The Review *of* Economics *and* Statistics

VOL. CIII DECEMBER 2021 NUMBER 5

THE DYNAMIC ELECTORAL RETURNS OF A LARGE ANTIPOVERTY PROGRAM

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Abstract—Governments around the world use short-term reelection strategies. This is problematic if governments can maximize their reelection chances by prioritizing short-term spending before an election over long-term reforms. This paper tests whether longer program exposure has a causal effect on election outcomes in the context of a large antipoverty program in India. Using a regression-discontinuity framework, the results show that length of program exposure lowers electoral support for the government. The paper discusses a couple of potential explanations, finding that the most plausible mechanism is that voters hold the government accountable for the program's implementation quality.

I. Introduction

GOVERNMENTS around the world make extensive use of short-run reelection strategies before an election, and a large literature suggests that this is often a successful strategy.[1] This is a problematic phenomenon for three reasons: First, election outcomes may be heavily influenced by luck or strategic short-run spending before the election rather than by the incumbent's level of competence.[2] Second, governments will have an incentive to focus on policies with quick payoffs over potentially more ambitious policies whose benefits only materialize in the medium to long run.[3] And third, if programs are announced and implemented relatively shortly before an election, voters may be unable to find out how large the benefits actually are.[4] All of these issues taken together mean that the accountability mechanism of elections in democratic settings breaks down.

While these issues affect democracies worldwide, they are of particular importance in developing countries. Developing countries face large-scale socioeconomic problems, so consistent long-term policies are especially important. At the same time, many government programs suffer from low program awareness and problems with implementation quality, and research shows that short-run reelection strategies tend to be particularly successful.[5] It is therefore crucial to understand if developing country governments can derive medium-run benefits from the introduction of ambitious antipoverty programs.

How longer exposure to a government program affects election outcomes is a priori unclear. Two plausible explanations that predict an increase in government support are reciprocity for experienced program benefits and higher program awareness (Finan & Schechter, 2012). Longer exposure allows program benefits to be more fully realized and for more potential voters to experience the program. If beneficiaries reciprocate by voting for the incumbent, electoral support for the government increases over time. Similarly, awareness of a new policy may initially be low. Over time, households are more likely to learn of the scheme through word-of-mouth, other beneficiaries, or the media. This may allow them to become beneficiaries themselves or to hold a more informed opinion on the program.

But the medium-run effects of a government program could also be negative. For a program to have a longer-lasting impact on electoral support, program salience has to remain high. If voters become used to the program benefits or if they are less important than other issues, electoral support for the government will decline as the newness of the program wears off. A discrepancy between promised and actual program benefits could similarly lead to a decline in voter support over time. Longer exposure gives beneficiaries more

Received for publication March 14, 2018. Revision accepted for publication March 13, 2020. Editor: Shachar Kariv.

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I thank Manuela Angelucci, Raj Arunachalam, Arnab Basu, Jacob Shapiro, Jeffrey Smith, Rebecca Thornton, Dean Yang, and participants at the CSAE Conference, the Annual Conference on Economic Growth and Development in Delhi, the Delhi School of Economics Winter School, Cornell University, and the University of Michigan for valuable comments, feedback and suggestions. Nabaneeta Biswas, Taylor Gates, Hanna Han, and Jenica Moore provided excellent research assistance. An earlier version of the paper was circulated under the title "Jai Ho? The Impact of a Large Public Works Program on the Government's Election Performance in India."

A supplemental appendix is available online at https://doi.org/10.1162/rest_a_00935.

[1]See Akhmedov and Zhuavskaya (2004), Alesina, Mirrlees, and Neumann (1989), Brender and Drazen (2005), Healy and Lenz (2014), and Shi and Svensson (2006). See Drazen (2000) and Healy and Malhotra (2013) for literature overviews.

[2]Empirical evidence suggests, for example, that U.S. voters have sometimes elected presidents who managed the economy less well overall because of slightly higher income growth in election years (Alesina et al., 1993; Bartels, 2008).

[3]Majumdar and Mukand (2004) set up a theoretical model that predicts that governments can be too conservative or too reckless early on during the election term but display inefficient policy persistence later. They also provide case studies that are consistent with this model.

[4]See Canes-Wrone et al. (2001) for a theoretical model that incorporates this factor.

[5]See Akhmedov and Zhuavskaya (2004), Brender and Drazen (2005, 2008), Drazen and Eslava (2010), and Finan and Schechter (2012).

The Review of Economics and Statistics, December 2021, 103(5): 803–817

https://doi.org/10.1162/rest_a_00935

time to experience the actual program benefits and adjust their initial expectations. If implementation quality is low or if the program is less effective than expected, this disappointment will be reflected in falling government support.

Finally, length of program exposure may not affect election outcomes at all. In countries with a long history of failed government initiatives, voter interest in a new antipoverty program may be low. Similarly, voters may be disillusioned with their ability to hold a government accountable for its performance or may be skeptical about finding a better alternative among the opposition. Many voters in developing countries also consider the importance of social group identities such as caste or religion when making their decision (Chandra, 2004). If identity trumps performance, government programs will have little influence on the election outcome.

To test the empirical impact of longer program access on voting behavior, a government program needs to be rolled out in a manner that can be exploited in a causal analysis. Such a setup is difficult to find in practice since many large government programs are implemented quickly and nonrandomly. This paper contributes to addressing the gap in the literature by focusing on the introduction of the world's largest public-works program, India's National Rural Employment Guarantee Scheme (NREGS). NREGS legally guarantees each rural household up to 100 days of manual public-sector work per year at the minimum wage. It is supposed to be a demand-driven program under which households self-select into employment at any time during the year. The goal of the program is to provide a predictable and flexible safety net for the rural poor and to reduce rural-to-urban migration. NREGS take-up is highest during the agricultural off-season, when there are few alternative employment opportunities in rural labor markets. NREGS was rolled out between 2006 and 2008 in three implementation phases before the government stood for reelection in 2009. By the time of the general election, districts from the earliest implementation phase (phase 1) had had access to NREGS for two full agricultural off-seasons. Phase 2 and phase 3 districts had experienced NREGS for one and zero full agricultural off-seasons, respectively. To analyze whether there are medium-run election benefits from NREGS, the paper therefore concentrates on comparing election outcomes in phase 1 and phase 2 districts.

The empirical analysis exploits information about the assignment algorithm of Indian districts to program phases. Each state first received a quota of treatment districts proportional to the prevalence of poverty in that state. In the second step, the state quota was filled with the poorest districts according to a poverty ranking. This algorithm generates state-specific treatment discontinuities with respect to the length of program exposure, which can be exploited in a regression-discontinuity (RD) design. The analysis also makes use of a newly digitized data set of polling-station wise election results for the 2009 Indian general election with close to 600,000 observations.

The main RD results show that votes for the government in phase 1 implementation areas are substantially lower than in phase 2 areas. The probability of winning the race at a polling station decreases by 19 percentage points in areas with longer NREGS exposure at the cutoff. This effect is robust across different empirical specifications, and the results are similar when aggregated to the parliamentary constituency level.

The main results are consistent with a loss of salience explanation or with voters holding the government accountable for low implementation quality. They do not support other mechanisms such as reciprocity or a rise in program awareness over time. To disentangle the remaining potential channels, I split the sample by a widely accepted classification of NREGS implementation quality. Further analysis reveals that NREGS take-up is much higher in phase 1 than in phase 2 districts in well-implemented areas, whereas there is little difference in take-up between phases in other districts. Consistent with this evidence, there are also heterogeneous treatment effects for election outcomes: The overall negative impact of longer NREGS exposure on government support is driven by areas with low implementation quality, whereas there is no drop-off in well-implemented areas. While these heterogeneous treatment effects are not causal and other explanations cannot be completely ruled out, they suggest that the explanation that is most consistent with the results is that voters are holding the government accountable for the program's implementation quality. Further quantitative, qualitative, and circumstantial evidence supports the importance of this mechanism.

These results contribute to our understanding of voting behavior in developing countries. While a large literature analyzes how voters make decisions, much of it focuses on understanding political business cycles by showing that voters are fairly myopic and put a lot of weight on outcomes shortly before the election.[6] A smaller literature has developed on understanding the electoral impact of antipoverty programs in developing countries, documenting large pro-incumbent effects.[7] My paper extends the existing research in a number of important ways. First, the dynamic variation in program rollout allows me to analyze how the length of program exposure affects voter behavior. Most of the literature on the electoral returns of government programs compares treatment and control areas and finds positive pro-incumbent effects. But it remains unclear whether the impacts would have been larger or smaller if the program had been introduced at a different time (Bartels, 2008; Cole, Healy, & Werker, 2012; Singer & Carlin, 2013). As the results of my paper show, the start date is important. Only De la O (2013) compares early and late treatment areas in her study of the electoral effects of Progresa in Mexico and finds a positive effect of length of program exposure on incumbent votes. This is the opposite of

[6]See the literature overviews in Drazen (2000) and Healy and Malhotra (2013).

