The impact of sodomy law repeals on crime

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Abstract

We exploit variations in the timing of decriminalization of same-sex sexual inter-course across US states to estimate the impact of these law changes on crime through difference-in-differences and event study models. We provide the first evidence that sodomy law repeals led to a decline in the number of arrests for disorderly conduct, prostitution, and other sex offenses. Moreover, in line with the hypothesis that sodomy law repeals enhanced mental health and lessened minority stress, we show that these repeals led to a reduction in arrests for drug and alcohol consumption.

Keywords Sodomy laws · LGBTQ+ · Crime

JEL Classification I18 · J15 · K14 · K38

1 Introduction

The sexual acts indicated as *sodomy* historically referred to both oral and anal sex, as well as bestiality (Merriam-Webster 2022). Sodomy laws are laws that criminal-ize these specific sexual activities. American colonies inherited these laws from the British Empire: sodomy was a crime punishable by death in most American colonies. Even after the US Declaration of Independence and throughout the nineteenth cen-tury, sodomy was a crime often punishable by a life sentence. These laws were used in modern US history to specifically target and persecute LGBTQ+ individuals.[1]

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[1] In this paper, we use the acronym LGBTQ+ to include lesbian, gay, bisexual, transgender, queer indi-viduals, and other sexual and gender minorities. The term sexual minority refers to individuals with same-sex attraction, same-sex sexual activity, and/or those who identify and use terms such as lesbian, gay, bisexual, or queer. Gender minorities are individuals whose current gender does not match their sex at birth. Cisgender individuals are people whose current gender aligns with their sex at birth.

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The years after WWI were characterized by a real "gay panic": a widespread belief that homosexuals were sexual predators targeting children and susceptible young adults to make them gay. The legal and social environment remained hostile even after WWII: between 6600 and 21,600 people—mostly men—are estimated to have been arrested each year from 1946 to 1961 for non-conforming gender or sexual behaviors. In the same period, tens of thousands of LGBTQ+ individuals were detained, blackmailed, or harassed by police officers (Eskridge 2008). In addition, sodomy laws were used against sexual minorities to limit their rights to adopt or raise children, to justify firing them, and to exclude them from hate-crime laws (ACLU 2019). Even in the 1990s and early 2000s, before the US Supreme Court deemed sodomy laws unconstitutional in 2003 (*Lawrence v. Texas*), the penalty for violating sodomy laws ranged from a $500 fine in Texas to a maximum life sentence in Idaho (GLAPN 2007).

This paper exploits exogenous variations in the timing of decriminalization of same-sex sexual intercourse across the US states to explore the effect of sodomy laws on crime rates. Specifically, it makes use of difference-in-differences and event study techniques to study whether sodomy law repeals affected the number of arrests for sex offenses, prostitution, disorderly conduct, and driving under the influence. By doing so, this article extends an extremely limited literature on sodomy laws not only in economics and political science but also in public health and other social sciences. A few studies have looked at the determinants of sodomy laws (Frank, Camp, and Boutcher 2010; Asal, Sommer, and Harwood 2013; Chang 2021) or at the effect of legalizing homosexuality across countries on attitudes toward sexual minorities (Kenny and Patel 2017). Yet, to the best of our knowledge, there is no study specifically looking at the impact of sodomy laws on crime or arrest rates.

Our identification assumption relies on the staggered implementation of these sodomy law repeals: before the US Supreme Court ruling in 2003, 36 states plus the District of Columbia had legalized same-sex sexual acts in their jurisdictions. Using data from the 1995–2018 Uniform Crime Reporting Program arrest database, we provide the first evidence that the elimination of sodomy laws led to a persistent decline in the number of arrests for disorderly conduct, prostitution, and other sex offenses.[2]

In addition, in line with the hypothesis that these law changes improved mental health for LGBTQ+ individuals, reduced minority stress (Meyer 1995), and led to a reduction of substance abuse as a coping mechanism, we report a reduction in arrests for drug and alcohol consumption following sodomy law repeals.

We deem the findings in this paper to be paramount for policymakers. While we acknowledge the limitations of our analysis in terms of external validity, we do believe this manuscript is of interest to practitioners and government officials since

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[2] It is worth emphasizing that the available data only allow us to estimate the impact of these law changes on arrest rates. These rates are a close proxy for criminal activity rates, but are not a perfect substitute for actual (unobserved) crime data. Relatedly, a reduction in arrest for sex offenses after the decriminalization of same-sex sexual intercourse does not imply that the actual rate of same-sex sexual intercourse decreased.

it helps international institutions such as the World Bank or the European Union in evaluating more accurately the costs and benefits of pressuring or suspending foreign aids to countries in blatant violation of basic LGBTQ+ rights. Indeed, there are several countries in the world in which same-sex sexual activity is still illegal (ILGA 2020), and some countries are passing laws explicitly targeting LGBTQ+ individuals (Economist 2014; Steer 2018). International institutions have to therefore balance the need to advance economic development without undue political interferences with the need to protect their employees working in those countries and not to foster human right violations. Our research highlights additional costs related to the enactment and enforcement of anti-LGBTQ laws such as sodomy laws, providing another valuable piece of information to guide international decision-makers. Likewise, this analysis points out to policymakers in countries that still persecute homosexuals the potential benefits from decriminalizing same-sex sexual intercourse.

This paper is organized as follows. Section 2 highlights our contribution to the literatures on LGBTQ+ issues and crime economics. Section 3 provides more detail on the institutional and historical background around the decriminalization of sodomy in the US Section 4 builds a conceptual framework to clarify the underlying mechanisms hypothesized to link the legalization of same-sex sexual acts with changes in arrest rates for specific crimes. Section 5 describes the data and the empirical methodology. Section 6 presents the econometric estimates, and Section 7 concludes.

2 Literature review

This paper contributes to two fields. First, within the literature on sexual minorities (Badgett, Carpenter, and Sansone 2021), this analysis is related to a growing number of studies estimating the impact of LGBTQ+ policies such as anti-discrimination laws and same-sex marriage legalization on socio-economic variables including health, migration, and labor market outcomes (Dee 2008; Francis, Mialon, and Peng 2012; Burn 2018; Carpenter, Eppink, et al. 2021a, b; Sansone 2019; Mann 2022; Aksoy, Carpenter, and Sansone 2022; Marcén and Morales 2022).[3] Moreover, there are a few studies specifically looking at sodomy laws that are closely related to our research question. For instance, Chang (2021) found that countries with high male-female sex ratios were less likely to decriminalize same-sex sexual activities, possible due to the fact that men are on average less tolerant than women with regards to homosexuality. Kenny and Patel (2017)—as well as Asal, Sommer, and Harwood (2013)—emphasized instead the role played by legal institutions: that is, the higher probability of having sodomy laws in countries that were formerly part of the British Empire. Kenny and Patel (2017) provided then evidence that the repeal of sodomy laws was correlated with changes in attitudes towards sexual minorities.

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[3] The importance of conducting research in this field have been highlighted in the literature in the last decades by, among others, Badgett (1995, 1998) and Lamos (1995).

Despite these studies, it is still not clear what the effects of the decriminalization of same-sex sexual activities have been on individual socio-economic and health outcomes such as labor market participation and employment rates among sexual minorities, the incidence of sexually transmitted diseases, mental health, and suicide rates. We therefore contribute to this literature by specifically analyzing the relationship between sodomy law repeals and arrest rates for crimes directly or indirectly linked to same-sex sexual acts. By doing so, we focus our attention on the specific microeconomic effects of sodomy law repeals on police behavior, arrest rates, and mental health outcomes rather than looking at the institutional or demographic factors leading a country to enact sodomy laws.

Second, this paper adds to a strand of the literature in crime economics exploring the effect of family and vice laws (Stevenson and Wolfers 2006; Cáceres-Delpiano and Giolito 2012; Heaton 2012). For instance, Stevenson and Wolfers (2006) and Cáceres-Delpiano and Giolito (2012) explored the effect of unilateral divorce laws on felonies ranging from domestic violence to sex offenses. Joining the findings of both paper, results are mixed: unilateral divorce leads to a decay in domestic violence but boosts other violent crimes. Relatedly, Heaton (2012) analyzed the impact of relaxing Sunday alcohol law restrictions on crime. The author found that removing such laws would have increased crime by different amounts depending on the offense. We extend this literature by analyzing the effects on crime of another kind of family and vice laws—that is, sodomy laws—which had usually been enacted on moral and religious grounds.

