The Importance of Being Local? Administrative Decentralization and Human Development*

Latika Chaudhary
Naval Postgraduate School

Lakshmi Iyer
University of Notre Dame

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Abstract

We examine the human development consequences of transferring responsibility for public service provision to local governments in India, using state-level variation in the timing of administrative decentralization reforms. We find that devolution of the responsibility for health functions from state to local governments, without concomitant authority over personnel or taxation, results in a worsening of neonatal, infant and under-5 child mortality. Such partial devolution results in worse indicators of public health provision, as well as lower rates of primary school completion. Our results cannot be attributed to differential pre-trends, omitted variables bias, or heterogeneous treatment effects.

JEL codes: H77; H75; D73; H41

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*Emails: lhartman@nps.edu; liyer@nd.edu. We are grateful to several seminar and conference participants for helpful comments. We thank Uriel Galace, Michael Morris and Maya Reddy for able research assistance, and the Helen Kellogg Institute for International Studies at the University of Notre Dame for financial support.

1 Introduction

This paper examines the human development consequences of administrative decentralization. Administrative decentralization refers to the transfer of responsibility for providing public services from the central government and its agencies to sub-national or subordinate levels of government.[1] Administrative decentralization is distinct from fiscal decentralization, which involves the transfer of tax-and-spend powers to sub-national governments. It is also distinct from political decentralization, which refers to the extent to which local governments are directly elected by citizens. The effectiveness of administrative decentralization thus depends on the incentives and capabilities of local officials, rather than the extent of fiscal resources or electoral accountability. Administrative decentralization is very common across the world: 123 countries have implemented administrative decentralization reforms between 1970 and 2014 (Tester, 2021). The development consequences of administrative decentralization are theoretically ambiguous, and previous empirical studies have found conflicting results.

We examine a major decentralization reform in India, known as the *Panchayati Raj* Act, which was implemented via a constitutional amendment in 1993. States were required to establish a three-tier system of local government, comprising of village, intermediate and district level governance bodies. The Act contained many provisions for a comprehensive program of administrative, fiscal and political decentralization. For administrative decentralization, 29 functional areas were slated for administrative devolution to these local government bodies, including education and public health facilities and staff, water provision, and sanitation. Based upon a

[1] Technically, administrative decentralization can occur via "deconcentration" to different levels of the central government, "delegation" to semi-autonomous bodies or "devolution" of authority to lower level governments. In a devolved system, local governments have clear and legally recognized geographical boundaries over which they exercise authority and within which they perform public functions (World Bank, 2001). We focus on administrative devolution in this paper, and will be using the terms "decentralization" and "devolution" interchangeably.

detailed reading of many official reports and documents, we document considerable variation in the timing of *de facto* administrative decentralization across the states of India. We also document variation in the process of decentralization: some states transferred (some) authority over employees to local governments as part of devolution ("full devolution"), while other states did not ("partial devolution"). Using these data, we conduct a difference-in-difference (DiD) estimation of the impact of decentralization on health and education outcomes, comparing outcomes before and after partial and full devolution, to outcomes in states that did not introduce any administrative devolution.

Our main result is that devolution of responsibility to local governments, without concomitant authority over personnel or over funds, is detrimental to child mortality and primary schooling. While there were nationwide improvements in child health outcomes in this period, states that conducted partial devolution of health functions experienced significantly lower gains in neonatal, infant and under-5 child mortality compared to states that did not devolve health functions. Infant mortality in these states increased by 1.36 percentage points (25% of the mean) after such partial devolution. We find no significant differences in child mortality rates between states that did a full devolution and those that did not. Similarly, primary school completion rates decreased by 4.2 percentage points after partial devolution, while there is no difference for states that conducted full devolution. We conduct a number of robustness checks and verify that our results are not driven by state- and time-varying factors such as other ongoing initiatives (particularly political decentralization), changes in state health budgets, differential pre-trends between the treated and untreated states, or heterogeneous treatment effects across early and late adopters of devolution.

We examine the interaction with other dimensions of decentralization. Political decentralization occurred prior to administrative decentralization in all the states, and controlling for its timing does not change our findings. While we do not have precise data on fiscal devolution, we find that the negative effects of partial devolution are

not present when there is some degree of fiscal devolution. This suggests that having control over funding can compensate, to some extent, for a lack of authority over employees.

We show evidence that partial devolution results in a decline in the quality of public service provision, such as the delivery of prenatal care and vaccination and the building of public schools. Consistent with this public service decline, we find that child mortality rate increases are higher among poor households, who are the most reliant on public services. Given the high level of son preference in India, we also find slightly higher mortality increases, and much larger declines in primary school completion, among girl children. This suggests that partial devolution is likely to increase pre-existing inequalities in human capital attainment.

We make three main contributions to the large literature on decentralization. First, we provide a well-identified study on the effects of administrative decentralization, that is not confounded with other dimensions of decentralization. Many prior reviews have found conflicting or inconclusive effects of decentralization on a range of outcomes (Bardhan and Mookherjee, 2006a; Treisman, 2007; Faguet and Pal, 2023). This is often due to conflating different concepts and definitions of decentralization (Faguet, 2021), as well as employing relatively weak identification strategies.[2] Many recent studies, based on credible DiD or regression discontinuity designs, nevertheless find conflicting results. Several studies have documented the positive effects of decentralization on service delivery, economic growth or well-being (Dahis and Szerman, 2021; Elacqua et al., 2021; Fleche, 2021; Narasimhan and Weaver, 2023), while several other studies have found negative effects of decentralized governance (Cassidy and Velayudhan, 2022; Cohen, 2024; Malesky et al., 2014).[3] Many of these studies focus on a package of policy measures incorporating elements of political, administra-

[2]In their review of 34 empirical studies of full decentralization, Channa and Faguet (2016) classify only ten of these as having "very strongly credible" or "strongly credible" identification strategies.

[3]See Gadenne and Singhal (2014) for a review of fiscal decentralization in developing countries and Mookherjee (2015) for a review of political decentralization.

tive and fiscal decentralization, or focus on a single type of decentralization without considering the role of other dimensions. We identify the effects of administrative decentralization after controlling for political decentralization and investigate how administrative decentralization effects are shaped by the presence or absence of fiscal decentralization. There are few analyses of such interaction effects along these different policy dimensions (Enikolopov and Zhuravskaya (2007) is a notable exception in the cross-country setting).

Second, our context allows us to compare the effects of partial versus full administrative decentralization. We are unaware of any other study that is able to do such an empirical comparison within the same country. Previous studies have typically focused on providing evidence towards specific mechanisms (e.g. Dal Bó et al. (2021) highlight the informational advantage of subordinate levels of government), examined heterogeneous effects of decentralization in different types of areas (Galiani et al., 2008) or provided descriptive case studies highlighting the importance of local governments having authority over local service providers (Ahmad et al., 2006).

Third, our results contribute to the theoretical understanding of when and how decentralization can work well. A large theoretical literature has highlighted both positive and negative consequences of decentralization. Devolving administrative authority to local governments can improve public service delivery because of better information availability, better knowledge of citizens' preferences or better monitoring capacity at the local level (World Bank, 2004; Bardhan and Mookherjee, 2006a). However, service delivery could worsen if state capacity is weaker at the local level, if local officials are more likely to be corrupt, if decentralization results in loss of economies of scale, or if local elites can more easily capture public resources (Oates, 1972; Smith, 1985; Besley and Coate, 2003; Bardhan and Mookherjee, 2005, 2006b).[4]

[4]Prior empirical studies have documented that better monitoring of public service providers results in improved service delivery (Björkman and Svensson, 2012; Duflo et al., 2012; Muralidharan et al., 2017, 2021).

Our results highlight that state capacity at the local level depends crucially on the governance structure, namely whether local governments have been given authority over personnel and/or funds. Partial decentralization resulting in worse outcomes than full decentralization is in line with theoretical models showing that the presence of "multiple principals" (in our case, state and local governments) can lead to free-riding in monitoring effort, resulting in greater moral hazard or adverse selection among the agents (Martimort, 1996; Dixit, 1997; Gailmard, 2009).[5]

The rest of the paper is structured as follows: Section 2 outlines the Indian decentralization reforms, Sections 3 and 4 describe our data, and Section 5 delineates our empirical strategy. Sections 6 and 7 document our results and Section 8 concludes.

2 Decentralization Reforms in India

2.1 India's *Panchayati Raj*

In April 1993, the 73rd and 74th amendments to the constitution of India, also known as the *Panchayati Raj* Acts, came into force.[6] Each state was required to set up a three-tier system of local government with village, intermediate and district level governance bodies, known as *panchayats* or *Panchayati Raj* institutions. In terms of administrative decentralization, 29 functional areas were to be devolved to these *panchayats*, including services such as water provision, sanitation, education, public health and roads (see Table A.1). The amendments included provisions for political decentralization: all members of these local bodies were to be directly elected by the people every five years, State Election Commissions were to be established to conduct

[5]In a non-decentralization context, the presence of multiple principals has been associated with worse delivery of public services (Gulzar and Pasquale, 2017) but also with lower regulatory bottlenecks resulting in greater firm entry (Dutta et al., 2022).

[6]The 73rd Amendment applied to rural bodies and the 74th amendment applied to urban local bodies. In 1996, the Indian Parliament extended the provisions to Scheduled areas via the *Panchayats* Extension to Scheduled Areas Act (PESA). See Center for Policy Research (2014) for details.

such elections, and one-third of all *panchayat* seats (and a third of all council head positions) were mandated to be filled by women. To facilitate fiscal decentralization, the amendments called for State Finance Commissions to be periodically set up, which would provide recommendations on revenue-sharing and make grants to these local government institutions.

Part of the impetus for the 1993 legislation was to improve public service delivery and human development outcomes (Chaudhuri, 2006). India was ranked 130 out of 189 countries in the U.N.'s Human Development Index in 2018. Despite government policies in the 1970s and 1980s that largely equalized access to education and health facilities (Banerjee and Somanathan, 2007), India's public services suffered from widespread absenteeism and lack of effort by service providers (Chaudhury et al., 2006; Pritchett, 2009). As a result, a majority of patients chose to visit private doctors (Das et al., 2016), and 45% of primary school students were enrolled in private schools in 2020.

While some *panchayats* did exist before the 1990s, Ghatak and Ghatak (2002) argue that most of these were generally ineffective since elections were not held regularly and they did not assume any active role in public service provision. A few states, such as Gujarat, Maharashtra and West Bengal, did have effective *panchayats* and regular elections prior to the 73$^{\text{rd}}$ and 74$^{\text{th}}$ Amendments. However, all states had to modify their existing legislations along several dimensions to be in compliance with the constitutional amendments.

2.2 Progress of decentralization reforms

Most states amended or passed new *Panchayati Raj* Acts immediately in 1993 and 1994. All of these new state legislations called for the devolution of education and public health services, among others, to village, intermediate and district *panchayats* in accordance with the constitutional amendment. However, our perusal of these laws reveal that they varied considerably in their specificity. For example, Gujarat

clearly specified the powers and duties for each of the three *panchayat* tiers, and Maharashtra's legislation enabled *panchayats* to appoint and pay staff using their own funds. However, some other states had only vague language such as "Village panchayats *may* perform functions related to health."

There was generally slow progress in actually devolving responsibility over public service provision to *panchayats*, with state health and education departments retaining control in most states (Chaudhuri, 2006). Disappointed with the slow and uneven pace of administrative devolution, the central government in the 2000s began to ask states to move faster. The Ministry of *Panchayati Raj* became a separate ministry in May 2004 and took on a stronger advocacy role. The central Planning Commission published a report asking states to (a) conduct an "activity mapping" exercise for each devolved function that would unbundle the functions into smaller units of work and articulate the powers and duties vis-à-vis those smaller units to each *panchayat* tier, and (b) pass executive orders to operationalize these activity mapping exercises (Government of India, 2006). The report also highlighted that many states had not devolved the functionaries, i.e. made public employees fully accountable to local governments. Rather, public workers were still managed and monitored by their state-level departments. We observe an increase in the number of states enacting administrative devolution after this report. However, as late as 2015, more than two decades after the constitutional amendment, seven out of 25 states had not yet undertaken the steps required for effective administrative decentralization (details in section 3.1).

Progress on fiscal decentralization has been even slower. *Panchayats* are funded from four sources: central grants based on the recommendations of the five-yearly Central Finance Commissions; funds from centrally sponsored schemes such as the National Rural Employment Guarantee program (NREGA); loans and grants from state governments based on the State Finance Commission recommendations; and their own sources of taxes and user fees on public services. Most local govern-

ments remain highly dependent on the state or central government for their revenues: as late as 2015, local governments generated only 8% of revenues from their own sources of taxation and fees (Government of India, 2016a), compared to 6% in the 1990s (Government of India, 2000). Most accounts suggest that *panchayats* have failed to expand their revenues because they change tax rates infrequently, they lack administrative capacity, they do not own productive assets and they are unable to charge user fees on state or central government properties (Government of India, 2016b).

Political decentralization has progressed faster than administrative or fiscal decentralization. By 2010, all states had conducted local government elections with the one-third gender quota as specified by the constitutional amendments, though there was considerable variation in the timing of elections across states (Iyer et al., 2012). The effects of this gender quota have been examined by many prior studies, some of which find that women's political representation changes policy outcomes towards those preferred by women (Chattopadhyay and Duflo, 2004; Iyer et al., 2012), while others find no effect or even a lower efficiency of pro-poor targeting (Bardhan et al., 2010; Rajaraman and Gupta, 2012; Afridi et al., 2017). Starting in 2006, many states have increased the gender quota to one-half of all local council positions (Iyer and Triyana, 2022).