[7]See the studies of conditional cash transfer programs in Brazil (Zucco, 2010), Colombia (Baez, Camacho, Conover, & Zárate, 2012; Nupia, 2011), Mexico (De la O, 2013), the Philippines (Labonne, 2013), Romania (Pop-Eleches & Pop-Eleches, 2012), and Uruguay (Manacorda, Miguel, & Vigorito, 2011).

the effect I find in India, where longer exposure has a negative impact on the government's election performance. Additionally, the analysis in De la O (2013) is limited by having access to only about 500 villages and aggregated election data at the precinct level. In contrast, I have access to a large sample of polling-station-level election data across Indian states and across three implementation phases. This allows me to take a more nuanced look at the dynamic patterns and to disentangle a number of potential explanations.

Second, the dynamic variation also allows me to test how long after its initial introduction NREGS remains an important topic for voters. Most of the literature focuses on analyzing voters' responses to policy initiatives introduced shortly before an election. The results in this paper show that the electoral effects of large government programs like NREGS can persist much longer. In addition to De la O (2013), only one other paper addresses this point.[8] Bechtel and Hainmueller (2011) show that the concentrated disaster relief program after a flood in East Germany led to increased pro-incumbent support in more than one election. But this program was temporary, was of high implementation quality, and was implemented in a developed country after a severe natural disaster. My paper instead focuses on the dynamic effects of a large antipoverty program with implementation quality problems in a developing country and confirms that the electoral impacts of government programs can be much longer lasting than suggested by the literature.

Third, I exploit variation in the implementation quality of NREGS to explore the heterogeneous treatment effects of voter responses over time. The literature focuses exclusively on the analysis of well-implemented programs, either in the developed country context or with the study of well-working conditional cash transfer programs mostly in Latin America. NREGS provides the often more common case of a government program that faces severe implementation quality challenges in some areas. The results are consistent with voters attaching a lot of importance to implementation quality and being willing to hold the government accountable for the working of the program. This improves our understanding of the electoral benefits of antipoverty programs.

Fourth, the results in this paper contribute to the broader literature of how citizens vote in developing countries. The findings are consistent with evidence that better-informed voters increase the electoral accountability of governments and reduce malpractice.[9] In the Indian context, the results suggest that voters believe that their vote is important for holding governments accountable for their actions and that they are not disillusioned with the process even with a history of past failed government initiatives and empty campaign promises. Some of the results of the extended analysis also suggest that voters carefully think about who to hold accountable for the working of NREGS in a federal system, where a large part of the responsibility for a successful implementation lies with lower tiers of government that may be controlled by the opposition.

The rest of this paper is organized as follows: Section II describes necessary background information about the working of NREGS and the Indian electoral system. Section III discusses the rollout of NREGS and the empirical estimation strategy, and presents the data sources and some summary statistics. Section IV discusses the results. Section V concludes.

II. Background

A. *India's Political System and the 2009 General Election*

India has a first-past-the-post electoral system. In each parliamentary constituency, the candidate with the most votes wins the seat in the parliament's lower house, the Lok Sabha. The autonomous Election Commission of India (ECI) sets the election dates and monitors the electoral process. The ECI has a good reputation as a neutral institution ensuring fair and smooth elections. It is regularly identified as the most trusted institution by citizens in surveys and has the power to subject party behavior to a strict code of conduct in the weeks before the election (Centre for the Study of Developing Societies, 2009). The rules include specifications meant to level the playing field between the incumbent government and the opposition once elections have been called, for example, by prohibiting governments from implementing any program that could be used as an electoral incentive.

On election day, the index finger of each voter is marked with indelible ink to avoid voter fraud, and ballots are cast using electronic voting machines. Election officials are randomly assigned to polling stations and are informed of their assignment only the day before the election when they report for duty. This ensures that election officials are assigned to an unfamiliar area and have little time to manipulate the voting process (Banerjee, 2014). Voter turnout in Indian elections tends to be high and is generally higher the lower the socioeconomic status. Especially poor citizens often see voting as their duty and as an opportunity to affect government policy, since elections are one of the few occasions when politicians visit villagers and listen to their concerns (Banerjee, 2014; Yadav, 1999).

During the period analyzed in this paper, coalition governments were common. An alliance of political parties, the United Progressive Alliance (UPA), won the 2004 general election.The coalition included a big national party, the Indian National Congress (INC), and thirteen smaller parties with mostly regional strongholds.[10] Nevertheless, the UPA

[8]Manacorda et al. (2011) document that the pro-incumbent effects of a conditional cash transfer program in Uruguay persist for three months after the program is terminated.

[9]See Pande (2011), Banerjee, Kumar, Pande, and Su (2011), Healy and Lenz (2014), and Kendall, Nannicini, and Trebbi (2015).

[10]The small UPA member parties of the 2004 government are Rashtriya Janata Dal, Dravida Munnetra Kazhagam, Nationalist Congress Party, Pattali Makkal Katchi, Telangana Rashtra Samithi, Jharkhand Mukti Morcha, Marumalarchi Dravida Munnetra Kazhagam, Lok Jan Shakti Party, Indian

government was a minority government and depended on external support from other parties.[11]

For administrative and security reasons, the election was held in five phases between April 16 and May 13, and the results were announced on May 16. Voting took place at over 800,000 polling booths across the country. Pre-polls had suggested a close race between the UPA government coalition and the opposition with a slight edge for the UPA, so the strong performance of the UPA and its biggest party, the INC, came as a surprise for most experts (Ramani, 2009): the UPA won 262 of the 543 seats (2004: 218), with INC winning 206 seats, an increase of 61 seats relative to the 2004 election results.[12]

B. *NREGS and the Election*

The NREGS is one of the largest and most ambitious government antipoverty programs in the world.[13] The scheme is based on the National Rural Employment Guarantee Act (NREGA), which was passed in the Indian parliament in August 2005. It provides a legal guarantee of up to 100 days of manual public-sector work per year at the minimum wage for each rural household. There are no other eligibility criteria, so households self-select into NREGS work and can apply at any time.[14] NREGS was nonrandomly rolled out across India in three phases: 200 districts received the program in February 2006 (phase 1), 130 additional districts started implementation in April 2007 (phase 2), and the remaining rural districts got NREGS in April 2008 (phase 3) (Ministry of Rural Development, 2010). The Election Commission had decided in 2006 that NREGS would not be allowed to be extended to more districts after the announcement of elections in any state, and that with very few exceptions, employment would need to be provided in ongoing projects during that time.[15] These provisions came into effect with the start of the election campaign.

While NREGS is in theory available year-round, in practice there were implementation delays between a couple of weeks to a couple of months in many districts, so that official start date and actual start date of NREGS differ substantially.[16] The seasonality of NREGS is also well documented. Take-up of the scheme is highest in the agricultural off-season (typically March to May) when few alternative employment opportunities are available in many rural labor markets (Imbert & Papp, 2015). Together with the ECI rules, this implies that by the time of the election in 2009, citizens had had access to NREGS for two, one, or zero full agricultural off-seasons in phase 1, phase 2, and phase 3 districts, respectively.

A number of papers analyze the economic impacts of the employment guarantee scheme. Using difference-in-difference approaches, empirical analyses often suggest low overall benefits but positive impacts on public employment and private-sector wages in the agricultural off-season, in areas with high implementation quality, and among casual workers (Azam, 2012; Berg, Bhattacharyya, Durgam, & Ramachandra, 2018; Imbert & Papp, 2015). Zimmermann (2019) uses a regression-discontinuity framework and finds that NREGS is primarily used as a safety net rather than as an additional form of employment and does not lead to an overall increase in public-sector employment, the casual private-sector wage, or household income.

