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Does Marriage Equality Promote Credit Access? Evidence from Same-sex Marriage Laws

By *Jens Hagendorff, Duc Duy Nguyen, Vathunyoo Sila*

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JEL classifications: G21, J12, J15, K36
Key words: Banks, mortgages, same-sex marriage

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1. Introduction

Understanding the factors affecting the allocation of credit to and by economic agents is important to scholars in the corporate finance and household finance fields. A growing literature analyzes access to housing finance and reports that applicants from minority groups face hurdles to accessing mortgage credit (see, e.g., Ambrose et al., 2021; Bartlett et al., 2022; Berkovec et al., 1998; Buchak and Jorring, 2021; Munnell et al., 1996). This includes same-sex applicants who, after controlling for borrower and loan characteristics, are more likely to be denied mortgage loans than different-sex applicants (Sun and Gao, 2019). Motivated by studies that show that the legalization of same-sex marriage (SSM) is accompanied by improved public attitudes toward sexual minorities (Aksoy et al., 2020; Bishin et al., 2016; Ofosu et al., 2019), our paper investigates whether the legalization of SSM affects access to credit for same-sex borrowers.

Using detailed mortgage application data from 2004 to 2017, we classify applications where the main applicant and the co-applicant entered the same sex on the application form as same-sex applications.[1] To identify whether SSM legalization affects the likelihood that same-sex applications are denied, our estimations exploit the staggered state-level introduction of SSM legislation. This setup allows us to contrast the denial rates of same-sex applicants post-SSM legalization against those for a matched sample of applications by joint applicants.

Specifically, we match same-sex applicants post-SSM legislation to applications in three nontreatment groups: (i) same-sex applicants before-SSM legislation, (ii) different-sex applicants before-SSM legalization, and (iii) different-sex applicants post-SSM legalization. To remove as many composition differences in application pools as possible based on HMDA data, we perform our matching based on an

[1] This approach follows Sun and Gao (2019) who demonstrate its accuracy in tracking estimates of the number of same-sex households in Gallup and Census Bureau surveys over several decades. A key advantage of this approach is its comprehensive coverage, because coverage based on surveys is confined to the geographic areas and time periods covered by specific surveys. Additionally, while our indicator is estimated with error, surveys also suffer from measurement errors when respondents are reluctant to engage with questions about their sexual orientation. To reduce concerns that we misclassify family members as same-sex borrowers, we demonstrate that our results hold when we restrict the sample to applications in which the applicant and co-applicant are of different races.

exhaustive list of observable applicant characteristics, including the applicants' sex, race, ethnicity, income, the loan amount, loan type, and the loan purpose. Critically, matching same-sex applicants post-SSM to those before-SSM allows us to control for potential selection bias. For instance, same-sex couples could be more likely to co-apply for a mortgage after SSM legalization, causing changes in the quantity and characteristics of these applicants around SSM legalization.

We also saturate the models with bank*county*year fixed effects to absorb time-varying unobserved trends, e.g., local changes in underwriting standards and demographic, social, or economic factors (including anti-discrimination laws, adoption laws, and attitudes towards sexual minorities). Conceptually, our analysis compares mortgage outcomes before and after SSM legislation in the same bank, county, and year between two otherwise identical pairs of joint applicants—one that is different-sex and one that is same-sex.

We find that following the recognition of SSM, the "denial gap" between same-sex and different-sex applicants *increases*. Before SSM legalization, same-sex applicants are 4.05% more likely than different-sex applicants to be denied credit than different-sex applicants. After SSM legalization, the gap increases by 88 basis points. Given that the average denial rate in our sample is 17.38%, the increase in the denial gap between same-sex and different-sex applicants corresponds to a substantial marginal effect of 5.06%.

When we perform separate regressions for different-sex and same-sex applications, we find that the denial rate for applications by different-sex applicants remains unaffected by SSM laws. This implies that our findings are indeed due to an increase in the denial rate among same-sex applicants. Our results are also robust to controlling for pre-SSM state-level attitudes toward sexual minorities, captured by previous bans on SSM or antidiscrimination laws that include sexual orientation.[2]

[2] In unreported analyses, we also estimate a Cox hazard model and rule out significant relationships between the passage of SSM laws and macroeconomic factors (e.g., GDP per capita and GDP growth at both national and state levels) or the state-level proportion of votes cast for Republican candidates in presidential elections.

To further address the concern that our results capture unobserved credit quality differences between same-sex and different-sex applications, we conduct several additional tests. First, we show that the effect of SSM legalization on the denial rate is observed across different segments of applicant quality (based on observables in our data such as income, the loan-to-income ratio, and loan amount), suggesting that the increasing denial gap is observable even among better quality same-sex applicants. This alleviates the concern that the increased denial gap is due to same-sex applicants being less creditworthy post SSM in ways that are not observable in our data. Second, we show that the results hold when we interact all control variables with SSM laws and, in another specification, with bank*county*year fixed effects. These specifications allow each bank to have different underwriting models for different counties in different years.

In more detailed analyses, we explore which mechanisms contribute to the increased denial gap between same-sex and different-sex applicants. According to the first mechanism, opinion backlash, SSM legalization causes greater disapproval of same-sex relationships (Flores and Barclay, 2016; Ofosu et al., 2019). We present two findings that do *not* support this mechanism. First, we show that the increase in the denial gap post-SSM is not affected by how socially conservative the county or state is. Second, we analyze whether loan officers deny applications made by same-sex borrowers for noneconomic reasons by imposing stricter standards on them. If so, the loans made to same-sex borrowers would be of higher quality than the loans made to different-sex applicants. However, we find no relationship between SSM legalization and the quality of the loans originated to same-sex borrowers (measured by applicants' FICO scores, loan-to-value ratios, and mortgage defaults). Further, we confirm that these non-results on quality hold across different segments of applicant credit risk, including applicants who could be considered marginal, such as those with a high loan-to-value ratio.

Our findings are consistent with a second mechanism: increased information frictions between loan officers and same-sex borrowers after SSM legalization. Although decisions on mortgage originations rely primarily on credit scores and other "hard" information (Stein, 2002), many lenders also process "soft" information on applicants such as cues on the perceived strength of the couple's relationship, their emotional attachment to the property, spending habits, employment stability, and the quality of the documentation submitted (Arentsen et al., 2015; Keys et al., 2010). Consistent with our intuition, anecdotal evidence indicates that one lender declined a mortgage to a same-sex borrower because “gay marriage had just been legalized and the bank wasn't sure how it was going to work out.”[3]

We argue that if loan officers are less familiar with applications from same-sex applicants, they will find it more challenging to process soft information on this group of applications. If soft information is less likely to move the needle toward positive lending decisions for same-sex applicants, mortgage decisions for this group will more likely be based on hard information.[4]

Our data offer broad support for this conjecture. First, loan decisions that involve same-sex borrowers become more standardized after SSM legalization, and contain less soft information. Second, consistent with loan officers *temporarily* struggling to collect and process soft information on same-sex borrowers, we find that when loan officers have more exposure to applications from same-sex borrowers (e.g., when banks receive more applications from same-sex applicants), the dispersion in lending outcomes between same-sex and different-sex applicants is reduced. Finally, FinTech lenders, which rely less on human loan officers (Buchak et al., 2018) for loan decisions, report no difference in denial rates or the use of soft information for same-sex mortgage applications following SSM.

Our paper contributes to several active research areas. First, our analysis is related to a growing literature that studies differential lending practices to different groups. While this literature mainly focuses on differences in lending practices along the lines of race, ethnicity, and gender (e.g., Ambrose et al., 2021; De Andrés et al., 2021; Bayer et al., 2018; Bhutta and Hizmo, 2021; Calomiris et al., 1994; Munnell et al., 1996; Ross and Yinger, 2002), Sun and Gao (2019) show that same-sex mortgage applicants also experience lower approval rates. We contribute to this body of literature by exploring the reasons for the same-sex

[3] See, e.g., Meet the man hoping to battle LGBTQ bank discrimination with a new credit union, *ABC News* (October 9, 2019), https://abcnews.go.com/US/meet-man-hoping-battle-lgbtq-bank-discrimination-credit/story?id=65947300.

[4] Our arguments around the role of soft versus hard information in mortgage originations to minority applicants follow those presented in Calomiris et al. (1994). The authors develop a model around how racial differences between loan officers and applicants cause a cultural disconnect that leads lenders to place more emphasis on hard information and less emphasis on soft information to avoid the costly collection of the latter.

denial gap. We use the extension of marriage rights to same-sex couples as a shock to demand for housing finance, and find that information processing frictions play a role in explaining the denial gap.

Second, we contribute to the literature that examines whether SSM legislation shapes attitudes toward sexual minorities. The extant literature has produced mixed evidence on whether "laws change minds" and improve public attitudes or, conversely, generate an opinion backlash (Aksoy et al., 2020; Bishin et al., 2016; Flores and Barclay, 2016; Ofosu et al., 2019). However, questions remain over how accurate polling evidence is if individuals are reluctant to respond honestly about discriminatory views (Coffman et al., 2017). While our paper also addresses whether laws change minds, it does so by drawing on the observed behavior of agents in a financial market. Our findings question the extent to which the changes in attitudes reported in the literature translate into changes in the economic treatment of same-sex couples. Along this line, we broadly contribute to the literature the examines the impact of SSM legalization on various economic outcomes for same-sex couples (e.g., Carpenter, 2004; Jepsen and Jepsen, 2015; Sansone, 2019; Tilcsik, 2011).

Finally, our analysis is related to the literature on information production and monitoring between borrowers and lenders (for a review, see Liberti and Petersen, 2019). Residential mortgage originations are predominantly based on hard borrower information. The view that mortgages are hard information products underpins massive securitization markets (Stein, 2002) as well as the rise of FinTech lenders that rely on hard information to make instant underwriting decisions (e.g., Berg et al., 2020; Stulz, 2019, for consumer lending). To date, evidence on the soft information content in mortgages is restricted to the mortgages of low-income borrowers (Arentsen et al., 2015; Ergungor, 2010; Keys et al., 2010). Our results contribute to this work by suggesting that mortgages issued by banks (but not by Fintech lenders) contain a non-negligible soft information element. When banks deal with a group of applicants whose soft information they are less familiar with, they reject more applications from that group.[5] This implies that routine mortgage lending by banks is less automated than some of the literature assumes.

2. The road to marriage equality for same-sex couples

The road to marriage equality for same-sex couples in the US has been bumpy. The interplay between state and federal courts and state legislatures has been crucial for the eventual legalization of SSM. Owing to this institutional interplay, the introduction of SSM was piecemeal and staggered across time and states. This aids our identification of the effects of SSM laws on loan origination to same-sex borrowers.

After initial campaigns for marriage equality were met with little success,[6] Massachusetts became the first state to legalize SSM. In 2003, the Massachusetts Supreme Judicial Court in *Goodridge v. Department of Public Health* required Massachusetts to recognize SSM from May 17, 2004. This was followed by the legalization of SSM in 13 other states, although the timing and methods of implementation differed between them. SSM was legalized through state court decisions (in California,[7] Connecticut, and Iowa), state legislative changes (in Delaware, District of Columbia, Minnesota, New Hampshire, New York, Rhode Island, and Vermont), or public state referendums on the issue (in Maine, Maryland, and Washington).

In its landmark ruling in *United States v. Windsor* on June 26, 2013, the Supreme Court declared Section 3 of the Defense of Marriage Act (DOMA) unconstitutional. DOMA's definition of marriage as "a

[5] Similarly, evidence from the corporate lending literature suggests that lenders focus on known corporate borrowers, with whom they have existing relationships, over new unknown borrowers. Dell'Ariccia et al. (2021) show that banks shy away from informationally difficult corporate borrowers (as measured by their use of intangible assets). Similarly, De Jonghe et al. (2020) show that, following funding shocks, banks allocate more funding to sectors and borrowers with which they are familiar.

[6] In 1972, the Supreme Court dismissed the appeal of a ruling by the Minnesota Supreme Court (*Baker v. Nelson*) that a state statute limiting marriage to heterosexual couples "did not offend" the US Constitution. Following this, various states introduced explicit bans on SSM (e.g., Maryland in 1973, California and Florida in 1977, and New Hampshire in 1987). This culminated in the 1996 Defense of Marriage Act, which prohibited recognition of SSM at a federal level and allowed states to refuse recognition of SSM granted in other states.

[7] California changed its legal treatment of SSM several times. The state first issued marriage licenses to same-sex couples in June 2008 after the Supreme Court of California ruled that barring same-sex couples from marriage violated the state's constitution. However, the issuance of such licenses was halted between November 2008 and June 2013 due to a state constitutional amendment barring SSM.

legal union between one man and one woman" had effectively prohibited the recognition of SSM at the federal level. Consequently, the *United States v. Windsor* decision forced the federal government to recognize the validity of SSM licenses issued by some states. The decision also ushered in a new wave of SSM legalization in 22 states through either court decisions or changes to state legislation.

