Racial and Ethnic Biases in Rental Housing:

An Audit Study of Online Apartment Listings

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ABSTRACT

In this study we use a novel audit methodology to measure racial and ethnic rental housing discrimination in Toronto. We sent 5,620 fictitious email inquiries to individuals offering apartments for rent on a popular internet listing service. Because deception is involved, we first secured approval for this research from the University of Toronto's ethics review board. To manipulate perceived race/ethnicity, emails were randomly assigned names identified with five groups—Caucasian, Black, E/SE Asian, Arab/Muslim, and Jewish. Each landlord received a set of inquiries. Email text, send order and intervals were randomly varied. Property location, type, and responses were coded. Results show that inquiries by Muslim/Arab individuals are least likely to be replied to, followed by Black, E/SE Asian, Caucasian/White, and Jewish. Further analysis of the email text illustrates different forms of 'opportunity denying' and 'opportunity diminishing' treatment. Maps based on the address data visually illustrate the ecology of discrimination in Toronto.

INTRODUCTION

Residential segregation by race and ethnic status is a prominent feature of North American cities (Fischer and Massey 2004).[1] A complex set of causes contribute to observed residential patterns, including poverty, family background, cultural preferences, social networks, and the presence of local ethnic institutions (Pettigrew 2004). Housing discrimination also remains an important contributor to segregation (Dion 2001).

There have been few efforts in Canada to quantify the extent of housing discrimination. With a population that is more than 40 percent foreign born, Toronto is one of North America's most ethnically diverse metropolitan regions. Despite the city's adopted motto, "Diversity, our Strength", research consistently suggests that housing discrimination plays an important role in racial and ethnic residential segregation in the city of Toronto. However, research to date has focused on measuring perceptions of discrimination among ethno-racial minority groups using survey methods. In a Review of 21 unrelated quantitative studies conducted from 1957 to 1996, Novac, Darden, Hulchanski, and Seguin (2002) found that racial and ethnic discrimination is a continuing problem for some groups in the private rental housing sector.[2]

The Ontario Human Rights Commission (2004) found that the denial of opportunities to apply for rental housing or to view properties remains a common complaint of racialized persons in urban Canada. In this regard, landlords may use subtle screening methods to bypass certain individuals in the tenant selection process. For example, racialized persons may be advised that an apartment has already been rented only to have another-race friend inquire about the availability of the accommodation and be told that it is still available. There are several human rights cases in Ontario that have

dealt with racial discrimination in rental housing.[3] There has also recently been a documented increase in discrimination against persons identified as, or perceived to be Muslim, Arab and South Asian since September 11, 2001 (OHRC 2007). The Ontario Human Rights Commission has reported an increase in reports of individuals being subjected to islamophobia by housing providers when attempting to secure rental accommodations[4]

Compared to the US, Canadian research has been far less rigorous in quantifying the extent of discrimination (Novac et al 2002: 2). Limitations in the scale and methodology of the various studies make it difficult to generalize their 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, Lewis, Ehrlich and Che 2005; Dion 2001; Novac et al 2002). 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).

Current Study

Rather than trying to infer the significance of discrimination from surveys or residential patterns in the census, in this study we directly assess racial and ethnic discrimination in the rental housing market using a novel audit methodology. We sent fictitious email inquiries to apartment advertisers posted on a popular internet listing service. The recent emergence of internet apartment listings as a mainstream form of rental housing

advertising has made 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 apartment listing services such as 'Criagslist' (www.Craigslist.org) have become popular websites to post rental ads. The ads contain a description of the apartment and the landlord provides his or her email for prospective tenants to respond to.

To manipulate perceived race or ethnicity, emails were randomly assigned names identified with five groups—Caucasian, African-American/Canadian, E/SE Asian, Arab/Muslim, and Jewish. The four racialized 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. Most other audit studies have studied black-white differences only. In our audit experiment, each 'landlord' received inquiries from a set (5 inquiries) of either female or male apartment seekers. The email text, send order, and intervals between emails were systematically varied. All information from the listing (e.g. property location) and text from the landlord's response were recorded and coded. Our study, including our data collection procedures and safeguards of confidentiality, were approved by the University of Toronto's Research Ethics Board.[5]

Our audit methodology is innovative because it relies on 'racialized'[6] 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. This approach addresses one of the lead criticisms of traditional face to face audits-- researchers cannot match actors in all dimensions that might affect success in securing the rental unit, except for race (e.g. height, weight, age, dialect, dress, hairdo,

interview styles). The use of racialized names rather than actors has recently been applied in labor market audit studies (Aura and Hess 2004; Bertrand and Mullainathan 2004). For example, Bertrand and Mullainathan (2004) found that White sounding names on resumes received 50 percent more callbacks for interviews than African American names.