Furthermore, inside the field of crime economics, this paper is connected to a recent branch of empirical studies analyzing sex crimes and prostitution (Cunningham and Kendall 2011a, b; Bhuller et al. 2013; Bisschop, Kastoryano, and van der Klaauw 2017; Cunningham and Shah 2018; Ciacci and Sviatschi 2022; Ciacci 2023; Stadtmann and Sonnabend 2019; Cameron, Seager, and Shah 2021; Ciacci 2018; Cunningham and Shah 2022; Ciacci 2021), including sex work among trans people (D'Ippoliti and Botti 2017). The main results of this current line of research are that decriminalizing prostitution decreases rape cases, while criminalizing prostitution—either by criminalizing its sale or its purchase—leads to an increment in rapes. A thorough review of the key findings in this literature can be found in Cunningham and Shah (2022). Our study contributes to this literature by showing how the repeal of laws used to criminalized same-sex sexual activity and to persecute sexual minorities led to a reduction in arrests for sex crimes and prostitution, thus highlighting the importance of a policy ignored in the literature so far.

To the best of our knowledge, this manuscript is one of the first papers to link LGBTQ+ rights and crime, thus adding to the studies by Nikolaou (2022) and Sansone (2019) analyzing the impact of same-sex marriage legalization on hate crimes. In this way, it is one of the first studies to estimate the effect of LGBTQ+ policies and laws on crime in general and arrest rates in particular. With this aim in mind, our analysis is built on previous studies looking at victimization and incarceration rates among sexual and gender minorities (Flores et al. 2020, 2021; Meyer et al. 2017; MAP 2016). In particular, Meyer et al. (2017) noted that the rate of incarceration for sexual minorities was approximately three times higher than the general US incarceration rate. In addition, a report by MAP (2016) emphasized that LGBT people

of color were even more likely to be overrepresented in the criminal justice system. Our paper complements these previous findings by emphasizing the role played by an LGBTQ+ policy in reducing arrest rates for crimes directly or indirectly related to same-sex sexual activity.

More generally, this paper provides a new and important contribution to the literature on the economic effects of civil and social right reforms affecting stigmatized and marginalized populations such as the Civil Right Act (J. J. I. Donohue and Heckman 1991; Hersch and Shinall 2015), the legalization of interracial marriage (Fryer 2007), the Americans with Disabilities Act (Acemoglu and Angrist 2001; Hotchkiss 2004), abortion and family-planning reforms (J. J. Donohue and Levitt 2001; Goldin and Katz 2002; Bailey 2006; 2010), and the banning of sex discrimination in schools (Stevenson 2010). Within this literature, this paper provides further evidence that policies affecting human rights can have large effects on a wide range of outcomes, including crime rates.

3 Institutional context underlying the econometric strategy

While sodomy laws were historically used to also outlaw certain sexual practices between different-sex partners, as well as sexual intercourse between a person and an animal, such laws were used in modern American history to target individuals engaging in same-sex (anal or oral) sexual acts. Indeed, the Supreme Court emphasized the right of privacy for married and then unmarried different-sex couples in *Griswold v. Connecticut* (1965) and *Eisenstadt v. Baird* (1972), respectively. Therefore, an adult individual could legally engage in consensual sexual activities with a different-sex adult partner in private. As later re-emphasized in *Bowers v. Hardick* (1986), such protection did not extend to same-sex couples; thus, sexual minority adults remained at risk of being prosecuted for sodomy even while engaging in consensual same-sex sexual acts in their own homes. In line with this focus of sodomy laws on sexual minorities, Chang (2021) also equated sodomy law repeals with the decriminalization of homosexuality, and ILGA (2020) discussed sodomy laws in their report on state-sponsored homophobia.

Sodomy law decriminalization occurred in two ways: repeal through state legislatures and state supreme court decisions ruling the laws unconstitutional.[4] Before 1980, the call for decriminalization was primarily made by legal experts trying to persuade states to modernize their criminal codes (Eskridge 2008). Illinois became the first state to decriminalize consensual sodomy in 1961. Connecticut did the same in 1969.

Slowly, gay and lesbian movement activists, rather than legal experts, became responsible for initiating the attempts to decriminalize sodomy in the last two decades of the XX century (Bernstein 2003). At the same time, there was also a shift in the primary policy venue used to challenge sodomy laws: as legal activist

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[4] Table A1 in the Online Appendix provides additional details on the chronology of sodomy law decriminalization.

organizations specializing in judicial challenges began to lead the battle to decriminalize sodomy, they shifted the movement's attention to the judicial system rather than the legislative arena. The move to the courts was largely based on the assumption that judges would be less influenced by public opinion than legislators would, which was particularly important as the federal and state legislatures entered the more conservative Reagan and Bush years (Clendinen and Nagourney 1999; Kane 2007). Indeed, historically the US Supreme Court had already protected the right to distribute pro-homosexual writing through the public mail service in 1958 (*One Inc. v. Olesen*), while at the same time, during the so-called Lavender Scare, the federal government was systematically firing thousands of the US government employees because they were suspected to be homosexual (Johnson 2004).

At the federal court level, the gay and lesbian movement attempted to decriminalize sodomy in the early 1980s through a challenge of the Georgia state sodomy law. The challenge reached the US Supreme Court in 1986 (*Bowers v. Hardwick*). However, by 5 to 4 decisions, the Georgia law was found constitutional, and the court ruled that states had the right to criminalize specific sexual acts. Following this defeat, gay and lesbian activists started to challenge sodomy laws under state constitutions, which can add to rights guaranteed by the US constitution. Thanks to this strategy, same-sex sexual intercourse was decriminalized in Kentucky in 1992 (*Commonwealth v. Wasson*), Tennessee in 1996 (*Campbell v. Sundquist*), and Montana in 1997 (*Gryczan v. Montana*). By the end of 2002, 36 states plus the District of Columbia had decriminalized sodomy in their statutes (GLAPN 2007; Eskridge 2008). Finally, the US Supreme Court ruled 6-3 that Texas' sodomy law was unconstitutional (*Lawrence v. Texas*) on June 26, 2003, making all remaining sodomy laws invalid. In the majority opinion of the Court, Justice Kennedy admitted that such law "demeans the lives of homosexual persons." It is worth noting that, in line with the aforementioned claims that sodomy laws were used in the US to target sexual minorities and to deny same-sex couples a right to privacy, both Michael Hardwick (the man in *Bowers v. Hardwick*) and John Lawrence (the man in *Lawrence v. Texas*) were arrested while engaging in consensual same-sex sexual acts with other adults in their homes (Weinmeyer 2014).

Before this landmark decision from the U.S. Supreme Court, there is ample evidence that sodomy laws were systematically used to arrest, fire, and openly discriminate LGBTQ+ individuals, as well as encourage or justifying hate crimes (Lyman 1998; Eskridge 2008). For instance, after WWII, the FBI created a data bank of known homosexuals: 1700 applicants were denied government employment between 1947 and 1950 because they had a record of "homosexuality or other sex perversions" (Eskridge 2008). In states such as California and Florida, homosexual teachers and university professors were regularly fired for "immoral conduct" in the 1950s and 1960s. In the same period, the armed forces separated between 2000 and 5000 persons, especially women, as suspected homosexuals (Williams and Weinberg 1971).

Between 1940 and 1970, sodomy laws generated four times more appellate decisions than in the 1910–1940 period. There was a rapid jump in the number of arrests, with thousands of individuals arrested each year between 1946 and 1961. Racial and ethnic minorities were often targeted. In 1948, 483 people were arrested for sodomy

in Los Angeles; 1555 were arrested for homosexual solicitation. In Atlanta alone, hundreds of police officers were devoted to anti-homosexual stings and cleanup operations in the 1950s. Many states also allowed homosexuals to be detained in medical facilities for an indefinite amount of time (Eskridge 2008). National and local newspapers routinely reported about police raids, undercover operations, and arrests on sodomy charges (New York Times 1968; Castaneda and Gaines-Carter 1992; Ringle 1972), in some cases even involving congressmen (Sherwood and Stevens 1981), or still after the sodomy laws had been ruled unconstitutional (Robertson 2013; MAP 2016). The police and the army were using these strategies to persecute sexual minorities and arrest individuals not only for same-sex sexual behavior, but also for "disorderly activities" such as effeminate behavior (interpreted as perverted or as illegal sexual solicitation) or gender non-confirming dressing codes (Boyd 2003).

