Impact evaluation of national rural employment guarantee scheme in India

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Note to readers

This final impact evaluation grantee report has been submitted in partial fulfilment of the requirements of grant OW4.1223 awarded under Open Window 4. 3ie is making it available to the public in this final report version as it was received. The report does not follow the 3ie report format. It comprises several articles rather than one comprehensive report as we require for publication in the 3ie impact evaluation report series. The report has been reviewed and is technically sound. No further work has been done.

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Abbreviations and acronyms

AC	Assembly Constituency
AP	Andhra Pradesh
BDO	Block Development Officer
BJP	Barathiya Janata Party
BPL	Below Poverty Line
CBB	Commercial bank branch
DDD	Triple difference
DID	Difference in difference
FA	Field assistant
GDD	Growing degree days
GP	Gram Panchayat
IAP	Integrated Action Plan
INC	Indian National Congress
IV	Instrumental variable
MGNREGA	Mahatma Gandhi National Rural Employment Guarantee Act
MGNREGS	Mahatma Gandhi National Rural Employment Guarantee Scheme
MIS	Management Information System
MLA	Member of the Legislative Assembly
MLA	Members of the Legislative Assembly
MPCE	Monthly per capita expenditure
MPDO	Mandal Parishad Development Officer
NBA	Nirmal Bharat Abhiyan
NSS	National Sample Survey
OLS	Ordinary least squares
PEEP	Public Evaluation of Entitlement Programmes
PSM	Propensity score matching
RD	Rural development
SC	Scheduled castes
SC/ST	Scheduled castes and tribes
SEGC	State Employment Guarantee Council
ST	Scheduled tribes
TDP	Telugu Desam Party
UPA	United Progressive Alliance
YSR	Y.S. Rajashekara Reddy

Executive summary

In this project, we exploit different data sets to investigate the targeting, implementation, and impact of India's Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS), the largest public works project in the world. MGNREGS began in 2006, following the 2005 Act that provided each rural household with a legal right to be employed up to 100 days per year at a state-level minimum wage. Since its inception, MGNREGS has generated more than 18 billion person days of work, at a cost of US$ 44.6 billion.

We present in this report seven independent and related studies. Two studies use the national representative National Sample Survey data to provide a complete picture of MGNREGS implementation and targeting. The other five studies use a three-round household survey data and administrative data in Andhra Pradesh, one of the few states praised for its implementation quality, to examine the impacts of MGNREGS on welfare and household labor supply and to provide empirical evidence on the extent to which project spending follows patterns of politically-influenced non-programmatic distribution. We next provide a brief description to each of the studies.

The first study, titled "The 'discouraged worker effect' in public works programs: evidence from the MGNREGA in India" addresses the decline both in total MGNREGS expenditure as well as in the person days employed since its peak in 2009. Using nationally representative data from two rounds of India's National Sample Survey the 66th Round (2009-10) and the 68th Round (2011-12), we find evidence consistent with a "discouraged worker effect". Changes in demand for MGNREGA employment are negatively and significantly associated with the uncertainty of obtaining MGNREGA work in the district, as represented by rationing rates at the district level. Declining demand for MGNREGS can be readily (mis)interpreted as an indicator of program success in that MGNREGS has reduced poverty and people no longer need an employment guarantee, or growing program irrelevance due to growth in alternate employment opportunities. Therefore, these findings are critically important to nuanced and accurate interpretation of the observed decline in MGNREGA participation. Program decline may be largely a result of local implementation failures that discourage workers despite the continuing need for the employment guarantee as a safety net.

The second study, titled "Women participation and rationing in the employment guarantee scheme", looks at the extent to which MGNREGS includes women, with a particular focus on vulnerable sub-populations of women such as widows and mothers of young children who typically face serious constraints in the context of labor market participation. Using data from the National Sample Survey, the study finds that while the MGNREGS has indeed been inclusive of women, substantial variations across states and exclusion of vulnerable groups of women demand attention. This paper is published in *Economic and Political Weekly*.

In the third study, titled "Welfare and poverty impacts of India's National Rural Employment Guarantee Scheme: Evidence from Andhra Pradesh", we explore short- and medium-term poverty and welfare effects on participating households. The findings suggest that MGNREGS participants significantly increased consumption (protein and energy intake) in the short run and accumulated more nonfinancial assets in the medium term. The benefits are most pronounced for the poor, scheduled castes and tribes and households supplying casual labor. We also find that increased income from casual labor was the primary channel through which the effects were realized. MGNREGS participation has no effect on private investment in land improvement.

The fourth study, titled "General equilibrium impacts of National Rural Employment Guarantee Scheme on welfare and poverty reduction", is closely related to the third one. It uses the same data as in the third study but focus on the general equilibrium (or spillover) effects of MGNREGS on welfare and poverty. The results suggest that MGNREGS has positive effects on consumption expenditure and nutritional intake for households in program areas and for non-participating households likewise. The welfare effects are more pronounced for poor households.

The fifth study is titled "Disaggregated labor supply implications of guaranteed employment in India". It explores the household level labor allocation effects – disaggregated by gender, age group, task, and season – associated with MGNREGS in Andhra Pradesh. Our results suggest that participation in the MGNREGS prompted an increase in overall household labor supply by about 13 days only in the summer slack labor season, mostly attributed to adult women. This expansion, though, is not large enough to evade "crowding out" of some labor previously offered to non-MGNREGS labor tasks, particularly private casual labor opportunities, and more so in the main agricultural seasons than the summer slack season. Despite overall labor displacement, women are found to increase their time spent on farm in the rabi agricultural season while men in a small number of surveyed households spend more time on migration labor activities in all three seasons studied. Time spent on paid and unpaid activities, including household chores, do not increase for youth and children in MGNREGS-participating households, suggesting no within-household substitution of labor towards younger members.

The sixth study is titled "Preferential resource spending under an employment guarantee: The political economy of MGNREGS in Andhra Pradesh". We investigate whether the spending of MGNREGS at the mandal level is susceptible to political influence, using the administrative data on MGNREGS expenditures from 2006/2007 to 2012/2013 fiscal year. Our results offer optimism about the MGNREGS bureaucracy as well as some critiques to guide reforms. We find no evidence of partisan-based spending before the 2009 election and find that the political leaning of a mandal played only a small part in fund distribution after the 2009 election. Most variation in public works expenditures is explained by the observed needs of potential beneficiaries, as the scheme intended. So the distortionary effect of politically-driven resource allocation is very modest, likely on account of the distinct demand-driven characteristics of the scheme and the local political context at the time. This paper is published in the *World Bank Economic Review*.

The seventh study, titled "Party based clientelism and household responses: A case of public employment guarantee scheme in India", is closely related to the sixth paper. Instead of looking at political influence on project spending at the mandal level, in this paper we look at the role of political influence on MGNREGS participation at the household level in Andhra Pradesh. In a setting of partisan politics where ruling politicians make preferential transfers to bolster political support, we study citizen political activism as a determinant of clientelistic transfers in MGNREGS. We find robust evidence of political clientelism targeting rival party supporters. Furthermore, we find that politicians target voters active in political campaigns, and specifically those who are active in areas where citizen involvement in political process is less common. We argue that the evidence sheds new light on vote buying both as a means to mobilize support form swing voters, and as a means to influence the behavior of political activists.

To summarize, our studies have six main findings.

1. The declining demand for MGNREGS in recent years is due to local implementation failures that discourage workers and not due to the program becoming irrelevant.
2. MGNREGS has been inclusive of women. However, the substantial variations both across states and the exclusion of vulnerable groups of women demands continued attention.
3. In Andhra Pradesh, MGNREGS significantly improves the welfare of participating households, especially the poor, scheduled castes and tribes, and the casual laborers.

4. In Andhra Pradesh, MGNREGS has significantly positive spillover effects in that nonparticipating households also benefit from higher consumption expenditure and nutritional intake. The spillover effects are more pronounced for the poor.
5. MGNREGS participation increases the total household labor supply with considerable “crowding out” effects which are more pronounced in the two main agricultural seasons than in the summer season and more for females than males.
6. MGNREGS fund allocation at the mandal level is largely based on the needs of potential beneficiaries as intended, although we do find some evidence that political affiliation and activism plays a role at the local (gram panchayat) level.

Overall, our findings support MGNREGS’ achievement as an effective social safety net program while also highlighting areas that need improvement.

1. The "discouraged worker effect" in public works programs: Evidence from the MGNREGA in India[1]

Sudha Narayanan, Upasak Das, Yanyan Liu, and Christopher B. Barrett

1.1 Introduction

Workfare programs in developing economies have long been recognized for their role in providing social security to vulnerable populations (Subbarrao, 2003; von Braun 1998). Many of these programs are self-targeting in nature, on account of the nature of work involved and also because wage rates are typically set at lower than market wages. The demand driven nature of these programs allows those who need it most to select themselves in, while those who have access to better opportunities select themselves out, thereby avoiding problems associated with targeting (Basu, 1991; Besley and Coate, 1992; Braun, 1998; Ravallion, 2003). Some of these workfare programs have been designed as entitlement programs, with employment on public works guaranteed on demand.

There is substantial literature on whether self-targeting really works. Specifically, these address whether participants of the program are from among the poor or whether the elite capture program benefits instead – either due to the exercise of socio-political power or due to multiple market failures that cause poorer, rather than better-off, individuals to self-select out (Braun, 1998; Barrett and Clay, 2003)[2]. There is also research on whether these programs (perhaps inadvertently) exclude potential beneficiaries who seek assistance, a phenomenon known as administrative rationing (Dutta, et al., 2012; Liu and Barrett, 2013). While these sorts of implementation failures have been well documented, the ultimate consequences for potential beneficiaries' behaviour remain relatively under-researched. For example, does poor implementation undermine access to the planned safety nets in ways that can affect expressed demand for public employment, leading to underutilization of the program? This paper examines the Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA) in India to see if implementation failures discourage potential beneficiaries from seeking work.

Labor market research on developed countries notes a "discouraged worker effect", that workers are less likely to seek work in downturns of the business cycle (that hold lower probability of getting a job) since the benefit-cost calculus of doing so would lead them to be worse off than if they were to remain unemployed or do unpaid work at home (See Benati, 2011, for a review of literature). We apply this idea to the context of a public works program, the MGNREGA in India.

There has been little systematic research of discouraged worker effects in the context of public works programs in developing countries although this phenomenon might be widespread. Much of the existing literature comes from India. For example, Khera (2008) documents such a phenomenon for drought relief works in the Indian state of Rajasthan. Recent evidence on the MGNREGA itself suggests that the uncertainty of securing work discourages workers from actively demanding work, who choose instead to wait passively and take up work if and when it is supplied (Drèze and Khera, 2014 in ten states in India; Himanshu, et al., 2015, in Rajasthan). These studies document the

[1] We are grateful to the participants at the seminar "The MGNREGA in India: Taking Stock, Looking Ahead" conducted in Mumbai, March 26-28, 2014. Sourabh Ghosh, Christopher Marciniak and Maribel Elias assisted with securing some of the data used in this paper.

[2] The broader questions of elite capture in development programs are discussed in Bardhan and Mookherjee (2000) and Platteau (2004).

possible presence of a discouraged worker effect but do not explicitly test for it. In this paper, we empirically test this hypothesis using nationally representative data.

We hypothesize that implementation failures in the MGNREGA might manifest in either or both of two forms, first as administrative rationing of work – i.e., denying employment to those who apply – and second, as delays in wage payments. Given that work under the MGNREGA is a demand-driven, legal entitlement, these implementation problems potentially affect worker demand for employment under the program. This is especially relevant in a political context where the future of the MGNREGA itself has been uncertain and its relevance has been questioned.[3]

We use nationally representative household data from two rounds of India's National Sample Survey, the 66th Round (2009-10) and the 68th Round (2011-12), combined with relevant district level data from various other sources to test for a discouraged worker effect both at the household and district levels. We find evidence consistent with a discouraged worker effect – a 10 percent increase in a district's administrative rationing rate decreases the probability that a household seeks work by 3.4-3.5%. For poor households, the discouragement effect of administrative rationing appears somewhat stronger, 3.8-3.9%. These results hold in the analysis at the district level as well– changes in district-level demand for MGNREGA employment are negatively and significantly associated with the uncertainty of obtaining MGNREGA work in the district, represented by rationing rates at the district level. The district level demand rate decreases by 8.9-9.2% in response to a 10% increase in rationing rate.

By contrast, we find no consistently robust evidence that delays in wage payments influence household-level demand for MGNREGS work. Payments delays appear to influence an individual household's probability of seeking work or district level demand rates only in some specifications. We examine reasons for this results later in the paper, but note here that this result is consistent with the widespread finding of wage inelastic labor supply (Blundell and MaCurdy 1999; Skoufias, 2004) since payment delays effectively reduce the present value of earnings. Wage delays however matter significantly when there are negative rainfall shocks.

Given that administrative rationing is a consistently significant source of discouragement, we then examine the correlates of administrative rationing and find that rationing is associated strongly with indicators of implementation ability. Removing the time invariant differences across districts using panel data that presumably strips out states' differential capacity to implement the program, politics appears to play only a limited role. The identity of the political party in power seems to matter more for pro-poor rationing, though these results are not robust. The most consistent correlate of administrative rationing appears to be negative rainfall shocks, indicating that perhaps administrative capacity is stressed and undermined with surges in demand in response to deficit rainfall.

While this study focuses on one program in India, it aims to make a broader contribution to understanding specific aspects of the lifecycle of workfare guarantees and the trajectories of welfare programs in general. Do programs decline because they outlive their usefulness or do they contain ingredients (that may or may not be manipulable) of their own demise?

The paper is organized as follows. Section 1.2 describes the MGNREGA in India and discusses the motivating issues in detail. Section 1.3 describes the data and model. Section 1.4 discusses administrative rationing and its correlates. Section 1.5 concludes.

[3] The MGNREGA was implemented in 2005 by the United Progressive Alliance (UPA). Since the Bharatiya Janata Party (BJP) won the general elections in India in 2014, there has been a debate on whether or not the MGNREGA should continue.

1.2 The MGNREGA, then and now

The MGNREGA is arguably the largest public workfare program in the world and has generated more than 18 billion person days of work, involving expenditures of US$ 44.6 billion since its inception in 2006.[4] The MGNREGA has been at an interesting juncture. When the Act was passed on September 5, 2005, its stated goal was to improve livelihood security for rural households by providing up to one hundred days of guaranteed wage employment in every financial year to every household whose adult members volunteer to do unskilled manual work (Government of India, 2013). Permissible works, typically provided within the village, include water conservation and harvesting, land development, horticulture and plantations, and rural connectivity, to name a few. Workers are paid piece rate according to a schedule of rates established by state governments for different tasks performed in different soil conditions. The program had a phased-in rollout starting with the 200 districts deemed the poorest and from there expanding to cover all of India's districts over the three year period 2006-08.

Administrative data suggest that the MGNREGA peaked in 2009 and has since declined both in the total expenditure as well as in the person-days employed (Figure 1.1). The reasons for MGNREGA's decline have been a focus of debate. One proposed explanation is that the "MGNREGA has done its job" and is perhaps no longer needed.[5] This view stems from the hypothesis that declines in demand reflect growth in attractive alternate opportunities for workers, who therefore self-select out of MGNREGA work more than they did previously. A second explanation is that the program is now better targeted.[6] It is hypothesized that in the early years of the program a lot of rural workers obtained a job card to be able to work under the MGNREGA without clear expectations of the benefits of the program. Exposure to the program over time has reduced uncertainty over program costs and benefits, inducing many people to self-select out, even without improvement in alternate employment options. Both of these explanations imply that more people self-select out than in the earlier years and that the decline in MGNREGA's scale is natural and desirable.

Others contest these views, especially the former, by pointing out that there is in fact a large unmet demand for MGNREGA work (Himanshu, et al., 2015; Khera, 2015; Mukhopadhyay, 2012). This claim is based mostly on survey data from workers in specific geographies, that suggests that poor implementation – specifically, unmet demand for work – has undermined the demand-driven design of the employment guarantee, discouraging workers from actively seeking work. These studies are based on surveys that ask workers how much they would like to work and/or whether or not they have sought work but not obtained it. For example, the 2013Public Evaluation of Entitlement Programmes (PEEP) Survey asked MGNREGA workers across twenty districts in ten states how many days of employment they would like to have over the year, assuming that they are paid on time. An overwhelming majority (83%) answered '100 days', the maximum entitlement. However, only 8% had actually done 100 days of MGNREGA work in 2012-13 (Drèze and Khera, 2014). In an earlier survey,only 13% of the survey households in the six Hindi speaking states secured 100 days of work (Drèze and Khera, 2011). Das (2015) and Dey and Bedi (2010) observe unmet demand in parts of West Bengal with the latter's

[4]Days generated are until financial year 2014-15 and expenditures include current financial year 2015-16 in cumulated in nominal terms valued at the exchange rate in November 2015 (http://mnregaweb4.nic.in/netnrega/all_lvl_details_dashboard_new.aspx)

[5]Former Member of the Planning Commission at a seminar titled Labour Dynamics in India organized by the International Crop Research Center in the Semi Arid Tropics (ICRISAT) in New Delhi, September 15, 2014.

[6] A Ministry of Rural Development official's statement in a conference titled The MGNREGA in India: Taking Stock, Looking Ahead, March 26, 2014.

survey finding that workers get only 10% of their desired number of days. In Surguja in Chhattisgarh, a relatively well-performing district in terms of the average number of person days of employment generated, 32.7% of sample workers reported that they faced problems getting any work.[7] These findings are reinforced in a government-initiated survey of MGNREGA workers in three states (National Sample Survey, 2011). In principle, MGNREGA is a demand-driven program where anyone who seeks work would have to be granted work according to prescribed guidelines, failing which they are entitled to an allowance. In its implementation in many parts of India, however, the program appears to be supply-driven so that work is provided by the local administration and workers do not proactively seek work. There have been instances too of workers seeking work but not getting work – i.e., they are administratively rationed out – for various reasons (Dutta et al 2012; Liu and Barrett, 2013).

There is also growing evidence that MGNREGA workers often face significant delays in wage payments, ranging anywhere between three months to over a year, even as the Act stipulates a 15 day window for wage payments. In the PEEP Survey, around 66% of respondents waited over 15 days. Similarly, close to 48% of a 1600 household survey in Surguja district, Chhattisgarh, claimed they faced problems regarding timely payments.[8] These delays, many claim, have diminished laborers' interest in MGNREGA employment (Khera 2010) and lead to a significant loss in welfare (Basu and Sen, 2015).[9]

These latter claims offer directly testable hypotheses: do program implementation failures, represented both by the uncertainty of securing work due to administrative rationing as well as by wage payments delays and/or uncertainties, cause potential beneficiaries to self-select out of the program? Further, if there is indeed evidence of a "discouraged worker effect", what factors are associated with administrative rationing or delays in wage payment in the first place?

Much has been written about the varied record of MGNREGA implementation across states.[10]Political will is often identified as a key factor and states that have better technical capacity tend to implement the MGNREGA relatively well (Narayanan and Lokhande, 2013; Comptroller and Auditor General of India, 2015). It has also been observed that poorer states tend to administratively ration more (Dutta, et al., 2012). Studies suggest that there are no discernable patterns relating to political party affiliation (Khera 2015; Sheahan, et al., 2016;). Khera (2015) points out that the better performers in terms of the average days generated were in fact the states that were administered by parties that were not in power at the center, although there is also evidence of local government power to deny wage-seekers work based on their political affiliations or proximity to the village leader (Das, 2015; Himanshu, et al., 2015).

There is a substantial difference across states in not just the extent of administrative rationing but also the degree to which rationing favors (or at least does not disfavor) the poor (Table 1.1).[11] National Sample Survey (NSS) data, a source we describe in detail in the next section, suggest that relative to 2009-10, for the country as a whole, work seeking and administrative rationing fell across the whole household expenditure distribution, the latter more than the former, resulting in increased participation rates

[7] Baseline report (unpublished) of the Stanford University's Liberation Technology Program project titled "Combating Corruption with Mobile Phones".

[8] *Ibid.*

[9] This has been reported fairly widely in the popular presss. See for example http://www.thehindu.com/sunday-anchor/is-the-mgnrega-being-set-up-for-failure/article7265266.ece, Accessed May 31, 2015.

[10] See Drèze and Oldiges (2006) for an early assessment across states and Government of India (2012) for an annotated bibliography of studies.

[11] Discerning readers will note that in 2009-10 in some states the share of households seeking work exceeded that holding job cards. 2009-10 was a drought year and the program was still in its early stages, suggesting that not all people who wanted work had applied for job cards.

conditional on job seeking for all but the very poor households (Figures 1.2a-c). The NSS data indicate that a greater proportion of the poor seek work and participate in the MGNREGA relative to those who are not poor. The data also suggest that rationing rates fell by nearly half, from 44% to 23% nationwide (Table 1.1) and became effectively uniform across the expenditure distribution, whereas in 2009-10 rationing rates were moderately pro-poor (Figure 1.2b).

The key hypotheses we test in this paper are therefore: is prior administrative rationing, delayed wage payments, or both associated with reduced worker demand for MGNREGA employment? Are any such effects distributionally regressive, discouraging poor households more than the non-poor? Which district-level factors are associated with such poor implementation?

1.3 Testing for a Discouraged Worker Effect

1.3.1 Data and Empirical Strategy

To test the discouraged worker hypothesis, we use data from two NSS rounds, the 66th Round (2009-10) and the 68th Round (2011-12).[12] These "thick rounds" covered 59,129 and 59,700 rural households, respectively.[13] Both rounds include questions on the sample household's participation in MGNREGA. Questions common to both surveys ask whether or not the household possesses a job card, whether any member of the household sought work, and whether any member of the household actually worked.

For the household level analysis, we use household level data from the 68th Round (2011-12) on whether or not any member of the household sought work in the past 365 days (representing a household's expressed demand for work) and combine these with district level rationing rate and district level delays in wage payments from the 66th Round (2009-10), representing the sources of potential discouragement. These are described in detail later in the section. For the district level analysis, we construct a district-level data from these two rounds, using work-seeking rate at the district level as indicative of demand (See Appendix 1 (Section 8.1) for Data Sources and Methods).

A few data issues merit attention. First, some districts have very few observations. We restrict the sample to those districts with a sample size over 30.[14] We also trim the bottom and top 5% of the monthly per capita expenditure (MPCE, in rupees) for the entire sample.[15]Second, discrepancies have been documented between the NSS data and the management information system data maintained by the Ministry of Rural Development (Government of India, 2012; Imbert and Papp, 2012; Narayanan and Das, 2014). While we acknowledge these discrepancies, this work focuses on the NSS data alone and aims to provide a robust analysis of the NSS data rather than attempting to explain or reconcile across the data sets the research questions concerning the discouraged worker hypothesis.

We test the discouraged worker hypothesis first at the household level (whether or not a household seeks work in the presence of implementation failures) and then at the district level (represented by the demand rate at the district) using the econometric strategy described below.

Household Analysis

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[12]The NSS 68th Round (July 2011 - June 2012) and the NSS 66th Round (July 2009 - June 2010) surveys include schedules on Employment and Unemployment and Household Consumer Expenditure.

[13] These surveys include information on 281,237 individuals in 2009-10 and 280,763 in 2011-12.

[14] Data from the NSS are representative at the district level only since the 61st Round (2004-5).

[15] For the figures we plot households on a scale of log MPCE, ranging from 5 to 9.

The first model (Model 1) regresses household-level demand for MGNREGA work in 2011-12 (i.e., whether or not the household sought MGNREGA work in 2011-12) as a function of lagged (2009-10) district level rationing rates and variables representing wage payments delays.

The district rationing rate represents the proportion of district households who sought but did not get work during 2009-10 pertaining to the 66thNSS round. Under the maintained hypothesis that administratively rationing rates are relatively well known throughout the population – if only impressionistically – the discouraged worker hypothesis would imply that higher administrative rationing rates are associated with lower subsequent probability that a household would seek MGNREGA work since workers expect a high probability of not obtaining work.[16]

Variables representing different aspects of delays in wage payments are constructed from administrative data reported annually at the district level. These administrative data report the proportion of muster rolls for which wage payments were delayed between 15-29 days, 30-59 days, 60-89 days, and 90 or more days. We use these data to construct three different variables: the proportion of muster rolls that are delayed for 90 days or more (representing uncertainty in wage payments), the proportion of muster rolls that have any delay, and an average number of days of delay. This last variable is a coarse measure, wherein we treat the minimum of each class interval reported (i.e., 15, 30, 60 and 90 days) as the delay and weight it by the proportion of muster rolls in each class interval. This is obviously a lower bound estimate on the average days of delay but is the best feasible estimate in these data. Since it is not clear whether short delays are less likely than long delays to discourage workers and likewise whether finite delays are tolerated more than uncertainty in payments, we investigate the use of these different variables to reflect the different aspects of wage delays, in turn representing implementation failures. As it turns out, the proportion of musters experiencing delayed wage payments is only modestly correlated with both the average delay (0.35) and with the proportion of muster rolls that are significantly delayed (0.32). The discouraged worker effect would appear as a negative and statistically significant coefficient estimate on the regression of seeking MGNREGA work by a household on any of these three variables, especially for the proportion of muster rolls whose delay is greater than 90 days. We use this latter as our preferred variable to represent delays in wage payments.

The discouraged worker hypothesis implies that a higher rationing rate in the district and / or delays in wage payments would reduce the probability that a household seeks work in the MGNREGA in the following period.

In general, the prospective endogeneity of past delays in wage payments is only of moderate concern since for a typical worker, his/her desire to work under the MGNREGA itself is unlikely to cause an increase in payment delays at the district level that too in the past. Yet, district level unobservable factors that affect household demand could also influence rationing rates and delays in payments. For example, the year 2009 saw banks waive debts for a large number of farmers, who had loans with banks and owned less than a hectare of land. Such a scheme imposes burden on work effort of bank staff and could aggravate delays in wage payments that are routed through banks. At the same time, these debt waivers represent implicit transfers that make workers less dependent on the MGNREGA in the subsequent period. Likewise, weather shocks might persist over time, influencing demand over a longer period. We, therefore, estimate the probit model and

[16] Work is obtained under the MGNREGA via a written application submitted to the Gram Rozgar Sewak or Field Assistant in the village. While there is no fee associated with applying for work, the cost it involves in terms of time and effort could be non-trivial.

account for the potential endogeneity of both past payments delay as well as rationing rates using a set of instruments to achieve identification.

We instrument for delay in wage payments with commercial bank branch (CBB) expansion, which offers an exogenous source of variation that influences payments delays but should have no independent effect on MGNREGA job seeking. Bank branches are likely to be established in areas of high commercial and economic activity, while the Government of India has had a long history of promoting, even mandating, expansion of bank branches in rural areas (Kochar, 2011). More recently in 2009, the government identified unbanked districts and villages; 72,721 villages were identified for branch expansion by 2012.[17]As a result, bank branch expansion is exogenous to MGNREGA and not confined to specific types of places. It is unlikely that banks open branches in anticipation of MGNREGA payments since these are by and large no-frills zero balance accounts that hold little commercial appeal for bankers. We use district level commercial bank branches in urban as well as rural areas since, in practice, job seekers in rural villages often access urban branches for wage transactions. We use these data in two different forms: the number of branches per job card, the rate of expansion of branches over a two-year period (i.e., between 2011-12 and 2009-10).[18] Both banks and post offices are involved in wage payments and the relative importance of these two varies across regions and (somewhat less) over time. Overall, around 39% of the muster roll payments were made through post offices and the rest (61%) through banks in 2011-12 and 2009-10, as per the MGNREGA administrative data. While in principle, this variable may be correlated with outside opportunities that may also contribute to demand for the MGNREGA, controls such as change in district level MPCE and change in the composition of labor types serve as proxies for outside opportunities and should ensure that this instrument satisfies the exclusion restriction condition.

We instrument for lagged rationing rate with indicators of staffing constraints. Qualitative research suggests that there exists a "technical capacity deficit" in many states (Shrivastava, 2015). There is also evidence to suggest these staffing constraints are on account of the political priorities of the state rather than of lack of personnel to fill the posts and therefore likely to be unrelated to district characteristics such as backwardness. For example, there is often a unilateral rejection of the MGNRGEA itself by higher level state functionaries.[19] We argue that staffing shortages undermine state capacity to implement the program and manifests as higher rationing rates.[20]In theory, it is possible that the greater the number of MGNREGA staff, the greater the awareness of the program among the potential workers and hence it is plausible that it has a direct effect on demand. While a proactive village functionary (Gram Rozgar Sewak) can influence and raise awareness within the village, staff at the district and block levels are far less likely to influence demand rates directly and we use the latter set of variables. Another reason this

[17]. F. No.21/13/2009-FI, Government of India Ministry of Finance Department of Financial Services.

[18]Likewise, we also used the number of post office branches with delivery services per job card, but do not present these results. We have data on post offices for 2015 but job cards data for all the years. In the absence of annual data for post offices, we use the 2015 data for post offices but job cards data for 2009-10 under the maintained hypothesis that the post office network has not expanded over these years.

[19]Shrivastava (2015) points out that the "capacity deficit" is sometimes because of an outright rejection of the Act. In the state of Uttar Pradesh, a senior functionary reportedly said "If matters were in my hand, I would have thrown away the existing contractual staff under MGNREGA, [and] forget about hiring any more" (pg.64, *ibid.*). Elsewhere in Madhya Pradesh, Nayak (2015) documents similar problems and in the authors' own fieldwork in Maharashtra; local functionaries mentioned that if they did try to implement MGNREGA they would be in trouble.

[20]We test for this in a very basic sense by estimating a cross section regression of district level rationing rate on various factors that could potentially explain rationing and find the block level staff availability is a significant correlate (Appendix Table 1 (Section 8.1))

is not a concern is because staff are not paid based on performance indicators. In Maharashtra, an incentive system was introduced only recently in 2013,where village functionaries were offered a bonus for the number of person days generated. This is however not during the period studied here. Further, the roles defined for each of the MGNREGA functionaries do not include activities that would likely influence demand patterns systematically.

The estimated model 1 is therefore:

$$Pr(Y_{hit} = 1) = F(\beta_0 + \beta_1 R_{it-1} + \beta_2 P_{i\,t-1} + \beta_3 X_{hi\,t} + \beta_4 Z_{i\,t} + \beta_5 W_i + \varepsilon_{hit}) \quad (1)$$

$$R_{it-1} = \gamma_1 + \gamma_2 Staff_{i\ \langle-1} + \gamma_3 Z_{it-1} + \gamma_4 W_i + \epsilon_r \quad (2)$$

$$P_{it-1} = \pi_1 + \pi_2 CB_{i\,t-1} + \pi_3 Z_{it-1} + \pi_4 W_i + \epsilon_p, \quad (3)$$

where $Y_{hit} = 1$ if any individual in household *h* in district *i* sought work in time *t* (2011-12) and $Y_{hit} = 0$ otherwise. F(.) is a standard normal distribution function.R_{it-1} is the rationing rate for district *i* at *t-1*(2009-10) and $P_{i\,t-1}$ is the extent of wage delays, R_{it-1} is instrumented for in the regression. $Staff_{i\,t-1}$ comprises proportion of block level MGNREGA positions that are left vacant and block level MGNREGA staff per village, $CB_{i\,t-1}$ refers to the growth of commercial bank expansion over the preceding two years. The discouraged worker hypothesis would imply negative and statistically significant coefficient estimates on both variables. X_{hit} refers to household level characteristics drawn from the NSS data and district level characteristics – those that vary over time (Z_{it}) and those that don't (W_i). These district level characteristics include the proportion of marginalized communities in the district (specifically those who belong to the Scheduled Tribes and Scheduled Castes), district literacy rate, the timing of the introduction of the program in the district (whether it is a Phase 1, 2 or 3 district), among others. All these variables control for both, the general awareness level relating to the program and proxies for the economic status of the district, both of which might influence worker interest in the MGNREGA and work seeking. We also include a binary variable for districts that come under the Integrated Action Plan (IAP).[21]

To account for weather shocks, we include the annual positive and absolute value of negative deviation of rainfall from its decadal average divided by the standard deviation of the decadal annual rainfall. These enter separately to capture possible asymmetries in the relationship. We also use a measure of the relative attractiveness of the MGNREGA that would influence current demand, proxied by the wage gap, at the district level, between the MGNREGA and a relevant alternative, the average wage of the bottom decile of the wage distribution for casual labor in agriculture and off farm.

Alongside the probit model, we estimate a Linear Probability Model (LPM) version for Model 1 (Model 1a), both as an alternate specification and to test the validity of instruments used in Model 1. Equation 1 is now therefore[22]

$$Y_{hit} = \beta_0 + \beta_1 R_{it-1} + \beta_2 P_{i\,t-1} + \beta_3 X_{hi\,t} + \beta_4 Z_{i\,t} + \beta_5 W_i + \varepsilon_{hit}, \quad (4)$$

where $Y_{hit} = 1\ or\ 0$ and estimated along with Equations (2) and (3). We cluster the standard errors at the district level in the probit model and use robust standard errors for the LPM. In addition to the above, we estimate versions of Models 1 and 1a to allow for interaction effects of average delay in wage payments with rainfall shocks to allow for the possibility

[21]The IAP was a package of assistance directed at selected tribal and backward districts under the Backward Region Grant Fund (BRGF) program.

[22]In the absence of apparent consensus on whether or not the probit or the LPM should be privileged in the context of IV estimation, we estimate both and report the correlation between the predicted probabilities from the two models.

that when there is no negative rainfall shock, delays in wage payments might be better tolerated and might not generate a discouraged worker effect. But if wage payments delays occur when households are already suffering from a negative rainfall shock and especially dependent on MGNREGA earnings for essential cash liquidity, payments delays may have a more adverse effect on subsequent labor supply. Given that the measure of delayed wage payments data is not available for all the districts in the analysis, we use a missing data dummy to avoid dropping observations from the analysis.[23] We run these models separately for the subpopulation that is poor, with monthly per capita expenditure (MPCE) below the official poverty line in the state of domicile.

District level analysis

We supplement the household analysis with district level analysis, where we test for a discouraged worker effect using the district demand rate in the context of poor MGNREGA implementation relative to other explanations that might attenuate worker interest in the program. The dependent variable is the difference in the MGNREGA work demand rate in the district between 2011-12 and 2009-10. The demand rate for district *i* in year *t* ($D_{it} \in [0,1]$) is the proportion of sampled rural households in the district that reported “seeking” work under MGNREGA. We test whether the district’s past MGNREGA implementation record – reflected in the 2009-10 administrative rationing rate and wage payments delays – is negatively and statistically significantly associated with change in worker demand over time.

We implement a “naive” least squares model that regresses the difference in demand rate between the 68th and 66th Round ($\Delta D_i \equiv D_{i,t} - D_{i,t-1}$) on administrative rationing and payments delays in the 66th Round, controlling for other the labor market attributes such as wage gap and changes in the structure in terms of sectoral distribution of workers.[24]

$$\Delta D_i = \beta_0 + \beta_1 R_{it-1} + \beta_2 P_{i\,t-1} + \beta_3 D_{it-1} + \beta_4 \Delta Z_{it} + \beta_5 W_i + \varepsilon_i \qquad (5)$$

One potential issue is that a district may suffer a fall in MGNREGA job seeking if it had an extraordinarily high demand rate in 2009 due to time-varying idiosyncratic factors (e.g., weather shocks, among others) not controlled for in differencing the dependent variable. In order to control for possible mean reversion, the model includes the demand rate in 2009-10 (66th Round) as a control. For example, if the demand rate was very high in 2009-10, the fall in demand to 2011-12 might be high as well, conditional on other factors, generating a negative regression-to-the-mean effect in the demand rate. The demand rate for 2009-10 may also independently affect implementation, for example by overtaxing administrative staff or the financial infrastructure, such that both rationing rates and delays in wage payments might be associated with the level of demand as a result. We therefore need to control for the demand rate in 2009-10 while testing for a discouraged worker effect.

We also control for the change from 2009-10 to 2011-12 in time-varying district characteristics, ΔZ_i, which might separately induce intertemporal change in jobseeking. To represent change in the availability of alternate employment opportunities we use a proxy for the district’s economic growth, computed as the difference in the average MPCE between the two years. We also include alternate measures: the inter-temporal difference

[23] The proportion of observations for which data are missing ranges from 0.46 to 0.48. It is possible that there is a systematic difference between those states that report this data and those that do not. The results on delayed payments must therefore be interpreted with care.

[24] This model is formulated to reflect closely the articulation of the discouraged worker hypothesis. We also estimate a model on levels, using demand rate in 2011-12 instead of the difference in demand rate as a dependent variable.

in the proportion of workers whose main work in the week before the survey was farming, non-farm occupations, casual work in agriculture, or casual work in non-farm sectors.[25] These variables would only measure associations since these could be partly influenced by the operation of the MGNREGA itself although estimates suggest that the scale of MGNREGA relative to the overall rural labor market is too small to make a large impact on sectoral distribution of workers. Moreover, given that we study MGNREGA demand or work-seeking, not actual participation, the case is stronger for their inclusion. Variables representing the wage gap differences are meant to reflect the fact that nominal MGNREGA wage did not increase very much until 2012 over this period even as other wages rose. So working under the MGNREGA would seem less attractive in 2011-12 relative to 2009-10.

The extensive set of controls mitigates significantly – but not entirely – the likely problem of endogeneity of wage payments delays and administrative rationing rates, since the lagged terms are predetermined, we control for base period demand and for a host of other factors that might independently affect change in MGNREGA job seeking and also be correlated with lagged payments delays or administrative rationing. There could nevertheless be more unobservable factors that induce bias in the estimates of interest.

We attempted to estimate models that address the potential endogeneity of delays in wage payments, rationing rate and demand, relying on a Two Stage Least Squares (2-SLS) model using instruments for the endogenous variables to achieve identification. We used the same set of instruments as with the household level analysis, with commercial bank branch presence and expansion in the lagged delayed payments equation and number of staff at the block level for lagged rationing rates. In addition we also use the Growing Degree Days (GDD) for the dominant crop for the major cropping season in the district as controls for lagged demand rate. GDD measures the cumulative exposure of a crop to temperature and thus has a close relationship to plant physiological growth and yields and hence to agricultural income shocks (see Appendix 2 (Section 8.1) for details). In addition, we also use the number of days in the growing period when the temperature stayed above the maximum threshold and the number of days the temperature remained above the optimum for the crop's yield levels. These thresholds and the optimal range of temperatures differ across crops and we compiled these norms relevant to India from scientific experiments conducted by agronomists (Appendix 2 (Section 8.1)). The GDD has a close correlation with crop loss and hence agricultural distress (Harou, et al, 2014; Lobell et al. 2012). Moreover, this is perhaps a more sophisticated measure for the district, since across a district one would expect less variation in the experience of temperature than with rainfall that is known to vary widely across villages within the same district. This can therefore be expected to influence rationing rate that year if this is associated with a surge in demand. But one would not expect it to have an independent effect on demand rate two years later, especially when rainfall shocks are included as explanatory variables for demand in 2011-12.

The model (Model 2) we estimate is therefore

$$R_{it-1} = \gamma_1 + \gamma_2 Staff_{i\,t-1} + \gamma_3 D_{it-1} + \gamma_4 Z_{it-1} + \gamma_5 W_i + \epsilon_r \qquad (6)$$

$$P_{it-1} = \pi_1 + \pi_2 CB_{i\,t-1} + \pi_3 D_{it-1} + \pi_4 Z_{it-1} + \pi_5 W_i + \epsilon_p \qquad (7)$$

$$D_{it-1} = \phi_1 + \phi_2 GDD_{i\,t-1} + \phi_3 Z_{it-1} + \phi_4 W_i + \epsilon_d \qquad (8)$$

where R_{it-1} is the district 2009-10 rationing rate, $P_{i\,t-1}$ represents the measure(s) of delay in wage payments in 2009-10, each reflecting the information that becomes available to prospective MGNREGA workers subsequent to their demand for work in 2009-

[25] The recall window is not a concern since the survey is balanced across seasons across the districts.

10. Controls include D_{it-1}, the 2009-10-demand rate, a range of time invariant district characteristics, W_i, and changes of a set of time varying district level characteristics measured both in 2009-10 and in 2011-12 (ΔZ_i). Details of Model 2 are available in Appendix 3 (Section 8.1).

We also used a control function approach (Model 3) as an alternative for addressing endogeneity assuming, somewhat restrictively, that the endogenous variables are generated independently of one another (Wooldridge, 2015). We use staff capacity, bank branch expansion and GDD as sources of exogenous variation. We report these in an Annexure 1 & 3. It turns out that the results don't appear to be very different from the least squares model. Coefficients estimated from the district level regression models should be interpreted as correlational relationships and not causal. These models are estimated for both the whole sample and for just the subsample of districts for which delay in payments data are available.

The descriptive statistics for data used in Models 1-3, household level and district levels, are presented in Tables 1.2 and 1.3 and a complete list of the data sources and metrics computed available in Appendix 1 (Section 8.1).

1.3.2. Results and Discussion

The household-level estimated average marginal effects (Model 1) and the IV coefficients from the second stage in the LPM (Model 1a), both reported in Table 1.4 suggest that household interest in MGNREGA employment, represented by whether or not they seek work, is negatively and significantly associated with the lagged administrative rationing rate in the household's district, controlling for a host of confounding household and district level characteristics (with full results in Appendix Tables 2-7 (Section 8.1)). A 10% increase in rationing rates at the district level reduces the probability that a household seeks work by 3.4 to 3.9%. The LPM coefficients suggest a decline in work seeking probability relative to a 10% increase in rationing rate in the range of 8.4-9.2%. [26]Instrument validity tests based on the LPM suggest that the instruments are valid and the model is identified (Appendix Table 5-7(Section 8.1)), justifying a causal interpretation of this relationship.

In contrast, there is no consistent evidence that the discouragement effect on account of payment delays matters, except in the LPM model – which suggests strongly that wage delays are another source of discouragement. Even there, wage delays seem to be comparatively less influential in determining the chances that a household seeks work. A Shorrocks-Shapely decomposition of the pseudo-R-squared from the IV-Probit model, following Shorrocks (1982), indicates that lagged rationing rate accounts for about 38.6% of the pseudo R-squared, whereas the variables associated with delayed payments account for about 4%.

Table 1.5 presents the results for district level analysis from Models 2 and 3 with full results presented in Appendix Table 8 (Section 8.1) and Appendix 3 (Section 8.1). Tests for over identifying restrictions for identification in the overidentified model failed suggest that instruments are invalid. Lagged administrative rationing is indeed negatively and statistically significantly associated with a decline in demand rates at the district level across both the `naive' least squares, 2SLS and the control function models (Model 2, Table 1.5). A 10% increase in the rationing rate is associated with suppression in work seeking by 2.2-4%.[27] Variables representing delays in wage payments have the expected

[26]The correlation coefficient between the predicted probability of seeking work in the LPM and the probit model is high at 0.77 to 0.84 but not high enough to render the choice of model irrelevant.

[27]Running this model in levels instead of differences yields qualitatively similar results. We also run the model for the subsample for which there is no missing payments delay data and the results do not change. These are not presented in the paper but can be obtained from the authors.

sign in some specifications (Table 1.5) but not in others and not across the variables that represent these delays. In the district analysis, payments delays have a statistically significant negative effect on MGNREGA labor supply when a negative rainfall shock hits, signaling that individual workers' confidence in MGNREGA as a safety net is lessened by payments delays and gets reflected in district demand rates (Table 1.5).

In general, one would have expected variables representing aspects of wage delays to be a key source of discouragement, especially the case for poor households, for whom payment delays are likely most costly due to binding liquidity constraints that drive up their shadow interest rate. One plausible reason for the absence of evidence of a discouraged worker effect for wage payment delays could be the problem of missing data; we are able to secure data only for around half of the districts for the years considered. A second reason is that these data represent delays for wages paid and do not include those wages that were left unpaid. To the extent that we do not factor in the proportion of wage liabilities that remain, that presumably is a strong source of discouragement, these results reflect this. A third reason could be that delays in wage payments are an entrenched feature of the program right since its inception so that payments delays are likely to be subsumed into peoples' expectations and the 2009-10 payments delays were consistent with people's priors, and therefore did not discourage workers in 2011-12 relative to 2009-10.[28]

The lagged demand rate, a pre-determined endogenous variable included to control for possible mean reversion, is negatively associated with change in demand and statistically significant in some specifications. As one would expect, negative rainfall shocks are associated with increases in demand, indicating that shocks tend to push people to seek employment under the MGNREGA. Districts, where the proportion of tribal population is high, tend to have higher demand, as do districts with higher literacy rates, a proxy for awareness.

There is limited evidence to support the hypothesis that as the general economic conditions improve, demand for MGNREGA work tends to fall. The change in the proportion of the district workforce employed in agriculture, either as a farmer or as a casual farm worker, is positively and statistically significantly associated with change in demand for MGNREGA. These seem to suggest that the alternate explanations for the decline of MGNREGA uptake are perhaps not credible.

1.4 Administrative rationing, pro-poor rationing and its correlates

The results in the previous section suggest that administrative rationing is a consistently important factor that depresses worker interest in MGNREGA program participation, whether we study demand for work at the level of district aggregates or individual households. This section therefore attempts identify correlates of administrative rationing.[29]Are there systematic factors associated with administrative rationing rates? Further, to what extent are these factors related to whether such rationing is pro-poor? Specifically, we are interested in understanding if any such correlates are largely political in nature or if they are more related to district-level administrative capacity relative to demand for the program.

To answer these questions, we use the NSS data as a district level panel dataset for 2009-10 and 2011-12. The panel data enables us to difference out some time invariant unobservable factors (such as chronic administrative capacity deficit) that might affect inter-district variation in administrative rationing or wage payments delays, as well as

[28] Conversations with consultants based with the Ministry of Rural Development suggest that this may be the case in several states.

[29]We do not attempt a similar analysis with delays in wage payments for these years owing to missing data.

MGNREGA labor supply. We use the rationing rate for each district in each round (R_{it}) as the dependent variable and model these as a function of various time varying characteristics at the district level (Z_{it}), including district fixed effects (α_i). Demand rate (D_{it}) is instrumented for with the GDD, as in previous models.

$$R_{it} = \alpha_i + \delta D_{it} + \varphi Z_{it} + \varepsilon_{it} \qquad \text{(Model 4)}$$
$$D_{it} = \phi_1 + \phi_2 GDD_{i\,t-1} + \phi_3 Z_{it-1} + \phi_4 W_i + \epsilon_d$$

In order to capture weather shocks we include in Z_{it} the annual positive deviation of rainfall from its decadal average divided by the standard deviation of the decadal annual rainfall as well as the annual negative deviation. In the absence of time varying data at the district level, on MGNREGA staffing and administrative vacancies (that get differenced out in the panel; see Appendix Table 1 (Section 8.1)), in order to capture an aspect of implementation efficiency, we use a proxy – performance in achieving project targets in the area of sanitation. The Nirmal Bharat Abhiyan (NBA) is the total sanitation campaign launched by the Government of India in 1999. NBA falls under a different department than MGNREGA but under the same ministry. The goal of NBA is to achieve complete coverage of all habitations and hence is, by design, not selective.[30] We use data on the percentage of planned or targeted facilities installed that have been completed as reflective of bureaucratic efficiency of the ministry implementing MGNREGA in the district.

Political factors – e.g., the political party in power, election victory margins – could potentially play a substantial role in determining who gets work and who does not. Recent evidence suggests that politics plays only a limited role (Sheahan et al., 2016) although there is substantial literature suggesting that patronage and clientelism play a significant role in public policy implementation. Other time-invariant controls include variables that represent the socio-economic profile of the district – the proportion of population belonging to the Scheduled Castes and Tribes, whether or not it is an IAP district, etc.

We then gauge whether such rationing is pro-poor through three approaches, each involving a different sub-sample for Model 4. We first restrict our analysis to households below the official poverty line of the specific state. In the second approach, we obtain the proportion of poor households in the district and use these as weights to compute weighted rationing rates, described in detail Appendix 1 (Section 8.1). Third, we use the inverse of monthly per capita expenditure (MPCE) as household weights to obtain a weighted rationing rate (For details of these computations, see Appendix 1 (Section 8.1). These are denoted as Models 5, 6 and 7 respectively). Table 1.6 presents the results of these three sub-sample regressions along with the full sample regression. Here too we use an IV approach, where demand is instrumented with GDD and the number of days in the growing period that experience greater than optimum and threshold temperatures (explained in Appendix 2 (Section 8.1) with full results reported in Appendix Tables 9 (Section 8.1)).

Demand rates in a district are positively and statistically significantly associated with rationing rates only in the Least Squares models; in the IV models, the coefficient estimates all turn statistically insignificant and are negative. The strongest correlate of the administrative rationing rate appears to be idiosyncratic shocks coming from rainfall deficits. Considering that this association exists controlling for demand rates, it appears therefore that rainfall shocks make extraordinary demands on district administrations independently of MGNREGA demand. This is conceivable since drought relief is typically

[30] After 2012, the Government of India allowed construction of toilets under the NBA as a permissible work of the MGNREGA. Since our data are from 2011-12, we can treat NBA as functionally unrelated and therefore exogenous to MGNREGA implementation.

the responsibility of the district administration and is often undertaken without an expansion in staff capacity.[31]

Explicit proxies for bureaucratic efficiency are not significantly associated with rationing rates. This needs to be interpreted in the light of the fact that differences across states in administrative capacity that presumably does not change quickly over time, has already been differenced out. The presence of banking infrastructure is negatively associated with rationing rates, suggesting that payments infrastructure helps obviate district administrations' tendency to ration work, presumably because processing payments is smoother, although anecdotal evidence from the field and the very small size of these effects suggest that this is limited.

Political factors are only weakly associated with rationing rates. While the identity of the political party representing the district matters, it is true only for certain variables that reflect UPA representation and they are not robust. For example, while share of United Progressive Alliance (UPA) votes seems to be associated with lower rationing rate, the proportion of constituencies under UPA rule does not seem to matter, nor whether or not UPA won any seat in the district.[32] The identity of the party seems to matter more for pro-poor rationing. When the proportion of constituencies within a district under control of the UPA increases to 1 from 0, the proportion of household below the poverty line rationed falls by a statistically significant 14.8%, with smaller and less precisely estimated impacts when we use rationing rates weighted by the proportion below the poverty line. Districts that have had elections more recently have lower rationing rates than those for which elections were held in the more distant past.[33] These findings are in line with previous observations that politics has limited influence over MGNREGA allocation decisions at the level of local administration (Sheahan, et al., 2016).

1.5 Concluding remarks

This paper explores the consequences of implementation failures of public workfare programs, as manifest in administrative rationing of eligible participants and in wage payments delays, using the example of the MGNREGA in India. In particular, we find strong support for the 'discouraged worker' effect in both district- and household-level data with respect to administrative rationing, but no clear support for the hypothesis arising from wage payments delays. We then examined the correlates of administrative rationing and found that rationing is associated most strongly with implementation ability, arising from the density of the supporting banking infrastructure and the extraordinary demands on district administration arising from drought shocks. Politics appears to play only a limited role in administrative rationing.

Where safety net programs offer temporary interventions in times of crisis, the ability to scale up a program during stress periods is critical. If increased administrative rationing is a natural consequence of drought shocks that temporarily overwhelm local governments and if such rationing discourages workers from subsequently seeking guaranteed employment under the program, implementation capacity can undermine program performance, especially serving the neediest households. Because declining demand for the program can be readily (mis)interpreted as an indicator of program success – graduating people from needing an employment guarantee – or growing program irrelevance – due to growth in alternate employment options – these findings are

[31]Expansion of MGNREGA entitlements, for example from 100 days per household to 150 days per household is often a part of drought relief packages.

[32]The MGNREGA was the UPA's flagship social welfare program and the Indian National Congress that headed the alliance has historically been viewed as pro-poor.

[33]While it is the case that as this number is larger, it means that a district is closer to the next election, the years for which we have data are such that for no district is this figure higher than two.

critically important to nuanced and accurate interpretation of observed decline in MGNREGA participation. Program decline may be largely a result of local implementation failures that discourage workers despite continuing need for the employment guarantee program as a safety net.

The presence of a discouraged worker effect in public works programs such as the MGNREGA offers a cautionary tale in assigning causes to program uptake, especially those that are purported to be demand driven. It is, in theory, possible that a decline in participation is misconstrued as a measure of the success of the program when it could mean the opposite, implying decay instead, suggesting that it is important to investigate the factors that drive the lifecycle trajectories of programs rather than tracking outcome indicators without scrutiny.

