Coercive Policies and the Persistence of Violence: Evidence from Male Sterilizations in India

Aditi Singh* and Sarah Vincent†

June 11, 2026

Please click here for the most recent version

Abstract

Do coercive state-led programs have persistent effects on interpersonal violence? This paper investigates the long-run impact of a government-mandated sterilization campaign that predominantly targeted men during India's Emergency (1975–77). Launched in April 1976, the program was implemented with substantial variation in coercion intensity across districts over a period of ten months. Using newly digitized historical records, administrative crime data, and several nationally representative household surveys, we study whether the program generated unintended consequences for violence. Using a difference-in-differences strategy that exploits geographic variation in coercion intensity, we find that greater exposure to the program led to a persistent increase in violent crime. The increase is driven by murders and violence against women: murders rise by 17% and rapes by 33% in high-coercion districts. In contrast, we find little evidence of comparable increases in property crimes or other non-violent offenses. Event-study estimates indicate that these effects persist for decades. The results remain robust when controlling for a broad range of potential confounders. We investigate several mechanisms through which coercive sterilization may generate persistent increases in violence. First, we examine whether the program reduced fertility and demographic outcomes, which could in turn influence long-run patterns of violence. Using representative household survey data, we find no evidence that coercive sterilization reduced fertility. Second, we explore the program's long-run consequences for gender norms and psychological well-being. Districts more heavily exposed to coercive sterilization exhibit higher acceptance of intimate partner violence, lower female bargaining power and mobility, lower female sterilization rates, and lower male well-being decades after the Emergency. These findings are consistent with persistent social and psychological effects of coercive sterilization, including trauma, threatened masculinity and a change in gender norms, which may contribute to the long-run increase in interpersonal violence documented in the paper.

JEL Classification Numbers: J13, K42, N35, N45, 012, and 020

Keywords: Coercion, Male Sterilization, Violence, Persistence

*New York University Abu Dhabi, singh.aditi@nyu.edu

†Columbia University Heilbrunn Department of Population and Family Health, Mailman School of Public Health. Corresponding author. sv2890@cumc.columbia.edu

We are extremely grateful to Siwan Anderson, Renaud Bourlès, Mahesh Karra, Matt Lowe, Nathan Nunn, Avner Seror, and Roberta Ziparo for their support and guidance during this project. We thank Marcella Alsan, Timothée Demont, Maëlys de la Rupelle, Yannick Dupraz, Romain Ferrali, Nathalie Ferrière, Garance Génicot, Edward Glaeser, Claudia Goldin, Pauline Grosjean,

1 Introduction

Governments have long relied on coercion to implement economic and social policies. Population control programs, compulsory public-health interventions, forced labor systems, and other forms of state coercion use government authority to alter individual and household behavior. While the immediate consequences of such policies are often visible, they can also generate unintended economic and social consequences that persist long after the policies themselves have ended.

A growing literature documents the long-run effects of coercive policies on trust, health, human capital, and economic outcomes (Lowes and Montero, 2021; Nunn, 2008; León-Ciliotta et al., 2025). Related work shows that coercive or intrusive interventions can generate backlash against their intended goals (Fouka, 2019; Wheaton, 2020). Yet we know little about whether coercive state interventions can generate persistent increases in interpersonal violence. Because such policies often rely on threats, force, and violations of autonomy, they may alter social norms, individual behavior, and interpersonal relationships in ways that outlast the policies themselves.

In this paper, we examine whether a coercive sterilization campaign that predominantly targeted men in India between April 1976 and February 1977 shaped subsequent violence. The campaign was implemented during the Emergency, a period of suspended civil liberties and centralized political authority. Although family planning had long been part of India's development strategy, the Emergency campaign marked a sharp escalation: officials used coercion, intimidation, and, in some cases, violence to meet sterilization targets. The program resulted in 8.3 million sterilizations, including 6.2 million vasectomies, compared with approximately 1.5 million male sterilizations in the previous year, making it one of the largest coercive sterilization campaigns in history. We use this large-scale episode and its geographic variation to address the following question: can coercive public interventions lead to a persistent increase in interpersonal violence?

We create a novel dataset by digitizing district-level government and administrative records on sterilizations and crime from 1972 to 2013. Our measure of coercive exposure is based on the growth in the sterilized-couple share between 1975–76 and 1976–77, which captures the sudden escalation of sterilization activity during the Emergency.[1] Because the geographic intensity of the campaign was not randomly assigned, our empirical strategy relies on a difference-in-differences framework and places particular emphasis on assessing the plausibility of the parallel trends assumption. In our main specifications, we define high-coercion districts as those above the median of this distribution and estimate static and dynamic difference-in-differences models comparing high- and low-coercion districts before and after the program. We further examine the robustness of our results across a wide range of specifications, including the inclusion of pre-

Sara Lowes, Jérémy Lucchetti, Pushkar Maitra, Benjamin Marx, Laure Vandewalle as well as participants at seminars at VSE, BU, Columbia, Harvard, AMSE, UNSW and at CEPR, ASREC, LAGV, ICDE, World Cliometrics, EDGE conferences and workshops for very helpful comments. Thank you to the researchers at the Centre for Human Sciences for their help in gathering the data and feedback. We also want to thank Andréa Renk and Balasubramanyam Pattath who kindly shared data with us and Ravi and Julie-Marie Biffe for exceptional research assistance. This work was also supported by the French National Research Agency Grant ANR-17-EURE-0020 and from the Excellence Initiative of Aix-Marseille University – A*MIDEX.

[1] This measure was developed jointly with Pelras and Renk (2023), who use it to study the impact of coercive sterilizations on trust in medicine.

determined district characteristics and other potential confounders that may be correlated with both exposure to the sterilization campaign and subsequent crime trends.

We find that exposure to coercive sterilization increases violent crime rates by approximately 14.5% in districts with above-median coercion intensity, while other categories of crime remain largely unaffected. Importantly, this increase is partly driven by murders, a crime less sensitive to reporting behavior, which rise by approximately 17% and event-study estimates remain statistically significant through 2006. At the same time, we observe a large increase in rapes of approximately 33%, with effects remaining statistically significant in the most exposed districts until 2009. Given that rape in India overwhelmingly affects women, these results point toward the possibility that the increase in violence may partly reflect gender-based violence. However, because reported rape statistics may also be affected by changes in reporting behavior, awareness, or willingness to report crimes, we cannot fully disentangle whether the observed increase reflects a rise in underlying incidence, an increase in reporting, or both. Examining the age distribution of perpetrators, we document that most perpetrators belong to cohorts younger than the men directly targeted by coercive sterilization during the Emergency, suggesting that the persistence of violence cannot be explained solely by behavioral changes among directly exposed individuals and is consistent with longer-run social and intergenerational processes extending beyond the originally targeted population.

Our dynamic difference-in-differences estimates show no clear evidence of differential pre-trends, mitigating concerns that high- and low-coercion districts were already on different crime trajectories before the Emergency. We also examine several alternative explanations, including changes in reporting, policing capacity, administrative penetration, and other district-level trends. Murder and rape increases remain stable across specifications. The absence of similar patterns in property crimes and most non-violent offenses also makes it difficult to attribute the results to a general expansion in reporting or state capacity. The findings are robust to alternative measures of coercive exposure, including continuous coercion intensity, increases in sterilization shares, and specifications accounting for prior sterilization levels. Leave-one-district-out estimates further show that the results are not driven by a small number of influential districts. Overall, the evidence points to a persistent increase in serious violent crime in districts more exposed to the sterilization campaign.

Finally, we examine the mechanisms through which a coercive sterilization campaign directed primarily at men may have shaped later violence. One possible channel is fertility and demographic change. Prior work links fertility, family size, and reproductive access to household dynamics, gender relations, and long-run crime outcomes. Reduced fertility may increase women's labor force participation and bargaining power within the household, potentially generating backlash in contexts where traditional gender roles are challenged. Fertility changes may also alter population composition and age structure, which have been linked to variation in crime rates.(Aizer, 2010; Erten and Keskin, 2018; Bhalotra et al., 2025; Gruber et al., 1999; Donohue and Levitt, 2001). Using representative household survey data, we provide the first evidence on the fertility consequences of the Emergency sterilization campaign. Taken together, the fertility results provide little support for a demographic explanation of the violence effects. While we do not find evidence that coercive sterilization reduced fertility on average, we observe a small increase in fertility among households

with specific characteristics, particularly those without a son prior to the Emergency. However, these effects are modest in magnitude and are not consistently robust across specifications, suggesting limited evidence for a fertility backlash. These patterns stand in sharp contrast to the large and persistent increases in murders and rapes documented above, suggesting that changes in fertility and demographic composition were not the primary drivers of the long-run increase in violence.

We examine a second channel: the social and psychological consequences of coercive sterilization. Historical accounts describe the campaign as producing humiliation, trauma, and threats to masculine identity (Scott, 2014). Such experiences may have generated persistent changes in interpersonal behavior, family dynamics, and gender relations long after the Emergency ended. This interpretation is consistent with a growing literature showing that threats to masculinity can generate compensatory behaviors, including aggression and violence (Bosson et al., 2009; Reidy et al., 2009; Field et al., 2026), as well as evidence that exposure to coercion and violence can have long-lasting effects on family dynamics and subsequent behavior (Beigelman, 2026). Consistent with this interpretation, districts with greater coercive exposure show more harmful gender norms more than two decades later: higher reported intra-household violence, greater acceptance of intimate partner violence, lower female participation in some decisions, and greater restrictions on women's mobility. Additional evidence indicates lower life satisfaction among men in highly exposed districts, consistent with the possibility that the coercive campaign generated persistent psychological distress. Taken together, the evidence suggests both immediate and persistent effects. We observe increases in murders shortly after the Emergency, consistent with a direct impact on exposed cohorts. Yet the effects remain detectable decades later and are increasingly concentrated among younger perpetrators who were children, adolescents or not yet born during the campaign. While we cannot directly observe the process of transmission, the persistence of the effects, together with the fact that highly exposed districts exhibit lower male well-being and more restrictive attitudes toward women decades after the Emergency, is consistent with the intergenerational transmission of attitudes and behaviors documented in the broader literature. Overall, the findings suggest that the long-run consequences of coercive sterilization operated primarily through social and psychological rather than demographic channels.

The contribution of this paper is threefold. First, we contribute to the literature on the long run impact of institutions including coercive institutions (Acemoglu et al., 2005; Weber, 1946; Nunn, 2008; Lowes and Montero, 2021; Alsan and Wanamaker, 2018; Liu and Han, 2023; Fouka, 2019; Wheaton, 2020; De la Rupelle and Zhang, 2023; Beigelman, 2026; Assouad and Buccione, 2025). Existing work has documented persistent effects of coercive policies on trust, health, education, and economic outcomes. Closer to our setting, León-Ciliotta et al. (2025) and Pelras and Renk (2023) examine the long-run consequences of coercive sterilization campaigns in Peru and India, respectively, documenting persistent effects on healthcare utilization, immunization uptake, and child health. We extend this literature by providing causal evidence that coercive public policies may also generate persistent increases in interpersonal violence decades after implementation. More broadly, our findings suggest that historical interactions between states and populations may shape contemporary patterns of violence.

Second, this paper contributes to the literature on cultural economics and identity (Bisin and Verdier, 2001).

Previous work shows that violence is shaped not only by contemporaneous economic conditions but also by social norms, identity formation, and reactions to threats to social status (Akerlof and Kranton, 2000; Cao et al., 2021; Baranov et al., 2023). A growing literature further argues that masculinity is socially constructed, precarious, and often reinforced through backlash behaviors when traditional male identities are threatened (Bosson et al., 2009; Field et al., 2026). Psychological research suggests that threats to masculinity may trigger compensatory aggression and violence as men attempt to reassert masculine identity and social status (Reidy et al., 2009), while developmental psychology highlights adolescence as a critical period for the formation and reinforcement of masculine identities and gender norms (Way et al., 2014; Mahalik et al., 2006; Jakupcak et al., 2002). We contribute to this literature by studying whether interventions threatening notions of masculinity can affect violence and gender norms in the long run. Our findings suggest that coercive interventions targeting male reproductive capacity may generate persistent changes in violent behavior, particularly gender-based violence, that remain visible long after the directly exposed population ages.

Finally, we contribute to the literature on reproductive health and sterilization (Rao, 1997; De la Rupelle and Dumas, 2020; Byker and Gutierrez, 2021). Most of this literature focuses on female sterilization and documents adverse consequences for health, well-being, and healthcare utilization. We add to this literature by studying a context in which men became the primary targets of reproductive coercion. Beyond documenting persistent increases in violence, we provide some of the first evidence on the fertility consequences of the Emergency sterilization campaign. Despite the program's explicit objective of reducing fertility, we find little evidence of sustained fertility decline and, if anything, evidence of small backlash on fertility among some households. These findings suggest that the legacy of coercive reproductive policies may lie less in their demographic effects than in their broader social and behavioral consequences.

This paper is organized as follows. Section 2 gives the historical background of the Emergency period. Section 3 presents the datasets used for this paper. Section 4 states our empirical strategies. Section 5 shows the results. Section 6 shows robustness tests. In Section 7, we explore potential mechanisms that would drive the results. Section 8 concludes.

2 The Emergency and Coercive Sterilizations: Historical Background

2.1 Objectives of the Sterilization Program

During the late 1960s and early 1970s, India experienced a period of economic and political turmoil. Although the Green Revolution substantially increased agricultural productivity, the country continued to face recurring food shortages and economic instability. The international oil crisis of 1973 further increased the cost of imported oil, contributing to rising inflation and deteriorating economic conditions (Jaffrelot and Anil, 2021). Moreover, in June 1975, the conviction of Indira Gandhi, the Prime Minister of India and leader of the Indian National Congress, by the Allahabad High Court for electoral malpractices during the 1971

national election put her position in jeopardy.[2] In response, she declared a National Emergency on June 25, 1975, ostensibly to restore order and address mounting political unrest (Gwatkin, 1979; Jaffrelot and Anil, 2021).

In June 1975, the government expanded the use of sterilization incentives, offering radios or payment as incentives; however, the program faced participation rates that were deemed insufficient to meet their intended targets. In February 1976, the government introduced a comprehensive five-point program encompassing family planning, tree planting, a ban on dowry, an adult education program known as "each-one-teach-one", and the abolition of social caste distinctions.[3] Emphasizing the critical necessity of population control, the government argued that family planning was consistent with all religious beliefs, and therefore, no individual should be exempted from sterilization on religious grounds (Mehta, 2012). These assertions were notably supported by Western nations, with the World Bank providing 66 million US dollars in humanitarian aid to India between 1972 and 1980 to support the implementation of sterilization programs (Gupte, 2017). Consequently, compulsory sterilization became integrated into a broader poverty reduction initiative.

2.2 Practical Implementation

Prior to the Emergency, states had the authority to decide on family planning, and although the prospect of implementing compulsory sterilization was considered in certain states, no definitive actions were taken. However, with the imposition of the Emergency, Indira Gandhi introduced Constitutional amendments, centralizing the authority for formulating family planning programs within the central government. Subsequently, the National Population Policy was drafted in 1976, prompting the central government to rally state political leadership and local administration to establish sterilization targets and camps. These targets were computed based on past sterilization accomplishments, with each state being assigned quotas to be fulfilled by any means necessary. [4]

The central government advocated for state-level incentives and disincentives for family planning, and it decided to allocate aid to states based on their family planning performance. Additionally, it authorized and endorsed coercive measures for sterilization. State governments had the authority to withhold employee promotions and payments, including those of school teachers, until they either underwent sterilization themselves or met their assigned quotas. This was enforced through the threat of employment termination to ensure compliance. Furthermore, the government mandated the presentation of sterilization certificates for access to basic amenities such as housing, irrigation, ration cards, and public healthcare facilities. Households with two or three children were notably pressured into sterilization through a combination of monetary incentives and disincentives (Tarlo, 2003; Schlesinger, 1977; Jaffrelot and Anil, 2021). This aggressive family planning program resulted in 8.3 million sterilizations in 1976–77, the majority of which (6.2 million)

[2] Frequent disagreements within the Indian National Congress led to its split in 1969. Indira Gandhi subsequently won the 1971 general election under the slogan "garibi hatao" (eradicate poverty).

[3] Among these, the family planning program stood out as the most significant point and was the primary focus of implementation within the five-point program.

[4] District officers and police commissioners were awarded gold medals for their dedicated efforts in achieving these set targets (Gupte, 2017).

were vasectomies, a substantial increase compared to the 1.5 million vasectomies performed in 1975-76.

The procedure entailed the surgical interruption of the vas deferens, the conduits for sperm. This involved a minor incision in the scrotum, where the vas deferens were either cut or obstructed. At the time, vasectomies were considered a definitive sterilization method as reverse procedures were not performed.

While some district and state leaders may have shared Indira Gandhi's objectives, others likely complied out of fear, given the widespread imprisonment of political opponents without trial during the Emergency. In addition, Sanjay Gandhi, the son of Indira Gandhi in charge of the practical implementation of the sterilization policy, regularly toured districts, placing considerable pressure on local officials to meet sterilization targets and encouraging the use of increasingly coercive methods to demonstrate compliance (Jaffrelot and Anil, 2021). Although the precise determinants of coercion intensity across districts remain difficult to identify, substantial variation emerged in the implementation of the campaign. In Section 5, we explore several factors associated with the use of coercive sterilization methods.

There are documented instances of police violence, perpetrated against both men and women, as a means to coerce men into undergoing vasectomies. Scott (2014) presents evidence of various police brutalities, including the use of weapons and tear gas. She emphasizes that both men and women lived in a constant state of fear due to the actions of the authorities.

2.3 Perceptions of the Emergency

Scott (2014) examines the perceptions of the Emergency of various actors in Indian society. She initially highlights the lack of coherent planning in this program, indicating that states and districts were encouraged to compete with each other, leading to a sense of gamification.

In *The Shah Commission Final Report: General Observations* (1990), several official complaints from civilians who were wrongfully coerced into undergoing vasectomies are documented. The study reports that 548 unmarried men were sterilized and that nearly 2,000 men died as a result of botched surgeries. Similarly, Guha (2007) describes cases in which men of all ages, including those who were physically incapacitated, were forcibly taken to sterilization camps. Numerous testimonies further attest to the coercive nature of the campaign. For example, one respondent recalled being told that he could only keep his government job if he agreed to undergo sterilization:

> "The officers said you could keep your job only if you get sterilised. I didn't have time to think. When I reached my duty we were told this... I agreed to it because I had to save my job and bring up my family." – Tarlo (2003)

This testimony illustrates that coercion often extended beyond physical force and operated through threats to employment and family livelihoods. In many cases, men faced a choice between preserving their reproductive autonomy and maintaining their ability to provide for their families.

Scott (2014) highlights that men were primarily targeted for two main reasons. Firstly, the urgent pressure from the government on states and districts compelled them to prioritize vasectomies due to their less invasive and quicker nature compared to tubectomies (female sterilizations). Secondly, men, particularly state workers, were predominantly threatened through their employment, as men constituted the vast majority of the labor force.

Scott (2014) also documents that the program engendered a sense of emasculation among men for three primary reasons. Firstly, the state's imposition of fertility control undermined the traditional masculine role of determining the ideal number of children for the family. Secondly, the threat posed to the masculine identity as breadwinners destabilized their traditional role. Finally, the invasive nature of vasectomies, coupled with the stigma surrounding them and the perceived risk of loss of sexual abilities, further compounded feelings of emasculation. The masculine response to this program is documented by the emergence of the anti-Emergency slogan:

> "Indira Hatao, Indira Bachao"
> ("Remove Indira, Save Indira"; popularly interpreted during the Emergency as "Vote Indira Out and Save Your Manhood/Penis")
> – Scott (2014)

In 1977, a series of widespread protests erupted across India, demanding an end to the Emergency and the initiation of new elections. In response, Indira Gandhi, under counsel, opted to terminate the Emergency period and the coercive sterilization program, subsequently paving the way for the organization of elections. The public voted her out of power, electing two opposition party politicians from the Janata Party, Morarji Desai (1977-79) and Charan Singh (1979-80), as successive Prime Ministers, who did not re-conduct the sterilization program.

