

My Taxes Are Too Darn High: Why Do Households Protest Their Taxes?[†]

By BRAD NATHAN, RICARDO PEREZ-TRUGLIA, AND ALEJANDRO ZENTNER*

In the United States and globally, taxpayers can file a protest to legally reduce their property taxes. While tax protests can provide a unique opportunity to study the (un)willingness to pay taxes, they have received little attention from researchers. Using a field experiment and a quasi-experiment, we study what motivates households to protest their property taxes. We find that both expected savings and filing frictions play significant roles. We estimate the magnitude of filing frictions using a money metric. We also discuss how low-cost interventions targeted at disadvantaged groups can mitigate existing disparities in the system of tax appeals. (JEL C93, D91, G51, H71)

Property taxes are an important source of revenue for governments in the United States and around the world. For example, property taxes are the second largest source of tax revenues in the United States, generating an estimated \$547 billion in 2018 (TPC Briefing Book 2021).¹ In all US states, as well as in many other countries, households can file tax protests, also known as tax appeals, to legally reduce the amount they have to pay in property taxes.² These protests are consequential: while there is no guarantee, tax protests can substantially reduce

*Nathan: Rutgers Business School, Rutgers University (brad.nathan@business.rutgers.edu); Perez-Truglia: Anderson School of Management, University of California, Los Angeles (ricardo.truglia@anderson.ucla.edu); Zentner: Naveen Jindal School of Management, University of Texas at Dallas (azentner@utdallas.edu). Naomi Feldman was coeditor for this article. Dedicated in loving memory of Alberto Alesina (1957–2020). We are thankful for excellent comments from Youssef Benzarti, Raj Chetty, Austan Goolsbee, Emiliano Huet-Vaughn, Justin Holz, Steve Levitt, John List, Erzo Luttmer, Michael Norton, Stefanie Stantcheva, Matt Weinzierl, and other colleagues and seminar participants at the NBER-Public Economics, Stanford University, University of Chicago, Yale University, Universidad de San Andres, the American Economic Association, NTA, ZEW, Journees LAGV, LMU, RIDGE, and AEI. This project was reviewed and approved in advance by the Institutional Review Board at the University of Texas at Dallas (Legacy-IRB-20MR0126; title: “Effect of Information on Tax Property Payments”; creation date: April 23, 2020). The experiments were registered in the AEA RCT Registry #0005992 (Nathan, Perez-Truglia, and Zentner 2025b). The original preregistration was posted on May 24, 2020. However, after receiving interview requests from the media, we removed the preregistration (we were concerned that the media would divulge the hypotheses listed in the preregistration and, thus, contaminate the study). We reposted the preregistration on June 16, 2020, after the deadline to file a protest had passed (and before the data to conduct the analysis became available). Adrian Cadena Medina, Luisa Cefala, Dongwook Chun, Karl Dill, Santiago De Martini, and Xinmei Yang provided superb research assistance.

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¹For reference, the federal income tax generated \$1.7 trillion and the corporate income tax generated \$230 billion in 2019 (Stein and Ingraham 2019).

²See for example Dobay et al. (2019), who overview how property tax appeals work across states and provinces in the United States, Canada, and Australia and at the country level in Hong Kong, Ireland, New Zealand, South Africa, and the United Kingdom. World Bank (2019) overviews how appeals work across Latin American and Caribbean countries.

the tax amount due. Tax protests provide a unique opportunity to study the (un) willingness to pay taxes, yet they have been largely overlooked by researchers. To address this research gap, we study key factors that motivate households to protest their taxes.

Using a field experiment and a quasiexperiment in Dallas County (Texas), we show that both expected savings and filing frictions play significant roles in the decision to protest taxes. We focus on a single county purely for the logistical advantage of implementing a field experiment in a single location. Tax protests in Dallas County work in an almost identical way, as in all 241 counties in Texas, and although there are some differences, there are similarities in how they work in other US states and countries.³ Dallas County is the second largest county in Texas, with an estimated population of about 2.6 million in 2020—indeed, Dallas County has a larger population than 15 of the 50 US states (US Census Bureau 2021). The county is diverse in many dimensions including, but not limited to, ethnicity and political parties.⁴ Texas does not have a state income tax and therefore property taxes are a key source of revenue for the provision of government services. The average household in Dallas County was expected to pay around \$5,916 in property taxes in 2020, corresponding to an average tax rate of 2.01 percent of home value.⁵

The process of protesting property taxes in Dallas County can be summarized as follows. The Dallas Central Appraisal District (DCAD) formulates a proposed assessment of the property's market value. We refer to this amount as the proposed value. Property taxes are calculated on the basis of the proposed value. The DCAD notifies households of the proposed value and estimated tax amount. The homeowner has the option to file a protest—for example, arguing that the proposed value (and thus the corresponding tax due) is too high.⁶ Owners can protest directly on their own (which is the main focus of this study) either online or by mail, or they can hire an agent to protest on their behalf. The DCAD then responds to the homeowner's appeal.

Tax protests can significantly reduce the amount that a household pays in property taxes. In 2020, 8.40 percent of households in Dallas County filed a protest on their own, and an additional 8.42 percent of households protested with the help of an agent, resulting in a total protest rate of 16.82 percent. We refer to a protest as being successful if the DCAD lowers the effective assessed value. We estimate that 69.7 percent of protests were successful in 2020, resulting in average tax savings of \$485 (8.2 percent of the average property tax bill) in the first year alone.⁷

³For example, property owners must pay a filing fee in Alaska. Due to institutional factors, tax protests are less common in some contexts (e.g., in California, appraised values are updated only when properties are sold, and thus, there is much less incentive for households to file a protest).

⁴For instance, in the 2012 presidential election, Barack Obama received 57 percent of the votes, whereas Mitt Romney received 42 percent, and the remaining 1 percent of votes went to third-party candidates (Ansola-behere, Palmer, and Lee 2014).

⁵Throughout this study, we use the term *tax rate* to refer to the effective tax rate (computed as the total amount of property tax paid divided by its market value), rather than the jurisdictional tax rates.

⁶We describe the protest process in more detail in Section I.

⁷These estimates correspond to owners who protested directly in 2020. These and other estimates in the paper are based on administrative data as of November 2020 and, thus, do not include a few protests that may have been resolved after November 2020. Savings can accumulate in future years. There are also important dynamic considerations. For example, protesting every year can reduce the effects of potential future tax increases.

The average protest rate masks large heterogeneity. For example, wealthier homeowners have a much higher probability of protesting: the average protest rate is 42.0 percent for homes worth over \$500,000, but only 8.9 percent for homes worth less than \$100,000. Additionally, there are significant racial differences in the probability of protesting, even after conditioning on the home value.⁸ For example, Hispanic households are significantly less likely to file a protest than their White counterparts. Some individuals have been quite critical, arguing that “the property tax system is rigged against (...) little people” (Lieber 2020). For example, minority groups may be left behind because they are less savvy or because they cannot afford an agent (Doerner and Ihlanfeldt 2015).

We start by measuring the extent to which expected tax savings affect the decision to file a tax protest. Studying households’ responses to this basic economic incentive is not only important on its own, but this effect also plays a key role in that it serves as a money metric—that is, it allows us to translate the magnitude of our filing friction estimates into dollar amounts. To study the role of expected tax savings, we exploit quasiexperimental variation introduced by a feature of the Texas property code known as the homestead cap. All counties in Texas must use a cap when calculating taxes for households with homestead status.⁹ The homestead cap threshold generates a sharp kink in the marginal benefits from protesting. When the proposed value is below the threshold, a marginal reduction in the proposed value reduces the amount due in property taxes. However, when the proposed value exceeds the threshold, a marginal reduction in the proposed value has no effect on the tax amount. Using this exogenous variation, we find that a \$100 increase in the marginal benefits of protesting causes an increase of 2.14 percentage points in the probability of protesting.

We designed a field experiment to explore the role of filing frictions in the decision to protest taxes.¹⁰ We conducted the field experiment with a subject pool of 78,462 households. In Dallas County, any household can protest property taxes for free, meaning that protesting incurs no pecuniary costs. However, households may face filing frictions. These frictions include the hassle costs from filing taxes, such as the opportunity cost of time and the unpleasant nature of doing paperwork (Benzarti 2020; Sunstein 2021; Benzarti 2021). Additionally, our definition of filing frictions includes information frictions. For example, some households may not protest because they do not know how to protest or because they believe filing a protest is a lot harder than it really is. Of the 78,462 households in the subject pool, 50,394 were randomly assigned to receive a letter aimed at reducing filing frictions. We randomized each of these households into one of two letter types, with increasing degrees of treatment intensity. The basic aid letter included a step-by-step guide for filing a protest by mail or online. The extra aid letter included the same information

⁸There is an ongoing debate regarding the definition and classification of race and ethnicity, which extends beyond the scope of our paper. To maintain brevity, we will henceforth refer to race when discussing this topic.

⁹Homestead is a legal status that can be granted to a property that is the owner’s (owners’) primary residence. This status comes with several advantages related to property taxes (e.g., exemptions and caps) and other benefits (e.g., exemption from forced sale for debt collection).

¹⁰As part of the field experiment, we also included a second treatment arm related to fairness considerations. The design and results of this treatment arm are presented in a separate article (Nathan, Perez-Truglia, and Zentner 2024).

as the basic aid letter, as well as additional instructions on how to complete one of the most challenging aspects of the process: preparing an argument to support the protest.¹¹

The evidence from our field experiment indicates that filing frictions are of first-order importance in the decision to protest taxes. We find that receiving a letter had a large positive effect on the probability of filing a protest, and that the letter that offered more aid induced a larger effect. These effects are not only highly statistically significant, but economically significant too. For example, the extra aid letter increased the protest rate by 4.98 percentage points.¹² For reference, this effect is equivalent to 57.4 percent of the baseline protest rate.

We use multiple strategies to show that the letters worked through reducing information frictions, rather than merely making the protest option more salient. Moreover, to quantify the magnitude of the filing frictions, we combine the experimental and quasiexperimental estimates. Our back-of-the-envelope calculations indicate that filing frictions are on the order of \$232. Indeed, this estimate constitutes a lower bound, as our intervention is probably far from eliminating the filing frictions in full. This result suggests that filing frictions are a key reason why some households do not file a tax protest.

We also explore the potential of our low-cost intervention to address inequities in the system of tax appeals. We provide evidence of significant gaps in the system of tax appeals by wealth and race. Protest and tax savings rates are substantially lower for less wealthy households than for wealthier households. Moreover, even after conditioning on home values, protest and tax savings rates are significantly lower for Hispanic and Black households than for White households. In fact, our evidence for Dallas County is consistent with prior evidence on racial disparities in protest rates in Cook County, Illinois (Avenancio-León and Howard 2022). Our experimental results show that low-cost interventions can help mitigate unequal access to the protest system, but only if the aid is targeted at the disadvantaged groups in particular. On the contrary, providing universal aid to all households would, if anything, further widen the disparities.

Our study contributes to understanding tax appeals. Although tax protests are common in the United States and around the world (Dobay et al. 2019; World Bank 2019), they have received little to no attention in the economic literature. In comparison, other forms of tax compliance, such as tax evasion and tax delinquency, have been studied extensively (Luttmer and Singhal 2014; Slemrod 2019). One notable exception is Jones (2019), which uses data on the decision to protest taxes to provide a loss aversion test.¹³ Another notable exception is Avenancio-León and Howard (2022), which shows that property taxes are systematically higher for racial minorities and

¹¹ In the extra aid message, we included an argument tailored to each recipient that could be copied into the protest form. These letters presented information about another property close to the recipient's own property that was comparable in all observable characteristics and was recently sold for a lower price than the market value proposed by the DCAD.

¹² This is our preferred treatment-on-the-treated estimate, which accounts for some letters failing to be delivered or not being read by the subjects. In comparison, the raw intention-to-treat effect is 3.51 percentage points.

¹³ Jones (2019) shows that the probability of protesting increases when the assessed value is revised upward and decreases when the assessed value is revised downward, but the effect is much larger (in absolute value) for upward revisions than for downward revisions.

that some of those differences may be related to the system of tax appeals.¹⁴ We contribute to this literature by using experimental and quasiexperimental variation to measure the motives behind the decision to file a tax protest.

Our findings also contribute to the existing literature on how factors such as information complexity, salience, procrastination, or confusion can result in incomplete uptake of social benefits (e.g., Chetty, Looney and Kroft 2009; Bhargava and Manoli 2015). More precisely, the literature provides suggestive evidence that taxpayers forego significant amounts of money due to filing frictions (Benzarti 2020, 2021). We contribute to this literature by providing experimental evidence as well as measuring filing frictions with a money metric. We also show that targeted interventions can help mitigate unequal access by disadvantaged groups, contributing to the literature on the take up and targeting of social benefits (Alatas et al. 2012, 2016; Finkelstein and Notowidigdo 2019).

The remainder of the paper proceeds as follows. Section I describes the institutional context. Section II presents evidence on the effects of expected tax savings. Section III focuses on the role of filing frictions. Section IV presents evidence on economic and racial disparities. The last section concludes.

