The Political Competition over Life and Death

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Evidence from India

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Abstract

People from rich and equal societies live longer and are less likely to lose their offspring at an early age compared to people from poorer and more unequal societies – a pattern that we find across Indian states as well as across developing countries. Once we control for basic publicly provided goods, however, the impact of income and inequality vanish. In this paper we explore the indirect impact of the distribution of income on health. We suggest that the distribution of income determines the individual demand for health care and the allocation of its supply on public and private providers. As the rich may prefer more private provisions than the poor, inequality of income might generate a conflict of interests. In cases with little political competition we assert that the rich have more political influence than the poor, implying that high income inequality and little political competition should lead to bad health outcomes for poor groups as their access to health care is low.

We zoom in on infant mortality in India to explore these assertions empirically. To obtain causal estimates, we utilize the variation in political competition induced by the redistricting of electoral boundaries – the so-called Delimitation – that shifted about one quarter of the population of India to new constituencies in 2008. We present reduced for evidence for our hypothesis, in that higher income inequality that goes together with lack of competitive politics leads to higher post-neonatal mortality. We do not find any effect on neonatal mortality. As supporting evidence for our hypothesis, we show that regional pattern of households' preferences for government health care are consistent with our findings on post-neonatal mortality. We also show that our estimates can be reproduced in a cross-country setting, using a panel of about 100 developing countries.

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1 Introduction

To what extent does good health depend on income? On average, people in rich countries live longer and are much less likely to lose their offspring at an early age compared to people in poor countries (Deaton, 2013; Preston, 1975). Actually, the empirical association between income and health seems so strong that it is tempting to conclude that poor countries simply need material improvements and growth to obtain longevity and good health. But is it that simple?

The income-health relationship is unlikely to be linear, as the poor plausibly would obtain larger health gains from increased income than the rich (Deaton, 2003). Material improvements and growth that disproportionally benefit the rich may therefore do little to improve average health. Consistent with this, we document – in a sample of about 100 developing countries – a negative relationship between average health and inequality, conditional on average income. Moreover, if the distribution of income has a strong *direct* effect on longevity and health, most of the effect should remain when we account for other related variables such as health care, sanitation, water facilities and education. Once we control for such variables, however, the relationships between health on the one hand, and average income and inequality on the other, almost vanish completely. We see this in our sample of developing countries, as earlier demonstrated by Anand and Ravallion (1993). We also see it in a completely different sample of Indian states. This evidence suggest that the income-health relationship cannot be explained by material improvements alone.

In this paper we argue that a key channel, through which income and its distribution affect longevity and health, goes via the provision of health care. We suggest that the distribution of income determines the individual demand for health care and the allocation of its supply on public and private providers. As the rich may prefer more private provisions than the poor, inequality of income might generate a conflict of interests. In cases with little political competition we assert that the rich have more political influence than the poor, implying that high income inequality and little political competition should lead to bad health outcomes for poor groups as their access to health care is low. In situations where the political competition is fiercer, in contrast, the influence from rich groups over health issues becomes less pronounced and the access to public health care for all improves. Accordingly, the inequality of income might then be less associated with bad health outcomes for the poor.

To explore these assertions we conduct an empirical investigation, zooming in on infant mortality in India. It is empirical challenging to separate our hypothesis from other plausible channels linking health and the distribution of income. We cannot, for example, simply regress mortality on a measure of inequality, as such a setup would unable us to distinguish a separate effect of inequality from a potential concave relationship between income and health (see Deaton,

2003, for an excellent discussion of this). Another key empirical challenge is the endogeneity of political competition.

To tackle these challenges and to obtain causal estimates we utilize a large redistricting of electoral boundaries – the so-called Delimitation – that took place in India in 2008. Importantly for us, the redistricting was organized by an independent commission and the consensus view is that the process was carried out without great political influence (Iyer and Reddy, 2013). Still, there has been relatively little explicit empirical investigation of this. As part of our analysis, and to legitimate our identification, we therefore provide such an investigation. We test whether especially influential incumbent politicians, that were the most likely to be in a position to affect the process, experienced an *ex ante* more favourable redistricting of their constituencies, as compared to other incumbent politicians. We find no such evidence. This is consistent with findings of Iyer and Reddy (2013), who provide a similar analysis for the redistricting of State Assembly constituencies in Andhra Pradesh and Rajasthan. We therefore conclude that the boundary changes were political neutral on average, and plausibly orthogonal to factors affecting infant mortality.

Our empirical strategy seeks to exploit the variation in political competition and inequality induced by the Delimitation. In a nutshell, we regress mortality on income, inequality and political competition, using fixed effects for pre-Delimitation constituencies interacted with districts, and data solely from the period after the redistricting. Our estimates are hence identified by comparing the mortality risk of children that were shifted to new constituencies with the mortality risk of children that were not – all within the same district. Since families did not actually move, the setup can intuitively be interpreted as providing exogenous changes in the averages. A potential concern is that people that were redistricted may have influenced the level of political competition in their new constituency. To tackle this, we construct our measures of political competition based on the Parliamentary election prior to the redistricting. This election took place in April-May 2004, which was a few months before the Delimitation Commission commenced its work.

Our main data source is the National Family and Health Survey-4 (NFHS-4). From this data we construct a retrospective time series of mortality rates, which we combine with other secondary data sources, like the National Sample Survey and the Indian Census. We map all these data to election constituencies based on a number of geocoded maps. Our main outcome variables are neonatal mortality (deaths before 1 month of age) and post-neonatal mortality (deaths between 1 and 12 months of age). The motivation for separating infant mortality in these two concepts is that deaths that occur immediately after birth are likely to be caused by other factors than deaths that occur later in the first year of life. Hence, the policy efforts to reduce the two mortality concepts are potentially very different.

We find no impact of either inequality or political competition on neonatal mortality. For post-neonatal mortality, in contrast, we find a positive (i.e. harmful) effect of inequality, and that this effect is moderated by political competition. The magnitudes are relatively large. Suppose the Gini coefficient increases with 1%-point (3.8% of the sample mean), and that we evaluate this at the average level of political competition. Our estimates then imply a tiny increase in post-neonatal mortality of 0.00004%-points (0.3% of the sample mean). However, if we instead evaluate the increase in inequality at a level of political competition 1 standard deviation below the sample mean, our estimates imply a rise in post-neonatal mortality of as much as 0.045%-points (3.4% of the sample mean).

These estimates are remarkably stable when we add controls for (i) basic child and mother characteristics, (ii) population characteristics and (iii) the availability of publicly provided goods prior to the redistricting. Our findings are also robust to the use of a range of different measures of inequality and political competition. Furthermore, we test our identification through a placebo analysis. The mortality risk of infants from areas that were redistricted in 2008, born before the Delimitation, should not be affected by electoral outcomes in their (to-be) post-Delimitation constituencies. To check this, we run another set of regressions using the mortality risk of infants born during the period 2004-2008 as an outcome. All coefficients from these regressions are small and not even close to being statistically significant.

