Prisons and Homophobia[*]

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Abstract

Inmates' informal code often ascribes low status to persons perceived as passive homosexuals. We investigate whether prisons contribute to homophobia in the general population using an exogenous shock of the Soviet amnesty of 1953, which released 1.2 million prisoners. We document the spread of prison norms in localities exposed to the released ex-prisoners. In the long run, we find effects on anti-LGBTQ+ hate crimes, homophobic slurs on social media, and discriminatory attitudes. Using contemporary longitudinal data, we show that prison experience prompts a higher level of anti-gay sentiments among males and their families, while no discernible difference exists before incarceration.

Keywords: Tolerance, Homosexuals, Incarceration, Prison Culture, Russia, Australia
JEL codes: J15, M14, N34, N44, P00, Z13.

1 Introduction

Society-wide norms and values can be explained by history[1] (Nunn, 2021), current policies (Bau, 2021), or economic development (Inglehart and Welzel, 2005; Inglehart, 2018). Additionally, norms and culture are shaped by the *social organizations* with which people interact and participate, including schools, universities, businesses, bureaucracies, and notably, prisons. Such organizations have the potential to form their distinct norms and transmit them to the general population both vertically (participants socializing with younger generations of their families) and horizontally (participants influencing their non-participant peers). Sociologists have documented one of such norms in male prisons: self-governance by the informal code (Clemmer, 1940; Sykes and Messinger, 1960; Skarbek, 2014). It privileges an informal hierarchy and ascribes low status to people perceived to be passive homosexuals (Sykes, 1958; Einat and Einat, 2000) thus potentially leading to the creation and transmission of anti-gay norms. Given that there are currently more than 11 million prisoners worldwide and that world incarceration rates increased by 8% in the last ten years (Walmsley, 2019), it is possible that prisons can be an important source of homophobia in the general population.[2] This effect can result from first-hand experience of ex-prisoners, exposure of their families, and gradual socialization of larger parts of the population into prison culture.

In this paper, we investigate empirically whether prisons serve as a source of society-wide norms. We use the largest prison amnesty in history — Soviet amnesty of 1953 — to evaluate the impact of the influx of people with prison experience on anti-gay attitudes and behaviors. Caused by the unexpected death of Soviet dictator Joseph Stalin, the amnesty resulted in 1.2 million people who were convicted for general criminal offenses being released and settled in the proximity of Gulag labor camps.[3] We measure the exposure of each Russian municipality to the amnesty of 1953 as the sum of the number of released persons from all Gulag camps weighted by the distance from each camp to the municipality. Then, we first show that the amnesty immediately spread the prison culture to nearby areas, as measured by the increase in the number of "thieves-in-law" (individuals responsible for adjudicating disputes related to the prison code). Second, we find that areas affected by the amnesty exhibit more hate crimes against LGBTQ+ people, greater intensity of homophobic slurs on social media, and more homophobic attitudes of individuals measured by representative surveys. We augment this analysis with modern longitudinal data from Australia showing that (i) males who go to prison became more intolerant toward homosexual individuals, and (ii) that the intolerance further spreads to the members of their households.

The question of the impact of prisons on homophobia is extremely difficult to study. First, the places where former inmates live after they leave prisons are decidedly non-exogenous since ex-prisoners are likely to return to their previous place of living. Second, it is hard to study cultural change because the number of ex-prisoners in any given location is relatively slow compared to the general population. Finally, the

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[1]In particular by pre-industrial agriculture (Alesina, Giuliano and Nunn, 2013), environmental risk (Giuliano and Nunn, 2021), tightness of kinship networks (Enke, 2019), political self-governance (Guiso, Sapienza and Zingales, 2016), religious institutions and teachings (Becker and Pascali, 2019; Bergeron, 2020; Henrich, 2020), migration (Becker et al., 2020; Miho, Jarotschkin and Zhuravskaya, 2023), and sex ratios (Grosjean and Khattar, 2019).

[2]For example, in the United States, the country with one of the largest incarceration rates in the world, prisons on average admitted 437,000 people per year in 2009–2019 according to the Bureau of Justice Statistics (https://bjs.ojp.gov/).

[3]Russian acronym for the "main administration of the camps."

longitudinal survey data that tracks the incarceration status of individuals, as well as their attitudes towards gay persons, is scarce — this complicates the study of the impact of incarceration on individual anti-gay attitudes and on the anti-gay attitudes of their family members. In this study, we make advancements in all these directions.

To study the transmission of prison norms, including anti-gay attitudes, to the general public one needs to find an episode of an exogenously determined influx of people with prison experience into the population. One such episode is the Soviet amnesty of 1953. Soviet dictator Joseph Stalin died suddenly and unexpectedly after a stroke in 1953. A power struggle within the Soviet elite ensued and resulted in the amnesty of 1.2 million people in the following three months (Hardy, 2016). Many of the released prisoners stayed in nearby cities and towns (Dobson, 2009).[4] No reform of the Gulag system had been planned and arguably no amnesty would have been implemented if Stalin lived (Barnes, 2011). We hypothesize that, through the interactions of the ex-prisoners with local populations, a rapid increase in the number of people with prison experience may have a long-lasting effect on the presence of prison culture in the locations most exposed to the amnesty.

Prisoners abide by certain behavioral codes and form a strict internal hierarchy. This creates an informal institution for resolving disputes among the prisoners where higher-ranked individuals have more rights. Individuals in higher social strata are forbidden from sharing tableware, making physical contact, or taking items from those in the lowest strata, as such actions carry the potential of their own descent into the lower strata (Mironova, 2023). It is well-documented that passive homosexual individuals are at the bottom of the hierarchy (Clemmer, 1940; Sykes and Messinger, 1960; Skarbek, 2014).

We measure the presence of prison culture both historically and in modern days. Historically, we make use of a unique feature of Russia's context, namely, the presence of specific strata of criminals: thieves-in-law (*vory v zakone*). Upon leaving the prison, criminals may still live according to the prison norms, and when the demand for dispute resolution arises, an arbitrator is needed. In the Soviet Union, thieves-in-law were the ones who took this role (Lilin, 2010; Galeotti, 2018). They were usually selected from among the most respected members of the criminal community with a formal procedure of ascendance to this status. Their "coronation" made the criminal communities in all Soviet Union aware of the emergence of a new thief-in-law.[5] We argue that the rise of a thief-in-law in a particular location is a signal of a growing prevalence of prison culture in the society. We construct a panel dataset of the emergence of Russian thieves-in-law from 1922 to 2010 using textual data on their biographies from Prime Crime News Agency, an online resource on the Russian criminal community widely used in crime research and followed by criminals themselves (Lonsky, 2020; Varese, Lonsky and Podvysotskiy, 2021).

In contemporary settings, we use three measures of homophobia — a most salient by-product of prison culture. First, we use geo-coded data on crimes against LGBTQ+ persons from Kondakov (2017, 2021), who meticulously collected the data on cases in 2010–2015 in which the motive of hate against LGBTQ+ persons was established by a court. Second, we have scraped the most popular social network in Russia, *vk.com*, for the geo-referenced public postings containing common Russian homophobic slurs. Third, we use three geo-

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[4]The amnesty only applied to people who had been convicted for fewer than five years. Importantly, political prisoners, who were convicted for "counter-revolutionary activity" were not eligible for the amnesty because most of them had been convicted of more than 5 years of imprisonment (Shalamov, 1989).

[5]The "law" in "thieves-in-law" refers to the criminal informal code, not to the laws of the state.

referenced public opinion surveys — Life in Transition Survey, World Values Survey, and the Courier Survey by Levada Center — that contain questions about respondents' attitudes towards homosexual individuals.

We regress all of our outcomes on the exposure to the 1953 amnesty. We measure a location's exposure to amnesty using the sum of the number of released prisoners from *all* Gulag camps weighted by the distance from each camp to the location. While the location and the number of prisoners in Gulag were determined systematically, the amnesty was unexpected and chaotic (Hardy, 2016). We test this by (i) showing that the amnesty is not correlated to pre-1953 levels and changes in Gulag population or its industrial composition and (ii) showing that exposure to amnesty is uncorrelated with the pre-1953 changes and levels in the number of coronations of thieves-in-law, manufacturing output, population, sex ratios, or shares of the religious population.[6]

We first present evidence of the impact of the amnesty on the number of thieves-in-law coronations. Using an event-study design we show that locations more affected by the amnesty had a higher number of thieves-in-law coronations after 1953 but not before. The effect increases over time suggesting a self-reinforcing nature of the norms. On average, one standard deviation increase in exposure to the amnesty increases the number of coronations by 6 percent. Then we study the impact of the 1953 amnesty on modern-day homophobia in a cross-section. To further address endogeneity in the location of camps, we control for the coordinates, minimum distance, size, the convict-labor industries of the nearest Gulag camp, and total exposure to the prison population of the Gulag system. We find that exposure to 1953 amnesty is positively associated with all measures of present-day homophobia. A one-standard-deviation increase in the exposure to the amnesty of 1953 leads to a 13.3-percentage-point increase in the probability of a respondent being intolerant toward gay persons, a 2.7-percent increase in the number of hate crimes, and a 1.1-percent increase in the number of homophobic slur in social media.

Our results are robust to (i) controlling for the population, municipality type, regional fixed effects, and (for survey data) individual-level socio-economic and demographic controls, (ii) usage of the alternative measures of exposure to the amnesty, (iii) usage of the alternative (larger) unit of observation — county (*rayon*) — instead of municipality, (iv) usage of alternative spatial HAC standard errors, and (v) are not driven by a particular province. We document that our results are unlikely to be explained by any other factor by using a series of placebo estimates. We first show that exposure to the amnesty of 1953 yields a larger effect than exposure to any other change in the number of prisoners in the history of the Gulag. Second, we permutate amnesty size and labor camp location and find that the true estimates are always within the top 10 percentile of the magnitude of the effect. We also find no effect when using exposure to amnesty from female labor camps. Using survey data we confirm external validity by documenting the effect of the amnesty of 1953 on the homophobic attitudes in other ex-USSR countries with Gulag camps. We also document the effect on a contemporary measure of prison culture: consumption of music genres associated with the criminal underworld.

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[6]Note, that according to the qualitative literature on the matter (Healey, 2001, 2017) — Russian society had been tolerant of homosexual expressions before the Gulag was established. Even after male homosexuality was criminalized by Stalin in 1935, the enforcement had been sporadic and unsystematic until the 1950s. And the amnesty was a drastic and exogenous change in the number of released prisoners. Thus, our claim that Gulag contributed substantially to Russian homophobia is consistent with the qualitative evidence. Section 2 explores this issue in more detail.

We find no evidence that our results are driven by (i) the proximity to Gulag camps, (ii) possible economic (under)-development due to exposure to amnesty, (iii) crime rates, (iv) biased sex ratios or attitudes toward women, (v) other possible confounding factors such as religion, army, or migration, or (vi) deterioration of trust and social capital. Thus, we argue that the most plausible channel of the effect of amnesty on homophobic attitudes is exposure to prison culture. We provide suggestive evidence of this mechanism by showing the effect of the cumulative number of thieves-in-law and having a family member going through the Gulag.

One caveat of our analysis in the Russian context is that we don't have individual-level information on Gulag experience, current location, and homophobic attitudes to pinpoint the culture transmission mechanism. As a supplementary exercise, we use contemporary longitudinal survey data at the individual level from Australia. In particular, we use the Household, Income, and Labour Dynamics in Australia (hereafter, HILDA) survey to explore whether people who return from prison end up with a higher level of anti-gay attitudes than before incarceration. This longitudinal survey is conducted every year since 2001, and it allows us to use within-person variation from a non-trivial number of individuals who were incarcerated during this period. It also allows testing if the family members of incarcerated individuals change their attitudes towards gay persons. The survey is rich enough to allow controlling for age-, cohort-, and time-specific trends in a variety of socio-demographic and heritage characteristics as well as testing for pre-incarceration differences in anti-gay attitudes.

We find that incarceration decreases male respondents' level of acceptance of gay persons having equal rights by a 0.28-standard-deviation. At the same time, the effect on female ex-prisoners is smaller in magnitude and insignificant. We also document the spread of the attitudes to the family members: having a close-family member returning from prison decreases the approval of equal rights for gay people by a 0.14-standard-deviation. We find that these results are consistent with the influence of prison culture on homophobia specifically and not consistent with (i) pre-trends in homophobic attitudes, (ii) changes in attitudes toward women, (iii) changes in distrust toward out-groups, (iv) decline in social capital, (v) deterioration of mental health, or (vi) men changing their sexual orientation in prisons. Our results also (a) hold if we use the number of years in prison or the number of incarcerations, (b) are robust to the usage of alternative difference-in-differences estimates, (c) and are not driven by a particular subgroup of the population (age, education, religion, or state of residency), possible social desirability bias in the survey, or other types of measurement errors. Additionally, our results hold if, instead of within-person variation, we use between-person variation in propensity score matching estimation.

Our study makes several contributions. Most immediately, we contribute to the quantitative studies on the determinants of homophobia. This is an important question since discrimination against LGBTQ+ persons is still present in many parts of the world (Valfort et al., 2020) and is consequential for labor market participation (both on supply (Sansone, 2019) and demand side (Aksoy, Carpenter and Sansone, 2022)), wage gap (Aksoy et al., 2019), contributes to inequality in healthcare services (Saxby, Sonja and Petrie, 2020), and limits pro-social behavior (Aksoy, Chadd and Koh, 2021).[7] Studies in this literature have identified several

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[7]See Badgett, Carpenter and Sansone (2021) for a systematic review of studies on wage disparities and labor market discrimination of LGBTQ+ persons.

factors contributing to the anti-gay norms: historical bias in sex ratios (Baranov, De Haas and Grosjean, 2018; Chang, 2020; Brodeur and Haddad, 2021), historical religious missions (Ananyev and Poyker, 2021), modern Renewalist Christian denominations (Grossman, 2015), lack of legal recognition of same-sex marriage (Aksoy et al., 2020), and AIDS epidemic and scapegoating of LGBTQ+ persons (Fernández, Parsa and Viarengo, 2019). We propose a new potential source of homophobic attitudes — prisons and penitentiary policies — and offer several quantitative tests for this hypothesis.

More generally, we contribute to the literature on cultural change and persistence (summarized in Giuliano and Nunn, 2021). The factors of cultural changes explored in the literature include religion (Becker and Pascali, 2019; Bergeron, 2020; Henrich, 2020), slavery (Nunn, 2008; Nunn and Wantchekon, 2011), and historical stability of the environment (Giuliano and Nunn, 2021). We contribute to the discussion on the horizontal and vertical transmission of culture (Richerson and Boyd, 2008; Spolaore and Wacziarg, 2009; Aghion et al., 2010; Bisin and Verdier, 2011; Henrich, 2017) by providing an argument that mass imprisonment is also a significant determinant of norms and beliefs working through both channels. One of the close papers to ours is Miho, Jarotschkin and Zhuravskaya (2023), which finds that gender norms from ethnic Germans and Chechens deported by Stalin during World War II diffused with those of the local population. In this paper, we study the internal diffusion of new cultural norms from social organizations to the general population rather than external diffusion from (forced) migration policy.

Additionally, we contribute to the literature on the effects of prisons on the convicted individuals (Pager, 2003; Kling, 2006; Agan and Starr, 2018 on employment, Mueller-Smith, 2015; Dobbie, Goldin and Yang, 2018 on employment and recidivism, Aizer and Doyle Jr, 2015 on high school completion, Hjalmarsson and Lindquist, 2022 on health) as well as on their household members (Dobbie et al., 2018; Norris, Pecenco and Weaver, 2021 on outcomes of their children) and the larger society (Rose and Shem-Tov, 2019 on crime rates). Here we show the effect of incarceration on changes in the cultural norms of prisoners, their families, and larger societies, both in the short-run and in the long-run.

This paper also contributes to the literature on the effects of Stalin's repressions in the Soviet Union and Russia (summarized in Zhuravskaya, Guriev and Markevich, forthcoming). The literature has focused on long-term effects such as voting, economy, and trust (Mikhailova, 2012; Kapelko and Markevich, 2014; Toews and Vézina, 2020; Nikolova, Popova and Otrachshenko, 2022) and short-term effects such as famine (Markevich, Naumenko and Qian, 2021; Naumenko, 2021). Here, we show that homophobia in Russia is at least partly a Gulag legacy using both short-term and long-term evidence.

This paper proceeds as follows. Section 2 describes sociological theories on the relationship between homophobia and prison culture and introduces background information about homophobia in Australia, Russia, and the amnesty of 1953. Section 3 describes our data. Section 4 introduces our empirical specification, identifying assumptions, and results of the effects of the amnesty of 1953 on coronations of thieves-in-law and homophobia in Russia. Section 5 introduces our individual longitudinal data specification and studies the first-order effects of prisons on ex-prisoners and their household family members. Section 6 concludes.

2 Background: Homophobia, Prison Culture, and Amnesty of 1953

2.1 Homophobia and Prison Culture

How exactly might male prisons be producing homophobia? The most important potential channel is prison culture and inmate code. As Dolovich (2012) documents for the U.S. prisons, a set of norms emerge that privilege competition for status and power in an informal hierarchy. Such norms have been also documented in the Soviet underworld (Galeotti, 2018).[8] In such environments, qualities that are stereotyped as "feminine" are despised, and "passive" homosexuals are perceived as woman-like.[9] It has also been documented that in many cases, homosexual acts involve violence and coercion (O'Donnell, 2004).[10] Anti-gay norms have also been documented (Clemmer, 1940; Sykes, 1958; Sykes and Messinger, 1960; Einat and Einat, 2000; Skarbek, 2014) in male prisons around the world, e.g., in Australia, Israel, United States, etc.

Figure 1 − Countries With Larger Prison Population Are More Homophobic
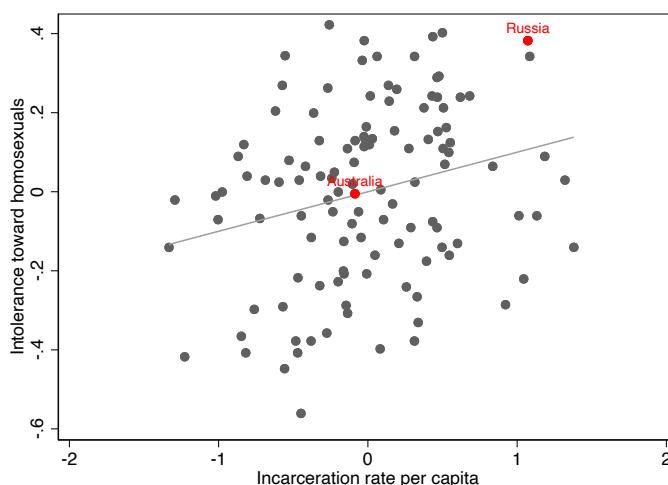
Notes: This Figure shows a residual plot from the country-level regression of incarceration per capita on intolerance toward homosexuals. The regression coefficient is 0.100, robust standard errors are 0.035, and the p-value is 0.005. The prison population in 2019 is from World Prison Brief (accessible at PrisonStudies.org). Intolerance toward homosexuals is from the 2019 Gallup World Poll. The question used in the Gallup survey is as follows: "Is the city or area where you live a good place or not a good place to live for gay or lesbian people?" The variable is constructed as the share of people that answered "Not a good place." Russia is in the upper-right corner. Australia is on the linear fit line in the center.

It is important to note that masculinity norms are not creating homophobia in prisons. Masculinity norms have been shown to proliferate in the contexts of intense male-to-male competition for status and

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[8]Galeotti (2018) documents that an aspiring member of a criminal organization had to undergo a set of highly risky, but largely performative acts, such as stealing a coat from a Chechen restaurant.

[9]Varlam Shalamov writes in Swindler's Blood: "The criminals [blatari] are all pederasts. Each of them in the camp is surrounded by young people with swollen and muddy eyes 'Zoikas,' 'Man'kas,' 'Verkas,' whom the criminal is feeding and with whom he sleeps" (Kuntsman, 2009). It should also be noted that an "active" partner in such relationships is not perceived as a homosexual and thus does not carry the stigma.

[10]Some scholars of prison informal order (Fleisher and Krienert, 2009) have pointed out that sexual relationships between men in prisons often do not involve violence and are not described as rape by inmates. However, according to Trammell (2011), homosexual relationships between men can sometimes be described as a "protective pairing," where a "husband" provides protection to a "weak" and "vulnerable wife." Thus, a person who goes through a prison experience arguably can be socialized into ascribing low status to "passive" homosexuals and expressing anti-gay attitudes later on.

scarce resources (Baranov, De Haas and Grosjean, 2018). While prisons may be viewed as settings where such competition can take place, the distinctive characteristics of male prisons, such as their regimented lifestyle and the absence of women, do not contribute to the set of norms associated with hegemonic masculinity. Most notably, the inmate code is primarily concerned with regulating differences in behaviors *between* different strata of men rather than prescribing the appropriate male conduct.[11] For example, the explicit goal of the inmate code is to limit violence and risk-taking by inmates within the followers of the code. The punishment for transgressions is often denigration to the lowest status equal to that of "passive" homosexuals (Mironova, 2023).

To demonstrate suggestive evidence that mass incarceration might be linked to homosexual attitudes, in Figure 1, we show a residual plot from a cross-country regression between the incarceration rates per capita from World Prison Brief and the respondents' evaluation of how welcoming their locations are for gay and lesbian individuals from Gallup World Poll data. We find that in the countries with higher incarceration rates, Gallup respondents are more likely to say that their locations are "not a good place" for gay persons: a 100-percent increase in incarceration rate per capita is associated with a 10-percentage-points increase in homophobia or its 0.37-standard-deviation. While such a graph, presented here for illustrative purposes, can suffer from a number of sources of endogeneity (such as economic development, history, and other aspects of culture), in the following sections we present a set of tests — with Australian and Russian data — that arguably permit more definitive causal conclusions.

2.2   Attitudes Toward Gay Individuals in Russia

According to representative surveys, the level of anti-gay attitudes in Russia is one of the highest in the world: 67 percent of World Values Survey respondents of the 2017–2020 wave in Russia stated that they would not like to have homosexual individuals as neighbors, only 12 percent agree that homosexual parents are as good as the heterosexual ones, and 58 percent of individuals say that homosexuality is never justifiable.[12] According to the human rights watchdog "SOVA Center," 16 people were beaten in 2020 for the reasons of anti-LGBT hate, while in 2019, 7 people were beaten and one person was killed. LGBTQ+ persons are routinely publicly insulted by politicians and celebrities.[13]

How deep are the roots of such attitudes? Recent historical research suggests that even though Orthodox Christianity considers homosexuality sinful, before Stalin's time it was not particularly stigmatized, and Gulags' prison culture became one of the main sources of homophobia in post-Stalin Russia (Healey, 2001,

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[11]According to Mahalik et al. (2003), masculinity norms are comprised of violence, winning, risk-taking, emotional control, playboyism, primacy of work, disdain towards homosexual individuals, dominance, self-reliance, pursuit of status, and self-reliance. Out of these norms, the inmate code does prescribe disdain towards homosexuals. As for the other norms, it is either silent or dictates the opposite. The code also does not prescribe which beliefs inmates must hold regarding the appropriate role of women in society.

[12]Such a high level is not explained by the recent legislation prohibiting "homosexual propaganda," since as early as 2006 (WVS, 5th wave) it was on the same level: 66 percent of Russian respondents said that would not like to have homosexual persons as neighbors then.

[13]It is important to point out that masculinity norms caused by male-biased gender ratio cannot explain the prevalence of homophobia in modern-day Russia. In general, Russia did not suffer from male-biased gender ratios. In fact, after World War II, in many regions, the sex ratios were female-biased due to the war casualties (Brainerd, 2017). Before that male population either disproportionately died during World War I and the Civil War or migrated out as soldiers of the White Army who flew the country after defeat in the Civil War. As can be seen in Table A.1, the average women-to-men ratio in 1959 — 14 years after the end of WWII — is 1.23; still skewed.

2017). According to Healey (2001), Russia imposed anti-sodomy laws later than Western European countries. Peter I forbade "sodomy" in 1716 but only in the army and navy. Civil anti-sodomy laws were first introduced in 1835 during the rule of Nicholas I; however, the punishment for it was only introduced in 1866.[14] Female same-sex relationships had never been criminalized. The criminalization of "sodomy," however did not change much in the culture and such offenses were rarely enforced. Russian society in that period was quite tolerant of the expressions of homosexuality. Criminal charges of "sodomy" in cases involving voluntary same-sex relationships were usually dropped without a trial. When such cases did reach a trial, judges were inclined to acquit the accused or to appoint relatively lenient punishment without a jail sentence. If the homosexual acts were found to be involuntary, then the accused was charged with both "sodomy" and sexual assault.

After the revolution of 1905, with the surge of *all* criminal convictions by 35% the number of people convicted for sodomy also increased. In total, in 1905–1913, 96 people were convicted for voluntary "sodomy" and 408 for involuntary "sodomy." Most of such cases, however, came outside the territory of modern Russia. Instead, they came from the territory of modern Ukraine and the territory of modern republics of the Caucasus and Central Asia. One of the suggested explanations was that such cases were fabricated by the police to arrest political dissidents, especially pro-independence campaigners. The Bolshevik revolution of 1917 was followed by the Golden Age of Russian queer culture with gay weddings (although not officially recognized) and regular cross-dressing parties. Homosexuality was entirely legal during this period. Stalin criminalized homosexuality in 1935, but the enforcement, as in the pre-revolutionary period, was rare. Figure 2 shows the number of "sodomy" convictions in the Soviet Union (solid line) and their share in the total number of convictions (dashed line): they surged in the 1950s, strongly hinting at the role of the Gulag system in promoting homophobic attitudes.[15]

In the Gulag camps, a hierarchical system emerged which consisted of several groups or "castes" (Abramkin and Chizov, 1992).[16] On the top were "blatnye," professional criminals with a high level of authority in charge of dispute resolution and overall management of the informal economy inside the camp. The biggest part of the prison population were "muzhiki" ("commoners") who had no voice in the dealings of the "blatnye." The lowest caste were "petukhi" ("roosters"), the untouchables with the reputation of being "passive" homosexual persons. Many individuals in this category ended up there because they were "punished" for transgressions by a sexual assault from another inmate, often informally sanctioned by the camp's administration. According to historian Irina Roldugina, "Homosexuality ... was closely related to humiliation, subordination, and violence. This system of violence and fear was beneficial for the camps' administration because it cemented their power."[17]

Overall, if a non-homophobic person ends up in prison where he observed roosters being untouchable and maintained these homophobic norms himself to not become a "rooster" himself, he may remain homophobic

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[14]The punishment was retracting of the titles (i.e., estates) and exile in Siberia. In 1900, the exile was replaced with 4–5 years in prison.

[15]At the same time, the state did not necessarily participate in the homophobic propaganda itself. We have counted only 21 mentions of gay issues (mostly unfavorably mentions of gay rights activism in the U.S. and other Western countries) in the Izvestia and Pravda newspapers in 1917–1991. The source is East View Information Services: https://dlib.eastview.com/browse/publication/9305/udb/870/.

[16]The term "caste" here is used by the scholars of this topic only metaphorically and no deep analogies with the Indian caste system is implied.

[17]Wonderzine.com: "From Stalin to "Petukhi": Why Russian Men Fear Anything Gay." URL: www.wonderzine.com/wonderzine/life/life/233347-homophobia.

even after leaving the prison.

Figure 2 – Number of Sodomy Convictions and Their Share in the Total Number of Crimes in Russia (RSFSR), 1935–1981
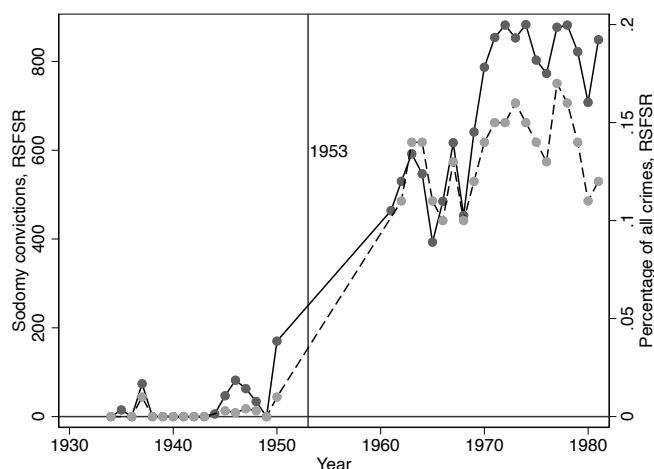
Notes: This Figure shows with a black line the number of convicted individuals under the sodomy laws in the Russian Soviet Federative Socialist Republic (RSFSR). The gray dashed line shows their share in the total number of convictions in RSFSR in that year. Data for 1951–1960 are not available. The share of sodomy convictions in the total number of convictions in 1961 is also not available, but for the whole USSR the total number of sodomy convictions was 705 and their share was 0.09%. Source: Tables 1 and 2 of Healey, 2001, Appendix, pp. 261–262.

2.3 Amnesty of 1953 and Its Aftermath

To investigate the impact of prison experience on homophobia among the population, we use the Soviet amnesty of 1953 which dramatically downsized the system of the labor camps in the Soviet Union. This system emerged right after the Bolshevik revolution and started expanding dramatically after Stalin had taken power in 1929. At its peak, it had grown to comprise 475 labor camps. By 1953, approximately 18 million people had passed through the system (Applebaum, 2003). Gulag was officially dismantled in January 1960.

After the death of Stalin in 1953, a power struggle within the Soviet elite ensued. Soviet Minister of Internal Affairs, Lavrentiy Beria, launched a campaign of reforms to Soviet law enforcement and the Gulag system. A part of Beria's proposed reform package was a sweeping amnesty. Beria argued that the Ministry of Internal Affairs should be free of its "economic responsibilities" (Elie, 2013). Some suspect that Beria advocated for the amnesty for political reasons (Solzhenitsyn, 1974), while others point out that the Gulag system became bloated and unmanageable (Galeotti, 2018). While Beria himself did not survive the post-Stalin power struggle (he was arrested and executed), his idea was implemented: 1,201,738 prisoners were freed from convict labor camps in 1953.[18]

Despite the amnesty's ambition, its execution was poor. Uncertainty in the rules about who is supposed to be free led to many career criminals being released. The released individuals were not offered any transportation options to their pre-conviction places of residence so they stayed in the nearby areas prompting

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[18]https://urokiistorii.ru/history_days/berievskaja-amnistija.

the surge in criminality in those places. For example, by June 1, 1953, 5,500 released individuals arrived in the Siberian city of Omsk. In the weeks after that, a wave of assaults followed (70 people were admitted to hospitals with knife wounds). Similar events were happening throughout the country, and the government largely lacked the capacity to intervene (Mamin, 2018).

The first-order effect of amnesty was the rise in prominence of a specific stratum inside the criminal community — thieves-in-law — who was in charge of maintaining the inmate code.[19] The inmate code had crystallized earlier and can be traced to the aftermath of the Russian Civil War and even earlier times (Galeotti, 2018), but the Gulag system changed it significantly. One of the most important factors was the so-called "bitch war" (*suchya voina*): a series of violent clashes between two groups of criminals: one of the groups (*vory*) saw itself as upholders of the old inmate code, while the other was accused of collaborating with the Soviet government and prison administration (*suki*). The war ended in 1953 with the *suki* prevailing. Nevertheless, they largely adopted the old *vory's* code but made it more stringent when it comes to the perceived "passive" homosexuality. Specifically, elaborate rules of "cleanliness" were adopted: perceived "passive" homosexual persons had to be segregated, use separate cutlery and dishes, and their belongings were never to be touched by others. The violators of this rule ran the risk of being shunned themselves (Mironova, 2023). In the aftermath of the process, the thieves-in-law solidified as a class of individuals in the criminal community whose role was to uphold the rules and resolve informal disputes. Bitch wars are important for us as they officially *institutionalized* homophobia in communities living according to prison norms. Hence, we use coronations of thief-in-laws as a proxy for both the prevalence of prison culture and its most salient feature — male homophobia.

In this paper, we use the location-level exposure to amnesty as an exogenous shock. We hypothesize that the released individuals bring their networks and norms, including anti-gay attitudes, with them. As they settle in their new homes, they gradually start to influence the attitudes of the local population due to the high visibility of their activity and immersion in economic, social, and family life.

2.4 Attitudes towards Homosexuality in Australia

The argument outlined earlier implies that even people who do not harbor anti-gay attitudes, when incarcerated are becoming more homophobic. Whether this is actually true is an empirical question that we address in this paper using not the Russian context, but the Australian one.

The main reason for looking at Australia is the availability of high-quality longitudinal survey data on attitudes toward gay marriage as well as the respondent's incarceration status. With these survey data, we can observe if people who get into prisons end up less supportive of marriage equality for gay persons. To the best of our knowledge, the survey data that trace peoples' attitudes towards homosexual individuals along with their incarceration experience are not available anywhere else.

As Australia traces its statehood to England's penal colony established in 1788, its legislation regarding homosexuality mirrored those of England as homosexual men were prosecuted under the "anti-buggery"

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[19]The systematic data on the actual crime rates (or homicide data) following/before the amnesty are not available but it is reasonable to assume that the overall effect on crime was ambiguous. The arrival of career criminals contributed to the criminal activity in the area but the dispute resolution mechanism provided by the thieves-in-law limited potential inter-gang violence (Siegel, 2012).

laws. After World War II, homosexuality was largely viewed by politicians and the public as a moral failure (Wotherspoon, 1989). After the end of the Cold War, the opinion started to shift with the process culminating in the legalization of the right to marry for homosexual couples in December 2017. This legislation followed the nationwide postal survey on the matter where 61.6 percent of Australians voted in favor of the legalization of same-sex marriage.

Despite the significant differences between the Australian context and the Russian one, fear of being labeled a homosexual is an important part of Australian prison culture (Richmond, 1978) as well as non-consensual male-to-male sexual intercourse and intercourse for protection (Richters et al., 2012). Given this evidence, we expect that people who are exposed to prison culture might end up less supportive of marriage equality for gay couples. It should be noted, however, that the incarceration rate in Australia is only 0.2 percent of the population. Thus it is reasonable to expect that such norms would spread only to the immediate family members but are unlikely to influence society at large.

3   Data

3.1   Data on Gulag

The information on the locations of Gulag camps comes from Mikhailova (2012), who uses the data collected by the Russian non-government organization "Memorial." Researchers of "Memorial" had compiled the locations and yearly estimates of number of prisoners for 462 out of 475 Gulag camps located in the Soviet Union.[20],[21] For every camp, we take the difference between its population between 1954 and 1953 to estimate the number of pardoned prisoners from each labor camp.