3 Data

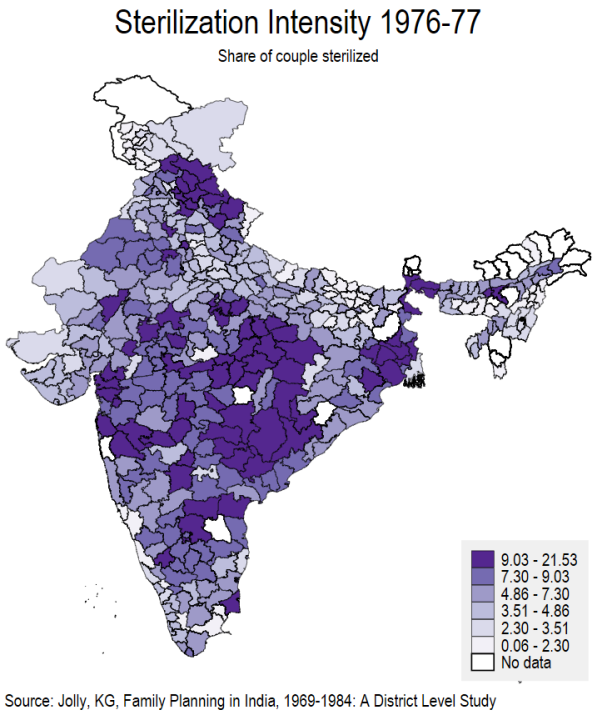
3.1 Treatment: intensity of coercion

To construct our treatment variable, we rely primarily on the comprehensive dataset provided by Jolly (1986), which documents the percentage of couples sterilized in all Indian districts spanning from 1970 to 1980. Figure 1 illustrates the district-level spatial distribution of sterilizations conducted during 1976-77. Complementing this dataset, we extend our research to include the data on tubectomies and vasectomies obtained from the Government's State or District statistical abstracts, covering approximately 66% of the total districts.[5] Furthermore, we supplement our analysis with information extracted from the annual yearbooks of the Ministry of Health and Family Welfare spanning the years 1971-72 to 2000-2001, providing

[5]This extensive sterilization dataset was collected through joint efforts with fellow researchers Balasubramanyam Pattath, Charlotte Pelras, and Andréa Renk. The dataset, alongside the one from Jolly (1986) that we digitized specifically for this study, is employed in Pelras and Renk (2023).

state-level insights into the prevalence of vasectomies, tubectomies, and the average age of men undergoing vasectomies.

Figure 1: Spatial distribution of sterilization intensity in 1976-77

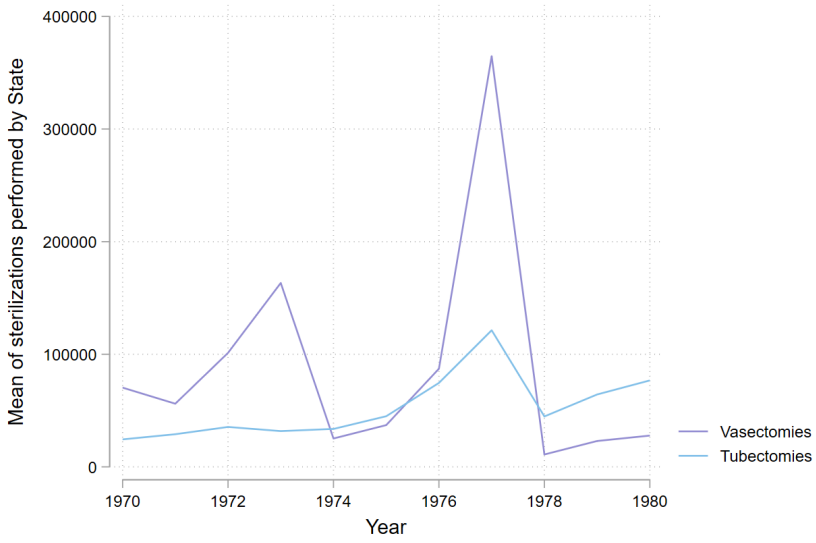


Notes: This map shows the district-level geographical variation of the percentage of sterilized couples in 1976-77, the year of implementation of the coercive sterilization program. Darker shades indicate that more individuals underwent sterilization during the program. Source: Jolly (1986)

Through analysis of state-level yearbooks, we observe from Figure 2 that vasectomies were the primary form of sterilization during this period. This indicates that the proportion of sterilized married couples can be used as a suitable proxy for understanding the spatial distribution of vasectomy rates in different districts. This enables us to measure differential exposure to the coercive sterilization campaign across districts. In Figure 3, we display the linear relationship between sterilizations on the left and vasectomies on the right. We sourced this data from the government's state or district statistical abstracts, where the percentage of married couples from Jolly (1986) is multiplied by the district-level population from the Indian Census of 1971. These graphs confirm the reliability of the Jolly (1986) dataset for studying sterilizations in India and its suitability as a proxy for vasectomies during the same period.

We acknowledge that the intense administrative pressure during the Emergency raises concerns about the accuracy of sterilization data. In particular, officials faced strong incentives to meet sterilization targets, creating the possibility that reported sterilization achievements were inflated or misreported. If present, such

Figure 2: Average of vasectomies and tubectomies performed in India 1970-80



Notes: Authors' computation using the annual state-level yearbooks of the Ministry of Health and Family Welfare. This graph represents the average of vasectomies and tubectomies performed in India between 1970 and 1980. It shows that most of the coercive sterilizations performed in 1976-77 were vasectomies and that there was a considerable increase in the number of vasectomies performed compared to previous years. The peak of vasectomies in 1973 was the result of the introduction of a voluntary vasectomy program.

manipulation could affect the measurement of coercion intensity across districts. At the same time, historical accounts emphasize both the extensive monitoring of program implementation and the widespread use of coercive measures Jaffrelot and Anil (2021), suggesting that recorded variation reflects meaningful differences in exposure. While some degree of over-reporting may have occurred, available evidence indicates that this was limited. To the extent that sterilizations were overstated in some districts, this would tend to attenuate our estimates by compressing true variation in exposure—effectively misclassifying some less exposed districts as highly exposed and as a result attenuate the estimated effects and bias them toward zero. We further assess data consistency by comparing multiple sources: Figures 3a and 3b show strong alignment between book reports and official state-level aggregates.

For the empirical analysis, we define district-level coercion intensity as:

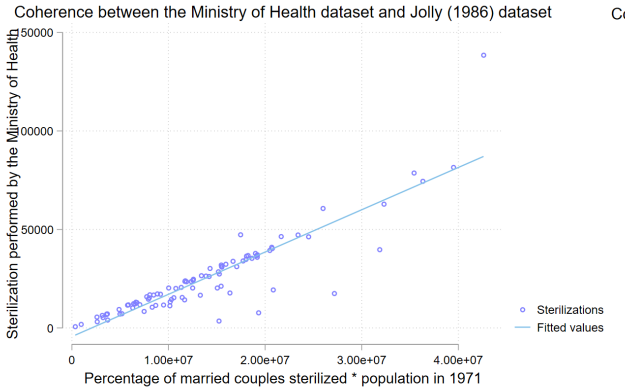
$$CI_d = \frac{Ster_d^{77} - Ster_d^{76}}{Ster_d^{76}}$$

Where Ster_d^{77} and Ster_d^{76} are percentages of sterilized couples in the year 1976-1977 and 1975-1976 respectively.

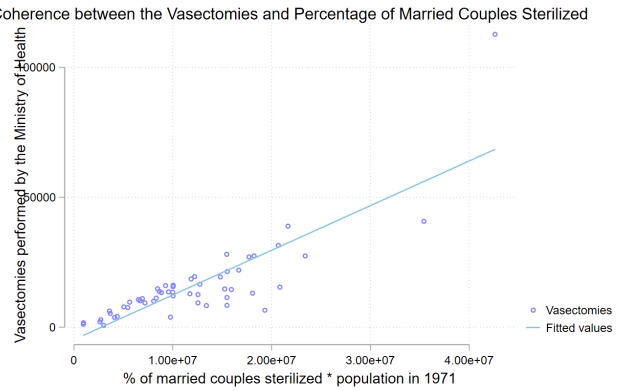
The variable (CI_d) measures the increase in the percentage of couples sterilized between 1975–76 and 1976–77. It can be interpreted as the number of times this percentage increased during the Emergency relative

Figure 3: Coherence between the Ministry of Health Dataset and Jolly (1986) 1976-77

(a) Sterilizations performed and percentage of sterilized married couples



(b) Vasectomies performed and percentage of sterilized married couples

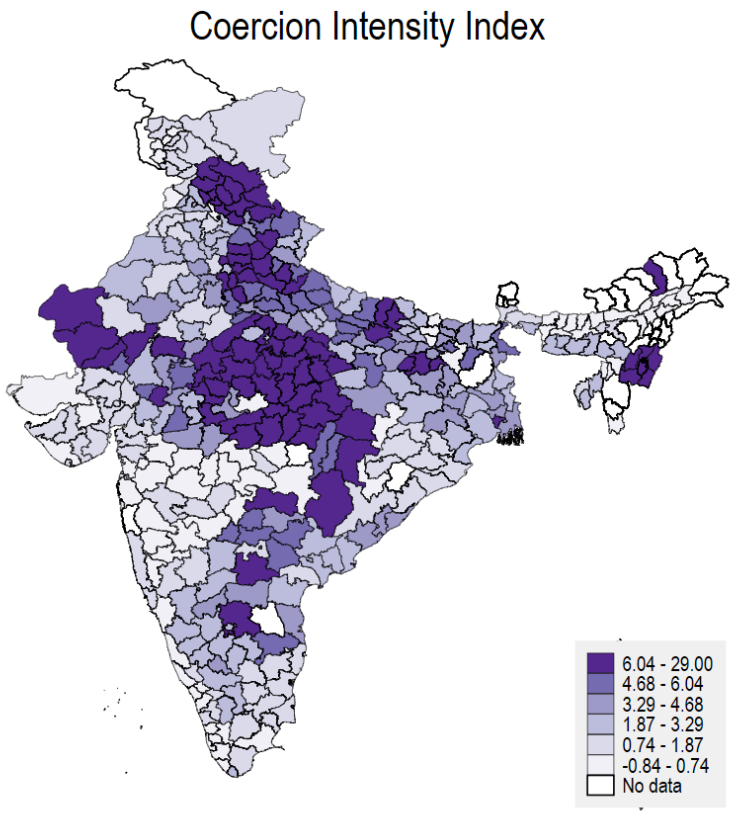


Notes: These figures plot the relationship between the number of sterilizations and vasectomies recorded by the Indian Ministry of Health and the district-level measure of sterilized couples constructed from Jolly (1986) and used in our analysis. To ensure comparability, the latter is converted into the number of sterilizations by multiplying the share of sterilized couples by district population in 1976–77. The strong positive relationship confirms that the measure derived from Jolly (1986) closely tracks the geographic distribution of sterilization activity and provides a relevant proxy for exposure to the vasectomy campaign.

to the previous year. Higher values indicate districts that experienced a larger expansion of the sterilization campaign and are therefore interpreted as having been exposed to greater coercion intensity. Figure 4 presents the spatial distribution of the coercion intensity index. The measure ranges from -0.84 to 26.72, indicating substantial variation across districts. A value of 1 implies that the percentage of couples sterilized doubled between 1975–76 and 1976–77. We discuss several alternative measures of coercion intensity in Section 6.

Finally, to ensure that extreme observations do not drive our results, we winsorize the coercion intensity measure at the 1% level. Figure A1 shows the distribution of CI_d before and after winsorization. Because CI_d is defined as a growth rate, it could in principle assign extreme values to districts with very low pre-Emergency sterilization baselines, where even a small absolute increase would generate a large proportional change. The distribution is right-skewed, but the bulk of identifying variation comes from the 0–6 range, and the mass in the upper tail is limited. This suggests that the results are unlikely to be driven by a small number of districts with artificially inflated growth rates due to low baselines. We confirm this directly by re-estimating our baseline specification sequentially dropping each district from the sample, and find that the estimates remain stable throughout (Section 6).

Figure 4: Coercion Intensity Index: All India



Notes: This map illustrates the geographic distribution of our treatment variable, coercion intensity, across Indian districts. The coercion intensity index is defined as the growth rate in the share of sterilized married couples between 1975–76 and 1976–77. A value of 1 corresponds to a 100 percent increase, or a doubling, of the sterilization share in 1976–77 relative to the previous year. Higher values indicate a larger expansion of sterilization activity during the Emergency and are interpreted as reflecting greater implementation pressure and coercion. Darker shades correspond to higher levels of coercion intensity.

3.2 Main outcome: crime rates at the district-level from 1972 to 2013

We digitized district-level crime data available on the Indian National Crime Records Bureau (NCRB) website, detailing annual reports of crimes reported to or by police forces, all under the Indian Penal Code, from 1972, the first year available, to 2013.

Gender information of perpetrators and victims at the district level is unavailable, but state-level aggregates are computed by gender. These tables reveal that men commit crimes at rates ranging from 4 to 4485 times more than women, with a mean of 90 times higher.

We categorize crimes into different types: violent crimes (murder, rape, and kidnapping), property crimes (robbery, counterfeiting, theft, dacoity[6], burglary, and trust), riots, and cheating crimes and then we run separate regressions for each category. The descriptive statistics of this dataset can be found in Table 2.

[6]Equivalent of banditry.

Rape crimes encompass all reported instances of rape. However, it's important to note that the Indian Penal Code excludes certain forms of rape, such as marital rape and same-sex crimes. Additionally, all sexual activity with a minor below the age of sixteen is considered rape under the law.

We are able to construct a district-level panel covering 199 districts between 1972 and 2013, out of approximately 360 districts in 1972. To ensure consistent district coverage over time, we exclude districts that were missing from the official records for more than two years, whether because reports were unavailable or not published. After matching the crime and sterilization datasets, we winsorize all crime variables at the 1st and 99th percentiles to limit the influence of extreme observations. The final sample contains 8,225 district-year observations. For property crimes, however, data are unavailable between 2000 and 2012 in the online records, reducing the sample to 5,873 observations.

As with most administrative crime data, reported offenses may understate the true incidence of crime, particularly for sexual offenses and other crimes that are less likely to be reported to the police. Reporting concerns are therefore an important consideration when interpreting crime statistics. We discuss the implications of differential reporting in the results section. By contrast, murders are far less susceptible to reporting biases and provide a relatively reliable measure of violent crime.

Table 1: Descriptive statistics of crime rates, pre- and post-treatment

	Overall	Pre-treatment	Post-treatment
Total crimes	2403.32	1725.23	2474.77
Violent crimes	97.28	58.00	101.42
Property crimes	831.11	1037.71	799.28
Riots	124.20	119.83	124.66
Rapes	20.79	5.75	22.38
Cheating	49.19	21.80	52.07
Murders	46.23	32.00	47.73
Observations	8225	784	7441

Notes: This table presents yearly average crime rates per 1 million inhabitants for the districts included in the main estimation sample. The pre-treatment period corresponds to years before the Emergency sterilization campaign, while the post-treatment period corresponds to years after its implementation. For property crimes, the dataset is available only until 2000, leading to a smaller number of observations in specifications using property crimes.

We note that the total number of crimes observed does not align with the sum of individual crime categories. This discrepancy arises because the records from the National Crime Records Bureau (NCRB) do not distinctly categorize every type of crime in their documents. Therefore, the differences between the observed total and the sum of individual categories account for other types of crimes, such as defamation cases, which are not explicitly delineated in the records.

To assess whether the districts included in our analysis differ from those excluded due to lack of data for more than two years, we perform a test of the difference of means between the two groups, as presented in Table 2, utilizing various variables from the 1971 Indian Census. Of the 199 districts included in our final crime analysis sample, 188 can be matched to districts in the 1971 Census. Conversely, 151 districts present in the 1971 Census are not included in the crime analysis sample. Overall, the retained districts are broadly comparable to the full set of districts observed in the 1971 Census. While some differences in demographic composition exist, these comparisons primarily provide descriptive evidence on the construction of the balanced district panel. Since our empirical strategy relies on comparisons between more and less exposed districts within the retained sample, such differences are unlikely to threaten the identification of the treatment effects.

Table 2: Summary statistics of districts: included vs excluded in the analysis

	(1)	(2)	(3)	(4)
	Mean in the dataset	Mean not in the dataset	Difference 2-1	P-value
Total population - 1971	1691862	1523772	-168091	0.15
Main Workers - 1971	558320	500139	-58181	0.13
Farmers - 1971	407749	325191	-82559	0.00
Household industry - 1971	20054	17099	-2955	0.13
Literates: ages 25-34 - 1971	75188	83783	8596	0.33
Literates: ages 35+ - 1971	107447	116828	9381	0.46
Primary school or higher- 1971	306778	337486	30707	0.39
Middle school or higher- 1971	141507	160846	19339	0.34
Total scheduled caste pop - 1971	270297	193308	-76989	0.00
Total scheduled tribe pop - 1971	98183	129516	31333	0.20
Observations	188	151	339	339

Notes: This table compares baseline district characteristics from the 1971 Indian Census between districts included in the final analysis sample and districts excluded due to missing crime records. Column (1) reports mean values for districts retained in the dataset, while Column (2) reports mean values for excluded districts. Column (3) presents the difference between the two groups, and Column (4) reports the corresponding p-values from tests of equality of means. Districts included in the final sample are broadly comparable to excluded districts along most pre-Emergency characteristics, although they exhibit a higher share of farmers and a larger Scheduled Caste population. All variables are measured prior to the Emergency sterilization campaign.

3.3 Additional variables and covariates

State capacity may be an important confounding factor in our setting. Districts located in states with stronger administrative capacity may have been better able to implement sterilization targets during the Emergency. At the same time, greater state capacity could also be associated with more effective crime reporting through a larger police presence and stronger administrative systems. While our identification strategy does not require treatment to be randomly assigned, controlling for state capacity allows us to examine whether differences in administrative capability are correlated with both coercion intensity and recorded crime rates.

To account for this possibility, we digitized annual state-level police force data from the National Crime Records Bureau (NCRB) for the period 1972–2013. District-level police force data are not available for this period, making state-level measures the most disaggregated information we could obtain. Because these data are unavailable after 2013, this year marks the end of our study period.

Oldenburg (1992) previously showed a positive correlation between male-female ratios and crimes, particularly murders. As the crime dataset does not provide any information on the specific characteristics of victims, studying the censuses could shed light in some areas. We use the 1971, 1981 and 1991 Indian censuses to compute sex ratios. We also track two age groups, specifically those aged 0-5 and 5-10 years old in 1971, as they would correspond to the 20-25 and 25-30 age groups in 1991. Due to our census data limitation which categorizes individuals as 35 and older, we were unable to follow older cohorts.

4 Empirical Strategy

4.1 Main specification

To estimate the effect of the 1976–77 coercive sterilization campaign on crime, we exploit cross-district variation in exposure to coercive sterilization and compare crime rates before and after the onset of the campaign. The crime panel covers 1972–2013, allowing us to examine both short- and long-run changes in crime.

Our main specification is a difference-in-differences design:

$$Y_{dst} = \eta_d + \theta_t + \beta \left(HighCoercion_d \times Post_t \right) + X'_{st}\gamma + \varepsilon_{dst} \tag{1}$$

where Y_{dst} is the crime rate per one million inhabitants in district d, state s, and year t. We examine total crime, violent crime, property crime, murder, rape, riots, and cheating. The variable $HighCoercion_d$ is an indicator equal to one for districts above the median of the coercion-intensity distribution and zero otherwise. The median value is 3.4, corresponding to a 3.4-fold increase in the sterilized-couple share between 1975–76 and 1976–77.$Post_t$ equals one for 1976 and later years, capturing the period after the onset of the coercive sterilization campaign. District fixed effects, η_d, absorb time-invariant differences across districts, while year fixed effects, θ_t, absorb shocks common to all districts in a given year. The vector X_{st} includes state-year police capacity, measured by police per capita. We control for more variables, notably potential confounders, in the robustness section. Standard errors are clustered at the district level. The coefficient of interest is β. It measures the differential change in crime rates in high-coercion districts after the onset of the campaign, relative to low-coercion districts.

The identifying assumption is that, in the absence of the campaign, crime rates in high- and low-coercion districts would have followed parallel trends. We examine this assumption using event-study specifications described below. In robustness checks, we examine sensitivity to both treatment measurement and speci-

fication choice. We re-estimate the model using the continuous coercion-intensity measure and alternative binary measures based on increases in the share of couples sterilized and past sterilization shares.

4.2 Event-study specification and threats to identification

The key identifying concern is that high- and low-coercion districts may have followed different crime trends even in the absence of the Emergency sterilization campaign. To assess this concern, we estimate an event-study specification:

$$Y_{dst} = \eta_d + \theta_t + \sum_{\substack{\tau=1972 \\ \neq 1976}}^{2013} \beta_\tau \left(HighCoercion_d \times \mathbf{1}\{t = \tau\} \right) + X'_{st}\gamma + \varepsilon_{dst}. \tag{2}$$

Here, $HighCoercion_d$ is the high-coercion indicator defined above, and $\mathbf{1}\{t = \tau\}$ is an indicator for year τ. We omit 1975, the year before the start of the coercive campaign, so each coefficient β_τ measures the difference between high- and low-coercion districts in year τ relative to 1975. The pre-1975 coefficients provide a test of whether high- and low-coercion districts were already on different trends before the campaign.

