Government Performance under Elite Capture*

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Abstract

I examine the effects of removing anonymity from the process of electing political representatives. Identification comes from a unique policy experiment in rural India that incentivizes local governments to elect their political representatives via public consensus as opposed to a secret ballot, and discourages multiple candidates from running for office. Consistent with the literature, we find that the move away from a secret ballot leads to the capture of political office by the local elite. We build on this finding in a novel way by examining the performance of the local government when its electorate is faced with a negative economic shock. We find that the implementation of a workfare scheme is significantly worse in villages that faced higher incentives to elect their representatives via public consensus.

Keywords Political Economy, Decentralization, Electoral Competition, Secret Ballot, Public Employment Programs, Welfare Programs

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1 Introduction

Decentralization reforms have swept across much of the developing world in the last few decades (Bardhan, 2000). Institutions that systematically marginalize the local elite often accompany these reforms, in order to prevent the capture of political office. This is due to the expectation that elite leadership would irrefutably decrease social welfare. However, the debate on the effects of elite capture on social welfare is far from settled. As Alatas et al (2013) note, local leaders may enjoy advantages in leadership skills, monitoring ability and socioeconomic capital over their counterparts. Their occupation of political office could, therefore, lead to improved governance and raise overall welfare.

In this paper, I examine the effects of a policy that crowds the local elite into political office, not just on politician identity, but also on government performance. I study a unique policy experiment in rural India that incentivizes village residents to elect their political representatives via public deliberation and discussion, as opposed to voting via a secret ballot. Empirical evidence suggests that the secret ballot is an effective safeguard against the elite capture of political institutions (Baland and Robinson, 2008; Beath et al, 2015). I leverage the existence of a village population threshold beyond which financial incentives for public consensus elections sharply increase. I then examine the identity of politicians and how they perform once in office by comparing villages on either side of the threshold. In India, negative rainfall shocks are powerful predictors of wage and employment decreases. As my measure of government performance, I focus on how many households are provided employment when a village is faced with a negative rainfall shock.

I first document important effects on the identity of politicians - politicians chosen via public consensus (as opposed to a secret ballot) are more likely to be male as well as more educated. In India, women have not traditionally occupied powerful positions in the public sphere, and education is strongly correlated with economic status. Next, I examine how responsive elected governments are to local economic conditions. When faced with a negative rainfall shock, I find that village governments elected via public deliberation provided significantly fewer households with jobs under a rural employment program.

Whether elite capture has a sizeable negative effect on social welfare is an open empirical question. The literature can be roughly divided into two camps, based on the nature of their findings. The first group of papers finds that the negative consequences of elite capture are sizeable and important (Besley et al, 2012; Acemoglu et al, 2014), and that these may be especially critical during times of economic distress (Caeyers and Dercon, 2012). The second camp finds that the costs of elite capture are either small or completely absent (Alatas et al, 2013; Bardhan and Mookherjee, 2006). This paper finds evidence of important negative effects of elite capture, thereby weighing in on the side of the first set of papers.

This paper highlights the potential effects of elite capture, but also contributes to our understanding of the link between political institutions and economic outcomes. A closely related literature links the secret ballot to a reduction in the influence of the elite on policy decisions (Baland and Robinson, 2006; Beath et al, 2016), but lack of data prevents them from concluding that these decisions lowered social welfare. This paper confirms their results on the heightened influence of elites as we move away from secret ballots, and builds upon them in a concrete way by focusing on a metric for government performance.

Why would elite capture lower the counter-cyclicality of a social security scheme? A counter-cyclical social security scheme is one of many policy tools that incumbent politicians can use to increase their chances of re-election. However, if the public consensus election increases the ability of the local elite to retain political power over time, they may be less incentivized to appease voters through improved governance. This is especially likely to be true if the primary users of social security schemes are the non-elite, while the elite determine who assumes political office.

In order to arrive at causal estimates of elite capture, I exploit the variation generated by a policy experiment in rural India. In particular, I use the fact that villages with populations greater than 5000 face substantially larger incentives for public consensus elections. This allows me to compare villages that faced higher incentives to elect their political representatives via public deliberation with those that faced lower incentives. That is, I set up a sharp regression discontinuity design, which assumes that villages close to the threshold of 5000 are as good as randomly assigned on either side of the population cutoff.

This setting also allows me to circumvent a common drawback of regression discontinuity designs - contamination by multiple treatments. Three features of the local political system change at the population threshold of 5000 - the election incentive grant, the number of political representatives, as well as reservations for women. Fortunately, the number of political representatives and reservations for women also change discontinuously at population thresholds other than 5000. These alternative thresholds can then be used to estimate the effects of these two contaminating treatments, which in turn can be differenced out from the estimate found at the threshold of 5000.

The rest of this paper is organized into four sections. Section 2 describes the institutional setting, as well as our data sources. Section 3 details the empirical specification, while results are presented in Section 4. Section 5 concludes.

2 Setting

This section provides detailed information about the functioning of village governments in Gujarat, the implementation of the Samras (Unanimous) Panchayat scheme, as well as the datasets

used in the empirical analysis.

Institutional Background

The Seventy Third Amendment to the Indian Constitution in 1992 mandated the creation of a three tiered local government system, at the district, block and village level (in descending order of size of jurisdiction) in all states in India. Our study focuses on elected councils at the village level, also called Gram Panchayats (henceforth, GPs) in Gujarat. GPs are directly elected by village residents, for a term of five years. The GP area as a whole directly elects the president of the village council. Further, the jurisdiction of each GP is divided into a number of wards, where efforts are made to ensure that the population of each ward is the same, as far as possible. Each ward elects a single representative. In Gujarat, the number of ward members is fixed at 7 for GPs with populations up to 3000, and increases by 2 for every multiple of 1000 thereafter. Figure 1 plots a fitted graph of the number of GP members elected in the 2011 elections against GP population in Gujarat.

GPs can collect some taxes and fees within their jurisdiction. However, most of their revenue comes in the form of grants from central and state governments. GPs are responsible for the provision and maintenance of various local public goods such as roads and irrigation canals, as well as the upkeep of sanitation services at the village level. They are also responsible for implementing social welfare programs like the National Rural Employment Guarantee Act (henceforth, NREGA), which can include identifying program beneficiaries. GPs are also required to organize and preside over two town-hall style meetings (called Gram Sabhas) every year.[1]

The 73rd Constitutional Amendment of India mandated reservations for women, as well as two disadvantaged minorities - Scheduled Castes and Scheduled Tribes - in Panchayats at the district, block and village level. At least 33% of president and council member seats were to be reserved for women. For Scheduled Castes and Scheduled Tribes, the proportion of reserved seats was to be as close as possible to their respective population shares in the state. The system of rotating, randomized reservations created exogenous variation in politician identity, and has lead to a large literature linking politician identity to policy outcomes such as public good provision (Chattopadhyay and Duflo, 2004; Besley et al. 2012; Rajaraman and Gupta, 2012; Dunning and Nilekani, 2013). In this paper we focus on an understudied aspect of the functions of local governments in general, and GPs in particular - their ability to tailor the implementation of social security nets to local labor demand shocks.

In the Indian context, NREGA is a large scheme that guarantees one hundred days of employment a year to each rural household. Both the nature and timing of public works, as well

[1] All village residents are invited to attend these meetings. In practice, these meetings are neither regularly held, nor well attended.

as beneficiaries of the program are to be decided upon by GPs.[2] This scheme has been studied extensively, with documented effects on consumption smoothing (Oldiges and Heidelberg, 2014), poverty reduction (Imbert and Papp, 2015), and the increased use of risky, profitable crops (Gehrke, 2013; Hari and Raghunathan, 2014).

The outcome that we analyze is closely related to Johnson (2009), who documents the responsiveness of employment generated under the scheme in response to weather induced income shocks in Andhra Pradesh, a southern Indian state. Since rural Gujarat is largely dependent on rain-fed agriculture, rainfall shocks during the monsoon season are powerful predictors of changes in wages and labor demanded in the months that follow.[3] We examine patterns of work provision (under NREGA) to rural households and individuals when villages are faced with negative rainfall shocks, and whether these differ depending on the mode of election of the local government.

Consensus Elections

Financial incentives that encourage consensus elections have been offered by many state governments in India, for differing periods of time. For instance, Andhra Pradesh has offered incentives since 1964, while more recent implementers include Punjab and Haryana, who first offered incentives in 2008 and 2010 respectively.

This study focuses on Gujarat, a state in Western India, for three reasons. One, the incentive amount jumps discontinuously beyond a certain population level. This is not true for states like Himachal Pradesh and Punjab. Two, the population distribution around the cutoff point 5000 is dense enough to be able to conduct our empirical analysis.[4] This is not true for Andhra Pradesh, where grant amounts increase discontinuously if population exceeds 15,000; the population distribution around the cutoff point 15,000 is extremely sparse - only 64 GPs lie within the population range $[14,000, 16,000]$.[5] Third, the scheme has been well implemented in Gujarat since 2001, so it is fair to assume that village residents understand its functioning, and believe that the government will pay out the promised grant amounts. Credibility has been a problem with recent implementers such as Punjab and Haryana, who failed to pay out the grants after the 2010 elections.[6]

Since 2001, Gujarat has provided villages with financial incentives to choose their GPs via con-

[2]The NREGA budget of each GP is calculated using 100 days of employment per rural household at a fixed wage. This budget is rarely exhausted, and the target of 100 days per year is usually not met.

[3]For instance, the State Rural Development Department reported that NREGA employment was amped up across the state in response to the recent drought in 2015.

[4]Figure 2 plots the distribution of GP population based on the 2001 Census.

[5]This estimate is based on village level population and GP Names provided in the 2011 Census.

[6]The state government is legally obligated to pay these amounts. See http://indianexpress.com/article/india/india-others/hc-rap-for-govt-for-failure-to-pay-panchayat-incentive for an instance where a legal case was filed against the Punjab government. As this case demonstrates, however, it may take many years for a legal case to be processed in court, and even more time before the state government complies with the court's orders.

sensus elections. That is, they encourage the village to hold public discussions and decide who should occupy each political seat. This prevents multiple political candidates from running for election, and the sole candidate to file nomination papers is declared as the *unanimous* winner. The policy has been fairly successful. In the 2011 elections, over a seventh of all GPs in Gujarat were elected *unanimously*.[7] This means that each council seat in these GPs was filled by someone who, at least on paper, faced no competition in getting elected.

The Gujarat State Election Commission (henceforth, Gujarat SEC) lists two reasons why it incentivizes consensus elections. The first is to promote unanimity and brotherhood among village residents, by preventing electoral conflicts. The second reason is cost-effectiveness, both for the candidate himself, as well as the Gujarat SEC. On the one hand, political candidates save time and money that they would have expended during the campaign period. The Gujarat SEC, too, benefits financially - a single candidate for a political post eliminates the need to set up polling booths and hire the associated personnel. These expenses are described in detail below.