Anecdotal evidence provides hints on the additional impact of these laws on the mental health and personal lives of LGBTQ+ individuals (Economist 2020). Similarly, the stigma and discrimination associated with homosexuality, even when sodomy laws were not enforced, was emphasized by Justice Kennedy when striking down Texas' sodomy law in *Lawrence v. Texas:*

> "If protected conduct is made criminal and the law which does so remains unexamined for its substantive validity, its stigma might remain even if it were not enforceable […]. When homosexual conduct is made criminal by the law of the State, that declaration in and of itself is an invitation to subject homosexual persons to discrimination both in the public and in the private spheres."

Relatedly, in her concurring opinion, Justice O'Connor highlighted both the direct and indirect effects of such sodomy laws:

> "Petitioners' convictions, if upheld, would disqualify them from or restrict their ability to engage in a variety of professions, including medicine, athletic training, and interior design. […] Indeed, were petitioners to move to one of four States, their convictions would require them to register as sex offenders to local law enforcement. […] Texas' sodomy law brands all homosexuals as criminals. […] The law legally sanctions discrimination against [homosexuals] in a variety of ways unrelated to the criminal law, including in the areas of employment, family issues, and housing. […] Because of the sodomy law, *being* homosexual carries the presumption of being a criminal."

In addition, lesbian and bisexual women often lost custody of their children after divorcing their husbands (Eskridge 2008). Relatedly, the McCarran-Walter Act of 1952, upheld by the US Supreme Court in *Boutilier v. INS* (1967), prevented the immigration of homosexuals because they were deemed to be "afflicted with psychopathic personality".

4 Conceptual framework

Although the data on the actual behaviors and decisions of police forces and sexual minority individuals before and after the legalization of same-sex sexual activities is scant and does not allow us to exactly pin down the underlying factors connecting sodomy law repeals with crime and arrest, it is still possible to hypothesize on potential channels.

A direct effect of sodomy law repeals that one would expect is a decline in the number of arrests for crimes directly related to sodomy and historically used by police forces to harass and arrest sexual minority individuals (Eskridge 2008). Indeed, after such law changes, it was not possible to consider a private consensual same-sex sexual act among adults as a sex offense. Consequently, the number of arrests for sex offenses such as offenses against chastity, common decency, and morals is the first outcome variable considered in our empirical analysis.

Similarly, after the repeal of sodomy laws, sexual minority individuals could meet in bars and other public places more easily and without fear of persecution. Individuals could then find more easily willing sexual partners, potentially leading to a reduction in the demand for prostitution, and thus a decline in arrests for such a crime. To this end, it is important to emphasize that there might be two effects at play: not only the one just mentioned via demand, but also another one via supply. Indeed, it might be argued that repealing sodomy laws could alter the opportunity cost of certain individuals selling prostitution (to this extent, see Ciacci 2023).[5] There is also evidence of a relation been laws on sex offenses such as prostitution and attitudes in the general population (Jakobsson and Kotsadam 2011). Similarly, decriminalizing same-sex sexual acts could have also helped integrate sexual minorities in the mainstream economy, thus reducing homelessness and the number of people relying on survival sex (MAP 2016). All these factors could have led to a reduction in the supply of prostitution (and in the number of arrests for this activity).

A related channel follows the findings in Ciacci (2023). The author studied the determinants of prostitution and found that unilateral divorce drove down prostitution arrests: his results suggest that improving gender equality might be associated with a decay in prostitution. In a similar vein, and in line with Sansone and Carpenter (2020), one may argue LGBTQ+ policies, inequalities, and attitudes are positively correlated with gender attitudes and inequalities. The results of our paper might be interpreted as supporting this notion. As a matter of fact, insofar as we believe that decriminalization of same-sex sexual intercourse measures an improvement also for transgender individuals marginalized in society, bisexual individuals, women partnered with men who could have been affected by sodomy laws, and more generally gender equality, such an improvement could reduce prostitution.

Furthermore, it is reasonable to expect police forces to reduce the amount of the budget allocated to the prosecution of an activity that is no longer considered a crime. This reaction would be consistent with savings in law enforcement resources

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[5] Clearly, it might be also postulated that the fall in demand might trigger a decay in supply as a result.

estimated following the decriminalization of other activities such as marijuana possession and use (Miron 2002, 2008; Egan and Miron 2006). Police agents would no longer spend a substantial amount of their time raiding gay bars, saunas, public bathrooms, and parks. They would also have lower levels of discretion when deciding when and why to arrest sexual minority individuals. At the same time, the decriminalization of same-sex sexual acts would change public attitudes towards sexual minorities (Kenny and Patel 2017), similarly to what researchers observed after the passage of other LGBTQ+ policies such as same-sex marriage laws (Sansone 2019; Flores and Barclay 2016; Tankard and Paluck 2017; Ofosu et al. 2019). The higher level of acceptance would facilitate the assimilation of sexual minority individuals in society. The overall effect is likely to be both a decline in disorderly conduct—that is, any behavior that tends to disturb the public peace or shock the public sense of morality—and in the number of arrests for disorderly conduct.

More generally, as argued by Woods (2017), the decriminalization of private consensual sodomy changed the criminal status quo, was associated with a drastic shift in scholarly and popular perceptions of sexual minorities, and contributed to the decline of the image of lesbian and gay individuals as deviant sexual offenders. Homosexuality stopped being viewed as a form of criminal sexual deviance in and of itself, and LGBTQ+ identities started being frames as linked to unjust hate-crime victimization. In other words, there was a paradigmatic shift from deviant sexual offenders to innocent hate crime victims (Woods 2017). It is therefore likely that these changes affected police behavior, general attitudes, criminalization, and arrest rates.

Finally, in addition to these potential changes in arrest rates for crimes directly related to same-sex sexual acts, the repeals of sodomy laws could have indirectly affected arrest rates for other crimes. Indeed, there is a large literature documenting drug abuse and higher rates of smoking and drinking behaviors among sexual minorities (Conron, Mimiaga, and Landers 2010; CDC 2018; NASEM 2020; Carpenter and Sansone 2021), and a related higher probability of driving under the influence of alcohol and drugs (Yockey, Vidourek, and King 2020; Yockey et al. 2022), as well as widespread mental issues (Bostwick et al. 2010; NASEM 2020). Equally, as discussed in Meyer (1995), sexual minority individuals are likely to suffer from minority stress due to internalized homophobia, anticipated rejection, constant efforts to hide their sexual identity, and actual experiences of discrimination and violence. There is in fact evidence of a relation between harassment, minority stress, and health outcomes in the LGBTQ+ community (Mays and Cochran 2001; Hatzenbuehler et al. 2009). Relatedly, Meyer et al. (2017) have used stressors related to family rejection, illegal drug abuse, and community-level marginalization and stigmatization to explain the higher proportion of sexual minorities among the incarcerated. Last but not least, connected to our previous discussion on the expected decline in arrests for prostitution, it has been shown that reducing the number of individuals engaged in prostitution correlates with a fall in drug addiction (DeRiviere 2006).

Based on these results in the literature, it is likely that the repeal of sodomy laws would have led to a more welcoming legal and social environment for sexual minorities. Consequently, there would have been a decline in the sources of minority stress

and a reduction in the need of using alcohol and drugs as coping mechanisms, resulting in fewer arrests for driving while mentally or physically impaired as the result of consuming alcoholic beverages or drugs. Other scholars have indeed found a relationship between mental health problems and driving under the influence (Stoduto et al. 2008; Choi et al. 2015). Such results would be consistent with previous studies showing that related LGBTQ+ policies—that is, same-sex marriage bans and (conversely) marriage equality laws—had large effects on mental health and risky behaviors such as alcohol use among sexual minorities (Dee 2008; Hatzenbuehler et al. 2010; Francis, Mialon, and Peng 2012; Flores, Hatzenbuehler, and Gates 2018; Hatzenbuehler et al. 2012). These results would also be in line with recent findings showing improvements in mental health among sexual minority men following the passage of employment anti-discrimination laws due to a reduction in animosity and prejudice (Mann 2022). Similarly, since disorderly conduct is also a crime often linked to alcohol and drug consumption, any reduction in substance use (as well as in reckless behavior under the influence of drugs) could potentially lead to a decline in arrests for disorderly conduct. In other words, the number of arrests for disorderly conduct could decline not only due to the aforementioned changes in police practices and general population attitudes, but also due to a reduction in alcohol and drug consumption.