To grasp the scope of the Gulag system Figure A.1 shows the map with camp locations and the total number of people that pass through each camp between 1923 and 1960. During that period, more than 20.8 million people went through Gulag; of them, 1.7 million died. Figure A.3 shows the time-series of the population of the Gulag system, where we can see that it spiked in 1953. By 1953 only 153 camps were operational (see map in Figure A.2 and the time-series of active camps in Figure A.4). Most of these camps were located on the territory of the Russian Soviet Republic with a few on the territory of the Ukrainian, Kazakhstanian, and Uzbekistanian Republics. An average camp was containing 10,500 prisoners.

The amnesty released approximately 1.2 million ex-prisoners; as a result 93 out of 153 camps existing by 1953 were permanently closed. The average camp released 5,353 prisoners; however, the standard deviation was large — 7,603 released prisoners. 18 camps did not decrease the number of prisoners.[22] The largest

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[20]For 13 camps we know the location but not their population. Because these camps were closed before 1953 it does not introduce measurement error in the number of prisoners amnestied in 1953.

[21]Gulag system had five types of labor camps. For this paper only two types of camps are applicable: (i) correctional labor camps — the largest group of Gulag camps, and (ii) special camps — 12 camps with more harsh work and living conditions. We do not use data on the People's Commissariat for Internal Affairs (NKVD) special camps that were established in the Soviet-occupied Eastern part of Germany in 1945 for interning the local population. These special camps were arranged in the former Nazi camps and were liquidated in 1950. We also do not use screening and filtration camps that were established in the USSR in December 1941 for temporary confinement of Soviet soldiers and the civil population from the Soviet territories occupied by Nazi Germany. These camps were also liquidated in 1950. Finally, we do not count prisoners-of-war (POW) camps. While captive German, Japanese, and their allied soldiers were working in labor camps, those who did not die in their harsh conditions were sent back to their home countries. The first POW extradition happened after 1955's Konrad Adenauer visit to Moscow and was not related to the amnesty of 1953.

[22]Conditional on releasing prisoners, the mean is 6,067 and the standard deviation is 7,824.

release was from the Correctional Labor Camp #16 near Bratsk, Irkutskaya Oblast — more than 47,000 prisoners were released. The map in Figure 3 shows the magnitude of the releases from each existing Gulag camp and Figure A.5 shows the histogram of all releases by labor camp in 1953. Table B.1 demonstrates that the amnesty was uncorrelated to the pre-1953 numbers of prisoners, changes in the number of prisoners, maximum capacity, and average population in labor camps. It was also not correlated to the industries in which the prisoners were employed. Additionally, Table B.2 shows that locations' exposure to the amnesty was uncorrelated to levels and changes in economic and demographic outcomes.[23]

Figure 3 – Number of Amnestied Prisoners in 1953 by Gulag Camp
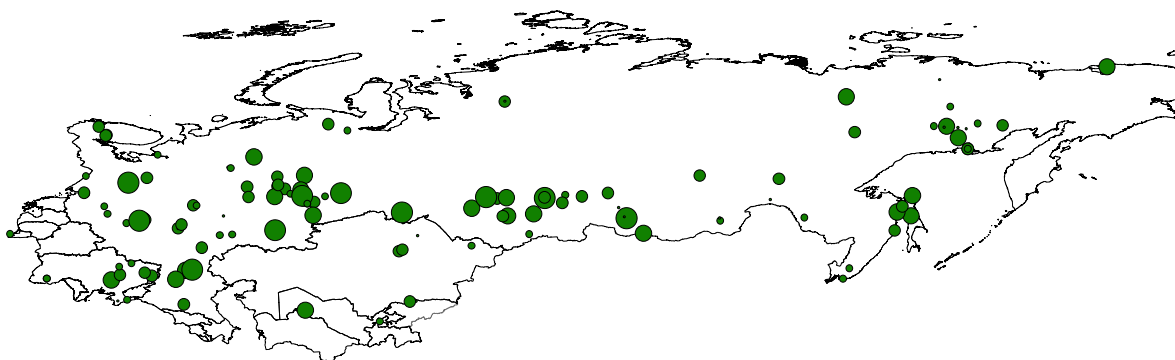
Notes: This map shows the location of 153 Gulag camps on the territory of the former Soviet Union that were operational in 1953. The size of the ball corresponds to the total number of prisoners that were released during the amnesty of 1953. 18 camps did not release any prisoners or slightly increased the number of prisoners. We set the number of amnestied prisoners from these camps to be equal to zero.

3.2 Data on Spread of Prison Culture in Russia

If our intuition on the effects of prison culture on the locations exposed to the amnesty is correct, we should expect an increase in the presence of thieves-in-law (*vory-v-zakone*): a stratum of criminals responsible for resolving disputes and upholding informal "understandings" of Russian inmate code (Galeotti, 2018). To test this hypothesis, we use data on the dates and places of the ascendance of individual criminals to the status of a thief-in-law (so-called, "coronations"). Such coronations represent a significant upward step in the career of a member of the criminal underworld. The process of selecting a new thief-in-law was long and elaborate. As Galeotti (2018, p. 63) puts it: "Candidates had to be well known within their community, with sponsors willing to attest to their being upright exemplars of the criminal code." Such coronations represent evidence of the robust presence of an underworld community as well as the importance of upholding "understandings."

Biographies of thieves-in-law come from the criminal news website http://primecrime.ru. It contains textual biographies of Soviet and Russian thieves-in-law.[24] We extracted the year and location of the coronation of each thief-in-law and removed those that happened outside of Russia (mostly, in Georgia). In

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[23]We discuss the construction of the measure of exposure to the amnesty in the next Section.

[24]According to Galeotti (2018), thieves-in-law are visible members of the criminal underworld. As a validity check, we have found that all thieves-in-law mentioned in Galeotti (2018) are also present on primecrime.ru. Thus we are unlikely to have consequential measurement error.

total, between 1922 and 2010 there were 452 coronations on Russian territory. For the 273 (60%) coronations we only observe the *rayon* of the coronation (the Russian equivalent of a county). For the rest of the 179 coronations, we observe the exact location (i.e., municipality) of the coronation. Because some years have only a few coronations, we bunch together 10 years bins creating *rayon*-decade-level panel.

3.3 Data on Homophobia in Russia

We use three measures to capture homophobia in Russia. All three measures are computed using recent (2010–2021) years. These measures capture different aspects of homophobia and estimating the effect of the amnesty of 1953 on all three of them is important for measuring anti-gay attitudes.

Hate crimes First, we use locations of hate crimes against LGBTQ+ persons collected by Kondakov (2017, 2021) in which the motive of hate against LGBTQ+ persons was established by a court. These data contain all locations that had a hate crime against gay persons in 2010–2015. We were able to uniquely match these locations to our sample of Russian municipalities and create a variable — inverse hyperbolic sine (hereafter, ihs)[25] of the total number of hate crimes conducted in a municipality in 2010–2015.[26]

Homophobic slurs on social media Another way to measure the geography of homophobia is to look at social media. The most popular social media website in Russia is vk.com (also known as "vkontakte"). It has more than 47.2 million users (more than 40% of the Russian internet audience).[27] It is the fourth most popular website in Russia after Yandex (local search engine), Google, and Youtube.[28] Vk.com's application programming interface allows scraping $1,000$ latest public posts by the coordinates of the places of their authors (determined by their Internet Protocol address). Thus, we have scraped those and calculated the ihs of the prevalence of the most common derogatory terms used against homosexual persons.[29]

Attitudes toward gay persons We use five representative surveys of the Russian population from 2010 to 2017 that have a question about attitudes toward homosexual persons and the location of the respondents. Survey data comes from three different sources: 7th wave (2017) of the World Value Survey (WVS), 2nd (2010) and 3rd (2016) wave of the Life in Transition Survey (LiTS), and the Courier survey by Levada Center (the Courier) for 2013 and 2015.[30] While all three organizations that conducted the survey are different, the surveys are representative and have the same wording of the question about the residential attitudes toward homosexual persons.

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[25]We use the inverse hyperbolic sine for such variables as the total number of hate crimes as it can be interpreted in the same way as a standard logarithmic variable but without needing to adjust for zero values (Burbidge, Magee and Robb, 1988). We continue using logs for variables that do not have zero values.

[26]This variable may have a non-classical measurement error. In more homophobic areas, the court can be more homophobic and not count the crime as a hate crime or police may not register such a crime at all. In this case, we may underestimate the number of hate crimes in areas more affected by the amnesty of 1953 and it would work against us finding a positive effect of amnesty on the incidence of hate crimes.

[27]This data may have a bias. Specifically, vkontakte users are young (25-34 years old) and middle-aged (35-64), 26,2% and 25.4% respectively, and are 54.4% women.

[28]See https://popsters.ru/blog/post/auditoriya-socsetey-v-rossii and https://inclient.ru/vk-stats/.

[29]All online data scraped for this paper was obtained on December 14th, 2021.

[30]WVS and LiTS have other waves with questions about attitudes toward homosexual persons but they don't have respondent's coordinates or city name to assign the treatment. The Courier has several other surveys with locations but with different questions on homophobia such as we can't combine them with other surveys.

In WVS and LiTS, the question we use is asked as follows: "On this list are various groups of people. Could you please mention any that you would not like to have as neighbors?" Homosexual persons are one of the groups that are proposed by the questionnaire. We construct our main variable of interest — $\mathbb{1}(\text{Dislike homosexuals}_{i(l)})$ — as a dummy variable equal to one if the respondent i (nested in municipality l) mentions homosexuals, and zero otherwise. In the Courier, the question is asked in a slightly different manner: "Would [you] like having people from this group [Homosexuals] as neighbors, dislike it, or not care?" If a respondent answered that they dislike having gay neighbors, we assign the value of 1 to the $\mathbb{1}(\text{Dislike homosexuals}_{i(l)})$ and 0 otherwise.

The survey question asks only about residential preferences and not labor markets or voting intentions. It is unlikely, however, that residential preferences are unrelated to preferences in other domains. Another potential problem is social desirability bias. Given that homosexuality is currently politicized in Russia in various ways, it is possible that people feel pressured to provide a particular answer. This only matters for our estimates, however, if the willingness to express "true" anti-gay preferences is correlated with the amnesty of 1953. In this case, it is a part of the mechanism: prison culture makes expressing anti-gay sentiments more acceptable (Coffman, Coffman and Ericson, 2017).

Overall our three measures capture three different aspects of the attitudes toward homosexuality and while each of them is limited in scope, together they show the big picture. And while each of them may have measurement error issues, they are of a different nature, thus robust results for all three measures would be indicative that these measurement errors are unlikely to drive our results. Table A.1 provides summary statistics of the Gulag, coronations, homophobia, and other Russian data. Appendix A contains additional details on variable construction.

Apart from these three measures, no other measure of homophobia aspects can be collected for a sufficient number of Russian municipalities. E.g., there is no data on workplace discrimination because national surveys don't have questions about sexual orientation, and data on gay pride expressions is not available because (*any*) public meetings are criminalized and public expressions of pride (e.g., rainbows in vk.com posts) were connected to risks of physical harm even before it was legally criminalized by the government. It is also worth pointing out that we have attempted the collection of mentions of gay issues in the Soviet newspapers Pravda and Izvestia. Unfortunately, during the USSR's existence those were rare: we have counted 21 mentions in the years 1917-1991, most of them related to the "pederasty" in Western capitalist countries being presented as a sign of their inevitable moral decline. Unfortunately, no region-level measure of homophobia can be constructed from these data.

3.4   Definition of Locations in the Russian Analysis

Because our treatment is computed on the location level, we also compute our outcomes at the location level. According to the population census of 2020, Russia has more than $144,000$ designated municipalities. We restrict our sample of municipalities to those with at least $1,000$ people in it.[31] The resulting sample

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[31]We arbitrarily chose the $1,000$ population cut-off due to complications in scraping the racial slur in social media: it would introduce measurement errors when misidentifying users in very small Russian villages. There are also a few large cities that are separated into a few municipalities (e.g., Moscow is divided into 12 such municipalities). We collapse them into unique city observations.

of cities, towns, and villages is $9,829$. Hence, we compute two of our outcomes — hate crimes against LGBTQ+ people and incidence of homophobic slurs — for each of these municipalities. The third outcome — intolerance from the representative surveys — is estimated on the individual level, but the treatment is computed on the respondent's location level, hence we use only 495 municipalities there.

We also use county (*rayon*)-level specifications. For these specifications, we use coordinates of the *rayon's* population-weighted centroid to construct exposure to amnesty. Russia has $2,314$ counties (some of them are quite large), each having at least one of our $9,829$ municipalities. Despite Gulag camps spanning many republics of the Soviet Union, we do not add other ex-Soviet countries to our baseline analysis because most of our outcome variables are measured only within Russia.

3.5 Household, Income and Labour Dynamics in Australia (HILDA) Survey

To investigate the impact of prisons on incarcerated individuals, we use the Australian Household, Income, and Labour Dynamics in Australia (HILDA) survey. It offers a nationally representative sample of individuals that it has followed since 2001. Overall, HILDA data cover 32,729 respondents from 2001 to 2019 who appeared at least twice in the survey. Our primary reason for using this survey is that unlike other longitudinal surveys from other countries (such as RLMS in Russia, GSOEP in Germany, and BHPS in the UK) it offers questions on whether the respondent had been incarcerated (as well as the respondent's family members), and also the question about the attitudes towards homosexual individuals. Thus it allows us to observe the LGBTQ+ related attitudes before and after incarceration.

The question that we use for the measure of intolerance is as follows: "Please, on a scale from 1 (strongly disagree) to 7 (strongly agree), to which extent do you agree with the statement that homosexuals should have equal rights?" As a result, we use an ordinal variable varying from 1 to 7. We further normalize it to have zero mean and standard deviation of one for the sake of interpretation. The question was asked not in all years from 2001 to 2019; it was only asked in 2005, 2008, 2011, 2015, and 2019. Hence, in the baseline specification, we restrict our data to only these years.

The question about incarceration asks whether a person "was in prison/jail during the last year." We assume that being in prison is an absorbing state because that person already experienced prison life. Thus for each of the five periods, we create a dummy variable $\mathbb{1}(\text{Respondent was in prison})_{i,t}$ equal to 1 if the person has answered that he/she was in prison in any year before year t (including years for which we don't have data on gay attitudes). Similarly, we construct a dummy for individuals whose family members served a term in prison and returned.[32] Table A.2 provides summary statistics for HILDA data.

4   Effect of Amnesty of 1953 on Prison Culture and Homophobia in Russia

In this Section, we report the results of the regression analysis for the effect of the amnesty of 1953 on the spread of prison culture and homophobic attitudes in Russia. Section 4.1 introduces an empirical

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[32]In our data, 3% of men and 1% of women at some point were incarcerated and 5% of men and 7% of women at some point had a close family member incarcerated. Hence, the identifying variation comes only from these respondents. The rest (even if they don't contribute to the identifying variation in prison experience) provide us with more variation that can help us better capture age-, cohort-, and year-specific trends and fixed effects.

specification to study the effect of amnesty on "coronations" of thieves-in-law and reports results. Section 4.2 introduces our empirical specification to study long-turn effects, reports the results on homophobia, and contains robustness and sensitivity checks. Section 4.4 discusses alternative explanations and mechanisms.

4.1 Amnesty 1953 and Thieves-in-Law Coronations

4.1.1 Empirical Specification and Identification

We start by estimating canonical difference-in-differences (DD) and fully dynamic difference-in-differences (FDDD) specifications. The canonical difference-in-differences specification is as follows:

$$\text{Ihs}(\# \text{ of coronations})_{i,t} = \gamma \cdot \text{Exposure to amnesty}_{i,1954-53} \times \text{Post-amnesty}_t + \mu_i + \lambda_t + \eta X_{i,t} + \epsilon_{i,t}, \quad (1)$$

where $\text{Ihs}(\# \text{ of coronations})_{i,t}$ is a the inverse hyperbolic sine of the number of thieves-in-law coronations happened in *rayon i* in decade t. The variable $\text{Exposure to amnesty}_{i,1954-53} \equiv \sum_{g \in G_{1953}} \left(\frac{\# \text{ released}_{g,1954-53}}{\text{Distance}_{i,g}^\sigma} \right)$ — is the exposure to the amnesty of 1953. We compute it in a way that each location in Russia is treated by *all* released prisoners from *all* Gulag camps, but released prisoners from the camps that are located farther away are counted with smaller weights than prisoners released from a nearby camp. For each *rayon i* we sum released prisoners in all camps weighted by distance from each camp to the *rayon*'s i population-weighted centroid.[33] In the baseline specification we assume linear decay of the effect of amnesty, i.e., the iceberg costs $\sigma = 1$. To make coefficients more interpretable we normalize exposure to the amnesty to have a mean of 0 and a standard deviation of 1. In this specification, we interact it with the Post-amnesty_t variable representing a dummy equal to one for the decades after 1953.

The fully dynamic specification is:

$$\text{Ihs}(\# \text{ of coronations})_{i,t} = \underbrace{\sum_{l=-3}^{0} \gamma_l \cdot \text{Exposure to amnesty}_{i,1954-53} \cdot \text{D}(w = l)}_{\text{pre-amnesty period}}$$

$$+ \underbrace{\sum_{l=1}^{5} \gamma_l \cdot \text{Exposure to amnesty}_{i,1954-53} \cdot \text{D}(w = l)}_{\text{post-amnesty period}} + \quad (2)$$

$$+ \mu_i + \lambda_t + \eta X_{i,t} + \varepsilon_{i,t},$$

where $\text{Ihs}(\# \text{ of coronations})_{i,t}$ is a the inverse hyperbolic sine of the number of thieves-in-law coronations happened in *rayon i* in decade t. Period indices run from -3 to 5 and represent the decade relative to amnesty $w = 0$ — decade period of 1953. The variable $\text{Exposure to amnesty}_{i,1954-53}$ — is the exposure to the amnesty of 1953. In this specification, we interact it with the $\text{D}(w = l)$ — a dummy equal to one if decade $w = l$. Periods from $l \in [-3; 0]$ represents pre-amnesty period and periods from $l \in [1; 5]$ represents post-amnesty period. Coefficients γ_l with $l \geq 1$ capture the effect of amnesty in the post period, and the ones with $l \leq 0$ capture pre-trends.

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[33]Figure A.6 shows the map of the variation in *rayon*-level exposure to the amnesty.

These specifications allow us not only to estimate the immediate effect of the amnesty on the spread of prison culture — and homophobia being its part as a result of "bitch wars" — but also allow us to absorb time-invariant variation coming from the endogenous location of the Gulag camps. In addition to the location (μ_i) and decade (λ_t) fixed effects, we also consider specification with controls ($X_{i,t}$); in particular, we are concerned that due to proximity to Gulags local economy may have unobservable trends in economic development and demographics that will also affect the need for enforcement of criminal norms. Hence we add controls for the Gulag labor camp-specific trends in some specifications. We cluster standard errors on the *rayon* level.

4.1.2 Results on Coronations of Thieves-in-Law

Table 1 contains the results of the canonical DD specification in Equation 1. Column I controls only for *rayon* and decade fixed effects. It suggests, that a one-standard-deviation larger exposure to the amnesty results in a 6.5-percent larger increase in the number of thieves-in-law coronations. To address possible confounding trends from local economic development driven by convict labor, in Column II we control for the interaction of log distance to the nearest Gulag camp and decade dummy. In Columns III and IV, we also add interactions of decade fixed effects with the total size of the nearest Gulag camp and total *rayon*'s exposure to the Gulag system. We measure the latter in the same way as our exposure to the amnesty but use total labor camp population throughout Gulag history instead of camp's number of amnestied prisoners — $\sum_{g \in G_{\text{All}}} \left(\frac{\# \text{ total prisoners}_{g,1923-60}}{\text{Distance}_{i,g}} \right)$. Finally, in case, these trends are driven by the industry in which convicts were employed, in Column VI we also control for the interaction of the industry fixed effects of the nearest labor camp with time fixed effects. Throughout Columns II–VI the coefficient of interest remains significant and does not change its magnitude much. In the most conservative specification of Column VI one-standard-deviation larger exposure to the amnesty results in a 6-percent larger increase in the number of thieves-in-law coronations.[34] These results are consistent with Lonsky (2020) who found that the proximity to the nearest Gulag camp was a strong predictor of the presence of the thieves-in-law.

Our results do not depend on the functional form of how we measure exposure to amnesty. Table B.4 reports the specification from Column V of Table 1 but uses alternative functional forms of weights to compute exposure to the amnesty. Column I shows the baseline measure for comparison. Our alternative measures of exposure to the amnesty are essentially a more flexible version than using different distance bins to omit (or include) exposure from more distant Gulag camps. We assume logarithmic and square root (i.e., slower) decay of exposure to the amnesty with distance in Columns II and III. We allow for a less skewed distribution of the number of amnestied prisoners, by using log and the square root of prisoners in the numerator in Columns IV and V. We also allow for polynomial iceberg costs (i.e., faster) in Column VI. Here quadratic terms in the denominator basically assign zero weights to faraway Gulag camps. All specifications yield significant coefficients comparable to the baseline, with the baseline and polynomial decay specifications being the most conservative and the logarithmic one in Column IV yielding the largest

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[34]Our results hold when we use alternative ways of computing standard errors. Panel A of Table B.3 clusters standard errors on the province level (83 clusters) and Panels B and C provide results for standard errors adjusted for spatial correlation on 150 and 300 km thresholds.

Table 1 – Locations More Exposed to Amnesty of 1953 Had Larger Increase in the Number of Coronations

	I	II	III	IV	V
	Dependent variable: Ihs # coronations (mean 0.007 st.dev. 0.115)				
Exposure to 1953 amnesty x Post amnesty	0.065***	0.065***	0.065***	0.060***	0.060***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
R-squared	0.376	0.377	0.377	0.408	0.415
Observations	23,260	23,260	23,260	23,260	23,260
Rayon FE	✓	✓	✓	✓	✓
Decade FE	✓	✓	✓	✓	✓
Min. distance to Gulag camp x time trends		✓	✓	✓	✓
Population of the closest Gulag camp x time trends			✓	✓	✓
Exposure to total Gulag population x time trends				✓	✓
Convict labor industry FEs of closest Gulag camp x time trends					✓

Notes: The unit of observation in this Table is a county (*rayon*)-decade. The dependent variable is an inverse hyperbolic sine of the number of coronations of thieves-in-law in *rayon* i decade t. Column II controls for the interaction of minimum distance from the population-weighted centroid of *rayon* to one of 475 ever-existing Gulag camps interacted with time fixed effects. Column III controls for the interaction of the total population of the nearest ever-existing Gulag camp interacted with time fixed effects. Column IV controls for the exposure to the total Gulag population. Column V controls for the convict labor industry fixed effects of the nearest ever-existing Gulag camp interacted with the time fixed effects. Standard errors clustered at the *rayon* level are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

magnitude of the coefficient. We do not have a prior, which functional form should be preferred, and use the baseline in Column I as the one with the most conservative estimators. Finally, Columns VII and VIII report specifications with the most simple measure of exposure to the amnesty: total number (and log number) of released prisoners in *rayon* i (i.e., only taking into account amnesty from Gulag camps located inside *rayon* i and assigning zero weights for outside camps). While the resulting coefficients are significant we prefer less arbitrary and more flexible specifications with decay rate in exposure to the amnesty.[35]

Our results are also not driven by a particular geographical region of Russia. Figure B.1 estimates the preferred specification from Column V of Table 1 dropping one Russian province at a time.[36] All estimated coefficients remain positive and significant. Dropping Chelyabinskaya Oblast decreases the coefficient the most from 0.060 to 0.056. This direction is logical as there were many large Gulag camps in Chelyabinskaya Oblast famous for their metallurgy and overbearing masculinity of local males.[37] Dropping St. Petersburg increases the coefficient the most from 0.060 to 0.062. This change is also in-line with the fact that St. Petersburg is the second largest Russian city with a large economy because of which it had a large number of coronations without having many convict labor camps around it.

While we observe coronations without measurement error only on the *rayon* level we can also re-estimate Equation 1 on the municipality level by using coordinates of the *rayon's* largest city for those observations where we don't know the exact city. In this trade-off, we get a classical measurement error in the outcome variable (that increases standard errors)[38] but we can more precisely capture exposure to amnesty, use

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[35] Treatment with the amnesty from the nearest Gulag camp is not applicable for the *rayon*-level specifications as there may be multiple camps in the same *rayon*. However, we discuss this measure later in Section 4.2.2.

[36] Russia has 83 provinces (or federal subjects) that include *oblasts*, *kraya*, ethnic republics, autonomous regions, and two cities, Moscow and St. Petersburg. We do not consider two temporarily occupied territories, the Autonomous Republic of Crimea and the city of Sevastopol, which are part of Ukraine.

[37] E.g., see https://russia.fandom.com/ru/wiki/%D0%A7%D0%B5%D0%BB%D1%8F%D0%B1%D0%B8%D0%BD%D1%81%D0%BA.

[38] Exposure to the amnesty does not correlate with a dummy for having no information on the precise municipality of the coronations. Thus we assume that this measurement error is random.

municipality fixed effects instead of *rayon* fixed effect, and allow us to be more consistent throughout the paper, as our dependent variables in the next section are measured on the municipality level. Table B.5 re-estimate Table 1 on the municipality-level. The estimates appear substantively similar and significant.

Figure 4 reports the results of the FDDD specification in Equation 2. Panel A presents results using the specification with the minimum set of controls (as in Column I of Table 1). We set a decade before the amnesty as a reference point. We see that within the next decade after the 1953 amnesty, locations with one standard deviation higher increase in exposure to the amnesty experienced by 2% increase in the number of thieves-in-law coronations in 1954–1963 and 1964–1973. The effect persisted and intensified over time, suggesting that prison norms became more pronounced over time, reaching approximately 5% in 1974–1983, and 10% after 1984.[39] At the same time, we see no pre-trends. Our results also hold when, in Panel B, we additionally control for the full set of controls from Column V. By doing so, we absorb possible variation coming from trends in local economic development.[40]

Figure 4 – FDDD Analysis: No Increase in Number of Thief-in-law Coronations Before 1953 and Increase After the Amnesty

Panel A: Baseline   Panel B: w Gulag characteristics × decade FEs
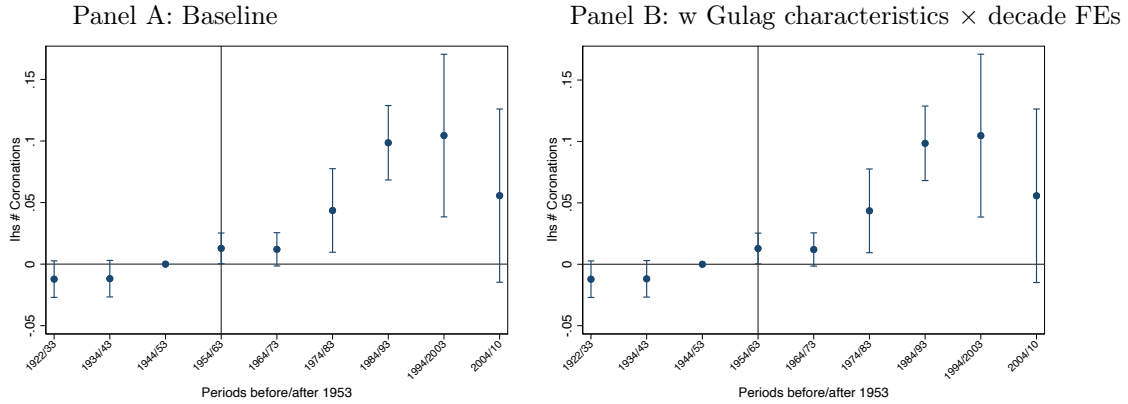
Notes: This Figure graphs the results of estimating Equation 2. The dependent variable is the inverse hyperbolic sine of the number of coronations of thieves-in-law. Panel A uses controls from Column I of Table 1. Panel B uses controls from the Column V of Table 1. The p-value for the joint significance of the pre-trend's coefficients is equal to 0.7222 in Panel A and 0.7557 in Panel B. This figure reports 95th-percent confidence bands. Columns I and II of Table B.6 contain the estimates for the specifications in Panel A and B, respectively. Standard errors clustered at the *rayon* level.

Finally, as we are concerned by the potential biases from the "forbidden comparisons" as well as heterogeneous effects, we re-estimate the event-study specification in Panel A of Figure 4 using the methodology by Borusyak, Jaravel and Spiess (2021).[41] The resulting estimates shown in Figure B.3 are qualitatively very similar to our ordinary least squares (OLS) one; hence, we conclude that the issues of forbidden comparisons and the biases due to heterogeneous treatment effects are unlikely to affect our estimates.

Overall, these results suggest, that in the aftermath of the amnesty of 1953, prison culture indeed spread and culminated in the coronation of thieves-in-law needed for its supervision. In the next section, we show how the amnesty affected one of the most salient manifestations of the prison culture — homophobia.

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[39]Results also hold if we bin the end-points, so that 2004-2010 is included in the 1993–2003 bin (Schmidheiny and Siegloch, 2019; Borusyak, Jaravel and Spiess, 2021).

[40]Our results also hold when, in Figure B.2, we re-estimate Equation 2 on the municipality level.

[41]We use it since it is the only method that provides a stable and documented implementation that can be deployed for continuous treatments.

4.2 Amnesty 1953 and Homophobia in the Long Run

In this Section, we explore the long-run effect of exposure to the 1953 amnesty on a set of outcomes related to homophobia. As about 1.2 million prisoners were released simultaneously and settled in the nearby areas, we expect this influx to bring prison culture into civilian life. Here we explore the effect of the amnesty on a variety of contemporary outcomes: expressions of homophobia in surveys, the number of hate crimes against LGBTQ+ persons, and homophobic slurs on social media.

4.2.1 Empirical Specification and Identification

We estimate the following specification:

$$y_i = \alpha + \beta \cdot \text{Exposure to amnesty}_{i,1954-53} + \eta X_i + \varepsilon_i, \tag{3}$$

where y_i is one of our measures of intolerance toward gay persons in location i. Our main explanatory variable — Exposure to amnesty$_{i,1954-53} \equiv \sum_{g \in G_{1953}} \left(\frac{\# \text{ released}_{g,1954-53}}{\text{Distance}_{i,g}} \right)$ — is the exposure to the amnesty of 1953.[42] Because Gulag locations were endogenous to the economic geography of the Soviet Union, weighting by distance to Gulag camps may confound our results. For example, a location near Gulag's labor camp may become an industrial center with a large number of low-skilled manufacturing workers who are homophobic due to socio-economic conditions rather than the amnesty of 1953. And because our measure of exposure to the amnesty is correlated with the distance to that nearby camp we can capture the effect of Gulag on the local economy rather than the effect of prison culture. In the event-study specification presented in the previous Section, we were able to absorb location fixed effects and directly test for pre-trends. For this specification, our identifying assumption is that exposure to the amnesty is uncorrelated to the factors that may affect homophobia conditional on controls related to the Gulag system. Hence, we assume, that conditional on the geography of Gulag camps (X_i), the number of released prisoners is exogenous to counterfactual future changes in the anti-gay sentiments. Because we always control for the distance and size of the nearest Gulag camp, total exposure to the Gulag camps system, and industrial composition of the nearest camp, our results are unlikely to be confounded by the (potentially endogenous) determinants of the location of the labor camps. The effect we capture is therefore not explained by the presence of the Gulag and its influence on the local economy but rather by the magnitude of the amnesty of 1953 from nearby camps.[43] Tables B.1 and B.2 additionally show that the amnesty is not correlated to pre-1953 levels and changes in Gulag population or its industrial composition and that the exposure to the amnesty of 1953 is uncorrelated to pre-1953 changes and levels in populations, manufacturing output, sex ratios, shares of the adherents of various religious denominations, and the number of thief-in-law coronations.[44] We cluster standard errors

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[42]Similarly to the specification in Section 4.1, here we also assume linear decay of exposure to the amnesty with the distance. We provide robustness for this measure later in the robustness section and Table B.9.

[43]Note, that controlling for Gulag's geography does not address the fact that amnesty may affect homophobia, not through prison culture but a change in economic conditions due to the inflow of a large number of ex-prisoners. We show that our results are not driven by this explanation separately in Section 4.4.

[44]We also need the amnesty to be uncorrelated with the pre-treatment levels and changes in homophobia; however, this assumption is plausible, because according to the historical and sociological literature discussed in Section 2, expressions of homosexual orientation were tolerated in Russian society before Stalin's tenure (Healey, 2001, 2017).

on the province level for specifications where our observation is the location (for crimes against LGBTQ+ and homophobic slurs in vk.com) and cluster on respondents' location for specification with survey data.

4.2.2 Results on Homophobia

Table 2 presents our results from the estimation of Equation 3. The dependent variable in Panel A is an inverse hyperbolic sine of the number of crimes against gay persons in the city. The dependent variable in Panel B is the inverse hyperbolic sine of the number of mentions of homophobic slurs in the last 1,000 posts on vk.com. The dependent variable in Panel C is a dummy, equal to one if the respondent said that he/she would not like homosexuals to be their neighbors. To make our coefficient of interest more interpretable we normalize exposure to the amnesty to have a mean of 0 and a standard deviation of 1.

Table 2 – Locations More Exposed to Amnesty of 1953 are More Homophobic Now

	I	II	III	IV	V	VI
Panel A:	Dependent variable: Ihs # crimes against LGBTQ+ (mean 0.024 st.dev. 0.307)					
Exposure to 1953 amnesty	0.0311***	0.0307***	0.0303***	0.0303***	0.0267***	0.0273***
	(0.0100)	(0.0102)	(0.0102)	(0.0102)	(0.0101)	(0.0101)
R-squared	0.011	0.011	0.011	0.011	0.015	0.018
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel B:	Dependent variable: Ihs # homophobic slur in VK (mean 0.008 st.dev. 0.127)					
Exposure to 1953 amnesty	0.0143***	0.0143***	0.0140***	0.0140***	0.0107*	0.0110**
	(0.0050)	(0.0051)	(0.0051)	(0.0052)	(0.0055)	(0.0054)
R-squared	0.013	0.013	0.013	0.013	0.035	0.037
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel C:	Dependent variable: 1(Dislike homosexuals) (mean 0.616 st.dev. 0.486)					
Exposure to 1953 amnesty	0.1201**	0.1277***	0.1281***	0.1180***	0.1165***	0.1337***
	(0.0466)	(0.0406)	(0.0414)	(0.0414)	(0.0392)	(0.0506)
Survey-year FEs	✓	✓	✓	✓	✓	✓
R-squared	0.005	0.006	0.006	0.012	0.012	0.066
Observations	6,519	6,519	6,519	6,519	6,519	6,519
Latitude & longitude		✓	✓	✓	✓	✓
Min. distance to Gulag camp			✓	✓	✓	✓
Population of the closest Gulag camp				✓	✓	✓
Exposure to total Gulag population					✓	✓
Convict labor industry FEs of closest Gulag camp						✓

Notes: The unit of observation in Panels A and B is a town/village with a population of at least 1,000 people. The unit of observation in Panel C is a respondent. The dependent variable in Panel A is an inverse hyperbolic sine of the number of crimes against LGBTQ+ people in 2010–2015. The dependent variable in Panel B is an inverse hyperbolic sine of the number of homophobic slurs in the latest 1,000 public posts on vk.com. The dependent variable in Panel C is a dummy equal to 1 if respondents would not like having homosexuals as their neighbors. All Columns in Panel C include survey-year fixed effects because it pools the data from five different surveys (the 7th wave (2017) of the WVS, 2nd (2010) and 3rd (2016) waves of LiTS, and 2013's and 2015's Levada Courier Survey). Panel C weights all observations using survey population weights. In Panels A and B standard errors clustered at the province level are in parentheses (83 clusters). In Panel C standard errors clustered at the location level are in parentheses (495 clusters). *** p<0.01, ** p<0.05, * p<0.1

Column I contains results for the bivariate regression and Columns II–VI gradually include additional controls, to keep our specifications in Table 2 as comparable to those in Table 1. Because, we are unable to control for the fixed effects of the location, in Column II, we flexibly control for the locations' coordinates. Column III controls for the distance to the closest existing Gulag camp and Column IV controls for its total size. We control for the total exposure to the Gulag system in Column V and dummies for the industry

compositions of the closest Gulag camp in Column VI. The resulting coefficients appear positive and highly significant in all specifications.

