Sterilizations and immunization in India: The Emergency experience (1975-1977)

Charlotte Pelras, Andréa Renk

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Sterilizations and immunization in India: The Emergency experience (1975-1977)*

Charlotte Pelras† Andréa Renk‡

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Abstract

This paper investigates whether the intense sterilization campaign in 1976-77 in India led to a decrease in demand for health services, with the idea that this coercive campaign could have generated distrust. We use administrative data to discuss and build a measure of coercion intensity. Outcome-wise, we focus on immunization and institutional delivery using survey data collected only a few years after the event. We take advantage of retrospective data to build a panel, to compare outcomes across siblings or children within the same village based on children's birth date. Results show a strong decrease in use of formal medicine, with a decline of 17% in the probability to receive any vaccine post-Emergency when coercion increases by one standard deviation. Heterogeneity analysis highlights distance to health infrastructure and parents' literacy matter.

Keywords: India, family planning, sterilization, immunization, Emergency, health demand, distrust
JEL Classification: I12, I15, I18, N35, N45

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†Paris School of Economics

‡Paris School of Economics & Université de Namur ✉ andrea.renk@unamur.be

1 Introduction

Vaccination currently prevents 2 to 3 million deaths a year, and is one of the most cost-effective ways to avoid diseases. Yet, many people choose not to vaccinate despite the availability of vaccines, to the extent that vaccine hesitancy is now considered among the top 10 threats to public health (WHO, 2019). The COVID-19 pandemic and subsequent European and United States vaccination trends and debates only further illustrate that supply is not the only constraint when it comes to vaccination. In India specifically, lack of demand for immunization is thought to be a key reason behind the low immunization rates[1] (Banerjee et al., 2021). In 2002, a polio epidemic spread across Northern India, with a six-fold increase in new cases over 2001 (WHO, 2003). Anecdotal evidence points to oral polio vaccines perceived at the time as a way to secretly sterilize children[2], echoing a particular event in India's history, still alive in memories to this day: the Emergency.

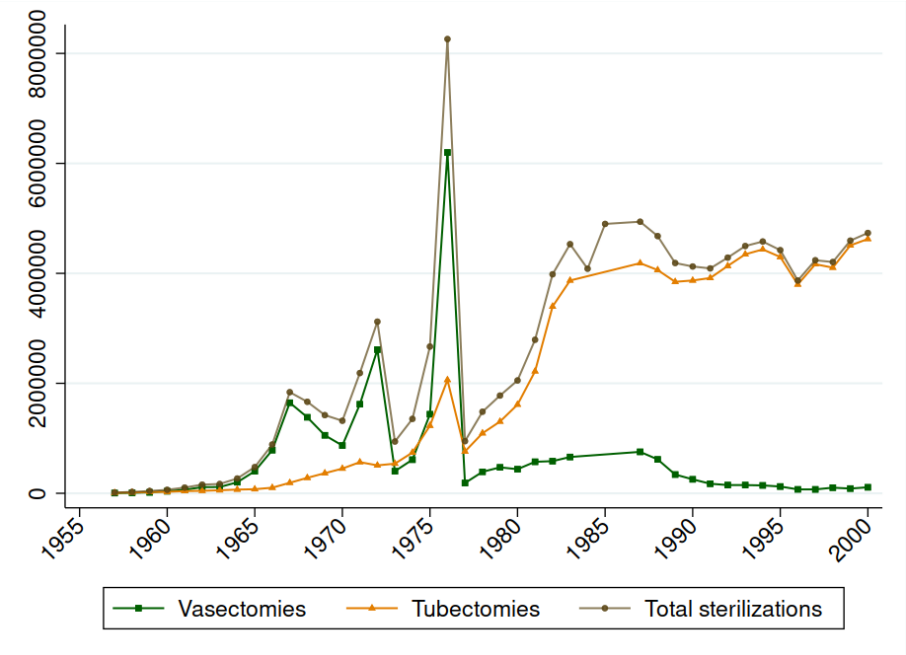
Prime Minister Indira Gandhi declared a state of Emergency in June 1975, which lasted for 21 months. Historians Jaffrelot and Anil (2021) frame the Emergency as a "constitutional dictatorship", a time when India was ruled by decrees, opponents were imprisoned and tortured, and press was censored. Yet, the most memorable event appears to be the massive sterilization campaign undertaken during this period, when more than 8 millions people were sterilized in only a few months, among which 75% were men (see Figure 1). Although the Emergency was much more than a family planning program, political claims to end the regime were shaped around this feature -"get rid of Indira and save your penis", and eventually led to the one time loss in the Lok Sabha elections of the Indian National Congress in three decades. For external observers, the relation between the aggressiveness of the family planning program and the electoral results was also clear (World Bank, 1983; Gwatkin, 1979). When people were asked about the Emergency two decades later, some remembered it as *nasbandi ka vakt* (time of sterilization) or believed the term "emergency" to mean "sterilization"(Tarlo, 2003). And today, when sterilization is involved, a parallel is often drawn with the Emergency times[3]. Note that such an aggressive sterilization campaign is not isolated in history, nor is it far in the past. In

[1]The share of children between 12 and 23 years old fully immunized in 2015 was 62% in India, as compared to 83% in Bangladesh (DHS). Also, the share of children having received DPT3 vaccine was 78% in India while it was 85% globally (WHO, 2014)

[2]Amy Waldman, "Distrust reopens the door for polio in India", *The 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]Soutik Biswas, "India's dark history of sterilisation", *BBC News*, November 14, 2014, written after 15 women died in a sterilisation camps in Chhattisgarh. Or Amrit Dhillon, "Male sterilisation order withdrawn after flurry of criticism", *The Guardian*, February 22, 2020, about a law allowing pay suspension of health worker for non-achievement of sterilization target, which was revoked after parallel was made with Emergency times.

Figure 1: Sterilizations over time



This figure presents the absolute number of sterilizations and the disaggregation by gender over time. Vasectomies are male sterilizations and tubectomies female sterilizations. 8 million represents about 1.4% of the total Indian population in 1971 *Source*: Ministry of Health and Family Welfare, Yearbooks 1971-72 to 2001.

the United States, the eugenics program in the 20th led to forced sterilizations in the 20th century. In Peru, about 270,000 low-income indigenous women were forced into sterilization from 1996 to 2000. Or in China, mandatory sterilizations part of the one-child policy were sometimes enforced against women's will, and today there is a strong suspicion that sterilization is enforced on the Uighurs to drastically reduce their population growth[4].

This paper investigates the impact of the sterilisation campaign during the Emergency on immunization of children in the short run. Although the Emergency only lasted less than 2 years, this event is still present in memories at least in one specific dimension, the aggressiveness of family planning. We are thus wondering if this forced sterilizations could have shaped later-on health demand, as other historical events in different contexts have[5]. Then, there are two main reasons to focus on immunization. First,

[4]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 a nice 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 in China (Wang, N., & Jialing, Z. (Directors and Producers), & Jorg, C., & Goldman, J., & Clements, C., & Hepburn, C. (Producers). (2019). *One Child Nation*), and for the suspected massive Uighurs sterilizations, see Conor Finnegan & 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).

[5]See for example Calvi and Mantovanelli (2018) on how Protestant medical missions in nineteenth century in India

vaccines encompass a strong trust component, as vaccine efficiency is not directly visible. Second, immunizations rates are still very low today in India; if the Emergency generated a distrust still persistent to this day, then a (strong) reaction should be observed in the short run.

To empirically investigate this question, we rely on short run survey data coupled with administrative data. Our main coercion variable is defined at the state level, and measures the "excessive" sterilizations performed during 1976-77, relative to official targets to account for differential state capacities. To capture trust in formal health institutions, our outcomes are any immunization, triple antigen vaccine (DPT) and institutional delivery. We then use retrospective data on children to construct (i) a panel of mothers, (ii) a panel of villages. Our main specification is twofold: first we have a mother fixed-effects model, exploiting variation in immunization across siblings and based on whether the child is born before or after the Emergency. A complementary model is one with village fixed-effects, allowing to relax some constraints associated to the first model at the cost of capturing less time-invariant unobservables.

Our results show that children born after the Emergency period have a significantly lower probability to have received vaccines or to be born in a hospital (institutional delivery) as compared both to their siblings born before or to older children in the same village. An increase in one standard deviation of our coercion variable leads to a decrease of respectively 17% and 34% in the probability to receive any vaccine and the triple antigen vaccine post-Emergency. Coefficients are even larger for institutional deliveries, with a decrease close to 70% of the sample mean, although these results are slightly less robust. These results are very stable across mother and village fixed-effects specifications, and further robustness tests confirm the strength of our results.

A heterogeneity analysis, based on distance to infrastructure and on several individual characteristics, complements our main results. We find distance to health infrastructure matters, but probably less than the literacy status of the parents. We do not find stronger results for the poor or minorities, contrary to what we expected as they were more heavily targeted during the Emergency. Yet for these groups we sometimes observe a general decrease in take-up, regardless of coercion intensity. The literacy results may suggest education or access to information could be important in relating experience on one dimension (sterilization) and implications on another (children immunization). One coherent explanation for our results is an increase in distrust towards health services following the Emergency. Although our data do not allow to study trust measure directly, our results along the health dimension is consistent with what is observed in the literature on trust and health demand in

explains current variation in health outcomes.

other contexts.

We contribute to different strands of literature. First, we contribute to the literature studying trust in medicine and health demand. The closest paper to ours would be Martinez-Bravo and Stegmann (2021), focusing on the disclosure in 2011 that the CIA's immunization campaign in Pakistan was in reality implemented to obtain proof of Osama Bin Laden's presence, which led to a significant anti-vaccine propaganda by the Taliban. They show that this information campaign led to a lower demand for formal medicine and immunization on the short term. Similarly, we estimate short-term response to a well-identified event, although contexts are very different. Focusing on the same event, recent working papers (Sur, 2021a,b) draw a parallel between the intensity of the sterilization program, instrumented by distance between Delhi and state capitals, and lower current-day vaccination rates and level of trust towards government hospitals and doctors. Although we believe the causal claim in these papers to be tricky as one can't exclude other reasons for differences in behavior towards health services between the Northern part of the country, which is mechanically closer to Delhi, and the Southern part over a four decade period, these results are aligned with what we observe in the short run. Among other important papers, Alsan and Wanamaker (2018) use the disclosure of an unethical and deadly experiment (Tuskegee syphilis experiment) in 1972 in the US to study medical mistrust and racial disparities in health and healthcare utilisation. They find that it led to a decline in trust and in demand for health care services, by people sharing similar characteristics to those directly involved in this experiment, in turn leading to an increase in mortality for this population. In the longer run, 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. This paper contributes to this literature by studying whether the Emergency program implemented in the 1970s can explain differences in trajectory of health services take-up, which might in turn affect the level of health. To a lesser extent, our paper also relates to the literature about determinants of health adoption that tries to to explain the low take up of health programs in developing countries (Dupas and Miguel (2017) for a review of randomized control trials on this question). Indeed, our study highlights the importance of a historical event in health demand, which could be persistent if core beliefs were shaped and are still partially impacting health demand today.

Finally, we also contribute to the more general scientific literature about the consequences of the Emergency in India. Although economic literature on this topic is rather scarce, political sciences did establish consequences of the Emergency on different dimensions. Hewitt (2007) establishes a

direct connection between the Emergency and the rise of political movements advocating Hindu nationalism (*Hindutva*) through higher competition for Hindu vote, and Basu (1985) demonstrates how the Emergency shaped subsequent family planning policies. Our contribution to this literature is to show that it also impacted immunization trajectories.

The paper is structured as follow. In the next section, we provide a description of family planning policies in India per period, along with a more general contextualisation of the Emergency itself. In section 3 we present our data, with a discussion on our coercion measure and our outcomes. In section 4, we present our empirical strategy based on the two complementary specifications, and section 5 presents balance tests. Our main results are in section 6, and robustness tests, including different coercion measures, are in section 7. In section 8, we present the heterogeneity analysis. In section 9, we discuss the interpretation of our results, and we detail what can be observed along mothers characteristics. Section 10 concludes.

2 Context

2.1 Two decades of family planning (1950s-70s)

In 1951, India Prime Minister Jawaharlal Nehru announced the creation of a state-sponsored family planning program, the world first policy of population limitation[6]. Although the creation of family planing demonstrates willingness to introduce population control, in her study historian Rebecca Jane Williams explains that at the time, economic growth was seen to be most important, as an increase in standard of living was expected to itself reduce the rate of population increase. This is the demographic transition theory, when after a phase of large population increase due to lower mortality rate, a decline in birth rates occurs as a result of modernization and industrialization. However in parallel, a new theory born in the United States started to be influential in India. In the 1940s, Princeton demographers "inverted" the relation between population growth and industrialisation in the demographic transition theory, by arguing that in non-industrialized countries, high fertility itself was impeding economic development. In the mid-1950s, the World Bank commissioned a study to model relations between economy and demography and vice-versa for India, leading to a publication very influential among India's policy makers[7], with key message that a reduction in population growth would produce important economic advantages (Williams, 2014).

[6]India's leadership has been very influenced by Malthusian ideas due to colonial administration, and India was also among the few non-Western countries with population measures (Caldwell, 1998).

[7]Coale, A. J., & Hoover, E. M. (1958). Population growth and economic development in low-income countries: a case study of India's prospects.

In practice, the first approach implemented for family planning in the 1950s in India was the "clinic approach": social planners were expecting people to come for advice and services to the clinics, an approach largely inspired by the European experience. This was met with limited success and the family planning program moved to the "extension education approach", under the idea that clinics were complicated to access, and that people needed to be educated about modern birth control devices and encouraged to use them (Vicziany, 1982a); free contraceptive devices such as condom and diaphragms were also distributed (Ministry of Health and Family Welfare, 1975). Then, in the mid-60s, the central government started assigning targets to the states ("target oriented and time bound approach") that were to be fulfilled by providing financial incentives to those who agree to be sterilized or to have an intra-uterine device insertion (acceptors) and to those who motivated them (motivators).

The first sterilisations started in 1956, and were soon considered to be the only long-term solution by officials, due to the low adoption rates of contraceptives (Connelly, 2006). And although legislated compulsory sterilization was publicly advocated by the Minister of State for Family Planning and was discussed again in 1972 by the government of India's Task Force on Family Planning (Gwatkin, 1979), much emphasis was on voluntarism, as there was "the desire to avoid compulsion and work exclusively by means of persuasion and incentives"[8]. Introduced in 1965, the intra-uterine devices gave new hopes to the Indian and foreign professionals. It was met with high acceptance rate in the first years, but then came an unanticipated popular reaction against the IUD due to its medical side effects, and IUD acceptance fell well below early figures. Then in 1971 the first "vasectomy camps" appeared: male sterilizations were conducted in temporary mobile field hospitals. Men only would be operated in these facilities, as male sterilizations (vasectomies), compared to female sterilizations (tubectomies), are much less invasive operations, thus safer and quicker to perform (Gwatkin, 1979). Table A1 in appendix provides a detailed overview of family planning performance over this period. The key take-up is that until the Emergency, men were involved in family planning: 74% of all sterilizations recorded before the Emergency were vasectomies.