We infer discrimination by examining differences in responses to the set of five inquiries in a given experiment, controlling for message type, order sent, and apartment and landlord characteristics. Our hypothesis is that the measured levels of net housing discrimination based on the ethnic name association will be statistically significant, equaling or exceeding the prevalence of anti-ethnic sentiments expressed in recent surveys conducted in Canada (Murdie 2002, 2003; Novac et al 2004). Reflecting the recent increases in reports of islamophobia, we expect that apartment seekers with Muslim/Arab names to experience highest levels of discrimination. It is important to note that our experiment gauges racialized discrimination during only the "first contact" stage of the apartment seeking process. A more complete study of rental housing discrimination would need to follow-up by evaluating interactions with landlords after this first stage.

WAYS OF MEASURING HOUSING DISCRIMINATION

Discrimination against minorities in housing has generally been measured in one of two ways (Fischer and Massey 2004). Government agencies have often reported rates of discrimination based on filed complaints. However, complaint data are limited for gauging housing discrimination. Perhaps the most important problem with complaint

data is that discrimination is not measured directly but is filtered through a reporting process (Lucas 1994; Fischer and Massey 2004; Shively 2001). Formal complaints may represent only a small proportion of incidents of housing discrimination because the majority of cases are believe to go unreported (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 has spurred social scientists to field audit studies. In the U.S., Selltiz (1955) conducted an 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 1993). For instance, Black and Hispanic auditors posing as prospective apartment renters were shown 25 percent fewer units than White auditors with comparable income qualifications (Dion 2001; Galster 1992; Riach and Rich 2002).

More recently, Fischer and Massey (2004) investigated discrimination of rental housing using a phone based auditing methodology where actors used either white middle-class English, black-accented English, or Black English Vernacular. White auditors were more favored over black auditors of the same gender. Blacks experienced a much lower access to units marketed by private landlords rather than professional agents. A number of 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).

The basic idea behind the audit study is to ascertain whether members of a particular minority group receive different treatment from landlords and realtors compared to non-racialized groups with similar socioeconomic characteristics and housing preferences (Fischer and Massey 2004). Researchers have established differential treatment by sending out paired testers of different races to inquire about a housing unit. After the encounter with the rental or sales agent, testers complete a detailed report. With a sufficient number of trials, it is possible to discern with statistical certainty whether minority auditors receive worse treatment and less access to housing. The most common form of audit study has involved face-to-face scenarios (Fischer and Massey's (2004) study using a phone based auditing methodology is a recent exception).

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). That is, audit methodology has become an important and accepted tool for gauging discrimination[7]

Comparable audit studies of housing discrimination in Canada have not been fielded (Novac, Darden, Hulchanski, Seguin 2002). The first Canadian effort to use the

audit method to detect housing discrimination was conducted in 1959. A few small-scale audit studies in Canada have since found that individuals from Black and Aboriginal communities, in particular, are subjected to discriminatory treatment when seeking to rent housing (See, for example, Dion 2001; Novac, Darden, Hulchanski, and Seguin 2002).

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. 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[8]

DATA AND METHODS

Data Source

We searched for rental vacancies in the Toronto area using Craigslist during the Spring and Summer of 2007 (http://toronto.craigslist.org, a free listing service). Data collection completed in September 2007. Each day of the week, about ten new rental postings were chosen at random from those available (on average, about 100 new advertisements are posted each day in the Toronto section of the site).

The Experiment

We developed software that samples daily from new apartment listings on the Toronto Craigslist website. The landlord email address associated with each sampled listing is sent a set of five unique inquiries from different-race but same-gender individuals. The software randomly assigns race (represented by a racialized name) to a unique but

generic email inquiry. There are 50 random names[9] and 5 unique but generic email inquiries, all of which are listed in Appendix A and B. Random assignment guarantees that significant differences found in the sample are due to the name manipulation. The send order of the five email inquiries is also randomized. 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.

