Homophobia and the home search: Rental market discrimination against same-sex couples in rural and urban housing markets

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ABSTRACT

Previous research has found discrimination against same-sex couples in the rental housing market. However, no studies have tested in rural markets, even though anti-LGBT attitudes may be more prevalent there due to conservative social norms and less frequent contact with LGBT people. I study whether the rate of discrimination against same-sex couples differs between rural and urban rental housing markets. I use a matched correspondence test design, sending email inquiries about the availability of rental homes to 445 landlords in 28 markets posing as either a same-sex or opposite-sex couple. Results compare rates of positive response between these groups, suggesting that landlords do not respond at substantially different rates to inquiries from same-sex or opposite-sex couples in rural or urban markets, nor do response rates differ between states with anti-discrimination ordinances and those without.

Introduction

Though attitudes toward lesbian, gay, bisexual, and transgender (LGBT) rights in the United States have shifted radically toward increased acceptance over the past 30 years, LGBT Americans continue to struggle for equal treatment under the law (Fetner, 2016; Rosenfeld, 2017). While the recent Supreme Court decision in *Bostock v. Clayton County* banning anti-LGBT employment discrimination may provide a legal basis for a future ban, there remain at present no federal laws prohibiting housing discrimination on the basis of sexual orientation. Unlike other protected classes like race, gender, or disability status, landlords are still free to discriminate on the basis of sexual orientation in many states by refusing to sell or rent properties or offering unequal terms to LGBT people, or even advertising that they will not rent to sexual minorities. Twenty-two states and the District of Columbia, as well as some local governments, have passed their own laws prohibiting sexual orientation-based housing discrimination, but LGBT Americans in the rest of the country may find themselves without legal recourse if they are refused housing due to their sexuality. Given enduring anti-LGBT attitudes and the lack of legal prohibitions on discriminatory behavior, it is thus perhaps not surprising that several recent correspondence studies have produced evidence of rental housing discrimination against same-sex couples across U.S. metropolitan areas (Friedman et al., 2013; Levy et al., 2017; Schwegman, 2019). These tests have found that same-sex couples are between 2 and 6 percent less likely

to receive responses to inquiries about housing availability. Schwegman (2019) finds this to be true even in markets with anti-discrimination legislation. When inquiries are answered, same-sex male couples are told about fewer available units than heterosexual couples (one fewer unit for every four tests) and quoted annual prices $272 higher (Levy et al., 2017).

While these studies have advanced our knowledge of the extent of discrimination against same-sex couples on the American rental market, they share a common shortcoming: they conduct their tests primarily in urban areas, where attitudes tend to be more liberal, support for LGBT rights is more common, and stricter protections are often in place (Parker et al., 2018; Wald et al., 1996). There is limited empirical evidence of housing discrimination against same-sex couples in rural communities, but Swank et al. (2013) report survey findings that lesbians, gays, and bisexuals living in small towns report higher levels of housing discrimination than peers living in urban areas. If this is true, existing studies may be significantly underestimating the extent of anti-LGBT housing discrimination, especially for the estimated 2.9–3.8 million LGBT Americans living in rural areas (Movement Advancement Project, 2019).

In this study, I assess whether rates of rental housing discrimination against same-sex couples differ between urban and rural areas in the United States through the use of a matched correspondence test design. In doing so, I extend the existing literature on housing discrimination against same-sex couples by widening its geographic scope, providing

information about the extent of discrimination in nonmetropolitan areas, where nearly 1 in 5 Americans live. This study is one of the first worldwide, and to my knowledge the first in the U.S., to use a correspondence test design to examine housing discrimination in rural areas. I contribute to the wider correspondence test literature by offering a methodology that addresses the specific challenges of conducting this kind of test outside of a major metropolitan area. In addition, I provide current information about the prevalence of housing discrimination, following other research showing an increase in overall anti-LGBT discrimination since the 2016 U.S. presidential election (GLAAD, 2018; Gonzalez et al., 2018). Here, I begin with a review of previous studies of housing discrimination against LGBT individuals and differences between rural and urban attitudes toward sexual minorities. Then, I discuss my research design and methodological considerations. Finally, I provide results and conclude with a discussion of the limitations of this work and avenues for future research. These results may help to inform future policymaking by suggesting an appropriate geographic level to enact anti-discrimination legislation, and helping organizations target resources to LGBT individuals in communities most affected by housing discrimination.

Background

While only a few studies to date have investigated the extent of housing discrimination against same-sex couples, there exists a robust literature on the topic of discrimination in the housing and labor markets. Several theoretical explanations of the mechanisms of discrimination have been proposed, of which the two most commonly discussed are "taste-based" discrimination (or "animus") as proposed by Becker (1957) and "statistical discrimination" as proposed by Arrow (1973) and Phelps (1972). Taste-based theories propose that decisionmakers (here, landlords) would choose majority applicants over minorities to avoid contact with members of minority groups. In this model, landlords are willing to pay an economic cost (i.e., their housing unit remaining vacant) to avoid this contact, reflecting a taste for discrimination. However, as pointed out by Schwegman (2019), landlords in high-demand markets will pay little to no cost in discriminating due to the large number of qualified applicants they can choose from. Turning away qualified applicants in rural markets where demand is generally low may be more costly.

Taste-based discrimination is often contrasted with statistical discrimination, which instead posits that decisionmakers discriminate not out of animus, but only to maximize their economic well-being. Because decisionmakers have imperfect information on which to base their decision (i.e., an email inquiring about an apartment, or even an application), they may infer an applicant's risk from the information they can observe. If a landlord perceives that an applicant's sexual orientation (or gender, race, etc.) could influence their ability to pay rent or to be a good tenant, perhaps due to perceptions that these minority groups earn less than their peers on average, they may be more likely to ignore or refuse minority applicants. While the literature has not definitively answered the question of which of these theories more accurately describes the mechanisms of housing discrimination, several recent meta-analyses have provided some evidence for statistical discrimination, at least as it relates to ethnic and racial discrimination (Auspurg et al., 2019; Flage, 2018; Rich, 2014). However, economic stereotypes associated with minority racial and ethnic groups may not apply to sexual minorities. While black or Hispanic groups may be associated with lower incomes, same-sex couples are often assumed to be relatively affluent "DINKs" ("double income, no kids") who belong to an educated, urban elite (Badgett, 2003). While these stereotypes may even benefit same-sex couples in situations like an apartment search where their economic status is being assessed, it may also signal an increased risk of turnover due to the mobility of highly educated people in the U.S.

