When Sterilizations Lower Immunizations: The Emergency Experience in India (1975-77)

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When Sterilizations Lower Immunizations : The Emergency Experience in India (1975–77)*

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Abstract

In the 1970s in India, an aggressive family-planning program resulted in more than eight million sterilizations. We study whether this campaign affected demand for health services, specifically children's immunization and hospital births. We show that excessive sterilizations led to a substantial decrease in the use of these services, especially vaccination. If half of the sterilizations in a state were excessive (compared to none), the probabilities of receiving any vaccine and the triple antigen vaccine were respectively 17% and 34% lower for children born after the event as compared to their older siblings or older children in the same village. We find evidence suggesting the mechanism is a decline in trust rather than a change in health supply or valuation of children.

Keywords: India, family planning, sterilization, immunization, vaccination, Emergency, health care use, trust

JEL Classification: I12, I15, I18, J13, N35

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1 Introduction

Vaccination currently prevents two to three million deaths a year, and it is one of the most cost-effective ways to avoid diseases. Yet many people choose not to be vaccinated despite the availability of vaccines, to such an extent that vaccine hesitancy is now among the top 10 threats to public health (WHO, 2019). In India specifically, lack of demand for immunization is thought to be a key reason for low immunization rates (Banerjee et al., 2021).[1] In 2002 a polio epidemic spread across northern India and anecdotal evidence suggests oral polio vaccines were perceived then as a way to secretly sterilize children,[2] echoing another event in India's history: the Emergency.

A state of emergency was declared in India in June 1975, and it lasted for 21 months. Historians Jaffrelot and Anil (2021) call the Emergency a "constitutional dictatorship"—a time when India was ruled by decrees, opponents were imprisoned and tortured, and the press was censored. Yet the most memorable event was the massive sterilization campaign, in which over eight million people, mostly men, were sterilized in a few months (see Figure 1). When asked about the Emergency two decades later, many remembered it as *nasbandi ka vakt* (the time of sterilization) or equated the term "emergency" with "sterilization" (Tarlo, 2003). Such an aggressive sterilization campaign is not historically unique. In the United States, the eugenics agenda in the 20th century was advanced through forced sterilizations; in Peru, about 270,000 low-income indigenous women were forcibly sterilized from 1996 to 2000; in China, mandatory sterilizations as part of the one-child policy were sometimes enforced against women's will, and today it has been argued that sterilization is forced on the Uighurs to drastically reduce their population growth.[3]

Although the Emergency lasted less than two years, the sterilizations are still widely remembered: today, when sterilization makes the news, a parallel is often drawn with the Emergency.[4] . This

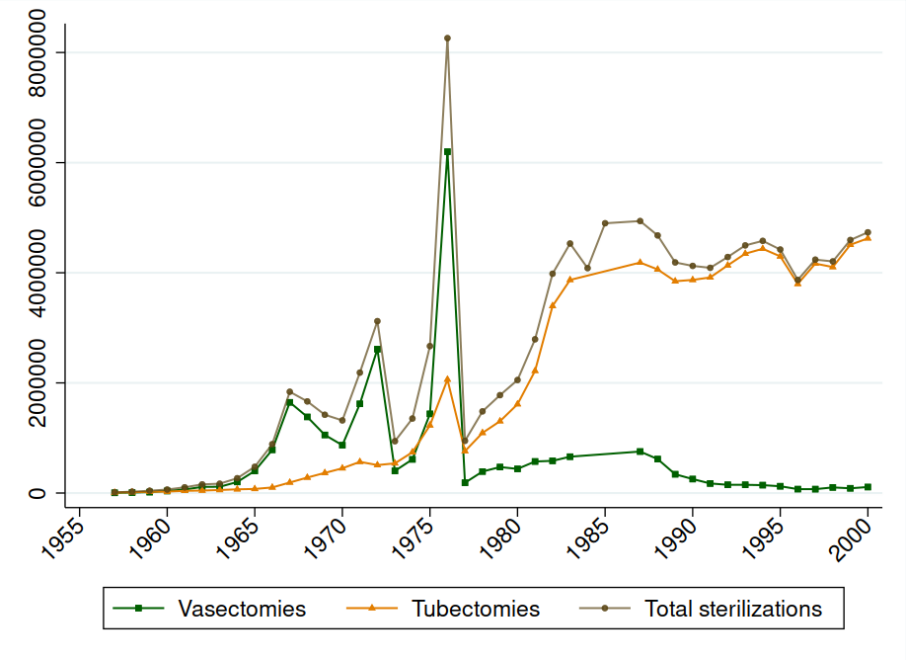
[1] The share of children between 12 and 23 years old who were fully immunized in 2015 was just 62% in India but 83% in Bangladesh (DHS). The share of children that received the DPT3 vaccine was just 78% in India but 85% globally (WHO, 2014).

[2] Amy Waldman, "Distrust Reopens the Door for Polio in India," *New York Times*, January 19, 2003. "Last year, Mrs. Jahan had heard the story circulating through her Muslim neighborhood that the polio vaccine would make her child sterile"; "The reason, according to government officials and community leaders, seems to be largely a rumor that the oral vaccine, given as drops, was part of a government population control scheme. No one knows how it started, but its effects are now clear."

[3] About the United States, see Lisa Ko, "Unwanted Sterilization and Eugenics Programs in the United States," January 29, 2016. [url] (accessed August 12, 2021) for an overview and audio-visual resources; about Peru, see Kimberly Theidon, "First Do No Harm: Enforced Sterilizations and Gender Justice in Peru," April 29, 2015. [url] (accessed August 12, 2021); about China, the documentary *One Child Nation* collects testimonies about the implementation of the one-child policy (Wang, N., and Jialing, Z. [directors and producers], and Jorg, C., Goldman, J., Clements, C., and Hepburn, C. [producers], 2019, *One Child Nation*); and on the suspected mass sterilization of Uighurs, see Conor Finnegan and Victor Ordonez, "China Conducting Mass Sterilization on Muslim Minorities That Could Amount to Genocide: Report," ABC News, June 30, 2020. [url] (accessed August 12, 2021).

[4] Soutik Biswas, "India's Dark History of Sterilisation," BBC News, November 14, 2014, written after 15 women died in a sterilization camp in Chhattisgarh. See also Amrit Dhillon, "Male Sterilisation Order Withdrawn after Flurry of

Figure 1: Sterilizations over time in India



This figure presents the evolution of the absolute number of sterilizations and the numbers disaggregated by gender. Vasectomies are male sterilizations, and tubectomies are female sterilizations. Eight million represents about 1.4% of the total Indian population in 1971. *Source:* Ministry of Health and Family Welfare, Yearbooks 1971–72 to 2001.

paper studies whether these forced sterilizations shaped demand for health services, as other historical events have. For instance, Calvi and Mantovanelli (2018) shows how Protestant medical missions in 19th-century India explain current variation in health outcomes; and Lowes and Montero (2021) find that places in former French Equatorial Africa with more colonial medical campaigns in the past are also the places where contemporary foreign medical interventions are less successful. We focus on the short-run impact of this aggressive family-planning program on demand for modern health services. If the Emergency shaped beliefs, then we expect to see a change in behavior directly after the event.

More specifically, this paper investigates the impact of the sterilization campaign on immunization of children and on hospital births in the short run, based on survey data (collected a few years after the event) and administrative data. Our main coercion variable is defined at the state level; it measures the "excessive" sterilizations performed during 1976/77 as compared to an "achievable" level defined by the central government. We use survey data on children to construct a panel of mothers and a panel of villages. Our method is a difference-in-differences design, exploiting spatial variation in intensity of coercion and exploiting temporal variation based on children's date of birth; and we add fixed effects to capture time-invariant unobservables.

Our results show that as coercion intensity increases, children born after the Emergency have a significantly lower probability of receiving vaccines or being born in a hospital than both their siblings born earlier and older children in the same village. If half of the sterilizations were excessive (compared with none), there is a decrease of 34% and 17% in the probabilities of receiving the triple antigen vaccine and receiving any vaccine after the Emergency, respectively. Given the 37 million children aged 0 to 6 in 1981 in India, it amounts to 6 million fewer children vaccinated with triple antigen. The magnitude is even larger for hospital births, although these results are slightly less robust. All these results are very stable across both mother- and village-fixed-effects specifications, and further robustness tests confirm the strength of our results. A heterogeneity analysis, motivated by the possibility of differential targeting within states, complements our results. Based on historical research describing the campaign's focus on the poor as well as intelligence reports showing that Muslims and scheduled tribes were differentially targeted, we test how our results differ depending on these two characteristics. We find our coefficients to be more precisely estimated for the poor. Using a vast minority-group sample, we find again our results, and in addition mean analysis suggests a more general decrease in use of health services. We also study distance from health care facilities on the premise that those closer to it were more easily targeted or more likely to witness the sterilizations'

Criticism," *Guardian*, February 22, 2020, about a law allowing suspension of pay for health workers for failure to achieve their sterilization target. The law was repealed after a parallel was drawn to the Emergency.

negative side effects; we find that those closer to rural hospitals had a stronger reaction. We discuss potential mechanisms, including a change in supply of health services and a change in perceived value of children due to a fertility shock. Indeed, variation in youngest children's vaccination status with respect to the intensity of the Emergency could be due to states' differential investments in the public health sector, or changes in parental investment in children post-Emergency. Instead, we find support for a trust mechanism, motivated by two findings. First, states where coercion was most intense were more likely to vote out the incumbent party in subsequent elections. Second, under the assumption that education proxies for news consumption, we find that more informed people in higher coercion-intensity states were less likely to have their children immunized than their counterparts in low-coercion states.

Our research is motivated by the literature studying health care demand and trust in medicine. The closest paper is Martinez-Bravo and Stegmann (2021), which focuses on the CIA's immunization campaign in Pakistan, undertaken to confirm Osama bin Laden's location and which led to a significant antivaccine propaganda effort by the Taliban. The authors show that this led to lower demand for modern medicine and immunization in the short term. Similarly, we estimate a short-term response to a well-identified event. Focusing on the Emergency, two recent working papers (Sur, 2021a,b) find a negative association between the coercion intensity of the sterilization program (on the one hand) and current-day vaccination rates and level of trust in government hospitals and doctors, which aligns with our own results. Our paper is the first to make a strong causal claim. Meanwhile, Alsan and Wanamaker (2018) use the disclosure of an unethical and deadly experiment (the Tuskegee syphilis experiment) to study medical mistrust and racial disparities in health and health care use. They find that the disclosure led to a decline in trust and in demand for health services from people sharing similar characteristics to those directly involved in the experiment, in turn leading to an increase in mortality for this population. Similarly, Lowes and Montero (2021) show that exposure to often-intrusive colonial medical campaigns in former French Equatorial Africa, which had negative side effects and deployed ineffective medications, is associated with lower trust in medicine today. Although we do not use direct measures of trust in medicine, our results suggests residents deemed state representatives responsible, consistent with the trust explanation.

We also contribute to the literature about the effects of family-planning programs on children's health outcomes. In their meta-analysis, Miller and Babiarz (2016) find a positive effect of family-planning programs on child mortality but no effect when family planning is not bundled with other health services. Focusing on the Programa de Salud Reproductiva y Planificación Familiar in Peru, in

which many indigenous women were forcibly sterilized between 1996 and 2000, Battaglia and Pallarés (2020) find overall positive effects of contraception on infant mortality and breastfeeding. However, when they focus on indigenous women who were sterilized, they find few benign impacts and conclude that "coercive or aggressively implemented family-planning programs may not confer health benefits on children." To our knowledge, our paper is the first to document a causal detrimental effect of a coercive family-planning program on children's immunization.

In the next section, we describe family-planning policies in India before the Emergency and put the Emergency in context. In section 3 we present our data and our coercion and outcome measures. Section 4 presents our empirical strategy based on two complementary specifications, and section 5 presents balance tests. Our main results and robustness checks are in section 6. In section 7, we present the heterogeneity analysis. In section 8, we discuss possible mechanisms. Section 9 concludes.

2 Context

2.1 Two Decades of Family Planning (1950s–1970s)

In 1951 Indian prime minister Jawaharlal Nehru announced the creation of a state-sponsored family-planning program. It was the world's first population-limitation policy, although debate on contraception or development as the best solution to limit population growth was not settled at the time in India.[5] By the end of the 1950s, it became consensual that a reduction in population growth would lead to important economic gains.In the 1940s, Princeton demographers had inverted the posited relation between population growth and industrialization by arguing that in non-industrialized countries, high fertility was impeding economic development. In the mid-1950s, the World Bank commissioned a study to model the relationship between the economy and demographics in India, resulting in a publication[6] that proved very influential among India's policy makers (Williams, 2014).

In the early 1950s, family planning followed the European-style clinic approach: people would come to clinics for advice and services. Given the approach's limited success, and based on the understanding that clinics were inaccessible, policy makers turned to the extension-education approach: people would be educated about modern birth control devices and encouraged to use them (Vicziany, 1982a); and free contraceptive devices such as condoms and diaphragms would be distributed (Ministry of Health

[5]Nehru and his Minister of Health Rajkumari Amrit Kaur, among others, expected economic growth to then limit population growth, following the demographic-transition theory, in which a country's mortality rate falls, population increases greatly, the country modernizes and industrializes, and birth rates decline (in that order) (Williams, 2014).

[6]Coale, A. J., and Hoover, E. M. (1958). *Population Growth and Economic Development in Low-Income Countries: A case Study of India's Prospects.*

and Family Welfare, 1975). In the mid-1960s, in the target-oriented and time-bound approach, the central government started assigning to the states targets to meet by providing financial incentives to people who agreed to be sterilized or have an intrauterine device (IUD) inserted. This approach lasted until 1996.

The first sterilizations started in 1956, and officials, observing the slow uptake of contraceptives in general, soon considered sterilizations (definitive contraceptive) the only long-term solution (Connelly, 2006). Although compulsory-sterilization legislation was advocated by the minister of state for family planning and by the government's Task Force on Family Planning (Gwatkin, 1979), policy makers emphasized "the desire to avoid compulsion and work exclusively by means of persuasion and incentives."[7] Introduced in 1965, the IUD was promising. But after a fast initial uptake, the public turned against the IUD because of its side effects, and uptake fell drastically (see Table A5 in appendix) (Gwatkin, 1979). In 1971 the government established the first sterilization camps in the form of temporary mobile field hospitals. Only men were sterilized there, as vasectomies are much less invasive than tubectomies and thus safer and quicker to perform (Gwatkin, 1979). Before the Emergency, 74% of all sterilizations were vasectomies (Table A5 in appendix).

2.2 The Emergency (1975–77)

The late 1960s and early 1970s were a period of political unrest and economic crisis in India. Following the Green Revolution and the ensuing agrarian crisis, peasants revolted; industrial workers frequently went on strike; and the Indian National Congress (INC), the most influential political party after India's independence, split in 1969 because of diverging views. Indira Gandhi, head of the INC, announced a leftward turn under the slogan *garibi hatao* ("abolish poverty"); the INC won the 1971 elections and Indira Gandhi became Prime Minister. The information in the rest of this section is based on Jaffrelot and Anil (2021) book except where we specify otherwise.

On June 25, 1975, the government declared a state of emergency, citing internal disorder. Gandhi's inner circle was prepared for an authoritarian turn at least a few months earlier, but one motivation for this precise timing seems to have been the verdict that Gandhi was guilty of electoral malpractice, jeopardizing her position as prime minister. On the night that the Emergency was proclaimed, 676 politicians were arrested, and over the Emergency period 140,000 people,[8] including politicians,

[7] Myrdal, G. (1972). *Asian Drama; an Inquiry into the Poverty of Nations* (Vol. 2). Pantheon, p. 893, from Vicziany (1982a). And, as Vicziany further highlights based on Myrdal's work, "Persuasion and incentives were seen by Indian politicians as an alternative to compulsion which, in the cold-war climate of the 1950s and 1960s, had become identified with the communist method of modernization."

