

INSTITUTO TECNOLÓGICO AUTÓNOMO DE MÉXICO



**Cambios en sesgos implícitos,
discriminación y crecimiento
económico: Evidencia a partir de la
legalización del matrimonio igualitario**

TESIS

QUE PARA OBTENER EL TÍTULO DE
LICENCIADO EN ECONOMÍA

PRESENTA

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Resumen

En Economía se ha puesto poco o nulo énfasis en los matices del comportamiento económico de las minorías históricamente discriminadas. [Hsieh et al. (2019)] es una de las excepciones. Los autores muestran como el 44 por ciento del crecimiento económico per cápita en EE. UU., de 1960 a 2010, se debe a una reducción en los obstáculos a la acumulación de capital humano y en la discriminación laboral que las mujeres y los negros enfrentan. A la fecha, no existe un estudio que investigue si existe un mecanismo similar para el caso de las minorías sexuales, y la legalización escalonada del matrimonio igualitario en EE. UU. es un experimento natural que permite realizar investigación causal que podría arrojar luz sobre esta pregunta. Usando un índice de búsquedas homofóbicas en Google, encuentro que la legalización del matrimonio igualitario tiene efectos opuestos en la prevalencia de sentimientos homonegativos dependiendo del método de legalización, judicial o legislativo. Los estados que legalizaron el matrimonio igualitario judicialmente, son 35 por ciento más homofóbicos 10 años después; mientras que los estados que legalizaron por vía legislativa, son 25 por ciento menos homofóbicos 4 años después. A su vez, estos cambios en la prevalencia de sentimientos homonegativos probablemente se traducen en cambios en el desempeño de la economía. De acuerdo con [Black et al. (2007)], los hombres homosexuales se tienden a concentrar en industrias en donde la mayoría de las trabajadoras son mujeres, pues tienden a enfrentarse con menor discriminación. Con base en este hecho, construyo una variable con el PIB de las cinco industrias con el menor porcentaje de mujeres por estado. Encuentro evidencia de una reducción de un 23.93 por ciento en esta medida del PIB 10 años después de la legalización del matrimonio igualitario por vía judicial. Argumento que el mecanismo detrás de este resultado es un empeoramiento en la asignación de trabajadores homosexuales en la economía. Así, aun cuando normativamente las minorías sexuales deben tener igualdad de derechos, imponer este cambio judicialmente puede resultar contraproducente.

Abstract

Mainstream economics has been, with a few exceptions, blind to heterogeneities in economic behavior across minority groups. Recently, Hsieh et al. (2019) showed that declining obstacles to human capital accumulation and labor market discrimination for blacks and women explain 44 percent of growth of U.S. GDP per person between 1960 and 2010. There has not been a formal study, to date, that investigates if a similar mechanism exists for sexual minorities. The way in which same-sex marriage (SSM) was legalized in the U.S., a staggered roll-out design, allows for the possibility of causal research. Using a homophobic (Google) search index (HSI) I find that whether SSM legalization occurred via the legislature or the judiciary had dramatically different effects, ranging from 25 percent lower to 35 percent higher levels on the HSI, respectively. These changes in homonegative sentiment likely had an impact on economic performance. Building on Black et al. (2007)'s finding that gay men tend to concentrate in industries where the majority of workers are women, as they might experience less discrimination, I construct a variable of the chained GDP of the five industries that have the least share of women, by state. I find evidence suggesting that 10 years after SSM legalization via the judiciary, this GDP measure was 23.93 percent lower, had SSM not been legalized that way. I argue that an increased misallocation of gay men workers might have driven this worsened economic performance. Whilst normatively sexual minorities ought to have equal rights, there may be more and less effective ways to achieve this goal.

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Chapter 1

Introduction

Mainstream economics has been, with a few exceptions, blind to heterogeneities in economic behavior across minority groups. It was not until Becker (1995), the seminal work on the economics of discrimination, that the field started to open up. Nonetheless, the understudy of discrimination issues and its effect on economic outcomes remains a relevant problem. This research is an effort to diminish this gap.

Recently, Hsieh et al. (2019) showed that declining obstacles to human capital accumulation and labor market discrimination for blacks and women explain 44 percent of growth of U.S. GDP per person between 1960 and 2010. Misallocation of resources is the main mechanism behind this finding. In 1960, white men accounted for 94 percent of doctors and lawyers. By 2010, they made up for just over 60 percent. Similar patterns occurred throughout the economy, particularly in high-skill occupations. It was not that black men and women were not capable of performing these jobs, nor that they did not have a preference for them; it was because of the obstacles they

encountered that white men did not. There has not been a formal study, to date, that investigates if a similar mechanism exists for LGBTQ people. This is particularly relevant given that recent polls suggest LGBTQ populations estimates have been highly underestimated and hence their impact in the economy is likely to be higher than previously thought. (Jones, 2021) Synthetically, I investigate if same-sex marriage (SSM) legalization leads to changes in discrimination towards sexual minorities, and if so, whether this translates in changes in economic performance.

The way in which same-sex marriage was legalized in the U.S., a staggered roll-out design, allows for the possibility of causal research. Same-sex marriage legalization in the U.S. started in 2004, when the Massachusetts Supreme Judicial Court ruled in *Goodridge v. Department of Public Health* that it was unconstitutional under the Massachusetts Constitution to allow only opposite-sex couples to marry. It became legal nationwide in 2015 with *Obergefell v. Hodges*, a landmark decision of the Supreme Court of the United States (SCOTUS). Figure 1.1 shows the timing of same-sex marriage legalization at the state level. I exploit the variation in the timing to test whether there was a change in private attitudes, or bias, against sexual minorities. Instead of using a Two-Way Fixed Effects (TWFE), as it is invalid when treatment effects are not homogeneous across groups and time, I use a stacked regression and the estimator proposed by Callaway and Sant'Anna (2020).

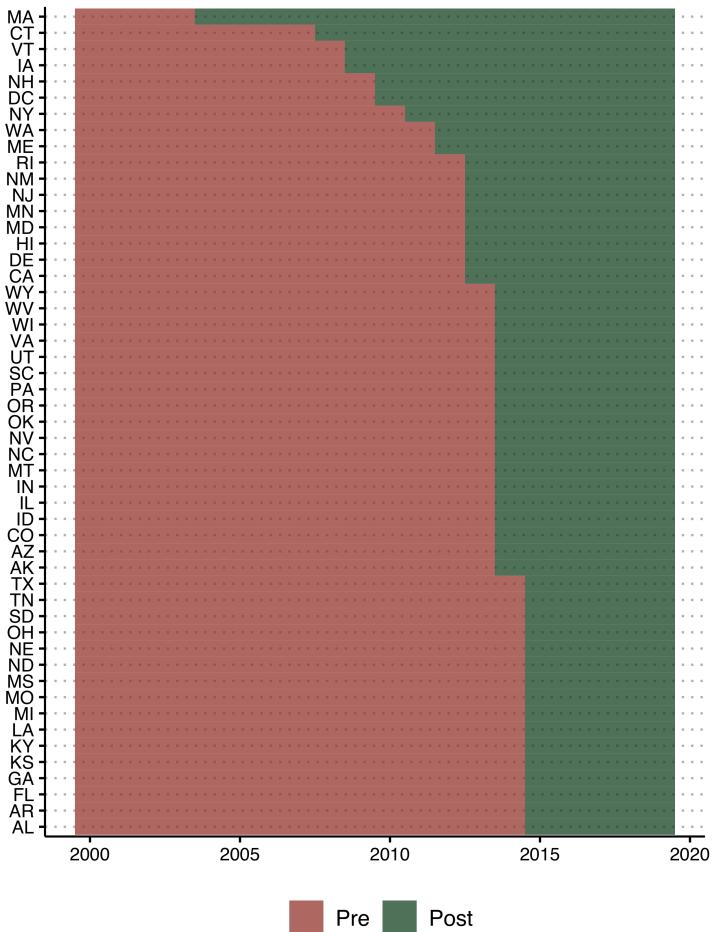


Figure 1.1. Timing of Same-sex marriage legalization in the U.S.

The path towards same-sex marriage legalization has not been linear. In 1993 Hawaii State Supreme Court (HSSC) was the first to consider legal challenges to bans on same-sex marriage. However, in 1998 before HSSC considered the final appeal of the case, voters modified the state Constitution to restrict marriage to different-sex

couples. In the midst of the debate, many other states followed Hawaii and constituted a constitutional ban on same-sex marriage. Hatzenbuehler et al. (2010) examines the relation between living in states that instituted bans on same-sex marriage during the 2004-2005 elections and the prevalence of psychiatric morbidities in LGB populations. His findings suggest that living in states where same-sex marriage bans were instituted signify an increase in the prevalence of psychiatric morbidities of: 248.2 percent for general anxiety disorder; 41.2 percent for alcohol use disorder; 36.6 percent for any mood disorder; and 36.3 percent for psychiatric comorbidities.