A third theoretical explanation, the contact hypothesis, may help to explain potential differences between anti-LGBT discrimination in rural and urban settings. The contact hypothesis, as first proposed by Allport (1954), suggests that contact between majority and minority groups (at least under "optimal conditions") can reduce prejudice. Psychological research suggests that contact theory may help explain the dramatic shift in pro-gay attitudes in the past few decades (Rosenfeld, 2017), with those who reported knowing gays or lesbians reporting more positive attitudes and less homophobia toward gays and lesbians more generally (Herek and Capitanio, 1996; Herek and Glunt, 1993; Walch et al., 2010). Lauster and Easterbrook (2011) also find that gay male couples experience less housing discrimination in Vancouver, Canada neighborhoods with higher LGBT populations, supporting the idea that increased contact and familiarity will yield less discriminatory behavior. This also suggests that rural Americans may be more likely to discriminate as they are less likely to have a close friend or family member who identifies as LGBT (Movement Advancement Project, 2019).

Even when studying urban areas, the few previously conducted studies on anti-gay housing discrimination in the U.S. and other Western countries have largely found evidence of discrimination, at least for same-sex male couples. As in this study, most previous studies on this topic use an audit or correspondence test design, either employing in-person auditors or phone or email correspondence to inquire about housing availability. While these designs have most commonly been used to study ethnic and racial discrimination (see meta-analyses by Auspurg et al. (2019) and Flage (2018)), they are increasingly being used to study a wider range of topics like sexual orientation (see Gaddis (2018) for a review).

The most recent and most geographically expansive of these studies focused on same-sex couples was conducted by Schwegman (2019), who found evidence of discrimination against same-sex male couples in his correspondence study of 94 U.S. cities. Schwegman also finds that nonwhite same-sex male couples face even greater discrimination, compounding the effects of sexual orientation and race. However, he finds that lesbian couples do not face significant discrimination no matter their race. Levy et al. (2017) find concurring results in their in-person and remote tests in the Los Angeles and Dallas-Fort Worth metropolitan areas, finding that landlords told gay male couples about fewer units than lesbian or heterosexual couples. However, Friedman et al. (2013) find that both gay male and lesbian couples received fewer responses from landlords in a matched-pair correspondence study of 50 metropolitan areas. Additionally, smaller local studies find evidence of discrimination in Greensboro, North Carolina (Sills et al., 2018), Richmond, Virginia (Housing Opportunities Made Equal of Virginia, 2015), and several cities in Michigan (Michigan's Fair Housing Centers, 2007; Page, 1998). One contradictory finding comes from Murchie and Pang (2018), who suggest that gay couples receive preferential treatment from landlords compared to heterosexual couples in their randomized correspondence test in the largest 20 U.S. metropolitan areas. However, unlike others, this study compares single people and couples, and thus cannot rule out that landlords simply favor two-person (and thus potentially double-earner) households to single people. Results from other Western countries are also mixed: while Ahmed and Hammarstedt's study (2009) finds evidence of discrimination against same-sex couples in the Swedish housing market, Mazziotta et al. (2015) find no such evidence in German cities.

While there is evidence of housing discrimination against LGBT Americans, there is still little information describing rural-urban differences in anti-gay discrimination, other than some limited survey data. Cities have long been seen as a refuge by LGBT individuals in the U.S., a place where many move to escape perceived discrimination in smaller communities (Weston, 1995). There is some empirical evidence of this: research from the 1990s indicates that people living in rural areas, and those living in the U.S. South or Midwest generally report higher levels of anti-LGBT prejudice, although attitudes nationwide have shifted dramatically since this time (Herek, 1994). More recently, Swank et al. (2013) find that LGBT people living in small towns report higher levels

of housing discrimination than those in urban areas, perhaps the only quantitative information to date describing levels of housing discrimination by type of community. Further survey data describes attitudes in rural and urban communities: a 2018 survey indicates that a narrow majority (52%) of rural respondents do not support same-sex marriage, a 17-point differential from urban respondents (Parker et al., 2018). Another recent survey finds that residents of rural areas are twice as likely, and residents of the Southern U.S. are three times as likely, to be targeted by sexuality-based online harassment (Costello et al., 2019). While information is limited, these results suggest that rural gays and lesbians may be at especially high risk of discrimination due to more prevalent anti-LGBT sentiment.

Experimental design

The present research will evaluate the research question: Does the prevalence of housing discrimination against same-sex couples differ between rural and urban areas in the United States? Following the available literature on the differences in LGBT acceptance between rural and urban areas, I hypothesize that housing discrimination will be more prevalent in rural areas than in urban areas. Based on the results of previous tests, I expect state-level anti-discrimination legislation to have no effect on discrimination. I also hypothesize that same-sex male couples will experience discrimination, while same-sex female couples will not.

While housing discrimination can take on many forms, from explicit statements precluding LGBT applicants to subtler requests for additional information or less polite responses, the present research will focus on rates of non-response and negative response as a measure of discrimination. There are several reasons for this: first, recent research finds that subtler forms of discrimination are not widely used against same-sex couples (Schwegman, 2019). Second, testing for these forms of discrimination is methodologically challenging, requiring more advanced methods of assessing the emotional content of text responses. Finally, research from the field of racial discrimination suggests that ambiguous, subtle forms of discrimination (i.e. not replying to an email) have become more common than outright discrimination (such as an explicitly biased or derogatory response) as social norms increasingly discourage such blatantly discriminatory behavior (Pettigrew and Meertens, 1995). While this transition to subtler forms of discrimination has not been studied extensively in the context of anti-LGBT discrimination, one Spanish study provides some initial evidence of this phenomenon (Molero et al., 2017).

To test the prevalence of housing discrimination, I conduct an email-based correspondence test examining whether property owners posting online classified advertisements respond at different rates to same-sex couples than to heterosexual couples. Following the majority of previous studies in the field of housing discrimination, I use a matched design. In a matched correspondence test, researchers send two nearly identical requests and examine differences in response between the two. Here, the requests differ only by the identity of the partner mentioned in the email (and some minor wording changes). The advantage of such a design is that it allows for a test of within-respondent variation, testing the same property manager's response to two fictional applicants.

