Weighing in on Service Delivery: Regression Discontinuity Evidence from India

Subarna Banerjee*

Sebastian Vollmer†

Abstract

This paper examines how legislative redistricting shapes the delivery of public services. We study India's 2008 delimitation, which redrew state assembly constituencies and shifted the number of seats allocated to districts after a three-decade freeze. Using pre-delimitation malapportionment as a running variable, we implement a regression discontinuity design to estimate the reduced-form effect of gaining legislative weight on health service delivery in rural India. Districts just above cutoff, those marginally gaining legislative weight, expanded visible, low-cost, and credit-claimable services: higher enrollment in state-sponsored health insurance, an expansion of sub-health centers, and stronger outreach through the Integrated Child Development Services (ICDS) program. Maternal and child care also improved: antenatal care coverage rose, reliance on public providers increased, and both mothers and children received more consistent postnatal checks. By contrast, centrally administered schemes, higher-tier infrastructure, institutional deliveries, and downstream child health outcomes remained unaffected. Testing for heterogeneity within the RD framework, we find that responsiveness was strongest in historically competitive districts. Our results show that boundary reforms can alter governments' incentives, leading to expansions in basic services most amenable to political credit-claiming, especially where electoral competition was strong.

Keywords: redistricting, malapportionment, health, India

JEL Codes: I18, H75, D72, P46, O15

*Department of Economics, University of Göttingen, Göttingen, Germany

† Department of Economics & Centre for Modern Indian Studies, University of Göttingen, Göttingen, Germany

1. Introduction

It is well documented that the distribution of public resources is often non-neutral and dictated by political survival, whether through electoral competition, partisan alignment, or identity-based representation (Bardhan et al., 2024; Bhalotra & Clots-Figueras, 2014; Burgess et al., 2015; Kailthya & Kambhampati, 2022; Keefer & Khemani, 2005; Pande, 2003). However, a crucial dimension of distributive politics remains less explored: how do shifts in the legislative weight of administrative regions, induced by redistricting, shape public service delivery.

We address this question in the context of India's 2008 delimitation, which redrew state legislative assembly constituencies after a three-decade freeze. Unlike settings where redistricting is frequent or politically manipulated, this reform was a rare, quasi-exogenous shock to political representation in the world's largest democracy, characterized by a multi-party, winner-take-all system. Overseen by an independent, non-partisan commission (Iyer & Reddy, 2013), the exercise sought to equalize constituency populations within each state, thereby eliminating state-level malapportionment. Since assembly constituencies are nested within districts – the level at which administrative functions and service delivery occur – the reform mechanically altered the number of seats districts held in state legislatures: some districts gained constituencies, while others lost. Importantly, these seat reallocations were determined by pre-delimitation population disparities rather than political bargaining.

Exploiting institutional rules that governed the delimitation process, we use pre-delimitation malapportionment as a running variable in a regression discontinuity framework to estimate the intention-to-treat (ITT) effect of gaining legislative weight on service delivery. We focus on health service delivery outcomes in rural India, where deficiencies are most acute and provision is primarily state-driven. Unlike urban areas, where private providers play a significant role, rural service delivery is more directly attributable to state action, making it a politically salient and central domain of welfare. Specifically, we focus on indicators such as coverage under state-sponsored health insurance schemes, access to and use of maternal and child health services, the reach of early childhood development programs, and the expansion of frontline health infrastructure. These outcomes capture the supply and utilization of health services and, because of their high visibility, also function as credit-claiming devices that are frequently deployed in election campaigns (Heath et al., 2025; Kalita & Croke, 2023; Shroff et al., 2015).

Our regression discontinuity estimates, which exploit quasi-exogenous changes in districts' relative weight in the state legislature, reveal a consistent pattern of selective responsiveness in health service delivery. Districts just above the malapportionment cutoff, those that experience marginal gains in legislative weight, saw significant increases in enrollment in state-run health insurance schemes, alongside expansions in sub-health centers and outreach through the Integrated Child Development Services (ICDS) program. In maternal care, districts at the cutoff registered higher uptake of antenatal services overall, with women receiving care more likely to rely on public providers. Gains also extended to postnatal care, with both mothers and newborns more likely to receive follow-up checks at multiple points after delivery. Complementing this, households were more likely to receive benefits under ICDS, reported greater contact with health workers, and administrative district-level data show expanded ICDS coverage. By contrast, we find no effects on centrally administered schemes, higher-tier infrastructure such as primary or community health centers, or downstream outcomes such as height- or weight-for-age z-scores, institution delivery rates, vaccination rates. To substantiate the mechanism, we complement our quantitative results with qualitative evidence from state election campaigns. News reports, party manifestos, and speeches show that state health insurance schemes and frontline outreach are repeatedly branded, publicized, and promised as electoral guarantees.

Taken together, our results suggest that boundary reforms reshaped incentives toward health services that are politically salient but shallow in scope. State governments responded by expanding visible, low-cost, and credit-claimable programs, while capital-intensive infrastructure and downstream health outcomes remained largely unaffected. These effects were strongest in historically competitive districts. By documenting this selective responsiveness, we extend theories of distributive politics beyond partisanship and alignment to the spatial allocation of representation, and demonstrate how the effects of institutional reforms are mediated by the degree of electoral competition. Our findings position legislative weight as a foundational, yet previously underexplored, dimension of distributive politics in large democracies.

This paper speaks to two broad strands of literature. First, it is related to existing evidence on distributive politics that suggest electoral incentives push governments toward interventions that are individually experienced and politically credit-claimable. Nath (2014) shows that weakened re-election incentives lead to elite capture of discretionary funds, while Bardhan et al. (2024) demonstrate that voters reward governments for excludable, visible welfare benefits but not for broad public goods. Second, it connects to existing research on redistricting, malapportionment, and federal program design. Comparative work shows that correcting malapportionment reallocates resources, from U.S. reapportionment shifting fiscal transfers (Ansolabehere et al., 2002) to Japan and Latin America where rural or overrepresented regions secured disproportionate benefits (Horiuchi & Saito, 2003; Gibson, Calvo & Falleti, 2004; Samuels & Snyder, 2001). However, majority of the evidence focuses on fiscal transfers or aggregate budgets, with less attention to how redistribution manifests in specific service delivery sectors that are electorally salient, such as health. Furthermore, most of these studies examine settings where redistricting is shaped by partisan bargaining, making it difficult to cleanly separate political strategy from the mechanical effects of reweighting representation.

A growing India-focused literature shows that unequal representation has substantive distributive consequences. Bhavnani (2021) finds that malapportionment causes unequal economic development, while Bhavnani (2015) demonstrates that larger constituencies are systematically disadvantaged in cabinet inclusion. More recent work shows that the delimitation re-orient political incentives. Tariq (2025) shows that increases in Muslim political weight translated into new schools and greater representation, Cagé et al. (2023) find that electorally more important areas experienced rapid expansions in newspaper markets, and Kukreja (2024) documents gains in political trust in districts that gained seats. Collectively, these studies highlight that malapportionment and its correction matter not only for development but also for political inclusion and accountability. What remains less understood, however, is which types of public services expand when legislative weight increases, and why. Our paper addresses this gap by showing that India's 2008 delimitation selectively spurred expansions in visible, low-cost, credit-claimable health services, while leaving capital-intensive infrastructure and downstream outcomes unaffected.

The rest of the paper is structured as follows. Section 2 gives the background of the institutional and political context of India's 2008 delimitation. Section 3 outlines our empirical strategy, including the construction of the malapportionment measure and the regression discontinuity design, and also details the data sources and outcome measures. Section 4 reports the main results, followed by Section 5, which examines heterogeneity across electoral competitiveness. Section 6 discusses policy implications and connections to broader theories of distributive politics, and concludes.

2. Background

Following the 1971 Census, India imposed a freeze on redistricting through the 42nd Constitutional Amendment Act of 1976, later extended by the 84th Amendment (2001) until after the 2026 Census. The freeze aimed to encourage states to pursue family planning without risking political representation losses from slower population growth.

The first major redrawing of constituencies in over three decades took place in the 2008 delimitation[1]. The allocation of seats across states remained fixed but intra-state adjustments were permitted on the basis of the 2001 Census. The exercise, overseen by the independent Delimitation Commission under the Delimitation Act of 2002, sought to restore parity in representation across districts. The Commission set out the institutional rules that guided the delimitation procedure.

Assembly constituencies were required to be contained within a single district, thereby aligning electoral boundaries with administrative jurisdictions[2]. The total population of the state (2001 Census) was required to be divided by the total number of assembly constituencies in the state, to obtain the State average per assembly constituency. This state average was the guiding factor for delimiting the constituencies in a manner that each constituency, so far as practicable, had an equal population.

