

**Racial and Ethnic Biases in Rental Housing:  
An Audit Study of Online Apartment Listings**

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**ABSTRACT**

As rental markets move online, techniques to assess racial/ethnic rental housing discrimination should keep pace. We demonstrate an audit method for assessing discrimination in Toronto's online rental market. As a multicultural city with less segregation and more diverse visible minorities than most U.S. cities, Toronto lends itself multi-name audit studies. We sent 5,620 fictitious email inquiries to landlords offering apartments on *Craigslist*, a popular Internet classifieds service. Each landlord received one inquiry each from five racialized groups—Caucasian, Black, E/SE Asian, Muslim/Arabic, and Jewish. In our experiments, “opportunity denying” discrimination (exclusion through non-response) was ten times as common as “opportunity diminishing” discrimination (e.g., additional rental conditions). We estimate Muslim/Arabic-racialized men face the greatest resistance, with discrimination occurring in 12 percent of experiments. The level of discrimination is modest but significant for Asian men (7 percent), Blacks (5 percent), and Muslim/Arabic women (5 percent). Discrimination was evenly spread throughout the city.

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**INTRODUCTION**

Residential segregation by race and ethnicity is a prominent feature of North American cities (Ross and Turner 2006, Fischer and Massey 2004). Previous research indicates that discriminatory practices in the housing market is a significant factor in this process (Dion 2001; Yinger 1995). Studying this process has been a longstanding methodological challenge, resulting in the deployment of careful, time consuming, and often expensive audit studies such as the U.S. Housing Discrimination Study. Yet, the housing market is rapidly moving online, thereby prompting two questions: First, are there changes in the quantity or quality of discrimination in the housing market as a consequence of this shift? Second, can audit studies be reliably adapted to this emerging and potentially dominant medium?

A key motivating concern in this work is whether the face of discrimination has changed in light of both new opportunities for finding rental housing and new concerns about overt racism. As Ross and Turner point out in their analysis of the changes between the 1989 and 2000 Housing Discrimination Studies (both in-person national audit studies), Blacks and Hispanics in America are still subject to discriminatory practices. However, this discrimination has moved towards steering and discriminatory financing and away from overt rejection and expressions of racism. Consequently, it is fair to ask whether landlords are becoming less racist or whether such racism in the

housing market is being kept quiet, and expressed passively. If it is the latter, then in-person audit studies may not pick up on passive racism. A second concern is whether socioeconomic status is replacing any given racialized category as a target of discrimination. Indeed, changes in the HDS from 1989 to 2000 suggest this might be the case (c.f., Ross and Turner 2005, Zhao, Ondrich and Yinger 2006). This is reinforced by work from Bertrand and Mullainathan (2004) and Carpusor and Loges (2006) who suggested discrimination based on class markers in other markets.

With the rise of the now highly popular online housing market (Kroft and Pope 2008), there is the opportunity for such passive racism to manifest itself in a new manner – by simply ignoring requests from visible minority groups. This online housing market primarily involves asynchronous communication (via web postings and email messages). This means landlords can reply to renters on their own time, and reply selectively to desirable tenants. With overt rejection or deception, a renter can raise a complaint. But ignoring email allows the landlord ‘plausible deniability’ while still maintaining a desirable clientele.

Online audit studies are emerging as a sensible way to address the need for research into online housing discrimination (Carpusor and Loges 2008, Ahmed, Andersson and Hammarstedt 2010). In Sweden, Ahmed and Hammarstedt (2008) found that a Muslim/Arabic name was almost half as likely to receive a response as the traditional Swedish name. Like this earlier work, we employ randomized names. However, building on this work we employ fixed effects models as used in past work on the HDS (c.f., Zhao, Ondrich and Yinger, 2006). Yet, we build on this earlier work in several ways – first, by focusing on Toronto, Canada rather than in America, we can

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provide a comparative baseline for an area where Blacks and Hispanics are not the largest visible minority populations. Second, by using a computer-assisted technique we show how to scale up the size of experiments while simultaneously fine-tuning a host of subtle methodological considerations to increase validity (such as ensuring semi-regular timing for stimuli). Third, by randomly assigning message texts that mark some tenants as desirable, we can test for independent effects of employment as well as interactions with certain racialized categories. Finally, we reframe the discussion of online housing discrimination in terms of “opportunity diminishing” and “opportunity denying” treatment, a concept previously considered in terms of hiring discrimination (Turner, Fix and Struyk, 1991). Through a focus on these two notions we can assess the prevalence of discrimination at first contact in the housing market as well as posit a difference in the act of discrimination between online asynchronous interaction (via email) and synchronous interactions (such as face-to-face meetings or telephone calls).

To conduct this study, we chose Toronto for both practical and theoretical reasons. With a population that is over 40 percent foreign born, Toronto is one of North America’s most ethnically diverse metropolitan regions (Statistics Canada 2008). This multiculturalism is an important aspect of the city’s culture, as evinced by Toronto’s adopted motto, “Diversity, our Strength”. However, multiculturalism is not uniformly welcomed in the housing market. Reviewing 21 unrelated quantitative studies conducted from 1957 to 1996, Novac et al., (2002) found that racial and ethnic discrimination in Canada is a continuing problem for some groups, especially in the private rental housing sector. More recently, the Ontario Human Rights Commission (2005) found that the denial of opportunities to apply for rental housing or to view properties remains a

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common complaint of racialized persons in urban Canada.<sup>1</sup> To deny opportunity, landlords can use subtle screening methods to bypass certain individuals in the tenant selection process. For example, persons from one racialized group may be advised that an apartment has already been rented only to have a friend from another group be told that it is still available. There are several human rights cases in Ontario that have dealt with racial discrimination in rental housing.<sup>2</sup> These cases also show “opportunity diminishing” treatment at the contact stage: during a meeting or viewing the landlord projects discomfort, reluctance or disinterest to a potential tenant from one racialized group only to show warmth and interest to someone from another racialized group.

Compared to research in the U.S., there are few rigorous studies of housing discrimination in Canada (Novac et al. 2002: 21). A study commissioned by the Canadian Mortgage and Housing Corporation (CMHC) recently reported that “without more rigorous research, housing discrimination will remain an unquantified social problem in Canada” (Novac et al. 2002: 3). Limitations in the scale and methodology of the various studies make it difficult to generalize findings, but most suggest that discrimination remains a significant problem. Acknowledging the value of this prior work, several scholars have argued for more rigorous attempts to measure housing discrimination in Canada (Engeland et al. 2005; Novac et al. 2002; Dion 2001). Yet, this is exacerbated by the fact that the rental housing market is rapidly evolving, as landlords and tenants increasingly seek each other online rather than through newspapers. Accordingly, this study seeks to contribute both to the particular issue of quantifying housing discrimination in Toronto as well the general issue of how to adequately measure discrimination in the rapidly expanding online housing market.

