An Empirical Analysis of Sexual Orientation Discrimination

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AN EMPIRICAL ANALYSIS OF SEXUAL ORIENTATION DISCRIMINATION

J. Shahar Dillbary and Griffin Edwards *

ABSTRACT

This study is the first to empirically demonstrate widespread discrimination across the United States based on perceived sexual orientation, sex and race in the mortgage lending process. Our analysis of over five million mortgage applications reveals that any FHA loan application filed by same-sex male co-applicants is significantly less likely to be approved compared to the white heterosexual baseline (holding lending risk constant). The most likely explanation for this pattern is sexual orientation based discrimination—despite the fact that FHA loans are the only type of loan in which discrimination on the basis of sexual orientation is prohibited.

Moreover, we find compelling evidence to support the intersectionality theory. According to this theory when sex and race unite, a new form of discrimination emerges that cannot be explained by sexism and racism alone. The data unequivocally indicates that the race and sex of same-sex applicants play a role and result in a unique and previously unobserved pattern. This discriminatory pattern plagues every region in the U.S., and it transcends party lines (i.e., it is present in red, blue and swing states). Furthermore, upending conventional wisdom, the data reveals that big banks discriminate at the same rate as small banks, and lenders in urban environments are as discriminatory as rural lenders. Prior studies failed to reveal this phenomenon due to data constraints and design flaws. These studies relied on testers posing as applicants, and none could investigate how intersectionality influences lending practices.

Despite the grim results, a silver lining exists. We find that the pattern of discrimination diminishes or disappears in states and localities that pass anti-sexual orientation discrimination laws. These findings have important and timely implications. In 2017 a new bill offering nationwide protection from sexual orientation credit discrimination was introduced. The same year has experienced tectonic changes in Title VII jurisprudence. Our study can reinvigorate the debate and help policy makers tailor remedies that would correct the discriminatory pattern this study unravels.

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I. INTRODUCTION

Twenty years ago, a gay couple entered their local bank in Arroyo Grande, California to ask for a mortgage loan. Excited, they filled out the application. But the festive event took a surprising turn. The couple was quickly requested to leave and even close their existing accounts. “It was bank policy,” so they learned, “not to offer home loans to gay applicants.”

While recent years brought more legal protections to members of the lesbian, gay and bisexual (LGB) community, our data suggests that they should not expect to be treated equally. This should not come as a surprise. Federal law and the majority of states do not prohibit lenders from discriminating against applicants based on their sexual orientation. Simply put, when it comes to mortgage lending, sexual orientation discrimination is the rule.

Not only is explicit sexual orientation discrimination permitted, it can be

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1 Telephone Interview with Ms. Renee Spears (on file with authors) (Dec. 30, 2017); http://loans.org/mortgage/articles/gay-discrimination-housing-industry (last checked Jan. 31, 2018).

2 See e.g., Obergefell v. Hodges, 135 S. Ct. 2584 (2015) (holding the states cannot ban same-sex marriage); infra note 217 and accompanying text.

3 Note that the “T” for transgender individuals is omitted. The reason is that courts have interpreted the prohibition against sex discrimination to also include discrimination against transgender applicants, or more broadly, gender identity discrimination. See infra Part II.A.1.
used by lenders as a “defense.” This defense is often raised when the mortgage applicant belongs to a protected group. For example, a lender who discriminated against a black applicant could escape liability if it shows that the source of discrimination was not the applicant’s race (a protected characteristic that gives rise to liability), but his sexual orientation. To be blunt, the bank can claim: “I discriminated against the applicant not because he was black, but because he was gay.”

There are a few exceptions. A small (but growing) number of states now prohibit sexual orientation discrimination in mortgage lending. Even in states where such discrimination is permissible, some strongholds exist: certain localities decided to prohibit what federal law and their state allow. For example, Michigan does not prohibit sexual orientation discrimination in mortgage lending, but the city of Ann Arbor does. The same is true for Atlanta, the only municipality in Georgia to protect LGB individuals. By contrast, two states, Arkansas and Tennessee, prohibit any local legislation that would protect against sexual orientation discrimination. In these states, the legality of sexual orientation discrimination is the law of the land. Finally, lenders of mortgages insured by the Federal Housing Administration, known as “FHA loans,” are not allowed to discriminate based on sexual orientation. But as the data reveals, sexual orientation discrimination—even in FHA loans—not only exists, but is prevalent.

In what follows, we present the first econometric evidence of widespread bias in mortgage lending on the basis of perceived sexual orientation. Using data provided by the Home Mortgage Disclosure Act (HMDA), we evaluate the probability of home loan acceptance for virtually every FHA loan between the years 2010 and 2015. The dataset is unique in a number of respects. First, it is large, containing more than five million observations. This allows us to show that the discrimination is widespread, statistically significant and robust. Second, the dataset is rich enough to allow us to estimate acceptance rates for perceived gay couples of all gender and racial compositions (e.g., applications filed by two black

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4 We use the term “defense” here loosely to mean that the defendant was able to stop the plaintiff from proving her case. By contrast, an affirmative defense “is asserted only after the plaintiff establishes a prima facie case . . . against the defendant.” Davenport v. Cotton Hope Plantation Horizontal Prop. Regime, 508 S.E.2d 565, 571 (1998) (comparing the defenses of implied primary and implied secondary assumption of risk).

5 See infra notes 58-64 and accompanying text.

6 ANN ARBOR, MICH., ORD. NO. 14-25 (defining discrimination as making a decision based on one’s or “associates’ actual or perceived . . . sexual orientation” and prohibiting discrimination in lending).

7 ATLANTA, GA., ORD. NO. 28-104 (prohibiting businesses engaged in residential real estate related transactions from discriminating on the basis of actual or perceived sexual orientation).

8 See Ark. Code Ann. § 14-1-403 (“A county, municipality, or other political subdivision of the state shall not adopt or enforce an ordinance, resolution, rule, or policy that creates a protected classification or prohibits discrimination on a basis not contained in state law.”); Tenn. Code Ann. § 7-51-1802 (including a similar prohibition).

males, two white males, a white male and a black male, two black females, etc.). Lastly, it has a geographical level of granularity that allows us to examine small geographic areas—up to a neighborhood level.

The results are alarming. We find that same-sex male co-applicants (or pairs) are between 2.5 and 7.5 percentage points less likely to have their loan application accepted compared to the white heterosexual baseline. This is true despite the fact that the same-sex male pairs were identical in all reported respects to the heterosexual baseline. That is, same-sex male pairs filed a mortgage application with the same lender, in the same county, for the same loan amount, for the same purpose, had the same income, and posed the same level of risk to the lender. Nevertheless, discrimination rules. The results are statistically significant at the 99% level.

Moreover, we find compelling evidence to support the intersectionality theory. According to this theory when sex and race unite, a new form of discrimination emerges that cannot be explained by sexism and racism alone. The data unequivocally indicates that in addition to sex and sexual orientation, race also plays a significant role. The result is a unique and previously unobserved pattern. Although applications of all same sex male pairs are less likely to be accepted, male pairs with black applicants are substantially worse off. From most to least discriminated groups are (i) pairs consisting of two black males (denoted black male/black male), followed by (ii) pairs consisting of interracial pairs of black male/white male, (iii) interracial pairs consisting of white male/black male, and finally (iv) pairs consisting of two white males. The differences are significant. An application filed by a pair of two black males is three times less likely to be accepted compared to an application filed by a pair of two white males, and both pairs face discrimination compared to the heterosexual baseline.

Consistent with the social science literature, the data suggests that perceived gay male couples are treated differently than perceived lesbian couples. While every possible racial combination of same-sex male co-applicants is statistically disadvantaged, the treatment of same-sex female co-applicants is either indistinguishable or preferable compared to the white heterosexual baseline couple. Interestingly, however, we observe the exact same racial pattern as in the male pairs: within the female pair group, a pair of two black females is the least

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10 In a joint mortgage application filed by two individuals, one is listed as the “primary” applicant and the other as a “secondary” applicant. In our baseline, the white male is the primary applicant and the white female is the secondary applicant—the most common combination in the dataset.

11 Kimberle Crenshaw, *Demarginalizing the Intersection of Race and Sex*, 1989 U. CHI. LEGAL F. 139, 140 (explaining how courts’ failure to understand and properly analyze intersectional claims can leave subsets of protected groups—e.g., black females—without a Title VII remedy, and warning that “[b]ecause the intersectional experience is greater than the sum of racism and sexism, any analysis that does not take intersectionality into account cannot sufficiently address the particular manner in which Black women are subordinated”); Kimberle Crenshaw, *Mapping the Margins: Intersectionality, Identity Politics, and Violence Against Women of Color*, 43 STAN. L. REV. 1241, 1242, 1244 (1991) (“exploring the various ways in which race and gender intersect in shaping structural, political, and representational aspects of violence against women of color.”).
likely to be approved, followed by interracial pairs of black female/white female, then white female/black female pairs, and finally white female pairs.

This pattern of discrimination is not isolated to a specific geographical region or political ideology. Rather, we find evidence that this form of discrimination transcends geographical and political borders. In all four regions in the U.S., applications of same-sex male pairs are less likely to be accepted (although in certain cases the results are statistically insignificant).\(^{12}\) Interestingly, we find that interracial male co-applicants (i.e., white/black and black/white) face the most discrimination in the Northeast. Their applications are 12.2 percentage points less likely to be accepted compared to the baseline (the results are statistically significant at the 99% level). Splitting the data by political affiliation does not change the results in a meaningful way. It reveals that Democratic states are as discriminatory as Republican states overall, and, in fact, are the least tolerant to interracial male pairs. The same trend also holds irrespective of the size of the lender. That is, big lenders discriminate in the same way as small banks. Using a difference-in-differences framework, we do find, however, that efforts by states and localities to pass laws prohibiting sexual orientation discrimination tend to be successful in discouraging sexual orientation discrimination.

The Article contributes to the economic and empirical research in a number of ways. First, it highlights a new dimension of discrimination that has been previously ignored. Surprisingly, of the very few studies that attempted to explore sexual orientation discrimination, to date only one study focused on mortgage lending.\(^ {13}\) The study compared the treatment of testers posing as heterosexual couples with testers posing as same-sex couples with better credentials. However, the study suffered from severe design limitations. Most importantly, the couples posing as heterosexual and same-sex were of the same race. This design did not allow the researchers to test how the intersectionality of race, sex and sexual orientation influence home lending practices. For example, the study could not analyze whether black and white couples are treated differently, or whether black female couples are treated differently than white female couples. It overlooked the existence and magnitude of intersectional discrimination and was unable to reveal the patterns we observe here.

Second, our study is also the first to measure the presence and magnitude of sexual orientation discrimination regarding mortgages that are subject to the Equal Access Rule\(^ {14}\)—the only type of mortgage where discrimination based on

\(^{12}\) See infra Part II.D.


sexual orientation is prohibited nationwide. Third, our study indicates that the prior literature underestimated the magnitude of sexual orientation discrimination. The reason is the failure of these studies to distinguish between same-sex male couples and same-sex female couples. The data suggests that the second group—female couples—is treated as well or more favorably compared to male couples and even compared to the heterosexual baseline. Thus, studies that treated LGB individuals as one homogenous group likely underestimated the discrimination faced by gay males.

The study also suggests that the observed discrimination is not motivated by lenders’ attempts to assess the risk associated with the applicants by segmenting the market. Rather, because we compare applications with the same level of risk to the lender, it is more likely that the discrimination is motivated by bigotry (conscious or otherwise). The distinction is important. To eliminate discrimination, policymakers—legislators and regulators—must know the motivating force.

Our study is timely. In May 2017, a new bill offering nationwide protection from sexual orientation credit discrimination was introduced. In the same year Title VII jurisprudence experienced a tectonic change when the Seventh Circuit held, for the first time, that sexual orientation discrimination is prohibited under Title VII. A month later, the same holding was adopted by a federal court in the Southern District of New York, and in February 2018 the First Circuit joined what now seems like a trend. Our study can help reinvigorate the debate and help policy makers tailor remedies that would correct the discriminatory pattern this study unravels.

The rest of the Article continues as follows. Part II first outlines the law and reveals the perverse results of the sexual orientation discrimination defense. It then discusses two important forms of discriminatory practices and how two common remedies—which we later test—may affect these practices. Part II then turns to review the prior studies and their shortcomings. Part III discusses the study’s methodology and the results. Part IV provides concluding remarks.

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16 Hively v. Ivy Tech Cmty. Coll. of Ind., 853 F.3d 339 (7th Cir. 2017) (discrimination on the basis of sexual orientation is illegal sex discrimination under Title VII).


18 Franchina v. City of Providence, No. 16-2401, 2018 WL 550511, *1 (1st Cir. Jan. 25, 2018). Id. at *13 (holding that a plaintiff may recover under a “sex-plus claim . . . where, in addition to the sex-based charge, the ‘plus’ factor is the plaintiff's status as a gay or lesbian individual.”).
II. SEXUAL ORIENTATION AND THE LAW

A. Federal Law

The two main federal statutes prohibiting discrimination in mortgage lending are the Fair Housing Act (FH-ACT) and the Equal Credit Opportunity Act (ECOA). The first focuses on residential real estate transactions while the second focuses more broadly on any credit transaction. Together, they make it unlawful for any lender to discriminate against a protected applicant by way of denying a mortgage or providing unfavorable terms or conditions. The federal statutes, however, are limited in scope: they prohibit discriminatory lending practices if they are based on race, color, religion, national origin, or sex. Although the ECOA and FH-ACT include other bases for discrimination, neither protects against discrimination on the basis of sexual orientation. The result is that lenders can discriminate against LGB individuals (or those perceived as such) with impunity. There are, however, a few exceptions.

1. Discrimination on the basis of sexual orientation may be illegal if it also violates the prohibition against discrimination against a protected class. An example is declining to give a mortgage to a gay applicant for fear of contracting HIV. Such behavior is illegal discrimination on the basis of disability—a protected characteristic under the FH-ACT. This protection includes not only actual physical impairment, but also “being regarded as having such an impairment.”

Similarly, courts have interpreted the prohibition against sex discrimination broadly to include discrimination based on gender identity or perceived gender non-conformity. As a result, the ECOA and FH-ACT now afford protection to transgender applicants, and, under certain circumstances, to LGB individuals. The leading precedent is Price Waterhouse v. Hopkins, a Title

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21 42 U.S.C. § 3605(a) (“It shall be unlawful for any person or other entity whose business includes engaging in residential real estate-related transactions to discriminate against any person in making available such a transaction, or in the terms or conditions of such a transaction, because of race, color, religion, sex, handicap, familial status, or national origin”); 15 U.S.C. § 1691(a). (“It shall be unlawful for any creditor to discriminate against any applicant, with respect to any aspect of a credit transaction on the basis of race, color, religion, national origin, sex or marital status, or age (provided the applicant has the capacity to contract”).
22 See supra note 21.
23 15 U.S.C. § 1691(a) (making it unlawful to discriminate based on marital status and age); 42 U.S.C. § 3605(a) (prohibiting discrimination based on familial status and handicap).
26 See infra notes 36-37 and accompanying text.
VII decision.27 *Price Waterhouse* involved a female plaintiff whose application to join the accounting partnership was put on hold. It was clear that her gender played a role in the employer’s decision.28 In addition to legitimate criticism, some of the plaintiff’s colleagues described her as “macho” and advised her to take a “course at charm school.”29 The head of her office—her biggest supporter30—was more explicit. He advised the plaintiff that to improve her chances she “should ‘walk more femininely, talk more femininely, dress more femininely, wear make-up, have her hair styled, and wear jewelry.’”31 In a plurality opinion, the Supreme Court held that discrimination on the basis of gender-based stereotypes constitutes illegal sex discrimination.32 The decision was later construed as protecting transgender plaintiffs. Indeed, if discriminating against women who do not wear dresses constitutes sex discrimination, “[i]t follows that employers who discriminate against men because they do wear dresses and make-up, or otherwise act femininely, are also engaging in sex discrimination.”33

*Price Waterhouse*’s holding and its progeny were adopted in the mortgage-lending context.34 But even after *Price Waterhouse*, sexual orientation remains an *unprotected* characteristic.35 Still, as with the case of disability,

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28 490 U.S. at 231.
29 Id. at 235.
31 Price Waterhouse, 490 U.S. at 235.
32 Id. at 250–51 (plurality opinion); see also id. at 258–61 (White, J., concurring); id. at 272–73 (O’Connor, J., concurring).
33 Smith v. City of Salem, 378 F.3d 566, 574 (6th Cir. 2004). The extent of the protection, however, is still unclear. Some courts took the view that discrimination against a transgender plaintiff is unlawful only when it is based on gender stereotypes. See Etsitty v. Utah Transit Auth., 502 F.3d 1215, 1221, 1224 (10th Cir. 2007) (holding that “transsexuals may not claim protection under Title VII from discrimination based solely on their status as a transsexual . . . [r]ather, [a transgender individual’s] claim must rest entirely on the *Price Waterhouse* theory of protection as a man who fails to conform to sex stereotypes,” and concluding that an employer is not “required . . . to allow biological males to use women's restrooms”); Smith, 378 F.3d at 574. Other courts have taken a more expansive approach arguing that discrimination against transgender individuals is prohibited per se. See Glenn v. Brumby, 663 F.3d 1312, 1316 (11th Cir. 2011) (“[a] person is defined as transgender precisely because of the perception that [their] behavior transgresses gender stereotypes.”); Mia Macy, EEOC DOC 0120120821, 2012 WL 1435995, at *11 (Apr. 20, 2012) (“[i]ntentional discrimination against a transgender individual because that person is transgender is, by definition, discrimination ‘based on . . . sex,’ and such discrimination therefore violates Title VII.”). This also seems to be the view of the CFPB. See CFPB Letter infra note 39 at *3-4 (relying on Title VII jurisprudence). For a review of the two approaches, see Title VII, 1 Sexual Orientation and the Law § 10:5 (2016).
34 Rosa v. Park W. Bank & Trust Co., 214 F.3d 213 (1st Cir. 2000).
discrimination against LGB individuals may be illegal if it is based on perceived non-conformity with gender stereotypes (a protected characteristic post-Price Waterhouse). This means that a gay male applicant who was wearing women’s clothing would have a valid cause of action if his application was denied because the loan officer thought he did not meet stereotyped expectations of masculinity. If, however, the lender could show that sexual orientation was the sole reason for the discrimination—i.e., the applicant was discriminated because the loan officer believed he was gay—the plaintiff’s suit would fail. Put differently, the LGB plaintiff cannot simply argue that she was discriminated because of her sexual orientation. Rather, she needs to show that the discrimination was based on a protected basis like sex stereotyping. As Part II.B. demonstrates, however, such a showing is often impossible.