3 Data on Decentralization Progress

3.1 Measuring administrative devolution

We code effective administrative devolution as the date when activity mapping was operationalized i.e. the responsibilities for each *panchayat* tier was clearly delineated and brought into force via legislation or executive order. To find the dates of devolution for health and education functions over the period 1993 to 2015, we read several different government publications and reports (see Table A.2), as well as consulted

many state *Panchayati Raj* websites for specific government or executive orders. For some states, like Gujarat and Maharashtra, their *Panchayati Raj* legislations contain activity mapping provisions, so that the date of legislative enactment serves as the date of operationalization. Other states codified activity mapping and passed operationalizing orders several years after the passage of their *Panchayati Raj* legislation.[7]

What did activity mapping look like in practice? While there is some variation across states, we find that district *panchayats* typically coordinate participation and promotion of national and state health programs, pass information from lower levels of government to the state, and coordinate measures against epidemics and other infectious diseases. District and intermediate *panchayats* are authorized to undertake surveys and reports, procure medicines and medical equipment (e.g. X-ray machines), promote immunization and child welfare programs, and oversee construction of public health facilities such as dispensaries, primary health centers or subcenters. Village *panchayats* help with the execution of specific public health programs, identify land for primary health subcenter construction, manage cleaning and latrine construction, control stray dogs, remove animal carcasses, and support other such local functions.

With regard to education, the central government asked states to model their activity mapping exercise on Kerala. Village *panchayats* in Kerala manage government pre-primary and primary schools including the maintenance of schools and monitoring of teachers. They also manage libraries and implement literacy programs. In other states such as Assam, village *panchayats* promote enrollment and attendance in primary schools and monitor primary school teachers, while intermediate *panchayats* manage the school buildings and district *panchayats* oversee surveys and other

[7]Our sources exhibit some ambiguity about the date of functions devolution in the cases of Bihar and Rajasthan. For Bihar, official sources list the date of health devolution as 2014, but some descriptive accounts suggest that this may have happened as early as 2011. For Rajasthan, the official sources describe activity mapping and devolution in 2003, but field observations on select Rajasthani *panchayats* found that they were not performing any health functions (John and Jacob, 2016). We verify that our results are not sensitive to recoding these dates.

educational programs.

Our data reveals considerable variation in the timing of functions devolution (Table 1). Only a handful of states effectively devolved both health and education before 2000, and devolution activities increased after the publication of the Planning Commission report in 2006. Despite this progress, seven states (out of 25) had not devolved health functions as of 2015 and six had not devolved education. All but one state devolved education at the same time as health; in fact, many states devolved drinking water, family welfare and women and child development at the same time as well. Our estimates of the impact of health (or education) functions devolution should thus be interpreted as a result of the devolution of this larger policy bundle.

Why do dates of administrative decentralization vary across states? Many factors appear to play a role including a prior history of well-functioning local governments (e.g. Gujarat and Maharashtra), party ideology (e.g. West Bengal and Kerala devolved when Communist parties were in power), political factors such as intra-party competition (Bohlken, 2016), and nudges from the central government such as the 2006 Planning Commission report. In terms of our difference-in-difference analysis, factors such as a state's history or long-standing political institutions will be controlled for by the inclusion of state fixed effects. National factors such as central government actions are accounted for by the inclusion of year fixed effects. Our estimates may be biased if the timing of devolution happens to be correlated with pre-existing trends in our outcome variables and/or other state-specific budget or policy changes that also affect our outcomes. We conduct several robustness tests to verify that this is not the case.

3.2 Partial versus full administrative devolution

We track whether administrative devolution was accompanied by local authority over public employees, namely "the extent to which the government employees are deployed to *panchayats* and have been made accountable to *panchayats'* political executives and

whether *panchayats* have their own employees, the powers and functions of *panchayats* in terms of selection, appointment, salary payment, transfer, removal, etc" (Alok, 2014). In the absence of devolution, the hiring, monitoring and supervision of public service employees (doctors, nurses, teachers and other public health and education workers) is completely controlled by state-level departments. No state has devolved full control of all employees to local governments, but many states have allowed for more monitoring and supervision of public sector workers by *panchayats* and some hiring of lower level health and education workers as *panchayat* employees. For instance, in Madhya Pradesh and Maharashtra, *panchayats* can hire their own lower-level workers. In Kerala and Karnataka, public sector workers come under the dual control of *panchayats* and their respective state health or education departments. *Panchayats* in Kerala cannot hire or fire doctors that are hired by separate state cadres and paid from state funds, but they can hire lower level workers such as ambulance drivers or janitors (John and Jacob, 2016). We therefore make a distinction between administrative devolution reforms that involved no authority over employees ("partial devolution") and those that conferred some local authority over employees ("full devolution").

Our data sources are less precise when it comes to the exact date of employee authority devolution. For states where such provisions were included in legislation, we have a precise date. For others, we rely on information reported as of 2007 and 2015 (Government of India, 2008, 2016a). We adopt an "earliest possible date" rule as follows: For states that had devolved employees as of 2007, we code them as having done it at the same time as the devolution of responsibility over health functions. For states that had devolved health functions by 2007, and employees by 2015, we assign them an employee devolution date of 2008 because of the flurry of activity by states following the publication of the Status of *Panchayati Raj* report. For states that devolved health functions after 2007, and had devolved employees by 2015, we code them as having devolved employees at the same time as health functions. By this

measure, 13 of the 18 states that had devolved health functions had also devolved health employees by 2015.[8] We code the devolution of education employees in a similar manner.

3.3 Political and fiscal decentralization

We track the progress of political decentralization by coding the first year when the state conducted local council elections with the one-third gender quota, based on data from Iyer et al. (2012). Table 1 shows that political decentralization progressed much faster than administrative decentralization. By 2010, all states in our sample had implemented the gender quota.

Our data on fiscal or funds devolution is limited because there is no uniform and consistent database of annual local government finances, a fact noted and bemoaned by multiple Central Finance Commissions. This makes it difficult to measure the extent of fiscal decentralization using measures such as the fraction of local government revenues that are raised by them versus granted by upper levels of government. The 2015-16 Devolution Report includes the taxes collected by the different *panchayat* tiers in each state as of 2015 (Government of India, 2016b). We create a crude indicator of fiscal decentralization that equals one if *panchayats* at each tier (district, intermediate and village) report collecting their own taxes. By this measure, only five states had implemented fiscal decentralization; all these states also report their *panchayats* collecting user fees.

Since state and central government funding accounts for the bulk of local government revenues, we need to ensure that our results on administrative devolution are not driven by concurrent changes in such funding sources. We construct several

[8] As a consistency check, we examine the number of employees per 1000 population in 2015, which includes both the local government's own employees such as the village *panchayat* secretary and employees transferred to local governments such as public health workers (Government of India, 2016b). "Full devolution" states reported 1.5 employees per 1000 people compared to 1.1 for "partial devolution" states.

measures of local funding from state budget data: annual per capita state government spending on health and education, a larger category of annual social spending by the state (the sum of education, medical and public health, and water supply and sanitation), state government contributions to local bodies and per capita funding from the central government to local bodies (based on Central Finance Commission grants).

Before concluding this section, we want to emphasize that our specific measures of effective devolution of public health and education are more precise than aggregate devolution indices constructed by different organizations and scholars since the mid 2000s. This is both because these indices combine facets of administrative, fiscal and political decentralization into a single index, and because the components of these indices changed from year to year (see Government of India (2016b) for more details on the construction of the different indices).

4 Data on Human Development Outcomes

4.1 Health outcomes

We obtained data on health outcomes from the nationally representative National Health and Family Survey (NFHS) of 2015-16 (part of the widely used multi-country Demographic and Health Surveys), covering more than 568,000 households. The survey collects retrospective data on fertility and child survival from women aged 15-49, enabling us to construct a detailed cohort-level dataset.

We focus on child mortality as our main health outcome, for two reasons. First, infant mortality is a widely used measure of human development in both within-country and cross-country settings. Second, this outcome has the potential to be greatly affected by the functioning of the health system which provides pregnant mothers with information about proper nutrition and potential complications, services such as prenatal checkups and referrals to other health facilities, and inputs such as

prenatal vitamins and infant immunizations. Prior work, for example, in Uganda has shown that better monitoring of the public health facilities resulted in large declines in child mortality (Björkman and Svensson, 2012; Björkman-Nyqvist et al., 2017).

We compute child mortality at three early life stages: neonatal mortality is an indicator variable that equals one if a child died within the first month of birth, infant mortality indicates whether a child died within the first year of birth and under-5 mortality indicates whether a child died within the first five years of birth. Note that these variables are conditional on the child reaching the specified age e.g. under-5 mortality is not defined if the child was born less than five years prior to the survey. All these child mortality measures have been decreasing over the period 1990-2015 (Figure A.1).

Since these outcomes are conditional on a child being born, we also examine whether devolution is correlated with changes in the decision to give birth, and with changes in the sex of the child. For instance, if health facilities dramatically improved as a result of devolution, more families may decide to conceive children and the resulting increase in demand for health services may in turn have a detrimental effect (Malhotra, 2019). Prior literature has also shown that greater in-utero stress, nutritional or otherwise, leads to more girl births (Waldron, 1983; Low, 2000; Gluckman and Hanson, 2005). To account for these possibilities, we study two additional outcomes, namely fertility (a dummy for whether a woman gave birth in a specific year) and a dummy for whether the child born was female.

Our estimation sample includes birth cohorts born between 1990 and 2016. We drop states and districts where the *Panchayati Raj* Act was not applicable.[9] In our final sample, we have 25 states that account for 95% of India's population, covering almost 1.1 million births over 26 birth cohorts (see Table A.3 for summary statistics).

The NFHS surveys also ask questions about prenatal care provision and immu-

[9]These are the states of Jammu and Kashmir, Meghalaya, Mizoram and Nagaland, and a few hill districts in the states of Assam, Manipur, Tripura and West Bengal.

nization status of all births in the previous three or five years. To construct a partial panel of such variables over time, we pool together four waves of NFHS surveys.[10] We track the following prenatal care outcomes: whether a mother had three or more prenatal health visits over the course of her pregnancy, whether a tetanus shot was provided and whether iron supplements were provided. We also measure the immunization status of all children over the age of 12 months, who are required to have at least eight vaccinations in their first year of life (three polio shots, three shots of DPT and one each of BCG and measles vaccines). We construct indicators of whether the child had no vaccines, had at least one vaccine or was fully vaccinated. Finally, we obtained partial data on the number of health facilities (primary health centers and subcenters as well as community health centers) in 1985, 1990, 1997, 2002, 2007, 2012 and 2015 (Government of India, 2015).

4.2 Education outcomes

We use the 75^{th} round of the National Sample Survey (NSS) conducted between July 2017-June 2018 to construct measures of educational attainment. The NSS is a nationally representative survey of more than 500,000 individuals from 113,757 households across all Indian states. Our two main outcomes are primary school completion and middle school completion, since the devolution reform specified devolution of primary and middle schools (Table A.1). Typically, students are enrolled in primary education from ages 6 to 10, and in middle school (grades 6-8) from ages 11 to 13. To allow for potential delays in school enrolment and progression, we restrict our sample to individuals aged 14 and above for primary school completion, and to those aged 17 and above for middle school completion. These age restrictions imply that we are only able to examine school completion outcomes for cohorts exposed to devolution

[10]The NFHS 1992-93 has these data for births in 1988-1993, NFHS 1998-99 covers births from 1996-1999, the NFHS 2005-06 has data for births in 2001-2006 and the NFHS 2015-16 covers births in 2010-2016.

in year 2008 or earlier i.e., we are unable to examine the full range of devolutions for education outcomes. Since there have been large secular increases in schooling across India in the past several decades (see Figure A.2), we exclude very old cohorts from our analysis by restricting to individuals aged 35 and below at the time of the survey. Additionally, we examine state level panel data on the number of public schools over 1985-2016 and the number of teachers in public schools over 2001-2015, obtained from the website of the Ministry of Education.

5 Empirical Strategy

Our empirical strategy compares child mortality and education outcomes across birth cohorts that were exposed to devolution to those that were not. Variation in such exposure comes from the timing of devolution across different states. Our difference-in-differences (DiD) regression specification takes the following form:

$$Y_{ist} = \alpha_s + \delta_t + \beta * AnyDev_{st} + X_{ist}\gamma + \epsilon_{ist} \tag{1}$$

where Y_{ist} is the health or education outcome of individual i born in state s and birth year t. Our main explanatory variable $AnyDev_{st}$ is an indicator that equals one if the individual born in state s and year t is exposed to administrative devolution. For health outcomes, this indicator equals one if the state has devolved health functions at least one year before the individual is born, i.e., $AnyDev_{st} = 1$ if state s devolves health functions in year $t-1$ or earlier. We prefer this lagged specification, since devolution can affect child mortality outcomes via changes in effective prenatal care that take place over the course of pregnancy.

α_s and δ_t are fixed effects for state s and birth year t respectively, that control for time-invariant state characteristics and annual factors that affect all states such as national elections or the National Rural Health Mission introduced in 2005. X_{ist} controls for characteristics of the individual or household that could affect health or

education outcomes. For health outcomes, these include dummies for rural versus urban residence, caste and religion of the household, mother's age at birth and its square, mother's birth year, education, age at marriage and height (an indicator of the mother's nutritional and health history). We also control for the gender and birth order of child i in X_{ist}, since prior research has shown that health outcomes differ by birth order and gender (Jayachandran and Pande, 2017).