All details come from the Gujarat Panchayati Raj Act (1994). GP elections are conducted following a few steps. First, the Gujarat SEC notifies the GP about which seats are reserved for women, SCs and STs. An individual can contest the election if he or she belongs to the reserved category, or if the seat is unreserved. Interested candidates are invited to file nomination papers[8] within a few days of the initial announcement. All nominations are scrutinized to ensure that they satisfy the eligibility criteria, which vary by state. In Gujarat, candidates below the age of 21, or those who are not registered as voters, cannot stand for election in GPs. Candidates have a few days to appeal against the rejection of their nomination papers, as well as run election campaigns. At the end of this period, polling booths are set up within the GP. The day of polling is usually declared as a local holiday. Every individual above the age of 18 who is registered as a voter is eligible to vote in GP elections. Efforts are made to count votes on the same day as polling. Electoral personnel must be hired to ensure free and fair polls, which can include the scrutiny of nomination papers and election expenditure, detection and prevention of voter impersonation, maintenance of voting secrecy, scrutiny of doubtful/invalid votes, supervision of counting and recounting, as well as the declaration of final vote shares.

If the GP is able to agree upon a single candidate for a given political position, only one candidate files nomination papers. The state government benefits financially from this event, because it eliminates the need to undertake substantial election expenses, including the hire of election personnel described above.

The state government encourages consensus elections by providing untied grants to unanimously elected councils. The grant amount increases discontinuously with population, is

[7] There were over 13,000 GPs in Gujarat in 2011 - just under 2,000 were elected *unanimously*.

[8] Attach a sample (translated) nomination paper.

higher if an all-women council is chosen, and increases if the council is chosen unanimously for the second or third time.[9] Table 1 displays the grant amounts for each of these categories, in thousands of Indian rupees.[10] The grant amount is paid only if each and every GP member is elected unanimously. This means that, on average, a GP must agree upon eight people as ward members and a President, and these people will be the only ones to file official nomination papers to run for election.

Table 1: Samras Incentive Amounts by GP Type

Population	First Time	First Time All-Women	Second Time	Second Time All-Women	Third Time	Third Time All-Women
≤ 5000	200	300	250	375	312.5	468.75
> 5000	300	500	375	625	468.75	781.25

Table 1 shows that the incentives to unanimously elect the entire GP increase discontinuously as population increases beyond 5000, irrespective of the type of GP. This discontinuity in incentives is exploited to set up a regression discontinuity design, which is described in detail in Section 3.

Datasets

Multiple datasets are combined to be able to perform the empirical analysis. The 2001 Population Census provides village-level characteristics, including demographic information and public good availability. GPs may contain more than one village, and are mapped to villages using the Local Body Mapping data obtained from the Area Profiler website managed by the Ministry of Panchayati Raj.

Information on the 2011 GP elections was obtained from the Gujarat State Election Commission. This includes the list of unanimously elected GPs, as well as detailed information on each political candidate[11] including reservation category, gender, education and occupation. Since these datasets were available only in Gujarati, they were manually merged with the Local Body Mapping dataset described above. We are able to successfully match over 85% of the unanimously elected GPs with the Population Census. We assume that all other GPs are *not* elected unanimously.[12]

[9] The grant amounts for consensus elections were first introduced in 2001. Therefore, it is not possible for any given GP to be elected *unanimously* for the fourth time.

[10] 1000 rupees is approximately $15, and $45 in PPP terms.

[11] Information at the candidate level is available for over 75% of GPs.

[12] This is a reasonable assumption for our RD analysis, since the empirical strategy focuses only on observations within tight bandwidths of each cutoff. Less than 5 unmatched GPs lie within the bandwidths used in the Results section.

Monthly rainfall for each sub-district, measured at the closest point on a 0.5° latitude by 0.5° longitude grid, is from the Center for Climatic Research at the University of Delaware. Information on the monthly implementation of NREGA for 2011-15 is downloaded from its Public Data Portal. This includes information on the number of households and individuals who were provided work in each month. Unfortunately, it does not provide information on the number of days worked by each household or individual. However, we believe that the number individuals and households who were provided employment are good proxies for the implementation of the scheme, because the unconstrained NREGA budget of each GP ensures that the primary implementation constraint is due to supply of work by GPs, not demand for work (Gupta et al 2015).

3 Empirical Strategy

This section describes the implementation of the regression discontinuity design with multiple cutoffs. As noted above, the incentives for consensus elections increase discontinuously as population in the GP exceeds 5000. We also noted the number of GP members increases discontinuously as GP population exceeds 3000, and for every multiple of 1000 thereafter. Figure 1 plots the relationship between the number of GP members and population. We exploit the fact that we can observe the effects of additional members at cutoffs apart from 5000 to set up a difference-in-RD design - that is, we can isolate the impact of consensus elections on electoral and policy outcomes by differencing out the effect of additional members.[13]

We follow the suggestions of Hahn, Todd, and Van der Klaauw (2001) and Imbens and Lemieux (2008), and employ local linear regressions after restricting attention to observations close to each cutoff.[14] The identifying assumption is that unobservables vary smoothly at the cutoffs.

Let pop_{GP} denote the population of the Gram Panchayat. For ease of notation, we define a rescaled version of the GP population as $pop_g = \frac{pop_{GP}}{1000}$. Our empirical specification takes the following form:

$$E_g = \sum_{c=1}^{5}[\beta_c 1[pop_g > c] + \alpha_{c0} pop_g + \alpha_{c1}(pop_g - c)1[pop_g > c] + \gamma_c]1_{cb} + X_g + \epsilon_g$$

$$1_{cb} = 1[c - b_c \leq pop_g \leq c + b_c] \qquad c = 1, 2...5 \qquad b = \text{Bandwidth}$$

E_g denotes an election outcome at the GP level, such as proportion of unanimously elected seats, or an indicator for whether every seat was filled without opposition. Our specification controls for segment fixed effects γ_c, and also allows for distinct slopes to the left and right of each cutoff (α_{c0} and α_{c1} respectively). Of primary interest are the β_c coefficients, which test for discontinuities in E_g as population exceeds any of the 5 cutoffs. Since the prior literature

[13] This implicitly assumes that the interaction effects of the higher incentive and additional members are negligible.

[14] Results for higher order polynomial population controls are also reported in Section 4.

has documented effects of presidential reservations on the functioning of the GP, we include a vector of reservation category controls X_g, but most of our results are robust to their exclusion. This regression is run on data that is restricted to close neighborhoods 1_{cb} of each cutoff $Consensus$, defined by the bandwidth b_c. The choice of bandwidth is discussed in detail below. Since the running variable is discrete, standard errors are clustered on pop_g.

Let $Additional\ Members$ denote the effect of two additional GP members. We estimate $Additional\ Members$ using the four cutoffs closest to 5000.

$$\hat{Additional\ Members} = \frac{\hat{\beta}_3+\hat{\beta}_4+\hat{\beta}_6+\hat{\beta}_7}{4}$$

We do not pool the cutoffs and estimate $Additional\ Members$ directly, since this would lead to an estimate of the weighted average of the effects at each cutoff, with higher weights given to cutoffs with more observations.[15] In our sample, this would bias the average effect towards that of lower cutoffs.

We are now ready to estimate the effect of consensus election incentives, denoted by $Consensus$. Assuming additivity, we have:

$$\hat{\beta_5} = \hat{Consensus} + \hat{A}$$
$$\implies \hat{Consensus} = \hat{\beta_5} - \frac{\hat{\beta}_3+\hat{\beta}_4+\hat{\beta}_6+\hat{\beta}_7}{4}$$

Section 5 reports estimates for β_5, $Additional\ Members$ and $Consensus$ for all outcomes. In most specifications, β_5 and $Consensus$ are similar in terms of magnitude and statistical significance, while estimates of $Additional\ Members$ tend to be small and insignificant.

Policy Outcomes

This section describes a slight variant of the above specification for NREGA outcomes. The outcome of interest is the responsiveness of NREGA implementation to bad rainfall shocks. Since both the timing and quantity of rainfall are important determinants of agricultural employment and output, we focus on rainfall in the first month of the monsoon (July) in Gujarat.[16] To account for possible non-linearities in the production function with respect to rainfall, we follow the literature[17] and define a rainfall shock as rainfall lying below the 20^{th} percentile of the sub-district's rainfall distribution over the period 2000-2014. We assume that poor rainfall affects agricultural employment negatively for the next twelve months, with a fresh cycle beginning in July of the following year.

First, we show that the probability of facing a negative rainfall shock does not change discontinuously at any of the cutoffs.

[15] This is discussed in detail in Cattaneo et al. (2015).

[16] Over 80% of annual rainfall falls in the rainfall season, The monsoon season in Gujarat begins in July and lasts through September; July receives over 40% of all rainfall

[17] See Jayachandran (2006), Kaur (2014) and Santangelo (2016) for similar definitions.

$$NegativeShock_{gy} = \sum_{c=1}^{5}[\beta_c 1[pop_g > c] + \alpha_{c0} pop_g + \alpha_{c1}(pop_g - c)1[pop_g > c] + \gamma_c]1_{cb} + \epsilon_{gy}$$

Next, we turn to NREGA implementation outcomes. The NREGA Public Data Portal provides information on employment provided at the annual and monthly level. Annual NREGA data corresponds to the period April-March of each year. Since we would like to estimate the impact of rainfall on NREGA implementation for the period July-June, we focus on the monthly implementation data.

We restrict attention to GPs that faced negative rainfall shocks. This is to ease the interpretation of results. For instance, we may not want to interpret a reduction in NREGA employment during years of positive rainfall shocks as a good response. We then examine whether GPs that faced higher incentives to adopt consensus elections differed in their implementation of NREGA:

$$NREGA_{gmy} = \sum_{c=1}^{5}[\beta_c 1[pop_g > c] + \alpha_{c0} pop_g + \alpha_{c1}(pop_g - c)1[pop_g > c]]1_{cp} + \gamma_{my} + \epsilon_{gmy}$$
$$\text{I}\, NegativeShock_{gy} = 1$$

Since the regression specification utilizes multiple observations of each GP, we cluster on the discrete running variable population.[18] Month-year fixed effects absorb seasonality in employment, and make it clear that we are pooling multiple cross-sections.

Choice of Bandwidth

All regression specifications above restrict attention to observations within close proximity of each cutoff. For each outcome, optimal bandwidths for each cutoff are selected separately, following the procedure prescribed by Calonico et al. (2013). In addition, we report results for bandwidths that are smaller than the optimal ones, in order to minimize bias. This is because data-dependent bandwidth selection procedures may recommend bandwidths that are too large for the usual distributional approximations invoked in the literature to be valid (Calonico et al. 2013).

4 Results

Baseline Continuity Tests

We first test for evidence of sorting around the population cutoff. For instance, GPs may want to be listed as having more than 5000 residents, to receive larger samras grant amounts. However, this is unlikely to be the case in our setting, because the running variable is taken from the Population Census that was conducted 10 years prior to the introduction of the discontinuous incentives. Figure 2 presents the density of population surrounding the 5 cutoffs used in our analysis. Population is collapsed into bins of width 10, and no discontinuity in the vicinity of

[18] There is no need to cluster on an additional variable - the GP itself - since the running variable is fixed for the period under consideration, and hence, absorbs the identity of the GP.

any of the thresholds is evident. Since our running variable is discrete, we test for manipulation of population close to the cutoffs following the method suggested by Frandsen (2014).[19] Testing for discontinuities in the distribution of population close to thresholds, the p-values for each of these tests is above 0.2, and are reported along with Figure 2.