In a final key step, in *Obergefell v. Hodges*, the Supreme Court ruled on June 26, 2015 that SSM is a constitutional right. This introduced SSM to the remaining 15 states that had not yet legalized SSM at that point.[8] Therefore, *Obergefell v. Hodges* introduced nationwide access to marriage for same-sex couples, regardless of their state of residence.

Table A1 summarizes the dates and methods by which SSM was legalized in each state. In many states, SSM legalization hinged on court decisions, which are less likely to be influenced by public opinion than other adoption methods (Sansone, 2019; Trandafir, 2015). Further, many of the key rulings were narrow (e.g., the Supreme Court ruled 5–4 in *Obergefell v. Hodges*) and thus difficult to predict. This allows us to argue that the changes to SSM laws and their timing are plausibly exogenous to macroeconomic or state-level characteristics that could influence credit decisions.

3. Data and methodology

3.1. Loan origination data

Our primary source of data is the Home Mortgage Disclosure Act (HMDA) loan application registry. This repeated cross-sectional loan-level dataset is based on the mandatory reporting of all mortgage applications by qualified financial institutions. Institutions are required to report HMDA data to regulators if they have at least one branch office in a metropolitan statistical area and meet a minimum size threshold.[9]

[8] The states that had not legalized SSM prior to *Obergefell v. Hodges* were Alabama, Arkansas, Georgia, Kansas, Kentucky, Louisiana, Michigan, Mississippi, Missouri, Nebraska, North Dakota, Ohio, South Dakota, Tennessee, and Texas.

[9] Because the reporting threshold is relatively low, HMDA data cover most lenders and nearly 90% of mortgage applications in the United States (Cortés et al., 2016). In 2010, the median year in our sample, the reporting threshold is $39 million in book assets. HMDA's reporting criteria can be found at https://www.ffiec.gov/hmda/reporterhistory.htm.

Our sample includes mortgage applications from 2004 to 2017.[10] Following Cortés et al. (2016), we match the HMDA loan data with the Summary of Deposits data (maintained by the FDIC) that include the location of all bank branches. To infer the location of the loan officers who process the mortgage applications, our primary sample comprises loan applications that are filed with banks that have at least one branch in the same county as the mortgage property. For each loan application, we obtain borrower demographics (e.g., income, sex, and race), loan characteristics (e.g., the amount of loan applied for and its purpose), property type and location, the decision on the application (e.g., approved, denied, or withdrawn), the year in which the application decision is made, and a lender identifier.

We apply the following screening procedure. First, we exclude applications that were closed for incompleteness, withdrawn by the applicant before a decision was made, or contained missing loan or demographic information. Second, because identifying same-sex applicants requires having gender information for the main applicant and the co-applicant, we drop observations without a co-applicant.[11] Third, we drop mortgage applications from the sample if the mortgage property is non-owner occupied. Most non-owner-occupied mortgages finance investments in property (Robinson, 2012), and borrower information will be less important for the lending decisions for these applications than information on property characteristics such as the expected rental income. Table A2 summarizes our sample screening procedure and reports the corresponding observations that are dropped because of each screening step.

3.2. Identifying applications from same-sex borrowers

Because the sexual orientation of mortgage applicants is not reported in the HMDA dataset, we cannot directly identify same-sex couples in our sample. To identify potential same-sex couples, we follow

[10] Our sample starts in 2004 to ensure consistency in how the information on applicants is recorded over the sample period. In 2004, the HMDA recategorized how race and ethnicity are recorded and changed the definitions of mortgages that are classified as refinancing and home improvement loans. Additionally, the HMDA significantly increased the coverage by revising the reporting threshold to include all institutions with at least $25 million in mortgage loans. See https://www.minneapolisfed.org/article/2003/hmda-changes-are-on-the-way-new-rules-take-effect-in-2004.

[11] Observations without a co-applicant may include applications by both homosexual and heterosexual borrowers. Therefore, the inclusion of these applications makes the sexual orientation of borrowers undistinguishable to us and could bias our estimate of same-sex applications in the population of borrowers.

Sun and Gao (2019) and classify applications in our sample as made by a same-sex couple if the main applicant and co-applicant entered the same sex in the application form.

[Figure 1 around here]

Figure 1 demonstrates the validity of our measure of same-sex couples. The figure plots the proportion of same-sex applicants from the HMDA dataset with the proportion of same-sex households reported in the Census Bureau's American Community Survey (ACS). Because the ACS is based on household members reporting their relationship status, it provides a benchmark for our measure.[12] The figure shows a high correlation between the share of same-sex mortgage applicants and the share of same-sex households (the correlation coefficient is just under 70%).[13]

At a broader level, it is important to note that if our approach were to misclassify some same-sex applications, this would cause us to *underestimate* the true effect of SSM laws. That is because, after the passage of SSM legislation, the denial rates among misclassified same-sex applications should not change relative to those of different-sex applications. Any measurement error that arises from misclassifying applicants is likely to be uncorrelated with the regression error term and result in an attenuation bias of the coefficients toward zero.

Finally, it is also comforting to note that our results continue to hold when we restrict the sample to applications in which the applicant and co-applicant are of different races and/or ethnicities (in Section

[12] We note that the proportion of same-sex households in the ACS dataset (0.98%) is lower than the proportion of same-sex mortgage applications from HMDA (3.90%). There are three potential reasons that could explain this discrepancy. First, the ACS data only capture the proportion of same-sex couples who live in the same households at the time of the survey, whereas the proportion of same-sex applications in HMDA also captures same-sex couples that may not be cohabitating at the point when a mortgage application is made. Second, the ACS sampling methods target individuals and households such that the resulting sample resembles the overall US population. In contrast, the sample of individuals in HMDA represents the average individuals in the market for mortgages (who tend to be younger and wealthier and potentially more conformable identifying as same-sex couples (Newport, 2018)). Finally, accurately tracking the number of individuals that identify as LGBT remains challenging (Badgett et al., 2021; Gates, 2011; Laumann et al., 2001). The discrepancy across the two data sources may partially arise from classification errors in these sources.

[13] The correlation is conservative because we exclude the District of Columbia from the estimation. Given the District of Columbia has a high representation of same-sex households in both datasets, the correlation coefficient becomes very high (89%) when we include it. Internet Appendix IA1 further shows that we obtain similarly high correlations when comparing our measure of same-sex couples with the proportion of same-sex households and LGBT population using the estimates from Gallup Daily Tracking Survey in collaboration with UCLA Williams Institute (73% and 64% respectively, when the District of Columbia is excluded).

4.2). The latter rules out that our main results are due to applications from family members (e.g., siblings who live together). We also find that the proportion of same-sex applications in our full HMDA sample (3.9%) is very similar to the proportion of same-sex applications in a subset of HMDA sample where the applicant and co-applicant report different races (4.45%). This suggests that we are unlikely to overidentify same-sex couples in HMDA.

3.3. Matching approach

We test whether the mortgage denial rates for same-sex applicants are affected by SSM legalization. The key challenge when making inferences related to SSM is that we cannot observe whether the outcome of a particular mortgage application after SSM legalization would have been different if it had been submitted before the legalization of SSM. This could lead to a selection concern. Prior evidence suggests that SSM legalization led to increased demand for mortgages by same-sex borrowers (Miller and Park, 2018). This increase in demand could be accompanied by changes in the average quality of same-sex applications causing our results to be confounded by compositional changes in our sample after SSM legalization.[14]

To address this, we use matched-sample regressions in all our analyses. Mortgage applications by same-sex applicants are the treatment group. The nontreatment group includes all other mortgage applications by joint applicants (same-sex applications before SSM plus different-sex applications before and after SSM). We employ a matching approach where the matched applications in the control group minimize the distance between a vector of observed covariates across treated and nontreated applications. The matching approach places more emphasis on matched applications that are more similar to treated

[14] Miller and Park (2018) document an increase in same-sex mortgage applications after SSM recognition. Internet Appendix IA2 presents a similar analysis and shows that SSM recognition is associated with a 1.15% increase in the share of same-sex applications (from 3.9% before SSM was recognized to 5.05%). Internet Appendix IA2 also extends Miller and Park's analysis and shows that some characteristics of same-sex applicants (such as income, loan amount, and demographics) changed following the recognition of SSM.

observations when calculating the matching weights, thereby reducing estimation bias (see Cochran and Rubin, 1973; Jann, 2017).[15]

The matching covariates are the main applicant's income, sex, race, and ethnicity as well as the loan amount, loan type (whether the loan is a conventional loan or whether it is insured by the Federal Housing Administration, Veterans Administration, or Farm Service Agency or Rural Housing Service), and loan purpose (for home purchase, refinancing, or home improvement). We impose exact matches on categorical variables.[16] Thus, our matched applications are from applicants of the same sex, race, and ethnicity with similar incomes who apply for loans of similar amounts of the same loan type and for the same purpose. Furthermore, to ensure common support, our final matched sample excludes treated observations that lack matched observations in any of the three control groups (Atanasov and Black, 2016, 2019).[17]

[Table 1 around here]

Table 1 displays the summary statistics of our covariate-balanced sample of 14,632,079 credit applications. On average, 17.38% of the mortgage applications in our sample are denied. The average

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[15] Specifically, we use Mahalanobis distance kernel matching, which standardizes the distance between two observations using the sample variance–covariance matrix. Therefore, the distance between two observations along a covariate takes into account the standard deviation of that covariate. We find similar results when we perform a three-way matching using propensity scores calculated from the same set of covariates (see Column (1) of Internet Appendix IA3) or when we additionally stipulate that treatment and control applications have been submitted to the same bank.

[16] In Column (2) of Internet Appendix IA3, we impose the matching criteria more aggressively by matching treatment applications to control applications made to the same bank. Comparing loans made to the same bank by same- or different-sex applicants before/after SSM laws further alleviates the concern that the results are driven by the compositional change in risk profile of the applicants at the bank level. We find that our main findings remain the same.

[17] Another concern is that post-SSM, same-sex borrowers with lower credit quality self-select into joint applications for a mortgage (rather than submit a solo application). Because we are unable to identify the sexuality of solo applicants, we cannot rule out this concern directly. Fortunately, self-selection among same-sex borrowers is unlikely to be a major concern. Because credit quality also influences the decision of different-sex borrowers to apply for a joint mortgage, self-selection among same-sex vis-à-vis different-sex borrowers only becomes a threat to our interpretation if it is so strong that only the lowest-quality same-sex borrowers apply for a joint mortgage. To further alleviate this concern, we perform several additional tests (we do not tabulate the results for brevity). First, we show that the passage of SSM laws does not affect the composition of solo versus joint applications. Therefore, while self-selection among same-sex borrowers could exist, it does not *change* around the passage of SSM laws and is thus unlikely to explain our main findings. Second, we show that our results continue to hold when we use subsamples in which this self-selection concern is minimized. Specifically, our results are robust to us restricting the sample to (i) states in which the proportion of same-sex joint applications *decreases* (relative to the total number of joint applications) after the passage of SSM laws, and (ii) loans where the applicants' income is *above* the sample's median.

applicant earns a gross income of \$112,870 and applies for a loan of \$203,320. Internet Appendix IA4 presents the breakdown and the summary statistics of both the initial sample and the final matched sample. Our initial HMDA sample (Panel A) comprises 16,088,706 mortgage applications from the HMDA dataset for the period from 2004 to 2017. Panel B presents the summary statistics after a three-way covariate balancing using Mahalanobis distance kernel matching. Columns (3)–(10) in Panel B present the mean of the same variables when mortgage applications are divided into four groups: same-sex applications after the passage of state-level SSM laws; same-sex applications before the passage of state-level SSM laws; different-sex applications after the passage of state-level SSM laws; and different-sex applications before the passage of state-level SSM laws.

3.4. Empirical model

Credit origination decisions are shaped by demand- and supply-side factors. To account for these, we employ a difference-in-differences model for all mortgage applications with a co-applicant (same- and different-sex co-applicants) in a matched sample approach as described in Section 3.3. Our model contrasts the loan denial rates of same-sex applicants post-SSM legislation against a matched sample of applications by joint applicants (same-sex applications pre-SSM plus different-sex applications pre- and post-SSM) as follows:

$$y_{ibsct} = \alpha \times \mathit{Same\text{-}Sex}_{it} + \beta \times \mathit{Same\text{-}Sex}_{it} \times \mathit{SSM}_{st} + \mathbf{c}'_{it}\boldsymbol{\gamma} + \boldsymbol{\delta}_{bct} + \varepsilon_{ibsct}, \qquad (1)$$

where y_{ibsct} is the outcome of loan application *i* submitted to bank *b* in county *c* of state *s* in year *t*. For most of our analysis, y_{ibsct} equals 1 if the application is denied, and 0 if it is approved. $\mathit{Same\text{-}Sex}_{it}$ equals 1 when the main applicant and co-applicant are of the same gender, and 0 otherwise (see Section 3.2). SSM_{st} equals 1 if the mortgage application is submitted in a state during or after the year in which SSM is legalized in that state, and 0 in other years. Finally, $\mathbf{c}$ and $\boldsymbol{\delta}$ represent loan characteristics and bank*county*year fixed effects, respectively.

