Beneath the Ban of Abortion: Evidence from the USSR*

Sultan Mehmood
The New Economic School

Yaroslav Prokhorskoy
ESSEC Business School

Hosny Zoabi
The New Economic School

September 9, 2025

Abstract

We exploit Stalin's abrupt 1936 nationwide abortion ban as a sharp policy discontinuity to study the consequences of coercive reproductive restrictions. Using newly digitized archival data, we find that the ban not only increased birth rates but also increased infant and maternal mortality and increased illegal abortions, providing direct evidence of substitution toward unsafe procedures. We also find a rise in adolescent property crimes among cohorts born after the ban. These patterns cannot be explained by concurrent Stalinist policies. The findings reveal that sweeping limits on reproductive autonomy can reshape demographic patterns and trigger lasting social consequences.

Keywords: Abortion Ban, Demography, Public Health, Illegal Abortions, Crime.
JEL: I18, O13; J13; J16; K36; D72.

*We thank Siwan Anderson, Steven Durlauf, Oded Galor, Ben Marx, Felipe Valencia, Andrei Markevich, Eoin McGuirk, Stelios Michalopoulos, Felipe Valencia-Caicedo, Austin Wright and Katia Zhuravskaya for their comments and feedback. Rida Mian, Victoria Gorbunova and Daniil Gorbunov provided excellent research assistance. Address: The New Economic School, 121353, 45 Skolkovskoe shosse, The Urals Business Center, Moscow, Russian Federation. Mehmood: smehmood@nes.ru. Prokhorskoy: yprokhorskoy@nes.ru Zoabi: hzoabi@nes.ru.

"Demand the unconditional annulment of all laws against abortions..."

— Vladimir Lenin (Lenin, 1913).

1 Introduction

The right of women to obtain an abortion remains one of the most intensely debated and divisive issues of our time. One in four women in the world will experience an abortion during their lifetime (Bearak et al., 2022). Despite the United Nations affirmation of abortion as a human right (Office of the United Nations High Commissioner for Human Rights, 2020), restrictive abortion laws continue to impinge upon the reproductive freedoms of women, affecting 753 million—or 40%—of all reproductive-age women (Center for Reproductive Rights, 2024). The stark contrast between the formal recognition of abortion rights and actual global accessibility calls for careful empirical scrutiny of the societal effects, both intended and unintended, of abortion restrictions, which are of crucial policy and scholarly importance (Guttmacher Institute, 2022). Do abortion bans successfully achieve their intended demographic objectives, such as raising birth rates? How do these restrictions influence maternal and child well-being? And do such bans push abortion activity underground, resulting in unsafe, illegal abortions and fostering broader criminal behavior?

To answer these questions, this paper leverages a constellation of novel datasets and provides systematic empirical evidence, revealing that an abortion ban can have significant first-order effects on society. Focusing on the Soviet Union's 1936 abortion ban, which marked a dramatic policy reversal from a period of relatively accessible reproductive health services, we examine how the ban altered the broader societal landscape. Before the 1936 ban, the Soviet Union permitted abortions in all public hospitals, enabling women to exercise a certain degree of reproductive control, with access to abortions provided for free through the public healthcare system. The introduction of a strict prohibition on abortion by Joseph Stalin marked a dramatic policy shift.[1] The ban led to the blanket criminalization of all abortion services across the country, with the government

[1] Earlier in 1920, the Soviet Union, led by Vladimir Lenin, made history as the first nation in the modern era to legalize abortion and provide it free under its public healthcare system. Vladimir Lenin described the right to an abortion as an "elementary democratic right of citizens, men and women" and called the abortion restriction by the Czarist regime "nothing but the hypocrisy of the ruling classes".

mandating penalties for those implicated. Individuals, whether obtaining or aiding in an abortion, faced up to three years in prison. This included those involved in procuring or facilitating the termination of a pregnancy, encompassing doctors, spouses, and traditional birth attendants or midwives. The Soviet Union's sudden shift from legal abortion to a nationwide ban offers a valuable natural experiment to explore the effects of restricting abortion services, a policy in place and currently under debate in many parts of the world. This context offers an opportunity to investigate the direct demographic effects on fertility and mortality, alongside the indirect consequences, such as the impact on illegal abortions and crime.

Recent research has advanced our understanding of the consequences of abortion policy. Myers (2017, 2022) shows that the liberalization of abortion access in the United States led to lower fertility rates and a decline in pregnancy-induced marriages among young women and having positive consequences for their children (Farin, 2025), while Londoño-Vélez and Saravia (2025) documents how judicial discretion in Medellín shapes access to abortion, with denials by male judges associated with poorer maternal health and educational outcomes for children. Related work highlights the post-Roe context in the United States, where restrictions on abortion access have been met with shifts toward sterilization and contraceptive use (Gallen and Lu, 2025). While these studies illuminate both long-run consequences for family formation and human capital as well as short-run behavioral adjustments in fertility control, they remain necessarily limited to relatively short horizons. The historical setting we study allows us to follow consequences over many years, offering rare evidence on the durable demographic and social effects of abortion restrictions. Our analysis shifts attention to a markedly different historical and institutional setting: the Soviet Union's abrupt criminalization of abortion in 1936. We examine how this restrictive nationwide policy, implemented in a context with limited substitutes, led to substitution toward illegal abortion procedures and generated broader societal spillovers, including increased birth rates and infant mortality, a rise in informal abortions, and surges in property crimes such as vandalism and 'hooliganism'. Our study highlights how restrictive reproductive policy can function as a broader mechanism of demographic and social disruption.

Specifically, we document four key findings. First, the abortion ban increased birth rates, with more exposed regions, on average, experiencing higher births per woman following the ban. Second, we find a corresponding rise in female and infant mortality, suggesting an increase in unsafe abortion practices. Third, leveraging newly digitized Soviet-era crime data, we find evidence of a rise in illegal abortions, which is consistent with greater reliance on underground procedures following legal restrictions on abortion.

Fourth, we observe a rise in adolescent property offenses for cohorts exposed to the ban, with no corresponding increase in more serious crimes such as murder or armed robbery. This pattern may reflect shifts in household environments that increased the likelihood of low-level delinquency, potentially through reduced parental supervision (Hjalmarsson et al., 2021a; Donohue and Levitt, 2001).

Central to our analysis is an extensive data collection effort that includes photographing, digitizing, and consolidating historical records on legal and illegal abortions, alongside nationwide reports on demography, mortality, and crime from the Soviet Ministry of Population, Health, State Prosecution, and Statistics, respectively. Our data collection began with the conversion of pre-ban abortion figures from Soviet archival sources to digital format, which entered the public domain following the Soviet Union's disintegration in the 1990s. This dataset offers nationwide coverage and regional variation in abortions across all of present-day Russia (formerly known as the Russian Soviet Federative Socialist Republic, RSFSR). We use the pre-ban abortion rates, as a source of geographical variation in the intensity of abortions occurring in the Union before the prohibition. This data is sourced from the State Statistics Committee of the Soviet Union (*Goskomstat*). To estimate the direct effects of the abortion ban, we link region-level pre-ban abortion data with birth rates, adult mortality, and infant mortality data. These data are sourced from health and population statistics maintained by the Ministry of Population and Health in the Soviet Union. We further link this to Russian State Archival data on the age and cause of death of the Soviet population over time, allowing us to estimate the immediate demographic and health impacts of the abortion ban on cohorts most affected by the ban. To assess the indirect effects of the abortion ban, we gather a distinct measure for illegal abortions from previously confidential records of *Crimes in the Soviet Union*, maintained by the Prosecutor General's Office (*Archival Records Collection A461-80*) and declassified by President Yeltsin. Using these criminal records, we digitize detailed data on illegal abortions as well as other crimes, including vandalism and politically classified offenses such as "counterrevolutionary actions" or "refusal of government assignments," which were criminal offenses in the Soviet Union. These political charges are uncorrelated with illegal abortions. The absence of such an association supports the interpretation that abortion and vandalism reflect social disruptions rather than political protest or persecution.

The identification challenge in comparing regions more affected by restricted abortion access to those less affected is that these regions differ along many dimensions beyond their access to abortions. In particular, variation in pre-ban abortion rates may reflect underlying regional characteristics—such as pre-existing income levels, healthcare access, and cultural attitudes—that also influence demographic, health, or crime

outcomes. A key advantage of our setting is the ability to exploit quasi-random variation in pre-ban abortion rates across the Soviet Union, before and after the ban, using a difference-in-differences design. This framework allows us to trace the evolution of key outcomes—fertility, mortality, illegal abortions, and crime—in high- versus low-abortion regions before and after the reform.

Under the parallel trends assumption, the difference-in-differences design offers a credible strategy for identifying the causal effects of the abortion ban on demographic and societal outcomes. We provide several pieces of evidence to support this assumption. First, consistent with the abrupt introduction of the abortion ban by Stalin, we find no evidence of differential pre-trends between high- and low-abortion districts, mitigating concerns that post-ban differences reflect pre-existing demographic patterns rather than the policy's causal effect. Second, we apply the recently proposed Rambachan and Roth (2023) "more credible approach to parallel trends," which relaxes the assumption of strict parallel trends by allowing bounded deviations in post-ban outcomes. This method partially identifies causal effects and enables inference that remains valid even under limited violations of parallel trends, bolstering the internal validity of the empirical design. Third, we implement the recent framework by Callaway et al. (2024), which extends difference-in-differences to settings with continuous treatment intensity. Our findings remain robust under this approach, further reinforcing the credibility of our design. Lastly, we assess the robustness of our baseline difference-in-differences design using a more demanding triple-difference specification. This approach compares women of reproductive age—those most directly affected by the abortion ban—to women of non-reproductive age within the same region and year, before and after the policy change. By leveraging variation in exposure across age, region, and time, the design nets out region-by-year-specific shocks and strengthens identification. The inclusion of region-by-year fixed effects addresses concerns about unobserved confounders and mitigates potential general equilibrium effects (Hjalmarsson et al., 2021b), since both groups are exposed to the same macro-environment. Results remain consistent with our main specification.

Our findings indicate significant societal repercussions of the abortion ban. While the prohibition appears to have succeeded in achieving its demographic goal of increasing birth rates, it also resulted in a rise in female and infant mortality, likely due to greater reliance on unsafe abortion practices. This pattern is corroborated by a rise in illegal abortions. Moreover, the ban coincided with an uptick in minor public-order offenses such as vandalism and hooliganism, whereas other more violent crimes remained unaffected. The divergence in these trends supports the hypothesis that abortion restrictions contribute to higher incidence of social instability, particularly through mechanisms

related to disrupted household structures or 'fatherless' environments (Donohue and Levitt, 2001). Notably, vandalism among cohorts born before the abortion ban came into effect remains unaffected, suggesting that the observed changes are linked to the abortion restriction rather than to broader trends.

In terms of magnitude, regions with 10 additional pre-ban abortions per 1,000 women is associated with 5 additional births per 1,000 women after the ban. However, this fertility rise came at a cost: the same regions experienced about 1 additional infant death per 1,000 women, suggesting that nearly one out of every five additional births failed to survive infancy.[2] The rise in detected illegal abortions reinforces this interpretation: regions with 10 more pre-ban abortions per 1,000 women experienced an estimated 2.6 additional illegal abortions per one million women after the ban. This likely understates the true incidence, as reported cases reflect only a fraction of actual occurrences.[3] Finally, we observe that the same regions experienced approximately 27 additional cases of vandalism per million population, relative to pre-ban cohorts. Together, these results highlight the multifaceted societal repercussions of the abortion ban, extending beyond demographic and health outcomes to include increases in reported illegal abortions and certain categories of criminal activity documented in official records.[4]

To shed light on the potential pathways through which the abortion ban may have affected female and infant mortality, we examine administrative cause-of-death records from the Soviet Ministry of Health. If access to safe abortion declined after the ban, leading to greater reliance on informal or unsafe procedures, one would expect corresponding increases in pregnancy-related mortality. Our analysis reveals patterns consistent with this mechanism. Regions with 10 additional pre-ban abortions per 1,000 women experienced increases of 30 deaths from attempted abortions, 4 from miscarriage or premature birth, and 31 from broader pregnancy-related causes per 1,000 women. These figures correspond to relative increases of 85%, 25%, and 40%, respectively, compared to pre-

[2]These patterns may also reflect behavioral adjustments, such as intentional postponement of conception or changes in fertility preferences following the abortion ban or spillover effects (Angrist and Evans, 1999). The lack of access to reliable contraceptive methods in the 1930s Soviet Union also likely constrained women's ability to prevent pregnancies through other means, as suggested and analyzed in David (1974).

[3]To the best of our knowledge, this study provides one of the first pieces of systematic empirical evidence suggesting that banning legal abortion may contribute to increased reliance on underground abortion services. While prior work has hypothesized such substitution effects, empirical validation has remained elusive, in part because administrative crime records in democratic settings do not systematically track illegal abortions—likely a deliberate policy choice to avoid criminalizing vulnerable individuals in highly sensitive health contexts (for a detailed discussion, see Aghaei et al. (2017)).

[4]Because the abortion ban coincided with expanded maternal support, adverse outcomes, such as infant mortality, illegal abortions, and vandalism, likely represent lower bounds. We, nevertheless, address potential confounding by controlling for pre-ban variation in gynecological visits, midwife presence, and hospital availability.

ban averages. These associations point to heightened maternal health risks in areas with greater reliance on abortion services prior to the policy change. Similarly, infant mortality due to congenital weakness and premature delivery rose, whereas mortality from infectious diseases unrelated to reproductive health—such as influenza or typhus—remained stable. Placebo tests using mortality from other unrelated causes, such as tuberculosis, show no corresponding changes, further supporting the interpretation that these effects are specific to reproductive health.

To assess the robustness of our results, we perform several important identification checks, we outline three major ones here. First, we address potential confounding by other Stalin-era policies, such as political repressions (Zhukov and Talibova, 2018; Toews and Vézina, 2025) and the expansion of the radio network during this time (Lerner, 1964; Berkhoff, 2012). An alternative interpretation is that the observed increases in birth rates or vandalism could be driven by behavioral responses to political repression or radio propaganda, whose spread coincided with the abortion ban. Our large-scale data digitization allows us to account for these time-varying local variations in repression and radio penetration. We draw on detailed archival data on political repression from Memorial Archive, a leading source on Soviet-era state violence, and combine it with historical measures of radio coverage and signal strength constructed from Radio Journals (1929-1940). Adding these district and time-varying controls does not materially affect the estimates. Second, we acknowledge that our baseline difference-in-differences approach relies on the assumption of parallel trends. To support this, we provide evidence by demonstrating that the pre-trends are flat. Third, we acknowledge that regions with high versus low pre-ban abortion rates may differ along several dimensions. For instance, we document that higher-abortion regions tended to be wealthier prior to the ban. While our difference-in-differences strategy leverages within-region comparisons over time, and our triple-difference design further nets out region-by-year-specific shocks, we address concerns about baseline differences between high versus low abortion regions. Specifically, we digitize a comprehensive set of pre-treatment regional characteristics—including income levels, the number of hospitals, pediatric consultations, female hospital visits, and the number of paramedics, the latter serving as a proxy for baseline access to health infrastructure. Interacting these pre-treatment covariates with year fixed effects leaves the results unchanged, suggesting the design is not sensitive to baseline differences.[5]

[5]Specifically, this allows us to account for the possibility that regions with different levels of healthcare access prior to the abortion ban followed systematically different outcome trajectories, we interact baseline measures—such as hospital density, midwife availability, income, wages and female doctor visits—with year fixed effects. This approach helps control for the possibility that regions with stronger or weaker baseline infrastructure were on distinct paths irrespective of the abortion ban.

1.1 Related Literature

Our paper connects to several strands of the literature. First, our paper intersects with and expands upon the literature on demography and gender economics, especially research examining the effects of large-scale policy shifts. Specifically, we build upon the burgeoning literature on factors that expand or limit women's rights, as reviewed by Tertilt et al. (2022), which has been instrumental in augmenting women's bargaining power and catalyzing economic growth (Doepke and Tertilt, 2009; Hazan et al., 2019). The advent of the contraceptive pill is acknowledged as a transformative element in the progression of women's rights, conferring upon women unprecedented command over their reproductive choices. As highlighted by influential works of Goldin and Katz (2002), Goldin (2024), Bailey et al. (2012), Bailey (2010) and Bailey (2020), 'the pill' is believed to be revolutionary in empowering women's education, career and wages.[6] Our work extends the influential discourse on the liberation of women's reproductive freedoms through 'the pill' by presenting empirical evidence that, in contrast to the emancipating effects of contraceptive access, an abortion ban can restrict women's reproductive autonomy. This restriction not only harms women's and children's health but may also push women toward dangerous and illegal procedures to terminate a pregnancy. Our findings suggest that restrictive policies on women's reproductive choices result in an increase in birth rates while simultaneously compromising women's health and causing community-wide disruptions. Specifically, more women and children die, and certain crimes, such as illegal abortions and vandalism, increase. These societal changes post-abortion ban adds another significant cost to the development losses predicted by the demographic transition literature (Galor and Weil, 2000; Galor and Moav, 2002; Ashraf and Galor, 2011; Blanc and Wacziarg, 2020; Hazan et al., 2022; Blanc, 2023, 2024).[7]

Second, our paper contributes to an influential line of research exploring the relationship between reproductive rights and societal outcomes, particularly crime (Donohue and Levitt, 2001; Pop-Eleches, 2006; Donohue and Levitt, 2020). While Donohue and Levitt (2001) hypothesize that the expansion of abortion rights is likely to reduce fatherless crimes, their analysis does not provide direct evidence for this mechanism and has

[6] There, nevertheless, exist dissenting works that suggest a more limited impact of the pill. For example, see Myers (2017).

[7] In a similar vein, (Nunn and Wantchekon, 2011; Michalopoulos and Papaioannou, 2013; Alesina et al., 2013) study the influence of historical institutions on current economic and social structures provides a broader context for understanding the long-term effects of such policy changes.

been subject to intense debate.[8] Hjalmarsson et al. (2021b) makes significant progress by examining the effects of abortion access on crime and risky behavior in Romania, thereby shedding light on this important and controversial question in a new setting rather than re-evaluating the same data and historical context. They find that abortion reform in Romania affected crime levels; however, the effects attenuate once cohort size is accounted for, suggesting that the observed changes are driven by shifts in birth counts rather than changes in per capita offending. Expanding on this, we use criminal records to examine a broad range of crimes, finding significant impacts on vandalism and illegal abortions, with no comparable effect on other offenses prosecuted under Soviet law.[9] Our study contributes to this literature by leveraging the historical context of the 1930s Soviet Union—a setting where contraceptive technology was still in its infancy and access to reproductive restriction alternatives was limited (David, 1974). This setting allows us to examine how restrictive abortion policies, implemented in environments with limited substitutes, can push procedures underground, leading to an increase in illegal abortions and generating downstream societal effects, including increases in offenses such as vandalism. Importantly, we find no evidence of effects on more serious violent crimes. This pattern is broadly consistent with the hypothesis originally proposed by Donohue and Levitt (2001) and more recently revisited by Donohue and Levitt (2020).

Third, our paper advances the understanding of abortion policy's impact, a topic of significant policy and scholarly interest that has been previously studied largely through qualitative research, survey data, or cross-region correlations (Jones and Pineda-Torres, 2024). A new wave of literature attempts to provide causal evidence, using research designs such as synthetic control methods (White et al., 2023; Hjalmarsson et al., 2021b; Clarke, 2023).[10] Three of these are particularly relevant, as they also explore the causal impact of abortion policy shifts. First is the influential investigation by Myers (2017), which demonstrates that cohorts of young women who experienced the liberalization of abortion access in the United States subsequently exhibited a fall in fertility rates and a

[8] Donohue and Levitt (2001) claim that abortion liberalization reduces crime has drawn criticism. Joyce (2004) cites unaccounted time effects; Foote and Goetz (2008) highlight a coding error and the need for per capita crime rates. For a summary of this debate, see Hjalmarsson et al. (2021b).

[9] We obtain similar results whether we use crime levels or crime rates (i.e., total crimes divided by cohort size), suggesting that our findings are not explained by cohort size effects.

[10] Also related is the"Turnaway Study" by Foster (2020), which surveyed about 1,000 women seeking abortions across 30 US states, some of whom were denied based on gestational age. Pre-treatment differences in socioeconomic characteristics of women among two groups of women (Foster, 2018), along with survey attrition and recall issues (Miller et al., 2020), posed significant questions to the study's validity. In an attempt to address these well-known problems, Miller et al. (2023) recently employed a difference-in-differences approach with credit data, uncovering increased financial strain post-denial of abortion, but with limited statistical power, they were unable to detect effects beyond the financial distress.

lower incidence of "shotgun" or pregnancy-induced marriages. Second, and even more recently, Londoño-Vélez and Saravia (2025) documents how abortion access in Medellín is shaped by the gender of the presiding judge, with female judges more likely to grant access—an outcome associated with improved maternal health and higher educational attainment for children. While our work shares some overlap, we contribute by examining a wider range of outcomes, including illegal abortions, crime, infant mortality, and demographic shifts. We adopt an approach that examines both the direct and indirect societal effects of a nationwide abortion ban. Our first main contribution relative to earlier studies is to empirically document substitution toward illegal abortions in response to legal restrictions—an effect long hypothesized in the literature but previously unmeasured due to the absence of systematic data. Our setting offers one of the first systematic assessments of this substitution margin. Second, we provide evidence from an abortion ban in a large country (that covers all of present-day Russia), making our study relevant to the ongoing abortion debate in many countries, including today's Russia, a geopolitically important country (The Moscow Times, 2023).[11] Third, our setting features an abrupt transition from a highly liberal abortion policy under Vladimir Lenin to a highly restrictive one under Joseph Stalin. In both instances, the policies were implemented by centralized authoritarian leaders driven by ideological motives rather than changing political or religious attitudes (Solomon, 1992), positioning the abortion ban as an instructive natural experiment. Our empirical design, which incorporates a difference-in-differences using pre-treatment abortion rates and corroborating results through a triple-difference strategy, together with extensive administrative archival data—most of which were classified by the Soviet regime and used in scholarly work for the first time—enables us to examine societal level effects that have previously been challenging to study in prior scholarship. The administrative records that detail birth rates, maternal and child mortality segmented by cause of death and age, as well as crime data differentiated by offense type for almost the entire Soviet Union, provide insights that are challenging to obtain from contemporary data in both developing and developed nations and are even more scarce in historical research. Last, we augment the existing literature by delivering insights from a non-US setting, thereby enriching the body of evidence on abortion policy.[12] This initia-

[11] We also differ from prior work by linking abortion data to a broader range of outcomes, such as cause-specific maternal and child deaths, and political 'offenses.'

[12] Notable exceptions include Londoño-Vélez and Saravia (2025), who provide evidence from women who sought legal remedies to obtain reproductive rights in Medellín courts; Kornileva (2019), who examines the direct demographic consequences of the abortion ban in the USSR, including effects on birth rates and child mortality; and Pop-Eleches (2006), who studies the socioeconomic implications of an abortion ban in Romania. These studies offer valuable insights into the individual and societal effects of abortion policy shifts.

tive is in direct response to the growing critique that prior research is overly reliant on US data (see, e.g., Myers, 2022), which is particularly guarded and fraught with difficulties in determining the precise timing of legislative changes on abortion and relies on survey data. More broadly, our setting enables us to estimate both the direct effects of abortion restrictions—on fertility and maternal health—and indirect spillovers, including changes in illegal abortions, child health, and crime. By jointly examining these outcomes, we provide a unified empirical account of the dynamic trade-offs induced by a large-scale abortion ban.

The rest of the paper is organized as follows. Section 2 discusses the historical background of the abortion ban. Section 3 introduces the data and presents descriptive statistics. Section 4 details the empirical approach. Section 5 reports the main results, and Section 6 considers possible mechanisms. The final section offers some concluding remarks.

2 Background

Soviet Union and Liberalization of Abortion. In the early 20th century, the Russian Revolution of 1917 marked a seismic shift in the political and social landscape of the vast Russian Empire. The overthrow of the Tsarist regime in February 1917, followed by the Bolsheviks' October Revolution, paved the way for the establishment of a socialist government led by Vladimir Lenin. Shortly thereafter, the Union of Soviet Socialist Republics (USSR) was officially formed, consolidating power under the Communist Party and uniting the Russian Soviet Federative Socialist Republic (RSFSR) with neighboring socialist republics such as Ukraine, Belarus, Georgia, Armenia, and Azerbaijan. Our study focuses on the Russian Soviet Federative Socialist Republic (RSFSR)—the largest and most populous constituent of the Soviet Union—which we refer to simply as the Soviet Union throughout. Geographically, this area corresponds closely to the boundaries of present-day Russia. During the post-revolutionary societal restructuring, Vladimir Lenin legalized abortion in November 1920, establishing one of the most expansive frameworks for abortion rights of its era, granting women access to state-funded, legal abortion services (Goldman, 1993). The policy provided access to pregnancy terminations at state-sponsored hospitals at no cost. Numerous Soviet thought leaders advocated for the liberalization of abortion based on their ideology (Solomon, 1992). Vladimir Lenin himself demanded "annulment of all laws against abortions" and described the restriction on abortions as "nothing but the hypocrisy of the ruling classes. These laws do not heal the ulcers of capitalism, they merely turn them into malignant ulcers that are especially

painful for the oppressed masses" (Lenin, 1913). Leon Trotsky, another influential leader of the Russian Revolution, observed, "The birth of a child is for many women a serious threat to their position. That is why the revolutionary government brought a woman the right to an abortion." (Trotsky, 1936).[13]

Abortion Ban. The subsequent years saw Joseph Stalin consolidate power, advancing a sweeping state-building agenda centered on rapid industrialization and agricultural collectivization.[14] These policy shifts occurred alongside an intensification of coercive state capacity, including widespread repression (Zhukov and Talibova, 2018) and the systematic expansion of state-controlled radio network (Berkhoff, 2012). Within this evolving institutional landscape, the 1936 abortion ban appears as part of a coordinated policy bundle aimed at managing demographic outcomes and increasing the future population. This shift manifested in 1936 when Stalin issued a decree that abruptly reversed the liberal abortion policy previously established under Lenin. Specifically, on June 27, 1936, a law was enacted prohibiting abortions in all settings, citing health risks to women; only under exceptional and medically necessary circumstances could abortion be performed. Under this law, women who received illegal abortions faced fines, while medical practitioners who performed them risked two years of imprisonment. Relatives or spouses influencing a woman's decision to abort could face up to three years in jail.

Rationale for Abortion Ban. There are no specific, widely-cited quotes from Stalin or top Soviet officials that encapsulate an official stance against abortion, as public statements from the Soviet era were often couched in the language of state policy. However, perusal of propaganda literature just following the ban gives us clues on the state's rationale behind the 1936 ban on abortion. The ban on abortion in the Soviet Union, encompassing both private clinics and public hospitals, appears to be an initiative aimed at fostering population expansion to facilitate Stalin's ambitions for industrializing and modernizing the Soviet Union. As noted by a prominent state propaganda outlet discussing the abortion ban, it stated: "In the growth of our population we see the source of the country's wealth," (Rabotnitsa Journal, 1936). Similarly, another state-run magazine resonated with the emerging national sentiment, proclaiming, "We have so much to do! ... We need more and more fighters - builders of this life. We need people ... Abortion is an evil legacy of the order when a person lives by narrow personal interests, and not by

[13]Nadezhda Krupskaya, wife of Vladimir Lenin, similarly observed, "The fight against abortion should not be waged by persecuting mothers who undergo abortion, often at risk to their own lives; the struggle should be aimed at eliminating those social reasons that put the mother in such a position that it is either abortion or drowning." (Krupskaya, 1920).