[8] Amnesty International's estimates.

journalists, members of unions, and students, were jailed and denied trial.

Sanjay Gandhi, son of Indira Gandhi, did not hold any formal position in government but played an important role throughout the period, especially in family-planning matters. In February 1976, he complemented Indira Gandhi's 20-point economic program with his own 5-point program, including the family-planning component. The severity of the family-planning program is largely imputed to Sanjay Gandhi's personal influence, with field visits and pressure on chief ministers to encourage sterilizations. April 1976 was a turning point, when the government issued a formal statement that if a state deemed it "necessary to pass legislation for compulsory sterilization, it may do so" (Ministry for Health and Family Planning, April 1976).

The family-planning program during the Emergency was almost the same as before: each state received a target number of sterilizations and was in charge of implementing the program. However, now the central government put more emphasis on the family-planning program, putting much more pressure on public servants. All public sector workerswere incentivized to do their best to help meet the target. Often, public servants had weekly targets to meet,[9] and if they failed, they could see their pay suspended or could even be fired. Public sector workers were often pressured to get themselves sterilized, with their benefits or positions becoming conditional on how many children they had or their possession of a sterilization certificate. As a result, many non-eligible[10] and unwilling people were sterilized (Maharatna, 2002). In 1976/77 alone, more than eight million people were sterilized, largely within the first six months before Indira Gandhi announced a general election and abruptly interrupted the program in January 1977. 8 million corresponds to approximately 8% of couples in which the woman was aged 15–44 in 1971 and approximately 1.4% of India's total population in 1971. The poor were likely especially targeted, in line with the inverted demographic-transition theory: "prevent the poor from spawning poverty by sterilizing them" (Jaffrelot and Anil, 2021, p.164).

The Emergency ended in March 1977, after the INC lost the general election to a coalition that was dominated by the Janata Party and stressed "the atrocities committed during the Emergency and the malversations of the Congress' first family" (Jaffrelot and Anil, 2021, p.427). This loss came as a shock, and the "extraordinary victory of democracy over dictatorship ... became part of the mystique of India as the world's largest democracy" (Hewitt, 2007, p.13).

[9] Whom exactly received targets to fulfill varied across states. Teachers were often involved along with health-sector professionals, but in some states tax collectors or police officers among others were given targets.

[10] Non-eligible persons are unmarried persons, persons over 55 years old, and persons having fewer than two children.

3 Data and Coercion

3.1 Coercion Intensity

Data sources. Our main data source for our coercion variable is the Ministry of Health and Family Welfare's annual yearbooks from 1971/72 to 2000/2001. We were able to extract information on several categories, including targets and achievements per contraceptive method and per state, estimated number of couples using contraception ("protected couples"), and some information on health facilities.

Another relevant data source about family planning during the Emergency is the Third Report of the Shah Commission of Inquiry. In 1977 the newly elected central government appointed a Commission of Enquiry, headed by Chief Justice J. C. Shah, to investigate the "excesses, malpractices and misdeeds during the Emergency or in the days immediately preceding it" (Shah Commission, 1978). The report, finalized in August 1978, was based on thousands of documents that have been publicly available since 2011. It includes a general statement about the family-planning program and information on state-specific incentives, public servants, and complaints.

Definition. Our main coercion measure is state-level "excess" sterilizations—the difference between achieved sterilizations and the sterilization target set by the central government—divided by the sterilization target for 1976/77, when Sanjay Gandhi's influence was strongest and when about 1.4% of India's population was sterilized.

$$CoercionIntensity = \frac{Achievement_{1976/77} - Target_{1976/77}}{Target_{1976/77}}$$

The measure is at the state level because states were in charge of implementing family-planning policies and, during the Emergency, each state decided which public workers would be given targets and what incentives to introduce; further, chief ministers (states' top officials) were also under pressure, to such an extent that some risked losing their position if they did not comply with the objectives of the central government (Shah Commission, 1978; Jaffrelot and Anil, 2021). In exploiting a spatial exposure intensity, we are closer to the literature proving that direct exposure to violence matters—Bundervoet et al. (2009) on stunting rates and Bellows and Miguel (2009) on political participation—than to the literature that highlights indirect effects.[11] In other words, we exploit differences in coercion intensity and do not consider any possible national behavioral change, implying our estimates are conservative.

[11] It is possible the event was perceived as traumatizing for the country as a whole, especially given that policy making was extremely centralized. Relatedly, Silver et al. (2002) study psychological response of US citizens after the September 11 attacks and find that "psychological effects of a major national trauma are not limited to those who experience it directly, and the degree of response is not predicted simply by objective measures of exposure to or loss from the trauma."

One shortcoming of using the number of achieved sterilizations as the coercion measure is that it is contaminated by the demand for sterilization, but, for two reasons, we believe demand was likely very low. First, sterilizations in 1976/77 reached an all-time high in only a few months and in September 1976 alone, 1.7 million sterilizations were recorded—more than half of all sterilizations in the preceding year. The Shah Commission, established in 1977 to investigate the abuses committed during the Emergency, received 9,566 complaints of "compulsion and use of force in family planning program", 1,690 death reports, and 110,701 reports of non-eligible persons forced to undergo sterilization (Shah Commission, 1978).Second, Vicziany (1982a) demonstrates that the sterilization pattern before the Emergency is not consistent with models in which demand is important (classical diffusion model and demographic-transition models). In such models, the most educated and wealthy individuals are the first to adopt a new and desirable technology, after which the technology diffuses to the rest of society. But in India "the bulk of vasectomy adopters ... have been predominantly illiterate [and] poor." And when financial incentives were large or when harvests were small, sterilizations increased significantly, while when financial incentives were reduced (because of the central government's budgetary problems between 1973 and 1975), sterilizations dropped significantly, which demonstrates a strong response to monetary incentives and is inconsistent with strong and increasing demand. Hence, we believe that demand had a relatively small role to play in increasing sterilizations.

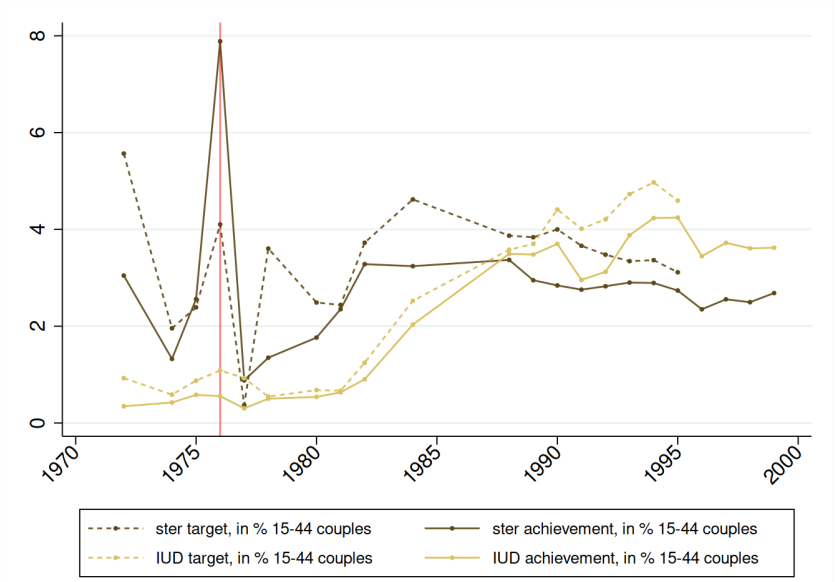
On the supply side, family planning was given the "highest priority" in the Fifth Plan (1974–79). Starting in 1966/67, the central government gave annual targets for each type of contraceptive to each state. It decided on a targeted decline of the birth rate and then split the contraceptive target among states based on a formula (Maharatna, 2002).[12] Figures 2 and A4 in the appendix present both targets and achievements for sterilizations and IUD insertions, respectively, in India and each state between 1970 and 2000. Given that the central government needed to optimize the allocation of sterilizations across states to fulfill its objective, targets represent the best available measure of the achievable level of sterilizations as perceived by the central government. We use these targets to account for different "optimal" levels of sterilization across states. Since they were computed based on a preexisting formula, they should not have been affected by the declaration of the state of emergency (further evidence is in appendix A.1).[13]

Our preferred measure is thus the deviations from state targets (or excesses) – other measures are

[12]We could not find the exact formula. We know population size, rural share of the population, financial inputs, level of economic development, female literacy, and performance gap are components of the formula.

[13]In practice, several states increased their targets and subsequently gave these new targets to their public servants. Whenever we refer to a target, we mean the original target assigned by the central government.

Figure 2: Targets and achievements (1972–98)



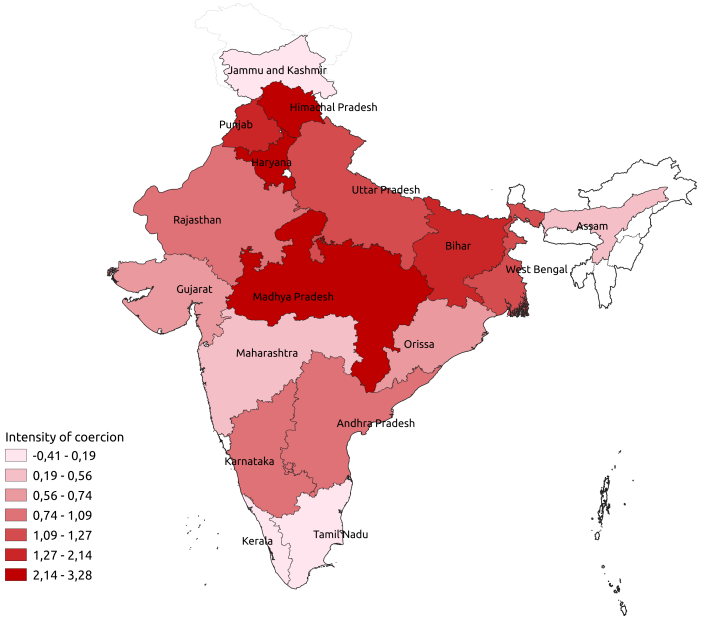
This figure presents the targets and achievements of sterilizations and IUD insertions, expressed as a percentage of couples of reproductive age (married woman between 15 and 44 years old) in each period. *Source:* Ministry of Health and Family Welfare, Yearbooks 1971–72 to 2001.

discussed later on, in the robustness tests. It is a continuous variable ranging from -0.41 to 3.29,[14] with average value of 1.08 (indicating the target was more than doubled) for the 17 largest states of India. Figure 3 presents its spatial distribution and shows that states close to Delhi have higher values on average, with Haryana's being the highest.

Reliability. Reassuringly, our measure is consistent with historical evidence that coercion intensity during the Emergency was greatest close to Delhi, and with the states of Gujarat and Tamil Nadu facing less intense coercion (Jaffrelot and Anil, 2021). Table 1 shows how our measure relates to policy measures drawn from the Shah Commission report. We focus on whether state officials acknowledged the state's use of coercion for family-planning purposes during the Emergency, the number of complaints related to family planning, the number of reported cases of sterilization of unmarried persons, the number of deaths following sterilization, and the number of complaints of arrest, detention, and abuse of power. Overall, we find more complaints and extreme cases per capita in high-coercion states.

[14] Values for two states are negative: Jammu and Kashmir (-0.41) and Kerala (-0.04). Given how we build our measure, it seemed appropriate to keep the negative values, as those who did not meet their targets did worse than those who barely achieved them. The results remain the same if we set the negative values to 0.

Figure 3: Spatial distribution of coercion measure



This figure presents the spatial distribution of coercion intensity during the Emergency, defined as the difference between achievements and target in 1976/77, divided by the target. Only major states are included.

Table 1: Coercion intensity compared to reported coercion

	Ackn. use coercion	FP compl. %	FP compl. /pop	Unmarried %	Unmarried /pop	Deaths %	Deaths /pop	Other compl. %	Other compl. /pop
Dummy variable for coercion intensity									
Low (8)	22.22	18.30	0.70	12.45	0.03	45.67	0.31	36.02	2.30
High (8)	77.78	81.70	3.08	87.55	0.18	54.33	0.36	63.98	4.02
Tercile value of coercion intensity									
Low (5)	11.11	14.70	0.91	12.26	0.04	26.33	0.29	20.67	2.13
Medium (6)	44.44	45.89	1.76	41.63	0.09	47.98	0.33	44.51	2.85
High (5)	44.44	39.41	3.80	46.11	0.24	25.68	0.44	34.81	5.58

This table presents the distribution of acknowledged use of coercion, reported cases of sterilizations performed on unmarried person, reported deaths and complaints related to family planning (abuse of power, arrests, and detentions) from the Shah Commission report with a dummy or tercile value of our main measure of coercion intensity. Two measures are provided: the share of total reported cases, and the number of cases per 100,000 people in 1971. For the latter measure, state population is aggregated by dummy or tercile value.

3.2 Survey Data

REDS 1982. To conduct our analysis, we use the 1982 wave of rural economic and demographic surveys (REDS). The ARIS[15]/REDS data set consists of a household panel complemented by cross-sectional observations to create a representative sample of the entire rural population in 1971, 1982, 1999, and 2006. As no demographic data are available from the 1971 wave, we only use the 1982 wave. It includes a demographic questionnaire—submitted to women between 15 and 50 years old in sampled households—from which we use retrospective data at the child level to construct a panel of mothers and a panel of villages with information on timing of births in relation to the Emergency.

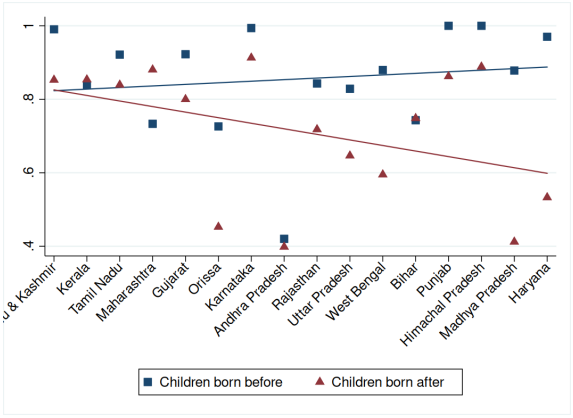
Outcome variables. Our main outcome variables are defined at the child level. The two sets of outcomes we consider are measures of willingness to use health services: vaccination status and hospital births. Immunization is a good measure of trust in the modern health sector, as vaccine efficiency is not observable by patients: the vaccine itself is not easily distinguishable by the human eye from any other substance, and its effect likely goes unnoticed by the patient, as it prevents disease rather than curing it. Information on vaccination status is available for children younger than 20 years old born of women between 15 and 50 years old. Since we have retrospective data and we use date of childbirth rather than date of vaccination, we focus on whether the child received the triple antigen vaccine, administered to children between 0 and 5 years old, and whether they received any vaccine (see discussion in appendix A.2). The latter is a valid variable only under the assumption that a child's age at the time of receiving their first vaccine is unrelated to coercion intensity, which unfortunately we cannot verify. The second type of outcome we consider is hospital births, available for a mother's last two live births. Choosing to give birth in a hospital captures trust in modern health services, and the date of birth gives precise information on realization of the outcome with respect to the Emergency period. It can also capture distrust of vaccination, as vaccines are sometimes administered right after birth in hospitals. The type of birth attendant would also have been a good outcome measure, given the high level of home births at that time in India, but unfortunately this is not available in our data.

