Poverty Alleviation or Political Calculation?

Implementing India's Rural Employment Guarantee Scheme

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Abstract

Impact evaluations of poverty alleviation programs can give rise to biased estimates if politicians make implementation decisions with electoral returns in mind. Using a novel data set, I examine the effect of electoral competitiveness on the implementation of India's National Rural Employment Guarantee Scheme. Results indicate a negative relationship between fund utilisation in the program and the vote share of the Indian National Congress, the main driver of the NREGS. These findings challenge the applicability of traditional probabilistic voting models in the Indian context, according to which transfers are made to competitive constituencies and are partially consistent with ideas of electoral opportunism whereby opposing parties can leverage public programs to claim credit. Using methods proposed by Altonji et al. (2000) and Oster (2013), I conclude it unlikely that unobservables can explain away these results.

Background

Large scale poverty alleviation programs have become increasingly common in the developing world. Conditional cash transfers in Latin America and work-fare programs in India, Malawi, Ethiopia and other countries in Africa are the most salient of these. Recent evaluations suggest that such programs have been effective in reducing poverty and improving human development outcomes, at least in the short run (de Brauw et al., 2014; Soares et al., 2010; Gertler, 2004; Rivera et al., 2004; Schultz, 2004). Their ensuing

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popularity with the electorate and the corresponding returns electoral returns has led to them being referred to as "instruments of electoral investment" for the incumbent party (Diaz-Cayeros et al., 2012).

An enduring problem in evaluating electoral returns to poverty alleviation programs is that they are not randomly implemented. To overcome this, studies attempt to exploit randomized design or phased implementation designs to make causal claims. These broadly yield positive electoral returns, although the actual impact varies with the nature of the program and duration of exposure. For instance, early enrolment in Mexico's PROGRESA/Oportunidades led to a nine per cent increase in incumbent support (De La O, 2013). The program was seen as more desirable if there was a longer window for people to benefit, and for the incumbent to claim credit. Similarly, Brazil's incumbent government received electoral rewards from CCTs[1] although there were decreasing returns over time (Zucco, 2011). In both countries, there was little evidence of an electoral backlash from those paying for benefits to vote against politicians who initiated and maintained these policies. In contrast, when studying the electoral returns of India's National Rural Employment Guarantee Scheme (NREGS), Zimmermann (2012) found an inverse U-shaped relationship between duration of access and votes for the incumbent coalition.

Despite the use of randomized design strategies or discontinuities in the implementation process, estimates of program outcomes would still be biased if politicians were guided by electoral motives while making choices about locations, rollout schedules and intensity of implementation. Politicians have several levers to control design, implementation and impact of such programs. The first of these is through program placement: a series of papers demonstrates that estimates of performance can be biased if programs are not placed randomly (Pitt et al., 1993; Duflo et al., 2007). If programs are placed where demand or need is already high (low), impact estimates will be upward (downward) biased. Placement may also be affected by opportunities for clientelism whereby political entities can indulge in discretionary rewarding of supporters through private benefits that can be withheld in case of non-compliant behavior. This would provide an incentive for politicians to select a particular location and/or group for a targeted program (Stokes et al., 2013; Roniger, 2004).

Recent evidence suggests that politics does affect program placement in India. Gupta (2006) found that official selection criteria were often supplemented by political considerations in the case of the National Food for Work program, and the final choice of districts balanced by the goal of spreading early phase districts across states. Moreover, districts whose leaders were affiliated to the incumbent party or its allies appeared to have an advantage in receiving services earlier. Electoral campaign rhetoric is also indicative of

[1]Bolsa Família, Bolsa Escola, and Bolsa Alimentação.

politicians prioritising those regions or villages where they receive greater support[2]. For instance, Vaishnav and Sircar (2012) find spatial and temporal variance in program implementation that can be traced to state-level election cycles and results in Tamil Nadu.

A second mechanism through which politics influences program performance is the efficiency of implementation or targeting. Politicians may be involved in corrupt practices or leakages such as deliberate implementation delays, mismanagement of public funds or in extreme cases, outright theft. Leakage is a particularly salient issue in India, where poverty alleviation programs persist over long periods and many programs grant a high level of discretion to implementing officials (Kapur and Mukhopadhyay, Kapur and Mukhopadhyay; Gupta, 2013).

Although India's NREGS is one of the most heavily studied safety net programs in recent times, literature on the political antecedents of the program is relatively sparse. Most studies on its implementation focus on targeting mechanisms, elite capture and state capacity in terms of ability to provide work to households demanding it (Dutta et al., 2014, 2012; Liu and Barrett, 2013; Jha et al., 2009). Separate strands deal with accountability failures (Aiyar and Samji, 2006), leakages (Niehaus and Sukhtankar, 2013a,b), and the role of information in ameliorating this (Shankar and Gaiha, 2013; Shankar et al., 2011). Current studies on the political economy of implementation recognize the potential bias stemming from political motivations to electoral returns, but do not address it explicitly (Zimmermann, 2012). Inferences from other studies examining the political contexts of NREGS are somewhat limited since they are based in specific districts, and consequently do not recognize the importance of national level political priorities. For instance, Johnson (2009) does not find any impact of party affiliation of local leaders on NREGS outcomes; however, this is based on a subset of villages in one state where the local election constituency mapped onto a single Gram Panchayat[3], and only where INC experienced a close margin of victory or loss. Similarly, another study in specific districts across four states finds heterogeneous and non-linear effects of political competition in local elections on scheme participation (Shankar and Gaiha, 2013).

In this paper, I focus on political motives behind poverty alleviation programs by examining if electoral outcomes affected placement and implementation of NREGS in India. A national level examination is particularly interesting since the program was implemented in the aftermath of an unexpected loss of the ruling National Democratic Alliance (NDA) headed by the Bharatiya Janata Party (BJP) and the emergence of the Indian National Congress (INC)-led United Progressive Alliance (UPA). A widely held belief was that this was a result of the NDA's concentration on urbanisation-led

[2]See for instance, Trinamul Congress' Member of Parliament, Shatabdi Roy's speech available online at `https://www.youtube.com/watch?v=M0jQmphZU0g`.

[3]A Gram Panchayat is a locally elected village government.

growth which failed to trickle down to the majority of the rural poor who ostensibly did not buy into NDA's "India Shining" election campaign. A universal rural employment guarantee program was a prominent component of INC's manifesto and subsequently a high priority of the UPA government. Therefore, operational choices regarding NREGS may have been influenced by ideas relating to patronage by rewarding supporters, or opportunism by swinging voters towards the incumbent party.

This paper fills a gap in the existing literature on the program by examining whether implementation choices were driven by INC's competitiveness in the previous elections, and whether close elections mattered. Through this, I aim to throw insight on motivations of governments across the developing world with regard to both design and implementation choices of poverty alleviation programs.

1 Overview of NREGS

With an outlay of nearly one per cent of India's GDP in 2012-13 and covering 48 million households, India's NREGS is the largest rights-based employment and social protection initiative in the world. The program provides a legal guarantee for 100 days of paid employment to rural households willing to do unskilled manual labor on locally planned public works at a notified wage that varies across states, and is periodically revised to account for inflation. This short-term safety net protects rural households from the risk of consumption falling below some crucial level (Ravallion et al., 2013), and reduces the urgency of seasonal and distress migration. By requiring that the projects be selected through local participation, it also promotes investments in public goods and local institutions.

The idea of an employment guarantee as a safety net is not a novel one in India - it can be traced back to the famine codes of the 1880s when the British administration used a self-selection mechanism for providing famine relief. In more recent times, the foundation for NREGS was laid by civil society campaigns in Rajasthan (Dreze, 2010). The idea of a universal employment guarantee found favour with the INC before the national elections of 2004 and NREGS was made a prominent feature of the party's election manifesto. After emerging as the single largest party, INC headed a coalition government with considerable support from the Left parties who were riding their best electoral performance in recent times. The Left was a strong advocate of the employment guarantee and consequently, NREGS was the first point on the coalition's common minimum program.

