Disparities in Tax Negotiations: Evidence from Field Experiments with Homeowners and Assessors*

Justin Holz
University of Michigan

David Novgorodsky
Walmart

Andrew Simon
University of Virginia

September 2025

Abstract

Across fiscal systems, tax burdens are shaped not only by statutory rules but also by negotiations between taxpayers and administrators. We design two complementary field experiments, guided by a theoretical model, to examine how taxpayer initiative and administrator discretion in negotiations contribute to disparities in property tax appeals. In a correspondence study, assessors are less responsive to requests for help appealing from Black and female homeowners, with mechanisms varying by homeowner and assessor characteristics. In a homeowner experiment, information frictions and perceived discrimination reduce appeals among disadvantaged groups, and appeal outcomes differ by homeowner race and gender. Together, these findings reveal that the negotiation process intended to correct misassessments may instead exacerbate inequities, with disparities driven by behavior on both sides of the market.

*This research was conducted with approval from the University of Chicago Institutional Review Board, IRB22-0469 and IRB23-0588. We thank Carlos Avenancio-Leon, Nathan Anderson, Chris Berry, John Bound, Eric Floyd, Jacob Goldin, Jim Hines, Troup Howard, Ania Jaroszewicz, Tim Layton, John List, Donald Moynihan, Michael Ricks, Robert Ross, Sally Sadoff, Anya Samek, Marta Serra-Garcia, Kevin Stange, Francis Wong, Pengju Zhang, Alejandro Zentner and audiences at the International Institute of Public Finance 2023 Annual Congress, the Michigan Tax Invitational, the National Tax Association 2023 Conference, NBER 2024 SI Real Estate, the University of Michigan Public Finance Seminar, The University of Michigan SBEE Seminar, the UChicago Inclusive Economy Lab, The University of San Diego Rady, The Advances in Field Experiments Conference, the University of Notre Dame Law School, the University of Chicago Experimental Workshop, the Office of Tax Analysis, the University of Maryland, the University of Virginia, and the University of Pittsburgh. This study has been pre-registered under AEARCTR-0009302 and AEARCTR-0011563. We thank the John Mitchell Lab, the Russell Sage Foundation, and the UChicago Experiments Initiative for their generous support. This paper was previously circulated as "Racial Inequality in Property Tax Appeals: Evidence from Field Experiments with Homeowners and Assessors."

1. Introduction

Across tax systems, liabilities depend not only on statutory rules but also administrator discretion and taxpayer initiative. Tax administrators, for example, decide whom to audit, which penalties to charge and forgive, and the assessments of assets like homes and intellectual property. Taxpayers, in turn, can challenge the administrator's decision-making, but doing so requires initiative to recognize the opportunity to negotiate and then the knowledge of how to engage effectively. As a result, the initial discretion by the tax administrator, selection into negotiations by the taxpayer, and the behavior by both parties during the deliberations all shape tax liabilities. Combined, these design features create institutional settings in which differences across taxpayers in information, trust, and behavior can interact with systemic bias to generate unequal tax burdens across demographic groups (Bhargava and Manoli, 2015; Brown, 2018; Avenancio-León and Howard, 2022; Elzayn et al., 2025).

We study how these common design features of administrator discretion and taxpayer initiative create disparities in the property tax system. Property taxes are the primary tool used to finance local governments, accounting for 70% of local tax collections and 11% of the total taxes paid in the US (Tax Foundation, 2025a,b). This system's structure underscores the importance of actions on both sides of the market for shaping liabilities, especially since the ground truth (market values) is unobservable for most homes. On the administrative side, county assessors exercise substantial discretion in estimating assessed values that determine initial liabilities and handling disputes through appeals. These processes are prone to errors, with differences between the "true" and estimated assessed values systematically differing across groups (Ross, 2017; Berry, 2021). On the taxpayer side, an individual must have initiative to figure out how to navigate the appeal process, including negotiating effectively.

In principle, the appeals process is meant to correct misvaluations by tax administrators and improve targeting in the property tax system by reducing overassessments for low-income and disadvantaged homeowners. Appeals *could* provide relief for these home-

owners and make burdens more equitable. In practice, however, the system may worsen targeting if administrator bias favors already advantaged groups, like white and male homeowners, who are also more likely to show initiative in appealing. For example, despite facing upward bias in assessments, minority homeowners are less likely to appeal (Avenancio-León and Howard, 2022; Ihlanfeldt and Rodgers, 2021). These patterns raise concerns that the two features of the system—taxpayer onus in filing appeals and assessor discretion in valuation and appeals—interact to unequal outcomes. When disadvantaged groups are less likely to negotiate and assessors have leeway that allows bias, then appeals-based relief may exacerbate disparities, leaving disadvantaged groups with persistently inflated liabilities. Understanding whether these mechanisms exist and how they interact is important as each points to a different policy lever. One could try to simplify taxpayer participation (e.g. outreach or automatic reviews) or curb assessor discretion (e.g. standardized valuations) to make the property tax system more equitable.

We investigate the drivers of gender and racial disparities in property tax appeals using two complementary field experiments, one with county assessors and another with homeowners. We design and interpret the results of each experiment through the lens of an economic model to jointly study the role of taxpayer initiative (who appeals and how) and administrative discretion (how officials respond to and rule on appeals). Through this framework, we highlight how beliefs, information frictions, and discrimination exacerbate inequality, with different implications by gender and race. Together, these two experiments allow us to separately examine how behavior on each side of the market, as well as their interactions during these negotiations, contributes to disparities in tax burdens.

First, we use a correspondence study with county tax assessors across six states to test whether assessors treat certain homeowners seeking to negotiate differently based on their demographics. We sent realistic email inquiries to assessors, posing as homeowners seeking information about how to appeal. We randomize the implied race and gender of the homeowner and whether the email mentions having evidence that the assessed value is too

high. By observing assessors' responses, we measure whether homeowners' demographics influence the assessors decision to provide help and whether providing concrete evidence mitigates any such bias to examine both the extent and nature of discrimination.

Our findings reveal large disparities in assessor treatment of homeowners, driven by distinct mechanisms by gender and race. Emails sent from female and Black homeowners receive 10 and 17% fewer replies from assessors than identical inquiries from white or male homeowners, respectively. While responses to minority homeowners did not improve when the e-mail mentioned evidence, the gap between male and female homeowners disappeared with evidence. These findings suggest that assessors discriminate against female homeowners due to statistical discrimination and against minority homeowners due to animus.

Second, we conduct a field experiment to study whether differential information frictions and perceived discrimination affects homeowners' initiative in negotiations. 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. We randomize a subset of participants to receive a combination of information about the average financial benefits, procedural guidance, and a financial protection against unsuccessful appeals in terms of a researcher-backed last-dollar insurance. We match our survey data to county administrative data on appeals and appeal outcomes to measure the differences in negotiation behavior and the treatment effects on the gaps in property tax burdens.

Before the experiment, female and minority homeowners were 27 and 38% less likely know that they could appeal compared to male and non-minority homeowners. These knowledge gaps translated into lower appeal rates in pre-experimental data and among our control group, who learned they could appeal but received no further guidance or incentives,[1] despite both groups being significantly more likely to state they intend to appeal.

We estimate heterogeneous responses to information and incentives to provide evidence that the drivers of these gender and racial gaps are distinct. Minority homeowners

[1]These findings are consistent with past evidence (e.g. Weber and McMillen, 2010; Doerner and Ihlanfeldt, 2014; Ross, 2017; Avenancio-León and Howard, 2022).

do not respond to information that the average financial benefits from appealing are higher, but do respond to our researcher-backed last-dollar insurance (14.4 percentage point increase), while non-minority homeowners respond to information but not insurance. Based our conceptual framework, these results are consistent with minority homeowners correctly perceiving that they face discrimination, as we document in the correspondence study.[2] In contrast, the gender gap disappears when women receive information, aligning with evidence that assessor negotiation with women depends on perceived credibility. Despite our treatments closing the appeal gaps, assessor discretion in handling appeals does not reduce the outcome gaps.

Overall, our results indicate that reducing disparities requires addressing both taxpayer initiative and administrative discretion, as policies that only target one side of the market can backfire. Focusing only on administrator discretion would have only small effects because few homeowners appeal, and those who do are mainly white and male. Removing only homeowner information frictions in appeals would widen the Black-white gap by about $2400 (7.6% of the baseline from Avenancio-León and Howard (2022)) and the gender gap by $2300. Removing both information frictions and differential treatment appeal outcomes, including homeowners' perceptions of discrimination, would instead shrink the Black-white assessment gap by $1700 (5.5% of the baseline) and the gender gap by $2000.[3]

Our central contribution is the documentation of the causal mechanisms through which tax negotiations produce disparities in property taxes, disentangling the roles of administrative and taxpayer behavior. Our complementary experiments with assessors and homeowners demonstrate that the interplay between taxpayers and administrators generates a double disadvantage, which helps explain the persistence of the property tax assessment

[2]We provide additional evidence these patterns are not driven by other factors, like knowledge differences or risk preferences.

[3]When resolving information frictions, we calculate the increase in appeals from providing information and a step-by-step guide multiped by the percentage change in assessed value from appealing (by demographic). We then convert this to a dollar amount for a homeowner with an assessed value of $250K, the average overall in the county. When additionally resolving discrimination, we calculate the increase in appeals for minority homeowners from the last-dollar insurance and equalize the percent reduction in assessed values from appealing by demographic.

gap. Together, our findings on both sides of the market suggest that the appeals process, designed to mitigate biases and inaccuracies in assessments, may instead exacerbate inequality. Female and minority homeowners not only appeal at lower rates but also receive less help when they do and systematically experience poorer outcomes, indicating that the current system fails to ensure fairness. Our results therefore highlight how seemingly race and gender-neutral policies have disparate impacts across groups (e.g. Herd and Moynihan, 2019; Small and Pager, 2020), affecting gaps (e.g. Agan and Starr, 2018; Brandon et al., 2023).

Beyond tax systems, we highlight that gender- and race-based negotiation disparities, more broadly, operate through distinct mechanisms. In laboratory and labor market settings, women are less likely to perceive situations as negotiable, partly due to greater social penalties for self-advocacy (Bowles et al., 2007; Castillo et al., 2013; Leibbrandt and List, 2015). We find that similar mechanisms affect gender gaps in tax administration; women were 43% less likely than men to appeal, echoing that women often "don't ask" (Babcock and Laschever, 2021). Among those who do negotiate, final outcomes jointly depend on potential gender differences in tactics (Andersen et al., 2021; Goldsmith-Pinkham and Shue, 2023; Roussille, 2024) and the beliefs and biases of administrators (Ayres and Siegelman, 1995; Exley et al., 2020; Egan et al., 2022). Our results show that while information on how to negotiate well and persuade administrators can help close the gender gap in initiative, it neither closes outcome gaps nor racial disparities that stem from animus discrimination. Therefore, it is not sufficient to only "fix" the negotiator or "fix the institutions" (Recalde and Vesterlund, 2023) as policies on both sides of the market are needed.

To make these contributions, we design field experiments on both sides of the market, guided by an economic model. We extend previous work on tax disparities that considers each side in isolation.[4] First, the correspondence study documents that assessors discriminate (Jowell and Prescott-Clarke, 1970; Bertrand and Duflo, 2017; Kline et al., 2022) unlike other public officials (Gaddis et al., 2021), reflecting their high amount of discretion. By

[4]There is some evidence in low-capacity settings on interactions but without simultaneously intervening on both sides. See Jensen and Weigel (2024).

randomizing the content of the correspondences, we additionally identify the nature of discrimination separately for gender and race (List, 2004; Guryan and Charles, 2013; Buzard et al., 2025), and further explore moderators to consider assessor biases. Second, we build on information provision experiments (e.g. Haaland et al., 2023; Giaccobasso et al., 2022; Nathan et al., 2025).[5] We design information and financial incentives to parse mechanisms that distort taxpayer initiative and link the survey to administrative records to further study assessor discretion in appeal outcomes. Overall, we highlight the importance of measuring both sides of the market to study systemic discrimination as taxpayer initiative and assessor discretion compound one another (Bohren et al., 2025).

2. Conceptual Framework

We consider the interactions between homeowners and assessors in the appeals process to highlight how administrator discretion and taxpayer initiative in negotiations affect tax burdens and disparities. This process consists of three stages, shown in Figure 1. In this game, homeowners decide whether to appeal based on how they anticipate assessors will handle their case, and assessors decide how to handle the appeal based on homeowner behavior. This section describes the game and its implications for homeowner and assessor behavior.

2.1 Set-up

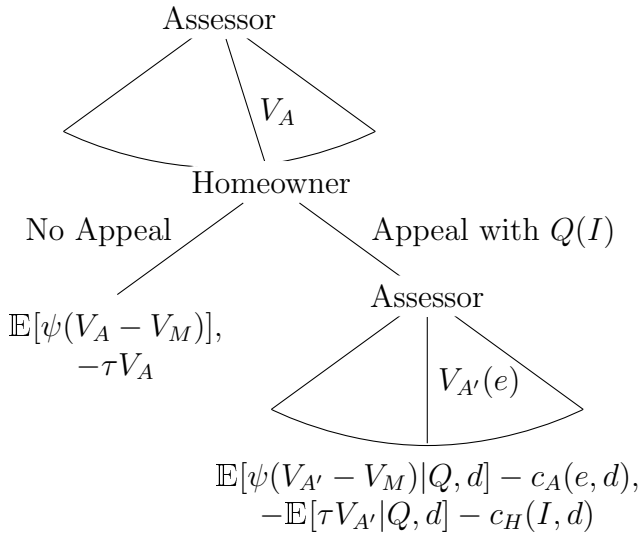
First, the assessor determines the assessed value, V_A. The assessor's objective is to construct V_A so that, on average, the values match each home's fair market value, V_M, which is not directly observable. This goal aligns with a targeting objective where higher-net-worth individuals, who on average have higher incomes and higher valued homes, should pay more to fund public services. Overassessments of low-income individuals, in particular, undermine targeting by increasing their tax burdens relative to their high-income neighbors. This objec-

[5]We specifically consider how beliefs and information frictions affect disparities, (as in Alesina et al., 2021; Haaland and Roth, 2022; Akesson et al., 2022), and its implication for tax and housing policies (as in Bérgolo et al., 2017; Perez-Truglia and Troiano, 2018; Bottan and Perez-Truglia, 2025).

tive also aligns with state laws requiring assessors to attempt to assess homes as accurately as possible.[6]

Based on this objective, the assessor's payoff is determined by the expected discrepancy between V_A and V_M and any costs from effort, e, from interacting with a homeowner with demographics d, during the appeal process, $c_A(e, d)$. More specifically, the assessor faces a penalty that is an increasing function of the discrepancy, $\psi(V_A - V_M)$. Although assessors intend for $\mathbb{E}[V_A - V_M] = 0$, their choices in how to estimate V_M may lead to relatively higher V_A for some homeowners.