The main coefficient of interest in (1) is β, a difference-in-differences coefficient. The first difference is between applications submitted in states that recognize SSM and applications submitted in states that do not recognize SSM when the application is made. The second difference is between same-sex and different-sex applicants. Our key identifying assumption is that in the absence of SSM legalization, the difference in the denial rates between same-sex and matched different-sex applicants will not change.

The rich dataset allows us to saturate the models with bank*county*year fixed effects and absorb any *time-varying* unobserved trends, both at the state- and the bank-level. These unobserved trends could include, at the state-level, changes in local demographics, social or economic factors, as well as any changes in anti-discrimination laws, same-sex couple adoption laws, and other protections of sexual minorities. At the bank level, the fixed effects control for changes in underwriting standards. Conceptually, the interacted fixed effects allow us to identify the outcomes of applications from same-sex and different-sex borrowers before and after SSM legislation in the same bank–county–year. Therefore, a positive (negative) coefficient on *Same-Sex * SSM* would indicate that after SSM legalization, same-sex borrowers face a higher (lower) mortgage denial rate than different-sex borrowers who apply for mortgages at the same bank, in the same county, and in the same year.[18]

Finally, we follow Sun and Gao (2019) and control for several observable loan-level underwriting variables, including *Ln Applicant Income*, *Loan/Income*, and indicator variables for whether the applicant is *Male*, *Hispanic*, *African American*, *Asian*, or *Other Races*; whether the purpose of the loan is *For Refinance* or *For Home Improvement*; and whether the loan is Federal Housing Administration-insured (*FHA Loan*), Veterans Administration-guaranteed (*VA Loan*), or Farm Service Agency or Rural Housing Service-insured (*FSA/RHS Loan*). All variables are defined in Table A3 in the Appendix.

[18] For robustness, our subsequent analyses in Section 4.2 also allow banks to place different weights on different loan and borrower characteristics by interacting bank*county*year fixed effects with various loan and borrower characteristics, such as applicant income and loan type.

4. Main analysis

4.1. Baseline results

Table 2 presents our baseline loan-level regressions that examine the effect of SSM legalization on the likelihood that mortgage applications by same-sex borrowers are denied. The dependent variable is *Denied*, which equals 1 if a loan is denied, and 0 otherwise. All regressions include bank*county*year fixed effects.

[Table 2 & Figure 2 around here]

We find that after SSM legalization, same-sex borrowers face a higher likelihood of mortgage denial compared with different-sex borrowers. The coefficients on *Same-Sex*SSM* are positive and statistically significant below the 1% level across all columns. The magnitude of the coefficient estimates on *Same-Sex*SSM* is stable as we progressively include more control variables in the model.

Further, the results are economically meaningful. Before SSM legalization, same-sex applicants are 4.05% more likely to be denied credit. After SSM legalization, this denial gap increases by 88 basis points to 4.93% (= 0.0405 + 0.0088). The increase in the denial gap corresponds to a substantial marginal effect of 5.06% from the average denial rate in our sample of 17.38% (= 0.0088 / 0.1738). Figure 2 displays the denial gap between same-sex and different-sex applicants for different years around the passage of SSM legalization.[19]

Because our models include bank*county*year fixed effects, the estimated effects of *Same-Sex*SSM* enable comparison of the change in denial rates between same-sex and different-sex applicants who apply for mortgages at the same bank, in the same county, and in the same year. Finally, the coefficients on the control variables have the expected signs. For instance, lower income, female, non-white, and Hispanic applicants are more likely to have their mortgage applications denied. The previous literature

[19] We exclude California from this analysis because of the reversal of SSM laws in California. We also exclude and Massachusetts because Massachusetts legalized SSM before the start of the sample period.

argues that these applicants tend to have a less detailed credit history and thus face a higher likelihood of loan denial (Ergungor, 2010).

4.2. *Robustness of the baseline results*

Are the results due to different-sex applications? Our results could be driven by different-sex applicants experiencing a lower denial rate after SSM legalization, rather than by same-sex applicants facing an increased denial rate. While the previous literature reports no evidence that SSM laws impact the behavior of different-sex households (e.g., Sansone, 2019; Trandafir, 2015), we address this concern by running separate regressions for same-sex and different-sex borrowers.[20] The results in Panel A of Table 3 indicate that while same-sex borrowers face a statistically significant 1% higher mortgage denial rate post-SSM (at the 5% level, Column (1)), the effect is statistically insignificant and indistinguishable from zero for different-sex borrowers (Column (2)). This implies that our main results can be attributed to an increase in mortgage denials for same-sex borrowers and not to a reduction in denials for different-sex borrowers.

Alternative definition of SSM. In our main analyses, we define SSM as equal to 1 if the mortgage application is submitted in a state on or after the year in which SSM is legalized. Panel B of Table 3 shows that our results are robust to an alternative definition of SSM. Specifically, we define SSM as equal to 1 starting the year after SSM is legalized. We find that our results continue to hold, with the coefficient of 0.008 being very similar in magnitude to those in our baseline results.

Do we correctly identify same-sex borrowers? To alleviate concerns that we misidentify family members (e.g., fathers and sons, mothers and daughters, or siblings) as same-sex borrowers, Panel C of Table 3 re-estimates our main regressions using only applications where the main applicant and the co-applicant have entered different races or ethnicities on the application form. We find that our results still hold.

[20] We run one regression that includes only loans submitted by same-sex borrowers and one that includes only loans submitted by different-sex borrowers. The main coefficient of interest is *SSM*, which compares the mortgage denial rate faced by same-sex (and separately by different-sex borrowers) before and after SSM legalization. Because there is no variation within each bank–county–year in these regressions, we cannot include bank*county*year fixed effects. The regressions instead include bank, county, and year fixed effects.

Is there a difference between male and female same-sex borrowers? In Panel D of Table 3, we distinguish between the reported sex of same-sex applications by defining *Female Same-Sex* as =1 if both the applicant and the co-applicant are female, and *Male Same-Sex* as =1 if both the applicant and the co-applicant are male. We introduce both variables to our regression model and interact them with *SSM*. The results indicate that the denial gap (the coefficients on *Female Same-Sex* and *Male Same-Sex*) as well as the change in the denial gap after SSM (the coefficients on *Female Same-Sex*SSM* and *Male Same-Sex*SSM*) are positively significant for both female and male same-sex applicants. This suggests that both female and male same-sex couples are more likely to be denied credit and that the denial rates for both groups have increased after same-sex marriage legalization.

Stacked regressions with "clean" control states. Recent econometric studies show that a staggered treatment design, such as the one we use in this paper, could lead to a biased estimation of causal effects, particularly when treatment effects evolve over time (Baker et al., 2022; Goodman-Bacon, 2021). This bias arises when the treated observations are potentially compared with control observations that have recently been treated, causing the estimated effect to capture the treatment effect that is in the process of materializing in the control observations.

To alleviate this concern, Panel E of Table 3 uses a stacked regression design (see, e.g., Cengiz et al., 2019). Specifically, we separate observations in our baseline sample into different year cohorts based on the years in which SSM laws are passed. For each year cohort, we create a cohort-specific dataset that includes observations from the treated states (i.e., states in which SSM laws pass in that year) and observations from "clean" control states (i.e., states that do not legalize SSM within a 7-year window (t=–3,…,+3) around the passage of SSM laws in treatment states).[21] Using this more stringent criterion for admissible control groups reduces the possibility that our estimates are driven by recently treated observations. Finally, we stack the cohort-specific datasets to obtain the average effects of SSM legalization. Crucially, we stack the datasets by years relative to SSM legalization (rather than calendar

[21] Under these criteria, our treated states are restricted states that passed SSM laws during the period between 2008 and 2011, namely California, Connecticut, District of Columbia, Iowa, New Hampshire, and Vermont.

years) to prevent past treated units from being used as control units. Conceptually, this approach is equivalent to a setting where all events happen at the same time rather than being staggered over time.

Following Cengiz et al. (2019), we further saturate our regression model by interacting all fixed effects with dataset-specific indicator variables. Thus, bank*county*year fixed effects from the baseline model becomes cohort*bank*county*relative time fixed effects. Consistent with our baseline results, Panel E shows that the estimate on *Same-sex*SSM* is positive and significant.

Does our matching account for quality differences in the pool of same-sex applicants post-SSM legalization? Panel F of Table 3 partitions the sample (by the median) based on several proxies of application quality: applicant income (Columns (1)–(2)); loan/income ratio (Columns (3)–(4)); and loan amount (Columns (5)–(6)). We then re-estimate the baseline specification in Column (5) of Table 2 for each resulting subsample. As shown in Panel F, the coefficients on *Same-Sex*SSM* are positive and statistically significant across all subsamples of applicant quality, suggesting that the increasing denial gap is observable even among better quality same-sex applicants. This alleviates the concern that the increased denial gap is due to same-sex applicants being less creditworthy post SSM in ways that are not observable in our data.

Variations in underwriting models based on loan and borrower characteristics. A related concern is that the main coefficient (*Same-Sex*SSM*) captures the differences in how applicant groups match with lenders with different lending standards (see Ross and Yinger, 2002). To alleviate this concern, we present several analyses that allow for variation in underwriting models across banks, geographical areas, and times.

[Table 3 around here]

Specifically, in Panel G of Table 3, we progressively alter our baseline specification (Column (5) of Table 2) by interacting loan and borrower characteristics with various fixed effects. First, Column (1) adds the interactions between all control variables and *SSM*. This alleviates the concern that the estimate on *Same-Sex*SSM* picks up changes in banks' underwriting models before and after SSM legalization. Column (2) then includes the interactions between all control variables and bank*year fixed effects. This allows each bank to assign different weights to different borrower and loan characteristics in different years.

Column (3) replaces the interactions between the control variables and *SSM* with the interactions between the control variables and the county*year fixed effects. This allows for the possibility that lenders take into account time-varying economic and credit risks at the local level. Finally, Column (4) presents our tightest specification. We interact all control variables with bank*county*year fixed effects.[22] Consequently, we allow each bank to have different underwriting models for different counties in different years. Across all specifications in Panel G, the coefficients on *Same-Sex*SSM* remain positive, statistically significant, and of an economic magnitude that is similar to our baseline results.

Other robustness tests. Panel H shows that our results are not driven by any particular time periods, banks, or geographic areas. Columns (1) and (2) divide the sample into the time periods 2004–2012 and 2013–2017. The Supreme Court's *United States v. Windsor* decision in 2013 resulted in several states legalizing SSM in short succession, potentially hindering the measurement of the effect of SSM legalization. However, we do not find this to be the case. Instead, the coefficients on *Same-Sex*SSM* are positive and statistically significant in both time periods. Column (3) shows that our results are not driven by the inclusion of mortgage applications submitted during the 2008–2009 financial crisis.

We also show that our results are not driven by the largest lenders in the sample. Column (4) shows that our results continue to hold after excluding the largest ten lenders (based on the number of mortgage applications in our sample). Further, in Columns (5) and (6), we show that credit decisions are not driven by extreme house price movements (e.g., Brueckner et al., 2012). Using a county-level house price index from Zillow,[23] we exclude observations that are in the top and bottom 10% of house price appreciation in the county. Column (7) restricts the sample to observations in counties adjacent to state borders to alleviate the concern that our results capture unobserved differences in borrowers' creditworthiness across different states. We find that the coefficients on *Same-Sex*SSM* are similar to our baseline results.

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[22] For the results in Panel G of Table 3, we replace the continuous control variables (*Ln Applicant Income* and *Loan/Income*) with decile indicator variables. These further allow for the possibility that the relationships between these variables and the lending outcome are nonlinear.

[23] Zillow's housing data are available at: https://www.zillow.com/research/data/. We use the Zillow Home Value Index for Single-Family Homes as a time series at the county level.

In Internet Appendix IA5, we follow Bartlett et al. (2022) and add census tract-level proxies for three additional underwriting variables: the reported applicant's *FICO Score*, *Loan-to-Value* ratio, and *Debt-to-Income* ratio. Because these variables are not available at the loan-level from HMDA, we measure them at the census tract-level using originated loans from Black Knight's McDash database. This dataset covers approximately two-thirds of the US mortgage market and contains information on applicants' risk characteristics. We find that the coefficient on *Same-Sex*SSM* is very similar to our baseline results. This indicates that the underwriting variables included in our baseline model sufficiently account for the credit risk of our sample applicants. Finally, and to further alleviate concerns about pre-trends, Columns (1)–(3) of Internet Appendix IA6 use shorter time windows in the form of [–1, +1], [–2, +2], and [–3, +3] years, respectively. Our results are robust to our use of the shorter time windows around SSM legalization.

