STATISTICAL DISCRIMINATION OR PREJUDICE? A LARGE SAMPLE FIELD EXPERIMENT

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Abstract—A model of racial discrimination provides testable implications for two features of statistical discriminators: differential treatment of signals by race and heterogeneous experience that shapes perception. We construct an experiment in the U.S. rental apartment market that distinguishes statistical discrimination from taste-based discrimination. Responses from over 14,000 rental inquiries with varying applicant quality show that landlords treat identical information from applicants with African American– and white-sounding names differently. This differential treatment varies by neighborhood racial composition and signal type in a manner consistent with statistical discrimination and in contrast to patterns predicted by a model of taste-based discrimination.

I. Introduction

Racial and ethnic discrimination pervades many markets in the United States. Roughly half of the discriminatory cases reported by federal agencies involve race or ethnicity, and the number of reported new incidents outpaced population growth over the past ten years.1 The literature posits two major sources of racial discrimination: taste based and statistical. Racial prejudice produces taste-based discrimination, while statistical discrimination occurs in an environment of imperfect information in which agents form expectations based on limited signals that correlate with race.2 The result of both types of discrimination, however, is the same: similar individuals who differ only by their race experience different outcomes. A simple examination of differential outcomes sheds little light on the source of discrimination.

Employing an e-mail correspondence experiment in the U.S. rental apartment market, we test whether statistical or taste-based discrimination can explain differential outcomes between white and African American rental applicants. We extend Aigner and Cain’s (1977) and Morgan and Vardy’s (2009) models of statistical discrimination to test the key feature of statistical discriminators: heterogeneous experience.3 The model posits that landlords differ in their perceptions of signals due to past experience in the screening and rental process and place a greater weight on signals from the familiar group than the unfamiliar group in making decisions. We contrast the predictions with those of taste-based discrimination, in which prejudiced landlords use information independent of race to predict expected tenant quality but derive lower marginal utility from renting to the out-group. We show that lower marginal return to signal of quality for minority groups is consistent with both statistical and taste-based discrimination, highlighting the empirical hurdle involved in attempting to separate the two explanations. The model guides our experimental design.

Using vacancy listings on Craigslist.org, an online classified ad website, across 34 U.S. cities, we send inquiry e-mails with two key components to 14,000 landlords. We use the common, racial-sounding first names employed by Bertrand and Mullainathan (2004) to associate applicants with race, and the inquiry e-mail contains differing—but limited—pieces of information about the applicants: positive, negative, and no signals beyond race. In the no-signal inquiry, landlords receive e-mails with racial-sounding names as the only signal. In the positive information inquiry, the fictional applicant informs the landlord that she is a nonsmoker with a respectable job. In the negative information inquiry, the applicant tells the landlord she has a below-average credit rating and smokes. The independent variable codes landlords’ responses to capture an invitation to the inquiry for future contact. Although the outcome reflects only a positive response during the initial inquiry phase of a screening process, any differential treatment in screening will influence final outcomes in the same direction. Since residential locations are tied closely to characteristics associated with welfare, such as the type of job held, crime levels, and school quality, our focus on the rental apartment market is policy relevant. As the dominant source of online classifieds for apartment listings in the United States, Craigslist is frequented by one-third of the white and black U.S. adult population. The growing prevalence of online interactions in real estate, employment, finance, and auctions suggests the results extend beyond the rental apartment market.

The experiment yields four major results. First, when no-signal inquiry e-mails are sent, applicants with African American–sounding names have a 9.3 percentage point lower positive response than applicants with white-sounding names. Second, using a difference-in-differences (DiD) estimator, we show that the racial gap in response rates widens in the switch from negative to positive information. Both findings are consistent with statistical discrimination and prejudice, as they follow from landlords placing more weight on signals sent by white applicants than black appli-
cants or landlords receiving a greater marginal utility of renting from white applicants than from black applicants. Thus, the research design requires further refinement to separate the explanations.

Third, the model defines the notion of a “surprising” signal, where the no-signal base case acts as a benchmark for uninformative expectations and as a means to quantify surprise relative to the better-than-expected (positive) and worse-than-expected (negative) signals. This notion of a surprising signal is difficult to introduce in a job application setting in which résumés are required, as it is impossible to provide 0 information about education or experience in a résumé. With differential weighting of signals by race, statistical discrimination predicts that a surprising positive signal will not necessarily shrink the racial gap in the base case, but that a negative surprise will. In contrast, the taste-based discrimination model shows that a surprising positive signal will widen the racial gap. Our empirical results are consistent with statistical discrimination.

Finally, we exploit neighborhood racial composition as a source of heterogeneity in landlord experiences with, or preferences for, different racial groups. By allowing a signal’s noise to depend on race, the statistical discrimination model presents another testable hypothesis: a landlord’s relative experience with a given race increases the relative weight she places on the signals from that racial group. Conversely, taste-based discrimination predicts that if landlords exhibit out-group prejudice and that if their race and racial preferences are correlated with neighborhood racial composition, as the share of blacks in a neighborhood increases, discrimination against black applicants in the base case will attenuate. Surprising negative information will, in contrast, hurt black applicants more. As the share of blacks in a neighborhood increases, a surprising positive signal closes the racial gap observed relative to the base case, while a surprising negative signal does little to close it. More importantly, the base case racial gap persists across all types of neighborhoods and contradicts the prediction of taste-based discrimination.

This paper extends the large body of research on racial discrimination. With the exception of List (2004) and Levitt (2004), past evidence of statistical discrimination is inconclusive. Altonji and Pierret (2001) and Bertrand and Mullainathan (2004) find significant racial gaps in wages and job interview callback rates, respectively, but weak support for statistical discrimination. The related literature on racial profiling in the context of police searches, such as Knowles, Persico, and Todd (2001) and Antonovics and Knight (2009), shows mixed evidence of racial prejudice. Audit studies, such as Yinger (1986) and Page (1995), show discrimination against minorities in U.S. housing markets. These results may suffer from the confounding factors inherent to in-person audit studies, as they rely on actors who often differ in many dimensions. In contrast, Carpensor and Loges (2006) and Ahmed and Hammarstedt (2008) pioneered the use of e-mail correspondence design to study ethnic discrimination in rental housing markets.

Our approach differs from those of previous studies and contributes to the literature in several ways. We model explicitly how and why statistical and taste-based discrimination in the screening process can each predict lower marginal return to signal for the discriminated-against group, which potentially resolves some contradictory hypotheses and findings in the discrimination literature. We extend Aigner and Cain’s (1977) and Morgan and Vardy’s (2009) nonprejudice discrimination framework and juxtapose it with a prejudice discrimination framework to permit a research design that separates statistical and taste-based explanations empirically. Our framework also allows for landlord risk aversion, and our findings are robust to this specification. In other empirical work, Ahmed, Andersson, and Hammarstedt (2010) and Bosch, Farre, and Camero (2010) argue that statistical discrimination implies reduced discrimination against minorities with increased positive information. They show, however, an unchanging racial gap in the likelihood of positive responses when positive information is introduced in rental e-mail inquiries and argue that the finding supports taste-based discrimination. Bertrand and Mullainathan (2004), in contrast, argue that the finding of lower or equal marginal returns to credentials for minorities is inconsistent with taste-based discrimination. On the other hand, Hanson and Hawley (2011) argue that decreased reply differences between white and African American rental applicants from improving the prose quality of e-mails suggest statistical discrimination. Because reply rates decrease with class for white applicants, however, their results may be driven by the higher fraction of negative replies to low-class e-mails. Our novel modeling and estimation framework demonstrates the difficulty of separating statistical discrimination from taste-based discrimination in most field experiments.