Figure 1: Assessor-Homeowner Interactions



Notes: This figure presents the extensive form game between assessors and homeowners in determining the homeowners' assessed values. The game has three stages: (1) assessors determine the assessed value, then (2), homeowners decide whether to appeal, and (3) when a homeowner appeals, the assessor makes a final determination of the property value. Payoffs are listed first for the assessor and then for the homeowner.

In the second stage, the homeowner receives the assessed value from the assessor, creating a property tax liability of τV_A, based on the local tax rate, τ. After learning V_A, the homeowner has two options. As a first option, she can accept the valuation and pay the corresponding amount in taxes ("No Appeal"). Her payoff is this case is the negative of her taxes paid, $-\tau V_A$. Her second option is to enter into a negotiation with the assessor

[6]For example, under Indiana State Statute IC 6-1.1-37-2, doing otherwise is a Class A misdemeanor that carries a punishment of imprisonment for not more than one year, and a fine of not more than $5000.

by filing an appeal. If she chooses to do so, she faces a cost $c_H(\cdot)$ from the administrative burdens that depend in part on the difficulty of providing high-quality evidence that her home is overassessed and the stresses associated with the negotiating with the assessor. Therefore, this cost is a function of both the homeowner's information and beliefs about the appeal process, I, and her demographics, d, including gender and race. c_H depends on demographics both directly because negotiating with a discriminating assessor is more costly and potentially indirectly based on how it determines the difficulty in finding high-quality evidence across neighborhoods. In the appeal, the homeowner proposes a counteroffer to V_A based on evidence, Q.

Finally, if the homeowner appeals, then she and the assessor negotiate over the final assessed value $V_{A'}$ based on both V_A and Q. In practice, $V_{A'} \leq V_A$.[7] Handling the appeal to change the assessed value to $V_{A'}$ is costly for the assessor, with c_A based on the time and effort, e, required to consider the new evidence, investigate the case, and negotiate with the homeowner. We assume $|V_{A'} - V_M|$ is decreasing in e. Under animus discrimination, c_A is also a function of the homeowners' demographics as assessors face an additional cost of interacting with non-white male homeowners. If the appeal is successful, $S = 1$, then $V_{A'} < V_A$ and if it is not successful, $S = 0$, then $V_{A'} = V_A$.[8] Homeowners' benefits from appealing depend in part on how much effort assessors exert as that determines $V_{A'}$.

2.2 Assessor discretion

We now consider, through the lens of this framework, how assessor and homeowner engagement in the appeal process creates disparities. We first consider the assessor's choice of effort and reevaluation of the assessed value in the final subgame. The assessor has two potential reasons to provide relatively low effort for some homeowners and either deny their appeals

[7] In theory, it is possible for an appeal to lead to a higher assessed value if the assessor learns something new about the property, for example, because of an unpermitted improvement. Empirically, we do not see increases in the data from our experiment.

[8] In practice, a homeowner who is unhappy with the decision could appeal $V_{A'}$ to bring the case before a judge and argue against the assessor in court. For simplicity, we view this as a continuation of the negotiation process with the assessor and collapse this into one stage of the game.

or only minimally reduce their assessed values. First, if animus discrimination increases the cost of negotiating with certain homeowners, for example based on their race, then assessors will not find it worthwhile to correct the assessed values—the cost of negotiating is more than the benefits of matching the assessed value to the market value.

Second, the assessor may believe that the assessed value is close to the market value for some homeowners and so the refinement is not worth the effort. Under race or gender based statistical discrimination, differences in assessor effort could arise because the assessor believes that the discrepancy between V_A and V_M is relatively small for certain homeowners based on their demographics, i.e. $\mathbb{E}[V_A - V_M | d = D] < \mathbb{E}[V_A - V_M | d = D']$. Although this belief would be incorrect for racial minorities (Avenancio-León and Howard, 2022), it may be consistent with the fact that appeals by minority homeowners are less likely to be successful. These group differences can come from inaccurate statistical discrimination (Bohren et al., 2019) as well as more systemic issues like differences across neighborhoods in the quality of potential evidence (Weber and McMillen, 2010). In this case, homeowner provided evidence can play an important role to update the assessor's beliefs about the discrepancy in valuations especially for minority homeowners.

While our analysis focuses on negotiations during the appeal process, this framework additionally has two implication for disparities in assessments. First, by backward induction, the assessor should set V_A to discourage homeowners from filing an appeal and therefore avoid paying c_A. Since white, male homeowners are more likely to appeal than others, potentially because of differences in c_H, the assessor should strategically set $V_A < V_M$ for this group. Mechanically, this leads other groups who are less likely to appeal with $V_A > V_M$ to ensure $\mathbb{E}[V_A - V_M] = 0$, as mandated by law. Second, the appeals process can affect the assessor's beliefs about which types of homes are overassessed based on Q, leading them to change their assessment processes in ways that favor homeowners who are more likely to appeal.

2.3 Homeowner Appeal Behavior

We now consider homeowner's decision of whether to file an appeal to enter a negotiation with the assessor, based on her beliefs about how the assessor will evaluate her case. By backward induction shown in Figure 1, the homeowner appeals if:

$$\mathbb{E}[\tau(V_A - V_{A'})|Q, d] > c_H(I, d) \tag{1}$$

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

Based on Equation 1, homeowners appeal when the expected benefits from a reduced tax burden exceeds the cost of navigating the process, both finding evidence and negotiating with the assessor. Both costs and benefits likely differ by race and gender, creating disparities in appeal rates and outcomes. Minority and female homeowners could face higher costs if they have less or worse knowledge of how to appeal, including providing evidence Q, based on differences in I. Even absent perceived discrimination, minority and female homeowners may also have pessimistic beliefs about the financial benefits.[9]

Disparities in appeal rates can also arise from perceived (or actual) discrimination by assessors as homeowners choose whether to appeal based on their expected treatment by assessors. Discrimination has two potential effects. First, it lowers the financial benefits of appealing as it leads assessors to invest less effort and reduce $V_{A'}$ by less as discussed above. Second, discrimination increases the costs of engaging with the assessor through c_H. These effects can come from direct interactions with the assessors or the administrative burdens (Ray et al., 2023). Both of these effects reduce the gains from appealing for homeowners, and therefore drive down appeal rates.

In line with this framework, we design two experiments with assessors and homeowners that allow us to measure which factors drive racial and gender disparities in property

[9]These differences could be due to either incorrect beliefs or other differences that are correlated with homeowner characteristics. For example, if minority homeowners live in homes with fewer high-quality comparables, then it will be more difficult to argue that the original assessment is incorrect.

tax appeals. We first present a correspondence study with assessors to measure how their effort in the appeals process differs with homeowner characteristics. In this experiment, we additionally consider how evidence provided by different types of homeowners differentially shapes assessor effort as a test of animus verse statistical discrimination. Moreover, since the nature and extent of discrimination and assessor beliefs are key primitives of the model, we test whether assessor behavior varies with their characteristics. We then present a framed field experiment with homeowners to understand the drivers of racial and gender appeal gaps, including anticipated discrimination by assessors. Finally, we measure the outcomes of appeals in the homeowner experiment to provide additional insights on homeowner and assessor interactions.

3. A Correspondence Study with County Assessors

We conducted a correspondence study with county assessors to provide clear evidence of whether there is discrimination in appeals and the underlying mechanisms. Discrimination by government officials is important to document as it affects disparities in tax burdens and trustworthiness of the government, and thereby affects how homeowners engage in the appeal system. Minority and female homeowners may be less likely to file an appeal to correct excess tax burdens if appealing requires negotiating with a discriminating assessor. Moreover if assessors discriminate, minority and female homeowners who do appeal will face worse outcomes, widening existing disparities in tax burdens.

In the experiment, we posed as homeowners and emailed county assessors across several states for help navigating the appeal process.[10] Based on our conceptual framework in Section 2, we vary the homeowner demographics, d, and whether the email contained evidence that the homeowner was overassessed, Q. Every email requested information on how to appeal the assessment, a copy of the required documents, the deadline, and advice

[10]The experiment 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 E, we discuss the ethical considerations of the study using the framework of Asiedu et al. (2021).

on best practices. More concretely, the emails asks for advice on how to negotiate effectively.

Given the administrative burdens of filing an appeal, providing sufficient information over email is time-intensive for the assessor, requiring effort. An assessor may therefore selectively choose to not interact with certain individuals out of distaste (animus discrimination) or because she does not believe that their properties are overassessed (statistical discrimination). Given these two potential mechanisms, racial minorities and women may face different types of discrimination.

Responses to homeowners' requests for help are a natural measure of assessor behavior in this setting. Many assessors request that homeowners directly contact them before filing to discuss the appeal, and in some counties these contacts are required to initiate the process.[11] Email responses are therefore similar to interview requests in labor-market correspondence studies, serving as a test for discrimination in the first step in the process while providing evidence of discrimination throughout (Quillian et al., 2020). We later consider discrimination in final appeal outcomes using an experiment with homeowners in Section 4.

3.1 Experimental Design and Data

We conducted the correspondence study in six states with appeal deadlines between mid-June and mid-September in 2023: 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.

Based on the framework in Section 2, we randomize two features the emails: the name to signal the demographics of the homeowner and whether the homeowner states she has evidence that she is overassessed. These treatments allow us to both document discrimination against women and racial minorities and test for the underlying mechanisms. Since the emails

[11] These requests therefore function as natural instances of citizen engagement where, as part of their job, officials should expect to receive requests or information and respond (Colmer et al., 2023; Hurst et al., 2024).

are designed to be identical regardless of the race and gender of the homeowner, differences in response rates by demographics indicate discrimination. Additionally, if this difference is due to statistical discrimination where assessors believe that certain types of homeowners are less likely to be overassessed than others based on their race and gender, then providing evidence, Q, should be more informative about overassessment for former group and therefore reduce the response gap. However, if the difference is due to animus discrimination, then including evidence in the email should not reduce the response gap as evidence does not change the distaste of interacting with certain individuals through c_A.[12]

Each assessor in the sample received six emails, one each from {white, Black, Hispanic} $\times$ {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 of homeowners.[13] We additionally randomized three of the emails to each assessor 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).[14] We staggered e-mails to prevent assessors from learning that the e-mails are fictitious and find no evidence that discrimination is driven by assessors learning that the e-mails are fictitious over time (See Appendix A).

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. We additionally observe the gender and race of each assessor based on information available online, including photos and written profiles. Based on this data, we estimate the response rates by homeowner demographics overall and separately for emails with and without evidence to test for discrimination in the market and the mechanism. Additionally, since assessor beliefs and

[12]We would expect, if anything, providing evidence to increase the assessor's cost as it creates additional work to estimate the true assessed value and update it. Without evidence, the assessor does not have a reason to doubt the original estimate of the market value and the time cost of engaging with a homeowner is minimal.

[13]We only include first names so that county assessors would not be able to try to identify an individual based on the full name. Some email platforms require a last name to make an account and for these we included an initial.

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

discrimination are important primitives in the model, we consider how these disparities vary by assessor characteristics, namely gender.[15] A full description of the experimental design appears in Appendix A.

3.2 Evidence of Assessor Discrimination

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

$$R_{cf} = \gamma_0 + \sum_d \gamma_d D_{cf} + \lambda_s + \varepsilon_{cf} \tag{2}$$

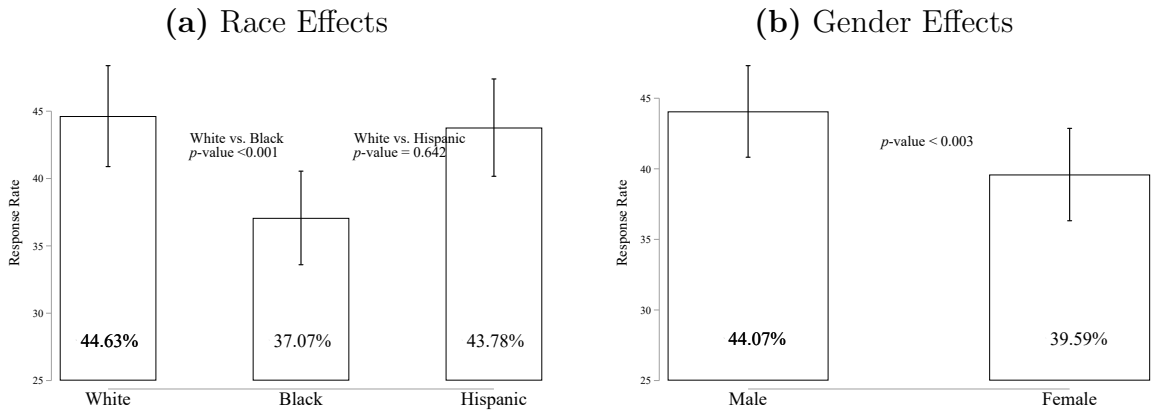
where R_{cf} is 100 if the assessor from county c in state s responded to the email from a fictitious homeowner f of demographic, d, and 0 otherwise. D_{cf} is an indicator that is 1 if the homeowner is of demographic d. We additionally include state fixed effects, λ_s. In each specification, we focus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

Overall, 42% of emails received a response from an assessors. Figure 2 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 10% (4 pp) fewer responses than male homeowners.

Differences by homeowner race and gender in responses highlight that assessors have sufficient discretion to selectively exert effort to engage more with only certain types of homeowners. These differences can have direct effects on who appeals because white and male homeowners are more likely to get information that helps to reduce information and filing frictions and appeal outcomes if the information improves appeal quality. A non-response from the assessor may also deter racial minority and female homeowners if it signals that the assessor will be more hostile in negotiations during the appeal. For example, Quillian

[15] We note that only 18 percent of assessors in our sample are non-White.

Figure 2: 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.

et al. (2020) finds evidence from labor market audit studies that discrimination in the final stage of job offers is much larger than discrimination in callbacks.

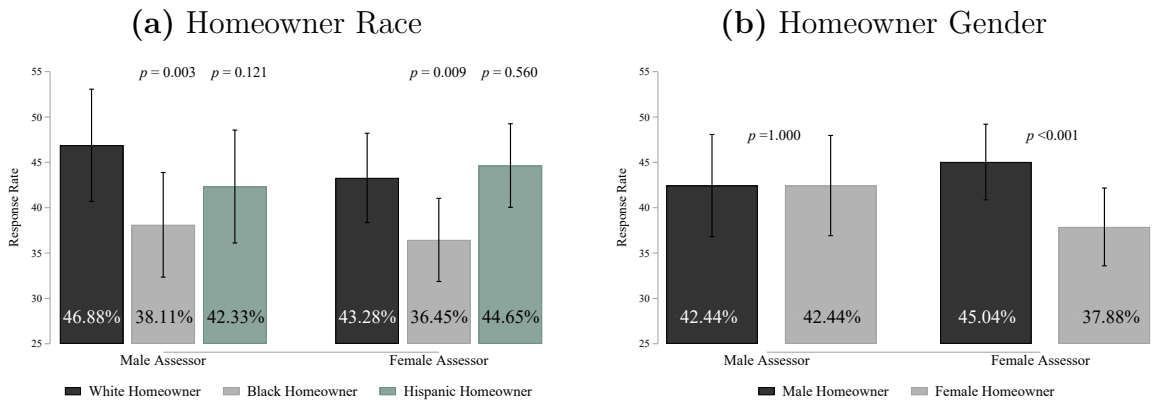
We next turn to understanding the extent assessor discrimination. We first use our within-assessor design to document the distribution of assessor discrimination (Czibor et al., 2019; Kline et al., 2022).[16] Appendix Figure B1 shows that in just over two-thirds of counties, assessors respond to some but not all of the fictitious homeowners, reflecting their discretion in choosing with whom to engage. Moreover, while a majority of assessors respond to a larger share of the white and male homeowners than black and female homeowners, a considerable number respond to a larger share of disadvantaged homeowners. These differences across assessors in the likelihood they respond to different homeowners may stem from variation in prejudice and beliefs.