A growing literature suggests that this is due to general implementation problems and substantial state heterogeneity in the effectiveness of NREGS. While the goal of the program was to create employment and improve local development through public-works projects, in practice the scheme focuses mostly on drought-proofing measures rather than on infrastructure improvement.[17] Especially in poorer states, rationing of NREGS employment and corruption in the form of ghost workers and wage underpayment are common (Dutta, Murgai, Ravallion, & van de Walle, 2012; Niehaus & Sukhtankar, 2013a). Most households receive substantially fewer days of employment than the promised 100 days despite large interest in the scheme, and Niehaus and Sukhtankar (2013a, 2013b) find that an increase in the minimum wage was not passed through to workers in the state of Orissa. This state heterogeneity is also routinely found in field reports of the employment guarantee scheme, where the program seems to work relatively well in the so-called star states (Andhra Pradesh, Chhattisgarh, Madhya Pradesh, Rajasthan, and Tamil Nadu) but faces severe challenges in the rest of the country (see Dreze & Khera, 2009; Khera, 2011). Overall, the literature therefore suggests that while NREGS creates economic benefits, they tend to be substantially lower than the promised benefits of the scheme.

Union Muslim League, Jammu and Kashmir Peoples Democratic Party, Republican Party of India, All India Majlis-e-Ittehadul Muslimen, and Kerala Congress. Before the 2009 general elections, four parties left the government coalition: Telangana Rashtra Samithi, Marumalarchi Dravida Munnetra Kazhagam, Jammu and Kashmir Peoples Democratic Party, and Pattali Makkal Katchi. The empirical results are robust to excluding these parties from the UPA definition. Additional parties joined the UPA for the 2009 elections, but I use the 2004 definition for my empirical analysis.

[11] The parties lending outside support include the Communist Party of India (Marxist), the Communist Party of India, the Revolutionary Socialist Party, the All India Forward Bloc, the Bahujan Samaj Party, and the Samajwadi Party.

[12] See the online appendix for additional details.

[13] For more details on the scheme see Dey, Dreze, and Khera (2006), Government of India (2009), Ministry of Rural Development (2010), and Zimmermann (2019).

[14] Men and women are paid equally, and at any given time, at least one-third of the NREGS workforce is supposed to be female. Wages are the state minimum wage for agricultural laborers, although NREGA specifies national floor and ceiling values for the minimum wage.

[15] See http://www.righttofoodindia.org/data/ec2006nregacodeofconduct.jpg.

[16] Economist and social activist Jean Dreze noted in September 2006: "Most of [the Indian states] are in breach of the Act for failing to put in place a 'Rural Employment Guarantee Scheme' (REGS) within six months." More information is at http://www.thehindu.com/todays-paper/tp-opinion/national-employment-guarantee-inaction/article18464791.ece.

[17] The breakdown of projects (2008–2009) was 46% water conservation, 20% provision of irrigation facility to land owned by lower-caste individuals, 18% land development, 15% rural connectivity (roads), and 1% any other activity (Ministry of Rural Development, 2010).

The Indian National Congress (INC) made the explicit promise to implement an employment guarantee scheme in its manifesto for the 2004 general election.[18] It was the main opposition party at the time and listed the program as an important measure that they would implement to improve the living conditions of the poor. Once in office, the new UPA government coalition faced substantial pressure from social activists, community organizations, workers' organizations, and other segments of civil society to keep its promise.[19] A first draft of an employment guarantee act, similar to the final provisions of NREGA and written by social activists Nikhil Dey and Jean Dreze, was circulated in fall 2004.[20]

When NREGS was passed in parliament a year later, it differed importantly from past, failed government development initiatives with respect to its legal status, scope, and prominence in the government's agenda. While previous antipoverty programs had usually been temporary and limited to particularly underdeveloped areas or to specific households, the employment guarantee scheme was open to all rural households across the country, making it the world's largest public-works program. These ambitious characteristics made it the flagship program of the UPA government. Various tiers of government were involved in a large-scale awareness campaign for the scheme, and NGOs, social activists, and other organizations were active in providing information about program details and worker rights. Advertisement materials by all players stressed the differences of NREGS relative to earlier schemes and especially the fact that NREGS had created a legal right for work.[21]

During the 2009 general election campaign and despite the mixed implementation quality of NREGS across states in practice, the government stressed NREGS as one of its main successes and as an integral part of the overall vision to create a better life for the country's poor.[22] While election campaigns had often focused on the poor, political experts stress that in contrast to the mere lip service of previous campaigns, the introduction of NREGS improved credibility.[23] Many commentators therefore believe that one important factor for the UPA's unexpected electoral success in 2009 was its focus on welfare policies and especially NREGS (Ramani, 2009).[24] The pro-poor election campaign is widely believed to have resonated with the electorate, and INC leaders have also claimed that the electoral victory was in large part due to NREGS. (See Khera, 2011.)

III. NREGS Rollout and Empirical Strategy

A. *NREGS Rollout and the Assignment Algorithm*

NREGS was rolled out in three implementation phases according to an algorithm. As Zimmermann (2019) explains, the algorithm has two stages. First, each Indian state is allocated a quota of treatment districts, which is proportional to the percentage of India's poor living in that state as measured by the headcount ratio times the rural state population. This ensures interstate fairness in program assignment. Second, the quota of treatment districts for each state is filled with the poorest districts based on a development ranking.

Head count ratio data come from 1993–1994 nationally representative National Sample Survey (NSS) data (Planning Commission, 2009). The development index comes from a 2003 Planning Commission report, which created an index of economic underdevelopment using three variables for seventeen major states: agricultural wages, agricultural productivity, and the district proportion of low-caste individuals (Scheduled Castes and Scheduled Tribes) (Planning Commission, 2003).[25] Districts were ranked on their index values, and the same ranking is used for all implementation phases.[26]

The two-step algorithm creates state-specific treatment cutoffs between implementation phases, one between phase 1 and phase 2, and the other one between phase 2 and phase 3.[27] Since the general elections took place in 2009 when all rural districts had NREGS, the phasing in of the program provides variation in the length of time districts had been implementing the scheme. Ranks are made phase- and state-specific and are normalized so that a district with a normalized state-specific rank of zero is the last program-eligible district in a

[18]The manifesto states: "A national Employment Guarantee Act will be enacted immediately. This will provide a legal guarantee for at least 100 days of employment on asset-creating public works programmes every year at minimum wage, for every rural household." (http://aicc.org.in/web.php/making_of_the_nation/resolution_detail/13#.WGkVBOEWPEY).

[19]Economist and social activist Jean Dreze argued that in contrast to previous failed initiatives, NREGS would create a legal right to work that would be enforceable in courts. This would hold the government accountable and make it more likely that the scheme would be a long-term initiative. See Jean Dreze's article in the well-known newspaper *The Hindu* from November 2004: http://www.thehindu.com/2004/11/22/stories/2004112205071000.htm. See also http://www.ipc-undp.org/pub/IPCOnePager16.pdf.

[20]As Nikhil Dey and Jean Dreze note in October 2004: "Workers' organisations have been demanding a national Employment Guarantee Act (EGA) for many years. This 'primer' was prepared to facilitate public discussion of this issue at all levels—from remote villages to the national capital." See http://www.sacw.net/Labour/EGAprimer.html.

[21]See the online appendix for additional details.

[22]See the online appendix for additional details.

[23]See the comments on the election results by political science professors Thachil at casi.ssc.upenn.edu/iit/thachil and Kumbhar at www.mainstreamweekly.net/article1382.html.

[24]Other explanations include the strong leadership skills of INC leaders Sonia and Rahul Gandhi, the corruption-free image of Prime Minister Manmohan Singh, intraparty problems in the opposition party BJP, and regional factors (Economic and Political Weekly, 2009; Ramani, 2009).

[25]Data on the outcome variables were unavailable for the remaining Indian states. I therefore restrict the empirical analysis to these seventeen states: Andhra Pradesh, Assam, Bihar, Chhattisgarh, Gujarat, Haryana, Jharkhand, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Orissa, Punjab, Rajasthan, Tamil Nadu, Uttar Pradesh, and West Bengal.

[26]Therefore, a district just above the cutoff for phase 1 by design is at the top of the poorest districts that remain untreated after phase 1 and is therefore prioritized in phase 2.

[27]In addition to the algorithm, the government had a separate list of 32 districts heavily affected by Maoist violence. See Planning Commission (2005). These districts were not subject to the algorithm, and all received NREGS in the first implementation phase. In order to closely replicate the algorithm used, I drop these districts from the sample. The results are robust to including them and assigning them a predicted treatment status based on their economic development index values.

state in a given phase.[28] This means that data can be easily pooled across states. To analyze the medium-run impacts of NREGS exposure, the empirical analysis concentrates on the cutoff between phase 1 and phase 2, although results for the cutoff between phase 2 and phase 3 will also be reported.