Pooled outcomes at state level. To give an overall idea of the trends in our data, in Figure 4 we aggregate our outcomes at the state level and plot them on the y-axis with the rank of our coercion-intensity variable on the x-axis. This exercise is not ideal, as REDS data are not representative at the state level and as the number of states is low, but it gives a general idea of the trends when all children are pooled in two groups: born before or after the Emergency. Overall, for children born

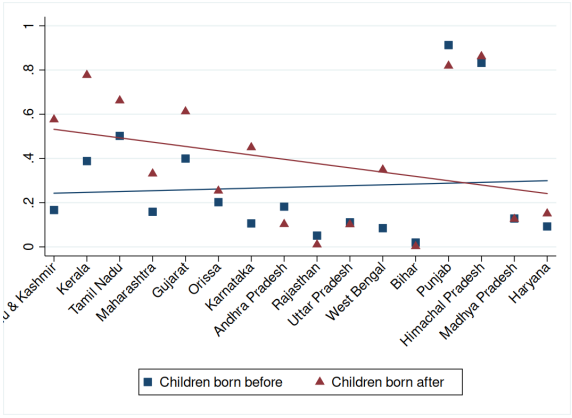
[15] Additional Rural Incomes Survey.

after the Emergency, we observe a downward slope that is especially striking for vaccine outcomes.

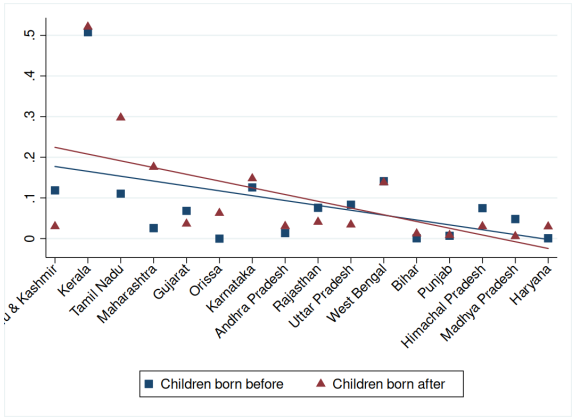
Figure 4: Coercion intensity and state-level outcomes



(a) Any immunization



(b) Triple antigen (DPT)



(c) Hospital births

Outcomes in REDS 1982 are at the state level; sample is split based on whether child is born before or after the Emergency. States are ordered based on the intensity of coercion. As REDS 1982 data are not representative at the state level, and given the low number of states, these results are mostly illustrative.

4 Empirical Strategy

4.1 Difference-in-Differences with Fixed Effects

We use a difference-in-differences strategy by comparing children born before and after the Emergency, and across levels of coercion intensity. Our identifying assumption is that but for the Emergency, the differences in trends across coercion-intensity levels would have stayed the same. To capture as many unobservables as possible, we include fixed effects in two specifications—one at the mother level, the other at the village level—and we view these specifications as mutually complementary.

Mother fixed effects. First we develop the specification with mother fixed effects. We compare, for each mother, differences in her children's immunization status depending on whether they were born before or after the event, to account for the maximum amount of time-invariant unobservables. Formally, what we estimate is

$$Y_{it} = \beta C_s * T_t + \gamma X_i + \mu_j + \delta_t + \epsilon_{it}, \tag{1}$$

where child i is born of mother j at time t in state s, Y is the outcome, T_t is a dummy for being born after April 1976, C_s is coercion intensity, X_i is controls at the child level (child's sex, child's sex interacted with the born-after dummy—to allow for potential differences in the effect of gender after the Emergency—and rank fixed effects), δ_t is year-of-birth fixed effects, μ_j is mother fixed effects, and ϵ_{it} is the error term. C_s will be absorbed in μ_j, and T_t will be absorbed in δ_t.

The specification implies that the effect will be identified solely for mothers who had children before *and* after the Emergency. This will lead to a selection bias if mothers observed in high-coercion states had different immunization preferences from those who would not have been sterilized but for the Emergency.[16] Another limitation is the validity of the coefficients for the rank fixed effects since for all observations the first child was born before the Emergency.

Village fixed effects. In the specification for village fixed effects, we compare, within villages, differences in immunization rates between children born before and after the event. This restricts the sample less than the previous specification. We no longer control for time-invariant unobservables at the mother level, but we still control for such unobservables at the village level. To increase precision of the estimates, we add controls at the household and mother levels, in addition to child-level controls already present in the previous fixed-effects specification. Formally, we estimate

$$Y_{it} = \beta C_s * T_t + \gamma_1 X_i + \gamma_2 X_j + \gamma_3 X_h + \mu_v + \delta_t + \epsilon_{it}, \tag{2}$$

where child i is born of mother j at time t in household h in state s; Y is the outcome; T_t is a dummy for being born after April 1976; C_s is coercion intensity; X is controls at the child, mother, and household levels; δ_t is a year-of-birth fixed effect; μ_v is village fixed effects; and ϵ_{it} is the error term. Child controls are child's sex, child's sex interacted with after-the-event fixed effects, and rank fixed effects. Mother controls are years of schooling, a literacy dummy, age, age squared, number

[16] In section B.4, we investigate in more depth differences across mothers based on timing of their children's birth. Overall, differences across mothers based on births timing are unrelated to coercion intensity.

of living children, and dummies for having at least one living son and for having a literate husband. Household controls are a poor-household dummy (below the 1979/80 rural poverty line), religion of head of household, caste of head of household, household size, whether the household is a nuclear family, number of children below 10, and higher level of education between a household's head and main earner.

To get a sample centered around the date of the event, we drop children older than 15 years old from the sample in the main specification (data was collected 7 years after event).

4.2 Clustering and p-Value Correction

In our main specification, we cluster standard errors at the state level. It is likely that both the regressors and the errors are correlated within each state, as states represent a very important administrative level (in charge of family-planning and health policies, for instance). And since the state is the level of definition of our treatment variable, there is clustering in the assignment, implying we should cluster at this level (Abadie et al., 2017).[17] As there are only 16 states in our analysis, we face an over-rejection bias because of the small number of clusters. Following Cameron and Miller (2015), we use a wild cluster bootstrap resampling method to obtain the correct p-value. However, with this method, we cannot directly estimate standard errors. All tables thus provide noncorrected standard errors clustered at the state level, while the bootstrapped p-value is provided separately.

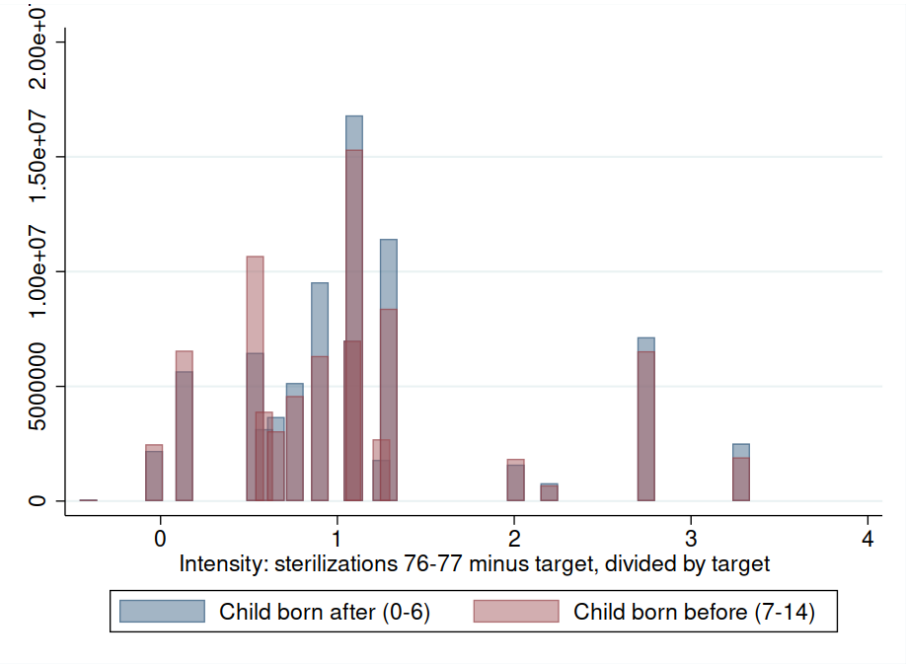
5 Balance Tests

Sample distribution. The distribution of our weighted sample can be seen in Figure 5. We use the universe of children aged 0 to 14 (the sample of our main specification), with children aged 0 to 6 being the treated children (born after the event) and those aged 7 to 14 being the controls (born before the event). In the specification with mother fixed effects, only a fraction of these children are used for the identification. Notice that a large share of observations are between 0.5 and 1. We can also see that the lowest value of our coercion variable (-0.41, for Jammu and Kashmir) is not used in the estimation. Finally, we have treated and control children for each coercion value, although the share of each group varies slightly.

Balance tables. Given the continuous nature of our explanatory variable, we perform balance tests

[17] Although the implementation and our measure are at the state level, district-level clustering may have been relevant as well. There may have been variations across districts within a state, driven, for example, by different strategies of different public servants. And access to health services may be more similar within a district than within a whole state. Our standard errors are much smaller when we cluster at the district level, so we present the most conservative values.

Figure 5: Sample distribution over treatment variable



Distribution of weighted-children sample used in main analysis with respect to coercion intensity.

by regressing coercion intensity on different sample characteristics. Formally, we test

$$Y_{is} = \beta C_s + \epsilon_{is}, \tag{3}$$

where Y is our outcome defined at level i in state s, C is our coercion variable, and ϵ is an error term. We present the weighted average and standard deviation of the variable, along with the β coefficient, the associated wild bootstrapped p-value (see section 4.2), and the number of nonmissing observations.

In Table 2, we observe clear differences in age, education, and some fertility outcomes across mothers: those living in high-coercion states are on average younger and less educated, and they marry younger and have more sons. At the child level, we see a difference in children's gender associated with coercion intensity: the gender gap in favor of sons is about 2 percentage points for a one-point increase in coercion intensity among alive children, but the gap is exclusively driven by children born before the Emergency. Children are also younger on average as coercion intensity increases, although the finding is much less striking for the children sample used in the analysis. Regarding household and village characteristics (Table A6 in the appendix), households are more often non-nuclear and slightly bigger. On average, villages are slightly more isolated from health care facilities and services in higher-coercion states.

Table 2: Balance table: mother and child characteristics

	mean	sd	coef	p-val	obs
Panel A: Mother *characteristics*					
Age	32.87	8.94	-0.66	0.02	4853
Literate	0.33	0.47	-0.14	0.06	4838
Years in school	1.59	2.93	-0.82	0.08	4841
Age at first marriage	15.15	3.14	-1.03	0.09	4777
Age at first child	19.11	3.27	-0.01	0.95	4853
Happy family is small	0.83	0.37	0.04	0.37	4797
Ideal family size	3.64	1.48	-0.09	0.23	4643
Nb living children*	3.30	1.86	0.05	0.75	4852
Nb daughters*	1.59	1.33	-0.11	0.42	4853
Nb sons*	1.72	1.30	0.16	0.06	4853
Nb pregnancies*	4.13	2.38	-0.08	0.67	4853
Nb births*	3.89	2.25	0.03	0.88	4853
Has son(s)*	0.83	0.37	0.01	0.71	4861
Husband literate	0.56	0.50	-0.09	0.21	4838
Panel B: Child *characteristics*					
Age child	11.59	7.73	-0.65	0.02	16245
Child is dead	0.17	0.38	-0.01	0.25	19597
Girl (alive children)	0.48	0.50	-0.01	0.06	16246
Girl (born-alive children)	0.48	0.50	-0.01	0.06	19000
Rank	3.20	2.06	0.00	0.96	19597
Panel B1 : born after the Emergency (0 to 6)					
Age child	3.21	1.99	-0.03	0.73	5118
Child is dead	0.10	0.30	0.00	0.92	5675
Girl (alive children)	0.49	0.50	-0.00	0.52	5118
Girl (born-alive children)	0.50	0.50	0.00	0.60	5583
Rank	3.43	2.16	-0.01	0.91	5675
Panel B2 : born before the Emergency (7 to 14)					
Age child	9.80	1.92	-0.04	0.35	5055
Child is dead	0.17	0.38	-0.01	0.43	6059
Girl (alive children)	0.49	0.50	-0.03	0.02	5056
Girl (born-alive children)	0.49	0.50	-0.02	0.01	5852
Rank	3.34	2.13	0.04	0.44	6059

Column *mean* refers to the weighted average in the full sample, *sd* the standard deviation in the sample, *coef* the coefficient associated with coercion intensity in the linear regression of characteristics on coercion intensity, *p-value* the wild bootstrap p-value of this coefficient, and *obs* the number of nonmissing observations. $*$ indicates age fixed effects were added to the regression as controls. *Happy family is small* is a dummy variable taking the value of 1 if respondent agrees with the statement "A small family is a happy family."

Table 3: Coercion intensity and immunization outcomes

	(1) Any immun	(2) Any immun	(3) TA vaccine	(4) TA vaccine	(5) Born hosp	(6) Born hosp
Intensity*After	-0.138	-0.136	-0.073	-0.070	-0.104	-0.060
	(0.041)	(0.018)	(0.034)	(0.024)	(0.057)	(0.020)
Wild boot. p-val	0.014	0.010	0.225	0.061	0.163	0.057
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes
Rank FE & Gender	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Mother	Village	Mother	Village	Mother	Village
Observations	9478	9072	9458	9052	6532	6270
R^2	0.22	0.52	0.10	0.57	0.11	0.39
Dep Var Mean *After=0*	0.84	0.83	0.18	0.19	0.07	0.07
Dep Var Mean	0.75	0.75	0.20	0.22	0.08	0.08
Identifying children	5359	9072	5347	9052	1180	6270
Identifying mothers	1527	3815	1524	3808	587	3856

Standard errors clustered at state level are in parentheses, and corrected p-value is reported below (wild bootstrapping correction). Household weights. Estimation is run using mother (even columns) and village fixed effects (odd columns) for each outcome of interest (whether the child has received any vaccine, whether they received the triple antigen vaccine, and whether they were born in a hospital). Coercion intensity is measured as the difference between achievements and target in 1976/77 divided by target in 1976/77; in our weighted sample of children 0 to 14, its mean is 1.10 and its standard deviation 0.74. *After* takes the value of 1 for children born after the Emergency (0 to 6 years old) and 0 for children born before it (7 to 14 years old for symmetrical sample). The parameter *Coercion* cannot be estimated, as it is absorbed in mother or village fixed effects, and the parameter *After* is absorbed in the birth-year fixed effects.

6 Results

6.1 Main Results

Our main results are presented in Table 3. Our coefficient of interest, the interaction term between the born-after-Emergency dummy and coercion intensity, is negative across all three outcomes: whether the child received any vaccine, whether they received the triple antigen vaccine, and whether they were born in a hospital. Point estimates across our two main specifications are extremely close to one another, especially for the vaccine outcomes. The coefficient of interest is always significant at least at the 10% level in the village-fixed-effects specification, while that is true only for one outcome (any immunization) in the mother-fixed-effects specification. Given the closeness of the coefficients, the nonsignificance in the latter specification could be due to a loss of power, as the effect is identified with half of the observations, which can be seen in the last two rows of the table. If we use the state ranking instead of the coercion value or if we use a coercion dummy based on the median coercion value, the results are still negative and even more significant as can be seen in Table 4.