A potential driver of these across-assessor differences in behavior is that assessors vary in their costs and benefits of helping certain homeowners. We expect these attributes to be correlated with assessor characteristics, including gender. Out of the 410 assessors in

[16]Appendix Section A.5 presents evidence against the possibility that sending multiple emails to each assessor causes us to conclude discrimination from assessors spuriously.

our sample, 62% are female. We estimate versions of Equation 2 in which we additionally allow for heterogeneity by the assessor's gender and present the results in Figure 3. While we find evidence that male and female assessors racially discriminate against Black homeowners, only female assessors discriminate against female homeowners. These results suggest that differences across assessors, proxied here by gender, matter for how they interact with homeowners. Our findings are similar to evidence from Broder (1993) that female evaluators are harsher on other women, but the evidence across settings is mixed (Recalde and Vesterlund, 2023), likely pointing to the importance of gender differences in beliefs that are context specific.

Figure 3: Discrimination 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.

3.3 Mechanisms Driving Assessor Discrimination

Whether this unequal treatment comes from prejudice (animus) or beliefs (statistical discrimination) has important consequences for the homeowner's value of filing an appeal, as discussed in Section 2. If the discrimination is animus, then prejudice against certain home-

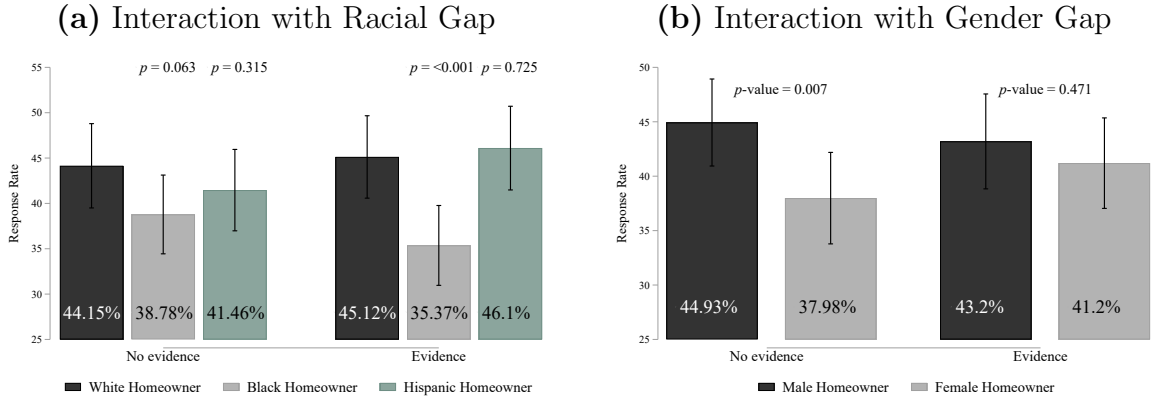
owners will make their negotiation more difficult and the assessor may invest less effort to help reduce their tax burdens, regardless of the quality of evidence included. If, instead, the discrimination is statistical, then appealing is valuable for certain homeowners when they can present high-quality evidence that changes the assessor's beliefs. Therefore, the policy solutions for each type of discrimination differ. We cross-randomized homeowner identity with whether the homeowner included evidence to parse these types of discrimination across different types of homeowners.

To test whether and for which types of homeowners discrimination is animus or statistical, we estimate heterogeneous treatment effects of providing evidence on the response gaps, across all assessors and then separately for male and female assessors. We first estimate a version of Equation 2, where we additionally interact the indicators for homeowner demographics with whether the email included evidence. We present our results in Figure 4. The figure includes the estimated fraction of homeowners who receive a response by race and gender 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.

We find that providing evidence has no effect on the average racial gap between Black and white homeowners, but has a meaningful effect for gender gap. While evidence does not affect the likelihood of receiving a response for Black, white, or male homeowners, this treatment increases responses by 8.5% for female homeowners and closes the gender gap by more than 70%. While evidence considerably reduces the magnitude of the gap and we cannot reject that male and female homeowners are equally likely to receive a response when providing evidence, our estimate of the effect of evidence for female homeowners is not statistically different from 0 ($p = 0.226$). Through the lens of our framework, these results suggest that discrimination against Black homeowners is due to animus discrimination while discrimination against female homeowners is due to statistical discrimination.

We additionally find that providing evidence increases the likelihood that an as-

Figure 4: Effect of Evidence



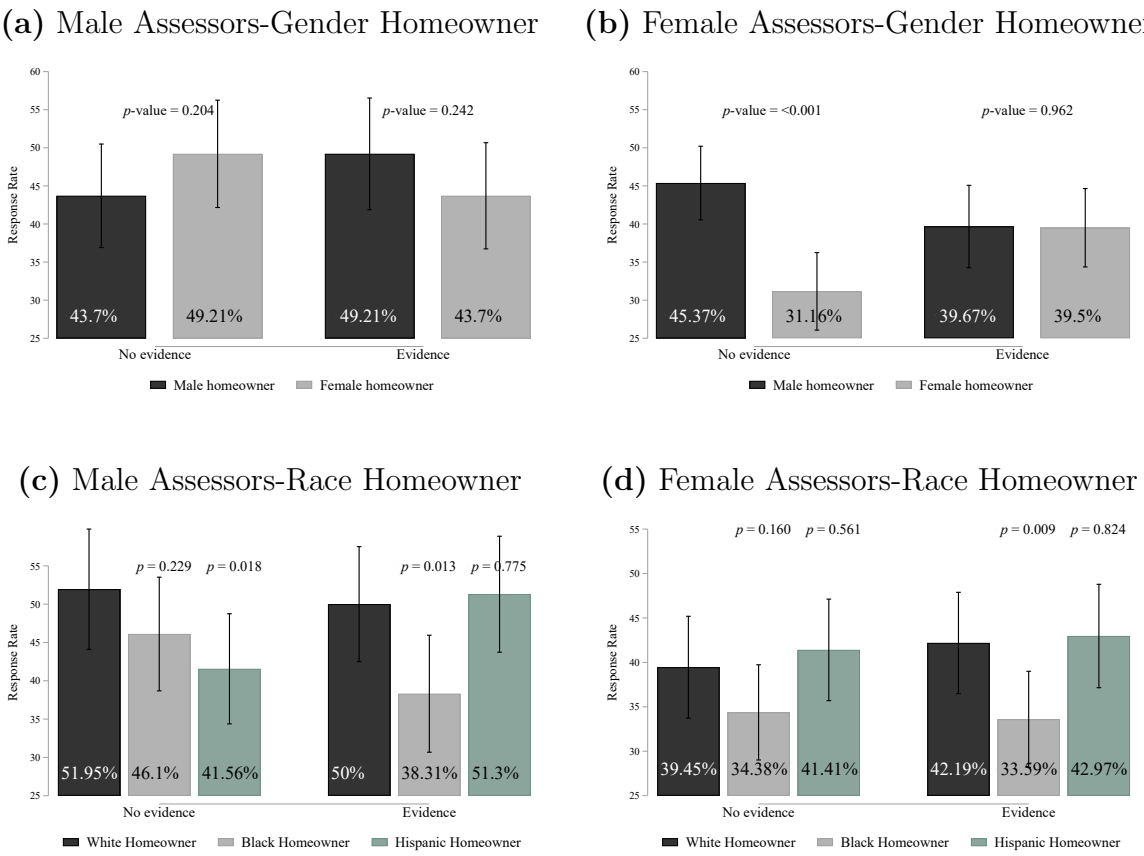
Note: This figure displays the average response rates to fictitious homeowners in the correspondence experiment. Panel (a) displays the average response rates by the intended race of the fictitious homeowners and whether the homeowner provided evidence to the assessor that their assessment was incorrect. 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.

sessor responds to a Hispanic homeowner by 4.6 pp (11%) ($p = 0.100$). This treatment therefore completely offsets the 2.7 percentage point (6%) gap between Hispanic and white homeowners when neither provide evidence, although this baseline gap is not statistically distinguishable from zero. Acknowledging that these results are imprecise, they suggest that Hispanic homeowners face statistical discrimination.

Homeowner provided evidence should mainly be effective at changing the behavior of assessors who believe that female and Hispanic homeowners are less likely to be overassessed. We therefore reconsider the treatment effects of providing evidence on response gaps by assessor gender, a characteristics that is potentially related to assessor beliefs, and present the results in Figure 5. We first focus on homeowner gender gaps in panels (a) and (b). 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 14.2 pp (31%, $p < 0.001$) fewer response than male homeowners when they do not provide evidence, and that the gap almost entirely

closes when homeowners provide evidence—the evidence treatment increases the likelihood a female assessor responds to a female homeowner by 8.4 pp ($p = 0.010$).

Figure 5: Effect of Evidence on Response Gaps by Assessor Gender



Note: This figure displays the average response rates to fictitious homeowners in the correspondence experiment. Panels a and b present the average response rates by combinations of assessor and homeowner gender, with and without evidence. Panels c and d present average response rates by combinations of assessor gender and homeowner race, both with and without evidence. Both panels also display 95% confidence intervals and p-values of tests of equality of response rates for each group relative to male homeowners in panels a and b and relative to white homeowners in panels c and d conditional on whether the email contained evidence.

In Panels (c) and (d) we also see that both discrimination against Hispanic homeowners is limited to male assessors and that evidence completely closes the Hispanic-white response gap. Without evidence, Hispanic homeowners receive 10.3 pp (20%) fewer responses than white homeowners from male assessors ($p = 0.018$). Providing evidence increases the likelihood a male assessor responds to a Hispanic homeowner by 9.7 pp ($p = 0.019$), with

no effect on white homeowners. In Panel (d), we see that female assessors are less likely to respond to Black homeowners, and no less likely to respond to Hispanic homeowners compared to white homeowners both with and without evidence. These results suggest that male assessors statistically discriminate against Hispanic homeowners, and female assessors do not discriminate against Hispanic homeowners, but against Black homeowners.

Overall, the results from the correspondence study with county assessors suggest that female, Black, and Hispanic homeowners face particular barriers to appealing. While information about how to properly provide evidence may be sufficient for improving outcomes for female and Hispanic homeowners, it will have less of an impact on reducing animus discrimination against Black homeowners. We next consider these potential mechanisms of information frictions and perceptions of discrimination in a framed field experiment with homeowners.

4. Homeowner Barriers to Appealing

As a complement to the correspondence study with assessors, we now consider the role of taxpayer initiative on the taxpayer side of the market. In Section 2, we showed that assessor discrimination lowers the potential reduction of the tax burden and increases the cost of negotiating, deterring homeowners from filing an appeal. We conduct an experiment with homeowners in Marion County, Indiana to understand the drivers of gender and racial disparities in appeals. That is, to what extent are these gaps driven by differences across homeowners themselves or by assessor discrimination? We design treatments, in light of the framework in Section 2, to investigate how information frictions in the expected returns, costs from navigating the process, and perceived discrimination affect homeowners' selection into appealing. We additionally look at gender and racial differences in the appeal outcomes to further understand the role of assessor discretion during appeal negotiations in shaping tax disparities.

4.1 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. 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.[17] 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.

Assessed values for Marion County are finalized by July. Then, April of the following year, the 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. The notification also informs households of their right to appeal their assessment and directs interested households to resources to learn more. For many homeowners, these letters have low salience (Cabral and Hoxby, 2012; Hayashi, 2014).

Households who disagree with their assessed value have until June 15, an approximately two-month window after assessment mailing, to submit an initial appeal. 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.[18]

[17] 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.

[18] 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.

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 as it affects their standing in the negotiation process.[19]

4.2 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, and provide information and incentives that change the costs and benefits of appealing. We then link the survey data to administrative records to observe who appeals. 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 four conditions: (1) *Appeal Information*, (2) *Appeal Aid*, (3) *Insurance*, and (4) *Control*. We outline each treatment below and discuss their interpretations based on our framework in Section 2.

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

[19]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.

appeal gaps exist because female or 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 these homeowners. However, the extent to which different types of homeowners update their beliefs about their own likelihood of success also depends on their perceptions of assessor discrimination; for homeowners who face discrimination, the county average overstates the benefits they should expect to receive.

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. (2025), 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, $c_H(Q, d)$, that are due information frictions. However, this treatment does not resolve differences in costs from animus discrimination.

We further build on Nathan et al. (2025) to study how differences in knowledge of how to negotiate (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.[20] As in Equation 1, knowledge about how to provide sufficient evidence affects both the cost and benefits of appealing. Additionally, by measuring homeowners' choices of comparable homes as evidence, we consider whether gender and racial differences in the likelihood of success and reduction in taxes owed from appealing are due to differences in the quality of evidence provided based on both their choices and the availability of evidence in their neighborhoods.

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

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 appeal costs, c_H.

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

$$\mathbb{L}\Big[\underbrace{75\mathbb{L}(1-\mathbb{P}[S|Q,I,d])}_{\text{Lottery benefit if unsuccessful}}+\underbrace{\mathbb{P}[0<\Delta T<75|Q,I,d]\big((75-\Delta T)\big)}_{\text{Lottery benefit if receive minimal tax relief}}\Big]\geq 0 \tag{3}$$

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

Based on Equation 3, 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 insurance. If discrimination is a barrier for some 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. 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. Based on the information we provide, the average homeowner in our sample should expect to receive about \$400 from appealing,

making $75 a left-tail outcome.[21] However, they may still value *Insurance* since it reduces the left-tail risk.[22] Based on this model-based interpretation of the *Insurance* treatment, an increase in appeal rates over the *Appeal Aid* treatment indicates that homeowners perceive that they face discrimination. We 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, 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 the property tax system 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 and appeal outcomes.

4.3 Disparities in Negotiation Initiative

Before turning to the experimental results, we first document racial and gender differences in homeowners' baseline initiative to appeal, as well as appeal outcomes, and explore potential correlates of these differences. Establishing these baseline patterns serves two purposes. First, it allows us to quantify the extent of disparities in appeal behavior that exist absent our interventions. Second, it helps identify the channels, such as knowledge of the appeals

[21] The $400 estimate is based on the county effective tax rate of 1% and the average home value of about $250K.

[22] This function of insurance is similar to Cullen et al. (2023).

process, prior experience with the tax system, or perceptions of discrimination, that may drive these disparities. This descriptive evidence provides a benchmark against which we can interpret the treatment effects.