The primary disadvantage of matched designs is the risk of detection: that a respondent finds the inquiries suspicious for whatever reason and changes their behavior accordingly. In this case, a property manager may find it unusual to receive two similar requests within a certain window of time, or they may accidentally receive two inquiries from the same alias due to screening error. They may then decide not to respond to either, assuming them to be fraudulent, or they may even perceive the idea of the test and more carefully respond to both when they may have otherwise acted differently. The effects of time represent an additional risk, as a unit could be rented between the first and second inquiry. However, the impact of these factors can be tested by comparing response rates between first and second email inquiries.

Correspondence testing has also been criticized on ethical grounds, as this research design necessarily involves deception, as well as testing unknowing subjects without their informed consent. However, such a design is perhaps the only ecologically valid way of studying a phenomenon like anti-LGBT housing discrimination, behavior that is not only subject to strong social desirability bias but is also illegal in some jurisdictions. Following the recommendations of Zschirnt (2019), the present study adheres to ethical standards by seeking to minimize inconvenience to landlords and anonymizing information such that no individual landlords can be identified.

Sampling

Listings were randomly selected from Craigslist (craigslist.org), the most popular classified advertising website in the United States and one of the 50 most-visited websites in the country at the time of writing (Alexa Internet, 2019). As noted by Murchie and Pang (2018), Craigslist is widely used by researchers as it uses a consistent format across cities, has a nationwide reach, and has a large number of users, estimated at 60 million per month as of 2018. Craigslist divides listings into hundreds of local markets that roughly cover the nation, although the boundaries of these markets are left undefined. In rural areas, markets can cover wide geographic areas, even whole states in a few cases. I selected twenty-eight markets from these options, fourteen rural and fourteen urban, as listed in Table 1. The twenty-eight markets are selected from fourteen states, seven with state-level anti-discrimination legislation and seven without, to allow for a comparison between these markets. While Schwegman (2019) finds little effect of state-level protections in his study, this study offers some additional evidence of the effectiveness of these laws. Additionally, eleven of the fourteen center cities in urban markets (with the exception of Wichita, Virginia Beach, and Charlotte) have enacted local-level protections against LGBT housing discrimination, although these are unlikely to cover the entire metropolitan area given the uneven nature of local legal protections. States were chosen to ensure geographic diversity, and each state selected includes a large metropolitan area as well as a nonmetropolitan area listed on Craigslist. In each state, the urban area selected is the largest metropolitan area in the state whose center city also lies within the state (excluding the

Table 1

Markets selected for analysis, as named on Craigslist. 1: Market included in Schwegman (2019). 2: Market included in Murchie and Pang (2018).

	State	*Urban market*	*Rural market*
States with anti-discrimination protections (year enacted)	California (1992)	Los Angeles[1,2]	Siskiyou County
	Colorado (2008)	Denver[1,2]	Western Slope
	Illinois (2005)	Chicago[1,2]	Southern Illinois
	New York (2002)	New York City[1,2]	Potsdam-Canton-Massena
	Maryland (2001)	Baltimore[1]	Eastern Shore
	Oregon (2007)	Portland	East Oregon
	Wisconsin (1982)	Milwaukee	Northern WI
States without anti-discrimination protections	Kansas	Wichita[1]	Salina
	Michigan	Detroit Metro[1,2]	Upper Peninsula
	Ohio	Cleveland[1]	Athens
	North Carolina	Charlotte	Boone
	Pennsylvania	Philadelphia[1,2]	Poconos
	Texas	Dallas / Fort Worth[1,2]	Texoma
	Virginia	Norfolk / Hampton Roads	Southwest VA

Washington, D.C. area in Virginia and Kansas City in Kansas).

The rural markets were selected based on the volume of listings currently found on their Craigslist page, so as to exclude markets with an insufficient number of listings to sample from. This may be a potential source of bias, as these markets may be more likely to include slightly larger communities, tourist destinations, or communities with colleges that drive a larger-than-average local housing demand. To evaluate this, I present demographic and political information in Table 2. In all cases, the rural markets selected have smaller city sizes, a larger share of non-Hispanic white population and population over 65,[1] lower mean income and educational attainment, and a higher Trump vote share in the 2016 election than their corresponding urban markets, all of which conforms to expectations. While twenty-four of the twenty-eight markets have roughly average (3–4 percent) shares of young people ages 18–21, four markets have shares over 5%, indicating the presence of colleges in Athens, Ohio; Boone, North Carolina; Potsdam-Canton-Massena, New York; and Michigan's Upper Peninsula. However, these markets do not vary widely on other demographic and political characteristics from the other nonmetropolitan markets, and thus are still broadly representative of these communities.[2]

I randomly sample listings from those available using a semi-automated randomized method. Posts are collected daily in batches using Phantombuster,[3] an online web scraping tool, and first inquiries are sent within a day of the initial post, maximizing the likelihood that the unit remains unfilled. The size of the batch collected in each market pair is set by the number of posts made in the past day in the market with the fewest listings, excluding market pairs in which one market has fewer than two listings. In practice, this resulted in batches of two to five emails per day per market. Listings advertising rent-to-own units, units restricted to seniors, disabled people, or single individuals, short-term rentals (like Airbnbs) and those seeking roommates for an already occupied unit were excluded. Also excluded were obviously fraudulent posts, those clearly outside the geographic area (i.e. vacation homes) or those that do not list an email address. Because many landlords list more than one unit on Craigslist, or may post the same unit multiple times, effort is made to screen out posts from already contacted landlords so as not to bias results or risk detection. Posts are manually scanned for duplicate address, housing development, company, and contact information. While this may bias the sample toward landlords with fewer units, previous research has shown that this is unlikely to bias the results (Auspurg et al., 2020).

Determining sample size is uniquely challenging in audit studies as it relies on the expected degree of concordance, i.e., how often the two testers receive the same outcome. In the most recent such test conducted, Schwegman (2019) suggests that a high degree of concordance is likely. Comparing same-sex male couples with heterosexual couples, he finds that 21.5% of responses were sent only to heterosexual couples and 15.6% were sent only to same-sex male couples. In my pretest (details below), I find a similar rate of concordance in rural markets (15.6% vs. 9.4%, respectively). Using the response rates from my pretest results as an input for McNemar's test calculations from Vuolo et al. (2016), I expect that a sample size of 394 listings should be adequate in order to reach a sufficient statistical power $(1 - \beta = 0.8)$ and significance $(\alpha = 0.05)$.