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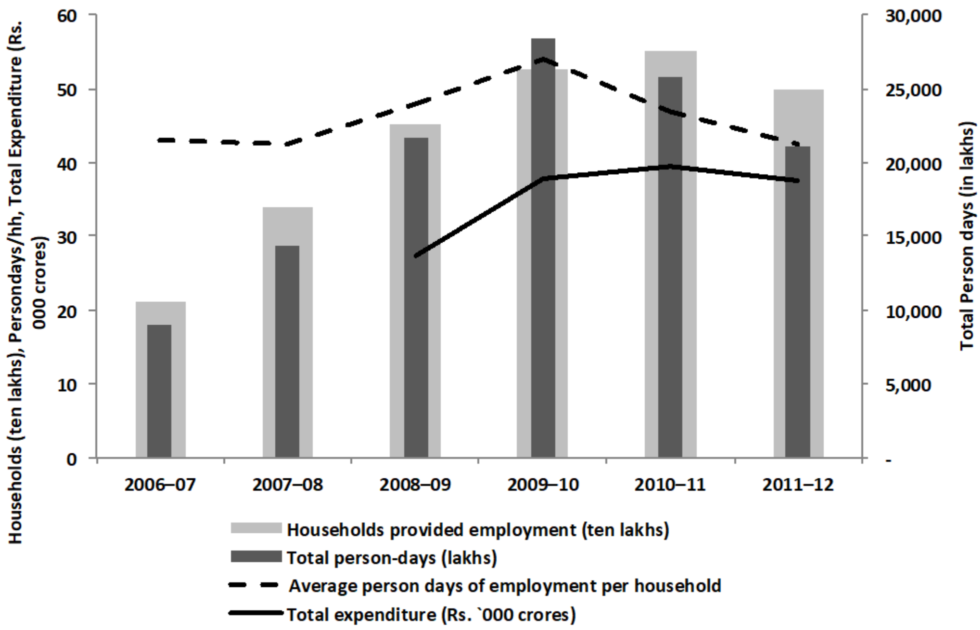
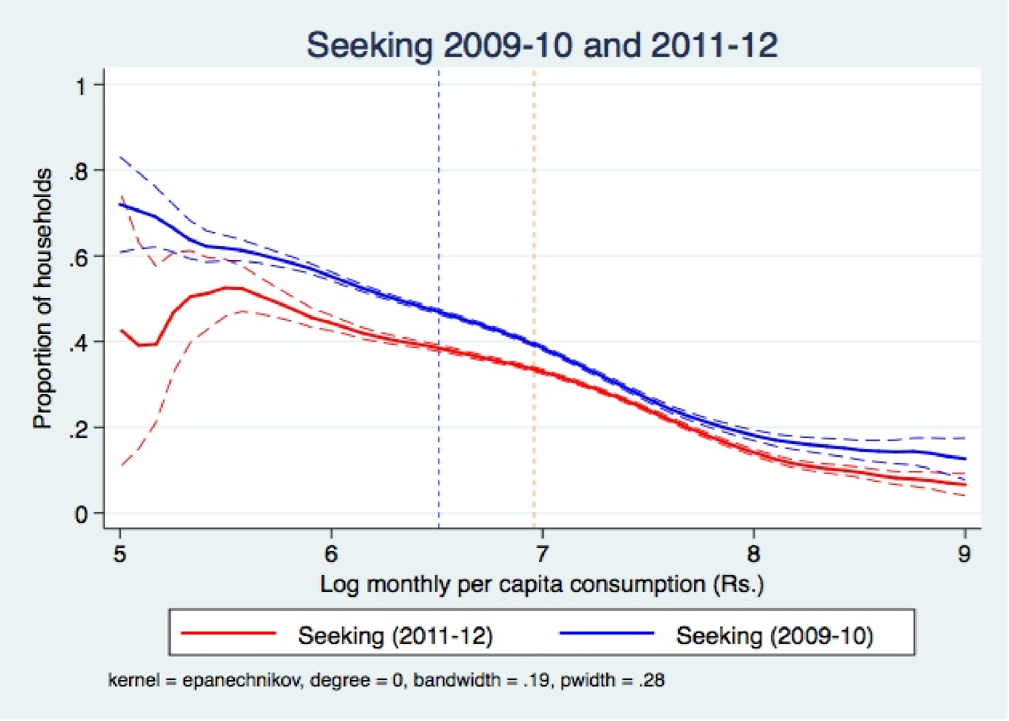
1.7 Figures and Tables

Figure 1.1: MGNREGA implementation in India, 2006-07 to 2014-15



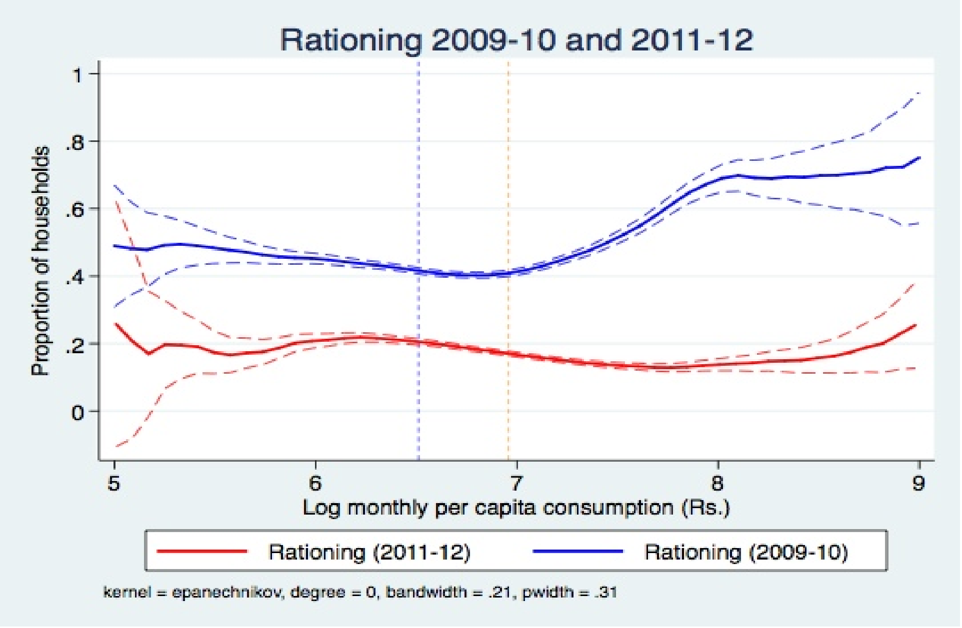
Source: Government of India (2012); www.nrega.nic.in . Accessed May, 2015.

Figure 1.2a: Work seeking rates in the MGNREGA, 2009-10 and 2011-12



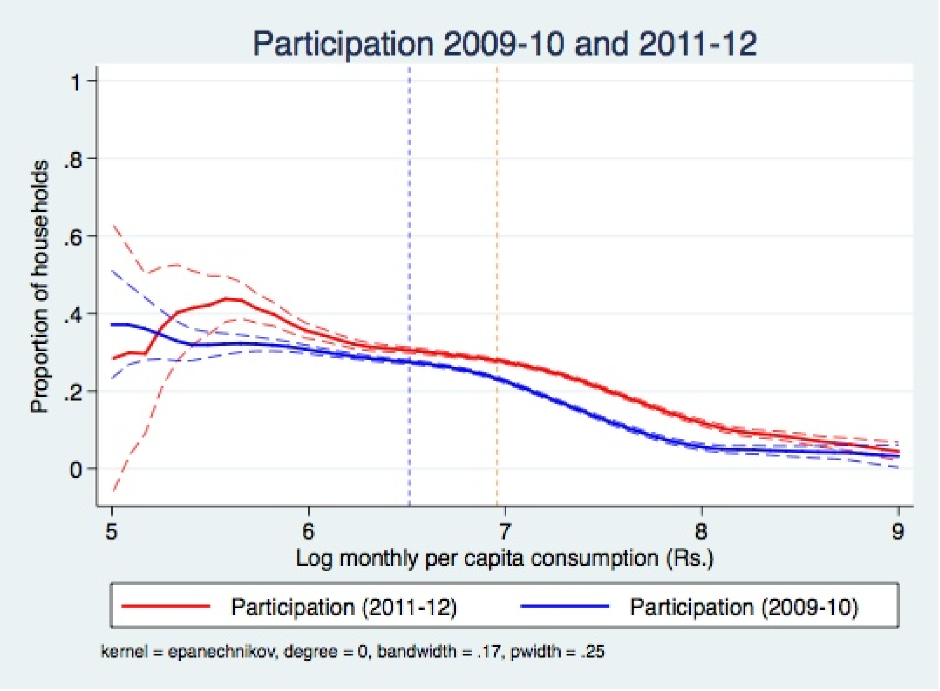
Note: The dashed vertical lines represent the Tendulkar poverty lines for each year, the red for 2011-12 and the blue for 2009-10. The dashed lines associated with each local polynomial regression are the 95% confidence intervals.

Figure 1.2b: Rationing rates in the MGNREGA, 2009-10 and 2011-12



Note: The dashed vertical lines represent the Tendulkar poverty lines for each year, the red for 2011-12 and the blue for 2009-10. The dashed lines associated with each local polynomial regression are the 95% confidence intervals.

Figure 1.2c: Participation rates in the MGNREGA, 2009-10 and 2011-12



Note: The dashed vertical lines represent the Tendulkar poverty lines for each year, the red for 2011-12 and the blue for 2009-10. The dashed lines associated with each local polynomial regression are the 95% confidence intervals

Table 1.1: Seeking, Rationing and Participation rates (2011-12 and 2009-10)

	2009-10					2011-12				
	Share of total households					Share of total households				
States	Job card	Seeking work	Participated	Rationing Rate*	Rationing Rate (poor)*	Job card	Seeking work	Participated	Rationing Rate*	Rationing Rate (poor)*
Andhra Pradesh	0.434	0.472	0.354	0.249	0.240	0.495	0.384	0.321	0.165	0.139
Arunachal Pradesh	0.220	0.515	0.215	0.582	0.627	0.406	0.383	0.352	0.080	0.105
Assam	0.287	0.413	0.182	0.559	0.539	0.364	0.312	0.230	0.262	0.313
Bihar	0.172	0.461	0.099	0.785	0.788	0.223	0.184	0.105	0.428	0.448
Chhattisgarh	0.589	0.69	0.479	0.306	0.259	0.727	0.617	0.561	0.091	0.078
Goa	0.161	0.077	0.022	0.719	0.664	0.041	0.041	0.041	0.000	0.000
Gujarat	0.300	0.382	0.215	0.438	0.353	0.238	0.144	0.066	0.541	0.482
Haryana	0.066	0.195	0.051	0.738	0.735	0.058	0.050	0.046	0.081	0.02
Himachal Pradesh	0.454	0.418	0.334	0.202	0.177	0.498	0.386	0.334	0.135	0.165
Jammu & Kashmir	0.187	0.334	0.097	0.709	0.693	0.368	0.324	0.297	0.081	0.058
Jharkhand	0.306	0.517	0.192	0.628	0.635	0.352	0.304	0.218	0.283	0.302
Karnataka	0.151	0.228	0.08	0.648	0.506	0.202	0.150	0.097	0.351	0.307
Kerala	0.196	0.232	0.112	0.517	0.362	0.291	0.198	0.186	0.060	0.037
Madhya Pradesh	0.697	0.646	0.406	0.371	0.327	0.643	0.317	0.205	0.352	0.321
Maharashtra	0.134	0.277	0.044	0.84	0.769	0.167	0.116	0.048	0.582	0.593
Manipur	0.729	0.805	0.765	0.049	0.034	0.775	0.744	0.736	0.010	0.007
Meghalaya	0.506	0.611	0.457	0.253	0.208	0.717	0.706	0.660	0.065	0.022
	2009-10					2011-12				
	Share of total households					Share of total households				
States	Job card	Seeking work	Participated	Rationing Rate*	Rationing Rate (poor)*	Job card	Seeking work	Participated	Rationing Rate*	Rationing Rate (poor)*
Mizoram	0.914	0.949	0.913	0.038	0.000	0.951	0.950	0.939	0.012	0.020
Nagaland	0.667	0.747	0.588	0.213	0.424	0.937	0.882	0.859	0.026	0.022
Orissa	0.404	0.507	0.22	0.567	0.532	0.469	0.355	0.238	0.331	0.312
Punjab	0.086	0.312	0.052	0.833	0.115	0.121	0.106	0.073	0.312	0.339
Rajasthan	0.710	0.732	0.618	0.155	0.144	0.674	0.517	0.409	0.210	0.168
Sikkim	0.458	0.460	0.441	0.041	0.025	0.631	0.593	0.578	0.026	0.020
Tamil Nadu	0.396	0.414	0.335	0.19	0.115	0.483	0.425	0.398	0.064	0.038
Tripura	0.801	0.860	0.782	0.091	0.052	0.797	0.786	0.772	0.018	0.004
Uttar Pradesh	0.211	0.35	0.162	0.536	0.504	0.264	0.227	0.191	0.159	0.142
Uttaranchal	0.343	0.406	0.292	0.28	0.357	0.358	0.316	0.276	0.128	0.159
West Bengal	0.593	0.658	0.432	0.344	0.305	0.599	0.516	0.381	0.261	0.249
India	0.348	0.447	0.249	0.444	0.423	0.384	0.300	0.231	0.231	0.232

Source: National Sample Survey, 66th Round and 68th Round.

Notes: *Rationing rate is the total households seeking but not getting work/total households seeking work.Rationing rate for the poor is the total number of households below the poverty line who seek but do not get work as a fraction of total households below the poverty line who seek work.This is computed using the entire sample, without trimming.

Table 1.2: Summary Statistics for household level analysis (Model 1)

Variable	Mean/Proportion	Standard Deviation
Rationing Rate (2009-10)	0.48	0.31
Average days of delay in wage payment (2009-10)	17.17	22.02
Average proportion of payments with over 90 days delay (2009-10)	8.47	16.80
Average percentage of wage payments delayed (2009-10)	9.25	18.80
Proportion of Scheduled Tribe households	0.16	
Proportion of Scheduled Caste households	0.17	
Proportion of Other Backward Class households	0.40	
Proportion of Upper Caste households	0.27	
Proportion of Hindu households	0.76	
Proportion of Muslim households	0.12	
Proportion of households belonging to other religions	0.11	
Proportion of landless households (0 hectares)	0.44	
Proportion of marginal landholders (0 to 1 hectares)	0.37	
Proportion of small landholders (1 to 2 hectares)	0.09	
Proportion of other landholders (More than 2 hectares)	0.09	
Proportion of households engaged in agricultural and non-agricultural labour	0.23	
Proportion of households self-employed in non-agriculture	0.26	
Proportion of households self-employed in agriculture	0.28	
Proportion of households engaged in other occupations	0.23	
Age of the household head (years)	46.89	14.14
Proportion of female household heads	0.16	
Proportion of households where head is illiterate	0.32	
Proportion of households where head is educated below primary level	0.11	
Proportion of households where head is educated between primary and middle level	0.31	
Proportion of households where head is educated between secondary and higher secondary level	0.18	
Proportion of households where head is educated above higher secondary level	0.08	
Monthly per capita consumption (‘000 Rs.)	1.365	0.591
Number of adult earning members	3.00	1.56
District Level Variables used in the Household Analysis		
Proportion of people from Scheduled Caste (districtwise)	0.17	0.10
Proportion of people from Scheduled Tribes (districtwise)	0.17	0.261
Literacy rate in the district	0.61	0.10
Integrated Action Plan district (1=Yes)	0.09	
MGNREGA Phase 1 district (1=Yes)		
MGNREGA Phase 2 district (1=Yes)	0.26	
MGNREGA Phase 3 district (1=Yes)	0.40	
Positive deviation of rainfall in 2011-12 (in standard deviation units)	9.30	2.75
Absolute value of negative deviation of rainfall in 2011-12 (in standard deviation units)	8.49	1.77
Positive deviation of rainfall in 2009-10 (in standard deviation units)	6.48	2.79
Absolute value of negative deviation of rainfall in 2009-10 (in standard deviation units)	10.61	1.92
Proportion of target in toilet construction achieved over the past three years	0.13	0.30
Difference between NREGA wages and bottom decile wages in 2011-12 (rupees)	-58.13	27.60

Table 1.3: Summary Statistics of district level variables (Models 1-3)

Variable	2009-10 Mean	2009-10 Standard Deviation	2011-12 Mean	2011-12 Standard Deviation
Demand rate	0.40	0.21	0.28	0.21
Rationing rate	0.50	0.31	0.26	0.26
Participation rate	0.22	0.20	0.24	0.20
Weighted rationing rate (See Appendix 1 (Section 8.1) for details)	0.20	0.17	0.11	0.13
Demand rate among households below poverty line	0.52	0.25	0.38	0.25
Rationing rate among households below poverty line	0.46	0.33	0.25	0.28
Participation rate among households below poverty line	0.30	0.26	0.30	0.25
Proportion among households below the official state poverty line	0.41	0.20	0.40	0.20
Average MPCE (Rs.)	1066	314	1422	413
Proportion with agriculture as the main occupation	0.29	0.10	0.30	0.10
Proportion with non-agriculture as the main occupation	0.23	0.09	0.25	0.09
Proportion with agricultural labour as the main occupation	0.12	0.07	0.09	0.06
Proportion with non-agricultural labour as the main occupation	0.17	0.09	0.15	0.09
Commercial Bank branches (hundreds)	137.2	126.4	159.9	148.5
Bank branch expansion over the two years preceding 2009-10, as percentage of branches in 2007-08	11.8	7.5	17.2	11.9
Absolute value of positive rainfall deviation	6.5	2.8	9.4	2.9
Absolute value of negative deviation of rainfall (in standard deviation units)	10.7	2.0	8.4	1.7
Proportion of targets in toilet construction achieved over the past three years under the Nirmal Bharat Abhiyan	0.14	0.28	0.12	0.08
Percentage of wage payments delayed	11.6	20.8	11.6	20.8
Approximate average delay in payment (days)	20.2	22.3	20.2	22.3
Proportion of payments with over 90 days delay	9.8	17.1	9.8	17.1
Growing Degree Days (GDD) current year	4959.9	1514.1	4816.0	1505.2
Growing Degree Days (GDD) lagged year	4911.6	1511.4	4858.9	1495.1
Job cards (in `0000 numbers)	17.73	15.08	22.44	18
Time invariant characteristics	**Mean /Proportion**	**Standard Deviation**		
Proportion of people from the Scheduled Castes	0.17	0.10		
Proportion of people from the Scheduled Tribes	0.19	0.28		
Literacy rate	0.59	0.10		
Integrated Action Plan district (1=Yes)	0.11	0.31		
MGNREGA Phase 1 district (1=Yes)	0.36			
MGNREGA Phase 2 district (1=Yes)	0.24			
MGNREGA Phase 3 district (1=Yes)	0.41			
Number of post offices (delivery) in 2015	254.7	172.7		
UPA victory in the district (1=Yes)	0.37			
Proportion of constituencies in the district won by UPA	0.34	0.34		
Ratio of UPA votes to non-UPA votes	0.90	1.97		
Share of seats reserved for SC/ST	0.35	0.31		
Proportion of electorate who voted (relative to eligible population)	0.67	0.14		
Years elapsed since previous election	3.1	1.35		
Election coincides with survey year (1=Yes)	0.31			

Source: Appendix 1 (Section 8.1) for data sources and description. Some of the district level variables are used as explanatory variables in Model 1 and 1a.

Table 1.4: The Discouraged Worker: Household level analysis Second stage results of IV Probit and IV Linear Probability Model (Model 1 and 1a)

Dependent variable: Seeking work	Probit (estimated average marginal effects, with standard errors in parentheses, clustered at district level)				Linear Probability Model (With robust standard errors)			
Sample & Specification (□) Variables of interest (□)	(a) All households	(b) All households (with rain-delay interaction)	(c)Poor households	(d) Poor households (with rain-delay interaction)	(e) All household s	(f) All households (with rain-delay interaction)	(g) Poor households	(h) Poor households (with rain-delay interaction)
Lagged Rationing Rate (2009-10)	-0.357^{**} (0.142)	-0.341^{**} (0.137)	-0.394^{**} (0.174)	-0.382^{*} (0.220)	-0.891^{***} (0.051)	-0.922^{***} (0.055)	-0.853^{***} (0.086)	-0.920^{***} (0.125)
Proportion of payments with over 90 days delay in 2009-10	0.00315 (0.0652)	-0.0249 (0.0597)	0.000032 (0.0711)	-0.0302 (0.0875)	-0.043^{**} (0.018)	-0.040^{**} (0.016)	-0.029 (0.028)	-0.038 (0.036)
Average delay in wage payments interacted with the absolute value of negative deviation in rainfall (in standard deviation units) in 2009-10		-0.0434 (0.0725)		-0.0458 (0.138)		-0.020 (0.023)		-0.055 (0.067)
Number of observations	47131	47131	15476	15476	47131	47131	15476	15476
Wald chi-squared test of exogeneity	Chi-2(2) 4.9^{*}	Chi-2(3) 16.08^{***}	Chi-2(2) 3.73	Chi-2(3) 7.63^{*}				
Under identification test: Kleinberg Papp rank LM statistic					414.11^{***}	735.444^{***}	153.819^{***}	98.328^{***}
Weak instrument test: Kleinberg Papp Wald F statistic					203.024	257.696	81.878	16.186
Stock and Yogo critical values					7.03 (10% maximal IV size)	Not available	7.03 (10% maximal IV size)	Not available

Notes: For coefficients on the probit regression and for the full set of regression results and for the Linear Probability Model with diagnostics for IV, please see Appendix Tables 2 & 3 (Section 8.1) respectively. Poor households refer to households whose Monthly per capita expenditure (MPCE) is below the official state poverty line. These regressions are therefore for the subsample of poor households. For marginal effects, standard errors in parentheses and for linear model, t-statistic in parentheses.$^{*}p< 0.10$, $^{**}p< 0.05$, $^{***}p< 0.01$. The instrument validity tests are conducted for the LPM model and reported here. Instruments: For the IV delay in payments and rationing rate have been instrumented with Proportion of block level MGNREGA positions vacant and the percentage change in bank branches over the two years preceding 2009-10 for (a), (c), (e) and (g) and when rain-delay interaction is included this is instrumented with Number of block level MGNREGA staff per village for (b), (d), (f), (h)

Table 1.5: The Discouraged Worker Effect: District level analysis (Model 2 and Model 3)

	Least Squares		Two stage least squares (Model 2)		Control function approach (Model 3)	
Dependent variable: Change in district level demand rate	**Full sample**	**Sample with no missing data**	**Full sample**	**Sample with no missing data**	**Full sample**	**Sample with no missing data**
Lagged Rationing Rate (2009-10)	-0.257***	-0.242***	-0.400***	-0.334***	-0.234***	-0.223
	(0.02)	(0.03)	(0.07)	(0.07)	(0.08)	(0.14)
Proportion of payments with over 90 days delay in 2009-10	-1.358**	-1.817***	0.077	1.924	-0.028	0.374
	(0.56)	(0.61)	(2.32)	(1.64)	(1.32)	(1.91)
Average delay in wage payments interacted with the absolute value of negative deviation in rainfall (in standard deviation units) in 2009-10	-24.629***	-29.524***	-8.251	-33.446***	-20.391**	-33.280**
	(7.21)	(8.30)	(20.13)	(10.30)	(9.91)	(14.05)
Demand Rate (2009-10)	-0.638***	-0.685***	-0.618***	-0.764***	-0.464***	-0.626***
	(0.03)	(0.05)	(0.05)	(0.08)	(0.09)	(0.15)
Predicted error term from the control function					-0.145*	0.032
					(0.09)	(0.14)
Number of Observations	550	283	551	284	522	267
R-squared	0.630	0.681			0.608	0.643

Notes: For the full set of regression results and other diagnostics, and for alternate estimations using Two Stage Least Squares and Control Function, please see Appendix Table 8, Appendix 3 (Section 8.1) . Tables 1.1 and1. 2 respectively. Variables used in the 3SLS and control function as controls for the endogenous variables include the following: Lagged rationing rate: A set of variables including proportion of block staff vacant, the number of block level staff per village, block and per job card, bank branches per job card, GDD; for proportion of payments with over 90 days delay in 2009-10, delayed interacted with absolute value of negative deviation in rainfall in 2009-10 we use block level vacancy and block staff per village and expansion of bank branches; and for demand rate in 2009-10 we use GDD variables, village level functionaries per job card and per village and program expenditures per job card and per village.

Table 1.6: Correlates of rationing and pro-poor rationing: district level panel data (Model 4,5,6,7)

Dependent variable: Rationing /Weighted rationing rate at the district level	All (Model 4)		Poor subpopulation (Model 5)		Weighted by proportion below poverty line (Model 6)		Weighted by inverse monthly per capita expenditure (Model 7)	
	Naive	IV Second Stage	Naive	IV Second Stage	Naive	IV Second Stage	Naive	IV Second Stage
Demand Rate	0.378***	-1.024	0.184**	-0.420	0.195***	-0.368	0.277**	-0.959
	(0.080)	(0.760)	(0.078)	(0.305)	(0.041)	(0.314)	(0.131)	(0.835)
Whether UPA won any seat in the district (1=Yes)	0.064	0.090	0.089	0.113	0.013	0.024	0.101	0.134
	(0.055)	(0.081)	(0.060)	(0.070)	(0.022)	(0.031)	(0.071)	(0.093)
Proportion of constituencies under UPA	-0.100	-0.150	-0.148*	-0.194**	-0.042	-0.062	-0.120	-0.170
	(0.075)	(0.107)	(0.083)	(0.096)	(0.032)	(0.042)	(0.096)	(0.121)
Share of UPA votes	-0.009	-0.012**	0.001	-0.001	-0.007***	-0.008*	-0.010	-0.012
	(0.008)	(0.005)	(0.012)	(0.011)	(0.002)	(0.004)	(0.012)	(0.008)
Years since election	0.018**	0.023**	0.020**	0.021**	0.014***	0.016***	0.017*	0.019*
	(0.007)	(0.010)	(0.008)	(0.008)	(0.004)	(0.004)	(0.010)	(0.011)
Survey year is an election year (1=Yes)	0.002	0.058	0.019	0.042*	0.021**	0.044**	-0.017	0.026
	(0.019)	(0.038)	(0.020)	(0.024)	(0.009)	(0.017)	(0.027)	(0.042)
Polling percentage (divided by 10)	0.017	0.053	0.011	0.018	-0.013	0.001	-0.004	0.020
	(0.040)	(0.057)	(0.037)	(0.043)	(0.019)	(0.025)	(0.052)	(0.065)
Proportion of seats reserved for SC/ST candidates	0.157	0.029	0.298	0.189	0.135	0.084	0.074	-0.054
	(0.189)	(0.211)	(0.217)	(0.225)	(0.107)	(0.105)	(0.296)	(0.299)
Monthly per capita expenditure (INR `000)	-0.086	-0.211**	-0.025	-0.046	-0.109***	-0.159***	-0.085	-0.198
	(0.055)	(0.098)	(0.059)	(0.067)	(0.023)	(0.041)	(0.090)	(0.122)
Absolute value of negative deviation of rainfall	0.041***	0.068***	0.047***	0.062***	0.018***	0.029***	0.044***	0.067***
	(0.006)	(0.016)	(0.006)	(0.010)	(0.003)	(0.007)	(0.008)	(0.017)
Positive deviation of rainfall	0.006	0.006	0.006	0.006	0.001	0.002	0.003	0.003
	(0.004)	(0.005)	(0.004)	(0.004)	(0.002)	(0.002)	(0.006)	(0.006)
Proportion of targets achieved in sanitation	0.019	0.237	-0.012	0.109	0.037	0.124*	0.135	0.316
	(0.106)	(0.173)	(0.092)	(0.114)	(0.037)	(0.068)	(0.151)	(0.202)
Number of commercial bank branches (`000s)	-0.022***	-0.038***	-0.024***	-0.031***	0.001	-0.005	-0.028***	-0.042***
	(0.001)	(0.012)	(0.006)	(0.008)	(0.003)	(0.005)	(0.009)	(0.015)
Number of job cards (`00,000)	-3.272	-4.691	-3.371	-4.856*	-1.084	-1.654	-1.592	-2.880
	(2.033)	(2.881)	(2.222)	(2.509)	(0.977)	(1.158)	(2.744)	(3.322)
Constant	0.116		0.009		0.048		0.411	
	(0.301)		(0.285)		(0.140)		(0.419)	
N	1054	1026	1030	984	1054	1026	1072	1060
Test of endogeneity		5.59***		4.56**		4.195***		2.919*
Underidentification test: Kleiberg Paap rank LM statistic		9.20***		19.82***		9.20***		10.8***
Weak instrument test: Cragg-Donald Wald F statistic		9.1		28.18		9.103		10.55
KleibergPaap Wald F statistic		9.65		26.95		9.648		11.21
Stock and Yogo critical values		8.96 (15% maximal IV size)		16.38 (10% maximal IV size)		8.96 (15% maximal IV size)		8.96 (15% maximal IV size)

See Appendix 1 (Section 8.1) for details on computation of the weighted rationing rate. In all the IV regressions we use as the instrument for demand rate the number of days that the temperature was above the optimal threshold for the dominant crop in the district over the cropping season for that year.

1. The “discouraged worker effect” in public works programs: Evidence from the MGNREGA in India[34]

Sudha Narayanan, Upasak Das, Yanyan Liu, and Christopher B. Barrett

1.1 Introduction

Workfare programs in developing economies have long been recognized for their role in providing social security to vulnerable populations (Subbarrao, 2003; von Braun 1998). Many of these programs are self-targeting in nature, on account of the nature of work involved and also because wage rates are typically set at lower than market wages. The demand driven nature of these programs allows those who need it most to select themselves in, while those who have access to better opportunities select themselves out, thereby avoiding problems associated with targeting (Basu, 1991; Besley and Coate, 1992; Braun, 1998; Ravallion, 2003). Some of these workfare programs have been designed as entitlement programs, with employment on public works guaranteed on demand.

There is substantial literature on whether self-targeting really works. Specifically, these address whether participants of the program are from among the poor or whether the elite capture program benefits instead – either due to the exercise of socio-political power or due to multiple market failures that cause poorer, rather than better-off, individuals to self-select out (Braun, 1998; Barrett and Clay, 2003)[35]. There is also research on whether these programs (perhaps inadvertently) exclude potential beneficiaries who seek assistance, a phenomenon known as administrative rationing (Dutta, et al., 2012; Liu and Barrett, 2013). While these sorts of implementation failures have been well documented, the ultimate consequences for potential beneficiaries' behaviour remain relatively under-researched. For example, does poor implementation undermine access to the planned safety nets in ways that can affect expressed demand for public employment, leading to underutilization of the program? This paper examines the Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA) in India to see if implementation failures discourage potential beneficiaries from seeking work.

Labor market research on developed countries notes a “discouraged worker effect”, that workers are less likely to seek work in downturns of the business cycle (that hold lower probability of getting a job) since the benefit-cost calculus of doing so would lead them to be worse off than if they were to remain unemployed or do unpaid work at home (See Benati, 2011, for a review of literature). We apply this idea to the context of a public works program, the MGNREGA in India.

There has been little systematic research of discouraged worker effects in the context of public works programs in developing countries although this phenomenon might be widespread. Much of the existing literature comes from India. For example, Khera (2008) documents such a phenomenon for drought relief works in the Indian state of Rajasthan. Recent evidence on the MGNREGA itself suggests that the uncertainty of securing work discourages workers from actively demanding work, who choose instead to wait passively and take up work if and when it is supplied (Drèze and Khera, 2014 in ten states in India; Himanshu, et al., 2015, in Rajasthan). These studies document the

[34] We are grateful to the participants at the seminar “The MGNREGA in India: Taking Stock, Looking Ahead” conducted in Mumbai, March 26-28, 2014. Sourabh Ghosh, Christopher Marciniak and Maribel Elias assisted with securing some of the data used in this paper.

[35]The broader questions of elite capture in development programs are discussed in Bardhan and Mookherjee (2000) and Platteau (2004).

possible presence of a discouraged worker effect but do not explicitly test for it. In this paper, we empirically test this hypothesis using nationally representative data.

We hypothesize that implementation failures in the MGNREGA might manifest in either or both of two forms, first as administrative rationing of work – i.e., denying employment to those who apply – and second, as delays in wage payments. Given that work under the MGNREGA is a demand-driven, legal entitlement, these implementation problems potentially affect worker demand for employment under the program. This is especially relevant in a political context where the future of the MGNREGA itself has been uncertain and its relevance has been questioned.[36]

We use nationally representative household data from two rounds of India's National Sample Survey, the 66th Round (2009-10) and the 68th Round (2011-12), combined with relevant district level data from various other sources to test for a discouraged worker effect both at the household and district levels. We find evidence consistent with a discouraged worker effect – a 10 percent increase in a district's administrative rationing rate decreases the probability that a household seeks work by 3.4-3.5%. For poor households, the discouragement effect of administrative rationing appears somewhat stronger, 3.8-3.9%. These results hold in the analysis at the district level as well– changes in district-level demand for MGNREGA employment are negatively and significantly associated with the uncertainty of obtaining MGNREGA work in the district, represented by rationing rates at the district level. The district level demand rate decreases by 8.9-9.2% in response to a 10% increase in rationing rate.

By contrast, we find no consistently robust evidence that delays in wage payments influence household-level demand for MGNREGS work. Payments delays appear to influence an individual household's probability of seeking work or district level demand rates only in some specifications. We examine reasons for this results later in the paper, but note here that this result is consistent with the widespread finding of wage inelastic labor supply (Blundell and MaCurdy 1999; Skoufias, 2004) since payment delays effectively reduce the present value of earnings. Wage delays however matter significantly when there are negative rainfall shocks.

Given that administrative rationing is a consistently significant source of discouragement, we then examine the correlates of administrative rationing and find that rationing is associated strongly with indicators of implementation ability. Removing the time invariant differences across districts using panel data that presumably strips out states' differential capacity to implement the program, politics appears to play only a limited role. The identity of the political party in power seems to matter more for pro-poor rationing, though these results are not robust. The most consistent correlate of administrative rationing appears to be negative rainfall shocks, indicating that perhaps administrative capacity is stressed and undermined with surges in demand in response to deficit rainfall.

While this study focuses on one program in India, it aims to make a broader contribution to understanding specific aspects of the lifecycle of workfare guarantees and the trajectories of welfare programs in general. Do programs decline because they outlive their usefulness or do they contain ingredients (that may or may not be manipulable) of their own demise?

The paper is organized as follows. Section 1.2 describes the MGNREGA in India and discusses the motivating issues in detail. Section 1.3 describes the data and model. Section 1.4 discusses administrative rationing and its correlates. Section 1.5 concludes.

[36] The MGNREGA was implemented in 2005 by the United Progressive Alliance (UPA). Since the Bharatiya Janata Party (BJP) won the general elections in India in 2014, there has been a debate on whether or not the MGNREGA should continue.

1.7 The MGNREGA, then and now

The MGNREGA is arguably the largest public workfare program in the world and has generated more than 18 billion person days of work, involving expenditures of US$ 44.6 billion since its inception in 2006.[37] The MGNREGA has been at an interesting juncture. When the Act was passed on September 5, 2005, its stated goal was to improve livelihood security for rural households by providing up to one hundred days of guaranteed wage employment in every financial year to every household whose adult members volunteer to do unskilled manual work (Government of India, 2013). Permissible works, typically provided within the village, include water conservation and harvesting, land development, horticulture and plantations, and rural connectivity, to name a few. Workers are paid piece rate according to a schedule of rates established by state governments for different tasks performed in different soil conditions. The program had a phased-in rollout starting with the 200 districts deemed the poorest and from there expanding to cover all of India's districts over the three year period 2006-08.

Administrative data suggest that the MGNREGA peaked in 2009 and has since declined both in the total expenditure as well as in the person-days employed (Figure 1.1). The reasons for MGNREGA's decline have been a focus of debate. One proposed explanation is that the "MGNREGA has done its job" and is perhaps no longer needed.[38] This view stems from the hypothesis that declines in demand reflect growth in attractive alternate opportunities for workers, who therefore self-select out of MGNREGA work more than they did previously. A second explanation is that the program is now better targeted.[39] It is hypothesized that in the early years of the program a lot of rural workers obtained a job card to be able to work under the MGNREGA without clear expectations of the benefits of the program. Exposure to the program over time has reduced uncertainty over program costs and benefits, inducing many people to self-select out, even without improvement in alternate employment options. Both of these explanations imply that more people self-select out than in the earlier years and that the decline in MGNREGA's scale is natural and desirable.

Others contest these views, especially the former, by pointing out that there is in fact a large unmet demand for MGNREGA work (Himanshu, et al., 2015; Khera, 2015; Mukhopadhyay, 2012). This claim is based mostly on survey data from workers in specific geographies, that suggests that poor implementation – specifically, unmet demand for work – has undermined the demand-driven design of the employment guarantee, discouraging workers from actively seeking work. These studies are based on surveys that ask workers how much they would like to work and/or whether or not they have sought work but not obtained it. For example, the 2013Public Evaluation of Entitlement Programmes (PEEP) Survey asked MGNREGA workers across twenty districts in ten states how many days of employment they would like to have over the year, assuming that they are paid on time. An overwhelming majority (83%) answered '100 days', the maximum entitlement. However, only 8% had actually done 100 days of MGNREGA work in 2012-13 (Drèze and Khera, 2014). In an earlier survey,only 13% of the survey households in the six Hindi speaking states secured 100 days of work (Drèze and Khera, 2011). Das (2015) and Dey and Bedi (2010) observe unmet demand in parts of West Bengal with the latter's

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[37]Days generated are until financial year 2014-15 and expenditures include current financial year 2015-16 in cumulated in nominal terms valued at the exchange rate in November 2015 (http://mnregaweb4.nic.in/netnrega/all_lvl_details_dashboard_new.aspx)

[38]Former Member of the Planning Commission at a seminar titled Labour Dynamics in India organized by the International Crop Research Center in the Semi Arid Tropics (ICRISAT) in New Delhi, September 15, 2014.

[39] A Ministry of Rural Development official's statement in a conference titled The MGNREGA in India: Taking Stock, Looking Ahead, March 26, 2014.

survey finding that workers get only 10% of their desired number of days. In Surguja in Chhattisgarh, a relatively well-performing district in terms of the average number of person days of employment generated, 32.7% of sample workers reported that they faced problems getting any work.[40] These findings are reinforced in a government-initiated survey of MGNREGA workers in three states (National Sample Survey, 2011). In principle, MGNREGA is a demand-driven program where anyone who seeks work would have to be granted work according to prescribed guidelines, failing which they are entitled to an allowance. In its implementation in many parts of India, however, the program appears to be supply-driven so that work is provided by the local administration and workers do not proactively seek work. There have been instances too of workers seeking work but not getting work – i.e., they are administratively rationed out – for various reasons (Dutta et al 2012; Liu and Barrett, 2013).

There is also growing evidence that MGNREGA workers often face significant delays in wage payments, ranging anywhere between three months to over a year, even as the Act stipulates a 15 day window for wage payments. In the PEEP Survey, around 66% of respondents waited over 15 days. Similarly, close to 48% of a 1600 household survey in Surguja district, Chhattisgarh, claimed they faced problems regarding timely payments.[41] These delays, many claim, have diminished laborers' interest in MGNREGA employment (Khera 2010) and lead to a significant loss in welfare (Basu and Sen, 2015).[42]

These latter claims offer directly testable hypotheses: do program implementation failures, represented both by the uncertainty of securing work due to administrative rationing as well as by wage payments delays and/or uncertainties, cause potential beneficiaries to self-select out of the program? Further, if there is indeed evidence of a "discouraged worker effect", what factors are associated with administrative rationing or delays in wage payment in the first place?

Much has been written about the varied record of MGNREGA implementation across states.[43]Political will is often identified as a key factor and states that have better technical capacity tend to implement the MGNREGA relatively well (Narayanan and Lokhande, 2013; Comptroller and Auditor General of India, 2015). It has also been observed that poorer states tend to administratively ration more (Dutta, et al., 2012). Studies suggest that there are no discernable patterns relating to political party affiliation (Khera 2015; Sheahan, et al., 2016;). Khera (2015) points out that the better performers in terms of the average days generated were in fact the states that were administered by parties that were not in power at the center, although there is also evidence of local government power to deny wage-seekers work based on their political affiliations or proximity to the village leader (Das, 2015; Himanshu, et al., 2015).

There is a substantial difference across states in not just the extent of administrative rationing but also the degree to which rationing favors (or at least does not disfavor) the poor (Table 1.1).[44] National Sample Survey (NSS) data, a source we describe in detail in the next section, suggest that relative to 2009-10, for the country as a whole, work seeking and administrative rationing fell across the whole household expenditure distribution, the latter more than the former, resulting in increased participation rates

[40]Baseline report (unpublished) of the Stanford University's Liberation Technology Program project titled "Combating Corruption with Mobile Phones".

[41]*Ibid.*

[42] This has been reported fairly widely in the popular presss. See for example http://www.thehindu.com/sunday-anchor/is-the-mgnrega-being-set-up-for-failure/article7265266.ece, Accessed May 31, 2015.

[43] See Drèze and Oldiges (2006) for an early assessment across states and Government of India (2012) for an annotated bibliography of studies.

[44] Discerning readers will note that in 2009-10 in some states the share of households seeking work exceeded that holding job cards. 2009-10 was a drought year and the program was still in its early stages, suggesting that not all people who wanted work had applied for job cards.

conditional on job seeking for all but the very poor households (Figures 1.2a-c). The NSS data indicate that a greater proportion of the poor seek work and participate in the MGNREGA relative to those who are not poor. The data also suggest that rationing rates fell by nearly half, from 44% to 23% nationwide (Table 1.1) and became effectively uniform across the expenditure distribution, whereas in 2009-10 rationing rates were moderately pro-poor (Figure 1.2b).

The key hypotheses we test in this paper are therefore: is prior administrative rationing, delayed wage payments, or both associated with reduced worker demand for MGNREGA employment? Are any such effects distributionally regressive, discouraging poor households more than the non-poor? Which district-level factors are associated with such poor implementation?

1.8 Testing for a Discouraged Worker Effect

1.8.1 Data and Empirical Strategy

To test the discouraged worker hypothesis, we use data from two NSS rounds, the 66th Round (2009-10) and the 68th Round (2011-12).[45] These "thick rounds" covered 59,129 and 59,700 rural households, respectively.[46] Both rounds include questions on the sample household's participation in MGNREGA. Questions common to both surveys ask whether or not the household possesses a job card, whether any member of the household sought work, and whether any member of the household actually worked.

For the household level analysis, we use household level data from the 68th Round (2011-12) on whether or not any member of the household sought work in the past 365 days (representing a household's expressed demand for work) and combine these with district level rationing rate and district level delays in wage payments from the 66th Round (2009-10), representing the sources of potential discouragement. These are described in detail later in the section. For the district level analysis, we construct a district-level data from these two rounds, using work-seeking rate at the district level as indicative of demand (See Appendix 1 (Section 8.1) for Data Sources and Methods).

A few data issues merit attention. First, some districts have very few observations. We restrict the sample to those districts with a sample size over 30.[47] We also trim the bottom and top 5% of the monthly per capita expenditure (MPCE, in rupees) for the entire sample.[48]Second, discrepancies have been documented between the NSS data and the management information system data maintained by the Ministry of Rural Development (Government of India, 2012; Imbert and Papp, 2012; Narayanan and Das, 2014). While we acknowledge these discrepancies, this work focuses on the NSS data alone and aims to provide a robust analysis of the NSS data rather than attempting to explain or reconcile across the data sets the research questions concerning the discouraged worker hypothesis.

We test the discouraged worker hypothesis first at the household level (whether or not a household seeks work in the presence of implementation failures) and then at the district level (represented by the demand rate at the district) using the econometric strategy described below.

Household Analysis

[45]The NSS 68th Round (July 2011 - June 2012) and the NSS 66th Round (July 2009 - June 2010) surveys include schedules on Employment and Unemployment and Household Consumer Expenditure.

[46] These surveys include information on 281,237 individuals in 2009-10 and 280,763 in 2011-12.

[47] Data from the NSS are representative at the district level only since the 61st Round (2004-5).

[48] For the figures we plot households on a scale of log MPCE, ranging from 5 to 9.

The first model (Model 1) regresses household-level demand for MGNREGA work in 2011-12 (i.e., whether or not the household sought MGNREGA work in 2011-12) as a function of lagged (2009-10) district level rationing rates and variables representing wage payments delays.

The district rationing rate represents the proportion of district households who sought but did not get work during 2009-10 pertaining to the 66thNSS round. Under the maintained hypothesis that administratively rationing rates are relatively well known throughout the population – if only impressionistically – the discouraged worker hypothesis would imply that higher administrative rationing rates are associated with lower subsequent probability that a household would seek MGNREGA work since workers expect a high probability of not obtaining work.[49]

Variables representing different aspects of delays in wage payments are constructed from administrative data reported annually at the district level. These administrative data report the proportion of muster rolls for which wage payments were delayed between 15-29 days, 30-59 days, 60-89 days, and 90 or more days. We use these data to construct three different variables: the proportion of muster rolls that are delayed for 90 days or more (representing uncertainty in wage payments), the proportion of muster rolls that have any delay, and an average number of days of delay. This last variable is a coarse measure, wherein we treat the minimum of each class interval reported (i.e., 15, 30, 60 and 90 days) as the delay and weight it by the proportion of muster rolls in each class interval. This is obviously a lower bound estimate on the average days of delay but is the best feasible estimate in these data. Since it is not clear whether short delays are less likely than long delays to discourage workers and likewise whether finite delays are tolerated more than uncertainty in payments, we investigate the use of these different variables to reflect the different aspects of wage delays, in turn representing implementation failures. As it turns out, the proportion of musters experiencing delayed wage payments is only modestly correlated with both the average delay (0.35) and with the proportion of muster rolls that are significantly delayed (0.32). The discouraged worker effect would appear as a negative and statistically significant coefficient estimate on the regression of seeking MGNREGA work by a household on any of these three variables, especially for the proportion of muster rolls whose delay is greater than 90 days. We use this latter as our preferred variable to represent delays in wage payments.

The discouraged worker hypothesis implies that a higher rationing rate in the district and / or delays in wage payments would reduce the probability that a household seeks work in the MGNREGA in the following period.

In general, the prospective endogeneity of past delays in wage payments is only of moderate concern since for a typical worker, his/her desire to work under the MGNREGA itself is unlikely to cause an increase in payment delays at the district level that too in the past. Yet, district level unobservable factors that affect household demand could also influence rationing rates and delays in payments. For example, the year 2009 saw banks waive debts for a large number of farmers, who had loans with banks and owned less than a hectare of land. Such a scheme imposes burden on work effort of bank staff and could aggravate delays in wage payments that are routed through banks. At the same time, these debt waivers represent implicit transfers that make workers less dependent on the MGNREGA in the subsequent period. Likewise, weather shocks might persist over time, influencing demand over a longer period. We, therefore, estimate the probit model and

[49] Work is obtained under the MGNREGA via a written application submitted to the Gram Rozgar Sewak or Field Assistant in the village. While there is no fee associated with applying for work, the cost it involves in terms of time and effort could be non-trivial.

account for the potential endogeneity of both past payments delay as well as rationing rates using a set of instruments to achieve identification.

We instrument for delay in wage payments with commercial bank branch (CBB) expansion, which offers an exogenous source of variation that influences payments delays but should have no independent effect on MGNREGA job seeking. Bank branches are likely to be established in areas of high commercial and economic activity, while the Government of India has had a long history of promoting, even mandating, expansion of bank branches in rural areas (Kochar, 2011). More recently in 2009, the government identified unbanked districts and villages; 72,721 villages were identified for branch expansion by 2012.[50]As a result, bank branch expansion is exogenous to MGNREGA and not confined to specific types of places. It is unlikely that banks open branches in anticipation of MGNREGA payments since these are by and large no-frills zero balance accounts that hold little commercial appeal for bankers. We use district level commercial bank branches in urban as well as rural areas since, in practice, job seekers in rural villages often access urban branches for wage transactions. We use these data in two different forms: the number of branches per job card, the rate of expansion of branches over a two-year period (i.e., between 2011-12 and 2009-10).[51] Both banks and post offices are involved in wage payments and the relative importance of these two varies across regions and (somewhat less) over time. Overall, around 39% of the muster roll payments were made through post offices and the rest (61%) through banks in 2011-12 and 2009-10, as per the MGNREGA administrative data. While in principle, this variable may be correlated with outside opportunities that may also contribute to demand for the MGNREGA, controls such as change in district level MPCE and change in the composition of labor types serve as proxies for outside opportunities and should ensure that this instrument satisfies the exclusion restriction condition.

We instrument for lagged rationing rate with indicators of staffing constraints. Qualitative research suggests that there exists a “technical capacity deficit” in many states (Shrivastava, 2015). There is also evidence to suggest these staffing constraints are on account of the political priorities of the state rather than of lack of personnel to fill the posts and therefore likely to be unrelated to district characteristics such as backwardness. For example, there is often a unilateral rejection of the MGNRGEA itself by higher level state functionaries. [52]We argue that staffing shortages undermine state capacity to implement the program and manifests as higher rationing rates.[53]In theory, it is possible that the greater the number of MGNREGA staff, the greater the awareness of the program among the potential workers and hence it is plausible that it has a direct effect on demand. While a proactive village functionary (Gram Rozgar Sewak) can influence and raise awareness within the village, staff at the district and block levels are far less likely to influence demand rates directly and we use the latter set of variables. Another reason this

[50]. F. No.21/13/2009-FI, Government of India Ministry of Finance Department of Financial Services.

[51]Likewise, we also used the number of post office branches with delivery services per job card, but do not present these results. We have data on post offices for 2015 but job cards data for all the years. In the absence of annual data for post offices, we use the 2015 data for post offices but job cards data for 2009-10 under the maintained hypothesis that the post office network has not expanded over these years.

[52]Shrivastava (2015) points out that the “capacity deficit” is sometimes because of an outright rejection of the Act. In the state of Uttar Pradesh, a senior functionary reportedly said “If matters were in my hand, I would have thrown away the existing contractual staff under MGNREGA, [and] forget about hiring any more” (pg.64, *ibid.*). Elsewhere in Madhya Pradesh, Nayak (2015) documents similar problems and in the authors’ own fieldwork in Maharashtra; local functionaries mentioned that if they did try to implement MGNREGA they would be in trouble.

[53]We test for this in a very basic sense by estimating a cross section regression of district level rationing rate on various factors that could potentially explain rationing and find the block level staff availability is a significant correlate (Appendix Table 1 (Section 8.1))

is not a concern is because staff are not paid based on performance indicators. In Maharashtra, an incentive system was introduced only recently in 2013,where village functionaries were offered a bonus for the number of person days generated. This is however not during the period studied here. Further, the roles defined for each of the MGNREGA functionaries do not include activities that would likely influence demand patterns systematically.

The estimated model 1 is therefore:

$$Pr(Y_{hit} = 1) = F(\beta_0 + \beta_1 R_{it-1} + \beta_2 P_{i\,t-1} + \beta_3 X_{hi\,t} + \beta_4 Z_{i\,t} + \beta_5 W_i + \varepsilon_{hit}) \quad (1)$$
$$R_{it-1} = \gamma_1 + \gamma_2 Staff_{i\,\langle-1} + \gamma_3 Z_{it-1} + \gamma_4 W_i + \epsilon_r \quad (2)$$
$$P_{it-1} = \pi_1 + \pi_2 CB_{i\,t-1} + \pi_3 Z_{it-1} + \pi_4 W_i + \epsilon_p, \quad (3)$$

where $Y_{hit} = 1$ if any individual in household *h* in district *i* sought work in time *t* (2011-12) and $Y_{hit} = 0$ otherwise. F(.) is a standard normal distribution function.R_{it-1} is the rationing rate for district *i* at *t-1*(2009-10) and $P_{i\,t-1}$ is the extent of wage delays, R_{it-1} is instrumented for in the regression. $Staff_{i\,t-1}$ comprises proportion of block level MGNREGA positions that are left vacant and block level MGNREGA staff per village, $CB_{i\,t-1}$ refers to the growth of commercial bank expansion over the preceding two years. The discouraged worker hypothesis would imply negative and statistically significant coefficient estimates on both variables. X_{hit} refers to household level characteristics drawn from the NSS data and district level characteristics – those that vary over time (Z_{it}) and those that don't (W_i). These district level characteristics include the proportion of marginalized communities in the district (specifically those who belong to the Scheduled Tribes and Scheduled Castes), district literacy rate, the timing of the introduction of the program in the district (whether it is a Phase 1, 2 or 3 district), among others. All these variables control for both, the general awareness level relating to the program and proxies for the economic status of the district, both of which might influence worker interest in the MGNREGA and work seeking. We also include a binary variable for districts that come under the Integrated Action Plan (IAP).[54]

To account for weather shocks, we include the annual positive and absolute value of negative deviation of rainfall from its decadal average divided by the standard deviation of the decadal annual rainfall. These enter separately to capture possible asymmetries in the relationship. We also use a measure of the relative attractiveness of the MGNREGA that would influence current demand, proxied by the wage gap, at the district level, between the MGNREGA and a relevant alternative, the average wage of the bottom decile of the wage distribution for casual labor in agriculture and off farm.

Alongside the probit model, we estimate a Linear Probability Model (LPM) version for Model 1 (Model 1a), both as an alternate specification and to test the validity of instruments used in Model 1. Equation 1 is now therefore[55]

$$Y_{hit} = \beta_0 + \beta_1 R_{it-1} + \beta_2 P_{i\,t-1} + \beta_3 X_{hi\,t} + \beta_4 Z_{i\,t} + \beta_5 W_i + \varepsilon_{hit}, \quad (4)$$

where $Y_{hit} = 1\ or\ 0$ and estimated along with Equations (2) and (3). We cluster the standard errors at the district level in the probit model and use robust standard errors for the LPM. In addition to the above, we estimate versions of Models 1 and 1a to allow for interaction effects of average delay in wage payments with rainfall shocks to allow for the possibility

[54]The IAP was a package of assistance directed at selected tribal and backward districts under the Backward Region Grant Fund (BRGF) program.

[55]In the absence of apparent consensus on whether or not the probit or the LPM should be privileged in the context of IV estimation, we estimate both and report the correlation between the predicted probabilities from the two models.

that when there is no negative rainfall shock, delays in wage payments might be better tolerated and might not generate a discouraged worker effect. But if wage payments delays occur when households are already suffering from a negative rainfall shock and especially dependent on MGNREGA earnings for essential cash liquidity, payments delays may have a more adverse effect on subsequent labor supply. Given that the measure of delayed wage payments data is not available for all the districts in the analysis, we use a missing data dummy to avoid dropping observations from the analysis.[56] We run these models separately for the subpopulation that is poor, with monthly per capita expenditure (MPCE) below the official poverty line in the state of domicile.

District level analysis

We supplement the household analysis with district level analysis, where we test for a discouraged worker effect using the district demand rate in the context of poor MGNREGA implementation relative to other explanations that might attenuate worker interest in the program. The dependent variable is the difference in the MGNREGA work demand rate in the district between 2011-12 and 2009-10. The demand rate for district *i* in year *t* ($D_{it} \in [0,1]$) is the proportion of sampled rural households in the district that reported "seeking" work under MGNREGA. We test whether the district's past MGNREGA implementation record – reflected in the 2009-10 administrative rationing rate and wage payments delays – is negatively and statistically significantly associated with change in worker demand over time.

We implement a "naive" least squares model that regresses the difference in demand rate between the 68[th] and 66[th] Round ($\Delta D_i \equiv D_{i,t} - D_{i,t-1}$) on administrative rationing and payments delays in the 66[th] Round, controlling for other the labor market attributes such as wage gap and changes in the structure in terms of sectoral distribution of workers.[57]

$$\Delta D_i = \beta_0 + \beta_1 R_{it-1} + \beta_2 P_{i\,t-1} + \beta_3 D_{it-1} + \beta_4 \Delta Z_{it} + \beta_5 W_i + \varepsilon_i \qquad (5)$$

One potential issue is that a district may suffer a fall in MGNREGA job seeking if it had an extraordinarily high demand rate in 2009 due to time-varying idiosyncratic factors (e.g., weather shocks, among others) not controlled for in differencing the dependent variable. In order to control for possible mean reversion, the model includes the demand rate in 2009-10 (66[th] Round) as a control. For example, if the demand rate was very high in 2009-10, the fall in demand to 2011-12 might be high as well, conditional on other factors, generating a negative regression-to-the-mean effect in the demand rate. The demand rate for 2009-10 may also independently affect implementation, for example by overtaxing administrative staff or the financial infrastructure, such that both rationing rates and delays in wage payments might be associated with the level of demand as a result. We therefore need to control for the demand rate in 2009-10 while testing for a discouraged worker effect.

