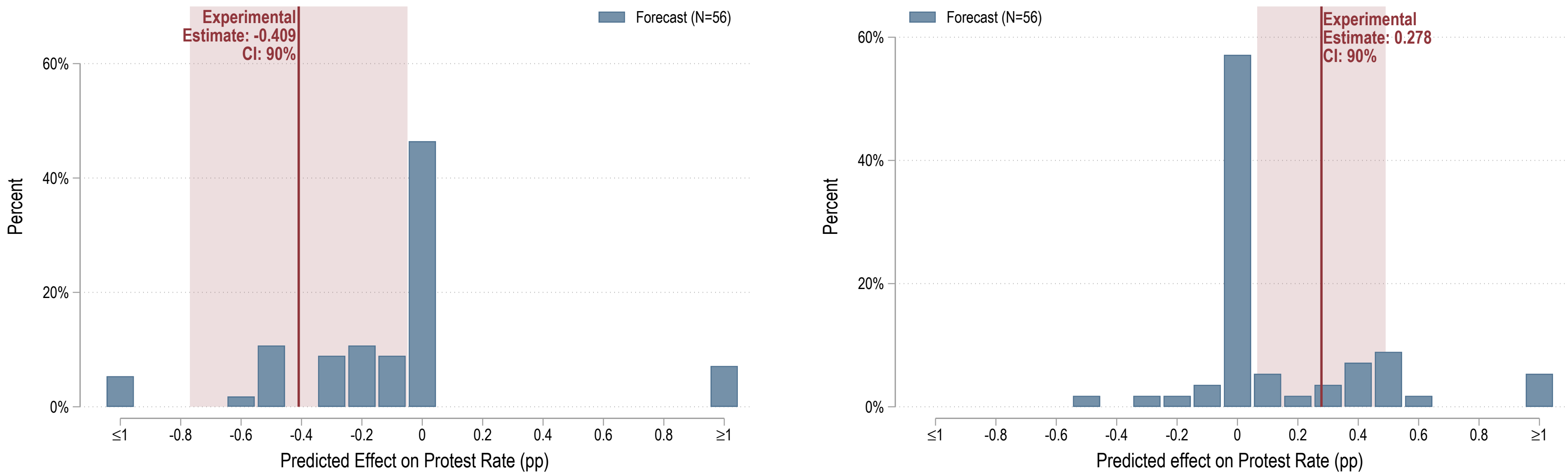
Figure A.7: Hypothetical Question about Property Taxes and Public Schools



Notes: This figure 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 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".* 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).

Figure A.8: The Effects of School Share Perceptions 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 files 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 so these coefficients can be compared directly to the 2SLS estimates. In both panels, we pooled responses that were greater than 1 in absolute value into the corresponding extreme bins. The vertical red solid line corresponds to the experimental estimate based on the 2SLS specification reported in Table 3. The shaded area (in pink) corresponds to the 90% confidence interval. The full questionnaire for the prediction survey can be found in Appendix J.

Table A.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 (%)	3.86	6.06	30.71	5.16
	(0.03)	(0.09)	(1.00)	(0.08)
2021 Agent Protested (%)	7.67	7.35	4.60	7.23
	(0.04)	(0.09)	(0.46)	(0.09)
2020 Owner Protested (%)	7.97	8.83	18.06	8.50
	(0.04)	(0.10)	(0.84)	(0.10)
2020 Agent Protested (%)	8.06	6.32	1.66	6.27
	(0.04)	(0.09)	(0.28)	(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 A.2.

Table A.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 (%)	0.259	0.295	0.031
	(0.012)	(0.012)	
Female (%)	41.261	41.604	0.855
	(1.358)	(1.291)	
Age	50.049	49.337	0.072
	(0.292)	(0.269)	
Race: White (%)	41.997	44.467	0.190
	(1.363)	(1.303)	
Education: Grad. Degree (%)	39.863	42.680	0.133
	(1.352)	(1.297)	
Prior Belief: School Share (%)	37.042	37.717	0.392
	(0.564)	(0.549)	
Prior Belief: Recapture Share (%)	2.546	3.072	0.465
	(0.496)	(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 A.3: Response Rates by Treatment Group

	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 A.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 A.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	-	0.596	-	0.033
	(0.149)	-	(0.297)	-	
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	-	0.309	0.579
	(0.158)	(0.152)	-	(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	-	-	0.155	0.568
	(0.159)	-	-	(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	-	0.153	-	0.156	0.587
	-	(0.153)	-	(0.156)	
Age	-	-	-	-	
	-	-	-	-	
Race	-	0.461	0.481	0.157	0.283
	-	(0.266)	(0.277)	(0.157)	
Education	-	-	-	-	
	-	-	-	-	
Political party	-	-	-	-	
	-	-	-	-	
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 I. 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 A.5: Balance Checks Survey Sample: By Treatment Group

	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 who were 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 A.6: Joint versus Separated Specifications for Treatment Effects

	P_D^{2021} (1)	I^{2021} (2)	P_D^{2021} (3)	I^{2021} (4)	P_D^{2021} (5)	I^{2021} (6)
Effects of School Share:						
With Children	-0.409*	-0.408*	-0.407*	-0.377	-0.342*	-0.384*
	(0.219)	(0.234)	(0.218)	(0.231)	(0.194)	(0.207)
Without Children	0.278**	0.269*	0.278**	0.268*	0.263**	0.229*
	(0.129)	(0.144)	(0.129)	(0.145)	(0.124)	(0.138)
Difference (Children - No Children)	-0.687***	-0.678**	-0.685***	-0.645**	-0.605***	-0.613**
	(0.255)	(0.275)	(0.253)	(0.273)	(0.230)	(0.249)
Cragg-Donald F-Statistic	30.10	30.22	995.69	996.28	1,204.59	1,196.89
Mean Outcome (Baseline):						
With Children	33.86	47.20	31.15	48.46	31.15	48.46
Without Children	28.83	44.87	29.18	46.51	29.69	47.71
Observations	2,110	2,090	2,110	2,090	2,325	2,302
Includes School and Recapture Variables (Pref. Spec.)	√	√				
Excludes Recapture Variables			√	√	√	√
Exclude all outliers (Pref. Spec.)	√	√	√	√		
Exclude only school share outliers					√	√

Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. This table 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. In odd columns, the dependent variable is an indicator that takes the value 100 if the subject protested directly in 2021. In even columns, the dependent variable is an indicator that takes the value 100 if the subject answered "very likely" to the question on the likelihood to protest in 2021. Estimates reported in columns (1) and (2) are based on the 2SLS econometric model given in model 4 and discussed in Section 4.4. These results are the same as the ones reported in Table 3. Columns (3) and (4) are based on regressions that exclude all variables related to the recapture share feedback, but in both cases, use the same definition of outliers used in columns (1) and (2). This specification compares individuals who received the school feedback treatment with individuals who did not receive it, regardless of their recapture treatment status. Columns (5) and (6) also exclude recapture variables. However, in this case, only individuals who are outliers for the school share belief gap are excluded, as opposed to our preferred specification which excludes individuals who are outliers either in school share beliefs or recapture share beliefs.Mean outcomes at baseline correspond with 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 A.7: 2SLS Estimates: More Detailed Definition of Children's School Attendance

	P_D^{2021} (1)	I^{2021} (2)	P_A^{2021} (3)	P_D^{2020} (4)
Effects of School Share:				
With Children in Public Schools $^{(i)}$	-0.399*	-0.401*	0.014	0.104
	(0.216)	(0.231)	(0.123)	(0.180)
With Children, not in Public Schools $^{(ii)}$	0.630	0.248	-0.082	-0.312
	(0.402)	(0.412)	(0.112)	(0.283)
Without Children $^{(iii)}$	0.200	0.278*	-0.013	-0.051
	(0.140)	(0.157)	(0.061)	(0.108)
Difference (i) - (ii)	1.029**	0.649	-0.096	-0.416
	(0.453)	(0.468)	(0.166)	(0.335)
Difference (i) - (iii)	0.599**	0.679**	-0.027	-0.155
	(0.258)	(0.280)	(0.138)	(0.210)
Difference (ii) - (iii)	0.431	-0.030	-0.069	-0.261
	(0.427)	(0.444)	(0.130)	(0.305)
Cragg-Donald F-Statistic	10.12	10.03	10.12	10.22
Mean Outcome (Baseline):				
With Children in Public Schools	33.86	47.20	7.09	25.98
With Children, not in Public Schools	29.41	45.59	4.41	16.18
Without children	28.70	44.72	4.01	23.46
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 (4) discussed in Section 4.4. corresponding to the school share treatment effect but using a more detailed definition of the household structure in terms of children's school attendance. More specifically, we present the coefficients for three groups of households: (i) with children attending public schools (25.5%), (ii) with children, not attending public schools, e.g., home schooling, charter schools, private schools, etc. (12.6%), and (iii) without school-age children (61.9%). 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 subject's protest likelihood in 2021 ("Do you intend to protest this year?"). The dependent variable in column (3) corresponds to an indicator variable that takes the value 100 if the subject used an agent to protest in 2021, whereas in column (4) corresponds to an indicator variable that takes the value 100 if the subject protested directly in 2020. Mean outcomes at baseline correspond are computed using the group of subjects who did not receive feedback about the school share nor recapture share (the control group).

Table A.8: 2SLS Estimates: Effects on Subsequent Protesting Behavior

							Total Protests		Any Protests	
	P_D^{2021}	$P_{D,won}^{2021}$	P_D^{2022}	$P_{D,won}^{2022}$	P_D^{2023}	$P_{D,won}^{2023}$	$\#P_D^{2021-2023}$	$\#P_{D,won}^{2021-2023}$	$AnyP_D^{2021-2023}$	$AnyP_{D,won}^{2021-2023}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Effects of School Share										
With Children	-0.409*	-0.450**	-0.280	-0.118	-0.167	-0.341***	-0.856**	-0.909***	-0.439*	-0.456**
	(0.219)	(0.190)	(0.208)	(0.188)	(0.172)	(0.132)	(0.430)	(0.351)	(0.242)	(0.223)
Without Children	0.278**	0.136	-0.009	0.074	0.025	0.013	0.294	0.222	0.144	0.125
	(0.129)	(0.111)	(0.118)	(0.107)	(0.095)	(0.076)	(0.234)	(0.203)	(0.140)	(0.132)
Difference (Children - No Children)	-0.687***	-0.586***	-0.271	-0.192	-0.192	-0.354**	-1.150**	-1.132***	-0.583**	-0.581**
	(0.255)	(0.220)	(0.240)	(0.217)	(0.198)	(0.153)	(0.493)	(0.407)	(0.280)	(0.259)
Cragg-Donald F-Statistic	30.10	30.10	30.10	30.10	30.10	30.10	30.10	30.10	30.10	30.10
Mean Outcome (Baseline):										
With Children	33.86	20.47	25.98	22.05	14.96	11.81	74.80	54.33	47.24	35.43
Without Children	28.83	19.39	22.70	19.39	14.80	7.91	66.33	46.68	45.41	34.18
Observations	2,110	2,110	2,110	2,110	2,110	2,110	2,110	2,110	2,110	2,110

Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. This table reports 2SLS estimates of equation (4) discussed in Section 4.4. corresponding to the school share treatment effect extending the post-protest period until 2023. The dependent variable in column (1) is an indicator variable that takes the value 100 if the subject protested directly in 2021, whereas in column (2) it takes the value of 100 if the subject protested directly, and the protest was successful. Columns (3) to (6) replicate the analysis for protests filed in 2022 and 2023, respectively. Columns (7) through (10) pool the post-treatment period (2021-2023). In columns (7) and (8) the outcome of interest is the number of protests and successful protests in 2021-2023 (multiplied by 100 to simplify comparison with columns (1) and (2)), respectively. In column (9), the outcome variable is 100 if the homeowner protested at least once in the 2021-2023 period, and 0 if they never protested. In column (10) we use an analogous definition but for successful direct protests. Mean outcomes at baseline correspond are computed using the group of subjects who did not receive feedback about the school share nor recapture share (the control group).

Table A.9: 2SLS Estimates: Effects on Subsequent Market Value and Tax Savings

	P_D^{2021} (1)	$P_{D,won}^{2021}$ (2)	ΔMV^{2021} (3)	ΔT^{2021} (4)	$\Delta MV^{2022,2021}$ (5)	$\Delta T^{2022,2021}$ (6)	$\Delta MV^{2023,2021}$ (7)	$\Delta T^{2023,2021}$ (8)
Effects of School Share								
With Children	-0.409*	-0.450**	-0.047**	-0.011	-0.108*	-0.057	-0.014	-0.045
	(0.219)	(0.190)	(0.021)	(0.012)	(0.065)	(0.056)	(0.090)	(0.084)
Without Children	0.278**	0.136	0.005	0.003	0.006	-0.001	-0.014	0.002
	(0.129)	(0.111)	(0.009)	(0.007)	(0.041)	(0.044)	(0.053)	(0.058)
Difference (Children - No Children)	-0.687***	-0.586***	-0.052**	-0.014	-0.114	-0.057	0.001	-0.046
	(0.255)	(0.220)	(0.022)	(0.014)	(0.076)	(0.072)	(0.104)	(0.102)
Cragg-Donald F-Statistic	30.10	30.10	30.10	30.10	30.10	30.10	30.10	30.10
Mean Outcome (Baseline):								
With Children	33.86	20.47	1.13	0.84	-19.96	6.64	-30.86	16.88
Without Children	28.83	19.39	1.66	1.04	-19.64	11.13	-30.63	23.32
Observations	2,110	2,110	2,110	2,110	2,110	2,110	2,110	2,110

Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. This table reports 2SLS estimates of equation (4) discussed in Section 4.4. corresponding to the school share treatment effect extending the post-protest period until 2023 and focusing on market value and tax savings. The dependent variable in column (1) is an indicator variable that takes the value 100 if the subject protested directly in 2021, whereas in column (2) it takes the value of 100 if the subject protested directly, and the protest was successful. Columns (3) reports the effects on percent market value *savings* associated with the protest, comparing 2021 post- and pre-protest assessed values. Negative estimates indicate that post-protest market values are higher. Column (4) uses percent savings in post-protest estimated tax liabilities as the dependent variable. Again, negative values indicate that post-protest taxes are higher. Columns (5) and (6), and (7) and (8) replicate the analysis but this time comparing 2022 with 2021, and 2023 with 2021, respectively. Mean outcomes at baseline correspond are computed using the group of subjects who did not receive feedback about the school share nor recapture share (the control group).