II. Discrimination in Screening and Testable Implications

We present a model to guide our research design and distinguish between statistical and taste-based discrimination in the rental apartment market. A landlord seeks to maximize the expected utility of interviewing each applicant, subject to a capacity constraint of \( M \) interviews and a constant marginal cost of interview \( c \). The expected utility derived from each applicant depends on the stream of future rental income (tenant quality) from renting the apartment successfully. This quality is
summarized by $\theta$. Although the rent is preannounced, $\theta$ may still vary as a result of default, lease renewal, and so on. Hence, the landlord forms a predicted quality $\hat{\theta}$, a random variable for each applicant and maximizes the expected utility $E[U(\hat{\theta})]$.

Consider the following four-stage process of matching potential tenants to apartments:

1. Inquiry: An applicant with quality $\theta$ selects publicly posted rental units to send costless inquiries with signal $x$ to landlords.
2. Screening: Given signals $X_T = \{x_1, \ldots, x_T\}$ received from $T$ independent applicants, the landlord forms a set of predicted qualities $\Theta_T = \{\hat{\theta}_1, \ldots, \hat{\theta}_T\}$ and responds to $n$ applicants.
3. Interview: Interviews, which include credit and reference checking, reveal the true quality $\theta$ and cost applicants and landlords $m$ and $c$, respectively.
4. Decision: The candidate with the highest true quality $\theta$ is offered the apartment.

This setting is similar to that of Morgan and Vardy (2009), in which employers have one vacancy, want only “competent” workers, and receive signals about the workers’ types. That model has an additional stage in which true type is not revealed until hiring is complete. Our experiment can distinguish only statistical and taste-based discrimination occurring at stage 2 of the process, and the results are consistent with Morgan and Vardy’s (2009) partial equilibrium, one-sided search approach. Nevertheless, we consider stages 2 through 4 in the model and discuss the implications of strategic signaling by applicants.

### A. Statistical Discrimination

Suppose that signal $x$ proxies the true quality $\theta$ noisily with a race-specific error $e_r$:

$$x_r = \theta_r + e_r$$  \hspace{1cm} (1)

where $\theta_r \sim N(\mu_r, \sigma_{\theta_r})$, $E(e_r|\theta_r) = 0$, $\text{var}(e_r|\theta_r) = \sigma_{e_r}^2$, $E(x_r) = \mu_r$, and $\var(x_r) = \sigma_{\theta_r}^2 + \sigma_{e_r}^2$. The assumptions mostly correspond to those in Aigner and Cain’s (1977) model, except that we permit the signal mean to vary by race.

Landlords have a sample of inquiries $x$ and applicants’ true quality $\theta$ acquired during past iterations of stages 2 and 3. Using this sample, a landlord can estimate the following forecasting regression for each race $r$:

$$\hat{\theta}_r = \hat{\mu}_r + \hat{\gamma}_r x_r$$  \hspace{1cm} (2)

where $\hat{\mu}_r$ is the ordinary least squares (OLS) estimator of the intercept term; $\hat{\gamma}_r$ is the estimator of the marginal effect of signal $x_r$; and $r$ is $W$ for whites and $B$ for blacks. Estimates of $\hat{\mu}_r$ and $\hat{\gamma}_r$ will vary across landlords because of their differing experiences. Equation (2) is similar to the posterior belief in Morgan and Vardy’s (2009) or Balsa and McGuire’s (2001) model. We assume, however, that landlords do not have prior beliefs about the true means and variances of quality and signal but rather form posterior beliefs using OLS estimates obtained via past samples. Indeed, Bayes’s rule yields the same $\hat{\mu}_r$ and $\hat{\gamma}_r$ under a joint normality assumption.\(^8\)

A landlord observes signal $\tilde{x}$ and race $r$ from an applicant in stage 2 and predicts the quality by plugging $\tilde{x}$ into equation (2):

$$\tilde{\theta}_r = \tilde{\mu}_r + \tilde{\gamma}_r \tilde{x}_r.$$  \hspace{1cm} (3)

We call $\tilde{\gamma}_r$ the information weighting parameter for race $r$ since it informs a landlord how much to weight a signal from an applicant of race $r$.\(^9\)

Some applicants may reveal only their race in an e-mail inquiry.\(^10\) In this case, the landlord infers quality using the average signal $(\bar{x}_r)$ observed among race $r$ in past e-mail inquiries in the following forecasting regression:

$$\hat{\theta}_r = \hat{\mu}_r + \hat{\gamma}_r \bar{x}_r.$$  \hspace{1cm} (4)

Equation (4) is equivalent to the landlord using some average $\theta$ among $r$ to form a prediction.\(^11\)

### Statistical discrimination under risk neutrality and its implications.

For a risk-neutral landlord with race-invariant utility, if the total number of applicants is $T \leq M$, the landlord responds positively to $n$ of the $T$ independent applicants, where each yields $E[U(\hat{\theta}_r)] \geq c$. If applications exceed capacity ($T > M$), the landlord will sort all applicants by $E[U(\hat{\theta}_r)]$ and will invite the top $M$. As utility and cost are race invariant, the decision rule is in line with Morgan and Vardy’s (2009) color-blind threshold. Here, the decision rule will be some $\hat{\theta}$.

As statistical discrimination influences a landlord’s decision through $\hat{\theta}_r$, differential outcomes by race arise through the OLS estimators in equation (2):

$$\hat{\gamma}_r = \frac{\text{cov}(\theta_r, x_r)}{\text{var}(x_r)},$$  \hspace{1cm} (5)

$$\hat{\mu}_r = \hat{\theta}_r - \hat{\gamma}_r \bar{x}_r.$$  \hspace{1cm} (6)

\(^8\) As landlords are not interested in the causal relationship between quality and signal, but only predictions that yield the lowest variance, OLS is sensible. It can be generalized to a Bayesian framework. For instance, landlords have prior beliefs that yield the intercept and slope of equation (2) and update them as they observe more realizations of quality and signal.

\(^9\) This is not strictly a parameter, but a landlord’s estimator of the parameter of the regression model.

\(^10\) See section III for an example of such an inquiry.

\(^11\) See section IIb for the implications when applicants strategically reveal no information.
Here, $\text{cov}(\bar{\theta}_r, \bar{x}_r)$ is the sample covariance between quality and signal, $\text{var}(x_r)$ is the sample variance of the signal, and $\bar{\theta}_r$ and $\bar{x}_r$ are the sample average of quality and signal of applicants, respectively. Despite the unobservability of $\mu_r^l$ and $\gamma_r$, we can experimentally manipulate the race of applicants and signals sent by applicants to examine whether landlord responses are consistent with the model’s predictions. If each landlord’s sample of $\bar{\theta}_r$ and $\bar{x}_r$ were observable, we could average the numerator and denominator of the information-weighting parameter across the sample of landlords $(n_l)$ to obtain

$$\gamma_r = \frac{(1/n_l) \sum \text{cov}(\bar{\theta}_r, \bar{x}_r)}{(1/n_l) \sum \text{var}(x_r)}. \tag{7}$$

Similarly, we have the average of the intercept term:

$$\mu_r^l = \frac{(\bar{\theta}_r)}{(1/n_l)} - \gamma_r \frac{(\bar{x}_r)}{(1/n_l)}. \tag{8}$$

In a large sample, equations (7) and (8) yield the means.

Given the assumption $E[\text{cov}(\bar{\theta}_r, \bar{x}_r)] = \sigma^2$, equation (7) shows that any differences in the noise of signals, $E[\text{var}(x_r)]$, can induce differences in the weight placed on the same signal from different races. For example, landlords with $E[\text{var}(x_B)] > E[\text{var}(x_W)]$ will have $\gamma_B < \gamma_W$.\textsuperscript{12} For applicants with objectively identical signals except race, the predicted quality is greater for white than for black applicants. Note that we have not placed any restrictions on the means signals or on noise across race. Differences in mean signal or noise could stem from fundamental racial differences in signals such as income or credit scores of the population or simply from those observed by landlords in their samples.