2.2 The Emergency (1975-77)

The late 1960s and early 1970s were a period of political unrest in India. There were peasants uprisings due to the agrarian crisis following the Green Revolution, frequent strikes in the industrial sector, and

[8]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".

the Indian National Congress (INC) itself, the most important and influential political party since India's independence, split in 1969 due to diverging views. In this context, Indira Gandhi, head of the INC, proclaimed a leftward turn under the slogan "*garibi hatao*" (abolish poverty) and largely won the 1971 elections. Then, the 1973 oil crisis further aggravated the economic crisis, and a more drastic turn was taken shortly afterwards[9].

On June 25th 1975, a state of emergency was declared, under motive of internal disorder. Evidence shows Indira Gandhi's inner circle was prepared for an authoritarian turn at least a few months before, but one motivation for this precise timing seems to have been the verdict of electoral malpractices on the person of Indira Gandhi, jeopardizing her position as Prime Minister. 676 politicians were arrested on the same night as the Emergency was proclaimed, and over the period many people[10], including politicians, journalists, unionists and students, were sent to jail and denied right to trial. A "20-point" economic program was devised to bring back economic growth and allegedly the improve conditions of the poor. Ex-post, it appears that despite some land redistribution, the poor and landless peasants have not much benefited economically from the regime, which turned out to be more in the favour of the industrial middle class.

Sanjay Gandhi, son of Indira Gandhi, did not held any formal position in government but he had an important role throughout the period, especially in family planning matters. In February 1976, he complemented Indira Gandhi's 20-point economic program by his own 5-point program, including the family planning component. The severity of the family planning program is largely associated to the personal influence of Sanjay Gandhi, expressed as pressures on chief ministers and field visits to encourage sterilizations. In this paper we consider a turning point in April 1976, shortly after Sanjay Gandhi became a more public figure, when a formal statement was issued, where if a state deemed "necessary to pass legislation for compulsory sterilization, it may do so" (Ministry for Health and Family Planning, April 1976).

The methods used during the Emergency were the same as the ones used in the previous period: each state had its own target number of sterilizations and IUD insertions, and was in charge of the implementation of the program. However one key difference with the previous period is that more emphasis was put on the family planning program at the highest political level, and resulted into much more pressure on public servants. Every public sector worker, even at the local level such as teachers, were supposed to be informed of the local target and participate their best to have it fulfilled,

[9] Historical events are oversimplified here. We highly recommend Christophe Jaffrelot and Pratinav Anil's recent book *India's First Dictatorship: The Emergency, 1975–1977* to readers interested in learning more about this specific period in India's history. Information provided in this section 2.2 is based on this book.

[10] About 140,000 according to Amnesty International's estimates.

through a scheme of positive/negative incentives specific to each state. Often, public servants had weekly targets to fulfill, and if they failed they could see their pay suspended, or even be fired. These public servants, teachers, tax collectors, police and possibly others depending on the state, were often themselves pressured to get sterilized, as some benefits or positions started to be conditional on the number of children or the ability to present a sterilization certificate.

In 1976-77 alone, more than 8 million people were sterilized[11], with most of this accomplishment coming within six months, as the program was abruptly interrupted in January 1977 after Indira Gandhi announced general elections. During this period, it is likely that the poor were most targeted to follow the recommendations of the "inverted" transition theory, as illustrated by this publication from the Ministry of Health in 1976: "poverty breeds over-population and over-population aggravates poverty [thus main thrust should be among] lower strata of society, from villages and slums in cities".

2.3 Following the Emergency

The Emergency ended in March 1977, after the INC largely lost the general elections in favour of an opposition coalition, dominated by the Janata Party and united over "[highlighting] the atrocities committed during the Emergency and the malversations of the Congress' first family" (Jaffrelot and Anil, 2021, p.427). This electoral result was extremely surprising, 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).

The aggressiveness of the family planning program was very important in Prime Minister Gandhi's defeat. An analysis of India's leading newspapers' publications in the six weeks immediately preceding the elections finds family planning was considered an issue in 274 of the 400 articles studied (Gwatkin, 1979), and for external observers such as the World Bank, the relation between family planning and elections outcome was also very direct[12]. Yet this political drawback did not last long: Indira Gandhi then won by a landslide the next general elections, in 1980.

One could also question the impact of the family planning campaign on fertility: did the government manage to achieve their objective, reduction of fertility, by using such an aggressive strategy? Given the high number of sterilizations (1.4% of 1971 population), in theory it should have. In official guidelines, sterilization acceptors should have already two or three children depending on the state,

[11] This figure corresponds to approximately 8% of couples with a woman aged 15-44 in 1971, and to 1.4% of total population in India in 1971

[12] "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).

but given the average births per women in 1971 was 5.5, strict enforcement of guidelines should still have reduced overall fertility[13]. However, there is suspicion that due to the aggressiveness of the program, family planning policies were actually less implemented in the years afterwards[14]. In this line, Basu (1985) argues that one consequence of the Emergency was to impose more responsibility for terminal birth control on women, due to a shift in government policies; such an evolution is clearly visible in Figure 1. And today, tubectomies are the most common contraceptive in India, despite questionable health benefits (De La Rupelle and Dumas, 2020) and risks associated to the surgery, while 3 men over 8 think contraception is the responsibility of women only (NFHS, 2015). Unfortunately, our survey data does not allow to formally study this question, but two different administrative data sources help shed some light on this point. First, Figure 2 presents crude birth rates for the periods 1971-76 and 1976-1981 at the state level (Census of India, 1985), with on the x-axis a ranking of the states by absolute number of sterilizations divided by married couples with women aged 15 to 45 (eligible couples). We see a clear decrease in birth rates across the two periods for all states, but there does not seem to be any differences related to the actual number of sterilizations. In other words, if there was a change in fertility induced by these 8 million sterilizations, it was not large enough to be detected with crude birth rates.

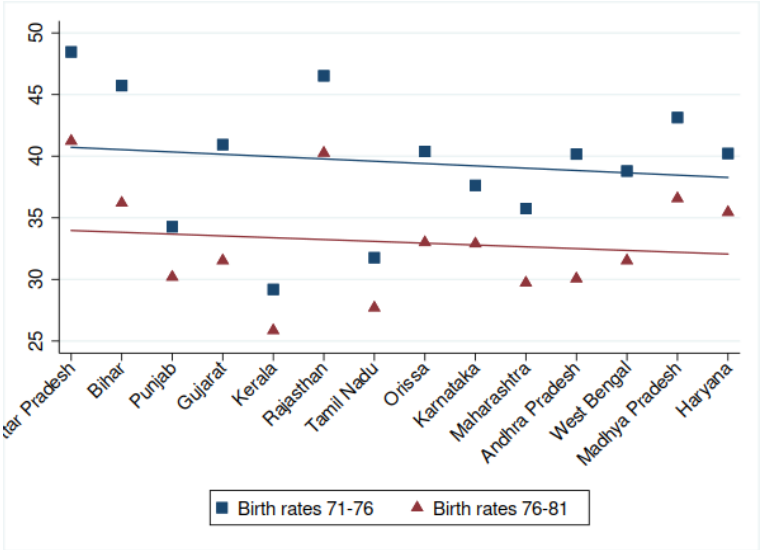
Second, Figure 3 presents take-up of contraceptives across time, based on administrative data from the Ministry of Health and Family Planning -Figure A2 in Appendix presents the state-level trends, ordered also by absolute number of sterilizations per eligible couples. We see a small decrease in take-up of all type of contraceptives right after the Emergency, including in sterilizations, followed by a period of stagnation. The pattern is observe virtually in all states (although sometimes there is only stagnation), suggesting a change in family planning policies in general, in line with insights described in previous paragraph.

To sum up, overall we do not see a variation in birth rates, despite a high number of sterilizations, and one reason is probably the slow down in family planning policies.

[13] Qualitative evidence shows non-eligible people, for instance people older than 55 years old were sterilized, as well as unmarried men or people having less than 2 children. Guidelines were thus not always strictly enforced, but although some people sterilized should not enter overall fertility, the total number of sterilizations is such that we still expect to see some variation.

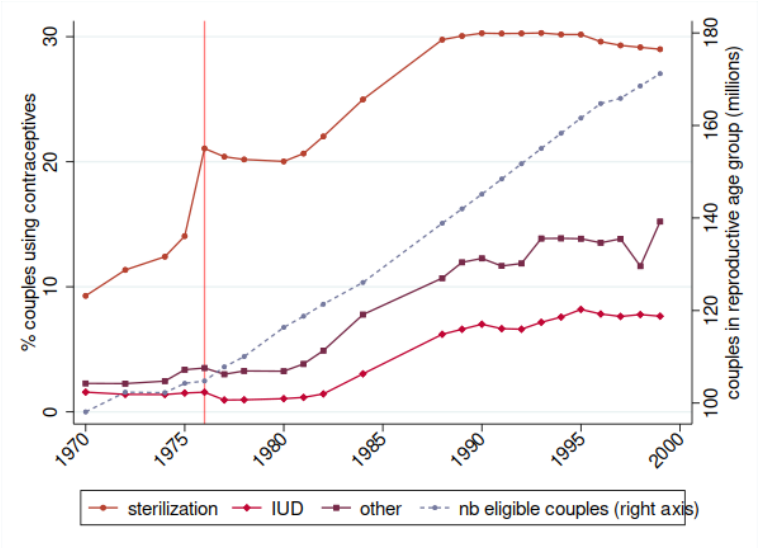
[14] "Family planning, sterilization, contraception - all of these have become dirty words in post emergency India" in Lewis M. Simons, "Compulsory sterilization provokes fear, contempt", *The Washington Post*, 1977, July 4. Similar idea is evoked in the Banerjee and Duflo's book *Poor Economics* (2001): "tainted by the Emergency, family-planning policies in India retreated into the shadows and in the shadows they have remained".

Figure 2: State birth rates



This figure presents birth rates for the periods 1971-76 and 1976-1981, using census data coupled with reverse survival method from the sample registration system (SRS). States are ranked by absolute number of sterilizations divided by number of eligible couples in 1976-77. Trends are also very 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 3: Contraceptives use over time



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

3 Data & Coercion

In this section, we present our data sources for our dependent and independent variables. First we focus on the administrative data and the definition of our independent variable, the coercion intensity measure, and second we present our survey data, and discuss our outcome variables.

3.1 Coercion intensity

Data sources. To describe and define coercion, we used two important administrative data sources: the Shah Commission report and the Ministry of Health and Family welfare annual yearbooks. In 1977, the newly elected central government appointed a Commission of Enquiry headed by Shri Justice J.C. Shah, to inquire the "excesses, malpractices and misdeeds during the Emergency or in the days immediately preceding it" (Shah Commission, 1978). The report was finalized in August 1978, based on thousands of documents, available for consultation since 2011 in the National Archives in Delhi. A general statement about family planning implementation and information on state-wise incentives and disincentives policies, public servants involved and complaints per state can be found in the report.

Then our main source to build our coercion variable are the Ministry of Health and Family Welfare annual yearbooks, over the period 1971-1972 to 2000-2001. These yearbooks are publicly available online, and with data entry in December 2020, we were able to exploit information on several categories. These categories include targets and achievements per contraceptive method and per state, estimated number of protected couples in the population, and some information on health facilities.

Definition. Our main coercion measure is a state-level variable, defined as the difference between achievement and target, divided by target, in sterilizations for the year 1976-77, when Sanjay Gandhi's influence was the strongest and when about 1.4% of Indian population was sterilized in only a few months. This measure captures aggregated "excesses", as it measures over-achievement on the original formula-based targets set by the central government. Historically, the state-level is relevant: states were in charge of the implementation of family planning policies, and during the Emergency, each states refined their schemes, deciding which public workers would receive targets, and what incentives to introduce; chief ministers (head of states) were also under pressure, to the extent that some were threatened to be replaced if they did not comply with the objectives of the Union government (Shah Commission, 1978; Jaffrelot and Anil, 2021). Our source of variation is thus spatial, with a continuous measure based on over-achievement of sterilizations. With this we are closer to the literature where the direct exposure to violence has been proved to matter -Bundervoet et al. (2009) on stunting rates or Bellows and Miguel (2009) on political participation- than to the literature which highlights the

importance of indirect effect too[15]. In other words, we exploit the differences in intensity and do not consider there could be a national change in behavior, implying our estimates are conservative.

One limitation with using a coercion measure based on actual achievements is that it is contaminated by the demand (willingness) for sterilization in the population. Yet, Vicziany (1982a) demonstrates that the sterilizations pattern observed before the Emergency is not consistent with models where demand would be important (classical diffusion model or theory of demographic transition). In such models, the most educated and wealthier individuals should adopt first a new and desirable technology, and then only the technology will be diffused to the rest of the society. This has not been the case in India as "the bulk of vasectomy adopters (...) have been predominantly illiterate [and] poor". And when financial incentives were large or when there were bad agricultural seasons, sterilizations increase significantly, while when financial incentives had to be reduced because of budgetary problems of the Union government between 1973 and 1975, sterilisations also dropped significantly, which demonstrates a strong elasticity to monetary incentives, not coherent with a strong and increasing demand. Hence, we believe that demand had lesser role to play in our context.

If "natural" demand is close to null, then another contender for the coercion measure can be the number of sterilizations per population. However, this would fail to account for state capacities. One way to account for these is to use the formula-based targets set by the Union government, accounting for a number of state characteristics[16]. These targets are designed to set an optimal level of sterilization for each state (at least as defined by the supply side), and given it is formula-based, it defined similarly for all states. Our preferred measure is thus the deviation from the state-wise targets, considered as state "excesses", in the year of interest, as compared to a measure based on achieved sterilizations only. Figure 4 and Figure A1 in appendix present both targets and achievements in sterilizations and IUD insertions respectively in India and per state for all available years.

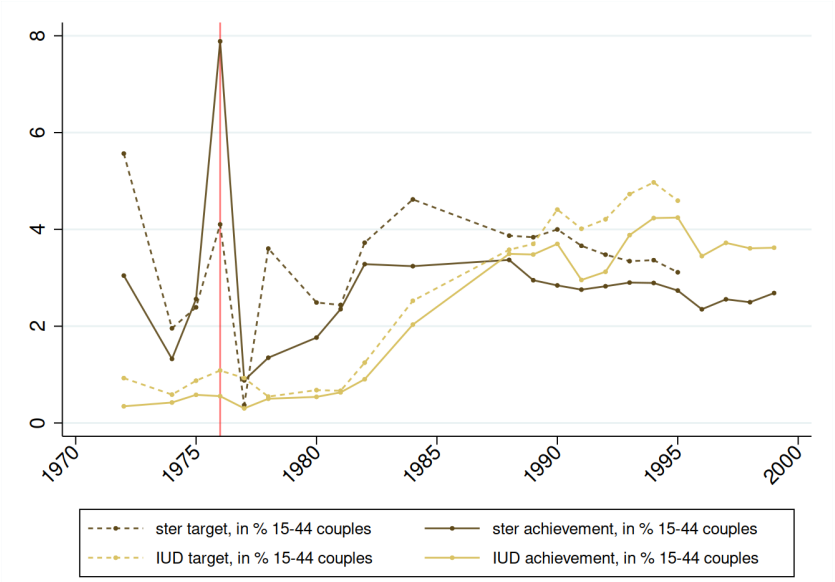
Our final coercion intensity measure is a continuous variable, ranging from -0.41 to 3.29[17]. In our weighted sample, its average value is 1.10 (more than doubling of the original target), and standard deviation is 0.74. Figure 5 presents its spatial distribution: states close to Delhi have on average

[15]In our context it is possible the event was perceived as traumatizing for the country as a whole, especially in a situation where policy-making became extremely centralized. Relatedly, Silver et al. (2002) study psychological response of United-States citizens after the 11th September 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".

