Racial Inequality in Property Tax Appeals: Evidence from Field Experiments with Homeowners and Assessors*

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Abstract

Despite minority homeowners facing a disproportionate property tax burden, they are less likely to take action to reduce their taxes by filing an appeal. We use two field experiments, one with homeowners and the other with county assessors, to understand the drivers of racial differences in homeowner appeal behavior, and the implications for inequality in the property tax system. In a survey experiment with Indiana homeowners, we show that information frictions and perceived discrimination contribute to the racial appeal gap. We additionally find racial differences in the treatment of homeowners by assessors based on both appeal outcomes and through a complementary correspondence study in which assessors are less likely to provide aid to minority homeowners who want to appeal. We additionally document unequal treatment of female homeowners by assessors. Overall, our results indicate that appeals are not effective at reducing biases in assessments.

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1. Introduction

Property taxes play an essential role in the funding of public goods and services, such as roads, schools, and police forces, in the United States. By law, the amount of property taxes owed should only be a function of a home's sale value and the local community tax rate, with limited deductions. However, since individual homes are infrequently sold, local governments must create an estimate of the sale value called the assessed value. Recognizing that these assessments are subjective and may have errors, a taxpayer can appeal if she believes that the county assessor has misestimated her property's value.

Biases in the assessment process mainly affect minority homeowners in low-priced homes, leading them to pay more than their fair share in property taxes (Avenancio-León and Howard, 2022b; Berry, 2021). Such racial differences imply that minority households effectively subsidize the public services of other households in their local area.footnoteMargo (1984) argues that this redistribution is due to systemic discrimination, although Higgs (1984) disputes this claim. Avenancio-León and Howard (2022b) argue that the gap comes from the fact that assessments are less sensitive to neighborhood attributes than sale prices. Moreover, relatively high property taxes exacerbate the racial wealth gap (Perry et al., 2018; Menendian et al., 2021), place minority homeowners at greater risk for financial distress (Anderson and Dokko, 2008; Atuahene, 2018; Wong, 2020), and affect household finances (Zhao and Burge, 2017; Hayashi, 2020). Despite the bias in assessments and its consequences, minority homeowners are less likely to appeal their assessed values to reduce the excess tax burden (Avenancio-León and Howard, 2022b; Ihlanfeldt and Rodgers, 2021).

We conduct two field experiments to understand the drivers of disparities and inequality in property tax appeal behavior, and the extent that appeals can reduce the bias in tax burdens. We ask: to what extent are the racial and gender appeal gaps driven by beliefs, information frictions, and discrimination? We invited homeowners in Indianapolis, Indiana—the fifteenth largest city in the U.S.—to participate in an information provision

experiment about the appeals process. Through the survey, we randomized a subset of taxpayers to receive: (1) information about the financial benefits of appealing, (2) information and a step-by-step guide on how to appeal, or, (3) information, the guide, and a financial incentive to appeal in the form of last-dollar insurance.[1] The design allows us to understand how biased beliefs, information frictions, and perceived discrimination affect both individual appeal behavior and the appeal gaps. We match our survey data to county administrative data on appeals and appeal outcomes to measure the effects on the gaps in property tax burdens. We additionally conducted a correspondence study of county assessors as another test for the role of discrimination.

Without being provided with information, a guide of the appeals process, or incentives, minority (Black and Hispanic) homeowners in our experiment are 16% (1.2 pp) less likely to file an appeal to lower their property taxes than non-minority homeowners.[2] The racial appeal gap is large even though minority homeowners are 75% more likely to state that they would like to appeal than non-minority homeowners, and believe the financial benefits to appealing are higher than non-minority homeowners believe. However, minority homeowners perceive that they face discrimination, and they have less prior (pre-experiment) knowledge of and experience with the appeals process—only 40% of minority homeowners knew they could appeal compared to 64% of non-minority homeowners. We design our treatments to help us understand the extent that each of these factors drive the racial appeal gap.

From our two experiments, we provide evidence that both perceived and actual discrimination contribute to the racial appeal gap. In our experiment with homeowners, we find that minority homeowners are 14.4 percentage points more likely to appeal when provided with our researcher-backed last-dollar insurance for appealing, compared to receiving the

[1]Homeowners are eligible to receive last-dollar insurance if they appeal and receive less than a $75 tax reduction (based on a change in the assessed value) from the county. It pays out in the bad state of the world where the homeowner goes through the trouble to appeal but receives little to no money back from the government.

[2]This is consistent with past evidence (e.g. Weber and McMillen, 2010; Doerner and Ihlanfeldt, 2014; Ross, 2017; Avenancio-León and Howard, 2022b). Additionally, in the year before our experiment, the racial gap in appeals is 45%.

step-by-step guide. However, minority homeowners do not respond to information that the *average* financial benefits from appealing are higher than they initially believed. We interpret these results, based on our conceptual framework, as evidence of discrimination—minority homeowners only respond to a financial incentive where the likelihood of receiving the money is independent of race, but they do not respond to a financial incentive from the county which they believe discriminates based on race. Additionally, non-minority homeowners only respond to the information about average financial benefits, but not the last-dollar insurance. Since homeowners in our insurance treatment also receive information about the average financial benefits, only those who believe they would receive far less money back than the average person, for example because of discrimination, should respond to the insurance. We rule out other possible interpretations of this effect that may be due to characteristics that are correlated with race, such as differences in home values and the cognitive costs of filing an appeal. We also find suggestive evidence that minority homeowners face larger information and filing frictions (Nathan et al., 2020) which widen the racial appeal gap.

We additionally provide evidence of discrimination in the property tax system based on assessors' behavior in the appeals process and a correspondence study. While our treatments greatly increase the number of both minority and non-minority homeowners who appeal, the treatments lead only non-minority homeowners to have successful appeals that reduce their tax burdens. Moreover, in our correspondence study, county assessors are less likely to respond to minority homeowners' request for aid in the appeals process. This racial gap in responses does not decrease when minority homeowners tell the assessor that they have evidence (e.g., a recent appraisal) that their assessed value is too high. Interpreting these results through a model of assessor behavior suggests the discrimination against minority—especially Black— homeowners is due to animus discrimination.

We also document a gender gap in appeals, appeal outcomes, and assessor behavior. We first find that female homeowners in the control group of our homeowner experiment are 43% (3.6 pp) less likely to file an appeal. However, female homeowners who learn the

true financial benefits of appealing significantly respond and become 233% more likely to file an appeal; men do not respond to this same information. Therefore, correcting biased beliefs completely eliminates the gender gap. The appeal guide has a large effect on appeal behavior, but the effect is not different for female and male homeowners. Our last-dollar insurance treatment has no further effect on appeal rates for female or male homeowners.

While our treatments greatly increase the number of female and male homeowners who appeal, only male homeowners are statistically significantly more likely to have a successful appeal. This provides evidence that assessors discriminate based on the homeowner's gender given that we find no evidence of gender differences in how homeowners choose comparables. In the correspondence study, female homeowners are also 10% less likely to receive a response from the assessor, which provides additional evidence of discrimination. Moreover, when homeowners include evidence that their assessed values are in fact too high, the gender gap in responses closes by about 70%. This baseline gender gap in responses and the shrinking of the gap from providing evidence comes entirely from female assessors. Through a model of assessor behavior, this implies that female assessors statistically discriminate against female homeowners.

Our main contribution is documenting the key drivers of inequality in the property tax system. We show that minority and female homeowners are less likely to appeal, and when they do, they face unequal treatment by county assessors, which supplements past work on assessments (e.g. Berry, 2021; Avenancio-León and Howard, 2022b; Ihlanfeldt and Rodgers, 2021, 2023). Property tax appeals are, therefore, not a remedy for the bias in assessments. Other policies are needed to alter assessors' behavior during the assessment and appeals process.[3] Our work therefore highlights the tax incidence implications of the unequal treatment of taxpayers (e.g., Moran and Whitford, 1996; Bloomquist, 2019; Gale, 2021; Goldin and Michelmore, 2022; Elzayn et al., 2023), especially homeowners (Brown,

[3]For example, Avenancio-León and Howard (2022a) suggest assessment growth caps can discipline assessor errors by lowering the correlation between neighborhood amenities and falsely high assessed home values.

2018).[4] In our setting, we show that both perceived and actual discrimination by county assessors—local elected officials—creates unequal outcomes by race and gender, which builds on this past work that has mainly focused on federal policy and the role of the IRS.

To make this contribution, we build on similar information provision experiments (e.g., Haaland et al., 2020), and especially Nathan et al. (2020) and Giaccobasso et al. (2022), who also study property tax appeals, and correspondence studies (e.g., Jowell and Prescott-Clarke, 1970; Bertrand and Duflo, 2017). Our combination of both a survey experiment with homeowners and correspondence study with assessors allows us to gain a thorough understanding of the drivers of appeal gaps by considering both sides of the market. We design both pieces through the lens of economic frameworks to disentangle which factors matter. The survey experiment allows us to understand how beliefs and information frictions matter for racial disparities (Alesina et al., 2021; Haaland and Roth, 2022; Akesson et al., 2022), taxpayers, (Bérgolo et al., 2017; Perez-Truglia and Troiano, 2018; Craig and Slemrod, 2022), and homeowners (Bottan and Perez-Truglia, 2020).[5] We also study the role of discrimination by providing last-dollar insurance to mitigate downside risk concerns that are especially relevant for minority homeowners who face discrimination and therefore would otherwise not want to go through the troubling of figuring out how to file an appeal.[6] We then use the correspondence study to provide further evidence of discrimination and to test whether the discrimination is animus and statistical (as in List, 2004; Neumark, 2012; Guryan and Charles, 2013; Brandon et al., 2023).[7]

Finally, by studying racial and gender differences among homeowners, our work also

[4]Since property tax appeals are a legal activity taxpayers can use to reduce taxes owed, our results also improve our understanding of racial differences in tax compliance and morale (Slemrod, 2007; Luttmer and Singhal, 2014; Holz et al., 2023; Gil et al., 2023).

[5]Information provision experiments have also been used to understand the role of misperceptions across a variety of policies, like redistribution (Cruces et al., 2013; Kuziemko et al., 2015), education finance (Lergetporer et al., 2018; Giaccobasso et al., 2022; Hurst et al., 2024), and the gender pay gap (Settele, 2022), among others.

[6]This mechanism and function of insurance is similar to those found in Cullen et al. (2023).

[7]Since county assessors are local elected officials, our correspondence study also helps us understand how the role of political elites in shaping policy, specifically racial and gender inequality (e.g. Habel and Birch, 2019; Dinesen et al., 2021; Magni and de Leon, 2021; Kertzer and Renshon, 2022; Aggarwal et al., 2022).

improves our understanding of housing market disparities more broadly. Similar to the racial appeal gap, discrimination adversely affects how prospective minority homeowners find and finance their homes (e.g., Ondrich et al., 2003; Ross et al., 2008) and refinance their mortgages (Hodge et al., 2007; Bayer et al., 2018). Additionally, information frictions—like inattention—borrower sophistication, and financial literacy also contribute to the racial mortgage refinancing gap (Agarwal et al., 2013; Keys et al., 2016; Bialowolski et al., 2020).[8] Further, we show that gender differences in both appeal likelihoods and outcomes can contribute to gender differences in (housing) wealth accumulation (e.g., Goldsmith-Pinkham and Shue, 2023).

2. Conceptual Framework

We consider a homeowner's decision of whether to appeal her home's assessed value to reduce her tax burden. The homeowner receives an estimated assessed value from the government, V_A, and faces a local tax rate, τ, creating a tax burden of τV_A. She can either pay this amount to the government and not file an appeal or pay cost c to file an appeal. If the appeal is successful ($S = 1$), then her assessed value is lowered to $V_{A'} < V_A$, and her new tax burden is $\tau V_{A'}$.[9] As with many administrative burdens, the perceived cost of appealing to the homeowner, c, depends on costs homeowners encounter when they search for information on how to appeal, comply with the requirements, and experience the stresses associated with the appeal process (Herd and Moynihan, 2019). This cost may also include a monetary fee.[10]

The homeowner files an appeal if the perceived benefits from appealing outweigh the cost given her beliefs and knowledge about the appeals process, $\mathcal{I} = \{\mathcal{B}, \mathcal{K}\}$, and demographics, including race and gender, d_{rg}:

[8] Financial frictions, such as low creditworthiness, negative home equity, or liquidity constraints (Caplin et al., 1993; Archer et al., 1996; Campbell, 2006), also matter for mortgage refinancing.

[9] We assume that if the appeal is unsuccessful, the tax burden does not change. In theory, the tax burden could increase, for example, if the government learns about recent undisclosed changes in the property. However, empirically this is unlikely.

[10] For example, in Indiana, filing by mail requires printing and mailing the form. Florida charges homeowners $15 to file, regardless of the outcome.

$$\mathbb{E}[\tau(V_A - V_{A'})|\mathcal{I}, d_{rg}] > c(\mathcal{K}, d_{rg}) \tag{1}$$

$$\implies \underbrace{\mathbb{P}[S|\mathcal{I}, d_{rg}]}_{\text{Likelihood of success}} \underbrace{\mathbb{E}[\tau(V_A - V_{A'})|\mathcal{I}, d_{rg}, S = 1]}_{\text{Financial benefits if successful}} > \underbrace{c(\mathcal{K}, d_{rg})}_{\text{Cost of appealing}}$$

Based on Equation 1, few homeowners may choose to appeal their assessed value because the cost of appealing is high. Not all homeowners may be aware that they can legally appeal their assessed value, which implies an infinite cost, or they may not know how to properly appeal (i.e. provide evidence). In this case, information frictions play an important role for who appeals. Alternatively, few homeowners may appeal because they perceive the benefits to be low—either because their perceived likelihood of success is low or the expected reduction in assessed value is small—suggesting a role for incorrect beliefs and other financial considerations in the decision of whether to appeal.

Equation 1 also highlights several potential drivers of the racial appeal gap. Minority homeowners may have less or worse knowledge of the appeals process and therefore face a higher cost of appealing, $c(\cdot)$. The differences in knowledge could concern the ability to appeal, information about how to file an appeal, and how to provide sufficient evidence. With limited knowledge, it would take a homeowner more time and effort to navigate the appeals process successfully. Moreover, a lack of knowledge about how to appeal and appeal properly could lead homeowners to file low-quality appeals that have a low likelihood of success or result in a small reduction in their taxes even if successful. Finally, the facially neutral appealing procedures may lead to intentional or unintentional higher psychological burdens on minorities (Ray et al., 2023).

Second, minority homeowners may have more pessimistic beliefs about the benefits. If minority homeowners believe that the likelihood of success is lower or that the possible reduction in taxes owed is smaller than non-minority homeowners believe, then their perceived expected benefits would also be lower. Since minority homeowners are less likely to

appeal and therefore have less experience with the system, they may have relatively biased beliefs.

Finally, minority homeowners may be less likely to appeal because of actual or perceived discrimination in the appeals process. Discrimination raises the costs of engaging with the system while lowering benefits, making it less likely that appealing is worth the effort for minority homeowners. Discrimination may also affect the extent to which minority homeowners receive help from government officials during the process. However, we would expect that discrimination in assessments to lead minority homeowners to appeal more often, all else equal, because the financial benefits are increasing in the government's error, $V_A - V_{A'}$.[11]

In addition to deciding whether to appeal, homeowners must choose how to appeal. More specifically, an appeal is more likely to be successful and result in a greater decrease in taxes owed if the homeowner provides high-quality evidence. Differences in knowledge about the appeals process by race may also affect both the decision to appeal and the types of evidence provided. If minority homeowners face higher costs of finding high-quality evidence, because of differences in $\mathcal{I}$, or face more discrimination when providing low-quality evidence, then they will be less likely to file as well.

We conduct a framed field experiment to understand which factors stop homeowners from appealing and which drive the racial appeal gap. While our discussion in this section focus on race, these mechanisms may also contribute to the gender appeal gap as well. Our treatments, described in the next section, change homeowners' beliefs about the likelihood of success and the financial benefits of appealing, the costs of appealing, and the financial benefits from appealing. This allows us to measure the role of each potential mechanism.

[11]For our framework, we consider how race and knowledge of the appeals process determine the appeal gap, all else equal. However, the racial gap may also be due in part to other differences between minority and non-minority homeowners, like differences in the true sale value of their homes. Minority homeowners, on average, live in homes with lower sale values and therefore they receive smaller proportional decreases in their assessed values as a result of a successful appeal, but they are also more likely to be overassessed (Berry, 2021).

3. Experiment 1: Homeowners' Decision to Appeal

3.1 Property Tax Appeals in Indiana

The property tax system in Indiana is similar to that in most U.S. states. Each homeowner receives an annual valuation (the assessed value) for their property. Then, the local government determines the property tax liability by applying a locality-specific set of tax rates and deductions. Collection of these taxes is a key source of funding for local governmental units such as counties, cities, and townships, and the taxes cover a wide variety of local services including the construction, operation, and maintenance of local infrastructure. Homeowners who disagree with the assessed value of their property have the right to file an appeal.

In April of each calendar year, the Marion County assessor's office conducts a preliminary assessment of all properties within the county, using a proprietary formula combined with trending factors. It then submits this "pseudo-assessment" to the state's Department of Local Government Finance (DLGF). Analysts and assessors at the DLGF analyze the set of assessed values to confirm that they are a reasonable representation of the comparable sales in the prior calendar year.[12] However, since the sale price is unobserved for the majority of homes in a given year that were not sold and must be imputed, there is potential for bias that may deferentially impact certain groups.

The DLGF then notifies the county whether the proposed assessed values are acceptable or require further revision. Typically, assessed values for Marion County are finalized by July. Then, nearly nine months pass until April of the next calendar year when the Marion County assessor's office sends each household a notification by mail. This notification informs the household of their assessed value and gross property tax liability (prior to applying any credits or deductions), and reminds them that the first property tax payment is due the following month in May and the second tax payment is due in November. In

[12]This process is common to most localities in the United States and is known as a ratio study because the key objective is to get the ratio of the pseudo-assessed value to the sale price (net of trending factors) as close to 1 as possible.

addition, the notification informs households of their right to appeal their assessment and directs interested households to resources to learn more.