With random assignment of race $r = \{W, B\}$ and in the absence of additional signals, we can examine whether landlord responses are consistent with an average landlord having $\bar{\theta}_W > \bar{\theta}_B$, $\bar{\theta}_W = \bar{\theta}_B$, or $\bar{\theta}_W < \bar{\theta}_B$. Equations (4) and (8) imply that

$$E(\bar{\theta}_r) = (1 - \gamma_r)\bar{\mu}_r + \gamma_r\mu_r = \mu_r. \tag{9}$$

Given past findings of discrimination against black applicants, we expect:

**Hypothesis 1.** On average, a white applicant is more likely to receive a positive response than a black applicant in the no-signal base case.

If we assign a negative signal $-\bar{x}_- < 0$ or a positive signal $\bar{x}_+ > 0$ and race randomly to different applicants and present them to randomly selected landlords, we can use a DiD approach to test whether $\gamma_W > \gamma_B$, $\gamma_W = \gamma_B$, or $\gamma_W < \gamma_B$.

Equation (2) implies that the mean difference between black and white applicants sending a positive signal is

$$E(\bar{\theta}_B|\bar{x}_+) - E(\bar{\theta}_W|\bar{x}_+) = (\mu_B^l - \mu_W^l) - (\gamma_W - \gamma_B)\bar{x}_+. \tag{10}$$

Similarly, the mean difference between black and white applicants sending a negative signal is

$$E(\bar{\theta}_B|-\bar{x}_-) - E(\bar{\theta}_W|-\bar{x}_-) = (\mu_B^l - \mu_W^l) + (\gamma_W - \gamma_B)\bar{x}_-. \tag{11}$$

Taking the difference of equations (11) and (10) yields

$$E(\bar{\theta}_B - \bar{\theta}_W|\bar{x}_+) - E(\bar{\theta}_B - \bar{\theta}_W|-\bar{x}_-) = (\gamma_W - \gamma_B)(\bar{x}_+ + \bar{x}_-). \tag{12}$$

The extent of dependence across signals of tenant quality that landlords obtained through experience influences the average sample variance of signal $\text{var}(x_r)$.\textsuperscript{13} Massey and Denton’s (1987) and Iceland, Weinberg, and Steinmetz’s (2002) descriptions of residential segregation and neighborhood sorting imply that signals are positively correlated within a racial group. Landlords renting in neighborhoods that are predominantly white are relatively more experienced dealing with white tenants than black tenants. These landlords’ average sample variance of signals from white applicants will be smaller than that from black applicants because of neighborhood sorting.\textsuperscript{14} Since the average landlord in a national sample rents in a predominantly white neighborhood, we expect $\gamma_W > \gamma_B$. This prediction is also consistent with previous studies (for example, Bertrand and Mullainathan, 2004, and Ahmed et al., 2010) that find smaller marginal returns to signal for blacks than for whites. So we have:\textsuperscript{15}

**Hypothesis 2.** On average, the positive response gap between white and black applicants is larger with a positive signal sent than with a negative signal sent.

If we (randomly) send a negative signal that is below the mean signal of race $r$, $-\bar{x}_- < E(\bar{x}_r)$, or a positive signal that is above the mean, $\bar{x}_+ > E(\bar{x}_r)$, to landlords, we can validate whether signals lead to differences in responses that are consistent with $\gamma_W > \gamma_B$.

Call the difference between the signal a landlord observes and her expected signal for the no-signal base case a “surprise in signal.” With an identical positive signal for black and white applicants and $E(\bar{x}_W) > E(\bar{x}_B)$,\textsuperscript{16} we have

\textsuperscript{12} Here, assuming quality variance $(\sigma^2)_{\theta}$ is the same across race is crucial. Note that $\text{cov}(\bar{\theta}_r, \bar{x}_r) = \text{var}(x_r) = \sigma^2$, given the assumption that $\bar{\theta}_r \sim N(\mu_r, \sigma^2)$. We discuss the robustness of our results to this assumption in section V.

\textsuperscript{13} The fact that signals are not i.i.d. has no bearing on the landlord’s decision to use OLS, since the landlord cares only about getting the best linear prediction.

\textsuperscript{14} Appendix A shows how differences in the variance of signals across racial groups may arise.

\textsuperscript{15} We assume that applicants are truthful and that there is a separating equilibrium in signals by type.

\textsuperscript{16} As evident in past studies showing African Americans having lower average social-economic backgrounds than white Americans (Harris, 2010).
\[ [\bar{x} - E(\bar{x}_B)] > [\bar{x} - E(\bar{x}_W)]. \] The experimentally manipulated negative information will be a greater surprising signal for whites than for blacks: \(-[\bar{x} - E(\bar{x}_W)] < [\bar{x} - E(\bar{x}_B)].\) Depending on the relative sizes of \(\gamma_w\) and \(\gamma_b,\) the patterns of landlord responses will differ.

In case 1, where \(\gamma_w = \gamma_b,\) a surprising signal, whether positive or negative, will be weighted equally for blacks and whites. As \([\bar{x} - E(\bar{x}_B)] > [\bar{x} - E(\bar{x}_W)],\) the positive signal benefits black applicants more than white applicants. As \(-[\bar{x} - E(\bar{x}_W)] > [\bar{x} - E(\bar{x}_B)],\) the negative signal hurts white applicants more than black applicants. Hence, it follows that

\[
\gamma_b[\bar{x} - E(\bar{x}_B)] > \gamma_w[\bar{x} - E(\bar{x}_W)],
\]

(13)

\[-\gamma_b[\bar{x} + E(\bar{x}_B)] > -\gamma_w[\bar{x} + E(\bar{x}_W)].\]

(14)

That is, compared to the no-signal base case, the gap in expected quality between the two racial groups closes in the presence of either positive or negative information (case 1 of figure 1).

In case 2, where \(\gamma_w > \gamma_b,\) expression (14) is unambiguously satisfied, but the relationship in equation (13) may not be true. Thus, when \(\gamma_w > \gamma_b,\) negative information will shrink the gap in expected quality between blacks and whites, but positive information will not necessarily narrow the gap (case 2 of figure 1). Finally, in case 3, where \(\gamma_w < \gamma_b,\) expression (13) will be satisfied, but expression (14) will not necessarily be satisfied. In this case, the positive treatment will narrow the racial gap, but the negative treatment may not (case 3 of figure 1). Therefore, given hypothesis 2, we have:

**Hypothesis 3.** On average, negative information will shrink the racial gap observed in the base case, but positive information will have an ambiguous effect on the racial gap observed in the base case.

The model implies that whether \(\gamma_w > \gamma_b, \gamma_w = \gamma_b,\) or \(\gamma_w < \gamma_b\) for an average landlord depends on whether \(\text{vár}(x_B) < \text{vár}(x_B), \text{vár}(x_W) = \text{vár}(x_B),\) or \(\text{vár}(x_W) > \text{vár}(x_B)\) on average. If the relative size of \(\gamma,\) varies with \(E[\text{vár}(x)]\) in the direction predicted by the model, it suggests that landlords’ behaviors are consistent with our model of statistical discrimination.