We also control for the change from 2009-10 to 2011-12 in time-varying district characteristics, ΔZ_i, which might separately induce intertemporal change in jobseeking. To represent change in the availability of alternate employment opportunities we use a proxy for the district's economic growth, computed as the difference in the average MPCE between the two years. We also include alternate measures: the inter-temporal difference

[56] The proportion of observations for which data are missing ranges from 0.46 to 0.48. It is possible that there is a systematic difference between those states that report this data and those that do not. The results on delayed payments must therefore be interpreted with care.

[57] This model is formulated to reflect closely the articulation of the discouraged worker hypothesis. We also estimate a model on levels, using demand rate in 2011-12 instead of the difference in demand rate as a dependent variable.

in the proportion of workers whose main work in the week before the survey was farming, non-farm occupations, casual work in agriculture, or casual work in non-farm sectors.[58] These variables would only measure associations since these could be partly influenced by the operation of the MGNREGA itself although estimates suggest that the scale of MGNREGA relative to the overall rural labor market is too small to make a large impact on sectoral distribution of workers. Moreover, given that we study MGNREGA demand or work-seeking, not actual participation, the case is stronger for their inclusion. Variables representing the wage gap differences are meant to reflect the fact that nominal MGNREGA wage did not increase very much until 2012 over this period even as other wages rose. So working under the MGNREGA would seem less attractive in 2011-12 relative to 2009-10.

The extensive set of controls mitigates significantly – but not entirely – the likely problem of endogeneity of wage payments delays and administrative rationing rates, since the lagged terms are predetermined, we control for base period demand and for a host of other factors that might independently affect change in MGNREGA job seeking and also be correlated with lagged payments delays or administrative rationing. There could nevertheless be more unobservable factors that induce bias in the estimates of interest.

We attempted to estimate models that address the potential endogeneity of delays in wage payments, rationing rate and demand, relying on a Two Stage Least Squares (2-SLS) model using instruments for the endogenous variables to achieve identification. We used the same set of instruments as with the household level analysis, with commercial bank branch presence and expansion in the lagged delayed payments equation and number of staff at the block level for lagged rationing rates. In addition we also use the Growing Degree Days (GDD) for the dominant crop for the major cropping season in the district as controls for lagged demand rate. GDD measures the cumulative exposure of a crop to temperature and thus has a close relationship to plant physiological growth and yields and hence to agricultural income shocks (see Appendix 2 (Section 8.1) for details). In addition, we also use the number of days in the growing period when the temperature stayed above the maximum threshold and the number of days the temperature remained above the optimum for the crop's yield levels. These thresholds and the optimal range of temperatures differ across crops and we compiled these norms relevant to India from scientific experiments conducted by agronomists (Appendix 2 (Section 8.1)). The GDD has a close correlation with crop loss and hence agricultural distress (Harou, et al, 2014; Lobell et al. 2012). Moreover, this is perhaps a more sophisticated measure for the district, since across a district one would expect less variation in the experience of temperature than with rainfall that is known to vary widely across villages within the same district. This can therefore be expected to influence rationing rate that year if this is associated with a surge in demand. But one would not expect it to have an independent effect on demand rate two years later, especially when rainfall shocks are included as explanatory variables for demand in 2011-12.

The model (Model 2) we estimate is therefore

$$R_{it-1} = \gamma_1 + \gamma_2 Staff_{i\,t-1} + \gamma_3 D_{it-1} + \gamma_4 Z_{it-1} + \gamma_5 W_i + \epsilon_r \quad (6)$$

$$P_{it-1} = \pi_1 + \pi_2 CB_{i\,t-1} + \pi_3 D_{it-1} + \pi_4 Z_{it-1} + \pi_5 W_i + \epsilon_p \quad (7)$$

$$D_{it-1} = \phi_1 + \phi_2 GDD_{i\,t-1} + \phi_3 Z_{it-1} + \phi_4 W_i + \epsilon_d \quad (8)$$

where R_{it-1} is the district 2009-10 rationing rate, $P_{i\,t-1}$ represents the measure(s) of delay in wage payments in 2009-10, each reflecting the information that becomes available to prospective MGNREGA workers subsequent to their demand for work in 2009-

[58] The recall window is not a concern since the survey is balanced across seasons across the districts.

10. Controls include D_{it-1}, the 2009-10-demand rate, a range of time invariant district characteristics, W_i, and changes of a set of time varying district level characteristics measured both in 2009-10 and in 2011-12 (ΔZ_i). Details of Model 2 are available in Appendix 3 (Section 8.1).

We also used a control function approach (Model 3) as an alternative for addressing endogeneity assuming, somewhat restrictively, that the endogenous variables are generated independently of one another (Wooldridge, 2015). We use staff capacity, bank branch expansion and GDD as sources of exogenous variation. We report these in an Annexure 1 & 3. It turns out that the results don't appear to be very different from the least squares model. Coefficients estimated from the district level regression models should be interpreted as correlational relationships and not causal. These models are estimated for both the whole sample and for just the subsample of districts for which delay in payments data are available.

The descriptive statistics for data used in Models 1-3, household level and district levels, are presented in Tables 1.2 and 1.3 and a complete list of the data sources and metrics computed available in Appendix 1 (Section 8.1).

1.3.3. Results and Discussion

The household-level estimated average marginal effects (Model 1) and the IV coefficients from the second stage in the LPM (Model 1a), both reported in Table 1.4 suggest that household interest in MGNREGA employment, represented by whether or not they seek work, is negatively and significantly associated with the lagged administrative rationing rate in the household's district, controlling for a host of confounding household and district level characteristics (with full results in Appendix Tables 2-7 (Section 8.1)). A 10% increase in rationing rates at the district level reduces the probability that a household seeks work by 3.4 to 3.9%. The LPM coefficients suggest a decline in work seeking probability relative to a 10% increase in rationing rate in the range of 8.4-9.2%. [59]Instrument validity tests based on the LPM suggest that the instruments are valid and the model is identified (Appendix Table 5-7(Section 8.1)), justifying a causal interpretation of this relationship.

In contrast, there is no consistent evidence that the discouragement effect on account of payment delays matters, except in the LPM model – which suggests strongly that wage delays are another source of discouragement. Even there, wage delays seem to be comparatively less influential in determining the chances that a household seeks work. A Shorrocks-Shapely decomposition of the pseudo-R-squared from the IV-Probit model, following Shorrocks (1982), indicates that lagged rationing rate accounts for about 38.6% of the pseudo R-squared, whereas the variables associated with delayed payments account for about 4%.

Table 1.5 presents the results for district level analysis from Models 2 and 3 with full results presented in Appendix Table 8 (Section 8.1) and Appendix 3 (Section 8.1). Tests for over identifying restrictions for identification in the overidentified model failed suggest that instruments are invalid. Lagged administrative rationing is indeed negatively and statistically significantly associated with a decline in demand rates at the district level across both the `naive' least squares, 2SLS and the control function models (Model 2, Table 1.5). A 10% increase in the rationing rate is associated with suppression in work seeking by 2.2-4%.[60] Variables representing delays in wage payments have the expected

[59]The correlation coefficient between the predicted probability of seeking work in the LPM and the probit model is high at 0.77 to 0.84 but not high enough to render the choice of model irrelevant.

[60]Running this model in levels instead of differences yields qualitatively similar results. We also run the model for the subsample for which there is no missing payments delay data and the results do not change. These are not presented in the paper but can be obtained from the authors.

sign in some specifications (Table 1.5) but not in others and not across the variables that represent these delays. In the district analysis, payments delays have a statistically significant negative effect on MGNREGA labor supply when a negative rainfall shock hits, signaling that individual workers' confidence in MGNREGA as a safety net is lessened by payments delays and gets reflected in district demand rates (Table 1.5).

In general, one would have expected variables representing aspects of wage delays to be a key source of discouragement, especially the case for poor households, for whom payment delays are likely most costly due to binding liquidity constraints that drive up their shadow interest rate. One plausible reason for the absence of evidence of a discouraged worker effect for wage payment delays could be the problem of missing data; we are able to secure data only for around half of the districts for the years considered. A second reason is that these data represent delays for wages paid and do not include those wages that were left unpaid. To the extent that we do not factor in the proportion of wage liabilities that remain, that presumably is a strong source of discouragement, these results reflect this. A third reason could be that delays in wage payments are an entrenched feature of the program right since its inception so that payments delays are likely to be subsumed into peoples' expectations and the 2009-10 payments delays were consistent with people's priors, and therefore did not discourage workers in 2011-12 relative to 2009-10.[61]

The lagged demand rate, a pre-determined endogenous variable included to control for possible mean reversion, is negatively associated with change in demand and statistically significant in some specifications. As one would expect, negative rainfall shocks are associated with increases in demand, indicating that shocks tend to push people to seek employment under the MGNREGA. Districts, where the proportion of tribal population is high, tend to have higher demand, as do districts with higher literacy rates, a proxy for awareness.

There is limited evidence to support the hypothesis that as the general economic conditions improve, demand for MGNREGA work tends to fall. The change in the proportion of the district workforce employed in agriculture, either as a farmer or as a casual farm worker, is positively and statistically significantly associated with change in demand for MGNREGA. These seem to suggest that the alternate explanations for the decline of MGNREGA uptake are perhaps not credible.

1.9 Administrative rationing, pro-poor rationing and its correlates

The results in the previous section suggest that administrative rationing is a consistently important factor that depresses worker interest in MGNREGA program participation, whether we study demand for work at the level of district aggregates or individual households. This section therefore attempts identify correlates of administrative rationing.[62]Are there systematic factors associated with administrative rationing rates? Further, to what extent are these factors related to whether such rationing is pro-poor? Specifically, we are interested in understanding if any such correlates are largely political in nature or if they are more related to district-level administrative capacity relative to demand for the program.

To answer these questions, we use the NSS data as a district level panel dataset for 2009-10 and 2011-12. The panel data enables us to difference out some time invariant unobservable factors (such as chronic administrative capacity deficit) that might affect inter-district variation in administrative rationing or wage payments delays, as well as

[61] Conversations with consultants based with the Ministry of Rural Development suggest that this may be the case in several states.

[62]We do not attempt a similar analysis with delays in wage payments for these years owing to missing data.

MGNREGA labor supply. We use the rationing rate for each district in each round (R_{it}) as the dependent variable and model these as a function of various time varying characteristics at the district level (Z_{it}), including district fixed effects (α_i). Demand rate (D_{it}) is instrumented for with the GDD, as in previous models.

$$R_{it} = \alpha_i + \delta D_{it} + \varphi Z_{it} + \varepsilon_{it} \qquad \text{(Model 4)}$$
$$D_{it} = \phi_1 + \phi_2 GDD_{i\,t-1} + \phi_3 Z_{it-1} + \phi_4 W_i + \epsilon_d$$

In order to capture weather shocks we include in Z_{it} the annual positive deviation of rainfall from its decadal average divided by the standard deviation of the decadal annual rainfall as well as the annual negative deviation. In the absence of time varying data at the district level, on MGNREGA staffing and administrative vacancies (that get differenced out in the panel; see Appendix Table 1 (Section 8.1)), in order to capture an aspect of implementation efficiency, we use a proxy – performance in achieving project targets in the area of sanitation. The Nirmal Bharat Abhiyan (NBA) is the total sanitation campaign launched by the Government of India in 1999. NBA falls under a different department than MGNREGA but under the same ministry. The goal of NBA is to achieve complete coverage of all habitations and hence is, by design, not selective.[63] We use data on the percentage of planned or targeted facilities installed that have been completed as reflective of bureaucratic efficiency of the ministry implementing MGNREGA in the district.

Political factors – e.g., the political party in power, election victory margins – could potentially play a substantial role in determining who gets work and who does not. Recent evidence suggests that politics plays only a limited role (Sheahan et al., 2016) although there is substantial literature suggesting that patronage and clientelism play a significant role in public policy implementation. Other time-invariant controls include variables that represent the socio-economic profile of the district – the proportion of population belonging to the Scheduled Castes and Tribes, whether or not it is an IAP district, etc.

We then gauge whether such rationing is pro-poor through three approaches, each involving a different sub-sample for Model 4. We first restrict our analysis to households below the official poverty line of the specific state. In the second approach, we obtain the proportion of poor households in the district and use these as weights to compute weighted rationing rates, described in detail Appendix 1 (Section 8.1). Third, we use the inverse of monthly per capita expenditure (MPCE) as household weights to obtain a weighted rationing rate (For details of these computations, see Appendix 1 (Section 8.1). These are denoted as Models 5, 6 and 7 respectively). Table 1.6 presents the results of these three sub-sample regressions along with the full sample regression. Here too we use an IV approach, where demand is instrumented with GDD and the number of days in the growing period that experience greater than optimum and threshold temperatures (explained in Appendix 2 (Section 8.1) with full results reported in Appendix Tables 9 (Section 8.1)).

Demand rates in a district are positively and statistically significantly associated with rationing rates only in the Least Squares models; in the IV models, the coefficient estimates all turn statistically insignificant and are negative. The strongest correlate of the administrative rationing rate appears to be idiosyncratic shocks coming from rainfall deficits. Considering that this association exists controlling for demand rates, it appears therefore that rainfall shocks make extraordinary demands on district administrations independently of MGNREGA demand. This is conceivable since drought relief is typically

[63] After 2012, the Government of India allowed construction of toilets under the NBA as a permissible work of the MGNREGA. Since our data are from 2011-12, we can treat NBA as functionally unrelated and therefore exogenous to MGNREGA implementation.

the responsibility of the district administration and is often undertaken without an expansion in staff capacity.[64]

Explicit proxies for bureaucratic efficiency are not significantly associated with rationing rates. This needs to be interpreted in the light of the fact that differences across states in administrative capacity that presumably does not change quickly over time, has already been differenced out. The presence of banking infrastructure is negatively associated with rationing rates, suggesting that payments infrastructure helps obviate district administrations' tendency to ration work, presumably because processing payments is smoother, although anecdotal evidence from the field and the very small size of these effects suggest that this is limited.

Political factors are only weakly associated with rationing rates. While the identity of the political party representing the district matters, it is true only for certain variables that reflect UPA representation and they are not robust. For example, while share of United Progressive Alliance (UPA) votes seems to be associated with lower rationing rate, the proportion of constituencies under UPA rule does not seem to matter, nor whether or not UPA won any seat in the district.[65] The identity of the party seems to matter more for pro-poor rationing. When the proportion of constituencies within a district under control of the UPA increases to 1 from 0, the proportion of household below the poverty line rationed falls by a statistically significant 14.8%, with smaller and less precisely estimated impacts when we use rationing rates weighted by the proportion below the poverty line. Districts that have had elections more recently have lower rationing rates than those for which elections were held in the more distant past.[66] These findings are in line with previous observations that politics has limited influence over MGNREGA allocation decisions at the level of local administration (Sheahan, et al., 2016).

1.10 Concluding remarks

This paper explores the consequences of implementation failures of public workfare programs, as manifest in administrative rationing of eligible participants and in wage payments delays, using the example of the MGNREGA in India. In particular, we find strong support for the 'discouraged worker' effect in both district- and household-level data with respect to administrative rationing, but no clear support for the hypothesis arising from wage payments delays. We then examined the correlates of administrative rationing and found that rationing is associated most strongly with implementation ability, arising from the density of the supporting banking infrastructure and the extraordinary demands on district administration arising from drought shocks. Politics appears to play only a limited role in administrative rationing.

Where safety net programs offer temporary interventions in times of crisis, the ability to scale up a program during stress periods is critical. If increased administrative rationing is a natural consequence of drought shocks that temporarily overwhelm local governments and if such rationing discourages workers from subsequently seeking guaranteed employment under the program, implementation capacity can undermine program performance, especially serving the neediest households. Because declining demand for the program can be readily (mis)interpreted as an indicator of program success – graduating people from needing an employment guarantee – or growing program irrelevance – due to growth in alternate employment options – these findings are

[64]Expansion of MGNREGA entitlements, for example from 100 days per household to 150 days per household is often a part of drought relief packages.

[65]The MGNREGA was the UPA's flagship social welfare program and the Indian National Congress that headed the alliance has historically been viewed as pro-poor.

[66]While it is the case that as this number is larger, it means that a district is closer to the next election, the years for which we have data are such that for no district is this figure higher than two.

critically important to nuanced and accurate interpretation of observed decline in MGNREGA participation. Program decline may be largely a result of local implementation failures that discourage workers despite continuing need for the employment guarantee program as a safety net.

The presence of a discouraged worker effect in public works programs such as the MGNREGA offers a cautionary tale in assigning causes to program uptake, especially those that are purported to be demand driven. It is, in theory, possible that a decline in participation is misconstrued as a measure of the success of the program when it could mean the opposite, implying decay instead, suggesting that it is important to investigate the factors that drive the lifecycle trajectories of programs rather than tracking outcome indicators without scrutiny.

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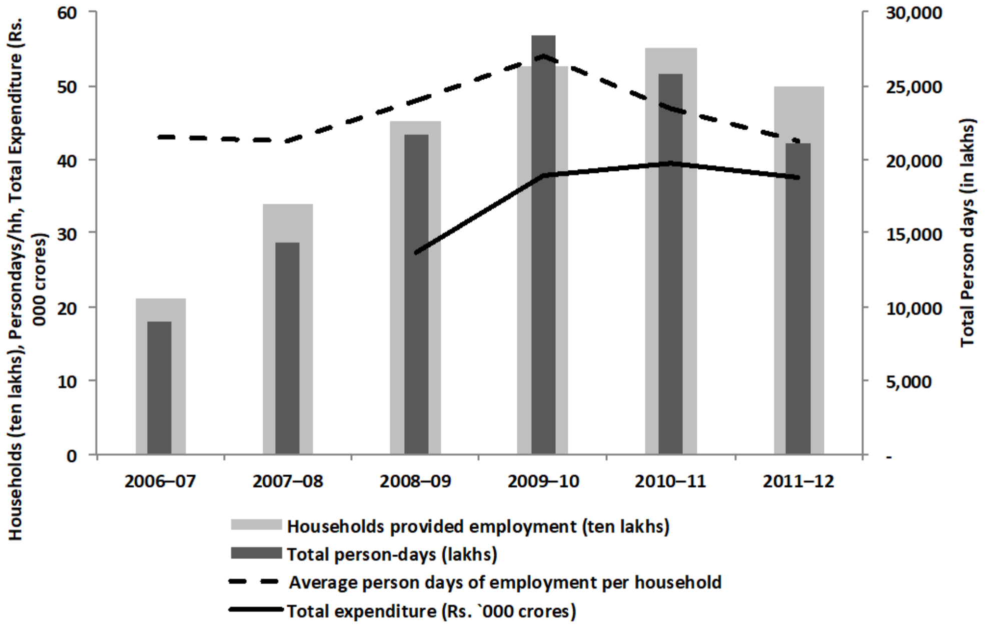
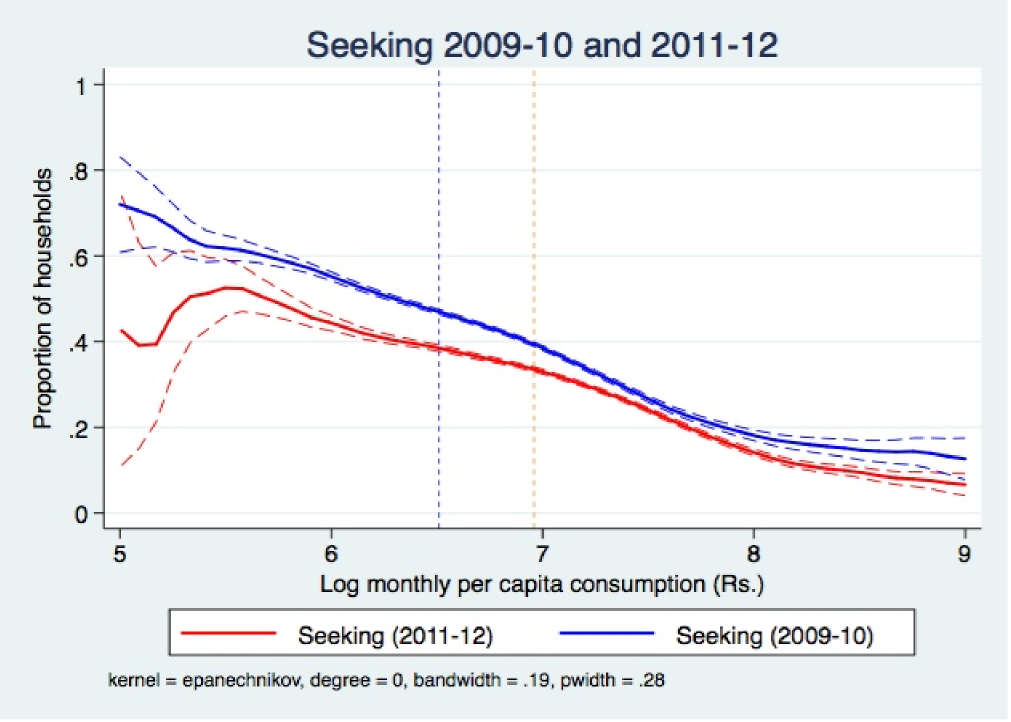
1.7 Figures and Tables

Figure 3.1: MGNREGA implementation in India, 2006-07 to 2014-15



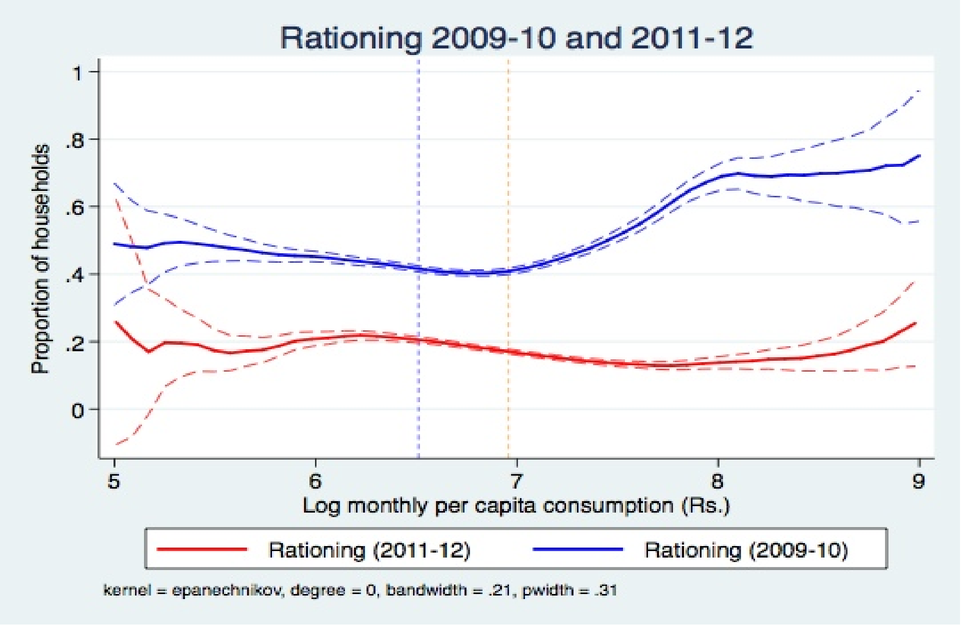
Source: Government of India (2012); www.nrega.nic.in . Accessed May, 2015.

Figure 1.4a: Work seeking rates in the MGNREGA, 2009-10 and 2011-12



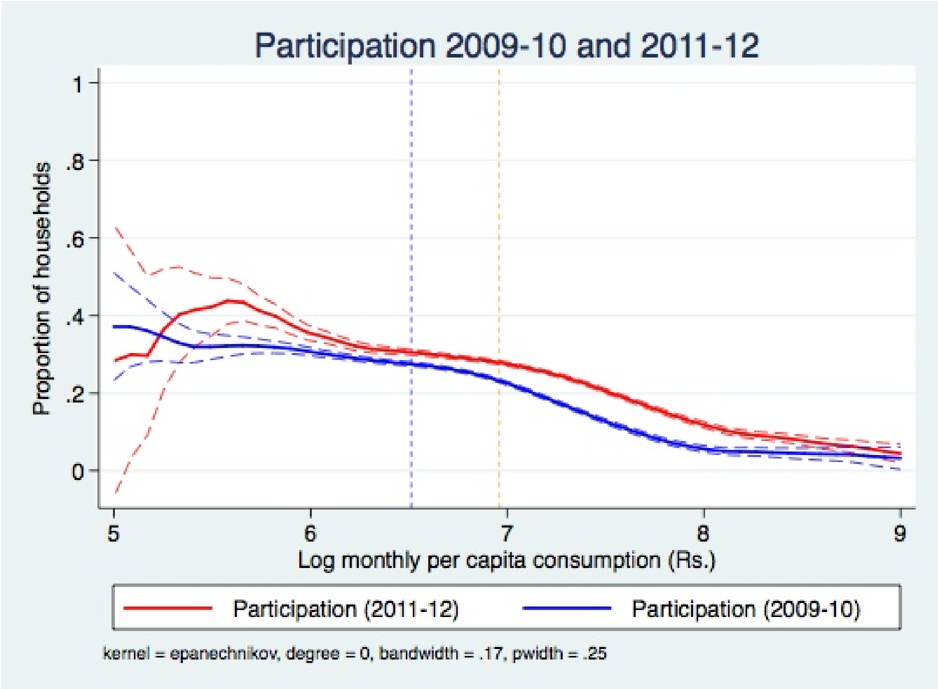
Note: The dashed vertical lines represent the Tendulkar poverty lines for each year, the red for 2011-12 and the blue for 2009-10. The dashed lines associated with each local polynomial regression are the 95% confidence intervals.

Figure 1.2b: Rationing rates in the MGNREGA, 2009-10 and 2011-12



Note: The dashed vertical lines represent the Tendulkar poverty lines for each year, the red for 2011-12 and the blue for 2009-10. The dashed lines associated with each local polynomial regression are the 95% confidence intervals.

Figure 1.2c: Participation rates in the MGNREGA, 2009-10 and 2011-12



Note: The dashed vertical lines represent the Tendulkar poverty lines for each year, the red for 2011-12 and the blue for 2009-10. The dashed lines associated with each local polynomial regression are the 95% confidence intervals

Table 4.1: Seeking, Rationing and Participation rates (2011-12 and 2009-10)

	2009-10					2011-12				
	Share of total households					Share of total households				
States	Job card	Seeking work	Participated	Rationing Rate*	Rationing Rate (poor)*	Job card	Seeking work	Participated	Rationing Rate*	Rationing Rate (poor)*
Andhra Pradesh	0.434	0.472	0.354	0.249	0.240	0.495	0.384	0.321	0.165	0.139
Arunachal Pradesh	0.220	0.515	0.215	0.582	0.627	0.406	0.383	0.352	0.080	0.105
Assam	0.287	0.413	0.182	0.559	0.539	0.364	0.312	0.230	0.262	0.313
Bihar	0.172	0.461	0.099	0.785	0.788	0.223	0.184	0.105	0.428	0.448
Chhattisgarh	0.589	0.69	0.479	0.306	0.259	0.727	0.617	0.561	0.091	0.078
Goa	0.161	0.077	0.022	0.719	0.664	0.041	0.041	0.041	0.000	0.000
Gujarat	0.300	0.382	0.215	0.438	0.353	0.238	0.144	0.066	0.541	0.482
Haryana	0.066	0.195	0.051	0.738	0.735	0.058	0.050	0.046	0.081	0.02
Himachal Pradesh	0.454	0.418	0.334	0.202	0.177	0.498	0.386	0.334	0.135	0.165
Jammu & Kashmir	0.187	0.334	0.097	0.709	0.693	0.368	0.324	0.297	0.081	0.058
Jharkhand	0.306	0.517	0.192	0.628	0.635	0.352	0.304	0.218	0.283	0.302
Karnataka	0.151	0.228	0.08	0.648	0.506	0.202	0.150	0.097	0.351	0.307
Kerala	0.196	0.232	0.112	0.517	0.362	0.291	0.198	0.186	0.060	0.037
Madhya Pradesh	0.697	0.646	0.406	0.371	0.327	0.643	0.317	0.205	0.352	0.321
Maharashtra	0.134	0.277	0.044	0.84	0.769	0.167	0.116	0.048	0.582	0.593
Manipur	0.729	0.805	0.765	0.049	0.034	0.775	0.744	0.736	0.010	0.007
Meghalaya	0.506	0.611	0.457	0.253	0.208	0.717	0.706	0.660	0.065	0.022
	2009-10					2011-12				
	Share of total households					Share of total households				
States	Job card	Seeking work	Participated	Rationing Rate*	Rationing Rate (poor)*	Job card	Seeking work	Participated	Rationing Rate*	Rationing Rate (poor)*
Mizoram	0.914	0.949	0.913	0.038	0.000	0.951	0.950	0.939	0.012	0.020
Nagaland	0.667	0.747	0.588	0.213	0.424	0.937	0.882	0.859	0.026	0.022
Orissa	0.404	0.507	0.22	0.567	0.532	0.469	0.355	0.238	0.331	0.312
Punjab	0.086	0.312	0.052	0.833	0.115	0.121	0.106	0.073	0.312	0.339
Rajasthan	0.710	0.732	0.618	0.155	0.144	0.674	0.517	0.409	0.210	0.168
Sikkim	0.458	0.460	0.441	0.041	0.025	0.631	0.593	0.578	0.026	0.020
Tamil Nadu	0.396	0.414	0.335	0.19	0.115	0.483	0.425	0.398	0.064	0.038
Tripura	0.801	0.860	0.782	0.091	0.052	0.797	0.786	0.772	0.018	0.004
Uttar Pradesh	0.211	0.35	0.162	0.536	0.504	0.264	0.227	0.191	0.159	0.142
Uttaranchal	0.343	0.406	0.292	0.28	0.357	0.358	0.316	0.276	0.128	0.159
West Bengal	0.593	0.658	0.432	0.344	0.305	0.599	0.516	0.381	0.261	0.249
India	0.348	0.447	0.249	0.444	0.423	0.384	0.300	0.231	0.231	0.232

Source: National Sample Survey, 66th Round and 68th Round.

Notes: *Rationing rate is the total households seeking but not getting work/total households seeking work.Rationing rate for the poor is the total number of households below the poverty line who seek but do not get work as a fraction of total households below the poverty line who seek work.This is computed using the entire sample, without trimming.

Table 1.5: Summary Statistics for household level analysis (Model 1)

Variable	Mean/Proportion	Standard Deviation
Rationing Rate (2009-10)	0.48	0.31
Average days of delay in wage payment (2009-10)	17.17	22.02
Average proportion of payments with over 90 days delay (2009-10)	8.47	16.80
Average percentage of wage payments delayed (2009-10)	9.25	18.80
Proportion of Scheduled Tribe households	0.16	
Proportion of Scheduled Caste households	0.17	
Proportion of Other Backward Class households	0.40	
Proportion of Upper Caste households	0.27	
Proportion of Hindu households	0.76	
Proportion of Muslim households	0.12	
Proportion of households belonging to other religions	0.11	
Proportion of landless households (0 hectares)	0.44	
Proportion of marginal landholders (0 to 1 hectares)	0.37	
Proportion of small landholders (1 to 2 hectares)	0.09	
Proportion of other landholders (More than 2 hectares)	0.09	
Proportion of households engaged in agricultural and non agricultural labor	0.23	
Proportion of households self employed in non-agriculture	0.26	
Proportion of households self employed in agriculture	0.28	
Proportion of households engaged in other occupations	0.23	
Age of the household head (years)	46.89	14.14
Proportion of female household heads	0.16	
Proportion of households where head is illiterate	0.32	
Proportion of households where head is educated below primary level	0.11	
Proportion of households where head is educated between primary and middle level	0.31	
Proportion of households where head is educated between secondary and higher secondary level	0.18	
Proportion of households where head is educated above higher secondary level	0.08	
Monthly per capita consumption (‘000 Rs.)	1.365	0.591
Number of adult earning members	3.00	1.56
District Level Variables used in the Household Analysis		
Proportion of people from Scheduled Caste (districtwise)	0.17	0.10
Proportion of people from Scheduled Tribes (districtwise)	0.17	0.261
Literacy rate in the district	0.61	0.10
Integrated Action Plan district (1=Yes)	0.09	
MGNREGA Phase 1 district (1=Yes)		
MGNREGA Phase 2 district (1=Yes)	0.26	
MGNREGA Phase 3 district (1=Yes)	0.40	
Positive deviation of rainfall in 2011-12 (in standard deviation units)	9.30	2.75
Absolute value of negative deviation of rainfall in 2011-12 (in standard deviation units)	8.49	1.77
Positive deviation of rainfall in 2009-10 (in standard deviation units)	6.48	2.79
Absolute value of negative deviation of rainfall in 2009-10 (in standard deviation units)	10.61	1.92
Proportion of target in toilet construction achieved over the past three years	0.13	0.30
Difference between NREGA wages and bottom decile wages in 2011-12 (rupees)	-58.13	27.60

Table 1.6: Summary Statistics of district level variables (Models 1-3)

Variable	2009-10 Mean	2009-10 Standard Deviation	2011-12 Mean	2011-12 Standard Deviation
Demand rate	0.40	0.21	0.28	0.21
Rationing rate	0.50	0.31	0.26	0.26
Participation rate	0.22	0.20	0.24	0.20
Weighted rationing rate (See Appendix 1 (Section 8.1) for details)	0.20	0.17	0.11	0.13
Demand rate among households below poverty line	0.52	0.25	0.38	0.25
Rationing rate among households below poverty line	0.46	0.33	0.25	0.28
Participation rate among households below poverty line	0.30	0.26	0.30	0.25
Proportion among households below the official state poverty line	0.41	0.20	0.40	0.20
Average MPCE (Rs.)	1066	314	1422	413
Proportion with agriculture as the main occupation	0.29	0.10	0.30	0.10
Proportion with non-agriculture as the main occupation	0.23	0.09	0.25	0.09
Proportion with agricultural labour as the main occupation	0.12	0.07	0.09	0.06
Proportion with non-agricultural labour as the main occupation	0.17	0.09	0.15	0.09
Commercial Bank branches (hundreds)	137.2	126.4	159.9	148.5
Bank branch expansion over the two years preceding 2009-10, as percentage of branches in 2007-08	11.8	7.5	17.2	11.9
Absolute value of positive rainfall deviation	6.5	2.8	9.4	2.9
Absolute value of negative deviation of rainfall (in standard deviation units)	10.7	2.0	8.4	1.7
Proportion of targets in toilet construction achieved over the past three years under the Nirmal Bharat Abhiyan	0.14	0.28	0.12	0.08
Percentage of wage payments delayed	11.6	20.8	11.6	20.8
Approximate average delay in payment (days)	20.2	22.3	20.2	22.3
Proportion of payments with over 90 days delay	9.8	17.1	9.8	17.1
Growing Degree Days (GDD) current year	4959.9	1514.1	4816.0	1505.2
Growing Degree Days (GDD) lagged year	4911.6	1511.4	4858.9	1495.1
Job cards (in `0000 numbers)	17.73	15.08	22.44	18
Time invariant characteristics	**Mean /Proportion**	**Standard Deviation**		
Proportion of people from the Scheduled Castes	0.17	0.10		
Proportion of people from the Scheduled Tribes	0.19	0.28		
Literacy rate	0.59	0.10		
Integrated Action Plan district (1=Yes)	0.11	0.31		
MGNREGA Phase 1 district (1=Yes)	0.36			
MGNREGA Phase 2 district (1=Yes)	0.24			
MGNREGA Phase 3 district (1=Yes)	0.41			
Number of post offices (delivery) in 2015	254.7	172.7		
UPA victory in the district (1=Yes)	0.37			
Proportion of constituencies in the district won by UPA	0.34	0.34		
Ratio of UPA votes to non-UPA votes	0.90	1.97		
Share of seats reserved for SC/ST	0.35	0.31		
Proportion of electorate who voted (relative to eligible population)	0.67	0.14		
Years elapsed since previous election	3.1	1.35		
Election coincides with survey year (1=Yes)	0.31			

Source: Appendix 1 (Section 8.1) for data sources and description. Some of the district level variables are used as explanatory variables in Model 1 and 1a.

Table 1.4: The Discouraged Worker: Household level analysis Second stage results of IV Probit and IV Linear Probability Model(Model 1 and 1a)

Dependent variable: Seeking work	Probit (estimated average marginal effects, with standard errors in parentheses, clustered at district level)				Linear Probability Model (With robust standard errors)			
Sample & Specification (□) Variables of interest (□)	(a) All households	(b) All households (with rain-delay interaction)	(c)Poor households	(d) Poor households (with rain-delay interaction)	(e) All household s	(f) All households (with rain-delay interaction)	(g) Poor households	(h) Poor households (with rain-delay interaction)
Lagged Rationing Rate (2009-10)	-0.357**	-0.341**	-0.394**	-0.382*	-0.891***	-0.922***	-0.853***	-0.920***
	(0.142)	(0.137)	(0.174)	(0.220)	(0.051)	(0.055)	(0.086)	(0.125)
Proportion of payments with over 90 days delay in 2009-10	0.00315	-0.0249	0.000032	-0.0302	-0.043**	-0.040**	-0.029	-0.038
	(0.0652)	(0.0597)	(0.0711)	(0.0875)	(0.018)	(0.016)	(0.028)	(0.036)
Average delay in wage payments interacted with the absolute value of negative deviation in rainfall (in standard deviation units) in 2009-10		-0.0434		-0.0458		-0.020		-0.055
		(0.0725)		(0.138)		(0.023)		(0.067)
Number of observations	47131	47131	15476	15476	47131	47131	15476	15476
Wald chi-squared test of exogeneity	Chi-2(2) 4.9*	Chi-2(3) 16.08***	Chi-2(2) 3.73	Chi-2(3) 7.63*				
Underidentification test: Kleinberg Papp rank LM statistic					414.11***	735.444***	153.819***	98.328***
Weak instrument test: Kleinberg Papp Wald F statistic					203.024	257.696	81.878	16.186
Stock and Yogo critical values					7.03 (10% maximal IV size)	Not available	7.03 (10% maximal IV size)	Not available

Notes: For coefficients on the probit regression and for the full set of regression results and for the Linear Probability Model with diagnostics for IV, please see Appendix Tables 2 & 3 (Section 8.1) respectively. Poor households refer to households whose Monthly per capita expenditure (MPCE) is below the official state poverty line. These regressions are therefore for the subsample of poor households. For marginal effects, standard errors in parentheses and for linear model, t-statistic in parentheses. $^{*}p< 0.10$, $^{**}p< 0.05$, $^{***}p< 0.01$. The instrument validity tests are conducted for the LPM model and reported here. Instruments: For the IV delay in payments and rationing rate have been instrumented with Proportion of block level MGNREGA positions vacant and the percentage change in bank branches over the two years preceding 2009-10 for (a), (c), (e) and (g) and when rain-delay interaction is included this is instrumented with Number of block level MGNREGA staff per village for (b), (d), (f), (h)

Table 1.5: The Discouraged Worker Effect: District level analysis (Model 2 and Model 3)

	Least Squares		Two stage least squares (Model 2)		Control function approach (Model 3)	
Dependent variable: Change in district level demand rate	**Full sample**	**Sample with no missing data**	**Full sample**	**Sample with no missing data**	**Full sample**	**Sample with no missing data**
Lagged Rationing Rate (2009-10)	-0.257***	-0.242***	-0.400***	-0.334***	-0.234***	-0.223
	(0.02)	(0.03)	(0.07)	(0.07)	(0.08)	(0.14)
Proportion of payments with over 90 days delay in 2009-10	-1.358**	-1.817***	0.077	1.924	-0.028	0.374
	(0.56)	(0.61)	(2.32)	(1.64)	(1.32)	(1.91)
Average delay in wage payments interacted with the absolute value of negative deviation in rainfall (in standard deviation units) in 2009-10	-24.629***	-29.524***	-8.251	-33.446***	-20.391**	-33.280**
	(7.21)	(8.30)	(20.13)	(10.30)	(9.91)	(14.05)
Demand Rate (2009-10)	-0.638***	-0.685***	-0.618***	-0.764***	-0.464***	-0.626***
	(0.03)	(0.05)	(0.05)	(0.08)	(0.09)	(0.15)
Predicted error term from the control function					-0.145*	0.032
					(0.09)	(0.14)
Number of Observations	550	283	551	284	522	267
R-squared	0.630	0.681			0.608	0.643

Notes: For the full set of regression results and other diagnostics, and for alternate estimations using Two Stage Least Squares and Control Function, please see Appendix Table 8, Appendix 3 (Section 8.1) . Tables 1.1 and1. 2 respectively. Variables used in the 3SLS and control function as controls for the endogenous variables include the following: Lagged rationing rate: A set of variables including proportion of block staff vacant, the number of block level staff per village, block and per job card, bank branches per job card, GDD; for proportion of payments with over 90 days delay in 2009-10, delayed interacted with absolute value of negative deviation in rainfall in 2009-10 we use block level vacancy and block staff per village and expansion of bank branches; and for demand rate in 2009-10 we use GDD variables, village level functionaries per job card and per village and program expenditures per job card and per village.

Table 1.6: Correlates of rationing and pro-poor rationing: district level panel data (Model 4,5,6,7)

Dependent variable: Rationing /Weighted rationing rate at the district level	All (Model 4)		Poor subpopulation (Model 5)		Weighted by proportion below poverty line (Model 6)		Weighted by inverse monthly per capita expenditure (Model 7)	
	Naive	IV Second Stage	Naive	IV Second Stage	Naive	IV Second Stage	Naive	IV Second Stage
Demand Rate	0.378***	-1.024	0.184**	-0.420	0.195***	-0.368	0.277**	-0.959
	(0.080)	(0.760)	(0.078)	(0.305)	(0.041)	(0.314)	(0.131)	(0.835)
Whether UPA won any seat in the district (1=Yes)	0.064	0.090	0.089	0.113	0.013	0.024	0.101	0.134
	(0.055)	(0.081)	(0.060)	(0.070)	(0.022)	(0.031)	(0.071)	(0.093)
Proportion of constituencies under UPA	-0.100	-0.150	-0.148*	-0.194**	-0.042	-0.062	-0.120	-0.170
	(0.075)	(0.107)	(0.083)	(0.096)	(0.032)	(0.042)	(0.096)	(0.121)
Share of UPA votes	-0.009	-0.012**	0.001	-0.001	-0.007***	-0.008*	-0.010	-0.012
	(0.008)	(0.005)	(0.012)	(0.011)	(0.002)	(0.004)	(0.012)	(0.008)
Years since election	0.018**	0.023**	0.020**	0.021**	0.014***	0.016***	0.017*	0.019*
	(0.007)	(0.010)	(0.008)	(0.008)	(0.004)	(0.004)	(0.010)	(0.011)
Survey year is an election year (1=Yes)	0.002	0.058	0.019	0.042*	0.021**	0.044**	-0.017	0.026
	(0.019)	(0.038)	(0.020)	(0.024)	(0.009)	(0.017)	(0.027)	(0.042)
Polling percentage (divided by 10)	0.017	0.053	0.011	0.018	-0.013	0.001	-0.004	0.020
	(0.040)	(0.057)	(0.037)	(0.043)	(0.019)	(0.025)	(0.052)	(0.065)
Proportion of seats reserved for SC/ST candidates	0.157	0.029	0.298	0.189	0.135	0.084	0.074	-0.054
	(0.189)	(0.211)	(0.217)	(0.225)	(0.107)	(0.105)	(0.296)	(0.299)
Monthly per capita expenditure (INR `000)	-0.086	-0.211**	-0.025	-0.046	-0.109***	-0.159***	-0.085	-0.198
	(0.055)	(0.098)	(0.059)	(0.067)	(0.023)	(0.041)	(0.090)	(0.122)
Absolute value of negative deviation of rainfall	0.041***	0.068***	0.047***	0.062***	0.018***	0.029***	0.044***	0.067***
	(0.006)	(0.016)	(0.006)	(0.010)	(0.003)	(0.007)	(0.008)	(0.017)
Positive deviation of rainfall	0.006	0.006	0.006	0.006	0.001	0.002	0.003	0.003
	(0.004)	(0.005)	(0.004)	(0.004)	(0.002)	(0.002)	(0.006)	(0.006)
Proportion of targets achieved in sanitation	0.019	0.237	-0.012	0.109	0.037	0.124*	0.135	0.316
	(0.106)	(0.173)	(0.092)	(0.114)	(0.037)	(0.068)	(0.151)	(0.202)
Number of commercial bank branches (`000s)	-0.022***	-0.038***	-0.024***	-0.031***	0.001	-0.005	-0.028***	-0.042***
	(0.001)	(0.012)	(0.006)	(0.008)	(0.003)	(0.005)	(0.009)	(0.015)
Number of job cards (`00,000)	-3.272	-4.691	-3.371	-4.856*	-1.084	-1.654	-1.592	-2.880
	(2.033)	(2.881)	(2.222)	(2.509)	(0.977)	(1.158)	(2.744)	(3.322)
Constant	0.116		0.009		0.048		0.411	
	(0.301)		(0.285)		(0.140)		(0.419)	
N	1054	1026	1030	984	1054	1026	1072	1060
Test of endogeneity		5.59***		4.56**		4.195***		2.919*
Underidentification test: Kleiberg Paap rank LM statistic		9.20***		19.82***		9.20***		10.8***
Weak instrument test: Cragg-Donald Wald F statistic		9.1		28.18		9.103		10.55
KleibergPaap Wald F statistic		9.65		26.95		9.648		11.21
Stock and Yogo critical values		8.96 (15% maximal IV size)		16.38 (10% maximal IV size)		8.96 (15% maximal IV size)		8.96 (15% maximal IV size)

See Appendix 1 (Section 8.1) for details on computation of the weighted rationing rate. In all the IV regressions we use as the instrument for demand rate the number of days that the temperature was above the optimal threshold for the dominant crop in the district over the cropping season for that year.

3. Welfare and poverty impacts of India's national rural employment guarantee scheme: Evidence from Andhra Pradesh

Klaus Deininger and Yanyan Liu[67]

3.1 Introduction

Persistent high rates of extreme poverty and gender inequality, together with increased frequency of natural and man-made disasters, have increased policymakers' interest in public works programs as a form of productive safety nets. The ability to set wages in a way that is self-targeting and fosters gender equality, combined with the opportunity to construct physical infrastructure that can enhance growth and wages in the long term, makes such programs very attractive compared to available alternatives. At the same time, however, there has been concern that implementing these programs successfully carries high administrative requirements and that where these controls are not in place, large amounts of resources may be wasted or end up lining the pockets of local officials.

With a budget of US$7.8 billion in 2011/12 alone, India's National Rural Employment Guarantee Scheme (NREGS) is one of the largest programs of this kind globally.[68] It guarantees employment for up to 100 days per fiscal year at wages that are equal for men and women, thus serving not only as an insurance substitute but also having the potential to enhance female empowerment. Implementation, at least in some states, also includes some innovative features, such as making all project-related data available on the internet, directly depositing payments into beneficiaries' accounts, and regular *social audits* which have uncovered significant fraud within the program.[69] While this suggests that NREGS could herald a new generation of such programs, the implementation has been controversial.

Supporters point to awareness and participation rates, especially by females (greater than 50 percent), that are significantly above those in earlier or comparable programs, as well as anecdotal evidence suggesting that the program has made clear contributions to decentralization, transparency of political processes, and female empowerment (Dreze and Khera 2011; Khera and Nayak 2009). Critics note the program's high cost, inefficiency in transferring resources, and serious corruption (Niehaus and Sukhtankar 2014). They point out that high program wages not only cause leakage and corruption that may undermine program impacts but, instead of helping people move out of agriculture, may in fact encourage return migration to rural areas. To better understand whether these arguments are justified, empirical analysis of NREGS impacts is needed.

Beyond a large body of descriptive and case study evidence, most quantitative studies of NREGS have focused on general equilibrium impacts through price and wage effects, using the program's phased roll-out to identify treatment effects based on repeated cross-sections or

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[68] Since 2009, NREGS is referred to as Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS). We use the earlier name throughout the paper.

[69] Social audits are audits conducted with active involvement by primary stakeholders. They resulted in discovery of frauds on a significant scale. Some officials embezzled money by "creating fake muster rolls, inflated bills, exaggerated measurements, and non-existent works, all through bribes and cuts from wage seekers". (http://125.22.8.66/SocialAudit/).

administrative data (Azzam 2012; Imbert and Papp 2014; Zimmerman 2014). While this approach has provided important insights, it is not well suited to identifying the channels through which program effects materialize, and assessing behavioral responses to determine whether, for example, the program crowds out other forms of income source. Studies on partial equilibrium effects of NREGS on participating households can provide insights regarding these issues. However, such studies have been scant. Ravi and Engler (2015) and Afridi, Mukhopadhyay, and Sahoo 2016 are probably the only two empirical papers that rigorously examine NREGS effects on participating households. Ravi and Engler (2015) looks at the effects on food security, financial inclusion and health. However, this study relies on a selective sample of poor, women-headed households in one district in Andhra Pradesh and contaminated baseline (which was conducted one year after NREGS started). Afridi, Mukhopadhyay, and Sahoo (2016) looked at female participation on children's education outcomes.

This paper adds to the literature on partial equilibrium impacts of NREGS on direct beneficiaries. We investigate NREGS impacts on key welfare indicators including consumption expenditure, nutritional intake, and asset accumulation as well as the channels through which the impacts materialize. NREGS participation can, in principle, yield at least two types of direct benefits. First, a transfer effect will directly increase income by either paying higher wages than those received in the market or providing employment at times when there is no demand from other sources. Second, some of the income gained can be channeled toward savings and investment to strengthen households' resilience in the longer term, an effect that may be enhanced if wages are deposited into a savings account.

Using a three-round panel of some 4,000 households in the Indian state of Andhra Pradesh, combined with administrative data on household participation in NREGS. The fact that the data were collected in 2004, before NREGS had been conceived; in 2006, when the implementation was just starting; and in 2008, when the program was fully operational throughout the state, together with the program's phased roll-out, allows us to distinguish short- and medium-term effects. We use the 2006/2008 (before/after) household panel data to assess program effects on the treated, to answer some of the policy issues raised in this context. The two pre-intervention rounds (2004/2006) allow us to test the parallel trend assumption of our difference-in-difference (DID) strategy.

We find positive effects of NREGS on nutritional intake in the short term and accumulation of nonfinancial assets in the medium terms. Benefits are concentrated within poor, scheduled castes and tribes, and those relying on casual labor. The effects are materialized through higher income from casual labor. We find no evidence yet of NREGS having enhanced incentives for on-farm investment.

The paper is structured as follows. Section 3.2 describes key features of NREGS, its implementation in Andhra Pradesh, and evidence on its impact from the literature. Section 3.3 introduces administrative and household survey data, descriptive statistics, and our methodology. Section 3.4 presents estimates of program impacts in the short and medium term, heterogeneity of impacts by caste and labor market participation status, and impact pathways via labor markets and land-related investments. Section 3.5 concludes by drawing out implications for policy and further research.

3.2 Program Nature and Existing Evidence

Although NREGS is a flagship program for India's government, states—which by law are responsible for implementing it—diverged widely in their approaches and levels of success, with some states using innovative ways to increase transparency and accountability and ultimately the program's welfare impacts, while other states are called out for serious flaws in program implementation (Bhatia and Drèze 2006; Liu and Barrett 2013). A number of features make Andhra Pradesh a model in this area. Still, partly due to data limitations, evaluations of direct program effects are scant, often unable to fully appreciate the heterogeneity of impacts.

3.2.1 Program Design and Implementation

Following passage of the Mahatma Gandhi National Rural Employment Guarantee Act in 2005, the NREGS was rolled out across all of India's rural areas, proceeding in three phases from the poorest to more affluent districts in February 2006, April 2007, and April 2008. Program expenditure increased from the equivalent of US$2.1 billion in 2006/07 to US$8.9 billion in 2010/11, providing payment for nearly three billion workdays. While responsibility for the allocation of funds to specific projects lies with the states, the central government budget covers 100 percent of wage and 75 percent of nonwage expenditures.

Building on lessons from a long tradition of food-for-work schemes (Dutta et al. 2012b; Subbarao 1997), NREGS features three important innovations (Khera and Nayak 2009). First, it establishes a legal right for households to be employed for up to 100 days per year; in fact, individuals who apply but do not receive work within a period of two weeks are entitled to unemployment compensation. Second, the minimum wage rate, set at the state level, applies both to males and females, making the program particularly attractive to women, who normally receive significantly lower wages than men (Deininger, Jin, and Nagarajan 2013). Amenities such as crèches, which by law must be provided at the work sites to encourage women's participation, are also expected to reduce gender discrimination. Third, to improve the productive capacity of rural areas in the long term and thus make the program sustainable, there is a desire to focus work on productive infrastructure such as irrigation systems, minor roads, and land improvement.

To participate in NREGS, rural households first need to be registered at the local *gram panchayat*, which results in the issuance of a job card and entry of the applicants' names into a register of all job seekers called the *muster roll*.[70] Once work has been performed, workers are to be paid within a period of two weeks or less. In practice, these regulations are not always followed, and performance varies enormously across states (Comptroller and Auditor General of India 2008). Reviews of the program found that many job seekers were unable to obtain the desired level of work, at least initially (Dutta et al. 2012b). Local decision-makers were found to use NREGS strategically to maximize rents (Niehaus and Sukhtankar 2014), consistent with the wide variation in quality and transparency of implementation across the country.

[70] As per the program regulations, the job card, which must contain photographs of all the adult members of the household, is to be delivered to applicants free of charge within 15 days of application. In principle, once a household has a job card, that household is expected to indicate demand for work (less than or equal to 100 days) under NREGS for the following year. Based on household demand as ratified by the village meeting (*gram sabha*), a work plan at the *gram panchayat* is elaborated and submitted upward for consolidation. Projects are sanctioned at the district level, and the *gram panchayats* are responsible for the allocation of work among job seekers. In practice, the process is often more top-down, based on central budget allocations, and even information about available projects is not always available to job seekers.