Our results can be interpreted as reduced form estimates off the effect of inequality and political competition on the provision of basic public goods that benefit the poor. We provide some auxiliary evidence for this hypothesis. First, using the same regression framework as above, we check whether factors that are likely to reduce the risk of dying during delivery are related to inequality and political competition. Consistent with the null result on neonatal mortality, we find no impact on outcomes such as place of delivery and whether health personnel were present during the time of birth. Second, we check whether households' preferences for government health care services are related to inequality and political competition. These regressions show that households that ended up in a constituency with low inequality and fierce political competition are much more likely, as compared to other households, to state a preference for government health care, and to have actually visited one in the recent past. This result is consistent with our hypothesis that inequality in combination with lack of political competition might erode the support for publicly provided health care.

As further auxiliary evidence we show that the relationships between mortality, inequality and political competition can be reproduced in our sample of 100 developing countries.[1] We also provide evidence on life expectancy that are consistent with those for infant mortality. That is: inequality is harmful for health, but only in countries characterized by weak political com-

[1] In this setting we are not able to separate between neonatal and post-neonatal mortality.

petition.[2] In this cross-country setting, we can additionally shed some light on the mechanisms by adding controls for some key public facilities. When adding these controls, the coefficients of inequality and political competition cease to be significant all together, suggesting that the association with health goes via public policies.

Our paper communicates with several strands of literature. First, it is related to the empirical literature on the effects of inequality on health (Lynch *et al.*, 2001; Judge *et al.*, 1998; Wagstaff, 2003; Wilkinson, 2002, see e.g.).[3] Second, our paper relates to the literature on the effect of democratization on health and government health care spending (see e.g. Kudamatsu, 2012; Ross, 2006). Most of this literature studies democratization and political competition at the *extensive* margin. In contrast, our paper investigates changes in political competition at the *intensive* margin in a setting where democracy already is well established. Fujiwara (2015) is another example of this, studying a large de facto enfranchisement of less educated citizens in Brazil. The paper finds positive effects on utilization of prenatal services and on newborn health.

Finally, our paper communicates with studies that – is in some way or another – use the redistricting as identification. Jensenius and Chhibber (2018) use the changes in electoral boundaries to study how politicians manipulate the allocation of public resources, focusing on the type of networks politicians are linked to. Bardhan *et al.* (2015) examine how electoral competition at higher levels affects allocation of resources to local governments and voter behavior in West Bengal, exploring the variation caused by some villages being redistricted while others were not. Nath (2014) studies electoral competition and spending across different types of public good, exploring the fact that some constituencies changed reservation status. If the reservation changed, it became harder for the incumbent to be re-elected, and in some cases, it even shut down their re-election concerns completely. Consistent with the findings in our paper, Nath (2014) concludes that politicians – in absence of electoral pressures – are more likely to spend on projects that are desired by the rich.

The rest of the paper is organized as follows. 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. Results and details on our identification strategy are presented in Section 4. Concluding remarks are given in Section 5.

[2] In this analysis, we measure political competition based on the Political Rights index from the Freedom House.

[3] Deaton (2003) provides a survey of this literature.

2 Empirical and theoretical motivation

2.1 The vanishing impact of income and inequality

To motivate our investigation we first show how average health outcomes across countries are strongly associated with average income levels and inequality. After controlling for some basic publicly provided goods, the direct impact of income and inequality is much weaker, if present at all.

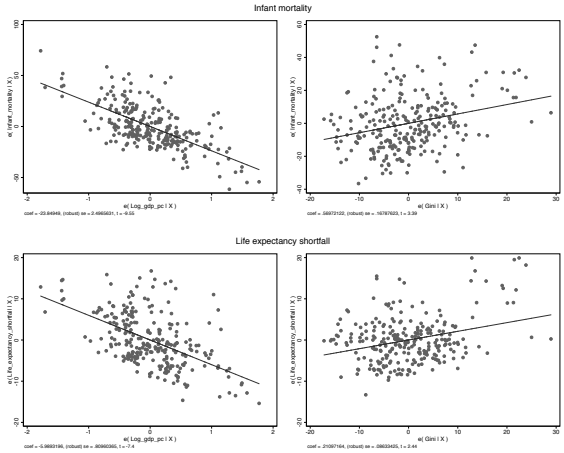
Consider the partial cross-country correlations in Figure 1. The data are from an unbalanced sample of about 100 developing countries during the period 1994 to 2013 (see appendix A for details). The figure shows that people in richer countries are likely to live longer (upper panel) and that they are much less likely to lose their children in early ages as compared to people in poorer countries (lower panel). The figure also indicates that higher inequality, once controlling for average income, is associated with lower life expectancy and higher infant mortality.

Few would claim that these patterns represent causal relationship. The impact on health from average income and inequality must necessarily go through *some* channels. One way of getting a clue of these channels is to add controls and see how the relationships change. We therefore add four variables, capturing basic publicly provided goods: the population shares with clean drinking water and sanitation, government health care expenditure per capita, and teacher-pupil ratio in government elementary schools. The latter variable should be interpreted as a proxy for the quality of government schooling. Figure 2 shows the partial correlation, similar as in Figure 1, once controlling for these variables. As can be seen from the regression line, the relationships between health, income and inequality are much weaker, if present at all.

[INDIA FIGURES HERE]

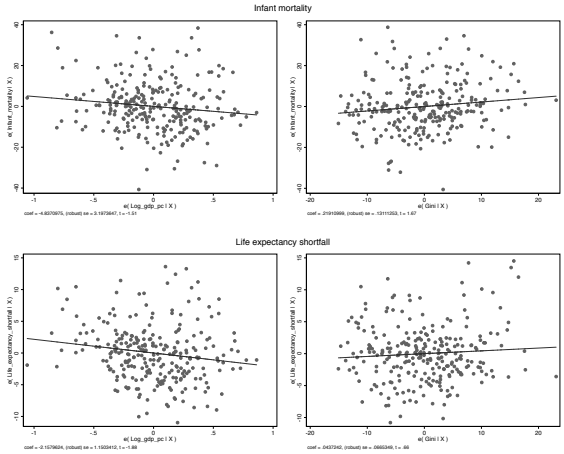
The lesson we draw from this exercise is that the link from the distribution of income to longevity and health must be indirect and that the impact of health provisions seems to be strong. This suggests that we should explore possible mechanisms that link the distribution of income to health outcomes by considering how the distribution of income affects the effective provision of health care. We are thus interested in the determination of the private and political demand for health care, assuming that the allocation of health personal are to a large extent determined by a combination of private demands and the politically articulated support for public health provisions. To set the stage we consider a simple illustrative model of private and public provision of health care.

FIGURE 1: Average health, income and inequality



Note: Partial correlations from unbalanced panel regressions, using data from 98 developing countries over the period 1994 to 2013. The variables in the regressions are log gpd per capita, the gini coefficient, 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 latter three are inspired by previous literature (see Filmer and Pritchett, 1999; Rajkumar and Swaroop, 2008).

FIGURE 2: Average health, income and inequality—with controls for basic publicly provided goods



Note: Partial correlations from unbalanced panel regressions, using data from 98 developing countries over the period 1994 to 2013. The controls for publicly provided goods are: the population shares with clean drinking water and sanitation, government health care expenditure per capita, and teacher-pupil ratio in government elementary schools.