[14]Anchored in the First Five-Year Plan (1928–32), this vision mobilized resources toward heavy industry at the expense of consumer goods, while forcibly restructuring the agrarian sector through the collectivization of peasant land.

the life of the collective." (Soltz, 1937). During the Stalinist period, abortion propaganda largely avoided gendered moral appeals, instead invoking themes of collective responsibility and national duty. The focus of our study is limited within the Stalinist period, during which the state's messaging around abortion did not actively seek to reconfigure gender norms (Nakachi, 2021). Soviet discourse emphasized medical risk over any normative commitment to gender or family structure. One contemporary noted, "No moral ideals, no communist ideals even, are allowed to interfere. Marriage laws, abortion permits, raise simply the question: What is good for healthy survival?" (Strong, 1927).[15] This historical context provides a relatively clean empirical setting to assess the societal consequences of a top-down reproductive policy intervention, capturing both its intended demographic impacts and its unintended effects on fertility behavior, maternal health, and crime. Figure 1 provides a concise chronological summary of some pivotal events surrounding the 1936 criminalization of abortion in the Soviet Union.[16]

Abortion Ban and Financial Cushion. While the state's public justification for the 1936 abortion ban emphasized national duty and demographic growth, the policy itself did not operate in isolation. Alongside the criminalization of abortion, the Soviet government introduced a bundle of expanded maternal benefits—including increased allowances for newborn care and child feeding and extended maternity leave—intended to subsidize motherhood and promote fertility (Kollontai, 1921). These concurrent financial benefits likely mitigated the policy's impact by lowering the economic cost of childbirth, potentially more so in regions with higher pre-ban abortion rates. In the absence of this support, the adverse effects may have been substantially more pronounced. Although our empirical strategy focuses on the impact of the abortion restriction itself, the simultaneous rollout of maternal welfare provisions introduces an important interpretive nuance. Increases in adverse outcomes—such as illegal abortions, infant mortality, or social disorder (e.g., vandalism)—should be viewed as lower-bound estimates of the abortion ban's full effect, as these outcomes were potentially attenuated by concurrent state subsidies for motherhood. Conversely, observed increases in birth rates, the most consistently documented outcome in prior work, may reflect an upper bound, insofar as they were jointly driven by coercive restrictions and expanded incentives. To mitigate con-

[15]The shift toward gendered moral appeals—emphasizing maternal responsibility and personal morality of women—emerged only much later, following the re-legalization of abortion in the 1960s. Slogans such as "Abortion Will Deprive You of Happiness!" reflect this later transition in state discourse (Randall, 2011), but fall outside the time frame of our analysis.

[16]We restrict our analysis to the pre–World War II period, as postwar data are subject to substantial limitations. Much of the available information post-war relies on extrapolations (Avdeev et al., 1995), making it difficult to credibly study either the reversal of the abortion ban in 1955 or the broader liberalization of abortion policy that followed.

cerns about pre-existing regional variation in abortion conditions, we control for baseline access to reproductive healthcare, including gynecological visits, midwife density, and hospital availability.

3 Data

The foundation of our research is a large-scale collection and digitization of historical data from the Soviet Archives in Moscow, covering diverse sectors such as health, population, crime, and various other aspects of the Soviet Union's historical landscape, including state radio coverage. The key outcome and explanatory variables come from the State Archive of the Russian Federation (GARF) and were originally classified and utilized exclusively by the USSR's Central Authority for planning and intelligence purposes. To conduct our analysis, we photographed and digitized previously classified documents. Examples of the raw data on births and deaths are shown in Figure A1 and Figure A2. We collected data from the pre-war period, and our analysis focuses on the years surrounding the abortion ban, from 1930 to 1940, within the territory of modern-day Russia, historically, the RSFSR (Russian Soviet Federative Socialist Republic).[17] The patterns in the data align with significant historical events in the USSR, such as the famine of 1932–1933. This period, marked by a spike in mortality and a decline in birth rates, highlights the dataset's relevance and reliability in capturing the demographic and social realities of the time (as shown in Figure A3 in Appendix A). These observations are consistent with Mitrokhina and Herrera (2024), who investigate the puzzling persistence of data accuracy in autocratic regimes, such as the Soviet Union and modern-day Russia. They attribute this phenomenon to enduring institutional arrangements rooted in the Russian Empire, including the legal framework for data collection, which remained intact despite regime changes.[18] Building on this foundation, we next describe the core variables used in our analysis and briefly outline how we define and interpret treatment exposure to the 1936 abortion ban. An organized list of sources and variable descriptions is available in Appendix Table B1. Because regional names and administrative boundaries have changed over time, Appendix B describes our procedure for standardizing regional identifiers, closely following the best practices outlined by Giuliano and Matranga (2021). Descriptive statistics for our main variables of interest are presented in Table A1.

Births and Infant Mortality. To measure the direct effect of the abortion ban, we draw

[17] World War II devastated the Soviet Union and severely disrupted administrative capacity, rendering systematic data collection nearly impossible during and immediately after the conflict.

[18] This view is corroborated by several studies, for instance, those discussed in a recent review by Zhuravskaya et al. (2024).

on birth and infant mortality records from the Bureau of Statistics of the USSR. Birth data are disaggregated by maternal age across modern-day Russia and normalized by the corresponding female population. Mortality records include child deaths classified by age and gender and are further supplemented with cause-specific death statistics. These data introduce an additional dimension of variation, enabling a richer analysis of mortality by age and cause. We further merge these statistics with hospital-based mortality reports to construct a dataset that links maternal and child deaths to pregnancy-related causes. The hospital records provide detailed cause-of-death classifications, allowing us to identify over a dozen categories, including deaths from attempted abortions, premature births, miscarriages, septic infections following childbirth, and other pregnancy-related complications. For infants, causes of death such as congenital weaknesses and premature birth are also documented, which may be linked to abortion-related risks, particularly in a setting where access to safe procedures and post-abortion care is restricted (under criminalized abortion).

Illegal Abortions and Vandalism. Using crime data from the Prosecutor General's Office of the USSR, we assess how the abortion ban influenced patterns of state-defined offenses, including cases involving illegal abortion and property-related crimes such as vandalism. These reports, prepared by regional chief prosecutors for each region, were consolidated by our research team to provide nationwide coverage of modern-day Russia, documenting a wide range of crimes across regions. While initially classified, the dataset provides a rare glimpse into Soviet-era crime trends, including the introduction of "illegal abortions" as a crime category following the abortion ban. This allows for a rare empirical opportunity to trace behavioral substitution away from legal abortion and toward informal alternatives. The reports also allow us to examine whether the abortion ban affected broader patterns of criminal activity. In particular, we examine whether criminalized abortions co-occur with other categories of state-recorded offenses, such as political crimes, to assess whether the observed patterns reflect broader enforcement dynamics, rather than arising from the restriction of reproductive access. While political offenses may reflect broader unrest, we focus on a distinct category, "hooliganism", which includes vandalism and property-related crimes and is more plausibly associated with abortion-restriction, as suggested in prior literature (Donohue and Levitt, 2001). We draw on annual crime reports disaggregated by age group (adolescents aged 16–18 and adults over 18) to investigate whether the abortion ban coincided with changes in these offense patterns. As shown in Figure A4 in Appendix B, these data also allow us to trace exposure to the abortion ban among the first cohorts born after the policy change, and compare their crime outcomes to those of individuals not yet born at the time of the ban. Since we

have crime data from 1950, we use the earlier cohorts (16 to 18-year-olds) as a control in an event study design, as they were born before the abortion ban.

Abortion Ban Exposure. The key independent variable in our analysis is pre-ban abortion intensity, measured as the number of abortions per 1,000 people. These data are sourced from Soviet Ministry of Health, providing nationwide coverage across the RSFSR—the territory comprising modern-day Russia. The variation in these pre-ban abortion rates from January to June 1936, serves as our primary source of cross-sectional exposure, which we interact with a post-ban year indicator. Figure 2 visualizes this variation, with abortion rates ranging from 2.2 per 1,000 women in Komy AO to 38.9 per 1,000 in Moscow. Interestingly, wealthier regions—including Moscow, Leningrad (33.3), and Azovo-Chernomorsky Krai (19.7)—exhibit the highest abortion rates, while poorer, rural regions report substantially lower rates. As shown in Figure 3, pre-treatment abortion rates are positively correlated with both income per capita and the availability of midwives across regions. This pattern suggests that regional variation in abortion intensity is more likely explained by access to public health services—reflecting both supply-side capacity and the ability to seek care—rather than by poverty-driven demand for abortion services.

Measurement Error. While the data are drawn from official Soviet records, we acknowledge the possibility of measurement error—particularly underreporting in rural regions. Under standard assumptions, classical measurement error in pre-treatment abortion rates would attenuate our estimated treatment effect toward zero. If, however, underreporting is non-classical or correlated with post-treatment outcomes, this could bias our estimates. To mitigate this concern, we control for baseline income, the availability of midwives, hospital capacity, and pre-treatment rates of female hospital visits—variables that may jointly capture both demand- and supply-side determinants of abortion access, as well as variation in administrative capacity. These controls are interacted with year fixed effects to flexibly absorb baseline regional heterogeneity. Reassuringly, our results remain robust to the inclusion of these controls. For outcomes beyond fertility, particularly infant mortality, illegal abortion, and vandalism, such bias implies our estimates likely understate the true effects. These forms of behavior are more likely to occur in informal settings, making them harder to observe in administrative records. Thus, the effects we report should be interpreted as lower-bound estimates.

Control Variables. Our regressions account for two time- and space-varying variables that are likely correlated with other policy shifts occurring around the abortion ban: Stalin's purges (political repressions) and the spread of radio network during the same period when abortion was criminalized. To measure political repressions, we use data

from the Russian Memorial Society, a human rights organization established during the *perestroika* era (the period of political and economic reform in the Soviet Union). Memorial's mission to preserve the memory of Soviet-era repression victims has resulted in a dataset that meticulously tracks political repressions across the Soviet Union, documenting the scale of victimization by region. To account for concurrent exposure to state propaganda, we construct a region-level measure of average annual radio signal strength, reflecting Stalin's large-scale expansion of the Soviet radio network during the same period. This measure is constructed using regional radio transmission data, as referenced in Radio Journals (1929-1940). Additional controls are all the available baseline socioeconomic characteristics. These controls include regional income and wages, the number of hospitals, female hospital visits (recorded as visits to gynecologists in Soviet archives, likely reflecting reproductive-related care by women), and midwife density, each interacted with year fixed effects. These indicators are sourced from reports by Soviet state authorities for the year 1935 and the first half of 1936, which we digitized for this study. Population data is drawn from the Soviet censuses conducted in 1926, 1937, and 1959, with extrapolations made using the method detailed in Appendix B4 .

4 Empirical analysis

Difference-in-Differences. To estimate the effect of an abortion ban, we use regional variation in the pre-ban abortions interacted with a post-ban dummy variable. Our baseline specification is as follows:

$$Y_{rt} = \beta \; Pre\ Ban\ Abortions_r \times Post\ Ban_t + X_{rt}\psi + \theta\, W_r \times \sigma_t + \gamma_r + \sigma_t + \epsilon_{rt} \qquad (1)$$

where indexes r corresponds to region, and t denotes year. The variable Y_{rt} represents a range of outcomes, including births or infant deaths per woman, illegal abortions, and vandalism offenses; γ_r and σ_t, are region and year fixed effects, respectively. *Pre Ban Abortions* 1936 represents the number of abortions per woman in each region before the ban. The *Post Ban* variable is a binary indicator that assumes a value of one for all regions starting in 1937, reflecting the post-ban period when the abortion ban was in effect and zero otherwise. The interaction term between Pre-Ban Abortions $\times$ Post Ban serves as our primary variable of interest. The coefficient on this interaction term β serves as the difference-in-differences estimator, quantifying the effect of the abortion ban on the outcome under consideration. $\mathbf{X}$ is a matrix of time-varying controls that include repressions per capita and radio signal strength in the region. Meanwhile, $\mathbf{W}$ is a vector

of pre-treatment baseline characteristics—such as income per capita, hospitals and midwives per capita, doctor visits and wages—which are interacted with year dummies. We cluster standard errors at the region level, consistent with the level of treatment variation. We also report results using wild cluster bootstrap and Conley spatial standard errors to account for small-sample and spatial correlation concerns, respectively (Cameron et al., 2008; Conley, 1999). The results are similar across approaches.

While our treatment varies continuously in intensity, it is introduced uniformly across all regions at a single point in time: 1936 serves as the pre-treatment period, and from 1937 onward, all units receive some level of treatment based on pre-ban abortion intensity. This setup is consistent with the two-period continuous treatment framework discussed in recent work by Callaway et al. (2024), where no units are treated in the first period and all receive varying treatment doses in the second. As they note, identification in such settings is still possible when we use the low-intensity treatment dose as a control group. In our context, we find that results are essentially unchanged when adopting this approach, treating regions with the lowest observed pre-ban abortion intensity as a comparison group.

Event Study. To examine the dynamics of treatment effects and test for pre-trends, we extend the baseline difference-in-differences specification by replacing the single post-ban indicator with a full set of year-by-treatment exposure interactions. This approach yields the following event study specification:

$$Y_{rt} = \sum_{t \neq 1935} \beta_s \; \textit{Pre-Ban Abortions}_r \times D_t + \gamma_r + \sigma_t + \epsilon_{rt} \tag{2}$$

In this specification, D_t denotes year-specific indicators (excluding 1935 as the reference year), interacted with the continuous measure of pre-ban abortion exposure. The coefficients β_s trace out the relationship between pre-ban abortion intensity and each outcome over time, relative to the year preceding the ban, allowing to examine the evolution of treatment effects following the abortion ban.

Triple Difference. When data on maternal and child age groups are available for an outcome, we exploit an additional layer of identifying variation by estimating a triple-difference specification. This approach leverages variation across regions, over time, and between age-defined subpopulations—specifically, reproductive-age versus non-reproductive-age women for births, and infants versus older children for mortality within region-year.

This approach is implemented by estimating the following specifications:

$$Births^{j}_{rt} = \beta \; PreBan\ Abortions_r \times PostBan_t \times Reproductive\ Age_j + \\ + \zeta_{rt} + \eta_{rj} + \lambda_{tj} + \epsilon_{rtj} \tag{3a}$$

$$InfantDeaths^{j}_{rt} = \beta \; PreBan\ Abortions_r \times PostBan_t \times Infants_j + \\ + \zeta_{rt} + \eta_{rj} + \lambda_{tj} + \epsilon_{rtj} \tag{3b}$$

Our analysis leverages a triple-difference framework to examine the impact of the abortion ban on demographic outcomes, with age groups indexed by j—representing mothers' age groups in equation (3a) and children's age groups in equation (3b). Specifically, the coefficient α captures the differential effects of the ban on birth rates among reproductive-age women versus non-reproductive-age women (3a) and on infant mortality relative to mortality rates of older children (3b). The use of infants and older children as comparison groups offers several advantages. Infants are more biologically vulnerable to early-life shocks, and are therefore more likely to reflect short-run impacts of changes in maternal health or prenatal environments (Currie and Rossin-Slater, 2015; Simmons et al., 2021). In contrast, older children were born prior to the policy and are less likely to be directly affected by the ban, making them a reasonable benchmark. The triple-difference design also helps address potential general equilibrium concerns, such as region-wide changes in public health or economic conditions, that might influence all women or children within a region. By comparing across age groups within region-year cells, we aim to net out such broader shocks, although we remain cautious about residual sources of differential exposure. We include region-by-year fixed effects ζ_{rt} to account for time-varying regional factors such as shifts in healthcare access or economic conditions. Region-by-age group fixed effects η_{rj} capture age-specific differences within regions, and year-by-age group fixed effects λ_{tj} allow for cohort-specific national shocks. These fixed effects help mitigate concerns about unobserved factors that may vary systematically across both time and age groups. We continue to cluster standard errors at the region level, reflecting the level of treatment variation in the baseline.

We also estimate an event study version of the triple-difference model to examine the dynamics of treatment effects and test for pre-existing trends across subpopulations. Specifically, we replace the post-ban indicator with a full set of year dummies interacted with both pre-ban abortion intensity and the relevant subgroup indicator (e.g., reproductive-age women or infants). This approach allows us to trace the evolution of treatment effects over time and assess the parallel trends assumption within the triple-

difference framework by comparing treated and control subgroups across years. The specifications are as follows:

$$Births^{j}_{rt} = \sum_{t \neq 1935} \beta_s \; Pre\text{-}Ban\; Abortions_r \times ReproductiveAge_j \times D_t + \zeta_{rt} + \eta_{rj} + \lambda_{tj} + \epsilon_{rtj} \tag{4a}$$

$$InfantDeaths^{j}_{rt} = \sum_{t \neq 1935} \beta_s \; Pre\text{-}Ban\; Abortions_r \times Infants_j \times D_t + \zeta_{rt} + \eta_{rj} + \lambda_{tj} + \epsilon_{rtj} \tag{4b}$$

The coefficients β_s represent year-specific differences in outcomes for subgroups defined by pre-ban abortion intensity and age category (e.g., reproductive-age or infant status), relative to the 1935 baseline. Fixed effects—ζ_{rt}, η_{rj}, and λ_{tj}—help account for confounding that varies across regions, years, and age groups. Standard errors are clustered at the regional level, consistent with our baseline specification.

5 Results

The results section is organized as follows. We begin by outlining descriptive patterns in demographic and crime outcomes surrounding the 1936 abortion ban. We then present the main results in two parts. First, we examine the direct effects of the policy on birth and infant mortality. Second, we turn to the policy's indirect effects, focusing on illegal abortions and vandalism. Finally, we assess the validity of the identification strategy using event study trends, some key robustness checks, and a triple-difference design.

5.1 Descriptive Analysis

We begin by outlining descriptive patterns in the data. Figure A3 plots births and infant deaths per woman from 1930 to 1940. As shown in Panel A, there is a decline in births coinciding with the famine years, in line with broader historical trends. Around the time of the 1936 abortion ban, both birth and infant death rates exhibited an upward trend. Turning to crime data, we first describe the structure of the dataset. Figure A4 depicts how our vandalism measure is constructed by cohort, and identifies the cohorts exposed to the abortion ban. Figure A5 illustrates changes in illegal abortions and vandalism over time. Panel A shows no illegal abortions recorded prior to the 1936 ban, when abortion was legal. Panel B of Figure A5 presents the raw data on vandalism cases. We observe

a noticeable increase in vandalism among cohorts born after the abortion ban, whereas rates for earlier cohorts remain relatively flat.

5.2 Direct Effects

To explore potential direct effects of the abortion ban, we examine trends in birth and infant mortality rates across regions stratified by pre-ban abortion intensity. By dividing regions into terciles based on abortions per 1,000 women, we investigate how demographic outcomes evolve differentially in more versus less exposed areas. Figure A7 plots births per 1,000 women (Panel A) and infant mortality per 1,000 women (Panel B) across terciles of pre-ban abortion intensity. Regions in the highest tercile exhibit sharper increases in both outcomes following the ban, consistent with greater exposure to the policy. These patterns offer suggestive difference-in-differences evidence of a more pronounced response in areas with higher initial abortion rates. Notably, these high-intensity regions initially exhibit the lowest levels of births and infant mortality, suggesting a "catch-up" effect after the abortion ban with lower tercile abortion regions. Importantly, the trends across the three terciles are similar before the ban, indicating that differential pre-trends are unlikely to explain the results. These patterns are consistent with estimates from an event study specification that includes region and year fixed effects, as shown in Figure 4. To further examine these patterns, we employ a triple-difference design that accounts for variation across regions, time, and demographically relevant subgroups—comparing reproductive- and non-reproductive-age women for birth outcomes, and infants versus older children for mortality. This strategy enables a more refined comparison across differentially exposed populations within region-year cells. As shown in Figure 5, we observe limited divergence prior to the policy and more pronounced post-ban differences, relative to the 1935 baseline. To account for potential time-varying confounders, we include controls for concurrent Stalin-era policies, such as the intensification of political repression and the expansion of state radio infrastructure. We also adjust for baseline regional differences in economic and healthcare conditions, such as average income, hospital density, and gynecological and pediatric visits, as well as the number of midwives. The estimates in Table 1 suggest increases of approximately 0.5 births and 0.1 infant deaths per woman following the abortion ban. These magnitudes imply that for every 10 abortions prevented, about 5 resulted in additional births, suggesting that not all pregnancies are carried to term, potentially due to continued, though reduced, access to informal abor-

tions.[19] These estimates correspond to a 10% increase in birth rates and an 11% increase in infant mortality rates, relative to the 1935 pre-ban means.

5.3 Indirect Effects

We next turn to potential indirect effects of the abortion ban, focusing on illegal abortions and vandalism. As in our earlier analysis, we divide regions into three terciles based on pre-ban abortion intensity. Figure A8 plots the evolution of illegal abortions (Panel A) and vandalism (Panel B) across these terciles. In both outcomes, regions in the highest tercile display a more pronounced increase following the ban. These patterns are consistent with the difference-in-differences estimates reported in Table 2, Panel A, based on the specification in equation (1). Column 4 includes region and year fixed effects, along with controls for political repression per capita and radio coverage. The estimates suggest that approximately 0.3 additional individuals per 1,000 are convicted for illegal abortions following the abortion ban in high-abortion-intensity regions. To put the magnitude in perspective, the estimates suggest that an increase of 10 abortions per 1,000 women is associated with approximately 2.6 additional illegal abortions per woman. This finding offers some of the first quantitative evidence of substitution from legal to underground abortion practices following a state-imposed ban.

The indirect effects of the abortion ban appear to extend beyond illegal abortions to include vandalism for cohorts exposed to the ban. Our data record incidents involving both adolescents (ages 16 to 18) and adults (18 and above), allowing us to examine how outcomes may have shifted across age groups. Panel B of Table 2 reports the estimated effects of the abortion ban on vandalism for both groups. In each case, post-ban vandalism rates increase relative to pre-ban levels, with magnitudes corresponding to an approximate 28% rise. To assess whether these increases reflect pre-existing trends, Figure 6 presents event study estimates using cohorts born just before the abortion ban as the reference group. If the observed increase in vandalism were part of a general trend, we would expect to see rising vandalism rates across these earlier cohorts as well. Instead, we find no evidence of a differential upward trend in vandalism among these groups prior to the ban.[20] The observed increase in vandalism is suggestive of broader social responses to the abortion ban. The findings are consistent with the view that abortion policy can affect

[19] For every 10 additional pre-ban abortions per woman, the estimates imply approximately four pregnancies that would otherwise have occurred but did not—suggesting continued but possibly informal abortion access, though other factors—such as behavioral adaptation or broader fertility responses—may also play a role.

[20] This is consistent with patterns shown earlier in Figure A5, where vandalism rates remain stable among cohorts born before the ban.

youth behavior in the medium run.[21]

The rise in illegal abortions and vandalism does not appear to reflect a broader increase in criminal prosecutions during the period. In Figure 7, we examine trends in other crimes, including murder, bribery, theft, and banditry, and find no corresponding increase among cohorts exposed to the abortion ban. This suggests that the observed patterns are not part of a general rise in crime. Moreover, as shown in Figure A9, illegal abortions are not systematically correlated with other categories of crime, including political offenses that were heavily prosecuted under Stalin's regime. This is consistent with qualitative accounts suggesting that these acts were not framed as political or "counter-revolutionary" in nature. Instead, the patterns documented in Table 2 are more plausibly interpreted as downstream consequences of the abortion ban itself, rather than artifacts of broader enforcement trends.

5.4 Identification Checks.

We next discuss five major empirical checks aimed at probing the key identifying assumptions and potential threats to the validity of our baseline estimates.[22] The first relates to the parallel trends assumption, which is central to the difference-in-differences framework. While we cannot verify this assumption directly, we find no evidence of differential pre-trends between high- and low-abortion-intensity regions, suggesting the identifying assumption is not inconsistent with the data (see event study figures in the main text).

Second, to evaluate the robustness of our findings to potential violations of the parallel trends assumption, we implement the sensitivity bounds approach recently proposed by Rambachan and Roth (2023). This method allows for partial identification by bounding deviations from the parallel trends assumption, rather than assuming identical trends between treated and control units in the absence of treatment. Figure A10 illustrates that our main results continue to hold under several plausible deviations, suggesting that modest violations of parallel trends are unlikely to fully account for the estimated effects.

Third, we implement the continuous treatment framework developed by Callaway et al. (2024), which accommodates treatment variation in intensity across units. Specifically, we divide regions into four groups based on the distribution of pre-ban abortion intensity and estimate separate coefficients for each quantile, using the lowest-intensity group as

[21] This interpretation is in line with the hypothesis proposed by Donohue and Levitt (2001), who link abortion restrictions to increased youth crime via higher rates of unwanted births and weakened family structure.

[22] A more extensive set of robustness exercises is included in the dedicated robustness section, with supporting tables and figures provided in the Appendix.

the baseline. This approach allows us to assess whether the effects of the abortion ban scale with initial exposure levels. As shown in Figure A11, we find a clear monotonic pattern: regions with higher pre-ban abortion rates experience larger increases in both births and infant deaths per woman after the ban. These results corroborate our baseline findings and provide additional support for the robustness of our estimates under an alternative identification strategy that does not rely on untreated units.

Fourth, we address the possibility that other contemporaneous policies under Stalin, such as political repression or the expansion of state radio, may have evolved differently across regions with varying levels of pre-ban abortion intensity. If, for example, regions with higher baseline abortion rates also experienced greater repression or propaganda activity over time, and these policies independently influenced fertility or social outcomes, our estimates may partly capture the effects of those policies rather than the abortion ban itself. In Table 1 (Column 3) and Table 2 (Column 4), we control for region-by-time-varying factors, including political repression and radio coverage. The estimated effects remain similar, suggesting that these concurrent policies are unlikely to fully account for the observed outcomes.

Lastly, we note that our difference-in-differences strategy relies on the assumption that there are no unobserved region-by-year shocks that systematically bias the estimates. While the absence of differential pre-trends, the robustness to bounded deviations from parallel trends, and controls for baseline characteristics interacted with year fixed effects (e.g., income, wages, midwife density, hospital access, and female healthcare utilization) help mitigate these concerns, they cannot fully rule out the influence of unobserved confounders. To further address this limitation, we leverage an additional layer of variation where data allow, comparing within-region-year exposure across demographic subgroups differentially affected by the policy. Table 3 shows results from the triple-difference specification, estimated in settings where age cohort variation can be used. Specifically, Panel A reports estimates from equation (3a), focusing on reproductive-age women. This subgroup is motivated by birth rate estimates by age cohort (Figure 8). Panel B applies a similar framework to mortality outcomes, using equation (3b) to estimate effects on infants. Children above age one—who exhibit little change in mortality patterns (Figure 9)—serve as a comparison group.[23] The triple-difference estimates are

[23]Note that the distinction between reproductive-age women is based on observed birth patterns in data, which show no effect on births for certain age groups(after age 30). This pattern likely reflects underlying biological constraints (Figure 8). Similarly, existing research and data suggest (Figure 9) that infant mortality rates were substantially higher than those for older children, both due to biological vulnerability in early life and the compounded effects of poor maternal health and limited neonatal care. These empirical patterns provide motivation for our age-based subgroup comparisons in the triple-difference design.

consistent with our main results, suggesting that region-by-year shocks are unlikely to fully account for the observed effects. This additional layer of variation helps mitigate concerns about general equilibrium forces or unobserved confounders biasing the estimates.

6 Mechanisms

Abortion Risks Mechanism. A plausible channel through which the abortion ban may have affected health outcomes is by increasing reliance on unsafe abortion procedures. If the abortion ban curtailed legal access to abortion, women with unwanted pregnancies may have turned to informal or unregulated methods, potentially resulting in higher maternal and infant mortality. This mechanism would predict a rise in female deaths due to pregnancy-related complications, as well as in infant deaths from conditions associated with unsafe or unsuccessful abortion attempts.

We are fortunate to have access to detailed administrative data that records causes of death at a disaggregated level. These data enable us to directly examine whether mortality due to abortion-related complications increased following the ban. Table 4, Panel A, presents estimates focusing on female deaths closely linked to pregnancy, including attempted abortions, miscarriages, and septic infections. Female deaths by these causes exhibit a notable increase in the post-ban period, with the most pronounced changes observed for deaths explicitly tied to abortion attempts and pregnancy-related complications. This pattern is visualized in Figure A12, which shows a sharp rise in abortion-related female deaths beginning immediately after the ban, with no comparable trend in earlier pre-abortion ban years.

To test the specificity of this pattern, Panel B of Table 4 reports estimates for female deaths due to causes unlikely to be affected by the abortion ban, such as typhus and tuberculosis—diseases transmitted through airborne or environmental exposure rather than through reproductive health channels. These outcomes provide a test of whether the observed patterns are confined to channels plausibly influenced by the abortion ban. Female deaths from these causes remain largely unchanged, lending further support to the view that the observed rise in maternal mortality is more plausibly linked to abortion-related risks.

We also examine whether infant health deteriorated along similar lines. Table 5 disaggregates infant deaths into those plausibly related to abortion complications (e.g., congenital weakness, premature birth), and causes less likely to be linked to reproductive health shocks. Panel A shows an increase in abortion-related causes of infant death, while Panel

B shows no systematic change in less-related causes of infant deaths. These findings are consistent with the interpretation that the abortion ban may have increased health risks for both women and infants by encouraging unsafe procedures and exacerbating vulnerabilities during pregnancy and childbirth.

Cohort Size and Vandalism. While the evidence above points to elevated health risks as a consequence of the abortion ban, we also found that the abortion ban had broader social consequences, particularly in the form of increased vandalism. One possible mechanism explaining the rise in crime is a compositional or behavioral response. That is, increased vandalism is explained by larger cohort sizes following the abortion ban, rather than changes in the composition of those born. Prior work by Hjalmarsson et al. (2021b) on Romania finds that crime increases after abortion restrictions attenuate once adjusted for cohort size. Their findings underscore the importance of distinguishing level effects (more individuals) from rate effects (more crime per individual), particularly when evaluating the long-run consequences of abortion policy. To address this concern in our context, we follow their empirical strategy and re-estimate our results using crime rates per cohort rather than crime counts [24]. Specifically, we normalize crime outcomes by the size of the birth cohort affected by the abortion ban. As shown in Figure A13 and Table A2 in Appendix A, our results remain robust: both the magnitude and statistical significance of the effects persist when expressed as vandalism rates. This provides evidence that the observed increase in crime is not merely a result of more individuals being born, suggesting that the increase in crime is not solely driven by larger cohort size. Instead, the pattern appears consistent with changes in the average propensity to offend, potentially reflecting selection or early-life conditions.