Different from other states, Field Assistants (hired by the state government) rather than locally elected leaders (gram panchayats) play a central role in implementation and supervision of the program in Andhra Pradesh (Maiorano 2014). The responsibilities taken by Field Assistants include preparation of a list of projects to be undertaken, supervision of ongoing projects, identification of potential interested workers, assignment of these workers to specific work sites, and management of financial flows.

Andhra Pradesh is considered one of a few star performers in terms of the quality of program implementation (Dreze and Khera 2011; Liu and Barrett 2013) for several reasons. An emphasis on promoting self-help groups among poor women that started in the late 1990s is likely to have allowed Andhra Pradesh a head start in implementing NREGS. Strong self-help group coverage, a federated structure, and various efforts to promote convergence with local government (Deininger and Liu 2013a, 2013b) allowed quick mobilization of the target group once NREGS became effective. Importantly, all self-help groups had elaborated "livelihood plans" to identify opportunities for small-scale labor-intensive investment, providing a starting point for a list of projects to be implemented under the program.

On top of existing institutional advantages in implementing NREGS, the state of Andhra Pradesh took distinct measures to hold officials more accountable (Aiyar and Samji 2009), in response to a predecessor program that was marred by high levels of corruption (Deshingkar and Johnson 2003). First, key program information (muster rolls, lists of work performed and wages paid) is made available online for access by the public, making it easy to trace participants, work sites, and payments. Second, bank accounts were opened for all participants, and modern payment systems are used to reduce fraud and transaction costs while simultaneously encouraging saving. By ensuring that payments are made to the individuals who did the work, the use of such payment systems has, according to some observers, improved female empowerment (Johnson 2008). Finally, to quickly identify deviations from the rules and punish responsible officials, Strategy and Performance Innovation Unit (SPIU) under the state's Rural Development Department was established in 2006 to conduct social audits involving a wide range of stakeholders regularly in all the state's Gram Panchayats.[71]

Despite the recognized sound performance of NREGS in AP, it is well-documented that NREGS work is rationed administratively, similar to many other states in India (Narayanan and Das 2014; Das 2015). Liu and Barrett (2013) find that 25 percent of households in AP sought but could not obtain a MGNREGS job in 2009-10. Ravi and Engler (2015) estimate rationing rates in extensive margin (i.e., demanded a job but not offered one) of 43 percent in 2007 and 21 percent in 2009 for Medak district in AP. This "supply driven" approach to work availability in AP is also discussed in Maiorano (2014) and Sheahan et al. (2014). The actual rationing rate is likely to be higher because individuals who wish to participate in NREGS may be discouraged from job seeking because of widespread rationings (Narayanan et al. 2016).

3.2.2 Approaches to and Evidence from NREGS Evaluations

In light of the program's size and importance, a large literature aims to assess the impacts of NREGS. Descriptive evidence suggests that the quality of program implementation varied across states (Liu and Barrett 2013) but that the program seems to have allowed households to

[71] The responsibility for conducting social audits of MGNREGA projects was transferred to the Society for Social Audits, Accountability and Transparency (SSAAT) in May 2009. See Afridi and Iversen (2014) for more details on the procedure and findings of social audits in Andhra Pradesh.

mitigate the impacts of consumption shocks due to variations in rainfall (Johnson 2009). As it provides a larger relative wage increase for females than for males, it is not too surprising to see positive program impacts on females at the descriptive level, with knock-on effects on their offspring (Dev 2011).

While the program targets the poor, significant rationing remains (Dutta et al. 2012a), so that some benefits may be captured by elites (Niehaus and Sukhtankar 2014), reducing the program's effectiveness in transferring resources to the poor (Shankar, Gaiha, and Jha 2011). This is consistent with the finding that access to information significantly affected poor people's ability to benefit from the program (Jha, Bhattacharyya, and Gaiha 2011b), and the presence of a positive association between landholding and NREGS (that is, less poverty targeting) (Jha et al. 2009).

Establishing an appropriate counterfactual is the key challenge for any impact assessment and, in this case, it is particularly difficult now that the program operates nationally and prior phasing in was not random but instead gave preference to poorer districts. A growing number of studies use the phasing in of the program to assess district-level impacts of the program, often relying on repeated cross-sections of National Sample Surveys (NSSs). To the extent that the underlying assumptions are justified, this would provide an estimate of the *intention to treat* effect of the program on wages or employment at the district level.

Using this approach, Imbert and Papp (2015) find that the program provided direct and indirect gains (via general equilibrium effects) of similar magnitude. They find that the quality of implementation varies significantly across states, as indicated by the fact that the estimate of program effects almost doubles (to 9 percent) in the states with the best implementation performance. Increases were focused on low-wage, low-skilled public employment; in fact, wages for better-paying jobs decreased. Seasonality in wage labor demand was important: the average daily earnings of casual laborers increased by 4.5 percent during the dry season but were unaffected in the rainy season. Their results imply that program-induced wage increases redistribute income from net buyers to net suppliers of labor but that the impact on labor force participation remains limited. Using the same framework, Azam (2012) finds that increases in female wages are larger than increases in male wages. Wages for female casual workers were estimated to have increased 8 percent more in NREGS districts as compared to non-NREGS districts.

Using a regression discontinuity approach based on an index by the Planning Commission that defines eligibility for the program by ranking districts by poverty, Zimmermann (2012) finds that NREGS has had limited impact on male wages and levels of employment but some effects on females, with wage impacts concentrated in the agricultural off-season. Kloner and Oldiges (2012) construct a nationwide district panel (188 in phase 1 and 103 in phase 2) using NSS consumption data from 2005 and 2008 to assess the program's impact on poverty gaps and consumption patterns. This study finds positive effects on welfare, especially for scheduled castes and tribes, and nonfood spending (Kloner and Oldiges 2012).[72] Because NSS data lack information on wages in agriculture, Berg et al. (2012) uses administrative data on gender-specific wages for agricultural and unskilled tasks (at the district level) for more direct inference of agricultural wage gaps, which are most relevant for the poor. Results from this analysis

[72] Both the Planning Commission's "backwardness" index and the intensity of implementation (as measured by the number of days actually worked) are used to control for this.

suggest that the program affects unskilled wages but leaves the gender wage gap unaffected (Berg et al. 2012).

Beyond possible general equilibrium effects, impacts on participating households can help identify ways in which benefits from NREGS participation materialize. Such analyses request information on households' actual participation status which is lacking in the NSS surveys. Relying on a small household panel dataset in one district in Andhra Pradesh, Ravi and Engler (2013) compare participants to households that were denied access and find that NREGS had large impacts on food, and nonfood per capita expenditure (found to have increased by 9.6 percent, and 23 percent, respectively). NREGS participation is estimated to have increased the likelihood of a household having a savings account by 21 percent and total savings by Rs. 19 (Ravi and Engler 2013). However, interpreting the results as NREGS impacts is problematic, because the baseline survey was conducted in 2007, one year after the program became available. The credibility of the results also depends on the assumption that non-random initial rationing of nonparticipants (Dutta et al. 2014) was not correlated with subsequent changes in outcomes. Afridi, Mukhopadhyay, and Sahoo (2016) relies on a large sample focuses on education outcomes instead and finds that higher female participation in NREGS (instrumented by *mandal*-level rainfall shocks and lagged NREGS fund sanctioned) increased children's time spent in school and led to better grade progression.

This study adds to the still small literature on partial equilibrium effects of NREGS on direct beneficiaries. Similar to Ravi and Engler (2013) we also look at welfare and poverty effects of NREGS, relying on a much larger data set covering five districts in AP. However, the abundance of our data allows for investigation of heterogeneous effects of NREGS on households who differ by phase, caste, and occupation, to better understand the mechanisms through which the effects were realized. In addition, the availability of two pre-intervention rounds allows us to test for parallel trend assumption to support our empirical strategy of DID.

3.3 Data

To assess program impacts, we combine panel household survey data from before and after the program became available, together with administrative data on participation, using difference-in-difference (DID) estimates together with propensity score matching. The phased introduction of the program allows us to distinguish short-term from medium-term effects, overall as well as for subgroups in the population.

3.3.1 Data Sources and Variable Construction

We combine a three-round panel household survey with administrative data on NREGS participation, and the Indian Population Census and Indian Village Amenities Census of 2001. The household survey includes information on some 4,000 households in 480 villages from five districts in Andhra Pradesh that were interviewed in 2004, 2006, and 2008.[73] This allows us to

[73] Villages were randomly selected in these districts, and then households in these villages. The number of sampled households is 4,759 in 2004, 4,693 in 2006, and 4,533 in 2008. The attrition rate is 3.1 percent

use the 2004 and 2006 household survey rounds as preprogram baselines to obtain DID and DDD estimates of program effects.[74] Moreover, three of the sample districts were covered by NREGS in 2006 under phase 1, so the 2008 survey data allow us to draw inferences regarding medium-term effects of NREGS. The remaining districts were included in phases 2 and 3, which will indicate short-term program effects.

The household survey includes information on demographic status, spending on food and nonfood items,[75] asset endowments, income from casual labor,[76] and investments in land improvement. Survey information plus a qualitative exercise was used to assign households by poverty status either to the poorest of the poor, the poor, the not-so-poor or the non-poor.[77] Inclusion of the NREGS job card number in the 2008 survey provides a link between our household-level data and administrative records on program participation. Administrative data, available online, include job card information for all wage-seeking (registered) households; muster roll information such as wage rate, total workdays, and payments for each worker; and characteristics and completion status of all NREGS work.

The household data allow us to use changes in nutritional intake as a measure of short-term program effects and asset endowments to capture medium-term effects. We measure nutritional intake by multiplying physical quantities of the more than 30 food items in the questionnaire's consumption section with their caloric and protein content based on India's main reference to compute calories and protein consumed (Gopalan, Rama Shastri, and Balasubramanian 2004).[78] Nonfinancial assets include consumer durables, equipment, and livestock.[79] Consumption and assets are in per capita terms based on adult equivalent measures throughout.[80]

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from 2004 to 2006 and 3.4 percent from 2006 to 2008. We have a total of 4,460 panel households across the three rounds.

[74] Although the 2006 round was collected from August to October, shortly after the launch of NREGS at the start of 2006, contamination in 2006 is minimal, as only 29 of the 2,467 sample households in Phase 1 districts actually worked under NREGS when surveyed in 2006. These 29 households are dropped from the sample.

[75] Although the survey instrument is less disaggregated than that of the NSS, it follows the overall structure used there.

[76] NREGS income is included in the casual labor income. However, we cannot distinguish NREGS income from other casual labor income.

[77] The manual used in the process defines the poorest of the poor as those who can eat only when they get work and who lack shelter, proper clothing, social respect, and means to send their children to school. The poor have no land, live on daily wages, and need to send school-age children to work in times of crisis. The not-so-poor have some land, have proper shelter, send their children to public schools, are recognized in society, and have access to bank credit as well as public services. The non-poor have more than 5 acres of land; have no problem obtaining food, shelter, and clothing; can hire laborers, send their children to private schools, use private hospitals, and lend rather than borrow money; and have considerable social status.

[78] For fruits or vegetables where the survey includes only aggregate spending, we use the 55th round of the NSS to derive the price and caloric content of a representative basket of these consumed in Andhra Pradesh.

[79] Asset values were measured as of December 2003 in the 2004 survey, as of June 2006 in the 2006 survey, and as of June 2008 in the 2008 survey. Financial assets were excluded due to concerns about misreporting.

[80] The adult equivalent measures for caloric and protein consumption are obtained using nutritional requirements by sex and age as weights, that is, weights are 1.2 for adult males, 0.9 for adult females, 1.0 for adolescents (12 to 21 years), 0.8 for children aged 9 to 12, 0.7 for children aged 7 to 9, 0.6 for children aged 5 to 7, 0.5 for children aged 3 to 5, and 0.4 for children younger than 3 (Gopalan, Rami

Finally, we have information on casual labor income (including income from NREGS) for each member in the household over the past year. We also know if, during the last three years (i.e. since the last survey), a range of land-related investments[81] were undertaken on the household's land and whether, in the case of a positive response, NREGS had contributed to such activity.

Indian Population Census and Indian Village Amenities Census of 2001 provide village level information on the population in terms of caste, literacy, and primary occupation as well as the status of available amenities including roads, medical facilities, and agricultural credit societies.

3.3.2 Descriptive Statistics

Table 3.1 summarizes the evolution of access to job cards and NREGS participation by program phase and household poverty status. In phase 1 districts, about 50% households overall have received job cards by 2008. The job card holders account for 55 percent among the poorest and the poor households, as well as 44 percent and 30 percent among not-so-poor and non-poor households, respectively. Rates of job card issuance overall were, at 36 percent and 40 percent, slightly lower in phase 2 and phase 3 districts, respectively, but there was greater emphasis on the poor, especially in phase 3 districts, where about 43 percent of the poorest held job cards, compared to 15 percent of nonpoor households. However, holding a job card does not necessarily mean that a household participates in NREGS employment. Participant households are those with at least one member participating in NREGS. Actual participation was, at 41 percent of the total (46 percent of the poorest and the poor) and some 50 days supplied by the average household in 2008, higher in phase 1 than in phase 2 (30 percent) and phase 3 districts (19 percent).

In line with program regulations, we do not find significant differences in wages between males and females. It is thus not surprising to see that female levels of participation are higher than those of males: female labor accounts for 63 percent in phase 1, 60 percent in phase 2, and 50 percent in phase 3 districts in 2008 of the total NREGS labor. Not surprisingly, the average total NREGS payment to participant households is much higher in Phase 1 districts than that in Phase 2 and 3 districts in 2008 (Rs 4103 in Phase 1 versus Rs 1540 in Phase 2 and Rs 955 in Phase 3).

Table 3.2 summarizes household welfare indicators in phase 1 districts by participation status in 2004, 2006, and 2008. NREGS participants had lower consumption, assets, and energy and protein intakes than nonparticipants in each of the three years. Table 3.3 reports sample means of household characteristics for NREGS participants and nonparticipants by phases and results from logit regressions of NREGS participation in Phase 1 districts and Phases 2 and 3 districts, clustered at the village level. These results suggest higher participation by the poor, scheduled castes and tribes, casual laborers, and those with lower initial consumption. Literacy, male headship, and holding a leadership position in the village are associated with higher participation levels in Phase 1 districts. While this suggests pro-poor

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Shastri, and Balasubramanian 2004). For income and overall consumption, we assign the weight of 0.78 for anyone older than 60 or younger than 14.

[81] These investment activities include silt application, borewell, land leveling/terracing, establishing orchard, bunding for soil and water conservation, deepening well, installing dug well, cleaning bushes and other vegetation, etc.

targeting, village leaders are likely to affect the allocation of work, and a lack of program awareness by illiterate households seems to constrain participation in phase 1 villages.

Table 3.4 reports sample means of selected village level variables from Population Census and Indian Village Amenities Census of 2001 by phase. Compared with Phase 1 villages, the sample villages in the Phases 2 and 3 have poorer access to medical facility, higher population density, and higher proportion of scheduled caste/tribe population and agricultural laborers.

3.4 Analytical Approach

3.4.1 Average Treatment Effects

We seek to estimate the average treatment effect on the treated (ATET) of NREGS on welfare indicators including consumption expenditure, nutritional intake, and asset accumulation. We define the treated group as households that had at least one member working under NREGS. The challenge is to construct a valid control group from NREGS non-participating households, we need to consider the selection issue from both the supply side and the demand side. On the demand side, households participated in NREGS voluntarily, partially based on their unobserved characteristics. On the supply side, NREGS demonstrates a “rigid top-down” nature in Andhra Pradesh and the allocation of NREGS fund from the state level to the local does not reflect the demand from the local (Maiorano 2014). Therefore, a household in a community with more NREGS fund is more likely to participate in NREGS than one in a community with less fund, even if they are identical otherwise. The widespread administrating rationing (unmet demand) as explained earlier may have considerably dampened the self-selection problem of NREGS participation. Given it is difficult to identify valid instrumental variables, we rely on DID combined with propensity score matching (PSM), to provide estimates of NREGS impacts on these beneficiaries.

To illustrate our empirical approach, let *t* = 0, 1, 2 indicate year 2004, 2006, and 2008, respectively. Let $T_{it} = 1$ if a household *i* is treated at *t*, and $T_{it} = 0$ otherwise. With Y_{it}^{T} as the outcome under treatment and Y_{it}^{C} the counterfactual outcome, the gain from being treated is $(Y_{i2}^{T} - Y_{i2}^{C})$. Our interest is in the average effect of treatment on the treated (ATET), $E(Y_2^T - Y_2^C \mid T_2 = 1)$, that is, the expected difference between actual and counterfactual outcomes, Y_2^T and Y_2^C, for treated households ($T_2 = 1$). Since Y_2^C is unobservable, we cannot estimate ATT directly.

DID estimates, $E(Y_2 - Y_1 \mid T_2 = 1) - E(Y_2 - Y_1 \mid T_2 = 0)$, provide an unbiased estimate of ATT if the parallel trend assumption, $E(Y_2^C - Y_1 \mid T_2 = 1) = E(Y_2 - Y_1 \mid T_2 = 0)$, holds. Defining the selection bias at *t* as $B_t = E(Y_t^C \mid T_2 = 1) - E(Y_t^C \mid T_2 = 0)$, the parallel trend assumption is equivalent to $B_1 = B_2$, or selection bias being constant in 2006 and 2008. This condition will not hold if household characteristics or initial conditions affect subsequent changes of the outcome variables so that their distributions in the treatment and control groups differ from each other.

Combining DID with PSM can address the bias from observables and time-invariant unobservables but not time-variant unobservables. Access to two rounds of pre-intervention data allows us to test whether the parallel trend assumption holds for 2004–2006. The null hypothesis is $E(Y_1 - Y_0 \mid T_2 = 1) = E(Y_1 - Y_0 \mid T_2 = 0)$, or $B_0 = B_1$. The rationale is that if the selection bias was constant in 2004 and 2006, we can be confident that it was also constant in 2006 and 2008.

To match participants, we use a propensity score (PS)-matched kernel method, which estimates $\left[\sum_{D_i=1}(Y_i - \sum_{D_j=0} W_{ij} Y_j)\right] / N_1$, where N_1 is the number of treated households, W_{ij} is the weight corresponding to households *i* (treated) and *j* (untreated), and $W_{ij} = G[(P(X_j) - P(X_i))/b_n] / \left[\sum_{D_k=0} G[(P(X_k) - P(X_i))/b_n]\right]$, where $G(.)$ is a kernel function and b_n is a bandwidth parameter. $P(X)$ is the propensity score, defined as $P(X) = \Pr(D = 1 \mid X)$, where *X* is a vector of observables. We use bootstrapping with 500 replications to estimate the standard errors for the PS-matched kernel method. We choose the PS-matched kernel method instead of the more commonly used nearest-neighbor matching to obtain valid bootstrapped standard errors (Abadie and Imbens 2006a, 2006b). We also trim off the observations with a PS lower than 0.1 or higher than 0.9, following Crump et al. (2009).

3.4.2 Impact pathways

To understand the impact pathways, we estimate NREGS impacts on participating households by phase, caste, poverty status, and occupation. We interpret the impacts on participants in Phase 1 districts as medium-term effects and impacts on participants in Phase 2 and 3 districts as short-term effects. We expect NREGS to have more pronounced welfare and poverty effects on marginalized and poor participating households as they rely on fewer income-generating opportunities. Because direct program effects transmitted through casual labor markets, we expect participating households with members primarily engaged in casual labor in the initial period to benefit more from NREGS.

To further explore the impact pathways, we investigate the extent to which NREGS participation affects casual labor income and investment to enhance land productivity. We use the 2006 and 2008 panel data to estimate

$$\Delta y_{i,t=2} = \beta_0 + \beta_1 NREGS_{i,t=2} + x^0_{i,t=1}\beta_2 + u_{i,t=2}, \qquad (2)$$

where $\Delta y_{i,t=2}$ is the change in casual labor income or the change in land investment between 2006 and 2008 for household *i*;[82] $NREGS_{i,t=2}$ is an indicator variable that equals 1 if the household participated in NREGS between July 2007 and June 2008, the reference period for casual labor income in the 2008 survey, and 0 otherwise; $x^0_{i,t=1}$ is a vector of control variables including

[82] The casual labor income in our survey includes NREGS income.

caste, poverty category, literacy, female headship, household size, number of adults, land holdings, and district of residence;[83]; and $u_{i,t=2}$ is a random error term. Our parameter of interest is β_1 which measures the effect of NREGS on casual labor income or land investment.

To examine the plausibility of the DID identification, we estimate

$$\Delta y_{i,t=1} = \gamma_0 + \gamma_1 NREGS_{i,t=2} + x^0_{i,t=0}\gamma_2 + u_{i,t=1}, \tag{3}$$

where $\Delta y_{i,t=2}$ is the change in casual labor income or the change in land investment between 2004 and 2006; $x^0_{i,t=0}$ includes the same set of control variables as in equation (2) but take the 2004 values. We estimate equation (3) as a falsification test and expect $NREGS_{i,t=2}$ to be insignificant if the DID estimation equation (2) is valid.

3.5 Empirical Results

3.5.1 Estimates of Program Impacts

Results from DID estimates with and without PSM for medium-term (in phase 1 districts) and short-term (in phase 2 and 3 districts) impacts are in the two top and bottom panels of Table 3.5. Our outcome variables include total value of consumption, caloric and energy intake, and nonfinancial assets in logs, providing a rough estimate of the percentage change in the outcomes of interest. [84] We estimate the PS at the household level based on logit regressions in which the dependent variable takes one for participating households and zero otherwise, and the explanatory variables include the initial outcome variable, household level variables summarized in Table 3.3 and village level variables in Table 3.4. The 2006–2008 data provide an estimate of program impacts, while 2004–2006 data serve as a test for the assumption of parallel trends. For 2004–2006, DID rejected the parallel trend assumption, suggesting pre-program changes in consumption and nutritional intake were lower for participants than for nonparticipants. In contract, after matching and trimming, DID plus PSM fail to reject this assumption at any conventional significance level for all outcomes and for both Phase 1 households and Phases 2 and 3 households, consistent with the notion that participation of NREGS is not mainly determined by self-selection.

For the medium term (Phase 1 districts), DID plus PSM results from 2006-2008 data point toward a positively significant impact (17 percent increase) on accumulation of nonfinancial assets. In the short term (Phase 2 and 3 districts in the bottom panels), DID plus PSM suggest positive effects of NREGS participation on energy and protein intake, an increase of 6.8 and 6.7 percent points respectively. Our results are consistent with the notion that most immediate program impacts involve improving nutrition, as suggested by others (Jha, Bhattacharyya, and Gaiha 2011a), possibly followed by asset accumulation in the medium term.

To explore whether NREGS disproportionally benefits the marginalized, we repeat the above analysis for scheduled castes and tribes compared to others. Table 3.6 reports the DID plus PSM results. The results that pass the test for parallel trends are bolded hereafter. We find that significant medium-term effects on accumulation of nonfinancial asset emerge for scheduled

[83] We did not difference the control variables because they are mostly unchanged from 2006 to 2008.

[84] In addition to being more robust to local inflation, using logs pulls in outliers and changes the distribution of outcomes so as to give poorer households greater weight.

castes and tribes but not for other castes (Column 1). Similarly, in the short term, the results point toward higher levels of growth in consumption, energy, and protein intake due to NREGS benefits that are exclusively concentrated among scheduled casts and tribes (Column 2).

Table 3.7 reports the estimated effects from DID plus PSM on the poorer households (classified as Poorest of the Poor or Poor) versus the less poor (classified as Not So Poor or Not Poor). The results are consistent with those reported in Table 3.6 as expected: the welfare and poverty reduction effects are more pronounced for the poorer households than the less poor.

The same analysis is repeated for the households with and without members primarily engaged in casual labor in the initial period (Table 3.8). We find positively significant impacts of NREGS participation for households relying on casual labor but not for thee households without a primary casual laborer. Throughout Tables 3.6-3.8, asset accumulation is significant in the medium term and nutritional intakes are significant in the short term, consistent with the results in Table 3.5.

3.5.2 Impact Pathways through Labor Markets and Investment in Land Improvement

Table 3.9 reports the effects of NREGS participation on total casual income at the household level, for the female members in the household, and for male members, respectively. Results from equation (2) are in columns 1, 3, and 5 and results from equation (3) are in columns 2, 4, and 6. We note that NREGS income is included in the total casual labor income. NREGS participation led to a significant increase in casual labor income overall and for male and female participants separately, with estimated magnitudes of Rs. 3,304, 1,797, and 1,522 for total, female, and male casual labor income, respectively. In all cases, the falsification tests using pre-program income levels support the parallel trend assumption. Administrative data put mean NREGS-induced transfers to program participants in the July 2007 to June 2008 period at Rs. 3,340 per household, close to the increase in total casual labor income estimated here (Rs. 3,304).

The fact that our survey includes information on investment in land improvement allows us to explore the extent to which NREGS helped increase agricultural investment. If increased labor income from NREGS or complementarities with investments on common land prompt NREGS participants to increase land-related investment, higher levels of land productivity could be one channel for income and thus consumption expenditure and nutritional intake to increase. We estimate equation (2) using as a dependent variable the change in land investment on households' own land between 2006 and 2008 for the total sample and subsamples of scheduled castes and tribes and other castes. The results are reported in columns 1, 3, and 5 of Table 3.10. We also estimate equation (3) as a falsification test using 2004 and 2006 data and report the results in columns 2, 4, and 6. The results in Table 3.10 suggest that NREGS participation has no effects on the propensity to make land-related investment. Therefore, we conclude that NREGS-related welfare effects on participants arise from increased labor income but that higher incomes do not increase participants' propensity of investment in land improvement.

3.6 Conclusion and Implications

Our study complements a large literature on general NREGS impacts by exploring effects on participants in Andhra Pradesh, an Indian state with a good implementation record. Methodologically, two rounds of pre-program data allow the test for parallel trends assumption of double differences and the use of an identification strategy that combines double difference estimates with propensity score matching. Substantively, we contribute to the literature in a number of ways. First, we show that short-term direct impacts on energy and protein intake differ from the more general investment impacts observed in the medium term. Second, we find that direct impacts are almost exclusively concentrated within scheduled casts and tribes, the poorer, and those with members relying primarily on the casual labor market. Third, we find that the program effects are materialized through increased casual labor income but find no evidence that investment in land improvement is a potential pathway.

While our findings suggest that NREGS is well targeted and has significant impacts, Andhra Pradesh is generally considered to be one of the better-performing states in terms of NREGS implementation. Using similar pre- and post-program data at the household level to extend the analysis to other states where implementation is much weaker could allow researchers not only to measure the aggregate impact of the program but also to better understand the impact of specific implementation arrangements (for example, social audits or electronic funds transfers), an area that would be of great interest to policymakers.

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3.8 Tables

Table 3.1: Summary of actual MGNREGS participation by household poverty status

	Phase 1			Phase 2		Phase 3
	2006	**2007**	**2008**	**2007**	**2008**	**2008**
Having a job card						
All households	0.438	0.492	0.503	0.322	0.356	0.399
Poorest of poor households	0.492	0.537	0.547	0.295	0.338	0.429
Poor households	0.493	0.540	0.554	0.422	0.463	0.470
Not-so-poor households	0.350	0.427	0.436	0.252	0.273	0.293
Nonpoor households	0.233	0.288	0.302	0.266	0.269	0.146
Participation in MGNREGS work						
All households	0.049	0.328	0.414	0.035	0.298	0.188
Poorest of poor households	0.056	0.381	0.457	0.038	0.286	0.211
Poor households	0.045	0.355	0.460	0.047	0.382	0.226
Not-so-poor households	0.052	0.260	0.356	0.021	0.230	0.128
Nonpoor households	0.060	0.056	0.102	0.051	0.077	0.000
Number of observations	2,397	2,397	2,397	838	838	751
Female participation (% of time)						
All households	0.536	0.590	0.631	--	0.599	0.503
Poorest of poor households	0.446	0.607	0.650	--	0.632	0.520
Poor households	0.597	0.580	0.606	--	0.586	0.415
Not-so-poor households	0.655	0.595	0.633	--	0.639	0.650
Nonpoor households	0.403	0.456	0.633	--	0.411	0.444
Female wages received (Rs/day)						
All households	84	79	81	--	52	84
Poorest of poor households	78	80	82	--	53	87
Poor households	82	79	81	--	54	78
Not-so-poor households	99	79	82	--	48	84
Nonpoor households	36	78	76	--	47	91
Male wages received (Rs/day)						
All households	80	79	81	--	43	83
Poorest of poor households	85	82	82	--	46	83
Poor households	77	76	81	--	47	83
Not-so-poor households	77	78	80	--	34	80
Nonpoor households	67	79	79	--	32	75
Total amount received per household (Rs)						
All households	796	2,623	4,103	1,907	1,540	995
Poorest of poor households	710	2,674	4,335	2,045	1,558	1,065
Poor households	717	2,665	4,182	1,728	1,703	962
Not-so-poor households	973	2,360	3,687	1,887	1,314	925
Nonpoor households	1,342	2,849	3,027	2,480	1,147	639
Number of observations	117	787	993	29	250	141

Source: Authors' computation from household survey and administrative data.
Note: -- not reported due to small number of observations.

Table 3.2: Means of household outcomes by participation status and year, phase 1 and phase 2 & 3 districts

	2004 Participant		2006 Participant		2008 Participant	
	No	Yes	No	Yes	No	Yes
Phase 1 districts						
Consumption per capita (Rs/year)	7,401	6,576	9,972	8,549	13,125	12,533
Energy intake per capita (Kcal/day)	2,014	2,004	2,446	2,246	2,542	2,444
Protein intake per capita (g/day)	45	44	50	47	53	51
Total nonfinancial assets per capita (Rs/year)	3,092	2,140	4,446	3,038	6,499	5,601
Number of households	1,383	1,008	1,344	998	1,410	1,017
Phase 2 and 3 districts						
Consumption per capita (Rs/year)	6,835	5,854	9,177	8,297	10,114	9,374
Energy intake per capita (Kcal/day)	2,070	1,991	2,324	2,239	2,365	2,338
Protein intake per capita (g/day)	46	44	48	45	49	48
Total nonfinancial assets per capita (Rs/year)	2,818	1,688	2,900	2,633	4,097	3,786
Number of households	1,329	435	1,297	430	1,345	439

Source: Authors' computation from household survey and administrative data.

Table 3.3: Summary statistics and logit regression of NREGS participation, using 2006 data

	Sample Means				Logit Regressions			
	Phase 1		Phases 2 &3		Phase 1		Phases 2 & 3	
	Part.	Nonp.	Part.	Nonp.	Coeff.	t-stat.	Coeff.	t-stat.
Household lives in hamlet	0.34	0.36	0.39	0.30	-0.0591	(-0.61)	0.341**	(2.66)
Household is poorest of poor	0.43	0.36	0.40	0.39	0.402**	(3.09)	0.0410	(0.23)
Household is poor	0.33	0.26	0.38	0.29	0.490***	(3.86)	0.253	(1.48)
Primary occupation is casual labor	0.72	0.56	0.77	0.60	0.432***	(4.09)	0.568***	(3.91)
Household is scheduled caste	0.34	0.20	0.33	0.14	0.251*	(2.16)	0.978***	(5.84)
Household is scheduled tribe	0.09	0.08	0.28	0.24	0.0280	(0.16)	0.604***	(3.66)
Household is other caste	0.13	0.27	0.13	0.24	-0.309*	(-2.23)	-0.104	(-0.54)
Somebody can write	0.81	0.76	0.75	0.72	0.377**	(3.00)	0.146	(0.94)
Household size	4.27	4.03	4.09	3.93	0.00304	(0.09)	-0.00276	(-0.06)
Head female	0.08	0.12	0.14	0.20	-0.602***	(-3.71)	-0.270	(-1.46)
Leader in village committee or self-help group	0.13	0.10	0.10	0.07	0.330*	(2.30)	0.425	(1.95)
Consumption per capita (Rs/year)	8,549	9,972	8,297	9,177	-0.0000257*	(-1.98)	-0.00000096	(-0.06)
Nonfinancial assets per capita (Rs)	3,038	4,446	2,633	2,900	-0.00000547	(-0.75)	0.0000057	(0.57)
Energy intake per capita (Kcal/day)	2,246	2,446	2,239	2,324	-0.000197	(-1.08)	0.0000320	(0.16)
Protein intake per capita (g/day)	46.66	50.03	45.20	47.67	0.00791	(0.81)	-0.00500	(-0.47)
Number of observations	998	1,344	340	1,295	2,342		1,635	
Pseudo R-squared					0.076		0.083	

Source: Authors' computation from household survey and administrative data.
Notes: *t* statistics are in parentheses. Significance level: *: 10%, **: 5%, ***: 1%.

Table 3.4: Sample means of village characteristics for Phase 1 districts and Phase 2 & 3 Districts

	Phase 1	Phase 2 & 3	difference
Access to medical facility in village (=1)	0.76	0.63	***
Availability of agricultural credit societies in village (=1)	0.25	0.19	
Paved approach road to village (=1)	0.83	0.85	
Distance to nearest town from village (km)	39.79	41.45	
Population density (households per hectare) in village	0.65	0.85	**
Percent of village population that is SC/ST	0.28	0.37	***
Percent of village population that is illiterate	0.53	0.55	
Percent of village population that is a cultivator	0.16	0.14	*
Percent of village population that is an agricultural laborer	0.14	0.17	**
Percent of village population that is a marginal worker	0.13	0.13	
Percent of village population that does not work	0.47	0.48	
Number of observations	271	176	

Source: Authors' computation from the Indian Population Census and Indian Village Amenities Census of 2001
Notes: t statistics are in parentheses. Significance level: *: 10%, **: 5%, ***: 1%.

Table 3.5: Double difference estimates of impacts on program participation

Outcomes	(1) DD			(2) DD and PSM		
			PHASE 1 DISTRICTS			
Estimated impact: 2006 and 2008 panel						
Consumption (log)	0.078	(0.022)	***	**0.033**	**(0.024)**	
Energy intake (log)	0.025	(0.021)		**0.004**	**(0.023)**	
Protein intake (log)	0.019	(0.018)		**0.006**	**(0.020)**	
Nonfinancial assets (log)	0.355	(0.062)	***	**0.171**	**(0.068)**	**
Number of observations	991+1321=2312			928+1197=2125		
Test for parallel trends assumption: 2004 and 2006 panel						
Consumption (log)	-0.027	(0.023)		-0.01	(0.024)	
Energy intake (log)	-0.074	(0.021)	***	-0.023	(0.022)	
Protein intake (log)	-0.045	(0.019)	**	-0.012	(0.021)	
Nonfinancial assets (log)	-0.054	(0.071)		-0.076	(0.077)	
Number of observations	991+1321=2312			949+1243=2192		
			PHASE 2 and 3 DISTRICTS			
Estimated impact: 2006 and 2008 panel						
Consumption (log)	0.013	(0.034)		**0.050**	**(0.039)**	
Energy intake (log)	0.027	(0.029)		**0.068**	**(0.034)**	**
Protein intake (log)	0.038	(0.027)		**0.067**	**(0.030)**	**
Nonfinancial assets (log)	0.147	(0.134)		**0.076**	**(0.164)**	
Number of observations	427+1274=1701			390+1133=1523		
Test for parallel trends assumption: 2004 and 2006 panel						
Consumption (log)	0.046	(0.033)		-0.033	(0.037)	
Energy intake (log)	-0.009	(0.027)		-0.03	(0.029)	
Protein intake (log)	-0.014	(0.026)		-0.038	(0.029)	
Nonfinancial assets (log)	0.298	(0.128)	**	0.108	(0.169)	
Number of observations	427	1274	1701	397	1168	1565

Source: Authors' computation from household survey and administrative data.
Notes: All figures in per capita terms. As explained in the text, the estimates in the lower panel test the parallel trend assumption. DD = double difference estimation, PSM = propensity score matching. Robust standard errors are in parentheses. Significance level: *: 10%, **: 5%, ***: 1%.

Table 3.6: Estimates of program participation impacts on scheduled castes and tribes versus other castes

	(1)			(2)		
Outcomes	**Phase 1 Districts**			**Phase 2 and 3 Districts**		
Scheduled castes and tribes						
Consumption (log)	**0.008**	**(0.044)**		**0.097**	**(0.047)**	**
Energy intake (log)	-0.011	(0.040)		**0.102**	**(0.042)**	**
Protein intake (log)	-0.016	(0.034)		**0.084**	**(0.041)**	**
Nonfinancial assets (log)	**0.219**	**(0.126)**	*	**0.003**	**(0.182)**	
Number of observations	393+327=720			242+421=663		
Other castes						
Consumption (log)	**-0.011**	**(0.029)**		**-0.072**	**(0.064)**	
Energy intake (log)	**0.024**	**(0.026)**		**-0.009**	**(0.053)**	
Protein intake (log)	**0.029**	**(0.024)**		**0.019**	**(0.050)**	
Nonfinancial assets (log)	**0.021**	**(0.102)**		**0.167**	**(0.246)**	
Number of observations	543+902=1445			148+712=860		

Source: Authors' computation from household survey and administrative data.
Notes: All figures in per capita terms. TD = triple difference estimation; PSM = propensity score matching. Robust standard errors are in parentheses. Significant level: *: 10%, **: 5%, ***: 1%.

Table 3.7: Estimates of NREGS participation impacts for poor and non-poor households

Outcomes	(1) Phase 1 Districts			(2) Phase 2 and 3 Districts		
Households classified as Poorest of the Poor or Poor						
Consumption (log)	**0.037**	**(0.027)**		**0.090**	**(0.045)**	**
Energy intake (log)	**-0.001**	**(0.026)**		**0.104**	**(0.041)**	**
Protein intake (log)	**-0.002**	**(0.024)**		**0.101**	**(0.037)**	***
Nonfinancial assets (log)	**0.162**	**(0.079)**	**	**0.166**	**(0.166)**	
Number of observations	713+741=1454			304+763=1067		
Households classified as Not So Poor or Not Poor						
Consumption (log)	**0.035**	**(0.043)**		**-0.138**	**(0.079)**	*
Energy intake (log)	**0.025**	**(0.035)**		**0.004**	**(0.099)**	
Protein intake (log)	**0.035**	**(0.033)**		**-0.012**	**(0.073)**	
Nonfinancial assets (log)	**0.166**	**(0.114)**		**-0.222**	**(0.204)**	
Number of observations	215+456=671			86+370=456		

Source: Authors' computation from household survey and administrative data.
Notes: All figures in per capita terms. TD = triple difference estimation; PSM = propensity score matching. Robust standard errors are in parentheses. Significance level: *: 10%, **: 5%, ***: 1%.

Table 3.8: Estimates of NREGS participation impacts for households with and without primary casual laborers

	(1) PHASE 1 DISTRICTS			(2) PHASE 2 and 3 DISTRICTS		
Household with a primary casual laborer						
Consumption (log)	**0.060**	**(0.029)**	**	**0.057**	**(0.040)**	
Energy intake (log)	**0.023**	**(0.028)**		**0.080**	**(0.038)**	**
Protein intake (log)	**0.019**	**(0.025)**		**0.077**	**(0.036)**	**
Nonfinancial assets (log)	**0.191**	**(0.088)**	**	**0.115**	**(0.151)**	
Number of observations	675+676=1351			301+697=998		
Household with no primary casual laborer						
Consumption (log)	**-0.033**	**(0.043)**		**-0.027**	**(0.082)**	
Energy intake (log)	**-0.049**	**(0.034)**		**0.05**	**(0.074)**	
Protein intake (log)	**-0.023**	**(0.029)**		**0.043**	**(0.068)**	
Nonfinancial assets (log)	**0.117**	**(0.116)**		**-0.142**	**(0.260)**	
Number of observations	253+521=774			89+436=525		

Source: Authors' computation from household survey and administrative data.
Notes: All figures in per capita terms. TD = triple difference estimation; PSM = propensity score matching. Robust standard errors are in parentheses. Significance level: *: 10%, **: 5%, ***: 1%.

Table 3.9: Double difference regression to assess impact of participation on casual labor income

	All individuals		Females		Males	
	2006/08	2004/06	2006/08	2004/06	2006/08	2004/06
	(1)	(2)	(3)	(4)	(5)	(6)
Household participated	**3,304.4*** **	**-47.68**	**1,796.9*** **	**56.16**	**1,522.0** **	**-212.9**
in NREGS in 2008	(4.53)	(-0.11)	(5.71)	(0.29)	(2.55)	(-0.55)
Household located in	-683.3	631.8	-209.4	90.09	-748.5	655.9
hamlet	(-0.99)	(0.96)	(-0.71)	(0.36)	(-1.24)	(1.10)
Being very poor	4,167.5***	-1,141.1**	1,265.4***	79.56	3,213.7***	-1,241.2***
	(5.18)	(-2.03)	(3.72)	(0.34)	(4.70)	(-2.60)
Being poor	2,731.3***	-248.9	967.9***	263.7	1,950.8***	-582.5
	(3.72)	(-0.48)	(2.94)	(1.24)	(3.11)	(-1.31)
Scheduled tribe	79.91	1,835.3***	164.1	624.0**	165.3	1,194.0**
	(0.09)	(3.04)	(0.39)	(2.25)	(0.21)	(2.44)
Scheduled caste	267.4	-1,098.4	67.10	-280.8	98.24	-983.0*
	(0.25)	(-1.55)	(0.16)	(-0.90)	(0.11)	(-1.79)
Nonbackward caste	-2,693.2***	-318.5	-1,166.7***	32.48	-2,010.8***	-305.6
	(-3.33)	(-0.59)	(-3.31)	(0.13)	(-2.89)	(-0.67)
If any member can write	915.2	-1,187.3**	-226.3	-456.5**	945.0	-1,013.2**
	(1.05)	(-2.30)	(-0.56)	(-1.98)	(1.33)	(-2.32)
Female headed	-2,151.3**	1,400.2*	-556.3	506.9	738.6	1,543.5*
	(-2.48)	(1.92)	(-1.27)	(1.46)	(0.71)	(1.78)
Number of female adults	392.1	-1,215.1***	658.8**	-468.4**	-108.7	-820.6**
	(0.70)	(-2.79)	(2.19)	(-2.31)	(-0.22)	(-2.22)
Number of male adults	-752.0	527.3	-417.2**	132.8	-511.8	299.0
	(-1.48)	(1.42)	(-2.04)	(1.00)	(-1.07)	(0.91)
Household size	2,251.1***	1,932.4***	706.7***	810.8***	1,369.1*	1,036.3**
	(2.64)	(3.49)	(2.65)	(3.37)	(1.69)	(2.34)
Household size squared	-178.4**	-164.6***	-52.28**	-70.06***	-112.2	-87.02*
	(-2.10)	(-2.86)	(-2.31)	(-2.83)	(-1.40)	(-1.95)
Amount of irrigated	-386.3**	176.8	-283.4***	104.1**	-133.3	88.61
land owned (ac.)	(-2.58)	(1.47)	(-4.57)	(2.16)	(-1.04)	(0.88)
Amount of rainfed land	-346.0***	45.82	-133.5**	4.388	-227.4**	42.64
owned (ac.)	(-2.61)	(0.32)	(-2.25)	(0.08)	(-2.14)	(0.38)
Total land owned	3.965***	-3.279	1.710***	-0.899	2.449**	-2.424
squared	(2.82)	(-0.96)	(2.75)	(-0.64)	(2.18)	(-0.89)
District dummies	YES	YES	YES	YES	YES	YES
Number of observations	3,621	3,711	3,468	3,632	3,195	3,358

Source: Authors' computation from household survey and administrative data.
Notes: t statistics are in parentheses. Significance level: *: 10%, **: 5%, ***: 1%.

Table 3.10: Double difference regression of private land investment effects of NREGS participation

	All households		Scheduled Castes and Tribes		Other Castes	
	2006/08	**2004/06**	**2006/08**	**2004/06**	**2006/08**	**2004/06**
If household participated in NREGS	**0.00799**	**-0.00701**	**0.0179**	**-0.000785**	**0.00237**	**-0.0151**
	(0.51)	(-0.40)	(0.90)	(-0.03)	(0.11)	(-0.63)
If household located in hamlet	-0.0334*	-0.00497	-0.0309	-0.0215	-0.0352	0.00858
	(-1.95)	(-0.23)	(-1.36)	(-0.67)	(-1.51)	(0.30)
Being very poor	-0.0112	-0.0515**	-0.00834	-0.0826*	-0.0259	-0.0423
	(-0.65)	(-2.18)	(-0.21)	(-1.84)	(-1.22)	(-1.44)
Being poor	-0.00765	-0.0327	-0.0231	-0.0501	-0.00108	-0.0327
	(-0.44)	(-1.57)	(-0.56)	(-1.17)	(-0.05)	(-1.31)
Scheduled tribe	0.00796	0.00105	-0.0226	-0.0323		
	(0.50)	(0.05)	(-0.91)	(-1.05)		
Scheduled caste	0.00750	0.0219				
	(0.29)	(0.60)				
Nonbackward caste	-0.0322	-0.0388			-0.0350	-0.0390
	(-1.50)	(-1.52)			(-1.62)	(-1.51)
If any member can write	-0.0126	0.0211	-0.000995	0.0126	-0.0174	0.0284
	(-0.76)	(1.12)	(-0.04)	(0.48)	(-0.86)	(1.07)
Female headed	0.00827	0.0845***	-0.0119	0.0801**	0.0231	0.0922***
	(0.45)	(3.72)	(-0.40)	(2.07)	(1.09)	(3.32)
Number of female adults	0.00280	-0.0211	0.00936	-0.0218	-0.000432	-0.0186
	(0.22)	(-1.30)	(0.42)	(-0.91)	(-0.03)	(-0.90)
Number of male adults	-0.00296	0.0121	-0.00189	0.0452**	-0.00385	-0.00654
	(-0.29)	(0.96)	(-0.12)	(2.20)	(-0.30)	(-0.42)
Household size	-0.00914	-0.0596***	-0.0303	-0.0591***	-0.00229	-0.0511**
	(-0.61)	(-3.73)	(-1.14)	(-2.61)	(-0.13)	(-2.44)
Household size squared	0.00149	0.00544***	0.00373	0.00550**	0.000651	0.00441**
	(1.03)	(3.47)	(1.38)	(2.56)	(0.39)	(2.19)
Irrigated area of land owned	-0.0168***	-0.0362***	-0.0326	-0.0723***	-0.0135**	-0.0320***
	(-2.81)	(-4.68)	(-1.32)	(-2.85)	(-2.13)	(-3.93)
Nonirrigated area of land owned	-0.0182***	-0.0259***	-0.00554	-0.0340**	-0.0192***	-0.0262***
	(-2.92)	(-3.21)	(-0.41)	(-1.99)	(-2.73)	(-2.80)
Square term of total land owned	0.000141**	0.000949***	0.0000589	0.00309***	0.0000816	0.000866***
	(1.98)	(3.50)	(0.45)	(3.29)	(1.14)	(3.01)
District dummies and constant term	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3,390	3,547	1,291	1,338	2,099	2,209

Source: Authors' computation from household survey and administrative data.
Notes: Robust standard errors are in parentheses. Significance level: *: 10%, **: 5%, ***: 1%.

4. General Equilibrium Impacts of National Rural Employment Guarantee Scheme on Well-being, Poverty Reduction, and Female Social Capital and Empowerment

Yanyan Liu

4.1 Introduction

India's National Rural Employment Guarantee Scheme (NREGS) is one of the largest social safety net programs in the world. This scheme, launched in 2006 and then gradually expanded to cover the entire nation, guarantees employment for up to 100 days per year to per rural household at the statutory minimum wage. The stated objective of NREGS is to reduce poverty. This objective is expected to be realized through at least three channels: income transfers to direct participants of NREGS, improved infrastructure, and increased wage and/or employment.

This study looks at the general equilibrium effects of NREGS on households' wellbeing, poverty reduction, and female social capital and empowerment. The empirical literature on NREGS effects has focused on wage and labor market participation (Azam 2012; Imbert and Papp 2015; Zimmermann 2012) and education outcomes for children (Afridi, Mukhopadhyay, Sahoo 2013; Islam and Sivasankaran 2015; Mani et al. 2014). On effects on wellbeing and poverty reduction, the related studies are Deininger and Liu (2013), Ravi and Engler (2015), Klonner and Oldiges (2014), and Bose (2017). Deininger and Liu (2013) find that NREGS participation led to higher nutritional intake and asset accumulation. Ravi and Engler (2015) find larger NREGS effects on food and nonfood per capita expenditure. Both studies look at the partial equilibrium effects on welfare and poverty reduction for direct beneficiaries of NREGS. Klonner and Oldiges (2014) and Bose (2017) both examine the general equilibrium effects of NREGS on consumption expenditure, using National Sample Survey (NSS), and find positive significant effects especially for lean season and for marginalized population. On female social capital and women empowerment, the closest study is Amaral et al. (2015) which uses district-level police-reported crimes and finds that NREGS increases violence against women.

This study adds to the literature on NREGS effects through two aspects. First, it examines the general equilibrium effects of NREGS on nutritional intake, in addition to consumption expenditure. Nutritional intake is imputed based on the quantities of a variety of food items and is thus exempt from the possible bias due to location-specific inflation induced by NREGS. Second, I look at female social capital and empowerment based on a series of self-reported indicators. Female social capital is measured by the self-reported level of trust in different social groups. The measure of female empowerment is based on whether a woman can set aside money for her own use; go to different locations without asking permission from her husband or other males in the family; level of respect received from other family members; and whether ill-treated or beaten by male family members. The panel data allows for a consistent measurement of changes in these subjective measures for the same household (likely the same woman who is the spouse of the household head). To my knowledge, no previous study has empirically investigated the effects of NREGS on similar subjective measures of social capital and empowerment.

I use a three-round household panel data collected in 2004, 2006, and 2008 in Andhra Pradesh and estimate intention to treat (ITT) effects (that is, average treatment effects (ATE) on the households who live in the program districts) as well as effects on non-participants living in the treated districts. The identification strategy exploits variation in the timing of NREGS implementation across districts. I use difference in difference (DID) plus propensity score matching (PSM) to look at the NREGS effects between phases. The availability of the two pre-intervention data allows for tests for the parallel trend assumption. The results suggest that NREGS has positive effects on consumption expenditure and nutritional intake for households in program areas and for non-participating households as well. The welfare effects are more pronounced for poor households. The findings are consistent with the existing studies. The results also suggest NREGS significantly increased female social capital after one-year implementation and increased female empowerment after two-year implementation.

Sections 3.2 and 3.3 provide detailed description of NREGS program and the data. This study is organized as follows. Section 4.2 describes empirical method. Sections 4.3 and 4.4 presents results on wellbeing and poverty reduction and on female social capital and empowerment, respectively. Section 4.5 concludes.

4.2 Methodology

I aim to estimate the intention to treat estimator because the general equilibrium effects of NREGS (e.g., increased agricultural wage) tend to affect both direct and indirect beneficiaries in the program areas. Our identification strategy exploits the variation in the timing of NREGS implementation across districts. Because the 2008 survey interviewed both households who have access to NREGS (households in Phase 1 and Phase 2 districts) and those who cannot access NREGS yet (households in Phase 3 district). Intuitively I can compare outcomes for households in the Phase 1 and 2 districts with those who live in the Phase 3 district, and attribute the difference to the program impacts. Although households in the non-program areas may also be affected by NREGS through spillover effects (e.g., the higher wage rate in program districts may attract migrant worker from non-program districts) which can cause underestimation of the actual program impacts, I expect the bias to be small because of low migration rate across districts in India.

A direct comparison between households in different phases is likely to be biased due to non-random rollout of NREGS. The growth rate may also change by phase. Thus a simple difference in the outcomes would yield an estimate with upward bias. A DID estimator can mitigate time-invariant bias but cannot handle the difference in growth path due to different initial conditions. To identify actual program impacts, I resort to methodologies that make these two groups of households more comparable. I use PSM in combination with DID to mitigate the potential bias. The availability of two pre-NREGS data (2004 and 2006) allows for test for the parallel trend assumption of the DID identification.

In the empirical implementation, I use a PS-weighted regression method (Hirano, Imbens and Ridder 2003), which produces an estimate of the average treatment effects (ATEs) as the parameter, β, in a weighted least square regression using the data from 2006 and 2008 rounds as below:

$$y_{it} - y_{i,t-1} = \alpha + \beta D_i + \varepsilon_{it}, \tag{1}$$

where i indexes households and t indexes rounds; y_{it} is a welfare measure for household i in round t; D_i indicates location in the treatment districts; ε_{it} is a random error term. Let $\hat{P}(x_{t,t-1})$ denote the estimated propensity score (PS) based on logit regression of D_i on $x_{t,t-1}$, a vector of matching variables that are not affected by NREGS. The weights in equation (1) equal $1/\hat{P}(x_{t,t-1})$ for households in treated districts (that is, $D_i = 1$) and $1/[1-\hat{P}(x_{t,t-1})]$ for households in control districts. I also trim off the observations with a PS lower than 0.1 or higher than 0.9, following Crump et al. (2009). We estimate equation (1) using the 2004 and 2006 rounds to test for the parallel trends assumption.

Besides the ITT estimator, I also estimate the effects of NREGS on non-participating households by restricting the sample to NREGS nonparticipants only. This estimate measures the effects of NREGS realized through general equilibrium (spillover) channels such as increased market wage and improved local infrastructure.

4.3 Results on wellbeing and poverty reduction

The economic outcome variables I look at include total value of consumption, caloric and energy intake, and nonfinancial assets in logs, providing a rough estimate of the percentage change in the outcomes of interest.[85] I estimate the PS at the household level based on logit regressions in which the dependent variable takes one for participating households and zero otherwise, and the explanatory variables include household controls (including the initial outcome variable, whether a household lived in a hamlet, poverty status, primary occupation, castes, literacy, female headship, and leadership in a village committee or self-help group) and village controls (including access to medical facility in village, availability of agricultural credit societies in village, availability of paved approach road to village, distance to nearest town, population density, percent of village population that is scheduled caste/tribe (SC/ST), percent population that is a cultivator, percent population that is an agricultural laborer, percent population that is a marginal worker, and percent of population that does not work).

