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The Political Competition over Life and Death - Evidence from Infant Mortality in India

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Abstract

We argue that economic inequality harms social provisions for the poor, but that higher political competition can mitigate this effect. We test this hypothesis using a large redistricting of electoral boundaries in India and find that higher inequality causes more post-neonatal infant deaths, but only when there is weak political competition. We further show that government health centers located in constituencies with low political competition and high inequality are disfavored, indicating that the effect on mortality operates via changes in public provision. Finally, we show that the same mechanisms are at play in the implementation of the MGNREGA employment program.

JEL codes: O15, D72, P46

Keywords: Health, infant mortality, income inequality, political competition

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1 Introduction

Over the last two decades, there has been a clear improvement in child survival worldwide. Total deaths of children under five fell from 12.5 million in 1990 to 5.3 million in 2018. Yet, the survival rates vary importantly across and within countries and many children still die from preventable diseases and injuries (Hug *et al.*, 2019). What explains the huge variation in these unnecessary deaths?

We study the role of income inequality and political competition in explaining the variation in child mortality in India, emphasizing that the economic causes are indirect and operate via health care provision at the local level. Our hypothesis – which is outlined in a simple theoretical framework – is that economic inequality harms social provisions for the poor, but political competition mitigates the effect. Based on empirical investigations, we show the mechanisms are particularly important for child survival, but are also relevant for a wider range of social provisions. Indeed, we find the same mechanisms are at play in the implementation of the National Rural Employment Guarantee Act.

The concept is intuitive. On the one hand, higher economic inequality allocates resources and power to the wealthy. As a consequence, their favorite policies and arrangements can crowd out those that address the needs of the poor. On the other hand, numbers matter in a democracy and there are more poor people than wealthy people in India. Political competition can empower the majority, as an incumbent who wants to be reelected must be sensitive to the needs of the poor. This explains why political competition can mitigate the harmful consequences of higher inequality for the poor.

We explore this hypothesis by utilizing variations across parliamentary constituencies in India. Our main data source is the National Family and Health Survey from 2015-2016, from which we construct a retrospective time series of child mortality. Using several detailed geocoded maps, we combine this with other secondary data sources like the National Sample Survey and the Census. Our main outcome variables are neonatal mortality that counts deaths before one month of age, and post-neonatal infant mortality that counts deaths between one and twelve months of age. We make this distinction because the policies needed to prevent deaths change according to the age of the infant. Post-neonatal infant deaths in India are largely caused by preventable diseases, such as sepsis, pneumonia or diarrhoea (Rai *et al.*, 2017). Hence, these deaths can be avoided through quite simple interventions like vaccination and Oral Rehydration Salts. Neonatal deaths are to a larger extent caused by infections, low birthweight, birth asphyxia and birth trauma (Million Death Study Collaborators and others, 2010), and major reductions in these types of deaths depend on the provision of individualized clinical care (Lawn *et al.*, 2005).

To obtain exogenous variation in our variables of interest we utilize a large redistricting of electoral boundaries – the so-called Delimitation – that took place in 2008. Our strategy is to regress mortality on constituency-level inequality and political competition, using data for the period after the redistricting and fixed effects for *pre*-delimitation parliamentary constituencies interacted with districts and year. Our estimates are thus identified by comparing the mortality risk of infants who were allocated to new constituencies with the mortality risk of infants born in the same year, the same district and the same pre-delimitation constituency, but who were not allocated to the new constituency.

The key identification assumption is that no factors but the changes at the constituency level affect the relative mortality risk. We think this is a plausible assumption, given that our identifying variation is at a fine geographical level. Essentially what is needed, is as-good-as-random allocation of households to constituencies *within districts*. We test this assumption in different ways. First, we look for evidence of gerrymandering by investigating whether influential incumbent politicians experienced an ex ante more favorable redistricting than other incumbent politicians. We do not find any evidence of this.[1] Second, we

[1] The Delimitation was organized by an independent Commission and the consensus view is that it was carried out without great political influence (Bardhan *et al.*, 2018; Iyer and Reddy, 2013). With the exception of Iyer and Reddy (2013), who provide an analysis for the

show that villagers who were allocated to new constituencies do not differ from villagers who were not, for a wide range of observables. Third, we conduct a placebo exercise based on infants born prior to the Delimitation. We do not find an impact on our variables of interest for these infants.

Our main finding is that economic inequality *causes* more post-neonatal infant deaths, but only in situations where there is a lack of political competition. To interpret the magnitudes, consider a one standard deviation increase in measured inequality. For average levels of political competition this does not affect child mortality. If instead we have a level of political competition that is one standard deviation below the sample mean, the same rise in inequality increases post-neonatal infant mortality by as much as 0.18 percentage points, or 13 percent of the sample mean. The estimates are robust to different measures of inequality and political competition and to several alternative specifications. For neonatal mortality, we find no significant impact of inequality.

Our results can be seen as reduced form evidence for how basic health care benefits the poor. Post-neonatal infant deaths are largely "unnecessary", as they can be avoided through simple policy interventions, while major reductions in neonatal deaths require investments in clinical care. A further interpretation is therefore that we are identifying small policy changes (low-hanging fruit) that are sufficient to affect the survival of older infants but not the survival of newborns.

We provide three types of evidence to support our hypothesis that the effects on mortality are more important in less contested constituencies. First, we use information in the National Family Health Survey to study supply and demand for public health care. We show that government health centers located in constituencies with *low political competition* and *high inequality* are disfavored: they have fewer staff and provide less services, such as immunization and postnatal care. None of these effects are found in the placebo regressions

redistricting of state assembly constituencies in Andhra Pradesh and Rajasthan, there has been little empirical investigation of this. We therefore perform our own test for the fifteen states included in the analysis.

we run based on information prior to the Delimitation.

The second type of supporting evidence comes from identifying similar mechanisms in a different context. If the relationship between economic inequality and political competition matters for the provision of basic public health care, it should also matter for other types of government programs for the poor. To test this, we gather a gram panchayat level dataset on implementation of the National Rural Employment Guarantee Scheme (MGNREGA) during 2011-2014. MGNREGA is the world's largest employment program, guaranteeing 100 days of minimum-wage employment, every year, for each rural household in India. Using this data, we find similar patterns as in our mortality regressions.

The third supporting evidence comes from a sample of 98 low- and middle-income countries, allowing us to show that the relationship between mortality, inequality, and political competition goes in the same direction across countries as well. In this setting, we can conduct a crude test of the mechanism by adding controls for key public provisions. When adding controls for health care expenditure, sanitation, water facilities, and education, our coefficients of interest cease to be significant all together, suggesting that the association with health outcomes goes via public policies.[2]

Our paper contributes to, and builds on, the small literature of how inequality affects the distribution of power in a society. Just to articulate collective demands requires coordination that can be more problematic in large groups with low incomes, than in smaller groups with high incomes. The presence of such unequal influence is supported by investigations by Bardhan and Mookherjee (2000) and Baland and Platteau (1997), and is also consistent with evidence that participation in social activities is lower in more unequal societies (Alesina and La Ferrara, 2000). A high concentration of income among wealthy individuals does not create similar free-riding problems (Olson, 1965), making the

[2]In this setting we are not able to separate between neonatal and post-neonatal infant mortality. These results are in line with earlier work by Anand and Ravallion (1993) on the vanishing impact of per capita income as a separate determinant of longevity.

implementation of their preferred policies more likely.

Such unequal influence is particularly relevant in studies of the effects of inequality on health (Lynch *et al.*, 2001; Judge *et al.*, 1998; Wagstaff, 2003; Wilkinson, 2002; Deaton, 2003). Like Kudamatsu (2012) and Ross (2006), we emphasize the effect of democracy on health and public health care spending. In contrast to most studies, however, we explore changes in political competition at the *intensive* margin in a setting where democracy already is well established.[3] Our causal inferences rely on variation created by redistricting. Previous studies have, in different ways, used this variation to investigate how politicians divert the allocation of public resources (Bardhan *et al.*, 2018; Jensenius and Chhibber, 2016; Nath, 2014). Consistent with the findings of Nath (2014) we conclude that in absence of electoral pressures, politicians in unequal societies are more likely to implement a level of social provision desired by the rich.

We start in section 2 by presenting some empirical and theoretical motivations. In Section 3, we describe our empirical setup and our main data sources. Details on identification and data are presented in section 4. In section 5 we provide the basic empirical results, and in section 6 we establish further evidence on the mechanism. Section 7 is the conclusion.

2 Empirical and theoretical motivation

To motivate our investigation we first show that average health outcomes are strongly associated with levels of income inequality across countries. We then present a small theoretical model that elaborates on the link from income inequality to social provisions for the poor, and how this link depends on political competition.

[3] One exception is Fujiwara (2015) who studies a large enfranchisement of less educated citizens in Brazil.

2.1 The vanishing impact of income inequality

It is well established that higher average income is associated with better health outcomes (Preston, 1975). Indeed, the higher the average income in a country, the higher the likelihood of surviving childhood and enjoying a long and healthy life. This paper focuses on the association between income inequality and average health.