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### CURRENT STUDY

In this study we use an email-based audit methodology to directly assess initial racial and ethnic discrimination in the rental housing market rather than inferring housing discrimination indirectly from surveys or complaint data. The emergence of internet apartment listings as a mainstream form of rental housing advertising makes it possible to field a systematic audit based on responses to email where the apartment seeker's names are randomly varied. Over the past several years, free listing services such as *Craigslist* ([www.Craigslist.org](http://www.Craigslist.org)) have become popular websites to post rental ads. The ads contain a description of the apartment and a means for prospective tenants to contact landlords. *Craigslist* allows landlords to hide their email address (they are given an ad-specific address that forwards to their real email address). Also, unlike classified newspaper ads, *Craigslist* gives the landlord substantial space to discuss the rental unit as well as post pictures, all free of charge. Postings on *Craigslist* are not vetted by staff.<sup>3</sup>

To infer discrimination, we examine differences in response patterns to a set of five inquiries that are roughly similar in appearance but are signed using racially encoded names. We randomly varied other aspects of the appearance of the five inquiries (such as timing, order and content). By controlling for these variations in regression models we are able to isolate the consequence of inquiring about an apartment using racially encoded names.

We hypothesize that the measured levels of net housing discrimination based on the ethnic name association will be statistically significant and reflect the prevalence of anti-ethnic sentiments expressed in recent surveys conducted in Canada (Murdie 2002, 2003; Novac et al. 2002). That is, names associated with Blacks will receive fewer

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responses than those from Caucasian and Jewish names. Additionally, given recent increases in reports of increased discrimination against persons identified as, or perceived to be Muslim, Arab and South Asian (or what is sometimes called “Islamophobia” in the mainstream media), we further hypothesize that apartment seekers with Muslim/Arabic names will experience high levels of discrimination relative to the other test groups. This is reinforced by a previous online audit study (Ahmed, Andersson and Hammarstedt 2010) showing significant discrimination against Muslim/Arabic names relative to the majority population. Furthermore, building on the insights of that earlier study, we do not expect to find a variation in discriminatory practices based on message content.

Since this study takes place through email, we further hypothesize that this medium will foster substantially more “opportunity denying” treatment, or exclusion through non-response, than “opportunity diminishing” treatment, or discrimination through additional unwarranted conditions. This is because email is an asynchronous medium (Hogan 2010; Whittaker and Sidner 1996). By contrast, the telephone and face-to-face are both synchronous, meaning that the prospective tenant and the landlord reciprocally engage in a single event (i.e., a conversation). To reject a tenant outright in person may cause discomfort or breach norms of politeness. Rejection is an active process of saying “no”. However, it is still possible to make an undesired tenant feel uncomfortable and give subtle cues of the tenant’s undesirability (thereby signaling opportunity diminishing, c.f., Turner, Fix and Struyk 1991). Work with the 2000 U.S. Housing Discrimination Study has demonstrated a high degree of what could be considered opportunity diminishing treatment to in-person auditors through steering practices and worse financing rates, rather than outright denying (Ross and Turner 2006).

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They note this as a change from outright rejection or deception shown to the Black auditors in the 1989 HDS. However, further analysis has suggested that overall rates of discrimination in person, via email and by telephone may be comparable in many contexts (Ross and Galster 2007). While this study does not compare across media, it does give a clear baseline for email in this context.

Our experiment gauges racialized discrimination during only the “first contact” stage of the apartment seeking process. Housing discrimination is a cumulative process, where prospective tenants are screened, called back, differentially surveilled, asked for credit checks and so forth. We believe our approach is a reliable means for assessing initial discrimination. However, a more complete study of rental housing discrimination would need to follow-up by evaluating interactions with landlords after this first stage.

### **THE AUDITS STUDY AS A MEASURE OF HOUSING DISCRIMINATION**

Discrimination against minorities in housing has generally been measured in one of two ways: complaints and audits (Ross and Turner 2006, Fischer and Massey 2004).

Government agencies have often reported rates of discrimination based on filed complaints. Unfortunately, such complaints are filtered through a formal reporting process (Lucas 1994; Fischer and Massey 2004). Consequently, formal complaints may represent only a small proportion of incidents of housing discrimination (Fisher and Massey 2004). Complaints also only refer to incidents that people perceive to be discriminatory.

The limitations of complaint-based data for assessing housing discrimination have spurred social scientists to field audit studies. In the U.S., Seltiz (1955) conducted an

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early audit study to quantify racial discrimination of Black clients in New York City restaurants, sending both Black and White auditors to a restaurant to observe relative treatment. The audit technique has been commonly used in the U.S. in connection with federal Fair Housing laws (see Riach and Rich 2002 for a comprehensive review). In the 1970s and 1980s, the U.S. Department of Housing and Urban Development funded two large housing audits that uncovered significant levels of discrimination of racialized persons in several major U.S. cities (Wienk et al., 1979; Yinger 1998). For instance, Black and Hispanic auditors posing as prospective apartment tenants were shown twenty-five percent fewer units than White auditors with comparable income qualifications (Yinger 1998). Housing audit studies were also fielded in Europe in the 1960s and 1970s, with the work of Daniel (1968), Jowell and Prescott-Clarke (1970) and Bovenkerk et al. (1979). See Dion 2001, Galster 1992, and Riach and Rich 2002, for expanded reviews of audit studies.

Audit studies represent a quasi-experimental research design (Campbell and Stanley 1966). They offer researchers more control and greater internal validity than other designs commonly used in the social sciences. For these reasons, data from audit studies are generally accepted as providing strong evidence of racial discrimination (Fix and Turner 1998), notably by U.S. courts (see Metcalf, 1988; Massey and Lundy 2001). Among the methods for detecting discrimination, audit methodologies are generally considered the most rigorous and robust approach for quantifying housing discrimination (Novac et al. 2002). The most common form of audit study has used face-to-face encounters. Other forms do exist. In the housing market, Fischer and Massey's (2004) innovative study using a phone based auditing methodology is one recent exception. In

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the Labor market, Bertrand and Mullainathan (2004) used resumes to field an audit study using only names. In more recent years, social scientists have looked to the Internet for the next generation of audit studies (Ahmed, Andersson and Hammarstedt 2010, Carpusor and Loges 2006).

Despite their methodological power, audit methods are not without problems: First, audit studies that rely on face-to-face interaction have been criticized because it may be impossible to erase the numerous differences between the actors in the pair (c.f., Ross and Turner 2005). Second, personal interviews are expensive to field. Third, in light of the increasingly diverse urban populations, audit studies have been criticized for limiting the test groups to White, Black, and Hispanics rather than a broader range of ethnic groups. As detailed below, we believe our method can meet these three challenges with newfound precision, efficiency and breadth.