Hatzenbuehler (2011) has shown that the social environment surrounding lesbian, gay and bisexual youth may contribute to their higher rates of suicide attempts, once controlling for individual risk factors. He estimates that suicide risk for LGB youth is 20 percent higher than their heterosexual counterparts in an unsupportive environment.¹ In a study with a more direct relationship with my research, Everett et al. (2016), the authors study the effect of civil union legalization (a close substitute to same-sex marriage for some authors) on perceived discrimination, depressive symptoms, and indicators of hazardous drinking for sexual minority women. They find that civil union legislation was associated with lower levels in all these variables. More recently, Perales and Todd (2018) expand the available evidence to Australia. The authors analyze the effects of the 2017 plebiscite regarding same-sex marriage legalization. They find that

¹The social environment is measured considering: (1) proportion of same-sex couples living in the counties; (2) proportion of Democrats living in the counties; (3) proportion of schools with gay-straight alliances; (4) proportion of schools with anti-bullying policies specifically protecting LGB students; and (5) proportion of schools with anti-discrimination policies that included sexual orientation.

LGB people report comparatively worse life satisfaction, mental health and overall health in constituencies with higher shares of ‘No’ voters, controlling for a large set of individual- and aggregate-level confounds.

Discrimination, then, has real effects in the well-being of sexual minorities. Moreover, it has adverse effects for society as a whole. As shown by Hsieh et al. (2019), discrimination hampers economic growth. This research is an attempt to analyze the effects of discrimination on economic growth in the case of LGBTQ populations.

A growing number of researchers consider that social norms determine if, when and against whom discrimination is acceptable. Same-sex marriage legalization constitutes a shift in such norms. It is then possible to test whether and how same-sex marriage legalization caused changes in social norms, these in discrimination, and discrimination on economic growth. More specifically, my research consists of two questions:

Question 1. *What is the effect of SSM legalization on social norms and discrimination towards sexual minorities?*

Question 2. *If discrimination was affected by SSM legalization, what was the effect on the economy?*

The rest of the paper is distributed as follows. Chapter 2 summarizes the literature of changes in attitudes towards LGBTQ populations, and the literature of changes in economic outcomes caused by same-sex marriage legalization or changes in attitudes towards sexual minorities. Chapter 3 motivates and explains the data sources used. Chapter 4 briefly summarizes the recent advances in the *Difference-in-Differences* methodology for causal identification and lays down the methods used. Chapter 5 shows the results, discusses

them and provides robustness checks. Finally, Chapter 6 provides some concluding remarks.

Chapter 2

Literature Review

In this Chapter I first summarize the literature on changes in attitudes towards LGBTQ populations due to SSM legalization. The conclusion is that the evidence is mixed. A main concern with the previous studies is that most of them do not consider a potentially differentiated impact through its method of legalization: via the legislature or the judiciary. Constitutional Law theory, and specifically the judicial backlash thesis, suggests that whereas legalization via the legislature would lead to a positive shift in attitudes towards LGBTQ populations, legalization through the judiciary may lead to backlash. Next, I review the literature on the impact of same-sex marriage legalization and changes in attitudes towards sexual minorities on economic outcomes.

2.1 Changes in public opinion

Focusing on gay men and men who have sex with men, Lamontagne et al. (2018) measure homophobia at the country level and with it,

demonstrate the negative social, economic and health consequences of homophobia in low- and middle-income countries. For instance, they find that a 10 percent increase in GDP per capita is associated with a 1 percentage point reduction in the mean homophobic climate index. Hence, it is economically relevant to measure changes in the population's homophobic biases.

[Donovan and Tolbert (2013)] is one of the first studies to investigate whether major events concerning minority groups change public opinion towards them. Using the American National Election Study (ANES) panel data, the authors test whether an individual's assessment of gays and lesbians changed if the person resided in a state where same-sex marriage was on the ballot in 2004, compared with their baseline evaluations in 2002. Same-sex marriage was banned in every state that had a referendum. They find that the marriage debate, and not the standing of the *status quo*, had a stigmatizing effect on attitudes towards gays and lesbians in states where marriage was on the ballot. These results are in line with [Hatzenbuehler et al. (2010): the discussion of minority rights led to negative mental health outcomes for sexual minorities.

[Kreitzer et al. (2014)] study the effect of the 2009 Iowa Supreme Court ruling, *Varnum v. Brien*, which upheld that the state's limitation of marriage rights to opposite-sex couples violated the State Constitution's equal protection clause. Using survey data from the Iowa Social Science Research Center (ISSRC) collected in the spring of 2009 and a re-interview after the court's decision, they find that the signaling of new social norms pressured some respondents to modify their expressed attitudes. Specifically, respondents whose demographic characteristics would predict support for marriage equality, but previously did not, were more likely to shift their opinions to be

consistent with the new state law. The main problem with these findings is that it is impossible to know if the public opinion towards sexual minorities *truly* shifted positively or if it is a function of individuals having to conform to the signaled shift in social norms. Perhaps their implicit biases do not change, and discriminatory practices are upheld.

A large proportion of the public opinion literature centers on the existence of backlash after challenges to the pre-existing social norms. [Bishin et al. \(2016\)](#) define opinion backlash as “a large, negative, and enduring shift in opinion against a policy or group that occurs in response to some event that threatens the *status quo*”. This seems to be now the standard definition, and throughout this paper I would ascribe to it.

The authors examine if SCOTUS’ *Obergefell v. Hodges* prompted backlash against gays and lesbians mainly using an online survey experiment using Amazon Mechanical Turk (AMT) as a subject recruitment program with a sample of 2,402 respondents. They began the survey on June 7 and stopped the survey on June 17, days before SCOTUS rendered *Obergefell v. Hodges* on June 26, 2013. Their method consists of exposing subjects to one of five randomly assigned news excerpts that contain information about gays and lesbians and compare it to one baseline condition that contains no information regarding them; the measure of backlash is the difference between the mean opinion in the baseline condition from the mean opinion in the treatment. It is worth noting that while they find negative results, particularly, when analyzing backlash prompted by legalization through the judiciary, indicating backlash, they dismiss them as “in no case are the effects significant or large”. Moreover, their methods are not suitable to test their definition of backlash, which emphasizes an

enduring shift in public opinion.

More generally, Flores and Barclay (2016) study the effects of state-level and nation-wide policy changes regarding same-sex marriage during 2013: SCOTUS' *Windsor v. United States*, where they declare the Defense of Marriage Act (DOMA) as unconstitutional, and state legalizations of same-sex marriage. Using data from the American National Election Study (ANES) from 2012 and a re-contact study in 2013, the authors find that residents of states that had same-sex marriage policy introduced had the greatest reduction of anti-gay attitudes. They are the first to analyze alternative mechanisms through which public opinion changes may occur: consensus, legitimacy, polarization and backlash. According to them, the underlying mechanism of their findings is that of *consensus*, i.e., policy and judicial acts may simply be the enactment of the majority opinion and bear no subsequent impact on mass attitudes, and/or, that of *legitimacy*, i.e., laws may represent the acceptability of an issue, so judicial and legislative actions may enshrine shifts in public opinion through people's own respect for the rule of law.

While most of the literature has focused on the U.S., Abou-Chadi and Finnigan (2019) study the European case. Combining data from eight waves of the European Social Survey (2002 – 2017) with data on legislation, the authors analyze the effects of same-sex marriage, registered partnerships, and marriage bans on attitudes toward gays and lesbians. They argue that different same-sex relationship policies vary in their impact. Whereas registered partnership laws construct a distinct target population that receives new benefits, marriage equality sends an unambiguously positive signal and reduces the perceived group difference through inclusion into existing rights. Their findings suggest that marriage has a positive effect, while bans and registered

partnerships have a negative effect. Their main similarity to my research is that they consider heterogeneities on attitude changes depending on the signal the policy shift sends to the population.

The problem with prior studies cited before is that they focus on self-reported biases against LGBTQ populations. Ofosu et al. (2019) are the first to analyze changes in implicit biases, which are a better proxy for the probability of discrimination against minorities. Using data from the Implicit Association Test² (IAT) they tested whether state-by-state same-sex marriage legislation was associated with decreases in anti-gay implicit bias. Surprisingly, they find that moderating this effect was whether states passed legislation locally: although states passing legislation experienced a greater *decrease* in bias following legislation, states that never passed legislation demonstrated *increased* anti-gay bias following federal legalization. This is the first study to explicitly consider the possibility for a heterogeneous effect by the legalization method.

According to the Bishin et al. (2016) definition of opinion backlash, backlash after same-sex marriage legalization is possible regardless of its method of legalization. Even though Flores and Barclay (2016) do mention that backlash theorists consider court action more likely to prompt adverse reactions, as legislative or direct democratic institutions are inherently responsive to the majority will, they do not elaborate on it. As mentioned, apart from Ofosu et al. (2019), no other study has considered the possibility for differences in same-sex marriage legalization depending on its legalization method. I contribute to this literature by bridging this gap and analyzing the changes from 2004 to 2019 using a homophobic (Google) search index

²<https://implicit.harvard.edu/implicit/aboutus.html>

that measures implicit bias.