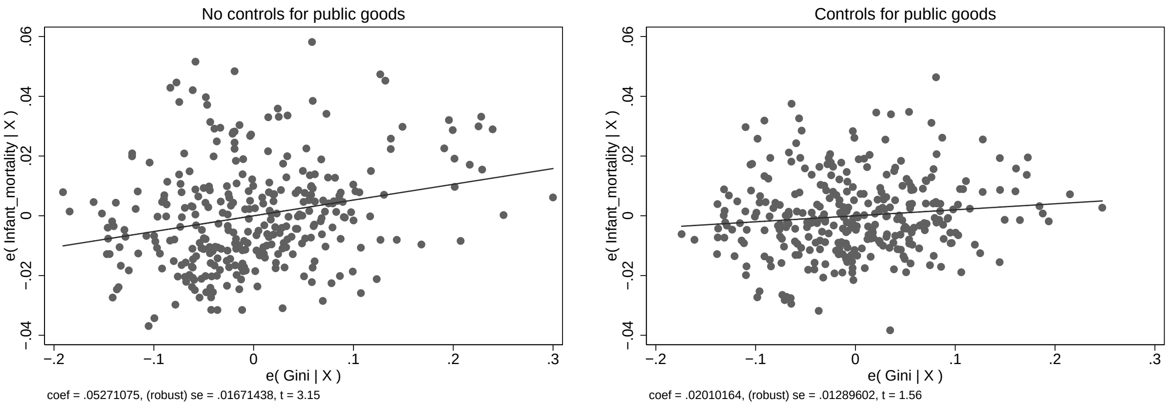
To motivate our case, consider the cross-country relationship between inequality and infant mortality in Figure 1. The data are from an unbalanced panel of 98 low- and middle-income countries during the period 1994 to 2013. We control for average income, time fixed effects and other basic country-level characteristics,[4] meaning that the figure presents partial correlations. The left panel reveals a strong positive relationship between inequality and infant deaths. It thus suggests that children from unequal societies, conditional on their average income, are less likely to survive their first year than children in more equal societies. To get an indication of which channels matter for the empirical association, we add controls capturing key publicly provided goods to see how the relationship changes. In the right panel we show the partial correlation when controlling for the population share with clean drinking water, the population share with proper sanitation, government health care expenditure per capita, and the average teacher-pupil ratio in government elementary schools (a proxy for the quality of teaching). Strikingly, these four controls are sufficient to offset the relationship between inequality and infant mortality.[5]

The lesson we draw is that publicly provided goods seem to be an important determinant of the relationship between income inequality and health. This suggests that we should explore how the distribution of income affects the effective provision of health care and of social provision for the poor more broadly.

[4] The country-level characteristics are the urbanization rate, and dummies indicating the country is tropical and predominately Muslim.

[5] Note that we obtain the same result for life expectancy.

FIGURE 1: Income inequality vs. infant mortality



The figure shows the partial correlation between the Gini coefficient and infant mortality. The vertical axis shows the residuals from a regression of infant mortality on all controls expect the Gini, while the horizontal axis shows the residuals from a regression of the Gini on the other controls. The controls are log GDP per capita, the population share living in urban areas, a dummy variable for tropical countries, a dummy for whether the country is predominantly Muslim and time fixed effects. The right panel additionally controls for the population shares with clean drinking water and sanitation, government health care expenditure per capita, and the teacher-pupil ratio in government elementary schools.

2.2 A model of social provision for the poor

In what follows, "social provision for the poor" includes not just health care but also access to any other service, such as clean water, sanitation, education and public job opportunities. There are important hindrances to such social provisions at the community level.

Consider an incumbent local politician who can decide the social provision p for the poor at a cost $b(p)$, which is increasing and convex in p. Providing p makes him less able to cater to the special interests of the rich. As long as the incumbent politician has limited resources x to allocate, he can only extract rents from rich members in his constituency by reserving parts of the total capacity x to their special demands.

Saving on the social provision for the poor to allocate these resources to the rich can contribute to the politician's residual income. The relevant part of this residual income is represented by $\pi = (x-p)f - b(p)$, showing how his potential rent extraction can go up by reducing p via cost reductions $b(p)$, misallocating the new idle resources $(x-p)$ to the rich for a price f given by their willingness to pay. The value of f depends positively on the incomes of the rich, which can vary over time. Thus, while his residual income π is declining in the social provision for the poor, it is increasing in the wealth of the rich.

Diverting resources to the rich through discretion and rule bending may give the incumbent immediate gains. These gains, we assert, must be sufficiently high relative to the possible political costs. Since there are many poor voters a lower social provision p can harm his chances of being reelected. The perceived probability of being reelected Φ relies on a persistence in how people vote. Previous elections are therefore informative about the coming political contests. An incumbent who obtained a share s of the votes in a previous election with a social provision p_{-1}, believes he can attract some of the voters who did not vote for him by expanding $p \geq p_{-1}$. His perceived chance of being reelected is $\Phi = s + \alpha(p - p_{-1})(1 - s)$, where the positive parameter $\alpha < 1$ captures the ability to attract voters. The vote share $(1-s)$ is a proxy for electoral pressure, or simply how competitive the election is. A high value of s indicates a secure tenure of the incumbent without much competition or electoral pressure.

Accordingly, choosing the level of social provision for the poor, the incumbent must trade-off the immediate rent, his residual income, π and the expected prospects of being reelected, EV. Let $V = (x - p)\,f - b(p) + \Phi\beta EV$ be the politician's net pay-off as incumbent, with β indicating the discount factor. Maximizing V for $p \leq x$ leads us to the first order condition:

$$f + b'(p) \geq \beta\alpha(1 - s)EV. \tag{1}$$

In this expression, the incumbent's marginal opportunity cost of social provision for the poor, $f + b'$, is greater than, or equal to, the expected marginal gain of being reelected. Equation (1) holds with equality when the optimal p is less than the capacity x (that is as long as $f + b'(x) > \beta\alpha(1 - s)EV$); otherwise $p = x$. We see immediately that an incumbent who faces no electoral pressure as the opponents have no chance of winning, $s = 1$, has no interest in social provisions for the poor.

To derive the full impact of political competition, however, we also have to incorporate how the value of incumbency depends on s. The value of incumbency in the steady state (where $p = p_{-1}$) is $EV = E\pi/(1-\beta s)$, showing that a higher s now raises the value of incumbency, which in isolation should make it more tempting for the incumbent to *increase* the social provision for the poor to improve his chances to become reelected.

The net effect of the two opposing effects of a higher s can be seen by inserting EV in the first order condition (1) to obtain:

$$f + b'(p) \geq h(s)\, E\pi \quad \text{where} \quad h(s) = \frac{\alpha\beta(1-s)}{1-\beta s} < 1 \quad \text{and} \quad h'(s) < 0. \qquad (2)$$

Here $h(s)$ captures the weight on future earnings as incumbent. We are interested in what levels of political competition tempt the incumbent to divert resources away from social provision for the poor. To do this we define a threshold value of the weight on future earnings $\hat{h}$ that just balances the incumbent's future expected earnings prospects $E\pi$ to his opportunity costs of social provision for the poor *at full capacity utilization*, $p = x$. The threshold is given by $\hat{h} = [f + b'(x)]/E\pi$. The critical electoral pressure, $(1-\hat{s})$, needed to discourage diversion of resources is the vote share that solves $h(s) = \hat{h}$. If no such solution exists, $(1-\hat{s})$ is unity and there is no level of electoral pressure that can discipline the incumbent.

Given the convexity of $b(p)$ we have that $p = x$ for electoral pressure higher than the critical level $((1-s) \geq (1-\hat{s}))$, and $p < x$ for electoral pressure lower than the critical level $((1-s) < (1-\hat{s}))$. In the latter case, the tenure of the incumbent is so secure that his social provision p is determined by (2) with equality. Since the left-hand side of this equation is increasing in p and the right-hand side is independent of p, the equation has a unique solution.

We are interested in the effects of income inequality and the absence, or presence, of political competition.

Higher income inequality can harm the social provision for the poor. It is clear from the discussion above that as long as the incumbent's tenure is sufficiently secure, $s > \hat{s}$, a higher level of income inequality reduces p. Since higher inequality means higher incomes for the rich and f is increasing, we find from (2) that $dp/df < 0$ as long as $s > \hat{s}$. As inequality increases, the opportunity costs of social provision to the poor become higher and the value of the social provision declines.

More political competition can benefit the social provision for the poor. More competition means higher political pressure $(1 - s)$ and thus a higher $h(s)$. This implies that the expected marginal gains to the incumbent of social provision for the poor increases. From (2) we then see that he prefers a higher level of p. In other words, a less secure tenure of the incumbent mitigates the effects of higher inequality. This is particularly evident when the electoral pressure $(1 - s) \geq (1 - \hat{s})$, which implies $p = x$ and $dp/df = 0$. Accordingly, with a sufficiently high electoral pressure the tenure of the incumbent becomes so insecure that higher inequality has no effect at all on his social provision for the poor.

To test the hypothesis we estimate both the reduced form impacts of political competition and income inequality on child mortality, and the effects of these measures on different social provisions for the poor. The effects of partial variations of inequality and political competition are interesting, but the main insight from the model that we explore is whether the negative impact of higher inequality vanishes when political competition is high. As we will explain in Section 4, we do this by estimating the impact of the interaction of inequality with political competition.

3 Institutional background

This section provides details on the Indian context that forms the basis for testing the mechanisms as outlined above. We explain how voters elect their members of parliament, and emphasize that these politicians also act as strongmen in local politics. We then describe the redistricting of electoral boundaries that took place in 2008, which we use in our empirical analysis.

3.1 Administrative and electoral levels in India

India has four administrative levels (states, districts, sub-districts and villages/wards), and first-past-the-post elections at five levels (lower house of the parliament (Lok Sabha), state assembly, district council, sub-district council and village council). The three bottom tiers make up the so-called Panchayati Raj system of local governance.

Public health care is primarily under the responsibility of the states, but the lower levels of government play a crucial role—as they do for the provision of other government services and programs. Officials at the district level typically gather healthcare demands of local governments, and based on this, present budget proposals to the state governments. Most decisions on the allocation of funds across local governments, however, are decided at the district level (Kailthya and Kambhampati, 2016).

Our analysis focuses on electoral competition at the parliamentary level. One member of parliament (MP) is elected in each of the 543 parliamentary constituencies (PCs). The constituencies are drawn by the Delimitation Commission of India. They do not cross state borders, but may cross the boundaries of administrative districts. The parliamentary level is relevant for power at the local level, as MPs play a significant role in local politics. First, MPs are members of the district level councils of all the districts that geographically overlap with their constituencies. They are thus invited to local meetings, where the distribution of resources is discussed. Second, MPs receive a yearly budget to be spent within their constituencies, through the so-called Members of Parliament Local Area Development Scheme (MPLADS). The size of this program has increased 100-fold since it was initiated in 1993, and is currently 5 million rupees per MP per year (MoSPI, 2016). Several of the MPLADS projects relate to healthcare, such as the purchase of ambulances and equipment for local health clinics (Swaminathan *et al.*, 2019).