In its present form of not indulging in explicit targeting and guaranteeing employment at a statutory minimum wage, NREGS draws heavily from the Maharashtra Employment Guarantee Scheme of 1973, the difference being the legislation passed in 2004 gives the

scheme a legal basis and makes state governments liable to pay unemployment insurance to those demanding but not receiving work. Besides, the scale of implementation is unprecedented in that it spans all rural districts.

The program itself was implemented in three phases. It was formally launched on February 2, 2006 in a group of 200 "backward" districts. Additional districts were added over time: 130 districts were selected in 2007-2008 and the remaining received NREGS with effect from April 1, 2008. NREGS now covers the entire country with the exception of union territories and other entirely urban districts.

Being a centrally sponsored scheme, the central (federal) government is responsible for the bulk of NREGS financing while individual state governments bear the burden of implementation. The actual funding allocation from the central government is based on labor demand estimated at the village level by the Gram Panchayat, that is then aggregated at Block and District levels. The central government bears the complete wage cost of unskilled manual labor and 75 per cent of the material cost including the wages of skilled and semi-skilled workers [4]. Since state governments bear the cost of unemployment insurance, they have an incentive to establish and expand the program.

2 Conceptual framework

The existing literature on distributive politics indicates several pathways through which politicians can attempt to leverage public programs or goods to influence voters. While some of these may be seen as programmatic in nature if distribution rules are public and there is legal enforcement of the method of selecting and rewarding beneficiaries (Stokes et al., 2013; Diaz-Cayeros et al., 2012), in other cases there may be concerns of partisan bias or clientelistic behavior on part of politicians. Pork is a blunt instrument of partisan bias since benefits are neither excludable nor targeted at individuals. Clientelism on the other hand is characterised an exchange of benefits for electoral support. A necessary condition for clientelism is that parties should have a robust mechanism on the ground to enforce post-transfer compliance as beneficiaries may not have lingering incentives to vote against their true preferences if they are different from the party making the transfer. Stokes et al. (2013) classifies patronage and vote or turnout buying as special cases of clientelistic behavior depending on whether benefits are targeted at party affiliates or general voters.

When allocating benefits of public programs, assumptions about political and voter

[4] In some cases, the Centre may choose to bear part of a state government's costs – for instance in Bihar, the Centre meets the cost of salaries of contract workers, training, and monitoring systems, amounting to about six per cent of the state's scheme-specific budget (Dutta et al., 2014).

beliefs lead to radically different implications for the kind of electoral investment adopted by politicians. Voters can be divided into three categories – core supporters, swing voters, and backers of the opposition. In this context, the Cox-McCubbins model assumes that a politician's core voters are the most electorally responsive and least risky constituency since the politician has the greatest amount of information about them (Cox and McCubbins, 1986). Hence, a risk-averse politician's primary objective would be to consolidate one's existing base by initially distributing program benefits to core voters. If the degree of risk aversion of a political player can be judged from one's vote share or victory margin in the previous election, we would expect NREGS to be placed earlier or implemented better in the traditional bastions of the INC, or other parties in the UPA coalition.

Models proposed in Lindbeck and Weibull (1987) and Dixit and Londregan (1996), on the other hand, predict that rational politicians should target swing voters to maximize their chance of election since core supporters will vote for their preferred choice regardless of the outcomes of the distribution process. If this were to be applicable, NREGS' implementation quality would be better implemented where INC or affiliated parties had a relatively narrow victory. A third model claims that given the secret ballot, parties cannot know the final choice of the voter and hence concentrate on mobilizing and facilitating hitherto uninterested voters to go and cast their vote (Nichter, 2008).

A further strand of literature on program implementation argues that political will is the key factor to determining outcomes. Based on this, theoretical models explaining implementation can be classified based on the "motivation of competing parties or candidates" (Bardhan and Mookherjee, 2010). Downsian models of political competition emphasize the role of political competition and electoral opportunism, while models derived from Lipset (1960) and Wittman (1973) stress on ideology of political actors in determining policy choice. Applying this to NREGS, the latter set of models predicts that since NREGS was INC's brainchild, implementation should be better in regions where INC support is high. On the other hand, Downsian models suggest that after obtaining a majority, further increases in INC's vote share will not affect NREGS implementation.

I use two regression strategies to test whether any of these ideas hold for NREGS. Both strategies use measures of NREGS implementation and political competition, details of which are in Section 4. If INC's primary goal was to perpetuate its clientelistic network or distribute pork, NREGS implementation would be better where it had won convincingly, or where there was a history of INC rule. This would mean a positive coefficient estimate for the political competition variable. The same would hold true if the ideology half of Bardhan and Mookherjee (2010)'s "political will" argument was applicable, although history would arguably not be important in this case. On the other hand, if INC's aim was to target swing voters, the scheme would be executed well in PCs with close elections,

implying a positive sign for a linear term of political competition and a negative sign for a quadratic term. Finally, if a model based on Downsian competition as illustrated in Bardhan and Mookherjee (2010) was the dominant narrative, strategic objectives - for instance, credit claiming by INC's opponents for a well-implemented program, as also INC not stressing on quality of rollout once it had gained a majority - could be at play.

3 Data

The data for this paper was gathered from several sources. Table 3 below summarizes the data, relevant time lines and sources.

First, I use national-level electoral data for 2004 from the Election Commission of India on the number of parties, candidates and their party affiliations, votes polled by each, as well as the number of registered voters. This data is defined at the level of parliamentary constituencies (PCs). In 2004, 5,435 candidates contested for 543 positions in the lower house of the Indian Parliament, each position corresponding to a PC. Of these, 3,050 candidates were affiliated to one of 215 political parties; the rest ran as independent candidates. While this may give an impression of intense competition, 2,768 candidates – nearly 51 per cent of those in the fray – failed to garner even one per cent of votes in their respective PCs. I eliminate such candidates, irrespective of their party affiliation, for ease of analysis. Winners of these elections form the lower house of the Indian Parliament and are responsible for passing laws, authorising government budgets and representing the interests of their constituencies.

Second, I use census data from the 2001 Indian Census to obtain district-wise measures of rural and total population, the number of households and the rural literacy rate. Third, since NREGS is a demand driven scheme, demand for work is likely to be higher in regions with a larger proportion of population below or close to the poverty line. In the absence of district level poverty estimates, I use district level controls indicative of the level of poverty from the third round of the District Level Household and Facility Survey (DLHS-3) conducted during 2007-08. Covering 1,000 to 1,500 households in each state and 611 districts in India, DLHS-3 data are representative at the district level. I aggregate the household level data to obtain estimates for the proportion of SC and ST households and the proportion of households with BPL cards, electricity, toilets and a kutcha (non-permanent) house in a district.

The final, and most significant, component of my data are the Delivery Monitoring Unit reports (also referred to as the Monthly Progress Reports, or MPRs) on NREGS outputs and outcomes from the Indian government's official website `www.nrega.nic.in`. I use data for the first three financial years of NREGS implementation – 2006-07, 2007-08

and 2008-09 – before the subsequent national elections in 2009.

I focus on financial data for three main reasons. First, the central government is responsible for the major part of the NREGS finances since it is a centrally sponsored scheme. Funds are transferred directly from the federal Ministry of Rural Development to districts to reduce delays; in practice this has the additional effect of reducing state discretion in financial allocations to districts, and in determining fund usage. Second, financial data is audited by government entities and corroborated by different levels of government. Records of fund release are kept by the federal and state governments and these cannot deviate substantially from each other without good reason. There are also bounds to over-reporting expenditure since states cannot spend much more than the sum of their current receipts and previously unspent balances. Third, in the absence of good quality data on employment and asset creation, finances could be a better indicator of the relative importance that government places on a program. In this case, members of Parliament have greater control over finances, rather than actual employment or asset creation, and could therefore be in a better position to dictate desired implementation intensity.

From the perspective of employment, a widespread concern is that government officials at the state and lower levels have an incentive to over-report employment in the MPR data. The first, and perhaps more obvious reason, is that corrupt officials fake muster rolls by overstating true employment both in terms of the number of households offered employment and days of actual employment. Second, overstating employment reduces the state's liability to pay unemployment insurance to households not receiving work within 15 days of demand.