Recent work on difference-in-differences with continuous treatments shows that treatment-intensity estimates can be difficult to interpret when treatment effects vary across exposure levels (Callaway et al., 2024). A continuous-treatment coefficient compares districts not only before and after the policy, but also across different levels of exposure. If districts with higher exposure differ systematically from districts with lower exposure, or if the effect of an additional unit of exposure varies across the distribution, the resulting estimate may combine dose-response effects with selection across exposure levels. For this reason, our main specification relies on a binary high-coercion indicator, while continuous exposure measures are presented as robustness checks.

A related concern is that districts exposed to higher levels of coercion may have differed systematically from other districts even before the Emergency. To address this possibility, we allow baseline district characteristics—including caste composition, occupational structure, religious composition, literacy, population density, and rural population share—to generate differential trends over time. We implement this by interacting baseline characteristics with linear time trends and, in a more demanding specification, with year fixed effects. We also account for variation in administrative capacity by controlling for state-year police force strength, measured as police personnel per capita.[7] Because crime rates can be particularly noisy in smaller districts, we additionally report population-weighted specifications.

A broader concern is that other policies or shocks may have affected the same districts after the Emergency. While we cannot rule out all subsequent district-level shocks, such factors would need to be correlated with Emergency-era coercion intensity and generate differential changes in crime in the same districts after 1976. Several features of the setting make this explanation less likely. First, the Emergency sterilization campaign was unusual in its scale, nationwide reach, and coercive implementation, making it difficult to

[7] Since policing may itself respond to crime, we have verified that our results are not driven by the inclusion of this control.

identify a subsequent policy that would generate a similar pattern of district-level exposure.[8] Second, the Emergency was followed by substantial political change, making it unlikely that the same pattern of local coercive implementation persisted unchanged after the program ended. Consistent with this view, Pelras and Renk (2023) document long-run political consequences of exposure to the sterilization campaign. Finally, the inclusion of district and year fixed effects, controls for police capacity, and specifications allowing for differential trends by baseline characteristics further reduces concerns that our results are driven by smooth pre-existing trends or unrelated post-Emergency shocks.

5 Results

This section presents the effects of coercive sterilization on crime rates, examines their persistence over time, provides supporting evidence for the identification strategy, and explores whether the observed increase in violence exhibits gendered dimensions.

5.1 Main Effects of Coercive Sterilization on Crime

Table 3: Impact of coercive sterilizations on crime rates 1972-2013, more exposed districts vs less exposed districts

	(1) Total crimes	(2) Property crimes	(3) Violent crimes	(4) Rapes	(5) Murders	(6) Cheating	(7) Riots
Post × High Coercion	-24.25	-141.5**	14.12***	6.937***	8.050***	-5.425	-29.38***
	(179.3)	(62.87)	(5.052)	(2.638)	(2.210)	(7.436)	(10.90)
State police per capita	-6462.8***	-711.9***	-207.5***	-162.9***	-46.38**	141.1***	51.86
	(1172.6)	(259.6)	(38.35)	(17.54)	(19.95)	(30.57)	(65.24)
Outcome mean	2403.32	831.11	97.28	20.79	46.23	49.19	124.20
Observations	8225	5873	8225	8225	8225	8225	8225

Notes: This table presents the results of estimating equation 1, where the outcome is the crime rate per one million inhabitants. High coercion is defined as districts with coercion intensity above the median value of 3.4. This corresponds to 4.4 times the sterilization share observed in the previous year. The unit of observation is the district-year. All specifications control for state-level police force capacity. Standard errors, clustered at the district level, are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Tables 3 and 4 present the effects of coercive sterilization policies on crime rates using, respectively, a binary measure of exposure defined as districts above the median coercion intensity (equal to 3.4) and a continuous measure based on the increase in sterilization rates between 1975–76 and 1976–77.

Across specifications, we find a persistent increase in violent crime following greater exposure to coercive sterilization, while other categories of crime remain largely unaffected. This pattern is observed across multiple forms of violence, including murders and rapes. In the binary specification, districts with high coercion experience an increase of approximately 6.9 rapes and 8.0 murders per million inhabitants, corresponding

[8] Other initiatives associated with Sanjay Gandhi's agenda included tree planting, adult education campaigns, anti-dowry efforts, measures aimed at reducing caste discrimination, and urban slum-clearance programs. While some of these initiatives were controversial, none replicated the scale and district-level variation of the sterilization campaign.

Table 4: Impact of coercive sterilizations on crime rates 1972-2013, continuous coercion intensity

	(1) Total crimes	(2) Property crimes	(3) Violent crimes	(4) Rapes	(5) Murders	(6) Cheating	(7) Riots
Post × Coercion Intensity	22.11	-0.580	1.567***	1.093***	0.504**	-1.319***	-0.692
	(13.94)	(5.159)	(0.585)	(0.338)	(0.245)	(0.387)	(0.685)
State police per capita	-6537.2***	-739.5***	-211.1***	-165.8***	-47.19**	144.8***	51.07
	(1168.7)	(261.2)	(38.71)	(17.78)	(20.15)	(31.12)	(65.90)
Outcome mean	2403.32	831.11	97.28	20.79	46.23	49.19	124.20
Observations	8225	5873	8225	8225	8225	8225	8225

Notes: This table presents the results of estimating equation 1, where the outcome is the crime rate per one million inhabitants. Coercion intensity is measured as the growth rate in the share of sterilized couples between 1975–76 and 1976–77. A value of 1 corresponds to a doubling of the sterilization share relative to the previous year. The unit of observation is the district-year. All specifications control for state-level police force capacity. Standard errors, clustered at the district level, are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

to increases of about 33% and 17% relative to baseline means, respectively. Similar patterns emerge in the continuous specification, where a one-unit increase in coercion intensity, corresponding to a doubling of sterilization rates between 1975–76 and 1976–77, is associated with increases of 1.09 rapes and 0.50 murders per million inhabitants. The comparatively larger effects for rapes suggest that gendered violence may represent an important component of this broader increase in violence.

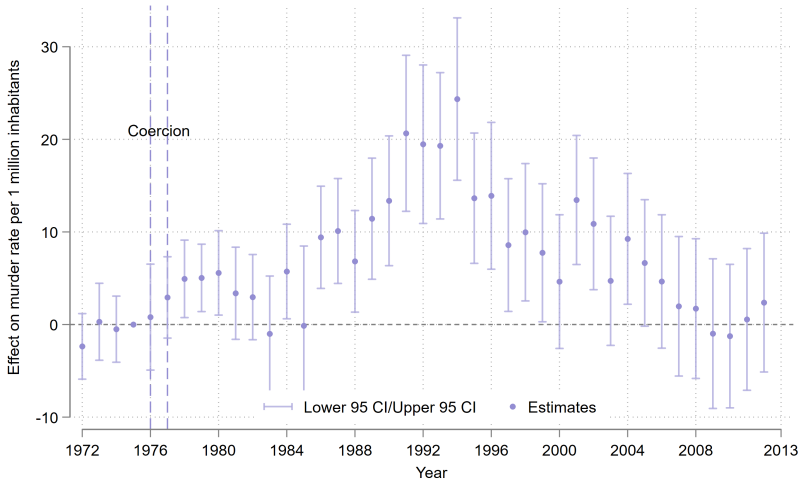
5.2 Dynamic Effects and Persistence

Figures 5, 6 and A2 present event-study estimates for rape, murder, and riots. For rape and murder, the pre-treatment coefficients do not show a clear differential trend between high- and low-coercion districts, consistent with the parallel trends assumption. The riot estimates do not point to a broad rise in collective violence. Rather, the effects appear concentrated in interpersonal forms of violence, especially rape and murder. Additionally, in Figure 6, the confidence intervals widen in later periods despite the balanced district panel, suggesting that the pattern is not driven by changes in sample composition. One possible explanation is increasing cross-district heterogeneity in reported rape rates over time, including differences in reporting practices or institutional capacity. We return to these issues in Section 6.3.

Figures 5 and 6 suggest that the estimated effects emerge gradually following the implementation of coercive sterilization and remain visible for several decades. For rapes, estimated effects remain statistically significant until the end of our study period in 2013 for the most exposed districts, while increases in murders persist until 2006. The attenuation of the murder effect does not appear to be driven by less-exposed districts catching up. As shown in Figure A3, murder rates remain persistently higher in highly exposed districts throughout the period, but gradually decline after the mid-1990s, reducing the gap between more- and less-exposed districts. Taken together, these patterns suggest that the effects of coercive sterilization were highly persistent, though the impact on murders gradually weakened over time while the effects on rapes remained detectable until the end of the sample period.

Finally, we provide descriptive evidence on the age profile of perpetrators to contextualize the persistence

Figure 5: Event Study: Impact of coercion intensity on murders 1972-2013

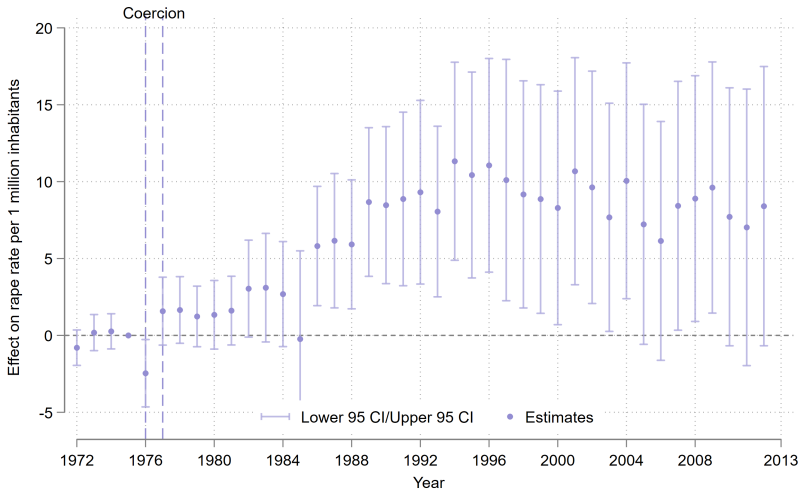


Notes: This figure plots the estimated β coefficients from equation 2. The dependent variable is the number of murders per one million inhabitants. The event is centered on 1976, and the coefficient for 1975 is normalized to zero. Vertical lines represent 95% confidence intervals. The treatment group consists of districts with coercion intensity above the median value of 3.4, corresponding to approximately 4.4 times the sterilization share observed in the previous year. The control group consists of districts below the median. Each coefficient measures the difference in murders between treated and control districts in a given year relative to 1975. The specification includes district and year fixed effects, controls for state-level police force capacity, and clusters standard errors at the district level.

of the crime effects. Figure 7 plots the age distribution of persons arrested for rape and murder in India using Bureau of Crimes data from 1988–2020. Across both crime categories, most arrested persons are in the 18–30 and 30–50 age groups throughout the period. This age composition is useful for interpreting the timing of our results. Ministry of Health reports indicate that the average age of men undergoing vasectomy during the Emergency was approximately 35. By 1988, the first year for which perpetrator-age data are available, men sterilized during the Emergency would have been around 46 years old on average, and would have aged out of the main perpetrator age groups over the subsequent decades.

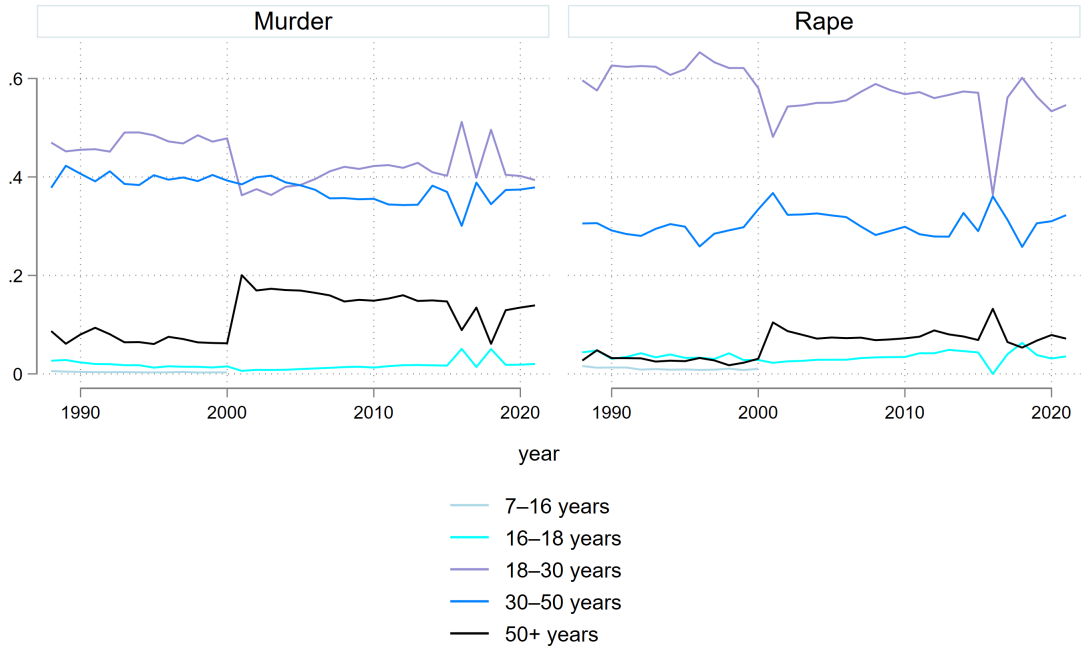
The age distribution suggests that later increases in rape and murder are unlikely to reflect only offending by the average directly sterilized or indirectly impacted men. Instead, the persistence of the crime effects is consistent with broader social transmission or exposure among cohorts who were children, adolescents, or younger adults during the Emergency and entered the main offending ages in later years.

Figure 6: Event Study: Impact of coercion intensity on rapes 1972-2013



Notes: This figure plots the estimated β coefficients from equation 2. The dependent variable is the number of rapes per one million inhabitants. The event is centered on 1976, and the coefficient for 1975 is normalized to zero. Vertical lines represent 95% confidence intervals. The treatment group consists of districts with coercion intensity above the median value of 3.4, corresponding to approximately 4.4 times the sterilization share observed in the previous year. The control group consists of districts below the median. Each coefficient measures the difference in rapes between treated and control districts in a given year relative to 1975. The specification includes district and year fixed effects, controls for state-level police force capacity, and clusters standard errors at the district level.

Figure 7: Age Distribution of Rape and Murder Perpetrators in India, 1988–2020



Notes: This figure reports the distribution of male perpetrators of rape and murder across age groups in India between 1988 and 2020. For both crimes, the majority of perpetrators belong to the 18–30 and 30–50 age groups throughout the sample period. Given that the average age of men undergoing vasectomy during the Emergency was approximately 35 years in 1977, many perpetrators observed in later years belong to cohorts that were too young to have been directly exposed to the sterilization campaign. The age distribution is therefore consistent with the persistence of violence beyond the originally exposed generation and suggests the possibility of intergenerational transmission of the program's social consequences.

5.3 Identification: Parallel Trends and Potential Confounding Factors

In addition to the event-study figures, we formally test for differential pre-trends during the pre-Emergency period. For this exercise, treated districts are defined as those above the median of the coercion-intensity distribution, where the median is 3.4. We estimate pre-trends by interacting the treatment indicator with a linear time trend over 1972–1975. As shown in Table A1, we fail to reject the null of no differential pre-trend for the main outcomes of interest, including rapes and murders. The only exception is riots, for which we detect evidence of differential pre-treatment trends. This result suggests caution when interpreting the estimated effects on riots and is consistent with the less stable pattern observed for this outcome across specifications. By contrast, the absence of differential pre-trends for rapes and murders supports the identifying assumption that high- and low-coercion districts followed similar trajectories prior to the Emergency.[9]

We also examine whether coercion intensity is systematically related to observable district characteristics measured prior to the Emergency. Although our identification strategy does not require coercion exposure to be randomly assigned, systematic relationships with baseline characteristics could indicate potential sources of confounding. Using data from the 1971 Census and the 1971 Lok Sabha elections, we relate coercion intensity to a range of pre-Emergency demographic, socioeconomic, and political characteristics, including population size, occupational composition, educational attainment, literacy, Scheduled Caste and Scheduled Tribe shares, religious composition, and the district-level share of elected candidates from the Indian National Congress.

Figure A4 presents the estimated relationships. Districts experiencing greater coercion intensity tended to have lower literacy rates, lower educational attainment, and somewhat larger Scheduled Caste populations. By contrast, coercion intensity is not systematically associated with population size, occupational structure, religious composition, or political alignment with Congress. These correlations raise the possibility that differences in socioeconomic conditions across districts could contribute to the patterns we document. We therefore conduct a series of robustness exercises that explicitly account for these potential confounders by allowing districts with different baseline characteristics to follow distinct trajectories over time and by controlling flexibly for pre-Emergency demographic and socioeconomic conditions. As shown in the robustness section, our main findings remain largely unchanged, suggesting that differential trends linked to observable district characteristics are unlikely to explain the estimated effects of coercive sterilization on violence.

5.4 Gendered Dimensions of Violence

The crime data do not report individual-level victim characteristics. This limits our ability to directly examine whether the increase in violence was concentrated among men or women. The interpretation is clearer for rape: nearly all reported rape cases in India involve female victims, so the increase in rape points to an

[9] For aggregate violent crime, the pre-period pattern is also somewhat less stable. This appears to be driven by components such as kidnapping and riots, outcomes for which we do not find consistent post-treatment effects across specifications. We therefore place greater weight on the disaggregated results for rapes and murders when interpreting the effects of coercive sterilization on violence.

increase in violence against women. For murder, victim sex is not available. We therefore use district-level sex ratios as suggestive evidence on whether high-coercion districts experienced broader changes in gender composition after the campaign. This exercise is motivated by prior work linking gendered violence and excess female mortality to changes in sex ratios (Oldenburg, 1992).

Using the Indian censuses of 1971, 1981, and 1991, we estimate specifications analogous to our main difference-in-differences design, replacing crime rates with male-to-female ratios. Table A8 shows small increases in sex ratios in high-coercion districts over time, both for the overall population and for the age groups we are able to follow across census waves. We do not observe comparable changes at birth, which makes it less likely that the pattern is driven by changes in sex-selective fertility.

We note that sex ratios cannot identify the sex of murder victims and may also reflect migration, household composition, or other demographic changes. Nevertheless, the estimates provide suggestive evidence that the consequences of coercive sterilization were not gender-neutral. Taken together with the increase in rape, the sex-ratio results are consistent with the possibility that coercive exposure shaped later violence in gendered ways. We return to this interpretation in Section 7, where we examine gender norms, male well-being, and attitudes toward women.

6 Robustness

Although our empirical strategy exploits variation in exposure to a historical policy shock, concerns may remain regarding treatment measurement, differential trends across districts, reporting behavior, and the influence of individual districts. In this section, we evaluate each of these possibilities through a series of robustness exercises. Reassuringly, the main findings remain stable across specifications, suggesting that the documented increases in interpersonal violence are not driven by these alternative explanations.

6.1 Alternative measures of coercion intensity and specifications

We examine whether the crime results are sensitive to how exposure to the sterilization campaign is measured. Our main treatment measure is based on the growth in the sterilized-couple share between 1975–76 and 1976–77. This measure captures the sharp escalation in sterilization activity during the Emergency and was constructed using district-level sterilization records digitized jointly with Pelras and Renk (2023). A potential concern is that growth measures may place greater weight on districts with low pre-Emergency sterilization levels. We therefore consider alternative treatment definitions that capture different margins of exposure.

First, we use sterilization levels in 1976–77 directly. We define districts with above-median sterilization rates in 1976–77 as highly exposed and interact this indicator with the post-Emergency period. To account for pre-existing family-planning activity, we include interactions between the post indicator and above-median sterilization levels in 1975–76 and 1972–73. The 1975–76 control captures sterilization activity immediately

before the Emergency, while the 1972–73 control captures earlier exposure to family-planning implementation, including the pre-Emergency increase in sterilizations during that period. This helps distinguish the effect of high Emergency-era sterilization exposure from districts with persistently high or earlier-rising sterilization activity. The corresponding specification is:

$$Y_{d,s,t} = \theta_t + \eta_d + \beta_1 \left(HighSter_d^{76-77} \times Post_t \right) + \beta_2 \left(HighSter_d^{75-76} \times Post_t \right) + \beta_3 \left(HighSter_d^{73} \times Post_t \right) + X_{s,t} + v_{d,s,t}. \tag{3}$$

Here, $HighSter_d^{76-77}$ equals one for districts with sterilization rates above the median in 1976–77. The variables $HighSter_d^{75-76}$ and $HighSter_d^{73}$ are defined analogously for pre-Emergency sterilization activity. This specification compares districts with high Emergency-era sterilization levels while accounting for earlier differences in family-planning implementation.

Second, we define treatment using the increase in the sterilized-couple share between 1975–76 and 1976–77. This measure captures the absolute change in sterilization exposure and is less sensitive to small baseline values than the growth measure.