It is also likely the MPs affect outcomes at the local levels through informal channels, such as pressuring local bureaucrats. Maheshwari (1976) studied MPs in the 1970s and found that they spent considerable time handling requests from their constituents and furthermore, that most of these requests fell within the jurisdiction of the state and not the central government. More recently, Bussel (2018) finds that MPs in Bihar, Jharkhand and UP spend close to a quarter of their reported working time meeting their constituents.

3.2 The 2008 Delimitation

When the constitution of India was drafted, the plan was to carry out a Delimitation every ten years to (i) equalize population sizes across constituencies and (ii) reserve constituencies for scheduled castes and scheduled tribes in proportion to updated measures of their population shares. In the 1970s, however, it was decided to keep the political boundaries fixed for three decades, because the increasing political representation of areas with a higher birthrate created a perverse incentive to the implementation of family planning programs (Jensenius, 2013).

The process of redrawing the map started in mid-2004 and was based on population characteristics from the 2001 Census. At the onset it was decided that the number of seats in the national legislature would remain fixed for each state. Thus, the redistricting exercise only shuffled voters across constituencies *within* Indian states. The redistricting was organized by an independent three-member Commission. In each of the states, this Commission was advised by ten associate members, consisting of five MPs and five members of the State Assembly (MLAs). These associate members had no formal voting power, but they were closely involved in the process.

A draft of the new electoral boundaries was distributed widely and public comments were invited. After this process, the Commission submitted their final report, which was approved by the President of India in August 2008. On average, about one quarter of the rural population was allocated to a new constituency. Table 1 displays statistics by state. The first column provides the number of constituencies within the state, the second column the average number of voters per constituency, and the final column the share of voters who were allocated to a different constituency.

Table 1: Changes in parliamentary constituencies

	Number of constituencies (1)	Number of voters per constituency (in mill.) (2)	Average share of voters allocated to a new constituency (3)
Andhra Pradesh	42	1.38	.27
Bihar	40	1.36	.29
Chhattisgarh	11	1.41	.15
Gujarat	26	1.40	.20
Haryana	10	1.21	.17
Karnataka	28	1.47	.16
Kerala	20	1.09	.34
Madhya Pradesh	29	1.31	.15
Maharashtra	43	1.50	.28
Odisha	21	1.29	.17
Punjab	13	1.30	.29
Rajasthan	25	1.48	.27
Tamil Nadu	38	1.07	.28
Uttar Pradesh	80	1.45	.30
West Bengal	40	1.25	.24
All	466	1.35	.25

The estimates are based on our own calculations using digitalized maps. The number of parliamentary constituencies in the table does not always correspond to the actual number of constituencies, as we have excluded constituencies that are all urban.

Many countries go through similar redistricting processes, and influential politicians are often accused of tweaking the redistricting to create safer seats for themselves. Despite the Delimitation Commission being independent, we cannot exclude that politicians played a role in the process. Iyer and Reddy (2013) study the redistricting in two large states, Andhra Pradesh and Rajasthan, and conclude that the boundary changes "were politically neutral for most parts". Bardhan *et al.* (2018) study the redistricting in West Bengal and come to the same conclusion. To validate our identification, we replicated Iyer and Reddy (2013) for all 15 states in our sample using geocoded maps. The details of this exercise are presented in Appendix B, but we summarize the main results here. First, we confirm the redistricting was done in the intended direction: since the purpose was to equalize population sizes within states, the greatest absolute population changes occur in the smallest and largest constituencies. Second, we do not find evidence of gerrymandering. We focus on potential political interference by the associate members, since these politicians were the ones most likely to be able to affect the process. We find no evidence of an ex ante more favorable redistricting for the MPs who serve as associate members.

4 Empirical setup

Having established that the redistricting was politically neutral, we now explain how we utilize the boundary changes to identify our relationships of interest.

4.1 Identification

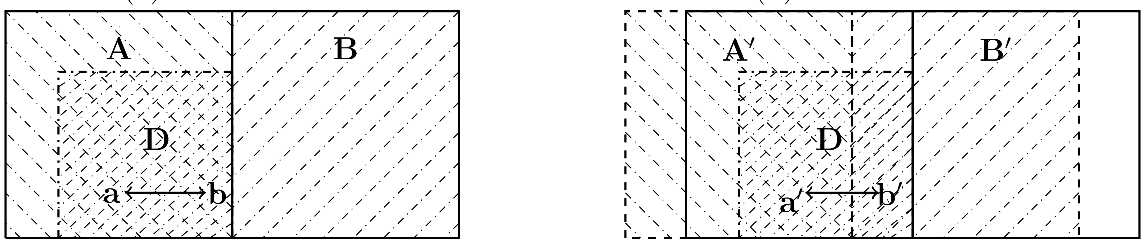
We exploit the boundary changes to identify causal estimates of the relationship between infant deaths on the one hand, and income inequality and political competition on the other hand. In section 2 we illustrated the role of income inequality and political competition for public health care provision. In the following, we infer that there is a strong link between health care provision and health outcomes, and use mortality of infants as our main outcome. We use the same methodology outlined below when we later explore other aspects of public provision.

We compare the mortality risk of infants born in the same year who share the same pre-delimitation constituency and district. We implement this by including fixed effects for pre-delimitation constituency interacted with district and birth year. We include districts in the fixed effects, since this is an important administrative unit for social provision for the poor (see Section 3).

Our identification is illustrated in Figure 2. Imagine two infants, a' and b', born a year after the Delimitation in district D. Prior to the Delimitation, they would have been born in the same pre-delimitation constituency A, but due to the boundary changes infant a' belongs to post-delimitation constituency A' and infant b' belongs to post-delimitation constituency B'. We explore differences in mean income, inequality and political competition across the post- constituencies A' and B' and investigate how these differences affect the relative mortality risks of the infants a' and b'. The key identification assumption is that no factors other than changes at the constituency level due to the redistricting affect their relative mortality risk. We use data on infant a and b to test for this. As these infants were born prior to the Delimitation, their relative mortality risk should not be affected by differences between the post-delimitation constituencies A' and B'. We can thus compare their relative mortality risk as a placebo.

FIGURE 2: Illustration of the Identification Strategy

(a) Pre-delimitation (b) Post-delimitation



A remaining concern is that redistricted people may directly have influenced the explanatory variables of interest in their new constituency. To deal with this, we calculate mean income, inequality and political competition for constituencies A' and B' based on the boundaries of the pre-delimitation constituencies A and B. In practise, we define the corresponding pre-delimitation constituency as the one that has the largest population overlap with the post-delimitation constituency of interest. For political competition, we use data from the election prior to the Delimitation. This election took place in April-May 2004 and should not have been affected by the Delimitation as the Commission only started its work in July 2004.

We run the following baseline specification:

$$m_{idklt} = \beta_0 + \beta_1 q_k + \beta_2 v_k + \beta_3 z_k + \beta_4 (v_k \times z_k) + \theta'_{dkt} + X'_{1,dkl}\gamma_1 + X'_{2,idkl}\gamma_2 + \epsilon_{idklt}, \tag{3}$$

where m_{idklt} is the mortality of child i born in year t in district d and post-delimitation constituency l. The latter corresponds to the pre-delimitation constituency k. Mean income is denoted by q_k, inequality is denoted by v_k, and political competition is denoted by z_k. To ease the interpretation, we standardize these variables by subtracting their mean and dividing by their standard deviation. The fixed effects are denoted by θ'_{dkt}. The standard errors, ϵ_{idklt}, are clustered at the district $\times$ pre-delimitation constituency level.

As stated in Section 2 we are especially interested in the coefficient β_4, capturing the interaction effects. To be sure that the estimated coefficients are not driven by other observable factors, we add two types of controls. First, we control for area characteristics at the pre-delimitation constituency $\times$ post-delimitation constituency $\times$ district level ($X'_{1,dkl}$). This includes the literacy rate, the population share of scheduled caste and scheduled tribes, the population share of children below six years of age and the availability of public amenities prior to the Delimitation. Second, we control for the following child level characteristics ($X'_{2,idkl}$): gender, religion and whether or not the child is a twin.

4.2 Data and measurement

Our analysis is based on several data sources, which we link through geocoded maps. Our main source of geospatial data is the InfoMap village and constituency maps. This subsection describes the sources and how we construct our key variables.

We use the 2015-2016 National Family and Health Survey (NFHS) as our data source on mortality.[6] The NFHS interviews women aged 15 to 49 years

[6]This survey is the same as the Demographic and Health Survey (DHS).

old and measures the complete birth record of these women. The survey data contain information on the timing of all births, and if the child died, the age in months when death occurred. Based on this we are able to construct a retrospective time series of infant deaths. The 2015-2016 survey interviewed about 700,000 women, which is a much larger sample than in the previous NFHS waves, making it meaningful for the study of mortality at a fine geographical level. Another advantage is that the 2015-2016 survey provides GPS coordinates for survey clusters. These clusters roughly coincide with gram panchayats, i.e. the lowest official authorities in India that are composed of five to fifteen contiguous villages. We combine the cluster coordinates with geocoded maps of constituency boundaries to allocate survey respondents to constituencies.

We distinguish between neonatal and post-neonatal infant mortality. Neonatal mortality is defined as deaths before four weeks of age. As in Bhalotra and van Soest (2008), we include deaths up to one month to allow for age-heaping. Post-neonatal infant mortality is then defined as deaths between one and twelve months of age. We distinguish between the two types of mortality as the policies needed to reduce them are likely to be different. While post-neonatal infant deaths can be reduced through interventions that focus on pneumonia, diarrhea, malaria and vaccine-preventable conditions, achieving major reductions in early neonatal deaths will depend on the provision of individualised clinical care (Lawn *et al.*, 2005). For both neonatal and post-neonatal infant mortality, we construct a binary variable taking the value of one if the child died and zero otherwise, provided that the child was fully exposed to the particular mortality concept (see e.g. Rutstein, 2005). To clarify, this means that the sample used to calculate post-neonatal infant mortality only includes children born at least 12 months before the end of the period, while the sample for neonatal mortality includes children born at least one month before the end of the period.