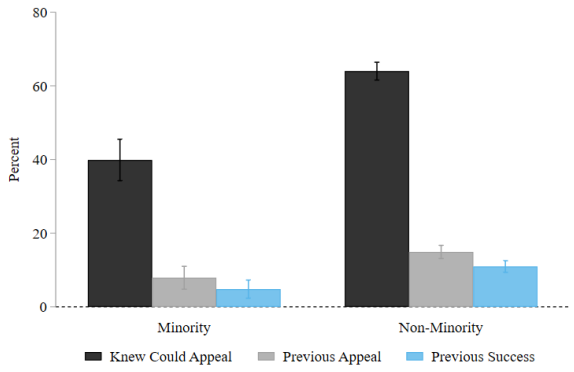
Sentiments There are widespread beliefs that property tax assessments are biased with only 20.75% agreeing that there is no income discrimination in assessments and 18.78% agreeing that there is no income discrimination in appeals. Similarly, only 20.5% of subjects believe there is no racial discrimination in assessments and 19.5% believe the is no racial discrimination in appeals (Appendix Table D3).

Appeal Process Knowledge and Experience 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 ($p < 0.001$), and 7 percentage points less likely to have previously appealed ($p < 0.001$), as shown in Panel (A) of Figure 6). We also find that minority homeowners are 6 percentage points less likely to have had a successful appeal that reduced their tax burdens ($p < 0.001$). Similarly, female homeowners were 18 percentage points less likely than male homeowners to have known they were able to appeal ($p < 0.001$), 4.6 percentage points less likely to have previously appealed ($p = 0.007$) and 3 percentage points less likely to have had previous success appealing ($p = 0.040$), shown in Panel (B).

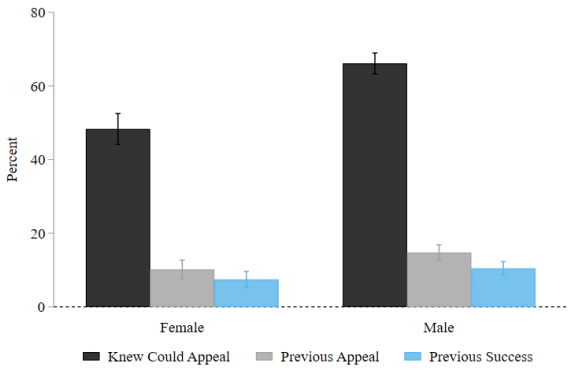
Perceived Costs to Appeal Both beliefs about discrimination and information frictions should affect homeowners' perceived cost to appeal.[23] We plot the distribution of perceived costs as a percent of the individual's tax burden by race and gender in Appendix Figure D2. Minority homeowners' perceived costs are 8.0 pp higher than non-minority homeowners ($p < 0.001$) and female homeowners' are 2.7 pp higher than male homeowners ($p = 0.035$). This relatively large difference for minority homeowners potentially indicates the costs associated

[23]To measure perceived costs, we ask "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?" We then convert this to a percent of the tax burden to make comparable across homeowners.

Figure 6: Differences in Knowledge of Appeals and Pre-Survey Behavior



Panel A: Racial Differences



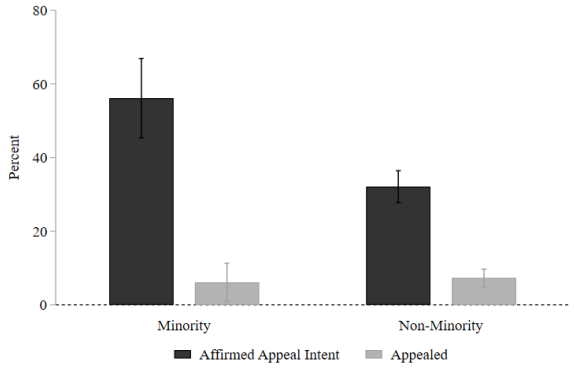
Panel B: Gender Differences

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

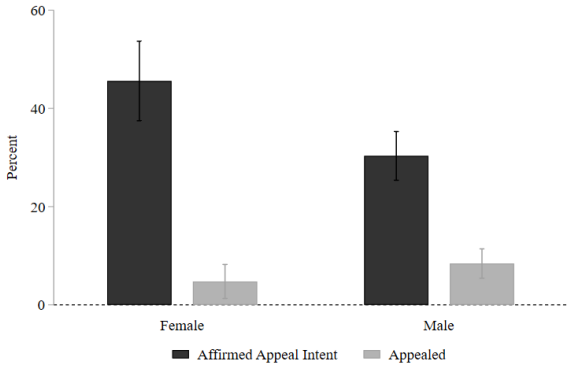
with interacting with a prejudice assessor, as we document in the correspondence study, in addition to information frictions.

Baseline Selection into Negotiation Using our control group, we present average homeowner appeal intentions and behavior after the experiment. Figure 7 displays the gaps in appeal intentions and behavior for those randomized into the control group. Overall, 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 that they are overassessed because they face discrimination in the assessment process and so the financial returns are greater, but they are less likely to actually file an appeal. Similarly, female homeowners are 10.25 percentage points ($p < 0.01$) more likely to affirm an intention to appeal while 3.7 percentage points ($p = 0.12$) less likely to actually appeal than male homeowners.

Figure 7: Differences in Behavior for Untreated Homeowners



Panel A: Racial Differences



Panel B: Gender Differences

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

Negotiation Tactics 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. Importantly, respondents make these decisions regardless of whether they ultimately choose to appeal. 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 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 1 presents the multinomial logit

Table 1: Determinants of Taxpayer Choice of Comparable by Race and Gender

	(1) Non-Minority	(2) Racial Difference	(3) Men	(4) Gender Difference
Distance	-0.27***	-0.01	-0.30***	-0.08
	(0.06)	(0.14)	(0.07)	(0.12)
Effective Year Built	-0.00	-0.00	-0.00	0.00
	(0.00)	(0.01)	(0.00)	(0.01)
Year Built	0.01***	-0.00	0.01**	0.00
	(0.00)	(0.01)	(0.00)	(0.00)
Same Neighborhood	0.74***	-0.44	0.67***	-0.24
	(0.13)	(0.32)	(0.16)	(0.25)
Log Sale Price	0.80*	0.47	0.52	0.59
	(0.45)	(0.99)	(0.54)	(0.83)
Number of Bathrooms	0.12	0.05	0.10	-0.01
	(0.09)	(0.22)	(0.11)	(0.19)
Number of Bedrooms	0.18**	-0.23	0.15	-0.06
	(0.08)	(0.18)	(0.10)	(0.15)
Log Home SQFT	-1.09***	0.99*	-0.97***	0.44
	(0.25)	(0.54)	(0.30)	(0.46)
N	4,015	4,790	2,750	4,340

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 non-minority homeowners in the *Appeal Aid* and *Insurance* treatments who select a comparable home. Column (2) presents the estimated difference by race. Columns (3) and (4) present analogous results for women and men, excluding homes with both a man and woman named on the deed. Each observation is an individual-option so that each individual. Standard errors are clustered at the individual level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

coefficient estimates from the choice model for non-minority homeowners in Column (1) and the difference between minority and non-minority homeowners in Column (2). Similarly, we present estimates for male homeowners in Column (3), and the difference between female and male homeowners in column (4).

Our results confirm that homeowners, on average, understand what constitutes appropriate evidence for a successful appeal. Homeowners choose a comparable home that is in their neighborhood. We also find that homeowners tend to choose relatively expensive homes from options in which all sale prices are less than their assessed value. This suggests that they acknowledge that a comparable sale price that is too low has a lower likelihood of

being considered 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.

Moreover, our results in Table 1 indicate that there are minimal differences in evidence by race or gender. 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.[24]

4.4 Mechanisms Driving Disparities

In this section, we use behavioral responses to our randomized treatments to understand which factors drive the appeal gaps. We estimate the effect of each of our treatments relative to homeowners of the same demographics in the control group, separately by race and gender using the following specification:

$$\begin{aligned} Y_i = &\left[\beta_{1D}\text{Info}_i + \beta_{2D}\text{Appeal Aid}_i + \beta_{3D}\text{Insurance}_i\right] \times \mathbb{1}[d_i = D] \\ &+ \left[\beta_{1D'}\text{Info}_i + \beta_{2D'}\text{Appeal Aid}_i + \beta_{3D'}\text{Insurance}_i\right] \times \mathbb{1}[d_i = D'] + X\alpha + \varepsilon_i \end{aligned} \tag{4}$$

where Y_i, our outcome of interest, is an indicator that is 100 if the homeowner of demographics $d = \{D, D'\}$ filed an appeal of the assessed value of her home with the county, and 0 otherwise. We estimate two versions of this specifications where d is either race (minority and non-minority) or gender (female and male).

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

By randomization, the βs identify the causal effect of our treatments for homeowners by on their demographics. Since our treatments are nested, β_{2d} gives the effect of both receiving information and aid on how to appeal relative to homeowners of the same demographic in *Control*, and β_{3d} gives the effect of receiving information, aid on how to appeal, and the last-dollar insurance relative to *Control*. X is a vector of baseline characteristics.[25] Figure 8 presents the treatment effect estimates. We describe the treatment effects by race, shown in Panel (A), then by gender, shown in Panel (B).

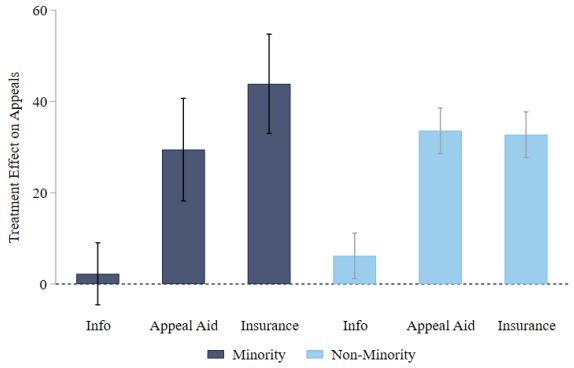
Mechanisms Driving Racial Discrimination Compared to homeowners in our *Control* condition, learning about the average financial benefits from appeal in the *Appeal Information* treatment widens the racial appeal gap to 5.1 percentage points or 38.2%. This information only leads non-minority homeowners to become statistically significantly more likely to appeal ($p = 0.016$), while the effect on minority homeowners is smaller and not different from 0 ($p = 0.522$).[26] Interpreting the effects using Equation 1, these effects of information may be consistent with minority homeowners having relatively large filing costs, c_H, and/or discrimination. We use our other two treatments to provide evidence on the relative importance of each for the racial gap.

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). 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. 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

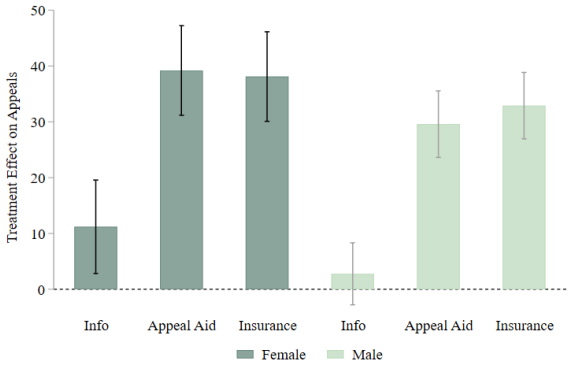
[25]The vector X contains indicators for whether the homeowner is a minority, has received at least a bachelor's degree, is female, has a child in public school, and is elderly (at least 65 years old), and is a Republican, as well as the log of her home's assessed value, and her prior beliefs about the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

[26]However, we cannot reject that *Appeal Information* had different effects on minority and non-minority homeowners.

Figure 8: Heterogeneous Treatment Effects on Appeal Filing



Panel A: Racial Differences



Panel B: Gender Differences

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 based on Equation 4. The sample includes all 1844 homeowners in the survey experiment. Panel (A) displays differences by race. Panel (B) displays differences by gender.

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 racial gap.

Finally, we find that providing *Insurance* only increase the appeal rate for minority homeowners. This financial incentive 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. 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 4.2. 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. However, when provided with an external financial benefit that pays

out exactly when they have a much worse than average outcome, since it is last-dollar, they chose to appeal. Moreover, non-minority homeowners, who do not perceive that they face discrimination, learn from the *Appeal Information* that the expected financial benefits are much larger than $75 and therefore are unlikely to further benefit from *Insurance*. We consider other potential implications and interpretations of this *Insurance* effect later in this section.

Mechanisms Driving Gender Discrimination Female homeowners in our *Appeal Information* treatment are about 11 percentage points, or about 233%, more likely to file an appeal than those in *Control*. However, we find that males do not respond; the effect is only 2.8 percentage points and not different from 0.[27] 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 ($p = 0.343$), although this increase is not enough to close the gap in intentions. Above *Appeal Information*, the step-by-step guide has nearly the same percentage point effect on the fraction of female and male homeowners who appeal. Finally, we find no added effect of providing insurance over appeal aid, consistent with homeowners believing that, given expected returns, their assessed values will fall by enough so that they will not be eligible for this financial incentive.

The pattern of treatment effects by race and gender suggests that the taxpayer-side mechanisms underlying these gaps are distinct. First, only the researcher-backed financial incentive meaningfully reduces the racial appeal gap, consistent with the presence of animus discrimination documented in the correspondence study. Second, providing credible information about the financial benefits of appealing closes the gender appeal gap, highlighting the role of information frictions that may themselves arise from the differential treatment we observe in the correspondence study. These results underscore that disparities are shaped by both taxpayer initiative and administrative discretion, and by the interaction between the two.

[27]The difference between women's and men's responses to *Appeal Information* is 8.4 pp ($p = 0.084$).

Alternatives Explanations for the Heterogeneous *Insurance* Effects Interpreting the effect of providing *Insurance* through Equations 1 and 3, our results suggest that racial minorities choose not to appeal because they perceive the discrimination we documented in Section 3. We now consider whether the heterogeneous insurance 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: assessed values, knowledge of the appeals process, education, and 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 4. We plot these estimated effects and the effects for minority and non-minority homeowners as well as male and female homeowners in Figure 9.[28]

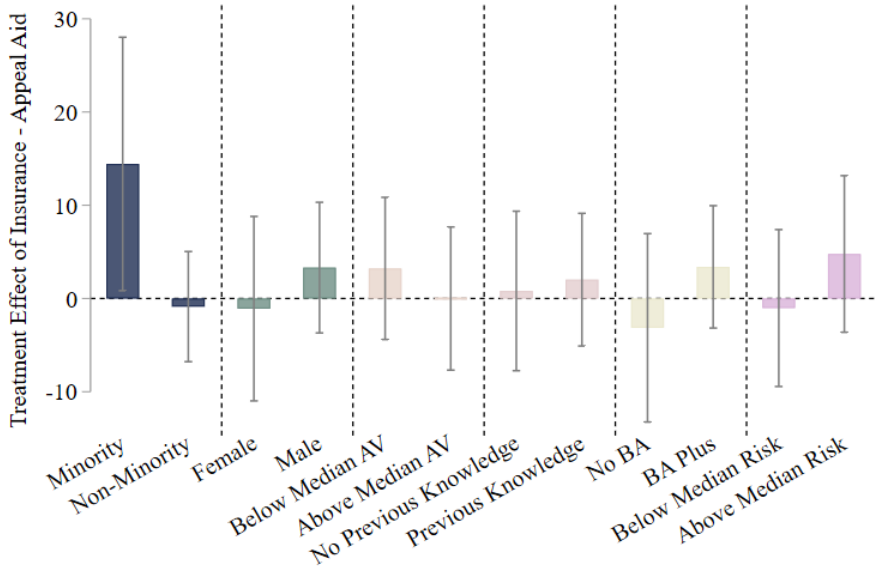
Overall, we find no evidence that the *Insurance* effect is not driven by other characteristics. While we may expect the financial incentives from this insurance to be stronger for homeowners with low assessed values, larger information frictions on how to appeal well, or who are more risk-averse, we only find considerable heterogeneity by race. Our results therefore suggest an important role for perceived discrimination. As shown in the first two bars of Figure 9, 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.[29] 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.