Table A.10: Effects on Survey Outcomes

	Unsatisf. with Govt. Services (1:10) (1)	Unfair Amount of Taxes (1:10) (2)	Against Recapture (1:10) (3)	Taxes Are Too High (-100,0,100) (4)	Less Tax Worse Services (-100,0,100) (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)
Cragg-Donald F-Statistic	30.34	30.39	30.37	30.39	30.42
Mean Outcome (Baseline):					
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 4 and discussed in Section 4.4 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. Column (1) reports the treatment effects on a variable that represent homeowners dissatisfaction 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 with 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 I.

B Recapture Share Treatment Arm

The principle of reciprocal motivation could have implications for tax redistribution. When taxpayers learn that their tax dollars are being spent in communities other than their own, they may be less willing to pay taxes because they do not receive benefits from the taxes they pay. We initially designed a second treatment arm (i.e., recapture treatment) similar to our school share treatment arm to explore this additional hypothesis. While ex-ante, we expected that both treatment arms would be adequately powered to detect effects, ex-post, we found that we were underpowered for the recapture treatment arm. Hence, estimates of the effects of the second treatment arm, which provides information about the share of funds being recaptured, are unfortunately imprecise and thus largely inconclusive. For transparency, we still report the analysis for the second treatment arm in this appendix.

As a preview of the main findings, we find that consistent with the hypothesis of reciprocal motivation, the belief about recapture share does not have significant effects on the decision to file a tax appeal among households *without* children – although this finding must be taken with a grain of salt due to the lack of sufficient statistical power. We do not find evidence of significant effects for households *with* children – however, the coefficient is so imprecisely estimated that we cannot rule out large positive effects.

B.1 Recapture Share Survey Module

To design the recapture treatment arm, we exploited variation in the degree of redistribution of property taxes across school districts that occurs in some states, and in particular, the fact that some (poorer) school districts receive additional funds due to the recapture system while other (richer) districts send part of their tax revenues away. In Texas, this redistribution is dictated by legislation often referred to by the media as "Recapture Plan" or "Robin Hood Plan."[82] Hence, we measure households' perceptions about the share of their school funding that is redistributed away from or toward their school district and provide factual information about this process. For the sake of brevity, from now on, we refer to this as the "recapture share." For example, a recapture share of 10% would mean that 10% of the district's school tax revenue is not spent in that district but instead transferred to disadvantaged school districts.

As with the school share, we can measure the causal effects of the perceived recapture share using the information-provision experiment. Take the example of a district that is a net contributor to the recapture plan. According to the reciprocal motivation mechanism, the belief about the recapture share should not affect the decision to file a tax appeal for

[82] For the full history of property tax recapture in Texas, see for example Villanueva (2018).

households *without* children because the diverted funds are being used for a service that does not benefit them directly. By contrast, households *with* children should be more likely to protest upon learning that some of their tax payments are being diverted to other districts because they were benefiting directly from the diverted funds.

Some subjects may not know about or understand recapture. Thus, we start this survey module with a summary of the recapture system.

> "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."

The rest of the module follows the same structure as Section 3.3 of the main paper for Step 1 through Step 3. We elicit beliefs about the recapture share in two stages. First, we ask respondents to guess if their school district will receive more, the same, or less taxes than what households in their district paid in school taxes. The following stage is quantitative in nature. If the respondent selects "More" (or "Less") in the first question, we ask them to guess how much *more* (or *less*) funding their school district will receive as a share of the 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).

When providing 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:

> "<u>Notes:</u> 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.2 Accuracy of Prior Beliefs and Belief Updating

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 can be informed about the recapture system through its media coverage. Also, it is probably widely known that the recapture system redistributes from more to less advantaged districts. As a result, if a homeowner knows whether he or she lives in a more or less advantaged district, that information alone may be enough to form a decent guess about the recapture share.

Figure B.1(a) shows a histogram of the degree of misperceptions about the recapture share before the information provision. The x-axis corresponds to the difference between the actual recapture share and respondents' perceptions. A minority of subjects have accurate perceptions: around 20% of subjects guess the recapture share to be within ± 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 substantially less pronounced than that of the school share (16.57). The fact that misperceptions for the recapture share are smaller than those for the school share implies 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: on average, subjects overestimate the recapture share by just 0.28 pp. This can be seen directly from Figure B.1(a), which shows that households are roughly equally likely to be in the left half of the histogram (corresponding to overestimation) as in the right half (corresponding to an underestimation).

Figure B.2.(a) shows the distribution of misperceptions, including the outliers for the recapture share and Figure B.2.(b) presents the results on misperceptions about the recapture share broken down for household *with* and *without* children. While there is a bit more dispersion among the households *with* children than for the households *without* children, the two distributions in Figure B.2.(b) are quite similar. In both cases, few homeowners provide an answer that is within 5 pp 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 similar distributions of the misperceptions for both types of households.

Figure B.1(b) illustrates how subjects update their beliefs in reaction to the information provision about the recapture share using a binned scatterplot. The x-axis corresponds to the gaps in prior beliefs, and the y-axis denotes the belief updating. That is, for each bin, the magnitudes on the x-axis represent the theoretical revisions we would expect if the respondents were to fully respond to the information provided, while the magnitudes on the y-axis show the revisions observed in practice. The red circles on Figure B.1(b) correspond to the subjects who received feedback on 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 the perception gap is associated with a revision that is 0.632 pp higher. The gray squares correspond to subjects who did not receive information about the recapture share. 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. 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. Although large, this rate of information pass-through (0.533) is quite lower than the corresponding rate for the school share (0.757). Figures B.3(a) and B.3(b) show belief updating for households *with* and *without* children in public schools separately. The recapture share belief updating is similar for households *with* children as for households *without* children.

Many reasons help explain the weaker updated beliefs about recapture. For example, respondents may feel more confident in their prior beliefs about recapture, or have lower trust in the recapture feedback. Indeed, the recapture estimates we use for the feedback are based on several assumptions, so subjects may naturally find the recapture feedback less persuasive. Last, subjects may pay less attention to recapture feedback due to survey fatigue, as this information appears later in the survey. The most important implication of the weaker belief updating for recapture share (relative to school share) is that it will result in less variation in posterior beliefs and, thus, less precisely estimated 2SLS coefficients.

As we did for the school share treatment, we also analyze the possibility of cross-learning, but in this case, from the school share treatment to recapture beliefs. More specifically, homeowners may update their recapture share beliefs in response to the school share feedback. For reference, Figure B.4(a) reproduces Figure A.5, but for this specific type of cross-learning. Figure B.4(b) shows no differences in the relation between the recapture share gap and the recapture share belief update between homeowners who received the school share treatment and homeowners who did not. This result shows the lack of cross-learning effects from the school share feedback to the recapture share belief update.

As a complement to Figure B.1, Figure B.5 illustrates if, from the recapture treatment arm, individuals learn about the direction of the recapture share. For this analysis, we use the following question that we asked in the survey twice, both before and after the information provision, "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." In the first two columns of the figure, we report the share of correct answers to this question before the information provision. In the latter two columns, we replicate the analysis for the posterior beliefs, i.e., after the information provision. The figure illustrates that there are no differences in the prior misperceptions about the direction of the recapture share by treatment arm: 45.25% of individuals that were not selected to receive the recapture feedback provide an accurate guess on the direction of the recapture share, while this percent is 46.71% for individuals in the recapture feedback group. The p-value of the difference of means between these two groups is 0.759. This means that only half of the homeowners have accurate beliefs about whether they live in an ISD that is a net giver or net receiver to the recapture system. After the information provision, we see that 74.40% of individuals who received the information on the actual recapture value provided a correct answer. This strongly differs from the share of correct answers for ndividuals who did not receive the recapture feedback: 45.92%. The differences between these two groups are statistically significant (p-value <0.001), as well as the double difference (Posterior versus Prior, by treatment arm). In general, these results provide evidence consistent with Figure B.1 and show that individuals who received the recapture feedback are indeed learning, at least, about the direction of the recapture share.

B.3 Recapture Effect Estimates

Analogous to Figure 1 in the main paper, Figure B.6 illustrates how the feedback affected, on average, the perceptions about the recapture share (panel (a)) and the probability of protesting directly (panel (b)) for households *with* children and *without* children. Panel (a) shows that the provision of recapture feedback, on average, did not induce major changes in beliefs about the recapture share neither for households *with* nor *without* children. While the average effect for households *with* children is -3.50 p.p. and statistically significant (p-value=0.009 and 0.018 for estimates without and with controls, respectively), this effect is economically insignificant. For household *without* children, the differences are even smaller, and not statistically significant (p-value=0.129 and 0.354, respectively). Furthermore, the differences in the average effects between households *with* and *without* children are small (p-value=0.137, and 0.114, respectively). Estimates of average effects must be interpreted cautiously since they do not use the information on prior and posterior beliefs and averages across individuals with very different priors. This is particularly important in a context where individuals do not have a systematic bias in their prior beliefs, as discussed in Section B.2. Given the symmetry in the prior beliefs gap about the recapture share (a mean error of -0.28p.p in Figure B.1), there is no reason to expect positive or negative effects of the recapture share treatment on average beliefs. Panel (b) shows that for households *with* children, the information on recapture share decreases the probability of filing a tax protest. However, panel (b) shows that this decrease vanishes when computing the difference conditional on the controls used in the regressions, which suggests that the unconditional effect might be spurious.

Next, we exploit all the information collected in the survey regarding prior and posteriors beliefs on recapture share, as we did for the school share effects in the main paper. This approach allows us to account for the fact that homeowners who face the same information shock might update their beliefs differently, depending on their priors and learning ability.

Let r_i^{post} be the posterior belief about the funds recaptured from individual i's school district, in percentage points. Positive values indicate that individual i's district is a net contributor to the recapture system; for example, $r_i^{post} = 10$ means that 10% 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; for example, $r_i^{post} = -10$ means that the school district can spend the school taxes it raises plus an additional 10% from the amount recaptured. We use the following econometric specification:

$$P_i^{2021} = {}_0 + {}^{R}_{C} \cdot C_i \cdot r_i^{post} + {}^{R}_{NC} \cdot (1 - C_i) \cdot r_i^{post} + {}_1 \cdot C_i + \epsilon_i \qquad \text{(B.1)}$$

The two parameters of interest are $^{R}_{C}$ and $^{R}_{NC}$ for households *with* and *without* children, respectively. As shown by Prediction 2 in the conceptual frameworks developed in Appendix C we expect $^{R}_{C} > 0$ and $^{S}_{NC} = 0$ (and, as a result, $^{R}_{C} - {}^{R}_{NC} > 0$). As mentioned in Section 4.4, we estimate the effects of school share and recapture share jointly in the same 2SLS regression. Thus, we identify the effects of the recapture share using 2SLS to exploit the variation in posterior beliefs induced exogenously by the information provision experiment.

The 2SLS estimates for the recapture share are presented in panel (a) of Table B.1. In column (1) of Table B.1, the dependent variable indicates if the subject protests directly in 2021. The causal effects of the beliefs about the recapture share are very imprecisely estimated, so the results for this treatment arm are largely inconclusive. Consistent with the hypothesis of reciprocal motivation, the belief about recapture share does not have significant effects on the decision to file a tax appeal among households *without* children: the coefficient is positive (0.491) and borderline statistically insignificant (p-value=0.106). This finding must be taken with a grain of salt since the coefficient is imprecisely estimated. Thus, we cannot rule out large effects, positive or negative. For households *with* children, we do not find evidence of significant effects. The coefficient for households *with* children is negative (-0.020) but statistically insignificant (p-value=0.967). Again, this coefficient is so imprecisely estimated that it does not really constitute evidence against the hypothesis of reciprocal motivation because we cannot rule out very large positive effects. More precisely, the 95% confidence interval ranges between -0.965 and 0.925, which is substantially large compared to the effects documented for the school share treatment arm. Likewise, the difference between the coefficients for households *with* versus *without* children is statistically insignificant (p-value=0.362), but it is very imprecisely estimated so we cannot rule out large differences. The coefficients from column (2) of Table B.1 show that the results for recapture share are similar if we look at the intention to protest from the answers in the survey instead of the actual protest decision. As in column (1), the estimates from column (2) are all statistically insignificant.