I. Institutional Context

A. Property Taxes in Dallas County

In Texas, counties collect property taxes that they use to fund various services, including schools, parks, roads, and police and fire departments. In Dallas County, the tax assessor contractually collects property taxes. Although the county collects property taxes on both residential and business properties, this study focuses on single-family residential homes. We use publicly available administrative data from the DCAD (Dallas Central Appraisal District 2024). For each home in the county, the data include information on ownership; address; property characteristics (e.g., number of bedrooms); and historical yearly data on proposed and certified market values, exemption amounts, taxable values, tax rates, and details on property tax protest records. Whenever needed, we complement the administrative records with other data sources. The amount of property tax due is calculated by multiplying the proposed value by the tax rates for the various jurisdictions within the county.¹⁵ Some households have special exemptions, such as the homestead exemptions discussed in detail in Section II below.¹⁶

¹⁴ Some other studies look at property taxes more generally, without focusing on protests. For example, Cabral and Hoxby (2012) provides evidence on how the salience of property taxes can affect equilibrium tax rates. Tax appeals have also received the attention of legal scholars (Hayashi 2014).

¹⁵ Replication data and code for this study can be found in the online repository Inter-University Consortium for Political and Social Research 193542 (Nathan, Perez-Truglia, and Zentner 2025a).

¹⁶ Within Dallas County, the four types of taxing jurisdictions are the county (including a community college, a hospital, and the county itself), 31 cities, 16 independent school districts (ISDs), and 12 special districts (source: <https://comptroller.texas.gov/taxes/property-tax/county-directory/dallas.php>). Supplemental Appendix A.1.1 provides additional details about property taxes in Dallas County and data sources.

B. Tax Protests

Homeowners have the right to protest if they disagree with the DCAD's proposed assessment value. Among other reasons, homeowners can protest if they believe that the proposed value of their property is too high relative to the market value of comparable houses that were sold in the county, if their properties' proposed values are too high relative to the proposed values of comparable houses in the district, or if there are errors in the public records of the property (e.g., an incorrect number of bedrooms). For example, according to 2020 data for households that protested on their own, 91.87 percent of them selected the option that "the value is over market value" in their online forms. When filing a protest, homeowners can also provide an opinion of value, which is how much they believe their property was actually worth as of January 1.

Homeowners can file protests on their own. We refer to this type of protest as direct protests. Because such protests are the focus of this paper, we always refer to this type of protest unless we explicitly note otherwise. Instead of protesting on their own, homeowners can hire an agent to protest on their behalf. In exchange for representation, agents typically charge a flat fee and a percentage of the tax savings (which can be as high as 50 percent of the tax savings). Protests through agents are less relevant to our study for a few reasons. Most importantly, we mailed the letters to the owners, so it is unlikely that the information given to the owners will affect the agents' decisions. Moreover, we designed the field experiment specifically to reduce the filing frictions of protesting directly, which, if anything, should crowd out protests through agents. In addition, the timing of the setting and of our intervention makes it more difficult for protests through agents to be affected by both the type of quasiexperimental and experimental variation used in our research design. According to anecdotal accounts, households often sign contracts with agents months before the proposed values are announced. If these contracts have a flat fee, agents would have an incentive to protest mechanically, especially if their cost of protesting is low. Indeed, the decision to protest through an agent may have been made years ago, as agents offer long-term contracts to automatically protest on the owner's behalf every year. In addition, homeowners in all counties in Texas need to complete and submit a form to their appraisal district to remove an agent, which creates stickiness for those households and implies that agents often protest every year on behalf of owners.¹⁷ For these reasons, the baseline specifications focus on direct protests but, for the sake of completeness, we also report results for protests through agents.

The timing of the protest process is quite simple. Each year, the DCAD appraises the value of all homes in the county based on the market values of the properties as of January 1. The DCAD shares the proposed values with homeowners through its website and, for a fraction of households, by mailing a notice of appraised value.¹⁸

¹⁷ While there is not any publicly available data on who entered into these long-term contracts, we do find some suggestive evidence in the protest data: households that protested through an agent in a given year have a high likelihood of protesting again through an agent in the following year. For example, of the homeowners who protested through agents in 2019, 62.67 percent protested again through agents in 2020; in contrast, of the homeowners who protested directly in 2019, only 28.62 percent protested again directly in 2020.

¹⁸ Notifications are mailed to households meeting certain criteria such as increased appraised value, ownership change, loss of homestead exemption, rendered property, or new property.

Households have one month from the notification date to file a protest. The DCAD's notifications include estimated taxes, which are based on each property's proposed value. The term *estimated* is used to indicate that, technically, property tax rates are determined later in the year. Thus, to estimate the tax amount due in the notice of appraised value, the county uses the jurisdictional tax rates from the previous year. In practice, tax rate changes are rare, so the approximation error is often negligible. In any case, these estimated taxes are the relevant object of study, as they represent the subjects' expectations at their time of deciding whether to protest.¹⁹ In 2020, the DCAD presented the proposed values on May 15; as a result, the deadline to protest was June 15.

One key feature of this setting that is important for the interpretation of the results is that there is significant ambiguity in estimating market values. Because conducting full in-person appraisals is prohibitively expensive, the DCAD has to come up with its best guess for the market value of each property using statistical models and large datasets (e.g., recent home sales). The imperfections in these estimates are perhaps best illustrated by publicly available data from websites such as Zillow.com (Zillow 2020) and Redfin.com (Redfin 2020). When these companies publish estimates of the market value for the same property, their estimates tend to differ significantly, especially if that property has not been on the market recently. In fact, the challenge of estimating home values has recently caused one of these companies financial trouble (Parker and Friedman 2021). This ambiguity in market values leaves room for owners to complain about the DCAD's value assessments.²⁰ In a sense, households are not really correcting estimates that are obviously wrong. Instead, they are simply presenting a data point (e.g., the sales price of a neighboring home) to support their protest.

Based on our conversations with officers from some of the county appraisal districts in Texas, their prevailing view is that households use the subjective nature of the appraisal process not to complain about the county's estimate of their home value per se, but simply as an excuse to complain about their taxes being too high. We provide suggestive evidence in support of this view: using an independent estimate of household market value (Redfin 2020), households still file protests when their properties have been under-assessed by the government.²¹ This view is also consistent with responses to open-ended questions from households in the sample. Our survey (introduced in Section IIIB below) included an open-ended question on why the household will, or will not, protest in 2020. Most households (55.9 percent) mentioned that their taxes were too high.²² For example, a respondent wrote that the decision to protest was because their taxes were "way too high."

¹⁹ See Supplemental Appendix A.1.1 for more details. The jurisdictional tax rates for 2019 and 2020 were quite similar except for the city jurisdiction of Cockrell Hill (which decreased from 0.95 percent in 2019 to 0.85 percent in 2020) and Garland ISD (which decreased from 1.39 percent in 2019 to 1.26 percent in 2020).

²⁰ Households may have an advantage over the DCAD in that they know more about the specific attributes and condition of their own homes. On the other hand, households face a significant informational disadvantage in that they do not have access to the same models, data, and expertise available to the DCAD.

²¹ In Supplemental Appendix A.1.5 we show that protests increase with the difference between the Redfin estimate and the proposed value, but that homeowners whose proposed values are under the Redfin estimate still file protests (Redfin 2020).

²² These statistics are computed from hand-coding the answers to the open-ended question included in the survey, as a fraction of the nonmissing responses. There were a total of 1,116 respondents who mentioned that their taxes were too high.

Filing a protest directly is simple. Households can protest using either a paper form mailed by the DCAD to households that received a notification by mail, a form from the Texas comptroller that can be printed from the internet or via a simple online tool called uFile. To protest online, households need to search for their own name or address on a website, click on their account, and then follow some straightforward steps. In 2020, about 75 percent of direct protests were filed online while the remaining 25 percent were filed by mail.

Protests can be resolved at different stages. Some protests are resolved because the owners accept the settlements proposed by the county. These settlements may be offered through informal channels, such as email or phone exchange with a DCAD staff member. If no agreement is reached, the protest advances to a formal hearing with a quasijudicial entity called the Appraisal Review Board (ARB).²³ The formal hearing does not involve risk: if the DCAD schedules a hearing and the household does not attend, the protest is dismissed without penalty. Of the 2020 direct protests that contain information on the form in which it was resolved, we find that 43.7 percent were resolved informally, 35.2 percent were resolved after a formal hearing, and 21.1 percent were either withdrawn or dismissed.²⁴ After protests are resolved in one way or another, the final assessed home values (from hereon, certified values) and tax amounts are calculated. Taxes become payable on October 1, 2020, and, if unpaid, become delinquent after January 31, 2021.

Since we conducted our experiment in 2020, it is important to gauge how the COVID-19 emergency affects the external validity. The emergency did not change the procedure households needed to follow to protest their property taxes. For example, the uFile tool to protest property taxes online was in use many years prior to 2020. The emergency also did not affect the way informal settlements were offered, since they were offered by email or by phone prior to 2020. The only change in response to the emergency was that formal hearings were conducted over the phone with a single board member—indeed, the DCAD continued to offer homeowners the choice to conduct ARB hearings over the phone after 2020. Consistent with this institutional knowledge, the rates in which households protested and the success rates of those protests were similar in 2020 relative to previous years (for more details, see Supplemental Appendix A.1.5). Additionally, our survey included an open-ended question on the reasons why households choose to protest or not. Only a small minority (3.9 percent) of the respondents mentioned the pandemic as one of the reasons behind their decision on whether to protest or not.

C. Categorizing Households by Race

In order to study racial disparities in property tax protests, we need information on race for each household. Administrative data from the DCAD do not include information on race. To assign race to households, we use Ethnicolr—a race prediction

²³ Homeowners can reschedule the hearing one time without demonstrating any cause, including those that miss a scheduled hearing if the request is made within four days of the missed scheduled hearing. Formal hearings are typically conducted in person before a panel of three independent board members proposed by the DCAD and appointed by the local administrative district judge of Dallas County.

²⁴ If all else fails, the homeowner has the option to contest the decision in court.

algorithm trained on data from voter registrations—Wikipedia, and the US Census (Chintalapati, Sood, and Laohaprapanon 2018). For each individual listed as an owner of the household, this algorithm uses the first and last names and assigns a probability that the owner is White, Hispanic, Black, or Asian.²⁵ We then average the probabilities among the individual owners of the household and assign the household to the racial group with the highest probability. In Supplemental Appendix A.1.3 we show that this method of inferring race is fairly accurate, using census tract-level data from the 2020 Census as well as individual-level data provided by a private vendor.²⁶

D. Samples of Interest

To aid in the interpretation of the results, we provide some basic descriptive statistics about the households in the sample. We focus on the sample of 423,607 single-family homes that were subject to property taxes in Dallas County in 2020, after excluding some potentially problematic cases such as households with missing data.²⁷ The average home in this sample has a value of \$306,000 and pays \$6,150 annually in property taxes. About 38.6 percent of the households are categorized as White, 30.4 percent as Hispanic, 20.2 percent as Black, and the remaining 10.8 percent as Asian. For a subsample of these subjects, a private vendor provided data on individual-level characteristics such as age and political affiliation.²⁸ These data indicate that the average subject is 52 years old and we estimate that 57.2 percent of the households are likely Democrat while the remaining 42.8 percent are likely Republican. We conduct the field experiment using a subsample of the main sample that includes 78,462 households (discussed further in Section IIIC below).²⁹

E. Protest Rates

In a given year, only a small share of households file a protest. In 2020, for example, 8.40 percent of homeowners in the main sample of 423,607 single-family homes protested directly.³⁰ If we include protests through agents too, the protest rate is 16.82 percent. This rate of protests has been quite stable in recent years: for example, in the same sample of households from 2020, we find that 13.82 percent protested in 2017, 15.09 percent protested in 2018, and 13.89 percent protested in 2019. Even when looking over longer time horizons, it is still true that a minority of households file a protest: for example, in the same sample of households from

²⁵ The algorithm categorizes Middle Easterners, Indians, and Far Easterners as Asian.

²⁶ One caveat is that this method is less accurate at distinguishing Black from White individuals (often classifying Black individuals as White). As a result, our estimates are likely to underestimate the Black–White gap.

²⁷ For more details on the definition of this sample, see Supplemental Appendix A.1.4.

²⁸ The company used the names and addresses to merge the records at the individual level. For more details on these data, see Supplemental Appendix A.1.2.

²⁹ For detailed descriptive statistics across the different samples, see Supplemental Appendix Table A.1.

³⁰ In all the analyses presented in this paper, we include protests that were marked as received by the DCAD through July 15, 2020. For more details, see Supplemental Appendix A.1.2.

2020, 23.5 percent protested directly (34.5 percent overall) at least once in the 5-year period between 2015–2019.³¹

II. Expected Tax Savings

A. *Quasiexperimental Design*

We study the role of expected tax savings in the decision to file a protest. For causal identification, we leverage a quasiexperimental design based on the so-called homestead cap. In Texas, homeowners have the option to apply for homestead status for their main residence. One of the advantages of this status is that the Texas property code ensures that any rise in the assessed value of a homestead property is capped at 10 percent per year, commonly known as the homestead cap. This regulation creates a sharp kink in the expected tax savings from protesting.