4.3. Different implementation modes of SSM

The implementation of SSM laws varies across states. Specifically, the introduction of SSM can be traced back to court decisions, statutory changes, or public referendums. This variation in the mode of implementation helps sharpen our identification strategy. Because SSM legislation that hinges on a court decision is less likely to be influenced by public opinion, changes in loan underwriting to same-sex applicants following court decisions help us make a stronger case for policy exogeneity.

[Table 4 around here]

Similarly, SSM legislation instigated by state legislatures was often unexpected and contrary to contemporary public opinion on the topic. By contrast, a referendum is likely to be a response to increasing awareness and changes in attitude toward SSM within the state that could also correlate with changes in lending policies. In line with our expectation, Table 4 indicates that while SSM implemented via court orders and state legislation is associated with an increased likelihood that same-sex borrowers are denied credit, the effect is insignificant in the states that pass SSM via a referendum.

4.4. Mortgage cost

In addition to mortgage underwriting, we test whether SSM legalization affects the cost of credit (conditional on mortgages being approved). Bayer et al. (2018) show that differences in the price of mortgage credit based on race and ethnicity can be explained by certain borrowers matching with high-cost lenders. Similarly, high-profile US Department of Justice cases were brought against various banks over differentials in the mortgage credit prices they charge to different ethnic groups.[24]

[Table 5 around here]

To test the effect of SSM legalization on the cost of credit, we obtain the contractual interest rates for a subsample of approved mortgages from the Fannie Mae loan origination and performance data.[25,26] This dataset covers approximately one-quarter of the US mortgage market and provides loan-level monthly status updates, including information on loan delinquencies, applicants' risk characteristics, and interest rates. All regressions include bank*county*year fixed effects. Because the Fannie Mae dataset includes only conventional loans, we do not control for indicators of different loan types. Table 5 shows that the coefficient on *Same-Sex*SSM* is not statistically significant, suggesting that SSM laws affect the likelihood of mortgage approval but not the interest rates charged to borrowers of approved mortgages. Further, the results also suggest that lenders do not offset the higher denial rate by offering same-sex applicants lower interest rates on their approved loans.

[24] See, for instance, "Justice Department reaches settlement with Wells Fargo resulting in more than $175 million in relief for homeowners to resolve fair lending claims", the US Department of Justice: https://www.justice.gov/opa/pr/justice-department-reaches-settlement-wells-fargo-resulting-more-175-million-relief.

[25] The Fannie Mae Single-Family Loan Performance Data is publicly available and can be accessed at: https://www.fanniemae.com/portal/funding-the-market/data/loan-performance-data.html. While the McDash database also has loan origination and performance data, the data license does not allow us to identify the lender that originates each mortgage. This prevents us from including bank*county*year fixed effects.

[26] We note that contractual interest rates do not fully reflect the actual borrowing cost, because they ignore the impact of loan fees and other costs charged by the lender, which we do not observe in our dataset.

5. Economic channels

In this section, we explore which mechanisms contribute to explaining the increased denial gap between same-sex and different-sex applicants post-SSM. We examine two potential channels: (1) opinion backlash and (2) information frictions.

5.1. *Mechanism 1: Opinion backlash*

The political science literature argues that legislation on salient events can cause an opinion backlash, resulting in greater disapproval of an issue (Bishin et al., 2016).[27] Consistent with this, Ofosu et al. (2019) find that antigay sentiment increased in states that were "forced" to recognize SSM following the 2015 US Supreme Court ruling.

To test for the backlash mechanism, we examine how our main results vary by local attitudes. We interact *Same-Sex*SSM* with various proxies for *Local Attitudes*: (1) *Religiosity*, which is the number of religious adherents divided by a county's population;[28] (2) *Antidiscrimination Law*, which is a dummy variable that equals 1 if the state has antidiscrimination policies with respect to sexual orientation in effect for employment and housing (Gao and Zhang, 2017);[29] and (3) *Same-Sex Marriage Ban*, which is a dummy variable that equals 1 for states that passed an SSM ban through ballot initiatives (McVeigh and Maria-Elena, 2009).

[Table 6 around here]

Table 6 displays the results. The main coefficient of interest is *Local Attitudes*Same-Sex*SSM*, which captures the heterogeneity in the increase in the denial gap across different proxies for local attitudes.

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[27] To date, public opinion research suggests that backlash caused by the recognition of SSM is rare (Aksoy et al., 2020; Bishin et al., 2016). However, there are questions over the accuracy of polls if individuals are reluctant to respond honestly about discriminatory views (Coffman et al., 2017). Our study draws on actual behavior in a marketplace and should therefore be particularly suitable for isolating backlash based on observed behavior (rather than based on what respondents tell pollsters).

[28] The data are collected by the Association of Religion Data Archives (ARDA) for 2000 and 2010. Following Callen and Fang (2015), we interpolate the data for the remaining years.

[29] The data on state-level antidiscrimination policies are from the Center for American Progress Action Fund; see https://www.americanprogress.org/wp-content/uploads/issues/2012/06/pdf/state_nondiscrimination.pdf.

In all three columns, we find that the coefficient on the triple interaction is not statistically significant. Accordingly, none of the four proxies for local attitudes can explain variation in the same-sex denial gap. This is at odds with the backlash explanation.

It is also worth pointing out that the interaction terms between *Same-Sex*Antidiscrimination Law* (Column (2)) and *Same-Sex*Same-Sex Marriage Ban* (Column (3)) are both statistically insignificant. This indicates that the effect of SSM laws on the denial gap does not appear to be driven by pre-existing attitudes toward same-sex relationships.

As a second test of the backlash mechanism, we examine the ex-post performance of loans extended to same-sex and different-sex applicants following SSM recognition. If opinion backlash caused loan officers to deny applications for noneconomic reasons, loan officers would impose stricter standards on same-sex applicants relative to different-sex applicants following SSM recognition. This would imply that loans extended to same-sex applicants are of higher quality than loans to different-sex applicants.[30] In contrast, if the data were to show that same-sex and different-sex loans are of equal quality, it is unlikely that opinion backlash explains our results.[31]

[Table 7 around here]

We use three proxies to contrast the quality of loans to same-sex and different-sex applicants: the reported *FICO* score and the *Loan/Value* ratio of the originated mortgage (both supplied at the time of the application), and loan defaults. Similar to Cortés et al. (2016), *Default* equals 1 if a loan becomes 90-day

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[30] Because the compensation of most loan officers is partly tied to the volume of originated loans and their repayment, our arguments are consistent with Becker's (1957) taste-based discrimination model in which individuals voluntarily relinquish income to cater to a prejudice. For a more detailed discussion of this approach and the general challenge of inferring bias from lending data, see Gary Becker, "The Evidence Against Banks Doesn't Prove Bias" *Business Week*, April 19, 1993.

[31] Admittedly, our test cannot completely rule out a backlash-based explanation (see, e.g., Dobbie et al, 2021; Ross, 1996). If the "true" default rate in the population was different between majority and minority groups, there would be scenarios that are consistent with our findings that same-sex and different-sex borrowers have similar observable credit quality and opinion backlash. For instance, under the assumptions that (1) same-sex borrowers are on average more likely to default than different-sex borrowers; (2) loan officers are not aware of the higher default likelihood of same-sex borrowers; and (3) loan officers are biased against same-sex applicants (due to opinion backlash), biased loan officers could unintentionally cause same-sex borrowers to have similar default rates to different-sex borrowers.

delinquent during the first three years of its life, and 0 otherwise.[32] The credit quality data come from the Fannie Mae loan origination and performance data. All regressions include bank*county*year fixed effects, and the control variables similar to those in Table 5. The mortgage default regressions also control for borrowers' ex-ante risk characteristics (*FICO* score and *Loan/Value* ratio).

Table 7 displays the results. Across all outcome variables, the coefficients on *Same-Sex*SSM* are statistically insignificant and economically indistinguishable from zero. Thus, there is no relationship between SSM legalization and the quality of loans originated to same-sex borrowers. In Internet Appendix IA7, we further confirm that the non-result on mortgage defaults holds across all segments of applicant credit risk, including applicants who could be considered marginal, i.e., those with a high *Loan/Value* ratio. Overall, given that same-sex borrowers after SSM legalization do not exhibit superior credit quality to different-sex borrowers, the results are at odds with a backlash-based explanation.

5.2. Mechanism 2: Information frictions

Information frictions between loan officers and same-sex borrowers could also explain the higher mortgage denial rates for same-sex borrowers. Stiglitz and Weiss (1981) show that a lack of information about a borrower's credit quality leads to credit rationing. To mitigate information frictions, decisions over mortgage originations are known to rely on credit scores and other hard information that is easy to collect and verify (Stein, 2002). However, Arentsen et al. (2015), Keys et al. (2010), and others show that it is beneficial for some mortgage lenders to process soft information as well. Unlike hard information, soft information is not easy to codify and requires a conscious effort by lenders to collect and process.

If the average loan officer is less familiar with applications from same-sex applicants, they will find it more difficult to process soft information associated with this group of applications. Examples of soft information include the couple's relationship, their emotional attachment to the property, income

[32] The advantage of focusing on the early years of a loan's life is that the borrower characteristics will more closely resemble those at the time the application was submitted for review (Rajan et al., 2015).

sources, spending habits, employment stability, and the quality of the documentation provided by the borrowers.

Our argument is based on Calomiris et al.'s (1994) "cultural affinity" hypothesis. The authors use racial differences between loan officers and applicants to argue that when loan officers are culturally disconnected from the applicants, they will place a greater emphasis on hard information when evaluating borrowers. Minority applicants could then face higher loan denial rates because—if loan officers do not engage in additional and costly collection of information on such applicants—the hard information requirements for loan approvals will be relatively higher for them. To test for the role of information frictions, we perform various tests that explore the role of soft information in mortgage decisions.

First, we provide direct evidence on the importance of hard versus soft information for lending decisions that involve same-sex applicants. We employ a methodology similar in spirit to that of Fisman, et al. (2017) and Skrastins and Vig (2019). For each bank–county–year, we estimate a loan-level regression in which the mortgage denial decision is a function of all observable loan and borrower characteristics, and collect the residuals from each regression. We then use the regression residuals to calculate R^2 separately for same-sex and different-sex observations.[33] Thus, for each bank–county–year, we have an R^2 for same-sex applications and another for different-sex applications. Because the R^2 captures the extent to which loan officers rely on observable hard information to make approval decisions, $1–R^2$ denotes the use of soft information. Each observation is weighted by the number of applications employed in the lending decision regression. The unit of analysis is at the bank–county–year level.

[Table 8 around here]

Table 8 shows the estimated coefficient on *Same-Sex* is negative and significant, suggesting that loan officers employ less soft information to make a lending decision on same-sex applications before the

[33] For each group of same-sex and opposite-sex observations, R^2 is defined as the variance of the residuals scaled by the variance of the denial indicator variable in that group. This approach ensures that the regression model and estimated model parameters are the same for both same-sex and different-sex applications in each bank–county–year. In unreported results, we reach the same conclusion if we estimate the measure of soft information using two separate regressions for same-sex and different-sex applications.

passage of SSM laws. Importantly, the passage of SSM laws is associated with a further reduction in the use of soft information in mortgage lending decisions involving same-sex applicants. In the post-SSM period, the use of soft information involving same-sex applicants decreases by 1.34% (= 0.0117 / 0.8729). This is consistent with the view that as loan officers process more applications from same-sex borrowers in the post-SSM period, existing information frictions between loan officers and same-sex borrowers mount and cause elevated denial rates for the latter.

An alternative interpretation of the results in Table 8 is that loan officers are unwilling (rather than unable) to incorporate soft information into loan decisions involving same-sex applications in the post-SSM period. However, if this was the case, the gap in soft information should persist over time. Instead, the results in Table 8 point to a temporary struggle by loan officers to collect and process soft information on same-sex borrowers. This pattern is consistent with the dynamics of belief-based discrimination described in Bohren et al. (2019). Initial discrimination—in our case, predicated on biased beliefs that result when loan officers lack soft information on same-sex applicants that are comparable to those they can obtain for different-sex applicants—will be mitigated over time once loan officers process more soft information on same-sex applicants and update their beliefs.

[Tables 9 and 10 around here]

To produce some evidence consistent with loan officers updating their beliefs after they gained more experience in interpreting and collecting soft information for same-sex borrowers, we examine whether previous exposure to applications from same-sex borrowers mitigates the impact of information frictions. For instance, loan officers in states with a higher proportion of same-sex applicants should have an informational advantage when making lending decisions.[34] Similarly, loan officers who work for banks with more applications from same-sex borrowers should also have an advantage in collecting and processing soft information on same-sex borrowers. Table 9 controls for *Same-Sex*SSM* and interactions

[34] This is consistent with prior research suggesting that previous dealings with a particular group of borrowers play a crucial role in shaping mortgage outcomes through the collection of soft information. For instance, Ergungor (2010) finds that a bank's presence in low-income neighborhoods increases mortgage lending to low-income borrowers.

with *% Same-Sex Applications,* the annual percentage of same-sex applications received in each county (Column (1)) and bank (Column (2)), respectively. The interaction terms between *SS Share*Same-Sex*SSM* are negative and statistically significant at the 1% level. This is consistent with our information-based explanation that loan officers with more exposure to same-sex borrowers are less likely to deny applications by same-sex borrowers.