Table 4: Rank and dummy for coercion intensity

	(1) Any immun	(2) Any immun	(3) TA vaccine	(4) TA vaccine	(5) Born hosp	(6) Born hosp
Coercion intensity: dummy *(median)*						
Intensity*After	-0.132	-0.202	-0.117	-0.157	-0.181	-0.110
	(0.052)	(0.047)	(0.052)	(0.043)	(0.088)	(0.035)
Wild boot. p-val	0.007	0.003	0.080	0.007	0.175	0.035
Coercion intensity: rank						
Intensity*After	-0.024	-0.028	-0.015	-0.017	-0.024	-0.014
	(0.008)	(0.004)	(0.007)	(0.004)	(0.011)	(0.004)
Wild boot. p-val	0.010	0.007	0.144	0.005	0.172	0.049
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes
Rank FE & Gender	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Mother	Village	Mother	Village	Mother	Village
Observations	9478	9086	9458	9066	6532	6277
Dep Var Mean *After=0*	0.84	0.83	0.18	0.19	0.07	0.07
Dep Var Mean	0.75	0.75	0.20	0.22	0.08	0.08
Identifying children	5359	9086	5347	9066	1180	6277
Identifying mothers	1527	3821	1524	3814	587	3861

Standard errors clustered at state level are in parentheses, and corrected p-value is reported below (wild bootstrapping correction). Household weights. Estimation is run using mother (even columns) and village fixed effects (odd columns) for each outcome of interest (whether the child received any vaccine, whether they received the triple antigen vaccine, and whether they were born in a hospital). Coercion intensity is measured as the difference between achievements and target in 1976/77 divided by target in 1976/77; in our weighted sample of children 0 to 14, its mean is 1.10 and its standard deviation 0.74. *After* takes the value of 1 for children born after the Emergency (0 to 6 years old) and 0 for children born before it (7 to 14 years old for symmetrical sample). The parameter *Intensity* cannot be estimated, as mothers' characteristics are only observed in 1982; the parameter *After* is absorbed in the birth-year fixed effects.

The magnitude of our estimates is large: when half of the sterilizations are excessive as compared to none, there is a decrease of 14 percentage points in the probability that a child born after the Emergency receives any vaccine compared to her older sibling(s) or children born before the Emergency in the same village, and there are decreases of 7.3 percentage points in the probability of receiving the triple antigen vaccine and 6 to 10 percentage points in the probability of being born in a hospital. Given that the average vaccination rate for any vaccine is 75% in our sample, while it is about 20% for the triple antigen vaccination in particular, the findings imply respectively 17% (14%) and 34% (27%) declines in these types of vaccinations as compared to the sample mean as coercion intensity increases by one (one standard deviation).

6.2 Event Study

We can also present our results as an event study to observe when exactly vaccination rates decrease under the village-fixed-effects specification. We build two-year groups to reduce the risk of overfitting, and we interact this group variable with our coercion-intensity variable. The event-study graph also allows us to check for pretrends. Our key assumption is that but for the Emergency, the difference in trends across coercion levels would have stayed the same (that is, the parallel-trends assumption). Figure 6 presents the different values of our coefficient of interest, with the 1975/76 group as the reference. Overall, the event study confirms our immunization results and makes the parallel-trends assumption more plausible. We do observe lower point estimates after the Emergency as compared to the previous period, although the results are nonsignificant with the wild bootstrapped confidence intervals. We also observe that the decrease seems to start slightly before, consistent with the idea that children do not necessarily receive vaccines right after birth. Institutional-births results are less clear, as we can observe a small peak right after the Emergency, contrary to what we expected. Combined with the low number of institutional births and the lower significance of these results in our main specification, these results appear less robust than the immunization results.

6.3 District-Level Measure

States were in charge of implementing family-planning policies, and, during the Emergency, states designed specific incentives and the chief ministers (states' top officials) faced heavy pressure to achieve their targets (Shah Commission, 1978; Jaffrelot and Anil, 2021). However, the district level might be meaningful if it more precisely captures personal exposure. Considering the popular narrative of excesses by overzealous public workers, the district level might be more precise in capturing decision-makers' individual actions given that this level is also administratively important. There are still two important limitations in using a district-level measure: sterilization data are not fully available; and targets are set at the state level, apparently with no fixed rule (at least not one in common across all states) for assigning district targets within states. We found information for about half districts of our sample, but quite selected: low-intensity states are overrepresented, and children are on average more vaccinated and more likely to be born in hospitals. To rebuild our measure, we estimate the state target to be divided across districts based on population only.

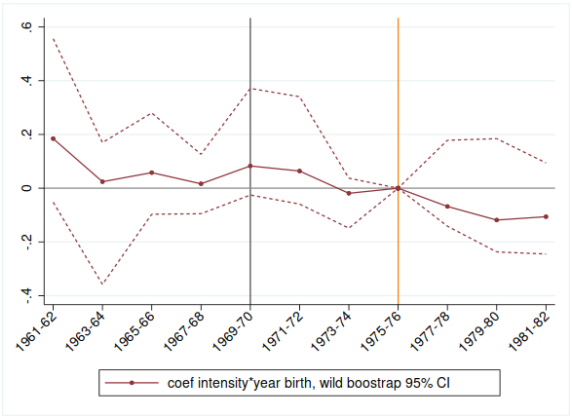
Table 5 presents the results: first with the full sample of our main measure, as in the main estimation; then with the sample restricted to the sample for which we have sterilization information at the district level (district sample), while keeping the state-level coercion intensity measure; and

Table 5: District coercion intensity and immunization outcomes

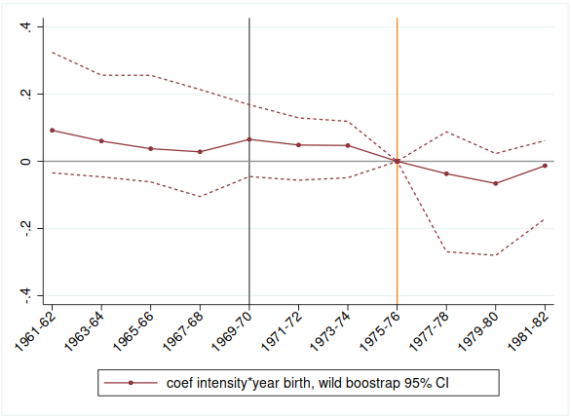
	Any immunization			Triple Antigen vaccine			Born in hospital		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: mother *fixed-effects specification*									
Intensity*After	-0.138	-0.058	-0.026	-0.073	-0.059	-0.038	-0.104	-0.060	-0.024
	(0.040)	(0.029)	(0.015)	(0.038)	(0.052)	(0.027)	(0.060)	(0.044)	(0.025)
P-value	0.001	0.054	0.095	0.060	0.260	0.177	0.088	0.177	0.342
Av. coercion val.	1.11	0.81	0.98	1.11	0.81	0.98	1.11	0.81	0.98
Observations	9478	3951	3951	9458	3945	3945	6532	2840	2840
Identifying mothers	1527	628	628	1524	627	627	587	259	259
Panel B: village *fixed-effects specification*									
Intensity*After	-0.136	-0.087	-0.047	-0.070	-0.085	-0.058	-0.060	-0.053	-0.028
	(0.036)	(0.026)	(0.012)	(0.026)	(0.036)	(0.013)	(0.026)	(0.032)	(0.020)
P-value	0.000	0.001	0.000	0.010	0.025	0.000	0.054	0.152	0.236
Av. coercion val.	1.10	0.79	0.93	1.10	0.79	0.93	1.10	0.79	0.93
Observations	9072	3789	3789	9052	3783	3783	6270	2723	2723
Identifying mothers	3815	1630	1630	3808	1629	1629	3856	1679	1679
Coercion level	State	State	District	State	State	District	State	State	District
Sample	All	District	District	All	District	District	All	District	District
Dep Var Mean *After=0*	0.83	0.84	0.84	0.19	0.32	0.32	0.07	0.10	0.10
Dep Var Mean	0.74	0.81	0.81	0.22	0.39	0.39	0.08	0.14	0.14

Standard errors clustered at district level are in parentheses, and p-values are in brackets. Household weights. *After* takes the value of 1 for children born after the Emergency (0 to 6 years old) and 0 for children born before it (7 to 14 years old). Coercion intensity is based on the difference between achievements and targets either at the state or district level; for the district level, targets are inferred based on population allocation within states.

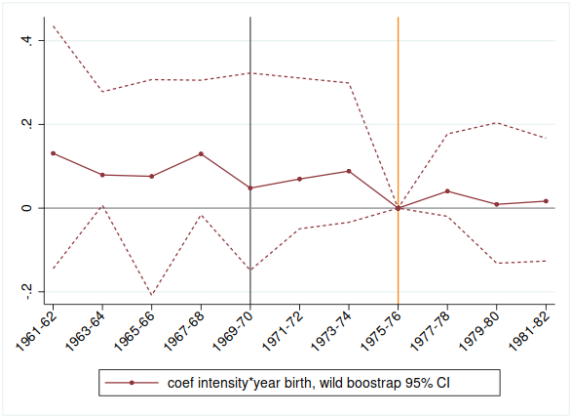
Figure 6: Event study with age groups



(a) Any immunization



(b) Triple antigen vaccine



(c) Born in hospital

Event study under village-fixed-effects specification, using two-year age groups. Each point is the value of the coercion intensity interacted with each age group. Confidence intervals are built with wild bootstrapping to correct for the small number of clusters; such intervals are not necessarily centered, as they are based on an empirical distribution. The orange line represents the timing of the Emergency, while the gray line delimits the before sample used in the analysis.

finally we use the district-level measure for the district sample. Standard errors are clustered at the district level across all specifications. Results show the interaction between coercion intensity and the born-after dummy is always negative, and the coefficients in the village specification are highly significant for vaccine outcomes when we use the district-level measure.

6.4 Robustness

Our results are robust to different tests.

Breastfeeding as placebo outcome. To test whether our results reflect mothers' reluctance to use the modern health sector, and not a change in their behavior towards children's health , we use

breastfeeding as a placebo outcome. We find no change in duration of breastfeeding (detailed results in section B.1 in appendix).

Different coercion measure. In section B.2 in the appendix, we discuss other measures of coercion intensity, the main one being the state-wide increase in sterilizations from the previous year. Using this measure, coefficients associated with the different outcomes are always negative, and the coefficients are very significant for our two immunization outcomes, although their magnitudes are slightly lower.

Different sample for TA vaccine. An important underlying assumption is that the timing of immunization does not vary with coercion intensity. Since the triple antigen vaccine is only supposed to be administered to children aged between 0 and 5, in section B.3 we estimate our coefficients with a sample constrained to children who where at least 4 years old both at the date of the survey and at the time of the Emergency. The coefficients are very stable, but the standard errors increase.

Mothers' characteristics. In section B.4, we analyze mothers' characteristics in relation to coercion intensity and birth timing. We focus on two sets of characteristics: fertility, and opinion and knowledge. Across all characteristics, mothers sometimes differ in the birth date of their children, but they do not differ more between high-coercion and low-coercion states. This ameliorates the concern that our results would reflect a selection bias if mothers who were not sterilized had different immunization preferences than those who were sterilized, although this concern was already mitigated since point estimates are extremely close to one another in the mother- and village-fixed-effects specifications.

7 Heterogeneity Analysis

Next, we test for heterogeneous effects along two main dimensions, on the premise that certain groups may have been more targeted: poor people or minorities, and people closer to health infrastructure. We split the sample for each characteristic studied (creating dummies) because it leads to a more straightforward interpretation and allows us to present the sample size and mean for each group directly. Doing so implies that controls might differently affect outcomes for the two groups, which we do not believe is a problem in this case. We focus on the village-fixed-effects specification because of the larger number of identifying observations; the results for mother fixed effects are in section C.3 in the appendix.

7.1 Poor People and Minorities

A common ideology connected fertility, poverty, and economic growth, and policy makers likely focused sterilization efforts on the poor (see section 2.2). If the poor were indeed differentially targeted, we expect their reaction to immunization to be stronger. We construct a poverty dummy variable from consumption per capita which takes a value of 1 if consumption per capita in the household is below the 1979/80 rural poverty line (for all of India). Historical evidence also indicates a special focus on Muslims and scheduled tribes (Williams, 2014), implying that minority status may be relevant as well. We build a minority dummy variable equal to 1 if the household head is non-Hindu or belongs to a scheduled caste or tribe and 0 otherwise. Results are presented in Table 7. In Panel A, in which we split the sample by poverty status, the coefficients are more precisely estimated for the poor. In Panel B, coefficients are still negative. And interestingly, for non-minority, children born after the Emergency are more vaccinated with triple antigen and more often born in hospitals as compared to the overall sample, while for minority it is reversed, suggesting a general decline in the use of these services. A similar trend is also observed for any immunization, as the gap between the two means is wider for minority as compared to non-minority. One possible interpretation is that the Emergency reduced health care demand overall for people belonging to a minority group, which we can't capture in our estimates.

7.2 Health Care Facilities

Testing how results vary with distance to health care facilities is also important, as people closer to facilities may have been more exposed to sterilization efforts if the facilities had to perform sterilization operations for recurrent achievement of targets (weekly or monthly), given that sterilization camps are by definition temporary.[18] Those closer to facilities may also have experienced more side effects of complicated sterilizations if some patients were brought in for urgent care. To study such heterogeneity, we build a health-facility dummy taking a value of 1 if a village is within five kilometers of a primary health center (PHC) or rural hospital and 0 otherwise. Based on this rationale, we expect those closer to facilities to react more strongly. Table 7 presents our results. Because sample sizes are quite unbalanced, the interpretation is not straightforward for distance to PHCs: magnitudes vary slightly but not all in one direction, and increased precision for any immunization coefficient may be driven solely by sample size. In contrast, even with unbalanced sample sizes, the magnitude of the coefficient of interest for those within five kilometers of a rural hospital is larger, consistently with our conjecture.

[18] However, based on the evidence we have, we cannot exclude the possibility that during the Emergency, camps stayed open for long periods in any location.

Table 6: Heterogeneity for poor people and minorities

	Any immunization		TA vaccine		Born in hospital	
	(1)	(2)	(3)	(4)	(5)	(6)
	No	Yes	No	Yes	No	Yes
Panel A: Poor						
Intensity*After	-0.156	-0.100	-0.073	-0.067	-0.022	-0.111
	(0.050)	(0.018)	(0.033)	(0.021)	(0.029)	(0.055)
Wild boost. p-val	0.037	0.002	0.094	0.049	0.566	0.214
Observations	4951	4181	4939	4173	3583	2732
Av. coercion	1.20	1.04	1.20	1.04	1.20	1.04
Dep Var Mean *After=0*	0.85	0.81	0.26	0.15	0.10	0.04
Dep Var Mean	0.74	0.74	0.27	0.18	0.11	0.06
Panel B: Minority						
Intensity*After	-0.155	-0.065	-0.051	-0.059	-0.064	-0.041
	(0.025)	(0.020)	(0.031)	(0.027)	(0.021)	(0.028)
Wild boost. p-val	0.010	0.028	0.129	0.119	0.037	0.259
Observations	5760	2600	5752	2592	4038	1744
Av. coercion	0.98	1.25	0.98	1.25	0.98	1.25
Dep Var Mean *After=0*	0.84	0.79	0.19	0.23	0.07	0.08
Dep Var Mean	0.78	0.69	0.24	0.21	0.09	0.06

Standard errors clustered at state level are in parentheses, and corrected p-values are reported below coefficients (wild bootstrapping correction). Household weights. Coercion intensity is measured as the difference between achievements and target in 1976/77 divided by target in 1976/77; average coercion value in each group is reported in the group columns. *After* takes the value of 1 for children born after the Emergency (0 to 6 years old) and 0 for children born before it (7 to 14 years old for symmetrical sample). *Poor* takes the value of 1 if a household's consumption per capita is below the 1979/80 rural Indian poverty line and 0 otherwise. *Minority* takes the value of 1 if the household head is non-Hindu or belongs to a scheduled caste or tribe and 0 otherwise. Regressions using village fixed effects include the same controls as the main regressions (see section 4.1).