Consider Column VI of Panel A, a one-standard-deviation increase in the exposure to the amnesty increases the number of crimes against gay persons by 2.7 percent or 1.3 of its mean. In Column VI of Panel B, a one-standard-deviation increase in the exposure to the amnesty increases the number of homophobic slurs on social media by 1.1 percent or 1.4 of its mean. Panel C presents results with the survey data on individual intolerance toward homosexuals. Note, that in addition to the controls in Panels A and B, in Panel C we always control for the survey-year fixed effects to address the fact that the data is pooled from five different survey waves. The resulting coefficient in Column VI suggests that a one-standard-deviation increase in the exposure to the amnesty increases the probability of a respondent being intolerant toward gay persons by 13.3-percentage-points (22 percent of the mean).[45]

Overall, we find that exposure to the amnesty of 1953 positively affects all three measures of intolerance toward homosexuals. The effect is statistically significant and explains a large share of the variation in these variables. As these measures are based on different dimensions of discrimination against gay persons and generated by different data-generation processes we see this as compelling evidence that the amnesty of 1953 had a profound effect on cultural acceptance of homosexuality in Russia.

4.3   Robustness Checks and External Validity

Here, we provide additional robustness and sensitivity checks. We consider robustness to (i) inclusion of additional geographic and individual controls; (ii) exclusion of each province at a time; (iii) usage of alternative functional forms of exposure to the amnesty; (iv) usage of alternative ways of computing standard errors; and (v) usage of coarser, *rayon*-level aggregation.

We first show that our results hold when we control for a set of additional controls in Table B.7. Column I contains the baseline specification for reference. In Column II, we control for the location-type fixed effects. This control is more exogenous than the contemporary population as it only classifies locations into official categories based on location's importance: provincial capital, city, township, and big (*poselok*) or small (*selo*) village. Then we show that our results hold when we control for the log of the population in 2020 (Column III). The population itself is a "bad" control as it is affected by the Gulag system as shown in Mikhailova (2012); however, for the specification with the ihs number of crimes against gay persons, it may be beneficial to flexibly control for the population on the right-hand side because large cities may have more anti-gay crimes mechanically. It is less important to control for the population for the other two outcomes of interest, as a homophobic slur is measured relative to the total posts on social media and we use population weights supplied by the survey in the individual-level homophobia regressions. Indeed, the coefficient of interest for

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[45]Note that the coefficients for, both, extreme level of homophobia (hate crimes in Panel A) and every-day level of passive homophobia (language in social media in Panel B) have very similar magnitudes, e.g., in terms of their means. The magnitudes of the coefficients are larger for the survey-data results in Panel C. Residential homophobia is more comparable with the homophobic slur in its mundane nature and one would expect the estimands to be more similar. We believe that the difference happens for two reasons. First, individual survey data is based on a sample of data while slur is measured for all municipalities in Russia. Second, slurs are measured very precisely with people unconsciously using them in their posts but in Panel C we combine the direct effect of amnesty on homophobia and its effect via social desirability bias, i.e., respondents saying that they are homophobic because they know that this is the most accepted behavior in their location (Coffman, Coffman and Ericson, 2017).

the specification with the ihs of crimes in Column II decreases in magnitude; however, remains significant. Coefficients for the specifications with other dependent variables remain stable and also significant. In Column IV, we show the robustness of our results to the inclusion of the regional (federal districts) fixed effects.[46] These regions are large enough to have enough identifying variation but at the same time may absorb some geographic variation in the vast Russian territory. Reassuringly, our results hold. In case the effect is driven by ethnic republics that may have different demographics, in Column V we add a dummy for them. Finally, in Column VI, we show that our results are robust to the inclusion of province fixed effects, essentially allowing each province to be on a separate intercept.[47,48]

In Table B.8, we employ an additional set of individual-level demographic controls, available for our survey-based measure of homophobia. To address the possible differential effects of prison culture on different demographics, Column I controls for the gender and age of the respondent. Column II includes dummies for marital status. Columns III and IV include respondents' ethnicity and religion fixed effects, respectively. Column V additionally controls for respondents' education. Finally, we control for the respondent's occupation and household income in Column VI. Our results remain highly significant throughout all specifications.

To demonstrate that our results are not driven by any specific province, Figure B.4 reports the robustness of our preferred estimate in Column VI for all our three dependent variables to dropping one province at a time. The estimated coefficient always remains significantly different from zero. For the number of hate crimes (Panel A), dropping Volgogradskaya Oblast, decreases the coefficient the most, from 0.0273 to 0.0247. Dropping the Sahalinskaya Oblast — an island in the Pacific Ocean — increases the coefficient the most, from 0.0273 to 0.0385. For the homophobic slur (Panel B), dropping Tomskaya Oblast, decreases the coefficient the most, from 0.0110 to 0.0100. Dropping the Sahalinskaya Oblast also increases the coefficient the most, from 0.0110 to 0.0157. For the survey-based responses (Panel C), dropping Volgogradskaya Oblast, decreases the coefficient the most, from 0.1337 to 0.1110. Dropping the city of Moscow increases the coefficient the most, from 0.1337 to 0.6731. This aberration may be explained by the fact that Moscow constitutes 10% of the survey data and that Moscow is a relatively progressive city and attracts population being the most prosperous region of Russia while having a lot of Gulag camps nearby in 1953. For consistency, we choose to keep Moscow (despite being an outlier), especially as the estimate is more conservative in its magnitude.

Our results hold if we use alternative measures for exposure to the amnesty. In the baseline measure, we used Exposure to amnesty$_{i,1954-53} \equiv \sum_{g \in G_{1953}} \left(\frac{\# \text{ released}_{g,1953-52}}{(\text{Distance}_{i,g})^{\sigma}} \right)$, $\sigma = 1$; however, we can assume a faster (or slower) decay of the effect of inflow of prisoners. These alternative measures of exposure to the amnesty are essentially a more flexible version than using different distance bins to omit towns far away from gulags, as larger σ assigns zero weights to faraway Gulag camps. Hence, we repeat our baseline results

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[46]Russia is divided into eight federal districts representing a collection of provinces: Central, Northwestern, Southern, North Caucasian, Volga, Ural, Siberian, and Far Eastern.

[47]Note, that the coefficient for individual intolerance toward gay persons (Column VI of Panel C) significantly increases in magnitude. This happens because the city of Moscow's fixed effect is now included. We provide relevant discussion below, where we show the robustness of our results to the omission of one province at a time in Figure B.4.

[48]Note, that adding province fixed effects is not a straightforward thing to do. We think that it is not correct to include province fixed effects because in this case, we would identify within-province exposure to the amnesty and control for within-province proximity to Gulag camps. While our baseline results hold when we include province fixed effects, we are unsure whether these results can be interpreted meaningfully.

but with alternative measures of exposure to the amnesty (as we do in Table B.4 in Section 4.1). Panel A of Table B.9 contains results for the ihs number of crimes against gay persons, Panel B — ihs incidences of homophobic slurs, and Panel C — individuals' homophobia. All specifications result in qualitative similar estimates. Column I reports baseline estimates from Column VI of Table 2 for comparison. Columns II–V show specifications with a slower rate of decay and specification in Column VI — faster. All specifications in Columns II–V appear significant, suggesting the robustness of our results to alternative weights. Specification in Column VI, with polynomial weights $(\text{Distance}_{i,g}^2 + \text{Distance}_{i,g})^{-1}$, appears to be not significant in Panels A and B but significant in Panel C. We believe that because Russia is large and has a considerable distance between cities and villages, in specifications in Panels A and B, where observation is every settlement with a population above 1,000, very few locations will be treated by the amnesty if its effects decay will be too fast. Panel C uses individual-level observations from nationally representative surveys. Hence, most of them are urban and since Gulag camps were often located near cities, more observations are likely to be quite close to labor camps to be treated even with fast-decay weights. Overall, we find that our results appear to be robust to the usage of alternative measures of exposure to the amnesty, with slower-decay weights producing more significant results.[49] Additionally, in Table B.11 we show that our results are robust to specification where we use the total number of prisoners released within 50, 150, 300, 500, 750, and 1,000 km of the location.[50]

Table B.12 shows the robustness of our location-level results from Table 2 to alternative methods of clustering. Our results hold when instead of clustering by province, we use spatial HAC, adjusting for possible spatial correlation in exposure to the amnesty.

Finally, we also replicate Panels A and B of Table 2 on the *rayon* level to be comparable with the *rayon*-level specification in Section 4.1.[51] For this we compute *rayon*-level number of crimes against LGBTQ+ and homophobic slur and show results in Panels A and B of Table B.13. The coefficients of interest remain positive and significant, consistent with our preferred location-level specification.

Permutations and placebo estimates In this section, we provide a set of additional placebo tests that demonstrate that our results are not driven by potentially unobserved factors that might correlate with exposure to the amnesty of 1953.

First, in Figure B.5 we plot the coefficients of our baseline specification in Column VI but use the number of released prisoners for all other years from 1929 to 1960.[52] Results for the numbers of crimes

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[49]We believe that it is incorrect to use the number of released prisoners from the nearest Gulag camp as the alternative measure of exposure to the amnesty. As Figure 3 suggests, some locations may be surrounded by several closely located Gulag camps resulting in severe measurement error. As a result, we would underestimate the exposure of locations with several nearby Gulag camps and overestimate the exposure of locations far away from Gulag camps. Nevertheless, for completeness, we report these results in Table B.10; while the resulting coefficients have the correct sign, they are insignificant, in line with our understanding of the measurement error. Moreover, all the R-squares are smaller than in the corresponding specifications of Table B.9 suggesting that exposure to the amnesty from the nearest Gulag does not explain the same amount of variation in homophobia as exposure from the multiple nearby camps.

[50]We can see the effect of the amnesty across all Panels; however, Panels A and B exhibit larger and more significant coefficients with thresholds below 500 km while Panel C — for thresholds above 150 km. We believe that the latter result is difficult to interpret since Panel C uses only a subset of Russian municipalities.

[51]We do not replicate Panel C because the observation there is a respondent, and aggregation of the exposure to the amnesty from respondent's location to their *rayon*'s centroid location just introduces measurement error.

[52]Even though the Gulag labor camps system appeared in 1922 until 1928 it was only one labor camp at the Solovetsky islands in the White Sea and the first camp-level decrease in the number of prisoners happened in 1929 when the Solovetsky labor camp on the island was closed and moved across the strait to the mainland town of Kem'.

against LGBTQ+ are shown in Panel A of Figure B.5. The coefficients from each regression are placed chronologically from 1929 (with almost zero variation from only two labor camps) to the end of the Gulag system in 1960. The baseline coefficient for the amnesty of 1953 (in gray) is on the red line indicating it as the reference and is the largest in magnitude. We see pre-1953 coefficients are mostly clustered around zero except one for 1931 and 1939, although they are insignificant on any conventional level. This is likely to happen due to little variation in the number of released prisoners: the Gulag system was mostly steadily growing (see Figures A.3 and A.4) until 1941 and continued to grow after its local minimum in 1946. The coefficients for 1954 and 1956 appear to be positive and not well-defined zeroes; however, they are smaller than the true estimate and are statistically insignificant. Panels B and C show results of similar exercises with the number of homophobic slurs in VK and individuals' homophobia, respectively. Some of the coefficients are positive and significant but are small in their magnitude. This is consistent with the fact that some released prisoners may still choose not to return home thus contributing to the growth of prison culture. The true coefficients are by far the largest in magnitude suggesting, that the amnesty of 1953 was indeed the most significant event in the history of Gulag's prison releases. Overall, it is safe to say that only the amnesty of 1953 consistently affected our outcomes on intolerance toward gay persons.

Second, we use the fact that homophobia does not appear in female prisons and that we have information on what labor camps were female Gulag camps.[53,54] We compute exposure to the amnesty of 1953 from them and replicate our baseline results in Table B.14. We find no effect of the release of female prisoners on homophobia across all Panels. All the coefficients are way smaller in magnitude than the baseline and are negative. These results are in line with our hypothesis that only male ex-prisoners spread homophobia although one needs to note that variation in Table B.14 results from computing the exposure to the released prisoners from just five camps.

Third, in the spirit of Dell and Olken (2020), we permutate the location of the labor camp and the size of the amnesty.[55] Then we compute exposure to amnesty based on these counterfactual Gulag camps. We do it 500 times and then run our baseline specification. Figure B.6 compares our true point-estimates to the distribution of point-estimates obtained using counterfactual amnesties from the 153 labor camps existing in 1953. The true coefficient is within the 5th percentile of all counterfactual coefficients for all three dependent variables. Similarly, Figure B.7 compares our true point estimates to the factual amnesty size but permutating the location of the 153 existing labor camps between 475 ever-existing labor camps. We see that the true point estimates (for all three outcome variables) are in the 10th percentile of the coefficients received from the placebo regressions. This permutation test shows that the location of camps really existing in 1953 and the real size of the amnesty rather than anything else related to the location of Gulag camps are of specific importance for current anti-gay sentiments.

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[53]All camps could have both male and female prisoners but five labor camps had female-specific complexes: Akmolinsk Camp of Wives of Traitors to the Motherland in Karaganda, Temnikovsky labor camp, Podgorny labor camp, Dzhangirsky labor camp, and Balahninsky labor camp.

[54]Later, in Section 5, using longitudinal data, we demonstrate that prison experience affects homophobia only through males.

[55]When we permutate amnesty location, we use the real locations of 475 camps that we know to permutate locations of 153 camps existing in 1953 with their true amnesty sizes. When we permutate amnesty size, we draw amnesty size without replacement from the 153 camp-amnesty observations.

Effect of amnesty of 1953 on other Soviet Republics   We study the effect of the amnesty of 1953 on homophobia only in Russia because we don't have data on various sets of homophobic outcomes in other post-Soviet countries.[56] However, since LiTS contains a question on residential homophobia, we test, whether the results we find in Panel C of Table 2 hold for other Soviet Republics. For this, we re-estimate Panel C of Table 2 on the sample of all Soviet Republics that ever had Gulag labor camps (without Russia) and additionally add country fixed effects. Table B.15 reports these results. The resulting coefficients are significant across all Columns, suggesting the effect of amnesty on residential homophobia not only in Russia but also in other post-Soviet countries exposed to Stalin's Gulag system.

Effect on consumption of 'prison' music   We can also test whether exposure to the amnesty affected other outcomes related to the prison culture. Probably the loudest expression of prison culture in Russia is music preference, as a whole genre was invented to romanticize prison life and its culture. This genre — Russian Chanson — was created in Gulag camps by evolving from the harsh romance, restaurant, and Odesa street songs of the 1920s, and was officially recognized as a stand-alone prison music genre in 1991.[57]

We collected data on the consumption of Russian Chanson in two steps. First, we perused the official charts of Russian music and chose the top 10 Chanson musicians.[58] Second, we downloaded *rayon*-level searches of these musicians from the Russian most popular search engine — Yandex. Then we replicate our baseline specification (Equation 3) on *rayon*-level using the inverse hyperbolic sine of music searches while flexibly controlling for the log of the total number of searches as the dependent variables. We report results in Panel A of Table B.16. We find a strong positive effect of exposure to amnesty on the consumption of prison romanticism music. We additionally show results for specifications where we use consumption of music by two famous Russian rappers Egor Kreed and Slava Marlow (in Panels B and C) and Alla Pugacheva — Russian most celebrated singer for more than 60 years (in Panel D); however, we find no robustly significant effect on consumption of non-prison music.

4.4   Alternative Explanations

Endogenous proximity to Gulag camps   Locations closer to Gulag camps may be different in terms of local economic composition. There is consistent evidence that Gulag labor camps were strategically placed to supply a coerced labor force for big industrial construction sites, timber production, mines, water channels, and railroad construction (Gregory and Lazarev, 2003; Khlevniuk, 2004; Miller and Smith, 2015; Gallen, 2019). As a result, it (differentially) affected the long-run economic development of these locations (Mikhailova, 2012; Toews and Vézina, 2020) and, because modernization is generally associated with more

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[56]Additionally, according to Healey (2001, 2017), while the Russian society tolerated expressions to homosexuality, other places that later became parts of the Soviet Union exhibited substantial anti-gay sentiments during the time of the Russian Empire. In particular, Healey (2001) mentions Azerbaijan, Georgia, and the Zaporizhzhia region of Ukraine.

[57]Songs of this genre usually combine a simple melody with the singer's ruminations on the hardship of life of a career criminal, sentimental expressions of appreciation of the narrator's mother, as well as general observations about the unfairness of life. One of the famous examples of this genre is the 1998 song by Mikhail Krug, "Vladimirsky Central" ("The Central Prison of Vladimir"). The song is about a career criminal celebrating the coming of spring in captivity and remembering his first love. More information on Russian Chanson can be found here: https://en.wikipedia.org/wiki/Blatnaya_pesnya.

[58]In particular, we used searches for Villie Tokarev, Grigorii Leps, Lesopoval, Denis Maydanov, Katya Ogonek, Nikolay Rastorguev, Mikhail Schufutinsky, Zheka, Mikhail Krug, and Ivan Kuchin.

inclusive values (Inglehart and Welzel, 2005), could affect cultural norms such as attitudes toward gay individuals.

Our specification, however, allows us to directly control for the endogenous locations of Gulag labor camps by controlling for the distance to the closest labor camp, its size and industry, total exposure to the Gulag system, and the coordinates of the location.[59] This is possible because our identifying variation comes from the exogenous number of released prisoners of that unique amnesty rather than total exposure to labor camps itself. As a result, while the existence of labor camps could affect (both, positively and negatively) attitudes toward gays directly through the economic development of the region, our specification absorbs this effect and while we can't identify it separately it does not confound our results. It is also worth keeping in mind that the specification in Equation 2, where we estimate the effect of exposure to the amnesty on the emergence of prison culture as measured by the coronations of thieves-in-law, does include location fixed effects and yields substantively similar results (see Section 4.1).

Overall, our effect is driven by the variation in amnesty exposure rather than the potentially endogenous location of camps. Other *mechanisms*, however, remain plausible: such as the effect of amnesty being driven by economic underdevelopment, gender norms, religion, and other factors. We address these concerns below.

Economic (under)-development   One of the important concerns is that the amnesty itself affected local economic development as ex-convicts could hinder economic growth through criminal activities. We address this concern by showing that conditional on Gulag controls, exposure to the amnesty does not correlate with economic outcomes. Table B.17 replicates Table 2 but uses log average household income and population as the main dependent variables. We observe that in none of the columns, the exposure to the amnesty is significantly associated with contemporary wages. The amnesty exposure is positively correlated with the current population, but the coefficient becomes insignificant and its magnitude drops fourfold once we control for the exposure to the total Gulag population. These results suggest, that the effect of amnesty on the economy was not economically strong and the long-run economic development is likely to be driven only by the existence of labor camps rather than how many people were released in 1953.

Additionally, economic underdevelopment is unlikely to explain our results, as findings by Mikhailova (2012) show that areas near Gulag camps have higher levels of population and regional economic development. Hence, it is likely that this mechanism works against us finding the negative effect of amnesty on tolerance toward gay persons.

Change in crime rates   An alternative explanation would be not the horizontal and vertical spread of homophobia norms among exposed locations but a direct effect on the number of criminals and/or the spread of homophobia only between the local criminals.

To demonstrate that this mechanism does not explain our results we, first, show that the amnesty by itself does not explain crime rates and the number of criminals in Russian locations today. Unfortunately,

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[59]We use all ever existing labor camp locations when computing this minimal distance/size/industry even if the camp was already closed by 1953. We do so because we intend to absorb the confounding effect of endogenous labor camp location and omitting already closed camps would introduce non-classical measurement error. However, results hold if we compute these variables using only a set of existing in 1953 camps.

we do not have the number of general crimes for *all* location or even *rayons*. However, we obtained data on the number of crimes and number of criminals for the 200 largest Russian cities for 2010–2015 — the same years as data on crimes against gay persons.[60] Table B.18 replicates specification in Table 2 but uses the log number of crimes per capita as the dependent variable. The positive and significant coefficient in Column I indicates that more exposed cities have a larger number of crimes. However, the magnitude and significance of the coefficient fall when we start to control for the coordinates and total Gulag exposure in subsequent columns, essentially having zero coefficients in our preferred specification with Gulag geography controls (Column VI). Hence, the crime is likely to be explained by Gulag-driven economic factors rather than that one amnesty in 1953.[61] Nevertheless, such regressions would be confounded by the first-order effect of the Gulag system on economic development and by the effect of the amnesty on the number of thieves-in-law decreasing inter-gang violence and pushing the coefficient toward zero. Hence we propose a more clean placebo test below.

Prison culture is specific about discrimination of only male "passive" homosexuals and feminine men but not lesbians. Because we observe the victim of the crime against LGBTQ+ in our data, we separately construct variables for crimes against 'gay men,' 'gay women,' and 'trans and other LGBTQ+ persons' and re-estimate our baseline regression with these variables. Panel A of Table B.19 shows results for the crimes against gay men. We find that our estimates are significant and very similar to those in Table 2 suggesting that they drive our baseline results. At the same time, we find no effect of the exposure to the amnesty on the number of crimes against lesbians in Panel B or trans persons in Panel C.[62]

Change in social values unrelated to prison norms The question remains whether our results are driven by the spread of the prison culture rather than the general change of social values. Here we provide evidence against this alternative explanation.

We show that exposure to the amnesty of 1953 does not affect the usage of non-homophobic derogatory language on social media. If prison culture makes people just hate everyone rather than just gay persons we would also see the effect on the usage of derogatory language to other groups or of general curse words. For this, we scrape the number of such derogatory words used on the VK social network in the last 1,000 posts of each location. Then we replicate Panel B of Table 2 but use these non-homophobic slurs in social media as the dependent variables. Panel A of Table B.20 contains results for derogatory language toward women. Panel B — derogatory (non-homophobic) language used in the description of men. In Panel C the dependent variable is the number of times the most common Russian family of swear words with the root 'huj' (хуй) and its derivatives are used. We find no significant estimates throughout all specifications, suggesting that the amnesty only affects the homophobic aspect of the language.[63]

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[60]The alternative is to use the Russian Bureau of Statistics province-level data but it would reduce the number of observations to 83 and introduce measurement error in exposure to the amnesty.

[61]Figure B.8 shows coefficients for the effect of the amnesty on the number of crimes for every year where the crime data are available (from 1997 to 2017). None of the coefficients is significant at the conventional level.

[62]Note that our results for trans persons in Panel C need to be taken with caution as we don't have a lot of variation in this variable.

[63]We also considered measuring the usage of derogatory language toward ethnic minorities and/or immigrants but there are too many possible words that can be counted as those and it makes the construction of such variable unrealistic without strong assumptions. However below we show no effect on trust toward foreigners using survey data.

Religion It was documented that religion can be an important determinant of homophobia. To address this concern we show that our amnesty was uncorrelated to the latest pre-1953 available data on the prevalence of Orthodox Christianity and other major religious denominations (Catholics, Protestants, and Muslim) from the 1897 census data (see Table B.2).[64] Hence, our treatment is unlikely to capture any confounding effect from local religiosity that can transmit homophobia.

Biased sex ratios and attitudes toward women The effect on homophobia may also be either confounded by the biased sex ratios or caused by them if enough male ex-prisoners change the local sex ratios. Regarding the former, we show in Table B.2 that exposure to the amnesty of 1953 does not correlate with the levels (1897) and changes (1897-1959) in sex ratios. Moreover, after the Civil War and two World Wars, the sex ratios were skewed in the opposite direction (Brainerd, 2017) making it more difficult for us to find a positive effect on intolerance. Regarding the latter, as the average prison release was 4,500 people, we do not think that this is enough to dramatically change the sex ratios of any Russian town. Additionally, sex ratios after World War II were very correlated with the geographical coordinates; i.e., the largest ratio was in the west and it was converging to the 50/50 level in the east. As we control for the location-level coordinates in our baseline specification, this control may be even better than the province-level sex-ratio controls.

Finally, in Table B.21, using our survey data, we show that amnesty did not affect attitudes toward women that would be affected if the effect we capture is about masculinity norms rather than prison culture. We use nine different questions on different aspects of attitudes and discrimination toward women; however, none appear to be significant. Additionally, in Panel A of Table B.20 we show no effect on derogatory language toward women in social networks. We later confirm these results in Section 5.4, where we find no effect of an individual's incarceration on attitudes toward women.

Army We also consider the possibility of our results being driven by the Soviet Red Army. First, we need to note that there is no evidence, that the Soviet army had elements of the prison culture in the 1950s. By the end of the USSR, the army developed a hierarchy of abuse that resembled those that had emerged in prisons (Duggleby, 1998; Herspring, 2005). This hierarchy — hazing (*dedovschina*) — was driven by the fact that conscripts serve for two years and when newly conscripted soldiers arrive, those who already served for one year may abuse the newcomers. Thus the army service at present may have a separate and independent effect on homophobia in Russia. However, given that the emergence of *dedovschina* is traced back to the conscription reform of 1967 (Herspring, 2005), it is unlikely that it confounds our results that are based on the amnesty of 1953.

Trust and social capital Nikolova, Popova and Otrachshenko (2022) suggest that the Gulag system eroded trust and social capital in locations neat Gulag labor camps. In Table B.22 we check the effect of amnesty on related outcomes: the respondents' level of general trust and trust toward family, strangers, and foreigners/migrants.[65] We find no effect of amnesty on trust measures. Later in Section 5.4 we support

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[64]The question about religion was not asked in USSR censi. That's why we can't show the correlation of the exposure to the amnesty with the changes in religiosity in 1897–1959.

[65]Surveys that we use don't have questions on attitudes toward homeless/poor people or ethnic minorities.

these findings using longitudinal data and within-person variation. Overall, while the proximity to Gulag camps might have an independent effect on the measures of social capital, we find no effect of the influx of ex-prisoners on such outcomes suggesting that our findings are not driven by the decline in trust toward out-group members.

Migration   The effect of the amnesty on homophobia could be also overestimated if there was a selective migration and more homophobic people moved into areas affected by the amnesty while less homophobic — moved out. However, in the context of Russia, it is unlikely the case. The Soviet Union and then Russia had (and still has) a system of internal migration restrictions ("*propiska*") similar to the Chinese *hukou* system. Migration in the Soviet Union was mainly driven by the allocation of university graduates to jobs via central planning mechanisms. Partial relaxation of this policy in the 2000s-2010s led to a situation when migrants could get temporary local *propiska* if they received substantial income or if their employer agreed to subsidize it. Hence, it would not create selective migration based on homophobia but rather based on job opportunities.[66]

4.5   Mechanisms

Thieves-in-law   Previously we showed that exposure to the amnesty increased the number of coronations of the thieves-in-laws, criminals whose role is to uphold prison cultural norms. Hence, these coronations are the most reasonable proxy for our mechanism — the strength of prison culture. To test it, we estimate the following specification:

$$y_i = \alpha + \theta \cdot \text{Ihs}(\# \text{ of coronations})_{i,1953-2010} + \eta X_i + \varepsilon_i, \tag{4}$$

where y_i is one of our measures of intolerance toward gay persons in location i and $\text{Ihs}(\# \text{ of coronations})_{i,1953-2010}$ is an inverse hyperbolic sine of the total number of coronations of thieves-in-law in that location after 1953.

Table B.23 reports the results. Results in Panels A and B can be interpreted as elasticities: a one-percent increase in the number of coronations of thieves-in-law increases the number of crimes against gay persons by 0.95 percent and increases the number of homophobic slurs on social media by 0.38 percent. Panel C contains results for individual homophobia: a one-percent increase in the number of coronations of thieves-in-law increases the probability that the respondent is intolerant toward gays by 1.3-percentage-points.[67]

To evaluate what portion of the effect of the amnesty of 1953 can be attributed to the coronations of thieves-in-law, representing the strength of local prison norms, we apply mediation analysis. Under the assumption that the exposure to the amnesty of 1953 is exogenous conditional on the location of the Gulag system, the amnesty affects homophobia either directly or through the mediator — coronations. To make the computation, in Table B.24 we write down the effect of exposure to amnesty on homophobia (X on Y) and coronations (X on M) that we estimated previously. Then we additionally re-estimate Equation 4 with

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[66]Most internal migration happens from the rural areas to regional centers and from those to the few largest cities (Moscow, St. Petersburg, or Yekaterinburg). See Markevich and Mikhailova, 2013 for the most thorough overview of Soviet/Russian demographics and economic geography. Hence, population controls in Table B.7 should account for the most attractive migration destinations.

[67]We match survey respondents to the number of coronations that happened in their *rayons* between 1953 and 2010.

exposure to the amnesty to get the effect of M on $Y|X$. We compute the effect of the amnesty on the number of crimes against gay persons through coronations in Column I. We multiply the direct effect of amnesty on coronations (0.020) by the effect of coronations conditional on the amnesty (0.944) divided by the total effect of amnesty on homophobia (0.027). Almost 70.2% ($\frac{0.9443 \times 0.0203}{0.0273}$) of the total effect of the amnesty on the number of crimes goes through the coronations, suggesting that prison culture is the major mechanism of the effect. Similarly, our results suggest that the coronations explain the 71.8% effect of homophobic slurs (Column II) and the 50.2% effect on individual homophobia (Column III).

Family history In this section, we provide evidence that prison culture is the mechanism behind the effect of amnesty on changes in attitudes toward gays. The 3rd LiTS survey (2016) contains the question of whether the respondent's immediate family members served sentences in labor camps. We use this question to estimate the effect of having immediate family members (parents or grandparents) in labor camps on respondents' anti-gay attitudes. We estimate the following specification:

$$\mathbb{1}(\text{Dislike homosexuals})_i = \beta \cdot \mathbb{1}(\text{Family member was in labor camps})_i + \eta X_i + \varepsilon_i, \tag{5}$$

where $\mathbb{1}(\text{Dislike homosexuals})_i$ is our dependent variable from Panel C of Table 2 — a dummy variable equal to one if the respondent i would not like to have homosexuals as neighbors, and zero otherwise. $\mathbb{1}(\text{Family member was in labor camps})_i$ is a dummy variable equal to one if the respondent said that they had an immediate family member who was in a labor camp, and zero otherwise. Matrix X_i represents a set of geographic and individual controls.[68]

Table 3 − Respondents Whose Close Relatives Were in Labor Camps are More Homophobic: Survey Data (LiTS, 2016)

	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
Sample: LiTS 2016	Dependent variable: 1(Dislike homosexuals) (mean 0.69 st.dev. 0.46)											
Parents/Grandparents sent to labor camp	0.2187***	0.2357***	0.2315***	0.2326***	0.2357***	0.2057***	0.2034***	0.2059***	0.2044***	0.2010***	0.1877***	0.2039***
	(0.0376)	(0.0342)	(0.0351)	(0.0364)	(0.0364)	(0.0493)	(0.0512)	(0.0548)	(0.0562)	(0.0549)	(0.0576)	(0.0537)
R-squared	0.004	0.016	0.021	0.021	0.021	0.131	0.135	0.139	0.140	0.149	0.151	0.150
Observations	1,507	1,507	1,507	1,507	1,507	1,508	1,509	1,510	1,507	1,507	1,507	1,507
Latitude & longitude		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Min. distance to Gulag camp			✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Population of the closest Gulag camp				✓	✓	✓	✓	✓	✓	✓	✓	✓
Exposure to total Gulag population					✓	✓	✓	✓	✓	✓	✓	✓
Convict labor industry FEs of closest Gulag camp						✓	✓	✓	✓	✓	✓	✓
Age & gender							✓	✓	✓	✓	✓	✓
Marital status								✓	✓	✓	✓	✓
Ethnicity FEs									✓	✓	✓	✓
Religion FEs										✓	✓	✓
Education FEs											✓	✓
Log income & occupation FEs												✓

Notes: The unit of observation in this Table is a survey respondent. The dependent variable is a dummy equal to 1 if respondents would not like having homosexual persons as their neighbors. The explanatory variable is a dummy equal to one if the respondent's immediate relatives (parents or grandparents) were in Gulag and zero otherwise. This Table is using data from the 3rd (2016) wave of LiTS. Standard errors clustered at the primary sampling unit level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Column I of Table 3 reports the results of this regression without any controls and Columns II–XII gradually add baseline geographic and additional individual-level controls. We find a strong positive correlation, suggesting that descendants of Gulag camps' prisoners are more likely to be intolerant toward gays even

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[68]We use the same set of controls as in the baseline survey-data Table 2 and Table B.8 but we do not include survey-wave-year fixed effects because we only have one wave with this question.

conditionally on such factors as income and education. Note that these results show a suggestive mechanism that bridges the connection of the Gulag system with current homophobia levels. Later, in Section 5 we show the causal effect of prison experience on an individual's homophobia using within-person variation in prison experience. There, Equation 5 is most comparable to the specification in Panel C of Table 4 where we show that respondents become more homophobic if a family member returns from prison.

Here we demonstrated suggestive evidence, that family members of people who went through Gulag camps are more homophobic than others even when we control for the socio-economic and demographic factors. In the next Section 4.2.2 we causally show that household members of ex-prisoners become more homophobic using longitudinal data. Thus we conclude that the amnesty of 1953 increased intolerance toward gay persons through the spread of prison cultural norms.

5 Effect of Prisons on Individual Outcomes: Evidence from Australian Longitudinal Data

To strengthen the mechanism and to establish the link between the prison experience and an individual's anti-gay attitudes, we turn to an individual-level, longitudinal analysis. In this Section, we use the Australian longitudinal HILDA survey and identify the effect of prison from within-individual variation. This analysis allows us to estimate the effect of prison on anti-gay attitudes of men and women and the effect of prison on their household members. This specification also allows us to directly test whether anti-gay individuals are more likely to end up in prison, thereby corroborating the absence of pre-existing trends in anti-gay attitudes. While the context of Australia is different from Russia, the prison culture in male prisons is still similar and we only study the transmission of cultural norms within family, thus making these results likely to be generalizable.