Finally, Table 10 performs a placebo analysis using a separate sample of mortgages originated by FinTech lenders. Unlike traditional lenders, FinTech lenders use lending technology where the loan application process is almost entirely online and involves no human loan officers (Buchak et al., 2018). Therefore, compared with traditional lenders, FinTech lenders would not experience changes in how soft information on same-sex applications is processed. Using a sample of mortgages advanced by FinTech lenders, Table 10 shows that SSM legalization has no effect on (1) the mortgage denial rate or (2) the use of soft information in mortgages processed by FinTech lenders.

6. Conclusions

We analyze a large sample of applications for mortgage finance by same-sex and matched different-sex borrowers. We show that following the extension of SSM to same-sex borrowers, the loan denial gap faced by same-sex borrowers increases. Our analyses find no support for explanations that the increasing loan denial gap is due to public opinion backlash or banks applying higher standards to same-sex applicants than different-sex applicants. Rather, our results suggest that information frictions between loan officers and same-sex borrowers play a role in the disparate impact on same-sex applications.

Like other studies which analyze discrepancies in denial rates across loan applications with different characteristics, we acknowledge that our results might be subjected to omitted variable problems. Specifically, it is possible that the same-sex couple status of borrowers correlates with characteristics that are used by lenders but not observed in our data. For instance, marital status is considered important information that determines borrower riskiness (Black et al., 1978; Ladd, 1982), but we are unable to observe whether the same-sex applicants in our sample are married same-sex couples. It is plausible that, after SSM legalization, unmarried same-sex couples are considered riskier by lenders, resulting in an increased denial rate.

Overall, while our findings are robust to different econometric specifications and several additional data sources, it would be fruitful for future studies to explore alternative explanations of our results, such as one in which lenders' approval decisions of same-sex applications are legitimate business decisions that reflect how they measure and quantify credit risks.

Nonetheless, our results demonstrate that the mortgage denial gap between same-sex and different-sex borrowers has increased post SSM legalization. While we do not conclude that this increase arises from anti-LGBT sentiment, we believe that the documented results are important because they demonstrate that legal changes embarked upon in the spirit of improving equality for a minority group may not generate the envisaged effects. As such, our results warrant further investigation by financial institutions, relevant government bodies, as well as other non-governmental organizations.

Our results are also consistent with the view that, even in the highly integrated and standardized US mortgage finance market, routine underwriting decisions rely on non-negligible soft information on borrowers. Our results also contain an upbeat message regarding access to housing finance. As banks gain more experience in dealing with same-sex borrowers over time, disparate outcomes in the mortgage market are likely to reduce over time.

Finally, while a body of literature suggests that SSM improves public opinion on sexual minority groups, our results question the extent to which the changes in attitudes that respondents report to pollsters lead to changes in behavior toward minority groups. Our analysis of observed behavior in the mortgage market suggests that respondents do not necessarily "do as they say."

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Figure 1 Scatter plot of same-sex households and same-sex mortgage applications

This figure presents a scatter plot of the relationship between the state-level percentage of same-sex households from the American Community Survey (ACS) dataset (on the vertical axis) and the percentage of same-sex mortgage applications at the state level in the HMDA dataset (on the horizontal axis). The ACS identifies same-sex households based on responses from householders where a spouse or unmarried partner is reported to be of the same sex as the respondent. The data cover 50 states between 2005 to 2017 since the start of ACS data in 2005. District of Columbia is excluded from this scatter plot because it has a very high representation of same-sex households in both datasets. The dotted line represents the predicted values from an OLS regression.

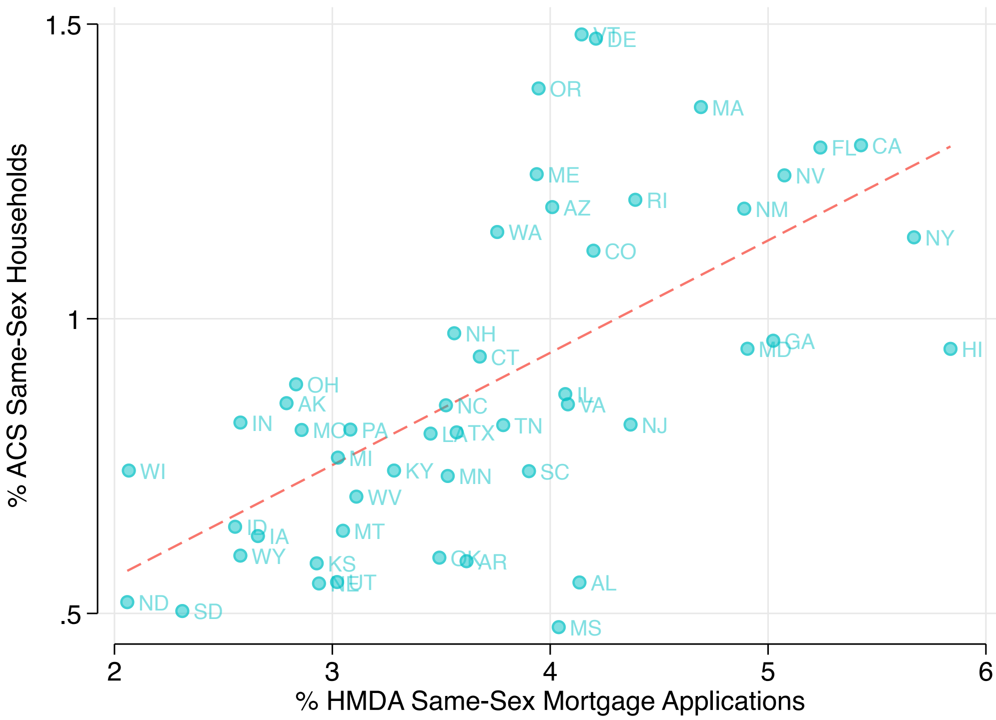

Figure 2 Dynamic effects of same-sex marriage laws on the mortgage denial gap

This figure plots the coefficient of the interaction between *Same-Sex* and time dummies for [–5; ≥+5] years relative to the passage of same-sex marriage (SSM) legislation. The dependent variable is *Denial,* which equals 1 when a mortgage application is denied and 0 otherwise. The control variables are the same as in Column (5) in Table 2. Bank*County*Year fixed effects are included in this analysis. We exclude mortgage applications for properties in California and Massachusetts. The legends indicate the 95% and 99% confidence intervals of the coefficient estimates.

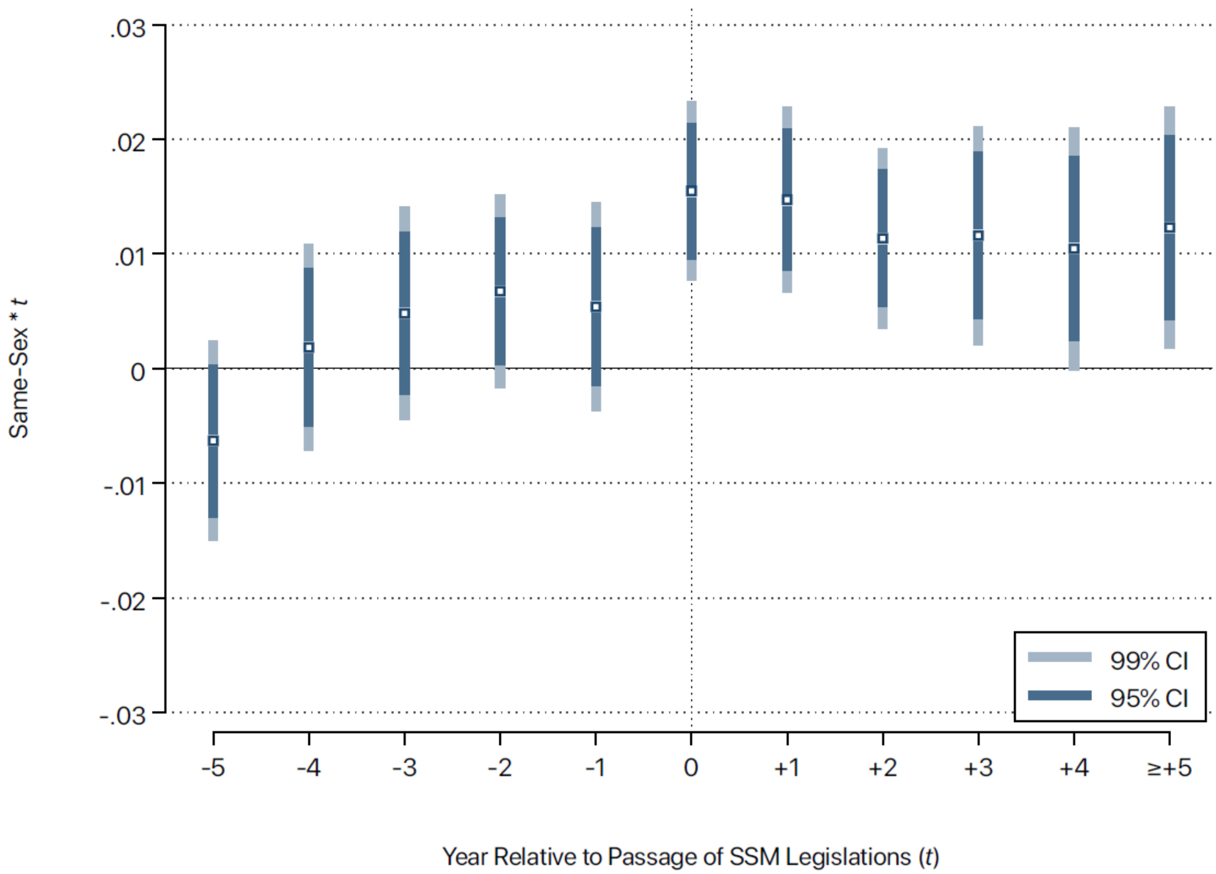

Table 1: Summary statistics

This table presents the summary statistics for key variables. The sample comprises 14,632,079 residential mortgage applications from the HMDA loan application registry between 2004 and 2017. *Denial* is an indicator variable which equals 1 if a loan application is denied, and 0 otherwise. *Same-Sex* is an indicator variable which equals 1 for applications in which the main applicant and the co-applicant are of the same reported sex, and 0 otherwise. SSM is an indicator variable which equals 1 if the mortgage application is submitted in a state during or after the year in which same-sex marriage is legalized. Table A3 defines other variables. Internet Appendix IA4 reports detailed summary statistics on treatment group (same-sex applications in a state following the state's recognition of SSM) and control groups (same-sex applications before SSM, different-sex applications after SSM, and different-sex applications before SSM).

		Standard		Percentiles			
	Mean	Deviation	Min	25th	50th	75th	Max
Denial	0.1738	0.3789	0.0000	0.0000	0.0000	0.0000	1.0000
Same-Sex	0.0390	0.1936	0.0000	0.0000	0.0000	0.0000	1.0000
SSM	0.3088	0.4620	0.0000	0.0000	0.0000	1.0000	1.0000
Ln Applicant Income	4.5498	0.5754	3.0445	4.1589	4.5218	4.9053	6.5539
Ln Loan Amount	4.9751	0.9089	1.6094	4.5109	5.0814	5.5797	7.0031
Loan/Income	1.9409	1.1644	0.0746	1.0746	1.7945	2.6279	6.3673
Male	0.8386	0.3679	0.0000	1.0000	1.0000	1.0000	1.0000
Hispanic	0.0587	0.2351	0.0000	0.0000	0.0000	0.0000	1.0000
Black	0.0174	0.1307	0.0000	0.0000	0.0000	0.0000	1.0000
Asian	0.0465	0.2106	0.0000	0.0000	0.0000	0.0000	1.0000
Other Races	0.0034	0.0584	0.0000	0.0000	0.0000	0.0000	1.0000
FHA Loan	0.0502	0.2184	0.0000	0.0000	0.0000	0.0000	1.0000
VA Loan	0.0089	0.0940	0.0000	0.0000	0.0000	0.0000	1.0000
FSA/RSH Loan	0.0022	0.0468	0.0000	0.0000	0.0000	0.0000	1.0000
Home Improvement	0.0997	0.2996	0.0000	0.0000	0.0000	0.0000	1.0000
Refinance	0.5924	0.4914	0.0000	0.0000	1.0000	1.0000	1.0000
% Same-Sex (State-Year)	3.9971	1.7825	1.4218	3.0420	3.7187	4.5210	16.7842
% Same-Sex (Bank-Year)	3.7607	6.2757	0.0000	0.0000	2.0860	4.8000	100.0000