We use different datasets to examine our proposed mechanism. To obtain data on public health clinics, we use the District Level Household and Facility Survey (DLHS) for 2007-2008 and 2012-2013. From this data we extract information on the number of medical employees and the services they provide. To conduct our analysis of MGNREGA, we extract gram panchayat level data from the MGNREGA Public Data Portal for the financial years 2011-2012, 2012-2013 and 2013-2014. Using this data, we construct two outcome variables at the gram panchayat level: (i) the number of workers, and (ii) the total amount disbursed to laborers' bank and post office accounts. More details on how we compile these two datasets are provided in Appendix E.3 and Appendix E.4.

Data on electoral outcomes are taken from the Indian National Election and Candidates Database (Jensenius and Verniers, 2017). This dataset contains the number of votes the most important candidates received in each constituency. We use the election results from 1999 and 2004. Consistent with the model in Section 2, we measure political competition using the vote shares of the incumbents and their opponents. In the illustrative model we focused on the simple case where the electoral pressure was represented by one minus the most recent vote share of the incumbent $(1 - s)$ capturing whether the incumbent won overwhelmingly or not. One useful extension of this measure is one minus the Herfindahl-Hirschman index. A high value of the Herfindahl-Hirschman index implies that the votes cast have a high concentration on the incumbent, meaning that the electoral pressure is likely to be low. For constituency k, the political competition is thus measured by:

$$1 - HHI_k = 1 - \sum_{c=1}^{n} s_{ck}^2,$$

where s_{ck} is the vote share of candidate c. We use this as our baseline measure, but we explore alternative measures of political competition in the robustness analysis, including one minus the vote share of the incumbent.

Data on household expenditure, which we use to calculate average expenditures and inequality, are taken from the the National Sample Survey (NSS). This is a national-wide representative household survey, usually conducted every five years, with a sample size of more than 100,000 households in each round. We use the 2009-2010 (66th round) survey for our main analysis and the 2004-2005 (61st round) survey for our placebo analysis.

Our analysis requires estimates of mean expenditures and inequality at the level of election constituencies. However, the NSS data does not include identifiers for constituencies, nor does it provide geocodes for where households are located. The finest geographical unit we can identify is the district. Sometimes these districts perfectly coincide with constituencies, sometimes they do not. Another challenge is that some district boundaries changed during our study period. We tackle this as follows. We first convert the districts that changed between 2001 and the NSS surveys back to the district boundaries as of the 2001 Census. In most cases a single district was split into two parts, so this adjustment is unproblematic. Based on the geocoded maps of election constituencies and 2001 Census villages, we then calculate the population share of each district in the different constituencies. A share can be interpreted as the probability that a household belongs to a particular constituency, conditional on the district it resides in. Finally, we calculate mean expenditures and inequality for a constituency by weighting households by their probability of residing in that particular constituency.[7]

We construct the control variables using the Census of India for 2001. This dataset includes basic population characteristics and a large set of amenities for all Indian villages.

4.3 Sample selection and summary statistics

The Delimitation changed electoral boundaries *within* states, while the number of constituencies for each state remained the same. Since the small states of India only have one or two parliamentary constituencies each, we exclude them from the analysis. We also exclude Assam, Jammu & Kashmir and Jharkhand since they never implemented the Delimitation. We are left with a sample of 15

[7]This, therefore, assumes that expenditures are uniformly distributed across space within districts.

large states, which are listed in Table 1. According to the 2011 Census, these states account for about 90 percent of the total rural population.

We limit our main analysis to children born in rural areas of India. There are two reasons for doing so. First, our identification assumption may not hold for urban areas, as it is much less likely that cities are allocated to new constituencies than rural areas. Second, the amenity data of the Census only cover rural India, so we cannot control for background characteristics in urban areas. Note, however, that political competition, average expenditures and inequality are measured at the constituency level, which may include some urban areas.

In our mortality regressions we focus on the time period between the 2009 and 2014 parliamentary elections. We exclude infants whose mothers moved after their birth, as well as infants whose mother was a visitor in the location where she was interviewed.

Table 2 provides summary statistics for the key variables. About 3.4 percent of the children die during the first month, and 1.4 percent after the first month but before age one. The average Gini coefficient across constituencies is 27.6 and our measure of political competition, one minus the Herfindahl-Hirschman index, equals 0.62.

Table 2: Summary Statistics

	Level (1)	Observations (2)	Mean (3)	Std. Dev. (4)
Neonatal mortality	Individual	116917	.0341	.1814
Post-neonatal infant mortality	Individual	90236	.0135	.1154
Gini coefficient	Constituency	447	.2760	.0623
1-Herfindahl-Hirschman of vote shares	Constituency	447	.6170	.0813
Mean expenditures	Constituency	447	1124	392.7

Data sources: The health outcomes are based on the 2015-2016 NFHS, inequality and mean expenditures are based on the NSS survey from 2009-2010 and the measure of political competition on data from the 2004 parliamentary election.

5 Empirical evidence

In this section we describe our main results and discuss how robust they are.

5.1 Assessing the empirical model

Our identification assumes that changes in inequality and political competition due to the redistricting are unrelated to factors affecting mortality once we include our fixed effects. To investigate whether our data are consistent with this, we first look for possible differences in observables between villagers who are allocated to new constituencies and those who remain in their original ones. To do so, we run the following regressions:

$$y_{idklt} = \beta_0 + \beta_1 T_{idklt} + \theta'_{dkt} + \epsilon_{idklt}, \qquad (4)$$

where y_{idklt} is the observable of interest for child i born in year t (that is, all the variables included in $X'_{1,dkl}$ and $X'_{2,idkl}$ in equation (3)), who resides in district d, pre-delimitation constituency k and post-delimitation constituency l, and T_{idklt} is a dummy indicating the individual was allocated to a new constituency ($k \neq l$). The fixed effects are denoted by θ'_{dkt}, as in (3). Table 3 displays the coefficient of interest, β_1, for each of the dependent variables. We run the regressions separately for the samples of neonatal and post-neonatal infant mortality. The table shows that those who changed constituency do not differ significantly from those who did not.

We also test whether the observables are jointly significant. We do this by placing the dummy T_{idklt} on the left-hand side and all the listed observables, in addition to the fixed effects, on the right-hand side. The F-tests from this exercise are .99 and .76 for the neonatal and the post-neonatal infant mortality samples, respectively. This implies that the observables are far from being jointly significant.

Table 3: Balance table

	Panel A: Neonatal	Panel B: Post-neonatal
	(1)	(2)
Area characteristics:		
Share of scheduled caste	.0003	.0004
	(.0033)	(.0033)
Share of scheduled tribe	.0031	.0032
	(.0054)	(.0054)
Share of children below 6 years old	-.0001	-.0001
	(.0008)	(.0008)
Share of literate people	-.0036	-.0037
	(.0035)	(.0035)
Share of villages with a primary school	-.0015	-.0019
	(.0073)	(.0074)
Share of villages with a primary health centre	.0024	.0023
	(.0020)	(.0019)
Share of villages with a primary health sub-centre	.0018	.0011
	(.0049)	(.0049)
Share of villages with tap water	-.0036	-.0038
	(.0097)	(.0097)
Share of villages with electricity	-.0062	-.0063
	(.0104)	(.0104)
Share of villages connected with a paved road	-.0050	-.0054
	(.0079)	(.0079)
Individual characteristics:		
Child is a girl	-.0100*	-.0065
	(.0058)	(.0057)
Child is a twin	-.0001	.0004
	(.0022)	(.0022)
Religion: Hindu	.0115	.0107
	(.0111)	(.0111)
Religion: Muslim	-.0074	-.0075
	(.0107)	(.0106)
Religion: Christian	-.0004	-.0004
	(.0014)	(.0015)
Religion: Sikh	-.0026	-.0021
	(.0035)	(.0035)
Religion: Buddhist	-.0024*	-.0020*
	(.0012)	(.0012)
Number of observations	116917	90236

The first column reports the differences (and standard deviations) between the means in the group of villagers who were allocated to new constituencies and those who were not for the sample of neonatal infant mortality. The second column provides the same information for the sample of post-neonatal infant mortality. All regressions include pre-delimitation constituency × district × birth year fixed effects. Robust standard errors, clustered at the pre-delimitation constituency × district level, are shown in the columns (2) and (4). *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

5.2 Main results

Table 4 presents the main results based on Equation (3). The first three columns show the impact of our variables of interest on neonatal mortality, and the last three columns on post-neonatal infant mortality. As discussed in Section 4, we distinguish between the two types of mortality as the policies needed to reduce them are very different. While post-neonatal infant deaths can be reduced through simpler interventions, the reduction in neonatal deaths depends on the provision of individual clinical care. As such, it is not entirely surprising that our variables of interest do not influence neonatal mortality (column (1) to (3)). In the following we therefore focus on the estimates for post-neonatal infant mortality only. We always control for average expenditures. In Column (5) we also include controls for area characteristics ($X'_{1,dkl}$), and in Column (6) we add child characteristics ($X'_{2,idkl}$). As expected, given the seemingly balanced sample, the estimated coefficients change little when we include these controls.

At average levels of political competition, inequality does not impact post-neonatal infant mortality. Since we have standardized our independent variables, this can be seen directly from the inequality coefficient. However, the interaction term is negative and significant and its magnitude is important. Suppose the Gini coefficient increases by one standard deviation (corresponding to a 6.2 percentage points increase). If political competition is one standard deviation below its sample mean, this rise in inequality leads to an increase in post-neonatal infant mortality of 0.18 percentage points, or 13 percent of the sample mean.