I use Phase 3 households as control and Phase 1 and Phase 2 as treatment respectively. However I fail to pass the parallel trend tests, even after reweighting and trimming based on PS. This result suggests that the Phase 3 households may not form a suitable control group. I then use Phase 1 households as treatment and Phase 2 as control. I fail to reject the parallel trend tests for the all the outcome variables at any conventional significance levels, as shown in the lower panel of Table 4.1. This is consistent with the notion that the Phase 2 district (Nellore) in our sample is predicted by the algorithm in Zimmerman (2013) to receive NREGA in the first implementation phase and so are the three Phase 1 districts in our sample.[86]

In the remaining of this section, the analyses are based on this strategy: using Phase 1 households as treatment group and Phase 2 households as control. The control group is contaminated in the sense that Phase 2 households started to access NREGS in 2007 while most of the outcomes are measured in 2008. From Table 3.1, we note that NREGS participation is considerably lower in both the extensive margin and the intensive margin. The participation rate of Phase 1 households is 41.4% and the average total amount received per

[85] In addition to being more robust to local inflation, using logs pulls in outliers and changes the distribution of outcomes so as to give poorer households greater weight.

[86] Personal communication.

participating household is 4,103 rupees, in comparison with 29.8% participation and 1,540 rupees per participating household in Phase 2 district. This empirical strategy allows us to identify the "treatment effect" as the difference in impact between two years and one year of program exposure. Unless the one year exposure to NREGS incurred a loss in the program areas (which is unlikely), the estimate will be a lower bound of true program impacts after two years' exposure.

The upper panel of Table 4.1 presents the impacts on all households who live in the program areas. We compute the DID and DID plus PSM estimates between Phase 1 and Phase 2 households. The lower panel reports the results of parallel trend tests. After weighting and trimming, I fail to reject the parallel trend hypotheses, suggesting that we are able to form a comparable control group. DID plus PSM results (Column 2 of upper panel in Table 4.1) point to statistically significant NREGS impacts on consumption expenditure and energy and protein intake for households in treated districts. The magnitudes of the impacts are not trivial: 24.6 percent increase in consumption expenditure, 12.5 percent increase in energy intake, and 12.8 percent increase in protein intake.

I next drop the direct beneficiaries of NREGS in the sample and estimate the impact of NREGS on non-participating households. The results are reported in Table 4.2. Still we have statistically significant estimates of impacts on consumption expenditure and energy and protein intake. The Magnitudes of the estimated impacts are relatively close to those in the ITT estimation in Table 4.1, suggesting the existence of large spillover effects of NREGS. These results are consistent with the simulation results in Imbert and Papp (2015).

The simulation results in Imbert and Papp (2015) suggest that poor households benefit more from NREGS than the nonpoor. I next examine the impacts for the poor and non-poor households separately. The poverty status is assigned based on the state's 2001 "below poverty line" census which is routinely used to determine eligibility for government programs) complemented by an effort of "participatory identification of the poor" that added vulnerability and social exclusion to quantitative census indicators. The resulting lists, which assigned all households to the poorest of the poor, poor, not so poor, or non-poor, were confirmed by village assemblies.[87] I group the "poorest of the poor" and the "poor" to the category of "poor" and the "not so poor" and "non-poor" to the category of "non-poor". Table 4.3 reports the estimated impacts on the poor households (column 1) and nonpoor households (column 2). The upper panel reports the ITT estimation and the lower panel reports the estimation for non-participating households only. The poor households benefited from higher consumption expenditure, energy intake and protein intake, while the nonpoor benefit from higher consumption expenditure and protein intake but not energy intake. The point estimate of impact on protein intake is lower for nonpoor than the poor. The results are reasonable as the nonpoor likely have already fulfilled their requirement for energy intake. Although the nutritional impacts are lower for the nonpoor than the poor, these two groups have similar impacts on consumption expenditure, different from the prediction of Imbert and Papp (2015).

[87] The manual used in the process defines POP as those who can eat only when they get work and who lack shelter, proper clothing, respect in society, and cannot send their children to school; The poor have no land, live on daily wages, and need to send school going children to work in times of crisis. The "not so poor" have some land, proper shelter, send their children to public schools, are recognized in society, and have access to bank credit as well as public services. The non-poor, having land of at least 5 acres, no problem for food, shelter, clothing, can hire laborers, send children to private schools, use private hospitals, lend rather than borrow money, and have considerable social status.

The estimates for non-participating households show qualitatively consistent and quantitatively close results, compared with the ITT estimation.

To further explore the underlying mechanism of NREGS effects, I further estimate the NREGS impacts for ST/SC versus non-ST/SC households and for households with at least one primary casual laborer versus households without any primary casual laborers. The results are reported in Tables 4.4 and 4.5, respectively. Households in each of the four categories benefited from higher consumption expenditure and nutritional intake. However, we do not see large difference in magnitudes of these impacts between different groups.

4.4 Results on female social capital and empowerment

I generate an index of female social capital as the mean of 13 variables indicating level of trust in individuals of the same or different caste or religion from within or outside the village as well as in government officials and police, all on a 1–5 scale. The index of female empowerment is generated as the mean of 10 binary variables indicating whether a woman can set aside money for her own use; go to the market, clinic, or community center; visit friends; or work in fields outside the village without asking permission from her husband or other males in the family; whether a woman receives high respect by other family members; whether a woman is never beaten or ill-treated by her husband.

I use Phase 3 households as control and Phase 1 and Phase 2 as treatment respectively to examine the medium-term and short-term effects of NREGS. I estimate NREGS effects using the 2006/2008 panel and test for parallel trends using the 2004/2006 panel. The ITT results for DID alone and DID plus PSM are reported in Column 1 and Colum 2, respectively, of Table 4.6. The set of variables used for matching are the same as those described in section 4.3. Different from the DID method, the DID plus PSM method passes the parallel trends test, suggesting that, after trimming and reweighting, the control group formed from households in the Phase 3 district is comparable to the treatment group. The DID plus PSM results point to a significant positive effect on NREGS on both the social capital index and the empowerment index in medium term. While the estimated effects on both indices are positive in short-term, only the effect on social capital index is statistically significant. These findings suggest that female social capital increases immediately after NREGS implementation, while the economic empowerment improvement takes longer to materialize. This is not surprising: trust level is determined only by females' own perception; empowerment is determined by the interactions between family members thus the changes of female empowerment tend to be more difficult than female social capital.

I next separately estimate the effects of NREGS for scheduled caste/tribe and for other castes. The results presented in Table 4.7 suggest that for marginalized households (scheduled caste/tribe), NREGS effect on female empowerment only materialize in the medium term but not in short term. In contrast, females belonging to other castes tend to benefit from NREGS more quickly. Table 4.8 presents the estimated effects of NREGS for poor households and non-poor households separately. The findings are similar the those in Table 4.7: the non-poor households tend to benefit from NREGS more quickly regarding female empowerment.

4.5 Conclusion

This study estimates the general equilibrium effects of NREGS on wellbeing, poverty reduction, female social capital and empowerment. I use a three-round household panel data collected in 2004, 2006, and 2008 in Andhra Pradesh and estimate intention to treat effects (that is, average treatment effects (ATE) on the households who live in the program districts) The identification strategy exploits variation in the timing of NREGS implementation across districts. I use difference in difference (DID) plus propensity score matching (PSM) to look at the NREGS effects between phases. The availability of the two pre-intervention data allows for tests for the parallel trends assumption. The results suggest that NREGS has positive effects on consumption expenditure and nutritional intake for households in program areas and for non-participating households as well. The welfare effects are more pronounced for poor households. The findings are consistent with those in Imbert and Papp (2015), Klonner and Oldiges (2014) and Bose (2017). I also find that NREGS positively improves female social capital and empowerment with the latter materializes later than the former.

4.5 References

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4.6 Tables

Table 4.1: Impact of NREGS on wellbeing and poverty between Phase 2 and Phase 3 households

	(1)			(2)		
Outcomes	**DID**			**DID+PSM**		
Estimated impact: 2006 and 2008 panel						
Consumption (log)	0.249	(0.031)	***	**0.246**	**(0.042)**	***
Energy intake (log)	0.113	(0.026)	***	**0.125**	**(0.044)**	***
Protein intake (log)	0.128	(0.023)	***	**0.140**	**(0.043)**	***
Nonfinancial assets (log)	-0.079	(0.090)		**0.060**	**(0.163)**	
Number of observations	2426+830=3256			2492+732=3224		
Test for parallel trends assumption: 2004 and 2006 panel						
Consumption (log)	-0.093	(0.028)	***	-0.045	(0.046)	
Energy intake (log)	-0.015	(0.022)		0.018	(0.041)	
Protein intake (log)	0.005	(0.021)		0.033	(0.037)	
Nonfinancial assets (log)	0.128	(0.104)		0.16	(0.180)	
Number of observations	2426+830=3256			2587+765=3352		

Table 4.2: Impact of NREGS on wellbeing and poverty between Phase 2 and Phase 3 non-participating households

	(1)			(2)		
Outcomes	**DID**			**DID+PSM**		
Estimated impact: 2006 and 2008 panel						
Consumption (log)	0.219	(0.035)	***	**0.283**	**0.050**	***
Energy intake (log)	0.114	(0.029)	***	**0.129**	**0.043**	***
Protein intake (log)	0.139	(0.027)	***	**0.188**	**0.043**	***
Nonfinancial assets (log)	-0.181	(0.104)	*	0.081	0.17	
Number of observations	1435+567=2002			1479+495=1974		
Test for parallel trends assumption: 2004 and 2006 panel						
Consumption (log)	-0.09	(0.032)	***	-0.056	0.062	
Energy intake (log)	-0.01	(0.026)		0.008	0.044	
Protein intake (log)	-0.001	(0.025)		0.002	0.04	
Nonfinancial assets (log)	0.235	(0.122)	*	0.353	0.186	*
Number of observations	1435+567=2002			1550+523=2073		

Table 4.3: NREGS impacts on wellbeing and poverty for households with at least one primary casual laborer versus without primary casual laborer, for all and non-participating samples

Outcomes	(1) With at least one primary casual laborer			(2) Without primary casual labors		
Impacts on all households						
Consumption (log)	0.255	(0.056)	***	0.299	(0.046)	***
Energy intake (log)	0.116	(0.056)	**	0.120	(0.050)	**
Protein intake (log)	0.125	(0.041)	***	0.182	(0.046)	***
Nonfinancial assets (log)	-0.144	(0.226)		0.145	(0.170)	
Number of observations	1593+480=2073			899+252=1151		
Impacts on non-participating households						
Consumption (log)	0.293	(0.067)	***	0.268	(0.059)	***
Energy intake (log)	0.173	(0.054)	***	0.110	(0.053)	**
Protein intake (log)	0.204	(0.042)	***	0.169	(0.048)	***
Nonfinancial assets (log)	0.029	(0.250)		0.145	(0.181)	
Number of observations	836+288=1124			643+207=850		

Table 4.4: NREGS impacts on wellbeing and poverty for poor versus nonpoor households, for all and non-participating samples

	(1)			(2)		
Outcomes	**Poor**			**Nonpoor**		
Impacts on all households						
Consumption (log)	0.269	(0.054)	***	0.278	(0.052)	***
Energy intake (log)	0.146	(0.052)	***	0.072	(0.066)	
Protein intake (log)	0.163	(0.050)	***	0.112	(0.063)	*
Nonfinancial assets (log)	-0.067	(0.191)		0.107	(0.173)	
Number of observations	1703+495=2198			789+237=1026		
Impacts on non-participating households						
Consumption (log)	0.295	(0.067)	***	0.241	(0.052)	***
Energy intake (log)	0.137	(0.061)	**	-0.004	(0.117)	
Protein intake (log)	0.194	(0.050)	***	0.15	(0.073)	**
Nonfinancial assets (log)	-0.001	(0.206)		-0.02	(0.154)	
Number of observations	926+316=1242			553+179=732		

Table 4.5: NREGS impacts on wellbeing and poverty for scheduled tribe and caste (ST/SC) versus non-ST/SC households, for all and non-participating samples

	(1)			(2)		
Outcomes	**ST/SC**			**Non-ST/SC**		
Impacts on all households						
Consumption (log)	0.332	(0.085)	***	0.282	(0.047)	***
Energy intake (log)	0.134	(0.060)	**	0.121	(0.058)	**
Protein intake (log)	0.151	(0.051)	***	0.167	(0.044)	***
Nonfinancial assets (log)	0.016	(0.289)		0.139	(0.167)	
Number of observations	903+330=1233			1589+402=1991		
Impacts on non-participating households						
Consumption (log)	0.393	(0.104)	***	0.234	(0.066)	***
Energy intake (log)	0.135	(0.087)		0.064	(0.063)	
Protein intake (log)	0.147	(0.073)	**	0.15	(0.051)	***
Nonfinancial assets (log)	-0.233	(0.340)		0.003	(0.181)	
Number of observations	436+183=619			1043+312=1355		

Table 4.6: NREGS impacts on female social capital and empowerment, for Phase 1 and Phase 2 households

Variable	(1) DID			(2) DID+PSM		
2006/2008 Phase 1 and Phase 3 (medium-term effects)						
Female social capital	0.493	(0.059)	***	**0.458**	**(0.082)**	*****
Female empowerment	0.133	(0.023)	***	**0.082**	**(0.031)**	*****
observations	2425+870=3295			2291+798=3089		
2004/2006 Phase 1 and Phase 3 (test for parallel trends)						
Female social capital	-0.209	(0.061)	***	-0.074	(0.074)	
Female empowerment	0.04	(0.022)	*	-0.048	(0.034)	
observations	2425+870=3295			2304+811=3115		
2006/2008 Phase 2 and Phase 3 (short-term effects)						
Female social capital	0.684	(0.077)	***	**0.492**	**(0.243)**	****
Female empowerment	0.143	(0.027)	***	**0.062**	**(0.057)**	
observations	830+870=1700			761+565=1326		
2004/2006 Phase 2 and Phase 3 (test for parallel trends)						
Female social capital	0.188	(0.077)	**	0.134	(0.175)	
Female empowerment	-0.075	(0.025)	***	0.007	(0.049)	
observations	830+870=1700			771+573=1344		

Table 4.7: NREGS impacts on female social capital and empowerment by caste, for Phase 1 and Phase 2 households

Outcome	Scheduled tribe / caste			Other castes		
2006 / 2008 Phase 1 and Phase 3 (medium-term effects)						
Female social capital	0.462	(0.155)	***	0.502	(0.120)	***
Female empowerment	0.185	(0.053)	***	0.089	(0.039)	**
observations	797+332=1129			1494+466=1960		
2006 / 2008 Phase 2 and Phase 3 (short-term effects)						
Female social capital	0.597	(0.241)	**	0.563	(0.093)	***
Female empowerment	-0.003	(0.097)		0.183	(0.046)	***
observations	343+178=521			418+387=805		

Table 4.8: NREGS impacts on female social capital and empowerment by poverty status, for Phase 1 and Phase 2 households

Outcome	Poor			Non-Poor		
2006/2008 Phase 1 and Phase 3 (medium-term effects)						
Female social capital	0.464	(0.091)	***	0.532	(0.097)	***
Female empowerment	0.081	(0.028)	***	0.085	(0.046)	*
observations	1571+570=2141			720+228=948		
2006/2008 Phase 2 and Phase 3 (short-term effects)						
Female social capital	0.322	(0.231)		0.430	(0.154)	***
Female empowerment	-0.001	(0.062)		0.119	(0.063)	*
observations	343+178=521			418+387=805		

5. Disaggregated labor supply implications of guaranteed employment in India

Megan Sheahan, Yanyan Liu, Sudha Narayanan, and Christopher B. Barrett

5.1 Introduction

This paper explores various household level labor supply effects induced by participation in a government-sponsored employment guarantee program with self-selection at its core. Major government employment schemes, often in the form of public works projects, aim to provide income transfers to the poorest segments of the population beset with few employment options and low wages and may also serve as a shock to overall household labor supply as well as task- and member-specific time allocation.[88] The case of India's Mahatma Gandhi National Rural Employment Guarantee Scheme (hereafter, MGNREGS) is no different, apart from the unique constitutional "right to work" under-pinning its implementation. The massive scope of the scheme – inclusive of all rural areas of India and employing around 50 million people every year (Khera 2011) – however suggests the potential for more extensive labor market effects and for a range of complementary shifts in how particular groups of individuals spend their time. The entry of MGNREGS offers households, particularly poor households, the opportunity to reassemble their labor supply portfolio.

MGNREGS participation may alter overall household labor supply, the allocation of time to specific paid and unpaid work, and the gender and age composition of non-MGNREGS work. Policy makers should be attuned to these various implications of MGNREGS employment for a number of reasons. Most obviously, the program may have the potential to "crowd out" time previously allocated to work in the private sector, causing newfound constraints in the labor market and potentially no increase in the aggregate amount of time worked by individual laborers. There are fears that such crowding out, should it exist, would adversely affect agriculture specifically, a highly labor intensive industry in India. One could also imagine how MGNREGS participation may "crowd in" other types of work, including time spent on household enterprises or own-farm, especially where MGNREGS wages can be used to invest in these endeavors. Policy makers may view this latter case as a positive labor market outcome where these activities are highly productive and growth-inducing.

Moreover, the equality in wages offered to males and females via MGNREGS – in addition to a number of other program features that make it particularly attractive to women – may generate widespread female participation. Where women have historically undertaken much of the home-based unpaid work, female MGNREGS participation could prompt a shift in some of those tasks towards other household members, including youth and children who may be pulled out of school as a result. On the other hand, adult MGNREGS participation could bring more income into the household and reduce the need for younger members to participate in paid work. How these opposing forces net out is an important empirical question thus far unexplored. Ultimately, the decision to participate in MGNREGS – by both female and

[88] These programs are similar but not identical to the increasingly popular conditional cash transfer (CCT) programs where income is provided to targeted households conditional on some outcome or action. Indeed, government employment schemes can be considered a special case CCT program whereby the conditioning mechanism is work.

male adults – could bring about a range of important labor supply outcomes that may differ by gender, age group, and task.

A number of empirical studies have sought to untangle some of the important overall labor market participation changes induced by MGNREGS in a general equilibrium framework.[89] Imbert and Papp (2015) use district level data and exploit the phased roll out of the program to find a near one-to-one "crowding out" of private sector labor supply due to MGNREGS implementation. Azam (2012) uses the same data and same identification strategy but performs analysis at the individual level to find a positive intent-to-treat impact on labor market participation of females only. Zimmermann (2012) also uses the same data set but a regression discontinuity design which suggests more modest impacts on the labor market. While important to our understanding of the country-level impacts of MGNREGS, especially in its inception years, these papers are unable to discern the labor market effects of actual MGNREGS participation by households due to data constraints and the research methods chosen. Partial equilibrium questions, on the other hand, are potentially more salient in a self-selection context and where participation rationing is known to be widespread, with both factors highly relevant to MGNREGS.

Furthermore, these papers offer an overall look at labor market effects, with little to no attention to disaggregation by gender and age group of the household member, specific paid and unpaid work types, and agricultural season. Islam and Sivasankaran (2014) use data from three states to find that younger children spend more time in school while older children spend more time working outside of the household when MGNREGS is operating in their district, implying a reduction in child labor on account of MGNREGS. They show the importance of considering a range of time allocation effects separated by age group, but still are only able to estimate intent-to-treat effects and do not consider actual program participation. The gender of the MGNREGS participant may also induce different household labor supply effects, further necessitating a more detailed partial equilibrium exploration. We know, for example, that education outcomes for children in MGNREGS participating households have opposite effects depending on whether it is the mother or father who participates in the program (Afridi, Mukhopadhyay, Sahoo 2012). This finding encourages a more careful assessment of the labor supply effects specific to participant gender, especially given the many ways in which MGNREGS was expected to a be female-empowering program.

This paper extends the current research frontier by looking more specifically at various household level labor supply effects of MGNREGS participation. Unlike the many existing studies that rely on the National Sample Survey (NSS) data, we employ household panel data from primarily poor households in the state of Andhra Pradesh (AP) collected before (2004 and 2006) and after (2008) full MGNREGS phase in matched with administrative project data to answer household labor supply questions by comparing participants with non-participants. First, how does MGNREGS participation affect total household labor supply and the allocation of time to specific tasks? Do these effects vary by gender and age of the household member/worker, gender of the MGNREGS participant, or agricultural season? Second, how does one day of work devoted to a MGNREGS project "crowd out" (or "crowd in") time spent on paid and unpaid work at the household level? Again, does disaggregating by gender, task, or season reveal any important patterns?

[89] The studies listed here are in addition to several theoretical pieces on labor market effects described by Basu, Chau, Kanbur (2009); Basu (2011); Mukherjee and Sinha (2013); and others.

Our identification strategy relies on difference-in-difference estimation combined with propensity score weighting to counter the non-random selection into the program as well as the well-documented occurrence of work rationing within MGNREGS. Our results are drawn from one state where implementation has been applauded and demand levels are relatively high, however the findings may be relevant to other Indian states and countries considering how to better tailor their cash-for-work programs or labor market interventions.

5.2 Background: MGNREGS and household labor options

MGNREGS follows from the Mahatma Gandhi National Rural Employment Guarantee Act (hereafter, MGNREGA) passed in 2005 granting rural citizens the "right to work" on local and small-scale infrastructure projects (land improvement and clearing for community use, road and agricultural waterway creation, etc.) at a set wage. The legal entitlement that necessitates MGNREGS makes it the only government employment program like it in the world. Individuals self-target into the program at the village level where projects are determined before seeking funding approval at higher levels of the MGNREGS bureaucracy. While MGNREGS is a national program, it is implemented by individual states and relies heavily on more local level government (districts, sub-districts, and villages) to ensure that the program proceeds as "demand driven." Program benefits were phased in over three sets of districts based on an algorithm ranking poverty, or "backwardness," level.[90] The first phase districts were identified as the poorest and gained access to funds in the 2006/07 fiscal year, the second phase in 2007/08, and the third phase in 2008/09.

In rural India, most households derive their income from a variety of sources, including but not limited to private employment, casual labor, and home-based farming or enterprise development. Trends in employment suggest that off-farm sources of income have expanded relative to agriculture, especially from construction; agriculture, however, continues to be a source of income for a majority of rural households (e.g., Reddy *et al.* 2014; Chand and Srivastava 2014). During the last three decades, the participation rate for males in the rural labor force remained steady at about 56 percent while the rate for females declined from about 33-34 percent in 2004-05 to 26.5 percent by 2009-10. Bonded labor and child labor have declined rapidly, while seasonal migration remains an important component of household livelihood strategies (Deb *et al.* 2014). Typically, labor in India is classified as either permanent or casual/temporary, where permanent workers are considered "attached" to a landlord and spend most (if not all) of their time on this one activity whereas casual/temporary workers move between employers with much greater frequency. MGNREGS offers an additional income-generating means for rural laborers to consider adding to their overall household labor supply portfolio. How MGNREGS participating households account for this new income source – by either adding on top of existing labor days or displacing time spent on other tasks – has the potential to change the very nature of rural labor markets.

The new employment opportunity offered via MGNREGS is available at a uniform wage for everyone making it, by many measures, a "pro-women" program (Government of India 2012; Holmes, Sadana, Rath 2011; Khera and Nayak 2009). Not only are the wages offered to males and females equivalent, unlike the private market, but the administration established a female participation target of one-third of all beneficiaries, an aim far exceeded in many

[90] Zimmermann (2012) describes this algorithm and ranking process, both in theory and in practice, in more detail.

states with an average of 47 percent across all of India (Narayanan and Das 2014). The perceived appeal of MGNREGS employment likely goes well beyond gender-neutral wages; non-wage factors may also induce high female participation including greater dignity, the ability to work in groups and control time, child care supposedly offered on site, the provision of work near household dwellings, and the psychological sense of security (Narayanan 2008; Sudarshan 2011).

This explicit focus on women, coupled with the well-entrenched social norms that make for a male-dominated labor market in India and the fact that household work is culturally left to women, raises novel questions about if and how household labor supply outcomes shift or compensate for the time a given individual – male or female – spends on MGNREGS. For example, does male participation in MGNREGS alter the intensity and composition of women's work differently than women's participation in MGNREGS on male's non-MGNREGS work? Moreover, if males were employed elsewhere before the start of MGNREGS but MGNREGS employment is more attractive or necessary during spells of low labor demand, then male MGNREGS participation may only mean a shift in type of labor for males and have no effect on female work. But, for women, MGNREGS may provide a new avenue for work outside the home and, therefore, either add onto the work already done or cause a substitution of household work towards male household members or children.

While MGNREGS is exclusively offered to adults (age 18 and older), participation by an adult household member may have consequences for the time allocation of younger household members. The income effect of adult MGNREGS participation could reduce youth and children's household and work responsibilities. On the other hand, adult participation could newly burden household youth and children with tasks, like household work or own farm maintenance, to compensate for the absence of one or both parents. Both effects were uncovered for specific sub-populations in analysis of Ethiopia's Productive Safety Net Program, which also offers public sector work opportunities only to adults (Hoddinott, Gilligan, Taffesse 2009). In India, these effects have been studied in an aggregate general equilibrium context (Islam and Sivasankaran 2014) or more specifically with respect to effects on child schooling (Afridi, Mukhopadhyay, Sahoo 2012); true household time allocation evidence for MGNREGS participating households, however, remains anecdotal at best.

We also expect there to be important seasonal dimensions to the questions we ask. In AP, where there are two main agricultural seasons (*kharif* and *rabi*) and a slack season (summer), seasonality is relevant for a number of reasons.[91] First, there is an implicit scaling down of MGNREGS works in AP during the peak agricultural seasons in order to safeguard the interests of farmers and because "earth works" projects during the rainy season are not possible. Second, as with many other states in India, there is very little irrigation and therefore very little agricultural work available in the dry summer season. Third, wages can vary dramatically by season given the changing demand for labor. In particular, off-peak (peak) season wages may be lower (higher) than MGNREGS wages. Given these many disaggregated household questions that follow from our interrogation of gender, age group, task, and season-specific effects, we seek to better understand the relationship between time allocation decisions of individual members with the introduction of MGNREGS.

[91] *Kharif* season (usually June to October) marks the rainy season that begins with the onset of the summer monsoon. *Rabi* (usually November to February) is the winter season and summer (usually March to May) is the dry season with very little, if any, rainfall. The relative importance of the two agricultural seasons varies by district.

5.3 Data

The data come from three rounds (2004, 2006, and 2008) of panel household surveys administered under the World Bank's Andhra Pradesh Rural Poverty Reduction Project (APRPRP).[92] APRPRP was designed to reach 560 disadvantaged mandals (sub-districts) in sixteen districts, covering a large portion of AP (World Bank 2012). The external impact evaluation approach, carried out by the Centre for Economic and Social Studies (CESS), from which these data are derived, focused on five of the sixteen districts (Kadapa, Warangal, Nalgonda, Nellore, and Visahakapatnam), broadly representative of the three macro-regions within Andhra Pradesh (Telangana, Rayalaseem, and Coastal).[93] Within these districts, villages were randomly selected to be part of the sample, then households randomly sampled within qualitatively-assigned wealth stratification levels: poorest of the poor, poor, not so poor, and not poor.[94] The number of sampled households is 4,759 in 2004, 4,693 in 2006, and 4,533 in 2008.

The wealth of information collected via these questionnaires allows us to also answer various labor supply questions related to MGNREGS participation. Moreover, the timing of data collection allows us to observe two pre-MGNREGS years (2004 and 2006) and one within-MGNREGS year after all three phases of districts had access to the program (2008), as well as the varied effects as observed by districts in each of the three MGNREGS phases. Questionnaires were administered separately to a female and male respondent from each household, with some overlap but mostly separate questions. For example, time allocation questions for each member of the household age 10 and older were asked specifically of males. Village level questionnaires were administered to ascertain information better collected through key informant interviews; some of our important variables are derived from this complementary data set.

During 2008 data collection, enumerators also recorded the MGNREGS job card number of households that participated in MGNREGS, allowing us to link the household survey data with publicly available and audited MGNREGS administrative data that provide the exact number of days and specific dates individuals worked on MGNREGS projects. Further, the names of individuals provided in the administrative data allow us to match to our household survey data and identify the gender of the MGNREGS participant.[95] Importantly, the structure of the household survey and detailed administrative data allow us to uncover the seasonal dimensions of MGNREGS. In terms of other important external data, rainfall values are derived from geospatial datasets (from the Tropical Rainfall Measuring Mission at NASA) linked to the revenue village boundaries in our study area. We also match village level characteristics as observed before the start of MGNREGS from the Indian Population Census and Indian Village Amenities Census, both administered in 2001.

[92] For details on this program and its impacts, see Deininger and Liu (2013a, 2013b). Because nearly all households in our sample are APRPRP beneficiaries, we do not expect the impacts of this program to contaminate our results.

[93] Note that our study relies on data before Andhra Pradesh split into two states in 2014.

[94] See Deininger and Liu (2013c) for more details on how each category is defined.

[95] Of the full set of households that provided job card information, only 14 were not able to match with the administrative data. These households are dropped from our sample. We have confidence in the MGNREGS administrative data for two reasons: (1) the routine verification via social audits and (2) the results from an author-conducted data verification exercise in select AP villages in 2014. In the latter, household responses to recall questions about MGNREGS wages, number of days worked, and types of assets created matched entries in post-office or bank books where available.

Our final data set – dropping all observations with important missing values – includes 3,725 households in the five districts comprising 448 revenue villages within AP. Of the five districts included in our sample, three gained access to MGNREGS in 2006, one in 2007, and one in 2008. For the three agricultural seasons captured in the household survey, households in phase one and two districts are eligible for MGNREGS in the *kharif* and *rabi* seasons covered in the 2008 survey while the phase three districts gain access to MGNREGS starting only in the summer season (during which the 2008 fiscal year begins). When matching the household survey data with the administrative data, we find that 116 households participated in a MGNREGS project during the summer season captured in the 2006 survey. We drop these households from our data set in order to credibly produce uncontaminated estimates given that we use 2006 as our baseline.[96] Due to the nature of the APRPRP beneficiaries, relatively poorer households are oversampled. This skewness works in our favor since MGNREGS participants are also expected to be at the lowest end of the income distribution.[97]

5.4 Estimation approach

In the following sub-sections, we discuss the models and identification strategies employed to estimate the aggregate and task-specific time allocation effects of MGNREGS-participating households and the number of days worked on MGNREGS projects.

5.4.1 ATET of MGNREGS participation

Our first objective is to estimate the various household labor supply effects resulting from any (binary) MGNREGS participation, the average treatment effects on the treated (ATET). To do this, we estimate several models for household *j* in revenue village *v* and district d during survey year t that can be described by:

$$\boldsymbol{L_{jvdt}} = \alpha_{jvd} + \alpha_1 \boldsymbol{m}_{jvdt} + \alpha_2 \boldsymbol{x_{jvdt}} + \alpha_3 \boldsymbol{z_{vdt}} + \varphi_{dt} + \varepsilon_{jvdt} \qquad (1)$$

where $\boldsymbol{L}$ represents a vector of labor and task outcome variables, $\boldsymbol{m}$ is the vector of binary MGNREGS treatment variables describing whether a household member participated in the scheme, $\boldsymbol{x}$ is a vector of household observable characteristics, $\boldsymbol{z}$ is a vector of village characteristics, φ are district-year fixed effects, α_{jvd} are household fixed effects, and ε is the error term. This model can be estimated separately by agricultural season in order to better reveal the important labor market variation within a year.

The vector $\boldsymbol{L}$ includes not only total household labor, but also a range of other labor supply variables including paid and unpaid non-MGNREGS labor, labor time split by sub-category (private casual labor, farm servant (salaried) labor, non-farm self-employment, migration, other salaried work, own-farm, and household chores), female and male labor, and labor split between age groups within the household (adults age 18 and older, youth age 14-17, and children age 10-13). We also specify $\boldsymbol{m}$ as specific to any MGNREGS participation by an adult household member, but also separately of adult female and male participants.

[96] This represents 4.7 percent of the total household level sample before dropping.

[97] However, without the ability to weight the sample based on the original stratification methodology, this means our data are not necessarily representative of the population of households in the revenue villages or districts sampled. Those were not, however, statistically representative of AP state anyway.

To estimate equation (1) and the ATET effect embodied in α_1, we rely on a difference-in-difference (DID) approach whereby the difference between the 2008 and 2006 survey years can be used to estimate the impact of MGNREGS participation on aggregate household labor supply by treating the households that do not participate in MGNREGS in 2008 as the "control" group. The DID estimator offers the opportunity to explore unbiased causal relationships by controlling for time invariant unobservable characteristics of households, conditional on the acceptance of the parallel trend assumption, the assumption that changes in the dependent variable over time would have been exactly the same for both the participating and non-participating groups in the absence of MGNREGS work opportunities. We test the parallel trend assumption using the difference between 2006 and 2004, when MGNREGS was not available to the households.[98]

The causal interpretation of α_1 can be further undermined if non-random self-selection into MGNREGS is a concern, rendering our assumption of independence between treatment and outcome invalid. In a perfect MGNREGS-implementation environment, the "right to work" nature of the program should imply that the decision to participate in MGNREGS $\boldsymbol{m}$ would be subject to rampant selection effects. In practice, however, it is well-documented that MGNREGS work is rationed through a number of direct and indirect avenues which may occur for any number of administrative or political reasons (Narayanan and Das 2014; Das 2015).

The first is denial of a job card, the document necessary to apply for work. Second, after securing a job, an individual may not be able to access work, either through inadequate jobs and days available given the number of projects in progress, or because the skill, strength, or stamina needed for the work is too great, or the distant location of the work sites ration less skilled or healthy or more remotely located individuals. Liu and Barrett (2013) find that 44 percent of households sought but could not obtain a MGNREGS job nationwide in 2009-10, 25 percent in AP specifically. Ravi and Engler (2015) estimate rationing rates (i.e., demanded a job but not offered one) of 43 percent in 2007 and 21 percent in 2009 for Medak district in AP. Himanshu, Mukhopadhyay, and Sharan (2015) show how job card holders passively wait for work days to be offered by *sarpanches* in Rajasthan. This "supply driven" approach to work availability is similar to reports from AP (Maiorano 2014; Sheahan *et al.* 2014). Third, rationing may occur through the tacit repression of demand. Individuals who may otherwise wish to participate may be discouraged from expressing demand on account of delayed wage payments, participant intimidation, and general frustrations with program administration. For example, Narayanan *et al.* (in-progress) show that disappointment with implementation leads to "worker discouragement" and the reduced probability of seeking MGNREGS work.[99]

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[98] Here we test whether MGNREGS participating households as observed in 2008 experienced a common trend in labor supply changes between 2006 and 2008 as they did between 2004 and 2006 when MGNREGS was not an option. This is done by inserting the treatment variable m at t=2008 into the difference-in-different estimation for 2006/2004 and testing for the significance of its coefficient estimate.

[99] Barriers to MGNREGS participation may be most significant for women. Holmes, Sadana, and Rath (2011) found that women in Madhya Pradesh remain subject to entrenched social norms about what type of work is acceptable and received fewer MGNREGS work days as a result. This unequal access to work is further exacerbated for single women – never-married, divorced, separated, and widowed – who are expected to work alongside a man or are denied job cards because administrators inaccurately claim they do not constitute "a household" (Bhatty 2008).

We expect that this well-documented rationing severely dampens non-random selection effects. We do, however, also estimate our DID models using a propensity score weighting (PSW) method proposed by Hirano, Imbens, and Ridder (2003) that seeks to balance participants and non-participants. This involves using a logit model to predict the treatment variable, then using the predictions to calculate weights for application in the DID estimates.[100] We follow the sample trimming method proposed by Crump *et al.* (2006, see Theorem 5.3), as employed by others using these same data (Deininger and Liu 2013a). In our logit models, we include all control variables found in equation (1) as well as the lagged labor outcome variable (Chen, Mu, Ravallion 2009; Jalan and Ravallion 1998; Mu and Walle 2011), necessitating a unique weight for each model specification. This method is "double robust" in that if the main model is mis-specified but the selection function is correctly specified, the estimates based on the reweighted regression are still consistent (Wooldridge 2007).

DID estimation can also suffer from over-stated standard errors when serial correlation is pronounced (Bertrand, Duflo, Mullainathan 2004). However, the very limited time scale over which our panel data are observed and the fact that our differences essentially fall into a two pre- and post- MGNREGS intervention periods renders this common critique inconsequential in our case. All variables included in $\boldsymbol{x}$ and $\boldsymbol{z}$ for both our DID and DID-PSW estimation are described in more detail in Section 5.6. All standard errors are clustered at the revenue village level to limit the effects of potential heteroskedasticity and correlation across nearby observations.

5.4.2 "Crowding in/out" of time to other tasks on account of time spent on MGNREGS

In addition to the ATET effects of MGNREGS participation, we also test whether one day of MGNREGS participation by a household member influences the time spent on particular income generating activities or household tasks by any household member, i.e., if there are inter-individual, intra-household labor reallocation effects. We adapt from Datt and Ravallion (1994) as well as Imbert and Papp (2015) to specify the following model:

$$\boldsymbol{L_{jvdt}} = \beta_{jvd} + \beta_1 \boldsymbol{d}_{jvdt} + \beta_2 \boldsymbol{x_{jvdt}} + \beta_3 \boldsymbol{z_{vdt}} + \rho_{dt} + \epsilon_{jvdt} \tag{2}$$

where $\boldsymbol{d}$ is a vector containing variables that describe the number of days spent working on MGNREGS, ρ are district-year fixed effects that vary by year, β_{jvd} are household level fixed effects, ϵ is the error term, and $\boldsymbol{L}$, $\boldsymbol{x}$, and $\boldsymbol{z}$ are the same vectors as defined in equation (1). As before, $\boldsymbol{L}$ is defined with respect to our full set of labor/activity outcome variables and $\boldsymbol{d}$ contains variables specified with respect to any household member and separately by female and male participants.

Equation (2) is estimated in a DID panel framework whereby the difference between 2004 and 2006 time periods serves as one observation and the difference between 2006 and 2008 serves a second. Because $d = 0$ for all observations in the 2004/06 difference and for non-MGNREGS participating households in 2008, β_1 represents the estimated magnitude of the change in time spent on other work on account of one day spent on MGNREGS. If $\beta_1 < 0$, then MGNREGS labor "crowds out" time spent on other activities; if $\beta_1 > 0$, then MGNREGS labor "crowds in" labor for that activity or period. Based on others' findings

[100] For participating households, the weight is equal to one; for non-participating households, the weight is equivalent to $\widehat{ps}/(1-\widehat{ps})$ where $\widehat{ps}$ is the predicted propensity score.

we expect to observe “crowd out” effects on private labor time (Imbert and Papp 2015) and time spent on farm (Islam and Sivasankaran 2014).

Like the ATET estimates, β_1 will not serve as a convincing causal estimate if endogeneity is a concern. In an ideal implementation world, the number of days devoted to MGNREGS work would be jointly determined with the number of days allocated to any other type of work, leading to simultaneity issues in addition to the inherent program selection effects. Again, as in Section 4.1, we argue that rationing – not being able to work the number of days one would like, even with a job card in hand – diminishes the worry of endogeneity. We also use the difference between the ATET estimates derived from DID and DID-PSW, corroborated by test of the parallel trend assumption on which consistent DID estimates depend, as a signal of which labor categories or aggregates may be most susceptible to endogeneity concerns in this case. While not a pure test, we offer it as an illuminating check in the absence of any convincing instrumental variables (IVs).[101] To the extent that simultaneity and selection still matter beyond the known rationing, then our estimates are better interpreted as partial correlations conditional on many controls, and not necessarily as truly causal estimates.

5.5 Variable construction and descriptive statistics

5.5.1 MGNREGS participation

The MGNREGS job card information collected at survey time is matched with publicly available MGNREGS administrative data to ascertain which surveyed households worked on the program, when, and with what intensity (recorded as number of days worked). The percent of surveyed households in our data who worked on MGNREGS are provided by season and MGNREGS phase in Table 5.1. Since exact start and end dates for employment on a MGNREGS project are provided in the administrative data, we are able to assign the exact number of days worked to the agricultural seasons that match our household survey data. This table displays not only household level participation, but participation by gender and also the incidence of households with both female and male MGNREGS workers. In summer 2008, about 25 percent of our sampled households participated in MGNREGS, the highest of any season. When looking across all three seasons included in the 2008 survey, we find that about 31 percent of households worked on MGNREGS at some point (not shown), not dissimilar from the 35 percent Liu and Barrett (2013) calculate for the 2009 fiscal year using the NSS data for all of AP. In most cases, the percentage of households with a female MGNREGS participant is slightly higher than the households with a male participant. Nearly half of all MGNREGS participating households have both a female and male dedicating time to the program in a given season. Table 5.2 provides the unconditional (including zeros) average number of days worked on MGNREGS by season, with the largest value (about 6) in summer. When restricting to only participants, the average number of days is about 23 in *kharif*, 19 in *rabi*, and 23 in summer (not shown).

[101] In analysis by Afridi, Mukhopadhyay, and Sahoo (2012), MGNREGS participation is instrumented with rainfall shock in May-June and the number of projects in progress, both at the mandal level. When specifying these variables at the village level instead, neither of these variables – in addition to a host of many others attempted – prove to be good IVs (while relevant using the F-value>10 “rule of thumb,” the standard errors are massive when using as IVs).

5.5.2 Non-MGNREGS labor supply options

The household survey contains a full module on individual household member labor time allocation by agricultural season over the last full year for any household member age 10 and older. Labor days are split into seven categories: (i) casual agricultural and non-agricultural labor, (ii) farm servant salaried labor (more permanent agricultural work), (iii) non-farm self-employment (self-employment and economic activities derived from common property resources, like firewood collection), (iv) migration, (v) other salaried work (including services), (vi) own farm (crop production and livestock maintenance), and (vii) family chores.[102] In all labor categories, the number of days worked are recorded. In some sub-categories, the hours worked per day are also observed, in which case we standardize based on an eight hour work day.[103] We match the MGNREGS administrative data with the household survey data to "net out" the private and public sector casual labor by season, household, and gender within household. One day of work under any task is treated as equivalent to one day of work under another.

Table 2 shows the average number of days worked per household in more aggregated labor categories: paid non-MGNREGS labor includes casual labor, farm servant labor, non-farm self-employment, migration, and salaried work while unpaid non-MGNREGS labor includes own farm labor and household chores. Across seasons and years, days spent on paid non-MGNREGS work dominate those devoted to unpaid non-MGNREGS work. The introduction of MGNREGS in 2008 does not result in a large increase in total days worked between 2006 and 2008. Indeed, the average falls slightly in *kharif* and increases only marginally in *rabi* and summer. The percentage of households supplying labor to each minor category can be found in Table A1 of the appendix (Section 8.2) for the full household, Table A2 (Section 8.2) for household females by age group, and Table A3 (Section 8.2) for household males by age group. Private casual labor is clearly the type of paid work from which most households derive their income, followed by work on their own farm, and then by non-farm self-employment. The portion of households who work as salaried farm servants is very low (3 percent), although not lower than the percent of households who migrate for work (2 percent). Reported youth (age 14-17) and child (age 10-13) labor is miniscule. 2-5 percent of households report female or male youth casual labor.

5.5.3 Household characteristics

We include a range of household characteristics as controls, with summary statistics in Table A4 of the appendix (Section 8.2). The choice of which characteristics to include is motivated by similar work by Datt and Ravallion (1994) and analysis using the same AP data set by Deininger and Liu (2013). The first set of these variables are time invariant: (i) the poverty status of the household as classified by the stratification procedures used during data collection and (ii) household caste. These variables are inserted as linear terms, not differences, in all of our difference-in-difference models. The second set of household

[102] The one category of labor observed in the survey instrument that we purposefully exclude is the "other" category because it includes, by definition, time spent unemployed.

[103] Standardizing to the number of days worked instead of number of hours worked is preferred since most labor categories in the survey only provide days and many assumptions would be necessary to convert to hours. Moreover, MGNREGS work is observed in days, allowing for more direct comparison.

characteristics is time variant and, therefore, included as differences between survey waves. These include a range of household composition variables, gender of the household head, literacy of household members, land ownership and irrigation characteristics, shocks experienced by the household recently, and membership with the APRPRP self-help group.

5.5.4 Village characteristics

Village level control variables are also descriptively explored in Table A4 of the appendix (Section 8.2). Most of the village level variables we include can be interpreted as baseline characteristics observed before the introduction of MGNREGS, as derived from the Indian Population Census and Indian Village Amenities Census of 2001. Here we specify variables that seek to describe the population of the village in terms of caste, literacy, and primary occupation as well as the status of available amenities including roads, medical facilities, and agricultural credit societies. Because these variables are observed as static to us, we include them simply as linear (non-differenced) terms in our DID models.

The two important time variant village characteristics we observe are rainfall and wages. We include village level contemporaneous rainfall levels to control for weather-induced labor market shocks.[104] The casual daily wage rates come directly from the village survey that accompanies the household surveys. Members of the community are asked to recall casual wages separately for females and males in the village split by "peak" and "lean" agricultural seasons over the last year. We apply the "peak" wages to the *kharif* and *rabi* seasons and the "lean" wages to the summer season. Missing values at the village level are replaced with median values by mandal and, if necessary, by district. Nominal wages are adjusted to real levels using the consumer price index (CPI) specific to rural laborers in AP as released by the Directorate of Economics and Statistics. We deflate by aggregating months across agricultural seasons, setting the summer season observed in the 2008 survey (the most recent season) as the base. We include both the simple average of the adjusted male and female wages as well as a ratio of the male to female wages as control variables. We treat the wage rate as exogenous from the perspective of households at a given point in time since it represents a prevailing community level wage, and shifts in household labor supply will not have an immediate impact on wage levels.[105]

5.6 Regression results

5.6.1 ATET of MGNREGS participation

Regression results for our DID and DID-PSW estimates of equation (1) are displayed for any household MGNREGS participation in Table 5.3, female participation in Table 5.4, and

[104] The rainfall level is a proxy for exogenous demand for work of various types – casual agricultural labor, own farm labor, etc. – in a given season. Kochar (1999) finds that male household members in central India offer more hours to the labor market in the event of *unanticipated* crop shortfalls, not only forecasted ones, even when insurance markets are available. As such, we include the contemporaneous rainfall level in our model.

[105] Changes in agricultural technology induced by the potential contraction in private labor supply on account of MGNREGS (a link suggested by Bhargava 2014) could lead to changes in labor demand by private landlords and therefore effect labor supply for both MGNREGS participating and non-participating households. We argue that any of these unobserved changes are accounted for in the village level casual wage rate.

male participation in Table 5.5. Because separate regressions must be run to produce individual weights for each DID-PSW model, we do not display the full set of underlying logit propensity score estimation results. Instead, we show the marginal effects for six illustrative logit models in Table A5 of the appendix (Section 8.2), where results are in line with expectations.[106] The parallel trend tests for the DID and DID-PSW models can be found in appendix Tables A6, A7, and A8 (Section 8.2), respectively. Of the 48 labor supply outcomes related to each of the three treatment variables, only 4 (about 8 percent) do not pass the parallel trend assumptions for the DID-PSW models (columns 4-6), an improvement over the DID-only results (i.e., 6 do not pass for any participant, 5 for female participants, and 10 for male participants). We make note of these cases alongside our results in Tables 5.3-5.5 and only report the findings in this section where we are not concerned about the biasing effects of endogeneity.

The ATET estimates for MGNREGS participation by any household member reveal that total household labor supply only increases significantly in the summer slack season, where participation leads to an increase in about 12 days worked (Table 5.3). This effect results almost entirely from increases in adult female work, with only a small (and marginally statistically significant) portion from adult male work. Female and male participation separately also lead to increases in total household labor supply in the summer season (Tables 5.4 and 5.5); female participation leads to more days worked by adult females (13-14 days) and male participation contributes to more time worked by adult males (11-13 days days). When combined with the overall household labor supply result, it is apparent that female participation dominates in summer, when female non-MGNREGS wage rates are especially low and far inferior to men's wage rates. Female MGNREGS participation also increases female time spent working in the *rabi* season by about 7 days while overall changes in time worked in the *kharif* season is unchanged on account of MGNREGS participation regardless of gender.

Importantly, we find no increase in time spent on any work activities by youth or children of either gender, no matter which parent is a MGNREGS participant. This suggests that adult participation on MGNREGS does not have the unintended consequence of diverting time away from productive capital formation for youth and children over time in order to meet other household or enterprise obligations.[107]

Because these effects may change when limiting our sample to households with only one MGNREGS participant (i.e., when males and females do not work on MGNREGS work sites together), we also explore these same relationships on the relevant sub-samples of households. Appendix Table A9 (Section 8.2) shows the results specific to female MGNREGS participation (i.e., households with male MGNREGS participation are dropped), which maintains between 79-84 percent of the original sample, depending on season. We still find that female MGNREGS participation increases overall household labor supply by 11-13 days and total female adult labor supply by about 15 days, both in the summer season. When looking at the effects of male MGNREGS participation and dropping households with female

[106] Here we note the importance of household, village, and district level characteristics that are both time variant (lagged) and invariant in explaining MGNREGS participation for females and males. When estimating Shapely values to determine which groups of variables contribute most to the R-squared (fit) of one model (female MGNREGS participation in summer season), we find that household invariant characteristics account for 25 percent of the variation, household variant for 23 percent, village invariant for 13 percent, village variant for 6 percent, and district invariant for 33 percent.

[107] Our data do not allow us to make these same claims for children under age 10 but have no reason to assume the results would be any different.

MGNREGS time (which keeps 84-88 percent of the original sample), we find that our previously mentioned results mostly disappear. Appendix Table A10 (Section 8.2) shows that male participation in the absence of female participation does not increase overall household or adult labor supply. Our ATET results, therefore, are driven by female participation in MGNREGS, whether or not she is accompanied by a male household member. This is a very important finding, consistent with expectations and with the expressed gender equity objectives of the program.

Since the average number of days on MGNREGS for participating households is 23 in the summer season and 19 in the *rabi* season, the fact that our household, female adult, and male adult ATET values fall short of these suggests that the addition of days worked is likely displacing some non-MGNREGS labor. Indeed, we find that time spent on paid non-MGNREGS work does fall on account of program participation by any, female, or male adult household member in all three seasons. These negative impacts are quite large in the main agricultural seasons: with ATET values upwards of -20 days for male participants and greater than -15 for female participants. More specifically, any adult participation also leads to a statistically significant decrease in non-farm self-employment in the *rabi* season, which is derived from both female and male participation. Time spent on salaried labor also drops across seasons, no matter the participant. Household farm servant labor also decreases by about 7 days in the *kharif* season on account of male MGNREGS participation only. The fact that we see no overall increase in labor supply in the main agricultural season to combat these several decreases in other types of paid labor foreshadows the results of our "crowding out" analysis below.

We reject the null of parallel trends in the summer season for the unpaid and own-farm labor sub-categories in both the any participant and female participant models, implying that our PSW scheme is unable to fully reweight the participants and non-participants to balance the sample. While we are able to pass the test for male participation, we find no statistically significant time effects on unpaid labor derived from own-farm or household work across any of the seasons. If MGNREGS participants are largely operating on a subsistence basis, then these results make sense: household tasks and farm work are obligatory to maintaining a family.

5.6.2. Time allocation consequences on account of a day of MGNREGS work

Table 6 provides estimates of time allocation consequences on non-MGNREGS labor days on account of total number of days spent on MGNREGS projects — by labor type, season, and gender — with both within- and cross-gender effects, as explained by equation (2). This table focuses on total household labor supply (inclusive of anyone above age 10) but then specifically on adult female and male labor. Because we find no evidence of any ATET effects of MGNREGS participation on youth and child time, we relegate the results of these same regressions to appendix Tables A11 (youth) and A12 (children over 10) (Section 8.2). In the write-up that follows, we only discuss those instances where the ATET effects pass the parallel trend test and offer similar magnitudes between DID and DID-PSW methods. The line numbers listed in parenthesis in the text below are included to help guide readers around Table 6.

We estimate that a one day increase in the number of MGNREGS days provided by any household member "crowds out" 1.4 days of non-MGNREGS labor in the *kharif* season, 0.4 in the *rabi* season, and 0.6 in the slack summer season. When moving across the table to the columns that are specific to MGNREGS labor supplied by females and males, we learn that the displacement effects are similarly high for days spent working on MGNREGS projects by both genders. One day of MGNREGS work by any household adult displaces a marginally higher amount of adult female total non-MGNREGS time (line 11) than male time (line 21) in *kharif* (-0.7 versus -0.5), *rabi* (-0.3 versus no significant effect), and summer (-0.3 versus -0.2). Female MGNREGS days displace more female non-MGNREGS time whereas male MGNREGS day displace more male non-MGNREGS time, and with similar magnitudes across matched seasons.

Paid labor takes the largest hit, particularly private casual labor for both male and female adults, although more so for females. [108] These results are not only apparent in the summer season, but also consistently across the *kharif* and (sometimes) *rabi* seasons too. While significant and larger than the other effects, the “crowding out” of private casual labor is quite small in magnitude across the seasons and genders (apart from adult female labor in *kharif* season); in no case does one day of MGNREGS work completely displace a full day of private casual labor, the employment type that receives the largest number of days from the population under study. The male displacement of private casual labor (line 24) in the *kharif* season is entirely driven by male work on MGNREGS however the reduction in female private casual labor (line 14) is induced by both female and male time spent on MGNREGS across seasons.

Of other paid non-MGNREGS work, both women and men reduce their time spent on salaried work (lines 8, 18, and 28), but with estimated magnitudes of no greater than -0.1 days across any of the three seasons. Farm servant labor falls for adult males in the *kharif* season on account of days provided by males to MGNREGS (line 25), inducing an overall effect at the household level. Very few households (around 3-4 percent) engage in farm servant labor but they are considered “salaried” laborers in the survey. The MGNREGS labor option, therefore, not only reduces the time spent on casual/temporary labor opportunities, but also more formal/permanent jobs. It may be the case that these individuals do not have full time contracts (or do but are able to shirk on them) and can act as private casual laborers who move between MGNREGS and their other duties at will. While we do observe some very small “crowd in” effects in non-farm household enterprise work, our sub-category that includes both self-employment and income derived from the sale of common property resources (lines 16 and 26), we must interpret these results and magnitudes purely as correlations given our inability to pass the parallel trend tests for a number of the related ATET specifications.