Signaling

Each post that meets selection criteria (i.e. randomly selected and not

a duplicate or fraudulent post) triggers two scripted emails to the responsible property manager. Emails signal an applicant's sexual orientation through a reference to a partner with a gender-specific name, a signaling strategy used in similar studies (Friedman et al., 2013; Lauster and Easterbrook, 2011; Levy et al., 2017; Schwegman, 2019). This strategy precludes the study from analyzing potential discrimination against single gays or lesbians, or transgender individuals, but is easier to signal naturally in an introductory email.[4] Following Friedman et al. (2013), names were selected from those listed on the Social Security Administration's list of popular American names of the 1980s and 1990s, so as to imitate renters between age 20 and 40. I assign the names "Christopher," "Michael" and "Joshua" for males and "Jessica," "Emily" and "Sarah" for females. Names were chosen to be unambiguously gendered, and not identified with minority racial or ethnic categories so as not to conflate the effects of race-based and sexuality-based discrimination.[5]

Email responses were slightly adapted from Schwegman (2019) and Murchie and Pang (2018), both of whom signaled class in their studies by using two emails varying in the number of grammar and spelling errors. Rather than varying the class of my applicants, here I adapt their "low-class" emails, including more broken syntax and language errors. I chose the "low-class" rather than the "high-class" email script to minimize detection, as an interview with a property manager working in the rural Midwest (in markets not studied here) confirmed that this type of language was typical of inquiries in their area. Email inquiries will read as shown in Table 3, with names and terms in brackets changed to match the desired sexual orientation.

Each valid post selected receives two email inquiries from applicants of the same gender, but differing sexual orientation, as shown in Table 4. The order of inquiries is randomized so that the possible effect of time does not bias results. For example, if the first email is randomly assigned as a same-sex female couple, the second will be a heterosexual couple with a female author. Some recent studies suggest that landlords may prefer to rent to female applicants (Ahmed and Hammarstedt, 2008; Murchie and Pang, 2018), so varying the gender of the email author serves to avoid conflating the effects of gender and sexual orientation. The specific language of the emails varies between the first and second messages as well to avoid detection (see variations in Table 3), but the same information is provided so as to minimize the chance that specific wording is responsible for any variation in response rate.

The second email is sent two days after the first. Many matched studies have used a shorter window of time between emails (usually a few hours), to avoid the potential impact of time on response rates. However, a lower volume of applicants in rural markets necessitates a slower pace of inquiries to avoid suspicion. Thus, it will be important to check differences in response rates between first and second emails to evaluate whether the 48-hour delay impacted my findings. If a property manager responds to the email inquiry, a response will be sent one day later thanking them for the response and informing them that the applicant has found another apartment. This is not only beneficial to further the illusion of a real applicant, but also to avoid an unethical disruption to the property manager's business or to other applicants.

Emails are sent from generic name-based addresses with numbers added at random based on availability (i.e. "firstname.lastname123@gmail.com"). Craigslist uses an anonymized email system such that email addresses are not revealed to a landlord until after their first response, but the emails selected should be regarded as ecologically

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[1] This is true of all rural markets except for the Athens, Ohio market, which has a lower share of over-65 population than the urban Cleveland market.

[2] In analyses not presented here, excluding these markets (as well as the Southern Illinois and Maryland Eastern Shore markets, which also contain large colleges) did not significantly change the main results.

[3] For more information, visit http://www.phantombuster.com.

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[4] While Murchie and Pang (2018) attempt a signaling strategy based on quotes in an email signature, this may be seen as unusual by landlords and thus bias results.

[5] Previous research has shown that black male couples may face higher risks of discrimination (Schwegman 2019), however such analysis is beyond the scope of the present study. The combination of racial and homophobic discrimination in rural housing markets is a worthy topic for future research.

Table 2.

Demographic and political characteristics of selected markets. Data: American Community Survey 2018 (5-year), MIT Election Data and Science Lab (2017).

State	Market	Population of largest city, 2018	Percent college age (18–21)	Percent over 65	Percent non-Hispanic white	Mean household income	Bachelor's degree or higher	Trump vote share, 2016
All	*United States*	*8,398,748 (New York)*	*4.1%*	*15.2%*	*61.1%*	*$90,021*	*31.5%*	*46.1%*
CA	Los Angeles MSA	3,990,456 (Los Angeles)	4.1%	13.1%	29.8%	$99,888	33.7%	27.6%
	Siskiyou County	7,556 (Yreka)	3.4%	24.0%	76.7%	$63,208	22.5%	55.3%
CO	Denver MSA	716,492 (Denver)	3.2%	12.3%	64.3%	$101,397	42.9%	37.9%
	Western Slope	63,374 (Grand Junction)	3.9%	16.3%	78.8%	$80,162	35.9%	52.1%
IL	Chicago MSA	2,705,994 (Chicago)	3.9%	13.6%	52.2%	$97,991	38.5%	29.8%
	Southern Illinois	25,902 (Carbondale)	3.9%	18.8%	88.3%	$61,038	18.7%	68.8%
KS	Wichita MSA	389,255 (Wichita)	3.8%	14.1%	72.1%	$72,871	30.4%	57.7%
	Salina	46,716 (Salina)	4.4%	16.9%	81.7%	$66,294	25.9%	64.5%
MD	Baltimore MSA	602,495 (Baltimore)	3.8%	14.8%	57.0%	$103,145	39.5%	37.9%
	Eastern Shore	32,809 (Salisbury)	4.9%	19.7%	68.7%	$78,270	26.4%	56.1%
MI	Detroit MSA	672,662 (Detroit)	3.5%	15.6%	66.6%	$81,193	30.6%	42.5%
	Upper Peninsula	21,355 (Marquette)	5.2%	20.9%	87.9%	$59,496	23.9%	56.4%
NC	Charlotte MSA	872,498 (Charlotte)	3.6%	12.5%	60.1%	$87,084	35.7%	48.6%
	Boone	19,562 (Boone)	10.6%	18.6%	91.3%	$59,226	30.2%	56.9%
NY	New York MSA	8,398,748 (New York)	3.5%	14.4%	35.4%	$103,184	38.6%	20.8%
	Potsdam-Canton-Massena	12,883 (Massena)	6.6%	16.2%	88.7%	$63,827	22.2%	50.2%
OH	Cleveland MSA	383,793 (Cleveland)	3.6%	17.5%	70.0%	$76,663	30.7%	40.0%
	Athens	24,688 (Athens)	13.5%	13.9%	91.4%	$56,547	24.5%	47.3%
OR	Portland MSA	653,115 (Portland)	3.3%	13.9%	72.3%	$93,443	41.1%	28.8%
	Eastern Oregon	17,671 (Hermiston)	3.9%	18.0%	73.4%	$62,677	17.9%	66.0%
PA	Philadelphia MSA	1,584,138 (Philadelphia)	4.0%	15.2%	60.7%	$96,686	38.8%	32.2%
	Poconos	10,384 (East Stroudsburg)	4.2%	18.9%	77.5%	$74,695	22.9%	56.8%
TX	Dallas-Fort Worth MSA	1,345,037 (Dallas)	3.9%	10.7%	46.3%	$93,534	34.4%	50.4%
	Texoma	38,521 (Sherman)	3.9%	17.3%	75.8%	$70,460	20.5%	74.5%
VA	Norfolk-Hampton Roads MSA	450,189 (Virginia Beach)	4.5%	12.8%	54.8%	$83,476	30.3%	42.7%
	Southwest Virginia	17,035 (Bristol)	3.0%	20.3%	93.7%	$54,318	20.2%	77.4%
WI	Milwaukee MSA	592,025 (Milwaukee)	3.9%	14.7%	66.9%	$83,215	35.1%	43.2%
	Northern Wisconsin	13,661 (Chippewa Falls)	2.7%	21.7%	91.7%	$65,057	20.0%	59.4%