The overall prediction success rate of the assignment algorithm is 83% in phase 1 and 82% in phase 2. It is calculated as the percent of districts for which predicted and actual treatment status coincide.[29] This means that there is some slippage in treatment assignment in both phases. Nevertheless, the algorithm performs quite well in almost all states, and the prediction success rates are considerably higher than would be expected from a random assignment of districts, which are 40.27% for phase 1 and 37.45% for phase 2, respectively. Deviations from the algorithm are most likely explained by the political reality of Indian politics, which required negotiation, although this does not create problems for the internal validity of the analysis. As shown below, there is no evidence of a discontinuity in political variables at the cutoff at baseline.[30]

The main assumption in an RD design is that treatment areas were unable to perfectly manipulate their treatment status, the length of exposure to NREGS. Observations close to the cutoff should then differ only with respect to their treatment status (Lee, 2008). In the case of the two-step algorithm, this implies that manipulation did not occur in either step. The data sources used and transparency in the index creation make this plausible. The head count poverty ratio used data from the mid-1990s, which had long been available by the time the NREGS assignment was made. The economic underdevelopment index was also constructed from outcome variables collected in the early 1990s, eliminating the opportunity for districts to strategically misreport information. Additionally, the original Planning Commission report proposed targeting the 150 least developed districts, but NREGS cutoffs were higher than this even in phase 1 (200 districts in phase 1). Finally, the Planning Commission report lists the raw data as well as the exact method for the index creation.

Figures 1a and 1b focus on the distribution of index values over state-specific ranks. They plot the relationship between the Planning Commission's index and the normalized state-specific ranks for the phase 1 and phase 2 cutoffs, respectively. In general, poverty index values are smooth at the cutoff of 0, suggesting again that manipulation is not a big concern.[31]

[28]Rank data in the seventeen major states are complete for all rural districts.

[29]Prediction success rates for phase 2 are calculated after dropping phase 1 districts.

[30]See the online appendix for details. The Indian government was a minority government and had to rely on outside support for its policy initiatives. In a federal system, implementation of central programs also depends on the cooperation of lower levels of government, which may be ruled by the opposition. Deviating districts do not systematically differ from nondeviating districts on economic or political outcomes at baseline, with the exception that opposition-governed areas were given preferential treatment.

[31]The Frandsen test (Frandsen, 2017), similar to the McCrary test for discrete running variables, also does not reject the null hypothesis of no manipulation. See online appendix.

Figures 1c and 1d show the probability of receiving NREGS in a given phase for each bin, as well as fitted quadratic regression curves and corresponding 95% confidence intervals on either side of the cutoff. The graphs demonstrate that the average probability of receiving NREGS jumps down about 40 percentage points at the discontinuity in both phases. This suggests that there is indeed a discontinuity in the treatment probability at the cutoff.

B. *Data and Variable Creation*

The primary data source used in the empirical analysis is election data for the 2009 general election from the Election Commission of India.[32] Documents containing polling-station election results were digitized to create a data set of election outcomes for the states with NREGS algorithm information. The data contain the names of all candidates, their party affiliation, and the number of votes received per candidate at the polling station, as well as some limited candidate background information like gender, age, and broad caste category. Unfortunately, information on the number of eligible voters is often missing, so voter turnout cannot be studied at this disaggregated level.

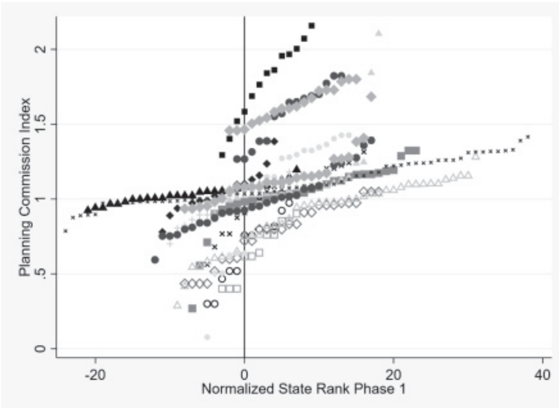
A common problem with using election data is that election constituency boundaries do not coincide with administrative boundaries. Parliamentary constituencies are created to ensure a roughly equal vote-to-seat ratio across the country, and each parliamentary constituency in India elects one politician to the Indian parliament. NREGS was rolled out at the district level, however, so different parts of the district can be part of different parliamentary constituencies. The use of polling station data allows matching each observation to the correct district regardless of its parliamentary constituency, so there is no concern about measurement error when combining election data with other information.

To the election data I merge information on the poverty index rank from the 2003 Planning Commission Report, district population size from the 2001 Census, as well as information on a district's NREGS phase. This creates a data set containing close to 600,000 polling stations.[33] Since India has a first-past-the-post system, receiving the most votes is a more relevant outcome than the achieved vote share. My analysis therefore focuses on the winning outcome, although the vote share results are qualitatively similar. I create index variables equal to 1 if a given party or alliance won the most votes at a polling station and 0 otherwise. Since the politician with the most votes in the parliamentary constituency (rather than at the polling station level) is elected to parliament, robustness checks that aggregate the data to the parliamentary constituency level demonstrate that the polling station results

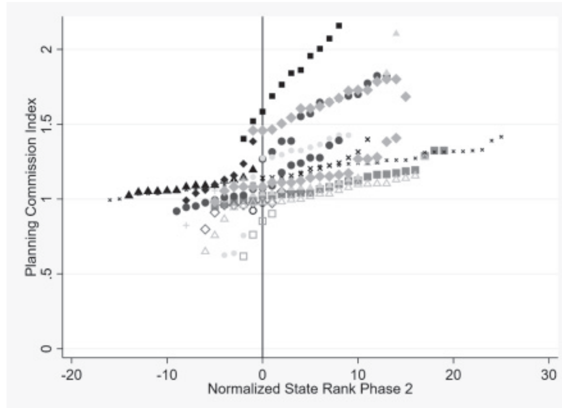
[32]Data are publicly available at http://eci.nic.in.

[33]Occasionally election results for a few polling stations cannot be digitized due to problems with the scanned documents available on the Election Commission of India website. In a few cases, hyperlinks are not working and do not allow a download of the election outcomes. This affects the state of Jharkhand, which does not have any working links on the website, but for all of the other states, these issues are not large concerns.