For households without a homestead cap, when a protest successfully lowers the proposed value of a home, that results in a reduction in the amount of taxes owed. However, for households with homestead status, this is not always the case. If the proposed value falls below the homestead cap, any marginal reduction in the proposed value will result in a corresponding marginal reduction in the tax bill. On the other hand, when the proposed value exceeds the homestead cap, a marginal reduction in the proposed value will have no impact on the amount of taxes owed. For example, if the proposed value is \$1 above the cap, the first dollar reduction in the assessed value will not affect the tax bill, but subsequent dollar reductions will. In such a scenario, the cap has minimal significance in the expected savings from protesting. However, if the proposed value is \$15,000 above the cap, none of the initial \$15,000 reduction in the assessed value will affect the tax burden. Consequently, the cap substantially impacts the expected benefits derived from protesting. A binding cap reduces the marginal tax savings obtained through protesting.³²

B. *Graphical Analysis*

Our analysis of the effects of the homestead cap threshold on protest rates is based on the main sample of 423,607 single-family homes. About 74 percent of these households were approved for homestead status in 2020, and, thus, their homestead caps may be binding. The remaining 26 percent of households constitute the basis for a falsification test. Our baseline analysis focuses on direct protests. The reason is that the main outcome of interest for the experiment presented in Section III below is direct protests, so we provide results that are directly comparable. However, later we show that the results are robust if we include protests by agents too.

Figure 1 shows the baseline results in graphical form. This figure is a binned scatterplot of the relationship between a given outcome and the distance to the homestead cap threshold. In each panel, the horizontal axis measures the relative

³¹ For more details, see Supplemental Appendix A.1.5.

³² For a more formal discussion, Supplemental Appendix A.2 provides a stylized model.

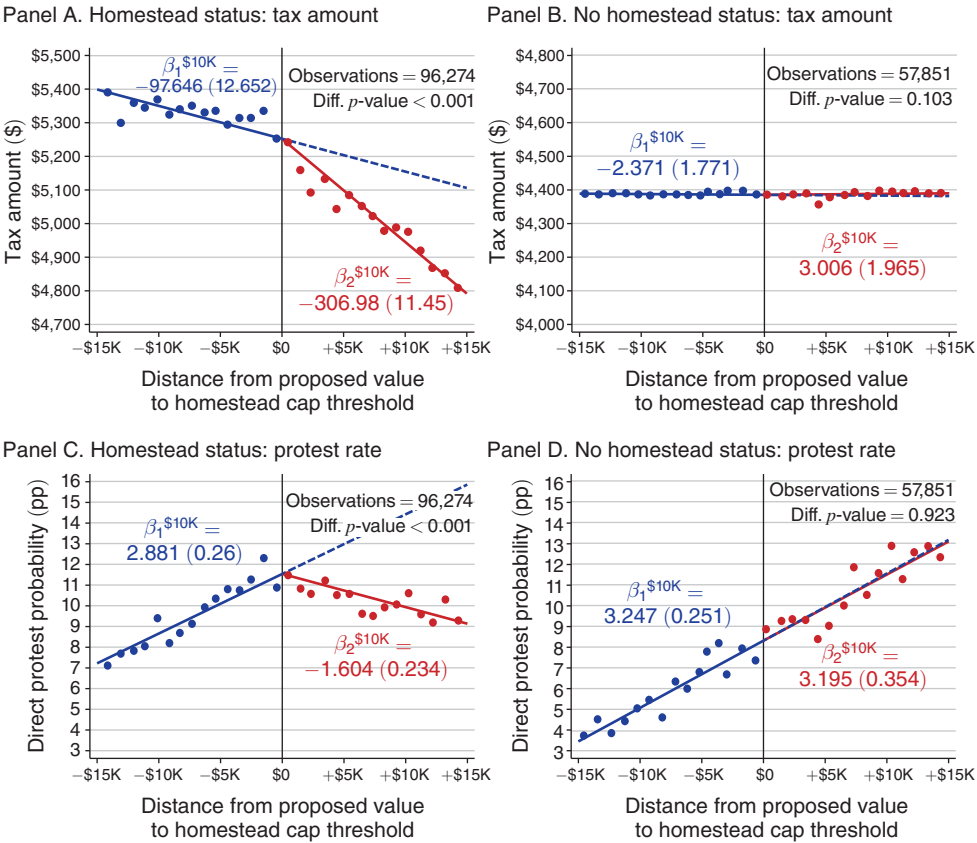


FIGURE 1. EFFECTS OF THE HOMESTEAD CAP ON THE TAX AMOUNT AND THE PROBABILITY OF PROTESTING DIRECTLY

Notes: This figure features binned scatterplots of the relationship between a given outcome (indicated on the y-axis of each panel) and the distance between the 2020 proposed value and the 2020 potential homestead cap threshold (defined as 110 percent of the appraised value in the previous year). All regressions control for the proposed value, a dummy for whether the household protested in the previous year, and a set of school district dummies. The sample is restricted to properties for which the proposed value is within \$15,000 of the potential homestead threshold. For ease of exposition, we normalize all coefficients so that they correspond to the effects from a \$10,000 increase in the proposed value. The lines correspond to linear regressions, with normalized slopes reported next to them along with robust standard errors (in parentheses) and the number of households (in brackets in the top right corner). The panels on the left half (A and C) correspond to households with 2020 homestead status, while the panels on the right half (B and D) correspond to households without 2020 homestead status. The dependent variables are as follows: $Tax\ amount$ is the estimated tax amount based on 2020 proposed values, and P_{2020} is an indicator variable that takes the value 100 if the household protested directly in 2020 and 0 otherwise.

difference between the home’s proposed value and the homestead cap threshold. To be conservative, the baseline results are based on a narrow band around the threshold ($\pm \$15,000$).³³ The blue dots correspond to households with proposed values below the homestead cap threshold, with the blue line corresponding to the linear fit (and the slope of this line in blue as well). For ease of exposition, we normalize

³³ In Supplemental Appendix A.3, we show that the results are robust if we use less conservative bands.

all coefficients so that they correspond to the effects from a \$10,000 increase in the proposed value. In turn, the red dots and red lines correspond to the households with proposed values above the homestead cap threshold. The results from Figure 1 include a set of basic characteristics of the household as control variables: the proposed value, an indicator variable for whether the household protested in the previous year, and a set of indicator variables for school district.

The two panels on the left of Figure 1 (panel A and panel C) correspond to the properties with homestead status, for which the homestead cap threshold can be binding. The two panels on the right of Figure 1 (panel B and panel D) correspond to properties without homestead status, for which the homestead cap threshold should be irrelevant, and thus can serve as a falsification test.

We start with Figure 1, panel A in which the outcome variable measured on the vertical axis corresponds to the tax amount based on the 2020 proposed value.³⁴ The blue slope in the left half of the figure corresponds to properties right below the homestead cap threshold, and the red slope in the right half of the figure corresponds to the properties right above the threshold.³⁵ Note that the slope on each side represents just an association and, thus, only the difference between the two slopes can be interpreted as a causal effect. As expected, there is a sharp kink at the threshold: upon reaching the homestead cap threshold, households pay a lower tax amount than they would have without the homestead cap. This kink is large in magnitude and statistically significant. Therefore, we reject the null hypothesis that the slope to the left of the threshold (-97) is equal to the slope to the right of the threshold (-306), with a p -value < 0.001 .

The following thought experiment illustrates how the homestead cap threshold affects the marginal benefits from protesting. Consider a household with a proposed value that is \$10,000 above the homestead cap threshold—that is, at $+\$10,000$ on the x -axis of Figure 1, panel A. The vertical gap between the red line and the dashed blue line, estimated at around \$209 ($= -306 - (-97)$), corresponds to the tax amount that is capped.³⁶ What happens when a successful protest decreases the proposed value by \$10,000? In the presence of the homestead cap, the \$10,000 reduction in home value would not affect the tax amount. That is, in the presence of the homestead cap, the household would not benefit from the \$209 in tax savings.

In sum, for a household at $+\$10,000$ on the x -axis of Figure 1, panel A, the marginal benefit from protesting decreases by \$209. If households care about the expected tax savings from protesting, we expect the \$209 reduction in the marginal benefit from protesting to reduce the probability of protesting. To explore this expectation, Figure 1, panel C is identical to Figure 1, panel A except that instead of the tax amount, the vertical axis corresponds to the protest rate (i.e., an indicator variable that takes the value 100 if the owner protested directly in 2020

³⁴ For an alternative view, Supplemental Appendix A.3.2 reproduces the whole exercise but using the tax rate as a dependent variable instead of the tax amount.

³⁵ As a robustness check, Supplemental Appendix A.3.5 shows that there is no bunching at the homestead cap.

³⁶ The red regression line predicts that when the proposed value is \$10,000 above the homestead cap, the tax amount is \$306 lower. The dashed blue line predicts that when the proposed value is \$10,000 above the homestead cap, the tax amount is \$97 lower. The difference between these two figures is \$209 ($\$306 - \97).

and 0 otherwise). As expected, we find a sharp kink in Figure 1, panel C exactly at the homestead cap threshold. Again, this kink is large in magnitude and statistically significant. We can reject the null hypothesis that the coefficient to the left of the threshold (2.881) is equal to the coefficient to the right of the threshold (-1.604), with a p -value < 0.001 .

We can combine the results from Figure 1, panel A and Figure 1, panel C to quantify the magnitude of the effects of the homestead cap. Figure 1, panel A indicates that an additional \$10,000 above the homestead cap threshold causes an average \$209 reduction in the tax amount. In turn, Figure 1, panel C indicates that being \$10,000 above the homestead cap threshold causes a reduction in the protest probability of 4.485 percentage points. Taking the ratio of these two estimates, we conclude that for each \$100 reduction in the tax amount, the protest probability decreases by 2.14 percentage points. In other words, the decision to protest is responsive to the expected tax savings from protesting. In Section III, we will use this estimate to calculate a money metric for the filing frictions.

Falsification Tests.—The previous analysis corresponds to properties that have a homestead status exemption (and, thus, are subject to the homestead cap). Next, we reproduce the analysis using properties that do not have a homestead status exemption (and, thus, are not subject to the homestead cap). This analysis provides a sharp falsification test because we should not observe any kinks in this latter group. A kink would suggest that the effects are not due to the homestead cap threshold but are instead due to some other confounding factors. The two panels in the right half of Figure 1 (i.e., panel B and panel D) correspond to the properties without homestead status for which the hypothetical homestead cap threshold is defined as 110 percent of the assessed value in the previous year (2019). As expected, on the panels on the right side of Figure 1, we find no kinks at the homestead cap threshold; therefore, we cannot reject the null hypothesis that the coefficients are equal below and above the homestead cap threshold. Most importantly, the coefficients are precisely estimated in the right-hand panels of Figure 1, meaning that we can rule out large kinks as those shown in the left-side panel of Figure 1, as well as small kinks.

For a second falsification test, we follow the logic of an event-study analysis. We reproduce the analysis from Figure 1, panel C for the properties with homestead status, but the dependent variable is whether the household protested in 2019 instead of 2020. Whether the 2020 proposed value ends up above or below the 2020 homestead cap threshold should not affect whether a household protested in 2019. In fact, a significant effect of a cap in 2020 on the protest probability in prior years would suggest a confounding factor affecting the results from Figure 1, panel C. Moreover, we can extend this logic and reproduce the analysis for one year prior (2019) and for each year for which we have data (2015–2018). Figure 2, panel A presents the results from this event-study analysis. The rightmost coefficient (year 2020) corresponds to the effect of the homestead cap threshold on the protest rate (i.e., the difference between the two slopes reported in Figure 1, panel C). The rest of the coefficients are estimated with an identical regression, except for using a different year. For example, the 2015 coefficient corresponds to a regression in

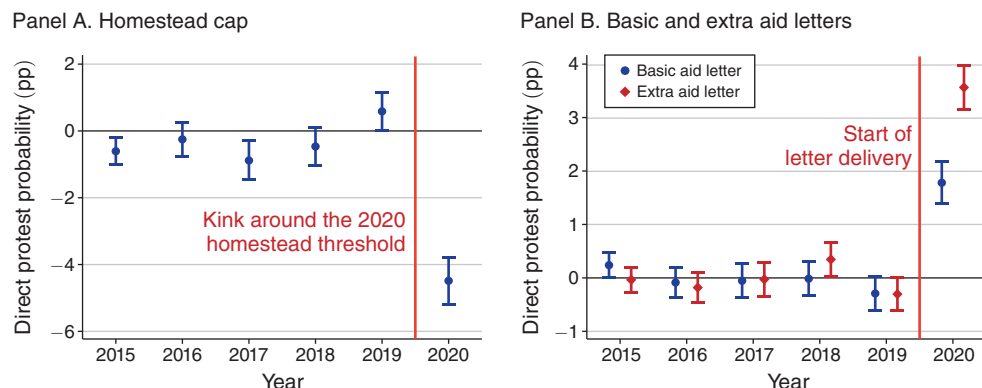


FIGURE 2. EVENT-STUDY FALSIFICATION TESTS

Notes: Point estimates with 90 percent confidence intervals are in brackets, based on robust standard errors. Panel A: The blue dots represent the difference between the slopes before and after the threshold as in Figure 1, panel C, but varying the year. Results are based on single-family homes with 2020 homestead status and an absolute difference between the proposed value and the potential homestead cap of less than \$15,000. Panel B: The blue dots represent the effects of the basic aid letter (relative to the no letter group), while the red diamonds represent the effects of the extra aid letter. In panel A, point estimates are calculated the same way as in Figure 1. For ease of exposition, we normalize all coefficients so that they correspond to the effects from a \$10,000 increase in the proposed value. In panel B, point estimates are computed the same way as in column 1 of Table 2 for the effect of the letters on whether households protest directly in 2020. In panels A and B, the dependent variable in each regression is whether households protested directly in the year shown.

which the dependent variable takes the value 100 if the household protested in 2015 and 0 otherwise.³⁷ As expected, the coefficients are consistently close to zero for each falsification year (2015–2019) and always highly statistically different from the 2020 coefficient.³⁸

C. Regression Analysis

Next, we present robustness checks for the findings presented above. Reproducing the whole graphical analysis from Section IIB for the different specifications and dependent variables is not practical. To be concise, we can summarize the results in a single parameter, as explained below. Consider the sample of households with homestead status. Let Y_i be the outcome of interest, such as an indicator variable that takes the value 100 if household i protested directly in 2020. Let us define A_i as property i 's proposed value and $\bar{A}_i$ as its potential homestead cap threshold (i.e., 110 percent of the appraised value from the previous year).