Tables 5 and 6 show that the results for serious violent crimes are stable across alternative treatment definitions. When exposure is defined using above-median sterilization levels in 1976–77, high-exposure districts experience approximately 37% higher rape rates and 12% higher murder rates. When exposure is defined using the increase in the sterilized-couple share between 1975–76 and 1976–77, the estimated effects are approximately 47% for rape and 17% for murder. These estimates are close to the baseline effects of 33% for rape and 17% for murder. Notably, the levels-based specification also generates significant increases in property crimes and riots, outcomes for which the evidence is otherwise weak or inconsistent across specifications. We interpret these results cautiously. Unlike our preferred measure, which captures the sudden increase in sterilization activity during the Emergency relative to the previous year, the levels measure could reflect both Emergency coercion and pre-existing differences across districts in family-planning intensity, administrative capacity, public-health infrastructure, and state involvement in population control. As a result, it is likely to capture a broader set of district characteristics than the growth-rate measure. While the appearance of riot effects under some alternative measures is not inconsistent with the possibility that coercive interventions generated broader social tensions and collective violence, these estimates are sensitive to specification choice and exhibit evidence of differential pre-trends. By contrast, the growth-rate measure yields a more selective and stable pattern concentrated in serious interpersonal violence, particularly rapes and murders, which is consistent with the interpretation that it more effectively isolates the coercive component of the Emergency sterilization campaign. We therefore retain it as our preferred specification.

These treatment definitions capture related but distinct margins of the Emergency sterilization campaign. The growth-based measure captures proportional escalation relative to pre-Emergency exposure; the increase in the sterilized-couple share captures the absolute percentage-point change in exposure; and the 1976–77 sterilization-level measure captures how widespread sterilization became during the campaign, af-

ter accounting for earlier sterilization activity. Since these measures are not numerically equivalent, we do not expect identical magnitudes across specifications. The relevant robustness benchmark is whether the main pattern is stable across plausible definitions. Across these measures, the evidence remains most consistent for rape and murder, while estimates for total crime, property crime, cheating, and riots are more sensitive to the treatment definition.

We also examine whether the results depend on the cutoff used to define high exposure. In Table A2, we classify districts using the median coercion-intensity cutoff from the full sterilization sample rather than the crime estimation sample. The main conclusions remain similar, reducing the concern that the findings are driven by a particular treatment threshold.

Table 5: Robustness to alternative treatment definition: high sterilization levels in 1976–77

	(1) Total crimes	(2) Property crimes	(3) Violent crimes	(4) Rapes	(5) Murders	(6) Cheating	(7) Riots
High sterilizations 1976-77 × Post	336.7*	200.7***	15.70***	7.752***	5.323**	-2.739	26.78**
	(176.0)	(58.99)	(5.066)	(2.548)	(2.245)	(7.708)	(10.64)
High sterilizations 1975-76 × Post	-96.51	188.1***	-14.85***	-7.712***	-8.344***	-0.337	28.22***
	(178.2)	(58.89)	(5.069)	(2.447)	(2.222)	(7.700)	(10.65)
High sterilizations 1973 × Post	-129.3	-54.25	-8.774*	0.925	-4.516*	-2.957	-22.00*
	(208.4)	(62.18)	(5.035)	(2.551)	(2.299)	(7.367)	(11.83)
State police per capita	-6535.2***	-766.7***	-212.1***	-164.2***	-48.05**	140.3***	41.36
	(1172.0)	(263.6)	(38.52)	(17.59)	(20.12)	(30.03)	(65.59)
Outcome mean	2398.69	828.21	97.10	20.78	46.14	48.98	123.76
Observations	8267	5903	8267	8267	8267	8267	8267

Notes: This table presents the results of estimating equation 3, where treatment is defined using the level of sterilization activity in 1976–77 rather than the growth-based coercion intensity measure used in the main analysis. Districts are classified as high sterilization if their sterilization share in 1976–77 exceeds the median value. To account for pre-existing differences in family-planning intensity across districts, all specifications additionally control for the sterilization share in 1975–76. The unit of observation is the district-year. All specifications control for state-level police force capacity. Standard errors, clustered at the district level, are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table 6: Robustness to alternative treatment definition: changes in sterilization levels between 1975–76 and 1976–77

	(1) Total crimes	(2) Property crimes	(3) Violent crimes	(4) Rapes	(5) Murders	(6) Cheating	(7) Riots
High change in sterilizations × Post	438.8**	162.4***	21.95***	9.788***	7.760***	0.710	23.28**
	(177.9)	(62.12)	(5.050)	(2.600)	(2.269)	(7.415)	(10.85)
State police per capita	-6570.7***	-792.6***	-213.7***	-164.7***	-48.49**	139.5***	40.12
	(1169.7)	(259.5)	(38.49)	(17.65)	(20.06)	(29.97)	(65.66)
Outcome mean	2398.69	828.21	97.10	20.78	46.14	48.98	123.76
Observations	8267	5903	8267	8267	8267	8267	8267

Notes: This table presents the results of estimating equation 1, where coercion is measured as the change in the share of sterilized couples between 1975–76 and 1976–77 rather than the growth rate used in the main analysis. The unit of observation is the district-year. All specifications control for state-level police force capacity. Standard errors, clustered at the district level, are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

6.2 Additional controls and time trends

We next examine whether the results are driven by differential crime trends associated with pre-Emergency district characteristics. We begin by interacting baseline district characteristics with a linear time trend. We then estimate a more demanding specification that interacts the same baseline characteristics with year fixed effects. These characteristics include caste composition, educational attainment, occupational structure, religion, population density, and rural population share. This sequence allows us to test whether the results survive increasingly flexible controls for differences in pre-period district characteristics.

Tables A3 and A4 show that the main violent-crime results are not explained by these differential trends. The estimates for rape and murder remain positive, and the murder results are especially stable across specifications. The estimates for riots and other non-violent crimes are more sensitive, reinforcing our interpretation that the campaign affected specific forms of interpersonal violence rather than crime more generally.

Finally, we re-estimate the preferred specification weighting observations by district population. Population weighting gives greater weight to larger districts and addresses the concern that per-capita crime rates may be especially noisy in smaller districts. As shown in Table A5, the murder estimate remains positive and statistically significant, and violent crime also increases. The rape estimate remains positive but becomes smaller and less precise. Overall, the population-weighted results suggest that the core violent-crime findings are not driven by a small number of less populous districts.

Taken together, these robustness checks show that the main results are not driven by differential trends in baseline district characteristics or by the influence of small districts. The evidence is most consistent for serious violent crimes, especially murder and rape. By contrast, total crime, property crime, cheating, and riots are more sensitive to specification and treatment definition. We therefore interpret the results as evidence of a persistent increase in specific forms of interpersonal violence rather than a generalized increase in criminal activity.

6.3 Reporting and State Capacity

A potential concern is that the estimated increase in rape reflects changes in reporting or recording rather than changes in underlying violence. This concern is important because sexual violence is more sensitive to reporting practices than most other crime categories. The increase in murder, however, is harder to explain through reporting alone, since murders are less likely to go unreported and are less dependent on changes in social acceptance of reporting.

We investigate this possibility by examining whether the estimated treatment effects vary with state-level police capacity. Table A6 reports a triple-difference specification that interacts treatment exposure, the post-Emergency period, and police capacity. The results show that the increase in rape is concentrated in states with greater police capacity: while the baseline treatment effect is negative, the interaction between coercion

exposure and police capacity is positive and highly significant. This pattern is consistent with the idea that stronger policing and reporting institutions amplify the observed increase in reported rapes. By contrast, we find no evidence that police capacity moderates the effect on murders. The triple interaction is small and statistically insignificant, suggesting that differential reporting capacity is unlikely to explain the increase in murders. Interestingly, riots display the opposite pattern: the interaction with police capacity is positive and significant, while the baseline effect is strongly negative. This suggests that the negative association between coercion and riots is concentrated in states with lower police capacity and becomes substantially weaker in states with stronger policing institutions. Taken together, these results indicate that reporting and institutional capacity may influence the measured effects for crimes that are more sensitive to detection and recording, such as rape and riots, but they do not explain the increase in murders. More broadly, the findings suggest that changes in reporting conditions alone are unlikely to account for the overall pattern of persistent interpersonal violence documented in the paper.

Second, the pattern across crime categories is difficult to reconcile with a generalized reporting or state-capacity explanation. If high-coercion districts simply had stronger institutions, greater administrative penetration, or better crime recording, we would expect increases across a wider set of offenses. Instead, Table A9 shows that the effects are concentrated in serious violent crimes, especially rape and murder. We do not find similarly stable increases for property crimes, cheating, breach of trust, kidnapping, dacoity, or riots. This selective pattern suggests that the results do not reflect a broad increase in crime reporting or administrative capacity.

Third, we find no evidence that districts with higher coercion intensity were located in states with greater police capacity prior to the Emergency (Table A7). This reduces the concern that exposure to coercive sterilization is simply proxying for pre-existing reporting infrastructure. We also examine whether the rape results are driven by the later period, when reporting practices may have changed more substantially. Figure A6 restricts the event study to 1972–2000. The effect appears after the Emergency and grows through the 1990s.

Finally, coercion intensity is unlikely to reflect only pre-existing local political conditions. Historical accounts emphasize that sterilization targets were often imposed from above and that local officials faced strong pressure from the central government to meet quotas. Jaffrelot and Anil (2021) describe Sanjay Gandhi's direct involvement in the campaign, including visits to districts and pressure on local administrators, which contributed to uneven coercive implementation across the country. Taken together with the absence of systematic pre-Emergency differences in police capacity and the concentration of effects in serious violent crimes, the evidence is more consistent with the consequences of coercive implementation than with a general reporting, state-capacity, or administrative-penetration explanation.

6.4 Influence of Individual Districts

As an additional robustness check, we sequentially exclude each district from the sample and re-estimate our baseline specification. Appendix Figure A5 reports the resulting leave-one-district-out estimates for

rapes and murders. The estimated treatment effects remain positive and quantitatively similar across all specifications, indicating that the results are not driven by any single district. For murders, the coefficient varies only modestly around the baseline estimate, while a similar pattern emerges for rapes. These findings suggest that the observed increases in violent crime reflect a broad relationship between coercive sterilization and subsequent violence rather than the influence of a small number of highly exposed districts.

7 Mechanisms

We next examine why exposure to coercive sterilization may have affected interpersonal violence long after the Emergency. The results above are most stable for serious violent crimes, especially rape and murder, while the estimates for other crime categories are less consistent across specifications. This pattern motivates a closer examination of mechanisms related to fertility, household dynamics, gender norms, trauma, and male well-being.

7.1 Conceptual Framework: Coercive Sterilization and Interpersonal Violence

Figure A7 summarizes the framework guiding the mechanism analysis. We distinguish between three broad channels. The first is fertility and demographic change. Coercive sterilization could affect later violence by changing fertility, household composition, and gender relations within the household. Prior work shows that fertility reductions can increase women's labor supply and bargaining power, but that shifts in women's roles may also generate backlash and exposure to violence within households (Aizer, 2010; Erten and Keskin, 2018; Bhalotra et al., 2025). A related literature links fertility decline to long-run changes in crime through parental investment and early-life conditions (Donohue and Levitt, 2001; Gruber et al., 1999). Although the latter studies focus on abortion access and often find reductions in later crime, they highlight that reproductive policy can shape violence through demographic and household channels. We therefore test whether fertility and household-composition changes can account for the violence effects we document.

The second channel is a trauma channel. Historical accounts describe the Emergency sterilization campaign as involving threats, arrests, administrative pressure, and public humiliation (Scott, 2014). A growing literature shows that exposure to coercion, violence, humiliation, and loss of control can generate persistent effects on psychological well-being, family dynamics, and subsequent behavior (Oehme et al., 2012; Becker-Blease and Freyd, 2005; Mrug et al., 2016; Beigelman, 2026). Such experiences may affect interpersonal relationships long after the original shock has ended, contributing to the persistence of violence across generations. The fact that the effects remain visible decades after the Emergency is consistent with evidence that exposure to traumatic events during childhood and adolescence can have lasting consequences for later attitudes and behavior.

The third channel relates to threats to masculinity. Because the campaign disproportionately targeted male reproductive capacity, it may have been experienced as a threat to masculinity, virility, and household au-

thority.[10] This interpretation is consistent with a growing literature showing that threats to male status can generate compensatory behaviors, including aggression and violence (Bosson et al., 2009; Reidy et al., 2009; Field et al., 2026). Developmental psychology further suggests that norms surrounding masculinity, toughness, and the suppression of vulnerability are often formed and reinforced during adolescence (Way et al., 2014; Mahalik et al., 2006; Jakupcak et al., 2002; Matavelli, 2025). Figure 7 provides descriptive context for the persistence of the effects: by the time perpetrator-age data become available, the average directly sterilized man would have aged out of the main offending ages, while a large share of arrested perpetrators are substantially younger. Although we cannot directly observe the transmission process, this pattern is consistent with the possibility that social and gender-related consequences of the campaign were transmitted to cohorts who were children or adolescents during the Emergency.

These channels are not mutually exclusive. Fertility responses, trauma and threats to masculine identity may jointly affect household relations, gender norms, and interpersonal behavior. Given that the most stable crime effects are concentrated in rape and murder, while effects for other crime categories are less robust, these mechanisms provide a plausible set of margins to examine empirically. We therefore study whether highly exposed districts exhibit changes in fertility, acceptance of violence, women's autonomy in household and health decisions, gender norms, female sterilization, and male well-being.

7.2 Fertility

The first mechanism we examine is whether coercive sterilization affected later violence through changes in fertility and household composition. Since the stated objective of the Emergency sterilization campaign was to reduce fertility, we begin by examining post-1976 fertility outcomes.

7.2.1 Data

To study the effect of coercion intensity on fertility, we use the National Sample Surveys (NSS) of 1986-87, 42nd round. The dataset is the Survey on Maternity, Child Care, Family Planning, and Utilisation of the Public Distribution System. We restrict the sample to include only the immediate family of the head of the household because the data only gives a clear characterization of the children of the head of the household. This leaves us with 75% of the sample and 54,044 households.[11]

Outcome variables. Our primary outcome variable focuses on the count of births to examine the program's effects on fertility. This count encompasses all mothers within the age range of 15 to 50, who are either spouses of the household head or household heads themselves.

Control variables. The 42nd round of NSS encompasses a comprehensive set of individual and household

[10] Opponents of Indira Gandhi used slogans after the Emergency that explicitly framed sterilization as an attack on manhood; one such slogan is translated as "Vote Indira Out and Save Your Penis" (Scott, 2014).

[11] For example if household i has two daughters-in-law and three grandchildren, we will not be able to distinguish the mothers based on the serial number of grandchildren because all grandchildren have the same serial number i.e. 6.

characteristics, which we utilize for controlling factors such as age, sex, education levels of household members, caste, and occupation sectors in our analysis. Detailed summary statistics of the sample are presented in Table A10. Additionally, we augment these variables with district controls from the 1971 census.

We also use the 1961, 1971, 1981 and 1991 Indian censuses to compute the share of female labour force participation, defined as the share of working women in the population of women older than 15 at the district level.

7.2.2 Empirical strategy

To assess the effect of the excessive sterilization in 1976-77 on the number of births, we utilize spatial differences in coercion intensity and household-level variation in the number of children a couple had prior to the coercive campaign. Since the program primarily targeted husbands with children, we focus on children born after 1977 in households with already existing children before 1977, specifically in districts with high coercion intensity. These households were most likely affected by the program, making it the key area to observe its effects. We estimate whether the association between coercion intensity and subsequent fertility differs by the number of children observed in the household before the Emergency period.

We estimate the following specification:

$$\begin{aligned}\text{PostChild}_{id} = \alpha + \gamma \text{CI}_d + \sum_{n\in\{1,2,3,4+\}} \beta_n \text{PreChild}_{id}^n + \sum_{n\in\{1,2,3,4+\}} \mu_n \left(\text{HighCoercion}_d \times \text{PreChild}_{id}^n\right) \\ + \psi_1 \text{MAge}_{id} + \psi_2 \text{MAge}_{id}^2 + \mathbf{X}_i' \phi_1 + \mathbf{Z}_d' \phi_2 + \delta_s + \varepsilon_{ids}.\end{aligned} \tag{4}$$

The dependent variable, PostChild_{id}, is the number of children born after 1976 who are observed in household i in district d in the 1986–87 NSS household roster. The indicators PreChild_{id}^n equal one if household i has n children born before 1976 observed in the household roster, where $n \in \{1,2,3,4+\}$. Households with no observed children born before 1976 form the omitted category. The interaction terms $\text{HighCoercion}_d \times \text{PreChild}_{id}^n$ allow the association between coercion exposure and subsequent observed fertility to differ by the household's observed pre-1976 child count.

The controls MAge_{id} and MAge_{id}^2 denote the mother's age and age squared in 1976. The vector $\mathbf{X}_i$ includes household-level controls, while $\mathbf{Z}_d$ includes district-level controls. State fixed effects, δ_s, absorb persistent differences across states. Since CI_d varies at the district level, identification comes from comparing districts with different coercion exposure within the same state, conditional on the included household- and district-level controls. Standard errors are clustered at the district level.

A limitation of the NSS data is that it does not contain complete birth histories. Instead, our child-count variables are constructed from children observed in the 1986–87 household roster. As a result, they may differ

from completed fertility if children had died before the survey or, particularly for older cohorts, had already left the household. We therefore interpret PostChild $*$ id and PreChild $*$ id^n as measures of observed roster children rather than total births. To the extent that missing children reflect departures from the household or mortality unrelated to exposure to coercive sterilization, the resulting measurement error would attenuate estimated fertility effects toward zero. Consequently, both negative and positive effects on observed children are likely to represent conservative estimates of the underlying fertility response. Because observed child counts may also reflect differences in child survival, we additionally examine child mortality outcomes.

We estimate the following event-study specification using district-year counts of children observed in the NSS household roster:

$$Y_{dt} = \alpha_d + \eta_t + \sum_{\tau \in \mathscr{T}, \tau \neq 1976} \beta_\tau \left(\text{HighCoercion}_d \times \mathbf{1}\{t = \tau\} \right) + v_{dt}, \tag{5}$$

where Y_{dt} denotes the number of children born in year t and observed in district d in the 1986–87 NSS household roster. District fixed effects are denoted by α_d, year fixed effects by η_t, and $\mathscr{T}$ includes the years 1972–1985. The year 1976 is omitted as the reference year. The coefficients β_τ capture the difference between high- and low-coercion districts in year τ, relative to the corresponding difference in 1976.

7.2.3 Impact of coercion intensity on fertility and labour force participation

Table 7 examines the fertility consequences of the sterilization campaign. This question is central to understanding the program's effectiveness, as reducing fertility was its primary objective. Contrary to this objective, we find evidence of a limited fertility backlash. Households in high-coercion districts that had already begun childbearing exhibit higher levels of subsequent fertility, suggesting that coercive implementation may have undermined rather than reinforced family-planning goals. One possible explanation is that exposure to coercive sterilization increased resistance to fertility control efforts, either by generating distrust toward state family-planning programs or by inducing households to accelerate childbearing in anticipation of future interventions.

In the full sample, high-coercion districts have fewer observed post-1976 children among households with no children observed before 1976. However, this negative association is offset, and eventually reversed, for households that already had children before 1976. The interaction terms are positive and statistically significant for households with two, three, and four or more observed pre-1976 children. These estimates indicate that the program was not uniformly associated with lower fertility. For higher-parity households, exposure to coercion is instead associated with more children observed after 1976.[12]

The split by son status provides evidence on one possible mechanism. Columns 3 and 4 restrict the sample to households with no son observed before 1976. In this group, the baseline high-coercion coefficient is

[12] Appendix Table A12 examines the extensive margin of fertility, using an indicator for whether the household has any child born after 1976 and observed in the 1986–87 NSS household roster. The estimates are directionally consistent with the main fertility results.

close to zero, while the interaction terms are positive, especially among households with two observed children before 1976. This pattern is consistent with households responding to coercive sterilization risk by continuing or accelerating fertility when they had not yet had a son. The result is consistent with the large literature on son preference in India (Sen, 1992): if parents feared that future fertility might be restricted, households without sons had stronger incentives to have another child. Columns 5 and 6 show that the response is not confined to households without sons. Among households with at least one son observed before 1976, the interaction terms remain positive for households with two or more observed pre-1976 children, although the magnitudes are generally smaller.

We also examine heterogeneity by caste status. The effects are stronger among non-minority caste households in Table A11. This pattern might be consistent with the interpretation that households with greater access to information, or local knowledge about the coercive campaign were better able to anticipate the risk of future state intervention. These households may therefore have adjusted fertility more strongly in response to the perceived possibility that the program could be reintroduced.