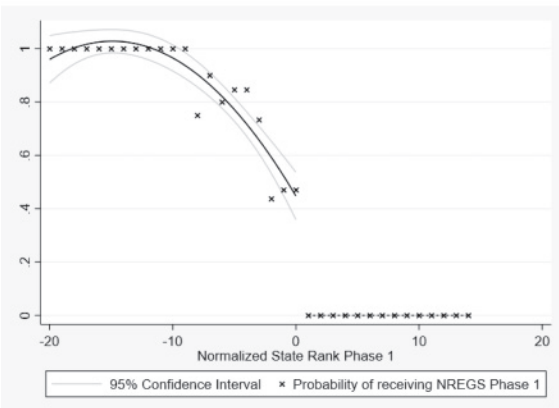
FIGURE 1.—DISTRIBUTION OF INDEX, DISCONTINUITIES, AND BALANCE TESTS BY PHASE



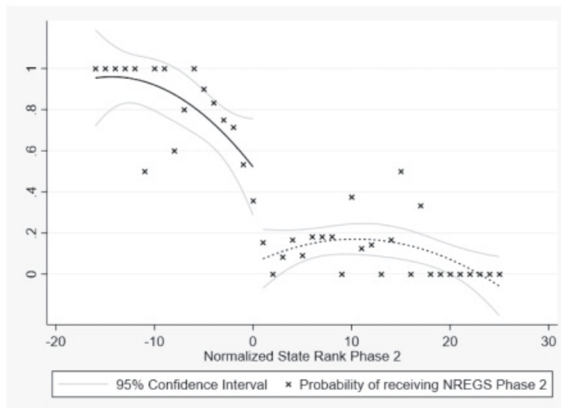
(a) Distribution of Index - Phase 1



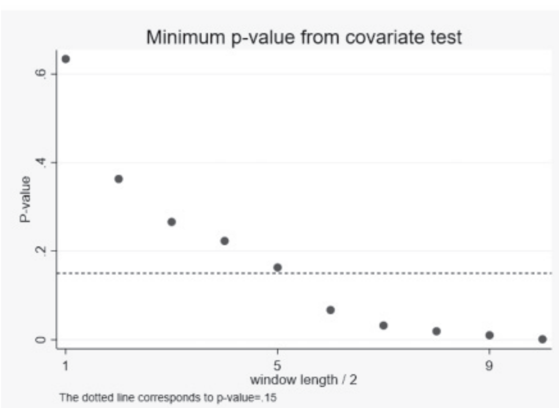
(b) Distribution of Index - Phase 2



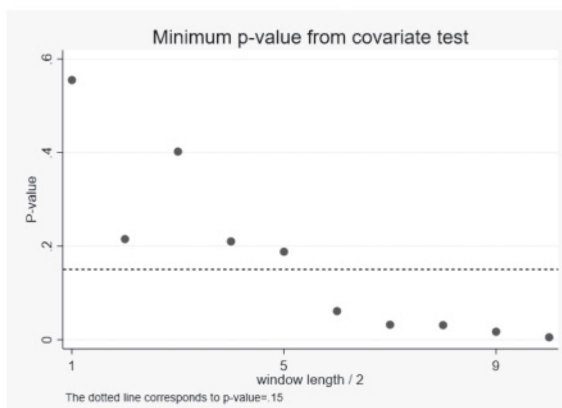
(c) Phase 1 Discontinuity



(d) Phase 2 Discontinuity



(e) Phase 1 Minimum P-Value Window Selection



(f) Phase 2 Minimum P-Value Window Selection

The first row plots distribution of district poverty index by state. The second row shows treatment discontinuities for each phase, dropping the phase far away from the cutoff (phase 3 in panel c, and phase 1 in panel d). Negative and zero normalized state rank numbers are districts predicted to NREGS based on the government algorithm. The third row plots the minimum *p*-value of any of the baseline variables in a balance test for different analysis windows around the cutoff. window length/2 is the symmetric interval around the cutoff, so 4 corresponds to the main analysis window of [−3,5].

for the outcome variables also hold at the higher level. The empirical analysis focuses on the UPA government coalition and its main party, the INC.

Table 1 shows some summary statistics for the primary variables of interest at the polling station level. For comparison, the table also reports the corresponding statistics for election results at the parliamentary constituency level. While there are minor differences between the results at both levels, the probability of a candidate from the Indian National Congress (INC) to win its parliamentary constituency

TABLE 1.—SUMMARY STATISTICS

	Polling Station Level	Parliamentary Constituency Level
INC win	0.3105	0.3681
UPA win	0.3938	0.4375
BJP win	0.2062	0.2014
INC vote share	27.90	27.58
UPA vote share	35.07	35.95
BJP vote share	19.70	18.83
Voter turnout		0.6023
Observations	586,903	432

Vote shares given in percent. INC (Indian National Congress), UPA (United Progressive Alliance), BJP (Bharatiya Janata Party). UPA is the name of the government coalition. For the government elected in 2004, the UPA consisted of the following parties: Indian National Congress, Rashtriya Janata Dal, Dravida Munnetra Kazhagam, Nationalist Congress Party, Pattali Makkal Katchi, Telangana Rashtra Samithi, Jharkhand Mukti Morcha, Marumalarchi Dravida Munnetra Kazhagam, Lok Jan Shakti Party, Indian Union Muslim League, Jammu and Kashmir Peoples Democratic Party, Republican Party of India, All India Majlis-e-Ittehadul Muslimen, and Kerala Congress.

or polling station is between 31% and 37%. The corresponding victory likelihood for the government coalition is about 40%, and for the main opposition party BJP, it is about 20%. On average, an INC candidate receives about 28% of the vote and a government coalition candidate about 35%. Voter turnout in the average parliamentary constituency is 60%. This information comes from 432 parliamentary constituencies and 586,903 polling stations across all phases.

To analyze the economic impacts of NREGS, I use household survey data from the National Sample Survey (NSS) from 2007–2008. This is the only employment and wage survey that was carried out after the introduction of NREGS and before the 2009 election. At this point, both phase 1 and phase 2 districts had access to the program, but phase 3 districts were still untreated. A representative sample of households in each district was interviewed in each season. I use this data set to create variables for NREGS employment, household expenditures, and migration outcomes.

As a measure of NREGS implementation quality, I create an indicator variable equal to 1 if a constituency belongs to a star state (Andhra Pradesh, Chhattisgarh, Madhya Pradesh, Rajasthan, and Tamil Nadu), and 0 otherwise (Dreze & Khera, 2009; Khera, 2011).

C. *Empirical Specification*

The algorithm creates state-specific district ranks that can be used as a running variable in a fuzzy regression-discontinuity design. The first stage is strong with an F-statistic of 220.80. Two main estimation techniques will be used: the local randomization approach and the more standard parametric estimation. The local randomization approach is a newer method in the RD literature (Cattaneo, Titiunik, & Vazquez-Bare, 2016, 2017). In contrast to the more traditional techniques, which assume that all variables other than the treatment are smooth and monotonic at the cutoff, local randomization assumes that given the appropriate choice of an estimation window close to the cutoff, observations on both sides can be treated as randomly assigned. Estimation then proceeds as in an experiment, and finite sample adjustments ensure that the method has power even for small samples in the vicinity of the cutoff. The estimation window is a window in which the hypothesis of balanced baseline variables cannot be rejected, which is similar to a balance table test in an experiment. Cattaneo et al. (2017) strongly suggest the use of this technique rather than nonparametric estimation techniques when the running variable is discrete.

The main results therefore use the local randomization approach with a two-stage least square estimation. Figures 1e and 1f show the minimum p-value of any variable used in the baseline test for different windows around the cutoff. The baseline variables include important socioeconomic characteristics (public, family, and private casual employment, landholding, log per capita expenditures, the log daily wage, and years of education), and the figures show that for both phase 1 and phase 2 cutoffs, the covariates are balanced for small windows around the cutoff. To be conservative, the horizontal dotted line, which represents a p-value of 0.15, is used as the minimum acceptable p-value in the balance test. Based on these results, the suggested window length is 5. The cutoff occurs at rank 1, so a suggested window length of 5 corresponds to a window of $[-4,6]$. To be more conservative since the p-value for window length 5 is only slightly above 0.15 for phase 1, the main analysis will use a window of $[-3,5]$. This corresponds to a window length/2 of 4 in the figures. Standard errors are obtained by bootstrapping with 1000 repetitions.[34] Tables 2 and 3 show baseline tables for the analysis window for a large number of economic outcomes and political variables, finding that phase 1 and phase 2 areas are balanced on all of these outcomes. This further supports the assumption that observations in the analysis window do not systematically differ from each other with the exception of length of NREGS exposure.

In addition to the local randomization approach, the main results are also reported using the intent-to-treat version of the standard parametric estimations with linear regression lines on both sides of the cutoff.[35] This leads to the following regression equation:

$$y_{ij} = \beta_0 + \beta_1 nregs_i + f(rank, nregs) + \epsilon_{ij},$$

y_{ij} is an election outcome variable in polling station i and district j, and the coefficient of interest is β_1. $f(.)$ is a function of predicted NREGS receipt *nregs*, and the district's rank based on the state-specific normalized index *rank*.

Figure 2 also shows the nonparametric relationships for the main outcome variable at both polling station and parliamentary constituency levels and plots linear polynomial regression curves.

[34]Cluster bootstrapping does not change the p-value of the results in the very large majority of cases, supporting the assumption of random assignment in the chosen window.

[35]Gelman and Imbens (2019) discourage the use of higher-order polynomials due to noisier estimates, large weights for observations far away from the cutoff, and misleading confidence intervals. The results are qualitatively robust to using other parametric specifications.