Data Management

In a few instances, the same landlord placed more than one advertisement during the data collection period. Each rental advertisement was treated independently with the exception of filtering restrictions that prevented the same landlord from being tested twice. A given landlord can have only one ID number, and all new sampled listings were cross-checked with previously used emails prior to sending out any test emails, preventing any individual or company from being included twice.[10]

Distinguishing 'Opportunity Denying' and 'Opportunity Diminishing' treatment

We coded email responses from landlords in several ways that are consistent with prior audit studies. We measure discrimination as 'opportunity denying' and 'opportunity diminishing' treatment. 'Opportunity denying' treatment covers acts of denial of any information (e.g. no response, variation in stated vacancy) that contrasts with what is offered to others who inquire. 'Opportunity diminishing' treatment refers to being told of less favorable rental terms, less flexibility in showing times, or less positive information about the apartment than others who inquire. For example, if one ethnic name receives

an email response while another does not from the same landlord, it is considered opportunity denying (information about send order will help us evaluate whether non-response is time-related rather than race-related). If a racialized name receives a response such as "you must fill out a credit check and pay a fee and deposit," while the non-racialized individual does not, it is considered opportunity diminishing. The rates of opportunity denying and diminishing treatment over all experiments will be summarized by the race/ethnicity and gender of the inquirer, the order of inquiry, message type, and landlord and apartment characteristics (e.g. rental rate, dwelling type, commercial vs. private landlord).

RESULTS

Descriptive Results

Rank-Sums of Response Rates

Table 1 and 2 report differences in receiving a response to individual inquiries (n=5,620). Table 1 reports the rank-sum totals by group and gender using response rate ranks for each of the unique 50 names (Appendix C ranks each unique name from 1 to 50 by response rate, with a rank=1 being the lowest response rate and a rank=50 the highest response rate). The rank-sums in Table 1 vary significantly by both racialized group and gender. Shading indicates statistically significant differences in rank-sum scores. For men, the Arab names have the lowest rank-sum scores, meaning that inquiries by Muslim/Arab individuals are least likely to be replied to. They are followed by Blacks and E/SE Asian names (statistically tied), and then Caucasians and Jewish names

(statistically tied). For women, the rank-sums of Arab and Black names are significantly lower than those of E/SE Asians, Caucasians, and Jewish names.

[Table 1 about here]

Response Rates by Group and Other Variables

Table 2 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% of men and 75% of women receive a response. Consistent with the rank-sum results, Arab male names have the lowest response rate, followed by Black and E/SE Asian names (for men only).

[Table 2 about here]

The first message sent has the lowest response rate. Our explanation for this unanticipated result is that people respond to more recent email messages first because they are at the top of their inbox queue. Because the first message sent is more likely to be further down in the inbox when individuals check their email, this may explain the lower response rate. Among the message versions, only version C had significantly different (lower) response rates, although the differences are modest.

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 (OHRC 2007) Analysis of within-experiment differences below will better determine if within-experiment or across-experiment differences in response rates account for these private-commercial differences in response rates. A size proportion of landlords could not be

classified as private or commercial based on the limited information available from the experiment.

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 that the landlord was less serious about renting the apartment.

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

Response Patterns within Experiments

Detecting discrimination requires looking at the differential pattern of responses within the 1,124 experiments. We report statistics for two basic types of unequal treatment within 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 and text response can occur in the same experiment, so they are not mutually exclusive. Figure 1 shows that unequal treatment by exclusion is about ten times more common than unequal treatment through text response (346 vs. 36 incidences in 1,124 experiments, respectively).

[Figure 1 about here]

Table 3 reports within-experiment patterns of unequal treatment by exclusion (top panel) and response text (bottom panel). Starting with the top panel, in only 16% (175) of experiments are there no responses to any of the five inquiries, while in 84% (949) there is at least one response. Among experiments with at least one response, most responses follow an "egalitarian" response pattern (64%, n=603), while only 36% follow an unequal response pattern (n=346).

[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 Arab (6.8%) followed by Black (3.7%) and E/SE Asian (3.2%). 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 32 possible combinations ($2^5 = 32$ combinations, each occurring 36%/32=1.125% of the time), then the first 7 excluded group combinations are more likely than by chance alone (that is, Arab only, Black only, E/SE Asian only, Caucasian only, Jewish only, Black & Arab, Arab & S/SE Asian all are greater than 1.125%).