Table 7: Heterogeneity based on distance to health facilities

	Any immunization		TA vaccine		Born in hospital	
	(1)	(2)	(3)	(4)	(5)	(6)
	More 5km	Less 5km	More 5km	Less 5km	More 5km	Less 5km
Panel A: Primary Health Center						
Intensity*After	-0.160	-0.093	-0.025	-0.105	-0.068	-0.061
	(0.049)	(0.080)	(0.025)	(0.063)	(0.029)	(0.027)
Wild boost. p-val	0.027	0.329	0.330	0.168	0.111	0.133
Observations	5113	2634	5098	2630	3521	1813
Av. coercion	1.21	0.92	1.21	0.92	1.21	0.92
Dep Var Mean *After=0*	0.80	0.84	0.16	0.24	0.05	0.05
Dep Var Mean	0.73	0.79	0.16	0.31	0.06	0.09
Panel B: Rural Hospital						
Intensity*After	-0.119	-0.283	-0.035	-0.153	-0.078	-0.006
	(0.023)	(0.033)	(0.022)	(0.037)	(0.025)	(0.023)
Wild boost. p-val	0.013	0.027	0.082	0.029	0.067	0.817
Observations	6497	1340	6477	1340	4480	958
Av. coercion	1.09	1.00	1.09	1.00	1.09	1.00
Dep Var Mean *After=0*	0.80	0.89	0.20	0.19	0.06	0.17
Dep Var Mean	0.74	0.78	0.21	0.32	0.07	0.17

Standard errors clustered at state level are in parentheses, and corrected p-values are reported below coefficients (wild bootstrapping correction). Household weights. Coercion intensity is measured as the difference between achievements and target in 1976/77 divided by target in 1976/77; average coercion value in each group is reported in the group columns. *After* takes the value of 1 for children born after the Emergency (0 to 6 years old) and 0 for children born before it (7 to 14 years old for symmetrical sample). Regressions using village fixed effects include the same controls as the main regressions (see section 4.1).

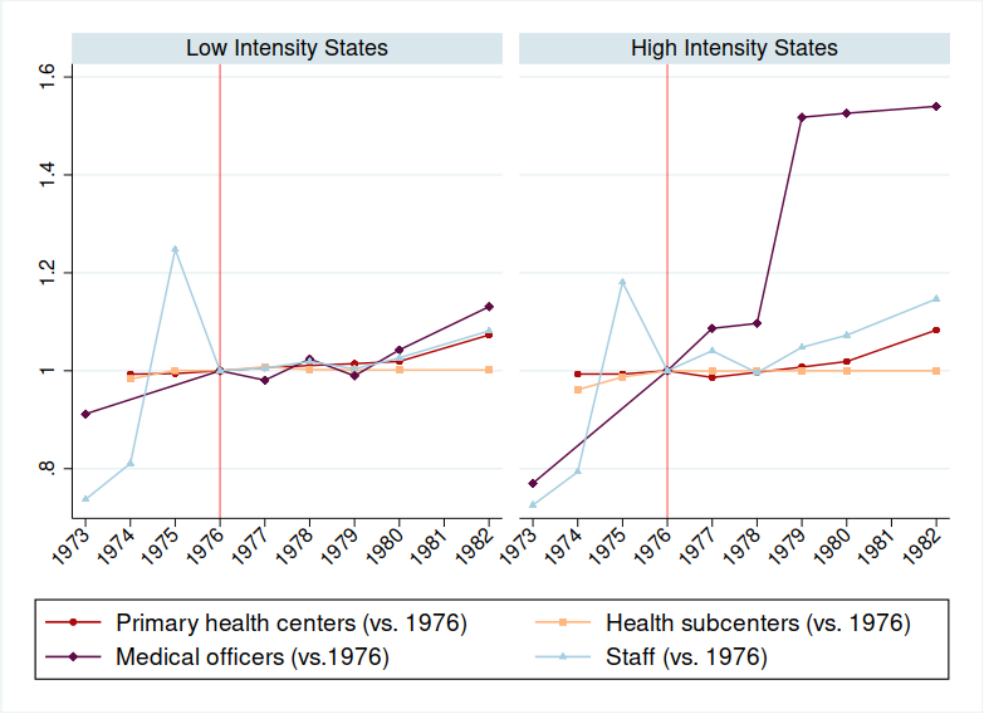
However, these results should be considered carefully: the two groups are highly unbalanced, and it appears most gains in immunization occurred in villages close to rural hospitals.

8 Mechanisms

8.1 Supply

One important source of omitted variable bias would be if health care supply was lower after the Emergency in high-coercion states than low-coercion ones, as we cannot capture time-variant characteristics through mother or village fixed effects. This possibility would imply that what we observe is the result of a change not in demand but supply. Since we use data collected shortly after the event, such a change would need to have happened relatively quickly to be a major issue. Still, in

Figure 7: Public health care supply across time



This figure presents the evolution of health facilities (primary health centers and health subcenters) and employed staff (medical officers and all staff) across time for high- and low-intensity states, where groups are based on the median value of our main coercion variable. Variations are obtained by taking the average in each group for each variable and comparing it to the value in 1976 to capture the evolution. *Source:* Ministry of Health and Family Welfare, Yearbooks 1971–72 to 2001.

Figure 7 we use Ministry of Health and Family Welfare annual yearbooks to study variation in health care provision. Overall, high-intensity states do not seem to have seen a larger decrease in health care supply on the two main measures available in these yearbook data: facilities and staff. If anything, the opposite is true. The sole potentially important difference lies in the number of PHCs. It seems their numbers increased in low-intensity states sooner after the Emergency than in high ones. Yet the number of health subcenters increased more in high-intensity states right before the Emergency than in low-intensity states, possibly signifying different strategies in the choice of facilities.

However, there might be unobservables we cannot account for. For instance, public servants in charge of immunization after the Emergency may have been afraid to go places where family planning was coercive during the Emergency, but we cannot observe this.

8.2 Demand: Fertility and Value of Children

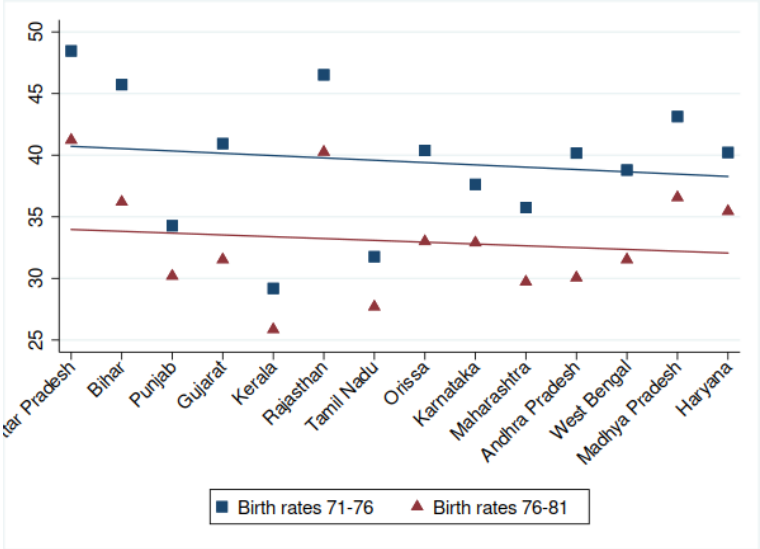
It could be that changes in immunization of children were driven by a perceived change in the value of children due to a fertility shock. But was there a fertility shock? Or in other words, did the government achieve its objective of reducing fertility? Given the high number of sterilizations (1.4% of the Indian population in 1971), it should have. Under official guidelines, only people who already had two or three children (depending on the state) should have been sterilized. Given that the average number of births per woman in 1971 was 5.5, strict enforcement of the guidelines thus should have reduced overall fertility.[19] Yet the post-Emergency period was followed by a tremendous backlash on family-planning matters that lasted for at least a few years, during which time "family planning became a dirty word[20] "(Maharatna, 2002), possibly leading to higher fertility rates due to unmet contraceptive needs.

Our survey data do not allow us to formally study this mechanism, but two administrative data sources do shed light on changes in aggregate fertility. First, Figure 8 presents crude birth rates for the periods 1971–76 and 1976–81 at the state level (Census of India, 1985), with the x-axis representing a ranking of the states by number of sterilizations divided by number of married couples in which the women are aged 15 to 45 ("eligible couples"). The figure shows a clear decrease in birth rates in all states but does not show any birth-rate differences related to the number of sterilizations. In other words, if the eight million sterilizations in 1976/77 concentrated in high coercion states induced a change in fertility, the effect is not large enough to be detected with crude birth rates. Second, Figure 9 presents uptake of contraceptives across time, based on administrative data from the Ministry of Health and Family Planning (Figure A5 in the appendix presents state-level trends). We can see a small decrease in uptake of all types of contraceptives right after the Emergency, including sterilizations, followed by a trough. The pattern occurs in virtually all states (although sometimes without an initial decrease). This is in line with Basu (1985), who finds that the Emergency led to a shift in government family-planning policies that put the birth control burden on women only.Today, tubectomies are the most common contraceptive in India, and fully three in eight men think contraception is the responsibility of women only (NFHS, 2015). To sum up, we do not see variation in birth rates, despite the high number of sterilizations. One probable reason is the decreased intensity of family-planning policies. Thus, we do not believe that the Emergency resulted in a fertility shock, at least not at the aggregate level.

[19] Qualitative evidence shows that non-eligible people—for instance, people older than 55 years old—were sterilized, as were unmarried men and people having fewer than two children. The guidelines were thus not always strictly enforced.

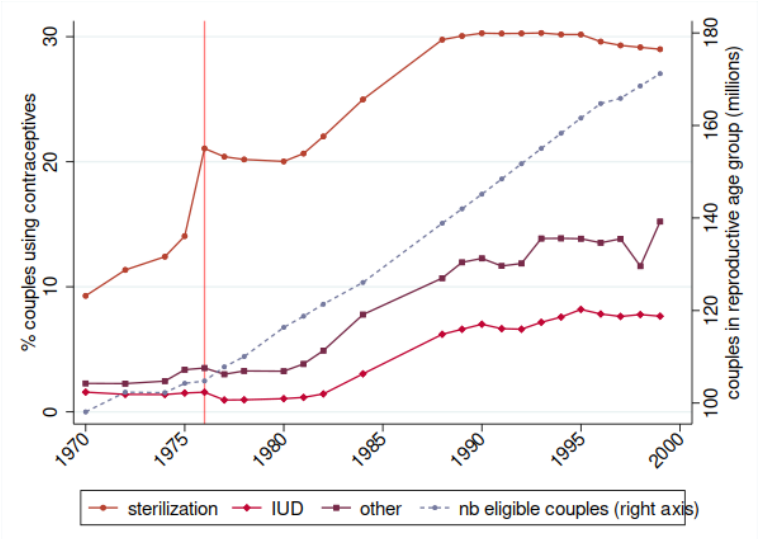
[20] Karan Singh, minister for health and family planning from 1973 to 1977.

Figure 8: State birth rates



This figure presents birth rates for 1971–76 and 1976–81 using census data coupled with the reverse survival method from the sample registration system. States are ranked by absolute number of sterilizations divided by number of eligible couples in 1976/77. Trends are also parallel if we use the value rather than the rank. *Source:* Census of India 1981, *Estimates for vital rates for the decade 1971–81*, 1985

Figure 9: Contraceptive use over time



This figure presents the percentage of couples using contraceptives, including any contraceptive method and specifically sterilization (left axis). It also present the general evolution of the number of eligible couples (married women aged 15-44) across time (right axis). *Source:* Ministry of Health and Family Welfare, Yearbooks 1971–72 to 2001.

Yet it is possible that a shock hit middle-aged mothers in particular, especially since the family-planning program during the Emergency was targeted at couples who already had several children. If that was the case, quantity-quality trade-off theory predicts immunization of children would have increased, but we find the opposite. Furthermore, in section B.1 we do not find variation in duration of breastfeeding, which would signify variation in the perceived value of children. For these two reasons, we do not believe a change in the perceived value of children is driving our results.

8.3 Demand: Breach of Trust

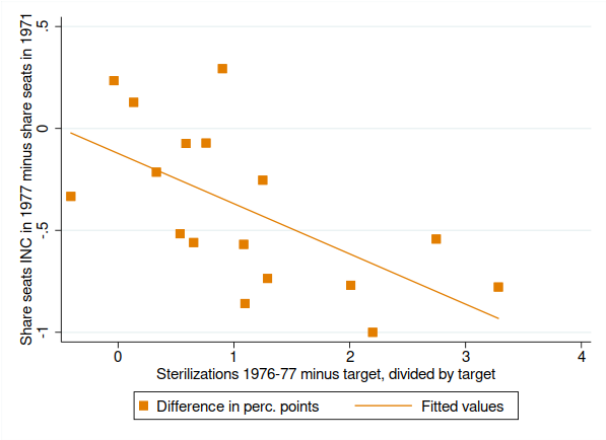
Political outcomes. Another interpretation is that the coercive policy generated distrust, reducing demand for health services. We know from existing literature that extreme events can lead to such outcomes, and anecdotal evidence suggests it could be the case here as well. We conjecture that since the Emergency is today widely and bitterly remembered for its family-planning component, a measure capturing abuses in sterilization must be reflected in such discontent and in turn in mistrust. Unfortunately, we cannot test this conjecture directly, as we have no trust measure in our data. We thus use political outcomes as a measure of trust in the government, on the premise that if attitudes toward policy makers changed, attitudes regarding health services may also have changed.

We know the aggressiveness of the family-planning program was critical to Prime Minister Gandhi's defeat in the 1977 elections. An analysis of 400 of India's leading newspapers' publications in the six weeks before the elections finds that 274 articles treat the family-planning program as a problem (Gwatkin, 1979), and external observers such as the World Bank saw a very direct relation between the family-planning program and election outcomes.[21] To test this conjecture, we compare (i) the share of seats won by the incumbent party (INC) in 1977 and (ii) voter turnout between 1971 and 1977 (Figure 10). We can see a steep negative slope for number of seats, suggesting that the INC indeed lost the largest number of seats where coercion was most intense. We can also see a positive (although less striking) relationship for voter turnout, suggesting stronger political mobilization in more intensively coercive states. To adopt a longer-term perspective, we estimate the following equation using the share of votes as the outcome variable:

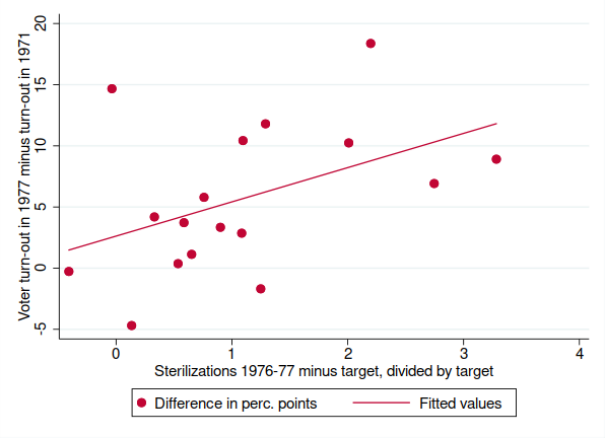
$$Y_{st} = \beta C_s * Elec_t + \mu_s + \delta_t + \epsilon_{st}$$

[21] "The most visible consequence of this increased political support for family planning was a dramatic increase in the number of sterilizations (8.3 million in 1976–77 compared with 1.4 million in 1975–76). These results, however, were accompanied in some cases by exertion of undue pressure by overzealous workers, which became an issue in the elections of March 1977 which led to a change of Government" (World Bank, 1983).