[16]These targets were already used in the previous years, and if we don't have the exact formula, we know that the population size, rural share of the population, financial inputs, level of economic development, female literacy and performance gap from previous periods mattered.

[17]Values for two states are negative: Jammu and Kashmir (-0.41) and Kerala (-0.04). Given how we build our measure, it seemed fair to keep also negative values, as those who did not meet their targets did "worse" than those that barely achieved them. Results are stable if we set the negative values to 0.

Figure 4: Targets and achievements (1972-1998)



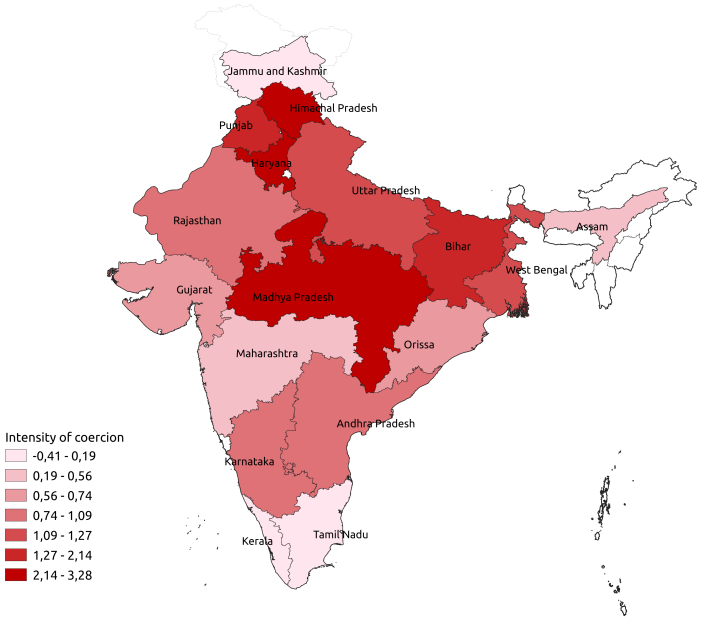
This figure presents the targets and achievements of sterilizations and IUD insertions, expressed as a percentage of couples in reproductive age group (married woman between 15 and 44 years old) in each period. *Source*: Ministry of Health and Family Welfare, Yearbooks 1971-72 to 2001.

higher values, Haryana being the highest.

Reliability. Historical evidence suggests Emergency was most intense close to Delhi, while Gujarat and Tamil Nadu were relatively "soft" states (Jaffrelot and Anil, 2021), which is coherent with our measure. To further test its reliability, we compare our measure to voting behavior just after the end of Emergency, since it has been established that the ruling party (INC) lost the power in the subsequent general election of 1977, due to the harsh Emergency measures (cf section 2.3). Hence we compare our coercion measure to the differences between 1971 and 1977 general elections in i) share of seats won by the INC, ii) voter turn-out. Figure 6 shows that our measure is negatively associated with change in the seats won by the INC, and positively associated with change in the voter turn-out. This illustrates that our measure is correlated to a form of discontent in the population.

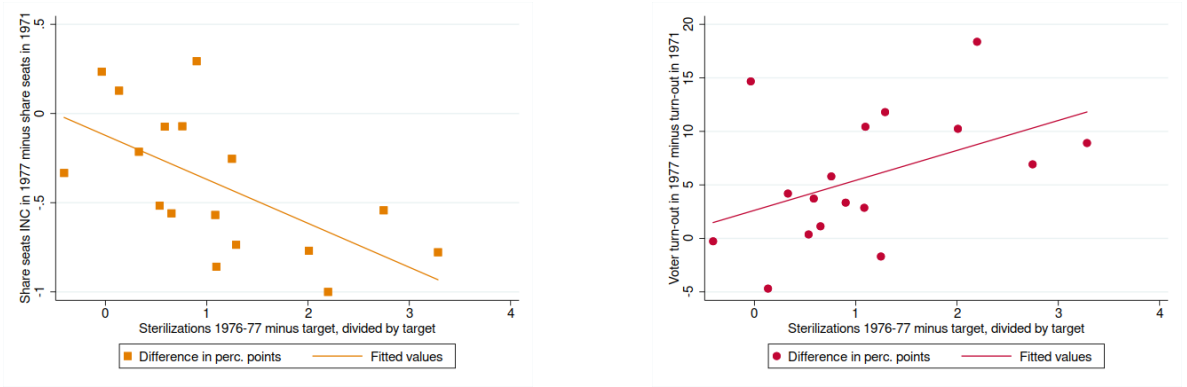
Although we believe this measure to be the most accurate, we will discuss different measures and present associated results in section 7.2: two measures based on our current components, with one measure based solely on achievements, and the other based solely on (revised) targets, both based on increase from previous year; one measure based on vasectomies with respect to population; and finally, our current measure at the district level, available only and imperfectly for a limited number of districts.

Figure 5: Spatial distribution of coercion measure



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

Figure 6: Correlation between coercion intensity and voting outcomes



(a) Share of seats obtained by INC

(b) Voter turn-out

This figure presents correlations between our preferred coercion measure and voting outcomes at state-level: the difference in share of seats obtained by INC in general elections before and after the Emergency, and the difference in voter turn-out. *Source for electoral data*: Election Commission of India.

3.2 Survey data

REDS 1982. Ideally, we would have panel data before and after the event -to be able to control for pre-existing behaviors, data about health behaviors, about the need for health services versus use of healthcare services and measures of trust. If these type of data do exist at least partially for India, it is only for the recent decades, thus without the panel dimension. Also, it is relevant to focus on the short-run: if indeed the Emergency shaped beliefs towards vaccination, then the change of behavior should appear right after the event. Thus we use one of the few available datasets close to our period of interest, that is the 1982 wave of the rural economic and demographic surveys (REDS).

ARIS/REDS data is a household panel, complemented with cross-sectional observations to have a representative sample of the entire rural population in 1971, 1982, 1999 and 2006. As no demographic data is 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 (i) a panel of mothers, (ii) a panel of villages, with information on birth timing relative to the Emergency. These data contain information at different levels (from village to child) to characterize our sample; we will detail these variable in section 5, when we will do the balance tests.

Outcome variables. Our main outcome variables are defined at the child level. The two sets of outcomes we consider are vaccination status and institutional delivery, as measures of willingness to use health services. We believe immunization to be a good measure of trust towards health services, as vaccine efficiency is not really "visible": the vaccine itself is not easily distinguishable from any other substance by the human eye, and its effect will likely go unnoticed, as it prevents catching up a disease, rather than curing it. Information on vaccination status is available for children younger than 20 years old of all women aged between 15 and 50 years old. To be able to compare whether children were less vaccinated after the Emergency, having the date at which the child received the vaccine would be ideal. However, we only have the vaccination status of the child, that we combine to her age to obtain a proxy for the date of vaccination. In practice, this limits the vaccines we can study due to two types of problem.

The first issue preventing us to using all available vaccines relates to the introduction or abolition of certain vaccines: if vaccines are introduced after the Emergency, then children vaccinated will necessarily be so 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). On the opposite, smallpox was

eradicated in 1978, thus children should not be vaccinated against this virus in the most recent period[18]. The second type of issue is due to the timing of vaccination with respect to the age. The tuberculosis vaccine was administered to children up to 19 years old until 1981-82, when recommendation became to vaccinate children below 2 years old (Sokhey et al., 1989); given the large age frame before 1982, date of birth is not a good proxy for vaccination date. Then, the triple antigen vaccine is a good candidate. This vaccine started to be promoted at least since the early 1970s to children up to 5 years old (Sokhey et al., 1989); such an age limitation implies that we can exploit the timing of birth. Finally, we still use the overall measure of whether the child has received any vaccine at all. Although it is less precise and possibly exposed to problems described above, it is the most general measure we can have and the one capturing most directly child's exposure to any immunization. This measure is valid only under the assumption that there is no difference in age at first vaccine depending on coercion intensity, which unfortunately we can not verify in our data.

The second outcome we consider is institutional delivery, available for the last 2 live births. Choosing to give birth in a hospital can capture a distrust towards formal health services, and the date of birth gives precise information on realization of outcome with respect to the Emergency period. These outcomes can also capture distrust towards vaccination, as vaccines are sometimes administered right after birth in hospitals.

Pooled outcomes at state-level. To give an overall idea of the trends in our data, we can aggregate our outcomes at the state level and plot them with in x-axis the rank of our coercion intensity variable, in Figure 7. 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 still gives a general idea of the trends when all children are simply pooled, based on whether they are born before or after the Emergency. Overall, we observe a systematic more downward slope for children born after the Emergency, quite striking when focusing on vaccine outcomes.

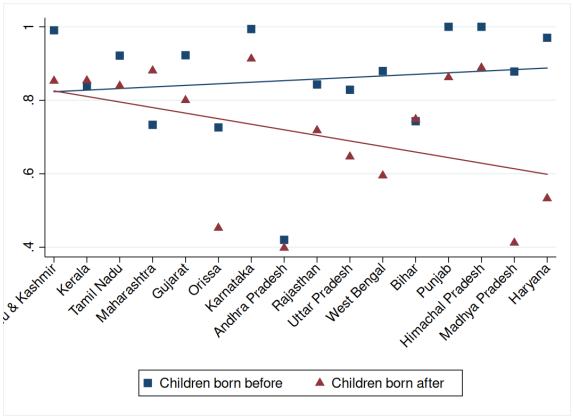
4 Empirical strategy

4.1 Complementary specifications: across siblings and within village

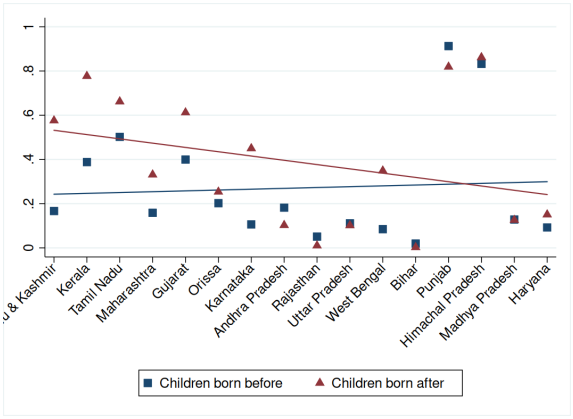
Our source of identification comes from spatial variation in the intensity of coercion, and temporal variation based on age of the child. We use a difference in difference strategy, by comparing children born before and after the Emergency, with a continuous value of treatment, the coercion intensity.

[18] In the data, some young still are declared to be vaccinated against smallpox, possibly reflecting measurement error or confusion across different types of vaccine.

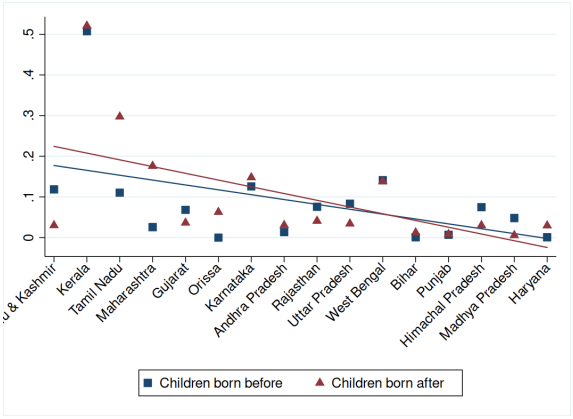
Figure 7: Coercion intensity and state-level outcomes



(a) Any immunization



(b) Triple antigen (DPT)



(c) Institutional delivery

Outcomes in REDS 1982 at the state level, sample split based on whether child is born before or after Emergency. State are ordered based on the intensity of coercion. As REDS 82 data are not representative at the state level and given the low number of states, these results are mostly illustrative.

Our identifying assumption is thus that if it were not for the Emergency, the difference in trends across coercion intensity levels would have stayed the same. To be able to capture as much unobservables as possible, we opt for a difference-in-difference strategy with fixed effects, in two different specification: one with fixed-effects at the mother level, the other at the village level, and we view these specifications as complementary for the analysis.

First we build the mother fixed-effects specification: we compare for a same mother differences in immunization status of her children depending on whether they are born before or after the event. Formally, what we estimate is thus:

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

with child i born in t, from mother j in state s, Y being the outcome, T_t a dummy for being born after April 1976, C_s the coercion intensity, X_i controls at the child level, composed of child's sex times born after to allow for potential difference in gender effect after Emergency, and rank fixed-effects, δ_t year of birth fixed-effects, μ_j mother fixed-effects and ϵ the error term. Note that the coercion intensity term C_s will be absorbed in mother fixed-effect, and the after term T_t will be absorbed in the birth year fixed effects.

Such a specification implies that effect will be identified solely on mothers who have had children before and after the Emergency. This will lead to a selection bias if mothers observed in high coercion states have different immunization preferences as those who would not have been sterilized if it were not for the Emergency. Another limit is the validity of the coefficients for the rank fixed-effects, since the first child will always be born before the Emergency.

These limits can be addressed by using a village fixed-effect specification. We compare, within villages, differences in immunization rates, between children born before and after the event, which is is much less restrictive on the sample since it will include all mothers who had children before or after the Emergency. In this 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 mother fixed-effects specification. Formally, we estimate

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

with child i born in t, from mother j in household h in state s, Y being the outcome, T_t a dummy for being born after April 1976, C_s the coercion intensity, X_i^1 controls at the child, mother and household levels[19], δ_t year of birth fixed-effects, μ_j mother fixed-effects and ϵ the error term.

In order to have a sample centered around the date of the event (data collected 7 years after event), only children strictly below 15 years old are kept in the sample in the main specification.

4.2 Clustering and p-value correction

In our setting, it is necessary to cluster standard errors at the state level. Indeed, we believe it is likely that both the regressors and the errors are correlated within state: it is a very important

[19]Child controls: child's sex*after and rank fixed-effects; mother controls: years of schooling, literate, age, number of living children, dummy for at least one living son, husband literate; household controls: poor household (based on consumption per capita), Muslim head, Hindu head, upper, ST/SC or other backward caste, household size, type of household (nuclear, multiple or extended)

administrative level, for instance in charge of family planning and health policies; states are also large areas, with specific population and history. In addition, since it is the level of definition of our treatment variable, then mechanically there is clustering in the assignment, implying we should cluster at this level (Abadie et al., 2017).