Households who disagree with their assessed value have until June 15, an approximately two-month window after assessment mailing, to submit an initial appeal of their property's assessed value. In our experiments, we focus on *subjective* appeals, which involves the homeowner's contention that the assessed value reflects an inflated measure of the property's true market value.[13] As part of a formal appeal, households are recommended to provide evidence and arguments to support their claim that the property value assessment is incorrect. There is no formal rubric for what types of evidence taxpayers should submit to increase their chances of a successful appeal, but at a minimum, the appeal must include a proposed assessed value and a rationale for the new value. Differences across homeowners in the quality of the appeal or evidence provided may also contribute to the unequal burden of property taxes paid.[14]

3.2 Experimental Design and Data

In April 2022 and 2023, we recruited homeowners by mail to participate in an online survey. The survey allows us to measure the subject's initial beliefs, provide information to change their beliefs, and then observe whether they choose to appeal their property taxes. Each year we mailed 80,000 homeowners in Indianapolis (Marion County) an invitation to participate in an online survey experiment, with an individualized link so that we could match them with the administrative data. 1844 Homeowners consented to participate and were randomly assigned to one of three treatment groups or a control group: (1) *Appeal Information*, (2) *Appeal Aid*, (3) *Insurance*, and (4) *Control*. We outline each treatment below.

[13]Homeowners can also submit an *objective* appeal, which involves correcting a technical detail regarding the home that would lead to an overestimate of the assessed value. Such technical details include the square footage, number of bedrooms and bathrooms, and the presence of a basement just to name a few examples.

[14]Our conversations with the DLGF suggest that certain heuristics do exist in evaluating the strength of the rationale. For example, estimated home values arising from models, such as Zillow's Zestimate, may play a part in a rationale, but will be considered as strictly weaker evidence than actual home sales. The DLGF may also apply similar heuristics in terms of questions regarding the geographic radius and (in)exactness of matching attributes (e.g., number of bedroom and architectural style) for allegedly comparable properties.

The *Appeal Information* treatment informs homeowners of both the likelihood that an appeal is successful and the average tax reduction for successful appeals in their county. For example, based on county administrative data for 2022, 73% of homeowners who appeal were successful and the average reduction in taxes owed for these successful homeowners was 22%. These two numbers make up the left hand side of Equation 1. This treatment moves homeowners' beliefs about the financial returns to appealing closer to the truth. If racial differences exist because minority homeowners believe the likelihood they will be successful is relatively low, then this treatment can help close the gap; the left-hand side of Equation 1 would increase more for minority homeowners in that case. However, the extent to which different types of homeowners update their beliefs about their own likelihood of success also depends on their beliefs about discrimination since if there is discrimination, then the county average would overstate the benefits for minority homeowners.

The *Appeal Aid* treatment provides the same information as *Appeal Information* and additionally gives detailed information about how to construct and submit an appeal as part of a walk-through process resulting in a completed appeal form. As in Nathan et al. (2020), we inform homeowners of the appeals process and give them a completed form to submit to their local government appeal board. Since we provide the appeal aid through a survey, we can additionally link homeowner appeal filing to their beliefs and other behavior, like appeal intentions. This treatment aims to minimize the costs of filing, $c(\mathcal{K}, d_{rg})$, that are due information frictions. However, this treatment does not resolve differences in costs from discrimination.

We further build on Nathan et al. (2020) to study how differences in knowledge of how to construct an appeal affects the racial gap. We show homeowners five options of comparable homes to be included on the appeal form as evidence and allow them to choose one. This allows us to assess the differences across homeowners in the types of evidence they provide.[15] As in Equation 1, knowledge about how to provide sufficient evidence affects

[15]All options provided are valid appeal options based on the requirements outlined by the county. Homeowners must decide whether to choose a relatively high- or low-price option. A lower-price option should

both the cost and benefits of appealing. Additionally, by measuring homeowners' choices of comparable homes as evidence, we consider whether racial differences in the likelihood of success and reduction in taxes owed from appealing are due to differences in the quality of evidence provided.

Finally, the *Insurance* treatment increases the expected financial benefits of appealing by providing last-dollar "insurance," as well as the *Appeal Information* and *Appeal Aid* content. Homeowners in this treatment who also file an appeal are entered into a lottery with a 1 in 4 chance of winning. Lottery winners are guaranteed \$75, either from the researchers or the government. That is, if the government reduces taxes owed after the appeal by less than \$75, we pay the homeowner the remaining balance even if the government does not change the assessed value at all ($V_A = V_{A'}$). This treatment provides insurance since it pays out in the "bad" state of the world in which the appeal is rejected but the homeowner was made worse off from paying the filing costs.

With insurance, the financial benefit of appealing, compared to the benefits for homeowners in the other treatments given in Equation 1, increases by:

$$\underbrace{75\mathbb{L}(1-\mathbb{P}[S|F])}_{\text{Lottery benefit if unsuccessful}} + \underbrace{\mathbb{P}[0<\Delta T\leq 75|F]\big((75-\Delta T)\mathbb{L}\big)}_{\text{Lottery benefit if receive minimal tax relief}} \geq 0 \tag{2}$$

where $\Delta T = \tau(V_A - V_{A'})$ is the change in taxes owed from appealing, and $\mathbb{L} = 0.25$ is the probability of winning the lottery conditional on filing an appeal. Insurance (weakly) shifts the distribution of expected benefits to the right.

Based on Equation 2, homeowners who believe that they will get little to no reduction in their taxes owed from appealing, for example, because they perceive the county discriminates against them, will especially benefit from this insurance. If discrimination is a barrier for minority homeowners to appeal, then we would expect the insurance to increase appeals, but positive information about the average benefits in the county alone would not.

lead to a larger financial benefit if successful, but may be less likely to be successful.

However, homeowners who expect to receive a considerable reduction based on the *Appeal Information* content, should not respond to the *Insurance* benefit because it is last-dollar and they expect to receive more than $75 from the government. However, they may still value *Insurance* since it reduces the left-tail risk. We interpret the effects of the *Insurance* treatment through the lens of our conceptual framework to understand the potential role of perceived racial discrimination and additionally test for the extent to which other factors besides discrimination that are correlated with race may matter.

The *Control* group does not receive any additional information or incentives. However, all homeowners in the sample are informed or reminded of the existence of the appeals process and their right to appeal their assessment. The *Control* group therefore has some information frictions resolved compared to homeowners not in our sample.

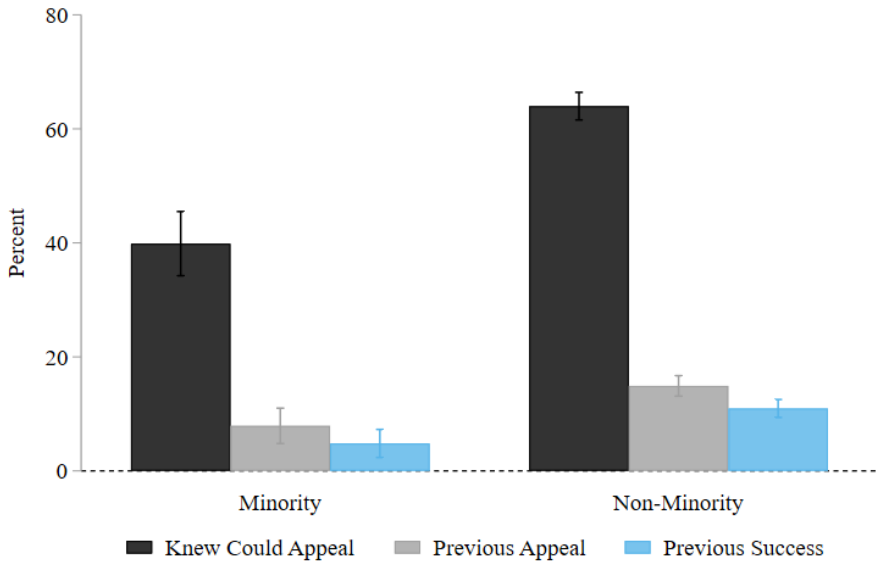
Before randomization, we first elicit homeowner demographics as well as prior beliefs and experiences with the appeals process to measure beliefs and knowledge. All treated homeowners, that is, those not in the *Control* condition, then receive the information on the county appeal success rate and reduction in taxes owed from appealing. Based on that information, we elicit homeowners' posterior beliefs about the appeals process. Finally, we ask homeowners additional questions about their views on taxation and about their appeal intentions. At the end of the survey, homeowners in the *Appeal Information* and *Insurance* conditions are provided with a step-by-step guide to appeal. After the appeal filing deadline, we link the experiment data to administrative property tax records on appeal filings.

3.3 Racial Gaps in Appeals and Knowledge

We first document the racial appeal gap in our sample in terms of both knowledge and past experiences between minority (Black and Hispanic) and non-minority homeowners. Figure 1 presents racial differences in knowledge of the appeals process, previous experience appealing, and previous successful appeals that led to a reduction in taxes owed.

Overall, minority homeowners have less information about the appeals process and

Figure 1: Racial Differences in Knowledge of Appeals and Previous Behavior



Note: This figure presents the past knowledge and experiences with the appeals process, by race, for the 291 minority homeowners and the 1516 non-minority homeowners (of 1844 total) who answered these questions in our survey.

less experience appealing. Minority homeowners are 24 percentage points less likely than non-minority homeowners to have been aware before the experiment that they could appeal their assessed value and 7 percentage points less likely to have previously appealed. We also find that minority homeowners are 6 percentage points less likely have had a successful appeal that reduced their tax burdens. Differences in knowledge of the appeals process may cause minority homeowners to include lower-quality evidence in their appeals, leading them to be less successful. While minority homeowners have less knowledge of and experience with the appeals process, they are also relatively more optimistic about their likelihood of success and of the financial benefits (Table D1). However, homeowners in general understate the actual likelihood of success (73%) and slightly understate the actual reduction in their tax burden (22%).

To better understand the baseline drivers of the appeal gap, in Figure 2, we also present average homeowner appeal behavior, overall and by race, for those randomized into

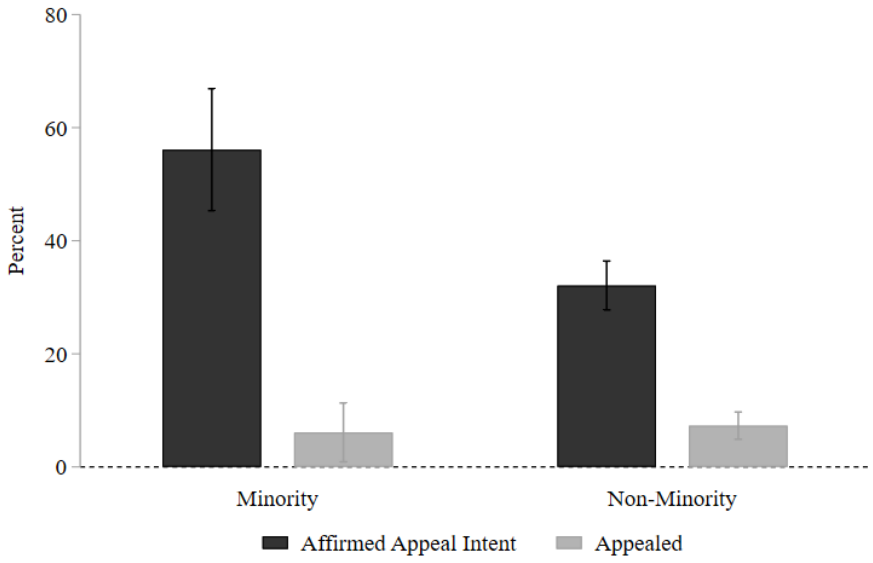
our control group who did not receive information, aid, or financial incentives. Many more homeowners state that they would like to appeal than actually file, 35.8 compared to 7.1%. This difference between intentions and actual filings is especially large for minority homeowners. Minority homeowners are about 24.0 percentage points more likely to affirm that they would like to appeal than non-minority homeowners, perhaps because they believe the financial returns are greater, but they are less likely to actually file an appeal. Our experimental design allows us to measure the extent to which information frictions about the appeals process contribute to this difference.

In addition to appeal behavior, we measure homeowners' perceptions of discrimination and the cost of appealing to understand the drivers of intentions and filings (Table D2). Fewer than 20% of homeowners, regardless of race, strongly or somewhat agree that there is no discrimination by race in the assessment and appeals process. Moreover, minority homeowners are more likely to believe that there is discrimination based on both race and income in the assessment and appeals process. Beliefs about discrimination may therefore contribute to a higher percent of minority homeowners stating that they pay more than their fair share. While we would expect perceptions of discrimination to lead minority homeowners to have higher perceived costs of appealing and to be less likely to affirm their intent, we do not find evidence of this, which suggests a role for other factors.

4. What Are the Drivers of Racial Differences in Homeowner Appeal Behavior?

We measure how homeowners respond to our information, aid, and incentive treatments to understand which factors drive the racial appeal gap. We estimate the effect of each of our treatments relative to the control group, separately for minority, m, and non-minority, n,

Figure 2: Racial Differences in Behavior for Untreated Homeowners



Note: This figure presents the percent of homeowners in the *Control* group, by race, who affirmed their appeal intent in our survey and the percent who filed an appeal with the county. We also include 95 percent confidence intervals. The sample includes 82 minority and 452 non-minority homeowners.

homeowners from the following specification:

$$Y_i = \left[\beta_{1m}\text{Info}_i + \beta_{2m}\text{Appeal Aid}_i + \beta_{3m}\text{Insurance}_i\right] \times \mathbb{1}[\text{Minority}_i] \tag{3}$$
$$+ \left[\beta_{1n}\text{Info}_i + \beta_{2n}\text{Appeal Aid}_i + \beta_{3n}\text{Insurance}_i\right] \times \mathbb{1}[\text{Non-Minority}_i] + X\alpha + \varepsilon_i$$

where Y_i, our outcome of interest, is an indicator that is 100 if the homeowner of race $r = \{m, n\}$ filed an appeal of the assessed value of her home with the county, and 0 otherwise. By randomization, the βs capture the causal effect of our treatments for minority and non-minority homeowners. Since our treatments are nested, β_{2r} gives the effect of both receiving information and receiving aid on how to appeal relative to homeowners of the same race in *Control*, and β_{3r} gives the effect of receiving information, receiving aid on how to appeal, and receiving an added financial incentive to appeal also relative to *Control*. X is a vector

of baseline demographics.[16] Figure 3 presents the treatment effect estimates.

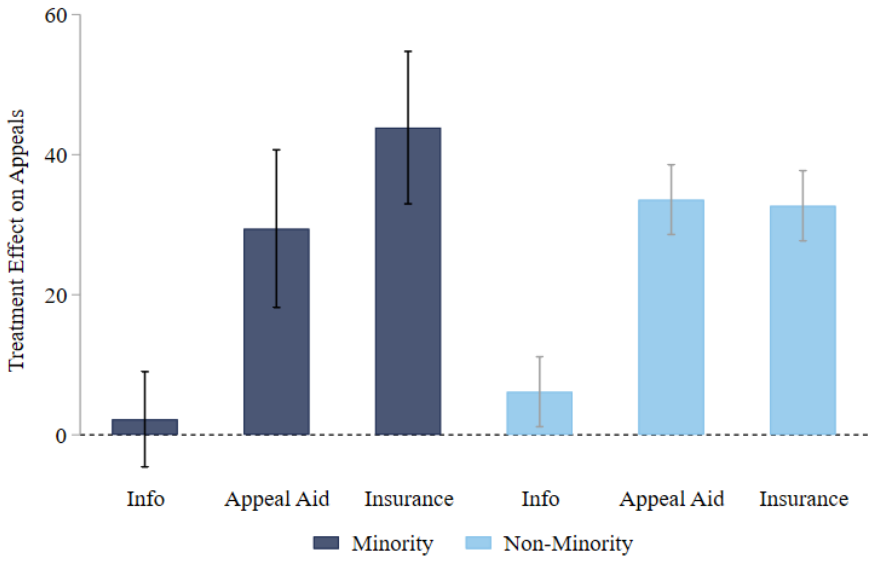
Without any information, aid, or insurance, only 6.1% of minority, and 7.3% of non-minority homeowners in our *Control* group file an appeal—a 16% gap. These appeal rates are large, especially compared to the county overall and the homeowners' behavior in our sample from the previous year. This potentially reflects the implicit treatment of our *Control* condition that, at a minimum, informs homeowners that they are legally able to appeal to reduce their tax burden.

The *Appeal Information* treatment widens the racial appeal gap to 5.1 percentage points or 38.2% since it only leads non-minority homeowners to become statistically significantly more likely to appeal ($p = 0.016$), while the effect on minority homeowners is smaller and not different from 0 ($p = 0.522$). However, we cannot reject that *Appeal Information* had different effects on minority and non-minority homeowners. Based on Equation 1, these effects of information may be consistent with minority homeowners having relatively large filing costs, c, and/or discrimination. We further consider these hypotheses in Sections 4.3 and 4.4.

The *Appeal Aid* condition increases the likelihood that non-minority homeowners appeal by 33.6 percentage points and 29.4 percentage points for minority homeowners ($p < 0.001$ for both minority and non-minority homeowners). Resolving information frictions on how to file an appeal and provide proper evidence substantially increases the fraction of homeowners who appeal by 460% and 482% for non-minority and minority homeowners, respectively. However, *Appeal Aid* has almost no additional effect on the racial appeal gap, over providing *Appeal Information* alone. The appeal gap slightly increases to 5.3 percentage points, but falls as a percent to 13.1%, with the difference driven by the large number of homeowners who are now induced to appeal. Overall, these results suggest that information frictions are an important driver for why few homeowners appeal, but not necessarily for the

[16] X contains indicators for whether the homeowner is a minority, has received at least a bachelor's degree, is female, has a child in public school, and is elderly (at least 65 years old), and is a Republican, as well as the log of her home's assessed value, and her prior beliefs about the financial benefits of appealing for herself and on average for the county.

Figure 3: Treatment Effects on Appeal Filing by Race



Note: This figure presents our treatment effects on the likelihood a homeowner filed an appeal with the county, as well as the 95 percent confidence intervals, separately for minority and non-minority homeowners based on Equation 3. The sample includes all 1844 homeowners in the survey experiment.

racial gap.

Finally, we find that providing *Insurance* only affects minority homeowners. It increases the likelihood a minority homeowner appeals by 43.9 percentage points ($p < 0.001$) compared to minority homeowners in *Control*, and 14.4 percentage points ($p = 0.037$) compared to minority homeowners who received *Appeal Aid*. However, *Insurance* has no added effect on non-minority homeowners over *Appeal Aid* and *Appeal Information*. The relative effect of *Insurance* by race reverses the appeal gap so that minority homeowners are 10 percentage points more likely to appeal than non-minority homeowners, which is closer in line with the racial difference in appeal intentions.