Taken together, these results suggest that coercive sterilization may have generated a behavioral backlash. Rather than simply reducing fertility, the program appears to have increased subsequent observed fertility among households for whom the threat of future fertility restriction was most salient.[13]

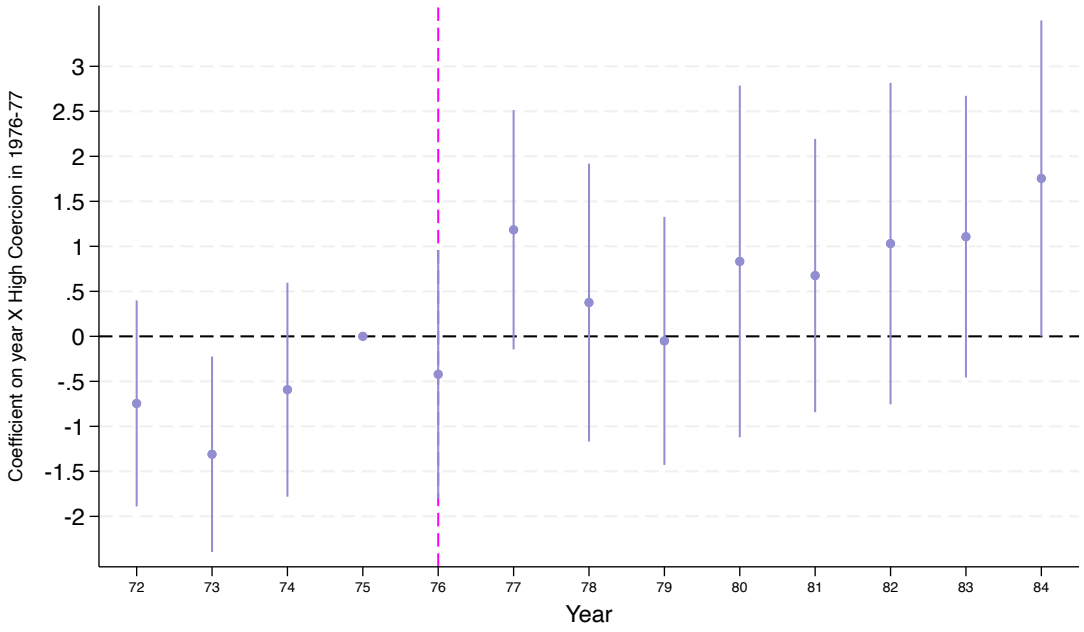
Figure 8 helps to understand the birth results through an event-study analysis. The event-study estimates show that there is an increasing trend in observed births around 1982. This timing coincides with Indira Gandhi's return to power and a period of heightened political instability. The pattern is consistent with households in districts previously exposed to coercive sterilization anticipating a possible return of such policies and responding with a fertility backlash.[14]

Given that the program had no effect on reducing fertility, we rule out changes in demographic factors as plausible mechanisms for observed violence.

[13] Because the NSS does not contain complete birth histories, the child-count variables are based on children observed in the 1986–87 household roster. The estimates should therefore be interpreted as effects on observed roster children rather than complete fertility histories.

[14] In Appendix Table A14, we estimate the effect on post-1976 fertility using the district-level sterilization share in 1976–77. The interaction coefficients are negative. This measure captures how widespread sterilization had become during the campaign, while the measure in our specification: high-coercion captures exposure to coercive implementation. The contrast is consistent with a broader feature of coercive policy: it can affect outcomes directly through implementation but it can generate behavioral responses in the short or medium run that work in a different direction.

Figure 8: Event Study: Births 1972-1985



Notes: This figure plots the estimated event-study coefficients from equation 5. The outcome is the number of observed children in the household. The event is centered on 1976, the first year of the Emergency sterilization campaign, and the coefficient for 1975 is normalized to zero. The treatment group consists of districts with coercion intensity above the median value of 3.4, corresponding to approximately 4.4 times the sterilization share observed in the previous year, while the control group consists of districts below the median. Each coefficient measures the difference in the number of observed children between treated and control districts relative to 1975. Vertical lines represent 95% confidence intervals.

Table 7: Impact of coercive sterilizations on number of births

	Whole sample		No sons born pre 1976		At least one son pre 1976	
	Poisson	OLS	Poisson	OLS	Poisson	OLS
High Coercion	-0.10***	-0.09**	-0.00	-0.01	-0.11***	-0.07
	(0.02)	(0.04)	(0.02)	(0.04)	(0.04)	(0.06)
One child pre 76 × High Coercion	0.07***	0.06*	0.03	0.05		
	(0.03)	(0.04)	(0.04)	(0.07)		
Two children pre 76 × High Coercion	0.24***	0.20***	0.15***	0.19**	0.16***	0.13*
	(0.05)	(0.05)	(0.05)	(0.09)	(0.06)	(0.07)
Three children pre 76 × High Coercion	0.34***	0.25***	0.16	0.23	0.24***	0.16**
	(0.06)	(0.05)	(0.10)	(0.18)	(0.06)	(0.06)
Four children pre 76 × High Coercion	0.33***	0.16***	0.08	0.04	0.20***	0.06
	(0.07)	(0.06)	(0.28)	(0.37)	(0.08)	(0.08)
Observations	54401	54401	23282	23282	31119	31119
Clusters	338	338	337	337	338	338
Hh Controls	Y	Y	Y	Y	Y	Y
Dist. Controls	Y	Y	Y	Y	Y	Y
State Fixed Effects	Y	Y	Y	Y	Y	Y
Mean of dep. var.	1.23	1.23	1.70	1.70	0.89	0.89
Joint p-value	< 0.001	< 0.001	0.076	0.237	< 0.001	< 0.05

Notes: This table presents the results of estimating equation 4, where the outcome is the number of observed children in the household. The unit of observation is the household. District-level controls include literacy rates, the share of workers, the share of farmers, the share employed in household industries, middle-school attainment, and the shares of Scheduled Castes and Scheduled Tribes in the population. Standard errors are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

7.2.4 Robustness on fertility: Placebo tests and alternative treatments

As a robustness and sensitivity check, we test whether the fertility pattern depends on choosing 1976 as the cutoff year. We keep the high-coercion classification fixed and re-estimate the main specification using alternative cutoff years to define pre- and post-period child counts. This changes the household's observed child-count category before the cutoff, while leaving the district-level treatment classification unchanged. Each point in Figure A8 comes from a separate regression based on a different cutoff year, and the figure plots the corresponding interaction coefficients. This is a moving cutoff-year check rather than an event-study design.

The coefficients are close to zero or negative for cutoffs in the late 1960s, rise as the cutoff approaches the Emergency period, and remain positive afterward. The pattern is strongest for households with two or more observed children before the cutoff. These results show that our findings are not sensitive to choosing 1976 alone and support the interpretation that the fertility response is linked to the timing of the coercive

sterilization campaign rather than to an arbitrary cutoff year.

Table A13 presents additional checks on the fertility results. Column 1 uses the number of children born before 1976 and observed in the NSS household roster as the dependent variable. The coefficient on High Coercion is small and statistically insignificant, suggesting that high- and low-coercion districts did not differ systematically in observed pre-1976 fertility after conditioning on the baseline controls and state fixed effects.

Columns 2 and 3 examine whether the results depend on using the binary high-coercion measure. Column 2 replaces the dummy with the continuous coercion-intensity measure. The estimates follow the same pattern as the baseline results: the main coefficient is negative, while the interactions with observed pre-1976 child count are positive, especially for households with one, two, and three children before 1976. Column 3 instead uses the raw increase in sterilizations. The estimates provide weaker evidence in the same direction: the main coefficient is negative, and the interaction for households with four or more observed pre-1976 children is positive, but the other interaction terms are small and imprecisely estimated.

Columns 4 and 5 return to the high-coercion dummy and add controls for pre-period sterilization growth and pre-period increases in sterilization, respectively. We measure pre-period sterilization growth using the change from 1972 to 1973, since there was a notable surge in sterilizations during this period,, as illustrated in Figure 2. In both specifications, these pre-period measures are interacted with the observed pre-1976 child-count categories. The coefficients on High Coercion and its interactions remain close to the baseline estimates. This indicates that the main fertility results are not driven by districts that were already on different family-planning trajectories before the Emergency.

Overall, the robustness checks support the interpretation that the fertility response is linked to coercive sterilization exposure during the Emergency period rather than to pre-existing fertility differences or baseline family-planning activity.

Appendix Section 8 presents additional checks addressing the fact that the NSS records children observed in the household roster rather than complete birth histories. We first show that the NSS results are robust to flexible age controls, alternative weighting, and sample restrictions that reduce concerns about older children leaving the household; we then use NFHS-1 birth histories to examine fertility directly and to test whether differential child mortality can explain the NSS results.

7.3 Threat to Masculinity and Normalization of Violence

The social and gendered channels imply that coercive sterilization may have altered gender relations, women's autonomy, and attitudes toward the use of violence.

The social and gendered channels imply that coercive sterilization may have altered gender relations, women's autonomy, and attitudes toward the use of violence. To assess these mechanisms, we use the 1998–99 Demographic and Health Survey, the first nationally representative household survey in India to include detailed

questions on domestic violence. The survey interviews women aged 15–49. Our analysis sample consists of married women who are either household heads or spouses of household heads, yielding 27,449 women. This analysis is descriptive rather than causal: the survey was conducted more than two decades after the Emergency and does not allow us to cleanly distinguish direct exposure from longer-run household or community transmission.

A cohort-based analysis is difficult in this setting. The 1998–99 survey interviews women aged 15–49, so older women who experienced the Emergency as adults are largely outside the sample. Some respondents may be married to men directly exposed to the campaign, while younger respondents may have been exposed to its longer-run consequences through families and communities. For this reason, we do not interpret age-cohort comparisons as identifying direct exposure effects. Instead, we use the survey to examine whether high-coercion districts display persistent differences in outcomes related to the proposed mechanisms.

Our estimating equation is:

$$Y_{isd} = \alpha + \beta HighCoercion_d + X_d'\gamma + X_i'\kappa + \theta_s + \varepsilon_{isd}, \tag{6}$$

where Y_{isd} is an outcome for individual i in state s and district d. The variable $HighCoercion_d$ is the district-level coercion-intensity dummy. The vector X_d includes district-level controls measured before the Emergency, including literacy, middle-school attainment, Hindu share, caste and tribe composition, and the agricultural population share. The vector X_i includes individual-level controls such as the respondent's partner's age and education. We report specifications with and without state fixed effects, θ_s. State fixed effects absorb time-invariant differences across states, including broad differences in institutions, culture, and survey reporting. However, because coercion intensity varies substantially across states, including state fixed effects also removes part of the relevant variation in exposure. The specifications with state fixed effects therefore provide a stricter within-state comparison, while the specifications without state fixed effects use both within- and across-state variation in coercion intensity.

Table A15 examines outcomes related to violence and attitudes toward violence. The first set of outcomes captures whether the respondent reports having experienced physical violence by the person listed in the column heading. The second set captures whether the respondent believes that wife beating is justified under specific circumstances, such as if a wife is unfaithful or does not cook properly. Table A16 examines women's agency and related household outcomes, including participation in own-health decisions, mobility restrictions, and female sterilization.

The results are consistent with persistent differences in gender relations and attitudes toward violence in more exposed districts. In Table A15, higher coercion intensity is associated with higher reported violence by boyfriends and greater acceptance of wife beating along some dimensions.[15] In Table A16, higher

[15]The coefficient on violence perpetrated by husbands is positive and not statistically significant without state fixed effects, and negative and statistically significant when state fixed effects are included. Descriptively, reported husband-perpetrated violence is higher in high-coercion districts (0.936) than in low-coercion districts (0.891). Given these mixed estimates and the very high baseline prevalence of this outcome, we do not emphasize it in the main discussion.

coercion intensity is associated with lower female participation in health decisions and greater mobility restrictions, while women in these districts are more likely to report working.

Taken together, these patterns are consistent with the social and gendered mechanisms described above. Districts more exposed to coercive sterilization display weaker female autonomy, more restrictive mobility, and greater acceptance of intimate partner violence in some contexts. The acceptance-of-violence results also speak to the normalization-of-violence channel, suggesting that exposure to coercive state action may be associated with more permissive attitudes toward the use of violence within interpersonal relationships. These results do not establish a causal mechanism, but they suggest that the long-run effects of the campaign may have operated partly through persistent differences in gender norms, household bargaining, and attitudes toward violence.

7.4 Male Well-being and Long-run Psychological Consequences

We next examine whether coercive sterilization is associated with long-run differences in male well-being. This exercise speaks to the trauma and masculinity channels discussed above, but should be interpreted as descriptive evidence rather than a direct test of psychological trauma. We use the Longitudinal Ageing Study in India (LASI), a nationally representative survey of individuals aged 45 and older conducted in 2017–2018. We focus on male respondents and merge individual-level data with district-level coercion intensity.

The outcomes capture several dimensions of well-being and behavior: depression, self-reported life dissatisfaction, a standardized index of dissatisfaction across life domains, and an index of risk behaviors based on smoking and alcohol consumption. We estimate the same cross-sectional specification as in equation 6. All specifications include individual controls for religion, caste, rural residence, age, education, marital status, and self-reported health; district controls from the 1971 Census; and state fixed effects. Standard errors are clustered at the district level.

Table A17 reports the results. Higher coercion intensity is associated with worse subjective well-being among men. In particular, men in more exposed districts are more likely to report life dissatisfaction and score higher on the dissatisfaction index. We do not find statistically significant associations with depression or risk-taking behavior. These patterns suggest that the long-run consequences of coercive sterilization may be reflected more clearly in subjective well-being than in clinical depression or self-reported risk behaviors. While not causal evidence of trauma, the results are consistent with persistent psychological and social costs among men in districts more exposed to the campaign.

8 Conclusion

Can coercive public interventions lead to a persistent increase in interpersonal violence? This paper studies the long-run consequences of India's Emergency sterilization campaign, implemented between 1976 and 1977. Although the program formally targeted sterilization more broadly, implementation disproportion-

ately affected men and relied on administrative pressure, coercion, and, in some cases, direct violence. This episode provides a large-scale historical setting to examine whether coercive public interventions can generate persistent social consequences beyond their stated objectives. We construct a new district-level panel by digitizing administrative records on sterilization and crime outcomes.

Our empirical strategy exploits geographic variation in exposure to the campaign, measured by the sharp increase in sterilization activity between 1975–76 and 1976–77. We estimate static and dynamic difference-in-differences specifications comparing districts with higher and lower exposure before and after the campaign. We find that greater exposure to coercive sterilization is associated with a persistent increase in serious violent crime. In high-exposure districts, rape rates increase by approximately 33 percent and murder rates by about 17 percent relative to baseline means. The event-study estimates indicate that these effects persist well beyond the Emergency period. By contrast, the evidence for other crime categories is less stable across outcomes and specifications. This pattern suggests that the campaign did not generate a generalized increase in all crime, but rather a more concentrated increase in interpersonal violence. The fact that both rape and murder increase also suggests that reporting changes alone are unlikely to fully explain the results, although reporting conditions may affect the measured magnitude of rape effects.

We then examine mechanisms. We do not find evidence that coercive exposure reduced post-1976 fertility, and the estimated fertility effects are small. The evidence is more consistent with social norms and psychological channels. Districts more exposed to coercive sterilization display more restrictive gender norms, lower female autonomy, greater acceptance of intimate partner violence in some contexts, and lower male life satisfaction decades later. These patterns are descriptive, but they suggest that the long-run effects of the campaign may have operated partly through changes in gender relations, attitudes toward violence, and male well-being rather than through demographic change alone.

The findings contribute to evidence that violence can reflect not only contemporaneous economic and social conditions, but also historical experiences of coercion. They suggest that coercive public policies may have persistent effects on social relations and interpersonal violence decades after implementation.

More broadly, our findings also speak to contemporary debates on reproductive health and family planning. Reproductive health policy has increasingly emphasized autonomy, informed choice, and rights-based approaches. This remains especially relevant in India, where family planning continues to rely heavily on sterilization and where the contraceptive burden falls disproportionately on women. Our results suggest that the history of coercive reproductive policy may have consequences that extend beyond fertility outcomes, shaping trust, gender relations, and interpersonal violence over the long run. Understanding these legacies is important for designing public policies that achieve demographic and health objectives without undermining autonomy, social well-being, and confidence in public institutions.

References

Acemoglu, Daron, Simon Johnson, and James A Robinson, "INSTITUTIONS AS A FUNDAMENTAL CAUSE OF LONG-RUN GROWTH," 2005.

Aizer, Anna, "The Gender Wage Gap and Domestic Violence," *American Economic Review*, September 2010, *100* (4), 1847–59.

Akerlof, George and Rachel Kranton, "Economics and Identity," *The Quarterly Journal of Economics*, 01 2000, *115*, 715–753.

Alsan, Marcella and Marianne Wanamaker, "Tuskegee and the health of black men," *The Quarterly Journal of Economics*, 2018, *133* (1), 407–455.

Assouad, Lydia and Giulia Buccione, "The Arab Slave Trade and the Diffusion of Islam in Africa," September 2025. Working paper.

Baranov, Victoria, Raphael Haas, and Pauline Grosjean, "Male-biased Sex Ratios and Masculinity Norms: Evidence from Australia's Colonial Past," *Journal of Economic Growth*, 2023.

Becker-Blease, Kathryn A. and Jennifer J. Freyd, "Beyond PTSD: An Evolving Relationship Between Trauma Theory and Family Violence Research," *Journal of Interpersonal Violence*, 2005, *20* (4), 403–411. PMID: 15722494.

Beigelman, Marie, "Sugar, Hardship, and Aftermath of Slavery," May 2026. Working paper.

Bhalotra, Sonia, Diogo Britto, Paolo Pinotti, and Breno Sampaio, "Job Displacement, Unemployment Benefits and Domestic Violence," *Forthcoming Review of Economic Studies*, October 2025, (2127).

Bisin, Alberto and Thierry Verdier, "The Economics of Cultural Transmission and the Dynamics of Preferences," 2001, *97* (2), 298–319.

Bosson, Jennifer, Joseph Vandello, Rochelle Burnaford, Jonathan Weaver, and Syeda Wasti, "Precarious Manhood and Displays of Physical Aggression," *Personality social psychology bulletin*, 03 2009, *35*, 623–34.

Byker, Tanya and Italo A. Gutierrez, "Estimating the Impact of Sterilization under a Government Campaign Using Contaminated Treatment Data," *Economic Development and Cultural Change*, 2021, *70* (1), 159–202.

Callaway, Brantly, Andrew Goodman-Bacon, and Pedro HC Sant'Anna, "Difference-in-Differences with a Continuous Treatment," 2024.

Cao, Yiming, Benjamin Enke, Armin Falk, Paola Giuliano, and Nathan Nunn, "Herding, Warfare, and a Culture of Honor: Global Evidence," Working Paper 29250, National Bureau of Economic Research September 2021.

Erten, Bilge and Pinar Keskin, "For Better or for Worse?: Education and the Prevalence of Domestic Violence in Turkey," *American Economic Journal: Applied Economics*, January 2018, *10* (1), 64–105.

Field, Erica M., Madeline McKelway, and Alessandra Voena, "Gender Norms and Development," 2026.

Fouka, Vasiliki, "Backlash: The Unintended Effects of Language Prohibition in U.S. Schools after World War I," *The Review of Economic Studies*, 05 2019, *87* (1), 204–239.

Gruber, Jonathan, Phillip Levine, and Douglas Staiger, "Abortion Legalization and Child Living Circumstances: Who Is the "Marginal Child"?," 1999, *114* (1), 263–291.

Guha, Ramachandra, *India After Gandhi: The History of the World's Largest Democracy* 01 2007.

Gupte, Prajakta R, "India:"The Emergency" and the politics of mass sterilization," *Education About Asia*, 2017, *22* (3), 40–44.

Gwatkin, Davidson R, "Political will and family planning: the implications of India's emergency experience," *Population and Development Review*, 1979, pp. 29–59.

III, John J. Donohue and Steven D. Levitt, "The Impact of Legalized Abortion on Crime," 2001, *116* (2), 379–420.

Jaffrelot, Christophe and Pratinav Anil, *India's First Dictatorship*, Oxford University Press, 2021.

Jakupcak, Matthew, David Lisak, and Lizabeth Roemer, "The Role of Masculine Ideology and Masculine Gender Role Stress in Men's Perpetration of Relationship Violence," 2002, *3* (2), 97–106.

Jolly, KG, *Family Planning in India, 1969-1984: A District Level Study*, Delhi: Hindustan Publishing Corporation, 1986.

la Rupelle, Maëlys De and Christelle Dumas, "Health consequences of sterilizations," FSES Working Papers 503, Faculty of Economics and Social Sciences, University of Freiburg/Fribourg Switzerland March 2020.

_ **and Xiaowei Zhang**, "Family planning, son preference and domestic violence: evidence from the "Later, Longer, Fewer" campaign in China," 01 2023.