Does the linear interaction term capture the mechanism we are interested in? To test this, we estimate a specification where we replace the continuous measure of political competition by categorical variables indicating whether the level of political competition is low, intermediate or high. Using the interme-

Table 4: Baseline regressions

	Neonatal mortality			Post-neonatal infant mortality		
	(1)	(2)	(3)	(4)	(5)	(6)
Inequality	.0012	.0010	.0010	.0002	-.0001	-.0001
	(.0021)	(.0023)	(.0023)	(.0016)	(.0016)	(.0016)
Political competition	-.0001	.0000	.0003	.0003	.0007	.0007
	(.0020)	(.0021)	(.0021)	(.0015)	(.0016)	(.0016)
Inequality ×	.0014	.0015	.0013	-.0018**	-.0018**	-.0019**
Political competition	(.0013)	(.0013)	(.0014)	(.0009)	(.0009)	(.0009)
Observations	116917	116917	116917	90236	90236	90236
Average expenditures	Yes	Yes	Yes	Yes	Yes	Yes
Area characteristics	No	Yes	Yes	No	Yes	Yes
Child characteristics	No	No	Yes	No	No	Yes

All regressions include pre-delimitation constituency × district × birth year fixed effects. Robust standard errors, clustered at the pre-delimitation constituency × district level, are shown in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

diate level as the reference, the specification is now as follows:

$$m_{idklt} = \beta_0 + \beta_1 q_k + \beta_2 v_k + \beta_3^{low} z_k^{low} + \beta_3^{high} z_k^{high} + \beta_4^{low}(v_k \times z_k^{low}) + \beta_4^{high}(v_k \times z_k^{high}) \tag{5}$$
$$+ \theta'_{dkt} + X'_{1,dkl}\gamma_1 + X'_{2,idkl}\gamma_2 + \epsilon_{idklt},$$

where z_k^{low} and z_k^{high} are binary variables taking value one if the political competition is low or high, and zero otherwise.

Table 5 presents estimates of the interaction term for varying limits of the categorization. The limits for low and high competition are based on deciles from the distribution of political competition in our sample. In Column (1), we define low competition as values below the fourth decile and high competition as values above the sixth one. These thresholds are subsequently changed to the third and the seventh deciles in Column (2), and so on. The results confirm our proposed mechanism: the impact on post-neonatal infant mortality is driven by a strong impact of inequality when political competition is slack.

Table 5: The effect of inequality on post-neonatal infant mortality, using a categorization of political competition

	Post-neonatal infant mortality			
	Categorization of political competition:			
	Low: $< 40\%$ High: $> 60\%$	Low: $< 30\%$ High: $> 70\%$	Low: $< 20\%$ High: $> 80\%$	Low: $< 10\%$ High: $> 90\%$
	(1)	(2)	(3)	(4)
Inequality	.0011	-.0007	-.0007	-.0013
	(.0019)	(.0018)	(.0016)	(.0016)
Low political competition	.0016	-.0002	-.0011	.0002
	(.0026)	(.0021)	(.0022)	(.0031)
High political competition	.0041	-.0022	-.0009	.0036
	(.0037)	(.0035)	(.0044)	(.0056)
Inequality $\times$ Low political competition	.0005	.0035	.0055**	.0072**
	(.0025)	(.0022)	(.0027)	(.0032)
Inequality $\times$ High political competition	-.0041	-.0017	-.0026	.0023
	(.0026)	(.0024)	(.0020)	(.0040)
Observations	90236	90236	90236	90236

All regressions include controls for average expenditure, area and child characteristics, as well as pre-delimitation constituency $\times$ district fixed effects. Robust standard errors, clustered at the pre-delimitation constituency $\times$ district level, are shown in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

5.3 Placebo analysis

We now test our identification strategy using data on infants born between the 2004 and the 2009 parliamentary elections. The mortality risk of these infants, born before the Delimitation, should not be affected by electoral outcomes in their (future) post-delimitation constituencies. To check this, we make use of the NSS survey from 2004-05 to calculate mean expenditures and inequality, and outcomes from the 1999 parliamentary election to calculate political competition.

Admittedly this is not a perfect placebo, as the boundary changes were discussed before 2008. We therefore cannot completely rule out that the post-delimitation constituencies played a role even before they formally got their own MP. However, most of the coefficients in Table 6 are small and none of them are statistically significant.

TABLE 6: Placebo regression, using births in 2004-2008

	Neonatal mortality			Post-neonatal infant mortality		
	(1)	(2)	(3)	(4)	(5)	(6)
Inequality	-.0009	-.0005	-.0003	-.0036	-.0037	-.0037
	(.0028)	(.0028)	(.0028)	(.0024)	(.0024)	(.0024)
Political competition	.0022	.0021	.0018	-.0025	-.0035	-.0035
	(.0030)	(.0030)	(.0030)	(.0024)	(.0024)	(.0024)
Inequality $\times$	-.0021	-.0025	-.0023	-.0012	-.0007	-.0007
Political competition	(.0022)	(.0022)	(.0022)	(.0017)	(.0016)	(.0016)
Observations	114685	114685	114685	87976	87976	87976
Average expenditures	Yes	Yes	Yes	Yes	Yes	Yes
Area characteristics	No	Yes	Yes	No	Yes	Yes
Child characteristics	No	No	Yes	No	No	Yes

All regressions include pre-delimitation constituency $\times$ district $\times$ birth year fixed effects. Robust standard errors, clustered at the pre-delimitation constituency $\times$ district level, are shown in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

5.4 Robustness analysis

In this section we explore the robustness of our estimates. The details are presented in Appendix C, but we summarize the main results here.

We first test the robustness of our results to alternative measures of political competition and inequality. For political competition, these include (i) one minus the vote share of the winning party, (ii) one minus the margin of victory, and (iii) the effective number of parties. For each of these indices, a higher level indicates more political competition. The alternative measures for inequality are (i) p90/p10 (the ratio of the average income of the richest 10 percent to the poorest 10 percent), (ii) the mean log deviation, and (iii) the Theil index. The estimates in Appendix C show that our results are relatively robust to the alternative measures of political competition and that they become somewhat stronger when using the alternative measures of inequality.

One concern is that our interaction term picks up other factors that are correlated with inequality and political competition. Therefore, we run a set of "horse race" regressions where we interact both inequality and political competition with several other variables. We first add interactions with mean expenditures. Second, we add interactions with the full set of area controls (population characteristics and access to publicly provided amenities), and third with the child characteristics. Our estimates are robust to all of these rather demanding specifications.

Finally, we provide three additional robustness tests. In the first, we calculate mean expenditures and inequality using the post-delimitation boundaries l instead of the pre-delimitation boundaries k. In the second test, we add dummies for the five largest political parties in 2009 to check whether our findings are driven by party ideology rather than political competition. In the final test, we add controls for the reservation status of the constituencies in 2009. Our estimates are robust to all of these alternative specifications as well.

6 Exploring the mechanism

We interpret the estimates in the previous section as a reduced form effect of inequality and political competition, operating via public provision for the poor. In this section we provide supporting evidence for this interpretation. First, we investigate outcomes related to public health care. Second, we study the Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA), a public program that is unrelated to health, but of first order interest to the poor. We therefore presume a similar mechanism may be at play as for health care. Finally, we use cross-country data to study the mechanism in a broader setting.

6.1 Public health care: Supply and demand

Our approach is to replace mortality by outcomes related to supply and demand for public health care. All the outcomes in this section are positive (they are likely to improve health) and thus regression coefficients of the opposite sign to the mortality regressions, i.e. $\beta_2 < 0$ and $\beta_4 > 0$, should be seen as support for our interpretation.

We first consider the demand for public health care. The National Family and Health Survey includes a question on where households normally go for treatment when sick.[8] From this we construct an outcome variable taking the value of one when the response is a government health care facility. Note that the question is asked with reference to present time (2015-2016), which is after our main estimation period. With this caveat, Column (1) of Table 7 reveals a positive and significant interaction term between inequality and political competition. The effect is relatively modest in size: if political competition is one standard deviation below the mean, a rise in inequality of one standard deviation reduces the likelihood of stating a preference for public health care by 0.93

[8]The exact question is as follows: *When members of your household get sick, where do they generally go for treatment?* We chose this question because it is the broadest of the survey questions related to usage of public health care.

percentage points (2.1 percent of the sample mean).

Our main presumption is that the effect on child mortality is caused by changes on the supply side of public health care. To study this more directly, we make use of the District Level Household and Facility Survey from 2012-2013.[9] We focus on Primary Health Centers (PHCs). These health clinics are the cornerstone of the rural health system in India, and are the first contact point between villagers and government medical officers.[10] The survey data reports several characteristics of the health clinics that we could explore. Given the multiplicity of possible hypotheses to test, we construct one summary index for key health care staff and two indices for services provided at the clinic. The staff index is constructed as the average of three binary variables, capturing whether the PHC has a doctor, a nurse and a midwife. The service indices are similarly constructed as the average of six and five binary variables which capture different services that should be important for neonatal and post-neonatal infants, respectively.

Estimation results are presented in Columns (2) to (4) in Table 7. We find a positive and significant interaction term for all three indices. If we again evaluate political competition at a level one standard deviation below the sample mean, a one standard deviation rise in inequality reduces the staff index by 0.021, the first service index (related to neonatal mortality) by 0.016 and the second service index (related to post-neonatal infant mortality) by 0.041. These numbers correspond to 3.2, 2.2 and 6.9 percent of the sample mean, respectively. In Appendix D we present the results for all sub-indices.

We also conduct a placebo exercise on the outcomes in Table D4 in Appendix

[9] We only make use of the facility part of this survey, since the household part does not cover the most disadvantaged states of India.

[10] The government health care system in rural India consists of three main tiers. The first tier is the sub-centers, which are supposed to cover a population of 3,000 to 5,000 and have a sanctioned strength of one male and one female health worker. The PHCs make up the second tier, and cover a population of 20,000 to 30,000. They provide curative, preventive and promotive health care, and act as a referral unit for the sub-centers. The third tier, the Community Health Centers (CHC), provide specialist care and act as referral centers for the PHCs.

Table 7: Demand for and supply of public health care at primary health centers

	NFHS 2015-2016	DLHS 2012-2013		
	Household chooses public health care (1)	Staff (2)	Services 1 (3)	Services 2 (4)
Inequality	.0019	.0026	.0138	-.0164
	(.0079)	(.0176)	(.0197)	(.0203)
Political competition	.0045	.0068	-.0067	-.0090
	(.0056)	(.0149)	(.0190)	(.0177)
Inequality $\times$	.0093***	.0238**	.0297**	.0250**
Political competition	(.0031)	(.0112)	(.0134)	(.0120)
Observations	190197	5675	5675	5675
Mean dependent variable	.445	.664	.727	.602

All regressions include controls for average expenditure and area characteristics, as well as pre-delimitation constituency $\times$ district fixed effects. The regression in Column (1) additionally controls for the religion of the household head. Robust standard errors, clustered at the pre-delimitation constituency $\times$ district level, are shown in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

D. To do so, we use the District Level Facility and Household Survey from 2007-2008.[11] All the coefficients are close to zero and none of them are statistically significant. In all, the results in this section support our interpretation of the mortality findings and suggest that inequality affects the supply and demand for public health care.