For both the male and female experiments (columns 3-6), the most commonly excluded names are Arab (7.5% and 6.3%, respectively). Black names were more often excluded in the female than male experiments (4.8% vs. 2.6%, 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, if is difficult to make finer distinctions between the remaining excluded group combinations with these descriptive results. The fixed-effects models discussed below provide a more powerful framework for distinguishing patterns of unequal treatment.

The bottom panel of Table 3 describes unequal responses based on how landlords responded rather whether they responded. This analysis of the text of the emails found 36 experiments with unequal 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.

Although overall there are few cases of unequal treatment from the textual analysis, the unequal negative treatment was disproportionately directed at Black and Arab respondents, while the few cases of imposing fewer conditions was directed at Caucasian and Jewish names.[11]

Fixed-Effects Results

I 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) (Hsiao 1990). Fixed-effects models also 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, β is the vector of coefficients, x_{ic} is a vector of regressors with variation within cases, α_i is the individual effect and u_{ic} is the error term.

Table 4 reports the fixed-effects logistic estimates. [12] Separate models were estimated for all experiments, male experiments, and female experiment samples. Statistically significant results with $p \leq .05$ are shaded for visual clarity. 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 groups (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]

The results across models are generally consistent. In the overall model, the reported odds-ratios indicate significantly lower odds of Arab/Muslim (0.34), Black (0.57), and E/SE Asian (0.66) names of receiving a response than Caucasian names (the reference category). For example, in the experiments where there is variation in responses, the odds of a Muslim/Arab receiving a response are 66 percent less than those

of a Caucasian receiving a response. Likewise, the odds of a Black name receiving a response are 43 percent less than the odds of a Caucasian name receiving a response. Similar results hold for the male and female models, with the exception of the E/SE Asian female names not being significantly different from Caucasian names in response rates (1.04, $p=0.901$). The result for Muslim/Arab women is also only marginally significant (0.60, $p=0.054$). On the other hand, the results for Muslim/Arab men are especially strong and significant (0.21, $p<0.001$). Overall, the fixed-effects logistic results suggest that ethnic biases in online rental housing markets disadvantage Muslim/Arab 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 relative to Caucasian names.

For reasons that are unclear, 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 respond to more recent emails in the top of their inbox queue first, and the first message is more likely to be lower in the queue when people check their email.

Ecology of Discrimination

(Section forthcoming mid-March)

Using the address data, we are creating a unique set of maps of the ecology of discrimination in Toronto. We are mapping the response rates using the address link information provided in the apartment listing. Our forthcoming maps will visually convey the distribution of discrimination by neighborhood, and correlate discrimination to tract level measures of income, ethnicity, and age.

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Appendix A. Email Messages Used

To add to the realism, these messages were obtained by sampling from real messages received from prospective tenants who wanted to rent an apartment in the author's house in 2006.

Email #A

```
Hello,

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#
```

Email #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#
```

Email #C

```
My name is #first# #last#. Is the apartment still up for rent?  It
sounds good. I can stop by when it is good for you/Thanks
```

Email #D

```
Hello, I am inquiring about your rental apartment on craigslist.
http://toronto.craigslist.org/apa/#pid#.html

Is it still available?

I am available to come by to see it over the next few days.