The theoretical framework I consider for the differences in the changes in public opinion depending on the legalization method is drawn from Constitutional Law theory. Siegel (2017) explains that according to the judicial backlash thesis, courts striking down popular legislation to vindicate minority rights are not only ineffective, but counterproductive: “judicial decisions “shutting down” politics could frustrate democratic majorities in ways that would produce more virulent politics than might have resulted had judges refused to intervene”. In plain terms, as the judiciary is not legitimized to make political decisions (as they are not democratically elected), when they settle controversial debates through rulings, a backlash occurs. I theorize, then, that a backlash would occur only in states where the method of legalization was judicial.

2.2 Economic outcomes

It was not until the 2000s that economists started to study LGBTQ issues formally. Badgett (2010) is a pioneer work that aims to argue, from an economic perspective, that same-sex marriage signified a Pareto improvement in society. The author argues that same-sex couples gain economic rights through same-sex marriage, as well as those related to taxation, state employee benefits, and dissolution, which provide an important and valuable economic benefit. Demand for same-sex marriage before its legalization is another proxy for the economic value of marriage to same-sex couples.

While Badgett (2010) provides a broad economic conceptualization of same-sex marriage, Carpenter and Eppink (2017) analyze its potential effects in same-sex couples income and labor decisions using

National Health Interview Survey (NHIS) data from 2013 to 2015. They reproduce a well-documented finding that self-identified lesbians earn significantly more than comparable heterosexual women. They also show that self-identified gay men also earn significantly more than comparable heterosexual men, a difference on the order of 10 percent of annual earnings. Nonetheless, self-identified gay men have lower employment rates³. The increase in gay men earnings are the authors' biggest puzzle.

They consider the fact that nation-wide, according to the Pew Research Center, the proportion of adults who favored same-sex marriage increased from 35 to 55 percent from 2001 to 2016, thus suggesting a clear improvement in attitudes towards LGBTQ populations. This is confirmed through the comparison of well-designed correspondence studies that point to the possibility that bias against LGBTQ individuals in the job search process has declined. In particular, Tilcsik (2011) performed an audit study in which he sent over 1,700 resumes to job advertisements in 2005. He found that randomly assigned “gay” resumes (as signaled through participation in an LGBT organization) received significantly fewer callbacks than otherwise similar resumes without such treatment.⁴ More recently, however, Bailey et al. (2013) which manipulated sexual orientation through participation in an LGBT student organization, and Acquisti and Fong (2015) which manipulated sexual orientation via a Facebook profile, found no significant differences in callback rates

³In particular, gay men are estimated to be 5.4 percentage points less likely than comparable heterosexual men to be in full-time work, while bisexual men are 11.9 percentage points less likely to be in full-time work.

⁴The magnitude of the difference was about as large as the black/white callback difference documented in Bertrand and Mullainathan (2004).

for gay candidates compared to other candidates in 2010 or 2013.

Carpenter and Eppink (2017) take into account the decrease in bias against LGBTQ populations in the U.S., and investigate whether it explains the earnings increase of gay men, a pattern that according to Clarke and Sevak (2013) has been going on since the 1990s and which they attribute to this decrease in negative bias. They argue that although there has likely been a reduction in the extent of labor market discrimination against gay men, it is unlikely to explain the overall patterns observed in the NHIS, as they remain underemployed relative to their heterosexual counterparts. Another reason is that lesbian couples do not show the same pattern. The authors are also skeptical of an overall improvement of the pattern of progress for LGBTQ rights. They mention, though unfortunately they do not cite their reference, that there were substantial increases in LGBT-related harassment reported to governments and police agencies in the wake of major policy rulings on same-sex marriage, and that there is still pervasive anti-LGBT sentiment throughout the United States.

They propose an alternative explanation: it is possible that increased legal access to marriage induced greater changes in gay coupling behavior than in lesbian coupling behavior, to the extent that lesbian households already functioned effectively as a “married” unit. These Becker-based dynamics make it possible that changing legal access to gay marriage would have induced larger changes in home versus market-based specialization within gay male households than the associated change in lesbian households. The main evidence for this hypothesis is that in the NHIS gay men have significantly lower employment rates than otherwise similar heterosexual men.

Nonetheless, according to Sansone (2019), the effect of same-sex marriage legalization on same-sex couples’ employment is unclear

ex-ante. On the one hand, following a standard Becker model, these changes could have discouraged individuals in a same-sex couple from both being employed at the same time. On the other hand, it is possible that SSM legalization drove a shift in social norms and a reduction in discrimination against sexual minorities; a more tolerant working environment may have then increased both the labor supply and demand of individuals in same-sex couples.⁵ Sansone (2019) finds evidence supporting the increase in labor supply and demand of individuals in same-sex couples due to a reduction in discrimination against sexual minorities. Specifically, he finds that same-sex marriage legalization increased the probability that both partners in same-sex couples were working by 2.4 percentage points, which is equivalent to an almost 4 percent increase from his baseline employment levels.

He devotes several efforts to testing for the plausibility that a decrease in discrimination against sexual minorities was the main driver in the observed short-run increases in employment. For instance, he adds an indicator for whether states introduced other policies affecting LGBT individuals in the time period considered (a constitutional ban on same-sex marriage, legalized domestic partnership and civil unions, anti-discrimination laws, and allowed or prohibited adoptions by same-sex couples). He finds that favorable policies towards sexual minorities increase the probability that both partners in same-sex couples were working, while unfavorable policies diminish it.

⁵Sansone (2019) elaborates on the different theories that may explain discrimination against sexual minorities: taste-based discrimination, statistical discrimination and discrimination based on social norms. There is no need to explore each of them in particular, as a reduction in any or all of this would be in line with an observed increase in the labor supply and demand.

Additionally, he considers the fact that, according to [Plug et al. (2014)], LGBTQ workers tend to sort into tolerant occupations, as comparing twins with different sexual orientations, gays, lesbians and bisexuals were less likely to work in occupations with prejudiced workers. As well as the fact that, according to [Black et al. (2007)], workers in male same-sex couples were in occupations with a higher proportion of women than workers in different-sex couples. He then tests if same-sex marriage legalization induced a shift of minority workers towards historically tolerant occupations (or to disclose their sexual orientation if already employed in these sectors). He finds that the probability of being employed in an occupation with a majority of female workers decreased by 1.4 percentage points after same-sex marriage legalization.

Linking these findings to those of [Hsieh et al. (2019)], it is possible to test whether sectors with a higher share of men, i.e., potentially less tolerant occupations, exhibit a better or worse economic performance after legalization of same-sex marriage. This is the second question of the present research in specific terms. If same-sex marriage results in a reduction of homonegative attitudes, and historically less tolerant occupations show an improvement in economic performance, then, given all the available evidence, it is likely due to a shift towards a more inclusive society overall.

Chapter 3

Data

In this Chapter I first motivate and describe the homophobic search index (HSI) I construct to use as a proxy for implicit anti-gay sentiment. Next, I describe the variables used from the American Community Survey (ACS), which act as controls in most of my analysis. Finally, I describe the GDP statistics used, and explain the construction of a GDP variable that is the sum of the five North American Industry Classification System (NAICS) industries with the lowest share of women in 2000 by state.

3.1 Google trends: Homophobic search index

Most studies on the impact of same-sex marriage on public opinion rely on survey data. Nonetheless, as Coffman et al. (2017) show, it is likely that these measures underestimate the prevalence of homonegative attitudes. In a series of online experiments using a large and diverse but nonrepresentative sample, they compare population estimates from the standard methodology of asking sensitive questions

to measures from a “veiled” methodology. The veiled method increased self-reports of anti-gay sentiment. Particularly, in the workplace, respondents were 67 percent more likely to disapprove of an openly gay manager when asked with a veil, and 71 percent more likely to say it should be legal to discriminate in hiring on the basis of sexual orientation. Their results motivate the use of alternative methods to measure homonegative sentiment.

Google search queries are a great alternative, particularly, as holding a homonegative bias may be socially unacceptable and people are likely to self-censor with unveiled methods. As Stephens-Davidowitz (2014) puts it, google searchers are online and likely alone, circumstances that make it easier to express taboo thoughts.

Stephens-Davidowitz (2014) used Google searches to estimate the Barack Obama’s 2008 election costs of racial animus. Defining an area’s racially search rate as the percent of Google Searches, from 2004 to 2007, that included the word “nigger” or its plural as the main independent variable, and the comparison of Obama’s 2008 votes to that of John Kerry’s 2004, a white Democratic candidate, as the dependent variable, he estimates that if Obama were white or the whole country had the racial attitudes of the most tolerant areas, he would have gotten 4.2 percentage points more votes than he did.

Similarly, I construct a homophobic search index (HSI) at the state level using the search rate for the words “faggot”, “faggots”, “fag”, “fags”, as well as their misspellings, “fagot” and “fagots”⁶. In order to understand the search index, it is worth considering the way Google

⁶The motivation behind considering their misspellings is that they are related to homonegative sentiment. Google shows related queries to every given word. Consistently, “fagot” and “fagots” are related to “god hates fags”.

reports data.⁷

Search results are normalized to the time and location of a query by the following process: Each data point is divided by the total searches of the geography and time range it represents to compare relative popularity. Otherwise, places with the most search volume would always be ranked highest. The resulting numbers are then scaled on a range of 0 to 100 based on a topic’s proportion to all searches on all topics.