Yet, we only have a few number of clusters (16), implying we face an over-rejection bias by clustering at the state level in our regressions. Following Cameron and Miller (2015), we use a wild cluster bootstrap resampling method to obtain the correct p-value. However, with such a method, one can't directly estimate standard errors. All tables will thus provide (incorrect) standard errors clustered at state level, while the bootstrapped p-value will be provided separately.

5 Balance tests

Sample distribution. The distribution of our weighted sample can be seen in Figure 8. We use the universe of children 0 to 14, the sample of our main specification, with children 0 to 6 the treated children (born after), and 7 to 14 the controls (born before). Note that in the mother fixed-effect specification, only a fraction of these children will be used for the identification. The first noticeable element is that a large share of observations are between 0.5 and 1, a distribution that is consistent with the distribution of the coercion intensity itself. We can also see that the lowest value of our coercion variable (-0.41, Jammu and Kashmir) is not used in the estimation. Second, we see we have treated and control children for each coercion value, although the share of each group varies slightly, with more children born before the Emergency in lower coercion values, and more children born after on average for values close to 1.

Balance tables. Given the continuous nature of our explanatory variable, we perform balance tests by regressing our coercion intensity variable on different sample characteristics. Formally, we just test:

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

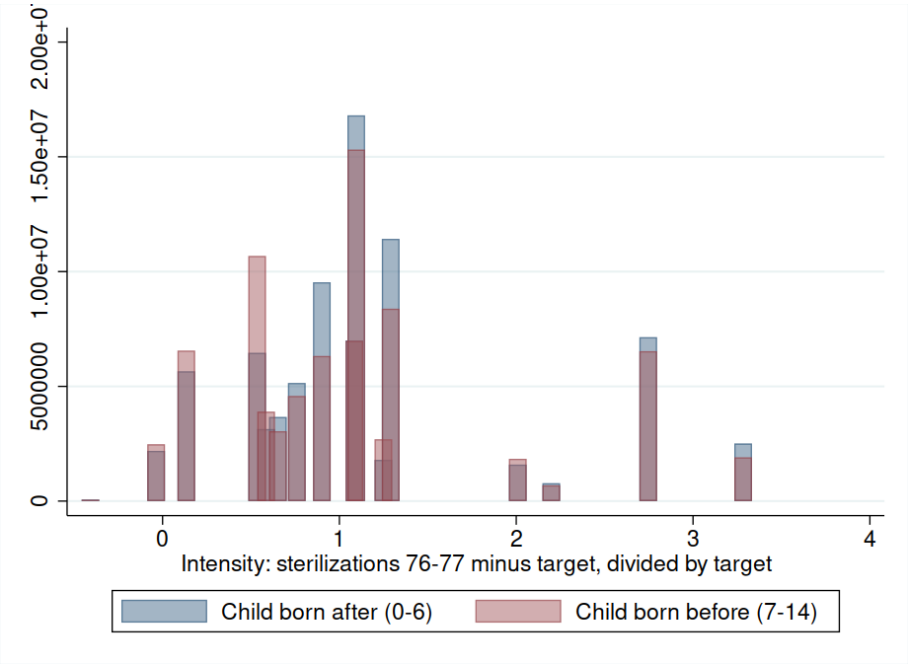
with Y our outcome defined at level i in state s, C our coercion variable and ϵ an error term. As we still need to cluster our standard errors at the state level (cf section 4.2), we provide the weighted average and the standard deviation of the variable in our sample, the value of β and the wild bootstrapped p-value associated, and the number of non-missing observations. Table 1 (child and mother characteristics) and Table A2 in appendix (village and household characteristics) present the results.

Table 1: Balance table: mother and child characteristics

	mean	sd	coef	p-val	obs
Panel A: Mother *characteristics*					
Age	32.85	8.95	-0.66	0.02	4852
Literate	0.33	0.47	-0.14	0.06	4837
Years in school	1.59	2.93	-0.82	0.08	4840
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	4852
Happy family is small	0.83	0.38	0.04	0.36	4743
Ideal family size	3.64	1.47	-0.09	0.22	4589
Nb living children*	3.30	1.86	0.05	0.75	4852
Nb daughters*	1.58	1.34	-0.12	0.43	4852
Nb sons*	1.70	1.31	0.17	0.04	4852
Nb pregnancies*	4.14	2.39	-0.09	0.65	4796
Nb births*	3.89	2.25	0.03	0.88	4852
Has son(s)*	0.82	0.38	0.01	0.52	4852
Husband literate	0.56	0.50	-0.09	0.21	4837
Husband has salary	0.11	0.31	0.02	0.40	4777
Husband is self-employed farm	0.55	0.50	0.04	0.53	4777
Panel B: Child *characteristics*					
Age child	11.59	7.73	-0.65	0.02	16242
Girl (alive children)	0.48	0.50	-0.01	0.06	16247
Girl (born-alive children)	0.48	0.50	-0.01	0.05	19350
Child is dead	0.21	0.41	-0.01	0.18	20217
Rank	3.25	2.11	0.00	1.00	20217
Panel B1 : born after the Emergency (0 to 6)					
Age child	3.21	1.99	-0.03	0.73	5118
Girl (alive children)	0.49	0.50	-0.00	0.52	5118
Girl (born-alive children)	0.49	0.50	-0.00	0.95	5287
Child is dead	0.06	0.23	0.00	0.85	5366
Rank	3.42	2.16	-0.00	0.92	5366
Panel B2 : born before the Emergency (7 to 14)					
Age child	9.80	1.92	-0.04	0.34	5054
Girl (alive children)	0.49	0.50	-0.03	0.02	5054
Girl (born-alive children)	0.49	0.50	-0.02	0.02	5510
Child is dead	0.12	0.32	-0.00	0.47	5642
Rank	3.40	2.18	0.05	0.43	5642

Column *mean* refers to the weighted average in the full sample, *sd* the standard deviation in the sample, *beta* the coefficient associated to coercion intensity in the linear regression of characteristic on coercion intensity, *p-value* the wild bootstrap p-value of the this coefficient and *obs* is the number of non-missing observations. $*$ indicates age fixed-effects were added to the regression as controls.

Figure 8: Sample distribution over treatment variable



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

In Table 1, at the mother level, we observe clear differences in age, education measures and some fertility outcomes. Mothers living in high intensity states are on average younger, less educated and they marry younger. At the child level, we see a difference in children's gender associated to coercion intensity; gender gap is about 4 percentage points for an increase in 1 in coercion intensity, and is more important for children born before the Emergency, suggesting differential mortality rates. Regarding household and village characteristics (Table A2 in appendix), households are on average larger, and more often composed of several married couples (multiple households). Point estimates also suggest that on average, villages are slightly more isolated with respect to health facilities and services in higher coercion states, although only few characteristics are significantly different with respect to coercion intensity.

6 Results

Main results. Our main results are presented in Table A4. Our coefficient of interest, the interaction term between born after Emergency and coercion intensity, is negative across all three outcomes: whether the child has ever received any vaccine, whether she received the triple antigen vaccine, or whether she was born in a hospital. Point estimates across our two main specifications, mother and

Table 2: Coercion intensity and immunization outcomes

	(1) Any immun	(2) Any immun	(3) TA vaccine	(4) TA vaccine	(5) Born hosp	(6) Born hosp
Coercion*After	-0.139	-0.140	-0.073	-0.077	-0.105	-0.061
	(0.041)	(0.018)	(0.034)	(0.027)	(0.057)	(0.021)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes
Rank FE & Gender	Yes	Yes	Yes	Yes	Yes	Yes
More controls	No	Yes	No	Yes	No	Yes
Wild boot. p-val	0.011	0.003	0.223	0.070	0.160	0.060
Observations	9478	9308	9458	9288	6532	6411
Mean *After=0*	0.84	0.83	0.18	0.18	0.07	0.07
Mean	0.75	0.75	0.20	0.20	0.08	0.08
Fixed effects	Mother	Village	Mother	Village	Mother	Village
Identifying children	5353	9308	5341	9288	1178	6411
Identifying mothers	1525	3896	1522	3889	586	3941

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) for each outcome of interest (whether the child has received any vaccine, the triple antigen vaccine and whether she was born in a hospital). Coercion intensity is measured as the difference between achievements and targets in 1976-77 divided by target in 1976-77; in our sample of children 0 to 14, its mean is 1.18 and standard deviation 0.908. *After* takes value 1 for children born after Emergency (0 to 6 years old) and 0 for children born before (7 to 14 years old for symmetrical sample). Note: (i) the parameter *Coercion* can't be estimated as it is absorbed in mother or village fixed-effects, (ii) the parameter *After* is absorbed in the birth year fixed effects.

village fixed-effects, are extremely close to one another, especially for the vaccine outcomes. Coefficient of interest is always significant at least at the 10% level in the village fixed-effects specification, while it is so only for one outcome, any immunization, in the mother fixed-effects specification. Given the closeness of coefficients, the non-significance could be due to a loss of power, as effect is identified with half observations, as can be seen in the last two rows of the table. If we use the state ranking instead of the coercion value, or a coercion dummy based on the median coercion value, results are still negative and even more significant (see Table A3 in appendix).

In terms of magnitude, doubling the sterilizations, as compared to the state initial target, is associated to a decrease of 14 percentage points in the probability that a child born after the Emergency receives any vaccine compared to her sibling(s) or children born before in the same village, and to a decrease in 7.4 percentage points in the probability to receive the triple antigen vaccine, while it corresponds to a 6 to 10 percentage points decline in the probability to be born in a hospital. These magnitudes are very large: probability for a child to have received any vaccine is 75% in our sample, while it is 20% for triple antigen vaccination, implying respectively a 17% and 34% decline in these

type of vaccinations as compared to the sample mean as coercion intensity increases by one standard deviation. Table **??** in appendix presents the result on the other vaccine outcomes, which we believe are less relevant (see discussion in section 3.2).

Event study/parallel trends. We can also present our results as an event study, to observe when exactly we start to observe a decrease in vaccination rates. To have enough points, we use only the village fixed-effects specification. And given the fixed-effects are demanding, instead of using the precise year of birth, we build 2-years groups to reduce the risk of over-fitting, and interact this group variable with our coercion intensity variable. Such an event study graph also allows to check for pre-event trends. Indeed, our key assumption is that if it were not for the Emergency, the difference in trends across coercion intensity levels would have stayed the same (parallel trends assumption). Figure 9 presents the different values of our coefficient of interest, with the 1975-76 group as reference. Overall, the event study confirms our 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 we do not have significance with the wild bootstrapped confidence intervals. We also observe that the decrease seem to start slightly before, coherent with children not necessarily receiving vaccines right after their birth (see the discussion on outcomes in section 3.2).

7 Robustness

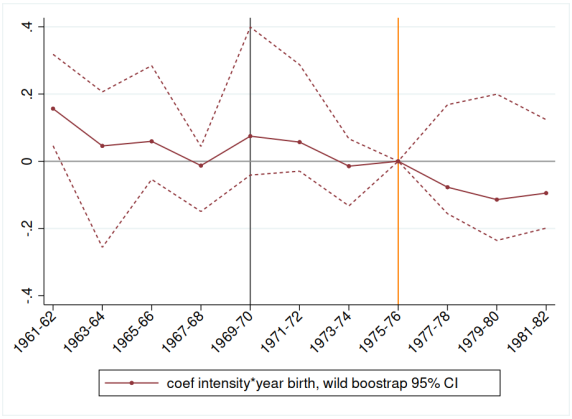
7.1 Placebo Emergency date

To further test the robustness of our results to pre-existing trends, we run a placebo test. We consider the Emergency occurred six years before its accurate date, in April 1970. Instead of the 0 to 14 years old children sample, we focus on children between 6 and 19 years old, in order to limit contamination of children born after the Emergency while still keeping enough observations. In Table 3, we can see that overall, coefficients are very close to zero, except for the institutional birth variable, where coefficients are a bit larger, although non significantly different from 0. This further reinforces our confidence in identifying an effect due to the coercive family planning implementation during the Emergency.

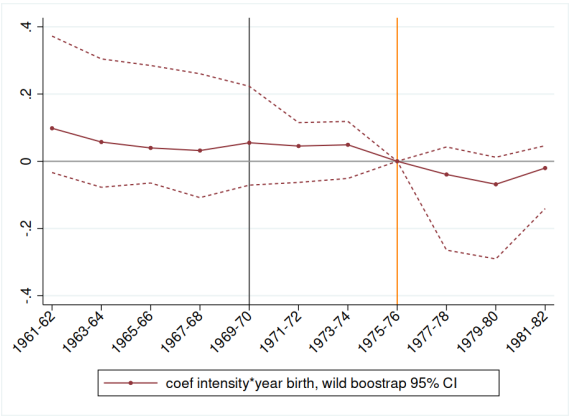
7.2 Other coercion measures

As detailed in section 3.1, our coercion intensity measure is designed to capture excesses, as compared to an "optimal" level of sterilizations, the formula-based targets allocated by the Union government to

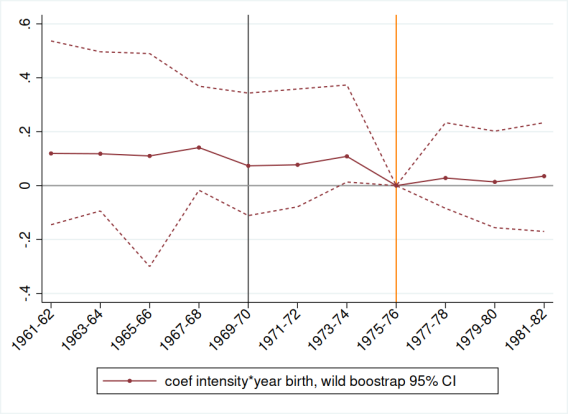
Figure 9: Parallel trends with age groups



(a) Any immunization



(b) Triple antigen vaccine



(c) Born in hospital

Parallel trends under village fixed-effects specification, using 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 based on an empirical distribution. The orange line represents the timing of the Emergency, while the grey line delimits what is the "before" sample used in the analysis.

each state. In this section, first we present a variation of this measure, estimated at the district level, with the caveat that we only have district-level sterilization information for (selected) few districts. Then we discuss other measures of coercion intensity: one focusing solely on achievements with respect to previous year and one solely based on targets, as it is the two components entering our main variable; we also consider vasectomies per population, as we know demand for vasectomies was extremely low, to have a more direct measure of sterilization per capita.

District measure. States were in charge of the implementation of family planning policies, and during the Emergency, we know states designed specific incentives and disincentives, and that the chief ministers (head of states) received a lot of pressure (Shah Commission, 1978; Jaffrelot and Anil, 2021).