Beliefs about financial benefits from appealing As an additional test of the drivers of the appeal gaps, we consider how homeowners' beliefs about the financial benefits of appealing change with *Appeal Information* by race and gender. If, for example, minority homeowners perceive that they face discrimination, then they will not change their beliefs

[28]We present the full set of treatment effects by homeowner characteristic in Figure D3.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 D4.

[29]In Figure D4, we also show that the difference in the *Insurance* effect between minority and non-minority homeowners is statistically significant ($p = 0.043$).

Figure 9: 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 4, but adapted to the relevant subgroup. Different subgroups are separated by vertical dashed lines and colored differently. 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.

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, then that would suggest information frictions on how to appeal are the more relevant barrier.

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 D1.

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. We additionally find that female and male homeowners update similarly about the financial benefits, in contrast to our treatment effects that only female homeowners are induced to appeal from *Appeal Information*. The difference in these patterns, combined with the fact that female homeowners have higher perceived costs to appeal, potentially suggests that *Appeal Information* may additionally improve initiative for female homeowners because learning the many homeowners are successful conveys that the social costs are low, a barrier for women in many negotiating settings (e.g. Recalde and Vesterlund, 2023).

Appeal Intentions and Follow-Through Differences in intentions and filings shed further light on the potential mechanisms. We consider the effect of our treatments on four additional outcomes: {Affirmed Intent, Did Not Affirm Intent} × {Filed an Appeal, Did Not File an Appeal}. Homeowners who intent to appeal but are unable to follow through likely face significant information frictions in navigating the process, while homeowners who do not intent to appeal should believe that either the benefits are too low and/or the costs are too high of navigating the system.

For each of the four outcomes, we estimate Equation 4 and present the estimates in Table 2. Since the outcomes are mutually exclusive and collectively exhaustive, the coefficients sum to 0 within a row to illustrate how our treatments move homeowners across these behaviors.

Panel (A) of Table 2 displays the results by homeowner race. Our estimates that minority homeowners are much less likely to intent but not follow through after being provided with *Appeal Aid* indicate that they face significant barriers to navigating the process, including understanding how to provide evidence. While these barriers exist for some non-

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

	(1) Intent & Appeal	(2) Intent & No Appeal	(3) No Intent & Appeal	(4) No Intent & No Appeal
Panel A: Race				
Info × Minority	2.23	-4.55	-0.01	2.33
	(3.40)	(8.66)	(0.66)	(8.50)
Info × Non-Minority	5.56**	-0.31	0.60	-5.84
	(2.32)	(3.49)	(1.13)	(3.70)
Appeal Aid × Minority	21.28***	-28.04***	8.16***	-1.39
	(5.39)	(6.97)	(2.99)	(6.89)
Appeal Aid × Non-Minority	24.48***	-11.00***	9.09***	-22.57***
	(2.38)	(2.53)	(1.51)	(2.87)
Insurance × Minority	41.22***	-28.78***	2.64	-15.08**
	(5.56)	(6.71)	(1.65)	(6.35)
Insurance × Non-Minority	30.51***	-9.37***	2.19**	-23.33***
	(2.50)	(2.55)	(0.94)	(2.86)
Panel B: Gender				
Info × Female	8.73**	-2.60	2.46	-8.60
	(3.75)	(6.49)	(2.05)	(6.66)
Info × Male	3.14	2.76	-0.38	-5.52
	(2.66)	(4.02)	(1.10)	(4.28)
Appeal Aid × Female	29.37***	-17.64***	9.83***	-21.55***
	(3.97)	(4.87)	(2.31)	(4.87)
Appeal Aid × Male	20.53***	-11.30***	9.04***	-18.28***
	(2.80)	(2.85)	(1.80)	(3.41)
Insurance × Female	36.60***	-21.09***	1.51*	-17.01***
	(4.10)	(4.60)	(0.88)	(4.79)
Insurance × Male	29.90***	-7.70***	2.99**	-25.19***
	(2.96)	(2.96)	(1.21)	(3.33)

Note: This table presents treatment effect estimates by race and gender on four outcomes corresponding to {Affirmed Intent, Did Not Affirm Intent} × {Filed an Appeal, Did Not File an Appeal}. Each column and panel corresponds to a different regression. For example, Column (1) presents our treatment effects from Equation 4 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

minority homeowners as well, they are smaller in magnitude. Compared to only providing *Appeal Aid*, *Insurance* mainly works for minority homeowners by moving them from neither intending nor appealing to both affirming their intent and filing an appeal ($p = 0.032$). We do not find an analogous effect for non-minority homeowners who do not face discrimination and therefore have a smaller financial benefit from *Insurance*. Having greatly reduced the information frictions in appealing with *Appeal Aid*, these patterns provide evidence that *Insurance* only increases the expected benefits for minority homeowners, consistent with perceived discrimination.

Panel (B) of Table 2 displays the results by the homeowners gender. The *Appeal Information* treatment increases the share of female homeowners who both intend to appeal and follow through, primarily by moving them out of the group who neither intend nor appeal, rather than by converting those who intend but fail to act. This pattern suggests that providing information may shift perceived benefits or reduce perceived social costs by normalizing appeals through examples of successful cases. As with our treatment effects on filing, we see similar patterns here by gender for *Appeal Aid* and *Insurance*.

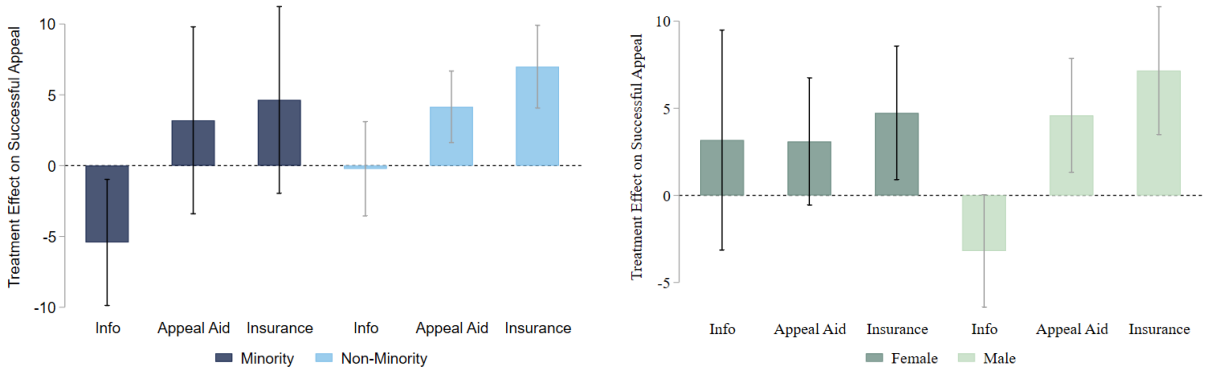
4.5 Disparities in Appeal Outcomes

Our previous results indicate that our treatments greatly increased appeal rates for disadvantaged groups who are more likely to be overassessed and that disadvantaged homeowners use similar negotiation strategies and tactics as more advantaged homeowners. Absent discrimination, we therefore expect to see the reduction in appeal gaps in the experiment translate into reduced assessment gaps.

To consider this, we estimate Equation 4, on whether the homeowner had an appeal that led to a reduction in her assessed value.[30] We consider intention-to-treat effects so that homeowners who do not appeal, mechanically, do not have a successful appeal. We present the results by race in Panel (a) of Figure 10 and by gender in Panel (b).

[30]We measure this outcome in August 2024. For participants in the 2022 wave of the experiment, about 77 percent are completed, while only 13 percent are completed for the 2023 wave.

Figure 10: Racial and Gender Disparities in Property Tax Appeal Outcomes



(a) Appeal Success by Race and Treatment (b) Appeal Success by Gender and Treatment

Note: This figure presents our treatment effects on the likelihood a homeowner successfully reduces his or her assessed value through an appeal, as well as the 95 percent confidence intervals based on Equation 4. The sample includes all 1844 homeowners in the survey experiment. Panel (a) displays differences by race. Panel (b) displays differences by gender.

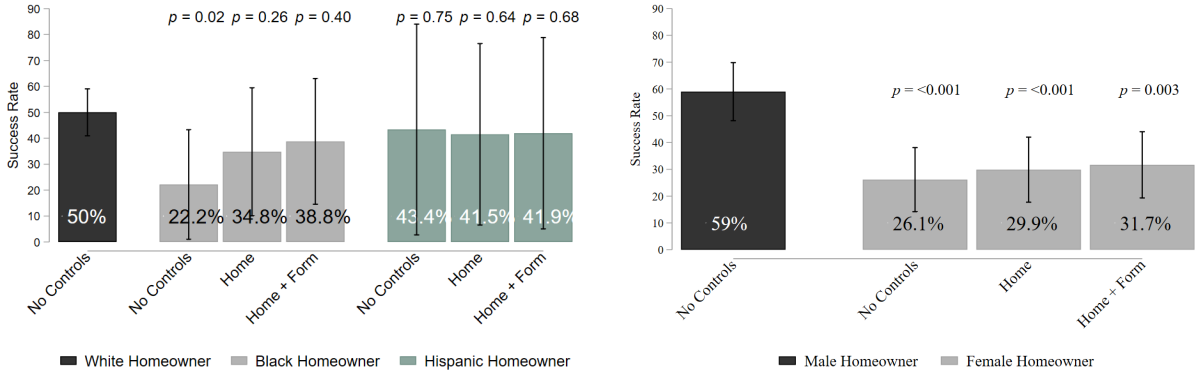
We find that our experiment led to similar appeal outcomes by race and gender across our treatments. The point estimates for the treatment effects on non-minority homeowners are larger but not statistically different from each other[31]. Panel (b) of Figure 10 shows similar results for the gender gap in appeal success. While our three treatments overall induce more female homeowners to file by 7.53 pp ($p = 0.045$), we find very similar ITT estimates by gender in appeal success, with larger point estimates for male than female homeowners in the *Appeal Aid* and *Insurance* treatments.

Our results imply that there is discrimination in the appeals process. Since the *Insurance* treatment led 14 pp more minority homeowners to file an appeal, and all three treatments led more female homeowners to file an appeal (joint effect of 7.53 pp, $p = 0.045$), absent differences in appeal quality and discrimination, we would also expect these homeowners to also be more likely to have a successful appeal. The similar results, and even larger but noisy point estimates in favor of more advantaged groups, provides evidence of discrimination.

[31]Perhaps except for the *Information* treatment with $p = 0.051$.

Figure 11 provides some evidence on the mechanisms determining differences in appeal outcomes. For this analysis we focus on homeowners in the *Appeal Aid* and *Insurance* treatments with finalized appeals where we can both observe the information content on their forms and their outcomes. The first presents a decomposition of how the race and gender gaps as a function of both home characteristics and negotiation tactics. If minority and female homeowners have worse appeal outcomes because their homes are more difficult to assess, then controlling for these features should reduce the gap. However, if disadvantaged homeowners have worse outcomes because of assessor discrimination, then these controls should not affect the outcome gaps.

Figure 11: Drivers of Differences in Appeal Outcomes



(a) Correlates of Racial Disparities

(b) Correlates of Gender Disparities

Note: The figure presents the likelihood that homeowners with finalized appeals successfully reduce their assessed value. In Panel (a), the far left black bar gives the unconditional success rate for white homeowners. The leftmost gray and green bars give the unconditional success rate for Black and Hispanic homeowners, respectively while the other bars show how the relative success gap (compared to white homeowners) changes with additional controls. Panel (b) plots analogous results comparing male and female homeowners. The sample include the 148 homeowners in the *Appeal Aid* and *Insurance* conditions where we can observe what is included on the form with finalized appeals.

In Panel (a) of Figure 11, we first present as the far left black bar that 50% of white homeowners with finalized appeals have a reduction in their tax burden. This provides evidence that our information and appeal aid worked to help reduce tax burdens. However, consistent with our treatment effects on appeals and appeal outcomes, we find that Black

homeowners are significantly less likely to have a successful appeal. These appeal outcome gaps can be meaningfully, but not fully, explained by differences in home characteristics (including home characteristics reduce Black-white gap by 12.6pp, $p = 0.066$), and the Black-white gap is no longer statistically different from 0. Additionally including content from the appeal form has minimal added effect since minority and non-minority homeowners pick similar comparables given available evidence. These results suggest a role for both systemic issues and assessor discrimination.

In Panel (b), we present similar evidence on the gender gap in appeal outcomes. 59% of male homeowners have a successful appeal, compared to only 26% of female homeowners. Removing potential differences in home characteristics and form content reduces the gender gap by about 5pp (17%), but this relatively small effect is not statistically different from 0, and therefore suggest assessor treatment during the negotiations after the form is submitted plays a relatively large role in creating gaps.

5. Conclusion

This paper investigates how taxpayer initiative and administrative discretion jointly shape disparities in tax negotiations. Using two complementary field experiments in the property tax system, we document consistent gaps by race and gender on both sides of the market. Female and minority homeowners are less likely to appeal overassessments, and when they do, they receive less assistance and less favorable outcomes. Mechanism tests indicate that gender gaps are driven primarily by statistical discrimination, while racial gaps are driven by animus discrimination. Taken together, these results show that a process intended to correct errors can instead exacerbate inequities when participation barriers and biased discretion align.

Leaving these sources of inequality in tax burdens unaddressed has consequences that extend beyond the property tax system. Higher property tax burdens place already disadvantaged homeowners at greater risk of financial distress (Anderson and Dokko, 2008;

Atuahene, 2018; Hayashi, 2020; Wong, 2020) and, in aggregate, contribute to widening racial and gender wealth gaps (Perry et al., 2018; Menendian et al., 2021; Dray et al., 2023). Our results highlight broader concerns in taxation and other settings in which negotiations can increase inequality. Disadvantaged individuals may face lower expected returns to entering into a negotiation and may also be treated less favorably in the process. Both selection into negotiation and treatment during the negotiation can widen disparities, as we find here. While improved information may increase participation and facilitate better outcomes in some cases, it is not sufficient to ensure equity when administrative discretion and systemic bias persist.