2. Agencies Interpretation and Regulatory Enforcement. The Consumer Financial Protection Bureau (CFPB)—the agency responsible for enforcing and administering the ECOA—has taken a broader view than the federal courts. Contrary to Price Waterhouse, the CFPB’s Director opined in a letter issued in 2016 that sexual orientation discrimination is a form of sex discrimination. The opinion relied on two grounds: (a) recent decisions issued by the Equal Employment Opportunity Commission (EEOC) and (b) the theory of discrimination by association. In the mortgage lending context, the theory prohibits a loan officer from denying an applicant based on her association with a person belonging to a protected class. For example, the doctrine prohibits a lender from discriminating against a white applicant whose spouse is black. The

36 Price Waterhouse, 490 U.S. at 241–42. The plaintiff in these cases only needs to show that non-conformity with gender stereotypes was a consideration even if other considerations, like the plaintiff’s sexual orientation, were also considered. See, e.g., Centola v. Potter, 183 F. Supp. 2d 403, 409 (D. Mass. 2002) (“[Plaintiff] does not need to allege that he suffered discrimination on the basis of his sex alone or that sexual orientation played no part in his treatment.”).

37 Price Waterhouse, 490 U.S. at 241–42.


40 Id.

41 The doctrine was adopted already in 1985 by the Federal Reserve in its official interpretation of Regulation B. 50 Fed. Reg. 48,018, 48,019 (Nov. 20, 1989). It was reaffirmed by the CFPB in the restated Regulation B. 76 Fed. Reg. 79,442, 79,443 (Dec. 21, 2011); 81 Fed. Reg. 25,323, 25325 (Apr. 28, 2016); Official Staff Commentary, 12 C.F.R. pt. 1002, Supp. I, 2(z)-1 (“As used in this part, prohibited bases refers not only to characteristics—the race, color, religion . . . of an applicant . . . but also to the characteristics of individuals with whom an applicant is affiliated or with whom the applicant associates. This means, for example, that under the general rule stated in § 1002.4(a), a creditor may not discriminate against an applicant . . . because of the race of other residents in the neighborhood where the property offered as collateral is located.”); see also Interagency Policy Statement on Discrimination in Lending, 59 Fed. Reg. 18,266, 18,268 (Apr. 15, 1994) (“[A] lender may not discriminate on a prohibited basis because of the characteristics of: [a] person associated with the credit applicant.”).
CFPB’s Director took the stance that the same theory prohibits discrimination against applicants based on the sex of their partners and, therefore, prohibits sexual orientation discrimination.\(^{42}\) Despite the CFPB’s expansive view and its efforts to solicit complaints from consumers, it is unclear how active and effective the agency is in dealing with discriminatory practices.\(^{43}\)

3. *FHA Mortgage Insurance & The Equal Access Rule.* There is one category of loans in which sexual orientation discrimination is wholly prohibited and on which our study focuses: FHA-backed mortgages. The prohibition is articulated in the Equal Access Rule adopted in 2012 by the Department of Housing and Urban Development (HUD), the agency responsible for administering the FH-Act.\(^{44}\) The rule prohibits lenders of mortgages insured by the Federal Housing Administration (commonly referred to as “FHA loans”) from considering applicants’ actual or perceived sexual orientation, gender identity, or marital status. This means that a lender would be in violation of the Equal Access Rule if it denied an FHA mortgage because the applicant was (or was believed to be) gay.\(^{45}\)

The rule, however, is limited in scope and has—as our study shows—a limited effect. To begin with, FHA loans comprise a significant but still limited portion of the market. According to HUD’s Office of Risk Management and Regulatory Affairs, in 2015 FHA single-family home insurance measured by loan count was only 17.9%—its highest point since 2010.\(^ {46}\) That market share drops to 14.3% if measured by dollar volume.\(^ {47}\) The upshot is that the majority of loans are not subject to the Equal Access Rule.

Moreover, the rule does not provide applicants with a private cause of action.\(^ {48}\) As a result, the sole remedy available to applicants who believe the rule was violated is to complain to HUD.\(^ {49}\) Few complaints, however, are filed and processed every year, and even fewer result in a charge of discrimination.\(^ {50}\)

\(^{42}\) CFPB Letter *supra* note 39 at 4 (emphasis added).

\(^{43}\) Unlike HUD, however, the CFPB does not provide any information about the number of complaints alleging discriminatory behavior. A search on its database yielded only a handful of discrimination-related claims, none of which seem to be in the mortgage-lending context.


\(^{47}\) *Id.*

\(^{48}\) Equal Access Rule, *supra* note 14, at 5666 (refusing to extend to interpret the FH-Act as protecting against discrimination on the bases of sexual orientation and gender identity and explaining that the rule does not “create additional protected classes in existing civil rights laws”).


\(^{50}\) According to the National Fair Housing Alliance (NFHA), in 2016 only 28,181 complaints were filed nationwide for housing discrimination. Of these, 4.86% were processed by HUD, and a meager number of 150 complaints (0.01%) included sexual orientation discrimination claims. The NFHA’s reports are available at [http://nationalfairhousing.org/reports-research/](http://nationalfairhousing.org/reports-research/) (last visited Jan. 31, 2018) (last visited Jan. 31, 2018).
B. Sexual Orientation Discrimination as a Defense

Not only is sexual orientation discrimination permissible, but it can also serve as a “defense.” The reason is the law of causation. In a discrimination case, the plaintiff has to show that the lender considered an illegitimate motive (e.g., the applicant’s race). In addition, the plaintiff must prove that the illegitimate motive was the cause of the discriminatory decision. Despite burden shifting frameworks, meeting the causation requirement is not easy. For members of the LGB community, it may be impossible.

To illustrate, consider an African American male with perfect credit whose application was refused. Suppose also that he came to the lender dressed in what some would consider a feminine attire. Here, the basis for the discriminatory action is unclear. It could be that the applicant was discriminated against because of his sex (male), his race (black), or his perceived gender identity (failing to meet stereotyped expectations of masculinity as a cross-dresser). In any of these cases, the applicant has a valid cause of action, but the lender may have a defense. It could be argued that the discrimination was based on the applicant’s actual or perceived sexual orientation (being gay or perceived as gay). Here, the question of the lender’s motive is imperative. If the sole reason for denying the application was an illegal consideration—for example, the male applicant’s effeminate dressing style—the plaintiff would prevail. In such a case, the denial is considered impermissible sex discrimination because it is based on the applicant’s non-conformity with sex stereotypes. By contrast, if the sole motivation for rejecting the application is the loan officer’s belief that the applicant is gay, the consideration is deemed “legitimate” and permissible.

Finally, suppose that the loan officer’s motivation was “based on a mixture of

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51 See supra note 21.
52 When a single motive guided the defendant’s decision, courts often apply the burden-shifting approach established in McDonnell Douglas Corp. v. Green, 411 U.S. 792 (1973); see also Husman v. Toyota Motor Credit Corp., 12 Cal. App. 5th 1168 at 1182-83 (Cal. Ct. App. 2017) (explaining that in mixed-motives cases, courts apply the Price Waterhouse framework). Under the McDonnell Douglas test, the plaintiff must first prove a discriminatory decision and offer facts suggesting the decision was based on an illegitimate motive. The burden then shifts to the defendant to show that the action had a legitimate motive. If the defendant meets the burden, the plaintiff must show that the lender’s reason is only pretextual or provide evidence of intentional discrimination. See also Dee Pridgen & Richard M. Alderman, Consumer Credit and the Law § 3:16 (2016) (noting that with the exception of the Seventh Circuit, “proof of discrimination in ECOA ‘disparate treatment’ suits will be subject to the analysis of McDonnell Douglas, as is the case with other discrimination claims, such as Title VII.”); Ring v. First Interstate Mortg., Inc., 984 F.2d 924, 926 (8th Cir. 1993) (“We have no doubt that the three-stage McDonnell Douglas/Burdine analysis applies to Fair Housing Act cases.”).
53 See also Price Waterhouse, 490 U.S. at 251 (noting that mere remarks based on sex stereotypes do not prove that gender considerations guided the challenged decision; rather, “[t]he plaintiff must show that the [defendant] actually relied on her gender in making its decision.”).
54 The example is based on Rosa, F.3d at 213; see infra notes 58-64 and accompanying text.
55 See, e.g., Husman, 12 Cal. App. 5th at 1182-83 (discussing the burden shifting procedures in single-motive and mixed-motives cases).
legitimate and illegitimate considerations.”56 In these cases, the lender can still avoid paying damages if it proves that the legitimate motive alone (e.g., denying the application because the applicant was perceived as gay) would have led it to make the same decision (i.e., denying the application).57

If this sounds too fantastic, consider Rosa v. Park W. Bank & Trust.58 In Rosa, a bank employee refused to give the plaintiff (Rosa), a transgender male wearing “traditionally female attire,” a loan application unless he “went home and changed.”59 Rosa brought an ECOA suit against the bank, claiming that requiring him to conform to gender stereotypes was a form of sex discrimination. The district court granted the bank’s motion to dismiss.60 Relying on Title VII jurisprudence and Price Waterhouse, the First Circuit reversed.61 It held that Rosa had a valid cause of action if the bank treated “a woman who dresses like a man differently than a man who dresses like a woman.”62 Such disparate treatment based on gender stereotyping would be considered discrimination on a prohibited basis: sex. By contrast, if the loan officer refused Rosa because he thought Rosa was gay, Rosa would have no federal cause of action.63 The ECOA—like the FH-

56 Price Waterhouse, 490 U.S. at 241.
57 Id. at 252 (holding that the defendant “must show that its legitimate reason, standing alone, would have induced it to make the same decision”). Under Price Waterhouse, such a showing fully exempted the defendant from liability. Id. This part of the decision, however, was short-lived. Two years later, Congress passed the Civil Rights Restoration Act of 1991, which severely limited the remedies available to a Title VII plaintiff but stopped short of immunizing the defendant from liability. See Harris v. City of Santa Monica, 56 Cal. 4th 203, 219–20 (2013) (“When an individual ‘proves a violation’ of Title VII and the [defendant] shows it ‘would have taken the same action in the absence of the impermissible motivating factor,’ a court can ‘grant declaratory relief, injunctive relief . . . , and attorney’s fees and costs’ directly attributable to the Title VII claim but ‘shall not award damages or issue an order requiring any admission, reinstatement, hiring, promotion, or payment . . . .’”) (quoting 42 U.S.C. § 2000e-5(g)(2)(B); Husman, 12 Cal. App. 5th 1168 (citing Harris with approval and noting that mixed motives cases are not infrequent). Given courts’ inclination to follow Title VII’s jurisprudence in ECOA and FH-Act cases, it is likely that the limited protection would also be applied in mortgage lending cases.
58 Rosa, 214 F.3d 213.
59 Id. at 214.
60 Id.
61 Id. at 216.
62 Id. at 215–16 (citing Price Waterhouse, 490 U.S. at 251 (“stereotyped remarks [including statements about dressing more ‘femininely’] can certainly be evidence that gender played a part.”)).
63 Id. The court reviewed the following possibilities:

It is reasonable to infer [a] that Brunelle[, the loan officer,] told Rosa to go home and change because she thought that Rosa’s attire did not accord with his male gender . . . If so, the Bank concedes, Rosa may have a claim . . . [b] that Brunelle refused to give Rosa the loan application because she thought he was gay, confusing sexual orientation with cross-dressing. If so, Rosa concedes, our precedents dictate that he would have no recourse under the federal Act . . . [c] that Brunelle simply could not ascertain whether the person shown in the identification card photographs was the same person that appeared before her that day. If this were the case, Rosa again would be out of luck. [d]
Act and other Titles of the Civil Rights Act—does not prohibit sexual orientation discrimination.\textsuperscript{64}

The sexual orientation defense carries a number of perverse consequences. First, it helps explain why discriminatory incidents are under-reported. The reason is that the defense allows defendants to put the sexual orientation of the plaintiff on trial—even when the plaintiff’s case relies solely on protected bases and even if the plaintiff is not a member of the LGBT\textsuperscript{65} community. For example, the black plaintiff who sues a lender for racial discrimination may worry that she will need to defend herself against the claim that her perceived sexual orientation was the real reason for the discrimination. As a result, plaintiffs who have a valid cause of action may avoid litigating in the first place. This is true for all types of victims, including heterosexual applicants who belong to a protected class.

Second, LGB individuals who do not feel comfortable disclosing their sexual orientation may avoid filing discrimination suits for fear that they will beouted, or simply because they do not feel comfortable putting their sexual orientation on trial.

Third, LGB individuals who are willing to disclose (or avoid hiding) their sexual orientation should think twice. If they do disclose their sexual orientation, they increase the risk that a court will treat their sex stereotyping claims as masking meritless sexual orientation allegations. \textit{Dawson}, a case involving an openly gay male, is such an example. The court was concerned that the plaintiff was merely trying to use a “gender stereotyping claim to bootstrap protection for sexual orientation into [the statute].”\textsuperscript{66} It explained that “[w]hen utilized by an avowedly homosexual plaintiff . . . gender stereotyping claims can easily present problems for an adjudicator.”\textsuperscript{67} The \textit{Dawson} court solved the “problem”—a suit filed by a gay plaintiff—by dismissing the case. By contrast, in \textit{Centola} the plaintiff “never disclosed his sexual orientation to anyone at work.”\textsuperscript{68} Based on this repeated and much-emphasized fact,\textsuperscript{69} the court concluded that the

\textsuperscript{64} \textit{Id.}

\textsuperscript{65} Our focus is on members of the LGB community. \textit{See supra} note 3. However, we refer to LGBT individuals when the discussion is also relevant to transgender individuals.

\textsuperscript{66} \textit{Dawson} v. Bumble \& Bumble, 398 F.3d 211, 218 (2d Cir. 2005).