Despite the potential mechanisms highlighted in the previous paragraphs, it is important to note that it is not possible to ex-ante predict a decline in arrest rates following the decriminalization of same-sex sexual acts. Indeed, sexual minorities could still continue to be profiled as more likely to engage in sex work or commit sexual offenses, they may still be seen as threatening and dangerous due to the perception that they fail to conform to the social norms of masculinity and femininity (Wilkinson 2008), thus leading to the potential backlash, persistent or even higher over-policing, further discrimination and social isolation, and subsequent arrest and incarceration (Meyer et al. 2017). Therefore, estimating the effect of such law changes is ultimately an empirical question, one that we answer with our data analysis.

5 Data and methodology

5.1 Data

This paper uses the 1995–2018 Uniform Crime Reporting (hereafter, UCR) Program arrest database (FBI 2020).[6] This database collects arrest data for 28 offenses as reported from law enforcement agencies. Since a person might be arrested multiple times in the same year, this dataset measures the number of times persons are

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[6] We are using all years whose complete arrest reports were available in the FBI UCR website (i.e.,1995–2018): https://ucr.fbi.gov/crime-in-the-u.s (Accessed: August/2020). We are including data up to 2018 to estimate the long-term impacts of the law changes. As discussed in Section 6, our main conclusions are robust to the exclusion of the more recent years.

arrested rather than the number of individuals arrested. In addition, UCR data follows an algorithm called the "hierarchy rule": according to this algorithm, criminal activities are classified depending on the most serious statutory offense (Chalfin 2014). For example, in our setting, a murder-burglary is classified as a murder.

It is worth noting that the UCR Program arrest data set is based on voluntary reporting by law enforcement agencies. This feature implies there might be the concern that crimes recorded by the database could fluctuate simply because of variations in the number of law enforcement agencies that decide to report crimes. Likewise, this feature might also raise concerns on potential measurement error in our dependent variable. Given the voluntary reporting of agencies and the "hierarchy rule," it seems reasonable to believe that if such data suffer of measurement error, its nature is random (or at least unrelated with the decriminalization of same-sex sexual activities). In addition, to the best of our knowledge, evidence of measurement error in the data provided by the UCR has only been found in police personnel data (Chalfin and McCrary 2018), which we do not use in our analysis. Yet, as an effort to address this issue, we keep track of the number of law enforcement agencies reporting crimes for each state in any year in our sample period and include this indicator as a control variable in our regressions, as discussed in more details in the next section.

Moreover, by using UCR data at the year level, instead of the month level, we avoid having measurement errors due to either over or under-counting issues. For example, if a certain arrest is reported in a month, then it is counted as 1 arrest in such a month. Nonetheless, if this arrest is eventually determined to be unfounded in the following month, then it is included as a −1 in such a month. Usually, scholars exploiting monthly data have to address such issues, yet we do not have this problem since we opted to use yearly UCR data.

Despite these data limitations, we believe that UCR data are the best—and probably only—data sources that can be used to analyze the impact of sodomy law repeals on crime. Indeed, most of these legal changes until the Supreme Court decision in 2003 occurred at times when data on LGBTQ+ individuals were almost non-existent, the use of the Internet was still limited (thus preventing us from analyzing any data on online dating or prostitution websites), and more granular and systematic data on police funding allocation were not available. Nevertheless, we still think that the importance of answering this research question with the available tools and resources outweighs such constraints.

Table C1 in the Online Appendix shows summary statistics for our main dependent variables, i.e., arrest rate for disorderly conduct, prostitution, other sex offenses, and driving after consuming alcoholic beverages or using drugs (per 1,000,000 residents).[7] We can observe that for all four variables mean and median are fairly close to each other; although, as expected, arrests for prostitution and other sex offenses

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[7] All variables used in the empirical analysis are described in detail in Section B of the Online Appendix.

happen more rarely than arrests for disorderly conduct or for driving under the influence.[8]

5.2 Event study model

Given the available data and documented law changes, it is then possible to estimate the following event study as our main specification:

$$Arrest\_rate_{st} = \alpha + \sum_{k=\underline{T}}^{\overline{T}} \beta_k Sodomy_{st}^k + \delta_s + \mu_t + x_{st}' \gamma_1 + LGBTQ_{st}' \gamma_2 + \varepsilon_{st}$$

where $Arrest\_rate_{st}$ is the reported arrest rate (per 1,000,000 residents, in natural logarithms) for a given crime in state s at time t. The logarithmic function ensures that the estimates are not driven by outliers. $Sodomy_{st}^0$ is an indicator equal to one if state s had decriminalized sodomy at time t, zero otherwise. $Sodomy_{st}^k$ are the resulting lead ($k < 0$) and lag ($k > 0$) operators. The specification includes state (δ_s) and year (μ_t) fixed effects, thus controlling for all time-invariant state characteristics, as well as for all macroeconomic shocks affecting all states in the same year. The vector of time-varying state-level controls (x_{st}') includes unemployment rate, income per capita, and the number of agencies reporting their crime data to the FBI. In order to control for additional factors potentially related to sodomy laws, $LGBTQ_{st}'$ accounts for other policies such as constitutional and statutory bans on same-sex marriage, same-sex marriage legalization, same-sex domestic partnership legalization, same-sex civil union legalization, LGBTQ+ anti-discrimination laws, and LGBTQ+ hate crime laws.[9] Standard errors are clustered at the state level (Bertrand et al. 2004).

5.3 Discussion on the exogeneity of the policy changes

A key concern when interpreting event study estimates as causal is that the timing of the sodomy decriminalization in each state should not reflect pre-existing differences in state-level characteristics. In this context, a first step is to include the aforementioned state fixed-effects and additional state controls that may be related with the decriminalization of same-sex sexual activities.

Furthermore, it is important to emphasize that, unlike other policy reforms such as unilateral divorce laws (Stevenson and Wolfers 2006), most sodomy laws in the 1990s and early 2000s—i.e., the law changes analyzed in our econometric model—were struck down following judicial decisions, not legislative processes (Table 1).

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[8] In addition, Table C2 in the Online Appendix displays summary statistics for the number of agencies across states in the considered sample period.

[9] Following Frank, Camp, and Boutcher (2010), one may be worried that other contemporaneous changes in criminal laws could be correlated with sodomy law repeals and actually drive the estimated changes in arrest rates. However, we found no systematic changes correlated with sodomy law repeals occurring between 1995 and 2003 in law changes for prostitution, driving under the influence, child sex-abuse, or loitering.

Table 1 Sodomy law repeals exploited in the main event studies and difference-in-differences models

State	Year	Method	Notes
Tennessee	1996	Judicial	*Campbell v. Sundquist*
Montana	1997	Judicial	*Gryczan v. Montana*
Georgia	1998	Judicial	*Powell v. Georgia*
Rhode Island	1998	Legislative	Enacted in 1998, effective in 1998
Maryland	1999	Judicial	*Williams v. Glendening*
Arizona	2001	Legislative	Enacted in 2001, effective in 2001
Minnesota	2001	Judicial	*Doe et al. v. Ventura et al.*
Arkansas	2002	Judicial	*Jegley v. Picado*
Massachusetts	2002	Judicial	*GLAD v. Attorney General*
USA	2003	Judicial	*Lawrence v. Texas*

Main sources: GLAPN (2007), Kane (2007), and Eskridge (2008). See also Table A1 for previous law changes

The exact timing of the court decisions was plausibly unexpected. Moreover, as already mentioned, judges often served lengthy terms and were less subject than politicians to the public opinion on homosexuality. Indeed, federal and state judges repealed these sodomy laws at the same time as voters and legislators in several states approved bans on same-sex marriages (Sansone 2019), and while President Clinton and the US Congress passed anti-LGBTQ legislation such as the *Defense of Marriage Act* defining marriage for federal purposes as the union of one man and one woman, as well as the *Don't ask*, *don't tell* policy barring openly gay, lesbian, or bisexual individuals from serving in the military. The independence of the justice system from public discussion and the opinion of the majority was emphasized by Justice Kennedy when striking down Texas' sodomy law in *Lawrence v. Texas*:

> "For centuries there have been powerful voices to condemn homosexual conduct as immoral. […] These considerations do not answer the question before us, however. The issue is whether the majority may use the power of the State to enforce these views on the whole society through operation of the criminal law. Our obligation is to define the liberty of all, not to mandate our own moral code"

In addition, Justice Kennedy noted that this point was supported by Justice Stevens' opinion in a previous related court case:

> "The fact that the governing majority in a State has traditionally viewed a particular practice as immoral is not a sufficient reason for upholding a law prohibiting the practice."