Several limitations should be noted. While the correspondence study covers a diverse set of counties, the homeowner experiment is conducted on a selected sample in a single county; results may differ in jurisdictions with different property tax structures, demographics, or administrative procedures. Our mechanism identification—animus versus statistical discrimination—relies on evidence from treatment interactions and assessor characteristics, but cannot fully rule out alternative explanations. In addition, some estimates are imprecise, particularly for subgroup analyses.

The findings suggest that reforms addressing both the initiation of, and responses within, the property tax appeals process could reduce observed disparities. On the participation side, procedural complexity and informational barriers are associated with lower appeal rates among certain demographic groups. Measures such as clearer public guidance, pre-populated forms, and accessible step-by-step instructions could reduce these barriers and facilitate broader participation. Within the process, variation in assessor responses across homeowner characteristics indicates that more standardized response protocols may help ensure greater consistency in administrative treatment. In addition, for properties with a high estimated likelihood of overassessment—particularly in areas with a history of elevated assessment ratios—administrative agencies could consider proactive review mechanisms that do not depend on homeowner-initiated appeals. While these changes would require addi-

tional administrative resources, and likely oversight given how discretion affects disparities, they have the potential to improve both the accuracy and the perceived impartiality of the property tax system.

References

Agan, Amanda and Sonja Starr, "Ban the Box, Criminal Records, and Racial Discrimination: A Field Experiment," *The Quarterly Journal of Economics*, 2018, *133* (1), 191–235.

Akesson, Jesper, Robert W Hahn, Robert D Metcalfe, and Itzhak Rasooly, "Race and Redistribution in the United States: An Experimental Analysis," Technical Report, National Bureau of Economic Research 2022.

Alesina, Alberto, Matteo F Ferroni, and Stefanie Stantcheva, "Perceptions of racial gaps, their causes, and ways to reduce them," Technical Report, National Bureau of Economic Research 2021.

Andersen, Steffen, Julie Marx, Kasper Meisner Nielsen, and Lise Vesterlund, "Gender differences in negotiation: Evidence from real estate transactions," *The Economic Journal*, 2021, *131* (638), 2304–2332.

Anderson, Nathan B and Jane K Dokko, "Mortgage Delinquency and Property Taxes," in "Proceedings. Annual Conference on Taxation and Minutes of the Annual Meeting of the National Tax Association," Vol. 101 National Tax Association 2008, pp. 176–185.

Asiedu, Edward, Dean Karlan, Monica Lambon-Quayefio, and Christopher Udry, "A call for structured ethics appendices in social science papers," *Proceedings of the National Academy of Sciences*, 2021, *118* (29), e2024570118.

Atuahene, Bernadette, "'Our Taxes Are Too Damn High': Institutional Racism, Property Tax Assessment, and the Fair Housing Act," *Northwestern University Law Review*, 2018, *112* (6).

Avenancio-León, Carlos F and Troup Howard, "The assessment gap: Racial inequalities in property taxation," *The Quarterly Journal of Economics*, 2022, *137* (3), 1383–1434.

Ayres, Ian and Peter Siegelman, "Race and gender discrimination in bargaining for a new car," *The American Economic Review*, 1995, pp. 304–321.

Babcock, Linda and Sara Laschever, "Women don't ask: Negotiation and the gender divide," 2021.

Bérgolo, Marcelo L, Rodrigo Ceni, Guillermo Cruces, Matias Giaccobasso, and Ricardo Perez-Truglia, "Tax audits as scarecrows: Evidence from a large-scale field experiment," Technical Report, National Bureau of Economic Research 2017.

Berry, Christopher R, "Reassessing the Property Tax," *Available at SSRN 3800536*, 2021.

Bertrand, Marianne and Esther Duflo, "Field experiments on discrimination," *Handbook of economic field experiments*, 2017, *1*, 309–393.

Bhargava, Saurabh and Dayanand Manoli, "Psychological frictions and the incomplete take-up of social benefits: Evidence from an IRS field experiment," *American Economic Review*, 2015, *105* (11), 3489–3529.

Bohren, J Aislinn, Kareem Haggag, Alex Imas, and Devin G Pope, "Inaccurate statistical discrimination: An identification problem," Technical Report, National Bureau of Economic Research 2019.

_ , Peter Hull, and Alex Imas, "Systemic discrimination: Theory and measurement," *The Quarterly Journal of Economics*, 2025, *140* (3), 1743–1799.

Bottan, Nicolas and Ricardo Perez-Truglia, "Betting on the house: Subjective expectations and market choices," *American Economic Journal: Applied Economics*, 2025, *17*

(1), 459–500.

Bowles, Hannah Riley, Linda Babcock, and Lei Lai, "Social incentives for gender differences in the propensity to initiate negotiations: Sometimes it does hurt to ask," *Organizational Behavior and human decision Processes*, 2007, *103* (1), 84–103.

Brandon, Alec, Justin Holz, Andrew Simon, and Haruka Uchida, "Minimum Wages and Racial Differences in Hiring: Evidence from a Field Experiment," 2023.

Broder, Ivy E, "Review of NSF economics proposals: Gender and institutional patterns," *The American Economic Review*, 1993, *83* (4), 964–970.

Brown, Dorothy A, "Homeownership in Black and White: The Role of Tax Policy in Increasing Housing Inequity," *U. Mem. L. Rev.*, 2018, *49*, 205.

Buzard, Kristy, Laura K Gee, and Olga Stoddard, "Who you gonna call? gender inequality in external demands for parental involvement," *The Quarterly Journal of Economics*, 2025, p. qjaf027.

Cabral, Marika and Caroline Hoxby, "The hated property tax: salience, tax rates, and tax revolts," Technical Report, National Bureau of Economic Research 2012.

Castillo, Marco, Ragan Petrie, Maximo Torero, and Lise Vesterlund, "Gender differences in bargaining outcomes: A field experiment on discrimination," *Journal of Public Economics*, 2013, *99*, 35–48.

Colmer, Jonathan Mark, Mary F Evans, and Jay Shimshack, "Environmental citizen complaints," 2023.

Cullen, Zoë, Will Dobbie, and Mitchell Hoffman, "Increasing the demand for workers with a criminal record," *The Quarterly Journal of Economics*, 2023, *138* (1), 103–150.

Czibor, Eszter, David Jimenez-Gomez, and John A List, "The dozen things experimental economists should do (more of)," *Southern Economic Journal*, 2019, *86* (2), 371–432.

Doerner, William and Keith Ihlanfeldt, "An empirical analysis of the property tax appeals process," 2014.

Dray, Sacha, Camille Landais, and Stefanie Stantcheva, "Wealth and property taxation in the United States," Technical Report, National Bureau of Economic Research 2023.

Egan, Mark, Gregor Matvos, and Amit Seru, "When Harry fired Sally: The double standard in punishing misconduct," *Journal of Political Economy*, 2022, *130* (5), 1184–1248.

Elzayn, Hadi, Evelyn Smith, Thomas Hertz, Cameron Guage, Arun Ramesh, Robin Fisher, Daniel E Ho, and Jacob Goldin, "Measuring and mitigating racial disparities in tax audits," *The Quarterly Journal of Economics*, 2025, *140* (1), 113–163.

Exley, Christine L, Muriel Niederle, and Lise Vesterlund, "Knowing when to ask: The cost of leaning in," *Journal of Political Economy*, 2020, *128* (3), 816–854.

Gaddis, S Michael, Edvard Larsen, Charles Crabtree, and John Holbein, "Discrimination against Black and Hispanic Americans is highest in hiring and housing contexts: A meta-analysis of correspondence audits," *Available at SSRN 3975770*, 2021.

Giaccobasso, Matias, Brad Nathan, Ricardo Perez-Truglia, and Alejandro Zentner, "Where Do My Tax Dollars Go? Tax Morale Effects of Perceived Government Spending," *National Bureau of Economic Research*, 2022.

Goldsmith-Pinkham, Paul and Kelly Shue, "The gender gap in housing returns," *The*

Journal of Finance, 2023, *78* (2), 1097–1145.

Guryan, Jonathan and Kerwin Kofi Charles, "Taste-based or statistical discrimination: the economics of discrimination returns to its roots," *The Economic Journal*, 2013, *123* (572), F417–F432.

Haaland, Ingar and Christopher Roth, "Beliefs about racial discrimination and support for pro-black policies," *Review of Economics and Statistics*, 2022, pp. 1–15.

_ , _ , and Johannes Wohlfart, "Designing information provision experiments," *Journal of economic literature*, 2023, *61* (1), 3–40.

Hayashi, Andrew T, "The legal salience of taxation," *U. Chi. L. Rev.*, 2014, *81*, 1443.

_ , "Countercyclical Property Taxes," *Va. Tax Rev.*, 2020, *40*, 1.

Herd, Pamela and Donald P Moynihan, *Administrative burden: Policymaking by other means*, Russell Sage Foundation, 2019.

Hurst, Reuben, Andrew Simon, and Michael Ricks, "Public good perceptions and polarization: Evidence from higher education appropriations," 2024.

Ihlanfeldt, Keith and Luke P Rodgers, "Explaining Racial Gaps in Property Assessment and Property Taxation," 2021.

Jensen, Andres and Jonathan Weigel, "No taxation without the state: bringing tax administration back into the study of tax capacity," 2024.

Jowell, Roger and Patricia Prescott-Clarke, "Racial discrimination and white-collar workers in Britain," *Race*, 1970, *11* (4), 397–417.

Kline, Patrick, Evan K Rose, and Christopher R Walters, "Systemic discrimination among large US employers," *The Quarterly Journal of Economics*, 2022, *137* (4), 1963–2036.

Leibbrandt, Andreas and John A List, "Do women avoid salary negotiations? Evidence from a large-scale natural field experiment," *Management Science*, 2015, *61* (9), 2016–2024.

Levitt, Steven D and John A List, "What do laboratory experiments measuring social preferences reveal about the real world?," *Journal of Economic perspectives*, 2007, *21* (2), 153–174.

List, John A, "The nature and extent of discrimination in the marketplace: Evidence from the field," *The Quarterly Journal of Economics*, 2004, *119* (1), 49–89.

_ , "The IRB is key in field experiments," *Science*, 2009, *323* (5915), 713–714.

Menendian, S., A. Gailes, and S. Gambhir, "Twenty-first century racial residential segregation in the United States," *The Roots of Structural Racism Project*, 2021.

Mullen, Lincoln, *gender: Predict Gender from Names Using Historical Data* 2021. R package version 0.6.0.

Nathan, Brad, Ricardo Perez-Truglia, and Alejandro Zentner, "My taxes are too darn high: why do households protest their taxes?," *American Economic Journal: Economic Policy*, 2025, *17* (1), 273–310.

Perez-Truglia, Ricardo and Ugo Troiano, "Shaming tax delinquents," *Journal of Public Economics*, 2018, *167*, 120–137.

Perry, Andre, Jonathan Rothwell, and David Harshbarger, "The devaluation of assets in black neighborhoods," *Library Catalog: www. brookings. edu*, 2018.

Quillian, Lincoln, John J Lee, and Mariana Oliver, "Evidence from field experiments in hiring shows substantial additional racial discrimination after the callback," *Social Forces*, 2020, *99* (2), 732–759.

Ray, Victor, Pamela Herd, and Donald Moynihan, "Racialized burdens: Applying racialized organization theory to the administrative state," *Journal of Public Administration Research and Theory*, 2023, *33* (1), 139–152.

Recalde, Maria P and Lise Vesterlund, "Gender Differences in Negotiation: Can Interventions Reduce the Gap?," *Annual Review of Economics*, 2023, *15*.

Ross, Robert, "The impact of property tax appeals on vertical equity in Cook County, IL," *Univerity of Chicago, Harris School of Public Policy Working Paper*, 2017.

Roussille, Nina, "The role of the ask gap in gender pay inequality," *The Quarterly Journal of Economics*, 2024, *139* (3), 1557–1610.

Small, Mario L and Devah Pager, "Sociological perspectives on racial discrimination," *Journal of Economic Perspectives*, 2020, *34* (2), 49–67.

Tax Foundation, "Property Taxes by State and County, 2022," https://taxfoundation.org/data/all/state/property-taxes-by-state-county/ 2025. Accessed June 9, 2025; based on fiscal year 2022 county-level property-tax collections in the U.S.

_ , "Sources of U.S. Tax Revenue by Tax Type," https://taxfoundation.org/data/all/federal/us-tax-revenue-by-tax-type/ 2025. Accessed June 9, 2025; data refer to 2023 tax revenue shares.

Tzioumis, Konstantinos, "Demographic aspects of first names," *Scientific data*, 2018, *5* (1), 1–9.

Weber, Rachel N and Daniel P McMillen, "Ask and ye shall receive? Predicting the successful appeal of property tax assessments," *Public Finance Review*, 2010, *38* (1), 74–101.

Wong, Francis, "Mad as hell: Property taxes and financial distress," *Available at SSRN 3645481*, 2020.

Online Appendix

A Additional Details of the Correspondence Experiment **49**
A.1 Email Content and Creation 49
A.2 County Sampling 54
A.3 email Procedures 55
A.4 Measuring Outcomes 56
A.5 Do Assessors Learn the Emails Are Fake Over Time? 56

B Correspondence Experiment Appendix Results **61**

C Homeowner Experiment Appendix **62**
C.1 Overview 62
C.2 Treatments 62

D Homeowner Experiment Appendix Results **65**

E Ethics Appendix **71**

A. Additional Details of the Correspondence Experiment

A.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 A1 below.

Table A1: 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 A2. 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 A2: 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).

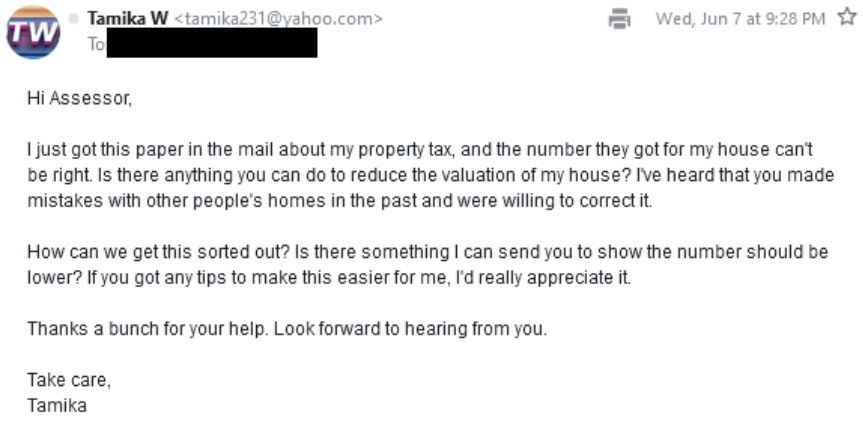
email Addresses: We assigned identities to email accounts of popular email providers. Each email addressed contained the first name of the fictitious homeowner, along with a series of arbitrary numbers. The email address also served as the only method through which assessors could contact the fictitious 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.