Given neighborhood sorting and positive covariance of signals, as the share of black residents in a neighborhood, \(S_B,\) increases, we expect \(\text{vár}(x_B)\) to decrease and \(\text{vár}(x_W)\) to increase on average. As \(S_B\) increases from 0 to 1, it is increasingly likely that \(\gamma_B \geq \gamma_W.\) The positive relationship between \(\gamma_B\) and \(S_B\) implies that the relationship between a surprising signal and shrinkage in the racial gap (hypothesis 3) will also vary with \(S_B.\) As \(S_B \to 1,\) the effect of a surprising positive signal in narrowing the racial gap in positive response rates will become more evident (case 3 in figure 1). Thus:

**Hypothesis 4.** Positive treatment should shrink the racial gap in positive responses relatively more in predominantly black neighborhoods. Conversely, negative treatment will shrink the racial gap in predominantly black neighborhoods.
white neighborhoods, but not necessarily in predominantly black neighborhoods.\textsuperscript{17}

\textbf{Risk aversion and implications.} To assess the implications of landlord risk aversion, assume that the landlord’s expected utility takes a mean-variance form (for example, exponential utility). Here, the variance of the predicted quality generates another source of differential treatment by race. Based on equation (3), the variance of predicted quality conditional on a given signal $\bar{x}$ from a race-$r$ applicant is

\[
\text{var}(\hat{\theta}_{r} | \bar{x}) = \sigma_{r}^{2} \left( \frac{1}{N_{r}} + \frac{(\bar{x} - \bar{x}_{r})^{2}}{\sum_{j=1}^{N_{r}} (x_{j} - \bar{x}_{r})^{2}} \right)
\]

\[
= \sigma_{r}^{2} \left( \frac{1}{N_{r}} + \frac{(\bar{x} - \bar{x}_{r})^{2}}{N_{r} \text{var}(x_{r})} \right),
\]

(15)

where $N_{r}$ is the number of past race-$r$ applicants that the landlord ever observed.

In the no-information base case, equation (15) shows that compared to a risk-neutral landlord, a risk-averse landlord further discounts applicant signals from the group with which she has relatively less experience. Risk aversion will thus widen the response gap between white and black applicants in hypothesis 1.

When a black applicant sends a surprising positive (negative) signal, the applicant’s conditional variance of predicted quality is larger (smaller) than that of a similar white applicant, holding all else equal, as $\frac{(\bar{x} - \bar{x}_{r})^{2}}{(\bar{x} - \bar{x}_{w})^{2}}$ is larger (smaller) than $\frac{(\bar{x} - \bar{x}_{r})^{2}}{(\bar{x} - \bar{x}_{w})^{2}}$ in equation (15). The larger is a positive (negative) surprising signal, the more (less) the black applicant is hurt from landlord risk aversion, making the effects of changing the signal on racial gap more pronounced. Thus, hypotheses 2 and 3 are similar with landlord risk aversion.

Since the effects of surprising signals on landlords’ positive responses may vary depending on the extent of landlord risk aversion, neighborhood racial composition must only proxy landlords’ relative experiences with different racial groups and not their degree of risk aversion. Otherwise, hypothesis 4 will examine only the extent of landlords’ risk aversion across neighborhoods under statistical discrimination instead of identifying how relative experience with a particular race shapes the information-weighting parameter. Furthermore, as the share of black residents in a neighborhood ($N_{B}$) increases, $N_{r}$ increases and $\text{var}(x_{B})$ decreases in equation (15). There is a possibility for positive responses to black applicants’ surprising positive signals to increase relatively less than positive responses to white applicants’ surprising positive signals as $S_{B} \to 1$. Here, the extent of risk aversion and the effect from shrinking $\text{var}(x_{B})$ on the variance of predicted quality are large enough to offset the increase in the information-weighting parameter. We assess whether our results are sensitive to this possibility in section V.

\textbf{B. Strategic Signaling and Truth Telling}

The landlord’s expected signal $\bar{x}_{r}$, assumed to be the average signal received in the landlord’s history, depends crucially on the applicants’ signal choices. If applicants instead send signals not directly tied to their type (for example, they strategically lie or do not reveal), the interpretation of differences in response rates may need to be altered.

Let there be three applicant types: high, average, and low. Suppose in stage 3 of the rental process that applicants pay a credit check fee, landlords observe true applicant type, and landlords reject any applicant who lied in the first stage. Here, applicants will never find it advantageous to lie about their type in the inquiry stage. Applicants who do reveal their type are still truthful but may still decide not to signal type at all.\textsuperscript{18}

Nonrevelation of type mimics our no-information case. High-type applicants will always find it advantageous to reveal their type to separate themselves, but low-type applicants can increase their response rate by nonrevelation and pooling with the average applicant type. Such a pooling equilibrium presents two problems for our analysis. First, the average signal $\bar{x}_{r}$ is no longer the analogue of the average type but rather the mean of low and average types. This lower average signal lowers the predicted response rate to no information and the interpretation of the gaps between it and other signals. Our predictions are robust to this scenario if landlords use the average of past no-information inquiries and interview results to forecast quality. Second, under pooling, it is no longer optimal for low-type applicants to send signals congruent with their type, and thus the signals we send do not fit into the landlords’ decision-making frameworks.

What does the low-type applicant’s nonrevelation strategy accomplish? With a higher expected response rate relative to full revelation, low-type applicants have increased the expected cost of interviewing. Suppose that some landlords would have rejected the low-type applicants if their signals matched their types. If there is a sufficient fraction of such landlords, low-type applicants will risk paying for interviews they are certain to fail. Sending a negative information e-mail thus benefits the low-type applicant through more accurate inference of the landlord’s likelihood of renting the apartment. Pooling adds significant noise and higher

\textsuperscript{17} Previous discriminatory behavior might contribute to segregation and variation in neighborhood racial composition. If this discrimination is statistical, then using neighborhood racial composition as the proxy for landlords’ relative experiences with different racial groups will lead to strong results because landlords’ experiences reinforced themselves in the samples observed.

\textsuperscript{18} The argument also applies to race revelation. If the average white applicant is of higher quality than the average black applicant, the former will always reveal their race in inquiries.
cost. We conclude that landlords treat negative information e-mails as coming from low-type applicants.

C. Taste-Based Discrimination

Differential outcomes by race may also arise from prejudice. Let a prejudiced landlord predict applicant quality based on a race-independent signal using a pooled OLS regression:

$$\hat{\theta}_i = \mu_k + \gamma \cdot x_i$$

(16)

The intercept and slope in equation (16) are race invariant, which results in similar invariance for sample means and variances.\(^\text{19}\)

Prejudiced landlords use the same decision rule for selecting applicants as above, but their utility is now race dependent. Assume that the landlord exhibits out-group prejudice such that a prejudice parameter, \(k\), discounts the (marginal) utility derived from an out-group applicant so that \(E[U(\theta_r)] > E[U(\theta_{r-})]\) when \(\theta_r = \theta_{r-}\). This framework contrasts with that of statistical discrimination, where \(E[U(\theta_r)] = E[U(\theta_{r-})]\) if \(\theta_r = \theta_{r-}\). As the predicted quality has race-invariant variance, we focus solely on the implications of risk-neutral landlords.

We introduce a prejudice parameter into the landlord’s utility function, which is similar to Knowles et al.’s (2001) race-varying marginal cost.\(^\text{20}\) Although our pooled OLS assumption is more restrictive, it avoids additional assumptions about how prejudiced landlords incorporate information differently by race, while simultaneously not discriminating statistically.\(^\text{21}\) If a prejudiced landlord instead incorporates information differently for each race by penalizing the out-group with a smaller slope and a lower or equal intercept in equation (16), testable hypotheses similar to the pooled OLS regressions will still emerge.

Taste-based discrimination testable implications. Given equation (16) and greater marginal utility for in-group applicants than for out-group applicants, if e-mail inquiries reveal only the races of applicants, then:

**Hypothesis 1A.** On average, a white applicant is more likely to receive a positive response than a black applicant in the no-signal base case.

Next, greater marginal utility for white than for black applicants implies that an increase in signal benefits the white applicant more than the black applicant:

**Hypothesis 2A.** On average, the response gap between white and black applicants when a positive signal is sent is larger than the response gap between white and black applicants when a negative signal is sent.