Balance tests for village demographics, public good availability and GP President reservation categories are presented in Table B.1. We test for discontinuities at the threshold 5000, and also report the average of the discontinuity estimates at other cutoffs. Among the 37 tests, 4 yield statistically significant estimates, which is to be expected at the 10% level of significance. Figures 3 and 4 present graphical evidence of balanced covariates for a subset of these variables.

Unanimous Elections

This section provides evidence that the samras grant was able to incentivize village residents to choose their political leaders without formal elections. We focus on two electoral outcomes. The first outcome is the proportion of GP members that were elected by consensus. This outcome measures the degree to which the financial incentive may have influenced village residents to choose their political leaders without formal elections, even if they did not manage to elect every single politician this way.

$$Prop\_Unopposed_g = \sum_{c=1}^{5}[\beta_c 1[pop_g > c] + \alpha_{c0} pop_g + \alpha_{c1}(pop_g - c)1[pop_g > c] + \gamma_c]1_{cb} + X_g + \epsilon_g$$

X_g includes three indicators for reservations for the post of GP President for three distinct categories - women, Scheduled Castes and Scheduled Tribes. Table E.1 reports results for specifications with and without these controls, while Figure 5 presents graphical evidence. The hike in incentive increases the proportion of GP members elected via consensus elections by 20%. We report the robustness of these results to higher order polynomial population controls in Table E.3.

The second outcome is an indicator for whether every GP member was chosen by consensus. This is the situation in which the GP would receive the samras grant.

$$1(All\ Unopposed)_g = \sum_{c=1}^{5}[\beta_c 1[pop_g > c] + \alpha_{c0} pop_g + \alpha_{c1}(pop_g - c)1[pop_g > c] + 1]\gamma_{cb} + X_g + \epsilon_g$$

Table E.2 reports results for specifications with and without presidential reservation controls, while Figure 5 presents graphical evidence. The hike in incentive increases the probability of a GP electing all of its representatives via consensus elections by over 10%. We report the robustness of these results to higher order polynomial population controls in Table E.3.

[19] The McCrary (2008) test, which is commonly used to test for sorting around the relevant thresholds, assumes a continuous running variable. In the case of a discrete running variable, it may falsely reject the null of no manipulation at too high a rate.

The financial incentive may work in yet another way - it may reduce the number of candidates running for election, even if that number does not fall all the way to 1. Table E.4 reports results for the following specification.

$$[\tfrac{Candidates}{Seats}]_g = \sum_{c=1}^{5}[\beta_c 1[pop_g > c] + \alpha_{c0} pop_g + \alpha_{c1}(pop_g - c)1[pop_g > c] + \gamma_c]1_{cb} + X_g + \epsilon_g$$

We find strong evidence that the average number of candidates for each GP member seat falls by at least 0.5 due to the financial incentive. This is a large effect, given that the average number of candidates per ward member seat is around 2. The analogous estimates for the post of GP President are negative, but insignificant.

Elected Leader Characteristics

This section examines whether consensus elections changed the observable characteristics of elected leaders. Since the GP President post tends to be heavily contested (much more so that GP member posts), we focus on the gender and education of the GP President. We find evidence that consensus elections crowd out female politicians and usher in more educated candidates.

Our regression specification takes the form

$$[President\,Characteristic]_g = \sum_{c=1}^{5}[\beta_c 1[pop_g > c] + \alpha_{c0} pop_g + \alpha_{c1}(pop_g - c)1[pop_g > c] + \gamma_c]1_{cb} + X_g + \epsilon_g$$

Table P.1 shows strong evidence that the incentive amount led to an increase in President education by approximately 4 years. This is a sizable effect, since GP Presidents on average have fewer than 8 years of education. Table P.2 presents weak evidence showing that although the samras scheme offered additional incentives for female politicians, the scheme may have crowded them out of the political sphere. Figure 6 presents our estimates at each of the 5 cutoffs - the β_5 estimate clearly breaks away from the estimates at other cutoffs, indicating that consensus election incentives may be crowding in male politicians that are more educated.

Consensus elections could crowd in politicians that are male and/or are more educated for two reasons. First, the majority of rural residents may consider these characteristics to be desirable for an effective political leader, and public deliberation helps shift candidates with these characteristics into political office. Under this hypothesis, we would expect to see publicly elected GPs perform better than GPs elected via secret ballot. Second, the local elite may be able to increase their influence over the electorate, since votes are now observable. For instance, they may be able to punish a vote against them, which would not be the case with a secret ballot. The hypothesis of elite capture is consistent with both the crowding out of female politicians (since women have historically not occupied powerful positions in the public sphere) and more educated politicians (since education is positively correlated with income). Under this scenario, we

would not expect to see government performance improve under the publicly elected GP. In order to separate between the two hypotheses, we turn to the implementation of the workfare program NREGA.

Program Implementation

We first show that the probability of facing a negative income shock is not significantly different for GPs that face different incentives under the samras scheme. In our setting, this is trivially achieved, since all GPs in Gujarat received less than the 20^{th} percentile of rainfall in July in 2012, and all GPs received above this level of rainfall in the other years 2011, 2013 and 2014.[20] Therefore, the probability of facing a negative rainfall shock is unaffected by whether the GP faces higher incentives to partake in consensus elections. This allows us to focus on GPs that faced negative rainfall shocks (all GPS in 2012) and interpret the difference in program implementation as a differential response to local negative economic shocks.

NREGA is meant to complement market demand for labor, and provide income insurance for families close to the poverty line. This insurance role is enhanced when demand for labor is low. Therefore, we test whether the responsiveness of NREGA is altered depending on the mode of election of the government. Our regression specification is

$$NREGA_{gmy} = \sum_{c=1}^{5}[\beta_c 1[pop_g > c] + \alpha_{c0} pop_g + \alpha_{c1}(pop_g - c)1[pop_g > c]]1_{cp} + \gamma_{my} + \epsilon_{gmy}$$

$$\text{where } NegativeShock_{gy} = 1$$

Tables N.1 and N.2 report results for two sets of outcomes - the number of households as well as number of individuals that were allotted work in a particular month in GP g. Due to the large number of observations of each GP, we are able to restrict attention to very small bandwidths, such as 1 or 2 per cent of each cutoff. The results for these bandwidths are reported as well.

The estimates suggest that when facing a negative labor demand shock, GPs that are offered higher consensus election incentives employ fewer households and people under the employment guarantee scheme. Figures 7 and 8 present evidence that this is a responsiveness issue, and not one of lower implementation overall - while there is a clear (negative) difference in implementation under a negative rainfall shock, there is no difference under normal rainfall conditions. Therefore, we find evidence against the improvement of government performance in publicly elected GPs. This supports the second hypothesis discussed above - consensus elections can facilitate the capture of political power by the local elite, who in turn have fewer incentives to improve governance.

[20] Rainfall data is not available for the year 2015.

Council Member Identities

So far, we have examined the effects of consensus elections on the identities of the council President, but have remained silent about those of the council members. Our analysis is complicated by the fact that reservations for council members themselves change with council size. Figure 10 displays the number of council members that are reserved for women, Scheduled Castes and Scheduled Tribes against population. The first panel shows that the number of female council members increases discontinuously at all cutoffs except 5000 and 8000. The bottom panel shows that the number of seats reserved for Scheduled Castes and Scheduled Tribes does not jump discontinuously at any of the population cutoffs.

The fact that two unreserved[21] seats are added at the cutoff 5000, while one of the seats added at each of the other cutoffs is always reserved for women could mean that the results discussed above may not be due to the consensus election incentives. Instead, they may be driven by the difference identity of additional council members. In this section, we provide some evidence that this mechanism is unlikely, by showing that the effects of adding two unreserved seats are not different from adding one unreserved seat and one female reserved seat.

Let the effect of adding two unreserved seats be denoted by $\beta_U + \beta_U = 2\beta_U$. We can estimate $2\beta_U$ using the cutoff 8000. Let the effect of an unreserved seat and a reserved seat be denoted by $\beta_U + \beta_R$. We can estimate $\beta_U + \beta_R$ by using the cutoffs 7000 and 9000, chosen for their proximity to the cutoff 8000.

Our empirical specification is analogous to the one discussed above

$$E_g = \sum_{c=7,8,9}[\beta_c 1[pop_g > c] + \alpha_{c0} pop_g + \alpha_{c1}(pop_g - c)1[pop_g > c] + \gamma_c]1_{cb} + X_g + \epsilon_g$$

$$1_{cb} = 1[c - b_c \leq pop_g \leq c + b_c] \qquad c = 7, 8, 9 \qquad b = \text{Bandwidth}$$

We then test that

$$2\beta_U = \beta_U + \beta_R$$
$$\text{or } \beta_8 = \tfrac{\beta_7 + \beta_9}{2}$$

Table R.1 reports estimates for $2\beta_U, \beta_U + \beta_R, \beta_U - \beta_R$ for all outcomes. In all specifications, the difference $\beta_U - \beta_R$ is not significant at the 10% level of significance. Figure 11 presents graphical evidence that the effect of two unreserved members is not different from the effect of two members when one of the seats is reserved for women.

5 Conclusion

We analyze the effects of introducing elements of deliberative democracy into the electoral process. In essence, we compare two forms of elections - public consensus elections with private

[21] In this section, we taken unreserved seats to mean that they are not reserved for women. Seats may be reserved for either Scheduled Castes or Scheduled Tribes.

secret ballot elections. Our setting is interesting because the choice of electoral system is left up to village residents themselves. To retrieve causal estimates, we exploit the existence of a population threshold, beyond which financial incentives for consensus elections sharply increase. Our results indicate that financial incentives can induce the electorate to choose their political leaders via public consensus. We also find that consensus elections crowd in more educated candidates, but may crowd out female politicians. Given an economic shock, governments chosen via public deliberation are worse at implementing social safety nets like workfare programs. These results are consistent with the literature documenting the negative effects of shifts away from anonymity in political processes (Baland and Robinson, 2006, Hinnerich and Pettersson-Lidbom, 2014, Beath et al. 2013).

In future work, we plan to investigate whether such institutional shifts are sticky - that is, whether a village that has chosen to elect its politicians via consensus elections is more likely to do so in the future, and whether long-term exposure to such a government has compounding effects on the welfare of its electorate.

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Tables

Table B.1: Baseline Covariates

Panel A: Demographics

Characteristic	β_5	$Additional\ Members$	Observations
Number of Households	52.16*	-16.42	1573
	(0.08)	(0.49)	
Male Population	-1.17	-0.27	1573
	(0.96)	(0.99)	
Female Population	1.17	0.27	1573
	(0.96)	(0.99)	
Scheduled Caste Population	33.44	19.77	1573
	(0.63)	(0.64)	
Scheduled Caste Male Population	17.9	9.32	1573
	(0.62)	(0.66)	
Scheduled Caste Female Population	15.54	10.45	1573
	(0.65)	(0.61)	
Scheduled Tribe Population	-447.85	27.49	1573
	(0.42)	(0.93)	
Scheduled Tribe Male Population	-233.26	15.15	1573
	(0.4)	(0.92)	
Scheduled Tribe Female Population	-214.59	12.33	1573
	(0.44)	(0.94)	

Panel B: Village Characteristics

Characteristic	β_5	$Additional\ Members$	Observations
Geographical Area	-151.03	-213.7	1573
	(0.74)	(0.35)	
Distance to Nearest Town	-0.06*	-0.04**	1573
	(0.08)	(0.03)	
Educational Facilities^{Notes}	0	0	1573
	.	.	
Medical Facilities	0.10	-0.05**	1573
	(0.22)	(0.03)	
Drinking Water Facilities^{Notes}	0	0	1573
	.	.	
Paved Road Approach^{Notes}	-0.01	0	1573
	(0.84)	(0.88)	
Power Supply	0	0	1573
	.	(0.18)	
Proportion of Irrigated Area	0.10	0.02	1573
	(0.13)	(0.66)	

Panel C: GP President Reservations

Characteristic	β_5	$Additional\ Members$	Observations
Female President	-0.10	-0.01	1055
	(0.58)	(0.94)	
SC President	0.03	-0.01	1055
	(0.54)	(0.80)	
ST President	-0.04	-0.03	1055
	(0.83)	(0.70)	

Notes: p-values are reported in brackets, and attention is restricted to a bandwidth of 200 around each cutoff. Educational Facilities, Drinking Water and Power Supply are available for all villages in this population range.