As described before, many states undertook only a partial administrative devolution by devolving responsibility for health functions to local governments but not devolving employee authority. Theoretically, full devolution may be more effective in improving public services (e.g. via better monitoring of personnel) or could make things worse (e.g. if local authorities can be more easily bribed to overlook employee absenteeism). We examine the impact of partial versus full devolution using the following specification:

$$Y_{ist} = \alpha_s + \delta_t + \beta_1 AnyDev_{st} + \beta_2 FullDev_{st} + X_{ist}\gamma + u_{ist} \tag{2}$$

where $AnyDev_{st}$ equals one if the state had conducted administrative devolution, while $FullDev_{st}$ equals one if the state had additionally devolved authority over employees. All other terms are the same as in equation (1). The coefficient β_1 therefore represents the impact of partial devolution i.e. devolving responsibility over functions but not authority over employees, and β_2 reflects the additional impact of devolving authority over employees. The impact of full devolution is therefore $\beta_1 + \beta_2$.

There are three main threats to identification in our setting. First, this DiD approach assumes that states that devolved health functions would, in the absence of devolution, have had parallel child mortality trends to states that did not devolve health. Second, the timing of decentralization across states may be correlated with other economic or political factors that may independently affect our outcomes. Third, our estimator may be biased in the presence of heterogeneous treatment effects that vary across early versus late reformers. We examine these concerns in detail in section 6.2.

6 Administrative Devolution and Health Outcomes

6.1 The effect of partial versus full devolution

Our estimates from equation (1) show that the devolution of health functions from state to local governments results in increases in neonatal, infant and child mortality (Table 2, columns 1, 3, 5). The estimate for infant mortality is statistically significant at the 10% level of significance, while that for under-5 mortality is significant at the 5% level. P-values from a wild bootstrap procedure are shown in brackets below the standard errors in parantheses, and lead to similar conclusions regarding statistical significance.

This overall detrimental effect is entirely attributable to states that conducted partial devolution i.e. did not devolve any authority over employees to local governments. The estimated β_1 coefficients from specification (2) indicate that partial devolution increases neonatal mortality by 0.75 percentage points, infant mortality by 1.36 percentage points and under-5 mortality by 1.62 percentage points (Table 2, columns 2, 4 and 6). These are large effects, corresponding to 19%, 25% and 24% of the sample means, and they are all statistically significant at the 1% level. These effect sizes are comparable to the effect of socioeconomic covariates: partial devolution increases neonatal mortality by 0.04 standard deviations (Table 2, column 2), comparable to the 0.08 standard deviation increase when mothers have no education compared to those who have some education.

The estimated β_2 coefficients are negative, statistically significant and similar in magnitude to the β_1 coefficients. Full devolution therefore increases neonatal, infant and under-5 mortality by 0.14, 0.38 and 0.6 percentage points respectively ($\beta_1 + \beta_2$), much smaller than the effects of partial devolution. We verify that the sum of the β_1 and β_2 coefficients is not statistically different from zero (see p-values in Table 2, columns 2, 4, 6). Disappointingly, our results show that full administrative decentralization does not lead to improvements in health outcomes.

6.2 Robustness checks

We subject our results from specification (2) to a series of robustness checks, as detailed below.

Differential pre-trends: To check whether health outcomes were trending differently in states that devolved, we plot year-by-year coefficients of the impact of devolution for five years before devolution and six years after devolution in an "event-study" graph (Figure 1). For each of our outcomes, the left hand plot shows the estimated β_1 coefficients, while the right hand plot shows the estimated β_2 coefficients, while the vertical line represents the timing of devolution. We find, reassuringly, that none of these coefficients is statistically significant in years prior to either partial or full devolution, while most of the post-reform coefficients are statistically different from zero. These significant effects can be discerned very quickly after the devolution date, and are stable for several years after devolution i.e. our results on mortality increases are not simply due to temporary transition issues.

State- and time-varying omitted variables: We show that our results remain similar to the baseline results of Table 2 when we control for the timing of gender quota implementation (Table 3, columns 1, 3 and 5). This is important to verify since prior research has shown a strong role of women leaders in improving health outcomes (Bhalotra and Clots-Figueras, 2014; Brollo and Troiano, 2016; Bhalotra et al., 2022). We show that the results remain very similar in magnitude and statistical significance when we control for annual state per capita spending on the category of medical and public health services (Table 3, columns 2, 4 and 6). Our results also remain similar if we control instead for other dimensions of spending such as per capita state social spending (which includes education, public health, water and sanitation), per capita state transfers to local bodies and per capita central government funding to *panchayats* (Table A.4). These results highlight that administrative devolution was not correlated with changes in state budgetary priorities. Additionally, our results are robust to controlling for state per capita GDP and state-specific time trends,

though we recognize that state per capita GDP may be endogeneously shaped by health outcomes (Table A.5).

Recoding specific devolution dates: We examine sensitivity to recoding of devolution dates that we were unsure of (see section 3.1), namely recoding Bihar's devolution date to 2011 rather than 2014, recoding Rajasthan as "not devolved," and changing the date of employee authority devolution to three years ahead for the states where the documents did not clearly specify a date (see section 3.2). While our estimated coefficients are somewhat smaller in magnitude (consistent with greater measurement error in our explanatory variables), our substantive conclusions are unchanged: partial devolution results in a significant worsening of child mortality outcomes, while the effect of full devolution ($\beta_1 + \beta_2$) is statistically indistinguishable from zero (Table A.6).

Changing the estimation sample: We rerun specification (2) after dropping families that report moving to their current area of residence after the child was conceived, or were recorded as visitors at the time of the survey.[11] This helps address the potential concern that families may migrate in response to better or worse quality of public services, as shown in some other settings (Urquiola, 2005). Our β_1 and β_2 coefficients retain their size and significance for all three child mortality outcomes even with this restriction (Table A.7, columns 1, 3 and 5). To rule out the concern that different types of households may choose to give birth before and after devolution, we re-run our regressions with mother fixed effects, which effectively controls for any time invariant unobservable differences across mothers that could be correlated with a child's health outcomes. While the estimated coefficients are slightly smaller in magnitude, they remain statistically significant (Table A.7, columns 2, 4 and 6).[12]

[11] Most migration in India is within-state, so that even if households have moved, they would still have been subject to the state's devolution reforms. The data do not allow us to distinguish between within-state and cross-state migrants, so that this restriction is likely to be more conservative than needed.

[12] Note that identification in this sample arises from women who have had multiple children,

We further verify that our results are not being driven by any one state-specific policy by rerunning our main regressions, dropping one state at a time. The resulting coefficients for both β_1 and β_2 lie within a relatively narrow band (Figure A.3).

Heterogeneous treatment effects: The DiD estimator is a weighted average of several different comparisons of "treated" units with "not-yet-treated" units and with "already treated" units. If states that devolve early have a different treatment effect than those that devolve later, then some of these comparisons may be entering with negative weights and thereby leading to biased and misleading DiD coefficients. To assess the validity of this concern, we first present our DiD estimates (based on equation 1) separately for the sample of states that did not devolve employee authority and for those that did, as recommended by de Chaisemartin and D'Haultfoeuille (2020). The results are shown in Table 4, panel A, and are consistent with those of Table 2: partial devolution leads to significantly worse child health outcomes across all three measures of child mortality (columns 1, 3, 5), while full devolution has no significant effect on child mortality outcomes (columns 2, 4, 6). Examining the weights involved in computing these DiD estimators (Goodman-Bacon, 2021), we find that the sum of the negative weights in any of these specifications is a maximum of 0.13 (out of a total of 1). We conclude that heterogeneous treatment effects by state are unlikely to be a source of bias in our analysis.

We also construct two alternative DiD estimators, as suggested by the recent literature. The first is based on de Chaisemartin and D'Haultfoeuille (2020), and compares outcomes from period (*t-1*) to period t (date of devolution) between groups that switch from untreated to treated with groups that are untreated at both dates (the "instantaneous" effect). A modified version of this compares the outcomes from period (*t-1*) to period (*t+6*), the sixth dynamic effect. Reassuringly, we find consistent results across the standard DiD estimates in panel A of Table 4 and the

and had births in both pre-devolution and post-devolution periods. Since this may not reflect the characteristics of the nationwide representative sample, we show this as a robustness check.

alternative estimates in panel B. In both cases, partial devolution increases neonatal, infant and under 5 mortality, though the coefficient magnitudes are smaller in panel B and some of the "instantaneous" effects lose statistical significance.

The second alternative DiD estimator is constructed using only the "never-treated" units as the control group (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). For ease of computation, we pool the data to the state-birth year level and run weighted regressions using counts of individuals in each state-birth year as weights. These alternative estimators are similar in sign and significance for states that did partial devolution, showing a rise in all three measures of child mortality (panel C, columns 1, 3, 5). This alternative DiD estimator shows increases in infant and under-5 mortality even for states that did full devolution (panel C, columns 4 and 6); however, these effects are not apparent in the year-by-year graphs based on this strategy (Figure A.4).

6.3 Do other dimensions of decentralization matter?

As discussed earlier, political decentralization was implemented in all states prior to administrative decentralization. We have already verified that the effects of administrative decentralization are not confounded by the effects of this earlier measure (see section 6.2). Since we do not have states that conduct administrative but not political decentralization, we cannot evaluate whether and how the two measures complement each other in shaping development outcomes.

We can examine such complementarities in the context of fiscal decentralization. We show results separately for states that had some funds decentralization (proxied by whether *panchayats* at all tiers report collecting any taxes in 2015) and states that had no funds decentralization. Among states with some funds decentralization, neither partial nor full devolution is associated with any significant change in child mortality rates (Table A.8, columns 1, 3 and 5). However, for states that had no funds decentralization, we see a significant rise in child mortality rates under partial

devolution and no change under full devolution (columns 2, 4 and 6). This strongly suggests that building the capacity of local governance institutions, via strengthening their financial ability to raise revenues and/or giving them supervisory authority, should be strongly emphasized in decentralization initiatives.

7 The Role of Public Service Delivery

We consider the mechanisms that might underlie the observed worsening of child mortality outcomes under partial decentralization. We first verify that fertility trends do not respond strongly to such measures. We then show three pieces of evidence that suggest worsened delivery of public services as the main determinant rather than any changes in private behavior or other state policies.

7.1 Fertility responses

Since child mortality outcomes are conditional on the birth of a child, we first verify that the deterioration of child mortality outcomes is not driven by increased fertility in response to devolution (which may increase the burden on public health facilities), or by a changing gender mix of children (since male children are typically more fragile at very young ages). We find that partial devolution has no effect on fertility, while full devolution results in a marginally significant decline (Table A.9, columns 1 and 2). While this may be indicative of better provision of family planning services under full devolution, the effect size is very small (only 0.45% of the mean). There is no significant effect of partial or full devolution on the probability of the child being a girl (columns 3 and 4).

7.2 Differential effects by wealth and child gender

We provide evidence consistent with the thesis that partial devolution results in a deterioration of the public health system, by examining heterogeneity of treatment

effects based on household resources. If devolution changes the functioning of the public health system, we expect it to have larger effects on poorer households who are more dependent on the public health system, while richer ones have the option to pay for private health care if public services deteriorate. We test this by running separate regressions for households in each of the five wealth quintiles.The NFHS survey provides a wealth index for each household, based on a principal components analysis of several variables including household ownership of assets such as televisions and bicycles, materials used for housing construction, and types of water access and sanitation facilities. We find that both the β_1 and the β_2 coefficients are larger in magnitude for poorer households than for the richer ones (Figure 2). We also see that the β_1 coefficients are not statistically significant for the two highest wealth quintiles.

A second dimension of heterogeneity is based on gender. Many previous studies have documented the high degree of son preference in India and consequently, the fact that girl children are often more neglected than boys (see, among others, Jayachandran and Kuziemko (2011)). We examine whether child mortality of girls suffers more than that of boys, following administrative decentralization. All but one of the estimated β_1 and β_2 coefficients are larger in magnitude for girls compared to boys, though they are not statistically different from those of boys (Table A.10). This is consistent with girls' mortality rates being more responsive to public health service quality, as families may be more willing to spend on private health care for boys to compensate for shortfalls in public provision.

7.3 Measures of health service provision

We examine data on the growth of health facilities as a potential channel to explain these shortfalls in service delivery (Government of India, 2015). Using five-yearly data from 1985 to 2015, we find that partial devolution states have somewhat fewer rural health facilities, though the difference is not statistically significant (Table 5, column 1). As before, the estimated β_2 is positive, indicating that the difference is

smaller for full devolution states.

More importantly, we show that partial devolution of health functions results in worse service delivery, as measured by prenatal care provision and indicators of child immunization. Pregnant mothers in states with incomplete devolution are significantly less likely to be provided tetanus shots and their children are more likely to be unvaccinated (Table 5, columns 3 and 5).[13] In both cases, these negative effects are reversed for states that additionally devolved (at least some) employee authority. Note that these regressions are based on an incomplete sample of birth cohorts due to the data constraints described in section 4.1.

7.4 Administrative devolution and education outcomes

We analyze primary school completion and secondary school completion as our main education outcomes, using specifications similar to 1 and 2. For primary school completion, $AnyDev_{st}$ equals one if state s has devolved education before the individual enters primary school, i.e., in years $(t + 5)$ or earlier. For middle school completion, DEV_{st} equals one if state s has devolved education functions in year $(t + 11)$ or earlier. Note that these indicators measure whether an individual was exposed to devolution throughout their primary or middle school years. We can compute an alternative measure of partial exposure to devolution as indicators for whether some of the individual's time in primary or middle school was subject to devolution. The measure of partial exposure would equal one if state s devolved education functions prior to $(t+10)$ for primary school completion, and prior to $(t+14)$ for middle school completion. Unlike the measure of full exposure that measures the impact of devolutions occurring in year 2008 or earlier, the measure of partial exposure would capture the impact of devolutions up to year 2014. As for health outcomes, we control for

[13]Increasing prenatal tetanus vaccination was associated with lower incidence of neonatal mortality in India (Visaria, 1988) suggesting a link between the lower incidence of tetanus shots in Table 5 to higher neonatal mortality in Table 2 among partial devolution states.

gender, marital status, rural versus urban residence, indicators for caste and religion categories, and household income. Standard errors are clustered at the state level, and we also report p-values using a wild bootstrap procedure.