Sincerely,
#first#

#first# #last#
```

Email #E

```
Are you still showing the apartment from the craigslist ad?  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#
```

Appendix B. Fictitious Names Used in the Audit Study (n=50)

African-Canadian Males	E/SE Asian Males
Jerome Brown	Li Chen
Jamal Banks	Park Wu
Tyrone Wright	Jun Liu
Delroy Carter	Kuan-Yin Lin
Leroy Davis	Fai Zhang
African-Canadian females	E/SE Asian Females
Tanisha Davis	Li Ming Zhang
Shaunika Banks	Ling Wu
Latoya Wright	Ting Liu
Ashanti Carter	Xia Chen
Lakisha Harris	An Lin
White/Caucasian Males	Jewish Males
Matthew O'Reilly	Seth Grossman
Peter McDonald	Sam Weiss
Rob Osborne	Daniel Levine
Paul Summers	Aaron Abramson
Michael Witherspoon	Isaac Cohen
White/Caucasian Females	Jewish Females
Susan Campbell	Rachel Grossman
Mary Stewart	Ruth Levine
Barbara Osborne	Sarah Weiss
Lisa Witherspoon	Miriam Abramson
Stephanie McDonald	Rebecca Cohen
Arab/Muslim Males	
Mohammed Al-Kharat	
Ahmed Seyyidin	
Ali Sa'ad-al-Hummam	
Abdullah Hussein	
Osama Mubbaarak	
Arab/Muslim females	
Aisha Al-kharat	
Amira Hussein	
Fahima Seyyidin	
Fatima Sa'ad-al-Hummam	
Iman Mubbaarak	

Appendix C. Email Inquiries Sent and Received by Group, Ranked and Color Coded

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

Appendix D. Logistic Regression of Apartment Availability on Race/Ethnicity, Send Order, Apartment Characteristics, Neighborhood Characteristics, and Landlord Characteristics

	Full Sample		Men		Women	
	OR	p	OR	p	OR	p
Black	0.85	0.105	0.84	0.211	0.86	0.290
Caucasian (omitted)						
Muslim/Arab	0.70	0.000	0.60	0.000	0.82	0.170
E/SE Asian	0.89	0.250	0.78	0.080	1.03	0.856
Jewish	1.01	0.958	1.00	0.989	1.00	0.976
Male	0.82	0.002	--	--	--	--
Message Version						
A (omitted)	--	--	--	--	--	--
B	0.92	0.381	0.86	0.296	0.97	0.844
C	0.72	0.001	0.71	0.012	0.74	0.031
D	0.89	0.268	0.91	0.501	0.87	0.344
E	0.92	0.394	0.89	0.426	0.95	0.715
Order						
first (omitted)	--	--	--	--	--	--
second	1.32	0.005	1.30	0.062	1.36	0.031
third	1.25	0.025	1.16	0.288	1.38	0.025
fourth	1.15	0.163	1.07	0.641	1.24	0.126
fifth	1.13	0.199	1.03	0.810	1.26	0.098
Landlord Type						
Private Landlord	--	--	--	--	--	--
Commercial Landlord	1.43	0.005	1.53	0.004	1.34	0.043
Unknown	0.96	0.339	1.11	0.447	0.95	0.272
Rental Rate						
< $750 per month	--	--	--	--	--	--
$750 - 999	1.43	0.000	1.18	0.212	1.83	0.000
$1,000-1,349	1.82	0.000	1.28	0.086	2.92	0.000
$1,350 or more	1.89	0.000	1.93	0.000	2.02	0.000
Not specified	0.29	0.000	0.21	0.000	0.39	0.000
Dwelling Type						
Bachelor/Studio	0.97	0.942	1.18	0.717	0.57	0.362
1 bedroom (omitted)	--	--	--	--	--	--
2 bedroom	0.89	0.160	1.03	0.805	0.79	0.047
3 bedroom	1.07	0.545	0.94	0.666	1.19	0.336
4-6 bedroom	0.81	0.339	0.99	0.974	0.52	0.045
Loft Condo	1.54	0.002	1.03	0.883	2.57	0.000
Not specified	0.93	0.525	0.79	0.102	1.18	0.290
Number of obs	5605		2815		2790	
LR chi2(23)	365.3		231.8		177.8	
Prob > chi2	0		0		0	
Pseudo R2	0.056		0.069		0.057	
Log likelihood	-3069		-1567		-1475	

Figure 1. Response Patterns within Experiments

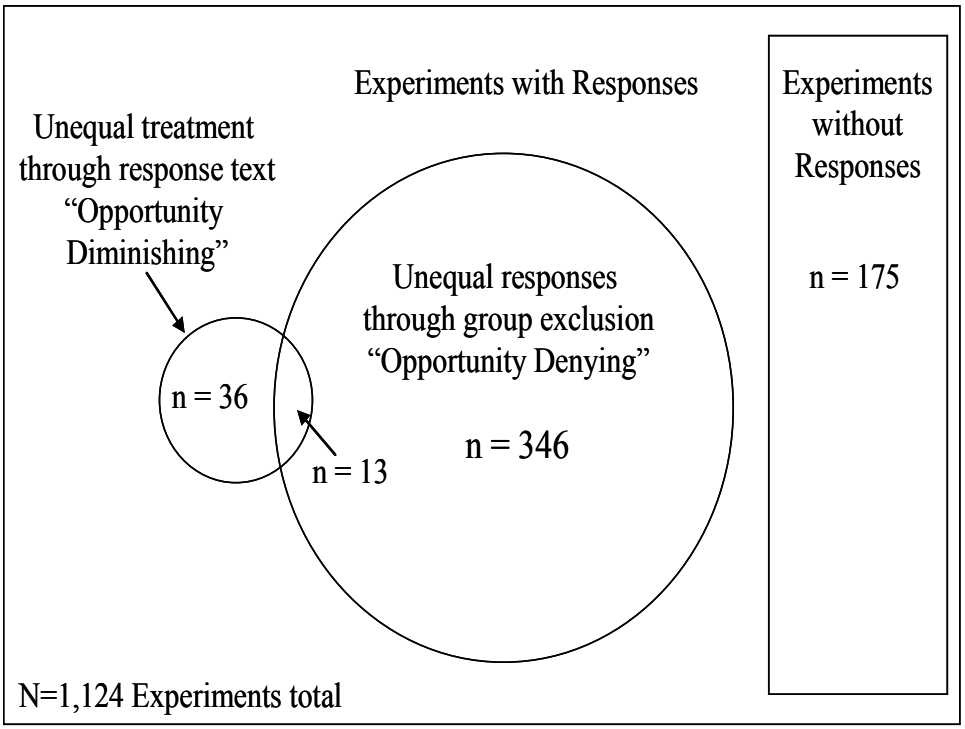

Table 1. Rank-sums of the Response Rates for 50 Unique Names used in the Experiments, by ethnicity and gender

		Male	Female
		519	756
Arab	144	31	113
Black	226	97	129
E/SE Asian	267	88	179
Caucasian	310	146	164
Jewish	328	157	171

Note. Each row-column cell is the sum of the ranks of response rates (from lowest=1 to highest=50) for the 5 unique names with the ethnicity-gender combination. Total rank-sum for 50 names is 1275

Table 2. 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			
Black/African-Canadian	20	0.71	0.73
Caucasian	20	0.75	0.76
Muslim/Arab	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 3. Within-Experiment Response Patterns (1,124 experiments, 5 inquiries per experiment)

	All Experiments		Male Experiments		Female Experiments	