Election Outcomes

Table E.1 : Proportion of Unanimously Elected Politicians

Panel A: Including Presidential Reservation Controls

Bandwidth	Optimal	80% of Optimal	200	4% of Cutoff
β_5	0.18*	0.19*	0.18*	0.19*
	(0.07)	(0.08)	(0.08)	(0.08)
$Additional\ Members$	-2e-3	-0.02	-5e-3	0.01
	(0.97)	(0.78)	(0.94)	(0.91)
$Consensus$	0.18*	0.2*	0.19	0.18
	(0.1)	(0.09)	(0.12)	(0.13)
Observations	1248	984	1055	828
Controls	Yes	Yes	Yes	Yes

Panel B: Excluding Presidential Reservation Controls

Bandwidth	Optimal	80% of Optimal	200	4% of Cutoff
β_5	0.19**	0.22**	0.22**	0.22**
	(0.05)	(0.05)	(0.05)	(0.05)
$Additional\ Members$	0.01	-0.01	4e-3	0.02
	(0.84)	(0.82)	(0.94)	(0.76)
$Consensus$	0.18*	0.23*	0.21*	0.2
	(0.09)	(0.06)	(0.09)	(0.11)
Observations	1280	1012	1084	854
Controls	No	No	No	No

Notes: p-values are reported in brackets. This table reports the robustness of our estimates to a variety of bandwidths. The optimal bandwidths (in increasing order of cutoff) are approximately 200, 250, 250, 350 and 300. The sign and p-values of the estimates of β_5 and $Consensus$ are similar, while estimates of $Additional\ Members$ tend to be precisely estimated zeroes.

Table E.2: Unanimously Elected Gram Panchayats

Panel A: Including Presidential Reservation Controls

Bandwidth	Optimal	80% of Optimal	200	4% of Cutoff
β_5	0.10	0.12	0.11*	0.11*
	(0.11)	(0.12)	(0.06)	(0.06)
$Additional\ Members$	4e-3	-0.01	5e-3	0.03
	(0.93)	(0.89)	(0.93)	(0.56)
$Consensus$	0.10	0.12	0.11	0.08
	(0.18)	(0.17)	(0.19)	(0.31)
Observations	1504	1202	1011	796
Controls	Yes	Yes	Yes	Yes

Panel B: Excluding Presidential Reservation Controls

Bandwidth	Optimal	80% of Optimal	200	4% of Cutoff
β_5	0.09*	0.11*	0.09*	0.09*
	(0.09)	(0.09)	(0.10)	(0.10)
$Additional\ Members$	0.01	0.01	0.02	0.04
	(0.83)	(0.88)	(0.61)	(0.33)
$Consensus$	0.08	0.10	0.07	0.05
	(0.19)	(0.16)	(0.27)	(0.40)
Observations	2208	1763	1475	1177
Controls	No	No	No	No

Notes: p-values are reported in brackets. This table reports the robustness of our estimates to a variety of bandwidths. The optimal bandwidths (in increasing order of cutoff) are approximately 350, 250, 200, 350 and 250. The sign and p-values of the estimates of β_5 and $Consensus$ are similar, while those of $Additional\ Members$ tend to be precisely estimated zeroes.

Table E.3. Robustness to Higher Order Population Polynomial Controls

Panel A: Proportion of Unanimously Elected Politicians

Highest Population Exponent	1	3	5	1	3	5
β_5	0.18*	0.22*	0.30*	0.19**	0.23*	0.25
	(0.07)	(0.09)	(0.10)	(0.05)	(0.08)	(0.18)
$Additional\ Members$	0.00	-0.03	0.04	0.01	-0.03	0.03
	(0.97)	(0.71)	(0.73)	(0.84)	(0.69)	(0.79)
$Consensus$	0.18*	0.25*	0.26	0.18*	0.26*	0.22
	(0.10)	(0.10)	(0.21)	(0.09)	(0.08)	(0.30)
Obs	1248	1992	3225	1280	2043	3320
Controls	Yes	Yes	Yes	No	No	No

Panel B: Unanimously Elected Gram Panchayats

Highest Population Exponent	1	3	5	1	3	5
β_5	0.10	0.19*	0.18	0.09*	0.16*	0.15
	(0.11)	(0.06)	(0.13)	(0.09)	(0.06)	(0.14)
$Additional\ Members$	4e-3	-0.06	-0.04	0.01	-0.03	-3e-4
	(0.93)	(0.41)	(0.5)	(0.83)	(0.52)	(0.99)
$Consensus$	0.10	0.25**	0.22*	0.08	0.19**	0.15
	(0.18)	(0.05)	(0.10)	(0.19)	(0.05)	(0.18)
Obs	1504	1853	3049	2208	2721	4429
Controls	Yes	Yes	Yes	No	No	No

Notes: p-values are reported in brackets, and data is restricted to the optimal bandwidth. The optimal bandwidths for proportion of unopposed elections vary depending on the degree of polynomial specified, and range from 200-500, 250-650, 250-650, 350-650 and 300-550 (in increasing order of cutoffs). The optimal bandwidths for unanimously elected GPs vary depending on the degree of polynomial specified, and range from 350-650, 250-550, 200-550, 350-550 and 250-550 (in increasing order of cutoffs). This table reports the robustness of our estimates to a variety of population controls. The sign and p-values of the estimates of β_5 and $Consensus$ are similar, while those of $Additional\ Members$ tend to be precisely estimated zeroes.

Table E.4 : Number of Candidates for Ward (non-President) Seats

Panel A: Including Presidential Reservation Controls

Bandwidth	Optimal	80% of Optimal	200	4% of Cutoff
β_5	-0.64**	-0.74***	-0.73***	-0.74***
	(0.02)	(0.01)	(0.01)	(0.01)
$Additional\ Members$	-0.214	-0.129	-0.109	-0.181
	(0.14)	(0.45)	(0.52)	(0.26)
$Consensus$	-0.43	-0.61*	-0.63**	-0.55*
	(0.17)	(0.06)	(0.05)	(0.08)
Observations	894	708	1055	828
Controls	Yes	Yes	Yes	Yes

Panel B: Excluding Presidential Reservation Controls

Bandwidth	Optimal	80% of Optimal	200	4% of Cutoff
β_5	-0.67***	-0.8***	-0.8***	-0.8***
	(0.01)	(4E-3)	(4E-3)	(4E-3)
$Additional\ Members$	-0.271*	-0.193	-0.153	-0.241
	(0.07)	(0.27)	(0.37)	(0.14)
$Consensus$	-0.4	-0.6*	-0.64**	-0.56*
	(0.19)	(0.07)	(0.05)	(0.09)
Observations	921	732	1084	854
Controls	No	No	No	No

Notes: p-values are reported in brackets. This table reports the robustness of our estimates to a variety of bandwidths. The optimal bandwidths (in increasing order of cutoff) are approximately 100, 200, 250, 300 and 300. The sign and p-values of the estimates of β_5 and $Consensus$ are similar, while those of $Additional\ Members$ tend to be precisely estimated zeroes.

Politician Characteristics

Table P.1: President Education

Panel A: Including Presidential Reservation Controls

Bandwidth	Optimal	80% of Optimal	200	4% of Cutoff
β_5	2.76	1.06	2.76	2.75
	(0.13)	(0.61)	(0.13)	(0.14)
$Additional\ Members$	-1.41*	-1.55	-1.42*	-2.07***
	(0.09)	(0.11)	(0.1)	(0.01)
$Consensus$	4.17**	2.61	4.18**	4.82**
	(0.04)	(0.25)	(0.04)	(0.02)
Observations	1539	1239	1054	827
Controls	Yes	Yes	Yes	Yes

Panel B: Excluding Presidential Reservation Controls

Bandwidth	Optimal	80% of Optimal	200	4% of Cutoff
β_5	2.93	1.28	2.93	2.93
	(0.11)	(0.53)	(0.12)	(0.12)
$Additional\ Members$	-1.4*	-1.56*	-1.4*	-2.02
	(0.08)	(0.09)	(0.1)	(0.02**)
$Consensus$	4.33**	2.84	4.32**	4.95**
	(0.03)	(0.2)	(0.03)	(0.02)
Observations	1580	1272	1083	853
Controls	No	No	No	No

Notes: p-values are reported in brackets. This table reports the robustness of our estimates to a variety of bandwidths. The optimal bandwidths (in increasing order of cutoffs) are approximately 350, 300, 200, 200 and 200.

Table P.2: Women Presidents

Panel A: Including Presidential Reservation Controls

Bandwidth	Optimal	80% of Optimal	200	4% of Cutoff
β_5	-0.14	-0.17	-0.17	-0.17
	(0.22)	(0.18)	(0.18)	(0.18)
$Additional\ Members$	0.06	0.06	0.08	0.04
	(0.35)	(0.39)	(0.24)	(0.59)
$Consensus$	-0.19	-0.22	-0.25*	-0.2
	(0.13)	(0.11)	(0.08)	(0.15)
Observations	1374	1122	1054	827
Controls	Yes	Yes	Yes	Yes

Panel B: Excluding Presidential Reservation Controls

Bandwidth	Optimal	80% of Optimal	200	4% of Cutoff
β_5	-0.20	-0.26	-0.26	-0.26
	(0.24)	(0.16)	(0.16)	(0.17)
$Additional\ Members$	0.06	0.07	0.09	0.05
	(0.41)	(0.42)	(0.33)	(0.58)
$Consensus$	-0.26	-0.33	-0.35*	-0.31
	(0.16)	(0.11)	(0.09)	(0.14)
Observations	1409	1152	1083	853
Controls	No	No	No	No

Notes: p-values are reported in brackets. This table reports the robustness of our estimates to a variety of bandwidths. The optimal bandwidths (in increasing order of cutoffs) are approximately 250, 300, 250, 250 and 300.

NREGA Implementation Under Negative Rainfall Shocks

Table N.1: Number of Households That Worked

Bandwidths: Optimal & Other Small Bandwidths

Bandwidth	Optimal	100	50	2%	1%
β_5	-11.60	-26.46**	-32.64***	-26.46**	-32.64***
	(0.11)	(0.04)	(0.01)	(0.04)	(0.01)
$Additional\ Members$	1.33	-2.43	0.90	-4.68	-3.59
	(0.72)	(0.71)	(0.93)	(0.5)	(0.7)
$Consensus$	-12.93	-24.03*	-33.53**	-21.78	-29.05*
	(0.11)	(0.10)	(0.05)	(0.14)	(0.07)
Observations	13224	6072	2712	4392	2184
Month*Year F.E.	Yes	Yes	Yes	Yes	Yes

Notes: p-values are reported in brackets. Attention is restricted to years with negative rainfall shocks.