³⁷ Note that since some control variables are lagged outcomes, the set of control variables will not be identical across all outcomes. For example, when the dependent variable is the 2020 protest choice, we control for whether the household protested in 2019. However, when the dependent variable is the 2019 protest choice, we cannot control for whether the household protested in 2019. Also, note that some households did not protest between 2015–2018 simply because they did not own the property. We include protest history controls back until 2015, because at the time we ran the field experiment, the DCAD provided protest data on its website from 2015 onward.

³⁸ Two of the falsification coefficients (for years 2015 and 2017) are borderline statistically significant but still small in magnitude.

TABLE 1—EFFECTS OF THE HOMESTEAD CAP ON THE TAX AMOUNT AND PROBABILITY OF PROTESTING

	T_{2020}	P_{2020}^d	P_{2020}^{agent}	P_{2020}^{all}	P_{2020}^{online}	P_{2020}^{mail}	P_{2020}^{won}	ΔMV_{2020}^d	ΔT_{2020}^d
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\mathbf{1}\{A_i > \bar{A}_i\} \cdot (A_i - \bar{A}_i)$	−209.334 (21.069)	−4.486 (0.443)	−1.120 (0.350)	−5.605 (0.529)	−3.639 (0.404)	−0.847 (0.207)	−3.577 (0.388)	−0.214 (0.030)	−0.462 (0.031)
Mean outcome	5,153.25	9.98	5.93	15.91	8.04	1.94	7.33	0.47	0.42
SD outcome	3,796.84	29.97	23.62	36.58	27.19	13.79	26.06	1.98	1.97
Observations	96,274	96,274	96,274	96,274	96,274	96,274	96,274	96,274	96,274

Notes: Robust standard errors are in parentheses. Each column presents results from a regression that follows the specification presented in equation (1) from Section IIC. The sample is restricted to properties for which the proposed value is within \$15,000 of the potential homestead threshold. For ease of exposition, we normalize all coefficients so that they correspond to the effects from a \$10,000 increase in the proposed value. The effect on the variable of interest, $\mathbf{1}\{A_i > \bar{A}_i\} \cdot (A_i - \bar{A}_i)$, represents the change in the slope before and after the homestead cap threshold in Figure 1. All regressions control for the proposed value, a dummy for whether the household protested in the previous year, and a set of school district dummies, as in Figure 1. The dependent variables are defined as follows: T_{2020} represents the tax amount in dollars; P_{2020}^d is an indicator variable that takes the value 100 if the owner filed a direct protest in 2020 and 0 otherwise; P_{2020}^{agent} indicates a protest through an agent in 2020; P_{2020}^{all} indicates any type of protest (direct or agent); P_{2020}^{online} indicates the household filed a direct protest online; P_{2020}^{mail} indicates if the household filed a direct protest by mail; P_{2020}^{won} indicates if a direct protest resulted in a reduction in the market value; ΔMV_{2020}^d is the percentage reduction in the market value due to protesting, which by construction takes the value 0 if the household did not protest or if the protest was unsuccessful; and ΔT_{2020}^d is the estimated percentage reduction in the tax amount due to protesting.

The relevant regression of interest is:

(1)
$$Y_i = \delta_0 + \delta_1 \cdot \mathbf{1}\{A_i > \bar{A}_i\} \cdot (A_i - \bar{A}_i) + \delta_2 \cdot (A_i - \bar{A}_i) + \mathbf{X}_i'\delta_{\mathbf{X}} + \chi_i.$$

$\mathbf{X}_i$ stands for the same set of control variables that we used in the graphical analysis to improve power: the proposed value, an indicator variable for whether the household protested in the previous year, and a set of school district dummies. The coefficient of interest is δ_1 , which corresponds to the difference in slopes between the red and blue lines from Figure 1. For ease of exposition, we normalize the difference between the variables A_i and $\bar{A}_i$ to tens of thousands of dollars (\$10,000s). Therefore, δ_1 corresponds to the effect of an increase in \$10,000.

The results are reported in Table 1. Columns 1 and 2 simply reproduce the main results presented in Figure 1, panel A and panel C. The dependent variable in column 1 corresponds to the tax amount. The coefficient indicates that the homestead cap threshold reduces the tax amount by \$209.3 for households that are \$10,000 above the threshold. This coefficient is identical to the difference between the red and blue slopes from Figure 1, panel A. The dependent variable in column 2 corresponds to an indicator variable that takes the value 100 if the household protested directly in 2020. The coefficient indicates that being \$10,000 above the homestead cap threshold causes a reduction of 4.486 percentage points in the direct protest probability—this coefficient is identical to the difference between the red and blue slopes from Figure 1, panel C.

In addition to direct protests, we can look at the effects of the homestead cap threshold on protests through agents. As discussed in Section I above, the timing of protesting through agents is quite different. It is possible that those who protest

through agents have signed their contract way before May 15, when the proposed values were announced. As a result, it would be less likely for the homestead cap threshold to affect the decision of those households because their decision to protest cannot be reversed or because their private costs are sunk. However, it is still possible that the homestead cap threshold could dissuade some households from signing up with an agent at the last moment. The results are presented in column 3, which is comparable to column 2, except that the dependent variable indicates whether the household protested through an agent (instead of indicating whether the household protested directly). The effects on agent protests are qualitatively consistent with the results of direct protests; however, the effects are quantitatively smaller for protests through agents, as expected for the reasons discussed before. In turn, the dependent variable from column 4 combines direct protests and protests through an agent. Given the small effects on protests through agents, the effects on total protests (column 4) are similar in magnitude to the effects on direct protests (column 2).

Homeowners can file a direct protest online using a tool called uFile, or they can protest by mail. Column 5 uses an indicator variable for direct online protests as the dependent variable, while column 6 has an indicator for direct protest by mail. Since most protests are filed online, it should not be surprising that the effects are stronger for the online protests than for the protests by mail.

Columns 7–9 of Table 1 provide evidence that the homestead cap threshold is consequential not only for the number of households who choose to protest, but also for the subsequent market values and tax amounts of the households. Column 7 examines the extensive margin—that is, the dependent variable is an indicator that takes the value 100 if the household received a reduction in market value from a direct protest and 0 if the protest was not successful or if the household did not protest directly to begin with. The coefficient from column 7 is large (-3.577) and statistically significant ($p\text{-value} < 0.001$), indicating that some of the protests dissuaded by the homestead cap threshold would have been successful. Columns 8 and 9 use alternative measures of the success of direct protests that take into account the intensive margin too (i.e., not only whether the home market value was revised downward, but also by how much). In column 8, the dependent variable is the percent reduction in the market value as a result of a direct protest—that is, it takes the value 0 if the protest was not successful or if the household did not protest, and it takes the value 10 if the protest was successful and resulted in a reduction of the market value of 10 percent. In column 9, the dependent variable is equal to the percent reduction in the estimated taxes due to a successful direct protest. The coefficients from columns 8 and 9 suggest that the effects are highly significant ($p\text{-value} < 0.001$) when considering these alternative measures of protest success.³⁹

³⁹For estimated taxes (column 9), part of the effect is mechanical: as the proposed value goes above the kink, the homestead cap becomes binding and, thus, the marginal effect of proposed values on estimated taxes goes down.

III. Filing Frictions

A. Conceptual Framework

There are no fees for filing a property tax protest in Texas. However, we hypothesize that households may face nonpecuniary costs, which we call filing frictions. These frictions include traditional hassle costs from tax filing, such as the opportunity cost of time and the unpleasant nature of doing paperwork (Benzarti 2020; Sunstein 2021a; Benzarti 2021). Additionally, our definition of filing frictions includes information frictions. That is, some households may not protest because they do not know how to protest or because they think that filing a protest is a lot harder than it really is.

Although filing a protest directly is easy in theory, for some households it may not be easy in practice. Some households may not even know where to start or may think that the process is more difficult than it actually is. Other households may be less sophisticated and, thus, need step-by-step guidance on how to protest. In fact, instructions on how to protest are not readily available. At the time of the experiment, only one official source online had instructions on how to file a protest: a pdf document posted on the DCAD website.⁴⁰ However, this long document includes broad instructions and is difficult to locate on the DCAD website. Other unofficial online sources, such as blog posts, were usually incomplete, outdated, and difficult to find. Moreover, those sources often had commercial interest, deliberately depicting the protest process as more complicated than it really is.

The filing frictions may be particularly large for a specific step in the protest procedure: providing an opinion on the value of the home and an argument supporting that opinion. This information can be obtained in different ways and usually involves finding a comparable property that recently sold for less than the proposed value of the protesting property. The recent sale price serves as the opinion of value, and information about the recent transaction, including the details of the comparable property, can be used as an argument. Finding a proper comparison property entails several steps. First, the homeowner could use a free online real estate service, such as Zillow (2020) or Redfin (2020), to search for and identify comparable properties that recently sold for less than the homeowner's proposed property value. This seemingly straightforward process could be daunting for people with limited internet access and skills or low financial literacy. Indeed, there is much evidence that households have trouble finding even easily accessible information, such as information on inflation or home prices (Cavallo, Cruces, and Perez-Truglia 2017; Bottan and Perez-Truglia, forthcoming).

B. Experimental Design

We designed a mailing intervention aimed at reducing filing friction. Subjects in our sample were randomly assigned to receive a letter or no letter. Figures 3 and 4

⁴⁰This document can be found at http://www.dallascad.org/Forms/Protest_Process.pdf.

show the first and second pages of a sample letter, with the addition of a red box that highlights the extra aid message that was randomized.⁴¹ The letters were sent on behalf of researchers at the University of Texas at Dallas and included several measures to show that they came from a legitimate source.⁴² The letters were personalized to recipients: each salutation at the top of the first page included the recipient's name, and their names and addresses were printed at the bottom of the second page (which appeared through the envelope window).⁴³

Subjects were randomized to receive no letter or one of two types of letters: the basic aid letter or the extra aid letter. The basic aid letter provided useful tips to help the recipient file a protest, all of which were printed on the first page of the letter (see Figure 3 for a sample). A key part of the first page is that it included instructions on how to file a protest using the project's website, which we designed to be concise, easy to follow, and as explicit as possible. Supplemental Appendix D shows screenshots of the entire website, including step-by-step instructions on how to file a protest online or by mail. These walkthroughs included hyperlinks to relevant websites and screenshots of a sample protest using information from a fictitious household for added clarity.

The extra aid letter is identical to the basic aid letter, plus additional instructions on how to protest. Figure 4, panel A shows an example of the second page of the letter for a recipient assigned to the basic aid treatment, and Figure 4, panel B shows an example of the second page for a recipient assigned to the extra aid treatment, with the extra aid message outlined by a red box with dashed lines (we added this box for expositional purposes and did not include it in the actual letters sent to subjects). The extra aid message provided additional information related to the opinion of value and supporting argument.