Furthermore, in her concurring opinion in *Lawrence v. Texas*, Justice O'Connor emphasized that:

"We have consistently held, however, that some objectives, such as "a bare . . . desire to harm a politically unpopular group," are not legitimate state interests. [...] We have never held that moral disapproval, without any other asserted state interest, is a sufficient rationale under the Equal Protection Clause to justify a law that discriminates among groups of persons.""

It is also worth mentioning that, even if one may worry that the most gay-friendly states were the first ones to introduce LGBTQ+ reforms such as the legalization of same-sex sexual acts and the introduction of marriage equality, this hypothesis is not supported by the fact that the order in which states decriminalized consensual sodomy is rather different from the order in which states legalized same-sex marriage. For instance, Massachusetts was the first state to legalize same-sex marriage (2004), but it was among the last ones to decriminalize sodomy (2002).[10] New York, one of the states with the largest LGBTQ+ populations, was not among the first states to legalize sodomy (1980), nor same-sex marriage (2011).[11]

6 Results

6.1 Sodomy law repeals lead to a reduction in arrest rates

The key finding of the paper is that sodomy law repeals led to a significant and persistent reduction in the arrest rates for crimes directly related to sodomy. Indeed, Fig. 1 shows a decline in arrests for sex offenses such as offenses against chastity, common decency, and morals. Figures 2 and 3 report similar reductions in arrests for prostitution and disorderly conduct, respectively. It is worth noting that, in all the graphs, none of the lead operators is statistically significant, thus supporting the parallel trends assumption. Moreover, the impact of decriminalizing sodomy on these crimes can be detected both in the year in which the law was abolished, as well as in the years afterwards, thus suggesting that these reforms had long-lasting effects.

We then provide evidence supporting the hypothesis that sodomy law decriminalization not only led to a direct decline of individuals arrested for related crimes, but it also had more general effects. In line with the discussion in the previous sections highlighting the negative environment created by sodomy laws and consistently with the hypothesis that the subsequent sodomy law repeals reduced minority stress (Meyer 1995) and led to a reduction of drinking and drug use as a common coping mechanism, Fig. 4 reports a clear and significant drop in the number of arrests for

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[10] The law prohibiting "any unnatural and lascivious act with another person" had no longer been used to prosecute consensual conduct in private between adults after *Commonwealth v. Balthazar* in 1974, but same-sex sexual intercourse could still be prosecuted in Massachusetts as "abominable and detestable crime against nature" (the standard terminology used in sodomy laws) until *GLAD v. Attorney General* in 2002.
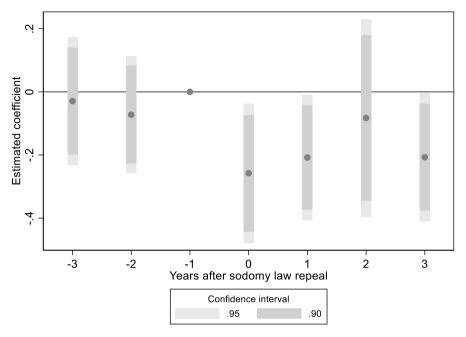
[11] In line with these claims, Table C3 shows that the associations between sodomy law repeals and constitutional bans on same-sex marriage, statutory bans on same-sex marriage, or same-sex marriage legalization are statistically indistinguishable from zero.

Fig. 1 Effect of sodomy law repeals on arrests for sex offenses. This figure analyzes the effect of sodomy law repeals on the arrest rate for sex offenses (excluding rape, prostitution, and commercial vice). Arrest rate (per 1,000,000 state residents) is in logarithms. First lead normalized to zero. First lag equal to one if a state repealed its sodomy laws in the previous year, second lag equal to one if a state repealed its sodomy laws 2 years before, third lag equal to one if a state repealed its sodomy laws 3 years before or earlier. Second lead equal to one if a state repeals its sodomy laws in 2 years, third lead equal to one if a state repeals its sodomy laws in 3 or more years. See also the "Data and Methodology" section. Source: FBI 1995–2018. *N*=1189

driving while mentally or physically impaired as the result of consuming alcoholic beverages or using drugs.

6.2 Difference-in-differences estimates support the findings from the event studies

Table 2 reports the results from the difference-in-differences regression model for our four outcome variables (sex offenses, prostitution, disorderly conduct, and driving under the influence). In other words, the table reports the estimated $\hat{\beta}$ from the following difference-in-differences model:

$$Arrest\_rate_{st} = \alpha + \beta Sodomy_{st} + \delta_s + \mu_t + x'_{st}\gamma_1 + LGBTQ'_{st}\gamma_2 + \varepsilon_{st}$$

where $Sodomy_{st}$ is an indicator equal to one if state s had decriminalized sodomy at time t, as well as in the following years, zero otherwise. The dependent variable ($Arrest\_rate_{st}$), state fixed effects (δ_s), year-fixed effects (μ_t), state controls (x'_{st}), and LGBTQ+ policy controls ($LGBTQ'_{st}$) are defined as in the event study model.
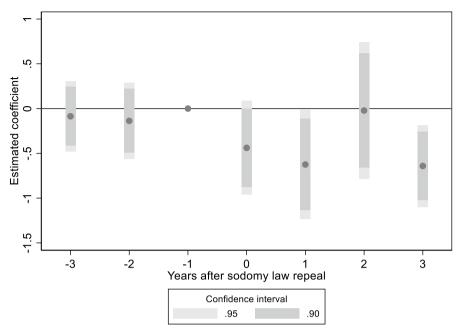
Fig. 2 Effect of sodomy law repeals on arrests for prostitution. This figure analyzes the effect of sodomy law repeals on the arrest rate (in logarithm) for prostitution and commercialized vice. See notes in Fig. 1. Source: FBI 1995–2018. *N*=1188
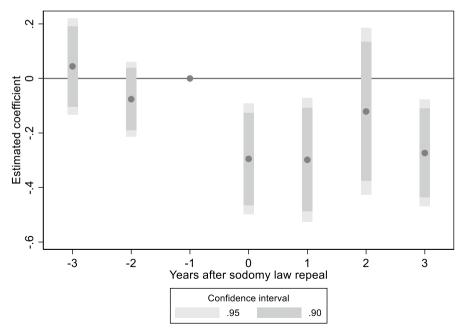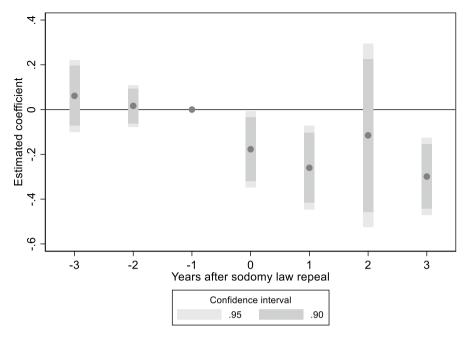

Fig. 3 Effect of sodomy law repeals on arrests for disorderly conduct. This figure analyzes the effect of sodomy law repeals on the arrest rate (in logarithm) for disorderly conduct. See notes in Fig. 1. Source: FBI 1995–2018. *N*=1179

Fig. 4 Effect of sodomy law repeals on arrests for driving under the influence. This figure analyzes the effect of sodomy law repeals on the arrest rate (in logarithm) for driving after consuming alcoholic beverages or using drugs. See notes in Fig. 1. Source: FBI 1995–2018. *N*=1188

Results are negative and statistically significant in all four regressions, thus supporting the main conclusions from Figs. 1, 2, 3, and 4.[12]

It is worth noting that our estimates are economically meaningful. According to Table 2, our findings suggest that sex offenses and prostitution rates decreased by roughly 16% and 37%, respectively, due to the decriminalization of same-sex sexual intercourse. Likewise, disorderly conduct and driving under the influence fell by about 24% and 26%, respectively.