Alternative Mechanisms. Finally, we leverage our large-scale data collection to explore alternative mechanisms and examine whether factors beyond unsafe abortions and pregnancy-related complications could account for the observed rise in female and infant mortality. One potential explanation is that baseline differences in healthcare quality—such as hospital availability, access to gynecologists or pediatricians, or the presence of midwives—may have varied systematically between high- and low-abortion-intensity regions, potentially confounding the observed effects. To account for this, we include controls for pre-ban hospital density, gynecologist and pediatric visit rates, and midwife availability, each interacted with year fixed effects. This allows for flexible controls for differential trends correlated with pre-treatment health infrastructure, without assuming that these differences are constant over time. The inclusion of these variables leaves our

[24] We examine this per-cohort specification for adolescents (aged 16–18) since we can calculate the size of each cohort using birth records from the 1930s.

main results largely unchanged, suggesting that differential access to healthcare services is unlikely to be the primary driver of the observed patterns (see Column 4 of Table 1).

We also consider the possibility that region-specific economic shocks—such as changes in industrial activity or agricultural output—may have affected health outcomes during this period. When these controls and their interactions with time are included in the baseline specifications, our results remain consistent. While these checks cannot fully rule out alternative explanations, they help narrow the set of plausible mechanisms and lend support to the interpretation that increased reliance on unsafe abortion procedures contributed to the rise in maternal and infant mortality following the ban.

7 Robustness

In this section, we assess the robustness of our findings through a series of sensitivity analyses. We examine whether the results are sensitive to alternative approaches to standard error estimation, including methods that account for spatial correlation, small-sample adjustments, and randomization-based inference. We also consider multiple hypothesis testing corrections, alternative model specifications (e.g., fixed effects structure), and variation in the sample period or regional coverage. In addition, we explore heterogeneity in outcomes by exposure to repression and propaganda. While these checks cannot rule out all alternative explanations, they help to assess the stability of the estimated effects of the Soviet abortion ban.

Standard Errors. We assess the robustness of our results to alternative inference procedures in three steps. First, we address the possibility that spatial correlation across regions—particularly due to similarities in pre-ban abortion intensity—may bias standard errors. To account for this, we apply Conley (1999) spatial corrections at varying distance thresholds. As reported in Table A4, the results remain statistically stable across different bandwidths. We extend this approach to other outcomes, including illegal abortions, vandalism, and cause-specific mortality, with consistent results (see Table A5 to Table A8). Second, given that our analysis is based on 56 clusters (regions), we also implement the Wild Cluster Bootstrap method proposed by Cameron et al. (2008), which is designed for inference in settings with a moderate number of clusters. While our cluster count exceeds the recommended rule of thumb of 42 clusters for reliable inference under asymptotic theory, as suggested by Angrist and Pischke (2008), we further validate our findings by employing a bootstrap technique that does not rely on asymptomatic theory. The estimated coefficient distributions, shown in Figure A14 and Figure A15, remain statistically significant at conventional levels using this approach. Third, we examine whether our

findings are sensitive to the level at which standard errors are clustered. While our baseline clusters at the region level—where treatment intensity varies—we re-estimate our results using both finer and coarser cluster definitions. The estimates, reported in Table A9 through Table A11, remain stable across specifications.

Randomized Inference. To assess whether our findings could arise by chance, we conduct a permutation test that randomly reassigns abortion intensity across regions. These placebo distributions are shown in Figure A16 and Figure A17, which plot the coefficient estimates obtained under random assignments of abortion intensity across regions. In each case, the observed treatment effect lies well outside the distribution of placebo estimates. These results indicate that the estimated effects are unlikely to be driven by random variation in treatment assignment. The consistency of this pattern across birth, mortality, and crime outcomes (illegal abortions and vandalism) provides additional support for the robustness of our findings.

Multiple Hypothesis Testing. Evaluating a set of outcomes, such as birth rates, infant mortality, illegal abortions, and vandalism, raises the likelihood of false positives if each hypothesis is tested independently. To address this, we apply multiple hypothesis testing corrections that adjust for the family-wise nature of our inference. To address this, we adopt a family-wise approach that groups these outcomes into a single family of interest. First, we report FDR-adjusted q-values across all key outcomes in Table A12. Second, we apply the Romano-Wolf procedure to control the family-wise error rate. In both cases, the results remain robust, providing additional confidence in the overall pattern of effects.

Alternative Fixed Effects. We next examine whether our results are sensitive to the level at which fixed effects are specified. In Table A13, we re-estimate our models using fixed effects defined at progressively larger geographic units, adjusting clustering accordingly. The results remain statistically significant and broadly consistent with the baseline estimates. Similar patterns are observed for other outcomes, including vandalism (Table A14) and illegal abortions (Table A15), suggesting that the findings are not driven by a specific choice of fixed effects specification.

Excluding Years and Regions. Building on the robustness of our findings to alternative fixed effects, we next assess whether results are sensitive to particular time periods or regions. In Figure A18 and Figure A19, we re-estimate the main specification while sequentially excluding each year from the sample. The results remain stable across all exclusions, suggesting that no single year drives the findings. We also sequentially exclude regions from the sample and observe similar consistency in Figure A20 and Figure A21. These patterns indicate that the estimated effects are not concentrated in a particular region or period, supporting the interpretation of a broadly distributed policy impact.

Heterogeneity by Repression and Propaganda Exposure. We further assess whether the estimated effects vary by baseline exposure to political repression or state propaganda. In Table A16 and Table A17, we compare estimates across regions with above- and below-median radio coverage. The results remain consistent across groups, suggesting limited heterogeneity by propaganda exposure. Similarly, in Table A18 and Table A19, we examine regions with different levels of repression and again find no systematic differences. While these comparisons are not definitive, they indicate that variation in state control mechanisms is unlikely to explain the observed effects. Taken together, these robustness checks suggest that the observed effects are not highly sensitive to modeling choices, sample composition, or alternative sources of heterogeneity.

8 Conclusions

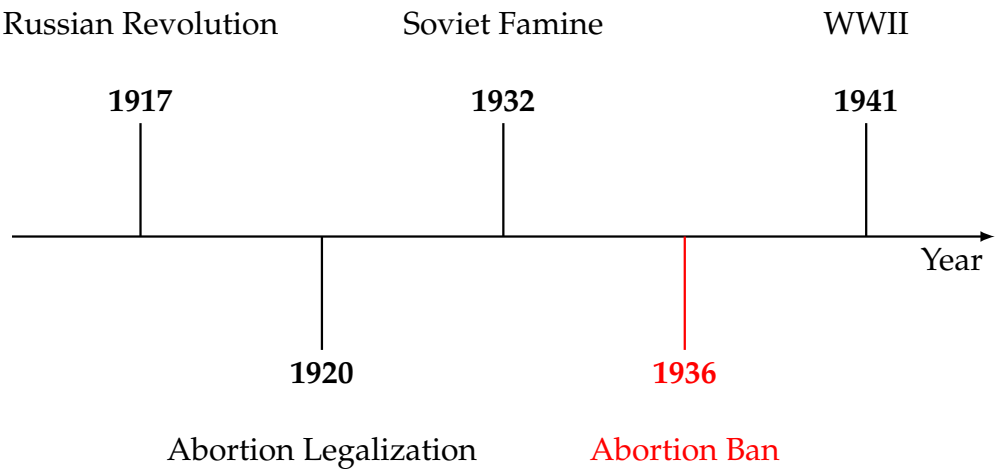
The regulation of reproductive rights has profound implications for societies worldwide, shaping demographic trends, public health outcomes, and social order. Restrictive abortion policies, in particular, remain a contentious issue in both developed and developing countries, with governments grappling with the trade-offs between demographic goals and individual rights. While public debates around abortion policy often focus on moral or ideological grounds, the broader societal implications, both direct and unintended, warrant systematic empirical analysis. Understanding how such restrictions shape reproductive behavior, health outcomes, and social dynamics is essential for evaluating their full policy impact.

This paper utilizes a natural experiment created by Stalin's 1936 abortion ban in the Soviet Union to examine its direct and indirect societal consequences. By capitalizing on regional variation in pre-ban abortion intensity and employing a difference-in-differences framework, we document significant demographic, public health, and social effects of this abrupt policy shift. Our findings suggest that the abortion ban succeeded in raising birth rates, consistent with its demographic objective. However, this increase was accompanied by elevated infant mortality, higher rates of female deaths, and rises in illegal—likely unsafe—abortions and vandalism. These patterns point to broader health and social costs associated with the policy, highlighting the trade-offs embedded in coercive reproductive interventions. Importantly, the observed effects do not appear to be fully explained by contemporaneous Stalinist policies, such as heightened repression or expanded propaganda efforts, or by baseline differences in healthcare access. Robustness checks, including falsification tests and a triple-difference design, yield consistent results across specifications.

Our findings contribute to the broader literature on reproductive rights and their societal implications. The abortion ban not only undermined maternal and infant health but also disrupted social stability, as reflected by increases in illegal abortions and vandalism. These consequences adds the demographic losses typically emphasized by demographic transition theory and underscore the trade-offs embedded in coercive reproductive policy. In this context, our study offers empirical evidence on the broader societal costs of such policies, emphasizing the need to weigh demographic goals against public health and social stability. We view the analysis of longer-run effects as a promising direction for future research, as additional data become available.

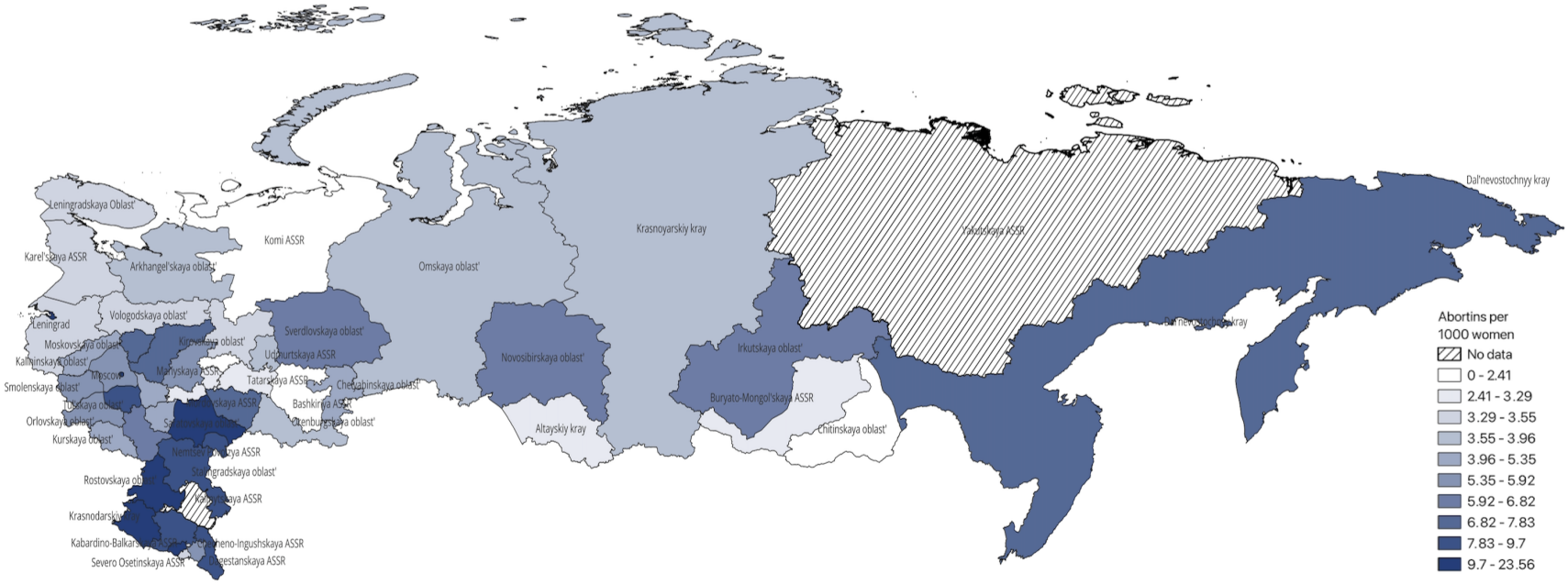
9 FIGURES AND TABLES

Figure 1: Key Events (1917–1941)



Note: This figure situates the 1936 abortion ban within its broader historical context. It marks major political and demographic events, beginning with the 1917 Russian Revolution and the 1920 legalization of abortion, continuing through the 1932 famine, and culminating in the 1936 policy reversal. The timeline also includes the onset of World War II in 1941. These events provide some context for the environment in which the abortion ban was introduced.

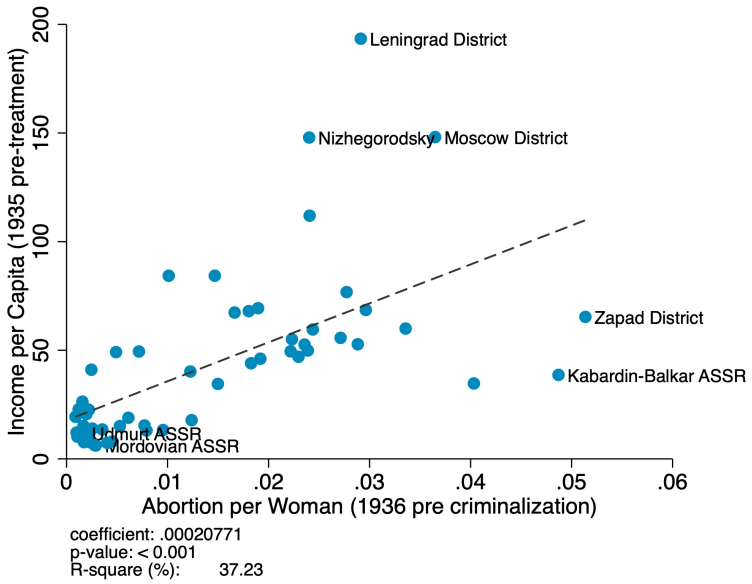
Figure 2: Pre-Ban Abortions per 1000 Women



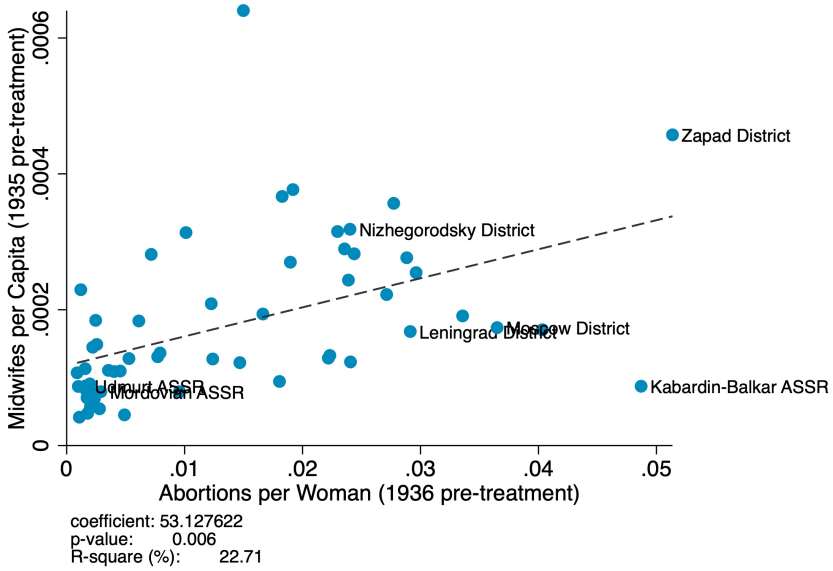
Note: The map shows regional variation in abortion rates per 1,000 women across the USSR during the first half of 1936, before the abortion ban was implemented. These data provide the pre-ban measure used in our empirical analysis and are sourced from the State Archive of the Russian Federation.

Figure 3: Abortion Rates and Baseline Region Characteristics

Panel A: Income Per Capita



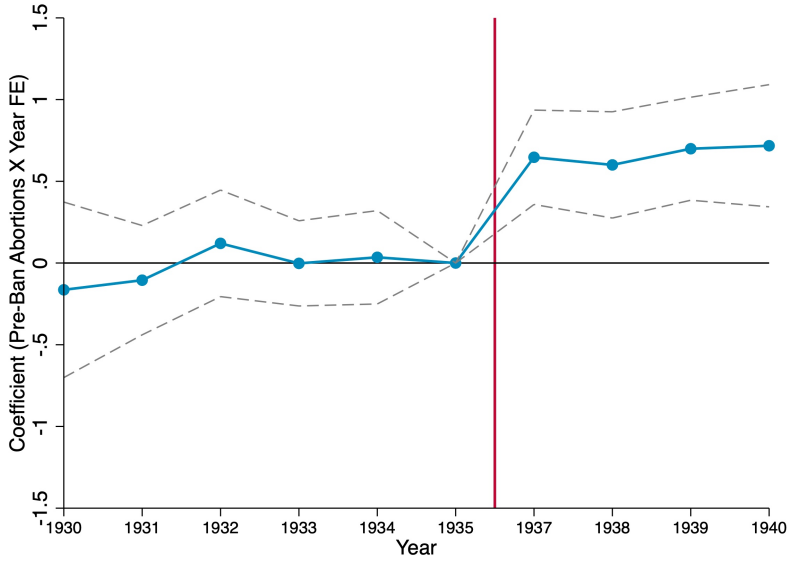
Panel B: Midwives Per Capita



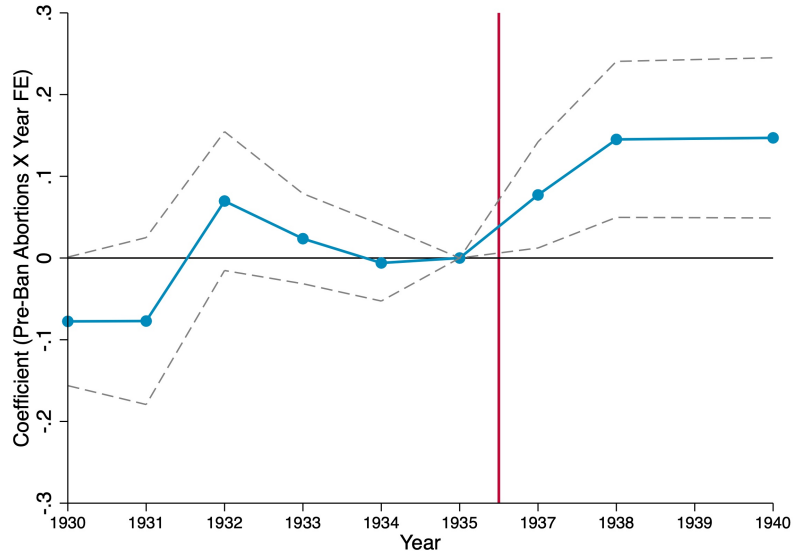
Note: The figure presents scatter plots relating regional abortion rates per woman in 1936 (pre-ban) to baseline characteristics across Soviet regions. Panel A shows income per capita in 1935, and Panel B displays the number of midwives per capita in 1935. Each point corresponds to a region; the dashed line indicates the fitted regression line. Regression coefficients, p-values, and R^2 are reported in each panel. All measures precede the policy change and are intended to characterize baseline variation.

Figure 4: Impact of Abortions Ban on Births and Infant Deaths Over Time

Panel A: Births per Woman



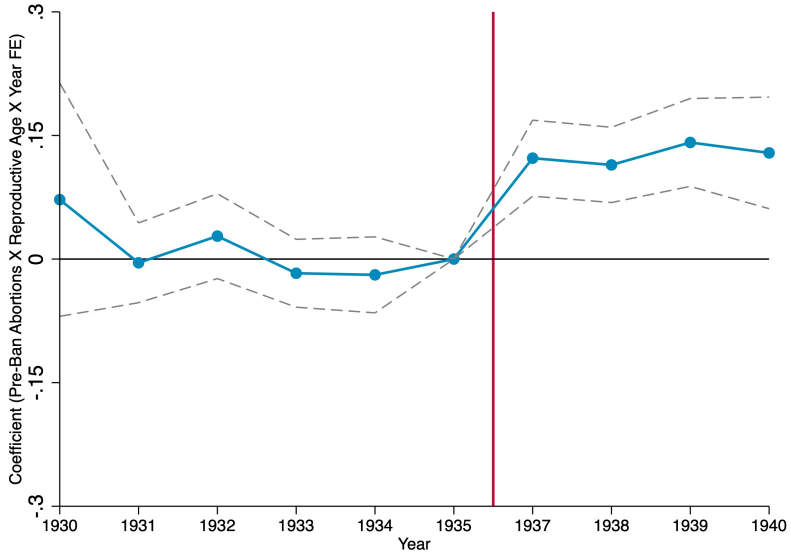
Panel B: Infant Deaths per Woman



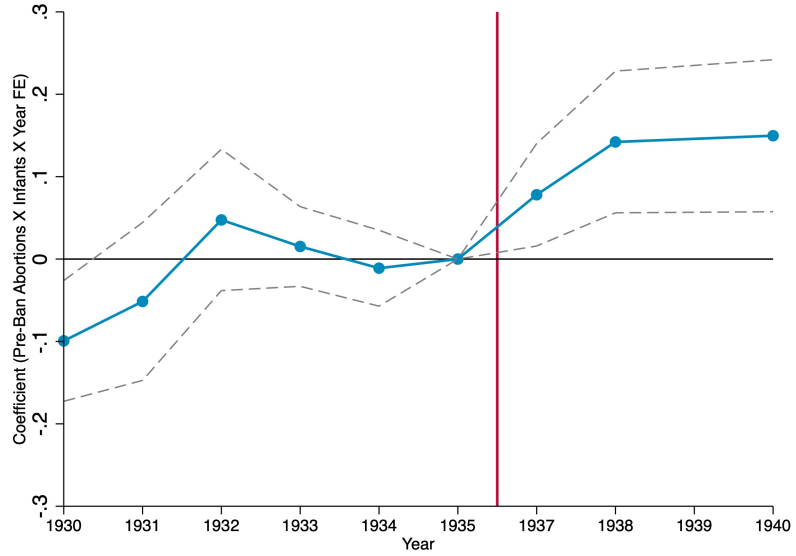
Note: The figure reports coefficients and 95% confidence intervals from regressions examining changes in births per woman (Panel A) and infant deaths per woman (Panel B) around the 1936 abortion ban. Each coefficient reflects the interaction of year dummies with the pre-ban abortion rate (measured in the first half of 1936). The 1935 coefficient is normalized to zero and serves as the reference year. All specifications include region and year fixed effects. The red vertical line marks the policy's implementation in July 1936. See Table 1 for related difference-in-differences estimates.

Figure 5: Impact of the Abortion Ban on Births and Infant Deaths Over Time: Triple-Differences

Panel A: Births per Woman



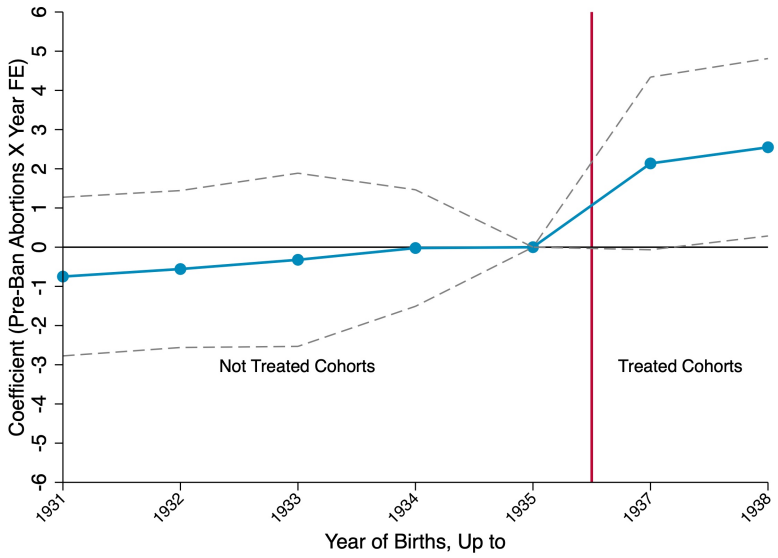
Panel B: Infant Deaths per Woman



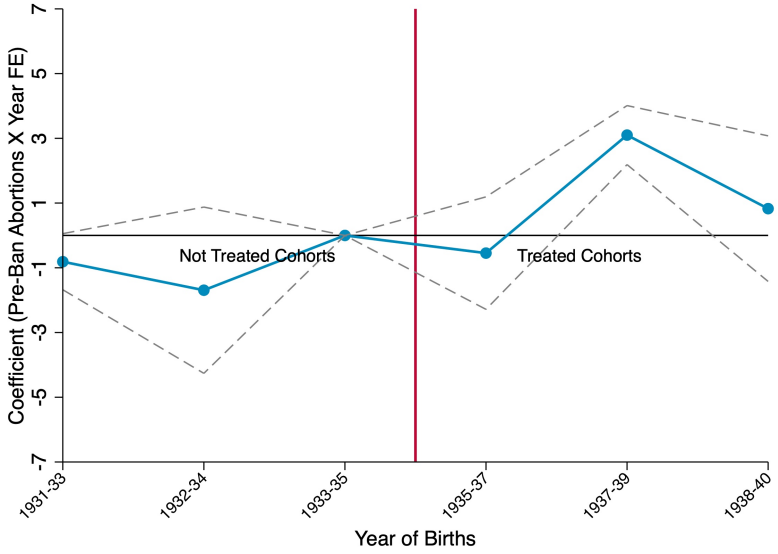
Note: The figure presents coefficients and 95% confidence intervals from regressions estimating the effects of the 1936 abortion ban on births per woman and child deaths per woman, relative to the last pre-ban year, 1935 in a triple differences framework. Year dummies are interacted with the *Pre-Ban Abortions 1936* variable. The *Post-Ban dummy* indicates the policy implementation year. The *Reproductive Age dummy* identifies cohorts under age 30, and the *Infants dummy* denotes cohorts under age 1. All regressions include region fixed effects, region-by-year fixed effects, region-by-age cohort fixed effects, and year-by-age cohort fixed effects. The red vertical line marks the policy's implementation in July 1936. Static triple-differences estimates corresponding to this specification, but averaged across post-ban years, are reported in Table 3.

Figure 6: Impact of Abortions Ban on Vandalism Over Time

Panel A: Vandalism Committed by Individuals Aged 18 and Older

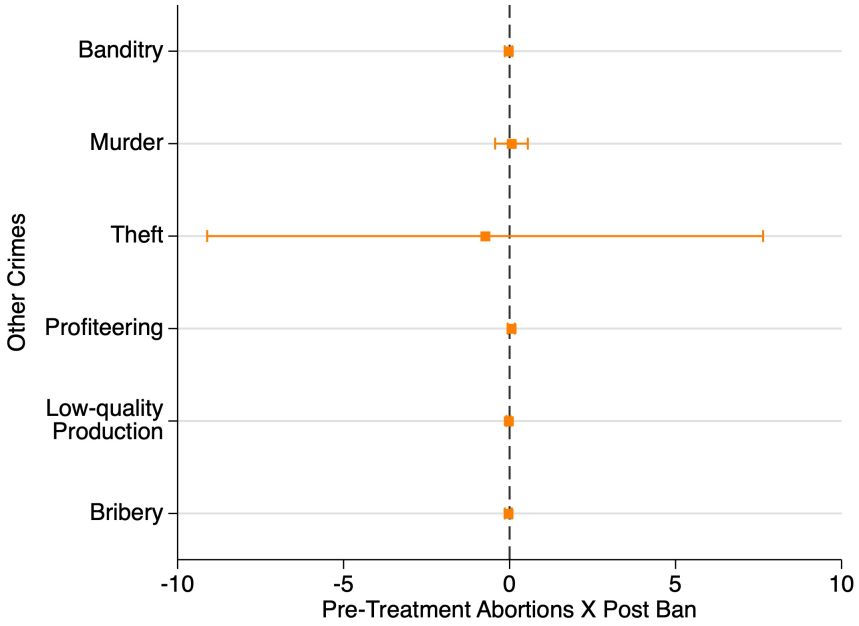


Panel B: Vandalism Committed by Individuals Aged 16–18



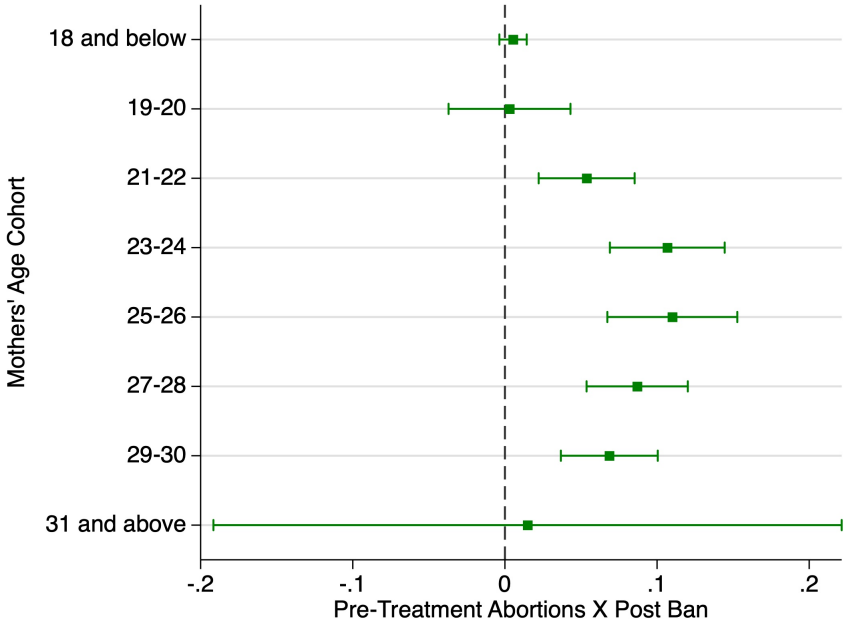
Note: The figure presents coefficients and 95% confidence intervals from regressions estimating the effects of the 1936 abortion ban on vandalism offenses. Panel A reports results for individuals aged 18 and above), and Panel B for individuals aged 16–18. Year-of-birth dummies are interacted with the Pre-Ban Abortions 1936 variable, defined as the number of abortions per woman in the first half of 1936. Estimates are plotted relative to cohorts born in 1951 (Panel A) and 1953 (Panel B), who were not exposed to the policy and serve as a natural comparison group. The x-axis indicates birth year, and the red vertical line marks the first cohort affected by the abortion ban implemented in July 1936. All regressions include region and year fixed effects. Corresponding static difference-in-differences estimates are reported in Table 2.