Finally, because an audit study is a field experiment and necessarily involves deception, it is worth addressing ethical considerations. Drawing upon Riach and Rich's (2004) review of the field, we note several important qualities of these studies: First, regarding deception, there is minimal harm to a respondent in that an audit study is conducted in the normal course of business. This is in contrast to laboratory experiments that often represent a unique and one-off experience. Second, participants are never identified in the course of the experiment, nor is there any follow-up contact seeking to verify the subject's identity. Third, any given subject can use the recourse of plausible deniability for a specific instance of discrimination. Fourth, dishonesty is already endemic in the markets in which audit studies take place (Riach and Rich 2004:460-462).<sup>4</sup>

## DATA AND METHODS

### RESEARCH SETTING

Because of its recognition as a leading source of online rental advertisements, we selected advertisements from *Craigslist* as our population for this study. The apartment listings on the Toronto section of *Craigslist* receives approximately thirty to fifty new listings everyday, far more than listed in *The Toronto Star*, the leading daily newspaper in the city. In other similar sized markets (in terms of usage and population) Craigslist has been shown to significantly affect the volume of classified listings in traditional media and increase the allocation of rental units (Kroft and Pope 2008).

Virtually every listing on *Craigslist* includes the number of bedrooms, the price, a preference for cats and dogs, time of posting and either the exact address or the nearest intersection. We make use of these variables in later analyses. In most cases, further scrutiny allowed us to also distinguish commercial from private landlords.

Between late March and early June 2007, we conducted experiments on approximately 10 listings per day. The last response to come back considered valid was from the 28th of June 2007, one week after the last set of experimental inquiries were sent out. Power calculations suggested that a sample  $n=5,000$  or more audits (or 1,000 experiments of 5 inquiries each) would be sufficient to detect statistical differences in response rates. We conducted 1,124 experiments of 5 inquiries each.

### THE EXPERIMENT

The experiments to assess racial discrimination include three specific components that we had to design and include in an automated software program: appropriate names, appropriate message text and a logic for sending out emails in a systematically varied and

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timely manner.

*Names:* Our audit methodology relies on racialized names and not real actors, ensuring comparability across race in all other factors. That is, we rely on the landlord's mental representation of these groups rather than relying on actual face-to-face encounters. The use of racialized names rather than actors has recently been applied in labor market audit studies (Aura and Hess 2010; Bertrand and Mullainathan 2004). We selected five racialized groups to be a part of this study: Anglo-Saxon Caucasian, African-American Black<sup>5</sup>, East/Southeast Asian, Muslim/Arabic, and Jewish. The non-Caucasian groups were chosen because they have reported experiences of housing discrimination in the past, have ethnically distinctive names, and have a significant presence in the greater Toronto area. For each of the groups we selected ten names, five female names and five male names. These fifty racialized names were created using a "most popular name" search on the Internet for each ethnic category. The names are listed in Appendix B by response rates. A generic Gmail email address was created and used for each name.

Since we are merely using names and not actors, our ability to include ethnic groups is limited only by sample size and a need to minimize undue burden on the respondent by flooding their inbox. Based on previous use of *Craigslist* we believed that five emails, and hence one email for each racialized group, was optimal.

*Message text:* For each of the five messages that were sent out, we randomly varied both the order as well as the text of the message. The texts of the message were based on an earlier natural experiment (one of the authors rented his house using *Craigslist*). They range from very sophisticated language and implied employment to very terse language

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with spelling mistakes. This allows us to test for interactions between message type and name, in case certain styles reinforced or undermined a specific racialized stereotype. The message texts can be found in Appendix A.

*Message Sequencing & Timing:* In order to give fair weight to each of the five messages being sent, we had to consider three temporal issues: interval, time of day, and regularity. We opted for an interval of approximately thirty minutes, meaning all five messages were sent in a two and a half hour range – long enough to be seen as separate, but short enough to warrant a response. Each interval randomly varies from thirty minutes within a five-minute range. Also, we sent messages twenty-three hours after the time of posting, and no earlier than 9 a.m.

While these considerations sound tedious, they are important for ensuring naturalistic conditions. We consider this technique a success since no single landlord replied questioning the authenticity of the messages. To attend to these details, we developed a software application, *Craigmailer* to queue messages, randomly assign names and send messages out at the specified time.<sup>6</sup> To avoid duplicates, any new listing was automatically crosschecked by *Craigmailer* with previous landlord data.<sup>7</sup>

*Matching replies to audits:* Finally, in order to analyze email coming back to the auditors' addresses we used a series of techniques to link the reply to the original posting. Again, to ensure a naturalistic message, the posting ID was hidden inside a false but clickable URL at the bottom of the message (such as <http://www.google.com/?o=12345678>). If the reply contained the original message text, this would allow us to match IDs. This procedure allowed us to match 71 percent of landlord replies. For the remaining unmatched responses, we could triangulate based on

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the unique combination of names sent to a specific landlord (e.g., male Arabic name 3, Caucasian name 4 and E/SE Asian name 2 were only sent to one unmatched landlord). Using this process, we were able to match an additional 27.2 percent of the received mail with the appropriate sender. Of the remaining “unmatchable” messages, half of them appeared to be spam sent to two specific addresses and the other half were corrupted.

Our study, including our data collection procedures and safeguards of confidentiality (described below), were approved by the University of Toronto’s Research Ethics Board.

### QUALITATIVE CODING FOR OPPORTUNITY AND GEOGRAPHY

We define opportunity *diminishing* treatment as treatment that involves additional conditions or barriers to access. We define opportunity *denying* treatment as an act (or lack thereof) that inhibits further interaction between auditor and landlord. To distinguish diminishing treatment from a simple offer to view the apartment, we read each incoming message for signs of opportunity diminishing treatment. We used a four code scheme: Personal information (e.g. employment status), extra conditions (e.g., a need for a deposit that was not mentioned to others), fewer conditions (e.g., offering parking to one prospective tenant only), and told the apartment taken before other email was returned that was told it was available.

We also coded the geographic location of the listing using latitude and longitude lookup of rental information on Google maps, often spot checking with additional information from the listing. These coordinates were then geolocated in specific census tracts for comparison with Canadian census microdata.

## RESULTS

### DESCRIPTIVE RESULTS

#### Response Rates by Group and Other Variables

Table 1 reports the response rates by group, send order, message version, landlord type, and apartment characteristics. Response rates are reported separately for male and female inquiries. Overall, 72 percent of men and 75 percent of women receive a response. Muslim/Arabic male names have the lowest response rate, followed by Muslim/Arabic female names, Black names and E/SE Asian male names. These names are then followed by E/SE Asian female, Caucasian and Jewish names.