Response/Non-Response Analysis	Freq	Percent	Freq	Percent	Freq	Percent
No responses	175	16%	96	17%	81	14%
At least one response	949	84%	468	83%	479	86%
Among experiments with at least one response						
Egalitarian Response patterns	603	64%	298	64%	309	65%
Unequal Response Patterns	346	36%	170	36%	170	35%
Exclusion Based						
Top Excluded Group Combinations (unequal text frequency)						
Arab only (1)	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%
Textual Analysis of Responses	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. 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						
Black	0.57	0.003	0.56	0.030	0.58	0.040
Caucasian (omitted)	--	--	--	--	--	--
Muslim/Arab	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	1376		717		659	
Number of groups 1	279		146		133	
LR chi2(12)	99.0		78.4		38.8	
Prob > chi2	0.000		0.000		0.000	
Log likelihood	-463.6		-232.6		-221.8	

Notes. Results with p≤.05 are shaded for visual clarity. Only covariates with within-experiment variation are included in estimated models. 1. full sample model: 775 groups (3828 obs) were dropped due to all positive or negative outcomes; male model: 383 groups (1894 obs) dropped due to all positive or negative outcomes; female model: 393 (1934 obs) dropped due to all positive or negative outcomes.

Maps will be Forthcoming Mid-March:

Figure 2. Map of Discrimination Intensity by Group

Figure 3. Map of Discrimination by Neighbourhood Characteristics

Notes

[1] One estimate by Dion (2001) pegs the cost of housing discrimination at four billion dollars for the Black and Hispanic Americans every year.

[2] The first Canadian study of perceived racial discrimination took place in 1969. A 1977 investigation of racial hostility in Toronto revealed incidents of racial harassment against homes of South Asian families. The 1992 Minority Survey in Toronto found black respondents were most likely to experience prejudice when moving into new neighbourhoods.

[3] For example, in *Richards* v. *Waisglass* (1994, 24 C.H.R.R. D/51 Ont. Bd. Inq.), 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 the complainant and the respondent met, the respondent appeared reserved, refused to take any information from her and stated that he wished to keep showing the apartment to other prospective tenants. When the complainant's friend, a White woman, went to see the apartment at the complainant's request, she was greeted warmly and was offered the apartment by the respondent. The respondent claimed that the reason he had acted differently with the complainant was because he had been tired the day they met; had thought that another person was going to take the apartment; and, had judged from the complainant's behaviour that she appeared "gregarious" and might have parties. The Board found that the respondent could not have come to a reasonable conclusion that the complainant would be loud and have noisy parties during their brief encounter and concluded that he had decided it was unlikely that the complainant would be financially stable and would likely have parties. The Board found that both assumptions were based on negative stereotypes about Black people. See also *Watson* v. *Antunes* (1998), CHRR Doc. 98-063 (Ont. Bd. Inq) in which a Board of Inquiry held that the respondent discriminated against the complainants, a Black woman seeking to rent an apartment and her mother who was assisting her, when she reluctantly showed them the apartment and then misled the mother about it being taken when she later called to rent it. Also, in *Baldwin* v. *Soobiah* (1983), 4 C.H.R.R. D/1890 (Ont. Bd. Inq.), a Board of Inquiry held that a *prima facie* case of discrimination in housing rental was established when the respondent made statements to potential tenants of a certain race that a property was rented, but then stated to potential tenants of another race that the apartment was still available. In other words, a pattern of refusals on the part of a landlord to rent to those of a particular ethnic origin was found to be evidence of unlawful discrimination.