2.2 Private and public demands for health care

We start by the individual preferences over consumption c, private health services y, and public health services z. Households may potentially have different incomes, but the same concave utility function of non-health consumption $u(c)$. The benefit to the health of the family members is measured in utilities h that depends positively on public and private health spending $h = h(z, y)$ (increasing and concave in each argument). Our assertion is that expected longevity of the members of the family is correlated with level its h. Private and public provisions can be complements or substitutes depending on what they cover. The total utility of a family is thus given by

$$v = u(c) + h(z, y) \tag{1}$$

A family that earns an income q, pays taxes tq, and spends y on private health goods. Realistically poor households with income below a threshold q_0 would not pay income taxes in a developing country. A household with $q \geq q_0$ has a disposable income $c = q - tq - y$ for private consumption of non-health goods. The taxes collected go to public health services that amount to z per capita. Public health services are financed by a balanced budget $t\bar{q} = z$.

We focus on aspects that are likely to be most empirically relevant. The first is how much private health goods y that a household acquires for given levels of public health spending. The household would then acquire more y till consumption utility costs equal the health benefits

$$v'_y = -u'(c) + h_y(z, y) \leq 0 \tag{2}$$

where $v'_y = dv/dy$ and $d^2v/dy^2 = v''_y < 0$.

A sufficiently poor family may be constrained to $v'_y < 0$ and $y = 0$ and would be completely dependent on public provision (which is the case as long as u' goes to infinity as c goes to zero while h_y is finite for $y = 0$).

Higher inequality in the distribution of income for a given mean where incomes over the mean increase and incomes below the mean decline, implies lower values of private health provision for the poor majority and higher health provision for the rich minority:

$$dy = \frac{(1-t)u''}{v''_y} dq \tag{3}$$

where $dq >$ for the rich minority and $dq < 0$ for the poor majority.

A higher public health provisions $dz > 0$ can be magnified by private access to health provision as long as the two are complements $h_{zy} > 0$ since

$$dy = \frac{h_{yz}}{v''_y} dz \tag{4}$$

Thus in this case there is a reinforcement effect of public provisions via household adjustments as long as the cross-derivative h_{yz} is positive. Higher public provisions that are topped up by higher private provisions increases the inequality in health outcomes. This case has also another downside: a declining public health provision is likely to be accompanied by local adjustment that make the situation even worse, implying that countries with low public spending have low private health spending as well – and perhaps low inequality in health outcomes.

For our questions the case where public and private provision are substitutes is of special interests. Households would then acquire less private health goods the higher the public provision. Thus in this case higher public provisions reduce the inequality in health outcomes. But as we shall see, private provisions may erode the political support for public provisions.

The ideal public health policy for a household with incomes q is determined by

$$v_z = -u'(c)\frac{q}{\bar{q}} + h_z(z, y) = 0 \tag{5}$$

together with (2). Clearly, if public and private provisions are *perfect substitutes* nobody would like a mixed system: rich households with income above the mean would prefer a system with pure private provision, while poor househlds with income below the mean would prefer a system with pure public provision. We see this directly by inserting $h_y = h_z$ in (2) and (5).

A case that might make a mixture of public and private provision attractable even for rich households is one where z and y have independent contributions to h but with different marginal rates. Public provision may cover a broad specter and thus have a less declining marginal contribution to h than private provisions that may be more specialized. One example is $h(z, y) = z + y^\alpha$ where $\alpha < 1$. In that case (2) and (5) imply that $\alpha y^{\alpha-1} = q/\bar{q}$ and thus a private demand that increases in the private income q, with an elasticity greater than on since $(dy/dq)(q/y) = 1/(1-\alpha) > 1$. Again the rich wants more private and less public health care than the poor. Higher income to the rich may actually reduce their ideal level of public provision since from (5) we obtain

$$\frac{dz}{dq} = [\theta\mu - 1]A \quad \text{where} \quad A = \frac{-v''_z u'}{\bar{q}} > 0, \quad \theta = [q - qz/\bar{q} - y/(1-\alpha)]/c \leq 1 \tag{6}$$

Here μ is the coefficient of relative risk aversion. Households who do not buy private health

services have $\theta = 1$. For richer households θ declines as their income q goes up. For sufficiently rich people we have $\mu\theta < 1$ implying that the ideal level of public provision z declines for higher incomes to the rich. The poor, in contrast, has so low incomes that they either don't pay taxes implying a high preferred level of z, or have $y = 0$ implying that their ideal public provision increases with their incomes as long as $\mu \geq 1$

No matter which of the two cases of public versus private provisions apply - whether they are perfect substitutes or independent but with different marginal impacts - our simple model has two broad predictions :

1. if average income of the country (or the state)goes up in a way that keeps relative income differences constant all groups demand more health care either in the form of public provision or in the form of private provision
2. income inequality matters for a given level of income per capita since it alters the political demand for the mixture of public and private provisions.

Combining these two results with a simple political mechanism can generate a pattern of health care that might account for some of the important differences that we observe. We assert that when political competition is fierce, it is more risky for candidates who are eager to win the election to cater for special non-majoritarian interests. Hence, when competition is fierce, we believe that the interests of the majority receive a higher weight in the decision making. When political competition is less fierce it is less risky and thus more likely to cater for the interests of the rich if that can generate other political benefits.

Consider 1) in this light we would expect that a high level of political competition can lead to a high level of h throughout the population generating a link between average income levels and longevity that we observe in average. If 2) is combined with a political situation where competition is not fierce, the interests of the rich are more likely to receive a higher weight. Hence, in this case higher inequality is likely to generate a lower average health care and may thus deteriorate the health in the population, explaining in part why citizens in states (or countries) with higher income inequality tend to live shorter.

3 Data

Our analysis is based on several data sources, which we link through a set of geocoded maps. This section describes these sources and how we construct our key variables.

3.1 Infant mortality

We use the 2015-2016 National Family and Health Survey (NFHS-4) as our data source for child mortality.[4] The NFHS interviews women aged 15 to 49 years and measures the complete birth record of these women. The survey data contains 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 mortality. The NFHS-4 interviewed about 700.000 women, which is a much larger sample than in the previous NFHS waves. This makes it possible to calculate mortality rates at a fine geographical level. Another advantage for us is that the NFHS-4 provides GPS coordinates for survey clusters. These clusters roughly coincide with Indian Gram Panchayats. We combine the cluster coordinates with geocoded maps of constituency boundaries to allocate women to constituencies.

We distinguish between neonatal and post-neonatal mortality. Neonatal mortality is defined as deaths before 1 month of age, while post-neonatal mortality is deaths between 1 and 12 months of age. The reason for distinguish between these two types of mortality, is that they are caused by different factors. Hence, the required policy efforts to reduce them are likely to be different. For both neonatal and post-neonatal, we calculate mortality using a binary variable taking the value of unit 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 be concrete, this means that the sample used to calculate post-neonatal mortality only includes children born at least 12 month before the end of the period, while the sample for neonatal mortality includes children born at least 1 month before the end of the period.