The results for time spent on migration are perhaps surprising. At the household level, time spent on MGNREGS projects actually increases the time spent on migration work during the *rabi* agricultural season (line 7). These results are driven exclusively by male migration (line 27), however both female and male MGNREGS participation induce more male migration. It should be noted, however, that a very small percentage of the households in our sample engage in migration in any season or year. Indeed, there are only 30 households that are both MGNREGS participants and migraters in any of the 2008 agricultural seasons. But of those 30, only one of them also had migration in a previous household survey (2004). So, while there is a positive relationship between MGNREGS participation and migration, it is best

[108] Imbert and Papp (2015) estimate a 1.5 percent decrease in private employment across all of India on account of the introduction of MGNREGS, but in a general – not partial – equilibrium sense.

not over-interpreted given the very few households to which it applies. Our findings are at odds with those from Imbert and Papp (2014) who study the rural-to-urban migration experience of a small collection of villages in northwestern India. Our results, however, do align more closely with the experience in China (Chau, Kanbur, Qin 2014) and some Latin American countries (Hagen-Zanker and Himmelstine 2012).

Given our inability to pass all of the parallel trend assumption tests for the ATET estimates related to unpaid labor for the any and female MGNREGS participation, we interpret our results with caution. Across all genders, we observe that one day of MGNREGS work has a small but significant “crowd in” effect for time spent by adult women on farm (line 19) but the opposite effects for adult males (line 29). Discussions with MGNREGS workers in AP reveals that some women do not mind earning the discounted MGNREGS wages relative to higher private casual labor wages if it means they can spend the first part of their day at a MGNREGS site and the second half (after 2pm) engaging in own-farm work like leveling and clearing. While farms are small across the study area, between one-quarter and one-third of households supply at least some of their time to own-farm cultivation, implying that these are not necessarily only landless individuals, indeed marginal and small farmers too, that choose to spend some of their time on MGNREGS. This same line of reasoning may also be relevant to our previously mentioned findings on non-farm self-employment.

The household chore labor category (where we do pass the parallel trend tests) shows a negative effect for female adults (line 20), male adults (line 30, and the overall household (line 10), but quite small in magnitude overall. Interestingly, female MGNREGS days are related to declining household chore time for themselves and for male adults, however male MGNREGS days have no significant effects on time spent on household chores by either gender. This implies that time spent on MGNREGS by male adults results in a status quo among household operations, but that adult female time spend on MGNREGS results in a slight reorganization of time. Indeed, female time spent on MGNREGS has no effect on her time spent on household chores in *rabi* and summer seasons but a significant negative effect (although small: -0.1 days) on adult male time dedicated to household work.

We briefly delve into the time allocation effects on youth (Appendix Table A11) and children over ten (Appendix Table A12). For female youth, there are virtually zero effects. We only find incredibly small statistically significant effects related to the reduction in time spent on private casual labor on account of adult male participation. For youth males, we find several instances of tiny “crowd out” (household chores, own-farm) effects as well as “crowd in” effects (farm servant, other salaried work). The magnitude of all of these effects are less than 0.1 (absolute value), indeed generally closer to 0.01, implying shifts of less than an hour per eight hour work day. For female children (over ten), again, our results so virtually no change in time spent on any included activity. The one statistically significant effect we do uncover is a very small (approximately 30 minute) increase in time spent on private casual labor on account of a day of male adult time spent on MGNREGS. But, recall that only one percent of households in our sample even have female children engaged in private casual labor, so this result is drawn from and applies to a very small segment of the population. Moreover, male children see an overall reduction in time they spent on all paid and unpaid work, although our results do not point to which sub-category of work in particular.

As with our ATET estimates, it does not appear that youth and children are negatively affected by being pulled out of school to compensate for adult time lost to household chores or other household enterprise time that adult household members participating in MGNREGS neglect. At the same time, we do not observe a reduction in youth or child time spent on paid or unpaid household activities when an adult household member garners MGNREGS employment. This may be due to the fact that so few households report youth and child time spend on these activities. But, for those 10 percent of households where female youth contribute time to household chores and, even more, the 7 percent of households where female children contribute to household work, this result could also be viewed negatively. Our data do not allow us to delve more deeply into the related schooling effects.[109]

5.7 Conclusions

This paper explores how participation in India's massive employment guarantee scheme, MGNREGS, changes overall household labor and time allocated to particular types of paid and unpaid tasks disaggregated by gender, age category, and agricultural season using a panel of households across five districts in Andhra Pradesh. These results imply that employment guarantee schemes affect not only overall labor market indicators, as studied in a general equilibrium framework by several other researchers, but also the complex decision making process of individuals and households, which may have a second order effect on women's empowerment and childhood schooling outcomes. This underscores the value of such disaggregated analysis when analyzing a government labor market intervention as massive and influential as MGNREGS.

In summary, we find that any MGNREGS participation expands total household labor supply in the summer season, mostly for and on account of adult females, but reduces the total number of days spent on paid non-MGNREGS work by several days across the two main agricultural seasons for both female and male adults. We uncover no evidence of increase in time spent on paid or unpaid work, including household chores, for youth and children – both male and female – in MGNREGS households relative to non-MGNREGS households, suggesting no within-household substitution of work burdens towards younger members. One day spent on MGNREGS work "crowds out" less than a day of paid non-MGNREGS work in two of three seasons and mostly draws from the pool of time previously allocated to private casual labor opportunities (agricultural and non-agricultural). Meanwhile, MGNREGS "crowds in" male migration (although for a very small set of households) and female time spent on-farm and in non-farm self-employment activities in *rabi* and *kharif* seasons, respectively, perhaps due to the flexible work hours offered by the program that allow for afternoon work elsewhere.

To date, the seasonal dimensions of labor supply response have been described qualitatively but not incorporated into econometric analysis. Our results suggest that labor seasonality is especially important when rural labor markets are prone to major swings in both supply and demand. Our current analytical approach, however, cannot address whether or how MGNREGS contributes to labor spillovers across agricultural seasons, a worthwhile topic for future research. Additionally, the apparent gender differences in our results illuminate how MGNREGS participation by males and females differently affect the time allocation decisions of males and females within the same household. Given the sometimes profoundly different results by gender, our results suggest that analyzing these effects at the

[109] Our data only allow us to observe youth and children who never attended school or dropped out of school completely or for a short term at any point on their life, nothing specific to the recent past. These cases are only relevant to about 1 percent of households, in both the baseline (2004) and endline (2008) years.

individual level, not just gender-disaggregated household level, may yield some additional important insights.

This line of research not only adds to the growing body of literature specific to MGNREGS, but also can inform other large scale labor market interventions under consideration by low- or middle-income agrarian nations. This example from India, made special by the underlying constitutional right to work, helps feed into a larger literature exploring if and how the dispersion of government welfare benefits impact the labor market and household labor supply (e.g., de Brauw *et al.* 2015). Further, it can help to inform a related debate about the trade-off between workfare programs and cash distribution as a means of welfare enhancement for poor households (e.g., Alik-Lagrange and Ravallion 2015).

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5.9 Tables

Table 5.1: Percent of surveyed households participating in MGNREGS in 2008

MGNREGS Phase	HH Member	Kharif	Rabi	Summer
1	Any	26.0	20.8	30.1
	Females	21.1	18.1	25.8
	Males	16.1	11.4	19.1
	Both	11.1	8.8	14.7
2	Any	15.1	15.9	19.2
	Females	12.0	10.6	16.4
	Males	7.6	12.3	12.8
	Both	4.6	7.0	10.1
3	Any	-	-	15.7
	Females	-	-	10.9
	Males	-	-	11.3
	Both	-	-	6.5
Total	Any	18.2	15.3	24.8
	Females	14.7	12.7	20.7
	Males	11.0	9.1	16.2
	Both	7.4	6.5	12.1

Notes: This table shows the percent of surveyed households with any MGNREGS participation by season in the 2008 survey, accomplished by matching job card details with publicly available MIS data. The “any” rows describe household where any member, regardless of gender, participated. The “female” and “male” rows describe households where a female or a male member, respectively, participated. The “both” rows describe the percent of households where both a female and a male member participated.

Table 5.2: Number of days households participate to each major category of labor

	Kharif (153 days)						Rabi (120 days)						Summer (92 days)					
	2004		2006		2008		2004		2006		2008		2004		2006		2008	
	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd
Non-MGNREGS paid	134.6	(92.4)	159.7	(118.3)	154.1	(115.8)	153.0	(107.0)	129.3	(94.6)	127.0	(91.8)	86.2	(67.5)	84.2	(71.3)	91.1	(69.4)
Non-MGNREGS unpaid	91.9	(72.1)	134.2	(99.5)	134.6	(101.3)	125.4	(95.0)	95.6	(69.3)	97.8	(74.9)	62.7	(46.4)	65.8	(44.6)	63.0	(44.6)
MGNREGS	0.0	(0.0)	0.0	(0.0)	4.2	(12.9)	0.0	(0.0)	0.0	(0.0)	2.9	(10.3)	0.0	(0.0)	0.0	(0.0)	5.8	(16.1)
Total	226.6	(107.9)	293.8	(143.7)	292.8	(144.8)	278.4	(130.1)	224.9	(111.2)	227.7	(112.3)	148.9	(79.5)	150.0	(82.7)	159.9	(82.6)

Notes: This table shows the average number of days per household dedicated to each major aggregated labor category as aggregated from both household survey responses and publicly available MIS data on MGNREGS participation. The non-MGNREGS paid category is inclusive of private casual labor, farm servant, non-farm self-employment, migration, and salaried work time. Non-MGNREGS unpaid work is inclusive of time spent on own-farm and household chores.

Table 5.3: ATET estimate of any household MGNREGS participation (binary) on household labor supply outcomes

	DID									DID-PSW								
	(1) Kharif			(2) Rabi			(3) Summer			(4) Kharif			(5) Rabi			(6) Summer		
Dependent variable (all measured in days)	coef	se.	sig	coef	se.	sig	coef	se.	sig	coef	se.	sig	coef	se.	sig	coef	se.	sig
HH labor, total	-1.6	(-6.8)		4.3	(-5.7)		12.6	(-3.7)	***	-0.6	(-7.1)		2.7	(-5.6)		12.3	(-3.8)	***
HH labor, females age 18+	2.2	(-3.8)		5.4	(-3.2)	*†	12.2	(-2.0)	***†	2.1	(-3.7)		4.5	(-3.1)		10.8	(-2.0)	***
HH labor, females age 14-17	-0.6	(-1.4)		0.0	(-1.0)		0.0	(-0.6)		-0.9	(-1.5)		0.3	(-1.1)		-0.1	(-0.7)	
HH labor, females age 10-13	-0.8	(-1.0)		-1.1	(-0.7)		0.0	(-0.5)		0.4	(-1.0)		-0.4	(-0.8)		-0.1	(-0.5)	
HH labor, males age 18+	-2.4	(-3.9)		1.1	(-3.4)		2.8	(-2.3)		-1.1	(-4.0)		0.2	(-3.4)		4.1	(-2.4)	*
HH labor, males age 14-17	1.4	(-1.2)		0.9	(-1.0)		-0.4	(-0.7)		0.7	(-1.4)		0.3	(-1.1)		-0.3	(-0.7)	
HH labor, males age 10-13	0.9	(-0.7)		-0.1	(-0.5)	†	0.3	(-0.4)		0.0	(-0.7)		-0.3	(-0.6)		-0.2	(-0.4)	
HH labor from paid non-NREGA work, total	-22.6	(-5.6)	***	-12.5	(-4.6)	***†	-7.0	(-3.3)	**	-18.0	(-5.8)	***	-15.8	(-4.6)	***	-10.8	(-3.4)	***
HH labor from private casual labor, total	-17.5	(-5.2)	***	-6.4	(-4.5)	†	-4.0	(-3.0)		-6.3	(-5.4)	†	-4.4	(-4.5)		-2.8	(-3.1)	
HH labor from farm servant labor, total	-0.7	(-2.0)	†	0.3	(-1.5)		-0.6	(-0.9)		-0.2	(-2.2)		-0.8	(-1.6)		-1.6	(-1.0)	*
HH labor from non-farm self-employ, total	-1.3	(-2.7)		-5.1	(-2.1)	**	-1.3	(-1.4)		-4.6	(-2.5)	*	-6.2	(-2.2)	***	-3.8	(-1.4)	***†
HH labor from migration labor, total	1.4	(-1.4)		1.2	(-1.2)		1.1	(-0.7)		1.7	(-1.3)		0.9	(-1.1)		0.7	(-0.7)	
HH labor from salaried labor, total	-4.5	(-2.9)		-2.5	(-2.3)		-2.2	(-1.4)		-7.4	(-2.9)	**	-4.8	(-2.3)	**	-2.9	(-1.5)	**
HH labor from unpaid non-NREGA work, total	-2.0	(-4.2)		-2.1	(-3.8)		-3.5	(-2.0)	*	-5.4	(-4.3)		-0.8	(-3.6)		-0.2	(-2.1)	†
HH labor from own-farm labor, total	-0.7	(-3.4)		-0.9	(-3.0)		-2.2	(-1.5)		-3.1	(-3.4)		-1.2	(-2.9)		0.5	(-1.6)	†
HH labor from HH work, total	-1.3	(-2.4)		-1.1	(-2.0)		-1.4	(-1.2)		-2.3	(-2.6)		0.8	(-2.0)		-0.7	(-1.3)	

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table contains the output of various specifications of equation (1). Only the ATET estimates for MGNREGS participation are included in the table. Only phase 1 and 2 households are included in the kharif and rabi analysis, while phase 3 households are added to the summer regressions. For a list of control variables, see Table A4 of the appendix (Section 8.2). District fixed effects included. All standard errors are clustered at revenue village level. † = does not pass parallel trend tests. Parallel trend test results can be found in Table A6 of the appendix (Section 8.2).

Table 5.4: ATET estimate of female MGNREGS participation (binary) on household labor supply outcomes

	DID									DID-PSW								
	(1) Kharif			(2) Rabi			(3) Summer			(4) Kharif			(5) Rabi			(6) Summer		
Dependent variable (all measured in days)	coef	se.	sig	coef	se.	sig	coef	se.	sig	coef	se.	sig	coef	se.	sig	coef	se.	sig
HH labor, total	-0.2	(-7.3)		10.1	(-6.0)	*†	13.2	(-4.0)	***	-0.2	(-7.3)		5.8	(-6.0)		14.1	(-4.1)	***
HH labor, females age 18+	1.9	(-4.1)		7.2	(-3.4)	**†	12.2	(-2.2)	***	4.2	(-4.1)		7.1	(-3.3)	**	12.7	(-2.2)	***
HH labor, females age 14-17	-0.6	(-1.5)		0.0	(-0.9)		0.2	(-0.6)		-0.2	(-1.7)		-0.1	(-1.0)		0.3	(-0.7)	
HH labor, females age 10-13	-1.1	(-1.1)		-1.3	(-0.8)		-0.3	(-0.5)		0.8	(-1.2)		-0.5	(-0.9)		-0.1	(-0.5)	
HH labor, males age 18+	-0.2	(-4.2)		5.4	(-3.7)		3.0	(-2.5)		-3.2	(-4.3)		2.1	(-3.7)		3.9	(-2.5)	
HH labor, males age 14-17	0.7	(-1.3)		0.5	(-1.0)		0.1	(-0.7)		-0.4	(-1.4)		-0.9	(-1.1)		-0.3	(-0.8)	
HH labor, males age 10-13	1.0	(-0.8)		0.1	(-0.6)	†	0.4	(-0.3)		0.3	(-0.8)		0.1	(-0.6)		-0.2	(-0.4)	
HH labor from paid non-NREGA work, total	-22.9	(-5.9)	***	-11.8	(-5.1)	**†	-7.2	(-3.4)	**	-17.4	(-6.0)	***	-15.4	(-5.1)	***	-10.6	(-3.4)	***
HH labor from private casual labor, total	-16.9	(-5.6)	***	-7.6	(-4.8)	†	-4.0	(-3.1)		-6.1	(-5.7)	†	-6.1	(-4.9)		-2.8	(-3.3)	
HH labor from farm servant labor, total	-0.7	(-2.4)		0.6	(-1.6)		-0.2	(-0.9)		-0.2	(-2.6)		0.0	(-1.7)		-1.2	(-0.9)	
HH labor from non-farm self-employ, total	-1.9	(-2.8)		-4.1	(-2.3)	*	-1.5	(-1.5)		-5.9	(-2.5)	**†	-5.2	(-2.4)	**	-4.1	(-1.4)	***†
HH labor from migration labor, total	1.2	(-1.7)		1.4	(-1.3)		1.0	(-0.8)		1.4	(-1.7)		0.9	(-1.1)		1.0	(-0.8)	
HH labor from salaried labor, total	-4.5	(-3.0)		-2.2	(-2.6)		-2.5	(-1.5)		-6.0	(-2.9)	**	-4.7	(-2.5)	*	-3.1	(-1.5)	**
HH labor from unpaid non-NREGA work, total	-1.3	(-4.4)		1.7	(-3.9)		-4.3	(-2.3)	*	-6.4	(-4.4)		1.0	(-3.7)		0.0	(-2.3)	†
HH labor from own-farm labor, total	1.7	(-3.6)		3.6	(-3.0)		-1.7	(-1.7)		-3.4	(-3.6)		2.2	(-2.9)		1.4	(-1.7)	
HH labor from HH work, total	-3.0	(-2.6)		-1.9	(-2.0)		-2.5	(-1.3)	*	-2.7	(-2.6)		-0.7	(-2.0)		-1.1	(-1.4)	

Notes: See notes for Table 5.3. † = does not pass parallel trend tests. Parallel trend can be found in Table A7 of the appendix (Section 8.2). For results on sub-sample of households without male MGNREGS participation, see Table A9 of the appendix (Section 8.2).

Table 5.5: ATET estimate of male MGNREGS participation (binary) on household labor supply outcomes

Dependent variable (all measured in days)	DID									DID-PSW								
	(1) Kharif			(2) Rabi			(3) Summer			(4) Kharif			(5) Rabi			(6) Summer		
	coef	se.	sig	coef	se.	sig	coef	se.	sig	coef	se.	sig	coef	se.	sig	coef	se.	sig
HH labor, total	-9.4	(-8.0)		3.2	(-7.0)		13.2	(-4.3)	***	0.1	(-8.0)		3.1	(-6.8)		10.7	(-4.3)	**
HH labor, females age 18+	-0.2	(-4.3)		3.2	(-4.0)	†	9.3	(-2.3)	***†	2.4	(-4.1)		2.2	(-3.9)		7.0	(-2.3)	***
HH labor, females age 14-17	-2.5	(-1.6)		-0.7	(-1.3)		-0.1	(-0.7)		-2.4	(-1.8)		-1.2	(-1.3)		-0.7	(-0.7)	
HH labor, females age 10-13	-0.5	(-1.1)		0.3	(-0.8)		-0.1	(-0.5)		-0.2	(-1.2)		-0.1	(-0.7)		-0.1	(-0.5)	
HH labor, males age 18+	-6.1	(-4.7)	†	1.8	(-4.3)		6.5	(-2.7)	**	0.9	(-4.7)	†	4.6	(-4.1)		6.1	(-2.8)	**
HH labor, males age 14-17	2.6	(-1.6)		2.1	(-1.4)		0.1	(-0.9)		1.7	(-1.7)		1.5	(-1.4)		0.5	(-0.9)	
HH labor, males age 10-13	0.1	(-0.7)		-1.0	(-0.6)	*†	0.0	(-0.5)		-0.7	(-0.6)		-1.0	(-0.6)		-0.2	(-0.4)	
HH labor from paid non-NREGA work, total	-29.6	(-6.7)	***	-15.1	(-5.7)	***†	-8.5	(-3.8)	**†	-23.0	(-6.6)	***	-19.4	(-5.5)	***	-14.1	(-3.8)	***
HH labor from private casual labor, total	-18.4	(-6.0)	***	-7.1	(-5.5)	†	-4.1	(-3.5)		-2.5	(-6.2)	†	-5.6	(-5.3)		-2.9	(-3.6)	
HH labor from farm servant labor, total	-6.8	(-2.4)	***†	-2.2	(-1.6)		-2.0	(-1.0)	*	-6.7	(-2.7)	**	-4.1	(-1.7)	**	-2.8	(-1.1)	**
HH labor from non-farm self-employ, total	-2.0	(-3.1)	†	-3.4	(-2.8)		-1.6	(-1.6)	†	-6.0	(-3.0)	**†	-4.5	(-2.7)	*	-4.9	(-1.5)	***†
HH labor from migration labor, total	2.0	(-1.6)		1.7	(-1.4)		1.5	(-0.8)	*	2.1	(-1.6)		1.3	(-1.4)		0.8	(-0.8)	
HH labor from salaried labor, total	-4.3	(-3.0)		-4.1	(-2.5)		-2.4	(-1.6)		-8.5	(-3.1)	***	-5.8	(-2.6)	**	-3.5	(-1.7)	**
HH labor from unpaid non-NREGA work, total	-4.4	(-5.3)		-2.9	(-4.9)		-3.1	(-2.4)		-1.4	(-5.4)		0.8	(-4.6)		0.0	(-2.4)	
HH labor from own-farm labor, total	-5.7	(-4.3)		-2.8	(-3.9)		-1.5	(-1.9)		-2.8	(-4.5)		-0.7	(-3.7)		0.5	(-1.9)	
HH labor from HH work, total	1.2	(-3.0)		-0.1	(-2.5)		-1.6	(-1.4)		0.8	(-3.0)		1.5	(-2.6)		-0.4	(-1.4)	

Notes: See notes for Table 5.3. † = does not pass parallel trend tests. Parallel trend tests can be found in Table A8 of the appendix (Section 8.2). For results on sub-sample of households without female MGNREGS participation, see Table A10 of the appendix (Section 8.2).

Table 5.6: "Crowding out" estimates of other activities on account of days spent on MGNREGS (full household and adult outcome variables)

	Dependent variable (all measured in days)	Total household MGNREGS labor days			Female MGNREGS labor days			Male MGNREGS labor days		
		Kharif	Rabi	Summer	Kharif	Rabi	Summer	Kharif	Rabi	Summer
1	*Total HH labor from all non-NREGA work*	-1.39***	-0.44**	-0.60***	-1.67***	-0.58*	-0.85***	-2.56***	-0.75*	-0.94***
2	*Total HH labor from paid non-NREGA work*	-1.03***	-0.55***	-0.42***	-1.22***	-0.71***	-0.60***	-1.91***	-0.96***	-0.64***
3	*Total HH labor from unpaid non-NREGA work*	-0.37***	0.11	-0.18***	-0.45**	0.13	-0.26***	-0.64**	0.20	-0.30***
4	Total HH labor from private casual labor	-0.80***	-0.38**	-0.32***	-1.03***	-0.57**	-0.50***	-1.36***	-0.48	-0.43***
5	Total HH labor from farm servant labor	-0.13*	-0.04	0.00	-0.08	-0.01	0.02	-0.35**	-0.15	-0.03
6	Total HH labor from non-farm self-employ	-0.05	-0.11	-0.06*	0.00	-0.14	-0.08	-0.21	-0.23	-0.10
7	Total HH labor from migration labor	0.05	0.07**	0.03*	0.05	0.10*	0.04	0.11	0.11**	0.03
8	Total HH labor from salaried labor	-0.09*	-0.09	-0.07**	-0.15	-0.10	-0.09*	-0.11	-0.20	-0.13*
9	Total HH labor from own-farm labor	-0.18	0.16	-0.10***	-0.09	0.25	0.13**	0.53**	0.18	-0.20**
10	Total HH labor from HH work	-0.19**	-0.06	-0.08**	-0.37***	-0.12	-0.12***	-0.11	0.02	-0.10
11	*Adult female labor from all non-NREGA work*	-0.72***	-0.30**	-0.28***	-1.08***	-0.50***	-0.46***	-0.94***	-0.26	-0.31***
12	*Adult female labor from paid non-NREGA work*	-0.56***	-0.40***	-0.22***	-0.83***	-0.63***	-0.37***	-0.75***	-0.48**	-0.23***
13	*Adult female labor from unpaid non-NREGA work*	-0.16**	0.10	-0.06*	-0.25**	0.12	-0.08*	0.19	0.22	-0.08
14	Adult female labor from private casual labor	-0.50***	-0.31***	-0.20***	-0.78***	-0.51***	-0.34***	-0.60***	-0.30	-0.17**
15	Adult female labor from farm servant labor	-0.04	-0.01	0.00	-0.05	0.00	-0.01	-0.06	-0.02	-0.01*
16	Adult female labor from non-farm self-employ	0.03	-0.06	-0.01	0.10*	-0.09	-0.02	-0.06	-0.10	-0.02
17	Adult female labor from migration labor	-0.02	0.01	0.01	-0.02	0.03	0.02	-0.02	-0.01	0.00
18	Adult female labor from salaried labor	-0.04	-0.04*	-0.02**	-0.08*	-0.06	-0.02**	-0.01	-0.06	-0.03
19	Adult female labor from own-farm labor	-0.05	0.10**	-0.03	-0.03	0.14**	-0.04	-0.14	0.17*	-0.04
20	Adult female labor from HH work	-0.11**	0.00	-0.03	-0.22***	-0.02	-0.05	-0.06	0.05	-0.04
21	*Adult male labor from all non-NREGA work*	-0.54***	-0.05	-0.24***	-0.45**	0.03	-0.27***	-1.32***	-0.27	-0.51***
22	*Adult male labor from paid non-NREGA work*	-0.56***	-0.40***	-0.22***	-0.83***	-0.63***	-0.37***	-0.75***	-0.48**	-0.23***
23	*Adult male labor from unpaid non-NREGA work*	-0.16**	0.02	-0.10***	-0.14	0.02	-0.14***	-0.38**	0.03	-0.16**
24	Adult male labor from private casual labor	-0.19**	0.02	-0.07*	-0.13	0.04	-0.07	-0.52***	0.00	-0.18*
25	Adult male labor from farm servant labor	-0.08	-0.05	0.00	-0.05	-0.03	0.02	-0.24**	-0.16*	-0.02
26	Adult male labor from non-farm self-employ	-0.08*	-0.04	-0.04	-0.08	-0.05	-0.05	-0.16	-0.09	-0.07
27	Adult male labor from migration labor	0.06*	0.06**	0.03**	0.06	0.07*	0.04**	0.13*	0.11**	0.04*

28	Adult male labor from salaried labor	-0.08	-0.05	-0.05*	-0.10	-0.03	-0.05	-0.15	-0.17	-0.11*
29	Adult male labor from own-farm labor	-0.11*	0.07	-0.06**	-0.04	0.12	-0.07*	-0.37***	0.04	-0.12*
30	Adult male labor from HH work	-0.05	-0.05	-0.04***	-0.10**	-0.10**	-0.07***	-0.01	-0.01	-0.04

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table contains the regression results for estimating equations (3) and (4). Standard errors clustered at the revenue village level (not included only for space reasons, available upon request). District fixed effects included in all specifications. Only phase 1 and 2 households are included in the kharif and rabi analysis, while phase 3 households are added to the summer regressions. See Table A4 of the appendix (Section 8.2) for a list of control variables used in all regression. The grayed portions of the table represent the gender-matched effects whereas the remaining white portions are cross-gender and gender-specific effects. Results for youth and children found in Tables A11 and A12 of the appendix (Section 8.2), respectively.

6. Preferential resource spending under an employment guarantee: The political economy of MGNREGS in Andhra Pradesh

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6.1 Introduction

While seemingly essential for poverty reduction in many contexts, it is well-known that public works spending is often subject to political manipulation (Cadot, Röller, Stephan 2006) whereby public resources are strategically awarded with the intention of garnering or rewarding political support instead of catering to economic needs (Kurer 1993; Powell 1970). Because politically-rooted spending may lead to sub-optimal social policy and undermine the usefulness of these projects as poverty-reducing or growth-enhancing tools (Kurer 1996), uncovering instances where funds are distributed based on non-economic reasons and curtailing the extent to which politics can infiltrate project allocation is crucial. This is especially important in light of the many government workfare programs used as short and long term strategies for targeting the poor.

India's Mahatma Gandhi National Rural Employment Guarantee Scheme (hereafter, MGNREGS), employing about 50 million men and women every year (Khera 2011), offers an interesting case for investigating the link between public works spending and politics for two important reasons. First, MGNREGS is derived from the Mahatma Gandhi National Rural Employment Guarantee Act (hereafter, MGNREGA) which grants citizens the "right to work" on local infrastructure projects at a set minimum wage, making it one of the only programs in the world to nest a government workfare program within a legal entitlement. MGNREGS, therefore, is ostensibly designed to be a self-targeting and demand-driven program, where labor is aggregated and public works are selected at the local level before final approval by higher level government authorities. Second, while the demand-driven nature of the program may suffice to counter the political manipulation of program funds, MGNREGS also put in place a suite of accountability and transparency mechanisms, including but not limited to publicly-available data and social audits. The extent to which these several unique features of MGNREGS have eliminated avenues for using the program for political reward or gain is a conjecture worth exploring.[110]

Given great heterogeneity in implementation records across states in India (e.g., Dutta *et al.* 2014, 2012), this paper investigates the correlates of MGNREGS spending at the mandal (sub-district) level in Andhra Pradesh (hereafter, AP).[111] AP acts as a useful case because it is one of the few states praised for its implementation quality, allowing us the opportunity to examine whether political manipulation can still exist despite overwhelmingly "good" program performance. Underpinning this implementation in AP, however, is a heavily "top-down" approach despite the "bottom-up" manner originally conceived by MGNREGA. The fact that directives frequently come from higher levels of government raises obvious questions about how MGNREGS can be influenced by political motivations at those levels, undermining the very spirit of an employment guarantee program.

[110] We recognize that other programs in India with similar profiles may also be manipulated for political gain, however choose to focus on MGNREGS given the unique features of the program and the context in which we explore this question.

[111] While the state-level may seem small, the population of AP during the period under study was nearly 50 million, larger than many countries in other regions of the world, and with high poverty levels. Moreover, it should be noted that the AP that we refer to this in this study is before its bifurcation in mid-2014.

Establishing a relationship between the safeguards or institutions embedded within MGNREGS and the political manipulation of the program would be an incredibly difficult, if not impossible, task. Instead we focus on the extent to which project spending at the mandal level follows patterns of politically-influenced non-programmatic distribution, as defined by Stokes *et al.* (2013), beyond the publicly stated target of the program, human needs (broadly defined), in a state where accountability mechanisms are known to exist and function. The timely occurrence of a national and state-level election in 2009, several years into the implementation of MGNREGS, allows us the opportunity to test for the political distribution of funds on either side of the successful re-election of the United Progressive Alliance (UPA) coalition. We use the mandal level as our unit of analysis, unlike the bulk of the MGNREGS evidence to date, because mandals best match the constituencies that form the state-level assembly, the governing body with ultimate control over MGNREGS spending and priorities (in addition to other reasons, see Section 6.4).

Other important political economy research on MGNREGS to date has focused on issues such as rent-seeking behavior (Niehaus and Sukhtankar 2013a) and leakage (Niehaus and Sukhtankar 2013b). There is recent, yet unpublished, literature that links MGNREGS fund allocation to politics. Most similar to our study, Gupta and Mukhopadhyay (2014) find some evidence of an inverted-U shaped relationship between vote shares of the ruling political party in panchayat samiti elections and MGNREGS spending at the sub-district level in the state of Rajasthan, but that the relationship reverses when looking specifically at very close elections. It is important to explore beyond this case study for various reasons, most importantly the colossal dissimilarities in implementation approaches and records between states. In AP, Johnson (2009b) finds no evidence of a relationship between the political affiliation of the most local leaders (gram panchayat and mandal parishad) and MGNREGS outcomes, implying that either MGNREGS is unsusceptible to capture by elected officials or that local leaders have little to no power to manipulate spending levels. Our research fits into and goes beyond this emerging literature by exploring how election outcomes at higher levels, including for the body with direct control over funding flows, are related to MGNREGS spending outcomes.

Our results offer optimism about the MGNREGS bureaucracy in AP as well as some critiques to guide reforms. We find no evidence of partisan-based spending in the initial years of program implementation although we do uncover a small amount of preference given to mandals that voted for the winning incumbent coalition in the 2009 elections in the following years. Even so, the overwhelming majority of MGNREGS spending to date flowed according to needs-based correlates, as the program intended, so the distortionary effect of politically-driven resource allocation is very modest, likely on account of the distinct demand-driven characteristics of the scheme and the local political context at the time. Through our analysis of MGNREGS, we also offer a range of hypotheses for empirically testing these political effects using any public project that straddles a major election and conclude with a discussion of the implications for the literature on the relative merits of central versus local control over program implementation.

6.2 Context

In this section, we provide a summary of MGNREGS implementation and the relevant local political context in AP. For more details on the timing of MGNREGS roll-out and the changing political situation between 2004 and 2012, see Figure 6.1.

6.2.1 MGNREGS in AP

MGNREGS implementation was phased in over three sets of districts categorized based on "backwardness" level. In the first phase the poorest districts gained access to funds in the 2006/07 fiscal year, with each of the remaining two phases joining in succession in the following years.[112] While MGNREGS is a national program implemented by individual states, the states are tasked with implementing it through a "bottom-up" approach to planning and selecting works. Section 16 (3) (4) of MGNREGA stipulates that every gram panchayat, the village level elected body, with participation from constituents, be responsible for aggregating local demand for work, developing a list of projects that would benefit the larger community, and proposing a timeline for completion. The long-run development and annual work plans are submitted to the district level, which aggregates the plans across mandals and then submits to the state level government for final approval of both the priorities of works and budget (Bhanumurthy *et al.* 2014).[113] It was envisaged that decentralized responsibility to determine which projects should move forward under MGNREGS would ensure their contextual appropriateness, reflect the local needs and priorities of the people, and facilitate a demand-driven approach.

Popular opinion and empirical studies claim, however, that factors apart from the intended "demand driven" targeting tactics, generally political ones, determine where MGNREGS funds are directed. The Central Employment Guarantee Council (2010) observed that work priorities across India tend to follow orders from state or district headquarters and do not reflect the stated needs and aspirations of the people. In AP specifically, Reddy (2012) observes that implementation has often been flush with directives from the state government on the prioritization of works. Maiorano (2014) further substantiates this claim in the AP, referring to the implementation approach as "supply driven" and "rigid top down" (p. 97). Maiorano finds that hired Field Assistants, not locally elected leaders, implement programs at the village level, undermining the power envisioned of the gram panchayat. The state government of AP, which employs and manages the Field Assistants, can exert direct control of the implementation process through these individuals. Another field report from AP by Chamorro *et al.* (2010) states that the supply of jobs (and therefore expenditures) seemed more determined by the Field Assistants than by actual demand from laborers. A "top down" approach to program implementation and spending directives may imply the political manipulation of funds by higher-level elected leaders.

At the same time, a growing collection of evidence exists to suggest that AP stands out as a "success story" above other Indian states in implementing MGNREGS. For example, Johnson (2009a) found little evidence that the political affiliation of the local level leader influenced any of the project outcomes in AP. Descriptive evidence from Johnson, Tannirkulam, and Larouche (2009) suggests that MGNREGS in AP has been better targeted to the intended beneficiaries than other government programs operating over the same time frame. Johnson (2009b) found that MGNREGS allowed households in AP to mitigate the negative income effects of weather-related shocks, implying the timely distribution of funds to needy households. As part of their cross-state analysis, Liu and Barrett (2013) note that AP is one of the eight states categorized as having "good" pro-poor implementation.

[112] In AP, 13 districts were included in phase 1 (2006/07), 6 districts in phase 2 (2007/08), and 3 districts in phase 3 (2008/09). For more details on what is known about the algorithm used to determine the district phase-in and how the intended design may have diverged from actual phase-in, see Zimmermann (2012).

[113] The panchayat village is the lowest level of administration in India followed by mandals (a term for sub-districts, or blocks, specific to AP) then districts within each of the 28 states.

6.2.2 Politics in AP

Because MGNREGS is implemented by the states and often the program signage and materials feature images of state-level political figures, like the Chief Minister, we expect that voters attribute MGNREGS spending to the political coalition in power within the Legislative Assembly, the state-level governing body.[114] At the time MGNREGA was passed in 2004, the Indian National Congress (hereafter, INC), the main party within the United Progressive Alliance (hereafter, UPA) coalition, had just wrested power from the regional party, Telugu Desam, in the AP's Legislative Assembly election. Y.S. Rajashekara Reddy (hereafter, YSR) took over as Chief Minister with an overt mission to address the agrarian crisis, an issue of contention in the run up to the election (Srinivasulu 2009). In his years in power, YSR oversaw the implementation of a large number of social welfare measures, the new MGNREGS among them. AP was the inaugural MGNREGS implementation state, further solidifying the scheme as YSR's flagship program. The state-level incumbent coalition in AP is the same incumbent coalition in the National Parliament, meaning it should be very clear to constituents that the UPA is strongly affiliated with MGNREGS program implementation.

The next election, both at the state and national level, was held in April 2009, just at the start of the 2009/10 fiscal year. Ethnographic evidence shows that the assembly constituency (AC) elections in 2009 in AP were characterized by candidates from all parties promising the distribution of funds and benefits under a number of social welfare programs (Elliott 2012), although MGNREGS is not among the schemes described. In AP, YSR was re-elected with a large margin ostensibly due, among other things, to the successful implementation of various social welfare programs. Re-election in India is rare,[115] so this signaled great satisfaction with YSR's first administration. However, soon after the elections YSR was killed in a helicopter crash and a struggle for power within the state and party ensued. After deep conflicts with members of the ruling INC party, in 2011 YSR's son, Jaganmohan Reddy, left to form his own party, the YSR Congress. In 2012, the YSR Congress successfully contested by-elections and won 16 of the 19 contested Legislative Assembly seats, with Jaganmohan Reddy himself winning a National Parliament seat and his mother, Y.S. Vijayamma, winning the State Assembly seat vacated previously on account of his father's death.

Another complicating issue in AP is the longstanding fight for state-succession by one of the three cultural regions, Telangana. Throughout his first tenure, YSR was a strong supporter of separation for these 10 districts who claim to lack representation and submit to general neglect of their needs (Ramdas 2013). Upon YSR's death in 2009, uncertainty surrounding the plan to move forward with succession meant the revival of the Telangana movement and violent protests throughout the region. The issue of a separate Telangana state eventually emerged prominent with the national government, which proposed a split of AP in December 2013. The upheaval surrounding YSR's death and the reinvigoration of the Telangana movement prompted considerable changes to the contours of the political context in AP after 2009.

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[114] To the contrary, data from the Public Evaluation of Entitlement Programmes (PEEP) Survey of 2013 shows that nearly three-quarters of surveyed households across ten states, of which Andhra Pradesh was not included, claimed that they did not know which leader was responsible for initiating MGNREGA and an additional 15 percent could not identify the correct leader. We expect AP is a special case given the match between national and state-level governing political parties and the fact that AP was the flagship MGNREGS state.

[115] Indian voters are said to possess an "incumbency aversion" (Elliott 2011; Linden 2004). As evidence, Maiorano (2014) finds that only 25 percent of incumbents throughout India successfully won re-election between 1980-2008.

6.3 Conceptual framework

Given the accumulated evidence suggesting both good implementation and the heavy-handedness of state-level and state-influenced administrators in AP, there is good reason to expect that both needs-based and politics-based motivations have been instrumental in guiding resource allocation and, ultimately, expenditures under MGNREGS.[116] Strictly, MGNREGS is a "right to work" program, not an anti-poverty program, meaning the government does not expressly target funds so much as approve, oversee, and possibly manipulate how they are spent. Self-targeting, however, implies that expenditures should be concentrated in "needier" areas.

In this context, these needs can be viewed as multi-dimensional so as to meet the immediate necessities of individuals while laying longer term foundations for rural economic growth. Because MGNREGS follows from the newly recognized "right to work" in India, we expect project funds to be spent more in areas with the need to safeguard volatile livelihoods through employment generation and the mitigation of labor market shocks, those more in line with expected demand. However, because groups of individuals with different types of livelihoods – e.g., cultivators versus agricultural laborers – have explicitly different needs, we expect expenditures to differ where one of these groups dominates the other.[117] Further, because MGNREGS activities are directed around projects, particularly as anti-drought measures, where the end result should contribute to increases in agricultural productivity, we also expect that areas with greater need for improving their infrastructure will receive more funds.

Apart from the stated aim of the program, it may be rational for policy makers to use their control over program funds to meet their potentially competing political objectives, introducing a "partisan bias in the allocation of public programs" (p. 9, Stokes *et al.* 2013). For the purposes of our analysis, we refer to this effect as the non-programmatic distribution of funds, following the distinction by Stokes *et al.* (2013). A program falls into this category when the distribution of benefits does not fully comply with criteria that are publicly available, including (although not necessarily) for partisan reasons. While MGNREGS spending criteria is not formulaic like many other government programs, we would expect that any relationship that emerges between spending levels and politics, after controlling for the factors that should dictate worker demand, is evidence of a deviation from purely demand-driven implementation.

While we presume that this non-programmatic, partisan-based distribution of funds may occur throughout implementation, there may be logical reasons that the

[116] We chose to focus our analysis on MGNREGS expenditures instead of allocations for a number of reasons: (i) allocation amounts are simply funds budgeted and may not actually be spent, potentially due to similar political economy reasons, (ii) allocations are formulaic as budgets are made based on the number of job card holders who worked in the MGNREGS in the previous year, (iii) the same state-governing body that makes final decisions about allocations also can directly influence implementation, and therefore expenditures, via the hired and perhaps politically motivated Field Assistants (Maiorano 2014), (iv) field reports show that Field Assistants may have more influence on who works under MGNREGS and when than expressed demand (Chamorro *et al.* 2010), and (v) expenditure data is theoretically less susceptible to manipulation due to the presence of social audits integrated into the MGNREGS and the fact that the information system is directly linked to actual financial transfers maintained by an external agency (not manual entries by the administration). Actual expenditures are a far more interesting variable to study; however, we use the terms interchangeably throughout.

[117] For example, cultivators depend on agricultural laborers to perform many on-farm functions and may not want a robust MGNREGS program since it may put upward pressure on wages and tighten the labor market on which they depend. On the other hand, agricultural laborers may desire a larger MGNREGS in order to secure employment opportunities, particularly in years with adverse agricultural conditions and reduced demand for hired labor.

politics-influenced expenditures may be especially acute at particular times in the election cycle. For example, leading up to a re-election, the incumbent political coalition may use project funds as a means of encouraging votes.[118] Indeed Maiorano (2014) claims that transforming state welfare schemes into a mechanism for winning re-election was part of the YSR's focus of MGNREGS in AP. The type of effect we are referring to does not necessarily imply direct "vote buying," bribing, or coercive actions taken on the part of politicians. Instead, we focus on a more general form of allocating funds to administrative areas with the intent of garnering political support.[119] After a successful election, politicians may also use their control over funds to reward their faithful constituents. Because the UPA coalition won both state and national-level re-election in 2009, we may expect that they used MGNREGS funds in the years after the election to "compensate" areas where their advantage was higher or to continue encouraging constituencies for the next election in 2014.[120] Because every given point in time sits both in front of a past election and behind a future one, there is no way to disentangle these potential political motivations.

6.4 Empirical methodology and hypotheses

In this section, we provide details on the model and hypotheses used to understand the nature of MGNREGS fund disbursements for the years surrounding the 2009 election. To estimate the extent to which state-level politics and the targetable needs of the population have influenced MGNREGS spending in the AP, we rely on one main specification but offer a range of alternative identification approaches and robustness checks, mainly relegated to appendices.

6.4.1 Politics-based distribution

To test whether the political leaning of an area influenced MGNREGS spending in AP, we estimate total MGNREGS spending per capita in mandal *i* in district *d* during fiscal year *t* using the following regression model:

$$(1) \quad MGNREGS_{idt} = \alpha_0 + \alpha_1 advantage_{id} + \alpha_2 advantage_{id}^2 + \alpha_3 \boldsymbol{needs}_{id} + \alpha_4 \boldsymbol{needs}_{idt} + \alpha_5 \boldsymbol{z}_{idt} + \mu_d + \tau_t + \varepsilon_{idt}$$

where $advantage$ captures the voting behavior of constituents in the most recent AC election (as defined below); $needs$ represents a vector of the observable "needs" of the mandal, both baseline (time-constant) ($needs_{id}$) and year-specific ($needs_{idt}$); z is a vector including other mandal-level controls, notably variables that characterize election particulars described later; μ represents district-level fixed effects which incorporate the phase during which the jurisdiction joined the MGNREGS; τ captures fiscal year fixed effects; and ε is a mean zero random error term. The fiscal years included in our analysis are the three years leading up to the 2009 election

[118] While there are several other means through which the incumbent party could attempt to encourage votes, state-level panel analysis in India by Khemani (2004) suggests that Indian politicians are more likely to target public investment projects and programs that funnel money towards small and marginal farmers by diverting funds from other areas of spending directly before elections. MGNREGS fits this set of characteristics well.

[119] This analysis builds on a long history of studies linking project allocation or spending with political objectives leading up to an election (e.g., Wright 1974; Brusco, Nazareno, Stokes 2004; Schady 2000).

[120] Like the pre-election literature, the study of post-election project allocation and politics has a long history in the political science literature (e.g., Finan 2004; Levitt and Synder 1995; Miguel and Zaidi 2003).

(2006/07, 2007/08, 2008/09) and the three fiscal years following the election (2010/11, 2011/12, 2012/13).[121] The model is estimated with data from all six years, then separately for the two time periods of interest (pre- and post-2009 election) using data described in the next section and summarized in Appendix S1 (Section 8.3).[122]

Because Indian elections are governed by a multi-party system and the candidate with the highest percentage of votes wins, we define the $advantage$ term as

$$advantage_{UPA} = \frac{votes_{UPA} - votes_{other}}{votes_{total}}$$

ranging from -1 to 1, where $votes_{UPA}$ is the total number of votes garnered by the UPA coalition in the matched AC, $votes_{other}$ is the total number of votes received by the non-UPA party with the most number of votes,[123] and $votes_{total}$ is the total number of votes cast in the constituency.[124] We define the $advantage$ term with respect to the UPA coalition because MGNREGS is a UPA flagship program and we expect that constituents will credit spending under this program to the political coalition that brought it about. We use a list of those parties that provided both "weak" and "strong" support to the UPA coalition before the elections when specifying this variable.[125] Instead of only including a linear $advantage$ term in our model, a square term is added to allow for potential non-linearities, including an inverted-U pattern, as found by Gupta and Mukhopadhyay (2014).

We utilize the AC level election results—as opposed to local level election or parliamentary constituency election results—for a number of reasons: (i) the state, led by the Members of the Legislative Assembly (MLAs), has ultimate implementation and funding authority over MGNREGS, (ii) MLAs, elected via the assembly constituency elections, in AP influence MGNREGS implementation via pressure on and oversight of Field Assistants hired by the Mandal Parishad Development Officer (MPDO), and (iii) the importance of the Field Officer role in AP means that locally elected officials play a much more marginal role than envisioned in the design of the program and perhaps in other states (Maiorano 2014).

Moreover, the mandal was chosen as our unit of analysis for three main reasons: (i) most studies about the many facets of MGNREGS to date only disaggregate to the district level which may obscure potentially important variation

[121] As the 2009 election occurred at the very beginning of the 2009/10 fiscal year, is uncertain how MGNREGS spending in that year would have been affected by the election, especially since most of the allocation decisions should have been made before the start of the fiscal year. Furthermore, 2009/10 was a drought year and was characteristic of widespread political upheaval following the death of YSR and the resurgence of the Telangana movement. For all of these reasons, we exclude the 2009/10 MGNREGS spending from our analysis.

[122] Given the district-wise phase in of MGNREGS, phase one mandals will have three observations over this time period whereas phase two mandals will have two and phase three only one. In order to ensure that our results are not influenced by this relative weighting, we also provide results by year.

[123] Where UPA lost, the total votes from the winning party are used. Where UPA won, the total votes from the second place party are used.

[124] Our definition of "advantage" differs from the often-cited definition provided by Gelman and King (1990). Our variable is also called "margins" in some work, including Asher and Novosad (2013).

[125] In the 2004 election, the UPA coalition includes 11 parties in AP: INC, MUL, RPI(A), LJNSP, RJD, RPI, TRS, CPI, CPM, AIMIM, PRBP. In the 2009 election, the coalition includes 6 parties in AP: INC, AIMIM, BSP, RJD, JD(S), SP. Independent candidates are considered non-UPA supporters throughout.

at the sub-district level, especially since MGNREGS is locally implemented, (ii) this variation is not only interesting and important but also nearly essential econometrically when confining our analysis to one state, and (iii) the elections most relevant to the case of AP MGNREGS implementation are the AC elections, constituency boundaries for which best match the mandal administrative boundaries. Since the administration of MGNREGS considers the mandal in its operation and because all data apart from election outcomes are observed at the mandal level, we keep the mandal (not the AC) as our unit of analysis.

We test the hypothesis that state-level administrators strategically spent MGNREGS funds based on their $advantage$ in the previous election, which serves as a measure of known political climate in the mandal, via the following joint null and alternative hypotheses:

$$H_O(1): \alpha_1 = \alpha_2 = 0;\ H_A(1): \alpha_1\ or\ \alpha_2 \neq 0$$

using the coefficient estimates from equation (1). If these null hypotheses are rejected, then past electoral advantage is associated with spending patterns. Because it may be the case that mandals with high levels of $advantage$ differ in unobserved ways from mandals with lower levels of $advantage$, and perhaps in a way that is correlated with $MGNREGS$ spending, we use various identification strategies as robustness checks (instrumental variables and first difference) in addition to a large number of other variable definition specification checks (see Appendix S2 (Section 8.3)).

6.4.2 Needs-based distribution

While the government is not tasked with spending based on specified criteria, we refer to and test for the presence of what we generically refer to as "needs-based targeting" using a series of variables that describe the state of the population of the mandal before MGNREGS began, all included in the vector $needs$. Because the needs of individuals and their communities may change, we use exogenous baseline characteristics from before the start of implementation for all static needs variables so as to overcome this potential issue.

We arrive at a list of variables that together encapsulate the "needs" of a mandal through several means. First, we refer partially to the task force report written by the Government of India Planning Commission (2006) which describes how districts are identified and targeted for wage employment schemes, allowing us to create variables that mimic, to a large extent, or act as proxies for this list, doing so at the mandal-level instead.[126] Second, because we are interested to study which groups have their needs considered when dispersing MGNREGS funds, especially cultivators versus (typically worse off) agricultural laborers, we include a number of variables that seek to describe the distribution of land and workers within the mandal. The variables we include describe population characteristics, the type and distribution of land within, and the infrastructure status of the mandal. We therefore find evidence of needs-based targeting if we can reject the null hypotheses:

$$H_O(2): \alpha_3 = 0;\ H_A(2): \alpha_3 \neq 0$$

[126] This report describes the following criteria as essential for selection of a district as needy: incidence of poverty, unemployment rate, agricultural wage rate, per hectare agricultural productivity, productivity per agricultural worker, SC/ST population, drought-proneness and desert-proneness, and rural connectivity.

using coefficient estimates from equation (1).

Further, we wish to understand to what extent MGNREGS accommodates the time-varying needs of the mandal, serving as a safety net against shocks, not just as a pro-poor transfer. AP is an agriculturally important and drought vulnerable state, therefore variation in rainfall levels over time is expressly important to households deriving some part of their income from agricultural cultivation or labor. In periods when rainfall is particularly bad, MGNREGS spending may increase to account for the resulting surplus ofunder-employed agricultural laborers if the needs of agricultural labors are truly considered.[127] Similar to Paxson (1992) and Dasgupta (2013), we create a rainfall shock variable for each of the two important seasons, *kharif* and *rabi*, that describes how many standard deviations from long-term average the current season rainfall level is. We conclude that MGNREGS accommodates the time-varying needs of the mandal if we can reject the null hypotheses

$$H_O(3): \alpha_4 = 0;\ H_A(3): \alpha_4 \neq 0$$

also using coefficient estimates from equation (1).

6.5 Data

The data used in this analysis come from a range of publically available sources. Because the written names of mandals and districts are often the unique observation in the underlying data sets, we successfully merged all data manually for 1,061 mandals from 22 districts in AP, about 96 percent of the 1,109 rural mandals found in these 22 districts in the Indian Population Census of 2001.[128] Definitions, data sources, and summary statistics for all of the variables used in our analysis can be found in Table S1.1 in Appendix S1 (Section 8.3).

One major feature of MGNREGS is the pursuit of transparency. To that end, an incredible amount of administrative information about projects and workers is available online.[129] Website management is handled by the state, with data input directly from the mandal administration and linked to electronic financial transactions data from the relevant institutions. While one may question the quality of government-reported project data, a major study on public works projects around the world praises the information technology system implemented by AP in particular (Subbarao *et al.* 2013), providing strong evidence that we need not be terribly skeptical of the data quality.[130] We downloaded reports from the website, which

[127] In India, there is also a process by which mandals are declared “drought stricken” and receive government funds, including more MGNREGS funds, to help with the short term crisis conditions. In AP, over 800 mandals were declared in drought in 2005, over 200 in 2006, nearly 1,000 in 2009, over 900 in 2011, and over 200 in 2012 fiscal years. However, because a government body, the Ministry of Rural Development, is in charge of these declarations and because the criterion for declaration are somewhat loosely defined, we expect politics may be a contributing factor in the decision and therefore do not consider this declaration in our analysis.

[128] There are 23 districts in AP; however, Hyderabad, the capital of the state, is excluded because it is an entirely urban district and therefore should not benefit from MGNREGS.

[129] On 11 September 2013, we downloaded mandal level spending data by fiscal year from the MGNREGS website for AP (http://MGNREGA.ap.gov.in) from the “report” section (reports/reports general/R1.6).

[130] The administrative data in AP are verified routinely through independent social audits in the gram panchayats across the state (see http://www.socialaudit.ap.gov.in). Verification exercises were also conducted by the authors in select villages in 2014 which suggest that the administrative data is reliable. Household interviews on wages earned and work done by job card holders match entries in post-office or bank books wherever these were available. Likewise individual recall data on the type of work done and number of days are also

include the total amount spent per fiscal year (April-March) at the mandal level, our dependent variable, as well as other variables used as robustness checks. We standardize these values using the rural population size.