District-level seat allocations were determined by comparing each district's population per seat to the state average. Districts that had been historically under-represented, where the population per seat exceeded the state average, were likely to gain seats, while over-represented districts were likely to lose or retain their pre-delimitation weight. In practice, this meant that the extent of pre-delimitation malapportionment, measured as the deviation of a district's population-per-seat ratio from the state average, mapped directly onto the probability of a district gaining or losing legislative weight. Because the number of seats at the state level was fixed, this reallocation was essentially a zero-sum game: gains in one district necessarily implied losses elsewhere. This mechanical redistribution of seats created sharp discontinuities

[1] Four states in the north-east – Assam, Arunachal Pradesh, Manipur, Nagaland, and also the state of Jammu Kashmir were unaffected by the 2008 delimitation. They are dropped from our analyses.
[2] Assembly constituencies are nested within districts.

in representation around the malapportionment threshold, which anchors our empirical strategy.

India's 2008 delimitation is highly unusual in comparative perspective. Unlike most democracies, India froze constituency boundaries after 1971, producing extreme malapportionment until the reform was finally implemented in 2008. This exercise, carried out by an independent and binding Delimitation Commission, simultaneously redrew boundaries and reallocated seats across states and districts, affecting hundreds of millions of voters in a single stroke. In contrast, most democracies undertake redistricting on a routine cycle every 5-10 years - as in the United States, Canada, the United Kingdom, Australia, New Zealand, South Korea, Taiwan, and Ireland, where adjustments are tied to census data and generally incremental. A second group of countries experiences irregular or politicized revisions (France, Italy, Japan, Kenya), where boundary changes occur roughly every 15-20 years and are shaped by government discretion, judicial intervention, or political contestation. In many proportional representation systems (e.g., the Netherlands, Nordic countries, Costa Rica, Uruguay, Chile, South Africa, Brazil), delimitation plays little to no role, since seat allocation follows fixed national or multi-member districts, though distortions may persist (e.g., Brazil's constitutional caps and floors favoring small states). Against this backdrop, India's 2008 delimitation stands out as a rare, one-time, large-scale redistricting shock in a majoritarian system (see appendix Figure A1), making it a uniquely powerful setting to study how electoral reforms affect service delivery.

3. Data

Our analysis draws on multiple data sources, combining delimitation commission documents with nationally representative micro-data surveys, infrastructure and services data collected by the Department of Rural Development. We map all data sources to the 2001 Census district boundaries to ensure comparability across time.

3.1. Delimitation Records

The primary institutional context comes from the Delimitation Commission Papers published by the Election Commission of India, which detail the rules, procedures, and outcomes of the 2008 redistricting exercise. These records specify the number of assembly seats assigned to each district before and after delimitation, allowing us to document how relative representation shifted across districts.

3.2. Public Service Delivery Outcomes

Our main outcomes come from two post-delimitation data sources, collected roughly a decade after the reform, allowing sufficient time for policy adjustments and multiple electoral cycles to play out. First, the National Family Health Survey (NFHS-5, 2019–21) provides individual-level information on health insurance coverage, antenatal and postnatal care, institutional deliveries, and child health outcomes. Second, we use the Antyodaya dataset (Asher & Novosad, 2019), which aggregates administrative records at the district level on frontline health infrastructure and the reach of the Integrated Child Development Services (ICDS). Together, these sources enable us to capture both household-level utilization and district-level provision of services.

3.3. Pre-treatment Covariates for Balance Checks

To examine pre-treatment comparability across districts, we use data from the 2001 Population Census, the 2002 District-Level Household Survey (DLHS), and district-level fertility estimates compiled by Guilmoto &Rajan (2002).

3.4. Historical Competitiveness

To examine heterogeneous effects by electoral context, we rely on competitiveness indices constructed by Dash et al. (2019), which cover state elections from 1985-2007. We use measures such as the margin of victory, adjusted multiparty margins, and party-system competitiveness to classify districts by their historical electoral environments.

The timing of our outcome data is central to our identification strategy. Both NFHS-5 and the Antyodaya dataset were collected more than a decade after delimitation, by which time multiple state electoral cycles had elapsed. This ensures that observed differences in service delivery reflect sustained adjustments in government resource allocation rather than transitional disruptions immediately following the reform.

4. Empirical Strategy

We leverage the fact that the 2008 delimitation reallocated state-assembly seats across districts using 2001 Census populations, with the mandate to (approximately) equalize constituency populations within each state. Let i index districts and s states. For each state s, let the state-specific quota (2001 census population-per-seat benchmark) as defined by the commission's apportionment rule be denoted as $Quota_s = \frac{Pop_{s,2001}}{Seats_{s,pre}}$, where $Seats_{s,pre}$ denotes the pre-delimitation seats allocated to state s.[3] We construct an intra-state, relative pre-delimitation malapportionment score as our running variable,

$$Z_{is} = \frac{Pop_{is,2001}}{Seats_{is,pre}} - Quota_s$$

Intuitively, $Z_{is} > 0$ indicates a district whose 2001 population per pre-delimitation seat exceeded the state quota. In other words, such a district was "underrepresented" relative to the state's average representation level. Define the treatment indicator: $D_{is} = 1\{Z_{is} > 0\}$, which turns on at the malapportionment threshold. While districts above the cutoff are more likely to receive increased weight by virtue of gaining constituencies, treatment is not deterministically

[3] Note that as per the delimitation rules, $Seats_{s,pre} = Seats_{s,post-delimitation}$

assigned. Rounding-off rules and geographic constraints would prevent a perfect mechanical mapping from Z_{is} to the final seat reallocation. We interpret our estimates as the intention-to-treat (ITT) effect of crossing the malapportionment threshold (Cattaneo et al., 2023; Cattaneo & Titiunik, 2022).

We implement a regression discontinuity at $z = 0$. For individual-level outcomes Y_{jis} (individual j in district i, state s), our specification is:

$$Y_{jis} = \alpha + \tau D_{is} + f_{-}(z_{is}) \cdot 1\{z_{is} < 0\} + f_{+}(z_{is}) \cdot 1\{z_{is} \geq 0\} + \lambda_s + \varepsilon_{jis}$$

Where f_{-} and f_{+} are side-specific local polynomials in z, λ_s are state-fixed effects. For each outcome, we estimate local linear regression discontinuity models with triangular kernel weights and mean-squared-error–optimal bandwidths. Standard errors are clustered at the district level for individual-level outcomes to reflect the level of treatment assignment. For district-level outcomes, Y_{is}, we estimate the analogous regression discontinuity with district as the unit of observation.

A key identifying assumption in the regression discontinuity design is that districts could not manipulate their relative malapportionment status around the delimitation threshold. We test this by examining the distribution of the running variable, pre-delimitation relative malapportionment, defined as the difference between a district's actual and population-implied seat share, using the McCrary (2008) density test. Negative values correspond to over-represented districts, while positive values capture under-represented districts. Figure 1 plots the density of the running variable on either side of the cutoff. The distribution appears smooth, with no evidence of a discontinuous jump at zero. Consistent with the formal McCrary test, this suggests that districts could not sort or manipulate their representation status around the threshold, lending credibility to the RD design.

<Figure 1 here>

Another identifying assumption is that districts just above and below the malapportionment cutoff were similar in pre-delimitation characteristics. We assess this assumption in three ways.

First, we use the District Level Household Survey–2 (DLHS-2, 2002–2004) to examine maternal and child health care outcomes: antenatal care, postpartum care, and delivery care, well before delimitation. As reported in Table A1, RD estimates show no systematic differences across the cutoff. Point estimates are small, precisely estimated, and never statistically distinguishable from zero. This indicates that pre-delimitation service utilization was smooth around the cutoff.

Second, we exploit DLHS-2 facility surveys to test for differences in village-level health infrastructure. Table A2 reports RD estimates for the presence of Primary Health Centres, sub-centres, dispensaries, and hospitals. Again, we find no evidence of discontinuities. All coefficients are close to zero and statistically insignificant, suggesting that the distribution of basic health infrastructure was balanced prior to delimitation.

Third, we use the 2001 Census Primary Census Abstract and Village Directory to check a wider set of socio-demographic and infrastructural covariates, including total population, caste composition (Scheduled Castes and Scheduled Tribes), literacy, and workforce composition, as well as educational and health facilities. Tables A3-A4 present these results. None of the discontinuities are statistically significant, and effect sizes are substantively negligible. Similarly, we find no evidence that the density of primary schools, secondary schools, hospitals, dispensaries, or health centres differed across the cutoff in 2001.