The large response to *Insurance* for minority homeowners, with no response by non-minority homeowners, is consistent with perceived discrimination as a driver of the racial appeal gap, as we discussed in Section 2. Some minority homeowners do not appeal because they believe that the government will discriminate against them and there will be little to no

financial benefit to appeal. However, when provided with an external financial benefit that is not related to their assessor's behavior, they chose to appeal. Consistent with our treatment effects, minority homeowners concerned about discrimination in the appeals process are also not induced to file an appeal from *Appeal Information* since the *average* financial benefits in the county overstate their perceived benefits. Moreover, non-minority homeowners, who do not perceive that they face discrimination, learn from the *Appeal Information* that the expected financial benefits are much lager than $75 and therefore are unlikely to further benefit from *Insurance*. We additionally consider other potential implications and interpretations of this *Insurance* effect in Section 4.1 and find no evidence that it is instead driven by the other factors that are potentially correlated with race.

4.1 Testing Explanations Besides Discrimination for the Effect of *Insurance*

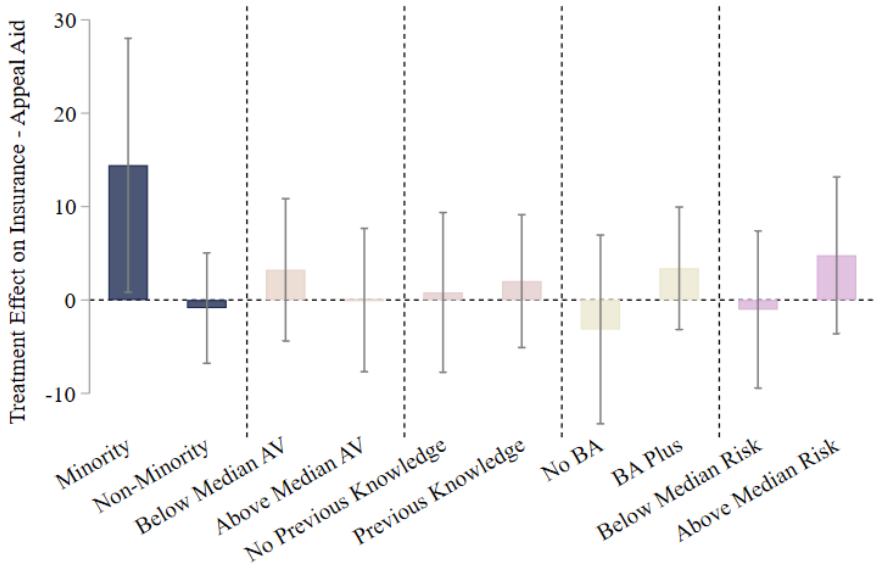
Interpreting the effect of providing *Insurance* through Equations 1 and 2, our results suggest that discrimination is a driver of the racial appeal gap. We now consider alternative explanations for this effect: is the effect is not explicitly related to race but due to correlates of race? Based on the data we collected through our survey and the administrative data, we consider four other mediators for the *Insurance* effect: differences in assessed values, differences in homeowners' knowledge of the appeals process, differences in homeowners' education, and differences in homeowners' risk preferences. For each mediator, we divide the sample into two groups and estimate heterogeneous treatment effects of *Insurance* compared to *Appeal Aid*, analogous to Equation 3.[17] We plot these estimated effects and the effects for minority and non-minority homeowners in Figure 1. We present the full set of treatment effects by homeowner characteristic in Figure C2.[18]

One alternative explanation for the *Insurance* effect is that minority homeowners have lower assessed values, so the financial benefits of the *Insurance* treatment are larger– they

[17]When we estimate the effects of *Insurance* compared to *Appeal Aid*, we include the full sample, but only present this one effect for each subgroup.

[18]We additionally estimate difference-in-differences specifications, comparing the relative effect of *Insurance* over *Appeal Aid* between the groups and present the results in Figure C1.

Table 1: Heterogeneous Effects of *Insurance* by Homeowner Characteristics



Note: This figure presents the estimated treatment effects of *Insurance* relative to *Appeal Aid* and 95 percent confidence intervals for five sets of subgroups on the likelihood that a homeowner files an appeal. We estimate five regressions, similar to Equation 3, but adapted to the relevant subgroup. Different subgroups are separated by vertical dashed lines and colored differently. From left to right, we present the effects for (1) minority and non-minority homeowners; (2) homeowners with below and above median assessed values in our sample; (3) homeowners who stated they did not know before the survey they could appeal, and homeowners who stated they did know; (4) homeowners who do not have a bachelor's degree and those who have at least a bachelor's degree; and (5) homeowners with below and above median risk preferences elicited in the survey.

are less likely to receive at least $75 from the county even if successful. However, we find no difference in the effect of *Insurance* for homeowners with below and above median assessed values. Another explanation is that insurance may especially help homeowners overcome the cognitive costs of filing an appeal by providing an incentive to induce greater effort. However, we show that there is no effect for both homeowners with and without previous appeal-specific knowledge. Additionally, we find no difference in the *Insurance* effect for those with a bachelor's degree (BA) or higher and those without a BA. If anything our point estimates suggest homeowners with a BA respond more, but minority homeowners are less likely to have earned a BA. Finally, we consider whether differences in risk preferences drive our results. *Insurance* can decrease the risk associated with appealing and therefore be especially effective for homeowners who are risk averse. At the same time, *Insurance* is a lottery and therefore may benefit more risk-loving homeowners. However, we find that risk preferences are not correlated with race, and there is no heterogeneity in the *Insurance* effect by risk preferences.

Overall, our results suggest an important role for discrimination. As shown in the first two bars of Figure 1, minority homeowners strongly respond to being provided *Insurance*, even after being given *Appeal Aid* and *Appeal Information*, while non-minority homeowners do not respond at all.[19] Since other factors that are potentially correlated with race do not seem to drive this result, we interpret the evidence as a race-specific effect, namely discrimination.

4.2 Appeal Intentions and Follow-Through

As a supplemental test of the drivers, we combine our survey data on appeal intentions with the administrative data from the county on appeal filings. Since the appeal intentions were elicited before homeowners attempted to file an appeal, difference in intentions and filings shed further light on the drivers, especially information frictions. We therefore consider

[19] In Figure C1, we also show that the difference in the *Insurance* effect between minority and non-minority homeowners is statistically significant ($p = 0.043$).

the effect of our treatments on four additional outcomes: {Affirmed Intent, Did Not Affirm Intent} × {Filed an Appeal, Did Not File an Appeal}. For each of the four outcomes, we estimate Equation 3 and present the estimates in Table 2. Since the outcomes are mutually exclusive and collectively exhaustive, the coefficients sum to 0 within a row. This allows us to understand how our treatments move homeowners across these two behaviors.

While we found that *Appeal Aid* had similar effects on appeal filing for minority and non-minority homeowners, our results in Table 2 suggest that the treatment potentially resolved different frictions for these homeowners. For minority homeowners, *Appeal Aid* mainly reduced the likelihood that they did not follow through on their intentions to appeal. This suggest that information frictions on how to appeal are especially important for minority homeowners—they want to appeal but cannot navigate the process without the step-by-step guide. However, for non-minority homeowners, the effect of *Appeal Aid* on appeal intentions and follow-through is much smaller at about 11 percentage points compared to 28 percentage points for minority homeowners. This suggests fewer non-minority homeowners face large information frictions or costs in filing.[20]

Our results for the effect of the *Insurance* treatment provide additional supporting evidence that perceived discrimination is a driver of the racial appeal gap. Compared to only providing *Appeal Aid*, *Insurance* mainly works for minority homeowners by moving them from not affirming their intent and not appealing to both affirming their intent and filing an appeal ($p = 0.032$ for test of *Insurance* × *Minority* = *Appeal Aid* × *Minority* in Column 4). Even after resolving information frictions, minority homeowners may still face larger appeal costs, c, from interacting with a discriminatory government, or smaller benefits in the potential reduction in taxes owed. However, *Insurance* increases their financial returns to appealing and therefore the effort they would be willing to exert to appeal before they see the step-by-step guide, increasing their appeal intentions. We do not find an analogous

[20] *Appeal Aid* also led more minority and non-minority homeowners who did not affirm their intentions to appeal to file an appeal anyway, highlighting the importance of information frictions in general. These homeowners may have believed that appealing was not worth it because the costs were too high, but then after seeing the step-by-step guide, which greatly reduced that cost, chose to appeal.

Table 2: Treatment Effects on Appeal Intentions and Follow-Through by Race

	(1) Intent & Appeal	(2) Intent & No Appeal	(3) No Intent & Appeal	(4) No Intent & No Appeal
Info × Minority	2.23	-4.55	-0.01	2.33
	(3.40)	(8.66)	(0.66)	(8.50)
Appeal Aid × Minority	21.28***	-28.04***	8.16***	-1.39
	(5.39)	(6.97)	(2.99)	(6.89)
Insurance × Minority	41.22***	-28.78***	2.64	-15.08**
	(5.56)	(6.71)	(1.65)	(6.35)
Info × Non-Minority	5.56**	-0.31	0.60	-5.84
	(2.32)	(3.49)	(1.13)	(3.70)
Appeal Aid × Non-Minority	24.48***	-11.00***	9.09***	-22.57***
	(2.38)	(2.53)	(1.51)	(2.87)
Insurance × Non-Minority	30.51***	-9.37***	2.19**	-23.33***
	(2.50)	(2.55)	(0.94)	(2.86)
N	1844	1844	1844	1844

Note: This table presents treatment effect estimates by race on four outcomes corresponding to {Affirmed Intent, Did Not Affirm Intent} × {Filed an Appeal, Did Not File an Appeal}. For example, Column (1) presents our treatment effects from Equation 3 where the outcome is 100 if the homeowner "Affirmed Intent" and "Filed an Appeal", and 0 otherwise. Heteroskedastic robust standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.01$

effect for non-minority homeowners who do not face discrimination and therefore have a smaller financial benefit from *Insurance*. They are instead moved in this way by *Appeal Information*.[21]

4.3 Appeal Costs

Our results suggest that both racial differences in information frictions and perceived discrimination contribute to the racial appeal gap. We further consider these mechanisms by examining racial differences in homeowners' perceived cost to appeal. Homeowners who perceive the appeals process to be costly in terms of their time and effort, because they have little information about how to appeal, should state that appealing is only worth the effort if they would receive a larger reduction in their taxes. Moreover, if minority homeowners face a cost of interacting with a discriminatory government, then they will have higher perceived costs as well.

[21]Since the only effect of *Insurance* relative to *Appeal Aid* for non-minority homeowners is in Column (3), this suggests that it only affects homeowners would would have appealed anyway.

Through the survey, we asked: "Suppose we could guarantee your tax bill would be reduced if you appeal. What is the smallest reduction in your bill (in dollars) that you would need to appeal?" Based on our conceptual framework and Equation 1, this question elicits a homeowner's perceived cost to appeal. We asked this question before offering *Insurance* so that homeowners only considered the potential reduction in taxes through the government appeals process. We therefore pool the *Appeal Aid* and *Insurance* treatments into an *Appeal Plus* group since, at that point in the survey, the two treatments were identical. Homeowners were asked for their perceived cost after choosing their comparable home for the appeal, but before seeing the step-by-step guide on how to appeal.

We plot the cumulative distribution function (CDF) of the responses normalized by each homeowner's taxes owed by treatment, separately by race, in Figure 4.[22] The normalization coverts the perceived cost to appeal to a percent of the total tax burden. This allows a better comparison across homeowners and relates it to our *Appeal Information* treatment in which homeowners learn the average percent reduction in taxes owed from a successful appeal. On average, across our whole sample, minority homeowners have higher perceived costs by 8.0 percentage points or 42.4% ($p < 0.001$)).[23]

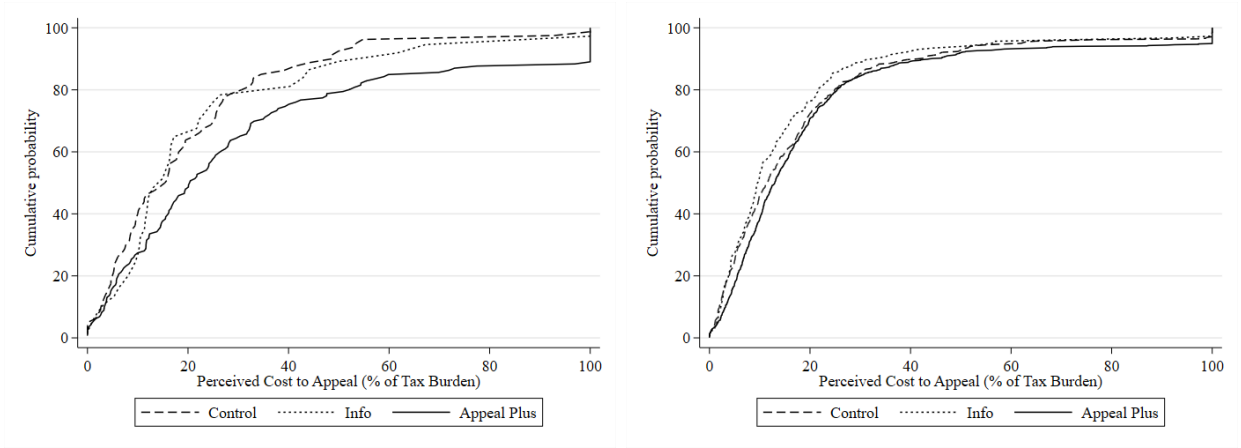
We find no effect of providing information about the average likelihood of success and financial benefits from appealing on homeowners' perceived costs to appeal (Wilcoxon rank-sum test $p = 0.381$ of *Information* compared to *Control*), overall or for minority and non-minority homeowners. This provides some validation that homeowners understood the survey question. The question intends to measure $c(\cdot)$, and so information about the benefits should not directly affect their responses.

However, walking homeowners through the appeals process, including picking their comparable home, shifts the distribution to the right so that these homeowners have higher perceived costs to appeal. This pattern holds for both minority and non-minority home-

[22]We censor the CDF at 100% of the tax burden since homeowners could not reduce their tax burden below 0. About 4.8% of homeowners say that they would need to receive at least their taxes due to appeal.

[23]We additionally find that the distributions are different based on a Kolmogorov–Smirnov test ($p < 0.001$).

Figure 4: Perceived Cost to Appeal



(a) Minority Homeowners

(b) Non-Minority Homeowners

Note: Each panel presents the empirical CDF for homeowner's perceived cost to appeal as a percent of their total tax burden. The CDFs are truncated at 100% of the taxes owed. Panel (a) shows the CDF for minority homeowners, and (b) for non-minority homeowners.

owners, although the magnitude of the effect is larger for minority homeowners.[24] Taken together, the differences in both the perceived costs of appealing, and how homeowners respond to learning about the appeals process by picking comparables suggest information frictions are likely more important for minority homeowners.

Since we find that having homeowners, especially minority homeowners, pick comparables affects their costs of appealing, we additionally consider whether this mechanism explains our *Insurance* effect result. In that the case, the *Insurance* result suggests that that racial differences in the cost from navigating the appeals process matter for the appeal gap, but not for discrimination. We test this potential mechanism by estimating Equation 3 and controlling for homeowners' stated cost of appealing. The estimated effect of *Insurance* over *Appeal Aid* for minority homeowners remains essentially unchanged—itslightly increases by 0.08. This implies that the *Insurance* effect is not driven by changes in the perceived cost of appealing by race.

[24]The Kolmogorov–Smirnov statistics comparing *Appeal Plus* to *Control* for minority homeowners is 0.176 ($p = 0.081$) and for non-minority homeowners is 0.091 ($p = 0.023$). If we compared *Appeal Plus* to *Appeal Information*, the racial differences in responses in much smaller—0.210 versus 0.154.

4.4 Beliefs about financial benefits from appealing

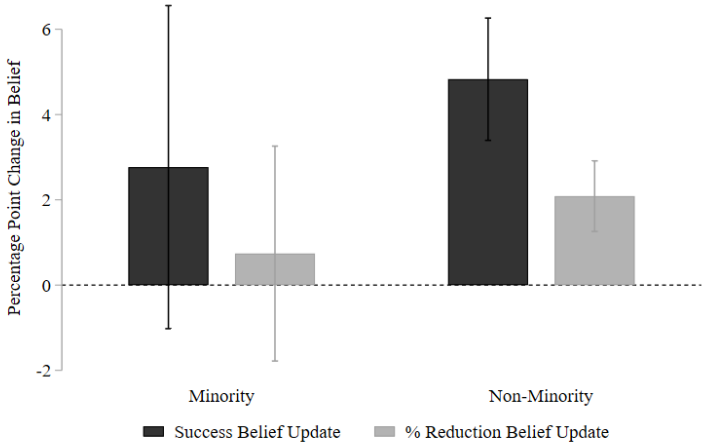
As an additional test of the drivers of the racial appeal gap, we consider how homeowners' beliefs about the financial benefits of appealing change with *Appeal Information*. If minority homeowners perceive that they face discrimination, then they will not change their beliefs about their own financial benefits when learning, for example, that the average homeowner has a higher chance of filing a successful appeal. However, if we instead find that minority homeowners update their beliefs after receiving *Appeal Information* at least as much as non-minority homeowners, but they are not induced to file an appeal, as we show in Section 4, then that would suggest information frictions on how to appeal are an important driver.

In the survey, we elicited all homeowners' prior beliefs about the likelihood that their appeal would be successful and the percent reduction in taxes owed if successful—as highlighted in Equation 1. We additionally elicited the posterior beliefs for treated homeowners who received information about the average statistics in the county. We present the average changes in beliefs, by race, in Table 3.

On average, minority homeowners do not change their beliefs about the likelihood of success or the potential reduction in taxes owed after receiving *Appeal Information*, but we see larger and more precise belief updating among non-minority homeowners. However, we are unable to reject that minority and non-minority homeowners update differently from this information.[25] Taken together with our earlier findings that only *Appeal Information* increases the likelihood that non-minority homeowners file and appeal, these results suggest that both perceived discrimination and information frictions contribute to the racial appeal gap.

[25]We additionally find larger differences in how minority and non-minority homeowners respond to news that the financial benefits are lower (Figure C1). For example, minority homeowners who learn that the percent reduction in taxes due after a successful appeal is lower than initially perceived revised their beliefs down by more than non-minority homeowners ($p = 0.094$).