Figure 7: Impact of Abortions Ban on Other Crimes



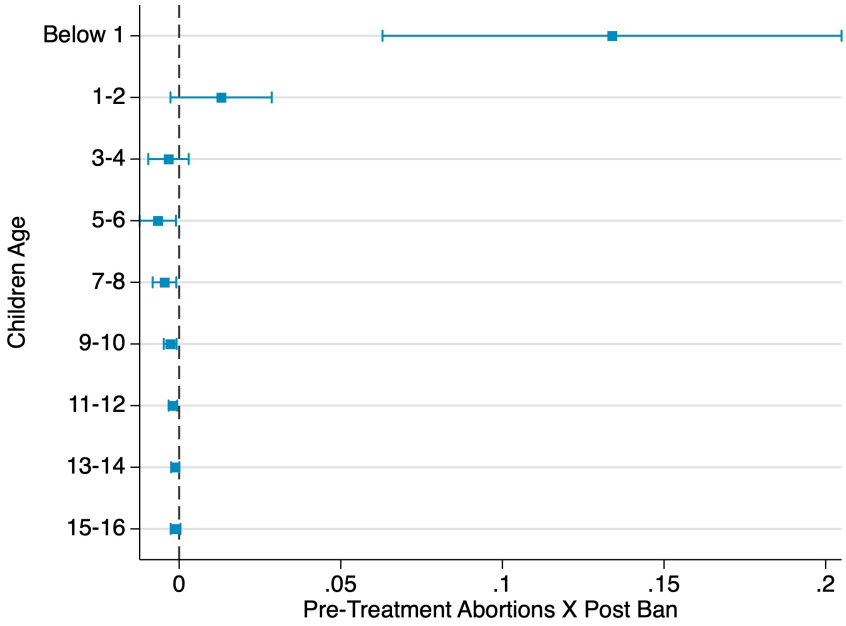
Note: The figure reports estimated coefficients and 95% confidence intervals from regressions examining the relationship between exposure to the 1936 abortion ban and subsequent involvement in other crimes. The key explanatory variable is the interaction between Pre-Ban Abortions 1936 (abortion rates per woman in the first half of 1936) and a Post-Ban dummy, equal to one for years in which affected cohorts reach adulthood. The policy is assumed to affect cohorts beginning in 1952, when those born after July 1936 ban enter adulthood. The x-axis shows the estimated interaction effects, and the y-axis lists different crime categories. These results describe associations between abortion ban exposure and criminal outcomes as defined by the Soviet criminal records.

Figure 8: Impact of the Abortion Ban on Birth Rates by Maternal Age



Note: The figure shows estimated coefficients and 95% confidence intervals from regressions examining the relationship between exposure to the abortion ban and birth rates across maternal age groups. The x-axis reflects the interaction between pre-ban abortion rates (measured in early 1936) and a post-ban period indicator. The y-axis groups mothers by age at childbirth.

Figure 9: Impact of the Abortion Ban on Child Deaths by Age Group



Note: The figure presents estimated coefficients and 95% confidence intervals from regressions examining the relationship between exposure to the abortion ban and child death rates across age cohorts. The x-axis reflects the interaction between pre-ban abortion rates (measured in early 1936) and a post-ban period indicator. The y-axis groups children by age at the time of death.

TABLE 1: Direct Impacts of the 1936 Abortion Ban on Births and Infant Death

Panel A: Abortion Ban and Births per Woman

	Births per Woman			
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	0.695***	0.673***	0.676***	0.532**
	(0.152)	(0.148)	(0.148)	(0.201)
Mean Dep. Var.	0.066	0.066	0.066	0.066
Mean Abortions per Woman	0.012	0.012	0.012	0.012
SD Abortions per Woman	0.012	0.012	0.012	0.012
Observations	460	460	460	460

Panel B: Abortion Ban and Infant Deaths per Woman

	Infant Deaths per Woman			
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	0.130***	0.126***	0.128***	0.083*
	(0.037)	(0.037)	(0.036)	(0.047)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Repressions per Capita	No	No	Yes	Yes
Radio Coverage	No	No	Yes	Yes
Pre-Ban Incomes × Year FE	No	No	No	Yes
Pre-Ban Wages × Year FE	No	No	No	Yes
Pre-Ban Hospitals × Year FE	No	No	No	Yes
Pre-Ban Gynecologist visits × Year FE	No	No	No	Yes
Pre-Ban Pediatric visits × Year FE	No	No	No	Yes
Pre-Ban Midwives × Year FE	No	No	No	Yes
Mean Dep. Var.	0.012	0.012	0.012	0.012
Mean Abortions per Woman	0.014	0.014	0.014	0.014
SD Abortions per Woman	0.013	0.013	0.013	0.013
Observations	418	418	418	418

Note: The table reports estimates from difference-in-differences regressions examining the association between exposure to the 1936 USSR abortion ban and demographic and child health outcomes. Panel A uses annual births per woman as the dependent variable; Panel B uses infant deaths per woman. The main explanatory variable is the interaction between Pre-Ban Abortions 1936 (measured as abortion rates per woman in the first half of 1936) and a Post-Ban dummy, equal to one for years following the policy change. All regressions include region fixed effects. Columns (2) through (4) sequentially add year fixed effects, time-varying controls (repressions per capita, radio coverage), and interactions between pre-ban regional characteristics (e.g., income, wages, healthcare demand and access) and year fixed effects. Standard errors are clustered at the regional level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 2: Indirect Impacts of the 1936 Abortion Ban on Illegal Abortions and Vandalism

Panel A: Illegal Abortions				
	Convicted for Illegal Abortion, per 1000 Women			
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	0.269*	0.271*	0.289*	0.315*
	(0.160)	(0.160)	(0.160)	(0.172)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Repressions per Capita	No	No	Yes	Yes
Radio Coverage	No	No	No	Yes
Mean Dep. Var.	0.048	0.048	0.048	0.048
Mean Abortions per Woman	0.027	0.027	0.027	0.027
SD Abortions per Woman	0.023	0.023	0.023	0.023
Observations	258	258	258	258
Panel B: Vandalism Cases per 1000 population				
	By Adults (treated at 1954)		By Adolescents (treated at 1952)	
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	2.665***	2.786***	0.361*	0.389**
	(0.834)	(0.858)	(0.179)	(0.170)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	No	Yes	No	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	0.260	0.260	0.040	0.040
Mean Abortions per Woman	0.027	0.027	0.028	0.028
SD Abortions per Woman	0.023	0.023	0.024	0.024
Observations	248	248	211	211

Note: The table presents estimates from difference-in-differences regressions examining patterns in illegal abortion convictions and vandalism offenses following the 1936 Soviet Union abortion ban. Panel A reports results for the number of women convicted for illegal abortions per 1,000 women. Panel B reports results for vandalism offenses per 1,000 population. In Panel B, Columns (1)–(2) correspond to adults (age 18 and above), and Columns (3)–(4) to adolescents (ages 16–18). The main regressor is an interaction between *Pre-Ban Abortions 1936*—the abortion rate per woman in the first half of 1936—and a *Post-Ban* dummy equal to one in years following the policyś implementation. All regressions include region fixed effects. In Panel A, year fixed effects are added in Column (2), controls for repressions per capita in Column (3), and radio coverage in Column (4). In Panel B, all specifications include both region and year fixed effects, with even-numbered columns additionally controlling for repressions per capita and radio coverage. Standard errors are clustered at the regional level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3: Impact of the 1936 Abortion Ban on Birth and Child Mortality by Cohort: Triple-Differences

Panel A: Impact on Births per Woman			
	Births, per Woman		
	(1)	(2)	(3)
Pre-Ban Abortions 1936 × Post-Ban × Reproductive Age	0.109**	0.099**	0.100**
	(0.047)	(0.041)	(0.041)
Mean Dep. Var.	0.008	0.008	0.008
Mean Abortions per Woman	0.013	0.013	0.013
SD Abortions per Woman	0.012	0.012	0.012
Observations	3582	3582	3582
Panel B: Impact on Child Deaths per Woman			
	Child Deaths, per Woman		
	(1)	(2)	(3)
Pre-Ban Abortions 1936 × Post-Ban × Infants	0.153***	0.137***	0.135***
	(0.039)	(0.034)	(0.034)
Region-Year Fixed Effects	Yes	Yes	Yes
Region-Age Group Fixed Effects	No	Yes	Yes
Year-Age Group Fixed Effects	No	No	Yes
Mean Dep. Var.	0.001	0.001	0.001
Mean Abortions per Woman	0.014	0.014	0.014
SD Abortions per Woman	0.013	0.013	0.013
Observations	7174	7174	7174

Note: The table reports estimates from difference-in-differences regressions examining the association between exposure to the 1936 USSR abortion ban and demographic and child health outcomes. The table reports estimates from triple-differences regressions evaluating the effects of the 1936 USSR abortion ban. In Panel A, the outcome is the number of births per woman, measured at the age cohort by region-year level. In Panel B, the outcome is the number of child deaths per woman, where child deaths are defined as deaths occurring before age 17. The main variable of interest is the interaction between *Pre-Ban Abortions 1936*—the abortion rate per woman in the first half of 1936—and indicators for the *Post-Ban* period and relevant cohort groups. The *Reproductive Age* dummy identifies cohorts under age 30, and the *Infants* dummy identifies cohorts under age one. All regressions include region-year fixed effects. Columns (2) and (3) additionally control for region-age cohort fixed effects, and Column (3) further includes year-age cohort fixed effects. Errors are clustered at the regional level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 4: Impacts of the Abortion Ban on Female Mortality by Cause of Death

Panel A: Abortion Related Causes				
	Deaths of Women 20-49 Years Old, per 1000 Women			
	Attempted Abortion	Premature Birth Miscarriage	Septic Diseases	Complications of Pregnancy
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	2.960***	0.463**	2.526*	3.147***
	(0.902)	(0.214)	(1.331)	(0.873)
Mean Dep. Var.	0.053	0.012	0.044	0.079
Mean Abortions per Woman	0.024	0.024	0.024	0.024
SD Abortions per Woman	0.009	0.009	0.009	0.009
Observations	135	135	135	135
Panel B: Other Causes				
	Scarlet Fever	Paratyphoid Fever	Typhus	Spinal Tuberculosis
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	1.081	0.055	-0.555	-0.679
	(2.140)	(0.215)	(1.635)	(0.664)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	Yes	Yes	Yes	Yes
Radio Coverage	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.107	0.005	0.042	0.016
Mean Abortions per Woman	0.024	0.024	0.024	0.024
SD Abortions per Woman	0.009	0.009	0.009	0.009
Observations	135	135	135	135

Note: The table reports estimates from difference-in-differences regressions evaluating the effects of the 1936 USSR abortion ban on female mortality by cause of death, measured per 1,000 women. Panel A presents estimates for causes plausibly linked to abortion. Column (1) includes deaths attributed to attempted abortion; Column (2) to premature birth and miscarriage; Column (3) to septic diseases; and Column (4) to other complications related to pregnancy. Panel B presents estimates for deaths from unrelated causes: Column (1) covers scarlet fever, Column (2) paratyphoid fever, Column (3) typhus, and Column (4) spinal tuberculosis. The key explanatory variable is the interaction between *Pre-Ban Abortions 1936*—the abortion rate per woman in the first half of 1936—and a *Post-Ban* indicator equal to one for years following the implementation of the abortion ban. All regressions include region and year fixed effects. Columns (1) through (4) also control for time-varying covariates, including repressions per capita and radio coverage. Standard errors are clustered at the regional level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 5: Impacts of the Abortion Ban on Infant Mortality by Cause of Death

Panel A: Abortion Related Causes				
	Infant Deaths, per 1000 Women			
	Congenital Weakness and Premature Birth		Congenital Heart Diseases	
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	18.199**	15.590**	1.074*	1.033*
	(7.974)	(7.556)	(0.584)	(0.591)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	No	Yes	No	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	1.045	1.045	0.012	0.012
Mean Abortions per Woman	0.024	0.024	0.024	0.024
SD Abortions per Woman	0.009	0.009	0.009	0.009
Observations	276	276	270	270
Panel B: Other Causes				
	Scarlet Fever	Paratyphoid Fever	Typhus	Spinal Tuberculosis
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	-1.084	-0.129	2.887	-1.746
	(1.350)	(0.104)	(3.292)	(1.171)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	Yes	Yes	Yes	Yes
Radio Coverage	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.016	0.000	0.227	0.115
Mean Abortions per Woman	0.024	0.024	0.024	0.024
SD Abortions per Woman	0.009	0.009	0.009	0.009
Observations	276	276	276	276

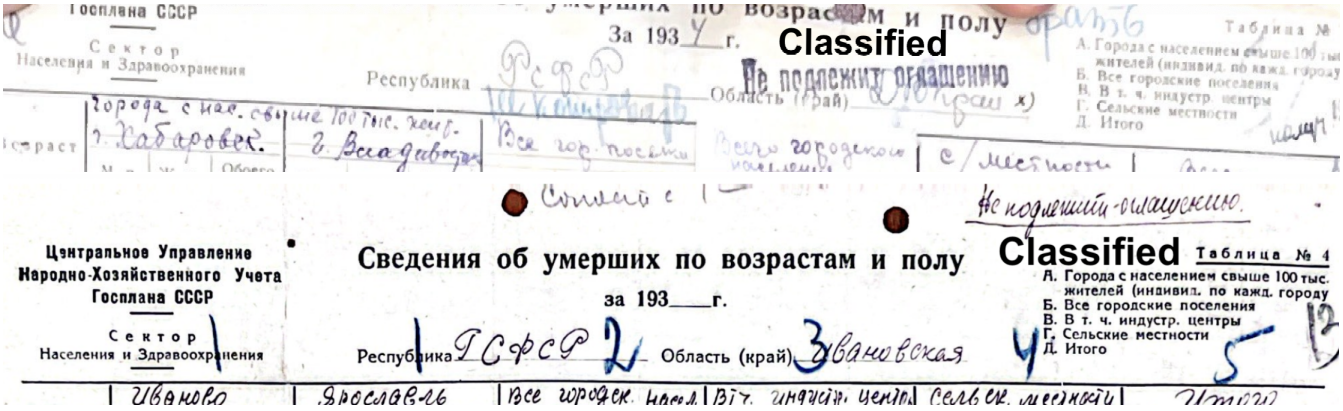
Note: The table reports estimates from difference-in-differences regressions examining the effects of the 1936 Soviet Union abortion ban on infant mortality by cause of death, measured per 1,000 women. Panel A focuses on causes plausibly linked to abortion. Columns (1) and (2) report deaths due to congenital weakness and premature birth; Columns (3) and (4) report deaths due to congenital heart conditions. Panel B presents estimates for causes unrelated to abortion: Column (1) includes scarlet fever, Column (2) paratyphoid fever, Column (3) typhus, and Column (4) spinal tuberculosis. The key explanatory variable is the interaction between *Pre-Ban Abortions 1936*—the abortion rate per woman in the first half of 1936—and a *Post-Ban* indicator equal to one for years following the implementation of the abortion ban. All regressions include region and year fixed effects. In Panel A, Columns (2) and (4) additionally control for repressions per capita and average radio coverage. To conserve space, all specifications in Panel B include the full set of fixed effects and controls. Standard errors are clustered at the regional level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A For Online Appendix

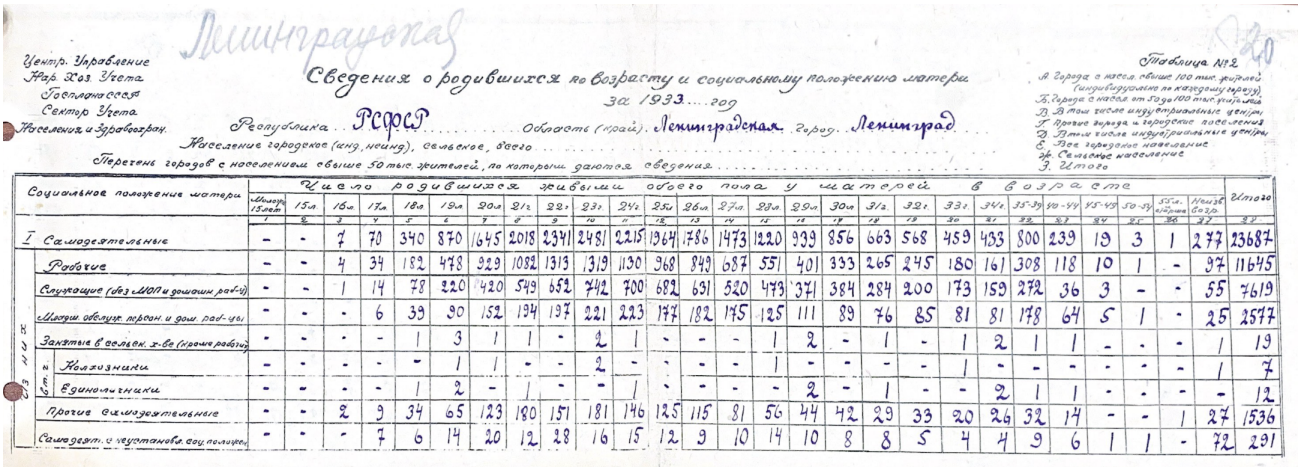
A.1 Raw Data and Additional Figures and Tables

Figure A1: Examples of Archival Documents

Panel A: Raw Data of Deaths by Age and Gender



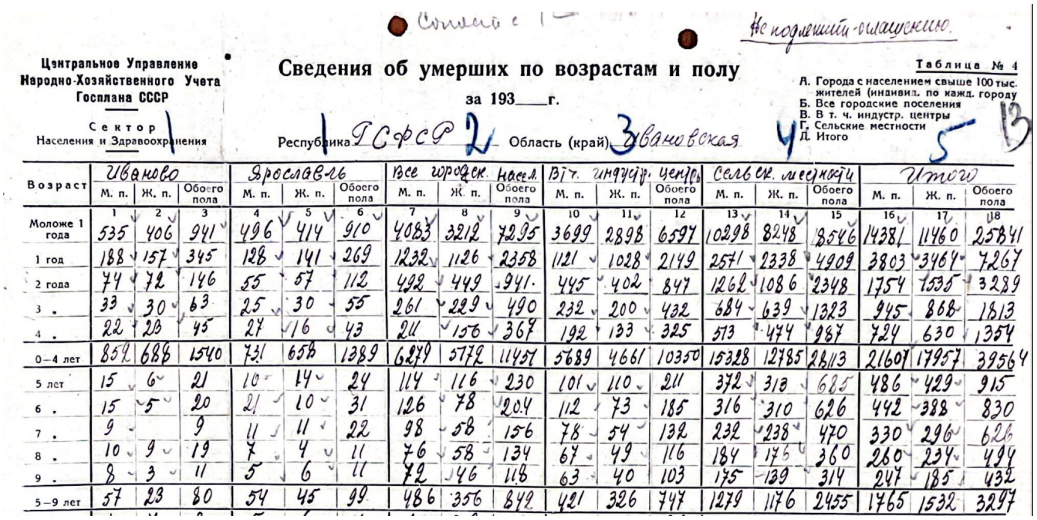
Panel B: Raw Data on Births



Note: The figure presents original archival documents collected from the State Archive of the Russian Federation (GARF). Panel A highlights classified documents with various categorization markers. Panel B provides birth statistics for Leningrad, where columns categorize the number of births by maternal age groups.

Figure A2: Examples of Archival Documents

Panel A: Raw Data on Deaths



Сдано с 1—

Не подлежит опубликованию.

Центральное Управление Народно-Хозяйственного Учета Госплана СССР

Сектор Населения и Здравоохранения

Сведения об умерших по возрастам и полу

за 193___г.

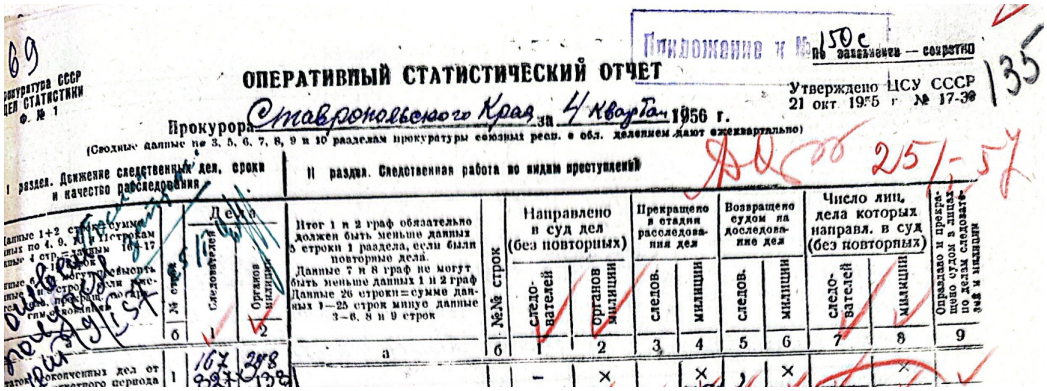
Республика РСФСР 2 Область (край) Ивановская 4 5 13

Таблица № 4

А. Города с населением свыше 100 тыс. жителей (индивид. по кажд. городу
Б. Все городские поселения
В. В т. ч. индустр. центры
Г. Сельские местности
Д. Итого

Возраст	Иваново			Ярославль			Все городск. насел.			В т. ч. индустр. центры			Сельск. местность			Итого		
	М. п.	Ж. п.	Обоего пола	М. п.	Ж. п.	Обоего пола	М. п.	Ж. п.	Обоего пола	М. п.	Ж. п.	Обоего пола	М. п.	Ж. п.	Обоего пола	М. п.	Ж. п.	Обоего пола
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
Моложе 1 года	535	406	941	496	414	910	4083	3212	7295	3699	2898	6597	10298	8248	18546	14381	11460	25841
1 год	188	157	345	126	141	269	1232	1126	2358	1121	1028	2149	2571	2338	4909	3803	3464	7267
2 года	74	72	146	55	57	112	492	449	941	445	402	847	1262	1086	2348	1754	1535	3289
3 .	33	30	63	25	30	55	261	229	490	232	200	432	684	639	1323	945	868	1813
4 .	22	20	45	27	16	43	211	156	367	192	133	325	513	474	987	724	630	1354
0—4 лет	852	686	1540	731	658	1389	6279	5772	11497	5689	4661	10350	15328	12785	28113	21607	17957	39564
5 лет	15	6	21	10	14	24	114	116	230	101	110	211	372	318	685	486	429	915
6 .	15	5	20	21	10	31	126	78	204	112	73	185	316	310	626	442	388	830
7 .	9		9	11	11	22	98	58	156	78	54	132	232	238	470	330	296	626
8 .	10	9	19	7	4	11	76	58	134	67	49	116	184	176	360	260	234	494
9 .	8	3	11	5	6	11	72	46	118	63	40	103	175	139	314	247	185	432
5—9 лет	57	23	80	54	45	99	486	356	842	421	326	747	1279	1176	2455	1765	1532	3297

Panel B: Raw Data on Crimes



Прокуратура СССР
ЦЕН СТАТИСТИКИ
ф. № 1

Приложение к № 150с
№ зарегистр. — секретно

ОПЕРАТИВНЫЙ СТАТИСТИЧЕСКИЙ ОТЧЕТ

Утверждено ЦСУ СССР
21 окт. 1955 г. № 17-30

Прокуратура Ставропольского Края за 4 кварт. 1956 г.

(Сводные данные по 3, 5, 6, 7, 8, 9 и 10 разделам прокуратуры союзных респ. и обл. делением дают ежеквартально)

I раздел. Движение следственных дел, сроки и качество расследования.

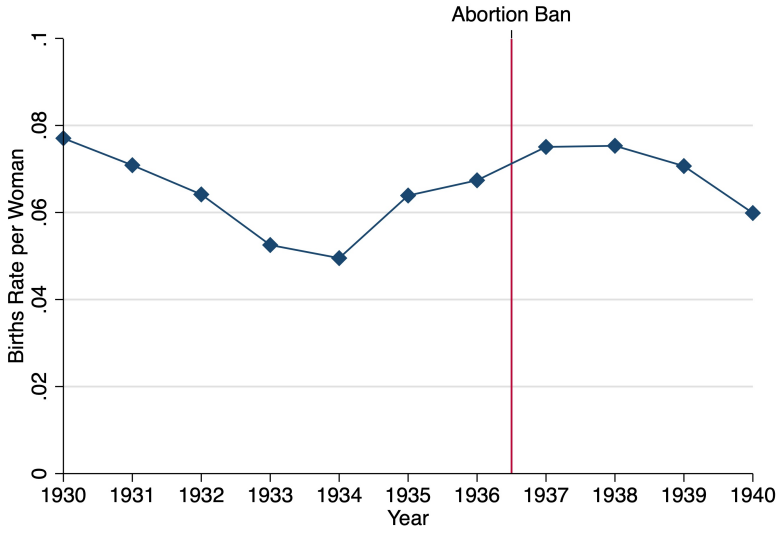
II раздел. Следственная работа по видам преступлений

Данные 1+2 строк сумме данных по 4, 9, 10 строкам	№ строк	Дела		Итог 1 и 2 граф обязательно должен быть меньше данных 5 строки 1 раздела, если были повторные дела. Данные 7 и 8 граф не могут быть меньше данных 1 и 2 граф. Данные 26 строки = сумме данных 1—25 строк минус данные 3—6, 8 и 9 строк	№ строк	Направлено в суд дел (без повторных)		Прекращено в стадии расследования дел		Возвращено судом на доследование дел		Число лиц, дела которых направл. в суд (без повторных)		
		Следственные	Органов милиции			Следователей	Органов милиции	следов.	милиции	следов.	милиции	следователей	милиции	Отправлено и прекращено судом в делах следователей и милиции
	1	2		а	б	1	2	3	4	5	6	7	8	9
Окончено дел от начала отчетного периода	1	167	288			—	×		×		×		×	

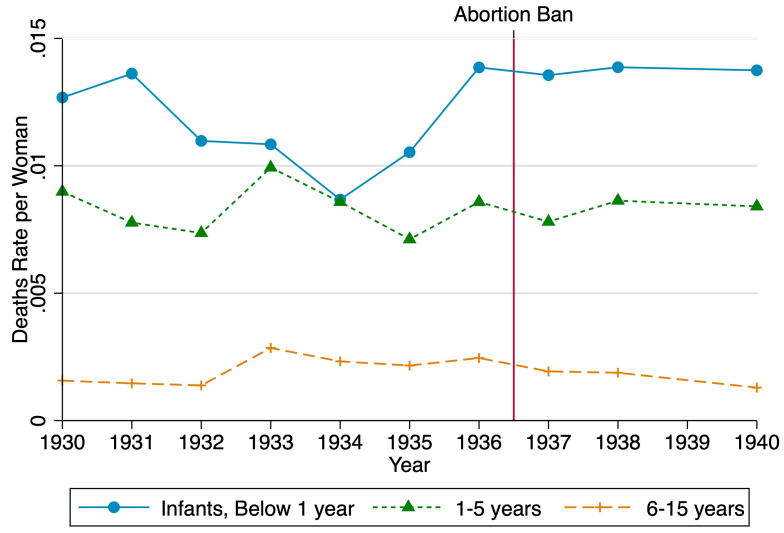
Note: The figure presents original archival documents collected from the State Archive of the Russian Federation (GARF). Panel A displays mortality tables for Ivanovskaya Oblast', with data disaggregated by urban, rural, and total population counts. The rows indicate death counts across different age groups. Panel B presents an example of crime data compiled by the Prosecutor's Office of the USSR.