This table reports the robustness of our estimates to a variety of bandwidths.The optimal bandwidths (in increasing order of cutoffs) are approximately 200, 250, 350, 100 and 200.

Table N.2: Number of People That Worked

Bandwidths: Optimal & Other Small Bandwidths

Bandwidth	Optimal	100	50	2%	1%
β_5	-59.83	-495.21*	-625.82**	-495.21*	-625.82**
	(0.70)	(0.08)	(0.02)	(0.08)	(0.02)
$Additional\ Members$	88.35	39.96	146.17	-0.81	71.36
	(0.16)	(0.76)	(0.51)	(0.99)	(0.7)
$Consensus$	-148.18	-535.17*	-771.99**	-494.4	-697.18**
	(0.38)	(0.08)	(0.02)	(0.11)	(0.03)
Observations	12912	6072	2712	4392	2184
Month*Year F.E.	Yes	Yes	Yes	Yes	Yes

Notes: p-values are reported in brackets. Attention is restricted to years with negative rainfall shocks.

This table reports the robustness of our estimates to a variety of bandwidths. The optimal bandwidths (in increasing order of cutoffs) are approximately 150, 200, 400, 150 and 250.

Robustness

Table R.1: Robustness to Council Member Reservations

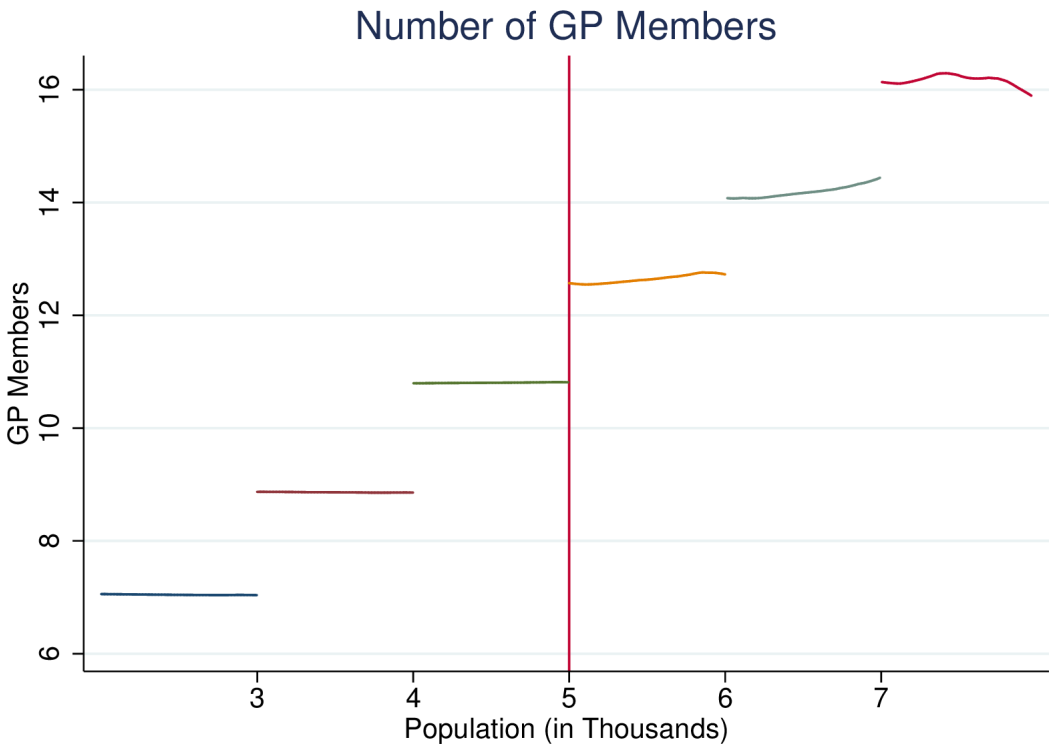
	Proportion of Uncontested Seats	All Uncontested	Council Member Candidates
$2\beta_U$	0.03	-0.05	0.21
	(0.88)	(0.69)	(0.71)
$\beta_U + \beta_R$	-0.04	-0.03	-0.13
	(0.7)	(0.58)	(0.75)
$\beta_U - \beta_R$	0.07	-0.03	0.34
	(0.75)	(0.84)	(0.63)
Observations	149	185	153
Controls	Yes	No	Yes

	President Education	Female President	Households Worked	Persons Worked
$2\beta_U$	1.78	0.1	-5.5	-30.38
	(0.56)	(0.67)	(0.54)	(0.90)
$\beta_U + \beta_R$	-0.41	-0.01	9.03	202.23*
	(0.86)	(0.94)	(0.21)	(0.08)
$\beta_U - \beta_R$	2.18	0.11	-14.52	-232.61
	(0.58)	(0.68)	(0.21)	(0.40)
Observations	121	158	1644	1248
Controls	Yes	Yes	Month*Year F.E.	Month*Year F.E.

Notes: All regressions restrict attention to data within the optimal bandwidth. p-values are reported in brackets. For NREGA outcomes, attention is restricted to years with negative rainfall shocks.

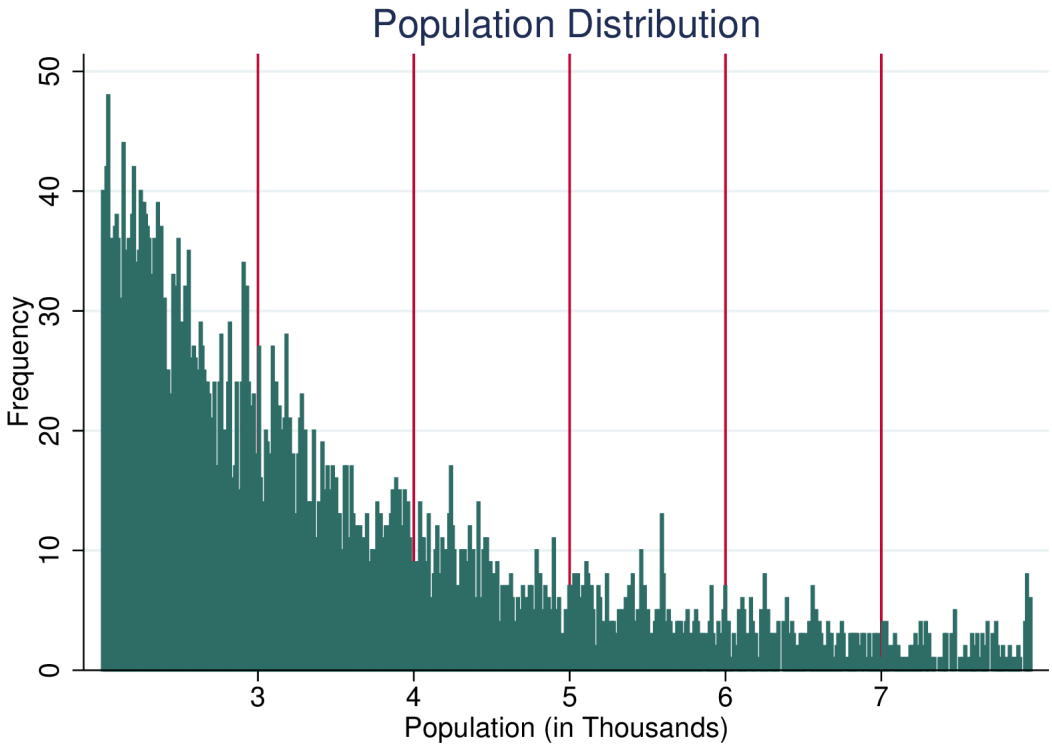
Graphs

FIGURE 1: NUMBER OF SEATS INCREASE WITH POPULATION



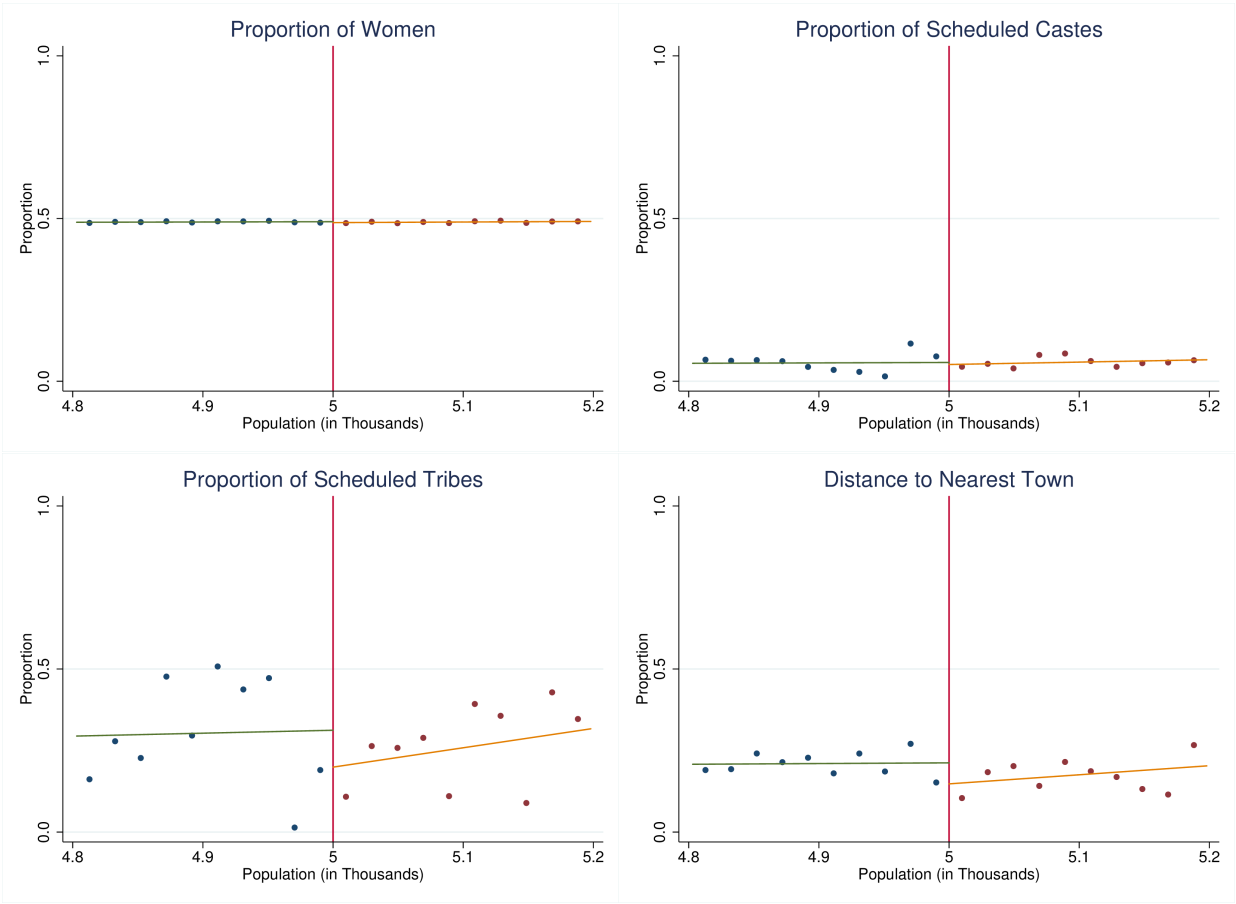
Notes: Fitted local polynomials show that the number of Gram Panchayat members increased discontinuously at each of the 5 population thresholds.