When a positive signal greater than the mean signal (\(\bar{x}\)) is sent, equation (16) combined with prejudice predicts that the average landlord’s response gap between white and black applicants will widen relative to the base case. When a negative signal below the expected signal for applicants is sent, the landlord’s response gap between white and black applicants will narrow. Therefore, the predictions differ slightly from those under statistical discrimination:

**Hypothesis 3A.** On average, negative information will unambiguously narrow the racial gap observed in the no-signal base case, but positive information will unambiguously widen the racial gap observed in the base case.

As the share of black residents in a neighborhood (\(S_B\)) increases, the probability that a landlord renting in that neighborhood is black also increases.\(^\text{22}\) This implies that the fraction of landlords having prejudice against black applicants decreases. Therefore, hypotheses 1A, 2A, and 3A will switch signs or directions as we move from a majority white neighborhood to a majority black neighborhood, leading to:

**Hypothesis 4A.** As the share of black residents in a neighborhood \(S_B\) increases, the response gap between white and black applicants in the base case decreases. In a majority black neighborhood, a surprising positive signal will unambiguously benefit a black applicant relatively more than a white applicant, while a surprising negative signal will unambiguously hurt a black applicant relatively more than a white applicant.

III. Experimental Design and Econometric Specifications

Craigslist serves as an ideal experimental platform to test the model implications because of its focus on e-mail communication.\(^\text{23}\) First, hypothesis 1A requires limiting the information available to agents to just race, which is straightforward in e-mail correspondence but difficult in audit/in-person studies or other correspondence experiments. Next, hypotheses 2A and 3A demand clear signals that are also unambiguously distinct (positive versus negative), which

\(^{19}\) We thank an anonymous referee for this suggestion.

\(^{20}\) If we instead assume race-varying marginal cost, the testable hypotheses remain the same.

\(^{21}\) In addition to having a prejudice parameter on the marginal search (interview) cost, Knowles et al. (2001) allow the police’s (landlords’) beliefs about the probability of guilt (predicted quality) of motorists (applicants) with certain characteristics (\(x\)) to differ by race, but they do not specify how landlords incorporate information differently by race while simultaneously not statistically discriminating. We take the position that prejudice is a utility or preference assumption and incorporate it only into the utility function.

\(^{22}\) The correlation between the share of black home owners and the share of black residents across public use micro areas is 0.96 in the American Community Survey 2009.

\(^{23}\) The experiment was conducted between September 2009 and October 2009.
and numerous precautions were taken to avoid sending multiple inquiries to the same landlord or the same listing.\textsuperscript{27} The sample excludes units with rents below the 20th and above the 90th percentile within each city to avoid sending e-mails to weekly rentals or homes for sale. Finally, e-mails were sent within 48 hours after the listing was posted. Table 2 lists the cities surveyed and their characteristics.\textsuperscript{28} The average share of black residents across neighborhoods is similar to the actual share of the black population in the greater metropolitan area in Census 2000.

### B. E-Mail Generation and Experimental Treatments

To maximize the probability that landlords will observe the racial-sounding names, the full name of the fictitious applicant is presented three times in every e-mail: first in the e-mail address, which is always of the form “first\_last\_<random number>\@domain.com”; second in the introductory sentence of the e-mail text; and third in the closing signature of the e-mail. First names chosen are those used by Bertrand and Mullainathan (2004). Surnames are sourced from the U.S. Census 2000 Family Name Survey. Resulting name combinations include Allison Bauer, Ebony Washington, Matthew Klein, and Darnell Booker. \textsuperscript{29}

Each e-mail text was generated by randomly selecting the text for each of the five elements numerated in the sample e-mails in exhibit 1. With the exception of the statement of quality, all text was pulled from the same pools:

1. an introductory \textit{hello} statement.
2. a \textit{statement of interest} in the apartment, which always includes the rent of the unit (to avoid confusion in case the landlord has posted multiple listings).

\begin{table}
\caption{Comparison of Craigslist Users and Non-Craigslist Users}
\begin{tabular}{lcccccccc}
\hline
 & \multicolumn{2}{c}{Full Sample} & \multicolumn{2}{c}{Craigslist\textsuperscript{a}} & \multicolumn{2}{c}{Non-Craigslist} & \multicolumn{2}{c}{Internet\textsuperscript{b}} & \multicolumn{2}{c}{Non-Internet} \\

 & Black & White & Black & White & Black & White & Black & White & Black & White \\

Age (years) & 42.81 & 47.94 & 35.72 & 40.85 & 38.88 & 47.84 & 37.49 & 44.39 & 53.87 & 61.82 \\
Male & 0.46 & 0.48 & 0.47 & 0.54 & 0.51 & 0.45 & 0.49 & 0.50 & 0.38 & 0.43 \\
College\textsuperscript{c} & 0.40 & 0.55 & 0.57 & 0.66 & 0.52 & 0.60 & 0.54 & 0.63 & 0.11 & 0.24 \\
Low income\textsuperscript{d} & 0.63 & 0.40 & 0.52 & 0.32 & 0.70 & 0.35 & 0.62 & 0.33 & 0.65 & 0.65 \\
Renter\textsuperscript{e} & 0.55 & 0.21 & 0.57 & 0.23 & 0.53 & 0.17 & 0.55 & 0.20 & 0.57 & 0.24 \\
Single\textsuperscript{f} & 0.40 & 0.19 & 0.41 & 0.21 & 0.43 & 0.21 & 0.42 & 0.21 & 0.36 & 0.13 \\
Full-time job\textsuperscript{g} & 0.41 & 0.45 & 0.63 & 0.57 & 0.43 & 0.49 & 0.52 & 0.53 & 0.20 & 0.16 \\
Internet user\textsuperscript{h} & 0.67 & 0.79 & – & – & – & – & – & – & – & – \\
Craigslist user\textsuperscript{i} & 0.30 & 0.39 & – & – & – & – & 0.44 & 0.49 & – & – \\
Sample size & 684 & 4,311 & 202 & 1,683 & 258 & 1,739 & 460 & 3,422 & 224 & 889 \\
\hline
\end{tabular}
\end{table}

\textsuperscript{24} The full (detailed) experimental design is available on request.

\textsuperscript{25} A report by AIMGroup shows newspaper classified advertisement sales fell from $16 million to $5 million between 2005 and 2009, while Craigslist’s revenue grew from $18 million to over $100 million over the same period.

\textsuperscript{26} Approximately 2.5\% of all U.S. Internet visits are to Craigslist, while other classified websites combined account for only 0.14\% of U.S. Internet visits.

\textsuperscript{27} The Institutional Review Board requires one inquiry per landlord so as to reduce potential harm. Since treatments are randomly assigned, landlords are on average identical across groups.

\textsuperscript{28} Roughly one-third of postings that lack cross-street information are placed in the greater metropolitan area.

\textsuperscript{29} White female, black female, white male, and black male, respectively. A full list of first names and surnames used is in table 9.
3. a **statement of quality**, which is randomly included (or not included) to define our treatments.

4. an **inquiry statement** regarding the availability of the unit (for example, “is this apartment still available?”). This gives the landlord a specific question to which to respond, allowing us to identify automated responses and to test for differences in positive responses between groups.

5. a **closing** that thanks the landlord and is always followed with the applicant’s full name.