Complications arise as Google limits the number of geographical areas that can be compared at the same time to five. To elide this limitation, I consider the approach taken by [Paul Goldsmith-Pinkham \(2020\)](#). I obtain data for each geographical area plus Los Angeles and renormalize each area by $\text{maxIndex}_s / \text{maxIndex}_{LA}$.⁸ This way, all states are relative to L.A. and comparisons can be made across time and geos. Another limitation arises from Google’s lack of reporting below an unknown absolute threshold. I follow [Stephens-Davidowitz \(2014\)](#) algorithm to retrieve this data and adjust it accordingly. The exact process of the construction of the homophobic search index is shown in Appendix [A].

It is worth stressing that while Google Search indisputably receives the most daily number of queries than any other search engine in the U.S., its representativeness has not been the same throughout its history. For instance, the U.S. search index for “weather”, a fairly common search query, displays an upward trend from 2004 to date.

⁷ According to Google Trends Frequently Asked Questions. <https://trends.google.com/trends/>

⁸ In Appendix [B] I show how the results are not driven by the choice of the comparison group. At most, different comparison groups would yield different scales of the HSI.

This likely reflects the increase in access to the internet. In order to control for this trend, in every regression where a search index is the dependent variable, I include the search index for “weather” as a control.

3.2 American Community Survey

As noted by Stephens-Davidowitz (2014), an area’s racially charged search rate is correlated with the share of black population. He attributes this to the racial threat theory, which states that the presence of an out-group can threaten an in-group and create racial animosity. Even taking into account that LGB populations can be a fuzzy and anonymous minority, it is likely that the homophobic search index is correlated with the percentage of LGB population in a given area. Using administrative tax records, Fisher et al. (2018) provide the best information regarding LGB demographics. Nonetheless, they are only available after 2013.

A second-best alternative is the yearly American Community Survey, which is mandatory and hence, this approach significantly increases response and data quality. Here I follow Sansone (2019) and construct the state-level controls he uses to estimate the effect of same-sex marriage on the joint probability of same-sex couples of being employed. I use the ACS between 2000 and 2019⁹, which consists of 51,401,670 observations, to compute state-level variables, but keep only observations for individuals aged between 18 and 65 as well as on their married or unmarried partners, 18,861,875 observations,

⁹Sansone (2019) uses only data between 2000 and 2016. I consider three additional years to increase sample size.

whenever computing the share of same- and different-sex couples.¹⁰

Specifically, the state-level controls used through this paper are: i) *share of same-sex couples*; ii) *share of unmarried different-sex couples*; iii) *share of adults between 18 and 35 years old*; iv) *share of people with 4+ years of higher education*; v) *share of adults between 18 and 65 that are employed*; vi) *the average total pre-tax personal annual income or loses*; vii) *proportion of the population that is black*; and viii) *proportion of the population that Hispanic*. Every variable is computed by year and state.

Following Sansone (2019), and the ideas of Plug et al. (2014) and Black et al. (2007), gay men tend to concentrate in industries where the majority of workers are women, mainly, as they might face less discrimination. Linking this to Hsieh et al. (2019), there is probably a misallocation of resources in industries where most workers are men regarding gay men that would be more productive in these industries but crowd out due to likely higher levels of discrimination.

As to give an idea of in which industries the majority of workers are women and in which men, I compute the share of women by occupation according to the North American Industry Classification System (NAICS) by state, using data from the 2000 Census. I present in Table 3.1 NAICS industries with the top 5 and lowest 5 shares of women in the U.S. in 2000, and how these shares evolved through time. Interestingly, the five industries with the least share of women

¹⁰The age distribution of the whole ACS ranges from less than one year old to 97 years old. I consider that Sansone (2019)'s approach to using the whole dataset to compute state-level controls is correct, as to compute the proportion of the population that is black or Hispanic it is relevant to take into account the whole distribution. For variables where this would not make sense, like income, the distribution of age, filtering for positive values of income, ranges from 15 years old to 97 years old, which also seems appropriate.

remain unchanged from 2000 to 2016-2019.¹¹ The top five industries remain relatively stable. The only exception is “Management of Companies and Enterprises”.

Table 3.1. NAICS industries with the top 5 and lowest 5 shares of women in 2000. (U.S.)

NAICS Industry	Pre	Post
Health Care and Social Assistance	80.78	79.18
Educational Services	70.10	69.40
Finance and Insurance	64.11	56.90
Accommodation and Food Services	57.43	54.80
Management of Companies and Enterprises	55.46	53.52
Transportation and Warehousing	26.61	26.14
Utilities	22.96	22.87
Agriculture, Forestry, Fishing and Hunting	21.22	23.99
Mining, Quarrying, and Oil and Gas Extraction	12.69	15.16
Construction	10.10	10.92

Post uses information from the 2016-2019 ACS, when SSM was legal in every state.

Pre uses information from the 2000 Census, when none of the states had legalized SSM.

3.3 GDP

I download Gross Domestic Product (GDP) data from the U.S. Bureau of Economic Analysis (BEA).¹² for every year from 2000 to

¹¹In this analysis I do not consider the NAICS categories of “Unemployed” and “Active Duty Military” as they are unproductive sectors.

¹²<https://www.bea.gov/data/gdp/gdp-county-metro-and-other-areas>

2019. The GDP data is disaggregated by state, year and NAICS industry. I consider the chained GDP to focus on real changes.

$$GDP_{j,t}^{Last5_{j,2000}} = \sum_{k \in Last5_{j,2000}} GDP_{k,j,t} \quad (3.1)$$

Using the shares of women by industry and state in 2000, as these appear stable through time, I construct the variable $GDP_{j,t}^{Last5_{j,2000}}$ represented synthetically by Equation 3.1, which is the sum of the five industries k with the lowest share of women in 2000 by state j and year t . Recalling that the main driver of the economic growth caused by the declining obstacles to human capital accumulation and labor market discrimination for blacks and women from 1960 to 2010 in Hsieh et al. (2019) was the decrease in the misallocation of resources, the $GDP_{j,t}^{Last5_{j,2000}}$ would represent the most misallocated industries by state, with respect to gay men.

Chapter 4

Empirical strategy

Staggered rollout designs have been considered a more robust approach to a single *Difference-in-Differences* (DID) as the typical concern is that contemporaneous trends could confound the treatment effect, violating the main DID identification strategy, the existence of parallel trends. Prior work suggests that multiple treatment periods alleviate these concerns, and the standard approach has been to use Two-Way Fixed Effects (TWFE) to retrieve the treatment effects. According to de Chaisemartin and D'Haultfœuille (2020) 19 percent of all empirical articles published in the *American Economic Review* between 2010 and 2012 have used a TWFE regression to estimate the effect of a treatment on an outcome. However, this is a valid approach only when treatment effects are homogeneous across groups and time, which, for most applications, is implausible.

While this problem is relevant for any TWFE, it is particularly relevant for staggered rollout designs. Goodman-Bacon (2018) explains that the staggered DID using a Two-Way Fixed Effects regression is a weighted average of all possible two-group/two-period DIDs

estimators. The weights on the two-group/two-period DIDs are proportional to group sizes and the variance of the treatment dummy in each pair, which is highest for units treated in the middle of the panel. When treatment effects are constant over time, the TWFE yields a variance-weighted average of cross-group treatment effects and all weights are positive. He formalizes this idea by providing a decomposition using the weights of each two-group/two-period DID. He then makes clear that the problem with TWFE arises when *already-treated* units act as controls, as changes in their treatment effects over time get subtracted from the TWFE estimate.

Through some simulations, [Baker et al. \(2021\)](#) break down [Goodman-Bacon \(2018\)](#)'s findings even more. According to them, TWFE yields unbiased estimators *only* when the treatment effect does not vary in timing, or, when it does, when it is constant across groups, i.e., it is of the same magnitude. Whenever treatment effects vary over time and are heterogeneous across groups, the TWFE estimator is invalid. The bias is the most severe when treatment varies across time and groups, and the effect is dynamic through time.

The authors show that this problem can be so severe as to flip the sign of the estimates. For instance, [Beck et al. \(2010\)](#) analyze the effect of bank branching deregulation that occurred wholesale across the U.S. between the 1970s and the 1990s on income inequality using a TWFE approach. They find that income inequality *decreases* by boosting the incomes of the lower part of the income distribution. Using valid methods for inference when treatment effects are heterogeneous across time and groups, [Baker et al. \(2021\)](#) find that income inequality actually *increased* or stayed unmoved.

To avoid the problems with TWFE, I consider a stacked regression approach as well as the [Callaway and Sant'Anna \(2020\)](#) estimator.

[Baker et al. \(2021\)](#) show that both of these approaches yield unbiased estimates of the treatment effect even if the treatment varies across time, groups and is dynamic through time. My preferred method is the stacked regression approach as it is more flexible and allows for the inclusion of a greater number of controls than when considering [Callaway and Sant'Anna \(2020\)](#)'s doubly robust estimator. Both of these methods avoid using an *already treated* group as a control, which is the main concern with TWFE in a staggered rollout design.