A comparison of administrative data with that from the National Sample Survey (NSS) conducted in 2007-08 found that the estimates of employment from the latter range from 42 and 56 per cent of administrative employment (Imbert and Papp, 2011). The difference is higher in some states like Assam, Bihar and Jharkhand, while Punjab, Andhra Pradesh and Tamil Nadu data shows a high degree of convergence. The overall estimate in the study is lower than previous estimates of 70 per cent for a similar time period (Himanshu, 2010) and 50 per cent for an earlier period (Bhalla, 2010).

There is however skepticism about the accuracy of such comparisons since "NSS surveys are not particularly well suited to capture NREGA employment", and comparisons tend to underestimate actual days of employment (Dreze, 2011). Further, the mid-2008 implementation of mandatory wage payment through banks and post offices is likely to have ameliorated the problem of over-reporting and false muster rolls. Imbert and Papp (2011) agree that there is a likelihood that that NSS downward biases NREGS employment estimates since respondents may not be able to differentiate between public works

provided by NREGS and other work programs, as the former was still a new scheme. So while comparisons may not be entirely valid, and measures may have reduced discrepancy in later years, I still do not use employment data for the purpose of this study since the purported discrepancies for the relevant years are too large to ignore, and it is likely that it would have taken time for the effects to be reflected in the data.

3.1 Compiling the dataset

There are two methodological innovations in this paper to counter complications posed by the creation of new districts and the lack of congruence of electoral constituencies and administrative districts. In India, all administrative and survey data is collected at the district level but new districts have been created over time by a) splitting an existing district - I call these single parent districts, as also by b) combining pieces of several districts - which I call born of multiple parents. This naturally presents a problem for longitudinal studies using the district as an observation (see Kumar and Somanathan (2009) for a brief review). In the case of this study, several districts were created after 2001 and are present in the NREGS and DLHS datasets, but not in the 2001 census. To counter these discrepancies, I create a district level dataset for 2001 by merging new districts into their parent districts. This is a straightforward process for districts created from a single parent. For districts with multiple parents where I need further information about the contribution of individual parents, I use decadal growth rates included in the 2011 Primary Census Abstract to create an approximate population weight for each parent. I then use this to aggregate NREGS, DLHS and remaining census data. Table 1 shows the calculation of these weights for a hypothetical District C carved out of Districts A and B.

The second methodological innovation concerns the mapping of electoral constituencies to administrative districts. Since the Election Commission of India tries to equalize PC populations within a state, PC borders do not necessarily match those of the district. Hence, it is necessary to either map districts to PCs or vice-versa to create a meaningful unit of observation for which I have complete data. The relevant unit in this scenario is the electoral constituency since Members of Parliament (MPs) would be primarily concerned with obtaining funding for their particular PC irrespective of the number of districts it intersects with, and then in making sure that available money is spent. Hence, I create a PC level dataset by overlaying village, town and district level shapefiles upon PC boundaries, thereby mapping district level information to PCs. I then create population weights as follows:
Population weight of PC_j in $District_i$, $\pi_{ij} = \frac{P_{ij}}{P_i}$
where,

P_i = Total population of $District_i$
P_j = Total population of PC_j
P_{ij} = Population of $District_i$ lying within PC_j

After obtaining the requisite weights, I calculate a weighted average of the district level NREGS, Census and DLHS data so that they represent the relevant PC. While this is an improvement over the previous attempts at assigning districts to PCs based on less precise mapping methods (see for instance (Zimmermann, 2012; Vaishnav and Sircar, 2012)), imprecise maps can lead to erroneous assignment of villages or towns – that are represented as points rather than polygons on a shape file – to neighbouring PCs. To check this, I compare 2001 census data with population weighted district data from the GIS exercise aggregated to the state level[5]. As Table 2 indicates, there are low discrepancies for most states. Further analysis revealed a maximum inter-state assignment error of less than two per cent among some districts.[6] Assuming this to be the highest assignment error in my dataset, I eliminated all cases where less than two per cent of the population of a district intersected with a constituency. This should not be problematic since there is no reason to suspect that errors of assignment are systematically related to variables of interest.

It could be argued that an elected politician may not care equally about all districts within the PC. This would be more likely in instances when only a small part of the population of a district falls within the PC. Since I am using population based weights for each district, such cases will be assigned lower weights and should not substantially affect decision-making. However, there may be threshold or non-linear effects that cannot be accounted for in this approach.

Finally, in order to assign parliamentary constituencies to NREGS implementation phase, I summed the population weights of each district intersecting with a PC by implementation wave. PCs were assigned to an NREGS wave based on an aggregated cut-off weight of 0.5 – for instance, a PC was assigned to Wave 1 if the sum of weights of Wave 1 districts in that PC was greater than 0.5. A drawback with this approach is that districts in some PCs may be spread evenly over the three waves. However, this is true for only seven PCs of which one is primarily urban.[7] I exclude them from the dataset.

[5]I conduct a state level comparison since this is the lowest geographic or administrative division at which district and PC boundaries match.

[6]A small part of Srikakulam in the state of Andhra Pradesh and Pashchimi Singhbhum in Jharkhand were placed in Orissa state.

[7]The PCs of Pandharpur (Maharashtra), Outer Manipur (Manipur), Tripura East (Tripura), Khurja and Mirzapur (Uttar Pradesh), Garhwal (Uttaranchal), and Howrah (West Bengal) were excluded.

4 Method and Results

As a first step in answering the question of interest, I examine differences of means of socio-economic and NREGS variables by the phase of program implementation. While the exact algorithm to choose districts for the NREGA phase has not been made public, it is likely to have been based on an index created by the federal government's Planning Commission to identify districts for wage and self-employment (Zimmermann, 2012). The three key variables used to rank districts were the agricultural wage rate, agricultural productivity and the proportion of SC and ST population in the district (Commission, 2003). Apart from this, any district affected by left wing extremism, as also the district with the lowest index score in each state was included in the first wave. Hence, the program was rolled out in each state in the first wave.

Mapping the district level data onto parliamentary constituencies in Table 4 indicates that on average, there were indeed significant differences between PCs assigned to Wave 1 and 2, with the former being more "backward". The average household in a PC assigned to Wave 1 was more likely to be poor, be less literate, and be a part of the ST category. These households were also distinctly worse off with respect to housing status, access to electricity and toilets. Differences in terms of possessing BPL cards, housing status, and access to electricity persisted between Wave 2 and Wave 3 PCs.

Wave 1 PCs received a higher per capita central release under the NREGS, spent more on wages and in aggregate. However, expenditure performance – the ratio of NREGS funds spent to those available – was no different between Waves 1 and 2, but statistically significantly better in Wave 2 when compared to Wave 3. Finally, in terms of electoral outcomes, there were no differences in the population of registered voters or their turnout in Wave 1 vis-à-vis Wave 2 PCs, although the former were more likely to be reserved for marginalized groups. There were no significant differences between Waves 2 and 3.

Another set of differences could lie in terms of the PCs where the INC chose to contest. Table 5 shows differences in social, economic, electoral or implementation variables depending on whether INC contested from those PCs. While there are no differences in the electoral variables like the number of registered voters, turnout or the reservation status of a PC, INC seems to contest seats where a larger share of households have BPL cards. However, no other indicators of backwardness like access to electricity and literacy rates were significantly different. Finally, there were little or no differences among demographic, electoral or NREGS related observables between PCs where INC won or lost.

4.1 Regression model and results

Next, I use a cross-sectional regression model to examine the effects of political competition on NREGS implementation. The regression equation is:

$$\mathbf{Y} = \mathbf{X}'\beta + \mathbf{Z}'\gamma + u \tag{1}$$

where,

Y is a measure of NREGS implementation in a PC

X is a measure of political competition in a PC

Z is a vector of PC level covariates

The key measure of program implementation in this paper is the ratio of expenditure on NREGS to funds available. Available funds include not only the amount received by the district in that year, but also unspent balances from the previous year(s). Deviations from the perfect scenario where the ratio is equal or close to one could arise due to late receipt of funds, bottlenecks in spending, or administrative rationing. In each case, the MP has some say in the implementation process - for instance, a concerned member of Parliament can lobby for funds in case there are delays, or push district officials to spend in a scenario where money is left unspent.