Most time-invariant, needs-based variables come from the Indian Population Census from 2001, Indian Agricultural Census from 2005/06, and Indian Village Amenities Census from 2001, all of which were collected before the start of MGNREGS and act as a suitable baseline. Because MGNREGS is a program focused on rural employment, we limit our variables to population and land values that are observed only in rural areas, where possible. The time-varying, needs-based variables, all functions of observed rainfall levels across the two important rainfall seasons, *kharif* and *rabi*, are derived from geospatial data sets linked to mandal-level boundaries. In addition to these contemporaneous variables, we also include a measure of average and the standard deviation of yearly rainfall levels over a recent twelve year timeframe as controls for the agricultural potential of the area.

All elections outcome data were aggregated from various documents made available by the Election Commission of India, which includes number of votes by candidate and party for both the 2004 and 2009 elections. An AC can have several mandals in each and, therefore, we assign the results of the AC election to each component mandal.[131] The UPA advantage variables are created from these data. See Figure S1.1 in Appendix S1 (Section 8.3) for more details on the distribution of the advantage term across all mandals for both elections. While the advantage variable is our main covariate of interest, we include a number of control variables that seek to capture the idiosyncrasies of the AC elections. Because the AC boundaries and mandal boundaries are not always identical, we control for those cases where a mandal is split between two ACs (less than 1 percent of mandals in 2004 and 7 percent in 2009). Moreover, because we are interested in mandal-level MGNREGS expenditures, we collapse election results to the mandal level by taking a population-weighted average across the two ACs. To complicate matters, some AC boundaries were redrawn in 2008, between the 2004 and 2009 elections.[132] We therefore control for those cases where mandals contain a new or abolished AC in the regressions involving changes in UPA advantage over time (27 percent of mandals). Another feature is the presence of "reserved" elections where positions are set aside for scheduled castes and tribes (SC/STs), for which we also control. Finally, because voter turnout may be an indicator of voter awareness in India (Mookherjee 2012), we also include this value as a control.

6.6 Estimation results and discussion

In the following sections, we test our hypotheses related to the determinants of MGNREGS spending in the pre-2009 and post-2009 project implementation years. In so doing, we offer a number of potential reasons for our findings to further contextualize our results.

6.6.1 Politics-influenced distribution

First we explore to what extent the UPA funneled MGNREGS funds to areas where it had greater advantage in the previous election using data from all of the fiscal years under study, as displayed in the first column Table 6.1. This specification

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consistent with administrative data, as are the list of assets created since inception. Details on this exercise available upon request.

[131] There were 281 ACs in the 2004 election and 291 in the 2009 election, so the average AC contained 3-4 mandals.

[132] Gerrymandering is not a concern in AP.

yields a positive and statistically significant coefficient estimate on the linear $advantage$ term, but not on the quadratic term. This implies that there is no inverted U-shape or tapering of funds allocated to areas at the highest end of the advantage distribution (unlike Gupta and Mukhopadhyay 2014). As such, we reject (at the one percent level) the null hypotheses $H_O(1)$ that the $advantage$ of the UPA coalition in the previous election is not related to MGNREGS expenditure in the subsequent implementation years; indeed, the allocation of funds by AC members appears partisan.

More illuminating is running the same analysis split by pre- and post-2009 election years. The second column of Table 6.1 displays the results specifically for the first three years of the program, those leading up to the 2009 election. Due to the phase in of the program, we ensure that only those mandals eligible for MGNREGS funds in a particular year are included in the relevant fiscal year cross section.[133] Because mandals in phase 3 only started to receive MGNREGS funds directly before the 2009 election, our discussion related to pre-2009 election spending is most relevant to phase 1 and 2 mandals.

The individually and jointly statistically insignificant linear and quadratic $advantage$ terms mean that we fail to reject the null hypothesis $H_O(1)$ that politics played no part in MGNREGS fund allocation before the 2009 election. There exists no direct relationship between spending and past voting patterns, implying that UPA did not manipulate the new program by directing program resources with the express purpose of winning re-election in the pre-2009 fiscal years or rewarding their supporters in the 2004 election. These results hold under a number of robustness checks, including four IV specifications, and alternative variable specifications (see results in Appendix S2 (Section 8.3)). Additionally, because we might expect that the fiscal year directly before the 2009 election (2008/09) may have been characterized by more partisan-based spending than the earlier fiscal years, we estimate equation (1) on separate cross sections by fiscal year (Table S1.3 in Appendix S1 (Section 8.3)) but still find no year when we could reject the null hypothesis of partisan distribution.

The apparent lack of overtly politically-motivated funding in the pre-2009 years suggests that state-level administrators allowed the demand-driven nature of the program and accountability mechanisms to drive proper implementation. Instead, the ruling UPA coalition may have used the well-targeted nature of the program funds—directing them to areas with most need—as a means of drumming up support instead of simply funneling money based on how individuals voted in the 2004 election. Our extensive field research suggests that examples of both a lack of political interference and a good bureaucracy were successful in targeting the program in line with beneficiary needs during this time period. These strategies suggest that politicians perceived constituents as "retrospective voters," those influenced more by their experience with policy tools and are likely to vote for incumbents when policy outcomes have been favorable to them. If this was the approach, then it succeeded; YSR and his UPA coalition amassed great popularity and won re-election in 2009.[134]

The third column of Table 6.1 shows the results for the three fiscal years following the 2009 election, after the UPA coalition won a decisive victory in AP and

[133] It should be noted, however, that we do observe several "out of phase" mandals receiving MGNREGS funds a years before they should. This includes 6 phase 2 mandals in 2006/07 and 68 phase 3 mandals in 2007/08. While there could be political economy reasons for early phase in, this paper does not concern itself with that dimension.

[134] We explore how voters in the AC elections responded to MGNREGS expenditures in the years leading up to the election in Appendix S3 (Section 8.3), finding a positive and statistically significant relationship between the total amount of MGNREGS spending in a mandal before the 2009 election and the UPA advantage level in the 2009 election. See Appendix S3 (Section 8.3) for more details.

during which all areas were entitled to benefits under the program. In this case, we reject (at the one percent level) the null hypothesis $H_O(1)$ that the $advantage$ of the UPA coalition in the 2009 election is not related to MGNREGS expenditure in the years after the election, revealing the partisan nature of spending in the post-2009 years. Again, these results hold across a number of robustness checks, including a first difference model, as well as several sample selection and variable definition changes (see Appendix S2 (Section 8.3)). This null hypothesis is also rejected for each year when estimating separately by fiscal year (Table S1.4 in Appendix S1 (Section 8.3)). We conduct F-tests on the relative magnitude of these effects across years. When including all mandals and also when dropping mandals with a by-election in a given fiscal year,[135] we find that the 2010/11 and 2011/12 $advantage$ terms are not statistically distinguishable, but that 2011/12 is significantly larger than 2012/13. It is not surprising that 2011/12 may matter most since, with the formation of the YSR Congress that split from the main UPA coalition, it was likely the most important year politically. These events changed the contours of political alignments, and it is conceivable that it prompted parties to garner voter support in various ways, including through MGNREGS.[136]

Evidence of partisan spending in the post-2009 election years represents a departure from the pre-2009 era. We offer two possible explanations for this shift. First, it may have taken a few years for state level politicians to figure out how to use MGNREGS funds for political gain. Second, it may reflect the changing political climate in AP immediately after the 2009 election. Recall that YSR, the figurehead of MGNREGS in AP, was killed soon after his re-election and that a struggle for power ensued in the following years. Politically-influenced expenditures during this time may reflect that this disorder prompted politicians to use MGNREGS funds to secure their places in the AP political hierarchy moving forward, based on how their constituents voted in the most recent election. While our analysis is unable to uncover which mechanism is more likely, it does point to the important distinction between the pre- and post-2009 years.

Across all post-2009 years, we estimate an average partial effect of $advantage$ at 0.36 (significant at the 1 percent level), meaning a 1 percentage point increase in the $advantage$ of the UPA coalition in the 2009 election is correlated with about a 4 rupee per capita increase in annual MGNREGS expenditures in the years after the election. Given we observe an average MGNREGS allocation per capita of about 540 rupees in any given fiscal year (Table S1.1 in Appendix S1 (Section 8.3)), this means that a 1 percentage point increase in UPA $advantage$ is correlated with a less than 1 percent increase in the total MGNREGS funds allocated to a given mandal in the post-election years, a magnitude that is only sizable when considering relatively high levels of UPA $advantage$ or mandals where per capita expenditure levels are much larger than average. While hypotheses testing provides solid evidence for the existence of partisan-based spending, the magnitude and economic significance of these effects appears very small on average.

6.6.2 Needs-based distribution

In this section we investigate the hypothesis that MGNREGS funds were allocated based on various needs of the mandal. With respect to the baseline (time-

[135] By-elections occur when an elected member dies, resigns, or is disqualified. There were 11 ACs with a by-election in 2010/11, 2 in 2011/12, and 16 in 2012/13.

[136] The post-election results are also in line with estimates of a model that investigates how political turnover between UPA and non-UPA parties across the elections influenced MGNREGS spending. For more details, see Appendix S4 (Section 8.3).

invariant) labor-related needs described in $H_O(2)$, we find that mandals with a higher percentage of illiterate individuals received more funds across all panel model specifications but that areas with more SC/ST households received more funds only in the post-2009 years. Because we expect that lower caste and illiterate individuals are likely to require assistance through government programs like MGNREGS on account of their relative poverty and employment levels, these findings suggest that MGNREGS expenditures were targeted to the poorest and neediest areas both before and (even more so) after the 2009 election, after which time all districts had phased into the program. The fact that the SC/ST term becomes significant only in the post-2009 years may be an indication of increasing program awareness over time among this marginalized sub-population and/or the later inclusion of sanctioned projects on private SC/ST lands.

The coefficient estimates on the percentage of primary agricultural laborers and cultivators, on the other hand, show a changing story before and after the elections. Across most specifications, we find that mandals with a higher percentage of agricultural laborers receive more funds in the pre-election period but a lower amount of funds in the post-election period, with the opposite relationship observed for primary cultivators. Because the drought in 2009 and the after-effects were more severe than anything experienced in the pre-2009 years, these results may reflect that even primary cultivators required MGNREGS as a coping mechanism. Still, the fact that we observe an unexpected relationship with respect to casual agricultural laborers, another portion of the population requiring additional income in poor agricultural years, suggests that some of the well-targeted nature of MGNREGS eroded after the election and/or workers became discouraged over time due to rationing and delays in wage payments.

The coefficients on the static variables related to land or acting as proxies for the agricultural potential of the area suggest that these characteristics were also strong considerations when distributing MGNREGS funds to mandals. Mandals with a higher percentage of unirrigated land received more funds, which is not surprising given that land improvement and irrigation projects supposedly accounted for over 75 percent of total MGNREGS projects in AP (Deininger and Liu 2013). This implies that the funds were targeted to areas that stood to gain from the type of infrastructure projects facilitated by MGNREGS. Moreover, areas with more farms that fall into small or marginal size categories and areas with more inequality in land holding size receive less MGNREGS spending. Higher inequality in land ownership is indicative of fragmented rural communities where rich landowners have power over the landless. Our findings suggest that major landowners were able to lobby to keep MGNREGS less relevant in their communities.[137]

Our final set of static covariates describe other measures of infrastructure that likely function as proxies for a range of additional needs-based variables. We find that areas with more agricultural credit opportunities (a proxy for the robustness of agricultural institutions) and mandals containing more villages with medical facilities and paved approach roads (general infrastructure variables) receive less funds per capita. On the other hand, mandals with more remote villages receive more funding per capita. The direction and significance of these covariates are nearly identical in the years both before and after the 2009 election, suggesting spending has been well-matched to areas with more infrastructure needs across time.

[137] It is important to note that political party allegiances in AP are not split between the landed and the landless (also important for the interpretation of our results on agricultural laborers versus cultivators). Both rival political coalitions in AP have "vote banks" among powerful landed communities (Reddys and Kammas), before MGNREGS (Srinivasulu 2004; Suri 2002) and during the 2009 election time (Suri, Rao, Reddy 2009).

We also investigate our hypothesis related to the flexibility of MGNREGS to accommodate time-varying needs, $H_O(3)$, namely changing labor market dynamics between agricultural seasons and years, embodied in the rainfall shock in the current *kharif* and *rabi* seasons. In the pre-2009 election years, we observe that areas with less than average rainfall in both seasons were more likely to receive more funds and, for the *kharif* season, we also find a positive and significant relationship where the magnitude of those negative shocks was highest.

In the post-2009 years, the relationships are not as well-behaved. We observe that areas with more substantial below average *kharif* rainfall shocks receive more funds than those areas with less significant negative rainfall shocks. In the *rabi* season, areas with higher negative rainfall shock receive less funds. The post-2009 period, however, should function as a period where the rainfall needs were considered even more since "drought-affected mandals" were supposed to receive more money starting in 2011 via an increase in the number of days individuals were eligible for work (100 to 150) when rainfall levels were far below average. Using our exogenous rainfall shock variables, we find that mandals with higher rainfall shock in the *kharif* season may have benefited from this policy change, but that negative *rabi* season anomalies were not correlated with more MGNREGS spending in the post-election years. This is particularly unfortunate since the areas with higher percentages of agricultural laborers received fewer funds, meaning those households who rely more on casual agricultural labor opportunities may have had more difficulty earning income in the *rabi* seasons of these post-2009 years.

6.6.3 Summary of MGNREGS spending results

As a final exercise, we seek to understand which groups of variables (as categorized in Table S1.1 in Appendix S1 (Section 8.3)) were most strongly correlated with the distribution of program funds. To do this, we calculate Shapley values using the regression estimates from equation (1), which decompose the explained variance (measured by R2) into contributions over particular groups of regressors (Huettner and Sunder 2012). In other words, we calculate the mean marginal contribution of each group of variables to the overall model R2. These estimates for all years, pre-2009, and post-2009 model specifications are presented in Table 6.2.

Across all included fiscal years (column 1), we find that the voting variables that allow us to measure non-programmatic distribution can explain only about one percent of the variation in MGNREGS spending levels. By contrast, the four categories of variables that together encapsulate the needs of the mandal explain more than 43 percent of the variation. In the post-election period (column 3), where our results suggest that partisan politics had a stronger relationship with MGNREGS spending than in the pre-election years, we still find that the needs of the mandal far dominate the variation explained by the election variables. Indeed, even as the importance of the $advantage$ variables climbs to only 2.5 percent, the needs variables become even better predictors when all mandals are eligible for MGNREGS, explaining more than 63 percent of variation in expenditure patterns. This decomposition exercise also uncovers the fact that the statically observed needs-based variables are jointly better predictors of MGNREGS funding levels than the time-varying rainfall variables, suggesting that expenditures have not responded very flexibly to changing labor market dynamics over time, although they do flow to poorer areas more generally.

Our results do not offer guidance on why the non-programmatic distribution of funds appears more likely – although insubstantial – in the post-election years. Nonetheless, the fact that politics does not appear to have a major influence on spending levels suggests that the self-targeting, transparency, and accountability mechanisms integrated into the MGNREGA – including widespread information disclosure and social audits – reduce the potential for larger-scale political capture to take hold in AP. While we cannot establish any direct causal relationship between these safeguards and our results, we offer our findings as evidence, particularly alongside more local-level findings in AP by Johnson (2009b), that MGNREGS is less manipulable than public discourse suggests. The fact that preferential spending does not seem to have been a major contributor to MGNREGS spending is likely due to the fact that a transparent top down approach that functions through a strong bureaucracy offers few opportunities for local political operatives to capture rents. This offers an important lesson for program administrators everywhere.

6.7 Conclusions

India's innovative and massive Mahatma Gandhi National Rural Employment Guarantee Scheme was designed as a demand-driven program rooted in the constitutional "right to work" and incorporates a number of accountability and transparency mechanisms aimed at limiting the extent to which politics can influence program spending and implementation. The degree to which these intentions have come to bear is a question worth exploring, both for improving MGNREGS and for designing other major government-funded programs around the world. With great heterogeneity within India, we focus further on the experience in Andhra Pradesh, one state where implementation is heralded as a "success story" and where the political climate mimics the national level. By testing hypotheses related to covariates that broadly describe the "needs" of the mandal alongside voting trends at the assembly constituency level in both 2004 and 2009, we provide the first quantitative study to our knowledge that explores if partisanship among the UPA coalition at the state-level, where MGNREGS funding is approved and priorities are set, influenced mandal-level MGNREGS expenditures between 2006/07 and 2012/13.

We do not find evidence that the political leaning of a mandal before the 2009 election influenced MGNREGS expenditure levels, but do find consistent evidence (although with an effect small in magnitude) that the distribution of funds after the election was partially politically motivated, either to reward their loyal constituencies for their successful 2009 election or to encourage further support in the 2014 election. We offer two possible reasons for the late emergence of partisan effects: (i) a slow learning process among state-level politicians on how to allocate funds with political objectives in mind, and/or (ii) the political struggle that occurred in AP following the sudden death of the re-elected Chief Minister from the UPA coalition. The political stakes were especially high in the post-2009 election years, suggesting that even programs with considerable safeguards do become more susceptible to political influence where there is more to gain. Alongside these findings, we also observe that expenditures were well-aligned with the needs of the mandal, especially characteristics of the population, land, and infrastructure but also the changing labor market dynamics across years and agricultural seasons. Even in the post-2009 election period where past voting outcomes is a major correlate, we still find that the needs of the mandals explain far more of the variation in MGNREGS expenditures than the political variables. Moreover, we find evidence that aggregate MGNREGS spending in the pre-2009 election years is positively related to a shift in voting patterns towards the UPA coalition in 2009 (see Appendix S3 (Section 8.3)), implying that voters "rewarded" the governing coalition for implementing a well-targeted program in the initial years, evidence that overt partisanship in implementation was unnecessary to secure their win.

This paper contributes to the political economy literature by exploring the relationship between election outcomes and spending on large-scale government-sponsored programs. We also offer a set of testable hypotheses for investigating the incidence of partisan-based non-programmatic distribution, as defined by Stokes *et al.* (2013). Moreover, our findings shed light on the ongoing debate about the most appropriate level for managing public programs—central versus local—especially given concerns about political capture (e.g., Bardhan and Mookherjee 2005; Bardhan 2002). Our results suggest that even a heavy-handed central governing apparatus, not envisaged under MGNREGS but evolved as such in AP, can deliver benefits well-aligned with constituent needs. While we cannot test whether the "top down" approach is optimal, our results do indicate that centralized spending can be done benevolently. The state-level AP government took the power originally granted to local-level institutions because they perceived these bodies to lack the capacity necessary to implement a program as complex as MGNREGS (Maiorano 2014). But even this "top-down" strategy may only work when state-level authorities have sufficient experience themselves and are able to ensure that accountability mechanisms are not forsaken, implying that complementary investments in state-level capacity may be critical in some instances.

AP makes an important case study because it took a "bottom up" program design and built a "top down" implementation architecture above it. If we cannot find evidence of deep manipulation within this arrangement, then this suggests that a purely decentralized and local implementation approach is not the only way to achieve proper targeting and service delivery to marginalized citizens in other contexts. This evidence offers the possibility of meaningful devolution of program implementation attached to a bureaucratic structure that ensures local political actors do not hold a program hostage. While a useful testing ground for our hypotheses, the AP experience is in no way exceptional; it should be replicable not only within other MGNREGS environments but also across other contexts and programs that want to achieve the dual goals of local ownership and efficient implementation. Further study of the determinants of MGNREGS spending, particularly from states where implementation is less well-regarded, could shed light on which elements of program design limit politically-motivated spending in order to craft a program that contributes to poverty reduction and economic growth.

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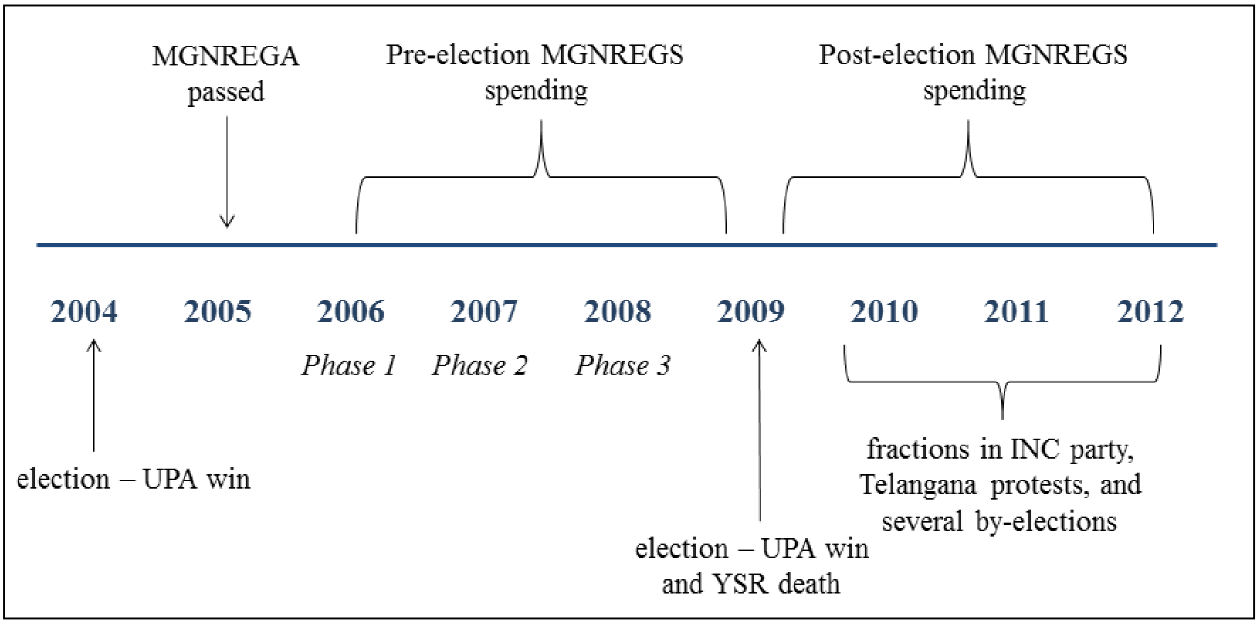
6.9 Figures and tables

Figure 6.1: Timeline of MGNREGS project implementation and political situation in Andhra Pradesh



Notes: Refer to Section 2 for more details.

Table 6.1: Regression results for MGNREGS expenditure models, preferred specification

	(1) All years (except 2009)	(2) Pre-2009	(3) Post-2009
UPA advantage in previous election	0.243***	-0.0144	0.357***
	(0.0528)	(0.0437)	(0.103)
UPA advantage in previous election, squared	0.202	-0.0297	0.269
	(0.186)	(0.146)	(0.413)
SC/ST percent (%)	0.00273**	0.000661	0.00406***
	(0.00107)	(0.000838)	(0.00149)
Illiterate (%)	0.00727***	0.00456***	0.00883***
	(0.00125)	(0.00108)	(0.00173)
Agricultural laborers (%)	0.00277*	0.00325**	0.000875
	(0.00160)	(0.00136)	(0.00215)
Cultivators (%)	0.00672***	0.00222	0.0102***
	(0.00217)	(0.00163)	(0.00305)
Unirrigated land (%)	0.000983***	0.000746***	0.00105**
	(0.000311)	(0.000247)	(0.000448)
Landholdings that are small/marginal (%)	-0.00427***	-0.00331***	-0.00504***
	(0.000989)	(0.000821)	(0.00136)
Land gini coefficient	-0.703***	-0.532***	-0.903***
	(0.198)	(0.163)	(0.281)
Long run average yearly rainfall (mm/hr)	2.214**	1.209	2.640*
	(1.056)	(0.807)	(1.480)
Long run st. dev. yearly rainfall (mm/hr)	5.078**	3.450**	6.245**
	(2.055)	(1.591)	(2.923)
Number of ag credit societies (in 1000s)	-0.00937***	-0.00619***	-0.0122***
	(0.00211)	(0.00142)	(0.00285)
% of villages with medical facilities	-0.162***	-0.177***	-0.166**
	(0.0567)	(0.0497)	(0.0780)
% of villages with paved road	-0.0727*	-0.0589*	-0.0870
	(0.0426)	(0.0341)	(0.0603)
Distance to nearest town from village	0.00219***	0.00103***	0.00293***
	(0.000379)	(0.000299)	(0.000562)
Kharif season rain < average (1=yes)	0.0105	0.0331***	-0.0309
	(0.0111)	(0.0122)	(0.0208)
Kharif < average * rain shock (abs. value)	0.0613***	0.0315***	0.0988***
	(0.0107)	(0.0106)	(0.0156)
Rabi season rain < average (1=yes)	0.00582	0.0222*	0.00189
	(0.0122)	(0.0124)	(0.0214)
Rabi < average * rain shock (abs.value)	-0.172***	-0.0279	-0.232***
	(0.0171)	(0.0184)	(0.0250)
Voter turnout in previous election (%)	-0.00482***	-0.00191	-0.00299
	(0.00143)	(0.00122)	(0.00222)
SC/ST reserved previous election (1=yes)	0.00171	0.0187	0.00646
	(0.0160)	(0.0150)	(0.0239)
Split between ACs in previous election (1=yes)	-0.0676**	-0.0919**	-0.0520
	(0.0310)	(0.0386)	(0.0324)
Constant	0.253	0.327*	0.712**
	(0.220)	(0.176)	(0.333)
Year dummy variables	Y	Y	Y
District dummy variables	Y	Y	Y
Observations	5,753	2,570	3,183
R-squared	0.508	0.495	0.509
Joint sig. of UPA advantage vars (p-value)	0.000	0.934	0.002

Note: *** p<0.01, ** p<0.05, * p<0.1. All reported results are estimated per Equation 1. Pre-2009 years include 2006/07, 2007/08, and 2008/09. Post-2009 years include 2010/11, 2011/12, 2012/12. Standard errors, shown in parentheses, are clustered at the mandal level (i=1,061). Only 1,039 of 1,061 mandals are included in the pre-2009 election years because there was no UPA coalition candidate in ACs matched to 22 mandals. Results split by fiscal year can be found in Appendix 6.3 (Section 8.3). A large number of robustness checks on these results can be found in Appendix 6.4 (Section 8.3).

Table 6.2: Decomposition of R2 for MGNREGS fund expenditure models

	(1) All years	(2) Pre-2009	(3) Post-2009
Non-programmatic distribution	1.0	0.1	2.5
Needs-based: labor-related	13.6	9.7	22.9
Needs-based: land-related	13.2	12.8	16.7
Needs-based: infrastructure-related	13.8	11.8	20.2
Needs-based: rainfall-variability	2.7	2.8	3.9
Election controls	2.2	2.3	3.1
District and year dummies	53.5	60.5	30.7
R-squared	***0.5076***	***0.4948***	***0.5077***
Observations	***5,753***	***2,570***	***3,183***

Notes: The included numbers represent Shapley values, or the percentage of the R2 that can be explained by a particular group of regressors. We calculate these values using the "rego" user-written command in Stata. See Table 6.3.1 in Appendix 6.3 (Section 8.3) for which variables are included in each of the six categories. The relevant matched regression results for these estimates are the specifications displayed in the matched columns of Table 6.1 (Equation 1 estimated with OLS).

7. Political Clientelism and Political Activism: Evidence from an Indian Public Works Program

Nancy H. Chau, Yanyan Liu, Vidhya Soundararajan

7.1 Introduction

Political parties are known to strategically redirect public resources through decentralized programs to secure and expand their party base. In a large and longstanding literature, such activities have been referred to as tactical redistribution, and/or political clientelism (Downs, 1957; Wright, 1974; Wyatt, 2013; Dixit and Londegran 1996). What determines the identity of the so-called swing voters?

In classic probability voting models, swing voters typically refers to those closest in the vicinity of the center of the political spectrum, since transfers to this group leads to a greater increase in political support than groups with more extreme ideological attachments (Lindbek and Weibull, 1987, Dixit and Londregan, 1996, 1998, Stokes, 2005). Other swing voter characteristics have included political affiliation (Cox and McCubbins 1986) and the tendency for reciprocal behavior (Finan and Schechter 2011), for example. Notably, voters in these earlier studies exclusively play the passive role of recipients of political transfers and information, and are otherwise not involved in the political process.

Contrary to the swing-voter theory which takes political preference as given and voters as passive in the political process, evidence to date suggest that voter preferences are mediated by voter information (Grossman and Helpman 1996, Wantchekon 2003) and clientelistic transfers have been shown to change depending on voter awareness about the political process and the practice of vote buying (Vincente 2014), and voter awareness about the qualification of candidates (Banerjee et al, 2011). Furthermore, not all voters are passive in the political process, and indeed, politically connected households or firms are often beneficiaries of political clientelistic transfers. Das (2014) addresses self-selection in job seeking under the MGNREGS and demonstrates that households affiliated with the local ruling political party and were politically active received more MGNREGS jobs than others in West Bengal. Besley, Pande and Rao (2005) and Markussen (2011) show that in southern Indian states, households where politicians reside and those affiliated to the village president's party are more likely to hold a Below Poverty Line card, respectively.[138]

To the extent that citizen political activism can raise / alter public awareness about politics and politicians, these observations raise the intriguing possibility that politicians may target of clientelistic transfers specifically to citizen political activists in order to engender political support from other swing voters and/or the political activists themselves. To date, while evidence of political clientelism abound, there has been very few theoretical studies in the area of political clientelism. One notable exception is Bardhan and Mookherjee (2012) which extends classical models of probabilistic voting by introducing clientelism in a setting where a public and multiple private goods can be used as transfers. In Robinson and Verdier (2013), politicians offer jobs rather than income or public goods as transfers so as to link the future utility of a voter to the electoral success of the ruling politician. Neither considers the potential role of targeting transfers to politically active voters with the explicit goal of altering the intensity of their political participation, and/or the tenor of their political

[138] More generally, there is a strong empirical literature examining clientelism at the aggregate level such as state, district, district sub-division (mandal or block), village, and election constituencies (Schady 2002; Case 2000; Sheahan et al. 2014, Gupta and Mukhopadhyay 2016; Dahlberg and Johansson, 2002; Asher and Novosad, 2015; Khemani 2004; Himanshu et al. 2015; Fried 2012).

campaigns.

Using a simple model of political clientelism, this paper contributes to this literature by introducing heterogeneous voter identities: i) swing voters who choose political allegiance when presented with transfers and/or information, and (ii) citizen political activists whose political participation can change the political allegiance of *other* voters. Given the opportunity, do politicians opt to influence voters by offering preferential transfers directly to swing voters, or to citizen political activists, or to both? The importance of citizen political activism in western democracies is a longstanding area of inquiry (Almond and Verba 1963). This study provides for the first time an empirical study on this issue from a developing country perspective, in the context of a decentralized public works program, the Mahatma Gandhi Rural Employment Guarantee Scheme in the southern Indian state of Andhra Pradesh, arguably one of the largest workfare program in the developing world as motivating evidence.

Our core data set is from a primary survey conducted in Andhra Pradesh by the World Bank in 2006.[139] It comprises 1,152 households in villages led by *sarpanch* (leaders) affiliated to the United Progressive Alliance (UPA) coalition of parties. We merged our survey with the administrative data on MGNREGS workdays and payments, using MGNREGS job-card IDs reported by the households in the survey.

In our empirical study, a key innovation is in tackling reverse causality by exploiting the timing of our survey which uniquely captures household political affiliation before they received MNREGS benefits. The MGNREGS was implemented in a staggered non-random fashion through the entire country in three stages. It came force in the 200 most backward districts in February 2006 (phase - I), extended to an additional 130 districts in April 2007 (phase-II), and all remaining rural districts in April 2008 (phase-III). Our survey in August 2006 captures political affiliation of households around or before the commencement of the MGNREGS program in phase-I and phase-II villages respectively. Using a rich set of explanatory variables that control for unobserved heterogeneity at the household level such as poverty status, land ownership, occupation, gender, and education of the household head, political involvement, awareness levels, and village level fixed effects, we effectively causally estimate the effects of political affiliation in 2006 on MGNREGS work and payments received cumulative in 2006 and 2007. Further, our analysis also explicitly differentiates outcomes based on the actual political party of the local leader (like Das 2014), a distinction that critically differentiates the leader's capacity to engage in clientelism, especially if certain limbs of program implementation are top-down like ours (more below).

Our results show robust evidence for political support-buying in a village economy. In villages governed by a UPA-*sarpanch*, compared to UPA-inactive households, UPA-rival active, unaffiliated, UPA-rival inactive, UPA-active households, in that order, obtained significantly higher days of work and payment cumulatively in 2006 and 2007. There are two interesting observations here. First, these results starkly contrast other studies on India which show that leaders patronize loyalist households by offering them more benefits, compared to swing groups or rival party supporters. Second, within UPA and UPA-rival parties, active members are targeted

[139] While clientelism could be studied at an individual or aggregate level, we pursue the former approach. Micro-level studies can potentially offer alternate perspectives compared to the aggregate level studies even in the same context. In case of decentralized programs, since local village bodies form the lowest body of governance and the point of contact for potential beneficiary households, the factors influencing such bodies to favor one household over the other under clientelistic programs, may differ from those influencing a bureaucrat to allocate program funds, for instance, across different states, districts or other higher-level governing bodies.

more than inactive members.

In exploring the mechanisms that drive these results, we find evidence that *sarpanch*es target active members because they are expected to act as political agents who can influence other households, consistent with the theory. Active households living in villages with a high proportion of politically active households receive less benefits than active members in villages with a low proportion of politically active households. This makes sense because from the *sarpanch*'s point of view, there is a bigger bang for the buck in less-active villages where there are more people for active members to target and influence. We interpret these as suggestive evidence that politicians target preferential transfers to two types of voter identities singled out in this paper.

While our rich set of control variables and the timing of our survey addresses most endogeneity concerns in our empirical model, some reverse causality concerns still remain in phase-I villages where some households may have received benefits between the program commencement and the interview, which can in turn influence their political affiliation[140]. To address this concern, we show that our results remain robust by repeating our analysis by dropping households who received MGNREGS benefits between program commencement and our survey month in phase-I villages. Further, since our survey does not capture households' job seeking activities under the program, endogeneity concerns arise to the extent that job seeking is correlated with political affiliation and activism. To test if this is the case, we examine if there is clientelism in job-card ownership, given that job-card ownership is a strong indicator of job-seeking because job-card is a prerequisite to MGNREGS employment, is free of charge and straightforward to obtain. Our results show no evidence of clientelism in job card ownership, which alleviates endogneity concerns.

The next section introduces the MGNREGS in India and Andhra Pradesh (section 7.2). Subsequent sections explain the theoretical model (section 7.3), describe the data (section 7.4), present the methodology (section 7.5), and present the results (section 7.6). We conclude in section 7.7.

7.2 The Mahatma Gandhi Rural Employment Guarantee Scheme

The MNREGS was a flagship program of the United Progressive Alliance (UPA) that held power in the India's central government for two consecutive five-year term periods spanning 2004 to 2009 and 2009 to 2014. The UPA was a coalition of center-left political parties that held power both at the center and in the state of Andhra Pradesh in our study period UPA members in 2004 state elections in Andhra Pradesh included the Indian National Congress (INC), Telangana Rashtriya Samiti (TRS), Communist Party of India (CPI), CPI (Marxist), and Majlis-e-Ittehadul Muslimeen (MIM). The non-UPA parties are the Telugu Desam Party (TDP) and the Barathiya Janata Party (BJP) comprised the UPA-rival group. We use the UPA coalition definition from footnote 11 in Sheahan et al. (2014).

Unlike other social welfare policies, MNREGS obtained constitutional recognition and came into force as a law in September 2005. In 2012-13, the program generated 2.3 billion person-days of employment for 49.9 million households nationwide, from a budget of USD million 47.93. Adults in rural households can demand up to 100 days of employment to be shared among its members. Infrastructure works like water conservation and water harvesting; drought proofing including afforestation; irrigation works; restoration of traditional water bodies; land development; flood control; rural connectivity, and other works notified by the

[140] This concern does not exist for phase-II villages where the MGNREGS program started after our survey.

government, are some important works permissible under the radar of the scheme. Further, the act sets a minimum limit to the wages to be paid on a time-rate basis or on a piece-rate basis, without gender discrimination. Certain transparency and accountability measures are supposed to be in place, through mechanisms like "squaring of accounts", conducting social audits to ensure accountability through public vigilance and participation of civil society, and finally the maintenance of records by the implementing agencies and ensuring their availability for evaluation and scrutiny.

Andhra Pradesh offers an interesting case to study because of its inherent political dichotomy. On the one hand, the state endeavors to stay transparent and efficient in implementing the MNREGS and initiated several measures to ensure accountability through real-time availability of data, and improved channels for public vigilance and civil society participation (Aiyar and Samji, 2009; Subbarao et al. 2013). On the other hand, the state's cash-for-vote electoral politics stunningly stands out from the rest of the country. Andhra Pradesh leads the Indian states in term of the total money seized during elections, a phenomena particularly acute in local elections (Centre for Media Studies, 2014)[141]. In this context, we examine the presence of clientelistic targeting under the seemingly transparent MNREGS program, by studying the effects of household affiliation to particular political party or coalition, on their receipt of MNREGS work and payment.

Figure 7.1 presents various stakeholders and within state work flows under the MNREGS as per the federal guidelines. Households request and obtain a job-card, which forms the basis of identification, and is a legal document where number of days worked are recorded in order to claim wages. Job-card holders can seek jobs to the Gram Panchayat (GP), the village level body or block office, stating the duration for which work is sought. Work requests from households are consolidated into a shelf of projects by the village gram panchayat, headed by the sarpanch, and presented to the officers at the block level (district sub-division). The block panchayat and programme officer scrutinizes and consolidates GP's plans and appeals for funds from the district headquarters. The district panchayat and programme officer in turn ensure administrative and technical approvals for this shelf of projects and release funds accordingly. The state apex body, State Employment Guarantee Council (SEGC) nominates the list of proposed works to the central government. Note that, the work generated under the scheme is initiated by household requests after which it goes through various local bodies for approval, before coming to a full circle back at the village level. In short, the entire scheme is supposed to work bottom-up.

In the villages of Andhra Pradesh, apart from the sarpanch (village leader), a state-appointed 'field assistant' (FA) (not elected) administers the scheme. Although as per the books, the FA is only required to measure works and maintain registers, his/her actual role seems to more powerful than that (Chamorro et al. 2010). Nevertheless, gram panchayat's stake in MNREGS implementation remains largely crucial because the sarpanch is concerned about support, vote-bank and re-election. With the clout they enjoy, UPA affiliated sarpanches could forge alliance with or control the state appointed FAs, who are already likely to be supporters of the UPA (Maiorano 2014). In this case, FAs and sarpanches are natural partners in action and plausibly work to achieve the same objectives (expanding UPA's support bank). In contrast, non-UPA sarpanches may seem weaker and not have the access to resources to engage in clientelism. Since these sarpanches are not natural allies with FAs, outcomes here are fuzzy and could be a result of power play between the two.

Andhra Pradesh's MNREGS endeavors to stay efficient and transparent in the

[141] Further, more than half the voters in Andhra Pradesh are distributed cash on eve of elections, and the state witnesses highest per-voter cash transfer in India.

middle and lower echelons of the state government. Several initiatives, along with continued support from the top-level state administration resulted in great success in generating jobs and obtaining people's trust. The state of Andhra Pradesh, unlike most others, initiated and institutionalized transparency and efficiency measures for improved functioning of the scheme. These measures along with AP' political commitment is often cited as an important reason for its successful track of job creation (Maiorano 2014). For example, social audits were mandated through the Strategy and Performance Innovation Unit, instituted under the state's department of Rural Development (RD) for better transparency. Efficient and honest officials were deliberately inducted into the RD department to ensure commitment and success of the MNREGS. Further, detailed records of each MNREGS participant were made publicly available over the Internet (Johnson 2009), making it the only state to have implemented an advanced information system for tracking participation data.

Despite these transparency and efficiency measures, and the political commitment, AP MNREGS lacks bottom up planning (Maiorano 2014; Chamorro et al. 2010). The set of works is decided not entirely based on demand, but after political priorities and technical feasibilities receive their due considerations. For example, Scheduled Caste/Scheduled Tribe's [142] private land development was a priority of the state government, owing presumably to the former's poor economic status and to retain their contribution to the wide support base for congress party [143]. That most villages cease offering employment in the agricultural peak season at the behest of a powerful farmer lobby supported by the state government, is another illustration of top-down planning [144]. Lastly, a Member of the Legislative Assembly (MLA)'s [145] affiliation to the Indian National Congress is a strong correlate of employment generation under MNREGS in that constituency, perhaps because of the presence of honest and competent officials in those constituencies (Maiorano 2014) [146].

Figure 7.2 details the within-state implementation details specific to Andhra Pradesh. Besides the sarpanch, a prominent figure in AP villages managing the MNREGS implementation is a field assistant who is chosen by the Block Development Officer (BDO) from a list of candidates that includes the GP's shortlist (up to three members) and any other "eligible candidates" directly applying to the BDO (Government of Andhra Pradesh, 2006). While the books suggest that role of a field assistant is limited to assisting the panchayat secretary[147] in maintaining records such as work muster rolls, materials procurement-consumption register, and measuring work done by households (Government of Andhra Pradesh, 2006), field assistants play a more powerful role in implementation than that anticipated in design

[142] The Scheduled Castes (SCs) and Scheduled Tribes (STs) are official designations given to various groups of historically disadvantaged people in India. The terms are recognized in the Constitution of India and the various groups are designated in one or other of the categories.

[143] Maiorano (2014) notes that over a quarter of work occurring in SC/ST's private lands "were not required or not meeting owner's needs."

[144] Figure 7.1 in Maiorano (2014) shows a notable dip in employment between July and December in many states

[145] The Legislative Assembly is the lower house of the state legislature in a bicameral system, whose members are representatives of people chosen through direct state-level elections.

[146] "In 2011–12 the average number of person-days generated per household was 67.07 in Congress MLA's constituencies as against 54.95 in non-Congress ones. Among the top ten performers, seven are Congress constituencies, which include those of the present chief minister and two cabinet ministers. Conversely, among the worst ten constituencies, only two belong to the Congress." (Maiorano 2014)

[147] "The Panchayat Secretary at the village level, is a staff working in the Gram Panchayat office in administrative tasks like recording decisions, keeping minutes, preparing budget estimates and reports, and does other sundry jobs like preparing notices, explaining circulars, organizing Gram Sabha meetings etc." source: http://www.yourarticlelibrary.com/politics/the-three-tier-system-of-panchayati-raj-in-india/4827/

(Chamorro et al. 2010). Maiorano (2014) notes that FAs provide job-cards and jobs, and decide on the list of projects, all of which are duties of the gram panchayat (of which sarpanch is a member) as per the operational guidelines.

Despite the presence of these two stakeholders, village sarpanches have an important stake in the implementation of MNREGS – to demonstrate performance and/or to appease their constituents – arising from their need to renew their positions of power. They can control or ally with FAs depending on their power position within the village and beyond, to influence project planning, job allocation, and payments. That the GPs also short list candidates in choosing FAs, indicate their leverage on FAs. Since FAs are state appointed, we can safely assume their allegiance to UPA-sarpanches (even though their political explicit affiliations are unavailable).

In what follows, we theoretically model the problem that a UPA sarpanch – the ruling party politician – face in an attempt to bolster political support through the tactical distribution of MNREGS benefits. Our objective is to extend canonical models of political support to account for voter level characteristics that may alter the pattern of political clientelism.

7.3 A Simple Model of Political Clientelism

There are two political parties, respectively the ruling party and the rival party. The electorate contains a large number (N) of heterogeneous voters. Voter heterogeneity originates from three sources: differences in (i) political preferences p, (ii) the propensity to participate in political activities k, and (iii) income y.[148] Denote $G(y)$ as the cumulative distribution function of y across the N heterogeneous voters, independent of political preference and activism. Each voter takes his own p, k and y as given, and chooses a political affiliation (a), to support either the ruling party (o), the rival party (r), or remain unaffiliated (u).

Within this structure of partisan politics, each voter exhibits one of three types of political preferences $p = o,r,u$, respectively pro-ruling party, pro-rival party, and neutral preferences. The fraction of voters in the electorate with political preference p is given by $\phi^p \in (0,1)$, wit $\sum_{p=o,r,u}\phi^p = 1$. Let θ_a^p be a preference parameter reflecting the desirability of political affiliation a for a voter with political preference p. In our context, the three types of political preferences respectively imply a preference ordering over $a = o,r,u$ for each p such that $\theta_p^p = \max\{\theta_o^p, \theta_r^p, \theta_u^p\}$.

We assume that subsets $\phi_{\bar{k}}^o > 0$ and $\phi_{\bar{k}}^r > 0$ of respectively the ϕ^o and ϕ^r number of voters with respectively pro-ruling and pro-rival party preferences are politically active ($k = \bar{k}$). The rest of the voters, respectively $\phi_1^o = \phi^o - \phi_{\bar{k}}^o$ and $\phi_1^r = \phi^r - \phi_{\bar{k}}^r$, are not politically active $k = 1(<\bar{k})$. Voters with neutral political preferences naturally do not engage in political activities, and thus $k = 1$ for these individuals, and thus $\phi_1^u = \phi^u$ and $\phi_{\bar{k}}^u = 0$. Thus, we assume that k is an individual attribute. We do so to incorporate situations where engagement in political activism is driven by the intensity of an individuals' political preference, or commissioned by political parties, or both.

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[148] In our empirical analysis, we include a vector of socio-economic characteristics in addition to income. These are discussed in detail in Section 7.4.

Let the preference function of each member of the electorate as:

$$\omega_k^p(a;y) = y + k^\gamma \theta_a^p, \quad \text{if } a \neq u$$
$$= y + \theta_u^p \quad \text{otherwise,} \tag{1}$$

where $\gamma \geq 0$ is strictly positive if voter-cum-political activists have more intense political preference than pure voters, and zero otherwise.

As a benchmark scenario where politically motivated transfers are absent, voter utility maximizing political affiliation (a) depends only on political preferences p. Thus, the number of voters who prefer political affiliation $a = o, r,$ and u respectively are ϕ^o, ϕ^r and ϕ^u respectively. Some of the ruling- and rival-party affiliates are furthermore active in political activities, and the number of such individuals are given by $\phi_{\bar{k}}^o$ and $\phi_{\bar{k}}^r$.

Political Clientelism in a Heterogeneous Voter Pool

The ruling politician targets public funds to bolster support based on the observable characteristics of a voter prior to any transfers. Thus, denote $b_k^p(y)$ as the amount of transfers targeted towards a voter with political preference p,[149] political activism k, and income y.

For legal and other institutional reasons -- such as when credible commitments are not forthcoming -- targeted transfers do not instigate an automatic *quid pro quo* political affiliation choice by the recipient voter to align with the ruling party. Instead, targeted transfers are taken to induce a change in the voter's preference ordering over political affiliation. This may occur via two distinctive routes. First, voter preference about the ruling party θ_o^p can vary as the amount of transfers a recipient voter receives varies -- we call this the direct effect of targeted transfers. Second, θ_o^p may also vary when voters encounter political active citizens who have received transfers from the ruling politician. Arguably, citizen activism impacts the tenor and information content of the political campaigns. When preferential transfers to politically active voters end up influencing the political preference ordering of inactive voters, we will say that transfers to politically active voters exhibit a spillover effect.

Clientelistic transfers are observable only by the direct recipient and the ruling politician. Thus, for politically active individuals, a change in preference ordering in favor of the ruling politician occurs only via the direct effect, applicable when politically active voters themselves receive transfers. But for political inactive voters, their preference ordering in favor of the ruling politician can change because they have themselves received transfers, because the political activists who provide them with information about the political environment have received transfers, or both.

Preference Ordering of Politically Active Citizens

For a politically active individual receiving targeted transfers, the party

[149]From (1), the political preference a voter is revealed by his (observable) choice of political affiliation prior to any targeted transfers.

preference parameter θ_o^p changes by $u_{\bar{k}}^p$ to reflect the amount of transfers b received:

$$\begin{aligned} \omega_{\bar{k}}^p(a;y,b) &= y+b+\bar{k}^{\gamma}\theta_o^p+u_{\bar{k}}^p(b;y)+\varepsilon \ \text{ if } a=o \\ &= y+b+\bar{k}^{\gamma}\theta_r^p \ \text{ if } a=r \qquad , \\ &= y+b+\theta_u^p \ \text{ otherwise.} \end{aligned} \tag{2}$$

Where ε is a uniformly distributed error term on $[-\bar{\varepsilon},\bar{\varepsilon}]$ to reflect voter-specific differences in how responsive their political preferences (u^p) are to transfers.[150] $u^p(\cdot)$ reflects the change the voter's preference towards the ruling party politician subsequent to the transfer. In particular, direct transfers elicit voter affinity if and only if u^p is increasing in b. The effectiveness of transfers in eliciting positive preference changes $\partial u^p/\partial b$ may be moderated by other voter specific characteristics. For example, if political activism implies relative immunity to political support buying (Lindbeck and Weibull 1987), then $\partial u^p/\partial b$ is decreasing in k. Furthermore, if high income status also renders individuals immune to political support buying because of diminishing marginal utility of income (Dixit and Londregan 1996, Stokes 2005), then $\partial u^p/\partial b$ is decreasing in y. Given that these are the salient features of $u^p(\cdot)$ to be captured, henceforth we make these transparent and approximate $u^p(\cdot)$ by the following quadratic function:

$$u_{\bar{k}}^p(b;y)=\left(\alpha_b^p+\alpha_{\bar{k}}^p\bar{k}+\alpha_y^p y-\alpha_{bb}b/2\right)b\equiv\left(\alpha^p(\bar{k},y)-\alpha_{bb}b/2\right)b, \tag{3}$$

where the sign of α_b^p shows the effectiveness of transfer b to translate into voter support, while $\alpha_{\bar{k}}^p$ and α_y^p show respectively the importance of political activism and income as moderating factors. We assume that α_{bb} is strictly positive to reflect diminishing marginal effectiveness of transfers in eliciting change in political preference.

Denote $S_{\bar{k}}^p(y)$ as the fraction of politically active voters with preference p and income y for whom a political affiliation with the ruling party maximizes utility. Using (2) and (3):

[150] As an alternative interpretation, may also reflect any random shifts in the status quo voter preference ( ) that occurs as a result of targeted transfers carried out by the ruling politician to own-party affiliates, rival-party affiliates and/or unaffiliated voters. It is straightforward to extend the model to include ranges of that are political preference specific . As will be evident in the sequel, doing so does not change any of the qualitative results obtained here.

$$\begin{aligned} S_{\bar{k}}^{p} &= \Pr ob(\arg\max_a \ \omega_{\bar{k}}^{p}(a;y,b) = o\}) \\ &= s_{\bar{k}}^{p} + \frac{1}{2\bar{\varepsilon}}\left(\alpha^{p}(\bar{k},y) - \frac{\alpha_{bb}b}{2}\right)b \end{aligned} \tag{4}$$

where $s_{\bar{k}}^{p}$ is a constant, given by:

$$s_{\bar{k}}^{o} = \frac{1}{2} - \frac{\max\{\bar{k}^{\gamma}\theta_r^o, \theta_u^o\} - \bar{k}^{\gamma}\theta_o^o}{2\bar{\varepsilon}}, \quad s_{\bar{k}}^{r} = \frac{1}{2} - \frac{\bar{k}^{\gamma}(\theta_r^r - \theta_o^r)}{2\bar{\varepsilon}}. \tag{5}$$

(4) and (5) together show that the maximal support that a politician can expect by appropriate selection of targeted transfers depends on political preference p, activism k and income y. We will examine this dependence in greater detail once the aggregate support maximizing transfers are determined in the sequel.

Preference Ordering of Politically Inactive Citizens

Now for a politically inactive individual receiving targeted transfers, the party preference parameter θ_o^p changes by u^p as before to reflect the direct effect of transfers. To reflect the spillover effect, we assume that the more frequent a politically inactive citizen encounters political activists, the greater the spillover effect, if any. The size of this spillover effect furthermore depends on the actual amount of transfers the political activists receive. Across political activists, there are two types of political preferences $p = o, r$ and a continuum of income levels y. As such, the size and sign of the spillover effect can potentially differ at each encounter with activists depending on their individual p and y.

With these motivations in mind, let the following expression reflect the size of the spillover effect associated with n^p encounters between a politically inactive voter and political activists:

$$\sum_{n=0}^{n^o} v^o(b_{\bar{k}}^o(y_n)) + \sum_{n=0}^{n^r} v^r(b_{\bar{k}}^r(y_n)).$$

We assume that n^o and n^r are Poisson distributed random variables where the average number of meetings is given by the population share of the two types of political active voters $\phi_{\bar{k}}^o / N$ and $\phi_{\bar{k}}^r / N$. $v^o(b_{\bar{k}}^o(y_n))$ and $v^r(b_{\bar{k}}^r(y_n))$ respectively are the changes in the ruling party preference made possible by encounters with politically active citizens, depending on the political preference p and the income y_n of the activist. We assume that each meeting is a random draw from the income distribution of political activists. Taken together, the spillover effect at the voter level is a random variable depending on voter-specific realizations of the number of meetings with activists n^p, and the income and political preference of these activists at each draw. We have thus

$$\begin{aligned}\omega_1^p(a;y,b) &= y+b+\theta_o^p+(\alpha^p(1,y)-\alpha_{bb}b/2)b+\varepsilon \\ &\quad +\sum_{n=0}^{n^o} v^o(b_{\bar{k}}^o(y_n))+\sum_{n=0}^{n^r} v^r(b_{\bar{k}}^r(y_n)) \text{ if } a=o \\ &= y+b+\theta_r^p \text{ if } a=r \\ &= y+b+\theta_u^p. \text{ otherwise.}\end{aligned} \tag{6}$$

At each encounter between a political inactive citizen and an activist, greater transfers to the political activist elicits voter affinity towards the ruling politician if and only if v^p is increasing in $b_{\bar{k}}^p(y)$. To highlight this relationship, we approximate $v^p(\cdot)$ by the following:

$$v^p(b)=\beta_o^p+\beta^p b,$$

where the magnitude and sign of β^p shows the importance and direction of the spillover effect. The constant β_o^p may be positive of negative, which shows the impact of political activism on ruling party support, when unadulterated by preferential transfers.