4.1 Callaway and Sant'Anna (2020)

[Callaway and Sant'Anna \(2020\)](#) consider the problems with the TWFE in a staggered rollout design and propose an estimator that eludes them. In order to do this, they construct a disaggregated causal parameter which they call “the group-time average treatment effect, i.e., the average treatment effect for group g at time t , where a “group” is defined by the time period when units are first treated”. In the canonical DID setup with two periods and two groups, these parameters reduce to the Average Treatment of the Treated (ATT).

By estimating the ATT for each group and time period, they do not directly restrict heterogeneity with respect to the period in which units are first treated, or the evolution of treatment effects over time. Their proposed parameter allows for aggregation schemes that deliver a single overall treatment effect parameter such as: i) how average treatment effects vary with length of exposure to the treatment (event-study-type estimands); and ii) how average treatment effects vary across treatment groups.

4.2 Stacked regression

Following [Cengiz et al. (2019)] and [Baker et al. (2021)] I create event-specific datasets (for every year one or many states legalize SSM), including the outcome variable and controls for the treated state and all other states up to when they become treated. Thus, I am only including the *untreated* observations for the states not in that cohort. This way, I have only “clean controls” for every group. I then stack these event-specific datasets in relative time to calculate the event-studies relative to the reference period, which is the treatment year.¹³ According to [Baker et al. (2021)], by stacking and aligning events in event-time, this approach is equivalent to a setting where the events happen contemporaneously, and it prevents using past treated units as controls, which may occur with a staggered design.

After constructing the stacked dataset, I consider event-study models of the form given by 4.1. An advantage of event-study functional forms is that they help evaluate the credibility of the parallel trends assumption. As [Baker et al. (2021)] notes, the only difference in terms of functional form regarding a simple event-study regression is the need to saturate the group and time fixed effects with indicators for each event-specific dataset. I restrict the time window of the event study to $[-5, 1]$ and $[1, 10]$. One main difference between the stacked regression event-studies and a simple event-studies is that in the former, there is no need to include the full set of relative time indicators, as it is the case with the latter. (Baker et al., 2021)

¹³For every stacked regression, I cluster standard errors at the event-specific (or cohort) level.

$$y_{j,t,G} = \gamma_{j,G} + \tau_{t,G} + \sum_{k \neq 0} \delta_k \mathbb{I}[t - G_j = k] + \varepsilon_{j,t,G} \quad (4.1)$$

In Model 4.1, $\gamma_{j,g}$ and $\tau_{t,g}$ are the individual and time saturated fixed effects. $\mathbb{I}[t - G_j = k]$ is an indicator for being k years from the treatment starting for cohort G . G_j simply states that state j was treated with cohort G .

$$\log(HSI)_{j,t,G} = \gamma_{j,G} + \tau_{t,G} + \bar{X}'_{j,t,G} + \sum_{k \neq 0} \delta_k \mathbb{I}[t - G_j = k] + \varepsilon_{j,t,G} \quad (4.2)$$

In particular, Model 4.2 is the specification I use to test for the effect of same-sex marriage on the homonegative implicit bias. I consider the natural logarithm as the dependent variable as to have a cleaner interpretation of the results. $\bar{X}'_{j,t,G}$ is the set of state-year specific controls described in Chapter 3, plus the constructed search index for “weather”.

$$\log(GDP_{j,t,G}^{Last5_{j,2000}}) = \gamma_{j,G} + \tau_{t,G} + \bar{X}_{j,t,G} + \sum_{k \neq 0} \delta_k \mathbb{I}[t - G_j = k] + \varepsilon_{j,t,G} \quad (4.3)$$

Model 4.3 is the specification I use to test, indirectly, if same-sex marriage had an effect on economic growth, considering the channels laid by Hsieh et al. (2019). Recall that $GDP_{j,t}^{Last5_{j,2000}}$ is the sum of the five NAICS industries with the lowest share of women in 2000 by state j and year t . I consider its log to have an approximate percentage change interpretation of the results. $\bar{X}_{j,t,G}$ only includes the set of controls described in Chapter 3.

Chapter 5

Results

The two main research questions of this investigation are: i) What is the effect of SSM legalization on social norms and discrimination towards sexual minorities?; and ii) If discrimination was affected by SSM legalization, what was the effect on the economy? Regarding the first question, I find suggestive evidence that the effect of same-sex marriage on sexual minority discrimination is not homogeneous across its legalization method. In short, states that legalized it through the judiciary seem to have become more homophobic relative to not having legalized SSM through a court ruling. On the other hand, the opposite appears to have happened in states that legalized it through the legislature. As per the second question, I find that the patterns flip, considering the GDP of the five industries that have the least share of women by state. At the same time, in the states where same-sex marriage was legalized through the judiciary, gay men become even more likely than their heterosexual counterparts to work in industries where the majority of workers are women. Both of these findings are suggestive evidence that a greater increase in

homonegative attitudes led to a greater misallocation of resources and this in turn to lower performance in these selected industries.

5.1 Changes in public opinion

Figure 5.1 shows the event-studies by legalization method of same-sex marriage of the log of the constructed homophobic search index. The left panel considers as treated units only states where it was legalized through a court decision. Recalling the Bishin et al. (2016) definition of opinion backlash: “a large, negative, and enduring shift in opinion against a policy or group that occurs in response to some event that threatens the status quo”, the plotted coefficients show evidence for it.

The first thing to note from Figure 5.1 is that the parallel trends assumption seems to hold. The lead point estimates do not follow a clear upward or downward trend, and each coefficient is not statistically different from zero. Assuming this is the case, the HSI increased 24.08 percent 8 years after SSM was legalized via the judiciary, had SSM not been legalized that way. Under the same assumptions, 9 and 10 years later, this search index is 36.06 and 35 percent higher than the year SSM was legalized, respectively.¹⁴

¹⁴ Considering a log transformation of a variable as the dependent variable in an Ordinary Least Squares regression yields a coefficient with an interpretation of an approximate percentage change. The correct percentage change is obtained using the formula $\% \Delta y = 100(e^\beta - 1)$ where y is the non-transformed dependent variable and β the coefficient of interest. I obtain the percentage change this way for all the results presented non-graphically.

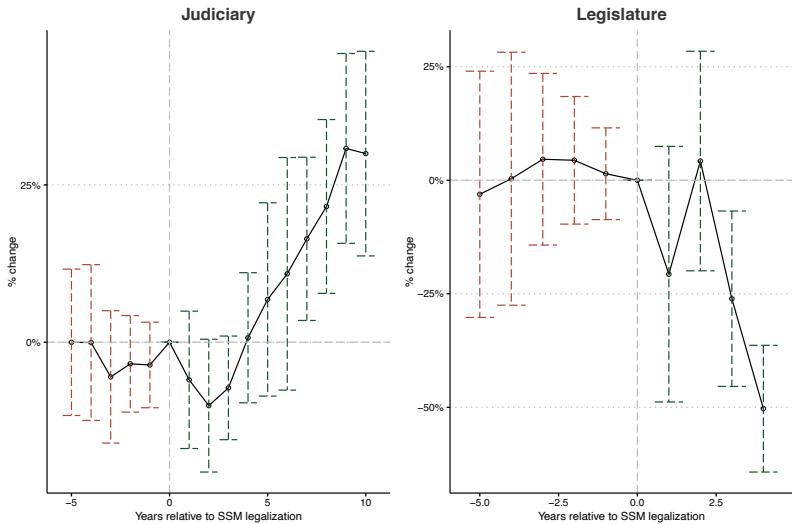


Figure 5.1. Event study by legalization method using stacked regressions. $\log(HSI)$ as dependent variable.

The opposite is true for the states that legalized SSM through the legislature, but the change seems to happen at a faster rate. Again, assuming that indeed the parallel trends assumption holds, which is plausible in this case too as all the pre-period coefficients are not statistically significant different from zero, the homophobic search index is 14.52 and 25 percent lower on average, 3 and 4 years after SSM legalization, than it would have been had SSM not been legalized through the legislature. The rest of the lag coefficients are collinear with the saturated fixed effects. Nonetheless, Figure 5.2 shows the Callaway and Sant'Anna (2020) doubly robust estimator equivalent to Figure 5.1, without considering any covariates. Even though most of the coefficients are not statistically significant different from zero, the point estimates show a clear downward trend. This, then, is suggestive evidence that states that legalized same-sex marriage through the

legislature had a positive shift in their social norms, which in turn made discrimination against sexual minorities less likely, as proxied through the homophobic search index.

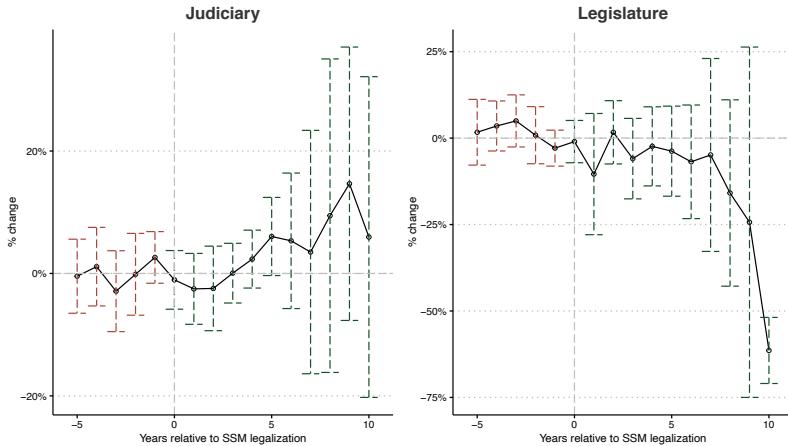


Figure 5.2. Event study by legalization method using Callaway and Sant'Anna (2020). $\log(HSI)$ as dependent variable.