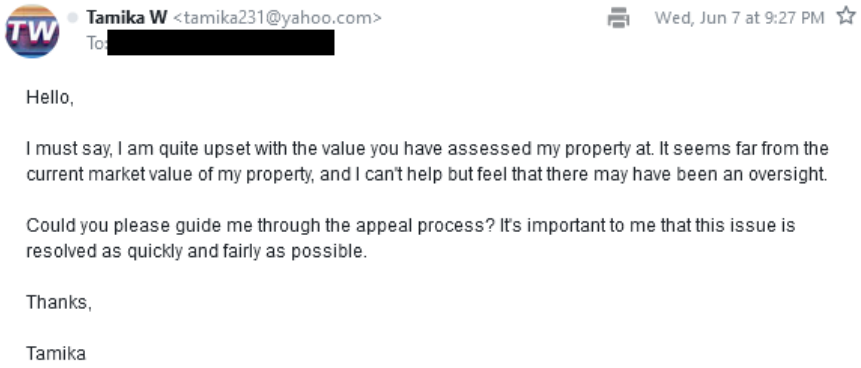
The primary content of the emails appears in the six examples below (See Figures A1, A2 , A3, A4, A5, and A6). Additionally, we randomly chose whether the email contained an additional sentence stating that the assessed value of the homeowner's property was too high. These messages either told the assessor 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 A1: Correspondence Study email Version 1



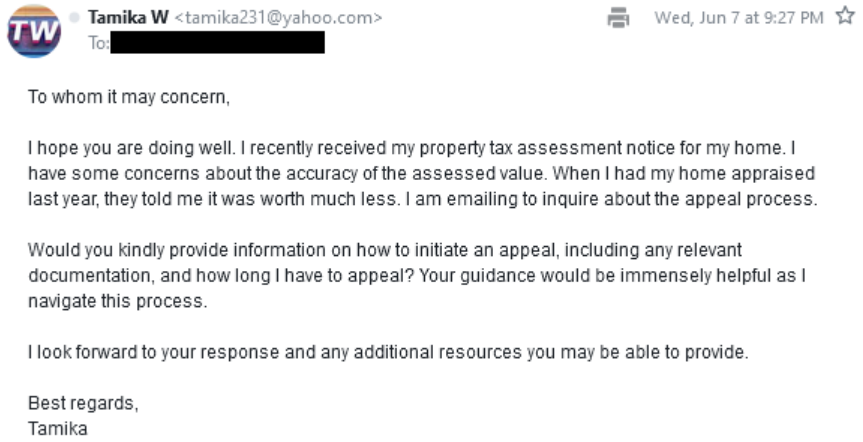
Note: This figure displays one version of the email sent to the assessors. The subject of the email was "Mistake on property tax assessment?" We randomized whether this email contained a sentence describing evidence that the assessment was too high.

Figure A2: Correspondence Study email Version 2



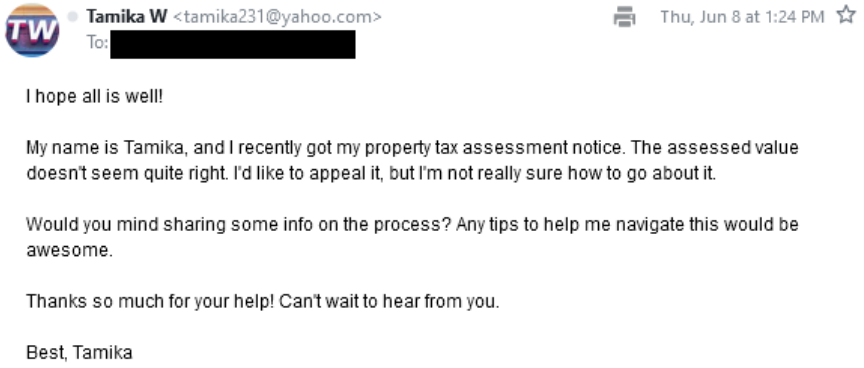
Note: This figure displays one version of the email sent to the assessors. The subject of the email was "Disappointed with property tax assessment." We randomized whether this email contained a sentence describing evidence that the assessment was too high.

Figure A3: Correspondence Study email Version 3



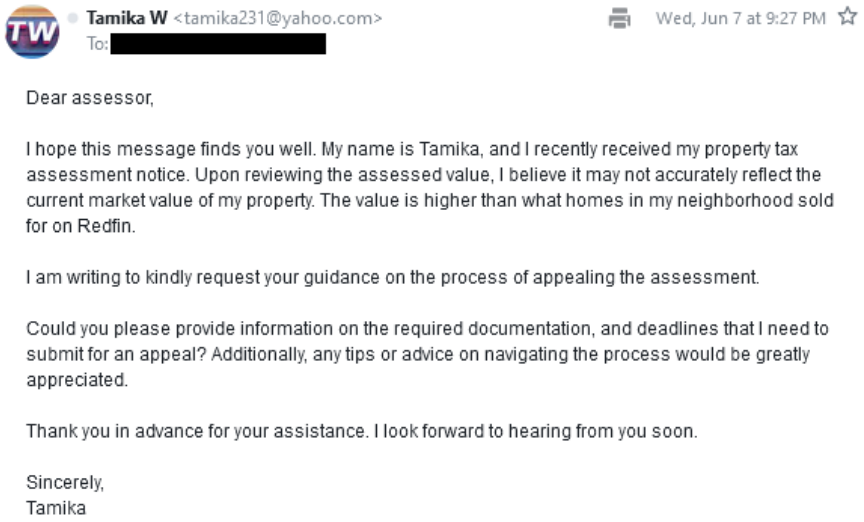
Note: This figure displays one version of the email sent to the assessors. The subject of the email was "Inquiring About Property Tax Appeal Procedures." We randomized whether this email contained a sentence describing evidence that the assessment was too high.

Figure A4: Correspondence Study email Version 4



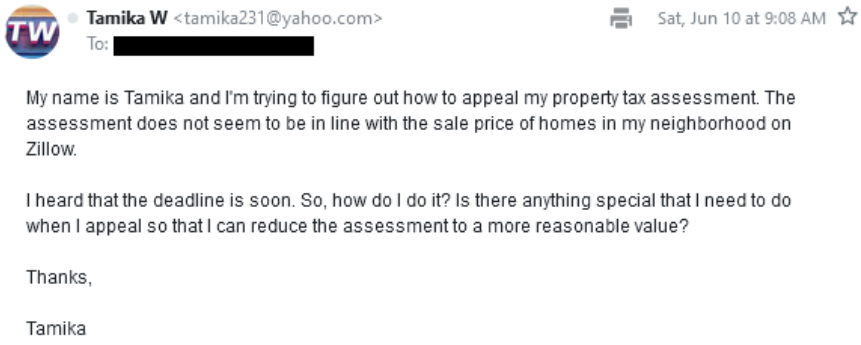
Note: This figure displays one version of the email sent to the assessors. The subject of the email was "Quick Question on Property Tax Appeal." We randomized whether this email contained a sentence describing evidence that the assessment was too high.

Figure A5: 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 A6: 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.

A.2 County Sampling

Our sample consists of the assessors in Indiana, Nebraska, Missouri, Idaho, and Pennsylvania.[32] 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 A3 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 A3: 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.

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

A.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 A.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 A4. 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 A4: 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.

A.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.

A.5 Do Assessors Learn the Emails Are Fake Over Time?

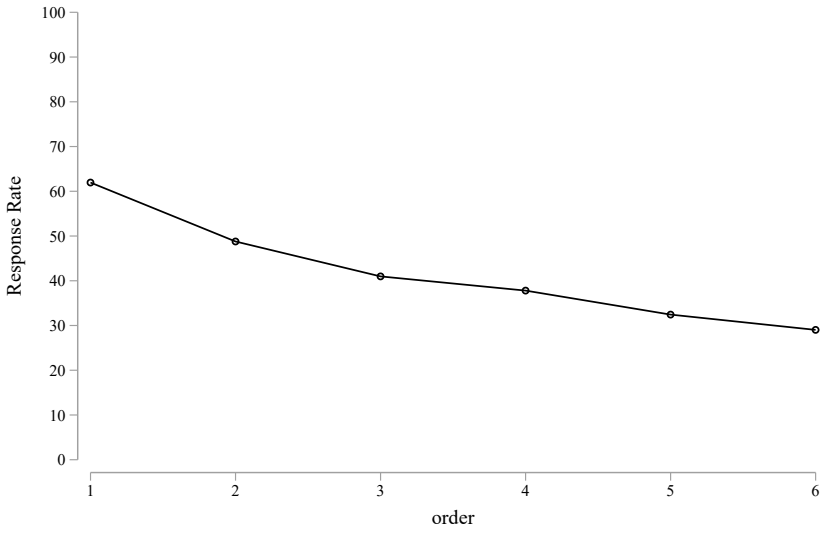
One potential concern with our analysis is that our results are driven by assessors learning over time that the emails come from fictitious homeowners. Under this interpretation, response rates would be lower for women and minorities because the assessors are more likely to learn that these individuals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

Indeed, Figure A7 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.

In Table A5, 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

Figure A7: 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.

the fictitious homeowner's identity and the email order. As in Figure A7, 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 A6 and A7 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 A5: 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 A6: 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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

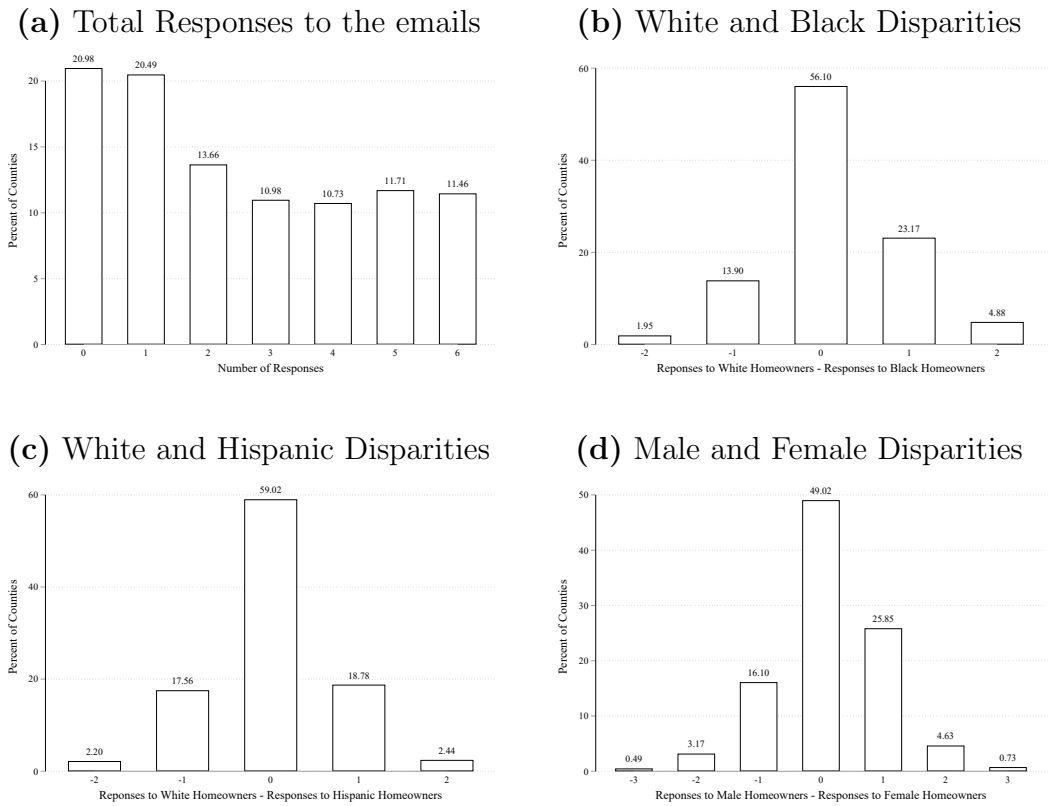
Table A7: Treatment Balance: Second through Sixth email in Correspondence Study

	(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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

B. Correspondence Experiment Appendix Results

Figure B1: Distribution of Within-County Response Rates by Homeowner Type

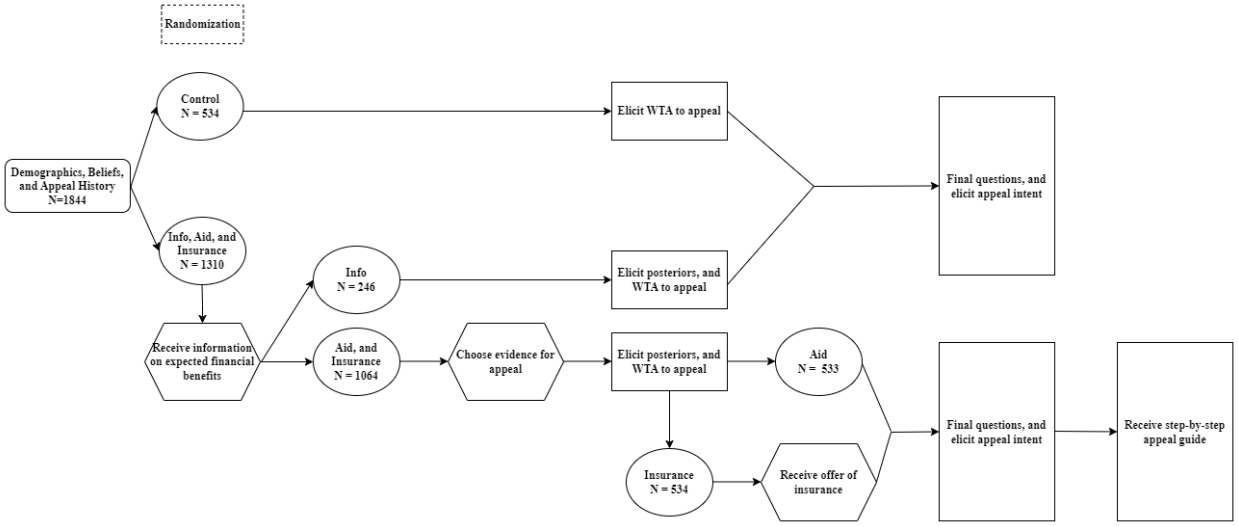


Note: Panel (a) displays the frequency of the number of responses from each assessor. Each assessor could respond up to six times. Panel (b) displays the difference in the number of responses to fictitious white and Black homeowners. Panel (c) displays the difference in the number of responses to fictitious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

C. Homeowner Experiment Appendix

C.1 Overview

Figure C1: Survey Experiment Flowchart



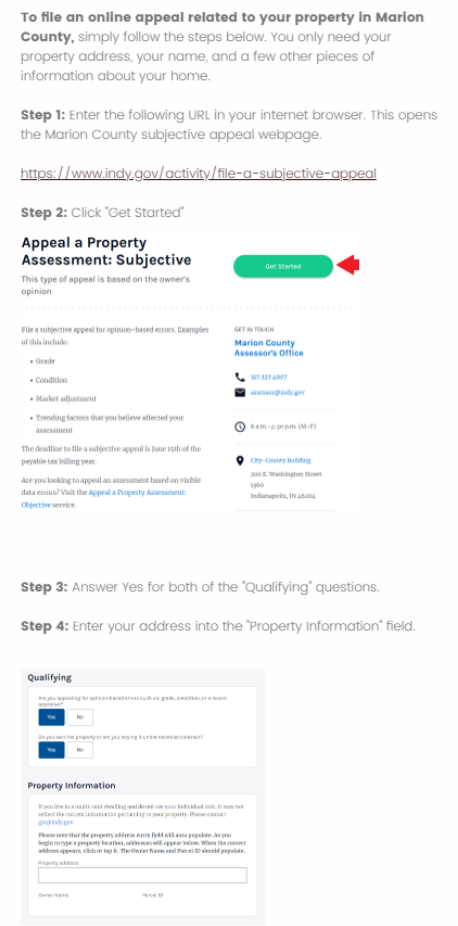
C.2 Treatments

Figure C2: *Information* Treatment

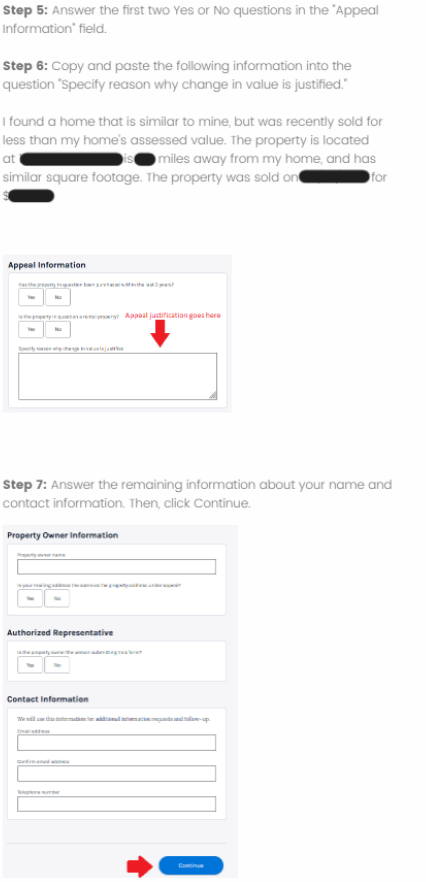
The following table gives some information about what happened when homeowners in Marion County appealed their property's assessed value **last year (2022)**. Please take a moment to review this information.