The size of such results is aligned with the literature. Namely, our estimated impact on sex crimes is comparable to the impact of other related policies on sex crimes such as rape. For instance, our results on sex crimes seem to be a lower bound of the effects of policies affecting the sex market on (reported) rape. Indeed, Cunningham and Shah (2018) found that the decriminalization of prostitution reduced rape by 30%, while Ciacci (2018) determined that the criminalization of the purchase of prostitution raised rape by roughly 47%. As a whole, the crime literature

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[12] All estimated coefficients are reported in Table C4 It is worth noting that almost none of the coefficients for the other LGBTQ+ policies are statistically significant. Furthermore, the sign of these coefficients flips across regressions, thus suggesting that the link between such laws and these crimes is ambiguous. Table C5 further shows that the main estimates for sodomy law repeals do not change substantially when excluding controls for same-sex marriage legalization, as well as constitutional and statutory bans on same-sex marriage. One can also show that results for sex offenses, prostitution, and disorderly conduct are robust to the inclusion of state-specific time trends.

Table 2 Effect of sodomy law repeals on sex offenses, prostitution, disorderly conduct, and driving under the influence, difference-in-differences estimates

	Sex offenses	Prostitution	Disorderly conduct	Driving under the influence
	(1)	(2)	(3)	(4)
Sodomy law repeal	−0.170*	−0.464**	−0.277***	−0.297***
	(0.094)	(0.176)	(0.092)	(0.079)
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
State control	Yes	Yes	Yes	Yes
LGBTQ+ policies	Yes	Yes	Yes	Yes
Observations	1189	1188	1179	1188
Adjusted-R^2	0.762	0.681	0.822	0.805

This table analyzes the effect of sodomy law repeals on the arrest rate for sex offenses other than rape or prostitution (column 1), prostitution and commercialized vice (column 2), disorderly conduct (column 3), and driving under the influence of alcohol or drugs (column 4). Arrest rate (per 1,000,000 state residents) is in logarithms. Time-varying state-level controls: unemployment rate, income per capita, and the number of agencies reporting their crime data to the FBI. LGBTQ+ policies: constitutional and statutory bans on same-sex marriage, same-sex marriage legalization, same-sex domestic partnership legalization, same-sex civil union legalization, LGBTQ+ anti-discrimination laws, and LGBTQ+ hate crime laws. Standard errors clustered at the state level reported in parentheses. Source: FBI 1995–2018. *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$

found results of similar size: e.g., Bisschop, Kastoryano, and van der Klaauw (2017) found that regulating prostitution via licenses reduced rape and sexual abuse by about 30–40% in the Netherlands. Our results are around one third the size of these findings.[13] Our results are also aligned with Ciacci and Sviatschi (2022): their findings highlight that adult entertainment establishments reduce sex crimes by 13%. An estimate which stands out for its similar size to the 16% we found. Yet, as it might be expected, our results are larger than the effects of policies unrelated to the sex market. To this extent, Bhuller et al. (2013) estimated that in Norway broadband internet use might respectively explain 8% of rapes and 5% of sex crimes that occurred in 2006.

Similarly, even if the effect on prostitution might seem large at first sight, a careful comparison of this effect with the findings of the literature reveals this is not the case. First, using the effects of decriminalizing prostitution as a benchmark highlights our results are a lower bound compared to those. In this regard, it is important to note that there is evidence in the literature that decriminalizing prostitution might decrease prostitution by about 40% and increase massage parlors workers (a proxy

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[13] It is worth emphasizing that we are merely comparing in this paragraph our estimates to other related estimates in the crime literature. Our measurement of arrest for sex offenses is correlated but not equivalent to the number of reported rapes. We are not arguing that sodomy law repeals led to a decline in arrest for sex offenses through a reduction in rapes (indeed, rapes are not included in Fig. 1), nor we are claiming that sexual minorities are more or less likely to be involved in rape cases.

of indoor prostitution) by 200% (Cunningham and Shah 2018). Hence, depending on the estimate used in the comparison, the effect of sodomy laws on prostitution arrests is either as large as the effect of decriminalization or about 80% smaller. Second, as for sex crimes, our effects are larger than those of policies not connected to the sex market. Indeed, Ciacci (2023) found that unilateral divorce led to a reduction in female prostitution of about 10% and he found evidence indicating that this reduction was supply-driven. Given the fact that a priori decriminalization of sodomy might have had an effect on both the demand and supply of prostitution (as discussed in the conceptual framework), it is not surprising to find that its effect on prostitution was relatively larger than its effect on sex offenses.

Likewise, the effect of disorderly conduct and driving under the influence is not as large as it might appear at first sight. As a matter of fact, our results suggest that slightly less than one third of the arrests for these two crimes are led by the ban on same-sex sexual intercourse. To the best of our knowledge, we do not have clear estimates for comparison as we have for sexual offenses and prostitution. However, it is important to take into account that we are considering data on arrested aggressors of disorderly conduct and driving under the influence and not on the whole pool of people committing such crimes. For these kinds of crimes, arrests might be more likely to occur in desperate situations as it might be in the case of people who suffer considerable stress since they cannot live their sexuality freely. Indeed, people in these circumstances might drink alcoholic beverages and then commit reckless actions that might increase the probability of being caught by the police. Hence, our treatment variable is triggering a certain group of people which might comprise a larger share in the set of arrested aggressors for these offenses since their probability of being arrested is substantially higher in the first place.[14]

6.3 Testing the credibility of the event study and difference-in-differences estimates

The Online Appendix reports several extensions and robustness checks. To further explore the event-study estimates, column 1 of Tables C6-C9 report the estimated coefficients shown in Figs. 1, 2, 3, and 4. As also evident from these figures, most of the estimated effects on arrest rates in the years' sodomy laws were repealed and afterwards are statistically significant at the 5% or 1% levels.

We explore our results further using the recent advances in the literature analyzing difference-in-differences and event study techniques. Namely, we rely on three papers: Borusyak and Jaravel (2018), de Chaisemartin and D'Haultfœuille (2020), and Goodman-Bacon (2021). These papers are part of a large recent strand

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[14] It is then worth comparing our results to other findings in the LGBTQ+ literature. Indeed, even if (as already mentioned in the literature review) to our knowledge no other study has analyzed the effect of LGBTQ+ policies on arrest rates, Nikolaou (2022) found that same-sex marriage legalization in the US decreased by 21% the incidence of hate crimes motivated by sexual orientation bias. This estimate is comparable in magnitude to the main estimates reported in Table 2. Sansone (2019) also found a decline in hate crimes following same-sex marriage legalization, although his estimates are not statistically different from zero after including additional policy controls and state trends.

Table 3 Difference-in-differences robustness checks

	Logs	Levels	State		Agencies	
	σ	σ	t stat	ρ	t stat	ρ
	(1)	(2)	(3)	(4)	(5)	(6)
Sex offenses	0.07	12.50	-0.44	-0.03	-0.01	0.00
Prostitution	0.15	17.08	-0.44	-0.03	-0.01	0.00
Disorderly conduct	0.11	143.63	-0.36	-0.02	-0.08	-0.01
Driving under the influence	0.12	513.34	-0.45	-0.03	-0.01	0.00

This table reports different estimates as suggested in De Chaisemartin and d'Haultfœuille (2020). It shows the minimum amount of treatment effect heterogeneity needed to lead to a situation of opposite signs between the average treatment effects and the difference-in-differences estimator for our main specification in logs (column 1) and in levels (column 2). Columns 3 and 4 show the t statistic and size of the correlation between such weights and states, respectively. Columns 5 and 6 repeat this last analysis across agencies

of the literature that points out potential issues of two-way fixed-effect estimators in difference-in-differences designs. Indeed, such estimators rely on parallel trends assumption for unbiasedness and are a weighted average of multiple 2×2 difference-in-differences treatment versus control estimates. To this extent, it is crucial to delve into the plausibility of the parallel trends assumption before interpreting such estimates as causal. Likewise, it is key to approach these weights since some of them might be negative and flip the sign of the weighted average—and, consequently, of the estimates.