The independent variable of interest is a measure of political competition for the INC in the 2004 elections. This is defined as follows:

$$incgap_i = \begin{cases} \frac{\text{INC votes}_i - \text{Rank 2 party votes}_i}{\text{Registered voters}_i}, & \text{if INC won} \\ \frac{\text{INC votes}_i - \text{Rank 1 party votes}_i}{\text{Registered voters}_i}, & \text{if INC lost} \end{cases}$$

In concordance with the existing literature on electoral returns of poverty alleviation programs (De La O, 2013; Zucco, 2011), I use the population of eligible voters instead of the actual voters in order to control for possible endogenity of turnout in response to INC's pre-election announcement of the scheme [8].

Since NREGS is a demand driven scheme, the demand for work is likely to be higher in regions with a larger proportion of population below or close to the poverty line. In the absence of district level poverty estimates, I use district level controls indicative of the level of poverty.

Given the time period under consideration (2006-07 to 2008-09) and the phased implementation of NREGS, data is available for three years for Wave 1 districts, two years for Wave 2 districts and one year for Wave 3 districts. Instead of creating an unbalanced

[8]The sign and significance of the coefficient of interest does not change even if I use the number of actual voters; however the magnitude is lower due to the higher variance associated with the modified variable.

panel, I use an ordinary least squares estimation method, using an absorptive regression to take state-specific differences into account. This entails the subtraction of state-level means from both dependent and independent variables, thereby absorbing state-specific effects before regressing adjusted outcomes on the adjusted covariates (McCaffrey et al., 2010). I use three-year average figures for Wave 1 PCs, two-year averages for Wave 2 and 2008-09 data for Wave 3.

Regression results are shown in Table 6. The coefficient on the single linear regression in Column 1 differs from Column 2 in that the former does not control for different intercepts for each state. As seen in Table 5, there do not seem to be many observable differences between those PCs that were chosen by the INC and those which were not; however since this does not rule out selection on unobservables, I include an additional variable that indicates whether INC contested a particular PC in Specifications 5 to 9 of Table 6 to soak up possible bias.

In each case, the coefficient estimate on the variable of interest is negative, indicating that expenditure performance declined as INC's margin of victory increasedd, or that expenditure performance was better in those PCs where the INC lost by a large margin. Moreover, the magnitude of the coefficient indicates that expenditure performance deteriorated by 0.185 to 0.195 percentage points as the INC gap increased by an additional percentage point. While this seems like a fairly small effect, it should be kept in mind that the denominator of the variable of interest is the number of eligible voters, i.e., those whose names were registered on the electoral roll; hence we are talking of very large absolute magnitudes when we refer to a 1 percentage point increase in the INC gap variable. Although turnout varies considerably, the magnitude of the numbers can be inferred from the average PC comprising of 1.25 million voters with a turnout of 59.22 per cent.

The negative coefficient estimate rules out the patronage and ideology hypothesis; this is further supported by electoral history not mattering for program execution as seen in Specifications 7 to 9 of Table 6. Urgency of implementation by INC in close elections can also be eliminated since the quadratic term in Specification 6 is not significant. This leaves Downsian competition as a plausible mechanism where opportunistic political parties, apart from INC, tried to leverage the program to suit their political ends.

One could argue that these variables are not enough to account for factors like climate shocks which could lead to increased labor demand and therefore higher spending[9]. However, it should be kept in mind that the implementation wave reflects not only time and duration of implementation, and therefore intensity, but also agricultural productivity

[9]A drawback with the current estimation strategy is that these climate shocks would likely occur in one year and I use variables that are averaged over two or three years in some cases.

and wage, which are likely to be related to regions with low rainfall as long as rainfall patterns have not changed much in a decade. Further, the magnitude of the coefficient does not change even after adding average rainfall or deviations in rainfall in the concerned years in Specification 9 of Table 6, and it continues to be negative and significant at the five per cent level of significance or better.[10]

Finally, electoral history of the constituency also does not seem to matter – specifications 7 to 9 include variables that indicate whether the seat was retained by either INC or BJP over the last one or two general elections held in 1999 and 1998. None of these are significant, individually or jointly. Given this, I use Specification 5 as my primary model.

Decomposing expenditure performance into its components could point to the mechanism behind this observed trend - whether the trend is driven by lower expenditures on NREGS in the numerator, or an increase in the release of funds by the government or higher unspent in the denominator. I therefore run separate regressions with per capita rural expenditure and per capita rural central release with the same set of control variables. Results in Table 7 indicate that the former is negatively related to the INC gap, but only at the 10 per cent level, while NREGS release is not statistically significant. This finding is indicative of lower (higher) expenditure and possibly higher (lower) unspent balances in PCs where INC had a large victory margin (lost by a large margin).

4.2 Close elections

Close elections are likely to provide a stronger identification mechanism of the effect of electoral variables or political competition on NREGA implementation or funding. This is because conditional on other characteristics, winners of close elections are likely to be random (Lee and Lemieux, 2009) and therefore more likely to be more conscious of NREGS as an electoral tool. In this sub-section, I ascertain whether there is a differential impact on implementation in a sub-sample of PCs where the margin of victory was low. I experiment with multiple cutoffs in two variables that indicate the margin of victory in order to examine trends in coefficients on the variable of interest over different ranges. While the first measure is the previously defined *incgap*, the second measure is a party-neutral indicator of the victory margin, standardized to ameliorate the problem of endogenous turnout and defined as:

[10]District-level rainfall data for 2004 to 2011 was obtained from the Indian Meteorological Department. Average rainfall was calculated as the arithmetic mean of rainfall over the relevant years. Volatility was calculated as the ratio of the standard deviation of rainfall over the concerned years to the difference of the average rainfall over the relevant years from the average rainfall over 2004-11.

$$wingap_i = \frac{\text{Rank 1 party votes}_i - \text{Rank 2 party votes}_i}{\text{Registered voters}_i}$$

I then create categorical variables for close elections defined as:

$$wingap_{k,i} = \begin{cases} 1, & \text{if } wingap_i \leq k \\ 0, & \text{otherwise} \end{cases}$$

where, k is a measure of the degree of closeness of elections.

Regression results in Tables 8 and 9 indicate that while *incgap* does not help explain NREGS implementation in close elections, *wingap*, when interacted with the implementation wave has a positive effect on expenditure performance when the margin of victory is capped at three per cent [11]. Since it is negative and greater in magnitude than the *wingap3* coefficient, the net relationship between close elections and expenditure performance has a negative sign. While the decomposition of the aggregate effect of close elections through the interaction terms is a possible indicator that NREGS was in fact implemented better – at least in terms of making sure that available funds were spent well – in electorally competitive PCs that received the program early, the relationship seems unstable for lower and higher cut-offs and therefore tenuous without additional analysis.

5 Robustness checks

I use methods proposed by Altonji et al. (2000) (henceforth AET) and Oster (2013) that examine coefficient movements with the addition of controls in order to assess the amount of bias in the coefficient of the variable of interest to check for statistical identification. Both methods rely on the assumption that observable covariates included in the regression are informative about unobservable covariates which could not be included.

5.1 AET procedure

The AET method provides an "informal way to assess selectivity bias based on measuring the ratio of selection on unobservables to selection on observables that would be required if one were to attribute" the entire causal effect of the variable of interest to selection bias. In other words, the method seeks to determine how important the effect of omitted

[11] This refers to the margin of victory as defined in *wingap*. Using "per cent" is not strictly accurate since by definition, the number of actual voters will always be less than the number of potential (registered) voters. Hence, a victory margin of three as indicated by *wingap* can be scaled up to an actual victory margin of about five per cent since the average turnout is about 60 per cent.

variables would have to be to reduce the magnitude of observed coefficient on electoral outcomes to zero.

I use Specification 5 of Table 6 to estimate the AET ratio. Results indicate that selection on unobservables would have to be 1.7 times as important as selection on observables to reduce the observed effect to zero. Based on the argument in Altonji et al. (2000), such a high ratio seems extremely unlikely since I use state level dummies to control for unobservables varying across states. These could include administrative capacity, the party in power at the state level, incentives and priorities of state-level political players, including the line ministry of the rural development department in charge of implementing NREGS.