\textsuperscript{67} \textit{Id.} (emphasis added). For other Title VII decisions accepting the defendant’s sexual orientation discrimination defense, see \textit{Vickers} v. Fairfield Med. Ctr., 453 F.3d 757, 763 (6th Cir. 2006) (citing \textit{Dawson} with agreement); \textit{Simonton} v. Runyon, 232 F.3d 33, 38 (2d Cir. 2000) (noting that the “[plaintiff]’s sexual orientation was known to his co-workers” and holding that \textit{Price Waterhouse}’s sex stereotyping theory “would not bootstrap protection for sexual orientation into Title VII because not all homosexual men are stereotypically feminine, and not all heterosexual men are stereotypically masculine”); \textit{Ayala-Sepulveda} v. Municipality of San German, 661 F. Supp. 2d 130, 137 (D.P.R. 2009) (citing \textit{Simonton} with agreement).


\textsuperscript{69} The court repeated the fact that plaintiff did not reveal his sexual orientation at work four times. \textit{Id.} at 407, 410, 412.
discrimination suffered by the Centola plaintiff was likely based on gender stereotypes. This conclusion led the court to reject the defendant’s motion for summary judgment.70

Dawson and Centola highlight a real concern. In many cases, it is impossible to separate sexual orientation discrimination claims from sex stereotyping claims. Recognizing this difficulty, courts often refer to the line between sexual orientation and sex stereotypes as one that is “hardly clear,”71 “hard to draw,”72 one that “does not exist,”73 and “illusory and artificial.”74 “[S]tereotypes about sexuality,” they explain, are simply too “related to our stereotypes about the proper roles of men and women.”75 This difficulty has led many courts to outright reject gender stereotyping discrimination claims for fear that they are framed to mask a sexual orientation discrimination claim.76 The teaching of cases like Centola and Dawson is that LGB applicants who want to avoid that fate should hide their true sexual orientation. The concern is broader. Because the test focuses on “perceived” sexual orientation, all applicants have the incentive to conform to societal expectations concerning gender stereotypes. That is, all applicants may be less likely to be discriminated if they (pretend to) conform to established gender norms.

By contrast, LGB applicants whose sexual orientation is known to the loan officer may be pressured to adopt mannerisms stereotypically associated with the opposite sex (e.g., a homosexual male may want to wear women’s clothing or act femininely). If they do not, they run the risk that any future claim of discrimination will be easily dismissed (since sexual orientation discrimination is permissible while gender stereotyping discrimination is not). To see this, consider the following example:

Example. A married gay male with a perfect credit score enters a bank and fills out a mortgage application. The loan officer is aware of the fact

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70 Id. at 410. But see Dandan v. Radisson Hotel Lisle, No. 97 C 8342, 2000 WL 336528, at *1, *4 (N.D. Ill. Mar. 28, 2000) (rejecting the plaintiff’s claim that “if the co-workers do not know his sexual orientation, the verbal abuse can only be attributed to the fact that he is a man” and explaining that “whether [defendant’s] co-workers knew or only suspected what his sexual orientation is makes no difference” because discrimination based on sexual orientation, real or perceived, is not actionable under Title VII”). Still, disclosing the fact that one is a member of the LGB community may increase the likelihood not only of facing discrimination, but also that a court would dismiss one’s sex discrimination claim.
72 Prowel v. Wise Bus. Forms, 579 F.3d 285, 291 (3d Cir. 2009) (dismissing the lower court “holding that [the plaintiff’s] sex discrimination claim was an artfully-pleaded claim of sexual orientation discrimination” because the analysis of “the facts and inferences in favor of [the plaintiff] leads [to the conclusion] that the record is ambiguous on this dispositive question”).
74 Id.
75 Centola, 183 F. Supp. 2d at 408.
76 As explained below, this trend is changing. See infra note 217-218 and accompanying text.
that the applicant is gay—perhaps because submitting the marriage certificate was necessary for the application process. Based solely on the applicant’s sexual orientation, the loan officer rejects the application.

If the gay male applicant appears to be stereotypically masculine, he may have a hard time showing that he was discriminated against on the basis of a protected characteristic. By contrast, a gay male who fails to conform to stereotypes associated with his gender (e.g., if he wears women’s clothing or appears to be effeminate) will likely have an easier time stating a prima facie claim. The reason is that “cases applying Price Waterhouse have interpreted it as applying where gender non-conformance is demonstrable through the plaintiff’s appearance or behavior.”77 Thus, unless the plaintiff can prove that “his appearance or mannerisms . . . were perceived as gender non-conforming in some way,” his action is destined to fail.78 In the above example, the applicant may thus be better off if he wears what is considered women’s clothing even if he prefers not to. Behaving in such a gender non-conforming manner against one’s natural tendencies, however demeaning and ludicrous, has another benefit. It shifts the burden to the defendant to show that its motive was based solely on the applicant’s perceived sexual orientation.

Another perverse outcome—a slight variant of the one immediately described above—relates to the role of gender-based stereotypes. Under Price Waterhouse, discrimination based on such stereotypes is illegal sex discrimination. As a result, discriminating against a woman who walks, talks, and dresses like a man is prohibited. But if a loan officer instead relies on such stereotypes to infer that the applicant is homosexual and then discriminates solely on the basis of homosexuality, the discrimination is not actionable. To illustrate, consider again the male applicant with a perfect credit score whose application was denied because the loan officer believed he was gay, perhaps because the loan officer thought he seemed effeminate. If the lender cites the applicant’s (perceived) sexual orientation as the reason for denying the application, and can prove that sexual orientation was the sole basis for the denial, the lender will not be liable for the discrimination. The sexual orientation discrimination defense, therefore, allows a loan officer to rely on gender stereotypes to inform the lender’s belief that the applicant is gay, and then permissibly discriminate against that applicant because he is gay, despite Price Waterhouse’s prohibition against discrimination based on gender stereotypes.

Finally, the sexual orientation defense likely dilutes the protection afforded to transgender applicants against gender identity discrimination. In cases where the gender identity of the applicant visibly “transgresses gender

77 Vickers, 453 F.3d at 763.
78 Id. (emphasis added). See also Dawson, 398 F.3d at 221 (holding that “one can fail to conform to gender stereotypes in two ways: (1) through behavior or (2) through appearance” and dismissing the complaint after noting that the “[plaintiff] makes no assertion with respect to behavioral non-conformance”) (emphasis added).
stereotypes,” the lender may have an easier time raising the sexual orientation defense. As Dawson and Centola suggest, in these cases transgender applicants may be better off if they hide their transgender identities. Thus, the law not only allows discrimination based on sexual orientation, but also incentivizes applicants to hide their true gender identity or sexual orientation in some cases and misrepresent them in others.

The concern that discriminatory practices are rampant and underreported is real. In fact, there is reason to believe that the number of unreported discriminatory incidents increased—and considerably so—between 2010 and 2016. During this period the housing and lending markets experienced a substantial increase in activity. This suggests that the number of applicants facing discrimination increased as well. By contrast, as Figures 1 and 2 below illustrate, the number of complaints and their breakdown by year, processing agency and subject matters remained mostly stable, implying under-reporting.

Two trends suggest that under-reporting is an even larger concern in the context of sexual orientation discrimination. First, the total number of complaints in mortgage lending (filed under all bases) monotonically decreased from 1,568

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79 Glenn, 663 F.3d at 1316; Mia Macy, EEOC DOC 0120120821 11 (2012), 2012 WL 1435995 (“A person is defined as transgender precisely because of the perception that [their] behavior transgresses gender stereotypes.”).


81 Based on compilation of data from the NFHA annual reports. See supra note 50.

82 “Others” includes, among other things, the following categories: sexual orientation, gender identity, age, domestic violence, and criminal background, as well as marital and military status. Id.
cases in 2010 to an all-time low of 333 cases in 2016, a 78% decrease.\textsuperscript{83} During this period, however, housing starts\textsuperscript{84} more than doubled, and mortgage debt for residential housing increased by 40.81%\textsuperscript{85}. Second, in 2013, the first year after the enactment of the Equal Access Rule, the number of sexual orientation discrimination complaints reached its peak at 250, but since, the number has steadily declined.\textsuperscript{86} These trends are illustrated in Figure 3 below. Together they suggest that, in mortgage lending, sexual orientation discrimination is even more under-reported than other types of discrimination.

![Figure 3: Trend of Complaints in Selected Substantive Areas and Classes](image)

In sum, federal law does not prohibit sexual orientation discrimination when it comes to mortgage lending. Rather, it views sexual orientation as a “legitimate” (if abhorrent) basis for discrimination. The result is under-reporting of all types of discriminatory incidents, more discrimination, and a myriad of perverse outcomes. Both the FH-Act\textsuperscript{88} and ECOA\textsuperscript{89} however, left the door open for state and local legislatures to provide broader protection. As explained below, however, the majority of states and local jurisdictions forwent the opportunity.

C. State and Local Laws

Although all states have enacted fair housing laws, only twenty-three states include a provision prohibiting sexual orientation discrimination in lending. Twenty of these states also prohibit gender identity discrimination. Table 1 lists the states that enacted fair housing laws prohibiting gender identity and/or sexual orientation discrimination, including the enactment and effective dates of the

\textsuperscript{83} Id.
\textsuperscript{84} The beginning of construction of new houses.
\textsuperscript{85} See supra note 80 and accompanying text.
\textsuperscript{86} See supra note 50.
\textsuperscript{87} Between 2010 and 2016, the number of sexual orientation complaints processed was 123, 101, 175, 268, 201, 164, and 150 respectively. Id.
\textsuperscript{88} 42 U.S.C. § 3615.
\textsuperscript{89} 15 U.S.C. § 1691d.
relevant statutes. Finally, two states, Arkansas and Tennessee, forbid their localities from adopting ordinances that would prohibit discrimination on a basis not recognized by the state.\textsuperscript{90} The result is that the same discriminatory behavior may be allowed in some states but not in others. Moreover, even in those states that do not prohibit discrimination in lending against members of the LGB community, discrimination may be prohibited in certain localities and counties. As Figure 4 below illustrates, the annual increase in the number of such political subdivisions sharply increased in 2010 and reached its highest point in 2013—the year following the enactment of the Equal Access Rule.

Table 1: State Anti-Discrimination Laws in Lending\textsuperscript{91}

<table>
<thead>
<tr>
<th>State</th>
<th>Sexual Orientation Passed</th>
<th>Effective</th>
<th>Gender Identity Passed</th>
<th>Effective</th>
</tr>
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<tr>
<td>CA</td>
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<td>10/10/1999*</td>
<td>09/06/2011</td>
<td>01/01/2012</td>
</tr>
<tr>
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<td>05/22/2014</td>
<td>08/06/2014</td>
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</tr>
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<td>01/01/2006</td>
<td>01/21/2005</td>
<td>01/01/2006</td>
</tr>
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<td>03/12/2015</td>
<td>05/12/2015</td>
</tr>
</tbody>
</table>

\textsuperscript{90} See supra note 8 and accompanying text.
\textsuperscript{91} California, the District of Columbia, Vermont and Wisconsin did not specify an effective date, and we assumed it was the same as the enactment date.
D. An Under-Studied Phenomenon

Of the very few studies that investigate sexual orientation discrimination, only one focuses on the mortgage lending market.\footnote{These studies are discussed in infra Part D.2. and Part D.3. respectively.} As explained below, that study was very limited in nature. It was a field experiment that was conducted in one state (Michigan), before the enactment of the Equal Access Rule, and had only 120 observations of which only 36 focused on home financing.\footnote{See infra notes 137-149 and accompanying text.} Importantly, due to its design it could not provide—not even anecdotally—answers to the questions we investigate here. This Section begins with a short overview of the economics of discrimination. It then reviews the leading studies on sexual orientation discrimination and the shortcomings of their designs.

1. The Economics of Discrimination

Discrimination in the home mortgage lending process is a topic that has received considerable attention both from academics and policy makers. In his seminal book, *The Economics of Discrimination*, Gary Becker provided a basis for much of the theoretical work on discrimination.\footnote{Gary S. Becker, *The Economics of Discrimination* (2d ed. 1971).} According to Becker, some individuals act as though they have a “taste,” or preference, for discrimination...
against a minority group. But discrimination comes at a cost: forgoing profitable transactions with members of the discriminated group.

Theory predicts that in a competitive market, this cost will drive out taste-based discrimination. For example, an employer who prefers to hire only white employees forgoes the benefits that talented non-white employees may bring. Those employees may be hired by other firms and possibly at a lower-than-average salary. As a result, non-discriminating firms may be able to offer better products or services at a lower price, and consequently drive the discriminating firm out of the market. In the mortgage lending context, the cost of discrimination can also be prohibitive. Rejecting applicants with good credit because they belong to a certain group may result in fewer profits and a reduction in value. This is the case, for example, if the prejudicial lender reaches a point where sales made to his preferred groups are exhausted. At that point, the prejudicial lender must either offer loans to all individuals or incur losses. Charging supra-competitive prices to members of a protected group (i.e., reversed red-lining) is also infeasible if enough lenders are willing to offer credit.

Markets, however, are not always competitive and, as a result, taste-base discrimination may persist. A different theory that explains why discrimination may persist in competitive markets, and can even be efficient, is statistical discrimination.

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95 GARY BECKER, THE ECONOMIC APPROACH TO HUMAN BEHAVIOR 6–9 (1976). The reason for discrimination may also be the preference of a third party who is in a position to penalize those who decline to discriminate. See, e.g., Cass R. Sunstein, THREE CIVIL RIGHTS FALLACIES, 79 CAL. L. REV. 751, 754 (1991) (providing as examples “the case of a shopkeeper whose customers do not like dealing with blacks or women, a commercial airline whose patrons react unfavorably to female pilots, a law firm whose clients prefer not to have black lawyers, [and] a hospital whose patients are uncomfortable with female doctors or black nurses” and noting that “in these cases, an employer who introduces norms of equality into the work force will be punished, not rewarded”).

96 Id. at 11–13.

97 Id. at 35–37.


99 United Cos. Lending Corp. v. Sargeant, 20 F. Supp. 2d 192, 210 (D. Mass. 1998) (“Redlining is the practice of denying the extension of credit to specific geographic areas due to the income, race, or ethnicity of its residents. . . . Reverse redlining is the practice of extending credit on unfair terms to those same communities.”).


101 Gary A. Dymski, The Theory of Credit-Market Redlining and Discrimination: An Exploration, 23 REVIEW OF BLACK POLITICAL ECONOMY 37–74 (Winter 1995) explaining that discrimination may occur if the number of prejudicial lenders is large enough to dictate the price to the neutral lenders; Cooter, supra note 98, at 344 (explaining that collusion by social groups can result in market power).

Under this theory, firms do not discriminate because they have a taste for discrimination. Rather, in a world of imperfect information, these firms resort to group characteristics or stereotypes as proxies to evaluate outcome-relevant attributes of individuals. In other words, these firms make the inference that because an individual belongs to a certain group, she possesses certain traits associated with that group. “In the classic textbook example, if employers believe (correctly) that workers belonging to a minority group perform, on average, worse than dominant group workers do, then the employers’ rational response is to treat [the two groups of workers] differently.”\(^{103}\) Another example is the use of a sex stereotype as a proxy in labor markets. Based on past experience, an employer may believe that, compared to men, women are more likely to leave their jobs during childbearing years. The behavior is rational and (likely) profit-maximizing even when the decision-maker relies on proxies that are “over-broad generalizations and far from entirely accurate.”\(^{104}\)

Redlining—the practice of denying services or raising prices to minority groups—can be the result of such statistical discrimination.\(^{105}\) Just like employers may rely on sex and race as proxies for performance,\(^{106}\) a lender may rely on similar proxies to estimate risk. As a result, what might appear to be systematic taste-based discrimination against a minority group might in fact simply be lenders avoiding loans in high-crime, low-income areas that happen to be heavily populated by the minority group. Economists refer to this form of discrimination as “statistical,” meaning discrimination that arises out of a risk assessment based on characteristics commonly held by that group. It is also referred to as rational discrimination,\(^{107}\) and some have argued that rational discrimination should be legally permitted.\(^{108}\)

There are, of course, other theories of discrimination.\(^{109}\) Our goal here is not to review every possible theory. Rather, following the empirical literature, we

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\(^{103}\) Jess Benhabib et al., 1A Handbook of Social Economics 135 (2011).

\(^{104}\) Sunstein, supra note 95, at 756 (noting that “race or sex in some contexts may be every bit as accurate a predictor of job performance as, say, test scores, education, and previous employment” and that “in some cases, reliance on more direct and individualized devices might be too costly to be worthwhile.”).

\(^{105}\) See supra note 99.