Our analysis of primary school completion rates yields similar results to those on child mortality. Partial devolution results in a 4.2 percentage point decline in primary school completion, corresponding to 4.6% of the sample mean (Table 6, column 1, β_1 coefficient). States that conducted full devolution show no difference in primary school completion compared to states that did not devolve (column 1, $\beta_1+\beta_2$). We find slightly different results in the case of middle school completion. Middle school completion is no different among states that devolved partially, compared to those that did not do any devolution. But, middle school completion is higher by 5.5 percentage points among states that did full devolution, corresponding to 7% of the sample mean (Table 6, column 2). The results on primary and middle school completion are robust to controlling for per capita state spending on education (Table A.11, columns 1 and 4), as well as controlling for the timing of political decentralization (columns 2 and 5). The results are larger in magnitude if we use a "partial exposure" to devolution measure, which equals one if a state devolved at any time during a child's tenure in primary school (columns 3 and 6).

We examine whether devolution affects education via the provision of physical resources such as public schools and teachers. We find that partial devolution states have significantly fewer public schools after devolution, while full devolution states are not significantly different from never-devolved states (Table 6, column 3). We also see somewhat fewer teachers in partial devolution states, though the difference is not statistically significant (column 4). While we do not have direct measures of education provision (such as teacher presence or test scores), we see larger effects for girls compared to boys, consistent with the idea that girls' education is more dependent on public provision compared to boys.[14] We find that partial devolution leads to

[14]Unlike the NFHS, the NSS surveys do not provide a wealth quintile, making it difficult to

significantly lower primary school completion for girls but not for boys (Table A.12, columns 1 and 2). Both girls and boys experience higher middle school completion under complete devolution, but the coefficient for girls is almost twice as large as for boys (columns 3 and 4). Finally, as with health outcomes, we see that there are no negative effects of partial or full devolution in states where local governments collected at least some taxes on their own, but partial devolution lowers primary school completion when there is no funds devolution (Table A.13, columns 1 and 2). Full devolution leads to better middle school completion, regardless of whether they devolved funds (columns 3 and 4).

8 Conclusions

We conduct the first analysis of the administrative devolution provisions of India's *Panchayati Raj* constitutional amendments. In contrast to prior studies on decentralization reforms, we examine the actual processes involved in administrative devolution, and find that these distinctions matter. In particular, partial devolution of health functions, without devolution of either employee authority or funds, results in a statistically significant increase in child mortality rates. Devolution of functions together with employee authority results in no net change in child mortality rates, suggesting that these reforms did not result in better functioning of the public health system in India. This is a disappointing result, since improving public service provision was one of the main drivers for the decentralization reforms.

Several pieces of evidence support the hypothesis that partial devolution results in a decline in the quality of public service provision. We find declines in the provision of prenatal care and immunization, and the effects are higher for poorer households who rely more on public services. Our results for education mirror those on health, suggesting that such declines in public service delivery are broad-based. An

conduct heterogeneity analyses by household wealth.

important policy implication is that decentralization policies need to be implemented with care, and that separating responsibility from authority can be detrimental for human development.

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Table 1: Timing of Administrative and Political Devolution Across States

	Administrative Decentralization		Political Decentralization
	# of states devolved health	# of states devolved education	# of states held local elections with gender quota
Before 1993	0	0	4
1993	1	1	1
1994-1997	3	3	14
1998-2002	0	0	2
2003-2006	5	5	3
2007-2012	7	8	1
2013-2015	2	2	0
Not devolved as of 2015	7	6	0

Notes: See Table A.2 for details of data sources. The 73rd amendment is not applicable to the states of Jammu & Kashmir, Meghalaya, Mizoram and Nagaland.

Table 2: Impact of Partial and Full Devolution on Child Mortality

	(1)	(2)	(3)	(4)	(5)	(6)
	Neonatal Mortality		Infant Mortality		Under 5 Mortality	
Any Devolution (β_1)	0.0034	0.0075***	0.0072*	0.0136***	0.0092**	0.0162***
	(0.0020)	(0.0019)	(0.0035)	(0.0029)	(0.0039)	(0.0034)
	[0.131]	[0.003]	[0.081]	[0.007]	[0.045]	[0.005]
Full Devolution (β_2)		-0.0061*		-0.0098**		-0.0102**
		(0.0030)		(0.0040)		(0.0044)
		[0.090]		[0.063]		[0.100]
$\beta_1 + \beta_2 = 0$ (p-value)		0.583		0.352		0.176
$\beta_1 + \beta_2 = 0$ (p-value), wild bootstrap		[0.629]		[0.412]		[0.223]
Mean of dep var	0.039		0.055		0.068	
Child Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mother Controls	Yes	Yes	Yes	Yes	Yes	Yes
Birth year and state FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,097,697	1,097,697	1,054,264	1,054,264	869,522	869,522

Notes: Robust standard errors clustered at the state-level in parentheses. p-value from wild bootstrap in brackets. *** p<0.01, ** p<0.05, * p<0.1. Child controls include indicator for girls and birth order fixed effects. Mother controls include indicators for Muslim, SC, ST, OBC, and rural, indicators for mother's education, mother's age at child birth and its square, mother's year of birth fixed effects, categorical variables for mother's height and age at marriage.

Table 3: Impact of Devolution on Child Mortality, Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)
	Neonatal Mortality		Infant Mortality		Under 5 Mortality	
	Control for timing of political decentralization	Control for per-capita state health spending	Control for timing of political decentralization	Control for per-capita state health spending	Control for timing of political decentralization	Control for per-capita state health spending
Any Devolution (β_1)	0.0075***	0.0072***	0.0136***	0.0125***	0.0162***	0.0155***
	(0.0019)	(0.0019)	(0.0028)	(0.0028)	(0.0034)	(0.0034)
	[0.003]	[0.002]	[0.006]	[0.007]	[0.005]	[0.006]
Full Devolution (β_2)	-0.0062**	-0.0058*	-0.0100**	-0.0086**	-0.0101**	-0.0088*
	(0.0029)	(0.0030)	(0.0038)	(0.0042)	(0.0042)	(0.0047)
	[0.078]	[0.114]	[0.054]	[0.106]	[0.099]	[0.162]
$\beta_1 + \beta_2 = 0$ (p-value)	0.603	0.560	0.365	0.318	0.172	0.112
$\beta_1 + \beta_2 = 0$ (p-value), wild boots	[0.647]	[0.610]	[0.431]	[0.376]	[0.223]	[0.147]
Mother and Child Controls	Yes	Yes	Yes	Yes	Yes	Yes
Birth year and State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,097,697	1,072,713	1,054,264	1,029,280	869,522	844,538

Notes: Robust standard errors clustered at the state-level in parentheses. p-value from wild bootstrap in brackets. *** p<0.01, ** p<0.05, * p<0.1. Child controls include indicator for girls and birth order fixed effects. Mother controls include indicators for Muslim, SC, ST, OBC, and rural, indicators for mother's education, mother's age at child birth and its square, mother's year of birth fixed effects, categorical variables for mother's height and age at marriage.

Table 4: Impact of Devolution on Child Mortality, Alternative DiD Estimators

	(1)	(2)	(3)	(4)	(5)	(6)
	Neonatal Mortality		Infant Mortality		Under 5 Mortality	
	No Employee Authority	Some Employee Authority	No Employee Authority	Some Employee Authority	No Employee Authority	Some Employee Authority
	Panel A: DiD Estimator, Split Sample					
Any Devolution (β)	0.0062***	0.0003	0.0119***	0.0018	0.0132***	0.0040
	(0.0018)	(0.0018)	(0.0035)	(0.0027)	(0.0034)	(0.0028)
Birth year and State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	511,135	586,562	490,975	563,289	405,326	464,196
	Panel B: de Chaisemartin and D'Haultfoeuille (2020) Estimator					
Any Devolution (β)	0.0047	0.0017	0.0073***	0.0027	0.0056	0.0027
Instantaneous treatment effect	(0.0032)	(0.0032)	(0.0024)	(0.0027)	(0.0029)	(0.0032)
Any Devolution (β)	0.0049**	-0.0008	0.0090***	0.0012	0.0070***	0.0034
Average Dynamic Treatment Effect (6)	(0.0023)	(0.0024)	(0.0020)	(0.0024)	(0.0020)	(0.0022)
	Panel C: State-Birthyear, Callaway and Sant'Anna (2021)					
Any Devolution (β)	0.0059***	0.0002	0.0118***	0.0047*	0.0110***	0.0079***
	(0.0015)	(0.0021)	(0.0016)	(0.0024)	(0.0026)	(0.0027)

Notes: Standard errors in parantheses, clustered at state level (panels A and C) or bootstrapped with state level clustering (panel B) . *** p<0.01, ** p<0.05, * p<0.1. There are no controls included in these specifications, other than state and year fixed effects.

Table 5: Impact of Devolution on Health Care Provision

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Indicators of prenatal care			Childrens' immunization		
	Log (# health facilities)	Three or more prenatal visits	Tetanus injection provided	Iron supplements provided	No vaccinations	Partially vaccinated	Fully vaccinated
Any Devolution (β_1)	-0.0539	0.0249	-0.140**	-0.0446	0.0619*	0.0303	-0.0921
	(0.0676)	(0.0610)	(0.0671)	(0.0514)	(0.0348)	(0.0499)	(0.0809)
	[0.4202]	[0.783]	[0.088]	[0.442]	[0.132]	[0.580]	[0.316]
Full Devolution (β_2)	0.107	0.00535	0.0768	0.0056	-0.0433	-0.0472	0.0904
	(0.0693)	(0.0725)	(0.0780)	(0.0521)	(0.0399)	(0.0486)	(0.0798)
	[0.1339]	[0.948]	[0.402]	[0.928]	[0.344]	[0.383]	[0.317]
$\beta_1 + \beta_2 = 0$ (p-value)	0.4767	0.427	0.137	0.124	0.541	0.623	0.976
$\beta_1 + \beta_2 = 0$ (p-value), wild bootstrap	[0.4532]	[0.482]	[0.212]	[0.180]	[0.592]	[0.648]	[0.980]
Mean of dep var	9.85	0.575	0.847	0.712	0.139	0.344	0.517
Observations	147	252,119	252,240	253,134	252,662	252,662	252,662

Notes: Robust standard errors clustered at the state-level in parentheses. p-value from wild bootstrap in brackets. *** p<0.01, ** p<0.05, * p<0.1. Health facilities data obtained at state level for years 1985, 1990, 1997, 2002, 2007, 2012 and 2015 from Government of India (2015); data for states that were split are aggregated to unsplit state level, to obtain a panel of 21 states over time. Column (1) includes controls for state and year fixed effects. Columns (2)-(7) include controls for state and birth-year fixed effects, indicator for girl child, birth order fixed effects, indicators for Muslim, SC, ST, OBC, and rural, mother's education categories, mother's age at child birth, and mother's year of birth fixed effects.

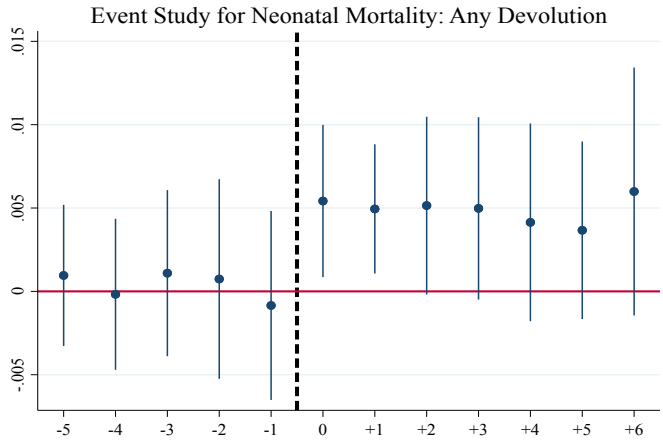
Table 6: Impact of Partial and Full Devolution on School Completion

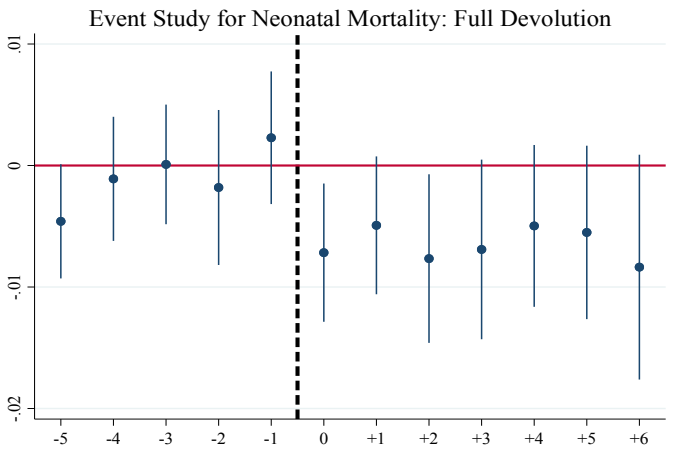
	(1)	(2)	(3)	(4)
	Primary school completion	Middle school completion	Log (# public schools)	Log (# public school teachers)
Any Devolution (β_1)	-0.0418**	-0.0160	-0.159***	-0.1068
	(0.0163)	(0.0173)	(0.0545)	(0.098)
	[0.098]	[0.375]	[0.0237]	[0.2897]
Full Devolution (β_2)	0.0189	0.0707***	0.0731	0.0596
	(0.0227)	(0.0208)	(0.0523)	(0.052)
	[0.525]	[0.011]	[0.1962]	[0.2530]
$\beta_1 + \beta_2 = 0$ (p-value)	0.220	0.0029	0.1511	0.6296
$\beta_1 + \beta_2 = 0$ (p-value), wild bootstrap	[0.285]	[0.017]	[0.1536]	[0.6830]
Mean of dep var	0.901	0.805	9.85	8.19
Observations	177,516	149,239	800	285

Notes: Robust standard errors clustered at the state-level in parentheses. p-value from wild bootstrap in brackets. *** p<0.01, ** p<0.05, * p<0.1. Columns (1) and (2) use indvidual level data from the National Sample Survey and control for state and birth-year fixed effects, indicators for gender, marital status, Muslim, Christian, Sikh and other religions, SC, ST and OBC, and rural households, and log of household's monthly income. An individual is exposed to devolution if they are born at least 5 years prior to decentralization (for primary school) and at least 10 years prior to decentralization (for middle school). Sample is restricted to individuals aged 14 to 35 (for primary school completion) and those aged 17 to 35 (for middle school completion). Data in column (3) is at state level for 1985-2016 and in column (4) for 2001-2011 and 2015, obtained from the Ministry of Education. Columns (3) and (4) control for state and year fixed effects.