Figure A3: Births per Woman and Child Deaths per Woman Over Time

Panel A: Births Per Woman

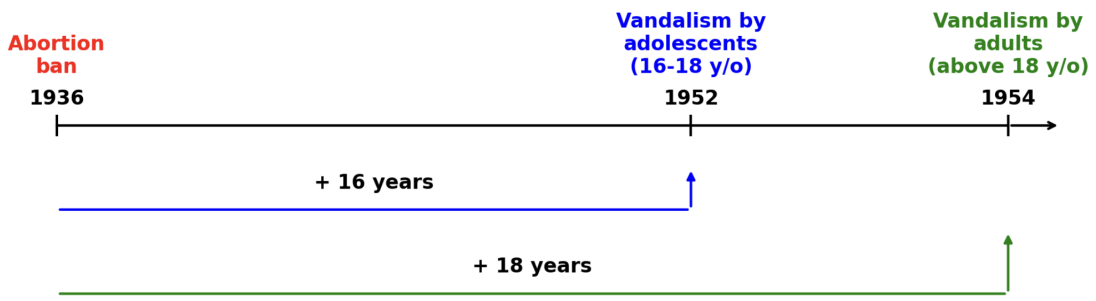


Panel B: Infant Deaths Per Woman



Note : The figure presents trends in births per woman and child mortality rates per woman over time. Panel A depicts the average number of births per woman across the entire sample period. Panel B presents child mortality rates per woman, segmented by age groups: below 1 year, 1–5 years, and 6–15 years. The blue shaded area represents the 1932–1933 Soviet famine, while the red vertical line marks the policy's implementation in July 1936

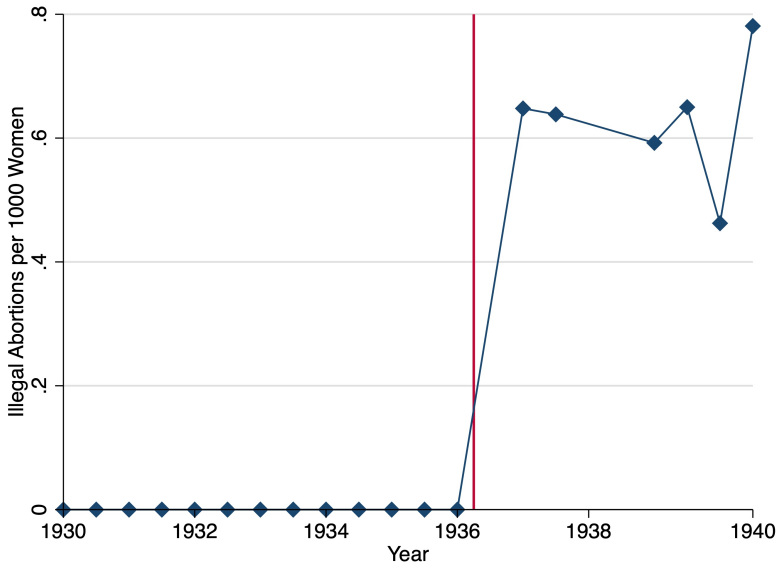
Figure A4: Timeline of Abortion Ban Exposure and Crime



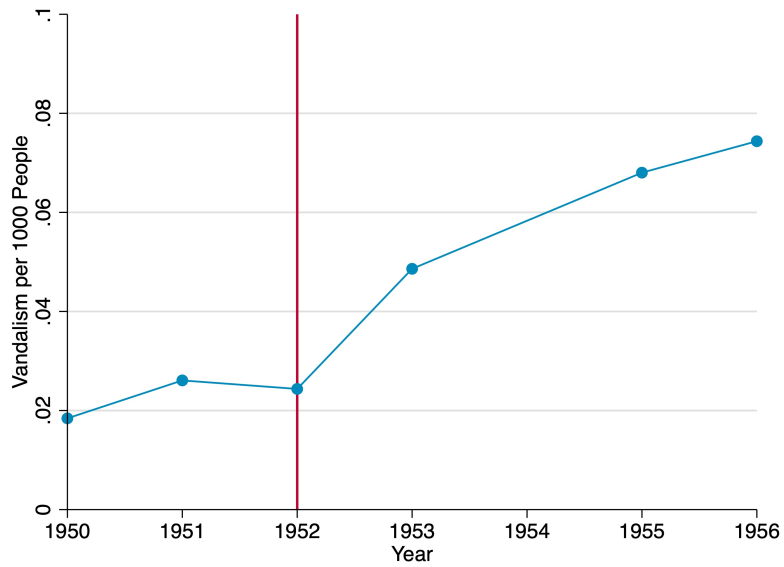
Note: The figure illustrates the time periods and treatment timing used for analyzing the impact of the 1936 abortion ban on vandalism among different age groups. Individuals born after the ban's implementation in 1936 were later exposed to treatment in 1952 and 1954. The adolescent group (ages 16-18) statistics include the treated cohort for the first time 16 years after the ban in 1952, while the adult group (ages 18 and above) statistics include the treated cohort for the first time 18 years after the ban in 1954. This timeline clarifies the staggered nature of treatment exposure used in the analysis.

Figure A5: Illegal Abortions and Vandalism Over Time

Panel A: Illegal Abortions per 1000 Women



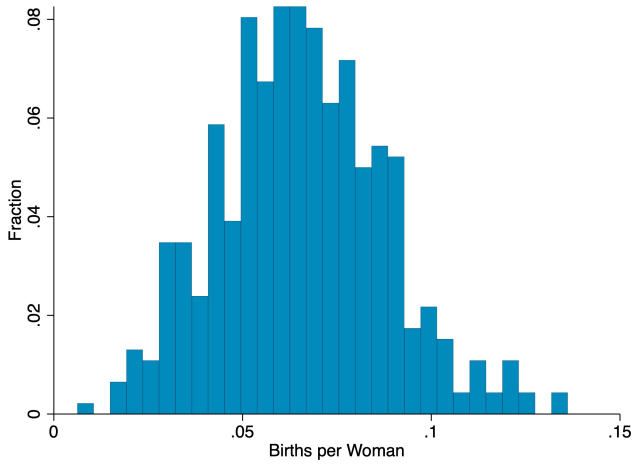
Panel B: Vandalism per 1000 Population



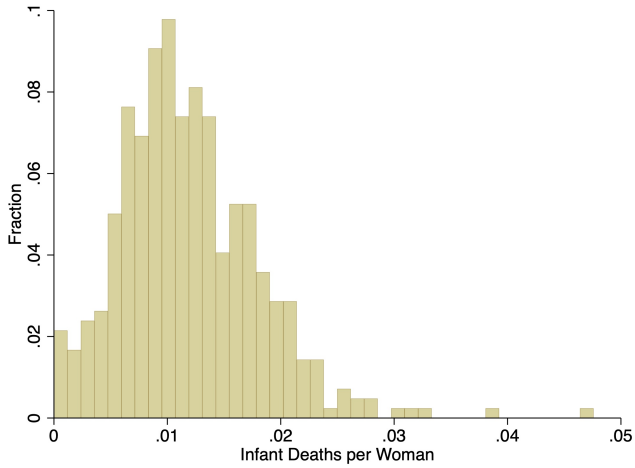
Note : The figure presents trends in illegal abortions and vandalism rates over time. Panel A depicts the average number of illegal abortions per 1,000 women after the 1936 ban on abortion. Panel B details depicts offenses per 1,000 population during the 1950s. The red vertical line in Panel A marks the implementation of the 1936 abortion ban, while in Panel B, it indicates the cohorts affected by the implementation of the abortion ban in mid-1936.

Figure A6: Distributions of Dependant Variables

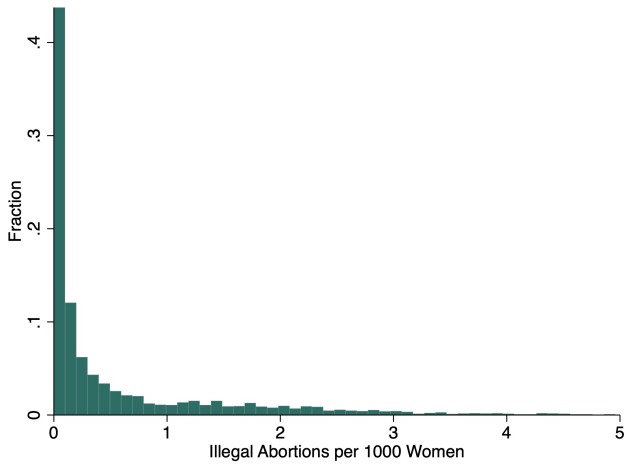
Panel A: Birth Rate



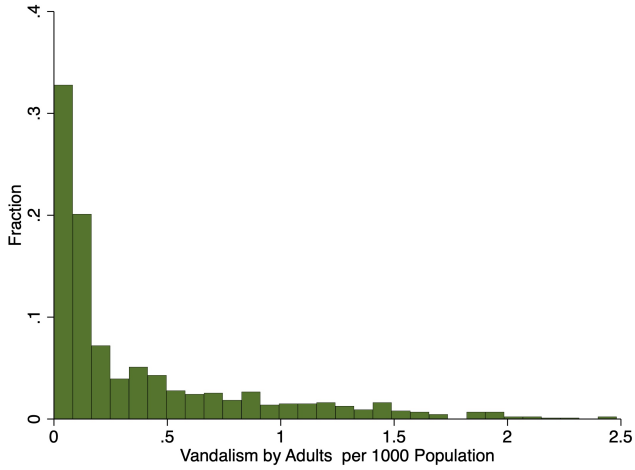
Panel B: Infant Deaths Rate



Panel C: Illegal Abortions



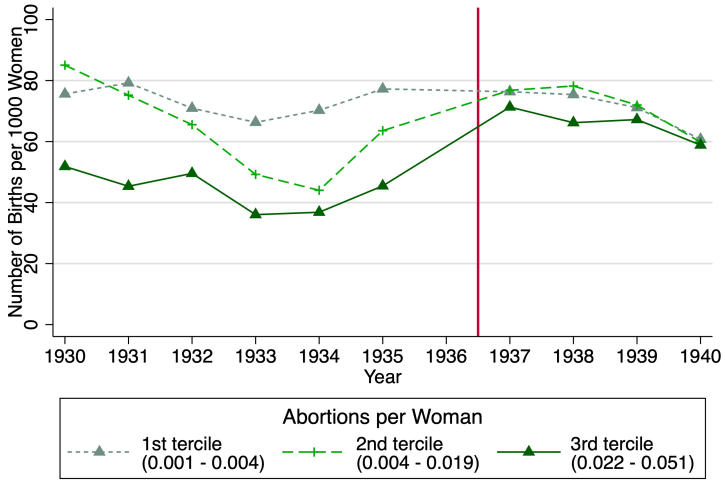
Panel D: Vandalism



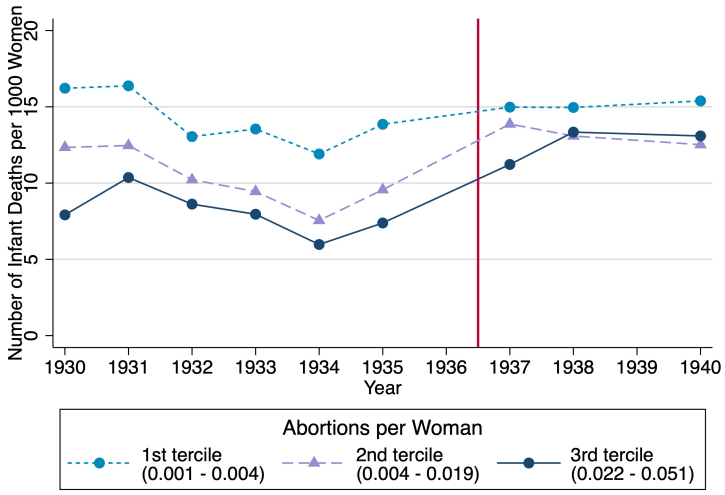
Note: The figure presents the distributions of key dependent variables analyzed in the study. Panel A shows the distribution of birth rates per woman, while Panel B illustrates the distribution of infant death rates per woman. Panel C captures the distribution of illegal abortions per 1,000 women, and Panel D presents the distribution of vandalism rates per 1,000 population. Each histogram represents the fraction of observations within each bin.

Figure A7: Births and Infant Deaths in the Three Terciles of Abortion per Woman

Panel A: Births per 1000 Women



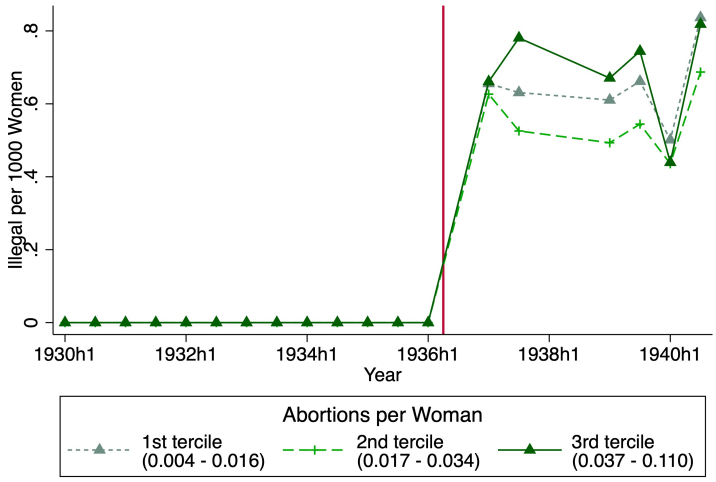
Panel B: Infant Deaths per 1000 Womn



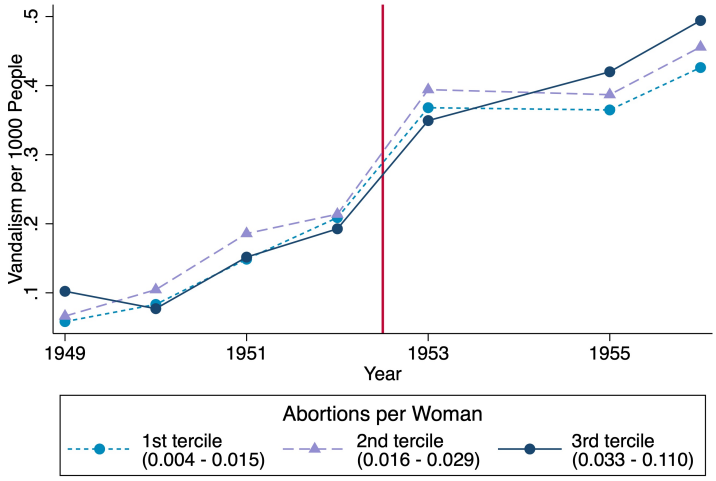
Note: The figure presents trends in births per 1000 women (Panel A) and infant deaths per 1000 women (Panel B) across three terciles of regions categorized by pre-ban abortion rates. The first tercile includes 19 regions with the lowest abortion rates (0.9–3.5 abortions per 1,000 women), the second tercile consists of 19 regions in the middle range (4.0–19.2 abortions per 1,000 women), and the third tercile comprises 18 provinces with the highest abortion rates (22.2–51.4 abortions per 1,000 women). The vertical line marks the implementation of the abortion ban in mid-1936.

Figure A8: Illegal Abortions and Vandalism in the Three Terciles of Abortion per Woman

Panel A: Illegal Abortions per 1000 Women

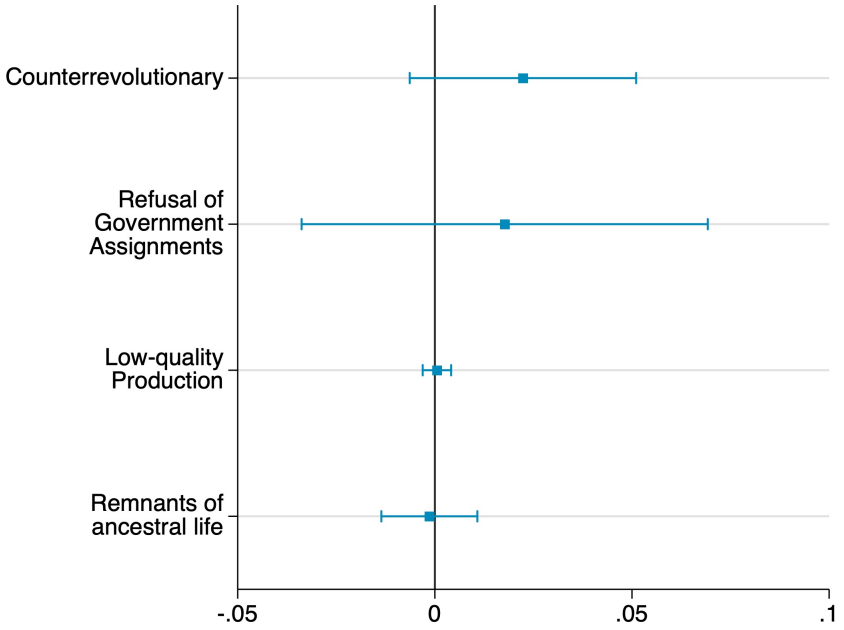


Panel B: Vandalism per 1000 People



Note: The figure presents trends in illegal abortions per 1,000 women (Panel A) and vandalism offenses per 1,000 people (Panel B) across three terciles of regions categorized by pre-ban abortion rates. The first tercile includes 13 regions with the lowest abortion rates (4–15 abortions per 1,000 women), the second tercile consists of 13 regions in the middle range (16.1–29.2 abortions per 1,000 women), and the third tercile comprises 12 provinces with the highest abortion rates (32.7–110.3 abortions per 1,000 women). The red vertical line in Panel A marks the implementation of the 1936 abortion ban, while in Panel B, it indicates the cohorts affected by the implementation of the abortion ban in mid-1936.

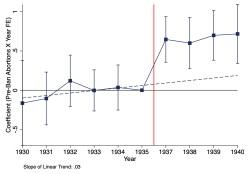
Figure A9: Correlation Between Criminal Abortion Cases and Other Crimes



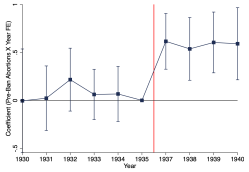
Note: The figure presents the estimated correlations between criminal abortion cases and various categories of reported crimes. The x-axis represents the correlation coefficient, while the y-axis lists different crime categories, including counterrevolutionary activities, refusal of government assignments, low-quality production, and remnants of ancestral life. Each estimate includes a 95% confidence interval.

Figure A10: Sensitivity to Parallel Trends Violations - Rambachan and Roth (2023)

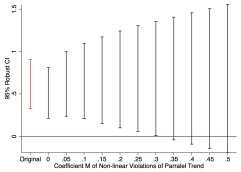
Panel A: Birth Rates



(a) Original Event-Study

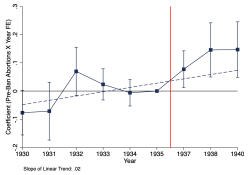


(b) Rotated Event-Study

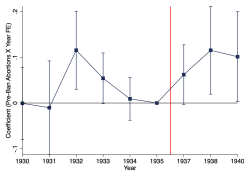


(c) Sensitivity

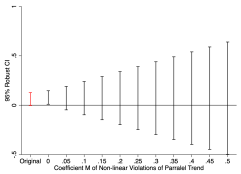
Panel B: Infant Death



(a) Original Event-Study

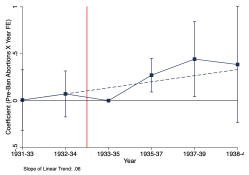


(b) Rotated Event-Study

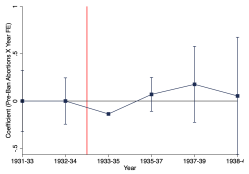


(c) Sensitivity

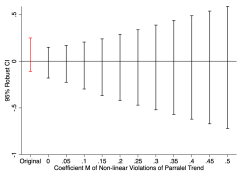
Panel C: Vandalism Adolescents



(a) Original Event-Study

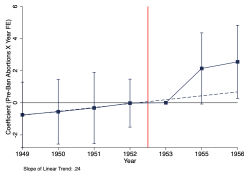


(b) Rotated Event-Study

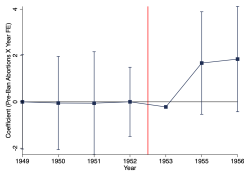


(c) Sensitivity

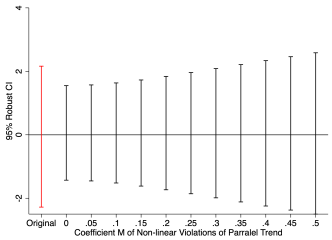
Panel D: Vandalism Adults



(a) Original Event-Study



(b) Rotated Event-Study

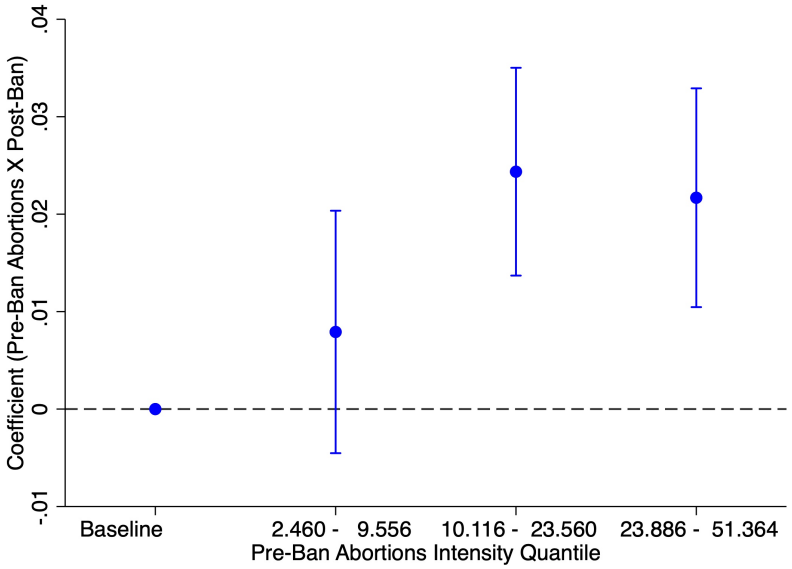


(c) Sensitivity

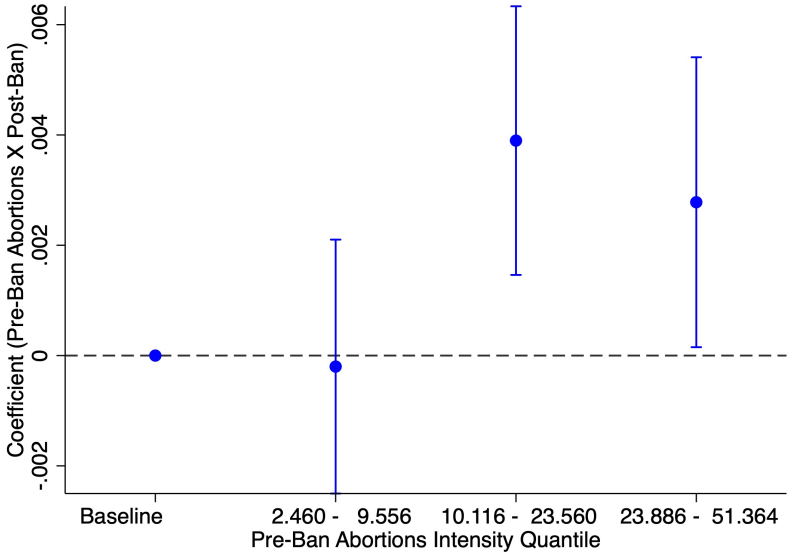
Note: The figure assesses the sensitivity of the event-study estimates to potential violations of the parallel trends assumption. In the left column (a), presents the event-study coefficients along with a linear trend estimated using pre-ban data, extrapolated to the post-ban period, column (b) shows the deviations of the coefficients from the linear time trend, column (c) reports the sensitivity to the linear extrapolation of the pre-ban coefficients using the honest approach to parallel trends by Rambachan and Roth (2023).

Figure A11: Robustness to Continuous Treatment Design - Callaway et al. (2024) Adjustment with Low-Exposure Comparison

Panel A: Births per Woman

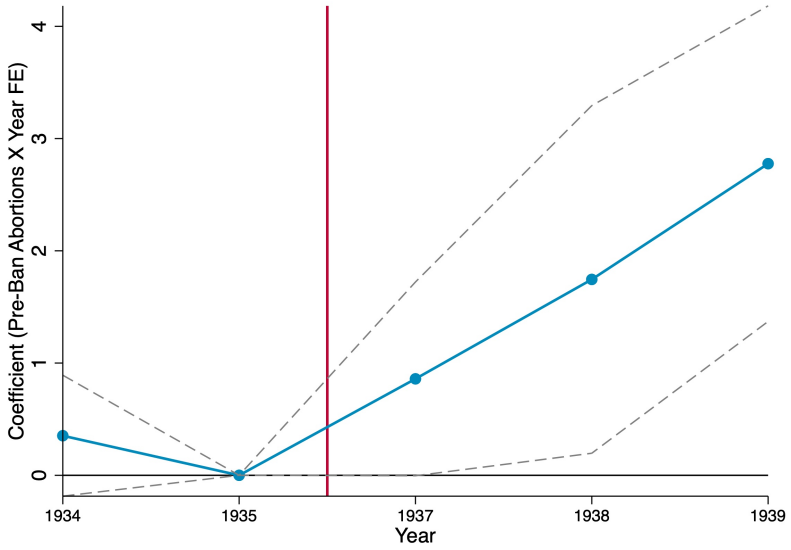


Panel B: Infant Deaths per Woman



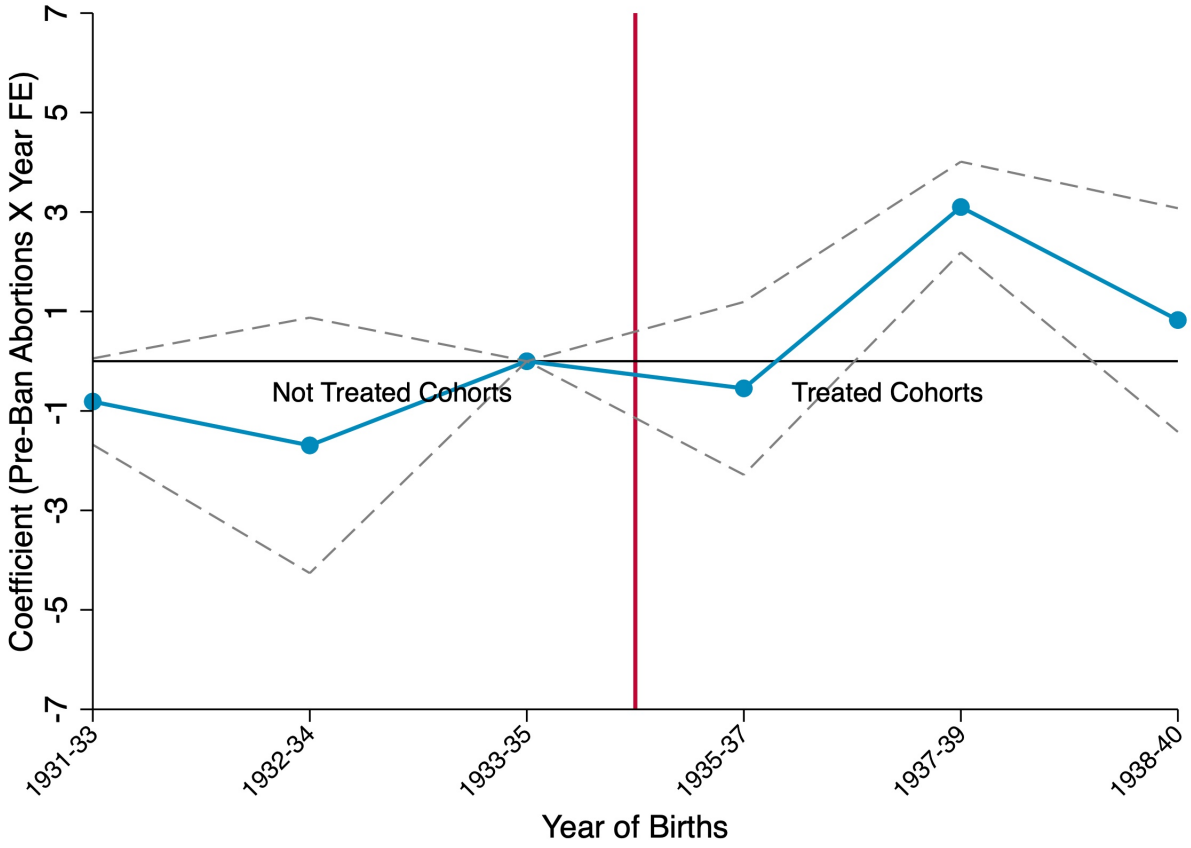
Note: The figure displays estimated effects of the 1936 Soviet abortion ban on births per woman (Panel A) and infant deaths per woman (Panel B), across regions grouped by pre-ban abortion intensity. Each coefficient corresponds to a separate intensity quantile, with the lowest-abortion regions serving as the reference group. The estimation follows the nonparametric approach proposed by Callaway et al. (2024), allowing for heterogeneous treatment effects by dosage. Pre-ban abortion intensity is measured as the rate of abortions per woman in the first half of 1936. Vertical lines represent 95% confidence intervals.