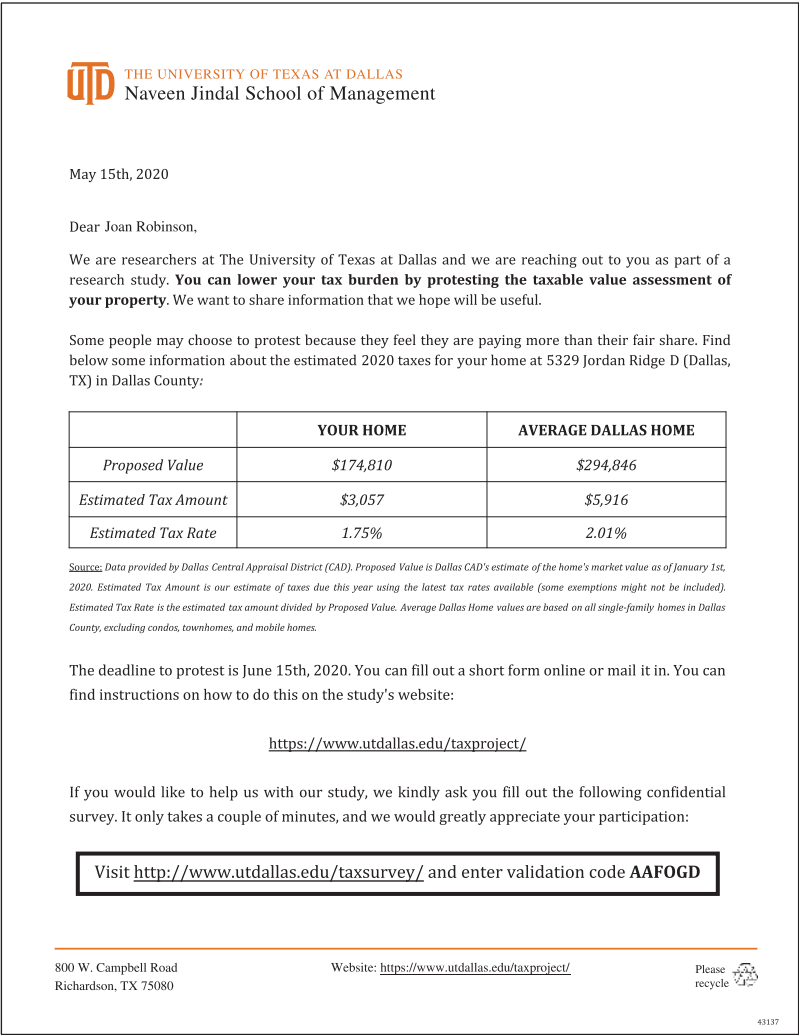
The first paragraph of the extra aid message offered some facts about the protest filing process, explaining that the simple process could be done without an agent and may not require a hearing (which could be intimidating to some subjects) for the DCAD to propose a settlement offer. Moreover, if a hearing were to be scheduled, there would be no risk for not attending. The message also provided an argument to be used in the protest. Specifically, we presented the most common approach—that is, arguing that based on a recent sale price of a comparable property, the proposed value for the property exceeds the market value.⁴⁴ To further simplify the use of this information, we presented this information as it would look on the actual protest form, with a check mark in the “Value is over market value” box, the comparable sale price in the “Opinion of value” field, and a sample handwritten note outlining

⁴¹ See Supplemental Appendix C for a full-page sample of the letter without the red box added.

⁴² The envelope featured the logo of the University of Texas at Dallas, a well-known institution in Dallas County; the name of one of the professors from that university; and nonprofit organization postage. See Supplemental Appendix B for a sample. The letter also featured the official logo of the University of Texas at Dallas in the header, as well as a physical address and the URL of the study's website. The website provided general information about the study (without discussing any hypotheses or what the study was about) and contact information for the researchers and the institutional review board.

⁴³ In cases where properties were jointly owned by multiple individuals (typically, husband and wife), we sent a single letter addressed to all listed owners.

⁴⁴ We identified one comparable property for all households in the subject pool, but we included this information only for recipients who were randomly selected for the extra aid letter.



Panel A. Extra aid message: no

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

Thank you for your attention!

Alejandro Zentner
Associate Professor
University of Texas at Dallas

40327
DOLORES M MORENO
5329 JORDAN RIDGE DR
DALLAS, TX 75236-1895
██████████-██████████-██████████-██████████

Panel B. Extra aid message: yes

If you'd like to file a protest, it is really simple. You do not need an agent. 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.

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. In that case, you could use the following:

☒ Value is over market value Opinion of value: \$760,000

And remember to attach a separate page (or file, if protesting online) with your argument:

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 5148 Ruegan Rd (Dallas, TX) is 0.29 miles away from my home, and has the same number of bedrooms and a similar square footage. That property was sold on 10/31/2019 for \$760,000.

You can find information about this sale by searching for the property's address on Zillow.com or Redfin.com. On these websites you can find other comparable properties to support your protest. You can also protest based on the appraised market values of comparable properties, which can be found on www.dallascad.org/SearchAddr.aspx.

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

Thank you for your attention!

Alejandro Zentner
Associate Professor
University of Texas at Dallas

40327
DOLORES M MORENO
5329 JORDAN RIDGE DR
DALLAS, TX 75236-1895
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FIGURE 4. SECOND PAGE OF THE SAMPLE LETTER

Notes: A sample of the second page of the letter used in the field experiment. Panel A does not contain the extra aid message, while panel B does in the section framed with the red dashed lines. This red frame in panel B was added to this figure for emphasis but does not appear in the actual letters.

On these websites you can find other comparable properties to support your protest.” Additionally, we mentioned that subjects could protest based on different arguments, offering the following message: “You can also protest based on the appraised market values of comparable properties, which can be found on www.dallascad.org/SearchAddr.aspx.”

We created an algorithm that identified one comparison property for each household by combining data from the tax rolls with data from recent property sales from Redfin (2020). For each recipient, the algorithm searched similar properties (e.g., same number of bedrooms, bathrooms, square footage, location) that were sold in late 2019 or early 2020 for between 5 percent and 20 percent less than the proposed value of the recipient's property.⁴⁵

Our letters were not designed to fully eliminate filing frictions, as recipients would still need to spend time filing and monitoring their protests, which has an opportunity cost. Some may also find the associated paperwork considerably unpleasant (Benzarti 2020, 2021). In that sense, our estimates provide a lower bound of the full magnitude of the filing frictions.

⁴⁵ In Supplemental Appendix A.4, we provide details about this algorithm and some descriptive statistics.

In the middle of the first page, all letters included a table with information related to the proposed values and estimated taxes of the recipient's property.⁴⁶ At the bottom of the first page, all letters included a URL to an online survey. To verify that recipients were legitimate subjects and to link survey responses at the household level, we included a unique five-letter code for survey access. The survey also provides a proxy for the dates that recipients opened the letters (Perez-Truglia and Cruces 2017; Bottan and Perez-Truglia, forthcoming).

C. Subject Pool and Implementation Details

From the main sample of 423,607 residential single-family properties, we focused on a subgroup of 78,462 homes for our field experiment subject pool. To arrive at this subsample, we excluded households that had already filed a protest before receiving our letters according to the latest available data from the DCAD, because our letter could not affect their behavior.⁴⁷ The most important condition was to focus on households for which our algorithm could find comparison properties for use in the extra aid message.⁴⁸ Though not identical, the subject pool is similar to the main sample in observable, pretreatment characteristics.⁴⁹ Additionally, Supplemental Appendix A.4 shows that, consistent with successful random assignment, the observable pretreatment characteristics are balanced across all treatment groups.

We timed the intervention so that our letters would arrive early enough before the protest deadline to influence the recipient's decision. We created the letters as soon as the administrative data, including 2020 proposed values, became available (on May 16, 2020). 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 May 20 and estimated that most would be delivered in the next couple of days. Consistent with this projection, we began to receive survey responses and website visits on May 21. Furthermore, we confirm that the post office scanned more than 90 percent of our letters by Friday, May 22, 2020, indicating that they reached the last mile before delivery. Based on data from previous years, most subjects file protests close to the deadline, which in 2020 was June 15. Therefore, we are confident that there was enough time between receipt of the letter and the protest deadline for the letter to influence most recipients' decision to protest. In fact, this view is consistent with the dates when the subjects responded to the survey and when they visited the website included in the letter.⁵⁰

⁴⁶The letters also include the values for the average Dallas home which we use for a second treatment arm related to fairness considerations discussed in a separate article (Nathan, Perez-Truglia, and Zentner 2024).

⁴⁷We initially selected a sample of 79,322 properties. However, due to a lag of a few days in the way the DCAD reports data, we dropped 860 from the subject pool because we discovered that they had already protested by the time we mailed the letters.

⁴⁸For more details, see Supplemental Appendix A.1.2.

⁴⁹See Supplemental Appendix A.1.4.

⁵⁰Results reported in Supplemental Appendix A.5.1.

D. Regression Model

We want to compare the probability of protesting between subjects who were sent a letter and subjects who were not sent a letter, as well as between subjects assigned to the different types of letters. Let Y_i be the outcome of interest, such as an indicator variable that takes the value 100 if household i protested directly in 2020 (the posttreatment period). We use a simple linear probability model:

$$(2) \quad Y_i = \eta_0 + \eta_{basic} \cdot L_i^{basic} + \eta_{extra} \cdot L_i^{extra} + (\mathbf{X}_i^{pre})' \boldsymbol{\eta}_X + \epsilon_i.$$

The variable, L_i^{basic} is an indicator that takes the value 1 if the household was mailed a basic aid letter and 0 otherwise. Likewise, L_i^{extra} is an indicator that takes the value 1 if the household was mailed an extra aid letter. Finally, $\mathbf{X}_i^{pre}$ is a vector of pretreatment controls. Unless noted otherwise, we use this exact same set of control variables in all regressions in the paper: the proposed value in levels and its annual growth; dummies for multiple owners, school, and special districts; number of years since the last property tax protest; an indicator variable for homestead status; growth in the proposed value relative to the previous year and for each year from 2015 to the previous year; and dummies indicating whether the household protested in each year and the outcome of the protest. Given that this is an experiment, the only goal of using pretreatment controls is to gain statistical power by reducing the variance of the error term (McKenzie 2012). Additionally, we use the pretreatment data to construct falsification tests.

E. Results

Table 2 presents the regression results. All regressions are based on the same specification given in equation (2), but they differ by the dependent variable. The dependent variable in Table 2, column 1 takes the value 100 if the owner protested directly and 0 otherwise. The basic aid letter increased the probability of protesting by 1.792 percentage points, an effect that is highly statistically significant (p -value < 0.001). The extra aid letter increased the protest probability even more, by 3.509 percentage points (p -value < 0.001).

Column 2 of Table 2 presents a falsification test using protests in 2019. Since the letters were sent in 2020, they could not possibly affect the decision to protest in 2019. As expected, the coefficients from column 2 are close to zero (-0.286 and -0.288) and statistically insignificant. Based on this same logic, Figure 2, panel B provides an event-study analysis. The rightmost coefficients show the effects of each type of letter on the probability of protesting in 2020—that is, identical to the two coefficients reported in column 1 of Table 2. The rest of the coefficients correspond to the same regression specification, but the dependent variables are protest indicators for the years 2015–2019, instead of 2020. As expected, the coefficients for each of the pretreatment years are close to zero, statistically insignificant, and precisely estimated.

Column 3 of Table 2 is the same as column 1 except that we use as the dependent variable an indicator variable that takes the value 100 if the owner protested using

TABLE 2—EFFECTS OF THE TWO TYPES OF LETTERS ON THE PROBABILITY OF PROTESTING

	P_{2020}^d	P_{2019}^d	P_{2020}^{agent}	P_{2020}^{all}	P_{2020}^{online}	P_{2020}^{mail}
	(1)	(2)	(3)	(4)	(5)	(6)
Basic aid letter ⁽ⁱ⁾	1.792	−0.286	0.030	1.822	1.591	0.201
	(0.249)	(0.199)	(0.181)	(0.283)	(0.230)	(0.111)
Extra aid letter ⁽ⁱⁱ⁾	3.509	−0.288	−0.122	3.387	3.295	0.215
	(0.258)	(0.198)	(0.179)	(0.288)	(0.240)	(0.111)
<i>p</i> -value (i) = (ii)	<0.001	0.995	0.409	<0.001	<0.001	0.907
Subsample						
Mean outcome (no letter)	8.67	6.14	6.07	14.74	7.09	1.57
SD outcome (no letter)	28.14	24.00	23.89	35.45	25.67	12.45
Observations	78,462	78,462	78,462	78,462	78,462	78,462
	P_{2020}^{won}	ΔMV_{2020}^d	ΔT_{2020}^d	P_{2020}^d	P_{2020}^d	SO_{2020}
	(7)	(8)	(9)	(10)	(11)	(12)
Basic aid letter ⁽ⁱ⁾	1.213	0.071	0.035	1.449	1.935	0.795
	(0.222)	(0.019)	(0.016)	(0.347)	(0.339)	(0.719)
Extra aid letter ⁽ⁱⁱ⁾	2.621	0.145	0.090	3.108	3.745	15.287
	(0.231)	(0.020)	(0.016)	(0.364)	(0.350)	(0.979)
<i>p</i> -value (i) = (ii)	<0.001	<0.001	0.001	<0.001	<0.001	<0.001
Subsample				I	II	
Mean outcome (no letter)	6.76	0.48	0.33	6.03	10.33	3.37
SD outcome (no letter)	25.10	2.24	1.86	23.80	30.43	18.05
Observations	78,462	78,462	78,462	30,356	48,106	5,026