5.2 Oster method

This builds on the AET ratio to look at the movement in R^2 along with the relationship between observables and unobservables, called the "proportional selection relationship" (Oster, 2013).The primary model estimated is:

$$\mathbf{Y} = \mathbf{X}'\beta + \mathbf{W_1} + \mathbf{W_2} + \epsilon \tag{2}$$

Here, $\mathbf{X}$ is the variable of interest, while $\mathbf{W_1}$ is a vector of observable covariates and $\mathbf{W_2}$ a set of unobservables that could potentially be related to $\mathbf{X}$ or $\mathbf{Y}$.

The proportional selection relationship is defined as:

$$\delta(\frac{\sigma_{1x}}{\sigma_{11}}) = \frac{\sigma_{2x}}{\sigma_{22}} \tag{3}$$

where,
σ_{ix}=$cov(\mathbf{W_i}, \mathbf{X})$
σ_{ii}=$var(\mathbf{W_i})$

In the above equation, δ is the coefficient of proportionality and therefore an indicator of the relative importance of observables vis-à-vis unobservables. Now, if $\delta = 1$, the treatment $\mathbf{X}$ is equally related to the observed and unobserved variables. Further, if the maximum R^2 that can be expected from the ideal model is 1, the treatment variable, observables and unobservables fully explain the variation in $\mathbf{Y}$

Now, starting with a simple regression of Y on X, comparing coefficients and R^2 values by adding additional controls, gives an estimate about the importance of unobservables. Results from Oster (2013)'s *bounding argument* exercise suggest that with a maximum R^2 of 1, the coefficient of proportionality is 2.47, i.e., unobservables would have to be 2.47 times as important as the observables to reduce the observed treatment effect to

zero.[12] This is the lower bound on delta since an R^2 of 1 is unreasonably high as there will almost always be a source of random variation, or some classical measurement error in the dependent variable [13]. The results therefore concur with the results of the AET procedure, suggesting that unobservables do not likely explain away the principal findings of the paper.

6 Discussion of results and policy implications

This paper indicates that implementation of NREGS is influenced by expediencies of political actors, and these are robust to selection on unobservables. However, the fact that expenditure performance in the program improved as the INC's margin of victory (loss) decreased (increased) does not neatly fit the predictions of most models discussed in Section 2. Importantly, this contradicts traditional ideas of probabilistic voting according to which the program should have been implemented better in constituencies where the INC would have felt more vulnerable. This is confirmed in Tables 8 and 9, where we observe that a narrow margin of victory or loss is unlikely to have made a difference to expenditure performance. The negative sign of the coefficient estimate also precludes the political economy of NREGS being described as that of ideology, clientelism or pork. My results are however closer to the Bardhan and Mookherjee (2010)'s quasi-Downsian idea that transcends simple competition to include possibilities for indulging in opportunistic behavior on part of politicians.

What could have led to this? I propose this to be a result of two complementary forces, viz., a sense of INC complacency in constituencies where it had won by a large margin in 2004, accompanied by other parties being able to identify opportunities for claiming credit, regardless of whether they were indeed able to do so. This aligns with Dreze (2011)'s view that INC would not have put its weight behind enacting a program of such political and fiscal influence had it been more confident of its chances.After interpreting its rural support as a binding contract with the rural poor, and the substantial presence of Left parties in the coalition, INC felt it was faced with little alternative but to enact the scheme. But once it hit the ground, NREGS was a party-neutral tool for credit-claiming. This could have an impact through the following route: as per official guidelines, each village is supposed to draft its own work plans through meetings with the village council, but in practice local elites can decide which projects are chosen (Dutta et al., 2014). Expenditure could then be accelerated by members of Parliament backing the NREGS

[12] I use INC's choice of constituency to contest, sex ratio and NREGS implementation wave as "m controls" since they are fully observed. For a detailed discussion, see Oster (2013, p. 11).

[13] Assuming lower values of maximum R^2 – 0.9 and 0.8 – result in delta values of 2.68 and 2.94 respectively.

and having strong connections among the local elite and bureaucracy. Importantly, the incumbent politician need not be affiliated to the INC to have such an impact. While I am not able to confirm whether these mechanisms are in fact at work, or to what extent one dominates the other at this point, this concurs with a comparison of NREGS as an orphaned child of the INC that was welcomed "with open arms" by the BJP, a party with a better record of program implementation[14] (Dreze and Oldiges, 2009). It is possible that the BJP realized that the scheme provided a valuable opportunity to expand the party's base beyond its traditional urban and middle to high income supporters, giving it a strong incentive to make sure that funds received were spent. Similarly, other regional parties with a goal towards national expansion would also gain from a well-implemented program if they could claim credit.

The relevance of electoral outcomes in the endogenity of program implementation suggests that while NREGS aimed to provide a safety net and alleviate poverty, implementation quality was diluted or strengthened depending on whether the INC was in power, and the margin of victory (or loss). This indicates that multiple goals for such a program could undermine its stated goal in regions where political incentives may not be as relevant. What can be done about this? *Prima facie*, having an independent implementation agency with no political stake in the program seems like a possible method to break the link between electoral outcomes and implementation. But such an agency may not be universally desirable as opportunistic tendencies could also induce better performance. Hence, a thorough analysis of the costs and benefits of better implementation and political competition would be needed to evaluate the net welfare effects of delinking implementation from politics.

This leads one to conjecture whether demand or supply side interventions could help in improving implementation. Traditionally, the supply side comprising of the central and state bureaucracy and lower level officials at the district level has been seen as unresponsive to immediate needs, subject to capture and suffering from a lack of capacity (Dutta et al. 2014). However, demand side interventions relating to informing people about their rights under the scheme per se were unlikely to be adequate in raising actual work or wages in Bihar (Ravallion, 2013). Instead, a combination of supply and demand side interventions aiming at increasing transparency in NREGS fund flows and hitherto implementation progress may prove to be useful in improving the outcomes of interest, viz. expenditure performance, employment and asset creation. Greater transparency can be achieved by institutionalizing regular monitoring and dissemination of information among stakeholders. Switching to a transaction-based MIS like the one used by Andhra Pradesh, would force data to be entered in real time. This in turn would yield verifiable

[14]This argument is for state governments rather than national elections.

information on what is happening on the ground and help identify possible bottlenecks and sources of inefficiency. Information dissemination whether through social audits or any other means, besides telling people what to expect from the program would also acquaint them with current progress and provide a platform to air their grievances.