[Table 1 about here]

The first message sent has the lowest response rate. We believe this is because most email clients sort messages in reverse chronological order, with older messages (i.e. the first ones to be sent) at the bottom of the list. Among the message versions, only C had significantly different (lower) response rate, although the differences are modest. We attribute this difference to the brevity of message C.

Private landlords are less likely to respond than commercial landlords. This is somewhat expected given that commercial renting agencies pledge to follow a code of conduct about responding to all inquiries. Prior research also suggests that private landlords are more likely to discriminate than commercial rental agents (Ahmed and Hammarstedt 2008, OHRC 2007, Carpusor and Loges 2006). Analysis below helps determine if differences in response rates account for these private-commercial distinctions. Eleven percent of landlords could not be classified as private or commercial based on the limited information available from the experiment.

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Inquiries about higher rent dwellings were more likely to receive a response (e.g. \$1,350 or more) than inquiries for less expensive dwellings (<\$750). Inquiries about ads where rent was not specified received the lowest response, perhaps suggesting those landlords were less serious about renting their apartments if they did not provide complete information.

The most common dwelling type was 1-bedroom (41 percent), followed by 2-bedroom (25 percent). Response rates are generally consistent for the most common dwelling type (73-75 percent for men; 73-81 percent for women). As with rental rate, when the dwelling type is not specified, the response rate is lower.

### Response Patterns within Experiments

We report statistics for two basic types of unequal treatment within the 1,124 experiments. First, landlords may simply respond to some respondents but not others. This is unequal treatment through exclusion, the main form of “opportunity denying” discrimination. Second, landlords may respond to all inquiries, but put certain conditions or make requests of some individuals but not others. This is unequal treatment through response text, and is “opportunity diminishing” discrimination. Unequal treatment by exclusion (denying) and text response (diminishing) can occur in the same experiment, so they are not mutually exclusive. Out of 1,124 total experiments, most had no apparent bias of either type (51 percent) or received no response at all (16 percent). Of the 33 percent of experiments with some unequal treatment, opportunity denying was much more common than opportunity diminishing. That is, unequal treatment by exclusion is ten times more common than unequal treatment through text response (30+1 percent vs. 2+1 percent).

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Table 2 reports opportunity diminishing response patterns. Textual analysis of email responses found only 36 experiments with clear opportunity diminishing treatment. Of these, 14 asked personal information that was not requested of the other inquirers (employment information, location of current residence, presence of young children), 2 added extra conditions to the rental not mentioned to the others (security deposit and “must sign yearly lease”), 4 offered fewer conditions or made concessions that were not made to the others (flexible start date, viewing anytime, and free parking), and 20 told one respondent that the apartment was taken *before* telling another respondent it was available. In this case, it is considered diminishing rather than denying because any contact (even a rejection) could lead to future negotiation or a follow-up. Although there are only a few cases of unequal treatment from the textual analysis, the unequal negative treatment was disproportionately directed at Black and Muslim/Arabic respondents, while the few cases of imposing fewer conditions was directed at Caucasian and Jewish names.<sup>8</sup>

[Table 2 about here]

Table 3 focuses on the opportunity denying response patterns within-experiments. Among the 84 percent (n=949) of experiments with at least one response to the five inquiries, 64 percent are labeled as having an “egalitarian” response pattern (n=603) because all inquiries received responses. In 36 percent (n=346) of these experiments, the response pattern is unequal because one or more group was excluded.

[Table 3 about here]

What groups are mostly likely to be excluded in these unequal patterns? Focusing on the overall results in the first two columns, the most common excluded groups are

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Muslim/Arabic (6.8 percent) followed by Black (3.7 percent) and E/SE Asian (3.2 percent). These percentages are significantly higher than would be expected by chance alone. That is, if we compare the observed patterns to what is expected if the probability of exclusion was the same for each of the 31 possible combinations ( $2^5 = 32$  combinations minus the “none” category, each occurring  $0.36/31 = 1.12$  percent of the time), then the first 7 excluded group combinations are more likely than by chance alone (that is, Muslim/Arabic only, Black only, E/SE Asian only, Caucasian only, Jewish only, Black & Muslim/Arabic, Muslim/Arabic & S/SE Asian all are greater than 1.12 percent).

For both the male and female experiments (columns 3-6), the most commonly excluded names are Muslim/Arabic (7.5 percent and 6.3 percent, respectively). Black names were more often excluded in the female than male experiments (4.8 percent vs. 2.6 percent, respectively). One possible reason for this is the Black female names are more Afro-centric than the Black male names (i.e., Tanisha, Latoya, Shaunika, Lakisha, Ashanti vs. Leroy, Delroy, Jerome, Jamal, and Tyrone). Aside from these major differences, it is difficult to make finer distinctions between the remaining excluded group combinations using these descriptive results. The fixed-effects models presented below are more instructive for distinguishing patterns of unequal treatment.

### FIXED-EFFECTS RESULTS

We estimate fixed effect logistic models to isolate the extent of unequal treatment for the different racialized groups. The fixed-effects logistic model is preferred to the standard logistic model because it can adjust for any unmeasured factors that influence the responses to the inquiries within a given experiment (e.g. a landlord’s friendliness, selectivity, time-pressure, or values of responding to everyone). Fixed-effects models also

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have the benefit of removing systematic biases that may exist in measurements of apartment and landlord characteristics (e.g. private vs. commercial, rental rate, dwelling type). Unless we control for these unmeasured factors associated with an experiment's pattern of responses, estimates of racialized group preferences may be distorted. The fixed-effect logistic model is  $\text{Logit}(p_{ic}) = x_{ic}\beta + \alpha_i + u_{ic}$ , where  $\text{Logit}(p_{ic})$  is the log-odds of receiving a response for individual  $i$  in case (i.e., experiment)  $c$ ,  $\beta$  is the vector of coefficients,  $x_{ic}$  is a vector of regressors with variation within cases,  $\alpha_i$  is the individual effect and  $u_{ic}$  is the error term.

Table 4 reports the fixed-effects logistic estimates.<sup>9</sup> Separate models were estimated for all experiment, male experiments, and female experiment samples. Statistically significant results with  $p < 0.05$  are noted. Because coefficients can be estimated only for covariates with within-experiment variation, all variables but racialized group, message version, and message order drop out. Also, only experiments with variation in the outcome are used in estimation (e.g. for the full sample model, 775 groups were dropped due to all positive or negative outcomes).