Figure A12: Time Varying Impact of Abortions Ban on Female Deaths Caused by Attempted Abortions (per 1000 women)



Note: The figure illustrates the coefficients and 95% confidence intervals from regressions estimating the effects of the 1936 abortion ban on deaths of women aged 20-49 (per 1000 women) due to attempted abortions. Year dummies are interacted with the *Pre-Ban Abortions 1936* variable, which represents the rate of abortions per woman in the first six months of 1936. The coefficient for 1935 is normalized to 0. Region and year fixed effects are included, with 1935 serving as the comparison group. The red vertical line marks the implementation of the abortion ban in mid-1936.

Figure A13: Impact on Vandalism - Adjusting for Cohort Size



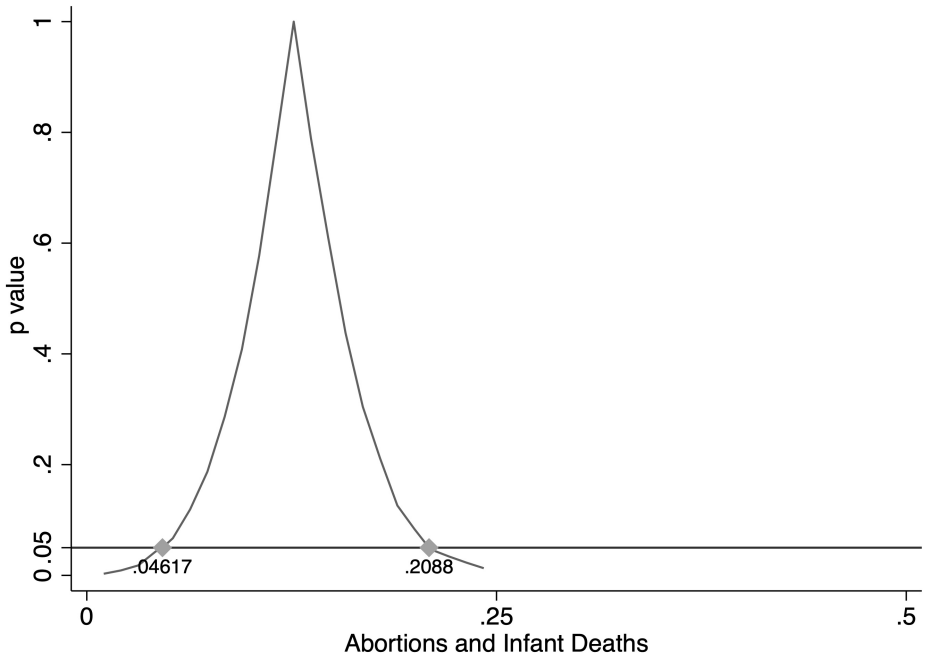
Note: The figure presents the coefficients and 95% confidence intervals from a set of difference-in-differences regressions estimating the impact of the 1936 USSR abortion ban on adolescent vandalism rates. Each point represents the interaction between the abortion rate per woman in the first half of 1936 (Pre-Ban Abortions 1936) and a year-of-birth dummy, capturing the differential effect of the ban by cohort. The vertical red line marks the first cohort fully exposed to the abortion ban (birth year 1936). Cohorts born before 1936 serve as the control group, while those born from 1936 onward are defined as treated. The outcome variable is the number of vandalism offenses committed by adolescents per 1,000 children born in each cohort. Quantitative results are summarized in Table A2.

Figure A14: Confidence Interval by Wild Bootstrap

Panel A: Births per Woman



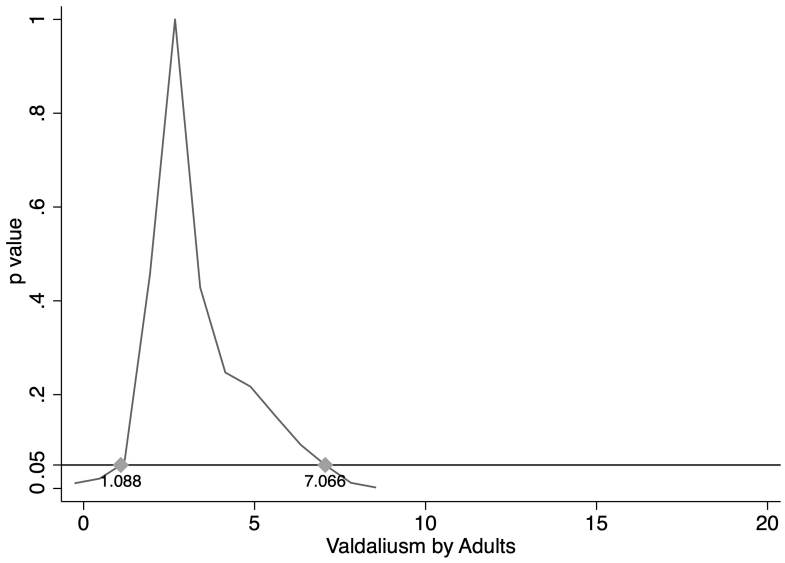
Panel B: Infant Deaths per Woman



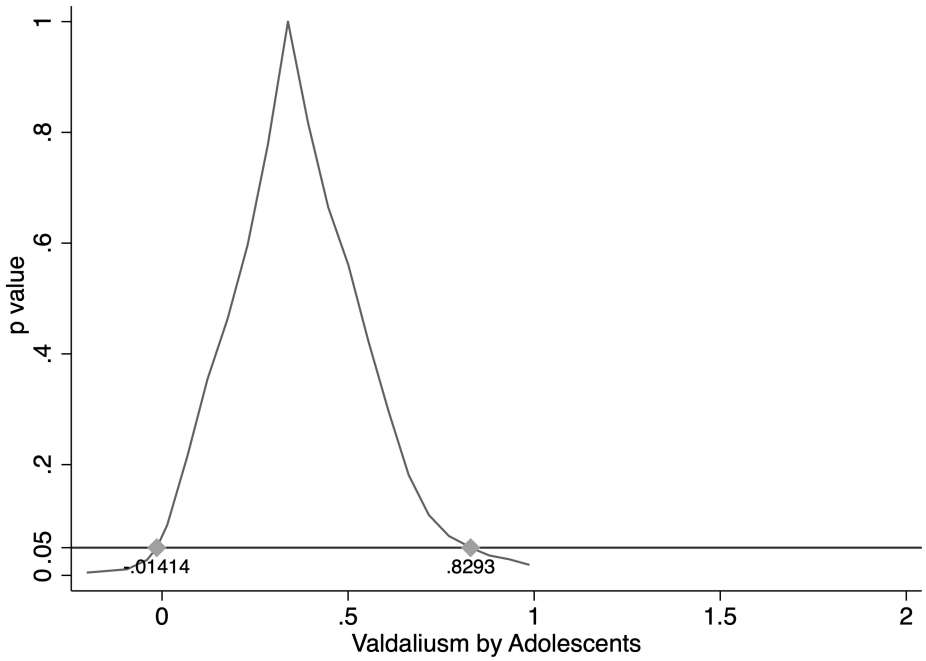
Note: The figure presents the estimated distribution curves for the interaction of pre-ban abortion rates with the post-ban dummy, using the wild bootstrap clustering method outlined by Cameron et al. (2008). Panel A illustrates the confidence interval (CI) for the estimated effect of the abortion ban on births per woman, while Panel B shows the CI for infant deaths per woman.

Figure A15: Confidence Interval by Wild Bootstrap

Panel A: Vandalism by Adults



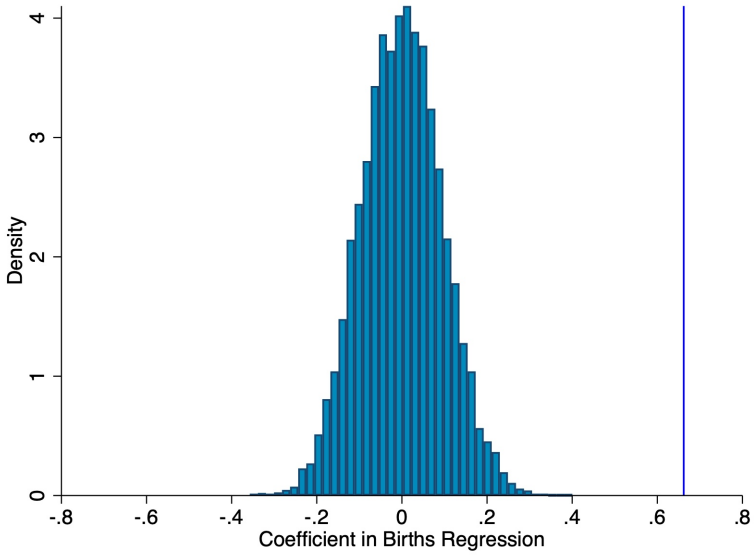
Panel B: Vandalism by Adolescents



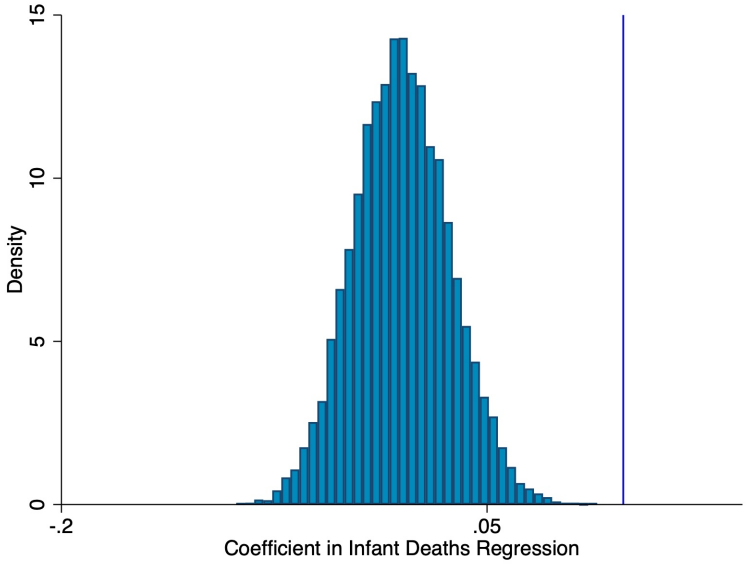
Note: The figure presents the estimated distribution curves for the interaction of pre-ban abortion rates with the post-ban dummy, using the wild bootstrap clustering method outlined by Cameron et al. (2008). Panel A illustrates the confidence interval (CI) for the estimated effect of the abortion ban on vandalism offenses committed by adults, while Panel B shows the CI for vandalism offenses committed by adolescents.

Figure A16: Permutation Inference Test Birth and Infant Death

Panel A: Births per Woman



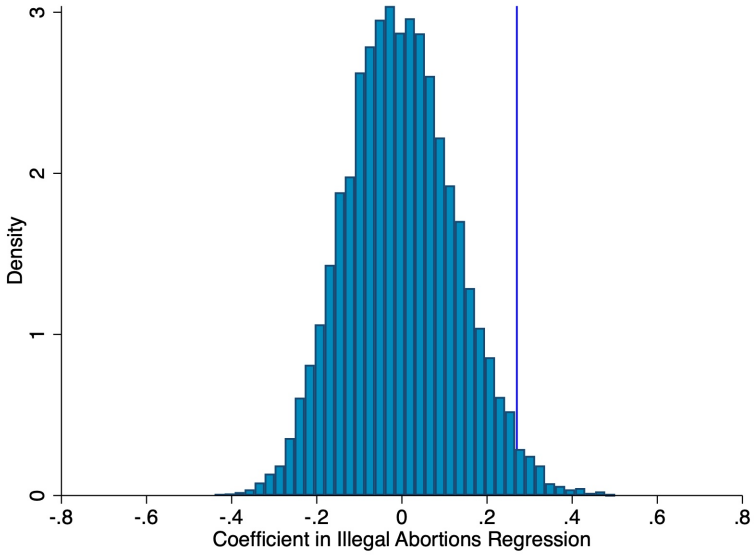
Panel A: Infant Death per Woman



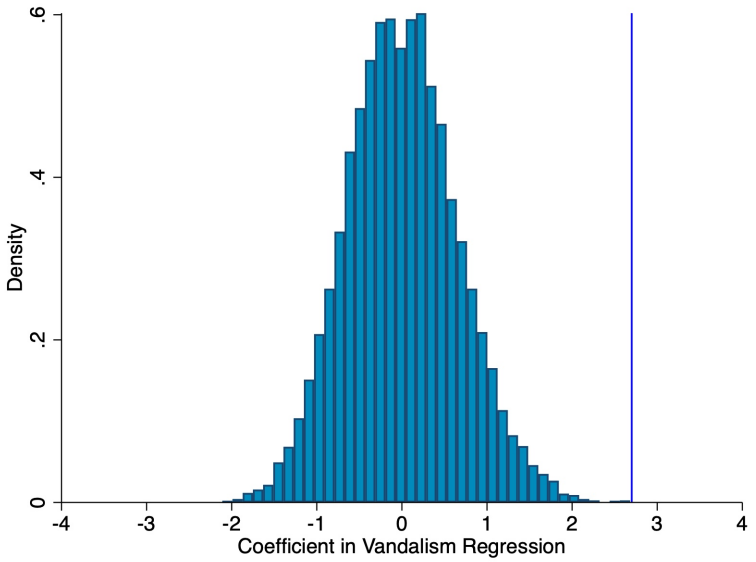
Note: The figure presents the results of a permutation inference test using 10,000 permutations to evaluate the robustness of the estimated effects of the abortion ban. Panel A shows the distribution of coefficient estimates for births per woman, while Panel B presents the distribution for infant deaths per woman. Each histogram represents the density of permutation estimates, with the vertical line indicating the actual estimated coefficient. The reported p-value for both coefficients is less than 0.001, suggesting statistical significance.

Figure A17: Permutation Inference Test Birth and Infant Death

Panel A: Illegal Abortions per 1000 Women



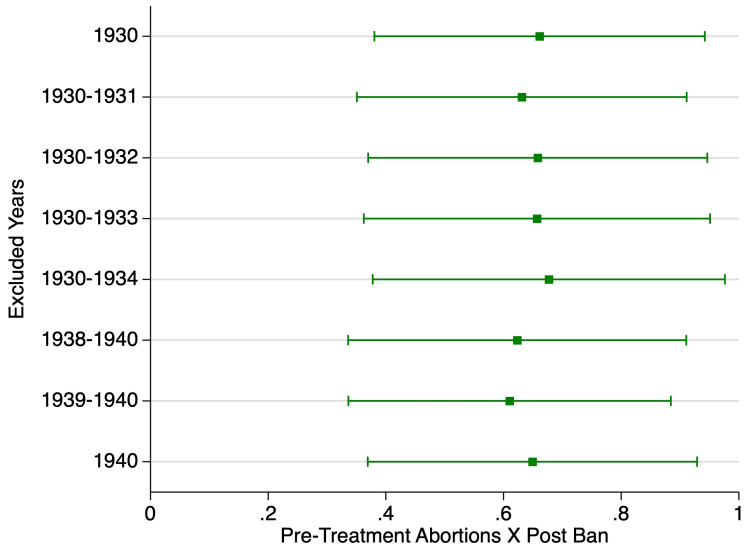
Panel B: Vandalism by Adults per 1000 Population



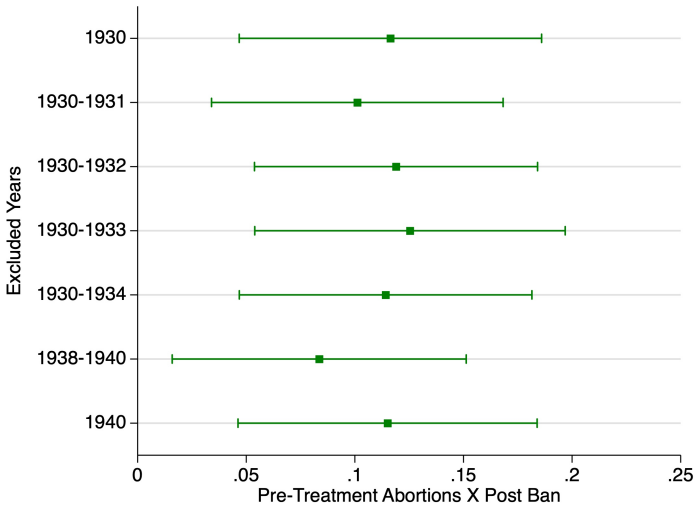
Note: The figure presents the results of a permutation inference test using 10,000 permutations to assess the robustness of the estimated effects of the abortion ban. Panel A displays the distribution of coefficient estimates for illegal abortions per 1,000 women, while Panel B shows the distribution for vandalism offenses per 1,000 population. Each histogram represents the density of permutation estimates, with the vertical line indicating the actual estimated coefficient. The reported p-value for both coefficients is less than 0.001, suggesting statistical significance.

Figure A18: Impact of Abortion Ban on Birth and Infant Deaths — Robustness to Time Period Choice

Panel A: Births per Woman



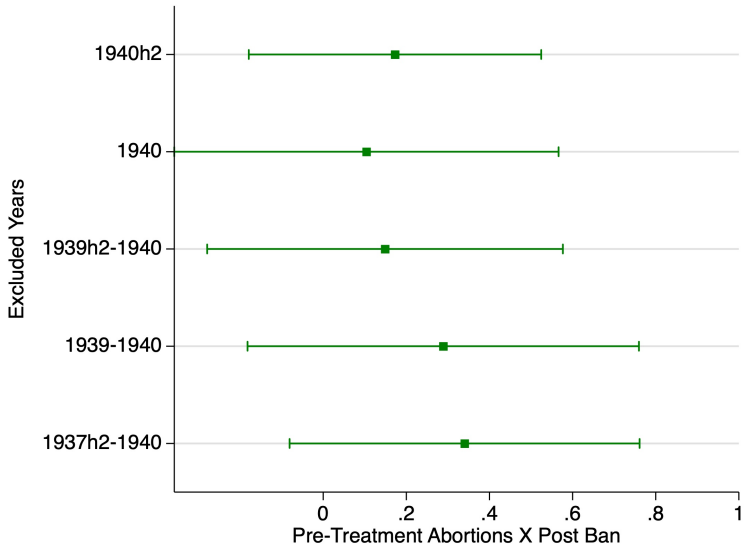
Panel B: Infant Deaths per Woman



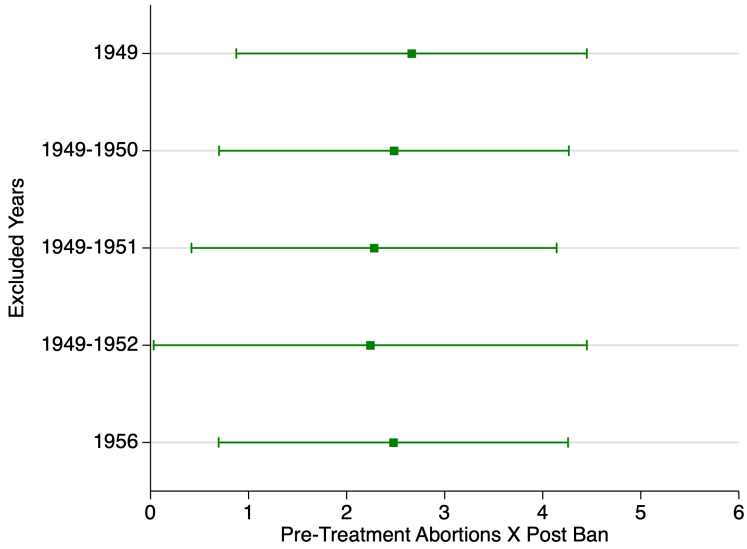
Note: his figure presents the estimated effects of the abortion ban on births per woman (Panel A) and infant deaths per woman (Panel B) across different time subsamples. Errors are clustered at the regional level. The dependent variables in Panel A and Panel B are the number of births and infant deaths per woman, respectively. The variable *Pre-Ban Abortions 1936* represents the number of abortions before the ban in the first half of 1936. The *Post Ban* dummy variable equals 1 from 1937 onward, indicating the full implementation of the abortion ban. Controls include the level of repressions and average radio penetration at the region-year level.

Figure A19: Impact of Abortion Ban on Illegal abortions and Vandalism — Robustness to Time Period Choice

Panel A: Illegal Abortions per 1000 Women



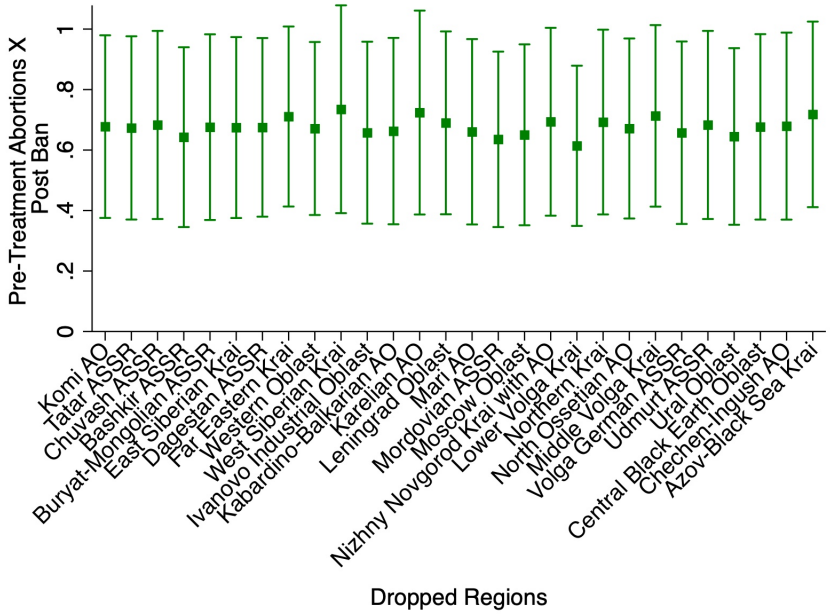
Panel B: Vandalism by Adults per 1000 Population



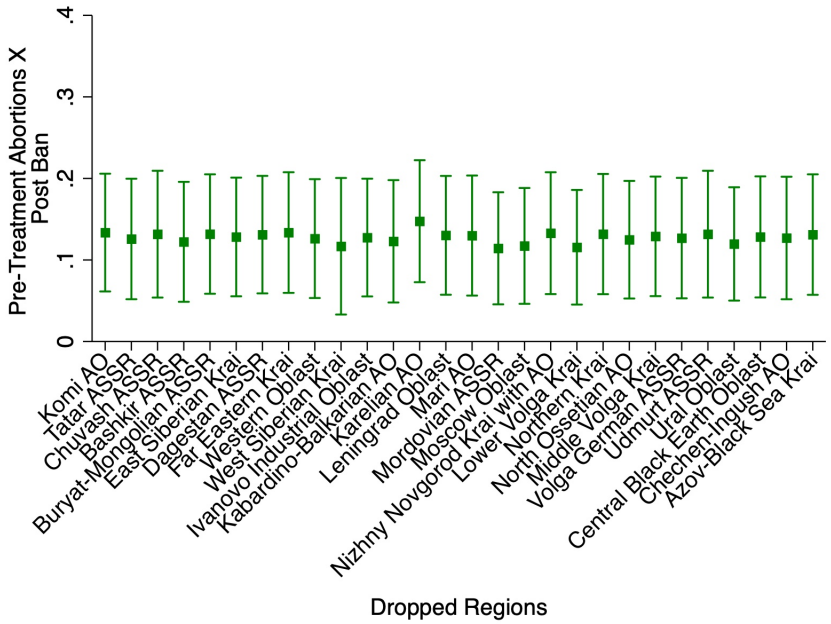
Note: The figure presents the estimated effects of the abortion ban on illegal abortions per 1,000 women (Panel A) and vandalism offenses per 1,000 population (Panel B) across different time subsamples. The variable *Pre-Ban Abortions 1936* represents the number of abortions before the ban in the first half of 1936. The *Post Ban* dummy variable equals 1 from 1937 onward. Controls include the level of repressions and average radio penetration at the region-year level.

Figure A20: Impact on Births and Infant Mortality by Sequentially Excluding Regions

Panel A: Births per Woman



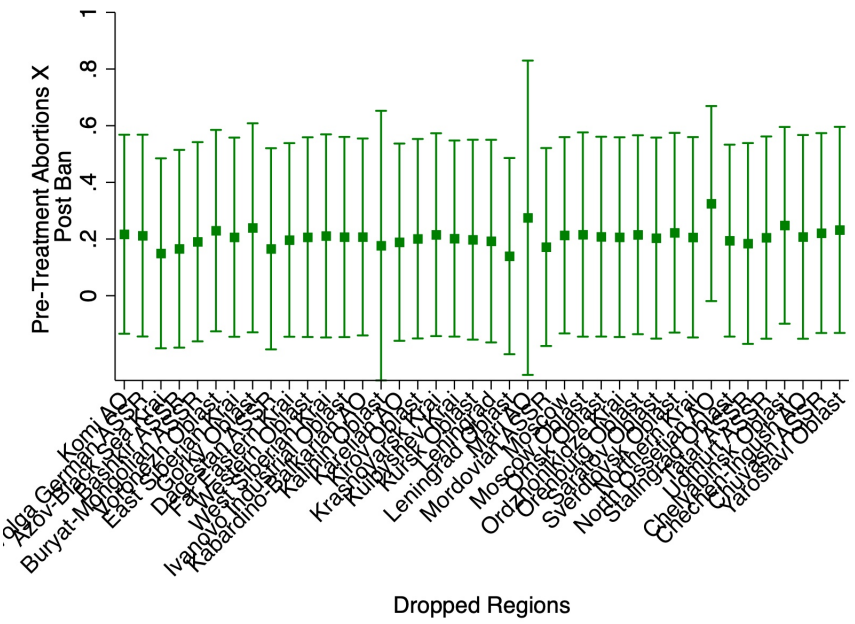
Panel B: Infant Deaths per Woman



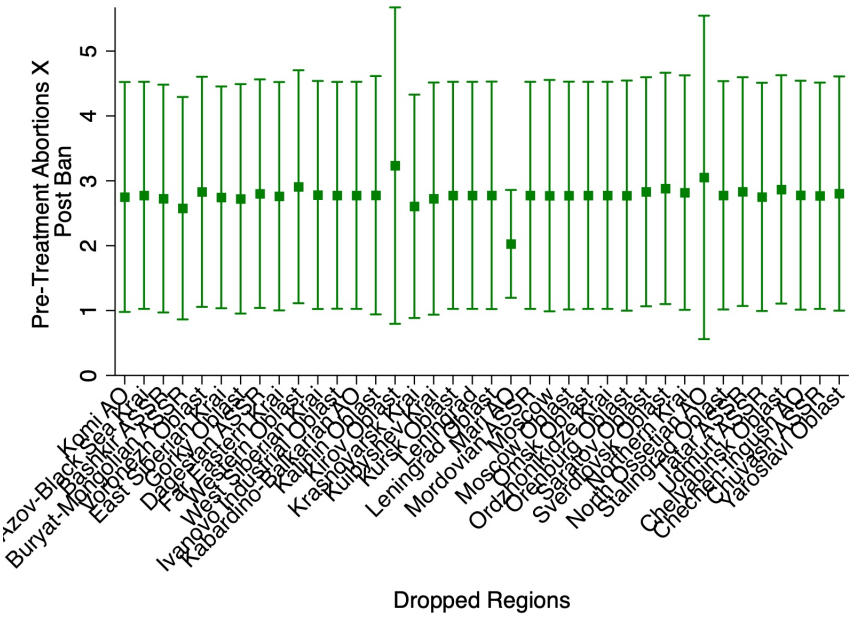
Note: The figure presents the estimated effects of the abortion ban on births per woman (Panel A) and infant deaths per woman (Panel B) while sequentially excluding individual regions to assess robustness. The variable *Pre-Ban Abortions 1936* represents the number of abortions before the ban in the first half of 1936. The *Post Ban* dummy variable equals 1 from 1937 onward. Controls include the level of repressions and average radio penetration at the region-year level.