León-Ciliotta, Gianmarco, Dijana Zejcirovic, and Fernando Fernandez, "Policymaking, Trust, and the Demand for Public Services: Evidence from a Mass Sterilization Campaign," *American Economic Journal: Economic Policy*, February 2025, *17* (1), 181–215.

Liu, Yiming and Yi Han, "Responsibility-Shifting through Delegation: Evidence from China's One-Child Policy," Rationality and Competition Discussion Paper Series 400, CRC TRR 190 Rationality and Competition June 2023.

Lowes, Sara and Eduardo Montero, "The legacy of colonial medicine in Central Africa," *American Economic Review*, 2021, *111* (4), 1284–1314.

Mahalik, James R., Shaun M. Burns, and Matthew Syzdek, "Masculinity and Perceived Normative Health Behaviors as Predictors of Men's Health Behaviors," 2006, *64* (11), 2201–2209.

Matavelli, Ieda, "We Don't Talk About Boys: Masculinity Norms Among Adolescents in Brazil," June 2025. Working paper.

Mehta, Vinod, *The Sanjay Story: From Anand Bhavan to Amethi*, Harper Collins, 2012.

Mrug, S, A Madan, and M. Windle, "Emotional Desensitization to Violence Contributes to Adolescents' Violent Behavior.," *Journal of abnormal child psychology*, 2016, *44.1* (March).

Nunn, Nathan, "The Long Term Effects of Africa's Slave Trades," *Quarterly Journal of Economics*, 2008, *123* (1), 139–176.

Oehme, Karen, Elizabeth A. Donnelly, and Annelise Martin, "Alcohol Abuse, PTSD, and Officer-Committed Domestic Violence," *Policing: A Journal of Policy and Practice*, 07 2012, *6* (4), 418–430.

Oldenburg, Philip, "Sex Ratio, Son Preference and Violence in India: A Research Note," *Economic and Political Weekly*, 1992, *27* (49/50), 2657–2662.

Pelras, Charlotte and Andréa Renk, "When sterilizations lower immunizations: The Emergency experience in India (1975–77)," *World Development*, 2023, *170*, 106321.

Rao, Vijayendra, "Wife-beating in rural South India: A qualitative and econometric analysis," *Social Science Medicine*, 1997, *44* (8), 1169–1180.

Reidy, Dennis E., Steven D. Shirk, Colleen A. Sloan, and Amos Zeichner, "Men Who Aggress against Women: Effects of Feminine Gender Role Violation on Physical Aggression in Hypermasculine Men," 2009, *10* (1), 1–12.

Schlesinger, Lee I., "The Emergency in an Indian Village," *Asian Survey*, 1977, *17* (7), 627–647.

Scott, Gemma, "Gender and Power: Sterilisation under the Emergency in India, 1975-1977," *The North West Gender Conference*, 2014, *Issue 5: Constructions of Gender in Research.*

Sen, Amartya, "Missing Women," *British Medical Journal*, 1992, *304* (March).

Tarlo, Emma, *Unsettling memories: Narratives of India's emergency*, Orient Blackswan, 2003.
The Shah Commission Final Report: General Observations

***The Shah Commission Final Report: General Observations**, Indian Journal of Public Administration, 1990, 36 (3), 695–701.*

Way, Niobe, Jessica Cressen, Samuel Bodian, Justin Preston, Joseph Nelson, and Diane Hughes, *""It Might Be Nice to Be a Girl... Then You Wouldn't Have to Be Emotionless": Boys' Resistance to Norms of Masculinity during Adolescence," 2014, 15 (3), 241–252.*

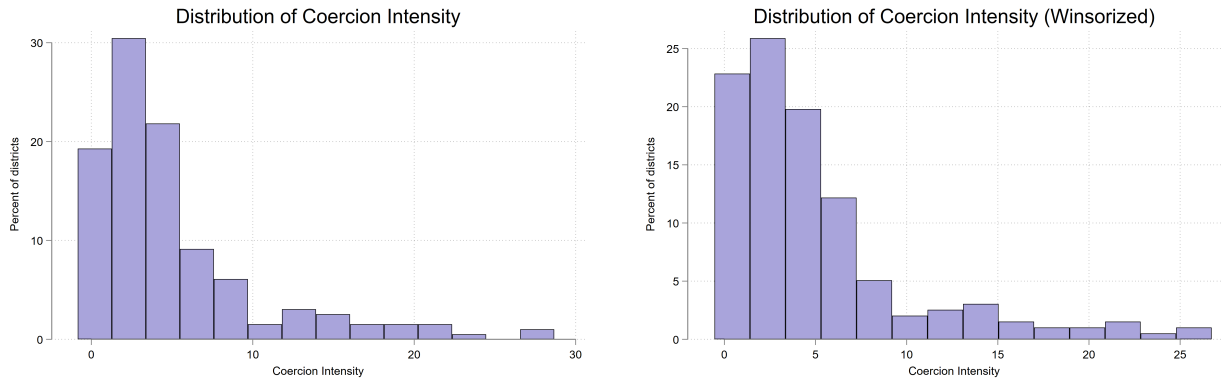
Weber, Max, *"Politics as a Vocation," 1946.*

Wheaton, Brian, *"Laws , Beliefs , and Backlash 1," Unpublished Manuscript, University of California Los Angeles, 2020.*

Appendix

Distribution of Coercion Intensity

Figure A1: Distribution of coercion intensity



(a) Raw coercion intensity

(b) Winsorized coercion intensity

Notes: The figure reports the distribution of the coercion intensity index across districts. The index is defined as the growth rate in the share of sterilized married couples between 1975–76 and 1976–77 and captures the intensity of the Emergency sterilization campaign. A value of 1 corresponds to a doubling of the sterilization share relative to the previous year. The left panel presents the raw distribution, while the right panel presents the distribution after winsorizing the coercion intensity measure at the 1st and 99th percentiles to reduce the influence of extreme observations. The winsorized measure is used throughout the main analysis.

Threats to identification

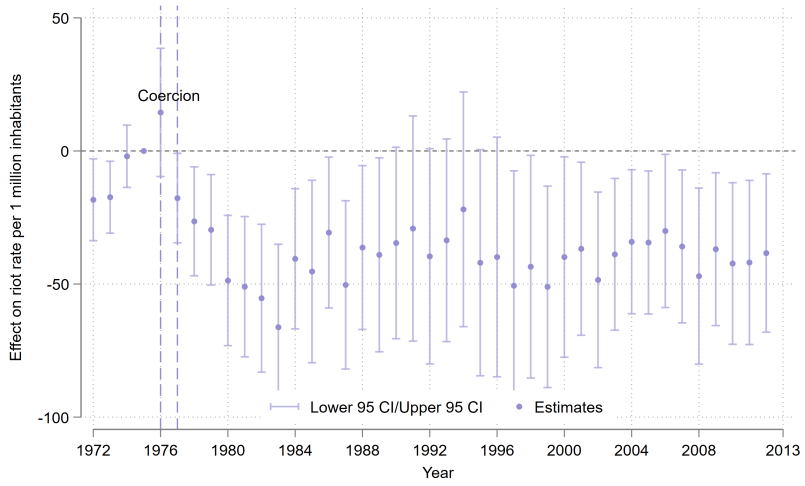
Table A1: Test of Differential Pre-Trends

	(1) Violent crimes	(2) Rapes	(3) Murders	(4) Cheating	(5) Riots
Pre-trend coefficient	1.792	0.225	0.530	-0.510	7.057***
	(1.284)	(0.193)	(0.522)	(0.674)	(2.543)
Obs.	788	788	788	788	788
R2	0.901	0.802	0.898	0.785	0.919

Notes: The table reports differential pre-trend estimates for crime outcomes in the pre-treatment period (1972–1975). The coefficient on Pre-trend captures the interaction between treatment status and relative year before 1976. Dependent variables include violent crimes, rapes, murders, and cheating (per capita, winsorized). All specifications include year fixed effects, district fixed effects, and controls for police per capita. Standard errors are clustered at the district level. *, **, *** denote significance at the 10%, 5%, and 1% levels.

Persistence

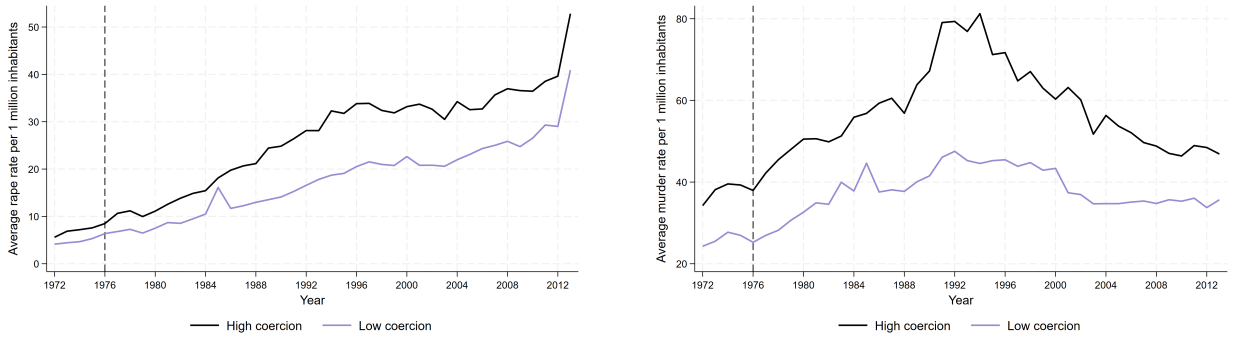
Figure A2: Event Study: Impact of coercion intensity on riots 1972-2013



Notes: This figure plots the estimated β coefficients from equation 2. The dependent variable is the number of riots per one million inhabitants. The event is centered on 1976, and the coefficient for 1975 is normalized to zero. Vertical lines represent 95% confidence intervals. The treatment group consists of districts with coercion intensity above the median value of 3.4, corresponding to approximately 4.4 times the sterilization share observed in the previous year. The control group consists of districts below the median. Each coefficient measures the difference in murders between treated and control districts in a given year relative to 1975. The specification includes district and year fixed effects, controls for state-level police force capacity, and clusters standard errors at the district level.

Evolution of Rapes and Murders: Whole period

Figure A3: Crime trends by coercion exposure



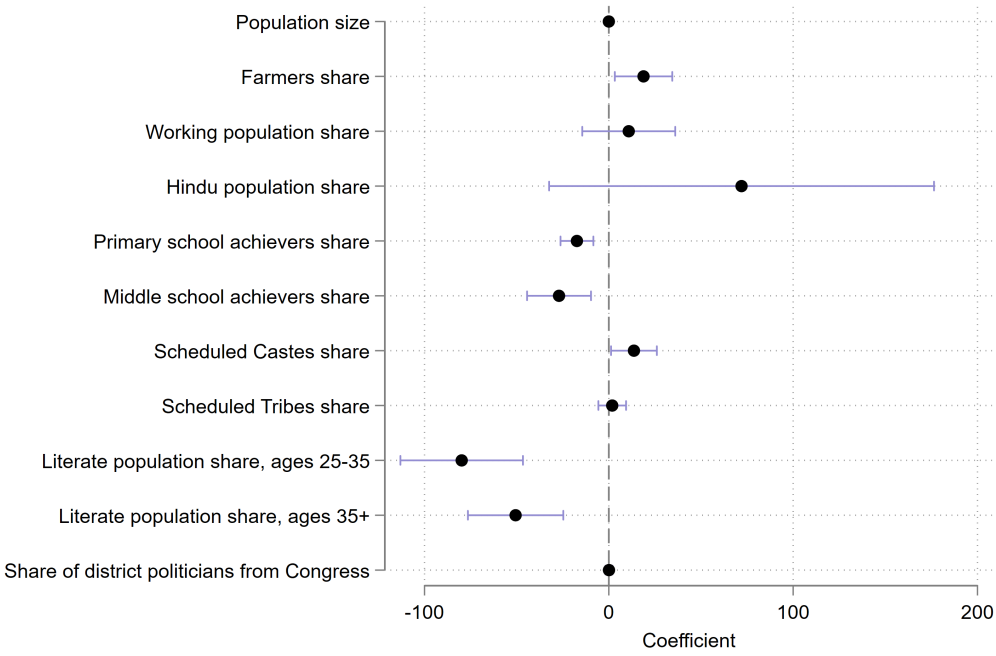
(a) Rapes

(b) Murders

Notes: This figure plots average rape and murder rates per one million inhabitants separately for districts above and below the median of the coercion-intensity distribution. The vertical dashed line marks the start of the coercive sterilization campaign in 1976.

Political and Institutional Correlates of Coercion

Figure A4: Correlates of coercion intensity



Notes: This figure reports coefficients and 95% confidence intervals from separate regressions of coercion intensity on pre-Emergency district characteristics measured prior to the Emergency. The characteristics include demographic, socioeconomic, educational, religious, and political variables drawn from the 1971 Census and the 1971 Lok Sabha elections. Standard errors are clustered at the state level. Coercion intensity is measured as the growth in sterilizations between 1975–76 and 1976–77. Districts with greater coercion intensity tended to exhibit lower literacy rates, lower educational attainment, and somewhat larger Scheduled Caste populations. While these correlations suggest that exposure was not randomly distributed across districts, the overall pattern does not point to a single dominant dimension driving coercion intensity. The robustness exercises presented later in the paper explicitly account for these potential confounders and show that the main results remain largely unchanged. .

Alternative Measures of Coercion Intensity

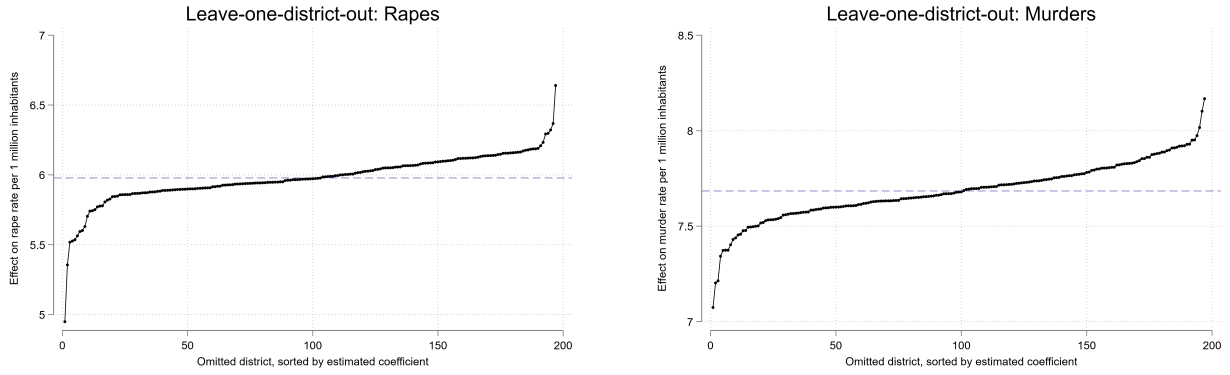
Table A2: Effect on crimes with a cutoff at the whole sample median

	(1) Total crimes	(2) Property crimes	(3) Violent crimes	(4) Rapes	(5) Murders	(6) Cheating	(7) Riots
Post × High Coercion	-156.0	-210.3***	12.50**	5.382**	8.985***	-9.984	-37.31***
	(170.8)	(54.03)	(4.891)	(2.430)	(2.130)	(7.949)	(10.96)
State police per capita	-6424.3***	-685.9***	-209.7***	-162.9***	-47.82**	141.4***	52.62
	(1166.0)	(258.0)	(38.69)	(17.63)	(20.17)	(30.02)	(65.68)
Outcome mean	2398.69	828.21	97.10	20.78	46.14	48.98	123.76
Observations	8267	5903	8267	8267	8267	8267	8267

Notes: This table reports estimates from a difference-in-differences specification using an alternative treatment definition. Districts are classified as highly exposed if their coercion intensity exceeds the median coercion intensity calculated using the full sterilization sample, including districts not present in the crime dataset. The median cutoff is 2.86. Crime outcomes are measured per one million inhabitants. All specifications include district and year fixed effects as well as controls for police personnel per capita. Additional district-level controls measured prior to the Emergency include population density, the share of Scheduled Castes (SC), the share of rural population, the share of the population with primary education, the share of the Hindu population, and the share of literate males. Standard errors are clustered at the district level and reported in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels.

Leave-One-District-Out Estimates

Figure A5: Leave-one-district-out estimates



(a) Rapes

(b) Murders

Notes: Each point reports the estimated treatment coefficient from Equation 1 after excluding one district from the sample. Districts are sorted according to the resulting coefficient estimate. The horizontal dashed line corresponds to the baseline estimate obtained using the full sample. The stability of the estimates across specifications indicates that the results are not driven by any individual district.

Additional Controls

Table A3: Robustness on crimes: Additional controls and time trends

	(1) Total crimes	(2) Violent crimes	(3) Rapes	(4) Murders	(5) Riots
Post × High Coercion	-138.5	8.928*	5.149*	8.723***	-27.46**
	(140.6)	(4.643)	(2.668)	(2.189)	(12.43)
State police per capita	-5144.9***	-130.8***	-123.7***	-16.65	-81.10
	(1013.5)	(33.02)	(15.84)	(18.27)	(76.77)
Outcome mean	2398.69	97.10	20.78	6.14	123.76
Observations	8225	8225	8225	8225	8225
Controls × linear trend	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the results of a static difference-in-differences specification as described in Equation 1. The dependent variables are total crimes, violent crimes, rapes, murders, and riots per one million inhabitants. The coefficient of interest is the interaction between a post-Emergency indicator and a dummy for high coercion districts, defined as districts with coercion intensity above the median level. All specifications include district and year fixed effects, state-level police force per capita, and interactions between pre-Emergency district characteristics (literacy rates, workers' share, farmers' share, household industry, middle school attainment, scheduled caste share, and scheduled tribe share) and a linear time trend. Standard errors are clustered at the district level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A4: Robustness on crimes: Additional controls

	(1) Total crimes	(2) Violent crimes	(3) Rapes	(4) Murders	(5) Riots
Post × High Coercion	-236.3	7.022	6.376*	6.552***	-33.89***
	(169.7)	(5.275)	(3.358)	(2.256)	(12.09)
Outcome mean	2398.69	97.10	20.78	6.14	123.76
Observations	8225	8225	8225	8225	8225
Controls × year FE	Yes	Yes	Yes	Yes	Yes

Notes: This table reports robustness checks allowing districts with different pre-Emergency characteristics to follow distinct trajectories over time. Specifically, year fixed effects are interacted with baseline district characteristics measured prior to the Emergency, including caste composition, educational attainment, religion, population density, and rurality. Outcomes are winsorized at the 1st and 99th percentiles to reduce the influence of extreme observations. The treatment variable equals one for districts with above-median coercion intensity and zero otherwise. All specifications include district and year fixed effects as well as controls for police personnel per capita. Standard errors are clustered at the district level and reported in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels.