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Appendix

Figure 1: Map of Indian PCs with electoral outcomes

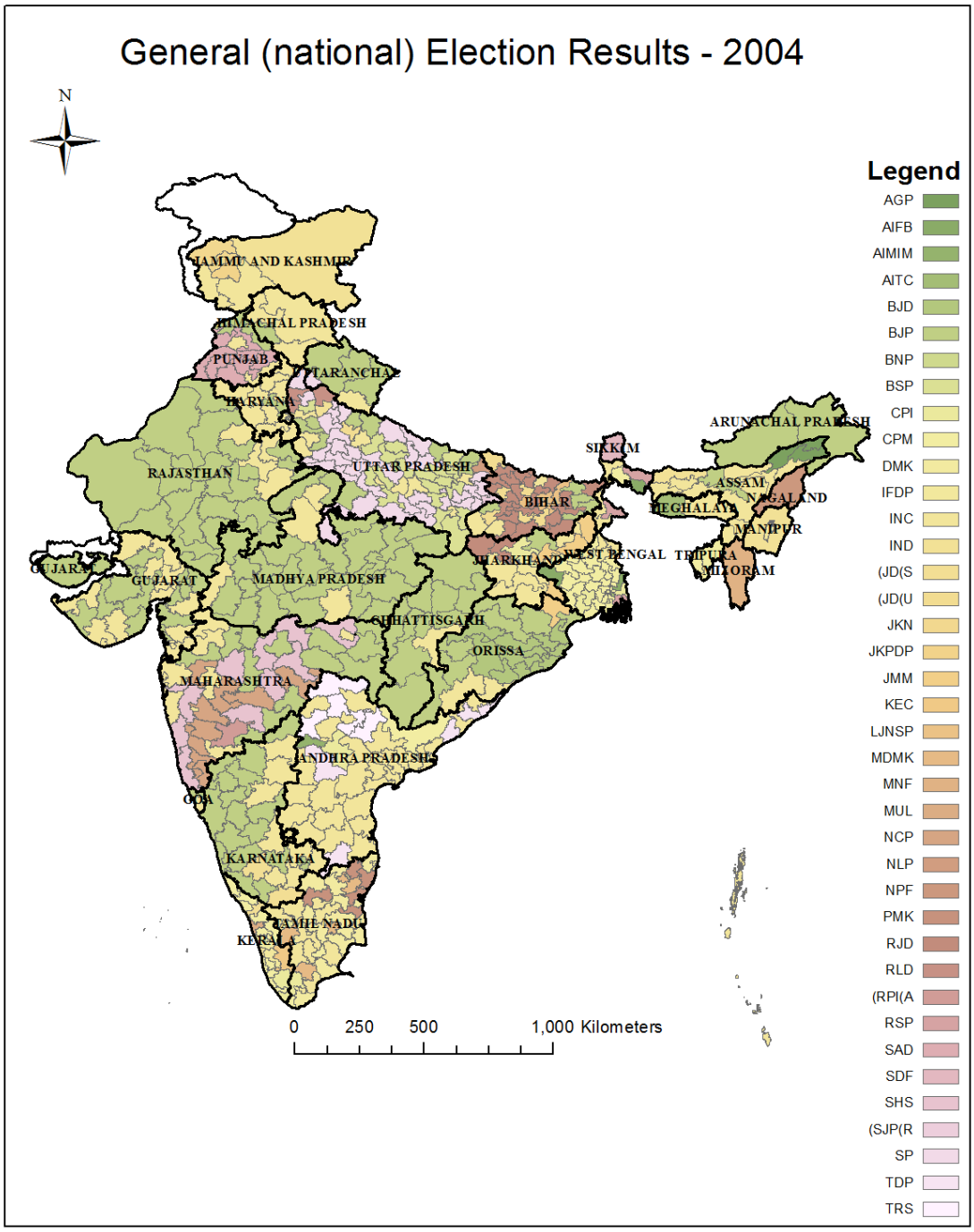

Figure 2: Overlay of PC boundaries on villages and towns in Bihar state

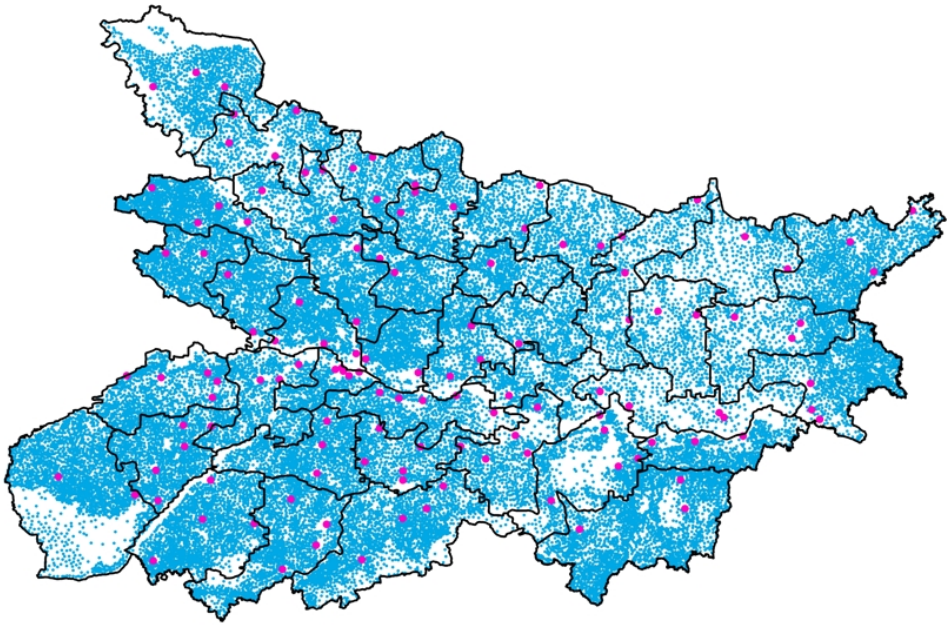

Table 1: Aggregation of districts with multiple parents

District	A	B	C
Population 2001	X_{1A}	X_{1B}	
Population 2011	X_{2A}	X_{2B}	X_{2C}
Decadal growth rate	r_A	r_B	r_C
Population 2001 (if the split had occurred before 2001)	$X'_{1A} = \frac{X_{1A}}{1+r_A}$	$X'_{1B} = \frac{X_{1B}}{1+r_B}$	
Part of parent district going to C	$X_{1A} - X'_{1A}$	$X_{1B} - X'_{1B}$	

Table 2: Comparison of Census and shape file population aggregation

S. No.	State/Union Territory	Population (Census 2001)	Population (shapefiles)	Difference
1	Andaman & Nicobar Islands*	356,152	116,198	206.50%
2	Andhra Pradesh	76,210,007	76,209,939	0.00%
3	Arunachal Pradesh	1,097,968	1,097,968	0.00%
4	Assam	26,655,528	26,424,770	0.87%
5	Bihar	82,998,509	83,004,868	-0.01%
6	Chandigarh*	900,635	808,515	11.39%
7	Chhattisgarh	20,833,803	20,833,803	0.00%
8	Dadra & Nagar Haveli*	220,490	50,463	336.93%
9	Daman & Diu*	158,204	57,348	175.87%
10	Delhi*	13,850,507	12,905,780	7.32%
11	Goa	1,347,668	1,347,668	0.00%
12	Gujarat	50,671,017	50,672,158	0.00%
13	Haryana	21,144,564	21,144,564	0.00%
14	Himachal Pradesh	6,077,900	6,077,900	0.00%
15	Jammu & Kashmir	10,143,700	10,143,700	0.00%
16	Jharkhand	26,945,829	26,945,829	0.00%
17	Karnataka	52,850,562	52,850,562	0.00%
18	Kerala	31,841,374	31,841,374	0.00%
19	Lakshadweep*	60,650	26,967	124.90%
20	Madhya Pradesh	60,348,023	60,348,023	0.00%
21	Maharashtra	96,878,627	96,284,822	0.62%
22	Manipur	2,293,896	2,174,914	5.47%
23	Meghalaya	2,318,822	2,318,822	0.00%
24	Mizoram	888,573	888,573	0.00%
25	Nagaland	1,990,036	1,951,359	1.98%
26	Orissa	36,804,660	36,805,164	0.00%
27	Puducherry*	974,345	625,426	55.79%
28	Punjab	24,358,999	24,352,724	0.03%
29	Rajasthan	56,507,188	56,507,188	0.00%
30	Sikkim	540,851	540,851	0.00%
31	Tamil Nadu	62,405,679	62,405,679	0.00%
32	Tripura	3,199,203	3,199,203	0.00%
33	Uttar Pradesh	166,197,921	166,200,868	0.00%
34	Uttarakhand	8,489,349	8,489,345	0.00%
35	West Bengal	80,176,197	80,047,123	0.16%
	India	1,028,737,436	1,025,700,458	0.30%