Table 2: Same-sex marriage laws and loan-level mortgage decisions

Same-Sex is a dummy that equals one if the reported sex of the main applicant is the same as the reported sex of the co-applicant, and zero otherwise. *SSM* is a dummy that equals one in a state during or after the year in which same-sex marriage is legalized in that state, and zero otherwise. The dependent variable is *Denial*, a dummy that equals one if the mortgage application is denied, and zero otherwise. The coefficient on *Same-Sex*SSM* estimates the change in the denial rate, after SSM laws, faced by same-sex applicants compared with that faced by different-sex applicants. Definitions of variables are listed in Table A3. In paratheses are standard errors clustered at the bank-year level; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent variable = *Denial*				
	(1)	(2)	(3)	(4)	(5)
*Same-Sex*SSM*	0.0091***	0.0077***	0.0085***	0.0091***	0.0088***
	(0.0017)	(0.0017)	(0.0017)	(0.0017)	(0.0017)
Same-Sex	0.0341***	0.0417***	0.0372***	0.0375***	0.0405***
	(0.0014)	(0.0013)	(0.0014)	(0.0014)	(0.0014)
Loan/Income	0.0007				0.0126***
	(0.0010)				(0.0008)
Ln Applicant's Income	-0.0922***				-0.0554***
	(0.0018)				(0.0018)
Male		-0.0252***			-0.0086***
		(0.0010)			(0.0008)
Hispanic		0.1305***			0.0945***
		(0.0027)			(0.0024)
Black		0.1753***			0.1488***
		(0.0033)			(0.0030)
Asian		0.0521***			0.0468***
		(0.0024)			(0.0022)
Other Races		0.0715***			0.0583***
		(0.0061)			(0.0060)
FHA Loan			0.0339***		0.0461***
			(0.0031)		(0.0028)
VA Loan			-0.0259***		-0.0015
			(0.0045)		(0.0043)
FSA/RSH Loan			0.0209***		0.0363***
			(0.0073)		(0.0073)
For Refinance				0.1740***	0.1867***
				(0.0041)	(0.0038)
For Home Improvement				0.0777***	0.0904***
				(0.0021)	(0.0020)
Bank*County*Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	14,632,079	14,632,079	14,632,079	14,632,079	14,632,079
Adjusted R-squared	0.160	0.160	0.146	0.159	0.182

Table 3: Additional analyses

Panel A estimates the effect of SSM legalization on separate samples of same-sex and different-sex mortgage applications. Panel B redefines SSM as equal to 1if the mortgage application is submitted in a state after the year in which SSM is legalized in that state, and 0 otherwise. Panel C estimates the baseline model on a subsample of mortgage applications in which the main applicant and the co-applicant are of a different race and/or ethnicity. Panel D splits *Same-Sex* indicator variable into two variables: *Female Same-Sex*, which equals 1 if both the applicant and the co-applicant are female; and *Male Same-Sex* which equals 1 if both the applicant and the co-applicant are male. Panel E presents stacked-regression results. Specifically, we separate sample observations into different year cohorts, based on the years in which SSM laws are passed. For each year cohort, we create a cohort-specific dataset that includes observations from the treated state (i.e., SSM laws passed in that year) and observations from "clean control states," defined as states that do not legalize SSM within the 7-year window around the passage of SSM laws in treatment states (i.e., t=-3,…+3). We then stack these cohort-specific datasets by years relative to SSM legalization to obtain the coefficient estimates. Panel F splits the sample by applicant quality. Panel G presents results with additional interactions between all control variables and various fixed effects. Panel H presents additional robustness checks on various subsamples. *Same-Sex* is a dummy that equals one if the reported sex of the main applicant is the same as the reported sex of the co-applicant, and zero otherwise. *SSM* is a dummy that equals one in a state during or after the year in which same-sex marriage is legalized in that state, and zero otherwise. Definitions of variables are listed in Table A3. In paratheses are standard errors clustered at the bank-year level; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Separate regressions for same-sex and different-sex applications

Dependent variable = *Denial*	Same-Sex (1)	Different-Sex (2)
SSM	0.0100**	0.0000
	(0.0041)	(0.0017)
Control Variables	Yes	Yes
Bank fixed effects	Yes	Yes
County fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Observations	479,068	13,119,171
Adjusted R-squared	0.123	0.088

Panel B: SSM =1 starting in the year after same-sex marriage is legalized

Dependent variable = *Denial*	(1)
*Same-Sex*SSM*	0.0080***
	(0.0017)
Same-Sex	0.0411***
	(0.0013)
Bank*County*Year fixed effects	Yes
Control Variables	Yes
Observations	14,169,604
Adjusted R-squared	0.178

Panel C: Different race and/or ethnicity

Dependent variable = *Denial*	(1)
*Same-Sex*SSM*	0.0169***
	(0.0066)
Same-Sex	0.0273***
	(0.0046)
Bank*County*Year fixed effects	Yes
Control Variables	Yes
Observations	575,342
Adjusted R-squared	0.266

Panel D: Female vs male same-sex applications

Dependent variable = *Denial*	(1)
*Female Same-Sex*SSM*	0.0127***
	(0.0021)
Female Same-Sex	0.0094***
	(0.0017)
*Male Same-Sex*SSM*	0.0049***
	(0.0023)
Male Same-Sex	0.0692***
	(0.0018)
Bank*County*Year fixed effects	Yes
Control Variables	Yes
Observations	14,632,079
Adjusted R-squared	0.183

Panel E: Stacked regressions

Dependent variable = *Denial*	(1)
*Same-Sex*SSM*	0.0145***
	(0.0036)
Same-Sex	0.0407***
	(0.0024)
Cohort*Bank*County*Relative time fixed effects	Yes
Control Variables	Yes
Observations	6,435,905
Adjusted R-squared	0.171

Panel F: Split by applicant quality

Dependent variable = *Denial*	High Applicant income (1)	Low Applicant income (2)	High Loan/Income (3)	Low Loan/Income (4)	High Loan Amount (5)	Low Loan Amount (6)
*Same-Sex*SSM*	0.0074***	0.0109***	0.0071***	0.0094***	0.0070***	0.0105***
	(0.0022)	(0.0025)	(0.0023)	(0.0022)	(0.0023)	(0.0023)
Same-Sex	0.0375***	0.0440***	0.0421***	0.0395***	0.0406***	0.0452***
	(0.0018)	(0.0017)	(0.0019)	(0.0015)	(0.0019)	(0.0014)
Bank*County*Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,280,842	7,460,161	7,418,797	7,259,655	7,354,738	7,595,368
Adjusted R-squared	0.170	0.223	0.188	0.253	0.174	0.249

Panel G: Interacting control variables with fixed effects

Dependent variable = *Denial*	(1)	(2)	(3)	(4)
*Same-Sex*SSM*	0.0073***	0.0091***	0.0080***	0.0066***
	(0.0018)	(0.0019)	(0.0021)	(0.0024)
Same-Sex	0.0409***	0.0404***	0.0393***	0.0393***
	(0.0014)	(0.0015)	(0.0016)	(0.0018)
Controls**SSM*	Yes	Yes	No	No
Controls*Bank*Year fixed effects	No	Yes	Yes	No
Controls*County*Year fixed effects	No	No	Yes	No
Controls*Bank*County*Year fixed effects	No	No	No	Yes
Bank*County*Year fixed effects	Yes	Yes	Yes	Yes
Observations	14,632,079	14,432,005	14,301,554	13,507,491
Adjusted R-squared	0.187	0.227	0.300	0.267

Panel H: Other robustness checks

Dependent variable = *Denial*	2004-2012 (1)	2013-2017 (2)	Excl. 2008-2009 (3)
*Same-Sex*SSM*	0.0114***	0.0073**	0.0073***
	(0.0028)	(0.0030)	(0.0017)
Same-Sex	0.0409***	0.0396***	0.0410***
	(0.0018)	(0.0028)	(0.0013)
Bank*County*Year fixed effects	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes
Observations	8,726,988	4,535,027	12,444,403
Adjusted R-squared	0.150	0.208	0.184

Dependent variable = *Denial*	Excl. 10 Largest Lenders (4)	Excl. 10% Counties with Highest House Price Growth (5)	Excl.10% Counties with Lowest House Price Growth (6)	Only Counties Adjacent to State Borders (7)
*Same-Sex*SSM*	0.0081***	0.0087***	0.0105***	0.0103***
	(0.0023)	(0.0018)	(0.0017)	(0.0026)
Same-Sex	0.0384***	0.0412***	0.0383***	0.0398***
	(0.0016)	(0.0014)	(0.0013)	(0.0020)
Bank*County*Year fixed effects	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes
Observations	7,168,238	12,601,764	12,618,521	4,872,008
Adjusted R-squared	0.276	0.188	0.184	0.197

Table 4: Different modes of SSM implementation

This table modifies the model in Column (5) of Table 2 by replacing the *SSM* indicator variable with four indicator variables: *State Court Decisions*, *Federal Court Decisions*, *State Legislations*, and *Referendums*, which equal one when same-sex marriage is legalized through state court decisions, federal court decisions, state legislations, and state referendums respectively, and zero otherwise. The control variables include *Loan/Income, Ln Applicant's Income, Male, Hispanic, Black, Asian, Other Races*, *FHA Loan*, *VA Loan*, *FSA/RHS Loan and For Refinance*, *For Home Improvement*. *Same-Sex* is a dummy that equals one if the reported sex of the main applicant is the same as the reported sex of the co-applicant, and zero otherwise. *SSM* is a dummy that equals one in a state during or after the year in which same-sex marriage is legalized in that state, and zero otherwise. Definitions of variables are listed in Table A3. In paratheses are standard errors clustered at the bank-year level; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent variable = *Denial*	(1)
*Same-Sex*State Court Decisions*	0.0084***
	(0.0022)
*Same-Sex*Federal Court Decisions*	0.0101***
	(0.0024)
*Same-Sex*State Legislations*	0.0098***
	(0.0030)
*Same-Sex*Referendums*	0.0048
	(0.0046)
Same-Sex	0.0405***
	(0.0014)
Bank*County*Year fixed effects	Yes
Control Variables	Yes
Observations	14,632,079
Adjusted R-squared	0.182

Table 5: Same-sex marriage laws and mortgage costs

The dependent variable, *Interest Rate (%)*, is the contractual interest rate of approved loans. *Same-Sex* is a dummy that equals one if the reported sex of the main applicant is the same as the reported sex of the co-applicant, and zero otherwise. *SSM* is a dummy that equals one in a state during or after the year in which same-sex marriage is legalized in that state, and zero otherwise. Definitions of variables are listed in Table A3. In paratheses are standard errors clustered at the bank-year level; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Interest Rate (%)
	(1)
*Same-Sex*SSM*	-0.0059
	(0.0208)
Same-Sex	0.0355***
	(0.0116)
Loan/Income	-0.0725***
	(0.0066)
Ln Applicant's Income	-0.1110***
	(0.0111)
Male	0.0215***
	(0.0078)
Hispanic	-0.0459*
	(0.0259)
Black	-0.0116
	(0.0748)
Asian	-0.0644**
	(0.0328)
Other Races	0.0228
	(0.0895)
For Refinance	-0.1721***
	(0.0098)
Bank*County*Year fixed effects	Yes
Observations	196,016
Adjusted R-squared	0.891

Table 6: The same-sex borrower denial gap and local attitudes

This table presents cross-sectional results for four geographical proxies for local attitudes as indicated at the top of each column. *Religiosity* is the number of religious adherents divided by a country's population. *Antidiscrimination Laws* is an indicator variable that equals 1 for states in which the law explicitly prohibits discrimination based on sexual orientation and gender identity. *Same-Sex Marriage Ban* is an indicator variable that equals 1 for states that passed a same-sex marriage ban through ballot initiatives (McVeigh and Maria-Elena, 2009). *Same-Sex* is a dummy that equals one if the reported sex of the main applicant is the same as the reported sex of the co-applicant, and zero otherwise. *SSM* is a dummy that equals one in a state during or after the year in which same-sex marriage is legalized in that state, and zero otherwise. Definitions of variables are listed in Table A3. In paratheses are standard errors clustered at the bank-year level; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent variable = Denial		
Proxy for *Local Attitudes* =	*Religiosity*	*Antidiscrimination Laws*	*Same-Sex Marriage Ban*
	(1)	(2)	(3)
*Local Attitudes*Same-Sex*SSM*	-0.0031	0.0031	0.0036
	(0.0142)	(0.0034)	(0.0034)
*Local Attitudes*Same-Sex*	0.0119	-0.0003	-0.0026
	(0.0110)	(0.0027)	(0.0028)
*Same-Sex*SSM*	0.0088***	0.0068***	0.0068***
	(0.0017)	(0.0025)	(0.0027)
Same-Sex	0.0405***	0.0407***	0.0419***
	(0.0014)	(0.0020)	(0.0024)
Bank*County*Year fixed effects	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes
Observations	14,631,852	14,632,079	14,632,079
Adjusted R-squared	0.182	0.182	0.182

Table 7: Same-sex marriage legalization and the credit quality of the originated mortgages