Table 3: Placebo: event 6 years before

	(1) Any immun	(2) Any immun	(3) TA vaccine	(4) TA vaccine	(5) Born hosp	(6) Born hosp
Coercion*After	0.001	-0.005	-0.015	-0.001	-0.035	-0.073
	(0.021)	(0.030)	(0.013)	(0.032)	(0.066)	(0.056)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes
Rank FE & Gender	Yes	Yes	Yes	Yes	Yes	Yes
More controls	No	Yes	No	Yes	No	Yes
Wild boot. p-val	0.932	0.859	0.313	0.972	0.741	0.257
Observations	8343	8217	8328	8202	3612	3550
Mean *After=0*	0.72	0.72	0.16	0.16	0.06	0.06
Mean	0.79	0.78	0.18	0.18	0.08	0.08
Fixed effects	Mother	Village	Mother	Village	Mother	Village
Identifying children	5079	8217	5069	8202	675	3550
Identifying mothers	1372	3166	1371	3162	334	2342

Standard errors clustered at state level are in parentheses, and corrected p-value is reported below (wild bootstrapping correction). Household weights. A placebo date, 6 years before the actual event, is used here. Only children between 6 and 19 years old are kept in the sample, in order to limit the "contamination" with younger children while still imposing some symmetry in our sample based on the date of the event.

However, both to capture more precisely personal exposure and for statistical purposes due to the limited number of states, a more disaggregated measure, such as the district level, might be preferable; also considering the narrative of "excesses" by "overzealeous" public workers, the district might be more precise in capturing decision makers' individual action given this level is also very important administratively. There are still two important limitations with using a district-level measure: the availability of sterilizations data, and targets set at the state level, with no fixed rule (to our knowledge, or at least not common across all states) for district target allocation within states. We managed to find information for about half of our sample, although mostly concentrated in lower intensity states. And to rebuild our measure, we estimate the target to be proportionally distributed within the state based on population.

Table 4 presents the results, first with our main measure on the full sample, as in the main estimation, then when we restrict the sample to those for which we have sterilization information at district level (district sample), and finally we use the district-level measure, on the district sample. In this specification, we cluster standard errors at the district level across all specifications. There are two main takeaways from this table. First, the district sample is quite different than the total sample: children are on average more vaccinated, and more are born in hospitals. Second, coefficients' magnitude vary, but the interaction term between coercion intensity and born after is always negative;

Table 4: District coercion intensity and immunization outcomes

	Any immunization			Triple Antigen vaccine			Institutional delivery		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: **mother** fixed-effects specification									
Coercion*After	-0.139	-0.058	-0.026	-0.073	-0.059	-0.038	-0.105	-0.060	-0.024
	(0.039)	(0.029)	(0.015)	(0.039)	(0.052)	(0.027)	(0.060)	(0.044)	(0.025)
	[0.001]	[0.054]	[0.095]	[0.062]	[0.260]	[0.177]	[0.084]	[0.176]	[0.342]
Identifying children	5353	2157	2157	5341	2152	2152	1178	521	521
Identifying mothers	1525	628	628	1522	627	627	586	259	259
Panel B: **village** fixed-effects specification									
Coercion*After	-0.140	-0.095	-0.051	-0.077	-0.084	-0.055	-0.061	-0.041	-0.020
	(0.035)	(0.025)	(0.013)	(0.028)	(0.034)	(0.015)	(0.027)	(0.030)	(0.017)
	[0.000]	[0.000]	[0.000]	[0.007]	[0.018]	[0.001]	[0.028]	[0.173]	[0.248]
Identifying children	9308	3863	3863	9288	3857	3857	6411	2771	2771
Identifying mothers	3896	1658	1658	3889	1657	1657	3941	1708	1708
Observations (max)	9478	3951	3951	9458	3945	3945	6532	2840	2840
Mean *After=0*	0.84	0.85	0.85	0.18	0.31	0.31	0.07	0.10	0.10
Mean	0.75	0.82	0.82	0.20	0.37	0.37	0.08	0.13	0.13
Coercion level	State	State	District	State	State	District	State	State	District
Sample	All	District	District	All	District	District	All	District	District

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

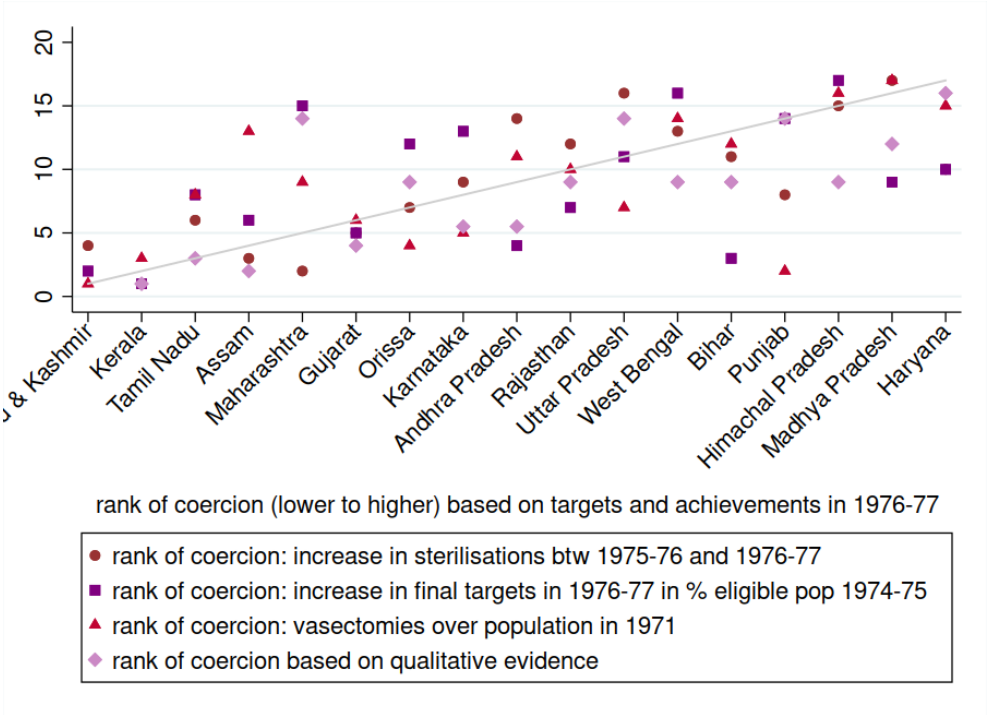
coefficients are strongly significant in the village specification for the vaccine outcomes.

Other measures at state level. Other coercion intensity measures, defined at the state level, are interesting to discuss. First, following Sur (2021a,b), we build an intensity measure based on the increase in sterilizations as compared to the previous year, 1975-76, still with this idea to be able to capture excesses. Second, we build an intensity measure based exclusively on declared targets: when states received their target, some revised it, often upwards. We take the difference in these (revised) targets in 1976-77 and original targets in 1975-76, expressed as percentage of the number of eligible couples, that is number of married couples with the woman between 15 and 45, in 1974-75. This measure aims to capture how much was the objective increased, accounting for the structure of population. Third, we build a measure based solely on vasectomies,to have a more straightforward measure of sterilizations per capita, and as we know demand was close to null (Vicziany, 1982a). This measure is defined as absolute number of vasectomies in 1976-77, divided by population in 1971. Finally, we build an intensity index based on the qualitative evidences in the Shah Commission report. This last measure includes whether there were pay-cuts or termination of service for public servants not fulfilling their quotas, if teacher and tax collectors were motivators, if police was motivators, if there was large-scale resistance and if the state submitted a low proposal for forced sterilization.

Figure 10 presents the ranking of these other coercion variables, as compared to the ranking of the main one: given their different nature, comparing rank is the most straightforward. Figure A3 and A4 in appendix present respectively value-based comparison of the different variables, and the spatial distributions. Rank comparison analysis suggests that except for the measure based exclusively on targets, all other variables are positively correlated with our measure, while the most intense states (Haryana, Madhya Pradesh, Himachal Pradesh) remain the same. Table A5 in appendix shows death reports and complaints gathered as part of the Shah Commission report, to test how relevant are our different coercion variables, transformed as dummy based on median value, to capture (declared) abuses; there again we find our coercion measure fitting quite well the available evidence.

Results for the different measures at the state level are in Table 5. To be able to compare better magnitude of coefficients, all coercion variables were divided by their standard deviation. Overall, coefficients are always negative, although often smaller and less significant than with our preferred coercion intensity variable. Still such consistency further demonstrates that something quite specific, related to family planning during the Emergency, has negatively impacted subsequent health demand.

Figure 10: Comparison of coercion measures



Different coercion variables are reported here, for comparison purposes. Given their different nature, comparing ranks is the most straightforward. Rank of coercion in the x-axis is from lower (left) to higher (right), and the grey 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 as compared to the previous year, (iii) is the difference in revised or non-revised targets in 1976-77 compared to original targets in 1975-76, divided by number of couples with wife 15-45 in 1974, (iv) is the absolute number of vasectomies performed in 1976-77 over population in 1971.

Table 5: 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						
Coercion*After	-0.137	-0.138	-0.072	-0.076	-0.104	-0.060
	(0.040)	(0.018)	(0.033)	(0.027)	(0.057)	(0.020)
Wild boot. p-val	0.011	0.003	0.223	0.070	0.160	0.060
Standardized coercion intensity: increase in sterilizations						
Coercion*After	-0.091	-0.094	-0.046	-0.066	-0.114	-0.040
	(0.032)	(0.015)	(0.022)	(0.020)	(0.053)	(0.012)
Wild boot. p-val	0.018	0.014	0.170	0.005	0.154	0.114
Standardized coercion intensity: increase in targets						
Coercion*After	-0.016	-0.034	-0.015	-0.032	-0.025	-0.009
	(0.009)	(0.014)	(0.013)	(0.013)	(0.024)	(0.010)
Wild boot. p-val	0.049	0.075	0.525	0.317	0.446	0.437
Standardized coercion intensity: vasectomies over population						
Coercion*After	-0.115	-0.095	-0.039	-0.037	-0.050	-0.032
	(0.020)	(0.016)	(0.020)	(0.020)	(0.026)	(0.011)
Wild boot. p-val	0.155	0.187	0.370	0.257	0.172	0.134
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes
Rank FE & Gender	Yes	Yes	Yes	Yes	Yes	Yes
More controls	No	Yes	No	Yes	No	Yes
Observations	9478	9308	9458	9288	6532	6411
Mean *After=0*	0.84	0.83	0.18	0.18	0.07	0.07
Mean	0.75	0.75	0.20	0.20	0.08	0.08
Fixed effects	Mother	Village	Mother	Village	Mother	Village
Identifying children	5353	9308	5341	9288	1178	6411
Identifying mothers	1525	3896	1522	3889	586	3941

Standard errors clustered at state level are in parentheses, and corrected p-value is reported below (wild bootstrapping correction). Household weights. Coercion intensity variables were standardized (value divided by standard deviation, from state 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 as compared to the previous year, (iii) is the difference in revised or non-revised targets in 1976-77 compared to original targets in 1975-76, divided by number of couples with wife 15-45 in 1974, (iv) is the absolute number of vasectomies performed in 1976-77 over population in 1971.

Table 6: Coercion intensity and triple antigen vaccine, different samples

	(1) Regular samp.	(2) Regular samp.	(3) Second samp.	(4) Second samp.
Coercion*After	-0.073	-0.077	-0.068	-0.078
	(0.034)	(0.027)	(0.057)	(0.042)
Birth year FE	Yes	Yes	Yes	Yes
Rank FE & Gender	Yes	Yes	Yes	Yes
More controls	No	Yes	No	Yes
Wild boot. p-val	0.223	0.070	0.489	0.273
Observations	9458	9288	4683	4604
Mean *After=0*	0.18	0.18	0.14	0.14
Mean	0.20	0.20	0.20	0.20
Fixed effects	Mother	Village	Mother	Village
Identifying children	5341	9288	2009	4604
Identifying mothers	1522	3889	814	2872

Standard errors clustered at state level are in parentheses, and corrected p-value is 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 (instead of 8 to 15) and the "after" sample of children 4 to 7.

7.3 Sample exercise for TA vaccine

An important underlying assumption is that the timing of immunization does not vary with coercion intensity. Indeed, 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 the younger, born after the Emergency, will not have received it yet. One way to address this concern is to study a different sample, relevant for the triple antigen vaccine which should not be administered to children above 5 years old (see section 3.2). In what we call the "second sample", we focus on children born after the Emergency who are at least 4 years old at the date of the survey, with children who were at least 4 at the time of the Emergency[20].

Table 6 present the results. Point estimates of our coefficient of interest are rather stable, but standard errors largely increase. Given the large reduction in sample size, we believe the stability of the coefficients demonstrates our coefficient are not biased due to differential vaccine timing, even if the coefficients are much less significantly different from zero.

[20]Ideally we would have used the 5 years old threshold, but it was too limiting in terms of sample size

8 Heterogeneity analysis

To further understand our results, we test for heterogeneous effects along two main dimensions: distance to health infrastructure and several mothers' characteristics. As our aim is to understand who drives our results, we chose to split the sample over each characteristic studied (dummies), because it leads to a more straightforward interpretation and allows to present relevant information (sample, means) for each group directly.

Health infrastructures. First, testing how results vary with respect to health infrastructure seems relevant: those closer to health facilities may have been more exposed, if health facilities also had to perform sterilization operations for recurrent achievement of targets (weekly or monthly), given camps are by definition temporary in one location[21]. Those closer to health facilities may also have seen more side effects following complicated sterilization interventions if some patients operated in camps were brought in for urgent care. To be able to study such heterogeneity, we build a health infrastructure dummy, taking value 1 if the village is within 5 kilometers of either a rural hospital, a primary health center or a health sub-center, and 0 otherwise, while it is missing if any of the three information is missing. Table 7 presents our results. Again, we can see that our point estimates are very close to one another across our specifications, except for the last column. For any immunization, point estimates are fairly similar for either type of villages, although only significantly negative for those close to health infrastructure with mother fixed-effects. For the triple antigen vaccine outcome, point estimates are extremely small for villages far from health infrastructures, while for villages close to at least one health infrastructure, a doubling in sterilizations as compared to target is associated to a significant decrease of about 15 percentage points in the probability to receive the vaccine, which is about twice the magnitude we find in the main results. One element possibly driving this result is the different levels of immunization across these two village types: on average, 12% of children are immunized in far villages, versus 30% in close villages.

Mother characteristics. Second, it seems relevant to focus on three mother characteristics: poverty, minority and literacy. We know there was an ideology relating fertility, poverty and economic growth, and that policy makers did encourage to focus sterilizations efforts on the "lower strata of society" (see section 2.2). Thus since the poor were more targeted, we would expect their reaction towards immunization to be stronger. One limit though is the quality of the original poverty variable, defined at the household level and not per capita, while based on descriptive statistics, we know number

[21] However, based on the evidence we have we can't exclude that during the Emergency, camps stayed for long periods in one location only.