To illustrate how imprecisely estimated the recapture effects are, note, for instance, that the standard error for recapture share in the case of households *without* children is 135% larger than the corresponding standard error for school share (0.304 in Table B.1 vs. 0.129 in Table 2 in the main text). In other words, the effects of the recapture share should be more than twice as high as those of the school share 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 B.2, misperceptions about recapture share are smaller (mean absolute difference of 11.36 pp) than those about school share (mean absolute difference of 16.57 pp).

Second, as also documented in Section B.2, conditional on a level of misperceptions, subjects updated their beliefs more strongly in response to the feedback about school share than to the feedback about recapture share.

In panel (b) of Table B.1 and Figure B.7(b), we report the reduced form estimates, i.e., a regression of the outcome of interest on the instrument (Recapture Share Belief Gap $\times$ Recapture Feedback Shown). While this specification takes into account heterogeneity in prior beliefs, it does not account for imperfect learning. As expected, the reduced form effects illustrate a similar conclusion as the 2SLS estimates; the results suggest that the recapture treatment did not affect protests nor intention to protest for either households *with* or *without* children. The estimates also suggest that differences in the effects between the two groups are negligible. Differences in the magnitude between the reduced form estimates and the 2SLS estimates are explained by imperfect learning. For each percentage point in the prior belief gap, the information treatment on recapture share only corrects about 0.53p.p. This is illustrated in panel (c) of Table B.1 where we show in regression form the results on belief updating in the first stage, similar to the results presented in figure B.1 in Section B.2.

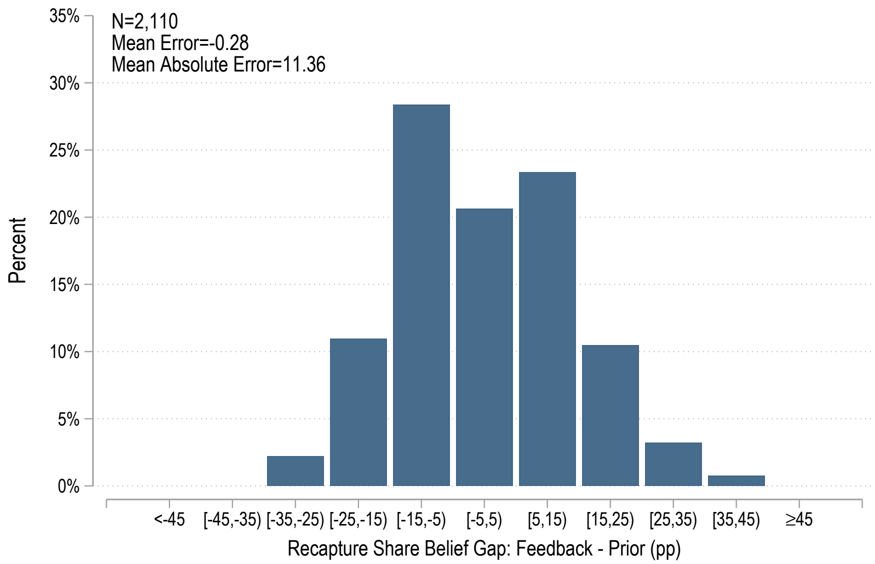
Table B.2 shows the same outcome variables presented for the school share treatment in Table 3 in the main text but for the recapture share treatment. The results show that the null results for the recapture share treatment mostly hold across these various outcome variables. Columns (1) and (2) of Table B.2 reproduce the results in panel (a) of Table B.1 for reference. In column (3), we show that the effects on protests through agents are also null. More precisely, column (3) of Table B.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 0.445 and 0.323 for households *with* and *without* children, respectively), and the difference between the two coefficients (-0.099) is also statistically insignificant (p-value=0.749). The coefficients from column (4) show the event-study falsification exercise: i.e., the dependent variable is whether the household protested in 2020. As expected, the estimates are close to zero and statistically insignificant. The coefficients from column (4) (0.164 and -0.039 for households *with* and *without* children, respectively) are statistically insignificant (p-values of 0.164 and 0.867); the difference between the two (0.203) is also statistically insignificant (p-value=0.664). Furthermore, Figure B.7(a) presents similar results for the whole pre-treatment period (2016-2020), and the same conclusion holds. The lack of effects of the recapture treatment is also evident when we extend the post-treatment period to 2023. In particular, columns (8) and (9) show that when considering either the probability of protesting at least once in 2021-2023 or the cumulative number of protests in this period, the recapture treatment effect is also mostly null.

Regarding the effects of the recapture share treatment on successful protests and subsequent changes in market values, some of the results reported in Columns (5) through (7) of Table B.2 seem somewhat surprising: there are some statistically significant effects for households *without* children and some borderline significant effects for the difference between households *with* and *without* children. Given that, as reported in columns (1) and (2), there are no significant effects on the protest decisions, these results should be taken with a grain of salt, as they are likely spurious. To investigate this further, we again exploit the pre-treatment information included in the data. Panel (a) in Figure B.8 shows that the coefficients on effects on successful protests oscillate around zero during the pre-treatment period, and in some cases, they are even large and statistically significant (e.g., in 2018). This oscillating pattern is consistent with our cautious interpretation and suggests that statistically significant estimates correspond most likely to spurious effects.

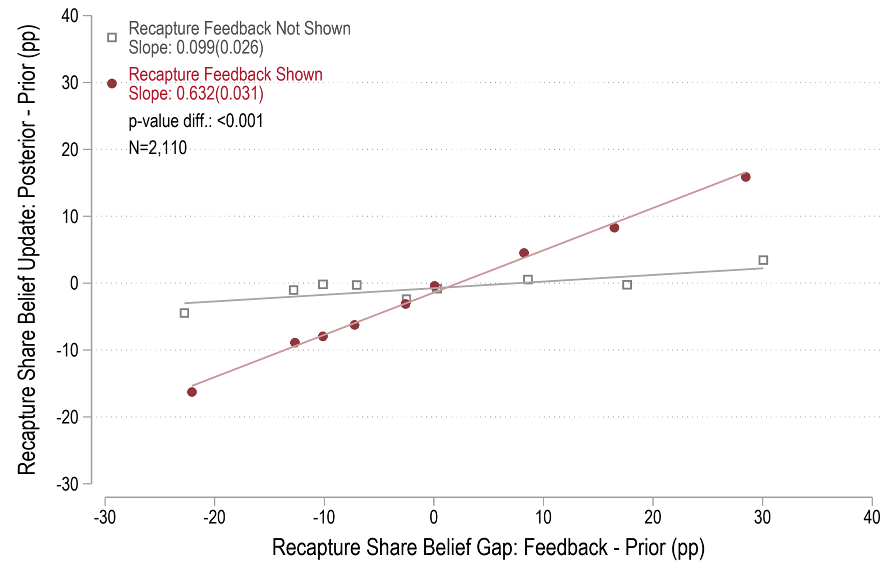
In the same spirit as Table 3 for the school share analysis, Table B.3 presents a series of robustness tests. Overall, these tests suggest that the (lack of) effects for the recapture share is not explained by the inclusion/exclusion of additional baseline control variables, the definition of outliers, or inattention. Furthermore, the results hold when estimates are re-weighted to account for selection into the experimental sample or to survey response.

Figure B.1: Perceptions about the Share of School Taxes Affected by Recapture

(a) Gap in Prior Beliefs



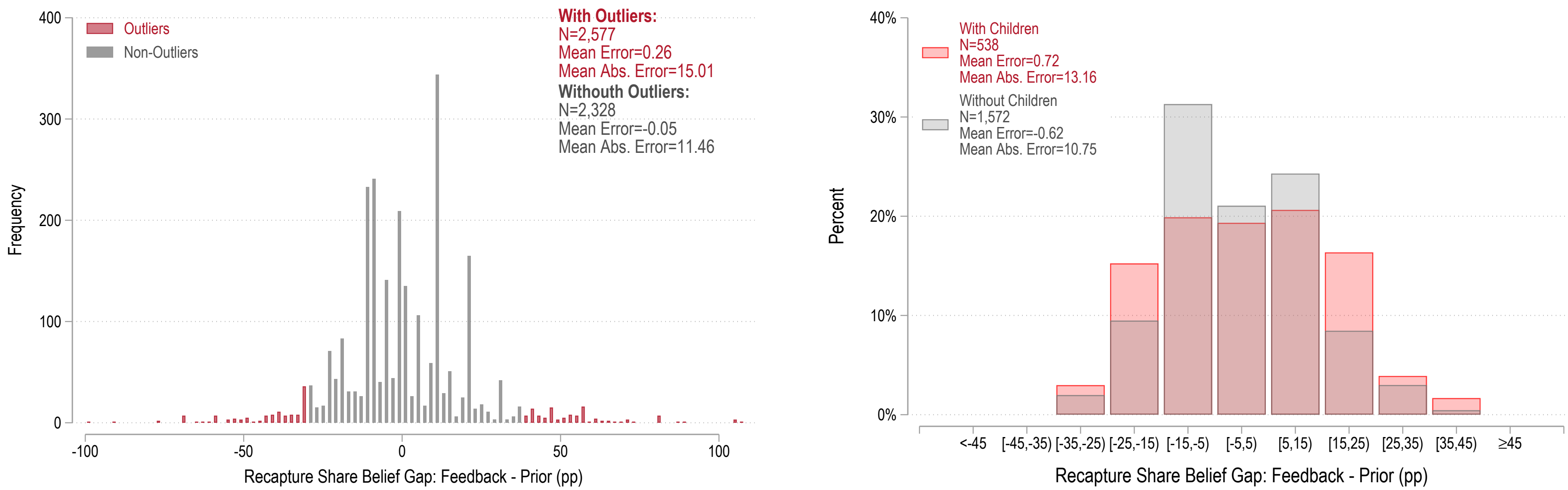
(b) Belief Updating



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 10 pp 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 mean error, and the mean absolute error. Panel (b) shows how respondents update their beliefs using a binned scatterplot (using ten bins corresponding to each decile of the Recapture Share Belief Gap). 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 with 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 B.2: Distribution of the Recapture Share Gaps

(a) Recapture Share Gap (Full) **(b)** Recapture Share Gap by *With*/*Without* Children

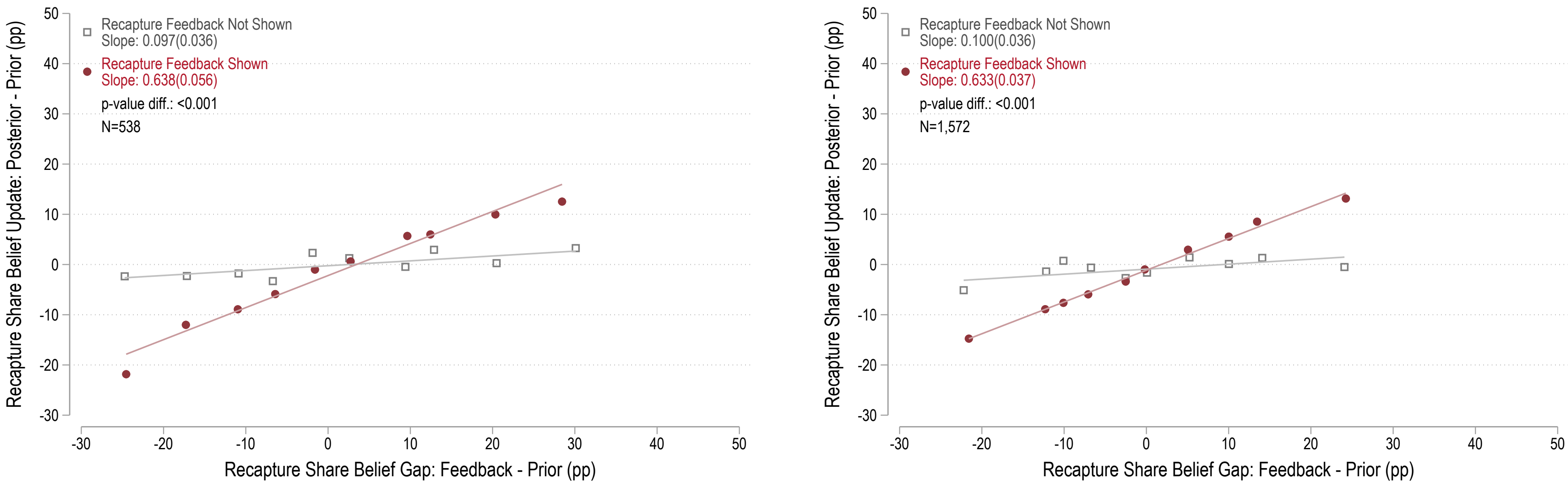


Notes: This figure illustrates the distribution of the recapture share beliefs gaps, defined as the difference between the actual recapture share (shown as feedback if selected into the recapture share treatment group) and the prior belief reported by respondents of the survey. In panel (a), gray bars represent observations that are between the 5% and 95% of the recapture share distribution. Red bars represent observations in the top and bottom 5%. 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 (see Figure A.3.), 215 for extreme misperceptions in recapture share belief gap, and 34 observations for extreme misperceptions in both beliefs. More specifically, 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 panel (a) 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 (i.e., bars of 2 pp width). Panel (b) breaks down the analysis by households *with* children in public schools (red bars) and households *without* children in public schools (gray bars), based on the online survey data for the main experimental sample (N=2,110). 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.3: Belief Updating: Households *With* Children versus *Without* Children