The dependent variables are *FICO/100*, the FICO score reported in the originated mortgage divided by 100; *Loan/Value,* the loan-to-value ratio of the originated mortgage; *Default*, a dummy variable that equals one if the mortgage becomes 90 days delinquent during the first three years of its life. *Same-Sex* is a dummy that equals one if the reported sex of the main applicant is the same as the reported sex of the co-applicant, and zero otherwise. *SSM* is a dummy that equals one in a state during or after the year in which same-sex marriage is legalized in that state, and zero otherwise. Definitions of variables are listed in Table A3. In paratheses are standard errors clustered at the bank-year level; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent variable =	*FICO/100*	*Loan/Value*	*Default*
	(1)	(2)	(3)
*Same-Sex*SSM*	-1.4885	-0.7104	-0.0115
	(1.7892)	(0.6250)	(0.0104)
Same-Sex	0.0531	0.4647	0.0112
	(1.3078)	(0.3455)	(0.0080)
Loan/Income	0.2223	4.9040***	0.0008
	(0.4487)	(0.1452)	(0.0022)
Ln Applicant's Income	4.4135***	6.4256***	-0.004
	(0.8472)	(0.2642)	(0.0035)
Male	1.5577**	-0.3864	0.0011
	(0.6953)	(0.2487)	(0.0042)
Hispanic	-11.8178***	2.4936***	0.0216
	(2.1509)	(0.7669)	(0.0148)
Black	-11.6124	2.3095	-0.0019
	(7.5035)	(2.3108)	(0.0347)
Asian	-0.5591	1.0643	0.0117
	(2.6353)	(0.8069)	(0.0194)
Other Races	-29.0498***	9.7591***	0.0428
	(9.7596)	(2.5285)	(0.0733)
For Refinance	-6.4668***	-12.8459***	0.0159***
	(0.8636)	(0.2621)	(0.0050)
FICO/100			-0.0011***
			(0.0001)
Loan/Value			0.0006***
			(0.0002)
Bank*County*Year fixed effects	Yes	Yes	Yes
Observations	195,691	196,016	195,691
Adjusted R-squared	0.515	0.617	0.530

Table 8: How important is soft information in mortgage denials?

For each bank-county-year, we estimate a loan-level regression in which the mortgage denial decision is a function of all observable loan and borrower characteristics, and collect the residuals from each regression. We then use the regression residuals to calculate R^2 separately for same-sex and different-sex observations. Thus, for each bank-county-year, we have an R^2 for same-sex applications and another for different-sex applications. Because the R^2 captures the extent to which loan officers rely on observable hard information to make approval decisions, $1–R^2$ denotes the use of soft information. *Same-Sex* is a dummy that equals one if the reported sex of the main applicant is the same as the reported sex of the co-applicant, and zero otherwise. *SSM* is a dummy that equals one in a state during or after the year in which same-sex marriage is legalized in that state, and zero otherwise. Definitions of variables are listed in Table A3. In paratheses are standard errors clustered at the bank-year level; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable =	Soft information ($1-R^2$)	
	(1)	(2)
*Same-Sex*SSM*	-0.0129**	-0.0117**
	(0.00416)	(0.00422)
Same-Sex	-0.0269***	-0.0273***
	(0.00252)	(0.00271)
County fixed effects	Yes	No
Bank*Year fixed effects	Yes	Yes
County*Year fixed effects	No	Yes
Observations	33,751	33,749
Adjusted R-squared	0.259	0.407

Table 9: How familiar are banks with mortgage application from same-sex borrowers?

In Column (1), *% Same-Sex Applications by State-Year* is the number of same-sex mortgage applications divided by the total number of mortgage applications in each state-year. In Column (2), *% Same-Sex Applications by Bank-Year* is the number of same-sex mortgage applications divided by the total number of mortgage applications in each bank-year. *Same-Sex* is a dummy that equals one if the reported sex of the main applicant is the same as the reported sex of the co-applicant, and zero otherwise. *SSM* is a dummy that equals one in a state during or after the year in which same-sex marriage is legalized in that state, and zero otherwise. Definitions of variables are listed in Table A3. In paratheses are standard errors clustered at the bank-year level; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent variable = *Denial*	% Same-Sex Applications by State-Year (1)	% Same-Sex Applications by Bank-Year (2)
*% Same-Sex Applications*Same-Sex*SSM*	-0.0037***	-0.0026***
	(0.0013)	(0.0009)
*Same-Sex Share*Same-Sex*	0.0045***	0.0014*
	(0.0010)	(0.0007)
*Same-Sex*SSM*	0.0089***	0.0103***
	(0.0018)	(0.0017)
Same-Sex	0.0397***	0.0398***
	(0.0013)	(0.0017)
Bank*County*Year fixed effects	Yes	Yes
Control Variables	Yes	Yes
Observations	14,632,079	14,632,079
Adjusted R-squared	0.182	0.182

Table 10: Placebo tests using FinTech lenders

This table reports placebo tests using a sample of mortgages originated by FinTech lenders. We follow Buchak et al. (2018) and classify a lender as a FinTech lender if it allows borrowers to obtain pre-approvals online. *Same-Sex* is a dummy that equals one if the reported sex of the main applicant is the same as the reported sex of the co-applicant, and zero otherwise. *SSM* is a dummy that equals one in a state during or after the year in which same-sex marriage is legalized in that state, and zero otherwise. The dependent variable in Panel A is *Denial*, a dummy that equals one if the mortgage application is denied, and zero otherwise. The dependent variable in Panel B is 1– R^2, where R^2 captures the extent to which loan officers rely on observable hard information to make approval decisions. Definitions of variables are listed in Table A3. In paratheses are standard errors clustered at the bank-year level; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: SSM legalization and mortgage denial by FinTech lenders

Dependent variable = *Denial*	(1)
*Same-Sex*SSM*	0.0029
	(0.0050)
Same-Sex	0.0227***
	(0.0042)
Bank*County*Year fixed effects	Yes
Control Variables	Yes
Observations	1,035,784
Adjusted R-squared	0.198

Panel B: SSM legalization and the use of soft information by FinTech lenders

Dependent Variable =	Soft information (1-R^2)	
	(1)	(2)
*Same-Sex*SSM*	0.0273	0.0188
	(0.0248)	(0.0293)
Same-Sex	-0.0402	-0.0332
	(0.0249)	(0.0287)
County fixed effects	Yes	No
Bank*Year fixed effects	Yes	Yes
County*Year fixed effects	No	Yes
Observations	2,908	2,884
Adjusted R-squared	0.210	0.434

Table A1: Same-sex marriage legalization by state

Table A1 reports the date on and method by which same-sex marriage was legalized in each state.

State	Method of Enactment	Date of Enactment
Alabama	Federal court decision	June 26, 2015
Alaska	Federal court decision	October 12, 2014
Arizona	Federal court decision	October 17, 2014
Arkansas	Federal court decision	June 26, 2015
California	State court decision	May 15, 2008
	Federal court decision	August 4, 2010
Colorado	State court decision	July 9, 2014
Connecticut	State court decision	October 10, 2008
Delaware	Legislative statue	May 7, 2013
District of Columbia	Legislative statue	December 18, 2009
Florida	Federal court decision	August 21, 2014
Georgia	Federal court decision	June 26, 2015
Hawaii	Legislative statue	November 13, 2013
Idaho	Federal court decision	October 7, 2014
Illinois	Legislative statue	November 20, 2013
Indiana	Federal court decision	September 4, 2014
Iowa	State court decision	April 3, 2009
Kansas	Federal court decision	June 26, 2015
Kentucky	Federal court decision	June 26, 2015
Louisiana	Federal court decision	June 26, 2015
Maine	Referendum	November 6, 2012
Maryland	Referendum	March 1, 2012
Massachusetts	State court decision	November 18, 2003
Michigan	Federal court decision	June 26, 2015
Minnesota	Legislative statue	May 14, 2013
Mississippi	Federal court decision	June 26, 2015
Missouri	Federal court decision	June 26, 2015
Montana	Federal court decision	November 19, 2014
Nebraska	Federal court decision	June 26, 2015
Nevada	Federal court decision	October 7, 2014
New Hampshire	Legislative statue	June 3, 2009
New Jersey	State court decision	September 27, 2013
New Mexico	State court decision	December 19, 2013
New York	Legislative statue	June 24, 2011
North Carolina	Federal court decision	October 10, 2014
North Dakota	Federal court decision	June 26, 2015
Ohio	Federal court decision	June 26, 2015
Oklahoma	Federal court decision	July 18, 2014
Oregon	Federal court decision	May 19, 2014
Pennsylvania	Federal court decision	May 20, 2014
Rhode Island	Legislative statue	May 2, 2013
South Carolina	Federal court decision	November 12, 2014
South Dakota	Federal court decision	June 26, 2015
Tennessee	Federal court decision	June 26, 2015
Texas	Federal court decision	June 26, 2015
Utah	Federal court decision	June 25, 2014
Vermont	Legislative statue	April 7, 2009
Virginia	Federal court decision	July 28, 2014
Washington	Referendum	February 13, 2012
West Virginia	Federal court decision	October 9, 2014
Wisconsin	Federal court decision	September 4, 2014
Wyoming	Federal court decision	October 17, 2014

Table A2 Sample construction

The sample comprises residential mortgage applications from the HMDA loan application registry between 2004 and 2017. The table reports each step in our sample screening procedure and the number of observations that are dropped because of each screening decision.

	#Observations
HMDA mortgage applications filed with banks that have at least one branch in the same county as the mortgaged property	63,232,605
Less:	
Mortgage applications that were closed for incompleteness or withdrawn by the applicant before a decision was made	(18,198,984)
Mortgage applications without co-applicants	(24,721,015)
Mortgage applications with missing loan or demographic variables	(1,119,741)
Mortgage applications for non-owner occupied properties	(2,404,159)
Sample before covariate balancing	16,088,706
Final sample after covariate balancing	14,632,079

Table A3: Definitions of variables

Variable	Definition
Denial	= 1 if the application is denied by the financial institution, and = 0 if the application is approved and the mortgage is originated.
Same-Sex	= 1 if the reported sex of the main applicant is the same as the reported sex of the co-applicant, and = 0 otherwise.
Different-Sex	= 1 if the reported sex of the main applicant is different from the reported sex of the co-applicant, and = 0 otherwise.
SSM	= 1 if the mortgage application is submitted in a state in the year or after the year in which same-sex marriage is legalized in that state, and 0 otherwise.
State Court Decisions	=1 if same-sex marriage is legalized through a state court decision, and =0 otherwise.
Federal Court Decisions	=1 if same-sex marriage is legalized through a federal court decision, and =0 otherwise.
State Legislations	=1 if same-sex marriage is legalized through a state legislation, and =0 otherwise.
Referendums	=1 if same-sex marriage is legalized through a public referendum, and =0 otherwise.
Ln Applicant Income	Natural logarithm of applicant's gross annual income the lender relies on when making the credit decision (in thousands of dollars).
Ln Loan Amount	Natural logarithm of loan amount (in thousands of dollars).
Loan/Income	Loan amount divided by gross annual income the lender relies on when making the credit decision.
Male	= 1 if the main applicant's reported sex is male, and = 0 if the main applicant is female.
Hispanic	= 1 if the main applicant's reported ethnicity is Hispanic or Latino, and = 0 if the main applicant is not Hispanic or Latino.
Black	= 1 if the main applicant's reported race is Black of African American, and = 0 otherwise.
Asian	= 1 if the main applicant's reported race is Asian, and = 0 otherwise.
Other Races	= 1 if the main applicant's reported race is American Indian, Alaska Native, Native Hawaiian, Other Pacific Islander, and = 0 otherwise.
FHA Loan	= 1 if the loan is insured by the Federal Housing Administration, and = 0 otherwise.
VA Loan	= 1 if the loan is guaranteed by the Veterans Administration, and = 0 otherwise.
FSA/RSH Loan	= 1 if the loan is guaranteed by the Farm Service Agency or the Rural Housing Service, and = 0 otherwise.
Home Improvement	= 1 if the loan's purpose is for home improvement, and = 0 otherwise.
Refinance	if the loan's purpose is for refinancing, and = 0 otherwise.
FICO/100	The FICO score of the originated mortgage divided by 100
Loan/Value	The loan-to-value ratio of the originated mortgage.
Default	=1 if the mortgage becomes 90 days delinquent during the first three years of its life, and = 0 otherwise.
Religiosity	The number of religious adherents divided by a country's population.
Antidiscrimination Laws	=1 for states in which the law explicitly prohibits discrimination based on sexual orientation and gender identity, and = 0 otherwise
Same-Sex Marriage Ban	=1 for states that passed a same-sex marriage ban through ballot initiatives (McVeigh and Maria-Elena, 2009); and = 0 otherwise.
Unverifiable Information	= 1 if the loan officer indicate that the application was denied due to unverifiable information, and = 0 otherwise.
Soft information ($1–R^{2)}$	1-R^2 from the estimations of a lending decision model regressions at the bank-county-year-same-sex level.
% Same-Sex Applications by State-Year	The number of same-sex mortgage applications divided by the total number of mortgage applications in each state-year.
% Same-Sex Applications by Bank-Year	The number of same-sex mortgage applications divided by the total number of mortgage applications in each bank-year.