6.2 National Rural Employment Guarantee Scheme (MGNREGA)

If the relationship between economic inequality and political competition matters for the provision of basic public health care, it might also matter for other types of government programs that benefit the poor. One such program is MGNREGA, the world's largest employment scheme that guarantees 100 days of minimum-wage employment per year to rural households. MGNREGA is funded jointly through central and state government budgets, but is implemented at the local level. Typical work consists of building and maintaining

[11] Appendix E.3 lays out the details on how we combine this survey with our other data.

local public goods such as wells, ponds and dams. In this section we explore micro data on MGNREGA implementation and find a similar relationship with inequality and political competition as we did for public health.

The employment scheme has brought about positive effects, including increased wages (Imbert and Papp, 2015) and better living conditions for rural households more generally (Ministry of Rural Development, 2012). The effect on child health and survival is less clear (Chari *et al.*, 2019; Spears and Lamba, 2013). On the one hand, the program emphasises female participation which might improve female empowerment and investment in children. In addition, child health might benefit from improved public goods due to MGNREGA projects. On the other hand, female participation could crowd out time spent by mothers with their children, which is likely to reduce child health. We therefore find it unlikely that our estimated effects of inequality and political competition on child survival are driven by MGNREGA implementation in any important way.[12]

The implementation of NREGA is highly uneven across India. Gulzar and Pasquale (2017) show that the top decile of households in 2013 worked 98 days per year while the bottom decile worked only 17 days. They also document a large variation even within small geographical areas. In principle, this variation should be due to differences in demand for work. However, previous research has shown that differences in implementation are almost entirely due to the supply side (Dutta *et al.*, 2014; Khosla, 2011; Maiorano, 2014).

Implementation can be harmed by a myriad of bottlenecks, which – to a varying degree – depend on local bureaucrats and politicians. In theory, the bureaucrats at the district and block level administration should be most critical for implementation, as they are the ones approving documents, generating new projects and selecting locations (Gulzar and Pasquale, 2017). Officially, politicians play little or no role. They may however still be able, and willing, to

[12]Note however that this potential channel and the channel going through public health care, discussed above, are not mutually exclusive. Note also that it does not pose a threat to our identification.

pressure bureaucrats to improve implementation. Previous research documents that MLAs apply pressure to target certain blocks within their constituencies (Maiorano, 2014) and to initiate certain type of work projects (Aiyar and Samji, 2009). In order to gain votes, MPs are likely to act in a similar fashion. The MPs have the capabilities to do this, as they are part of the district council and they play a key role in local politics more generally (see Section 3). The findings of Gupta and Mukhopadhyay (2016) support this view. The authors use data from Rajasthan to show that larger amounts of MGNREGA funds were allocated to blocks where the incumbent party at the state level – the Indian National Congress (INC) – had a lower seat share. This effect, however, is only found in districts where the MP was from INC, suggesting that MPs are indeed able to affect the implementation of the program.

We use our gram panchayat level data set to conduct the analysis, focusing on the following two outcomes: (i) the number of workers, and (ii) the total amount disbursed to laborers' bank and post office accounts. The average number of workers per year is 328 (with a standard deviation of 564), while the average amount dispersed per year and gram panchayat is INC 775,560 or about USD 11,000 (with a standard deviation of INC 1,734,300). We use these outcomes on the left-hand side in specification (3). As before, we include pre-delimitation constituency $\times$ district $\times$ year fixed effects, where the year now indicates the financial year for which the data was obtained. We also include the area controls, aggregated at the gram panchayat level. For the ease of interpretation, we standardize the outcome variables to mean zero and standard deviation one.

Table 8 presents the results. The coefficients of interest are similar for both outcomes, but more precisely estimated for the amount of rupees disbursed, which is the broader measure of the two. A one standard deviation rise in inequality, when political competition is at its sample mean, reduces the number of workers and the amount disbursed by around 0.020 standard deviation. If political competition instead is one standard deviation below the mean, the same rise in inequality reduces the number of workers by 0.029 standard deviations and the amount disbursed by 0.032 standard deviations. This corresponds to 16.5 workers (5.0 percent of the sample mean) and INR 56,000 or about USD 800 (7.2 percent of the sample mean) per year, respectively.

TABLE 8: Implementation of MGNREGA, 2011-2012 to 2013-2014

	Number of workers (1)	Amount disbursed (2)
Inequality	-.0181	-.0183*
	(.0114)	(.0101)
Political competition	-.0037	.0020
	(.0114)	(.0084)
Inequality × Political competition	.0111*	.0140**
	(.0059)	(.0061)
Observations	451231	451231

All regressions include controls for average expenditure and gram panchayat characteristics (population characteristics and public goods), as well as pre-delimitation constituency × district fixed effects. Outcome variables are standardized to mean zero and standard deviation one. Robust standard errors, clustered at the pre-delimitation constituency × district level, are shown in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

6.3 Cross-country evidence

In this section we use the sample of 98 low- and middle-income countries that we introduced in Section 2 to provide additional evidence in support of our mechanism. We do not have information on vote shares in this cross-country setting. Instead we proxy political competition by the *The Competitiveness of Participation* index from Polity IV. Using this index, we construct a variable taking the value of one if the country is labeled as "competitive" and zero otherwise. Appendix A presents more details on the data sources and construction.

Table 9 presents cross-country regressions that correspond to specification (3). All regressions include year fixed effects and control for average income, the population share living in urban areas, a dummy variable for tropical countries and a dummy for whether the country is predominantly Muslim. To ease the comparison with our baseline estimates we standardized the Gini coefficient to mean zero and standard deviation one. Note that a standard deviation corresponds to .091 Gini points in the cross-country setting and .062 in our India sample.

Column (1) shows that the relationships go in the same direction as predicted by our model: infant mortality is strongly correlated with inequality and political competition significantly moderates its harmful impact. Taken at face-value, the coefficients suggest that one standard deviation higher inequality is associated with a 0.52 percentage points increase in the share of infants dying. The sum of the inequality coefficient and the interaction term is not significantly different from zero, meaning that we do not find such an effect for countries with political competition. In Column (2) we provide a crude test of our proposed mechanism by adding the four measures of publicly provided goods and services: health care expenditure, sanitation, water facilities and education. Doing this, all coefficients of interest become smaller in magnititude and cease to be statistically significant, suggesting that the association between inequality and mortality goes via public policies.

Table 9: Infant Mortality – Cross-Country

	(1)	(2)
Inequality	.0052***	.0017
	(.0015)	(.0012)
Political competition	.0006	-.0036
	(.0036)	(.0034)
Inequality ×	-.0094**	.0016
Political competition	(.0037)	(.0031)
Observations	316	316
Mean dependent variable	.0447	.0447
Controls for publicly provided goods	No	Yes

All columns include time binds fixed effects and control for log GDP per capita, the urbanization rate, and whether the country is tropical and predominantly Muslim. Results in column (2) also control for key publicly provided goods: the population shares with clean drinking water and sanitation, the government health care expenditure per capita and the teacher-pupil ratio in primary schools.
Standard errors, clustered at the country level, are provided in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

7 Conclusion

Our paper focuses on political strongmen at the local level. Using a simple theoretical framework, we show that – as long as the tenure of incumbent politicians is secure – a high income gap between the rich and the poor can tempt them to cater more for the wealthy than for the destitute. As such, they may offer social provisions for the poor that are insufficient to protect the health of children. Sufficiently high electoral pressure can make the underprovision for the poor disappear as it becomes too costly from a political point of view – even when inequality is high.

We find empirical support for this hypothesis in the case of India. Using a large redistricting of electoral boundaries to obtain exogenous variation in our variables of interest, we have explored the reduced form impact of measures of political competition and income inequality on infant mortality. Higher economic inequality leads to more post-neonatal infant deaths, but only in situations with a lack of political competition. We provide further evidence to support our story that the effects on mortality go through changes in how political contests affect policy. We show that government health centers located in constituencies with low political competition and high inequality are worse off: they have fewer staff and provide less services, such as immunization and postnatal care. Not surprisingly, households are less likely to use government health care in these constituencies.

Our mechanism is more general, as it is also at play in other welfare programs. Indeed, based on information from the National Rural Employment Guarantee Scheme we show that a rise in inequality reduces the number of workers and the amount disbursed when political competition is at its sample mean. If political competition is above its mean, however, the number of workers and the amount disbursed are both higher.

In conclusion, our study makes a case for the importance of democratic accountability of local political strongmen in the context of a low income country. Without sufficient electoral pressure, politicians in a constituency with high inequality do not offer sufficient social provision for the poor. The consequences can be fatal for the most vulnerable children. Sufficient electoral pressure, however, saves lives and improves child survival.

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Appendix A Cross-country analysis

This appendix describes the data behind our cross-country analysis. Our data cover 98 low- and middle-income countries. Since many of the variables we rely on do not have annual series we collapse them into four-year binds. If a variable is collected more than once within one of these binds we use the average value. Doing this, we end up with five time periods: 1994-97, 1998-2001, 2002-05, 2006-09 and 2010-13. We now discuss the different variables and the source of information.

We extract data on infant mortality, GDP per capita (PPP adjusted) and the Gini coefficient from the World Development Indicator Database (WDI). Infant mortality, in our application, captures the share of live births that survive to the age of one year. Our proxy for political competition comes from the *Competitiveness of Participation* index from Polity IV. We construct a variable taking the value of one if the country is labeled as "competitive" and zero otherwise. The description of the competitive category is as follows: *"There are relatively stable and enduring, secular political groups which regularly compete for political influence at the national level; ruling groups and coalitions regularly, voluntarily transfer central power to competing groups. Competition among groups seldom involves coercion or disruption. Small parties or political groups may be restricted in the Competitive pattern."*

We use four measures to capture key publicly provided goods and services: the per capita public health care expenditure (PPP adjusted), the population share with improved sanitation, the population share with water facilities and the logarithm of the pupil-teacher ratio in primary schools. The sanitation and water measures are taken from the WDI database, whereas the expenditure data are extracted from the WHO Global Health Expenditure Database. The other country variables are the population share living in urban areas, a dummy variable for tropical countries (whether the country lies within 20 degrees of the equator) and a dummy for whether the country is predominantly Muslim (more than 90% of the population). The urbanization rate and the Muslim variable are taken from the WDI database, while we construct the dummy variable for tropical countries based on latitudes/longitudes from La Porta *et al.* (1999).