Figure 10: Coercion intensity and electoral outcomes: 1971 vs. 1977



(a) Share of seats obtained by INC



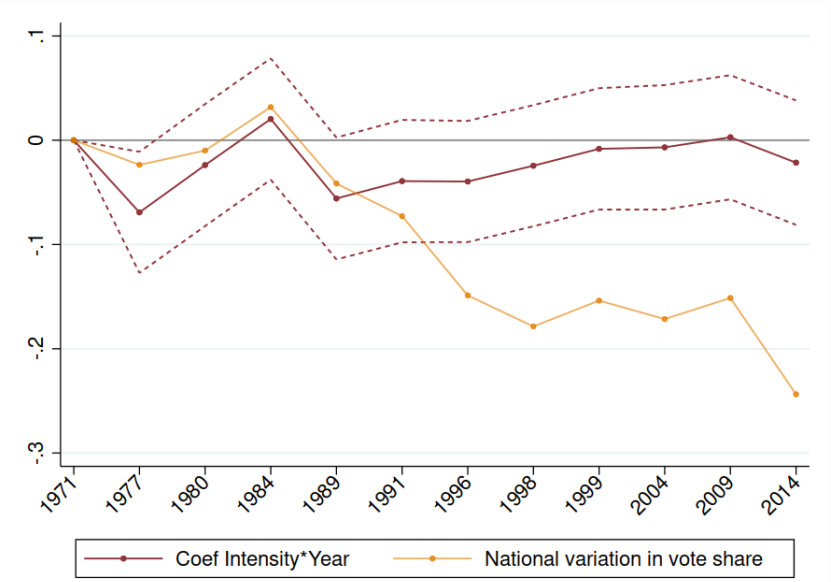
(b) Voter turnout

Differences in (a) the share of seats obtained by the incumbent party, Indian National Congress (INC), and (b) voter turnout in each state between the 1971 and 1977 Lok Sabha (Parliament of India) elections. *Data source:* Electoral Commission of India.

Figure 11 displays values of β over time, along with the national variation in vote share across time. We can see that the decline in the INC's vote share in 1977 was driven by coercion intensity, but this effect did not recur, except perhaps in the 1989 elections, which were marked by a corruption scandal directly implicating the prime minister, Rajiv Gandhi.

Information (education) channel. Although sterilizations during the Emergency did trigger an important electoral reaction, and they are widely remembered to this day, about 1.4% of the total population was sterilized in 1971, meaning more than 98% were not directly impacted, and yet the magnitude of our coefficients suggests a large impact. Information may thus have been an important channel: to react despite not being directly affected, people need to have been aware of what was

Figure 11: Coercion intensity and INC vote share over time



This figure presents the coefficient of the interaction term between coercion intensity during the Emergency and an election dummy to explain variation in vote share of the Indian National Congress (INC) at state level (with state and election fixed effects). *Data source:* Electoral Commission of India.

happening. To proxy for information, we use education level, on the premise that more educated people are more likely to consume news.[22] We use several education variables: mothers' literacy, husbands' literacy, and whether the household's head or main earner has a higher education level than primary school. Overall, education seems to matter, especially for the triple antigen vaccine: the more intense the coercion, the less likely are more educated people to have their younger children immunized. If more educated people are more likely to have been informed, then our results suggest that informed people reacted more to what occurred in their state. The results could also imply that we underestimated the effect of the Emergency, as it may have induced a national change in behavior via information diffusion.

9 Conclusion

In this paper, we investigated whether the coercive sterilization campaign in 1976/77 during the state of emergency led to a decrease in demand for modern health services. Our main result is that children born after the Emergency were increasingly less likely to be immunized than their older siblings or older children in the same village as coercion intensity increased. The result is very robust and can be

[22]We cannot test this directly. We only know that on average, literate mothers are about two times more likely to say they know a source of advice or distribution for family planning.

Table 8: Heterogeneity based on education

	Any immunization		TA vaccine		Born in hospital	
	(1)	(2)	(3)	(4)	(5)	(6)
	No	Yes	No	Yes	No	Yes
Panel A: Mother literacy						
Intensity*After	-0.112	-0.196	-0.027	-0.189	-0.029	-0.070
	(0.024)	(0.066)	(0.023)	(0.074)	(0.013)	(0.049)
Wild boost. p-val	0.056	0.013	0.444	0.069	0.105	0.284
Observations	6182	2890	6173	2879	4168	2102
Av. coercion	1.24	0.84	1.24	0.84	1.24	0.84
Dep Var Mean *After=0*	0.84	0.81	0.17	0.25	0.04	0.13
Dep Var Mean	0.72	0.79	0.17	0.30	0.04	0.16
Panel B: Husband literacy						
Intensity*After	-0.095	-0.186	-0.027	-0.125	-0.026	-0.041
	(0.018)	(0.043)	(0.020)	(0.038)	(0.013)	(0.024)
Wild boost. p-val	0.025	0.017	0.287	0.070	0.082	0.113
Observations	3211	5861	3208	5844	2209	4061
Av. coercion	1.23	1.00	1.23	1.00	1.23	1.00
Dep Var Mean *After=0*	0.73	0.91	0.15	0.23	0.03	0.10
Dep Var Mean	0.66	0.81	0.15	0.27	0.03	0.12
Panel C: Head or main earner higher than primary education						
Intensity*After	-0.105	-0.165	-0.031	-0.149	-0.066	-0.007
	(0.021)	(0.048)	(0.016)	(0.055)	(0.026)	(0.041)
Wild boost. p-val	0.018	0.016	0.097	0.146	0.111	0.809
Observations	5786	3292	5772	3286	3973	2301
Av. coercion	1.15	1.01	1.15	1.01	1.15	1.01
Dep Var Mean *After=0*	0.80	0.88	0.16	0.26	0.04	0.14
Dep Var Mean	0.71	0.82	0.16	0.32	0.05	0.13

Standard errors clustered at state level are in parentheses, and corrected p-values are reported below the coefficients (wild bootstrapping correction). Household weights. Coercion intensity is measured as the difference between achievements and target in 1976/77 divided by target in 1976/77. *After* takes the value of 1 for children born after the Emergency (0 to 6 years old) and 0 for children born before it (7 to 14 years old for symmetrical sample). Regressions using village fixed effects include the same controls as the main regressions (see section 4.1).

observed with various coercion measures. We attribute the result to an increase in distrust in health services in general due to the coercion, although we could not test the conjecture directly.

As seen in section 2.1, the international community was involved from the very early stages of family-planning policies in India, through its conceptual framework that motivated the policies and through its financial aid. In 1975/76, India received $9.39 billion in aid compared to $4.2 billion on average in the three preceding years, largely to finance the family-planning program (Jaffrelot and Anil, 2021). From administrative records, we observe that during the Emergency, about one-tenth of the total family-planning budget came from direct foreign assistance (Ministry of Health and Family Welfare, 1978). And in 1981 Prime Minister Indira Gandhi was the first recipient of the United Nations Population Award, rewarding "the most outstanding contribution to the awareness of population questions or to their solutions" (Population Council, 1983). Thus, to some extent our results could also be considered an unintended consequence of aid intervention.

Outside the scope of the research question, we conclude by calling attention to the event itself, the massive sterilization of millions of (largely poor) people. As climate change is upon us, discussions about population control may gain momentum again, and it is important to remember that throughout history, the burden was often disproportionately carried by populations far removed from the decision-makers.

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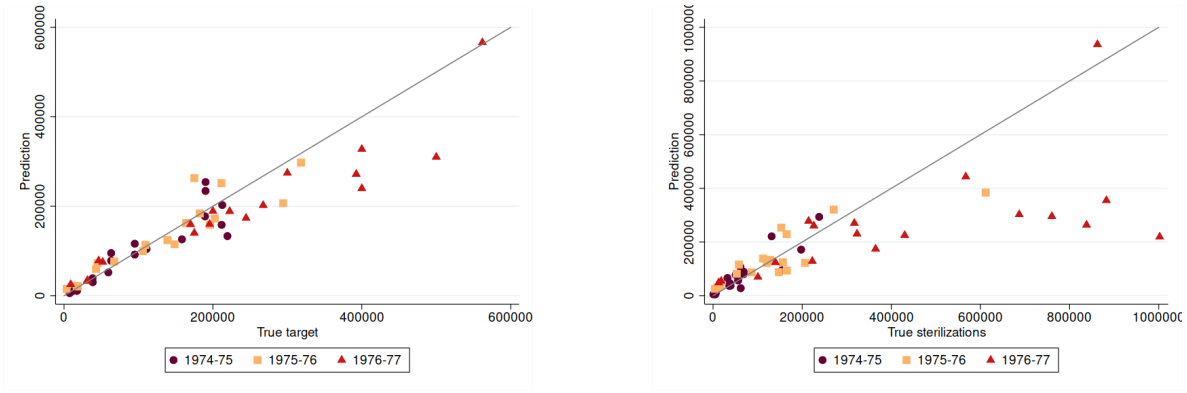
Figure A1: Targets and sterilization predictions



(a) Targets (b) Sterilizations

This figure presents predicted as compared to true values separately for the targets and for actual sterilizations; 1974/75 and 1975/76 values are used to predict 1976/77 values based on variables reported to have entered the central government's formula for assigning state targets.

A Details on Variables

A.1 Nonmanipulated Targets

One key element of our measure is the target, which we consider a good proxy for a "good" level of sterilizations as opposed to "excessive" sterilizations. One limitation of the measure is our limited knowledge of the formula for assigning targets: we only know it includes population size, rural share of the population, financial inputs, level of economic development, female literacy, and performance gap from previous periods. To make sure these components are good predictors, and to test whether we have reason to believe there could have been manipulations of the states' target at the time of the Emergency, we predict our 1976/77 target using the targets from the two previous years. We use normalized measures of the above variables, we run an OLS regression,[23] and we use the estimated parameters to predict the target value. We do the same with actual sterilizations. In Figure A1, we can see that the prediction is more accurate for targets than sterilizations. This is reassuring.

A.2 Immunization Variables

To determine whether children were less likely to be vaccinated after the Emergency, knowing the date at which they received vaccines would be ideal. However, we only have children's vaccination status, which we combine with their age to obtain a proxy for the date of vaccination. This limits the number

[23]The data are extracted from the yearbooks. Based on OLS regression, projected population and previous year's sterilizations are clearly the most important factors.

of vaccines we can study for two reasons. The first reason relates to the introduction or abolition of certain vaccines: if vaccines are introduced after the Emergency, then any vaccinated children must have been vaccinated after the Emergency. This is the case for polio vaccines, available since 1979/80 in urban areas and shortly after in rural areas (Sokhey et al., 1989). The opposite is true of smallpox, which was eradicated in 1978, so children were not vaccinated against this virus in the most recent period.[24] The second reason concerns the age at which a child is vaccinated. The tuberculosis vaccine was administered to children up to 19 years old until 1981/82, when experts recommended vaccinating children below 2 years old (Sokhey et al., 1989); given the large age range before 1982, date of birth is not a good proxy for vaccination date. The triple antigen vaccine, which is very similar to today's DPT vaccine, is less problematic on both fronts. This vaccine was promoted for children up to 5 years old in the early 1970s (Sokhey et al., 1989); the age limitation implies that we can exploit the timing of birth. Finally, we still use the measure of whether a child has received any vaccine at all. Although it is less precise and possibly subject to the problems described above, it is the most general measure and the one capturing most directly a child's exposure to any immunization.

[24] In the data, some youths are still declared to be vaccinated against smallpox, possibly reflecting measurement error or confusion of different vaccines.

Table A1: Placebo outcome: duration of breastfeeding

	(1) Breastfed	(2) Breastfed	(3) Breastfed only	(4) Breastfed only
Intensity*After	-0.165	-1.155	0.629	0.600
	(0.973)	(0.997)	(0.692)	(0.505)
Wild boot. p-val	0.880	0.424	0.426	0.326
Birth year FE	Yes	Yes	Yes	Yes
Rank FE & Gender	Yes	Yes	Yes	Yes
Fixed effects	Mother	Village	Mother	Village
Observations	6428	6182	6481	6229
R^2	0.38	0.55	0.17	0.45
Dep Var Mean *After=0*	22.93	23.32	10.90	11.05
Dep Var Mean	19.17	19.62	9.93	10.10
Identifying children	1151	6182	1173	6229
Identifying mothers	573	3829	584	3843

Standard errors clustered at state level are in parentheses, and corrected p-values are reported below (wild bootstrapping correction). Household weights. The outcomes are duration of breastfeeding in months (any breastfeeding and exclusive breastfeeding).

B Robustness Tests

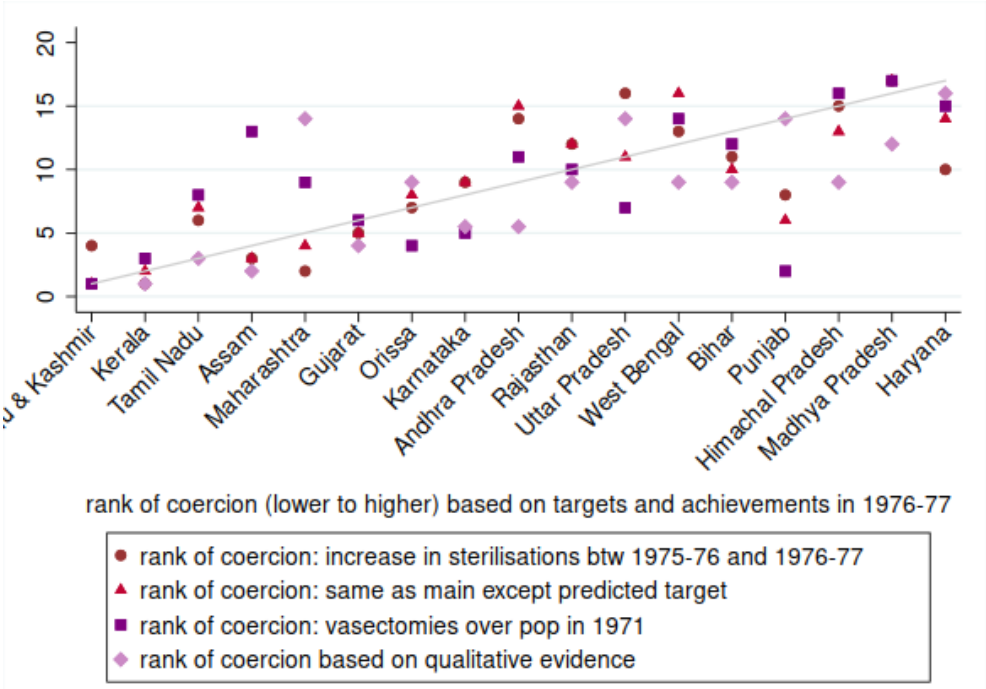
B.1 Placebo Outcome: Breastfeeding

Our objective is to see whether the Emergency caused reluctance to use health services. An ideal placebo outcome would be related to children's health care but not to the modern health sector. We use length of breastfeeding, available for a mother's last two births. Our placebo test is successful: in Table A1, we can see that none of the coefficients are significant, suggesting no change related to coercion intensity for duration of breastfeeding, which we know to be important for young children's health.

B.2 Other Coercion Measures

As detailed in section 3.1, our coercion-intensity measure is designed to capture excess sterilizations, where the formula-based targets are assigned by the central government to each state. In this section, we discuss other coercion-intensity measures, all defined at the state level. First, following Sur (2021a,b), we build an intensity measure based on the increase in sterilizations over the previous year, 1975/76, with the goal still being to capture excesses, except now the comparison is to the previous year rather than the achievable sterilization level. This is not our preferred measure, because it is highly

Figure A2: Comparison of coercion measures



Different coercion variables are reported here for comparison purposes. Given their different natures, rank is the most straightforward comparison. Rank of coercion on the x-axis runs from lower (left) to higher (right), and the gray line is the 45° line. The different coercion variables are (i) main coercion-intensity variable, based both on sterilization achievements and targets, (ii) the increase in sterilizations over the previous year, (iii) the difference in revised or nonrevised targets in 1976/77 compared to original targets in 1975/76 divided by number of couples in which the wife is aged 15-45 in 1974, and (iv) the absolute number of vasectomies performed in 1976/77 over population in 1971.

dependent on a one-year realization and because the target itself includes past performances, along with other dimensions. Second, we build an intensity measure based on predicted targets, rather than actual ones. Third, we build a measure based solely on vasectomies to arrive at a more straightforward measure of sterilizations per capita, as we know demand was close to null (Vicziany, 1982a). This measure is defined as the absolute number of vasectomies in 1976/77 divided by population size in 1971. Compared with the approach based on excess sterilizations, it is less relevant because the family-planning trajectories differed across states in the years before 1976/77, but the different nature of the variable makes it worth studying. Finally, we build an intensity index based on the qualitative evidence in the Shah Commission report. This measure includes whether public servants faced pay cuts or termination for not fulfilling their quotas, whether teachers and tax collectors were motivators, whether police were motivators, whether large-scale resistance occurred, and whether the state submitted a low target for forced sterilization.