First, we rely on the methodology developed by de Chaisemartin and D'Haultfœuille (2020) to assess the robustness of our difference-in-differences estimates. De Chaisemartin and D'Haultfœuille (2020) show that since the classical difference-in-differences estimator is a weighted sum of average treatment effects and these weights might be negative, there could be instances in which the difference-in-differences estimator sign is driven by negative weights. To address this issue, they suggest the usage of different statistics.

Negative weights may arise if treatment effects are heterogeneous. Given the staggered enactment of sodomy laws across the US states, there might be a concern that treatment effects vary across states. Likewise, given the debate on agencies self-reporting arrest numbers, there might be also the concern that treatment effects vary across agencies. To assess the plausibility of this concern, we estimate the percentage of positive and negative weights: indeed, de Chaisemartin and D'Haultfœuille (2020) allow us to compute the shares of positive and negative weights. With this aim in mind, we would like to explore whether we find empirical support for our difference-in-differences estimator. In particular, for its sign.

We find that about 58% are strictly positive while 42% are strictly negative out of 1049 average treatment-on-the-treated cases. The negative weights sum to −0.93. One may therefore worry that the average treatment effects and the difference-in-differences estimator could have an opposite sign. To address this concern, we compute the minimum amount of treatment effect heterogeneity needed to lead to a situation

Table 4 Difference-in-differences robustness checks

	Sex offenses	Prostitution	Disorderly conduct	Driving under the influence
	(1)	(2)	(3)	(4)
Baseline DiD	−0.180	−0.358	−0.283	−0.393
Earlier T vs later C	−0.134	−0.481	−0.173	−0.041
Weight	0.016	0.016	0.016	0.016
Later T vs earlier C	−0.392	−0.512	−0.552	−0.364
Weight	0.088	0.088	0.088	0.088
T vs already treated	−0.161	−0.340	−0.259	−0.402
Weight	0.896	0.896	0.896	0.896
Main DiD	−0.170	−0.464	−0.277	−0.297
Observations	1189	1188	1179	1188

This table reports different estimates as suggested in Goodman-Bacon (2021). This table presents average treatment effects and its corresponding weights for three different groups depending on treatment timing: earlier treated (T) versus later control (C), later treated (T) versus earlier control (C) and treated (T) versus already treated. This table also shows our main estimates from Table 2. Baseline DiD presents the estimates with only year and state fixed effects (i.e., without any other control variable)

of opposite signs between the average treatment effects and the difference-in-differences estimator. Table 3 respectively shows this statistic for our main specification in logs (column 1) and levels (column 2). Computations for the specification in levels are large, offering supporting evidence that a huge variation of heterogeneity is necessary to flip the sign of the difference-in-differences estimator with respect to the average treatment effects. However, computations for the regression model in logs might seem small in size, thus this might raise doubts about our estimates. To this extent, Columns 3 and 5 report the t-statistic of the correlation of the weights respectively across states and agencies, while Columns 4 and 6 report the size of such a correlation. For the four considered outcome variables, we find that t statistics lie far away from statistical significance thresholds and that the correlation is close to zero in size. All in all, these results suggest that our difference-in-differences estimates can be interpreted as causal effects since the intensity of the treatment—across both state and agency cells—is uncorrelated with the weights attached to them.

Second, Goodman-Bacon (2021) suggests an alternative approach to assess whether the difference-in-differences estimator sign is due to negative weights or heterogeneous effects by exploring average treatment effects and its weights dividing treated groups depending on when they received treatment. Table 4 displays average treatment effects and its corresponding weights for three different groups depending on treatment timing: earlier treated versus later control, later treated versus earlier control, and treated versus already treated. This table also reports our main estimates from Table 2 ("Main DiD"), while "Baseline DiD" presents the estimates only with year and state fixed effects (i.e., without other control variables). With this aim in mind, we want to analyze if we find empirical support for the size

and sign of our difference-in-differences estimator across treated groups depending on treatment timing.

Column 1 shows the results for sex offenses. The four estimates (i.e., baseline DiD and average treatment effects for each comparison group) are negative. Moreover, except for later treated versus earlier control (whose estimates are larger in absolute value), estimates are similar in size ranging from −0.134 to −0.180. A similar pattern is displayed for disorderly conduct (column 3), where estimates range from −0.173 to −0.283 except for later treated versus earlier control, while for driving under the influence (column 4) all coefficients are similar in magnitude except for earlier treated versus later control (whose weights are the smallest). Finally, for prostitution (column 2) estimates are similar in size across the four estimates and similar to our main estimates. Moreover, weights are positive for each of the comparison groups.

These computations are also key to address concerns on already treated states. In this regard, there might be concerns about how the inclusion of these states affects regression results with respect to states treated during the sample period. Results of Table 4 show that estimates of treated versus already treated states are in line in sign and size with both our main estimators and the baseline DiD. Indeed, these results are not statistically different from the main DiD: for our four regressions, each one of them lies in the 95% confidence interval of their corresponding main estimate.[15]

Third, assuming the year in which a sodomy law was repealed is as if it were randomly assigned (conditionally on state fixed effects), Borusyak and Jaravel (2018) suggested to test linear pre-trends dropping two pre-trend indicators and using an F test of significance for the remaining pre-trend indicators. This F test compares the residual sums of squares under the main specification (unrestricted) and the specification where two pre-trend indicators were dropped (restricted). Columns 2–3 of Tables C6–C9 omit $Sodomy_{st}^{-2}$ or $Sodomy_{st}^{-3}$, respectively. The last row of these two columns displays the p value of the F test of the significance of the remaining lead operator: we find no statistical evidence suggesting that our results are driven by linear pre-trends. Altogether, these findings suggest our main estimates are not driven by either heterogeneous effects or negative weights.

6.4 Additional extensions and robustness checks

This section considers different empirical checks that might strengthen our analysis and address its limitations. These checks range from using additional crimes linked to substance usage as the dependent variable, to changing the functional form of the dependent variable, to considering different time windows in the event study, and to limiting the length of the sample period depending on the timing of the treatment variable. Exploring these specifications might be useful to have a more

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[15] Similarly, Table C10 modifies the main difference-in-differences model by removing the 28 states whose sodomy laws were repealed before 1995. The sample size is substantially reduced, so the confidence intervals are considerably larger than those in Table 2. Nevertheless, all estimates remain negative and close in magnitude to those in Table 2. The coefficients for sex offenses, prostitution, and disorderly conduct are still statistically significant.
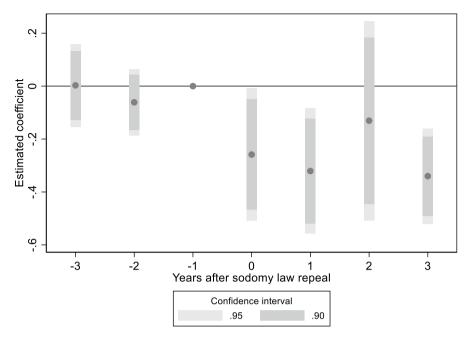
Fig. 5 Effect of sodomy law repeals on arrests for drug abuse. This figure analyzes the effect of sodomy law repeals on the arrest rate (in logarithm) for drug abuse violations. See also notes in Fig. 1. Source: FBI 1995–2018. N=1189

comprehensive view of the empirical findings. In the rest of this section, we explore each one of them in detail.

To further support our claim that sodomy law repeals led to changes in police practices, a reduction in minority stress, and a decline in substance abuse, we have looked at other related crimes connected to addictive substance usage. In line with the estimates plotted in Fig. 4 measuring the impact of sodomy law repeals on arrest rate for driving after consuming alcoholic beverages or using drugs, we observe similar reductions in the number of arrests for drug abuse (Fig. 5) and liquor laws violations (Fig. 6). These findings further emphasize the relationship between sodomy law repeals, police practices, and alcohol or substance usage.[16]

The Online Appendix includes additional tables and figures addressing different concerns regarding the robustness of our estimates. There might be doubts related to the functional form of the dependent variable, the sample period, and the lags and leads window of the event study analysis. One may worry that our results may depend on some functional form assumptions. Indeed, as common in the literature, one may wonder whether our conclusions would change when using the dependent variable in levels rather than in logarithm. Nonetheless, the main results do not change when measuring arrests in levels rather than logarithms (Figures C1–C4).