Notes: Robust standard errors are in parentheses. Each column presents results from a different regression with two main independent variables: *Basic aid letter* is an indicator variable that takes the value 1 if the subject was randomly chosen to receive a basic aid letter and *Extra aid letter* is an indicator variable that takes the value 1 if the subject was randomly chosen to receive an extra aid letter. The omitted category is comprised by subjects who were randomly chosen not to receive a letter. The regressions in this table include the following controls: the proposed value in levels and its annual growth; dummies for multiple owners, school, and special districts; number of years since the last protest; a dummy for homestead status; and for each year since 2015, a dummy indicating if the household protested in that year and the outcome of the protest (if any) as a percent-reduction in the market value. The dependent variables are defined as follows: P_{2020}^d is an indicator variable that takes the value 100 if the owner filed a direct protest in 2020 and 0 otherwise; P_{2019}^d is an indicator variable that takes the value 100 if the owner filed a direct protest in 2019 and 0 otherwise; P_{2020}^{agent} indicates a protest through an agent in 2020; P_{2020}^{all} indicates any type of protest (direct or agent); P_{2020}^{online} indicates the household filed a direct protest online; P_{2020}^{mail} indicates if the household filed a direct protest by mail; P_{2020}^{won} indicates if a direct protest resulted in a reduction in the market value; ΔMV_{2020}^d is the percentage reduction in the market value due to protesting, which by construction takes the value 0 if the household did not protest or if the protest was unsuccessful; ΔT_{2020}^d is the estimated percentage reduction in the tax amount due to protesting; and SO_{2020} is the suggested opinion defined for the subsample that protested directly online and provided an opinion of value, and it takes the value 100 if the subject provided an opinion of value within half a percentage point of the value we selected for their extra aid message. Column 10 corresponds to the sample that were not mailed an official notification from the DCAD. Column 11 corresponds to the sample that were mailed such a notification.

an agent and 0 otherwise. This specification can shed light on whether the letters prompted homeowners to hire or fire their agents. The results show that our letters did not have an effect, positive or negative, on the protests through agents. This result suggests that our letters did not crowd out the protests through agents. This finding is consistent with anecdotal accounts that households often sign long-term contracts with agents before the proposed values are announced. So, by the time they received our letters, they may want to save some money by protesting directly, but it is probably too late for them to fire their agents. Column 4 uses a dependent

variable that includes direct protests and protest through agents. The coefficients are similar to the coefficients from column 1 (by construction, the sum of the coefficients in columns 1 and 3 equals the coefficient in column 4).

Column 5 examines whether our website played an important role in the effects of the letters. Of all households that visited the walkthroughs on our website, 94.8 percent looked at the online walkthrough and the rest looked at the mail walkthrough. As a result, we would expect that the effects of our letters acted primarily through online protests. To test this hypothesis, in column 5, the dependent variable is an indicator that takes the value 100 if the household protested directly online and 0 if the household protested by mail or did not protest at all. The evidence suggests that the vast majority of the effects operated through online protests—that is, the coefficient on the basic aid letter from column 5, 1.591, is 88.7 percent as large as the corresponding coefficient from column 1, 1.792. In other words, and consistent with the data on the visits to the website, we find that a strong majority (88.7 percent) of the additional protests induced by our letter were conducted online. Column 6, which uses direct protests by mail in 2020 as the dependent variable, is consistent with the rest of the results presented here. As expected, the sum of the estimates in columns 5 and 6 is equal to the estimates in column 1, which combines online protests and protests by mail.

Columns 7–9 of Table 2 provide evidence that the letters were consequential not only for the number of households who chose to protest but also on their subsequent assessed home values and estimated taxes. In column 7 of Table 2, the dependent variable takes the value 100 if the household protested directly and received a discount on their market value assessment and 0 otherwise. The coefficients remain economically and statistically significant. The ratio between the coefficients on the basic aid and extra aid letters in columns 7 and 1 suggests that 67.6 percent ($= 1.213/1.792$) and 75 percent ($= 2.621/3.509$) of the protests that were induced by these letters, respectively, were successful. This success rate is comparable to the corresponding success rate of 78 percent ($= 6.76/8.67$) observed in the control group.⁵¹ In other words, the additional protests induced by our letters were, on average, roughly as successful as the protests in the control group. In columns 8 and 9, we use alternative measures of the success of direct protests that take into account the intensive margin too (e.g., not only whether the market value was revised down, but by how much). In column 8, the dependent variable is the percent reduction in the market value as a result of a direct protest. In column 9 the dependent variable is equal to the percent reduction in the estimated taxes due to direct protests. The results from columns 8 and 9 are consistent with the results from column 7.

F. Causal Mechanisms of the Effects of the Aid Messages

In this section, we examine evidence related to the causal mechanisms underlying the effects of aid messages. Our favorite interpretation is that the letters

⁵¹ This success rate is based on the ratio of the share of direct protests that were successful (6.76, from column 7) to the share of households that protested directly (8.67, from column 1).

increased the likelihood of protesting by mitigating the underlying filing frictions. An alternative interpretation could be that our letters reduced filing frictions by acting as a reminder of the opportunity to protest. However, this explanation is unlikely in our context, as proposed property taxes are quite salient around the time subjects received our letter. Moreover, we have several pieces of evidence that indicate that the reminder effect was probably minor.

The first and most direct piece of evidence is based on a comparison between the two types of letter. If the letters had acted as a simple reminder, then their effects should have been the same regardless of whether the letter included the extra aid or not. Column 1 of Table 2 indicates that the effect of the basic aid letter was positive (1.792 percentage points) and statistically different from the effect of the extra aid letter (3.509 percentage points). The difference between the coefficient estimates indicates that the extra aid message, on its own, had an effect of 1.717 percentage points ($= 3.509 - 1.792$), which was highly statistically significant (p -value < 0.001). This result demonstrates that the reminder mechanism might, at most, explain the effects of the basic aid letter only.

However, we provide direct evidence that the reminder effect did not even play a significant role in the basic aid letter. For this test, we exploit heterogeneity in the mailing of a DCAD notice of appraised value to households (see Supplemental Appendix E for a sample notification). Starting May 15, 2020, all homeowners could download their notice from the DCAD website. On the same day, the DCAD mailed these notifications to households meeting certain criteria (e.g., increased appraised value, ownership change, loss of homestead exemption, rendered property, or new property).⁵² We mailed our letters on Wednesday, May 20, 2020. Thus, households should have received the official DCAD notification about five days before our letter. If our basic aid letter worked primarily through a reminder effect, it should have had a larger effect on households that did not receive the DCAD letter, since the other households would already have been reminded about the opportunity to protest through the DCAD letter. In columns 10 and 11 of Table 2, we split the sample into those who received or did not receive a DCAD notification, respectively. The effects of the basic aid letter on subjects who received and did not receive a notice are on the same order of magnitude (coefficients of 1.449 and 1.935, respectively) and are statistically indistinguishable from each other (p -value = 0.317). This finding suggests that the effect of the basic aid letter far exceeded a simple reminder effect.⁵³

Regarding the effects of the basic aid message, our preferred interpretation is that they were mainly the product of the walkthroughs provided on the project's website. A first piece of evidence for this interpretation relies on unsolicited feedback from participants. On the project's website, we provided an email address to contact researchers with concerns about the research project. Several subjects sent

⁵²For instance, the DCAD did not mail notices if the property value did not change or if the property value decreased.

⁵³Supplemental Appendix A.5.4 presents an additional robustness check. We show that the results are robust if we perform an even finer analysis to address potential differences between households that received the official notification or not—that is, comparing the effect of our letters between households who were slightly to the left versus slightly to the right of the threshold for receiving the DCAD notification.

emails expressing gratitude for the letter and website and many mentioned the walk-throughs. For example, some mentioned that they had wanted to protest for years but did not know how until receiving our letter. Similarly, the survey included an open-ended question for subjects to share any thoughts with the researchers. Many used that space to express gratitude, and some explicitly stated how the information in the letter and on the website helped them navigate the protest process.⁵⁴ Another source of supporting evidence for this mechanism stems from the data on the visitors to our website. We estimate that the basic aid message generated 903 additional direct protests.⁵⁵ We can compare this number of additional protests that were induced by the aid message to the 2,769 unique visits to the website walkthroughs, as recorded by Google Analytics.⁵⁶ Some of these visitors may have looked at the walkthroughs but did not protest, and some may have used the walkthroughs but would have protested even without them. If we assume that around one-third of those website visitors were induced to protest by our website, that would explain all 903 additional protests generated by the basic aid message. In other words, it would not be far fetched to attribute the entire effect of the basic aid message to the online walkthroughs.

Regarding the extra aid message, our preferred interpretation is that subjects either used our proposed argument as is or followed our instructions to come up with an argument of their own.⁵⁷ Indeed, we can provide some direct evidence that some subjects used our suggested argument in the extra aid message as is to complete their protest form. This test is based on data from 5,026 households in the subject pool who protested online. For these households, we observe the opinion of value that they entered on the online form. In column 12 of Table 2, the dependent variable takes the value 100 if the household provided an opinion of value in their protest that is within half a percentage point of the value we suggested in their extra aid message.⁵⁸ The bottom of column 12 shows that, in the control group with no letter, there was a 3.37 percentage points chance that a household would enter an opinion of value that coincided almost exactly with the value that we would have suggested if they had been assigned to the extra aid message treatment. In other words, it is highly unlikely that subjects will use an almost identical opinion of value as the one we would have suggested. For households that received the basic aid letter, that probability remained equally low, which is expected because the basic aid message did not include any information about our suggested value for their household. For households who received the extra aid letter, the frequency increased by a whopping 15.287 percentage points

⁵⁴ For instance, a respondent to the survey stated “Due to your letter I will protest. In the past, I have not had this information. Your explanations and details were very helpful.”

⁵⁵ We arrive at this figure by taking the effect of the basic aid letter (1.792 percentage points, from column 1) and multiplying it by the total number of letters sent that included at least the basic aid message (50,394).

⁵⁶ For more details, see Supplemental Appendix A.5.1.

⁵⁷ One alternative interpretation is that households reacted to the extra aid message because they learned about the probability of a successful protest. While we do not believe that it played a significant role, this mechanism is embraced by our definition of filing frictions, to the extent that our letter provided information that was unknown to homeowners.

⁵⁸ One limitation with this exercise, however, is that it is based on a subsample (households that protested online and entered a value in the “Opinion of Value” field) that is not random and thus introduces possibility endogeneity bias, despite the random assignment.

(p -value < 0.001). This evidence suggests that a substantial fraction of households that received the extra aid letter copied the suggested value from our letter directly into their protest forms.

Finally, there is an additional mechanism for the effects of the extra aid message that deserves attention: households may have reacted to the extra aid message due to fairness considerations. In Supplemental Appendix A.5.3, we provide direct evidence against that interpretation: using the data from the survey, we show that the extra aid message did not affect households' feelings of unfairness with their taxes.

G. Magnitude of the Effects

A challenge with interpreting the magnitudes of the effects in mailing experiments is noncompliance: for example, some households may not have received their letter or received a letter but did not read it. To correct for these types of noncompliance, we estimate the reading rate (i.e., the share of recipients that actually read the letter on time). Following Botton and Perez-Truglia (forthcoming), we combine estimates from different sources to approximate the reading rate. According to the US Monitor nonprofit standard mail delivery study, around 95 percent of standard nonprofit mailers are successfully delivered (Botton and Perez-Truglia, forthcoming). Based on data from the US Postal Service Household Diary Survey (Mazzone and Rehman 2019), we estimate that, conditional on delivery, around 74 percent of our letters were opened by the recipients.⁵⁹ If we combine these two estimates, we arrive at a reading rate of 70.3 percent ($= 0.95 \cdot 0.74$).⁶⁰ To account for this source of attenuation bias, we scale up the coefficients by a factor of 1.42 ($= 1/0.703$). The resulting scaled-up effects would be 2.55 percentage points for the basic aid letter and 4.98 percentage points for the extra aid letter, which are still conservative scale-up factors, as there may be additional sources of noncompliance.⁶¹

To translate the filing frictions into a money metric, we combine the results from the field experiment with the results on the homestead cap threshold discussed in Section II above. We focus on the effect of the most comprehensive letter. The scaled-up effect of the extra aid letter is 4.98 percentage points. According to the calculations using the homestead cap quasiexperiment reported in Section IIB, each \$100 reduction in the tax amount due to the homestead cap decreases the protest probability by 2.14 percentage points.⁶² These results indicate that the homestead cap would need to reduce the tax amount by \$232 to generate a reduction of 4.98 percentage points in the protest rate. These results imply that the average filing frictions

⁵⁹ This figure is based on the 2018 Household Dairy Survey recruitment sample and corresponds to the estimate of treatment of advertising mail reported in Figure 5.3 of (Mazzone and Rehman 2019). See Botton and Perez-Truglia (forthcoming) for more details.

⁶⁰ Another potential source of noncompliance is spillovers. If treated households shared information from the letters with neighboring households who were in the control group, that would introduce an attenuation bias in our estimates. However, we provide evidence that this form of noncompliance is negligible: Supplemental Appendix A.5.2 shows that the estimated spillovers are statistically insignificant and precisely estimated around zero.

⁶¹ For example, some households may have opened the letter too late, either after they filed a protest or after the protest deadline—whichever came first.