Table 3: Homeowners' Change in Beliefs about Financial Benefits from *Appeal Information*



Note: This figure presents the change in homeowners' beliefs after receiving information about the average financial return from appealing in Marion County, by race. The black bars present the average difference of homeowners' posteriors minus their priors of their own likelihood of having a successful appeal, by race. The gray bars present the average difference of homeowners' posteriors minus their priors about their own percent reduction in taxes owed from a successful appeal, by race. The sample includes the 1717 homeowners who are in the *Appeal Information*, *Appeal Aid*, and *Insurance* treatments.

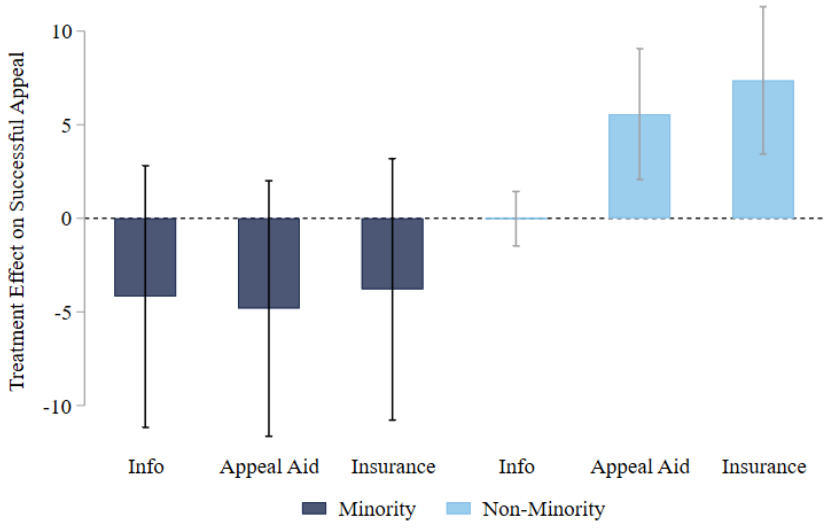
5. Racial Differences in Appeal Outcomes

If the main goal of the appeals process is to correct errors in the assessment process, then reducing the racial appeal gap should reduce the racial assessment gap. However, if there is also discrimination in the appeals process, as a majority of homeowners believe, then our treatments that increase the total number of homeowners who appeal can, instead, exacerbate the assessment gap. To consider this, we estimate the treatment effects, as in Equation 3, on whether the homeowner had a successful appeal that led to a reduction in her assessed value. We consider intention-to-treat effects so that homeowners who do not appeal, mechanically, do not have a successful appeal, and present the results in Figure 5.

Due to the timing of our experiment and the nature of the appeals process, we consider whether a homeowner in our April 2022 sample had a successful appeal by March 1, 2023. At this point, only about one-third of submitted appeals were finalized. While the current results may change as more appeals are resolved, this outcome reflects homeowners' information at

the time they receive their 2023 tax bill.

Figure 5: Treatment Effects on Successful Appeal



Note: This figure presents our treatment effects on the likelihood a homeowner filed an appeal that was successful and led to a reduction in taxes owed as of March 2023, as well as the 95 percent confidence intervals, separately for minority and non-minority homeowners based on Equation 3. The sample includes the 983 homeowners in the 2022 wave of the survey experiment.

While our treatments, especially *Insurance* and *Appeal Aid*, increase the number of minority and non-minority homeowners who appeal, the treatment only increase the likelihood of having a successful appeal for non-minority homeowners. Non-minority homeowners in those two treatments are about twice as likely to have their tax burdens reduced than homeowners in *Control*. Our results, therefore, indicate that the appeals process currently widens the racial assessment gap, potentially undermining its intended policy goal.

Our results imply that there is discrimination in the appeals process. If these differences are due to discrimination, the discrimination could be animus or statistical on the part of the assessors. The discrimination could be systemic if, for example, the racial differences in appeal success were related to how the housing market in minority and non-minority areas shapes the stock of available comparables homes that can be used as evidence. However, it could also be due differences in appeal content or quality, which we consider next.

5.1 Racial Differences in Appeal Quality

We now consider whether the racial difference in appeal success is driven by how different types of homeowners appeal and the evidence they provide. Through the appeals process, as in our survey, homeowners must select a comparable home as evidence that their assessment is above fair market value. Homeowners who select relatively small or inexpensive homes are potentially less likely to have their appeals approved, but those who select homes that are too expensive will not receive a large reduction in their taxes owed even if their appeals are successful.

During the survey, homeowners in the *Appeal Aid* and *Insurance* treatments are given five options of real homes to use as evidence in the appeal. Homeowners pick one comparable based on a list of attributes, like sale prices. We construct their choice sets so that each home is a viable appeal option, based on our discussions with the county administrators. We additionally ensure that the homes' recent sale price is less than the participant's assessed value, so that it could lead to a reduction in taxes owed, and that the home is not too small, based on the square footage, and could therefore lead to a successful appeal. We then estimate a multinomial logistic choice model to understand what factors homeowners find important and to see how different homeowners approach the appeals process. Table 4 presents the multinomial logit coefficient estimates from the choice model for minority homeowners in Column (1) and for non-minority homeowners in Column (2). We additionally present the difference between the estimated coefficients for minority and non-minority homeowners in Column (3).

Our results confirm that homeowners, on average, understand what constitutes appropriate evidence for a successful appeal. Homeowners choose a comparable that is in their neighborhood. We also find that homeowners pick relatively expensive homes from options in which all sale prices are less than their assessed value. This suggests that they acknowledge that a comparable with a too-low sale price has a lower probability of counting as high-quality evidence. Perhaps surprisingly, homeowners choose a comparable that is relatively

small based on its square footage from options in which the size is no less than 85% of their own home's size. Homeowners are therefore picking a comparable that is a similar size to their own, conditional on the sale price, whereas a large home would provide better evidence that their home's assessment was too high.

Table 4: Determinants of Taxpayer Choice of Comparable by Race

	(1) Minority	(2) Non-Minority	(3) Difference
Distance	-0.28**	-0.27***	-0.0071
	(0.13)	(0.058)	(0.14)
Effective Year Built	-0.0024	-0.00040	-0.0020
	(0.0057)	(0.0026)	(0.0062)
Year Built	0.0064	0.0067***	-0.00026
	(0.0059)	(0.0025)	(0.0064)
Same Neighborhood	0.29	0.74***	-0.44
	(0.30)	(0.13)	(0.32)
Log Sale Price	1.26	0.80*	0.47
	(0.88)	(0.45)	(0.99)
Number of Bathrooms	0.17	0.12	0.049
	(0.20)	(0.091)	(0.22)
Number of Bedrooms	-0.057	0.18**	-0.23
	(0.16)	(0.079)	(0.18)
Log Home SQFT	-0.096	-1.09***	0.99*
	(0.48)	(0.25)	(0.54)
N	775	4015	4790

Note: The table presents the multinomial logit coefficients from the choice model for how homeowners pick their comparable homes as evidence for the appeal. Column (1) includes minority homeowners in the *Appeal Aid* and *Insurance* treatments who select a comparable home. Columns (2) includes non-minority homeowners in the *Appeal Aid* and *Insurance* treatments. Column (3) presents the estimated difference between minority and non-minority homeowners. Each observation is an individual-option so that each individual in the sample appears five times. Standard errors are clustered at the individual level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Moreover, our results in Table 4 indicate that there are minimal differences in how minority and non-minority homeowners choose evidence for their appeals. The only marginally significant difference is that minority homeowners pick larger houses as comparables. If anything, we would therefore expect that minority homeowners provide less risky evidence

because this suggests the comparable is more similar to their home than the comparables non-minority homeowners choose. Together with point estimates on the sale price, this could help explain why previous research has found that minority homeowners receive lower discounts when they appeal, but it does not explain why they are less likely to be successful in the appeals process.[26]

Since we fine no differences in how homeowners choose evidence, or the quality of the appeal, discrimination likely plays a role for the racial differences in the likelihood of appeal success. First, assessors may discriminate in the appeals process against minority homeowners, as many minority homeowners believe (Table D2). In Section 7, we conduct a correspondence study to more formally consider this channel. Additionally, systemic discrimination may make it more difficult to evaluate the appeals of minority homeowners, if they live in areas where the evidence itself is qualitatively different, for example, if there are fewer comparable sales. Based on our sample from 2022, minority homeowners who appealed are about 20% less likely to have their appeals finalized than non-minority homeowners, but given our small sample, this difference is not statistically significant.

6. The Gender Property Tax Appeal Gap

In addition to the racial gap, there is also a gender gap in property tax appeals. Female homeowners in our *Control* group are 43% less likely to file an appeal than men (4.8% of female, and 8.4% of male homeowners appeal).[27] This gender difference in appeals is consistent with gender differences in negotiation more broadly (Recalde and Vesterlund, 2023). To consider the drivers of the gender property tax appeal gap, we estimate our treatment effects as in Equation 3, but by gender instead of race, and present the results on appeal filing in Panel (a) of Figure 6.

Our results indicate that beliefs about the net benefits from appealing explain the

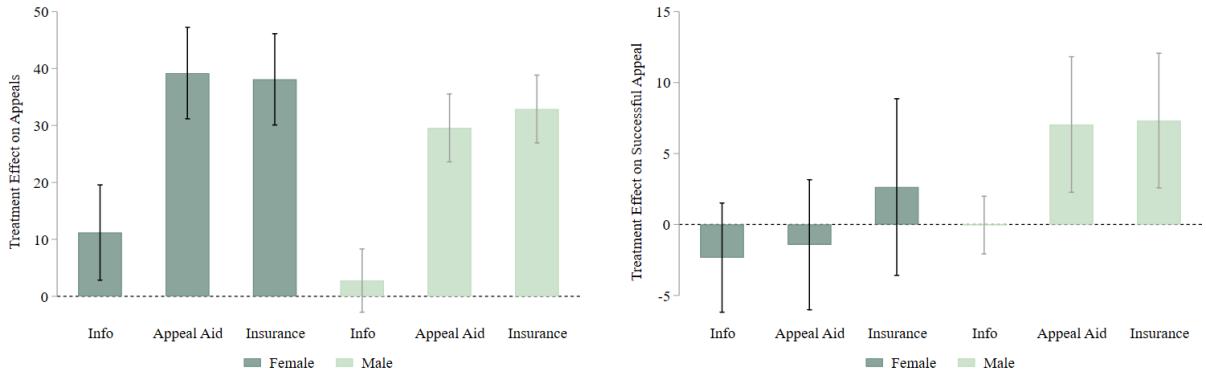
[26]In particular, we find that homeowners without previous knowledge of the appeals process pick less expensive homes than homeowners with previous appeal knowledge ($p = 0.012$).

[27]We impute gender for our sample based on the name of the person we contact to participate in our experiment. We exclude couples from this analysis.

gender appeal gap. Female homeowners in our *Appeal Information* treatment are about 11 percentage points, or about 233%, more likely to file an appeal than females in *Control*. However, we find that males do not respond; the effect is only 2.8 percentage points and not different from 0.[28] This reverses the gap so that, after providing information about the financial returns, female homeowners are 4 percentage points more likely to appeal, but this difference is not statistically different from 0 ($p = 0.343$).[29]

We do not find evidence that information frictions on how to file or perceived discrimination drive the gender gap. Additionally providing *Appeal Aid* on top of *Appeal Information* increases the likelihood that both female and male homeowners file an appeal by about 27 percentage points. Finally, we find no added effect of providing insurance over appeal aid.

Figure 6: Gender Disparities in Property Tax Appeals



(a) Treatment Effects on Appeal Filing

(b) Treatment Effects on Appeal Success

Note: Panel (a) presents our treatment effects on the likelihood a homeowner filed an appeal with the county, as well as the 95 percent confidence intervals, separately for female and male homeowners, based on Equation 3. The sample includes all 1844 homeowners in the survey experiment. Panel (b) similarly presents the treatment effects for the likelihood a homeowner has a successful appeal that leads to a reduction in taxes owed, for the 983 homeowners in the 2022 wave of the survey experiment.

While our treatments induce at least as many female homeowners to file an appeal as males, only males see a reduction in their tax burden (Panel (b) of Figure 6). This difference is not driven by differences in appeal quality, this is, how female homeowners pick evidence

[28]The difference between women's and men's responses to *Appeal Information* is 8.4 ($p = 0.084$).
[29]We additionally present treatment effects on appeal intentions in Table D4.

compared to men (Table D3). This suggests a role for discrimination in the appeals process.

7. Experiment 2: A Correspondence Study with County Assessors

The results from the survey experiment suggest that perceptions of discrimination contribute to the racial gap in property tax appeals and that women face similar issues as racial minorities. We also provide evidence of discrimination against homeowners in how the appeals are evaluated. To further understand discrimination, we conducted a complementary correspondence experiment with county assessors.

In this experiment, we posed as homeowners and emailed the county assessors across several states.[30] Every email requested information on how to appeal their property tax assessment, the appeal deadline, and additional advice on best appeal practices. We signaled race and gender using the homeowner's first name, similar to other correspondence studies (Bertrand and Mullainathan, 2004; Butler and Broockman, 2011; Costa, 2017; Montoya et al., 2020).[31] The name appeared in the email address and the body of the email.

In response to the emails, we measure whether the assessor responded to the requests from each fictitious homeowner. Assessor responses are useful for a few reasons. First, they are an important avenue through which homeowners can obtain information about how to appeal their property taxes. The results from Nathan et al. (2020) and our evidence suggest that information frictions are a substantial barrier to appealing. Second, studying the propensity of assessors to provide information to homeowners allows us to understand inequities in providing resources to minorities, which may contribute to systemic discrimination. Finally, email responses are a straightforward task that is homogeneous across counties and allows us to measure differential treatment absent confounding variables.

Differences in response rates by fictitious homeowner identity indicate discrimination

[30] In this experiment, we used deception and imposed time costs on the subjects which they were not compensated for and could not choose to opt out of. In Appendix G, we discuss the ethical considerations of the study using the framework of Asiedu et al. (2021).

[31] This approach differs from other methods designed to measure discrimination such as blinding (Uchida, 2021) or the incentivized resume rating method (Kessler et al., 2019).

in the treatment of homeowners. Differences in the content of the responses can indicate overall discrimination and discrimination in access to information or knowledge about appeals, and additionally reflect how much time and effort the assessors spent helping different types of homeowners. In this section, we first develop a simple model demonstrating how statistical and animus discrimination could lead to differences in treatment by assessors and then map that model to our experimental design. Finally, we present the results of this experiment.

7.1 Conceptual Framework

We consider an assessor's decision of whether and how to help a homeowner of race r and gender g appeal her assessed value. We model the assessor's objective as ensuring the true assessed value, V_A, is as close to the market value, V_M, as possible. This objective is chosen to align with state laws requiring assessors to attempt to assess homes as accurately as possible. For example, under Indiana State Statute IC 6-1.1-37-2, an assessor commits a Class A misdemeanor if she"knowingly assesses any property at more or less than what the official or representative believes is the proper assessed value of the property."[32] Assessors therefore face a penalty if the values are different, $\psi(V_A - V_M)$, which could be eliminated if the homeowner successfully appeals so that $V_{A'}$ equals V_M.

However, helping homeowners in the appeals process involves a cost of the assessor's time, $c_R(e)$, that depends on the advice provided over email, e. In our setting, this cost is both for the email response and time spent handling an appeal if the homeowner files one. Under animus discrimination, assessors face an additional cost of interacting with non-white male homeowners, given by $\rho(d_{rg})$ that varies with race and gender. The assessor will help the homeowner appeal by responding, $R_{r,g} = 1$, if:

$$R_{rg} = \mathbb{1}\{\underbrace{\mathbb{E}[\psi(V_A - V_M|r,g)] - \rho(d_{rg})}_{\text{Expected net benefit}} > \underbrace{c_R(e)}_{\text{Cost to respond}}\} \tag{4}$$

[32]The punishment for a Class A misdemeanor is imprisonment for not more than one year, and a fine of not more than $5000.

Moreover, the assessor must also chose the content of the email, e.

Based on Equation 4, a racial gap in responses could come from animus or statistical discrimination. Animus discrimination, $\rho(d)$, lowers the net benefit of responding to non-white homeowners, making it less likely that the expected net benefit will exceed the cost of writing a helpful response. Alternatively, if the cost of providing advice is increasing in the length of the interaction, assessors who respond will provide less helpful advice to minority homeowners when race is an argument in their cost function. A racial gap could also arise from statistical discrimination when assessors believe that minority homeowners are less likely to have an assessed value that is different from the market value. Although this belief would be incorrect (Avenancio-León and Howard, 2022b), it may be consistent with the fact that appeals by minority homeowners are less likely to be successful and other evidence of inaccurate statistical discrimination (Bohren et al., 2019).

We designed our experiment to help identify the presence of animus or statistical discrimination. Specifically, we cross-randomized the perceived homeowner identity with whether the homeowner included evidence in their email that $V_A > V_M$.

7.2 Experimental Design and Data

We conducted the correspondence study in five states with appeal deadlines between mid-June and mid-September—Florida, Idaho, Indiana, Missouri, Nebraska, and Pennsylvania. Across these states, there are 478 counties. We exclude counties where we cannot find appropriate contact information (e.g., only a phone number is listed) and several counties with appeal deadlines outside of this time window, giving 410 counties.

Assessors in our sample received six emails: one each from {white, Black, Hispanic} × {male, female} homeowners. We picked first names to signal race and gender from Tzioumis (2018), who uses data from the Loan Application Registers, which is required by the Home Mortgage Disclosure Act, to classify names.[33] We additionally randomized half of the emails

[33]We only include first names so that county assessors would not be able to try to identify an individual based on the full name.

to indicate that the homeowner had valid evidence that the assessed value was too high based on a recent appraisal or based on comparable home sales found online (Zillow or Redfin).[34]

For each email, we observe whether the assessor responded to the fictitious homeowner and the content of the response, if any, before the appeal deadline. Based on the content of our emails and as specified in our pre-analysis plan, we note whether a response included information about the deadline, whether the appeal form was attached, and whether the assessor offered additional aid for the appeal.[35] Finally, based on online photos from county assessor websites, we coded the gender and race of each assessor. A full description of the experimental design appears in Appendix E.

7.3 Results

We estimate racial and gender differences in assessor responses by homeowner characteristics from:

$$R_{cf} = \gamma_0 + \sum_d \gamma_d D + \lambda_s + \varepsilon_{cd} \tag{5}$$

where R_{cd} is 100 if the assessor from county c in state s responded to the email from a fictitious homeowner f of demographic, d, and 0 otherwise. D is an indicator that is 1 if homeowner f is of demographic d. We additionally include state fixed effects, λ_s. In each specification, we focus on discrimination by either race or gender.