Figure A21: Impact of Abortion Ban on Illegal abortions and Vandalism by Sequentially Excluding Regions

Panel A: Illegal Abortions per 1000 Women



Panel B: Vandalism by Adults per 1000 Population



Note: The figure presents the estimated effects of the abortion ban on illegal abortions per 1,000 women (Panel A) and vandalism offenses per 1,000 population (Panel B) while sequentially excluding individual regions to assess robustness. The variable *Pre-Ban Abortions 1936* represents the number of abortions before the ban in the first half of 1936. The *Post Ban* dummy variable equals 1 from 1937 onward. Controls include the level of repressions and average radio penetration at the region-year level.

TABLE A1: Summary statistics

	Mean	Median	SD	Min
Panel A: Dependent veriables (varies by year-region)				
Infant Deaths	11170.6	5791.5	14248.8	418
Infant Deaths, Boys	6126.7	3166.5	7809.5	418
Infant Deaths, Girls	5032.6	2584	6442.4	419
Births	64116.9	34402	77071.1	460
Children 1-5 y/o Deaths	7719.6	4416.5	9232.9	418
Criminal Abortions	39.54	18	58.27	256
Vandalism (16-18 y/o)	63.30	29.75	84.02	236
Vandalism (18 y/o)	424.2	267	457.9	249
Panel B: Population (varies by year-region)				
Population	1783462.4	970275.1	2018977.6	560
Male Population	838366.8	496682.1	932388.7	560
Female Population	945095.6	496413.7	1087776.7	560
Panel C: Pre-Ban Regions Characterisitcs (varies by region)				
Pre-Ban Abortions 1936	7834.3	3505.5	11994.8	78
Pre-Ban Midwifes 1936	8.291	8.550	2.984	74
Big Industry Labour Force	119000.3	17836.5	340888.2	58
Small Industry Labour Force	5004.0	3295.5	4894.8	58
Argiculture Labour Force	20386.9	519	37365.7	58
Big Industry Wage	210.0	198	51.14	58
Small Industry Wage	162.4	144.8	50.79	58
Argiculture Wage	140.3	136	27.50	58
Average Income	191.2	181.5	43.92	58
Number of Students at Schools	343896.2	224524	365084.6	48
Panel D: Time-Varying Control Variables (varies by region-year)				
Number of Repressions	3888.0	829.5	8708.2	280
Radio penetration	-156.4	-157.1	33.94	280

Note: The table presents descriptive statistics for the key variables used in the analysis, covering 56 regions across the USSR (modern-day Russia). Panel A reports summary statistics for dependent variables, including births, infant deaths, criminal abortions, and vandalism. Panel B presents population characteristics. Panel C provides pre-ban regional characteristics, including labor force composition, wages, and education levels. Panel D includes time-varying control variables, such as repressions and radio penetration. Data sources include records from the Ministry of Health, the Prosecutor General's Office, the Memorial Society, and radio journals.

TABLE A2: Crime Rate (Vandalism) Normalized by Cohort Size

	Vandalism Per 1000 children Born By Adolescents (treated at 1952)		
	(1)	(2)	(3)
Pre-Ban Abortions 1936 × Post-Ban	1.551**	1.588**	2.465**
	(0.598)	(0.644)	(0.890)
Region Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes
Repressions per Capita	No	No	Yes
Radio Coverage	No	No	Yes
Mean Dep. Var.	0.336	0.336	0.336
Mean Abortions per Woman	0.047	0.047	0.047
SD Abortions per Woman	0.042	0.042	0.042
Observations	110	110	110

Note: The table presents the results of difference-in-differences regressions estimating the effect of the 1936 USSR abortion ban on adolescent vandalism rates. The outcome variable is the number of vandalism offenses per 1,000 children born in each cohort. The key explanatory variable is the interaction term *Pre-Ban Abortions 1936* × *Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post-Ban* is a dummy variable that equals 1 for years affected by the abortion ban. All specifications include region fixed effects. Columns (2) and (3) additionally include year fixed effects. Column (3) further controls for time-varying covariates, including repressions per capita and radio coverage. Standard errors are clustered at the regional level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A3: Indirect Effects of the 1936 Abortion Ban on Crime Outcomes: Robustness to Additional Healthcare Controls

Panel A: Illegal Abortions per 1000 Women				
	Illegel Abortions			
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	0.508**	0.558**	0.566**	0.578**
	(0.199)	(0.229)	(0.221)	(0.246)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	Yes	Yes	Yes	Yes
Radio Coverage	Yes	Yes	Yes	Yes
Gynecologist visits × Year FE	Yes	Yes	Yes	Yes
Pediatric visits × Year FE	No	Yes	Yes	Yes
Midwifes and Paramedics × Year FE	No	No	Yes	Yes
Hospitals × Year FE	No	No	No	Yes
Mean Dep. Var.	0.047	0.047	0.047	0.047
Observations	256	256	256	256
Panel B: Vandalism Cases per 1000 Population				
	By Adults (treated at 1954)		By Adolescents (treated at 1952)	
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	3.011**	2.908**	0.283	0.225
	(1.305)	(1.144)	(0.246)	(0.251)
Region Fixed Effects	Yes	Yes	No	No
Year Fixed Effects	Yes	Yes	No	No
Repressions per Capita	Yes	Yes	Yes	Yes
Radio Coverage	Yes	Yes	Yes	Yes
Gynecologist visits × Year FE	Yes	Yes	Yes	Yes
Pediatric visits × Year FE	Yes	Yes	Yes	Yes
Midwifes and Paramedics × Year FE	Yes	Yes	Yes	Yes
Hospitals × Year FE	No	Yes	No	Yes
Mean Dep. Var.	0.256	0.256	0.039	0.039
Observations	223	223	190	190

Note:The table presents the results of difference-in-differences regressions estimating the effects of the 1936 USSR abortion ban, incorporating additional healthcare controls. The dependent variable in Panel A is the number of women convicted for illegal abortions per 1,000 women. Panel B examines vandalism offenses per 1,000 population, with Columns (1-2) focusing on cases committed by adults (treated by the 1954 abortion ban) and Columns (3-4) on cases committed by adolescents (treated by the 1952 abortion ban). The key explanatory variable is the interaction term *Pre-Ban Abortions 1936 × Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post Ban* is a dummy variable that equals 1 for years following the implementation of the abortion ban in 1936. All regressions include region and year fixed effects and control for time-varying covariates, including repressions per capita and radio coverage. In Panel A, Columns (1-4) additionally control for gynecologist visits, Columns (2-4) add pediatric visits, Columns (3-4) add midwives and paramedics, and Column (4) also includes hospitals, all calculated per 1000 women and interacted with year fixed effects. In Panel B Columns (1-4) control for gynecologist visits, Columns (2-4) add pediatric visits, Columns (3-4) add midwives and paramedics, and Column (4) add hospitals, all interacted with year fixed effects. Standard errors are clustered at the regional level * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A4: Direct Effects of the 1936 Abortion Ban on Birth and Infant Death Rates: Robustness to Conley Spatial Correlation

Panel A: Radius 500 km	Birth per woman			Infant death per woman		
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Ban Abortions 1936 × Post-Ban	0.695***	0.673***	0.676***	0.130***	0.126***	0.128***
	(0.157)	(0.156)	(0.156)	(0.041)	(0.041)	(0.040)
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	No	Yes	Yes
Repressions per Capita	No	No	Yes	No	No	Yes
Radio Coverage	No	No	Yes	No	No	Yes
Mean Dep. Var.	0.066	0.066	0.066	0.012	0.012	0.012
Observations	460	460	460	418	418	418
Panel B. Radius 1000 km						
Pre-Ban Abortions 1936 × Post-Ban	0.695***	0.673***	0.676***	0.130***	0.126***	0.128***
	(0.158)	(0.157)	(0.158)	(0.042)	(0.041)	(0.041)
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	No	Yes	Yes
Repressions per Capita	No	No	Yes	No	No	Yes
Radio Coverage	No	No	Yes	No	No	Yes
Mean Dep. Var.	0.066	0.066	0.066	0.012	0.012	0.012
Observations	460	460	460	418	418	418
Panel C. Radius 5000 km						
Pre-Ban Abortions 1936 × Post-Ban	0.695***	0.673***	0.676***	0.130***	0.126***	0.128***
	(0.137)	(0.137)	(0.138)	(0.042)	(0.041)	(0.040)
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	No	Yes	Yes
Repressions per Capita	No	No	Yes	No	No	Yes
Radio Coverage	No	No	Yes	No	No	Yes
Mean Dep. Var.	0.066	0.066	0.066	0.012	0.012	0.012
Observations	460	460	460	418	418	418

The table examines the robustness of the estimated effects of the 1936 USSR abortion ban to accounting for (Conley, 1999) spatial correlation, with births per woman and infant deaths per woman as the dependent variables in Columns (1-3) and (4-6), respectively. The key explanatory variable is the interaction term *Pre-Ban Abortions 1936* × *Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post-Ban* is a dummy variable that equals 1 for years following the 1936 abortion ban. All regressions include region fixed effects. Columns (2-3) and (5-6) add year fixed effects. Columns (3) and (6) further control for repressions per capita and radio coverage. Standard errors are clustered at the regional level. Standard errors also account for spatial correlation following (Conley, 1999), assuming that the correlation between the error terms of two observations is zero beyond 500 km, 1000 km, and 5000 km in Panels A, B, and C, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A5: Indirect Effects of the 1936 Abortion Ban on Illegal Abortions: Robustness to Conley Spatial Correlation

Panel A: Radius 500 km	Illegal abortions		
	(1)	(2)	(3)
Pre-Ban Abortions 1936 × Post-Ban	0.269**	0.271**	0.315**
	(0.128)	(0.126)	(0.132)
Region Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes
Repressions per Capita	No	No	Yes
Radio Coverage	No	No	Yes
Mean Dep. Var.	0.048	0.048	0.048
Observations	258	258	258
Panel B. Radius 1000 km			
Pre-Ban Abortions 1936 × Post-Ban	0.269***	0.271***	0.315***
	(0.086)	(0.097)	(0.106)
Region Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes
Repressions per Capita	No	No	Yes
Radio Coverage	No	No	Yes
Mean Dep. Var.	0.048	0.048	0.048
Observations	258	258	258
Panel C. Radius 5000 km			
Pre-Ban Abortions 1936 × Post-Ban	0.269***	0.271**	0.315***
	(0.091)	(0.105)	(0.114)
Region Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes
Repressions per Capita	No	No	Yes
Radio Coverage	No	No	Yes
Mean Dep. Var.	0.048	0.048	0.048
Observations	258	258	258

The table examines the robustness of the estimated effects of the 1936 USSR abortion ban to accounting for (Conley, 1999) spatial correlation, with the number of women convicted for illegal abortions per 1,000 women as the dependent variable. The key explanatory variable is the interaction term *Pre-Ban Abortions 1936* × *Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post-Ban* is a dummy variable that equals 1 for years following the 1936 abortion ban. All regressions include region fixed effects. Columns (2-3) add year fixed effects, and Column (3) further controls for repressions per capita and radio coverage. Standard errors are clustered at the regional level. Standard errors also account for spatial correlation following (Conley, 1999), assuming that the correlation between the error terms of two observations is zero beyond 500 km, 1000 km, and 5000 km in Panels A, B, and C, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A6: Indirect Effects of the 1936 Abortion Ban on Vandalism: Robustness to Conley Spatial Correlation

Panel A: Radius 500	Vandalism by Adults (treated at 1954)		Vandalism by Adolescents (treated at 1952)	
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	2.665***	2.862***	0.361**	0.394**
	(0.810)	(0.797)	(0.165)	(0.155)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	No	Yes	No	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	0.260	0.260	0.040	0.040
Observations	248	248	211	211
Panel B. Radius 1000km				
Pre-Ban Abortions 1936 × Post-Ban	2.665***	2.862***	0.361**	0.394**
	(0.835)	(0.833)	(0.162)	(0.155)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	No	Yes	No	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	0.260	0.260	0.040	0.040
Observations	248	248	211	211
Panel C. Radius 5000km				
Pre-Ban Abortions 1936 × Post-Ban	2.665***	2.862***	0.361**	0.394**
	(0.669)	(0.696)	(0.171)	(0.166)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	No	Yes	No	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	0.260	0.260	0.040	0.040
Observations	248	248	211	211

The table examines the robustness of the estimated effects of the 1936 USSR abortion ban to accounting for (Conley, 1999) spatial correlation, with vandalism offenses per 1,000 population as the dependent variable. Columns (1-2) report offenses committed by adults (treated at 1954), while Columns (3-4) report offenses committed by adolescents (treated at 1952). The key explanatory variable is the interaction term *Pre-Ban Abortions 1936* × *Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post-Ban* is a dummy variable that equals 1 for years affected by the abortion ban. The implementation of the abortion ban is in 1952 (Columns 1-2) and 1954 (Columns 3-4), reflecting the cohorts affected by the implementation of the abortion ban in July 1936. All regressions include region and year fixed effects. Columns (2) and (4) additionally control for repressions per capita and radio coverage. Standard errors are clustered at the regional level. Standard errors also account for spatial correlation following (Conley, 1999), assuming that the correlation between the error terms of two observations is zero beyond 500 km, 1000 km, and 5000 km in Panels A, B, and C, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A7: Effect of the 1936 Abortion Ban on Female Deaths by Causes: Robustness to Conley Spatial Correlation

Panel A: Radius 500	Deaths of Women 20-49 Years Old, per 1000 Women			
	Attempted Abortion	Premature Birth Miscarriage	Septic Diseases After Childbirth	Other Complications of Pregnancy
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	2.960***	0.463**	2.526*	3.147***
	(0.894)	(0.208)	(1.343)	(0.731)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	Yes	Yes	Yes	Yes
Radio Coverage	Yes	Yes	Yes	Yes
Observations	135	135	135	135
Panel B. Radius 1000km				
Pre-Ban Abortions 1936 × Post-Ban	2.960***	0.463**	2.526*	3.147***
	(1.041)	(0.192)	(1.378)	(0.738)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	Yes	Yes	Yes	Yes
Radio Coverage	Yes	Yes	Yes	Yes
Observations	135	135	135	135
Panel C. Radius 5000km				
Pre-Ban Abortions 1936 × Post-Ban	2.960***	0.463**	2.526*	3.147***
	(0.916)	(0.203)	(1.528)	(0.905)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	Yes	Yes	Yes	Yes
Radio Coverage	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.053	0.012	0.044	0.079
Observations	135	135	135	135

Note: The table examines the robustness of the estimated effects of the 1936 USSR abortion ban to accounting for (Conley, 1999) spatial correlation, with female deaths per 1,000 women aged 20-49 as the dependent variable, categorized by cause. The key explanatory variable is the interaction term *Pre-Ban Abortions 1936* × *Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post-Ban* is a dummy variable that equals 1 for years affected by the abortion ban. The implementation of the abortion ban is in 1952 (Columns 1-2) and 1954 (Columns 3-4), reflecting the cohorts affected by the implementation of the abortion ban in July 1936. All regressions include region and year fixed effects and control for repressions per capita and radio coverage. Standard errors are clustered at the regional level. Standard errors also account for spatial correlation following (Conley, 1999), assuming that the correlation between the error terms of two observations is zero beyond 500 km, 1000 km, and 5000 km in Panels A, B, and C, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A8: Effect of the 1936 Abortion Ban on Infant Deaths by Causes: Robustness to Conley Spatial Correlation

	Infant Deaths, per 1000 Women					
	Congenital Weakness and Premature Birth			Congenital Heart Diseases		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Radius 500						
Pre-Ban Abortions 1936 × Post-Ban	18.022*	18.199**	15.590**	1.016	18.199**	15.590**
	(9.346)	(8.346)	(7.852)	(0.659)	(8.346)	(7.852)
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	No	Yes	Yes
Repressions per Capita	No	No	Yes	No	No	Yes
Radio Coverage	No	No	Yes	No	No	Yes
Mean Dep. Var.	1.045	1.045	1.045	0.012	1.045	1.045
Observations	276	276	276	270	276	276
Panel B. Radius 1000km						
Pre-Ban Abortions 1936 × Post-Ban	18.022*	18.199**	15.590**	1.016	18.199**	15.590**
	(9.806)	(8.487)	(7.625)	(0.653)	(8.487)	(7.625)
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	No	Yes	Yes
Repressions per Capita	No	No	Yes	No	No	Yes
Radio Coverage	No	No	Yes	No	No	Yes
Mean Dep. Var.	1.045	1.045	1.045	0.012	1.045	1.045
Observations	276	276	276	270	276	276
Panel C. Radius 5000km						
Pre-Ban Abortions 1936 × Post-Ban	18.022***	18.199***	15.590**	1.016*	18.199***	15.590**
	(6.994)	(6.237)	(6.053)	(0.519)	(6.237)	(6.053)
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	No	Yes	Yes
Repressions per Capita	No	No	Yes	No	No	Yes
Radio Coverage	No	No	Yes	No	No	Yes
Mean Dep. Var.	1.045	1.045	1.045	0.012	1.045	1.045
Observations	276	276	276	270	276	276

The table examines the robustness of the estimated effects of the 1936 USSR abortion ban to accounting for (Conley, 1999) spatial correlation, with infant deaths per 1,000 women as the dependent variable, categorized by cause. The key explanatory variable is the interaction term *Pre-Ban Abortions 1936* × *Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post-Ban* is a dummy variable that equals 1 for years affected by the abortion ban. The implementation of the abortion ban is in 1952 (Columns 1-2) and 1954 (Columns 3-4), reflecting the cohorts affected by the implementation of the abortion ban in July 1936. All regressions include region fixed effects. Columns (2-3) and (5-6) add year fixed effects. Columns (4) and (6) additionally control for repressions per capita and radio coverage. Standard errors are clustered at the regional level. Standard errors also account for spatial correlation following (Conley, 1999), assuming that the correlation between the error terms of two observations is zero beyond 500 km, 1000 km, and 5000 km in Panels A, B, and C, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A9: Direct Effects of the 1936 Abortion Ban on Birth and Infant Death Rates: Robustness to Alternative Clustering

Panel A: Province Clustering				
	Births per woman		Infant Deaths per woman	
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	0.673***	0.676***	0.126***	0.128***
	(0.137)	(0.139)	(0.034)	(0.034)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	No	Yes	No	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	0.066	0.066	0.012	0.012
Observations	460	460	418	418
Panel B: Federal Districts Clustering				
	Births per woman		Infant Deaths per woman	
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	0.673**	0.676**	0.126**	0.128**
	(0.268)	(0.270)	(0.052)	(0.052)
Federal Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	No	Yes	No	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	0.066	0.066	0.012	0.012
Observations	460	460	418	418
Panel C: Europe-Asia Clustering				
	Births per woman		Infant Deaths per woman	
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	0.673**	0.676**	0.126**	0.128**
	(0.045)	(0.051)	(0.009)	(0.008)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	No	Yes	No	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	0.066	0.066	0.012	0.012
Observations	460	460	418	418

Note: The table examines the robustness of the estimated effects of the 1936 USSR abortion ban using alternative clustering methods, with births per woman and infant deaths per woman as the dependent variables in Columns (1-2) and (3-4), respectively. The key explanatory variable is the interaction term *Pre-Ban Abortions 1936 × Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post Ban* is a dummy variable that equals 1 for years following the implementation of the abortion ban in 1936. Panel A clusters standard errors at the province level, Panel B at the federal district level, and Panel C at the Europe-Asia regional level. All regressions include region and year fixed effects. Columns (2) and (4) additionally control for repressions per capita and radio coverage. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A10: Indirect Effects of the 1936 Abortion Ban on Illegal Abortions: Robustness to Alternate Clustering

Panel A: Federal District Clustering	Convicted for Illegal Abortion, per 1000 Women		
	(1)	(2)	(3)
Pre-Ban Abortions 1936 × Post-Ban	0.271	0.289	0.315*
	(0.147)	(0.153)	(0.147)
Region Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Repressions per Capita	No	Yes	Yes
Radio Coverage	No	No	Yes
Mean Dep. Var.	0.048	0.048	0.048
Observations	258	258	258
Panel B: Europe-Asia Clustering	Convicted for Illegal Abortion, per 1000 Women		
	(1)	(2)	(3)
Pre-Ban Abortions 1936 × Post-Ban	0.271*	0.289	0.315*
	(0.041)	(0.052)	(0.030)
Region Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Repressions per Capita	No	Yes	Yes
Radio Coverage	No	No	Yes
Mean Dep. Var.	0.048	0.048	0.048
Observations	258	258	258

Note: The table examines the robustness of the estimated effects of the 1936 USSR abortion ban using alternate clustering methods, with convictions for illegal abortions per 1,000 women as the dependent variable. The key explanatory variable is the interaction term *Pre-Ban Abortions 1936 × Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post Ban* is a dummy variable that equals 1 for years following the implementation of the abortion ban in 1936. Panel A clusters standard errors at the province level, Panel B at the federal district level, and Panel C at the Europe-Asia regional level. All regressions include region and year fixed effects. Columns (3-4) additionally control for repressions per capita and radio coverage. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A11: Indirect Effects of the 1936 Abortion Ban on Vandalism: Robustness to Alternative Clustering

Panel A: Federal District Clustering				
	Vandalism by Adults (treated at 1954)		Vandalism by Adolescents (treated at 1952)	
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	2.665**	2.786**	0.361**	0.389**
	(0.858)	(0.931)	(0.148)	(0.161)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	No	Yes	No	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	0.260	0.260	0.040	0.040
Observations	248	248	211	211
Panel B: Europe-Asia Clustering				
	Vandalism by Adults (treated at 1954)		Vandalism by Adolescents (treated at 1952)	
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	2.665	2.786	0.361	0.389
	(0.561)	(0.642)	(0.194)	(0.216)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	No	Yes	No	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	0.260	0.260	0.040	0.040
Observations	248	248	211	211

Note: The table examines the robustness of the estimated effects of the 1936 USSR abortion ban using alternative clustering methods, with vandalism offenses per 1,000 population as the dependent variable. Columns (1-2) report offenses committed by adults (treated at 1954), while Columns (3-4) report offenses committed by adolescents (treated at 1952). The key explanatory variable is the interaction term *Pre-Ban Abortions 1936 × Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post Ban* is a dummy variable that equals 1 for years following the implementation of the abortion ban in 1936. Panel A clusters standard errors at the province level, Panel B at the federal district level, and Panel C at the Europe-Asia regional level. All regressions include region and year fixed effects. Columns (2) and (4) additionally control for repressions per capita and radio coverage. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A12: Impact of the 1936 Abortion Ban on Infant and Female Deaths by Causes: Multiple Hypothesis Testing

	Infant Death, per 1000 Women	Female Death, per 1000 Women			
	(1)	(2)	(3)	(4)	(5)
Pre-Ban Abortions 1936 × Post-Ban	15.590**	3.109***	0.527**	2.559*	3.222***
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Repressions per Capita	Yes	Yes	Yes	Yes	Yes
Radio Coverage	Yes	Yes	Yes	Yes	Yes
P-Value	0.001	0.001	0.029	0.064	0.001
Q-Value	0.003	0.003	0.030	0.040	0.003
Romano-Wolf	0.025	0.025	0.076	0.092	0.025
Mean Dep. Var.	1.045	0.055	0.012	0.045	0.079
Observations	276	136	137	137	137

Note: The table presents the effects of the 1936 USSR abortion ban on infant and female deaths, incorporating multiple hypothesis testing adjustments. The dependent variables are infant deaths per 1,000 women (Column 1) and female deaths per 1,000 women (Columns 2-5), categorized by specific causes. Infant deaths (Column 1) are due to congenital weakness and premature birth. Female deaths (Columns 2-5) include those caused by attempted abortions, premature birth and miscarriage, septic diseases after childbirth, and pregnancy complications, with the sample restricted to women under 50. The key explanatory variable is the interaction term *Pre-Ban Abortions 1936* × *Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post Ban* is a dummy variable that equals 1 for years following the implementation of the abortion ban in 1936. The table reports standard p-values, False Discovery Rate (FDR) q-values, and Family-Wise Error Rate (FWER) adjusted p-values using the Romano-Wolf method, accounting for multiple hypothesis testing across five null hypotheses. All regressions include region and year fixed effects and control for repressions per capita and radio penetration. Standard errors are clustered at the regional level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A13: Direct Effects of the 1936 Abortion Ban on Birth and Infant Death Rates: Robustness to Alternative Fixed Effects

Panel A: Federal Districts Fixed Effects				
	Births per woman		Infant Deaths per woman	
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	0.751***	0.728***	0.124***	0.115***
	(0.159)	(0.159)	(0.042)	(0.043)
Federal Districts Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	No	Yes	No	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	0.066	0.066	0.012	0.012
Observations	460	460	418	418
Panel B: Europe-Asia Fixed Effects				
	Births per woman		Infant Deaths per woman	
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	0.754***	0.726***	0.135***	0.125***
	(0.157)	(0.158)	(0.043)	(0.046)
Europe-Asia Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	No	Yes	No	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	0.066	0.066	0.012	0.012
Observations	460	460	418	418

Note: The table examines the robustness of the estimated effects of the 1936 USSR abortion ban using alternative fixed effects specifications, with births per woman and infant deaths per woman as the dependent variables in Columns (1-2) and (3-4), respectively. The key explanatory variable is the interaction term *Pre-Ban Abortions 1936 × Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post Ban* is a dummy variable that equals 1 for years following the implementation of the abortion ban in 1936. All regressions include year fixed effects. Columns (2) and (4) additionally control for repressions per capita and radio coverage. Standard errors are clustered at the regional level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A14: Indirect Effects of the 1936 Abortion Ban on Vandalism: Robustness to Alternative Fixed Effects

Panel A: Federal District Fixed Effects				
	Vandalism by Adults (treated at 1954)		Vandalism by Adolescents (treated at 1952)	
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	2.451***	2.622***	0.240	0.286
	(0.879)	(0.902)	(0.214)	(0.207)
Federal District Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	No	Yes	No	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	0.260	0.260	0.040	0.040
Observations	248	248	211	211
Panel B: Europe-Asia Fixed Effects				
	Vandalism by Adults (treated at 1954)		Vandalism by Adolescents (treated at 1952)	
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	2.470***	2.629***	0.219	0.258
	(0.885)	(0.911)	(0.214)	(0.209)
Europe-Asia Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	No	Yes	No	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	0.260	0.260	0.040	0.040
Observations	248	248	211	211

Note: with vandalism offenses per 1,000 population as the dependent variable. Columns (1-2) report offenses committed by adults (treated at 1954), while Columns (3-4) report offenses committed by adolescents (treated at 1952). The key explanatory variable is the interaction term *Pre-Ban Abortions 1936* × *Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post Ban* is a dummy variable that equals 1 for years following the implementation of the abortion ban in 1936. Standard errors are clustered at the regional level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A15: Indirect Effects of the 1936 Abortion Ban on Illegal Abortions: Robustness to Alternate Fixed Effects

Panel A: Federal District Fixed Effects	Convicted for Illegal Abortion, per 1000 Women		
	(1)	(2)	(3)
Pre-Ban Abortions 1936 × Post-Ban	0.292*	0.304*	0.309*
	(0.166)	(0.165)	(0.167)
Federal District Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Repressions per Capita	No	Yes	Yes
Radio Coverage	No	No	Yes
Mean Dep. Var.	0.048	0.048	0.048
Observations	258	258	258
Panel B: Europe-Asia Fixed Effects	Convicted for Illegal Abortion, per 1000 Women		
	(1)	(2)	(3)
Pre-Ban Abortions 1936 × Post-Ban	0.282*	0.290*	0.299*
	(0.161)	(0.162)	(0.162)
Europe-Asia Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Repressions per Capita	No	Yes	Yes
Radio Coverage	No	No	Yes
Mean Dep. Var.	0.048	0.048	0.048
Observations	258	258	258

Note: The table examines the robustness of the estimated effects of the 1936 USSR abortion ban using alternative fixed effects specifications, with convictions for illegal abortions per 1,000 women as the dependent variable. The key explanatory variable is the interaction term *Pre-Ban Abortions 1936* × *Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post Ban* is a dummy variable that equals 1 for years following the implementation of the abortion ban in 1936. All regressions include year fixed effects. Columns (2-3) additionally control for repressions per capita and radio coverage. Standard errors are clustered at the regional level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A16: Direct Effects of the 1936 Abortion Ban on Birth and Infant Death Rates: Comparing Regions with Above and Below Median Radio Signal

Panel A: Abortion Ban and Births per Woman				
	Radio Signal Below Median		Radio Signal Above Median	
	Births per Woman			
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	0.988***	0.676***	0.649***	0.745***
	(0.245)	(0.228)	(0.189)	(0.207)
Mean Dep. Var.	0.070	0.070	0.061	0.061
Observations	223	223	237	237
Panel B: Abortion Ban and Infant Death per Woman				
	Radio Signal Below Median		Radio Signal Above Median	
	Infant Deaths per Woman			
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	0.177***	0.014	0.126**	0.072*
	(0.060)	(0.061)	(0.046)	(0.038)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	Yes	Yes	Yes	Yes
Radio Coverage	Yes	Yes	Yes	Yes
Pre-Ban Incomes × Year FE	No	Yes	No	Yes
Pre-Ban Wages × Year FE	No	Yes	No	Yes
Pre-Ban Hospitals × Year FE	No	Yes	No	Yes
Mean Dep. Var.	0.013	0.013	0.011	0.011
Observations	202	202	216	216