(a) Recapture Share: *With* Children

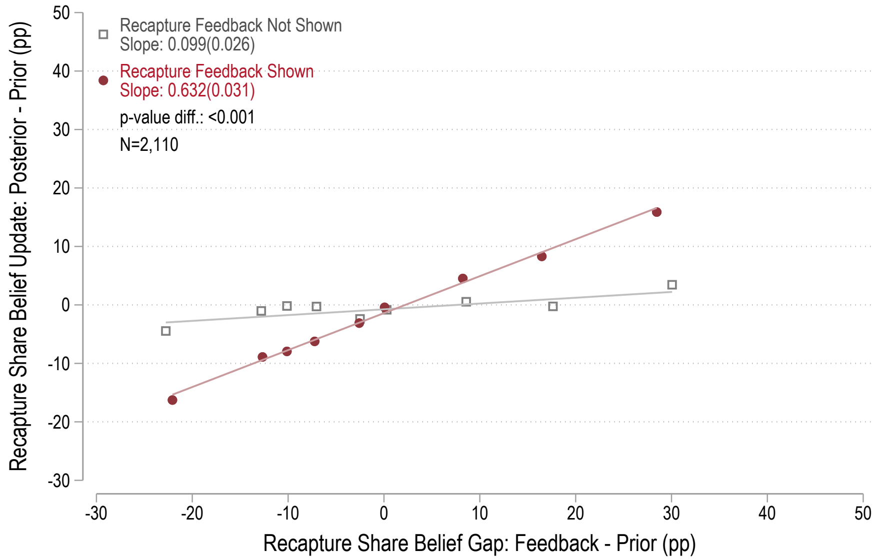
(b) Recapture Share: *Without* Children



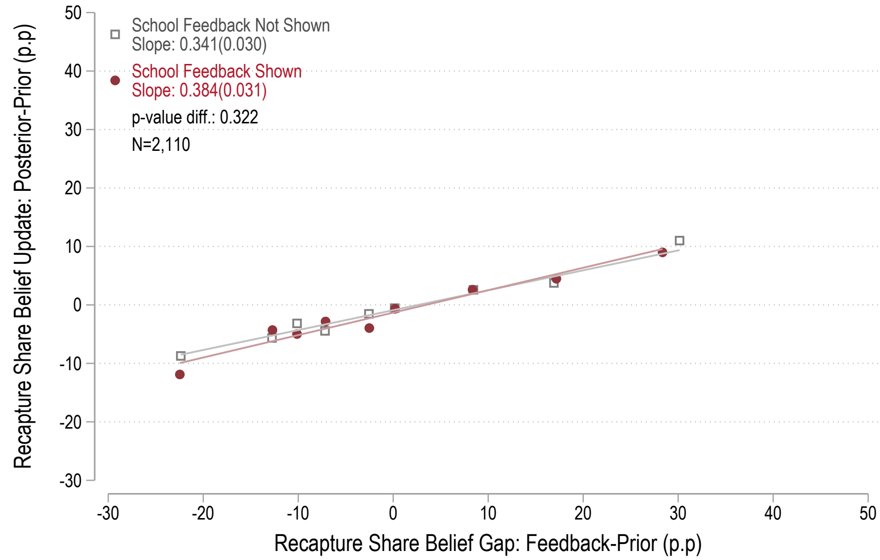
Notes: This figure illustrates the relation between the recapture shares belief gap and the size of the update (i.e., posterior belief minus prior belief) by treatment status. Both figures are analogous to Figure B.1(b), but separating the households *with* and *without* children in public schools. Gray squares represent the average update within each bin for the group of homeowners that were not selected into the 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 recapture share belief gap. The coefficient associated with 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.4: Cross-Learning in the Information-Provision Experiment

(a) Recapture Feedback on Recapture Share

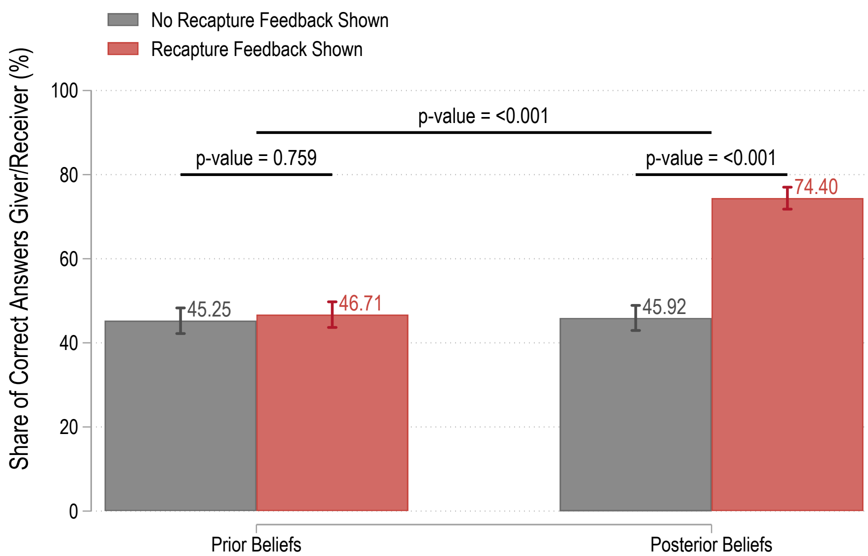


(b) School Feedback on Recapture Share



Notes: This figure illustrates the relation between the recapture shares belief gap and the size of the update (i.e., posterior belief minus prior belief) by treatment status. Panel (a) shows how the recapture share feedback affects belief updates about the recapture share, which is the same as Figure B.1(b). Panel (b) depicts how people learn about the recapture share from the school share feedback (i.e., cross-learning between experimental arms). Gray squares represent the average update within each bin for the group of homeowners that were not selected into the 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 recapture share belief gap. The coefficient associated with 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.

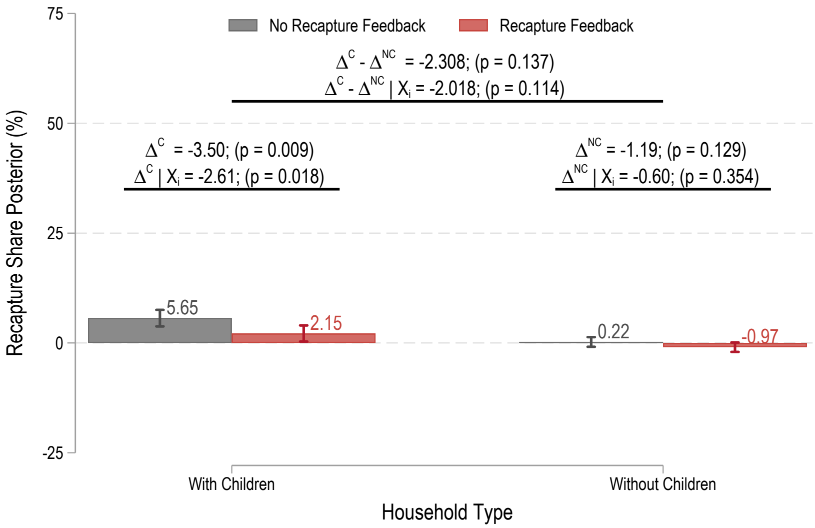
Figure B.5: Perceptions about the Direction of the Recapture Share (Net Givers versus Net Receivers)



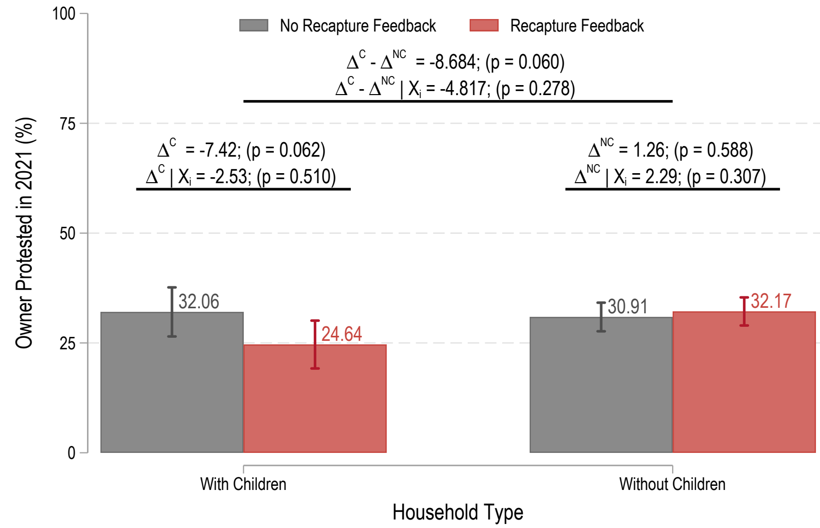
Notes: This figure illustrates how people update their beliefs about the direction of the recapture share, depending on their treatment status regarding recapture feedback. In the first two columns, we report the share of correct answers to the question "Do you think that your school district will receive more, the same, or less in funding than what households in your school district paid in school taxes in 2020," before the information experiment. School districts where the absolute value of the recapture share is less than 6p.p. are defined as receiving the same in school funding than what they paid in school taxes. In the latter two columns, we replicate the analysis for the posterior beliefs using the answers to the same question after the information provision. In addition, we report the p-value of the difference in the average share of correct answers between groups for the prior beliefs, posterior beliefs, and the double difference.

Figure B.6: Average Treatment Effects of Recapture Feedback

(a) Recapture Share Posterior



(b) 2021 Owner Protest

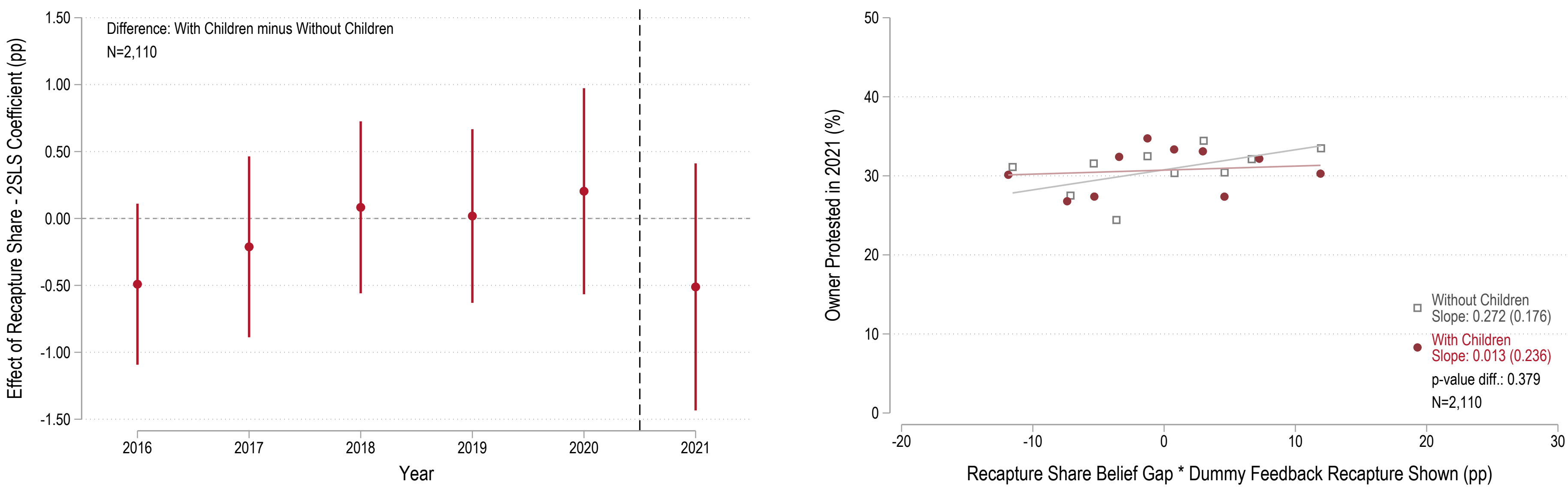


Notes: This figure illustrates the average treatment effects of the recapture feedback treatment. Panel (a) reports the mean of the recapture share posterior by treatment group, separately for households *with* and *without* children. Gray bars represent the average recapture share posterior for the group that did not receive the school feedback treatment, while red bars represent the average recapture share posterior for the group that did receive it. In addition, we report: 1) the difference in raw means between the treatment and the control group (Δ^{C} and Δ^{NC}), 2) the difference in means conditional on the covariates included in the baseline specification ($\Delta^{C}|X$ and $\Delta^{NC}|X$), 3) the corresponding p-value of the equality of means test for treated *versus* control groups within each household type, and 4) the p-value corresponding to the comparison of these differences between household types (i.e., a double-difference test). Panel (b) replicates the analysis but uses the probability of protesting as the dependent variable.