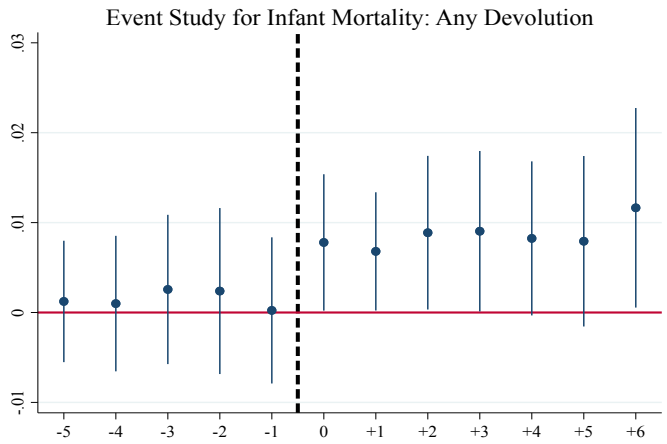
Figure 1: Year-by-year Effects of Administrative Decentralization on Child Mortality

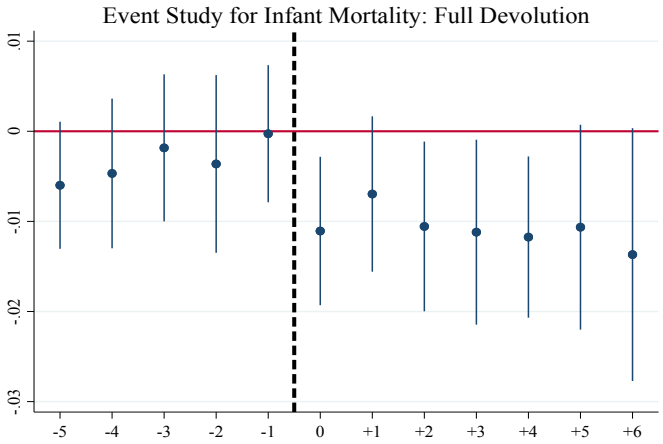
Panel A: Neonatal Mortality



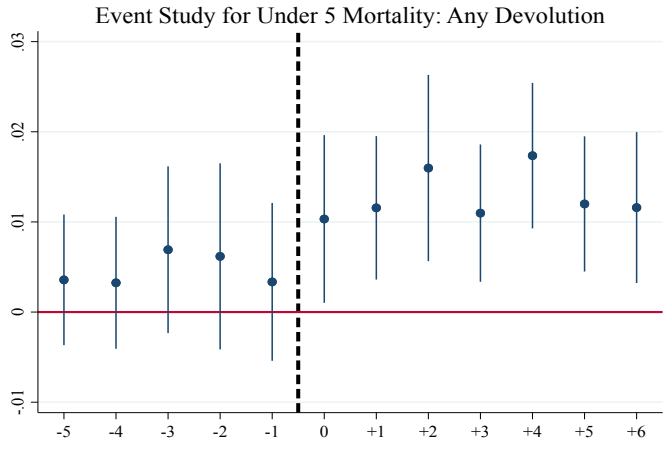


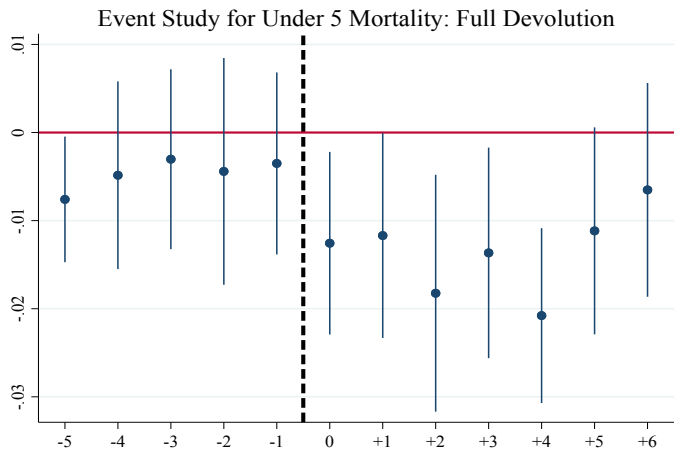
Panel B: Infant Mortality





Panel C: Under-5 Child Mortality

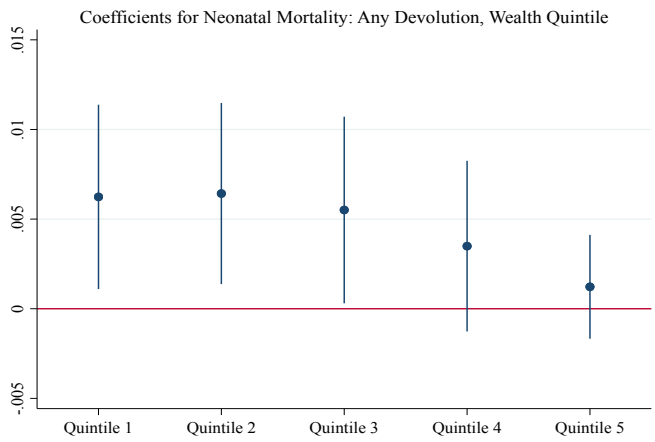


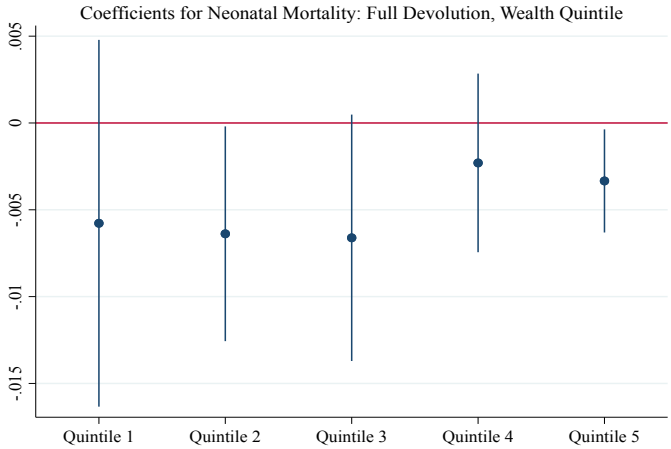


Figures on the left show the estimates β_1 for each year before and after the functions devolution, i.e., any devolution. Figures on the right show corresponding estimates β_2 for each year before and after the full devolution (of functions and employee authority). Dashed vertical lines indicate the timing of the reform.

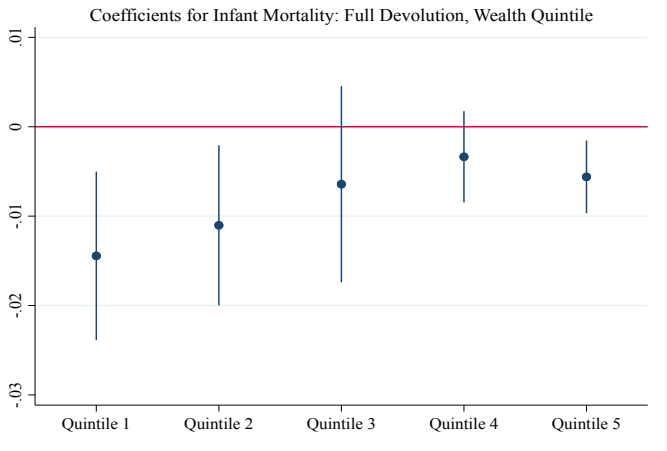
Figure 2: Heterogeneous Effects of Administrative Decentralization by Wealth on Child Mortality

Panel A: Neonatal Mortality

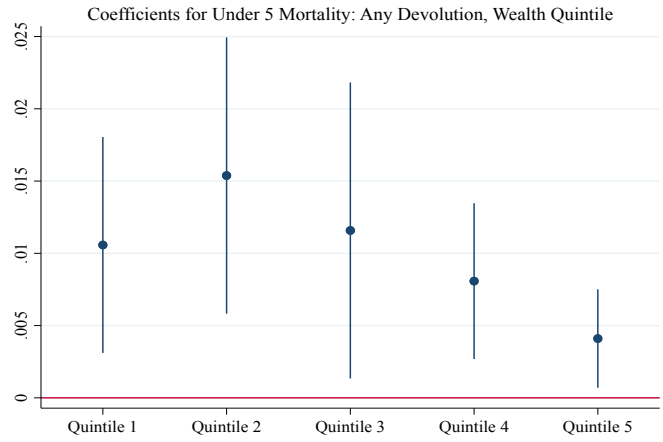


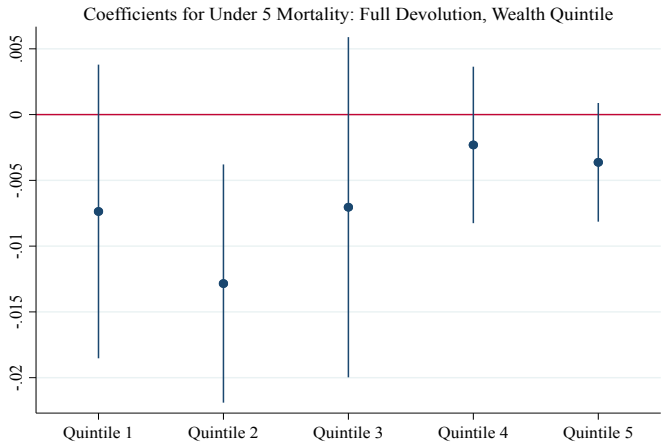


Panel B: Infant Mortality



Panel C: Under-5 Child Mortality





Figures on the left show the estimates β_1 for each wealth quintile after the functions devolution, i.e., any devolution. Figures on the right show corresponding estimates β_2 for each wealth quintile after the full devolution (of functions and employee authority).

The Importance of Being Local? Administrative Decentralization and Human Development

Latika Chaudhary
Naval Postgraduate School
lhartman@nps.edu

Lakshmi Iyer
University of Notre Dame
liyer@nd.edu

Appendix Tables and Figures
June 2024

Table A.1: Devolution Areas Listed in the 11th Schedule of the Constitution of India

1. Agriculture including agricultural expansion
2. Land improvement & implementation of land reforms
3. Animal Husbandry, Dairying and poultry
4. Fisheries Industry
5. Minor irrigation, water management and watershed development
6. Social forestry and farm forestry
7. Small scale industries involving food processing
8. Minor forest produce
9. Safe water for drinking
10. Khadi, village and cottage industries
11. Rural housing
12. Fuel and fodder
13. Rural electrification, including distribution of electricity
14. Road, culverts, bridges, ferries, waterways and other means of communication
15. Education including primary and secondary schools
16. Non-conventional sources of energy
17. Technical training and vocational education
18. Adult and non-formal education
19. Public distribution system
20. Maintenance of community assets
21. Welfare of the weaker sections especially SC/ST
22. Social welfare, including welfare of the handicapped and mentally retarded
23. Family welfare
24. Women and child development
25. Markets and Fairs
26. Health and sanitation including hospitals, primary health centres and dispensaries
27. Cultural activities
28. Libraries
29. Poverty Alleviation Programmes

Notes: Obtained from https://www.jagranjosh.com/general-knowledge/list-of-subjects-covered-in-the-11th-schedule-of-the-indian-constitution-1510219894-1; accessed August 2021.

Table A.2: Data Sources

State legislation: Individual state *Panchayati Raj* Acts

Progress of administrative decentralization (functions, functionaries, funds): State profiles in *The State of Panchayats 2007-08* (Government of India, 2008); *Rural Local Body, Core Functions and Finances,* A study commissioned for the Fourteenth Finance Commission by the Centre for Policy Research, New Delhi (2014); Annual Report 2015-16 Ministry of Panchayati Raj (Government of India, 2016a); *Devolution Report of 2015-16* (Government of India and Tata Institute of Social Sciences, 2016b).

Executive orders: Department of *Panchayati Raj* websites of various states.