Table A5: Robustness on crimes: Additional controls and time trends, adjusted by population

	(1) Total crimes	(2) Violent crimes	(3) Rapes	(4) Murders	(5) Riots
Post × High Coercion	-168.6	11.71**	2.242	10.55***	-30.98**
	(139.8)	(4.877)	(1.622)	(2.514)	(11.93)
State police per capita	-5919.6***	-113.4***	-123.1***	-2.598	-162.2**
	(1305.1)	(42.24)	(19.64)	(19.41)	(70.17)
Outcome mean	2398.69	97.10	20.78	6.14	123.76
Observations	8225	8225	8225	8225	8225
Controls × linear trend	Yes	Yes	Yes	Yes	Yes
Population weights	Yes	Yes	Yes	Yes	Yes

Notes: This table reports robustness checks for the main crime results. The estimates are obtained from equation 1 after controlling for baseline district characteristics interacted with a linear time trend. Baseline controls include literacy rates, occupational structure, caste composition, religious composition, population density, and rural population share measured prior to the Emergency. All specifications additionally control for state-level police force capacity and are weighted by district population. The unit of observation is the district-year. Standard errors, clustered at the district level, are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Police Capacity and Reporting

Table A6: Heterogeneity by Police Capacity

	(1) Rapes	(2) Murders	(3) Cheating	(4) Riots
High Coercion × Post	-21.665**	10.990*	24.986	-79.968***
	(8.334)	(6.009)	(16.868)	(22.979)
High Coercion × Post× Police	19.674***	-2.107	-21.124**	35.410***
	(5.691)	(3.981)	(8.887)	(12.633)
Obs.	8,267	8,267	8,267	8,267
R2	0.715	0.736	0.622	0.628

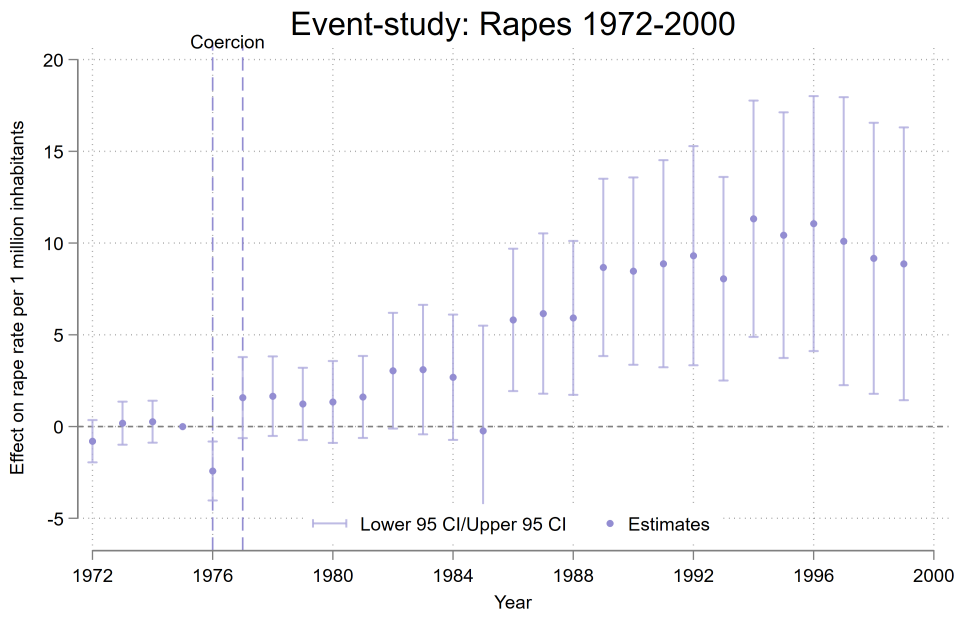
Notes: This table presents the results of estimating a triple-difference specification that interacts coercion intensity, a post-Emergency indicator, and state-level police capacity. Coercion intensity is defined as an indicator for districts above the median level of coercive sterilizations (3.4). The dependent variables are crime rates per capita. The coefficient on the triple interaction captures how the post-Emergency effect of coercion varies with police capacity. The unit of observation is a district-year. All specifications include district and year fixed effects. Standard errors are clustered at the district level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels.

Table A7: Correlation Between Coercion and Police Capacity pre-1976

	(1) Total Police Forces
High Coercion	-0.0001
	(0.0002)
Obs.	23
R2	0.002

Notes: This table reports the correlation between coercion intensity and police capacity in the pre-treatment period. Coercion intensity is defined as an indicator for districts above the median level of coercive sterilizations (3.4). Police capacity is measured as police per capita at the state level. The unit of observation is a state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels.

Figure A6: Event-study estimates for rape rates, 1972–2000



Notes: This figure reports event-study estimates of the effect of coercive sterilization on rape rates per one million inhabitants, restricting the sample to the period 1972–2000. Districts are classified as treated if their coercion intensity exceeds the median value. The omitted period is 1975. Vertical dashed lines indicate the beginning and end of the Emergency sterilization campaign. Standard errors are clustered at the district level.

Sex ratios

Table A8: Sex ratios and coercion intensity

Dep. Var. Sex ratio of the	(1) Total population	(2) 0-5 y.o in 1971	(3) 5-9 y.o in 1971
Coercion Intensity × Post	0.0007**	0.003***	0.002**
	(0.0003)	(0.0009)	(0.001)
Observations	991	966	966

Notes: This table presents the results of running the static difference-in-differences specification shown in equation 1 of coercion intensity on the sex ratios in India, defined as male relative to female. The unit of observation is a district. A positive coefficient means a decline in the population of women relative to men. *, **, and *** represent statistical significance at 10%, 5%, and 1% levels.

Individual Crime Categories

Table A9: Impact of coercive sterilizations on all crime rates 1972-2013, more exposed districts vs less exposed districts

	(1) Thefts	(2) Murders	(3) Rapes	(4) Kidnapping	(5) Dacoity	(6) Robbery	(7) Riots	(8) Burglary	(9) Cheating	(10) Counterfeiting	(11) Breach of Trust
Post × High Coercion	-94.12**	7.684***	5.978**	0.874	-11.38***	-10.97***	-32.16***	-46.43*	-4.670	-0.495*	-1.891
	(41.71)	(2.174)	(2.454)	(2.052)	(2.481)	(4.145)	(11.01)	(25.11)	(7.535)	(0.265)	(2.210)
State police per capita	-590.2**	-32.50	-151.2***	17.03	-38.45***	-102.9***	-11.47	-171.9	131.3***	1.169	20.80
	(250.8)	(19.76)	(17.77)	(16.98)	(11.95)	(23.07)	(71.59)	(120.0)	(30.89)	(1.544)	(18.30)
Outcome mean	516.89	46.14	20.78	30.21	13.17	34.77	123.76	215.61	48.98	1.44	24.67
Observations	8267	8267	8267	8267	8267	8267	8267	8267	8267	5903	8267

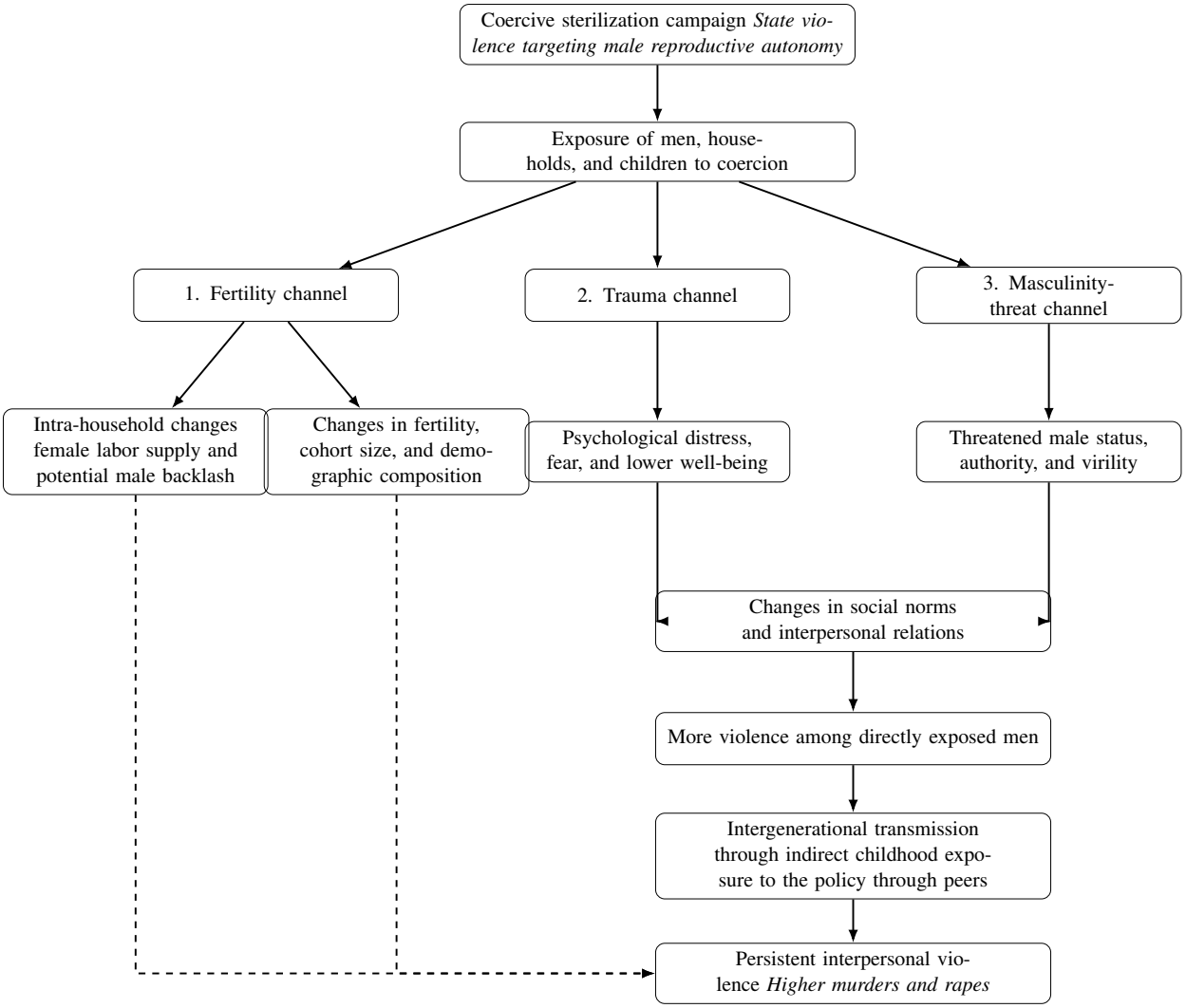
Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the results of running the static difference-in-differences specification shown in equation 1 of coercion intensity on crime rates per 1 million inhabitants. High coercion is defined at the median level of coercion intensity, 3.4 times more sterilization in a district than the previous year. The unit of observation is the district. We control for police forces at the state level. Standard errors are in the parentheses and clustered at the district level. *, **, and *** represent statistical significance at 10%, 5%, and 1% levels.

Conceptual Framework

Figure A7: Conceptual framework: coercive sterilization and persistent interpersonal violence



Notes: This figure presents the conceptual framework motivating the analysis. We distinguish between three channels through which coercive sterilization may affect interpersonal violence. The first is a fertility channel, whereby sterilization alters fertility decisions, intra-household dynamics, and demographic composition, potentially affecting violent behavior. The second is a trauma channel, whereby exposure to coercion, fear, and violence generates lasting psychological distress and lower well-being. The third is a masculinity-threat channel, whereby the campaign undermines men's perceived status, authority, and reproductive autonomy, potentially leading to backlash behaviors. The trauma and masculinity channels may alter gender norms and interpersonal relations, increasing violence among directly exposed men in the short and medium run. These effects may subsequently persist through intergenerational transmission, as children exposed to these environments internalize norms and behaviors through socialization. The framework is intended to organize the potential mechanisms linking coercive sterilization to long-run violence and does not imply that these channels are mutually exclusive. Outcomes shown in italics correspond to variables that we examine empirically in subsequent sections.

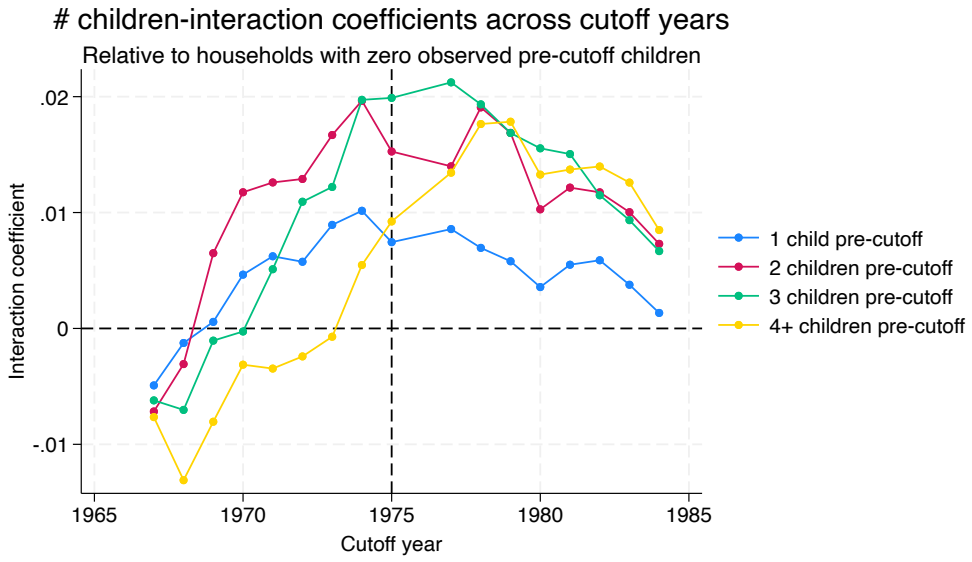
Fertility

Table A10: Summary Statistics: Maternity Survey

	N	Mean	s.d.
Household Characteristics			
Household with 1 child before 1976	54173	0.31	0.46
Household with 2 children before 1976	54173	0.23	0.42
Household with 3 children before 1976	54173	0.20	0.40
Household with $\geq$4 children before 1976	54173	0.14	0.35
Educated males	50937	0.49	0.46
Educated females	54173	0.27	0.41
Minority caste	54173	0.26	0.44
Rural	54173	0.66	0.47
Household size	54173	5.72	2.59
Mother Characteristics			
Age	54794	39.27	10.11
At least 8 years of education	54794	0.26	0.44
Husband has at least 8 years of education	54794	0.41	0.49
Number of living children	54794	2.81	1.74
Husband salaried or self-employed	54794	0.73	0.45

Notes: This table reports summary statistics for the 1986–87 National Sample Survey (NSS) maternity module used in the fertility analysis. The unit of observation is the household. Household characteristics include pre-Emergency fertility composition, education, caste, place of residence, and household size. Mother characteristics are reported for women observed in the maternity survey and include age, educational attainment, husband's education, number of living children, and husband's employment status. The sample is restricted to households containing an ever-married woman for whom fertility information is available.

Figure A8: Moving cutoff-year robustness: Fertility responses by observed pre-cutoff children



Notes: This figure reports coefficients from a series of regressions that re-estimate the main fertility specification using alternative cutoff years to define pre- and post-period child counts. The high-coercion district indicator is held fixed across specifications. For each cutoff year, the dependent variable is the number of children born after the cutoff year and observed in the 1986–87 NSS household roster. The plotted coefficients are interactions between the high-coercion indicator and indicators for the number of children observed before the cutoff year. The omitted group is households with no observed children before the cutoff. The vertical dashed line marks 1975, the start of the Emergency period. All specifications include household controls, district controls, state fixed effects, and controls for mother's age and age squared in the cutoff year. Standard errors are clustered at the district level.

Table A11: Heterogeneous effects of coercion intensity on number of births post-1976 based on the caste of the mother

	Caste group	
	Minority caste	Non-minority caste
High Coercion	-0.06	-0.10**
	(0.06)	(0.04)
One child pre 76 × High Coercion	0.06	0.07
	(0.07)	(0.04)
Two children pre 76 × High Coercion	0.13	0.23***
	(0.08)	(0.06)
Three children pre 76 × High Coercion	0.17*	0.27***
	(0.09)	(0.06)
Four children pre 76 × High Coercion	0.24**	0.14**
	(0.10)	(0.06)
Observations	14136	40265
Clusters	335	336
Hh Controls	Y	Y
Dist. Controls	Y	Y
State Fixed Effects	Y	Y
Mean of dep. var.	1.34	1.20
Joint p-value	0.116	≤ 0.001

Notes: This table presents the caste-heterogeneity results on the impact of coercion intensity on births. The unit of observation is the household. District controls include literacy rates, workers' share, farmers' share, household industry, middle school achievements, scheduled casts and tribes. *, **, and *** represent statistical significance at 10%, 5%, and 1% levels.

Table A12: Extensive Margin Fertility Results

	High coercion	Coercion intensity	Increase in sterilization
High Coercion	-0.017		
	(0.011)		
Coercion intensity		-0.002**	
		(0.001)	
Increase in sterilization			-0.003**
			(0.002)
One child pre-1976 × High Coercion	0.023*		
	(0.012)		
Two children pre-1976 × High Coercion	0.017		
	(0.015)		
Three children pre-1976 × High Coercion	0.015		
	(0.012)		
≥ 4 children pre-1976 × High Coercion	0.027*		
	(0.014)		
One child pre-1976 × Coercion intensity		0.004***	
		(0.001)	
Two children pre-1976 × Coercion intensity		0.002	
		(0.001)	
Three children pre-1976 × Coercion intensity		0.003**	
		(0.001)	
≥ 4 children pre-1976 × Coercion intensity		0.002	
		(0.001)	
One child pre-1976 × Increase in sterilization			0.003*
			(0.002)
Two children pre-1976 × Increase in sterilization			0.001
			(0.002)
Three children pre-1976 × Increase in sterilization			-0.000
			(0.002)
≥ 4 children pre-1976 × Increase in sterilization			0.000
			(0.002)
Observations	54401	54044	54173
Clusters	338	335	337
Hh Controls	Y	Y	Y
Dist. Controls	Y	Y	Y
State Fixed Effects	Y	Y	Y
Mean of dep. var.	0.21	0.21	0.21
Joint p-value	0.276	< 0.05	0.475

Notes: This table presents extensive-margin fertility results. The dependent variable is an indicator equal to one if the household has at least one child born after 1976 and observed in the 1986–87 NSS household roster. Column 1 uses the high-coercion district indicator. Column 2 uses the continuous coercion-intensity measure. Column 3 uses the increase in sterilization. All specifications interact the treatment measure with indicators for the number of children born before 1976 and observed in the household roster. The omitted category is households with no observed children born before 1976. Household controls include parental education shares, caste, and sector. District controls include the share of Scheduled Caste population, Scheduled Tribe population, urban population, and literate population. All columns include state fixed effects and controls for mother's age and age squared in 1976. Standard errors are in parentheses and clustered at the district level. The NSS does not contain complete birth histories; child counts refer to children observed in the 1986–87 household roster. *, **, and *** represent statistical significance at 10%, 5%, and 1% levels.

Table A13: Fertility Robustness Checks: Alternative Treatment Measures and Pre-Period Controls

	Pre-period fertility	Coercion intensity	Increase in sterilization	Adds pre-period growth controls	Adds pre-period increase controls
High Coercion	0.029			-0.075**	-0.087**
	(0.026)			(0.037)	(0.035)
Coercion intensity		-0.006			
		(0.004)			
Increase in sterilization			-0.016***		
			(0.005)		
One child pre 76 × Coercion intensity		0.007**			
		(0.003)			
Two children pre 76 × Coercion intensity		0.013**			
		(0.006)			
Three children pre 76 × Coercion intensity		0.022***			
		(0.006)			
Four children pre 76 × Coercion intensity		0.008			
		(0.007)			
One child pre 76 × Increase in sterilization			0.000		
			(0.005)		
Two children pre 76 × Increase in sterilization			0.002		
			(0.006)		
Three children pre 76 × Increase in sterilization			0.004		
			(0.006)		
Four children pre 76 × Increase in sterilization			0.012*		
			(0.007)		
One child pre 76 × High Coercion				0.050	0.064*
				(0.039)	(0.038)
Two children pre 76 × High Coercion				0.187***	0.205***
				(0.058)	(0.055)
Three children pre 76 × High Coercion				0.231***	0.251***
				(0.053)	(0.051)
Four children pre 76 × High Coercion				0.115**	0.151***
				(0.056)	(0.056)
Observations	54401	54044	54173	53235	54173
Clusters	338	335	337	335	337
Hh Controls	Y	Y	Y	Y	Y
Dist. Controls	Y	Y	Y	Y	Y
State Fixed Effects	Y	Y	Y	Y	Y
Alternative treatment: intensity	N	Y	N	N	N
Alternative treatment: increase	N	N	Y	N	N
Controls for pre-period growth	N	N	N	Y	N
Controls for pre-period increase	N	N	N	N	Y
Mean of dep. var.	1.58	1.23	1.23	1.23	1.23
Joint p-value		< 0.001	0.534	< 0.001	< 0.001

Notes: This table presents robustness checks for the fertility results. Column 1 uses the number of children born before 1976 and observed in the 1986–87 NSS household roster as the dependent variable. Columns 2–5 use the number of children born after 1976 and observed in the household roster. Columns 2 and 3 use alternative continuous treatment measures: coercion intensity and the increase in sterilizations. Columns 4 and 5 return to the high-coercion dummy and add controls for pre-period sterilization growth and pre-period increases in sterilization, each interacted with observed pre-1976 child-count categories. Pre-period sterilization growth is measured from 1972 to 1973, a period in which sterilizations also increased sharply. All specifications include household controls, district controls, and state fixed effects. Standard errors are clustered at the district level. Child counts are based on children observed in the 1986–87 NSS household roster, not complete birth histories. *, **, and *** represent statistical significance at 10%, 5%, and 1% levels.

Table A14: Impact of sterilization on number of births

	PPML	OLS
Sterilization share in 1977	0.000	-0.004
	(0.004)	(0.005)
One child pre-1976 × Sterilization share in 1977	-0.011***	-0.011**
	(0.004)	(0.005)
Two children pre-1976 × Sterilization share in 1977	-0.027***	-0.022***
	(0.007)	(0.007)
Three children pre-1976 × Sterilization share in 1977	-0.043***	-0.028***
	(0.009)	(0.007)
$\geq$ 4 children pre-1976 × Sterilization share in 1977	-0.026**	-0.010
	(0.010)	(0.008)
Observations	54173	54173
Clusters	337	337
Hh Controls	Y	Y
Dist. Controls	Y	Y
State Fixed Effects	Y	Y
Mean of dep. var.	1.23	1.23
Joint p-value	$\leq$ 0.001	$\leq$ 0.001

Notes: This table presents the results on the impact of sterilization intensity on the number of kids born post-1976. The unit of observation is the household. The specifications in col. (1) is estimated using Poisson & col. (2) using OLS. Household controls include the share of educated females in households, the share of educated males in households, and caste and urban sector dummies. District-level controls include the share of SC pop., ST pop., and literate pop. & urban pop. in 1971. Robust standard errors, clustered by district. *, **, and *** represent statistical significance at 10%, 5%, and 1% levels.