⁶² Supplemental Appendix Figure A.6 shows that the effects of expected tax savings are similar, although less precisely estimated, when restricting the sample to the same sample used in the field experiment.

cost is on the order of \$232. This is just a rough approximation. The effect of the extra aid letter gives a lower bound on the costs from protesting, as this letter did not eliminate the filing frictions completely—for example, subjects still had to follow the instructions to file the form and may need to take further action in the future, such as discussing a settlement in informal or formal hearings. This calculation also assumes that households care only about the costs and benefits for the current year, but in reality, there may be dynamic considerations as well.

As a sanity check, we compare our estimate of filing frictions to the fees charged by agents that protest on households' behalf. We identify one such company that offers the service for a flat fee. Assuming that the marginal customer is indifferent between hiring this agent or protesting directly, the flat fee should constitute a measure of the filing frictions of that marginal customer.⁶³ In 2020, the flat fee was \$139 for properties assessed below \$200,000, and \$305 for properties assessed between \$200,000 and \$500,000.⁶⁴ Those flat fees (\$139 and \$305) are on the same order of magnitude as our estimated average filing frictions cost (\$232), thus suggesting that our estimates are of the right order of magnitude.⁶⁵

The magnitude of the filing frictions can have some policy implications too—that is, these frictions may explain why some households protest less than others. Our findings can also explain an otherwise puzzling fact about tax protests: even though there is no fee to file a protest and no risk of increasing taxes, only a minority of households choose to protest their taxes each year. One potential explanation is that most households do not protest because they do not want to free ride on the taxes paid by others. Our findings point to a different explanation. On the one hand, among those who protested in 2020, the average amount of tax savings was \$338 (including both successful and unsuccessful protests). On the other hand, we estimate filing frictions of \$232 (and this is a lower bound). This comparison favors the interpretation that most households may want to free ride on the taxes paid by others, but they face significant filing frictions.

IV. Economic and Racial Disparities

The filing frictions documented above have the potential to create inequities in the tax appeal system. For instance, if poorer households or households from disadvantaged racial groups are more sensitive to filing frictions, they may end up protesting at lower rates and, in turn, paying higher tax amounts. In this section, we explore whether such inequities exist, and whether they can be mitigated by low-cost interventions like the one in our field experiment.

⁶³ This is just a rough approximation. For example, part of the willingness to pay for the service may be driven by the belief that the agent can negotiate higher tax savings than if one were to protest directly.

⁶⁴ Source: https://www.dfwtaxadvisor.com/practice_areas/property-tax-protest/, accessed on May 15, 2020.

⁶⁵ In theory, fees should not be lower than the filing frictions costs; otherwise, homeowners would presumably be unlikely to protest directly. This exercise is only presented as a sanity check of the order of magnitude and, among other things, does not take into account heterogeneity between homeowners.

A. *Economic and Racial Gaps in Protest Rates*

We start by providing a descriptive analysis of differences in protest rates by wealth and by race. The results are presented in Figure 5. When comparing protests across racial groups, we have to keep in mind that the distribution of home values differs substantially across some of these groups. To disentangle the differences by race from the differences stemming from home values, we break down the protest rates first by (approximate) quintiles of home value and then, within each of these quintiles, we further break down the data by the race of the household.⁶⁶ In both panels of Figure 5, the x-axis represents quintiles of home value. In Figure 5, panel A, the y-axis represents the probability of protesting (combining direct protests and protests through agents). Figure 5, panel B is similar to Figure 5, panel A, except that the y-axis represents the tax savings rate—that is, the percent reduction in estimated taxes as a result of protesting. For the baseline specifications, we include all types of protests, direct and by agent, because protests through agents make a significant contribution to the gaps by home value and race and, thus, it is important to include them for a more complete picture. However, we note that the findings are qualitatively similar if we focus instead on direct protests (results reported in Supplemental Appendix A.5.6).

The first result that stands out from Figure 5, panel A is that owners of more expensive homes have a substantially higher probability of protesting. For example, the protest rate is 14.2 percentage points for White households in the bottom quintile of home value (\$50,000–\$150,000), while it is 35.6 percentage points for White households in the top quintile ($\geq$ \$400,000). This difference is statistically significant (p -value < 0.001), and most importantly, it is economically large: households in the top quintile are more than twice as likely to file a protest compared to households in the bottom quintile. To study the tax savings that households in different home value quintiles obtain from the system of property tax appeals, Figure 5, panel B shows the savings rate by home value. The statistics in Figure 5, panel B do not condition on protesting. This panel shows that the higher protest rates of more expensive homes are consequential, insofar as they translate into higher savings for these homes. Moreover, we note that the savings favoring more expensive homes would be even larger if we measured tax savings in dollar terms instead of percent terms.

The fact that owners of more expensive homes are more likely to file a tax protest could be driven by different factors. In the spirit of the results in Section II above, richer households may be more likely to protest because they stand to gain more from protesting. Motivated by the results of Section III, richer households may be more likely to protest because they are subject to weaker information frictions.⁶⁷

⁶⁶ We break home values into quintiles for ease of exposition, but the findings are similar if we break them down into finer quantiles.

⁶⁷ The DCAD may systematically underassess poorer homes (relative to their respective market values). In such a case, poorer households would have weaker incentives to protest, thereby explaining (at least partly) why poorer households protest at lower rates. Supplemental Appendix A.5.7 provides some evidence suggesting that this mechanism may play a role.

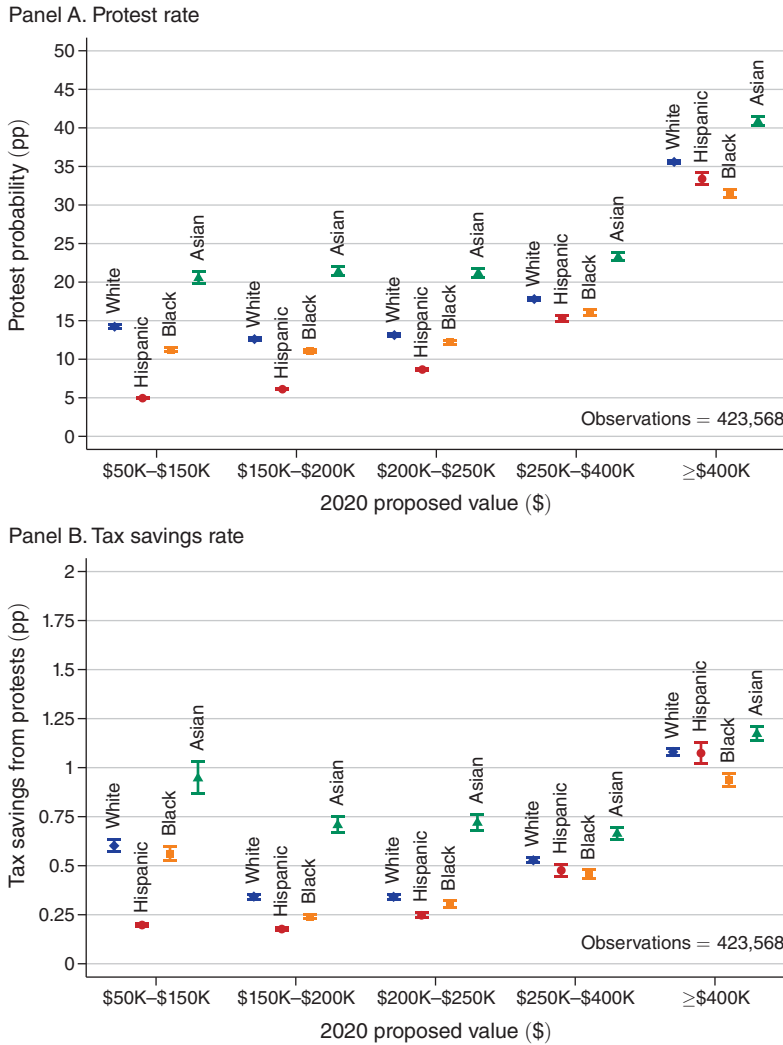


FIGURE 5. PROTEST RATES AND TAX SAVINGS, BY RACE AND HOME VALUE

Notes: This figure presents descriptive statistics by race for the main sample of 423,607 single-family homes in 2020. Ninety percent confidence intervals are in brackets, based on robust standard errors. The x-axis in each panel denotes the (approximate) quintiles of the proposed values announced by the DCAD on May 15, 2020. Each point in panel A represents the share of those households who protested (either directly or through an agent) in 2020, and each point in panel B represents the estimated percentage reduction in the tax amount due to protesting (either directly or through an agent) in 2020. Blue diamonds correspond to White households, red dots correspond to Hispanic households, yellow squares correspond to Black households, and green triangles correspond to the households identified as Asian.

The second observation from Figure 5, panel A is that within the given quintiles of home value, there are some systematic differences in protest rates by race. Relative to White homeowners with comparable-valued homes, Hispanics are less likely to protest. For example, among households in the bottom quintile of home value, the protest rates are 14.2 percentage points for White households versus 5.0 percentage points for Hispanic households—this difference is not only large, but also statistically significant

(p -value < 0.001). This gap is present in every quintile of home value. Figure 5, panel B shows that differences in protest rates have a material impact, as they translate into differences in tax savings favoring White homeowners. This Hispanic–White gap could be due to several factors. Some critics of the current system argue that demographic differences in protest rates may reflect inequitable access to the tax appeal system (Lieber 2020). They argue that minority groups are marginalized in the protest process due to factors such as limited knowledge of how to initiate a protest, lower levels of technological proficiency, or financial constraints that hinder their ability to hire an agent (Doerner and Ihlanfeldt 2015). For example, due to language barriers or differences in financial literacy, Hispanic households may face stronger filing frictions than White households. Indeed, this interpretation would be consistent with the results of Finkelstein and Notowidigdo (2019), which finds higher take up of social benefits (the Supplemental Nutrition Assistance Program) for White subjects and for subjects whose primary language is more likely to be English.⁶⁸

Due to its substantial magnitude, the subsequent analyses place special emphasis on the Hispanic–White gap. However, before delving deeper into this topic, it is important to briefly address the remaining racial disparities. Black households are systematically less likely to protest than White households. The Black–White gap in protests that we document for Dallas County is generally consistent with the evidence from Cook County presented by Avenancio-León and Howard (2022). However, the Black–White gap in our setting is different from the Hispanic–White gap in that it is smaller in magnitude and, as shown in Supplemental Appendix A.5.6, it is driven almost entirely by protests through agents.⁶⁹ Last, relative to White households, Asian households have a higher probability of protesting.

B. *Heterogeneous Effects of the Intervention*

Figure 6 explores the heterogeneity of the treatment effects in the field experiment by home value and race.⁷⁰ In both panels, the x -axis divides the sample by quintiles of home value, then breaks the data down further by Hispanic versus White households, and then further again by whether the household was in the control group, basic aid group, or extra aid group. As in Figure 5, Figure 6, panel A shows the protest rates (combining direct protests and protests through agents) while Figure 6, panel B corresponds to tax savings.⁷¹

Figure 6 shows that the basic aid and extra aid treatments had positive effects on protest probabilities across the board—that is, in all five quintiles of home value, and

⁶⁸ One potential concern when comparing White and Hispanic households is that the DCAD systematically assigns lower proposed values to Hispanic households. This would be a concern because in such a case, Hispanic households would have weaker incentives to protest than White households. Based on comparing home value estimates from Redfin (2020) and proposed values, we show evidence against this possibility in Supplemental Appendix A.5.7: if anything, Hispanic households would have stronger incentives to protest than White households.

⁶⁹ Avenancio-León and Howard (2022) finds differences in appeal rates and savings from appeals comparing White versus Black households and comparing White versus combined Black and Hispanic households. Our results show smaller differences in appeals and savings between White and Black households and larger differences between White and Hispanic households.

⁷⁰ For the sake of completeness, Supplemental Appendix A.5.5 reproduces the estimates in Table 2 separately for White and Hispanic households.

⁷¹ Supplemental Appendix A.5.6 reproduces the same figure, but focusing only on direct protests.