3.2 Political competition

We extract data on electoral outcomes from the The Indian National Election and Candidates Database (Jensenius and Verniers, 2017). This data set contains information on the number of votes for the most important candidates in each Parliamentary constituency, as well as the the reservation status of the constituencies. We make use of the elections results from 1999 and 2004.

Our baseline proxy for political competition is one minus the Herfindahl-Hirschman index, calculated based on votes shares for the different candidates. For constituency j, this measure can be written as:

$$1 - HH_j = 1 - \sum_{1}^{n} \gamma_c^2, \tag{7}$$

[4]This survey uses the same methodology as the Demographic and Health Surveys (DHS).

where γ_c is the vote share of candidate c. In addition, we construct three alternative measures of political competition for our robustness analysis. The first of this is one minus the vote share of the winning party. The winning vote share is informative because it shows whether a politician won overwhelmingly or not. As a second alternative measure we calculate the margin of victory, as the difference in vote shares between the winner and the runner-up (MoV). Finally, we calculate a measure of the effective number of parties (ENOP), developed by Laakso and Taagepera (1979), as:[5]

$$ENOP_j = \frac{1}{\sum_1^n \gamma_c^2}. \tag{8}$$

3.3 Village level amenities and socio-demographics

We extract data on basic population characteristics from the Socio-Demographic and Economic Census of 2001. This data cover all Indian villages and towns. We use the Infrastructure and Amenities Census to get information on the availability of publicly provided goods. This data lists the presents of a large set facilities for every Indian villages.. We construct three types of measures on the availability of public facilities. For health care, we use the share of villages with a Primary Health Center (PHC), and the share of villages with a sub-Primary Health Center (PHS). These centers are the most basic units of the government health system in India. The PHCs are supposed to have a least one trained doctor, and act as a referral unit for the smaller PHSs. For access to schooling, we compute the share of villages with primary schools and middle schools. Lastly, we calculate similar measures for other types of publicly provided facilities: The share of villages with electricity, the share of villages with tap water and the share of villages with paved roads.

3.4 Expenditure and inequality

Data on household expenditure, which we use to calculate mean expenditure 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 mainly make use of the 2009-10 (66th round) survey, but use the 2004-2005 (61st round) survey for our placebo analysis.

Our analysis requires estimates of mean expenditure and inequality at the level of election constituencies. However, the NSS data does not include identifiers for constituency, nor does it provide geocodes for where households are located. The finest geographical unit which we could identify is the Indian districts. Sometimes these districts perfectly coincide with constituencies,

[5]The name effective *parties* is not accurate in this setting, since we are using the vote shares of candidates, not parties.

but more often they do not. Another challenge is that some district boundaries changed during our study period. We tackle this as follows. Based on the geocoded maps of election constituency and 2001 Census villages, we first calculate the population share of each district in the different constituencies. These shares can be interpreted as probabilities of households reside in the different constituencies, conditional on which district they are from. We then 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, such that this adjustment is unproblematic. Finally, we calculate mean expenditure and inequality for each constituency, by weighting households by their probability of residing in the particular constituency.[6]

3.5 Sample selection and summary statistics

We limit our main analysis to children born in rural areas of India. There are two reasons for this. First, our identification assumption is not likely to hold for urban areas, as cities are much less likely to be shifted to new constituencies than rural areas. Second, the Census amenity data only cover rural India, and we are therefore not able to control for background characteristics of urban areas. Note, however, that political competition, average expenditure and inequality are measured at the constituency level, which include urban areas. We also restrict the analysis to the 17 largest Indian states. These states account for most of the rural population in India.[7] A few election constituencies from these states are all urban, and they are also excluded.[8] In total, we end up with a sample of 491 pre-Delimitation election constituencies. In our main specifications we exclude the six months before and after parliamentary elections, and focus on the time period 2009–2013.

A challenge of using retrospective data is that the women in the sample might have moved since they gave birth. The NFHS data include some questions on migration. In particular, it asks how long the respondents have resided in their current location. In our main analysis we exclude infants for which the mother have moved after their birth. We also exclude mothers that state that they are visitors in the location they were interview.

[6] This therefore assumes that expenditure is uniformly distributed across space within districts.

[7] According to the 2011 Census, they account for about 97 percent of the total rural population.

[8] Pune, Mumbai South, Mumbai North-West, Mumbai North-East, Mumbai East, Mumbai Central and Mumbai South-Central from Maharashtra; Madras Central from Tamil Nadu; Howrah, Dum Dum, Calcutta North-East and Calcutta North-West from West Bengal.

4 Institutional background

4.1 Administrative and electoral levels in India

India has four administrative levels (states, districts, subdistricts 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). Note that the electoral areas do not overlap perfectly with the administrative areas at all levels. The national and state level bodies are responsible for legislating, while much of development planning is done at lower levels of government. For example, the district council is responsible for much of public goods provision at the local level.

Our analysis will eventually make use of electoral competition at the Parliamentary level. These measures are likely to be relevant also at the local level, due to the role played by the Members of Parliament (MPs). First, the MPs are ex-officio members of the council of the districts that overlap with their constituencies. This means that they can sit in on meetings and potentially affect policies at the local level. Second, the MPs receive a yearly budget for infrastructure goods to be spent within their constituencies.

4.2 The 2008 Delimitation

The boundaries of both national and state electoral constituencies in India were redrawn in 2008, based on the so-called Delimitation Act. This was the first boundary change in three decades. The process of redrawing the map started in 2002, 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 *within* Indian states. The redrawing had two main goals: i) to equalize population sizes across constituencies, and ii) to reserve constituencies for Scheduled Castes (SC) and Scheduled Tribes (ST) in proportion to updated measures of their population shares.

Many countries go through similar redistricting processes on a regular basis, and sometimes influential politicians, often incumbents, are accused of tweaking the redistrict to create safer seats for themselves. This is often referred to as gerrymandering. Importantly for our purpose, the redistricting in India was organized by an independent commission and the consensus view is that the exercise was carried out without great influence from incumbent politicians. Still, there has been relatively little empirical analysis of the Delimitation. We cannot really know therefore what role politicians played in the process. Some more details are warranted. In each state, five elected representatives from the national parliament (MPs) and five from the

state legislature (MLAs) were selected as associate members of the Delimitation Commission in their own state. These representatives had no formal voting power, but were closely involved and asked to advice the Commission. After examining maps and population characteristics, the Commission published a preliminary draft of new electoral boundaries. This draft was distributed widely and publicly commented. After this process, the Commission submitted their final report, which was approved in August 2008.

4.3 Testing for political manipulation

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". To validate our identification, we carry out a similar empirical analysis for *all* states in our sample. To do this, we match pre- and post-Delimitation boundaries based on the geocoded maps. This able us to calculate the geographical overlap of pre- and post-constituencies. We then use the Census village maps to fill these overlaps with population characteristics.