5.1 Empirical Specification With The Longitudinal Survey Data

We construct a panel dataset of individuals for the years: 2005, 2008, 2011, 2015, and 2019. We estimate the following equation:

$$\text{Equal rights}_{i,t} = \beta \cdot \mathbb{1}(\text{Respondent was in prison})_{i,t} + \mu_i + \lambda_{s,t} + \eta X_{i,t} + \varepsilon_{i,t}, \tag{6}$$

where Equal rights$_{i,t}$ is the dependent variable measuring the level of support for homosexuals having equal rights by respondent i in year $t \in \{2005, 2008, 2011, 2015, 2019\}$. Because our main dependent variable is categorical and varies from 1 (strongly disagree) to 7 (strongly agree), for the sake of interpretability, we normalize it to have a mean of 0 and a standard deviation of 1. The variable $\mathbb{1}(\text{Respondent was in prison})_{i,t}$ is equal to 1 if the respondent was ever in prison before time t. Point-estimate $\widehat{\beta}$ measures the impact of being in prison on outcomes for the individual i in year t, conditional on individual fixed-effects (μ_i), state-specific time trends ($\lambda_{s,t}$), and individual controls ($X_{i,t}$) that include age-, cohort-, and time-specific trends in socio-economic and demographic characteristics. We cluster our standard errors on the respondents' level.

Alternatively, we have an event study design. It allows us to see how respondents' anti-gay attitudes

change over time after obtaining prison experience. Additionally, we can directly test for pre-trends in intolerance. Hence, we estimate the following equation:

$$\text{Equal rights}_{i,t} = \underbrace{\sum_{l=-3}^{-1} \gamma_l \cdot \mathbb{1}(\text{Ever was in prison})_i \cdot \text{D}(w = l)}_{\text{pre-prison period}} + \underbrace{\sum_{l=0}^{3} \gamma_l \cdot \mathbb{1}(\text{Ever was in prison})_i \cdot \text{D}(w = l)}_{\text{post-prison period}} +$$

$$+\mu_i + \lambda_{s,t} + \eta X_{i,t} + \varepsilon_{i,t}, \tag{7}$$

where Equal rights$_{i,t}$ is a measure of tolerance toward gay persons by respondent i in year t. Period $w = 1$ is the first year when the respondent was asked about her/his attitudes toward gay persons after being in prison. Period indices run from -3 to 3 and represent the position of periods relative to prison treatment before year $w = 1$. The variable $\mathbb{1}(\text{Ever was in prison})_i$ is a cross-sectional variable that is equal to 1 if respondent i was ever incarcerated at some point in our dataset and zero otherwise. We interact it with the D$(w = l)$ — a dummy equal to one if year $w = l$. Periods from $l \in [-3; -1]$ represents pre-prison period and periods from $l \in [0; 3]$ represents post-prison period. Coefficients γ_l with $l \geq 0$ capture the prison culture experience effect in the post-prison period, and the ones with $l < 0$ capture pre-trends.

5.2 Estimates from the Longitudinal Data

Canonical DD Table 4 shows the results of the estimation of Equation 6. Panel A estimates it for the sample of male respondents. In Column I, we only use respondent and year fixed effects. We show that being in prison is associated with a 0.23 standard deviation decrease in the respondent's support for equal rights for gays. In Columns II–VII, we sequentially add additional controls. In Column II, we control for state-year fixed effects to address possible changes in states' legislation and public goods provision. Column III adds religion-age and religion-cohort fixed effect to address a concern that people belonging to different religions may become more homophobic and more likely to be sent to prison over time as they age or their cohort ages. Column IV similarly controls for ethnicity-age and ethnicity-cohort fixed effects. In Column V we control for possible differential age and cohort trends in education. Column VI adds occupation-specific trends. In addition, to age- and cohort-specific trends, here we assume that certain occupations may become less profitable over time, thus causing people to commit crimes and be more intolerant of homosexual persons. Hence, we also add occupation-year fixed effects to address possible economy-specific time trends in occupation. Finally, in Column VII, we control for lagged income to address possible changes in income that can make a person more likely to commit a crime and change his attitudes toward minorities. The coefficient estimate for the prison-experience dummy is not statistically different from the one in Column I: being in prison is associated with a 0.28-standard deviation decrease in the respondent's support of equal rights for gay persons.

Panel B estimates Equation 6 on the sample of female respondents. The resulting coefficient is more than twice as small relative to the coefficient for the male respondents and is not significant across all Columns. This suggests, that the prison experience only affects the anti-gay sentiments of men while women released from prison do not become more intolerant. This result is consistent with the prison-specific masculinity

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mechanism described in Section 2.

Table 4 – Effects of Prison Experience on Reductions in Tolerance Toward Homosexual Persons

	I	II	III	IV	V	VI	VII
	Dependent variable: Homosexuals should have equal rights						
Panel A: Sample of men							
1(Respondent was in prison)	-0.230***	-0.234***	-0.301***	-0.298***	-0.292***	-0.279***	-0.279***
	(0.075)	(0.076)	(0.077)	(0.080)	(0.082)	(0.086)	(0.086)
R-squared	0.751	0.751	0.763	0.768	0.775	0.803	0.804
Observations	32,083	32,083	32,083	32,083	32,083	32,083	32,083
Panel B: Sample of women							
1(Respondent was in prison)	-0.147	-0.145	-0.115	-0.136	-0.153	-0.129	-0.129
	(0.104)	(0.104)	(0.107)	(0.110)	(0.112)	(0.126)	(0.126)
R-squared	0.767	0.768	0.778	0.783	0.789	0.806	0.806
Observations	36,466	36,466	36,466	36,466	36,466	36,466	36,466
Panel C: Sample of men							
1(Respondent's close family	-0.119**	-0.117**	-0.140***	-0.136**	-0.141**	-0.135**	-0.135**
member was in prison)	(0.053)	(0.053)	(0.054)	(0.055)	(0.055)	(0.055)	(0.055)
R-squared	0.764	0.764	0.770	0.775	0.779	0.792	0.792
Observations	32,083	32,083	32,083	32,083	32,083	32,083	32,083
Respondent FEs	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓
State-Year FEs		✓	✓	✓	✓	✓	✓
Religion x age & YoB FEs			✓	✓	✓	✓	✓
Ethnicity x age & YoB FEs				✓	✓	✓	✓
Education x age & YoB FEs					✓	✓	✓
Occupation x age & YoB & year FEs						✓	✓
Ihs Income							✓

Notes: The dependent variable is normalized (with a mean of 0 and a standard deviation of 1) degree of support for equal rights for gay persons (originally on a 1–7 scale). Panel A estimates Equation 6 on the sample of male respondents. Panel B estimates Equation 6 on the sample of female respondents. Panel C estimates Equation 6 on the sample of male respondents but uses a different explanatory variable — dummy whether the respondent's close family member ever was in prison. Ihs income is an inverse hyperbolic sine of the respondent's last financial year disposable regular income. All Columns include respondent and year fixed effects. Standard errors clustered at the individual level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Finally, Panel C estimates Equation 6 on the sample of male respondents, but instead of the main explanatory variable $\mathbb{1}(\text{Respondent was in prison})_{i,t}$ we use variable $\mathbb{1}(\text{Respondent's close family member was in prison})_{i,t}$. It is equal to one if a close family member of a respondent i was ever in prison before year t. We find, that men also become more anti-gay if their close family member returns from prison. Family members of ex-prisoners decrease their support for equal rights for gay persons by a 0.14-standard-deviation. This effect is exactly two times smaller than the direct effect on the males who experienced prison by themselves but is still statistically significant, consistently across all specifications. We hypothesize that this coefficient is smaller in magnitude than the one in Panel A for two reasons. First, it is not the effect of first-hand prison experience but rather a second-hand experience. Second, the coefficient may be attenuated because a returned-from-prison family member may be a woman. As we do not observe the gender of that family member, the inclusion of female ex-prisoners who do not contribute to the intolerance toward gay persons attenuates our coefficient. The effect of second-hand prison experience through close family members is driven exclusively by the subsample of men. Results are still significant (but smaller in magnitude) when we re-estimate Panel C on the full sample in Panel A of Table C.1; however, the effect disappears if we use

only the sample of female respondents (Panel B of Table C.1).[69]

Results are substantively similar if instead of a dummy for whether the respondent was in prison, we use a cumulative number of times that the respondent was incarcerated or the total number of years that he/she spent there. See Tables C.2 and C.3. Because we do not observe longitudinal data on close family members who returned from prison we cannot compute the number of years that they spent in prison; hence we can't replicate Panel C in Table C.3. Results also hold if we use population weights in Table C.4.

While we start by presenting a canonical differences-in-differences OLS design, it is worth pointing out that those designs were shown to produce biased estimates in the presence of significant effect heterogeneity. To demonstrate the robustness of our results, we test the same hypothesis using a set of recent methods from the "new difference-in-differences" literature (Borusyak, Jaravel and Spiess, 2021; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; De Chaisemartin and D'Haultfoeuille, 2022). Table C.5 contains results for these alternative methods. For the sample of males, all coefficients remain negative and significant, with the one computed using the methodology of De Chaisemartin and D'Haultfoeuille (2022) yielding the largest negative estimate (-0.23) and the one computed using the methodology of Sun and Abraham (2021) yielding the smallest (and barely significant on 90% level) estimate (-0.10). Panel B contains estimates for the sample of females. All of the coefficients are statistically insignificant and all but one — computed using the method of Borusyak, Jaravel and Spiess (2021) — are negative. Panel C contains results for the effect of ex-prisoners on their close family members. Methods by Borusyak, Jaravel and Spiess (2021), De Chaisemartin and D'Haultfoeuille (2022), and Sun and Abraham (2021) produce negative and significant coefficient (the largest in magnitude -0.15 by De Chaisemartin and D'Haultfoeuille, 2022). The estimate computed following Callaway and Sant'Anna (2021) is also negative (-0.065) but is statistically insignificant.[70] Overall, we believe that our main results appear to be robust to the alternative ways of constructing differences-in-difference estimates and while the OLS estimate appears to be not the most conservative in its magnitude it is the most straightforward and we prefer to keep it as the baseline.

Event-study design Panel A of Figure 5 plots the resulting coefficients of Equation (7) estimated on the sample of males for the specification with the full set of controls (Table's 4 Column VII of Panel A).[71] Similarly, Panels B and C of Figure 5 plot results for the female respondents and for men whose close family member has returned from prison. The first noteworthy feature is that neither specification exhibits pre-trends. We fail to reject the joint F-test that the pre-event γ_ls are zero in all three Panels. This suggests that the exact timing of the incarceration is not related to trends in homophobia and that respondents did not start to become more homophobic before their first incarceration. The second noteworthy feature is that four point-estimates for periods after incarceration experience have a similar magnitude as the point estimate of $\widehat{\beta}$ from the baseline specification in Table 4. Thus the effect is constant across all years and our

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[69]Naturally, we would also like to check the effect of third-hand prison experience on people living around ex-prisoner. However, we do not have zip-codes of the respondents in our data and even if they were available we would have too few zip-codes with multiple respondents.

[70]Note that the method proposed in Callaway and Sant'Anna (2021) omits all individuals who have missing observations in the panel (i.e., only keeps those to have a balanced panel). This may reduce the sample and lead to larger standard errors.

[71]Period $w = -1$, i.e., the latest year when the question on the equal rights had been asked before the incarceration, is specified as the baseline period.

Figure 5 – Event Study Analysis: No Increase in Intolerance Toward Gay Persons Before Year 0 and Large Increase Among Men Afterwards

Panel A: Sample of Men

Panel B: Sample of Women

Panel C: Close Family Member & Sample of Men
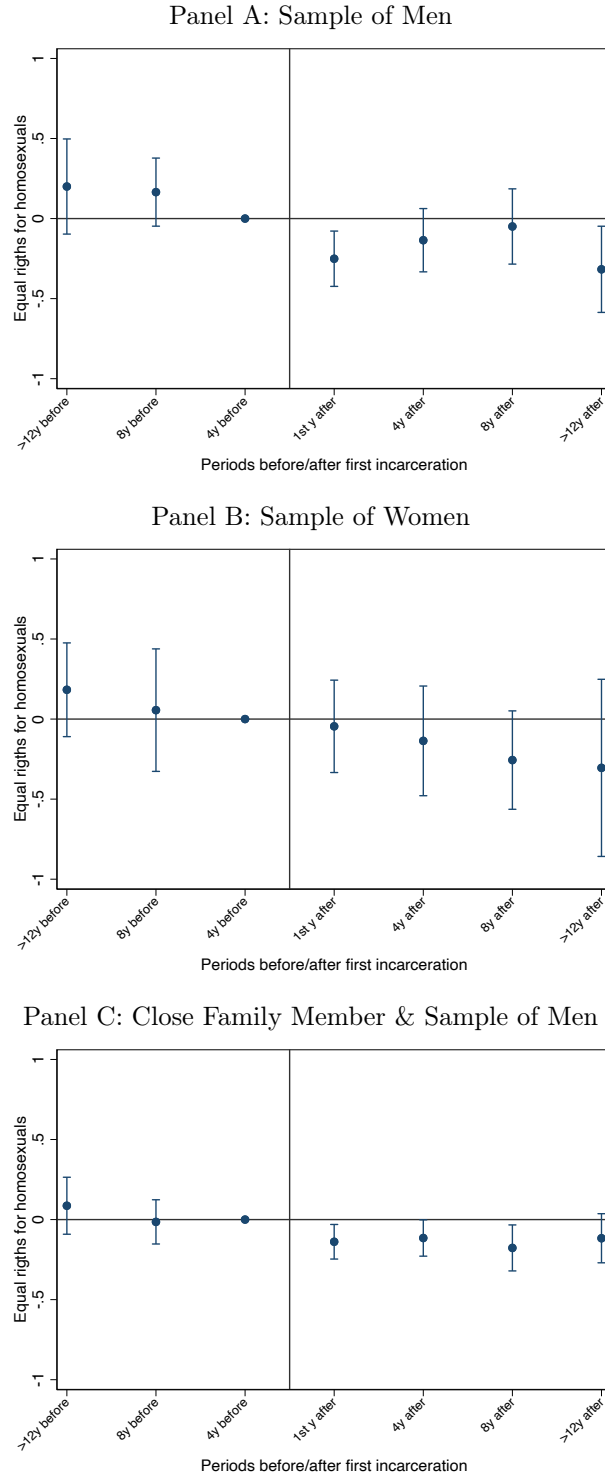
Notes: This Figure graphs the results of estimating Equation 7 for specification in Column VII of Table 6. Panel A is corresponding to the specification in Panel A of Table 6. Panel B is corresponding to the specification in Panel B of Table 6. Panel C is corresponding to the specification in Panel C of Table 6. Point estimates are reported in Appendix Table C.6. P-values for the joint significance of the pre-trend's coefficients are equal to 0.577 for Panel A, 0.471 for Panel B, and 0.718 for Panel C. This figure reports 95th-percent confidence bands. Standard errors clustered at the individual level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

baseline specification (6) captures the full-time path of the effect. To conclude, all event-study results are qualitatively similar to those obtained in Table 4.

5.3 Additional Robustness Checks

Measurement error There are two possibilities for measurement error in the explanatory variable. The first one arises if a respondent was incarcerated and released before enlisting in the survey. For example, if an individual j did not go to prison in 2001–2019 but was in prison in, e.g., 1998, then j's $\mathbb{1}(\text{Respondent was in prison})_{j,t}$ will be always equal to zero and she won't contribute to the identification because of individual fixed effect and thus won't bias our results. However, if respondent j is incarcerated again, she will be counted as switching from non-treated to the treated state while in reality she should be counted as always treated (and not contributing to the identifying variation). Such measurement error will work against us finding the effect of prison culture on intolerance toward gay persons among *men*, but at the same will help us to find zero effect among *women*. To address this concern, in Table C.7 we show that our baseline results hold on the sample of respondents who entered the survey at the age of 18 or younger.[72] Here, we assume that 16–18 years old had no time to go to prison yet.[73] Additionally, Figure C.1 shows that our results are not driven by a particular subsample of respondents' age-of-survey-entry. Dropping respondents that joined the survey at 19–45, 46–60, or after 60 barely moves the coefficient of interest.

The second potential error in the explanatory variable arises if respondents choose to under-report that they were in prison. Such a situation will make it more difficult for us to find a negative effect on attitudes toward gay persons among male ex-prisoners and household members of ex-prisoners but less difficult to find zero effect for female ex-prisoners. However, we find that the shares of released prisoners (according to the Australian Statistical Service) are quite close to the shares that we got from the HILDA survey.[74] While it is impossible to make any reasonable statistical analysis here, eyeballing suggests that respondents are unlikely to hide their prison experience. If this measurement error in the explanatory variable is driven by social desirability bias, following Blair, Coppock and Moor (2020), we additionally control for an interviewer's fixed effects. Appendix Table C.8 shows that our results hold.

Another source of measurement error bias is a non-classical measurement error in the dependent variable that correlates with prior incarceration. E.g., due to some individual characteristics, a respondent may pretend to be more homophobic if he were in prison. However, this concern is addressed by individual fixed effects or age- and cohort-specific characteristic-specific fixed effects that we absorb in Columns III–VI of Table 4.[75]

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[72]Because the number of individuals who entered the survey at the age of 18 or younger is small, adding a full set of age- and cohort-specific fixed effects from our preferred specification kills all the identifying variation. Hence, here we use the most parsimonious specification from Column I of Table 4.

[73]This assumption is likely to be true for the specifications with individual prison experience (Columns I–IV of Table C.7) than for specifications with second-hand prison experience (Columns V–VI) because a child can still be affected by returning from prison father/mother at any age. Nevertheless, it would be an attenuation bias working against us finding the negative effect.

[74]See www.abs.gov.au/statistics/people/crime-and-justice/prisoners-australia/latest-release#data-download.

[75]Alternatively, there may be a progressive trend such as people everywhere becoming more likely to be less homophobic. But because people who go to prison are less integrated into society, they may say what they think and not what society expects from them. In this case, we would have to find an effect of prison on homophobia among men when there is none. However, attitudes toward gay persons are not the only progressive issue that has been changing recently, and attitudes to women's rights mostly improving. However, we do not see the effect of prison on attitudes to women's rights (Table C.11), so this alternative

Finally, another potential explanation is that individuals did have anti-gay attitudes before prison but chose to hide those when responding to the survey. Later, after prison, they stopped hiding their views and responded sincerely. We view such "mainstreaming" of homophobia as one of the potential mechanisms of the effect. As Bursztyn, Egorov and Fiorin (2020) show, individuals, when placed in an environment where the extreme views appear acceptable are more likely to express such views and also less likely to sanction individuals who espouse them. Of course, we will never know how sincere the support of HILDA's respondents for the rights of homosexuals before the prison, but, as we show in Panel C of Figure 5, their — potentially well-hidden anti-gay attitudes — failed to influence even their close family members before their incarceration.

Alternative samples We probe the sensitivity of our results in several ways. First, we demonstrate that our results are not driven by any specific state. Panel A of Figure C.2 estimates the most conservative specification from Column VII of Table 4 Panel A dropping one state at a time. This may be potentially important because the Australian population is mainly concentrated in New South Wales (largest city Sydney) and Victoria (largest city Melbourne). The estimated coefficient always remains significantly different from zero. Dropping Queensland, decreases the coefficient the most, from -0.28 to -0.31. Dropping the Victoria, increases the coefficient the most, from -0.28 to -0.26. We perform the same exercise for Panels B and C of Table 4; these results are shown in Panels B and C of Figure C.2. All the results appear robust. Additionally, in Figures C.3 and Figures C.4, we show the robustness of our preferred estimate to dropping one religion or education group at a time.

Matching In this section, we relied on the identification from within-person variation in prison experience. This identification strategy uses only variation among respondents who switched their prison experience status to identify the coefficient of interest. In our data, only 3% of men and 1% of women were incarcerated.[76] To show that our results are not driven by very little variation we show that they are robust to using an alternative identification strategy based on matching on observable characteristics (i.e., using between-person variation). Luckily, HILDA contains a very comprehensive questionnaire. Following the approach proposed in Belloni, Chernozhukov and Hansen (2014), we choose a set of controls to estimate the propensity score.[77] Table C.9 contains the results for different types of matching estimators. Reassuringly, these matching estimates are substantively similar to our baseline results. In sum, while both of these results are based on different identifying assumptions and use a different identifying variation, the fact that they yield similar estimates (even in magnitude) suggests that our results are not an accidental artifact of the number of individuals going to prison being low.

To conclude, our results in this section show that men who went to prison become more anti-gay but women do not. The effect is not driven by individual or economic characteristics, or by people with anti-gay sentiments being more likely to be incarcerated. We document that homophobia spreads from ex-prisoners

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explanation becomes less plausible.

[76]Respondents that did not switch their prison status contribute to the estimation of the age- and cohort-specific fixed effects.

[77]In this double-robust matching procedure we run lasso on the outcome and treatment variable using the full set of variables available at HILDA. Then we choose a set of variables, that are significant correlates of both, and run a propensity score using that set of overlapping variables.

to other household members but the magnitude of this second-order effect is smaller.

5.4 Alternative Explanations

In this section, we address several important alternative explanations such as the results being driven by pre-incarceration differences in anti-gay attitudes, prisoners acquiring traditional masculinity norms, prisons decreasing the level of generalized trust, acquiring mental health issues, and others. In general, we find that those alternative explanations are unlikely to be driving our results.

Pre-trends in incarceration Within-person variation and a rich set of controls allow us to address the most likely source of unobserved trends that can possibly correlate with the higher probability of ending up in prison and developing anti-gay attitudes. The biggest concern that can invalidate our result is that anti-gay persons are just more likely to be criminals and end up incarcerated. While we show the absence of pre-trends in homophobia using the event-study specification in Equation 7, we can additionally address this alternative explanation by estimating the following specification that uses (i) all years in which the question about incarceration was asked and (ii) using a dummy for being in prison *last* year instead of *ever* being in prison. We estimate the following specification:

$$\mathbb{1}(\text{Was incarcerated last year})_{i,t} = \beta \cdot \text{Equal rights}_{i,t-1} + \mu_i + \lambda_{s,t} + \eta X_{i,t} + \epsilon_{i,t}. \tag{8}$$

Because the question on whether the respondent was in prison last year was asked every year, in comparison to the specification in Equation 6, here we use all years from 2006 (when the first question about attitudes toward gay persons was asked) until 2019. Here, our dependent variable $\mathbb{1}(\text{Was incarcerated last year})_{i,t}$ is equal to 1 if respondent i was incarcerated within a year prior to year t. The main explanatory variable Equal rights$_{i,t-1}$ measures respondent's i attitudes toward gay rights in the previously available period (i.e., the first available period before the incarceration). Thus for the periods 2005–2008, it is measured as respondent's gay rights attitude in the year 2005, for the period 2008–2011 — in 2008, for 2011–2015 — in 2011, and for 2015–2019 — in 2015. Essentially, this specification estimates how an individual's homophobia at period $t-1$ affects the probability of the respondent being sent to prison at period t.

Table C.10 presents the results. We find that men (Panel A) and women (Panel B) with anti-gay sentiments are not more likely to be incarcerated. Similarly, anti-gay respondents are not more likely to have a family member incarcerated (Panel C). Together with the absence of significant pre-trends in Figure 5, these results are reassuring of the absence of selection of homophobic trends in people admitted to prisons (conditional on individual fixed effects).

Masculinity norms and attitudes toward women The literature on masculinity norms (among others, Francis, 2011; Baranov, De Haas and Grosjean, 2018, 2020; Grosjean and Khattar, 2019; Teso, 2019; Chang, 2020; Brodeur and Haddad, 2021; Bazzi et al., 2022) suggests, that a high concentration of men in a community may lead to changes in cultural norms related to attitudes to women, risk, and even health practices (e.g., rectal prostate exam). Thus the observed effect can be explained by masculinity culture as

prisons have 100% biased sex ratios. To address this concern, in Table C.11 we replicate Column VII of Table 4 on the sample of men and women with three outcomes related to attitudes toward women that could be affected by the biased sex ratios and available in HILDA. Columns I and II show results for the effect of prison experience on males' and females' attitudes toward women, defined as a standardized ordinal variable for a respondent thinking that "whatever career a woman may have, her most important role in life is still being a mother." The coefficient of interest is insignificant in both Columns. We observe similar insignificant results in Columns III–VI, where we use alternative variables for attitudes toward women: "it is better for everyone involved if the man earns the money and the women stay home" and "it is not good for a relationship if the woman earns more than the man." These results suggest, that prison culture only affects norms formalized by the prison code but does not have effects on other cultural norms even in the case of extremely biased sex ratios.

Trust toward out-groups and social capital Our results may be also driven by an overall decline in male prisoners' social capital and trust toward out-group members during their prison term. Hence, using available questions from HILDA we construct two measures of social capital (hours per week that the respondent spends on volunteer/charity work and a measure of whether people in the respondent's neighborhood can be trusted) and one measure of trust toward out-group members (a measure of whether generally speaking, most people can be trusted). We show these results in Table C.12, where we replicate Column VII of Table 4 on the sample of men and women with three aforementioned measures as dependent variables. However, we find no significant effect of prison experience on these variables, suggesting that our results are not driven by the deterioration of social capital.[78] Overall, we do not see that prison experience decreases social capital and trust toward out-groups.

Mental health Another plausible alternative mechanism might be decreasing mental health: prison experience and related changes lead to distress and mental health issues (Armour, 2012). As a result, it might make people less accepting of gay rights. To test these explanations we analyze the following mental stress outcomes recorded in HILDA: experience of nervousness, calmness, and peacefulness, "feeling down," experiencing depression, and Kessler psychological distress scale (Andrews and Slade, 2001). First, in Table C.13 we show that mental health does not correlate with attitudes toward gays in a sample of men in Panel A and only one measure out of six suggests a correlation in a sample of women in Panel B (for whom we find no effect of prison experience on homophobia). Second, Table C.14 shows that even if we control for these six measures of mental health, they do not explain away our results. Thus, our results are unlikely to be mediated by mental health.[79]

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[78]Results in Columns I and II for the number of hours respondent spends for volunteer/charity work need to be taken cautiously as there is little identifying variation; it appears that people who go to prison are rarely doing any charity work. Nevertheless, we report these results because we want to use all applicable variables from HILDA to test alternative hypotheses.

[79]Gulag system, however, could have a stronger effect on the mental health of people imprisoned there. Nevertheless, mental health could be only a mechanism for an individual effect, and an individual's mental health problems can't explain the spread of cultural norms to the whole society and persistence over time long after people who suffered mental health trauma have perished.

Change in sexual orientation   Fleisher and Krienert (2009) suggested a brave theory that men who go to prison and experience sex with other men acquire homosexual orientation.[80] First, even if it can apply to some individuals, it suggests that prison experience should *increase* support for equal rights. We show that it decreases the support — thus such a process (even if it takes place) works against us finding a negative effect. Second, we show empirically that the prison experience does not change a person's sexual orientation. To demonstrate this, we use the question, of whether respondents identify themselves as gay to define a dummy equal to one if they identify themselves as LGBTQ+ and zero if identify themselves as a straight woman or man. Table C.15 shows results for the effect of prison experience on sexual orientation. We find, that men and women who went to prison are not likely to become homosexuals (Panels A and B). Similarly, we do not find the effect on the close family members in Panel C.

6   Conclusion

In March 2021, Russian journalist and YouTube blogger Yuri Dud' published an interview with a popular Russian stand-up comedian Evgeniy Chebatkov. In this conversation, Dud' asked Chebatkov: "You are a homophobe. How come?" Chebatkov, who indeed had expressed his anti-gay sentiments publicly on many occasions, responded after some equivocation: "My dad served time. My dad and his buddies were around. Their views influenced mine since my early years. ... I knew their stance intuitively."[81]

This paper makes a simple claim: prisons promote homophobia among men, and, if the incarceration rates are high enough in the country, the attitudes that emerge in prisons, get transmitted to the general population and exert a long-run influence on the hostility towards LGBTQ+ individuals.

We substantiate this claim in several ways. First, we investigate the potential transmission of the anti-gay norms to the general population. To demonstrate such a phenomenon takes place, we need an event in which many geographical locations in a country are exogenously exposed to the influx of people with prison experiences. We use the Soviet amnesty of 1953 that freed 53 percent of Gulag prisoners as an example of such an event. We find that places more exposed to the amnesty had an immediate increase in the number of thieves-in-laws' coronations — indicative of the intensification of prison culture. We also find that more affected by amnesty locations have more instances of hate crimes against LGBTQ+ individuals, have a higher rate of homophobic slurs on social media, and have a higher level of anti-gay sentiments expressed in the representative surveys. Second, we turn to longitudinal survey data from Australia to confirm that men (but not women) who have been to prison become more homophobic after they served their time than they had been before. The same empirical pattern is observed for the male members of their families.

It is important to point out that in our estimation using Russian data, we are not recovering the total effect of prison experiences on modern-day homophobia. Even after the amnesty, the USSR continued to have one of the largest prison populations in the world thus the same mechanism would continue to operate. There are several potential channels: ex-convicts influence their family members (like Chebatkov's father and his "buddies" influenced Chebatkov himself), they also shape their social norms in the local communities,

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[80]We find this theory deeply problematic for a variety of reasons.
[81]See www.youtube.com/watch?v=szLLlbmfRIk, time-code 1:28:20.

and, finally, they influence mass culture spreading the reach of the inmate code far beyond their initial audience.

Our results demonstrate an important source of homophobia that was previously under-explored in quantitative studies: prisons. When policymakers contemplate new reforms that can potentially increase the number of incarcerated individuals, they should take into account the potential effects on the level of anti-LGBTQ+ intolerance.[82] Also, an attempt to limit the prison culture of homophobia — through raising awareness and education of former inmates on important gender issues — should be implemented and rigorously tested.

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[82]High intolerance toward homosexuals has wide-ranging consequences: crimes against LGBTQ+ persons, workplace discrimination, harassment, and bullying. One of the potential consequences is the high rate of child suicides in Russia. While Russia has one of the largest child suicide rates in the world, it is impossible to pin down exactly how many of them can be attributed to bullying and isolation related to one's sexual orientation. Nevertheless, according to Russian LGBTQ+ activists, rampant homophobia can be one of the major reasons behind the suicides. According to Yelena Klimova, a founder of the project "Deti 404" aimed at helping LGBTQ+ teenagers, "Deti 404 receives at least four to five letters each week from kids with suicidal thoughts."[83] Suicide data for Russia is only available on the province level; however, they are certainly consistent with the prison culture influencing child suicides. As we show in Figure B.9, provinces with high numbers of suicides of individuals aged 0–17, according to Russia's State Statistics Agency are precisely those that had been more exposed to the amnesty of 1953. We leave a more detailed exploration of this important topic for further research.

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Online Appendix

to

"Prisons and Homophobia"

A   Additional Data Description

HILDA   Here we describe the construction of the variables from the Australian Household, Income, and Labour Dynamics in Australia (HILDA) survey. It offers a nationally representative sample of individuals from 2001 to 2019. The codebook for the HILDA data can be found here: https://www.online.fbe.unimelb.edu.au/HILDAodd/srchKeyWord.aspx. In all cases when respondents (i) refused to answer (coding -4 in HILDA), (ii) gave multiple responses (coding -5), or (iii) gave implausible values (coding -6) we set observation as missing.[84] We also omit all observations when the respondent appears in HILDA only once because such observations would be absorbed by individual fixed effects. In the end, we have 27,206 unique respondents from 2001 to 2019 that appear in the dataset at least twice.

- Equal rights — based on the variable **mchscr**: "Please, on a scale from 1 (strongly disagree) to 7 (strongly agree), to which extent do you agree with the statement that homosexuals should have equal rights?" We normalize it to have zero mean and standard deviation of one for the sake of interpretation. The variable is defined for the following years: 2005, 2008, 2011, 2015, and 2019.

- 1(Respondent was in prison) — based on the variable **lejls**: "Life events in the past year: Detained in jail/prison." Defined as a dummy equal to one if the respondent has answered that he/she was in prison in any year before year t and zero otherwise. The variable is defined for the following years: 2001–2019.

- 1(Respondent's close family member was in prison) — based on the variable **lejlf**: "Life events in the past year: Close family member detained in jail/prison." Defined as a dummy equal to one if the respondent has answered that her/his family member has returned from prison in any year before year t and zero otherwise. The variable is defined for the following years: 2001–2019.

- 1(Respondent was incarcerated last year) — based on the variable **lejls**: "Life events in the past year: Detained in jail/prison." Defined as a dummy equal to one if the respondent has answered that he/she was in prison in year $t-1$ and zero otherwise. The variable is defined for the following years: 2001–2019.

- 1(Respondent's close family member was incarcerated last year) — based on the variable **lejlf**: "Life events in the past year: Close family member detained in jail/prison." Defined as a dummy equal to one if the respondent has answered that her/his family member has returned from prison in year $t-1$ and zero otherwise. The variable is defined for the following years: 2001–2019.

- 1(Cumulative number of times that the respondent was incarcerated) — based on the variable **lejls**: "Life events in past year: Detained in jail/prison." Defined as the total number of times that the respondent has answered that he/she was in prison before year t. The variable is defined for the following years: 2001–2019.

- 1(Cumulative number of times that the respondent's close family member was incarcerated) — based on the variable **lejlf**: "'Life events in the past year: Close family member detained in jail/prison." Defined as the total number of times that the respondent has answered that his/her family member was in prison before year t. The variable is defined for the following years: 2001–2019.

- 1(Number of years that the respondent spent in prison) — based on the variable **lejls**: "Life events in the past year: Detained in jail/prison." Here we assume that if the respondent was in the survey in year t, was not in the survey for n years, and then reappears on year $t+n$ and answers that he/she was in prison, then he/she was in prison for n years. If the respondent was in the survey at year t and then on year $t+1$ he/she says that he/she was in prison last year, we count it as 1 year. The variable is defined for the following years: 2001–2019. Defined as the total number of times that the respondent has answered that he/she was in prison before year t. The variable is defined for the following years: 2001–2019.

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[84]This results in approximately 1% of the observation being missing. We check that the dummy for missing does not correlate with our treatment (dummy for ever being in prison) or with the respondent's tolerance toward homosexuals.

- 1(Respondent self-identify as a gay person) — based on the variable **lssexor**: "Sexual identity." Defined as a dummy equal to one if the respondent answered (i) gay or lesbian, (ii) bisexual, (iii) other, or (iv) unsure, and zero otherwise. The variable is defined for the following years: 2012 and 2016.

- Most important role in life — being a mother — based on the variable **atwkwrl**: "Please, on a scale from 1 (strongly disagree) to 7 (strongly agree), to which extent do you agree with the statement that whatever career a woman may have, her most important role in life is still of being a mother." We normalize it to have zero mean and standard deviation of one for the sake of interpretation. The variable is defined for the following years: 2001, 2005, 2008, 2011, 2015, and 2019.