[4] The number of complaints filed in the area of housing has fluctuated over time and has, in fact, been higher in recent years. For example, in 2000-2001, housing complaints amounted to 8% of the total number of complaints received by the OHRC, and in 2001-2002, this number was 7%.

[5] Deliberations both within academia and the judiciary have supported audit methods. Psychologists and sociologists have a lengthy history of deceptive research activity in laboratory and social settings, and have consequently developed strict codes of ethics;

for example The American Sociological Association's Ethical Standards (2000) and The British Psychological Society's Code of Conduct, Ethical Principles & Guidelines. Riach and Rich (2004) review the ethical issues associated with deceptive field experiments of discrimination.

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. A U.S. Federal court ruled that 'Many times the evidence gathered by a tester may be the only competent evidence available to prove that the defendant has engaged in unlawful conduct' (Fix, Galster and Struyk 1993: 16-18). The Ontario Human Rights Commission (2007) has recently endorsed the call for more systematic research into housing discrimination. Based on cumulative human rights cases, it reported that the most common problem that racialized persons continue to face is the denial of opportunities to apply for rental housing or to view properties. In this regard, it recognized that landlords may use subtle screening methods to bypass certain individuals in the tenant selection process. Only audit studies are suitable for measuring the extent of this discriminatory behaviour.

[6] '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 colour" as it expresses race as a social construct rather than as a description of persons based on perceived characteristics. See Ontario Human Rights Commission, Policy and Guidelines on Racism and Racial Discrimination (Toronto: Queen's Printer, 2005), online: <http://www.ohrc.on.ca>.

[7] Studies using this audit methodologies have been published in leading social science journals, including *American Economic Review* (Ayres and Siegelman 1995; Kenney and Wissoker 1994; Yinger 1986), *the Quarterly Journal of Economics* (Neumark et al 1996), *Journal of Economic Perspectives* (Darity and Mason 1998) and *The Economic Journal* (Riach and Rich 2002), *Social Problems* (Charles 2000), *Urban Affairs Quarterly* (Galster 1987), *Urban Affairs Review* (Massey and Lundy 2001), *City and Community* (Fischer and Massey 2004), and *the Journal of Urban Economics* (Page 1995).

[8] Canada Mortgage and Housing Corporation, Rental Market Report: Toronto CMA (October 2005) shows that 75 percent of new immigrant households rely, at least initially, on the rental market to meet their housing needs.

[9] These 50 racialized names (10 for each group) were created using a "most popular name" search on the internet for each ethnic category. The names are listed in Appendix B. Half the names for each group are male names and half are female. A generic email address was created and used for each name (e.g. Firstname_Lastname@gmail.com).

[10] Strict confidentiality protections were put in place, including the separation of personal identifying information (e.g. email, address) from the experimental responses.

[11] Note that the frequencies do not add up because in three experiments two unequal text responses were offered.
[12] I use Stata's xtlogit command with the fixed-effects option for estimation. Appendix D reports standard logistic estimates for comparison.