We first examine the extent of redistrict. Since the post-Delimitation boundaries had remained constant for three decades, the redrawing resulted in quite substantial changes: On average, about one quarter of the population was shifted from their original constituency to a new one. What we refer to as the original constituency is the pre-Delimitation constituency with the largest population share of the post-Delimitation constituency. Table 1 displays this statistics by states. Since the aim of the redistricting was to equalize population sizes within states, we would expect to see the greatest absolute population changes in the small and the large constituencies. That is, we expect a U-shaped relationship between original population size and change in population. That is what Iyer and Reddy (2013) found for Andhra Pradesh and Rajasthan. We test this formally for our sample in the regressions reported in Table 2. The regression estimates confirm the U-shaped relationship. This first piece of evidence suggests that the redistricting—as a minimum—was done in the intended direction.

We next investigate potential political interference in the redistricting process. It is difficult to do this based on post-Delimitation political outcomes, which are likely to be affected by a myriad of other factors. Instead, we focus on factors that ex-ante were likely to affect electoral prospects. 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 voters in the new constituency that were part of the original constituency, and iii) whether the constituency changed reservation status. If influential politicians were able to affect the process to achieve political gains 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 1: Re-districting and population size

	Average no of electors (in mill.)	No of PCs	Share of electors changing PC
	(1)	(2)	(3)
Andhra Pradesh	1.81	42	0.27
Assam	1.90	14	0.06
Bihar	2.07	40	0.29
Chhattisgarh	1.89	11	0.15
Gujarat	1.95	26	0.20
Haryana	2.11	10	0.17
Jharkhand	1.92	14	0.06
Karnataka	1.89	28	0.16
Kerala	1.59	20	0.33
Madhya Pradesh	2.08	29	0.15
Maharashtra	2.20	44	0.28
Orissa	1.74	21	0.17
Punjab	1.86	13	0.29
Rajasthan	2.26	25	0.27
Tamil Nadu	1.60	39	0.28
Uttar Pradesh	2.08	80	0.30
West Bengal	1.92	41	0.24
All states	1.96	497	0.24

Note: Excluded constituencies: 6 from Maharashtra: Pune, Mumbai South, Mumbai North-West, Mumbai North-East, Mumbai East, Mumbai Central and Mumbai South-Central. 1 from Tamil Nadu: Madras Central. 4 from West Bengal: Howrah, Dum Dum, Calcutta North-East and Calcutta North-West.

addition to these measures, we also construct a direct proxy for the expected change in vote share for the incumbent party. We compute this variable by averaging the vote shares from the 2004 election, weighting constituencies according to their shares in the post-Delimitation constituency. This thus makes the rather strong assumption the vote shares were uniformly distributed within the pre-Delimitation constituencies.[9]

In Table 3 we regress the above variables on a binary variable, taking the value of unity if the constituency had their MP as an associate member of the Delimitation Commission. These politicians are the ones most likely to be able to affect the process, and they are therefore our prime suspects. However, all of the coefficients in Table 3 are relatively close to zero, and none of them are significantly different from zero. This suggests that the redistricting did not create any large advantages for these incumbent politicians, nor did it significantly change their electoral prospects.

TABLE 2: Absolute population changes and initial population

	(1)	(2)
Eligible voters Pre-Delim	-0.954***	-1.077***
	(0.261)	(0.301)
Eligible voters Pre-Delim Squared	0.287***	0.309***
	(0.0805)	(0.0916)
Population share ST Pre-Delim	-0.0952**	-0.134**
	(0.0470)	(0.0562)
Population share SC Pre-Delim	-0.217***	-0.143
	(0.0775)	(0.103)
Observations	497	497
R^2	0.208	0.355
State FEs	no	yes

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are shown in the parentheses.

[9] An alternative would be to use vote shares from State Assembly elections. As of yet, we do not have access to geocode maps of these boundaries. We are therefore not able to map State Assembly constituencies to general election constituencies.

TABLE 3: Re-districting and electoral prospects (2004)

	Fraction old voters in the new PC (1)	**% increase in PC population** (2)	**Reserved for SCs** (3)	**Reserved for STs** (4)	**Expected change in vote share** (5)
MP member of	-0.00279	0.0469	-0.0240	0.0234	-1.362
Delimitation Commission	(0.0115)	(0.0420)	(0.0422)	(0.0215)	(0.888)
Observations	497	497	497	497	497
R^2	0.541	0.160	0.283	0.691	0.171
State FEs	yes	yes	yes	yes	yes

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are shown in the parentheses. Controls include the number of eligible voters and eligible voters squared, the population share of STs and the population share of SCs.

5 Empirical setup and identification

We seek to to exploit the boundary changes to identify casual estimates on the relationship between mortality, inequality and political competition. Our basic approach is to use fixed effects to group families who reside in the same district and pre-Delimitation election constituency. We include districts in the fixed effects, since this is an important administrative unit for the provision of public goods (see Section 4). Within these geographical units, we then compare the mortality risk of infants that were shifted to new constituencies—with potential different levels of inequality and political competition—with the mortality risk of infants that were not. Note that the district boundaries remained unchanged, such that all families within our fixed effects continued to belong to the same district also after the Delimitation. Hence, we are only exploiting the variation arising from the fact that families—without having to move—all the sudden belonged to different constituencies. The key identification assumption is therefore that these changes in inequality and political competition are unrelated to factors affecting infants' mortality risk.

5.1 Specification

We use the following baseline specification, where we regress the binary variable m, capturing whether or not infant i died during a given time period after birth, on average income q and inequality v at the post-Delimitation constituency level, as well as political competition z and its interaction with inequality:

$$m_{idjk} = \beta_0 + \beta_1 q_k + \beta_2 v_k + \beta_3 z_k + \beta_4 (v_k * z_k) + \theta'_{dj}\gamma_1 + \epsilon_{idjk}. \tag{9}$$

The subscript d denotes districts, j denotes pre-Delimitation constituencies, k denotes post-Delimitation constituencies, while θ'_{dj} denotes our fixed effects. The standard errors are always clustered at the pre-Delimitation constituency level and robust to heteroskedasticity. To check that the estimated coefficients are not driven by other observable factors, we add three types of controls. The first set of controls, $X'_{1,djk}$, include variables at the child level, such as gender

of the child and age of the mother at birth. We then add a set of demographic and socio-economic controls, $X'_{2,djk}$, aggregated over the group of people that ends up in the same post-Delimitation constituency, and that are located within the same fixed effect. Finally, we add a set of controls, $X'_{3,idjk}$, to capture the prevalence of publicly provided amenities prior to the redistricting, aggregated over the same set of people. The full list of controls are shown in Table X.

$$m_{idjk} = \beta_0 + \beta_1 q_k + \beta_2 v_k + \beta_3 z_k + \beta_4 (v_k * z_k) + \theta'_{dj}\gamma_1 + X'_{1,djk}\gamma_2 + X'_{2,djk}\gamma_3 + X'_{3,idjk}\gamma_4 + \epsilon_{idjk}. \quad (10)$$

The composition of people in the post-Delimitation constituencies is likely to itself affect the level of political competition. To avoid the potential endogeneity arising from this, we construct our measures of political competition based on election outcomes from the last Parliamentary election prior to the redistrict. This election took place in April-May 2004. As the Delimitation Commission started its work in July the same year, this election should not have been directly affected by the subsequent redistricting.