[Table 4 about here]

In the overall model, the reported odds-ratios indicate significantly lower odds of Muslim/Arab (0.34), Black (0.57), and E/SE Asian (0.66) names of receiving a response than Caucasian names (the reference category). For example, this is interpreted as follows—in the experiments where there is variation in responses, the odds of a Muslim/Arabic receiving a response are one third as likely as the odds of a Caucasian receiving a response. Likewise, the odds of a Black name-racialized auditor receiving a response are 43 percent less than the odds of a Caucasian name receiving a response.

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For the male and female models, the strength of the group findings varies. The results for Muslim/Arabic men are especially strong and significant (0.21,  $p < 0.001$ ) relative to those for Muslim/Arabic women (0.60,  $p = 0.054$ ). In the male model, the odds of a E/SE Asian male receiving a response (0.44,  $p = .002$ ) are significantly less than the odds for Caucasians or Jewish males, but in the female model, E/SE Asian female names are not significantly different from Caucasian females in the odds of receiving a response (1.04,  $p = 0.901$ ). At this point, we can only speculate about the reasons for such gender differences for E/SE Asian auditors. One possible answer is that there are much stronger local prejudices against E/SE Asian males relative to E/SE Asian females in terms of perceived desirability. The perceived desirability and openness to E/SE Asian females may mirror the greater cross-group friendship and intermarriage rates among E/SE Asian women than men in North America (Joyner and Kao 2005; U. S. Census Bureau 2003).

In separate models, we tested whether there were interaction effects between specific racialized names and specific message text. For example, would Jewish auditor be particularly likely to receive a house if their message sounded affluent, or would a Black auditor have discrimination mitigated (or exacerbated) by a certain message body? There were no significant interactions across a series of models accounting for all two-variable variations. This suggests that either the message texts were too homogenous, or that message text was not salient for landlord evaluations. This conforms to Ahmed, Andersson and Hammarstedt's (2010) work suggesting additional information from message text did not effect racialized discrimination in the online housing market.

Overall, the fixed-effects logistic results suggest that ethnic biases in online rental

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housing markets disadvantage Muslim/Arabic names the most, especially male ones, relative to Caucasian names. There are also substantial differences in responses from landlords to Black male and female names, and E/SE Asian men, relative to Caucasian names. Despite some hypothesized concerns about anti-Semitism, we found no significant differences in response rates to Jewish names relative to Caucasian names.

Paralleling the descriptive results in Table 1, message version C received significantly fewer responses than version A (the reference group). Possible reasons for this include the informal and unspecific nature of the inquiry and the unconventional name introduction of version C. The other covariate, message order, clearly shows that the second inquiry is more likely to receive a response than the first. As speculated above, this is likely because people view and respond to mail in reverse chronological order, making the first email in the queue the last one to be seen.

### DERIVED ESTIMATES OF RENTAL HOUSING DISCRIMINATION

We used the distribution of opportunity denying and diminishing responses along with the fixed effects odds ratios in Table 4 to estimate the likelihood of rental housing discrimination for a hypothetical population of tenants. Based on the combined results, Table 5 suggests that men with Muslim/Arabic names are most discriminated against (12 times out of 100), followed by E/SE Asian men (7 times out of 100), followed by Black men and women (5 times out of 100) and Muslim/Arabic women (5 times out of 100). It is important to note that these estimates reflect only the relative discrimination at the initial inquiry stage of securing rental housing. A complete measure of discrimination would reflect opportunity denying and diminishing at all stages of the apartment seeking process. Actual rates may be higher or lower depending on how landlords respond to

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subsequent face-to-face meetings. In some cases, follow-up personal interviews may exacerbate prejudices toward racialized groups, but in other cases they may diffuse such feelings. Also, we used racialized first and last names. Many members of racialized groups have Anglocentric first names combined with racialized last names. Further research is needed to examine different name combinations.

The overall estimates in Table 5 can be interpreted in both a positive and negative light. First, the estimated 5 to 12 percent chance of being discriminated against appear low compared to audit studies and survey based reports of perceived discrimination in housing (see Novac et al. 2002 and Reed 1991 for extensive reviews). For example, Fischer and Massey's (2004) study in Philadelphia found a 7 to 11 percent difference in gaining access between callers with middle class versus Black accented speech patterns, and they found a 24 to 29 percent difference in gaining access between callers using middle class versus Black English Vernacular. Garon's (1988) face-to-face audit study in Montreal found that 33 percent of Haitians and 16 percent of black Anglophone testers experienced blatant discrimination compared to the paired white applicants. Nine or more times out of ten in a rental situation directly opposite a Caucasian-racialized individual making similar inquiries, our other racialized groups can expect to receive similar treatment in responses to email inquiries, Although initial inquiries are only one step to securing an apartment, the growing importance of web-based listings as a gateway suggest a possible movement to greater equity in the rental market. On the other hand, the overall estimates suggest a long way to go. The cumulative effect of a 5 to 12 percent chance of being discriminated may be distressing, reinforcing widely held views about which neighborhoods are acceptable and which are

off limits to prospective tenants belonging to certain groups.

[Table 5 about here]

## **THE INFLUENCE OF GEOGRAPHY**

### TORONTO'S MULTICULTURISM AND LOW SEGREGATION

In this section, we address the question of whether the found effects can be considered indicative of Toronto generally, or whether certain areas of the city either foster or inhibit opportunity denying treatment. Based on this work, we do not find evidence for noteworthy differences in discrimination based on geographic locale within the city.

While there is no 'ideal' city for the analysis of housing discrimination, the greater Toronto area has a more uniform distribution of ethnic diversity across neighborhoods than other large North American cities. We operationalize neighborhood as a "census tract", a geographically bounded area for census measurements that typically comprises between 1000 and 5000 individuals in major metropolitan areas. The Greater Toronto Area is comprised of approximately 995 census tracts. The experiments are drawn from 398 (40 percent) of these.

Data from the 2006 Census indicate that ethnic diversity is evenly spread in metropolitan Toronto (Statistics Canada 2008). Three-quarters of the census tracts where experiments were conducted have at least twenty percent visible minorities. Ten percent of census tracts where experiments were conducted have seventy-five percent visible minorities. Moreover, this does not refer to a high concentration of a single minority group, but a high incidence of many different groups. Only three percent of census tracts have greater than fifty percent concentration of a single ethnicity other than Caucasian (1.5 percent of the study tracts were greater than fifty percent Chinese and 1.7 percent

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were greater than fifty percent South Asian, reflecting the high concentrations of these groups in Toronto's suburbs of Markham and Brampton, respectively).