Figure A2 presents the ranking of these other coercion variables, compared to the ranking of the main one. Given the variables' different natures, comparing rank is the most straightforward. Figure A3 presents their spatial distributions. The measure based on vasectomies yields the most different ranking, while the closest one is the measure based on reconstructed target, followed by the measure based on the increase in sterilizations.

Results for the different measures at the state level are in Table A2. To better compare the coefficients' magnitudes, all coercion variables are divided by their standard deviation in the children sample. The coefficients are always negative; and although the magnitude is slightly lower when we use the measure based on increased sterilizations, the statistical significance is higher. This consistency further demonstrates that family planning during the Emergency reduced subsequent health demand.

B.3 Different Sample for TA Vaccine

An important underlying assumption is that the timing of immunization does not vary with coercion intensity. If children get vaccinated systematically later in highly coercive states, our estimates will be biased, as older children will have had time to get the vaccine while younger children (born after the Emergency) will not have received it yet. One way to address this concern is to study a different sample for the triple antigen vaccine, which should not be administered to children above five years old (see section 3.2). In the second sample, we focus on children born after the Emergency who are at least four years old at the date of the survey, with children who were at least four at the time of the Emergency.[25]

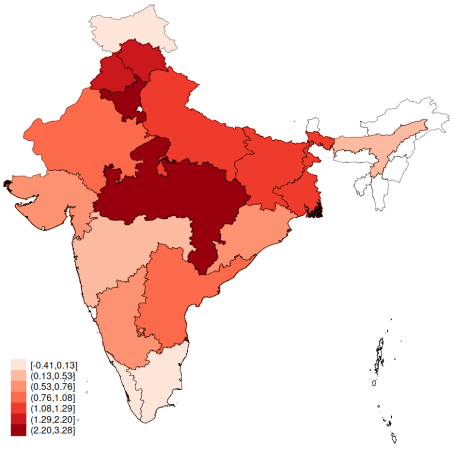
Table A3 presents the results. Point estimates of our coefficient of interest are rather stable, but standard errors largely increase. Given the large reduction in sample size, the stability of the coefficients demonstrates our coefficients are not biased because of differential vaccine timing, even if the coefficients are much less significantly different from zero.

B.4 Mothers' Characteristics

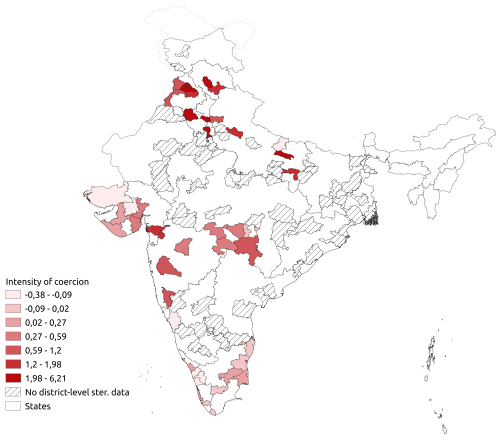
Our fixed-effects identification relies on comparing siblings based on their birth date; thus, only mothers with children born before *and* after the Emergency enter the mother-fixed-effect estimation and only mothers with children born before *or* after the Emergency enter the village-fixed-effect estimation. This means we face a selection bias if mothers who have not been sterilized have different immunization preferences than those who were sterilized during the Emergency, given that the likelihood of being

[25] Ideally we would use the five-year-old threshold, but it limits the sample size too much.

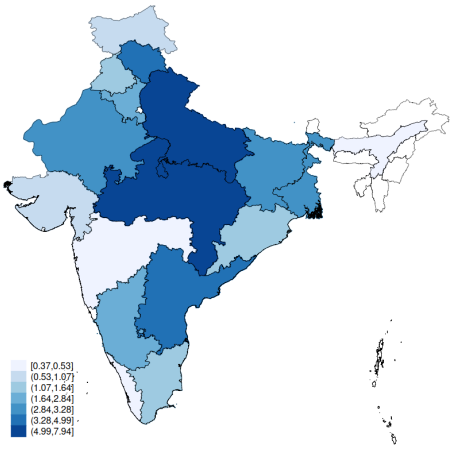
Figure A3: Spatial distribution of other coercion measures



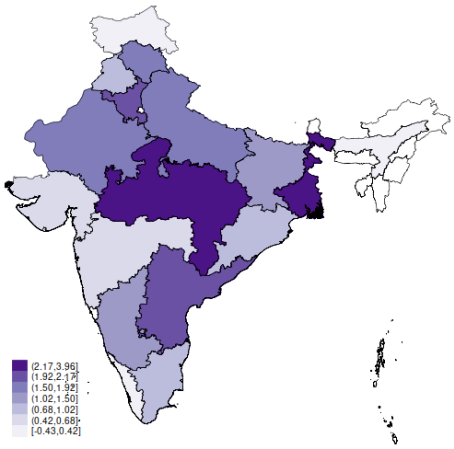
(a) Main coercion measure (ster-targ/targ)



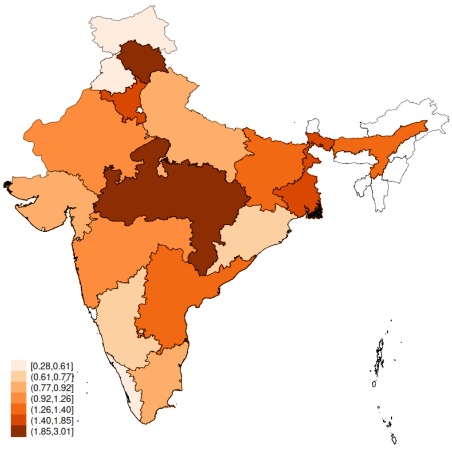
(b) District-level measure, based on main



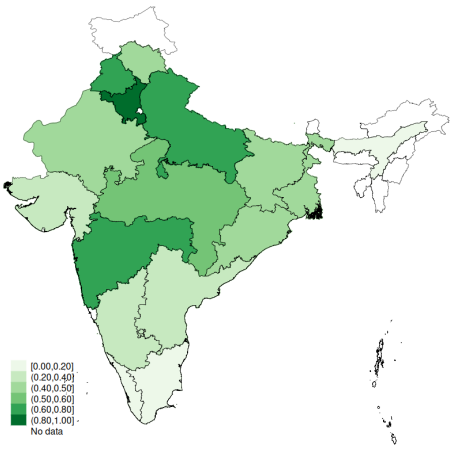
(c) Increase in sterilizations



(d) Reconstructed formula



(e) Vasectomies over pop in 1971



(f) Shah commission index

This figure presents the spatial distribution of the other coercion-intensity measures defined at the state level. (a) is the absolute number of sterilizations minus target in 1976/77 divided by target in 1976/77 (main measure); (b) is an adaption of measure (a) at district level; (c) is the increase in sterilizations over the previous year; (d) is the difference between achievements and predicted target in 1976/77 divided by predicted target; (e) is the absolute number of vasectomies performed in 1976/77 over population in 1971; (f) is an index based on the evidence from the Shah Commission.

Table A2: Coercion-intensity variables and immunization outcomes

	(1) Any immun	(2) Any immun	(3) TA vaccine	(4) TA vaccine	(5) Born hosp	(6) Born hosp
Standardized coercion intensity: main measure						
Intensity*After	-0.136	-0.134	-0.072	-0.069	-0.102	-0.059
	(0.040)	(0.018)	(0.033)	(0.024)	(0.056)	(0.019)
Wild boot. p-val	0.014	0.010	0.225	0.061	0.163	0.057
Standardized coercion intensity: increase in sterilizations						
Intensity*After	-0.089	-0.093	-0.047	-0.057	-0.113	-0.036
	(0.032)	(0.011)	(0.022)	(0.021)	(0.053)	(0.013)
Wild boot. p-val	0.022	0.006	0.168	0.014	0.164	0.177
Standardized coercion intensity: reconstructed target						
Intensity*After	-0.114	-0.105	-0.037	-0.045	-0.121	-0.038
	(0.025)	(0.011)	(0.023)	(0.019)	(0.067)	(0.015)
Wild boot. p-val	0.139	0.056	0.407	0.085	0.220	0.206
Standardized coercion intensity: vasectomies over pop						
Intensity*After	-0.113	-0.095	-0.039	-0.033	-0.048	-0.031
	(0.020)	(0.014)	(0.020)	(0.019)	(0.026)	(0.011)
Wild boot. p-val	0.184	0.094	0.368	0.247	0.180	0.150
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes
Rank FE & Gender	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Mother	Village	Mother	Village	Mother	Village
Observations	9478	9072	9458	9052	6532	6270
Dep Var Mean *After=0*	0.84	0.83	0.18	0.19	0.07	0.07
Dep Var Mean	0.75	0.75	0.20	0.22	0.08	0.08
Identifying children	5359	9086	5347	9066	1180	6277
Identifying mothers	1527	3821	1524	3814	587	3861

Standard errors clustered at state level are in parentheses, and corrected p-values are reported below (wild bootstrapping correction). Household weights. Coercion-intensity variables are standardized (value divided by standard deviation in children sample) for comparability purposes. The different coercion variables are (i) main coercion-intensity variable, based both on sterilization achievements and targets, (ii) the increase in sterilizations over the previous year, (iii) the difference between achievements and predicted target in 1976/77 divided by predicted target in 1976/77, and (iv) the absolute number of vasectomies performed in 1976/77 over population size in 1971.

Table A3: Coercion intensity and triple antigen vaccine, different samples

	(1) Regular samp.	(2) Regular samp.	(3) Second samp.	(4) Second samp.
Coercion*After	-0.073	-0.072	-0.063	-0.070
	(0.034)	(0.024)	(0.052)	(0.035)
Wild boot. p-val	0.225	0.054	0.377	0.233
Birth year FE	Yes	Yes	Yes	Yes
Rank FE & Gender	Yes	Yes	Yes	Yes
Fixed effects	Mother	Village	Mother	Village
Observations	9458	9066	5855	5629
R^2	0.10	0.57	0.15	0.59
Mean *After=0*	0.18	0.19	0.15	0.16
Mean	0.20	0.22	0.20	0.21
Identifying children	5347	9066	2653	5629
Identifying mothers	1524	3814	950	3111

Standard errors clustered at state level are in parentheses, and corrected p-values are reported below (wild bootstrapping correction). Household weights. Triple antigen vaccine should be administered to children below 5 years old. The before sample is composed of children 11 to 15 years old at the time of the survey (at least 4 during the Emergency) and the after sample includes children aged 4 to 7 at the time of the survey (at least 4 at time of survey).

sterilized could vary because of coercive measures.[26] As the point estimates of our coefficients between the mother- and village-fixed-effects specifications are very similar, this selection-bias concern appears unlikely to be driving our results, but it is still important to describe how mothers differ across coercion levels, based on the timing of their children's birth.

To compare mothers, we build three dummy variables: whether the mother had children only before the Emergency, before and after it, or only after it. In a second step we interact the dummies with coercion intensity. Regarding outcomes, we focus on two sets of characteristics: fertility, and opinions and knowledge. Formally, we estimate

$$Y_j = \alpha T_j + \gamma C_s + \beta C_s * T_j + X_j + \epsilon_j, \tag{4}$$

where mother j, state s, coercion intensity C_s, and T_j are dummy variables. X_j are control variables: whether mother is literate, whether household is poor, household size, caste of household head, religion of household head, whether household is nuclear, higher education level between household's head and main earner, village's distance to health facilities, district head quarter and town, whether roads

[26] In our sample, we do not observe a relationship between coercion intensity and probability of having one additional child after the Emergency.

are paved, whether a health care worker is present, and frequency of visits related to childbirth and immunization.

Table A4 presents the results. In Panel A, about fertility, we study age at first child, the number of living children, and whether the oldest child alive is a boy. In Panel B the focus is on opinions and knowledge: the share of women agreeing with the statement "A happy family is a small family," the ideal family size, and whether a respondent has at least one source of information on family planning. Some differences regarding timing of birth are fairly mechanical: once we control for age, those who had children before the Emergency had their children at a younger age, and those after the Emergency had children at an older age. Women who had children only before the Emergency have fewer living children, which could be because of survival probabilities, but given the results of columns (9) and (10) in Panel B, it could also be that they desired fewer children. Importantly, we do not find significant variations across mothers when timing of childbirth is interacted with our main intensity measure. Thus, across all studied characteristics, mothers are sometimes different based on the birth date of their children, but they are not more different in high-coercion states than in low-coercion states.

Table A4: Mothers and birth timing

Panel A: Fertility

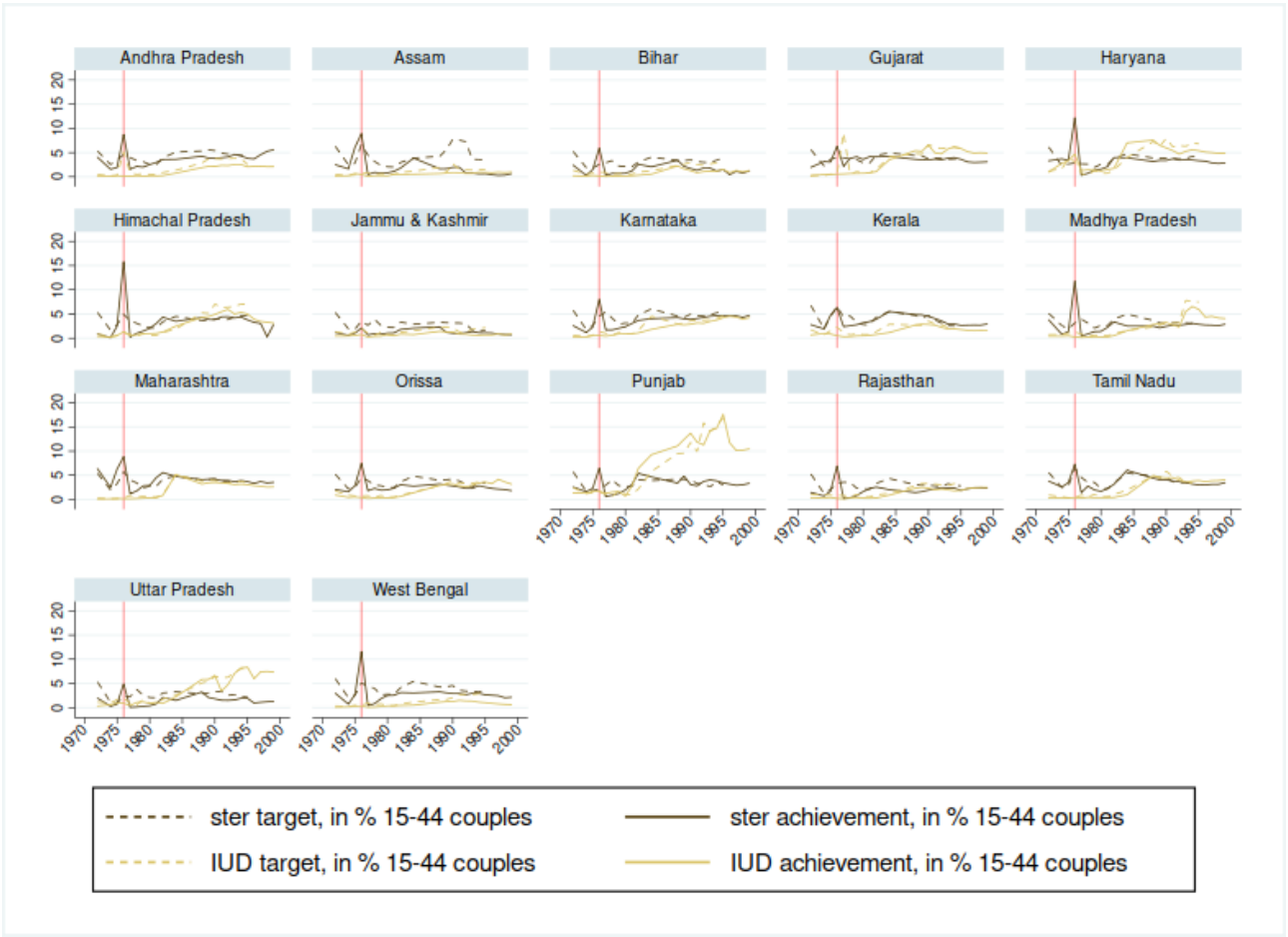
	(1)	(2)	(3)	(4)	(5)	(6)
	Age 1st child		Nb living children		Oldest is boy	
Child before	-2.149***	-1.913***	-0.922***	-0.686**	0.075	-0.010
	(0.506)	(0.556)	(0.230)	(0.259)	(0.065)	(0.073)
Child after	4.499***	4.823***	-1.316***	-1.196***	0.060	0.132
	(0.573)	(0.814)	(0.080)	(0.093)	(0.053)	(0.085)
Intensity		0.208		0.085		0.005
		(0.181)		(0.053)		(0.029)
Intensity*before		-0.221		-0.219		0.079
		(0.231)		(0.142)		(0.057)
Intensity*after		-0.289		-0.104		-0.065
		(0.349)		(0.064)		(0.052)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4625	4625	4624	4624	4625	4625
Mean *both*	19.35	19.35	4.11	4.11	0.54	0.54