5.2 Assessing the fixed effect model

Our identification assumption is that the changes in inequality and political competition due to the redistricting are unrelated to factors affecting infant mortality once we include pre-Delimitation interacted with district fixed effects. To investigate whether our data is consistent with this, we first look for possible differences in observables between the group of people being moved to new constituencies and those that remained in their original ones. Table 4 displays a set of regressions, where the the listed variables are the dependent variable, while a binary variable for individuals changing constituency is the only independent variable. The table shows that those changing constituency and the rest differ to a very limited extent on these observables.

6 Empirical evidence

6.1 Main results

Table 5 presents our main estimates based on Equation (9) and (10). The regressions in Column (1) and (2) do not include any additional controls. In Column (3) to (5) we subsequently add individual controls, socio-demographic population controls and controls for the availability of publicly provided goods in 2001.

TABLE 4: Balancing regression

	Constant	Changing PC
	(1)	(2)
Mortality rates, 2009-2013		
Postneonatal	0.0127	0.0021
Neonatal	0.0341	-0.0008
Expenditure and inequality 2009-10		
Average expenditure per capita (rupees)	973.8488	-28.5111*
Gini coefficient	0.2588	0.0002
Theil index	0.1337	0.0040
p90/p10	3.0153	-0.0981
Mean log deviation	0.1154	0.0002
Gini land	0.6754	-0.0011
Political competition 2004		
1- Herfindahl-Hirschman vote share index (HH)	0.6409	0.0031
1- Winning vote share	0.5447	0.0023
1- Margin of victory (MoV)	0.8846	0.0037
Effective number of parties (ENOP)	2.9515	0.0199
Village amenities 2001 (share of villages)		
Primary school	0.8186	-0.0053
Middle school	0.2928	-0.0065
Primary Health Centre	0.0413	0.0026
Sub-Primary Health Centre	0.1369	0.0022
Maternity and child welfare centre	0.0545	-0.0057*
Tap water	0.2784	-0.0020
Electricity	0.3792	-0.0078
Paved road	0.5324	-0.0002
Population characteristics 2001 (share of population)		
Scheduled caste (SC)	0.1694	0.0002
Scheduled tribes (ST)	0.1421	-0.0002
Below 6 years	0.1846	-0.0003
Literates	0.4437	-0.0038
Total workers	0.4137	0.0037
Agricultural labourers	0.0730	0.0004

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table shows regression estimates from a specification with pre-Delimitation constituency interacted with district fixed effects, and the listed variables as dependent variables and a binary variable for individuals changing constituency as the only independent variable.

For neonatal mortality, we find essentially no impact of any of our variables of interest. In the following we therefore focus on the estimates for postneonatal mortality (Panel A). As can be seen from the first column, we find no relationship between postneonatal mortality risk and inequality on average. Once we interact inequality with political competition, however, the estimated coefficients turn significant and stay remarkably stable when we start adding controls. The magnitudes are relatively large. Suppose the gini coefficient increases with 1%-point. Evaluated at the sample mean of political competition, the estimates imply a small increase in postneonatal mortality of 0.00004%-points (0.3% of the sample mean). However, if the level of political competition instead is 1 standard deviation below the sample mean, the estimates imply a rise in postneonatal mortality of 0.045%-points (3.4% of the sample mean).

6.2 Robustness analysis

In this section we explore the robustness of our baseline estimates. In Table 6 we make use of our three alternative measures of political competition. Our estimates are relatively robust to the use of these measures. In Table 7 we replace the Gini coefficient as a measure of inequality with three alternative measures, in addition to the Gini coefficient of land holdings. Our findings survive, and are even somewhat stronger, when using the Theil index, the p90/p10 or the mean

TABLE 5: Baseline regression

	(1)	(2)	(3)	(4)	(5)
Panel A. Postneonatal					
Mean expenditure	-0.00000638	-0.00000507	-0.00000433	-0.00000416	-0.00000454
	(0.0000118)	(0.0000106)	(0.0000106)	(0.0000109)	(0.0000104)
Inequality	0.00622	0.328*	0.322*	0.324*	0.330*
	(0.0313)	(0.175)	(0.172)	(0.171)	(0.168)
Political competition		0.138*	0.135*	0.138**	0.140**
		(0.0734)	(0.0706)	(0.0700)	(0.0693)
Inequality * Political competition		-0.505*	-0.501*	-0.504**	-0.509**
		(0.262)	(0.256)	(0.251)	(0.249)
Observations	83890	83890	83890	83885	83885
Panel B. Neonatal					
Mean expenditure	-0.0000110	-0.0000113	-0.00000960	-0.00000855	-0.0000114
	(0.0000113)	(0.0000113)	(0.0000117)	(0.0000117)	(0.0000114)
Inequality	-0.0113	-0.0446	-0.0727	-0.0628	-0.0735
	(0.0446)	(0.226)	(0.233)	(0.231)	(0.232)
Political competition		0.00397	-0.00745	-0.00173	-0.00623
		(0.101)	(0.106)	(0.104)	(0.106)
Inequality * Political competition		0.0525	0.0919	0.0701	0.0903
		(0.356)	(0.369)	(0.366)	(0.368)
Observations	111025	111025	111025	111016	111016
Child characteristics	no	no	yes	yes	yes
Population characteristics	no	no	no	yes	yes
Village amenities	no	no	no	no	yes

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at 491 Pre-constituencies are shown in the parentheses. All controls are at the pre-PC/post-PC/district-level.

log deviation as measures of inequality. The signs of the coefficients are similar for inequality of land, but the relationships are weaker and not significantly different from zero.

Table 8 displays three other robustness checks. In the regression in the first column we include the six months period before and after elections, which are excluded in our main analysis. This brings the coefficients somewhat closer to zero. As a second robustness we calculate mean expenditure and inequality using the pre-Delimitation boundaries. This tackles the potential worry that our estimates are driven by the fact that the population being moved to some extent (mechanically) affect mean expenditure and inequality in their new constituency. Our estimates are robustness to this alternative calculation. In the third column we add dummies for the five largest political parties in 2004, to check whether our findings are driven by party ideology rather than political competition. [10] Our findings are not sensitive to the inclusion of these dummies.

6.3 Placebo analysis

In Table 9 we test our identification strategy through a placebo analysis. The mortality risk of children from areas that were redistricted in 2008, born before the Delimitation, should not be affected by electoral outcomes in their (to-be) post-Delimitation constituencies. To check this, we run a set of regressions similar to those above, but using the mortality risk of infants born

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

TABLE 6: Robustness regression, alternative measures of political competition

	1-Win share	**1-MoV**	**ENOP**
	(1)	(2)	(3)
Mean expenditure	-0.00000359	-0.00000610	-0.00000337
	(0.0000102)	(0.0000108)	(0.0000102)
Inequality	0.261**	0.283*	0.169
	(0.122)	(0.165)	(0.104)
Political competition	0.132**	0.0855	0.0159*
	(0.0619)	(0.0563)	(0.00883)
Inequality * Political competition	-0.486**	-0.322*	-0.0570*
	(0.217)	(0.187)	(0.0330)
Observations	83885	83885	83885
Child characteristics	yes	yes	yes
Population characteristics	yes	yes	yes
Village amenities	yes	yes	yes

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at 491 Pre-constituencies are shown in the parentheses.