Taken together, these falsification tests strongly support the validity of our RD design. Districts on either side of the malapportionment threshold were statistically and substantively similar in maternal health outcomes, health infrastructure, and socio-demographic composition before delimitation. This bolsters confidence that the post-delimitation differences we document are indeed attributable to the reallocation of legislative representation rather than pre-existing differences or divergent pre-trends.

4.1.Changes in Weights of Administrative Units

Figure 2 confirms the mechanical link between pre-delimitation malapportionment and post-delimitation changes in districts' weight in the state legislature measured by seats of the district relative to the total seats of the state. Districts that were under-represented prior to delimitation (to the right of zero) were more likely to gain weight in the state legislature, while over-represented districts (to the left of zero) were more likely to lose weight.

<Figure 2 here>

Prior to the delimitation, the average district accounted for approximately 4.6 percentage points of its state's legislative assembly seats, calculated as the mean share of seats held by each district in the state legislature. Crossing the malapportionment threshold for seat allocation increased a district's share of seats by 0.28 percentage points, relative to the state total. This represents a 6% increase over the pre-delimitation mean and corresponds to approximately 0.3 standard deviations. Given the institutional stability and rarity of representational reallocation in India, this constitutes a substantively meaningful shift in political representation.

To contextualize this magnitude, consider that the average state assembly in India comprises roughly 215 Members of the Legislative Assembly (MLAs). A 0.28 percentage point increase in seat share translates to an expected gain of approximately 0.6 seats per district. While seats are indivisible, this average effect likely reflects that districts just above the threshold were significantly more likely to gain a full seat than those just below it, highlighting the discontinuous jump in legislative representation induced by the delimitation process.

Importantly, the magnitude of this threshold-based change is considerably larger than the average effect across the full distribution of districts. Using population data from the 2001 Census, we compute that the average district had 274,405 people per legislative seat prior to delimitation. Following the delimitation, this figure marginally decreased to 273,944.5, implying an average improvement in per capita representation of only 460.5 persons per seat, or about 0.16%. While the overall national shift was modest, the discrete increase in representation at the threshold is substantially more pronounced.

5. Results

5.1. Public Insurance Schemes

Table 1 reports regression discontinuity estimates for public health insurance coverage in rural India. Districts just above the malapportionment cutoff experience a 10-11 percentage point increase in state-run insurance coverage, significant at the 5 percent level. Bias-corrected and robust estimates are nearly identical, underscoring the consistency of the effect. By contrast, no changes are observed in RSBY or other non-state schemes, with point estimates close to zero. Overall insurance coverage increases by about 6-7 percentage points, but the estimates are imprecisely estimated and statistically indistinguishable from zero.

<Table 1 here>

To probe the composition of coverage, we also estimate treatment effects conditional on individuals reporting any form of health insurance (Table 2). Although conditional estimates restrict the sample to insured individuals and may therefore be subject to selection concerns, they remain informative for examining substitution across schemes. The results show that among those with coverage, individuals in districts just above the cutoff are 20 percentage points more likely to be enrolled in state-sponsored insurance schemes, but no more likely to hold coverage under the centrally administered RSBY. At the same time, we observe an 11-12 percentage point decline in the probability of holding other forms of insurance, indicating that state-led expansion partly crowds out alternative sources of coverage. Taken together with our unconditional estimates, these findings suggest that overall gains in insurance are driven almost entirely by state-run, visible, and credit-claimable programs, underscoring their salience as political goods.

<Table 2 here>

5.2.Antenatal Care

While shifts in public insurance schemes imply greater entry points into the state health system, we next examine whether such entry translates into changes in maternal and child health services uptake. The regression discontinuity estimates indicate that districts just above the malapportionment cut-off experience modest but significant improvements in maternal health service utilization (Table 3). Women in these districts are 3-3.5 percentage points more likely to receive any antenatal care for their last birth, significant at the 5-10 percent level. Conditional on receiving care, the estimates suggest that the increase is driven primarily by greater use of public facilities. The probability of receiving care from a public provider rises by about 2-2.5 percentage points, significant at the 10 percent level, while there is no corresponding increase in the use of private providers. These results suggest that the expansion of state-provided maternal health services increase uptake and shift users into state channels.

<Table 3 here>

5.3.Postnatal Care

We next investigate if these findings also translate to follow-up care once women enter the system (Table 4). For mothers, the probability of receiving a checkup before discharge increases by 6.5-7 percentage points, with additional gains of around 6-7 percentage points for checkups after discharge and 9-10 percentage points for checkups within the first two months. All effects are significant at the 1-5 percent level. Similarly, children born in these districts are more likely to receive postnatal checkups. Coverage rises by about 4.5-5 percentage points before discharge, 7-8 percentage points after discharge, and 7-7.5 percentage points within two months of birth, with estimates consistently statistically significant.

<Table 4 here>

5.4. Integrated Child Development Services

The regression discontinuity estimates indicate that districts just above the malapportionment cut-off experience consistent and statistically significant increases in access to ICDS benefits and frontline worker outreach.

<Table 5 here>

Women are 5.7-6.2 percentage points more likely to receive Anganwadi/ICDS benefits during pregnancy and 6.3-6.9 percentage points more likely to receive such benefits while breastfeeding, both significant at the 5-10 percent level. Similarly, children are 4.7-5.3 percentage points more likely to benefit from ICDS services.

In terms of outreach, contact with frontline workers rises substantially. The probability of a woman reporting contact with an Anganwadi worker, ASHA, or community health worker increases by 7.5-8.2 percentage points, highly significant at the 1 percent level. Contact with auxiliary nurse midwives (ANMs) or lady health visitors (LHVs) also rises by 4.4-5.0 percentage points, significant at the 5 percent level.

5.5.Antyodaya data

The Antyodaya data provide complementary evidence on service delivery in rural areas. Districts just above the malapportionment cut-off are significantly more likely to report the provision of ICDS benefits. The share of villages offering benefits to pregnant women rises by 0.87-0.90 percentage points, and benefits to breastfeeding mothers increase by 0.88-0.94 percentage points, both significant at the 5 percent level. These effects echo the household-level findings from NFHS, reinforcing that states selectively scale up ICDS services in electorally more salient areas.

<Table 6 here>

In terms of health infrastructure, the number of sub-centres in a village increases modestly, by about 2.5-2.7 percent, significant at conventional levels. By contrast, there is no evidence of change in the availability of higher-level facilities such as Primary Health Centres (PHCs) or Community Health Centres (CHCs).

Taken together, these results suggest that electoral incentives encourage expansion at the most local, visible, and service-intensive levels of delivery. Gains are concentrated in ICDS provision and sub-centres, where benefits are directly experienced by households, while larger and less frequent infrastructure such as PHCs and CHCs remain unaffected.

5.6. Qualitative Evidence

A key question is whether the observed expansions reflect state capacity to scale up low-cost services, or whether they are politically motivated responses to shifts in legislative weight. To shed light on this mechanism, we draw on contemporary campaign materials, manifestos, and news coverage of state elections. These sources consistently show that state-sponsored health programs, particularly health insurance schemes and community outreach, feature prominently in political messaging across parties and states. For instance, in recent election cycles, state governments have pledged to expand insurance coverage to ₹25–50 lakh per family, launched new branded schemes (e.g., Rajasthan's Chiranjeevi Yojana, Punjab's Mantri Shat Bima Yojna), and rolled out health outreach campaigns in the months immediately preceding polls. Such initiatives are repeatedly described as "health cards," "family protection schemes," or "guarantees," underscoring their high visibility to voters. To substantiate this interpretation, we compiled evidence from campaign manifestos and election coverage across states. Figure A2 in the Appendix shows how promises of expanded health insurance and outreach programs repeatedly appear in party platforms and campaign headlines, reinforcing the visibility and electoral salience of these interventions.

These examples support our interpretation that governments prioritize visible, electorally salient interventions when districts gain legislative weight. The focus on state-administered schemes, rather than centrally sponsored programs like Ayushman Bharat, also aligns with our empirical findings: politicians are more responsive where they can directly claim credit. Taken together, the qualitative evidence from electoral campaigns reinforces the quantitative patterns we document, strengthening the interpretation that boundary reforms induced selective expansions in politically credit-claimable services rather than neutral improvements in administrative delivery.