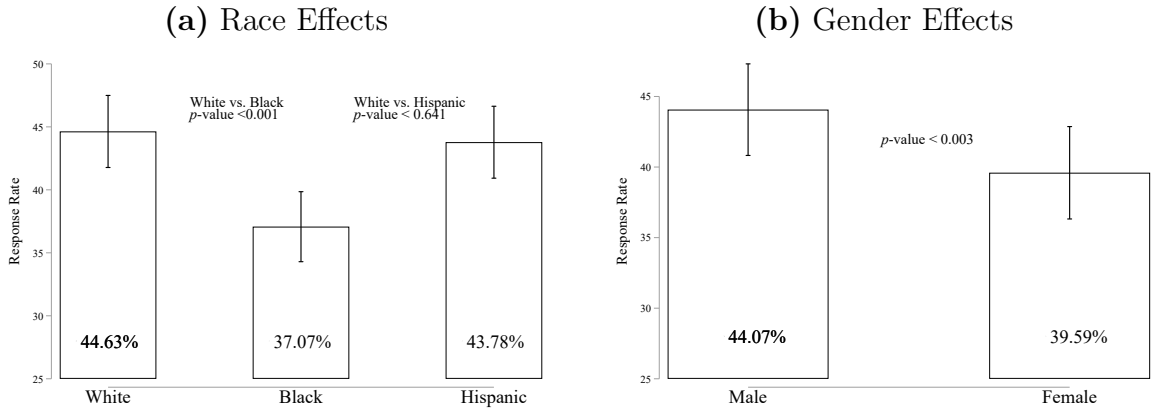
Overall, 42% of assessors responded to our emails. Figure 7 shows racial and gender differences in whether and how assessors responded to homeowners. Panel (a) shows that assessors responded to white and Hispanic homeowners at roughly the same rates (45 and 44%, respectively). However, Black homeowners received 17% (8 pp) fewer responses than white homeowners ($p < 0.001$). Similarly, Panel (b) shows that female homeowners receive

[34] We also randomize the six email templates and the order we send the emails.

[35] More specifically, we code aid as whether the response included information on how to appeal (e.g., how to provide evidence), a request for the homeowner to contact the assessor, or a request for additional information from the homeowner.

10% (4 pp) fewer responses than male homeowners.

Figure 7: Effect of Race and Gender on Response Rate



Note: This figure displays the average response rates to fictitious homeowners in the correspondence experiment. Panel (a) displays the average response rates by the intended race of the fictitious homeowners. Panel (b) displays the average response rate by the intended gender of the fictitious homeowners. Both panels also display 95% confidence intervals and p-values of tests of differences by fictitious homeowner identity. Standard errors are clustered by county.

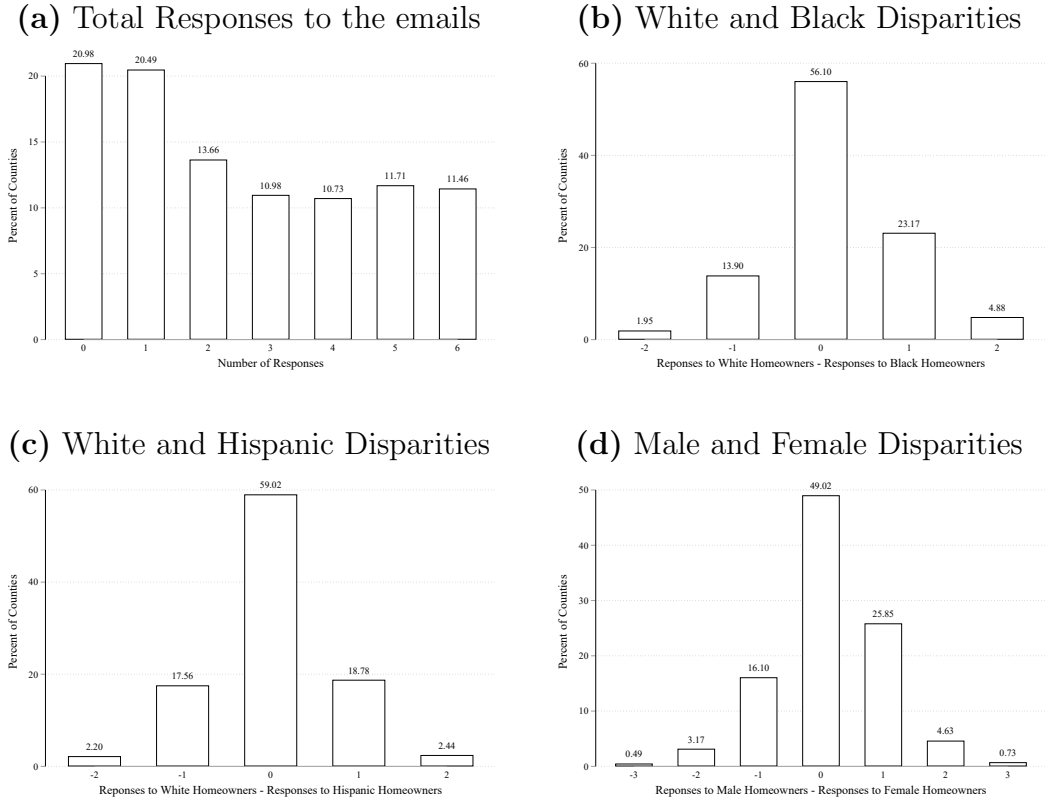
Because we sent multiple emails to each assessor, we have within-assessor variation and can examine the full distribution of treatment effects (Czibor et al., 2019). That is, we can also examine the disparate treatment of female and minority homeowners from the same assessor.[36] Panel (a) of Figure 8 shows the distribution of the number of responses to emails for each county. In about a third of counties, assessors responded to either all or none of the emails. In the remaining two-thirds, assessors responded to some, but not all, of the emails. Indeed, the modal outcome is for assessors to choose to respond to a single one of the six emails.

Panels (b) through (d) show the distribution of within-assessor treatment effects by fictitious homeowner identity. For example, because we sent 4 emails to each assessor from white or Black homeowners, the difference between emails to white or Black homeowners can range from –2 to 2. Positive values indicate that the assessors responded to more emails from white homeowners than Black homeowners, and negative values indicate that assessors

[36]Appendix Section F rules out the possibility that sending multiple emails to each assessor causes us to conclude discrimination from assessors spuriously.

were more likely to respond to Black homeowners. Similarly, we sent three emails from male and female homeowners, so these effects range from –3 to 3, with positive values indicating that assessors are less likely to respond to female homeowners. The results are largely in line with Figure 7. The same assessors who responded to emails from white or male homeowners chose not to respond to similar emails from Black or female homeowners.

Figure 8: Distribution of Within-County Response Rates by Homeowner Type



Note: Panel (a) displays the frequency of the number of responses from each assessor. Each assessor could respond up to six times. Panel (b) displays the difference in the number of responses to fictitious white and Black homeowners. Panel (c) displays the difference in the number of responses to fictitious white and Hispanic homeowners. Panel (d) displays the difference in the number of responses to fictitious male and female homeowners.

Next, based on our model of assessor behavior, we turn to evaluating whether the discrimination we find is due to statistical discrimination or animus. We estimate a version of Equation 5, where we additionally interact the indicators for homeowner demographics with whether the email included evidence that the assessed value was too high. If assessors

statistically discriminate, then evidence should shrink the response gap. However, if assessors discriminate based on animus, then evidence should have no effect. We present our results in Figure 9. The figure includes the estimated fraction of homeowners who receive a response by race and whether the email included evidence, as well as p-values of the test that the response rates are equal for homeowners by race or gender, separately by whether the email contained evidence.

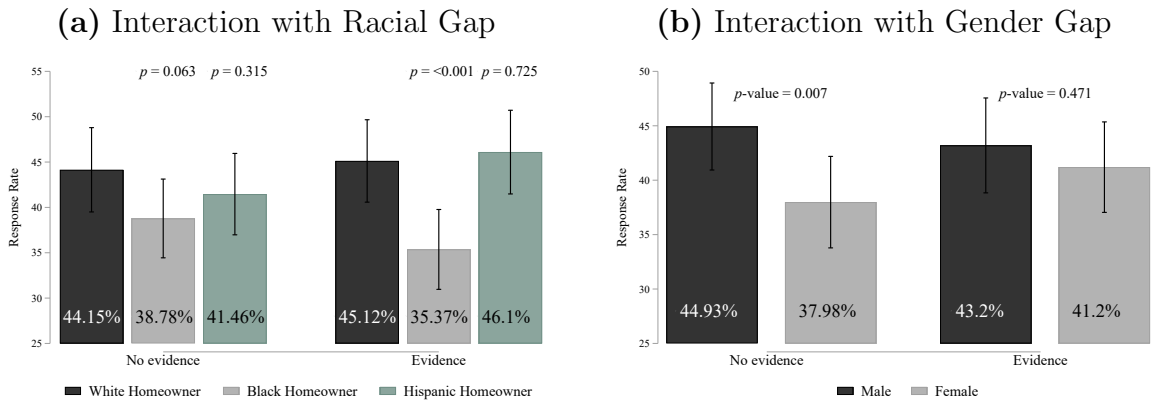
Overall, we find no effect of providing evidence on the response rate of assessors. Evidence increases response rates by about 2% ($p = 0.627$), and we do not find that including evidence reduces the gap in responses between Black and white homeowners.[37] However, providing evidence leads to a 8.5% increase in response rates for female homeowners and closes the gender gap by more than 70%. While evidence considerably reduces the magnitude of the gap and the gender difference in responses when providing evidence is no longer significant, the differential effect of evidence for female homeowners is not statistically different from 0 ($p = 0.261$). Through the lens of the model, these results provide some suggestive evidence that discrimination against Black homeowners is due to animus discrimination while discrimination against female homeowners is due to statistical discrimination.

Finally, we turn to examining descriptive heterogeneity by assessor sex. Out of the 410 assessors in our sample, 62% are female. We estimate versions of Equation 5 in which we additionally allow for heterogeneity by the assessor's sex. We find that both male and female assessors discriminate against Black homeowners, while only female assessors discriminate against female homeowners.

Based on these results, we reconsider how effective providing evidence is at reducing gaps by assessor sex. For example, since our results indicate that male assessors do not discriminate against female homeowners, we would not expect them to be induced to respond relatively more to female homeowners when both provide evidence—because there is no

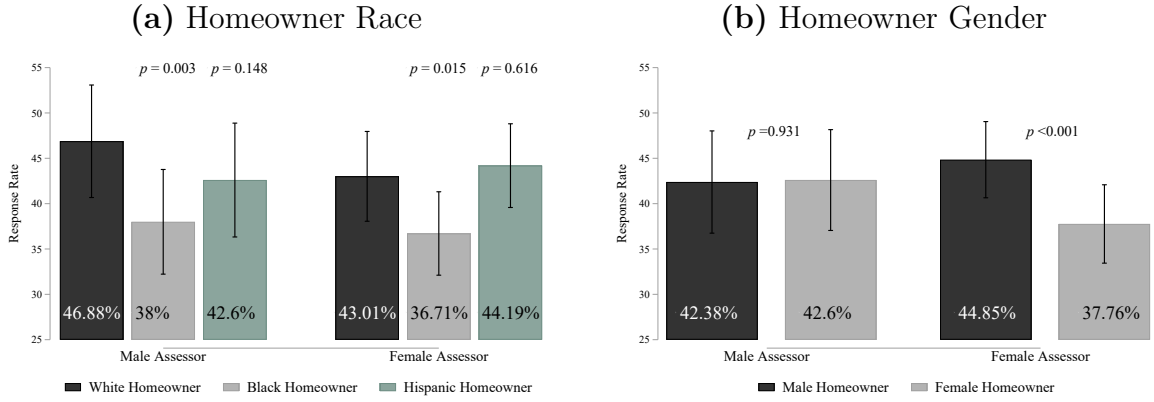
[37]When they do not provide evidence, Hispanic homeowners are 3 pp less likely to receive a response, and this gap completely closes when homeowners include evidence, but the differential effect on evidence for Hispanic homeowners is imprecise.

Figure 9: Effect of Evidence



Note: This figure displays the average response rates to fictitious homeowners in the correspondence experiment. Panel (a) displays the average response rates by the intended race of the fictitious homeowners and whether the homeowner provided evidence to the assessor that their assessment was incorrect. Panel (b) displays the average response rate by the intended sex of the fictitious homeowners and whether the homeowner provided evidence to the assessor that their assessment was incorrect. Both panels also display 95% confidence intervals and p-values of tests of equality of response rates for each group relative to white homeowners in panel (a) and male homeowners in panel (b), conditional on whether the email contained evidence.

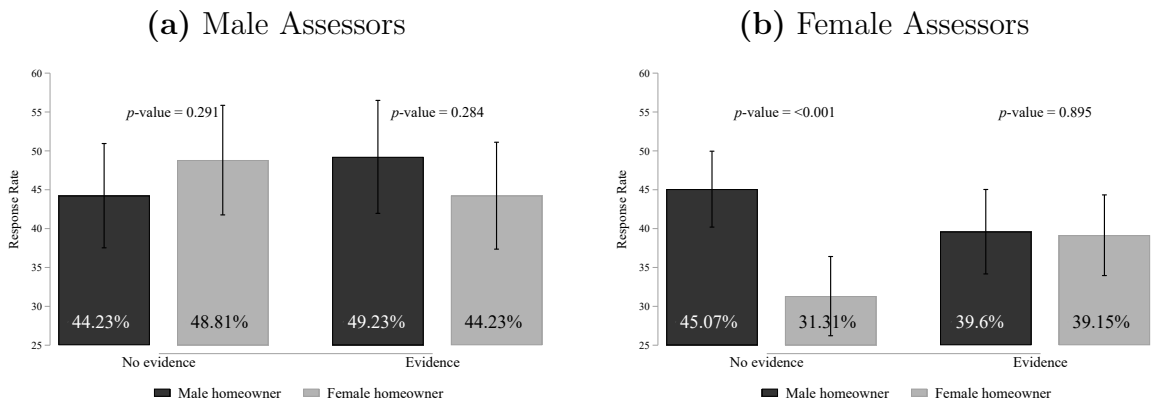
Figure 10: Response Differences by Assessor Gender



Note: This figure displays the email response rates for different types of fictitious homeowners, by the sex of the assessor. In panel (a), we present the response rates by assessor gender and homeowner race. The p-values correspond to tests that the response rate by male and female assessors are equal for white homeowners, and Black or Hispanic homeowners. In panel (b), we present the response rates by assessor gender and homeowner sex. The p-values correspond to tests that the response rate by each male or female assessors are equal for male and female homeowners. In all specifications, we include state fixed effects and cluster the standard errors by county.

gender gap in responses, evidence cannot close it. We would instead expect evidence to have a bigger impact on the behavior of female assessors. We therefore estimate the effect on the gender gap in responses separately for male and female assessors and present the results in Figure 11. For male assessors, shown in Panel (a), we find no gender gap in response rates when the emails contain or do not contain evidence. However, for female assessors, shown in Panel (b), we find that female homeowners receive 13.8 pp (30%, $p < 0.001$) fewer response than male homeowners when they do not provide evidence, and that the gap almost entirely closes when homeowners provide evidence. Providing evidence reduces the gender gap by 13.3 pp ($p = 0.013$).[38]

Figure 11: The Effect of Evidence on the Gender Response Gap by Assessor Gender



Note: This figure displays the email response rates for different types of fictitious homeowners, by the sex of the assessor and whether the email contained evidence. In panel (a), we present the response rates by homeowner sex and whether the email contained evidence for counties with a male assessor. Panel (b) presents analogous results for counties with female assessors. The p-values correspond to tests that the response rate by each male or female assessor are equal for male and female homeowners, separately by whether the email contained evidence. In all specifications, we include state fixed effects and cluster the standard errors by county.

8. Conclusion

By law, the amount each resident should contribute in taxes to help fund local public goods and services should be proportional to the value of her home. However, biases in home

[38]The racial gap in responses is similar for female and male assessors and we do not find heterogeneity by assessor sex in the effect of providing evidence.

assessments create inequality and inequity. While homeowners can appeal their assessed values to correct this bias, those who are most negatively impacted are less likely to appeal, and when they do, they are less likely to receive a reduction in their tax burden.

We conduct two field experiments, one with homeowners and one with county assessors, to better understand the drivers of these differences in both appeal behavior and appeal outcomes. Through both of our experiments we document that minority and female homeowners face discrimination in the appeals process. Discrimination reduces the potential benefits from filing an appeal while potentially increasing the costs. However, our results indicate that even if policies induced a greater share of minority and female homeowners to file an appeal—for example, by removing information frictions—it would not reduce inequality in property tax burdens. In our homeowner experiment, minority and female homeowners who appealed did not see the same reductions in their tax burdens as non-minority and male homeowners, even though they submitted similar evidence in the appeals process.

Overall, our results imply that property tax appeals are not an effective tool for correcting biases in property assessments. The appeal system does not accomplish its intended goal and may even exacerbate inequality based on race and gender. Other policies that reduce discrimination and hold assessors accountable are needed. However, since we find evidence that discrimination against minority homeowners is consistent with animus while discrimination against female homeowners is consistent with statistical, different policies are likely needed to mitigate each type of unequal treatment. Future work should examine alternative appeal processes that shift the administrative burden of appealing from the citizen to the county (Herd et al., 2013).