Figure B.7: The Effects of Recapture Share Perceptions on Protests: Additional Robustness Checks

(a) Event-Study Analysis

(b) Binned Scatterplot (Reduced Form)

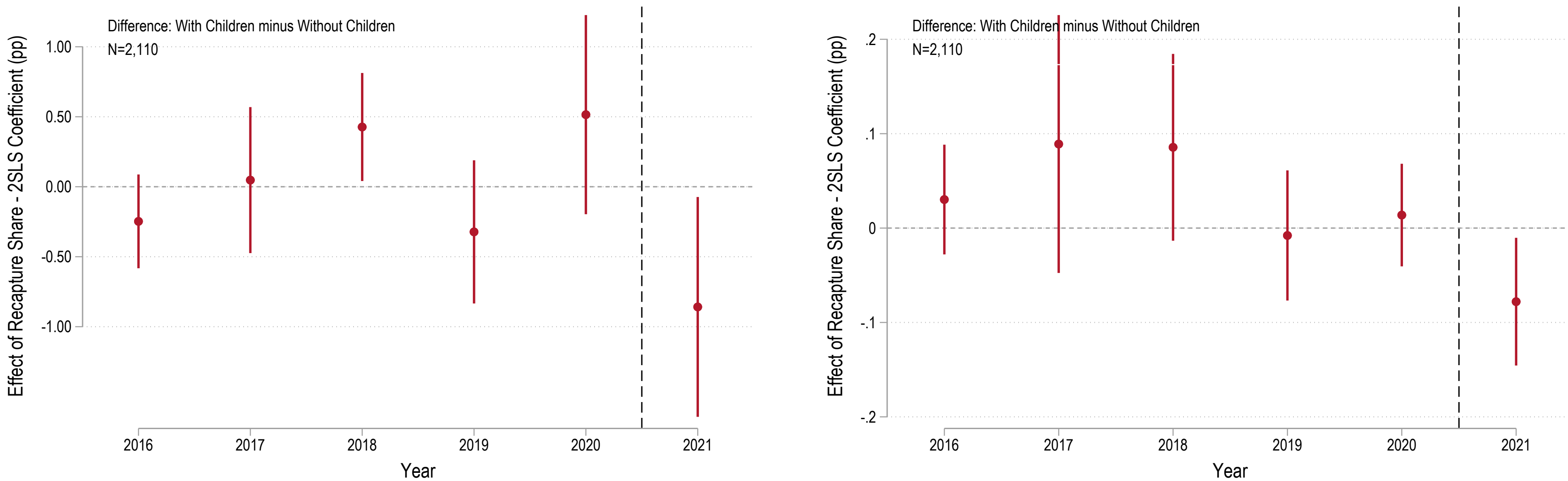


Notes: Panel (a) reports an event-study analysis of the differential effect of the recapture share belief on the protest probability for households *with* children versus *without* children. The estimates plotted in this figure correspond to the 2SLS point estimate based on equation (4), with 90% confidence intervals based on robust standard errors. The coefficient plotted for 2021 is the coefficient reported in the "difference" row of column (1) of Table B.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). The number of observations used in the estimation is reported in the upper left corner. Panel (b) depicts a 10-decile 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 (we use the same regression in Table B.1). In addition we report the p-value of the difference in the effect between the two groups and the number of observations used in the estimation.

Figure B.8: Treatment Effect of Recapture Share Perceptions on Successful Protests

(a) Effect on Succesful Protests

(b) Effects on Market Value Savings



Notes: This figure depicts an event-study analysis of the differential effect of the recapture share beliefs for households *with* children versus *without* children. In panel (a), the dependent variable takes the value of 100 if the subject protested directly in 2021 and the protest was successful in reducing the assessed market value, and 0 otherwise (i.e., either if the owner did not protest, or if the owner protest was unsuccessful). In panel (b), the dependent variable represents the percent market value savings as a result of the protest. This is defined as the difference between the post- and pre-protest assessed market value expressed as a percent of the pre-protest value. The estimates plotted in this figure correspond to the 2SLS point estimate based on equation (4), with 90% confidence intervals based on robust standard errors. In panel (a), the coefficient plotted for 2021 is the coefficient reported in column (5) corresponding to the "difference" rows in Table B.2. In panel b., it corresponds to estimates reported in column (6) of the same table. 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). The number of observations used in the estimation is reported in the upper left corner.

Table B.1: Main Results: Reduced-Form, and First-Stage

	(1)	(2)
	P_D^{2021}	I^{2021}
a. Effect of Recapture Share (2SLS)		
With Children	-0.020	-0.313
	(0.482)	(0.541)
Without Children	0.491	-0.101
	(0.304)	(0.325)
(Difference Children - No Children)	-0.511	-0.212
	(0.561)	(0.620)
	P_D^{2021}	I^{2021}
b. Effect of Recapture Share Belief Gap * Dummy Feedback Recapture Shown (Reduced Form)		
With Children	0.013	-0.157
	(0.236)	(0.267)
Without Children	0.272	-0.060
	(0.176)	(0.190)
(Difference Children - No Children)	-0.259	-0.097
	(0.294)	(0.329)
	s^{post}	s^{post}
c. Effect of Recapture Share Belief Gap * Dummy Feedback Recapture Shown (First Stage)		
With Children	0.529***	0.533***
	(0.067)	(0.067)
Without Children	0.550***	0.545***
	(0.051)	(0.050)
(Difference Children - No Children)	-0.021	-0.012
	(0.084)	(0.084)
Mean Outcome (Baseline)		
With Children	33.86	47.20
Without Children	28.83	44.87
Observation	2,110	2,090

Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. This table shows the 2SLS. reduced form, and first-stage estimates corresponding to the recapture 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. In panel (a), 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. 2SLS estimates reported are based on the econometric model given by model 4 and discussed in Section 4.4. These results are the same as the ones reported in Table 3. Panel (b) reports the reduced form effects, i.e., it represents the effect of the information included in the feedback message with respect to the prior beliefs without considering how much do subjects actually learn from the experiment. The dependent variables are in this case, the same as in panel (a). Finally, the first stage estimates are reported in panel c. Estimates in this panel use the recapture share posterior as the dependent variable and the coefficients reported can be interpreted as the update in recapture share posteriors per percentage point of prior recapture share misperception. Estimates reported in panels (b) and (c) are based on OLS regressions and use the same set of control variables as the main specification discussed in Section 4.4. Mean outcomes at baseline correspond with subjects who did not receive feedback about the school share nor recapture share.

Table B.2: 2SLS Estimates: Additional Results for Recapture Share

	P_D^{2021} (1)	I^{2021} (2)	P_A^{2021} (3)	P_D^{2020} (4)	$P_{D,won}^{2021}$ (5)	ΔMV^{2021} (6)	ΔT^{2021} (7)	$\#P_D^{2021-2023}$ (8)	$\mathrm{Any}_D^{2021,2023}$ (9)
Effects of Recapture Share:									
With Children	-0.020	-0.313	-0.223	0.164	-0.099	-0.035	-0.025	-1.076	-0.128
	(0.482)	(0.541)	(0.291)	(0.417)	(0.399)	(0.038)	(0.023)	(0.972)	(0.542)
Without Children	0.491	-0.101	-0.124	-0.039	0.759***	0.043**	0.024	0.892	0.498
	(0.304)	(0.325)	(0.125)	(0.234)	(0.270)	(0.019)	(0.015)	(0.548)	(0.322)
Difference (Children - No Children)	-0.511	-0.212	-0.099	0.203	-0.859*	-0.078*	-0.048*	-1.968*	-0.626
	(0.561)	(0.620)	(0.308)	(0.468)	(0.477)	(0.041)	(0.027)	(1.094)	(0.619)
Cragg-Donald F-Statistic	30.10	30.22	30.10	30.02	30.10	30.10	30.10	30.10	30.10
Mean Outcome (Baseline):									
With Children	33.86	47.20	7.09	25.98	20.47	1.13	0.84	74.80	47.24
Without Children	28.83	44.87	4.08	22.19	19.39	1.66	1.04	66.33	45.41
Observations	2,110	2,090	2,110	2,110	2,110	2,110	2,110	2,110	2,110

Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. This table reports 2SLS estimates of equation (4) discussed in Section 4.4 corresponding to the recapture share treatment effect. We present the coefficients for households *with* children and households *without* children, as well as the difference between these two types of households. 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 subject's protest likelihood in 2021 ("Do you intend to protest this year?"). The dependent variable in column (3) corresponds to an indicator variable that takes the value 100 if the subject used an agent to protest in 2021, whereas in column (4) corresponds to an indicator variable that takes the value 100 if the subject protested directly in 2020. Columns (5) through (8) report additional estimates for outcomes measured after the protesting period. In Column (5), the dependent variable is an indicator that takes the value of 100 if the protest by the owner was successful, and 0 otherwise. Column (6) reports the effects on the market value *savings* associated with the protest. Negative estimates indicate that post-protest market values are higher. Column (7) uses savings in post-protest estimated tax liabilities as the dependent variable. Again, negative values indicate that post-protest taxes are higher. The dependent variables in columns (8) and (9) consider the protesting behavior in the 2021-2023 period. In column (8), the dependent variable is the total number of protests in 2021-2023 (multiplied by 100 to make it comparable to estimates in columns 1 and 2). In column (9), the dependent variable is an indicator that takes the value 100 if the subject protested directly in 2021, 2022, or 2023. Mean outcomes at baseline correspond are computed using the group of subjects who did not receive feedback about the school share nor recapture share (the control group).

Table B.3: 2SLS Estimates: Robustness Checks

	Dependent Variable: P_D^{2021}							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
a. Effects of Recapture Share on P_D^{2021}								
With Children	-0.020	0.043	-0.120	0.091	0.018	0.134	-0.333	-0.351
	(0.482)	(0.477)	(0.538)	(0.416)	(0.331)	(0.439)	(0.604)	(0.698)
Without Children	0.491	0.430	0.275	0.411	0.243	0.465	0.262	0.672**
	(0.304)	(0.307)	(0.287)	(0.274)	(0.244)	(0.318)	(0.321)	(0.338)
Difference (Children - No Children)	-0.511	-0.387	-0.395	-0.319	-0.225	-0.331	-0.595	-1.023
	(0.561)	(0.558)	(0.610)	(0.500)	(0.394)	(0.525)	(0.671)	(0.765)
Observations	2,110	2,070	2,110	2,335	2,482	1,807	2,110	2,091
	Dependent Variable: I_D^{2021}							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
b. Effects of Recapture Share on I_D^{2021}								
With Children	-0.313	-0.222	-0.382	0.135	0.013	-0.059	0.056	0.560
	(0.541)	(0.536)	(0.593)	(0.451)	(0.373)	(0.492)	(0.621)	(0.720)
Without Children	-0.101	-0.125	-0.249	-0.129	-0.051	-0.051	0.043	0.119
	(0.325)	(0.325)	(0.303)	(0.291)	(0.265)	(0.338)	(0.356)	(0.385)
Difference (Children - No Children)	-0.212	-0.098	-0.133	0.264	0.063	-0.009	0.013	0.441
	(0.620)	(0.616)	(0.666)	(0.536)	(0.438)	(0.579)	(0.703)	(0.805)
Observations	2,090	2,070	2,090	2,309	2,454	1,807	2,090	2,071
Baseline Controls	√	√		√	√	√	√	√
Additional Controls		√						
5% Outliers	√	√	√			√	√	√
2.5% Outliers				√				
1% Outliers					√			
Attention Check						√		
Re-weighted (Exp.)							√	
Re-weighted (Univ.)								√

Notes: Significant at *10%, **5%, ***1%. Robust standard errors in parentheses. This table reports 2SLS estimates of equation (4) discussed in Section 4.6 corresponding to the recapture share treatment effect. Columns (1) and (2) correspond to our preferred specification reported in columns (1) and (2) in Table 3 (for reference). The rest of the columns in this table use the same dependent variables from 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) report estimates where no control variables are included at all. Columns (7) and (8) drop 2.5% of the outliers at each tail of the distribution (instead of the 5% used in the baseline specification). Columns (9) and (10) drop 1% of the outliers at each tail. Columns (11) and (12) restrict the samples to subjects who passed the attention check included in the questionnaire (see Appendix I for the survey). Columns (13) and (14) report re-weighted estimates where inverse probability weights are used to match the letter sample. Columns (15) and (16) do the same but for the universe of non-commercial properties. Estimates based on inverse probability weighted 2SLS regressions, with weights obtained from a Logit model. Baseline mean outcomes corresponding to subjects who did not receive any feedback.

C Conceptual Framework

C.1 Simple Conceptual Framework

To formalize the logic of the reciprocal motivation mechanism, we introduce a simple model of how the provision of government services and recapture affect 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* ($j = NC$). The probability that a household of type j protests its taxes is given by:

$$Prob(j\ protests) = \Phi(P_j), \tag{C.1}$$

where $\Phi()$ is the cumulative distribution function from a standard normal and $P_j \in (-\infty, +\infty)$ is a latent variable. Note that by construction, for any variable x, the sign of $\frac{\partial \Phi(P_j)}{\partial x}$ will be equal to the sign of $\frac{\partial P_j}{\partial x}$. For this reason, and for brevity, the following analysis focuses on the latent variable P_j. Let B_j be how much households in group j benefit from each dollar spent on government services. Consider the following relationship:

$$P_j = \ {}_0 + \quad \cdot B_j \tag{C.2}$$

where ${}_0$ is a constant and $\ < 0$ represents the reciprocal motivation: that is, when households benefit directly from government expenditures, they are less likely to protest their taxes.