Progress of political decentralization: Iyer et al. (2012)

Table A.3: Summary Statistics, National Family Health Survey 2015-16

Variables	Obs	Mean	SD	Min	Max
Child mortality and birth outcomes					
Neo Mortality (child death in first month)	1,097,697	0.039	0.194	0	1
Infant Mortality (child death in first year)	1,054,264	0.055	0.228	0	1
Under 5 Mortality (child death in first five years)	869,522	0.068	0.251	0	1
Fertility (whether woman had any birth in that year)	10,017,968	0.109	0.311	0	1
Dummy for girl birth	1,102,907	0.475	0.499	0	1
Birth Order					
First birth	1,102,907	0.348	0.476	0	1
Second birth	1,102,907	0.294	0.456	0	1
Third birth	1,102,907	0.175	0.380	0	1
Fourth birth	1,102,907	0.093	0.290	0	1
Fifth or higher birth	1,102,907	0.089	0.285	0	1
Mother characteristics					
Muslim	1,102,907	0.14	0.34	0	1
Schedued Castes (SC)	1,102,907	0.20	0.40	0	1
Scheduled Tribes (ST)	1,102,907	0.14	0.35	0	1
Other Backward Castes (OBC)	1,102,907	0.43	0.49	0	1
Rural	1,102,907	0.76	0.43	0	1
Mother's age at birth of child	1,102,907	23.97	4.84	13	49
Education categories					
No education	1,102,907	0.47	0.50	0	1
Primary education	1,102,907	0.15	0.36	0	1
Secondary education	1,102,907	0.32	0.47	0	1
More than secondary	1,102,907	0.05	0.22	0	1
Mother height categories					
Less than 148 cm	1,102,907	0.24	0.43	0	1
Between 148 and 151 cm	1,102,907	0.25	0.43	0	1
Between 151 and 155	1,102,907	0.25	0.43	0	1
More than 155 cm	1,102,907	0.25	0.43	0	1
Missing	1,102,907	0.01	0.11	0	1
Mother's age at marriage categories					
Less than 15	1,102,907	0.15	0.36	0	1
15<=age married<18	1,102,907	0.33	0.47	0	1
18<=age married < 21	1,102,907	0.29	0.45	0	1
Age married >=21	1,102,907	0.23	0.42	0	1
Missing	1,102,907	0.05	0.21	0	1

Table A.4: Impact of Devolution on Child Mortality, Controlling for State and Central Government Spending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Neonatal Mortality			Infant Mortality			Under 5 Mortality		
	Control for per-capita state social spending	Control for per-capita state transfers to panchayats	Control for per-capita federal grants	Control for per-capita state social spending	Control for per-capita state transfers to panchayats	Control for per-capita federal grants	Control for per-capita state social spending	Control for per-capita state transfers to panchayats	Control for per-capita federal grants
Any Devolution (β_1)	0.0068***	0.0070***	0.0077***	0.0119***	0.0123***	0.0141***	0.0155***	0.0140***	0.0167***
	(0.0018)	(0.0021)	(0.0018)	(0.0027)	(0.0032)	(0.0028)	(0.0033)	(0.0036)	(0.0036)
	[0.0039]	[0.0095]	[0.0020]	[0.0151]	[0.0096]	[0.0055]	[0.0074]	[0.0093]	[0.0053]
Full Devolution (β_2)	-0.0057*	-0.0051	-0.0063**	-0.0085*	-0.0075*	-0.0102**	-0.0092*	-0.0065	-0.0105**
	(0.0029)	(0.0031)	(0.0029)	(0.0042)	(0.0044)	(0.0040)	(0.0046)	(0.0044)	(0.0044)
	[0.1147]	[0.1853]	[0.0753]	[0.1092]	[0.1755]	[0.0532]	[0.1417]	[0.2265]	[0.094]
$\beta_1 + \beta_2 = 0$ (p-value)	0.637	0.422	0.580	0.370	0.225	0.340	0.125	0.0743	0.165
$\beta_1 + \beta_2 = 0$ (p-value), wild bootstrap	[0.679]	[0.487]	[0.626]	[0.426]	[0.306]	[0.398]	[0.162]	[0.113]	[0.223]
Mother and Child Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth year and State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,072,713	1,058,929	1,097,697	1,029,280	1,015,496	1,054,264	844,538	830,754	869,522

Notes: Robust standard errors clustered at the state-level in parentheses. p-value from wild bootstrap in brackets. *** p<0.01, ** p<0.05, * p<0.1. Child controls include indicator for girls and birth order fixed effects. Mother controls include indicators for Muslim, SC, ST, OBC, and rural, indicators for mother's education, mother's age at child birth and its square, mother's year of birth fixed effects, categorical variables for mother's height and age at marriage.

Table A.5: Impact of Devolution on Child Mortality, Controlling for State GDP and State Specific Trends

	(1)	(2)	(3)	(4)	(5)	(6)
	Neonatal Mortality		Infant Mortality		Under 5 Mortality	
	Control for per-capita state GDP	Control for state specific time trends	Control for per-capita state GDP	Control for state specific time trends	Control for per-capita state GDP	Control for state specific time trends
Any Devolution (β_1)	0.0045***	0.0038**	0.0082***	0.0036*	0.0074**	0.0030
	(0.0015)	(0.002)	(0.0025)	(0.002)	(0.0034)	(0.003)
	[0.0448]	[0.0858]	[0.0441]	[0.1621]	[0.2489]	[0.3113]
Full Devolution (β_2)	-0.0049*	-0.0043**	-0.0076*	-0.0049**	-0.0059	-0.0047*
	(0.0025)	(0.002)	(0.0037)	(0.002)	(0.0042)	(0.003)
	[0.1097]	[0.0548]	[0.1206]	[0.0713]	[0.2792]	[0.1229]
$\beta_1 + \beta_2 = 0$ (p-value)	0.865	0.684	0.844	0.340	0.569	0.298
$\beta_1 + \beta_2 = 0$ (p-value), wild bootstrap	[0.889]	[0.694]	[0.878]	[0.328]	[0.604]	[0.307]
Mother and Child Controls	Yes	Yes	Yes	Yes	Yes	Yes
Birth year and State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,084,229	1,097,697	1,045,600	1,054,264	860,858	869,522

Notes: Robust standard errors clustered at the state-level in parentheses. p-value from wild bootstrap in brackets. *** p<0.01, ** p<0.05, * p<0.1. Child controls include indicator for girls and birth order fixed effects. Mother controls include indicators for Muslim, SC, ST, OBC, and rural, indicators for mother's education, mother's age at child birth and its square, mother's year of birth fixed effects, categorical variables for mother's height and age at marriage.

Table A.6: Robustness of Results to Recoding Devolution Timing for Specific States

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Neonatal Mortality			Infant Mortality			Under 5 Mortality		
	Recode Bihar date to 2011	Recode Rajasthan as "not devolved"	Move employee authority date 3 years ahead	Recode Bihar date to 2011	Recode Rajasthan as "not devolved"	Move employee authority date 3 years ahead	Recode Bihar date to 2011	Recode Rajasthan as "not devolved"	Move employee authority date 3 years ahead
Any Devolution (β_1)	0.0075***	0.0075***	0.0053***	0.0136***	0.0136***	0.0097***	0.0162***	0.0163***	0.0108***
	(0.0019)	(0.0019)	(0.0017)	(0.0029)	(0.0027)	(0.0030)	(0.0034)	(0.0033)	(0.0038)
	[0.0031]	[0.0025]	[0.0060]	[0.0073]	[0.0053]	[0.0070]	[0.0052]	[0.0043]	[0.0082]
Full Devolution (β_2)	-0.0061*	-0.0058	-0.0042	-0.0098**	-0.0085	-0.0059	-0.0102**	-0.0076	-0.0044
	(0.0030)	(0.0037)	(0.0029)	(0.0040)	(0.0052)	(0.0041)	(0.0044)	(0.0054)	(0.0044)
	[0.0897]	[0.2289]	[0.2151]	[0.0632]	[0.2083]	[0.2367]	[0.1003]	[0.3037]	[0.4370]
$\beta_1 + \beta_2 = 0$ (p-value)	0.583	0.609	0.725	0.352	0.322	0.460	0.176	0.0985	0.254
$\beta_1 + \beta_2 = 0$ (p-value), wild bc	[0.630]	[0.647]	[0.765]	[0.412]	[0.394]	[0.516]	[0.222]	[0.154]	[0.305]
Mother and Child Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth year and State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,097,697	1,097,697	1,097,697	1,054,264	1,054,264	1,054,264	869,522	869,522	869,522

Notes: Robust standard errors clustered at the state-level in parentheses. p-value from wild bootstrap in brackets. *** p<0.01, ** p<0.05, * p<0.1. Child controls include indicator for girls and birth order fixed effects. Mother controls include indicators for Muslim, SC, ST, OBC, and rural, indicators for mother's education, mother's age at child birth and its square, mother's year of birth fixed effects, categorical variables for mother's height and age at marriage.

Table A.7: Devolution and Child Mortality, Robustness to Choice of Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Neonatal Mortality		Infant Mortality		Under 5 Mortality	
	Drop Visitors and Movers	Mother Fixed Effects	Drop Visitors and Movers	Mother Fixed Effects	Drop Visitors and Movers	Mother Fixed Effects
Any Devolution (β_1)	0.0063***	0.0078**	0.0127***	0.0097**	0.0154***	0.0112**
	(0.0015)	(0.0032)	(0.0022)	(0.0040)	(0.0026)	(0.0040)
	[0.0039]	[0.0150]	[0.0065]	[0.0233]	[0.0035]	[0.0176]
Full Devolution (β_2)	-0.0056**	-0.0077**	-0.0099**	-0.0091*	-0.0109**	-0.0103*
	(0.0027)	(0.0036)	(0.0036)	(0.0048)	(0.0040)	(0.0056)
	[0.0837]	[0.0158]	[0.0463	[0.0352]	[0.0610]	[0.0491]
$\beta_1 + \beta_2 = 0$ (p-value)	0.783	0.961	0.476	0.893	0.277	0.845
$\beta_1 + \beta_2 = 0$ (p-value), wild bootstrap	[0.810]	[0.951]	[0.535]	[0.871]	[0.328]	[0.814]
Child Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mother Controls	Yes	No	Yes	No	Yes	No
Birth year and State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	849,731	1,097,697	813,371	1,054,264	661,536	869,522

Notes: Robust standard errors clustered at the state-level in parentheses. p-value from wild bootstrap in brackets. *** p<0.01, ** p<0.05, * p<0.1. Child controls include indicator for girls and birth order fixed effects. Mother controls include indicators for Muslim, SC, ST, OBC, and rural, indicators for mother's education, mother's age at child birth and its square, mother's year of birth fixed effects, categorical variables for mother's height and age at marriage.

Table A.8: Does the Impact of Administrative Devolution Vary with Fiscal Devolution?

	(1)	(2)	(3)	(4)	(5)	(6)
	Neonatal Mortality		Infant Mortality		Under 5 Mortality	
	Funds - Yes	Funds - No	Funds - Yes	Funds - No	Funds - Yes	Funds - No
Any Devolution (β_1)	0.0029	0.0091***	0.0042	0.0163***	0.0019	0.0209***
	(0.0028)	(0.0027)	(0.0035)	(0.0037)	(0.0038)	(0.0038)
	[0.4238]	[0.0119]	[0.3575]	[0.0232]	[0.7226]	[0.0172]
Full Devolution (β_2)	-0.0082	-0.0070*	-0.0092	-0.0117**	-0.0016	-0.0151***
	(0.0046)	(0.0039)	(0.0050)	(0.0052)	(0.0054)	(0.0051)
	[0.2957]	[0.1752]	[0.2111]	[0.1059]	[0.8047]	[0.0735]
$\beta_1 + \beta_2 = 0$ (p-value)	0.167	0.458	0.155	0.330	0.913	0.232
$\beta_1 + \beta_2 = 0$ (p-value), wild bootstra	[0.253]	[0.511]	[0.248]	[0.385]	[0.859]	[0.277]
Mother and Child Controls	Yes	Yes	Yes	Yes	Yes	Yes
Birth year and State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	178,663	919,034	171,777	882,487	141,999	727,523

Notes: Robust standard errors clustered at the state-level in parentheses. p-value from wild bootstrap in brackets. *** p<0.01, ** p<0.05, * p<0.1. Child controls include indicator for girls and birth order fixed effects. Mother controls include indicators for Muslim, SC, ST, OBC, and rural, indicators for mother's education, mother's age at child birth and its square, mother's year of birth fixed effects, categorical variables for mother's height and age at marriage.

Table A.9: Impact of Devolution on Fertility and Girl Births

	(1)	(2)	(3)	(4)
	Mother's Fertility		Girl Birth	
Any Devolution (β_1)	-0.0003	0.0000	-0.0008	0.0029
	(0.0002)	(0.0002)	(0.0026)	(0.0049)
	[0.2309]	[0.8902]	[0.7708]	[0.5913]
Full Devolution (β_2)		-0.0005*		-0.0056
		(0.0003)		(0.0048)
		[0.1264]		[0.3320]
$\beta_1 + \beta_2 = 0$ (p-value)		0.084		0.290
$\beta_1 + \beta_2 = 0$ (p-value), wild bootstrap		[0.112]		[0.308]
Mean of Dep.	0.11		0.475	
Mother Controls	Yes	Yes	Yes	Yes
Child Controls	No	Yes	Yes	Yes
Birth year and State FE	Yes	Yes	Yes	Yes
Observations	10,017,968	10,017,968	1,102,907	1,102,907

Notes: Robust standard errors clustered at the state-level in parentheses. p-value from wild bootstrap in brackets. *** p<0.01, ** p<0.05, * p<0.1. Child controls are birth order fixed effects. Mother controls include indicators for Muslim, SC, ST, OBC, and rural, indicators for mother's education, mother's age at child birth and its square, mother's year of birth fixed effects, categorical variables for mother's height and age at marriage. Fertility regressions also control for time since the last birth.