TABLE 2.—BASELINE BALANCE TEST (ECONOMIC VARIABLES)

	Men			Women		
Variable	Phase 1	Phase 2	Difference	Phase 1	Phase 2	Difference
Public empl.	0.001	0.002	0.001	0.000	0.001	0.001
	(0.002)	(0.007)	(0.001)	(0.002)	(0.006)	(0.001)
Private empl.	0.313	0.309	−0.004	0.157	0.163	0.006
	(0.147)	(0.137)	(0.024)	(0.126)	(0.124)	(0.021)
Family empl.	0.538	0.518	−0.020	0.329	0.297	−0.033
	(0.158)	(0.153)	(0.026)	(0.176)	(0.164)	(0.029)
Log priv. wage	3.953	3.982	0.029	3.605	3.607	0.002
	(0.363)	(0.387)	(0.062)	(0.355)	(0.356)	(0.060)
Log per-cap. exp.	6.244	6.270	0.026	6.237	6.266	0.029
	(0.333)	(0.272)	(0.051)	(0.319)	(0.278)	(0.050)
Land	1,024.685	1,002.614	−22.071	987.723	976.492	−11.231
	(604.820)	(575.832)	(98.765)	(588.499)	(577.871)	(97.362)
Education	3.697	3.839	0.142	2.521	2.672	0.151
	(0.782)	(0.730)	(0.127)	(0.953)	(0.938)	(0.158)
Cons exp.	2,759.125	2,867.213	108.088	2,695.430	2,845.345	149.916
	(922.252)	(867.437)	(149.847)	(904.911)	(895.566)	(150.219)
HH agric. labor	0.264	0.276	0.012	0.262	0.279	0.017
	(0.184)	(0.166)	(0.029)	(0.182)	(0.173)	(0.030)
HH agric. selfempl.	0.396	0.376	−0.020	0.401	0.372	−0.029
	(0.184)	(0.156)	(0.029)	(0.181)	(0.145)	(0.028)
Observations	64	82	146	64	82	146

Data: NSS (2004–2005). District-level employment and log private wage for last week. Log per-capita expenditures (last thirty days), land (acres), and years of education. Consumption expenditures (rupees), percent of households that are agricultural laborers, and self-employed. Difference columns test whether differences are statistically significant.

TABLE 3.—BASELINE BALANCE TEST (POLITICAL VARIABLES)

Variable	Phase 1 (1)	Phase 2 (2)	Difference (3)
INC won	0.304	0.274	−0.030
	(0.464)	(0.449)	(0.095)
UPA won	0.406	0.384	−0.022
	(0.495)	(0.490)	(0.099)
BJP won	0.203	0.260	0.057
	(0.405)	(0.442)	(0.077)
INC vote share	28.468	30.569	2.101
	(20.538)	(19.640)	(4.171)
UPA vote share	34.761	37.450	2.689
	(19.521)	(17.279)	(4.175)
BJP vote share	20.761	19.816	−0.945
	(19.049)	(20.452)	(3.431)
Candidate age	51.391	53.767	2.376
	(11.486)	(10.580)	(2.098)
Candidate name length	17.843	17.785	−0.058
	(7.192)	(8.420)	(1.424)
Candidate SC	0.174	0.151	−0.023
	(0.382)	(0.360)	(0.062)
Observations	70	79	149

Data restricted to main analysis window. Baseline variables from 2004 general election, reported at parliamentary constituency level: INC (main government party), UPA (government coalition), BJP (main opposition party). Won variables equal to 1 if a party received the most votes. Vote shares reported in percent. Candidate variables: politician age, length of name, and whether belongs to Scheduled Castes. Difference column tests whether differences are statistically significant.

IV. Results

A. Main Results

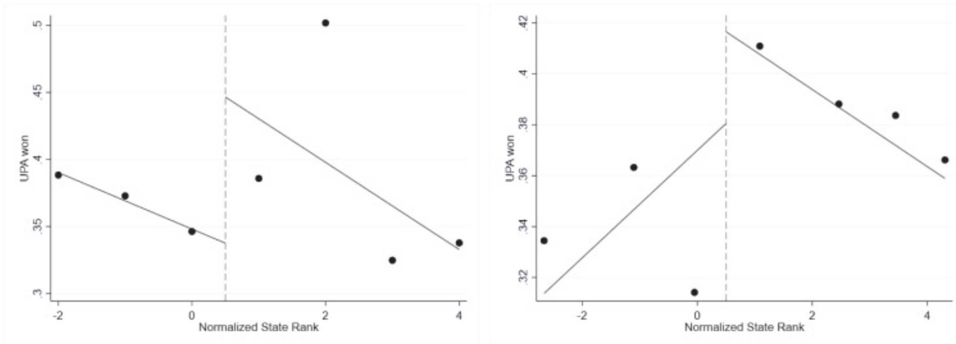
Table 4 and Figure 2 show the main results. Table 4 shows the results of the impact of longer NREGS exposure on the probability of the UPA government winning the most votes. Columns 1 and 2 report the results of longer program access using the local randomization approach, whereas columns 3 and 4 show the impacts when using the parametric approach. Since the election data come from polling stations, whereas the winner is determined at the parliamentary constituency level, the odd-numbered columns use the polling station data, whereas the even numbers collapse the data to the parliamentary constituency level.

Table 4 finds that longer NREGS exposure has a negative impact on electoral support for the government coalition. Polling stations in phase 1 districts are 19 percentage points less likely to register the largest number of votes for the UPA coalition government at the cutoff than phase 2 districts. This effect is highly statistically significant. When collapsed to the parliamentary constituency level, the probability of the government winning the constituency decreases by about 16 percentage points. The results are a bit smaller, but otherwise similar, when using the parametric estimation approach instead. The result is therefore robust across a number of different empirical specifications.

Figure 2 shows these effects graphically. Figure 2a uses the analysis window in the vicinity of the cutoff at the polling station level, whereas Figure 2b plots the empirical patterns at the parliamentary constituency level. The figures use the approach in Calonico, Cattaneo, and Titiunik (2015) to optimally undersmooth observations as well as linear parametric regression lines. Similar to table 4, the figures show a substantial drop in electoral success for NREGS districts on the left side of the cutoff, which corresponds to longer NREGS exposure.

Taken together, table 4 and figure 2 therefore show that the medium-run effects of NREGS access differ from a shorter program exposure. These results rule out hypotheses that longer access to the antipoverty program increases voter support or has no effect. Both an increase in awareness over time and the willingness of voters to reciprocate for the cumulative

FIGURE 2.—DISCONTINUITIES FOR UPA GOVERNMENT (PHASE 1)



(a) UPA Won Phase 1 Polling Station (b) UPA Won Phase 1 Constituency

The graphs use the optimal quantile-spaced binning procedure suggested by Calonico et al. (2015). Polynomials are fitted through the complete underlying data set and not just the bins.

TABLE 4.—IMPACT OF NREGS ON 2009 GOVERNMENT ELECTION RESULTS

Specification	UPA won	UPA won	UPA won	UPA won
Phase 1	-0.1900^{***}	-0.1641^{***}	-0.1115^{**}	-0.1231^{**}
	(0.0167)	(0.0066)	(0.0490)	(0.0534)
N	209,971	147	586,903	412
Data level	Polling station	Constituency	Polling station	Constituency
Method	Local rand.	Local rand.	Parametric	Parametric

$^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. Phase 1 refers to the early implementation phase with longer NREGS exposure as compared to phase 2. The won variables are indicator variables equal to 1 if the UPA coalition government received the most votes at a polling station or parliamentary constituency, and 0 otherwise. Data level indicates whether the estimates use the polling station information directly or election data aggregated to the constituency level. Results in columns 1 and 2 use the regression-discontinuity local randomization approach with an analysis window of [−3,5] around the cutoff. Standard errors are obtained by bootstrapping with 1,000 replications. Parametric specifications in columns 3 and 4 estimate linear regression lines on either side of the cutoff without restricting the analysis window. Standard errors are clustered at the district level.

benefits of NREGS by voting for the government coalition predict a positive effect similar to the existing literature. Similarly, an electorate that is disillusioned with their ability to hold politicians accountable or that considered NREGS to be far less important than other election topics would have led to a zero effect.

The two main explanations that are consistent with the results are that longer program access reduces the salience of NREGS in the election decision and that voters became more disappointed with the mismatch between promised and actual program benefits over time. Households in phase 1 districts had an additional year to get used to the program than phase 2 households. If access to the program was considered to be normal for these households by 2009, it was a less important topic in the election. Phase 2 households, on the other hand, had only experienced the program for one full agricultural off-season when the program is particularly attractive and may therefore have been more excited about the scheme.

Alternatively, the low implementation quality and implementation delays in many areas may have led to a disappointment among voters. Phase 1 district households may have formed a clearer opinion on the likely long-term benefits of NREGS once government officials have gained experience with implementing the program. Phase 2 households, on the other hand, had more limited time to form an opinion about the likely effectiveness of the program, making them less sure whether the observed implementation challenges would be a long-term problem with the program.

B. *Economic Impacts and Heterogeneous Effects*

To get a better sense of the plausibility of the two potential mechanisms, table 5 compares the economic impacts in phase 1 and phase 2 districts. Unfortunately, there are no available household survey data that were collected shortly before the 2009 election. A large representative employment survey was carried out between July 2007 and June 2008, however. In that time period, both phase 1 and phase 2 districts had access to the scheme. Phase 1 households experienced their second full agricultural off-season, whereas phase 2 households had access to NREGS for their first agricultural off-season. It can therefore be tested whether the economic benefits of NREGS were different in phase 1 than in phase 2 districts.