Table 3.

Sample email text, two variations. Bracketed phrases are filled in with names and genders according to treatment status.

Variation 1	Variation 2
"Hi! [My girlfriend] [NAME 2], saw your post CL and were interested in the rental. We do you need to know about us. Let us know! Thanks! [NAME 1] & [NAME 2]"	"Hi, [my boyfriend] [NAME 2] and I saw this online, is it still aviaable? We can give you references is you want. Thanks! [NAME 1] & [NAME 2]"

Table 4

Control and treatment conditions, with example names.

Email author gender	Control	Treatment
Male	Opposite-sex couple, "Michael Anderson and Sarah"	Same-sex male couple, "Christopher Clark and Joshua"
Female	Opposite-sex couple, "Emily Miller and Joshua"	Same-sex female couple, "Jessica Martin and Sarah"

valid when revealed in follow-up emails. Last names were selected from a list of the most common last names in the United States, and names associated with minority ethnic or racial groups were excluded so as not to vary the ethnicity of the applicant. Selected last names are reported in Table 4.

Data collection

Before conducting the main analysis, I conducted a pretest from February 10 to March 2, 2020 using this experimental design to ensure that email scripts are equivalent, that the test is not easily detectable, and that the treatment is clear. The pretest served to ensure that the technical infrastructure planned (i.e., web scraping tool, email automation) is sufficient to conduct the full test. To this end, I sent 115 matched inquiries to 66 listings in the urban Minneapolis / St. Paul and rural Southwest MN markets in Minnesota, a state not selected for the main analysis. While the sample size of this test is too small to make any significant inferences, these initial results are reported in Table 5. Results suggest that controlling for email order in final analyses may be important, as rates of response vary significantly between the first and second inquiry (74% vs. 51%), a disparity that is even wider in the Minneapolis market where apartments rent faster. The fast pace of the market also meant that six Minneapolis listings (and one rural listing) had been deleted by the time of the second email, which precluded me from carrying out the matched test. Rates of response did not vary substantially between email scripts (65% vs. 61%), suggesting that the current scripts were perceived to be roughly equivalent, as intended.

Table 5.

Net response rates from pretest data by sexual orientation and market. $N = 59$, number in brackets indicate number of listings in category.

Market	No response	Both couples	Only heterosexual couples	Only same-sex couples	Net discrimination
Southwest MN (rural)	25.0% [8]	50.0% [16]	15.6% [5]	9.4% [3]	6.2%
Minneapolis / St. Paul (urban)	22.2% [6]	44.4% [12]	14.8% [4]	18.5% [5]	−3.7%

Qualitative analysis of responses informed one small wording change to one email script, as a reference to an "apartment" caused confusion when inquiring about rental houses. Finally, no obvious instances of detection were encountered.

The full correspondence test was conducted from March 14 to March 31, 2020. It is worth noting that testing occurred during the global outbreak of the COVID-19 (coronavirus) pandemic, a period of time during which millions of Americans lost their jobs and residents of many U.S. states were asked to refrain from leaving their homes. Possible implications of this timing will be discussed further in the following sections. During this time, I sent 853 emails inquiring about 447 listings in twenty-eight markets. However, 39 matched inquiries could not be completed due to the listing being removed by the time of the second test. Removed listings are most likely to be deleted by landlords after finding a suitable tenant. Listings can also be removed by Craigslist if they are suspected to be fraudulent, but this should be rare in this test because I attempted to manually screen out clearly fraudulent listings. Because a landlord's decision to delete a listing is not random and could be affected by the treatment, these cases will remain in the analytical sample with the second email treated as a non-response. Removing two cases in which a property manager was inadvertently contacted multiple times, I am left with a final analytical sample of 890 inquiries (including 39 unsent emails counted as non-responses) and 445 listings. A breakdown of listings contacted by market is presented in Table 6.