C.1.1 The Effects of the School Share

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, roads). The two types of households benefit from the two types of government expenditure in the following manner:

$$B_C = \ {}^S \cdot S + \ {}^{NS} \cdot NS \tag{C.3}$$

$$B_{NC} = \ {}^{NS} \cdot NS \tag{C.4}$$

The parameters ^{S} and ^{NS} capture how households benefit from different types of expenditure. The parameter ^{S} denotes how much a household *with* children enrolled in public school benefits per dollar spent in public schools. ^{NS} denotes how much households (regardless of whether they have children) benefit per dollar spent on non-school government expenditures.

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. It is important to note that while we do not incorporate misperceptions into this simple framework, in practice, the "s" that matters is the one perceived by the taxpayer when deciding whether to protest. We thus can rewrite equations (C.3) and (C.4) as follows:

$$B_C = G \cdot (\quad ^{S} \cdot s + \quad ^{NS} \cdot (1 - s)) \tag{C.5}$$

$$B_{NC} = G \cdot \quad ^{NS} \cdot (1 - s) \tag{C.6}$$

Combining equations (C.2), (C.5), and (C.6), we obtain the following:

$$P_C = \quad _0 + \quad \cdot G \cdot (\quad ^{S} \cdot s + \quad ^{NS} \cdot (1 - s)) \tag{C.7}$$

$$P_{NC} = \quad _0 + \quad \cdot G \cdot \quad ^{NS} \cdot (1 - s) \tag{C.8}$$

Using equations (C.7) and (C.8), we can see what happens to protest rates if the school share increases. Let us start with households *without* children:

$$\frac{\partial P_{NC}}{\partial s} = - \quad \cdot G \cdot \quad ^{NS} > 0 \tag{C.9}$$

Intuitively, when the school share is increased, that unambiguously means that households *without* children benefit less from government services, and thus are more likely to protest. For the households *with* children, the effect could go either way:

$$\frac{\partial P_C}{\partial s} = \quad \cdot G \cdot (\quad ^{S} - \quad ^{NS}) \tag{C.10}$$

Intuitively, whether households *with* children are more or less likely to protest will depend on whether they benefit more from the school or non-school expenditures. If they prefer school expenditures ($\quad ^{S} > \quad ^{NS}$) then they will be less likely to protest when the school share increases. If they prefer the non-school expenditures ($\quad ^{NS} > \quad ^{S}$) then they will be more likely to protest when the school share goes up. In either case, if we subtract equation (C.9) from (C.10), we obtain the following:

$$\frac{\partial P_C}{\partial s} - \frac{\partial P_{NC}}{\partial s} = \quad \cdot G \cdot \quad ^{S} < 0 \tag{C.11}$$

In other words, when the school share goes up, while the effect on households *with* children may be negative or positive, it has to be smaller than the corresponding effect for household *without* children. The intuition is straightforward. When the school share goes up, both households *with* children and households *without* children lose in the non-school expenditures. However, for households *with* children, at least they gain in school expenditures. For that reason, even if the (latent) probability of protesting goes up for a household *with* children, it should go up less than for households *without* children because households *with* children at least have something to gain. This can be summarized in the following prediction:

Prediction 1: *When increasing the school share, the effect on the (latent) probability of protesting should be lower for households* with *children in public schools than for households* without *children in public schools.*

One special case worth mentioning is when households *with* children in public schools benefit more from school expenditures than non-school expenditures. Intuitively, 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). For that reason, it is plausible that the households *with* children prefer school expenditures over non-school expenditures:

Corollary 1: *If* $\;^{S} > \;^{NS}$*, an increase in the school share should negatively affect the (latent) protest probability of households* with *children in public schools and positively affect the (latent) protest probability of households* without *children in public schools.*

These predictions are based on some assumptions. First, this setup assumes that benefits from non-school services are the same for households *with* children as for households *without* children. However, the main predictions will still hold as long as the parameters are close enough between the two types of households. Second, our model assumes that households are entirely selfish, and thus households *without* children do not benefit at all from school spending. In practice, these taxpayers may feel happy to help other parents in the community, they may benefit from schools in the future, or value public schools because they had children in schools in the past. Alternatively, they may benefit from school spending for selfish reasons if, for instance, it reduces crime in the neighborhood. Nevertheless, in Appendix C.2.1, we show that the main prediction still holds under more general assumptions.

C.1.2 The Effects of Recapture of School Taxes

Extending this simple model to include redistribution of school taxes is straightforward. Non-school expenditures are still NS. School expenditures are now $S \cdot (1 - r)$, where $r \in [-\inf, 1]$ is what we previously defined as the recapture share and represents the direction and intensity of the effects of recapture on the funding available for the local school district.[83] If there is no recapture, or if there is recapture but the local school district does not lose or gain in net terms, $r = 0$, and we are back to the original model. A positive value of r means that the school district is a net contributor to the recapture system. More precisely, r is the fraction of school taxes raised in the district transferred to disadvantaged school districts and, therefore, cannot be spent in the local school district. For example, $r = 0.1$ would indicate that 10% of local school taxes are redistributed to other school districts. On the other hand, a negative value of r means that the school district is a net beneficiary of the recapture system and thus can spend more in schools than what the district raised in school taxes. More precisely, for each dollar raised locally in school taxes, the school district can spend an additional $-r$ dollars thanks to net transfers from wealthier school districts. For example, $r = -0.1$ would indicate that the local school district can spend the school taxes it collects plus an additional 10% from the amount recaptured.

We can extend equations (C.3) and (C.4) to incorporate recapture into the model:

$$B_C = \quad {}^{S} \cdot S \cdot (1-r) + \quad {}^{NS} \cdot NS \tag{C.12}$$

$$B_{NC} = \quad {}^{NS} \cdot NS \tag{C.13}$$

We combine equations (C.12) and (C.13) with equation (C.2), and then rearrange them as follows:

$$P_C = \ {}_0 + \quad \cdot \quad {}^{S} \cdot S \cdot (1-r) + \quad \cdot \quad {}^{NS} \cdot NS \tag{C.14}$$

$$P_{NC} = \ {}_0 + \quad \cdot \quad {}^{NS} \cdot NS \tag{C.15}$$

We can see what would happen if we increased the recapture share:

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

Households *without* children in the school district do not benefit from school taxes, regardless of whether their school district gives or receives funding from the recapture system, so their willingness to pay taxes is not affected by recapture.

$$\frac{\partial P_C}{\partial r} = - \quad \cdot \quad {}^{S} \cdot S \cdot > 0 \tag{C.17}$$

For households *with* children, in turn, more recapture means fewer benefits for their local

[83] The value of r can be below -1 because, in theory, a school district could receive through recapture more than 100% of the amount it raised in school taxes.

school district, and they are thus less willing to pay taxes.

We can also subtract (C.16) from (C.17) to show the following:

$$\frac{\partial P_C}{\partial r} - \frac{\partial P_{NC}}{\partial r} = - \quad \cdot \quad {}^{S} \cdot S \cdot > 0 \tag{C.18}$$

Again, there is an unambiguous prediction about the differential effect between households *with* children and *without* children. These results are summarized in the following prediction:

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

However, we must take this prediction with a grain of salt. Our setup assumes that households are totally selfish. However, this assumption may be misleading: as the survey data show, and contrary to the prediction of the selfish model, there is quite a bit of support for the recapture system. A more realistic model would include altruism. For example, when funds are transferred from advantaged to disadvantaged districts due to recapture, households may appreciate that their tax dollars are helping the most disadvantaged households, even if that means their children will have fewer resources. In Appendix C.2.2, we provide an extension of this framework that incorporates altruism and show that Prediction 2 may no longer hold.

C.2 Extensions to the Conceptual Framework

C.2.1 The Effects of School Share

Next, we discuss how the predictions of the conceptual framework change under more general assumptions. As in the simpler model, we allow the two groups of households *with* and *without* children to have different preferences regarding government expenditures in local public schools and on other local government services. However, this time, we do it more flexibly:

$$B_j = \ {}_j^S \cdot S + \ {}_j^{NS} \cdot NS \tag{C.19}$$

Where parameters ${}_j^k > 0 \ \forall j \in \{C, NC\}, \ \forall k \in \{S, NS\}$ capture how much different households benefit from different types of expenditures. The parameter ${}_C^S$ denotes how much a household *with* children enrolled in public school benefits per each dollar spent in public schools, while ${}_{NC}^S$ denotes the corresponding parameter but for households *without* children in public schools. That is, we allow households *without* children in public schools to benefit somewhat from the provision of public schools: e.g., they may get a warm glow effect from helping other parents in the community, schools may lower crime, they can benefit from schools in the future, or they value public schools because they had children in schools in the past. The only assumption that we impose is that households *with* children should enjoy school spending more than households *without* children enjoy school spending (${}^{S}_{C} > {}^{S}_{NC}$). The logic behind this assumption is that households *with* children get everything that the households *without* children get (e.g., happier neighbors, lower crime), but on top of that, households *with* children get education for their own kids.

Likewise, ${}^{NS}_{C}$ denotes how much a household *with* children enrolled in public school benefits per each dollar spent in non-school services, while ${}^{NS}_{NC}$ is the corresponding parameter for households *without* children in public schools.

Next, we can normalize equation (C.19) by total expenditures, G:

$$B_j = G \cdot (\ {}^{S}_{j} \cdot s + \ {}^{NS}_{j} \cdot (1 - s)) \tag{C.20}$$

Combining equations (C.2) in body of the paper and (C.20) and re-arranging terms, we obtain the following:

$$P_j = \ {}_{0} + \ \cdot G \cdot (\ {}^{NS}_{j} + s \cdot (\ {}^{S}_{j} - \ {}^{NS}_{j})) \tag{C.21}$$

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

$$\frac{\partial P_C}{\partial s} = \ \cdot G \cdot (\ {}^{S}_{C} - \ {}^{NS}_{C}) \tag{C.22}$$

$$\frac{\partial P_{NC}}{\partial s} = \ \cdot G \cdot (\ {}^{S}_{NC} - \ {}^{NS}_{NC}) \tag{C.23}$$

Now we take the difference between the last two equations:

$$\frac{\partial P_C}{\partial s} - \frac{\partial P_{NC}}{\partial s} = \ \cdot G \cdot (\ {}^{S}_{C} - \ {}^{S}_{NC} + \ {}^{NS}_{NC} - \ {}^{NS}_{C}) \tag{C.24}$$

Assume that, as in the baseline model, households *with* children enjoy the non-school expenditures at the same rate as households *without* children enjoy non-school expenditures (${}^{NS}_{NC} = {}^{NS}_{C}$). In that case, the whole term becomes negative, and Prediction 1 still holds. That is, the main prediction from the model still holds even if households *without* children benefit somewhat from the school spending.

The prediction can hold even if households *with* children enjoy non-school expenditures at a different rate than households *without* children enjoy non-school expenditures (${}^{NS}_{NC} \neq {}^{NS}_{C}$). If ${}^{NS}_{NC} > {}^{NS}_{C}$, then prediction 1 still holds. Indeed, prediction 1 can still hold even if ${}^{NS}_{NC} < {}^{NS}_{C}$, as long as the extent to which households *with* children like school expenditures more than households *without* children is greater than the extent to which households *without* children like school expenditures more than households *with* children $({}^{S}_{C} - {}^{S}_{NC} > {}^{NS}_{NC} - {}^{NS}_{C})$.

C.2.2 The Effects of Recapture Share

Next, we extend the baseline model in the body of the paper to include altruism and discuss its implications for the effects of recapture. For the sake of simplicity, we focus on the case of a school district that is a net contributor to the recapture system. For simplicity, we assume that households *with* and *without* children benefit equally from non-school spending: ${}^{NS}_{C} = {}^{NS}_{NC} = {}^{NS}$ and, as in the body of the paper, assume that households *without* children do not benefit from school expenditures. We include altruistic preferences towards disadvantaged school districts:

$$B_C = {}^{S} \cdot S \cdot (1 - r) + {}^{NS} \cdot NS + {}^{R} \cdot S \cdot r \tag{C.25}$$

$$B_{NC} = {}^{NS} \cdot NS + {}^{R} \cdot S \cdot r \tag{C.26}$$

The key difference lies in the parameter ${}^{R} > 0$, which captures how much households of a given type enjoy giving resources to more disadvantaged school districts. We combine equation (C.25) and (C.26) with equation (C.2) in the body of the paper and then re-arrange:

$$P_C = {}_{0} + \cdot {}^{S} \cdot S \cdot (1 - r) + \cdot {}^{NS} \cdot NS + \cdot {}^{R} \cdot S \cdot r \tag{C.27}$$

$$P_{NC} = {}_{0} + \cdot {}^{NS} \cdot NS + \cdot {}^{R} \cdot S \cdot r \tag{C.28}$$