6. Electoral Competition as a Mediator of Delimitation Effects

To assess whether the developmental consequences of the 2008 delimitation varied with the underlying degree of political competition, we employ the asymmetry-adjusted marginal seats (AMS) index as a pre-determined measure of electoral contestability. The AMS, developed by Dash et al. (2019), combines information on the share of marginal seats with the distribution of safe seats across parties, thereby capturing both the extent to which elections are close and whether political dominance is concentrated in a single party. The index ranges from 0 to 1, with higher values indicating more competitive electoral environments, where a greater proportion of seats are genuinely contestable and safe seats are more evenly shared across political parties. Using this pre-delimitation measure of competition allows us to examine whether the impact of delimitation was systematically different in districts that were historically more electorally competitive.

Methodologically, we implement the heterogeneous regression discontinuity (RD) framework introduced by Calonico, Cattaneo, Farrell, Palomba, and Titiunik (2025), which extends standard local polynomial RD estimators to allow for estimation and inference on subgroup-specific treatment effects. We stratify districts by whether they fall above or below the median AMS level prior to delimitation, corresponding to historically high- and low-competition environments, respectively, and estimate separate treatment effects for these subgroups. The difference in coefficients across subgroups is formally tested, providing direct evidence on heterogeneity by electoral competition.

<Table 8 here>

Table 8 presents heterogeneous RD estimates by pre-delimitation electoral competition, measured using the asymmetry-adjusted marginal seats (AMS) index. Panel A draws on Antyodaya data, while Panel B relies on NFHS household surveys. Across both data sources, a consistent pattern emerges: the developmental effects of the 2008 delimitation are significantly stronger in historically more competitive districts. In the Antyodaya data, ICDS coverage expanded much more in above-median AMS districts, with effects for pregnant and breastfeeding women exceeding 1.8 and 2.1 percentage points respectively, compared to small and statistically insignificant changes in less competitive areas. The inter-group differences are large and statistically significant, indicating that expansions in nutrition and childcare services were disproportionately concentrated in competitive districts. Similarly, while the availability of sub-centres increased modestly overall, the gains are concentrated in competitive environments, with the difference between above- and below-median AMS districts significant at the 10 percent level.

The NFHS results (Panel B) reinforce this interpretation. State-sponsored health insurance coverage rose sharply in both groups but the increase was nearly twice as large in competitive districts (40 vs. 21 percentage points), yielding a highly significant difference of 19 percentage points. For maternal and child health outcomes, the pattern is more nuanced. Antenatal care improved modestly across districts but without significant heterogeneity by competition. By contrast, postnatal care shows clear differences: competitive districts experienced substantially larger improvements, with a 3-4 percentage point higher effect relative to less competitive areas. Finally, for ICDS benefits, while the below-median AMS group shows no detectable change, competitive districts exhibit significant improvements, with the difference again statistically significant.

Taken together, these results demonstrate that the developmental returns to delimitation were not evenly distributed. Districts with higher levels of pre-delimitation electoral competition saw consistently larger expansions in state-led, credit-claimable services, particularly ICDS coverage, sub-centres, and state-run insurance, while less competitive districts benefited little. The evidence underscores the disciplining role of electoral competition: boundary reforms altered governments' incentives everywhere, but these incentives translated into tangible developmental gains only where representatives faced stronger electoral pressures.

7. Discussion

7.1. Plausibility and Magnitude of Effects

A natural concern is whether the observed shifts in legislative weight are too small to plausibly alter government behavior. On average, crossing the malapportionment threshold increased a district's seat share by 0.28 percentage points, equivalent to roughly 0.6 seats in a typical state assembly. While modest in absolute terms, this change represents hundreds of thousands of constituents in India's very large districts, where the average assembly constituency contains over 270,000 people. In tightly contested state legislatures, even a single additional seat can alter the balance of power, making small reweighting politically salient.

Moreover, the discontinuity does not merely capture marginal statistical variation. For districts on either side of the cutoff, being just above or just below determines whether they are perceived by political actors as "underrepresented" and hence deserving of additional seats. This binary shift can reorient expectations of electoral returns, making districts just above the threshold more attractive targets for distributive spending. The logic is consistent with prior work on U.S. reapportionment (Ansolabehere et al., 2002) and Japanese malapportionment (Horiuchi & Saito, 2003), where even modest reweighting induced measurable shifts in the allocation of public resources.

The magnitude of the representational shift also helps explain the selective nature of the effects we document. Small increases in legislative weight are unlikely to trigger large-scale, capital-intensive investments such as primary health centers or long-run improvements in child health outcomes, which require substantial fiscal and administrative resources. Instead, governments responded with visible, low-cost, and politically credit-claimable interventions, such as health insurance enrollment, expansions of sub-health centers, and ICDS outreach, that could be scaled up quickly and directly linked to politicians' efforts. In this sense, the modest magnitude of the reform is consistent with the pattern of shallow but electorally salient gains we observe.

The findings from India's 2008 delimitation contribute to a deeper understanding of how electoral institutions shape distributive politics. By shifting legislative weight across districts, the reform altered the incentives of state governments and their representatives, leading to expansions in health services that are visible, localized, and politically salient. The evidence from both household-level surveys and Antyodaya data points consistently to gains in sub-centres, ICDS services, and frontline outreach, precisely the types of interventions that allow politicians to claim credit directly from constituents. By contrast, higher-tier infrastructure, centrally administered schemes, and downstream health outcomes remained unaffected. This pattern reinforces a core insight from the political economy of distributive politics: politicians are strategic actors who selectively prioritize goods that maximize electoral returns.

The heterogeneity analysis underscores this mechanism by showing that the developmental impact of delimitation was not uniform but strongest in districts that were historically more competitive, as measured by the AMS index. This finding aligns with theories of accountability that stress the disciplining role of electoral competition. Where elections were closely contested and safe seats were more evenly distributed across parties, the expansion of legislative representation was translated into tangible improvements in service delivery. Conversely, in environments dominated by entrenched parties with weak competition, additional legislative weight had little effect. This asymmetry highlights the conditional nature of institutional reforms: their efficacy depends not only on the formal rules of representation but also on the competitiveness of the broader political environment.

Our interpretation is consistent with recent survey evidence on credit attribution in India's health sector. Heath et al. (2025) show that while many voters correctly assign credit for visible health programs such as insurance schemes to the appropriate tier of government, dissatisfaction is often displaced onto local governments, insulating higher tiers from blame. Political leaders actively brand and publicize state health schemes to maximize electoral returns while deflecting responsibility for failures in infrastructure. This helps explain why the expansions we observe are concentrated in visible, credit-claimable services, rather than in capital-intensive or long-term outcomes.

From a policy perspective, these results suggest that boundary reforms alone cannot ensure equitable developmental outcomes. While delimitation succeeded in shifting resources toward electorally salient districts, its benefits accrued disproportionately to areas with higher pre-existing competition, potentially reinforcing geographic inequalities in public service provision. Policymakers seeking to leverage redistricting as a tool for enhancing equity in representation and service delivery must therefore recognize its interaction with the structure of party competition. Complementary reforms that strengthen electoral competitiveness—such as curbs on incumbency advantage, greater transparency in campaign finance, or stronger enforcement of electoral rules, may be necessary to ensure that institutional changes translate into more broad-based developmental gains.

At a broader theoretical level, the findings speak to enduring debates on whether institutions or competition drive distributive outcomes. The Indian case illustrates that institutional reforms like delimitation can reshape incentives, but their impact is mediated by the extent of electoral competition. In doing so, the results bridge two strands of the literature: one emphasizing the role of formal rules of representation in shaping distributive politics, and another highlighting the disciplining effect of competitive elections. The interaction between these two dimensions helps explain why boundary reforms generate visible service delivery gains in some contexts but not in others.

Ultimately, the evidence presented here advances our understanding of the micro-foundations of distributive politics in large democracies. By documenting how boundary reforms interact with electoral competition to shape the incentives of politicians, the study shows that reforms aimed at improving representation must be evaluated not only in terms of their institutional design but also in light of the competitive environments in which they operate.

References

Ansolabehere, S., Gerber, A., & Snyder, J. (2002). Equal votes, equal money: Court-ordered redistricting and public expenditures in the American states. *American Political Science Review*, *96*(4), 767-777.

Bardhan, P., Mitra, S., Mookherjee, D., & Nath, A. (2024). How do voters respond to welfare vis-à-vis public good programs? Theory and evidence of political clientelism. *Quantitative Economics*, *15*(3), 655-697.

Bhalotra, S., & Clots-Figueras, I. (2014). Health and the political agency of women. *American Economic Journal: Economic Policy*, *6*(2), 164-197.

Bhavnani, R. R. (2018). The effects of malapportionment on cabinet inclusion: Subnational evidence from India. *British Journal of Political Science*, *48*(1), 69-89.

Bhavnani, R. R. (2021). The effects of malapportionment on economic development. *PloS one*, *16*(12), e0259150.