Results

Before presenting response rates by orientation and market type and results of my regression analysis, I will briefly describe my results qualitatively. Results of this test differ starkly from recent American tests in terms of response rate: while Schwegman (2019) and Murchie and Pang (2018) reported overall response rates between 35 and 40 percent, 54.9 percent of my inquiries (489 of 890) were met with responses. There could be a number of explanations for this discrepancy. First, rural landlords appear to be more likely to respond to inquiries (see Table 7), although urban response rates were also higher than in previous tests (nearly 20 points higher, at 55%). Second, differences in testing protocol (i.e. the two-day gap between emails) may have influenced response rates. Third, market conditions may have changed since these tests were conducted in late 2016 and early 2017. At the time of the present test, unemployment skyrocketed as a result of the coronavirus outbreak and associated business closures. Landlords may have felt additional pressure to fill units as potential renters may have been impacted by layoffs. Even those with steady employment are likely to avoid moving during the crisis if possible, as shown by early data from the initial COVID-19 outbreak in China as well as the 2003 SARS outbreak in Hong Kong suggesting that housing-related transactions decrease dramatically during pandemics (Gudell, 2020). However, the present results are consistent with response rates found in the pretest, which occurred largely before businesses began to close and restrictions on activity went into place in the U.S.

Unlike the pretest results, full test results showed little difference in response rates between first and second inquiries: 58.4% of first inquiries and 51.5% of second inquiries[6] were answered (compared to 74% and 51% respectively in the pretest). This could indicate that economic fallout from the coronavirus outbreak slowed down the pace of lease signings such that more apartments were still unleased after two days, or it may simply reflect differences between Minnesota markets and those in other states.

Response rates by orientation and market type

As a dependent variable, I measure an active positive response from a property manager, a binary variable (1 if positive response received and else 0). Here, both nonresponse and negative response are counted as negative, although I received so few negative responses (8 of 489 responses) that this distinction is insignificant. Automatic responses (those received within one minute of inquiry or obviously sent by a bot) are counted as nonresponses so as to count only those responses actively considered by a property manager. A comparison of baseline response rates by treatment (sexual orientation), market type, and state anti-discrimination legislation is provided in Table 7. This baseline comparison offers little evidence of strong discrimination against same-sex couples – overall response rates were similar for same-sex couples and opposite-sex couples. Unlike previous research, results show that same-sex male couples have the highest response rate of the four treatment groups, although this rate is roughly equal to that of the two female sender groups. Interestingly, same-sex female couples received responses least often of all four treatments in rural areas, while opposite-sex couples with male senders (who have the lowest response rates overall) fared best there. Same-sex male couples fare slightly less well than men in opposite-sex relationships in rural areas, but better in urban areas. However, these differences are relatively small, thus the test finds roughly equal response rates for all groups.

Table 8 breaks down response rates at the property manager level rather than the inquiry level. This offers a better assessment of discrimination, as it identifies landlords that chose to respond only to opposite-sex couples and not same-sex couples. Again, these results show little convincing evidence of discrimination overall, although rates of discrimination do differ between urban and rural areas. In urban markets, same-sex couples even received responses slightly more often than opposite-sex couples, although this difference is not significant, as confirmed by a McNemar paired difference in proportions test. A similar breakdown by state anti-discrimination legislation status (not presented here) produced similar results, indicating that the behavior of most property managers in my sample was not affected by the treatment or by state-level anti-discrimination laws.

To test whether these differences constitute a true null effect or merely a small, insignificant effect, I apply the two one-sided tests (TOST) equivalence testing procedure to the figures reported in Table 7 (Lakens, 2017). Even though the differences in response rates are small, these test results show that I can reject the null hypothesis overall, as there are still significant differences between response rates for same-sex and opposite-sex couples. Breaking this down into groups, there is also a significant difference between response rates in both rural and urban areas, states with and without anti-discrimination legislation, and among male senders. Interestingly, in all of these cases except rural areas, same-sex couples actually receive responses slightly more often than opposite-sex couples. I find null effects in only one group: differences in response rates between same-sex and opposite-sex female senders are insignificant and statistically equivalent to zero. Overall, these results suggest that male same-sex couples may receive very slight preferential treatment in urban markets and are slightly more likely to experience discrimination in rural markets.

Linear probability model

As in Schwegman (2019), I next use a linear probability model (LPM) to estimate the effects of sexual orientation and rural community on housing discrimination. The data in this study is particularly well-suited to the use of an LPM, as it uses a binary outcome variable and the data collection process used here is unlikely to generate predictions outside the unit interval. I begin with a simple LPM (Model 1) modeling probabilities of response (y_{il}) given an applicant i and landlord l with landlord fixed effects (λ_l), as in the following equation.

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[6] The rate for second inquiries is even higher (56.4%) when excluding the 39 listings that were deleted before the second test.

Table 6

Listings contacted by state and market type.

State:	CA	CO	IL	KS	MD	MI	NC	NY	OH	OR	PA	TX	VA	WI	Sum
Urban	13	26	23	17	10	17	15	9	14	18	17	18	13	13	**223**
Rural	13	26	23	17	10	17	15	9	14	18	16	18	13	13	**222**
Total	26	52	46	34	20	34	30	18	28	36	35	36	26	26	**445**

Table 7.

Baseline response rates by sexual orientation, market type, and state legal protections. Baseline rates are calculated by dividing the number of responses by the number of inquiries in each category. $N = 890$ inquiries.

Treatment	N	Overall	Urban	Rural	State protections	No state protections
Opposite-sex, male sender	227	52.9% [120]	45.2% [57]	62.4% [63]	54.7% [64]	50.9% [56]
Same-sex, male	227	55.9% [127]	54.8% [69]	57.4% [58]	58.1% [68]	53.6% [59]
Opposite-sex, female sender	218	55.5% [121]	54.6% [53]	56.2% [68]	55.1% [59]	55.9% [62]
Same-sex, female	218	55.5% [121]	55.7% [54]	55.4% [67]	56.1% [60]	55.0% [61]

Table 8.

Net discrimination by market type and sexual orientation. Right four columns as percentages of inquiries met with at least one response. Difference between urban and rural proportions (opposite-sex vs. same-sex) not significant (McNemar paired difference in proportions test result: $p = 0.305$, $\chi^2 = 0.5808$). $N = 445$ listings.