- Man earns the money and the woman takes care of the home and children — based on the variable **atwkbmw**: "Please, on a scale from 1 (strongly disagree) to 7 (strongly agree), to which extent do you agree with the statement that it is better for everyone involved if the man earns the money and the woman takes care of the home and children?" We normalize it to have zero mean and standard deviation of one for the sake of interpretation. The variable is defined for the following years: 2001, 2005, 2008, 2011, 2015, and 2019.

- It is not good for a relationship if the woman earns more than the man — based on the variable **atwkmmf**: "Please, on a scale from 1 (strongly disagree) to 7 (strongly agree), to which extent do you agree with the statement that it is not good for a relationship if the woman earns more than the man?" We normalize it to have zero mean and standard deviation of one for the sake of interpretation. The variable is defined for the following years: 2005, 2008, 2011, 2015, and 2019.

- Ihs (# hours volunteering/charity work) — based on the variable **lshrvol**: "Hours per week — Volunteer/Charity work." Because it has zero values we use its inverse hyperbolic sine instead of the log transformation. The variable is defined for the following years: 2001–2019.

- People in the neighborhood can be trusted — based on the variable **lslatr**: "Please, on a scale from 1 (strongly disagree) to 7 (strongly agree), to which extent do you agree with the statement that people in this neighborhood can be trusted?" We normalize it to have zero mean and standard deviation of one for the sake of interpretation. The variable is defined for the following years: 2006, 2010, 2014, and 2018.

- Most people can be trusted — based on the variable **lstrust**: "Please, on a scale from 1 (strongly disagree) to 7 (strongly agree), to which extent do you agree with the statement that generally speaking, most people can be trusted?" We normalize it to have zero mean and standard deviation of one for the sake of interpretation. The variable is defined for the following years: 2005, 2006, 2008, 2010, 2011, 2014, and 2018.

- Ihs (income) — based on the variable **tifdip**: "Financial year disposable regular income (Australian $)." Because it has zero values we use its inverse hyperbolic sine instead of the log transformation. Income is computed as financial year gross regular income minus taxes on financial year gross regular income. See the HILDA User Manual for details. To preserve the weighted mean, top-coded variables have a value substituted which is the weighted average value of all cases which exceed the threshold. This is always a value greater than the threshold. The variable is defined for the following years: 2001–2019.

- Mental Health: Been a nervous person — based on the variable **gh9b**. We set it to be equal to 0 if the respondent answers "A little of the time" and 5 if — "All of the time." We normalize it to have zero mean and standard deviation of one for the sake of interpretation. The variable is defined for the following years: 2001–2019.

- Mental Health: Felt so down in the dumps nothing could cheer you up — based on the variable **gh9c**. We set it to be equal to 0 if the respondent answers "A little of the time" and 5 if — "All of the time." We normalize it to have zero mean and standard deviation of one for the sake of interpretation. The variable is defined for the following years: 2001–2019.

- Mental Health: Felt calm and peaceful — based on the variable **gh9d** but we define it as "did not feel calm and peaceful." We set it to be equal to 0 if the respondent answers "All of the time" and 5 if — "A little of the time." We normalize it to have zero mean and standard deviation of one for the sake of interpretation. The variable is defined for the following years: 2001–2019.

- Mental Health: Felt down — based on the variable **gh9f**. We set it to be equal to 0 if the respondent answers "A little of the time" and 5 if — "All of the time." We normalize it to have zero mean and standard deviation of one for the sake of interpretation. The variable is defined for the following years: 2001–2019.

- Psychological distress: depressed — based on the variable **pddepr**. We set it to be equal to 0 if the respondent answers "None of the time" and 5 if — "All of the time." We normalize it to have zero mean and standard deviation of one for the sake of interpretation. The variable is defined for the following years: 2007, 2009, 2011, 2013, 2015, 2017, and 2019.

- Kessler Psychological Distress Scale (K10) score — based on the variable **pdk10s**. The variable runs from 10 to 50 but we adjust it to have a range from 0 to 40 for the sake of interpretation. The variable is defined for the following years: 2007, 2009, 2011, 2013, 2015, 2017, and 2019.

Gulag data   The data on the locations of Gulag camps come from Mikhailova (2012), which uses the data collected by the Russian non-government organization "Memorial."[85] Then we updated Mikhailova's data with the newest version of Memorial data.

- Exposure to 1953 amnesty (baseline) — Exposure to amnesty$_{i,1954-53} \equiv \sum_{g \in G_{1953}} \left(\frac{\# \text{ released}_{g,1954-53}}{\text{Distance}_{i,g}^{\sigma}} \right)$. $\#$ released$_{g,1954-53}$ is equal to the difference in the number of prisoners in labor camp g between the year 1953 and 1954 if camp g decreased in size/closed or equal to zero if the camp's population increased. Distance$_{i,g}$ is the distance (in km) from labor camp g and location i (rayon's population-weighted centroid in *rayon*-level specifications). In the baseline specification, we assume that σ — decay rate of the exposure to the amnesty — is equal to 1. G_{1593} is a set of Gulag camps active in 1953. We compute exposure to the 1953 amnesty in a way that each location in Russia is treated by *all* released prisoners from *all* Gulag camps, but released prisoners from the camps that are located farther away are counted with smaller weights than prisoners released from a nearby camp.

- Exposure to 1953 amnesty (alternative) — we use several alternative measures of exposure to amnesty. We either use an alternative decay rate of the effect with distance from the labor camp or adjust for the less skewed distribution in the number of released prisoners. Hence we use the following measures of alternative exposure to the amnesty:

  - $\sum_{g \in G_{1953}} \left(\frac{\# \text{ released}_{g,1954-53}}{\ln \text{Distance}_{i,g}} \right)$;

  - $\sum_{g \in G_{1953}} \left(\frac{\# \text{ released}_{g,1954-53}}{\sqrt{\text{Distance}_{i,g}}} \right)$;

  - $\sum_{g \in G_{1953}} \left(\frac{\ln \# \text{ released}_{g,1954-53}}{\ln \text{Distance}_{i,g}} \right)$;

  - $\sum_{g \in G_{1953}} \left(\frac{\sqrt{\# \text{ released}_{g,1954-53}}}{\sqrt{\text{Distance}_{i,g}}} \right)$;

  - $\sum_{g \in G_{1953}} \left(\frac{\# \text{ released}_{g,1954-53}}{\text{Distance}_{i,g} + \text{Distance}_{i,g}^2} \right)$.

- Min. distance to Gulag camp — is the distance (in km) from location i to the closest ever existing labor camp $g \in G_{\text{All}}$. For *rayon*-level specifications we use population-weighed centroid as the coordinates of the location. Results hold if in rayon-level specifications we set the minimum distance to the Gulag labor camp equal to zero if there was a Gulag camp in that *rayon*.

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[85]More on NGO Memorial can be found here: https://www.memo.ru/en-us/.

- Population of the nearest Gulag camp — $\sum_{t \in T_{\text{All}}} \left(\# \text{ prisoners}_{g,t} \right)$, i.e., it is the total population (for all $t \in T$ of the closest ever existing labor camp $g \in G_{\text{All}}$. For *rayon*-level specifications we use population-weighed centroid as the coordinates of the location. In rayon-level specifications where there were several Gulag camps, we use the total number of prisoners in that *rayon*'s Gulag camps.

- Exposure to total Gulag population — Total exposure to $\text{Gulag}_i \equiv \sum_{g \in G_{\text{all}}} \left(\frac{\sum_{t \in T_{\text{All}}} \left(\# \text{ prisoners}_{g,t} \right)}{\text{Distance}_{i,g}^{\sigma}} \right)$.
  $\# \text{ prisoners}_{g,t}$ is equal to the number of prisoners in labor camp g in year t. We sum it over all years $t \in T$. $\text{Distance}_{i,g}$ is the distance (in km) from labor camp g and location i (rayon's population-weighted centroid in *rayon*-level specifications). In the baseline specification, we assume that σ — decay rate of the exposure to the amnesty — is equal to 1. G_{All} is a set of Gulag camps active throughout all Gulag history. We compute total exposure to Gulag in a way that each location in Russia is treated by *all* prisoners in *all* years from *all* Gulag camps, but prisoners from the camps that are located farther away are counted with smaller weights than prisoners released from a nearby camp.

- Convict labor industry FEs — is a matrix of dummies for the convict labor industry of the closest ever-existing Gulag labor camp. Memorial defines 20 industries of Gulag camps: construction of extraction facilities, housing construction, industrial construction, infrastructural construction, fuel and energy industry, metallurgy, military industry, timber production, construction materials, machinery, food industry, coal mining, uranium mining, gold mining, tin mining, other metallic ore mining, apatite mining, stone quarrying, research and development, and service.

Thieves-in-law data Biographies of thieves-in-law come from the criminal news website http://primecrime.ru. It contains textual biographies of Soviet and Russian thieves-in-law. As a validity check, we have found that all thieves-in-law mentioned in Galeotti (2018) are also present on primecrime.ru. We extracted the year and location of the coronation of each thief-in-law and removed those that happened outside of Russia.

- Ihs (# coronations of thieves-in-law)$_{i,t}$ (*rayon*-level) — inverse hyperbolic sine of the number of coronations of thieves-in-law in *rayon* i decade t.

- Ihs (# coronations of thieves-in-law)$_{i,t}$ (location-level) — inverse hyperbolic sine of the number of coronations of thieves-in-law in location i decade t. For the coronations where we only observe *rayon* of the coronation, we assign them to the most populous location of that *rayon*.

- Ihs (# coronations of thieves-in-law)$_{i,\text{post-1953}}$ — inverse hyperbolic sine of the number of coronations of thieves-in-law in *rayon* i in all years after 1953.

- Ihs (# coronations of thieves-in-law)$_{i,1922-1953}$ — inverse hyperbolic sine of the number of coronations of thieves-in-law in *rayon* i in all years before the amnesty, i.e., from 1922 to 1953.

Crimes against LGBTQ+ The data on hate crimes against LGBTQ+ persons were collected by Kondakov (2017, 2021). This dataset only contains cases when the motive of hate against LGBTQ+ persons was established by a court. These data contain all locations that had a hate crime against gay persons in 2010–2015. We uniquely matched these locations to our sample of Russian municipalities. The data also contain information on whether the victim was a gay man, gay woman, trans, or other LGBTQ+ people.

- Ihs # of crimes against LGBTQ+ persons (*total*) — inverse hyperbolic sine of the total number of crimes against gay persons in location i in 2010–2015.

- Ihs # of crimes against gay men — inverse hyperbolic sine of the total number of crimes against gay men in location i in 2010–2015.

- Ihs # of crimes against gay women — inverse hyperbolic sine of the total number of crimes against lesbians in location i in 2010–2015.

- Ihs # of crimes against trans and other LGBTQ+ persons — inverse hyperbolic sine of the total number of crimes against gay persons who identify as trans or other in location i in 2010–2015.

Note, that # of total crimes against LGBTQ+ persons is equal to the sum of the crimes against gay men, gay women, and trans and other persons.

For *rayon*-level specifications we sum crimes over all locations within the *rayon*.

VK.com data We gather data on the language used in social media by scraping the most popular social media website in Russia vk.com (also known as "Vkontakte"). Vk.com's application programming interface allows scraping 1,000 latest public posts by the coordinates of the places of their authors. This data was collected in December 2021. We used *RVk* package for *R* programming language developed by Denis Stukal.[86]

- Ihs # homophobic slur in VK — inverse hyperbolic sine of the total number of times homophobic slur was used in the last 1,000 posts in location i (snapshot of December 2021). We used the following homophobic slurs: гомик, педик, пидор, пидорас.

- Ihs # male derogatory terms in VK — inverse hyperbolic sine of the total number of times derogative slur (non-homophobic) toward men was used in the last 1,000 posts in location i (snapshot of December 2021). We used the following word: мудак.

- Ihs # female derogatory terms in VK — inverse hyperbolic sine of the total number of times derogative slur (non-homophobic) toward women was used in the last 1,000 posts in location i (snapshot of December 2021). We used the following word: блядь. Note that this word can also indicate a general frustration and not be directed at any particular person.

- Ihs # common swear word in VK — inverse hyperbolic sine of the total number of times a common swear word (non-homophobic) was used in the last 1,000 posts in location i (snapshot of December 2021). We used the following words: хуй, хуя, хуи.

For *rayon*-level specifications we similarly collect VK data but use the area circled by the radius around *rayon*'s centroid when computing incidents of homophobic slur in the latest 1,000 posts. We choose a radius to maximize the area of the *rayon*. We also tried to do the average of location-level homophobic slur as the *rayon*-level and all results hold. Hence, we don't think that one measure is better than the other.

Survey data We use five representative surveys of the Russian population from 2010 to 2017 that have a question about attitudes toward homosexuals and the location of the respondents. Survey data comes from three different sources: the 7th wave (2017) of the World Value Survey (WVS), 2nd (2010) and 3rd (2016) wave of the Life in Transition Survey (LiTS), and the Courier survey by Levada Center (the Courier) for 2013 and 2015.

- $\mathbb{1}(\text{Dislike homosexuals}_{i(l)})$ — the dummy variable $\mathbb{1}(\text{Dislike homosexuals}_{i(l)})$ is pooled from different surveys. It is based on the variables **q333h** in LiTS (2010), **q429h** in LiTS (2016), and **Q22** in WVS (2017): "Could you please mention any that you would not like to have as a neighbor?" If the respondent i (nested in location l) chose "homosexuals" we assign a value of 1 and 0 otherwise. In the Courier Levada it is based on the variables **C10A** in the Courier (February 2013) and **qC10A** in the Courier (March 2015): "Would [you] like having people from this group [Homosexuals] as neighbors, dislike it, or not care?" If a respondent answered that they dislike having gay neighbors, we assign the value of 1 to the $\mathbb{1}(\text{Dislike homosexuals}_{i(l)})$ and 0 otherwise.

- Women are as competent as men to be business executives — based on the variable **q426a** in LiTS (2016): "Do you agree that - women are as competent as men to be business executives?" We set the dummy equal to one if the respondent answers "Strongly agree" or "agree" and zero otherwise.

- Men make better political leaders than women do — based on the variable **q426b** in LiTS (2016): "Do you agree that - men make better political leaders than women do?" We set the dummy equal to one if the respondent answers "Strongly agree" or "agree" and zero otherwise.

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[86]Available in Git repository: https://github.com/denisStukal/Rvk.

- A woman should do most of the household chores even if the husband is not working — based on the variable **q426c** in LiTS (2016): "Do you agree that - a woman should do most of the household chores even if the husband is not working?" We set the dummy equal to one if the respondent answers "Strongly agree" or "agree" and zero otherwise.

- It is important that my daughter achieves university education — based on the variable **q426d** in LiTS (2016): "Do you agree that - it is important that my daughter achieves university education?" We set the dummy equal to one if the respondent answers "Strongly agree" or "agree" and zero otherwise.

- It is important that my son achieves university education — based on the variable **q426e** in LiTS (2016): "Do you agree that - it is important that my son achieves university education?" We set the dummy equal to one if the respondent answers "Strongly agree" or "agree" and zero otherwise.

- Cohabiting partners should be married — based on the variable **q426g** in LiTS (2016): "Do you agree that - co-habiting partners should be married?" We set the dummy equal to one if the respondent answers "Strongly agree" or "agree" and zero otherwise.

- It is better for everyone involved if the man earns the money and the woman takes care of the home and children — based on the variable **q426h** in LiTS (2016): "Do you agree that - it is better for everyone involved if the man earns the money and the woman takes care of the home and children?" We set the dummy equal to one if the respondent answers "Strongly agree" or "agree" and zero otherwise.

- Equal rights for women as citizens are important — based on the variable **q414h** in LiTS (2016): "Important for the country - equal rights for women as citizens?" We set the dummy equal to one if the respondent answers "Strongly agree" or "agree" and zero otherwise.

- Most people can be trusted — based on the variable **q302** in LiTS (2010) and **q403** in LiTS (2016): "Generally speaking, would you say that most people can be trusted?" We set the dummy equal to one if the respondent answers "complete trust" or "some trust" and zero otherwise.

- Family can be trusted — based on the variable **q304a** in LiTS (2010) and **q405a** in LiTS (2016): "To what extent do you trust people from the following groups?" We set the dummy equal to one if the respondent mentions "family living with you" and zero otherwise.

- People in the neighborhood can be trusted — based on the variable **q304b** in LiTS (2010) and **q405b** in LiTS (2016): "To what extent do you trust people from the following groups?" We set the dummy equal to one if the respondent mentions "your neighborhood" and zero otherwise.

- Strangers can be trusted — based on the variable **q304c** in LiTS (2010) and **q405c** in LiTS (2016): "To what extent do you trust people from the following groups?" We set the dummy equal to one if the respondent mentions "strangers" and zero otherwise.

- Foreigners can be trusted — based on the variable **q304f** in LiTS (2010) and **q405d** in LiTS (2016): "To what extent do you trust people from the following groups?" We set the dummy equal to one if the respondent mentions "foreigners" and zero otherwise.

2020 Russian population Census data   Contemporary location-level data on Russia come from the Russian database of municipalities (https://data-in.ru/bdmo/) which uses Census data as well as other administrative data to provide information on the demographic characteristics of Russia's municipalities.

- Log population — log of location's population.

- Log income — log of average monthly household income, Russian rubles.

- Location type FEs — A set of dummies for the provincial capital, city, township, and big (*poselok*) or small (*selo*) village. In a few cases, these location types have local names (e.g., *aul* or *stanitsa*) and we assign them with their relevant location type manually.

Russian historical censi We use historical census data on Russian provinces collected by Kessler and Markevich (2020). The data is conveniently available here: https://ristat.org/ru/topics. We only use data for 83 provinces that constitute contemporary Russia.

- Log Population, 1959 — defined as a log total population in that province.

- Log Population, 1897 — defined as a log total population in that province.

- Sex ratios, 1959 — defined as the number of women divided by the number of men in that province.

- Sex ratios, 1897 — defined as the number of women divided by the number of men in that province.

- Log Manufacturing output, 1959 — defined as the log of total manufacturing output in that province in rubles of 1959.

- Log Manufacturing output, 1897 — defined as the log of total manufacturing output in that province in rubles of 1959.

- Share of Orthodox population, 1897 — defined as a share of the population that considers themselves Orthodox Christians divided by the number of people in that province.

- Share of the Catholic population, 1897 — defined as a share of the population considering themselves Catholic Christians divided by the number of people in that province.

- Share of the Protestant population, 1897 — defined as a share of the population considering themselves Protestant Christians divided by the number of people in that province.

- Share of the Muslim population, 1897 — defined as a share of the population considering themselves Muslim (all denominations) divided by the number of people in that province.

- Δ Sex ratios, 1959-1897 — defined as the difference between sex ratios in 1959 and 1897 in that province. Here we match Soviet provinces to the respective provinces in the Russian Empire.

- Δ Log manufacturing output, 1959-1897 — defined as the difference between logs of manufacturing output in 1959 and 1897 in that province. Here we match Soviet provinces to the respective provinces in the Russian Empire.

- Δ Log population, 1959-1897 — defined as the difference between logs of the population in 1959 and 1897 in that province. Here we match Soviet provinces to the respective provinces in the Russian Empire.

Yandex.ru data All data from Yandex.ru is scraped on the *rayon*-level during December 2021. We used the Yandex Wordstat service which provides, for every search term, the number of times it was searched from a particular location in the preceding month.

- Ihs # Chanson searches — inverse hyperbolic sine of the total number of searches for Chanson musicians in Yandex.ru in 2021. We use searches for Villie Tokarev, Grigorii Leps, Lesopoval, Denis Maydanov, Katya Ogonek, Nikolay Rastorguev, Mikhail Schufutinsky, Zheka (Evgenij Grigoriev), Mikhail Krug, and Ivan Kuchin (first name and last name, last name and first name, or last name).

- Ihs # Egor Kreed searches — inverse hyperbolic sine of the total number of searches for Egor Kreed in Yandex.ru in 2021.

- Ihs # Slava Marlow searches — inverse hyperbolic sine of the total number of searches for Slava Marlow in Yandex.ru in 2021.

- Ihs # Alla Pugacheva searches — inverse hyperbolic sine of the total number of searches for Alla Pugacheva in Yandex.ru in 2021.

- Log # total searches — Log of the total number of searches in Yandex.ru in 2021.

Other data

- Log child suicide — Log of the number of child suicides (from 0 to 17 years) by province. Suicide data for individuals aged 0-17 is by Russia's State Statistics Agency and is available at `www.data-in.ru/data-catalog/datasets/164/`. We used the latest data from 2019.

- Ethnic Republics fixed effects: A list of Russian Ethnic Republics can be found here: `https://en.wikipedia.org/wiki/Republics_of_Russia`.

- Intolerance toward homosexuals (country-level) — based on the question "Is the city or area where you live a good place or not a good place to live for gay or lesbian people?" from the Gallup World Poll (available here: `https://analyticscampus.gallup.com/`). We used the latest data from 2019.

- Incarceration rate per capita (country-level) — taken from the latest World Prison Brief (accessible at `PrisonStudies.org`, Fair and Walmsley, 2021).

Table A.1 – Summary Statistics: Effect of the 1953 Amnesty

Variable	Minimum level of aggregation	Mean	St. dev.	Min.	Max.	Source of original data
Exposure to the amnesty of 1953	location/municipality	844.22	1646.94	194	79,360	NGO Memorial
Minimum distance to Gulag labor camp (any from 1923-1960), km	location/municipality	107.1	102.6	0.02	2,129	NGO Memorial
Total exposure to Gulag labor camp system (from 1923-1960)	location/municipality	26,561	88,769	6,779	4,264,648	NGO Memorial
# of coronations of thieves-in-law (decade average, 1922-2010)	rayon/county*	0.02	0.94	0	131	Prime Crime News Agency
# of coronations of thieves-in-law (total post-1953)	rayon/county*	0.19	4.43	0	210	Prime Crime News Agency
# of crimes against LGBTQ+, 2010-2015	location/municipality	0.42	15.79	0	1,148	Kondakov (2017, 2021)
# of homophobic slur in VK, in last 1,000 posts, 2021	location/municipality	0.024	0.76	0	63	VK.ru
Individual homophobia (would not like homosexuals as neighbors), all surveys	location/municipality	0.68	0.47	0	1	NGO Memorial
Individual homophobia (would not like homosexuals as neighbors), LiTS	location/municipality	0.71	0.45	0	1	LiTS
Individual homophobia (would not like homosexuals as neighbors), WVS	location/municipality	0.66	0.48	0	1	WVS
Individual homophobia (would not like homosexuals as neighbors), Levada Courier	location/municipality	0.67	0.47	0	1	Levada
Parents/grandparents sent in a labor camp, LiTS (2016)	location/municipality	0.02	0.12	0	1	LiTS
# Chanson searches in Yandex, 2021	rayon/county	79.27	640.64	0	16,626	Yandex.ru
# Egor Kreed searches in Yandex, 2021	rayon/county	12.15	35.23	0	204	Yandex.ru
# Slava Marlow searches in Yandex, 2021	rayon/county	2.46	17.17	0	219	Yandex.ru
# Alla Pugacheva searches in Yandex, 2021	rayon/county	10.58	35.50	0	549	Yandex.ru
# of crimes against gay men, 2010-2015	location/municipality	0.39	15.47	0	1,148	Kondakov (2017, 2021)
# of crimes against lesbians, 2010-2015	location/municipality	0.06	2.24	0	137	Kondakov (2017, 2021)
# of crimes against trans persons, 2010-2015	location/municipality	0.03	3.12	0	310	Kondakov (2017, 2021)
# of female derogative slur in VK, in last 1,000 posts, 2021	location/municipality	0.00	0.07	0	4	VK.ru
# of male derogative slur in VK, in last 1,000 posts, 2021	location/municipality	0.01	0.10	0	4	VK.ru
# of swear words with root `huj' in VK, in last 1,000 posts, 2021	location/municipality	16.80	127.25	0	160	VK.ru
Population, 2020	location/municipality	13,206	72,614	1001	12,380,664	Population census, 2020
Average monthly income, rubles, 2020	location/municipality	7,510	10,645	205.53	80,762	Population census, 2020
Manufacturing output, mln. rubles, 1959	province	9,504	10,808	150	61,250	Manufacturing census, 1959**
Manufacturing output, mln. rubles, 1897 (in 1959 rubles)	province	7,227	13,488	28	69,409	Manufacturing census, 1897**
Share of Orthodox population, 1897	province	0.61	0.39	0.01	1.00	Population census, 1897**
Share of Catholic population, 1897	province	0.12	0.26	0.00	0.87	Population census, 1897**
Share of Protestant population, 1897	province	0.04	0.15	0.00	0.90	Population census, 1897**
Share of Muslim population, 1897	province	0.16	0.30	0.00	0.99	Population census, 1897**
Sex ratios (women/men), 1959	province	1.22	0.12	0.78	1.40	Population census, 1959**
Sex ratios (women/men), 1897	province	0.98	0.14	0.37	1.33	Population census, 1897**

Notes: * For the 40% of the coronations of thieves-in-law, we observe the location/municipality. Thus for robustness, by imputing the rest 60% of the observation with the location of the largest municipality in that *rayon* we are able to estimate location-level regressions. ** Historical census data are available from Kessler and Markevich (2020).

Figure A.1 – Location and Sizes of *All* Gulag Camps
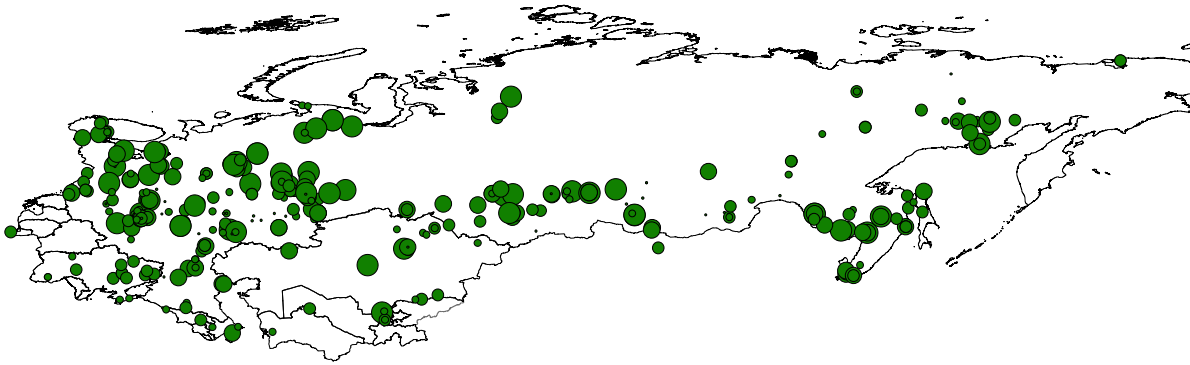
Notes: This map shows the location of 460 Gulag camps on the territory of the former Soviet Union. The size of the ball corresponds to the total number of prisoners that pass through each camp. 408 camps were located in the RSFSR, i.e., in Russia. Note, that one camp was located in Ulaanbaatar, the capital of Mongolia.

Figure A.2 – Location and Sizes of Gulag Camps in 1953
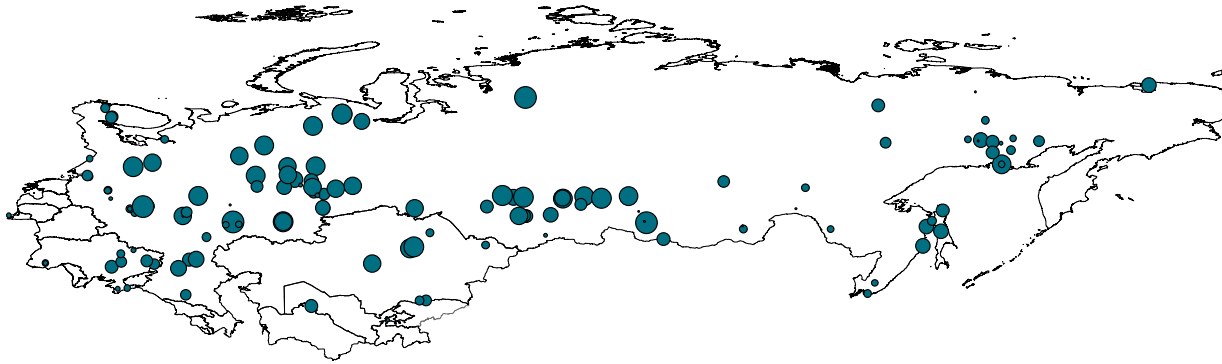
Notes: This map shows the location of 153 Gulag camps on the territory of the former Soviet Union that were operational in 1953. The size of the ball corresponds to the total number of prisoners that pass through each camp.

Figure A.3 – Population of Gulag Labor Camps, 1921–1960
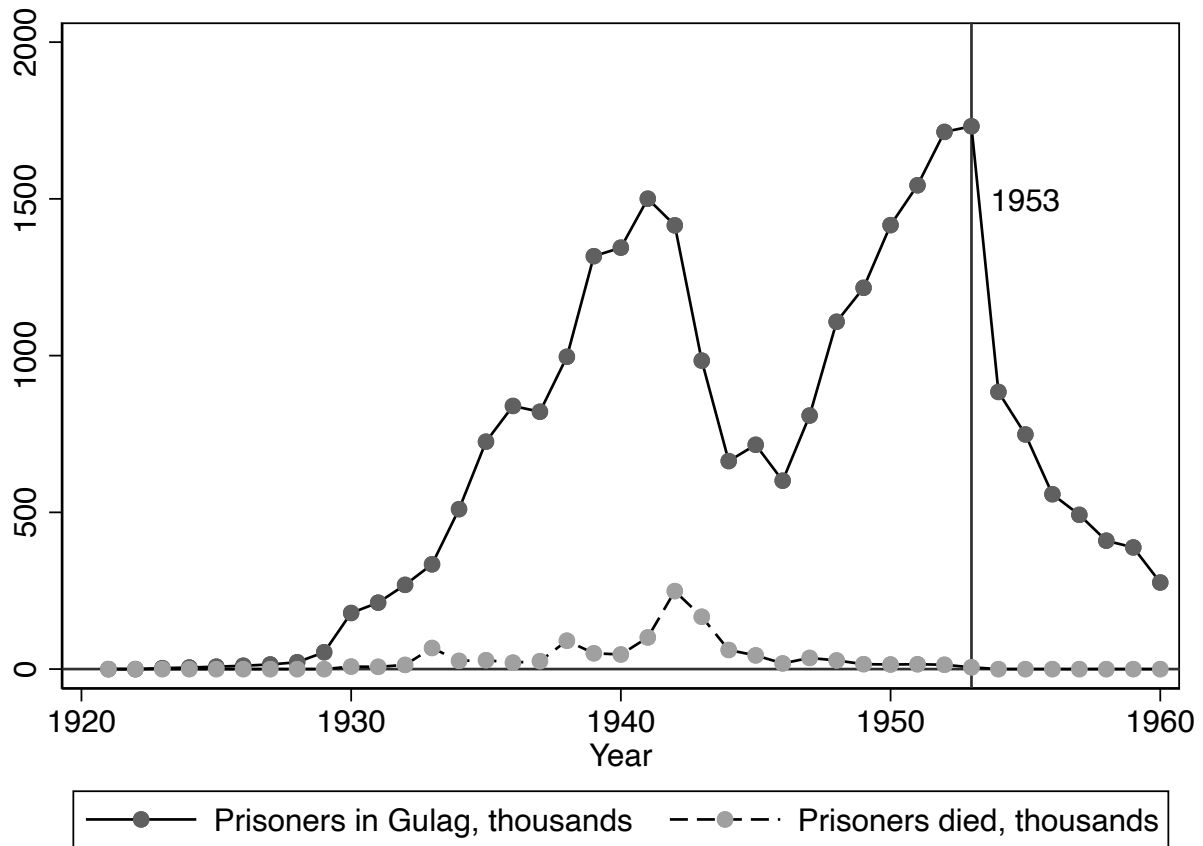
Notes: This Figure shows the population of all Gulag camps during Gulag's history, 1921–1960. The solid line shows the labor camps' population in thousands (stock variable). The dashed line shows the number of prisoners (in thousands) that died each year (flow variable). There are two local maximum of the prisoners, the first in 1941 at the beginning of the German invasion of the Soviet Union and the second in 1953 right before the death of Joseph Stalin. Prisoners' deaths have three local maximum, one in 1933 is related to the Soviet famine of 1932–1933, the second is related to the repressions of 1938, and the third is driven by famine and labor conditions during the first years of the war with Germany.

Figure A.4 – Number of Active Gulag Labor Camps, 1921–1960
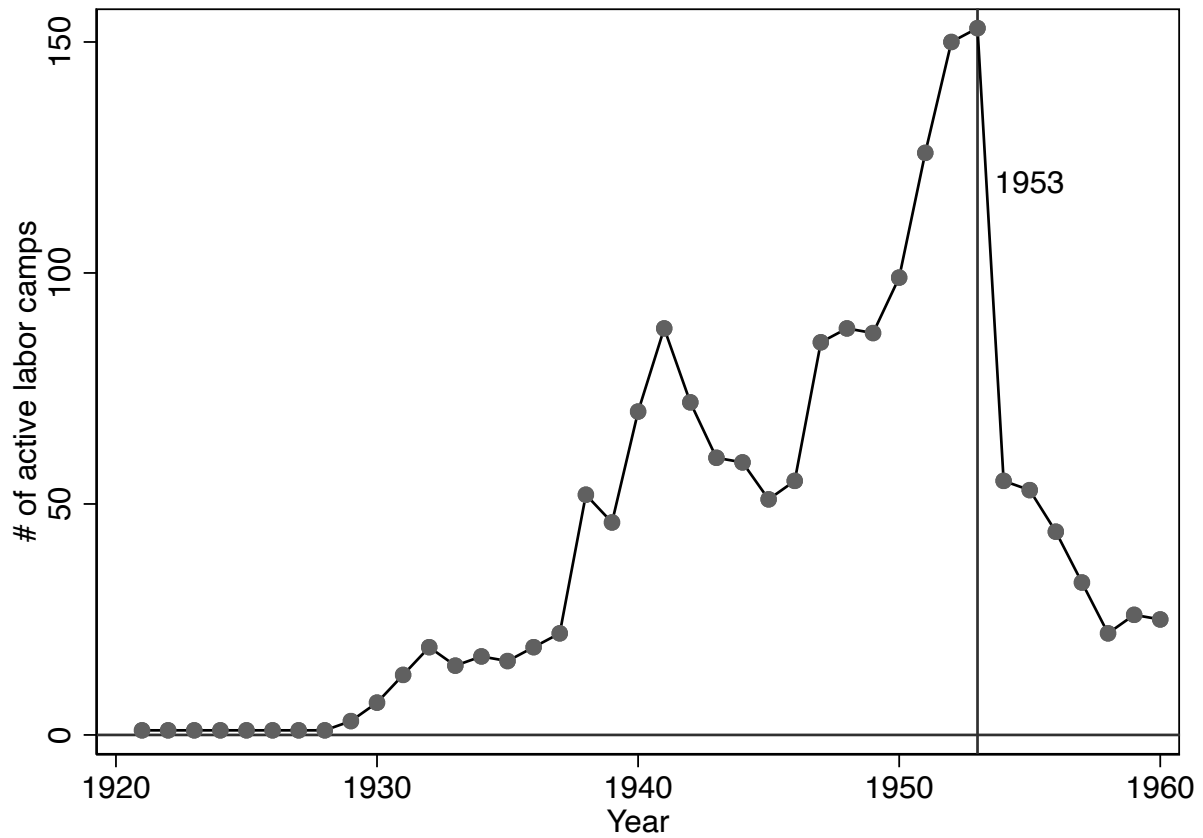
Notes: This Figure shows the number of active Gulag labor camps during Gulag's history, 1921–1960. There are two local maximum of prisoners, the first during the first in 1941 at the beginning of the German invasion of the Soviet Union and the second in 1953 right before the death of Joseph Stalin.