If we take the derivative with respect to r for households *with* children:

$$\frac{\partial P_C}{\partial r} = \cdot S \cdot ({}^{R} - {}^{S}) \tag{C.29}$$

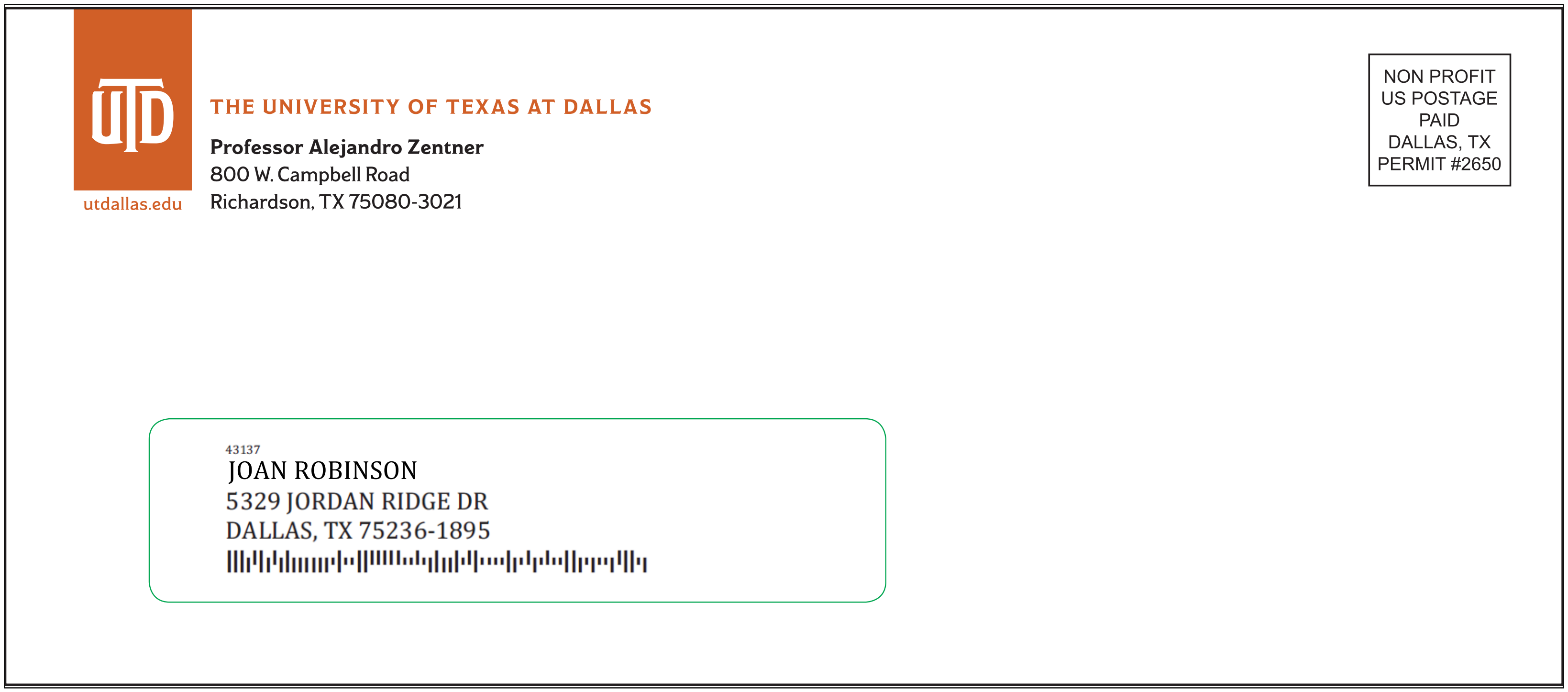
This is the first departure from prediction 2. For households *with* children, now two effects are pulling in opposite directions. On the one hand, we have the original effect that more recapture means fewer benefits for their own children and thus less willingness to pay taxes. On the other hand, they benefit from the warm glow or other increases in utility they derive from helping disadvantaged children and thus are more willing to pay taxes. The sign of the net effect depends on the relative strength of these two forces (i.e., ^{R} versus ^{S}).

Now let us take the derivative with respect to r for households *without* children:

$$\frac{\partial P_{NC}}{\partial r} = \cdot S \cdot {}^{R} > 0 \tag{C.30}$$

This is the second departure from prediction 2. Prediction 2 is based on a model that assumes that households *without* children do not benefit from school taxes and thus do not care about whether tax revenues are recaptured or not. Consistently, these households' likelihood of protesting did not depend on the extent of recapture. In the extended model, they actually enjoy the upside of recapture through the altruistic term and thus become less likely to protest when recapture increases.

D Sample Envelope



E 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.

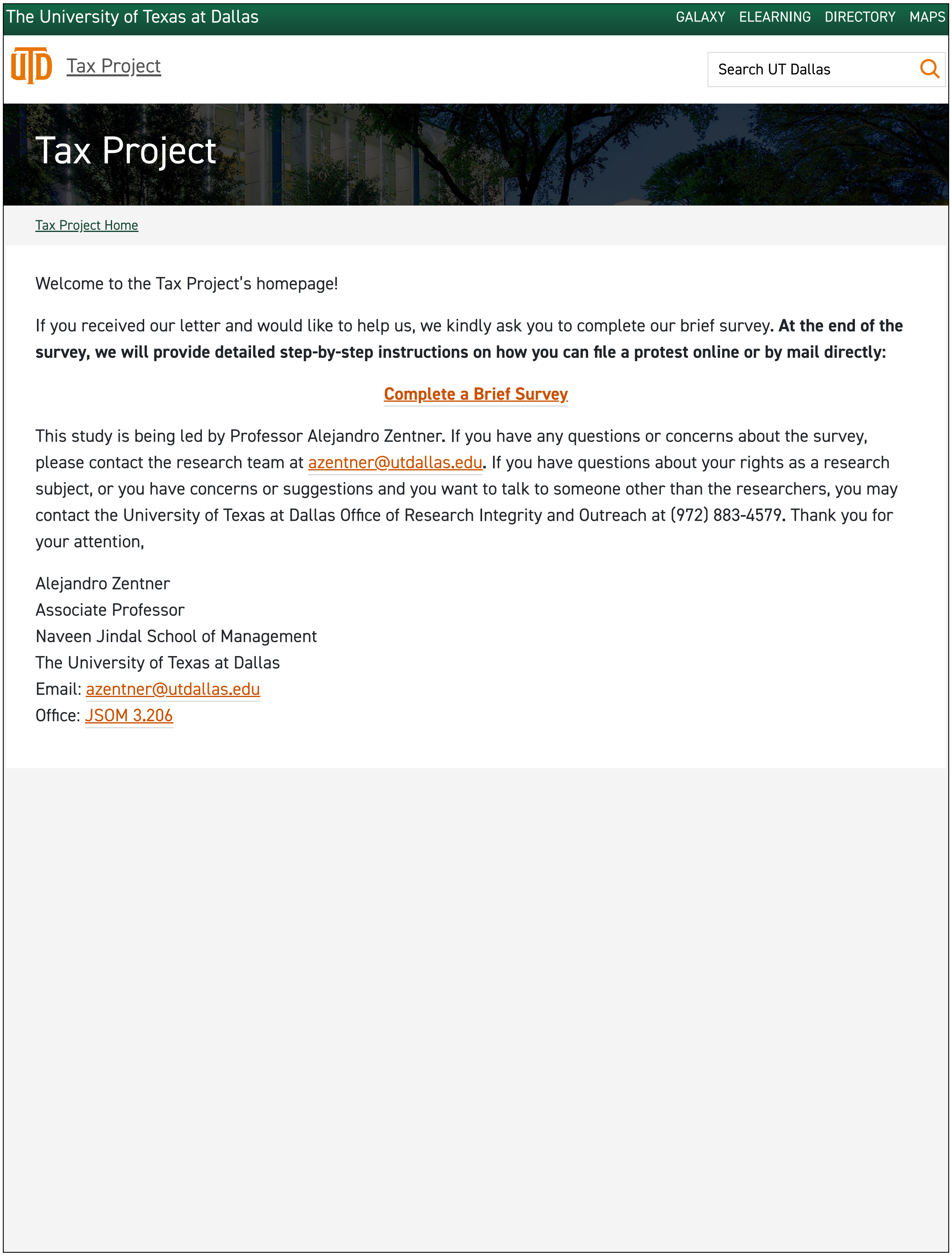
Thank you for your attention!

Alejandro Zentner

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

Joan Robinson
123 Fake Street

F Project's Website



The University of Texas at Dallas GALAXY ELEARNING DIRECTORY MAPS

Tax Project

Search UT Dallas

Tax Project

Tax Project Home

Welcome to the Tax Project's homepage!

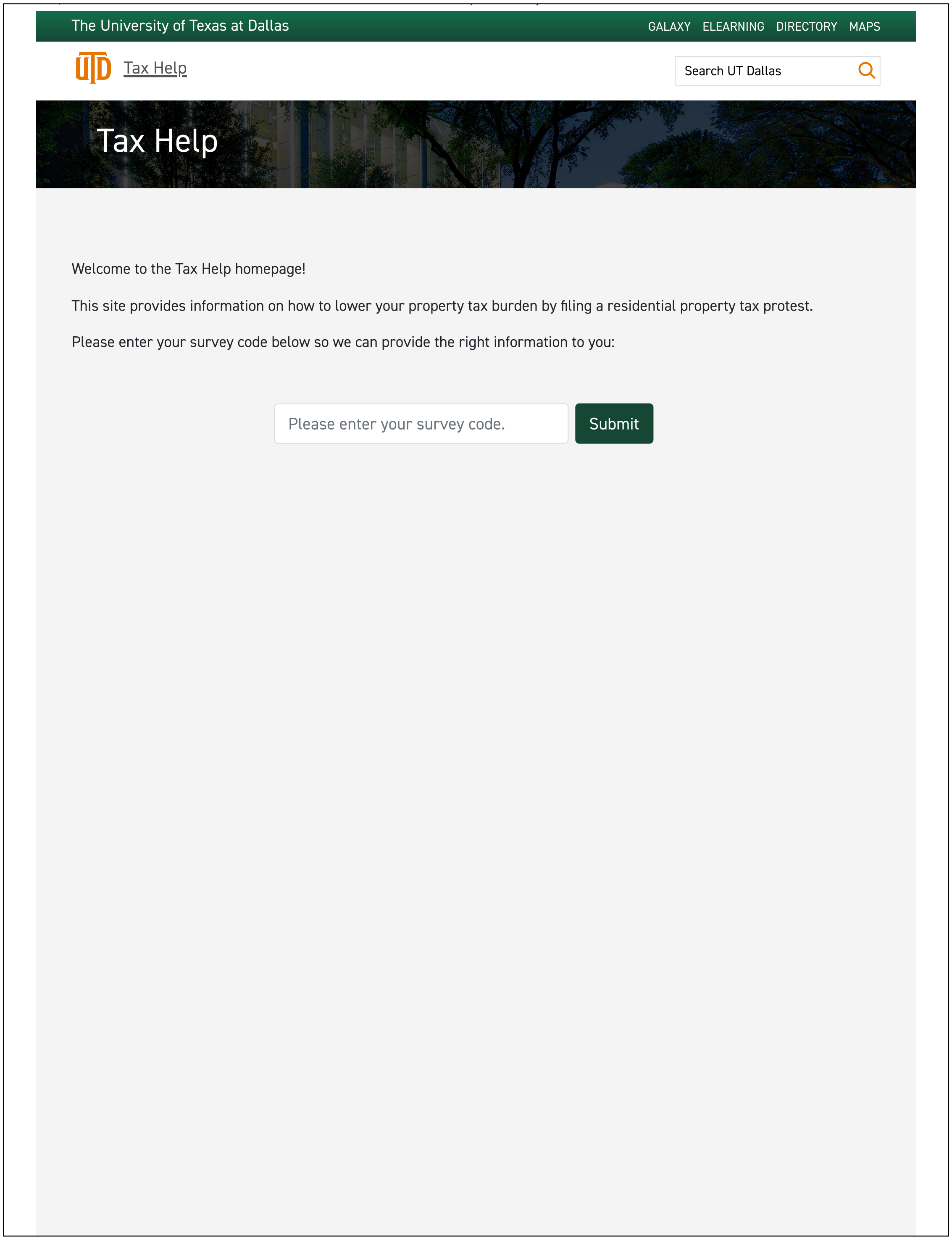
If you received our letter and would like to help us, we kindly ask you to complete our brief survey. **At the end of the survey, we will provide detailed step-by-step instructions on how you can file a protest online or by mail directly:**

Complete a Brief Survey

This study is being led by Professor Alejandro Zentner. If you have any questions or concerns about the survey, please contact the research team at azentner@utdallas.edu. If you have questions about your rights as a research subject, or you have concerns or suggestions and you want to talk to someone other than the researchers, you may contact the University of Texas at Dallas Office of Research Integrity and Outreach at (972) 883-4579. Thank you for your attention,

Alejandro Zentner
Associate Professor
Naveen Jindal School of Management
The University of Texas at Dallas
Email: azentner@utdallas.edu
Office: JSOM 3.206

G Tax Help Homepage



The University of Texas at Dallas GALAXY ELEARNING DIRECTORY MAPS

UTD 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

UTD 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.

Find Property By Owner Name

Search By: Owner Name Account Number Street Address Business Name Map

If you would like to access the **uFile** System to protest your account, please use the search function from this screen to locate your account and then select the **uFile Online Protest** link from the Account detail screen.

Enter at least the first two letters of the last name in the format
Last Name [space] First Name.