Burgess, R., Jedwab, R., Miguel, E., Morjaria, A., & Padró i Miquel, G. (2015). The value of democracy: evidence from road building in Kenya. *American Economic Review*, *105*(6), 1817-1851.

Cagé, J., Cassan, G., & Jensenius, F. (2023). Political Determinants of the News Market: Novel Data and Quasi-Experimental Evidence from India (No. 17920). CEPR Discussion Papers.

Cattaneo, M. D., Keele, L., & Titiunik, R. (2023). A guide to regression discontinuity designs in medical applications. *Statistics in Medicine*, *42*(24), 4484-4513.

Cattaneo, M. D., & Titiunik, R. (2022). Regression discontinuity designs. *Annual Review of Economics*, *14*(1), 821-851.

Dash, B. B., Ferris, J. S., & Winer, S. L. (2019). The measurement of electoral competition, with application to Indian states. *Electoral Studies*, *62*, 102070.

Daxecker, U. (2020). Unequal votes, unequal violence: Malapportionment and election violence in India. *Journal of Peace Research*, *57*(1), 156-170.

Gibson, E. L., Calvo, E., & Falleti, T. (2004). Reallocative Federalism: Overrepresentation and Public Spending in the Western Hemisherepp. 173–196, Federalism and Democracy in Latin America.

Guilmoto, C. Z., & Rajan, S. I. (2002). District level estimates of Fertility from India's 2001 Census. *Economic and political weekly*, 665-672.

Heath, O., Tillin, L., Mishra, J., Kumar, S., & Venkateswaran, S. (2025). Poor health: Credit and blame attribution in India's multi-level democracy. *World Development*, *185*, 106807. https://www.sciencedirect.com/science/article/pii/S0305750X24002778

Horiuchi, Y., & Saito, J. (2003). Reapportionment and redistribution: Consequences of electoral reform in Japan. *American Journal of Political Science*, *47*(4), 669-682.

Iyer, L., & Reddy, M. (2013). Redrawing the Lines: Did Political Incumbents Influence Electoral Redistricting in the World's Largest Democracy? Harvard Business School. https://www.hbs.edu/ris/Publication%20Files/14-051_6beba5c6-4c63-455d-9f02-0f6bd9364877.pdf

Kailthya, S., & Kambhampati, U. (2022). Political competition and public healthcare: Evidence from India. *World Development*, *153*, 105820.

Kalita, A., & Croke, K. (2023). The politics of health policy agenda setting in India: the case of the PMJAY program. *Health Systems & Reform*, *9*(1), 2229062.

Keefer, P., & Khemani, S. (2005). Democracy, public expenditures, and the poor: understanding political incentives for providing public services. *The World Bank Research Observer*, *20*(1), 1-27.

Kukreja, R. (2024). Does representation affect trust in political institutions?: Evidence from redistricting in India. *World Development*, *179*, 106563.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of econometrics*, *142*(2), 698-714.

Nath, A. (2014). Political competition and elite capture of local public goods. *Boston University*. https://static1.squarespace.com/static/56f5ac01746fb9744ebf00c2/t/57db8273ff7c509103796445/1474003571904/AnushaNath_MPLADEliteCapture.pdf

Pande, R. (2003). Can mandated political representation increase policy influence for disadvantaged minorities? Theory and evidence from India. *American economic review*, *93*(4), 1132-1151.

Samuels, D., & Snyder, R. (2001). The value of a vote: malapportionment in comparative perspective. *British Journal of Political Science*, *31*(4), 651-671.

Shroff, Z. C., Kumar, S., & Reich, M. R. (2015). Health insurance as a tool of electoral tactical redistribution in Tamil Nadu, India. *Asian Survey*, *55*(5), 991-1017.

Tariq, A. (2025). Constituencies of Change. *Available at SSRN 5320109*.

Data:

Asher, S., & Novosad, P. (2019). *Socioeconomic High-resolution Rural–Urban Geographic Dataset on India (SHRUG)*. Development Data Lab. Retrieved from https://www.devdatalab.org/shrug

Election Commission of India. (2008). *Delimitation of Parliamentary and Assembly Constituencies Order, 2008*. New Delhi: Election Commission of India.

International Institute for Population Sciences (IIPS) and Ministry of Health and Family Welfare (MoHFW). (2006). *District Level Household and Facility Survey (DLHS-2), 2002–04: India*. Mumbai: IIPS.

International Institute for Population Sciences (IIPS) and ICF. (2022). *India National Family Health Survey NFHS-5, 2019–21 [Dataset]*. Mumbai, India, and Rockville, Maryland, USA: IIPS and ICF.

Office of the Registrar General & Census Commissioner, India. (2001). *Census of India 2001: General Population Tables*. New Delhi: Office of the Registrar General & Census Commissioner, Ministry of Home Affairs, Government of India.

Tables & Figures

Table 1: Public Health Insurance Scheme

	(1) State Insurance	(2) RSBY	(3) Other	(4) All Insurance
Conventional	0.102**	0.009	-0.019	0.067
	(0.057)	(0.029)	(0.014)	(0.053)
Bias-corrected	0.112**	0.008	-0.023	0.069
	(0.057)	(0.029)	(0.014)	(0.053)
Robust	0.112**	0.008	-0.023	0.069
	(0.060)	(0.031)	(0.015)	(0.056)
Observations	454289	454289	454289	454289
Effective Obs. (left)	223289	241201	199977	228142
Effective Obs. (right)	180479	189813	162737	183248
Bandwidth	67919.35	94705.88	44016.73	76071.71

This table reports local linear regression discontinuity estimates of the impact of crossing the malapportionment cut-off on different forms of health insurance coverage. The dependent variable in each column is an indicator for whether the individual reports coverage under the specified scheme: state-run insurance (column 1), RSBY (column 2), other insurance schemes (column 3), and any form of insurance (column 4). Conventional, bias-corrected, and robust estimates are reported, with robust standard errors in parentheses, clustered at the district-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Public Health Insurance Scheme Conditional on Coverage

	(1) State Insurance	(2) RSBY	(3) Other
Conventional	0.176^{*}	-0.018	-0.100^{*}
	(0.110)	(0.075)	(0.057)
Bias-corrected	0.200^{**}	-0.021	-0.116^{**}
	(0.110)	(0.075)	(0.057)
Robust	0.200^{**}	-0.021	-0.116^{*}
	(0.116)	(0.081)	(0.061)
Observations	155216	155216	155216
Effective Obs. (left)	74121	79198	72387
Effective Obs. (right)	63036	67130	61894
Bandwidth	60426.95	86959.58	52269.06

This table reports local linear regression discontinuity estimates of the impact of crossing the malapportionment cut-off on the composition of health insurance coverage, conditional on individuals reporting any form of insurance. The dependent variable in each column is an indicator for whether the individual reports coverage under the specified scheme: state-run insurance (column 1), RSBY (column 2), and other insurance schemes (column 3). Conventional, bias-corrected, and robust estimates are reported, with robust standard errors in parentheses, clustered at the district-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Antenatal Care

	(1) Antenatal Care	(2) Public Provider	(3) Private Provider
Conventional	0.030*	0.024*	-0.001
	(0.016)	(0.013)	(0.014)
Bias-corrected	0.034**	0.023*	-0.001
	(0.016)	(0.013)	(0.014)
Robust	0.034**	0.023*	-0.001
	(0.017)	(0.013)	(0.014)
Observations	116542	108957	108957
Effective Obs. (left)	54027	50592	49824
Effective Obs. (right)	46723	43473	42982
Bandwidth	60739.73	59840.90	57035.91

This table reports local linear regression discontinuity estimates of the impact of crossing the malapportionment cut-off on antenatal care utilization. The dependent variable in column 1 is an indicator for whether the woman received any antenatal care for her last birth. Columns 2 and 3 condition on receiving care, and the dependent variable is an indicator for whether the source of care was a public (column 2) or private provider (column 3). Conventional, bias-corrected, and robust estimates are reported, with robust standard errors in parentheses, clustered at the district-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Postnatal Care

	(1)	(2)	(3)	(4)	(5)	(6)
	Check-up of Mother			Check-up of Child		
	Before Discharge	After Discharge	In 2 months	Before Discharge	After Discharge	In 2 months
Conventional	0.065***	0.060**	0.086***	0.044***	0.068**	0.065**
	(0.019)	(0.031)	(0.031)	(0.015)	(0.031)	(0.030)
Bias-corrected	0.070***	0.070**	0.096***	0.048***	0.078**	0.074**
	(0.019)	(0.031)	(0.031)	(0.015)	(0.031)	(0.030)
Robust	0.070***	0.070**	0.096***	0.048***	0.078**	0.074**
	(0.019)	(0.032)	(0.032)	(0.016)	(0.033)	(0.031)
Observations	101482	101482	101482	100968	101482	116484
Effective Obs. (left)	46936	46936	46458	47768	46303	52259
Effective Obs. (right)	39123	39123	38843	39310	38591	45613
Bandwidth	54094.62	54612.25	50742.48	58147.00	50282.89	51481.14