Element 3, the statement of quality, is included in approximately two-thirds of all e-mails. E-mails that do not include a statement of quality belong to the baseline treatment or base case. In this treatment, landlords know only the applicant’s name and interest in renting the apartment. The model assumes that landlords take the average signals by race or independent of race as a proxy signal in the base case. When the statement of quality is included, it discloses either positive or negative information. Positive information informs the landlord that the applicant has a good job and does not smoke. Negative information informs the landlord that the applicant smokes and has a bad credit rating.³⁰

### TABLE 2.—Cities Surveyed

| City               | Number of Observations | Number of Neighborhoods | Mean %Black across Neighborhoods | %Black in Metropolities Area | Mean Monthly Rent |
|--------------------|------------------------|-------------------------|----------------------------------|-----------------------------|------------------|
| Atlanta            | 304                    | 115                     | 27.4%                            | 29.4%                       | 757.43           |
| Austin             | 198                    | 61                      | 6.8                              | 7.4                         | 719.55           |
| Baltimore          | 499                    | 177                     | 28.1                             | 27.4                        | 848.16           |
| Boston             | 1,324                  | 413                     | 6.1                              | 6.6                         | 1,062.74         |
| Charlotte          | 241                    | 72                      | 24.1                             | 20.3                        | 725.55           |
| Chicago            | 596                    | 216                     | 15.9                             | 18.7                        | 1,087.60         |
| Cleveland          | 372                    | 151                     | 15.7                             | 18.2                        | 561.24           |
| Dallas             | 150                    | 51                      | 11.9                             | 15.0                        | 873.59           |
| Denver             | 744                    | 230                     | 6.2                              | 5.6                         | 728.71           |
| Detroit            | 461                    | 189                     | 16.0                             | 22.6                        | 596.73           |
| District of Columbia | 1,179                | 326                     | 24.3                             | 26.2                        | 1,353.51         |
| Houston            | 313                    | 99                      | 13.7                             | 17.4                        | 794.60           |
| Indianapolis        | 158                    | 82                      | 18.5                             | 14.0                        | 543.92           |
| Jacksonville       | 126                    | 51                      | 21.0                             | 21.5                        | 672.43           |
| Kansas City        | 276                    | 117                     | 14.6                             | 12.8                        | 589.39           |
| Los Angeles        | 1,029                  | 482                     | 7.4                              | 9.4                         | 1,186.57         |
| Louisville         | 239                    | 63                      | 14.9                             | 15.2                        | 549.50           |
| Memphis            | 112                    | 34                      | 36.3                             | 44.1                        | 662.22           |
| Milwaukee          | 219                    | 89                      | 11.9                             | 15.2                        | 621.27           |
| Minneapolis        | 761                    | 271                     | 7.9                              | 5.3                         | 761.04           |
| Nashville          | 181                    | 66                      | 20.1                             | 15.4                        | 794.80           |
| Oklahoma City      | 179                    | 76                      | 12.0                             | 11.3                        | 492.27           |
| Philadelphia       | 554                    | 203                     | 21.1                             | 19.5                        | 914.97           |
| Phoenix            | 273                    | 115                     | 3.6                              | 3.4                         | 607.15           |
| Portland           | 303                    | 124                     | 4.0                              | 2.7                         | 770.77           |
| Raleigh            | 255                    | 90                      | 22.2                             | 22.1                        | 645.14           |
| San Diego          | 793                    | 273                     | 5.1                              | 5.4                         | 1,045.49         |
| San Francisco      | 427                    | 132                     | 5.3                              | 5.2                         | 1,471.80         |
| San Antonio        | 86                     | 36                      | 5.4                              | 6.4                         | 612.77           |
| San Jose           | 255                    | 112                     | 2.6                              | 2.6                         | 1,171.98         |
| Santa Barbara      | 164                    | 40                      | 1.9                              | 2.3                         | 1,336.64         |
| Seattle            | 448                    | 182                     | 4.6                              | 4.2                         | 935.66           |
| Tampa              | 667                    | 220                     | 10.6                             | 9.8                         | 677.56           |
| Tucson             | 351                    | 78                      | 2.8                              | 2.7                         | 532.82           |
| Total              | 14,237                 | 5,036                    | 12.4                             | 12.9                        | 905.51           |

³⁰ A neighborhood is a Census tract if cross-street information of the posting is available; otherwise it is a metropolitan statistical area.

³¹ %Black is defined as the number of non-Hispanic blacks divided by all the population in the neighborhood; the mean is obtained by averaging %Black across neighborhoods within the same city.

³² %Black in metropolitan statistical area based on the 5% public use microsample.

³³ Mean rent is calculated using the rents of units we surveyed.

Population data sourced from Census 2000 Summary File 1 and the Integrated Public Use Microdata Series Census 2000 5% sample (Ruggles et al., 2010).

### EXHIBIT 1.—Representative E-mail Samples

| Positive Treatment | Negative Treatment |
|--------------------|--------------------|
| (1) Hello,        | (1) Hi,            |
| (2) My name is [Full Name], and I am writing in response to your listing for an apartment for [apartment rent]/month. (3) In case you’re interested, I do not smoke and I work full time as an architect. (4) Is this unit still available? (5) Thank you for your time, [Full Name] | (2) My name is [Full Name], I am responding to your Craigslist posting for an apartment listed at [apartment rent]/month. (3) Just so you know, I am a smoker and my credit rating is below average. (4) I realize places go fast sometimes, is this unit still available? (5) Thanks, [Full Name] |

³⁰ Revealing hard-to-verify characteristics such as habits and cleanliness is less realistic.
types are unambiguously positive or negative. This methodology does not aim to determine how any specific piece of information affects outcomes, but instead endeavors to test how positive or negative information in general affects outcomes.\(^\text{31}\) It is difficult to imagine a scenario in which a landlord would benefit from a tenant who smokes or has bad credit. Likewise, it is difficult to imagine a landlord being harmed because a tenant has a good job or does not smoke. Landlords typically verify characteristics such as credit-worthiness and smoking habits in the interview stage and commonly ask applicants to pay for credit rating checks. As we discuss in section IIB, it is rational for some low-type applicants to reveal their type in the e-mail inquiry because full revelation may reduce their cost of renting an apartment. Furthermore, our focus on how landlords treat negative signals differently by race ensures that any peculiarity in the sending of negative signals is differenced out. Finally, given the average characteristics of online classified advertisement users reported in table 1, the negative information is likely surprising to landlords, providing a strong treatment effect.

C. Categorizing Outcomes

The simplest dependent variable identifies a landlord response. Responses were further classified into one of several categories, narrowed to either positive or negative.\(^\text{32}\) Positive responses state that the unit is available and invite future contact in some manner. Negative responses include the nonresponse e-mails and those stating that either the unit is not available or that the unit is available, but in a discouraging manner. Each inquiry sent ended with a question such as, “Is the apartment available?” Some 95% of landlords who answered yes to that question also asked for further contact information (coded as a positive response). An e-mail response that simply read yes lacks any direct contact information or interest and likely meant the landlord was not encouraging the applicant for future viewing of the unit and was classified as being disinterested. Simple differences in the likelihood of simply receiving a response may be misleading, since one group may receive a larger share of negative responses than the other.

D. Econometric Specifications

We estimate four regression equations to test our hypotheses. First, the empirical specification to test hypotheses 1 and 1A is

\[
R_i = \alpha_W + \alpha_B B_i + u_i. \tag{17}
\]

\(R\) is 1 if the landlord owning apartment \(i\) responded positively, 0 otherwise; \(B\) is 1 for an applicant with an African American–sounding name, 0 otherwise; and \(u_i\) is an error term. We expect \(\alpha_B < 0.\(^\text{33}\)

Second, the following DiD specification tests hypotheses 2 and 2A:

\[
R_i = \alpha_{PW} + \alpha_{PB}(B_i) + \alpha_{NW}(N_i) + \alpha_{NB}(N_i \times B_i) + u_i. \tag{18}
\]

\(N\) takes the value of 1 for negative information and 0 for positive information. The omitted category is positive information for whites. If the average landlord weights signals from white applicants more heavily than signals from black applicants (\(\gamma_W > \gamma_B\)) or if the marginal utility for whites is greater than for blacks, we expect \(\alpha_{NB}\) to be positive, resulting in a greater marginal return to signal for white applicants.