FIGURE 2: HISTOGRAM OF POPULATION



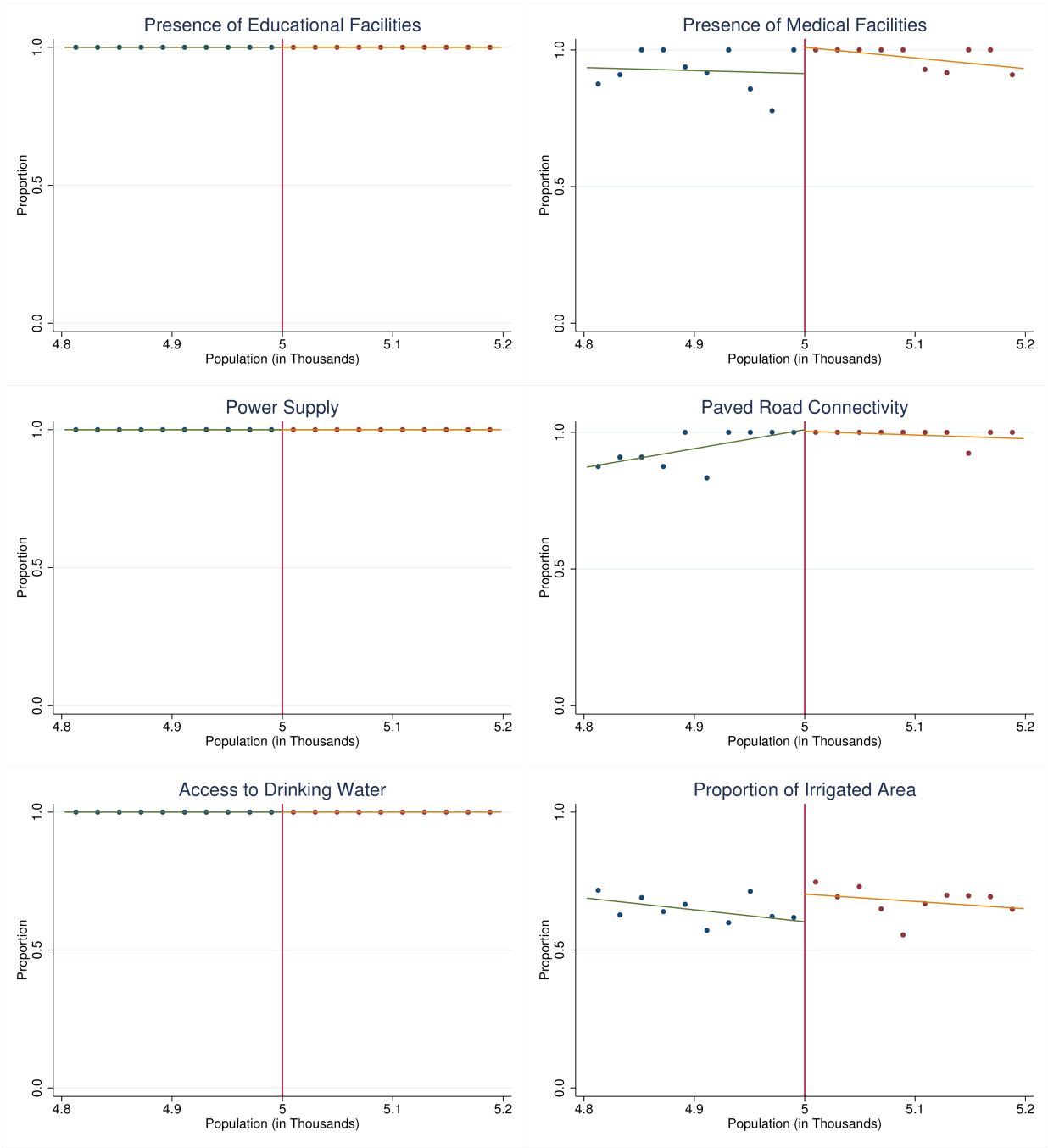
Notes: This figure shows that the population distribution did not jump discontinuously at any of the 5 population thresholds used in the analysis. Population is grouped into bins of 10. The RD Density Test proposed by Frandsen (2014) reports the following p-values for evidence of discontinuity in the population distribution at each cutoff (in increasing order): 0.5, 0.4, 0.2, 0.9, 0.5.

FIGURE 3: (BASELINE) ELECTORATE CHARACTERISTICS



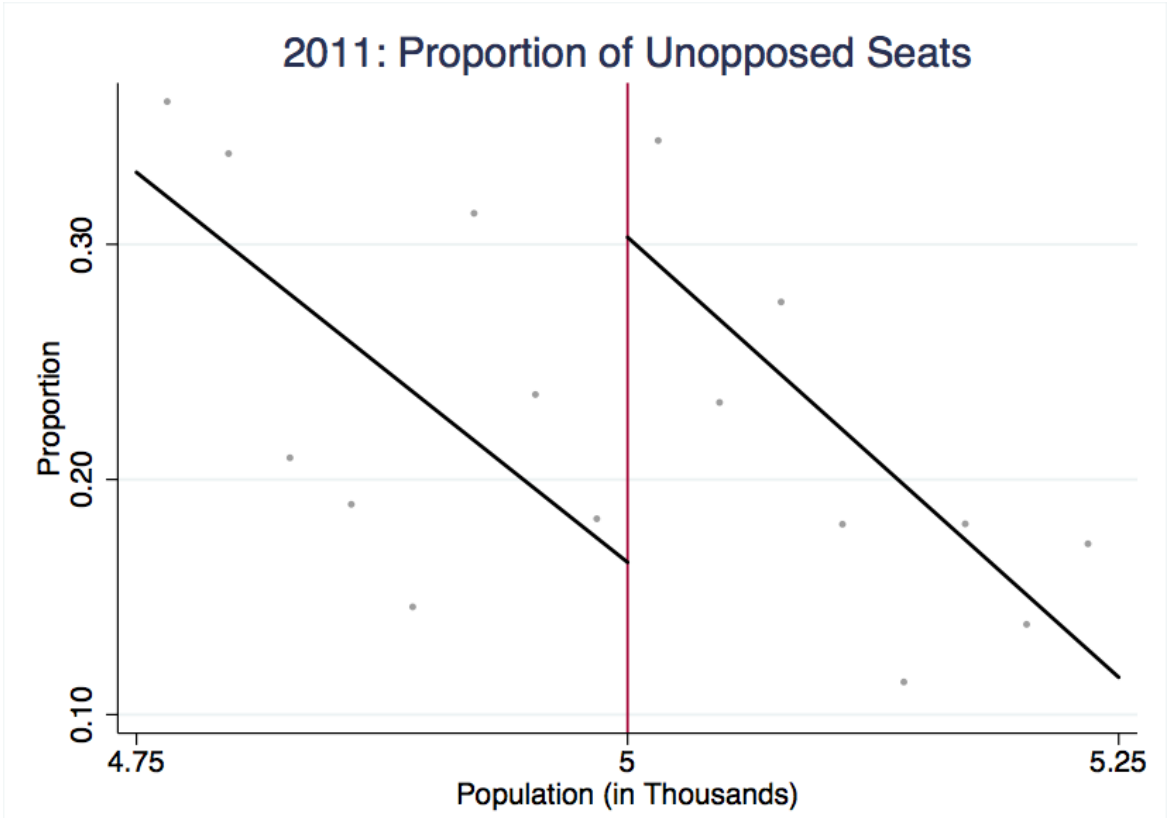
Notes: The above graphs that show that electorate characteristics do not vary discontinuously at the cutoff 5000. All variables are sourced from the 2001 Population Census.

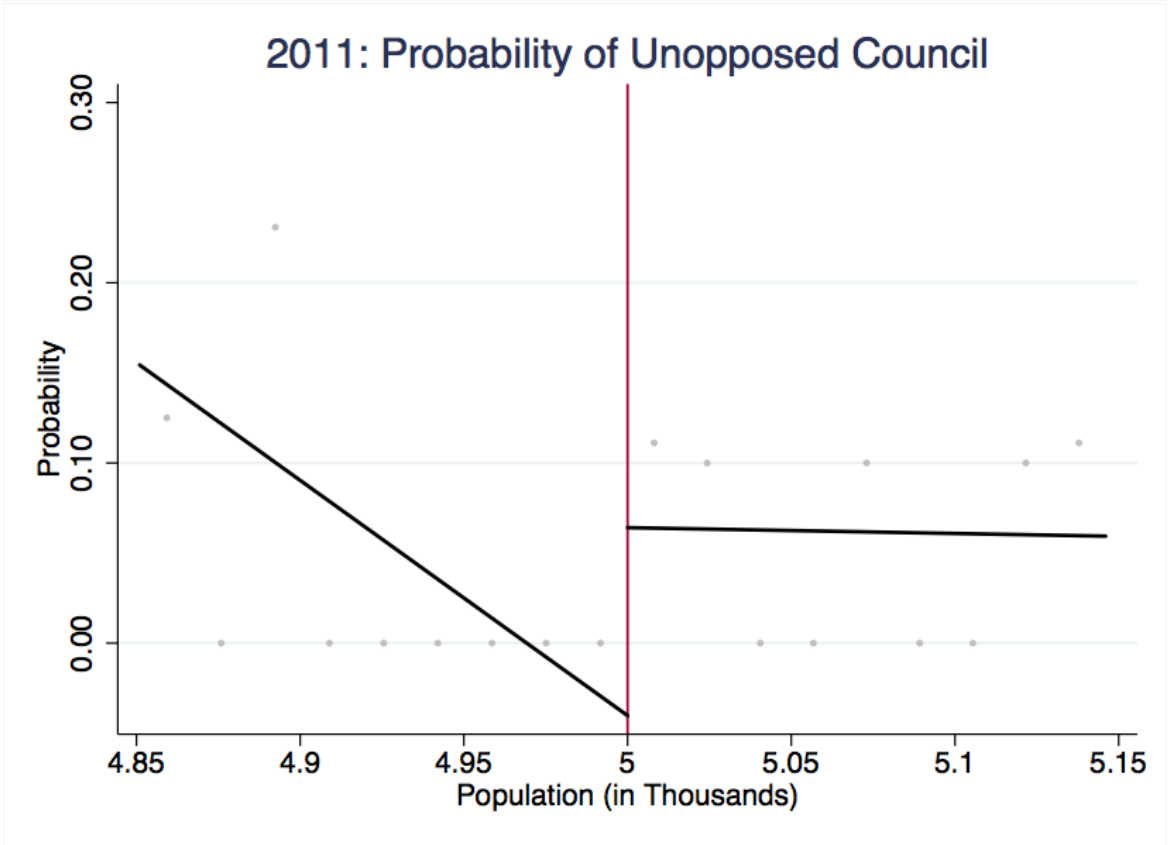
FIGURE 4: (BASELINE) VILLAGE ACCESS TO PUBLIC GOODS



Notes: The above graphs show that village characteristics do not vary discontinuously at the cutoff 5000. All variables are sourced from the 2001 Population Census.