\(^{106}\) Sunstein, supra note 95 at 755–57.


\(^{108}\) See e.g., Jack M. Guttentag & Susan L. Wachter, Redlining and Public Policy, Monograph Series on Finance and Economics (1980).

\(^{109}\) See e.g., McAdams, supra note 98, at 91–96 (explaining that discrimination may be the result of competition over social status between groups).
focus on the taste-based and statistical discrimination theories. This focus allows us to reveal and propose new ways to deal with some of the flaws that plague previous studies. It also allows us to shed new light on and challenge their findings and conclusions. Finally, these two theories have another benefit: they interact differently with the “Contact Hypothesis,” a theory we test. Under this theory, discrimination may be the result of ignorance, and, accordingly, can be reduced by contact with members of the minority group. If true, the empirical prediction is that areas with more inter-group contact experience less discrimination. The prediction holds when the discrimination is taste-based. By contrast, contact with minorities may reinforce statistical discrimination if it provides the decision-maker with new proxies that will allow it to segment the market. For example, a lender who learns that members of a certain minority group suffer from a higher unemployment rate may refuse to sell them loans or require higher interest rates. If the lender learns through contact that certain groups are less likely to bargain, the lender may attempt to command higher prices. With these two theories in mind—taste-based and statistical discrimination—we now turn to the world of practice.

2. Two Types of Studies: Econometric Approach and Field Experiments

Attempts to empirically address taste-based and statistical discrimination have essentially taken two forms: (a) the econometric approach and (b) field studies. As we explain below, these studies suffer from a number of theoretical and methodological limitations. Understanding the criticism these studies faced and the methodologies they used not only motivates and informs our study, but also allows us to extend the literature on discrimination in mortgage lending to discrimination based on sexual orientation.

a. The Econometric Approach.

The first approach is to maintain data at the individual level and assess the likelihood of loan acceptance. This is an attractive approach because lenders typically have guidelines and algorithms that drive the loan acceptance process. In a leading study, researchers were able to obtain all the


112 Stephen Ross & Margery Austin Turner, Housing Discrimination in Metropolitan America: Explaining Changes Between 1989 and 2000, 52 Social Problems 152–80 (2005) (suggesting that a landlord may discriminate against members of a minority group based on such information).
data associated with whether a loan should have been accepted or denied. They were thus able to control for every factor that, according to the banks, was a relevant consideration. The study concluded that an application from a black individual was 6 to 8 percentage points less likely to be approved than an application filed by a white individual with similar bank-relevant characteristics. Follow-up studies questioned the sensitivity of this result and argued that, if anything, it only applies to applications right on the fringe of acceptance. Others argued that the single most important factor to a loan application—risk of loan default—is not considered.

The criticism that received possibly the most attention was that this type of modeling did not address the source of the discrimination, i.e. whether it was the result of taste-based or statistical discrimination. That is, was the observed discrimination evidence of bigotry? Or was race just a proxy for some other neighborhood characteristic associated with the typical African-American application that lenders might rationally want to avoid? Later studies attempted to answer the motivation question by aggregating the data away from the individual level to the neighborhood level. These studies found much weaker evidence of racial (i.e., taste-based) redlining.

As we discuss below, we are able to address each of the concerns brought up by the racial redlining literature in a number of ways available to us thanks to the thoroughness of the HMDA data. By doing so, our study is not only the first to use regression-based analysis to study sexual orientation discrimination, but it also invites and sets the ground for future research.

b. Field Experiments. With very limited ability to obtain data on the individual level, “[m]uch of the research into housing discrimination, including

114 Id. at 33.
118 Id.
119 See infra Part III.A and Part III.B.
120 For example, we are able to limit the possibility of statistical discrimination by restricting the dataset to only FHA loans. Once issued, these loans carry the same risk to the bank regardless of any underlying characteristic that banks think they may need to account for that is associated with race, or sexual orientation. We also compare changes in lending rates by minority groups from the same bank in the same county.
HUD’s [Housing Discrimination Studies] had to resort to “paired testing.”"121 Under this methodology, “two testers assume the role of applicants with equivalent social and economic characteristics who differ only in terms of the characteristic being tested for discrimination, such as race, disability status, or marital status.”122

While most studies focus on racial discrimination in mortgage lending,123 only a few attempted to investigate sexual orientation discrimination. The first field experiment was conducted in Sweden in 2009 and found evidence of discrimination against same-sex couples.124 The authors sent out two fictitious applications for rental housing via the internet. One application was sent by a couple with a traditionally male and female name. The other application was sent by two distinctively male names, suggesting a gay couple. Each pair also presented itself as a “couple” to explicitly signal their sexual orientation. The authors then measured the rate at which each fictitious couple was called back. They found that, compared to the heterosexual couple, the homosexual couple was 14 percentage points less likely to receive a call-back.125 A follow up study carried out in much the same manner—email correspondence studies—found similar results in the Vancouver, Canada rental market.126

The two studies established some initial evidence toward the possibility of discrimination based on sexual orientation, but they have their limitations. Because each study focuses on a specific area and addresses only rentals, we hesitate to draw too much of a conclusion about how these results might translate to home sales through mortgages. This is especially so since anti-discrimination laws differ from one country to another, as do social norms.

A broader concern is whether correspondence studies, which rely on response rates to email inquiries, can serve as a proper measure of discrimination.

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122 Id.
123 See, e.g., Lauster & Easterbrook, supra note 110, at 389-90 (“Much has been written about discrimination in the rental market, but the literature is almost entirely focused on race and ethnicity.”).
124 Ali M. Ahmen & Mats Hammarstedt, Detecting Discrimination Against Homosexuals: Evidence from a Field Experiment on the Internet, 76 ECONOMICA 588 (2009) (“This paper presents the first field experiment studying discrimination against homosexuals on the housing market.”).
125 Id. at 594.
126 Lauster & Easterbrook, supra note 110, at 389. The author found that compared to the heterosexual baseline, male couples were 24 percent less likely to receive a positive response to a rental inquiry, but that lesbian couples were actual more likely to receive such a response. By contrast, lesbian couples were actually more likely to receive a positive response to rental inquiries than the heterosexual couple, though the effect was statistically insignificant. Id. at 296. The study also found support of the Contact Theory. In areas where individuals were more likely to have contact or familiarity with same-sex couples (e.g., downtown), landlords “demonstrated less discrimination against same-sex male couples” (the authors noted, however, that these areas were also the more expensive ones). Id. at 403.
To begin with, such studies cannot distinguish between taste-based and statistical discrimination. The distinction is critical as different forms of discrimination call for different remedies and measures. Moreover, it is also unclear if the response rate can serve as a proxy for discrimination at all. The Swedish and Canadian studies exemplify the problem with the methodology. In both, a nonresponse was considered a negative outcome and a sign of discrimination. By contrast, all responses were considered non-discriminatory outcomes, even though there are many ways bigoted landlords can mask discrimination through a response. Examples are email replies that raise difficulties of actually seeing the apartment and responses that redirect the applicant to a different property owner—both of which happened in the Vancouver study. It is also likely that some prejudicial landlords provide untruthful responses regarding occupancy. These responses might be strong evidence of actual discrimination, but they were considered a non-discriminatory outcome.

Another major challenge is whether the results, even if taken as valid, can be generalized. How much can a study in Sweden or Vancouver tell us about housing discrimination generally in the United States? In an effort to answer the question, HUD commissioned a similar email correspondence study. Touted as the “first large-scale . . . study to assess housing discrimination against same-sex couples” on a “national scale,” the 2011 study conducted 6,833 paired email correspondence tests across 50 randomly selected markets. The study found that compared to heterosexual couples, same-sex couples—both male and female—received significantly fewer responses as compared to heterosexual couples.

There was also some evidence that jurisdictions with state-level prohibitions against sexual orientation discrimination exhibited slightly more adverse treatment against same-sex couples compared with states without such prohibitions.

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127 Lauster & Easterbrook, supra note 110, at 398; Ahmed & Hammarstadt, supra note 124, at 592.
128 For example, a discriminating landlord could, hypothetically, respond positively to the inquiry from the homosexual couple but make it difficult for the couple to actually see the apartment. Examples of this in practice might look like scheduling a visit multiple weeks out at an inconvenient time or requesting a phone conversation to confirm date and time to tour the facility and never answering the phone.
129 Lauster & Easterbrook, supra note 110, at 398.
130 Samantha Friedman et al., An Estimate of Housing Discrimination Against Same-Sex Couples at 2 (U.S. Dep’t of Hous. & Urban Dev., Off. of Pol’y Dev. & Res. issuer), https://www.huduser.gov/portal/publications/fairhsg/discrim_samesex.html (last visited on Jan. 31, 2018) [hereinafter “HUD Study”]. Although the HUD study was conducted in 2011 before the Equal Access Rule was promulgated, it was released to the public in July 2013.
131 Id. at iv, 1, 5 (“The objective of this study is to develop the first national estimate of the level of housing discrimination against same-sex couples.”).
132 Id. at v.
133 Id. at vi ("[H]eterosexual couples were favored over gay couples in 15.9 percent of tests and over lesbian couples in 15.6 percent of tests.")
134 Id. at iv, vii 16-17.
The recent HUD study represents a new and improved generation of field experiments. Together with a recent study conducted in the automobile industry, it indicates that sexual orientation discrimination permeates many markets. But what the HUD and other studies did not and could not test is how sex and race interact. The automobile study included only white male testers, and in the HUD study “the only difference between the two emails was whether the couple was same sex or heterosexual.”

3. Sexual Orientation in Mortgage Lending

To date, only one study addressed sexual orientation discrimination in mortgage lending. The study was conducted in 2007 by four of Michigan’s Fair Housing Centers and included 120 pair-tests (the Michigan Study). Each test included two pairs: one posing as a heterosexual couple and the other posing as a same-sex couple with superior credentials (higher income, larger down payment, and better credit). The study found disparate treatment in 32 (27%) of the 120 tests and concluded that discrimination against same-sex couples is “widespread.” The Michigan Study’s conclusion, however, suffers from a number of limitations. To begin with, the study focused only on one state: Michigan. The sample size was also small: a total of 120 paired tests. Third, the study focused on three markets, of which only 36 (or 30%) of the 120 tests were dedicated to discrimination in “home financing.” Moreover, home financing exhibited the least amount of discrimination: 20% compared to rental (33%) and homes sales (25%). Fourth, the study was conducted in 2007, five years before the enactment of the Equal Access Rule. At that time, discrimination on the basis of sexual orientation was allowed with respect to all types of mortgages, including FHA loans. Therefore, the study could not estimate the effectiveness of the Equal Access Rule.

Importantly, like other field experiments, the paired tests in the Michigan Study were designed to test only one variable: sexual orientation discrimination. For that reason, in each test, the couples posing as same-sex couples and

135 Gneezy et al., supra note 110 at 5 (focusing on car dealerships in the Chicago area).
136 HUD Study, supra note 130, at iv (“The study measured the response of housing providers regarding the sexual orientation of couples and did not examine other characteristics . . . ”). Id. at vii.
137 For an explanation of the methodology and its limitations, see HUD’s Pair Testing Webpage, supra note 121 (“Much of the research into housing discrimination . . . relies on paired testing, a methodology in which two testers assume the role of applicants . . . who differ only in terms of the characteristic being tested for discrimination, such as race . . . ”).
139 Id. at 3.
140 Id.
141 Id.
142 Id. at 9.
143 Id. at 3.
144 Id. at 11.
heterosexual couples were “balanced for race.”\textsuperscript{145} Of these couples, the vast majority—113 (or 94\%)—were white, 5 were black, and 2 were inter-racial.\textsuperscript{146} This design did not allow the four centers to test how the interaction between sex and race influences the discriminatory practices identified.\textsuperscript{147} Nor could the study identify the effect of local ordinances\textsuperscript{148} or determine whether and how “differences between the ways lesbians and gay men are treated” impacted the findings.\textsuperscript{149}

III. THE DESIGN, DATA AND FINDINGS

A. The Design

Our study is the first attempt to fill the gap and shed light on the very issues that the Michigan Study identified as important but left unanswered. As explained below, unlike the field experiments, we study sexual orientation discrimination in all states, using a large number of observations (over five million), and focusing solely on mortgage lending. Importantly, our study is the first to try to investigate how race and sex impacts discrimination against same-sex applicants. Our data suggests that race is a critical factor, that lesbians and gay men are treated differently, and that state laws may have a real effect on discrimination against the LGB community.

Our study builds on the prior literature in a variety of ways. As explained in Part III.B. below, based on the critiques of the use of individual-level data, we construct a model that remedies some of the problems identified in prior studies. Our model allows us to look at the individual effects of potential mortgage discrimination. It also takes into account the fact that different minority groups may self-select into neighborhoods and into mortgage applications that have a higher risk of default.

1. Risk Considerations

We take a number of steps to ensure we do not mistake legitimate risk considerations (including proxies such as income of geographic effects) for discrimination. First, and exactly because of the concern that different applicants may carry different levels of risk, we focused only on FHA loans. Applicants for these loans must meet certain pre-determined criteria. Importantly, for applicants who met the criteria, income and credit scores have little importance. In the eyes

\textsuperscript{145} Id. at 3.
\textsuperscript{146} Id. at 10.
\textsuperscript{147} Id. at 11 (admitting that “more testing is needed to see how the race and sex of testers [that is, of applicants] are influencing factors in some housing markets.”).
\textsuperscript{148} Id. at 9 (“It is unclear whether [levels of discrimination found across the state diverged widely] due to . . . differences in . . . the presence of local ordinances protecting sexual orientation . . . ”). Although inconclusive, the Michigan Study reported that there was less evidence of discrimination in areas with such local ordinances (22\% compared to 30\% in areas without ordinances). Id. at 16.
\textsuperscript{149} Id. at 9. The Swedish, Canadian, and HUD studies suffer from the same limitation.
of the lender, these FHA loans carry the *same* level of risk since each loan is insured by the federal government.\(^{150}\)

Second, as we explained on the methodology section, while HMDA data is limited, we do have and control for the applicant’s income. That is, in addition to other controls, we compare loans of applicants with the *same* level of income. In addition, at the neighborhood level, we include county-by-bank fixed effects which controls for any differences across neighborhoods and banks. We are looking at how different compositions of race and gender affect loan acceptance within the *same* neighborhood by the *same* bank.

Third, in the event that the neighborhood of the home might actually just be a proxy for bad credit (i.e., bad economic neighborhoods generally attract applicants with bad credit), while we do not have the credit score of the applicant, we do know if the loan got denied *because of* an insufficiently poor credit score. Thus, while we do not know the intimate details of an applicant’s credit history, we do know and control for those applicants with bad enough credit to disqualify them for an FHA loan. As explained further below, our empirical design allows us to compare loan similarly situated applicants (same applicant income, same loan amount, same loan purpose, same risk the lender, etc.). This design—comparing loan acceptance rates within the same county by the same banks with multiple controls—has an important benefit. It offsets the concern that what we measure is actually just a proxy for some other neighborhood-level characteristic.

2. The Proportion of Same-Sex Gay Co-Applicants in the Data

Our design is still disadvantaged by a key element of sexual orientation discrimination. Other types of discrimination (e.g., racial or gender) are typically characteristics that are easily observed by both the researcher and the lender. In contrast, sexual orientation is not a salient characteristic that the lender, much less the researcher, can necessarily observe. As a result, we do not and cannot directly observe applicant’s sexual orientation. While initially this may seem like a fatal flaw in our analysis, it is important to remember that the loan officer also does not directly observe sexual orientation. The loan officer can only infer sexual orientation based on observed characteristics (e.g., the applicant’s style of dress, behavior, etc.) and the perceived relationship between the applicant and co-applicant. While we do not exactly observe sexual orientation, we do observe one important characteristic: whether the applicant is accompanied by a same-sex co-applicant. This is an important characteristic that loan officers observe.

We recognize that this is not a perfect proxy for the applicant’s *actual* sexual orientation. Indeed, co-applicants can be family members (e.g., father and son) or friends, to give a few examples. However, there is strong theoretical and empirical evidence that our estimates do actually measure sexual orientation based discrimination despite our inability to directly distinguish between same-sex homosexual co-applicants and same-sex heterosexual co-applicants.