We estimate the following DiD regression to test hypotheses 3 and 3A:

\[
R_i = \beta_W + \beta_B(B_i) + \beta_{PW}(P_i) + \beta_{PB}(P_i \times B_i) + \beta_{NW}(N_i) + \beta_{NB}(N_i \times B_i) + u_i. \tag{19}
\]

The omitted category is no information for whites. The coefficients \(\beta_{PB}\) and \(\beta_{NB}\) measure the extent of shrinkage in the racial gap of positive response rates in the presence of a (surprising) positive and negative signal. If hypothesis 3 is true, we expect that \(\beta_{NB} > 0\) and that the sign of \(\beta_{PB}\) will be ambiguous. If hypothesis 3A is correct, we expect that \(\beta_{NB} > 0\) and that \(\beta_{PB} < 0\). Hence, failing to reject \(\beta_{PB} \geq 0\) will cast doubt on taste-based discrimination.

Finally, the following empirical specification tests hypotheses 4 and 4A:

\[
R_i = \delta_W + \delta_{SW}(S_{Bi}) + \delta_{SB}(B_i) + \delta_{SBW}(S_{Bi} \times B_i) + \delta_{SPW}(P_i) + \delta_{SPWB}(S_{Bi} \times P_i \times B_i) + \delta_{NW}(N_i) + \delta_{SNW}(S_{Bi} \times N_i) + \delta_{NB}(N_i \times B_i) + \delta_{SNB}(S_{Bi} \times N_i \times B_i) + u_i. \tag{20}
\]

\(S_{Bi}\) measures the fraction of black residents in the neighborhood (% Black) in which apartment \(i\) is listed, and it ranges between 0 and 1. The terms \(S_{Bi}\) and \(S_{Bi} \times B_i\) allow the (unobserved) \(\bar{x}_i\) in the statistical discrimination model or the parameter \(k\) in the taste-based discrimination model to vary across different types of neighborhoods. If landlords’ experiences with black applicants increase the size of their information-weighting parameter, then we expect \(\delta_{SPWB}\) to be ambiguously signed. If landlords renting in a predominantly black neighborhood exhibit a preference for black residents relative to landlords renting in neighborhoods with a smaller share of black residents, then \(\delta_{S_{Bi}}\) should be positive. Moreover, if prejudice explains the observed racial differentials, the surprising positive informa-
tion helps black applicants more and surprising negative information hurts black applicants more as SPB increases. So $d_{SPB} > 0$ and $d_{SNB} < 0$.

IV. Results

Table 3 presents summary statistics of the experiment and data collected. Of the 14,237 inquiries sent, 9,229 (65%) received a response. Of these responses 6,597 (46%) were positive, as defined in section III. Figure 2 shows the distribution of the shares of black residents in census tracts of listed apartments—$S_{Bi}$ in equation (20). The measure ranges from 100% white to 98.45% black with a mean of 12.4% black residents. Table 4 verifies that the characteristics of our fictitious white and black applicants are statistically similar and not correlated with characteristics of listed apartments by treatment types.

A. Effective Informational Treatments

Table 5 reports response rates for the positive and negative treatments relative to the baseline of no signal, pooling all applicants. Comparing the intercept terms in columns 1 and 3 in table 5, which measure response and positive response rates, respectively, in the baseline treatment, reveals that roughly 18% of responses in the baseline were negative in some way. This means that the simple rate of response is likely to misrepresent whether landlords encouraged future contact. Furthermore, the estimates in columns 1 and 3 show that landlords are equally likely to reply to an e-mail inquiry, whether or not the applicant has revealed something positive about herself or himself but also that landlords are more likely to reply with a rejection if the applicant revealed nothing about her or his quality. Therefore, considering a no response as equivalent to a yes response is likely to invite error into our data interpretation.

Column 3 shows that applicants in the positive treatment group receive a significantly higher positive response rate than baseline applicants (0.57 versus 0.53). The effect of positive treatment is slightly higher for females than for males (0.04 versus 0.03). Column 4 shows that applicants in our negative treatment receive a significantly lower positive response rate than baseline applicants (0.32 versus 0.53). The differences illustrate that the treatments effectively manipulated landlord interest in the fictional applicants. Finally, the insignificant differences in response rates across gender and independent of race lead us to pool genders hereafter.

B. Hypotheses 1 and 1A: Black Applicants Receive Lower Response Rates

Column 1 in table 6 confirms hypotheses 1 and 1A that landlords, on average and without a signal of quality, are more likely to respond to applicants with white-sounding names than applicants with African American–sounding names. The statistically significant coefficient on black of $-0.093$ confirms previous findings of discrimination against African Americans or persons with African American–sounding names. Combined with the intercept estimate of 0.581, applicants with African American–sounding names receive 16% fewer positive responses.

| Variable                        | Mean   | SD    | Minimum | Maximum |
|---------------------------------|--------|-------|---------|---------|
| Sent on weekend                 | 0.272  | 0.445 | 0       | 1       |
| Monthly rent                    | 905.50 | 323.68| 350.00  | 2,000.00|
| Negative information            | 0.278  | 0.448 | 0       | 1       |
| Baseline treatment              | 0.297  | 0.457 | 0       | 1       |
| Positive information            | 0.425  | 0.494 | 0       | 1       |
| Male                            | 0.496  | 0.500 | 0       | 1       |
| Black                           | 0.498  | 0.500 | 0       | 1       |
| % male in neighborhood          | 0.497  | 0.041 | 0.25    | 1       |
| % black in neighborhood         | 0.124  | 0.162 | 0       | 0.984   |
| Response                        | 0.648  | 0.478 | 0       | 1       |
| Positive response               | 0.463  | 0.499 | 0       | 1       |

See definitions of monthly rent, % blacks in neighborhood, and neighborhood in the notes to table 2. “Black” and “male” refer to applicants with African American– and male-sounding names, respectively. Baseline treatment refers to e-mail text containing no information about credit rating, smoking, or occupation of an applicant. Negative treatment adds negative information about bad credit rating and smoking behavior to the baseline e-mail text. Positive treatment adds positive information about respectable occupation and nonsmoking behavior to the baseline e-mail text. Neighborhood demographic characteristics are sourced from Census 2000. Response indicates whether a landlord responded, and a positive response indicates whether a landlord responded positively to the inquiry. Positive response includes “available” and “available + if.” See the online supplementary materials for response categories. Number of observations for all variables $= 14,237$. 
C. Hypotheses 2 and 2A: Positive Information versus Negative Information

Column 2 in table 6 presents the DiD estimates for equation (18). It shows that the estimated effect of the negative treatment on black relative to white is significantly positive (0.042). It is consistent with hypothesis 2 that on average, $g_W > g_B$, so signals from white applicants receive relatively more weight in an average landlord’s estimate of quality. The estimates also fail to reject hypothesis 2A that prejudice against black applicants can generate a larger response gap between white and black applicants as the signal switches from negative to positive.

D. Hypotheses 3 and 3A: The Effects of Surprising Signals

If statistical discrimination can explain differential outcomes for the hypothesized relative sign of the information-weighting parameter, then the surprising negative signal will close the racial gap in column 1 in table 6. Conversely, surprising positive information will not necessarily do so (hypothesis 3). If prejudice instead explains differential outcomes, then the racial gap will amplify with positive information and will close with negative information (hypothesis 3A).

In column 3 in table 6, the statistically negative coefficient on negative information and the statistically positive coefficient on Negative Information $\times$ Black are consistent with both hypotheses 3 and 3A. Column 3 shows that disclosing negative information about an applicant’s quality leads to a 50% greater reduction in a white applicant’s probability of receiving a positive response.