Denote $S_1^p(y)$ as the expected fraction of politically active voters with preference p and income y for whom a political affiliation with the ruling party maximizes utility, with expectation taken over all possible income levels y_n of politically active citizens. Also let $\overline{b}_{\bar{k}}^p$ denote the average amount of targeted transfers received by politically active citizens with preference p :

$$\overline{b}_{\bar{k}}^p=\int_y b_{\bar{k}}^p(y)dG(y).$$

Using (6), it is straightforward to determine that:

$$\begin{aligned}S_1^p(y) &= \Pr ob(\arg\max_a\ \omega_1^p(a;y,b)=o\}) \\ &= s_1^p+\frac{1}{2\overline{\varepsilon}}\left(\alpha^p(1,y)-\alpha_{bb}b/2\right)b+\frac{1}{2\overline{\varepsilon}}\sum_{p=o,r}\phi_{\bar{k}}^p(\beta_o^p+\beta^p\overline{b}_{\bar{k}}^p)/N\end{aligned} \tag{7}$$

where s_1^p is a constant, given by:[151]

$$s_1^o=\frac{1}{2}-\frac{\max\{\theta_r^o,\theta_u^o\}-\theta_o^o}{2\overline{\varepsilon}},\quad s_1^r=\frac{1}{2}-\frac{\theta_r^r-\theta_o^r}{2\overline{\varepsilon}},$$

and

$$s_1^u=\frac{1}{2}-\frac{\theta_u^u-\theta_o^r}{2\overline{\varepsilon}}$$

[151]We assume henceforth that interior solutions apply.

(7) displays both the direct and spillover effects of targeted transfers. In particular, the size of the spillover effect depends critically on the share of political active individuals $\phi_{\bar{k}}^{p}$ in the citizen population, as well as the average targeted transfers the ruling politician makes to this group $\bar{b}_{\bar{k}}^{p}$.

Political Clientelism with Citizen Activism

The decision problem of the ruling politician can now be stated. Specifically, the ruling politician chooses targeted transfers $b_{k}^{p}(y)$ to voters with characteristics , k , and y in order to maximize the aggregate number of political affiliates, accounting for the cost of doing so:

$$\max_{b_k^p(y)} \int_y \sum_{p=o,r,u} \sum_{k=\bar{k},1} \phi_k^p \left(\rho S_k^p(y) - \lambda b_k^p(y)\right) dF(y). \qquad (8)$$

where ρ reflects the money equivalent gains from a unit increase in aggregate political support, and $\lambda \geq 1$ denotes the marginal cost of public funds. Using (1) - (7), the solution to (8) is given by:

$$b_1^p(y) = \frac{1}{\alpha_{bb}}\left(\alpha_1^p(y) - 2\lambda\bar{\varepsilon}/\rho\right) \qquad (9)$$

$$b_{\bar{k}}^p(y) = \frac{1}{\alpha_{bb}}\left(\alpha_{\bar{k}}^p(y) - 2\lambda\bar{\varepsilon}/\rho + (1-(\phi_{\bar{k}}^o + \phi_{\bar{k}}^r)/N)\beta^p\right). \qquad (10)$$

where, to recall, $\alpha_1^p(y)$ measures the direct effect of transfers to individuals with political preference p , activism k and income y :

$$\alpha_k^p(y) = \alpha_b^p + \alpha_k^p k + \alpha_y^p y.$$

The spillover effect of transfers to political activists with political preference p is

Focusing on the determinants of the direct effect , all else equal, the ruling politician preferentially targets low income individuals if and only if , as in Dixit and Londegran (1996) and Stokes (2005), for example. Meanwhile, all else equal and in the absence of spillover effects $\beta^p = 0$, preferential tranfers also target political inactive individuals, if and only if they are more susceptible to the influence of political support buying ($\alpha_k^p < 0$) as in Lindbeck and Weibull (1987). Across individuals with different political preferences, all else equal politically inactive voters with pro-rival and neutral political preferences receives favorable preferential transfers compared to pro-ruling party supporters if and only if

$$\alpha_b^p + \alpha_k^p + \alpha_y^p y > \alpha_b^o + \alpha_k^o + \alpha_y^o y, \quad p = r,u \tag{11}$$

which simply implies that the direct effect of a rupee spent gives rise to a higher increase in the support from rival party voters, accounting for the moderating effects of political activism and income.

Even when $\alpha_k^p < 0$ as hypothesized above, political activists may nonetheless receive preferential transfers if the spillover effects of targeted transfers to political activists are positive $\beta^p > 0$ and sufficiently large. To the extent that the signs of both β^p and α_k^p are uncertain, the preferential targeting of political activists may imply either that spillover effects are present via $\beta^p > 0$ are present, or that political activists are relatively susceptible to political support buying via $\alpha_k^p > 0$ as part of the direct effect of targeted transfers to political activists.

In order to disentangle this ambiguity, note furthermore from (10) that the magnitude of the spillover effects is inversely related to the share of politically active citizens $(\phi_{\bar{k}}^o + \phi_{\bar{k}}^r)/N$, while the direct effect is unaffected by this population share. Thus, if spillover effects are indeed present, we should expect that such effects are greater in locations where political activism is less common.[152]

Clearly the above admits a diversity of ordering of clientelistic transfers over voter type p , k and y . The following proposition highlights one such ordering that will be of particular relevance in the sequel:

Proposition 1 *If the following assumptions hold,*

1) there is diminishing marginal utility of income ($\alpha_y^p < 0$ *)*

2) the utility of ruling party supporters is the least responsive to ruling party transfers, while rival party voter utility is the most responsive: $\alpha_b^o(y) < \alpha_b^u(y) < \alpha_b^r(y)$

3) spillover effects exist and the effects are sufficiently strong for rival activists ($\beta^r >> 0$ *), while* β^o *is positive but relatively small,*

the ranking of clientelistic transfers to voter type p *, and* k *at constant* y *, in descending order of magnitudes, is given by: rival party active, neutral, rival party inactive, ruling party active and ruling party inactive voters. At given* p *and* k *,* $b_k^p(y)$ *is decreasing in* y *.*

A decrease in the fraction of politically active citizens triggers an increase in transfers to politically active voters if and only if spillover effects exist and are positive $\beta^p > 0, \quad p = o,r$ *.*

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[152]Across voters, transfers decreases with the spread of voters as the same amount of transfers will give rise to a smaller increase in support when is large. Transfers are also decreasing in the marginal cost of public funds . Furthermore, transfers are increasing in the money equivalent gains of political support as perceived by the ruling politician. All three of these factors are likely to be jurisdiction-specific, to be accounted for in our empirical analysis through village fixed effects.

In what follows, we take each of the above predictions of the model to the data.

7.4 Data Description

We use a dataset of 1,077 households from UPA-\textit{sarpanch} villages collected from a primary field survey in Andhra Pradesh. The survey was conducted in the months of August and September in 2006. Our core dataset spans four districts namely Kadappa, Nalgonda, Warangal and Nellore (first three belongs to phase-I, last one belongs to phase-II) of which the latter two currently belong to the state of Telengana.[153]

Our main set of variables are days worked and payments received under the MGNREGS scheme, and political affiliation and activism levels of households. The survey collected job-card numbers for all participating households, enabling us to merge into our survey, publicly available annual administrative data (available online at http://nrega.ap.gov.in), exhaustive in their coverage of participating households and information on workdays and payments.[154] Our surveys also collected data on households' affiliation to a particular political party if any, which we code into alliances namely, UPA, UPA-rival, and unaffiliated (the last category implies not affiliated to any party or affiliated to a few fringe parties) based on the coalition formation in the previous state election in 2004.[155][156]. Political affiliation of village \textit{sarpanch}-es are not directly available because elections to the Gram Panchayat do not run on party labels. However, we were able to deduce sarpanch's party affiliation from households' response to the three following questions in our survey: (1) Did you vote for the winner in the last GP election? (2) If so, is that vote for party affiliation reasons? (3) Which party are you affiliated to?. There are mixed responses on sarpanch's affiliation across households, reflecting that many villagers are unaware of this aspect about their sarpanch. Considering this heterogeneity in reporting, we assigned sarpach's party affiliation in a village to be the one that more than 50% of the sampled households reported in the village. In villages without this majority, we left sarpanch's affiliation blank. Households stating affiliation to a particular party were also questioned the intensity of participation in political activities, which we coded as "Politically Active" (campaigning, attending meetings, giving speeches and writing pamphlets, attending rallies and offering donations), and "Politically Inactive". The involvement level of all unaffiliated households were set to "inactive".

Table 7.1 provides the distribution of households in our sample based on a variety of characteristics. Highest representation is by backward caste (44.36%), followed by Scheduled Caste (29.34%), Forward Caste (18.75%), and Scheduled

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[153] Telangana was carved out of the north-western part of erstwhile Andhra Pradesh in 2013. Kadappa and Nellore are still a part of Andhra Pradesh

[154] The administrative data in AP are verified routinely through independent social audits in the gram panchayats across the state (see http://www.socialaudit.ap.gov.in). We also conducted verification exercises in select villages in 2014 which suggest that the administrative data is reliable. Household interviews on wages earned and work done by job-card holders match entries in post-office or bank books wherever these were available. Likewise individual recall data on the type of work done and number of days are also consistent with administrative data, as are the list of assets created since inception. Details of this verification exercise are available upon request.

[155] footnote{The same questions were asked to females and males in the household, but in this study we use data reported by males because unlike males, women participate less in politics, are politically less influential, and make fewer household economic decisions than men.}

[156] There could be a concern that since party affiliation is self-declared, it may not reflect actual voting behavior but a desire to show gratitude to the gram panchayat leaders. However, this is not a concern because the survey was conducted in the interviewees' house by enumerators, and panchayat leaders are not expected to see the information in the survey.

Tribe (7.55%). 57.2% household-heads are not formally literate (they may have received informal education), a little over a quarter (25.26%) received secondary education or below, and only about 17.53% receive high school education. The most popular occupation is casual work in agriculture and other sectors (39.06%), followed by salaried work in agriculture and other sectors (31.51%), own business or self-employed - agriculture/other sectors (14.50%). Nonworking adults, who are typically pensioners/rentiers, dependents, students and those focusing on households chores, form 10.16% of the sample. 3.21% own common property resources and manage livestock, 1.56% are engaged in other occupations.

Almost two-thirds households belong to "Poorest of the Poor and Poor" category, and 34.2% households belong to the "Not so poor/Not poor" category. 59.72% of the household heads indicate low awareness levels (indicated by their regular and frequent attendance of village meetings), and 40.28% have high awareness levels (indicated by their non-attendance or rare attendance in village meetings). 41.58% households are UPA-affiliated and politically inactive, 22.4% are UPA-affiliated and politically active, 9.98% are UPA-rival affiliated and politically inactive, 6.34% are affiliated to UPA-rivals and politically active. Rest 19.7% are unaffiliated.

Table 7.2 describes cumulative MGNREGS benefits in 2006 and 2007 in UPA sarpanch villages. Unaffiliated households obtained the highest proportion of job-cards (56.8%), followed by UPA-rival active households (53.42%), UPA active households(50.7%), UPA inactive households, and UPA-rival inactive households (44.34%). Days worked was highest among the unaffiliated households (21.36 days), followed by UPA-inactive households (18.82), UPA-rival inactive households (18.43), UPA active households (17.55), and UPA-rival active households (13.36). Payment received was highest among the unaffiliated households (INR 1627.69), followed by UPA-rival inactive households (1538.13), UPA active households (1514.24), UPA inactive households (1509.68), and UPA-rival active households (1118.54).

However these are merely descriptive statistics, and with the lack of control variables and an appropriate empirical framework, do not yield causal estimates of the effects of political affiliation on MGNREGS payment and days. We resort to understanding these relationships in the section below.

7.5 Empirical Methodology

To empirically examine if households obtain higher or lower MNREGS benefits based on their affiliation with a political coalition and their political activism as shown in the model, we consider the latent variable model below.

$$y_{vi} = \gamma_0 + \gamma_1 PolCatigories_{vi} + \gamma_2 X_{vi} + \gamma_3 D_v + \varepsilon_{vi}\,, \tag{12}$$

where y_{vi} is a latent variable determined by the above process, which in turn yields the following observables for household *i* at village *v* : (1) log(days+1), logarithm of days worked in MNREGS by household *i*, and (2) log(amount earned+1), logarithm of amount earned through MNREGS by household *i*, cumulative over 2006 and 2007 [157]. ε_{vi} is an orthogonal error term. This model motivates a tobit framework, where the threshold above which work is performed is zero (for both days worked and payment). $PolCategories$ is a vector that interacts household political affiliation (UPA, UPA-Rival, and unaffiliated) and activism (active and inactive). The interaction gives us

[157] log(x+1) is an effective method to deal with zero values in x.

five political categories: (1) UPA-inactive (2) UPA-active (3) UPA-rival Inactive (4) UPA-rival Active (5) Unaffiliated.

In the above equation, endogeneity concerns for the identification of the effects of political affiliation and activism arise from three sources. First, the data do not provide information on households' job seeking activities. Job seeking is correlated with MGNREGS employment because of the demand-driven nature of the program (an individual who does not seek a job would not get one). If job seeking is correlated with political affiliation and activism, this omitted variable problem would result in biased estimation. To test if this is the case, we run a probit model in which the dependent variable is whether the household owns a job card or not. The latent variable of the probit model follows the same specification as in the above equation. We use job card ownership to proxy for job seeking activities based on the strong anecdotal evidence in Andhra Pradesh that households seeking a job-card almost always obtain one. Given that owning a job card is the pre-requisition of MGNREGS employment and job-card issuance in Andhra Pradesh is free of charge and straightforward, we consider owning a job card as a good indicator that the household is engaged in MGNREGS job seeking. We then test the null hypothesis $\beta_1 = 0$. We would expect $\beta_1 \neq 0$ if job seeking is correlated with political affiliation and activism after controlling for the observables. Accordingly, if we fail to reject $\beta_1 = 0$, we are less concerned about this omitted variable issue.

Second, we care about the correlation of political affiliation and activism with other unobserved factors that also influences MNREGS benefits. This concern is diluted by our rich set of control variables described above. The poverty measure, in particular, having been developed from a combination of quantitative and participatory qualitative methods, is unique and precisely captures household's status. The village level fixed effects are crucial for identification because they capture supply side unobservables (including the availability of MNREGS jobs and funds at the village level) and some demand side factors such as rainfall, ratio of landlords versus landless, nonfarm opportunities, etc.

Third, we want to rule out reverse causality concerns wherein party affiliation itself is a result of benefits received under MNREGS. To illustrate how we tackle this concern, a time-line of events is presented in Figure 7.3, phase-1 villages where the program started in February 2006, we measure political affiliation in the survey months of August to October, just after the commencement of MNREGS but not long after. In phase-2 villages, we capture political affiliation of households in 2006, before the program started in April 2007. Thus identification in phase-2 villages are cleaner, but there is a lapse of six months after which the program started and before which we measure political affiliation in phase 1 villages where households in our sample may have received MNREGS benefits. We conduct a robustness check by repeating the analysis after removing households who received jobs in these six months.

The theoretical model suggests that, if spillover effects play a role in MGNREGS benefits allocation, we would observe the dependence of such effect on the share of political active individuals in the citizen population. To test this prediction, we interact the share of politically active households in the village ($ShareActive_v$) with a dummy variably indicates political activism ($Active_{vi}$) and run the regression below.

$$y_{vi} = \gamma_0 + \gamma_1 PolCategories_{vi} + \gamma_2 ACTIVE_{vi} * SHAREACTIVE_v + \gamma_3 X_{vi} + \gamma_4 D_v + \varepsilon_{vi} \tag{13}$$

Note that in the above regression, we do not include $ShareActive_v$ because it is absolved by the village fixed effects and thus cannot be identified. Our parameter of interest is

$$\gamma_2 = \frac{\partial y}{\partial ShareActive}\Big|_{Active=1}.$$

We expect γ_2 to be negative. That is, politically active households receive less (more) benefits if they reside in villages with a higher (lower) proportion of politically active households. Intuitively, spillover effects of targeting a politically active household are likely to be higher in a village with more inactive households.

7.6 Results

7.6.1 Main Results

Tobit regression results of MGNREGS days and payment on political affiliation and political activism are presented in Table 7.3. Note that since the coefficients are not marginal effects on actual outcome, but rather on the latent variable or the censored outcome, caution is needed in interpreting the reported magnitude directly.

Results indicate that political affiliation and activism play a significant role in explaining MGNREGS days worked and payment received (column 1 and 2 respectively) among participants. Compared to UPA-inactive households, more *latent* benefits are obtained by UPA-rival active (0.758 log days; 1.827 log payment), Unaffiliated households (0.728 log days; 1.429 log payment), UPA-rival inactive (0.525 log days; 0.881 log payment), and UPA active households (0.2 log days; 0.486 log payment), in that order. Marginal effects on *Expected censored outcomes* provide the same ordering of benefits across categories. Compared to UPA inactive households expected days worked and payment received are highest in UPA-rival active (0.257 log days; 0.632 log payment), Unaffiliated (0.246 log days; 0.486 log payment), UPA-rival inactive (0.174 log days; 0.293 log payment), UPA-active (.064 log days; 0.158 log payment). These significant coefficients and patterns indicate strong clientelistic practices in UPA-sarpanch villages.

Despite the observed clientelism, targeting under the program is also need-based to a large extent, illustrated by the following need-based coefficients. However, care should be taken in causally interpreting these, due to potential endogeneity issues. Households whose heads are casual workers (the most vulnerable of categories), obtain more MGNREGS work and payment compared to all other households. Heads who are not literate and who did not receive any informal education received more jobs and payment compared to those with higher education. Intuitively, MGNREGS employment and payment increases with household size. As expected, poorest-of-the-poor and poor households obtain more benefits compared to not-so-poor and non-poor households. Sarpanches offer less jobs to those who watch more TV (an indicator of awareness and wealth), again indicating potentially effective targeting. Sarpanches offer more jobs to those who rarely attend village meetings rather than those who frequently attend, indicating potentially effective targeting of those whose opportunity cost of attendance is high.

Older household heads obtain more jobs, but the influence of age decreases as age increases. This is consistent with the labor intensive nature of MGNREGS work which perhaps older people struggle with. An increase in land holdings is associated with higher MGNREGS work and payment, but the effects decline with high land-holdings. These results are intuitive because small and marginal farmers with little land may have other income sources (such as casual work or migration income) which may limit their incentives to pursue the MGNREGS. On the other hand, large land holders who also tend to be richer may not find the MGNREGS attractive because they may have other work even in the agricultural lean season (such as feeding animals, developing land, and maintaining machineries). Consequently, it is reasonable that farmers with medium-sized landholding were more likely beneficiaries of MGNREGS.

Compared to Forward Caste, lower caste households belonging to the Scheduled Castes (SC) and Other Backward Castes (OBC) receive higher jobs, as expected, since lower caste households are in general poorer with less access to basic needs.[158] Scheduled Tribe (ST) households however receive less work than Forward caste households, contrary to expectations. This is because tribal villages tend to be isolated with less visiting officers and rocky soil making it hard to implement or build infrastructure projects (Maiorano and Buddha 2014). These items do not get captured in village fixed effects.

7.6.2 Robustness

The lapse of six months between the commencement of the program (February 2006) and our survey (August-October 2006) for phase-I villages does not rule out the possibility that receiving MGNREGS benefits in these six months affect the measured political affiliation in our survey. However, in Table 7.4, we show that our results remain qualitatively robust if we estimate our empirical model excluding households that received MGNREGS work between March and their interview month (columns 1 and 2 for days worked and payment received respectively). The story narrows down even more. More starkly here, MGNREGS benefits appear to significantly reach the unaffiliated in politically active villages, but not the UPA-rivals. Effectively, UPA sarpanches target the active UPA-rivals, and through them also target the unaffiliated in villages with a higher proportion of active households.

Table 7.5 presents the results from a probit regression of job-card ownership on household characteristics for days worked (column 1) and payment (column 2). There is no evidence of clientelism in job-card seeking, as explained before. The six political variables are jointly insignificant as shown by the joint chi-square test statistics in the table (six degrees of freedom). This alleviates the concern about household self-selection on job-card and job seeking, and the latter particularly to the extent that the two are correlated.

Table 7.6 shows the tobit regression results when we redefine the political activism categories. In the new definition, only extremely active activities who are expected to engage and influence the political campaigns were placed in the politically active group (who are involved with campaigning, attending meetings, giving speeches and writing pamphlets, and are more likely to shape public opinion), and the households engaged in less extreme activities (attending rallies) and no activity households were placed in the less active group. The results from this regression are qualitatively similar to table 7.3.

7.6.3 Mechanism

We turn to the mechanism played out here. Table 7.7 shows that γ_2 in equation (13) is negative and significant. In other words, politically active households receive significantly less benefits if they reside in villages with a high proportion of politically active households (lower proportion of inactive households). Thus, UPA-sarpanches appear to target transfers to political activists, depending on the scale of the influence

[158] In India, there is strong evidence of caste differentials in favor of higher caste in consumption, income, education, occupations, and development indices (e.g. see Deshpande (2001), Hasan and Mehta (2006), Mehrotra (2006), Mohanty (2006), Srinivasan and Mohanty (2004), and Sundaram (2006)).

of political activists in the village. More transfers are targeted to active households if more inactive households reside in their village. We interpret these as suggestive evidence consistent with the two types of voter identities examined in our model.

Columns 1 and 2 in Table 7.8 present the regression of MGNREGS days and payment on household characteristics in non-UPA sarpanch villages. Notably, the political variables are not significant here. That clientelism is absent in non-UPA sarpanch villages reinforces our claim that non-UPA sarpanches lack the resources for engaging in clientelism and/or are not able to ally or dominate field assistants who likely belong to UPA to achieve common political objectives.[159] Further, as Gupta and Mukhopadhyay (2016) point out, since the program was originally conceived by the INC-led UPA government in 2006, there may be "leakage of goodwill" for non-UPA parties to engage in vote/support buying using the MGNREGS.

7.6.4 Discussion

While clientelism is significant, need-based variables that influence households to demand more jobs, such as occupation and education of the household head, poverty level, landholding, household head's age, are all significant with expected signs. In other words, the program also functions by the book with jobs allocated as per the household needs. Similar results were observed by Das (2014), who finds significant correlation between MGNREGS work and socio-economic indicators such as household land ownership, Below Poverty Line card ownership, and religion, as well the head's occupation and age\footnote{Das (2014) - Table 7.5, last column, page 208.}. Sheahan et al.(2014) show that the variation in the funding allocation at the sub-district level are explained far more by the needs rather than by the election variables, even though both set of variables are significant. For a program that emphasizes rights-based legal obligation of households in obtaining work and operating at a massive scale, the significant correlation between need based variables and MGNREGS work with the right sign should be significantly applauded. Our robust results on clientelism do not implicate the performance of the MGNREGS, but is instead consistent with the larger problems facing decentralization which has been observed in other government programs among various other political parties in several countries.

Our results differ from other studies in the Indian context that find local leaders patronizing loyalist households by offering them more benefits compared to swing groups or rival party members. Two aspects of our study could explain this difference\footnote{Prior literature on India as noted in the introduction finds that party loyalists and members are given preference in welfare programs (Das, 2014; Bardhan et al. 2009; Besley, Pande and Rao, 2005; Markussen, 2011)}. First, unlike previous studies, we address the issue of reverse causality using the unique timing of our survey. Second, the power position of the congress led United Progressive Alliance both in the center and in Andhra Pradesh was different from that of the Left front government in West Bengal where Das (2013) and Bardhan et al. (2009) are based on, which may lead to different strategies pursued by these different parties. Remarkably, the commonly cited Cox and Mccubins (1986)'s model that support empirical evidence on a risk-averse leader's preference for their own affiliates, also explicitly note in a separate section that a risk-neutral or risk-loving candidate already feeling secure about their loyalists, would tend to focus on expanding their party base by targeting others.

Additionally, our findings that village leaders in Andhra Pradesh target rivals

[159] We conducted similar analysis exclusively for UPA-rival sarpanches (comprising only BJP or TDP party sarpanches), and find no support for clientelism. These results are not presented in the paper, but are available upon request.

and the unaffiliated households, are different from Sheahan et al. 2015's in the same state that finds no partisan-influenced spending before the 2009 election and that the political leaning of a mandal played only a small part in fund distribution after the 2009 election. This contrast strengthens the initial motivation for our work, that within-village household-level resource allocation could be different from aggregate level resource allocation.

7.7 Conclusion

This paper begins by exploring a model of political clientelism based on observable voter attributes including political affiliation and political activism. We show here that the ruling party can increase its support base by directly targeting transfers to swing voters, or by indirectly targeting transfers to influence the messages of the politically active individuals. The former requires transferring funds to the relatively politically inactive individuals, while the latter requires the transfer of funds to the most politically active individuals.

We take these model predictions to data, and examine the clientelistic practices of local village leaders under the Mahatma Gandhi Rural Employment Guarantee Program (MGNREGS) in India, a public works program operating at a very high budget. Particularly, we ask if and how the political affiliation and activism of households affect how much MGNREGS benefits they receive. Our results provide robust evidence for clientelism in UPA-sarpanch villages, where the sarpanch is able to strategically allocate resources to opposition members/affiliates and unaffiliated households, compared to his/her own affiliates, in order to elicit support responses from them. within both UPA and non-UPA households, UPA sarpanches target active more than inactive households. Consistent with the theoretical model, we find that UPA-sarpanches offer more benefits to active households in less active villages where their scale of influence is high. Clientelism is absent in villages under a non-UPA sarpanch, consistent with their low financial ability and clout, and due to potential "leakage of goodwill" for them if they employ an UPA flagship program to buy support. This collective evidence sheds new light on vote buying both as a means to mobilize support form swing voters, as well as to influence the behavior of political activists themselves.

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7.9 Figures and tables

Figure 7.1: Hierarchy under MGNREGS in India

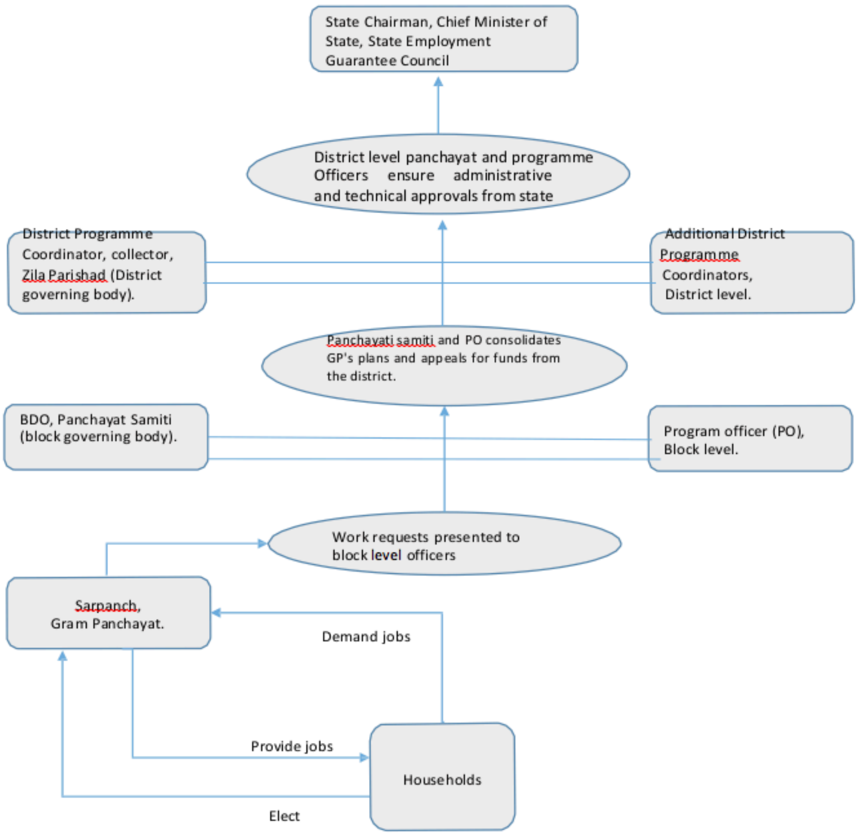

Figure 7.2: Hierarchy under MGNREGS in Andhra Pradesh

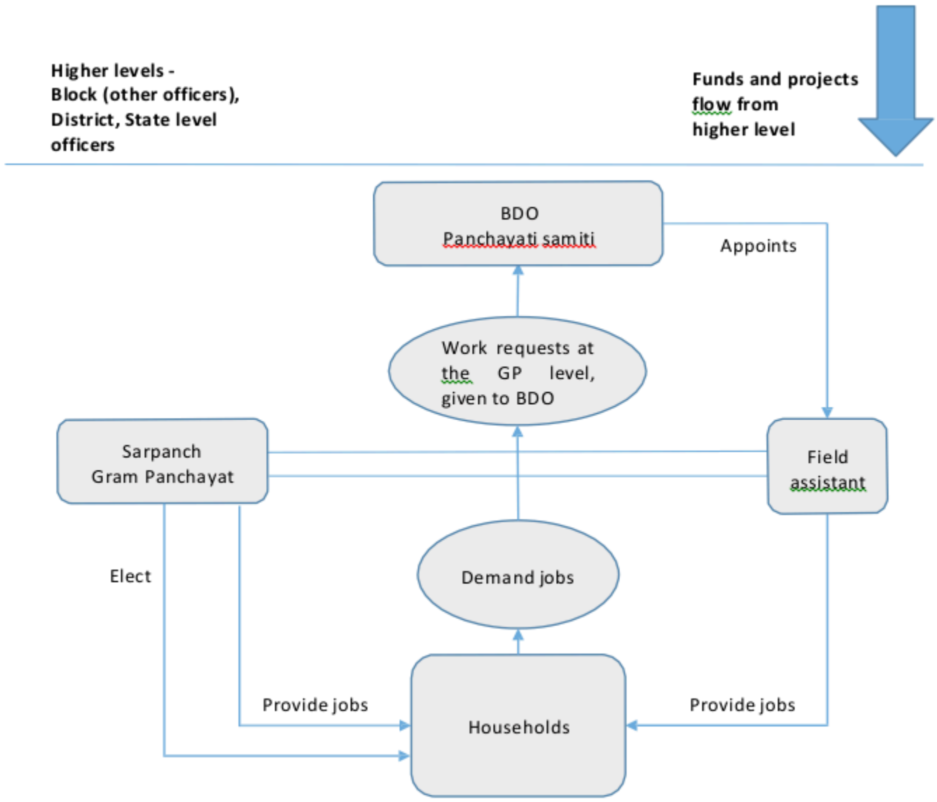

Figure 7.3: Timeline of events

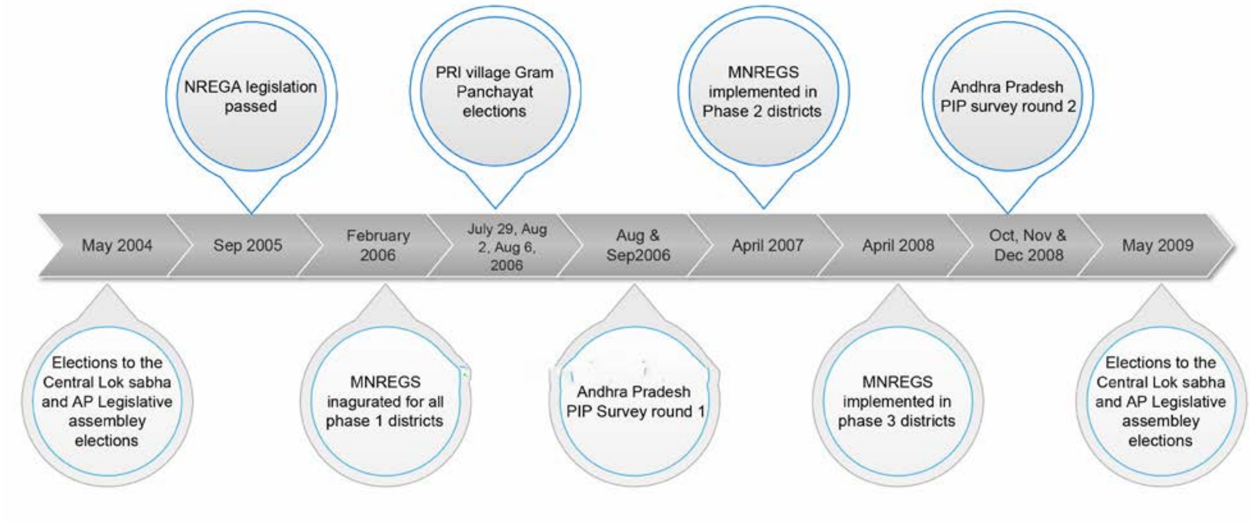

Table 7.1: Descriptive Statistics in UPA-Sarpanch Villages

Social group	
Scheduled Caste	29.34 %
Scheduled Tribe	7.55 %
Backward Caste	44.36 %
Other Caste	18.75 %
Education of the household head	
Not literate or received informal education	57.20%
Secondary education and below	25.26%
High schoolers and graduates	17.53%
Primary occupation of the household head	
Casual work (Agriculture/others)	39.06 %
Salaried work (Agriculture/others)	31.51%
Own business or self-employed (Agriculture/others)	14.50%
Nonworking adults	10.16%
Common property resources, Livestock management	3.21 %
Others	1.56%
Poverty status	
Not so poor/not poor	34.20 %
Poorest of the poor/poor	65.80 %
Attendance of village meetings (awareness)	
Almost or mostly (high awareness)	40.28%
Never or rarely (low awareness)	59.72%
Household political categories	
UPA, Politically Inactive	41.58%
UPA, Politically Active	22.40%
UPA-rival, Politically Inactive	9.98%
UPA-rival, Politically Active	6.34%
Unaffiliated	19.70%
Sample size	1,152

Note: Politically Active refers to involvement in party activities such as attending rallies, campaigning, attending meetings, giving speeches and writing pamphlets; Politically Inactive means that the household does not participate in political activities. The Unaffiliated category includes those not affiliated with any party or those affiliated with fringe parties that are non-UPA and non-UPA-rival. Nonworking adults are pensioners/rentiers, dependents, students and those focusing on household's chores.

Table 7.2: Political Categories and MGNREGS outcomes in UPA-Sarpanch Villages

Household affiliation	Job-Card Holding	Mean Days/annum	Mean Payment (INR)/annum
UPA, Politically Inactive	50.1%	18.82	1509.68
UPA, Politically Active	50.7%	17.55	1514.24
UPA-rival, Politically Inactive	44.34%	18.43	1538.13
UPA-rival, Politically Active	53.42%	13.36	1118.54
Unaffiliated	56.38%	21.36	1627.69

Note: Politically Active refers to involvement in party activities such as attending rallies, campaigning, attending meetings, giving speeches and writing pamphlets; Politically Inactive means that the household does not participate in political activities The Unaffiliated category includes those not affiliated with any party or those affiliated with fringe parties that are non-UPA and non-UPA-rival.% Job-card owned represents the proportion of households that obtained a job- card either in 2006 or 2007. Days worked and amount earned are summarized for job-card holders only. INR refers to Indian Rupees.

Table 7.3: Tobit regression of MGNREGS Benefits on Political Affiliation and Political Activism in UPA-Sarpanch Villages

VARIABLES	(1)	(2)
	Log Days	Log Payment
Household Political Affiliation (base: UPA-inactive)		
UPA active	0.200***	0.486***
	(0.0198)	(0.0433)
UPA-Rival inactive	0.525***	0.881***
	(0.0168)	(0.0359)
UPA-Rival active	0.758***	1.827***
	(0.0260)	(0.0569)
Unaffiliated	0.728***	1.429***
	(0.0166)	(0.0361)
Social groups (base: Forward Caste)		
Scheduled Caste	1.225***	2.687***
	(0.0254)	(0.0559)
Scheduled Tribe	-0.838***	-1.773***
	(0.0243)	(0.0525)
Other Backward caste	0.652***	1.571***
	(0.0252)	(0.0555)
Education of HH head(base:Not-literate/Informal Edu)		
Secondary and below	-0.0129	-0.118***
	(0.0183)	(0.0399)
Higher secondary and graduate	-1.056***	-2.389***
	(0.0230)	(0.0518)
Occupation of HH head(base:Casual work-Ag/others)		
Salaried work - Ag/others	-0.887***	-1.995***
	(0.0326)	(0.0709)
Own business or self-employed	-1.110***	-2.501***
	(0.0324)	(0.0703)
Nonworking adults and children, Family	-0.880***	-2.009***
	(0.0325)	(0.0698)
Common propoerty resources, livestock managament	-0.793***	-1.881***
	(0.0346)	(0.0741)
Others	-1.967***	-4.521***
	(0.0274)	(0.0593)

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Table 7.3 – *Continued from previous page*

VARIABLES	(1) Log Days	(2) Log Payment
Poorest of poor/poor (base: Not so poor/Not poor)	0.776***	1.662***
	(0.0150)	(0.0332)
Household size	0.0500***	0.112***
	(0.00256)	(0.00556)
Household head age	0.161***	0.342***
	(0.000302)	(0.000659)
Household head age squared	-0.00208***	-0.00444***
	(5.62e-06)	(1.23e-05)
Log land owned	0.345***	0.679***
	(0.0405)	(0.0879)
Log land owned squared	-0.167***	-0.290***
	(0.0157)	(0.0347)
None/rare participation in village meetings(base:Frequent attendance)	0.156***	0.290***
	(0.0103)	(0.0226)
Hours of TV watching (awareness)	-0.332***	-0.697***
	(0.00788)	(0.0174)
Constant	-23.37***	-48.57***
	(0.0133)	(0.0289)
Sigma	2.957***	6.485***
	(0.00624)	(0.0140)
F-value of the joint test of political variables	509.98	436.03
P-value of the F-test	0.00	0.00
Log pseudo likelihood	-1417.95	-1768.88
Pseudo R2	0.148	0.120
Observations	1,152	1,152
Clusters	166	166

Note: Robust standard errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include village fixed effects. Politically Active refers to involvement in party activities such as attending rallies, campaigning, attending meetings, giving speeches and writing pamphlets; Politically Inactive households do not participate in political activities. The Unaffiliated category includes those not affiliated with any party or those affiliated with fringe parties that are non-UPA and non-UPA-rival. The involvement level of all unaffiliated households was set to ”Politically Inactive”. Nonworking adults are pensioners/rentiers, dependents, students and those focusing on household’s chores.

Table 7.4: Robustness check, after dropping households without jobs between February 2006 and interview month. Tobit regression of MGNREGS benefits on Political Affiliation and Political Activism

	(1) Days	(2) Payment
Household Political Affiliation (base: UPA-inactive)		
UPA active	0.423***	0.981***
	(0.0234)	(0.0525)
UPA-Rival inactive	0.436***	0.820***
	(0.0218)	(0.0490)
UPA-Rival active	1.168***	2.782***
	(0.0293)	(0.0661)
Unaffiliated	0.991***	2.113***
	(0.0218)	(0.0491)
Social groups (base: Forward Caste)		
Scheduled Caste	1.515***	3.452***
	(0.0302)	(0.0694)
Scheduled Tribe	-0.521***	-1.114***
	(0.0285)	(0.0640)
Other Backward caste	0.808***	1.988***
	(0.0315)	(0.0712)
Education of HH head(base:Not-literate/Informal Edu)		
Secondary and below	-0.188***	-0.402***
	(0.0231)	(0.0525)
Higher secondary and graduate	-1.380***	-3.053***
	(0.0268)	(0.0620)
Occupation of HH head(base:Casual work-Ag/others)		
Salaried work - Ag/others	-0.589***	-1.461***
	(0.0356)	(0.0813)
Own business or self-employed	-1.100***	-2.653***
	(0.0397)	(0.0890)
Nonworking adults and children, Family	-0.587***	-1.527***
	(0.0363)	(0.0813)
Common property resources, Livestock management	-0.0825**	-0.331***
	(0.0406)	(0.0915)
Others	-1.645***	-4.023***
	(0.0307)	(0.0685)

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Table 7.4 – *Continued from previous page*

	(1) Days	(2) Payment
Poorest of poor/poor (base: Not so poor/Not poor)	0.356***	0.766***
	(0.0182)	(0.0418)
Household size	-0.00525*	0.0128*
	(0.00303)	(0.00682)
Household head age	0.0994***	0.231***
	(0.000364)	(0.000822)
Household head age squared	-0.00148***	-0.00338***
	(6.55e-06)	(1.50e-05)
Hours watched TV	-0.339***	-0.754***
	(0.00889)	(0.0204)
Log land owned	-0.228***	-0.518***
	(0.0397)	(0.0908)
Log land owned squared	0.129***	0.327***
	(0.0173)	(0.0396)
None/rare participation in village meetings(base:Frequent attendance)	-0.0146	-0.0368
	(0.0132)	(0.0299)
Constant	-21.00***	-47.94***
	(0.0159)	(0.0357)
Sigma	3.080***	7.008***
	(0.00789)	(0.0181)
F-value of the joint test of political variables	604.84	616.83
P-value of the F-test	0.00	0.00
Log pseudo likelihood	-1057.06	-1317.96
Pseudo R2	0.159	0.129
Observations	1,013	1,013
Clusters	161	163

Note: Robust standard errors clustered at the village level in parentheses. *** p<0.01,** p<0.05,* p<0.1. All regressions include village fixed effects. Politically Active refers to intense party activities such as attending rallies, campaigning, attending meetings, giving speeches and writing pamphlets; Politically Inactive households do not participate in political activities. The Unaffiliated category includes those not affiliated with any party or those affiliated with fringe parties that are non-UPA and non-UPA-rival. The involvement level of all unaffiliated households was set to ”Politically Inactive”. Nonworking adults are pensioners/rentiers, dependents, students and those focusing on household’s chores.

Table 7.5: Probit Regression of Job-Card Ownership on Political Affiliation and Political Activism

	(1)	(2)
Village leader's party	UPA	non-UPA
Household Political Affiliation (base: UPA-inactive)		
UPA active	0.169	0.0431
	(0.153)	(0.421)
UPA-Rival inactive	-0.0187	-0.174
	(0.157)	(0.318)
UPA-Rival active	0.364	-0.453
	(0.264)	(0.372)
Unaffiliated	0.223	-0.0760
	(0.166)	(0.294)
Social groups (base: Forward Caste)		
Scheduled Caste	0.570***	1.558***
	(0.188)	(0.437)
Scheduled Tribe	-0.0414	0.642
	(0.279)	(0.543)
Other Backward caste	0.366**	0.857**
	(0.169)	(0.360)
Education of HH head(base:Not-literate/Informal Edu)		
Secondary and below	0.0800	0.0981
	(0.133)	(0.250)
Higher secondary and graduate	-0.332**	-0.732**
	(0.167)	(0.350)
Occupation of HH head(base:Casual work-Ag/others)		
Salaried work - Ag/others	-0.126	-0.344
	(0.133)	(0.263)
Own business or self-employed	-0.286*	-0.728*
	(0.164)	(0.413)
Nonworking adults and children, Family	-0.137	0.0958
	(0.213)	(0.516)
Common property resources, Livestock management	0.00265	0.0749
	(0.335)	(0.559)
Others	-0.921**	0.991
	(0.419)	(0.760)

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Table 7.5 – *Continued from previous page*

	(1)	(2)
	Village leader's party	
	UPA	non-UPA
Poorest of poor/poor (base: Not so poor/Not poor)	0.299**	0.246
	(0.117)	(0.276)
Household size	0.0327	0.0535
	(0.0305)	(0.0669)
Household head age Household	0.0816***	0.112**
head age squared Hours	(0.0284)	(0.0512)
	-0.0009***	-0.00124**
watched TV	(0.000290)	(0.000559)
	-0.128***	-0.144*
	(0.0401)	(0.0827)
Log land owned	-0.0186	0.465
	(0.173)	(0.365)
Log land owned squared	-0.0194	-0.00701
	(0.0741)	(0.129)
None/rare participation in village meetings(base:Frequent attendance)	0.139	0.00286
	(0.113)	(0.228)
Constant	-2.760***	-3.608***
	(0.767)	(1.359)
F-value of the joint test of political variables	3.82	2.67
P-value of the F-test	0.43	0.61
Log pseudo likelihood	-606.79	-168.19
Pseudo R2	0.240	0.283
Observations	1,153	340
Clusters	166	53

Note: Robust standard errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include village fixed effects. Politically Active refers to intense party activities such as attending rallies, cam- paigning, attending meetings, giving speeches and writing pamphlets; Politically Inactive households do not participate in political activities. The Unaffiliated category includes those not affiliated with any party or those affiliated with fringe parties that are non-UPA and non-UPA-rival. The involvement level of all unaffiliated households was set to "Politically Inactive". Nonworking adults are pensioners/rentiers, dependents, students and those focusing on household's chores.

Table 7.6: Robustness check, Redefining political categories as Very-active and Less-active. Tobit Re- gression of MGNREGS Benefits on Political Affiliation and Political Activism

	(1) Days	(2) Payment
Household Political Affiliation (base: UPA-less active)		
UPA very active	0.103***	0.231***
	(0.0151)	(0.0331)
UPA-Rival less active	0.531***	1.051***
	(0.0152)	(0.0326)
UPA-Rival very active	1.136***	2.566***
	(0.0366)	(0.0793)
Unaffiliated	0.667***	1.280***
	(0.0138)	(0.0301)
Social groups (base: Forward Caste)		
Scheduled Caste	1.230***	2.690***
	(0.0250)	(0.0550)
Scheduled Tribe	-0.807***	-1.706***
	(0.0245)	(0.0524)
Other Backward caste	0.653***	1.561***
	(0.0254)	(0.0559)
Education of HH head(base:Not-literate/Informal Edu)		
Secondary and below	-0.0122	-0.111***
	(0.0187)	(0.0409)
Higher secondary and graduate	-1.049***	-2.365***
	(0.0226)	(0.0511)
Occupation of HH head(base:Casual work-Ag/others)		
Salaried work - Ag/others	-0.881***	-1.980***
	(0.0334)	(0.0726)
Own business or self-employed	-1.101***	-2.474***
	(0.0323)	(0.0700)
Nonworking adults and children, Family	-0.882***	-2.020***
	(0.0323)	(0.0696)
Common property resources, Livestock management	-0.793***	-1.883***
	(0.0342)	(0.0733)
Others	-1.895***	-4.298***
	(0.0240)	(0.0510)
Poorest of poor/poor (base: Not so poor/Not poor)	0.756***	1.613***

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Table 7.6 – *Continued from previous page*

	(1) Days	(2) Payment
	(0.0152)	(0.0336)
Household size	0.0509***	0.112***
Household head age	(0.00265)	(0.00578)
	0.161***	0.344***
Household head age squared	(0.000306)	(0.000670)
	-0.00209***	-0.00446***
Hours TV watched	(5.65e-06)	(1.24e-05)
	-0.327***	-0.684***
Log land owned	(0.00809)	(0.0178)
Log land owned squared	0.328***	0.631***
	(0.0410)	(0.0888)
None/rare participation in village meetings(base: Frequent attendance)	-0.161***	-0.271***
	(0.0160)	(0.0353)
	0.123***	0.203***
	(0.00973)	(0.0215)
Constant	-22.32***	-48.18***
Sigma	(0.0136)	(0.0297)
	2.957***	6.485***
	(0.00625)	(0.0140)
F-value of the joint test of political variables	691.09	582.01
P-value of the F-test	0.00	0.00
Log pseudo likelihood	-1418.05	-1769.15
Pseudo R2	0.148	0.120
Observations	1,152	1,152
Clusters	166	166

Note: Robust standard errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include village fixed effects. In the alternate definition of political activism used in this regression, Politically Very Active refers to intense party activities such as campaigning, attending meetings, giving speeches and writing pamphlets; Politically Less Active refers to households attending rallies, and also not participating in political activities. The Unaffiliated category includes those not affiliated with any party or those affiliated with fringe parties that are non-UPA and non-UPA-rival. The involvement level of all unaffiliated households was set to ”Politically Inactive”. Nonworking adults are pensioners/rentiers, dependents, students and those focusing on household’s chores.

Table 7.7: Testing the Mechanism of Spill-Over Effects from Active-Inactive members. Tobit Regression of MGNREGS Benefits on Political Affiliation and Political Activism

VARIABLES	(1) Days	(2) Payment
Household Political Affiliation (base: UPA-inactive)		
UPA active	0.248***	0.576***
	(0.0253)	(0.0552)
UPA-Rival inactive	0.525***	0.880***
	(0.0179)	(0.0384)
UPA-Rival active	0.804***	1.914***
	(0.0296)	(0.0647)
Unaffiliated	0.730***	1.433***
	(0.0175)	(0.0381)
Rival X Proportion of active households	-0.141***	-0.265**
	(0.0541)	(0.118)
Social groups (base: Forward Caste)		
Scheduled Caste	1.224***	2.685***
	(0.0256)	(0.0563)
Scheduled Tribe	-0.840***	-1.777***
	(0.0245)	(0.0529)
Other Backward caste	0.652***	1.569***
	(0.0252)	(0.0556)
Education of HH head(base:Not-literate/Informal Edu)		
Secondary and below	-0.0126	-0.117***
	(0.0184)	(0.0403)
Higher secondary and graduate	-1.057***	-2.392***
	(0.0232)	(0.0521)
Occupation of HH head(base:Casual work-Ag/others)		
Salaried work - Ag/others	-0.887***	-1.995***
	(0.0325)	(0.0706)
Own business or self-employed	-1.110***	-2.501***
	(0.0324)	(0.0704)
Nonworking adults and children, Family	-0.879***	-2.007***
	(0.0325)	(0.0698)
Common property resources, Livestock management	-0.790***	-1.876***
	(0.0347)	(0.0744)
Others	-1.964***	-4.516***
	(0.0286)	(0.0619)

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Table 7.7 – *Continued from previous page*

VARIABLES	(1) Days	(2) Payment
Poorest of poor/poor (base: Not so poor/Not poor)	0.776***	1.663***
	(0.0152)	(0.0336)
Household size	0.0500***	0.112***
	(0.00256)	(0.00557)
Household head age	0.160***	0.341***
	(0.000303)	(0.000662)
Household head age squared	-0.00208***	-0.00444***
	(5.63e-06)	(1.24e-05)
Hours watched TV	-0.333***	-0.697***
	(0.00788)	(0.0174)
Log land owned	0.344***	0.678***
	(0.0405)	(0.0878)
Log land owned squared	-0.167***	-0.290***
	(0.0157)	(0.0347)
None/rare participation in village meetings(base:Frequent attendance)	0.154***	0.287***
	(0.0103)	(0.0226)
Constant	-21.86***	-48.51***
	(0.0134)	(0.0291)
Sigma	2.957***	6.485***
	(0.00623)	(0.0139)
F-value of the joint test of political variables	549.01	483.17
P-value of the F-test	0.00	0.00
Log pseudo likelihood	-1417.94	-1768.88
Pseudo R2	0.1484	0.1204
Observations	1,152	1,152
Clusters	166	166

Note: Robust standard errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include village fixed effects. Politically Active refers to involvement in party activities such as attending rallies, campaigning, attending meetings, giving speeches and writing pamphlets; Politically Inactive households does not participate in political activities. The Unaffiliated category includes those not affiliated with any party or those affiliated with fringe parties that are non-UPA and non-UPA-rival. The involvement level of all unaffiliated households was set to ”Politically Inactive”. Nonworking adults are pensioners/rentiers, dependents, students and those focusing on household’s chores.

Table 7.8: Tobit regression of MGNREGS benefits on Political Affiliation and Political Activism in non- UPA sarpanch villages

	(1) Days	(2) Payment
Household Political Affiliation (base: UPA-inactive)		
UPA active	-0.0601	-0.0854
	(0.844)	(1.663)
UPA-Rival inactive	0.163	0.462
	(0.466)	(0.947)
UPA-Rival active	-0.969	-1.825
	(0.608)	(1.247)
Unaffiliated	-0.242	-0.226
	(0.483)	(0.973)
Social groups (base: Forward Caste)		
Scheduled Caste	2.213**	4.972***
	(0.921)	(1.815)
Scheduled Tribe	0.524	1.536
	(1.009)	(2.018)
Other Backward caste	1.254*	3.024**
	(0.758)	(1.483)
Education of HH head(base:Not-literate/Informal Edu)		
Secondary and below	-0.649	-1.289
	(0.442)	(0.900)
Higher secondary and graduate	-2.430***	-4.996***
	(0.830)	(1.622)
Occupation of HH head(base:Casual work-Ag/others)		
Salaried work - Ag/others	-1.261**	-2.423**
	(0.603)	(1.177)
Own business or self-employed	-1.630*	-2.867
	(0.898)	(1.777)
Nonworking adults and children, Family	-0.688	-1.039
	(0.944)	(1.917)
Common property resources, Livestock management	-2.205*	-4.391*
	(1.288)	(2.582)
Others	-0.937	-1.814
	(1.199)	(2.330)

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Table 7.8 – *Continued from previous page*

	(1) Days	(2) Payment
Poorest of poor/poor (base: Not so poor/Not poor)	0.504	0.994
	(0.590)	(1.183)
Household size	0.0882	0.183
	(0.124)	(0.250)
Household head age	-0.0546	-0.0918
	(0.0459)	(0.0950)
Household head age squared	0.000185	0.000123
	(0.000512)	(0.00107)
Hours watched TV	-0.311*	-0.607*
	(0.160)	(0.334)
Log land owned	1.091*	2.047
	(0.634)	(1.268)
Log land owned squared	-0.206	-0.334
	(0.299)	(0.588)
None/rare participation in village meetings(base:Frequent attendance)	0.242	0.498
	(0.433)	(0.890)
Constant	1.918***	3.705***
	(0.103)	(0.283)
Sigma	2.444***	4.738***
	(0.124)	(0.204)
F-value of the joint test of political variables	1.58	1.53
P-value of the F-test	0.180	0.193
Log pseudo likelihood	-450.95	-577.65
Pseudo R2	0.123	0.079
Observations	339	339
Clusters	53	52

Note: Robust standard errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include village fixed effects. Politically Active refers to intense party activities such as attending rallies, campaigning, attending meetings, giving speeches and writing pamphlets; Politically Inactive households do not participate in political activities. The Unaffiliated category includes those not affiliated with any party or those affiliated with fringe parties that are non-UPA and non-UPA-rival. The involvement level of all unaffiliated households was set to ”Politically Inactive”. Nonworking adults are pensioners/rentiers, dependents, students and those focusing on household’s chores.

Online Appendices

Online Appendix A

http://www.3ieimpact.org/media/filer_public/2018/08/23/ow41223-nrega-appendix-a.pdf

Online Appendix B

http://www.3ieimpact.org/media/filer_public/2018/08/23/ow41223-nrega-appendix-a.pdf

Online Appendix C

http://www.3ieimpact.org/media/filer_public/2018/08/23/ow41223-nrega-appendix-c.pdf