Table 7: Coercion intensity, health infrastructures and immunization outcomes

	Any immunization		TA vaccine		Born in hospital	
	(1) No infras.	(2) Infras.	(3) No infras.	(4) Infras.	(5) No infras.	(6) Infras.
Panel A: **mother** fixed-effects specification						
Coercion*After	-0.154	-0.153	0.005	-0.165	-0.196	0.061
	(0.053)	(0.058)	(0.025)	(0.048)	(0.089)	(0.074)
Wild boot. p-val	0.589	0.050	0.856	0.043	0.220	0.598
Identifying children	2132	2115	2130	2105	431	484
Identifying mothers	611	598	611	595	214	241
Panel B: **village** fixed-effects specification						
Coercion*After	-0.157	-0.138	-0.002	-0.132	-0.103	-0.052
	(0.054)	(0.071)	(0.029)	(0.032)	(0.052)	(0.038)
Wild boot. p-val	0.414	0.203	0.927	0.023	0.248	0.310
Identifying children	3703	3878	3695	3867	2523	2720
Identifying mothers	1556	1635	1553	1631	1556	1674
Observations	3703	3878	3695	3867	2523	2720
Mean *After=0*	0.80	0.83	0.11	0.25	0.05	0.04
Mean	0.74	0.76	0.12	0.30	0.06	0.07

Standard errors clustered at state level are in parentheses, and corrected p-value is reported below (wild bootstrapping correction). Household weights. Intensity of coercion is measured as the difference between achievements and targets in 1976-77 divided by target in 1976-77; in our sample of children 0 to 14, its mean is 1.18 and standard deviation 0.908. *After* takes value 1 for children born after Emergency (0 to 6 years old) and 0 for children born before (7 to 14 years old for symmetrical sample). The health infrastructure variable takes value 1 if the village is within 5 kilometers of either a rural hospital, primary health center or health sub-center. Due to missing values in these variables, our sample is reduced.

Table 8: Coercion intensity, poverty and immunization outcomes

	Any immunization		TA vaccine		Born in hospital	
	(1) Non-poor	(2) Poor	(3) Non-poor	(4) Poor	(5) Non-poor	(6) Poor
Panel A: **mother** fixed-effects specification						
Coercion*After	-0.101	-0.150	-0.042	-0.080	-0.101	-0.076
	(0.068)	(0.024)	(0.051)	(0.027)	(0.035)	(0.060)
Wild boot. p-val	0.347	0.024	0.559	0.132	0.144	0.282
Identifying children	2458	2895	2450	2891	625	553
Identifying mothers	727	798	725	797	312	274
Panel B: **village** fixed-effects specification						
Coercion*After	-0.134	-0.136	-0.090	-0.074	-0.033	-0.092
	(0.037)	(0.015)	(0.037)	(0.028)	(0.039)	(0.055)
Wild boot. p-val	0.027	0.021	0.096	0.072	0.525	0.244
Identifying children	4728	4750	4716	4742	3433	3099
Identifying mothers	2112	1869	2107	1867	2149	1875
Observations	4728	4750	4716	4742	3433	3099
Mean *After=0*	0.87	0.82	0.26	0.14	0.10	0.04
Mean	0.78	0.74	0.28	0.16	0.12	0.05

Standard errors clustered at state level are in parentheses, and corrected p-value is reported below (wild bootstrapping correction). Household weights. Coercion intensity is measured as the difference between achievements and targets in 1976-77 divided by target in 1976-77; in our sample of children 0 to 14, its mean is 1.18 and standard deviation 0.908. *After* takes value 1 for children born after Emergency (0 to 6 years old) and 0 for children born before (7 to 14 years old for symmetrical sample). *Poor* takes value 1 if per capita consumption is below median, 0 otherwise.

of adults per household varies with coercion intensity. Due to this issue, we build a second poverty variable, a dummy based on the median value of per capita consumption in the weighted sample. Overall, we do not systematically find that the poor react more, neither when we use the measure described above, or when we further restrict the sample to the bottom and top terciles.

Same as for the poor, there exists some historical evidence highlighting a special focus on Muslims and scheduled tribes (Williams, 2014). Based on this, we build a minority dummy variable, equal to 1 if the household head is Muslim or belongs to a scheduled caste or scheduled tribe and 0 if the head is Hindu but neither SC or ST; Table 9 presents the results. Coefficients appear fairly similar for probability to receive any immunization, and about the same magnitude as our main results. For the triple antigen vaccine and the probability to be born in hospital however, results appear to be stronger for the non-minority group. Yet, something interesting can be seen in the sample means. in these two outcomes. Children born before have about the same mean, while the mean calculated on

Table 9: Coercion intensity, minority and immunization outcomes

	Any immunization		TA vaccine		Born in hospital	
	(1) Non-minority	(2) Minority	(3) Non-minority	(4) Minority	(5) Non-minority	(6) Minority
Panel A: **mother** fixed-effects specification						
Coercion*After	-0.160	-0.167	-0.085	-0.040	-0.185	-0.004
	(0.039)	(0.072)	(0.040)	(0.027)	(0.070)	(0.014)
Wild boot. p-val	0.013	0.324	0.284	0.228	0.059	0.716
Identifying children	3751	1329	3740	1328	841	259
Identifying mothers	1067	374	1064	374	418	129
Panel B: **village** fixed-effects specification						
Coercion*After	-0.156	-0.137	-0.094	-0.024	-0.097	0.001
	(0.029)	(0.028)	(0.031)	(0.019)	(0.037)	(0.023)
Wild boot. p-val	0.006	0.111	0.055	0.263	0.050	0.977
Identifying children	6782	2186	6769	2179	4707	1458
Identifying mothers	2856	892	2852	889	2887	901
Observations	6782	2186	6769	2179	4707	1458
Mean *After=0*	0.84	0.82	0.17	0.17	0.06	0.06
Mean	0.75	0.75	0.20	0.16	0.08	0.04

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

the 0 to 14 years old children is lower than the mean for children 7 to 14 for the minority group for the triple antigen vaccine and births in hospital. One possible interpretation is that the Emergency had a negative impact on health demand overall, but in this case this impact does not depend on coercion intensity, and thus is not captured in our estimates.

Finally, we explore our results based on whether the mother said she could read or not, with the idea that literacy may capture a better access to information[22]. In Table 10, we find that literate mothers are responding more strongly as compared to non-literate ones: children born from a non-literate mother after the Emergency are on average 12 percentage points less likely than their sibling(s) or older children in the village whose mothers are non-literate, to have received any vaccine when state doubled its target, while the difference increases to about 20 percentage points when the mother is literate, with all coefficients significant at least at the 10% level. Larger reaction of children from

[22]We can not check this directly in the data. The only information we have is that on average, literate mothers are about 2 times more likely to say they know a source of advice or distribution for family planning.

literate mothers is also observed for the triple antigen vaccine with an even larger difference in point estimates, although significance is less. Difference in point estimates also goes in the same direction for institutional births, but it is hard to infer anything more as point estimates vary much more and none of the estimate is significantly different from 0. Note also that results are extremely similar if we observe literacy of the husband instead of the mother, as seen in Table A6 in appendix. One explanation for these results could be the role of information: if literate mothers are on average more informed, maybe they are more aware about what happened in the whole state or in the country, and react even if they were not personally exposed. Yet, we also need to account for the distribution of literacy in our sample when discussing these results. As literacy decreases with coercion intensity, as seen in the descriptive statistics, moments of our coercion variable in the two samples vary as well: in the literate sample, mean is .94 and standard deviation .80, while it is 1.30 and 0.93 in the non-literate sample. Also, across all outcomes, the increase between the two periods has been stronger for the literate sample, which may also explain larger point estimates as there is more margin for reduction.

9 Discussion

9.1 Interpretation

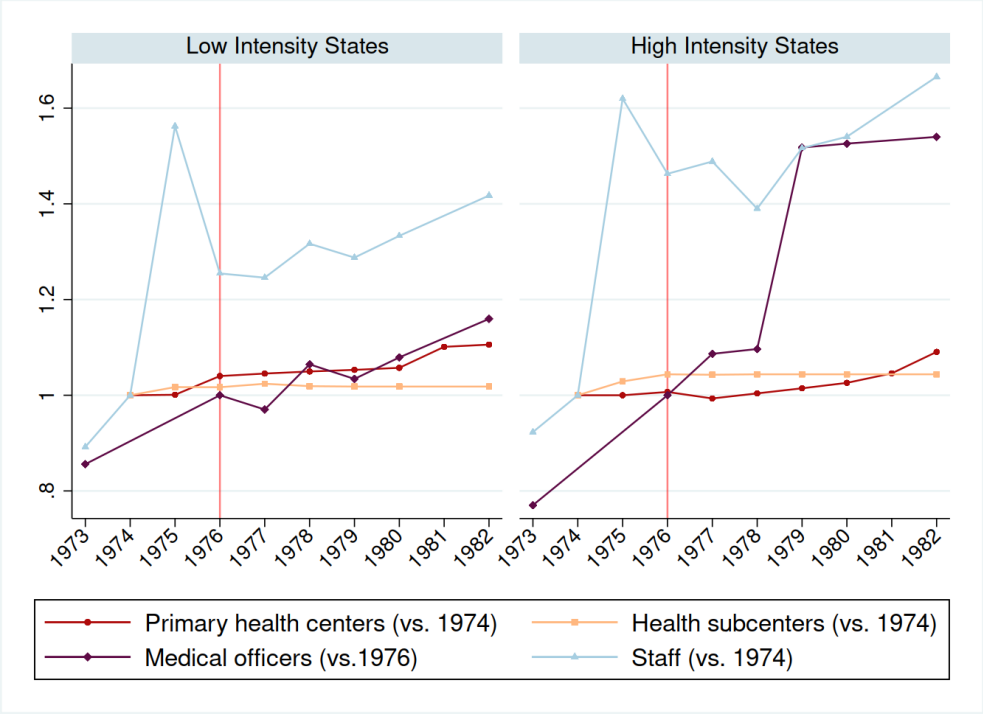
Health: supply or demand? In this paper, we study whether experience or knowledge of coercion in one dimension of health policies can decrease take-up in other health dimensions, and based on previous literature and anecdotal evidence, the main mechanism we have in mind is that it would go through a newly generated or increased distrust towards health policies in general. Yet, this is not something we can directly test for in our data as to our knowledge, trust measures around this time period in India do not exist. In addition, we face risks of omitted variable bias, as we can't capture time-variant characteristics through our fixed-effects. One important possible omitted variable bias would be if health supply was less post-Emergency in high coercion states, as compared to low ones. This would imply that what we observe is not the result of a change in demand, but a change in supply. On this point specifically, we use the Ministry of Health and Family Welfare annual yearbooks to study variation in public health provision, in Figure 11. Overall, high intensity states do not seem to have had lower health supply evolution on the two main dimensions we have, that is infrastructure and staff, if anything it would be the opposite. The sole potentially important difference is in the number of primary health centers. Indeed, it seems that in low intensity states, their numbers have increase sooner after the Emergency than it did in high ones. Yet, we see health sub-centers increased

Table 10: Coercion intensity, literacy and immunization outcomes

	Any immunization		TA vaccine		Born in hospital	
	(1) Non-literate	(2) Literate	(3) Non-literate	(4) Literate	(5) Non-literate	(6) Literate
Panel A: **mother** fixed-effects specification						
Coercion*After	-0.121	-0.205	-0.034	-0.210	-0.012	-0.136
	(0.039)	(0.074)	(0.029)	(0.091)	(0.011)	(0.070)
Wild boot. p-val	0.087	0.013	0.427	0.104	0.342	0.210
Identifying children	3781	1512	3776	1505	801	359
Identifying mothers	1061	443	1060	441	398	179
Panel B: **village** fixed-effects specification						
Coercion*After	-0.119	-0.193	-0.034	-0.216	-0.027	-0.060
	(0.020)	(0.070)	(0.026)	(0.066)	(0.019)	(0.087)
Wild boot. p-val	0.052	0.000	0.358	0.031	0.251	0.555
Identifying children	6429	2911	6420	2900	4321	2111
Identifying mothers	2634	1277	2631	1273	2659	1297
Observations	6429	2911	6420	2900	4321	2111
Mean *After=0*	0.84	0.82	0.16	0.22	0.03	0.13
Mean	0.74	0.79	0.17	0.28	0.03	0.16

Standard errors clustered at state level are in parentheses, and corrected p-value is reported below (wild bootstrapping correction). Household weights. Coercion intensity is measured as the difference between achievements and targets in 1976-77 divided by target in 1976-77; in our sample of children 0 to 14, its mean is 1.18 and standard deviation 0.908. *After* takes value 1 for children born after Emergency (0 to 6 years old) and 0 for children born before (7 to 14 years old for symmetrical sample). *Literate* takes value 1 if the mother is literate, and 0 otherwise.

Figure 11: Public health supply across time



This figure presents health infrastructures (primary health centers and health sub-centers) and employed staff (medical officers and all staff) evolution across time for high and low intensity states, groups based on the median value of our main coercion variable. Variations are obtained by taking the average in each group for each variable, and compare it to a value at one point in time to be able to capture evolution. *Source*: Ministry of Health and Family Welfare, Yearbooks 1971-72 to 2001.

more in high intensity states right before the Emergency, maybe denoting different strategies in the choice of infrastructures. Overall, there doesn't seem to be tremendously different trends, which we find reassuring.

Still, there might be unobservables that we can't account for, if for instance public servants in charge of immunization post-Emergency were afraid to go in places where family planning was intensively implemented during the Emergency; this we can not observe. The choice to focus on data collected soon after the Emergency has led us to use only a limited set of variables, as compared to what can be studied with most recent data. Still, we made this choice because no other dataset would allow for a comparison before and after the Emergency, necessary to have an idea of pre-Emergency behaviors. Given our results, the Emergency did have an impact on immunization outcomes, although we can't be certain it goes through an increased distrust.

Concurrent or complementary distrust. Assuming our results are indeed due to increased distrust, another question non-answered at this stage is what exactly induced it. Our measure of

coercion is solely based on sterilizations, thus family-planning, but Table A5 in appendix shows it is also correlated to complaints about other types of abuse (power abuse and arbitrary detention). Our main idea is that since the family planning component was crucial in the rejection of the government in the subsequent elections, and that to this day it seems to have been what has been most widely remembered for this period, a measure capturing "abuses" in sterilizations must be related to such obvious popular discontent, itself related to generation of mistrust. However, we can't exclude other dimensions were not important as well in explaining our results, for example a loss of confidence in information in general as the press was censored, or excessive arrests which might have deterred citizens to interact with formal governmental institutions.

National policy and foreign aid. Finally, as a side note, we would like to emphasis the role of the international community in family planning in India. As seen in section 2.1, the international community was involved from the very early stages in India, through the conceptual framework motivating population control policies, and also through their financial aid. In 1975-76, India received 9.39 billion dollars in aid compared to 4250 million on average in the 3 preceding years, while money largely helped to finance the family planning program (Jaffrelot and Anil, 2021). From administrative records, we indeed observe that during the Emergency, operations were partly financed by different international institutions: about one tenth of total family planning budget was 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). In this paper, we consider a violent family planning policy, but to some extent, this policy could also be considered as an unintended consequence of aid intervention.

9.2 Mothers' characteristics

As exposed in section 4.1, the mother fixed-effects identification relies on comparing siblings based on their birth date, thus only mothers with children born before and after the Emergency will enter the estimation. This induces we face a selection bias if mothers who have not been sterilized have different immunization preferences than those who were sterilized due to the Emergency, given the likelihood to be sterilized should vary due to coercive measures. As the point estimates of our coefficients between the mother and village fixed-effects specifications are very similar, this selection concern appears unlikely to drive our results, but it is still relevant to describe how different are mothers across coercion levels, based on the birth timing of their children.