Column 3 also shows that the marginal return to signaling a respectable occupation and nonsmoking behavior increases the positive response rate by 6.7% (0.039/0.581).
for whites. The coefficient on the interaction term Black × Positive Information is, however, statistically indistinguishable from 0. Both applicant types benefit from the inclusion of positive information, but the information does not widen or narrow the racial gap observed in the base case. This result is consistent with hypothesis 3 rather than hypothesis 3A, and it conforms to the statistical discrimination model’s prediction that \( g_W > g_B \). Although the positive information is much greater than the mean signal for black applicants, the relationship \( g_W > g_B \) attenuates any improvement in signal. Taste-based discrimination cannot generate this finding as it predicts that a positive surprise will widen the racial gap unambiguously.

E. Hypothesis 4 and 4A: Differences by Information Types across Neighborhoods

The final prediction states that the difference \( g_W - g_B \) is negatively related to the share of blacks (%Black) in a rental property’s neighborhood. An increase in this fraction equalizes the weighting parameters for black and white applicants. If prejudice instead explains the observed racial gap, then as the share of black residents and landlords increases, taste-based discrimination will lead to a shrinking racial gap in the baseline.

Column 4 in table 6 presents evidence that differential outcomes by race vary with the racial composition of an apartment’s neighborhood in a manner consistent with statistical discrimination. The positive coefficient on the interaction term Positive Information × Black × %Black indicates that as the share of black residents in a neighborhood increases, the surprising positive signal becomes more effective in closing the racial gap in positive response rates between white and black applicants. The insignificant positive coefficient on the interaction term Negative Information × Black × %Black indicates that in a predominantly black neighborhood, the negative signal does not decrease the racial gap between black and white applicants significantly when compared with the baseline treatment case. Thus, in neighborhoods in which residents are predominantly black, a surprising positive signal leads to significantly greater improvements in positive response rates. Although landlords in such a neighborhood are more likely to be black, it does not require that the landlord is black, only that the landlord is more familiar with black applicants. Our estimates confirm the key hypothesis 4: landlords’ relative past experiences with different racial groups shape their information parameters.

In contrast, column 4 in table 6 also reveals that the racial gap between white and black applicants in the base case does not vary across different neighborhoods. Furthermore, the share of black residents increases, surprising negative information does not hurt black applicants more than white applicants, as indicated by the positive coefficient of Negative Information × Black × %Black. Our results contradict the predictions of taste-based discrimination.

Finally, the results in columns 2 to 4 in table 6 show that positive information significantly increases the likelihood that applicants with African American–sounding names receive positive responses, ruling out the presence of lexico-
graphic search posited in Bertrand and Mullainathan (2004). Overall, the DiD specifications and the significant coefficient on Positive Information × Black × %Black produce different conclusions about statistical discrimination than those of previous studies. With the significant treatment effect of positive information, our estimator can identify differences in \( \gamma \) across race and illustrates that the lack of experience with a particular race influences the behavior of agents.

V. Robustness

The results are robust to several alternative specifications. First, our findings do not contradict the theoretical assumption of race-invariant quality variance or support the competing taste-based discrimination explanation. Second, we assume that landlords exhibit in-group favoritism and out-group animosity in the taste-based discrimination model. If black landlords are instead prejudiced against black applicants, then as the share of black residents and landlords increases, the surprising positive signal should unambiguously widen the racial gap favoring white applicants. Column 4 in table 6 rejects this possibility.

Landlord risk aversion presents an ambiguity that could limit our ability to rule it out as the main source of the results. We exploit differences in neighborhood racial composition to separate statistical and taste-based explanations, so racial composition must not be correlated with landlord risk aversion. Because risk aversion and wealth are highly correlated (Paravisini, Rappoport, & Ravina, 2010), neighborhood racial composition must not vary with property value. The high correlation (0.544) between average rent and property values of one-bedroom and studio nonfarm units in the American Community Survey 2009 justifies rent as a proxy for value. The correlation between the share of black residents and the rent of the apartment is insignificant. In unreported estimates, we also find that the inclusion of both apartment rent (relative to the neighborhood average) and its polynomial have no significant effects on our estimates. We conclude that risk aversion is not the main driver of our results.

Table 7 presents results using other definitions of positive responses. The estimates in columns 1 to 3 show that the estimated coefficients vary little across all three measures of positive responses. The results are also robust to use of names. Column 4 presents the estimation results of equation (20) without less common first names. All earlier conclusions about the testable implications remain.

34 The results are also robust to the exclusion of four Muslim-sounding first names: Hakim, Jamal, Kareem, and Rasheed.
Finally, the names chosen in our study could convey an applicant’s social background beyond race. We follow Bertrand and Mullainathan’s (2004) approach to examine whether average positive response rates are correlated with the social background of each name within each race-gender group, using the fraction of mothers of babies born with the names who have at least a high school diploma as a proxy. The within race-gender rank-order correlation test shows no evidence that positive response rate and social background are positively related (table 8).

VI. Conclusion

Statistical discrimination can explain the differential outcomes in rental apartment inquiry screening by race. We detail a model of statistical discrimination that provides testable implications about a parameter that connects signals, expectations, and race and contrast the model’s predictions with those of an alternative, taste-based model. We show some overlap between the implications of taste-based and statistical discrimination and illustrate that lower marginal returns to signal or credentials for black applicants than for white applicants is consistent with both statistical and taste-based discrimination.

When no information other than the race of an applicant is revealed to landlords, applicants with African American-sounding names receive 9.3 percentage points fewer positive responses than applicants with white-sounding names. The lack of a differential response to positive information casts doubt on taste-based discrimination as the dominant source of differential treatment. Landlord response rates across neighborhood racial compositions conform to the statistical discrimination model, in which agents use experience to predict applicant quality by race. Racial prejudice or lexicographic search alone cannot explain these results. The findings provide justification for policies aiming to promote clear information dissemination and to improve communication between different racial groups, as well as for social programs designed to eliminate inequality across racial groups.

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APPENDIX A

Derivation of the Expected Value of Sample Variance of Signal

Equation (7) states that the denominator of $\gamma_i$ in a sample of landlords is $(1/n_j) \sum \text{var}(e_{rk})$, where, for a particular landlord, the sample variance of signal for racial group $r$ is $\text{var}(e_{rk})$. In a large sample of landlords, the mean of the sample variance of signal is

\[
E[\text{var}(x_k)] = \text{var}(0) - \sum_{i} \sum_{j} \text{cov}(\theta_i, \theta_j) + \text{var}(\varepsilon) - \sum_{i} \sum_{j} \text{cov}(\varepsilon_{rk}, \varepsilon_{rl})
\]

$\text{cov}(\theta_i, \theta_j)$ is the pair-wise covariance of the noise of individual tenant $i$ and $j$ for all $i \neq j$; $\text{cov}(\varepsilon_{rk}, \varepsilon_{rl})$ is the pair-wise covariance of the noise of the signal between the individual tenant $k$ and $l$ for all $k \neq l$ in racial group $r$. If individuals are mutually independent, then $\text{cov}(\varepsilon_{rk}, \varepsilon_{rl}) = 0$. Neighborhood sorting means, however, that landlords are likely to meet similar individuals in neighborhoods in which they own properties and $\text{cov}(\varepsilon_{rk}, \varepsilon_{rl})$ is not 0. $\sum_{i} \sum_{j} \text{cov}(\varepsilon_{rk}, \varepsilon_{rl})$ is positive and large for $r$ if $r$ is the neighborhood majority, as there are more covariance terms. Thus, the majority group of a neighborhood will have smaller $E[\text{var}(x_k)]$. Whether $\text{var}(\varepsilon)$ is small, large, or constant across $r$ is not really crucial to the relationship between neighborhood sorting, majority group, and the information-weighting parameters.