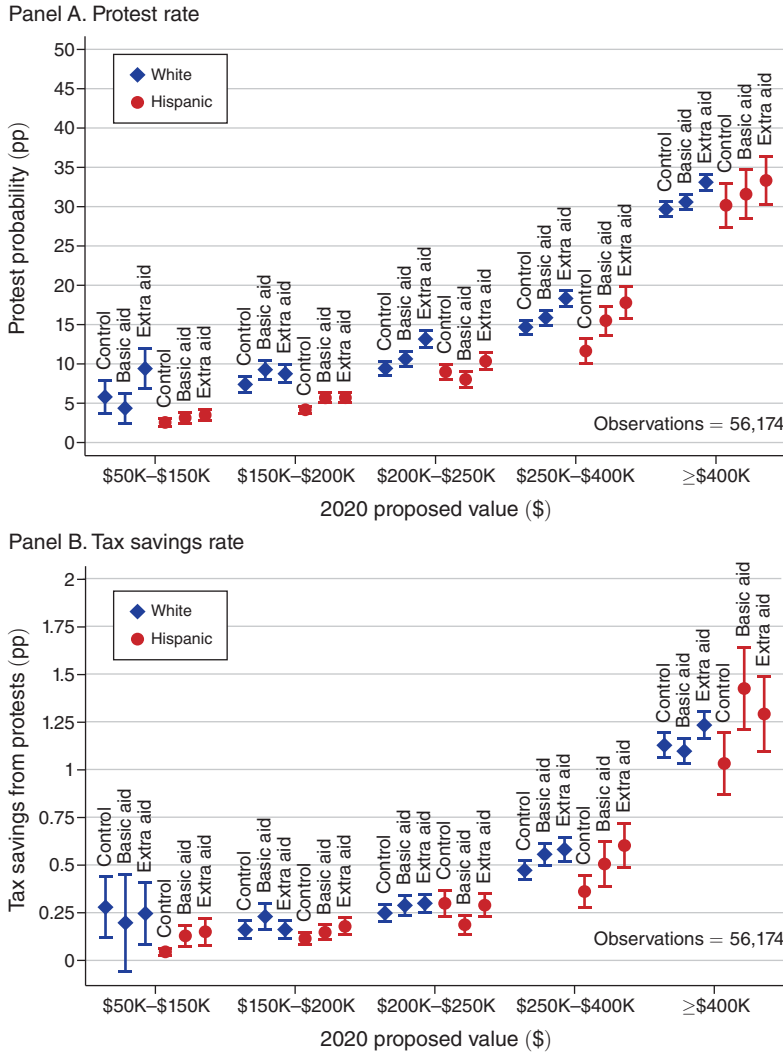


FIGURE 6. PROTEST RATES AND TAX SAVINGS, BY TREATMENT GROUPS AND HOME VALUE:
HISPANIC HOUSEHOLDS VERSUS WHITE HOUSEHOLDS

Notes: This figure presents protest rates by ethnicity based on the 56,174 households that the ethnicity algorithm classified as White or Hispanic, out of the 78,462 single-family homes in the field experiment’s subject pool (that had not protested before receiving the letter). Ninety percent confidence intervals are in brackets, based on robust standard errors. The x-axis in each panel denotes the (approximate) quintiles of the proposed values announced by the DCAD on May 15, 2020. Each point in panel A represents the share of those households who protested (directly or through an agent) in 2020, and each point in panel B represents the estimated percentage reduction in the tax amount due to protesting (directly or through an agent) in 2020. Blue diamonds correspond to White households, and red dots correspond to households identified as Hispanic.

or both Hispanic and White households.⁷² The magnitude of the treatment effects (in percentage points) are similar across households of different home

⁷²For the sake of completeness, Supplemental Appendix A.3.4 shows the results of the analysis of expected savings separately for White and Hispanic households.

values.⁷³ As an illustration, consider the impact of the extra aid letter (relative to no letter) for White households. Figure 6, panel A shows that, among White households, the extra aid letter increased the protest rate by 3.6, 1.4, 3.7, 3.6, and 3.4 percentage points in the bottom, second, third, fourth, and top quintile of home value, respectively, compared to the control group. Differences between these treatment effects are mostly small and statistically insignificant.

Figure 6, panel A also shows that, from an overall perspective, the effects of the intervention (in percentage points) are, if anything, slightly smaller for Hispanic households than for White households. Specifically, in the top two quintiles of home value, the effects are similar between White and Hispanic households, while in the bottom three quintiles, the effects are somewhat smaller for Hispanic households. Take, for example, households in the middle quintile of home value. According to Figure 6, panel A, the extra aid letter increased the protest rate for White households by 3.7 percentage points (p -value < 0.001), while the corresponding effect for Hispanic households was 1.4 percentage points (p -value = 0.186). Finally, Figure 6, panel B shows that the conclusions are similar to those discussed above when we focus on the tax savings as the dependent variable (instead of the protest rate).

C. Effects of Targeted and Untargeted Interventions

Next, we explore the potential for targeted and untargeted interventions to mitigate the economic and racial gaps in the protest rates that we document earlier. To measure the effects of targeted and untargeted interventions on economic and racial gaps, we turn to Table 3. This table shows the average protest rates broken down by treatment group (control, basic aid, and extra aid). Columns 1–3 present protest rates, combining direct and protests through agents, and columns 4–6 present tax savings rates in percentage points.

We start by discussing the gap between poorer and richer households. These results are presented in panel A of Table 3, which splits the sample into households with home values greater than or less than \$250,000 (corresponding to approximately the median home value). Column 1 shows that richer households protest at a higher rate than poorer households: 22.3 percent of richer households in the control group protested, while the corresponding share is 7.2 percent for poorer households. At baseline, the gap between richer and poorer households in column 1 is 15.2 percentage points (p -value < 0.001). Comparing column 1 with columns 2 and 3 indicates that, if anything, an untargeted intervention would widen the protest gap between richer and poorer households. The basic aid and extra aid letters increase the protest rate for both richer and poorer homeowners, but the effects are somewhat larger (in percentage points) for richer homeowners. As a result, the basic aid letter and extra aid letters, if anything, would widen the economic gap in protests.

⁷³ We compare the treatment effects in percentage points because that is the relevant metric to understand the impact on the economic and racial gaps. However, there are other ways to compare the treatment effects. For example, the baseline protest rates are lower for Hispanic households compared to White households. Therefore, if we measure the effects of the intervention as a proportion of the baseline rate, the magnitudes appear more comparable between White and Hispanic households.

TABLE 3—EFFECTS OF THE TWO TYPES OF LETTERS ON THE PROBABILITY OF PROTESTING AND TAX SAVINGS FROM PROTESTING, BY HOME VALUE AND RACE

	Protest rate (percentage points)			Tax savings rate (percentage points)		
	Control (1)	Basic aid (2)	Extra aid (3)	Control (4)	Basic aid (5)	Extra aid (6)
<i>Panel A. By home value</i>						
\$250,000	22.325 (0.352)	23.886 (0.378)	26.436 (0.389)	0.814 (0.022)	0.857 (0.023)	0.951 (0.024)
< \$250,000	7.175 (0.218)	8.528 (0.252)	9.311 (0.259)	0.201 (0.013)	0.211 (0.013)	0.226 (0.013)
Diff. above–below \$250,000	15.150	15.358	17.125	0.613	0.646	0.725
Diff. <i>p</i> -value	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Observations	28,068	25,012	25,382	28,068	25,012	25,382
<i>Panel B. White versus Hispanic households</i>						
White	18.694 (0.352)	19.796 (0.380)	21.967 (0.389)	0.655 (0.021)	0.680 (0.023)	0.735 (0.023)
Hispanic	8.139 (0.309)	9.190 (0.350)	10.178 (0.363)	0.254 (0.018)	0.306 (0.021)	0.345 (0.022)
Diff. White–Hispanic	10.556	10.606	11.789	0.401	0.374	0.391
Diff. <i>p</i> -value	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Observations	20,094	17,789	18,291	20,094	17,789	18,291

Notes: Robust standard errors are in parentheses. Average protest rates (columns 1–3) and tax savings rates (columns 4–6) for households in the field experiment sample. The control group is comprised of subjects who were randomly chosen not to receive a letter. The basic aid group contains subjects that were randomly chosen to receive a basic aid letter. The extra aid group contains subjects that were randomly chosen to receive an extra aid letter. “Protest rate” refers to households that filed a protest (directly or through an agent), while “tax savings rate” refers to households’ estimated percentage reduction in the tax amount due to protesting (directly or through an agent) in 2020. Panel A contains rows showing mean protest and tax savings rates for households with values above or below \$250,000 (corresponding approximately to the median home value), as well as the differences in these variables between these home value groups, while panel B contains rows showing mean protest and tax savings rates for White households and Hispanic households, along with the differences in these variables between these races. Difference *p*-values are shown in brackets.

For example, relative to the control group, the extra aid letter widened the gap from 15.2 percentage points to 17.1 percentage points (difference *p*-value < 0.001).

Next, consider a hypothetical targeted intervention that consists of sending extra aid letters to the poorer households only. The results from panel A of Table 3 indicate that such a targeted intervention would have reduced the protest gap by wealth. For instance, an extra aid letter targeted at poorer households would reduce the economic protest gap by 14.5 percent, from 15.2 percentage points to 13.0 percentage points. While that is far from fully closing the gap, it is nonetheless remarkable given that this is such a low-cost intervention. Last, columns 4–6 reproduce the regressions from columns 1–3, except that they use tax savings as the dependent variable (instead of the protest rate). The conclusions are similar to those presented above when we focus on this alternative outcome variable.

Panel B of Table 3 is similar to panel A except that instead of splitting the sample into poorer versus richer households, it divides the sample into White versus Hispanic households. White households protest at a higher rate than Hispanic households at baseline: column 1 shows that 18.7 percent of White households

protested versus 8.1 percent for Hispanic households—a gap of 10.6 percentage points (p -value < 0.001). This large gap is a combination of two facts documented above. First, Hispanic households tend to be poorer than White households, and poorer households are less likely to protest. Second, conditional on home value, Hispanic households are less likely to file a protest than White households. As was the case for poorer versus richer households, an untargeted intervention would widen the Hispanic–White gap. The basic aid and extra aid letters increase protest rates for both White households and Hispanic households, but the effects are slightly larger in magnitude for White households. For instance, relative to the control group, the extra aid letter widened the gap from 10.6 percentage points to 11.8 percentage points (difference p -value=0.082). However, the results would again be different under a targeted intervention. If we had sent additional aid letters only to Hispanic households, the results of panel B of Table 3 indicate that such a targeted intervention would have increased the protest rate among Hispanic households by 2.0 percentage points (p -value < 0.001). As a result, the protest gap between Hispanic households and White households would be reduced by 18.9 percent, from 10.6 percentage points to 8.6 percentage points. Last, columns 4–6 reproduce the regressions from columns 1–3, but using tax savings instead of the protest rate as the dependent variable. The conclusions from columns 4–6 are similar to those from columns 1–3.

V. Conclusions

Individuals in all US states can legally file a protest to reduce their property taxes. In this paper, we shed light on the factors driving the decision to file a tax protest. Using quasiexperimental variation, we show that expected monetary savings are of first-order importance for the decision of whether to protest taxes. Using a field experiment, we show that filing frictions also play a significant role in driving protest choices. Combining the experimental and quasiexperimental findings, we estimate that filing frictions are on the order of \$232.

We also document the existence of large economic and racial disparities in protest rates. Wealthier households protest at much higher rates than less wealthy households. And, conditional on home wealth, there are also racial disparities in the protest rates. Because our field experimental intervention has substantial effects on protest rates, we explore whether this type of low-cost interventions can help mitigate inequities in the system of tax appeals.

We show that this type of intervention could mitigate disparities by income and race, but only if targeted toward disadvantaged groups. These low-cost interventions could be promoted either internally by county assessors' offices or externally through nongovernmental organizations. For example, a nongovernmental organization named the Property Tax Appeals Justice has recently been registered in Texas to help with property tax appeals of poor and otherwise disadvantaged households. These interventions could be targeted to poorer households, Hispanic households, or any other groups believed to be left behind by the system of tax appeals.

Although our evidence is based on data from a specific US county, to the extent that tax protests work similarly across counties both within and outside Texas, the results should be generalizable to those other settings. Indeed, conducting the same

experimental and quasiexperimental designs from our study in other US counties would be straightforward. For instance, other counties also have a homestead cap for property taxes, and our mailing campaign could be conducted in other places. In this spirit, we provide detailed accounts of the implementation and data sources that other researchers can follow, and we are happy to share data, code, tips, and additional resources.

Tax protests provide a novel approach to study tax preferences with a revealed-preference approach.⁷⁴ Tax protests provide a measure of willingness to pay taxes that, unlike tax delinquency or tax evasion, does not involve illegal behavior. By leveraging publicly available data, researchers can implement interventions independently, which allows easier replication and makes them accessible to a wider range of investigators.⁷⁵ Furthermore, we believe that our framework can be adapted to study research questions beyond tax compliance and in diverse fields such as political economy, public economics, finance, and behavioral economics. Our novel setting has several features that we believe make it attractive to researchers. The effects on behavior are measured with objective data from administrative records in a naturally occurring context and are based on high-stakes choices. The experiment can be conducted entirely based on publicly available data without the need for nondisclosure agreements or data user agreements. The experiment can be implemented in a few weeks, and the final results may be ready in a couple of months. The mailing experiment is relatively cheap, costing less than \$0.25 per subject. Lastly, the experiment can be implemented on massive scales, potentially involving up to millions of subjects.⁷⁶

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⁷⁴ Social scientists have typically studied various dimensions of preferences for taxation and redistribution through survey data or laboratory experiments (e.g., Alesina and Giuliano 2011; Cruces, Perez-Truglia, and Tetaz 2013), which have some well-known limitations. For example, survey respondents may say that they want high redistribution due to social pressure (i.e., cheap talk), but they may act selfishly in real-world, high-stakes situations.

⁷⁵ Furthermore, conducting independent experiments allows for conducting some interventions that are interesting to researchers but may not be attractive to tax agencies.

⁷⁶ In Dallas County alone, it can potentially involve hundreds of thousands of subjects. By pooling multiple counties, it could be scalable to millions of subjects.

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