Gender Norms and Bargaining Power

Table A15: Intra-household violence and IPV acceptance

	Violence: Boyfriend		Violence: Other relative		Acceptance: Unfaithful		Acceptance: Goes out		Acceptance: Improper cook	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
High Coercion	0.001	0.004***	0.009*	0.006	0.100***	-0.007	0.065**	0.033	0.050**	0.010
	(0.002)	(0.001)	(0.005)	(0.006)	(0.034)	(0.034)	(0.026)	(0.030)	(0.024)	(0.028)
State FE	N	Y	N	Y	N	Y	N	Y	N	Y
Mean dep. var	0.004	0.004	0.020	0.020	0.412	0.412	0.386	0.386	0.271	0.271
Observations	5844	5844	5844	5844	27448	27448	27444	27444	27445	27445
R-squared	0.002	0.008	0.002	0.007	0.037	0.064	0.050	0.079	0.038	0.071

Standard errors in parentheses

* $p < 0.101$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports estimates from the specification described in equation 6, relating coercion intensity to measures of gender norms using the 1999 Demographic and Health Survey. Columns (1)–(4) report outcomes related to experienced violence, indicating whether the respondent has been subjected to violence by a boyfriend, and by other family members (excluding parents and siblings). Columns (5)–(10) capture attitudes toward domestic violence, indicating whether the respondent considers it justified for a husband to beat his wife if she is unfaithful, if she goes out without informing him or if she is an improper cook. A positive coefficient indicates higher reported exposure to violence or greater acceptance of such behavior. Standard errors are in the parentheses and clustered at the district level. *, **, and *** represent statistical significance at 10%, 5%, and 1% levels.

Table A16: Decision-making, mobility, work and sterilization

	Decision: Health care		Permission: Go to market		Permission: Visit relatives		Female sterilized	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High Coercion	-0.069**	-0.010	0.102***	0.008	0.062***	0.010	-0.062**	-0.038*
	(0.027)	(0.028)	(0.023)	(0.019)	(0.018)	(0.018)	(0.025)	(0.021)
State FE	N	Y	N	Y	N	Y	N	Y
Mean dep. var	0.497	0.497	0.698	0.698	0.767	0.767	0.472	0.472
Observations	27449	27449	27440	27440	27440	27440	27449	27449
R-squared	0.033	0.086	0.070	0.160	0.050	0.111	0.064	0.129

Standard errors in parentheses

* $p < 0.101$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the results of the specification described in equation 6, relating coercion intensity to measures of women's agency using the 1999 Demographic and Health Survey. Standard errors are clustered at the district level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Columns (1)–(2) report whether the woman participates in decisions regarding her own health. Columns (3)–(6) indicate whether the woman requires permission from her husband to go to the market or to visit relatives. Columns (7)–(8) report whether the woman is sterilized. For all outcomes, a higher value reflects greater participation in the activity or a higher likelihood of the outcome. Standard errors are in the parentheses and clustered at the district level. *, **, and *** represent statistical significance at 10%, 5%, and 1% levels.

Well-Being and Life Satisfaction

Table A17: Correlation between coercion intensity and individual well-being and life satisfaction

	(1) Depression	(2) Life Dissatisfied	(3) Life Not Satisfied Score	(4) Risk Behavior
High Coercion	0.007	0.017**	0.050**	-0.004
	(0.013)	(0.008)	(0.025)	(0.014)
Hh controls	Y	Y	Y	Y
Dist Controls	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Mean of dep. var.	0.16	0.11	-0.02	0.63
Observations	20956	20956	20956	20956

Notes: This table presents the correlation results between coercion intensity and individual well-being and life satisfaction of people who were between the ages of 0 to 40 during the Emergency period. The dependent variables include both binary indicators and a composite index. Depressed equals 1 if the respondent reports depression. Life dissatisfied equals 1 if self-reported life satisfaction is low (variable value ≤ 4). Risk behavior equals 1 if the respondent smokes or consumes alcohol. Life not satisfied score is a standardized index constructed as the mean of five binary indicators capturing dissatisfaction across domains (ideal life, excellent life, general satisfaction, and related measures). Each component is coded as 1 for low satisfaction (responses 1–3) and standardized (z-score) before aggregation; higher values indicate worse outcomes. All columns include individual and district controls, and state fixed effects. District controls from the year 1971 include population density, share of Scheduled Caste (SC) population, share of urban population, share of literate population, and share of agricultural population. Standard errors are in the parentheses and clustered at the district level. *, **, and *** represent statistical significance at 10%, 5%, and 1% levels.

Additional Robustness for Fertility Results: Infant Mortality

This appendix presents additional checks for the fertility results. The main analysis uses the 1986–87 NSS household roster. A limitation of these data is that they do not contain complete birth histories. Children who died before the survey or who had left the household by the survey date may not be observed. This concern is most relevant for older children born before 1976, and it may affect both the pre-1976 child-count categories and the post-1976 child-count outcome. We therefore conduct three sets of checks.

First, we assess the robustness of the NSS results directly. Appendix Table A18 shows that the estimates are robust to replacing the quadratic control for mother's age in 1976 with single-year age fixed effects and to dropping the sampling weights. The pattern remains similar: high-coercion districts have fewer observed post-1976 children among households with no observed children before 1976, while the interaction terms are positive for households with two or more observed pre-1976 children.

Appendix Table A19 addresses the possibility that older children born before 1976 are less likely to appear in the 1986–87 household roster. We re-estimate the main specification after restricting the sample to younger mothers, excluding older mothers, and adding state-by-age-bin fixed effects. These checks reduce the influence of households for which older pre-1976 children are more likely to have left the household. Across these specifications, the interaction terms remain positive for households with two or more observed pre-1976 children. The results are therefore not driven solely by older mothers or by age-related undercounting of older children.

Table A18: Robustness to Age Controls and Survey Weights

	Age FE	Unweighted
High Coercion	-0.104***	-0.058*
	(0.035)	(0.033)
One child pre-1976 × High Coercion	0.089**	0.026
	(0.037)	(0.033)
Two children pre-1976 × High Coercion	0.223***	0.147***
	(0.056)	(0.040)
Three children pre-1976 × High Coercion	0.258***	0.176***
	(0.052)	(0.043)
≥ 4 children pre-1976 × High Coercion	0.171***	0.150***
	(0.054)	(0.040)
Observations	54401	54401
Clusters	338	338
Mean dep. var.	1.234	1.234
Joint p-value	≤ 0.001	≤ 0.001
Sampling weights	Y	N
Hh controls	Y	Y
Dist. controls	Y	Y
State FE	Y	Y
Age controls	Age FE	Quadratic

Notes: This table presents robustness checks for the fertility results. The dependent variable is the number of children born after 1976 and observed in the 1986–87 NSS household roster. Column 1 replaces the quadratic controls for mother's age in 1976 with single-year mother-age fixed effects. Column 2 estimates the specification without sampling weights. High Coercion is a district-level indicator. The omitted category is households with no children born before 1976 and observed in the household roster. All specifications include household controls, district controls, and state fixed effects. Standard errors are in parentheses and clustered at the district level. The NSS does not contain complete birth histories; child counts refer to children observed in the 1986–87 household roster. *, **, and *** represent statistical significance at 10%, 5%, and 1% levels.

Second, we use NFHS-1 to examine fertility using birth-history data. Unlike the NSS roster, NFHS-1 records births reported by women in the survey. This allows us to check whether high-coercion areas show similar fertility patterns in a dataset that is less affected by the household-roster limitation. The NFHS-1 analysis includes approximately 268,000 births. Appendix Figures A9a–A9b present event-study estimates for weighted fertility measures. The estimates show that fertility rises in high-coercion areas after the Emergency period, consistent with the NSS evidence of higher subsequent fertility among households exposed to coercive sterilization. We view the NFHS-1 results as validation rather than as a replacement for the NSS analysis, since NFHS-1 was conducted later and covers women who were within the survey age range at that time.

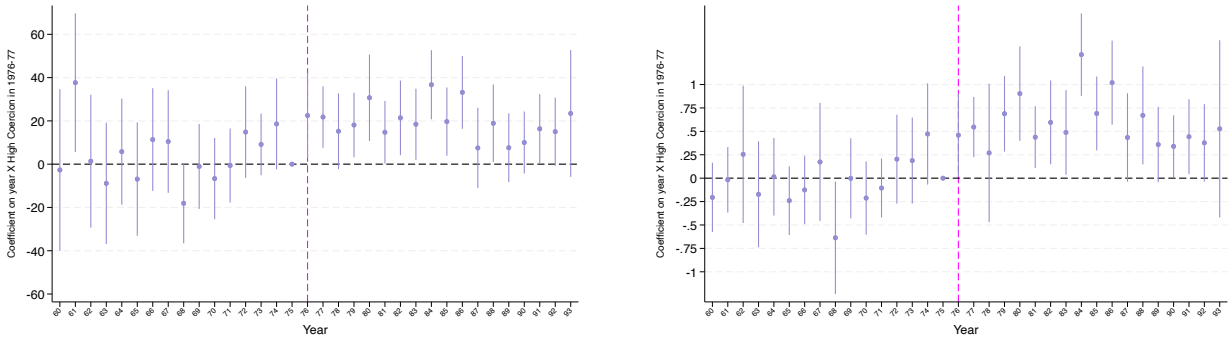
Third, we examine whether differential child survival could explain the NSS results. Since the NSS counts

Table A19: Robustness to Missing Older Children in the NSS Household Roster

	Age 14–35	Age 14–30	Excl. 45–50	Excl. 40–50	State × Age-bin FE
High Coercion	-0.115***	-0.095**	-0.097**	-0.104***	-0.064*
	(0.042)	(0.043)	(0.038)	(0.040)	(0.036)
One child pre-1976 × High Coercion	0.095**	0.074	0.072*	0.085*	0.046
	(0.047)	(0.051)	(0.041)	(0.045)	(0.036)
Two children pre-1976 × High Coercion	0.291***	0.293***	0.230***	0.268***	0.160***
	(0.072)	(0.075)	(0.059)	(0.066)	(0.057)
Three children pre-1976 × High Coercion	0.297***	0.251***	0.272***	0.290***	0.193***
	(0.067)	(0.074)	(0.054)	(0.061)	(0.052)
$\geq$ 4 children pre-1976 × High Coercion	0.281***	0.288***	0.176***	0.235***	0.095*
	(0.071)	(0.095)	(0.059)	(0.064)	(0.054)
Observations	39845	33136	49723	43833	54401
Clusters	337	337	337	337	338
Mean dep. var.	1.599	1.782	1.341	1.493	1.234
Joint p-value	$\leq$ 0.001	$\leq$ 0.001	$\leq$ 0.001	$\leq$ 0.001	$\leq$ 0.05
Sampling weights	Y	Y	Y	Y	Y
Age controls	Quadratic	Quadratic	Quadratic	Quadratic	State × age-bin FE
Fixed effects	Y	Y	Y	Y	State × age-bin

Notes: This table presents robustness checks addressing the possibility that older children born before 1976 may not be observed in the 1986–87 NSS household roster because they died, migrated, married, or formed separate households before the survey. The dependent variable is the number of children born after 1976 and observed in the household roster. Columns 1 and 2 restrict the sample to mothers aged 14–35 and 14–30 in 1976, respectively. Columns 3 and 4 exclude mothers aged 45–50 and 40–50 in 1976, respectively. Column 5 includes state-by-age-bin fixed effects. High Coercion is a district-level indicator. The omitted category is households with no children born before 1976 and observed in the household roster. All specifications include household controls and district controls. Standard errors are in parentheses and clustered at the district level. The NSS does not contain complete birth histories; child counts refer to children observed in the 1986–87 household roster. *, **, and *** represent statistical significance at 10%, 5%, and 1% levels.

Figure A9: NFHS-1 Fertility Event-Study Estimates



(a) General fertility rate

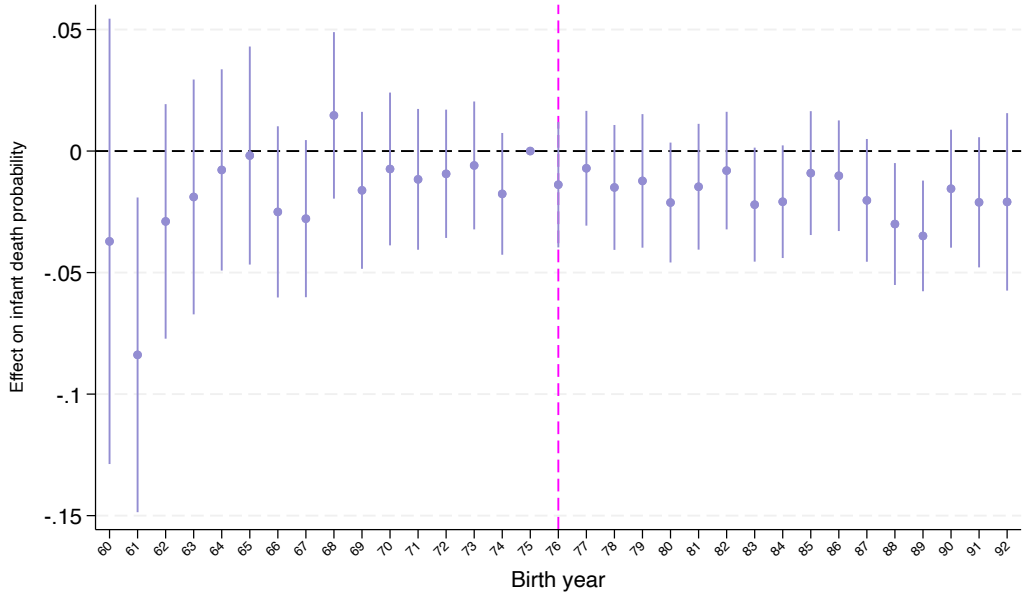
(b) Total fertility rate

Notes: This figure presents event-study estimates using NFHS-1 birth-history data for birth years 1960–1993. Panel (a) uses the general fertility rate (GFR), defined as births per 1,000 women aged 15–49 in a given year. Panel (b) uses the total fertility rate (TFR), constructed by summing age-specific fertility rates across five-year age groups from ages 15–49 and multiplying by five. TFR can be interpreted as the number of children a woman would have if she experienced the age-specific fertility rates observed in that year throughout her reproductive life. Both measures are constructed using NFHS women's sampling weights. The coefficients compare high-coercion districts to low-coercion districts in each year, relative to 1976. The vertical dashed line marks 1976. Shaded bands report 95% confidence intervals. The analysis uses approximately 268,000 births from NFHS-1. Standard errors are clustered at the district level.

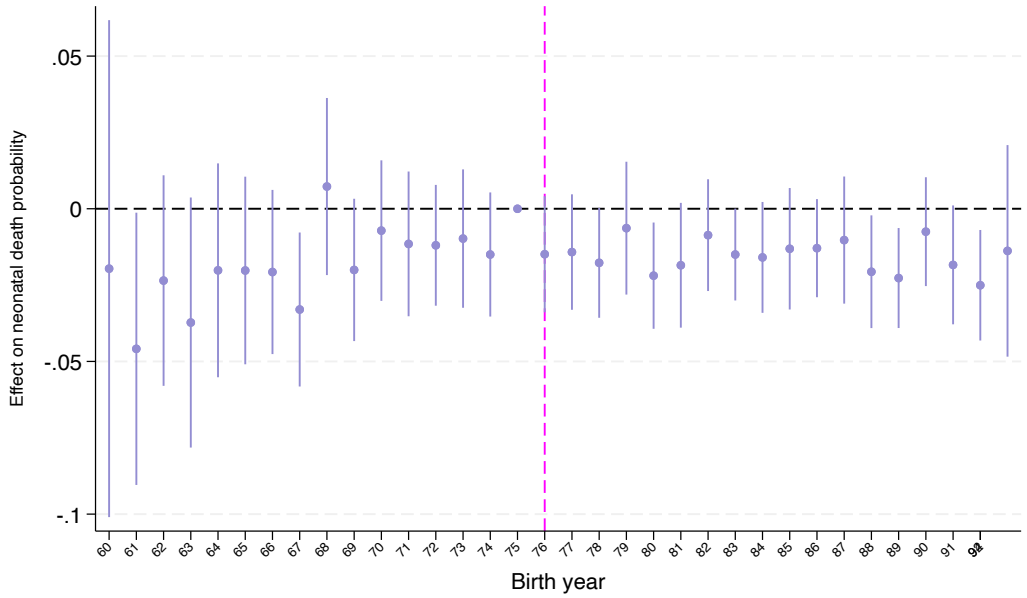
children observed in the household roster, lower mortality in high-coercion districts could mechanically increase the number of observed children even if births did not change. To assess this concern, Appendix

Figures A10a–A10c estimate event-study specifications for infant mortality, neonatal mortality, and under-five mortality using NFHS-1 birth histories. The estimates do not show a post-Emergency increase in child survival in high-coercion areas that could account for the NSS fertility results. If anything, the mortality estimates are generally negative or close to zero. This makes it unlikely that the main NSS findings are driven by differential child mortality rather than fertility behavior.

Figure A10: NFHS-1 Child Mortality Event-Study Estimates

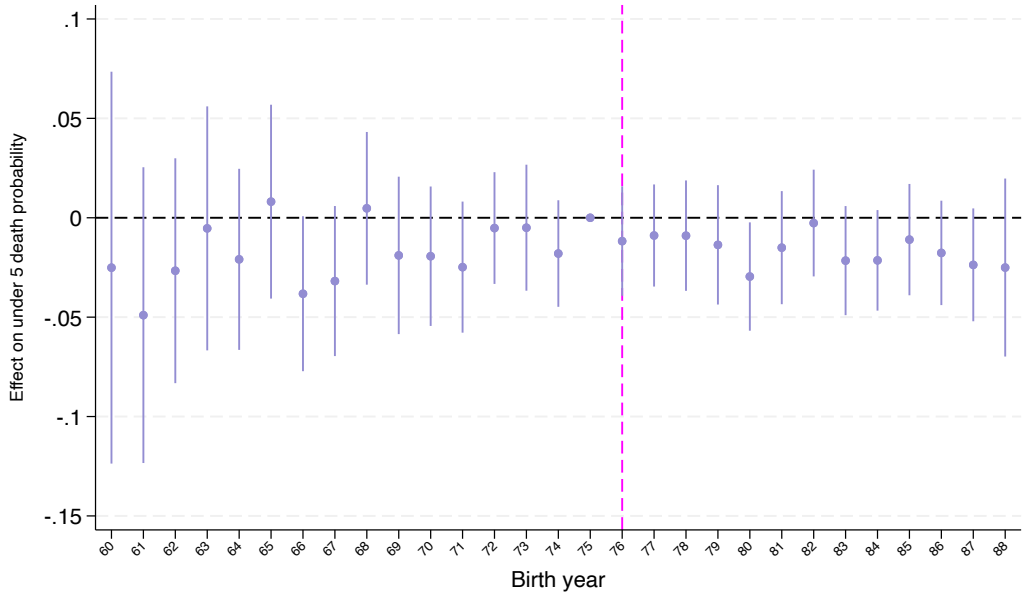


(a) Infant mortality



(b) Neonatal mortality

Figure A10: NFHS-1 Child Mortality Event-Study Estimates (continued)



(c) Under-five mortality

Notes: This figure presents event-study estimates for child mortality using NFHS-1 birth-history data. Infant and neonatal mortality are estimated for birth years 1960–1993, while under-five mortality is estimated for birth years 1960–1988 to allow children sufficient exposure time before the survey. The dependent variables are mortality indicators: infant mortality equals one if the child died before age one, neonatal mortality equals one if the child died within the first month of life, and under-five mortality equals one if the child died before age five. The coefficients compare children born in high-coercion districts to those born in low-coercion districts in each birth year, relative to 1976. The vertical dashed line marks 1976. Shaded bands report 95% confidence intervals. These estimates check whether differential child survival can explain the NSS fertility results, which are based on children observed in the 1986–87 household roster. Standard errors are clustered at the district level.

Together, these checks support the interpretation that the fertility results reflect a behavioral response to coercive sterilization exposure. The NSS estimates are robust to controls and sample restrictions designed to address the roster structure of the data, the NFHS-1 fertility results point in the same direction using birth-history information, and the mortality checks do not suggest that differential child survival explains the observed increase in post-1976 children.