Panel A of table 5 reports the impact of longer NREGS access on average household outcomes of interest at the district level, keeping the same local randomization window as the main results. Public employment measures the likelihood that the average household in a district worked under NREGS in the past week. The migration variables focus on the likelihood of a household member having migrated in the previous year, migrated temporarily, and migrated for work. Both remittances and household expenditures refer to the average value of money received and spent in rupees in the last thirty days.

As table 5A shows, a typical household in a phase 1 area is about 3 percentage points more likely to have worked under NREGS in the past week, suggesting that the program is more widely accessible in early implementation districts.

Table 5.—Impact of NREGS on Economic Outcomes

Specification	Public employment	Migrated last year	Temporary migration	Migrated for work	Remittances	Household expenditures
A: Phase 1 Overall						
Phase 1	0.0270***	0.0173***	0.1265***	0.0191***	328.20***	−541.37***
	(0.0007)	(0.0009)	(0.0119)	(0.0008)	(63.54)	(21.07)
N	146	146	146	146	146	146
B: Phase 1 Implementation Quality						
Phase 1 star	0.3067***	−0.0224***	−0.5649***	−0.0003	1030.82***	−787.11
	(0.0349)	(0.0011)	(0.1487)	(0.0019)	(58.82)	(525.52)
N	44	44	44	44	44	44
Phase 1 nonstar	−0.0029***	0.0219***	0.1732***	0.0215***	306.39***	−458.34***
	(0.0001)	(0.0002)	(0.0101)	(0.0009)	(29.05)	(26.07)
N	102	102	102	102	102	102

***$p < 0.01$, **$p < 0.05$, and *$p < 0.1$. Data: NSS (2007–2008). Phase 1 refers to early NREGS implementation. Star states are Andhra Pradesh, Chhattisgarh, Madhya Pradesh, Rajasthan, and Tamil Nadu. First four variables are district averages for NREGS employment and migration outcomes. Remittances are average district value of remittances in the last thirty days in rupees; household expenditures are average district value of expenditures in the last thirty days. Results use RD local randomization approach with analysis window [−3,5]. Standard errors obtained by bootstrapping with 1000 replications.

At the same time, the likelihood of having migrated recently overall or for work is about 2 percentage points higher in phase 1 districts, and temporary migration increases by about 13 percentage points at the cutoff. The higher migration rates are leading to an increase in remittances that the average household receives. Household expenditures are about 500 rupees lower in phase 1 than in similar phase 2 areas. Overall, these results paint a mixed picture of the effectiveness of NREGS. They suggest that awareness of NREGS is higher in the early implementation areas and that more households can form an opinion on the program's benefits based on their own experience. But while higher participation rates suggest that NREGS should work better in phase 1 areas, the increase in migration rates does not fit this story.

These overall impacts could mask heterogeneous treatment effects since NREGS was better implemented in the so-called star states than in other states. Panel B of table 5 therefore splits the sample up by this measure of implementation quality. In star states, phase 1 households are about 31 percentage points more likely to have worked under NREGS in the past week than phase 2 households. In nonstar states, the difference is economically insignificant at −0.3 percentage points. Households in phase 1 areas in nonstar states therefore do not benefit from increased access to NREGS employment with longer program exposure.

A similar discrepancy arises for the migration outcome variables. In star states, migration for work is unaffected by longer program access, whereas the likelihood of having migrated in the past year and of temporary migration decreases substantially. The impact of household expenditures is negative but imprecisely estimated, whereas there is an increase in remittances that is somewhat larger than the coefficient on household expenditures. This suggests that households are overall better off and have less reason to have a household member migrate since access to jobs through NREGS is now more available locally, unless the migration opportunity is lucrative and leads to high remittances. In nonstar states, however, the probability of having migrated increases, and the increase in remittances is much smaller than in star states. These impacts are consistent with a disappointment of the actual NREGS benefits among households with longer program exposure, which leads them to prefer migration to relying on NREGS as an employment opportunity.[36]

Table 6.—Impact on 2009 Government Election Results by Implementation Quality

Specification	UPA won	UPA won
Phase 1	−0.1978***	0.1947*
	(0.0107)	(0.1107)
N	148,524	61,447
Implementation quality	Nonstar	Star

***$p < 0.01$, **$p < 0.05$, and *$p < 0.1$. Phase 1 refers to the early implementation phase with longer NREGS exposure as compared to phase 2. Star states are Andhra Pradesh, Chhattisgarh, Madhya Pradesh, Rajasthan, and Tamil Nadu, which are more successful at implementing NREGS than the other states (Dreze & Khera, 2009; Khera, 2011). The won variables are indicator variables equal to 1 if the UPA coalition government received the most votes at a polling station and 0 otherwise. Results use the regression-discontinuity local randomization approach with an analysis window of [−3,5] around the cutoff. Standard errors are obtained by bootstrapping with 1000 replications.

While table 5B is not causal and could be driven by other differences between star and nonstar states that are separate from NREGS implementation quality,[37] the empirical patterns are consistent with NREGS working more successfully in star states than in nonstar states, which allows households with longer program access to make more informed decisions about optimal employment and migration behaviors. Analyzing the impact of longer NREGS exposure on electoral support for the government coalition separately for star states and nonstar states is therefore a test of whether the overall negative impact of longer program exposure on government votes is plausibly driven by implementation quality challenges rather than a loss in program salience.

Table 6 shows that the overall decline in electoral support in areas with longer NREGS exposure is driven by the nonstar

[36]These migration effects are consistent with extensive qualitative evidence (Jenkins & Manor, 2017).

[37]See the online appendix for more details on this point.

Table 7.—Further Impacts of 2009 Government Election Results

	A: INC Election Result		
Specification	INC won overall	INC won nonstar	INC won star
Phase 1	-0.2336^{***}	-0.2331^{***}	0.0710^{***}
	(0.0086)	(0.0128)	(0.0118)
N	209,971	148,524	61,447
		B: UPA-Governed States	
Specification		UPA won non-UPA	UPA won UPA
Phase 1		-0.1790^{***}	-0.2706^{***}
		(0.0217)	(0.0194)
N		138,357	71,614

$^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. Phase 1 refers to the early implementation phase with longer NREGS exposure as compared to phase 2. Star states are Andhra Pradesh, Chhattisgarh, Madhya Pradesh, Rajasthan, and Tamil Nadu, which are more successful at implementing NREGS than the other states (Dreze & Khera, 2009; Khera, 2011). The won variables are indicator variables equal to 1 if the UPA coalition government or the INC received the most votes at a polling station, and 0 otherwise. The INC is the main party in the government coalition. In panel B, non-UPA and UPA split polling station observations by whether they are in a state that has a UPA state government. The UPA-governed states include Andhra Pradesh, Assam, Haryana, Maharashtra, Rajasthan, and Tamil Nadu. Results use the regression-discontinuity local randomization approach with an analysis window of [−3,5] around the cutoff. Standard errors are obtained by bootstrapping with 1,000 replications.

states: The probability of winning the most votes decreases for the government by 20 percentage points in nonstar states but increases by about 19 percentage points in star states.

C. Discussion

The results in table 6 are consistent with voters holding the government accountable for the working of NREGS, while they are less consistent with a loss of salience story.[38] Voters reward the government for a good performance of NREGS in star states, whereas they express their disappointment with NREGS in nonstar states.

Whether such an explanation is plausible in this context can be further tested. If voters hold the government accountable for the working of NREGS, two additional implications are that the biggest government party should be especially affected and that it should be easier to hold the government accountable for the program's implementation quality in areas where the state government consists of UPA parties as well. The biggest party of the government coalition, the INC, promised a scheme like NREGS in its 2004 election campaign manifesto and focused on NREGS as one of the party's main successes in the 2009 election campaign. Similar to the results for the whole coalition government, panel A of table 7 shows that the overall impact of longer NREGS access on the INC's probability of winning the most votes is negative, with a decrease of about 23 percentage points. This impact is driven by the low-implementation quality areas, whereas the votes for the INC increase in star states.

Table 7B splits the sample into areas governed by parties of the government at the state level and those that are governed by the opposition. Voters wanting to hold the government accountable for the working of a program like NREGS face the issue that in a federal state like India, much of the responsibility for a successful implementation of the program lies with lower tiers of government. Voters may therefore be unsure who to blame or reward for the actually realized program benefits. This problem may be smaller in areas where state and central government are composed of parties from the same alliance. As table 7 shows, the percentage point drop in government support is larger in UPA-governed states than in states governed by the opposition.