To compare mothers, we build 3 dummy variables: whether the mother has had children only before the Emergency, before and after or only after[23], that in a second step we interact with coercion intensity. In terms of outcomes, we focus on two sets of characteristics: fertility and opinions and knowledge. Formally what we estimate is:

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

with mother j, in a state s with coercion intensity C_s and T_j our dummy variables. X_j are control variables: mother literacy, poor household, household size, caste and religion of head, household type, and villages' distance to health infrastructure, district head quarter, town, paved road, presence of health worker, frequency of visits related to delivery and immunization.

Table 11 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. The only important and significant difference we observe is those who have children only after the Emergency have less children, which is mechanical as they are younger. Point estimates for age at first child also show that with time, women tend to have their first child later. For sex of the oldest alive child, point estimates suggest sex selection is stronger among older children as coercion intensity increases, coherent with what we observe in child's balance table (Table 1). In panel B, we focus on whether the woman agrees with the statement that a small family is a happy family, her ideal family size and whether she knows at least one source of advice and supply for family planning. There we observe more systematic differences based on children's birth date. First, it seems the general message of family planning "a happy family is a small family" has been heard by younger generations, as women who only had children after the Emergency tend to agree more with this statement; when introducing a coercion intensity term, it is positive, but there is no difference both with coercion intensity and birth timing. For ideal family size, the younger generation of mothers also declares a lower number of children. When introducing the coercion intensity term, we see a strong association between our coercion measure and mothers' opinion, but again not differently so with respect to children birth date. About family planning knowledge, mothers who only had children before are less informed, and again only the simple intensity term is significant in column 10. Overall, the general insight is that opinion and knowledge variables at the mother level do vary with children timing of birth and coercion intensity, but not with both. Across all studied characteristics, mothers are sometimes different based on the

[23]Note that in our sample, we do not observe different probability to have one additional child after the Emergency based on coercion intensity.

Table 11: Mothers and births timing

Panel A: Fertility

	(1)	(2)	(3)	(4)	(5)	(6)
	Age 1st child		Nb living children		Oldest is boy	
Child before	-0.436	-0.340	-0.150	-0.144	0.101	0.030
	(0.395)	(0.588)	(0.232)	(0.232)	(0.047)	(0.077)
	[0.405]	[0.630]	[0.587]	[0.558]	[0.074]	[0.748]
Child after	0.170	0.412	-2.546	-2.541	-0.019	0.033
	(0.489)	(0.811)	(0.158)	(0.232)	(0.059)	(0.076)
	[0.743]	[0.660]	[0.000]	[0.000]	[0.857]	[0.674]
Intensity		-0.104		-0.028		0.003
		(0.281)		(0.102)		(0.017)
		[0.763]		[0.816]		[0.833]
Coercion*before		-0.096		-0.007		0.068
		(0.368)		(0.188)		(0.054)
		[0.815]		[0.971]		[0.483]
Coercion*after		-0.200		-0.003		-0.044
		(0.454)		(0.121)		(0.055)
		[0.689]		[0.984]		[0.458]
Observations	4680	4680	4679	4679	4680	4680
Mean *both*	19.29	19.29	4.14	4.14	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	-0.018	-0.011	-0.252	-0.328	-0.070	-0.131
	(0.037)	(0.042)	(0.119)	(0.201)	(0.031)	(0.055)
	[0.630]	[0.781]	[0.131]	[0.192]	[0.035]	[0.059]
Child after	0.115	0.122	-0.625	-0.581	-0.014	-0.015
	(0.059)	(0.057)	(0.110)	(0.149)	(0.076)	(0.080)
	[0.016]	[0.090]	[0.000]	[0.002]	[0.839]	[0.855]
Coercion		0.063		-0.175		-0.055
		(0.017)		(0.087)		(0.023)
		[0.070]		[0.157]		[0.042]
Coercion*before		-0.002		0.059		0.055
		(0.024)		(0.118)		(0.048)
		[0.924]		[0.648]		[0.712]
Coercion*after		-0.009		-0.028		0.004
		(0.031)		(0.067)		(0.036)
		[0.773]		[0.622]		[0.911]
Observations	4601	4601	4450	4450	4680	4680
Mean *both*	0.83	0.83	3.90	3.90	0.73	0.73

Standard errors clustered at state level are in parentheses, and corrected p-value is reported in brackets (wild bootstrapping correction). Household weights. Controls include: whether mother is literate, household is poor, household size, caste and religion of head, household type. Village controls are also included. Average mothers' age is 40.9 in the before sample, 32.7 in the both sample and 22.7 in the after sample.

birth date of their children, but they are not more different in high coercion states as they are in low coercion states.

10 Conclusion

In this paper, we investigate whether the intense sterilization campaign in 1976-77 during the state of Emergency led to a decrease in demand for health services. Our main result is that children born after the Emergency have a lower probability to be immunized as compared both to their older siblings and older children in the village as coercion intensity increases. These results are very robust, and can still be observed with differently defined coercion measures. The main channel we have in mind is that coercion increased distrust towards health services in general, although trust measures are unavailable over our period of interest.

Outside the scope of the research question, we will conclude by calling attention to the event itself, that is the massive sterilizations of million of (poor) people. As climate change is upon us, discussions about population control may gain momentum again, and it seems important to remember that in this case and in others, the burden was disproportionately carried by populations far away from the instances of decision.

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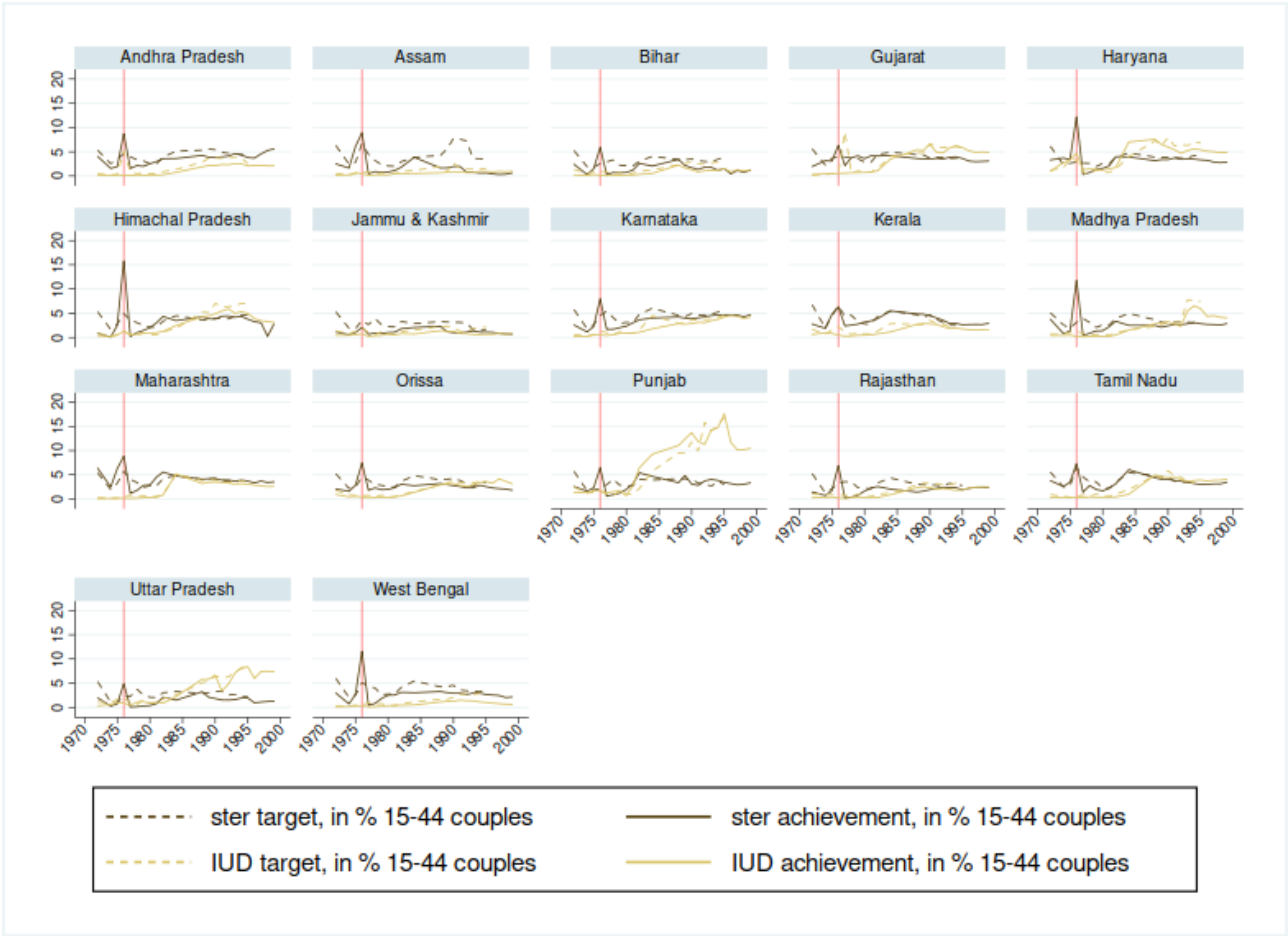
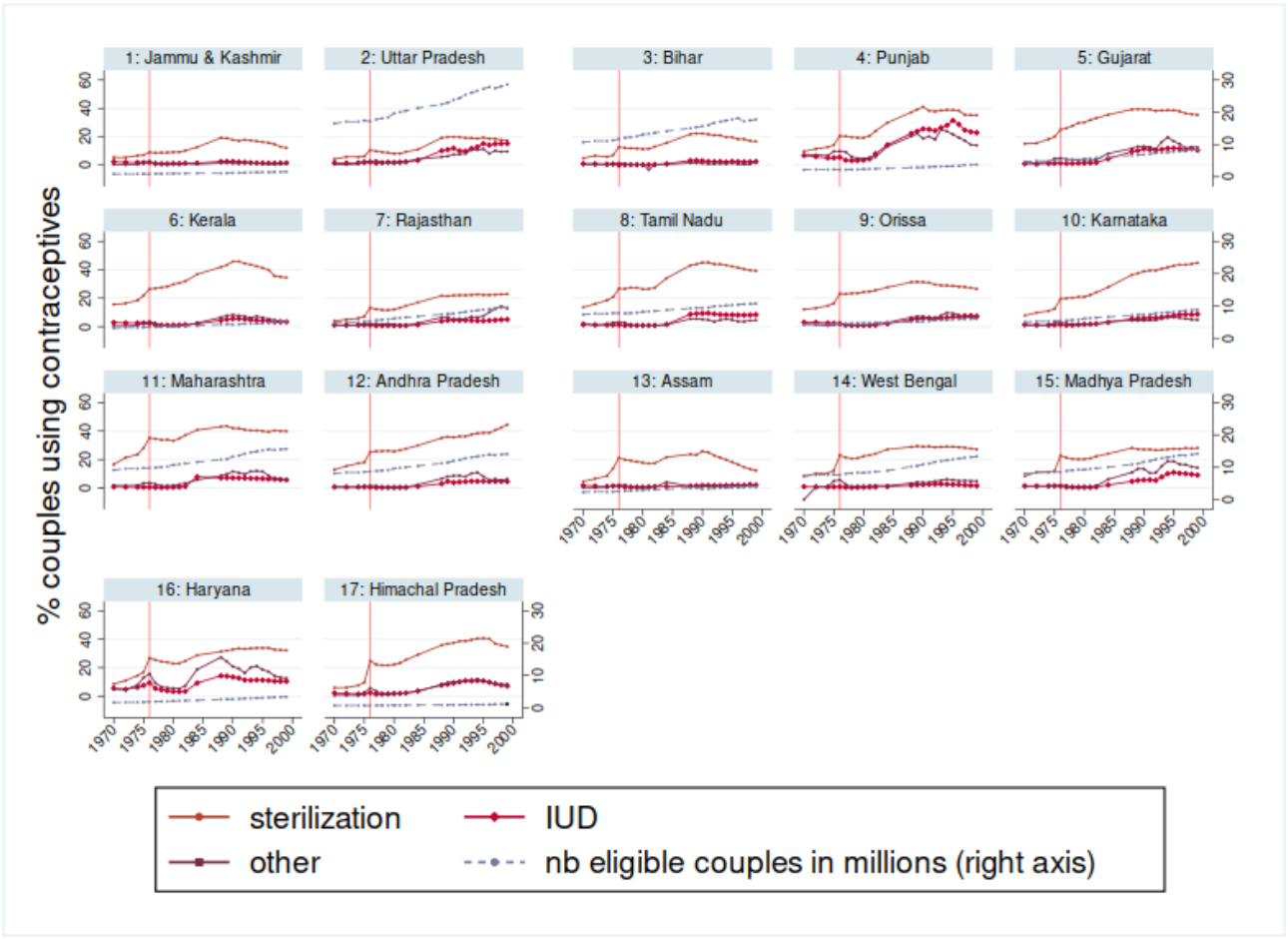
Figure A1: Targets and achievements at the state level (1972-1998)



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.

Figure A2: Contraceptive use over time at the state level (1972-1998)



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

Table A1: Sterilisations and IUD acceptors pre-Emergency

Year	Sterilisations	% 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 A2: Balance table: village and household characteristics

	mean	sd	coef	p-val	obs
Panel A: Village *characteristics*					
Dist to district hq	54.06	36.83	3.42	0.57	246
Dist to police station	17.72	13.24	3.04	0.03	245
Dist to railway station	30.44	52.56	4.38	0.78	233
Dist to post office	3.71	5.49	0.01	0.97	234
Dist town	16.05	16.90	3.71	0.39	249
Dist paved road	6.94	8.45	-0.12	0.90	249
Health worker in vil	0.62	0.49	-0.07	0.73	250
Health SC within 5km	0.20	0.40	-0.03	0.55	250
Rural hosp within 5km	0.17	0.37	-0.02	0.52	214
PHC within 5km	0.33	0.47	-0.07	0.06	213
FP clinic within 5km	0.77	0.42	-0.08	0.31	170
Freq visit immunization	0.56	0.78	-0.02	0.88	250
Freq visit general health	0.38	0.71	-0.07	0.56	250
Freq visit family planning	0.76	0.87	-0.02	0.82	250
Freq visit delivery	0.37	0.74	-0.14	0.04	250
Panel B: Household *characteristics*					
Rich (income hh)	0.05	0.22	0.02	0.09	3794
Poor (income hh)	0.72	0.45	-0.07	0.12	3794
Poor (cons per cap)	0.56	0.50	-0.01	0.85	3794
Upper caste	0.33	0.47	-0.00	0.95	3794
SC/ST caste	0.20	0.40	0.05	0.18	3794
Backward caste	0.31	0.46	-0.07	0.12	3794
Hindu	0.84	0.36	-0.01	0.88	3794
Muslim	0.12	0.33	0.02	0.73	3794
Household size	6.80	3.06	0.46	0.08	3794
Nb adults in hh	4.04	2.14	0.17	0.26	3794
Nb children in hh	2.76	1.80	0.29	0.04	3794
Nuclear hh	0.54	0.50	-0.11	0.08	3794
Multiple hh	0.33	0.47	0.09	0.04	3794
Extended hh	0.13	0.34	0.03	0.85	3794

Column *mean* refers to the weighted average in the full sample, *sd* the standard deviation in the sample, *beta* the coefficient associated to coercion intensity in the linear regression of characteristic on coercion intensity, *p-value* the wild bootstrap p-value of the this coefficient and *obs* is the number of non-missing observations. The sample is further divided into the groups used in the main analysis *Dist* characteristics are in kilometers. *Freq visit* takes value 0 if there is never any visits, 1 if sometimes and 2 if often.