Owner Name: Doe Jane [Search]

Account Type
- [x] RESIDENTIAL
- [ ] COMMERCIAL
- [ ] BPP

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 uFile Online Protest system for your property, click the link on the left titled "uFile Online 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)

Print Homestead Exemption Form

Print/Mail Account Protest Form

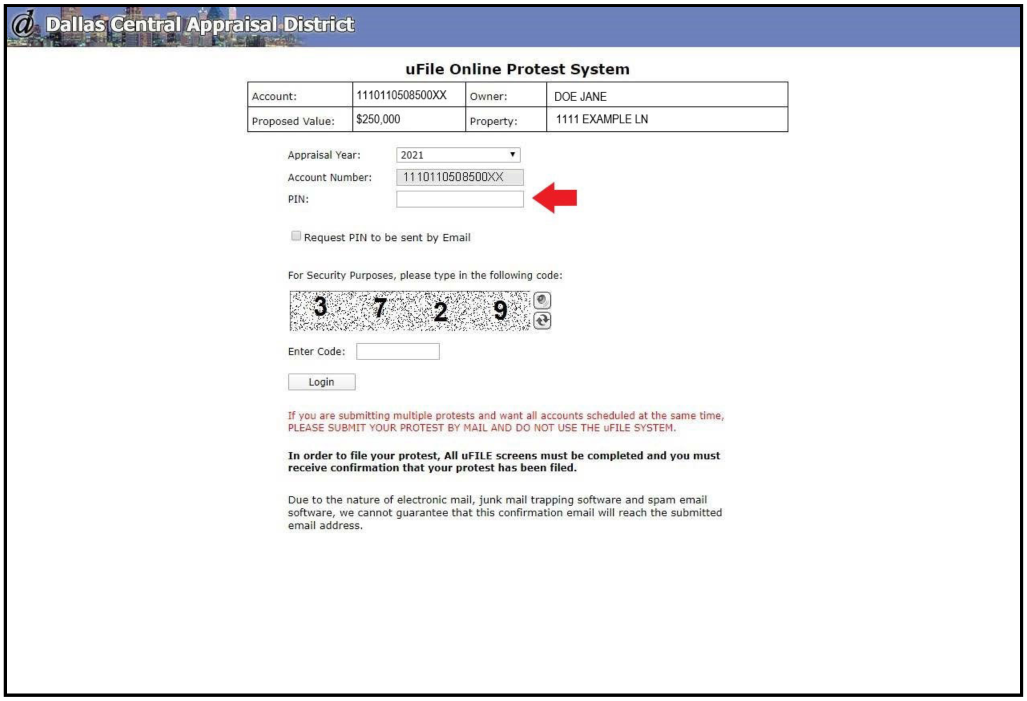
Legal Desc (Current 2021)

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

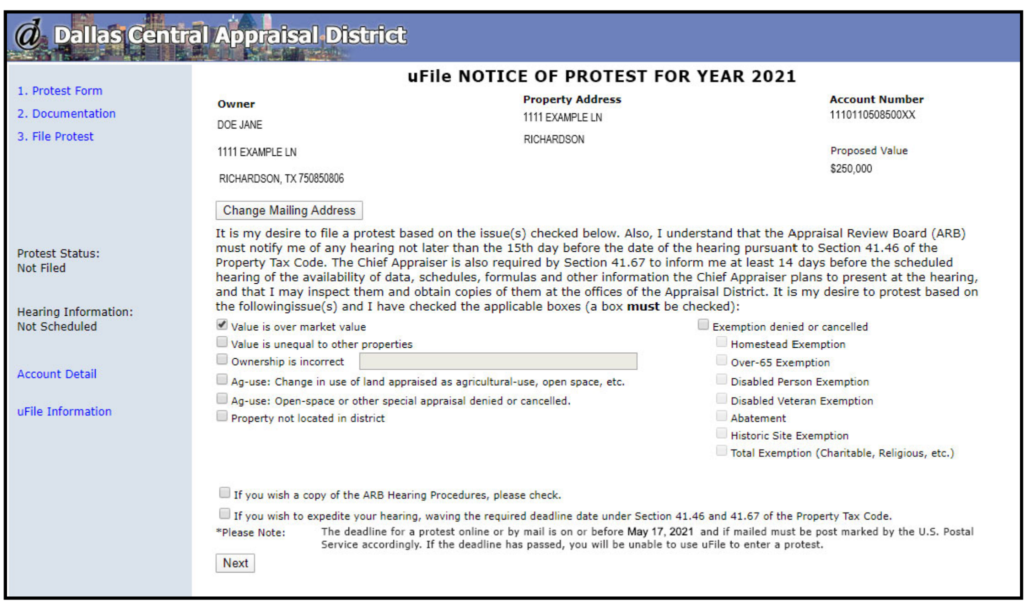
Value

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

Step 5. 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:

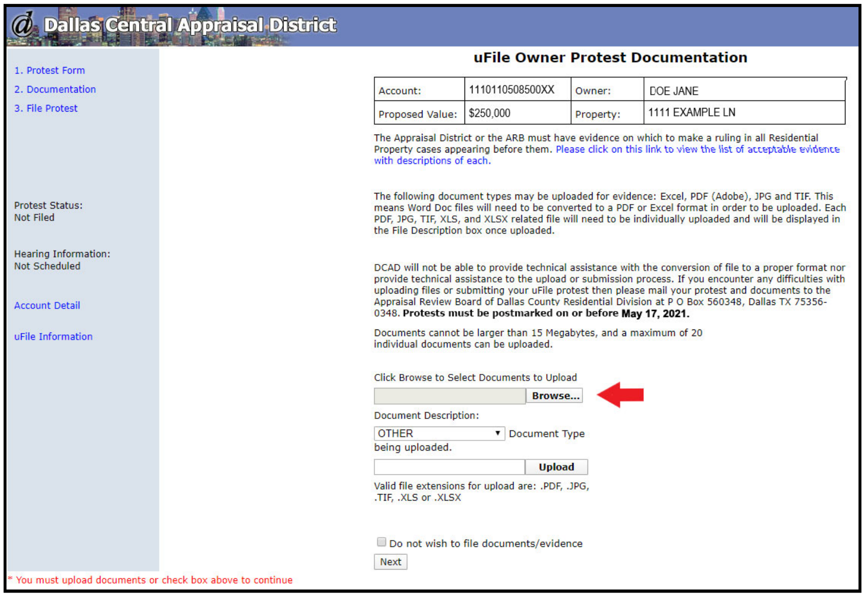
"I found a home that is similar to mine but was recently sold for less than my home's appraised market value. The property located at 1204 Example Ln (Richardson, TX) is 0.29 miles away from home, and has the same number of bedrooms and a similar square footage. The property was sold on 11/31/2020 for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

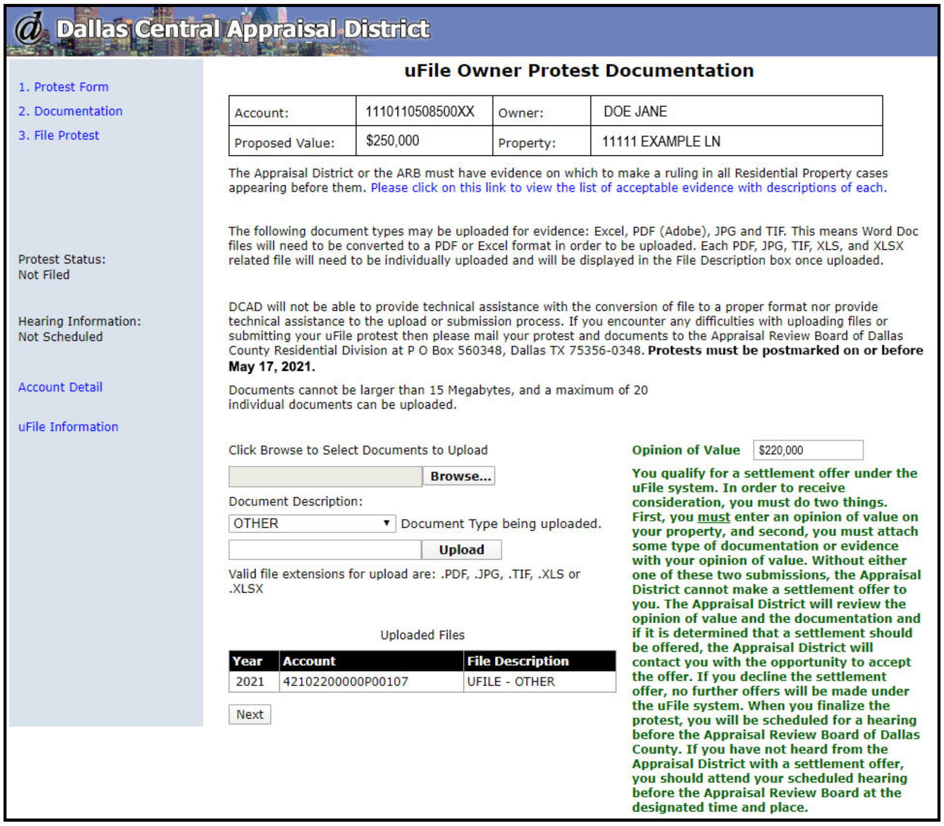
"I found a home that is similar to mine but that has a proposed value that is lower than that of my home. The property located at 1204 Example Ln (Richardson, TX) is 0.29 miles away from home, and has the same number of bedrooms and a similar square footage. The proposed market value is $220,000."

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.



Dallas Central Appraisal District

uFile Owner Protest Documentation

1. Protest Form
2. Documentation
3. File Protest

Protest Status:
Not Filed

Hearing Information:
Not Scheduled

Account Detail

uFile Information

Account:	1110110508500XX	Owner:	DOE JANE
Proposed Value:	$250,000	Property:	11111 EXAMPLE LN

The Appraisal District or the ARB must have evidence on which to make a ruling in all Residential Property cases appearing before them. Please click on this link to view the list of acceptable evidence with descriptions of each.

The following document types may be uploaded for evidence: Excel, PDF (Adobe), JPG and TIF. This means Word Doc files will need to be converted to a PDF or Excel format in order to be uploaded. Each PDF, JPG, TIF, XLS, and XLSX related file will need to be individually uploaded and will be displayed in the File Description box once uploaded.

DCAD will not be able to provide technical assistance with the conversion of file to a proper format nor provide technical assistance to the upload or submission process. If you encounter any difficulties with uploading files or submitting your uFile protest then please mail your protest and documents to the Appraisal Review Board of Dallas County Residential Division at P O Box 560348, Dallas TX 75356-0348. **Protests must be postmarked on or before May 17, 2021.**

Documents cannot be larger than 15 Megabytes, and a maximum of 20 individual documents can be uploaded.

Click Browse to Select Documents to Upload

Browse...

Document Description:

OTHER Document Type being uploaded.

Upload

Valid file extensions for upload are: .PDF, .JPG, .TIF, .XLS or .XLSX

Uploaded Files

Year	Account	File Description
2021	42102200000P00107	UFILE - OTHER

Next

Opinion of Value $220,000

You qualify for a settlement offer under the uFile system. In order to receive consideration, you must do two things. First, you must enter an opinion of value on your property, and second, you must attach some type of documentation or evidence with your opinion of value. Without either one of these two submissions, the Appraisal District cannot make a settlement offer to you. The Appraisal District will review the opinion of value and the documentation and if it is determined that a settlement should be offered, the Appraisal District will contact you with the opportunity to accept the offer. If you decline the settlement offer, no further offers will be made under the uFile system. 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 the Appraisal District with a settlement offer, you should attend your scheduled hearing before the Appraisal Review Board at the designated time and place.

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.

The University of Texas at Dallas GALAXY ELEARNING DIRECTORY MAPS

Tax Project Search UT Dallas

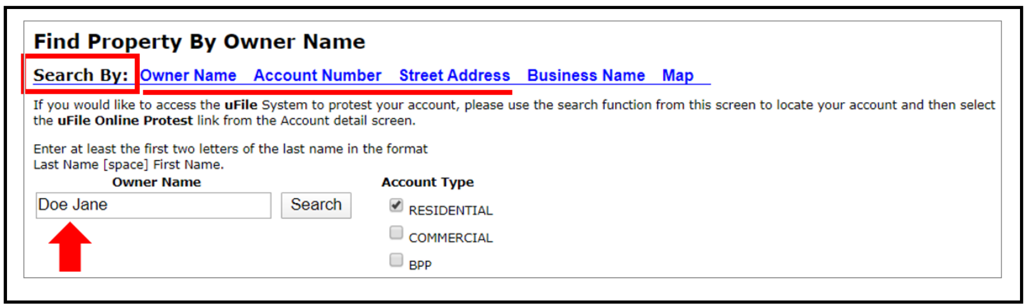
Walkthrough for Filing a Protest by Mail

Tax Project Home » 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: BLKXX LOTXX
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.

H Sample of Online 2021 Appraisal Notice

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

www.dallascad.org (214) 905-9402

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

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.

I 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:

UT DALLAS

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.

UT DALLAS

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

- 0
- 1
- 2
- 3
- 4 or more

UT DALLAS

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.

UT DALLAS

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.

If not selected to receive feedback

You have been randomly selected to not receive the data.

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

If selected to receive school feedback

You have been randomly selected to receive the following data:

In a previous question you indicated you believe that 50% of your total property taxes in 2020 went to cover school taxes. According to our best estimate (based on public data), **52.9%** of your total property taxes in 2020 corresponded to school taxes.

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.

If not selected to receive feedback

You have been randomly selected to not receive the data.

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

If selected to receive recapture feedback

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

UT DALLAS

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.

J Questionnaire: Prediction Survey

UCLA Anderson School of Management

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.

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[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.

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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).

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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).

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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.

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UCLA Anderson School of Management

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

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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.

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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

← →

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.

← →

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).

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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

← →

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

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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

← →

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

← →