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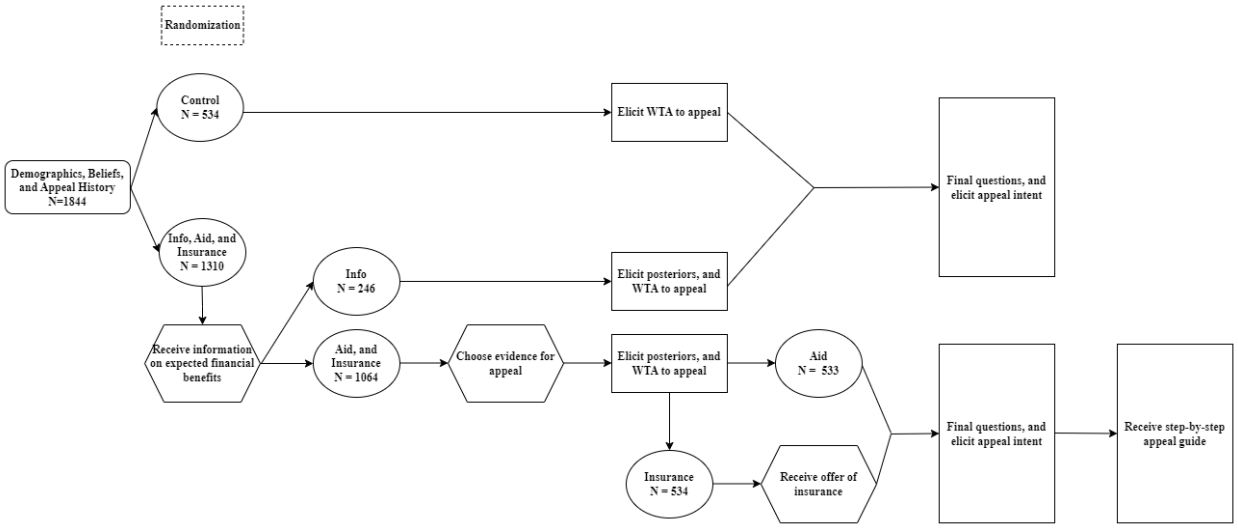
Online Appendix: Not for Publication

A. Details of the Homeowner Survey Experiment

Figure A1: Survey Experiment Flowchart



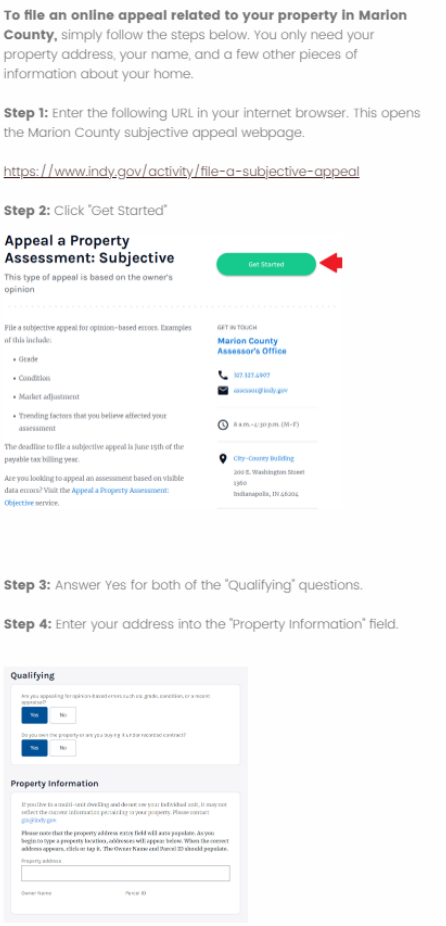
A.1 Treatments

Figure A2: *Information* Treatment

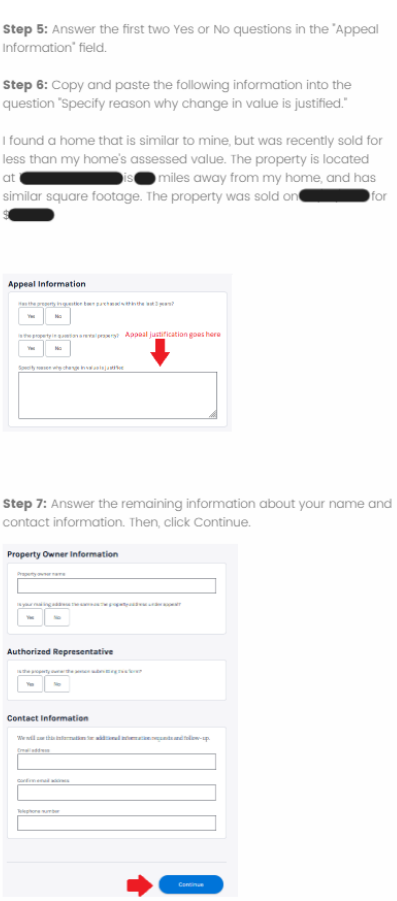
The following table gives some information about what happened when homeowners in Marion County appealed their property's assessed value **last year (2022)**. Please take a moment to review this information.

	Your Previous Guess	Actual Value	Difference
Percent of households that successfully appeal	14%	73%	-59
Average percent reduction in tax bill for those with successful appeal	14%	22%	-8

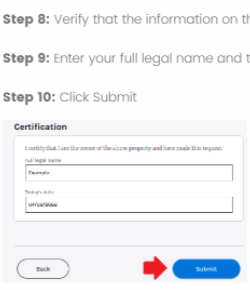
Figure A3: *Appeal Aid* Treatment (File Online Option)



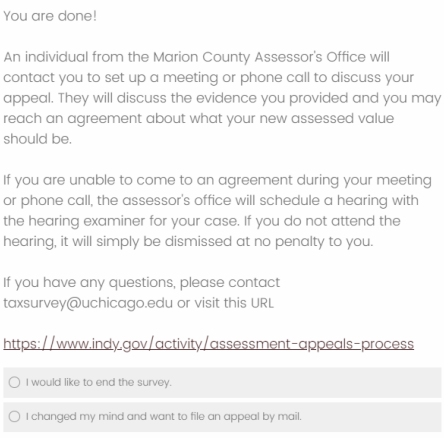
(a)



(b)

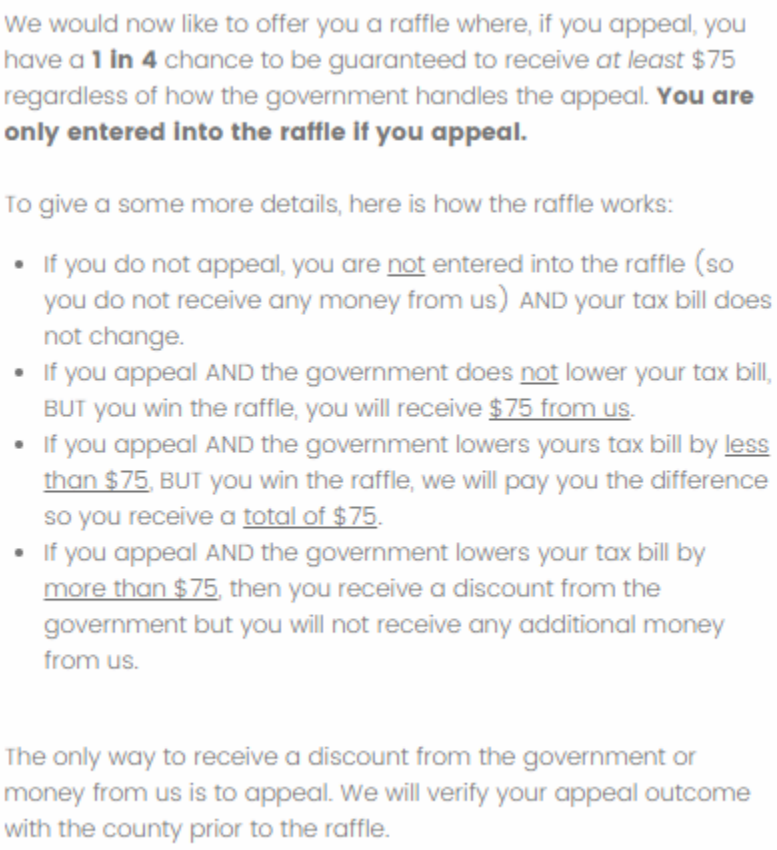


(c)



(d)

Figure A4: *Insurance* Treatment


We would now like to offer you a raffle where, if you appeal, you have a **1 in 4** chance to be guaranteed to receive *at least* $75 regardless of how the government handles the appeal. **You are only entered into the raffle if you appeal.**

To give a some more details, here is how the raffle works:

- If you do not appeal, you are not entered into the raffle (so you do not receive any money from us) AND your tax bill does not change.
- If you appeal AND the government does not lower your tax bill, BUT you win the raffle, you will receive $75 from us.
- If you appeal AND the government lowers yours tax bill by less than $75, BUT you win the raffle, we will pay you the difference so you receive a total of $75.
- If you appeal AND the government lowers your tax bill by more than $75, then you receive a discount from the government but you will not receive any additional money from us.

The only way to receive a discount from the government or money from us is to appeal. We will verify your appeal outcome with the county prior to the raffle.

B. Sample Characteristics and Treatment Balance in the Survey Experiment

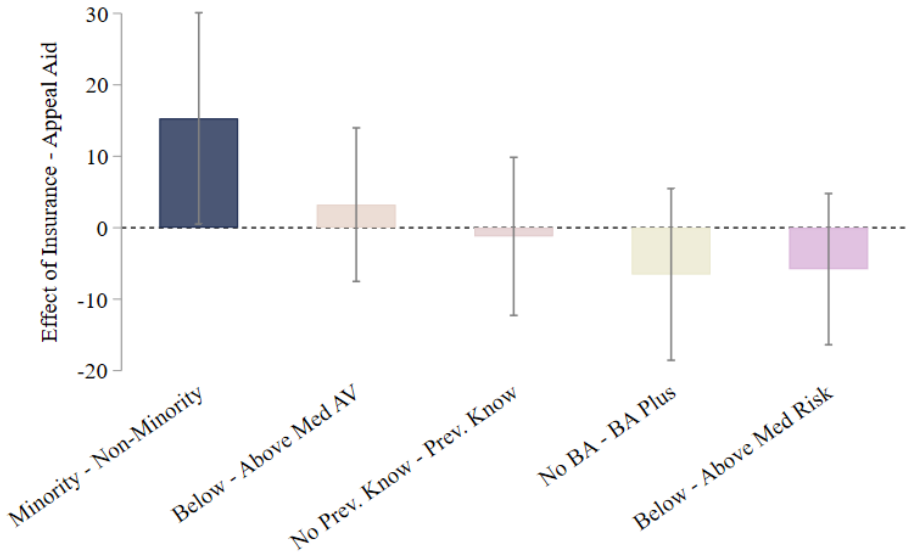
Table B1: Balance Across Treatment Arms in the Survey Experiment

	Treatment Arm					
	All (1)	Control (2)	Info (3)	Appeal (4)	Insure (5)	P-val (6)
Minority	15.94	15.36	17.07	15.20	16.76	0.84
	(0.85)	(1.56)	(2.40)	(1.56)	(1.62)	
Women	30.26	27.53	28.86	31.33	32.58	0.28
	(1.07)	(1.93)	(2.89)	(2.01)	(2.04)	
Bachelor's Degree	35.25	35.96	34.55	34.90	35.22	0.98
	(1.11)	(2.08)	(3.04)	(2.07)	(2.07)	
Graduate Degree	35.90	35.39	36.18	36.02	36.16	0.99
	(1.12)	(2.07)	(3.07)	(2.08)	(2.09)	
Some College	19.47	18.54	20.73	20.08	19.21	0.88
	(0.92)	(1.68)	(2.59)	(1.74)	(1.71)	
Republican	29.92	29.76	26.82	28.88	32.86	0.40
	(1.15)	(2.06)	(2.99)	(2.12)	(2.29)	
Assessed Value ($1k)	249.63	254.48	246.98	250.18	245.41	0.75
	(3.29)	(6.24)	(9.33)	(6.16)	(5.89)	
Age	54.27	53.88	54.63	53.96	54.80	0.73
	(0.36)	(0.67)	(0.99)	(0.67)	(0.68)	
Has Child in Public School	17.19	17.41	20.45	15.97	16.55	0.56
	(0.95)	(1.71)	(2.73)	(1.72)	(1.81)	
Previously Aware Could Appeal	60.10	62.55	60.08	59.54	58.19	0.53
	(1.15)	(2.11)	(3.18)	(2.15)	(2.17)	
Previously Appealed	22.99	23.78	25.87	21.22	22.59	0.72
	(1.28)	(2.35)	(3.68)	(2.32)	(2.41)	
Previous Appeal Successful	72.58	74.36	72.97	71.21	71.64	0.97
	(2.84)	(4.98)	(7.40)	(5.62)	(5.55)	
Prior Own Appeal Successful (%)	32.77	31.97	28.76	35.80	32.34	0.00
	(0.62)	(1.14)	(1.62)	(1.14)	(1.18)	
Prior Own Appeal % Reduction	16.82	16.90	16.49	18.13	15.55	0.13
	(0.42)	(0.76)	(1.26)	(0.84)	(0.71)	
Prior Others Appeal Successful (%)	26.69	26.83	26.62	27.64	25.62	0.52
	(0.52)	(0.98)	(1.42)	(1.00)	(0.93)	
Prior Others Appeal % Reduction	17.44	17.65	17.19	17.31	17.47	0.98
	(0.38)	(0.73)	(1.08)	(0.71)	(0.70)	
N	1,844	534	246	533	531	

Note: The table presents average characteristics and baseline beliefs for the full sample and by treatment arm as well as the p-value from a joint test that all conditions are equal. Heteroskedasticity-robust standard errors are reported in parentheses.

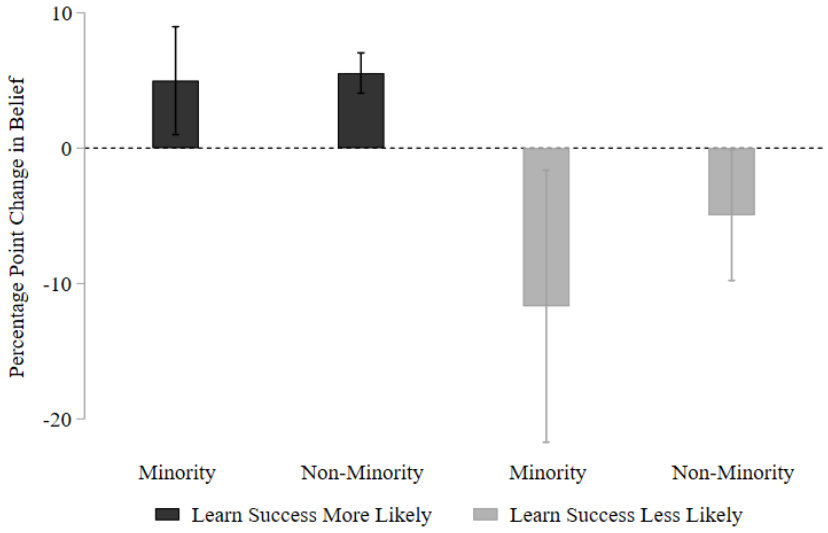
C. Homeowner Experiment Appendix Figures

Table C1: Relative Effects of Insurance Across Homeowner Characteristics

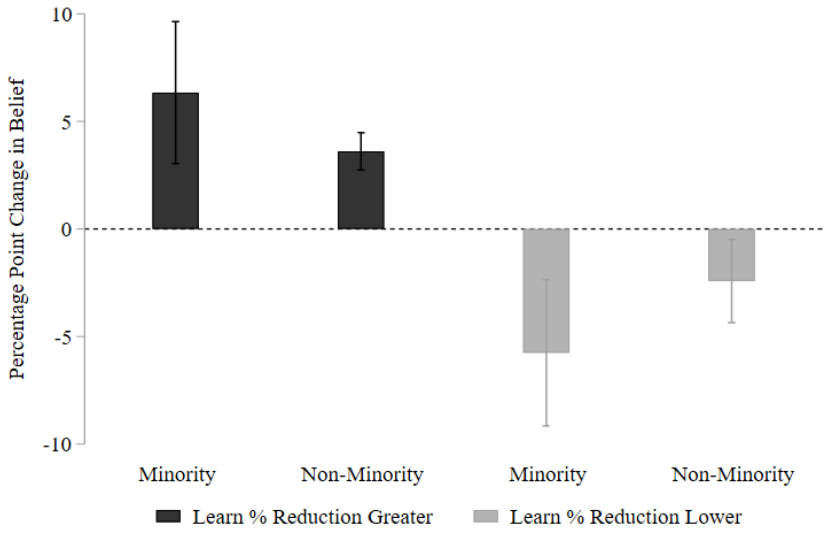


Note: This figure presents difference-in-difference estimates for the effect of *Insurance* relative to *Appeal Aid* across five subgroups, as well as the 95 percent confidence intervals, on the likelihood that a homeowner files an appeal. We estimate five regressions, similar to Equation 3, but adapted to the relevant subgroup. For example, the first bar presents the difference in treatment effects between minority and non-minority homeowners of receiving *Insurance* compared to those only receiving *Appeal Aid*. Different subgroups are separated by vertical dashed lines, and colored differently. From left to right, we present the effects for: (1) minority relative to non-minority homeowners; (2) homeowners with below median assessed values in our sample relative to those with above median assessed values; (3) homeowners who stated they did not know before the survey they could appeal relative to homeowners who stated they did know; (4) homeowners who have not received a Bachelor's degree relative to those who have at least a Bachelor's degree; and (5) homeowners with below median risk preferences elicited in the survey relative to those with above median risk preferences.

Figure C1: Belief Updating from Financial Information by Race and Prior Beliefs



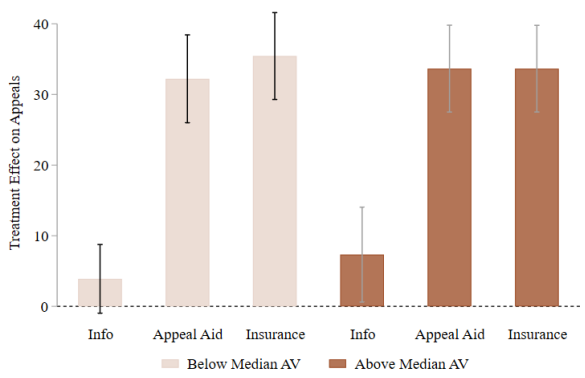
(a) Posterior - Prior on Appeal Success



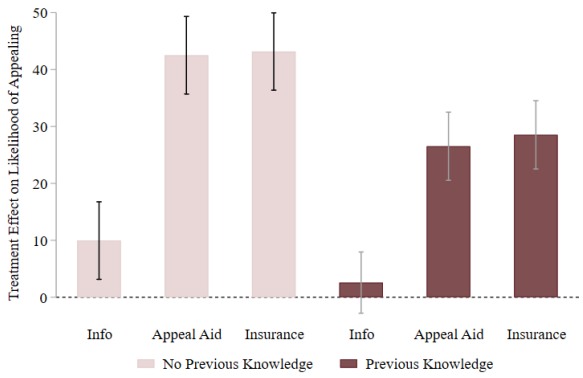
(b) Posterior - Prior on % Reduction in Taxes Owed

Note: Panel (a) presents the change in homeowners' beliefs, by race and by whether the homeowners' prior belief was below or above the true value, about the likelihood of having a successful appeal after receiving information about the average likelihood of success in Marion County. Panel (b) similarly presents the change in beliefs about the percent reduction in taxes owed for homeowners whose appelas are successful.