\(^{150}\) See *infra* notes 171–174 and accompanying text.
AN EMPIRICAL ANALYSIS OF SEXUAL ORIENTATION DISCRIMINATION

i. Theoretical Explanations. First, it is important to remember that the applicant’s true sexual orientation is irrelevant. Discrimination is not based on the actual sexual orientation of the applicant, but rather on the applicant’s perceived sexual orientation. Discrimination is the result of what the loan officer believes to be the case. By using same-sex co-applicants as a proxy for perceived sexual orientation, we are not only following the footsteps of other researchers,151 we are also following the legal test established in Price Waterhouse. This test focuses on the plaintiff’s “appearance,” “behavior,” and “mannerisms” as they were perceived by the loan officer.152

Moreover, our findings, if anything, are a conservative measure of the level of discrimination. The fact that we cannot distinguish between (a) same-sex heterosexual co-applicants and (b) same-sex gay co-applicant, actually makes our results stronger. In other words, we show that if the data include not just gay co-applicants, but also heterosexual co-applicants, then the true level of discrimination is actually higher than we report.

The reason is related to the first point. The loan officer cannot observe the co-applicants’ true sexual orientation. In some cases, the loan officer may have information that we cannot observe: for example, whether the same sex co-applicants are a father and son. In other cases, the loan officer may believe that the same sex co-applicants are a gay couple even if they are not. The concern, therefore, is that there are essentially two types of same-sex applications: (a) those applications where the co-applicants are clearly related, such as a father/son pairing (Group 1), and are therefore not (or less likely to be153) gay individuals, and (b) the rest of the same-sex applications where the relationship between the applicant and co-applicant is ambiguous to the lender (Group 2). As researchers, we cannot distinguish between Group 1 and Group 2. But if (i) the loan officer has a taste for discrimination and has additional information on the nature of the relationship either through last name or physical appearance (e.g., Group 1 looks like a father and son vs. Group 2 where it is unclear), and (ii) if the loan officer only actually discriminates against Group 2, then all that does is understate the magnitude of the effect of discrimination. In other words, the inability to distinguish between the two groups, if anything, biases our results towards zero.

To illustrate this point, consider the following example. Suppose the bigoted loan officer does not discriminate against Group 1 because he has knowledge that is not observable to us as researchers. In such a case, members of Group 1 are 0% more/less likely to have the loan approved (i.e., they will be

152 See supra notes 76–89 and accompanying text.
153 A parent and child may both be gay individuals or may be perceived as such by the loan officer. It could also be that one of them is a gay individual (i.e., the parent or the child may be a gay individual).
treated same as the white heterosexual benchmark). Now, since the loan officer is bigoted and does like to discriminate against the Group 2 type loans (perceived gay co-applicants), those loans are, say, 12% less likely to get accepted. The “true” level of discrimination is 12%. However, in our analysis, we necessarily are forced to clump Group 1 and Group 2 loans together. Our resulting estimates average the effect of Group 1 and Group 2, which in this hypothetical would result in an overall effect of loans 6% \((\frac{0+12}{2})\) less likely to be approved. Thus, if anything, this ambiguity only understates the level of discrimination (“true” level of 12% compared to the estimated effect of 6%), but does not invalidate our estimates.

ii. Empirical Evidence. We further this claim empirically in three ways. First, we track the rate of same-sex loan applications in states and local jurisdictions that passed laws prohibiting discrimination on the basis of sexual orientation.

![Figure 5: Change in Same-Sex Loan Applications Before and After the Passage of Anti-Discrimination Laws](image)

Figure 5 above tracks the proportion of same-sex loan applications over time that has been centered around the year the law passed (since not all laws are passed in the same year). The horizontal axis measures time in years before and after the law is passed, and the vertical axis measures the proportion of same-sex loans. As Figure 5 demonstrates, there is a marked increase in same-sex loans.
after the law passes that persists through the end of our data range. Under the assumption that the Group 1 (perceived heterosexual co-applicants such as parent-child) same-sex loans will not be affected by changes in anti-sexual orientation discrimination laws, Figure 5 suggests that laws are specifically opening the door for more Group 2 (perceived gay co-applicants) type loans.

Figure 5 is also consistent with previous research that same-sex loan applications increased after the passage of Obergefell vs Hodges. This study, conducted by HUD in 2016, exploited the “variation across states prior to the Supreme Court decision to investigate the effect of marriage laws on demand for mortgage credit.” By using the same methodology—looking at the reported sex of co-applicants—it concluded that states that passed same-sex marriage laws “experienced 8-13 percent increase in mortgage applications.”

Figure 6: The Relationship Between The Size of the LGBT Community and the Proportion of Same-Sex Loans by State

Second, Figure 6 suggests that there is reason to believe that many of the same-sex loan applications in our dataset are actually loans by gay co-applicants. In Figure 6, we compare the proportion of same-sex loans per state—the top line—to the actual proportion of individuals in the state that consider themselves

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154 Miller & Park, supra note 151.
155 Id. *1.
156 Id. *2.
part of LGBT community—the lower line. Both lines trend together, with a correlation coefficient of 0.71 suggesting a strong and robust correlation. Essentially, in states with a larger LGBT community, more same-sex applications are filed. Figure 6 therefore suggests that most of the same-sex applications we measure are, in fact, home loans applications file by gay co-applicants. The result of a third robustness test leading to the same conclusion is reported in Part III.C below.

B. The Model

1. The Data and Methodology

Our study relies on three datasets. The first two are proprietary, and include state- and local-level protection against anti-discrimination practices in mortgage lending (“Local Rules”). The third has publicly available data on home mortgages reported by financial institutions pursuant to the Home Mortgage Disclosure Act (HMDA). We study every home loan application in the U.S. reported to HMDA between the years 2010 and 2015—about five million observations. To keep the risk of the loan constant, we restrict the dataset to only include applications made for FHA loans where the applicant has a co-applicant.

Our outcome of interest is an indicator variable signifying whether or not the loan was accepted. The variable equals 1 if the loan was accepted and 0 if the loan was rejected. In addition to gender and racial make-up of applicant and co-applicant, we are able to control for a myriad of factors that influence the probability of whether a home loan is accepted. These include the applicant’s

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157 Data on the LGBT community comes from https://williamsinstitute.law.ucla.edu/visualization/lgbt-stats/?topic=LGBT&compare=percentage#comparison

158 All state-level fair housing laws were collected from Westlaw. For states that had sexual orientation protection, we recorded the date the statute was passed or the relevant section was added and the date it became effective. Assembling a database that includes all local and county ordinances was more challenging. First, no one source compiles all local laws. In addition, we found that some localities maintain extensive histories, while others do not. To address these issues, we reviewed a number of leading databases including Municode, Code Publishing, American Legal Publishing, General Code Corporation, Qcode, Coded Systems, and Conway Greene. We then compared the results to earlier lists by other organizations, such as the Human Rights Campaign, and attempted to track the legislative history—particularly the enactment and effective dates—for ordinances with sexual orientation protection.

159 We chose these years in an attempt to avoid the housing market crash and aftermath, as well as other legislation that may have affected home mortgages. See Joshua Miller & Kevin Park, Same-Sex Marriage Laws and Demand for Mortgage Credit, REVIEW OF ECONOMICS OF THE HOUSEHOLD (2016).


161 This measurement of loan application success is a common measure in the discrimination literature. See id. at 12.
income, loan amount, property type, \textsuperscript{162} loan purpose, \textsuperscript{163} whether or not the home will be owner occupied, whether or not the applicant had been preapproved, the applicant’s ethnicity, and the reason for denial, if any. We include each of these variables in each model to account for any observable factor that may influence the bank’s decision to accept or deny the loan.

To account for any national, unobserved trends in the data, we also include in each model year-fixed-effects. These dummy variables allow us to control for changes in home loan trends that are common across all loans in a given year.\textsuperscript{164}

Additionally, we control for variation between different banks in the same county and different branches of the same bank in different counties. To see why, suppose that Bank-I and Bank-II are large national banks with branches in numerous counties in the U.S. Bank-I may have different lending practices than Bank-II. Similarly, a branch of Bank-I in one county may have different lending practices than a branch of Bank-I in a different county. To control for these two forms of (inter-bank and intra-bank) variation, we create a dummy variable for each bank in each county. That is, we create a set of dummy variables for Bank-I for each county in each state and we do the same for all the other banks. These bank-by-county-fixed-effects absorb all cross-bank and cross-county differences. All the variation that remains is the differences in lending practices within banks within the same county. Put differently, including these fixed effects allows us to look at how the same bank in the same county treats different applications. These variables allow us to exploit the within-bank and within-county variation.\textsuperscript{165}

Discrimination is a comparative term. Accordingly, our comparison group is the white male/white female pair—the most common combination in the dataset. Our independent variables are a set of all the remaining fifteen possible gender and race combinations between a primary applicant and a co-applicant. We thus have a separate dummy variable for each of the following combinations:

\textsuperscript{162} There are three property types: single family, multifamily, and manufacturing housing.

\textsuperscript{163} The “loan purpose” can be home purchase, home improvement, or home refinancing.

\textsuperscript{164} For instance, the changing landscape of home mortgages in the U.S. following the housing market collapse would be accounted for with year-fixed-effects.

\textsuperscript{165} With the HMDA dataset, we are able to drill down to a geographical level of granularity finer than county and go all the way down to census tract, which typically consists of neighborhoods within a county with a population around 4,000. For computational reasons, we feel county-by-bank effects are more appropriate. One major empirical decision to balance in this Article is the tradeoff between very precise data and allowing for enough identifying variation. For instance, if we compared the same banks within a state, that would provide plenty of observations nested within each fixed effect, but it may oversimplify the mortgage process since geographic and economic conditions vary wildly within a state. On the other hand, drilling down to the neighborhood level provides the best comparison, but functionally, the analysis is weakened by the lack of diversity in application types. Put differently, if we compare Bank A in State I, there will be lots of applications to look at, but they will be for homes in potentially very different neighborhoods. On the other hand, if we look only in the same neighborhoods, there is a real possibility that a bank may not cover all fifteen types of race/gender combinations we analyze in this study. We believe that county-by-bank effects best balance the need for a tight comparison window while keeping a critical mass of observations in each window. The main results are, however, completely unchanged if we instead include census-tract-by-bank effects.
(1) white male/black male, (2) white male/white male, (3) black male/black male,
(4) black male/white male, (5) white female/black male, (6) white female/white male,
(7) black female/black male, (8) black female/white male, (9) white female/black female,
(10) white female/white female, (11) black female/black female, (12) black female/white female,
(13) white male/black female, (14) black male/white female, (15) black male/white female.

Formally, Equation 1 estimates the following linear probability model:

\[
L_{ibcy} = a_0 + b_1 bmbm_{ibcy} + b_2 bmwm_{ibcy} + b_3 wmbm_{ibcy} + 
\]
\[
b_4 wmmw_{ibcy} + b_5 bmbf_{ibcy} + b_6 bbfw_{ibcy} + b_7 bbfm_{ibcy} + b_8 wmbf_{ibcy} + 
\]
\[
b_9 wbfw_{ibcy} + b_{10} bmwf_{ibcy} + b_{11} bfwm_{ibcy} + b_{12} bfbm_{ibcy} + b_{13} wbfm_{ibcy} + 
\]
\[
b_{14} wbfm_{ibcy} + b_{15} bwfm_{ibcy} + X_{ibcy} + \tau_y + \rho_{bc} + e_{ibcy}
\]

Where \( L_{ibcy} \) represents whether or not loan application \( i \) was accepted at
bank \( b \) in county \( c \) in year \( y \). \( X_{ibcy} \) is a matrix of covariates that influence the
probability a home loan is accepted.\(^{166}\) \( \tau_y \) is a set of time-fixed effects, \( \rho_{bc} \) is the
set of bank-by-county fixed effects, and \( e_{ibcy} \) is the error term. The remaining
fifteen variables measure the effect of each unique pair of race and gender
combinations. Accordingly, the coefficient \( b_k \) can be interpreted as the percentage
point change in the probability of loan acceptance. The omitted group is a white
male applicant with a white female co-applicant.

2. Model Validity

HMDA data is rich and provides the most complete coverage of the loan
application process.\(^{167}\) Still, there are many concerns that need to be addressed.

\( a. \) Linear Probability Modeling. A restricted dependent variable, such as a
binary outcome of whether or not a loan was accepted, violates the assumptions of
ordinary least squares estimation (OLS). In part, because the dependent variable is
not continuous, but also because the standard errors are misestimated. Additionally, it is possible for a linear probability model (OLS applied to a binary
outcome variable) to produce model estimates that yield a nonsensical predicted
probability that is greater than one. Alternative estimation techniques such as logit
and probit models correct for this by constraining the model to be bound between
zero and one. These models, however, come with their own set of assumptions
and perform equally as poorly, if not worse, than linear probability models.\(^{168}\)

In the context of this Article, we are able to alleviate the typical concerns
associated with linear probability modeling. First, we adjust for bias in the
estimation of the standard errors by clustering the standard errors in each model at
the state level. Second, in our dataset, 47% of the loans we analyze were accepted.

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\(^{166}\) See supra notes 161–163 and accompanying text.

\(^{167}\) Miller & Park, supra note 159, at 6.

\(^{168}\) William Greene, *The Behaviour of the Maximum Likelihood Estimator of Limited Dependent
Thus, the oft-voiced critic that linear probability models perform poorly when there are very few events (i.e., no loans were accepted), or very few nonevents (i.e., almost all loans are accepted) is not an issue.\(^\text{169}\) Lastly, we are mostly interested in calculating marginal effects for each of the pair-combinations, and less concerned about making predictions or forecasts of the full model. Accordingly, the concern that a linear probability model could produce predictions of a probability greater (or less than) one is not an issue. We turn to review other potential pitfalls discussed in the home mortgage literature which are not specific to Equation 1.

\subsection*{b. Demographics as an Endogenous Instrument for Economic Conditions.}

Many early studies of home mortgage discrimination pointed to the possibility of race, or any other demographic, as nothing more than a proxy for another, unobserved variable.\(^\text{170}\) For instance, if African Americans disproportionately apply for home loans in more economically disadvantaged neighborhoods, lenders may be more likely to deny the loan application. The reason is not due to racial discrimination, but rather due to the perceived high risk of extending a loan to applicants residing in such neighborhoods. As mentioned previously, economists often refer to this type of discrimination as \textit{statistical discrimination}: discrimination that is based on a factor other than a demographic characteristic.

Our study finds more conclusive evidence that the motivation for discrimination is taste-based or bigotry than any previous econometric study. The reason is that, unlike with race, lenders are less likely to rely on perceived sexual orientation as a proxy for increased risk. Moreover, given the sheer magnitude of the dataset HMDA offers, we are able to control for lender by county-fixed-effects. That is, our analysis compares loan applications considered by the same lender from those who reside in the same county, which by definition has the same risk to the lender (they are all FHA loans).\(^\text{171}\) It is thus very likely that the reason for any disparate treatment was not based on factors relevant to risk-assessment, but on the applicants’ perceived sexual orientation.

\subsection*{c. Risks Observed by the Bank but not by the Researcher.}

There is also some concern that there are factors that the lenders are able to observe and include in a risk assessment of the loan application that we, as researchers, are not able to observe in the data. The most glaring example is credit scores, which is probably the single strongest indicator of risk and is a factor observed by the lender. However, despite its richness, HMDA does not include credit scores. However, as explained in Part III.A.1. above, our research design allows us to address and mitigate this concern in a myriad of ways, one of which is by focusing solely on FHA loans.\(^\text{172}\) The unique feature of these loans is that they carry the same low

\begin{footnotes}
\footnote{169} Gary King & Langche Zeng, \textit{Logistic Regression in Rare Events Data}, \textit{Political Analysis} 137–163 (2001).
\footnote{170} Carr & Megbolugbe, \textit{supra} note 117; Browne & Tootell \textit{supra} note 117.
\footnote{171} For computation reasons, we include county-by-bank fixed effects. Our results are insensitive to the inclusion of census-tract-by-bank fixed effects, which is an even more direct measure of neighborhood effects.
\footnote{172} For a recent article employing a similar strategy, see Buchak & Jorring, \textit{supra} note 160.
\end{footnotes}
level of risk to the lender. An applicant approved for an FHA loan pays an FHA insurance premium. In case of a default, the lender recoups the losses from the government. As a result, every FHA loan bears the same risk and expected return to the lender regardless of the demographic characteristics of the applicant. Accordingly, it is unlikely that disparate treatment in FHA loan denial can be traced to an unobserved (to the researcher) measure of risk.