### TRACT-LEVEL EFFECTS

To explore effects by census tract, we attached tract-level demographic information to our experiments. Using simple independent samples t-tests, we compared the composition of tracts where there was any evidence of opportunity denying treatment (31 percent of experiments) and tracts where the landlords replied to all emails. The continuous variable was percentage of either visible minorities in the census tract, or percentage of individuals who were Black, E/Se Asian or Muslim/Arabic. The independent samples grouping variable is whether the experiment showed discriminatory treatment (either in general or treatment for any of the five racialized groups in particular).

These results are non-significant at the 0.05 level in most cases, suggesting that further models would not be fruitful. The significant cases appear anecdotal and do not undermine the overall story. For example, the landlords that discriminated against Caucasian and Jewish names have listings in areas with significantly higher visible minority populations (Caucasians: mean diff. = 0.04,  $p=0.03$ , Jewish: mean diff. =0.04,  $p=0.04$ ). The mean difference of 0.04 means that areas where Caucasian (and Jewish) names received a response had, on average, four percent less visible minorities. Given that both Jewish and Caucasian names received the most email replies, we posit that these groups received slightly more preferential treatment in homogeneous neighborhoods instead of 'reverse racism' in heterogeneous neighborhoods.

Tests of tract-level variation for specific ethnic groups showed only one significant finding. Areas with higher percentage of Blacks were significantly *more* likely to

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discriminate against Black racialized names (mean diff=-0.017,  $p<0.001$ ). The mean difference of -0.017 means that areas where Blacks received a response had almost two percent less Blacks living in that area. We suspect that rental owners are not as likely to be Black as the people in that specific census tract, and are concerned about having too many Black people in the neighborhood. We consider this to be a more likely explanation than the alternative that Black rental owners would be especially reluctant to rent to their own. There were no statistically significant relationships (even at the 0.1-level) for Muslim/Arabic or E/SE Asian names, either in terms of the presence of members of that specific ethnic group or relative to the presence of visible minorities in general. Furthermore, the inclusion of tract-level information into the earlier fixed-effects model did not provide any noteworthy differences. This work suggested that moving to a three-level multilevel framework (auditors nested in landlords nested in tracts) would not be germane. In short, the discrimination found in Toronto is reasonably evenly distributed throughout the city.

Because we do not know the visible minority status of the landowners, however, we can only speculate on an interpretation of the findings here. This is, and may always be, a limitation of online audit studies, as we believe follow-up contact has the capacity to undermine the study as well as provide undue harm to the subjects. Nevertheless, we encourage other researchers to consider alternate methodologies that can clarify the mechanism at work here.

## SPATIAL AUTOCORRELATION AND STUDY LIMITATIONS

Our research design emphasized the deployment of a consistent number of experiments per day until we reached our sample size target. We anticipated a three month study

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window, and were reluctant to be in the field much longer. As a consequence of this sampling strategy our data is not fit for spatial auto-correlation. There are many tracts with experiments adjacent to tracts with no experiments. Also, the number of experiments per tract varies substantially. Since spatial autocorrelation statistics, such as Moran's I, take the geographic unit as the unit of analysis (Moran 1950), our sample cannot give reliable results. We believe that this may be an inevitable challenge for audit studies of this nature. To get an appropriate number and distribution of cases would require a much larger survey window and a far more deliberate attention to geographic placement. Such a strategy would have its own drawbacks, such as increased chance of contamination of landlords or exposure by a landlord (i.e. posting on Craigslist to 'beware' of our emails).

### **CONCLUSION**

Online interaction enables individuals to more carefully deliberate on what they say, as well as more easily avoid certain groups (Joinson 2002, Walther 2007). We find this to clearly be the case in the rental housing market. Offline, individuals can try to make an undesirable tenant uncomfortable. Online they can simply ignore perceived undesirables, and in many cases they do. For this reason, a framework that considers discrimination as "opportunity denying" and "opportunity diminishing" is of increasing relevance. In the case of email, individuals are not given the same cues (either verbal or visual).

Discrimination, when it exists, may be therefore internalized by the victim as a 'tough housing market', rather than externalized as unfair tenant selection practices. This consequence is especially unfortunate given our findings that opportunity denying

treatment happens to the least advantaged—those looking for small inexpensive apartments from private landlords (Table 1).

Opportunity denying treatment is particularly concerning, since it robs the discriminated of the signals by which they can rationally assess their situation, while reinforcing the denial that such inequalities exist. Past work on the discourses of discrimination have shown that culpability is a key strategy in maintaining this denial. That is, discriminators will employ discourses that blame the victims, absolves themselves of responsibility, externalize discrimination or rationalize stereotypes (Tilbury and Colic-Peisker 2006). Such discrimination requires intellectual labor and can be emotionally draining. It is much less demanding to simply ignore a request.

Of course, it is possible that landlords did not respond out of fear that they were being surveilled, and maybe even becoming aware of the study over the course of reading their mail. We doubt this to be the case, since we receive no messages questioning the authenticity of the respondent. Furthermore, our fixed-effects models only consider landlords where there was unequal treatment, whereas a suspicious landlord might send a token reply to all auditors.

In light of this situation, it is important that methodologies for assessing inequalities in the housing market keep pace with the technologies used by landlords and tenants. This paper demonstrates that such advances are possible and practical. This is done through an extension of existing audit study practices via scripting technologies to enable carefully controlled field experiments. The results of our field experiments are modest, but significant and coherent. To summarize, our derived estimates (Table 5) suggest Muslim/Arabic-racialized men face the greatest resistance from landlords, with

## RACIAL BIASES IN ONLINE RENTAL LISTINGS

opportunity denying and diminishing treatment 12 percent of the time, net of all other variables. The level of discrimination is also significant for E/SE Asian men (7 percent), Black men and women (5 percent), and Muslim/Arabic women (5 percent).

We believe that Toronto is a forward-looking view into rental housing discrimination dynamics in other cities because of its relatively high level of ethnic diversity and adoption of online tools. Although the results represent only the important initial stage of apartment seeking, they provide a valuable baseline that can be generalized and compared across contexts. Our approach can help assess not only the severity of discrimination but also examine it in comparison to other metropolitan areas. Such a method requires care and specific considerations in order to avoid contaminating the sample as well as providing consistent results. Yet, it is cost-effective, does not require traveling to or within the city and imposes minimal inconvenience on the study population. By using automated tools it is also possible to provide randomized controls on several variables (such as using many names, five message bodies and two genders) with minimal on-going effort. We believe that the benefits of this work will be increasingly precise estimates of opportunity diminishing and opportunity denying patterns of discrimination in rental markets.

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### APPENDIX A. Email message bodies used in the study.