Panel B: Opinions and knowledge

	(7)	(8)	(9)	(10)	(11)	(12)
	Happy family is small		Ideal family size		Knows 1+ source for FP	
Child before	0.106	0.111	-0.823***	-0.915**	-0.036	-0.065
	(0.057)	(0.068)	(0.254)	(0.339)	(0.046)	(0.068)
Child after	-0.007	-0.011	-0.137	-0.094	-0.083	-0.059
	(0.048)	(0.051)	(0.153)	(0.163)	(0.058)	(0.085)
Intensity		0.077		-0.170		-0.037
		(0.024)		(0.081)		(0.040)
Intensity*before		-0.006		0.090		0.028
		(0.018)		(0.122)		(0.046)
Intensity*after		0.001		-0.032		-0.021
		(0.039)		(0.076)		(0.031)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4571	4571	4433	4433	4587	4587
Mean *both*	0.83	0.83	3.89	3.89	0.74	0.74

Standard errors clustered at state level are in parentheses, and significance, indicated by *, is based on corrected p-value (wild bootstrapping correction). * denotes 10% level, ** 5%, and *** 1%. Household weights. Controls include whether mother is literate, whether household is poor, household size, caste of household head, religion of household head, whether household is nuclear, and higher education level between household's head and main earner. Village controls are also included. Average age of mothers is 40.9 in the before sample, 32.7 in the both sample, and 22.7 in the after sample.

Figure A4: Targets and achievements at the state level (1972–98)



This figure presents the targets and achievements of sterilizations and IUD insertions for each state, expressed as a percentage of couples in reproductive age group in each period. *Source:* Ministry of Health and Family Welfare, Yearbooks 1971–72 to 2001.

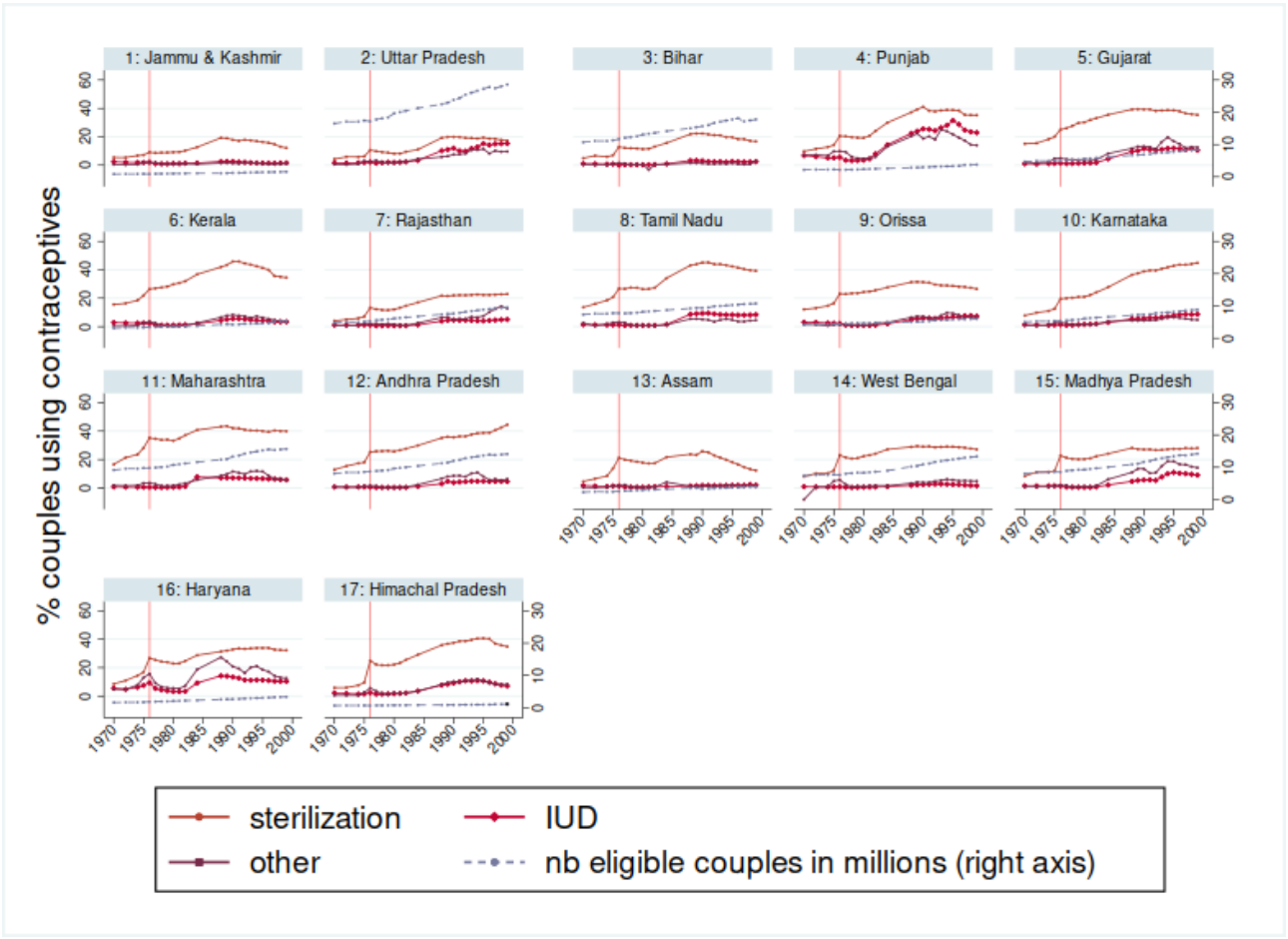
C Additional Figures and Tables

C.1 Family Planning in India

C.2 Balance tables

C.3 Heterogeneity Analysis with Mother Fixed Effects

Figure A5: Contraceptive use over time at the state level (1972–98)



This figure presents the percentage of couples using contraceptives, both for any contraceptive method and for sterilization specifically (left axis). It also presents the general evolution of the number of eligible couples (with a married woman aged 15-44) across time (right axis). *Source:* Ministry of Health and Family Welfare, Yearbooks 1971–72 to 2001.

Table A5: Sterilizations and IUD acceptors pre-Emergency

Year	Sterilizations	% women	IUD insertions
1957	13,736	69.77	
1958	25,148	63.46	
1959	42,302	58.32	
1960	64,338	41.56	
1961	104,585	38.92	
1962	157,947	28.86	
1963	170,246	32.67	
1964	269,565	25.37	
1965-March 1966	476,889	15.76	812,713
1966-67	887,368	11.49	909,726
1967-68	1,839,811	10.42	668,979
1968-69	1,664,817	16.92	478,731
1969-70	1,422,118	25.75	458,726
1970-71	1,319,589	34.09	475,848
1971-72	2,187,336	25.93	488,368
1972-73	3,121,856	16.29	354,624
1973-74	942,402	57.23	371,594
1974-75	1,353,859	54.80	432,630
Total	16,063,912	25.6	1,157,386

Source: Government of India, Ministry of Health and Family Welfare, *Family Welfare Program in India, Yearbook 1978–79*

Table A6: Balance table: village and household characteristics

	mean	sd	coef	p-val	obs
Panel A: Village *characteristics*					
Dist to district hq	54.27	36.95	3.46	0.57	243
Dist to police station	17.83	13.27	3.04	0.03	242
Dist to railway station	30.59	52.88	4.41	0.78	230
Dist to post office	3.71	5.52	0.01	0.96	231
Dist town	15.92	16.95	3.68	0.41	246
Dist paved road	7.00	8.49	-0.10	0.92	246
Health worker in vil	0.62	0.49	-0.07	0.71	247
Health SC within 5km	0.20	0.40	-0.02	0.62	247
Rural hosp within 5km	0.17	0.38	-0.02	0.53	211
PHC within 5km	0.34	0.47	-0.07	0.06	210
FP clinic within 5km	0.77	0.42	-0.08	0.31	167
Freq visit immunization	0.38	0.49	-0.01	0.86	247
Freq visit family planning	0.48	0.50	-0.04	0.47	247
Freq visit delivery	0.21	0.41	-0.08	0.01	247
Panel B: Household *characteristics*					
Poor	0.53	0.50	-0.02	0.77	3736
Age head	45.02	11.96	-0.16	0.90	3735
Household size	6.88	3.03	0.46	0.11	3736
Nuclear hh	0.54	0.50	-0.12	0.06	3736
Nb children less 10	1.86	1.44	0.24	0.05	3736
Upper caste	0.33	0.47	0.00	0.99	3736
Scheduled caste	0.14	0.34	0.01	0.78	3736
Scheduled tribe	0.06	0.23	0.04	0.44	3736
Backward class	0.31	0.46	-0.08	0.14	3736
Non-classified Hindu	0.02	0.15	-0.01	0.58	3736
Muslim	0.12	0.32	0.03	0.66	3736
Other religion	0.03	0.16	0.01	0.85	3736
Educ: no formal	0.50	0.50	0.11	0.05	3611
Educ: primary or below	0.14	0.35	-0.03	0.16	3611
Educ: btw prim and matric.	0.22	0.41	-0.09	0.12	3611
Educ: above matriculation	0.14	0.35	0.01	0.38	3611

Column *mean* refers to the weighted average in the full sample, *sd* the standard deviation in the sample, *beta* the coefficient associated with coercion intensity in the linear regression of characteristics on coercion intensity, *p-value* the wild bootstrap p-value of this coefficient, and *obs* the number of nonmissing observations. The sample is further divided into the groups used in the main analysis. *Dist* characteristics are in kilometers. *Health worker in vil* and *... within 5km* are dummy variables taking the value of 1 if they are true. *Freq visit* takes the value of 0 if there are never any visits, 1 if sometimes, and 2 if often. *Poor* is a dummy taking the value of 1 if consumption per capita is below the 1979/80 rural poverty line. *Educ* is the higher level of education between household's head and main income earner.

Table A7: Heterogeneity for poor people and minorities (mother fixed effects)

	Any immunization		TA vaccine		Born in hospital	
	(1)	(2)	(3)	(4)	(5)	(6)
	No	Yes	No	Yes	No	Yes
Panel A: Poor						
Intensity*After	-0.125	-0.123	-0.038	-0.083	-0.099	-0.080
	(0.079)	(0.019)	(0.044)	(0.027)	(0.042)	(0.063)
Wild boost. p-val	0.361	0.017	0.519	0.127	0.198	0.288
Identifying mothers	796	731	794	730	339	248
Dep Var Mean *After=0*	0.87	0.81	0.24	0.14	0.10	0.04
Dep Var Mean	0.76	0.74	0.25	0.17	0.11	0.05
Panel B: Minority						
Intensity*After	-0.125	-0.113	-0.016	-0.113	-0.168	0.073
	(0.042)	(0.035)	(0.035)	(0.045)	(0.056)	(0.064)
Wild boost. p-val	0.092	0.059	0.708	0.073	0.093	0.415
Identifying mothers	932	462	930	462	372	164
Dep Var Mean *After=0*	0.83	0.82	0.18	0.20	0.07	0.07
Dep Var Mean	0.77	0.73	0.23	0.19	0.09	0.05

Standard errors clustered at state level are in parentheses, and corrected p-values are reported below (wild bootstrapping correction). Household weights. Coercion intensity is measured as the difference between achievements and target in 1976/77 divided by target in 1976/77; in our sample of children aged 0 to 14, its mean is 1.18 and standard deviation 0.908. *After* takes the value of 1 for children born after the Emergency (0 to 6 years old) and 0 for children born before it (7 to 14 years old for symmetrical sample). *Minority* takes the value of 1 if the household head is Muslim or belongs to a scheduled caste or tribe and 0 if Hindu and not scheduled caste or tribe.

Table A8: Heterogeneity based on distance to health facilities (mother fixed effects)

	Any immunization		TA vaccine		Born in hospital	
	(1)	(2)	(3)	(4)	(5)	(6)
	More 5km	Less 5km	More 5km	Less 5km	More 5km	Less 5km
Panel A: Primary Health Center						
Intensity*After	-0.141	-0.168	-0.021	-0.160	-0.157	0.080
	(0.057)	(0.064)	(0.035)	(0.072)	(0.058)	(0.109)
Wild boost. p-val	0.142	0.064	0.614	0.128	0.104	0.630
Identifying mothers	882	418	880	417	334	158
Dep Var Mean *After=0*	0.82	0.83	0.15	0.23	0.05	0.04
Dep Var Mean	0.75	0.77	0.15	0.29	0.05	0.08
Panel B: Rural Hospital						
Intensity*After	-0.135	-0.250	-0.050	-0.170	-0.121	0.141
	(0.041)	(0.041)	(0.031)	(0.023)	(0.071)	(0.087)
Wild boost. p-val	0.001	0.072	0.183	0.003	0.223	0.258
Identifying mothers	1103	200	1100	200	414	82
Dep Var Mean *After=0*	0.82	0.83	0.15	0.23	0.05	0.04
Dep Var Mean	0.75	0.77	0.15	0.29	0.05	0.08

Standard errors clustered at state level are in parentheses, and corrected p-values are reported below (wild bootstrapping correction). Household weights. Coercion intensity is measured as the difference between achievements and target in 1976/77 divided by target in 1976/77; in our sample of children aged 0 to 14, its mean is 1.18 and standard deviation 0.908. *After* takes the value of 1 for children born after the Emergency (0 to 6 years old) and 0 for children born before it (7 to 14 years old for symmetrical sample). *Minority* takes the value of 1 if the household head is Muslim or belongs to a scheduled caste or tribe and 0 if Hindu and not scheduled caste or tribe. Regressions using mother fixed effects include the same controls as the main regressions (see section 4.1).