This table reports local linear regression discontinuity estimates of the impact of crossing the malapportionment cut-off on postnatal care utilization. Columns 1–3 report outcomes for mothers: whether the mother received a check-up before discharge, after discharge, or within two months of delivery. Columns 4–6 report analogous outcomes for children: whether the child received a check-up before discharge, after discharge, or within two months of birth. Conventional, bias-corrected, and robust estimates are reported, with robust standard errors in parentheses, clustered at the district-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: ICDS benefits

	(1) Anganwadi/ ICDS benefits when pregnant	(2) Anganwadi/ ICDS benefits when breastfeeding	(3) Anganwadi/ ICDS benefits to child	(4) Contacts with Anganwadi worker, Asha, or CHW	(5) Contacts with ANM or LHV
Conventional	0.057*	0.063*	0.047*	0.075***	0.044**
	(0.029)	(0.033)	(0.024)	(0.026)	(0.022)
Bias-corrected	0.062**	0.069**	0.053**	0.082***	0.050**
	(0.029)	(0.033)	(0.024)	(0.026)	(0.022)
Robust	0.062**	0.069*	0.053**	0.082***	0.050**
	(0.031)	(0.036)	(0.026)	(0.027)	(0.023)
Observations	131142	131142	127548	131142	131142
Effective Obs. (left)	62362	61845	59536	151387	152674
Effective Obs. (right)	54279	53887	51899	121486	121486
Bandwidth	70724.03	68643.67	63203.55	56922.97	58820.56

This table reports local linear regression discontinuity estimates of the impact of crossing the malapportionment cut-off on the receipt of anganwadi/ICDS benefits and contact with frontline health workers. Columns 1–3 report outcomes for whether the woman received ICDS benefits during pregnancy, while breastfeeding, or for her child. Columns 4–5 report outcomes for whether the woman was contacted in the last three months by an anganwadi worker, ASHA, or community health worker (column 4), or by an auxiliary nurse midwife (ANM) or lady health visitor (LHV) (column 5). Conventional, bias-corrected, and robust estimates are reported, with robust standard errors in parentheses, clustered at the district-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Antyodaya Data

	(1) ICDS benefits when pregnant	(2) ICDS benefits when breastfeeding	(3) Sub-centres	(4) PHC	(5) CHC
Conventional	0.867**	0.883**	0.027**	0.001	0.000
	(0.404)	(0.429)	(0.013)	(0.006)	(0.008)
Bias-corrected	0.903**	0.941**	0.025*	0.001	0.001
	(0.404)	(0.429)	(0.013)	(0.006)	(0.008)
Robust	0.903**	0.941**	0.025*	0.001	0.001
	(0.432)	(0.462)	(0.014)	(0.007)	(0.009)
Observations	515	515	515	515	515
Effective Obs. (left)	251	253	244	258	245
Effective Obs. (right)	192	197	185	200	186
Bandwidth	60846.20	67001.58	51837.00	72535.09	53668.98

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Child Health Outcomes

	(1) Institutional Delivery	(2) Financial Assistance	(3) HAZ score	(4) WAZ score	(5) BMI
Conventional	-0.023	0.023	16.302	-0.948	52.949
	(0.033)	(0.034)	(34.590)	(11.339)	(56.346)
Bias-corrected	-0.016	0.029	15.849	-0.695	46.021
	(0.033)	(0.034)	(34.590)	(11.339)	(56.346)
Robust	-0.016	0.029	15.849	-0.695	46.021
	(0.035)	(0.037)	(36.634)	(12.105)	(59.401)
Observations	116542	101482	107542	107856	107501
Effective Obs. (left)	54027	49231	51531	51371	50891
Effective Obs. (right)	47714	41459	45028	44813	44664
Bandwidth	63905.51	70551.44	75840.09	73626.03	69841.06

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Electoral Competitiveness as a Mechanism

	Below-median AMS	Above-median AMS	Difference (Above – Below)
Panel A: Antyodaya Outcomes			
ICDS coverage (Pregnant women)	1.414 (1.398)	1.854* (1.007)	0.439** (0.219)
ICDS coverage (Breastfeeding women)	0.498 (0.327)	2.120** (1.040)	1.622** (0.648)
Sub-centres	–0.045 (0.039)	0.036 (0.025)	0.081* (0.047)
Observations	515	515	515
Panel B: NFHS Outcomes			
State Insurance	0.208*** (0.015)	0.400*** (0.005)	0.192*** (0.025)
Antenatal Care	0.042*** (0.015)	0.032*** (0.007)	–0.010 (0.037)
Postnatal Care	0.063*** (0.022)	0.097*** (0.010)	0.034*** (0.019)
ICDS Benefits	0.068 (0.156)	0.074*** (0.011)	0.006*** (0.002)
Observations	105000	105000	105000

Table reports heterogeneous regression discontinuity (RD) estimates of the effects of the 2008 delimitation on service delivery, stratified by pre-delimitation electoral competition. Competition is measured using the asymmetry-adjusted marginal seats (AMS) index from Dash et al. (2019); districts above the median AMS are classified as historically more competitive. Estimates are obtained using the rdhte package (Calonico et al., 2025), with pre-delimitation malapportionment (zin) as the running variable and cutoff at zero. Estimation relies on local linear ($p = 1$) specifications with second-order bias correction ($q = 2$), triangular kernel, MSE-optimal bandwidths (bwselect = "mserd"), and HC3 robust standard errors. Coefficients are reported with standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figures

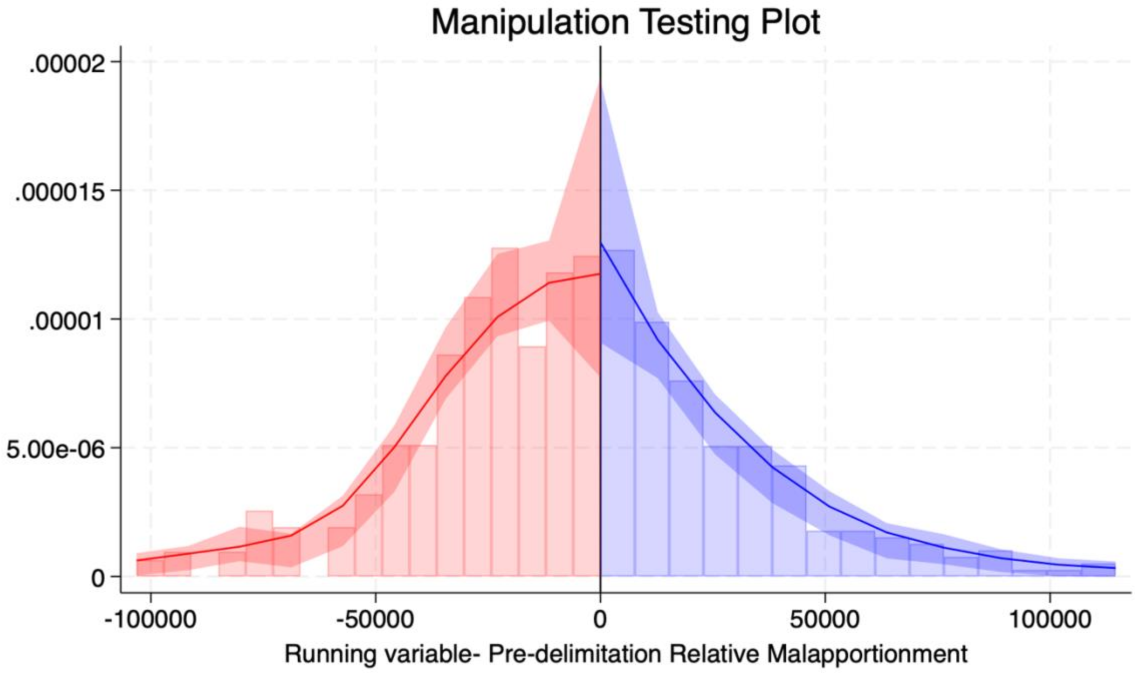


Figure 1: This figure presents the McCrary (2008) density test for the running variable, pre-delimitation relative malapportionment. Negative values correspond to districts that were over-represented prior to the 2008 delimitation, while positive values capture under-represented districts. The density of observations appears smooth across the cutoff at zero, with no evidence of a discontinuous jump, indicating the absence of manipulation around the threshold.