Table A3: 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: rank						
Coercion*After	-0.024	-0.027	-0.015	-0.019	-0.025	-0.014
	(0.008)	(0.004)	(0.007)	(0.004)	(0.011)	(0.004)
Wild boot. p-val	0.008	0.000	0.144	0.001	0.170	0.040
Coercion intensity: dummy						
Coercion*After	-0.137	-0.185	-0.117	-0.163	-0.188	-0.117
	(0.052)	(0.042)	(0.052)	(0.044)	(0.087)	(0.033)
Wild boot. p-val	0.006	0.001	0.082	0.006	0.147	0.025
Observations	9478	9308	9458	9288	6532	6411
Mean *After=0*	0.84	0.83	0.18	0.18	0.07	0.07
Mean	0.75	0.75	0.20	0.20	0.08	0.08
Fixed effects	Mother	Village	Mother	Village	Mother	Village
Identifying children	5353	9308	5341	9288	1178	6411
Identifying mothers	1525	3896	1522	3889	586	3941

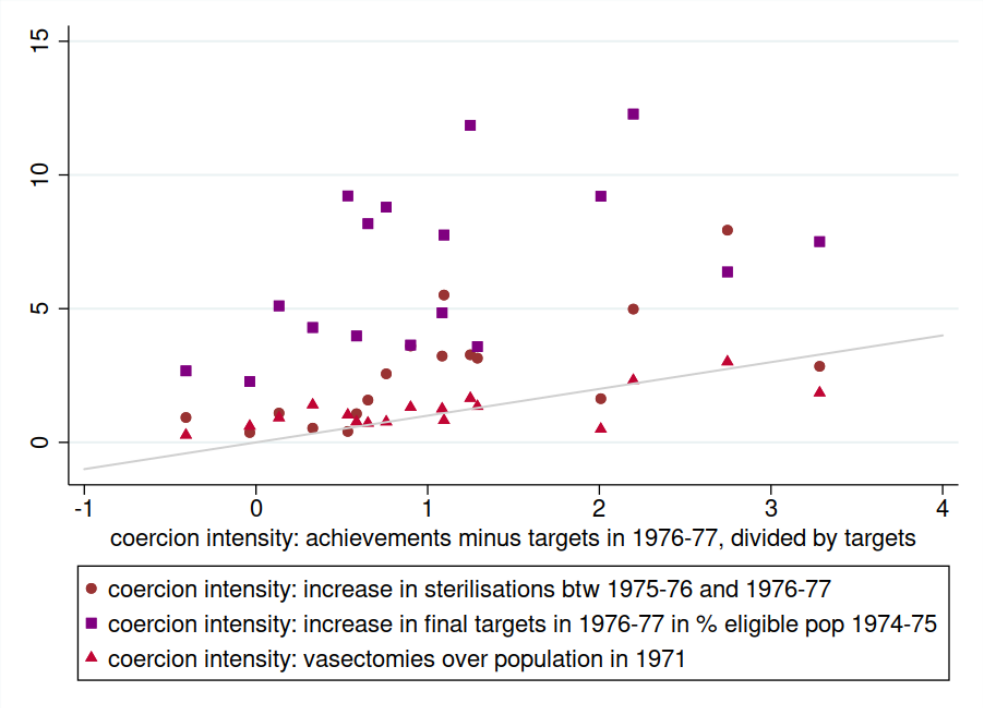
Standard errors clustered at state level are in parentheses, and corrected p-value is reported below (wild bootstrapping correction). Household weights. Estimation is run using mother (even columns) and village fixed effects (odd) for each outcome of interest (whether the child has received any vaccine, the triple antigen vaccine and whether she was born in a hospital). Coercion intensity is measured as the difference between achievements and targets in 1976-77 divided by target in 1976-77; in our sample of children 0 to 14, its mean is 1.18 and standard deviation 0.908. *After* takes value 1 for children born after Emergency (0 to 6 years old) and 0 for children born before (7 to 14 years old for symmetrical sample). Note: (i) the parameter *Intensity* can't be estimated as mothers characteristics are only observed in 1982; (ii) the parameter *After* is absorbed in the birth year fixed effects.

Table A4: Coercion intensity and other immunization outcomes

	(1) Smallpox	(2) Smallpox	(3) Tuberculosis	(4) Tuberculosis	(5) Polio	(6) Polio
Coercion*After	-0.132	-0.104	0.001	-0.022	-0.021	-0.013
	(0.040)	(0.031)	(0.020)	(0.027)	(0.028)	(0.019)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes
Rank FE & Gender	Yes	Yes	Yes	Yes	Yes	Yes
More controls	No	Yes	No	Yes	No	Yes
Wild boot. p-val	0.110	0.109	0.960	0.584	0.487	0.562
Observations	9458	9288	9458	9288	9458	9288
Mean *After=0*	0.78	0.78	0.14	0.14	0.11	0.10
Mean	0.69	0.69	0.13	0.13	0.13	0.13
Fixed effects	Mother	Village	Mother	Village	Mother	Village
Identifying children	5341	9288	5341	9288	5341	9288
Identifying mothers	1522	3889	1522	3889	1522	3889

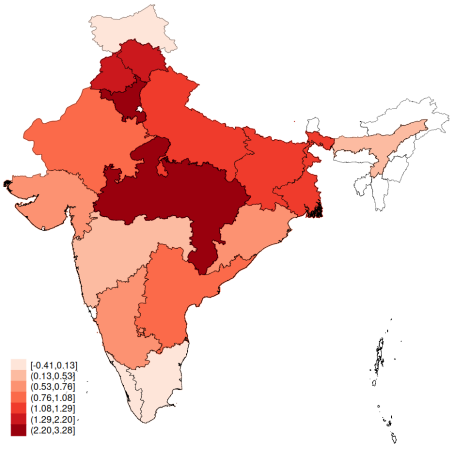
Standard errors clustered at state level are in parentheses, and corrected p-value is reported below (wild bootstrapping correction). Household weights. Estimation is run using mother (even columns) and village fixed effects (odd) for each outcome of interest (whether the child has received any vaccine, the triple antigen vaccine and whether she was born in a hospital). Coercion intensity is measured as the difference between achievements and targets in 1976-77 divided by target in 1976-77; in our sample of children 0 to 14, its mean is 1.18 and standard deviation 0.908. *After* takes value 1 for children born after Emergency (0 to 6 years old) and 0 for children born before (7 to 14 years old for symmetrical sample). Note: (i) the parameter *Coercion* can't be estimated as it is absorbed in mother or village fixed-effects, (ii) the parameter *After* is absorbed in the birth year fixed effects.

Figure A3: Comparison of coercion values across different measures

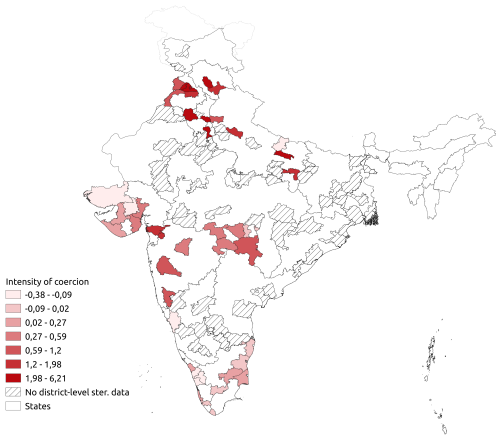


The values of the different coercion variables are reported here, for comparison purposes. The grey line is the 45 line. The different coercion variables are (i) main coercion intensity variable, based both on sterilization achievements and targets, (ii) the increase in sterilizations as compared to the previous year, (iii) is the difference in revised or non-revised targets in 1976-77 compared to original targets in 1975-76, divided by number of couples with wife 15-45 in 1974, (iv) is the absolute number of vasectomies performed in 1976-77 over population in 1971.

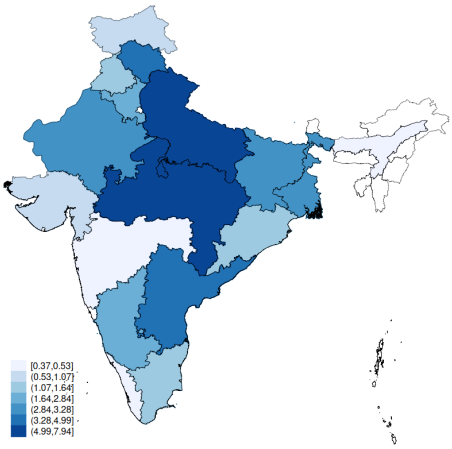
Figure A4: Spatial distribution of other coercion measures



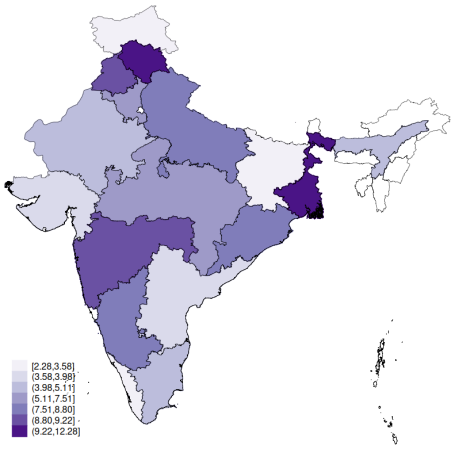
(a) Main coercion measure (ster-targ/targ)



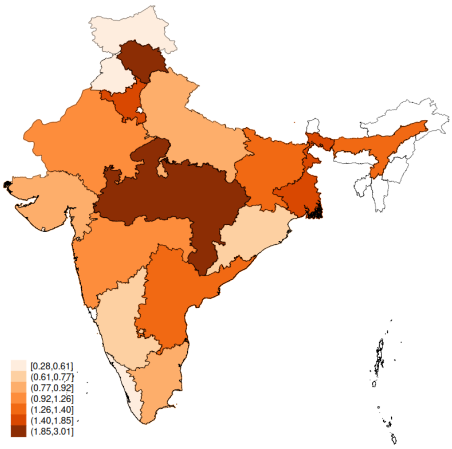
(b) District level measure, based on main



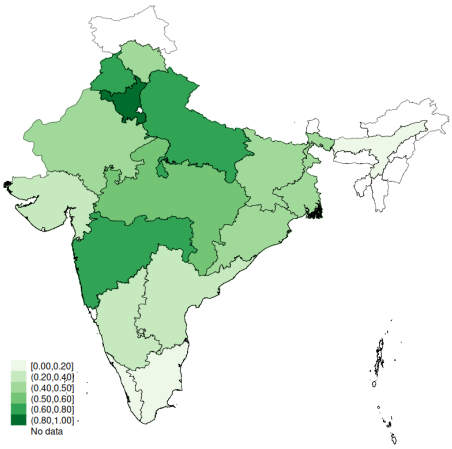
(c) Increase in sterilizations



(d) Increase in (revised) targets



(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 target (main measure); (b) is an adaption of measure (a) at district level; (c) is the increase in sterilizations as compared to the previous year; (d) is the difference in revised or non-revised targets in 1976-77 compared to original targets in 1975-76, divided by number of couples with wife 15-45 in 1974, (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 (details on construction in appendix).

Table A5: Complaints by value of coercion variables (dummies)

	Ackn. use coercion	Deaths %	Deaths /pop	FP compl. %	FP compl. /pop	Other compl. %	Other compl. /pop
Achievements - targets							
Low (8)	.250	45.67	.31	18.3	.70	36.02	2.3
High (8)	.875	54.33	.36	81.7	3.08	63.98	4.02
Increase in targets							
Low (8)	.375	39.38	.30	19.49	.85	35.16	2.55
High (8)	.750	60.62	.36	80.51	2.72	64.84	3.64
Increase in sterilizations							
Low (8)	.250	50.83	.35	29.95	1.17	43.7	2.83
High (8)	.875	49.17	.32	70.05	2.61	56.3	3.49
Vasectomies / population in 1971							
Low (8)	.375	45.67	.27	50.04	1.66	53.66	2.96
High (8)	.750	54.33	.43	49.96	2.23	46.34	3.45
Shah Commission Index							
Low (6)	.167	32.68	.31	8.06	.44	20.19	1.82
High (10)	.800	67.32	.35	91.94	2.70	79.81	3.90

This table presents the distribution of acknowledged use of coercion, reported deaths and complaints (related to family planning and others - power abuse and arrests and detention) from the Shah Commission report per type of coercion variables, transformed as dummy (below or above median) for clarity. For reported deaths and complaints, two measures are provided: the share of the total, and the number of deaths/complaints per 100,000 people in 1971. For the later, states population is aggregated by dummy value.

Table A6: Coercion intensity, husband literacy and immunization outcomes

	Any immunization		TA vaccine		Born in hospital	
	(1) Non-literate	(2) Literate	(3) Non-literate	(4) Literate	(5) Non-literate	(6) Literate
Panel A: **mother** fixed-effects specification						
Coercion*After	-0.110	-0.190	-0.019	-0.129	0.010	-0.139
	(0.029)	(0.063)	(0.025)	(0.037)	(0.016)	(0.078)
Wild boost. p-val	0.059	0.003	0.572	0.065	0.592	0.151
Identifying children	1989	3307	1986	3298	429	733
Identifying mothers	562	943	561	941	213	365
Panel B: **village** fixed-effects specification						
Coercion*After	-0.105	-0.179	-0.026	-0.154	-0.023	-0.055
	(0.016)	(0.047)	(0.017)	(0.041)	(0.009)	(0.028)
Wild boost. p-val	0.047	0.006	0.232	0.040	0.059	0.088
Identifying children	3403	5943	3400	5926	2318	4120
Identifying mothers	1404	2509	1403	2503	1422	2537
Observations	3403	5943	3400	5926	2318	4120
Mean *After=0*	0.74	0.91	0.14	0.21	0.03	0.10
Mean	0.68	0.81	0.14	0.25	0.03	0.11

Standard errors clustered at state level are in parentheses, and corrected p-value is reported below (wild bootstrapping correction). Household weights. Coercion intensity is measured as the difference between achievements and targets in 1976-77 divided by target in 1976-77; in our sample of children 0 to 14, its mean is 1.18 and standard deviation 0.908. *After* takes value 1 for children born after Emergency (0 to 6 years old) and 0 for children born before (7 to 14 years old for symmetrical sample). *Literate* takes value 1 if the husband is literate, and 0 otherwise.