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Message	Text
A	Can you please let me know if the place is still available? Can I come see it this week? You can get in touch with me through email. Thanks  #first# #last#
B	I am wondering if the apartment is still available nad if I can come se it. When are you showing?  Thank you #first# #last#
C	My name is #first# #last#. Is the apartment still up for rent? It sound good. I can stop by when it is good for you/Thanks
D	Hello, I would like to know if your rental apartment is available.  I am available to come by to see it over the next few days.  Sincerely, #first#  #first# #last#
E	Are you still showing the apartment? Im looking for a place to rent and yours looks like a good option...I can come by at your earliest convenience after work.  #first# #last#

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Note: To add to the realism, these messages were obtained by sampling from real messages received from prospective tenants who wanted to rent an apartment. Spelling and grammatical mistakes are intentional.

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### APPENDIX B. Email Inquiries Sent and Received Ranked by Proportion Receiving Response

Gender	Ethnicity	Name	Inquiries	Prop. Response	Rank
M	Muslim/Arabic	Osama Mubbaarak	113	0.56	1
M	Muslim/Arabic	Mohammed Al-Kharat	105	0.63	2
M	Muslim/Arabic	Abdullah Hussein	115	0.66	3
M	Muslim/Arabic	Ali Sa'ad-al-Hummam	110	0.66	4
M	Black/ AA	Leroy Davis	113	0.66	5
M	E/SE Asian	Fai Zhang	125	0.66	6
F	Muslim/Arabic	Fahima Seyyidin	122	0.68	7
M	Caucasian	Rob Osborne	110	0.68	8
F	Muslim/Arabic	Iman Mubbaarak	117	0.68	9
M	E/SE Asian	Kuan-Yin Lin	114	0.69	10
M	Black/ AA	Delroy Carter	111	0.69	11
M	Jewish	Daniel Levine	112	0.70	12
F	Caucasian	Susan Campbell	103	0.70	13
M	E/SE Asian	Park Wu	113	0.70	14
F	Black/ AA	Tanisha Davis	117	0.70	15
M	Black/ AA	Jerome Brown	121	0.70	16
F	Black/ AA	Latoya Wright	105	0.71	17
M	Caucasian	Paul Summers	112	0.71	18
F	Muslim/Arabic	Fatima Sa'ad-al-Hummam	103	0.72	19
F	Jewish	Miriam Abramson	107	0.72	20
M	Muslim/Arabic	Ahmed Seyyidin	121	0.73	21
M	Jewish	Aaron Abramson	124	0.73	22
M	E/SE Asian	Li Chen	98	0.73	23
F	Black/ AA	Shaunika Banks	104	0.74	24
F	Jewish	Ruth Levine	116	0.74	25
F	E/SE Asian	An Lin	110	0.75	26
M	Black/ AA	Jamal Banks	114	0.75	27
F	Caucasian	Mary Stewart	134	0.75	28
F	E/SE Asian	Ling Wu	120	0.75	29
F	Black/ AA	Lakisha Harris	129	0.75	30
M	Caucasian	Michael Witherspoon	110	0.75	31
F	Jewish	Sarah Weiss	135	0.76	32
F	Caucasian	Lisa Witherspoon	108	0.76	33
F	Muslim/Arabic	Aisha Al-kharat	108	0.76	34
M	E/SE Asian	Jun Liu	114	0.76	35
M	Jewish	Sam Weiss	104	0.77	36
F	E/SE Asian	Li Ming Zhang	113	0.77	37
M	Black/ AA	Tyrone Wright	105	0.77	38
M	Caucasian	Matthew O'Reilly	116	0.78	39
M	Jewish	Isaac Cohen	125	0.78	40
F	Caucasian	Barbara Osborne	100	0.78	41
F	E/SE Asian	Ting Liu	107	0.79	42
F	Black/ AA	Ashanti Carter	105	0.79	43
F	Muslim/Arabic	Amira Hussein	110	0.79	44
F	E/SE Asian	Xia Chen	110	0.79	45
F	Jewish	Rebecca Cohen	88	0.80	46
M	Jewish	Seth Grossman	99	0.80	47
F	Jewish	Rachel Grossman	114	0.80	48
F	Caucasian	Stephanie McDonald	115	0.82	49
M	Caucasian	Peter McDonald	116	0.82	50

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**TABLE 1.** Individual Inquiry Response Rates by Group, Message Version, and Apartment Characteristic

	Sample Percent	Proportion Receiving Response	
		Men	Women
Overall	100	0.72	0.75
Group			
Caucasian	20	0.75	0.76
Black/African-Canadian	20	0.71	0.73
Muslim/Arabic	20	0.65	0.72
E/SE Asian	20	0.71	0.77
Jewish	20	0.76	0.76
Order			
First	20	0.68	0.69
Second	20	0.75	0.77
Third	20	0.73	0.78
Fourth	20	0.71	0.76
Fifth	20	0.71	0.76
Message Version			
A	20	0.74	0.77
B	20	0.71	0.76
C	20	0.68	0.71
D	20	0.72	0.75
E	20	0.72	0.76
Landlord Type			
Private Landlord	72	0.67	0.71
Commercial Landlord	19	0.82	0.83
Unknown	11	0.70	0.73
Rental Rate			
< \$750 per month	16	0.69	0.70
\$750 - 999	26	0.73	0.77
\$1,000-1,349	21	0.74	0.84
\$1,350 or more	29	0.81	0.78
Not specified	7	0.31	0.44
Dwelling Type			
Bachelor/Studio	7	0.73	0.86
1 bedroom	41	0.73	0.76
2 bedroom	25	0.74	0.73
3 bedroom	12	0.75	0.81
4-6 bedroom	2	0.79	0.68
Loft Condo	1	0.82	0.70
Not specified	12	0.55	0.67
N (number of inquiries)	5620	2820	2800

**TABLE 2.** Opportunity Diminishing Response Patterns within 1,124 Experiments

<i>Textual Analysis of Responses</i>	All Experiments		Male Experiments		Female Experiments	
	Freq	Percent	Freq	Percent	Freq	Percent
Equal Responses	1,088	97%	545	97%	543	97%
Unequal Responses	36	3%	19	3%	17	3%
Requested personal information						
Black	5		2		3	
Arab	4		3		1	
Arab & Black	4		1		3	
Caucasian	1		1		0	
Extra conditions						
Black	2		1		1	
Fewer conditions						
Jewish	1		1		0	
Caucasian	3		2		1	
Told Apartment taken						
Arab	4		3		1	
Black	5		2		3	
E/SE Asian	5		2		3	
Caucasian	3		2		1	
Jewish	3		1		2	
N (number of experiments)	1,124		564		560	

*Notes.* <sup>a</sup>Personal information such as employment status not requested of other inquirers; Extra conditions such as need for deposit; Fewer conditions such as "I can show you apartment anytime" that was not repeated for others; Told apartment was taken before others in the sample were told it was available.