A major advantage of testing changes in homonegative attitudes using the HSI is that I consider this measure for every U.S. state from 2004 to 2019, and thus, the results have more external validity than if I had considered only one state or just one cohort. In contrast, Kreitzer et al. (2014) focus only on Iowa's 2009 SSM legalization. Flores and Barclay (2016) focus only on Maine and Washington's SSM legalization via the legislature.¹⁵ As stated before, even prior studies like Bishin et al. (2016) that consider more states, are not suited to test if SSM legalization caused an *enduring* shift in social norms. The authors' results rely on survey data from 41 days, and SSM was not even legalized

¹⁵It is worth noting that their results showing a decrease in homonegative attitudes are in line with the results shown in this paper using the HSI.

in any state within the time frame they study.

[Ofosu et al. (2019)] do test for changes in homonegative attitudes for every state between 2005 and 2016. The authors find qualitatively similar results with data from the Implicit Association Test (IAT): legalization via the judiciary prompts backlash, whereas via the legislature antigay bias decreases. The results in Figure [5.1] reinforce this finding. Even so, one main advantage of using the HSI instead of data from the IAT is that most of the respondents of this test are young women, so the results might not be representative. Google searches might suffer from the same concern of nonrepresentativeness, but it might be a lesser concern given the volume of search queries.

Independent of the relevance of these results on economic outcomes, which will be studied in Subsection [5.2], I consider they shed light on the Constitutional Law debate regarding the unintended consequences of judicial activism. According to [Siegel (2017)], opponents of SSM in the U.S. argued that even if there was a case for recognizing it, in a constitutional democracy that question is up for determination by legislatures, not courts. On this note, in the landmark SCOTUS case *Obergefell v. Hodges*, Chief Justice Roberts insisted that SSM had an undeniable appeal, but argued against making it the law of the land through a ruling as the SCOTUS is not a legislature.

Professor Siegel has made one of the most convincing cases for the irrelevance of the backlash question. She argues that the backlash framework is misguided as it evaluates these judicial decisions within a short time horizon, but when analyzing a longer time horizon the “constitutional understanding” of these issues shifts and hence what is labeled as backlash in the short term, in the longer term is just part of the transformation of the “constitutional culture”. For instance, SSM is now outside the scope of the public debate but other considerations

such as denial of private services to LGBTQ populations based on religious freedoms have taken its place. While this may be an interesting claim regarding the nuances of the social norm shifts, it does not relate to what most of the literature understands as backlash. Under Bishin et al. (2016)'s definition, which is testable, Figure 5.1 results counterargue Siegel's claims of a lack of backlash.

Making a weak case for the external validity of these results, let us consider the Colombian case. The Colombian Constitutional Court (CCC) might be, to my knowledge, one of the most active constitutional courts in the world advancing in the recognition of rights of sexual minorities. Andrade-Rivas and Romero (2017) mention that as of 2013, the CCC had issued 70 rulings in favor of freedom of expression and personal development with regard to LGBTQ rights, as well as had both legalized same-sex unions and their right to adopt by 2015. Bocanumenth (2021) reports how even though on paper the country has one of the strongest legal frameworks in Latin America defending the rights of LGBTQ populations (mostly due to judicial decisions), in practice these protections are rarely enforced. This lack of effective protections might reflect Baca et al. (2019)'s findings that out of nine countries in Latin America and the Caribbean, Colombia registered the highest number of killings of LGBTQ people over 2014 – 2019.

Normatively, I cannot be more certain that sexual minorities ought to have equal rights. However, the road towards this *ought to be* state of the world might be smoother when guided by positive findings. If there is empirical evidence suggesting that SSM legalization via the judiciary might be counterproductive, instead of turning a blind eye on it, it begs the question of what are the underlying mechanisms explaining this result. Then, more research following this line of thought would not only

be interesting, but useful in shedding light on how to advance towards a more equal society.

5.2 Economic outcomes

In order to analyze the (potential) effects of same-sex marriage legalization on economic performance, I replicate one of Black et al. (2007) findings: that male workers in same-sex couples concentrate in occupations with a higher share of women relative to men in different-sex couples. Using data from the 2000 Census, that reflects a point in time when SSM was illegal in every U.S. state, and data for the 2016-2019 ACS, that reflects a point in time when SSM was legal in every U.S. state, I estimate the Linear Probability Model expressed by 5.1 where $\mathbb{I}_{NAICS_i^{>50\%women}}$ is an indicator variable that activates when the NAICS industry where individual i works is conformed majoritarily by women. \mathbb{I}_{Gay_i} is an indicator variable that activates if individual i is in a same-sex couple relationship; and $\mathbb{I}_{Judiciary_i}$ is an indicator that activates if the SSM legalization method in state where individual i lives was through the judiciary.

$$\mathbb{I}_{NAICS_i^{>50\%women}} = \beta_0 + \beta_1 \mathbb{I}_{Gay_i} + \beta_2 \mathbb{I}_{Gay_i} \times \mathbb{I}_{Judiciary_i} + \varepsilon_i \quad (5.1)$$

Table 5.1 shows the results for the LPM using robust standard errors. According to these results, men in a same-sex couple that lived in a state that legalized SSM through the legislature were 12 percentage points more likely than men in a different-sex couple to work in an industry where the majority of workers are women in 2000, and 11.6 percentage points more likely to work in these industries in 2016-2019 (using industry shares from this period), so their likelihood decreased by

0.4 percentage points. This contrasts that, in states where the method of SSM legalization was through the judiciary, their likelihood to work in an industry where women are a majority, relative to men in different-sex couples, *increased* by 2.4 percentage points considering the same time-frame.

Table 5.1. Increased likelihood of gay men's working in NAICS industries where the majority workers are women.

	Pre	Post
(Intercept)	0.321*** (0.001)	0.346*** (0.001)
Gay men indicator	0.120*** (0.004)	0.116*** (0.004)
Judiciary indicator	-0.029*** (0.001)	-0.010*** (0.001)
Gay men x Judiciary	0.010** (0.004)	0.018*** (0.005)
Num.Obs.	2340145	1515498

Post uses the same information, but considers the 2016-2019 ACS.

Pre uses information for men that are reported as in a relationship in the 2000 Census.

Robust standard errors in parentheses.

The fact that post SSM legalization men in same-sex couples concentrate more in industries where the majority of workers are women in the states that legalized through the judiciary is indicative of a higher degree of discrimination, as research shows that they tend to work in less stigmatizing industries. This hypothesis is tested more

formally using the variable $GDP_{j,t}^{Last5j,2000}$ described in Chapter 3 through a DID analysis, using the Model 4.3. The event-study coefficients of this model are plotted by legalization method in Figure 5.3.

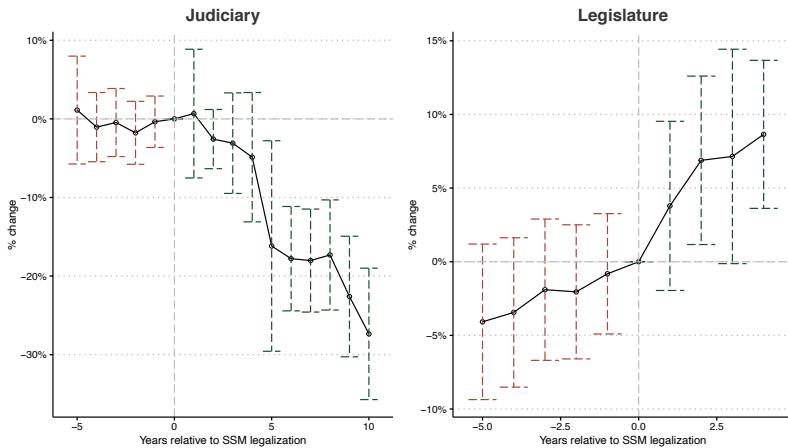


Figure 5.3. Event study by legalization method using stacked regressions. $\log(GDP_{j,t}^{Last5j,2000})$ as dependent variable.

In regard to states that legalized SSM through a court decision, and assuming again that there are parallel trends, Figure 5.3 states that relative to the reference period, the NAICS industries with the lowest share of women by state, decreased chained GDP by 23.93 percent ten years after SSM legalization through the judiciary, had it not legalized it that way. While the states that legalized through the legislature exhibit the opposite pattern, the parallel trends assumption seems more implausible to hold as the point estimates exhibit an upward trend, even if they are not statistically significant.