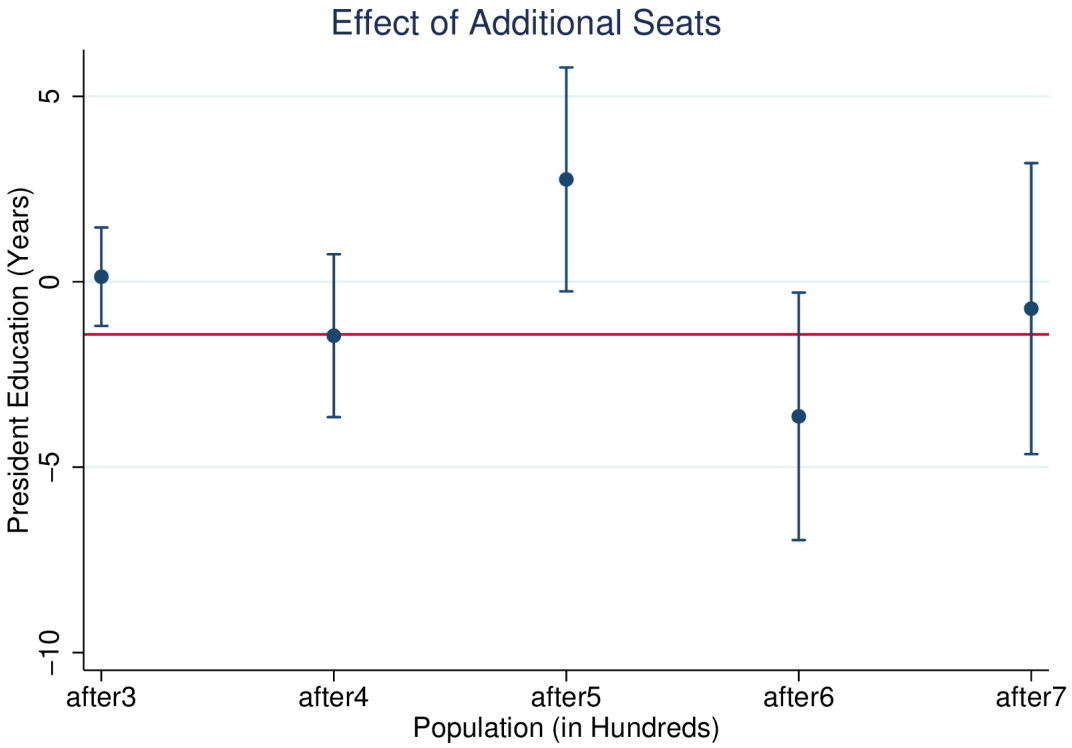
FIGURE 5: CONSENSUS ELECTIONS

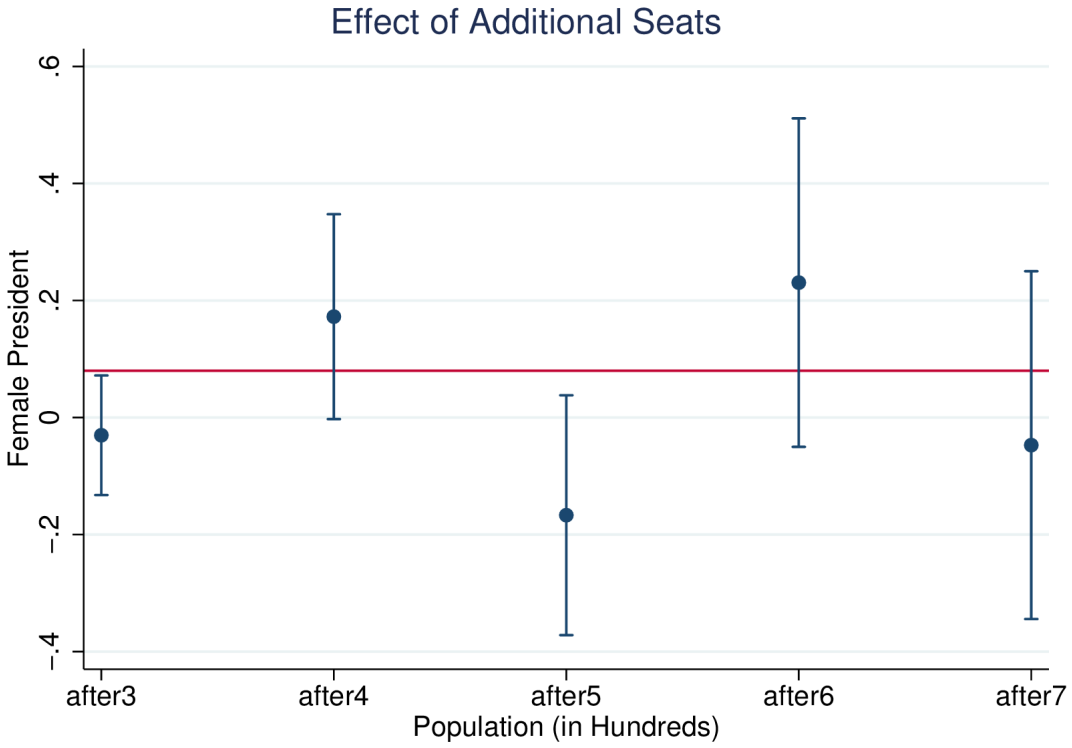




Notes: The above graphs show that Gram Panchayats that faced higher consensus election incentives were more likely to elect their leaders without opposition. The first panel shows that the proportion of seats that were filled without opposition increased discontinuously at the threshold. The second panel shows that the probability that the *entire* Gram Panchayat was chosen by consensus also increases discontinuously. Standard errors are clustered at the GP Population level.

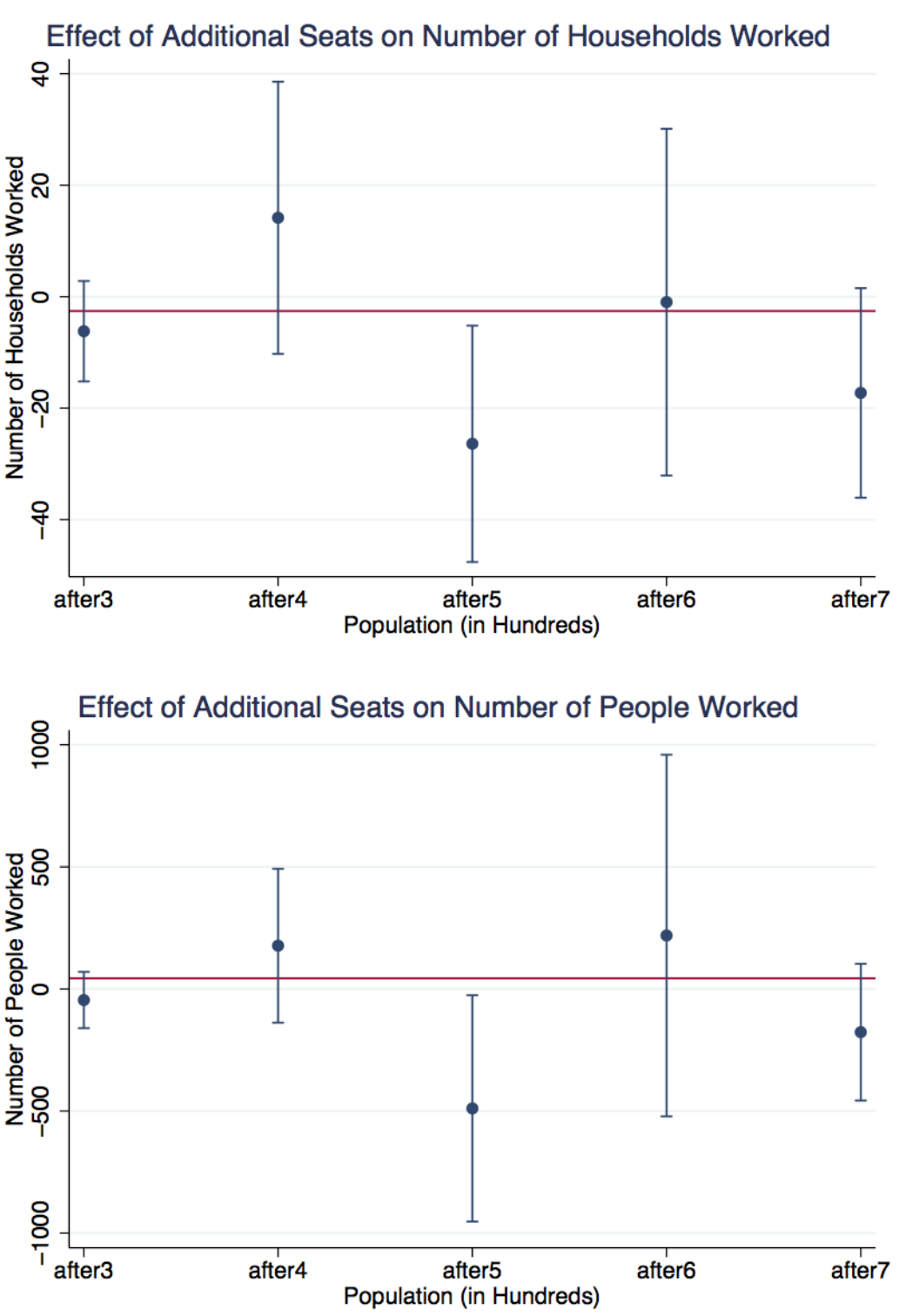
FIGURE 6: POLITICIAN EDUCATION AND GENDER





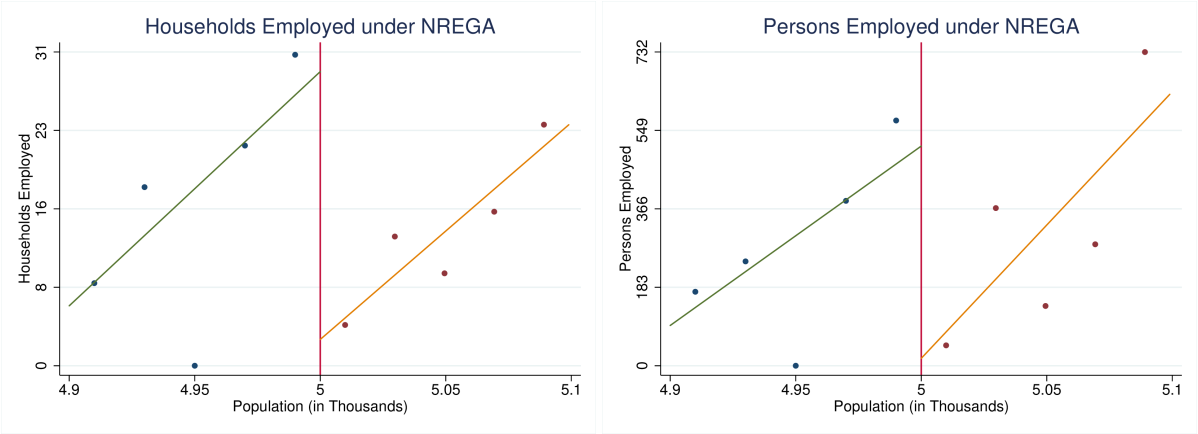
Notes: The above graphs display point estimates for each of the 5 cutoffs, along with confidence intervals at the 10% level. Data is restricted to bandwidths of 200 for each threshold. The red line plots the average effect of the four cutoffs $= \frac{\beta_3+\beta_4+\beta_6+\beta_7}{4}$. The β_5 estimate lies well away from the average effect at the other cutoffs. Standard errors are clustered at the GP Population level. Confidence intervals are computed at the 10% level of significance.