Market type	At least one response	Both couples	Only opposite-sex	Only same-sex	Net discrimination
Urban	69.1% [154]	35.4% [79]	13.9% [31]	19.7% [44]	−5.8%
Rural	73.4% [163]	41.9% [93]	17.1% [38]	14.4% [32]	2.7%

$$y_{il} = \beta_0 + \beta_1 \text{SameSex}_i + \lambda_l + \varepsilon_{il} \qquad (1)$$

The value $SameSex_i$ is a dummy variable equal to 1 if applicant i is a same-sex couple, or 0 if an opposite-sex couple. Thus, β_1 is the differential response rate for the same landlord l, such that 0 represents no discrimination between couples and −0.5 would represent a 50% reduction in the likelihood of a positive response for a same-sex couple. In Table 9, I present the results of running separate regressions for all couples, those in rural or urban areas, those in states with and without anti-discrimination legislation, and those with male or female senders. Given the very similar response rates across groups reported in Table 7, it is unsurprising that there are no significant results and coefficients are fairly close to zero, presenting no strong evidence for discrimination.

Next, I investigate whether market characteristics play a role in the rate of housing discrimination, with a particular focus on community type (rural or urban) and state-level anti-discrimination legislation. I will again employ linear probability models for ease of interpretation, such that the result will show the response rate differential of sexual

minorities receiving responses from property managers in rural and urban settings or in states with or without state-level anti-discrimination legislation, in percentage points. I employ the following models for these analyses, also adapted from Schwegman (2019).

$$y_{ilm} = \beta_0 + \beta_1 \text{SameSex}_i + \beta_2 \text{Rural}_m + \beta_3 (\text{SameSex}_i * \text{Rural}_m) + \varphi + \text{Edu} + \text{Inc} + \text{Over65} + 2016\text{Vote} + \varepsilon_{ilm} \qquad (2)$$

$$y_{ilm} = \beta_0 + \beta_1 \text{SameSex}_i + \beta_2 \text{Legis}_m + \beta_3 (\text{SameSex}_i * \text{Legis}_m) + \varphi + \text{Edu} + \text{Inc} + \text{Over65} + 2016\text{Vote} + \varepsilon_{ilm} \qquad (3)$$

In Model 2, $Rural_m$ represents a binary variable reflecting whether a listing is located in a rural or urban market, and in model 3, $Legis_m$ reflects whether a listing is located in a state with or without anti-discrimination legislation. Thus, β_2 represents the effect of these characteristics on response rates, and β_3 represents the interaction of these characteristics and a same-sex applicant. These models include fixed effects for state-level protections in Model 2 and community type in Model 3, represented by ϕ. I cluster standard errors at the property manager level using the REGHDFE package in Stata (Correia, 2016).

Other variables shown in the model represent several additional variables based on community demographics taken from the five-year estimates of the U.S. Census Bureau's 2013–2018 American Community Survey, which I use to examine the role of variance in local characteristics other than community type or legislation in response rates, a method similar to that used by Jacquemet and Yannelis in their labor market correspondence test in the Chicago area (2012). Note that these variables are not needed to produce unbiased estimates due to this study's randomized design, but they may nonetheless help to increase the explanatory power of the model. These are: educational attainment (percentage with a bachelor's degree or higher), mean income, and percentage of adults over 65 years of age. Because political party affiliation is also an established predictor of anti-LGBT prejudice (Herek and Capitanio, 1996), I will also include election results from the 2016 U.S. Presidential election, as provided by the MIT Election Data & Science Lab (2017). (These values are presented by market in Table 2.) However, the exact reporting of these variables is complicated by the imprecise geography of Craigslist markets. Here, I assume that the urban markets roughly correspond to their corresponding Metropolitan Statistical Area. In rural areas, I select data for the counties containing a majority of the listings found, acknowledging that a few listings may still fall outside this area. While I had initially also planned to include binary variables indicating which script was used and the order of the emails (whether first or second inquiry), rates of response were so similar across these categories (scripts 56.9% vs. 53.0%, order 58.4% vs. 51.5%) that they did not significantly impact results.

Results from these analyses are presented in Table 10. Here, I again find small effect sizes and nosignificant results. Overall, there is no

Table 9.

Differential response rates by market type and sex of sender. Standard errors (in parentheses) are clustered at the unit/landlord level.

Model 1	All couples	Urban areas	Rural areas	No state protection	State protection	Male sender	Female sender
Sexual orientation	.0157 (0.0271)	.0583 (0.0388)	−0.0270 (0.0378)	.0090 (0.0385)	.0223 (0.0382)	.0308 (0.0377)	−0.0000 (0.0391)
P	0.562	0.134	0.475	0.815	0.560	0.414	1.000
N (units)	445	223	222	221	224	227	218

Table 10.
Differential response rates by market type and demographic characteristics. Standard errors (in parentheses) are clustered at the unit/landlord level. * $p <$ 0.1, ** $p <$ 0.05, *** $p <$ 0.01.

	Model 2 (LPM)	Model 3 (LPM)	Model 2 (Logit)
Same-sex couple	.0583 (0.0389)	.0090 (0.0386)	.0614 (0.1357)
Rural market	.1370 (0.0898)	–	.6788** (0.3121)
Same-sex couple*rural market	−0.0853 (0.0543)	–	−0.3468 (0.2713)
State-level protection	–	.0304 (0.0563)	–
Same-sex couple*state-level protection	–	.0133 (0.0544)	–
Female sender	−0.0023 (0.0392)	−0.0023 (0.0392)	−0.0140 (0.1376)
Population over 65 (%)	−0.0149 (0.0103)	−0.0149 (0.0103)	−0.0586 (0.0379)
Population with bachelor's degree (%)	.0027 (0.0060)	.0027 (0.0060)	.0092 (0.0210)
Mean household income (in $1,000)	−0.0059* (0.0033)	−0.0059* (0.0033)	−0.0209* (0.0111)
Trump vote share, 2016 election (%)	−0.0040 (0.0026)	−0.0060 (0.0026)	−0.0193* (0.0098)
N (inquiries)	890	890	890

evidence that response rates vary between rural and urban markets, or states with and without anti-discrimination legislation. Additionally, none of the additional market variables added in Models 2 and 3 had a highly significant result on response rates. Only one variable had a marginally significant impact on response rates: an additional $1000 mean household income in a market results in a 0.6% lower response rate (at a significance level of 0.08). The difference in response rates in more affluent markets may reflect that these markets are simply more competitive, or that landlords in these markets were more reluctant to respond to inquiries written in a "low-class" style as used in this test. In an additional analysis not presented here, interacting these additional variables with the binary *SameSex* variable did not significantly affect the result.