**TABLE 3.** Opportunity Denying Response Patterns within 1,124 Experiments

<i>Response/Non-Response Analysis</i>	All Experiments		Male Experiments		Female Experiments	
	Freq	Percent	Freq	Percent	Freq	Percent
No responses	175	16%	96	17%	81	14%
At least one response	949	84%	468	83%	479	86%
Of experiments with at least one response						
Egalitarian Response patterns	603	64%	298	64%	309	65%
Unequal Response Patterns	346	36%	170	36%	170	35%
<i>Top Excluded Group Combinations</i>						
Arab only	65	6.8%	35	7.5%	30	6.3%
Black only	35	3.7%	12	2.6%	23	4.8%
E/SE Asian only	30	3.2%	16	3.4%	14	2.9%
Caucasian only	27	2.8%	11	2.4%	16	3.3%
Jewish only	24	2.5%	8	1.7%	16	3.3%
Black & Arab	20	2.1%	11	2.4%	9	1.9%
Arab & E/SE Asian	16	1.7%	10	2.1%	6	1.3%
All but Jewish	10	1.1%	7	1.5%	3	0.6%
All but Caucasian & Jewish	9	0.9%	7	1.5%	2	0.4%
23 other combinations	110	11.6%	53	11%	51	10.6%
N (number of experiments)	1,124		564		560	

Notes. Personal information such as employment status not requested of other inquirers; Extra conditions such as need for deposit; Fewer conditions such as "I can show you apartment anytime" that was not repeated for others; Told apartment was taken before telling another respondent it was available.

**TABLE 4.** Fixed-Effects Logistic Regression of Responses to Inquiries Conveying Apartment Availability

	All Experiments		Male Experiments		Female Experiments				
	OR	p	OR	p	OR	p			
Group (Caucasian omitted)									
Black	0.57	**	0.003	0.56	*	0.030	0.58	*	0.040
Muslim/Arabic	0.34	***	0.000	0.21	***	0.000	0.60		0.054
E/SE Asian	0.66	*	0.027	0.44	**	0.002	1.04		0.901
Jewish	1.02		0.927	0.96		0.871	1.10		0.726
Message Version (A omitted)									
B	0.81		0.271	0.69		0.166	1.04		0.889
C	0.36	**	0.010	0.36	**	0.009	0.37	*	0.011
D	0.67		0.059	0.71		0.202	0.62		0.084
E	0.73		0.101	0.69		0.176	0.77		0.353
Order (first omitted)									
Second	1.61	*	0.012	1.72	*	0.043	1.45		0.174
Third	1.16		0.413	1.10		0.707	1.28		0.369
Fourth	0.98		0.928	0.95		0.832	1.05		0.863
Fifth	1.00		0.995	0.95		0.842	1.01		0.964
Number of observations	137			6		717			659
Number of groups	279			279		146			133
LR $\chi^2$ (12)	99.0	***		78.4	***		38.8	***	
Log likelihood	-			-			-		
	463.6			232.6			221.8		

**TABLE 5.** Estimated Likelihood of Discrimination at Online Rental Inquiry Stage

	Men	Women	Combined
Caucasian	0.00	0.00	0.00
Black	0.05	0.05	0.05
Muslim/Arabic	0.12	0.05	0.08
E/SE Asian	0.07	0.00	0.03
Jewish	0.00	0.00	0.00

## Notes

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<sup>1</sup> ‘Racialization’ is the process by which societies construct races as real, different and unequal in ways that matter to economic, political and social life. This term is widely preferred over descriptions such as "racial minority", "visible minority" or "person of color" as it expresses race as a social construct rather than as a description of persons based on perceived characteristics (c.f., Ontario Human Rights Commission, Policy and Guidelines on Racism and Racial Discrimination, 2005).

<sup>2</sup> For example, in *Richards v. Waisglass* (1994), a Board of Inquiry found that the respondent discriminated against the complainant, a Black woman, because of her race when he refused to rent her an apartment. When they met, the respondent appeared reserved, refused to take any information from the complainant and stated that he wished to keep showing the apartment. At the complainant’s request, a female Caucasian friend viewed the apartment. She was greeted warmly and offered her the apartment. Among the respondent’s arguments for the discrepancy was his assessment that the complainant appeared “gregarious” and might have parties. The Board found that the respondent could not have come to a reasonable conclusion about the complainant’s potential behavior during their brief encounter, but that these assumptions were based on negative stereotypes about Black people. See also *Watson v. Antunes* (1998) and *Baldwin v. Soobiah* (1983), where in both cases the complainant was falsely told that the apartment was no longer available after an in person viewing.

<sup>3</sup> The latitude given to landlords on *Craigslist* has already led to a case of discriminatory housing practices. In 2006, the Chicago Lawyers’ Committee for Civil Rights Under Law sued *Craigslist* for allowing landlords to include discriminatory text in their advertisements. Evidence included postings that stated “NO MINORITIES [sic]” and “African Americans and Arabians tend to clash with me so that won’t work out”. The case was dismissed as *Craigslist* is given

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immunity under Section 230 of America’s “Communication’s Decency Act”. It states that Internet content providers “shall be treated as the publisher or speaker of any information provided by another information content provider” (*Chicago Lawyers’ v. Craigslist*, 2008).

<sup>4</sup> A field experiment that deceives field subjects but not agents (i.e. those being potentially discriminated against) with non-identifiability and minimal inconvenience conforms to professional codes of ethics (see the American Sociological Association’s Ethical Standards 2000; The British Psychological Society’s Code of Conduct, Ethical Principles & Guidelines 2000). Additionally, courts and judicial bodies have endorsed the argument that any costs of ‘minimal inconvenience’ involved are outweighed by the precise information provided on discrimination, which cannot be obtained by any alternate procedure (Fix, Galster, and Struyk 1993: 16-18).

<sup>5</sup> Our names are designed to suggest a potential African-American tenant rather than a tenant of African or Caribbean origin. However, as Statistics Canada does not differentiate Black people by country of origin we will generally refer to this group as Black.

<sup>6</sup> This program was written in *python* using *wxPython* (Rappin and Dunn 2004) for the interface and *libgmail* for properly interfacing with Gmail’s secure mail protocols. It is open source and available upon request. It is designed to work on Windows and Mac OS X.

<sup>7</sup> Strict confidentiality protections were put in place, including the separation of personal identifying information (e.g. email, address) from the experimental responses.

<sup>8</sup> Note that the frequencies do not add up to one hundred percent because in three experiments two unequal text responses were offered.

<sup>9</sup> We used Stata’s `xtlogit` command with the fixed-effects option for estimation.