Table A1 provides summary statistics for all these variables. In Table A2 we present the regressions underlying Figure 1. We run the following OLS specification:

$$Y_{it} = \gamma_0 + \gamma_1 Income_{it} + \gamma_2 Inequality_{it} + \gamma_3 X_{1it} + \gamma_4 X_{2it} + T_t + \epsilon_{it} \qquad (6)$$

where Y_{it} measures the average infant mortality and life expectancy shortfall in country i during time bind t, X_{1it} includes the urbanization rate, and dummies indicating whether the country is tropical and predominately Muslim, and X_{2it} captures key publicly provided goods, namely the population shares with clean drinking water and sanitation, the government health care expenditure per capita and the teacher-pupil ratio in primary schools, T_t are time binds fixed effects, and ϵ_{it} the standard errors, clustered at the country level.

Table A1: Summary Statistics

	(1)
Key variables:	
Infant mortality rate	.0450
	(.0300)
Gini coefficient	.4230
	(.0910)
Competitive in Polity IV	.0570
	(.2320)
Average per capita income (2011 PPP adjusted)	8.349
	(.9000)
Controls for publicly provided goods and services	
Log public health expenditure per capita (2011 PPP adjusted)	4.499
	(1.180)
Share of population with improved sanitation facilities	.5810
	(.2850)
Share of population with improved water facilities	.7900
	(.1730)
Pupil-teacher ratios	3.376
	(.4080)
Other controls:	
Urbanization rate	.4600
	(.1850)
Tropical country	.5320
	(.5000)
Predominantly Muslim	.1460
	(.3530)
Number of observations	316

Table A2: Regressions behind Figure 1

	(1)	(2)
Log GDP per capita	-.0243***	-.0031
	(.0025)	(.0034)
Inequality	.0527***	.0201
	(.0167)	(.0129)
Observations	316	316
Controls for publicly provided goods	No	Yes

All columns include time binds fixed effects and control for the urbanization rate, and whether the country is tropical and predominantly Muslim. In Column (2) we also control for key publicly provided goods: the population shares with clean drinking water and sanitation, the government health care expenditure per capita and the teacher-pupil ratio in primary schools.
Standard errors, clustered at the country level, are provided in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

Appendix B Testing for political manipulation

To validate our identification, we replicate the empirical analysis of Iyer and Reddy (2013). While they focused on Rajasthan and Andhra Pradesh only, we include the 15 states of our main analysis. To do this, we match pre- and post-delimitation boundaries based on the geocoded maps. This allows us to calculate the geographical overlap between pre- and post-delimitation constituencies. We then use the Census village maps to calculate population characteristics for the overlapping areas.

We first examine the extent of redistricting. As the pre-delimitation boundaries had remained constant for three decades, the redrawing resulted in quite substantial changes: on average, about one quarter of the population was allocated to a new constituency.[13] Since the aim of the redistricting was to equalize population sizes within states, we expect to see the greatest absolute population changes in small and large constituencies. That is, we expect a U-shaped relationship between the original population size and the change in the population. Iyer and Reddy (2013) find this is the case for Andhra Pradesh and Rajasthan and we confirm the U-shaped relationship for the 15 states in our sample in Table B1. This first piece of evidence suggests that the redistricting – as a minimum – was done in the intended direction.

We next investigate whether there was political interference in the redistricting process. It is difficult to do this based on post-delimitation political outcomes, as these are likely to be affected by a myriad of other factors. Instead, we focus on factors that were likely to affect electoral prospects before the Delimitation was implemented. One such factor is political campaigning costs. Following Iyer and Reddy (2013), we construct three variables intended to capture changes in campaigning costs: i) the percentage increase in the number of eligible voters (decreases are coded as zero), ii) the fraction of old voters that remained in the constituency, and iii) whether the constituency changed

[13]The "new" constituency is the post-delimitation constituency that most of the original constituency is allocated to.

Table B1: Absolute population changes and initial population

	(1)	(2)
Eligible voters pre-delimitation	-.8072***	-.9192***
	(.1557)	(.1941)
Eligible voters pre-delimitation squared	.2478***	.2691***
	(.0517)	(.0633)
Observations	465	465
State FEs	No	Yes

As in Iyer and Reddy (2013), we control for the population share of scheduled tribes and the population share of scheduled castes before the Delimitation. Robust standard errors are shown in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

reservation status. If influential politicians were able to affect the process we would expect their constituencies to have smaller increases in population, larger shares of old voters in the new constituencies and fewer changes in reservation status.

In Table B2 we regress each of these variables on a dummy that takes the value of one if the constituency had their MP as an associate member of the Delimitation Commission. We focus on these politicians as they are most likely to be able to affect the process and are thus our prime suspects. All the coefficients are relatively close to zero, and none of them are statistically significant. This suggests that the redistricting did not create advantages in terms of improved electoral prospects for these incumbent politicians.

Table B2: Redistricting and electoral prospects (2004)

	% increase in pop (1)	Fraction of old voters (2)	Reserved for SCs (3)	Reserved for STs (4)
MP member of Delimitation Commission	-.0050	.0183	-.0070	.0160
	(.0114)	(.0210)	(.0470)	(.0217)
Observations	465	465	465	465
State FEs	Yes	Yes	Yes	Yes

All regressions include controls for the number of eligible voters and eligible voters squared, the population share of scheduled tribes (ST) and the population share of scheduled castes (SC) before the Delimitation. Robust standard errors are shown in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

Appendix C Robustness regressions

This section provides details on our robustness checks. We first test the robustness of our results to alternative measures of political competition and inequality. For political competition, these include (i) one minus the vote share of the winning party, (ii) one minus the margin of victory (MoV), and (iii) the effective number of parties (ENOP). For each of these indices, a higher level indicates more political competition. The MoV is defined by the difference in vote shares between the winner and the runner-up. The ENOP, developed by Laakso and Taagepera (1979), is measured as follows:

$$ENOP_k = \frac{1}{\sum_{c=1}^{n} s_{ck}^2},$$

where s_{ck} is the vote share of candidate c in parliamentary constituency k. The name is not accurate in our setting though, as we use the vote shares of candidates instead of parties. The measure runs from one (if one candidate got all the votes) to the maximum number of candidates that ran in the election (if each of them got exactly the same vote share). The alternative measures for inequality are (i) p90/p10, the ratio of the average income of the richest 10% to the poorest 10%, (ii) the mean log deviation, and (iii) the Theil index.

Table C1 presents summary statistics, Table C2 shows the results for political competition and Table C3 for inequality. The findings are relatively robust to the alternative measures of political competition and somewhat stronger when we use the alternative measures of inequality.

Table C1: Summary Statistics

	Level (1)	Observations (2)	Mean (3)	Std. Dev. (4)
Political competition:				
1-Win share	Constituency	447	.5245	.0884
1-MoV	Constituency	447	.8793	.1012
ENOP	Constituency	447	2.75	.7032
Income inequality:				
p90-p10 ratio	Constituency	447	3.22	.9834
Mean log deviation	Constituency	447	.1319	.0644
Theil index	Constituency	447	.1554	.0824

Data sources: The inequality measures are based on the NSS survey from 2009-2010 and the measures of political competition on data from the 2004 parliamentary election.

Table C2: Robustness regression, alternative measures of political competition

	1-Win share (1)	1-MoV (2)	ENOP (3)
Inequality	-.0005	-.0009	-.0002
	(.0016)	(.0018)	(.0015)
Political competition	.0007	.0000	.0009
	(.0015)	(.0015)	(.0017)
Inequality ×	-.0017**	-.0011	-.0018*
Political competition	(.0008)	(.0008)	(.0010)
Observations	90236	90236	90236

All regressions include pre-delimitation constituency × district × birth year fixed effects. Robust standard errors, clustered at the pre-delimitation constituency × district level, are shown in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

Our interaction term may pick up other factors that are correlated with inequality and political competition. To test whether this is the case, we run “horse race” regressions where we interact inequality and political competition

Table C3: Robustness regression, alternative measures of inequality

	p90-p10 ratio (1)	Mean log deviation (2)	Theil index (3)
Inequality	-.0019	-.0004	.0004
	(.0014)	(.0014)	(.0013)
Political competition	.0005	.0008	.0008
	(.0015)	(.0016)	(.0016)
Inequality ×	-.0020***	-.0020***	-.0021***
Political competition	(.0007)	(.0007)	(.0008)
Observations	90236	90236	90236

All regressions include pre-delimitation constituency × district × birth year fixed effects. Robust standard errors, clustered at the pre-delimitation constituency × district level, are shown in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

with several other variables. Table C4 presents the results. In Column (1), we add interactions with mean expenditures. In Column (2), we add interactions with the set of area controls, $X'_{1,dkl}$ and in Column (3) with the child characteristics, $X'_{2,idkl}$. The estimates are robust to all of these rather demanding specifications.

The final set of robustness checks are presented in Table C5. Column (1) shows the results are robust to calculating the mean expenditures and inequality using the post-delimitation boundaries. In Column (2), we add dummies for the five largest political parties in 2004, to rule out that our findings are driven by party ideology rather than political competition.[14] The regression in Column (3), controls for the reservation status of the post-delimitation constituencies (dummies for scheduled castes and scheduled tribes reservation). Our estimates are robust to all of these specifications as well.

[14]These are: INC, BJP, SP, JD(U) and BSP.