	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

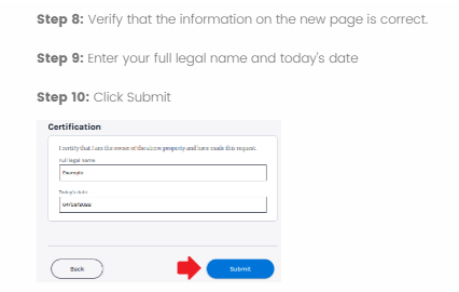
Figure C3: *Appeal Aid* Treatment (File Online Option)



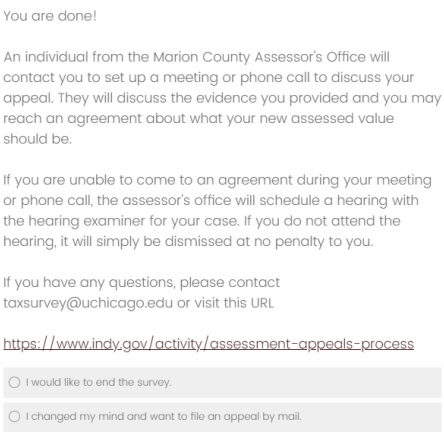
(a)



(b)

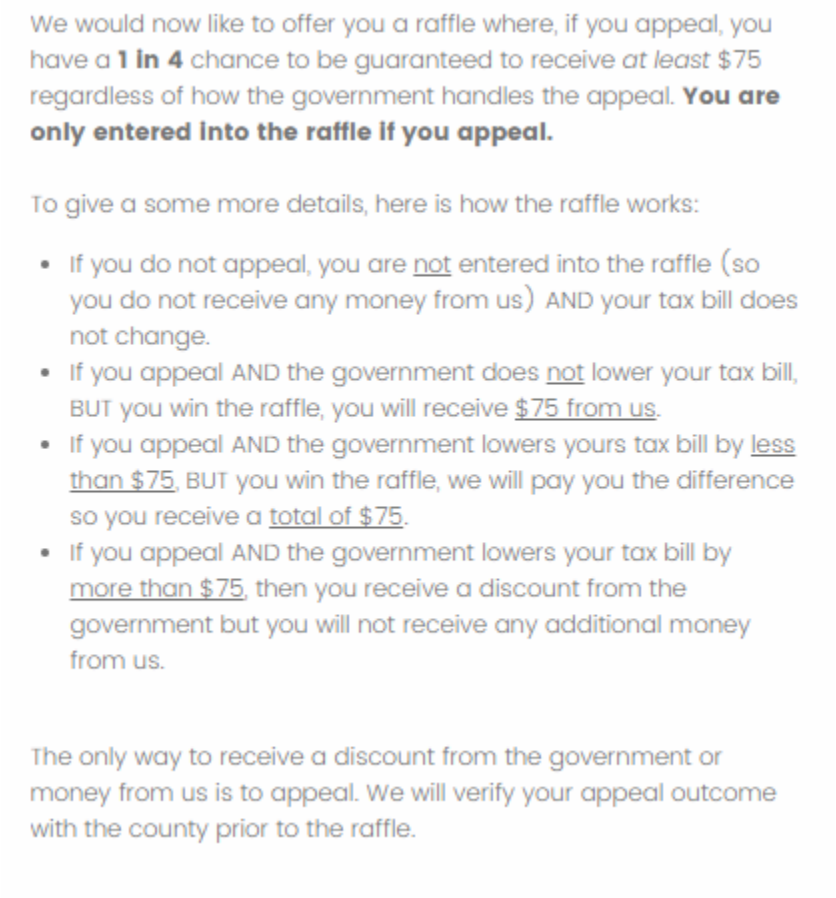


(c)



(d)

Figure C4: *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.

D. Homeowner Experiment Appendix Results

Table D1: 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.

Table D2: 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	

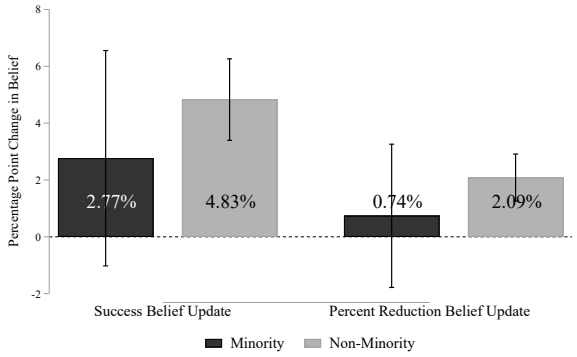
Note: The table presents summary statistics for homeowners in our survey experiment. Column (1) presents the overall average, Column (2) the average among minority homeowners, and Column (3) the average among non-minority homeowners. Column (4) presents the p-value for a test of equivalence between minority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

Table D3: Racial Differences in Appeal Experiences for *Control* Homeowners

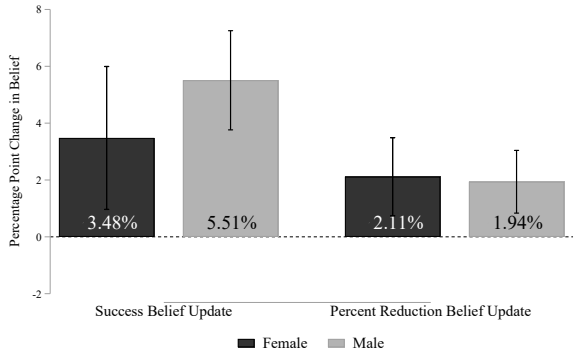
	Group			
	All	Black and Hispanic	White and Asian	P-val
	(1)	(2)	(3)	(4)
Affirmed Appeal Intent	35.77	56.10	32.08	0.00
	(2.08)	(5.51)	(2.20)	
Appealed	7.12	6.10	7.30	0.68
	(1.11)	(2.66)	(1.23)	
Perceived Cost to Appeal	23.75	22.51	23.98	0.62
(% of Tax Burden)	(1.24)	(2.66)	(1.39)	
Strongly or Somewhat 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.

Figure D1: Differences in Knowledge of Appeals and Pre-Survey Behavior



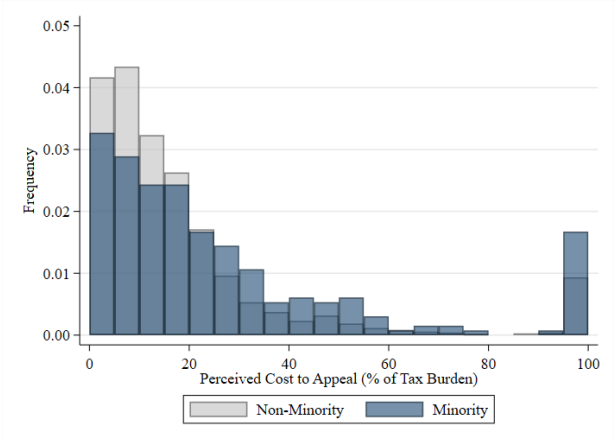
Panel A: Racial Differences



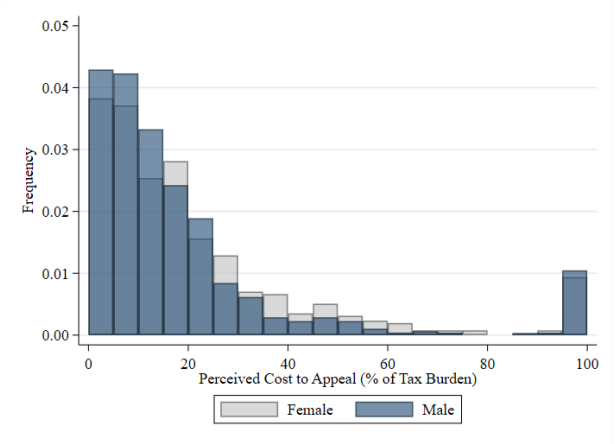
Panel B: Gender Differences

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

Figure D2: Perceived Cost to Appeal



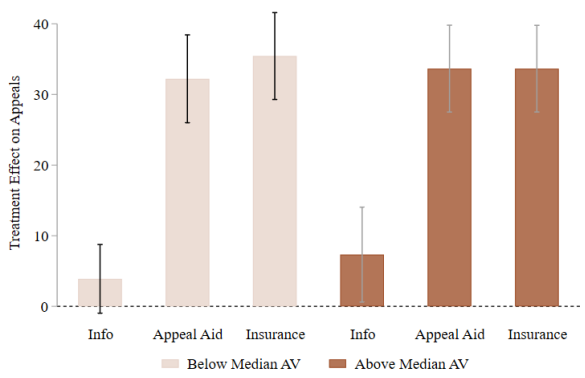
(a) Racial Differences



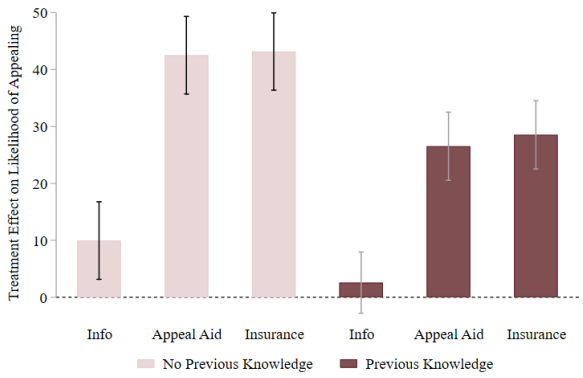
(b) Gender Differences

Note: Each panel presents a histogram of homeowner's perceived cost to appeal as a percent of their total tax burden, truncated at 100% of the taxes owed. Panel (a) shows the distribution by race, and (b) by gender.

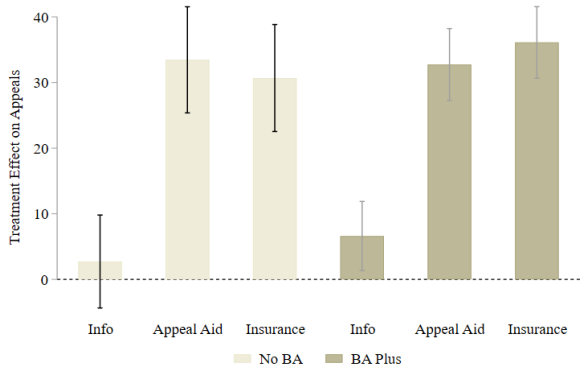
Figure D3: Ruling Out Other Explanations for the *Insurance* Effect



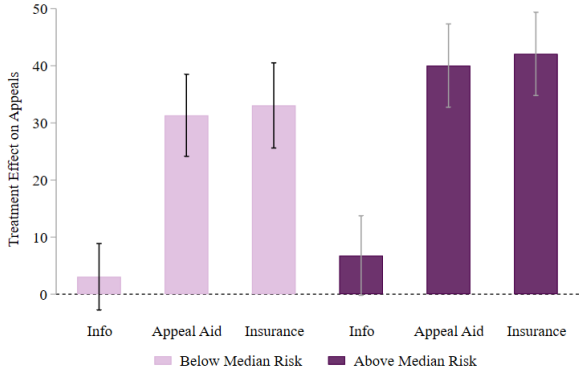
(a) Home Assessed Value



(b) Previous Knowledge of Appeals



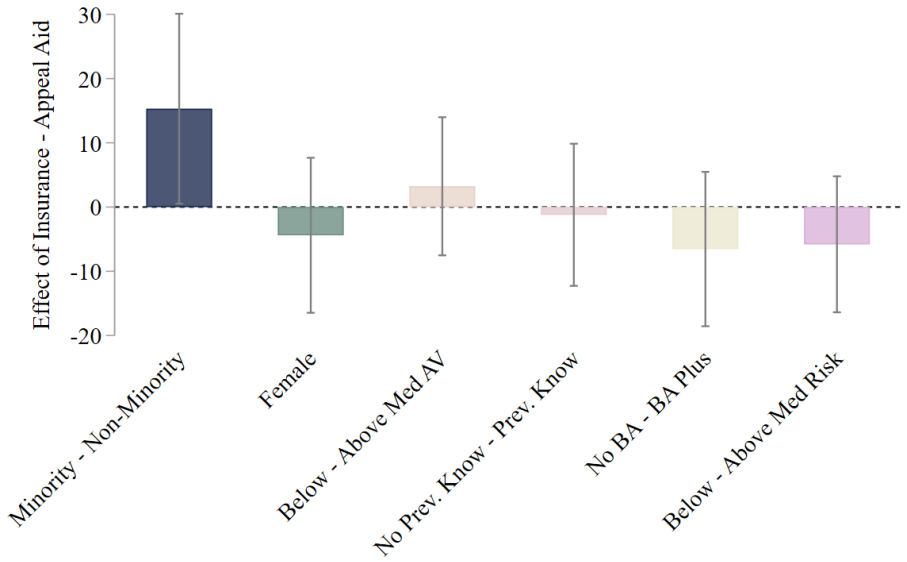
(c) Education



(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 4, 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.

Figure D4: 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 4, 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.

E. 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.