Note: with births per woman and infant deaths per woman as the dependent variables in Panels A and B, respectively. Panel A presents results for regions with below-median (Columns 1-2) and above-median (Columns 3-4) repressions levels. The key explanatory variable is the interaction term *Pre-Ban Abortions 1936 × Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post Ban* is a dummy variable that equals 1 for years following the implementation of the abortion ban in 1936. Standard errors are clustered at the regional level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A17: Indirect Effects of the 1936 Abortion Ban on Crime Outcomes: Comparing Regions with Above and Below Median Radio Signal

Panel A: Illegal Abortions				
	Radio Signal Below Median		Radio Signal Above Median	
	Convicted for Illegal Abortion, per 1000 Women			
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	0.127	0.145	0.310	0.339
	(0.256)	(0.279)	(0.231)	(0.259)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	Yes	Yes	Yes	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	0.045	0.045	0.051	0.051
Observations	126	126	132	132
Panel B: Vandalism Cases				
	Radio Signal Below Median		Radio Signal Above Median	
	Adults	Adolescents	Adults	Adolescents
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	2.677**	0.131	2.819***	0.382
	(1.268)	(0.230)	(0.788)	(0.267)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	Yes	Yes	Yes	Yes
Radio Coverage	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.291	0.047	0.232	0.033
Observations	119	104	129	107

Note: with convictions for illegal abortions per 1,000 women and vandalism offenses per 1,000 population as the dependent variables in Panels A and B, respectively. Panel A presents results for illegal abortion convictions in regions with below-median (Columns 1-2) and above-median (Columns 3-4) repressions levels. Panel B presents results for vandalism offenses, with Columns (1-2) showing cases committed by adults (treated in 1954) and Columns (3-4) by adolescents (treated in 1952). The key explanatory variable is the interaction term *Pre-Ban Abortions 1936* × *Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post Ban* is a dummy variable that equals 1 for years following the implementation of the abortion ban in 1936. Standard errors are clustered at the regional level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A18: Direct Effects of the 1936 Abortion Ban on Birth and Infant Death Rates: Comparing Regions with Above and Below Median Repressions Level

Panel A: Abortion Ban and Births per Woman				
	Repressions Level Below Median		Repressions Level Above Median	
	Births per Woman			
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	0.626***	0.892***	0.802***	0.386***
	(0.214)	(0.283)	(0.237)	(0.131)
Mean Dep. Var.	0.064	0.064	0.067	0.067
Observations	224	224	236	236
Panel B: Abortion Ban and Infant Death per Woman				
	Repressions Level Below Median		Repressions Level Above Median	
	Infant Deaths per Woman			
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	0.163***	0.119***	0.083	0.020
	(0.039)	(0.033)	(0.059)	(0.051)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	Yes	Yes	Yes	Yes
Radio Coverage	Yes	Yes	Yes	Yes
Pre-Ban Incomes × Year FE	No	Yes	No	Yes
Pre-Ban Wages × Year FE	No	Yes	No	Yes
Pre-Ban Hospitals × Year FE	No	Yes	No	Yes
Mean Dep. Var.	0.011	0.011	0.013	0.013
Observations	208	208	210	210

Note: with births per woman and infant deaths per woman as the dependent variables in Panels A and B, respectively. Panel A presents results for regions with below-median (Columns 1-2) and above-median (Columns 3-4) repressions levels. The key explanatory variable is the interaction term *Pre-Ban Abortions 1936 × Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post Ban* is a dummy variable that equals 1 for years following the implementation of the abortion ban in 1936. Standard errors are clustered at the regional level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A19: Indirect Effects of the 1936 Abortion Ban on Crime Outcomes: Comparing Regions with Above and Below Median Repressions Level

Panel A: Illegal Abortions				
	Repressions Level Below Median		Repressions Level Above Median	
	Convicted for Illegal Abortion, per 1000 Women			
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	0.102	0.108	0.399***	0.442**
	(0.335)	(0.350)	(0.112)	(0.158)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	Yes	Yes	Yes	Yes
Radio Coverage	No	Yes	No	Yes
Mean Dep. Var.	0.053	0.053	0.044	0.044
Observations	125	125	133	133
Panel B: Vandalism Cases				
	Repressions Level Below Median		Repressions Level Above Median	
	Adults	Adolescents	Adults	Adolescents
	(1)	(2)	(3)	(4)
Pre-Ban Abortions 1936 × Post-Ban	2.536***	0.296	2.862*	0.804**
	(0.870)	(0.187)	(1.451)	(0.359)
Region Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Repressions per Capita	Yes	Yes	Yes	Yes
Radio Coverage	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.260	0.040	0.260	0.039
Observations	121	99	127	112

Note: with convictions for illegal abortions per 1,000 women and vandalism offenses per 1,000 population as the dependent variables in Panels A and B, respectively. Panel A presents results for illegal abortion convictions in regions with below-median (Columns 1-2) and above-median (Columns 3-4) repressions levels. Panel B presents results for vandalism offenses, with Columns (1-2) showing cases committed by adults (treated in 1954) and Columns (3-4) by adolescents (treated in 1952). The key explanatory variable is the interaction term *Pre-Ban Abortions 1936* × *Post-Ban*, where *Pre-Ban Abortions 1936* represents the rate of abortions per woman in the first six months of 1936, and *Post Ban* is a dummy variable that equals 1 for years following the implementation of the abortion ban in 1936. Standard errors are clustered at the regional level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Data Appendix

B.1 Variable Description and Data Sources

TABLE B1: Description of variables

Variable	Description
Ministry of Health and Bureau of Statistics of the USSR (GARF, A374.23)	
Birth	Number of birth in between region year
Infant Death	Number of infants below 1 year died in between region year
Children Death	Number of children 1-5 years old died in between region year
Infant Death by Reason	Number of infants below 1 year died by causes of death in between region year
Female Death by Reason	Number of women died by causes of death in between region year
Pre-Ban Abortion 1936	Number of abortions made in hospitals within the regions at the first part of 1936
Pre-Ban Midwifes 1936	Number of obstetrician gynecologists in hospitals within the regions for the beginning of 1936
Prosecutor General Office of Soviet Union (GARF, A461.80)	
Criminal Abortions	Number of at home abortions registrated by police in between region year
Vandalism by Cohorts Above 18 Years	Number of vandalism cases committed by people above 18 years old in between region year
Vandalism by Cohorts 16-18 Years	Number of vandalism cases committed by people 16-18 years old in between region year

TABLE B1: Description of variables

Variable	Description
Time Varying Covariates (Memorial Data, Radio to All Sosiety, RGAE RF 1562.336[25], Zhiromskaya and Polyakov (2007))	
Level of Repressions	Number of repressions in between region year
Radio Signal Quality	Radio signal from the closes station in Db
Population	Number of people in between region for 1926, 1937, 1959
Population Female	Number of women in between region for 1926, 1937, 1959
Population Male	Number of men in between region for 1926, 1937, 1959
Additional Covariates (GARF, A374.15-21)	
Wages at Agriculture	Average wages at agriculture within the regions for March 1936
Wages at Industry	Average wages at industry within the regions for March 1936
Average Income	Average income per worker within the regions for March 1936
Hospitals	Number of hospitals within the regions for the beginning of 1936

Note:

B2 Decree of the People's Commissariats of Health and Justice. On the Protection of Women's Health (November 18, 1920).

Over the past decade, both in the West and in our country, the number of women resorting to the termination of their pregnancy has been increasing, as stated in the preamble, which occupies a large part of the regulatory document. The legislation of all countries is fighting this evil by means of punishment for both the woman who decides on the miscarriage and the doctor who performs it. Not leading to positive results, this method of struggle has driven this operation underground and made the woman a victim of greedy and often ignorant abortionists, who have turned the secret operation into their trade. As a result, up to 50% of women become ill from infection and up to 4% of them die.

The Worker-Peasant government takes into account all the evil of this phenomenon for the collective. By strengthening the socialist order and campaigning against abortions among the masses of working women, it fights this evil and, by widely implementing the principles of Protection of Motherhood and Infancy, anticipates the gradual disappearance of this phenomenon.

But as long as the moral remnants of the past and the difficult economic conditions of the present still force some women to decide on this operation, the People's Commissariat of Health and the People's Commissariat of Justice, protecting the health of women and the interests of the race from ignorant and greedy predators and considering the method of repression in this area to be absolutely ineffective, decree:

I. The free performance of the operation for artificial termination of pregnancy is permitted in the setting of Soviet hospitals, where its maximum safety is ensured.

II. It is absolutely forbidden to perform this operation by anyone other than a doctor.

III. Midwives or traditional birth attendants guilty of performing this operation are deprived of the right to practice and are brought before the People's Court.

IV. A doctor who has performed the operation of abortion in private practice for profit is also brought to trial.

Signed by:

People's Commissar of Health N. Semashko.

People's Commissar of Justice Kursky.

B3 Decree On the Prohibition of Abortions (June 27, 1936).

I. Due to the established harmfulness of abortions, prohibit their performance both in hospitals and special medical institutions, as well as at home by doctors and in the private apartments of pregnant women. Allow the performance of abortions exclusively in cases where the continuation of pregnancy poses a threat to the life or threatens serious harm to the health of the pregnant woman, as well as in the presence of severe hereditary diseases of the parents, and only in the setting of hospitals and maternity homes.

II. Establish criminal punishment for performing abortions outside of hospitals or in hospitals but in violation of the specified conditions, to the doctor performing the abortion — from 1 year to 2 years of imprisonment, and for performing abortions in unsanitary conditions or by persons without special medical education, establish criminal punishment of no less than 3 years of imprisonment.

II. Establish criminal punishment for coercing a woman to have an abortion — imprisonment for up to 2 years.

IV. In relation to pregnant women who perform abortions in violation of the specified prohibition, establish as criminal punishment, public censure, and in the case of repeated violation of the law on the prohibition of abortions — a fine of up to 300 rubles. Chairman of the All-Russian

Signed by:

Central Executive Committee M. Kalinin

Deputy Chairman of the Council of People's Commissars of the RSFSR D. Lebed

Acting Secretary of the All-Russian Central Executive Committee Novikov

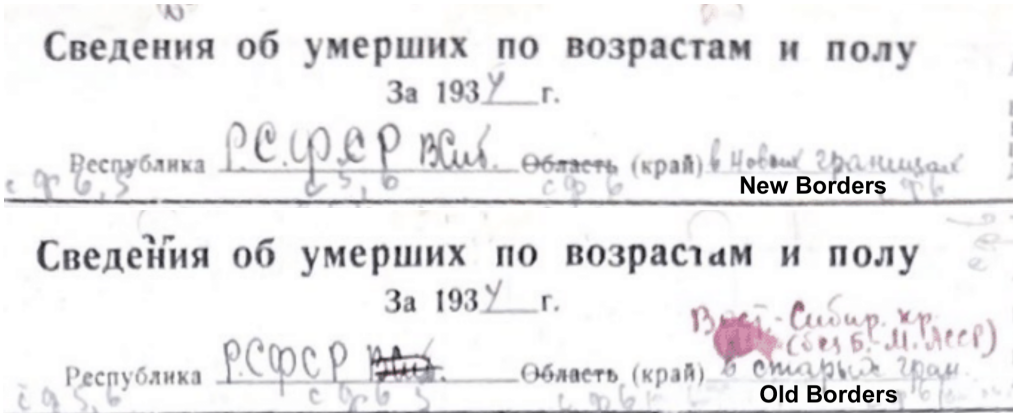
B4 Details on Region Matching.

The process of standardizing regional data is an important step in working with historical datasets, where administrative boundaries and naming conventions often shift over time. Following guidance from Giuliano and Matranga (2021), we adopt a structured approach to improve consistency in regional coverage across years.

We rely on the largest stable borders for each region as the basis of the analysis. When a region expanded by absorbing another and these changes could not be reliably tracked, we exclude it from the dataset from the year of expansion onward. In several cases, archival sources reported statistics under both the old and new administrative boundaries (see Figure B1), which helped address discrepancies in boundary definitions.

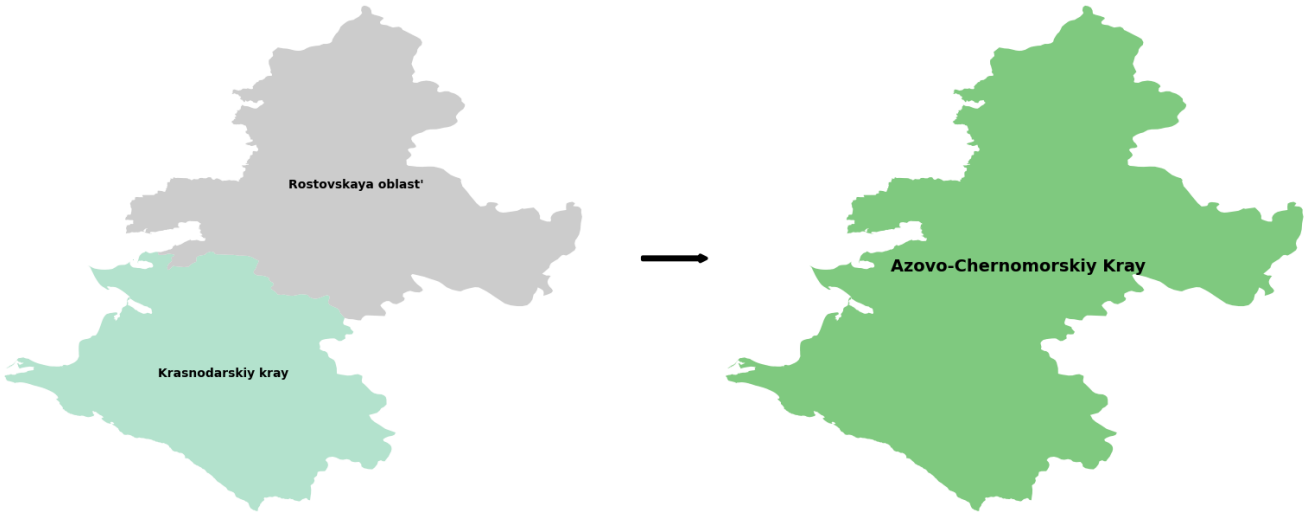
One example of this procedure is presented in Figure B2. In 1937, Azovo-Chernomorskiy Kray was split into Rostovskaya Oblast and Krasnodarskiy Kray. To maintain a consistent structure, we aggregate these units under the pre-division label "Azovo-Chernomorskiy Kray" throughout the dataset. Likewise, if one region fully absorbed another, such as Region A absorbing Region B in 1935, we treat the merged territory as a single unit across all periods. In cases where only partial territorial transfers occurred and could not be precisely reconstructed, we exclude the affected region from the analysis after the administrative change.

Figure B1: Example of Region statistics in old new borders



Note: The figure represents an example of statistics collected for the same region (Vostochno-Sibirskaya Oblast) in the year 1934 using old and new region borders.

Figure B2: Example of Region Matching



Note: The figure illustrates the process of region matching for consistent historical analysis. In 1937, Azovo-Chernomorskiy Kray was divided into Rostovskaya Oblast and Krasnodarskiy Kray. To maintain a stable regional structure across all years, these regions were merged into a single entity labeled "Azovo-Chernomorskiy Kray." Statistics from the separated regions were aggregated into unified data, as demonstrated in the figure.

B5 Details on Population Extrapolations.

Population data are drawn from the Soviet censuses conducted in 1926, 1937, and 1959. As a first step, we applied the procedure described in Appendix B4 to ensure consistent regional boundaries over time. For each inter-census period, we calculated annual population estimates by taking the total difference in population between the two censuses, dividing it by the number of years between them, and then incrementally adding this annual change to generate year-by-year estimates.

REFERENCES

Aghaei, Farideh, Abdolreza Shaghaghi, and Parvin Sarbakhsh, "A systematic review of the research evidence on cross-country features of illegal abortions," *Health promotion perspectives*, 2017, *7* (3), 117.

Alesina, Alberto, Paola Giuliano, and Nathan Nunn, "On the origins of gender roles: Women and the plough," *The quarterly journal of economics*, 2013, *128* (2), 469–530.

Angrist, Joshua D. and Jörn-Steffen Pischke, *Mostly harmless econometrics: An empiricist's companion*, Princeton, NJ: Princeton University Press, 2008.

_ **and William N. Evans**, "Schooling and Labor Market Consequences of the 1970 State Abortion Reforms," in Ronald G. Ehrenberg, ed., *Research in Labor Economics*, Vol. 18, Westport, Conn.: JAI Press, 1999, pp. 75–113.

Ashraf, Quamrul and Oded Galor, "Dynamics and Stagnation in the Malthusian Epoch," *American Economic Review*, May 2011, *101* (5), 2003–41.

Avdeev, Alexandre, Alain Blum, and Irina Troitskaya, "The History of Abortion Statistics in Russia and the USSR from 1900 to 1991," *Population an English Selection*, 1995, pp. 39–66.

Bailey, Martha, "Equal opportunities begin with contraception," *Nature*, 2020, *588* (7838), S177.

Bailey, Martha J, ""Momma's got the pill": How Anthony Comstock and Griswold v. Connecticut shaped US childbearing," *American economic review*, 2010, *100* (1), 98–129.

_ **, Brad Hershbein, and Amalia R Miller**, "The opt-in revolution? Contraception and the gender gap in wages," *American Economic Journal: Applied Economics*, 2012, *4* (3), 225–254.

Bearak, Jonathan Marc, Anna Popinchalk, Cynthia Beavin, Bela Ganatra, Ann-Beth Moller, Özge Tunçalp, and Leontine Alkema, "Country-specific estimates of unintended pregnancy and abortion incidence: a global comparative analysis of levels in 2015–2019," *BMJ global health*, 2022, *7* (3), e007151.

Berkhoff, Karel C, *Motherland in danger: Soviet propaganda during World War II*, Harvard University Press, 2012.

Blanc, Guillaume, "The Cultural Origins of the Demographic Transition in France," November 2023. working paper or preprint.

_ , "Demographic Transitions, Rural Flight, and Intergenerational Persistence: Evidence From Crowdsourced Genealogies," 2024.

_ **and Romain Wacziarg**, "Change and persistence in the age of modernization: Saint-Germain-d'Anxure, 1730–1895," *Explorations in Economic History*, 2020, *78*, 101352.

Callaway, Brantly, Andrew Goodman-Bacon, and Pedro H. C. Sant'Anna, "Difference-in-Differences with a Continuous Treatment," NBER Working Paper 32117, National Bureau of Economic Research February 2024. NBER Working Paper No. 32117.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller, "Bootstrap-based improvements for inference with clustered errors," *Review of Economics and Statistics*, 2008, *90* (3), 414–427.

Center for Reproductive Rights, "World's Abortion Laws," 2024.

Clarke, Damian, "The Economics of Abortion Policy," 2023.

Conley, T.G., "GMM estimation with cross sectional dependence," *Journal of Econometrics*, 1999, *92* (1), 1–45.

Currie, Janet and Maya Rossin-Slater, "Early-Life Origins of Life-Cycle Well-Being: Research and Policy Implications," *Journal of Policy Analysis and Management*, 2015, *34* (1), 208–242.

David, Henry P., "Abortion and Family Planning in the Soviet Union: Public Policies and Private Behaviour," *Journal of Biosocial Science*, 1974, *6* (4), 417–426.

Doepke, Matthias and Michéle Tertilt, "Women's Liberation: What's in It for Men?," *Quarterly Journal of Economics*, November 2009, *124* (4), 1541–1591.

Donohue, John J. and Steven D. Levitt, "The impact of legalized abortion on crime," *The Quarterly Journal of Economics*, 2001, *116* (2), 379–420.

Donohue, John J and Steven Levitt, "The impact of legalized abortion on crime over the last two decades," *American law and economics review*, 2020, *22* (2), 241–302.

Farin, Sherajum Monira, "Long-Run Intergenerational Impact of Abortion Legalization in the United States," *AEA Papers and Proceedings*, 2025, *115*, 303–307.

Foote, Christopher L and Christopher F Goetz, "The Impact of Legalized Abortion on Crime: Comment," *The Quarterly Journal of Economics*, 2008, *123* (1), 407–423.

Foster, Diana Greene, *The Turnaway Study: the cost of denying women access to abortion*, Simon and Schuster, 2020.

Gallen, Yana and Daisy Lu, "Post-Roe Planning: The Effect of Dobbs v. Jackson on Contraceptive and Sterilization Choices," Working Paper 2025-106, Becker Friedman Institute for Economics, University of Chicago, 5757 S. University Ave., Chicago, IL 60637 August 2025.

Galor, Oded and David N. Weil, "Population, Technology, and Growth: From Malthusian Stagnation to the Demographic Transition and Beyond," *American Economic Review*, September 2000, *90* (4), 806–828.

_ **and Omer Moav**, "Natural Selection and the Origin of Economic Growth," *Quarterly Journal of Economics*, November 2002, *117* (4), 1113–1191.

Giuliano, Paola and Andrea Matranga, "Historical data: where to find them, how to use them," in "The handbook of historical economics," Elsevier, 2021, pp. 95–123.

Goldin, Claudia, "Nobel Lecture: An Evolving Economic Force," *American Economic Review*, 2024, *114* (6), 1515–1539.

_ **and Lawrence Katz**, "The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions," *Journal of Political Economy*, August 2002, *110* (4), 730–770.

Goldman, Wendy, *Women, the State and Revolution: Soviet Family Policy and Social Life, 1917-1936*, Cambridge University Press, 1993.

Greene., Biggs MA Ralph L Gerdts C Roberts S Glymour MM Foster Diana, "Socioeconomic Outcomes of Women Who Receive and Women Who Are Denied Wanted Abortions in the United States," *American Journal of Public Health*, 2018, *108(3)*, 407–413.

Guttmacher Institute, "Unintended Pregnancy and Abortion Worldwide: Global and Regional Estimates of Unintended Pregnancy and Abortion," Fact Sheet March 2022. Accessed: July 2024.

Hazan, Moshe, David Weiss, and Hosny Zoabi, "Women's Liberalization as a Financial Innovation," *Journal of Finance*, 2019, *74* (6), 2915–2956.

_ , _ , **and** _ , "Women's Liberation and the Demographic Transition," 2022. CEPR Discussion Paper No. DP16838.

Hjalmarsson, Randi, Andreea Mitrut, and Cristian Pop-Eleches, "The impact of abortion on crime and crime-related behavior," *Journal of Public Economics*, 2021, *200*, 104468.

_ , _ , **and** _ , "The impact of abortion on crime and crime-related behavior," *Journal of Public Economics*, 2021, *200*, 104468.

Jones, Kelly M. and Mayra Pineda-Torres, "TRAP'd Teens: Impacts of abortion provider regulations on fertility education," *Journal of Public Economics*, 2024, *234*, 105–112.

Joyce, Theodore, "Further Tests of Abortion and Crime," *National Bureau of Economic Research Working Paper No. 10564*, 2004.

Kollontai, Alexandra, *The Labour of Women in the Evolution of the Economy*, London: Allison & Busby, 1921. Originally published as a pamphlet; reprinted in Selected Writings of Alexandra Kollontai, Allison & Busby, 1977. Translated by Alix Holt.

Kornileva, Maria, "Demographic Consequences of the Ban on Abortions in the USSR in 1936," 2019.

Krupskaya, Nadezhda, "The War and Childbirth," *Kommunistka*, 1920.

Lenin, Vladimir, "The Working Class and Neo-Malthusianism," *Pravda No. 137*, June 1913.

Lerner, Daniel, "Soviet Propaganda: Its Techniques, Doctrines, and Practices: Soviet Foreign Propaganda. Frederick Barghoorn. Princeton University Press, Princeton, NJ, 1964. ix+ 329 pp..," *Science*, 1964, *144* (3619), 675–677.

Londoño-Vélez, Juliana and Estefanía Saravia, "The Impact of Being Denied a Wanted Abortion on Women and Their Children," *The Quarterly Journal of Economics*, 2025, p. qjaf006.

Michalopoulos, Stelios and Elias Papaioannou, "Pre-colonial ethnic institutions and contemporary African development," *Econometrica*, 2013, *81* (1), 113–152.

Miller, Sarah, Laura R. Wherry, and Diana Greene Foster, "What Happens after an Abortion Denial? A Review of Results from the Turnaway Study," *AEA Papers and Proceedings*, May 2020, *110*, 226–30.

Miller, Victoria, Patrick Webb, Frederick Cudhea, Jianyi Zhang, Julia Reedy, Peilin Shi, Josh Erndt-Marino, Jennifer Coates, Renate Micha, Dariush Mozaffarian, Anjum Memon, and Getahun Lombamo, "Miller Webb et al. (2023) Children's and adolescents' rising animal-source food intakes in 1990–2018 were impacted by age, region, parental education and urbanicity (Nature Food)," *Nature Food*, 04 2023, *4*, 305–319.

Mitrokhina, Evgeniya and Yoshiko M. Herrera, "Institutional Resilience: The Persistence of Data Availability and Transparency despite Authoritarianism," September 2024. Prepared for conference on Institutional Resilience.

Myers, Caitlin Knowles, "The power of abortion policy: Reexamining the effects of young women's access to reproductive control," *Journal of Political Economy*, 2017, *125* (6), 2178–2224.

_ , "Confidential and legal access to abortion and contraception in the USA, 1960–2020," *Journal of Population Economics*, 2022, *35* (4), 1385–1441.

Nakachi, Mie, *Replacing the Dead: The Politics of Reproduction in the Postwar Soviet Union*, New York: Oxford University Press, 2021.

Nunn, Nathan and Leonard Wantchekon, "The slave trade and the origins of mistrust in Africa," *American economic review*, 2011, *101* (7), 3221–3252.

Office of the United Nations High Commissioner for Human Rights, "Information Series on Sexual and Rproductive Health and Rights," 2020.

Pop-Eleches, Cristian, "The impact of an abortion ban on socioeconomic outcomes of children: Evidence from Romania," *Journal of Political Economy*, 2006, *114* (4), 744–773.

Rabotnitsa Journal, *Central Committee of the Communist Party of the Soviet Union*, 1936.

Radio Journals, *The Society of Friends of Radio USSR*, 1929-1940.

Rambachan, Ashesh and Jonathan Roth, "A More Credible Approach to Parallel Trends," *The Review of Economic Studies*, 2023, *90* (5), 2555–2591.

Randall, Amy E., ""Abortion Will Deprive You of Happiness!": Soviet Reproductive Politics in the Post-Stalin Era," *Russian Review*, 2011, *70* (1), 89–109.

Simmons, Ryan A., Rebecca Anthopolos, and Wendy Prudhomme O'Meara, "Effect of health systems context on infant and child mortality in sub-Saharan Africa from 1995 to 2015, a longitudinal cohort analysis," *Scientific Reports*, 2021, *11*, 16263.

Solomon, Susan Gross, "The Demographic Argument in Soviet Debates over the Legalization of Abortion in the 1920's," *Cahiers du monde russe et soviétique*, 1992, pp. 59–81.

Soltz, Aron, "Abortion and alimony," *Labour No. 97*, June 1937.

Strong, Anna Louise, *Marriage and Morals in Soviet Russia*, New York: Vanguard Press, 1927.

Tertilt, Michéle, Matthias Doepke, Anne Hannusch, and Laura Montenbruck, "The Economics of Womens Rights," *Journal of the European Economic Association*, 2022, *20* (6), 2271–2316.

The Moscow Times, "Do Russians Support an Abortion Ban?," 2023.

Toews, Gerhard and Pierre-Louis Vézina, "Enemies of the People," *American Economic Journal: Macroeconomics*, 2025, *17* (1), 310–342.

Trotsky, Leon, *The Revolution Betrayed: What is the Soviet Union and Where is it Going?*, Pathfinder Books (America), 1936.

White, Kari, Gracia Sierra, Klaira Lerma, Anitra Beasley, Lisa G Hofler, Kristina Tocce, Vinita Goyal, Tony Ogburn, Joseph E Potter, and Samuel L Dickman, "Texas' 2021 Ban on Abortion in Early Pregnancy Was Associated with a Decrease in Abortions in Texas, an Increase in Abortions Out of State, and a Decrease in Overall Abortions," 2023.

Zhiromskaya, V. and Y. Polyakov, "All-Union Population Census of 1937: General Results," *ROSSPEN*, June 2007.

Zhukov, Yuri M. and Roya Talibova, "Stalin's Terror and the Long-Term Political Effects of Mass Repression," *Journal of Peace Research*, 2018, *55* (2), 267–283.

Zhuravskaya, Ekaterina, Sergei Guriev, and Andrei Markevich, "New Russian economic history," *Journal of Economic Literature*, 2024, *62* (1), 47–114.