Table C4: Robustness regression, additional interactions

	(1)	(2)	(3)
Inequality	.0009	-.0026	.0008
	(.0017)	(.0229)	(.0041)
Political competition	.0006	-.0396	-.0020
	(.0016)	(.0270)	(.0049)
Inequality ×	-.0024*	-.0023**	-.0016*
Political competition	(.0014)	(.0011)	(.0009)
Observations	90236	90236	90236
Interactions with:			
Average expenditures	Yes	No	No
Area characteristics	No	Yes	No
Child characteristics	No	No	Yes

All regressions include pre-delimitation constituency × district × birth year fixed effects. Robust standard errors, clustered at the pre-delimitation constituency × district level, are shown in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

Table C5: Robustness regression, other

	Post-delimitation boundaries (1)	Party dummies (2)	Reservation status (3)
Inequality	.0001	.0000	.0000
	(.0016)	(.0016)	(.0015)
Political competition	.0006	.0011	.0009
	(.0016)	(.0016)	(.0016)
Inequality ×	-.0018*	-.0020**	-.0020**
Political competition	(.0010)	(.0010)	(.0009)
Observations	90236	90236	90236

All regressions include pre-delimitation constituency × district × birth year fixed effects. Robust standard errors, clustered at the pre-delimitation constituency × district level, are shown in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

Appendix D Additional regressions, mechanisms and discussion

We presume that the effect of inequality and political competition on child mortality is driven by changes in the supply of public health care. Table 7 in Section 6 shows the impact on three indices that measure key health care staff and services provided at Primary Health Centers (PHCs). In this section we show the impact on the binary variables that are included in the indices, as well as a placebo exercise.

Table D1 provides results for the binary variables that make up the staff index. The variables indicate whether the PHC has a doctor, a nurse and a midwife. As can be seen, the positive interaction term from Table 7 is driven by doctors and midwives. Tables D2 and D3 similarly present estimates for the binary variables included in the two service indices. Finally, Table D4 provides the results of a placebo exercise on the outcomes in Table 7. We use the District Level Household and Facility Survey from 2007-2008 for this exercise. All coefficients are close to zero and none of them are statistically significant.

Table D1: Primary Health Centers (DLHS 2012-2013), Staff

	Doctors	Nurses	Midwifes
	(1)	(2)	(3)
Inequality	.0221	-.0160	.0018
	(.0245)	(.0250)	(.0255)
Political competition	.0125	-.0027	.0106
	(.0236)	(.0232)	(.0209)
Inequality × Political competition	.0247*	-.0100	.0567***
	(.0133)	(.0152)	(.0172)
Observations	5675	5675	5675
Mean dependent variable	.781	.458	.752

All regressions include controls for average expenditure and area characteristics, as well as pre-delimitation constituency × district fixed effects. Robust standard errors, clustered at the pre-delimitation constituency × district level, are shown in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

Table D2: Primary Health Centers (DLHS 2012-2013), Services I

	Antenatal care (1)	Women reg. 1st trimester (2)	Deliveries (3)	Postnatal care (4)	Newborn care (5)
Inequality	.0066	.0001	.0143	.0204	.0278
	(.0261)	(.0224)	(.0243)	(.0251)	(.0218)
Political competition	-.0052	-.0053	.0003	-.0219	-.0013
	(.0221)	(.0250)	(.0235)	(.0235)	(.0209)
Inequality ×	.0406**	.0443***	.0128	.0318*	.0188
Political competition	(.0173)	(.0160)	(.0159)	(.0163)	(.0160)
Observations	5675	5675	5675	5674	5675
Mean dependent variable	.792	.729	.659	.736	.717

All regressions include controls for average expenditure and area characteristics, as well as pre-delimitation constituency × district fixed effects. Robust standard errors, clustered at the pre-delimitation constituency × district level, are shown in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

Table D3: Primary Health Centers (DLHS 2012-2013), Services II

	Treatment diarrhoea (1)	Treatment ARI (2)	Immun. BCG (3)	Immun. measles (4)	Immun. polio (5)	Immun. DPT (6)
Inequality	-.0350	-.0431*	.0060	-.0106	.0000	-.0159
	(.0294)	(.0244)	(.0261)	(.0281)	(.0272)	(.0281)
Political competition	.0237	.0060	-.0075	-.0275	-.0238	-.0250
	(.0292)	(.0246)	(.0254)	(.0242)	(.0238)	(.0241)
Inequality ×	-.0046	-.0053	.0454***	.0515***	.0321**	.0307*
Political competition	(.0160)	(.0142)	(.0159)	(.0170)	(.0160)	(.0167)
Observations	5675	5675	5675	5675	5675	5675
Mean dependent variable	.499	.406	.693	.661	.685	.667

All regressions include controls for average expenditure and area characteristics, as well as pre-delimitation constituency × district fixed effects. Robust standard errors, clustered at the pre-delimitation constituency × district level, are shown in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

Table D4: Placebo regressions: Demand and supply for public health care (DLHS 2007-2008)

	Household chooses public health care	Staff	Services 1	Services 2
	(1)	(2)	(3)	(4)
Inequality	.0102	-.0130	.0138	.0037
	(.0100)	(.0176)	(.0156)	(.0173)
Political competition	.0009	-.0213	-.0098	.0053
	(.0101)	(.0146)	(.0151)	(.0155)
Inequality × Political competition	-.0036	.0045	.0011	.0167
	(.0063)	(.0091)	(.0114)	(.0111)
Observations	338489	5549	5549	5549
Mean dependent variable	.441	.691	.827	.715

All regressions include controls for average expenditure and area characteristics, as well as pre-delimitation constituency × district fixed effects. The regression in Column (1) additionally controls for the religion of the household head. Robust standard errors, clustered at the pre-delimitation constituency × district level, are shown in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.

Appendix E Data construction

In this section we outline how we construct our datasets.

E.1 Census of India 2001

We make extensive use of the 2001 Census of India. We map Census villages as of 2001 into pre- and post-delimitation constituencies based on geocoded maps. In total, there are 483,072 villages in the 2001 Census for the states in our sample. The Census's 2001-2011 concordance table makes it possible to link these villages to the 2011 Census based on state, district and village codes.[15] After doing this exercise, we have a sample of 481,156 Census villages as of 2001.

E.2 Mapping NFHS households to parliamentary constituencies

We use the GPS coordinates provided by the 2015-2016 National Family and Health Survey (NFHS) to allocate villages to parliamentary constituencies. The GPS coordinates are at the level of survey clusters, which roughly correspond to gram panchayats. To maintain confidentiality, the NFHS randomly displace the GPS coordinates with a maximum of two km for urban clusters and five km for rural clusters. An additional one percent of the rural clusters is displaced with a maximum of 10 km.

We derive parliamentary constituencies for each cluster by combining the survey coordinates with the constituency maps. We then merge this with our other data based on state, district, pre- and post-constituencies. When doing this merge, we lose close to two per cent of the NFHS households. All of these have a combination of district, pre- and post-constituencies that is not found for any Census village, which indicates either displacement of the survey coor-

[15] The concordance table is available here: http://censusindia.gov.in/pca/cdb˙pca˙census/cd˙block.html. As sub-districts are not consistently coded in the concordance table, we cannot use it in the matching. We drop village codes that have duplicates within state and district. This applies for 0.0005 per cent of the villages only.

dinates or inaccuracies in the constituency map. We drop these observations from our analysis.

E.3 District Level Household and Facility Survey

In this section we discuss how we merge the District Level Household and Facility Survey (DLHS) with our dataset.

The 2012-2013 DLHS survey provides geocodes for the location of surveyed health care facilities, which enables us to map facilities into pre- and post-delimitation constituencies, and link them to the Census village map. We focus on Primary Health Centers (PHCs). Out of the 7,204 surveyed PHCs from the 15 states in our sample, we are able to link 5,704 PHCs with our other data. The remaining PHCs have severe errors in their geocodes, e.g. they are located in the wrong states.

We also make use of the 2007-2008 DLHS survey. This survey does not have geocodes, and we cannot therefore directly link PHCs to constituencies. Instead, we proceed as follows. We first match villages in the DLHS with the village directory of the 2001 Census. To do this, we follow a procedure similar to Banerjee and Sachdeva (2015) and Calvi and Mantovanelli (2018) and match villages based on state, district, sub-district and population. We drop duplicates in terms of population within sub-districts in both datasets before merging the data. Overall, we are able to unambiguously match about 90% of the DLHS villages (and the same percent of the surveyed rural households) with the Census. Using the Census identifiers we are then able to link the DLHS villages to constituencies. We next link villages to PHCs based on information in the DLHS village questionnaire. Note that a PHC is usually linked to more than one village, and these villages can potentially be located in different constituencies. We proceed as follows. For each PHC, we list all DLHS villages that are linked to it. Among these villages, we then identify the most common pre- and post-constituency and impute these constituencies to the PHC. For about 90 per cent of the PHCs, all villages belong to the same constituency. In total we are able to successfully match 5,700 out of 7,394 PHCs with our other data using this procedure.

E.4 MGNREGA

Below we describe our procedure to create a gram panchayat level dataset on MGNREGA implementation.

We extract data for the financial years of 2011-2012 to 2013-2014 from the MGNREGA Public Data Portal. This data includes names of districts, sub-districts and gram panchayats but it has no information on Census identification numbers.

We are able to use the MGNREGA data for all 15 states in our main analysis, except for Rajasthan which has gram panchayat names written in Hindi letters. We cannot therefore match this data to the Census. The part of Andhra Pradesh that was carved out to form the new state of Telangana is missing in the MGNREGA dataset as well. As the Census directories for West Bengal and Madhya Pradesh do not contain gram panchayat names, we extract those from the Local Government Directory. We then merge these with the Census before merging with the MGNREGA dataset.

We first manually make sure that we correctly match districts and as many sub-districts as possible. We are able to match 4,604 sub-districts out of a total of 4,704 (excluding Rajasthan and the missing districts in Andhra Pradesh). Within each state, district and sub-district we then conduct fuzzy matching based on gram panchayat names (after cleaning the location names). We apply the `Masala merge` procedure, developed by Asher and Novosad (2017). The matching procedure is based on the Levenshtein algorithm but is modified to better suit names in Hindi.[16] We are able to match 76.5% of the 2011 Census villages to the MGNREGA dataset (and to the 2001 Census and parliamentary constituencies). This level of matching is comparable to other researchers doing

[16]The codes for the program can be found here: http://www.dartmouth.edu/ novosad/code.html

fuzzy matching in the Indian context (Asher and Novosad, 2017; Gulzar and Pasquale, 2017).