TABLE 7: Robustness regression, alternative measures of inequality

	Theil	**p90/p10**	**Mean log dev.**	**Gini Land**
	(1)	(2)	(3)	(4)
Mean expenditure	-0.00000633	0.00000349	-0.00000289	-0.00000676
	(0.0000106)	(0.00000997)	(0.0000109)	(0.00000877)
Inequality	0.275**	0.0239**	0.329**	0.147
	(0.128)	(0.00994)	(0.158)	(0.121)
Political competition	0.0636*	0.135**	0.0680*	0.153
	(0.0339)	(0.0533)	(0.0353)	(0.132)
Inequality * Political competition	-0.401**	-0.0434***	-0.511**	-0.208
	(0.178)	(0.0161)	(0.230)	(0.182)
Observations	83885	83885	83885	83885
Child characteristics	yes	yes	yes	yes
Population characteristics	yes	yes	yes	yes
Village amenities	yes	yes	yes	yes

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at 491 Pre-constituencies are shown in the parentheses.

TABLE 8: Robustness regression, other

	Incl. election periods	**Pre-Delim boundaries**	**Party dummies**
	(1)	(2)	
Mean expenditure	-0.00000737	-0.00000644	-0.00000805
	(0.00000797)	(0.00000888)	(0.0000104)
Inequality	0.234*	0.304**	0.353**
	(0.120)	(0.142)	(0.176)
Political competition	0.0995**	0.127*	0.143**
	(0.0498)	(0.0670)	(0.0707)
Inequality * Political competition	-0.349*	-0.455*	-0.533**
	(0.178)	(0.231)	(0.262)
Observations	111943	83885	83885
Child characteristics	yes	yes	yes
Population characteristics	yes	yes	yes
Village amenities	yes	yes	yes

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at 491 Pre-constituencies are shown in the parentheses.

during the period 2004-2008 on the left-hand side. The regression in Column (1) uses the exact same variables as our baseline regression from Table 5. In Column (2) we make use of the NSS survey from 2004-05 to calculate mean expenditure and inequality. In Column (3) we use lagged values of political competition, using the outcomes from the 1999 Parliamentary election. All coefficients are small and not even close to being statistically significant.

TABLE 9: Placebo regression, using birth in the period 2004-2008

	(1)	(2)	(3)
Mean expenditure	0.00000506	0.0000116	0.00000710
	(0.0000114)	(0.0000179)	(0.0000177)
Inequality	0.0567	-0.0926	-0.132
	(0.166)	(0.200)	(0.202)
Political competition	0.0229	-0.0169	-0.0638
	(0.0670)	(0.0772)	(0.0828)
Inequality * Political competition	-0.0880	0.0810	0.163
	(0.263)	(0.312)	(0.326)
Observations	85933	85933	82660
NSS survey year	2009-10	2004-05	2004-05
Election year	2004	2004	1999
Child characteristics	yes	yes	yes
Population characteristics	yes	yes	yes
Village amenities	yes	yes	yes

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at 491 Pre-constituencies are shown in the parentheses.

6.4 Auxiliary evidence

We interpret our findings as reduced form estimates on the effect of inequality and political competition on the provision of basic public goods that benefit the poor. In this section we provide some auxiliary evidence for this hypothesis.

The NFHS include some questions related to the period before and after birth, for children born i 2011 or later. Since our main study period starts in 2009, we are not able to include the full set of children from our main analysis.[11] Still, in Table 10 we explore some outcomes related to delivery and the period after–using a subsample of children born in the period 2011 to 2013. The regressions reported in the table use the following binary outcome variables: whether delivery took place at a government health facility, whether a doctor assisted the delivery, and whether a midwife assisted the delivery. This is outcomes that we expect would reduce the risk of dying during delivery. The fact that we find no relationships between these outcomes and our variables of interest is consistent with our previous estimates, showing no relationship between inequality, political competition and neonatal mortality.

To provide further auxiliary evidence for our hypothesis, we make use of some survey questions related to health care visits. These questions are not ideal for our purpose, since they are asked with reference to the time of the survey, and we cannot therefore create a time series. The first column in Table 10 uses a binary variable on the left-hand side that takes the value of unity if

[11] In addition, many questions are only ask for the last birth.

the respondent answered a government institution on the following questions: *When members of your household get sick, where do they generally go for treatment?* The coefficients of inequality and political competition go in the opposite directions as in our postneonatal regressions. Hence, these patterns are consistent with our hypothesis that inequality in combination with lack of political competition might erode the support for publicly provided health care.

The regressions in Column (2) and (3) further explore households' preferences for government health care. The outcome variable in the regression in Column (2) is a binary variable capturing whether the respondent, or any of its children, visited a government health care facility during the three months before the time of survey. The patterns are similar as in the first column. In Column (3), we use the same use outcome variable as in Column (2) but condition on the respondent having went a health care facility. This conditioning substantially reduces the sample. Still, the estimated coefficients have the same sign, and the interaction term is significant at 10% significancy level.

TABLE 10: Delivery – child-level

	Delivery at a public health facility	**A doctor assisted the delivery**	**A midwife assisted the delivery**
	(1)	(2)	(3)
Mean expenditure	-0.000106*	-0.00000486	-0.0000171
	(0.0000539)	(0.0000530)	(0.0000629)
Inequality	1.076	-1.094	-0.356
	(0.789)	(1.070)	(1.005)
Political competition	0.412	-0.235	-0.302
	(0.349)	(0.431)	(0.396)
Inequality * Political competition	-1.359	1.732	0.812
	(1.248)	(1.719)	(1.601)
Observations	84356	84356	84356
Mean dep.var	0.54	0.43	0.53
Child characteristics	yes	yes	yes
Population characteristics	yes	yes	yes
Village amenities	yes	yes	yes

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at 491 Pre-constituencies are shown in the parentheses.

TABLE 11: Preferred health care facility, and actual visits to government facilities – mother-level

	Household members generally go to a public health facility for treatment	**Actual visits to public health facilities last 3 months**	**Actual visits to public health facilities last 3 months, conditional on any visits**
	(1)	(2)	(3)
Mean expenditure	-0.0000140	-0.0000556*	-0.0000646
	(0.0000514)	(0.0000320)	(0.0000810)
Inequality	-1.702***	-0.944*	-2.325
	(0.646)	(0.562)	(1.432)
Political competition	-0.638**	-0.373*	-1.003
	(0.280)	(0.210)	(0.615)
Inequality * Political competition	2.569**	1.643**	3.775*
	(1.042)	(0.816)	(2.113)
Observations	82620	69315	18277
Mean dep.var	0.43	0.12	0.47
Mother characteristics	yes	yes	yes
Population characteristics	yes	yes	yes
Village amenities	yes	yes	yes

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at 491 Pre-constituencies are shown in the parentheses.