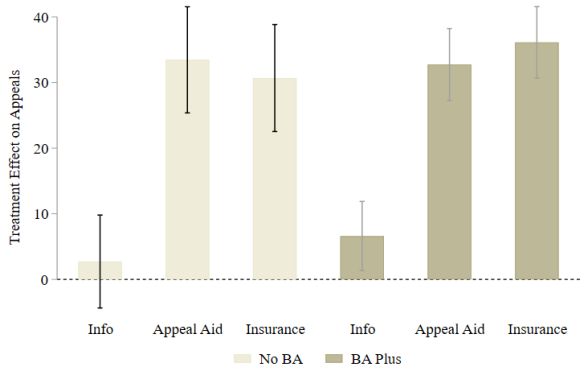
Figure C2: Ruling Out Other Explanations for the *Insurance* Effect



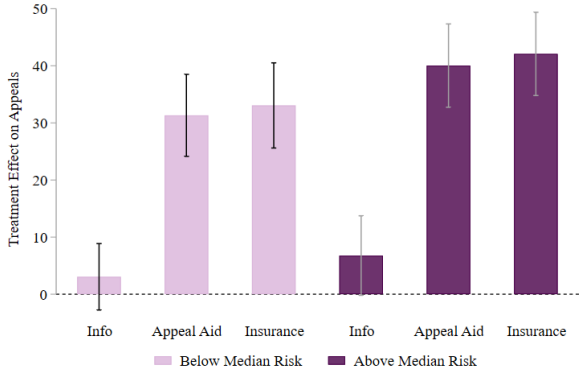
(a) Home Assessed Value



(b) Previous Knowledge of Appeals



(c) Education



(d) Risk Preferences

Note: This figure presents four sets of heterogeneous treatment effects for the likelihood a homeowner files an appeal with the county. Treatments effects are estimated as in Equation 3, but for the two subgroups of interest, rather than race. The four analyses are: (a) homeowners with below and above median assessed values in our sample; (b) homeowners who stated they did not know before the survey they could appeal and homeowners who stated they did know; (c) homeowners who do not have a bachelor's degree and those who have at least a bachelor's degree; and (d) homeowners with below and above median risk preferences elicited in the survey.

D. Homeowner Experiment Appendix Tables

Table D1: Racial Differences in Homeowner Characteristics and Beliefs

	Group			
	All (1)	Black and Hispanic (2)	White and Asian (3)	P-val (4)
Assessed Value ($1k)	249.63	201.70	258.72	0.00
	(3.29)	(7.34)	(3.62)	
Previously Aware Could Appeal	60.10	39.86	63.98	0.00
	(1.15)	(2.88)	(1.23)	
Previously Appealed	13.78	7.90	14.91	0.00
	(0.81)	(1.58)	(0.92)	
Previous Appeal Successful	9.96	4.81	10.95	0.00
	(0.70)	(1.26)	(0.80)	
Prior Own Appeal Successful (%)	32.77	41.43	31.11	0.00
	(0.62)	(1.69)	(0.65)	
Prior Own Appeal % Reduction	16.82	25.17	15.22	0.00
	(0.42)	(1.41)	(0.41)	
Prior Others Appeal Successful (%)	26.69	30.48	25.96	0.01
	(0.52)	(1.52)	(0.55)	
Prior Others Appeal % Reduction	17.44	24.71	16.04	0.00
	(0.38)	(1.38)	(0.36)	
N	1,844	294	1,550	

Note: The table presents summary statistics for homeowners in our survey experiment. Column (1) presents the overall average, Column (2) the average among minority homeowners, and Column (3) the average among non-minority homeowners. Column (4) presents the p-value for a test of equivalence between minority and non-minority homeowners.

Table D2: Racial Differences in Appeal Experiences for *Control* Homeowners

	Group			
	All (1)	Black and Hispanic (2)	White and Asian (3)	P-val (4)
Affirmed Appeal Intent	35.77	56.10	32.08	0.00
	(2.08)	(5.51)	(2.20)	
Appealed	7.12	6.10	7.30	0.68
	(1.11)	(2.66)	(1.23)	
Perceived Cost to Appeal	23.75	22.51	23.98	0.62
(% of Tax Burden)	(1.24)	(2.66)	(1.39)	
Strongly or Somewhat Agree that				
No Income Discrimination in Assessments	20.56	13.75	21.88	0.06
	(1.82)	(3.87)	(2.03)	
No Income Discrimination in Appeals	15.82	10.26	16.87	0.09
	(1.65)	(3.46)	(1.84)	
No Racial Discrimination in Assessments	19.85	10.98	21.46	0.01
	(1.73)	(3.47)	(1.93)	
No Racial Discrimination in Appeals	13.59	8.97	14.46	0.14
	(1.54)	(3.26)	(1.73)	
Pays More than Fair Share	51.31	58.54	50.00	0.15
	(2.16)	(5.47)	(2.35)	
N	534	82	452	

Note: The table presents summary statistics of baseline behavior using homeowners in the *Control* group of our survey experiment. Column (1) presents the overall average, Column (2) the average among minority homeowners, and Column (3) the average among non-minority homeowners. Column (4) presents the p-value for a test of equivalence between minority and non-minority homeowners.

Table D3: Determinants of Taxpayer Choice of Comparable by Gender

	(1) Female	(2) Male	(3) Difference
Distance	-0.38***	-0.30***	-0.078
	(0.095)	(0.068)	(0.12)
Effective Year Built	0.0018	-0.0018	0.0036
	(0.0038)	(0.0033)	(0.0050)
Year Built	0.0087**	0.0068**	0.0020
	(0.0037)	(0.0032)	(0.0049)
Same Neighborhood	0.43**	0.67***	-0.24
	(0.20)	(0.16)	(0.25)
Log Sale Price	1.11*	0.52	0.59
	(0.63)	(0.54)	(0.83)
Number of Bathrooms	0.093	0.099	-0.0062
	(0.15)	(0.11)	(0.19)
Number of Bedrooms	0.091	0.15	-0.056
	(0.12)	(0.096)	(0.15)
Log Home SQFT	-0.53	-0.97***	0.44
	(0.35)	(0.30)	(0.46)
N	1590	2750	4340

Note: The table presents the multinomial logit coefficients from the choice model for how homeowners pick their comparable homes as evidence for the appeal. Column (1) includes women in the *Appeal Aid* and *Insurance* treatments who select a comparable home. Columns (2) includes men in the *Appeal Aid* and *Insurance* treatments. Column (3) presents the estimated difference between women and men. Each observation is an individual-option so that each individual in the sample appears five times. Standard errors are clustered at the individual level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table D4: Treatment Effects on Appeal Intentions and Follow-Through by Gender

	(1) Intent & Appeal	(2) Intent & No Appeal	(3) No Intent & Appeal	(4) No Intent & No Appeal
Info × Women	8.73**	-2.60	2.46	-8.60
	(3.75)	(6.49)	(2.05)	(6.66)
Appeal Aid × Women	29.37***	-17.64***	9.83***	-21.55***
	(3.97)	(4.87)	(2.31)	(4.87)
Insurance × Women	36.60***	-21.09***	1.51*	-17.01***
	(4.10)	(4.60)	(0.88)	(4.79)
Info × Men	3.14	2.76	-0.38	-5.52
	(2.66)	(4.02)	(1.10)	(4.28)
Appeal Aid × Men	20.53***	-11.30***	9.04***	-18.28***
	(2.80)	(2.85)	(1.80)	(3.41)
Insurance × Men	29.90***	-7.70***	2.99**	-25.19***
	(2.96)	(2.96)	(1.21)	(3.33)
N	1,657	1,657	1,657	1,657

Note: This table presents treatment effect estimates by gender on four outcomes corresponding to {Affirmed Intent, Did Not Affirm Intent} × {Filed an Appeal, Did Not File an Appeal}. For example, Column (1) presents our treatment effects from Equation 3 where the outcome is 100 if the homeowner "Affirmed Intent" and "Filed an Appeal", and 0 otherwise. Heteroskedastic robust standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.01$

E. Details of the Correspondence Experiment

E.1 email Content and Creation

Names: The names in the experiment were drawn from Tzioumis (2018) who examines the racial demographics of the most common names appearing on mortgage applications. Each email was randomly assigned a first name from the designated race and sex with replacement. The list of first names used in the experiment appears in Table E1 below.

Table E1: Male and Female First Names Assigned by Race

	Distinctively Black	Distinctively Hispanic	Distinctively White
Distinctively Female	Tamika	Guadalupe	Beth
	Latoya	Rosa	Megan
Distinctively Male	Darnell	Javier	Todd
	Jermaine	Juan	Brett

Notes: This table lists the names used in the experiment. The names were taken from Tzioumis (2018).

We assess whether these names signal the intended race by displaying the probabilities that mortgage applicants are of each race by name. We also present statistics on the portion of Americans with each first name who is female according to the Social Security Administration. These statistics appear in Table E2. As this table shows, the empirical probability that individuals with the names we chose are of the intended race is extremely high. Similarly, the probability that individuals with the names we chose are of the intended sex is extremely high. However, we do not know the subjective probabilities assigned by the assessors in our sample. We view the estimates we present in the paper as intention-to-treat (ITT) effects, which are biased toward zero if the assessors misassign identity to the emailers.

Table E2: Empirical Likelihood that First Names Signal the Intended Identity

First Name	Intended Identity	$\mathbb{P}[Black\|\text{first name}]$	$\mathbb{P}[Hispanic\|\text{first name}]$	$\mathbb{P}[White\|\text{first name}]$	$\mathbb{P}[Female\|\text{first name}]$
Tamika	Black Female	89.02	3.66	7.32	99.60
Latoya	Black Female	91.40	4.30	4.30	99.60
Guadalupe	Hispanic Female	0.00	94.81	4.75	73.90
Rosa	Hispanic Female	4.44	72.91	20.24	99.30
Beth	White Female	0.36	0.52	98.57	99.80
Megan	White Female	0.48	0.74	98.30	99.80
Darnell	Black Male	82.19	0.00	17.81	8.33
Jermaine	Black Male	88.41	4.35	5.80	2.51
Javier	Hispanic Male	0.00	94.51	4.90	0.59
Juan	Hispanic Male	0.52	93.41	4.53	0.84
Todd	White Male	0.94	0.40	97.89	0.40
Brett	White Male	0.87	0.36	98.36	2.14

Notes: This table reports the first names used in the experiment and the probability that a person with a given name is of the intended race and sex. We present the probabilities that a person with a given first name is of the intended race as calculated by Tzioumis (2018) and of the intended sex using the procedure from Mullen (2021).

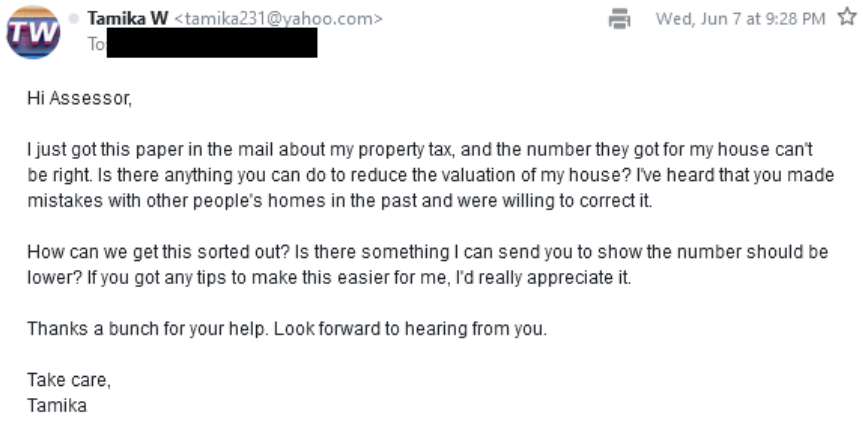
email Addresses: We assigned identities to email accounts of popular email providers. Each email addressed contained the first name of the fictitious homeowner, along with a series of arbitrary numbers. The email address also served as the only method through which assessors could contact the fictitious homeowner. Each email address had a separate account to ensure that we could match each assessor email back to the fictitious homeowner. We only gave our email address when submitting queries through forms instead of contacting assessors directly.

email Order: We sent six emails to each assessor. We randomized the order of these emails to be orthogonal to the emailer's identity and the email content.

email Contents: Each of these emails made three requests: (1) information about what forms/documents need to be filed; (2) information about the filing deadline; and (3) more information or advice on how to navigate the appeals process. Each email also contained the first name of the fictitious homeowner, to increase the salience of the emailer's identity. We introduced variation in the tone, the writing-level, and the degree of frustration in each of the emails to help disguise that all of the emails were written by the same people.

The primary content of the emails appears in the six examples below (See Figures E1, E2 , E3, E4, E5, and E6). Additionally, we randomly chose whether the email contained an additional sentence stating that the assessed value of the homeowner's property was too high. These messages either told the assessor that the fictitious homeowner had recently had an appraisal: "When I had my home appraised last year, they told me it was worth much less" or "The value is higher than what my home was appraised for recently." The other type of evidence referenced comparable homes that sold for less than the home: "The assessment does not seem to be in line with the sale price of homes in my neighborhood on Zillow" or "The value is higher than what homes in my neighborhood sold for on Redfin."

Figure E1: Correspondence Study email Version 1

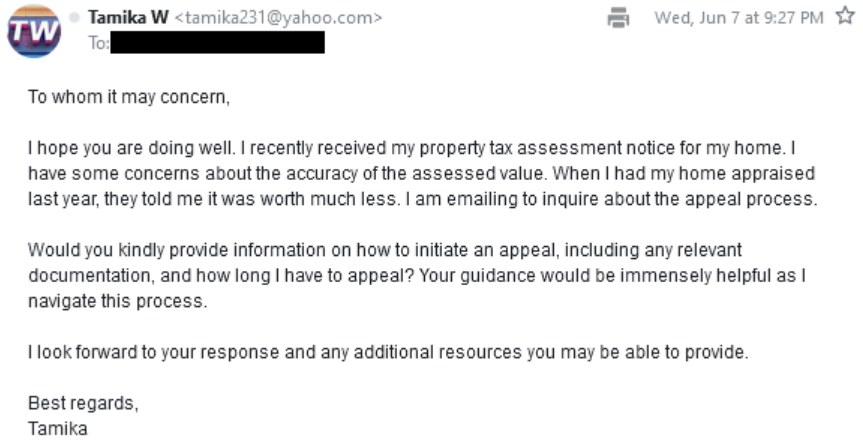


Note: This figure displays one version of the email sent to the assessors. The subject of the email was "Mistake on property tax assessment?" We randomized whether this email contained a sentence describing evidence that the assessment was too high.

Figure E2: Correspondence Study email Version 2

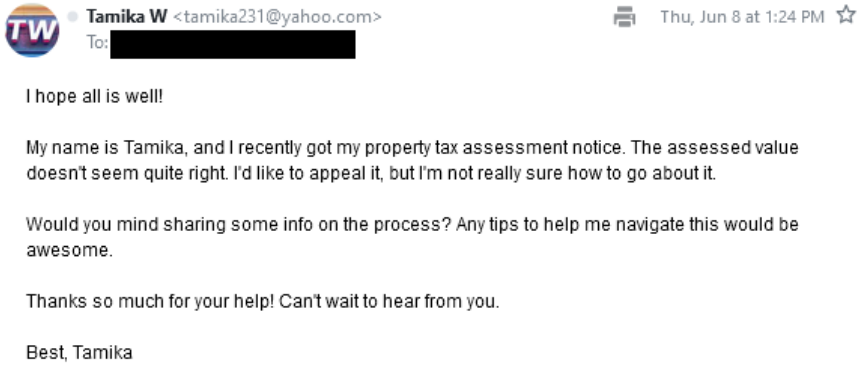
Note: This figure displays one version of the email sent to the assessors. The subject of the email was "Disappointed with property tax assessment." We randomized whether this email contained a sentence describing evidence that the assessment was too high.

Figure E3: Correspondence Study email Version 3



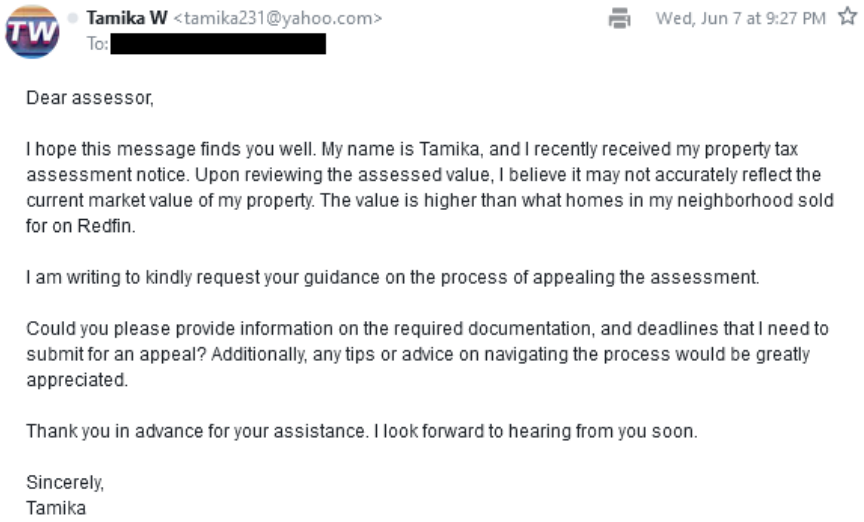
Note: This figure displays one version of the email sent to the assessors. The subject of the email was "Inquiring About Property Tax Appeal Procedures." We randomized whether this email contained a sentence describing evidence that the assessment was too high.

Figure E4: Correspondence Study email Version 4



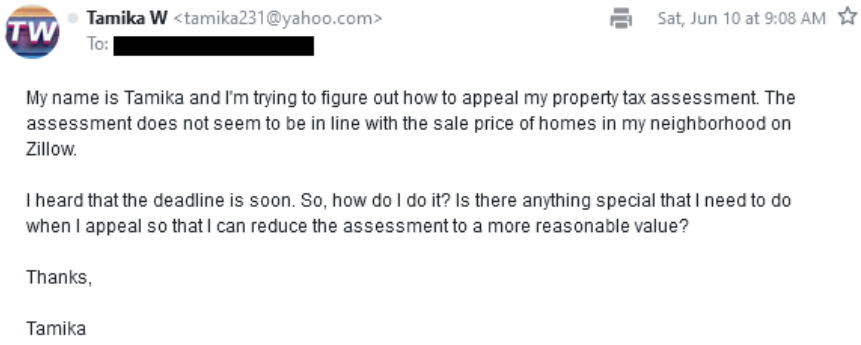
Note: This figure displays one version of the email sent to the assessors. The subject of the email was "Quick Question on Property Tax Appeal." We randomized whether this email contained a sentence describing evidence that the assessment was too high.