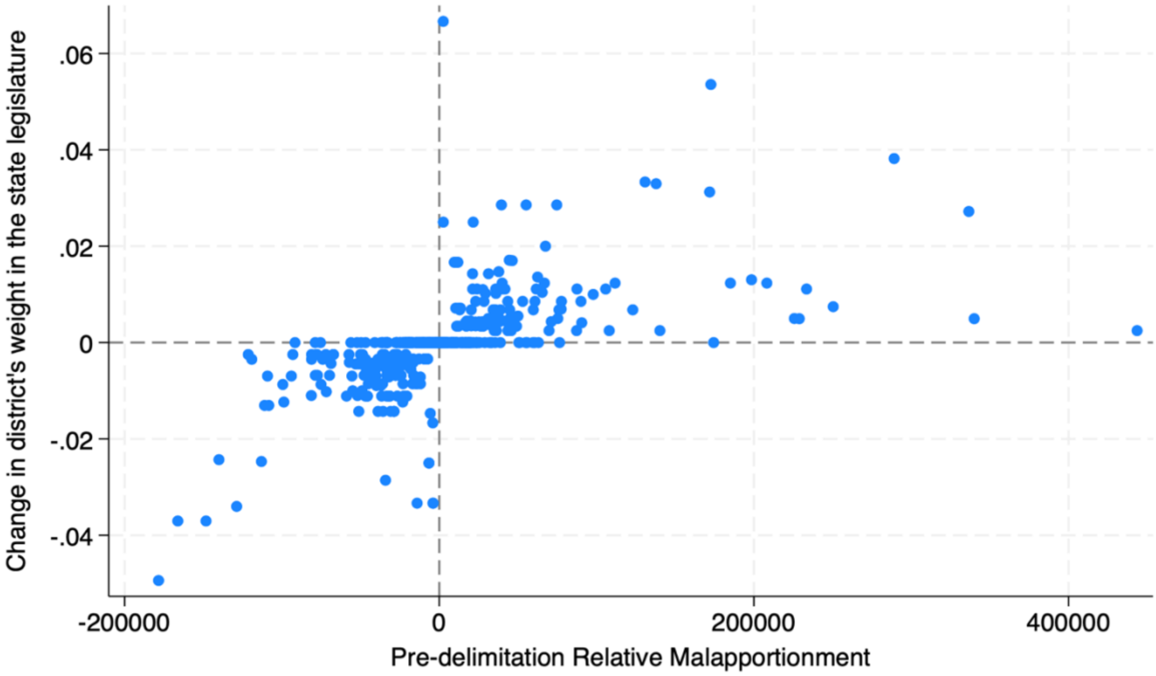


Figure 2: This figure plots the relationship between pre-delimitation relative malapportionment (x-axis) and the change in a district's weight in the state legislature after delimitation (y-axis). The discontinuity at zero reflects the intent-to-treat effect of assignment to under- versus over-represented status, rather than a sharp treatment-on-the-treated change, since not all districts adjusted fully in line with their pre-delimitation malapportionment.

Appendix

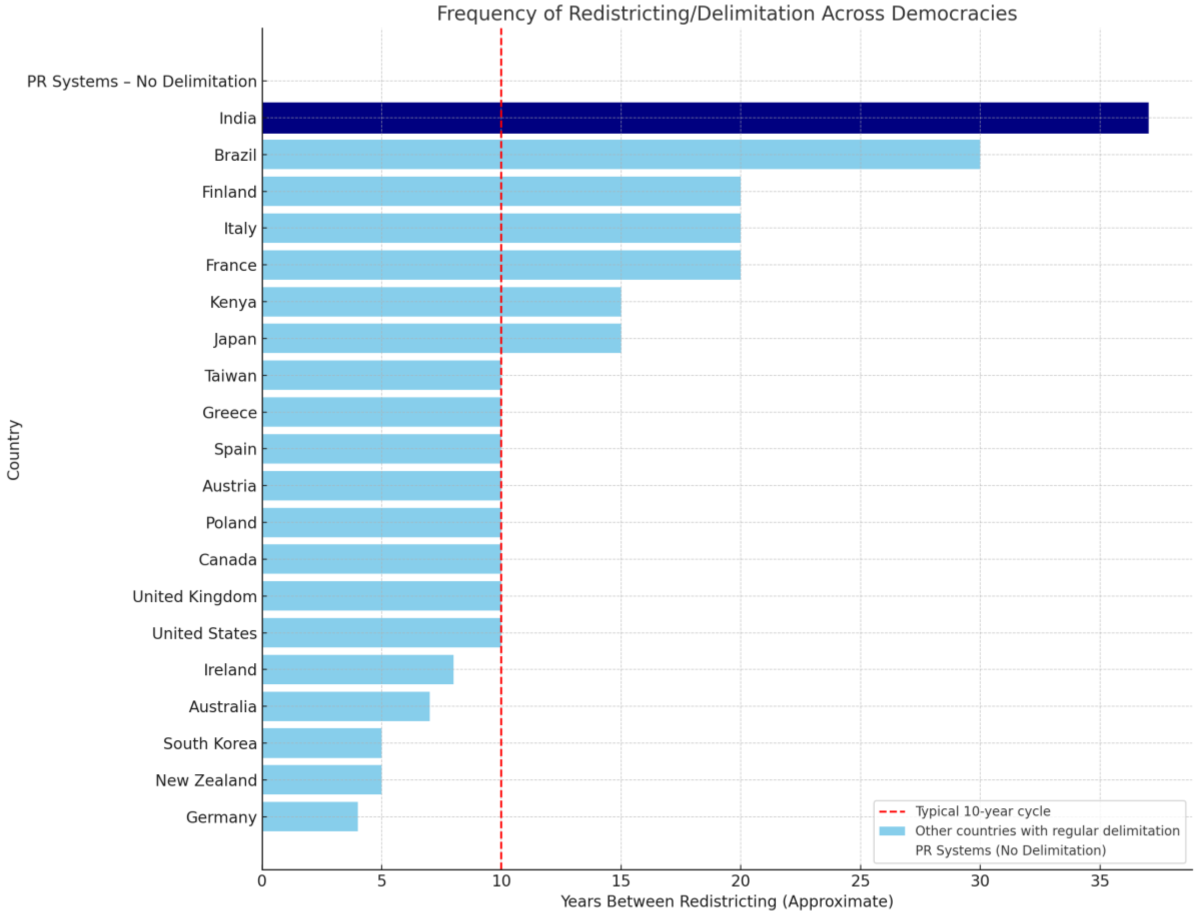
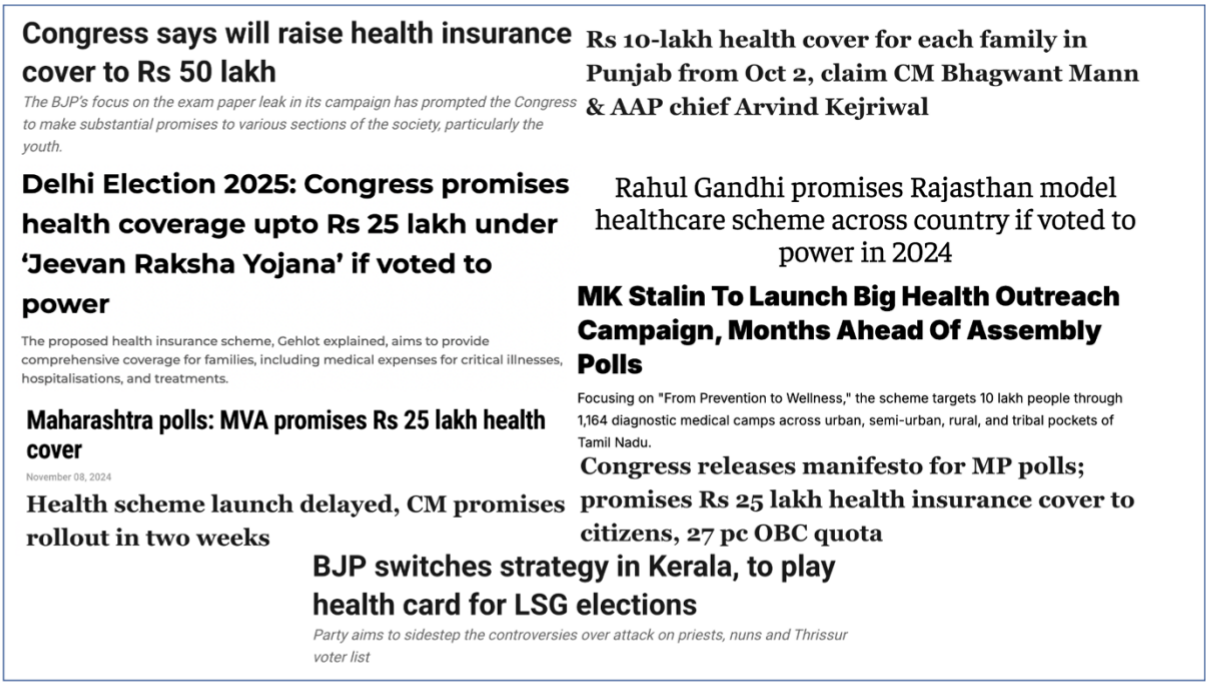


Figure A1: Frequency of Redistricting/Delimitation Across Democracies (Years Between Exercises)



Congress says will raise health insurance cover to Rs 50 lakh

The BJP's focus on the exam paper leak in its campaign has prompted the Congress to make substantial promises to various sections of the society, particularly the youth.