To test the robustness of my results, I also ran alternate specifications of all above models using logistic random effects regressions with random intercepts, as previously used by Auspurg, Hinz, and Schmid (2017) in their test of ethnic discrimination in the German housing market. Results for the logit version of Model 2 (including the $Rural_m$ variable and $Rural_m*SameSex_i$ interaction) are reported in Table 10. Signs and levels of significance remained generally similar to LPM results, offering evidence that these findings are fairly robust to changes in model specification. The only notable differences are that rural markets predicted considerably higher response rates (but the interaction of rural market and same-sex couple was not significant) and support for Donald Trump in the 2016 election also predicted slightly lower response rates in this model (at a significance level of exactly 0.05). Higher overall response rates in rural markets are consistent with lower market demand in these markets, making landlords more responsive to the smaller number of inquiries they receive.

Discussion

In the present study, I conduct a test of housing discrimination against same-sex couples in rural and urban markets in twenty-eight rural and urban markets in the United States. Results indicate that same-sex couples in my test were roughly equally likely to receive positive responses as opposite-sex couples when inquiring about housing availability in both rural and urban areas. There is even some very slight evidence that same-sex couples receive preferential treatment in urban markets, although this effect is very small. This result runs contrary to my expectations, and to previous research findings that same-sex male couples face higher rates of housing discrimination relative to same-sex female or opposite-sex couples. Additionally, I expected same-sex male

couples in rural markets to face even stronger discrimination due to less positive attitudes toward the LGBT community. However, my test revealed no significant differences in response rates between rural and urban areas. These results also do not show a significant difference in response rates between markets with anti-discrimination ordinances and those without (like previous findings by Schwegman (2019)), casting some doubt on their effectiveness in reducing levels of overall discrimination.

However, this work has several limitations. Perhaps the most important of these is that testing was conducted during the 2020 outbreak of the COVID-19 pandemic. Given that the response rates in my test were considerably higher than rates reported in recent tests, it is possible that reduced demand for housing prompted landlords to respond more often than they otherwise would. It is thus still possible that landlords view same-sex couples as tenants of last resort and would respond differently under normal circumstances. Further testing after the outbreak will be needed to assess whether the results presented here are truly indicative of a change in the prevalence of discrimination, or if they only represent conditions in this crisis scenario. If these findings are limited to the pandemic, this finding may provide some evidence for taste-based discrimination, as it suggests that landlords might choose not to discriminate when the cost of that discrimination (i.e. the risk of their unit being left vacant) is high.

These findings are also limited to a particular subset of the LGBT community: same-sex couples with typically Anglo-Saxon names. These results cannot be seen as representative of single LGBT individuals, or those with racially or ethnically distinctive names. Existing research provides some initial evidence that black same-sex male couples face particularly high rates of discrimination (Schwegman, 2019), but further studies should test this in rural markets as well. Additionally, the present research did not examine discrimination toward transgender individuals, another group that may face discrimination by property managers, as suggested by a recent study of discrimination against gender-non-conforming individuals in the Boston area (Langowski et al., 2017). Studies investigating the extent of discrimination against transgender individuals will likely need to employ an in-person audit design due to the difficulty of signaling transgender status in written correspondence. This would be another worthy topic for future study and may also vary between urban and rural communities due to differences in contact and attitudes.

In-person audit testing also provides another significant advantage over correspondence testing, in that it can evaluate discrimination throughout the housing search process. Due to its design, a correspondence test can only detect discrimination at the first step of the housing search: the initial inquiry. It is however possible that landlords still respond at the initial inquiry so as not to appear homophobic, or due to laws banning outright discrimination, while still choosing heterosexual applicants later in the process. In-person meetings may also make the applicant's sexual orientation clearer or more salient. Further research is needed to evaluate the full extent of discrimination against LGBT people throughout the housing search process.

Finally, this study only studied discrimination in twenty-eight markets and may not represent the extent of discrimination across the U.S., or in international markets. Though I have attempted to choose sites representative of nonmetropolitan areas in the United States, the practical needs of choosing sites with a suitable number of listings may make the results unrepresentative of rural America overall. I have also selected those in which Craigslist is a more popular source of housing listings, whereas property managers in some communities may use other means of offline advertisement, like "for rent" signs or local newspapers. Site selection also poses an additional challenge in estimating the effects of non-discrimination ordinances. While I have presented results for sites in seven states with and seven without state-level protections, local protections exist on a patchwork basis over many of the regions studied. It is outside the scope of the present research to evaluate the effect of these local ordinances, but this represents another possibility for future

research.

As one of the first studies to apply a correspondence test design in rural housing markets, this work also offers some methodological insights for future research. The greatest challenge in adapting a correspondence test to a rural market is avoiding detection. Spreading out paired inquiries over three days (such that each landlord was contacted at most once per day, including responses) seems to have effectively minimized this risk, although response rates may differ in different market conditions. While the purpose of the study was never obviously detected in the present work, two landlords sent suspicious replies after receiving similar inquiries. In both cases, the same landlord had received multiple tests despite painstaking efforts to review each listing, as it can be difficult to assess whether two listings were posted by the same person if no name or contact information is included. The risk of duplication is not limited to rural markets but is especially great there, as a small number of landlords may control a large portion of local listings. I would thus recommend that researchers take several steps to further reduce the risk of detection in future studies. First, using a greater number of aliases and scripts will help to limit the risk of detection even if a landlord is accidentally contacted multiple times, although care must be taken to ensure that these aliases and scripts are seen as equivalent. Additionally, testing over a longer period of time and/or testing in a larger number of markets (thus spreading out inquiries) may also help to lower risks. Selected markets should also be observed before beginning the study to assess (to the extent possible) whether a small number of agents dominate the market.

Overall, the present work seeks to identify the challenges same-sex couples face in finding homes, especially the approximately 3 million LGBT Americans living in rural communities. While previous work established clear evidence of anti-LGBT bias on the housing market, this study is the first to examine the extent of this discrimination in nonmetropolitan communities. The results presented here are encouraging: same-sex couples were treated largely the same as opposite-sex couples in both urban and rural markets. However, I note that this is far from a conclusive result, and it is much too soon to declare victory against anti-LGBT housing discrimination. Only further testing will clarify whether the effects found here were the result of a unique global crisis or whether they represent the continuation of a nationwide shift toward increased acceptance.

Declaration of Competing Interest

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