Figure E5: Correspondence Study email Version 5



Note: This figure displays one version of the email sent to the assessors. The subject of the email was "Seeking guidance on property tax appeals process." We randomized whether this email contained a sentence describing evidence that the assessment was too high.

Figure E6: Correspondence Study email Version 6



Note: This figure displays one version of the email sent to the assessors. The subject of the email was "Question about my property tax assessment." We randomized whether this email contained a sentence describing evidence that the assessment was too high.

E.2 County Sampling

Our sample consists of the assessors in Indiana, Nebraska, Missouri, Idaho, and Pennsylvania.[39] We chose these states because their property valuation and appeal deadlines occur between June 15th and early September. Across these states, there are 411 counties and assessors. We excluded some counties in Missouri, Idaho, Pennsylvania and Florida because we were not able to obtain assessor contact information. We also excluded Missouri counties with early deadlines not aligned with the rest of the state, including the 18 Class 1 and Charter counties. We also exclude six counties in Missouri because of an error in administering the treatments in those counties.

Table E3 summarizes the amount of observations, counties, and exclusions from each state in our sample. This table also includes information about the counties where we sent emails to assessors and the counties where we filled out forms that were available to contact the assessors.

Table E3: Sample Sizes by State

State	Observations	Total Counties	Counties emailed	Forms Submitted	Counties Excluded
	(1)	(2)	(3)	(4)	(5)
Indiana	552	92	92	0	0
Nebraska	558	93	93	0	0
Missouri	468	115	78	0	37
Idaho	234	44	37	2	5
Pennsylvania	252	67	42	0	25
Florida	396	67	66	0	1
Total	2,460	478	408	2	68

Note: This table displays the number of observations in each state along with information about the sampling from each state. The first column includes the number of observations included in the experiment. The second column displays the total number of counties that exist in the respective state. The third column presents the number of counties that we emailed. The fourth column presents the number of forms that we submited on a homeowners behalf. The last column represents the number of counties that were excluded from the experiment.

[39] The pre-analysis plan included Florida. We are still actively collecting that data.

E.3 email Procedures

After we obtained the set of assessors we intended to correspond with, we randomly assigned messages to assessors using the following approach:

1. For each assessor, independently randomly assign the fictitious homeowner's race and sex so that each county would receive one email from each of the six identities.

2. Randomize the order in which the assessor would receive an email from a fictitious homeowner of each type.

3. Randomly assign one of the six potential messages to each of the emails such that each assessor would receive exactly one of each message.

4. Randomly assign the presence of the evidence message and the contents of the evidence message, ensuring that within a state the presence and type of message was independent of other treatment characteristics.

5. Randomly draw profiles from the set of emails we created using the procedure in Appendix E.1.

We conducted the randomization separately for each state in our sample. The randomization procedure ensured that all of the characteristics of an email were orthogonal to each other. After each randomization, we sent emails to each of the states during the window between when homeowners were informed about their property tax assessments and the appeal deadline. We present the deadlines and email dates in Table E4. In the experiment, we sent exactly six correspondences to each of the assessors in our sample. Assessors received at most one correspondence from one of our fictitious homeowners per day.

Table E4: Appeal Deadlines and email Correspondences by State

State	Deadline	email 1	email 2	email 3	email 4	email 5	email 6
Indiana	6/15/2023	6/7	6/8	6/9	6/10	6/11	6/12
Nebraska	6/20/2023	6/13	6/14	6/15	6/16	6/17	6/18
Missouri	Varies 7/10/2023	6/20	6/22	6/24	6/27	6/29	7/5
Idaho	6/26/2023	6/13	6/14	6/15	6/16	6/17	6/18
Pennsylvania	8/1/2023	7/15	7/18	7/20	7/24	7/24	7/26
Pennsylvania	9/1/2023	8/1	8/4	8/8	8/11	8/16	8/25
Florida	9/5/2023 - 9/18/2023	8/25	8/28	8/30	9/1	9/3	9/5

Note: This table displays the appeal deadlines for counties in each state along with the dates of each email that we sent to assessors in those counties.

E.4 Measuring Outcomes

We assigned each email response to an assessor using the information provided by the email responses. In nearly all cases, the email would be a direct response to the email that we sent the assessors. We occasionally received responses from different email addresses or assessors instead of the homeowner we were trying to reach. In these cases, we would match based on the county name of the assessor who responded. We code any non-automatic response from the email address as a response.

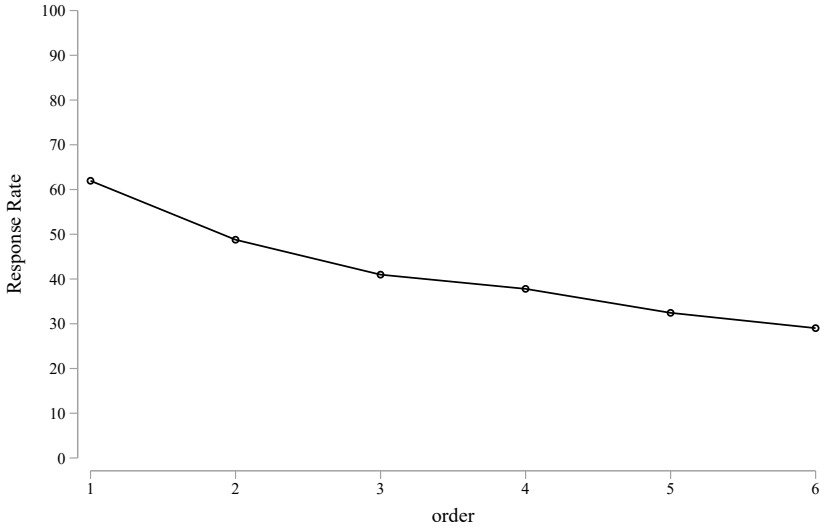
In addition to measuring whether we received any response from the assessors, we also recorded several pieces of information about each response. This information included (1) whether the assessor attached (or linked) the form we requested, (2) whether they discussed the form, (3) whether the assessor informed the fictitious homeowner of the response deadline, (4) whether the assessor provided any advice on how to fill out the form, (5) whether the assessor requested that the emailer contact the assessor through another means, and (6) whether the assessor requested more information from the emailer, such as their address. We collected the outcomes after the deadline for filing an appeal passed.

F. Do Assessors Learn the Emails Are Fake Over Time?

One potential concern with our analysis is that our results are driven by assessors learning over time that the emails come from fictitious homeowners. Under this interpretation, response rates would be lower for women and minorities because the assessors are more likely to learn that these individuals are not real homeowners and thus the lack of response would not be due to discrimination.

Indeed, Figure F1 shows that the probability of an assessor responding to an email decreases with the number of emails we sent to the assessor. However, there are other reasons we might expect the response rate to be decreasing over time, mainly that later emails are sent to the assessor closer to the deadline than earlier emails, giving assessors less time to respond. Moreover, in order for our results to be biased by assessors' learning that the emails come from fictitious homeowners, we would need that assessors are both more likely to learn that emails from female or minority homeowners are fake and that assessors are less likely to respond to emails that they think come from fictitious homeowners.

Figure F1: email Response Rate by email Number



Note: This figure displays the probability the assessor responds to an email by the order the email was sent to the assessor.

In Table F1, we present several robustness checks that investigate whether differences in response rates by the email number bias our results. For reference, Columns (1) and (2) replicate the main specification used in the paper and add county-level controls such as assessor characteristics and county characteristics. Column (3) introduces order fixed effects to restrict our analysis to within-order treatment effects. We find that these fixed effects slightly increase the estimated discrimination against Black homeowners and slightly decrease the estimated discrimination against female homeowners, although these differences

are small and do not change our conclusions.

Column (4) controls for the email order linearly and allows for interactions between the fictitious homeowner's identity and the email order. As in Figure F1, we see that later emails are less likely to receive a response than early emails. The coefficients on Black and female are more negative in Column (4) than Column (3), suggesting that the effects are strongest for the first email, but we cannot reject that the effect of race or gender do not vary with email number.

Finally, Columns (5) and (6) display the effects of our treatments separately for the first email sent to each assessor and the second through final email sent to each assessor. Column (5) corresponds to the estimates we would have gotten had we sent a single email to each assessor and Column (6) informs us about how the subsequent emails affected the assessors' behavior. Because we are relying on between-subject variation in Columns (5) and (6), we present balance Tables F2 and F3 which show that there are no economically significant imbalances across treatments.

We find that the estimated discrimination against Black, Hispanic, and female homeowners using only the first email is substantially larger than the discrimination estimates for the subsequent five emails. In the last five emails, we can still detect discrimination against Black homeowners, but are no longer able to detect discrimination against female homeowners. Together, these results suggest that some assessors may discover that the emails come from fictitious homeowners over time. However, even if this is the case, any learning attenuates our estimates of discrimination because assessors stop responding to any homeowners. Therefore, we conclude that the qualitative responses found in the correspondence study are driven by assessor discrimination and not spuriously driven by assessors learning that the emails come from fictitious homeowners.

Table F1: Effects of email Order

	(1) Response	(2) Response	(3) Response	(4) Response	(5) Response	(6) Response
Black Homeowner	-7.561***	-7.561***	-7.581***	-12.729**	-16.183***	-5.734**
	(1.950)	(1.954)	(1.846)	(5.263)	(5.744)	(2.611)
Hispanic Homeowner	-0.854	-0.854	-1.232	-1.285	-3.257	0.013
	(1.832)	(1.836)	(1.744)	(5.361)	(5.688)	(2.648)
Female Homeowner	-4.472***	-4.472***	-3.738***	-6.385	-14.011***	-1.997
	(1.497)	(1.500)	(1.437)	(4.511)	(4.755)	(2.141)
Black Homeowner × E-mail Order				1.442		
				(1.355)		
Hispanic Homeowner × E-mail Order				-0.058		
				(1.409)		
Female Homeowner × E-mail Order				0.732		
				(1.187)		
E-mail Order				-6.981***		
				(1.116)		
Constant	46.870***	44.011**	46.636***	71.198***	74.976***	40.722***
	(2.161)	(19.482)	(2.122)	(4.341)	(4.291)	(2.181)
County Level Controls	NO	YES	YES	YES	YES	YES
Order Fixed Effects	NO	NO	YES	NO	NO	NO
Sample	Full	Full	Full	Full	First E-mail	Emails 2-6
R-Squared	0.01	0.06	0.06	0.05	0.04	0.00
Observations	2,460	2,460	2,460	2,460	410	2,050

Notes: This table displays the probability of an assessor response by race, sex, the order of the email, and the interaction between race/sex and the order of the email. Column (1) presents the results from our main specification. Column (2) adds county level controls to the main specification. Column (3) adds email order fixed effects. Column (4) controls for email order linearly and interacts each of the main treatments with email order. Column (5) presents the results from the main specification for only the first email sent to each county assessor. Column (6) presents the results from the main specification controlling for the second through fifth emails sent to each county assessor. Standard errors are clustered at the establishment level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table F2: Treatment Balance: First-email in Correspondence Study

	(1) White	(2) Black	(3) Hispanic	(4) Female	(5) Evidence
Female Assessor (%)	-0.001	-0.000	0.001	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
White Assessor (%)	-0.000	-0.001	0.001*	0.001	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Total Population (1000s)	-0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Medium Income (1000s)	0.002	0.002	-0.004	0.004	0.000
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
White Per Capita Income (1000s)	0.014**	-0.012*	-0.002	-0.008	0.004
	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)
Black Per Capita Income (1000s)	0.002	0.001	-0.003**	-0.000	0.001
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)
Hispanic Per Capital Income (1000s)	-0.002	-0.001	0.002	-0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Median Homevalue (1000s)	-0.001*	-0.000	0.001**	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Percent White Residents (%)	-0.006**	0.005**	0.001	-0.001	-0.002
	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Percent Black Residents (%)	-0.012***	0.009*	0.003	-0.001	-0.003
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Percent Hispanic Residents (%)	-0.004	0.004	-0.000	-0.002	0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Missing County Level Data (%)	-0.000	0.001**	-0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	0.607**	0.173	0.220	0.591**	0.614***
	(0.250)	(0.204)	(0.224)	(0.233)	(0.232)
R-Squared	0.05	0.04	0.04	0.01	0.03
Observations	410	410	410	410	410

Note: The table presents the correlation between county level characteristics and treatment status in the correspondence study. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table F3: Treatment Balance: Second through Sixth email in Correspondence Study

	(1) White	(2) Black	(3) Hispanic	(4) Female	(5) Evidence
Female Assessor (%)	0.000	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
White Assessor (%)	0.000	0.000	-0.000*	-0.000	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Total Population (1000s)	0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Medium Income (1000s)	-0.000	-0.000	0.001	-0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
White Per Capita Income (1000s)	-0.003**	0.002*	0.000	0.002	-0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Black Per Capita Income (1000s)	-0.000	-0.000	0.001**	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Hispanic Per Capital Income (1000s)	0.000	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Median Homevalue (1000s)	0.000*	0.000	-0.000**	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Percent White Residents (%)	0.001**	-0.001**	-0.000	0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Percent Black Residents (%)	0.002***	-0.002*	-0.001	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Percent Hispanic Residents (%)	0.001	-0.001	0.000	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Missing County Level Data (%)	0.000	-0.000**	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.279***	0.365***	0.356***	0.482***	0.477***
	(0.049)	(0.040)	(0.044)	(0.046)	(0.046)
R-Squared	0.00	0.00	0.00	0.00	0.00
Observations	2,050	2,050	2,050	2,050	2,050

Note: The table presents the correlation between county level characteristics and treatment status in the correspondence study. The sample is the second through sixth emails sent to each assessor. Standard errors clustered at the county level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

G. Ethics Appendix

In this section, we describe the ethical considerations of the two experiments. We first note that we underwent ethical review at the Human Subjects Committee at the University of Chicago (IRB22-0469 and IRB23-0588), which played an important role in ensuring that the correspondence experiment upheld high ethical standards despite our decision not to get consent from subjects (List, 2009). Next, we follow the framework of Asiedu et al. (2021).

1. **Equipoise:** In the survey experiment, some treatments are preferable to others. For example, the control treatment does not receive the detailed aid needed to file an appeal. However, ex ante, each subject has the same chance of receiving aid. In the correspondence experiment, each subject received similar emails as the others by design. Therefore, we do not expect any treatment arm to dominate another treatment arm from the assessor's perspective. The emails that are least likely to elicit a response may be better from the assessor's view because she will not spend time responding to the email. However, we believe that this benefit is small since every email asks for similar information. The assessor, in practice, could send the same email to every one of the fictitious homeowners. That said, the subjects in our experiment would be better off in the status quo world of no emails. However, learning about discrimination due to taste-based or statistical motivations is not otherwise feasible. We believe, and the IRB agreed, that the benefits from the knowledge outweigh the small costs to the assessor.

2. **Role of the Researchers with Respect to Implementation:** The researchers had direct decision-making power over whether and how to implement the experiment. We disclosed the experiment to participants before the survey experiment, but we did not disclose the experiment to the participants before they received an email in the correspondence experiment.

3. **Potential Harms to Research Participants from the Interventions:** We do not expect the survey experiment to harm our subjects. The correspondence experiment potentially harms our subjects. Assessors' time is scarce, and we are having them spend it reviewing emails that are fictitious without obtaining the involved parties' consent or compensating assessors for their time. Moreover, Bertrand and Duflo (2017) notes that, in resume correspondence experiments, when an applicant declines an offer, employers may learn that applicants with similar attributes are unlikely to accept offers. They claim that this may lead to employers being less likely to offer jobs to candidates that share those attributes in the future. They also note that after receiving rejections from candidates, the employers may believe that the market is tighter than previously expected, which would be beneficial for real candidates, but detrimental for employers. However, other studies have found this to be unlikely in practice (e.g., Brandon et al. (2023)). These issues are not relevant in our setting where homeowners are not in competition for help filing an appeal.

   While our correspondence experiment potentially harms our subjects, identifying racial discrimination from taste-based or statistical motives requires randomly assigning race,

which is only available in a field experiment with fictitious applicants. Both of these mechanisms ended up being important drivers of the treatment effects in our study. Therefore, we believe that the benefits to society outweigh the harms to the subject. Moreover, obtaining consent for the experiment would likely bias our estimates against finding discrimination as employers may change their behavior if they knew that they were participating in a study (Levitt and List, 2007).

Moreover, we designed our experiment to limit the potential harm to participants. We sent assessors no more than six emails. This helped us minimize the burden on assessors. Second, because few emails were sent to each assessor, we cannot determine whether an individual assessor is discriminating.

4. **Potential Harms to Research Participants from Data Collection or Research Protocols:** We do not believe subjects experience any harm from data collection. Responding to the survey is a voluntary choice on behalf of the participants. In the assessor experiment, assessors' responding to an e-mail from fictitious homeowners is no different from their every day actions in their job. The assessor's responses were anonymized so that no individual could link a particular assessor's response decision to the assessor.

5. **Financial and Reputational Conflicts of Interest:** Holz, Novgorodsky, and Simon did not receive any form of financial compensation as part of the study. The research questions pursued in this study are novel and different from prior work conducted by the PIs. We perceive no reputational conflicts of interest.

6. **Intellectual Freedom:** This study was conducted without collaborating with organizations. The study was conceived and designed by the PIs, who maintained intellectual freedom throughout all stages of the project. At no point did an outside partner have undue influence on the analysis or the interpretation of the results.

7. **Feedback to Participants and Communities:** We intend to share our results with policymakers after our work is subject to peer review.

8. **Foreseeable Misuse of Research Results:** We recognize that the results are relevant for public policy in property tax assessments. We advise policymakers to acknowledge that only one of several potential relevant outcomes is studied in our setting. While we study appeals, there are many other aspects of the property tax system that can lead to racial disparities. Moreover, discrimination in response to emails does not necessarily imply that assessors treat minorities and non-minorities differentially in the setting of their assessments.