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[16] To further support this potential explanation related to mental health and minority stress, we complement our empirical analysis in Appendix D by showing suggestive evidence of a drop in the number of suicides among men following the decriminalization of same-sex sexual activities.
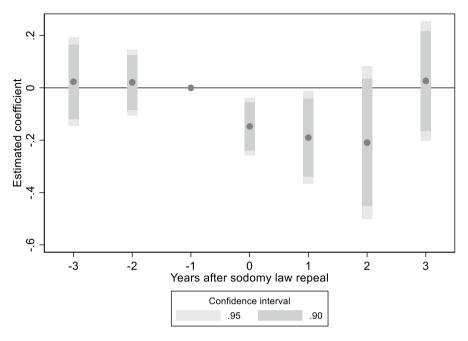
Fig. 6 Effect of sodomy law repeals on arrests for liquor law violations. This figure analyzes the effect of sodomy law repeals on the arrest rate (in logarithm) for liquor law violations. See also notes in Fig. 1. Source: FBI 1995–2018. *N*=1189

Moreover, even if our data allowed us to estimate long-term effects of sodomy law repeals, one may wonder whether the main event studies would look different when restricting the time frame: indeed, there is no policy variation after 2003. One additional potential concern is that the third lag in the event studies summarizes the impact of sodomy law repeals after three or more years; thus, it may be substantially affected by the number of years included in the analysis. Reassuringly, the event study plots look remarkably similar to those in Figs. 1, 2, 3, and 4 when focusing only on short-term or medium-term effects and stopping the analysis to 2010 (Figures C5–C8) or 2006 (Figures C9–C12). These restrictions also confirm that our analysis is robust to limiting the time frame to years before most states started to legalize same-sex marriage or introduced additional LGBTQ+ policies: for instance, only Massachusetts had legalized same-sex marriage before 2006.

Relatedly, in the main Figs. 1, 2, 3, and 4, we also decided to show only up to three leads and lags. As already mentioned, the third lag measures the effect of sodomy law repeals after three *or more* years. One may wonder if the long-term effects may be different from the medium-term ones. To investigate this hypothesis, we have increased the number of leads and lags up to 5 years (Figures C13–C16): the estimated effects for "lag 3", "lag 4", and "lag 5" are similar, thus providing further evidence of a persistent impact of sodomy law repeals. Conversely, the estimated coefficients for "lead 3", "lead 4", and "lead 5" remain statistically insignificant, thus supporting the parallel trend assumption.

Similarly, to test whether our main findings are driven by outliers, we have checked that excluding California—the state with the largest number of LGBTQ+ individuals—does not substantially alter the main findings (Figures C17–C20).

We have then addressed falsification tests. That is, we have considered crimes that we do not expect to be directly or indirectly related to sodomy law repeals. We have therefore shown as placebo tests that sodomy law repeals had no impact on the number of arrests for gambling (Figure C21), burglary (Figure C22), or arson (Figure C23). These findings reassure us that our main results in Figs. 1, 2, 3, and 4 are not due to specific time-varying state characteristics which led to general changes across all crimes, but are instead estimating the impact of sodomy law repeals on crimes that we ex-ante expected to be affected.[17]

One could also be the concerned that states whose policies were overturned because of *Lawrence v. Texas* might have had different trends in outcomes before the Supreme Court decision. We address this potential issue by adding linear trends differing for states affected by *Lawrence v. Texas* in the difference-in-differences regression model to allow differences in trends. As Table C11 shows, our main conclusions do not change.[18]

In addition, in view of Chang (2021)'s finding emphasizing the relation between sex ratios and sodomy laws, there might be the concern that sex ratios might be also driving our results. To address this issue, Table C15 augments the main difference-in-differences specifications presented in Table 2 by adding yearly sex ratios at the state level as a control variable. It is reassuring to find that the main estimates are not substantially altered by the inclusion of this control: the estimated coefficients for sodomy law repeals are actually more precise after the inclusion of sex ratio among the list of controls. The coefficient of sex ratio is statistically indistinguishable from zero in all specifications, and the adjusted R^2 remains unaltered between Table 2 and Table C15, thus suggesting that sex ratios may have played a smaller role in the context of sodomy law repeals in the USA for the time period considered in this analysis than in other countries or earlier time periods.

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[17] To further address concerns on potential measurement errors in the UCR data, we have used the number of agencies as the dependent variable in our difference-in-differences model (Pei, Pischke, and Schwandt 2019). We have found that the estimated coefficient of our treatment variable is statistically insignificant in any regression including or excluding our set of controls. This result suggests that our results are not driven by the number of reporting agencies varying over time.

[18] Similarly, Table C12 modifies the main difference-in-difference models in Table 2 by excluding states that repealed their sodomy laws only through the Supreme Court decision in 2003, while Table C13 excludes states that repealed their sodomy laws between 1996 and 2002. In both tables, estimates for prostitution, disorderly conduct, and driving under the influence remain statistically significant. The regressions for sex offenses have a negative coefficient associated with sodomy law repeal but lacking statistical significance. Yet, it is important to note that this lack of statistical significance is not due to a decrease in size of the point estimate but rather to a widening of standard errors. This suggests that this change might be ascribed to a loss of precision due to the reduction of sample size since these regressions make use of restricted samples. All regressions display estimated coefficients similar in magnitude to those in Table 2 across outcomes and with overlapping 95% confidence intervals. It is also worth mentioning that arrest rates in 1995 between states that repealed sodomy laws between 1996 and 2002 and states that repealed such laws only after the Supreme Court decision in 2003 are not statistically different from each other's (Table C14). Furthermore, states in these two groups do not appear to differ in term of observable state characteristics or other LGBTQ+ policies.

7 Conclusions

This paper has provided the first evidence that sodomy law repeals had an economic meaningful impact: they led to a reduction in the number of arrests due to sex offenses, prostitution, or disorderly conduct, as well as a decline in arrests linked to alcohol and drug consumption. Even if there are no data available to explore all potential mechanisms, our results on driving under the influence strongly suggest improvements in mental health being a key channel behind our results.

These findings are important from a policy perspective. Institutionalized homophobia is still prevalent worldwide: as of 2020, 70 countries have laws criminalizing homosexuality. In 11 of these countries, homosexuality is punishable by death (ILGA 2020). There is evidence from several countries that sexual minorities are discriminated in the labor market (Drydakis 2011; Hammarstedt, Ahmed, and Andersson 2015; Brown, Contreras, and Schmidt 2019), have lower income (Curley 2018), are more vulnerable to poverty (Schneebaum and Badgett 2019), and often lack health insurance (Carpenter, Gonzales, et al. 2021a, b). This study is a first step towards helping international institutions to evaluate more accurately the costs and benefits of pressuring or suspending foreign aids to countries in blatant violation of basic human rights (Economist 2014; Steer 2018). Furthermore, this analysis emphasizes the potential benefits from repealing sodomy laws still standing in several countries.

One may be rightly worried about external validity; however, it seems reasonable to believe that in any country in which there is a social movement protesting against the criminalization of same-sex sexual intercourse and/or where such laws are enforced and used by police agents to target sexual and gender minorities—as it was in the US case—the decriminalization of such sexual acts would lead to a fall in offenses committed. Yet, future research might explore whether this is the case using data from developing countries and other countries that might appear considerably different than the USA. However, at the moment, the evidence from this study is the only available one due to data limitations in other countries.

As already discussed, one limitation of this study is due to the potential measurement errors, inconsistent reporting, and the hierarchical rule used for classifying arrests by type of crime. In addition to the robustness checks implemented in the previous sections to address these concerns, it is worth mentioning that the UCR data remain the best data available to estimate the impact of sodomy law repeals on arrest and crime rates. A second limitation of this study is due to the lack of exact data on the number of arrests specifically for same-sex sexual intercourse in each year and in each state before the repeal of sodomy laws. Without such data, it is not possible to test whether the impacts of such repeals were larger in states in which police activity was more targeted against sexual minorities before the law changes.

To further expand our understanding of the consequences of sodomy laws, future research could investigate the effect of the decriminalization of same-sex sexual acts on other outcomes such as labor market outcomes and incidence of sexually transmitted diseases.

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Declarations

Conflict of interest The authors declare no competing interests.

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