Figure A.5 − Histogram of 1953 Amnesty by Gulag Camp (# and Inverse Hyperbolic Sine)

Panel A: Amnesty size, #   Panel B: $\sinh^{-1}$(Amnesty size, #)
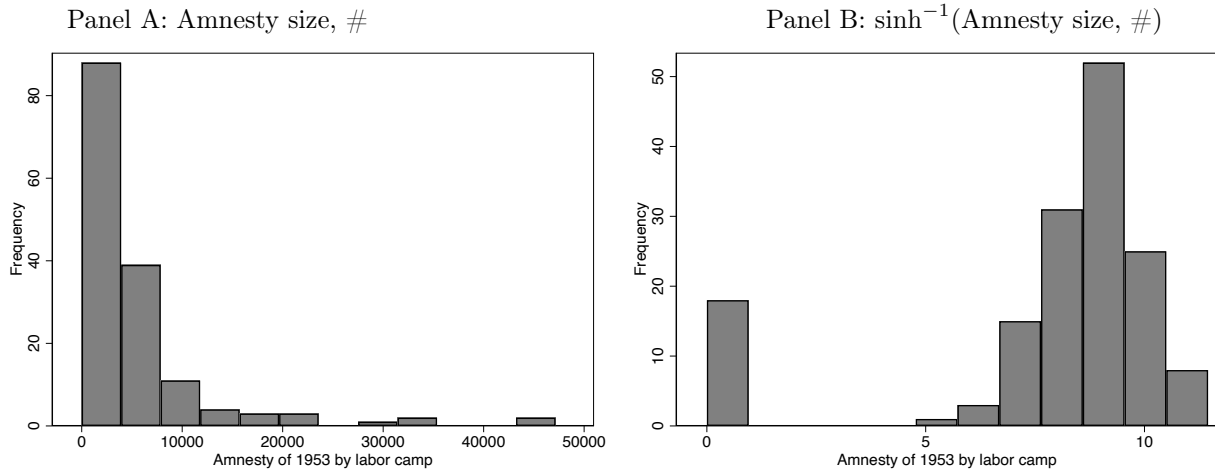
Notes: This Figure shows histograms of the amnesty of 1953 for 153 Gulag camps on the territory of the former Soviet Union that were operational in 1953. Panel A shows the raw numbers of released prisoners. Panel B uses inverse hyperbolic sine. 18 camps did not release any prisoners or slightly increased the number of prisoners. We set the number of amnestied prisoners from these camps to be equal to zero.

Figure A.6 − Rayon-Level Exposure to the Amnesty of 1953
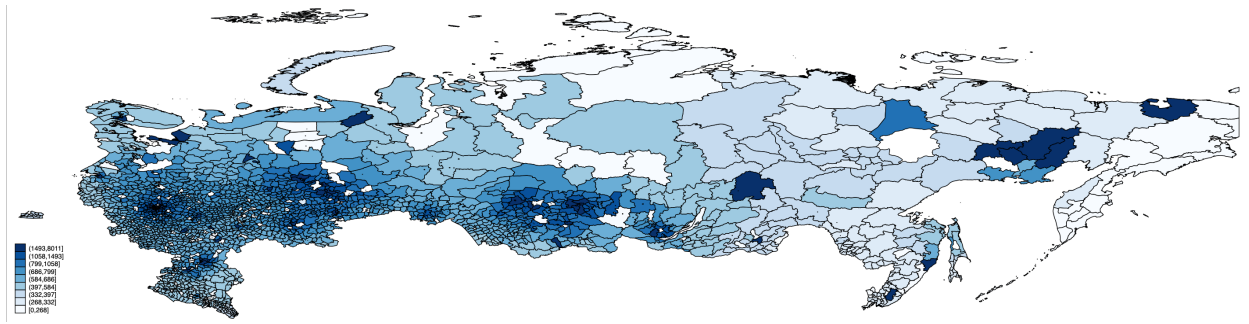
Notes: This map shows the rayon-level variation in the exposure to the amnesty of 1953.

Table A.2 – Summary Statistics: HILDA

Variable	Mean	St. dev.	Min.	Max.	# years question was asked
Sample: Men					
Homosexuals should have equal rights	4.39	2.30	1	7	5
Respondent ever was in prison	0.03	0.13	0	1	19
Respondent's close family member ever was in prison	0.05	0.22	0	1	19
# times respondent was in prison	0.02	0.18	0	6	19
# times respondent's close family members were in prison	0.08	0.40	0	10	19
# years respondent spent in prison	0.05	0.39	0	12	19
Respondent was incarcerated last year	0.008	0.06	0	1	19
Respondent's close family member was incarcerated last year	0.01	0.11	0	1	19
Respondent self-identify as a gay	0.01	0.10	0	1	2
# hours volunteering/charity work	0.79	3.24	0	128	19
People in the neighborhood can be trusted	4.70	1.38	1	7	4
Most people can be trusted	4.76	1.33	1	7	7
Most important role in life being a mother	5.12	1.73	1	7	6
Man earns the money and the woman takes care of the home and children	3.43	1.86	1	7	6
It is not good for a relationship if the woman earns more than the man	2.46	1.58	1	7	5
Income, Australian dollars	44,235	43,242	0	877,097	19
# times same respondent appears in HILDA	10.12	6.15	2	19	-
Sample: Women					
Homosexuals should have equal rights	5.04	2.20	1	7	5
Respondent ever was in prison	0.01	0.08	0	1	19
Respondent's close family member ever was in prison	0.07	0.25	0	1	19
# times respondent was in prison	0.01	0.10	0	6	19
# times respondent's close family members were in prison	0.12	0.60	0	13	19
# years respondent spent in prison	0.02	0.24	0	17	19
Respondent was incarcerated last year	0.002	0.04	0	1	19
Respondent's close family member was incarcerated last year	0.02	0.13	0	1	19
Respondent self-identify as a gay	0.02	0.13	0	1	2
# hours volunteering/charity work	0.90	3.25	0	128	19
People in the neighborhood can be trusted	4.71	1.43	1	7	4
Most people can be trusted	4.84	1.37	1	7	7
Most important role in life being a mother	5.38	1.81	1	7	6
Man earns the money and the woman takes care of the home and children	3.01	1.93	1	7	6
It is not good for a relationship if the woman earns more than the man	2.36	1.64	1	7	5
Income, Australian dollars	30,483	28,189	0	877,097	19
# times same respondent appears in HILDA	10.6	6.22	2	19	-

Notes: This Table shows summary statistics for the main outcome and explanatory variables from the HILDA longitudinal survey. In total, survey covers the years from 2001 to 2019. There are 288,073 observations in total, of them 136,456 — males. There are 27,206 unique respondents in the data, of them 13,219 — males.

B   Additional Results for the Effect of Amnesty of 1953 on Prison Culture and Homophobia in Russia

Table B.1 – Balance Table for the Numbers of Released Prisoners During the Amnesty of 1953

	I	II	III
	Coefficient	S.E.	P-value
Gulag economic geography:			
Log cumulative # of prisoners before 1953	-0.0011	(0.0042)	[0.8020]
Log # of prisoners before the amnesty, 1953	0.0635	(0.1445)	[0.6636]
Log maximum camp capacity	0.0001	(0.0003)	[0.7110]
Δ # of prisoners before the amnesty (1952-1953)	0.0026	(0.0047)	[0.5846]
Log average # of prisoners before 1953	-0.0991	(0.1038)	[0.3476]
Camp's longitude	-0.2079	(0.1968)	[0.2998]
Camp's latitude	0.0033	(0.0029)	[0.2647]
Camp employs prisoners in manufacturing (dummy)	-0.9626	(1.2299)	[0.4404]
~ in natural resources extraction	2.5861	(1.9039)	[0.1870]
~ in construction	-0.8723	(1.3849)	[0.5350]
~ in agriculture	0.0972	(0.1640)	[0.5585]

Notes: The unit of observation here is a camp. Column I contains the coefficient of the bivariate regression of exposure to the amnesty of 1953 on various outcomes. Column II reports robust standard errors for the pre-1953 coronations specification in the first line and robust standard errors in other regressions. Column III reports p-values. Note, that for the first two regressions with 'Log cumulative # of prisoners before 1953' and 'Log # of prisoners before the amnesty, 1953' we drop one outlier — Construction Correctional Labor Camp #16 that released almost 50,000 prisoners. Dummy for manufacturing industries assigns the value of one to those camps that employed prisoners in metallurgy, military industry, machinery, food industry, construction materials, and zero otherwise. Dummy for the extraction of natural resources assigns the value of one to those camps that employed prisoners in the fuel and energy industry, coal mining, uranium mining, gold mining, tin mining, other metallic ore mining, apatite mining, stone quarrying, and zero otherwise. Dummy for construction assigns the value of one to those camps that employed prisoners in the construction of extraction facilities, housing construction, industrial construction, infrastructural construction, and zero otherwise. Dummy for agriculture assigns the value of one to those camps that employed prisoners in agriculture and timber production, and zero otherwise. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table B.2 – Balance Table for Exposure to Amnesty of 1953

	I	II	III
	Coefficient	S.E.	P-value
Pre-treatment # of coronations of thieves-in-law:			
Ihs # of coronations of thieves-in-law, 1922-1953	-0.0011	(0.0042)	[0.8020]
Historical controls, levels:			
Share of Orthodox population, 1897	0.0635	(0.1445)	[0.6636]
Share of Catholic population, 1897	0.0001	(0.0003)	[0.7110]
Share of Protestant population, 1897	0.0026	(0.0047)	[0.5846]
Share of Muslim population, 1897	-0.0991	(0.1038)	[0.3476]
Sex ratios (women/men), 1897	-0.2079	(0.1968)	[0.2998]
Sex ratios (women/men), 1959	0.0033	(0.0029)	[0.2647]
Log population, 1897	-0.9626	(1.2299)	[0.4404]
Log manufacturing output, 1897	2.5861	(1.9039)	[0.1870]
Historical controls, changes:			
Δ Log manufacturing output, 1959-1897	-0.8723	(1.3849)	[0.5350]
Δ Log population, 1959-1897	0.9169	(1.0626)	[0.3958]
Δ Sex ratios (women/men), 1959-1897	0.0972	(0.1640)	[0.5585]

Notes: Observation for the pre-1953 coronations in the first line is *rayon*. All other observations are provinces. Column I contains the coefficient of the bivariate regression of exposure to the amnesty of 1953 on various outcomes. Column II reports robust clustered on province-level standard errors for the pre-1953 coronations specification in the first line and robust standard errors in other regressions. Column III reports p-values. Note, we do not use censi from 1926, 1937, and 1939 because they don't have data on manufacturing output and they are not publicly available on https://ristat.org/topics. *** p<0.01, ** p<0.05, * p<0.1

Table B.3 – Robustness for Table 1: Alternative Standard Errors

	I	II	III	IV	V
Panel A: ~Baseline clustered by province	Dependent variable: Ihs # coronations (mean 0.007 st.dev. 0.115)				
Exposure to 1953 amnesty x Post amnesty	0.065***	0.065***	0.065***	0.060***	0.060***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Panel B: ~ spatial HAC, 150 km					
Exposure to 1953 amnesty x Post amnesty	0.065***	0.065***	0.065***	0.060***	0.060***
	(0.015)	(0.015)	(0.015)	(0.013)	(0.013)
Panel C: ~ spatial HAC, 300 km					
Exposure to 1953 amnesty x Post amnesty	0.065***	0.065***	0.065***	0.060***	0.060***
	(0.015)	(0.015)	(0.015)	(0.013)	(0.013)
R-squared	0.376	0.377	0.377	0.408	0.415
Observations	23,260	23,260	23,260	23,260	23,260
Location FE	✓	✓	✓	✓	✓
Decade FE	✓	✓	✓	✓	✓
Min. distance to Gulag camp x time trends		✓	✓	✓	✓
Population of the closest Gulag camp x time trends			✓	✓	✓
Exposure to total Gulag population x time trends				✓	✓
Convict labor industry FEs of closest Gulag camp x time trends					✓

Notes: This Table replicates Table 1 but uses alternative methods of computing standard errors. Panel A clusters standard errors on the province level (83 clusters). Panels B and C report spatially corrected HAC standard errors with 150 and 300 km thresholds, respectively. Standard errors in Panels B and C start to be different only on the 4th digit after the dot. Standard errors clustered at the county (*rayon*) level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.4 − Robustness for Table 1: Alternative Measures of Exposure to the Amnesty 0f 1953

	I	II	III	IV	V	VI	VII	VIII
	Dependent variable: Ihs # coronations (mean 0.007 st.dev. 0.115)							
Exposure to 1953 amnesty x Post amnesty								
Baseline ∼ ∑ (# released)/(distance)	0.060***							
	(0.003)							
∑ (# released)/(log distance)		0.079***						
		(0.006)						
∑ (# released)/(sqrt. distance)			0.074***					
			(0.009)					
∑ (log # released)/(log distance)				0.079***				
				(0.008)				
∑ (sqrt. # released)/(sqrt. distance)					0.080***			
					(0.005)			
∑ (# released)/(distance + distance^2)						0.057***		
						(0.004)		
∑ (# released in rayon i)							0.075***	
							(0.003)	
log ∑ (# released in rayon i)								0.080***
								(0.005)
R-squared	0.415	0.454	0.444	0.453	0.453	0.410	0.509	0.516
Observations	23,260	23,260	23,260	23,260	23,260	23,260	23,260	23,260

Notes: This Table replicates Column V of Table 1 but uses alternative functional forms of weights when computing the measure of exposure to the amnesty. Column I provides the baseline coefficient from Column V of Table 1 for comparison. All exposures to the amnesty are normalized to have a mean of 0 and a standard deviation of 1. Standard errors clustered at the county (*rayon*) level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure B.1 – Results on Thieves-in-Law are Not Driven by a Particular Province
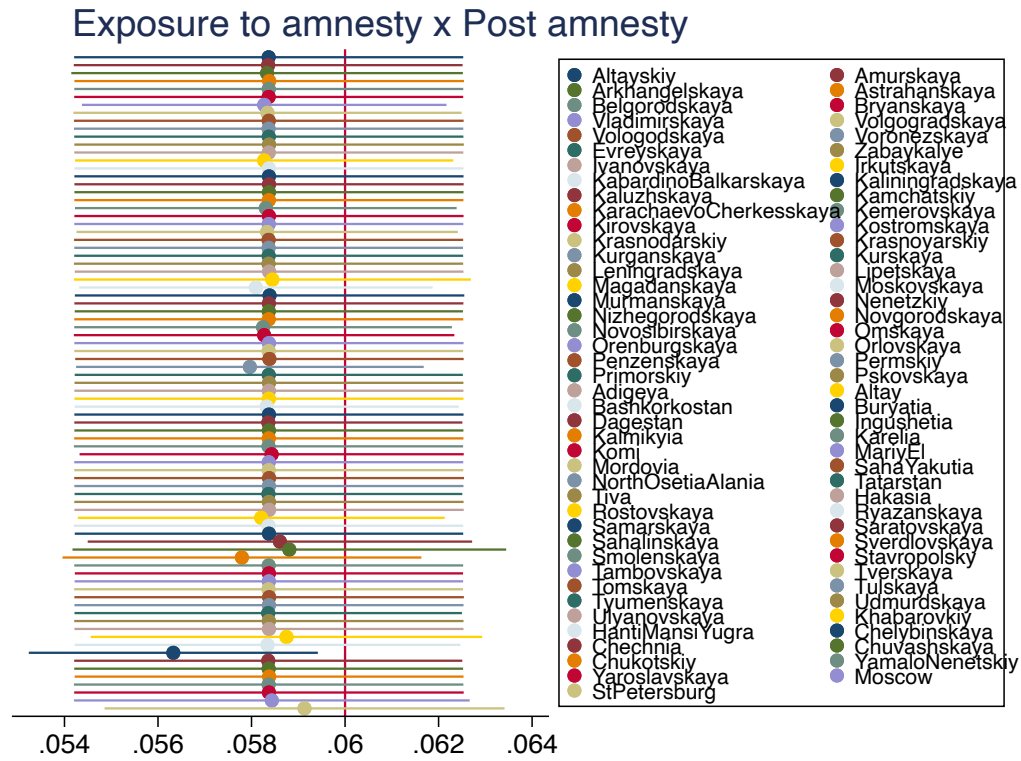
Notes: This Figure reports on the point-estimate and 95th-percent confidence band that results when re-estimating the specification in Column VI of Table 1, dropping one province at a time. The (red) vertical line is the baseline point estimate. The results are sorted left-to-right and top-to-bottom, i.e., Altayskiy Kray, Amurskaya Oblast, Arkhangelskaya oblast, etc. The results are sorted alphabetically, except for the cities of Moscow and St. Petersburgh, which are at the end.

Table B.5 − Robustness for Table 1: Municipality-Level Specification

	I	II	III	IV	V
	Dependent variable: Ihs # coronations (mean 0.002 st.dev. 0.056)				
Exposure to 1953 amnesty x Post amnesty	0.003**	0.003**	0.003**	0.003*	0.003*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
R-squared	0.296	0.296	0.296	0.298	0.299
Observations	98,290	98,290	98,290	98,290	98,290
Location FE	✓	✓	✓	✓	✓
Decade FE	✓	✓	✓	✓	✓
Min. distance to Gulag camp x time trends		✓	✓	✓	✓
Population of the closest Gulag camp x time trends			✓	✓	✓
Exposure to total Gulag population x time trends				✓	✓
Convict labor industry FEs of closest Gulag camp x time trends					✓

Notes: Panel A of this Table replicates Table 1 but uses municipality as a unit of observation instead of county (*rayon*). Standard errors clustered at the location level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure B.2 − FDDD Analysis: No Increase in Number of Thieves-in-Law Coronations Before 1953 and Increase After the Amnesty, Municipality-Level

Panel A: Baseline   Panel B: w proximity to Gulag × decade FEs
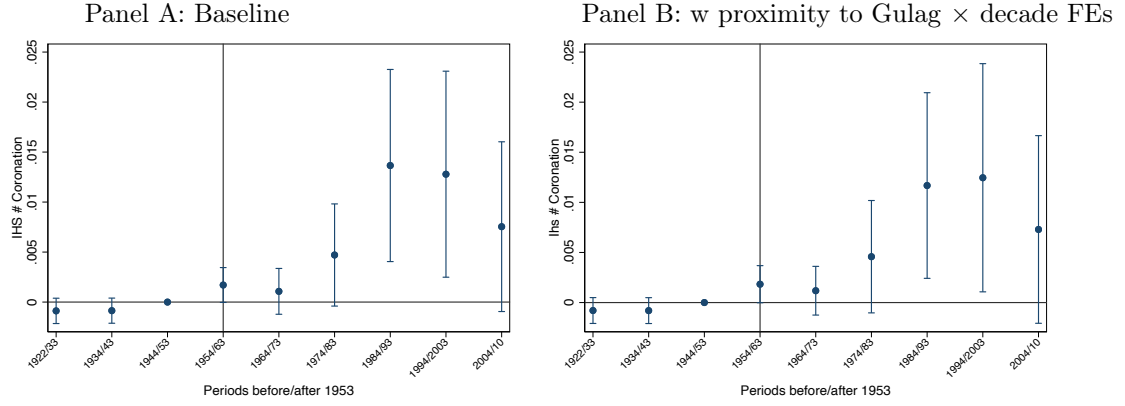
Notes: This Figure graphs the results of estimating Equation 2 but uses location-level coordinates instead of *rayon*-level. The dependent variable is the inverse hyperbolic sine of the number of coronations of thieves-in-law. The p-value for the joint significance of the pre-trend's coefficients is equal to 0.6669 in Panel A and 0.8208 in Panel B. This figure reports 95th-percent confidence bands. Columns III and IV of Table B.6 contain the estimates for the specifications in Panel A and B, respectively. Standard errors clustered at the location (*rayon*) level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.6 – Results for the Fully Dynamic Specifications in Figure 4 and Figure B.2

	I	II	III	IV
		Dependent variable: Ihs # coronations		
Observation	Rayon-decade		Location-decade	
Exposure to 1953 amnesty				
x 1922-1933	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.001)	(0.001)	(0.001)
x 1934-1943	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.001)	(0.001)	(0.001)
x 1954-1963	0.018***	0.018***	0.002	0.002
	(0.000)	(0.001)	(0.001)	(0.001)
x 1964-1973	0.018***	0.017***	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
x 1974-1983	0.053***	0.050***	0.005	0.005
	(0.003)	(0.003)	(0.003)	(0.003)
x 1984-1993	0.095***	0.077***	0.014**	0.012**
	(0.005)	(0.004)	(0.006)	(0.006)
x 1994-2003	0.119***	0.108***	0.013**	0.012*
	(0.004)	(0.007)	(0.006)	(0.007)
x 2004-2010	0.084***	0.078***	0.008	0.007
	(0.003)	(0.005)	(0.005)	(0.006)
Joint F-test for pre-trend coef., p-value	[0.7222]	[0.7557]	[0.6669]	[0.8208]
Rayon/location & decade FEs	✓	✓	✓	✓
Controls		✓		✓
R-squared	0.493	0.527	0.336	0.337
Observations	23,260	23,260	98,290	98,290

Notes: This Table reports coefficients for the event-study specifications in Figure 4 and Figure B.2. Standard errors clustered at the county (*rayon*) level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Figure B.3 – Robustness for Panel A of Figure 4: Robustness to the Methodology in Borusyak, Jaravel and Spiess (2021)
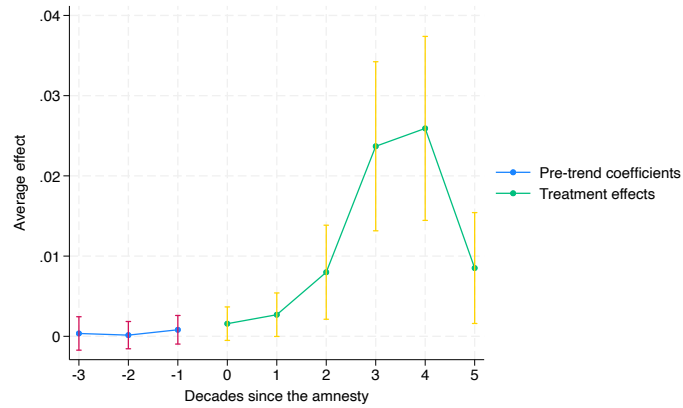
Notes: This Figure replicates the specification in Panel A of Figure 4 but uses the methodology proposed in Borusyak, Jaravel and Spiess (2021).

Table B.7 − Robustness for Table 2: Additional Controls

	I	II	III	IV	V	VI
Panel A:	Dependent variable: Ihs # crimes against LGBTQ+ (mean 0.024 st.dev. 0.307)					
Exposure to 1953 amnesty	0.0273***	0.0210**	0.0138*	0.0280***	0.0273***	0.0164**
	(0.0101)	(0.0090)	(0.0073)	(0.0104)	(0.0101)	(0.0077)
R-squared	0.018	0.057	0.101	0.018	0.018	0.023
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel B:	Dependent variable: Ihs # homophobic slur in VK (mean 0.008 st.dev. 0.127)					
Exposure to 1953 amnesty	0.0110**	0.0092*	0.0068*	0.0108*	0.0110**	0.0064**
	(0.0054)	(0.0051)	(0.0041)	(0.0055)	(0.0054)	(0.0027)
R-squared	0.037	0.057	0.083	0.037	0.037	0.056
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel C:	Dependent variable: 1(Dislike homosexuals) (mean 0.616 st.dev. 0.486)					
Exposure to 1953 amnesty	0.1337***	0.1507*	0.1060*	0.0942*	0.1407***	0.3240***
	(0.0506)	(0.0781)	(0.0544)	(0.0521)	(0.0511)	(0.0774)
Survey-year FEs	✓	✓	✓	✓	✓	✓
R-squared	0.066	0.067	0.067	0.101	0.067	0.166
Observations	6,519	6,519	6,519	6,519	6,519	6,519
Baseline controls	✓	✓	✓	✓	✓	✓
Location's classification FEs		✓				
Location's log population			✓			
Federal district FEs				✓		
Ethnic republics FEs					✓	
Province FEs						✓

Notes: This Table shows the robustness of Table 2 to the inclusion of additional controls. Baseline controls include controls from the corresponding specifications from Column VI of Table 2. Column II includes fixed effects for the type of location: provincial capital, city, township, and big (*poselok*) or small (*selo*) village. Column III includes control for the location's log of the population from the 2020 Population Census. Column IV includes fixed effects for eight federal districts representing a collection of provinces: Central, Northwestern, Southern, North Caucasian, Volga, Ural, Siberian, and Far Eastern. Column V includes a dummy for ethnic republics. Column VI includes province fixed effects. Standard errors clustered at the province level (in Panels A and B) and on respondents' location level (in Panel C) are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.8 – Robustness for Panel C of Table 2: Additional Individual-Level Controls

	I	II	III	IV	V	VI
	Dependent variable: 1(Dislike homosexuals)					
Exposure to 1953 amnesty	0.1280**	0.1282**	0.1281**	0.1219**	0.1105**	0.1273***
	(0.0504)	(0.0512)	(0.0512)	(0.0496)	(0.0490)	(0.0489)
Baseline controls	✓	✓	✓	✓	✓	✓
Age & gender	✓	✓	✓	✓	✓	✓
Marital status		✓	✓	✓	✓	✓
Ethnicity FEs			✓	✓	✓	✓
Religion FEs				✓	✓	✓
Education FEs					✓	✓
Log income & occupation FEs						✓
R-squared	0.069	0.071	0.071	0.079	0.089	0.096
Observations	6,522	6,522	6,522	6,522	6,522	6,522

Notes: This Table shows the robustness of Panel C of Table 2 to the inclusion of additional individual controls. Baseline controls include controls from the corresponding specifications from Column VI of Table 2. Income is a self-reported household's monthly income. We harmonized variables for marital status, ethnicity, religion, and education between the surveys. We use survey-specific occupation fixed effects, as occupations are not comparable between surveys. This Table uses survey population weights. Standard errors clustered at the location level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure B.4 – Robustness for Table 2: Results are Not Driven by a Particular Region

Panel A: Log # of Crimes Against LGBTQ+
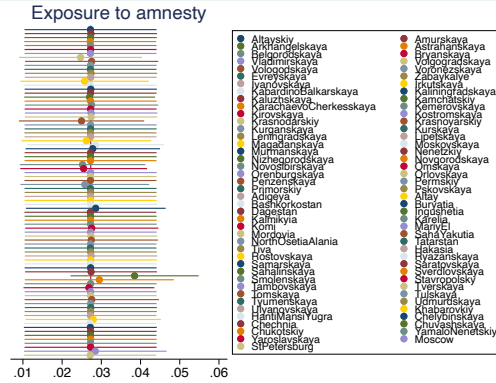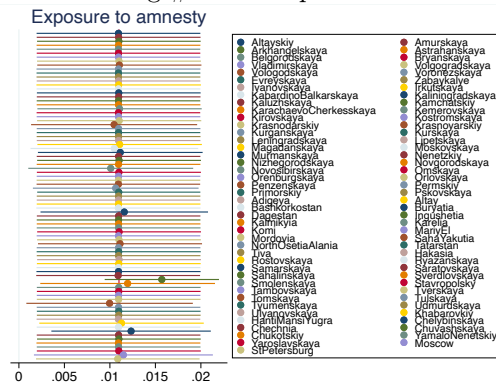
Panel B: Log # of Homophobic Slur in VK

Panel C: 1(Dislike Homosexuals)
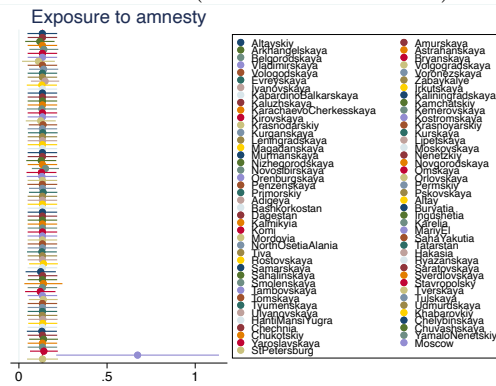
Notes: This Figure reports on the point-estimate and 90th-percent confidence band that results when re-estimating the specification in Column VI of Table 2, dropping one province at a time. The (red) vertical line is the baseline point estimate. The results are sorted alphabetically, except for the cities of Moscow and St. Petersburgh, which are at the end.

Table B.9 – Robustness for Table 2: Alternative Measures of Exposure to Amnesty (Alternative Decay Rates)

	I	II	III	IV	V	VI
Panel A:	Dependent variable: Log # crimes against LGBTQ+ (mean 0.024 st.dev. 0.307)					
Exposure to 1953 amnesty						
Baseline ~ ∑ (# released)/(distance)	0.0273***					
	(0.0101)					
∑ (# released)/(log distance)		0.0225**				
		(0.0099)				
∑ (# released)/(sqrt. distance)			0.0252**			
			(0.0104)			
∑ (log # released)/(log distance)				0.0208**		
				(0.0085)		
∑ (sqrt. # released)/(sqrt. distance)					0.0269**	
					(0.0107)	
∑ (# released)/(distance + distance^2)						0.0147
						(0.0113)
R-squared	0.018	0.013	0.014	0.012	0.014	0.012
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel B:	Dependent variable: Log # homophobic slur in VK (mean 0.008 st.dev. 0.127)					
Exposure to 1953 amnesty						
Baseline ~ ∑ (# released)/(distance)	0.0110**					
	(0.0054)					
∑ (# released)/(log distance)		0.0112*				
		(0.0059)				
∑ (# released)/(sqrt. distance)			0.0132**			
			(0.0054)			
∑ (log # released)/(log distance)				0.0065*		
				(0.0040)		
∑ (sqrt. # released)/(sqrt. distance)					0.0110**	
					(0.0054)	
∑ (# released)/(distance + distance^2)						0.0051
						(0.0056)
R-squared	0.037	0.034	0.036	0.031	0.033	0.032
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel C:	Dependent variable: 1(Dislike homosexuals) (mean 0.616 st.dev. 0.486)					
Exposure to 1953 amnesty						
Baseline ~ ∑ (# released)/(distance)	0.1334***					
	(0.0508)					
∑ (# released)/(log distance)		0.1272**				
		(0.0621)				
∑ (# released)/(sqrt. distance)			0.1812***			
			(0.0616)			
∑ (log # released)/(log distance)				0.1050**		
				(0.0481)		
∑ (sqrt. # released)/(sqrt. distance)					0.1423**	
					(0.0582)	
∑ (# released)/(distance + distance^2)						0.2389***
						(0.0521)
R-squared	0.066	0.067	0.067	0.065	0.066	0.068
Observations	6,519	6,519	6,519	6,519	6,519	6,519

Notes: This Table replicates Column VI of Table 2 but uses alternative measures of exposure to the amnesty of 1953. In Panels A and B standard errors clustered at the province level are in parentheses. In Panel C standard errors clustered at the location level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.10 – Robustness for Table 2: Alternative Measures of Exposure to Amnesty (Exposure to Only the Nearest Gulag Camp)

	I	II	III	IV	V	VI
Panel A:	Dependent variable: Log # crimes against LGBTQ+ (mean 0.024 st.dev. 0.307)					
Exposure to 1953 amnesty x Post amnesty						
(# released in closest)/(distance to closest)	0.0064					
	(0.0071)					
(# released in closest)/(log distance to closest)		0.0073				
		(0.0065)				
(# released in closest)/(sqrt. distance to closest)			0.0133			
			(0.0086)			
(log # released in closest)/(log distance to closest)				0.0025		
				(0.0043)		
(sqrt. # released in closest)/(sqrt. distance to closest)					0.0120	
					(0.0084)	
(# released in closest)/(distance + distance^2 to closest)						0.0015
						(0.0035)
R-squared	0.009	0.009	0.010	0.009	0.010	0.009
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel B:	Dependent variable: Log # homophobic slur in VK (mean 0.008 st.dev. 0.127)					
Exposure to 1953 amnesty x Post amnesty						
(# released in closest)/(distance to closest)	0.0110**					
	(0.0054)					
(# released in closest)/(log distance to closest)		0.0112*				
		(0.0059)				
(# released in closest)/(sqrt. distance to closest)			0.0132**			
			(0.0054)			
(log # released in closest)/(log distance to closest)				0.0065*		
				(0.0040)		
(sqrt. # released in closest)/(sqrt. distance to closest)					0.0110**	
					(0.0054)	
(# released in closest)/(distance + distance^2 to closest)						0.0051
						(0.0056)
R-squared	0.016	0.016	0.017	0.015	0.016	0.015
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel C:	Dependent variable: 1(Dislike homosexuals) (mean 0.616 st.dev. 0.486)					
Exposure to 1953 amnesty x Post amnesty						
(# released in closest)/(distance to closest)	-0.0122					
	(0.0139)					
(# released in closest)/(log distance to closest)		-0.0235				
		(0.0248)				
(# released in closest)/(sqrt. distance to closest)			-0.0249			
			(0.0252)			
(log # released in closest)/(log distance to closest)				-0.0194		
				(0.0234)		
(sqrt. # released in closest)/(sqrt. distance to closest)					-0.0258	
					(0.0232)	
(# released in closest)/(distance + distance^2 to closest)						0.0055
						(0.0167)
R-squared	0.065	0.065	0.064	0.063	0.064	0.062
Observations	6,519	6,519	6,519	6,519	6,519	6,519

Notes: This Table replicates Column VI of Table 2 but uses alternative measures of exposure to the amnesty of 1953. In Panels A and B standard errors clustered at the province level are in parentheses. In Panel C standard errors clustered at the location level are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table B.11 – Robustness for Table 2: Alternative Measures of Exposure to Amnesty (Thresholds of Effect w/o Decay Rate)