Table A.10: Does Administrative Devolution Affect Health Outcomes of Boys and Girls Differently?

	(1)	(2)	(3)	(4)	(5)	(6)
	Neonatal Mortality		Infant Mortality		Under 5 Mortality	
	Girls	Boys	Girls	Boys	Girls	Boys
Any Devolution (β_1)	0.0069***	0.0078***	0.0142***	0.0130***	0.0175***	0.0150***
	(0.0016)	(0.0027)	(0.0031)	(0.0030)	(0.0038)	(0.0036)
	[0.0083]	[0.0115]	[0.0117]	[0.0057]	[0.0070]	[0.0058]
Full Devolution (β_2)	-0.0065***	-0.0056	-0.0108**	-0.0087*	-0.0121**	-0.0084*
	(0.0022)	(0.0039)	(0.0040)	(0.0046)	(0.0052)	(0.0045)
	[0.0344]	[0.2289]	[0.0549]	[0.1211]	[0.1122]	[0.1486]
$\beta_1 + \beta_2 = 0$ (p-value)	0.863	0.447	0.475	0.301	0.339	0.094
$\beta_1 + \beta_2 = 0$ (p-value), wild bootstrap	[0.876]	[0.499]	[0.542]	[0.361]	[0.404]	[0.117]
Mother and Child Controls	Yes	Yes	Yes	Yes	Yes	Yes
Birth year and State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	521,395	576,302	500,769	553,495	412,189	457,333

Notes: Robust standard errors clustered at the state-level in parentheses. p-value from wild bootstrap in brackets. *** p<0.01, ** p<0.05, * p<0.1. Child controls include indicator for girls and birth order fixed effects. Mother controls include indicators for Muslim, SC, ST, OBC, and rural, indicators for mother's education, mother's age at child birth and its square, mother's year of birth fixed effects, categorical variables for mother's height and age at marriage.

Table A.11: Devolution and Education Outcomes, Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)
	Primary School Completion			Middle School Completion		
	Control for per-capita state education spending	Control for timing of political decentralization	Partial exposure to devolution	Control for per-capita state education spending	Control for timing of political decentralization	Partial exposure to devolution
Any Devolution (β_1)	-0.0311*	-0.0415**	-0.0292*	-0.0131	-0.0160	-0.0255
	(0.0166)	(0.0172)	(0.0147)	(0.0186)	(0.0173)	(0.0203)
	[0.3249]	[0.1060]	[0.0692]	[0.5475]	[0.3862]	[0.2449]
Full Devolution (β_2)	0.0134	0.0183	0.0526***	0.0662**	0.0701***	0.0956***
	(0.0222)	(0.0233)	(0.0174)	(0.0242)	(0.0205)	(0.0232)
	[0.6859]	[0.5421]	[0.0147]	[0.0430]	[0.0116]	[0.0045]
$\beta_1 + \beta_2 = 0$ (p-value)	0.232	0.203	0.148	0.002	0.005	0.004
$\beta_1 + \beta_2 = 0$ (p-value), wild bootstrap	[0.304]	[0.271]	[0.181]	[0.022]	[0.0237]	[0.010]
Individual and Family Controls	Yes	Yes	Yes	Yes	Yes	Yes
Birth year and state FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	165,548	177,516	177,516	137,271	149,239	149,239
R-squared						

Notes: Robust standard errors clustered at the state-level in parentheses. p-value from wild bootstrap in brackets. *** p<0.01, ** p<0.05, * p<0.1. Individual and family controls include indicators for gender, marital status, Muslim, Christian, Sikh and other religions, SC, ST and OBC, and rural households, and log of household's monthly income. An individual is exposed to decentralization if they are born at least 5 years prior to decentralization (for primary school) and at least 10 years prior to decentralization (for middle school). Sample is restricted to individuals aged 14 to 35 (for primary school completion) and those aged 17 to 35 (for middle school completion).

Table A.12: Differential Effects of Administrative Devolution on Boys vs Girls's Education

	(1)	(2)	(3)	(4)
	Primary School Completion		Middle School Completion	
	Girls	Boys	Girls	Boys
Any Devolution (β_1)	-0.0681***	-0.0168	-0.0293	0.0005
	(0.0232)	(0.0128)	(0.0222)	(0.0143)
	[0.0357]	[0.3673]	[0.2099]	[0.9740]
Full Devolution (β_2)	0.0330	0.0039	0.0865**	0.0505***
	(0.0355)	(0.0145)	(0.0313)	(0.0150)
	[0.4995]	[0.8080]	[0.0261]	[0.0106]
$\beta_1 + \beta_2 = 0$ (p-value)	0.228	0.297	0.031	0.001
$\beta_1 + \beta_2 = 0$ (p-value), wild bootstrap	[0.291]	[0.338]	[0.088]	[0.009]
Individual and Family Controls	Yes	Yes	Yes	Yes
Birth year and State FE	Yes	Yes	Yes	Yes
Observations	81,163	96,353	68,465	80,774

Notes: Robust standard errors clustered at the state-level in parentheses. p-value from wild bootstrap in brackets. *** p<0.01, ** p<0.05, * p<0.1. Individual and family controls include indicators for gender, marital status, Muslim, Christian, Sikh and other religions, SC, ST and OBC, and rural households, and log of household's monthly income. An individual is exposed to decentralization if they are born at least 5 years prior to decentralization (for primary school) and at least 10 years prior to decentralization (for middle school). Sample is restricted to individuals aged 14 to 35 (for primary school completion) and those aged 17 to 35 (for middle school completion).

Table A.13: Administrative Devolution and Education Outcomes, Does Fiscal Devolution Matter?

	(1)	(2)	(3)	(4)
	Primary School Completion		Middle School Completion	
	Funds -Yes	Funds - No	Funds -Yes	Funds - No
Any Devolution (β_1)	-0.0111	-0.0546**	0.0008	-0.0225
	(0.0153)	(0.0197)	(0.0194)	(0.0244)
	[0.5200]	[0.2326]	[0.9644]	[0.4001]
Full Devolution (β_2)	-0.0250	0.0432	0.0453***	0.0749**
	(0.0175)	(0.0290)	(0.0092)	(0.0273)
	[0.2865]	[0.3657]	[0.0523]	[0.0486]
$\beta_1 + \beta_2 = 0$ (p-value)	0.124	0.635	0.083	0.006
$\beta_1 + \beta_2 = 0$ (p-value), wild bootstra	[0.214]	[0.754]	[0.343]	[0.030]
Individual and Family Controls	Yes	Yes	Yes	Yes
Birth year and State FE	Yes	Yes	Yes	Yes
Observations	40,424	137,092	34,482	114,757

p<0.05, * p<0.1. Individual and family controls include indicators for gender, marital status, Muslim, Christian, Sikh and other religions, SC, ST and OBC, and rural households, and log of household's monthly income. An individual is exposed to decentralization if they are born at least 5 years prior to decentralization (for primary school) and at least 10 years prior to decentralization (for middle school). Sample is restricted to individuals aged 14 to 35 (for primary school completion) and those aged 17 to 35 (for middle school completion).