* *NREGS was not implemented in Union Territories.*

Table 3: Summary Statistics and Data Sources

Variable	Mean	SD	Min	Max	Source	Year
Socio-economic characteristics						
Population (log)	14.46	13.04	12.37	15.47	Census	2001
Proportion of SC households	0.2	0.08	0.01	0.61	DLHS	2007-08
Proportion of ST households	0.12	0.17	0	0.97	DLHS	2007-08
Households with kutcha house	0.34	0.22	0.02	1.23	DLHS	2007-08
BPL households	0.34	0.2	0.05	0.96	DLHS	2007-08
Households with no toilet	0.59	0.25	0	0.94	DLHS	2007-08
Households with electricity	0.66	0.27	0.09	1	DLHS	2007-08
Sex ratio	1.02	0.07	0.6	1.22	DLHS	2007-08
Adult literacy rate - rural	0.5	0.12	0.23	0.85	Census	2001
Electoral data						
Registered voters (log)	14.04	12.43	12.08	14.99	ECI	2004
Actual voters (log)	13.50	11.98	11.77	14.26	ECI	2004
Turnout	59.22	11.92	15.04	87.35	ECI	2004
Reserved PC	0.23	0.42	0	1	ECI	2004
INC gap	-0.11	0.16	-0.57	0.22	ECI	2004
NREGS data						
Expenditure/Available funds	0.66	0.21	0.06	1.27	NREGS MPR	2006-07 to 2008-09
Per capita rural release	637.27	794.87	1.28	5146.68	NREGS MPR	2006-07 to 2008-09
Per capita rural expenditure	646.4	832.91	1.88	5322.74	NREGS MPR	2006-07 to 2008-09
N	500					

Table 4: Comparison of means at PC level based on NREGS implementation wave

Variable	Wave 1 N = 170	Wave 2 N = 124	Wave 1 vs. Wave 2	Wave 3 N = 209	Wave 2 vs. Wave 3
Socio-economic characteristics					
Population (log)	14.44	14.43	-0.005	14.42	-0.013
Proportion of SC households	0.22	0.21	-0.004	0.18	-0.035*
Proportion of ST households	0.17	0.1	-0.078**	0.08	-0.019
Households with Kutcha house	0.46	0.38	-0.077**	0.23	-0.146***
BPL households	0.41	0.37	-0.038**	0.28	-0.092**
Households with no toilet	0.72	0.59	-0.131**	0.48	-0.114
Households with no electricity	0.55	0.61	0.067*	0.79	0.175***
Sex ratio	1.01	1.01	0.003	1.03	0.023
Adult literacy rate - rural	0.45	0.49	0.049**	0.55	0.051
Electoral characteristics					
Registered voters (log)	14.02	14.01	-0.011	14.03	0.022
Actual voters (log)	13.49	13.5	0.013	13.45	-0.052
Contestants	9.21	9.43	0.216	10.53	1.099
Turnout	59.86	61.97	2.107	57.16	-4.808
Reserved PC	0.35	0.2	-0.145***	0.14	-0.063
INC contest	0.72	0.77	0.051	0.81	0.034
NREGS					
Persondays - total	172.42	93.68	-78.743***	28.43	-65.249***
Per capita rural central release	1108.27	572.22	-536.049***	296.4	-275.822***
Per capita rural expenditure	1186.44	568.25	-618.190***	256.64	-311.611***
Per capita rural wage expenditure	790.01	386.72	-403.294***	189.8	-196.921***

Significance levels: * < 10% ** < 5% *** < 1%
State level cluster robust standard errors.

Table 5: Comparison of means based on INC contest decision, and INC victory

Variable	INC contested PC			INC victory		
	No contest N = 115	Contest N = 385	Difference	INC loss N = 258	INC win N = 130	Difference
Socio-economic characteristics						
Population (log)	14.41	14.43	0.027	14.44	14.43	-0.011
Proportion of SC households	0.2	0.2	-0.002	0.2	0.2	0.005
Proportion of ST households	0.08	0.13	0.047	0.12	0.13	0.011
Kutcha house	0.31	0.35	0.045	0.37	0.31	-0.062
BPL	0.28	0.36	0.083**	0.34	0.4	0.054
No toilet	0.62	0.58	-0.037	0.58	0.57	-0.009
Electricity	0.61	0.68	0.072	0.64	0.76	0.115*
Sex ratio	0.98	1.03	0.044***	1.03	1.03	-0.001
Literacy rate (rural)	0.48	0.51	0.028	0.51	0.5	-0.009
Electoral characteristics						
Registered voters (log)	14	14.02	0.024	14.03	14.01	-0.021
Turnout	59.04	59.32	0.275	58.49	60.97	2.48
Reserved PC	0.2	0.23	0.032	0.24	0.21	-0.036
NREGS						
Wave	1.94	2.12	0.179	2.13	2.09	-0.039
Persondays - total	79.63	97.19	17.562	98.21	95.17	-3.046
Expenditure/Available funds	0.59	0.68	0.092**	0.7	0.64	-0.065*
Per capita rural central release	455.29	693.17	237.883	731.08	617.94	-113.132
Per capita rural expenditure	475.64	698.7	223.059	741.05	614.64	-126.409
Per capita rural wage expenditure	355.61	466.57	110.959	482.97	434.02	-48.948

Significance levels: * < 10%, ** < 5%, *** < 1%
State level cluster robust standard errors.

Table 6: Effect of INC gap on NREGS implementation

Variables	Expenditure / Available funds								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
INC gap	0.011	-0.079*	-0.097**	-0.120**	-0.195***	-0.185**	-0.195**	-0.187**	-0.192**
	(0.08)	(0.04)	(0.05)	(0.05)	(0.06)	(0.08)	(0.08)	(0.08)	(0.08)
INC gap^2						0.042			
						(0.25)			
SC proportion				0.036	0.042	0.042	0.036	0.033	0.039
				(0.20)	(0.19)	(0.19)	(0.18)	(0.19)	(0.18)
ST proportion				0.122	0.122	0.122	0.123	0.11	0.121
				(0.08)	(0.08)	(0.08)	(0.08)	(0.09)	(0.09)
Kutcha house				0.101	0.095	0.095	0.09	0.1	0.083
				(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.09)
BPL				-0.272**	-0.271**	-0.271**	-0.272**	-0.274**	-0.288**
				(0.12)	(0.12)	(0.12)	(0.11)	(0.12)	(0.11)
No toilet				0.206*	0.205*	0.205*	0.210*	0.206*	0.234**
				(0.11)	(0.10)	(0.10)	(0.11)	(0.10)	(0.11)
Electricity				0.227**	0.231**	0.230**	0.236**	0.235**	0.241**
				(0.10)	(0.10)	(0.10)	(0.11)	(0.11)	(0.11)
Sex ratio				0.011	-0.014	-0.013	-0.014	-0.02	0.003
				(0.21)	(0.21)	(0.21)	(0.20)	(0.20)	(0.19)
Rural adult literacy rate				-0.529***	-0.551***	-0.553***	-0.557***	-0.548***	-0.543**
				(0.18)	(0.18)	(0.18)	(0.19)	(0.18)	(0.21)
NREGS Wave 2			-0.028	0.002	0	0	0.003	0.003	0.004
			(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
NREGS Wave 3			-0.108***	-0.062**	-0.062**	-0.062**	-0.065**	-0.068**	-0.066**
			(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
INC contested PC					0.044*	0.044*	0.044*	0.043*	0.042

					(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
INC retained from 1999							0.012	-0.022	-0.02
							(0.04)	(0.03)	(0.03)
BJP retained from 1999							0.041	0.021	0.026
							(0.03)	(0.03)	(0.03)
INC retained from 1998								0.068	0.069
								(0.07)	(0.07)
BJP retained from 1998								0.03	0.027
								(0.03)	(0.03)
Average rain									0
									0.00
Rain volatility									-0.025*
									(0.01)
Constant	0.659***	0.650***	0.699***	0.689***	0.682***	0.683***	0.675***	0.682***	0.622**
	(0.04)	(0.01)	(0.01)	(0.23)	(0.23)	(0.24)	(0.23)	(0.23)	(0.25)
State dummies	*N*	*Y*	*Y*	*Y*	*Y*	*Y*	*Y*	*Y*	*Y*
Observations	503	503	503	500	500	500	500	500	500
R-squared	0	0.434	0.472	0.529	0.531	0.531	0.534	0.537	0.541

State level clustered robust standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Effect of INC gap on NREGS release and expenditure