7 Conclusion

In this paper we argue that the distribution of income determines the individual demand for health care and the allocation of its supply on public and private providers. As the rich may prefer more private provisions than the poor, inequality of income might generate a conflict of interests. In cases with little political competition this may give the rich more political influence, implying that high income inequality and little political competition could lead to bad health outcomes for poor groups as their access to health care is low. In situations where the political competition is fiercer, in contrast, the influence from rich groups over health issues becomes less pronounced and the access to public health care for all improves. Accordingly, the inequality of income might then be less associated with bad health outcomes for the poor.

We provide evidence for this assertions using micro data and a large redistricting of electoral boundaries in India.

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Appendix A Cross-country analysis

The appendix describes the data behind Figure 1 and 2 in the main paper. It also provides cross-country regressions, similar to the cross-constituency regression presented in Section **??**.

A.1 Data and variables

Our data cover in total 98 countries, all developing countries broadly defined. Since many of the variables we rely on do not have annual series we collapse them into four-years 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, 1998-01, 2002-05, 2006-2009, 2010-13.[12]

Health outcomes. We extract data on infant mortality and life expectancy from the World Development Indicator Database. Following Anand and Ravallion (1993) we express life expectancy in terms of shortfall and not attainments. We use a life expectancy norm of 80, such that the shortfall is $80-$*life expectancy*.[13] Attainments and shortfalls are clearly related, but the shortfall measure brings out more clearly the difficulty of what has already been accomplished.[14] To arrive at estimates that are comparable to the previous literature we do not transform the infant mortality rates in the same way, but note that our findings hold when we apply a suitable transformation to reflect shortfalls.[15]

Public goods provision. We use three measures to capture public health care provision broadly defined: the per capita public health care expenditure (PPP adjusted), the population share with improved sanitation and the population share with water facilities. The sanitation and water measures are taken from the World Development Indicator database, whereas the expenditure data are extracted from the WHO Global Health Expenditure Database. Previous literature have shown that education attainments are important for health. As a proxy for schooling quality, and as an indirect measure of learning outcomes and literacy, we also include the logarithm of the pupil-teacher ratio in primary schools.

Political competition. Our proxy for political competition comes from the *Political Rights* index

[12] Our main findings do not change if we use 3 or 5 year binds.

[13] The highest observed value of life expectancy in our sample is 79.

[14] A simple example may clarify. Often we measure progress in percentage changes. A 5-year increase in life expectancy from 70 years to 75 years is a 7% rise, whereas a 5 year-increase from 40 years to 45 years is almost a 13% increase. The latter achievement thus appear much more substantial than the former. The shortfall measures better captures that it is harder to keep improving life expectancy. Using the norm of 80 years, the rise from 70 years to 75 years halves the shortfall, but only closes it by 12.5% in the case where life expectancy increases from 40 years to 45 years (see also UNDP, 1990, p. 13-14).

[15] The difference between attainment and shortfall is naturally much smaller for the mortality rates than for life expectancy, since low numbers—and thus large percentage changes for a given absolute change—are associated with high attainments

from Freedom House. We classify countries as politically competitive if they obtain the top rank in this index.[16]

Average income and inequality. We use GDP per capita (PPP adjusted) as our main measure of income. The advantage of using GDP, as compared to expenditure or income from household surveys, is that the measurement is relatively standardized and that we have data for many countries and time periods. Our measure of income inequality is the Gini coefficient. We extract all variables on income and inequality from the World Development Indicator database.

Other controls. The other controls we use are inspired by previous literature (see Filmer and Pritchett, 1999; Rajkumar and Swaroop, 2008). We include a variable capturing 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).

TABLE 12: Summary statistics

	mean	sd
Health outcomes		
Infant mortality rate	0.043	0.029
Shortfall in life expectancy	14.654	8.571
Income and inequality		
Average per capita income (2011 PPP adjusted)	8.402	0.900
Gini coefficient	0.420	0.089
Political competition (shares)		
Top rank in Freedom House	0.088	0.284
Health care		
Public health expenditure per capita (2011 PPP adjusted)	4.587	1.166
Share of population with improved sanitation facilities	0.799	0.169
Share of population with improved water facilities	0.595	0.283
Education		
Pupil-teacher ratios	3.358	0.404
Basic controls (shares)		
Urbanization rate	0.149	0.357
Tropical country	0.467	0.184
Dominantly Muslim	0.483	0.501

A.2 Cross-country correlations

Table 13 presents the regressions behind Figure 1 and 2 in the main paper. In Table 14 we go beyond this and estimate a specification corresponding to Equation (10) in the main paper. Infant mortality and life expectancy shortfall are both all strongly correlated with average income and inequality, as measured by the Gini coefficient. As in our main empirical application, we also find that political competition significantly moderates the harmfull impact of inequality, see Column (2) and (5). In Column (3) and (4) we include a four controls for publicly provided goods.

[16]Countries get the highest score in the Freedom House index if they: *enjoy a wide range of political rights, including free and fair elections. Candidates who are elected actually rule, political parties are competitive, the opposition plays an important role and enjoys real power, and minority groups have reasonable self-government or can participate in the government through informal consensus.*

Doing this, we can see that the interaction term between inequality and political competition ceases to be significant for both outcome. This suggests that the difference in how inequality affects health in countries with and without political competition stems from public policy.

TABLE 13: Cross-country regressions: Health, income and inequality

	Infant mortality		**Life expectancy shortfall**	
	(1)	(2)	(3)	(4)
Log GDP per capita	-0.0238***	-0.00484	-5.989***	-2.158*
	(0.00250)	(0.00320)	(0.810)	(1.150)
Gini	0.0570***	0.0219*	21.10**	4.372
	(0.0168)	(0.0131)	(8.633)	(6.653)
Observations	260	260	260	260
R^2	0.693	0.820	0.595	0.738
Controls for publicly provided goods	no	yes	no	yes

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at 98 countries are shown in the parentheses. All regressions include time fixed effects and the following basic controls: the urbanization rate, a dummy for tropical countries and a dummy for predominately Muslim countries.

TABLE 14: Cross-country regressions: Health, income, inequality and political competition

	Infant mortality			**Life expectancy shortfall**		
	(1)	(2)	(3)	(4)	(5)	(6)
Log GDP per capita	-0.0237***	-0.0233***	-0.00522	-5.936***	-5.791***	-2.458**
	(0.00251)	(0.00249)	(0.00328)	(0.815)	(0.808)	(1.112)
Gini	0.0559***	0.0612***	0.0200	20.69**	22.85***	4.250
	(0.0168)	(0.0166)	(0.0133)	(8.579)	(8.633)	(6.776)
Political competition	-0.00435	0.0580***	-0.00306	-1.613	23.63***	5.255
	(0.00489)	(0.0156)	(0.0174)	(2.167)	(6.476)	(5.599)
Gini∗Political competition		-0.00151***	-0.0000424		-0.612***	-0.190
		(0.000348)	(0.000411)		(0.151)	(0.137)
Observations	260	260	260	260	260	260
R^2	0.695	0.705	0.822	0.597	0.616	0.746
Controls for publicly provided goods	no	no	yes	no	no	yes

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at 98 countries are shown in the parentheses. All regressions include time fixed effects and the following basic controls: the urbanization rate, a dummy for tropical countries and a dummy for predominately Muslim countries.