FIGURE 7: ESTIMATES AT EACH CUTOFF UNDER A NEGATIVE SHOCK



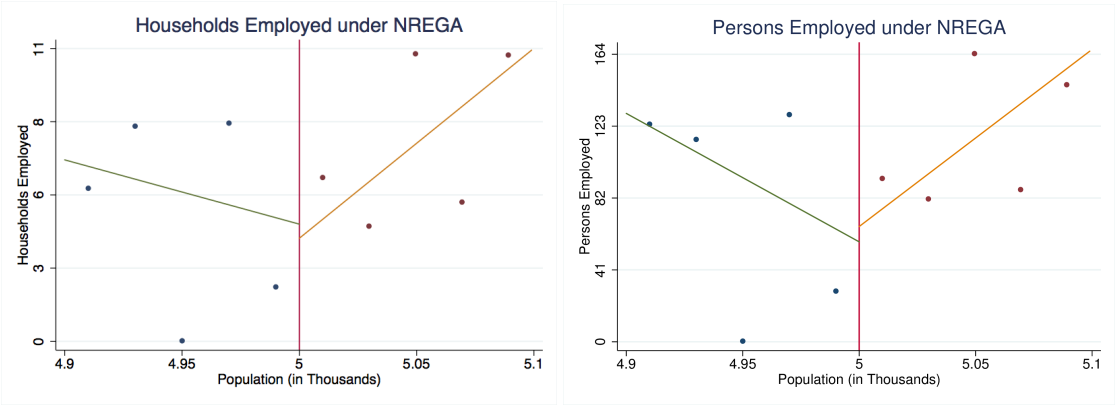
Notes: The above graphs show that during a negative rainfall shock, Gram Panchayats that faced higher consensus election generated less employment under NREGA. Standard errors are clustered at the GP Population level. Confidence intervals are computed at the 10% level of significance.

FIGURE 8: NREGA IMPLEMENTATION UNDER A NEGATIVE SHOCK



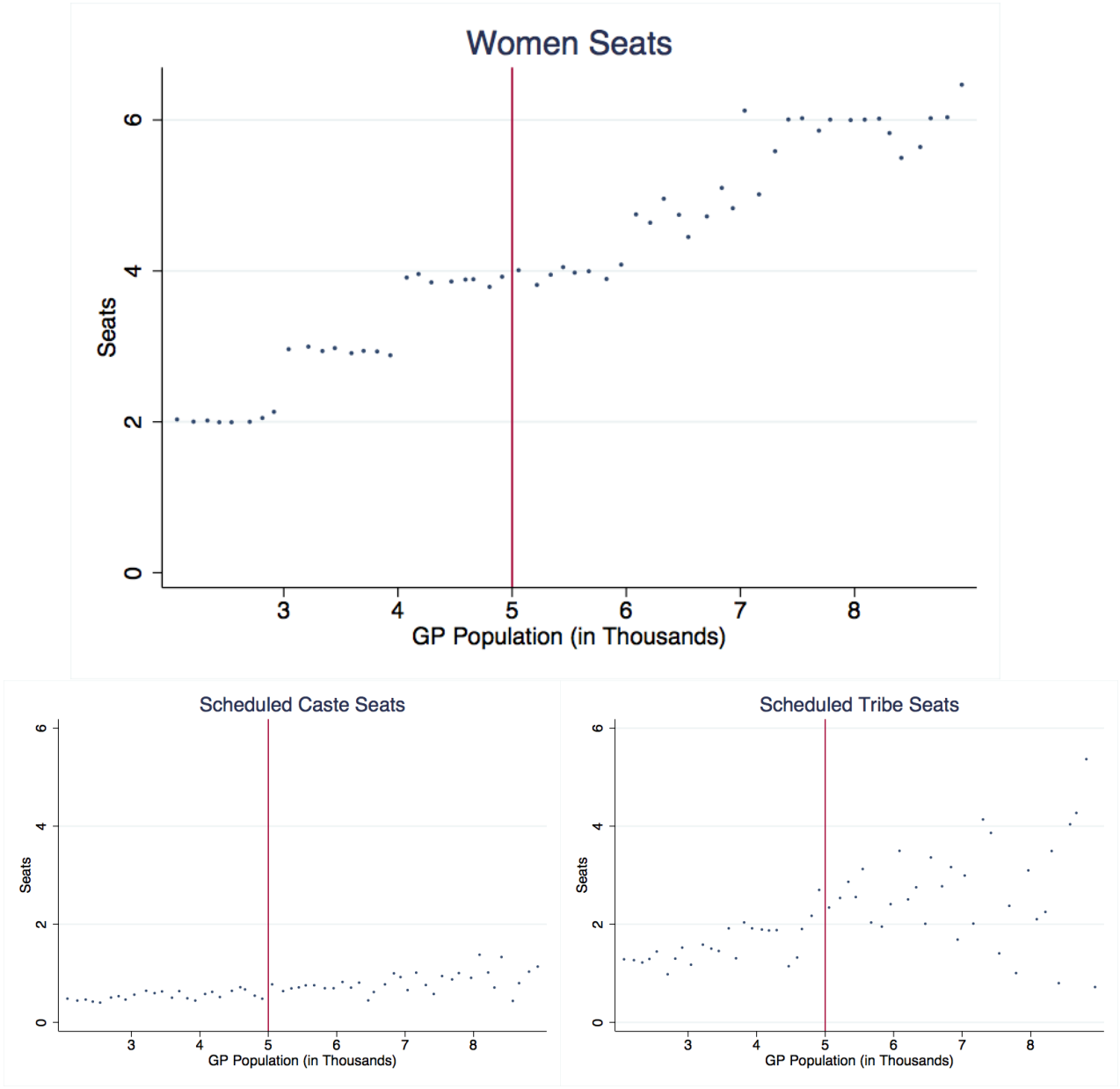
Notes: The above graphs show that during a negative rainfall shock, Gram Panchayats that faced higher consensus election generated less employment under NREGA.

FIGURE 9: NREGA IMPLEMENTATION UNDER REGULAR CONDITIONS (NON-SHOCK YEARS)



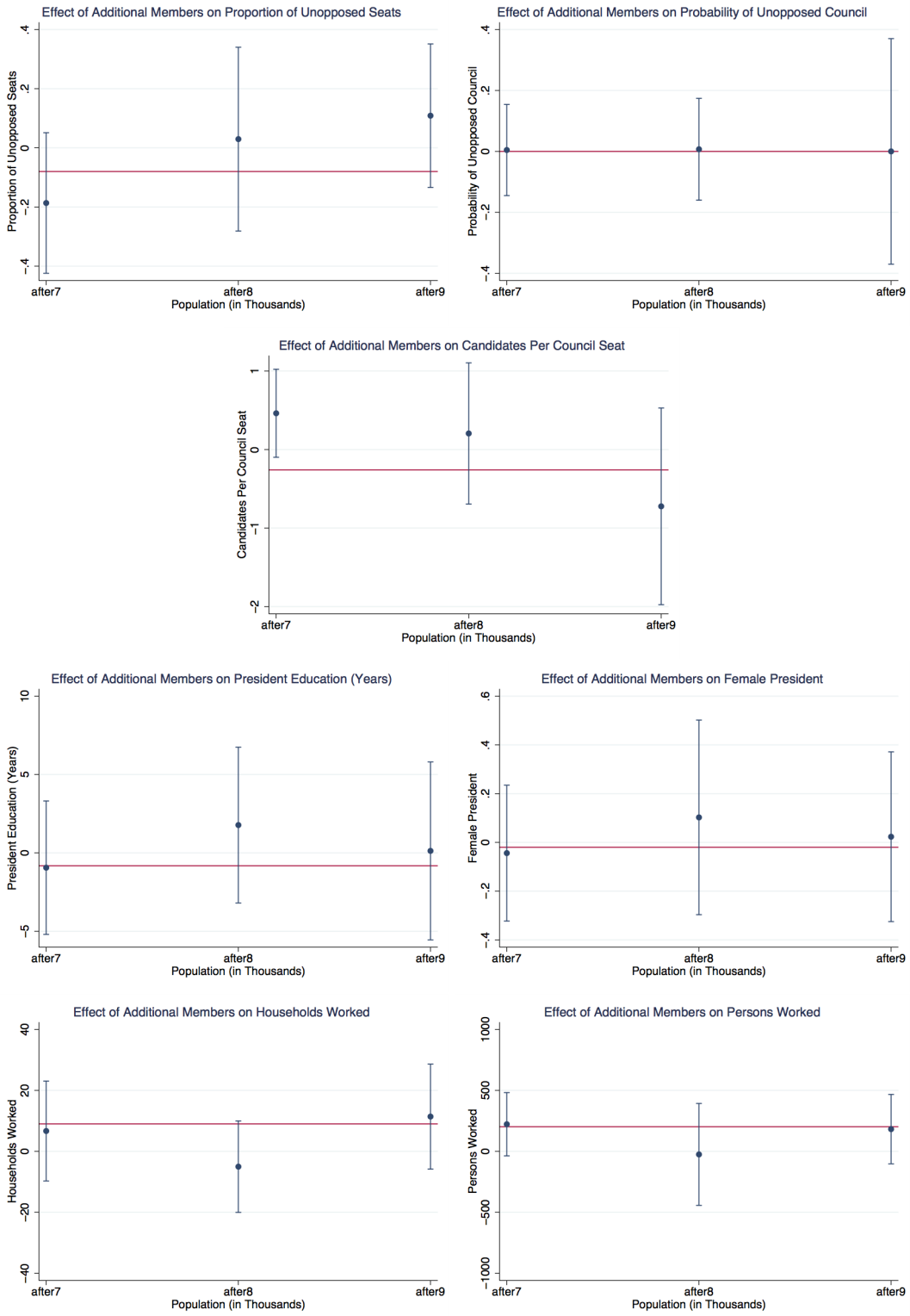
Notes: The above graphs show that in the absence of a negative rainfall shock, Gram Panchayats that faced higher consensus election incentives did not differ in their implementation of NREGA.

FIGURE 10: COUNCIL MEMBER RESERVATIONS



Notes: The above graphs show that the number of seats reserved for women increases discontinuously at the cutoffs 3, 4, 6 and 7. Seats reserved for Scheduled Castes and Scheduled Tribes do not increase at the cutoffs. These claims are verified in Table **??**.

FIGURE 11: ROBUSTNESS TO COUNCIL MEMBER IDENTITIES



Notes: The above graphs present evidence that the effects of additional council members are not different when one of the seats is reserved for women on our outcomes of interest. Data is restricted to the optimal bandwidth for each outcome. Confidence intervals are computed at the 10% level of significance.