One caveat, however, is that we cannot observe whether a loan was denied by the bank or by the FHA. The concern is that some loans were classified as being “denied” by the lender, when, in fact, the loan was denied by the FHA. We have reason to believe, however, that this happens infrequently. Additionally, and importantly, we are able to control for the reason the loan was denied, including whether the denial was based on credit history. This should mitigate any “exceptionally poor credit history” effect.

C. Results

The results for our main analysis of Equation 1 can be seen in Table 2 and graphically in Figure 7.

<table>
<thead>
<tr>
<th>Applicant</th>
<th>Co-Applicant</th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>White Male</td>
<td>Black Male</td>
<td>-0.038†</td>
<td>-0.043‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.017)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>White Male</td>
<td>White Male</td>
<td>-0.021‡</td>
<td>-0.025‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.005)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Black Male</td>
<td>Black Male</td>
<td>-0.087‡</td>
<td>-0.075‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.008)</td>
<td>(0.009)</td>
</tr>
<tr>
<td>Black Male</td>
<td>White Male</td>
<td>-0.070‡</td>
<td>-0.068‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.014)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>White Female</td>
<td>Black Male</td>
<td>0.012‡</td>
<td>0.040‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.004)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>White Female</td>
<td>White Male</td>
<td>0.012‡</td>
<td>0.037‡</td>
</tr>
</tbody>
</table>

 Each column in this table represents a unique regression where the unit of observation is at the individual loan application level. Column (1) includes year- and county-by-bank fixed effects and Column (2) includes the same fixed effects plus the other controls mentioned in the text. Each row represents the marginal effect of the probability of a loan getting accepted for the associated pairing of applicant and co-applicant where the comparison group is a white male applicant with a white female co-applicant. Robust standard errors clustered at the state level are reported below in parentheses. Statistical significance levels are marked as ^ p<0.10 † p<0.05 ‡ p<0.01.

\footnote{173} \textit{Id.}

\footnote{174} For a discussion on how loan applicants decide to apply for a FHA loan, see P.H. Hendershott et al., \textit{Debt Usage and Mortgage Choice: The FHA Conventional Decision}, 41 \textit{JOURNAL OF URBAN ECONOMICS} 202–217 (1997).

\footnote{175} Each column in this table represents a unique regression where the unit of observation is at the individual loan application level. Column (1) includes year- and county-by-bank fixed effects and Column (2) includes the same fixed effects plus the other controls mentioned in the text. Each row represents the marginal effect of the probability of a loan getting accepted for the associated pairing of applicant and co-applicant where the comparison group is a white male applicant with a white female co-applicant. Robust standard errors clustered at the state level are reported below in parentheses. Statistical significance levels are marked as ^ p<0.10 † p<0.05 ‡ p<0.01.
In Table 2, column (1) estimates Equation 1 with the inclusion of year and bank-by-county fixed effects, but with no other controls. Column (2) reports the results with the controls mentioned previously. In both models, the corrected standard errors clustered at the state level are in parenthesis. Each coefficient can be interpreted as the percentage point increase (if positive) or decrease (if negative) of a loan to be accepted for each applicant/co-applicant pair relative to a white male/white female applicant. For instance, from Column (2) in Table 2, a pair consisting of a white male applicant and a black male co-applicant is 4.3 percentage points less likely to have a loan accepted as a white male/white female pair asking for the same loan amount with the same income from the same lender in the same county. This means that if a white male/black male pair has a 45% chance of having a loan application accepted, we would expect a white male/white female pair to have a 49.3% chance of approval. This is so despite the fact that both pairs requested the same amount for the same purpose with the same income from the same lender in the same county and bear the same level of risk to the lender.

\[ \text{In Table 2, column (1) estimates Equation 1 with the inclusion of year and } \\
\text{bank-by-county fixed effects, but with no other controls. Column (2) reports the} \\
\text{results with the controls mentioned previously. In both models, the corrected} \\
\text{standard errors clustered at the state level are in parenthesis. Each coefficient can} \\
\text{be interpreted as the percentage point increase (if positive) or decrease (if} \\
\text{negative) of a loan to be accepted for each applicant/co-applicant pair relative to a} \\
\text{white male/white female applicant. For instance, from Column (2) in Table 2, a} \\
\text{pair consisting of a white male applicant and a black male co-applicant is 4.3} \\
\text{percentage points less likely to have a loan accepted as a white male/white female} \\
\text{pair asking for the same loan amount with the same income from the same lender} \\
\text{in the same county. This means that if a white male/black male pair has a 45%} \\
\text{chance of having a loan application accepted, we would expect a white male/white female} \\
\text{pair to have a 49.3% chance of approval. This is so despite the fact that both pairs} \\
\text{requested the same amount for the same purpose with the same income from the} \\
\text{same lender in the same county and bear the same level of risk to the lender.} \]
Figure 7: Effect of Gender and Racial Composition on Co-applicant Loan Acceptance

Figure 7 organizes the results in Table 2 from most negative to most positive and includes bands that represent 90% confidence intervals. To interpret Figure 7, focus first on the points at the center of the intervals. A point that lies below the zero line suggests the race/gender pairing is less likely to have a loan accepted, and a point above the line suggests the acceptance is more likely. Now focus on the intervals. If an interval intersects with the zero line on the horizontal axis, the estimated effect is not statistically significant at the 10% level.

To test once again whether the results are driven by same-sex gay co-applicants (rather than by heterosexual same-sex parent-child co-applicants),

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177 This figure displays graphically the empirical results from Column (2) in Table 2. The marginal effect for each gender and race combination is measured on the vertical axis, and each combination of race and gender is measured on the horizontal axis. The first letter in each horizontal axis label represents the race of the applicant, the second letter signifies the gender, the third letter represents the race of the co-applicant, and the last letter represents the gender of the co-applicant. For example, “bmbm” stands for a black male applicant with a black male co-applicant. In this specification and all other specifications, we make no assumption about the symmetry of an applicant and co-applicant. That is, there might be reason to believe wmbm may be treated differently from the symmetric pairing, bmwm, so we treat each as different. This allows us to check if there is a “primary applicant” or “secondary applicant” effect.

178 Two additional robustness tests are reported in Part II.A.2. See supra notes 154–157 and accompanying text.
we remove from the dataset any applicant or co-applicant that reports more than one race. The rationale is that keeping only “single race” applicants would (likely) exclude from the data parent-and-child co-applicants. The reason is that it is unlikely that a co-applicant who reports one race (e.g., black) will be the parent of the applicant with only one different race (e.g., white). We report the results of this regression graphically in Figure 8 below. Figure 8 shows that when the sample is restricted to the types of same-sex loans that are more likely representative of actual gay couples (Group 2179), the results hold and in some cases are slightly stronger.

Figure 8: Effect of Gender and Racial Composition on Application Success Where the Loans are More Likely to be Submitted by Gay Couples

The empirical evidence suggests that a large portion of the same-sex loan applications are actually loans submitted by gay couples. Moreover, the differences between the results reported in Figure 7 and 8 are also consistent with our theoretical prediction in Part III.A that our results are a conservative measure of discrimination and that the actual level of discrimination is higher than we observe.180

1. National Patterns in Discrimination

With this in mind, we can turn to analyze the results. Figure 7 provides strong evidence of systemic and widespread discrimination against gay male

179 See supra note 153 and accompanying text.
180 Id.
couples. More specifically, Figure 7 shows that any application with a pair of males is statistically less likely to be approved relative to the same white heterosexual pair. Within the pair-male groups, race plays a role. Although all male/male applications are less likely to be accepted, black male pairs are the least likely to be approved (-7.5 percentage points), followed by the interracial pairs of black male/white male (-6.8), white male/black male (-4.3) and white male pair (-2.5). Interestingly, the exact same pattern holds for female pairs. From the least to most likely to be approved are black female pairs, followed by interracial pair of black female/white female pair, white female/black female pair, and white female pairs. In the case of same-sex pairs (i.e., male/male and female/female), the data reveals some evidence of a “primary applicant” effect. The differences between interracial pairs, however, are statistically indistinguishable from one another.

Our results shed new light on earlier studies of discrimination. Previous research has suggested significant and persistent evidence of racial discrimination in mortgage lending. Recent estimates suggest that black pairs are six percentage points less likely to have a loan accepted. The evidence presented here suggests the possibility of a more nuanced story. While race seems to play an important role in the probability of getting a home loan, interracial applicants and even white male pair applicants are statistically less likely to get accepted. The fact that even white male pairs are less likely to get a loan in a similar fashion to other same-sex male pairs is important for at least two reasons. First, it is evidence of discrimination on the basis of sexual orientation that is consistent with the pair-studies literature. Second, the evidence of discrimination against white male pairs alleviates the concern that Figure 7 measures nothing more than racial discrimination. Finally, even after we control for the gender composition of the same-sex pair applicants, we do find that compared to the white heterosexual baseline, a pair consisting of a black male and a black female is still statistically less likely to have a home loan application accepted. The effect, however, is about half as small as previous estimates.

Another interesting finding in our results is the lack of symmetry of effect between perceived gay and perceived lesbian co-applicants. We find that every possible racial combination of male pairs is statistically disadvantaged when getting a loan application approved. By contrast, in every case, a female pair is either statistically indistinguishable from the baseline group or actually has a higher likelihood of getting the loan accepted. This result is actually consistent with much of the experimental literature on sexual orientation in housing and

181 Buchak & Jorring, supra note 160.
182 Buchak & Jorring, supra note 160 (6 percentage points); Munnell et al. supra note 113 (between 6 and 8 percentage points).
183 Lauster & Easterbrook, supra note 110, found a similar result, and while Friedman et al., supra note 130 found similar effects for gay men and lesbian women, the effect was larger for gay men.
coincides well with the growing body of literature about the social acceptability of lesbian but not gay male relationships.\textsuperscript{184} It is important to remember that we do not observe the nature of the relationship between the co-applicants in the data.\textsuperscript{185} However, unlike other salient bases for discrimination (e.g., race), sexual orientation is much harder to observe for the loan officer. In many cases, the loan officer does not know with certainty either the orientation of the applicants or their relationship. In the absence of a clear signal, the discriminatory loan officer (either overtly or inadvertently) likely uses the gender and race of the co-applicants as a proxy for sexual orientation. We, in effect, use the same proxy in this study. The proxy is admittedly imperfect. Surely there are times that the bias, even an intentional one, would be the result of a mistake. For instance, the loan officer may mistakenly believe that a pair of heterosexual male applicants is involved in a homosexual relationship. These heterosexual applicants may thus receive the same discriminatory treatment as if they were gay. In our eyes (and in the eyes of the law), this does not mean the analysis is flawed. All it means is that discrimination is happening against gay applicants and applicants who are perceived to be gay. What could result, potentially, is actually “over-discrimination”—that is, discrimination against the targeted group and, in addition, discrimination against others who are perceived as belonging to the targeted group.

2. Regional Patterns in Discrimination

The results reported in Table 2 and Figure 7 above measure the average within bank-county effect across the entire country. There may be reason, however, to believe that different regions in the U.S. or different types of banks discriminate differently. To control for this, we first divide the data into four Census regions, re-estimate Equation 1, and replicate Table 2 and Figure 7. Instead of looking at the effect of a gender and race applicant make-up of, say, black male/black male, we divide that single dummy variable into four variables that signify if the loan was filed by (a) a pair of black males in the West, (b) a pair of black males in the South, (c) a pair of black males in the Midwest, or (d) a pair of black males in the Northeast.\textsuperscript{186} We do this for each of our fifteen gender and race indicator variables of interest, and include them all in the regression. What results is an estimate of the probability of loan acceptance for each group by region. Those results are summarized in Table 3 and Figure 9 below.


\textsuperscript{185} See supra Part III.A.2 and notes 178–180 and accompanying text.

\textsuperscript{186} For more on Census regions, see \url{https://www.census.gov/geo/reference/gtc/gtc_census_divreg.html} (last visited Jan. 31, 2018).
Table 3: Probability of Loan Acceptance by Race and Gender and Region

<table>
<thead>
<tr>
<th>Applicant</th>
<th>Co-Applicant</th>
<th>Northeast</th>
<th>Midwest</th>
<th>South</th>
<th>West</th>
</tr>
</thead>
<tbody>
<tr>
<td>White Male</td>
<td>Black Male</td>
<td>-0.122‡</td>
<td>-0.018</td>
<td>-0.027^</td>
<td>-0.015</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.030)</td>
<td>(0.035)</td>
<td>(0.016)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>White Male</td>
<td>White Male</td>
<td>-0.023‡</td>
<td>-0.019‡</td>
<td>-0.020‡</td>
<td>-0.033‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.005)</td>
<td>(0.005)</td>
<td>(0.004)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Black Male</td>
<td>Black Male</td>
<td>-0.076‡</td>
<td>-0.086‡</td>
<td>-0.076‡</td>
<td>-0.052‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.006)</td>
<td>(0.015)</td>
<td>(0.013)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Black Male</td>
<td>White Male</td>
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<td>-0.072†</td>
<td>-0.061‡</td>
<td>-0.065†</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.010)</td>
<td>(0.031)</td>
<td>(0.020)</td>
<td>(0.032)</td>
</tr>
<tr>
<td>White Female</td>
<td>Black Male</td>
<td>0.033†</td>
<td>0.035‡</td>
<td>0.042‡</td>
<td>0.046‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.012)</td>
<td>(0.010)</td>
<td>(0.007)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>White Female</td>
<td>White Male</td>
<td>0.037‡</td>
<td>0.037‡</td>
<td>0.039‡</td>
<td>0.036‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.003)</td>
<td>(0.003)</td>
<td>(0.003)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Black Female</td>
<td>Black Male</td>
<td>0.006^</td>
<td>-0.001</td>
<td>0.007</td>
<td>0.005</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.003)</td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Black Female</td>
<td>White Male</td>
<td>0.027‡</td>
<td>0.041‡</td>
<td>0.006</td>
<td>0.021^</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.004)</td>
<td>(0.013)</td>
<td>(0.009)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>White Female</td>
<td>Black Female</td>
<td>-0.008</td>
<td>0.031</td>
<td>0.021</td>
<td>0.016</td>
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<td></td>
<td>(0.024)</td>
<td>(0.046)</td>
<td>(0.021)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>White Female</td>
<td>White Female</td>
<td>0.026‡</td>
<td>0.026‡</td>
<td>0.028‡</td>
<td>0.026‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.005)</td>
<td>(0.007)</td>
<td>(0.005)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Black Female</td>
<td>Black Female</td>
<td>-0.003</td>
<td>-0.027‡</td>
<td>-0.004</td>
<td>-0.040‡</td>
</tr>
<tr>
<td></td>
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<td>(0.007)</td>
<td>(0.010)</td>
<td>(0.008)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Black Female</td>
<td>White Female</td>
<td>0.033</td>
<td>-0.032</td>
<td>-0.007</td>
<td>-0.017</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.021)</td>
<td>(0.022)</td>
<td>(0.010)</td>
<td>(0.044)</td>
</tr>
<tr>
<td>White Male</td>
<td>Black Female</td>
<td>0.009</td>
<td>-0.009</td>
<td>-0.005</td>
<td>-0.001</td>
</tr>
<tr>
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<td></td>
<td>(0.012)</td>
<td>(0.007)</td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Black Male</td>
<td>Black Female</td>
<td>-0.019‡</td>
<td>-0.029‡</td>
<td>-0.019§</td>
<td>-0.021‡</td>
</tr>
<tr>
<td></td>
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<td>(0.004)</td>
<td>(0.005)</td>
<td>(0.003)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Black Male</td>
<td>White Female</td>
<td>0.017‡</td>
<td>0.002</td>
<td>0.005</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.002)</td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.003)</td>
</tr>
</tbody>
</table>

Sample Size 5,864,086

187 Unlike Table 2, each column in Table 3 reports results for the same regression that splits the effect of each race and gender pairing by region. This regression includes all the same controls and fixed effects as Column (2) of Table 3, and the standard errors are clustered at the state level. While we do not report the results by region without controls similar to Column (1) of Table 2, the results are available upon request and are virtually the same as the results reported in Table 3. Statistical significance levels are marked as ^ p<0.10 † p<0.05 ‡ p<0.01.
Table 3 and Figure 9 indicate that all regions in the U.S. exhibit the same pattern of discrimination we observe on the national level. In each region, all four groups (male, female, black, and white applicants) are discriminated against based on perceived sexual orientation. In other words, all applications filed by same-sex pairs are less likely to be accepted compared to the white heterosexual pair. In certain circumstances, the negative effect is statistically insignificant, but it is still negative. For instance, in the Midwest an application filed by an interracial same-sex pair consisting of a white male and black male application is statistically indistinguishable from zero. By contrast, an application filed by other same-sex male pairs is statistically less likely to get accepted by the same bank in the same county as the white heterosexual pair application. That same group of applicants—white male/black male—is also statistically insignificant in the West, but again, each of the other three highlighted same sex groups are still statistically worse off. Interestingly, the data suggests that this interracial pair is discriminated against most in the Northeast. In that region, applications from white male/black male pairs are 12.2 percentage points less likely to be approved. The result is not only the worst in magnitude, but is also the most statistically significant (at the 99% level).
Figure 9: Effect of Gender and Racial Composition on Co-applicant Loan Acceptance by Region

Based on the results in Figure 9, there is no evidence that any single region in the U.S. is driving the results. There may have been a temptation to blame certain regions of the U.S. for the discrimination that seems to be occurring. However, the data suggests that discrimination based on (perceived) sexual orientation is widespread and not isolated to any specific geographic region. Put differently, no region is insulated from those discriminatory behaviors.