Variables	Rural per capita expenditure		Rural per capita fund release	
	(1)	(2)	(3)	(4)
INC gap	−351.471*	−215.165	−216.370	−128.471
	(194.22)	(184.50)	(185.02)	(196.77)
SC proportion	−1053.780	−998.002	−904.566	−867.909
	(525.02)	(513.28)	(442.43)	(442.00)
ST proportion	1167.785	1236.784*	973.747	1028.476
	(572.06)	(570.78)	(555.62)	(551.21)
Kutcha house	552.959	539.565	510.927	493.964
	(318.32)	(303.94)	(276.68)	(258.89)
BPL card	246.376	268.460	521.229	537.765
	(351.21)	(349.07)	(346.99)	(354.81)
No toilet	−101.958	−95.899	−198.419	−197.816
	(228.53)	(224.15)	(226.01)	(227.67)
Electricity	273.839	249.574	232.135	204.368
	(259.59)	(262.90)	(232.04)	(241.28)
Sex ratio	484.930	482.125	400.660	423.712
	(517.08)	(501.13)	(453.37)	(438.09)
Adult rural literacy rate	−654.806	−615.819	−643.497	−598.298
	(457.35)	(441.59)	(446.43)	(435.45)
NREGS wave 2	−497.625***	−497.828***	−410.290**	−409.300**
	(128.82)	(133.53)	(114.42)	(119.53)
NREGS wave 3	−857.007***	−846.424***	−737.718***	−730.386***
	(181.40)	(181.80)	(161.23)	(163.53)
INC contested PC	76.313	49.139	52.951	34.309
	(62.73)	(61.69)	(62.34)	(64.52)
INC won from 1999 to 2004		−95.780		−58.469
		(93.92)		(92.85)
BJP won from 1999 to 2004		−136.817		−69.616
		(98.79)		(115.26)
INC won from 1998 to 2004		−132.281		−97.609
		(125.11)		(100.73)
BJP won from 1998 to 2004		51.000		−9.419
		(115.84)		(137.43)
Average rainfall		−0.034		−0.031
		(0.07)		(0.05)
Rainfall volatility		−28.346		−23.680
		(61.37)		(57.29)
Constant	543.824	623.672	572.735	614.258
	(596.06)	(571.45)	(564.73)	(553.80)
State dummies	*Y*	*Y*	*Y*	*Y*

Observations	500	500	500	500
R^2	0.756	0.760	0.772	0.775

State level clustered robust standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Effect of close elections for INC on NREGS implementation

Variable	Expenditure / Available funds							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
INC gap1	0.013	−0.017						
	(0.03)	(0.03)						
INC gap1 X Wave 2		0.025						
		(0.06)						
INC gap1 X Wave 3		0.048						
		(0.05)						
INC gap2			−0.016	−0.004				
			(0.02)	(0.02)				
INC gap2 X Wave 2				−0.036				
				(0.05)				
INC gap2 X Wave 3				−0.012				
				(0.04)				
INC gap3					−0.004	0.010		
					(0.02)	(0.03)		
INC gap3 X Wave 2						−0.073		
						(0.06)		
INC gap3 X Wave 3						0.001		
						(0.03)		
INC gap5							−0.000	0.009
							(0.02)	(0.03)
INC gap5 X Wave 2								−0.048
								(0.06)
INC gap5 X Wave 3								0.003
								(0.04)

NREGS wave 2	0.002	0.001	0.001	0.004	0.002	0.012	0.002	0.014
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
NREGS wave 3	−0.061*	−0.064*	−0.061*	−0.060*	−0.061*	−0.062*	−0.061*	−0.064
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
SC proportion	0.016	0.011	0.011	0.015	0.014	0.033	0.015	0.020
	(0.19)	(0.20)	(0.20)	(0.20)	(0.19)	(0.20)	(0.19)	(0.20)
ST proportion	0.111	0.112	0.113	0.113	0.112	0.110	0.111	0.112
	(0.09)	(0.08)	(0.08)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Kutcha house	0.117	0.118	0.112	0.111	0.113	0.122	0.114	0.120
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
BPL card	−0.276*	−0.279*	−0.273*	−0.276*	−0.274*	−0.281*	−0.275*	−0.277*
	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)
No toilet	0.196	0.197	0.201	0.204	0.199	0.202	0.198	0.200
	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)
Electricity	0.230*	0.230*	0.229*	0.229*	0.230*	0.238*	0.230*	0.237*
	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)
Sex ratio	0.008	0.005	0.013	0.018	0.011	0.017	0.010	0.009
	(0.20)	(0.20)	(0.20)	(0.21)	(0.20)	(0.20)	(0.20)	(0.20)
Adult rural literacy rate	−0.521**	−0.525**	−0.517*	−0.513*	−0.520*	−0.524*	−0.520*	−0.519*
	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)
INC contested PC	−0.012	−0.011	−0.008	−0.008	−0.010	−0.009	−0.011	−0.010
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Constant	0.714**	0.721**	0.706**	0.696**	0.709**	0.691**	0.711**	0.700**
	(0.23)	(0.23)	(0.23)	(0.24)	(0.23)	(0.23)	(0.23)	(0.22)
State dummies	*Y*	*Y*	*Y*	*Y*	*Y*	*Y*	*Y*	*Y*
Observations	500	500	500	500	500	500	500	500
R^2	0.524	0.524	0.524	0.525	0.524	0.526	0.524	0.526

State level clustered robust standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: Effect of close elections on NREGS implementation

	Expenditure / Available funds							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Win gap1	−0.012	0.021						
	(0.02)	(0.02)						
Win gap1 X wave 2		0.013						
		(0.05)						
Win gap1 X Wave 3		−0.081*						
		(0.04)						
Win gap2			−0.017	0.033*				
			(0.02)	(0.02)				
Win gap2 X Wave 2				−0.041				
				(0.04)				
Win gap2 X Wave 3				−0.095				
				(0.05)				
Win gap3					−0.008	0.039*		
					(0.01)	(0.02)		
Win gap3 X Wave 2						−0.067		
						(0.04)		
Win gap3 X Wave 3						−0.074*		
						(0.04)		
Win gap5							−0.003	0.028
							(0.01)	(0.01)
Win gap5 X Wave 2								−0.054
								(0.03)
Win gap5 X Wave 3								−0.043
								(0.03)

NREGS wave 2	0.002	0.001	0.002	0.010	0.002	0.019	0.002	0.026
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
NREGS wave 3	−0.061*	−0.051	−0.061*	−0.041	−0.061*	−0.040	−0.061*	−0.042
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
SC proportion	0.015	0.024	0.020	0.023	0.017	0.018	0.015	0.014
	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)	(0.20)	(0.19)	(0.20)
ST proportion	0.111	0.116	0.113	0.125	0.112	0.109	0.111	0.105
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Kutcha house	0.114	0.106	0.115	0.097	0.113	0.097	0.114	0.103
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
BPL card	−0.271*	−0.258	−0.268*	−0.273*	−0.270*	−0.279*	−0.274*	−0.281*
	(0.12)	(0.13)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)
No toilet	0.197	0.203	0.196	0.202	0.197	0.202	0.198	0.205
	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)
Electricity	0.227*	0.229*	0.226*	0.227*	0.230*	0.229*	0.229*	0.229*
	(0.10)	(0.11)	(0.11)	(0.11)	(0.11)	(0.10)	(0.11)	(0.11)
Sex ratio	0.013	0.026	0.011	0.024	0.011	0.016	0.010	0.007
	(0.20)	(0.21)	(0.20)	(0.21)	(0.20)	(0.21)	(0.20)	(0.20)
Adult rural literacy rate	−0.521*	−0.513*	−0.521*	−0.509*	−0.525*	−0.531**	−0.522*	−0.521*
	(0.19)	(0.19)	(0.19)	(0.18)	(0.19)	(0.19)	(0.19)	(0.19)
INC contested PC	−0.011	−0.015	−0.011	−0.011	−0.011	−0.008	−0.011	−0.008
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Constant	0.712**	0.684*	0.714**	0.686**	0.714**	0.702**	0.713**	0.704**
	(0.23)	(0.25)	(0.22)	(0.24)	(0.23)	(0.23)	(0.23)	(0.24)
Observations	500	500	500	500	500	500	500	500
R^2	0.524	0.528	0.525	0.531	0.524	0.529	0.524	0.527

State level clustered robust standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$