Internet Appendix

Does Marriage Equality Promote Credit Access? Evidence from Same-sex Marriage Laws

This version: 24 October 2022

Internet Appendix IA1: LGBT proportion of the population in different data sources

This table reports the average LGBT proportion of the state population surveys conducted by the American Community and Gallup-Williams. We report correlations with *% Same-Sex Applications*, the measure we use in the paper to indicate whether applicants are same-sex couples. The data cover 50 states between 2005 to 2017 since the start of ACS data in 2005. District of Columbia (DC) is excluded because it has a high LGBT proportion.

	Average Proportion of Same-Sex Households/Individuals	Correlation with HMDA % Same-Sex Applications	
		Exclude DC	Include DC
American Community Survey (% Same-Sex Households)	0.98%	0.6373	0.8975
Gallup-Williams (% Same-Sex Households)	0.54%	0.7346	0.8938
Gallup-Williams (% LGBT Population)	4.21%	0.6442	0.8572

Internet Appendix IA2: Mortgage applications following the recognition of same-sex marriage

Same-Sex is a dummy that equals one if the reported sex of the main applicant is the same as the reported sex of the co-applicant, and zero otherwise. *SSM* is a dummy that equals one in a state during or after the year in which same-sex marriage is legalized in that state, and zero otherwise. Panel A estimates the change in the propensity of a loan application being from same-sex applicants after SSM laws are passed. The dependent variable in Panel A is *Same-Sex*. In Panel B, the coefficients *Same-Sex*SSM* estimate the change between same-sex and different-sex applicants after SSM laws are passed in the main applicant's income, loan amount, loan-to-income ratio, sex, race, and ethnicity. Definitions of variables are listed in Table A3. In paratheses are standard errors clustered at the bank-year level; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: SSM legalization and the probability that an application is from same-sex borrowers

Dependent variable = *Same-Sex*	(1)
SSM	0.0115***
	(0.0015)
Bank*Year fixed effects	Yes
Observations	16,086,529
Adjusted R-squared	0.0098

Panel B: SSM legalization and the characteristics of same-sex applicants

Dependent variable =	*Ln Applicant Income* (1)	*Ln Loan Amount* (2)	*Loan/ Income* (3)	*Non-White* (4)
*Same-Sex*SSM*	-0.0138***	0.0185***	0.0282***	0.0005
	(0.0027)	(0.0041)	(0.0066)	(0.0020)
Same-Sex	-0.0516***	-0.1631***	-0.1189***	0.0231***
	(0.0017)	(0.0030)	(0.0044)	(0.0010)
Bank*County*Year fixed effects	Yes	Yes	Yes	Yes
Observations	16,073,089	16,073,089	16,073,089	16,073,089
Adjusted R-squared	0.201	0.379	0.230	0.130

Internet Appendix IA3: Alternative matching approaches

This table reports regression results using alternative matching approaches. Column (1) uses propensity score matching instead of Mahalanobis distance kernel matching. Column (2) uses Mahalanobis distance kernel matching, and additionally imposes exact matches between treatment and control observations based on their lender identifiers. *Same-Sex* is a dummy that equals one if the reported sex of the main applicant is the same as the reported sex of the co-applicant, and zero otherwise. *SSM* is a dummy that equals one for a state during or after the year in which same-sex marriage is legalized in that state, and zero otherwise. Definitions of variables are listed in Table A3. In paratheses are standard errors clustered at the bank-year level; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Propensity Score Matching (1)	Matching within the same bank (2)
Dependent variable = *Denial*		
*Same-Sex*SSM*	0.0081***	0.0178***
	(0.0016)	(0.0019)
Same-Sex	0.0452***	0.0371***
	(0.0013)	(0.0016)
Bank*County*Year fixed effects	Yes	Yes
Control variables	Yes	Yes
Observations	14,680,945	11,119,538
Adjusted R-squared	0.180	0.181

Internet Appendix IA4: Distance kernel matching

Panel A presents the mean and standard deviation of key variables from the unmatched full HMDA sample. The full HMDA sample comprises 16,088,706 mortgage applications filed between 2004 and 2017. Panel B presents the weighted mean and standard deviation of key variables after a three-way covariate balancing using Mahalanobis distance kernel matching (Cochran and Rubin, 1973; Jann, 2017). The covariate balanced sample comprises 14,581,079 mortgage applications. Both panels also present the breakdown of the applications into four groups: same-sex applications after the passage of the state-level same-sex marriage laws; same-sex applications before the passage of the state-level same-sex marriage laws; different-sex applications after the passage of the state-level same-sex marriage laws; different-sex applications before the passage of the state-level same-sex marriage laws.

Panel A: Before matching

	Full HMDA Sample		Same-Sex Applications				Different Sex-Applications			
			Pre SSM Laws		Post SSM Laws		Pre SSM Laws		Post SSM Laws	
	Mean (1)	St Dev (2)	Mean (3)	St Dev (4)	Mean (5)	St Dev (6)	Mean (7)	St Dev (8)	Mean (9)	St Dev (10)
Denial	0.183	0.387	0.270	0.444	0.257	0.437	0.183	0.386	0.174	0.379
Same-Sex	0.038	0.190	1.000	0.000	1.000	0.000	0.000	0.000	0.000	0.000
SSM	0.299	0.458	0.000	0.000	1.000	0.000	0.000	0.000	1.000	0.000
Ln Applicant Income	4.527	0.598	4.437	0.612	4.647	0.632	4.462	0.581	4.683	0.603
Ln Loan Amount	4.913	0.951	4.673	0.988	5.183	0.951	4.773	0.929	5.252	0.907
Loan/Income	1.908	1.190	1.741	1.219	2.207	1.326	1.786	1.146	2.196	1.230
Male	0.837	0.369	0.525	0.499	0.525	0.499	0.856	0.352	0.836	0.371
Hispanic	0.071	0.256	0.128	0.334	0.141	0.348	0.064	0.245	0.078	0.268
Black	0.034	0.180	0.059	0.235	0.041	0.199	0.035	0.185	0.027	0.162
Asian	0.050	0.218	0.054	0.226	0.116	0.320	0.034	0.182	0.084	0.277
Other Races	0.012	0.107	0.020	0.140	0.017	0.131	0.011	0.106	0.011	0.105
FHA Loan	0.057	0.232	0.132	0.338	0.119	0.323	0.056	0.230	0.050	0.219
VA Loan	0.018	0.133	0.005	0.072	0.009	0.093	0.016	0.127	0.023	0.151
FSA/RSH Loan	0.005	0.072	0.003	0.054	0.003	0.056	0.005	0.072	0.005	0.073
Home Improvement	0.110	0.313	0.117	0.322	0.103	0.304	0.114	0.317	0.102	0.303
Refinance	0.582	0.493	0.459	0.498	0.452	0.498	0.599	0.490	0.559	0.497
Observations		16,088,706		395,576		209,668		10,884,694		4,598,768

Panel B: After matching

	Matched HMDA Sample		Same-Sex Applications				Different Sex-Applications			
			Pre SSM Laws		Post SSM Laws		Pre SSM Laws		Post SSM Laws	
	Mean (1)	St Dev (2)	Mean (3)	St Dev (4)	Mean (5)	St Dev (6)	Mean (7)	St Dev (8)	Mean (9)	St Dev (10)
Denial	0.229	0.420	0.253	0.435	0.253	0.435	0.209	0.407	0.201	0.401
Same-Sex	0.493	0.500	1.000	0.000	1.000	0.000	0.000	0.000	0.000	0.000
SSM	0.500	0.500	0.000	0.000	1.000	0.000	0.000	0.000	1.000	0.000
Ln Applicant Income	4.642	0.627	4.636	0.625	4.641	0.624	4.643	0.630	4.649	0.627
Ln Loan Amount	5.184	0.940	5.175	0.937	5.179	0.940	5.185	0.940	5.197	0.941
Loan/Income	2.207	1.313	2.202	1.310	2.199	1.309	2.207	1.314	2.220	1.320
Male	0.527	0.499	0.525	0.499	0.525	0.499	0.529	0.499	0.529	0.499
Hispanic	0.137	0.344	0.138	0.344	0.135	0.342	0.139	0.346	0.138	0.345
Black	0.038	0.191	0.038	0.190	0.037	0.189	0.039	0.195	0.038	0.191
Asian	0.113	0.316	0.110	0.314	0.111	0.315	0.114	0.318	0.115	0.319
Other Races	0.013	0.113	0.012	0.107	0.011	0.107	0.015	0.120	0.014	0.119
FHA Loan	0.114	0.318	0.114	0.318	0.112	0.315	0.116	0.321	0.115	0.319
VA Loan	0.007	0.081	0.005	0.071	0.005	0.072	0.008	0.089	0.008	0.090
FSA/RSH Loan	0.003	0.051	0.002	0.049	0.002	0.048	0.003	0.054	0.003	0.053
Home Improvement	0.099	0.298	0.097	0.296	0.098	0.297	0.099	0.299	0.100	0.300
Refinance	0.457	0.498	0.457	0.498	0.458	0.498	0.457	0.498	0.457	0.498
Observations		14,632,079		369,113		201,477		9,744,025		4,317,464

Internet Appendix IA5 Additional controls for local credit risk

This table additionally controls for census-tract-level credit risk variables. *FICO Score (Census Tract*) is the average reported FICO score of borrowers at the census-tract-level. *Loan-to-Value (Census Tract)* is the average loan to property value of originated loans at the census-tract-level. *Debt-to-Income (Census Tract)* is the average debt of a borrower's income at the census tract-level. These variables are measured at the census tract-level using data on originated loans from McDash. *Same-Sex* is a dummy that equals one if the reported sex of the main applicant is the same as the reported sex of the co-applicant, and zero otherwise. *SSM* is a dummy that equals one in a state during or after the year in which same-sex marriage is legalized in that state, and zero otherwise. Definitions of variables are listed in Table A3. In paratheses are standard errors clustered at the bank-year level; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)
Dependent variable = *Denial*	
Same-Sex*SSM	0.0094***
	(0.0021)
Same-Sex	0.0402***
	(0.0015)
FICO Score (Census Tract)	-0.0002***
	(0.0000)
Loan-to-Value (Census Tract)	0.0174***
	(0.0067)
Debt-to-Income (Census Tract)	-0.0000
	(0.0001)
Bank*County*Year fixed effects	Yes
Control variables	Yes
Observations	12,445,237
Adjusted R-squared	0.171

Internet Appendix IA6: Narrower time window around SSM legalization

This table narrows the sample to a shorter time window around SSM legalization. Columns (1)–(3) respectively use [-1, +1], [-2, +2], and [-3, +3] years around SSM legalization. *Same-Sex* is a dummy that equals one if the reported sex of the main applicant is the same as the reported sex of the co-applicant, and zero otherwise. *SSM* is a dummy that equals one in a state during or after the year in which same-sex marriage is legalized in that state, and zero otherwise. Definitions of variables are listed in Table A3. In paratheses are standard errors clustered at the bank-year level; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	[-1, +1] (1)	[-2, +2] (2)	[-3, +3] (3)
Dependent variable = *Denial*			
*Same-Sex*SSM*	0.0197***	0.0179***	0.0143***
	(0.0046)	(0.0040)	(0.0036)
Same-Sex	0.0392***	0.0401***	0.0407***
	(0.0033)	(0.0028)	(0.0024)
Bank*County*Year fixed effects	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes
Observations	3,468,270	4,949,925	6,435,905
Adjusted R-squared	0.178	0.177	0.171

Internet Appendix IA7: SSM legalization and mortgage default, by segments of applicant credit risk

This table examines the relationship between SSM legalization and mortgage default for each quartile of applicants' Loan/Value ratio. The dependent variable is *Default*, a dummy variable that equals one if the mortgage becomes 90 days delinquent during the first three years of its life. *Same-Sex* is a dummy that equals one if the reported sex of the main applicant is the same as the reported sex of the co-applicant, and zero otherwise. *SSM* is a dummy that equals one for a state during or after the year in which same-sex marriage is legalized in that state, and zero otherwise. Definitions of variables are listed in Table A3. In paratheses are standard errors clustered at the bank-year level; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent variable = *Default*	*Lowest Q1*	*Q2*	*Q3*	*Highest Q4*
	(1)	(2)	(3)	(4)
*Same-Sex*SSM*	0.0281	0.0041	-0.001	-0.0272
	(0.0384)	(0.0250)	(0.0238)	(0.0361)
Same-Sex	-0.0123	0.0067	0.0015	0.0286
	(0.0294)	(0.0130)	(0.0160)	(0.0283)
Bank*County*Year fixed effects	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes
Observations	27,144	38,456	44,214	35,973
Adjusted R-squared	0.614	0.563	0.624	0.646

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