To explore this point further, we repeat the regional analysis above but instead of splitting the data by region, we split it by political party lines. To do so, we sort each state into one of three categories: states commonly considered “blue” states that basically always vote Democrat in a presidential election, “red” states that basically always vote Republican in a presidential election, or “swing” states that could go either way. The results for this regression are summarized in Table 4 and Figure 10.

<table>
<thead>
<tr>
<th>Applicant</th>
<th>Co-Applicant</th>
<th>Democrat</th>
<th>Republican</th>
<th>Swing</th>
</tr>
</thead>
<tbody>
<tr>
<td>White Male</td>
<td>Black Male</td>
<td>-0.068†</td>
<td>-0.040^</td>
<td>-0.008</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.026)</td>
<td>(0.021)</td>
<td>(0.021)</td>
</tr>
<tr>
<td>White Male</td>
<td>White Male</td>
<td>-0.033‡</td>
<td>-0.017‡</td>
<td>-0.021‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.005)</td>
<td>(0.005)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Black Male</td>
<td>Black Male</td>
<td>-0.068‡</td>
<td>-0.071‡</td>
<td>-0.088‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.009)</td>
<td>(0.010)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>Black Male</td>
<td>White Male</td>
<td>-0.087‡</td>
<td>-0.044†</td>
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<td>(0.019)</td>
<td>(0.020)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>White Female</td>
<td>Black Male</td>
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<td>0.042‡</td>
<td>0.044‡</td>
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<tr>
<td></td>
<td></td>
<td>(0.006)</td>
<td>(0.005)</td>
<td>(0.010)</td>
</tr>
</tbody>
</table>

188 While the interpretation of the intervals and the points in the intervals is the same as in Figure 7, it is important to note that each region subfigure in Figure 9 is derived from the same regression—reported in Table 4—aimed to tease out regional effects in discrimination based on sexual orientation. More on the specifics of how each coefficient was estimated can be found in Table 4.

189 Those states are WA, OR, CA, HI, NM, IL, ME, VT, NY, MA, CT, RI, NJ, DE, and MD.

190 AK, AZ, UT, ID, WY, MT, ND, SD, NE, KS, OK, TX, LA, AR, MO, IN, KY, TN, WV, MS, AL, GA, and SC.

191 NV, CO, MN, IA, WI, MI, OH, PA, NH, VA, NC, and FL. These characterizations are based on the outcomes of previous presidential elections as reported by www.270towin.com though the results are insensitive to alternative specifications of swing states.

192 This Table is organized in the same manner as Table 5. Each column includes estimates for each applicant/co-applicant pairing from the same regression that includes all the controls and fixed effects. Each column also clusters the robust standard errors at the state level. Statistical significance levels are marked as ^ p<0.10 † p<0.05 ‡ p<0.01.
<p>| | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
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</tr>
</thead>
<tbody>
<tr>
<td>White Female</td>
<td>White Male</td>
<td>0.036‡ 0.039‡ 0.038‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.003) (0.003) (0.003)</td>
</tr>
<tr>
<td>Black Female</td>
<td>Black Male</td>
<td>0.007^ 0.009† -0.002</td>
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<tr>
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<td></td>
<td>(0.004) (0.003) (0.005)</td>
</tr>
<tr>
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<td>White Male</td>
<td>0.023‡ 0.011 0.022</td>
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<td>(0.006) (0.011) (0.014)</td>
</tr>
<tr>
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<td>Black Female</td>
<td>0.005 0.020 0.024</td>
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<td>(0.016) (0.033) (0.024)</td>
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<td>White Female</td>
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<td>(0.005) (0.005) (0.005)</td>
</tr>
<tr>
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<td>Black Female</td>
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</tr>
<tr>
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<td></td>
<td>(0.010) (0.008) (0.010)</td>
</tr>
<tr>
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<td>White Female</td>
<td>-0.008 -0.015 0.005</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.029) (0.016) (0.018)</td>
</tr>
<tr>
<td>White Male</td>
<td>Black Female</td>
<td>0.005 -0.006 -0.009</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.006) (0.005) (0.005)</td>
</tr>
<tr>
<td>Black Male</td>
<td>Black Female</td>
<td>-0.020‡ -0.016‡ -0.030‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.002) (0.002) (0.003)</td>
</tr>
<tr>
<td>Black Male</td>
<td>White Female</td>
<td>0.004 0.008^ 0.003</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.003) (0.004) (0.002)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Sample Size</th>
<th>5,864,086</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>R Squared</td>
<td>0.42</td>
</tr>
</tbody>
</table>

To compare the patterns of discrimination based on political party lines to those observed in the national pattern, Figure 10 includes the graph from Figure 7. Similar to the analysis by region, we see no obvious pattern that follows party lines. While there are some differences for specific groups across party lines, most of those differences are small and statistically insignificant. Thus, while the magnitude of the effect varies slightly, the application of a pair consisting of two black males is no better positioned to be accepted in, for example, Utah, than Oregon or Wisconsin (all else equal). One interesting exception is that democratic states are the least tolerant to two groups of interracial male pairs. The results are not only substantially stronger in magnitude—in the case of the black/white male pair, the chance to be approved in a democratic state is about half compared to those in republican states—but they are also most statically significant. More broadly, Figure 10 demonstrates that these lending patterns cannot be simply attributed to the mindset of a specific region or a certain political philosophy. These patterns of discrimination are widespread and can be observed in virtually every geopolitical segment of the U.S.
One last potential source of variation that may be driving the results is not necessarily the makeup of the community, but rather the lenders themselves. Though unlikely, given the within bank-county analysis we conduct in this study, there may be something about the way large versus medium or smaller banks operate that may inform the results presented here. For example, one could argue that larger banks are less likely to discriminate, because, among other possible reasons, they may have better procedures or they have higher exposure. To test this, we split out the data by the ten largest banks in terms of loan applications received. These banks make up about 40% of the loan applications in our database. We then compare the “Top 10” banks to all banks in the smallest 25% of loan applications handled and all other medium banks in between. The results, presented in Table 5 and Figure 11, suggest a very similar pattern. Large, medium, and smaller banks act very much in the same way.

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193 This figure replicates the results presented in Figure 9 in every way except where each coefficient is split by party lines instead of region. Additionally, the main result from Figure 2 is included in the top left quadrant by way of comparison.

194 In our dataset, those banks include loandepot.com, Flagstar Bank, Freedom Mortgage, Advanced Financial Services, JP Morgan Chase, Quicken Loans, Bank of America, and three different classifications for Wells Fargo.
### Table 5: Probability of Loan Acceptance by Race and Gender and Bank Size

<table>
<thead>
<tr>
<th>Applicant</th>
<th>Co-Applicant</th>
<th>10 Largest Banks</th>
<th>Medium Banks</th>
<th>Smallest Banks</th>
</tr>
</thead>
<tbody>
<tr>
<td>White Male</td>
<td>Black Male</td>
<td>-0.019</td>
<td>-0.056†</td>
<td>-0.060†</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.022)</td>
<td>(0.025)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>White Male</td>
<td>White Male</td>
<td>-0.026‡</td>
<td>-0.023‡</td>
<td>-0.026‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.004)</td>
<td>(0.005)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Black Male</td>
<td>Black Male</td>
<td>-0.040‡</td>
<td>-0.098‡</td>
<td>-0.102‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.009)</td>
<td>(0.013)</td>
<td>(0.013)</td>
</tr>
<tr>
<td>Black Male</td>
<td>White Male</td>
<td>-0.056‡</td>
<td>-0.062‡</td>
<td>-0.105‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.016)</td>
<td>(0.020)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>White Female</td>
<td>Black Male</td>
<td>0.031‡</td>
<td>0.042‡</td>
<td>0.057‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.006)</td>
<td>(0.007)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>White Female</td>
<td>White Male</td>
<td>0.036‡</td>
<td>0.039‡</td>
<td>0.039‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.002)</td>
<td>(0.003)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Black Female</td>
<td>Black Male</td>
<td>0.025‡</td>
<td>-0.010†</td>
<td>-0.013†</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.004)</td>
<td>(0.005)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Black Female</td>
<td>White Male</td>
<td>0.019†</td>
<td>0.020†</td>
<td>0.014</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.013)</td>
</tr>
<tr>
<td>White Female</td>
<td>Black Female</td>
<td>0.010</td>
<td>0.020</td>
<td>0.009</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.022)</td>
<td>(0.021)</td>
<td>(0.034)</td>
</tr>
<tr>
<td>White Female</td>
<td>White Female</td>
<td>0.024‡</td>
<td>0.030‡</td>
<td>0.025‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.005)</td>
<td>(0.007)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Black Female</td>
<td>Black Female</td>
<td>0.017†</td>
<td>-0.031‡</td>
<td>-0.026†</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.007)</td>
<td>(0.010)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>Black Female</td>
<td>White Female</td>
<td>-0.032</td>
<td>0.005</td>
<td>0.018</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.022)</td>
<td>(0.022)</td>
<td>(0.025)</td>
</tr>
<tr>
<td>White Male</td>
<td>Black Female</td>
<td>-0.014†</td>
<td>0.002</td>
<td>0.013^</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.006)</td>
<td>(0.006)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Black Male</td>
<td>Black Female</td>
<td>-0.003</td>
<td>-0.035‡</td>
<td>-0.033‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.004)</td>
<td>(0.002)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Black Male</td>
<td>White Female</td>
<td>-0.003</td>
<td>0.011‡</td>
<td>0.008^</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.005)</td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
</tbody>
</table>

Sample Size 5,864,086
R Squared 0.42

---

195 In this table, each race and gender pairing was sorted by whether the application went to a “big bank,” medium bank, or a smaller bank. The results in this table are generated by the same regression and are organized similarly to the previous two tables. Statistical significance levels are marked as ^ p<0.10 † p<0.05 ‡ p<0.01.
It is important to note, however, that among the largest banks, the magnitudes of the effects are smaller in ways that are, at times, meaningful. For instance, an application filed by a pair consisting of two black male co-applicants at a large bank is only four percentage points less likely to get accepted by the Top 10 banks, compared to nine at all other banks. Also, the effect for applications filed by interracial pairs consisting of a white male and a black male co-applicants is much smaller for big banks and statistically insignificant.

Figure 11: Effect of Gender and Racial Composition on Co-Applicant Loan Acceptance by Bank Size

3. Remedies for Reversing Trends of Systematic and Widespread Bias

The evidence we present here suggests a systematic and widespread bias against FHA loan applications filed by any male pair, regardless of race. It may be the case, though, that the reason for these discriminatory patterns is more nuanced. For instance, it could be that the results are not an artifact of regional differences in attitudes or political opinions, but the lack of contact with the affected group. Assuming this is true, what can be done to remedy the situation?

a. The Contact Theory. To understand the Contact Theory, suppose the average loan officer identifies as straight and has well-established biases against the LGB community—specifically, biases against gay men. Research shows that

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196 Both subfigures come from a single regression reported in Table 6. This figure reads and interprets the same as each previous figure.
efforts to reduce such intergroup bias in a meaningful and enduring way demand thorough and intense intervention lasting over a long period.\textsuperscript{197} One type of intervention that has had some reported effect in reducing intergroup bias is sustained contact with individuals of the affected group.\textsuperscript{198} The idea behind the prevailing (yet debated) “Contact Hypothesis” is that, under certain conditions, intergroup bias would decrease as individual contact between different groups’ members increases.\textsuperscript{199}

Could it be that our results are being driven by a lack of sustained contact with the affected groups? We test this by comparing loan acceptance rates between urban and rural environments. Under the Contact Hypothesis, one would expect to observe less discrimination in urban environments. Based simply on sheer population numbers, it is likely that the average loan officer operating in an urban area has a higher chance of sustained contact with gay men. We loosely define an environment as urban if its population density is above average, and rural if its population density is below average.\textsuperscript{200} Table 6 and Figure 12 below report the results for each of the gender and race pair variations in these environments.

<table>
<thead>
<tr>
<th>Applicant</th>
<th>Co-Applicant</th>
<th>Rural</th>
<th>Urban</th>
</tr>
</thead>
<tbody>
<tr>
<td>White Male</td>
<td>Black Male</td>
<td>-0.039\textsuperscript{†}</td>
<td>-0.048\textsuperscript{†}</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.015)</td>
<td>(0.022)</td>
</tr>
<tr>
<td>White Male</td>
<td>White Male</td>
<td>-0.025\textsuperscript{‡}</td>
<td>-0.025\textsuperscript{‡}</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.003)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Black Male</td>
<td>Black Male</td>
<td>-0.082\textsuperscript{‡}</td>
<td>-0.067\textsuperscript{‡}</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.010)</td>
<td>(0.009)</td>
</tr>
<tr>
<td>Black Male</td>
<td>White Male</td>
<td>-0.054\textsuperscript{‡}</td>
<td>-0.082\textsuperscript{‡}</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.018)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>White Female</td>
<td>Black Male</td>
<td>0.039\textsuperscript{‡}</td>
<td>0.041\textsuperscript{‡}</td>
</tr>
</tbody>
</table>

\textsuperscript{197} For a review of intergroup bias and attempts to decrease it, see Thomas F. Pettigrew & Linda R. Tropp, \textit{A Meta-Analytic Test of Intergroup Contact Theory}, 90 \textit{JOURNAL OF PERSONALITY AND SOC. PSYCHOLOGY} 751–83 (2006).
\textsuperscript{198} As an example, Shana Levin et al., \textit{The Effects of Ingroup and Outgroup Friendships on Ethnic Attitudes in College: A Longitudinal Study}, 6 \textit{GROUP PROCESSES & INTERGROUP RELATIONS} 76–92 (2003) found that college students exposed to intergroup relationships were more likely to report having friends from those groups and less likely to display biased behaviors in subsequent college years.
\textsuperscript{199} David Brookman & Joshua Kalla, \textit{Durably Reducing Transphobia: A Field Experiment on Door-to-Door Canvassing}, \textit{SCIENCE} 220 (2016).
\textsuperscript{201} See Tables 3 through 5 for more information on interpreting this table.
Table 3: Effect of Gender and Racial Composition on Co-Applicant Loan Acceptance by Population Density

<table>
<thead>
<tr>
<th>Gender Combination</th>
<th>Co-Applicant Loan Acceptance Rate</th>
<th>Standard Error</th>
</tr>
</thead>
<tbody>
<tr>
<td>White Female</td>
<td>0.035‡</td>
<td>0.003</td>
</tr>
<tr>
<td>Black Female</td>
<td>0.005</td>
<td>0.003</td>
</tr>
<tr>
<td>White Male</td>
<td>0.040‡</td>
<td>0.003</td>
</tr>
<tr>
<td>Black Male</td>
<td>0.005</td>
<td>0.005</td>
</tr>
<tr>
<td>Black Female</td>
<td>0.020‡</td>
<td>0.007</td>
</tr>
<tr>
<td>White Male</td>
<td>0.017^</td>
<td>0.009</td>
</tr>
<tr>
<td>White Female</td>
<td>0.004</td>
<td>0.018</td>
</tr>
<tr>
<td>Black Female</td>
<td>0.005</td>
<td>0.003</td>
</tr>
<tr>
<td>White Male</td>
<td>0.010</td>
<td>0.010</td>
</tr>
<tr>
<td>Black Female</td>
<td>-0.011^</td>
<td>0.007</td>
</tr>
<tr>
<td>White Male</td>
<td>-0.023</td>
<td>0.022</td>
</tr>
<tr>
<td>Black Female</td>
<td>0.010</td>
<td>0.020</td>
</tr>
<tr>
<td>White Male</td>
<td>-0.013‡</td>
<td>0.004</td>
</tr>
<tr>
<td>Black Female</td>
<td>-0.026‡</td>
<td>0.002</td>
</tr>
<tr>
<td>White Male</td>
<td>0.000</td>
<td>0.004</td>
</tr>
<tr>
<td>Black Female</td>
<td>0.010‡</td>
<td>0.003</td>
</tr>
</tbody>
</table>

Sample Size: 5,864,086
R Squared: 0.42

Figure 12: Effect of Gender and Racial Composition on Co-Applicant Loan Acceptance by Population Density

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202 This figure reports the regression results of Table 3. See Figure 5 for more details on the construction and interpretation of this table.
We find the same pattern of systemic discrimination with virtually no evidence of any difference in loan application acceptance between rural and urban environments. While the point estimates vary slightly, the differences are not significant, and each of the four disadvantaged pair groups is still less likely to get a loan approved in a statistically significant way. Thus, if comparing rural to urban settings is a reasonable proxy for association with gay men, it appears that contact alone may not reduce intergroup bias.

b. State and Local Laws. As explained earlier, discrimination based on sexual orientation is not prohibited under federal law. In light of federal inaction, 23 states and over 400 localities passed laws expressly prohibiting discrimination based on sexual orientation in lending. These “local” laws may influence lenders’ behaviors. The reason could be intrinsic. Expressive law theorists, for example, argue that “the mere existence of [a] law helps to shape and define [people’s] world views” and increase compliance. Or the reason may be extrinsic. For example, it could be that local laws result in increased compliance due to enforcement efforts or fear thereof. Alternatively, it could be that jurisdictions that adopt local anti-discrimination laws do so because they are already more accepting of gay applicants.