Figure A.1: Child Mortality Outcomes

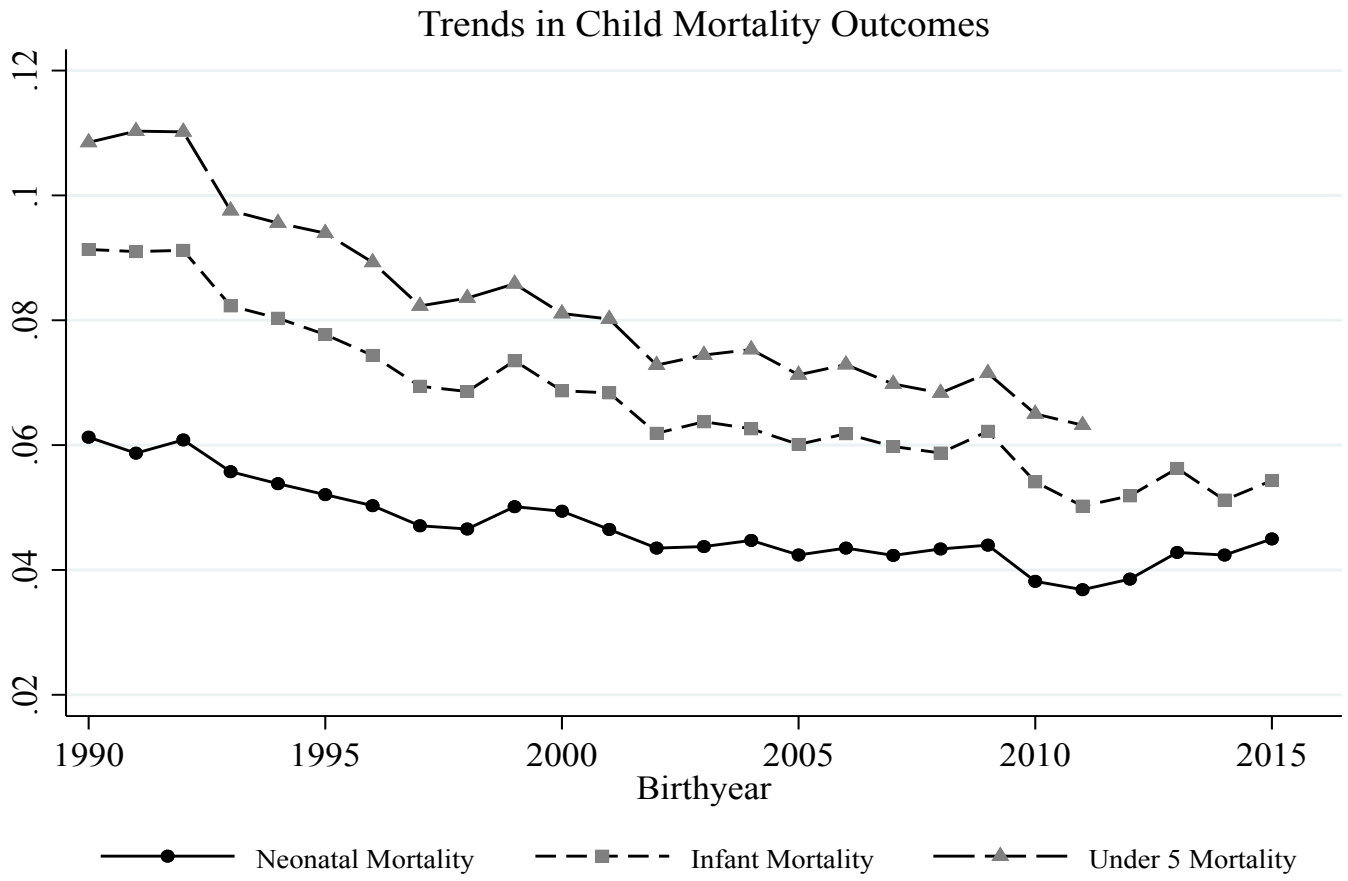


Figure A.2: Trends in School Completion Outcomes

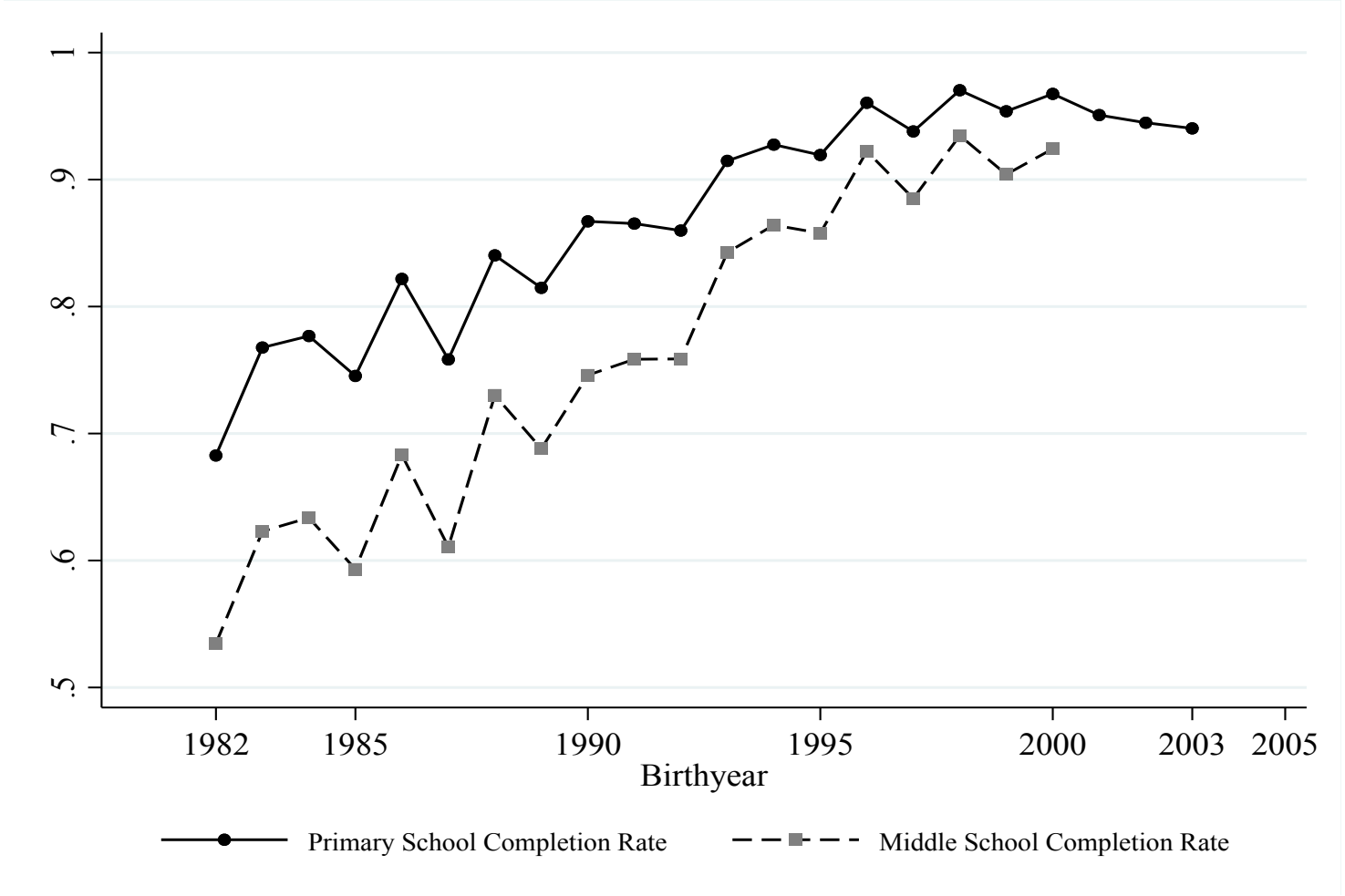
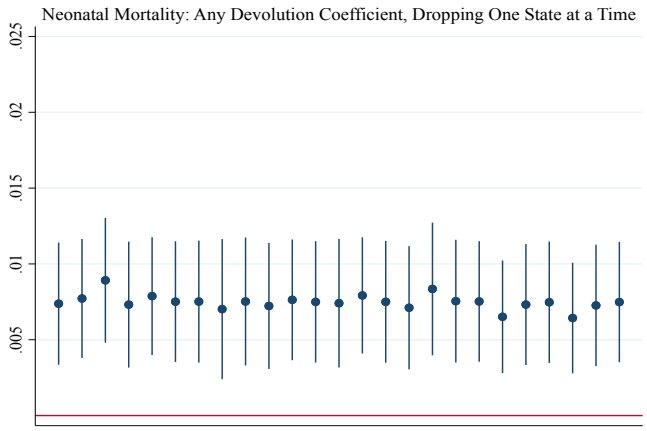
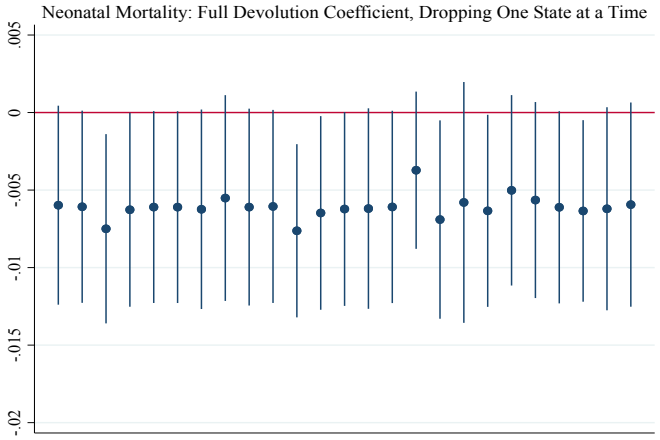


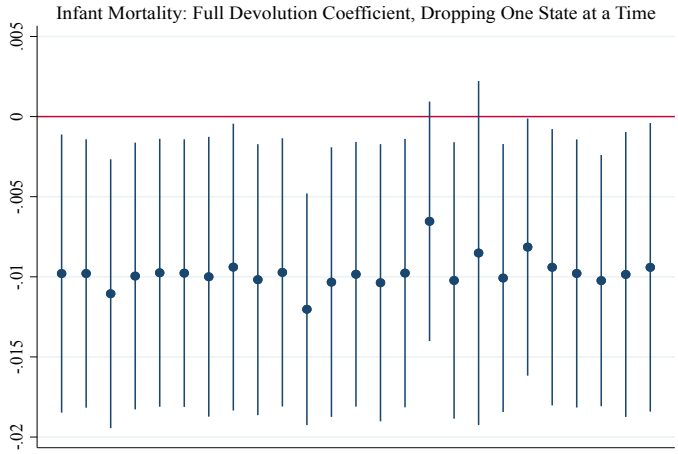
Figure A.3: Coefficients on Administrative Decentralization, Dropping One State at a Time

Panel A: Neonatal Mortality

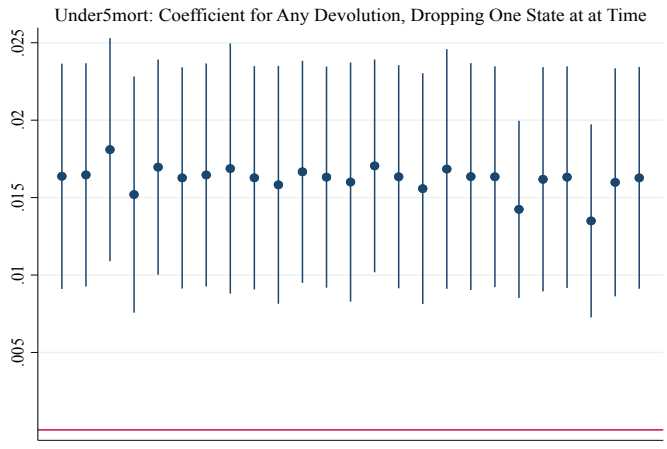


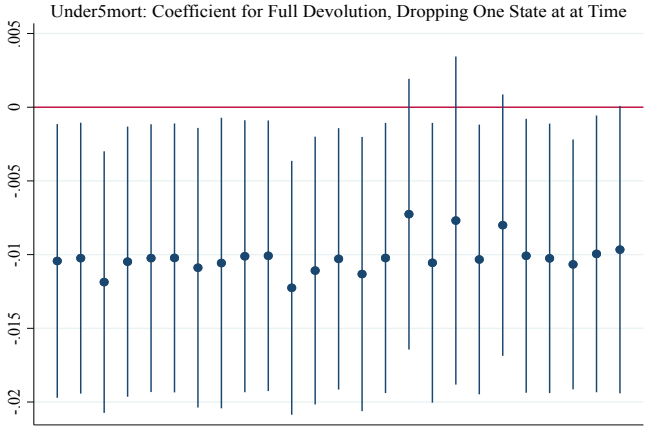


Panel B: Infant Mortality



Panel C: Under-5 Child Mortality

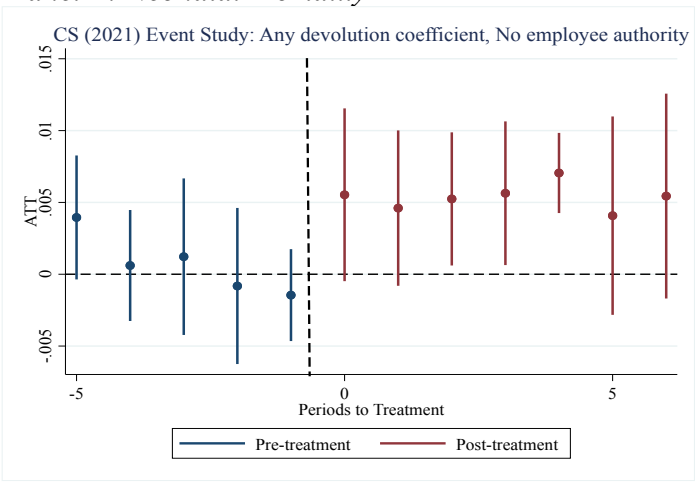


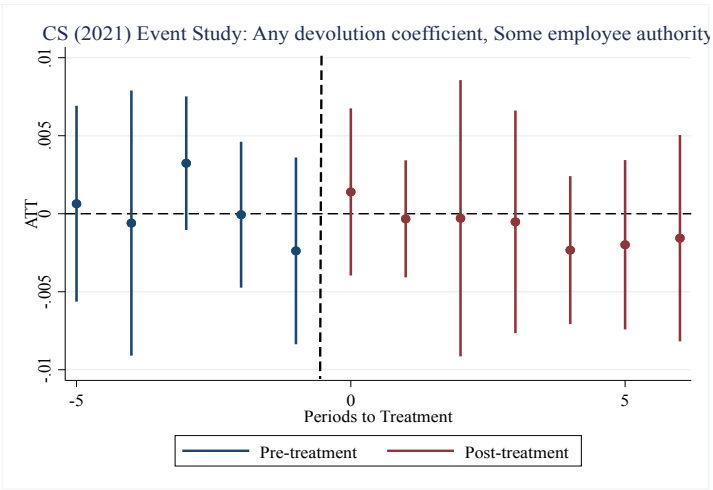


Figures on the left show the coefficients on "any devolution" (β_1), dropping one state at a time. Figures on the right show the coefficients on "full devolution" (β_2), dropping one state at a time.

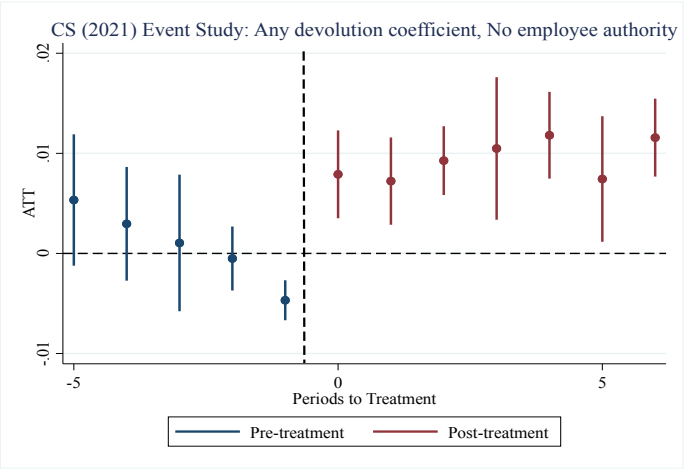
Figure A.4: Callaway and Sant'Anna (2021) Event Study Estimates

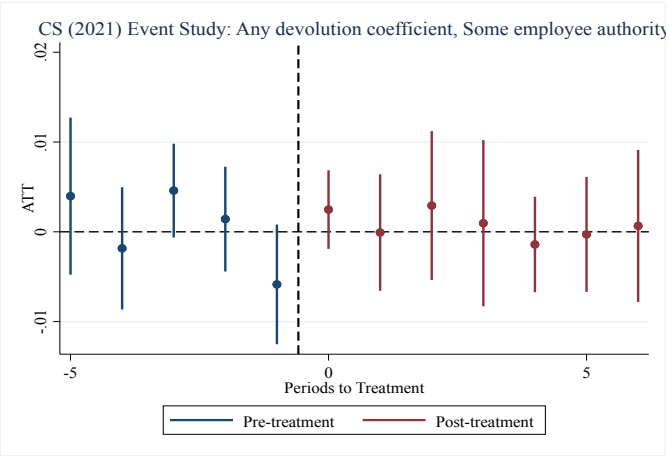
Panel A: Neonatal Mortality



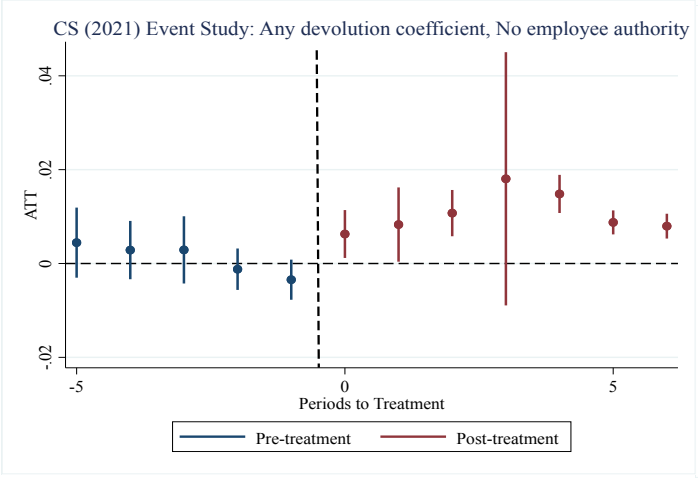


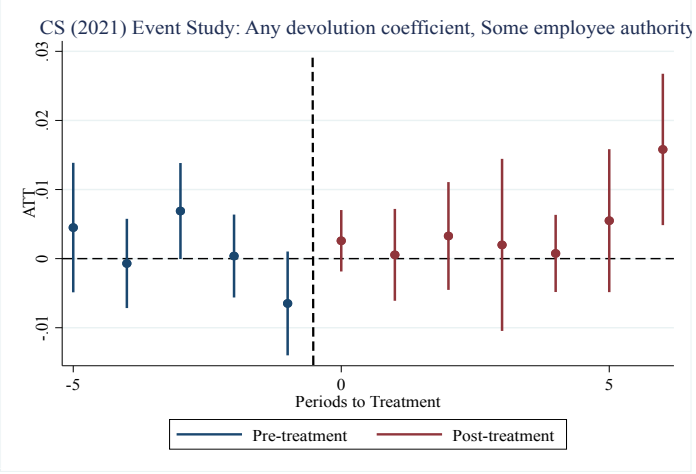
Panel B: Infant Mortality





Panel C: Under-5 Child Mortality





Figures on the left show the estimates β_1 for each year before and after the partial devolution of functions without any devolution of employee authority. Figures on the right show corresponding estimates β_2 for each year before and after full devolution (of functions and employee authority). Dashed vertical lines indicate the timing of the reform. All estimates use "never-devolved" states as the control group.