Rs 10-lakh health cover for each family in Punjab from Oct 2, claim CM Bhagwant Mann & AAP chief Arvind Kejriwal

Delhi Election 2025: Congress promises health coverage upto Rs 25 lakh under 'Jeevan Raksha Yojana' if voted to power

The proposed health insurance scheme, Gehlot explained, aims to provide comprehensive coverage for families, including medical expenses for critical illnesses, hospitalisations, and treatments.

Rahul Gandhi promises Rajasthan model healthcare scheme across country if voted to power in 2024

MK Stalin To Launch Big Health Outreach Campaign, Months Ahead Of Assembly Polls

Focusing on "From Prevention to Wellness," the scheme targets 10 lakh people through 1,164 diagnostic medical camps across urban, semi-urban, rural, and tribal pockets of Tamil Nadu.

Maharashtra polls: MVA promises Rs 25 lakh health cover

November 08, 2024

Congress releases manifesto for MP polls; promises Rs 25 lakh health insurance cover to citizens, 27 pc OBC quota

Health scheme launch delayed, CM promises rollout in two weeks

BJP switches strategy in Kerala, to play health card for LSG elections

Party aims to sidestep the controversies over attack on priests, nuns and Thrissur voter list

Figure A2: Political Prominence of State Health Programs in Electoral Campaigns
Selected headlines from national and state media (2023–25) illustrate how political parties across India prominently feature state-sponsored health insurance schemes and outreach campaigns in their manifestos and campaign strategies. Sources include The New Indian Express (Nov 22, 2023), Financial Express (2024), Times of India (2024–25), NDTV (2024), Citizen Matters (2024), Economic Times (2024), and others.
Integration into Results Section

Table A1: DLHS 2 – Service Utilization

	(1) Received ANC	(2) Went for ANC	(3) Received Postpartum Care	(4) Went for Postpartum Care	(5) Received Delivery Care
Conventional	-0.014	-0.003	-0.003	0.003	-0.003
	(0.011)	(0.013)	(0.007)	(0.008)	(0.007)
Bias-corrected	-0.015	-0.003	-0.002	0.003	-0.001
	(0.011)	(0.013)	(0.007)	(0.008)	(0.007)
Robust	-0.015	-0.003	-0.002	0.003	-0.001
	(0.012)	(0.014)	(0.008)	(0.008)	(0.008)
Observations	42000	40811	42000	40811	42000
Effective Obs. (left)	22482	22058	23195	22321	23195
Effective Obs. (right)	14907	16337	15062	16337	14907
Bandwidth	71013.85	80426.77	75000.59	85458.57	74203.68

This table reports local linear regression discontinuity estimates at the malapportionment cutoff using DLHS-2 data. Outcomes include whether women received antenatal care, visited a facility for antenatal care, received postpartum care, visited a facility for postpartum care, and received delivery care. Conventional, bias-corrected, and robust confidence intervals are shown following Calonico, Cattaneo, and Titiunik (2014). Standard errors are clustered at the district level. No coefficient is statistically significant at conventional levels. Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Health Infrastructure (DLHS-2)

	(1)	(2)	(3)	(4)
	PHC	Sub-centres	Dispensaries	Hospitals
Conventional	-0.029	-0.013	-0.022	-0.001
	(0.024)	(0.033)	(0.028)	(0.015)
Bias-corrected	-0.032	-0.011	-0.027	-0.003
	(0.024)	(0.033)	(0.028)	(0.015)
Robust	-0.032	-0.011	-0.027	-0.003
	(0.027)	(0.035)	(0.030)	(0.017)
Observations	13622	13622	13622	13622
Effective Obs. (left)	6948	7032	7004	6920
Effective Obs. (right)	5252	5252	5252	5224
Bandwidth	71061.93	74502.94	72521.26	69188.71

This table reports RD estimates for the presence of primary health centres, sub-centres, dispensaries, and hospitals using the DLHS-2 facility survey. Estimation follows the same procedure as in Table A1. Standard errors in parentheses, clustered at the district level. No discontinuity is detected for any health infrastructure variable. Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Socio-demographic Covariates (Census 2001 PCA)

	(1) Total Population	(2) SC	(3) ST	(4) Literate	(5) Total Workers	(6) Main Workers	(7) Marginal Workers
Conventional	-5.95e+04	-2.39e+04	44941.324	-5.09e+04	22620.301	5992.043	17754.038
	(2.08e+05)	(43845.489)	(39841.031)	(1.24e+05)	(78152.833)	(63619.394)	(19595.301)
Bias-corrected	-8.99e+04	-2.38e+04	36119.580	-6.40e+04	17266.199	1494.114	17325.939
	(2.08e+05)	(43845.489)	(39841.031)	(1.24e+05)	(78152.833)	(63619.394)	(19595.301)
Robust	-8.99e+04	-2.38e+04	36119.580	-6.40e+04	17266.199	1494.114	17325.939
	(2.21e+05)	(47132.063)	(41456.996)	(1.31e+05)	(83309.859)	(67669.765)	(20769.048)
Observations	516	516	516	516	516	516	516
Effective Obs. (left)	271	275	253	270	275	273	273
Effective Obs. (right)	208	209	193	206	209	208	208
Bandwidth	92245.09	105169.95	65286.98	87882.74	99971.06	94223.72	96736.30

This table reports RD estimates for population, caste composition, literacy, and workforce characteristics using the 2001 Census Primary Census Abstract. All specifications use local linear regressions with MSE-optimal bandwidths. Standard errors in parentheses, clustered at the district level. Estimates show no evidence of imbalance across the cutoff. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Village-level Infrastructure (Census 2001 Village Directory)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Primary Schools	Middle Schools	Secondary Schools	Senior Secondary Schools	Hospital	Dispensary	Maternity and child Welfare	Child Welfare Centres	Health centres	Primary Health Centres	Health Sub-centres
Conventional	5.202	-6.112	-0.245	-3.010	-3.147	-2358.415	0.439	13.163	-0.689	-0.363	40.754*
	(134.191)	(43.416)	(18.617)	(4.291)	(3.573)	(17776.32)	(13.421)	(17.412)	(2.360)	(4.380)	(22.180)
Bias-corrected	-10.271	-8.938	-0.544	-2.504	-3.769	-4317.924	-2.259	11.272	-0.899	-1.130	42.036*
	(134.191)	(43.416)	(18.617)	(4.291)	(3.573)	(17776.32)	(13.421)	(17.412)	(2.360)	(4.380)	(22.180)
Robust	-10.271	-8.938	-0.544	-2.504	-3.769	-4317.924	-2.259	11.272	-0.899	-1.130	42.036*
	(141.975)	(47.191)	(20.084)	(4.582)	(3.813)	(18761.67)	(13.988)	(18.427)	(2.523)	(4.661)	(23.757)
Observations	507	507	507	507	507	507	507	507	507	507	507
Effective Obs. (left)	236	239	239	237	235	239	228	239	237	235	239
Effective Obs. (right)	236	239	240	236	232	239	229	241	236	235	239
Bandwidth	92192.34	101843	106820.09	96158.24	82271	102270.98	74830.26	107168.5	94287.6	88025.1	102174.84

This table reports RD estimates for the presence of educational and health facilities using the Census 2001 Village Directory. Outcomes include counts of primary, middle, secondary, and senior secondary schools; hospitals, dispensaries, maternity and child welfare centres, child welfare centres, health centres, primary health centres, and health sub-centres. Coefficients are small in magnitude and statistically indistinguishable from zero, consistent with covariate balance at the cutoff. Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$