Whatever the reason may be, it is clear that the minority of states and localities that adopted these laws were not selected randomly to do so. This is important because, hypothetically, the best way to measure the efficacy of a law, policy, or any type of policy intervention, would have been to randomly assign the law to half the states and localities and keep the remaining half as a control group. In this hypothetical, states and localities would have no control over whether they got the law; thus, any difference in the underlying characteristics between the states and localities would be random. The reality, however, is that anti-discrimination laws are not randomly assigned to state and local jurisdictions. Similarly, states and localities that declined to adopt such protections did not randomly choose to refrain from doing so. This calls into question their validity as the comparison group and could bias the results. This bias would manifest if we

203 The Equal Access Rule, which prohibits lenders of FHA loans from engaging in this form of discrimination, is an administrative rule. As HUD explicitly admits, the Rule does not create a new right for aggrieved parties. Equal Access Rule, supra note 14, at 5670–71.
205 As a hypothetical, imagine a variable called “tolerance towards the LGB community.” If this were a variable we could measure, we might find that this variable is highly correlated with the passage of local laws prohibiting sexual orientation discrimination. That is, localities that are more tolerant towards the LGB community might be more likely to pass laws protecting LGB individuals. The “tolerance towards the LGB community” variable would most likely also be correlated with our outcome of interest in our regressions: the probability of getting a loan approved for a perceived gay couple. Thus, if we were to just compare loan acceptance rates from (state and local) jurisdictions with laws protecting LGB individuals to jurisdictions without such laws, we might misinterpret any difference as caused by the law when, in fact, the driving force of the difference is not the law, but rather the “tolerance towards the LGB community” variable.
simply compared states with laws to states without laws.\textsuperscript{206} To alleviate this potential source of statistical bias, we focus only on states and localities that changed their laws within the time window of our dataset, 2010-2015. This allows us to compare the same local jurisdiction to itself before and after it passed the law. Only three states—Maine, Nevada, and Utah—and 174 local municipalities changed their laws during this window. These states and localities processed almost a quarter million loan applications from 2010 to 2015.

To analyze the effectiveness of local laws, we employ a “difference-in-differences” regression technique. This technique, common in policy analysis,\textsuperscript{207} is a method that helps alleviate the lack of an appropriate baseline group by focusing on two baselines. The method works as follows. First, we focus on jurisdictions that adopted local laws. Each such jurisdiction is compared to itself. Specifically, we calculate the acceptance rate in that jurisdiction before the local law was passed and compare it to the rate after it was passed. This generates the “first” difference. For example, Nevada passed a law prohibiting sexual orientation discrimination in 2011—the effective date. Therefore, the first step is comparing Nevada to itself. To calculate the first difference, the regression calculates the difference in loan acceptance rates for Nevada before and after the effective date. That is, we compare the acceptance rate in 2011-2015 to the rate in 2010.

This first difference, however, is not enough. It could be that during the year the law changed, other factors that are not unique to Nevada influenced the results. For example, it could be that a regional crisis influenced the acceptance rate in Nevada. To account for this, the regression calculates the change in acceptance rates between 2011 and 2015 to 2010 in jurisdictions that did not adopt a local rule. This is the second difference. It then compares the first difference to the second one.\textsuperscript{208} The idea is that while the assignment of the law is still not random, using this technique filters out any factor relevant to the outcome of interest. Thus, all that remains is the effect of the law and random noise not relevant to the law or the outcome.\textsuperscript{209}

\textsuperscript{206} This is the case because states with laws are probably fundamentally different from states without laws.
\textsuperscript{207} For the appropriate use of difference-in-differences estimators, see Marianne Bertrand et al., \textit{How Much Should We Trust Differences-in-Differences Estimates?}, 119 QUARTERLY JOURNAL OF ECONOMICS 249–75 (2004).
\textsuperscript{208} Specifically, to calculate the second difference, the regression first focuses on jurisdictions that did not adopt a local law. For each of these jurisdictions, it compares the second difference: the acceptance rate before and after the effective date. The regression then compares the first difference (e.g., the acceptance rate in Nevada in the period between 2011-2015 minus the acceptance rate in Nevada in 2010) to the second difference (e.g., acceptance rate in all jurisdictions that did not adopt such laws for the period of 2011 to 2015 minus all the acceptance rates in these states in 2010). Formally, this technique requires the inclusion of group-fixed effects that flag the treated local and state jurisdictions, a time-fixed effect that flags the treated time, and an interaction between the two.
\textsuperscript{209} One of the assumptions of this modeling technique, however, is that the treated state or locality—that is, the jurisdiction that passed a law in our time window—looked much like the
The results for the difference-in-difference estimates of the effect of state and local anti-discrimination laws are presented in Table 7 and Figure 13.

Table 7: Probability of Loan Acceptance by Race and Gender in States with Changes to City or State Law²¹⁰

<table>
<thead>
<tr>
<th>Applicant</th>
<th>Co-Applicant</th>
<th>State Law</th>
</tr>
</thead>
<tbody>
<tr>
<td>White Male</td>
<td>Black Male</td>
<td>-0.086</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.074)</td>
</tr>
<tr>
<td>White Male</td>
<td>White Male</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.008)</td>
</tr>
<tr>
<td>Black Male</td>
<td>Black Male</td>
<td>-0.031†</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.015)</td>
</tr>
<tr>
<td>Black Male</td>
<td>White Male</td>
<td>-0.014</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.057)</td>
</tr>
<tr>
<td>White Female</td>
<td>Black Male</td>
<td>0.012</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.015)</td>
</tr>
<tr>
<td>White Female</td>
<td>White Male</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.003)</td>
</tr>
<tr>
<td>Black Female</td>
<td>Black Male</td>
<td>-0.024‡</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.006)</td>
</tr>
<tr>
<td>Black Female</td>
<td>White Male</td>
<td>-0.004</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.041)</td>
</tr>
<tr>
<td>White Female</td>
<td>Black Female</td>
<td>0.045</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.048)</td>
</tr>
<tr>
<td>White Female</td>
<td>White Female</td>
<td>0.009</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.005)</td>
</tr>
<tr>
<td>Black Female</td>
<td>Black Female</td>
<td>-0.019</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.023)</td>
</tr>
<tr>
<td>Black Female</td>
<td>White Female</td>
<td>-0.067</td>
</tr>
</tbody>
</table>

²¹⁰ In this table, each column represents the difference-in-differences estimate of the effect of the passage of a state law for each applicant/co-applicant pairing. Like other tables, this regression, estimated at the individual loan application level, includes fixed effects for year, bank-by-county, and all other controls included in each regression. Additionally, this regression includes group-fixed effects and a state/locality law time effect to capture the difference-in-differences estimator. Statistical significance levels are marked as ^ p<0.10 † p<0.05 ‡ p<0.01.

jurisdictions that serve as controls prior to the passage of the law, in terms of the outcome variables. This assumption is necessary for valid inference. See Edwards et al., Looking Down the Barrel of a Loaded Gun: The Effect of Mandatory Handgun Purchase Delays on Homicide and Suicide, (Univ. of Ala. Legal Studies Research Paper No. 2629397, 2017), https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2629397. In the context of our study, we compare what happened to perceived gay couples in treated jurisdictions before the law changed and find that they looked very similar to the control states.
Prior to a discussion of these results, it is important to sound a caveat. These results are derived from changes in only a handful of cities and states and, as a result, the precision of some of these estimated effects may suffer. This is evident in some of the larger confidence intervals portrayed by the vertical bands in Figure 13 below.

![Figure 13: The Effect of State- and Local-Level Laws Prohibiting Sexual Orientation Discrimination on the Probability of Loan Acceptance](image)

Each difference-in-difference estimator of the effect of the passage of a state anti-sexual orientation discrimination law on each race/gender group is represented by the dot. The bands represent confidence intervals. For more information on how to interpret this graph, see the previous figures.
With that in mind, the results presented here are quite interesting. To this point, regardless of how we slice the data—by region, politics, bank size, or intergroup proximity—we find that four groups of same-sex loan co-applicants—black male/black male pairs, black male/white male pairs, white male/black male pairs and white male/white male pairs—are consistent. These groups are approved for the same loans with the same income at the same banks in the same counties at lower rates. With the introduction of local anti-discrimination laws, a different picture emerges. Of the four disadvantaged groups, three are statistically indistinguishable from zero, meaning there is no evidence of discrimination in lending. The fourth group—a pair consisting of two black males—is still statistically significant and negative, meaning that group is still less likely to have a loan approved, but the magnitude is smaller compared to the main results. In Table 7, a pair of two black males is 7.5 percentage points less likely to have a loan approved, all else equal. With the passage of a state/locality anti-discrimination law, that rate drops in half to 3.1. The results suggest that state- and locality-level attempts to discourage discrimination may be fruitful.212

We end with a grim note. The analysis of the two potential mechanisms to discourage discrimination based on sexual orientation leads, at best, to mixed results. If population density is a good proxy for intergroup contact, we find no evidence that contact in more urbanized areas reduces the bias towards gay male couples. However, local attempts to enact laws designed to protect against sexual orientation discrimination may hold promise in reducing discrimination.

IV. CONCLUSION

Discrimination in home mortgage lending has, unfortunately, a long history in the United States. Recent efforts to stave off discrimination in mortgage lending have included the creation of mortgage application databases to which lenders are required to report. Drawing on this data, the Article presents the first evidence of systematic, nationwide bias against perceived gay male applicants that transcends every geographical and political boundary in the U.S. The data further suggests that prejudice, rather than statistical discrimination, is the driving force. The law has much to do with the current situation. With few exceptions, federal law and the majority of states do not prohibit lenders from discriminating against applicants based on their sexual orientation (although some localities do). In these jurisdictions, sexual orientation discrimination is not only legal, it is a defense that may allow a discriminatory lender to exculpate itself.

This study has important implications beyond the housing and mortgage lending markets. Sexual orientation discrimination has also been a burning topic in Title VII (employment) and Title IX (education) cases. In 2015, the same year in which the Supreme Court decided that the states are required to license and

212 As explained earlier, however, the results should be taken with caution given that they are derived from changes in laws in only a few states and localities.
recognize same sex marriages, the EEOC held that Title VII’s prohibition against sex discrimination includes sexual orientation. The controversial decision remains an outlier. For over half a century, the U.S. Courts of Appeals resisted extending the prohibition against sex discrimination to sexual orientation. Although the Supreme Court has never spoken on the question, things may soon change. A few months ago, the Seventh Circuit, overruling previous precedents, held that sexual orientation discrimination is a form of sex discrimination and, therefore, is prohibited under Title VII. The decision sent shockwaves throughout the legal community. And although the Seventh Circuit’s ruling was limited to Title VII purposes, it marks the beginning of a more dramatic change that may spread across jurisdictions and Titles.

This Article can inform the ongoing debate in Title VII and other laws in pari materia. Indeed, with one exception, all U.S. Circuit Courts now adopt and

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215 See, e.g., Brief for the U.S. as Amicus Curiae Supporting Appellees, Zarda v. Altitude Express, 855 F.3d 76 (2d Cir. 2017) (No. 15-3775), 2017 WL 3277292, at *7 (“[U]ntil the Seventh Circuit’s en banc decision in Hively earlier this year, the ten other Courts of Appeals to have addressed the issue had uniformly joined this Court in holding that Title VII’s prohibition on sex discrimination does not encompass sexual orientation discrimination.”).
216 In Hively v. Ivy Tech Cmty. Coll. of Ind., 853 F.3d 339, 341 (7th Cir. Apr. 4, 2017) the Seventh Circuit became the first court to hold that “discrimination on the basis of sexual orientation is a form of sex discrimination” (explaining “the common-sense reality that it is actually impossible to discriminate on the basis of sexual orientation without discriminating on the basis of sex.”). Id. at 351. Interestingly, the Seventh Circuit is the one circuit that has explicitly rejected the Title VII framework in FH-Act and/or ECOA cases. See Latimore v. Citibank Fed. Sav. Bank, 151 F.3d 712, 715 (7th Cir. 1998) (rejecting McDonnell Douglas burden shifting in favor of direct or circumstantial evidence).
217 Recent developments in the Second Circuit are illustrative. On April 18, 2017, the Second Circuit upheld its precedent that sexual orientation discrimination is not prohibited by Title VII. Zarda v. Altitude Express, 855 F.3d 76 (2d Cir. 2017). The decision did not last long. On May 3, 2017, two weeks after the decision in Zarda, the District Court for the Southern District of New York defied the Second Circuit’s precedent. See Philpott v. New York, 16 CIV. 6778 (S.D.N.Y. May 3, 2017). Relying on the Seventh Circuit decision in Hively, it held that Title VII’s prohibition against sex discrimination also prohibits sexual orientation discrimination. Id. at 2. On May 25, 2017, the Second Circuit granted Zarda’s request for rehearing en banc to consider whether Title VII prohibits discrimination on the basis of sexual orientation through its prohibition of discrimination “because of . . . sex.” On July 26, 2017, the Department of Justice (DOJ) filed an amicus curiae brief on behalf of the United States arguing that “the en banc Court should reaffirm its settled precedent holding, consistent with the longstanding position of the Department of Justice, that Title VII does not reach discrimination based on sexual orientation.” Brief for the U.S. as Amicus Curiae Supporting Appellees, Zarda v. Altitude Express, 855 F.3d 76 (2d Cir. 2017) (No. 15-3775), 2017 WL 3277292. at *1. On February 26, 2018 the Second Circuit, rejected the DOJ’s argument and reversed its decision. Deciding en banc, it held that sexual orientation discrimination is a “subset of sex discrimination,” thereby overruling Simonton and Dawson. Zarda v. Altitude Express, Inc., No. 15-3775, 2018 WL 1040820, at *1, *5 (2d Cir. Feb., 26, 2018).
apply Title VII’s jurisprudence in mortgage lending.\textsuperscript{218} The same is true for the agencies in charge of enforcing the ECOA and FH-Act.\textsuperscript{219}

We end with an open invitation. Sexual orientation discrimination research is in its very early stages, with the first major study conducted overseas as late as 2009. We hope that our study will add to an important discussion that, to date, involves many theories but little empirical evidence. In that sense, a major contribution of our study is in setting the grounds for a new form of econometric studies in the area.

\textsuperscript{218} See supra note 216.
\textsuperscript{219} See supra Parts II.A.2 and II.A.2.