Overall, the empirical results suggest that voters care enough about the implementation of NREGS to hold the government accountable for the realized benefits. Indian voters appear to believe that their vote will affect government policy and realize how the federal structure interacts with the implementation of NREGS in practice. An early implementation of the program paid off in well-implemented areas. In other areas, an early program implementation allowed voters to learn about the low actual program benefits and to decrease their electoral support.

The plausibility of this interpretation of the results relies on a few assumptions. Voters should predominantly vote for political parties rather than candidates, since the UPA government at the center is held responsible. They should believe in the power of their vote to change government policy and should regard NREGS as an important issue in the election. The online appendix provides a wide range of quantitative and qualitative evidence that they hold up in practice. It also shows circumstantial evidence from newspaper articles and web searches that interest in NREGS was at its highest level since the NREGS start in 2006 during the election months, making a loss of salience story less plausible.

D. Robustness Checks and Extensions

Table 4 already showed that the main results of the paper are robust to the level of data aggregation and the chosen RD estimation technique. The online appendix finds that they are also robust to a change in the chosen analysis window around the cutoff in the local randomization approach and to using the margin of victory instead of the probability of winning as a dependent variable.[39] It also provides additional details on some of the underlying assumptions of an implementation quality explanation, including the plausibility that voters initially had high expectations of NREGS that were adjusted down over time, and the question whether enough voters were likely to be aware of NREGS.

Tables 8 and 9 extend the analysis in two directions. Table 8 shows the main results of length of exposure on government support at the cutoff between phase 2 and phase 3. Two factors make the phase 2 implementation cutoff different from the

[38]The loss in salience could be lower in star than nonstar states if it takes longer to get used to a working program, but one would not expect the impact to be large and positive.

[39]It also addresses additional potential concerns with the results, such as further baseline tests and a more detailed discussion of the NREGS algorithm.

Table 8.—Impact on 2009 Government Election Results (Phase 2)

Specification	UPA won overall	UPA won overall	UPA won nonstar	UPA won star
Phase 2	-0.2155^{***}	-0.2376^{***}	-0.3756^{***}	0.0950^{***}
	(0.0233)	(0.0161)	(0.0036)	(0.0095)
N	137,855	99	106,104	31,751
Data level	polling station	constituency	polling station	polling station

$^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. Phase 2 refers to the early implementation phase with longer NREGS exposure as compared to phase 3. Star states are Andhra Pradesh, Chhattisgarh, Madhya Pradesh, Rajasthan, and Tamil Nadu, which are more successful at implementing NREGS than the other states (Dreze & Khera, 2009; Khera, 2011). The won variables are indicator variables equal to 1 if the UPA coalition government received the most votes at a polling station or parliamentary constituency and 0 otherwise. Results use the regression-discontinuity local randomization approach with an analysis window of [−3,5] around the cutoff. Standard errors are obtained by bootstrapping with 1,000 replications.

Table 9.—Impact of NREGS on Variance of Votes for the Government

	A: Variance of Votes			
	Phase 1		Phase 2	
Specification	UPA won	INC won	UPA won	INC won
Earlier Exposure	-0.2547^{***}	-0.2148^{***}	-0.2152^{***}	-0.1563^{***}
	(0.0058)	(0.0051)	(0.0044)	(0.0030)
N	147	147	99	99
	B: Within Parliamentary Constituency			
	Phase 1		Phase 2	
Specification	UPA won	INC won	UPA won	INC won
Earlier Exposure	-0.0020	-0.0159^{***}	-0.0320^{***}	-0.0354^{***}
	(0.0035)	(0.0034)	(0.0036)	(0.0034)
N	82,212	82,212	77,197	77,197

$^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$. Phase 1 refers to the early implementation phase with longer NREGS exposure as compared to phase 2, phase 2 indicates the cutoff between phase 2 and phase 3. The won variables are indicator variables equal to 1 if the UPA coalition government or the INC received the most votes at a polling station, and 0 otherwise. The INC is the main party in the government coalition. Results use the regression-discontinuity local randomization approach at the district level with an analysis window of [−3,5] in panel A. Simple regression analysis is used in panel B since neighboring districts in the same parliamentary constituency do not have to be close to the implementation cutoffs. Standard errors are obtained by bootstrapping with 1,000 replications. The outcome variable in panel A is the standard variation of the UPA won variable on either side of the cutoff, with the standard deviation calculated at the district level.

phase 1 cutoff. First, the nonrandom rollout of NREGS with priority to the most economically disadvantaged districts ensures that districts at the Phase 2 implementation cutoff are less disadvantaged than those at the first implementation cutoff. If voters in these districts have different preferences from those in poorer districts, we may therefore find different results from phase 1. This makes phase 1 and phase 2 cutoff results not directly comparable. On the other hand, the phase 2 cutoff can still be used to analyze whether voters vote differently in areas with longer NREGS exposure than similar areas with shorter program access, and doing so strengthens the external validity of the previous empirical analysis.

Second, at the phase 2 cutoff, areas with two years of official NREGS exposure are compared to similar districts with one year of NREGS access at the time of the 2009 election. While phase 2 districts had access to NREGS for one full agricultural off-season, this is not the case for phase 3 districts. Voters in phase 3 districts may therefore have very limited experience with NREGS and its benefits, and unfortunately there are no household survey data that would allow a comparison of the household employment and migration situation around the time of the election. This makes it more difficult to disentangle potential mechanisms.

Given these caveats, table 8 presents the results for the second implementation cutoff. The results are similar to table 4. The impact of longer access to NREGS on votes for the UPA coalition government is negative, with a decline of the winning probability of about 22 percentage points. Similar to phase 1, this negative impact is driven by the low-implementation quality areas, whereas there is an increase in votes in the star states.

If voters have more access to information about the actual NREGS benefits over time and vote accordingly, one may assume that that also affects the variance of electoral support at the cutoffs. Table 9A tests whether the variance of election outcomes decreases with longer NREGS exposure. It shows that this holds at both cutoffs, supporting the idea that voter preferences become more similar over time, consistent with having received more signals about the actual implementation quality of NREGS.

Panel B of table 9 looks at within-parliamentary variation of votes. Since NREGS was rolled out at the district level whereas voters vote based on electoral constituency boundaries, there are parliamentary constituencies that include voters from more than one district and from different NREGS implementation phases. Within a parliamentary constituency, the political parties, candidates standing for election and election campaign strategies are the same. Table 9B shows that once these factors are controlled for, votes for the INC and UPA are overall negative for both phases in the areas with longer NREGS access, although the effect for the UPA government is small and statistically insignificant for phase 1. This supports the hypothesis that voters vote based on NREGS exposure rather than due to differences in the local candidates or election campaign strategies.

Overall, the results in this section suggest that the impact of longer NREGS exposure on electoral support for the government is negative and robust to different empirical specifications and that a plausible explanation consistent with a variety of additional tests is that voters hold the government accountable for the implementation quality of the program.

V. Conclusion

This paper analyzed the impact of the Indian National Rural Employment Guarantee Scheme (NREGS) on the government parties' election performance in the next general election. It exploits the rollout of the program in a fuzzy RD design with state-specific treatment discontinuities. The results show that electoral support for the incumbent declines substantially with longer exposure to NREGS. This impact is robust across empirical specifications.

This negative impact is not consistent with a variety of potential explanations, including a disillusioned electorate that does not believe that a new antipoverty program would work or rising awareness of the program over time. One explanation consistent with the main results, as well as with a number of heterogeneous treatment effects, is that voters hold the government accountable for the implementation quality of the program. The decrease in electoral support is driven by low-implementation quality areas, whereas there is an increase in support for the government in well-implemented areas. While not all other explanations of these empirical patterns can be ruled out, a wide range of qualitative, quantitative, and circumstantial evidence is consistent with this mechanism.

These results show that there are impacts from longer program exposure even in the medium run, which has been understudied in the literature. They suggest that the anticipated implementation quality is a key factor for the optimal timing of government policies. If implementation quality is expected to be low, from the government's viewpoint a political initiative is best implemented shortly before an election, which is consistent with evidence from around the world that spending in many countries increases in the election year. In well-implemented areas, the timing is much less important, and longer program exposure may actually improve the government's election performance. Governments in developing countries may therefore be losing out on substantial election benefits from good governance if they mainly focus on short-term reelection strategies.

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