Focusing on the judiciary case, the results of the Model 4.3 show compelling evidence for the existence of a decrease in economic

performance due to increased discrimination of sexual minorities after SSM legalization through a court decision. To set things in context, consider that from the 42 percent growth in U.S. GDP per person between 1960 and 2010 because of declining obstacles to accumulation of human capital and labor market discrimination, 36 percent were due to the former, while only 8 percent due to the latter. (Hsieh et al., 2019)

Assuming that the compositional effect of declining obstacles to accumulation in human capital and labor market discrimination on economic performance from Hsieh et al. (2019), are similar to those regarding sexual minorities, it is unlikely that ten years from the legalization of SSM obstacles to accumulations in human capital of LGBTQ populations have declined considerably. Consequently, the channel that likely explains the results from Figure 5.3 is labor market discrimination. Moreover, the results from the LPM shown in Table 5.1 are in line with this hypothesis.

On that note, it is important to consider that the case for sexual minorities is likely to be different to Hsieh et al. (2019). The main difference, is that the authors do not identify a single event that drives their findings, so it is likely that their results are not as concentrated as the ones for sexual minorities appear to be. Additionally, they are considering the effect in the whole U.S. economy. Here I am concentrating in the industries where the effect is more likely to be readily apparent.

5.3 Robustness checks

At the beginning of this research I hypothesized that I would be able to use a version of the racial animus search index constructed by Stephens-

Davidowitz (2014) as a placebo test. As it was in the research plan, I show the results in Figure 5.4, again, by SSM legalization method. Shockingly, the coefficients display a similar pattern than those using the homophobic search index.

Does this mean that the results from Figure 5.1 are not robust? Not quite, as this is not really a placebo test because there is no reason to think that racial attitudes are independent of social norm shifts regarding sexual minorities. Mainly, as it is highly probable that if someone is homophobic, they are also racist. Also, the definition of opinion backlash used throughout the paper refers as its cause the threatening of the *status quo*. In this sense, as both blacks and sexual minorities are not part of the *status quo*, both of these minority groups represent a threat to it. These results point to an interesting area of future research regarding intersectionality.

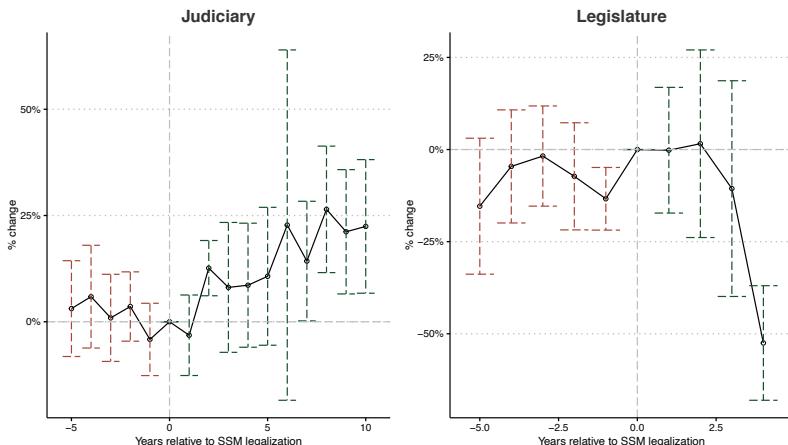


Figure 5.4. Event study by legalization method using stacked regressions. Log of racial animus index as dependent variable.

Instead of the placebo test using Stephens-Davidowitz (2014) racial

animus search index, I consider Cunningham (2021)'s comment that specific patterns in the data can be derived by particular controls included in the main specification. In order to test if the results shown in Figures 5.1 and 5.3 are robust to different sets of controls, I run the same calculations removing, with replacement, one control at a time. I focus on the states that legalized SSM through the judiciary. The results are shown in Appendix C. It is reassuring that every plot shows the same pattern as those using the full set of controls.

Chapter 6

Concluding remarks

Do all same-sex rights are created equal? My research, using a measure of implicit bias for anti-gay attitudes, suggests that this is not the case. The literature of public opinion changes due to SSM remains far from settled. I hypothesize that this is mainly due to the lack of considering heterogeneities. More recent research does place more weight on these potentially different effects via method of legalization. Aksoy et al. (2020), published the most recent study on the topic, contributing to bridging this gap as the authors' main focus is to test for differentiated effects of SSM across demographies. Having a better understanding of the different channels through which social norms shift, means more and better tools to keep moving forward. Normatively, one can agree that more equal rights for sexual minorities imply a Pareto improvement in society. The small print is that there are more and less effective ways to get there, whilst some may end up being counterproductive. Even more so, as this research provides suggestive evidence that some of these ways have real effects on the economy.

A note of caution. It is easy to make generalizations whether some

policies are “good” or “bad” using reduced form estimates, as the ones used in this paper. However, it is important to emphasize that these are only partial equilibrium findings. For instance, Seror and Ticku (2021) find that the effect of SSM on enrollment in priestly studies fell after legalization of SSM for both methods of legalization. The decrease in enrollment is actually higher for states that legalized it through a court order. The authors attribute these findings to a decrease in the cost of coming out, hence, making marginal enrollees better off. With this in mind, the net effect of SSM on sexual minorities’ well-being in states that legalized it through the judiciary is not clear.

More and better research regarding minorities help us build a fairer and more equal society. This research is my attempt to contribute towards this goal.

Appendix A

Google Trends estimation for non-reported values

Google Trends is a great source for retrieving high quality data that, in my view, has been underused for social science research. It provides insights that survey data are not likely to deliver. Most surely, if the research topic carries some taboo weight. Still, its main downside is Google's unreported absolute threshold that, when crossed, yields zeros in its index.

Using Stephens-Davidowitz (2014) algorithm idea of considering an auxiliary search term that most likely yields an index above the threshold such as “weather” or “food”, the threshold problem can be overcome almost entirely.

The idea is that if term “ M ” does not cross the threshold for many observations, one can search for “ $M + A$ ”, where “ A ” is the auxiliary term to get above this threshold. Then, if the raw volume search were provided, the solution would be to simply subtract “ A ” from “ $M + A$ ”.¹⁶

¹⁶For details, see Stephens-Davidowitz (2014).

As this is not the case, complications arise from sampling, rounding and normalizing. The first two are solved by constantly retrieving samples.

In order to get a normalized search index that is comparable across time and geos I obtain data for each geographical area plus Los Angeles (L.A.) and renormalize each area by $\maxIndex_s / \maxIndex_{LA}$, as suggested by [Paul Goldsmith-Pinkham (2020)]. The reason I consider L.A. is that without renormalizing, it was the geo I found to constantly be the maximum for “faggot(s)” and “weather”, relative to every other geo¹⁷, and hence, [Stephens-Davidowitz (2014)]’s algorithm is directly applicable.

Once the index is scaled, I compute Model A.1, where B is the set that contains positive values for all three queries but below the maximum, 100. γ_g is a geo fixed effect. In the results from regression A.1, I get that α_0 is positive and α_1 is negative, which is consistent with the idea of subtraction mentioned above. I then use these coefficients to predict the observations that have zeros reported.

It is worth noting, that the share of the predicted observations decreases as the sample size increases. As of the closing of this document, the share of predicted geo-month observations and number of samples used is 6.9 percent and 39, respectively. The analysis conducted throughout this paper was at the geo-year level.

$$M_{b \in B} = \gamma_g + \alpha_0(M + A)_{b \in B} + \alpha_1 A_{b \in B} + \varepsilon \quad (\text{A.1})$$

¹⁷This would be helpful if in the future the analysis is expanded to the Digital Market Areas (DMA) level.

Appendix B

Alternative homophobic search indexes

The homophobic search index I consider throughout this paper consists of the words “faggot”, “faggots”, “fag”, “fags”, as well as their misspellings, “fagot” and “fagots” and is renormalized considering Los Angeles (L.A.) as a comparison group. This approach may arise two concerns. First, L.A. might be different from the rest of the country and then using it as a comparison might introduce noise to the search index. Second, the words chosen for the queries might not be a good proxy for homonegative sentiment.

Google Trends data are normalized to the times and locations of a query, so using different geographical areas as comparisons would, at most, change the scale of the search index but not the general patterns in the data. As explained in Appendix A the reason why I consider L.A. as a comparison group is that, without renormalizing, it was the most appropriate geographical area to use Stephens-Davidowitz (2014)’s algorithm to obtain data at the Digital Market Area (DMA) level. None

of the analyses made throughout the paper are at the DMA level. Still, as Google only provides samples of search queries which are constantly changing, considering a greater number of samples do improve data quality. As of the closing of this document, I had gathered 39 samples using L.A. as a comparison group.

Nonetheless, to show how the results would not have changed had I considered a different geographical area I show in Figure B.1 the event study for a homophobic search index considering the U.S. as a comparison group using 2 query samples. Figure B.1 exhibits quite similar results to those shown using the HSI with L.A. as a comparison group. The differences are probably due to the difference in the number of samples used in the construction of each search index.