	I	II	III	IV	V	VI
Panel A:	Dependent variable: Log # crimes against LGBTQ+ (mean 0.024 st.dev. 0.307)					
Exposure to 1953 amnesty (thresholds)						
50 km	0.0233*					
	(0.0121)					
150 km		0.0177				
		(0.0122)				
300 km			0.0135			
			(0.0089)			
500 km				0.0133*		
				(0.0070)		
750 km					0.0094	
					(0.0075)	
1000 km						0.0080
						(0.0068)
R-squared	0.014	0.011	0.010	0.010	0.009	0.009
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel B:	Dependent variable: Log # homophobic slur in VK (mean 0.008 st.dev. 0.127)					
Exposure to 1953 amnesty (thresholds)						
50 km	0.0128***					
	(0.0044)					
150 km		0.0105**				
		(0.0051)				
300 km			0.0086*			
			(0.0045)			
500 km				0.0079**		
				(0.0039)		
750 km					0.0067	
					(0.0041)	
1000 km						0.0052
						(0.0037)
R-squared	0.024	0.021	0.019	0.018	0.017	0.016
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel C:	Dependent variable: 1(Dislike homosexuals) (mean 0.616 st.dev. 0.486)					
Exposure to 1953 amnesty (thresholds)						
50 km	0.0299					
	(0.0762)					
150 km		0.1014				
		(0.0675)				
300 km			0.1489***			
			(0.0527)			
500 km				0.1553***		
				(0.0476)		
750 km					0.1460**	
					(0.0626)	
1000 km						0.1436**
						(0.0631)
R-squared	0.065	0.066	0.067	0.069	0.067	0.067
Observations	6,519	6,519	6,519	6,519	6,519	6,519

Notes: This Table replicates Column VI of Table 2 but uses alternative measures of exposure to the amnesty of 1953. All explanatory variables are normalized (with a mean of 0 and standard deviation of 1) and constructed as a sum of released prisoners within a radius (specified threshold) of that location. In Panels A and B standard errors clustered at the province level are in parentheses. In Panel C standard errors clustered at the location level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.12 – Robustness for Table 2: Alternative Spatially Adjusted Standard Errors

	I	II	III	IV	V	VI
Panel A: ~baseline Panel A with 150 km cutoff	Dependent variable: Ihs # crimes against LGBTQ+ (mean 0.024 st.dev. 0.307)					
Exposure to 1953 amnesty	0.0311***	0.0307***	0.0303***	0.0303***	0.0267***	0.0273***
	(0.0093)	(0.0093)	(0.0094)	(0.0094)	(0.0091)	(0.0091)
R-squared	0.011	0.011	0.011	0.011	0.015	0.018
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel B: ~baseline Panel A with 300 km cutoff						
Exposure to 1953 amnesty	0.0311***	0.0307***	0.0303***	0.0303***	0.0267***	0.0273***
	(0.0092)	(0.0092)	(0.0093)	(0.0093)	(0.0091)	(0.0091)
R-squared	0.011	0.011	0.011	0.011	0.015	0.018
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel C: ~baseline Panel B with 150 km cutoff	Dependent variable: Ihs # homophobic slur in VK (mean 0.008 st.dev. 0.127)					
Exposure to 1953 amnesty	0.0143***	0.0143***	0.0140***	0.0140***	0.0107*	0.0110**
	(0.0052)	(0.0050)	(0.0051)	(0.0051)	(0.0055)	(0.0055)
R-squared	0.013	0.013	0.013	0.013	0.035	0.037
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel D: ~baseline Panel B with 300 km cutoff						
Exposure to 1953 amnesty	0.0143***	0.0143***	0.0140***	0.0140***	0.0107*	0.0110**
	(0.0052)	(0.0050)	(0.0051)	(0.0051)	(0.0055)	(0.0055)
R-squared	0.013	0.013	0.013	0.013	0.035	0.037
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Latitude & longitude		✓	✓	✓	✓	✓
Min. distance to Gulag camp			✓	✓	✓	✓
Population of the closest Gulag camp				✓	✓	✓
Exposure to total Gulag population					✓	✓
Convict labor industry FEs of closest Gulag camp						✓

Notes: This Table replicates Panels A and B of Table 2 but uses alternative ways of computing standard errors. Spatially corrected standard errors with a 150 km (300 km) threshold are in parentheses in Panels A and C (Panels B and D). *** p<0.01, ** p<0.05, * p<0.1

Table B.13 − Robustness for Table 2: *Rayon*-Level Results

	I	II	III	IV	V	VI
Panel A: ~baselin Panel A on rayon-level	Dependent variable: Ihs # crimes against LGBTQ+ (mean 0.101 st.dev. 0.622)					
Exposure to 1953 amnesty	0.0645**	0.0646**	0.0614**	0.0614**	0.0551**	0.0567**
	(0.0269)	(0.0274)	(0.0269)	(0.0269)	(0.0264)	(0.0261)
R-squared	0.010	0.010	0.012	0.012	0.014	0.023
Observations	2,314	2,314	2,314	2,314	2,314	2,314
Panel B: ~baseline Panel B on rayon-level	Dependent variable: Ihs # homophobic slur in VK (mean 0.033 st.dev. 0.260)					
Exposure to 1953 amnesty	0.0316**	0.0319**	0.0303**	0.0303**	0.0263**	0.0266**
	(0.0128)	(0.0127)	(0.0126)	(0.0126)	(0.0128)	(0.0122)
R-squared	0.014	0.015	0.017	0.017	0.022	0.029
Observations	2,314	2,314	2,314	2,314	2,314	2,314
Latitude & longitude		✓	✓	✓	✓	✓
Min. distance to Gulag camp			✓	✓	✓	✓
Population of the closest Gulag camp				✓	✓	✓
Exposure to total Gulag population					✓	✓
Convict labor industry FEs of closest Gulag camp						✓

Notes: This Table replicates Panels A and B of Table 2 but uses a different unit of observation — it uses a county (*rayon*) instead of municipality. Standard errors clustered at the province level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.14 – Amnesty From Female Labor Camps Has No Effect on Homophobia

	I	II	III	IV	V	VI
Panel A:	Dependent variable: Ihs # crimes against LGBTQ+ (mean 0.024 st.dev. 0.307)					
Exposure to 1953 amnesty	-0.0025	-0.0031	-0.0035	-0.0035	-0.0023	-0.0029
	(0.0025)	(0.0025)	(0.0025)	(0.0025)	(0.0024)	(0.0025)
R-squared	0.000	0.001	0.001	0.001	0.008	0.010
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel B:	Dependent variable: Ihs # homophobic slur in VK (mean 0.008 st.dev. 0.127)					
Exposure to 1953 amnesty	-0.0020*	-0.0019*	-0.0022**	-0.0022**	-0.0012	-0.0004
	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0009)	(0.0008)
R-squared	0.000	0.000	0.002	0.002	0.028	0.030
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel C:	Dependent variable: 1(Dislike homosexuals) (mean 0.616 st.dev. 0.486)					
Exposure to 1953 amnesty	-0.0052	-0.0058	-0.0049	-0.0070	-0.0035	-0.0083
	(0.0179)	(0.0178)	(0.0179)	(0.0176)	(0.0188)	(0.0164)
Survey-year FEs	✓	✓	✓	✓	✓	✓
R-squared	0.005	0.006	0.006	0.012	0.012	0.066
Observations	6,519	6,519	6,519	6,519	6,519	6,519
Latitude & longitude		✓	✓	✓	✓	✓
Min. distance to Gulag camp			✓	✓	✓	✓
Population of the closest Gulag camp				✓	✓	✓
Exposure to total Gulag population					✓	✓
Convict labor industry FEs of closest Gulag camp						✓

Notes: This Table replicates Table 2 but uses exposure to the amnesty from female Gulag labor camps. Standard errors clustered at the province (*oblast'*) level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure B.5 − Placebo Exposure to the Amnesty for All Gulag's Years

Panel A: Log # of Crimes Against LGBTQ+
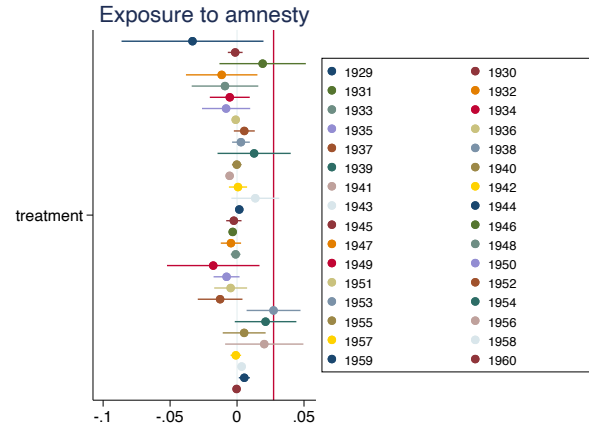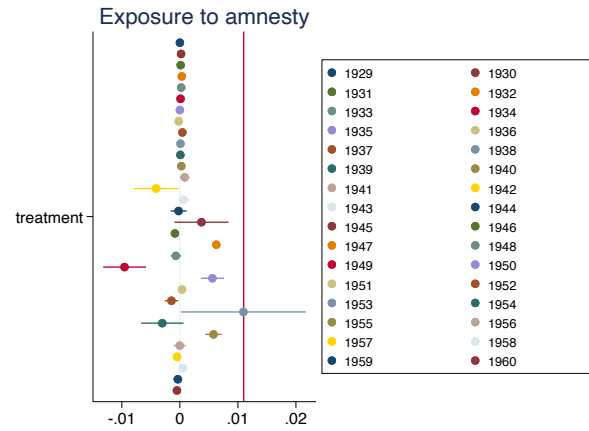
Panel B: Log # of Homophobic Slur in VK

Panel C: 1(Dislike Homosexuals)
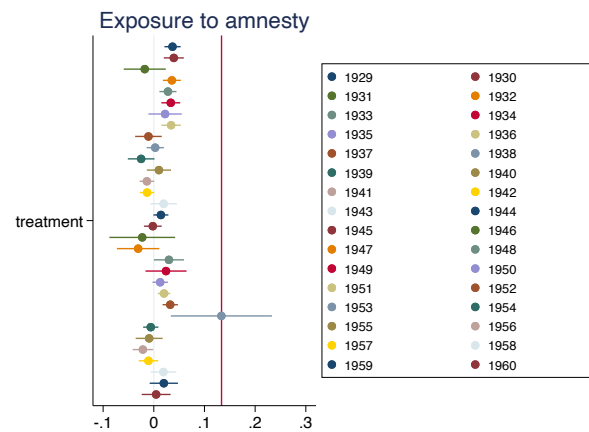
Notes: This Figure reports on the point-estimate and 95th-percent confidence band that results when re-estimating the specification in Column VI of Table 2 but uses amnesty in every year from 1929 to 1960. The (red) vertical line is the baseline point estimate for the amnesty of 1953. The results are sorted numerically from 1929 to 1960.

Figure B.6 – The Effect of Contrafactual Amnesty Size

Panel A: Log # of Crimes Against LGBTQ+

Panel B: Log # of Homophobic Slur in VK

Panel C: 1(Dislike Homosexuals)

Notes: In this Figure, we take the most demanding specification from the baseline results, i.e., Column VI of Table 2. This The figure shows estimated coefficients using 500 placebo amnesties (drawn from the factual distribution of the amnesties with replacement) for the 153 Gulag labor camps existing in 1953. The red vertical line is the true point estimate of β. Panel A reports results for the inverse hyperbolic sine of the number of crimes against LGBTQ+ as the dependent variable. Panel B reports results for the inverse hyperbolic sine of the number of homophobic slurs in VK as the dependent variable. Panel C reports results for the dummy for an individual's homophobia as the dependent variable. In Panel A 25 estimates are larger than the true (4.8 percentile). In Panel B 6 estimates are larger than the true (1.2 percentile). In Panel C one estimate is larger than the true (0.02 percentile).

Figure B.7 – The Effect of Contrafactual Gulag Camps Locations

Panel A: Log # of Crimes Against LGBTQ+
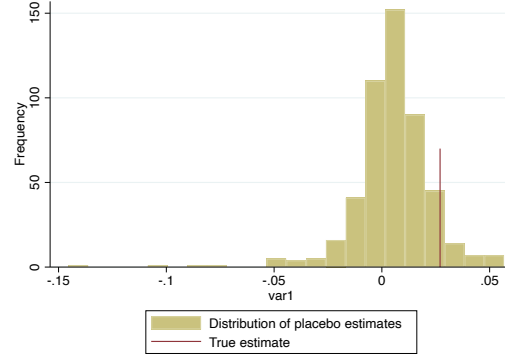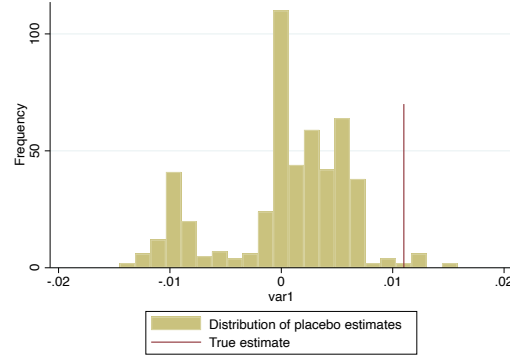
Panel B: Log # of Homophobic Slur in VK

Panel C: 1(Dislike Homosexuals)
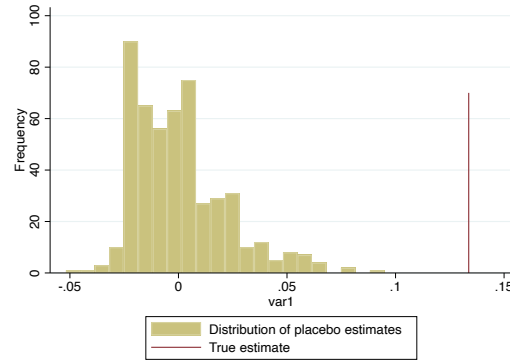
Notes: In this Figure, we take the most demanding specification from the baseline results, i.e., Column VI of Table 2. This figure shows estimated coefficients using 500 placebo amnesties for the 153 factual amnesties randomly assigned to 475 ever-existing Gulag labor camps. The red vertical line is the true point estimate of β. Panel A reports results for the inverse hyperbolic sine of the number of crimes against LGBTQ+ as the dependent variable. Panel B reports results for the inverse hyperbolic sine of the number of homophobic slurs in VK as the dependent variable. Panel C reports results for the dummy for an individual's homophobia as the dependent variable. In Panel A 34 estimates are larger than the true (6.8 percentile). In Panel B 10 estimates are larger than the true (0.2 percentile). In Panel C one estimate is larger than the true (0.02 percentile).

Table B.15 − Effect of Amnesty of 1953 on Residential Homophobia in Post-Soviet Countries (Without Russia)

	I	II	III	IV	V	VI
Panel C: ~ All Soviet Republics with Gulag camps	Dependent variable: 1(Dislike homosexuals) (mean 0.574 st.dev. 0.494)					
Exposure to 1953 amnesty	0.4503**	0.4444*	0.4033*	0.4624*	0.4386*	0.5376***
	(0.1910)	(0.2367)	(0.2447)	(0.2527)	(0.2375)	(0.1897)
Country FEs	✓	✓	✓	✓	✓	✓
R-squared	0.070	0.076	0.076	0.079	0.080	0.126
Observations	14,255	14,255	14,255	14,255	14,255	14,255
Latitude & longitude		✓	✓	✓	✓	✓
Min. distance to Gulag camp			✓	✓	✓	✓
Population of the closest Gulag camp				✓	✓	✓
Exposure to total Gulag population					✓	✓
Convict labor industry FEs of closest Gulag camp						✓

Notes: This Table replicates Panel C of Table 2 but uses data on all post-Soviet countries (without Russia) that had Gulag labor camps. In particular, we use data from Azerbaijan, Georgia, Kazakhstan, Kyrgyzstan Tajikistan, Ukraine, and Uzbekistan. Armenia, Belarus, Estonia, Latvia, Lithuania, and Moldova had never had labor camps. We don't add them to the sample because exposure to the amnesty of 1953 would be collinear with the distance to the Russian border when country fixed effects are added. We also don't use Turkmenistan's data because LiTS for Turkmenistan is not available. This Table weighs all observations using survey population weights. Standard errors clustered on the location level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.16 – Locations More Exposed to Amnesty of 1953 Consume More 'Prison' Music (Russian Chanson)

	I	II	III	IV	V	VI
Panel A:	Dependent variable: Ihs # Chanson searches in Yandex (mean 0.488 st.dev. 1.77)					
Exposure to 1953 amnesty	0.0174***	0.0181***	0.0146**	0.0146**	0.0179***	0.0144**
	(0.0055)	(0.0059)	(0.0058)	(0.0058)	(0.0067)	(0.0068)
R-squared	0.000	0.001	0.001	0.001	0.001	0.010
Observations	2,326	2,326	2,326	2,326	2,326	2,326
Panel B:	Dependent variable: Ihs # Egor Kreed searches in Yandex (mean 0.597 st.dev. 1.69)					
Exposure to 1953 amnesty	0.0140	0.0143	0.0114	0.0103	0.0208	0.0205
	(0.0231)	(0.0228)	(0.0223)	(0.0222)	(0.0244)	(0.0237)
R-squared	0.000	0.000	0.000	0.001	0.002	0.025
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel C:	Dependent variable: Ihs # Slava Marlow searches in Yandex (mean 0.114 st.dev. 0.781)					
Exposure to 1953 amnesty	0.0335*	0.0327*	0.0256	0.0254	0.0306	0.0271
	(0.0195)	(0.0194)	(0.0184)	(0.0184)	(0.0202)	(0.0193)
R-squared	0.002	0.002	0.006	0.006	0.007	0.025
Observations	6,519	6,519	6,519	6,519	6,519	6,519
Panel B:	Dependent variable: Ihs # Alla Pugacheva searches in Yandex (mean 0.527 st.dev. 1.59)					
Exposure to 1953 amnesty	0.0222	0.0233	0.0197	0.0188	0.0274	0.0266
	(0.0238)	(0.0236)	(0.0230)	(0.0228)	(0.0249)	(0.0241)
R-squared	0.000	0.001	0.001	0.002	0.002	0.028
Observations	6,519	6,519	6,519	6,519	6,519	6,519
Latitude & longitude		✓	✓	✓	✓	✓
Min. distance to Gulag camp			✓	✓	✓	✓
Population of the closest Gulag camp				✓	✓	✓
Exposure to total Gulag population					✓	✓
Convict labor industry FEs of closest Gulag camp						✓

Notes: The unit of observation in this Table is *rayon*. The dependent variable in Panel A is a log number of searches of Chanson singers relative to the total number of searches on Yandex.ru in 2021. We use searches for Wili Tokarev, Yuri Leps, Lesopoval, Denis Maydanov, Katya Ogonek, Nikolay Rastorguev, Mikhail Schafutinsky, Zheka, Mikhail Krug, and Ivan Kuchin. The dependent variable in Panel B is a log number of searches for Rapper Egor Kreed relative to the total number of searches in Yandex.ru in 2021. The dependent variable in Panel C is a log number of searches for Rapper Slava Marlow relative to the total number of searches in Yandex.ru in 2021. The dependent variable in Panel D is a log number of searches for the most famous Russian pop singer Alla Pugacheva relative to the total number of searches on Yandex.ru in 2021. All columns have the same controls as in the baseline specification in Table B.13. Standard errors clustered at the province level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.17 − Amnesty of 1953 Has no Effect on Income in 2020

	I	II	III	IV	V	VI
Panel A:	Dependent variable: Log avarage monthly income in 2020 (mean 2.89 st.dev. 4.4)					
Exposure to 1953 amnesty	0.1662	0.1325	0.1019	0.1026	0.1176	0.0840
	(0.1707)	(0.1509)	(0.1451)	(0.1464)	(0.1505)	(0.1253)
R-squared	0.001	0.010	0.012	0.012	0.012	0.065
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel B:	Dependent variable: Log population in 2020 (mean 8.8 st.dev. 1.2)					
Exposure to 1953 amnesty	0.1511***	0.1460***	0.0964**	0.0963**	0.0208	0.0211
	(0.0521)	(0.0506)	(0.0371)	(0.0371)	(0.0398)	(0.0393)
R-squared	0.016	0.017	0.034	0.034	0.047	0.054
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Latitude & longitude		✓	✓	✓	✓	✓
Min. distance to Gulag camp			✓	✓	✓	✓
Population of the closest Gulag camp				✓	✓	✓
Exposure to total Gulag population					✓	✓
Convict labor industry FEs of closest Gulag camp						✓

Notes: This Table replicates Table 2 but uses different dependent variables — log average household income (in Panel A) and log population (in Panel B). Standard errors clustered at the province level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.18 − Amnesty of 1953 Has no Effect on Crime Rates in 2010–2015

	I	II	III	IV	V	VI
	Dependent variable: Log number of crimes per capita					
Exposure to 1953 amnesty	0.0701***	0.0217	-0.0051	-0.0022	-0.0025	-0.0093
	(0.0169)	(0.0194)	(0.0212)	(0.0214)	(0.0214)	(0.0274)
R-squared	0.030	0.312	0.352	0.355	0.356	0.488
Observations	200	200	200	200	200	200
Latitude & longitude		✓	✓	✓	✓	✓
Min. distance to Gulag camp			✓	✓	✓	✓
Population of the closest Gulag camp				✓	✓	✓
Exposure to total Gulag population					✓	✓
Convict labor industry FEs of closest Gulag camp						✓

Notes: This Table replicates Table 2 but uses the log number of crimes per capita as the dependent variable in 2010–2015 (years for which hate crimes are available). The unit of observation is a municipality (as in Panels A and B of Table 2) but it is a subsample of the 200 largest Russian cities. Standard errors clustered at the province level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1
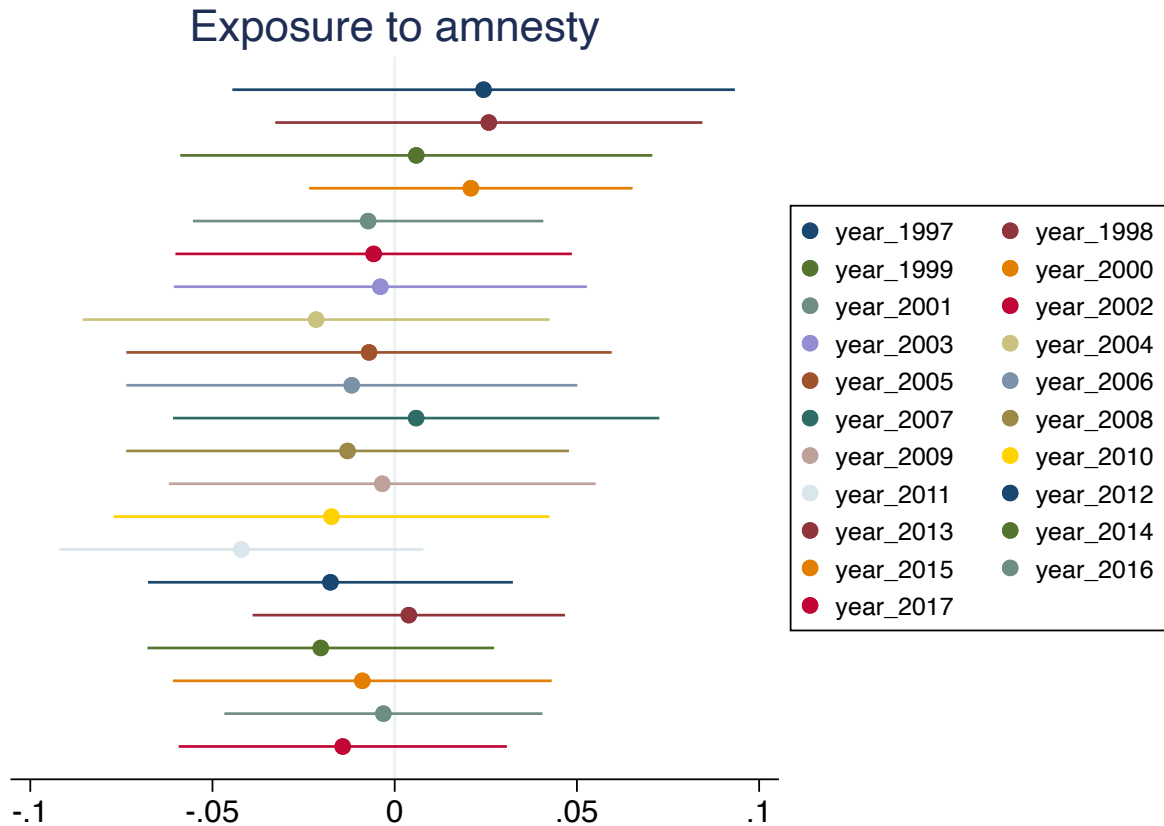
Figure B.8 − Amnesty of 1953 Has no Effect on Crime Rates in 1997–2017

Notes: Each coefficient in this Figure comes from a specification similar to one in Column VI of Table B.18 but uses the number of crimes for every available year.

Table B.19 − The Effect On Crimes Against LGBTQ+ Persons Is Driven By Crimes Against Gays But Not Lesbian or Trans Persons

	I	II	III	IV	V	VI
Panel A:	Dependent variable: Ihs # crimes against gays (mean 0.023 st.dev. 0.299)					
Exposure to 1953 amnesty	0.0311***	0.0307***	0.0303***	0.0303***	0.0267***	0.0272***
	(0.0100)	(0.0102)	(0.0102)	(0.0103)	(0.0101)	(0.0101)
R-squared	0.011	0.011	0.011	0.011	0.016	0.018
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel B:	Dependent variable: Ihs # crimes against lesbians (mean 0.002 st.dev. 0.033)					
Exposure to 1953 amnesty	0.0000	-0.0001	-0.0001	-0.0001	-0.0001	-0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
R-squared	0.000	0.000	0.000	0.000	0.000	0.004
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel C:	Dependent variable: Ihs # crimes against trans persons (mean 0.001 st.dev. 0.068)					
Exposure to 1953 amnesty	0.0000	-0.0000	-0.0001	-0.0001	-0.0000	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
R-squared	0.000	0.000	0.000	0.000	0.000	0.001
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Latitude & longitude		✓	✓	✓	✓	✓
Min. distance to Gulag camp			✓	✓	✓	✓
Population of the closest Gulag camp				✓	✓	✓
Exposure to total Gulag population					✓	✓
Convict labor industry FEs of closest Gulag camp						✓

Notes: This Table replicates Table 2 but uses different dependent variables. The dependent variable in Panel A is the inverse hyperbolic sine of the number of crimes against gays. The dependent variable in Panel B is the inverse hyperbolic sine of the number of crimes against lesbians. The dependent variable in Panel C is the inverse hyperbolic sine of the number of crimes against trans persons. Standard errors clustered at the province level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.20 – No Effect on Non-Homophobic Derogatory Language in Social Media

	I	II	III	IV	V	VI
Panel A:	Dependent variable: Ihs # female degotaive slur in VK (mean 0.004 st.dev. 0.054)					
Exposure to 1953 amnesty	-0.0005	-0.0006	-0.0006	-0.0006	-0.0009	-0.0010
	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0010)	(0.0010)
R-squared	0.153	0.154	0.154	0.154	0.155	0.157
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel B:	Dependent variable: Ihs # male degotaive slur in VK (mean 0.009 st.dev. 0.101)					
Exposure to 1953 amnesty	-0.0004	-0.0006	-0.0005	-0.0006	-0.0017	-0.0016
	(0.0014)	(0.0013)	(0.0013)	(0.0013)	(0.0011)	(0.0011)
R-squared	0.233	0.233	0.233	0.233	0.245	0.246
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel C:	Dependent variable: Ihs # swear words with root `*huj*' in VK (mean 0.169 st.dev. 1.027)					
Exposure to 1953 amnesty	0.0667	0.0690	0.0703	0.0705	0.0666	0.0677
	(0.0455)	(0.0460)	(0.0465)	(0.0464)	(0.0478)	(0.0480)
Survey-year FEs	✓	✓	✓	✓	✓	✓
R-squared	0.712	0.712	0.712	0.712	0.713	0.714
Observations	6,519	6,519	6,519	6,519	6,519	6,519
Latitude & longitude		✓	✓	✓	✓	✓
Min. distance to Gulag camp			✓	✓	✓	✓
Population of the closest Gulag camp				✓	✓	✓
Exposure to total Gulag population					✓	✓
Convict labor industry FEs of closest Gulag camp						✓

Notes: This Table replicates Table 2 but uses different dependent variables. The dependent variable in Panel A is the inverse hyperbolic sine of the number of derogatory slurs against women. The dependent variable in Panel B is the inverse hyperbolic sine of the number of derogatory slurs against men. The dependent variable in Panel C is the inverse hyperbolic sine of the number of general swear words with the root '*huj*.' Standard errors clustered at the province level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.21 − Amnesty of 1953 Has no Effect on Attitudes Toward Women

	I	II	III	IV	V	VI	VII	VIII	IX
					Dependent variable:				
	Women are as competent as men to be business executives	Men make better political leaders than women do	A woman should do most of the household chores even if the husband is not working	It is important that my daughter achieves university education	It is important that my son achieves university education	My opinions are taken into account in decisions made by the household	Cohabiting partners should be married	It is better for everyone involved if the man earns the money and the woman takes care of the home and children	Equal rights for women as citizens are important
Exposure to 1953 amnesty	0.0284	0.0855	0.2344	-0.7089	0.4548	-0.7208	-0.8183	0.5143	-0.2937
	(0.6938)	(0.8308)	(1.0871)	(0.8424)	(0.8037)	(0.6583)	(1.1307)	(1.1431)	(1.0305)
R-squared	0.145	0.090	0.119	0.128	0.151	0.097	0.086	0.076	0.116
Observations	1,445	1,417	1,449	1,284	1,278	1,413	1,391	1,410	1,458

Notes: This Table replicates Column VI of Panel C of Table 2 but uses different dependent variables. Here we only use LiTS (2016) data. Courier (2013, 2015) and LiTS (2010) don't have questions about attitudes toward women. WVS has one similar question (whether men are better executives) but the answers are not comparable. Hence, we use LiTS which has more questions and the largest number of observations. The results hold if we arbitrarily convert LiTS's and WVS's ordinal variables to a dummy and pool them. These results are available on request. Standard errors clustered at the province level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.22 − Amnesty of 1953 Has no Effect on Trust and Social Capital

	I	II	III	IV	V
	Dependent variable:				
	Most people can be trusted	Family can be trusted	People in the neighborhood can be trusted	Strangers can be trusted	Foreiners can be trusted
Exposure to 1953 amnesty	0.0830	0.0039	-0.0900	0.1171	0.0285
	(0.1959)	(0.0091)	(0.0550)	(0.0826)	(0.1128)
R-squared	0.104	0.023	0.059	0.059	0.121
Observations	6,525	6,525	6,525	6,525	6,525

Notes: This Table replicates Column VI of Panel C of Table 2 but uses different dependent variables. Here we only use LiTS data. Courier (2013, 2015) doesn't have questions about trust. WVS has similar questions but the answers are not comparable. Hence, we use LiTS which has more questions and the largest number of observations. The results hold if we arbitrarily convert LiTS's and WVS's ordinal variables to a dummy and pool them. These results are available on request. Standard errors clustered at the province level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.23 – Locations That Had More Coronations of Thieves-in-Law after 1953 Are More Homophobic Now

	I	II	III	IV	V	VI
Panel A:	Dependent variable: Ihs # crimes against LGBTQ+ (mean 0.024 st.dev. 0.307)					
Ihs # coronations of thieves-in-law	0.9572***	0.9566***	0.9562***	0.9563***	0.9501***	0.9505***
	(0.1309)	(0.1309)	(0.1308)	(0.1308)	(0.1299)	(0.1305)
R-squared	0.250	0.250	0.250	0.250	0.252	0.254
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel B:	Dependent variable: Ihs # homophobic slur in VK (mean 0.008 st.dev. 0.127)					
Ihs # coronations of thieves-in-law	0.3884***	0.3884***	0.3880***	0.3881***	0.3813***	0.3815***
	(0.0871)	(0.0871)	(0.0871)	(0.0871)	(0.0872)	(0.0874)
R-squared	0.238	0.238	0.239	0.239	0.256	0.258
Observations	9,829	9,829	9,829	9,829	9,829	9,829
Panel C:	Dependent variable: 1(Dislike homosexuals) (mean 0.616 st.dev. 0.486)					
Ihs # coronations of thieves-in-law	0.0122*	0.0122*	0.0122*	0.0130*	0.0132*	0.0132*
	(0.0073)	(0.0073)	(0.0073)	(0.0075)	(0.0075)	(0.0069)
Survey-year FEs	✓	✓	✓	✓	✓	✓
R-squared	0.002	0.003	0.003	0.009	0.010	0.064
Observations	0.002	0.003	0.003	0.009	0.010	0.064
Latitude & longitude		✓	✓	✓	✓	✓
Min. distance to Gulag camp			✓	✓	✓	✓
Population of the closest Gulag camp				✓	✓	✓
Exposure to total Gulag population					✓	✓
Convict labor industry FEs of closest Gulag camp						✓

Notes: This Table replicates Table 2 but uses a different explanatory variable — inverse hyperbolic sine of the number of coronations of thieves-in-law between 1953 and 2010. Standard errors clustered at the province level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.24 – Mediation Analysis

	I	II	III
	Dependent variable:		
	Ihs # crimes against LGBTQ+	Ihs # homophobic slur in VK	1(Dislike homosexuals)
X on Y	0.027	0.011	0.134
X on M	0.020	0.020	3.529
M on Y|X	0.944	0.379	0.019
Effect through M	0.702	0.719	0.502

Notes: This Table computes what share of the effect of amnesty on homophobia goes through the coronations of the thieves-in-law. The point-estimates for X on Y come from the Column VI of Table 2. The point-estimates for X on M come from using the specification from Column VI of Table 2 but using inverse hyperbolic sine of the number of the post-1953 coronations as the dependent variable. The point estimates for M on $Y|X$ come from the estimation of Equation 4 while controlling for the exposure to the amnesty of 1953. Here we assume linear effects and exogeneity of our treatment — exposure to the amnesty of 1953 conditional on the controls (specification in Column VI of Table 2).

Figure B.9 – Provinces More Exposed to the Amnesty of 1953 Have More Child Suicides

coef = 1.0042496, (robust) se = .36623107, t = 2.74

Notes: This Figure shows a residual plot from the province-level regression of log child suicides in 2019 on exposure to the amnesty of 1953 conditional on the log population. Suicide data for individuals aged 0-17 is by Russia's State Statistics Agency and is available at www.data-in.ru/data-catalog/datasets/164/.