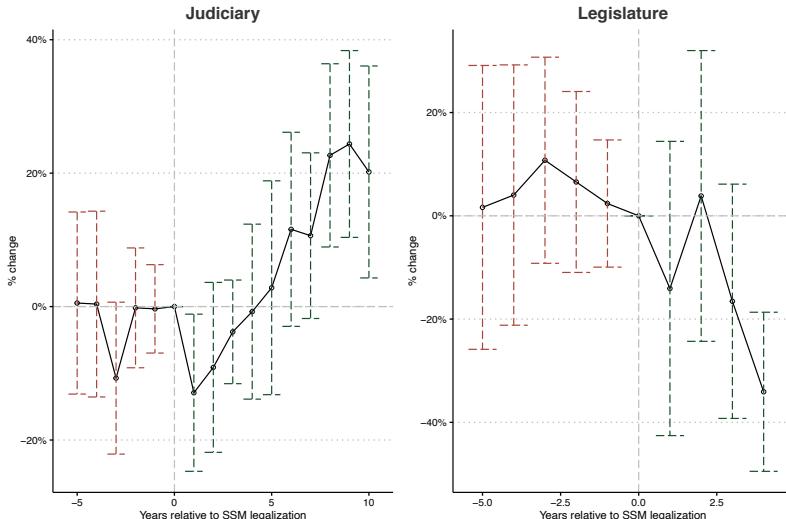


Figure B.1. Event study by legalization method using stacked regressions. $\log(HSI')$ as dependent variable, where HSI' uses the U.S. as the comparison group.

Regarding the second concern, that the words considered in the construction of the homophobic search index might not be the best proxy for homonegative sentiment. Figure B.2 shows the event study for a homophobic search index using the words “leviticus”, “sodomy” and “faggot” using 2 query samples. Sansone (2019) uses these words to construct a search index and make a similar analysis than the one presented in this paper: to analyze if homonegative attitudes changed after SSM legalization. The results considering this alternative HSI are similar to those using the HSI I construct. The main difference is that there seems to be a downward trend in homonegative sentiment before SSM legalization. The upward trend afterwards is in line with the backlash thesis. Following, Stephens-Davidowitz (2014), the reason why I do not consider a search index consisting of different words is to avoid data mining.

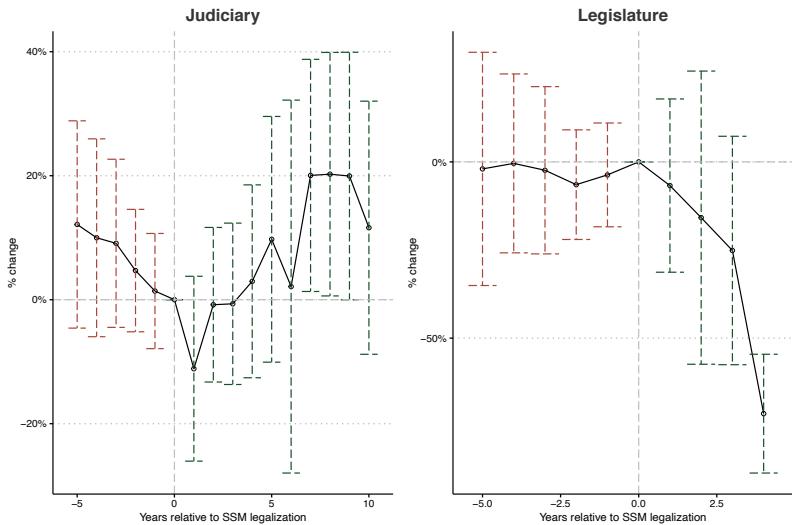


Figure B.2. Event study by legalization method using stacked regressions. $\log(HSI'')$ as dependent variable, where HSI'' consists of the words “leviticus” + “sodomy” + “faggot”.

Appendix C

Additional figures and tables

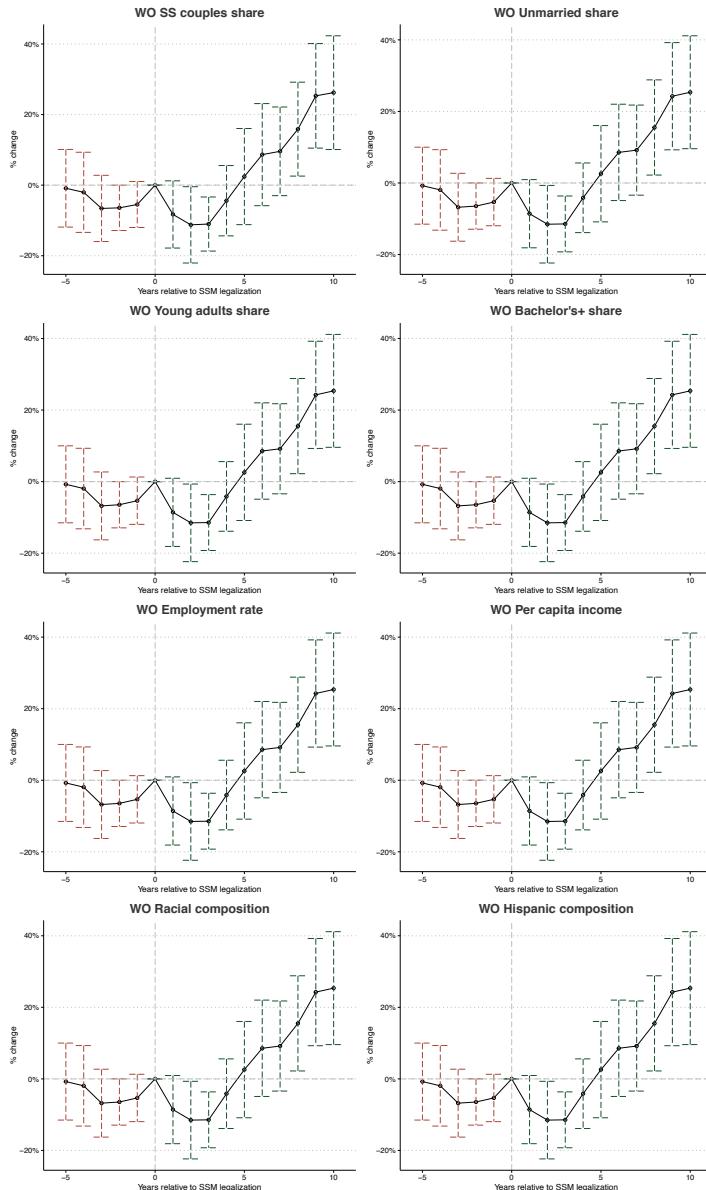


Figure C.1. $\log(HSI)$ event studies relative to SSM through the judiciary. Removing covariates with replacement.

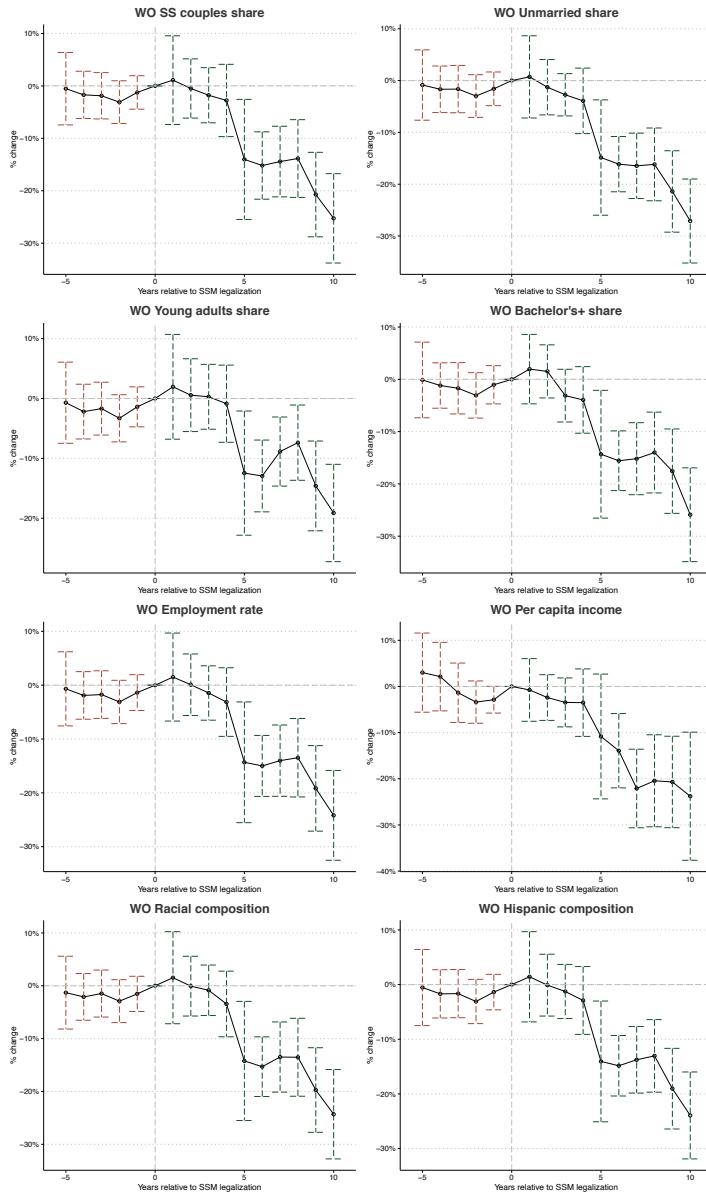


Figure C.2. $\log(GDP_{j,t}^{Last5j,2000})$ event studies relative to SSM through the judiciary. Removing covariates with replacement.

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