A49

C   Additional Results for Australian Longitudinal Data

Table C.1 – Robustness for Table 4 Panel C: Alternative Samples

	I	II	III	IV	V	VI	VII
	Dependent variable: Homosexuals should have equal rights						
Panel A: ~Panel C w All respondents							
1(Respondent's close family	-0.023*	-0.022*	-0.025*	-0.025*	-0.033**	-0.028**	-0.029**
member was in prison)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Female FE	✓	✓	✓	✓	✓	✓	✓
R-squared	0.764	0.764	0.770	0.775	0.779	0.792	0.792
Observations	68,549	68,549	68,549	68,549	68,549	68,549	68,549
Panel B: ~Panel C w Sample of women							
1(Respondent's close family	-0.013	-0.013	-0.012	-0.013	-0.022	-0.043	-0.023
member was in prison)	(0.015)	(0.015)	(0.015)	(0.015)	(0.016)	(0.027)	(0.016)
R-squared	0.767	0.768	0.778	0.783	0.789	0.803	0.806
Observations	36,466	36,466	36,466	36,466	36,466	36,466	36,466
Respondent FEs	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓
State-Year FEs		✓	✓	✓	✓	✓	✓
Religion x age & YoB FEs			✓	✓	✓	✓	✓
Ethnicity x age & YoB FEs				✓	✓	✓	✓
Education x age & YoB FEs					✓	✓	✓
Occupation x age & YoB & year FEs						✓	✓
Ihs Income							✓

Notes: This Table replicates Panel C of Table 4 but uses different samples. Panel A estimates it on the sample of both, male and female respondents. Panel A additionally controls for the respondent's gender fixed effects. Panel B estimates it on the sample of female respondents. Standard errors clustered at the individual level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table C.2 – Robustness for Table 4: Alternative Exposure to Prison Culture (# of Times in Prison)

	I	II	III	IV	V	VI	VII
	Dependent variable: Homosexuals should have equal rights						
Panel A: Sample of men							
# times respondent was in prison	-0.158***	-0.158***	-0.196***	-0.196***	-0.193***	-0.175***	-0.175***
	(0.058)	(0.059)	(0.062)	(0.064)	(0.064)	(0.067)	(0.067)
R-squared	0.751	0.751	0.763	0.768	0.775	0.803	0.803
Observations	32,083	32,083	32,083	32,083	32,083	32,083	32,083
Panel B: Sample of women							
# times respondent was in prison	-0.095	-0.094	-0.081	-0.092	-0.109*	-0.066	-0.066
	(0.058)	(0.058)	(0.060)	(0.062)	(0.064)	(0.075)	(0.076)
R-squared	0.767	0.768	0.778	0.783	0.789	0.806	0.806
Observations	36,466	36,466	36,466	36,466	36,466	36,466	36,466
Panel C: Sample of men							
# times respondent's close family	-0.038	-0.037	-0.047*	-0.046*	-0.056**	-0.043*	-0.044*
member was in prison	(0.025)	(0.026)	(0.027)	(0.027)	(0.027)	(0.026)	(0.026)
R-squared	0.751	0.751	0.762	0.768	0.775	0.803	0.803
Observations	32,083	32,083	32,083	32,083	32,083	32,083	32,083
Respondent FEs	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓
State-Year FEs		✓	✓	✓	✓	✓	✓
Religion x age & YoB FEs			✓	✓	✓	✓	✓
Ethnicity x age & YoB FEs				✓	✓	✓	✓
Education x age & YoB FEs					✓	✓	✓
Occupation x age & YoB & year FEs						✓	✓
Ihs Income							✓

Notes: This Table replicates Table 4 but uses different explanatory variables. Panels A and B use the total number of times that the respondent went to prison by year t instead of a dummy. Panel C uses the total number of times that the respondent has a close family member returning to prison by year t instead of a dummy. Note, that if more than two family members return from prison in the same year we can't distinguish them and, thus, we may undercount it. Standard errors clustered at the individual level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table C.3 – Robustness for Table 4: Alternative Exposure to Prison Culture (# of Years in Prison)

	I	II	III	IV	V	VI	VII
	Dependent variable: Homosexuals should have equal rights						
Panel A: Sample of men							
# years respondent spent in prison	-0.158***	-0.158***	-0.196***	-0.196***	-0.193***	-0.175***	-0.175***
	(0.058)	(0.059)	(0.062)	(0.064)	(0.064)	(0.067)	(0.067)
R-squared	0.751	0.751	0.763	0.768	0.775	0.803	0.803
Observations	32,083	32,083	32,083	32,083	32,083	32,083	32,083
Panel B: Sample of women							
# years respondent spent in prison	-0.095	-0.094	-0.081	-0.092	-0.109*	-0.066	-0.066
	(0.058)	(0.058)	(0.060)	(0.062)	(0.064)	(0.075)	(0.076)
R-squared	0.767	0.768	0.778	0.783	0.789	0.806	0.806
Observations	36,466	36,466	36,466	36,466	36,466	36,466	36,466
Respondent FEs	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓
State-Year FEs		✓	✓	✓	✓	✓	✓
Religion x age & YoB FEs			✓	✓	✓	✓	✓
Ethnicity x age & YoB FEs				✓	✓	✓	✓
Education x age & YoB FEs					✓	✓	✓
Occupation x age & YoB & year FEs						✓	✓
Ihs Income							✓

Notes: This Table replicates Table 4 but uses different explanatory variables. Panels A and B use the total number of years that the respondent spent in prison by year t instead of a dummy. To compute the number of years that a person spent in prison we assume that if the person appears in the data in year t and then is not present in the survey for j years and re-appears in year $t + j + 1$ and says that she/he was in prison last year, we count that she/he spent $j + 1$ years in prison. Note, that we can't estimate Panel C from Table 4 here because we do not have longitudinal data on close family members who returned from prison to compute the number of years that they spent in prison. Standard errors clustered at the individual level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table C.4 – Robustness for Table 4: Specification with Population Weights

	I	II	III	IV	V	VI	VII
	Dependent variable: Homosexuals should have equal rights						
Panel A: Sample of men							
1(Respondent was in prison)	-0.285***	-0.287***	-0.363***	-0.361***	-0.375***	-0.391***	-0.392***
	(0.106)	(0.106)	(0.107)	(0.110)	(0.111)	(0.109)	(0.110)
R-squared	0.755	0.755	0.771	0.778	0.787	0.820	0.820
Observations	32,083	32,083	32,083	32,083	32,083	32,083	32,083
Panel B: Sample of women							
1(Respondent was in prison)	-0.188*	-0.183*	-0.140	-0.159	-0.151	-0.112	-0.113
	(0.107)	(0.106)	(0.108)	(0.111)	(0.115)	(0.135)	(0.135)
R-squared	0.773	0.774	0.788	0.794	0.802	0.822	0.822
Observations	36,466	36,466	36,466	36,466	36,466	36,466	36,466
Panel C: Sample of men							
1(Respondent's close family	-0.106*	-0.105*	-0.126**	-0.112*	-0.116*	-0.115**	-0.115**
member was in prison)	(0.060)	(0.060)	(0.060)	(0.059)	(0.060)	(0.056)	(0.056)
R-squared	0.754	0.755	0.771	0.777	0.787	0.819	0.820
Observations	32,083	32,083	32,083	32,083	32,083	32,083	32,083
Respondent FEs	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓
State-Year FEs		✓	✓	✓	✓	✓	✓
Religion x age & YoB FEs			✓	✓	✓	✓	✓
Ethnicity x age & YoB FEs				✓	✓	✓	✓
Education x age & YoB FEs					✓	✓	✓
Occupation x age & YoB & year FEs						✓	✓
Ihs Income							✓

Notes: This Table replicates Table 4 but uses HILDA's population weights. Standard errors clustered at the individual level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table C.5 – Robustness for Table 6: Alternative Methods of Estimating Differences-in-Differences Estimates

	I	II	III	IV	V
	Dependent variable: Homosexuals should have equal rights				
Method	OLS (baseline)	Borusyak et al. (2021)	de Chaisemartin and D'Haultfoeuille (2022)	Callaway and Sant'Anna (2021)	Sun and Abraham (2021)
Panel A: Sample of men					
1(Respondent was in prison)	-0.279***	-0.193***	-0.229***	-0.129**	-0.103*
	(0.086)	(0.081)	(0.067)	(0.066)	(0.060)
Panel B: Sample of women					
1(Respondent was in prison)	-0.129	0.097	-0.152	-0.039	-0.076
	(0.126)	(0.095)	(0.123)	(0.075)	(0.119)
Panel C: Sample of men					
1(Respondent's close family member was in prison)	-0.135**	-0.081*	-0.148*	-0.065	-0.068*
	(0.055)	(0.048)	(0.088)	(0.067)	(0.041)

Notes: This Table re-estimates the most conservative specification from Column VII of Table 6 but uses a different method of computing the average post-treatment differences-in-differences coefficient. Column I contains the baseline OLS estimate for reference. Column II uses the method proposed in Borusyak, Jaravel and Spiess (2021). Column III uses the method proposed in De Chaisemartin and D'Haultfoeuille (2022). Column IV uses method proposed in Callaway and Sant'Anna (2021). Column V uses the method proposed in Sun and Abraham (2021). Standard errors clustered at the individual level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table C.6 – Event-Study Coefficients for Figure 5

		I	II	III
		Dependent variable: Homosexuals should have equal rights		
	Sample	Men	Women	Men
	Event	R's incarceration	R's incarceration	R's close family member incarceration
>12 years before event		0.200	0.183	0.086
		(0.181)	(0.149)	(0.108)
8 years before event		0.165	0.056	-0.014
		(0.129)	(0.195)	(0.084)
1st year after event		-0.251**	-0.045	-0.139**
		(0.105)	(0.147)	(0.066)
4 years after event		-0.135	-0.136	-0.115*
		(0.120)	(0.175)	(0.069)
8 years after event		-0.049	-0.256	-0.177**
		(0.143)	(0.157)	(0.087)
>12 years after event		-0.317*	-0.305	-0.117
		(0.164)	(0.282)	(0.093)
Joint F-test for pre-trend coef., p-value		[0.5773]	[0.4713]	[0.7185]
R-squared		0.810	0.806	0.806
Observations		32,083	36,466	32,083

Notes: This Table estimates event-study specification 7. We use the same (most demanding) set of controls as in Column VII of Table 4. Column I corresponds to Column VII of Panel A, Column II — Panel B, and Column III — Panel C. The event is the first time when a respondent answered that he/she was in prison last year (or a close family member returned from prison last year). Thus all periods there are in relative terms. Because the question about attitudes toward homosexuals was asked every 4 years, periods also represent 4-year intervals. Standard errors clustered at the individual level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table C.7 – Robustness for Table 4: Results Hold on Subsample of Respondents that Entered Survey by the Age of 18

	I	II	III	IV	V	VI
	Dependent variable: Homosexuals should have equal rights					
Sample	Men ≤18y.o.	Men >18y.o.	Women ≤18y.o.	Women >18y.o.	Men ≤18y.o.	Men >18y.o.
1(Respondent was in prison)	-0.246**	-0.249**	-0.113	-0.146		
	(0.115)	(0.097)	(0.126)	(0.134)		
1(Respondent's close family member was in prison)					-0.224*	-0.100*
					(0.137)	(0.056)
R-squared	0.673	0.760	0.706	0.767	0.673	0.759
Observations	4,995	27,088	5,691	30,775	4,995	27,088

Notes: This Table replicates Column I of Table 4 but uses different samples. Columns I, III, and V only use respondents who entered the survey at the age of 18 or earlier. Columns II, IV, and VI only use respondents who entered the survey at the age of 19 or later. Standard errors clustered at the individual level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table C.8 – Robustness for Table 4: Specification with Interviewer Fixed Effects

Sample	I	II	III
	Dependent variable: Homosexuals should have equal rights		
	Men	Women	Men
1(Respondent was in prison)	-0.280***	-0.141	
	(0.087)	(0.127)	
1(Respondent's close family			-0.123**
member was in prison)			(0.056)
R-squared	0.809	0.811	0.809
Observations	32,083	36,466	32,083

Notes: This Table replicates Column VII of Panel A, B, and C of Table 4 but controls for the interviewer fixed effects. Standard errors clustered at the individual level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure C.1 – Results are Not Driven by a Particular Age-Bin of Respondents Entering HILDA Survey
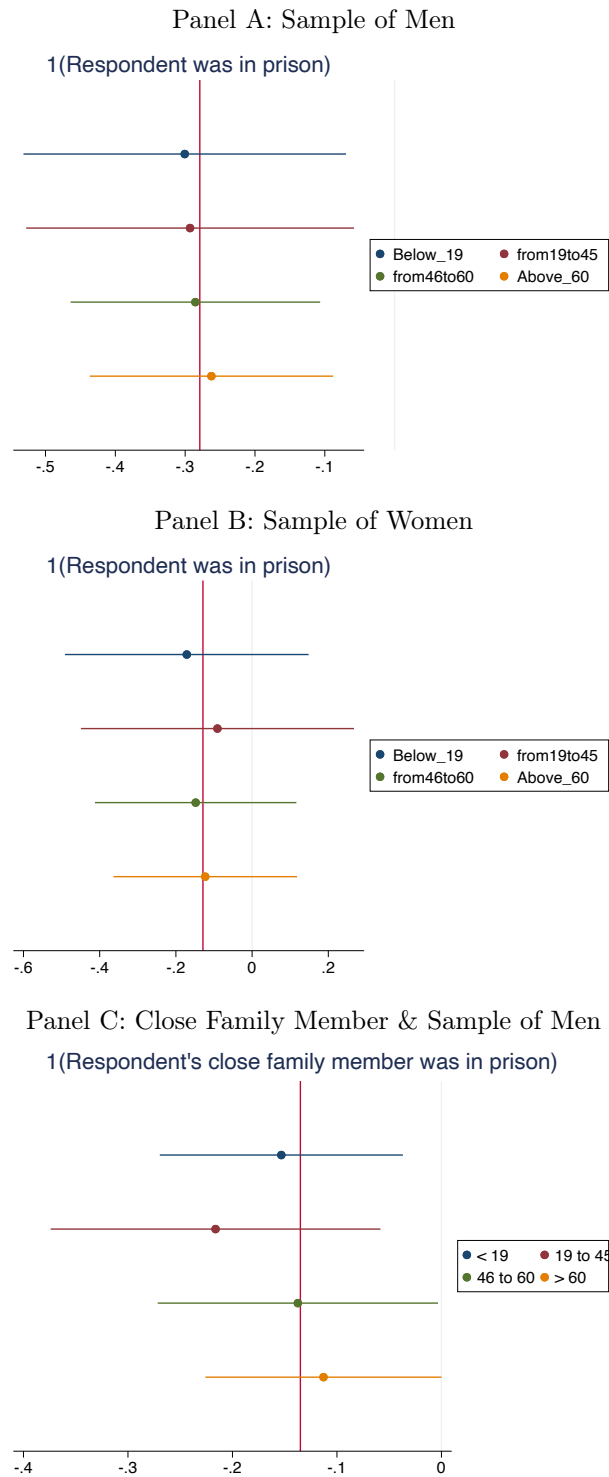
Panel A: Sample of Men

Panel B: Sample of Women

Panel C: Close Family Member & Sample of Men

Notes: This Figure reports on the point-estimate and 90th-percent confidence band that results when re-estimating the speci-
fication in Column VII of Table 4, dropping one age bin (age of the respondent entering the HILDA survey for the first time)
at a time. The (red) vertical line is the baseline point estimate. The results are sorted top-to-bottom, i.e., omit below 19 age
group, then 19–45, then 46–60, and above 60.

Figure C.2 – Results are Not Driven by a Particular State
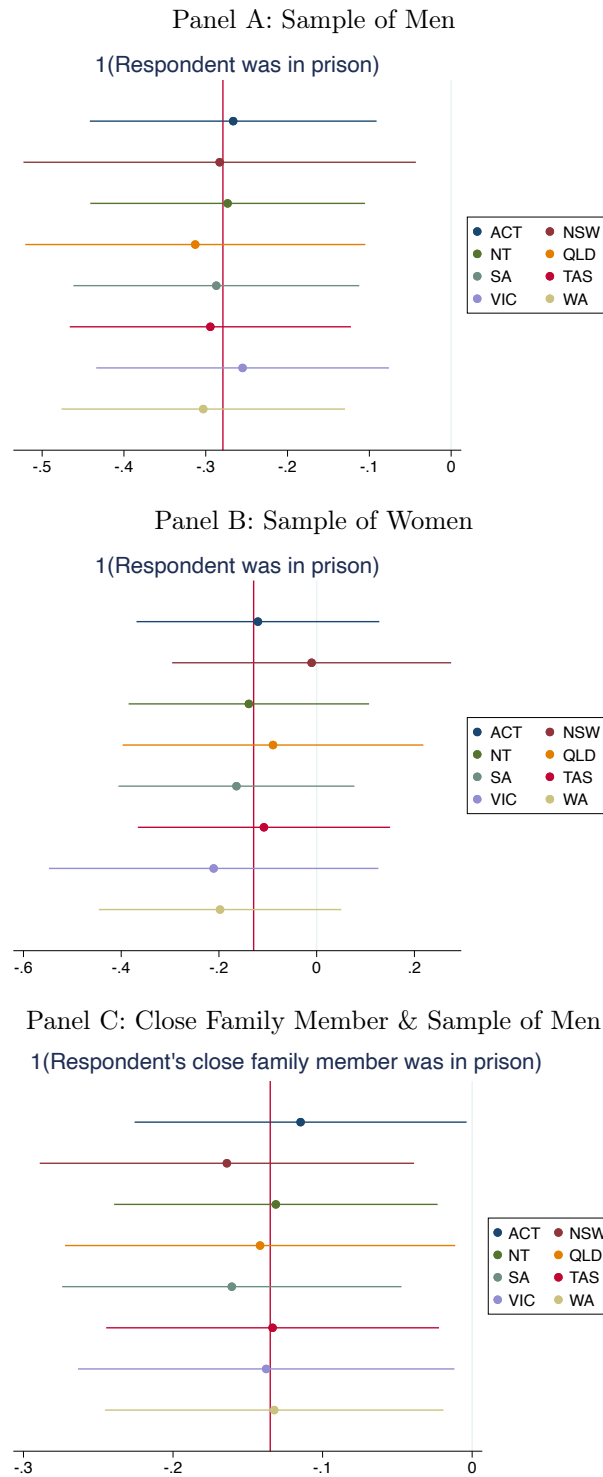
Panel A: Sample of Men

Panel B: Sample of Women

Panel C: Close Family Member & Sample of Men

Notes: This Figure reports on the point-estimate and 90th-percent confidence band that results when re-estimating the specification in Column VII of Table 4, dropping one state at a time. The (red) vertical line is the baseline point estimate. The results are sorted alphabetically, i.e., omit the Australian Capital Territory, then New South Wales, then Northern Territory, etc.

Figure C.3 − Results are Not Driven by a Particular Religious Group
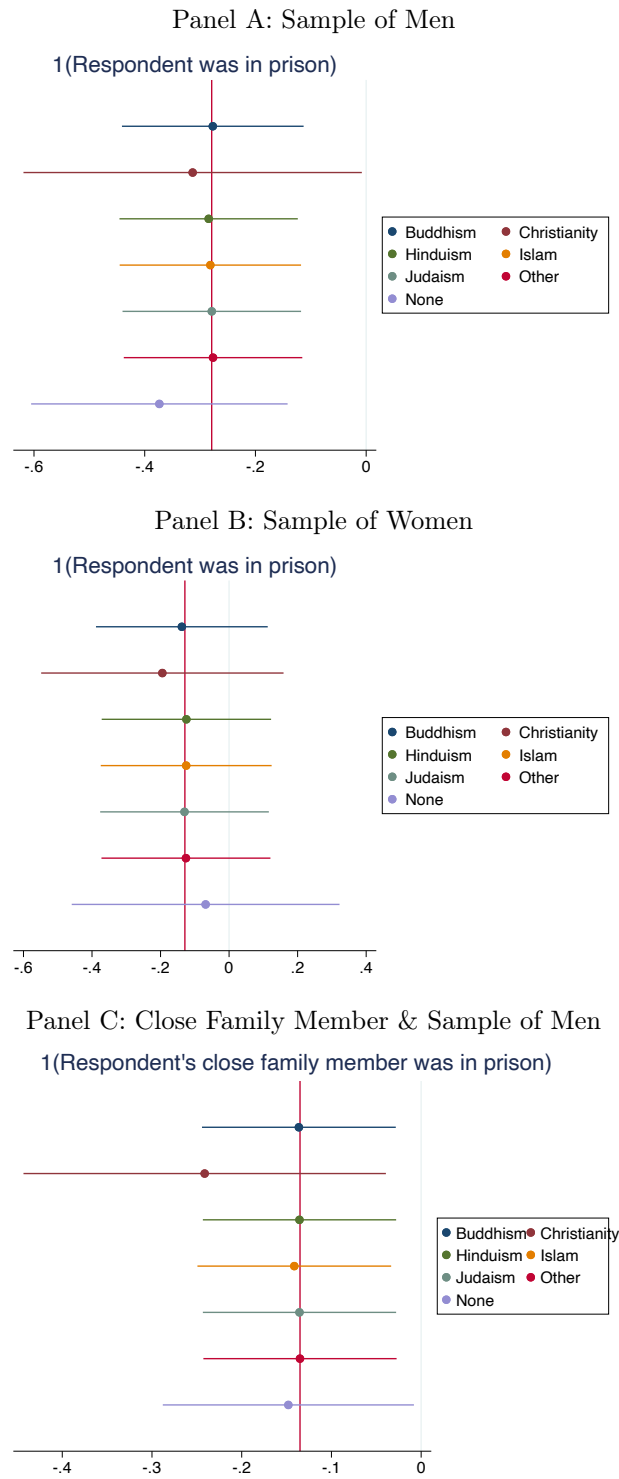
Panel A: Sample of Men

Panel B: Sample of Women

Panel C: Close Family Member & Sample of Men

Notes: This Figure reports on the point-estimate and 90th-percent confidence band that results when re-estimating the specification in Column VII of Table 4, dropping one religious group at a time. The (red) vertical line is the baseline point estimate.

Figure C.4 – Results are Not Driven by a Particular Educational Group

Panel A: Sample of Men

Panel B: Sample of Women

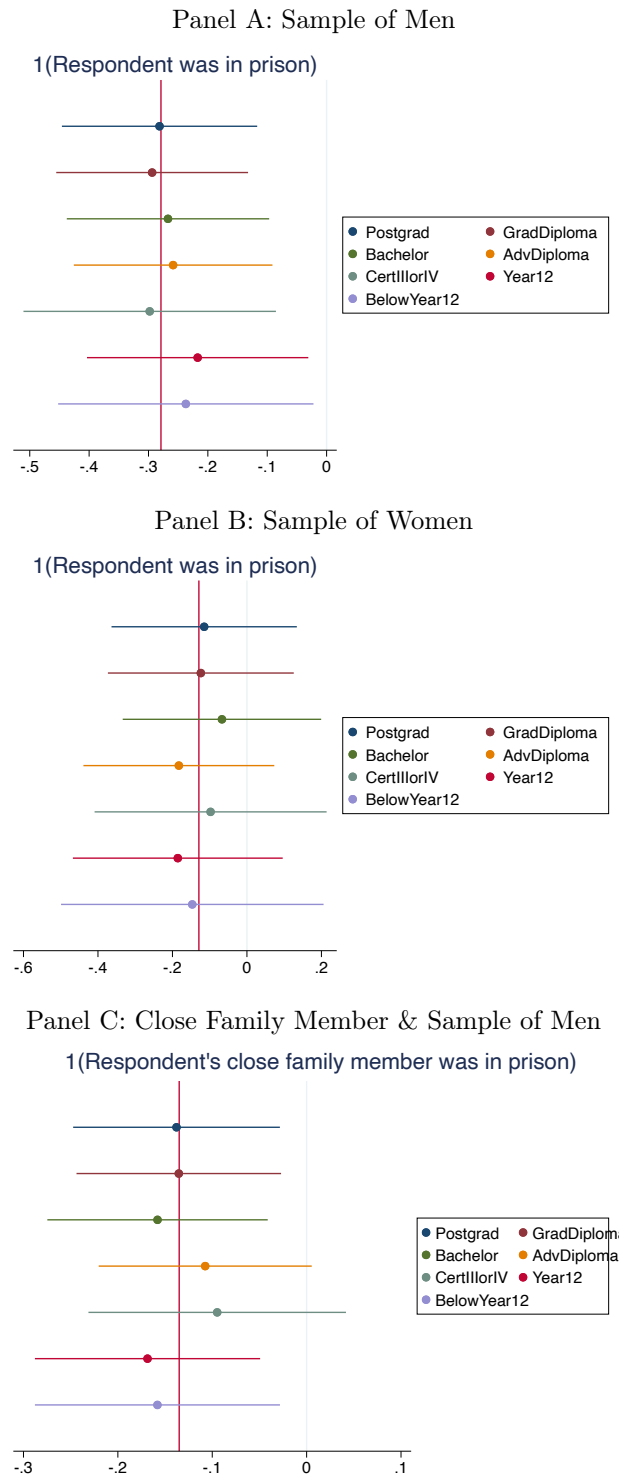Panel C: Close Family Member & Sample of Men

Notes: This Figure reports on the point-estimate and 90th-percent confidence band that results when re-estimating the specification in Column VII of Table 4, dropping one education group at a time. The (red) vertical line is the baseline point estimate.

Table C.9 − Effect of Prison Experience on Reduction in Tolerance Toward Homosexuals: Matching Estimation

	I	II	III	IV	V	VI
	Dependent variable: Homosexuals should have equal rights					
Sample	Men		Women		Men	
Matching	Nearest neighbor	Kernel	Nearest neighbor	Kernel	Nearest neighbor	Kernel
ATT: 1(Respondent was in prison)	-0.276	-0.238	-0.122	-0.129		
	(0.061)	-	(0.093)	-		
	[0.075]	[0.036]	[0.096]	[0.065]		
ATT: 1(Respondent's close family member was in prison)					-0.109	-0.115
					(0.036)	-
					[0.039]	[0.027]
# treated	541	541	227	227	1,598	1,598
# controls	541	29,478	227	33,678	1,708	30,438

Notes: All blocks are balanced. Standard errors computed using analytical standard errors are in parentheses. Bootstrapped standard errors are in brackets *** p<0.01, ** p<0.05, * p<0.1

Table C.10 – Homophobic Persons Are Not More Likely To Be Incarcerated

	I	II	III	IV	V	VI	VII
	Dependent variable: 1(Respondent was incarcerated)						
Panel A: Sample of men							
Homosexuals should have equal rights	-0.00042	-0.00042	-0.00024	-0.00027	-0.00025	-0.00033	-0.00033
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
R-squared	0.232	0.233	0.249	0.256	0.260	0.282	0.282
Observations	86,700	86,700	86,700	86,700	86,700	86,700	86,700
Panel B: Sample of women							
Homosexuals should have equal rights	0.00007	0.00007	0.00006	0.00007	0.00008	0.00006	0.00006
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
R-squared	0.194	0.195	0.212	0.224	0.229	0.244	0.244
Observations	99,915	99,915	99,915	99,915	99,915	99,915	99,915
Panel C: Sample of men	Dependent variable: 1(Respondent's close family member was incarcerated)						
Homosexuals should have equal rights	-0.00042	-0.00042	-0.00048	-0.00043	-0.00029	-0.00053	-0.00053
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female FE	✓	✓	✓	✓	✓	✓	✓
R-squared	0.274	0.275	0.284	0.291	0.299	0.329	0.329
Observations	86,700	86,700	86,700	86,700	86,700	86,700	86,700
Respondent FEs	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓
State-Year FEs		✓	✓	✓	✓	✓	✓
Religion x age & YoB FEs			✓	✓	✓	✓	✓
Ethnicity x age & YoB FEs				✓	✓	✓	✓
Education x age & YoB FEs					✓	✓	✓
Occupation x age & YoB & year FEs						✓	✓
Ihs Income							✓

Notes: Standard errors clustered at the individual level, are in parentheses. *** p$<$0.01, ** p$<$0.05, * p$<$0.1

Table C.11 – Prison Experience Does Not Affect Attitudes Toward Women

Sample	I	II	III	IV	V	VI
			Dependent variable:			
	Most important role in life -- being a mother		Man earns the money and the woman takes care of the home and children		It is not good for a relationship if the woman earns more than the man	
	Men	Women	Men	Women	Men	Women
1(Respondent was in prison)	-0.071	-0.005	-0.005	0.058	-0.127	0.096
	(0.093)	(0.095)	(0.095)	(0.127)	(0.117)	(0.171)
R-squared	0.641	0.725	0.725	0.734	0.624	0.632
Observations	27,772	27,780	27,780	32,081	27,762	32,048

Notes: This Table replicates Column VII of Table 4 but uses different outcome variables. Columns I, III, and V estimate regression in the sample of men. Columns II, IV, and VI estimate regression in the sample of women. All three dependent variables are ordinal variables varying from 1 to 7 that we normalize to have zero mean and standard deviation of one for the sake of interpretation. Standard errors clustered at the individual level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table C.12 – Prison Experience Does Not Affect Social Capital and Trust

Sample	I	II	III	IV	V	VI
			Dependent variable:			
	Ihs (# hours volunteering/charity work)		People in the neighborhood can be trusted		Most people can be trusted	
	Men	Women	Men	Women	Men	Women
1(Respondent was in prison)	-0.013	0.033	0.153	-0.018	0.051	-0.045
	(0.030)	(0.045)	(0.121)	(0.183)	(0.119)	(0.171)
R-squared	0.571	0.536	0.728	0.700	0.744	0.718
Observations	110,720	125,829	20,976	24,356	21,043	24,453

Notes: This Table replicates Column VII of Table 4 but uses different outcome variables. Columns I, III, and V estimate regression in the sample of men. Columns II, IV, and VI estimate regression in the sample of women. All three dependent variables are ordinal variables varying from 1 to 7 that we normalize to have zero mean and standard deviation of one for the sake of interpretation. Standard errors clustered at the individual level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table C.13 – Mental Health Does Not Correlate with the Attitudes toward Gay Persons

	I	II	III	IV	V	VI
	Dependent variable: Homosexuals should have equal rights					
Measure of X	Been a nervous person	Nothing could cheer you up	Did not feel calm and peaceful	Felt down	Depressed	Kessler psychologic al distress score
Panel A: Sample of men						
Mental health/Psychological distress measure	0.002	-0.004	-0.008	-0.004	0.011	-0.001
	(0.007)	(0.007)	(0.006)	(0.007)	(0.011)	(0.002)
R-squared	0.804	0.805	0.805	0.805	0.854	0.854
Observations	27,265	27,265	27,265	27,265	17,015	17,015
Panel B: Sample of women						
Mental health/Psychological distress measure	-0.006	0.000	-0.003	0.006	-0.003	-0.003**
	(0.006)	(0.006)	(0.005)	(0.005)	(0.009)	(0.001)
R-squared	0.807	0.807	0.807	0.807	0.850	0.850
Observations	31,577	31,577	31,577	31,577	19,915	19,915

Notes: This Table replicates Column VII of Table 4 but uses different explanatory variables. Panel A estimates regressions on the sample of men. Panel B estimates regressions on the sample of women. Explanatory variables in Columns I–V are ordinal variables varying from 0 to 5 that we normalize to have zero mean and standard deviation of one for the sake of interpretation. The explanatory variable in Column VI is ordinal variables varying from 0 to 40. Standard errors clustered at the individual level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table C.14 – Robustness for Table 4: Controlling for Respondents' Mental Health

	I	II	III	IV	V	VI	VII
	Dependent variable: Homosexuals should have equal rights						
Panel A: Sample of men							
1(Respondent was in prison)	-0.282***	-0.279***	-0.264***	-0.292***	-0.420***	-0.409**	-0.419***
	(0.085)	(0.085)	(0.085)	(0.085)	(0.160)	(0.160)	(0.161)
R-squared	0.805	0.805	0.805	0.805	0.854	0.854	0.857
Observations	27,244	27,244	27,244	27,244	17,003	17,003	17,003
Panel B: Sample of women							
1(Respondent was in prison)	-0.116	-0.126	-0.080	-0.177	-0.155	-0.070	-0.031
	(0.125)	(0.126)	(0.113)	(0.128)	(0.160)	(0.140)	(0.147)
R-squared	0.807	0.807	0.807	0.807	0.850	0.850	0.851
Observations	31,541	31,541	31,541	31,541	19,896	19,896	19,896
Panel C: Sample of men							
1(Respondent's close family member was in prison)	-0.142***	-0.134**	-0.132**	-0.128**	-0.169*	-0.185**	-0.163*
	(0.055)	(0.055)	(0.055)	(0.054)	(0.087)	(0.088)	(0.088)
R-squared	0.804	0.805	0.805	0.805	0.854	0.854	0.857
Observations	27,244	27,244	27,244	27,244	17,003	17,003	17,003
Baseline controls	✓	✓	✓	✓	✓	✓	✓
Been a nervous person	✓						✓
Felt so down in the dumps nothing could cheer you up		✓					✓
Did not feel calm and peaceful			✓				✓
Felt down				✓			✓
Psychological distress, depressed					✓		✓
Kessler psychological distress score						✓	✓

Notes: This Table replicates Column VII of Table 4 but adds additional control variables. Note, that the number of observations is smaller than in Table 4 because of the missing values in variables describing mental health and because variables "Depressed" and "Kessler psychological distress score" were not asked every year. Standard errors clustered at the individual level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table C.15 – Men are Not More Likely to Become Gay in Prison

	I	II	III	IV	V	VI	VII
	Dependent variable: 1(Respondent self-identify as a gay)						
Panel A: Sample of men							
1(Respondent was in prison)	-0.008	-0.005	-0.000	0.014	0.007	-0.029	-0.029
	(0.046)	(0.046)	(0.047)	(0.043)	(0.042)	(0.038)	(0.038)
R-squared	0.790	0.791	0.804	0.806	0.826	0.875	0.875
Observations	11,554	11,554	11,554	11,554	11,554	11,554	11,554
Panel B: Sample of women							
1(Respondent was in prison)	-0.009	-0.009	0.039	0.044	0.069	0.111	0.111
	(0.071)	(0.070)	(0.057)	(0.057)	(0.060)	(0.083)	(0.082)
R-squared	0.784	0.785	0.806	0.805	0.822	0.856	0.856
Observations	13,576	13,576	13,576	13,576	13,576	13,576	13,576
Panel C: Sample of men							
1(Respondent's close family	-0.009	-0.013	-0.010	-0.008	-0.014	-0.038	-0.038
member was in prison)	(0.030)	(0.030)	(0.031)	(0.032)	(0.032)	(0.040)	(0.040)
R-squared	0.790	0.791	0.804	0.806	0.826	0.875	0.875
Observations	11,554	11,554	11,554	11,554	11,554	11,554	11,554
Respondent FEs	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓	✓
State-Year FEs		✓	✓	✓	✓	✓	✓
Religion x age & YoB FEs			✓	✓	✓	✓	✓
Ethnicity x age & YoB FEs				✓	✓	✓	✓
Education x age & YoB FEs					✓	✓	✓
Occupation x age & YoB & year FEs						✓	✓
Ihs Income							✓

Notes: This Table replicates Table 4 but uses a different dependent variable — dummy, for a respondent to self-identify with the LGBTQ+ community. Standard errors clustered at the individual level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1